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400 Virginia Ave., S.W. Washington, D. C. 20024

Telephone: (202) 554-2345

SUBCOMMITTEE ON STRUCTURAL ENGINEERING

OF THE

ADVISORY COMMITTEE OF REACTOR SAFEGUARDS

Wednesday, July 1, 1981
Cochiti Room
Regent Hotel
Albuquerque, New Mexico

The meeting was convened, pursuant to notice, at 8:18 a.m., Dr. Chester Siess, Chairman, presiding.

PRESENT:

ACRS

J. Carson Mark
David Ward
Michael Bender

Consultant

Zenon Zudans

PRESENT (continued):

Designated Federal Employee

Richard Savio

NRC staff

James F. Costello
Roger Kenneally

SPEAKERS:

Walter Von Riesemann
Charles Anderson
Elton Endebrock
Joel Bennett

I N D E X

P R O C E E D I N G S

DR. SIESS: Good morning. I am Chester Siess, Chairman of the ACRS Subcommittee. On my left is Mr. Michael Bender, on the far right, Dr. Carson Mark, and the vacant seat here belongs to Mr. David Ward, who will be in shortly -- he either can't find the room or thinks it was 8:30. We have one of our Subcommittee consultants present today, Dr. Zenon Zudans, sitting at the end of the table.

The purpose of the meeting is to discuss three of the research programs that are being carried out by the Office of Nuclear Regulatory Reseach of NRC, and what is now called -- I think they call it the Mechanical Structural Engineering Branch. I prefer to call it the Structural Mechanical Engineering Branch.

The projects -- one project is on safety margins for containments, being carried out at Sandia Laboratory, and a second one is on the safety margins for Category I structures, which is being carried out at Los Alamos National Laboratory -- it is LANL now, isn't it?

MR. MARK: Right.

DR. SIESS: You are not scientific any more. Just national. And a project on buckling of steel containments, also at Los Alamos. We will take them up in that order. Our meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the Government and

Sunshine Act, and we have a designated federal employee, Mr. Richard Savio, from the ACRS staff, who will be back shortly.

The rules for participation by the public in today's meeting have been announced as part of the notice that appeared in the Federal Register. A transcript will be kept -- it is not being kept at the moment, but that is not important. The reporter is using a tape recorder, so whoever speaks, to be on the record, should use the microphone.

Please give your name when you speak, so that it will be on the record -- at least the first time.

We have received no written statements from members of the public and no request to make oral statements, so we won't take any time on that matter. The meeting will go from now until close of business, with some interruption at some time for lunch, I am not sure when. Mr. Bender has to leave about 1 o'clock and we will accommodate him to the extent possible, but we will take whatever time is needed to discuss things completely.

We have got, I think, a fairly leisurely schedule and we have time to go into anything we want to within the announced framework. Are there any questions or comments from any members of the Subcommittee at this time? You have the agenda, which will be the three items in the order I indicated.

Then we will start with an overview or perspective on the research program and Dr. James Costello is going to present that, I believe, from the NRC research staff. Jim?

MR. COSTELLO: My name is James Costello with the Office of Nuclear Regulatory Research. I think I would like to spend a few minutes this morning just giving a little bit of background on the three programs being discussed. The first one we are talking about is entitled Containment Safety Margins, being performed at Sandia Laboratory. The principal investigator is Walter Von Riesemann.

The principal question which motivates the research is an attempt to get a handle on where and how and what load level a containment will lose its capacity to contain.

The second program that we will discuss is the one on safety margins for Category I structures. It is similarly motivated. The contractor is Los Alamos National Laboratory. The co-principal investigators are Chuck Anderson and Elton Endebrock. The NRC Program Manager is Roger Kenneally, who is here today.

MR. MARK: Mr. Costello, do these questions -- I am sure they include overpressure. Do they also include seismic and other such disturbances?

MR. COSTELLO: That is right. There are also, as Walter Von Riesemann will discuss, we are also looking down the road on attempting to get a handle on containment capacity under a lateral seismic type of load.

DR. ZUDANS: Plus the thermal effects that are associated with pressure, and all those things. Plus missiles and many other things. There is more than one failure mode.

MR. COSTELLO: To get back to the containment question, on the containment we are looking, first, at pressure. We are giving serious thought and planning to look at capacity under lateral load representative of seismic loading. We have not, and we are not including localized loadings like missile effects.

DR. ZUDANS: When you are talking about capacity, you cannot exclude anything, because those are not things that you can superimpose.

DR. SIESS: Yes, but the immediate objective of this program is in relation to the post-accident hydrogen and post-accident overpressure.

DR. ZUDANS: Well, that is accompanied by temperature, too.

DR. SIESS: Yes, but am I correct, the immediate concern here, the first step in this Sandia program, relates to the graded core cooling rulemaking? Not LOCA pressures -- what is it? steam overpressure? -- I forget what mode of failure it was in WASH-1400.

MR. COSTELLO: That is correct, Professor Siess.

DR. SIESS: Now, is it strictly the static? To what extent is the hydrogen burn or detonation or local impulsive loading from a local detonation in the picture, and at about what stage would you say?

MR. COSTELLO: Dynamic or pulse loads we intend to get to after static overpressure, if they turn out to be of significant interest. The major thrust is ability to predict performance under pressure loads.

MR. BENDER: I am confused. We may as well get the air cleared right now. The containments initially were designed on the basis of the peak pressure releases from a double-ended pipe break essentially, with some thermal loading. Then, more recently --

DR. SIESS: And seismic.

MR. BENDER: And seismic events were put into the picture somewhere along the way. More recently we have looked at questions having to do with the capability of containments with some kind of hydrogen pressure loading. Now, what conditions are you addressing here when you talk about safety margins?

MR. COSTELLO: Okay, I guess -- let me, I guess, just emphasize what we are looking at here is the fundamental question of capacity under static overpressure, capacity under dynamic pressures, and capacity, we think we will try to get, under lateral loadings if we can figure out how to do it.

MR. BENDER: Superimposed separately or how?

MR. COSTELLO: Separately.

MR. BENDER: But don't you need to combine them in some way? I am not talking about statistically now, but I am just talking about --

MR. COSTELLO: I guess I would like to emphasize that what we are after is the basic building block in assessing capacity under a wide range of load scenarios.

DR. SIESS: Well, look, let's back up a bit. In the first place, you have a project going on, I think it is at MIT, that is looking at containment loads from the standpoint of hydrogen. Right?

MR. COSTELLO: That is correct.

DR. SIESS: This would be both burn and detonation and possible local detonations. These are potential loadings. Curtis yesterday told us about the work that is being done, I forget where, looking at threats to containment -- that was the term he used, which involved a lot of other things besides loads on containment.

So, the idea of beyond DBA loads is still being developed.

MR. COSTELLO: That is right.

DR. SIESS: As Mr. Bender put it, the original object of the containment was to contain the LOCA -- that is a very significant pressure for the kind of structure we are talking about and that has been licked, we have got them built; they are not always leaktight, but structurally they take the loads.

The seismic is another one. Our present design practice does combine seismic and LOCA loads with reduced load factors and, of course, there are the temperature loads. Now, the new thrust, particularly on this first project, is a direct consequence of TMI-2 and the degraded core cooling rulemaking -- and some day I am going to find out what "degraded" modifies. I don't think it modifies "rulemaking," but I have never been sure whether it is the cooling or the core that was degraded -- probably both.

That rulemaking is some distance away and there is a big thrust in research to try and get some basis for making that rule. We don't know yet what we are going to end up asking the containment to contain. There are already some preliminary rules or interim rule on hydrogen that says you have got to contain, what is it?, 75 percent -- the near terms are 75 and the -- I know it is 75 percent in one rule and 100 percent in another, and the Division 2 conditions, you know, these are extremist type things.

That we know is out. Whether it is going to stand up, we don't know. We don't know yet what else the degraded core rulemaking is going to say containments are supposed to contain. But we do know right now that we are concerned about predicting simply the pressure capacity combined with the temperature from a steam overpressure event, which was one of the WASH-1400 events, and probably the slow hydrogen burn, because we have seen preliminary rulemaking that requires that.

We have had the Zion-Indian Point study where somebody had to estimate what the containment could do. We have had Sequoyah and the ice condensers where somebody has to estimate it. We heard presentations yesterday from GE on the MARK-III and all of that.

So, if I understand the current activity, which is F.81, what is left of it, and FY82, is starting off on this project with the basically simple overpressure, and I am not sure whether temperature is in it.

MR. COSTELLO: Temperature is not.

DR. SIESS: It is a step-by-step process.

MR. BENDER: I would like at least to sort out what I understand it to be. If I look at overpressure in the sense in which we heard yesterday, it would be the pressure generated by continual heating of the core without heat removal mechanisms.

DR. SIESS: That is one.

MR. BENDER: And we may be trying to find out how high you could go in order to set a margin between when you might release whatever it is, if you want to use venting for that mechanism, and what the capability is of the containment.

DR. SEISS: That is one aspect.

MR. BENDER: Is that the aspect we are concentrating on now?

MR. COSTELLO: Overpressure.

DR. SIESS: Now, Mr. Zudans is concerned about temperature. I have got a feeling that when we look at the design limits, at least in the interim rule on hydrogen, you are so far into the inelastic range, that the temperature effects have just about wiped themselves out.

DR. ZUDANS: I have no qualms with whatever is being done. I think that it is correct to be clear at the very beginning what it is that we are after. That is the whole issue. I am not saying that you are not doing the right thing. When I read that principal question that you are asking yourself, you are asking yourself a question that goes very far into the nonlinear range, and whatever you do, you lost the luxury of any kind of a superimposition.

You may have degraded materials properties in a structure, so if you choose to just look at the pressure, you understand that that is not the whole picture.

MR. COSTELLO: That is correct, yes.

DR. ZUDANS: Well, if that is what is satisfactory right now, and maybe it is in connection with this degraded core cooling issue, whether or not to use cement, maybe that is good enough, but one thing you cannot ignore, and that is degraded materials properties of a structure.

That is absolutely necessary. Now, I will say that this is not the whole picture and when you talk about failure modes, you are out of the simple realm; it is a complex matter.

DR. SIESS: Well, there won't be linearity and the temperature forces will probably be wiped out -- you are talking about temperature effects on the materials?

DR. ZUDANS: That is right. I am not so concerned about the temperature being able to destruct the structure, no.

DR. SIESS: But this idea that this thing is going to go on and on and get into seismic, as soon as you start talking about the seismic resistance, you are into about three other research projects.

DR. ZUDANS: But you cannot do them separately.

DR. SIESS: I am not concerned about how separate they are. Right now I am trying to find out what -- to get clear what the questions are that research is working on. This is one project, and we will see where it fits into the whole picture.

This project is not going to answer all the questions; it may not even answer the ones we are asking.

DR. ZUDANS: I just want to know what this project is supposed to answer.

DR. SIESS: Do you agree that I stated what it --

MR. COSTELLO: I think that is reasonable, yes.

DR. SIESS: Let me get something else clear.

MR. BENDER: Maybe you are clear, but I am not.

DR. SIESS: I am trying to clarify the questions, Mike. There are two objectives in this meeting. One is to get us informed about the nature of the research. The other is to give us a basis for making recommendations to the Commission and to the Congress about research programs and budgets. Now, as far as the latter is concerned, the recommendations that we will be making to the Commission next week, and to the Congress next February, relate to the FY83 program, which is, you know, a fair distance away.

So, one thing we need to get clear, Jim, as we go through this, is what work is -- as you talk about the work, some of it is FY81, it is underway right now, in progress, has been going on; some of it is FY82, which is the next stage in the thing, and clearly some of it is going to be FY83, and I doubt if we are going to be hearing much that isn't going into '83, but if there is something that is going to be finished by '82, we are still interested in it, in knowing what is going on, but we need to know that it is not an '83 program as far as commenting on the budget.

I would like to keep that aspect of it straight. Now, we will go back and let Mr. Bender continue his questioning.

MR. BENDER: I wanted to -- I accept the idea that we are looking at pressure. Then I have to look at when the pressure is imposed and what things exist at that time. I guess the presumption I would make is that while I don't know that we need to deal with degraded materials properties, I listened yesterday to TVA's presentation of the temperature that was associated with their filtered containment system, and the temperature was 750 degrees Fahrenheit, which says it is up in the range where the strength of steel is not --

DR. SIESS: Was that under containment or in the filter?

MR. BENDER: It was in the fluid.

But I am not trying to define that temperature. All I am trying to say is, if we are going to look at the pressure, we have to look at the associated conditions and temperature is one thing, the thermal distribution of the structure is another. We start with some thermal distribution and how fast it changes in the structure with time is an important consideration.

Now, I would like to know that we are addressing that whole thing. If I just get the pressure instruments and the rest of it is dominating, the answer is not going to be very useful. So, I hope that question that you have got written up there that says containment structure failure modes and associated load levels, when load levels mean the combination of circumstances that exist when the pressure is applied.

MR. COSTELLO: I think that is a fair statement of what the real question is. Now, I would like to respond to your comment by saying we are realistic, we try to be realistic and not delude ourselves that a single research program is going to answer all the questions.

MR. BENDER: I am not trying to decide on the research program. I am trying to decide what the question is and then decide -- I am playing Dr. Siess' game, and he suggested it.

DR. SIESS: Let's make a distinction. I think we could make a distinction in nomenclature, it may not be the one you use and we can find another one, between a research program and a research project. Let's relate a program to the question and then the projects to the subquestions or the elements of the question.

Now, you may want to use project and contract, I don't know what would be appropriate nomenclature, but research has certain -- well, NRC has certain questions it needs to answer and some of them are going to end up assigned to you to answer, and some of them NRR is going to get at through other things, and some of them the Commission may decide without any data, I don't know.

But the program overall and the individual projects -- of course, one concern is how the individual projects do relate to each other. As I mentioned earlier, you have got one project looking at loads and another project looking at the structure in response to those loads, which, depending on the expertise required, is a perfectly logical division.

MR. COSTELLO: And, as you pointed out, a great number of undertakings in the accident area Curtis talked about yesterday.

Let me try and recover from my perhaps disastrous attempt to --

DR. SIESS: Well, we just took you farther than --

MR. BENDER: We are trying to understand it ourselves.

MR. COSTELLO: To offer the wide question and let the individual presentations by the principal investigators hone in on what the actual tasks -- or what actual tasks are being done in the projects in attempts to get a grip on those questions.

DR. SIESS: Jim, one of the problems at this stage of the presentation, and it will probably come up a little bit later, usually in these things people devote a great deal of time to telling us what they are doing, but before we get into the what, we really are trying to clear on the why. To me, the why is in terms of questions and if we don't really know what question we are trying to answer, it is a little hard to judge either the probability of getting the answer or the usefulness of the answer when we get it.

Clearly, in this area, because of the uncertainty on which way the degraded core rulemaking is going to go, I don't think anybody on the Commission has very clear just what the questions are, because nobody knows where they are going and they are sort of exploring the questions now, and maybe the direction we go will depend on which direction we can go -- I sort of hope so. It is not going to be all that idealistic.

DR. ZUDANS: Are we now clear as to what your main question is? I read it, from what you said, that you will take care of overpressure in this limited scope and later on you proceed to look at lateral loads. I didn't hear you commit that you will look at the pressures, but also consider the total environment that exists at that time, as Mike defined, meaning temperature.

Because you may have failure modes that have nothing to do with overpressurization.

MR. COSTELLO: That is correct.

DR. ZUDANS: Your seals may degrade and the temperature then leak.

MR. COSTELLO: That is correct.

DR. ZUDANS: Does that fall under that question?

MR. COSTELLO: That is part of the question. As Dr. Von Riesemann makes his presentation today, you will find out that degradation of seals is not something being considered inthe Sandia program.

MR. BENDER: Well, if you will define the bounds of the project that you are working on in addressing the more general question --

DR. SIESS: We see a problem right here. This is the structural group and they are, I think, quite legitimately working on the structural aspects of the problem, rather than what I might call mechanical -- seals are mechanical. But then that raises the question of -- seals are certainly part of the question -- is there some level, you know, above the Branch that realizes that seals are part of the question and somebody has been assigned that part of it?

MR. COSTELLO: The answer to your first question about existence is definitely yes, the Division level. The second question, I think the answer is yes, but I don't know, because I haven't been assigned it.

But there is recognition. In fact, I had a discussion at some length on a few occasions with Mr. Arlotto about what parts of the problem are being covered by the work Sandia is doing and what parts are not.

MR. BENDER: Has he asked that question?

MR. COSTELLO: Yes, sir.

MR. BENDER: He understands the problem?

MR. COSTELLO: Oh, yes.

Well, let me, if I can, then, pick up rather quickly and offer you the broad brush questions,as we see them, about underlying or motivating the research effort at Los Alamos National Laboratory on safety margins for Category I structures.

The tough question that is going to involve a lot of interaction with other disciplines is the first one; that is, do you know, can you set deformation limits reliably and given that you can, can you predict well enough how the structure will perform for some postulated loading, so that you can decide whether or not you will have equipment function in Category I structures.

Again, this is --

MR. BENDER: Maybe you had better define Category I structures, just to be sure we all know.

MR. COSTELLO: Okay, Category I structures other than containment.

MR. BENDER: This is auxiliary buildings, interior shield walls, reactor pedestal?

MR. COSTELLO: Other structures other than the containment, yes.

MR. BENDER: Steam generator supports, et cetera. Or is it limited to concrete structures?

MR. COSTELLO: I believe the first thrust is limited to concrete structures. The Program Manager is Roger Kenneally, and he is here today.

MR. KENNEALLY: Roger Kenneally, NRC staff. Chet, on the initial undertaking we are looking at typical Category I structure buildings, seismic Category I. These are the fuel buildings, the auxiliary buildings, and the like. Naturally, turbine buildings wouldn't be included in this. We are not going into the steam generator supports or reactor pedestals currently.

DR. SIESS: This is mostly outside containment?

MR. KENNEALLY: That is correct.

DR. ZUDANS: The turbine building is not Category I, is it?

MR. KENNEALLY: That is right, and we are not looking at that.

MR. BENDER: Could I ask, are there deformation limits that could be associated with seismic events, pressure releases, or impacts? I am trying to understand what deformation limits you are dealing with. Which things should I be thinking about?

MR. KENNEALLY: In terms of deformation, we are really trying to figure out, we are trying to define what is failure of the Category I structure. Is it the structure itself collapsing or breaking apart, and we are looking at it as it cannot perform its intended function. Is that function to protect equipment or the like, and we are trying to see what deformations might be acceptable before we have to worry about piping breaking and all that.

DR. SIESS: I think you left out a step. In looking at -- and correct me, if I am wrong -- in looking at safety margins, you are not stopping at an elastic limit state?

MR. KENNEALLY: That is correct.

DR. SIESS: And I will use the term "limit state" -- it is not that formal, but it is a good word. You are looking at inelastic behavior and as soon as you start looking at inelastic behavior, a possible limit state is a deformation, an excessive deformation for some function of the structure or something that is attached to it or held up by it.

Now, there are other limit states, but most people have the feeling that the deformation limit state is likely to govern some aspects of it, and it certainly can't be ignored, because most of these structures aren't just sitting there to be structures. They are sitting there for some other function, which may be impaired by deformation.

The implication of inelastic behavior is very, very strong.

MR. BENDER: But there are certain service conditions associated with deformation. If it were the support for a primary cooling pump, the floor that it sits on, then there would be something associated with the change in position of the pump that would be governing it. I guess the floor itself doesn't serve any purpose except to keep the pump in place for that particular application.

Is that the way you are trying to deal with it?

MR. KENNEALLY: Initially the first phase of the program is to try to get an idea of the deformation. It will be the third phase when we get in and actually do some fairly large-scale testing and we haven't really developed a good third phase program plan yet, where we can say what is the actual equipment within that we are trying to look at.

The Structural Branch really isn't worried about the functioning of the equipment. We are worried about the structure and the support of the equipment.

DR. SIESS: This project now does not address the first question.

MR. KENNEALLY: We are working toward that.

MR. BENDER: But you are not there yet.

MR. KENNEALLY: That is right.

MR. BENDER: You are going to deal, then, with how you decide whether a wall will stay in place. It is about that general, isn't it?

DR. SIESS: I think what you are trying to do now -- maybe it you are not, maybe you should be -- is to develope means for predicting with some kind of reliability the load deformation characteristics of the structures. Then later on somebody else will decide or you, depending on the function, at what deformations the component or some element has failed and then you will be able to say, well, at this load, that deformation will be reached and that is the limit.

The initial thrust is really the structure itself and the load deformation characteristics.

MR. BENDER: If y u do that, then you are going to have to decide how the load is going to be applied.

DR. SIESS: Oh, yes.

DR. ZUDANS: Well, the first question cannot be answered by structures people. It has nothing to do with that.

DR. SIESS: Well, some aspects of it may involve structure.

DR. ZUDANS: I don't see how. It is operability of equipment; that has nothing to do with structure.

DR. SIESS: Well, I see, the way it is stated, you are right.

DR. ZUDANS: That has to come from someone else and say here are the limits that we can tolerate.

MR. COSTELLO: You are correct, Dr. Zudans, and that is why I said it is one of the hardest -- of the two questions listed there, that is going to be the harder one to get the grip on.

DR. ZUDANS: Someone else has to tell you what are the deformation limits and then you have to look at --

DR. SIESS: But you are fortunate here in that you can look at the structure, first, and give that other person your load deformation curve and let him decide what load his equipment is not going to work at.

DR. ZUDANS: But then you need the whole spectrum of loads.

DR. SIESS: Only those loads that produce deformations of a certain kind.

DR. ZUDANS: There is no load that does not produce deformation.

DR. SIESS: Well, some don't produce large ones.

MR. BENDER: I suspect we are using up their time trying to understand the question.

DR. ZUDANS: I think it is better to put it like Chet's philosophy, and I agree with that. Make it clear at the beginning what are we going to listen to.

DR. SIESS: So, basically, the Structures Branch is looking now, and I would suggest probably through '83, at that second question up there.

DR. ZUDANS: That would be okay, that would be all right.

MR. COSTELLO: Your perception is correct. That is the bulk of our money being spent on question 2.

DR. SIESS: Who is looking at the first one?

MR. COSTELLO: There is some work going on in mechanical engineering.

DR. SIESS: What about SSMRP, fragility?

MR. COSTELLO: There is likely to be some there, also.

DR. SIESS: Reliability of pumps and valves?

MR. COSTELLO: We hope that the answers will come and we will be able to mesh these efforts together. The fact that Dr. Zudans points out, that it is not a structural engineering undertaking -- on the other hand, the undertaking will be meaningless without out.

MR. BENDER: The bottom line is.

DR. SIESS: And here the liaison is pretty clear, because the Mechanical Structural Branch is --

DR. ZUDANS: The SSMRP program told us that they had to go and define all limits for every system and then include it in their considerations. That is where that information should be developed and given to you. If it is already done, I don't know.

MR. COSTELLO: I can assure you it is not.

MR. BENDER: This is a chicken and egg proposition. If you try to define every limit state for every piece of equipment, it is such a massive job you would never get it. We are trying to find out whether we need to define it very discretely. If we can show that the deformations in the structures supporting them are such that the equipment doesn't move very much, I suppose we won't have to worry about that equipment.

Hopefully, that is the attack you are going to make on it.

DR. SIESS: Of course, it works the other way. If somebody has got equipment that can move 6 feet, we won't have to worry about the structure falling down.

MR. BENDER: That is right, that is the other half of the egg.

DR. SIESS: We really don't have to worry about how accurate it is. I don't think we are going to find many in that category.

Now, this project is characterized by an emphasis on inelastic behavior and dynamic?

MR. COSTELLO: That is correct.

DR. SIESS: And, basically, it is dynamic-inelastic is what you are combining there. You are trying to get near an ultimate limit state in terms of either deformation or load.

MR.COSTELLO: That is correct.

DR. SIESS: Now other work has been done on inelastic-dynamic analysis -- this is more than analysis, because you are going to try to verify it -- and some of that was done under a technical assistance contract, wasn't it?

MR. COSTELLO: I am not so sure which --

DR. SIESS: Is there anything in research on inelastic-dynamic analysis? This was one of the 6 projects on research to improve safety that never got started.

MR. COSTELLO: I don't think it ever did.

DR. SIESS: It was 380-428(?). Do you remember that? Improved seismic analysis and improved seismic analysis turned out, in most people's minds, to mean an inelastic seismic analysis, and that is one category of dynamic, is seismic. We never got anything started on it in research?

MR. COSTELLO: I guess to some extent we might consider that would be subsumed into the long-range of the SSMRP.

DR. SIESS: That is the trouble. Every time I turn around, something is being subsumed into SSMRP.

MR. COSTELLO: It is a big barracks bag.

DR. SIESS: Yes, and when you say long-range on SSMRP, my mind goes out beyond my term on ACRS, and maybe anybody's term on ACRS.

Is that all you wanted to do on that one?

MR. COSTELLO: Yes, I thought I would like to get the questions up there.

DR. SIESS: Will you or the other presenters sort of give us the time history on this stuff?

MR. COSTELLO: They will. The other presentations will involve the technical scope and something about the programmatic time schedule.

The last one we will talk about today is also at Los Alamos. Joel Bennett is here with Chuck Anderson and the NRC Program Monitor is Boris Browzin, who is not here today. He just got back from overseas.

The scope of this undertaking is smaller than the other two and the questions are, the motivating questions are more precise, less general. The questions, I say, are fairly precise, at least by comparison with the ones discussed earlier, and relate mainly to the current state of design practice for steel containments and how well the current buckling design rules work.

DR. SIESS: Now, does dynamic lateral loads mean seismic loads, or does it also include internal loads from a nonuniform pressure detonation close to the wall, or something of that sort?

MR. COSTELLO: The intent, my intent in writing the words this way was to include that. One has to have some sort of lateral load to get a potential for buckling.

MR. MARK: It would include tornado?

MR. COSTELLO: Conceivably, yes, but I doubt that that would be a dominant load.

DR. SIESS: We don't have any steel containments subject to tornado except FFTF, I think.

MR. COSTELLO: That is correct. Also, there is a shield building around --

DR. SIESS: Yes, they have all got a shield building that is supposed to protect from tornadoes and from external missiles, too.

MR. COSTELLO: To get back to your question, the initial concern that was raised, the question of the adequacy of the ASME rules when they first came up, was the possibility of getting a buckling under a seismic load, which would give you a large lateral load. But there are other lateral loads and from the wider question that would keep them in there.

DR. SIESS: One of the others that has come up is the ice condenser, where there is an asymmetry in the internals of the ice condenser and there can be a lateral pressure load that is unsymmetrical. I think Dr. Zudans is the instigator of that one.

DR. ZUDANS: I think that the first part of your question probably should be re-phrased -- not that it is wrong -- there are really no rules that would handle structures like ice condensers. The ASME rules are directed to a uniform pressure or a uniform lateral load.

DR. SIESS: But they are being used, aren't they?

DR. ZUDANS: Well, that is where the problem is. Everybody uses his own set of rules, and I think this problem is very important, and is probably properly addressed, but it does have to include the combination of loads.

MR. BENDER: I am confused about shapes at the moment. The ice condensers are sort of boxish --

DR. ZUDANS: No, the containment shell itself.

MR. BENDER: Is it the shell we are talking about?

MR. COSTELLO: The steel shells.

MR. BENDER: And is it for freestanding shells?

MR. COSTELLO: Yes.

MR. BENDER: Loaded by asymmetric pressure conditions?

MR. COSTELLO: Either asymmetric pressure or seismic.

DR. SIESS: It is essentially something that will tend to produce an overturning and a hi[illegible] compression, probably vertical compression, on one part of the shell and not uniformly.

MR. BENDER: With or without other kinds of loads?

With other kinds of loads imposed?

MR. COSTELLO: In the test program? No.

SPEAKER: The accident load and whatever else is there.

DR. SIESS: Well, that would be the pressure load plus accident. What hasn't been added in right now is this lateral load that might induce buckling. There are no rules for it. I think that is what Zenon --

DR. ZUDANS: Yes. The bigger issue is the fact that those structures are not clean cylinders. They are manufactured cylinders, they are imprecise, they are full of imperfections and full of holes, they are reinforced and non-reinforced and, therefore, there is no single set of rules that now would apply.

What is really lacking is a data base. They need experiments. I read one of your reports, that is, that fine set of experiments; I don't know what else will be produced. This is a good program and I hope that things work out all right.

MR. COSTELLO: Well, if you have no further questions --

DR. SIESS: Well, I do, and it is sort of general. I just want to mention it, because it is going to color some of the discussion that I will have later. Getting back to my simplistic definition of research as what you do to answer questions, NRC has a lot of different kinds of questions and asks a lot of different kinds of questions, and one thing that concerns me in looking at particularly the containment safety margins program, is what is an appropriate way for NRC to get questions answered.

Clearly, we need to know what kind of pressures containments can take before they begin to, as you expressed it, lost containment capacity and leak, let fission products get out to the public. That is their functional design basis. You could say we don't care whether they stand up or not, as long as they don't leak.

Now, we need to know that, we know, because we have been asking people that. Now, there are what? 75 operating plants? and more than that many under construction, and there must be at least 30 significantly different containment designs, and I mean with relatively gross differences.

There are some obvious differences between PWR's and BWR's, there are differences between prestressed concrete, steel, and prestressed and reinforced, and then within each family there are all sorts of differences.

No simple gross simplified calculations will tell you anything about the containment capacity or leak capacity, particularly. So, if you want to know what the capacity is for containment on unit one of such-and-such a plant, one way to get the answer is to ask the applicant or licensee -- it seems to be easier to ask the applicant, because he hasn't got a license yet, but there are means that the NRC has developed for asking licensees questions and getting answers. I forget what the legal procedure is, but they can do it.

Now, it has always seemed to me that so-called regulatory research, which I think Congress coined the term, has two objectives. One is to know what questions to ask and the other is to know enough to know when you get the right answer.

Neither one of those is easy and knowing what question to ask or asking the right question is probably one of the most difficult things any of us faces, because it is easy to ask the wrong question and get a perfectly good answer to it, which isn't going to help anybody.

So, clearly, if NRC wants to go out and ask licensees and applicants how much pressure can your containment take before it fails, that is not a good enough question. You have got to tell them what you mean by failure and failure is clearly going to be leaking at some rate, which could probably be put in terms of a hole of such-and-such a size somewhere in the containment boundary.

You are going to have to tell them to what extent temperature and these other environmental conditions have to be taken into account. I don't think NRC knows how to ask that question yet. In fact, I have been listening to people talk about vented filtered containments that relate to when the containment will fail and they don't know what they mean by failure.

The MARCH code people couldn't tell me what they meant by failure -- that was a sudden release of pressure and energy. But I wouldn't have the slightest idea if I was looking for a 3-inch hole or a 6-foot diameter hole. Three-foot is a good diameter hole, because that is the one we are putting in the interim rule, isn't it? That is supposed to vent a containment fairly fast.

So, knowing when you get the right answer is probably equally difficult, because it is a difficult thing. I think that it is quite appropriate for NRC to sponsor and pay for research which will help them and their contractors who are consultants eventually know what questions to ask and get the expertise to know when they are getting good answers.

I don't think it is appropriate for NRC to undertake the job of developing the techniques, the analyses, the verification or validation of those, to be able to sit down and calculate the capacity of every containment of every type that exists today.

As I look at the original statement of the program, it is hard to tell that you are not doing the latter.

MR. COSTELLO: Oh, okay, I would like to respond to that by saying I agree with you on the matter of principle and, perhaps, law, that the NRC should not make the assessment of the applicant's or licensee's plant -- the applicant does that, the NRC judges whether his submission is adequate or not. Also, there is the fact that I don't think we could afford it anywhere within our reserach budget, the number of containments and types around.

We are focusing on experiments to find out what happens to find out if we can predict what happens and to shorten -- to better scope things so that when we do ask -- when a scenario-dependent question like how much hydrogen burn can you take gets asked, or the successor to that question gets asked, the staff will be able to phrase it in a structural engineering context with less ambiguity.

DR. SIESS: Jim, NRC has already asked this question. They have asked it of Indian Point, Zion, Sequoyah, offshore power systems -- I know of those particular ones, because I have heard people give the answers -- and I have seen other people using the answers.

Now, I don't know that the question was asked right. Apparently some people think it hasn't. But I have gotten the impression that the people have been taking these answers and using them. One of your questions has to be, have we been asking the right question of these particular plants and has anybody questioned whether we are getting the right answer?

MR. COSTELLO: Oh, I think on the face of it, Professor Siess, people would question. When you have 20 different estimates for the same containment varying from lowest to highest by a factor of about 3, I think there has to be some question there.

DR. SIESS: Yes, but you are thinking of Sequoyah.

MR. COSTELLO: That is the one I remember having the largest --

DR. SIESS: Now, we went through that and we got a considerable range. The Subcommittee narrowed it down -- we thought we were smarter than some of the other people. But, again, the question we were asking is, what is the ultimate strength of that containment? Except for some looks at penetration that they said they had looked at, and we don't really know how they looked at them, and a couple of questions about the equipment hatch where they said they had to be fit up, nobody really looked at whether those pressures represented 1 percent a day leak rate, or 1/10 percent, or 2 percent, or 3 percent.

We were taking some of those steel containments up to pretty good strains. It would mean a diameter change of maybe a foot. I don't know whether the equipment hatch stayed leaktight with that kind of a change or not, and I am not sure how it was looked at.

MR. BENDER: Well, you are hitting on a few points that we probably ought to emphasize more. We are trying to get something that we would like to define as the ultimate strength, but we don't eally know what ultimate strength means. This doesn't necessarily mean the place where you get --

DR. SIESS: I don't like the word "strength." It is leak rate that we are concerned with.

MR. BENDER: It is where the service capability is destroyed or degraded to the point where it is unacceptable. It seems to me in all of these things we need to ask ourselves, is that the question we are trying to decide upon. Now, I will ask it about the buckling of steel containment.

When we are doing research on buckling, what are the applications that give us concern about buckling and how do we relate them to the capability of the containment. I am confused about that right now. I don't know of a case which addresses that particular issue. Is there one?

DR. SIESS: I think what Mike is saying, look at your buckling program and you find that the containment will buckle. Now, that isn't the question. The question is, will it leak? Maybe the damn thing can buckle and still not leak more than 2/10 percent a day. I think that is unlikely, but --

DR. ZUDANS: The question is legitimate, but the general understanding is that buckling is always associated with large deformations. It doesn't mean that it loses the load-carrying capability; it deforms. Now, somebody else will have to decide how much it can deform.

DR. SIESS: Well, if it were a simple steel shell, it could probably deform a lot, but if it deforms like that in the neighborhood of a personnel lock or an equipment hatch or large penetration, I am not sure whether it has its containment integrity.

DR. ZUDANS: That is correct.

DR. SIESS: And that has to be the criterion. Of course, there are things hung on it. You don't want the crane to fall down if it is hung on the containment, because that would be sort of messy. But, again, the function of a containment, basically, is not structural.

Now, the structural integrity may be important to the other function, and I am quite sure it is, but the function of the containment is to contain. If a 6-inch hole is enough to dump out the stuff that the people worrying about in the MARCH code and in the CRAC code, in the consequences analyses, and so forth, then I would be willing to put my money right now on the fact that that 6-inch hole is going to be around some discontinuity in that containment -- by discontinuity, I mean a penetration, or something of that sort -- and those things probably vary by an order of magnitude greater than the variation in structural containment design.

Somebody is going to have to be looking at that. Now, there is a way out of this, I think, that y[illegible] may have to adopt. Structural engineers, all I know about, really, are pretty good at telling you when a structure will stand up, and we are just lousy at telling you when it will fall down. We can predict up to this point we think it is pretty good, but we can't tell you where the end point is.

We can make tests and then we don't even have to do analyses to prove that out. The figures that we got when we were looking at the MARCH code, where they had 131 psig as a failure point for Indian Point, I think it was, he was taking that as a nice absolute and comparing everything to that from his MARCH code calculations.

I was told, somebody else may have been present at that meeting where the 131 was presented, but I was told that the engineers said at 131 -- maybe it was 130 -- at 130 they had about 90 percent confidence that it wouldn't fail. I think that corresponded to 2/10 percent strain -- it might have been 2/10 percent offset -- they had 90 percent confidence it wouldn't fail at that level.

Of course, as they got higher, the confidence level went down. I think that is true if you talk fail or leak rate. For any containment at some pressure there is a spectrum of leak rate at that pressure with some probability associated with each one.

The higher I get with the pressure, the higher the probability of a certain leak rate, because there are all sorts of random effects in there. It may be that the people that are making rules, and that is what this is aimed toward in the end, will decide that we will compute a structural capacity deformation type thing. We will keep it in the near elastic thing, a very small deformation, and we have fairly high confidence that is good, and we won't bother trying to see how much more we can get, that there is just no point in trying to compute the margin to a 6-inch hole when we can say that at this level we have got high confidence that there won't be any hole bigger than what you would get on an integrated leak rate test, which isn't zero, incidentally.

You may come up with the idea that you don't want somebody to compute ultimate, but you want them to compute a high confidence level of maintaining containment and we will work there, and when I go argue about that tail in the curve where the uncertainty gets to be too great -- that is a perfectly legitimate engineering approach, it is a legitimate licensing approach.

I guess it will work in the legal end. I hate to bring that up, but we can't ignore it. So, these are the things I think you need to be thinking about before you get too far into a program that is down to calculating --

MR. COSTELLO: I think that is a good point and I appreciate your advice on it.

DR. ZUDANS: I would like to make a comment and maybe a question at the same time. I would like to clearly understand which procedure NRC staff follows now and what should they follow. For example, in some instances it appears that your effort is directed towards defining limit states, be it for containment or be it for other structures.

I have a little bit of a difficulty in accepting that position. I feel that NRC staff should have the capability to evaluate the limit states computed and defined by the licensees and not to prescribe the limit states that licensees should evaluate, because that kind of restricts the scope of what licensees can do, and they are probably better equipped to do that than NRC is.

Make sure that the licensee has, in fact, considered all limit states. That means you do have to have the capability to define the limit states on your own, but not that the main objective. The main objective, in my opinion, would be for you to be able to take the submittal from the licensee which says here are the limit states, as Chet described, one is the leak rate, the other one is structural collapse, another one exceeds some deformation limit -- there could be many.

You should be able to say, aha, this set is complete, because I also know the technology and mode, and I can prove that, really, there is nothing else to be done.

MR. COSTELLO: Do you mean complete or correct?

DR. ZUDANS: Complete.

Complete is the definition. Correct, that is another question.

DR. SIESS: What you are saying is good, except if NRC would simply state limit states, that is almost the same as performance criteria.

DR. ZUDANS: I would hate them to state it, because it is prescriptive.

DR. SIESS: Well, it is not prescriptive if you -- it tends to be a performance-oriented type requirement. You tell us whether you meet --

DR. ZUDANS: That is not a limit state. That is something else.

DR. SIESS: A limit state, to me, would be the pressure at which the containment leaks 10 percent a day, or 100 percent a day, or 10 percent an hour. You tell me what the pressure is. That is performance criteria. But the thing is, the industry, in many cases, would rather have the NRC tell them what they want, so they can get it the first ime, and not go through three rounds of questions.

With that approach I mentioned, it may be stopping at some level where you have a high confidence level. I think it is worthwhile exploring that with the industry people to see if that wouldn't be a better way of getting at a limit, whether they wouldn't be satisfied to work to that limit -- even on existing plants. There is a tendency to try to push that existing plant as high as you can get it, but if going an extra 20 psi is going to take two years of argument and research to prove, it may not be worth it to anybody.

I mean, if they are trying to go an extra 20 or 30 psi to avoid a vented/filtered containment, they may spend -- maybe the industry wants to do the work to get beyond that point; that is another thing. The NRC can say we will accept at this level with confidence. If you want to do the research to raise the confidence level at some further distance into inelastic behavior, if you want to be able to do the research to raise the confidence level out there to where we think it is back here, then you do it.

Then they could make the cost-benefit analysis and decide whether they want to spend the money for research.

MR. BENDER: I would like not to lose that proviso. It is limit state under specific conditions.

DR. SIESS: Under all the conditions we can think of that are applicable.

DR. ZUDANS: Like the leak rate limit could be quite different, depending where you are in the accident.

MR. BENDER: Exactly. If the accident is one which deforms the structure when there is no pressure, I may not care how big the opening is. If the pressure is 100 psi, I may want a very small opening, so the combinations have to be put together.

DR. SIESS: Well, it is a leak rate.

DR. ZUDANS: It results in a leak rate, but a leak rate limit cannot be set unless you decide under which conditions you are looking at those leak rate limits. I think that the limit state concept is a good one, I think it was coined in SSMRP, and I like it --

DR. SIESS: No, it goes back beyond --

DR. ZUDANS: Maybe it goes back.

MR. COSTELLO: That goes back to European practice of 50 years ago.

MR.BENDER: It is a bad term, because it has been used in an entirely different way than we are using it.

DR. SIESS: It comes out better in French, and they are the ones that invented it. We spent a lot of time trying to translate it into English.

DR. ZUDANS: See, I don't know whether what I said before got across clearly. I suggest --

DR. SIESS: It is a regulatory philosophy --

DR. ZUDANS: -- we be more concerned about being able to assess the completeness of limit states presented by the licensee, rather than predefine the limit state for the licensees to work at. That means that you do not -- your capability has to be the same. It does not limit what you have to know, because you have to evaluate it for completeness.

DR. SIESS: I think that is a good place to leave it, because research's job is to get the capability. How it is used is licensing's job. At this point in time we are not talking licensing. I am not even sure they are represented, are they?

MR. COSTELLO: They are not, no.

DR. SIESS: You know, this idea of asking questions, it starts off with licensing asks the questions and then presumably research translates the ones that can be answered. Now, I am afraid sometimes we put the question to what we can get an answer to and that is not entirely wrong. It is better than the other way around.

Any other questions for Jim?

The other people are going to get questions along those lines, and I thought it was wise to get some of this underlying thinking out, so that you know what is behind some of these questions.

I might just mention in passing, some of you haven't been around that long, but back in the old AEC days and DRDT's Division of Reactor Development Technology, they developed a water reactor safety plan at one time -- at least developed one on paper, it was yay thick and assigned priorities -- and their policy was that if it had to do with containments, AEC didn't do it.

The AE's were designing this ungodly collection of different types of containment and, by gosh, they could do the research on them. There was nothing standardized. I am not sure that was actually carried out and I am not proposing it as a rule by any means. I think it is something we need to keep in mind, that NRC cannot solve all the problems on this complete complex of containment types.

As was pointed out earlier, different containment types are also going to have different limit state requirements. PWR's and BWR's, MARK-II's and MARK-III's end up quite differently at the degraded core cooling and containment.

Okay, Jim, next item.

MR. COSTELLO: I think at this time we will have Dr. Walter Von Riesemann from Sandia Laboratory talk, and you are aiming for about 45 minutes, Walter?

DR. VON RIESEMANN: Based on extrapolation, I would say five hours.

DR. SIESS: We have got the time, Walt, but we want to at least get through your project and Anderson's and Los Alamos before Mr. Bender has to leave. I think he has covered most of his major concerns about the containment buckling thing and we will carry that on.

We will get this with the idea that we do want to get the both of them before whatever time we break for lunch -- it may be as late as 1 o'clock, because Mr. Bender has to leave at 1. Then we come back after lunch and go into more depth. So, if people will keep that in mind and try to hold the questioning. I may cut it off a little bit at one point or another, because we can resume this afternoon.

DR. VON RIESEMANN: My name is Water Von Riesemann and the other investigators here are Al Dennis and Ron Woodfin. Tom Blejwas was unable to be here today. The planned presentation today will cover the objectives and approach of the program, the background study that was conducted on containment types, previous tests, the Phase I study which consisted of planning activity which looked at all containments, which looked primarily at analysis, modeling and load simulation, and then the Phase II activities, the multi-year effort, the program execution.

I will discuss the long-range program and the initial activity. The objective of the program, in a broad sense, is the development and verification of a reliable method to predict the ultimate load capacity and failure modes of light water reactor containment under accident conditions in severe environments.

The containment types that we plan to look at are steel -- I will describe this in a moment -- reinforced concrete and prestressed concrete. The loadings that we will look at will be the internal pressurization, static and dynamic, and earthquakes. As was mentioned previously, we are not going to look at missiles.

We are trying to be somewhat scenario-independent, so if a new loading requirement comes up, the result of the program will be applicable. The dynamic loadings are primarily due to hydrogen detonation, and they can be asymmetric and spatially varying. As was mentioned before, the work is being done in the Structural Engineering Section of Mechanical Structural Engineering Branch of NRC Research. It was initiated in June 1980.

I have listed on the vu-graph the licensing and safety issue, to come up with reliable prediction methods for capabilities for the containment structures.

DR. SIESS: Look at "reliable" there. It has a strong implication of a kind of level of confidence.

DR. VON RIESEMANN: Right. In other words, backed, in a sense, by experimental data, if you will.

MR. WARD: Do you mean by that you are going to try to understand quantitatively what the uncertainties are? So, let us say, this would fit into a probablistic analysis?

DR. VON RIESEMANN: I am not sure whether we will. A lot will depend on the results of the initial experiments, what kind of scatter we get, how the containments fail. Very little is known about that.

DR. SIESS: Of course, if you are looking at the confidence level or reliability in a probablistic sense, you have got to keep in mind somewhere that there is a considerable uncertainty in the load. I am not sure -- you know, we always like to reduce uncertainty everywhere, but if the uncertainty of the load dominates the thing, you are not really going to improve it a heck of a lot by decreasing the uncertainty.

DR. VON RIESEMANN: Particularly in the earthquake situation.

DR. SIESS: Well, the other one may come out to be just as bad, I don't know.

DR. VON RIESEMANN: The difficulty at the moment is that the current ASME/ACI design rules are essentially based on elastic response of the containment, and it is very difficult to extrapolate the failure level. The other problem is that we looked at the existing data base on experiments, which is really inadequate to come up with numbers and, also, corollary in numerical methods has not been qualified for doing this type of analysis.

The question comes up, why are we interested at all in the ultimate capacity, and I should maybe put quotation marks around "ultimate." Why are we interested in failure modes, leak rates? It does interact in determining the safety margin of the containment and, as was mentioned previously, the safety margin is dependent on load combinations, not just one number.

The emergency preparedness sequences, the rules you develop there depend on the containment capability. Risk studies depend on it. If you look at what are called severe accident mitigation studies and, yesterday, for example, filtered venting containment design equipment, they need to know this number.

If you look at these various topics, they all have different needs, so what is failure to one person is not failure to another necessarily.

DR. ZUDANS: You said "this number." You may have more than one sequential failure mode within the same sequence. There is really a whole spectrum of advancement you are looking for, not just a single number.

DR. SIESS: You know, I would be a lot more comfortable if we talked about containment systems and not just containment. By systems I mean the penetrations and the locks and all of that. To be sure we are not just thinking of that darned structure --

DR. VON RIESEMANN: I didn't define that, but we are including the, for example, equipment hatch, the personnel lock, penetrations, the skirt at the bottom, hold-down bolts. We are not including the isolation valve, though, in this study at this moment.

MR. BENDER: Are you including things like electrical penetration?

DR. VON RIESEMANN: We are looking at both electrical and steam line penetrations. They are, at this point, we think, not of severe consequence. I think the equipment hatch and personnel lock would be of primary concern.

MR. BENDER: Penetrations are not a concern for some conditions, but you didn't tell me what the condition was.

DR. VON RIESEMANN: Well, what I was saying, without saying the words, I believe the potential failure or leak path will not be by an electrical penetration; it will be at another point.

That is without fact in hand.

MR. BENDER: All right. I didn't object, I just want to understand.

MR. MARK: I assume, when you speak of penetration, the penetration might be, really, absolutely impervious to being disturbed if you push on it, but if it is also anchored in place, it can have back effect on the rest of the structure and that, I guess, is part of the picture you are thinking of.

DR. VON RIESEMANN: Let me, for example, look at the equipment hatch for a moment. The cover could fail, the seal could fail, and I am thinking now of the steel one, the sleeve could fail, or the area right around the penetration in the shell could fail.

With the exception of the seal, we will look at the failures of those items.

DR. SIESS: And failure means just opening a joint, for example?

DR. VON RIESEMANN: I would like not to define failure explicitly in this program but, rather, come up, if you will, with what is happening and, say, a leak rate, if we can measure it.

DR. SIESS: Let me just interrupt for a minute. Can anybody here give me some idea of how big a hole I need to have in, say, a 2 million cubic foot containment at 100-150 psi to dump everything out to atmospheric, say, in 8 hours?

DR. VON RIESEMANN: I can give you another number. Oak Ridge did some calculations on Indian Point, that size containment, at design pressure, a leak rate of 1/10 of 1 percent per day is equivalent to a 16th-of-an-inch diameter hole.

DR. SIESS: That is roughly 50-60 psi. Yes, I remembered that figure.

DR. VON RIESEMANN: But I have not seen any figures for your question.

DR. SIESS: One tenth percent a day is pretty low. You want something that is over 1000 times that, say, 10,000 times that leak rate. That would take 1000 days to dump it out -- the decay would take longer. Somebody ought to have a feel for what size hole we are looking for.

MR. MARK: In one of the presentations yesterday, a 7-inch pipe was adequate to look after the LOCA pressure and keep it from going off the map, so that means they were letting out quite a bit of stuff.

DR. SIESS: A 7-inch pipe. If we are talking about a 3-foot diameter pipe for a filtered/vented in near-term plants -- in some of the designs yesterday, somebody had 6 24-inch diameter pipes --

DR. ZUDANS: TVA.

DR. SIESS: TVA had 24 1-foot diameter, GE had several 24-inch ones. But I really think, you know, as you get into this, somebody needs to get -- I am assuming that somebody can tell you that dumping it in an hour or 8 hours is the kind of accident they are worrying about, or things like that, and are we talking about -- if we are talking about 3-foot, then a 1-foot penetration we don't need to worry about. That is a different accident.

Are we talking about this one or this one?

DR. VON RIESEMANN: I have spoken to the consequence people, the risk studies people at Sandia, and asked them that question sort of in reverse. What information would you need to know, and one of the things, of course, too, is the time into the accident when the failure occurred in the containment and how long it takes to dump.

DR. SIESS: Well, the time into the accident really doesn't -- well, it affects what you are doing, because the temperature-pressure condition can be different.

DR. VON RIESEMANN: Well, it affects the inventory in the containment. That is not my problem, but it will be of interest to the consequence people.

DR. ZUDANS: Your problem will be the time -- the special time history, how quickly the pressure is built up, and the hole alone does not determine how quickly you get rid of it. It is determined by what is behind that hole, so the whole system has to be looked at. There is no simple answer like this hole will unload that much.

DR. VON RIESEMANN: It is a little bit simpler, perhaps, in a steel containment than it would be in a concrete.

DR. SIESS: You see, that 16th-of-an-inch hole you can forget about, because nobody has ever made an integrated leak rate test yet that I have seen that they could even get the thing pumped up to 60 psi without going around and fixing some valves.

So, the thing is, it sits there before any accident at all, it is not going to be leaktight, according to my figures. Every time they make an integrated leak rate test, they start it and then they stop it and go around and fix some penetration or valve seats that aren't closing properly, because they can't get the pressure on it.

So, there is some leak rate that is inherent in this thing before there is anything else going on. We are going to have to live with that, unless they change the regulations. That is a lot bigger than that 16th-of-an-inch hole.

DR. VON RIESEMANN: The approach being used in the program is two phases, the planning effort, which will look at containments, of course, and looking at the background modeling, load simulation, and the end product is to recommend the program. Phase II is the combined analytical experimental effort, that is a multi-year effort, looking at analysis, scale model tests and what we call separate effects experiments -- we don't have a good name for that -- looking at the penetration, bolts, welded regions, components, et cetera.

DR. SIESS: I wish you would use that LOPCCS terminology. It has a bad taste right now -- where does Phase I come in in terms of time?

DR. VON RIESEMANN: Right now we are in-between Phase I and II, essentially, okay, and I will get to it in a few moments.

The end product of the program will be qualified analytical methods, benchmark data and, of course, the knowledge of how these containments behave under these loadings.

MR. BENDER: I think I want to pause for a minute here and be sure I understand. There are a lot of analytical methods that exist and there is some data, and I am not sure how long it would take to get everything that you might perceive the need for. Is this program intended to establish the method or to define what is needed in terms of methods?

DR. VON RIESEMANN: What we intend to do is use the experimental results and use it with a limited number of computer codes and see how well they do. Now, if they don't do very well, then -- depending on what the NRC decides -- we might have to go and do development of programs.

MR. BENDER: What you are doing is qualifying the available methods?

DR. VON RIESEMANN: Well, what I am talking, using an ASME term, if you will, qualification of a code for a specific loading condition and geometry, in the sense that it will do that problem. Now, our work might say the codes are not available to do that.

MR. BENDER: I am trying to sort the problem out for myself. The methods exist, as shown by what was done at Sequoyah. That was a set of methods for evaluating containment structure. I could decide that this program is to determine whether those methods are valid. I could also decide that this program is one which determines whether other methods are needed besides those.

Perhaps I could develop some methods. Now, are we doing all three of those alternatives?

DR. VON RIESEMANN: Well, one thing, we are providing data that anyone can take and use with their computer code and see how well they do.

MR. BENDER: That is pressure deformation characteristics.

DR. VON RIESEMANN: Strain rates, yes.

DR. ZUDANS: For structures other than containment.

DR. VON RIESEMANN: Well, for their own type of containment, perhaps.

DR. ZUDANS: No, no, you say you provide data base. Data base is related to some experimental work which either you find in the literature or you perform, and those are not going to be on the containment structures.

MR. BENDER: That is the data part of it. I was asking about the qualification part.

DR. VON RIESEMANN: Let me back up a moment. When the computer code is written, the terminology is used that it is verified in the sense that it does what it was slated to do, if you will, as far as the theory is concerned. But then it isn't used, say, on an actual structure that you are going to be using.

For example, an axisymmetric finite element analysis can be used for many different kinds of structures; well, you want to qualify that code for that structure. That is what I am saying, qualification. We take the results of the tests that we have, run a computer program for those conditions, and see whether it matches or doesn't match.

MR. BENDER: Let me go back, again. Some programs already exist. You are going to take those programs and exercise them to find out whether they can be verified by the data that you are developing?

DR. VON RIESEMANN: Yes.

MR. BENDER: Is there anything beyond that plan?

DR. VON RIESEMANN: Jim, perhaps you can answer that.

MR. COSTELLO: That is not an intent.

MR. BENDER: Which is not an intent?

MR. COSTELLO: It is not our intent to develop the one set of codes that will do the problem. I don't think we can. I think we want to focus our effort on getting an understanding of what happens and data against which predictive methods can be checked.

MR. BENDER: Well, I work better with cases, and I know this is an oversimplification, I will use the Sequoyah case, where we did, in fact, use three different methods, maybe four. We got three sets of answers and we selected one, which was somewhere in-between the several, and right now I would be inclined to say I would like to know which one of them was the best one to use.

Is that the approach you are trying to take here? To take these data and find out which of the several analytical methods is the best one?

DR. VON RIESEMANN: Yes, sir, that is a likely outcome. It is also likely that lots of people will expend their own time and money checking their codes against this data.

MR. BENDER: Yes, that is likely to be an outcome, I agree.

MR. VON RIESEMANN: Well, we expect that.

MR. BENDER: But that is a by-product.

DR. SIESS: No, I think that may be the main product. I don't think the NRC ought to be verifying -- validating is the term they use in the local ECCS program, which I assume we shouldn't refer to -- but there they validate a code by checking the results against physical evidence.

What kind of physical data would you be thinking of? A load deformation curve?

DR. VON RIESEMANN: Say on the static test, we would be measuring load, deflection, strain, those quantitites.

DR. SIESS: All right, but, now, all of those aren't important. For example, I couldn't care less about stress.

SPEAKER: Why not?

DR. SIESS: Because I don't care what the stress is if there is no deformation. I am really interested in deformation. This isn't going to be a petite failure. Say you have a load range. You must have somewhere in the regulatory process some idea of how closely they ought to be able to check that and have something that is valid for use in making decisions.

Somewhere that has to come in. Somebody has to have some feel of telling somebody that you have got to be able to check this within plus or minus 25 percent or plus or minus 2-1/2 percent. I think that is a part of the program.

Incidentally, it looks like some of your benchmarking of computer codes is getting taken care of in a few other programs, Jim.

DR. ZUDANS: I would like to pursue a little bit further Mr. Bender's question. Is it not your original intent, at least at the current state in the program, to develop a new universal computer code to achieve the objective?

DR.VON RIESEMANN: First, our intent is to check our data against existing codes, a limited number, if you will.

DR. SIESS: Vice versa. Check existing codes against your data.

DR. VON RIESEMANN: Right. Then, if they are in agreement at that point, that is the end. If there isn't agreement, then it depends on the NRC, whether they want to, in fact, develop material models, say, to put into existing codes, whether that is a deficiency, or to develop a brand new computer code.

DR. ZUDANS: Now, this program does not yet include any of those phases?

DR. VON RIESEMANN: They do not include that, no.

DR. ZUDANS: That means you plan to go fairly deep into the codes that you choose to evaluate?

DR. VON RIESEMANN: Yes.

DR. ZUDANS: That also means that you plan to, in fact, identify not only that they defective, but in which way they are defective. Then you will come up with a series of recommendations. Either the users or the owners fix those identified deficiencies or else they are beyond repair and your recommendation is to develop a new code, and that would be a new program, not this program.

DR. VON RIESEMANN: That is the way I see it.

DR. ZUDANS: Is that the correct interpretation?

MR. COSTELLO: Again, we are doing a bit of crystal ball gazing here. However, my feeling is that there is a great deal of computational expertise, capacity and willingness out there in the world. There is not, out there in the world, an ability to do the kinds of tests, the ability or willingness to do the kinds of tests to get the qualifications data.

DR. SIESS: I am glad you qualified it. I think the ability is there. It just takes money. The people are there. Now, willingness is not necessarily voluntary. There is an awful lot of the industry that does things because the NRC tells them they have got to do it.

DR. ZUDANS: Okay, I would like to complete this argument. So, I agree that that is fine so far. Now, we also know, in particular, you and I, we know definitely, there are a dozen or so codes that would claim they can do everything you want to do today.

And now if your objective is to see how well they really can do it, that is a fine objective, and if you devise it for that purpose, you are really undertaking a very difficult job, because in a nonlinear range there is no such thing as a unique solution. Very specific circumstances will lead you to a completely different answer. So, it is not an easy thing to say I will take the test, bend the beam and validate the code on the basis of that.

That is not going to work. So, you have to have a much more sophisticated approach, and I hope that that is what you are really doing.

DR. VON RIESEMANN: I missed the point on bending the beam. That is one of the things that is used for --

DR. ZUDANS: But that is such a simplistic thing.

DR. VON RIESEMANN: That is the clarification end, and then the qualification is getting into the more complicated structure.

DR. ZUDANS: You cannot, with great assurance, qualify a code on a one-dimensional system and turn around and apply that to a three-dimensional system. It is some place in your picture. You have to have something that resembles the real thing that you want to address with this code.

DR. SIESS: Now, let me make a couple of points. One is that this need for validation, or whatever the proper term is, confronting the theory with experimental evidence, comes about chiefly because you are going into the inelastic range. Would you have the same problem if you didn't go very far in the inelastic range?

DR. VON RIESEMANN: I think the problems would be less. In that regard, your earlier suggestion --

DR. SIESS: So, that is one thing to keep in mind. Now, as Zenon said, if you really want to be sure that the code works on a complex structure in the inelastic range, it is a real job, because no matter how many things you check out on, you are never quite sure that there isn't some aspect of the geometry or the loading condition, something unique to some code or some system or structure, that it doesn't work on.

In a way, it is like validating an ECCS code. I guess one question is, is it appropriate for NRC to do this, and you can argue this both ways. It is certainly desirable that NRC have the confidence in the codes.

Now, presumably the present users of the codes have confidence in them which may be entirely misplaced. If you pin them down as to why they think the codes work, they probably won't know. But you need to have confidence and if the only way you can get the confidence is by comparing them with experimental data, then you can look and say how do I get the experimental data?

I can go out and get it myself and test people's codes against it, and make them test them against it, or I can tell them that I want their codes tested against experimental data. Now, I am not really in a position to say which one is going to be most cost-effective for the total economy, whether NRC pays for this, or the applicant pays for it and you go through several rounds of questioning and re-testing and so forth.

Somebody ought to be thinking about that. I know the Commission is beginning to think about who does what, how much can we get the industry to do and really be effective in it. I think you could get industry to do everything, but I am not sure that is the best way for NRC to get the confidence it needs.

I think that really needs to be thought about, because, as you say, there are lots of these codes. The bottom line, to me, is that NRC needs to have some confidence in the results.

Now, Walt, you added on an item to your end product that wasn't on your slide.

MR. COSTELLO: Professor Siess, can I comment on your remark? We have done some thinking about that. It seems to me that, again, a quite possible outcome after this experimental program is complete, is that we will find out that, indeed, the hypothesis that certain types of penetration are of most concern is substantiated.

We may further find out that state-of-the-art computer codes cannot reliably predict what you need to know and that, again, that dreadful word, "separate effects" tests on full scale penetration problems may be necessary to answer the question on will this particular type of penetration -- at what load will this fail.

In that case, I could see that it would happen that the staff would say we are confident that this is where failure is going to be, and put the burden on the owner or applicant to go and perform his own separate effects test.

Now, it may be -- that could happen -- but, again, that is crystal-balling.

DR. SIESS: Walt stated the licensing and safety issue very well. It was to provide a basis for staff decision for reliable prediction of containment capacity, and we will take capacity in terms of containment function. Now, one result from this research project could be answers that would settle everything in your mind.

Another result would be a good set of questions which you ask of applicants and licensees which, when answered, will give you the desired level of assurance and basis for staff decision. I would commend strongly that you think of this project as a way of getting good questions, because I think you will find the success much more easily measurable and much more easily attained than if you think this project is going to answer all the questions.

I don't think it is our job to answer all the questions about this.

MR. BENDER: The end product, which you stated as qualified analytical methods, might better be methods of qualifying analytical methods.

DR. VON RIESEMANN: Yes.

MR. BENDER: Because I think that is what you are really going to have.

DR. VON RIESEMANN: Also, as I mentioned, which is not on the vu-graph, the knowledge of the behavior of the containment -- I think that is what Professor Siess was getting at -- knowing how these things behave, to some extent, and what questions to ask, you know. Where are the weak points, if you will.

DR. SIESS: You are not going to end up with a knowledge of how all these different kinds of containments --

DR. VON RIESEMANN: No way.

DR. SIESS: But you are going to end up, I hope, with knowing what you need to know or what information you need to get.

DR. VON RIESEMANN: What is important, in fact.

Looking at this question -- you know, if I plot load versus deflection and if we are going to be conducting experiments, I would like to conduct them way out to what you might call failure. Now, the analytical methods that you might be concerned with might only be down in this region. But it is very inexpensive, if you will, to conduct the experiments out further.

The next few vu-graphs I don't want to elaborate on too long. You have certainly seen cross sections of containments, I am sure, enough. Let me just flip up a few and make a few comments.

DR. ZUDANS: Mr. Bender asked a question that arouses my curiosity now. You answered yes and I just want to make sure that you really meant yes. You said that instead of qualified analytical methods, which means specific codes that you choose to run through your sequence, you also give the qualification method of codes that are as yet not written. Is that your intent?

DR. VAN RIESEMANN: That is an NRC function. We can give them the information we have from the test results and then they have to set up some guidelines, if you will, on what is acceptable.

DR. ZUDANS: But that is not a product of your work.

MR. BENDER: There is some contradiction. If all you are going to do is deliver methods -- what was said earlier was, you want to be able to allow people to come in and offer methods of analysis and to check them out. So, I have to say you are not developing the methods yourself. You are using some existing methods to find out what you have to do, but the end product is going to be a way of qualifying the methods that exist.

A by-product will be those methods which have been qualified and will probably be usable, knowing the NRC, since they exist. But if somebody else wants to offer something comparable, then they would come in and say, well, do it the way Sandia did it. Have I stated it incorrectly?

DR. SIESS: We are using some terms loosely, because I think Walt used "qualifying" as a very specific thing, that the algorithm was applicable to the structure. And I was using the term "validating" where I now compare the predictions of the analysis of the mathematical model with what would actually happen to the real structure, which obviously you never get completely, but that is what you are trying to develop, some level of confidence about the ability to predict what will happen to that containment out there when the accident occurs.

But "qualifying" you used in a different sense, didn't you?

DR. VON RIESEMANN: I don't believe I did. Qualifying I am looking at, taking the actual results that we are going to be getting from the containment tests --

DR. SIESS: Okay, I am sorry. I misunderstood you.

DR. VON RIESEMANN: Verification is the step previous.

DR. SIESS: Okay, verification.

DR. VON RIESEMANN: There are a lot of terms -- "benchmark" -- that are used loosely. "Validation" is used, "verification," "qualification," and "certification." They have different meanings, obviously, for different people. It is a study in itself, almost.

DR. SIESS: What you really want is some confidence that you can use the answer for some licensing decision.

DR. VON RIESEMANN: Right.

DR. SIESS: That is a pretty loose statement, but most of our decisions are not made on anything much tighter than that.

DR. VON RIESEMANN: Professor Siess, in view of the time, can I skip the containment cross sections?

DR. SIESS: I was just looking at your vu-graphs and there is some point at which I think we might want to stop and continue this afternoon as we get into more detail. There are really three phases of the discussion here. One is, why are you doing what you are doing, and that is addressed partly to you and partly to the research staff.

The second is -- I guess, what you are doing in terms of scope and then the third is how, which is getting down to the methodology. I think the how part, to the extent that you have some of that in here, we could defer to this afternoon, because I would like to get an hour or so on the other program before break, before lunch, but that still leaves us plenty of time.

But you might think about a stopping point there. We have plenty of time. Don't throw anything out that you wanted to present. I do think people have seen enough pictures of containments.

DR. VON RIESEMANN: The only point I was going to raise on a few of these, D.C. Cook, for example, an ice condenser, reinforced concrete with a steel liner, different than the Sequoyah type, and the terminology varies from person to person. Some of these are called freestanding steel and some people call them hybrid.

Design pressure, obviously, on the ice condensers are fairly low, 10.8 psi for Sequoyah.

DR. SIESS: Incidentally, the steel one is different in another respect. There are some steel vessels, steel containments, that are code vessels.

DR. VON RIESEMANN: Yes, and have a lipsoidal bottom. And there are even some spherical containments. The difference you find from one containment to another is that, for example, in Sequoyah there is nonuniform thickness along the wall. In Watts Barr it is essentially uniform. Penetrations are reinforced in the Sequoyah, they are not in Watts Barr. I could go on and on on that -- Professor Siess alluded to this before.

DR. SIESS: Is there any standardization by AE on those? I suspect that is varied with time.

DR. VON RIESEMANN: Yes, it varies with the requirements, if you will, of the NRC, of the utility, and of the ASME code. All three interact.

DR. SIESS: Those are all time-dependent. Well, the utility may not be. NRC requirements change with time, the ASME changes with time.

DR. VON RIESEMANN: In some cases, for example, in Watts Barr, the overpressure is not the controlling feature, but a lateral load is, so it has a greater capacity for internal pressure. MARK-III, for example, can come either freestanding steel or reinforced concrete -- all different types.

DR. SIESS: MARK-II's have got at least four different designs, and there are only 8 of them, I believe, 11 of them.

DR. VON RIESEMANN: I will skip quite a few vu-graphs down to this one, which gives a summary, which of course is moving every day, this is dated now, of the operating and future containments in the United States. We categorized them by PWR's and BWR's and then across the top by, if you will, structural type, concrete and steel. Prestressed concrete, conventional reinforced, other type concrete --

DR. SIESS: What is the "other" in there?

SPEAKER: Some early MARK-II's.

DR. SIESS: What about Gonay(?) that is prestressed in only one direction?

SPEAKER: There are two of those plants; they are in there, too.

DR. SIESS: What is the other one? Bellefonte?

DR. VON RIESEMANN: We have this in our report.

DR. SIESS: And you have got one concrete MARK-I.

MR. BENDER: Why aren't the French tests listed in here?

DR. VON RIESEMANN: I am not there yet.

MR. BENDER: Oh, I am sorry. I apologize.

DR. VON RIESEMANN: Well, we looked at different types of containments and put them in the big boxes, if you will, because within the prestressed concrete, of course, is three-buttress, six-buttress, all the variations on the theme. We just have an inventory there and we looked at what was available -- not available, what is in existence and coming down the pike.

DR. SIESS: I wonder if there is any design that there were more than about six made? Perkins would be in the new ones.

SPEAKER: Palisades, Turkey Point, Crystal River and Okoney(?) are almost identical.

MR. DENNIS: There is a tendency right now to go to three-buttress design prestressed concrete containment, and most of those are coming on-line in the future. Those tend to be following the same design methods.

DR. SIESS: Those are Bechtel?

MR. DENNIS: Bechtel is a large contractor, and I believe there are some other contractors.

DR. SIESS: But how about the Trojan-type design? How many did they do like that?

MR. DENNIS: I know that South Texas, I think, is the same type of design. Most of them utilize the ring girder.

DR. SIESS: As I mentioned, there is one MARK-I in concrete you haven't got in here. Two units, Brunswick. That is a real oddball.

MR. DENNIS: I apologize. There is a revised copy of that.

DR. VON RIESEMANN: We did that to see what is out there and what types to look at, because obviously we cannot test all different containments. We are trying to look at three generic types, as it turned out, a freestanding steel or hybrid, as it is called by some people, a reinforced concrete, and a prestressed. Now, even that is a big mouthful, obviously, because of the variations on the theme.

DR. SIESS: The hybrid designation, I think, referred to the freestanding steel with the flat bottom, because, you see, it is not a code structure. The ones that had the toroidal bottom were not called hybridgs.

DR. VON RIESEMANN: Right.

DR. SIESS: The other was steel top, concrete bottom, in effect.

DR. VON RIESEMANN: We have talked to people in the industry and they use various terminology.

Let me now get to another phase of the original background study. It was to look at what, in fact, had been tested in containment types around the world. The Canadians tested a Candu type containment, about a 14 scale. They didn't have any penetrations. They use a plastic liner, they don't use a steel liner.

They used hydraulic pressurization and there was fairly good agreement with the modified Bosor 5 code -- that code was written by Lockheed and was modified at the University of Calgary. The failure on that particular containment was around 150 psi gauge.

In Japan they have done some tests, too, on reinforced concrete containments, both internal pressurization and also lateral tests. We were not able to get any analytical work on those tests.

In India they have done a test on a 12 scale prestressed concrete containment. They used vinyl paint as a liner. They had 6 penetrations, but they had a lot of difficulty in the test and the failure occurred at a very low level, about 20 psi, and they could never really get failure -- it was essentially leakage through the liner.

The largest test that we know of to date was done in Poland, a 10 scale, prestressed concrete. That included equipment hatch and personnel locks in a steel liner, and they used water pressurization, and so the values given there are equivalent to the change in the head, if you will, from the top to the bottom of the containment.

You asked a question about --

MR. BENDER: My recollection is that the French did some work on containment --

DR. SIESS: Those were vessels.

MR. BENDER: No, I am not talking about concrete pressure -- I am talking about their early gas-cooled reactors, and I am trying to think of the name now.

DR. VON RIESEMANN: A breeder reactor?

MR. BENDER: Not the breeder. Some gas-cooled --

DR. SIESS: I didn't think they had a containment.

MR. BENDER: Some of their early ones had smaller experimental reactors. I will have to look it up. They did do some work.

DR. VON RIESEMANN: It is sometimes hard to flesh out the work that has been done. Dr. Stephenson is on contract to NRC and is looking at what is being done around the world. The French, I think, are interested in doing some tests in the future, but we don't know of any that have been done.

We are still looking, if you will, at all types of loading. I haven't concentrated just on the pressurization. There were tests done in Germany with shakers and explosives on the containment, but they are very low level. In Japan, they do essentially, I think, on every containment, again at low level -- shaker type.

Fukushima actually underwent an earthquake in about a quarter G-free field, but we cannot get hold of any of the analytical correlations that they have performed. In the US there have been some very low level tests, two sinusoidal tests.

DR. SIESS: Let me ask Jim Costello, are you making any attempt through your international program to get some of that Japanese data?

MR. COSTELLO: Yes, sir, and we are beginning to have brighter prospects. There was, I believe, some signing of documents last month some time, which would indicate that some trade is in process.

DR. SIESS: Well, even if you had to pay for it, it would probably be a hell of a lot cheaper than doing it yourself, and the Japanese do very fine experimental work. You can have a lot of confidence in it.

MR. COSTELLO: We have great hope of being able to get it. For a while there it looked as if they weren't interested in trading; now it seems they are. So, I am told we have an agreement in principle as of last month.

DR. VON RIESEMANN: I am also in correspondence with Professor Shibata at the University of Tokyo to see what information we can obtain. When we were over there two years ago, a visiting team from NRC, they presented some of the data, but we were not allowed to take any back.

The conclusion of this phase of the program, really, is that testing to date has been very limited. Current design methods do not permit extrapolation of failure, so we answer a question I guess we could have done before, but we need to conduct a combined analytical experimental program on containments.

In Phase I activities, which is a planning phase, consisted of forming an advisory peer review group looking at the similitudes, scaling laws, looking at containment, critical structural elements, what scale factors should we use, or how small scale model can be used, is another way of phrasing it, looking at load simulation and then recommending a program, and I will cover that now in the next few vu-graphs.

DR. SIESS: Well, are your scaling questions primarily related to the dynamic behavior?

DR. VON RIESEMANN: We are looking at all aspects at this stage.

DR. SIESS: Because the state behavior -- it seems to me that if you analyze the model you are doing, you get a great deal of confidence. The uncertainties in the analysis are a lot bigger than the uncertainties in the model, except for materials scaling. But if you know the properties and can put the properties in your analysis and get some agreement, I think that lends a pretty high degree of confidence on static -- it doesn't on dynamic.

DR. VON RIESEMANN: We have been mainly concentrating, though, on the static at the moment for the fine detail.

The advisory group that we formed -- and, obviously, we could have picked many people, but had to keep it down to some sizable number -- we picked people from industry that are familiar with the steel containments, and the concrete. We picked people that have an expertise in concrete and concrete testing, also in scale modeling, also in the general background on containments and just recently we added Ian Wall from EPRI to the list. He has not been on the advisory group until just about a week ago.

DR. SIESS: He did not get to the meeting in Chicago?

DR. VON RIESEMANN: He did not get to the meeting in Chicago, no. We have had two meetings with the advisory group, one in Bethesda or, rather, Silver Spring, and one in Chicago.

MR. BENDER: This is a good list. My only observation is that it is lacking in people who are familiar with the service question. There ought to be a few people on this list who are familiar with how the structure has to behave under the accident conditions.

I don't find that knowledge in this list. I really think you ought to look at finding one or two people -- I would think probably two would be best -- that are thinking about that aspect, so that when people discuss the matter, what is it that determines whether the deformation is okay or not, there is somebody there to answer, here are the kinds of criteria you ought to be thinking about -- the kind of studies that the offshore power people have done are perhaps the sort that I would want them to be looking at.

I think you ought to look at people that have that kind of understanding.

DR. SIESS: I think, to paraphrase what Mike is saying, and maybe you won't agree, but this advisory group is aimed, I think, at helping you answer questions, in other words, how to go about the program, and I think he is suggesting some people that would help you be sure you are asking the right questions.

I would think that, rather than adding them to this group, where they would be bored to death for a good bit of it, you might want to consider a separate group which would involve some of the people that are doing the research -- some of them in Sandia now.

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DR. VON RIESEMANN: We have talked to some additional people: Richard Orr, Adolph Walser, the people doing the filter vented containment, we talked with them, the IDCOR people at Oak Ridge or Knoxville.

DR. SIESS: But Walser and Orr are more users than askers.

DR. ZUDANS: I think that you need people who understand all the systems perfectly so that they can be very useful in defining limits. All these people are structural people, including Orr.

DR. SIESS: To people that know why they are interested in the leaks.

DR. ZUDANS: That is right.

DR. SIESS: And how it relates to degraded core rulemaking to give it a real high level objective, how it fits into MARCH code calculations.

DR. VON RIESEMANN: We are talking to people at Sandia, for example, the severe accident sequence analysis, which is another research program out of NRC. They are interested in the global question, if you will.

MR. BENDER: I am just worried about it becoming too narrow in its perspective.

DR. VON RIESEMANN: Good point, yes.

I have, I think, more handouts here than I am going to show view graphs, but one of the questions we looked

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at which you will not be able to read in the view graph, but you do have a handout, is looking at the failure modes for the free-standing steel, for example, containment, looking at the various scales that one might choose, looking at the failure modes that we hypothesize, looking at stag loading, pressure loading, the dynamics and also sizing loading and assessing whether, in fact, it will scale or will not scale.

So, this was an input choosing the scale that I will talk about in few moments for the test program.

DR. SIESS: I am just wondering if this might not be --

DR. VON RIESEMANN: Better for this afternoon?

DR. SIESS: A good spot to stop.

DR. ZUDANS: Could I ask him one question?

DR. SIESS: Just a moment. What do you think, Mike --

DR. VON RIESEMANN: If you look ahead, maybe you can see some questions that you want --

MR. BENDER: I don't want to go through the testing details. In fact, I --

DR. SIESS: I will tell you what, gentlemen, let's take a 10 minute break during which Mr. Bender can look ahead and see if he has some questions. If not, we may switch over to the other program and come back at this stage this afternoon.

DR. ZUDANS: Can I raise my question?

Why are you concerned so much about scaling because you are not going to model real containment anyway?

DR. VON RIESEMANN: We are not going to model what?

DR. ZUDANS: Real containment.

DR. SIESS: That is a good question. Think about it. Let's do it later.

(Thereupon, a brief recess was taken.)

MR. BENDER: I just wanted to make a couple of points. This program appears to be one of developing some model tactics to show the characteristics of these structures when they are loaded and that is a typical way of making the valuations of structures and you can hardly argue with it. But we do know that in many cases the shell structures are so thin that when you try to scale them down, it is not clear that the materials are the same, that the structural properties are the same. I can make a general conclusion that you will have trouble with that, just based on what has been done historically. And, so, you may as well face up to it.

Now, there do exist a number of shell structures around the country. Many of them have been abandoned but are owned by the DOE and it would make very good sense to me to try to search to see if you can use those structures and load them and try to get experiments done on a bigger scale without having to invest in a facility.

DR. SIESS: Better yet. See if you can get DOE to

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test them.

MR. BENDER: Well, I don't care how you do it. The NRC needs to get them tested and whether they spend their own money or somebody else's is a moot point, as far as I am concerned. It is all the taxpayer's money anyhow.

The second point that I wanted to make and I think I would like to make fairly strongly is that we have a lot of containments around that the industry claims has this capability and, as a matter of fact, I would guess that once you formulate this approach you are formulating, you will have people coming in and wanting to argue that the containment structures are now able to take a lot more and we would like to get rid of some garbage because they can. That is a good motive and we shouldn't discourage it.

In order to be able to do that, it would be nice to be able to demonstrate that some of the existing structures that exist in these installations do have such capabilities. I think the program ought to try to invite the industry to come in and do some tests on that existing containment that take the pressures up higher than they have been taken before in order to get a better handle on what their capability is.

Now, there is some risk in that and in the past when you asked people to do that they said, well, if it is not a requirement, we don't want to do it. But I think in the present mood in which degraded core cooling is being dealt with, where we are going to have to address questions like hydrogen combustion and the like and people are going to want to claim more structural capabilities, it is not unreasonable to say, look, I don't want to wait until the public interest is challenged to find out whether that capability exists.

MR. SIESS: But you are not proposing that anybody take an existing nuclear power plant containment into the inelastic range.

MR. BENDER: No, but I think they can take the pressures somewhat higher than they have taken them. Some of them might go as high as you wanted to go without getting into the inelastic range. But my point is if this thing is all model testing, that is about all I read into it right now, further down the road maybe some independent structural tests, separate effects test, if you want to call them that, but not presently planned, I am not going to be comfortable and you are not going to be comfortable that the results are going to translate. We tried that when we were working on the prestress concrete reactor vessels, where the scaling problem was not nearly as different as it is here and we had a lot of agony over it.

There has been some of this kind of thing done in connection with cooling towers that might give you some guidance, but I don't know how much. My inclination is to say without more thought to whether the scaling is practical, you ought not to get carried away by your model program. That is where I want to stop.

DR. VON RIESEMANN: Let me make a few comments to your comments.

One, we did look at some existing facilities in the United States. We at least had surveyed those available. We have not pursued the next question of asking them are they available for our usage. There are in South Carolina some facilities, Argonne, various parts around the country. We are at least looking at that aspect.

We are aware of the difficulty in modeling in material properties and we are conducting separate material tests to determine what the effect will be in change of size, fracture mechanics, welding, those questions that come up.

We might not necessarily use a scale fitness, if you will, for the scale model test. It might go a little thicker and still have credible results.

The other point about people taking their containments to higher levels, the problem, of course, comes about with the ASME code and all the regulations involved. But that is feasible to some extent for some containments.

MR. BENDER: I guess I am just trying to say that anybody can do the easy research and get answers that aren't usable.

DR. SIESS: Acceptable.

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MR. BENDER: Acceptable. Fine. If the answers are not going to be acceptably usable, then I am not so sure you ought to start.

DR. VON RIESEMANN: One of the concerns of the NRC when they brought the program to us was, in fact, to have a credible program and look at this problem with scales. And we are suggesting that we do testing at at least two different scales to take care of some of the questions of size effects, okay, because we realize that will be raised.

The other question is why are we concerned with scales, a question Dr. Zudans raised. Well, we don't want to introduce failure modes into our scale models that don't exist in the containment nor vice versa. We want to be able to model these failure modes. Scale modeling, no one has ever said it is easy, but full scale testing is very expensive.

MR. BENDER: Well, it is expensive if you have to build a structure, but if the structures exist --

DR. SIESS: It is expensive.

MR. BENDER: -- you still are a lot better off --

DR. VON RIESEMANN: One of our concerns with even a full scale is that the cost of doing those tests and the cost, if you will, of buying that facility might be very high.

MR. BENDER: Dr. Siess might be right in saying this is a place where you ought to be putting some pressure on the DOE to absorb costs. They are there to do such kinds of

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things in support of the safety of a nuclear reactor business. NRC can't do everything with its own resources. The industry needs to absorb some costs, too, and I think e whole regulatory protest needs t. work with the whole industry.

DR. VON RIESEMANN: Well, DOE's white water reactor safety program has a recommendation in it to do some containment evaluation and testing, but that is down the pike a bit. Whether that will be funded or not, I don't know.

MR. BENDER: Well, I am going to stop with just one last point and that is this. We are busily here trying to develop a regulatory approach and to some degrees the reason for doing the research is to help the regulatory approach along. And if we can't see that the results are going to be really applicable in that way, it is hard to encourage doing the work.

DR. SIESS: I think there is a point here that Mike has made -- I don't know at what level it has to be considered -- but it is very important. The idea is to get a reliable, acceptable -- and by that, I mean, accepted to somebody -- estimate of what when the containment ceases to function as it is supposed to. Now, one of the users is the people that are doing degraded core rulemaking. Acceptable to whom? Acceptable to Jim Costello, to his boss, to Franz Schauer, who is licensing, or Harold Denton, the commissioners, licensing board, public intervenors? You know, there is a whole level of things. Model tests have always been questioned by some

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people, including the people that do them and more likely, some of the people that don't do them. But if you really want to be critical, you can question a full scale test because it didn't look like the same structure and I think somebody has to do some thinking about at what level and to whom these things have to be acceptable.

I am sure research staff thinks primarily in terms of acceptability within the NRC. When you get over into licensing, those people have respect for the hearing board and you now hear words like "This research program is intended to provide the data to make the licensing process transparent to the public." Those are beautiful words. I haven't the slightest idea what they mean and how the public is defined. But I saw that in a justification for a $5 million research program.

Now, if somebody can tell me what that really means, I think I could define research programs a little bit better, transparent to the public. I am not sure. But this is something that we have to think about and when you start questioning -- you know, you are doing validity of models and looking at all the modeling scaling parameters, even when you are satisfied, then the question is at what other levels you have to be satisfied. Now, I don't know how you go about that. You have your board of consultants, which are going to be maybe not as critical as they should be. Maybe they should take a devil's advocate approach. Maybe they are. I don't know.

NRC staff, they have to bring in NRR somewhere to find out what is acceptable to NRR because those are the people who are going to have to apply it and appeal before the licensing board and defend it. I think that this is a very important thing is to keep in mind your ultimate user and who you have to convince. It is not just you and it is not just Jim.

MR. COSTELLO: I guess I would like to respond to that. I think if research is correct that in research programs we tend to look at sufficiency for NRC purposes. We also tend to look at sufficiency as judged by the technical community and we do tend to focus on those two. We have instituted a peer review panel. From my attendance at the two peer review panels, I can assure you that the members are not tame and have, indeed, been critical, constructively critical, and have to some extent, in a number of instances, caused changes in the plans. We are getting our money's worth, if you will, out of that panel.

DR. SIESS: Now, I am going to make a comment that I don't intend to apply to this project particularly, but there is research being done by NRC that I am convinced would not be done if the licensing boards did not exist. You understand what I am saying? Now, it goes far beyond the research that is needed to make a judgement or to reinforce a judgement and it is being done in such a way that you can almost see it

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being addressed to a hearing. I don't consider this good engineering. It may be good regulation within the present climate. But that is what I was getting at. That is a part of the constituency for research and you talk to the NRR people -- a lot of the NRR questions that research is spending money to answer are questions that arise simply because they might make their case before a licensing board, which is not a peer group really. It is not the same as your peer group. I mean, there are technical people on there, but it is an entirely different forum. That condition exists. It is not going to change and I think we have to recognize that some of the questions arise because of that reason and they have to be answered within that context.

Now, we are engineers. We can probably do it.

DR. ZUDANS: Can I now ask a practical question?

DR. SIESS: If it cannot be postponed until this afternoon or if it is on the immediate subject?

DR. ZUDANS: No. It is related to what Mike said.

DR. SIESS: Okay, then.

DR. ZUDANS: Although you said that you looked around the country where facilities exist, did you not make in this program a conscientious effort to identify the specific containment sites that exist that could be tested, provided all things agree to it?

DR. VON RIESEMANN: Well, we have identified the

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actual facility and the containment structures.

DR. ZUDANS: That exist?

DR. VON RIESEMANN: That exist. That were, we think, available. That could be tested.

DR. ZUDANS: Couldn't then NRC proceed to find out what is necessary to be tested because it could avoid tremendous expense.

DR. SIESS: Assuming that the program is going to go that far.

DR. ZUDANS: I think this is a quite practical proceeding in principle. That is the way the program should be directed.

DR. SIESS: It is. Still, I think it is $20 million or $100 million in 10 years, some number in some time, to qualify these things in the inelastic range. I would certainly want to look at what is involved in saying we qualify them only in the elastic range as an alternative. In terms of the public health and safety, I would want to look at it, because I think it is an alternative. And it is for new plants, I know. This is only one aspect of handling those degraded cores.

We heard people talking about a vent filter system that triggered a design pressure. They weren't a bit interested in going above design pressure. Maybe they had made the decision it would be better for them to trigger that system at design pressure than to try to qualify the design to twice

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design pressure. And, yet, I can go to the twice design pressure and still stay elastic on a lot of these containments if I want to leave out load factors and feed factors. You know, I don't need all the margins. I am not talking about a DBA. And the basic philosophy, I think, is that we are not going to call for all these margins at these degraded core -- we don't know. They might want degraded cores to meet all the safety margins we have now. But there are alternatives. And if the question gets too difficult to answer maybe we can turn around and ask another question that will work for the protection of the health and safety of the public just as well.

That is the only objective we have. We are not advancing engineering knowledge here.

MR. BENDER: Chet, I would like to offer a postscript if I can. In leafing through this thing, I had hoped to see in here a tabulation on containment systems, if I can use that term. I think it has been suggested here a couple of times -- of what the things are that one wants to know.

Ice condensors have one kind of characteristic and you can even divide it into two pieces. There is the free-standing ice condensors and those where the shell is butted against the concrete. Then you have another one for certain kinds of preset containments. Then you have another for the light bulbs in BWR's and I think it would be useful in order to have a catalog of knowledge that is needed to take these various systems and identify for each one of them what the things are you want to know in order to have a way of checking against the modeling if that is what you plan to do or the catching of full scale structures. I am sure that I can identify structures in this country that have each of the capabilities that are in the model. I don't know whether they are representative of the way in which the structures are built today for new containments or for existing containments that are testable, but that is a challenge you have anyhow.

But I would like to encourage you to try to get that kind of a tabulation in being. It would be educational to the NRC as well as to you to do it, because we don't really know what all the issues are yet.

That is my postscript.

DR. VON RIESEMANN: What we plan to do on the free-standing field, we will talk about that this afternoon.

Ron, did you have a comment?

MR. WOODFIN: Ron Woodfin, Sandia Labs. In response to the use of the existing structures in our studies we did not find anything that appeared to be an existing structure which might be available for testing which was close to being representative of anything else that is currently in use as a containment structure.

Our study was not exhaustive and you may know of one that we couldn't find.

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MR. BENDER: I haven't done a survey either, but I guess I would have to say that neither of the models are going to be very representative so you have to say -- it is a relative thing. Nothing is going to be representative.

MR. WOODFIN: Most facilities weren't even cylindrical. Most of them were rectangles, rectangular type things.

MR. BENDER: You just didn't look very hard.

MR. WOODFIN: We found some that were cylindrical. Those were the ones that --

MR. BENDER: ETCR has a good example --

DR. MARK: Could I just ask in exact connection with what you were saying? I presume that some of these things that you have on your list are DOE's items, maybe at Idaho, maybe at Hanford, maybe at Clemton or whatever. They have a tremendous decommissioning program on paper, at least. It might be very worthwhile going through those and finding out, because if something is about to be decommissioned, the costs for making use of it shouldn't be very great and the cost of not making use of it might be quite great.

DR. SIESS: Let me add one caution about testing actual structures, full size. I have never seen an example of tests on an existing structure that succeeded in answering very many questions. They are very good for asking questions and if you go into such a program or think about it, I would

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suggest you think about such a program as a way of asking good questions and don't get yourself too involved in hoping you are going to answer them or everybody is going to be disappointed.

I was told that by one of my professors many, many years ago and I have been involved in some float hill structure tests and I know of a lot of others and they all fit that category just beautifully. They are good for asking questions. They are a complete flop for answering them.

But that is not bad. Asking the right questions is pretty important.

Okay. Do you know where you stopped? Let's go on then to the next item on the program, which is the safety margins Category 1 structure, which is being carried out a little north of here. We were invited to meet up there and in view of the weather as it has turned out today, maybe we were wise to make them come down here. Chuck Anderson.

DR. ANDERSON: My name is Charles Anderson of Los Alamos and Dr. Siess is right. Los Alamos vanished under a rain cloud this morning and, although, we got here late, it is obvious we got here in time.

DR. SIESS: Well, you got here in time for your presentation. You missed some very interesting philosophical discussion prior.

DR. ANDERSON: I have been here for quite awhile.

What I am going to do is just summarize the few

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programs done at Los Alamos that Jim talked about initially and I am going to leave the technical details until after lunch. I guess I don't have to tell you to interrupt any time.

We are working on two programs. I have them both on this first view graph. They both deal in construction. They don't have exactly the same program objectives, but what I am looking at here is what we are trying to do is apply experimental and analytical interventions needed to assess multiple loadification.

Now, this might include an evaluation of the capacity of the structure and ultimate load. It also might include other factors, such as dynamics. In dynamics you have your evaluation.

We are looking at two types of nuclear plant structures. Mixed concrete and steel nuclear plant building such as auxiliary buildings, fuel handling buildings. Generally, these are box-type reinforced concrete structures of a more conventional design.

Second program looks at fuel containment, where the structural failure is buckling. The program started about a year ago and I will just tell you where we are in the programs -

DR. SIESS: Stay close to that mike. It is the one that is recording. He will yell at you if it gets too bad.

DR. ANDERSON: Okay. On the first program dealing

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with Category 1 concrete structures, this is a schematic of a typical building and typified by presence of floor slabs and shear walls, exterior walls, interior walls, columns and lots of plant equipment on the inside, much of it is sensitive equipment. One of our long range goals is not only to predict structural response and open the load, but to indicate the effect of structural response on sensitive equipment.

Both of our programs are set up according to the following size here. We have a peer review committee. We don't have all the test facilities at Los Alamos and we are planning to do basically model tests that Walt alluded to previously. We don't plan any full scale tests. These buildings are enormous. The auxiliary building can be 400 or 500 feet long, probably a hundred feet high and several hundred feet wide.

The containment structure, you have seen the size of it on Walt's chart.

My division leader says that this chart shows the way it works. NRC gives us the money and we send them back paper basically.

DR. SIESS: That is what I thought that little guy was carrying.

DR. ANDERSON: We coordinate the activities. We are doing most of the analysis. It is a coordinated program. As Walt mentioned previously, one of the goals would be to

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evaluate or find out which computer codes would be most applicable at predicting structural behavior in the inelastic range or at ultimate load.

We don't have the best facilities in general, at least, for the large dynamic tests. We plan to use test facilities, for instance, at the Earthquake Engineering Research Center. We have looked at test facilities in Japan. We have contacts with people with Japanese facilities and further down the road on the program, we will be looking at how these facilities might be used to test a relatively large scale Category 1 structure.

Now, you will see how we a[illegible] starting. The program starts simply. We start -- before we are running, we walk quite a bit. We are looking at, first of all, breaking the, for instance, auxiliary building into basic structural elements that contribute mainly to the ultimate load behavior of that structure. Initially, these activities are centered at looking at shear wall behavior at ultimate load. Enhanced damping at the shear walls as they crack, stiffness degradation, et cetera.

We are presently performing some experiments on really small-scale walls. We intend to then test larger scale walls. We then intend to test a structural system and this could be a three or four-story model of an auxiliary building complete with interior structural elements, exterior shear walls, floor slaps, as well as some modeling, perhaps, of sensitive plant equipment.

DR. ANDERSON: These are the tasks on the program chairing us out through FY 1983. We started off by doing a survey of Category 1 structures and how they are analyzed by and designed by the architect/engineering firm. We visited Bechtel, TVA and Sargent & Lundy. Generally, the methods that are used in designing these plant structures, the ones that are safety-related, are based on elastic methods and do not consider, in general the inelastic behavior of the plant structure.

A particular concern was voiced by some of the -- at least one of the vendors, architect/engineering firms, as to the role that damping plays when you have inelastic structural behavior, when you have a cracked shear wall and we have focused on that as one of the things to look at.

We have reviewed the literature on concrete model testing. Needless to say, there are not tests on representative nuclear plant structures, which differ somewhat from conventional box-like reinforced concrete structures in that the walls on these structures are very thick, ranging, I believe, from about 18 inches to 48 inches and generally towards the larger thickness.

We have developed a program plan after doing the review and talking to the architect/engineering firm. You have a copy of that program plan, which identifies the first two phases of the program fairly accurately as we see them and discusses in more generalities what we call the Phase 3 experiment for testing a multi-story, reinforced concrete Category 1 structure.

As part of the program, this is an incidental part, we are working with a consulting firm looking at the strength of masonry walls that, it turns out, are used as interior walls in many of these plants. The design rules, if there are any, are questionable on masonry walls and some of these walls are in either questionable shape or there is questions about boundary conditions, how they are supported and what we are attempting to do here is to eventual recommend sections for these interior masonry walls.

DR. SIESS: Did your survey indicate whether anybody is using masonry walls in new plants?

DR. ANDERSON: I do not believe that is so.

DR. SIESS: I wonder whether the reaction to the problem has been to eliminate them or to try to improve the design of them.

DR. ANDERSON: It is my understanding that the walls were put in after the plant was built. That is the cause of the problem.

DR. SIESS: Some of them had them designed in. They designed them in.

DR. ANDERSON: The ones that are giving the problems are the ones that were put in later because they couldn't attach, for instance, the top of the walls into the structure, which they could if it was being built initially. I guess, it is my feeling there are no plants incorporating -- no new plants incorporating masonry walls.

DR. ZUDANS: TMI --

DR. SIESS: It wasn't TMI. It was Trojan.

DR. ZUDANS: No, I mean, the actual plan requirements after TMI.

DR. SIESS: But that came out of the Trojan, I think. That is a catchall for everything they talked about that year. Some plants have used masonry walls much, much more than others. Some of them had very few and some had quite a few. Now, they are finding that they weren't even reinforced the way they were designed.

DR. ANDERSON: That is one of the problems. And some of them do support Category 1 equipment.

DR. SIESS: They went in and hung air lines on them.

DR. ANDERSON: That study is to be completed by next June. It is an incidental part of the program but it does help us in, again, trying to appreciate --

DR. SIESS: Who is your subcontractor on this?

DR. ANDERSON: It is Trans Science, a small company in LaJolla and Professor Higgimeier as the owner, proprietor, whatever.

DR. ANDERSON: Presently, we are designing small scale shear wall and testing them statically with the aim of predicting the stiffness degradation when the concrete cracks

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and we will shortly begin some dynamic tests to evaluate damping characteristics of these shear walls. And I am going to leave that go until after lunch and Elton Endebrock will talk about this.

DR. ZUDANS: On that procedure in general, I think that we stress sometimes, or maybe I read it in your draft report, that you make up a section from elements, study these elements in different conditions with some reports of failure. I think if you move a shear wall out from the wall, if you move a shear wall out of the wall and mount the side of the shear wall by itself you have wrong boundary conditions and you lose the three-dimensional behavior which is not going to be elasticated in any such test. How are you going to account for that?

DR. ANDERSON: We are setting the walls individually. True. They do have a fairly heavy flange top and bottom.

DR. ZUDANS: Not on the other side.

DR. ANDERSON: Excuse me.

DR. ZUDANS: Not on the other side.

DR. ANDERSON: No, not on the other side. What we hope to do is get some measure of the damping characteristics of the individual wall. Now, eventually this will be put into larger models and incorporate multiple shear walls and we will both analyze and test those structural systems. But that is further down the road. That is two years away. So, there will be a final model that will incorporate multiple shear walls.

You may be right. Their behavior in the structural system may be different from their individual behavior. Hopefully not so.

MR. BENDER: I don't know where to interject this question, so, I am going to interject it now. When I looked at the program objectives back on the first slide, I had to ask myself what is it that we mean when we say "ultimate load behavior" in this particular case because if you are going to determine damping properties, they have to be for some reason.

Are we trying to find out how the structure behaves when it failed?

DR. ANDERSON: That is basically it. When it is near its ultimate capacity.

MR. BENDER: Are we trying to relate that to whether it will be near its ultimate capacity?

DR. ANDERSON: In terms of load, we will identify that. Now, it may turn out that these structures are so strong that no credible earthquake could ever fail them, in which case one could then shift the problem to looking at the behavior of sensitive equipment on their own.

MR. BENDER: Some of them will be vulnerable and some won't. I think I have to challenge the question of trying to test something to the point of cracking without knowing whether we want to know what happens at the point of cracking.

DR. ANDERSON: Well, I think we do want to know.

MR. BENDER: Why do I want to know?

DR. ANDERSON: Well, for instance, an earthquake sited, say, in California, the plant is built for a certain seismic design criteria and ten years later a fault is located nearer the plant and the earthquake load criteria goes up and the question is shall we run the plant or not because it was only designed for the reduced criteria.

Now, if you have an idea of the behavior of that structure as it approaches or goes into the inelastic range, those numbers can be very valuable in relicensing the plant. I mean, that is an instance.

MR. BENDER: That Three Mile Island is often given and it worries the hell out of me because it requires you to speculate on which structures will be challenged at some future time in life.

DR. ANDERSON: Well, specific structures would be challenged. In the Three Mile Island instance the problems were related with the containment. The containment is designed and tested for 55 PSI. Beyond the accident you are wondering what about pressures greater than that. What is the ultimate capacity of the building?

MR. BENDER: I had an accident in mind when I dealt with that one; namely, the hydrogen explosion and it wasn't Three Mile Island incidentally. It was in connection with some other containment in which that accident postulated. I don't find the same kind of question being addressed here and I am not so sure I understand the questions.

DR. SIESS: Well, Mike, I don't agree with you. In any probablistic approach the earthquake beyond the design for safe shutdown earthquake does not have zero probability. If somebody attempts to do a WASH-1400 type analysis, including seismic effects, and there is at least one member of the ACRS that is strongly in favor of that, we are going to have to know something about behavior beyond the SSE.

I suspect that most of these buildings will enter the inelastic range not tremendously far beyond the design basis. Now, if it is three times the design basis before they get inelastic, as you say, we may find that there is just no concern with them. But if cracking represents an inelastic range, which I am sure it does in all the materials I have ever dealt with, these things are going to go inelastic probably at the SSE. I am not sure. And if we want to know what the margins are for low probability earthquakes beyond the design basis, we have to know this.

Zenon, you had a question?

DR. ZUDANS: This is in respect to Mike's question of how far do you go in elastic range once you establish a idyllic state.

MR. BENDER: I am not sure what it is that you are trying to establish. As a matter of fact, I would like to know

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what the damping properties are as a function of the extension into the inelastic range, but I don't hear that coming out of the kind of discussion.

DR. ANDERSON: Well, there are several things. The ultimate load capacity itself under static conditions might be one thing. The effective damping of the structure at various stages in the inelastic range up to the ultimate load might be another thing. But we are looking at that, or will be looking at that. The stiffness of the structure as it degrades as you approach the ultimate load, that is another thing. Those can be studied perimetrically as relative to the ultimate load. In other words, we can go in between the design load and the ultimate load --

DR. SIESS: You don't approach the ultimate load monotonically either. This is cyclic loaded.

DR. ANDERSON: In the dynamic cases.

DR. SIESS: Yes. And you are interested primarily in dynamic cases, are you not?

DR. ANDERSON: Primarily.

DR. SIESS: No static loads that are likely to exceed the design loads for these types of structures, are there. There is pipe whip and high energy pipe break and earthquake and tornado and those are all of some dynamic, not all are cyclic.

DR. ANDERSON: But the damping itself may -- perhaps, we may be able to describe it in single experiments as a function of how far -- as a function of strength, for instance, and then incorporate the damping -- those damping factors into a dynamic analysis where you would have larger damping as the structure oscillated in the inelastic --

DR. SIESS: I wish I didn't hear that word "ultimate load" so much. You are interested in the behavior only up to ultimate, but not just at ultimate.

DR. ANDERSON: Not just at ultimate.

MR. BENDER: I think that is probably the point I am trying to make and maybe it was said better just now. I want to see how it progresses beyond what it was originally intended to be designed to. But I don't know how far I want to go and it is the incremental change from the design base that exists now to some level above it that seems to be the most interesting thing to know about and not necessarily up to where the structure has reached the point of total failure.

DR. ANDERSON: But that information itself is lacking as you go into the inelastic range --

MR. BENDER: I have no trouble with that at all. It is just more a matter of establishing what it is we are trying to develop.

DR. SIESS: I have a suspicion that we will find out from this why people aren't designing for inelastic behavior. But that doesn't mean that you don't want at some point in time to be able to analyze the inelastic behavior and it is not just for those old plants --

DR. ANDERSON: No, no. This is not just for those old plants.

Phase 1 then involves this small scale model, shear wall model. Phase 2 -- these models are like 1/30 of scale, which if you question modeling at all, it should absolutely cause you to say it is no good at all, I guess.

DR. SIESS: But if you can't analyze that simple model --

DR. ANDERSON: That is right. My computer code doesn't know the difference between that small model and a large-scale model.

DR. SIESS: And you will find out the things you left out.

DR. ANDERSON: Right.

Phase 2 experiments are a larger-scale shear wall and they will incorporate small but cross typical reinforcing wire. Along with all of this will be analytical modeling and evaluation of computer codes. And I think I can talk about that a little bit on the next slide.

Then there is the Phase 3, in which we will build this multi-story structure, test it at a large capacity seismic facility, such as at Berkeley or, perhaps, Japanese facilities.

DR. SIESS: We will only test the cyclic?

DR. ANDERSON: Well, we are doing right now static tests. We are going to --

DR. SIESS: I mean, even static cyclic.

DR. ANDERSON: Yes, we will be starting quasi-static cyclical tests on the small-scale shear wall.

DR. SIESS: Now, there have been some fairly large-scale shear walls tested under cyclic, not dynamic, but cyclic loading and, as you pointed out, they are not necessarily representative of the kinds of things that we see in a nuclear plant, the reinforcement and other things.

DR. ANDERSON: Wall thickness, right.

DR. SIESS: But are we sure that those differences are significant in terms of the applicability of the analysis on ultimate behavior? I mean, you might well find out that the nuclear-type wall is just another step down the scale from what PCA tested or something and that you can go back and get information from other tests. That would be one thing I would look for. There are differences. Whether the differences make a difference, I don't know.

DR. ANDERSON: I think our conclusion was that nobody has dynamically tested a shear wall structure in the inelastic range. There are pieces --

DR. SIESS: Large scale.

DR. ANDERSON: Fairly large scale.

DR. ZUDANS: That would be a major decision problem for you or for the program as such, because once you say dynamics you are forced to pick a history that you will load it with. If you do the -- ,

DR. ANDERSON: Seismic loading.

DR. SIESS: Most of the machines can put in a simulated earthquake. You are primarily --

DR. ANDERSON: Primarily looking at earthquake loading. I feel certain we would do a lot of sinusoidal testing of these walls prior to doing earthquake tests.

DR. SIESS: But there must be quite a few small-scale model tests under simulated earthquake loading. Matisozan (has made dozens of them at Illinois and I am sure he is not alone. A lot of other people have shakers with that kind of capability and the Japanese -- I haven't looked thoroughly at that, but they must have tested a lot of things. But they don't look like your plants.

DR. ANDERSON: That is right. These structures are going to be difficult to test because the problem of scale, the massiveness of the specimen and if it is a bottom story on it, normal stress is going to require --

DR. SIESS: It is not clear that the validity of an analysis has been checked out on dynamic tests of other types of structures will be in question for this type of structure. It may take only a certain number of tests of nuclear-type structures to find out that the analysis that was

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validated on something else would be just as good there. The differences may not invalidate the analysis. See what I am getting at?

DR. ANDERSON: Yes.

DR. SIESS: You don't necessarily have to reinvent the wheel, but you should be looking for what use you can make of all the other work that somebody has done, because there is going to be a limited amount you can do.

DR. ANDERSON: With a limited budget.

DR. SIESS: With an unlimited budget.

DR. ANDERSON: Or even with an unlimited budget.

DR. SIESS: Give me an unlimited budget and I can think of enough tests to keep you busy for the next century and there will still be questions when you get through.

DR. ANDERSON: Okay. Here is the experimental program plan, not the analytical part. As I mentioned, we, right now are --

DR. SIESS: You are using view graphs we don't have. I just call that --

DR. ANDERSON: This is one you don't have and I will get you a --

DR. SIESS: Just so we get them for the record. And that one is a little hard to read so, give us time.

DR. ANDERSON: Right now, we are into the Phase 1 of the experimental program analysis, dynamic tests on small-scale shear wall. Phase 2 is larger scale. And then Phase 3 is the multi-story test. The analysis on small-scale shear walls, generally, we are using simple one, two and three degree of freedom systems and Elton will describe what he is doing on that this afternoon.

We are also doing some finite element analysis in attempt to create cracking of these walls using one of our in-house computer codes. And those types of analyses will be carried on into Phase 2. In the third phase of the program it is hopeless to even think of using a finite element analysis for a multi-story structure and one must resort to reducing the number of degrees of freedom of each structure and trying to incorporate overall properties of slabs and shear walls.

Now, there are some codes -- at least two codes that are available for studying these types of building systems in the inelastic range and we have a contract to evaluate one of these codes. Professor Cheng at the University of Missouri at Rolla is going to take a building system which he has now in-hand and try to analyze it with his code, which has some INRES-3D and I don't know what that all means.

There is also a code that was developed at Berkeley for looking at inelastic behavior building systems and possibly we can evaluate that code also.

Elton is not going to show you but here is our shear wall model that we are calculating now.

DR. SIESS: Did that one on the left fail by overturning?

DR. ANDERSON: No. You press a little button and the computer rotates it, you see.

This is a shear wall, a vertical wall, on the right-hand side here, two top and bottom slabs. The loads are applied parallel to those slabs erected along the shear wall. Actually, we are starting to predict cracking of the wall. So, it looks like it is a problem that we can do and the results will be corre ated with the experiments that are going t done. This won't tell us what damping of cracked walls produce --

DR. SIESS: Is this reinforced walls?

DR. ANDERSON: This is reinforced.

DR. SIESS: That is just the schematic model.

DR. ANDERSON: The reinforcement is -- there is reinforcement smeared into the concrete properties. We have about .5 percent reinforcement equal direction above the shear wall. And, again, Elton will talk about that this afternoon.

Okay. The other program is the "Buckem Program." Maybe I should stop and see if there are questions.

DR. SIESS: I have one question. I guess I would like to address it to Jim Costello. As I read the report that Elton sent us, it seemed to me that this had many aspects of the ill-fated benchmark and computer codes program. Can you tell me what relation, if any, this has to what was proposed

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in that? I know it doesn't have the comprehensiveness. But this seems to be benchmarking computer codes for predicting the behavior of shear wall type Category 1 structures under seismic loading.

MR. COSTELLO: That is right. The same comment I think you made with regard to the containment program. The ill-fated and now departed benchmarking effort was intended as a stopgap measure, a short-term solution, utilizing only whatever test results that could be culled from the literature and strained to be considered applicable.

DR. SIESS: It covered containment buildings and pressure loadings and other things, too, did it not?

MR. COSTELLO: Yes, sir, and a lot of the earthquake calculations, too, the seismic calculations. It was very broad and not very deep and it was intended as a stopgap until such time as experimental data was available.

DR. SIESS: Now, this differs from that in one major respect and that is that it will probably involve developing a new code for the inelastic dynamic analysis. Or do you expect to find codes --

MR. COSTELLO: That is a long term goal of the program, if it is a goal at all. In this program the experimental work is going to be nine times the analytical work. The analytical work is being used to guide the experiments, the planning of the experiments. It is also to some extent being used to check against the experiments.

DR. SIESS: You said there are a couple, maybe more, codes for inelastic dynamic analysis.

MR. COSTELLO: Of building systems.

DR. SIESS: Of building systems. There are not any that you are confident right now are likely to be applicable to this?

MR. COSTELLO: They have not been checked out.

DR. SIESS: Now, you were going to check out some of them.

MR. COSTELLO: Right. That is a part of the program. The code is also being used to help us design the experiment initially.

DR. SIESS: But, now, in the unlikely event that the code checks out, then we are home free.

MR. COSTELLO: It is an unlikely event.

DR. SIESS: If it doesn't, that means that you then modify the code to do the things that it didn't do properly.

MR. COSTELLO: Or design a new code entirely.

DR. SIESS: Or design a new code and I am not sure at what point the modification becomes a new code. But it is your objective to come out with a -- not only to validate codes, but to come out with a validated code.

DR. ZUDANS: I would like to make a point. I think at this stage we know well enough that there are codes that can handle these things, except for --

DR. ANDERSON: Which code would you propose to handle --

DR. ZUDANS: Any of these codes could handle your problems as long as you know what the material properties are.

DR. ANDERSON: I disagree. The problem we are talking about is a multi-story, complex building system. If you apply one of the usual, non-linear codes, you will need the biggest computer in, you know, the next hundred years.

DR. ZUDANS: I didn't finish yet. The context really is that we cannot exercise the work itself, because of what you just said. So, you are not in a position to develop any new code and now if you want to rock the entire panel.

DR. SIESS: Well, we have been analyzing buildings for years without finite elements and I think you can devise a technique where you can get number properties, even if numbers are shear walls from whatever you need, finite element analysis and/or tests and then the complex is analyzed by other types of codes.

DR. ANDERSON: That is basically what the code -- this drain tabs code does. But you do need data to put into those codes and inelastic range.

DR. SIESS: You need member-type data.

DR. ANDERSON: Member data, right.

DR. ZUDANS: You can do all those things and make member-type data for elastic range. As soon as you are inelastic, you are doomed. Forget about it. You will never develop anything to represent the entire shear panel.

DR. SIESS: I think you can.

DR. ANDERSON: This afternoon, I hope -- are you up to it, Elton.

DR. SIESS: You wouldn't say the same thing about a beam.

DR. ZUDANS: No, because beam is smaller --

DR. SIESS: We have been designing buildings for years successfully before anybody thought of three-dimensional elements and the three dimensional element was the beam.

DR. ZUDANS: You designed for ultimate capacity. When it was built, it was built. You were not concerned where the cracks were.

DR. SIESS: No, no. I disagree with you.

DR. ANDERSON: All I can say to answer that question is maybe it will turn out that way, but there are two codes that do model with a far reduced number of degrees of freedom in elastic behavior of shear panel, columns, floor slab systems.

DR. ZUDANS: Sure. You can approximate everything. The question is how good it is and the question is how good do you want it to be.

DR. ANDERSON: Then, perhaps, I will go on and just briefly summarize the containment buckling work and this afternoon Joel Bennett will go into the details.

DR. SIESS: That is fine.

DR. ANDERSON: They have done quite a number of experiments. What we are looking at is the scale of the pressurized water reactor system and we are looking at the behavior of the shell when the failure -- the ultimate behavior or the inelastic behavior when the failure, if by geometric instability or buckling. We have some specific tasks that have been laid out for us on the program. It is not a, in the sense of the previous program, it is not a general look at things. We have some specific things to look at.

DR. SIESS: Would you like to put those in the form of specific questions you are trying to answer at some stage in the game.

DR. ANDERSON: Okay. The specific questions we are trying to answer, one is the applicability of the ASME area replacement rule for reinforcing containment-like shells and the ASME rule relates to the reduction of stress around the penetration and the question is does the same rule apply for prevention of buckling.

DR. SIESS: In other words if that rule is applied will the shell behave the same as it would without the openage through it.

DR. ANDERSON: That is correct. The results of our experiment are -- Joel will go into these -- in general, the result says that it penetrates the cylinder and we have done our experiments initially on cylinders and you take the area that is removed and suitably place it around the hole in the cylinder. I guess the best we can say is that it can't hurt, but it may not increase the buckling load one twit.

Now, under certain situations it will increase the buckling load and he will describe what those situations are.

DR. ZUDANS: This will be discussed later?

DR. ANDERSON: This will be discussed this afternoon.

I would sort of like to give you the general flavor of the program and the program plan.

If you look at FY '80, we are down to -- we are through the first three. The report has been written and has actually been published as a formal Los Alamos report.

Now, the former nuclear reactor regulation has a contract with Lockheed to develop computer codes, state of the art computer codes, for analysis of buckling of containment-like shells. The second part of our program is to design suitable experiments to benchmark that computer code. They do the calculations. We do the experiments. We have been working closely with Chicago Bridge and Iron to come up with a design of something that represents a containment shell and then test that shell and evaluate the buckling load. The shell is complex; although ours is cylindrical, .c has rib reinforcing and vertical stringers.

DR. SIESS: Let me ask you a question that comes strictly out of ignorance. If you are going to deal with steel shells with holes in them and you are concerned about buckling, it seems to me that there are two, at least two, possible strategies. One is you try to develop a method of analyzing the shell with holes in it. Now, for any configuration, you can analyze its predicted behavior.

The other would be to develop rules for reinforcing the holes, using the general terminology in such a way that the shell with reinforced holes would behave the same as the shell without holes and then use, presumably, existing analyses.

DR. ANDERSON: Right. The simpler analyses type.

DR. SIESS: Now, which --

DR. ANDERSON: Okay. The first three items up there dealt with your second method; namely,it answered the question can you take that and reinforce that hole using the ASME code rules to raise that buckling load to the buckling load of the unpenetrated cylinder. And the answer to that question is "no."

DR. ZUDANS: I think it is not that categoric, you know.

DR. ANDERSON: It is not categoric, but as a rule it is "no."

DR. SIESS: Assuming it is "no," then you still have the two options. One is can you change the rule so that the answer is "yes." NRC can write reg guides that supersede the codes.

DR. ANDERSON: That is a question that we have not addressed.

DR. SIESS: And the other one would be can you take -- develop a method of analysis for a shell with holes, reinforced holes, if necessary, or unreinforced, whatever, and that is your tact now.

DR. ANDERSON: That is a thing we are -- a task that we are evaluating right now. We will -- Lockheed will calculate the experiment that we come up with. We will run the experiment and then compare the answers.

DR. SIESS: So, if you are successful in developing a code that will handle the shell with holes or somebody is --

DR. ANDERSON: Somebody is. Right.

DR. SIESS: NRC is, because this is an old NRC project, then you leave ASME alone.

DR. ANDERSON: Correct.

DR. ZUDANS: What is the actual Lockheed assignment? Specifically, what do they have to develop? What kind of a code?

DR. ANDERSON: They have developed codes. Well they are a set of codes. BOSOR 5 is the latest one and STAGS-3C. Those are the codes they will apply to the problem.

DR. ZUDANS: Now, is there anybody on your staff that can ask the very specific questions relative to BOSOR 5, for example, what they plan to do there?

DR. ANDERSON: Joel, coul^ you answer specific --

DR. BENNETT: Not now.

DR. ANDER.^^N: Not now.

DR. BENNETT: The question is is there somebody here that can answer them. Okay. We will find out later.

DR. ANDERSON: He will be there. I will guarantee you.

DR. SIESS: That is an appropriate matter for this afternoon. Dr. Zudans will ask the question and I am sure he will be the only one who understands the answer.

DR. ZUDANS: You may be correct, but not abou the answer.

DR. ANDERSON: This second exercise with Lockheed will initially involve static experiments and evaluation of static buckling loads. They will then proceed on to constructing planning experiments and constructing models for looking at seismic behavior of these shells and seismically-induced instabilities, possibly coupled with some sort of an asymmetric loading, either due to the di_ _ rent masses attached to the containment shell or perhaps due to the loads from pipe breaks.

DR. SIESS: If you had your druthers, which would be the best strategy? Will the kind of code that can handle

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coals handle all the different loading asymmetric loadings as easily as, say, the codes for the virgin shell?

DR. ANDERSON: The unpenetrated shell?

DR. SIESS: Yes.

DR. ANDERSON: The calculations are tough. I spend a lot of my time doing calculations and, you know, I am becoming a little bit skeptical myself. These calculations of penetrated cylinders are very difficult. The ones we have done are strictly bifurcation buckling, very easy calculations. The ones in the inelastic range are going to be time consuming. It may not be a fruitful thing to look forward to.

DR. SIESS: Would they be any less time consuming and expensive if it was the code for the shell without holes?

DR. ANDERSON: I am sure of that, yes.

DR. SIESS: And as new loading conditions develop, you could treat those or make perimetric studies of loading conditions on the shell without holes much more easily.

DR. ANDERSON: As your other idea was indicating, if there was some way we could reinforce the holes and can make it behave as if it were unpenetrated, I think that would --

DR. SIESS: That would really be a more desirable approach, but doing that may be extremely difficult because of all the kinds of holes you might have.

MR. BENDER: I kind of got lost in the continuity of the discussion here. If the ASME code right now is inadequate to treat buckling and I think that is the statement you essentially made --

DR. ANDERSON: The ASME code as applied, as developed and applied, for reduction of stress around penetrations. I don't think the ASME code ever claimed that it was to be applied to the problem of buckling. Is that right, Joel?

DR. SIESS: They have rules for reinforcing.

DR. ANDERSON: We are all familiar -- I mean, you take the material out of the hole and put it around --

DR. SIESS: And there was an assumption thought of that that might work for buckling.

DR. ANDERSON: Right.

DR. SIESS: And you found out it doesn't.

MR. BENDER: Now, given that the code doesn't apply, what you are planning to do is develop a procedure for evaluating buckling that the regulatory staff could require?

DR. ANDERSON: I see your problem. These are essentially two different exercises that are going on here. The one exercise essentially evaluating the ASME code. The second exercise is more of a code validation. Now, whether these two meet, I am not sure.

DR. BENDER: One is a creation of a new method of analysis -- creation, not of a new method -- of a method. You may have one, but you are trying to be sure it is usable.

DR. SIESS: You are analyzing a different kind of structure.

MR. BENDER: In fact, just looking down the road, I would say if you get the method and the NRC is anxious to be sure that it is applied, it will try to press to get it made a part of the code, because that is what has been the history of every kind of analytical matter. They have a set of accepted analytical methods that people use and they sort of deal with them, not in a rigid sense, but --

DR. SIESS: Let me back it off a minute. See, one approach from the regulatory point of view would be to stop right here. You found out that you cannot trust the ASME reinforcement rules to make this thing behave like a shell without holes. Now, the staff could say if that is the way you test buckling on your containment, we don't accept it. Now, we want you to do a better job and then leave it up to the applicants, to hire Lockheed or whoever it is to develop the code and to validate it. And, of course, the staff in that process has got to have enough knowledge about it or you have to have enough knowledge if you are their contractor to know when somebody submits a code that it is suitably validated.

So, that is at least some argument for proceeding down this line in NRC. The result conceivably could be an NRC-developed code which they then tell the applicants this is a satisfactory code if you want to use it. I don't believe we have ever done that in the past. It is much more likely

NRC will tell them to do it. They will go get the NRC code and when they submit the results they will get 40 questions back. But I don't know any way out of that. NRC has never, to my knowledge, developed a code and said here, use it. We always develop it and say their best effort, but they are not conservative enough or something else. But I can see a stopping point here by one approach and then I can see going on.

DR. ANDERSON: And then a continuation on this exercise.

MR. COSTELLO: Well, I guess, to put some historical perspective on this, the choice of tasks was taken about two and a half years ago by NRR, who went out looking for who had the procurement.

DR. SIESS: For code.

MR. COSTELLO: Yes. The effort that was grafted on -- the joint effort grafted on between research and NRR was to develop experiments which could be used to validate that code.

DR. SIESS: Okay. The Lockheed code, is that the one NRR contracted for?

MR. COSTELLO: Yes, sir.

DR. SIESS: Okay. So, you picked up with what Lockheed developed for NRR. They wanted a tool you are going to do the validation.

DR. ANDERSON: That is correct. This code I don't think was developed entirely under NRC. The code has been around for quite -- or versions of it for --

MR. COSTELLO: I think Dr. Anderson is correct. The BOSOR code was not developed. The total cost was not borne under the contract. It was the application of the BOSOR code.

DR. ANDERSON: And it is felt to be -- that code and today's code are felt to be the state of the art code and if we are going to calculate this phenomena, buckling of penetrated cylinders, those codes have the best shot at it.

DR. ZUDANS: I would like to return just for a minute back to the whole bigger issue that is at stake at this point. Your problem was directed towards -- the big issues exists that there are no criteria by which to design a containment now in existence because the --

DR. ANDERSON: For buckling.

DR. ZUDANS: For buckling because the ASME thought that that is not designed for asymmetric buckling. It is not designed for any buckling of a structure that is penetrated. They have simple cylinder formulas which you apply and that is all they state. Also, what is found is that it is not that simple to do it because the computed buckling load based on a simple bifurcation analysis is an ideal shape load and real structures simply do not produce such high buckling loads.

So, I guess this is a well-thought out program and

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there is nothing wrong with what you are doing. It is just that what codes you use will be very important because BOSOR 5 cannot put on a structure asymmetric preload. That means they will have to modify. BOSOR cannot put holes in a structure.

DR. ANDERSON: That is correct.

DR. ZUDANS: The statics can do all of those things, but that is a continuum type of finite element program of which many exist and that could do all the job, but it is a very expensive proposition for BOSOR to do.

I would like to return back to your conclusion because it is a very far-fetching conclusion, when you said that the reinforcing around the hole does not restore the buckling strength of a structure to its original value. You based that conclusion on simple analysis of a cylinder that purpose and shape once without any holes and you were able to get a 97 percent of a theoretical buckling load. Once you cut a hole -- I am talking simple analysis, finite element analysis -- then you cut a hole in that structure and you generate only 15 percent of a buckling load. Then you put the reinforcing around that hole and you generated 74 percent of that structure and then you jumped to the conclusion the reinforcing around the hole does not restore original 97 percent -

DR. ANDERSON: Perfect cylinder.

DR. ZUDANS: That is totally incorrect inclusion

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because you analyzing perfect cylinder.

DR. ANDERSON: That is what the experiments were carried out on, fabricated cylinders. And they showed that the imperfections, if they dominate, you can reinforce that hole all you want.

DR. ZUDANS: I understood that your tests all showed at least as much --

DR. ANDERSON: No. Some of them reinforced 100 percent changed the buckling load not at all.

DR. ZUDANS: Well, I don't see here. I have a table on one of those pages --

DR. SIESS: Let's save that for later.

DR. ZUDANS: Let's save it. Maybe I misread something. Okay.

DR. SIESS: We can look at it page by page.

DR. ANDERSON: Okay. If you would like to stop. By some unusual coincidence, it is now almost noon in Albuquerque and I think our experience yesterday was that the restaurant wasn't particularly crowded. So, let's break for lunch. We will be back about 1 o'clock.

(Thereupon, at 12:00 noon, the meeting recessed, to reconvene at 1:00 p.m., the same afternoon, July 1, 1981.)

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AFTERNOON SESSION 1:07 P.M.

DR. SEISS: We will reconvene. You know one possibility, since we are going to go into a little more detail now would be simply to start with the Los Alamos people, since they have got farther to go.

Do you have any objection to that?

Okay, then let us do that, and as I said, we are getting into details, and I am sure you have got -- I didn't know how much more you had on the --

DR. ANDERSON: I am finished, and Elton will pick up.

DR. SEISS: Okay, and that is on the buckling?

DR. ANDERSON: No.

DR. SEISS: Oh, on both parts, okay.

DR. ANDERSON: He will do the Category 1 concrete structure, and Joel Bennett will do the --

DR. SEISS: Okay, that is right.

Then you have the floor, Elton.

DR.ENDEBROCK: I will go through some of the work that we have been doing, both analytical and experimental that has been performed to date on the structural margins to failure program.

I will skip the first one, the general information on the program. That has been taken care of and start with the program plan background. This is simply background information on how we got started, the program plan summary, some of the results to date and possibly some future activities.

Early in the programs one of the things we wanted to do was to make sure the information we generated could be used by somebody or somebody needed it, and so we did an extensive literature search on various topics and, also, visited various AE's.

The chief AE's were TVA, Bechtel and Sargent and Lundy, and one of the things that came out of those discussions was the desire to know more about damping characteristics as you got into the higher load levels, and so we did a rather extensive literature review then on what has been done on damping and so forth.

The type of information that we were looking at is listed below, type of plant layouts, what they looked like, codes and guides used in the design of the plants, any particular or unusual construction methods that any of the AE's may have employed, loads that control the structural element design. By this, I mean, for instance, the exterior walls of the plant, the size or the thickness ar determined by the missile penetration capabilities and not by loads as such, so things of that type and the types of analysis used, both linear and non-linear and so forth, and then one question we always ask is what they thought was the information that was needed that would be beneficial to them.

You have seen a lot of pictures or drawings of containments. I am not sure how many you have seen on typical plant layouts for the Category 1 type. I will just flash a few by suddenly.

There is one type, with the Category 1 being surrounding the containment in this particular case; another different type of layout. Okay, this type, this one the buildings are Category 1's are more disjointed and more in separate units than the last case, and in the next one I have, again, they are more as a single building rather than a separate unit.

All of these show the turbine building, but they, of course, are not Category 1, and we are not interested in the turbine building as such.

DR. SIESS: There have been some instances in the older plants where the turbine building did house some Category 1 components. I am pretty sure it was true at Diablo because they had to strengthen the turbine. Were the diesel generators in the turbine building? It was something like that, but I agree, it is rare.

DR. ENDEBROCK: It is normally not done. After our discussions with some of the AE's, the topics which they considered could use additional attention as far as they were concerned were damping, what would be the damping in particularly cracked shear walls. The rationale was that some felt that because the percentage of concrete in a shear wall that can be cracked is large compared to, say, a beam where you have localized cracking that the damping might be more. So that is one of the things we are focusing on; stiffness of cracked shear walls, also, enters in, and that, of course, goes in with the topic of stiffness degradation with load cycling and, also, around the industry many felt that the failure should be probably expressed as a displacement limit of some kind, and this we are considering as we go along with no answers as yet, and then of course the other is what to do with structure equipment interaction, and that was a common topic. So, structure interaction came up, but of all this list we are not considering that last one. We have nothing to do with soil structure interaction.

DR. SIESS: That makes you almost unique. I think everybody else in the world is considering it. I am glad that you are leaving it out.

DR. ENDEBROCK: Of course, our major goal is the structure equipment interaction effects toward the end of this program.

DR. ZUDANS: There you are. It is still interaction.

DR. SIESS: We cannot get away from interaction.

DR. ENDEBROCK: Okay, some of the items on our program plan and some of the things that we have done, just to briefly mention it, some of our goals on the analytical program were to either locate or we are not too much on development or inspire someone else to develop a program that would do a better job of predicting the behavior of reinforced concrete structures. Our idea is we supply some data, benchmark type problems and let maybe somebody else use that to tune one or to check it out.

The survey of the different finite element goals was made. This was already mentioned previously and that sort of covers it. Just skip over that, and one of the things we are doing in the process of this is developing small special purpose computer programs. That is mainly to help us in the design of the experimental program.

I will show some results of a program or two of this type a little bit later. These are not lengthy. They are relatively short programs. This one you have seen, also, a different phase of the testing program. We are somewhere in the middle of the first phase testing of the one story test structures right at the moment. Phase II is more planning, and of course, Phase III is by the end of Fiscal Year 1982, we will probably have a program plan for Phase III.

Notice Phase II experiments are just a larger structure than the first, and one of the purposes is to, I guess you might say for those who are worried about scaling to try to verify the behavior of scale models, and this only includes a few tests. You can use, say, normal reinforcing bars and so forth in a scale, but still it would be a scaled down version of a test structure.

DR. SIESS: Elton, when you have a test program like that to verify scaling, as you put it, and only a few tests, as you put it, what are you going to do when it doesn't verify scale?

DR. ENDEBROCK: Good question. I am not sure yet. We can always use it for information and for benchmark problems.

DR. SIESS: Oh, yes, it is useful, but then you know --

DR. ENDEBROCK: It is for our own, also, because one thing we want to make sure is that the gross behavior is roughly the same. That is the main thing we are trying to show.

DR. SIESS: But you are going into this with the idea, I believe that scaling will work, in other words, that you will verify.

DR. ENDEBROCK: Right.

DR. SIESS: But I guess you need a contingency plan there or at least the staff ought to be aware that they may have to have a contingency plan.

Suppose you tested at two scales, two small scale levels or three, and it showed clearly that there was a serious scaling effect?

DR. ENDEBROCK: Okay, then one thing we would know is we would have no confidence in translating the behavior to large scale.

DR. SIESS: That is an important conclusion which now means stop and let us figure out what we are going to do.

DR. ENDEBROCK: Right.

DR. SIESS: But if you tested it, too, and you saw a scaling effect, I guess it is possible to say, "Well, if I take a range of scales, I might be able to extrapolate." Nobody would believe it probably, but it is always possible. If you have got a good theory, it will work, but it is purely empirical. People seem to question it very seriously.

DR. ENDEBROCK: The heartburn about scaling came up in our peer review panel, and so we are trying to come up with ways to try to be a little more convincing that it wasn't that bad, and I guess we don't have a good plan, if it doesn't right now. That is not the entire purpose of them though, either.

DR. ZUDANS: Have you given it any thought at this time how are you going to represent the damping? How are you going to describe it? How many tests do you need there because damping will be a function of frequency and, also, the amplitude of your deformation, and I am just figuring a rather complex picture? How are you going to extract the information that other people can use afterwards? In other words, it varies all over the world.

DR. ENDEBROCK: Oh, you mean in case it doesn't work?

DR. ZUDANS: No, it is not a question of working. You will test in a large deformation range to establish damping values. Those damping values --

DR. SIESS: This is over and above the scaling question.

DR. ZUDANS: Yes, those damping values are functions of the frequency that you excite the structure to and, also, the amplitude of deformation.

DR. ENDEBROCK: I think this will be answered later on in the discussion.

DR. ZUDANS: That is fine. I just wondered whether you had given it thought.

DR. ENDEBROCK: Okay, going now into some of our analytical studies, the atypical force displacement relationship as far as shear wall looks something like this, and again for analytical reasons it is nice to have it idealized. So, we idealized it to the bilinear type of curve, such as this where K_1 is the initial stiffness, and K_2 is the softening part, and delta is the, well, you call yield point or the breakpoint when it starts softening.

DR. ZUDANS: This would be for slow loading.

DR. ENDEBROCK: Okay. We did it for all kinds, analytically now, that is. This, incidentally, in our actual analytical model that point is not that sharp. That is rounded. So, this was something just to get us started and get us fairly close to the actual behavior. There are arguments on whether the shape of this is correct and all that, I realize, but we were, again, gearing this to the design of the experiment, just to get some preliminary results.

Okay, we started with the classical approach, and that is to apply as an input a sinusoidal forcing function. Okay, then you compute response curves. For single degree of freedom everybody is familiar with the response type on those for the linear system, and so I won't show that by itself.

I do have one in dotted line on this particular vugraph though. One of the things we did with the sinusoidal forcing function and using the bilinear softening system is to develop a series of response curves using different inputs. The magnitude of the input was varied, and the characteristics then change with the level of the input.

Then with this we tried computing things like the equivalent stiffness. One of the things that always goes on is trying to use equivalent, trying to relate the non-linear effects to damping effects, mainly viscous,and so this was a way to check out to see how close this would come true.

For this particular case the input compared to the yield of the thing was a particular value 1 and the K_2 K ratio was zero.

The dark line is the one we actually generated from our program. Incidentally this computer program makes no assumption as to relationship between the acceleration velocity and displacement. It is a solution of differential equations, and so the only approximation as such is in the numerical technique itself and the way the equations are solved.

From this we are going to the non-linear range or into the softening. We can then pick off the frequency at which we get the peak value and then, also, from the height compute a damping value, and this is based on single degree of freedom viscous damping which is normally done. This is the normal procedure.

By taking then this natural frequency we can go back and compute the stiffness, the equivalent stiffness of the system.

You do this for a series of curves. You can come up with something like this. This is for different K_2 K ratios. The U over delta is the response divided by the yield point, and this then is equivalent to stiffness.

Okay, you just get curves, and they satisfy the physics of the problem.

DR. SIESS: That is the stiffness for which an elastic system --

DR. ENDEBROCK: This is for an inelastic system, right.

DR. SIESS: The KE.

DR. ENDEBROCK: Equivalent elastic system.

DR. SIESS: That is the stiffness for which an elastic system would have the same frequency.

DR. ENDEBROCK: Same frequency, right.

DR. ZUDANS: And the abscissa represents the deformation.

DR. ENDEBROCK: That is right. This is deformation. Our first --

DR. SIESS: I am sorry, what does the U over delta represent on that?

DR. ENDEBROCK: U is the response and the delta underneath is the distance to the yield of the force deflection relationship.

DR. SIESS: Go back to that figure where you had the FU plot and show me. Is U the maximum response?

DR. ENDEBROCK: No, this is acceleration in this one.

DR. SIESS: No, this figure?

DR. ENDEBROCK: Okay, that one?

DR. SIESS: Now, is U the maximum response?

DR. ENDEBROCK: U is the maximum response.

DR. SIESS: All the way out to the end of that?

DR. ENDEBROCK: Wherever. In this particular case U could be thought of as being maximum.

DR. SIESS: It was not clear to me whether U is the dynamic deformation or U is the point on that curve.

DR. ENDEBROCK: U is the dynamic response.

DR. SIESS: I understand.

DR. ENDEBROCK: I probably should have shown you the non-dimensional form first, and that would have shown that up a little bit better.

DR. SIESS: That is all right.

DR. ENDEBROCK: Then computing the equivalent stiffness and then the equivalent viscous damping based on the curve, the non-linear relationships and putting it into a linear system just to see what had come back, and this first is the response that we got from our computer program with no assumptions involved, just straight non-linear effects, and this particular thing the K_2 K_1 ratio is 2/10 and the input was equal to the yield displacement.

It still goes non-linear because of the response going out. Our purpose here though was to find out if you truly could say that you could pick out an equivalent stiffness and an equivalent damping and relate this to viscous damping and say that you can get equivalent response. This then is the linear system using equivalent stiffness and equivalent viscous damping. You notice you do not get anywhere near the same thing anymore.

DR. SIESS: What are the two curves?

DR. ENDEBROCK: This is the 2 degree of freedom system. One is upper mass and the other is the lower.

Incidentally these don't show up on a 1 degree of freedom system too well. This is why we have gone to 2. In fact, you really don't see the difference until you get to a second mode. Just one does not give you that much of a difference. In fact, you cannot see any difference using 1 degree of freedom.

DR. ZUDANS: But when you generated your equivalent elastic properties, you generated then from the information that you got at the first natural frequency of the non-linear system.

DR. ENDEBROCK: That is right.

DR. ZUDANS: Therefore there is no reason for you to expect that it will check the linear equivalent system at any other frequencies.

DR. ENDEBROCK: It doesn't, it turns out.

DR. ZUDANS: It does not.

DR. ENDEBROCK: For both degrees of freedom it doesn't even for the first frequency. Notice on the non-linear the deep response for both is about the same and when you throw in the equivalent system they are not, and so one of the things we just concluded that the idea of trying to represent non-linear systems with linear methods may not at all be applicable. You may run into problems.

These are just like the type of things we have been looking at just related to damping.

DR. SIESS: Haven't there been attempts to use a substitute linear system with a modified damping factor rather than a modified K effect?

DR. ENDEBROCK: Yes, and I can show you some results as we go along on that as well.

Okay, at this time I will switch from the sinusoidal input to an earthquake type record input to find an acceleration time history, but before we do that let us look at some system variables. The problem for our case was cast in this form. Now, to explain the various things the K_1 over M, of course, is the usual natural circular frequency. Theta in this case is a frequency characteristic of the earthquake record, and in our studies we did not know the exact value we should use there, and so when we plotted this particular quantity we applied frequency directly.

U is the relative displacement response. That is dynamic response. X is the absolute acceleration response. The K_2 K_1 I think are self-explanatory. That comes from the force deflection relationship. Now, this quantity delta, also, comes from the forced displacement relationship. The Y double dot peak is the peak acceleration of the acceleration input and the theta is the earthquake characteristic.

DR. SIESS: I assume that is a K_1 on the bottom line there.

DR. ENDEBROCK: Okay, I forgot the 1 there.

DR. ZUDANS: I have to return back to the other because something bothers me, and I would like you to answer. When you created the equivalent elastic characteristics, the K sub E that you called and you took the actual frequency and you modified the damping ratio to produce the peak and then you had tc do that by using the linear equation versus the non-linear, and that means if you would apply those computed quantities in a linear system you should get exactly that peak.

DR. ENDEBROCK: That is right. You should.

DR. ZUDANS: Why didn't you get that peak exactly?

DR. ENDEBROCK: Because all the methods for computing your damping from the response applies to a single degree of freedom system only, and this was a 2 degree of freedom system.

DR. ZUDANS: Oh, you changed the system.

DR. ENDEBROCK: Yes.

DR. SEISS: He derived it from a single degree and then applied it to a 2 degree.

DR. ZUDANS: That is then no surprise at all.

DR. SIESS: It might be to some people.

DR. ENDEBROCK: The quantity on this is really -- it has the most effect. We used this last term Y double d peak acceleration of the earthquake divided by the yield displacement of force deflection curve divided by theta squared. Keep it in mind we don't know what theta is though. The only thing we maintained in our studies it was a constant because we used the same earthquake record.

DR. SIESS: What is it a function of, the frequency content of the --

DR. ENDEBROCK: This is some frequency content or characteristic of the earthquake. Okay, this is, again, just to show a little more of the nomenclature involved, the frequency content characterization of the record which is theta, peak acceleration. The U is the relative displacement, story displacement, however you wish to call it, but X is absolute acceleration. We cast it as absolute because in our tests that is what we have to measure and experiment. So, we avoided the use of relative accelerations.

Okay, the earthquake record always generates controversy. We had to have something as an input. So, all we did was generate one that envelopes the NRC response, and this happened to be for about 2 percent, and the -- okay, the NRC is the dotted. This is just to give you an idea how well that enveloped it.

DR. SIESS: Did you generate that one yourself or is that one somebody --

DR. ENDEBROCK: No, we generated it ourselves.

DR. ZUDANS: That is an extremely good history.

DR. ENDEBROCK: In our case, with the tools we have there, it is the combination of a whole lot of luck and a little bit of art, probably to get to something like that.

DR. ZUDANS: That is an extremely good match. I have never seen anything like it. Lots of luck there?

DR. ENDEBROCK: Yes.

Okay, this is shown on the usual tripartite paper; however, our responses are not cast in this. So I will show the same thing in more of the manner in which our results will be presented. Our information will be the absolute acceleration divided by the peak response of the earthquake type. The response will be shown in that form, and then we are plotting directly against frequency. We did not know what the value of theta for the earthquake is and did not want to spend time to try to come up with anything on that, and so this is the type of curve, and this again is just to show how the relation between the generator earthquake and the NRC response technique.

The last parameter on the dimensionless forms is actually a measure of the input,and I have vugraphs of different magnitudes or different values and inputs to see how they come along. We did not know the value of theta. So, actually the one really represents peak acceleration divided by delta. These are dimensionless quantities. So, in the numerical solution it does not make any difference which you vary in a particular case. You can vary either one and still get the same result. Okay, this is to show an effect of damping, how it affects the response over the different frequency ranges, trying to use a damping value to match non-linear effects. Okay, for zero damping you see the curve, and incidentally that does not show up as well on this one. It will in the next. There is always a frequency in which this reverts back to the linear response. The acceleration on this does not show up too well. It will in the next one.

Okay, the K_2 K_1 is linear. When K_2 K_1 ratio is 1 it is linear solution, and now the K_2 K_1 is 1/2. That, of course, gets it into the non-linear type arrangement and that is represented by the solid line. So you can see the change in response this way, but to a certain point and then it goes linear, and you run into the other curve.

DR. SIESS: What is the damping for the solid curve?

DR. ENDBROCK: Zero. We use zero damping for all --

DR. SIESS: It just was not shown.

DR. ENDEBROCK: Except to show what effect damping would have. Then, of course, this is at zero damping which is the linear top one. So you just add damping to the system and consider it still linear. You bring down this curve, and notice it is not uniform over all frequency ranges. This, again, indicates that changing damping to account for non-linear effects is not too reliable over all frequency ranges. You have to be careful with that.

Okay, here are the same parameters at the top, same input. This is the displacement response though instead of -- relative displacement response instead of the acceleration, and notice one thing. This happens on all curves. When U over delta is 1, anything below that reverts back to the linear response, and so you can then go down and pick off the frequency above in which you will always have linear response regardless. This is, of course, for a particular earthquake record though, and so we have not studied the effect of different earthquakes.

Above then we do have non-linear response excursions into the softening part of course reflection relationship. Okay, again for the linear case which is the dotted line, zero damping and then adding damping to see how it affects the relative displacement, and the points join toward the bottom. This has a little better range where you could probably get non-linear effects by using damping values. This is viscous damping, but again not that good.

Now, just to show a relationship between that last one, I had the frequency f of 1. This is plotting for different values of input and the frequency which gives you the dividing point between linear and non-linear region. This type of curve may be useful to someone if they know their input parameter and whatever frequency. You could quickly take a look to see whether you even have to worry about non-linear effects. This would give you a quick look. Again, this is for one earthquake record though.

DR. ZUDANS: This is based on that stress/strain relationship that you assume?

DR. ENDEBROCK: That is right.

DR. ZUDANS: This curve could be shifted all over the place if you move either the delta or --

DR. ENDEBROCK: I don't think it could be shifted all over the place. I think it could be shifted some.

DR. ZUDANS: If you move delta down, it will be moved more into non-linear range; if you move it up, it will be --

DR. ENDEBROCK: Yes, but that --

DR. ZUDANS: Moving it up would not do anything.

DR. SIESS: That has got delta in it though.

DR. ENDEBROCK: That is the ratio, peak over acceleration to delta. That is a measure of the input.

DR. ZUDANS: Except that in a non-linear solution no such normalizing will work anyway. There will be a difference if you change that. For linear range, yes.

DR. SIESS: It would work below delta, you see. So that has to give you the right break point, doesn't it?

DR. ENDEBROCK: Even if it is curved you can come up with an intersection point.

DR. ZUDANS: I would like to see what that says.

It should work below.

DR. SIESS: As long as there is a linear region this ought to work.

DR. ENDEBROCK: Right.

DR. ZUDANS: But the linear region is defined by materials curve that was used. So if you change the curve the region will change.

DR. ENDEBROCK: But the delta is in here.

DR. ZUDANS: Yes, but the slope is not.

DR. ENDEBROCK: These are cast in non-dimensional parameters, and so all that is determined is determined by that whole quantity of variables.

DR. ZUDANS: You cannot analyze non-linear systems in non-dimensional parameters.

DR. SIESS: But it is a linear system.

DR. ZUDANS: This is a bilinear analysis that you are talking about now.

DR. SIESS: Yes, but he is talking about the upper part of this curve defines the linear part of it, and if it is linear, it is linear. All the rest of it is non-linear. It seems clear to me.

DR. ENDEBROCK: That simply means that at those frequencies or above that the response will always be linear. It will never go out into the non-linear range past your yield information. You can pick that anywhere on a curve.

If you were given a forced deflection relationship no matter what shape, as long as it was linear you could pick that at any place or input parameter here.

DR. SIESS: You can always define the end of the linear behavior.

DR. ZUDANS: Yes, but the end here is defined in two quantities. Delta is one of them, and the slope of the curve is another one, and it might change the slope of the materials curve with the same delta,however different linear range, because it will affect the linear response, but the finding is okay, the fact that there is a certain natural frequency.

DR. SIESS: Does the peak acceleration normalize out that stiffness?

DR. ENDEBROCK: Does the what?

DR. SIESS: You have got the frequency in here, and the frequency certainly depends on the stiffness, right?

DR. ENDEBROCK: The stiffness of the system, right.

DR. SIESS: So, if this is to be general for different stiffnesses, something has to normalize that out.

DR. ENDEBROCK: Okay, this is for a 1 degree of freedom system now. We have not gotten past that.

DR. ZUDANS: I think we can agree without difficulty that there is a range in response rates, all linear.

DR. ENDEBROCK: Right.

DR. ZUDANS: And whatever that curve looks like, it doesn't matter.

DR. ENDEBROCK: Incidentally, I don't know, maybe this will answer you better. The program as it computes this starts with a low stiffness and increases it, and so it is, well, sort of like the calculations in response spectra, where you are actually changing the system stiffness.

DR. SIESS: But this plot is intended to apply for any bilinear system.

DR. ENDEBROCK: Yes.

DR. SIESS: So the peak acceleration term must take into account somehow the mass of stiffness and normalize that out of the thing because otherwise --

DR. ENDEBROCK: Okay, yes, this is all --

DR. SIESS: The peak acceleration varies with mass and stiffness.

DR. ENDEBROCK: Not the earthquake. This is the earthquake record. The Y double dot Y peak is the earthquake record maximum acceleration. That is a fixed quantity. The response is X double dot.

DR. SIESS: Okay.

DR. ENDEBROCK: That does not show up on this particular graph.

DR. SIESS: If I take a non-linear system with a different mass and let us say a different stiffness, this curve would stil' .pply?

DR. ENDEBROCK: Yes. All you have to do, you know your input, and you know your natural frequency and see where they cross, how that falls on where your -- supose your Y peak over delta is 4, and the natural frequency of your system is 8. You go up. That is in the non-linear range.

DR. SIESS: I was actually looking at a point where it is 4, and the natural frequency is 16.

DR. ENDEBROCK: In that case it would be linear. You would never go into the non-linear.

DR. SIESS: You will be linear no matter what the stiffness of the mass is?

DR. ENDEBROCK: That is right.

DR. SIESS: Okay, I have not quite figured out why, but I will buy it.

DR. ZUDANS: For this material?

DR. ENDEBROCK: For this particular earthquake. This is for a one earthquake record only though.

DR. SIESS: This is for a particular record.

DR. ENDEBROCK: One of the common assumptions made in this with the response spectra is that the acceleration displacement, well, actually it is the displacement, the relative displacement, relative velocity and absolute acceleration are related through the natural frequency of the system. Okay, so we just computed then on one of the acceleration plots here, computed the pseudo acceleration and plotted it on there just to see how that would match up and see how good that approximation is and when you are going into the non-linear range.

Okay, the dotted line shows the pseudo acceleration and again it reaches a point though where that is true, mainly in the linear system. If the frequency gets high enough where you are linear, they are identical, and otherwise they are not. So, the assumption is good for the linear range, not really for the non-linear case.

To now see what effect the different stiffness ratios have on the response, this is displacement response; the ratio varied from zero which is your elastoplastic curve up to the linear case. K_2 K_1 is the linear. Looking at the responses, notice that has very little effect as to how soft your system would become. It is sort of surprising and somewhat a little bit up at the top, some difference in the displacement response, but not a whole lot.

DR. SIESS: This is peak response?

DR. ENDEBROCK: That is peak response, yes. Okay, that is for a Y over delta theta square input of .1 over theta square. Here it is for 1. Of course, the displacement increases, but again the scatter in the curves is very little actually.

DR. ZUDANS: Actually I don't know whether it is fair to say little because you are plotting in a logarithmic scale.

DR. SIESS: But it is still little.

DR. ZUDANS: If it is a factor of 8, it is not little.

DR. SIESS: None of them are a factor of 8.

DR. ZUDANS: It is a logarithmic scale that you are plotting, right?

DR. ENDEBROCK: Yes. Some of these others show up a lot better than that does.

DR. SIESS: There is a factor of 2 in there sometimes.

DR. ENDEBROCK: Let us put it this way, if you were working with different things, this looks small compared to some of the others.

DR. ZUDANS: Oh, yes, looking at thermohydraulic solutions, this is pretty low.

DR. SIESS: You have got a factor of 2 or maybe 3 at the most in there.

DR. ZUDANS: That is in structural response. That is a lot.

Also, this conclusion cannot be generalized beyond the fact that this is for this particular earthquake.

DR. ENDEBROCK: Right. It is limited to one earthquake. We feel that you will get similar results for different types of earthquakes. What will change is these wiggles, but you will have -- okay, it may shift somewhat, but it will be a very similar type of response.

DR. SIESS: How does this compare to some of the stuff? I know Newmark played around with with inelastic behavior. Had he done it on earthquake records or just on sinusoidal?

DR. ENDEBROCK: I think he used earthquake records. I am pretty sure he did.

DR. SIESS: Have you looked at that?

DR. ENDEBROCK: Yes, I don't have all the results of that here, but in fact, I am just getting a paper typed up now which goes through his, and the one thing we have checked, in fact, we have been checking with the Newmark response spectra for the linear system and the non-linear, and forthe linear thing there is no way. There is nothing we could find you could even argue with on what he has, and on the non-linear some of the statement he has like on the non-linear using basing it on elastic deformation instead of a total to tie the one end, that is very close to being right on. The only thing where his response curves come off is in the higher frequency range where they should all go to one to be linear, but he shows with different values of ductility, shows them at different levels, but they should reach a point and then merge into one, and he does not show that. That is about the only thing, and some of the assumptions made as for relations between the various quantities are, particularly on the linear. These type of plots really show that up and how good approximations they really are, and it is amazingly good. I don't have all those results here.

DR. SIESS: I stopped a long time ago being amazed at how good some of his approximations were.

DR. ENDEBROCK: Like I say, it was quite surprising how well they fit. Okay, that is for the input of 1. This is one for 10 which is a very large input, and things start going to pot now a little bit. This is, again, on a displacement type. They are getting separated considerably and particularly for the very soft system, the elastoplastic. The relative displacements now are getting much bigger. They are still bounded, but they are rather large. So, of course, that is for no strain hardening at all.

DR. ZUDANS: I notice that spike that you get is for no strain hardening at all, and that might be a numerical problem rather than physical.

DR. ENDEBROCK: No, we thought that at first, too, and so we did everything to check that out and change time steps, did everything and it still shows up, no matter what you do. You couldchange the input and get rid of it.

These points for certain systems just seem to occur and nearly always for the low or the K_2 K_1 is equal to zero or --

DR. SIESS: Does it have to be zero to do it?

DR. ENDEBROCK: No, we had some that got as high, I think like at 2/10, and it still had them.

DR. ZUDANS: I would say the difference that you have on this graph between .1 and zero suggests that it is a numerical problem.

DR. ENDEBROCK: Our studies on that didn't, because that was the first thing that came to our mind when we saw it, too, and so we tried to get --

DR. ZUDANS: How would that poor structure know that dramatic difference between slope that you almost cannot see --

DR. ENDEBROCK: This is very similar to the linear resonant type response. There is just something peculiar with the system or some frequency content in the earthquake record or something that drives it there.

DR. SIESS: And there is nothing to stop it.

DR. ZUDANS: Physically you reach the state where you have no added stiffness. So, you are working like --

DR. ENDEBROCK: Yes.

DR. ZUDANS: And the only thing that allows you to solve the problem at all is the inertia and the mass that you have in the system. In static cases it would blow up automatically at that point. In dynamic case it would require dramatic change in your step size, a reduction.

DR. ENDEBROCK: I am not sure what damping it would -- this is zero damping.

DR.ZUDANS: It does not matter.

DR. ENDEBROCK: With damping you can cut those out considerably.

DR. ZUDANS: Yes, but you see if you look at the condition where you have no longer added stiffness as you move around, it is incremental, because in a non-linear case you have to solve it -- I don't know how you solve the problem, whether you use something like the slope at that time and then you have zero stiffness.

So, you just follow rigid body motion.

DR. ENDEBROCK: That is okay, yes, but the thing is it has never blown up as such. There has always been a limit. Some of these numbers, also, get very large up here. It has never blown up on us as such. That may simply be because of the numerical procedure, also. I don't know, but these do appear. We had other studies where they, also, appeared. Rather than casting it in this fashion, it may occur for those low K_2 K_1 ratios.

DR. SIESS: Is this still the single degree of freedom system?

DR. ENDEBROCK: That is still the same system, yes.

DR. SIESS: Two degree of freedom?

DR. ENDEBROCK: No, this is one.

This is one. We have the results on one. We are looking at some of the others. We had a little bit on two degree of freedom which I will show. This is a large input, mind you though. This is very dependent on the input level.

DR. SIESS: A lot of energy.

DR. ENDEBROCK: This same type of series for a two degree of freedom system, okay, this is the two extremes, the linear and then where K_2 K_1 is zero. In this case the linear is very near the maximum, not always. There is a little bit there, but on the multidegree that we have done, except for isolated regions we have found that actually the response for the first floor the U_1 usually bounds nearly all the responses of the others, except I say at local points, and this shows up here except for a little right in here. It bounds the non-linear case, linear does.

The next level of input, okay, this is the linear system in the dark line. No, it is not either. That is the zero. The linear one is the light dotted line right in here. The lower story non-linear does bound nearly all the others. Notice the response on the top floor is way down except for certain frequencies, and then it jumps up, and in some cases this even goes way up in a spike.

Now, with the high input, again, things start to get a little wild with the real large inputs, but notice the second floor again, the response has very low ones. For certain frequency ranges it does spike up, but again it is the, except for this point, the non-linear response at the lower floor bounds all the others.

We have done various other studies, a lot more than I have given here mainly because we did not get too much information out of some of them. So, we have looked at various aspects. We are writing a report to give later information. I don't have that with me now.

So much for the analytical. Now to the experimental part of what we have done. Okay, our tests, we were doing static and dynamic tests of our shear wall structures. We wanted to obtain damping characteristics. I guess I forgot to mention one of the things, and I guess I did not bring the vugraph on it. Okay, I didn't. On our response studies using a sinusoidal input on a 2 degree of freedom system we looked at different types of damping, the main two being viscous and structural damping to see what kind of responses we got, and the interesting thing is when you use viscous damping it is frequency dependent. So the response in the higher mode dies out rapidly as your damping increases.

However, in structural it does not. The response at the second mode and the higher modes remains at a high level, as high as the first and in some cases even exceeds it. So, we hoped to maybe make use of this to determine the type of damping. When we do one of our two-story models we want to shake it in the linear range and look at the response of the second mode and see if we can distinguish whether that is primarily viscous or primarily structural type damping, and of course with test equipment and such this may not work out. Theoretically it indicates that you should be able to tell what type of damping you have, but when it gets to the practical case of a test I am not sure whether that will show up, but we are going to try that.

Okay, we want to determine the failure patterns and establish benchmark cases. The shear wall in this case was selected as our test structure. Number one, it was the one element that was mentioned the most in our AE interviews, and number two, after a rather extensive literature search it, also, happened to be the one element in category 1 building that had the least information known about it, and that was the main reason for selecting that particular structural element.

Okay, the sizing of the test structure, and that, of course, depends on the facilities, and since all the facilities are limited in what their input this is why we had to scale the structure down to be able to fail it, since our main interest is what happens from, say, the elastic limit on up to the highest load you can get until it collapses. The static loading setup looks something like this. Incidentally, this is cyclic static loading. As this is loaded, this is loaded in both directions and going to a higher level in each case, and so we do get load cycling effects even for the static test.

DR. SIESS: You have got a load hydraulic actuator at one end and an arrow that says, "Applied load" at the other.

DR. ENDEBROCK: That must be the draftsman's, but that should not be there on the other end. I am not sure how that got there. That is the first time I noticed it.

DR. SIESS: Oh, that is the direction, I see. Oh, it has got two arrows on it.

DR. ENDEBROCK: Yes, it says, "Load" on it though instead of d[illegible]ection.

Our test structures look like this, sort of like I beams with very thick flanges. Very early in the design we considered having walls which went at the ends, but because of the way we were going to conduct our tests, but changing the stress in the shear wall in different tests to see what effect normal load had on it, we avoided that because this way we felt we knew, had a better idea of what the actual stress in the shear wall would be, the normal load, and by putting those at the end, then you know it is a guess, and so we left those off because of that and the two story with of course another floor on it.

DR. SIESS: Have you got that dimension somewhere else?

DR. ENDBROCK: It is 18 inches long. The width of the shear wall is 1 inch, and its height is about 7.2 inches. The total length is 18 inches.

Okay, along with each test we do the standard concrete testing. Here is this amount of compression strain curves.

DR. SIESS: I assume you have got a so-called "micro concrete."

DR. ENDEBROCK: Yes. The maximum size aggregate was scaled down. So, it is a very small aggregate.

DR. SIESS: What kind of reinforcement?

DR. ENDEBROCK: The shear wall reinforcement we are using 1/2 inch hardware cloth. When we went through our trying to find sizes and so forth we found that and it came out to be a percentage of .5 percent reinforcing in each direction, and since the range on these was like from .3 to .6 we thought that was a good compromise and this would save us a lot of time from having to try to fabricate the mesh.

DR. SIESS: Does it contribute anything to the stiffness?

DR. ENDEBROCK: Apparently not. Our stiffnesses of these tests show them to be --

DR. SIESS: What is the connection on the hardware cloth, is it just the galvanizing?

DR. ENDEBROCK: They are essentially galvanized together. It is just like welded wire fabric in a sense. They are joined.

An example of some of the load cycling. Notice one thing is surprising. You do not get much ductility. There is a reason for this particular test, and two of the others. The load you get is very dependent on the amount of reinforcing at your wall, slab or roof interface and what happens is unless the reinforcing is much more, say, than in the wall further down, why you have a slipping failure at either the wall slab interface or the wall top interface, one of the two.

DR. SIESS: What are the three sets on there?

DR. ENDEBROCK: Those are different gages. This is load deflection. We put deflection gages right at the bottom of the wall and at the middle and the top and both sides. These are the three gages on the one side. Five is the lower; three is the middle or intermediate; and this is the top. For instance the displacement of 1 minus 3 gives you the relative displacement then in the wall, from the top to the bottom.

DR. SIESS: Do you tend to get that same kind of pinching or hourglass effect?

DR. ENDEBROCK: Yes.

DR. SIESS: That has been observed with just ordinary columns. Now, the lines going off on the left, that is the failure line?

DR. ENDEBROCK: This is when it failed. All goes to zero. This drops.

DR. SIESS: Is that dropoff then the stiffness of your loading apparatus or the stiffness of the --

DR. ENDEBROCK: These lines here are -- disregard those because that happened after failure. That was just the pins on the recording device.

DR. ZUDANS: This load was applied at one end of that upper flange?

DR. ENDEBROCK: Right.

DR. SIESS: For all practical purposes it was applied uniformly.

DR. ENDEBROCK: Applied horizontally.

DR. ZUDANS: Not uniformly.

DR. SIESS: Well, it was probably stiff enough that there is not much -- you are really applying a deformation at the top of the wall.

DR. ENDEBROCK: That is what we tried to get in the test.

DR. SIESS: And you would assume that the shear is uniformly distributed horizontally along there more or less.

DR. ENDEBROCK: We hope that, but again just the nature of connections and such, it probably is a little higher where the load is applied rather than on the opposite side.

DR. ZUDANS: On that figure you have depleted uranium cylinder and that was to push the actuator?

DR. SIESS: No, given axial load.

DR. ZUDANS: That is not the axial load.

DR. ENDEBROCK: We are trying to apply shear load to the test structure.

DR. SIESS: No, that load you have got on there is to put some compression in the wall presumably.

DR. ENDEBROCK: Okay, yes. You mean the top, the depleted uranium?

DR. SIESS: Yes.

DR. ENDEBROCK: This is to vary the normal load on the shear wall. On our tests we have done so far we have not included any added weight. It is just the weight of the top slab for the normal load on the wall, and we waNt to vary that somewhere down the line.

DR. SIESS: This test did not have the depleted uranium?

DR. ENDEBROCK: No, it did not have the depleted uranium.

DR. ZUDANS: The actuator structure is stiff enough so that it does not tend to tilt?

DR. ENDEBROCK: It is now. It was not at first.

DR. ZUDANS: I guess you had some good experiences

DR. SIESS: That is when you really appreciate small scale tests.

DR. ZUDANS: Really it is very difficult to know what you put in. It is just like a qualitative thing that you are getting.

DR. ENDEBROCK: Some of the types of testing that we wish to do in the future is these are the normal sinusoidal type input, and this is determined to try to find out damping values and type of -- for the single degree it is just damping values, period, because single degree would not tell you the difference, say, between like whether it is viscous or structural damping becuase you do not have the second mode response, and here the W_1 W_2 are the different normal loads on the structure, and that is where the depleted uranium comes in to give us different normal loads, and then you follow the usual test procedures, this mainly for the elastic range and then later get it at high enough levels to go non-linear.

Okay, I have a couple --

DR. SIESS: Did the mode of failure in that test surprise you at all?

DR. ENDEBROCK: Not really because other tests that have been reported reported the same kind of failure. What surprised me, I guess the most on one of them, the second one, we thought we had enough reinforcement to prevent it. We doubled the amount of reinforcing in the regular, in the part of the wall, and it still failed the same way.

DR. SIESS: You say you had a shear failure at the bottom of the wall?

DR. ENDEBROCK: Yes.

DR. SIESS: Just a sliding?

DR. ENDEBROCK: It just separates.

On the second test though we did get shear cracks, but that was the extent, it was not a shear failure, well, it was a sliding failure at the base. We suspect this is the type of failure if there is one in a Category 1 structure because you not only have the weakness of the reinforcing there, you also usually have a construction joint. So, it is possible that it would be one of the two would be the weak point.

DR. SIESS: You should be able to check that out with the shear friction theory.

DR.ENDEBROCK: Yes.

DR. SIESS: Did it check out at all?

DR. ENDEBROCK: We have not yet.

DR. SIESS: I would be interested in the result. I would like a check on the shear friction theory. That is really what I am looking for.

DR. ENDEBROCK: Okay, on the shear friction. Well, we will get some idea of that when we put the depleted uranium on and find out what the effect of normal loads. That will tell us something on that.

DR. SIESS: I think you are getting real friction, and shear friction is artifact.

DR. ZUDANS: Did the fracture develop starting at one of the ends and progress through?

DR. ENDEBROCK: Yes, it progressed, started in the end and --

DR. ZUDANS: That is where the high shear is anyway, distributionwise.

DR. ENDEBROCK: I think the cracking was initiated by the combination of flexure and shear, and then as you started cycling, it just started growing when the crack started, and then suddenly all you had was reinforcing there and then it just pushed it off.

DR. ZUDANS: I understand, but the shear alone would have the peaks at the end. It goes up from zero very high and then drops down. Further on you don't have any fear at all. Shear transfer works that way.

DR. SIESS: Would that be true at the bottom, Zenon?

DR. ZUDANS: It peaks at both surfaces the same way, top and bottom. There should be no difference. It is symmetric behavior.

DR. SIESS: And so applying it at the top it has got to go down through the wall to get to the bottom, I find it difficult to peak at the end. There is a load.

DR. ZUDANS: The wall would tend to distribute it uniformly. It would start out non-uniform at the top and then will become non-uniform at the bottom again.

DR. ENDEBROCK: I ran some computer type analysis on these looking for stresses, and in those particular cases the shear stress at the end has always dropped to zero and then built up to a maximum in the center.

DR. ZUDANS: But that would be like a regular beam bending theory that would apply.

DR. ENDEBROCK: That is right. I used the same dimensions as our test structure.

DR. SIESS: Horizontal shear you are talking about. The shear at the bottom of the wall at the load end, I cannot see as being very high. You cannot get down there.

DR. ENDEBROCK: This is a computer code printout or results from one of the computer code runs. The cracking at the particular cycle, when the first cracking starts shows cracks in these elements, and this is near. This is the base.

DR. SIESS: Wait a minute. Is this whole thing the wall now?

DR. ENDEBROCK: Yes, finite element diagram of the wall. This part is the bottom slab. This is the top and in here just the shear wall.

DR. SIESS: Group 1 and Group 2 are the slabs.

DR. ENDEBROCK: Right.

DR. SIESS: And Groups 3 and 4 are the walls.

DR. ENDEBROCK: Right. And the load, and these four cracked on one cycle and now the next cycle they are cracking in the next set.

DR. SIESS: What are the numbers on there?

DR. ENDEBROCK: These numbers are just note numbers.

DR. SIESS: Okay, just note numbers.

DR. ZUDANS: So it acts like a shear beam.

DR. SIESS: And those are cracking based on some principal tensile strength?

DR. ENDEBROCK: Yes.

DR. SIESS: Uni-axial?

DR. ENDEBROCK: I think it is uni-axial.

DR. SIESS: And the ones that say, "Time," on them are cracking, right?

DR. ENDEBROCK: Time 1 is first cracks that occur at one particular cycle, and then Time 2 is the next cycle.

DR. SIESS: Now, does the calculation give you the direction of the principal tensile stress.

DR. ENDEBROCK: Yes.

DR. SIESS: Would it be vertical more or less at Time 1?

DR. ENDEBROCK: No.

DR. ANDERSON: Charles Anderson, Los Alamos. We just did the calculations the other day, and I looked at the directions, and the crack plane is not vertical.

DR. SIESS: No, I would expect it to be more nearly horizontal.

DR. ANDERSON: Okay, a plane is horizontal, right.

DR. SIESS: The cracked plane?

DR. ANDERSON: Is not horizontal.

DR. SIESS: How much incline to the horizontal is it?

DR. ANDERSON: I looked at direction. Cosines were .5 and .9.

DR. SIESS: You have got the vertical tension in there due to the overturning, and then you would have the shear which by itself would give you 45 degree tension.

DR. ANDERSON: It wasn't like I thought it was going to be, and we really have not analyzed it.

DR. SIESS: Where is the substantial shear component in there?

DR. ANDERSON: We have not analyzed the data that thoroughly.

DR. ENDEBROCK: This is all I have-

DR. SIESS: That is a pretty coarse net mesh you have. There is a slide in here. Is that a picture of the thing?

DR. ENDEBROCK: Yes.

DR. SIESS: That looks pretty typical, doesn't it? You can hardly see through it. That is almost too small a model, but I assume your equipment dictates it.

Okay, the next item then will be buckling, right?

DR. BENNETT: I am Joel Bennett from Los Alamos National Laboratory, and I will reshow the slide that Chuck showed just to emphasize the dimensions of the containment shells that we are talking about in the buckling study. They have a radius to thickness ratio in the free-standing shells of around 460. This is typical.

It does make them a thin shell and subject to buckling. NRR requested us specifically to look at the area replacement method and what I am going to present to you first is a summary of the area replacement method, ASME area replacement method buckling investigation we carried out.

I will indicate why they specifically asked with the next slide. It was a premise put forth by C. D. Miller at CBI, and it was based on a mylar test of a single cylinder, and he indicated in his data that if the ASME area replacement method is followed then the buckling strength of a penetrated cylinder with a circular penetration will be increased above the value of the unpenetrated cylinder, and that is what initiated the request for us to look into this.

We first established -- bear in mind that the cylinders I am talking about are cylindrical sections with a radius to thickness ratio of 460 are commonly known in the industry, I think,as fabricated shells. There is no effort made to control imperfections such as in a laboratory test other than what would be considered normal engineering tolerance practice. So, we first took a look at some fabricated cylinders, and this does not show up too well, I am afraid.

DR. SIESS: What is the opposite of a fabricated cylinder?

DR. BENNETT: A laboratory cylinder where imperfections are controlled very closely.

They are not representative of field structures. This does not show up too well, I am afraid. So, I brought along a photograph of the same thing.

We looked at fabricated cylinders that are fabricated to normal shock tolerances. We looked at them without penetrations, first to establish what is commonly known as the knockdown factor. We compared this to the ASME recommended knockdown factor for tests of this sort. That is this is the ratio of the buckling load to the classical buckling load that you could compute a cylinder that was perfect, had no imperfections and the ASME curve, this value, and the R/T ratio of 460. We did three tests. The average fell around 25 percent of the classical buckling load, very close to the ASME curve.

DR. SIESS: This is no penetration?

DR. BENNETT: No penetration.

We next began our program to take a look at what happens if --

DR. SIESS: Excuse me a minute. Whose points are those solid points out there?

DR. BENNETT: Those are points taken out of the recommen- -- I don't know. They come with the code case in 284. They are on the --

DR. SIESS: They presumably are test points?

DR. BENNETT: Yes, those are test points.

DR. SIESS: And the curve is the --

DR. BENNETT: They are reference test points in the ASME code case 284, I think.

DR. SIESS: And the curve is what, the code curve?

DR. BENNETT: Yes.

We prepared for the shop some drawings like this and asked them to construct the best sort of cylinder they could. The purpose of this is to show you the size of these cylinders. They are about 18 by 9. Our idea was to test them under axial loads, applying the load at the top through a platen and strain gage them around the circumference and at the top.

DR. SIESS: These all have holes in them?

DR. BENNETT: The series I am talking about all have holes in them, and we varied the percentage reinforcing according to the ASME recommendations.

DR. SIESS: I am just trying to get it straight. The ones you showed us on the plot --

DR. BENNETT: Those did not have any holes. That established the knockdown factor for a fabricated cylinder of this type.

DR. SIESS: And these are the same kinds of cylinders fabricated by the same people. The only difference is they have a hole?

DR. BENNETT: They are supposed to be exactly the same cylinders, fabricated by the same people. The only difference is they have the hole.

DR. SIESS: Did you make any measurements to determine the profile?

DR. BENNETT: Yes.

For example, we took measurements of all these cylinders. This shows how the relative height varied after you put those plates on them and to some extent you can relate these types of measurements to how non-uniform your load is when you load it the way we did through a platen.

DR. SIESS: I am sorry, what is relative height in millimeters?

DR. BENNETT: That is a measure, if you pick a base point at zero degrees and you measure the variation in the height as you go around the cylinder from that point. That is what the relative height would be.

DR. SIESS: Is that the parameter that really governs this, height, how plane the top surface is or is it the outer roundness?

DR. BENNETT: Governs which?

DR. SIESS: You said the height of a cylinder is --

this is a cylinder, not quite, but just imagine it is. Is this height? It is to me.

DR. BENNETT: Yes, that is the relative -- if you took the two, the difference --

DR. SIESS: That is a measure of how out of plane the top --

DR. BENNETT: How out of plane the top surface is, right.

DR. SIESS: Now, if I load an cut-of-plane surface I am obviously going to get some very non-uniform --

DR. BENNETT: That is right, and this to some extent is a measure of how non-uniform your load will be.

DR. SIESS: Is the reduction in buckling load of a fabricated cylinder due to that effect or due to out of roundness?

DR. BENNETT: I think I can address that, but I don't think I can answer it.

We took some, what you would like to call out of roundness measurements. For instance, here is a profile of the cylinder. We, also, using these profiles, could reconstruct the cylinder, if we liked.

DR. SIESS: I assume this is exaggerated?

(Laughter.)

DR. BENNETT: This is somewhat exaggerated, but it is very representative of the imperfections you would see in a fabricated cylinder shell. As a matter of fact, the way they normally recommend doing this is with core gage measurements in large cylindrical shells.

DR. SIESS: This thing has a diameter of what?

DR. BENNETT: I believe it is 18 inches and, Dick, correct me if I am wrong, 20/1000.

DR. SIESS: Each five spaces is 1/100.

DR. BENNETT: Right. The weld area is shown here, and of course, in the ASME rules that is exempt actually from coming within a certain tolerance.

DR. SIESS: What is marked as weld is just one side.

DR. BENNETT: That is a seam weld.

DR. SIESS: That is the hole in there, right?

DR. BENNETT: I believe on this particular cylinder that was the seam. This is probably an example from one that did not have a hole.

DR. SIESS: I see what you mean.

DR. ZUDANS: What I could not understand on zero degrees you have a jump on line, what did that mean?

DR. BENNETT: Yes, this is so if you wish to you could reconstruct the vertical profile. So, we go up the cylinder from a known point and then around the cylinder, and then we come back to that point and we go up again, and this is actually an offset from a given location. Using all these curves you could actually reconstruct the cylinder if you wished.

This involves quite a lot of data. I should probably emphasize though that those imperfections that are measured there fall well within the ASME code limits. The one thing about these cylinders that would not, of course, is their thinness.

Here, also, is an example of which I have a picture for if you would like.

DR. SIESS: I am having trouble going from inches to limeters. Give me just a minute to get caught up. The wall is .508 millimeters, and how many inches in a millimeter?

DR. BENNETT: 20 mils.

DR. ZUDANS: The other way around.

DR. SIESS: So the 2 mils you have got on this scale is 1/10 millimeter, and the amplitude of this, double amplitude of these waves around here look like they are about 1/100 inch.

DR. RICHARD DOVE: The wall thickness is 20 mils.

DR. SIESS: Wall thickness is 20 mils. Okay, now, I have got some scale. That is interesting. About two wall thicknesses.

DR. BENNETT: That is right. If that helps it is about two wall thicknesses peak to peak, I believe.

I passed around a couple of pictures that show essentially what I have shown here. This is a cylinder beam tested with a hole in it. This particular one has 25 percent of the required ASME reinforcement that you would place in a pad sort of situation. This particular cylinder is the one that I passed around.

DR. SIESS: You are out of plane on the top, and then you just load it through a platen?

DR. BENNETT: Yes.

DR. SIESS: Do you have any strain measurements around that would indicate how uniform the load might be?

DR. BENNETT: Yes, I will try to address that. We learned a number of things from that, about how not to do buckling tests.

Typical strain gage measurement taken around the top looked something like this. Of course, buckling is clearly indicated on this gage here. We, also, observed it visually, and we can observe it in another set of records which I will show you next.

For example, this figure shows strain gage readings taken on either side of the cutout. In this particular case the buckling did occur at the cutouts, popping in both on the left and the right side almost simultaneously, and the records reflected that.

I might point out that we continued to load after first buckling to look at the post-buckling behavior of these cylinders, and they exhibited quite a lot of reserve strength.

The next plot I will show you shows you our results based on a number of different cylinders, and it, also, shows you the results that initiated this study based on a single mylar cylinder. The advantage of mylar is that you can buckle it once without a hole in it and determine what we had to determine with our average of three tests, that is the knockdown factor, but more than that you can determine that for that cylinder. Then you can begi. to cut your hole and buckle it as many times as you wish and reinforce your hole as you wish.

So, for this particular cylinder that C. D. Miller did which was one cylinder, the data is very good. For our test there is a lot of scatter. We spent some time now trying to examine why you would get such scatter.

I might point out that there is a test that has 100 percent reinforcing, the required reinforcement that the area replacement method requires and that the ratio of the buckling load to the buckling load of a cylinder without a hole in it is well less than 1. It is .8. Does everyone understand the ordinate and abscissa on this curve? This is the ratio of the buckling load of the cylinders with holes in them to the ratio of the average buckling load of the cylinders that we tested that had no holes, versus the percentage of area replaced over the area removed required by the ASME area replacement method.

If you replaced 100 percent of the area it would be along this line and this point here would mean that you had brought the cylinder back to its original strength.

DR. ZUDANS: What is the meaning then of the points on the ordinate axis, that they are not at one? These are three cylinders of which you got the average equal to one.

DR. BENNETT: Right.

DR. ZUDANS: So, therefore, .13 I am just wondering which cylinder does it belong to, the one on the axis on the bottom, on the axis on the top? You see now I did not perceive this thing. Now, what you are saying here that zero reinforcement, no holes -- oh, wait a minute, all these points are for holes?

DR. BENNETT: Yes.

DR. ZUDANS: Okay.

DR. SIESS: There are three there at zero. Oh, that is for percent reinforcement. All of these have holes?

DR. BENNETT: All of these points have holes, right. Notice that we cut a hole in one, and it would not weld above the average of the three without holes. We, also, cut some that did not go quite that high.

DR. SIESS: Now, the round points are your tests?

DR. BENNETT: We did two series of tests, and the round points are our tests.

DR. SIESS: And the triangles are whose?

DR. BENNETT: That is a test run by C. B. and I on one single cylinder. What he did is he would strip off the reinforcement and increase it, strip it off and increase. You can buckle --

DR. SIESS: Start down here at one and backed off.

DR. BENNETT: Right. You can buckle mylar over and over again. That is the nice advantage you have with mylar with very little degradation in the material.

DR. SIESS: You have got some points that are well above his curve and --

DR. BENNETT: There is a great deal of scatter.

DR. SIESS: Yes, I agree to that, and as far as 7 and 13 goes, those were nearly identical, intended to be identical specimens.

DR. BENNETT: That is right.

DR. ZUDANS: Seven was only 33 percent reinforced.

DR. SIESS: No, 7 was 100 percent. You are looking at the wrong figures.

DR. ZUDANS: I am looking at test number, not cylinder number.

DR. BENNETT: Cylinder 7 and 13 differed only very slightly in the percent reinforcing. One was 101, is that right, Professor, and one was 103, I believe, percent? They were meant to be identical,but our measurement showed they were not.

DR. SIESS: Is the variation or variability explainable or --

DR. BENNETT: That is what we addressed ourselves to, and I think I can explain some of it for you.

DR. SIESS: Because you have got a few of them now that lie right smack on these curves.

DR. BENNETT: That is right.

DR. SIESS: He was just lucky, wasn't he?

DR. BENNETT: When we saw those we felt very good about them.

DR. SIESS: Maybe mylar just fabricates better than stainless steel.

DR. BENNETT: That is possible.

DR. SIESS: Ever consider it for containments?

DR. BENNETT: It does have some advantages as a research material. Let me try to now address your question as to why the wide scatter. We think we know why you have such wide scatter. The nature of a fabricated shell is such that you do get a knockdown factor for a fabricated shell. This shows a fellow by the name of Starnes who did a series of tests where he plotted the buckling load now over the classical buckling load, so we are looking at a little bit different thing, versus a hole parameter size. We call it R bar. It is R over the square root of RT and arises naturally.

DR. SIESS: And that is classical for no holes.

DR. BENNETT: Right.

DR. SIESS: That is like your previous curve except that this has got a hole.

DR. BENNETT: That is right. This is representative of the so-called "knockdown" factors.

DR. SIESS: With a hole.

DR. BENNETT: Right.

As you begin to add the hole, obviously it starts to govern. However, with fabricated shells you will notice back here for cylinders with no hole you have the same order of effect as you do in adding a hole. Conceivably then if you add a cylinder, and we can prove this, I think, if you will let us take a mylar cylinder, and you buckle it, and you find where it buckles and go around and cut a hole, the hole will have no effect.

On the other hand, if I cut out the bad spot where it buckled, it is likely to have a very large effect. It may even raise the buckling load.

Furthermore, if you were to reinforce around that hole, it is liable to stiffen in that area and raise the buckling load even more, but that would have very little to do with the area replacement model.

DR. SIESS: I would think it would not make any difference. If it buckled over on this side, and I cut the hole over there, and the next time it buckled on this side --

DR. BENNETT: It has --

DR. SIESS: Reinforcing that hole it still ought to buckle over here at the same place.

DR. BENNETT: That is exactly right. That is our conclusion number two which I will present shortly.

DR. ZUDANS: It probably relates somehow to the way the load is applied at the end of the cylinder, because if it is a weaker spot you apply less load, and you go to the stronger side.

DR. SIESS: Now, the knockdown load simply comes about from variations from ideal shape, right?

DR. ZUDANS: That is a correct statement. It is the difference between real structure and ideally computed structure.

DR. SIESS: It has such a dominant effect that --

DR. ZUDANS: Not on everything but on axial cylinders.

DR. SIESS: And these differences are about the same order of magnitude whether there is a hole or not, but relatively they get fairly large.

DR. ZUDANS: That is right.

DR. BENNETT: We did an investigation on how non-uniform our loading was and what that effect is.

DR. ZUDANS: Could you return back to the other slide?

DR. BENNETT: Yes.

DR. ZUDANS: Those solid curves on the other slide, they are really derived from machined cylinders, right? There are no imperfections on them?

DR. BENNETT: No, I believe those curves are the bounds of 16 tests that were done on the mylar cylinders but they were very, very good mylar cylinders. They were made as perfect as they could make them. These were what we call laboratory cylinders.

DR. SIESS: What are the ASME knockdown figures?

DR. BENNETT: I believe if you go back to Slide No. 3 or so, you will --

DR. SIESS: It is a little hard to tell. What was the R/T for those?

DR. BENNETT: 460.

DR. SIESS: These were 460, too?

DR. BENNETT: Yes.

DR. SIESS: And ASME at 460.

DR. BENNETT: About 25 percent.

DR. SIESS: Twenty-five percent, and here you are up to 60 to 80 percent with no holes.

DR. BENNETT: You mean these tests, yes.

DR. SIESS: The mylar shells.

DR. BENNETT: They are laboratory specimens.

DR. ZUDANS: And it is, also, like that some of the effect on the reduction on this low end where there are no holes is due to the boundary conditions, how well they are represented to the solution that is represented by ideal buckling load. The P sub CL assumes free cylinder and it is free to expand, just axial load applied, and your testing cannot be performed that way. You clamp the ends in some cushioning material, and I am sure the other fellow did the same thing. So, if you would put those conditions in, you would probably be in a 1 on the upper end there.

DR. BENNETT: Yes, but I think that would defeat the purpose of this study.

DR. ZUDANS: Not really, because what would the poor fellow who designs a plant like that do? He will model this structure as it is. He does not have this artificial boundary condition. He can certainly correctly represent a boundary. Now, he computes the what he calls classical load by bifurcation. In fact, he already included some of the so-called "ASME" buckling knockdown factor already because his boundary conditions are real and that knockdown factor is based on non-real and boundary conditions. Here is the same thing. So, my feeling would be a lot better idea to compute the classical buckling load on the ideal structure as designed with correct boundary conditions and then apply a knockdown factor of two eight that is based on experimental results such as these.

So, I would rather see this left hand where there is no opening to be at 100 percent. Did I get myself across properly? You see, there is another knockdown factor that you are really not -- a portion of that factor is eaten up someplace else. The nomenclature becomes incorrect. You tested the cylinders, and so did they where the ends were not free to move radially, at least not fully free to move. They may have been able to rotate. You compared at load to classical load which is computed with ends completely free.

DR. BENNETT: That is the normal way that these things are presented.

DR. ZUDANS: But you see what it does is you are applying knockdown factor of 10 is good for some things but things that you tested and not good for the others because in others where you consider actual boundary conditions, and you consider the hole and the stiffeners and the stringers you already took care of some of that reduction, and therefore it is unfair to ask the user to apply that same .2 factor to it. In other words, if you continue to test such a structure, you would find high loads.

DR. SIESS: At what L/D do you get rid of that end effect?

DR. ZUDANS: Not at the one they have.

DR. SIESS: No.

DR. BENNETT: Do you know that, Dick? I think about 10.

DR. SIESS: As high as that? I would think about 3.

DR. ZUDANS: The buckling pattern starts at the boundary, maybe sometimes around the hole, right? I don't know. So, it is there.

DR. SIESS: What if your knockdown is due to let us say geometric imperfections in the radius?

DR. BENNETT: Some of it is.

DR. SIESS: Now, do you think the effect of the hole is of an entirely different kind than the effect of those geometric imperfections or is it just an aggravation or amplification of those?

DR. BENNETT: I don't know. I would imagine there is some -- if I had to guess, knowing how complex buckling is, I would say there was some interaction.

DR. SIESS: Buckling is not all that complex. It is just our calculations of it that are.

DR. ZUDANS: You can see the effect very clearly. If you keep on enlarging the hole, then you have a significant amount of bending in addition.

What it will do is produce more compressor strength around the hole in that case. In this case it did not. Your strains were smaller in this case at the edge, and in fact they reversed the signs after you buckled. It distressed the stress pattern completely, and also, the imperfections do the same thing.

DR. SIESS: It, also, changes the geometry of how it deflects.

DR. ZUDANS: I am not sure that I got across to you what I was saying.

DR. BENNETT: I don't think you did, but I --

DR. SIESS: Why don't you try putting it in writing for him.

DR. ZUDANS: No. I will try to say it again.

The linear theory of elasticity is able to compute bifurcation for any configuration you wish to take. So, let us go in steps. I took a perfect cylinder, and if I took an infinite cylinder I compute the formula that you used for your classical buckling. I could take a shorter cylinder and in fact compress it again by moving two planes together, but I could restrict the ends from moving, like you tested. That will result in another buckling load. It is still a correct classical buckling load.

DR. BENNETT: It would be higher than the other one?

DR. ZUDANS: It would be higher than the other one. Now, I could, also, introduce a hole in it and model it with a program like stacks or something else and again compute a classical buckling load. They are all legitimate classical buckling loads. Now, if you would take the same things that I analyzed and subject to test and precisely with the boundary conditions as I analyzed you would find that the buckling loads are less. The ratio between the two would be the knockdown factor, the legitimate knockdown factor. Now, if you define your knockdown factor as a reference to perfect infinite cylinder all the time, then you have to be careful how you analyze your system, because you will punish yourself. You see, you apply --

DR. BENNETT: I do understand what you are saying, and I think what you are getting at is it would be better to present this data as a knockdown factor on a cylinder that had the boundary conditions.

DR. ZUDANS: Correct.

DR. BENNETT: Unfortunately, well, maybe it is not unfortunate, it is an established industry practice for years and years to always present it relative to the classical load, and I think the reason for that is --

DR. ZUDANS: This isn't a classical load.

DR. BENNETT: No, I am sorry, the classical load for an infinite cylinder. The reason for that, I think, is that the experimentalist can never assure himself quite that he has the boundary condition that he is looking for, whereas the analyst can.

DR. ZUDANS: You see, the analyst can turn around and do the analysis on the experiment.

DR. BENNETT: That is true.

DR. ZUDANS: The analyst is more capable of doing that.

DR. BENNETT: I won't argue with that.

DR. ZUDANS: I have a point behind that. My point behind that is that you might find out, if you did go that way that there is not that great a variation in knockdown factors.

DR. BENNETT: I think there are. There have been some data presented that way, but this is the more normal way to present.

DR. ZUDANS: That is fine, anyway.

DR. SIESS: I guess one thing that bothers me is that with your test you are introducing a boundary condition that at least theoretically raises the buckling load and then a hole that --

DR. ZUDANS: I am sorry. It lowers, not raises. I made a mistake.

DR. SIESS: You mean the restrain band is the lower buckling?

DR. ZUDANS: Yes, because it adds the bending and does not allow it to --

DR. SIESS: Okay, I am sorry.

DR. ZUDANS: I misstated it. It lowers.

DR. BENNETT: We tried to analyze these tests a little further, and with the data that we had from our strain gages, and we did this by taking a look at the strain gages relative to uniform state of strain that you could compute should exist and I plotted here, for example, the gage above the hole. It is epsilon at zero degrees divided by this strain state that you should see, a uniform strain state at buckling. So this is measure of on the ordinate how non-uniform the loading was, and you can see that the non-uniformity of loading, for example, if this thing were one, you would have a uniform load. You can see that the non-uniformity of loading has a very large effect. In cases back in here where that ratio is a little higher, the buckling load is lower.

In short, you are loading the hole. In cases out in here where the ratio is near zero, you are loading away from the hole. So, the non-uniform loading in these tests has a great deal of effect.

DR. SIESS: Does it follow that when these strains are high, the strains are 180 degrees away are low and vice versa?

DR. BENNETT: In general that trend was, also, shown, but not totally, and that is because of this non-uniformity of the planes that we were loading. I think itis better to say that that was the general trend, but if you looked at all gages that was not an absolute.

DR. SIESS: The local effect as well.

DR. ZUDANS: That end block that you used was some relatively soft material, but these are strain gage readings. The end blocks were not rigid.

DR. BENNETT: That is right. We, also, did some analysis on this program, and before we did the analysis we took a look at computer codes that can do buckling, and we think the best ones around; this is an old slide that I showed to the peer review panel, the best ones around are probably the Lockheed codes for doing buckling calculations. I think the current version is called STAGS-C1. It is the Lockheed code. It has bifurcation buckling capability. It has non-linear collapse capability and BOSOR-4 and 5. The differences are that BOSOR-4 can do axisymmetric geometries with non-axisymmetric loadings and yet it can only do elastic buckling. BOSOR-5 can do elastic/plastic buckling but it cannot do the non-axisymmetric loading.

You had a question?

DR. ZUDANS: However, BOSOR-4 can only use the axisymmetric prebuckling load. It can compute non-axisymmetric distribution, and then it turns around and takes the worst meridian and worst circumferential thing. So, it is an approximation, and it is generally believed to be conservative. That is something that when you look at it you might find out how your tests compare. I suggested to NRC a long time ago that a code to be developed should be such that it is able to take asymmetric prebuckling load, but that would be similar to what I just said before with respect to boundary conditions. In other words, you compute part of your knockdown factor. Therefore what you should use is another multiplier or something less than that. I have no other comments. I have great respect for these codes otherwise.

DR. BENNETT: I have some respect for them except that we were unable to obtain STAGS-C1. It is supposed to be released through Cosmic, however.

DR. ZUDANS. But there are other STAGS versions.

DR. BENNETT: Yes, we had a version of STAGS but not the latest version of STAGS-C-1.

DR. ZUDANS: You ought to get Lockheed to give it to you or else it won't work anyway.

DR. BENNETT: That is right.

DR. SIESS: One of those, huh?

DR. BENNETT: We have BOSOR-4 and BOSOR-5 available at the laboratory. We did not use them on these problems. Obviously with a hole in the cylinder you cannot do too much with the axisymmetric --

DR. ZUDANS: And BOSOR-4 and 5 have other difficulties, too. It is limited in boundary conditions. It is limited in harmonics and whatnot.

There is another code.

DR. BENNETT: We have another code which we use in buckling. For bifurcation buckling it is the SPAR code, and I show you here the mesh that we developed to analyze the cylinders. SPAR means --

DR. SIESS: That stuff looks like it would be useful for those shear walls.

DR. BENNETT: It is a very fine mesh and you have to do this. This shows the three-dimensional shell code. I guess this shows the upper half or lower half depending on where you are standing of one-quarter of a cylinder.

DR. ZUDANS: What kind of boundary condition do you apply to this?

DR. BENNETT: This one was the free boundary condition.

DR. ZUDANS: Free, and along the lines that intersect the hole you took symmetry conditions?

DR. BENNETT: Yes.

The code that we used can only do the bifurcation buckling analysis. Here is the buckling mode, one of the buckling modes that was predicted by SPAR just as an example, and it does buckle around the hole. With this code, even though we can rotate these graphics output through spaces, it is very difficult to find a good representative picture of the buckling load. There are some better ones in the report. Buckling mode, I am sorry.

We investigated a number of things with this mesh.

One of the things that we investigated is we went back and we looked at our data, and we tried to characterize the imperfections in terms of a sinusoidal bearing radius and also a lean of the cylinder, and we tried to put this in the code, and we did two calculations on what we called types 1 and 2 imperfections.

In one set of cylinders the imperfections seemed to be the characteristic wavelength of about 280 degrees, whereas in another set they seemed to be at a characteristic wavelength of about 5. The amplitudes, I believe were about the same. They were like two wall thicknesses variation.

We, also, investigated with this mesh the non-uniformity of loading. We defined types 1 and 2 loading. Some of our data indicated that type 1 loading where the hole is overloaded we could have had as much as 26 percent difference between the load over the holes and the load on the backside which would be at the seam.

We, also, found data that indicated that we could have had the reverse case, as much as only 55 percent of the load over the hole as opposed to 145 percent over the -- not the hole, very idealistic cases, but nonetheless something that we can investigate.

DR. SIESS: Idealized. There is nothing idealistic about them, pretty realistic, I think.

DR. BENNETT: I am sorry. Nonetheless, there was some indication from the data that it could have been this bad in two cases. This shows a summary then of some of the analytical studies that we did. First we took a look at a perfect cylinder with a uniform load to see how well our code could predict the classical buckling load, and this is with a free boundary condition and we came within 97 percent of that.

We felt that that mesh size then was adequate to investigate these other cases.

DR. ZUDANS: The three cylinders that you tested and you took the average, okay, they were low anyway because they were imperfect, but if you could apply those. I take it back.

DR. SIESS: This is an imperfect theory on a perfect cylinder.

DR. ZUDANS: This is pretty perfect the way it looks.

DR. SIESS: Three percent is close.

DR. BENNETT: Here is the cylinder with the hole, no imperfection. It knocks down the classical load with R bar 3-1/2 which was about the size of the hole we were cutting, remembering that R bar is the ratio of the hole radius to the square root of RT of the cylinder. It knocked it down to 15 percent of the classical, whereas the type 1 and type 2 imperfections actually raised the buckling load a little bit. It is almost like you are corrugating the cylinder as you go --

DR. SIESS: Right. Where did you put the imperfection in relation to the hole?

DR. BENNETT: The imperfection that we modeled was the type that we measured. It was a sinusoidal varying radius with a lean.

DR. SIESS: Yes, but where was the hole in relation to the sine peaks?

DR. BENNETT The lean, actually we ran both cases, toward the hole and away from the hole. It did not seem to matter.

DR. SIESS: And it did not make any difference on type 1 and type 2 imperfections?

DR. BENNETT: There was some difference. The ratio, however, is about 16 percent.

DR. SIESS: What?

DR. BENNETT: The ratio of that load to the classical --

DR. SIESS: No, I said the type 1 and type 2 imperfections were essentially the same.

DR. BENNETT: Essentially.

DR. SIESS: Both of them 16 percent.

DR. BENNETT: The difference is in the roundoff at 16 percent, right. There was some difference.

DR. ZUDANS: And the non-uniform end load really represents the case where you applied total axial load the same as before, plus some bending.

DR. BENNETT: Yes.

DR. ZUDANS: So you would expect much higher marginal stress and you expect reduction.

DR. BENNETT: A much larger effect which is what we show in the next two.

I think that may be representative of tests where you try to do this.

We, also, ran the case --

DR. SIESS: Let me go back to the third and fourth lines a minute where you varied the imperfections. Without any imperfections and with the hole it was 15 percent. Adding the type 1 imperfections actually increased it, unless they did change it.

DR. ZUDANS: You corrugated the cylinders.

DR. SIESS: And then going to type 2 it did not change it.

DR. BENNETT: It changed slightly between type 1 and type 2. This is the five cycle, and it is a little more of a corrugation.

DR. SIESS: So you think the corrugation effect accounts for that increase? It is pretty small.

DR. BENNETT: That is kind of my opinion, but I would hesitate to make a blanket statement like that without investigating.

DR. ZUDANS: The experimental buckling pattern, did you observe -- I think there was a picture, the diamond pattern that you created at the end. Was that in any way resembling the tendency on imperfections to go by those same periodicity or not?

DR. BENNETT: No, it was not. You could find cases where you could make that correlation, but you could find cases where you could not.

DR. ZUDANS: There generally is a belief that if your imperfections are in the mode shape of your first buckling mode you get the greatest knockdown factor or at least the greatest reduction of buckling stress.

DR. BENNETT: That was why we were interested in running these two cases. We felt like that maybe there might be something to that. The five cycle is much closer to the number of waves that you would get in a perfect cylinder; pot cycles at 180. Actually I think it is 12 for a whole cylinder.

DR. SIESS: Your last case you ran with no imperfections.

DR. BENNETT: Yes.

DR. SIESS: What do you think would happen now if you had imperfections?

DR. BENNETT: I think that would be knocked down some. We can run that case. It is just that we had already drawn the conclusions when we ran this.

DR. ZUDANS: This here --

DR. SIESS: The imperfections did not have any significant effect when you had an unreinforced hole, and it would be interesting to know whether they still had no effect with the reinforced hole.

DR. BENNETT: We can certainly do that analysis.

DR. SIESS: Or if you think they really strengthened it some because of the corrugations, would they strengthen it more than 1 percent? You see, I don't know what imperfections would do on the cylinder without a hole which is what you are trying to make it look like.

DR. BENNETT: We can, also, do that analysis.

DR. SIESS: You may have a feel for it without doing it.

DR. BENNETT: I have a feeling that the imperfections would not knock down the classical load as much as putting the hole in it from this analysis.

DR. ZUDANS: That would be very interesting to see because you see, when you reinforce the same hole and raise the buckling load by almost all the way to the non- -- almost to the perfect cylinder --

DR. BENNETT: Well, 74 percent.

DR. ZUDANS: That is right. That is the analysis.

DR. SIESS: But imperfections of the kind you put in can actually be strengthening you see with corrugations. I assume if you put in deep corrugations you wuld get strengthening, wouldn't you?

DR. BENNETT: You might very well, but the strengthening effect probably you would never measure experimentally, I am sure. The fact is that as you can see it is very small.

DR. SIESS: Are these the kinds of imperfections you would expect to get in a containment shell?

DR. BENNETT: Yes, they are representative.

DR. SIESS: They don't have any that go this way?

DR. BENNETT: Oh, I am sure they do.

DR. SIESS: Wouldn't those be more of a problem for buckling? I don't see how you can do much worse than this, but --

DR. BENNETT: Dick, do you want to address that?

DR. SIESS: I have never seen any measurements on a containment except out of round. I don't know whether anybody has ever measured.

DR. BENNETT: We did measure that, and we could reconstruct the cylinder.

DR. SIESS: I said in a containment, not the thing you built in the lab. You see, they are built a lot differently.

They have got concrete on the backside and individual plates may have little bulges in them and welding around penetrations and just the fabrication of them. Of course, they are pretty big, too, but they are out of round by how much? Does anybody remember?

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DR. BENNETT: The cylinders that we have here are within the code, which calls for 1 percent of the diameter.

DR. SIESS: Yes, but 1 percent of the diameter -- but that was this way, you see, not around.

DR. BENNETT: Right, it was based on using core gauge measurements.

DR. ZUDANS: On this list of analysis, I think you are missing two analyses that would clarify that point that you want to make as a final conclusion here, one analysis with a cylinder without a hole with imperfections, which you do not seem to have there, right, without the hole with imperfections? And another analysis of 100 percent reinforced hole with imperfections.

The hole itself, with imperfections or no imperfections, did not seem to make any difference. So what it really tells me is that a hole was the major imperfection in this issue, right?

DR. BENNETT These two cases, that is right.

DR. ZUDANS: Now, it would also suggest that if you added imperfections to 100 percent reinforced hole, it may not reduce as much as I would like to see it reduced. I go back to 16 percent.

DR. SIESS: That is what I was asking for.

DR. ZUDANS: Yes.

DR. SIESS: Because if I look at the tables, to get to your conclusion, you essentially skip from line 2 to the bottom line.

DR. ZUDANS: Right.

DR. BENNETT: I agree with that, but our conclusion was based primarily on the tests.

DR. SIESS: And I really think that other case needs to be in there to keep somebody from questioning it.

DR. BENNETT: Right.

DR. SIESS: You might get a surprise, and it would be nicer to get at the analysis when you make tests.

DR. ZUDANS: Well, the point that you are raising with this is a very good one and certainly valuable. If that is the case, then we are in trouble, right? The general belief is that if you reinforce it, then you can write off the penetrations in terms of buckling.

DR. BENNETT: No, I believe the ASME has another code case that covers buckling. You have got to remember that the area replacement method is made to ensure that the stress in the shell or the strength of the shell is undiminished.

DR. ZUDANS: Correct.

DR. BENNETT: It was never meant to cover buckling.

DR. ZULANS: No.

DR. BENNETT: It was just suggested that it might be all right.

DR. ZUDANS: Well, that is what I said. I did not say ASME.

DR. SIESS: Well, it is the only game in town right now.

DR. ZUDANS: Pardon me?

DR. SIESS: It is the only game in town right now.

DR. ZUDANS: And I think that if you do that imperfection with the hole reinforced --

DR. SIESS: The thing is, your 74 percent does not look bad, because they are margins, I mean both. Now, if I am looking for something further down the line -- but that 74 percent is a heck of a lot higher than you've got back here.

DR. ZUDANS: Right.

DR. SIESS: Yes. Well, you would, I guess -- oh, no, I am sorry, that was a bobtail graph. The lowest you got was about 80 percent in your test, right?

DR. BENNETT: No. That is normaliz to a cylinder that has -- to a fabricated cylinder. It is normalized to the average of the first three tests that we made.

DR. SIESS: Okay, I am sorry.

DR. BENNETT: This is normalized to the classical buckling.

DR. SIESS: You got 80 percent of the cylinder without a hole.

DR. BENNETT: Right.

DR. ZUDANS: And if you reinforce --

DR. BENNETT: I came prepared. You are missing two cases.

DR. MARK: This data here relates to a hole that is scaled as in that first set of data you showed us, 460 for RT?

DR. SIESS: That R-bar thing.

DR. BENNETT: Yes. As a matter of fact, the whole size was picked to be representative of a whole size for a large equipment hatch in a containment shell.

DR. MARK: Okay.

DR. BENNETT: They typically have R-bars.

DR. MARK: That is the largest hole that was normally put in.

DR. BENNETT: Normally put in a containment.

DR. SIESS: While it is operating

DR. MARK: Now, are all holes circular?

DR. BENNETT: I think that --

DR. MARK: There are none of them oval-shaped, like submarine hatches?

DR. VON RIESEMANN: They are not all circular.

DR. BENNETT: They are not all circular? Walt says no.

DR. SIESS: What is not circular?

DR. VON RIESEMANN: MARK III equipment hatches.

DR. SIESS: Okay, MARK III's, I forgot. A dry containment's PWR's, I don't think I have ever seen anything but a circular hole.

DR. MARK: It would not really be surprising to see a big square one or an oval one, in particular.

DR. SIESS: Well, it is hard to make the doors. Round heads are easier to make than square ones, I think, and that is why they do it that way.

(Laughter.)

DR. BENNETT: Well, from the entire study, based on analyses and tests, and I should say that we did some subsequent tests on mylar cylinders to verify our steel tests, we drew two conclusions.

The first conclusion is that if the buckling strength of the cylindrical shell is lowered by penetration, then following the ASME ARM rule will raise the buckling strength of the shell, but it will not bring it back up to the unpenetrated value.

DR. ZUDANS: I think that maybe the statement is too strong, while you do have some basis. First of all, let's look at your -- you draw this conclusion, really, from cylinder 13, essentially.

DR. BENNETT: No, not just from one cylinder.

DR. ZUDANS: From which one else?

DR. SIESS: Well, you've got one test and one analysis.

DR. BENNETT: I can only point to one test and one analysis in this paper, but we have done some further testing.

DR. ZUDANS: But the analysis is incomplete. You are missing two cases to make that judgment, right? And the other thing is that, unfortunately, unlike with those guys that work with Mylar, you were not able to establish what is the initial strength of the cylinder that you would make a hole in. So you have a variation of these cylinders, the three ones that you tested without holes and you drew the average. Now, what was the range on those?

DR. BENNETT: Well, they are on the graph there. I think they are valued from 22 percent to 26 percent.

DR. ZUDANS: From 21, so that is like a 20 percent variation, right?

DR. BENNETT: Yes.

DR. ZUDANS: Now, if I picked your 13 case and assumed that came from something like a lower end of dose, then I would be at 100 percent anyway.

DR. BENNETT: Well, this conclusion was not drawn just on the basis of what you see here.

DR. SIESS: Yes, but there is a basic problem here, and that is that the phenomena you are dealing with are highly variable, and whether you are talking about the worst cases, which I think this is clearly true of, or some sort of a mean case really cannot be established. I do not think you have enough tests for a statistical, probabilistic conclusion, and absent that, I do not know what else you can do except say, under the worst conditions, it did not bring it back up.

DR. ZUDANS: Yes, that is right. The evidence is there, but it is not conclusive.

DR. SIESS: And your analysis needs a couple of more points to really check this out, because you have got a couple of cases where -- I cannot really tell from that figure whether your 6 and 7 come out stronger or not. It looks to me like they come out stronger, right? The figures are not numbered, so I cannot tell you which one it is, but it is the percent reinforcement versus ratio of P to PO.

DR. BENNETT: Cases 6 and 7 are definitely stronger, right.

DR. SIESS: Yes, and 13 is definitely lower, and the mean is about 1. Now, I guess, with a few more points, I might be able to conclude that on the average it brings it up, but there is a considerable variation, and there is some probability that it will be lower by as much as 20 percent or 30 percent or whatever.

DR. BENNETT: I would like to return to this slide which we discussed, and I point out again that the shells that do not have a hole in it have a knock-down factor that is well within the range of any shell out in this R-bar ratio when you put a hole in them.

DR. SIESS: Yes?

DR. BENNETT: And that, as a result -- let me give you an example. If you let me take a Mylar cylinder, which we have done, and buckle it -- it has no hole in it now, and I will buckle it -- if I cut out the buckled place and reinforce that place, I will raise that strength. But it has nothing to do with the area replacement method.

DR. SIESS: Yes.

DR. BENNETT: On the other hand, if I take that same cylinder and put a hole on the opposite side, and that hole is small enough to where it is not dominant, it will buckle the same place.

DR. SIESS: You are saying exactly what I said. They are random phenomena.

DR. BENNETT: Right.

DR. SIESS: In some cases, it may be higher --

DR. BENNETT: The reason is right here. These are fabricated shells, and the phenomena that we are talking about -- that is, reducing the buckling strength by introducing a penetration -- is well within the range of the buckling strength of the hole closing without penetration.

DR. SIESS: But the fact that the randomness can be explained does not reduce its randomness.

DR. BENNETT: That is right.

DR. SIESS: And when you get to designing one of these things or assessing the capacity of it, that randomness is still going to be in there.

Now, whether the staff is going to end up wanting to take the probabilistic approach to this or simply take a lower bound type thing, I do not know. I would think that considering the nature of the beast, you would be inclined to take a lower bound.

DR. ZUDANS: I would like to make one more comment. I think that you concluded, and that is what your test indicates, that adding a hole is nothing more than just adding another imperfection, and if your dominant imperfections in this fabricated cylinder were already large, then there is some minimum hole which will not affect the final result. I think that is what you concluded, and I think that is a very good conclusion.

DR. SIESS: Yes.

DR. ZUDANS: Now, I think that the reinforcing on the hole would only affect the result when the hole becomes dominant.

DR. SIESS: Yes, that is what he is saying.

DR. BENNETT: That is what I am saying.

DR. ZUDANS: So there is really hardly a way to make a general reference to either -- I guess it is maybe that statement then works out to be too strong, but the other conclusions that you make are excellent. They are really true.

DR. BENNETT: This statement is not in --

DR. SIESS: Well, you ought to get to your second conclusion.

DR. BENNETT: I am sorry. The first conclusion is not, as you know, in the report. However, maybe Chuck Anderson in his opening remarks said it better. Reinforcing the hole will certainly never hurt anything.

DR. ZUDANS: Well, there are other reasons why you have to reinforce it.

DR. BENNETT: Certainly.

DR. ZUDANS: You have to take the pressure load in.

DR. BENNETT: Right.

DR. SIESS: There are stresses to beat. You have to meet the code, anyway.

DR. ZUDANS: I think your tests are really great because they give some much better understanding.

DR. BENNETT: The second conclusion is essentially what we have been talking about.

DR. ZUDANS: Yes. It makes a lot of sense.

DR. SIESS: And I think you have thrown a lot of light on this.

DR. ZUDANS: Yes.

DR. SIESS: Gentlemen, let's take a short break, and then do you think we can finish this up in about 10 or 15 minutes?

DR. BENNETT: I can if there are not too many questions.

(Laughter.)

DR. SIESS: Oh, well. I haven't got a gavel, but I will muzzle this guy over here, and me, too. Let's take a break.

(Brief recess.)

DR. SIESS: We will resume.

DR. BENNETT: I am told I have 15 minutes, and I would like --

DR. SIESS: I would like to keep it to about that because --

DR. BENNETT: I would like to tell you about our next series of tests. They also are to accommodate NRR who has a contract with Lockheed, and they will be doing the analysis phase of this test or these tests to some extent.

This is to investigate a series of benchmark problems for the BOSOR 4, 5, and STAGS C1 codes. In the ones that we proposed for ring-stiffened cylinders -- I show this vu-graph just to show you how we sent out our initial proposal to industry, being Chicago Bridge and Iron, and to Lockheed. They commented on them and they sent back their results. They pointed out that these cylinders were over-reinforced, and they finally came back with their suggestion as to benchmark problems, which I will show on this slide.

Oh, I am sorry. I will skip down a few if you do not mind.

DR. SIESS: That is all right.

DR. BENNETT: And I will show it on this slide, which is actually a copy of a little shop drawing. You will notice that the ring stiffeners have been reduced somewhat in size from our initial proposal.

DR. SIESS: What is their prototype for that, one of the ice condensers?

DR. BENNETT: Yes, I believe so. CB&I is, I believe, the principal supplier of steel containments. We were told they have a very large percentage.

We plan on doing a little bit different type of test on this. We found this, as we showed to you in the last set of analyses or last set of experiments, that we had a lot of trouble with loading conditions, so we proposed to load this one a little bit differently. We are going to put a circle joint that can be moved to a given desired eccentricity-- this is to test the non-acting symmetric loading capability of a code like BOSOR 4--and load through a ball joint.

DR. SIESS: Now, is that head rigid? Or has that been taken into account, anyway?

DR. BENNETT: That head has been calculated to be rigid relative to the shell. An example of the types of pre-testing I have shown here. I have brought up -- these are mylar cylinders that we are pretesting. The cylinders that will be done in the benchmark tests will be steel. Obviously, we can do a lot of checking out of our loading scheme; we can do a lot of checking out of a number of things using mylar cylinders, and we can do it cheaply.

DR. SIESS: Now, on the benchmark problem, what are you giving them for imperfections? Just let them assume their imperfections?

DR. BENNETT: No. On the benchmark problem, we work with Lockheed to develop what he would like to see in terms of imperfections, and some of those are shown here. We are doing roundness profile sweeps at five points in between each of the six rings in the center section.

DR. SIESS: Are you going to supply the analysts with the actual profile?

DR. BENNETT: Yes.

DR. SIESS: Okay.

DR. BENNETT: That is what he wanted. We are doing axial profiles, also, and we are doing cord gauge measurements. The cord gauge measurements were requested by CB&I, and as you know, using cord gauge measurements at a discrete number of points around the shell, you can represent the imperfections with a 4A(?) series.

We have also changed our method of handling the boundary condition at the shell plate juncture. We are trying to plot this in into a slot that is made oversize to take care of any relative roughness that might be in the shell itself. Using the small mylar model that I have shown you on the table, you can do these kinds of things. You can check out your method for plotting them in. We did this sort of thing already.

DR. SIESS: Now, with the ring-stiffened shell, those vertical imperfections I was asking about earlier would tend to diminish in importance, wouldn't they?

DR. BENNETT: Yes.

DR. SIESS: That could probably disappear.

DR. BENNETT: The ring-stiffened cylinders will be tested first without a hole, and that is what we referred to, or without a penetration, as a baseline benchmark test.

(Laughter.)

That is not our terminology; that is Lockheed's. I think the reason they call it baseline benchmark test is they can model that with BOSOR 4 and 5.

DR. SIESS: And then the next one could be a standard baseline benchmark.

(Laughter.)

DR. BENNETT: The next series of tests will have holes in them, and basically, in these types of containments, we are told that there are four types of basic penetration. There is the penetration that interrupts no ring-stiffeners. There is a penetration that interrupts one, two, and three.

I have shown our scheme for proposing to interrupt these penetrations.

We next went to CB&I and we pointed out that we can reinforce these holes according to ASME criteria ourselves, which we did, but there are probably an infinite -- well, not infinite, but a number of ways of tying the framing, the rings by way of framing, into the reinforcing, and we asked for suggestions as to an industry standard on this, and they indeed sent them in.

In the next series of slides, which I will not show, I will skip down and show you a typical example. Here is one that they sent back of a hole that interrupts one ring stiffener in the framing detail that will be used to frame in the reinforcing. You will notice also that in this case they suggested we include a nozzle which is taken credit for in buckling design in doing these penetrations. That is what this configuration is here.

DR. ZUDANS: For holes this big, the area reinforcement does not work. They have to be detail-analyzed.

DR. BENNETT: That is right.

DR. ZUDANS: That is why those designs are different.

DR. BENNETT· That is right, and in general, what you like to do is ensure continuity of your rings in terms of bending it and area.

DR. ZUDANS: Right.

DR. MARK: They do handle the rings, their actual spacing and shape and dimensions, or they do not smear them out in the Lockheed code?

DR. SIESS: Oh, yes.

DR. BENNETT: In the STAGS Cl codes, you could actually model the structure. With the BOSOR -- well, you have no chance to model it just with BOSOR.

DR. MARKS: So it actually models the rings.

DR. ZUDANS: The BOSOR, I think, can handle discrete rings.

DR. BENNETT: Right, it can handle the discrete rings.

DR. ZUDANS. But it cannot handle the screed ribs, meridian stiffeners, which are not yet touched. That might be the next subject, right?

DR. BENNETT: No, the rib-stiffened cases, any time you see those, we are told by CB&I, they are put in as an after-design. It is much cheaper to design the shell to a thickness than put in rib stiffeners, and so we are not going to address those cases.

DR. SIESS: Now, are these details you are going to incorporate into the model?

DR. BENNETT: Yes.

DR. ZUDANS: They probably are not right for cases like MARK III, free-standing steel containment, because there are such significant non-symmetric loads that you need meridian stiffeners to take care of buckling, although now they pour the concrete on the outside, so it makes it more easy, too.

DR. SIESS: Are you going to make any tests that would compare, see whether it makes any difference what kind of stiffness you put around there?

DR. BENNETT: We are not.

DR. SIESS: And suppose some --

DR. BENNETT: We are going to stick with the industry standards, and if we are able to benchmark the codes, then supposedly he can do other calculations that would be --

DR. ZUDANS: And then the geometry --

DR. SIESS: How much of the detail on these holes can he model?

DR. BENNETT: He can model it in great detail if he has enough money and computer.

DR. SIESS: Would you put one of them on there, say, insert B, up on the screen a minute, or C, I don't care, or D, whichever one you come to first. Would you explain what all those lines are on there. Slide it over for the side view. I can understand that section.

DR. BENNETT: Oh, I am sorry. What insert B means?

DR. SIESS: Yes. Now, what are all those lines?

DR. ZUDANS: Those are the rings.

DR. BENNETT: Well, these are how you tie the reinforcing into the ring. Say you have interrupted --

DR. SIESS: Those are plates welded to the shell?

DR. BENNETT: Yes.

DR. SIESS: Where is the stiffening ring?

DR. BENNETT: The stiffening ring --

DR. SIESS: No, before there is a hole, those stiffening rings --

DR. ZUDANS: All those other lines are stiffening rings.

DR. BENNETT: Now, on your --

DR. SIESS: No, on your slide. I know where the stiffening ring is on the containment. I am just trying to figure what all those pieces are.

DR. BENNETT: This is the one that has been interrupted.

DR. SIESS: Okay. Now, what are the vertical lines either side of the hole?

DR. BENNETT: Those are the methods for tying the stiffening ring into the upper and lower ring.

DR. SIESS: Those are three -- oh, those are a whole series of -- all those horizontal lines are stiffening rings?

DR. BENNETT: Yes.

DR. ZUDANS: This is how they transfer the load.

DR. SIESS: Okay, and that is a vertical plate about the same size as the ring that is added in there?

DR. BENNETT: Yes.

DR. SIESS: And then two inclined plates that go out.

DR. BENNETT: Yes. This is their method for tying the -- to make the ring look like an interrupted fitting.

DR. SIESS: Then the plate is thickened within that other square?

DR. ZUDANS: Yes.

DR. SIESS: Yes. There is some pad reinforcing on the back and then a nozzle.

DR. SIESS: Okay. That is what I was just trying to understand.

DR. BENNETT: Okay. I do not know if you have any more questions about those tests, but those tests, the state of those is that we have produced our first baseline benchmark steel cylinder, and it is next going to metrology, our metrology lab, to have the imperfection measurements taken. And so we are in the process of doing those tests currently. We have done a lot of pretesting with our model.

DR. SIESS: How many people are going to run the analysis? Just Lockheed?

DR. BENNETT: Yes.

DR. SIESS: Now, that is interesting. How much confidence do you have that somebody else taking the same code will get the same answer?

DR. BENNETT: I don't think I can --

DR. ZUDANS: On the BOSOR, it is such a widespread use.

DR. SIESS: This is not BOSOR.

DR. ZUDANS: STAGS.

DR. SIESS: They are not going to use BOSOR, I said.

DR. ZUDANS: Without holes they will use BOSOR, right?

DR. SIESS: Well, the benchmark that I am interested in is with the holes. You are going to have one person make the analysis. That is the code developer, right?

DR. BENNETT: Well, I don't think -- Jim, that is sort of your question, because I do not know who is running the analysis other than Dave Bushnell and Elmo.

DR. SIESS: All you are supposed to do is provide the test data for the analysis? Okay.

DR. BENNETT: That is right. I put my answers in a sealed jar, mayonnaise jar, on Funk and Wagnall's front porch.

(Laughter.)

DR. ZUDANS: Now, is this the actual size that you will --

DR. BENNETT: No. This is essentially a half scale model of the cylinders we will be testing.

DR. ZUDANS: And the hole is to scale as shown now, the largest hole?

DR. BENNETT: Yes, the hole and the outlines that we show there in black are the ones that we will nibble out as we continue to test, and the ones that we will reinforce -- we will do a lot of pretesting yet with this mylar cylinder.

DR. ZUDANS: You are coming very close to the boundaries, to the ends.

DR. SIESS: For the benchmark model, how many tests will you make?

DR. BENNETT: However, STAGS Cl's, you would have no problem with that, if we supply them with proper boundary conditions.

DR. ZUDANS: No, of course not.

DR. SIESS: How many tests?

DR. BENNETT: This year we will do --

DR. SIESS: No, in the total program. All of these that you've got laid out here, all the different hole sizes.

DR. BENNETT: Well, there are two baseline benchmarks and there are four follow-on tests.

DR. SIESS: With different size holes?

DR. BENNETT: With different size holes.

DR. SIESS: With the standard reinforcement. And there will be analyses made on all cases?

DR. BENNETT: I do not know about that.

DR. SIESS: Jim?

MR. COSTELLO: I have not made any attempt to follow up recently on where things are in the Lockheed program.

DR. SIESS: Is that yours or is that NRR?

MR. COSTELLO: That is NRR.

DR. SIESS: We could -- we will probably be checking again pretty soon, but the understanding was this was the way we were going to put things together.

DR. SIESS: What is your feeling, anybody's feeling, about possible differences from different users with the same code? After all, they have to develop their model, and there are ways people can do it a little differently.

DR. BENNETT: Do you want my opinion?

DR. SIESS: Yes.

DR. BENNETT: Well, I happen to think that you have to be very skilled in using all of these codes. I do some analysis, and I know you can get quite a variety of different answers depending on your skill at doing the modelling.

DR. SIESS: Okay. So, really, what you do is bench-mark the code -- properly, it can do this or it cannot -- and then the question of quality control while using it is somebody else's problem, or another problem, at least.

DR. ZUDANS: I guess, from this point on, once NRC has generated this test, very closely controlled test, anybody can get the result, can test his own program against it.

DR. SIESS: Oh, yes.

DR. ZUDANS: That is the whole objective.

DR. SIESS: If there are any other programs.

DR. MARK: Will these be blind analyses?

DR. SIESS: Yes.

DR. MARK: Conducted before you obtain or disclose the measurements you actually make?

DR. BENNETT: Yes. Some of the analyses have already been run without the imperfection data, I am told. That does not mean they will not be rerun with the imperfection data.

DR. SIESS: He did not mean the imperfection data.

DR. MARK: It is fair enough to put the imperfection data in and anything else, but you can prescribe your experiment and carry it out in the way you prescribe and so they can make the calculations without knowing what you are going to get from the experiment.

DR. SIESS: Okay, that concludes your presentation?

DR. BENNETT: Well, there are a couple more, but I think I will conclude at that point.

DR. SIESS: Okay. Well, thank you very much. This has been enlightening.

We will now go back to Sandia if we can find the right paper that goes with it. Let us see, we continue with the handout we had this morning, Walt. We will use your handout from this morning?

DR. VON RIESEMANN: Yes, we will.

DR. SIESS: And was this event tree yours?

DR. VON RIESEMANN: Yes, that was mine.

DR. SIESS: That is an event tree, isn't it?

DR. VON RIESEMANN: A logic diagram of how we would conduct the program.

DR. SIESS: Okay, a logic diagram.

DR. VON RIESEMANN: In more detail than perhaps you ever wanted to find out or know.

(Laughter.)

DR. SIESS: How often do you revise it?

DR. VON RIESEMANN: We will revise it, I imagine, once we get into the program.

(Laughter.)

To conform to the program.

Let me backtrack just for a moment. What I am still talking about is the overall program in the planning phase, looking at the steel-reinforced and prestressed concrete vessels or containment buildings, looking at the internal pressurization, both static and dynamic, and looking at the earthquake loading.

In the Phase I activity, and I am going to be picking up something called Phase I planning, that vu-graph, form an advisory peer group, scaling laws, somewhere about halfway down, I imagine, in the packet. I will just take a moment. Actually, the first vu-graph I will be using is going back to the failure modes on the free-standing steel.

The reason we looked at this is to make sure that the scale models that we are going to use will have the failure modes that do exist in the full-scale containments.

Now, the checkmark means that, in our estimation, yes, it will scale; an X, it will not scale. The loading conditions again are seismic, internal explosion, and internal static pressure.

You notice one thing if you look a little bit at the table -- the problem with welds. That is always a difficulty in modeling those. We are proposing to do separate tests on that to take care of that variability.

Now, if you look at the table, then, it looks somewhere in between the 20th and the 50th scale is about the smallest scale we would like to use for the free-standing steel containments, and as you will see in a few moments, we are proposing to do scale tests at 1/32 scale and at 1/8 scale.

Are there any questions on that? I do not want to go into great detail on this. I would rather just give an overview this afternoon.

DR. SIESS: I assume you are working in English units, then.

DR. VON RIESEMANN: I inquired about that to some

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people from Europe, what about -- do you use 10th scale or 30th, and they use a binary system, too, 2, 4, 8, so it comes out the same way.

DR. MARK: Now, welds are the only things that you feel you cannot scale at the level of either 1/8 or 1/20?

DR. VON RIESEMANN: There are difficulties with welds. Given, you know, a lot of money, maybe one can do it, but we are talking reasonable --

DR. MARK: I was not complaining. I am not even surprised.

DR. ZUDANS: Walt, you say that you can scale plate and hoop tension for all scales, and I am just wondering whether you can do that.

DR. VON RIESEMANN: You say that you can scale hoop tension for plate throughout all the scales? It is going to be difficult to find a plate that scale. In other words, you have thicknesses that vary from an inch and a quarter to maybe half an inch,and if you talk about 50ths of that --

DR. SIESS: It is thin plate.

DR. VON RIESEMANN: Well, yes. I think we can do a fairly good job on hoop tension on that. Now, we are taking care of looking at material properties for the scale models versus the full size, okay, because we might have to use a different material again for economics and availabilty. A516 steel is not available, necessarily, in thin stock. So we want to get somewhat the same stress/strain curve.

There is a lot of detail that we are not going to present this afternoon.

DR. SIESS: Oh, yes. Now, on y ur welds, are you talking about real welds on these small sc le models?

DR. VON RIESEMANN: The real sma l ones will be perhaps welded and also using bonding material and stiffeners, okay?

DR. SIESS: Yes. I was going to say, I do not really see why you have to use a weld if you can make a connection with a material that has a known characteristic or that you can measure.

DR. VON RIESEMANN: I would like to skip the next two in your handout and look at the load simulation. Obviously, the static pressure really does not offer much --

DR. SIESS: Let me, before you get on, when you look at the hatches, especially the equipment hatch -- I was really thinking of a deformation where there might be a gap or something, and that does not scale.

DR. VON RIESEMANN: I think that question, Dr. Siess, might come up a little later when we show some sketches.

DR. SIESS: And that does not scale, but it is computable. Okay, fine.

DR. VON RIESEMANN: I am going down to the vu-graph on load simulation, looking at static pressure loading. We have decided to use pneumatic rather than water hydrostatic pressure. The reason is because of leakage characteristics, the change in head, and also the contained energy. But with this, you do have one penalty. You are going to have to watch about safety considerations, and we will, of course, record strains, deflections, and leak rate.

Instrumentation is a big concern in these experiments, and we are concentrating a lot of our effort on it.

DR. SIESS: I think the pneumatic is almost essential.

DR. VON RIESEMANN: But, interestingly, Professor Siess, all the tests to date that I have seen on containments have been hydrostatic.

DR. SIESS: I know, but what we are interested in here is essentially an opening size or a leak rate. I can certainly visualize failures where, under water, I just get a small opening and depressurize, whereas, if I had air, that opening would grow very rapidly into one that would be, you know, significantly different in size.

DR. VON RIESEMANN: We get the truer characteristic, if you will, the behavior of the containment, with this type of testing.

DR. ZUDANS: You can maintain pressure easier, but also, you store a lot more energy in the gas --

DR. VON RIESEMANN: Yes.

DR. SIESS: You've got a problem.

DR. ZUDANS: You really run yourself into --

DR. SIESS: We tested the PCRV's with that.

DR. ZUDANS: And the leakage rate measurement is more difficult.

DR. SIESS: Pardon?

DR. ZUDANS: The leakage rate measurement is more difficult but more realistic because you measure the same things.

DR. SIESS: I guess geometry could substitute for leakage measurement.

DR. ZUDANS: Well, if you want to take a chance on schematics, it is fine with me.

DR. SIESS: Well, it is not all that difficult. They've got a lot of wide open spaces. We went out in the cornfield to test those PCRV's, just backed off and watched them blow, you know, and then it took a few minutes to find the pieces.

DR. MARK: Your leak rate is just a PV measurement inside the container, anyway.

DR. VON RIESEMANN: Yes. That is -- I say that sort of jumping, you know, over that word "leak rate." That is not a very easy measurement, and there are a lot of concerns.

DR. MARK: It is impossible unless you know exactly where his leak is going to pop and where they all are.

DR. SIESS: There are going to be a lot of failure modes where you are not really concerned with measuring the leak rate.

DR. VON RIESEMANN: Yes. If, for example, in a steel containment, it might go such that essentially there is no leak rate, within, you know, error bounds, until the very end, and then it pops.

DR. SIESS: Some of them are going to be that. Some failure modes are going to be that.

DR. VON RIESEMANN: Now, concrete containments might be quite different. In the dynamic pressure loading, obviously, the problem we have there is that you only put one dynamic load on a containment. You are not going over an entire range, and so we are going to have to do special calibration tests for that to know the loading ahead of time, and we are going to use loading similar to a hydrogen detonation, a lot of, again, study to be done there.

One advantage of doing these dynamic tests, though, too, is we can model the interior structure to take that into account. We can also do asymmetric loading and look at that effect.

DR. ZUDANS: Could you not in fact use hydrogen?

DR. VON RIESEMANN: Yes, we might even use hydrogen. Is it repeatable or can we make sure it detonates? You know, from the experimental side, you want to make sure you can be able to detonate at a given time.

DR. ZUDANS: Well, stick your head in and check.

DR. SIESS: You are talking about the explosive type now.

DR. VON RIESEMANN: We might use another combustible gas or an explosive.

DR. SIESS: I have got a feeling in your static tests that that weak spot in a real containment is going to be the equipment hatch, simply from the distortions and the different thicknesses that are involved. I just cannot see how you can get one of these things over-pressured very far without the geometry changing now to where you cannot seal that hatch. I do not know. I may be wrong.

DR. VON RIESEMANN: Those flanges are about 3 inches thick. They are fairly rigid on the equipment hatch.

DR. SIESS: That is right.

DR. VON RIESEMANN: And we do not know, under load, how much deformation or rotation you will have in that plane.

DR. SIESS: Right, and the structure around it is not very rigid, and it is going to try to stretch out, and that thing is going to try to stay in place, and I don't know whether the containment is going to tear, it is going to distort that opening --

DR. VON RIESEMANN: It depends a lot on the ductility, then, in that region.

DR. SIESS: But if you end up getting a material failure, a tear in the steel, I don't think you have to worry too much about measuring leak rates.

DR. VON RIESEMANN: Right.

DR. SIESS: I think you are going to have a hole there that the people that want to know the answer are going to say, that is big enough.

(Laughter.)

Again, I would like to know how big is big enough, but I think, when it goes, with pneumatic loading, it is not going to be -- impulsive loading is something else. It is going to be a different story.

DR. ZUDANS: Unless the response in this static or dynamic loading is different from what is considered in a design. Those reinforcings are designed so that they do not produce more deformation than -- so they are just as strong as the shell itself.

DR. SIESS: Elastically.

DR. ZUDANS: Unless they are not properly fitted.

DR. SIESS: Elastically.

DR. ZUDANS: Yes.

DR. SIESS: But when the shell goes inelastic and starts straining out 3 percent, I don't think they are going to be inelastic and straining out 3 percent. I will bet you they are not designed to yield at the same time the shell yields.

DR. ZUDANS: They will represent the hot spot in the shell, then.

DR. SIESS: Yes, they will be a hot spot when the shell yields. And that will be true in the concrete one, too.

DR. VON RIESEMANN: Some of the equipment hatches, the covers are flat, potentially the failure point. In some containments, potentially, the basemat is a failure point.

DR. SIESS: The knuckle joint.

DR. VON RIESEMANN: And so we have to look at these.

DR. SIESS: Now, the concrete containment, from a leak standpoint, or steel containment, it is just a lot thinner. It is going to act differently.

DR. VON RIESEMANN: Yes. That liner, in a sense, will act like a balloon to some extent, being restrained by the concrete.

DR. SIESS: You may remember what happened at Midland when that water pipe burst inside the concrete, and water at about 100 psi got between the concrete and the containment liner, and it buckled that liner inward, I think it was around 3 or 4 feet, over a couple of hundred square feet, and it stopped where one of those channels was welded to a channel and buried in there. You know, it was a vectral(?) containment with vertical channels and horizontal angles, and it bent there and it cracked there, but it did not crack all the way through.

DR. ZUDANS: It is a mild steel.

DR. SIESS: It is a mild steel, and it just took one hell of a lot of deformation and some very localized.

DR. ZUDANS: And it zipped off many of those, because they are 14 inches round?

DR. SIESS: Oh, it ripped the angles out, and it ripped one set of channels out, I think. But this was amazing. You know, it pushed inward, but again, the ductility was there with all of this stuff welded onto it, and it was a penetration somewhere at the edge of the system, I think.

DR. VON RIESEMANN: I believe a similar experience happened at Indian Point. A steam line broke and they had buckling of the liner, also.

DR. SIESS: Yes, they heated it up and buckled it from the temperature, but it was not over -- that essentially buckled over one panel, but not nearly as far as this Midland thing. This was a mess. It went out.

DR. ZUDANS: Yes. That Indian Point case is interesting, because that is normally analyzed by AE's, you know, to show that it does not buckle at, say, 200 degree temperature. I wonder, what was the temperature then?

DR. SIESS: Well, it was probably closer to 500 than 200. It was a steamline. No, it was not. It was feed water line.

DR. MARK: Cold water.

DR. SIESS: No, it was feed water, and that was not that --

DR. VON RIESEMANN: Was it feed water?

DR. SIESS: Yes, feed water line.

DR. VON RIESEMANN: Okay.

DR. SIESS: It was not that cold. How cold is it?

DR. MARK: I thought it started because the pipe froze.

DR. SIESS: No, no, not this one. This was water.

DR. MARK: Oh, at Midland.

DR. SIESS: Midland was water that cooled -- cured the concrete. Indian Point was the feed water pipe that broke off right at the penetration, 180 degrees on top and that just made a spray that just went up the wall.

DR. ZUDANS: What temperature? About 350?

DR. SIESS: I don't know. This is boiling -- what is the feed water temperature of the steam generator?

DR. ZUDANS: It is preheated.

DR. SIESS: I don't know how much temperature rise you get in a steam generator. It goes out at 550, so -- it was over 200, I am pretty sure.

DR. ZUDANS: Oh, yes, much closer to 400, probably.

DR. SIESS: And it was not like LOCA conditions, because it sprayed hot water on there for quite a while, too, you see.

DR. ZUDANS: Yes, the feed water is preheated almost.

DR. VON RIESEMANN: I will skip the next vu-graph and get on to the earthquake simulation. There, as was mentioned previously this afternoon, of course, the technique that you use for doing the experiments is interrelated between what you define as an input and what loading technique is going to be used, and if you look at just loading devices, you have the base excitation, either shakers, explosives, or underground nuclear, say, at the Nevada test site, or forcing devices.

The forcing devices are normally better suited for a frame structure than they are for a cylindrical shell, and we are going to have to do, obviously, more study in this area to see which is the best technique to use for the containments.

DR. SIESS: What do you mean by forcing device? Pullback release?

DR. VON RIESEMANN: Well, there are different types. There is the eccentric mass, if you will, pullback. There are some explosive type cutters that are being used. People are quite ingenious, if you will, in loading structures.

DR. SIESS: But the only one that would really work to shake it anything like an earthquake would be an eccentric mass mounted on the base plate, and there ain't one made big enough to shake one of these.

DR. ZUDANS: Except if you tune it up.

DR. SIESS: Oh, I don't think you can put the energy into the whole structure with any --

DR. ZUDANS: Well, you can do wonders with tuning.

DR. SIESS: You cannot do it inelastic, because that is a cinch. You might get the thing -- if you could get it at resonant frequency, you could get it up, but as soon as it went inelastic, you wouldn't be resonant any more, and you would be dead.

DR. VON RIESEMANN: You have to be able to change frequency, yes.

DR. SIESS: You cannot do it, I do not think.

DR. VON RIESEMANN: On a comment made earlier --

DR. SIESS: It will bead on you.

DR. VON RIESEMANN: -- we are also worried about not getting involved in the soil/structure interaction using explosives. We might be testing soil or rock rather than containments, and that is not the object of the program.

If you look at one of the difficulties, and if you do scaling, and you are looking, say, just at 1G input -- now, we are not sure of what it would take to fail a containment, but if you look at the scale factors here, you have to increase the acceleration input, change the frequency.

Now, if you stay with that, then you look at what shakers are available, and this is a compilation of a major number of them around the world, it is very hard to get one to match what we need, so it is going to take some more effort to see what type of input we should use and what type of forcing device.

Even the Japanese testing table, the first one listed there, is a very large one, but they are, I believe, limited by frequency content, and they are going to test some containments. As I mentioned this morning, I have spoken to Professor Shibata trying to find some information. They supposedly are going to do a quarter scale PWR and a third scale, I think, BWR, but I do not think the failure.

DR. SIESS: You know, it would be nice to get up to 33 full scale, but you don't have to.

DR. VON RIESEMANN: Right.

DR. SIESS: Everything I ever read said that most of the items in containment, not equipment, but structures, are between 1 and 10, and I don't know how --

DR. VON RIESEMANN: Well, yes. Even if you used, say, 10 maximum frequency, we still have to have some concerns of the equipment availability.

DR. SIESS: Oh, yes.

DR. VON RIESEMANN: Obviously, it is going to be a give and take on this part of the program.

DR. SIESS: Well, see, on the real small ones, what is the highest frequency here, 500 at Wyle?

DR. VON RIESEMANN: Well, say, it is 16 if you are doing -- if you use the 33 cutoff, you are going to 500, say, hertz, and say if you cut it down to 10, you go up a factor of 16. What, 160, is that?

DR. SIESS: Yes. It scales.

DR. VON RIESEMANN: Yes.

Now, there are other questions that I am sort of bypassing. How do you scale earthquakes? That is another input.

DR. ZUDANS: Also very much. You did not show that slide, but you had a layout shown with a mat sitting --

DR. VON RIESEMANN: Yes.

DR. ZUDANS: If you do that, even if you don't want to talk about soil/structures interaction, you will have it there, regardless of what you do.

DR. VON RIESEMANN: Oh, that one is for -- we have to be very careful on those tests. Those are for the static tests. If we are testing the basemat, then we have to be very careful on, in fact, the soil conditions, the foundation modulus.

Now, the first test we are going to conduct will not be looking at the basemat failure.

DR. ZUDANS: Oh.

DR. VON RIESEMANN: This is in the free-standing steel type.

DR. SIESS: But you've got a pretty good -- you can bracket it. It goes from rigid to pretty flexible.

DR. VON RIESEMANN: Yes.

With that as a background of information, we did a lot of --

DR. SIESS: Now, how closely do you feel you have to simulate all of these things?

DR. VON RIESEMANN: The earthquake?

DR. SIESS: In the earthquake. Are you trying to check out now a code or are you trying to just learn something from the model itself?

DR. VON RIESEMANN: We want to learn -- well, ideally, you would like to do both. You like to learn about the failure modes of the containment under an extra severe earthquake, and you would like to be able to use that information as data, if you will, for computer analysis.

DR. SIESS: Just about where in your program does this seismic get into it? Now, we agreed earlier this morning that the current impetus is in terms of pressure. Some people are going to argue -- it got argued yesterday -- that the earthquake might be the thing that causes the core melt or degraded core, and that may not be zero probability by any means, but the probability that you are still having the earthquake a few hours after the accident, I think, starts dropping off fairly fast, you know.

MR. COSTELLO: I think, a few slides down, Walter has a layout of what we are shooting for in time phasing.

DR. SIESS: Okay.

DR. VON RIESEMANN: I will cover that point in a moment.

DR. SIESS: We are getting down the road a ways on that.

DR. VON RIESEMANN: Yes. But we are going to look at, ahead of time, ways of doing this testing without commiting, if you will, to hardware, because it does take some lead time, particularly if you want to use, say, a shaker in Japan. Negotiations would take a w..ile.

The other thing is that the NRC is conducting some programs under the SSMRP program on load combinations, and there were some questions this morning, and perhaps -- I am not sure of the results -- the bottom line might be that an earthquake in LOCA will be decoupled. In other words, you will not have to consider the two.

DR. SIESS: That would be helpful.

DR. VON RIESEMANN: From a risk, if you will, probabilistic standpoint.

DR. SIESS: But then, even so, as I understand this problem, it really extends into just earthquakes, period.

DR. VON RIESEMANN: Just earthquakes, period, yes.

DR. SIESS: What is the limitation on that shake table at CERL?

DR. VON RIESEMANN: Isn't that on here?

DR. SIESS: Yes, but where does it limit you? It has got a very high frequency capability, but its energy input is -- that is the second one on there.

DR. VON RIESEMANN: Yes. Ron, do you remember the --

DR. WOODFIN: I believe that it is --

DR. VON RIESEMANN: Why don't you get to a microphone?

DR. SIESS: See, that thing was developed and not for seismic but for blast loading.

DR. WOODFIN: I believe that that was lifted because of displacements, but I am not absolutely sure. I will have to go back to some of my work to see, and I have not really done that.

DR. SIESS: Okay, but it is pretty well up there on displacement. It will do almost 6 inches horizontal, which is really your main concern on much of this.

DR. WOODFIN: I don't currently remember exactly what it is. What I did was plot this spectrum for each, a spectrum representing the table that is on the previous page, and looked at each one of these sets of shaker characteristics to see where that spectrum fell relative to the shaker characteristics, and in every case, the spectrum fell outside somewhere. I don't remember what place it was without going back and looking at that data.

DR. VON RIESEMANN: Let me make sure we do not give you the impression that there are no facilities available.

DR. SIESS: No, I know they are available, but every one has got something different about it.

DR. VON RIESEMANN: And we've got to look at the loading systems they have, what can we do?

DR. SIESS: CERL is attractive because you know good and well it is available. In fact, they were trying to give it away a couple of years ago because they did not have any use for it, and they tested everything that they needed to against blasts, but that thing uses an accumulator to put one hell of a shock out, but --

DR. WOODFIN: That may have been part of the time duration --

DR. SIESS: I think that is part of the problem on it.

DR. WOODFIN: That may have been the problem.

DR. SIESS: I think you cannot get much of a time duration on it.

DR. ZUDANS: Add another accumulator.

DR. SIESS: No.

DR. ZUDANS: Could you focus on how do we relate in this program to SSMRP, because they are also looking for margins.

DR. VON RIESEMANN: Okay. The status of the SSMRP is that they have just about, I guess, completed Phase I, and they are looking at all systems as linear elastic. They are trying tc identify those parts of the entire system that contribute most to risk, and they are going to concentrate on that, okay? From what I know, they are not looking at containment failures; in fact, some of the input we get could be very useful to their program.

Did I state that properly, Jim?

DR. SIESS: But they had inelastic in their program.

DR. VON RIESEMANN: Yes, but not in the phase I.

DR. SIESS: No, but, you know, you are going down to 1990.

DR. MARK: They will be in Phase II by then.

(Laughter.)

DR. ZUDANS: I cannot see how they -- in their overall picture, of course, they have to include the containment, but you may be right that they are not looking at containment failure modes. This is why I am asking the question. I do not remember that. I know they look at the systems failure modes.

DR. SIESS: Well, strictly speaking, SSMRP ought to give us a framework for the whole seismic problem in every aspect of nuclear power plants.

DR. ZUDANS: Right, and margins.

DR. SIESS: Well, it will not tell us that much. It will raise more questions than it answers.

DR. ZUDANS: I guess it did already.

DR. SIESS: It will tell you what the forces are on the containment, but it is not going to be able to tell you whether containment is going to fail or not without -- if it could, we would not need this.

MR. COSTELLO: That is correct.

DR. SIESS: Now, if it works out, it will provide a framework. It may tell you, forget about this, that a good guess as to what the containment will take is probably good enough considering we do not know what the earthquake is. If somebody really looks at it for answers -- but this is all far enough down the line that I think we can not worry about it too much today.

MR. COSTELLO: Yes. Frankly, that is one reason why we have it far down the line, Professor Siess.

DR. SIESS: Yes.

MR. COSTELLO: A countervailing force, however, is the general uneasiness that occurs when people start talking about earthquakes and countermode failures of systems.

DR. SIESS: Yes, I know.

MR. COSTELLO: And this ebbs and flows, and it is flowing again.

DR. SIESS: I think that, as I said, SSMRP may tell us whether we have really got something to worry about, and there is a certain amount of logic in saying that there is some chance that we should worry about it; let's start thinking about it now. There are a lot of people thinking about how to test things. Some of them are thinking about buildings and some of them are thinking about containments, and there are different problems in the two, and I think NRC needs to get its act together some day and have some sort of an interagency or inter-NRC group looking at seismic problems and beginning to see what they want to work on. We need some prioritization of that, and right now, everybody is looking at it and everybody is talking about it, and frankly, half of them don't know what they are talking about, don't know what they are worrying about.

DR. VON RIESEMANN: I formed a sort of ad hoc, if you will, seismic -- I called it seismic -- interchange group with people from Lawrence Livermore Lab on it, people from DOE that are interested in it; people from NRC are in on it; people from USGS, NBS, and National Science Foundation, and EPRI.

The only purpose there was to put, if you will, on the table the research programs of each group, so at least we know what is going on and we don't duplicate effort.

DR. ZUDANS: That is good.

DR. SIESS: That is helpful. I'm not sure you've got everybody in it you ought to have in it. There is a lot of experience in other areas about how things perform in earthquakes, which let's don't ignore. We've got a lot of full scale tests.

DR. VON RIESEMANN: Yes.

DR. SIESS: They are not all very well instrumented, and some of them have been reasonably well instrumented, and none of them, for any practical purposes, apply to the kind of structures we are talking about, but I am not sure that some parts of it do not apply. We've got refineries and chemical plants, and I am not talking structure, I am talking components now, and a lot of that experience is around, and maybe some day we are going to have some more.

We have got a lot of nice, well instrumented nuclear plants around, and a few of them are in areas where the chances of getting an earthquake are pretty good.

DR. VON RIESEMANN: In the ASCE, in the structural division, there is a Committee on Seisic Analysis that I belong to, and Dr. Robert Kennedy, and one of the tasks there is in fact to document past experience in earthquakes of buildings comparable, if you will, to nuclear power plants and get a data base developed, and we are looking at that now.

DR. SIESS: I think one of the biggest uncertainties and one we are not getting anywhere on is soil structure interaction, and right now all I see going on is comparing two computer codes, which does not give me one heck of a lot of comfort, even if they agree perfectly. I have no confidence they are telling anything about what happens to a real structure. We have some tests, but I haven't seen the correlations yet.

DR. VON RIESEMANN: Well, are you speaking of the simquake?

DR. SIESS: Yes. The last I heard, they were still

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making analyses.

DR. VON RIESEMANN: There is a report out.

DR. SIESS: On the tests.

DR. VON RIESEMANN: On the tests.

DR. SIESS: Yes, but until you see the analysis --

DR. VON RIESEMANN: There was some difficulty, as you obviously k

DR. SIESS: Oh, yes.

DR. ZUDANS: A quick question. SSMRP: Do they generate fragility curves for containment?

DR. SIESS: No.

DR. VON RIESEMANN: I have not seen them for containments. They are working on them for components.

DR. ZUDANS: That I know.

DR. VON RIESEMANN: And they are doing it by a Delphi procedure.

DR. ZUDANS: That I know, yes.

DR. SIESS: But, you see, you could come in on a containment with a fragility guess that is probably a lot better than they are getting on any of the components.

DR. ZUDANS: I would agree with you.

DR. SIESS: You know, you can say, there is nothing going to happen, and at this level, it will probably fall down, and that range is not going to be so awfully big. It might be two or three to one. You look at some of the component fragility curves, and they are not anywhere near that good.

DR. VON RIESEMANN: And with that information on the containment, they can say whether it is a sensitive contributor, if you will, in the chain of events.

DR. ZUDANS: But if they do not incorporate that in their model, they will not be able to tell you anything.

DR. SIESS: But, you see, if I was asked to give a fragility curve for containment failure, I am going to come right back and say, what do you mean by failure? And I am not sure -- you know, it depends on what it is. If it is this degraded core thing, it is some leak rate. In some other case, it might be enough deformation -- I don't know, what else is it besides leak rate? That is all the containment is there for. Right? So I guess I am not quite sure under what conditions. If it is just an earthquake, I don't really care whether it looks or not. There is nothing to leak out.

DR. ZUDANS: But it could do other things. It could move so much that it would detach or damage some other equipment, hit the safety-related systems --

DR. SIESS: But that is not containment failure. That is failure of that equipment due to the seismic event.

DR. ZUDANS: But the containment would cause it.

DR. SIESS: Well, the shaking, yes. The containment causes everything because everything is inside the containment and very carefully attached to it.

DR. ZUDANS: Well, that makes this program more important, right?

DR. SIESS: But you've got to know how much the containment moves to know how much the pipe moves, but that does not do it by containment failure; that is just part of the analysis process. You've got to know how much the soil moves, too.

But containment failure due to earthquakes, some day we are going to get that one settled.

MR. COSTELLO: I am willing to offer an observation or a thought that I have had ever since this program started, and it is related to the hypothesis that if, as a result of earthquake, you do have sufficent deformation to cause leakage paths in the containment, then the public is at risk, because if something else happens as a result of the earthquake, it is going to come out.

DR. SIESS: Yes, but that is a lower probability type thing.

MR. COSTELLO: Certainly.

DR. SIESS: Somewhere, we have got to decide --

MR. COSTELLO: And the SSMRP should allow the meshing of that.

DR. SIESS: That is right, because it is looking at earthquakes larger than SSE and their probability, and they are the contributors to risk. We will have to hang a lot on that, and actually, that is in there. But I do not think they have tried to put an estimate on the probability that a given earthquake will produce a leak of a certain size in a containment.

But that is a very interesting question: Can an earthquake alone produce a leak? That is going to be a little harder to tell, too, because you will have to pressurize it after the earthquake to see.

DR. VON RIESEMANN: Well, the one scenario that might be hypothesized --

DR. SIESS: You see, I do not think you will ever answer that with your model tests.

DR. VON RIESEMANN: No.

DR. SIESS: Because if an earthquake is going to do something that produces a leak in containment, I think that 90 to 99 percent of the risk would be from a valve being actuated or some sort of a seal being opened as a result of it, and I do not think you can model those things at the level you are talking about. It is a component failure, not a containment failure.

MR. COSTELLO: Well, I am not so sure about the seal question. There again, we are not going directly into seals, but the logic that I see is that if we are sufficiently confident about our ability to predict local deformations under extreme loads, then it would seem that one can get a handle on seal performance; that is, certain kinds of seals can sustain certain deformations, and if you are going to get deformations larger than that, the seal will not seal.

DR. SIESS: Yes.

MR. COSTELLO: But it also may have an easy fix if it is a problem.

DR. SIESS: But you will not get that in tests of scale model containments. You might get it in component or separate effects tests or analysis, but what I meant was, building a 32nd scale model, you are not going to be able to model all the penetrations and pipes and valves and stuff.

MR. COSTELLO: That is correct.

DR. ZUDANS: Unless you made a special effort to define that as a failure mode and build it in. It is possible.

DR. SIESS: Well, I would not trust the model as far as I could throw it for that.

DR. ZUDANS: Why not?

DR. SIESS: I would rather go to the full size component mocked up in some way with a deformation and see if it could take it statically.

DR. VON RIESEMANN: To answer part of that question, what we plan on doing is doing component test. Also, as input from the model test, do some computer analysis, if you will, of the certain regions and the penetrations and see how that affects that penetration.

DR. SIESS: You know, when you get into that, there are two ways to get at it, and I am not sure -- you see, you model something or you build it and you see how it is affected. Now, that is an exploratory test, and I think if you can narrow it down to certain places where you think there is a pretty good chance of finding something, that is not a bad way to go. But it is the kind of test that, if it is not very carefully narrowed down, you can make a lot of them and not get anywhere, and that is about the time somebody looks at it and says, let's quit putting money in it.

DR. VON RIESEMANN: Let me get on to the long range problem, and I should mark on there fairly big, "Preliminary, Use with Caution," et cetera, et cetera. This is just some estimate of what it would take to do the three different types of containments, looking at them for both static pressurization, dynamic pressurization, earthquake loading, and also, we have two additional tasks on here, developing the techniques for loading the containment dynamically and also looking at the seismic load technology.

Now, please do not consider the times on there as absolute by any means.

DR. ZUDANS: We are certainly not going to see the result.

DR. VON RIESEMANN: But this is at least a piece of paper we talk from, what activities will have to occur. What we are going to do is use some program management techniques, see what the resources, what the schedules are, the attendant schedules, and see just how we can in fact execute the program.

The lines, of course, indicate not only experimental work but analytical work together.

DR. ZUDANS: Well, it is interesting. If you go that far in the future, there might be, really, real containments available for testing.

DR. VON RIESEMANN: Yes.

DR. ZUDANS: For example, Three Mile Island.

(Laughter.)

DR. VON RIESEMANN: I imagine they might want to sell it at a low -- really, a fairly high price.

DR. ZUDANS: If they cannot start it, what would they do with it?

DR. SIESS: Well, now, just looking at this from a more immediate point of view, for FY 83, you are still in the static pressure range, although you are beginning to look at your dynamic pressure technology.

DR. ZUDANS: Right.

DR. SIESS: And by then, you expect to have some results out of that MIT project on dynamic stuff.

MR. COSTELLO: That is correct.

DR. SIESS: And know what you are looking for.

DR. VON RIESEMANN: Well, there is also a hydrogen program at Sandia, so we are looking at both programs for input.

DR. SIESS: And you are not even talking about seismic until 1985.

DR. VON RIESEMANN: That is at least on this piece of paper.

DR. SIESS: Yes. That is about the right time scale, I would think, even assuming SSMRP goes along on schedule, or not any farther behind that it is now.

The thing I want to emphasize, if it has not come out already, is that I think the lessons we have learned are that the containment is a leakage -- it is a containment, by golly, it is not a structure -- it is a structure, but its function is containment, and that has to dominate our thinking.

The clean part of this is the static pressure part. We know that if we increase the static far enough, it is going to leak. That is a cinch. But we are not sure about those others, and some of that is going to be exploratory if we find out it is really still bugging us.

DR. VON RIESEMANN: Let me also add a word of caution. When the filtered-vented containment people talk of pressure spikes, they are not talking it in the sense of structural response; they are talking in their own systems, and that is a static load as far as the structure is concerned, what they are talking about, and it is possible that there might not be a large concern with really dynamic loading on containments.

DR. SIESS: Other than seismic, you mean.

DR. VON RIESEMANN: Other than seismic, yes. I am talking internal pressurization.

DR. ZUDANS: Yes, you are right. Even ice condenser is really static load.

DR. SIESS: Yes.

DR. VON RIESEMANN: It is not clear, and that is why we feel we should do the static first. You will get a lot of useful information from that, and it might answer, to some extent, the questions on the dynamic response.

DR. SIESS: You could have some local shock wave effects, couldn't you?

DR. VON RIESEMANN: No.

DR. SIESS: If you had a denotation that propagated the --

DR. VON RIESEMANN: Well, if you have a very -- you know, it depends on what you hypothesize now for the rate of loading.

DR. SIESS: A detonation would give you a dynamic loading, wouldn't it?

DR. VON RIESEMANN: It gives you dynamic loading, right.

DR. SIESS: A burn like Three Mile Island --

DR. VON RIESEMANN: A burn will give a static.

DR. SIESS: -- will be static.

DR. VON RIESEMANN: And if they take care of the detonation problem with some technique, you know, blow plugs or something, say, then that might not be a major concern.

DR. SIESS: That would sure simplify things.

DR. VON RIESEMANN: The next eye examination vu-graph is --

DR. SIESS: We've got the big one.

DR. VON RIESEMANN: You've got, yes, the blow-up.

DR. SIESS: I have, anyway.

DR. VON RIESEMANN: Yes, I think everything should have that.

DR. SIESS: In color!

DR. ZUDANS: We did not know where it belonged.

DR. VON RIESEMANN: What we show down there is a logic chart for the program, and this is, in a sense, generic for the any type of containment, and there are three major activities on the top, number 22, 1, and 15, from left to right, and what we are looking on the left side is, if you will, separate effects, looking at components, whatever you want to call them, materials tests, welds. We have the experiments running essentially down the center, and analysis on the right hand side, and these are interlocked, obviously, doing them concurrently, and the other thing is, we consider that any time you do a piece of work, you had better learn on what you have done, and you make some decisions on that basis, okay? If you are a fool in the beginning, don't stay a fool the rest of your life, if you will.

DR. SIESS: Learn from your mistakes.

DR. VON RIESEMANN: Right. And so we are going down with some initial experiments, and I will get to that in a moment, illustrating it with the steel. Then we will do some experiments with the penetrations. Then we will do, if you will, another test with penetrations but essentially a replica of, say, a larger scale model, and then do, down near the bottom, number 11, essentially the larger scale experiment, and then, you know, everything being fine, going down the yellow path -- failure modes were repeatable; size effects, there aren't any; and that is the end of that portion of the program. You report your results.

If you have difficulty, you branch out at any one of these areas, and you have to reexamine either what you have done or look at the objectives to the program, or perhaps do some analytical work.

DR. SIESS: Well, there ought to be something on here at a few spots that says, "Go back to NRC and find out whether they still have a question."

(Laughter.)

Well, we do not do that sometimes, and we find out that the questions have gone away for some reason or another, and you are still plugging away, working on it.

MR. COSTELLO: I am aware of that difficulty, Professor Siess, but I think I can feel reasonably certain that the ability to make reliabile prediction of containment capacity under static pressure loads --

DR. SIESS: That is not going to go away.

MR. COSTELLO: -- is something that now we say we needed 5 years ago.

DR. SIESS: I do not think that one is going to go away, because we can conceive of mechanisms where that pressure will continue to increase, and we know that if that happens, we are going to have a leak.

DR. ZUDANS: There may be some qualification needed, because, really, it is not clear how accurately do you have to know it.

DR. SIESS: Oh, no.

DR. ZUDANS: And I think that we can tell, without any tests, just by analysis, pretty close but not precisely, and if other mitigating devices are installed that do not challenge containment beyond the design pressure, then all of this is unnecessary, and therefore, there is a need to go back to NRC and ask that question.

DR. SIESS: If you eliminated steam explosion, if you put in a vented filter, if you really were willing to operate, then all you need to know is some kind of a bound on the containment capacity which you might be willing to settle for at the end of an elastic range.

DR. ZUDANS: Good enough.

DR. SIESS: The question would not go away, but it would be simplified considerably, and there are a lot of regulatory decisions that are likely to be made before we get all the answers, and they may change the nature of the questions, if not the basic question.

DR. ZUDANS: Right.

DR. SIESS: It is our job and Jim Costello's job and Roger's job to keep that in mind, and I assume it is NRR's job to keep them informed of what their problems are at any one time, if they know.

DR. ZUDANS: And in the same context, Chairman, who cares whether it is 10 percent or 10 percent more than what the elastic gross yielding would tell you, and that is a back-of-the-envelope calculation. I do not really see why is this precise knowledge needed, because even if you know it, you know that you will not take the continued pressure increase unless you have some other mitigating device. It will bust, regardless of how much it contains.

DR. SIESS: But you've got to back off a little bit. We've got containments designed and tested at around 60 psi.

DR. ZUDANS: Right.

DR. SIESS: And some calculations have indicated that they probably would take 130 psi --

DR. ZUDANS: Fine.

DR. SIESS: And people that have been thinking 60 psi are having trouble thinking 130 psi. This business is not one where -- you know you've got the margins, but you do not really want to believe they are there, and then somebody wants to get those margins with a much higher degree of assurance, and it is not whether it is 120 or 130 or 140 that people are questioning. There are some people that are not satisfied that it is 130 instead of 65, you know.

DR. ZUDANS: But 'at kind of answer can be generated much easier without, which is a tremendous problem.

MR. COSTELLO: Dr. Zudans, I guess I would frankl, submit that calculations unsupported by at least rudimentary tests will not turn out very convincing.

DR. ZUDANS: Well, but look. We have 71 or so containments operating, and they are all designed by calculations, and they all check exactly with the calculations.

Now, many computer codes exist now that can go a step further. They may not be able to follow to complete collapse, but they certainly go to major distortions and predict what will happen, and if that answer is not good enough, then I have to raise a question, why would it do to you if you could go 10 psi more?

DR. SIESS: Well, I think we agree. Ten psi we are not concerned about. But the reason people are comfortable where they are now at 60 psi is their margins, and they think those margins are big enough to take care of any uncertainties of the analysis of the material properties of the construction.

Now, when we are talking about 130 psi -- we say Indian Point -- that has got no margins.

DR. ZUDANS: Or we say it is going at that point.

DR. SIESS: That is right.

DR. ZUDANS: Now, what do we care?

DR. SIESS: But the people do not trust that 130.

They trust the 60 and are comfortable with it because they can point to the margins.

If you go into a licensing hearing and want to argue 130, you are not going to argue that on the same basis as the 60. The uncertainties that people can raise questions about cannot be answered right now, and they become a question.

Now, they may be only 10 psi in your mind, but a smart intervenor could make those 50 psi, and I think that is one of the reasons that this is necessary. It may not be for engineers, but --

DR. ZUDANS: I did not say really that this program is necessary. I am only saying that you may be able to do it in a lot simpler way.

DR. SIESS: Oh, yes.

MR. COSTELLO: Right.

DR. ZUDANS: Because you do not look for very high precision, but you look for reasonable assurance that what you say is right, and static tests alone may do that.

DR. SIESS: You see, that is asking a different question, and maybe -- and we talked about that earlier. Maybe it is a point to come back to it. If the question is, at what pressure -- or give me a reliable method for predicting at what pressure a containment will begin to leak excessively. Now, that is the kind of question that has been asked, and that is not the same as saying, give me a reliable way of estimating at what pressure I have confidence that the containment will not leak. Those are two different questions, and maybe the second one should be the one that we are asking.

Right now, I do not think it makes that much difference to the program, but I think that within the next year, there ought to be some discussion within the NRC somewhere to find out what they are -- would they be satisfied with reasonable assurance that, at pressure X, it will not leak; be able to ask applicants and licensees, tell me the pressure at which your containment has reasonable assurance it will not leak, and know how to evaluate the answer you get.

You see, that -- it may sound like the same question, but I will guarantee you will get a heck of a lot different answer.

DR. ZUDANS: Well, it is not the same question, really.

DR. SIESS: But right now, that is not the question that is being asked.

DR. ZUDANS: But if we just reflect what we talked about yesterday all day, about the dozen different mitigating methods and devices that presumably would prevent overpressurization of the containment, and therefore prevent containment rupture or whatever term you use for that, now, if we don't use those mitigating devices, no further testing or analysis of containment will prove to anybody ever that the containment will not bust if the pressure is allowed to build up, because you can just continue building it up.

DR. SIESS: No, but now what you have --

DR. ZUDANS: So what more --

DR. SIESS: Well, you have a useful quantity.

DR. ZUDANS: That is right.

DR. SIESS: If you know that it will bust at 170 psi, that affects your emergency plans.

DR. ZUDANS: Right.

DR. SIESS: That is important information, if you are going to let it bust.

DR. ZUDANS: But I am not going -- if I allow the situation to be retained that the pressure, continuous pressure build-up is allowed, if I assume that pressure build-up is allowed, it is an unacceptable situation. You have to have other mitigating devices.

DR. VON RIESEMANN: I am missing one point here. You are saying you are allowing the pressure to build up.

DR. ZUDANS: I said, if you did.

DR. VON RIESEMANN: But that is not reality.

DR. ZUDANS: But the reality will be that --

DR. VON RIESEMANN: It will come up to a peak and then drop off.

DR. ZUDANS: No, no. If you do not have heat removal capacity, it continues to build up.

DR. VON RIESEMANN: If you have heat removal capacity, yes.

DR. ZUDANS: Yes.

DR. SIESS: If you have heat removal capacity, but all the scenarios for degraded core cooling are the most likely ones, so you do not have containment heat removal capacity and you continue to pump water into a core that is perking along, and you continue to pour energy into the containment; there is no end to it.

DR. VON RIESEMANN: Right.

DR. SIESS: The one scenario on the BWR is the ATWS where there is 15 percent power level in there, and they have got all sorts of ways of keeping it cool, but you have got to get that energy out somewhere.

DR. ZUDANS: Now, if the DCC so-called rulemaking comes along, which will prevent this situation, will require some mitigating device, that means they will not be allow to over-pressurize the containment. What good does it do us to know that we can take 50 psi more than what we think now we can take?

DR. SIESS: A logical scenario would be that the Commission decided that they wanted a venting system.

DR. ZUDANS: That is right, a relief valve or --

DR. SIESS: Whether it is filtered or not, I do not know. And it turns out that to optimize that system and keep the costs reasonable, you want to vent at the highest possible pressure that will still give you reasonable assurance that it will go out through the vent and not through the containment, as high as they can reasonably get, and it might be just best to say, let's take the one where we -- end of elastic, 1/10 percent, 2/10 percent, 1 percent strain type of thing, make whatever studies you need to feel comfortable with that, and optimize the research program.

DR. ZUDANS: Or like in hydrotest pressure.

DR. SIESS: Pardon?

DR. ZUDANS: Hydrotest pressure. You know it does not like at that point.

DR. SIESS: Yes.

DR. ZUDANS: You open the vent at that point.

DR. VON RIESEMANN: Yes, but the thing is, the containment is only tested to 15 percent above.

DR. SIESS: There are people, we have had suggestions -- TVA's proposal was that that is when they would vent.

DR. ZUDANS: Exactly, and it is the right place to vent, because the design pressure is much higher than --

DR. SIESS: They may have simply decided it was easier, it was just as easy to vent there as it was to push it up a little higher.

DR. VON RIESEMANN: But isn't there then the question about risk of adding a system and comparing it to not adding?

DR. ZUDANS: That is right.

DR. SIESS: Oh, yes. That is part of --

DR. VON RIESEMANN: The tradeoffs, if you will.

DR. SIESS: That is part of the decision.

DR. ZUDANS: You may be forced politically by that time.

DR. SIESS: And there is a very serious question about adding a vent system that nobody will ever be willing to use.

DR. VON RIESEMANN: Yes, right.

DR. SIESS: I mean, on hydrogen mitigation, back in REG Guide 1.7, when it was clear for the kind of hydrogen they were assuming, you know, the metal-water reaction for the LOCA calculation, that you could vent that and you would add maybe 5 rem to that 300-rem dose you were calculating, which sounded negligible, and that was not an acceptable solution because it was immoral to deliberately release radioactivity. There was nothing immoral about increasing the leak rate, but to deliberately open the valve and let it out was wrong.

But these are not for us to worry about.

MR. COSTELLO: I think, in mitigation, I could say that perhaps -- it may just be a lack of coordination on the most pessimistic side. On the most optimistic side, one could say that perhaps there is a conscious effort to gain

Ta 8a

the value of diversity.

DR. ZUDANS: But, you see, what it tells me, and I am not critical of your problem -- please don't misunderstand me -- but it tells me that if the DCC rulemaking and they are required, all your effort is wasted.

DR. SIESS: No, not wasted.

DR. ZUDANS: What does it tell me? I mean, what do I need it for? I cannot have a situation. It is irrelevant.

DR. SIESS: Oh, some other structural engineers will find a use for it. We will call it spin-offs.

DR. ZUDANS: As a technology, yes. As a technology, okay.

DR. SIESS: I think that is enough on what is wrong with the way the Commission is being run.

DR. ZUDANS: Oh, I did not say they are running it wrong.

DR. SIESS: I did.

(Laughter.)

DR. ZUDANS: I only wondered that -- these people have such a talent, they can do lots of other jobs.

DR. SIESS: Yes. Well, we are worrying about that, too.

Let's go on and give us an overview of what --

DR. VON RIESEMANN: Let me give you just an overview of the initial program. We have talked to the Advisory

Panel now twice. We are going to look at the static pressure first, free-standing steel, which typifies the ice condenser in a MARK III, and the design pressures there are on the order of 15 psi, and they have a lot smaller volume, so the pressures can be a lot higher.

We are going to do three activities concurrently, the model experiments, the scales we are using to reproduce the failure modes that we are trying to look at. The range of scales that we are going to use will allow extrapolation. The number of tests, it is important to have them sufficient that we have credible results. It will be an analytical efforts and separate effects experiments or component tests.

DR. SIESS: That number of tests has to be at least three, doesn't it?

DR. VON RIESEMANN: At least three, yes.

DR. SIESS: Unless you get the first two to agree.

DR. VON RIESEMANN: I don't have a vu-graph of the 8th-scale, but I will get to that in a moment. What we are proposing, and we in fact are initiating work in looking at the 32nd scale first. The first set of experiments will be on the left, essentially just a cylinder with a hemispherical dome or perhaps a ellipsoidal dome because there is potential for buckling of those under internal pressure.

The next set of experiments will be with the ring stiffeners. Then phase II will be looking at the major penetrations, the equipment hatch and the personnel lock.

Now, phase III that we have shown there will essentially be a quarter of the 8th scale that we are proposing to do. We are not sure of all the boundary conditions yet. The bottom, if you will, skirt and the bottom line are in there and the tie-down bolts.

We are preparing drawings for these now and seeing the fabrication problems, if you will.

DR. SIESS: Now, the phase looks to me like it is just sort of exploratory, because --

DR. VON RIESEMANN: It does several things. One, it says, can we fabricate the model, can we test it, and it gives us a baseline, if you will, pressure load, and we will check, of course, with very simple analyses.

Then the one with the ring stiffeners, we will show the effect of those on the containment, and then the penetrations, we will show the effect of those on the behavior.

We have drawings here, but I do not want to get into those now, that show --

DR. SIESS: Now, suppose in your phase I, you do that first on one your left and the thing fails down at the bottom. Would there be any point at all in doing the one with the ring girders?

DR. VON RIESEMANN: No. That is why I say we want to learn in each step, and if we see some failure that is going to change the path of investigation, we ought to do it. Another way of saying it, it is an experimental program, not testing per se, and you should learn at each step.

DR. SIESS: Now, that bottom is the weak spot. I know it comes out the weak spot in some of the studies.

DR. VON RIESEMANN: Maybe I was misleading you. On the top left, the phase I, we will have a fairly heavy flange in there, not replicating a modeling, the actual bottom.

DR. SIESS: Okay.

DR. VON RIESEMANN: We will do some separate tests of those, perhaps.

DR. SIESS: I see. Those details vary quite a bit.

DR. VON RIESEMANN: Yes.

DR. ZUDANS: One question, Walt.

DR. VON RIESEMANN: Yes?

DR. ZUDANS: In order to be representative of actual containment, I would assume that these models would have to be designed as if they were the containments, and if you take all the loadings that are normally calculated to go on the containment, and there are dozens of them, you may find that you need stiffeners, in particular in the head area because it is unable to take the buckling loads sometimes, the wind loads.

DR. SIESS: Yes, that is the question. Are you going to essentially scale them down in certain characteristics from prototypes, or are you simply going to design models and analyze them and test them.

DR. ZUDANS: As if they were containment.

DR. SIESS: Or as if they were models.

DR. VON RIESEMANN: What we did was looked at what models, we looked at the typical, quote, unquote, if you will, containments that are out there, and this is developed on that basis, but it is not any specific plant, and we design the model then to incorporate the features we think are important.

DR. SIESS: Let me try to put it this way. A containment is designed for several loadings, only one of which you are going to be applying in your tests.

DR. VON RIESEMANN: Right.

DR. SIESS: I think the question is, are you going to design this for containment loadings, or are you going to design it just for your test loading?

DR. VON RIESEMANN: The basis of this structure is on the basis of containment loading and not just pressure loading.

DR. ZUDANS: Okay. For that reason, you have, you know, spray ring supports, and they are lots of concentrated loads around it that require stiffeners additional to those rings, almost certain.

DR. SIESS: Vertical stiffeners.

DR. ZUDANS: Yes.

DR. VON RIESEMANN: Again, if you look at -- it was brought out, I think, this morning, on some of the containments, they do not have any vertical stiffeners.

DR. ZUDANS: I know MARK III's do have.

DR. VON RIESEMANN: But they are down in the pool area where they are going to backfill --

DR. ZUDANS: Also upstairs. It depends how the head is designed.

DR. VON RIESEMANN: The difficulty we are running to is, obviously, there are so many different types of containments, and this is the one we have chosen as a model. Now, it is typical -- I should not say typical -- representative of something that is out there.

DR. SIESS: The things you need to include are the things that are going to be questions when somebody else tries to tell you what their containment is.

DR. ZUDANS: Look at Sequoyah. It has both ring stiffeners and meridian stiffeners.

DR. VON RIESEMANN: But look at Watsbar.

DR. SIESS: Excuse me. Dr. Mark is leaving and he wants to ask a question before he leaves.

DR. MARK: This may be absolutely outside your range, either of capability or need or interest. The question has come up about the capability of containments to stand negative pressure. Is there going to be any observations in your program which would allow one to comment on that, or perhaps there is no need for it. I am not sure. Maybe that is well enough known.

DR. RIESEMANN: We are not looking at it or not planning to do that in our program.

DR. ZUDANS: I think the other program should be looking at that, we listened to today.

DR. MARK: Well, the other program is not going to have --

DR. SIESS: You mean the buckling program?

DR. ZUDANS: Yes.

DR. MARK: The other program is not going to have, necessarily, as realistic tank models as these. I am not sure. In any event, the allegation that if you cool out the steam and the pressure drops, the whole thing will fall on its face, is probably wrong --

DR. ZUDANS: They could do that test very easily.

DR. MARK: But it should some time get a look.

DR. SIESS: I thinkthat the other program could do it by analysis very easily.

DR. MARK: Well, they can do theirs by analysis very easily, too, but nobody is going to believe them.

(Laughter.)

DR. ZUDANS: I think that here is an opportunity to do the negative pressure checking by condensing the steam in there.

DR. SIESS: Well, that is possible.

DR. MARK: Well, sorry to run.

DR. ZUDANS: That is a good question.

DR. SIESS: Thank you, Carson. See you next week.

DR. VON RIESEMANN: One of the problems we run into is that --

DR. SIESS: That only applies to steel containments.

DR. ZUDANS: Yes. Concrete does not care.

DR. VON RIESEMANN: Since so little investigations and experiments have been done on containments, we find with any group, it is always a little add-on, and the dollars are limited and so is the time.

Let me get on to, very briefly, the analytical effort. We are going to, of course, use this in prediction of the test results, in support of the test results. We are going to try to compare results with, the test results, if you will, with the codes. We are going to use both linear elastic, limit analysis, ultimate 2D and 3D in component analysis, and I might add that some of this already has begun.

In the what I call separate effects area, we are going to look at -- in fact, we started already -- in material properties, that difficulty in doing scale modeling, welds --

DR. SIESS: Let me go back to the analysis. I want to go back to what I said early this morning about how we are going to find out what containments can do if we really need to know. We are going to ask applicants. We are going to ask licensees. The major value of this program is to be able to know whether they are giving us a decent answer.

DR. VON REISEMANN: Right.

DR. SIESS: And if you make some of these simpler tests and you find out your analyses cannot predict what happened to them, then we know darn good and well that what somebody brings in as an answer is not going to be right, either. So it is finding the deficiencies in the analysis and, if possible, eliminating them.

Just when you say you cannot reproduce all the details that are in there, and that is not your job to reproduce them -- we are going to have to ask people to do their own calculations, and we want to know where the pitfalls are, and when they come in, we want to know where the bodies are buried.

DR. ZUDANS: That is a very good point.

DR. SIESS: We have got to get that out of this program.

DR. ZUDANS: Very good. Now, that would be good even if we did have DCC.

DR. SIESS: That is right. Oh, yes.

DR. VON RIESEMANN: In this area, as I mentioned, we are looking at material properties, welds, tie-down system, penetrations, and this question I think you were addressing, Professor Siess, the connection between a cylindrical shell and the base liner.

Well, to briefly summarize the current status, we have the drawings now for the 32nd scale and the 8th scale just about completed. A test facility and equipment and instrumentation are being readied. We are doing the analyses on the axisymmetric pressure case and also looking at separate effects.

The last sheet you have is simply a summary that the program is combined with an experimental/analytical program, and I think I will leave it at that. We can argue about the words here, I guess, for the rest of the evening, but we are looking at internal pressurization load and earthquake loadings.

I might say that we are looking at, experimentally, the behavior up to what I call ultimate. The analytical work might end up, as was mentioned this morning, appropriate only some limit beyond the yield point, because we all know that ultimate analyses are fairly complicated.

But it also might turn out that you might not have to do a very complicated analysis to come up with a handle on the ultimate load. This has been shown on simple tests on cylinders with nozzles, that very simple, after the fact, techniques were appropriate.

DR. SIESS: I look at this summary, which is a statement of objectives, and I would revise it to read, "A program to evaluate the reliability of prediction methods. for determining ultimate capacity of containments," and I think I would add two words there, "the capacity of containments to contain."

(Laughter.)

Just to keep reminding us that that is what containments are doing and not worrying about their structural capability, for example.

DR. VON RIESEMANN: Right.

DR. SIESS: And the earthquake loadings, I will leave for FY 84 or 85.

Do you have any more questions?

DR. ZUDANS: No, sir.

DR. SIESS: Well, thank you very much, Walt. We will be interested in following this. We will come down and watch you test on TV sometime. I assume you are going to have closed circuit to look at that thing.

DR. VON RIESEMANN: Oh, yes, we will have that all set up for you. We will not let you see the first test.

DR. SIESS: Oh, that will be the most interesting one.

(Laughter.)

DR. VON RIESEMANN: Yes, I know.

DR. ZUDANS: It might not work.

DR. SIESS: I think Sozan's first test on those PCRV's, his rubber liner failed, which is, I think, standard -- you are at least starting with a steel one. That is a big help.

DR. ZUDANS: The test will probably wind up by not being able to maintain the pressure at some point, and nothing visible on the structure.

DR. SIESS: Well, that is not so likely on the steel ones, I think, but it sure becomes a high probability on the concrete ones.

DR. ZUDANS: Are we finished now?

DR. SIESS: We are adjourned.

(Whereupon, at 5:00 p. m., the meeting was concluded.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

____

in the matter of: ACRS/Subcommittee on Structural Engineering

Date of Proceeding: July 1, 1981

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 Commission.

Michael Connolly

Official Reporter (Typed)

Michael Connolly

Official Reporter (Signature)

INTRODUCTORY STATEMENT BY ACRS SUBCOMMITTEE CHAIRMAN
STRUCTURAL ENGINEERING
JULY 1, 1981

The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Structural Engineering.

I'm C. Siess, Subcommittee Chairman.

The other ACRS Members present today are M. Bender and D. Ward

We also have present ACRS Subcommittee consultant: Z. Zudans.

The purpose of this meeting is to discuss containment structural integrity programs being performed at Sandia and LANL.

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

Mr. R. Savio is the Designated Federal Employee for this meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on June 16, 1981.

A transcript of the meeting is being kept and it is requested that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard.

We have received no written statements nor requests for time to make oral statements from any member of the public.

Do any members of the Subcommittee have any comments?

We will now proceed with the meeting, and I call upon Mr.______________ of the__________________to begin.

STRUCTURAL MARGINS TO FAILURE PROGRAMS

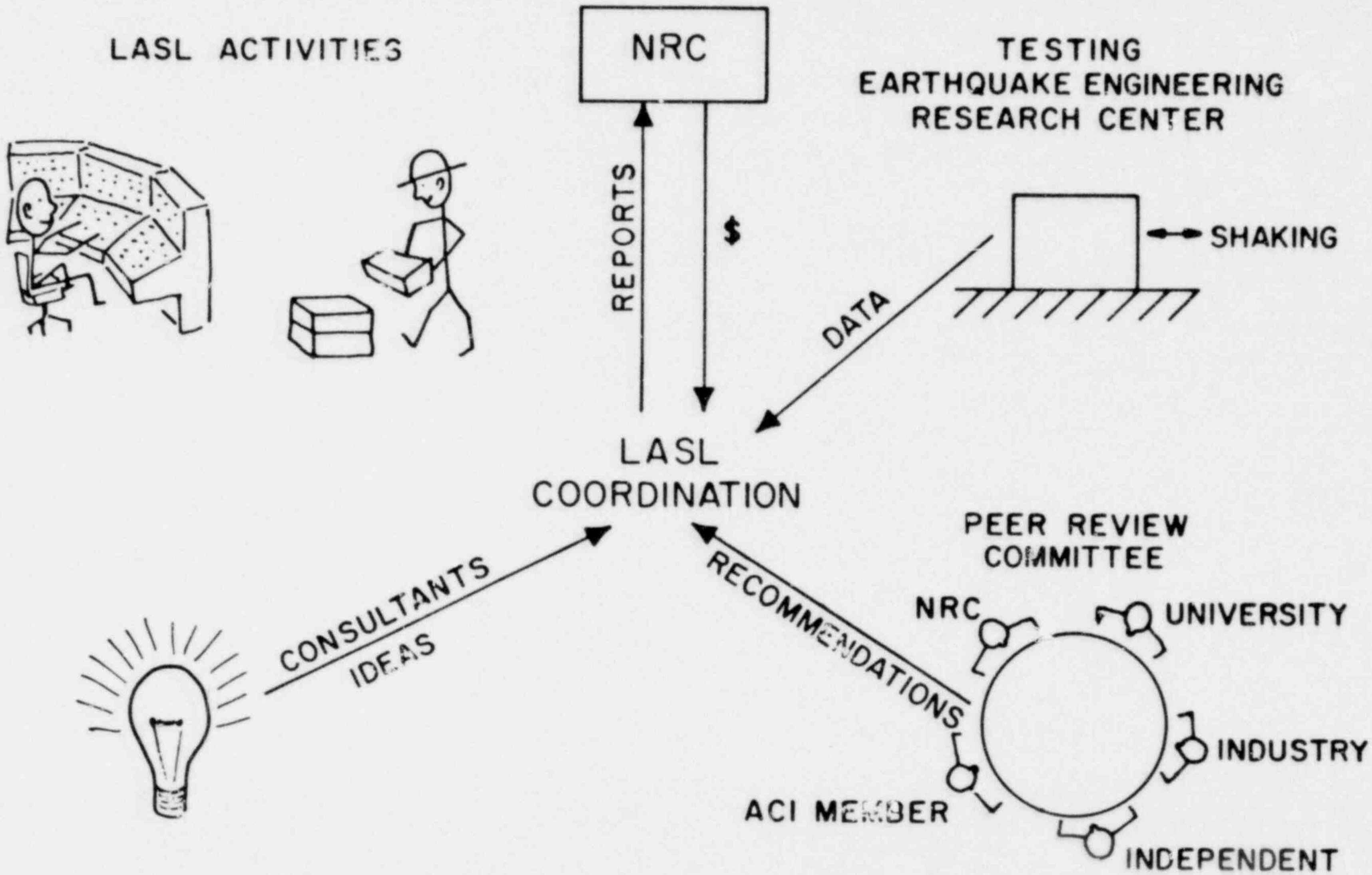
<u>Program Objectives</u>

To supply experimental and analytical information needed to assess the ultimate load behavior of the following Category I structures:

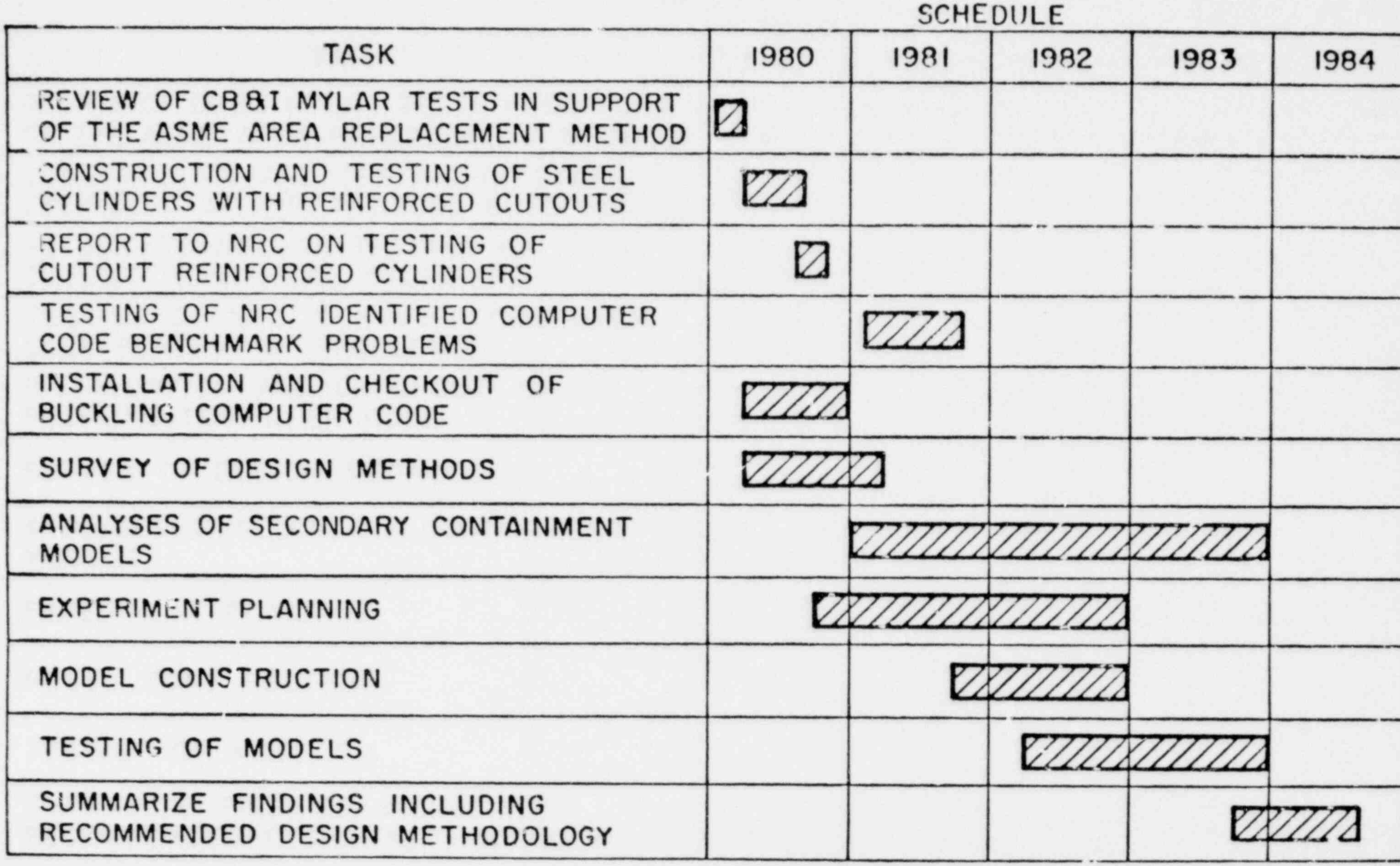
1. mixed concrete and steel nuclear plant buildings under seismic loads.
2. steel containment systems subjected to loads causing structural instability.

Los Alamos

STRUCTURAL MARGINS PROGRAM

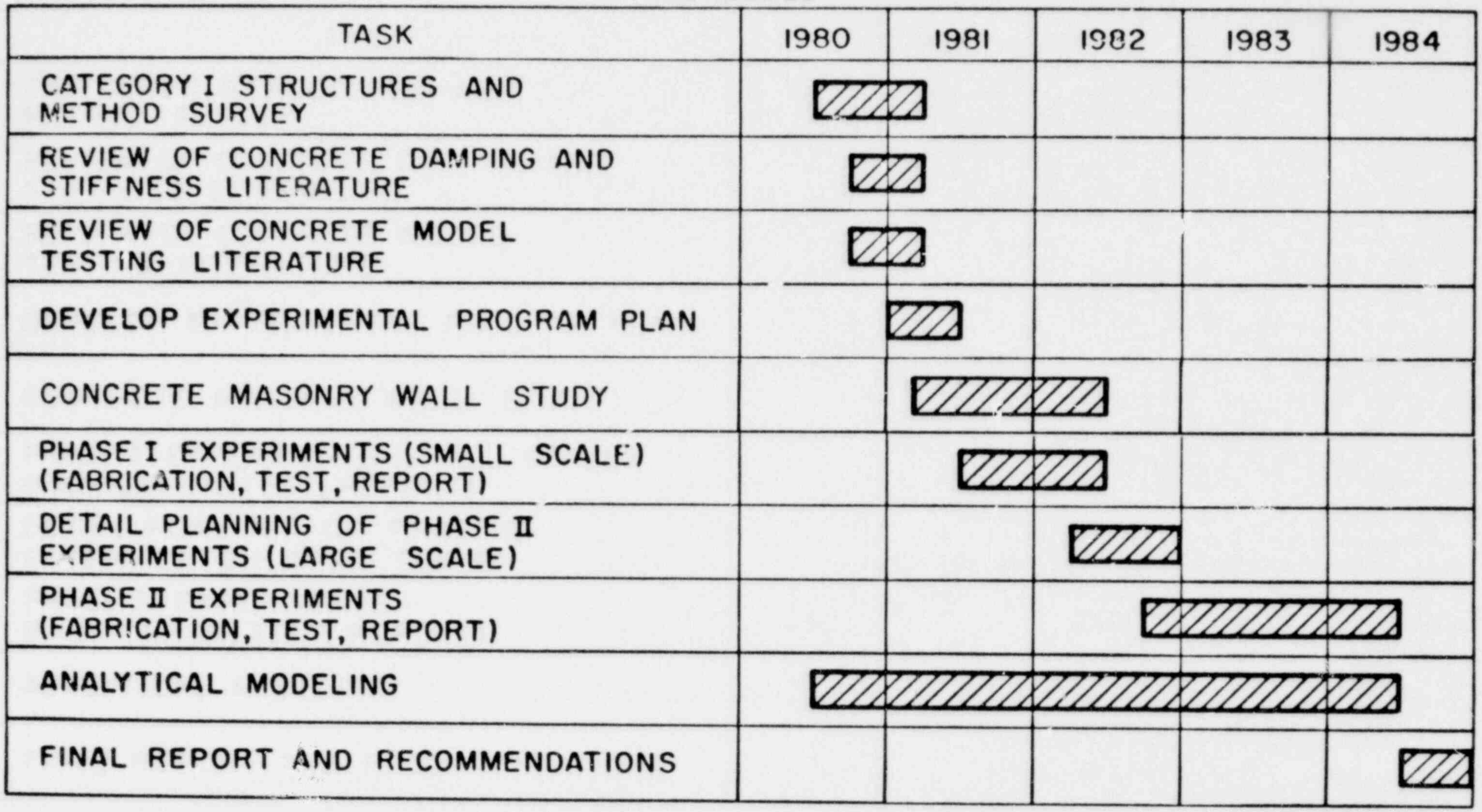


CONTAINMENT BUCKLING PROGRAM

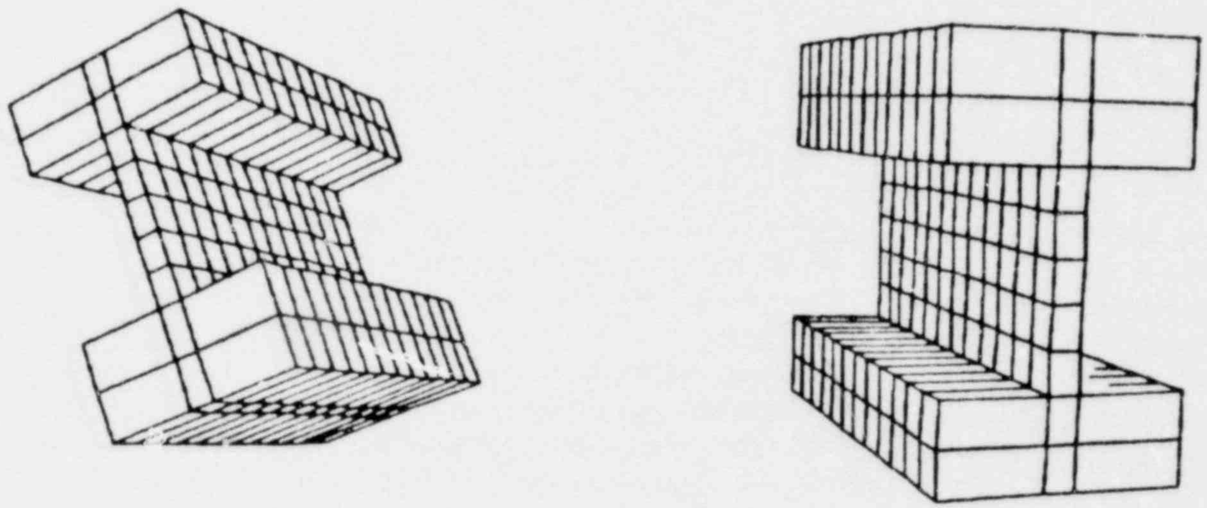


SAFETY MARGINS OF CATEGORY I STRUCTURES PROGRAM

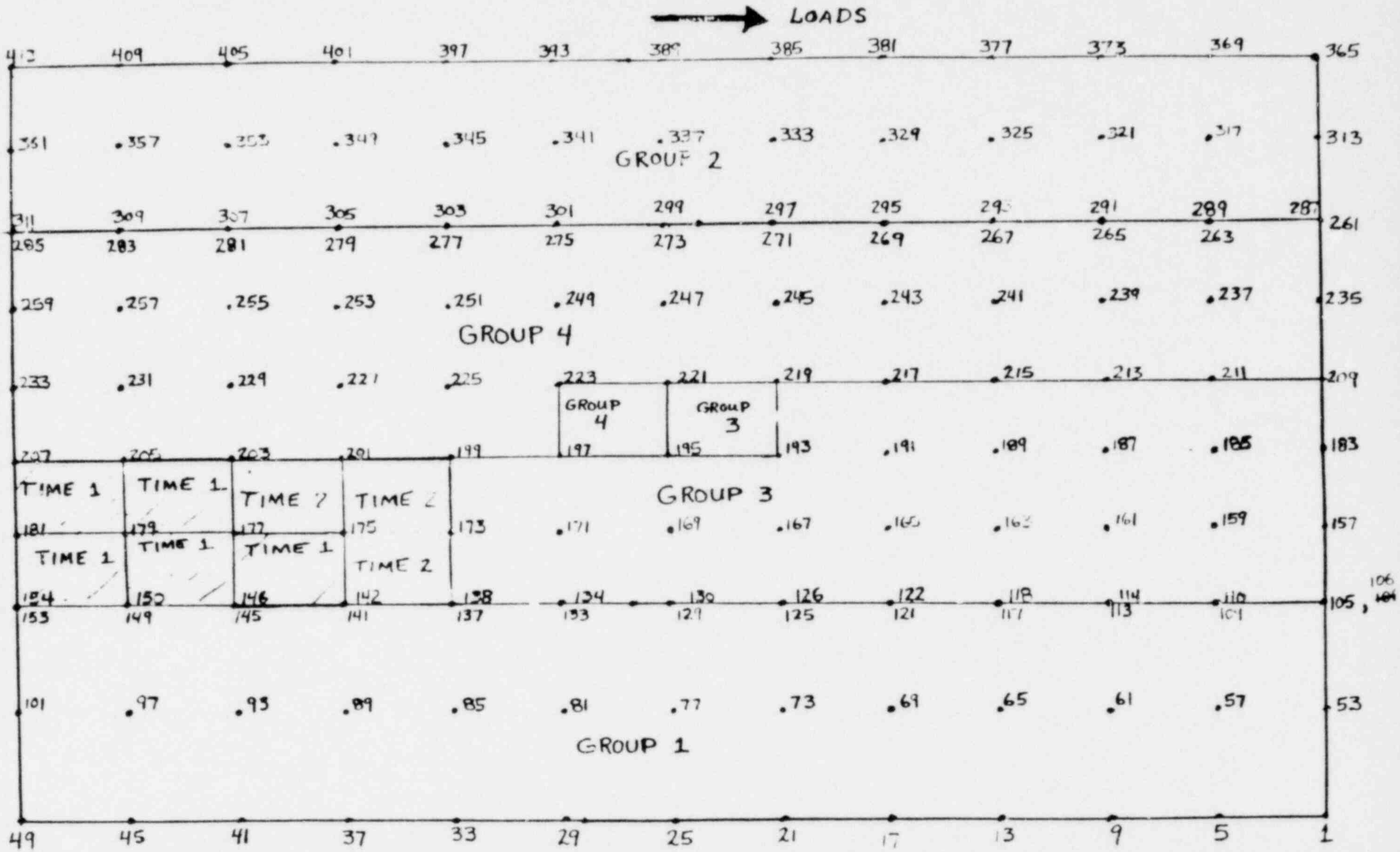
SCHEDULE



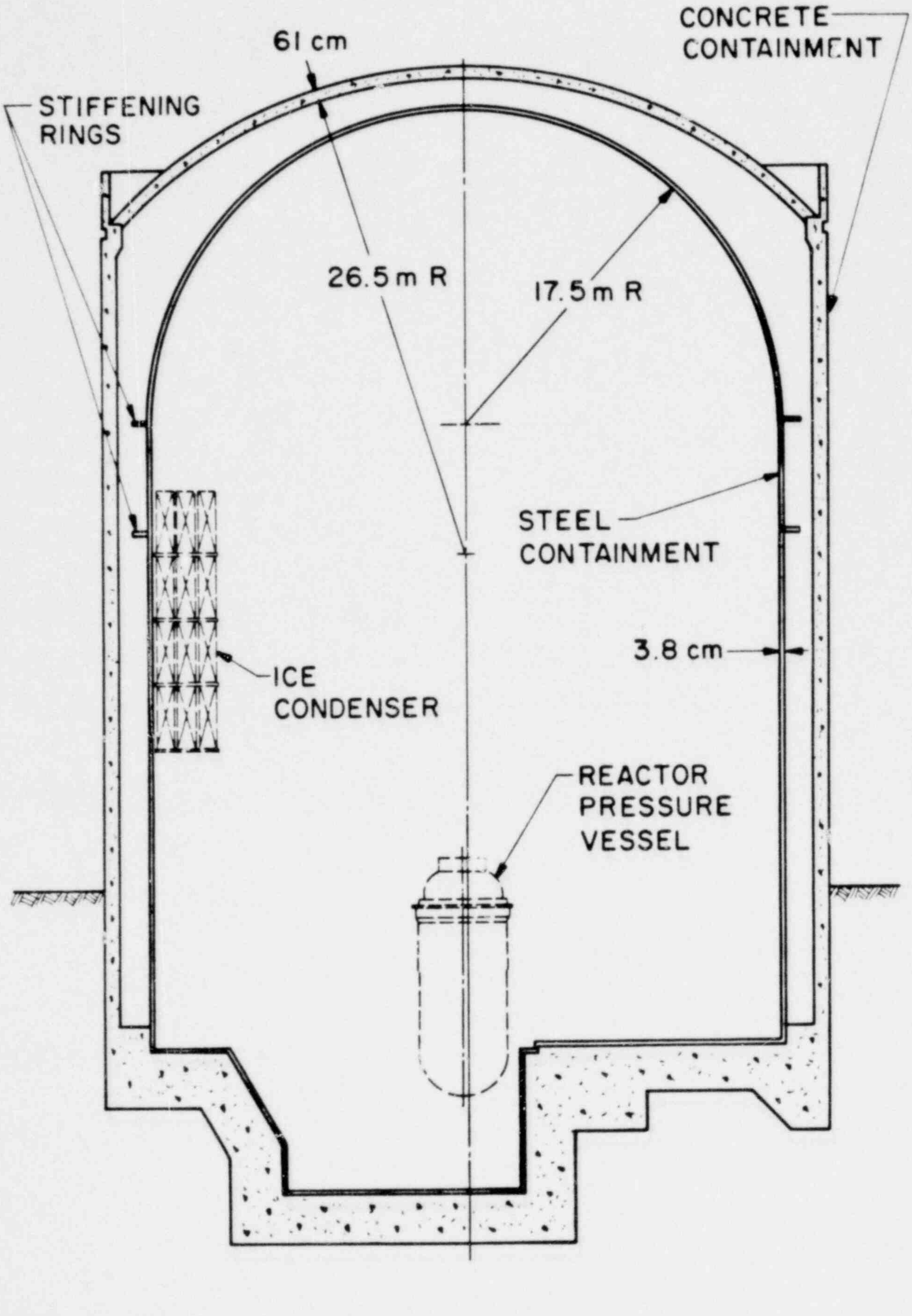
SHEAR WALL MODEL



PREDICTION OF SHEAR WALL CRACKS



Anderson (cont.)

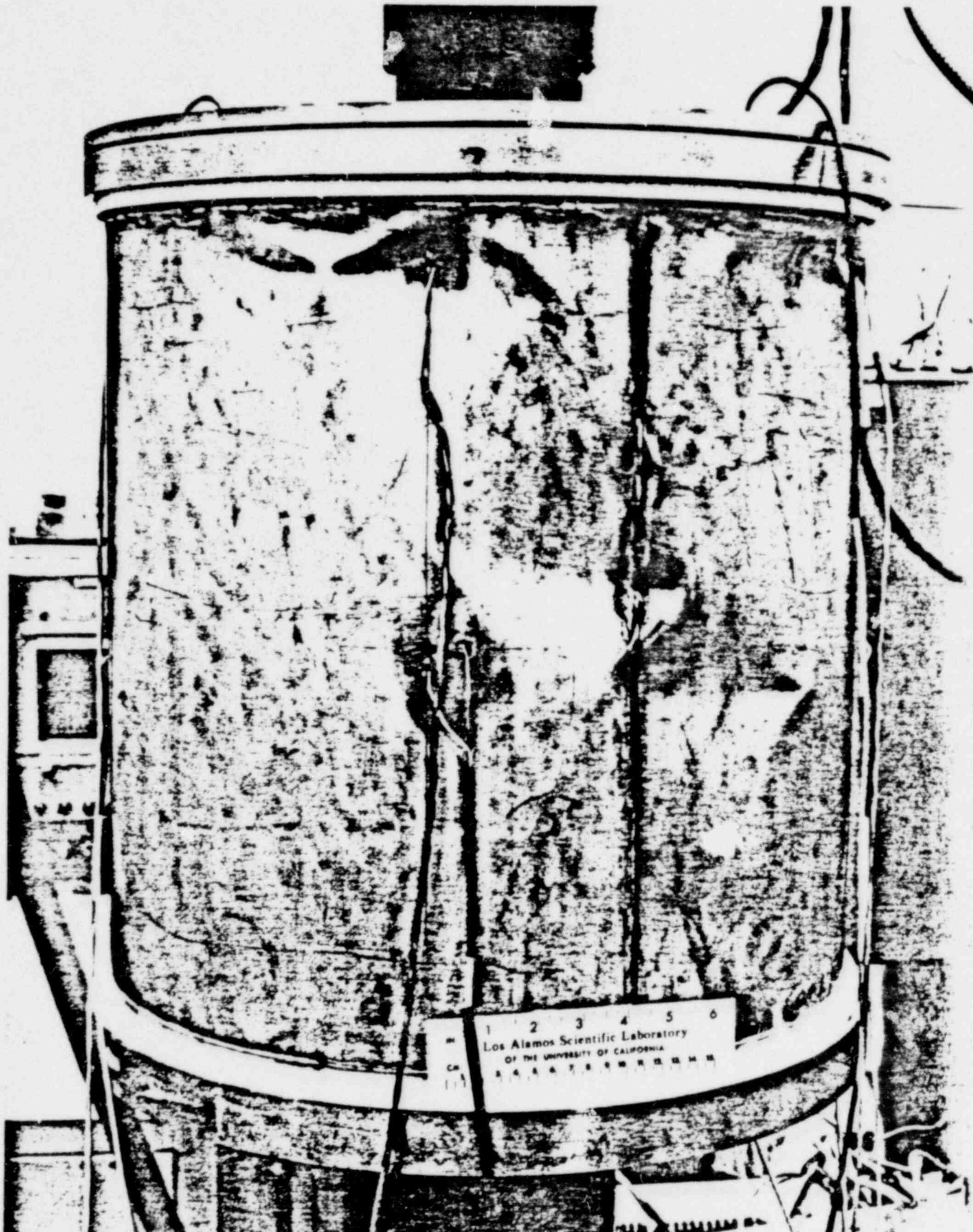


SUMMARY OF ASME AREA REPLACEMENT METHOD BUCKLING INVESTIGATION

PREMISE: C. D. MILLER – C.B.I.

– MYLAR TEST OF A SINGLE CYLINDER

– IF ARM RULE IS FOLLOWED, THEN BUCKLING STRENGTH OF A PENETRATED CYLINDER WILL BE INCREASED ABOVE VALUE OF UNPENETRATED CYLINDER.



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Los Alamos Scientific Laboratory
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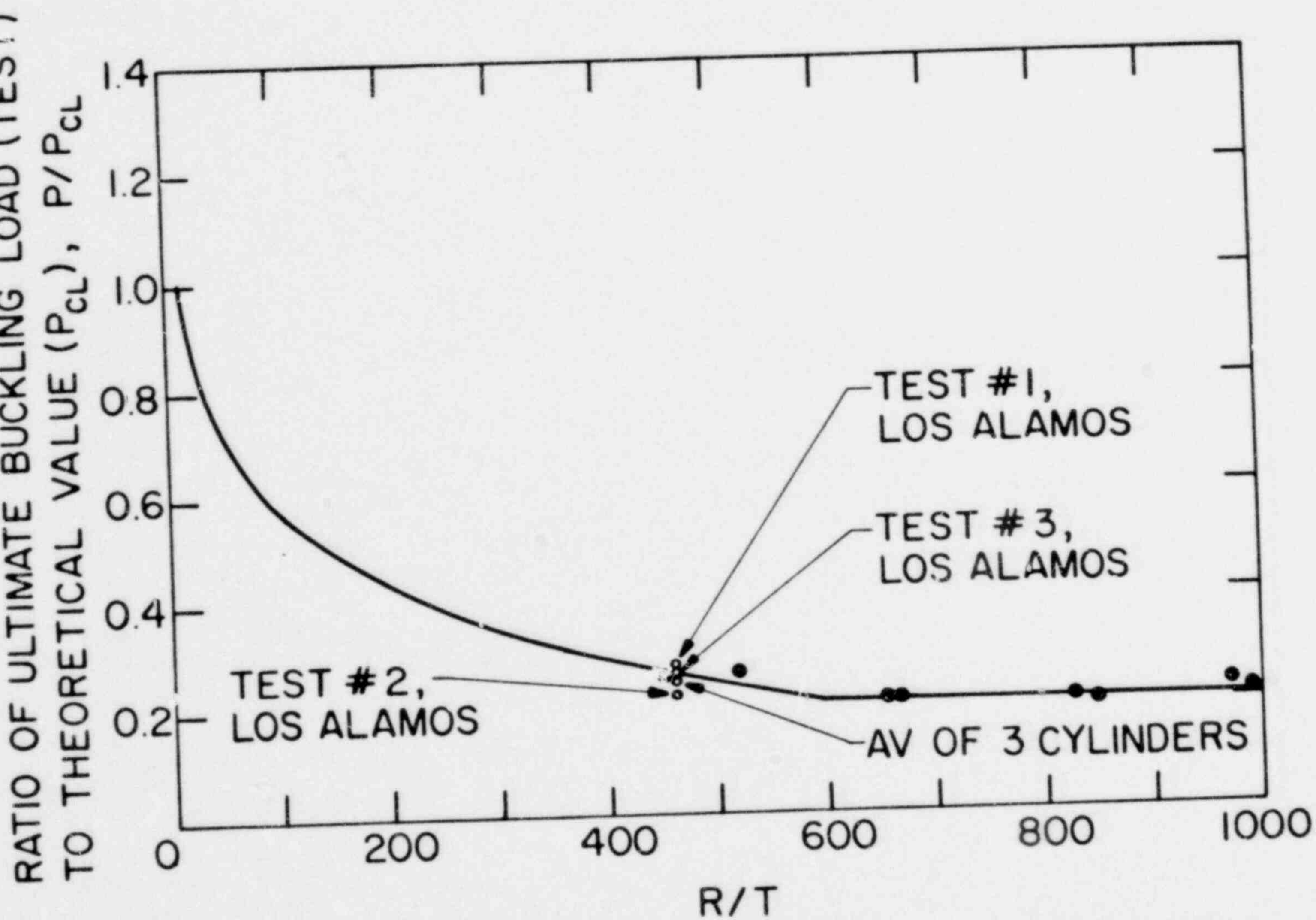
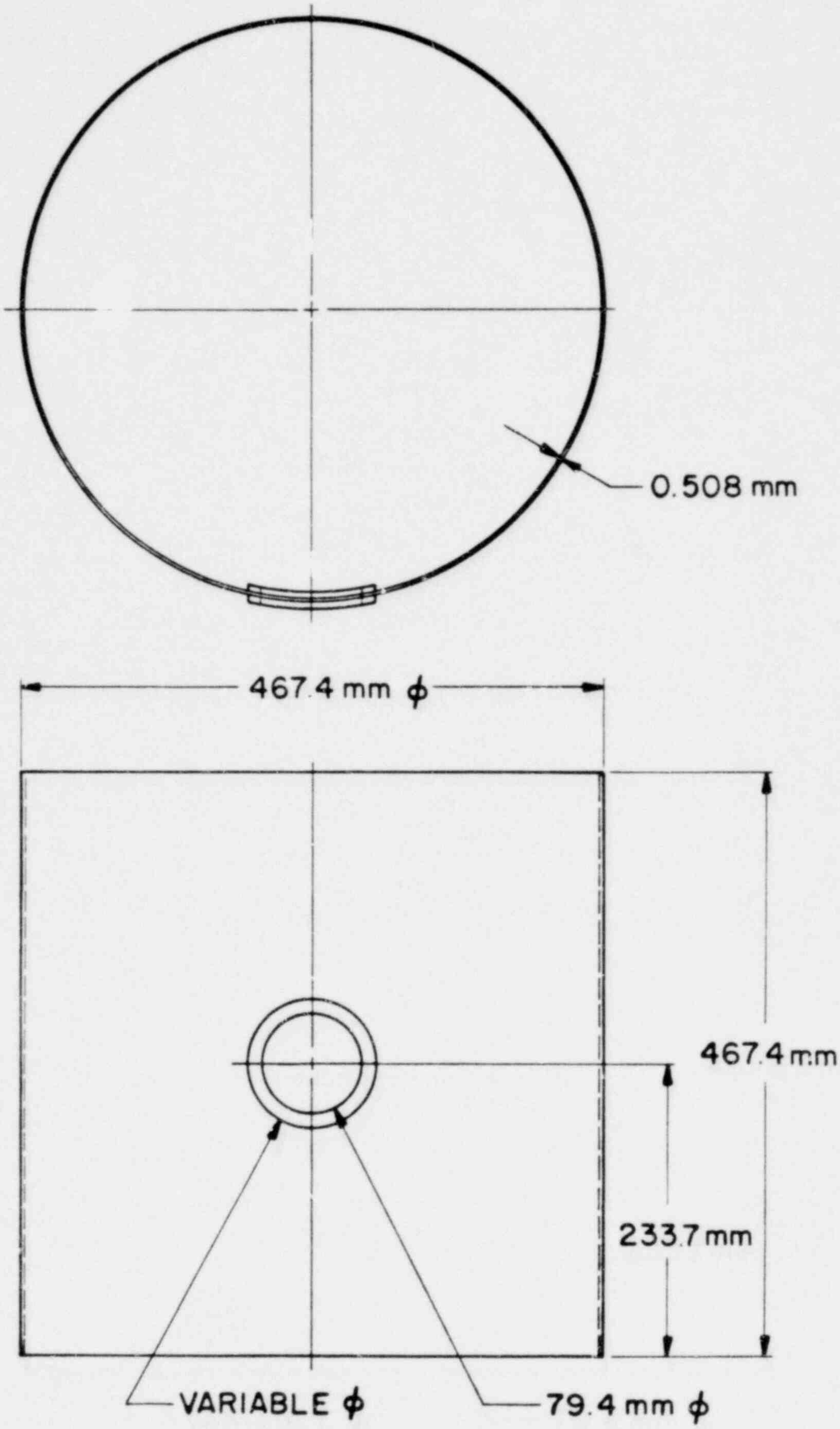
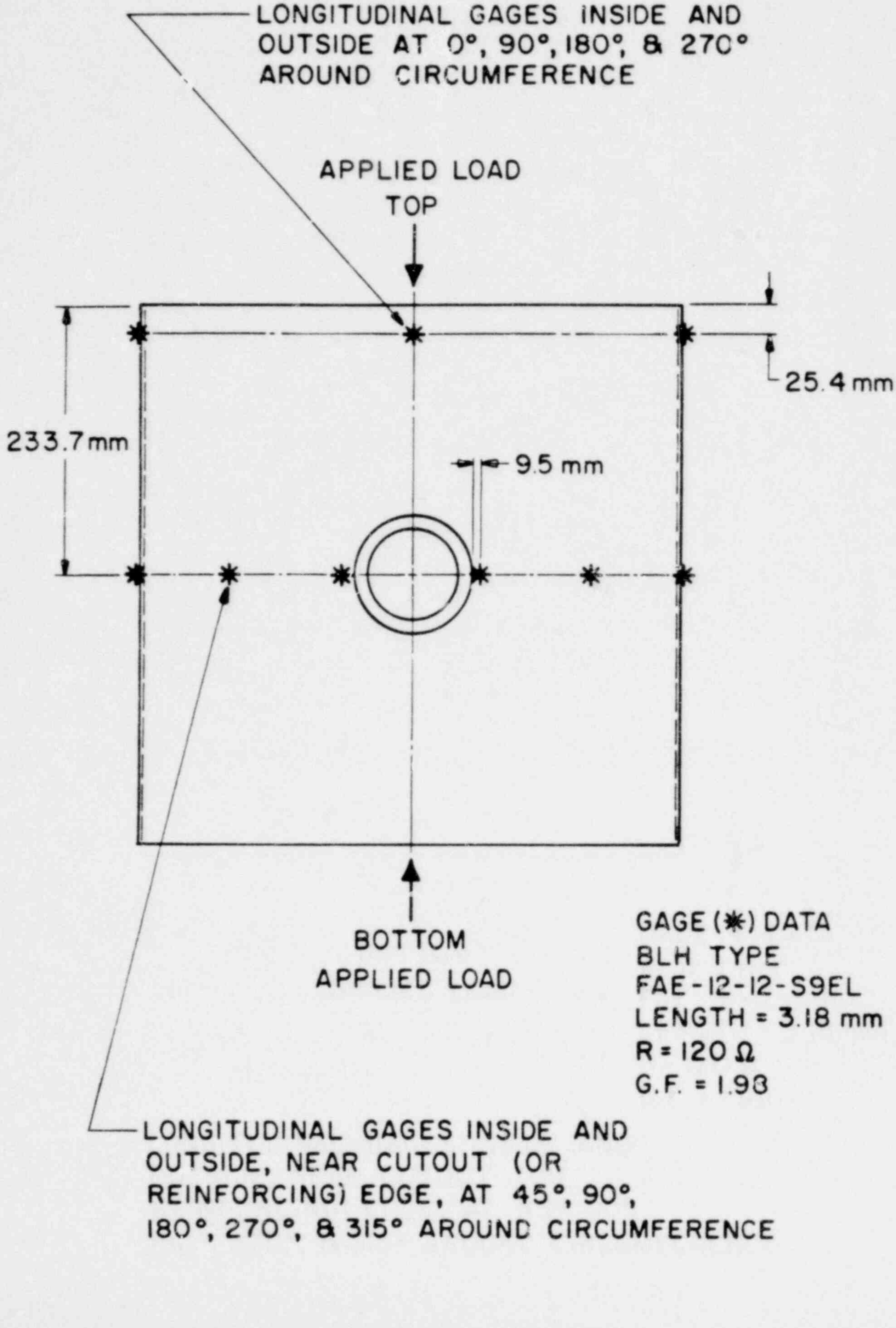
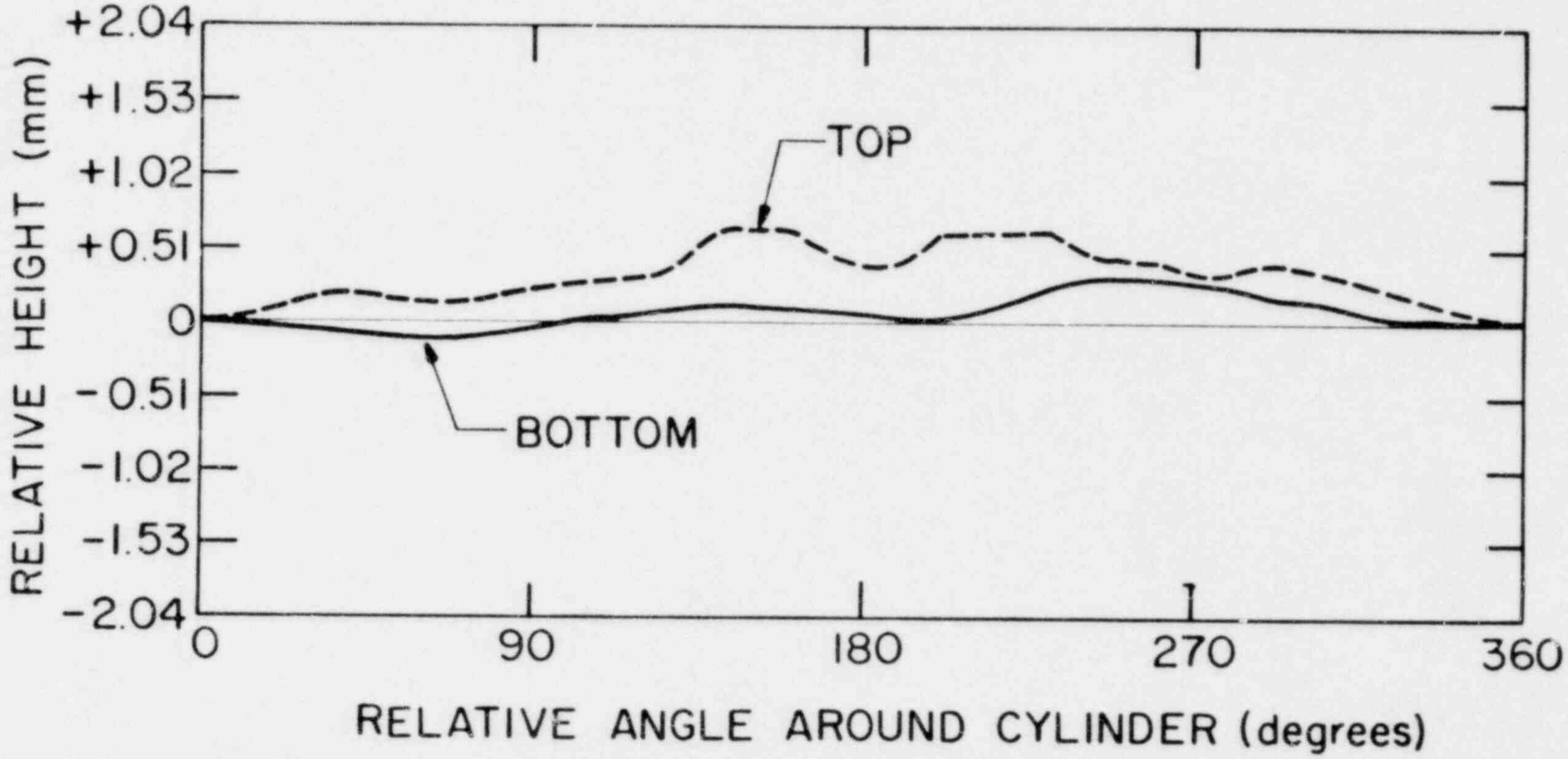

RATIO OF ULTIMATE BUCKLING LOAD (TEST) TO THEORETICAL VALUE (P_{CL}), P/P_{CL}
1.4
1.2
1.0
0.8
0.6
0.4
0.2
0
200
400
600
800
1000
R/T
TEST #1, LOS ALAMOS
TEST #3, LOS ALAMOS
TEST #2, LOS ALAMOS
AV OF 3 CYLINDERS

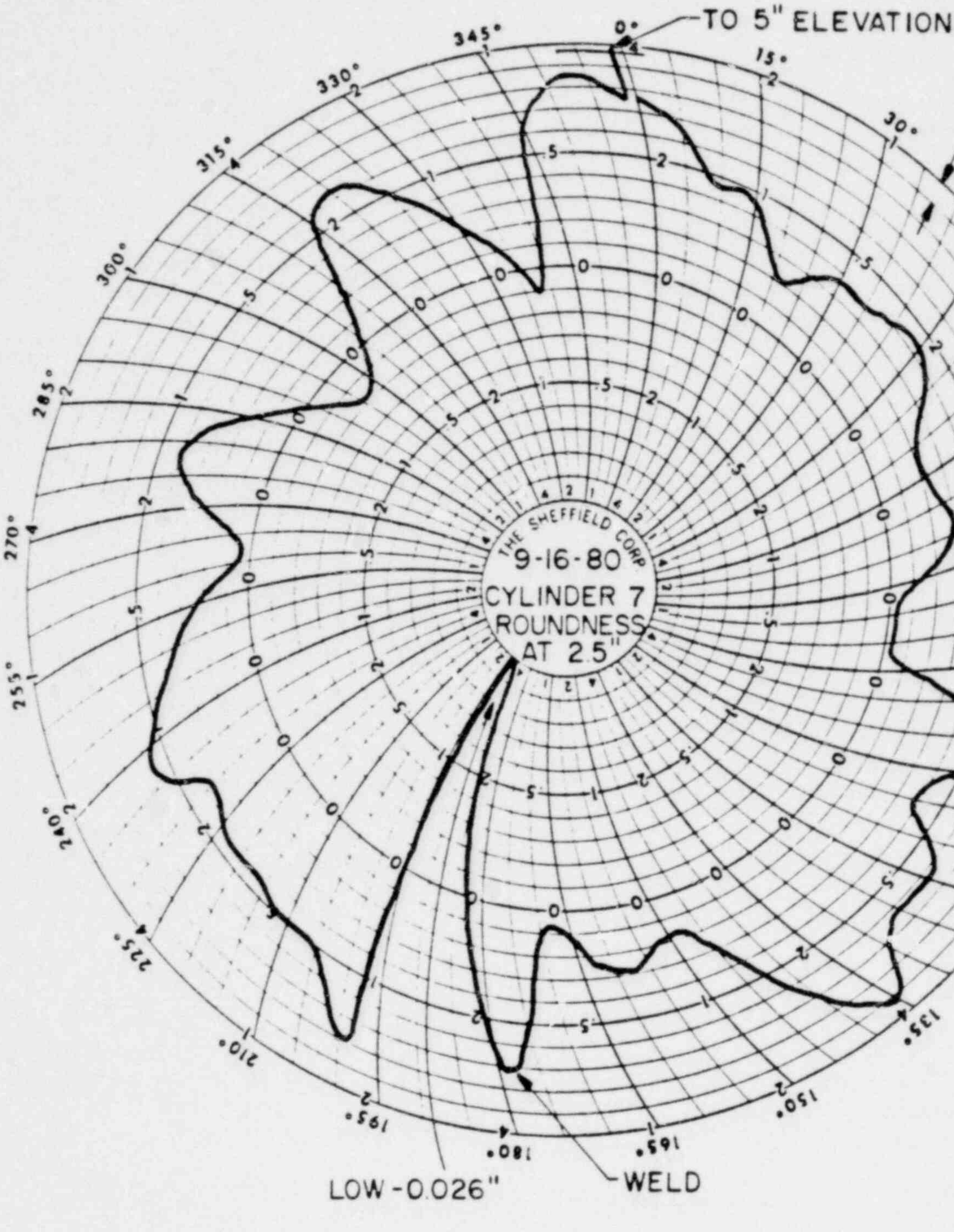


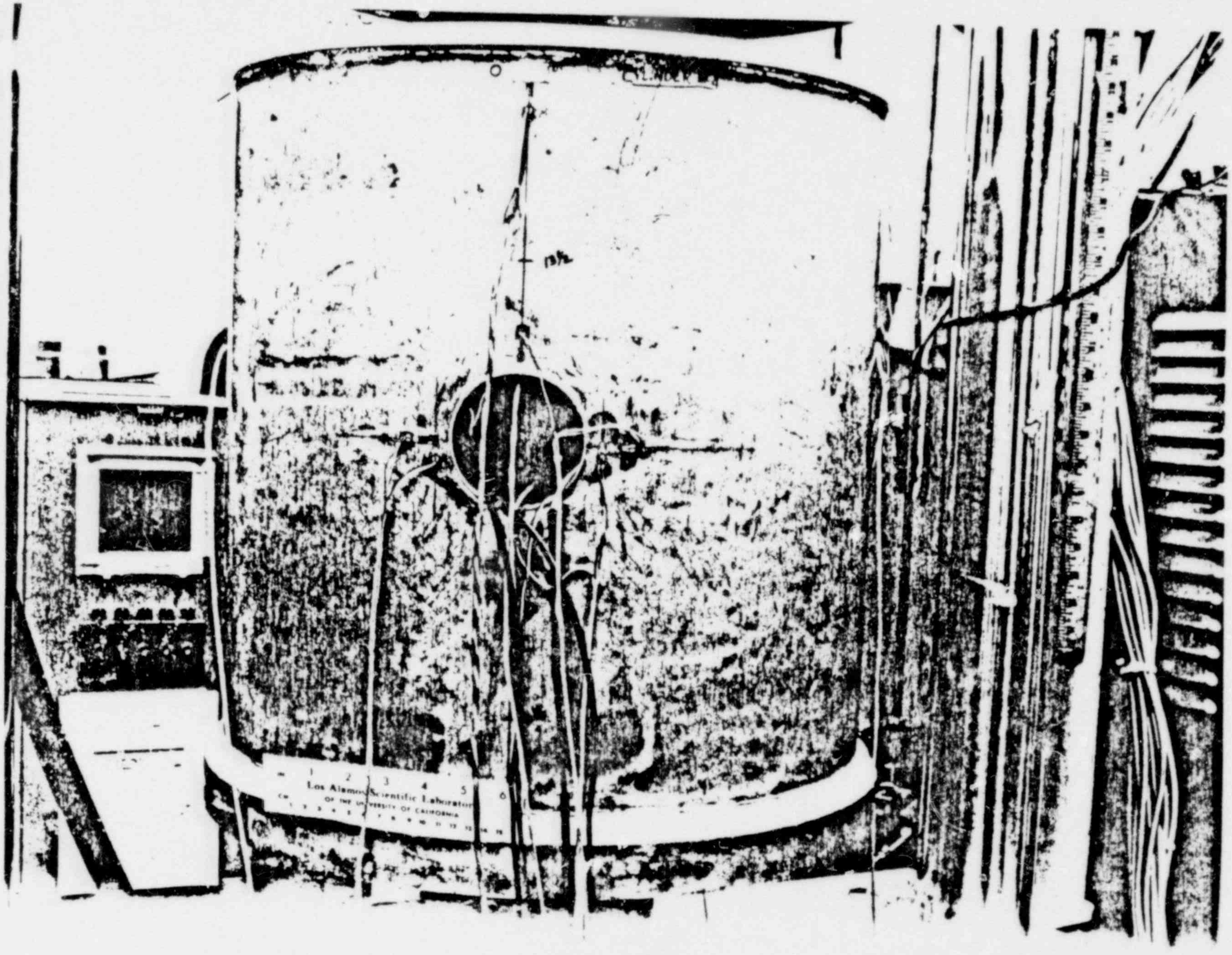
MATERIAL : STAINLESS STEEL STRIP,
WALL TOLERANCE < 0.025 mm


LONGITUDINAL GAGES INSIDE AND
OUTSIDE AT 0°, 90°, 180°, & 270°
AROUND CIRCUMFERENCE
APPLIED LOAD
TOP
25.4 mm
233.7mm
9.5 mm
BOTTOM
APPLIED LOAD
GAGE (✱) DATA
BLH TYPE
FAE-12-12-S9EL
LENGTH = 3.18 mm
R = 120 Ω
G.F. = 1.98
LONGITUDINAL GAGES INSIDE AND
OUTSIDE, NEAR CUTOUT (OR
REINFORCING) EDGE, AT 45°, 90°,
180°, 270°, & 315° AROUND CIRCUMFERENCE



+2.04
+1.53
+1.02
+0.51
0
-0.51
-1.02
-1.53
-2.04
RELATIVE HEIGHT (mm)
TOP
BOTTOM
0
90
180
270
360
RELATIVE ANGLE AROUND CYLINDER (degrees)


TO 5" ELEVATION
0.002"
THE SHEFFIELD CORP
9-16-80
CYLINDER 7
ROUNDNESS
AT 2.5"
LOW-0.026"
WELD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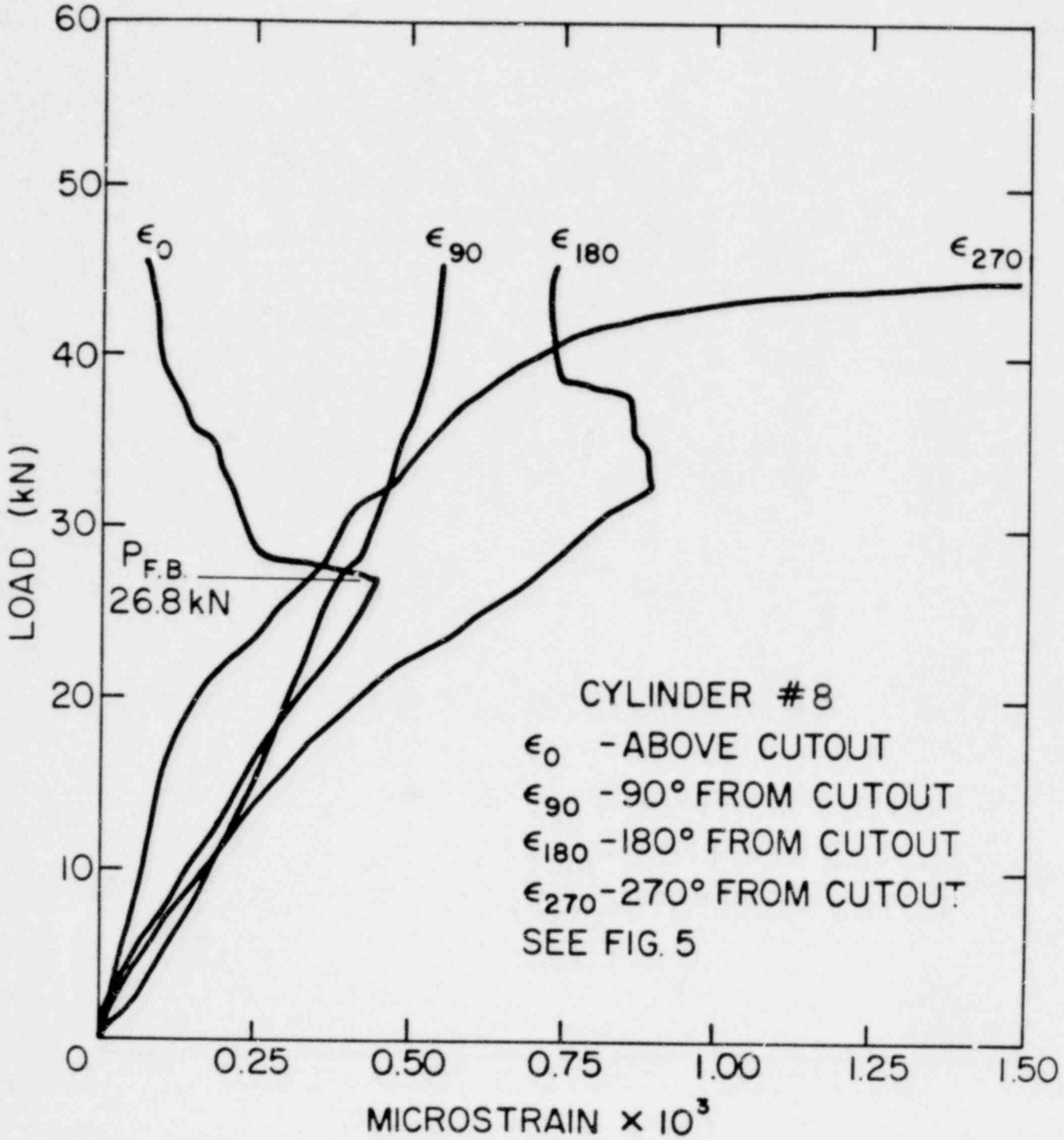
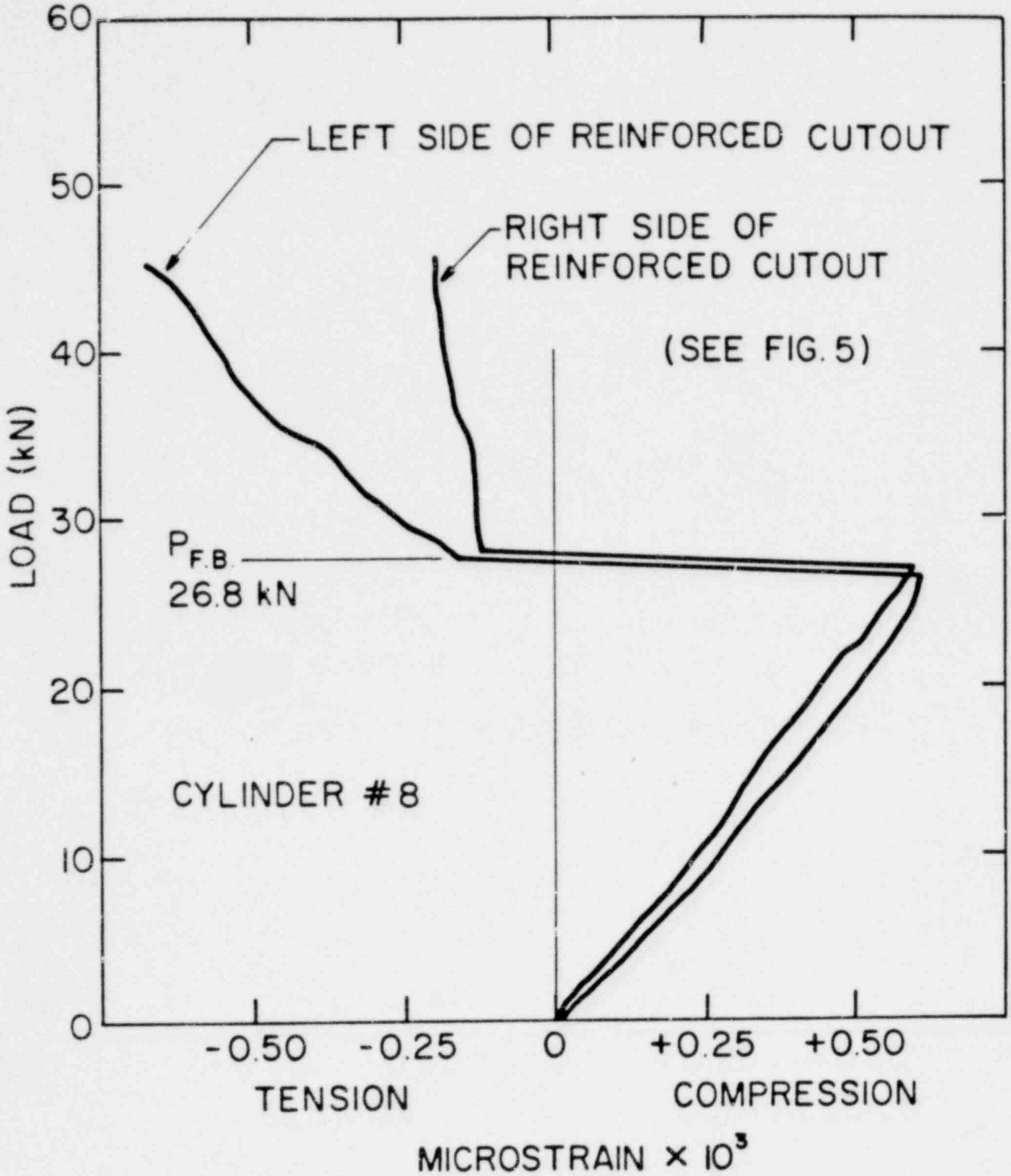
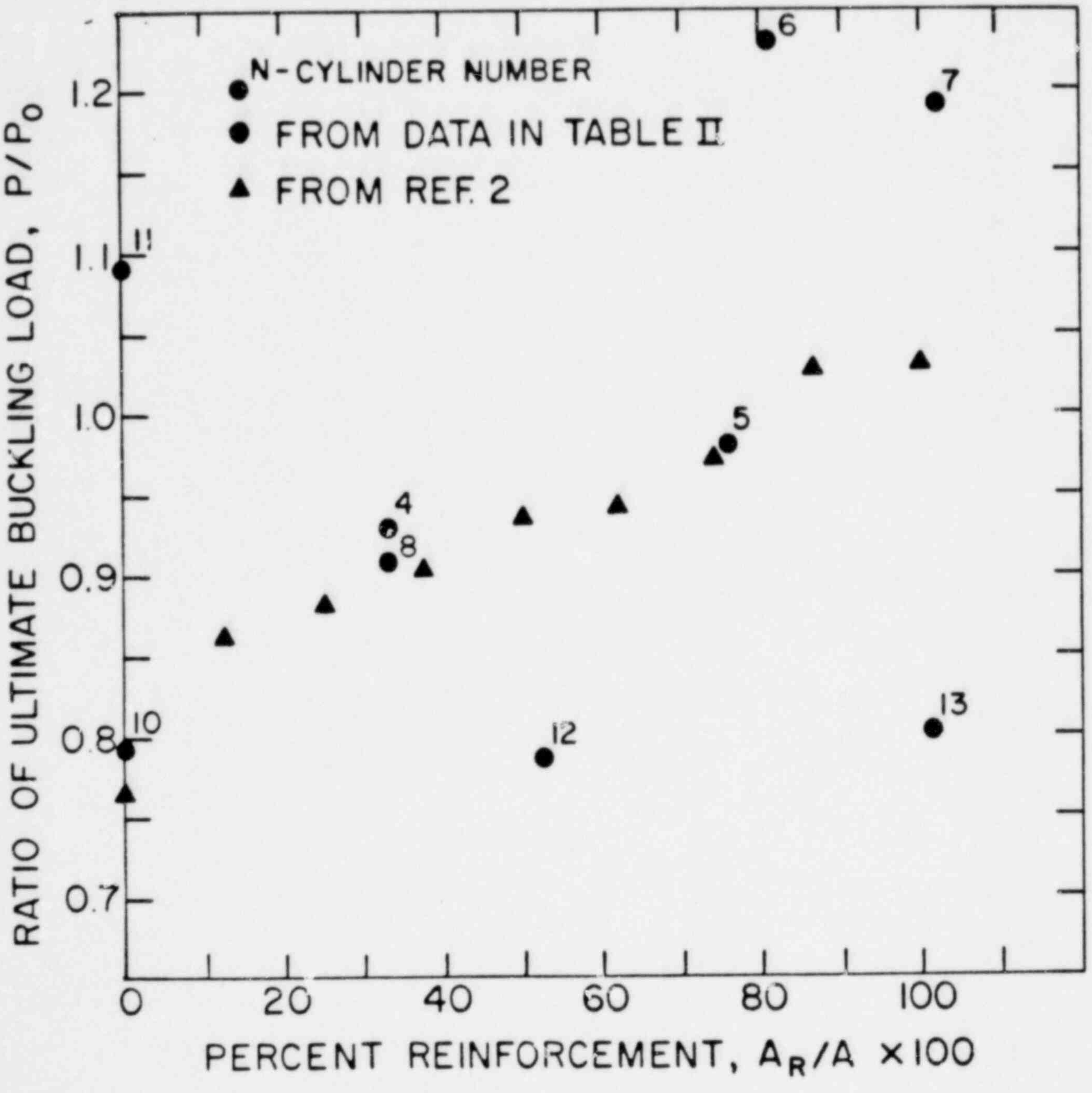
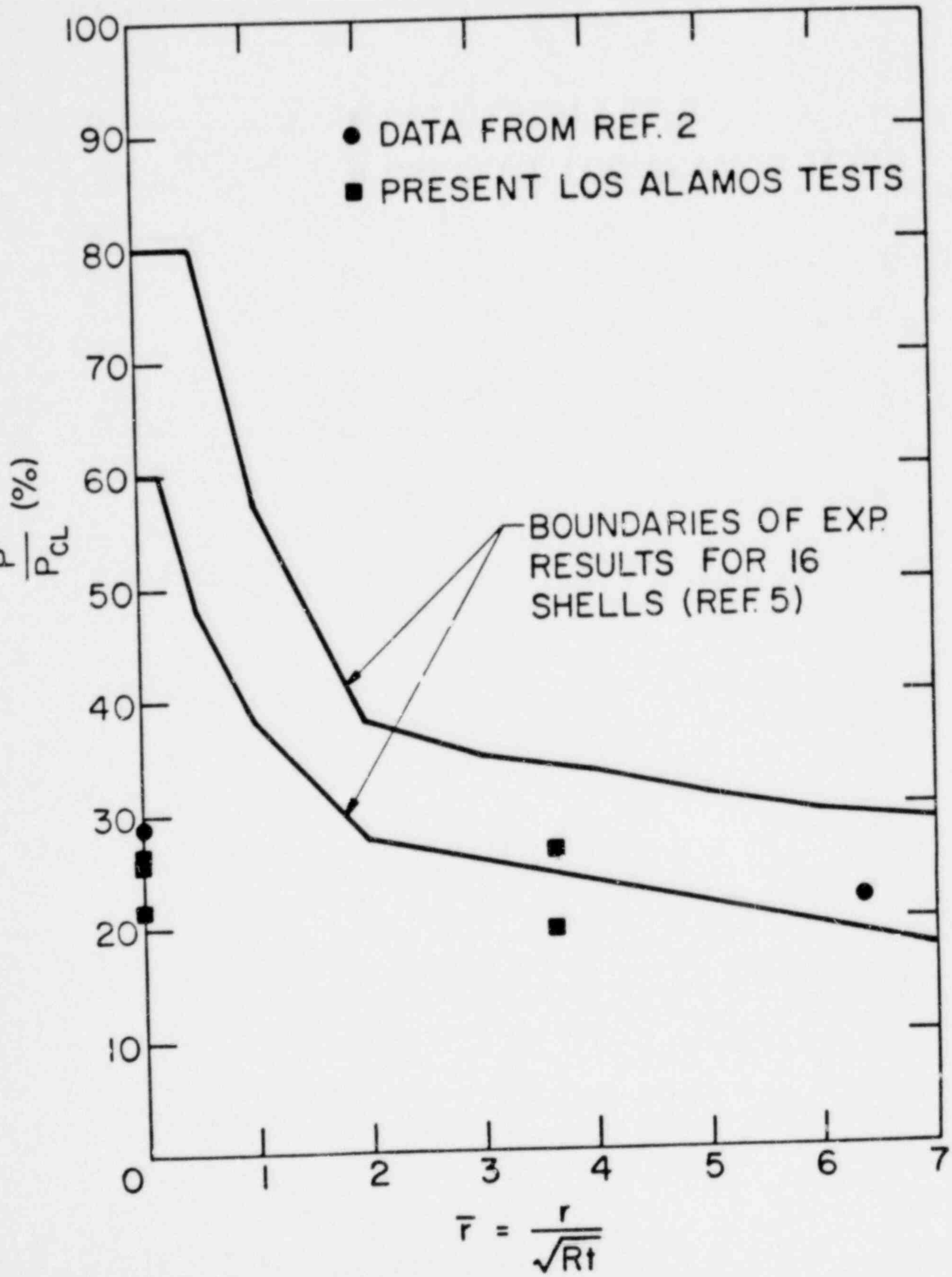


LOAD (kN)
60
50
40
30
20
10
0
0.25
0.50
0.75
1.00
1.25
1.50
MICROSTRAIN × 10³
ϵ_0
ϵ_{90}
ϵ_{180}
ϵ_{270}
$P_{F.B.}$
26.8 kN
CYLINDER #8
ϵ_0 - ABOVE CUTOUT
ϵ_{90} - 90° FROM CUTOUT
ϵ_{180} - 180° FROM CUTOUT
ϵ_{270} - 270° FROM CUTOUT
SEE FIG. 5

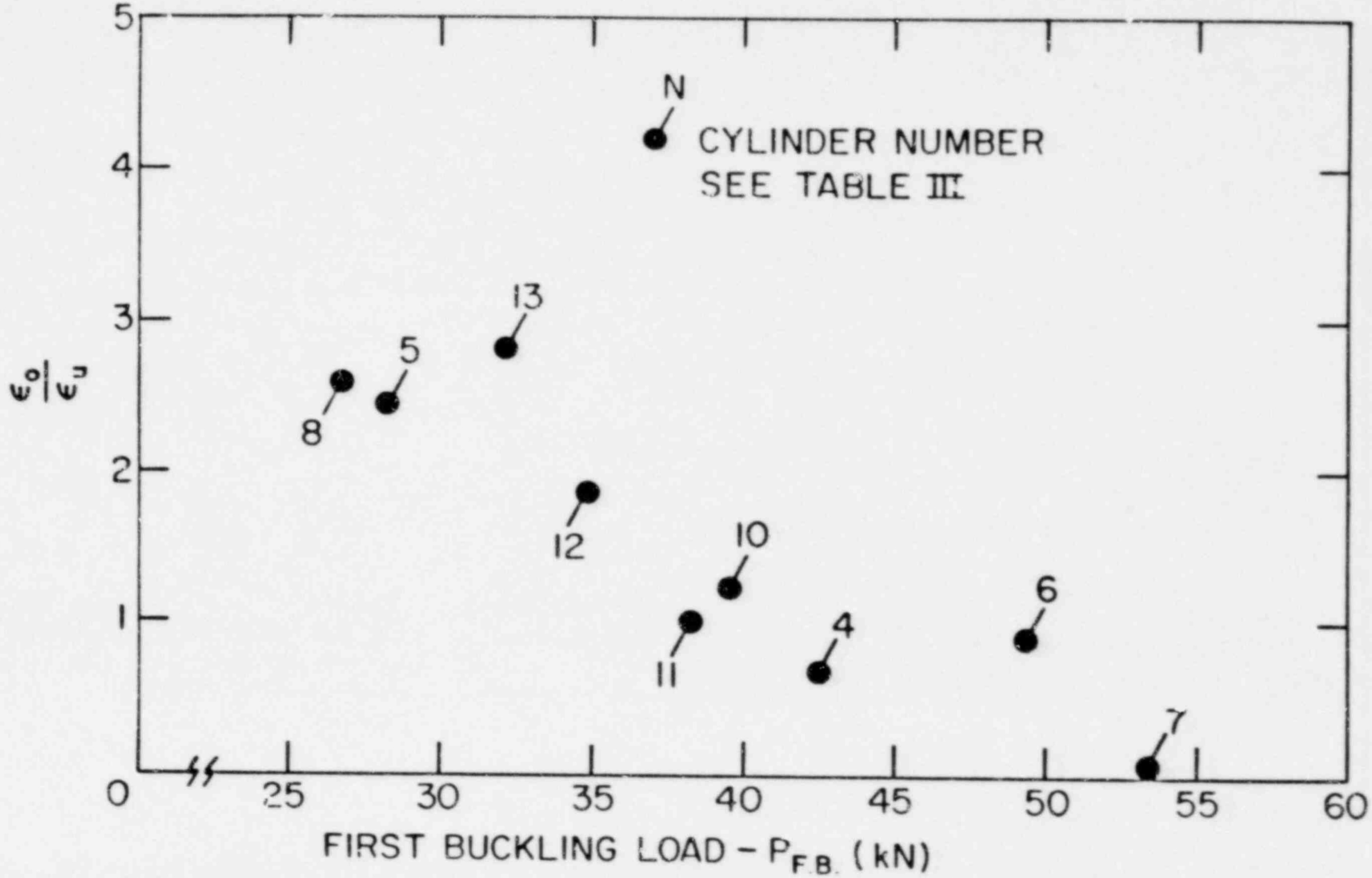


60
50
40
30
20
10
0
LOAD (kN)
LEFT SIDE OF REINFORCED CUTOUT
RIGHT SIDE OF REINFORCED CUTOUT
(SEE FIG. 5)
$P_{F.B}$
26.8 kN
CYLINDER #8
-0.50
-0.25
0
+0.25
+0.50
TENSION
COMPRESSION
MICROSTRAIN × 10^3


N-CYLINDER NUMBER
FROM DATA IN TABLE II
FROM REF. 2
RATIO OF ULTIMATE BUCKLING LOAD, P/P0
PERCENT REINFORCEMENT, AR/A ×100
1.2
1.1
1.0
0.9
0.8
0.7
0
20
40
60
80
100
4
5
6
7
8
10
11
12
13



100
90
80
70
60
50
40
30
20
10
0
1
2
3
4
5
6
7
● DATA FROM REF. 2
■ PRESENT LOS ALAMOS TESTS
BOUNDARIES OF EXP. RESULTS FOR 16 SHELLS (REF. 5)
$\frac{P}{P_{CL}}$ (%)
$\bar{r} = \frac{r}{\sqrt{Rt}}$


N
CYLINDER NUMBER
SEE TABLE III
13
5
8
12
10
11
4
6
7
0
1
2
3
4
5
25
30
35
40
45
50
55
60
FIRST BUCKLING LOAD - P F.B. (kN)

SMtoF - CB - current status cont.

COMPUTER CODE EVALUATION AND SELECTION

STAGSC1 - Structural Analysis of General Shells

- Lockheed code - (users group forming)
  - bifurcation buckling capability
  - nonlinear collapse anaylsis capability

BOSOR4&5 - Buckling and stress analysis of ring stiffened, branched Shells of Revolution

- Lockheed code
  - axisymmetric or nonaxisymmetric loading
  - elastic/plastic material capability

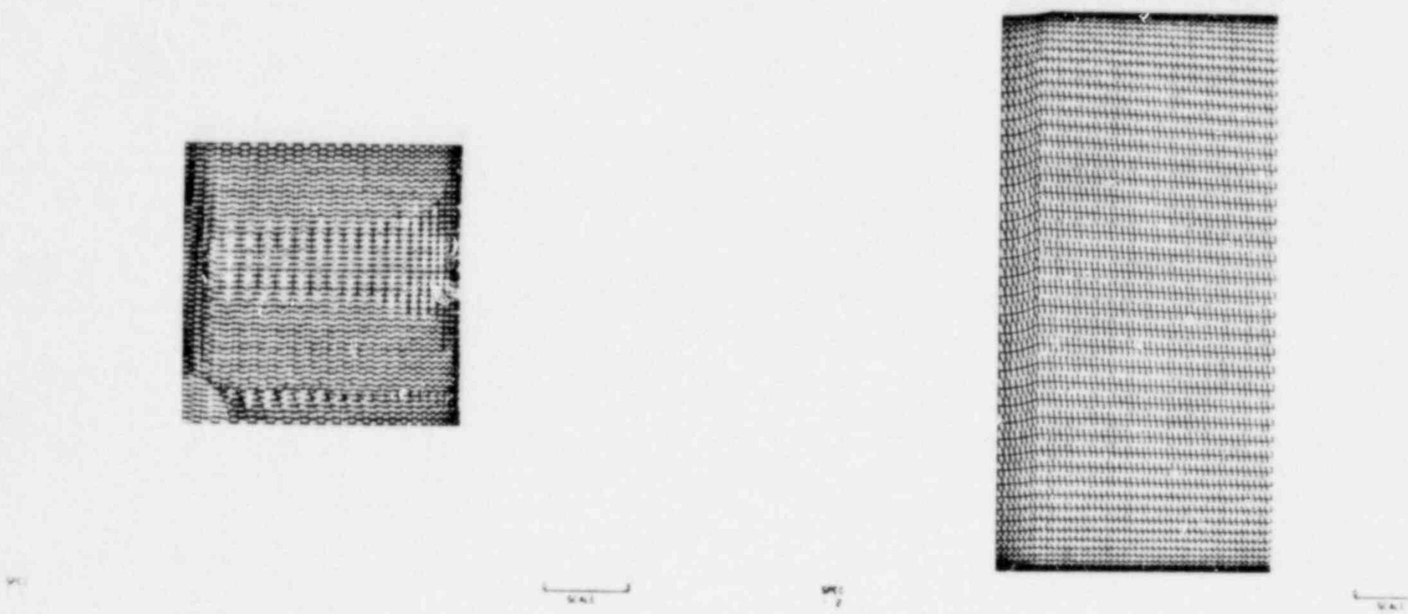
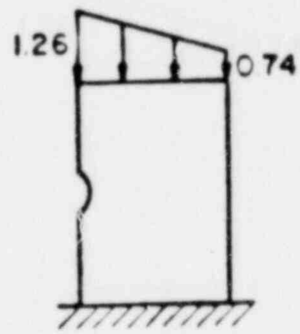
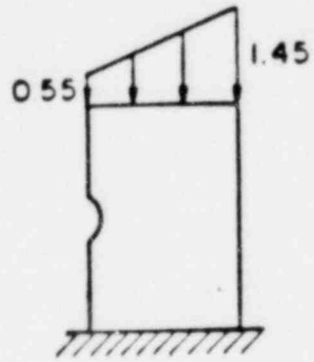


Fig. 12. Mesh used in SPAR analysis.

Fig. 13. Buckling mode predicted by SPAR.



a) TYPE I - HOLE OVERLOADED



b) TYPE II - HOLE UNDERLOADED

Fig. 15. Two types of nonuniform loading.

SUMMARY OF ANALYTICAL STUDIES

CASE	BUCKLING LOAD	P/P_0
PERFECT CYLINDER - UNIFORM LOAD	50,020	0.97
CYLINDER W/HOLE RBAR=3.5 (NO IMPERFECTION)	7,634	0.15
CYLINDER W/HOLE TYPE I IMPER.	7,804	0.16
CYLINDER W/HOLE TYPE II IMPER.	8,086	0.16
CYLINDER W/HOLE LOAD TYPE I	6,107	0.12
CYLINDER W/HOLE LOAD TYPE II	13,854	0.28
CYLINDER W/HOLE (NO IMPER.) (100 % REINFORCEMENT)	36,974	0.74

CONCLUSION

1. IF THE BUCKLING STRENGTH OF A CYLINDRICAL SHELL IS LOWERED BY A PENETRATION, THEN FOLLOWING THE ASME ARM RULE WILL INCREASE THE BUCKLING STRENGTH OF THE SHELL, BUT WILL NOT BRING IT BACK UP TO THE UNPENETRATED VALUE.

CONCLUSION

2. IF THE BUCKLING STRENGTH OF A CYLINDRICAL SHELL IS SO LOW THAT A PENETRATION DOES NOT LOWER IT FUTHER, THEN REINFORCING THE PENETRATION WILL HAVE LITTLE OR NO EFFECT ON THE BUCKLING STRENGTH OF THE SHELL.

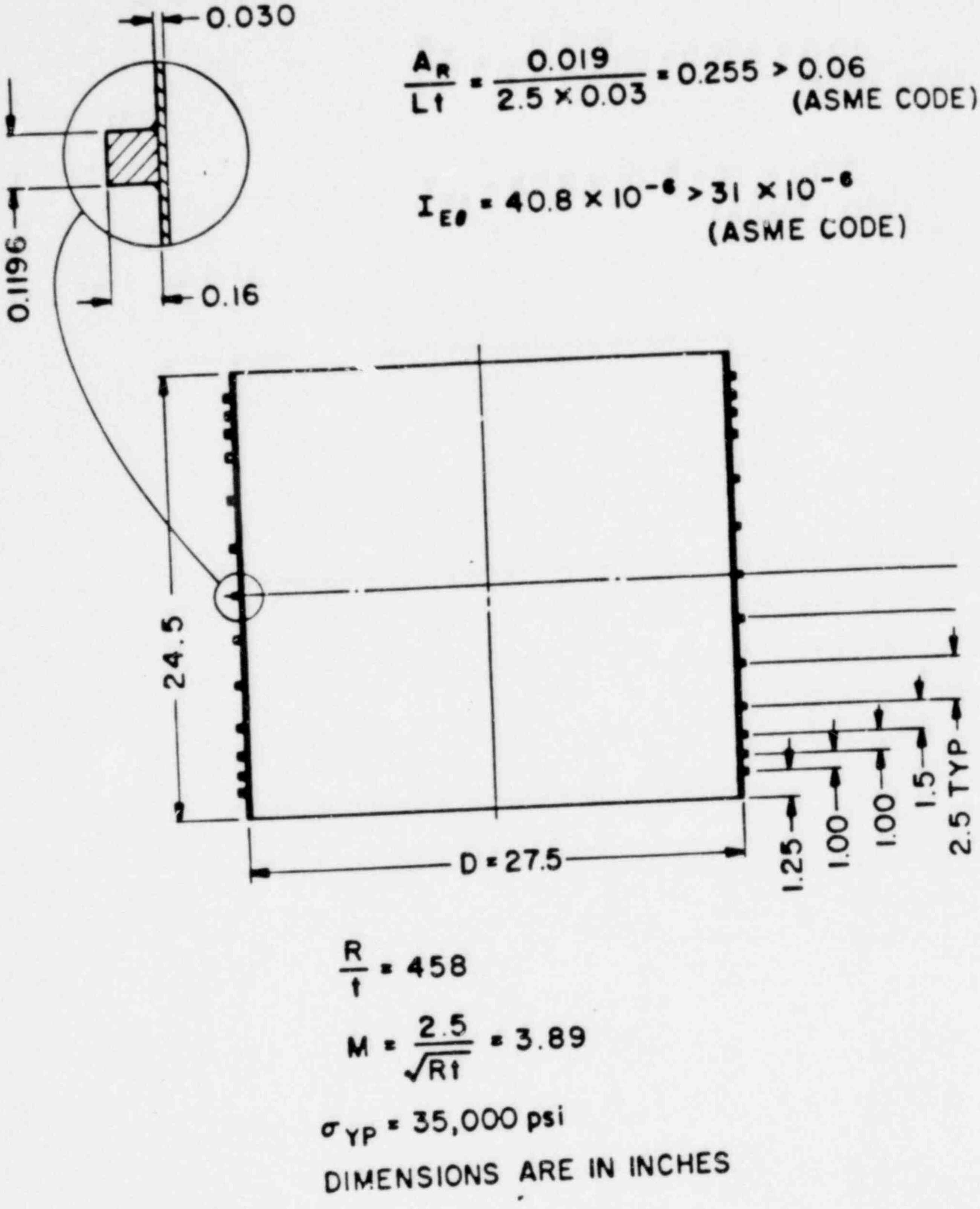
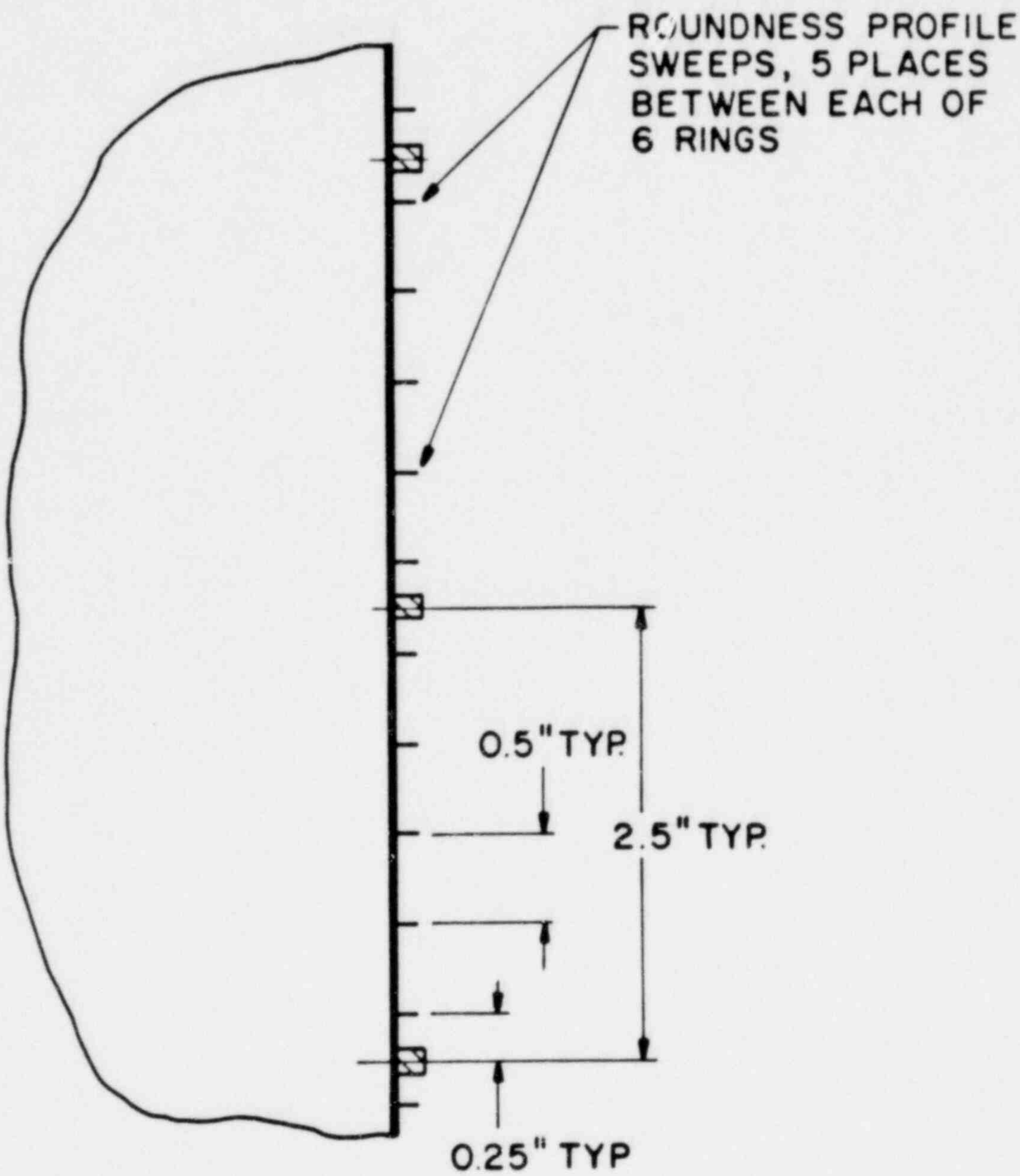


Fig. 1. Baseline Benchmark Experimental Model.


ROUNDNESS PROFILE SWEEPS, 5 PLACES BETWEEN EACH OF 6 RINGS
0.5" TYP.
2.5" TYP.
0.25" TYP

F 62

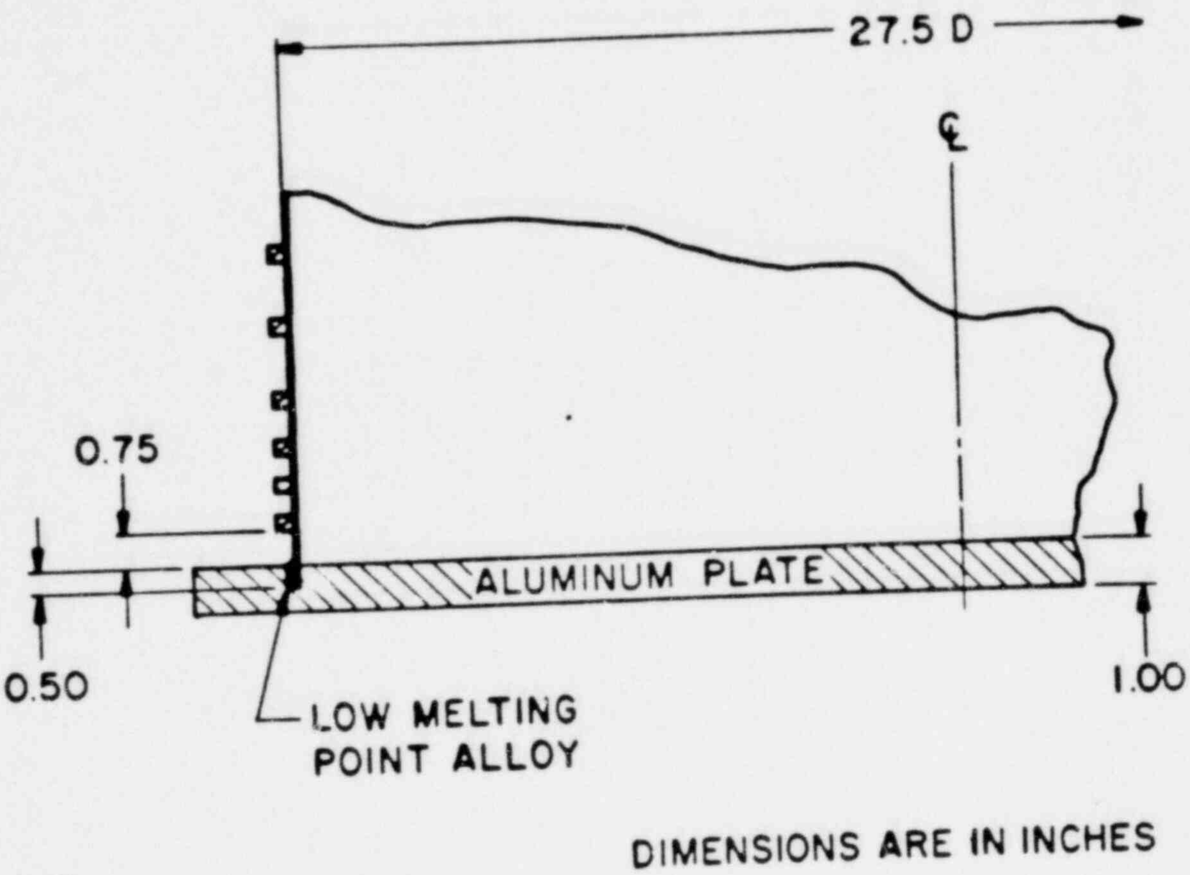


Fig. 3. Detail of end plate mounting.

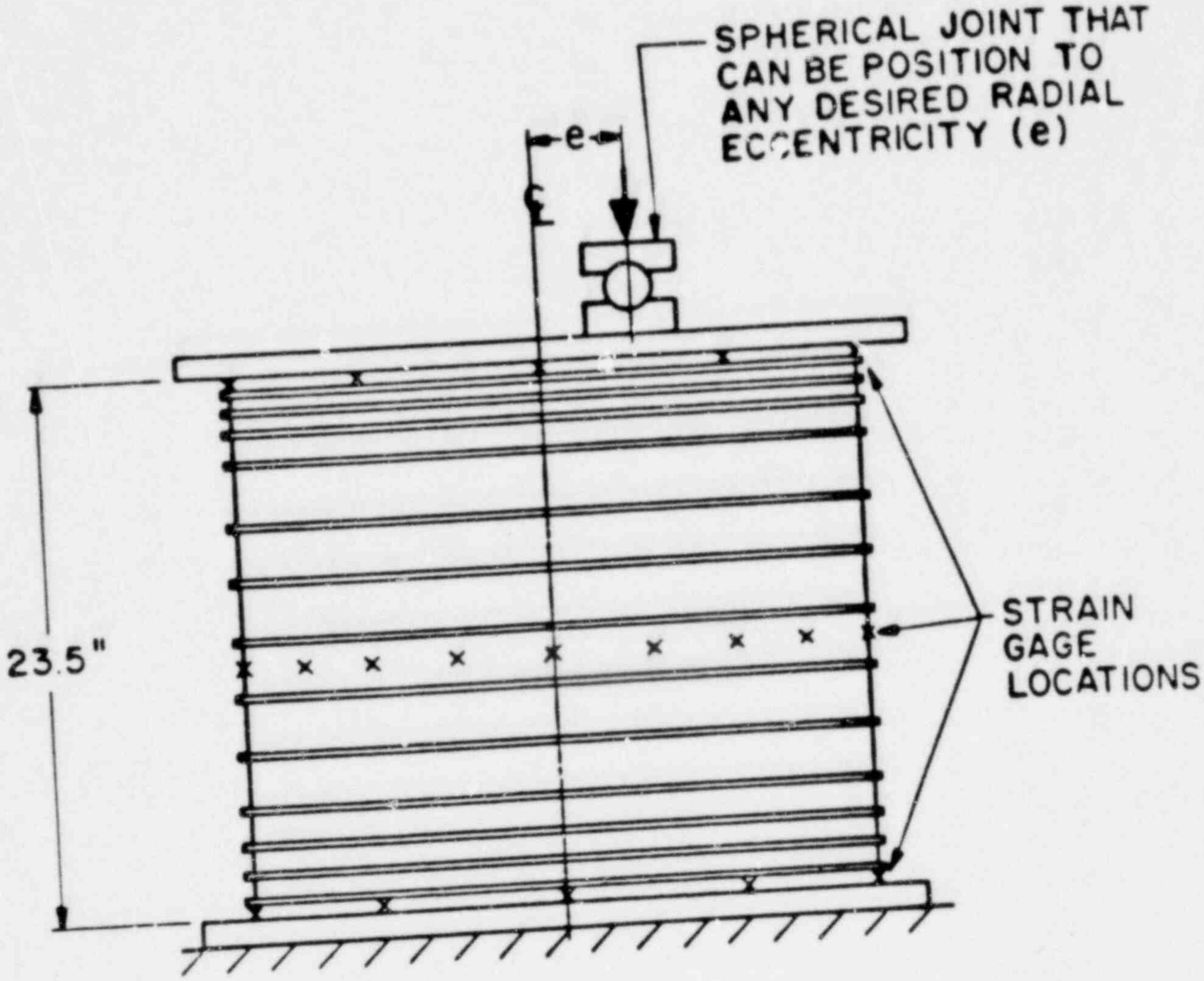
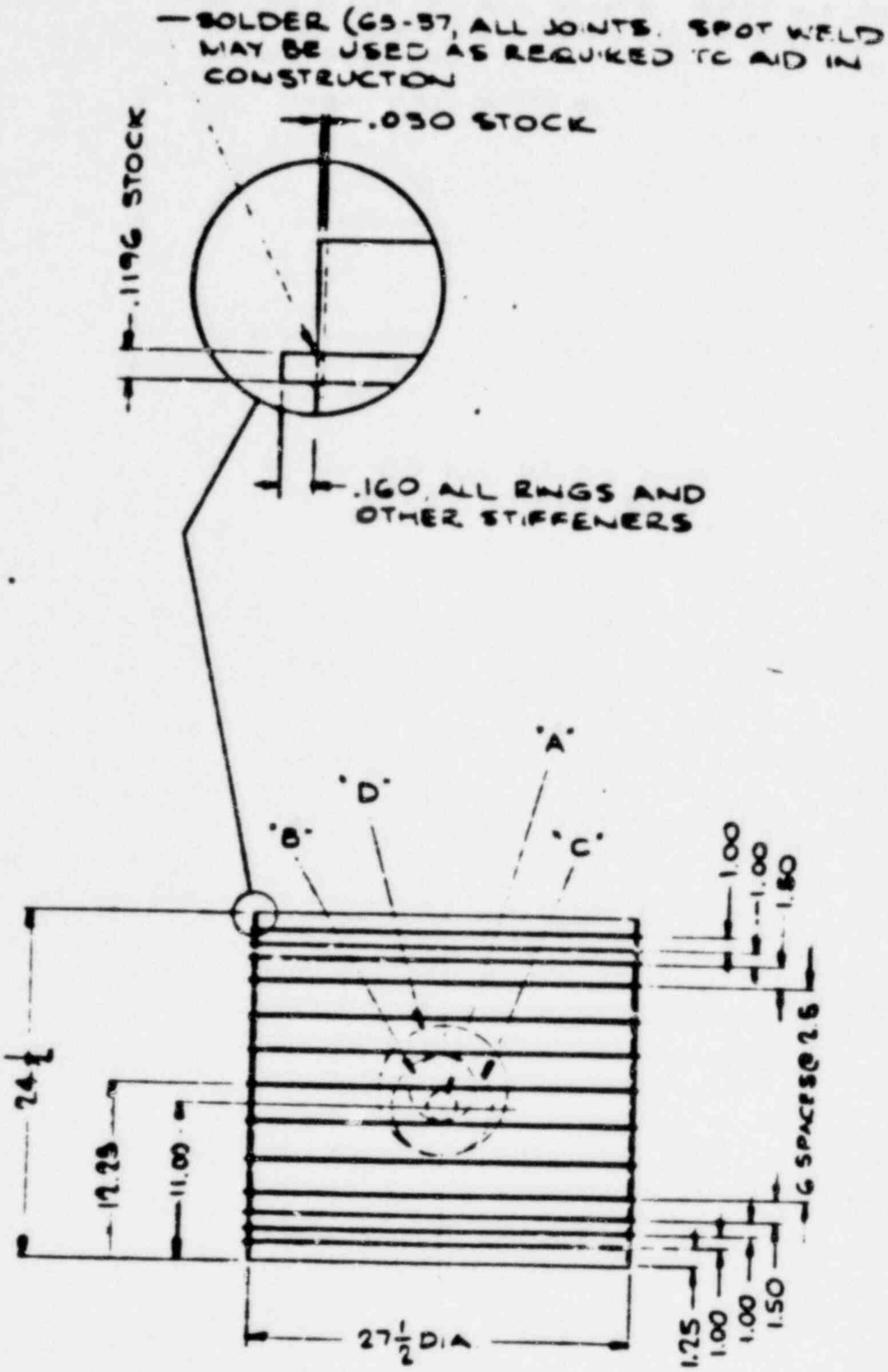


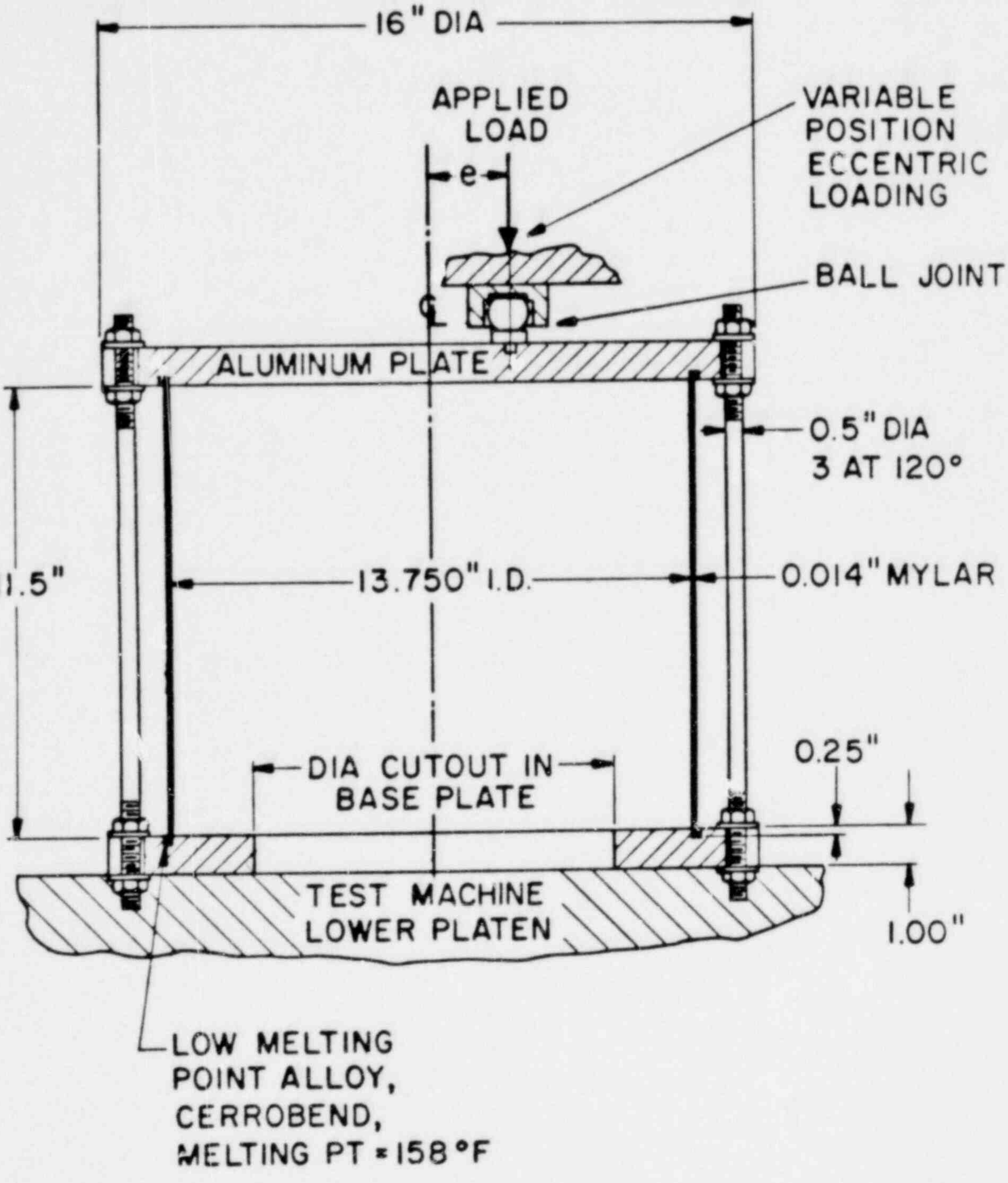
Fig. 4. Loading scheme and strain gage locations.



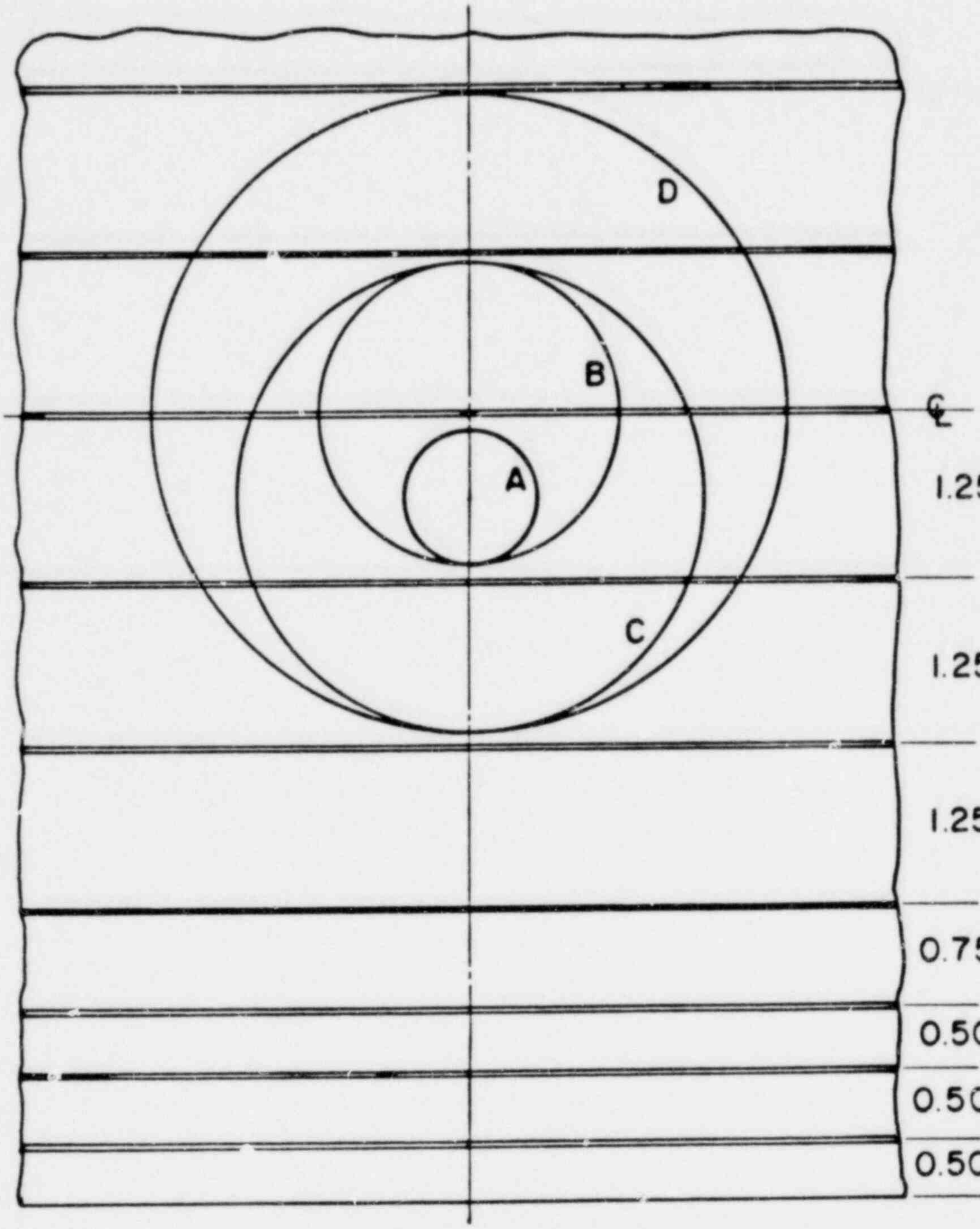
(200) - TEST CYLINDER

MATL: STEEL, A-366

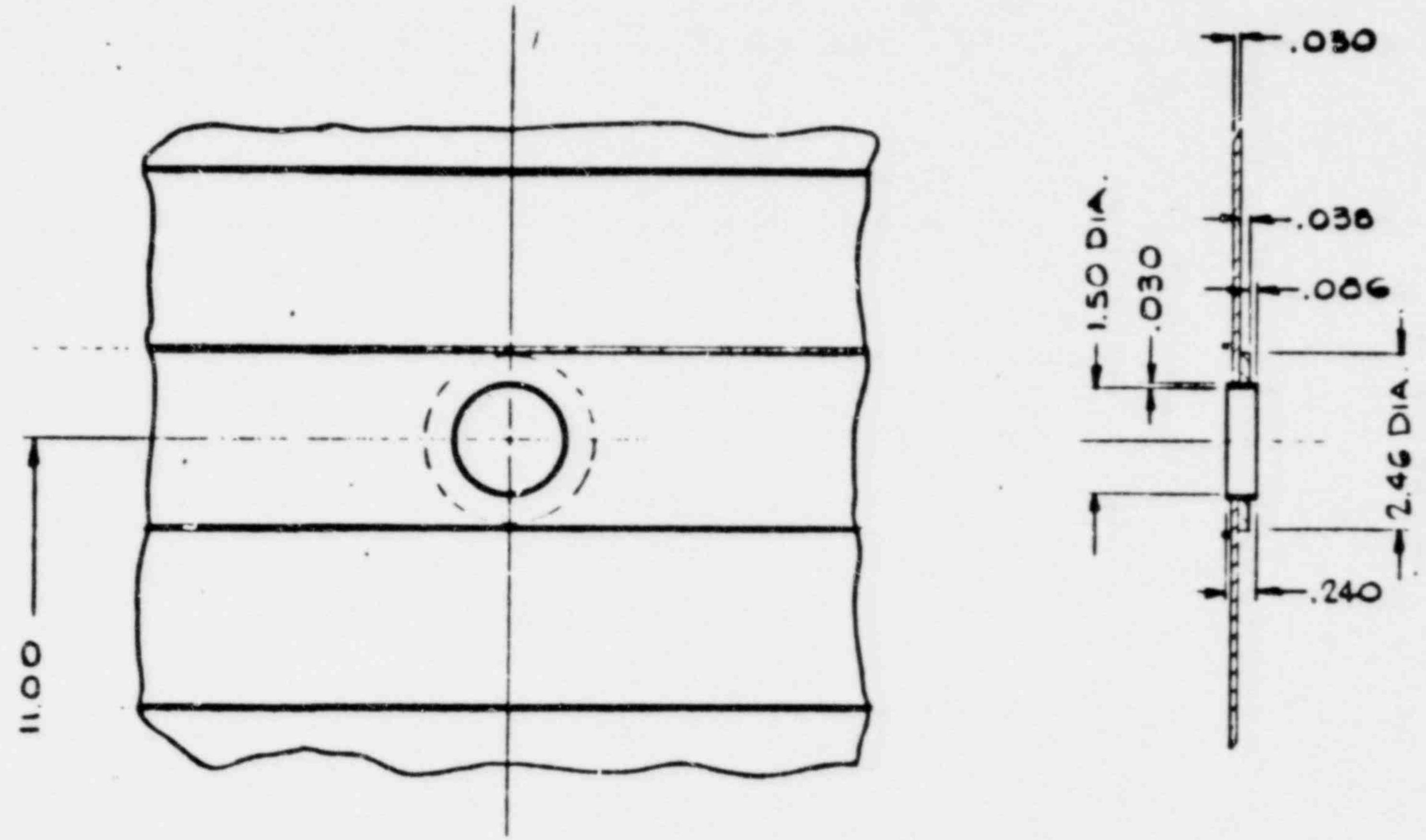
NOTE: * DENOTES ARC LENGTH


16" DIA
APPLIED LOAD
e
VARIABLE POSITION ECCENTRIC LOADING
BALL JOINT
ALUMINUM PLATE
0.5" DIA
3 AT 120°
11.5"
13.750" I.D.
0.014" MYLAR
0.25"
DIA CUTOUT IN BASE PLATE
TEST MACHINE LOWER PLATEN
1.00"
LOW MELTING POINT ALLOY, CERROBEND, MELTING PT = 158 °F

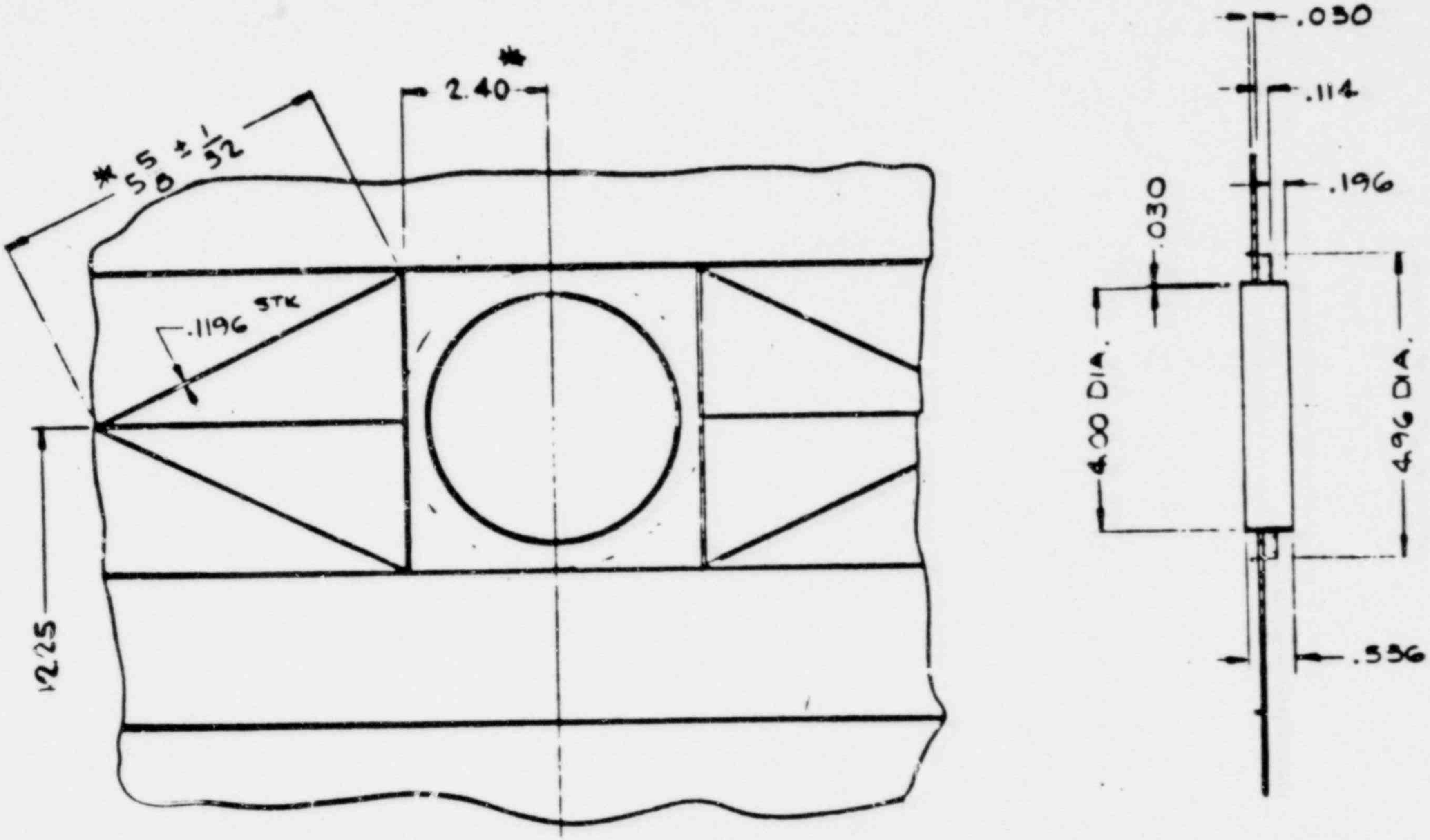
RINGS ARE SYMMETRICAL
ABOUT HORIZONTAL ℄



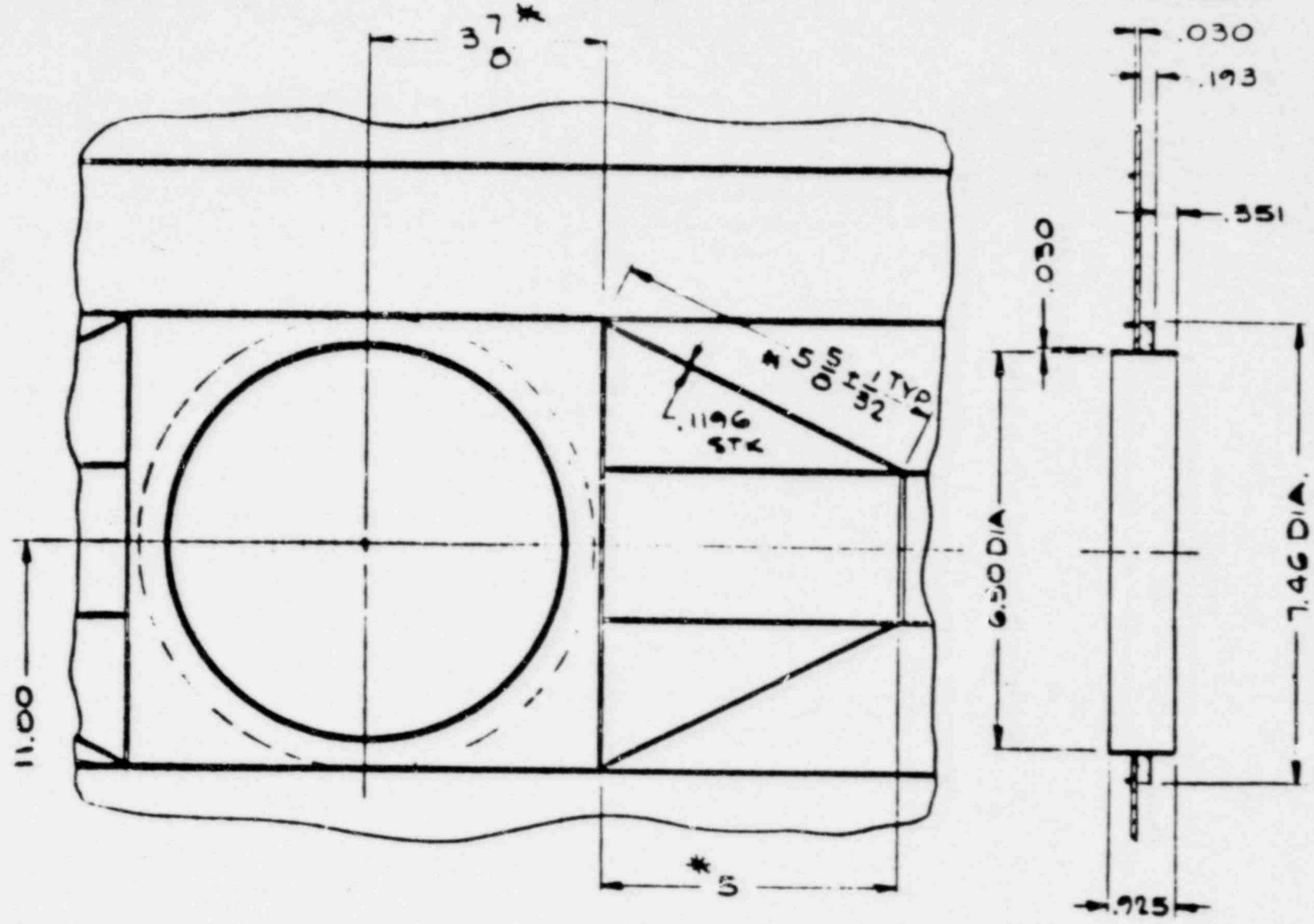
A = 1.00" DIA
B = 2.25" DIA
C = 3.50" DIA
D = 4.75" DIA



(201) INSERT 'A'

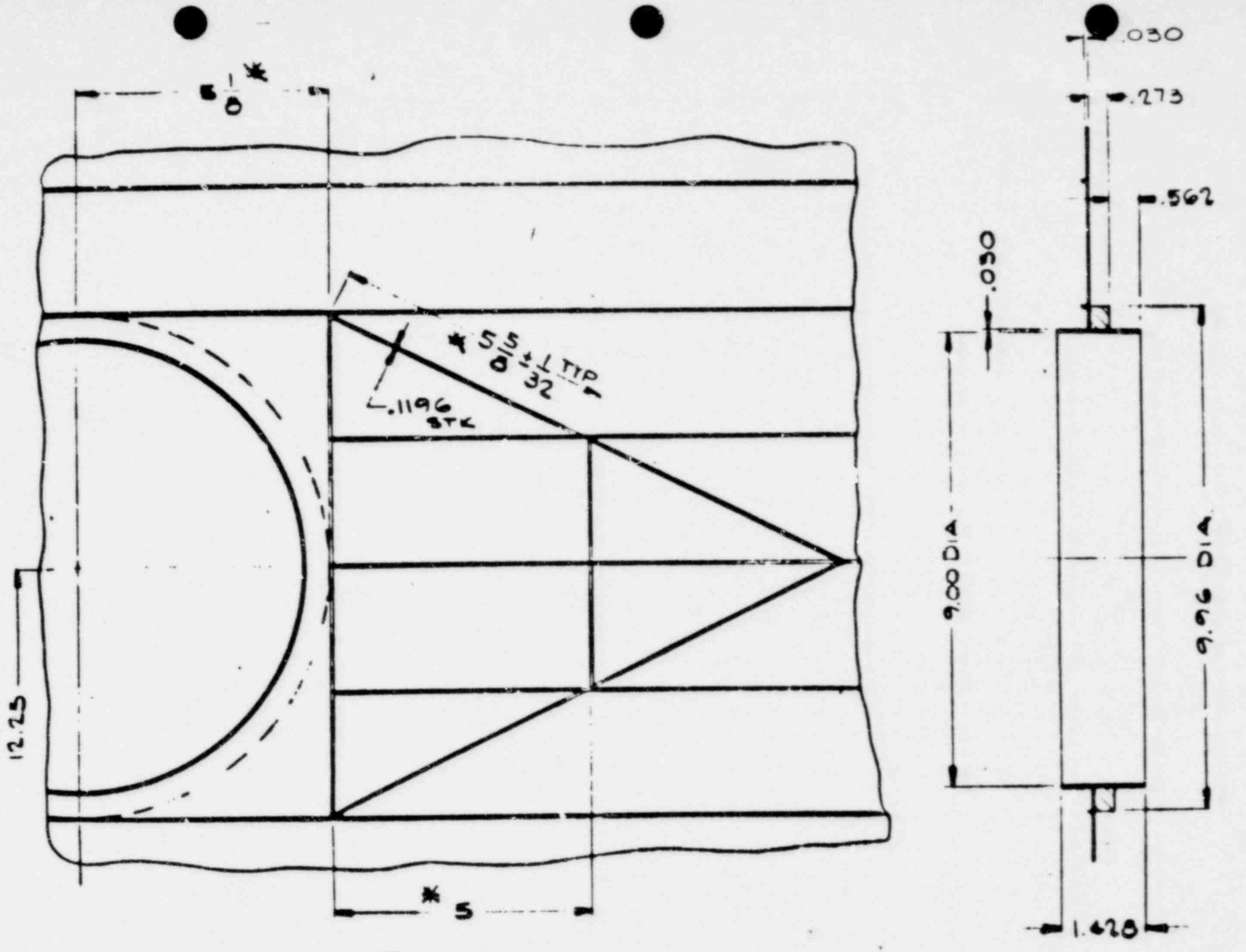


(202) INSERT 'B"



$3\frac{7}{8}$ *
11.00
* 5
* $5\frac{5}{8} \pm \frac{1}{32}$ TYP
.1196 STK
.030
.193
.351
.030
6.50 DIA
7.46 DIA.
.925

(203) INSERT "C"



5 1/8 *
* 5 5/8 ± 1/32 TYP
.1196 STK
12.25
* 5
.030
.273
.562
.030
9.00 DIA
9.96 DIA
1.628

(204) INSERT "D"

SMtoF-CB - current status cont.

STEEL CONTAINMENT DESIGN STUDIES

- – ASME NE-3000 as applied to containment designs
- – ASME code case N-284 - 'Metal Containment Shell Buckling' under study
- – Special criteria for floating island plants under study
- – Other related literature being reviewed

PEER REVIEW PANEL

Guidance on:

- – Overall program direction
  - – task priorities
- – Computer code benchmark experiments
  - – experiment selection and loadings
- – dynamic experiments on containment models
  - – scale
  - – loading definitions and their combinations

STRUCTURAL MARGINS TO FAILURE –CATEGORY I STRUCTURES PROGRAM

PRESEN ATION TOPICS:

- General Information on Program
- Program Plan Background Information
- Program Plan Summary
- Results to Date
  - Analytical
  - Experimental
- Future Activities

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STRUCTURAL MARGINS TO FAILURE – CATEGORY I STRUCTURES PROGRAM

<u>FUNDED BY:</u>

The Mechanical/Structural Engineering Branch,
Division of Engineering Technology,
Office of Nuclear Regulatory Research,
Nuclear Regulatory Commission

<u>RELATION TO OTHER NRC PROGRAMS</u>

This project is a part of a larger NRC Program whose objective is to increase confidence in the assessment of Category I nuclear power plant behavior

Related Programs are being funded at the other National Laboratories

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STRUCTURAL MARGINS TO FAILURE

– CATEGORY I STRUCTURES PROGRAM

PROGRAM OBJECTIVES

To supply experimental and analytical information needed to assess the structural capacity of Category I structures (excluding the reactor containment building)

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INFORMATION SEARCH

SOURCES

- A/E Firms
  - TVA
  - Bechtel
  - Sargent & Lundy
- Literature
  - Dynamic analysis methods
  - Damping effects

INFORMATION SOUGHT

- Typical Plant Layouts
- Design Practices
- Codes and guides
- Construction methods
- Loads that control element designs
- Static and dynamic analysis methods
  - Linear and nonlinear
  - Computer codes
  - Types of models
- Information needs as expressed by A/Es

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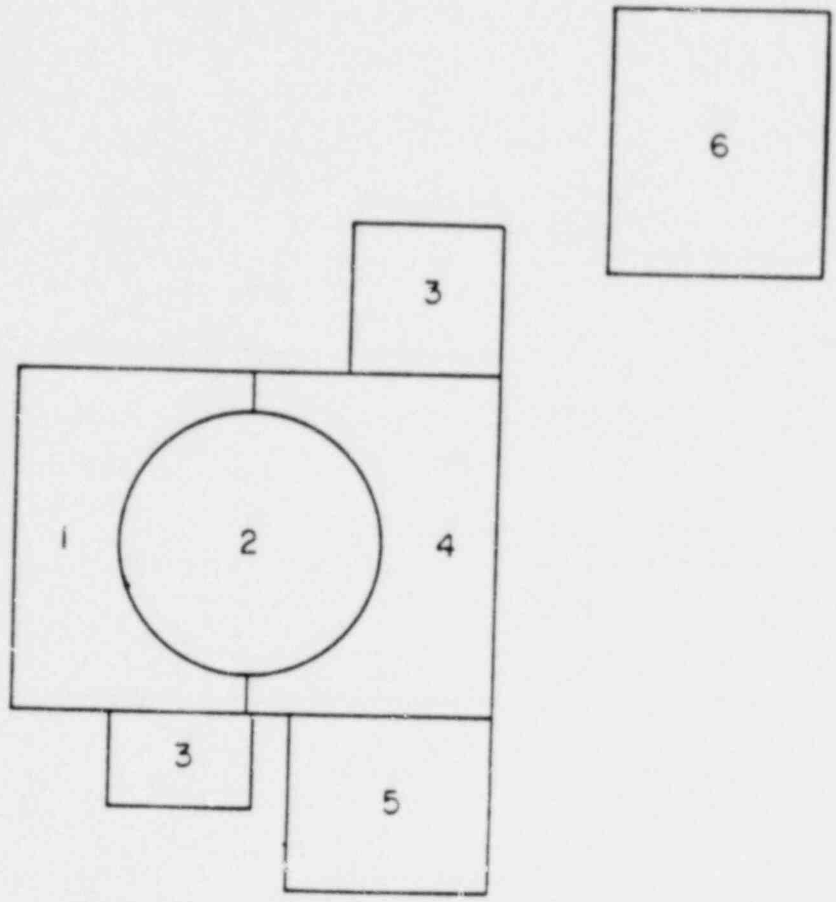
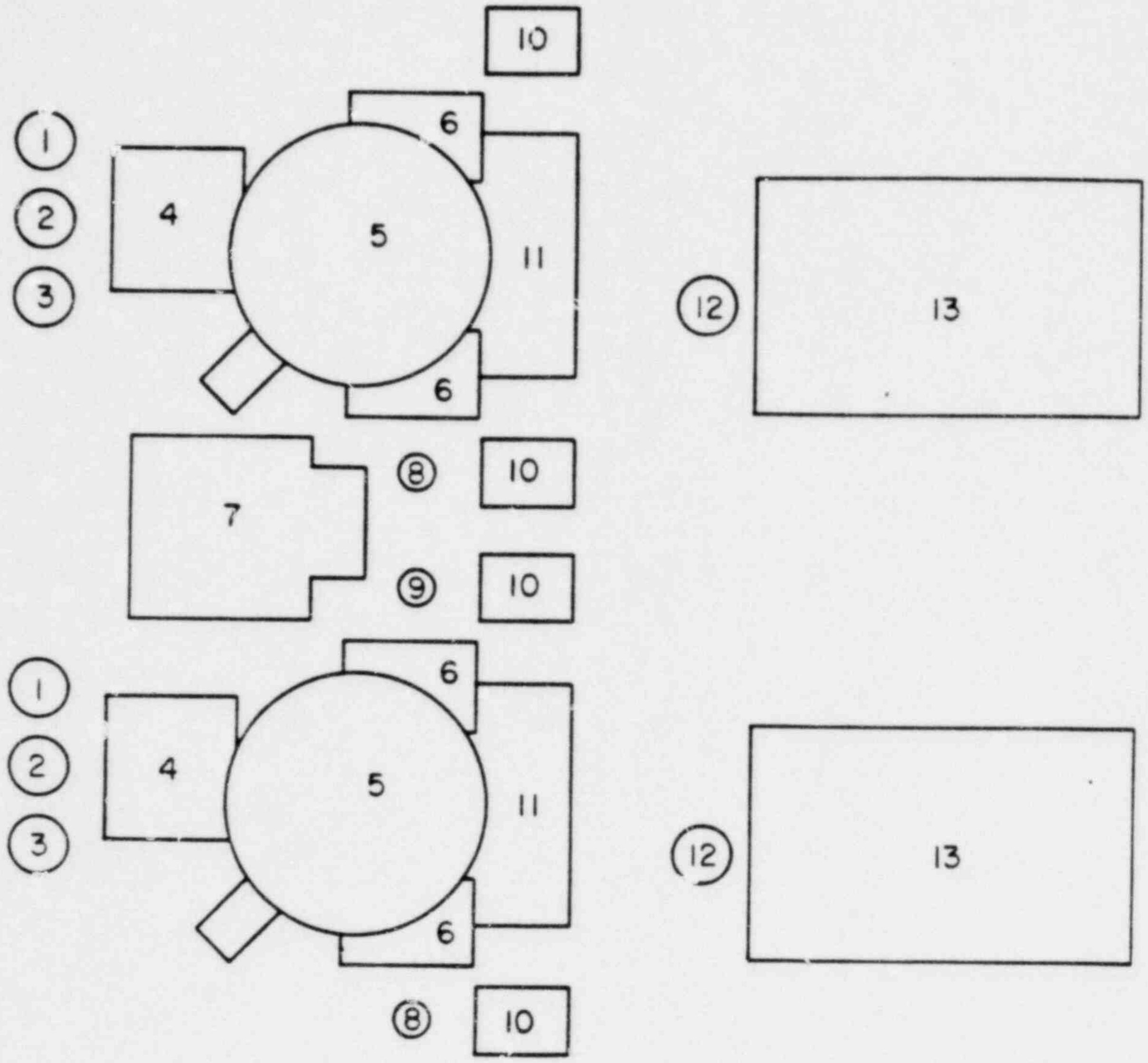
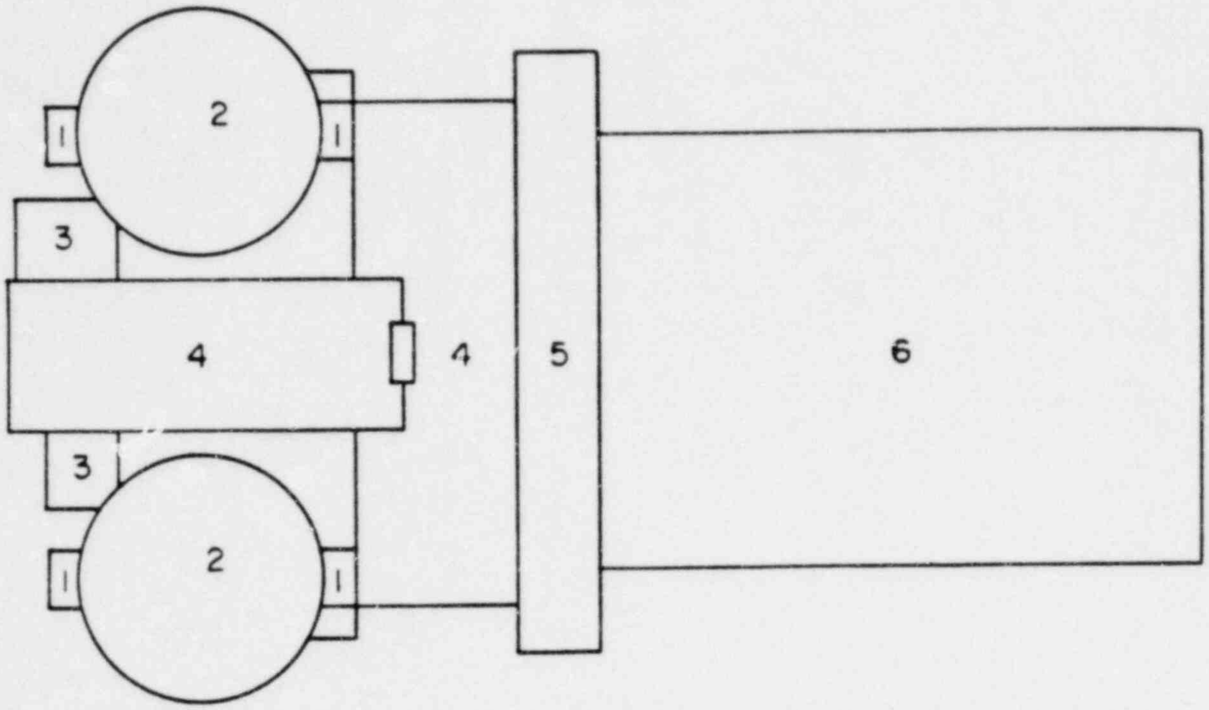


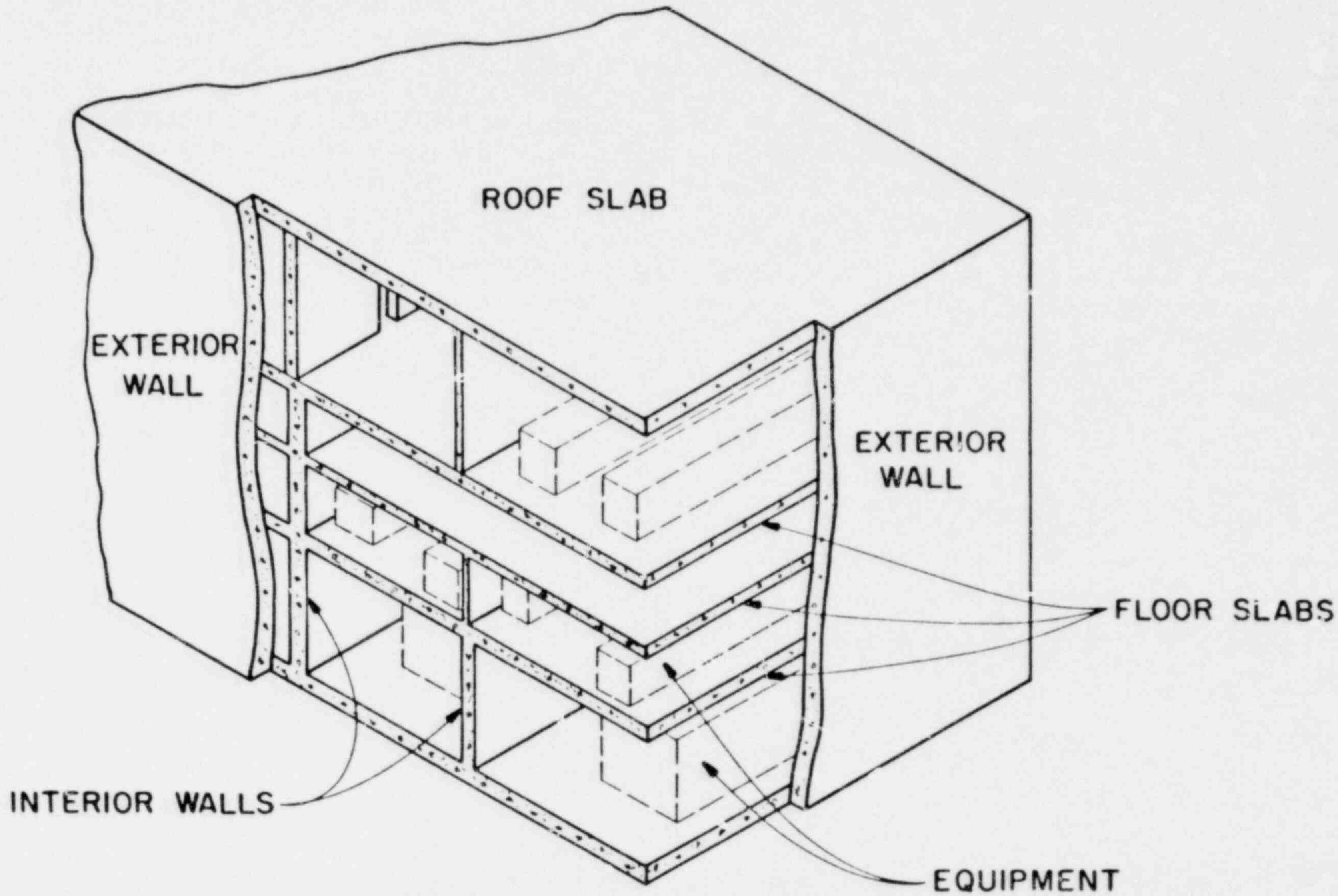
1 - FUEL BUILDING
2 - REACTOR BUILDING
3 - DIESEL GENERATOR BUILDING
4 - AUXILIARY BUILDING
5 - CONTROL BUILDING
6 - RADWASTE BUILDING



1 - HOLDUP TANK
2 - REACTOR MAKE-UP WATER TANK
3 - REFUELING WATER TANK
4 - FUEL BUILDING
5 - REACTOR BUILDING
6 - MAIN STEAM VALVE VAULT
7 - WASTE MANAGEMENT BUILDING
8 - EMERGENCY FEEDWATER TANK
9 - WATER REUSE TANK
10 - DIESEL GENERATOR BUILDING
11 - CONTROL BUILDING
12 - CONDENSATE STORAGE TANK
13 - TURBINE BUILDING



1 - STEAM VALVE VAULT
2 - REACTOR BUILDING
3 - ADDITIONAL EQUIPMENT BUILDING
4 - AUXILIARY BUILDING
5 - CONTROL BUILDING
6 - TURBINE BUILDING



TYPICAL CATEGORY I STRUCTURE

INFORMATION NEEDS

Damping in cracked shear walls
Stiffness of cracked shear walls
Stiffness degradation
Displacement limit
Structure-equipment interaction
Soil-structure interaction

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PROGRAM PLAN SUMMARY

– Analytical Program –

ULTIMATE GOAL:

Locate, develop, or inspire development of a code that reliably predicts margins to failure of reinforced concrete structures

SURVEY OF AVAILABLE GENERAL FINITE ELEMENT CODES WITH NONLINEAR CAPABILITIES

DEVELOPED SPECIAL PURPOSE CODES TO AID IN UNDERSTANDING CERTAIN BEHAVIOR CHARACTERISTICS

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PROGRAM PLAN SUMMARY

– Experimental Program –

PURPOSE OF TESTS:

Obtain static and dynamic load-deflection characteristics

Obtain damping characteristics of shear walls

Determine failure patterns

Establish benchmark cases for code verification/development

SELECTED TEST STRUCTURE IS THE SHEAR WALL

CONSIDERATIONS:

Available test facilities

Scaled shear wall structures

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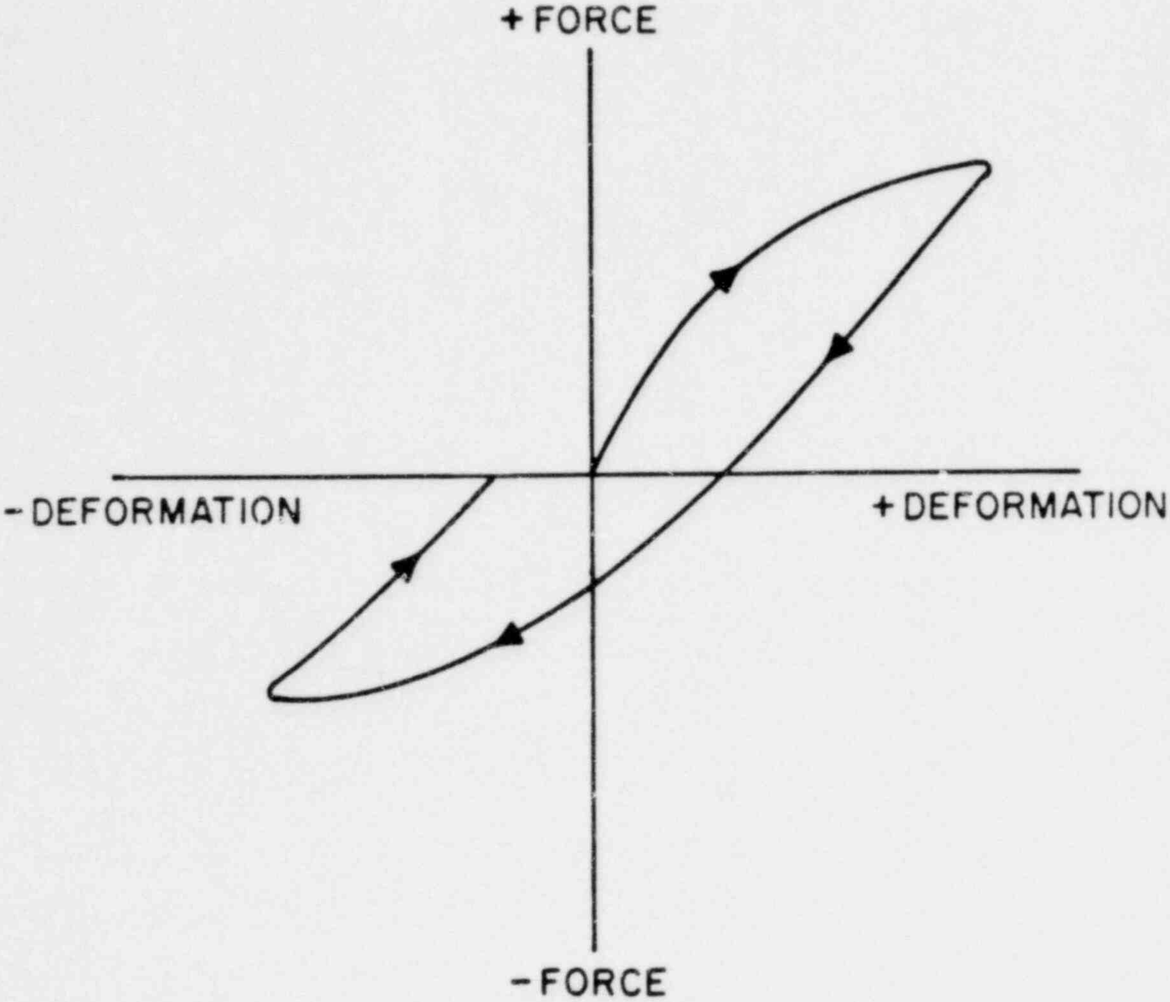
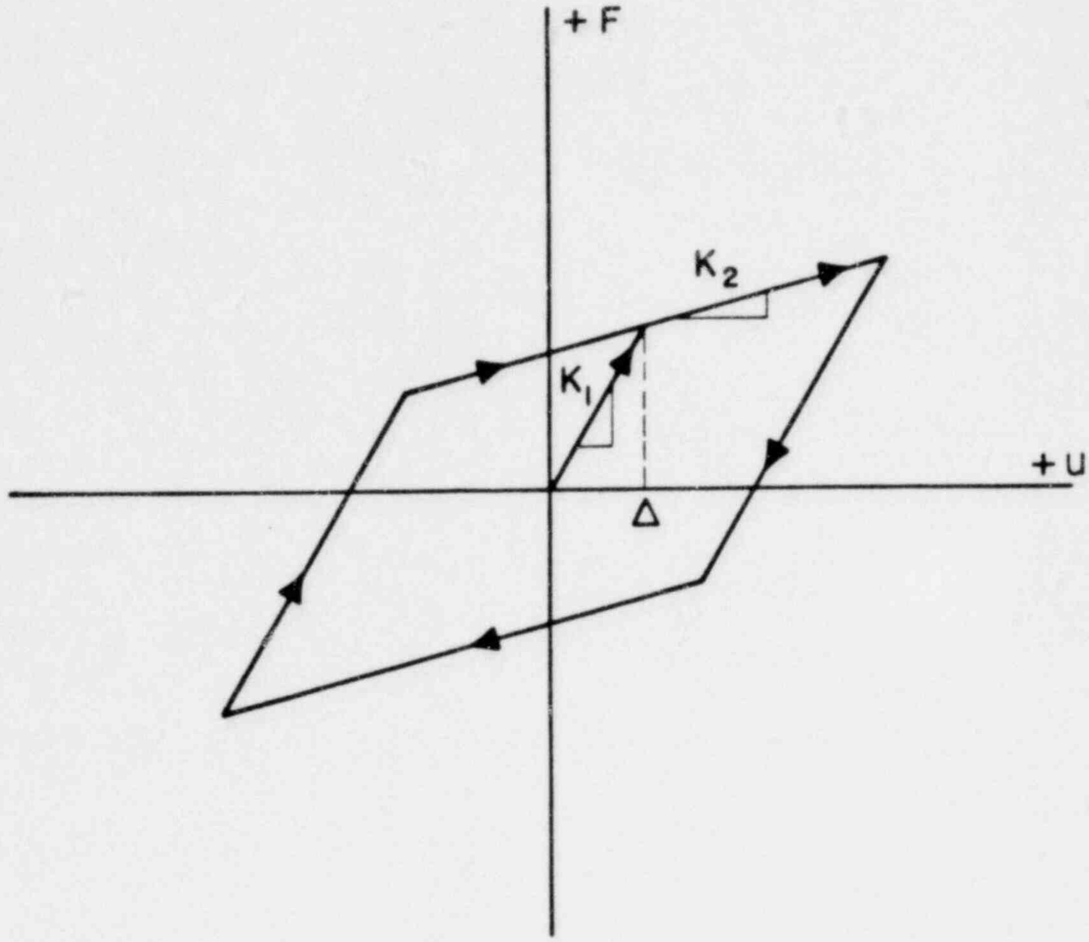
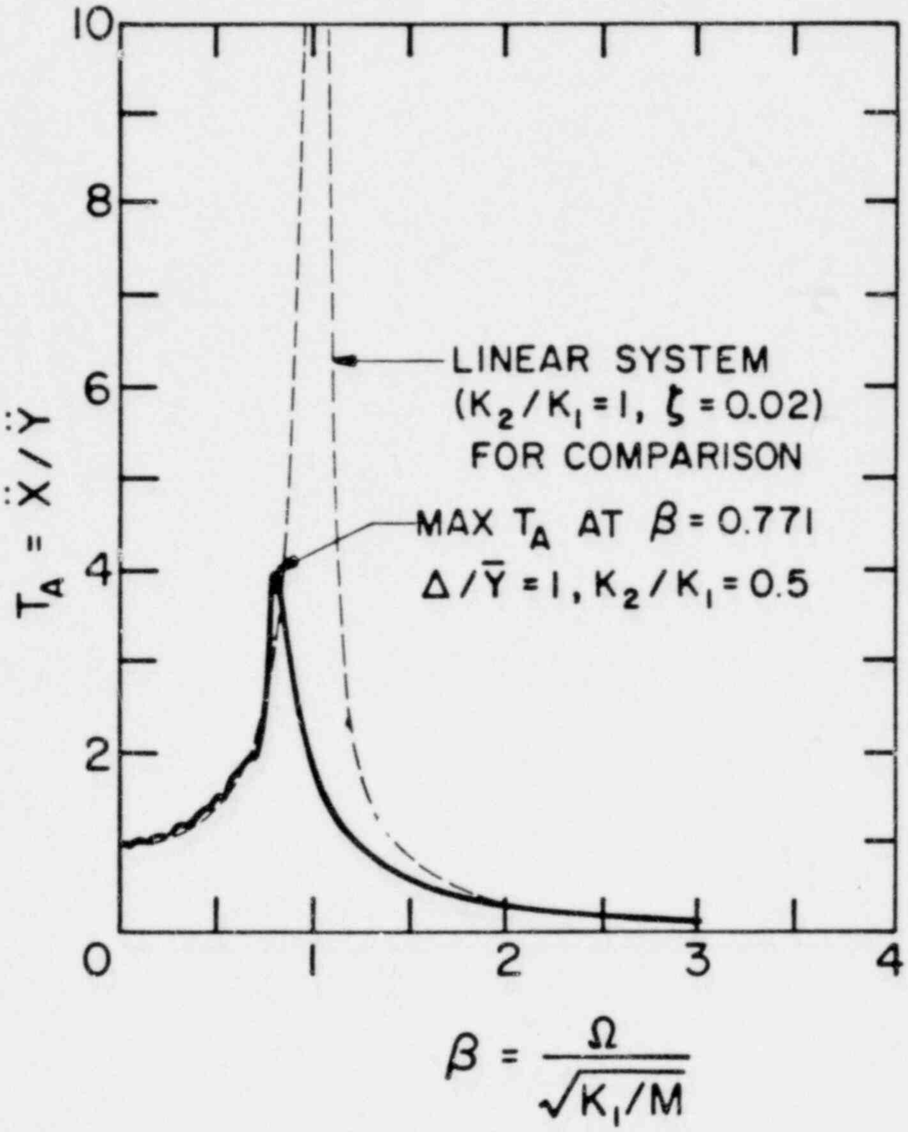
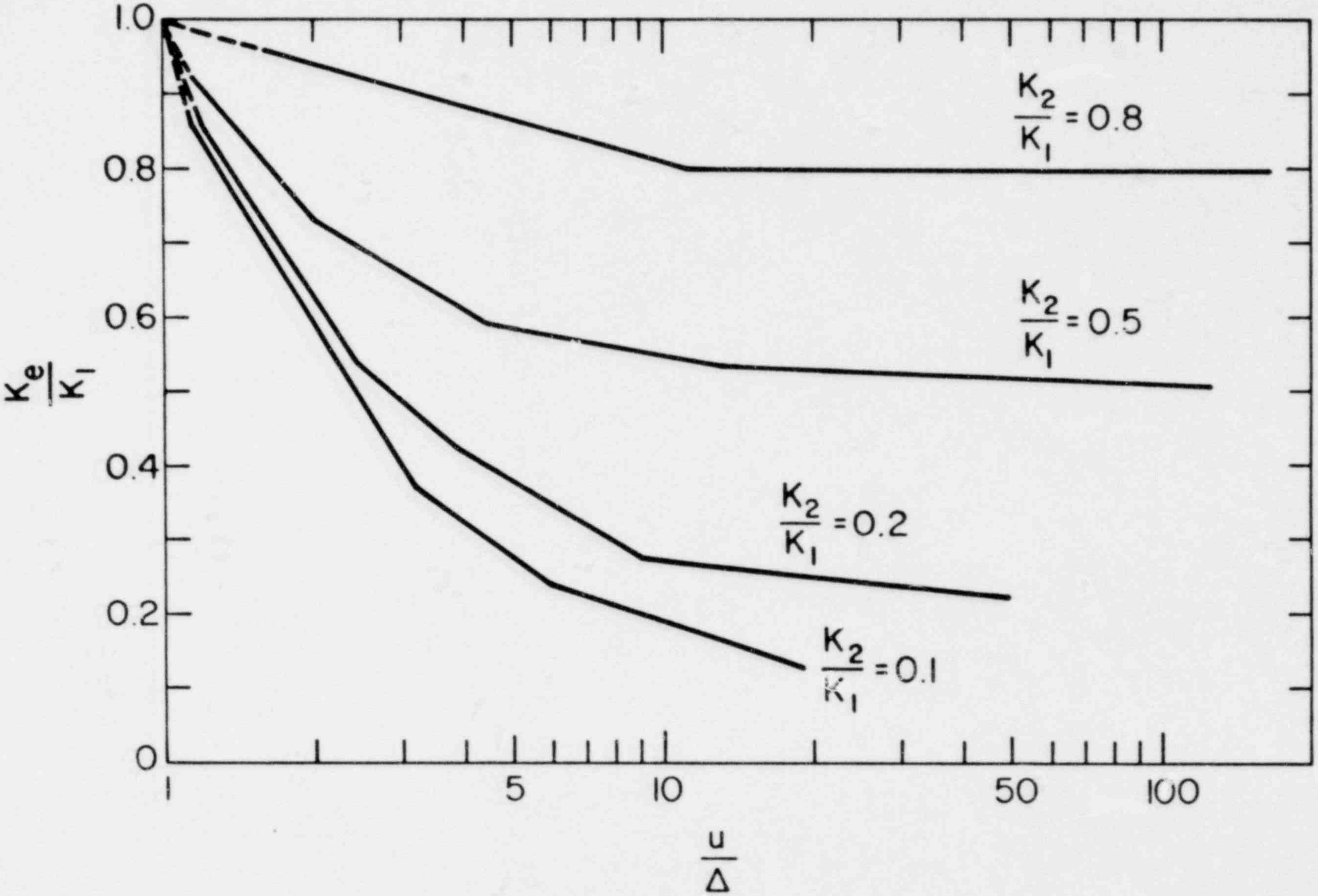

+ FORCE
- DEFORMATION
+ DEFORMATION
- FORCE



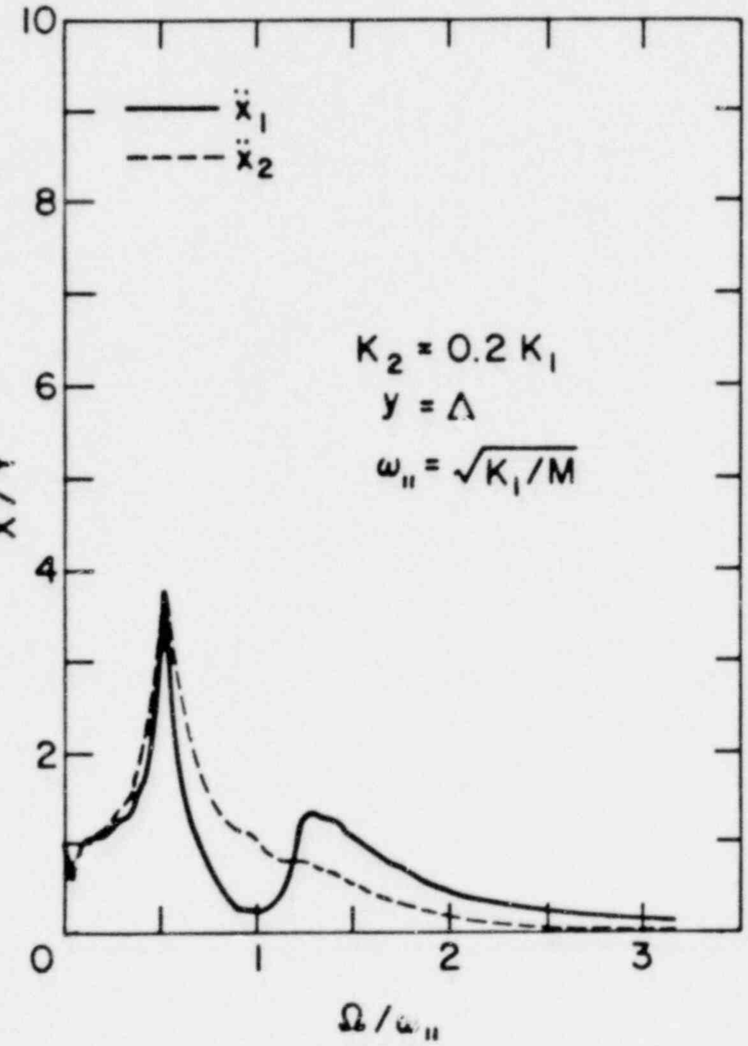
+ F
K2
K1
+ U
Δ



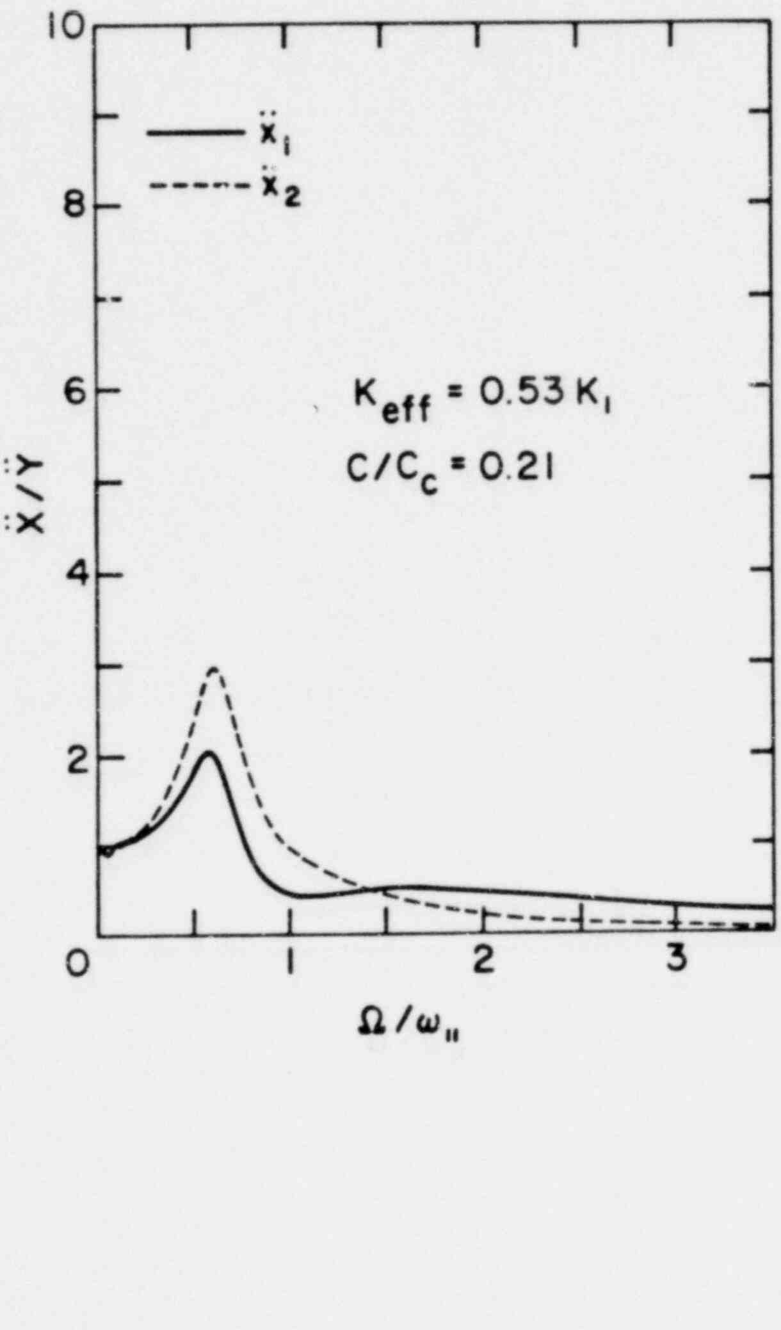
LINEAR SYSTEM
(K2/K1 = 1, ζ = 0.02)
FOR COMPARISON
MAX TA AT β = 0.771
Δ/Ȳ = 1, K2/K1 = 0.5
TA = Ẍ/Ÿ
β = Ω / √(K1/M)
0
2
4
6
8
10
1
3



1.0
0.8
0.6
0.4
0.2
0
1
5
10
50
100
K_e / K_1
u / Δ
$\frac{K_2}{K_1} = 0.8$
$\frac{K_2}{K_1} = 0.5$
$\frac{K_2}{K_1} = 0.2$
$\frac{K_2}{K_1} = 0.1$



10
8
6
4
2
0
1
2
3
$\ddot{x}_1$
$\ddot{x}_2$
$K_2 = 0.2\,K_1$
$y = \Delta$
$\omega_{11} = \sqrt{K_1/M}$
$\ddot{X}/\ddot{Y}$
Ω/ω_{11}



10
8
6
4
2
0
1
2
3
$\ddot{X}/\ddot{Y}$
Ω/ω_{11}
$\ddot{x}_1$
$\ddot{x}_2$
$K_{eff} = 0.53\,K_1$
$C/C_c = 0.21$

SYSTEM VARIABLES

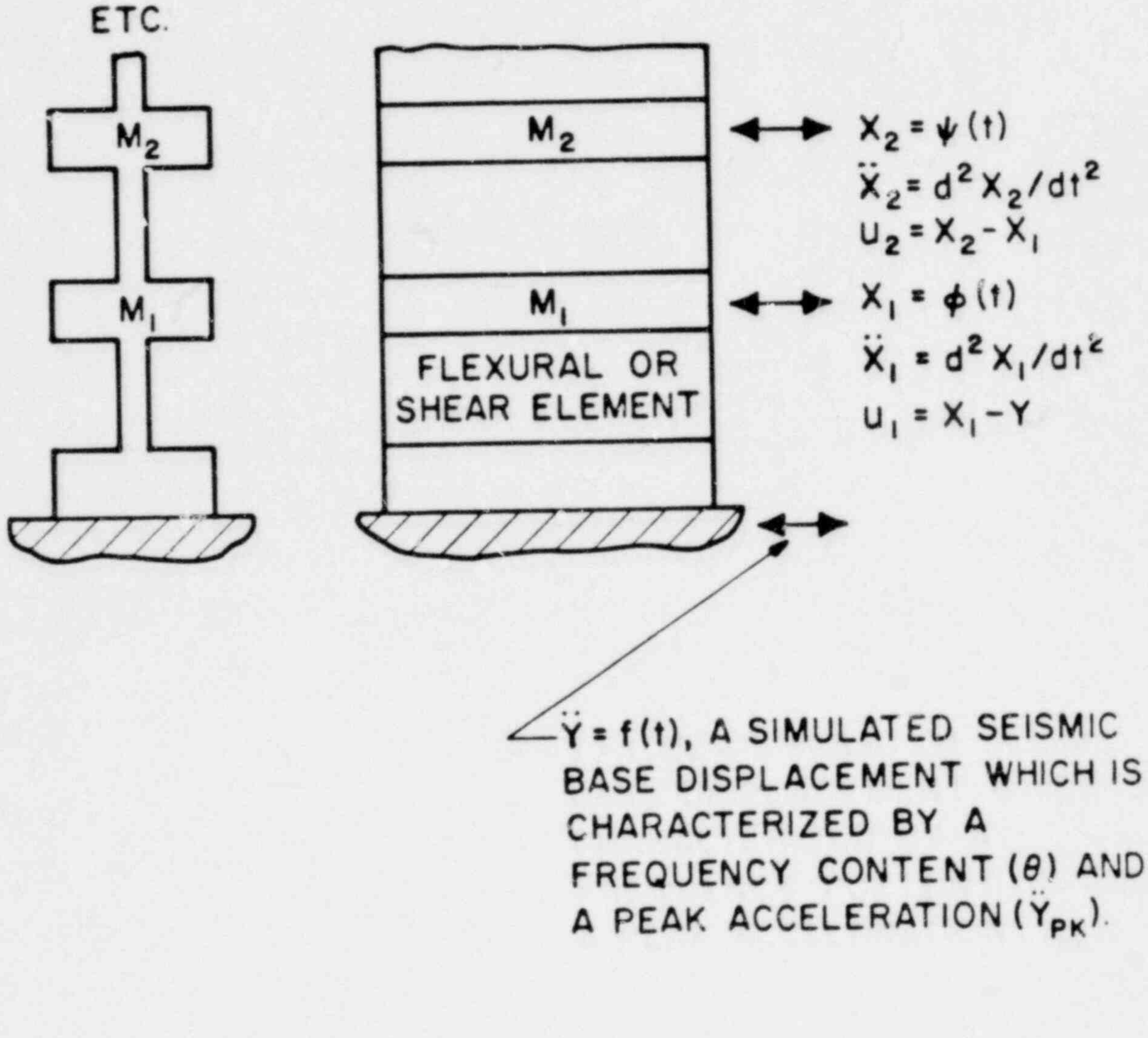
$$U = \Phi\,(\Delta,\ K_1,\ K_2,\ \ddot{Y}_{PK},\ \theta,\ M)$$

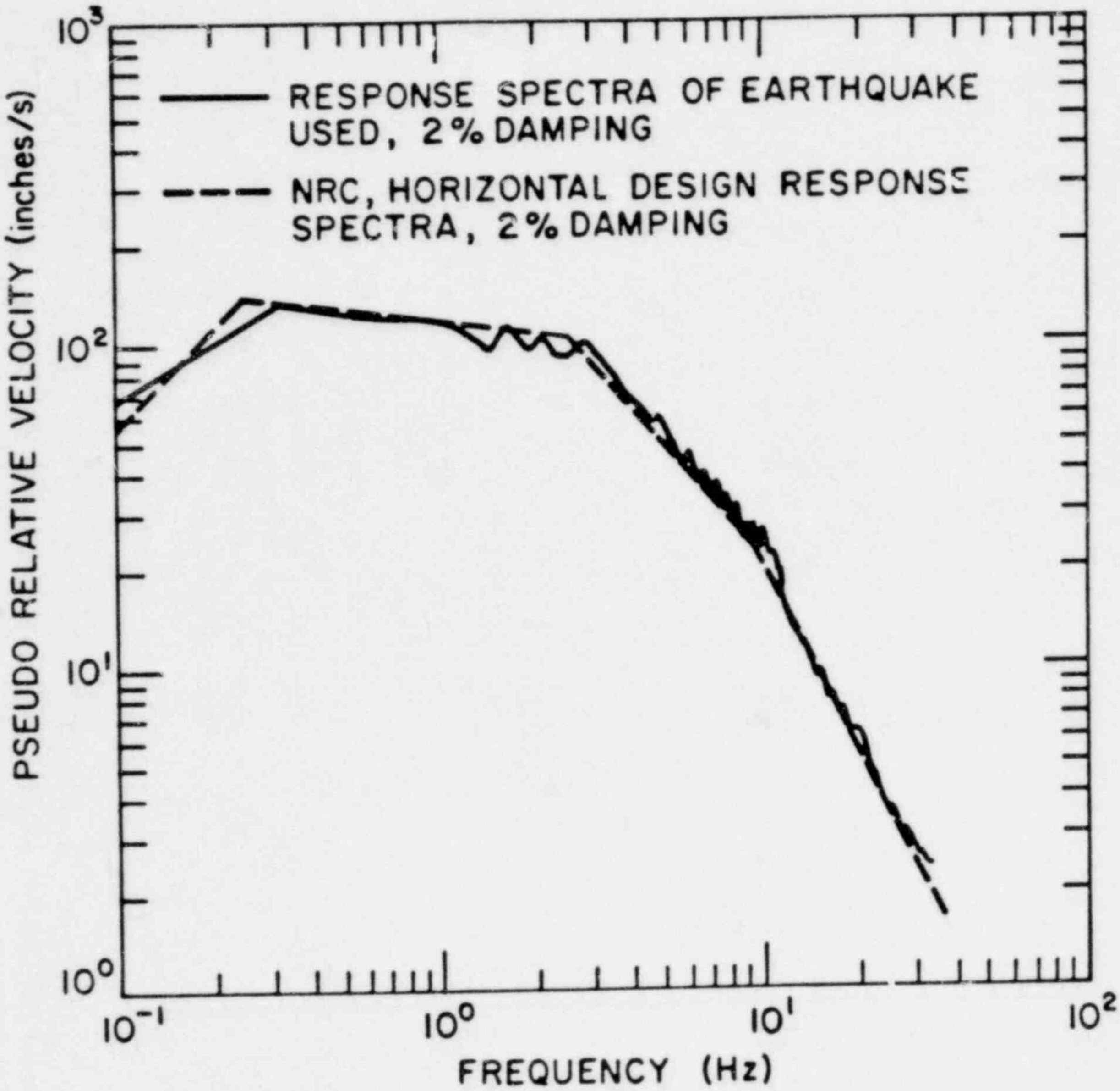
$$\ddot{X} = \Phi\,(\Delta,\ K_1,\ K_2,\ \ddot{Y}_{PK},\ \theta,\ M)$$

NONDIMENSIONAL FORM

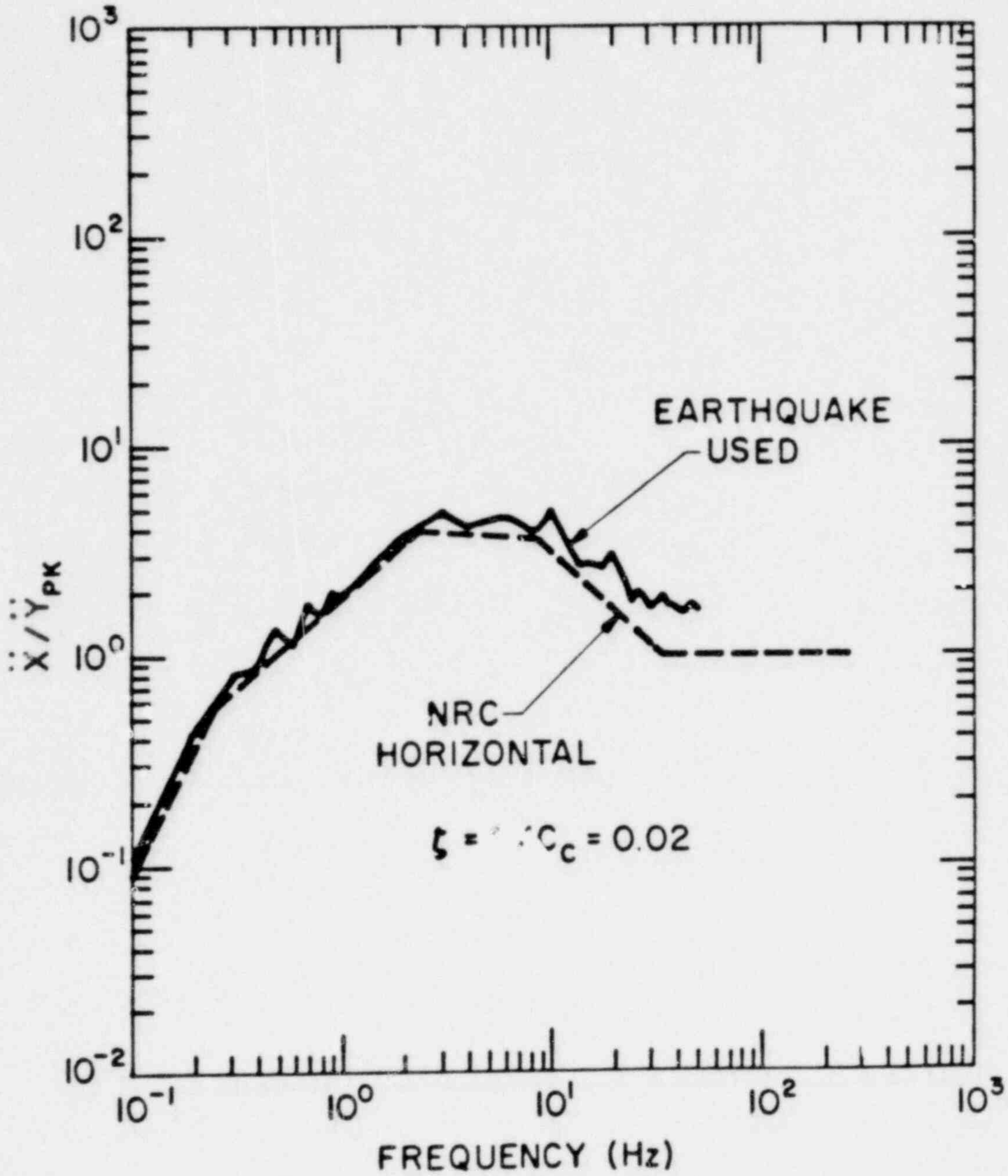
$$\frac{U}{\Delta} = \Psi\left[\frac{\sqrt{K_1/M}}{\theta},\ \frac{K_2}{K_1},\ \frac{\ddot{Y}_{PK}}{\Delta\theta^2}\right]$$

$$\frac{\ddot{X}}{\ddot{Y}_{PK}} = \Psi\left[\frac{\sqrt{K_1/M}}{\theta},\ \frac{K_2}{K},\ \frac{\ddot{Y}_{PK}}{\Delta\theta^2}\right]$$

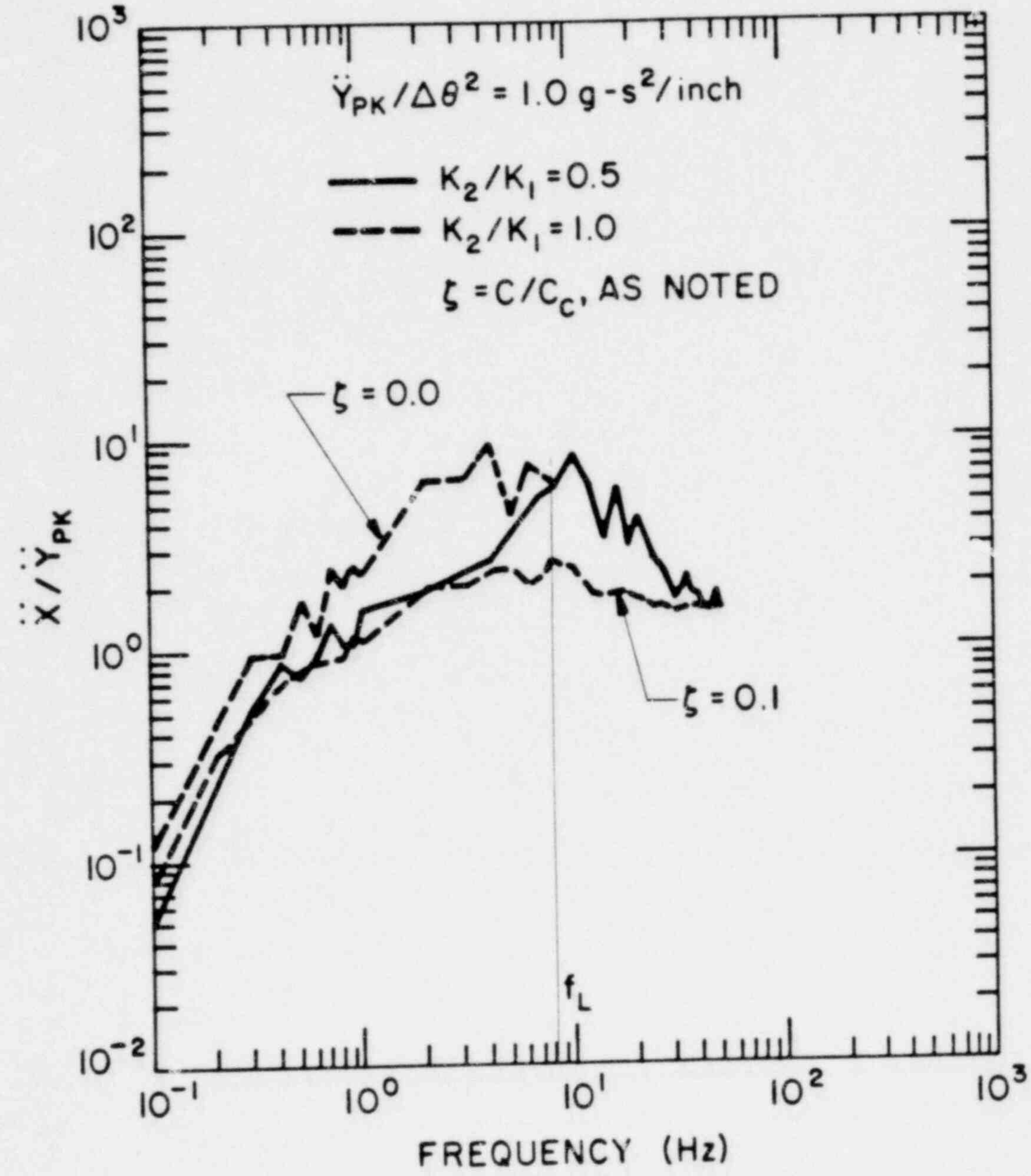

ETC.
M2
M1
M2
M1
FLEXURAL OR SHEAR ELEMENT
X2 = ψ(t)
Ẍ2 = d²X2/dt²
u2 = X2 - X1
X1 = φ(t)
Ẍ1 = d²X1/dt²
u1 = X1 - Y
Ÿ = f(t), A SIMULATED SEISMIC BASE DISPLACEMENT WHICH IS CHARACTERIZED BY A FREQUENCY CONTENT (θ) AND A PEAK ACCELERATION (ŸPK).



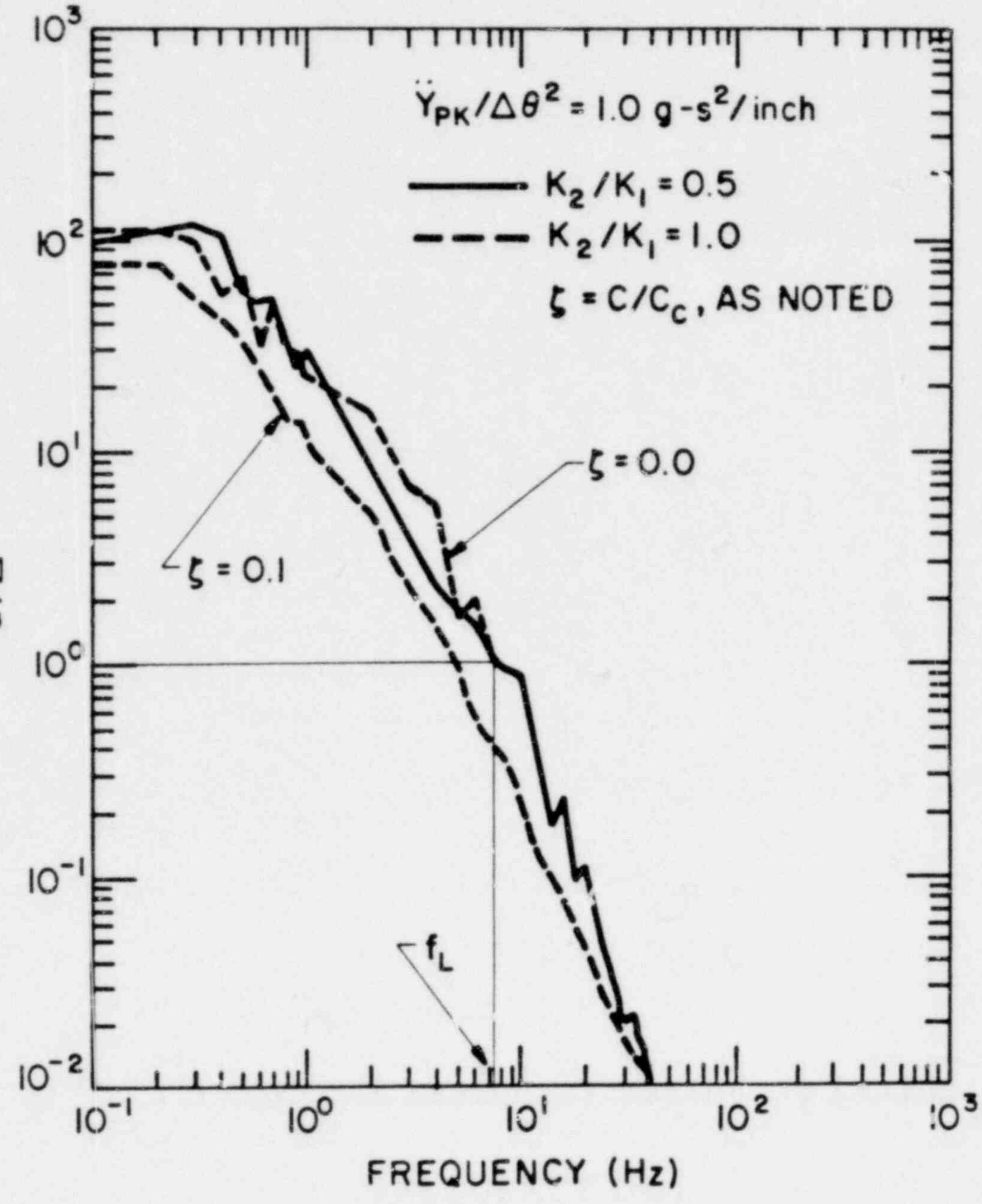
RESPONSE SPECTRA OF EARTHQUAKE USED, 2% DAMPING
NRC, HORIZONTAL DESIGN RESPONSE SPECTRA, 2% DAMPING
PSEUDO RELATIVE VELOCITY (inches/s)
FREQUENCY (Hz)
10^0
10^1
10^2
10^3
10^-1



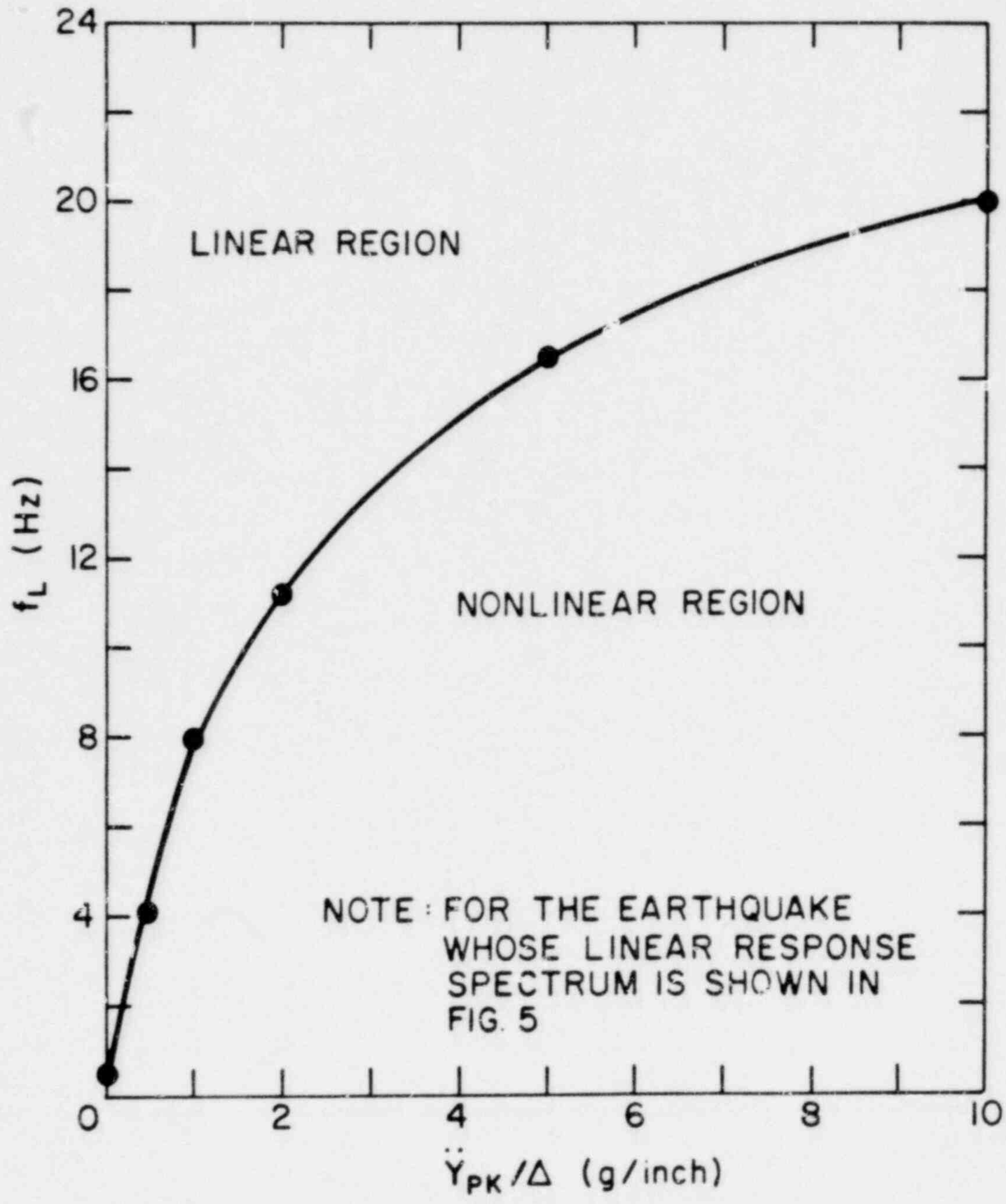
EARTHQUAKE
USED
NRC
HORIZONTAL
ζ = ·/Cc = 0.02
$\ddot{X}/\ddot{Y}_{PK}$
FREQUENCY (Hz)



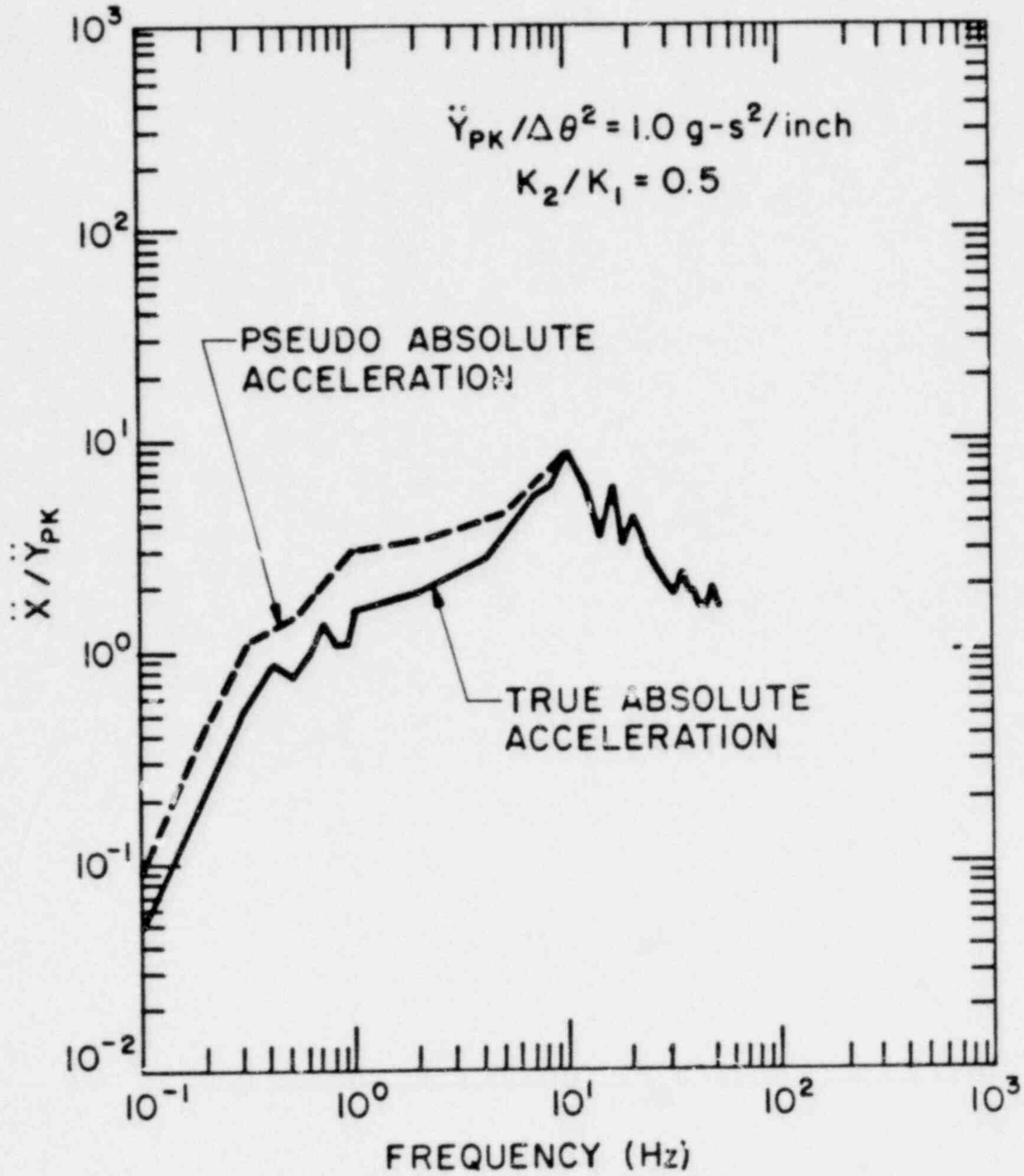
$\ddot{Y}_{PK}/\Delta\theta^2 = 1.0$ g-s²/inch
$K_2/K_1 = 0.5$
$K_2/K_1 = 1.0$
$\zeta = C/C_C$, AS NOTED
$\zeta = 0.0$
$\zeta = 0.1$
f_L
$\ddot{X}/\ddot{Y}_{PK}$
FREQUENCY (Hz)
10^{-2}
10^{-1}
10^{0}
10^{1}
10^{2}
10^{3}



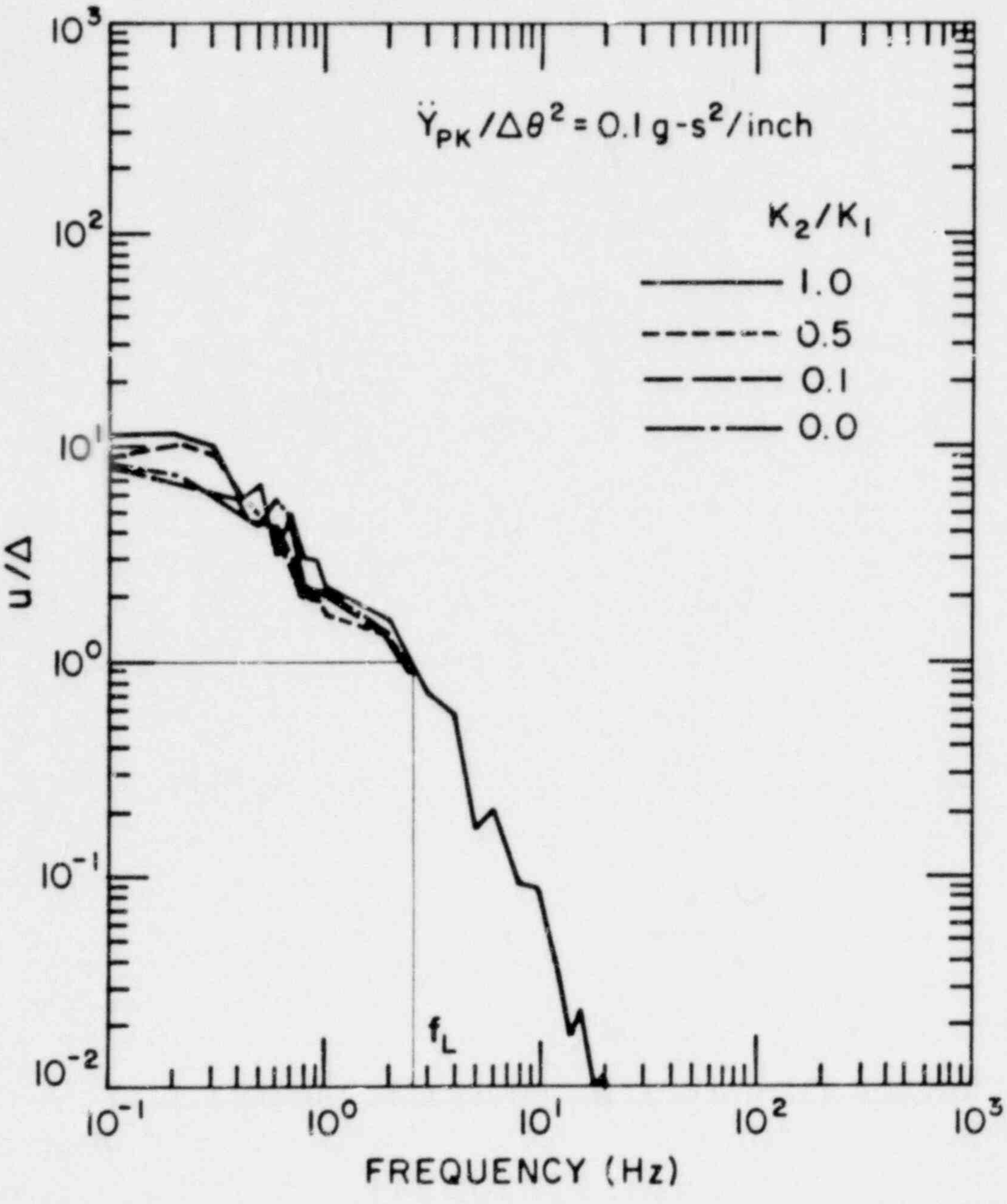
$\ddot{Y}_{PK}/\Delta\theta^2 = 1.0$ g-s²/inch
$K_2/K_1 = 0.5$
$K_2/K_1 = 1.0$
$\zeta = C/C_c$, AS NOTED
$\zeta = 0.0$
$\zeta = 0.1$
f_L
u/Δ
FREQUENCY (Hz)
10^{-1}
10^0
10^1
10^2
10^3
10^{-2}

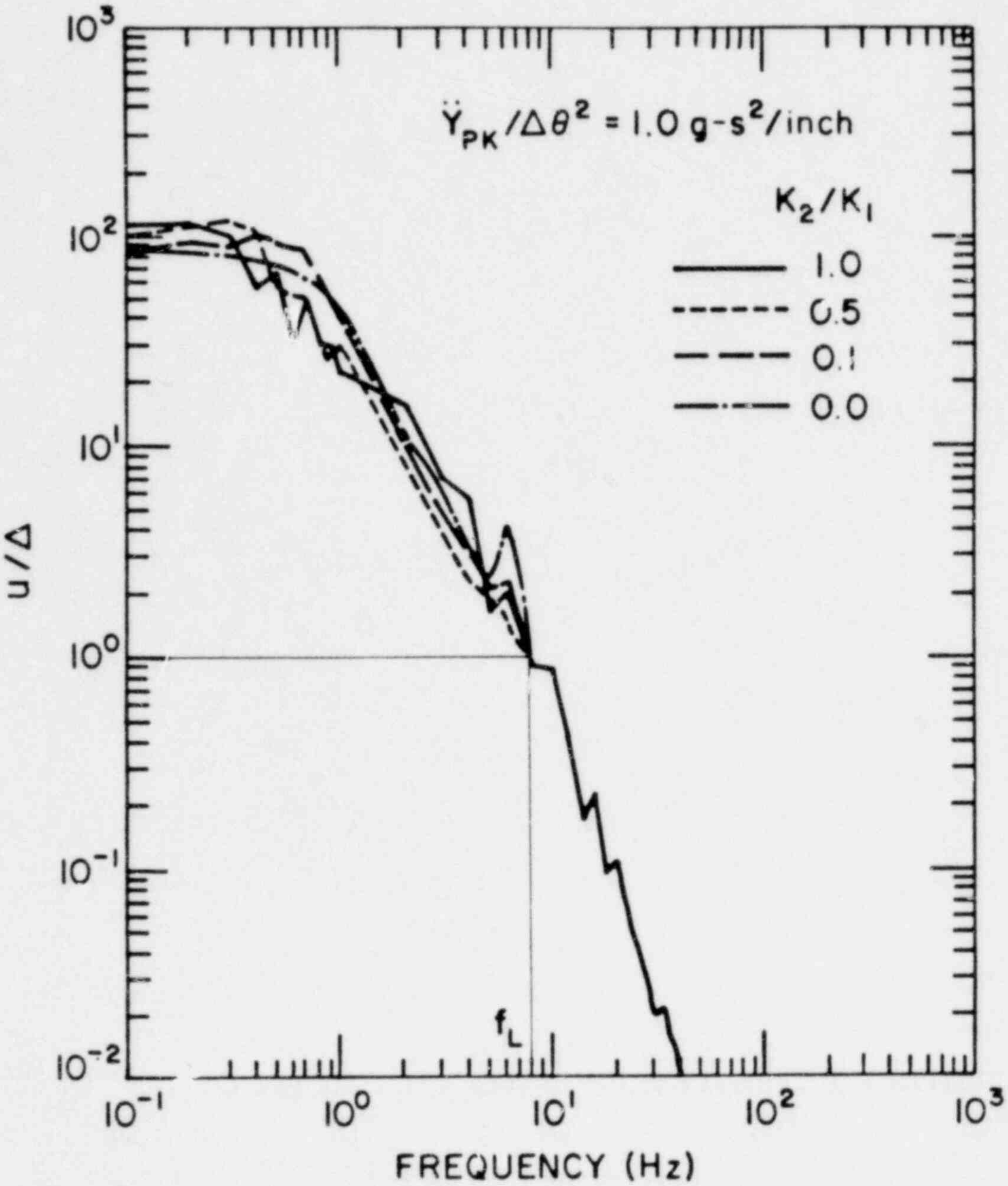


24
20
16
12
8
4
0
2
4
6
8
10
LINEAR REGION
NONLINEAR REGION
NOTE: FOR THE EARTHQUAKE WHOSE LINEAR RESPONSE SPECTRUM IS SHOWN IN FIG. 5
f_L (Hz)
$\ddot{Y}_{PK}/\Delta$ (g/inch)

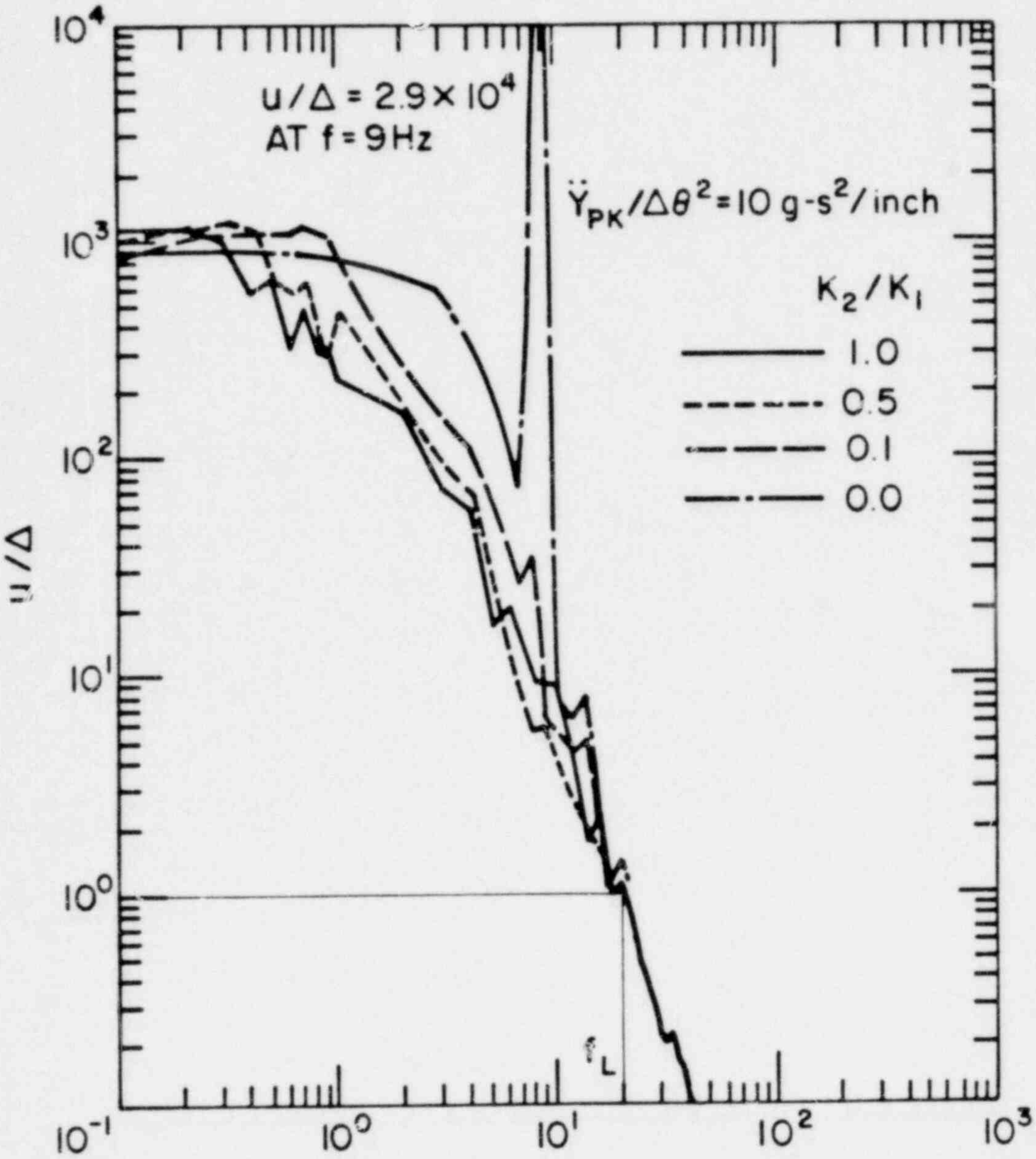


$\ddot{Y}_{PK}/\Delta\theta^2 = 1.0$ g-s²/inch
$K_2/K_1 = 0.5$
PSEUDO ABSOLUTE ACCELERATION
TRUE ABSOLUTE ACCELERATION
$\ddot{X}/\ddot{Y}_{PK}$
FREQUENCY (Hz)
10^{-2}
10^{-1}
10^0
10^1
10^2
10^3

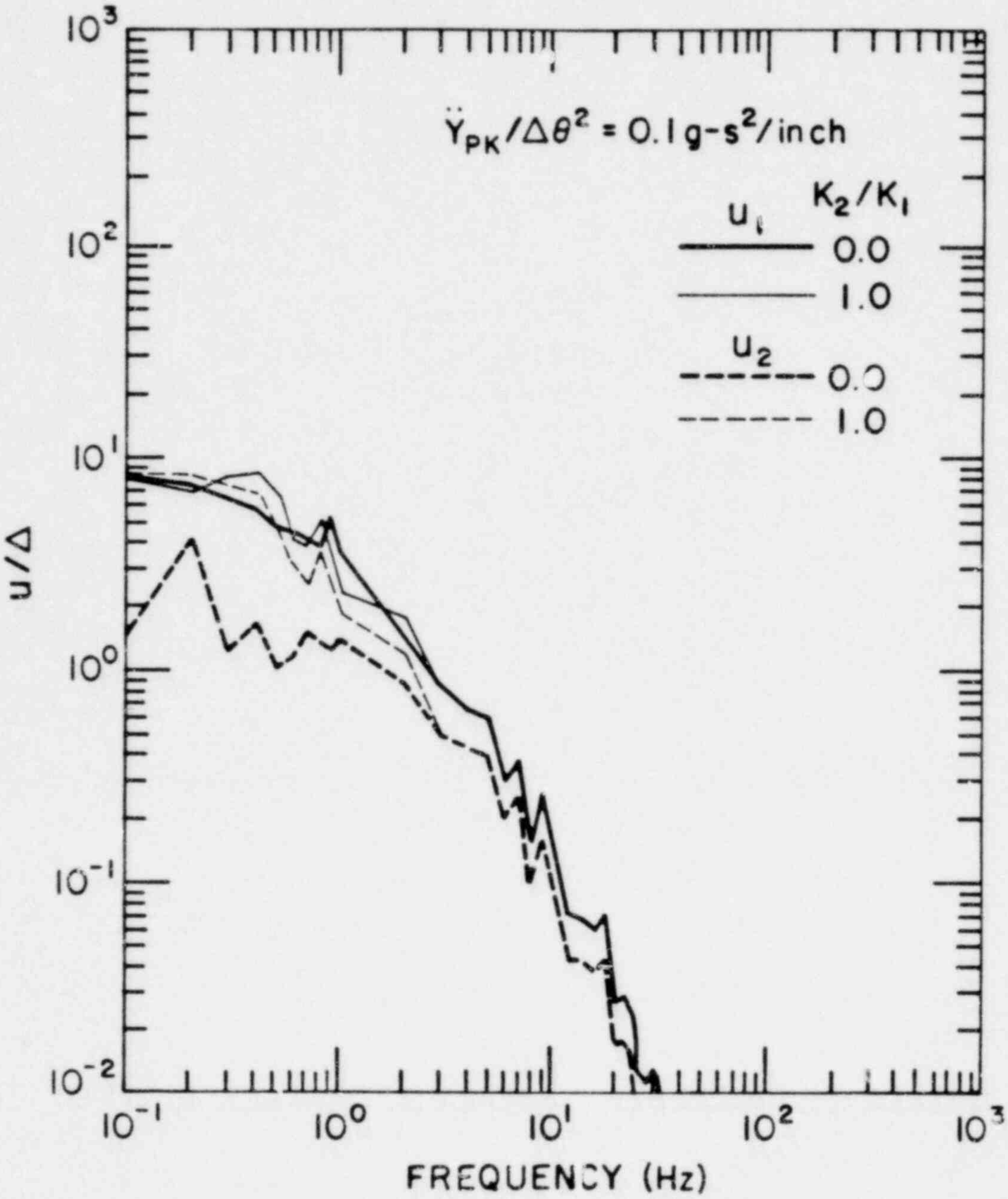

$\ddot{Y}_{PK}/\Delta\theta^2 = 0.1\ g\text{-}s^2/\text{inch}$
K_2/K_1
1.0
0.5
0.1
0.0
u/Δ
f_L
10^3
10^2
10^1
10^0
10^{-1}
10^{-2}
10^{-1}
10^0
10^1
10^2
10^3
FREQUENCY (Hz)



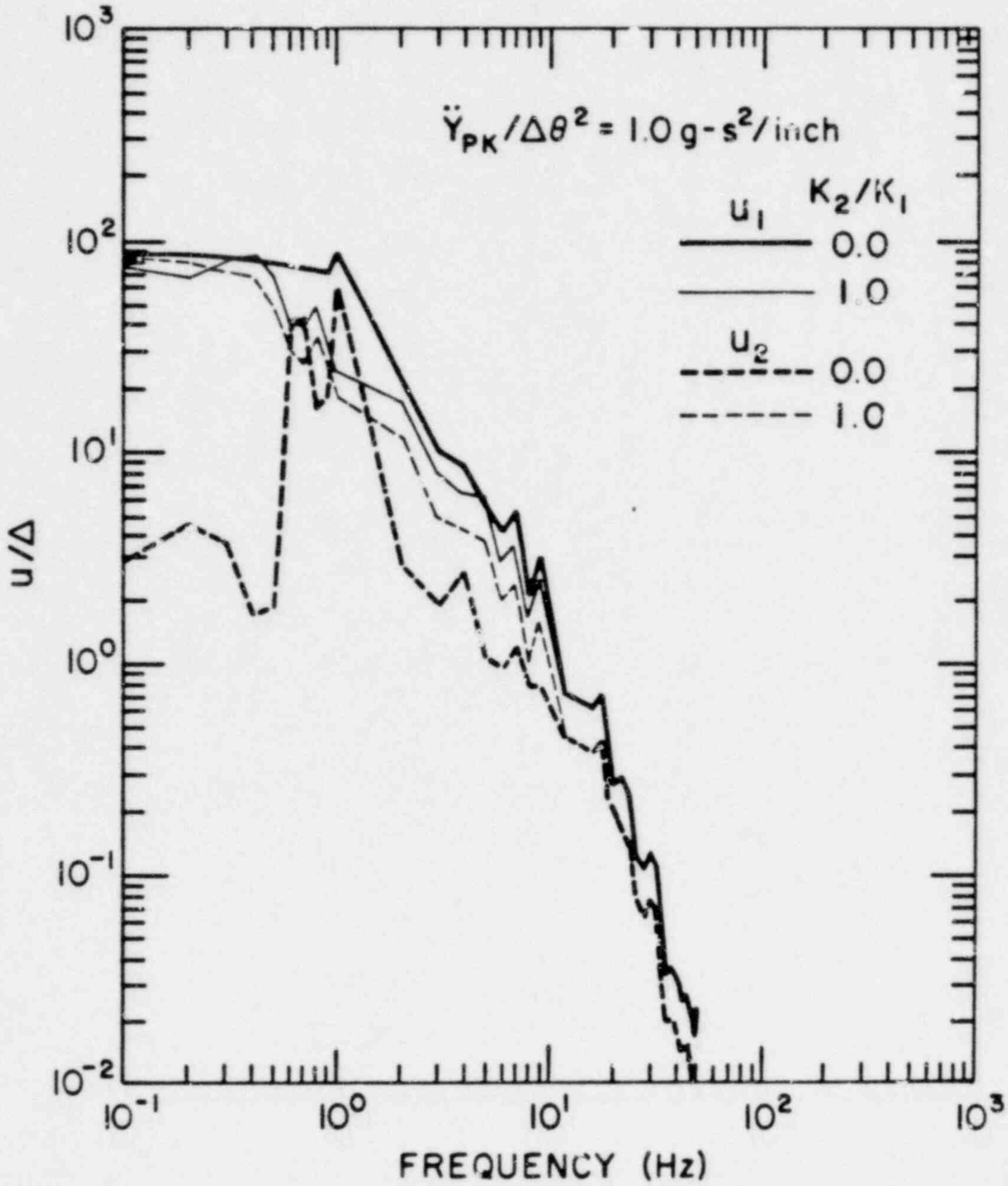
$\ddot{Y}_{PK}/\Delta\theta^2 = 1.0$ g-s²/inch
K_2/K_1
1.0
0.5
0.1
0.0
10^3
10^2
10^1
10^0
10^{-1}
10^{-2}
u/Δ
f_L
10^{-1}
10^0
10^1
10^2
10^3
FREQUENCY (Hz)



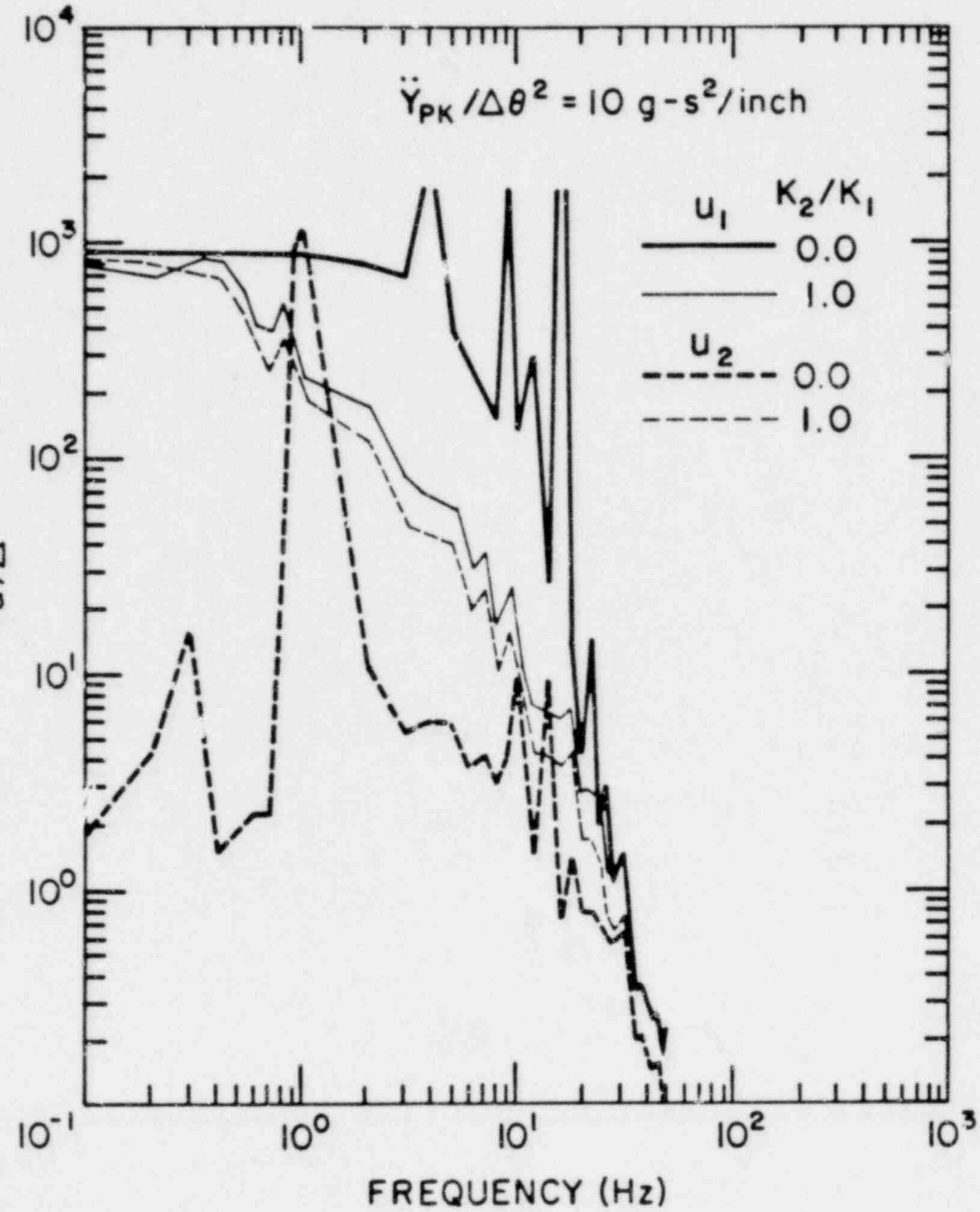
$u/\Delta = 2.9 \times 10^4$
AT f = 9Hz
$\ddot{Y}_{PK}/\Delta\theta^2 = 10$ g-s^2/inch
K_2/K_1
1.0
0.5
0.1
0.0
u/Δ
f_L
FREQUENCY (Hz)



$\ddot{Y}_{PK}/\Delta\theta^2 = 0.1\,g\text{-}s^2/inch$
u_1
K_2/K_1
0.0
1.0
u_2
0.0
1.0
u/Δ
10^3
10^2
10^1
10^0
10^{-1}
10^{-2}
10^{-1}
10^0
10^1
10^2
10^3
FREQUENCY (Hz)



$\ddot{Y}_{PK}/\Delta\theta^2 = 1.0$ g-s²/inch
u_1
K_2/K_1
0.0
1.0
u_2
0.0
1.0
u/Δ
FREQUENCY (Hz)



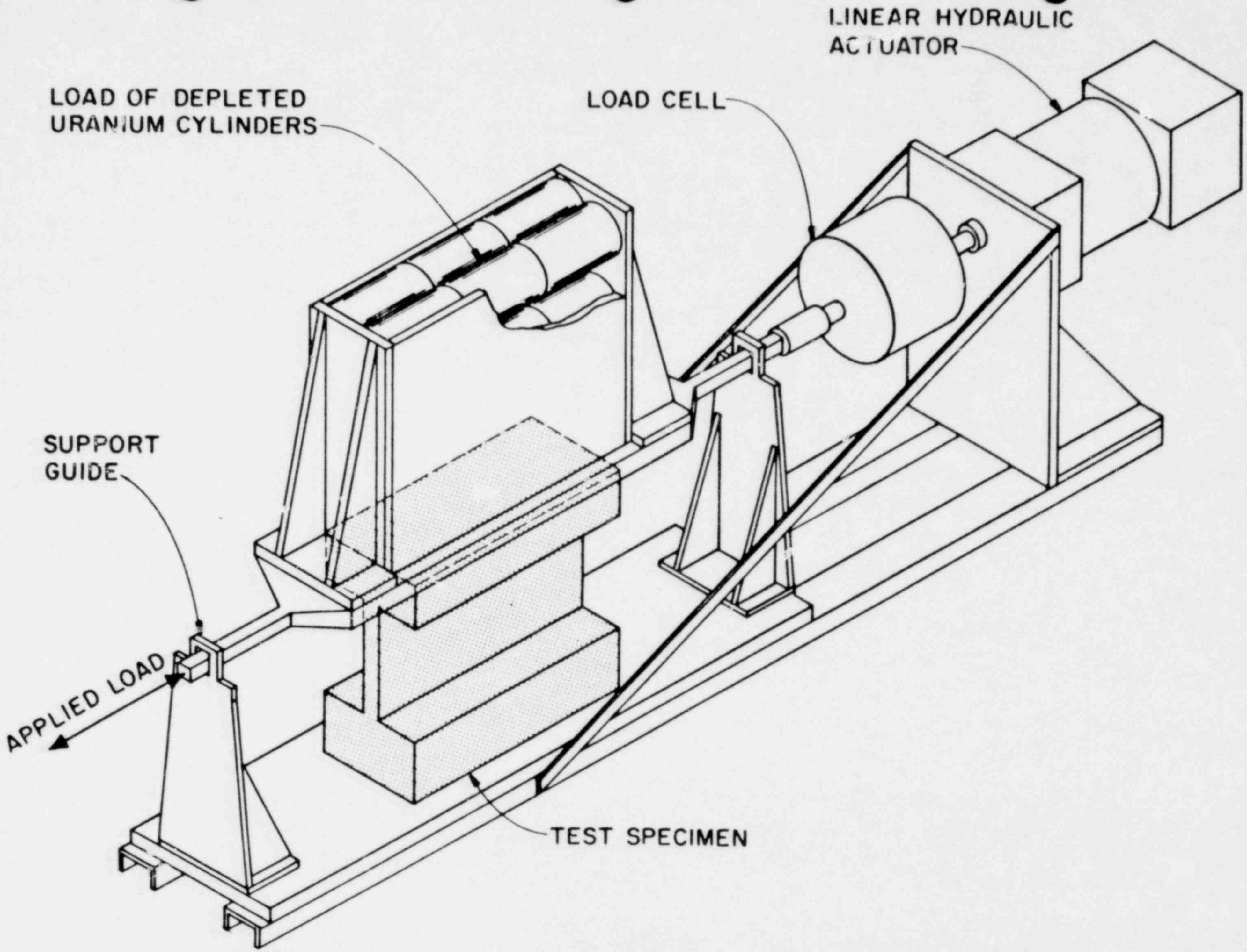
$\ddot{Y}_{PK} / \Delta\theta^2 = 10$ g-s^2/inch
u_1
K_2/K_1
0.0
1.0
u_2
0.0
1.0
10^4
10^3
10^2
10^1
10^0
u/Δ
10^{-1}
10^0
10^1
10^2
10^3
FREQUENCY (Hz)

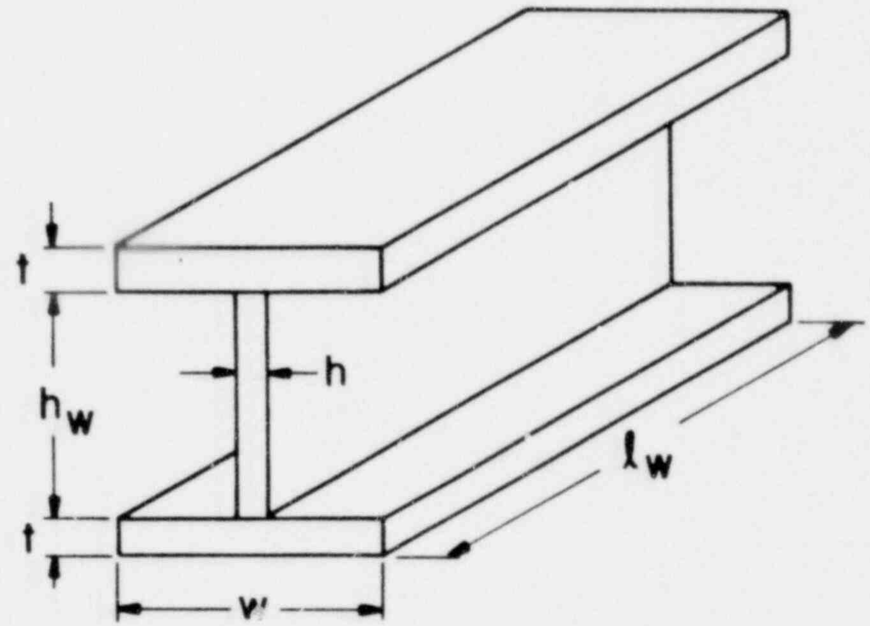
IMAGE EVALUATION
TEST TARGET (MT-3)

6"

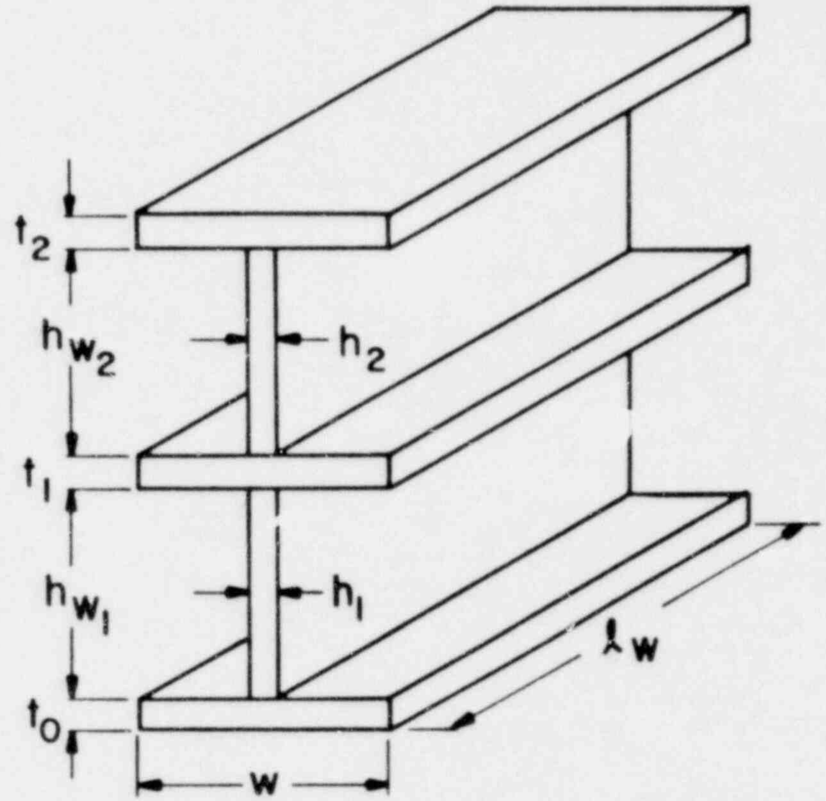
IMAGE EVALUATION
TEST TARGET (MT-3)

6"


LINEAR HYDRAULIC ACTUATOR
LOAD CELL
LOAD OF DEPLETED URANIUM CYLINDERS
SUPPORT GUIDE
APPLIED LOAD
TEST SPECIMEN



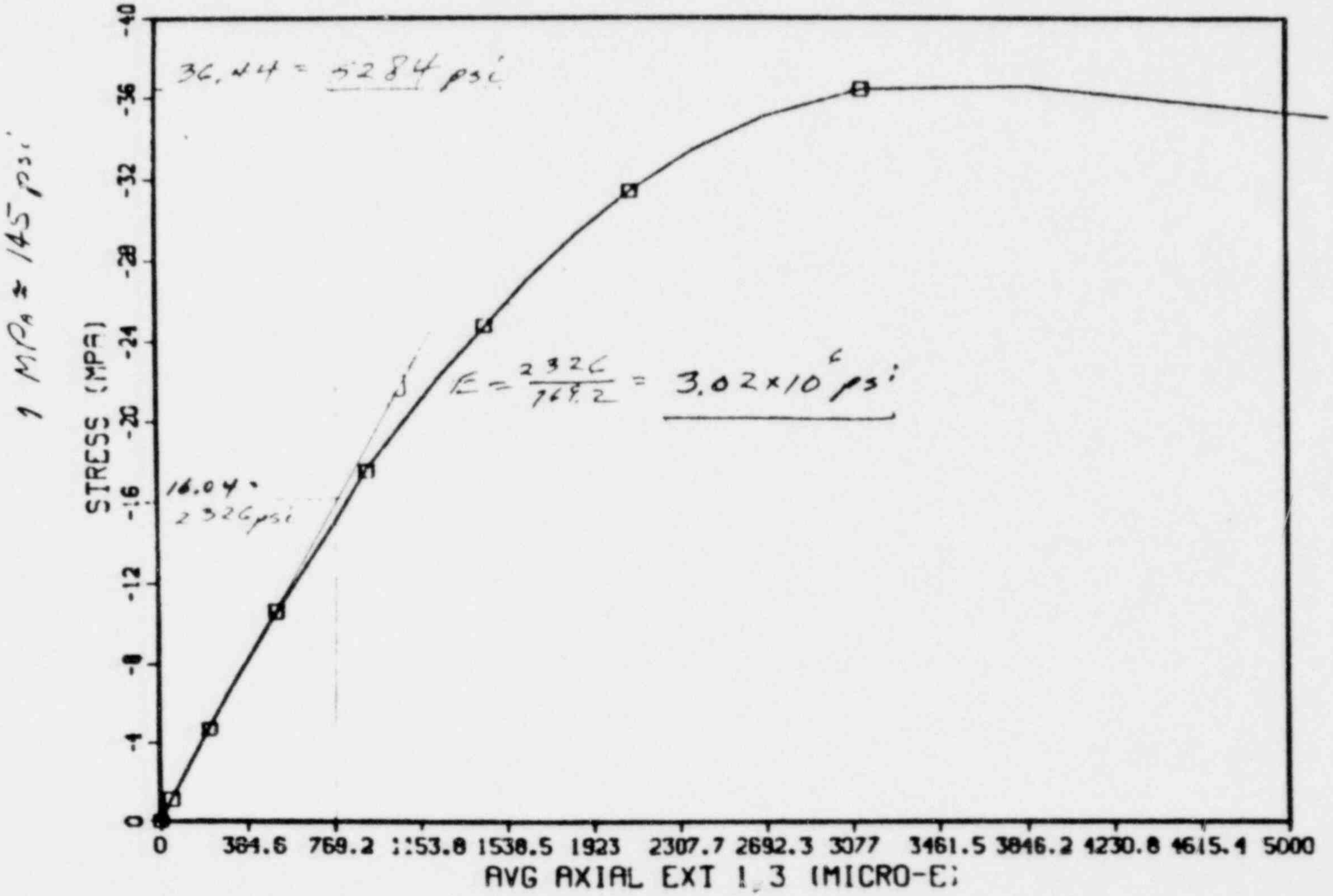
(a) ONE STORY SHEAR WALL



(b) TWO STORY SHEAR WALL

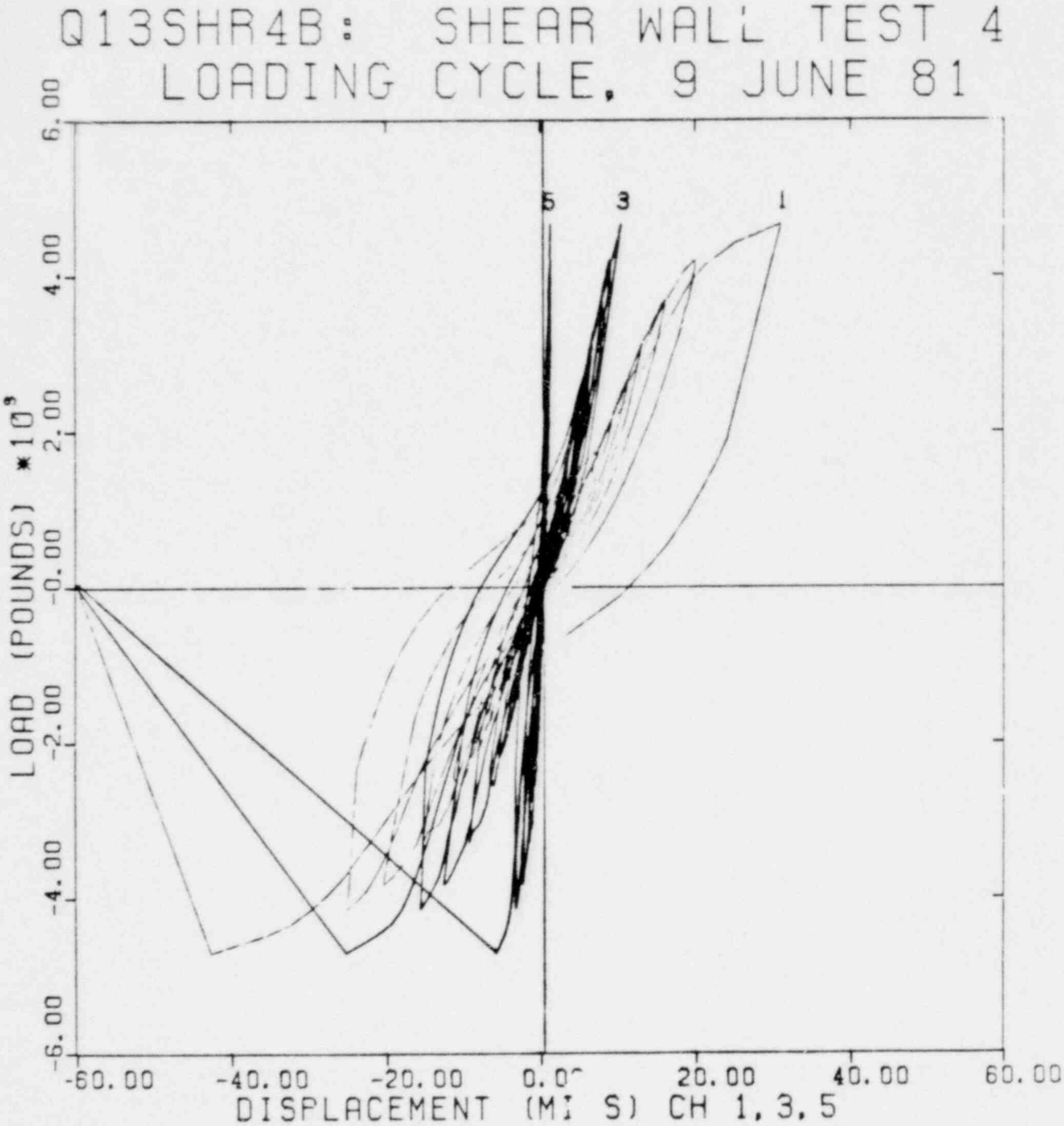
CORRECT STRESS CAL

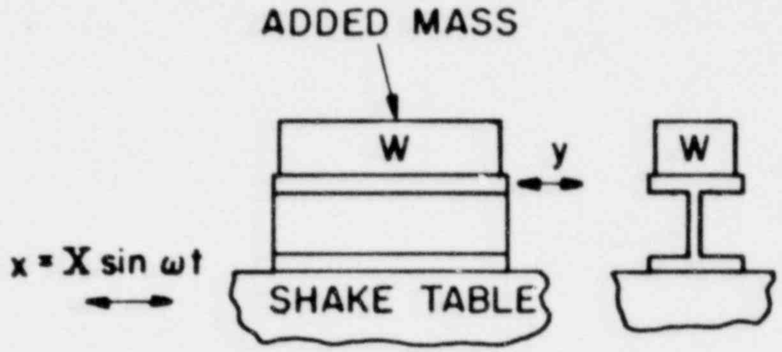
RDCON1 05:07:09:37:41



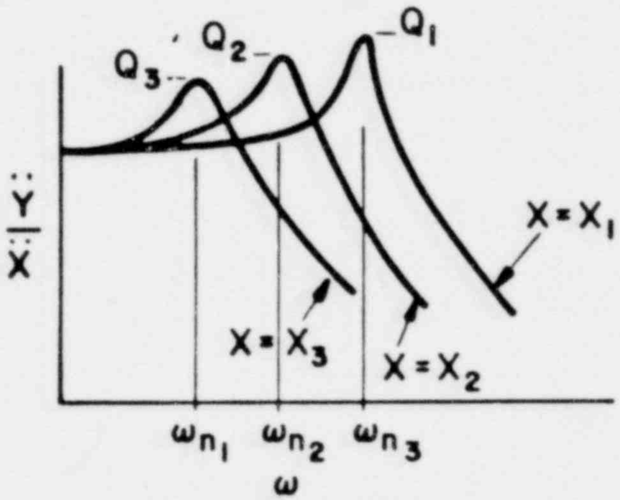
A Riccardi

LOS ALAMOS
WX-11 TEST UNIT



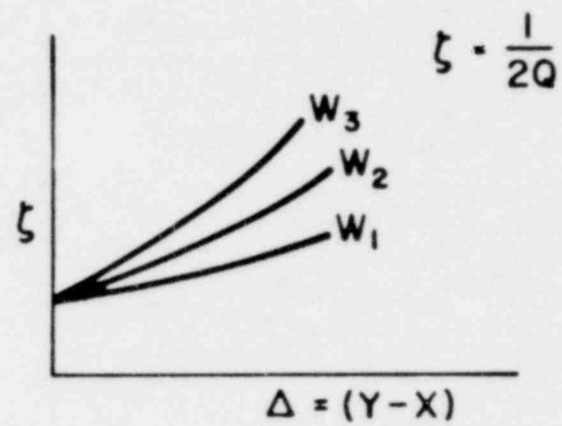


(a) METHOD OF LOADING



(b) MEASURED RESULTS

NOTE: SIMILAR CURVES FOR EACH VALUE OF W



(c) COMPUTED DAMPING

FUTURE ACTIVITIES

ANALYTICAL

Continue investigation of multi-story structure responses

Streamline computer codes

Write user manual

EXPERIMENTAL

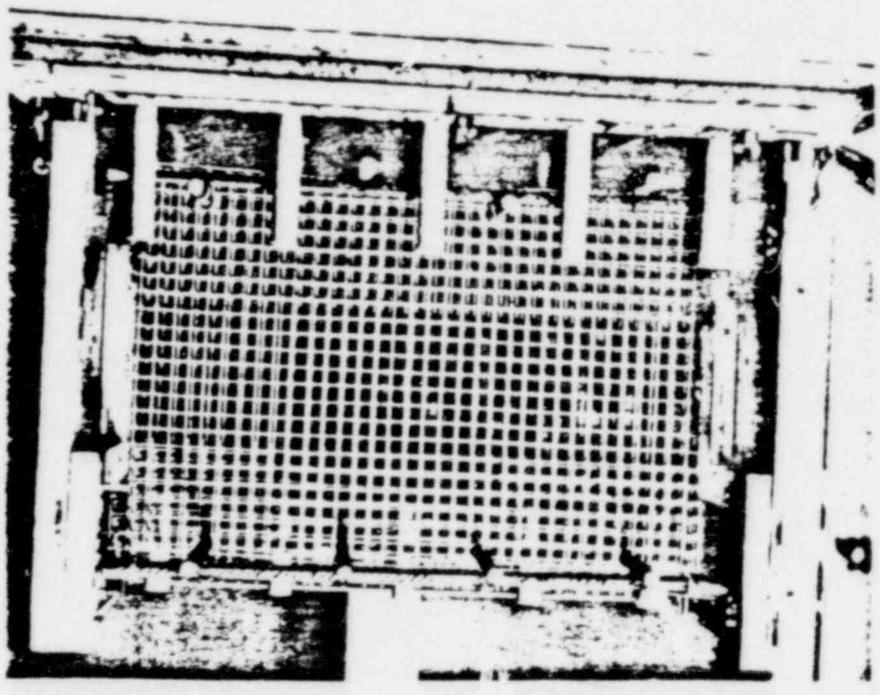
Complete static tests of small 1-story structures

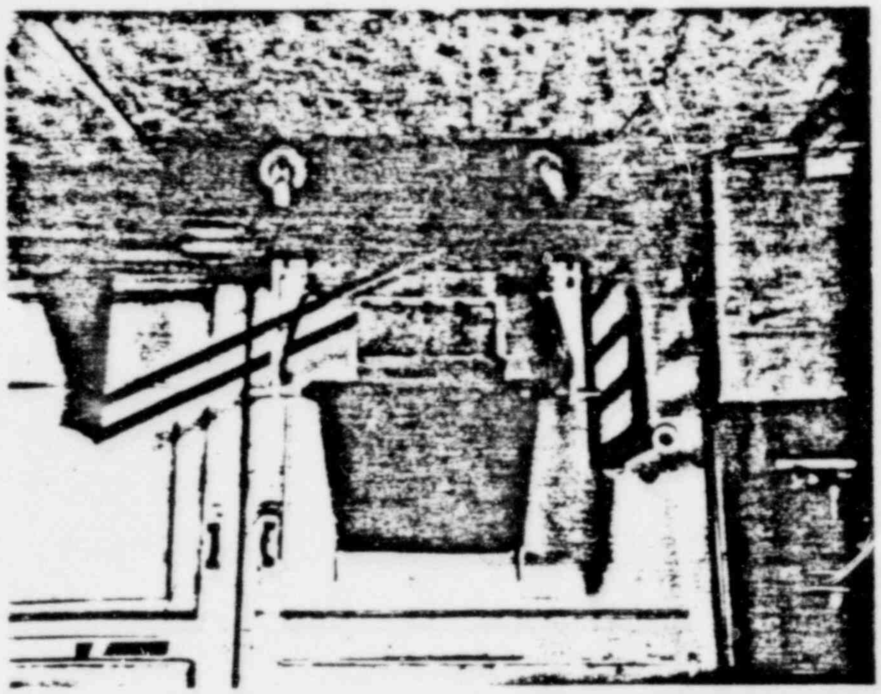
Start:

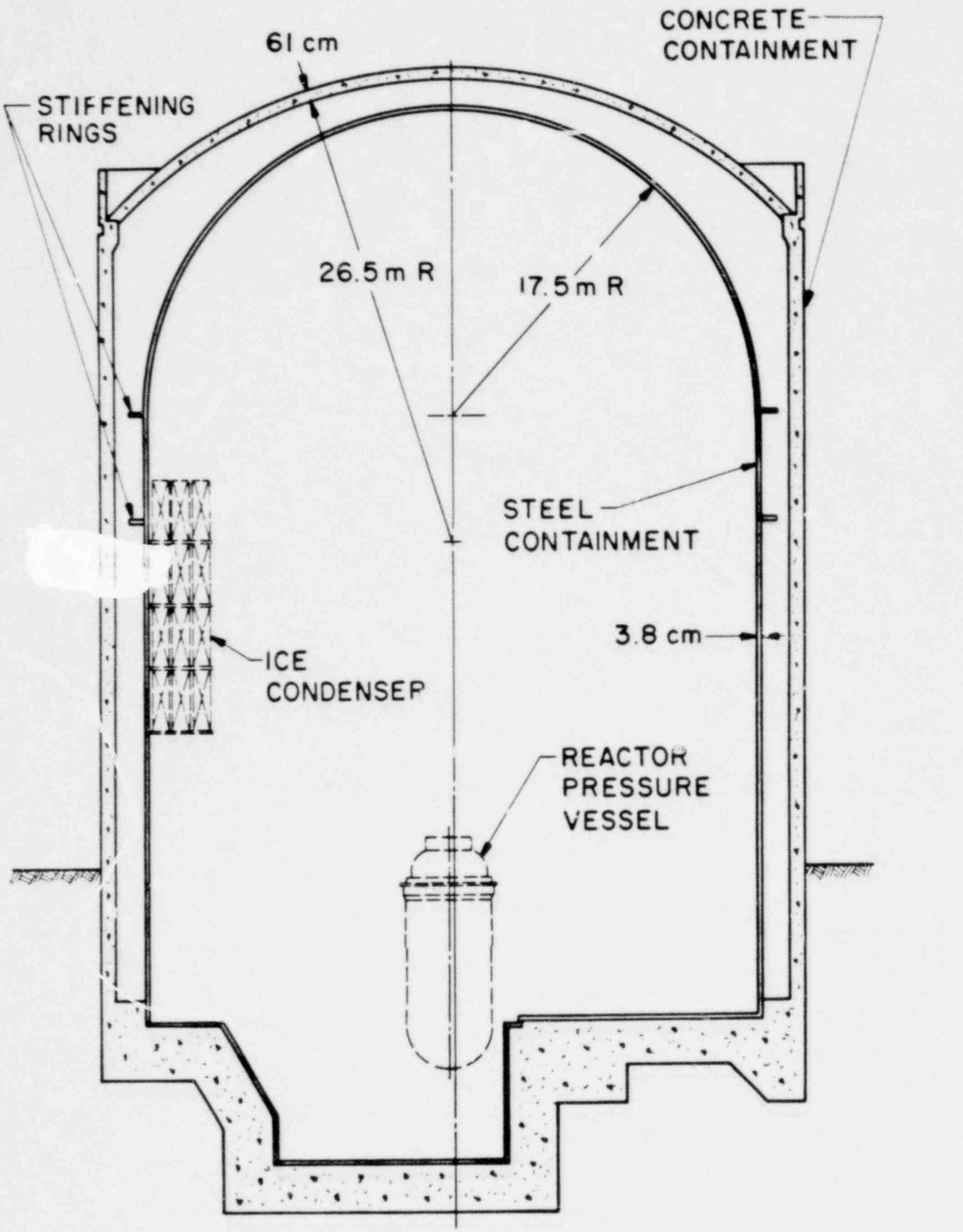
Static tests of small 2-story structures

Dynamic tests of small 1- and 2-story structures

Los Alamos





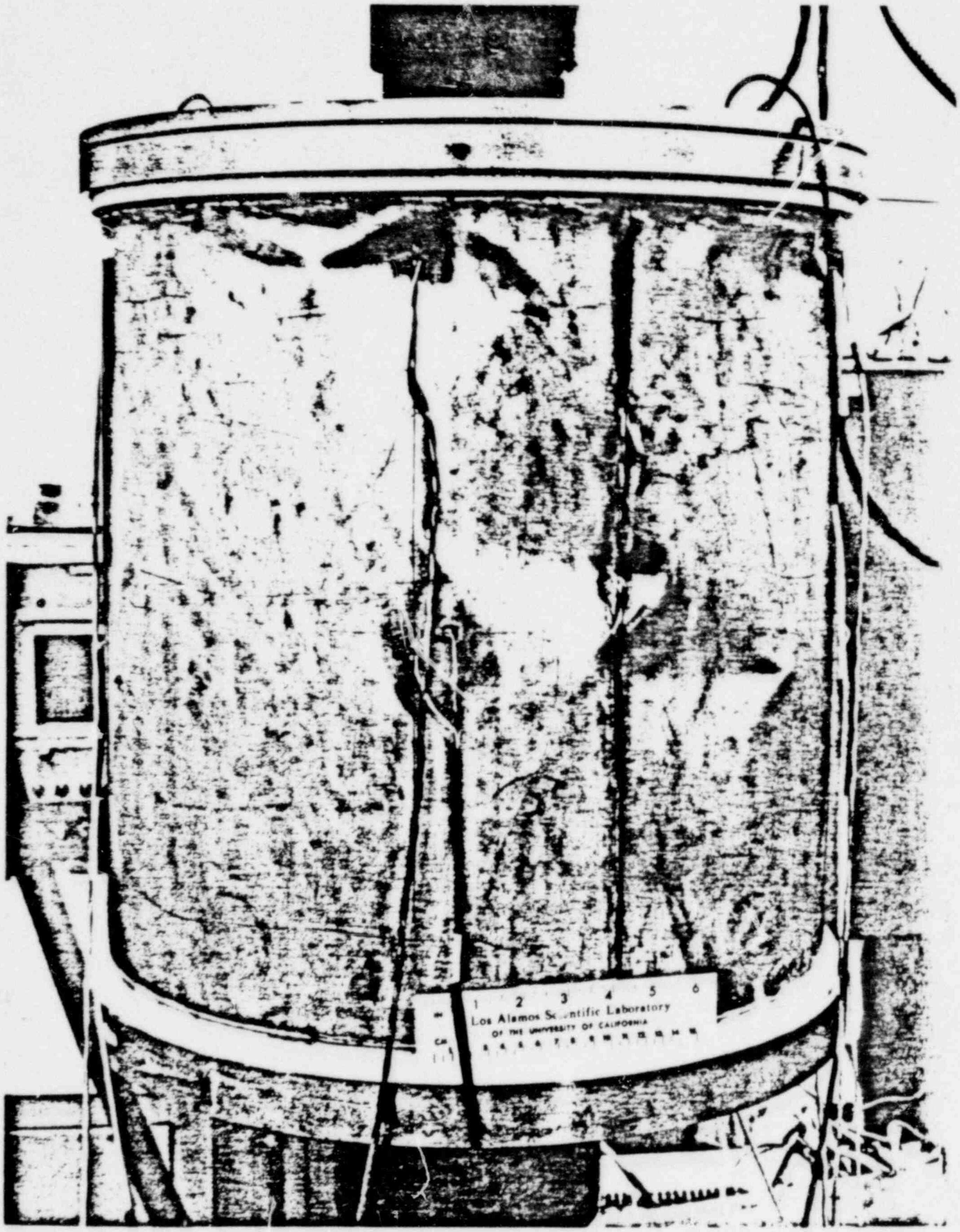

Bennett presentation
CONCRETE
CONTAINMENT
61 cm
STIFFENING
RINGS
26.5 m R
17.5 m R
STEEL
CONTAINMENT
3.8 cm
ICE
CONDENSER
REACTOR
PRESSURE
VESSEL

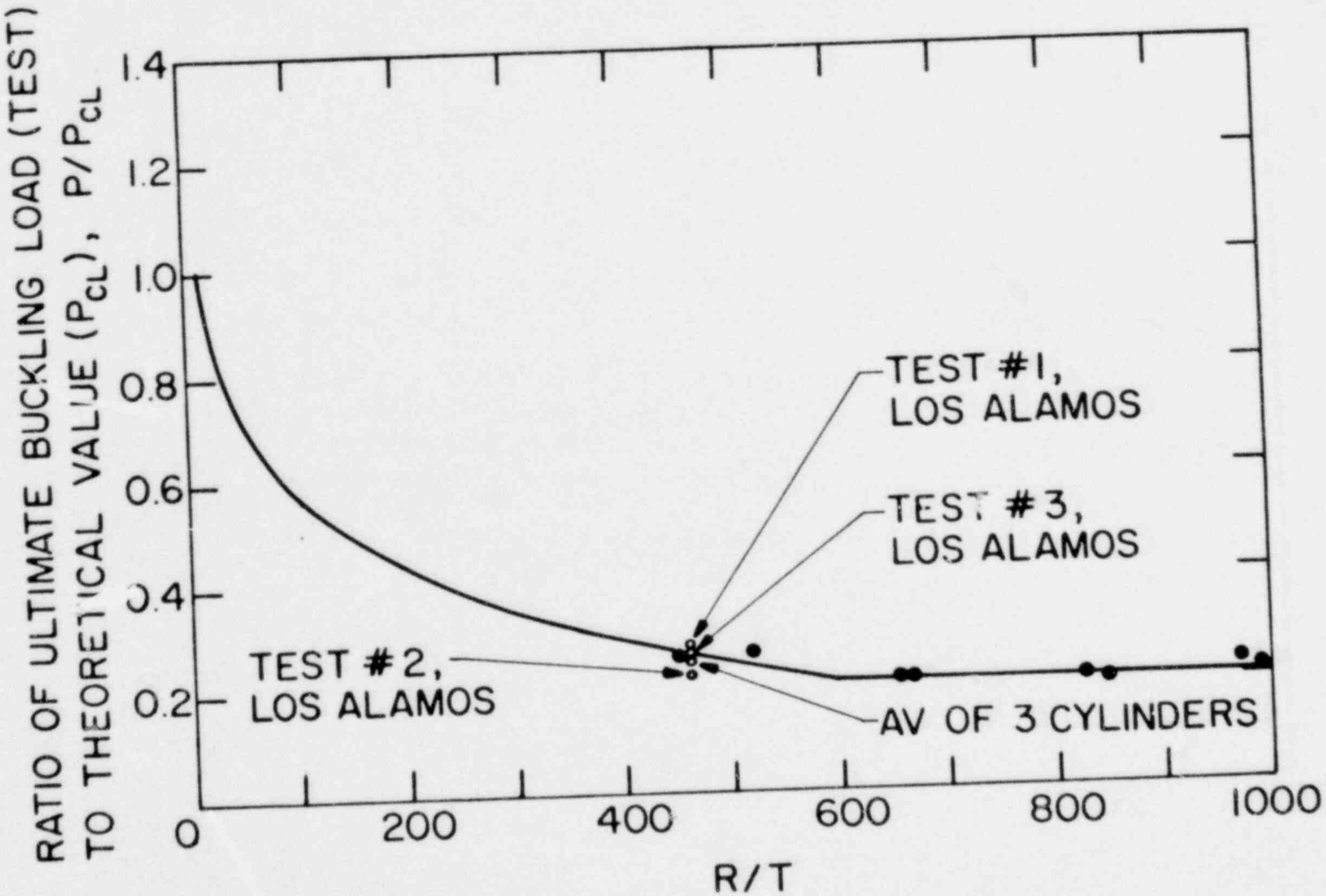
SUMMARY OF ASME AREA REPLACEMENT METHOD BUCKLING INVESTIGATION

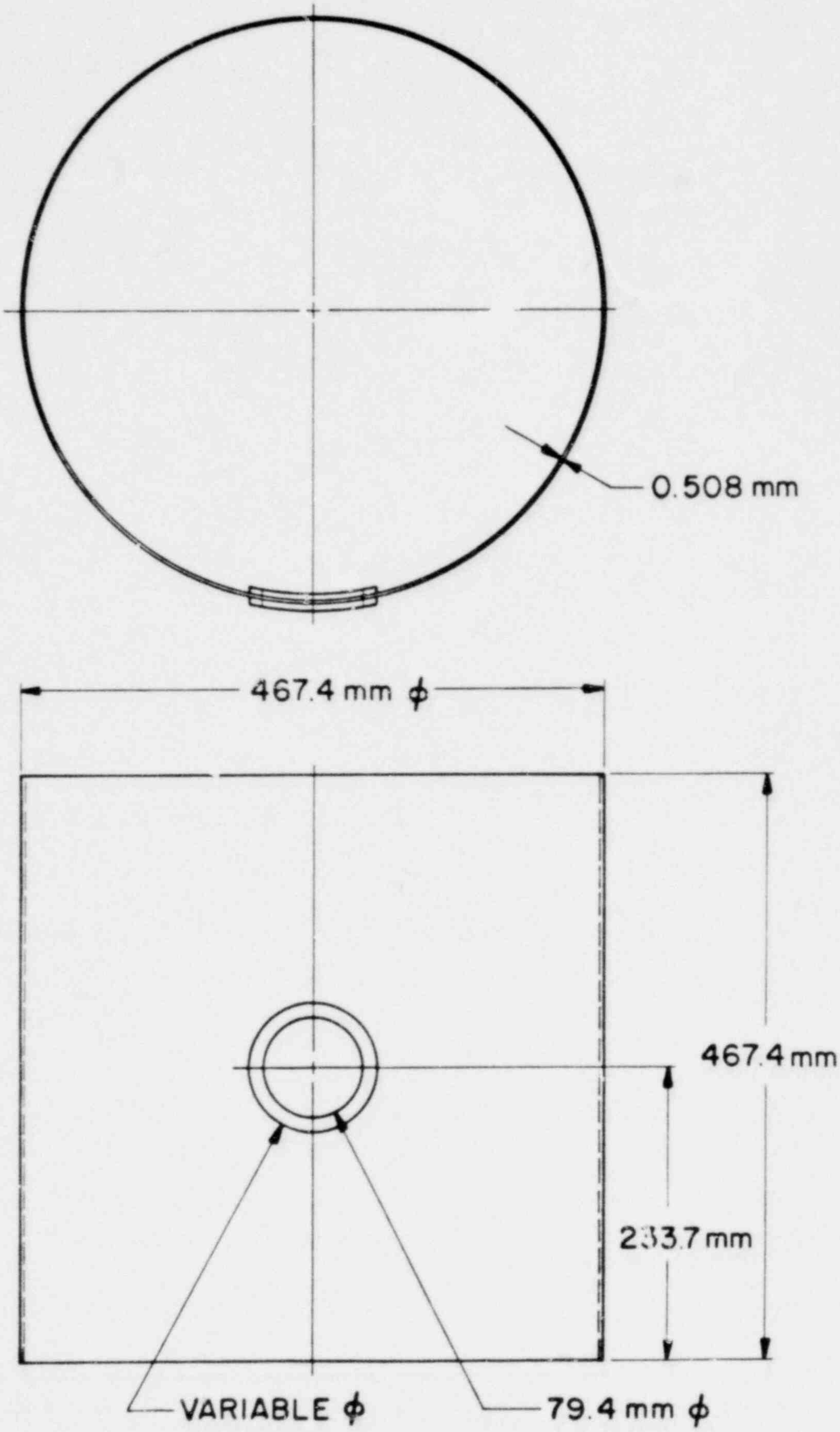
PREMISE: C. D. MILLER – C.B.I.

– MYLAR TEST OF A SINGLE CYLINDER

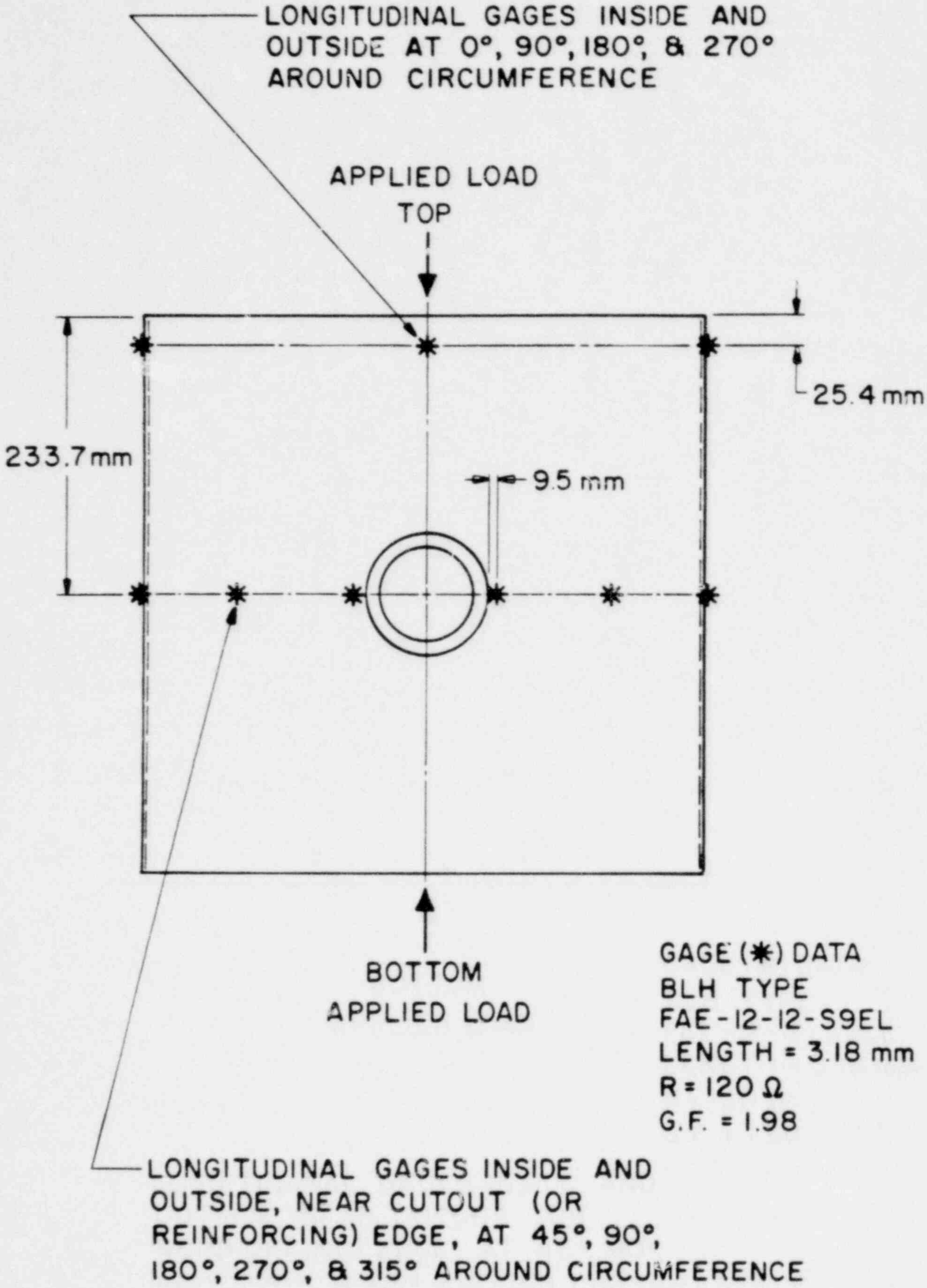
– IF ARM RULE IS FOLLOWED, THEN BUCKLING STRENGTH OF A PENETRATED CYLINDER WILL BE INCREASED ABOVE VALUE OF UNPENETRATED CYLINDER.

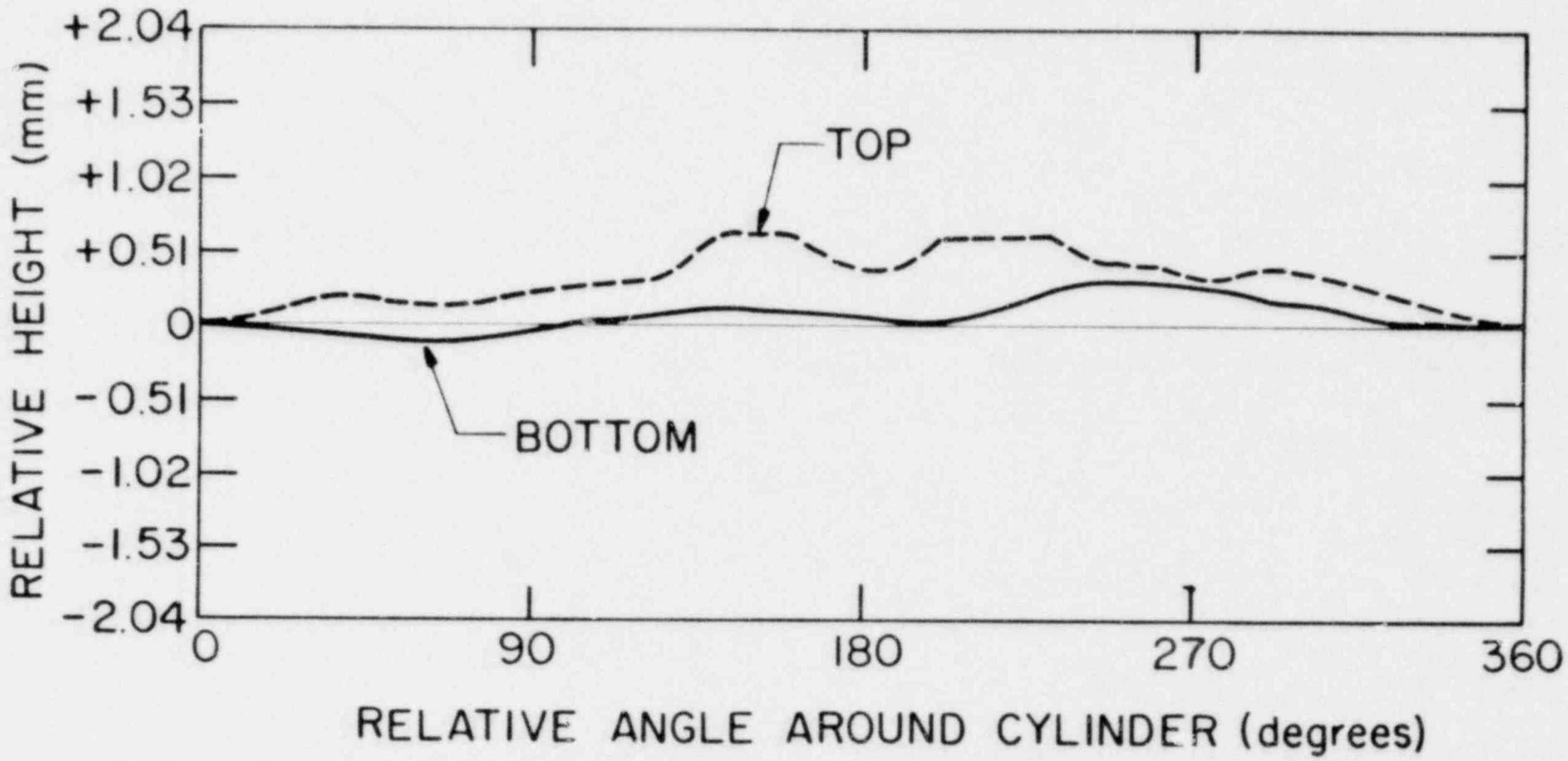

1 2 3 4 5 6
Los Alamos Scientific Laboratory
OF THE UNIVERSITY OF CALIFORNIA


RATIO OF ULTIMATE BUCKLING LOAD (TEST) TO THEORETICAL VALUE (P_{CL}), P/P_{CL}
1.4
1.2
1.0
0.8
0.6
0.4
0.2
0
200
400
600
800
1000
R/T
TEST #1, LOS ALAMOS
TEST #3, LOS ALAMOS
TEST #2, LOS ALAMOS
AV OF 3 CYLINDERS

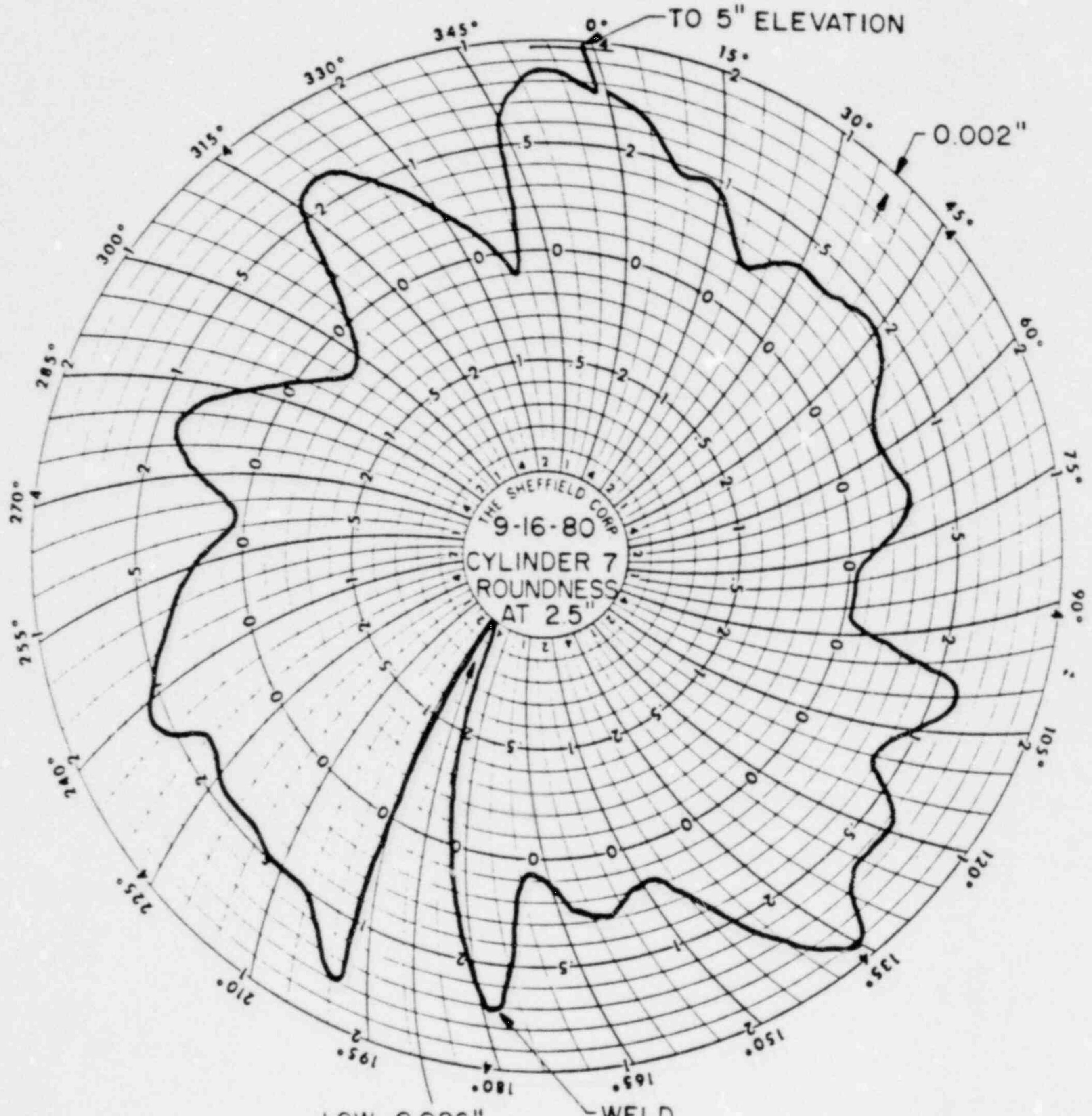


MATERIAL: STAINLESS STEEL STRIP,
WALL TOLERANCE < 0.025 mm

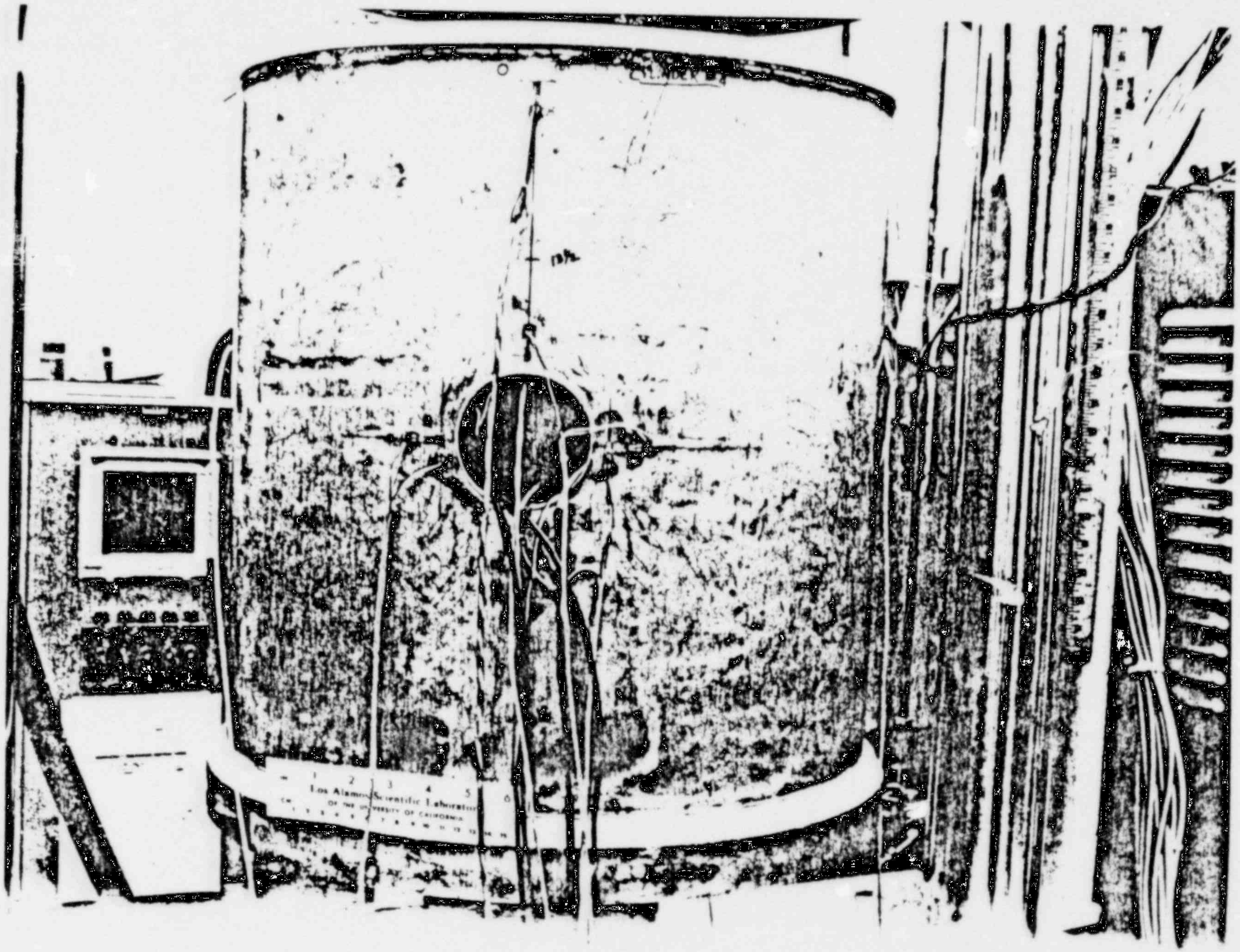

LONGITUDINAL GAGES INSIDE AND OUTSIDE AT 0°, 90°, 180°, & 270° AROUND CIRCUMFERENCE
APPLIED LOAD
TOP
25.4 mm
233.7 mm
9.5 mm
BOTTOM
APPLIED LOAD
GAGE (✱) DATA
BLH TYPE
FAE-12-12-S9EL
LENGTH = 3.18 mm
R = 120 Ω
G.F. = 1.98
LONGITUDINAL GAGES INSIDE AND OUTSIDE, NEAR CUTOUT (OR REINFORCING) EDGE, AT 45°, 90°, 180°, 270°, & 315° AROUND CIRCUMFERENCE

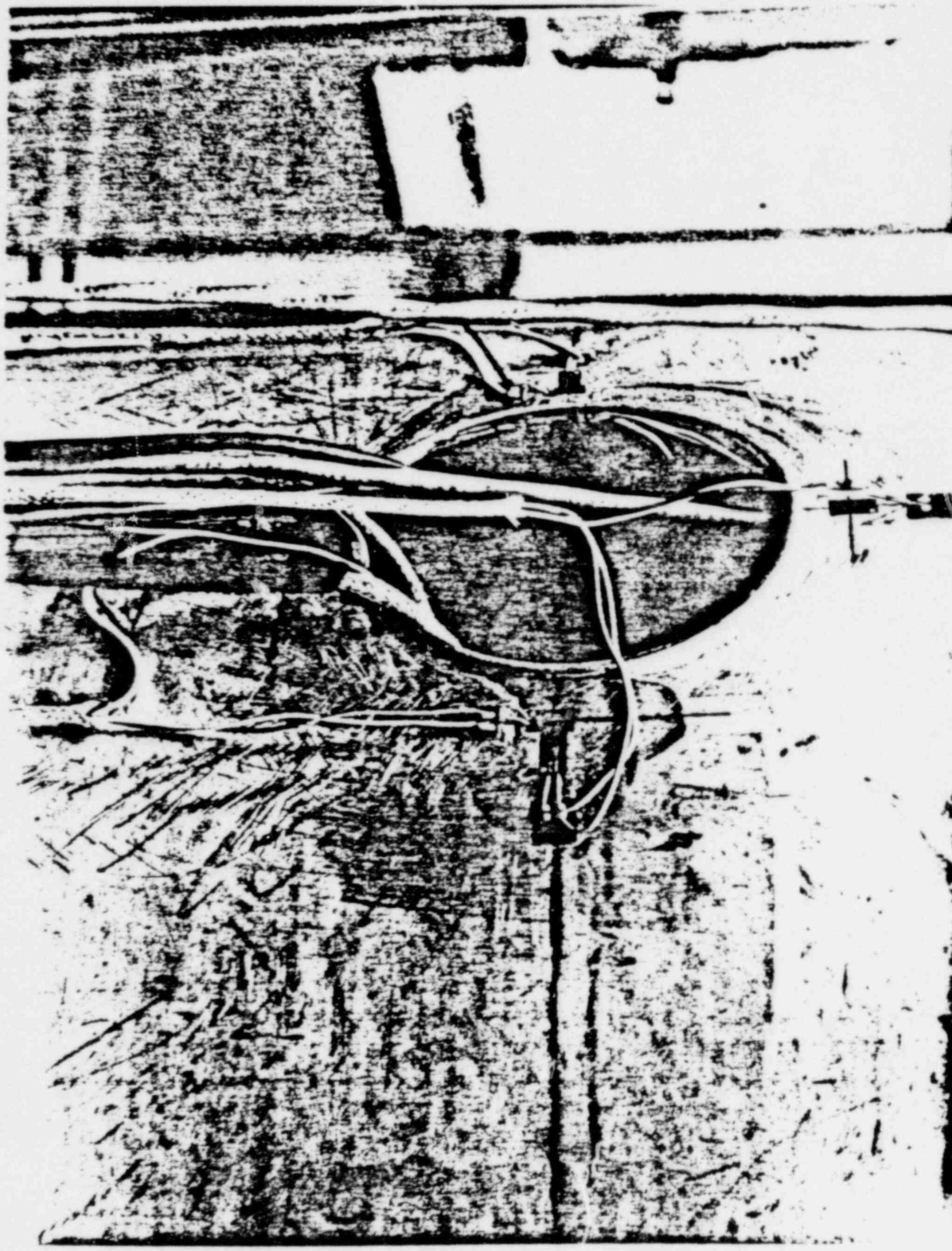


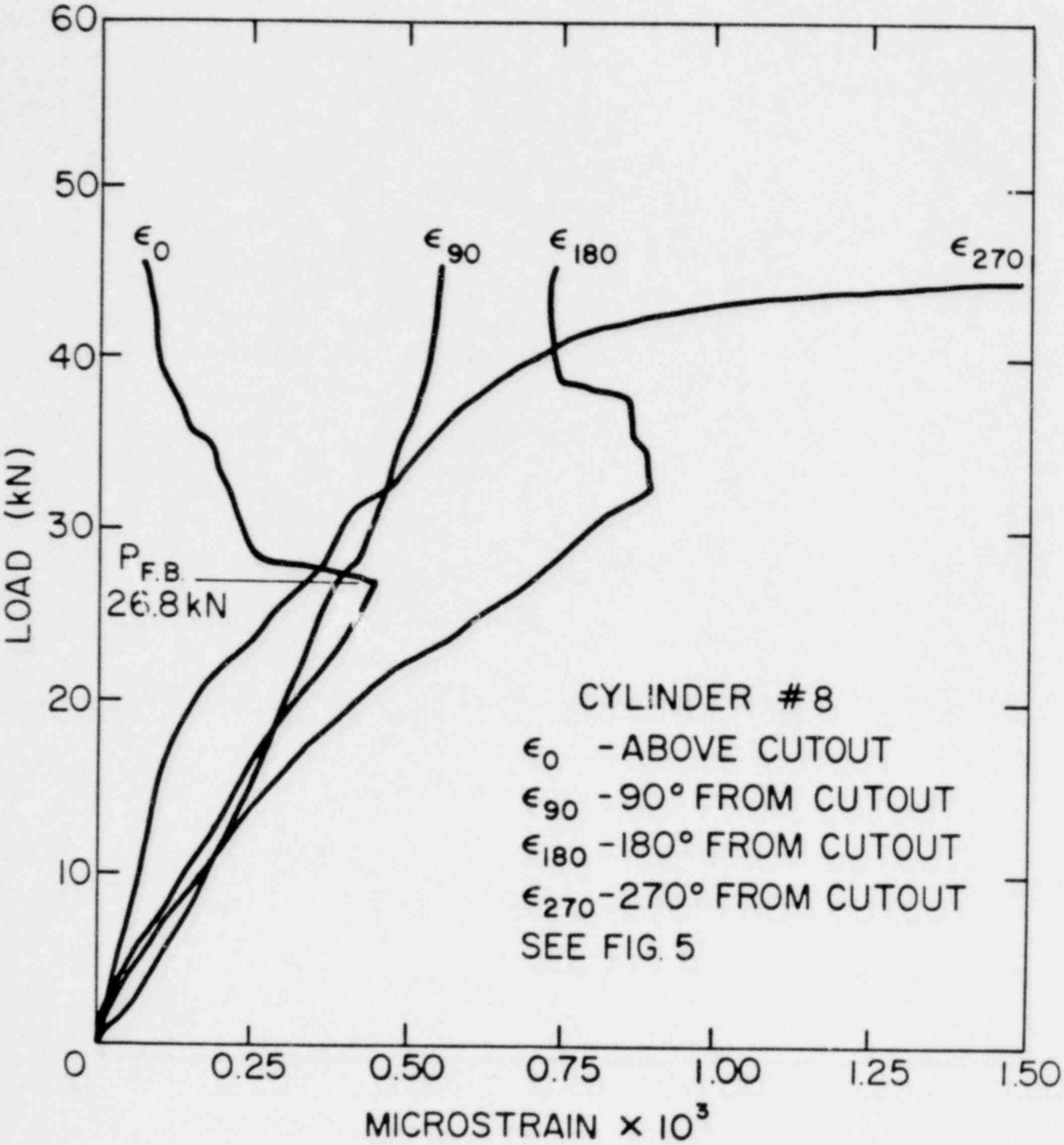
+2.04
+1.53
+1.02
+0.51
0
-0.51
-1.02
-1.53
-2.04
RELATIVE HEIGHT (mm)
TOP
BOTTOM
0
90
180
270
360
RELATIVE ANGLE AROUND CYLINDER (degrees)



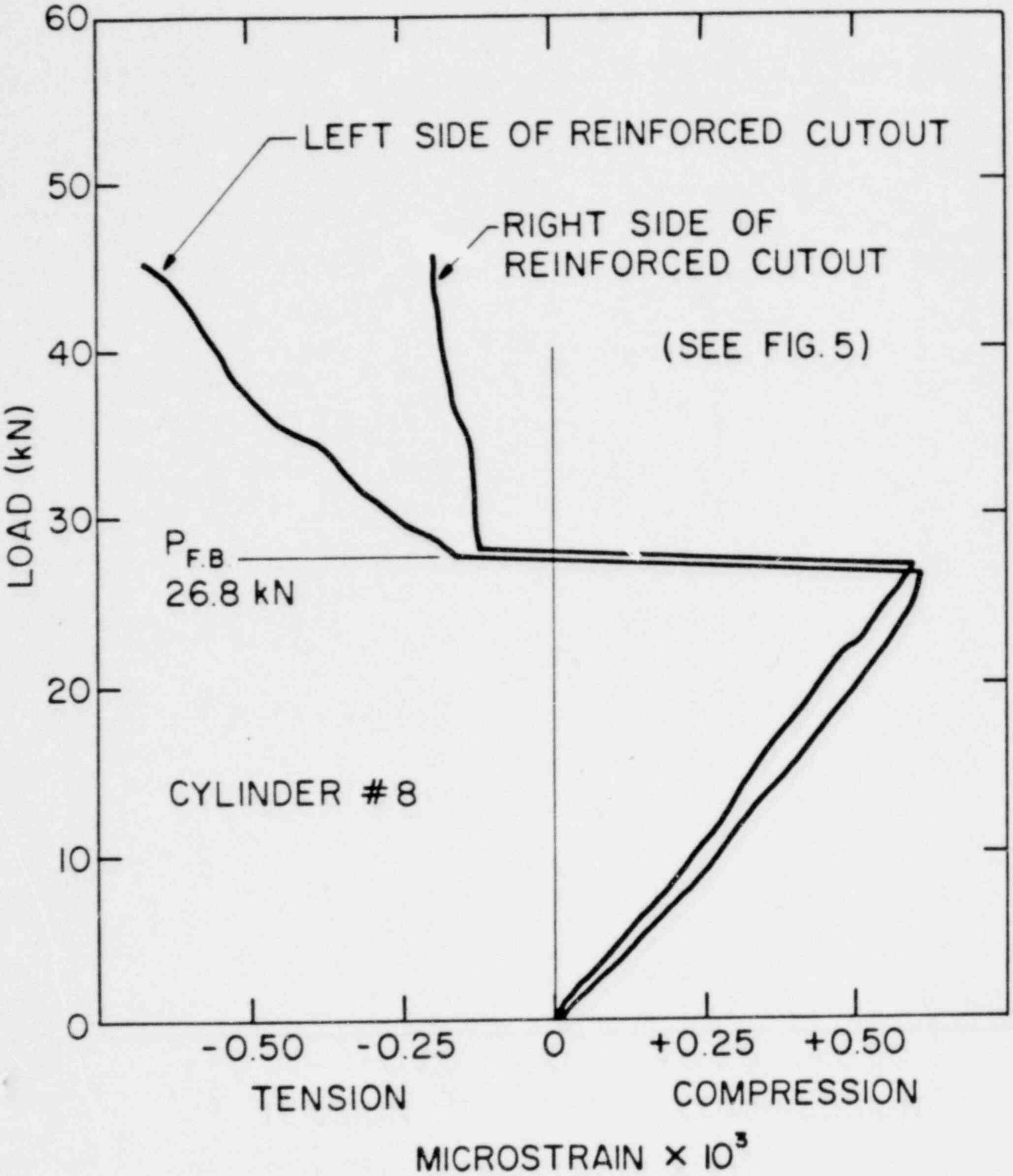
TO 5" ELEVATION
0.002"
THE SHEFFIELD CORP
9-16-80
CYLINDER 7
ROUNDNESS
AT 2.5"
LOW-0.026"
WELD


Los Alamos Scientific Laboratory
OF THE UNIVERSITY OF CALIFORNIA

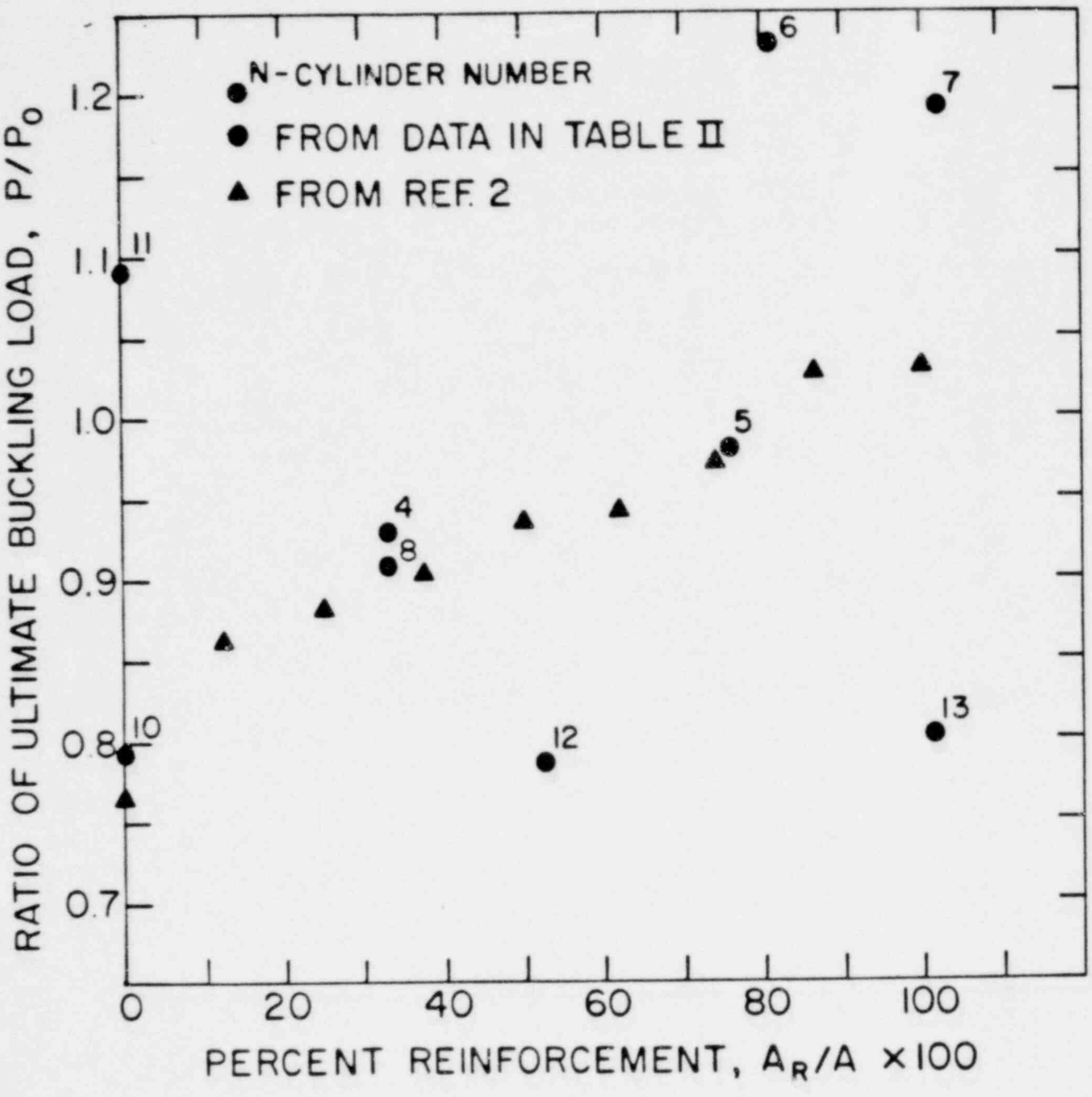




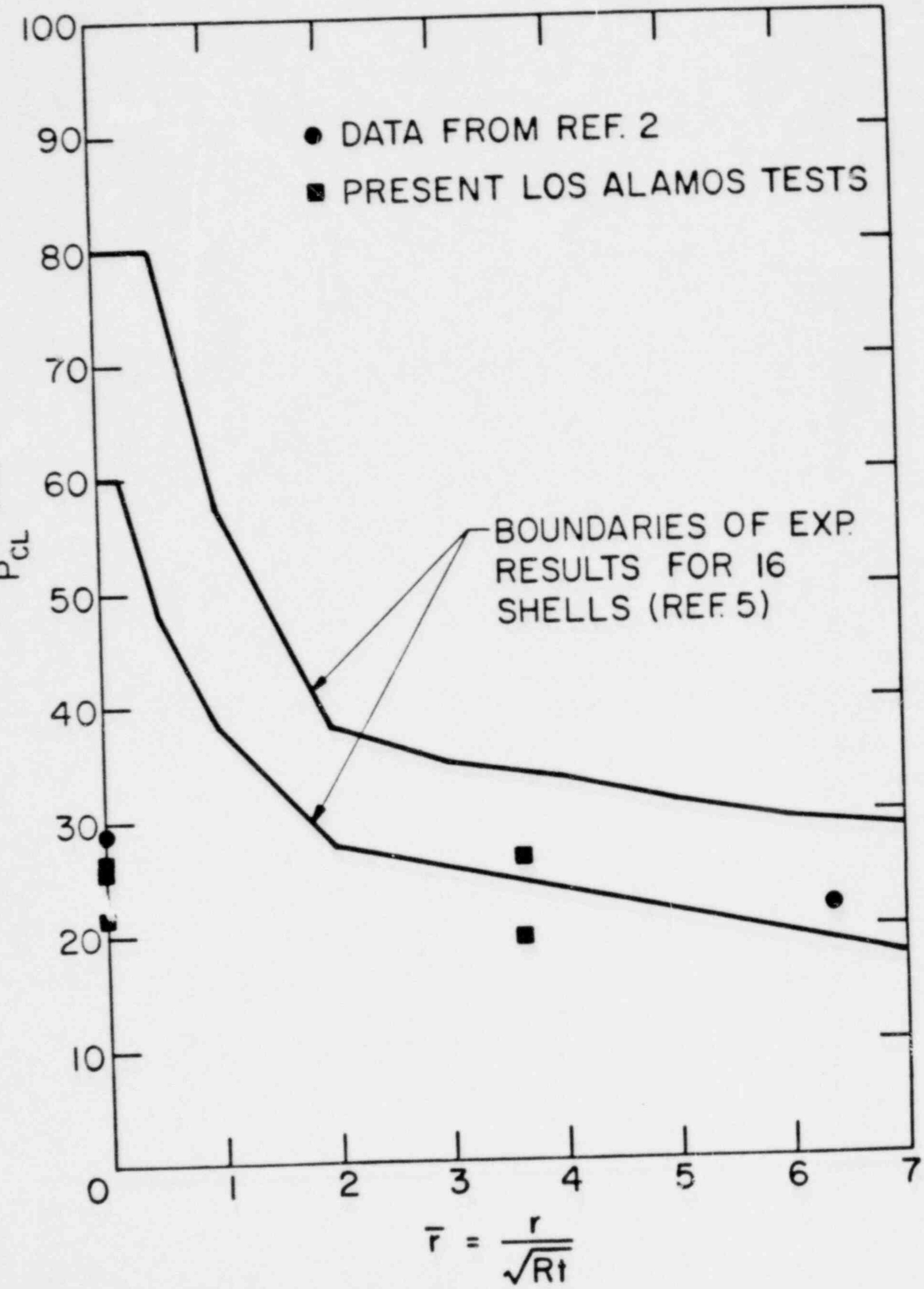
60
50
40
30
20
10
0
LOAD (kN)
0.25
0.50
0.75
1.00
1.25
1.50
MICROSTRAIN × 10³
ϵ0
ϵ90
ϵ180
ϵ270
P F.B.
26.8 kN
CYLINDER #8
ϵ0 -ABOVE CUTOUT
ϵ90 -90° FROM CUTOUT
ϵ180 -180° FROM CUTOUT
ϵ270 -270° FROM CUTOUT
SEE FIG. 5



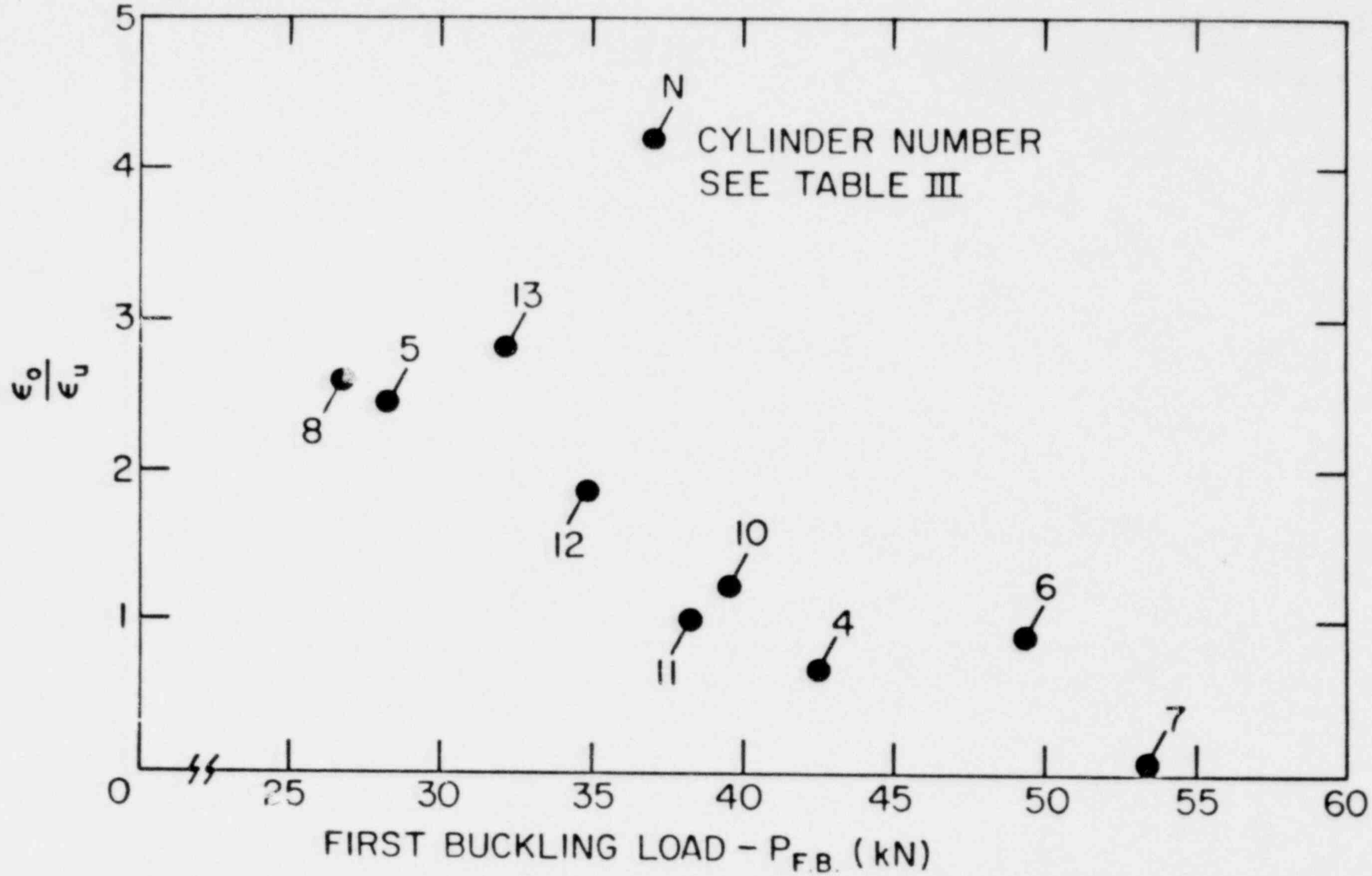
60
50
40
30
20
10
0
LOAD (kN)
LEFT SIDE OF REINFORCED CUTOUT
RIGHT SIDE OF REINFORCED CUTOUT
(SEE FIG. 5)
$P_{F.B.}$
26.8 kN
CYLINDER #8
-0.50
-0.25
0
+0.25
+0.50
TENSION
COMPRESSION
MICROSTRAIN × 10^3



RATIO OF ULTIMATE BUCKLING LOAD, P/P_0
N-CYLINDER NUMBER
FROM DATA IN TABLE II
FROM REF. 2
1.2
1.1
1.0
0.9
0.8
0.7
11
6
7
5
4
8
10
12
13
0
20
40
60
80
100
PERCENT REINFORCEMENT, $A_R/A \times 100$



100
90
80
70
60
50
40
30
20
10
0
1
2
3
4
5
6
7
DATA FROM REF. 2
PRESENT LOS ALAMOS TESTS
BOUNDARIES OF EXP. RESULTS FOR 16 SHELLS (REF. 5)
P/P_CL (%)
r̄ = r/√(Rt)



5
4
3
2
1
0
N
CYLINDER NUMBER
SEE TABLE III
13
5
8
12
10
11
4
6
7
25
30
35
40
45
50
55
60
FIRST BUCKLING LOAD - $P_{F.B.}$ (kN)

SMtoF – CB – current status cont.

COMPUTER CODE EVALUATION AND SELECTION

STAGSC1 – Structural Analysis of General Shells

- – Lockheed code – (users group forming)
  - – bifurcation buckling capability
  - – nonlinear collapse anaylsis capability

BOSOR4&5 – Buckling and stress analysis of ring stiffened, branched Shells of Revolution

- – Lockheed code
  - – axisymmetric or nonaxisymmetric loading
  - – elastic/plastic material capability

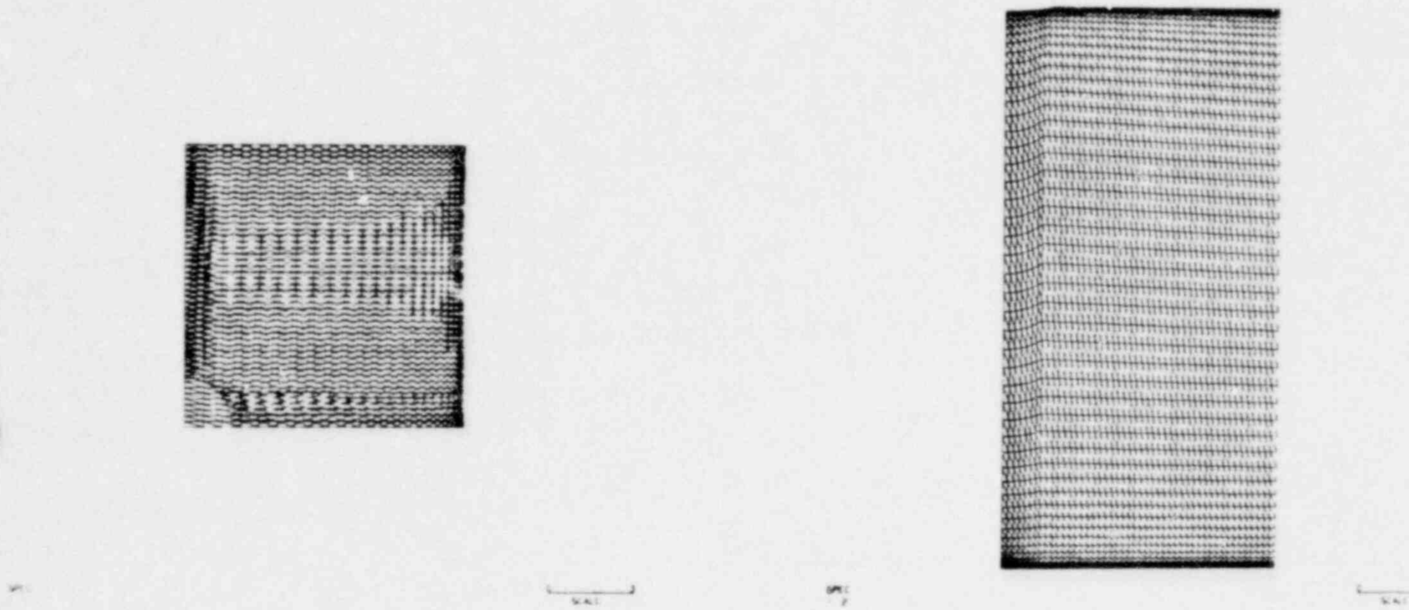
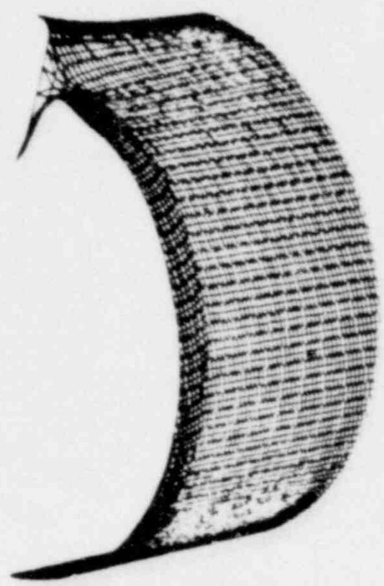
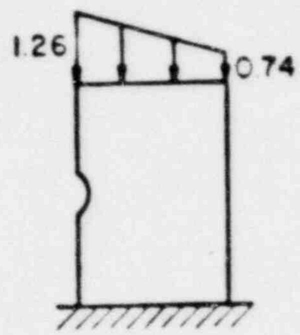


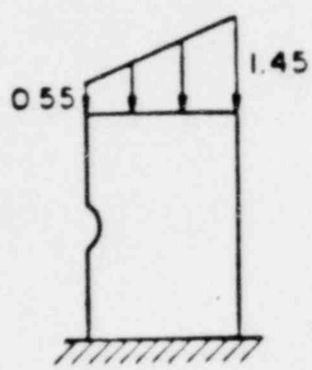
Fig. 12. Mesh used in SPAR analysis.



F'g. 13. Buckling mode predicted by SPAR.



a) TYPE I - HOLE OVERLOADED



b) TYPE II - HOLE UNDERLOADED

Fig. 15. Two types of nonuniform loading.

SUMMARY OF ANALYTICAL STUDIES

CASE	BUCKLING LOAD	P/P_0
PERFECT CYLINDER – UNIFORM LOAD	50,020	0.97
CYLINDER W/HOLE RBAR=3.5 (NO IMPERFECTION)	7,634	0.15
CYLINDER W/HOLE TYPE I IMPER.	7,804	0.16
CYLINDER W/HOLE TYPE II IMPER.	8,086	0.16
CYLINDER W/HOLE LOAD TYPE I	6,107	0.12
CYLINDER W/HOLE LOAD TYPE II	13,854	0.28
CYLINDER W/HOLE (NO IMPER.) (100 % REINFORCEMENT)	36,974	0.74

CONCLUSION

1. IF THE BUCKLING STRENGTH OF A CYLINDRICAL SHELL IS LOWERED BY A PENETRATION, THEN FOLLOWING THE ASME ARM RULE WILL INCREASE THE BUCKLING STRENGTH OF THE SHELL, BUT WILL NOT BRING IT BACK UP TO THE UNPENETRATED VALUE.

CONCLUSION

2. IF THE BUCKLING STRENGTH OF A CYLINDRICAL SHELL IS SO LOW THAT A PENETRATION DOES NOT LOWER IT FUTHER, THEN REINFORCING THE PENETRATION WILL HAVE LITTLE OR NO EFFECT ON THE BUCKLING STRENGTH OF THE SHELL.

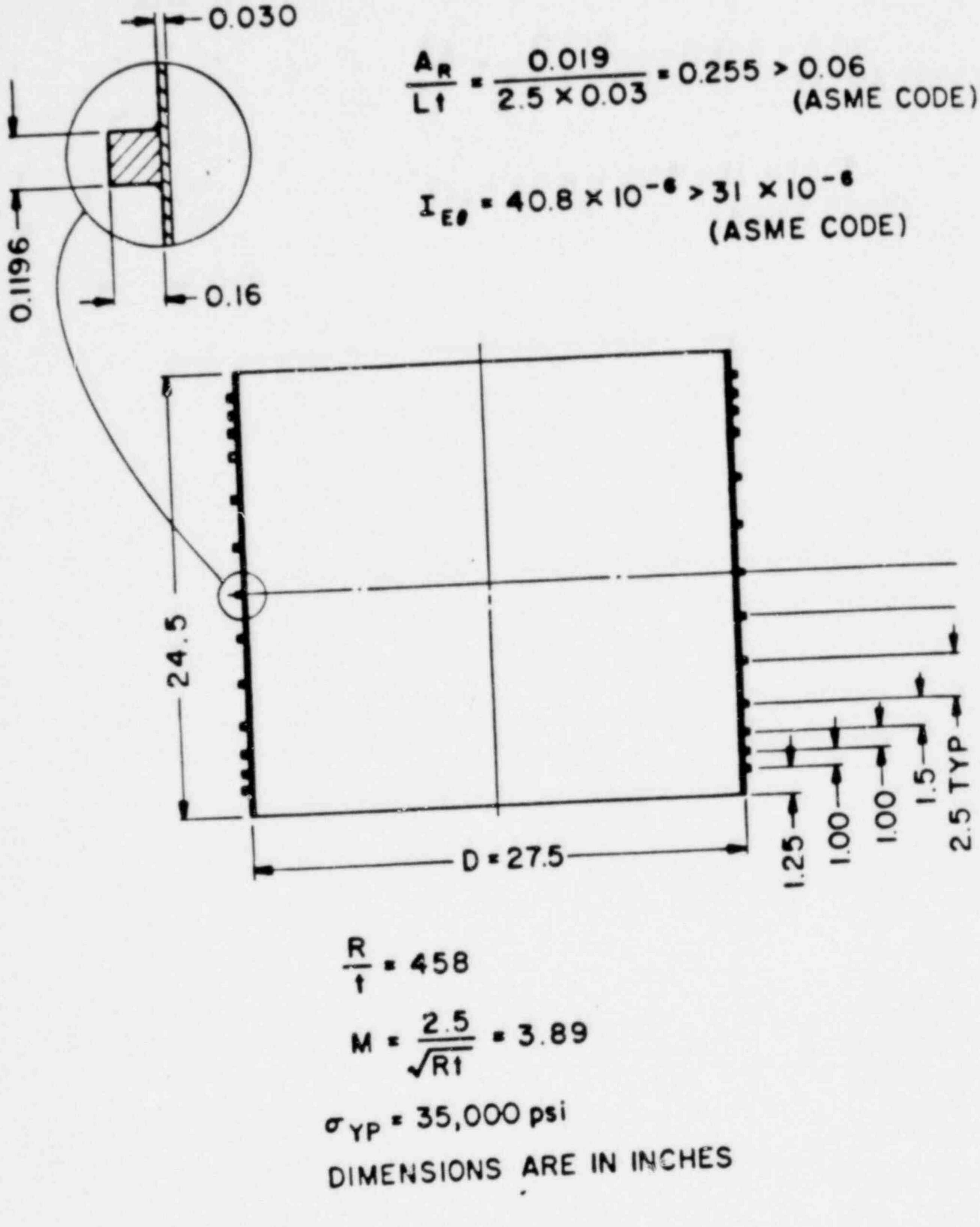
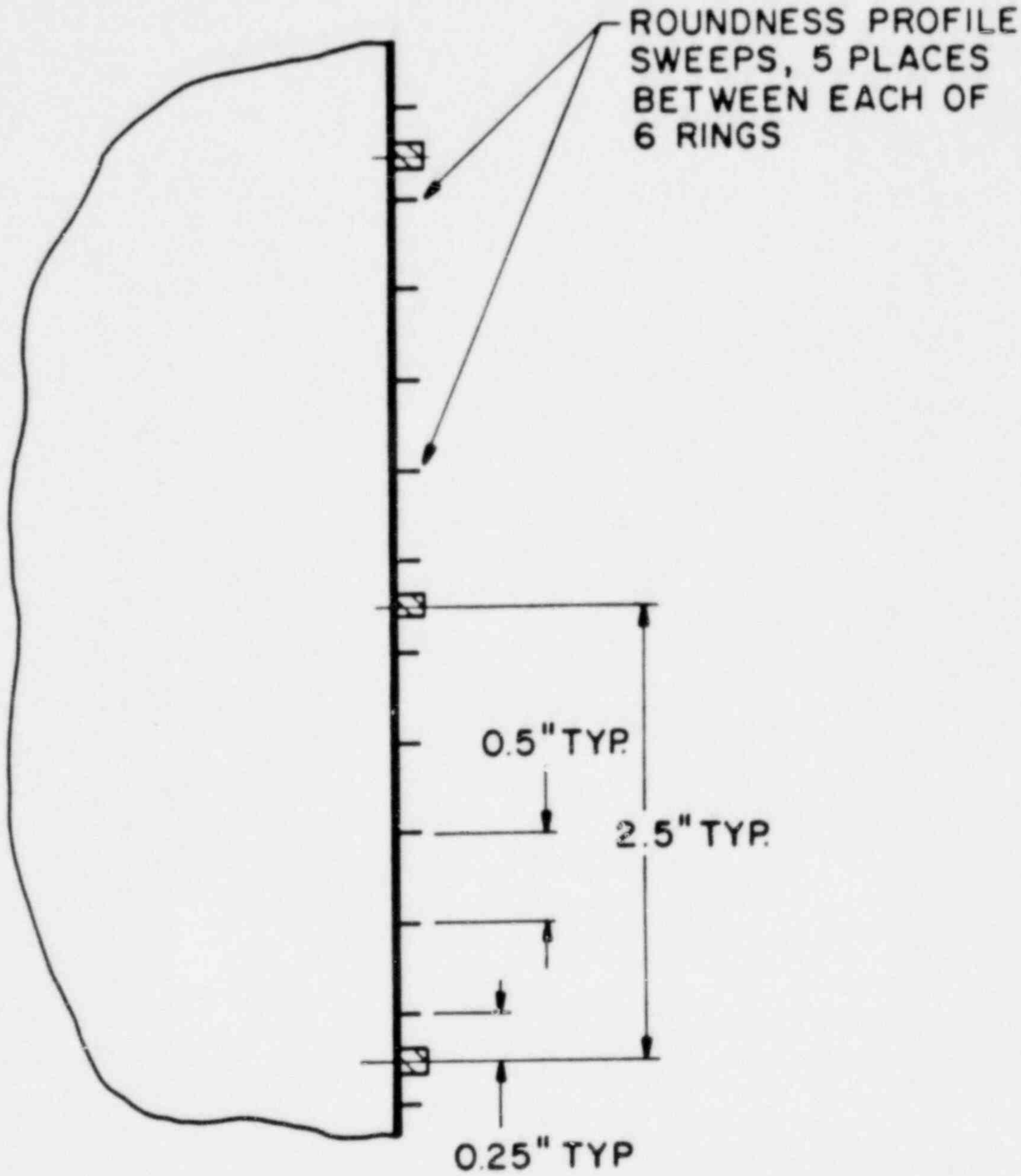


Fig. 1. Baseline Benchmark Experimental Model.


ROUNDNESS PROFILE SWEEPS, 5 PLACES BETWEEN EACH OF 6 RINGS
0.5" TYP.
2.5" TYP.
0.25" TYP

F 02

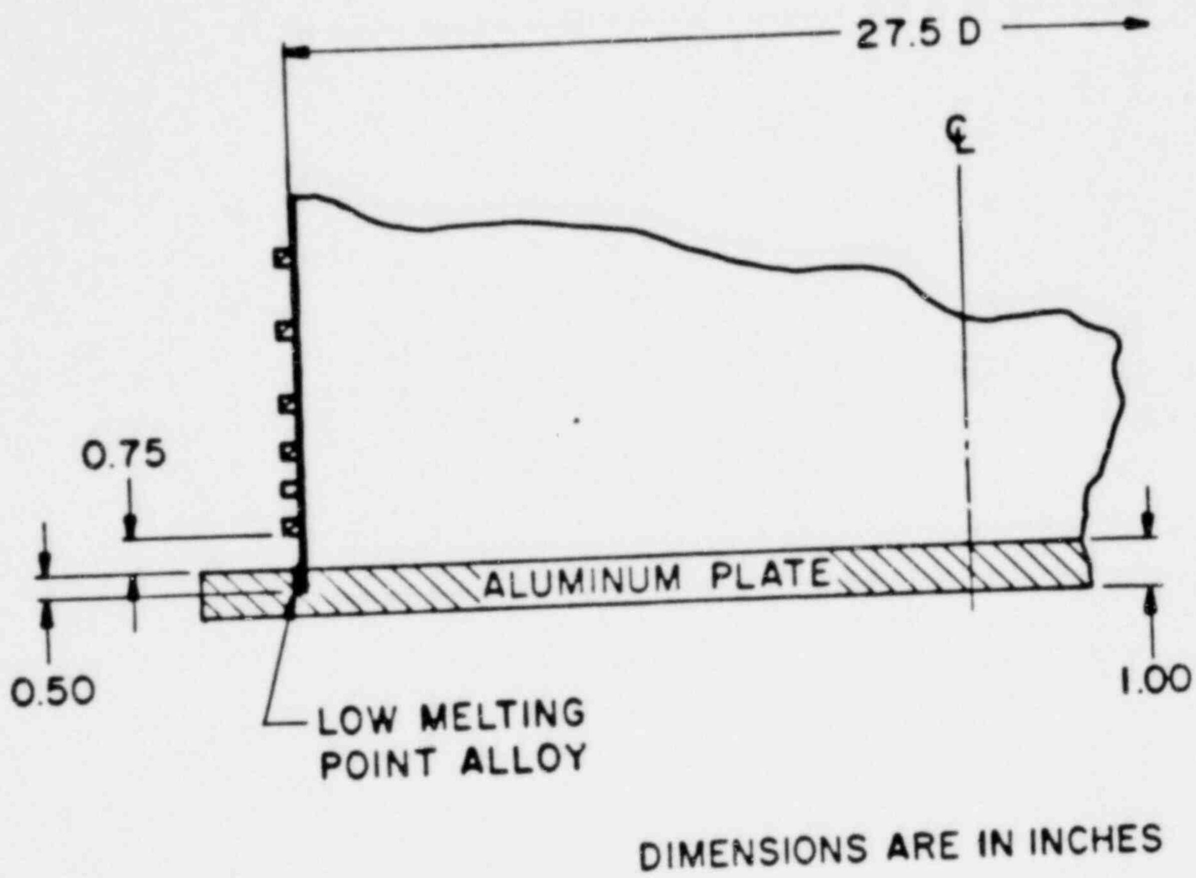


Fig. 3. Detail of end plate mounting.

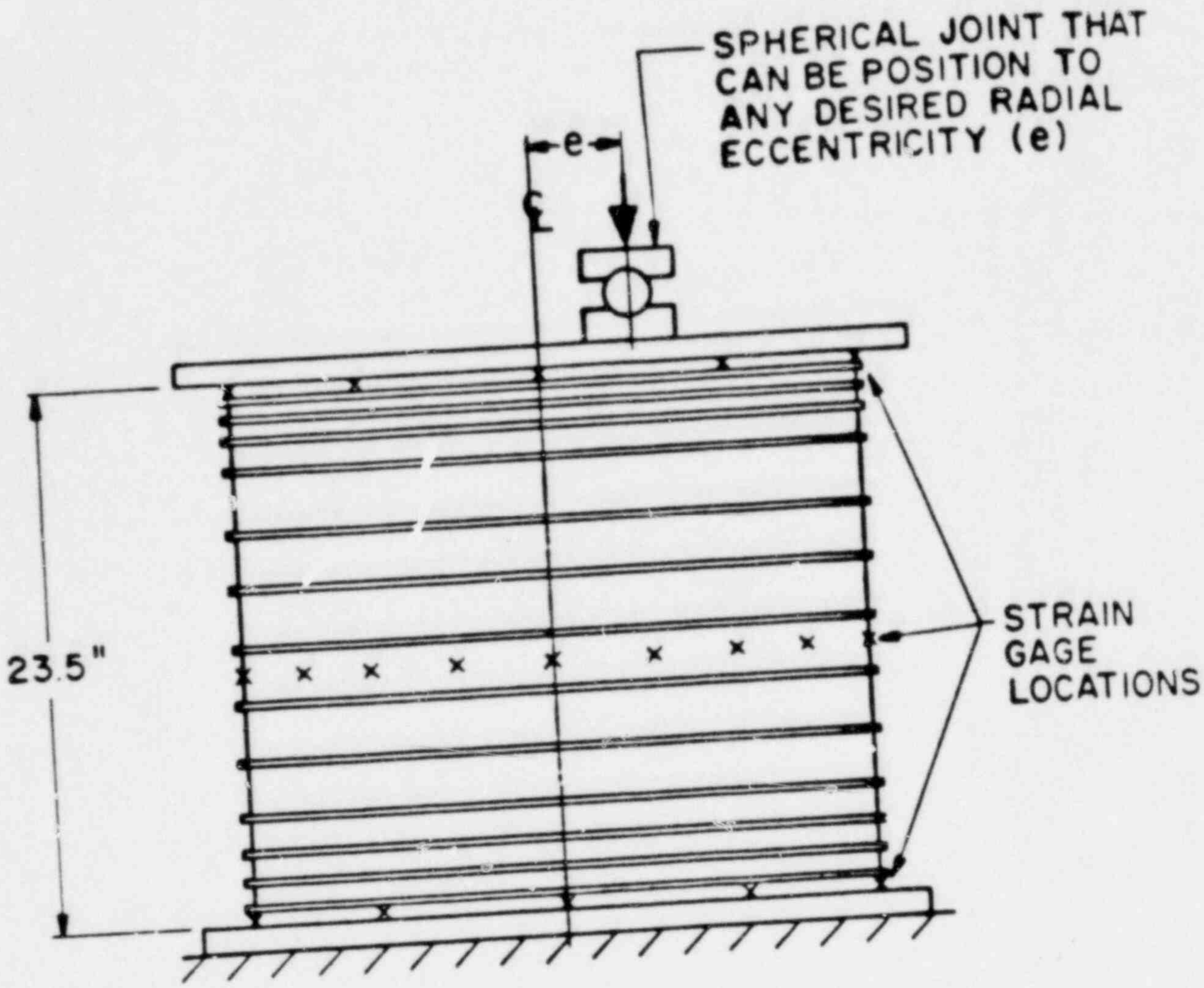
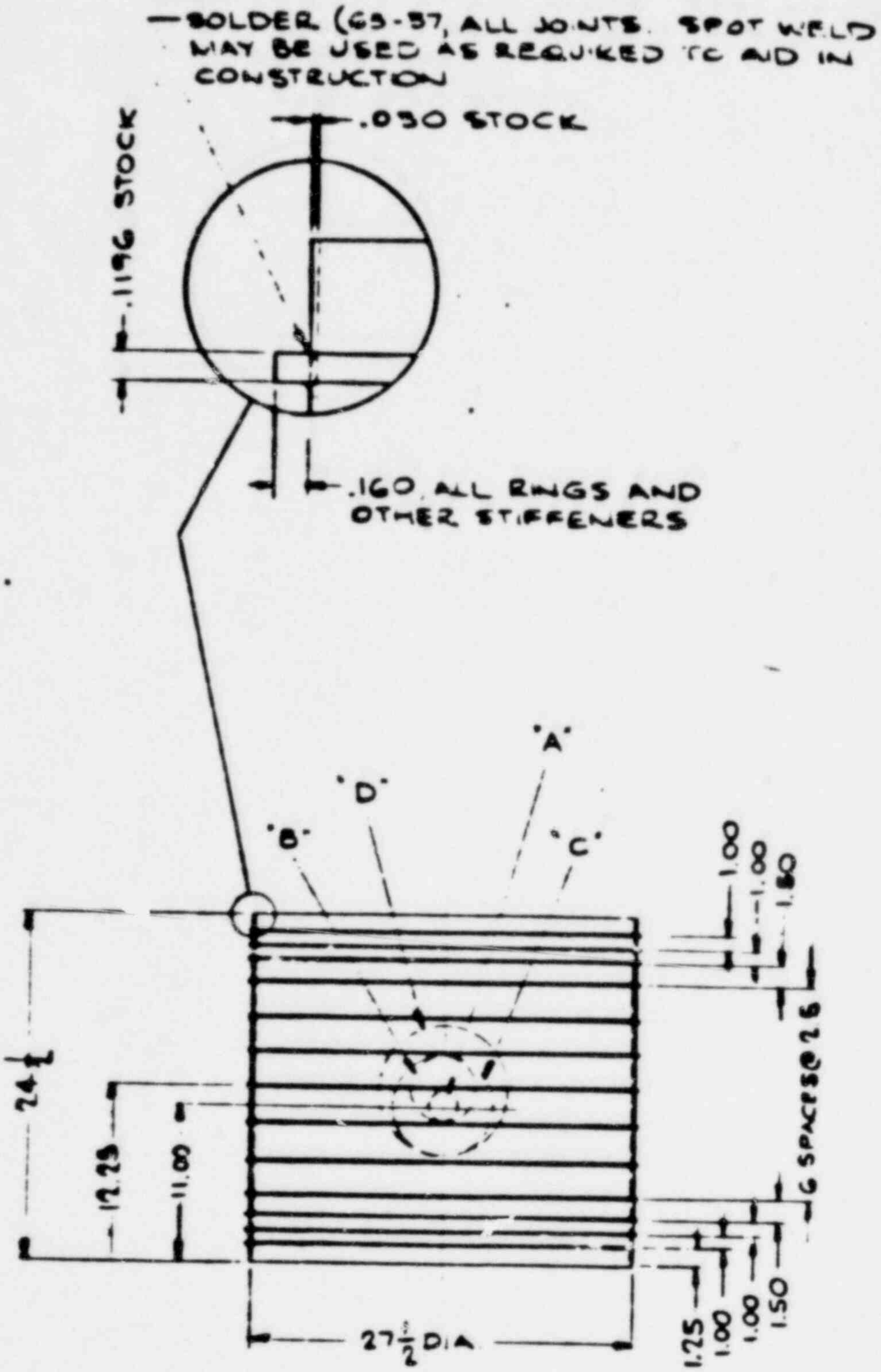


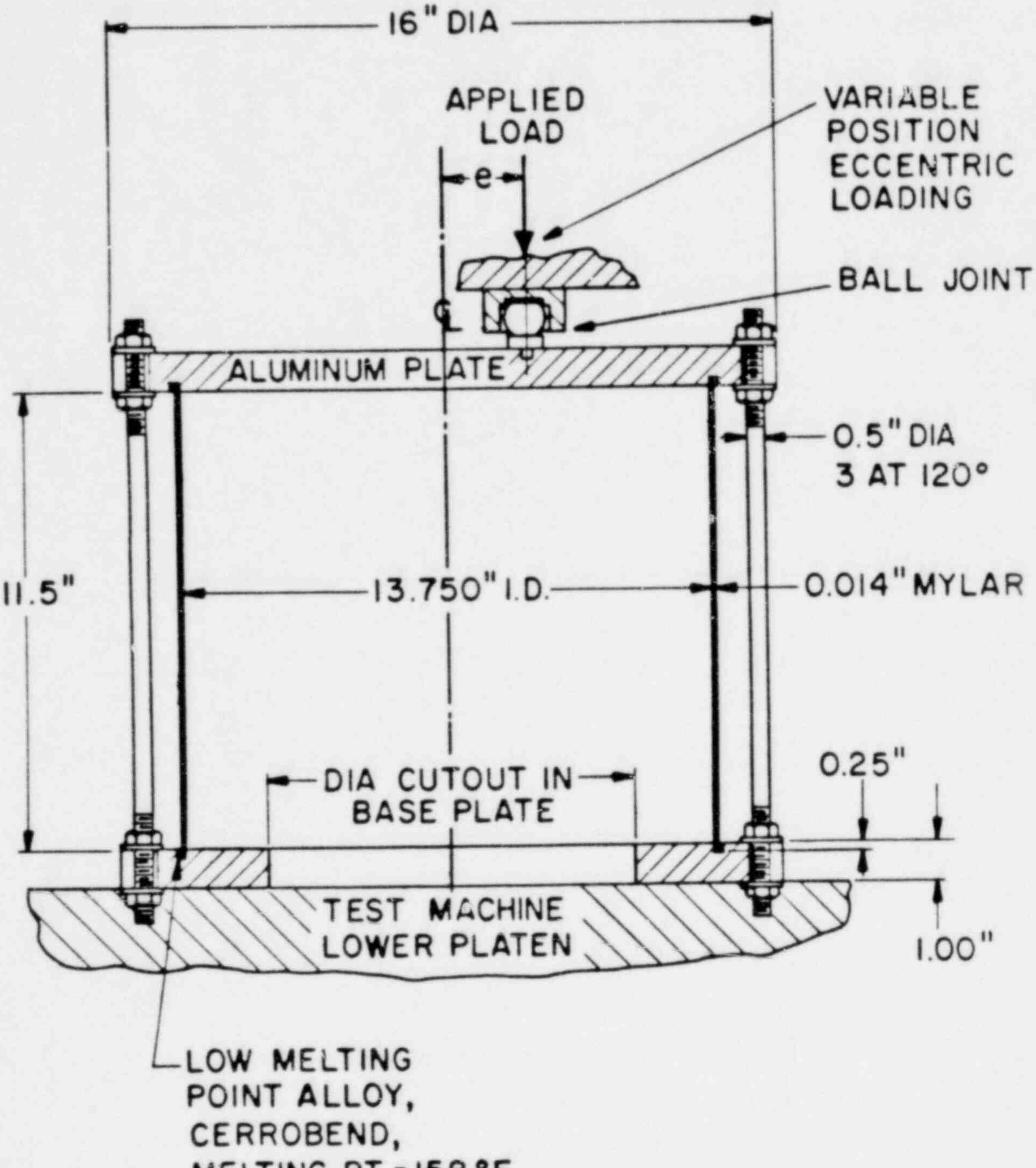
Fig. 4. Loading scheme and strain gage locations.



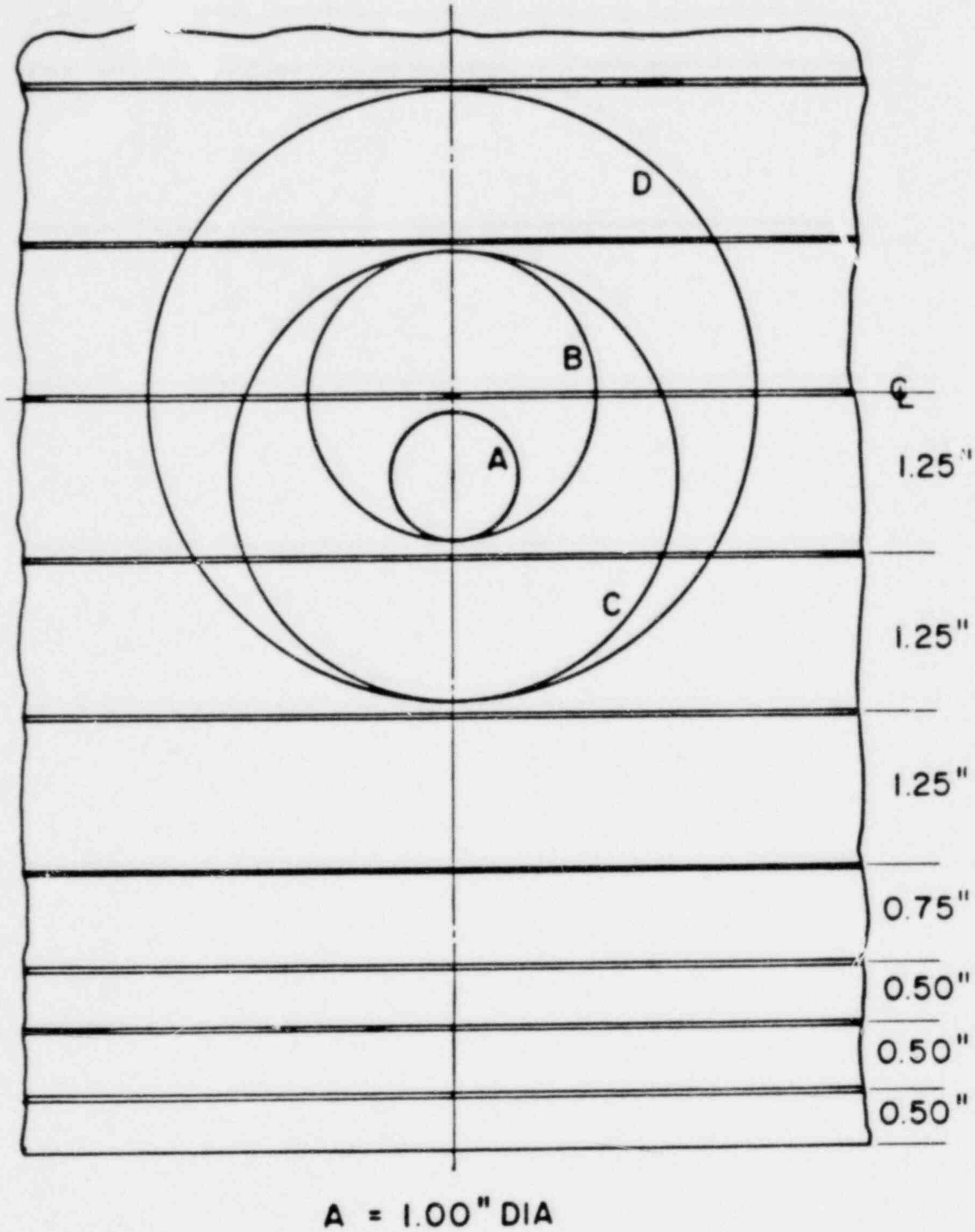
(200) - TEST CYLINDER

MATL: STEEL, A-366

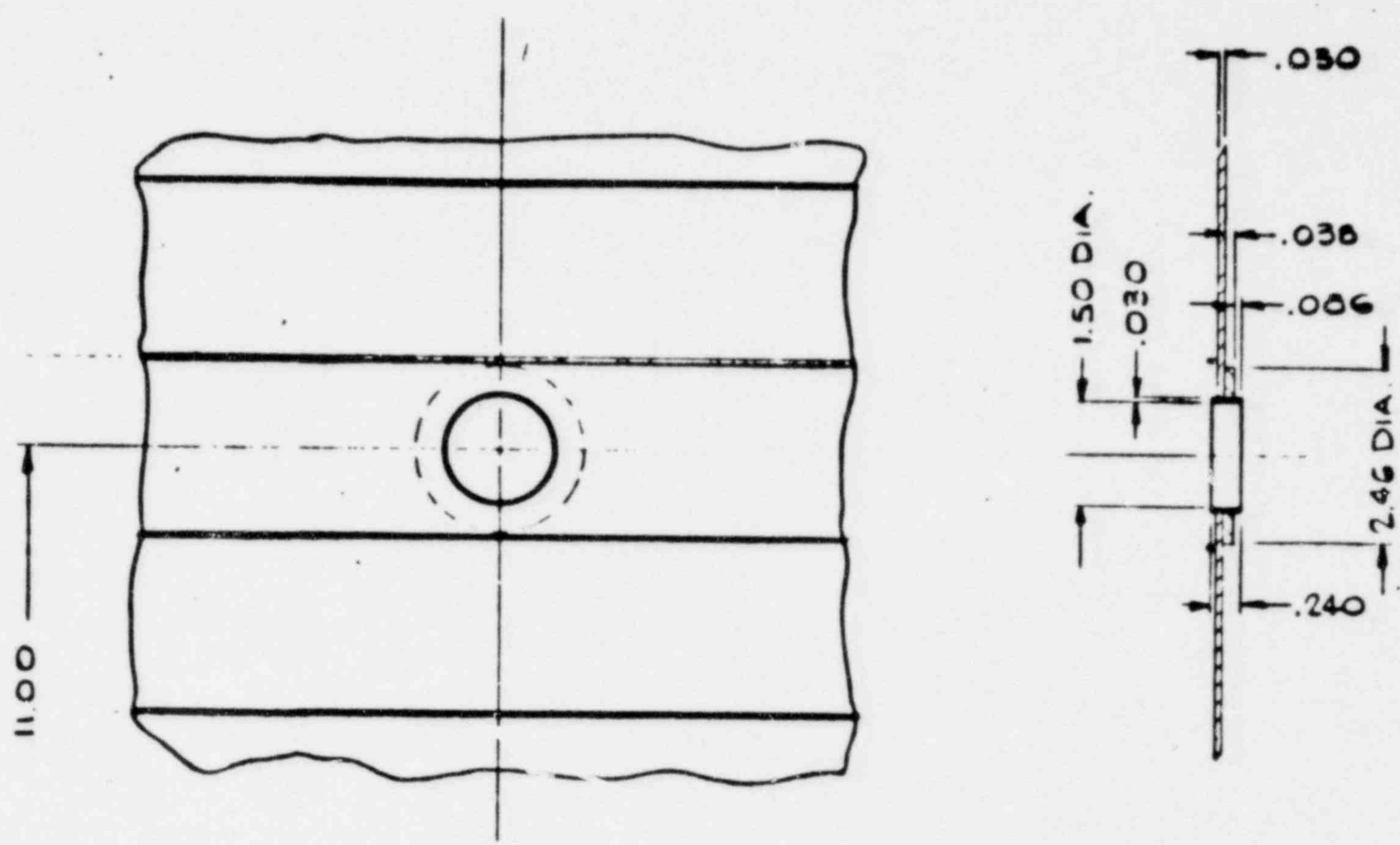
NOTE: ✱ DENOTES ARC LENGTH


16" DIA
APPLIED LOAD
e
VARIABLE POSITION ECCENTRIC LOADING
BALL JOINT
ALUMINUM PLATE
0.5" DIA
3 AT 120°
11.5"
13.750" I.D.
0.014" MYLAR
0.25"
DIA CUTOUT IN BASE PLATE
TEST MACHINE LOWER PLATEN
1.00"
LOW MELTING POINT ALLOY, CERROBEND, MELTING PT = 158 °F

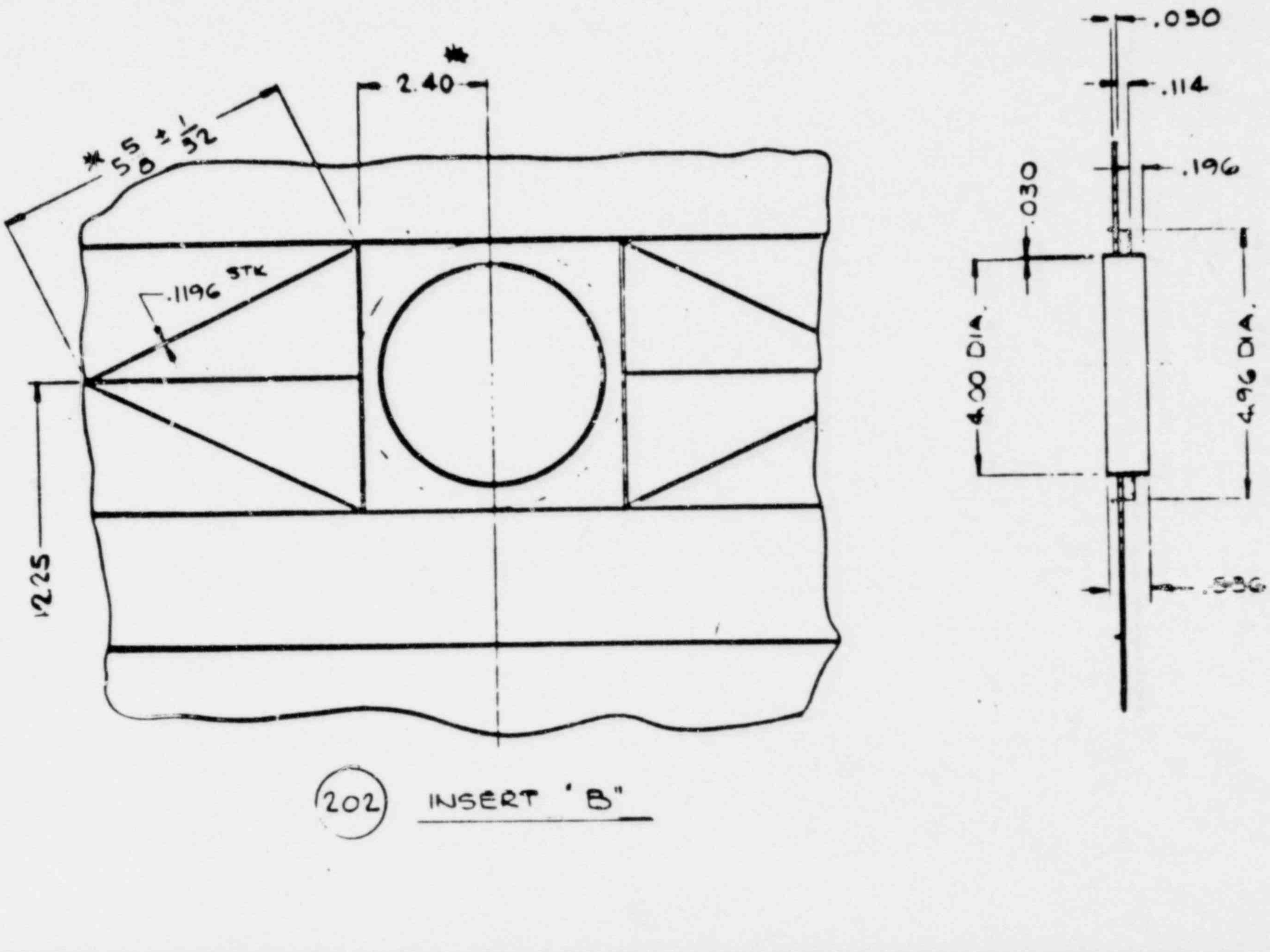
RINGS ARE SYMMETRICAL ABOUT HORIZONTAL ℄



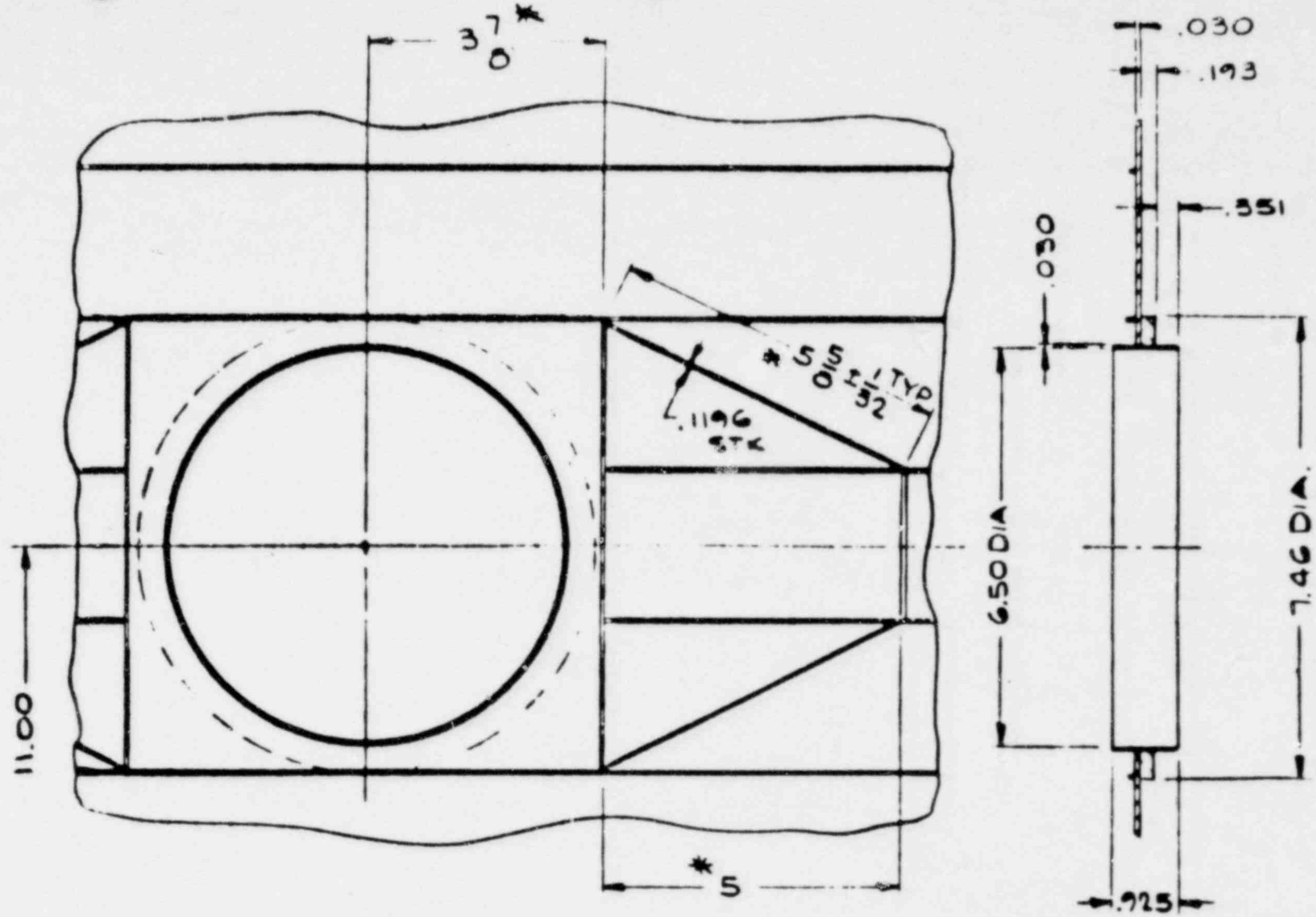
A = 1.00" DIA
B = 2.25" DIA
C = 3.50" DIA
D = 4.75" DIA



(201) INSERT 'A'

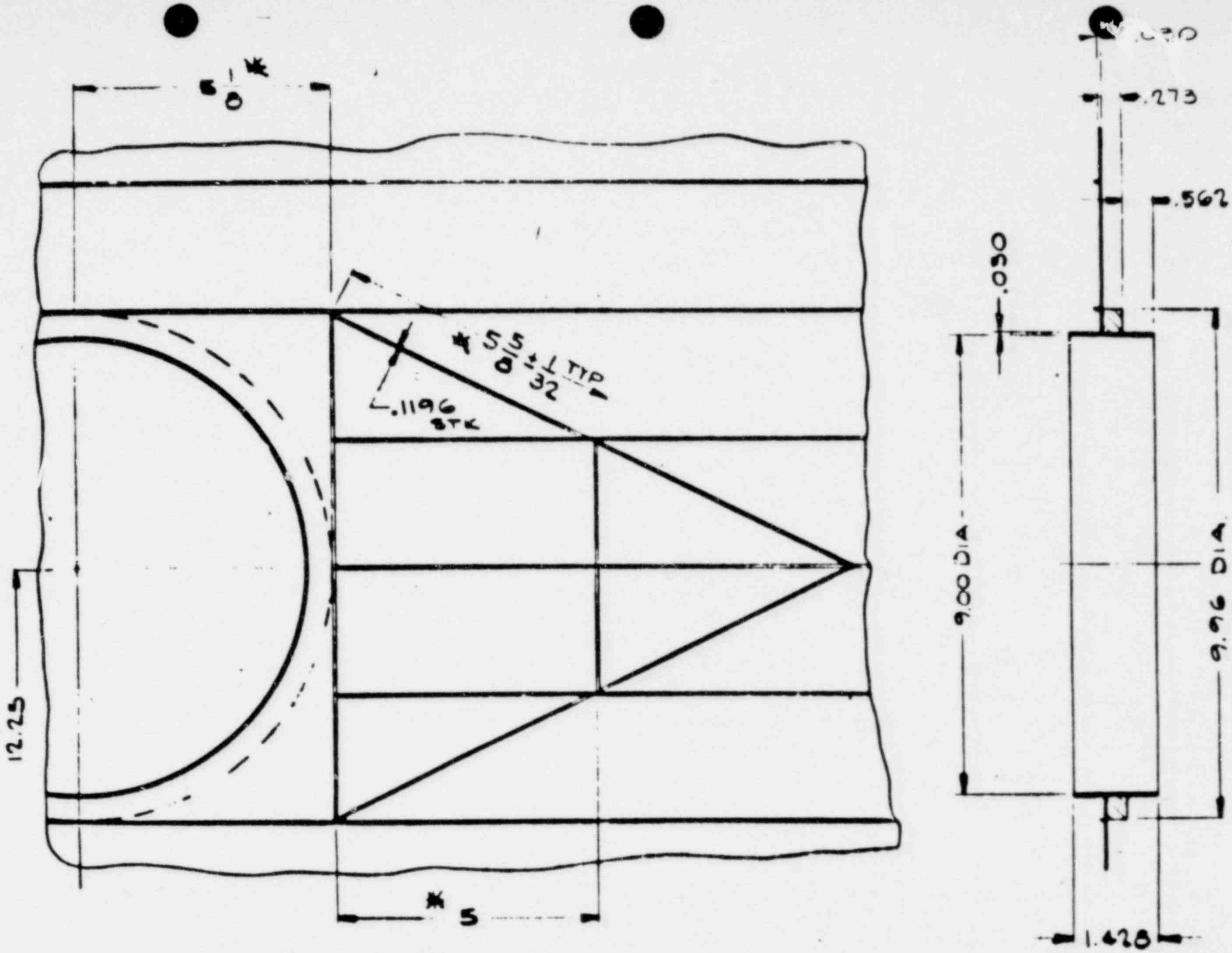


(202) INSERT 'B"



3 7/8 *
.030
.193
.351
.030
* 5 5/8 ± 1/32 TYP
.1196 STK
6.50 DIA
7.46 DIA.
11.00
* 5
.925

(203) INSERT "C"


5 1/8 *
* 5 5/8 ± 1/32 TYP
.1196 STK
12.25
* 5
.030
.273
.562
.030
9.00 DIA
9.96 DIA
1.428

(204) INSERT "D"

SMtoF-CB - current status cont.

STEEL CONTAINMENT DESIGN STUDIES

- – ASME NE-3000 as applied to containment designs
- – ASME code case N-284 – 'Metal Containment Shell Buckling' under study
- – Special criteria for floating island plants under study
- – Other related literature being reviewed

PEER REVIEW PANEL

Guidance on:

- – Overall program direction
  - – task priorities
- – Computer code benchmark experiments
  - – experiment selection and loadings
- – dynamic experiments on containment models
  - – scale
  - – loading definitions and their combinations

6/25/81

TENTATIVE
PRESENTATION SCHEDULE

Class 9 Accidents Subcommittee Meeting
Albuquerque, NM
June 30, 1981

June 30, 1981 Meeting Schedule

	Organization Speaker	Presentation Time	Approx. Time
Meeting with the NRC Staff and Contractors (Open Session)			
1.0 Subcommittee Chairman's Opening Remarks			8:30 am
2.0 How FVCS fits into the total NRR strategy for addressing core melt accidents	NRC/NRR J. Meyer	20 min	8:35 am
3.0 Overview - Program Plan on DCC Rulemaking (plan for determination of FVCS requirements)	NRC/RES M. Cunningham Sandia A. Benjamin/ B. Venado	20 min	9:10 am
4.0 Risk reduction potential of FVCS and how it is measured (case studies if available)	NRC/RES M. Cunningham Sandia A. Benjamim	30 min	9:40 am
COFFEE BREAK		10:10 -	10:20 am
	NRC/NRR J. Meyer UCLA W. Kastenberg	20 min	10:20 am
5.0 FVCS conceptual designs and cost estimates	NRC/RES M. Cunningham Sandia A. Benjamin	60 min	10:50 am
BREAK FOR LUNCH		12:20 -	1:20 pm
	TVA D. Renfro	30 min	1:20 pm

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TENTATIVE
PRESENTATION SCHEDULE

Class 9 Accidents Subcommittee Meeting
Albuquerque, NM
June 30, 1981

June 30, 1981 Meeting Schedule

		Organization Speaker	Presentation Time	Approx. Time
6.0	FVCS as alernative to underground siting - cost effectiveness, risk reduction potential, conceptual designs	Aerospace Corp. F. Finlayson	25 min	2:00 pm
7.0	Planned research on FVCS	NRC/RES R. Curtis	20 min	2:35 pm
	COFFEE BREAK			3:15 - 3:30 pm
8.0	General Electric Presentations - Feasibility of Unfiltered Venting of BWR Pressure Suppression Containments	GE	25 min	3:30 pm
9.0	EPRI Presentations - Review of Proposed Improvements, including Filtered/Venting of BWR Pressure Suppression and Ice Condenser Containments	EPRI	30 min	4:15 pm

Meeting with NRC Staff (Closed Session)

10.0	Foreign research being performed on FVCS and Class 9 Accidents	NRC/NRR J. Meyer	30 min	5:15 pm
11.0	Adjourn			6.00 pm