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MEETING

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UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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TRANSCRIPT OF PROCEEDINGS

BEFORE THE

NUCLEAR REGULATORY COMMISSION

ALBUQUERQUE, NEW MEXICO

IN RE: THE ADVISORY COMMITTEE)
ON REACTOR SAFEGUARDS)
STRUCTURAL ENGINEERING)

SUBCOMMITTEE MEETING

BE IT REMEMBERED THAT at 10:30 a.m., on Friday, the 22nd day of January, 1988, the above-entitled matter came on for hearing at the AMFAC Hotel, Valle Grande Room, 2910 Yale Boulevard, S.E., Albuquerque, New Mexico 87106, before CHET SIESS, Chairman; and the following proceedings were reported by William C. Beardmore, A Certified Shorthand Reporter of:

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APPEARANCES

SUBCOMMITTEE MEMBERS

Chet P. Siess
Jesse C. Ebersole
Paul G. Shewmon
David Ward
J. Carson Mark
Mike Bender
Elpidio G. Igne

TABLE OF CONTENTS

P R O C E E D I N G S

FRIDAY, JANUARY 22, 1987

MR. SIESS: This meeting will come to order. This a meeting of the ACRS Subcommittee on structural engineering. It says here I'm Dave Ward, Chairman for the subcommittee. That's wrong. I'm Chet Siess, Chairman of the Subcommittee. And the other ACRS members in attendance, starting on my right, Carson Mark, Dave Ward -- does the microphone work? -- Paul Shewmon -- can you hear me? -- Paul Shewmon, Jesse Ebersole, and Mike Bender, a consultant for us.

The purpose of the meeting is to review the concrete model test, future work on reinforced and prestressed containments, and future efforts on containment penetrations and seismic issues. Recognizant staff member of the meeting is Elpidio Igne, sitting on my left when he sits down.

The rules of participation in the meeting have been announced prior to notice and published in the Federal Register on January 14th. The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee, Acting Government, Sunshine Act.

We've received no written statements or requests to make oral statements from members of the public regarding

today's meeting. And, as usual, we're keeping a transcript, and I'll ask each speaker to first identify himself or herself, and then, I think, try to use the microphone if you have one. If not, just speak loudly enough so that the Reporter can hear you.

We're starting a little late. I don't who decided we could go out there and back in an hour and a half. It was a nice try. I think we will meet the luncheon time on the agenda sometime around 1 o'clock, maybe a little bit before, depending on where we are in the program and then just break at that point and come back.

I think everybody has a copy of the agenda. Al just passed out one which is Revision 2, which I haven't seen.

Does anybody have any questions about the agenda or do any of the subcommittee members have any other comments they would like to make at this time?

Looking at the agenda, Walt von Riesmemann is not able to be with us. He has an illness in the family and he had to leave town. But who --

MR. COSTELLO: I guess I could start.

MR. SIESS: Yeah, but who's going to take Walt's place?

MR. COSTELLO: Yes.

MR. SIESS: Who?

MR. COSTELLO: I'll do a lot of the introductions and things like that.

MR. SIESS: All right.

If there are no comments, let's start in then with Jim Costello from the NRC Research Staff.

OPENING COMMENTS

MR. COSTELLO: Good morning. Thank you, Mr. Chairman. I have passed out one package of handouts, and I would like to just emphasize a few things in them.

The discussions today center around our NRC-sponsored program on containment integrity. The overall objective is to provide a reliable method by which you can make estimates and maintain performance with emphasis on qualifying the method by comparison against experimental data.

The members of the Sandia team who will be making presentations today are outlined on this viewgraph except for Dr. von Riesemann, who was unable to be here.

The next two slides in your package are the way the Sandia presentations have been structured to meet what we believe is the intent of the agenda and also to utilize the expertise in certain areas of members of the Sandia Staff.

If this is agreeable with you, we are prepared -- everyone is available all day, so we are prepared to march through in this fashion or circle back at any time if you would like to.

Right now, we will be starting with the discussion of the concrete model tests.

MR. SIESS: Excuse me a minute, Jim.

MR. COSTELLO: Yes, sir.

MR. SIESS: Let me check with the Subcommittee and find out if anybody has to leave early. And by early, I would say before 4:30.

Okay.

MR. COSTELLO: And if it's agreeable to the Subcommittee, we will start off by the sequence of presentations on the concrete model tests. We'll then follow with future work on concrete containments. And then we'll come back to the completion of work on steel containments; that is, the application of results of the scale modeling tests to a Sequoyah containment. And then we will get separate topics beyond containment penetration, first try to focus on -- is there a problem question -- is there a problem, shall we say, inside the capacity of some containments.

MR. SIESS: Jim?

MR. COSTELLO: Yes, sir.

MR. SIESS: I like that organization, but let me suggest a way of approaching it here. I think what I would like to hear -- and I think it will come out the way you've got it -- is, what were the questions you were trying to answer about containments. At this stage of the game, what have we learned and then what are the remaining questions and what do you propose to do about them.

MR. COSTELLO: Well, I believe we have that --

MR. SIESS: I believe that's about the way you've got it covered. I would just like to get some emphasis on questions and answers or, rather, simply results.

MR. COSTELLO: I guess Item 3 -- the basis of Item 3 speak to what we think we've learned so far on concrete containers. Item 4 speaks to what we think we have to do to pose the question. Item 5 really speaks to coming to a close on containment closure on steel containment questions. And then 6 and 7 are mixes of -- over somewhere in the (inaudible) stage.

I guess if we run out of time, of course, it would be possible to defer Topic 7 until a future meeting, or you can have (inaudible), whichever the Chairman would prefer.

MR. SIESS: That sounds good.

MR. COSTELLO: With the Subcommittee's

agreement, I would like to ask Mr. Daniel Horschel, who conducted the tour out at the model this morning and who was the project engineer in charge of the model construction and test to give his presentation.

DISCUSSION OF THE CONCRETE MODEL TEST

Overview of the Test and Results of the Test and Posttest Inspections

MR. HORSCHEL: Thank you. My name is Daniel Horschel.

MR. SIESS: Excuse me, Dan. What Al is passing out is a quick look on the low pressure.

MR. HORSCHEL: Well, there are three things that he is passing out. One is a copy of the viewgraphs, one is a quick look at the low pressure testing, and the last is a quick look at the high pressure testing.

MR. SIESS: And Igne distributed the quick look on the high pressure to the members of the Subcommittee; so you might already have that.

MR. HORSCHEL: I would like to first begin with the objective of our containment testing. Again, this is to generate data by testing containment models. The data can then he used to qualify methods for reliably predicting the response of LOCA low water reactor containment buildings for severe accidents.

I would like to review some of the instrumentation that we have on the containment model. As I mentioned during the tour, we have about 300 channels bedded in the conrete wall. Most of those are strain gages. We also had some bondable gages on the rebar. We had 161 rosettes on the inside of the liner, for a total of 483 gauges. We had strip gages, which was a total of 101 single gages on the strip gages, 59 single gages, 137 displacement transducers, thermocouples. We had embedment gages in the concrete. Of course, we had pressure transducers, some resistence temperature detectors, inclinometers, weather. We had one bolt link and two flow meters, for a total of about 1,200 transducers that were contained in the model.

In addition to this, we also had about 12 video cameras and recorders that monitored the different places throughout the containment. One of those was actually inside the containment and (cough) control capabilities.

We also had several still cameras located at different stations about the model and photographed the containment during both the low pressure tests and the high pressure tests.

MR. SIESS: I assume that we wouldn't find much of interest at looking at the video.

MR. HORSCHEL: Since the containment is still there, that is basically true. About the only thing that we

would find more interesting -- we really don't have a copy of it in my presentation or you really couldn't see up there were the uplift of the basemat. That was much more noticeable during the pressure testing and that's only captured on the video -- well on the video.

Just a brief review of our testing schedule. We began our structural integrity test, which is a standard test on any full-sized containment, on July 6th through 10th. That's where we pressurized the containment in steps to 1.15 times its design pressure. We did mass cracks at six selected locations throughout this test. And that report that you have in your hand that was handed out really goes over that full test and some of the results from the structural integrity test.

As far as I'm concerned, nothing really unusual was found during that test.

We then continued on the next week in July and did integrated leak rate testing on the containment model. First we did it with no orifices in the containment to see what its non-leakage rate was in the system. The result from that was about .15 percent mass per day time from the containment, which we believe most of that was coming from actually leaks in the valve guide rather than the containment itself. We considered that to be a very acceptable level.

MR. MARK: At what pressure level was this you referred to?

MR. HORSCHEL: That's at 46 design -- at 46 psig which is the design pressure of the containment. That's all --

MR. MARK: You've got a seventh of a percent per day.

MR. HORSCHEL: .15 percent per day at 46 psig.

MR. SIESS: .15.

MR. HORSCHEL: We also put orifices in there and tested some small orifices to see what type of leak rates we would get with those. And one orifice gave us about 11 percent and another orifice gave us about -- I believe it was 35 percent leakage per day. One larger one was about .137 inches in diameter, the smaller one was .070 inches in diameter.

MR. SIESS: .07 inches in diameter gave you several --

MR. HORSCHEL: I believe it was about 11 percent.

MR. SIESS: So did anybody calculate what size opening it would take to get .15 percent?

MR. HORSCHEL: We didn't go through that. I'm sorry.

We did some leakage testing on their equipment hatches. And finally we concluded during the end of the month on July 28th through 30th the high pressure test.

The rest of my presentation today will really concentrate on the high pressure test.

MR. SIESS: Now, the high pressure test, it's true you went up over 150 psi?

MR. HORSCHEL: Up through 145 psi gages, yes.

This outlines our loading schedules for the first day. As you can see, we already had structural data during the SIT, so we took fairly large steps for the first 50 psi of the loading schedule. After we exceeded that, we began to reduce our loading step to about 5 psi. As we noticed, not in your response, is an interesting thing happening about the containment, we reduced them further to 2 to 3 psi steps. On average, each step took about an hour with all our instrumentation scans scanning the containment with our video cameras, and just reviewing data before we went onto the next pressure step.

MR. SIESS: At what pressure was the line unpredicted to yield?

MR. HORSCHEL: Maybe I could defer that to Randy Weatherby.

Randy, do you have a response?

MR. WEATHERBY: 110 psi.

MR. SIESS: 110? Thank you.

MR. HORSCHEL: As you can also see from our loading schedule that the test was conducted on around-the-clock basis. We were testing 24 hours a day.

Here you can see, as we went beyond 100 psi, we reduced our steps to about 10 psi for the remainder of the test.

MR. SIESS: Now, by steps, a full set of instrumentation read after each of those steps?

MR. HORSCHEL: Hand-wired instrumentation -- well, let me back up.

The majority of the answer is yes. We had those TRAC systems on the outside that made the displacement which you saw this morning. Those we wouldn't scan every time. Just because with their resolution, it wasn't appropriate at the lower level to do that. As we exceeded and got the higher pressures, we thought that we could make something in those TRAC systems, then those would be scanned. But for the most part the answer is yes.

MR. SIESS: Fine.

MR. HORSCHEL: Some points of interest along the way that we saw during the high pressure test was No. 1, at 125, we sensed some leakage coming from the vicinity of Equipment Hatch B. Now, how did we sense this? This was also with an acoustic detection system which we had

operating real time during the containment tests.

We had about eight sensors located about the containment so we actually couldn't pinpoint where it was coming from, but we could actually get a general vicinity.

MR. SIESS: These were outside the containment?

MR. HORSCHEL: Both. We had a couple on the outside, up there on the sleeves, and some on the inside on the lineer. We really couldn't attach it to the concrete, so it was all attached to the steel.

MR. SIESS: So when it says that some leakage was thought, was the thought referred to the location or to the existence of the leakage?

MR. HORSCHEL: The area where it was coming from, the location. They feel fairly comfortable with their acoustic detection system sensing that this frequency range does dictate a leak.

MR. SIESS: It was a leak at 125, but you don't know how big it was and you think it was in the neighborhood of A.

MR. HORSCHEL: Of B.

MR. SIESS: I'm sorry, of B. I was looking for a letter and the A was up there.

MR. HORSCHEL: Should have put quotes around it.

Along those lines, did try to take a leakage note measurement at 125, but the leakage was so small it really didn't come up in our resolution. If it's a very small leak, it would take a very long time to find -- to get a magnitude of that leak. The larger the leak is the less time it takes.

So we continued on. We started noticing something was coming from Equipment Hatch A -- in the vicinity of A -- at a pressure of about 138. We also noticed towards the conclusion of the test that we had about a half of an inch ovalization of the sleeve of Equipment Hatch A. And as I mentioned before, we had an uplift of the basemat. That was about three-eighths of an inch.

MR. SIESS: And that half-inch was enough to expose one of the seals?

MR. HORSCHEL: Yes. The three-nine (inaudible).

Obviously, it was old so it wasn't all the way around, just a couple of locations. I have some pictures of that I'll show you later.

MR. EBERSOLE: May I ask you, was it a basic objective to ensure that the liner would fail at a pressure well below that of catastrophic failure of the reinforced concrete?

MR. HORSCHEL: We just used typical design

procedures, and we really scaled our model from typical containment models.

MR. SIESS: Well, let me -- Jesse, these things are designed for LOCA loads with certain allowable stresses and margins. And the criteria you indicated just doesn't pan up. They don't think about severe accident loads of how it's going to fail. The whole design is based on allowable stresses and margins. The design is based on not failing, it's not based on failing. So they never get around to thinking about how --

MR. EBERSOLE: Well, the end result of it is that -- however, is, in fact, you do fail at a pressure of lower than this, it would cause catastrophe failure of the structured concrete.

MR. HORSCHEL: Certainly in this case. How it applies under the containments, I really don't want to speculate at this point.

MR. EBERSOLE: Okay.

MR. HORSCHEL: And finally, the cracks in concrete surface became much wider. And that is in lieu of forming new cracks. So I think that's an important thing that we learned from this test. Generally the cracks that you have in your SIT are the cracks that you'll see at the conclusion of a high-pressured event.

MR. SIESS: What kind of crack widths did you

have at the SIT, millimeter?

MR. HORSCHEL: They're measured, and again, they're in that report. We started turning them down if they were greater than 10 mils.

MR. SIESS: Okay.

MR. HORSCHEL: Some of them did exceed 10 mils, but there were very few in the SIT test.

MR. SIESS: And at the end of the test, how big were your cracks?

MR. HORSCHEL: It was very much as they did today. There were some cracks -- many cracks that were probably as wide as an eighth of an inch and a few 3/16 of an inch and possibly a little wider than that.

Let me mention some of the leakage tests that we conducted during the course of the high pressure tests.

The first one that we feel comfortable in reporting was done at 135, and we had about 11 percent mass per day. That converts to about eight standard cubic feet per minute.

MR. SIESS: Which hatch is which? Which had the double?

MR. HORSCHEL: B was the one that was doubled.

MR. SIESS: Okay.

MR. HORSCHEL: At higher pressures --

MR. SHEWMON: Is that the one we came out --

MR. HORSCHEL: You went in A. A came into play at about 138 psi. We did our first leak rate test when we sensed leakage coming from both areas at 140. The leakage then was recorded as being 13 percent mass per day, about 10 standard cubic feet per minute.

Our next leakage test was done at 143. The leakage then was recorded as being 62 percent mass per day, about 50 standard cubic feet per minute.

Finally, at our maximum pressure that we reached this at, we did three tests at 145. The first, we measured the leakage as being 231 percent mass per day, about 185 standard cubic feet per minute.

Now, that test was actually done from the whole containment model as we felt things were happening there.

With the two hatches on B, we decided to close the valve on the inner door and we would eliminate any leakage coming through the outer door at B. So we actually expected and we assumed really that leak was coming through B. We expected our leakage to be smaller. As you can see, it wasn't. We actually measured 352 percent mass per day or 275 standard cubic feet per minute.

MR. SIESS: How much time elapsed between those two readings?

MR. HORSCHEL: Between the conclusion of and

the beginning of the other, probably about five minutes.

MR. SIESS: You mean to close that change from 185 to 275 occurred in five minutes?

MR. HORSCHEL: Okay. We conducted the first test at 145 and got 234 percent per day. That test takes about 15 minutes long to conduct.

MR. SIESS: Well, we're talking about minutes?

MR. HORSCHEL: 15 minutes.

MR. SIESS: Okay.

MR. HORSCHEL: We stopped that test, changed the valve over, looked at the data, started our computer program to initiate the second test, which took probably about five minutes. And in eight minutes, we realized that we had about 352 percent mass per day.

MR. WARD: But the pressure in the building just stays constant during that test?

MR. HORSCHEL: You do pull it off of your valve off system and, indeed, we do lose with some pressure. We probably lost about two or three psi's through each one of these tests and then we filled it back up.

At the conclusion of the second test, the one where we measured 352 percent per day, we opened up the valve again and tried to bring it back up to 145, but we had trouble doing that. There finally was a conclusion of the

test. We were putting in about 4,000 standard cubic feet per minute and just couldn't bring the pressure back up.

MR. SIESS: So at 145, you essentially had somewhere a leak that was continuing to increase?

MR. HORSCHEL: Yes. I'll get into that in some more detail here.

MR. SIESS: You're not going to deal with the hatch problem.

MR. HORSCHEL: Now, ignoring that last measurement, 4,000 standard cubic feet per minute, you can see the dramatic increase in the leakage rate where it comes from the containment model, especially between 140, 143, and finally at 145. It had happened very suddenly.

MR. SIESS: How long did it take you to go from 140 to 143 to 1:45?

What I'm getting at is, suppose you had just sat there at 140. Would it possibly have continued to yield that leak.

MR. HORSCHEL: The material properties of the steel do show some creepage. I think if we did stay there for a period of time, especially at these upper pressure levels, it's possible you could have developed a large leak at a lower pressure level.

Conversely, I also feel if we loaded a little more rapidly, we might have been able to get a little higher

than 145.

For example, at 145, we were there for more than one hour while we were doing all these tests. We did standard instrumentation scans, we did the two-leak rate tests, and after we were putting this 4,000 standard cubic feet per minute into the model, we actually did another scan of the instrumentation.

MR. SIESS: Well, I look at that curve, and I would say, well, at the first little slope, the 140 up, that slope, I would like to extrapolate one line out and the other one up. Do you know what I mean?

Yeah, instead of going that way, I would go over from 140 and then up.

If you left it at 140, let's say it would have been reasonably stable. Now, you move it over to 140.

MR. HORSCHEL: This is 3.

MR. SIESS: Yeah. But you don't have any points between those two. What do you think the shape of the curve was between those two?

MR. HORSCHEL: I guess I would be --

MR. SIESS: See, what I'm postulating is, at about 142, if you would have just held it there, it would have gone up.

MR. HORSCHEL: I think that is very possible.

MR. SIESS: But not at 140, you don't think?

MR. EBERSOLE: Let me ask you this: The real rise in pressure would come either from temperature, which will be slow and you will be caught inside as well because of failure reject heat or it will come for something like a hydrogen explosion.

On the other side of the coin, what if you applied part of that pressure suddenly? Would you expect it to fail earlier or later or higher or lower pressure from the hydrogen explosion in which the pressure loading is applied in a very short time?

MR. SIESS: That's a future issue.

MR. HORSCHEL: It's a very different situation. It really would depend on the loading and what you're looking at. In a dynamic situation, actually those leaks don't mean anything. It's a very dynamic situation with a very big explosion. There's a very small spike inside, it probably won't have much effect on the equipment. It really depends on what range you're talking about. There's a lot of parameters that could go in there really answer your question.

MR. EBERSOLE: You would think, would you not, it would take a higher loading, though, witha fast application of pressure?

MR. SIESS: Sure.

MR. HORSCHEL: Sure. That's generally true,

yes.

MR. SIESS: And if it didn't rupture, it might not be any leak at all.

Incidentally, it's interesting if you look at that graph. It starts at 100, which is over twice the design pressure.

MR. HORSCHEL: I apologize for that. I should have pointed that out.

MR. SIESS: No, it's all right. That's fine. I'm not criticizing you there.

MR. HORSCHEL: I'm not sure whether all your viewgraphs are in, but let me put this one on. I think I'm a couple ahead of you.

These are leak rate measurements that we did after the test. We actually developed a small gun, if you will, to hold over these small tears in the liner and actually measured the flow through that to see how much flow we were getting through each of the small tears, not the large tears, but the small tears. We wanted to see how that came into play.

Remember, we had a small tear near the one personnel airlock. We were able to get about 8 standard cubic feet per minute through that tear at a pressure of 125 psi, and slightly greater than that at 145.

MR. SIESS: Just a minute. You've given

cubic feet per minute. I'm trying to think percent.

MR. HORSCHEL: Here I've referred to percent mass per day.

MR. SIESS: Oh, okay. I'm sorry.

MR. HORSCHEL: And I asked him for more accuracy in these, so keep that in mind.

But at 125, it was 12.2; and 145, it was 10.7. Those are typical for, say, three to four of these locations. So it also means to me that since we only had 11 percent to 13 percent at --

MR. SIESS: Wait a minute. Why is 10.7 and 8-plus less than 12.2 at 8?

MR. HORSCHEL: Because mass per day, you have to look at the pressures. Pressure is strongly influencing these. And that's why it's --

MR. SIESS: Okay.

MR. HORSCHEL: -- an actual reduction.

MR. SIESS: Okay. It's mass per day?

MR. BENDER: What do we know about the change in the measurements of the crack during this leakage period?

MR. SIESS: A little louder, Mike. I can't hear you.

MR. BENDER: Are the crack sizes stable or are they growing? What is it that you envision is happening?

MR. HORSCHEL: Well, the way you're asking the question, you're asking for my opinion. And that's all I can offer.

MR. BENDER: That's all I expect since you weren't there.

MR. HORSCHEL: If you look at the bottom insert plate, the one below the large major tear, can't you see a large neck region. And that neck region is almost a half an inch long. So if it would have torn, you would have about a half inch tear.

Down in the other areas where they didn't tear, they're all about a half of an inch in length. So I think the majority of the tears that you see in there more or less just happened suddenly and were stable. The big exception, of course, is our major tear, the one that was 22 inches long.

MR. SIESS: And you don't know whether it ran or just connected up several smaller ones?

MR. HORSCHEL: That's very true. If you look at the lower section, you can actually see some areas that are right next to where the start is, and you're not sure if one happened first or they kind of happened concurrently and grew together to be one large tear.

MR. BENDER: Well, let me pursue my point one step further.

The ones that were stable, is there any rationalization for why they were stable?

MR. HORSCHEL: The only thing I can think of -- and again, my opinion.

MR. BENDER: I understand.

MR. HORSCHEL: The ones that appeared to be stable were near a circular insert plate. To travel vertically, they would have had to go into thicker material.

Now, there was one where we didn't the cracked propogation. Of course, we just had a horizontal surface, so it could more easily propogate in a horizontal direction. And that's just the concentration of the --

MR. SIESS: But you don't really have any basis for saying that a crack propogated from a particular location? You have some cracks that apparently got larger, but you don't know whether that was several cracks connecting up.

MR. HORSCHEL: True.

MR. SIESS: I mean, you know. In a ductal material, cracks will both get wider and longer. But there was no dynamic propogation. Right?

MR. HORSCHEL: Yes.

Going back to --

MR. SIESS: Go back -- did you skip through Equipment Hatch A?

MR. HORSCHEL: Yeah.

MR. SIESS: Those were little tears?

MR. HORSCHEL: Yeah. We had four tears in our Equipment Hatch A.

MR. SIESS: Okay. Now, where were the stud pull-outs?

MR. HORSCHEL: That's Equipment Hatch B. And there you can see the stud pull-out right there.

MR. SIESS: Okay.

MR. HORSCHEL: That's the upper one as identified, and this is the lower one. So obviously, that upper one didn't tear that size hole until later on in this experiment. It certainly could have been there at a pressure of 140 psig's.

MR. SIESS: And that's a 3/16 inch maybe diameter hole, 40 percent?

MR. HORSCHEL: Yes.

MR. WARD: Dan, how did you make these flow measurements? You said a gun that --

MR. HORSCHEL: Essentially we had a hose from a nitrogen bottle, and then we actually had a handle arrangement seal that we could hold it on to the small tear.

MR. WARD: On the inside on the tear in the lining? Okay.

MR. HORSCHEL: And in line with that, we had

a pressure gage and a flow meter. So we just turned --

MR. WARD: Okay. You're just supplying flow from the -- all right. I understand.

MR. HORSCHEL: One thing to point out that you might catch with this is cross-talk between holes. We did find stuff between the upper left and upper right of the Equipment Hatch A. We pressurized one and we actually had to hold our finger over the other one because some of the air was coming back in. Same thing with B.

MR. SIESS: Did you do anything on the outside to see what kind of an area that stuff was coming out, like soap film or --

MR. HORSCHEL: No, not with soap film. We tried it with inert gas, but your sensor is so sensitive that as soon as it entered the outside, you would just -- we found it all over the place. But we really couldn't tell either qualitatively or quantitatively where it was coming through, the leakage was coming through.

MR. SIESS: You could have put a coding on there.

I'm not sure it makes any different. But, you know, once there's a hole in the liner, it's going to get out whether it goes through one concrete crack or six, it doesn't really make any difference.

MR. MARK: You were using nitrogen?

MR. HORSCHEL: For these tests here, yes, and also for the high pressure tests, we used nitrogen.

MR. SIESS: A little less viscous than air.

MR. HORSCHEL: For high pressure testing, it was just much more convenient to use the nitrogen. And since you use that for the high pressure test, we wanted the one/one correlation for this test.

MR. SHEWMON: Chet, it does make a difference in one regard, in how much particulate plates out in the process. Though that's a detail the NRC can't give anybody credit for.

MR. SIESS: And, of course, the model -- I don't think can model that very well because the -- I don't know whether that models the crack surface or not, you see. That wasn't part of this and it was a very good point. If it's a tortuous path, you deposit more stuff on the way out.

MR. HORSCHEL: And I'm sure the path -- well, actually, I would expect the path to be much more tortuous to a four-and-a-half-foot thick wall than it would be for --

MR. SIESS: I don't think so. These cracks were pretty big.

The surface of the crack might be different, because of the difference in the concrete.

MR. HORSCHEL: The post test inspection: One thing you find when you open up your equipment hatch, as we

mentioned before, that the seal was exposed at the 3:00 and 9:00 position. We also, of course, realizing concrete was intact and the area of the major tear, however, the wall was delaminated.

After the test, you could actually walk up to that and put your hands in that area, probably about a two foot by two foot square area, and it was actually cooler from the rush of nitrogen. You could also knock on that and you could distinctly hear a different sound in this area, as opposed to other areas on the containment wall.

There was little sign of distress that we saw in today's tour in the basemat-cylinder wall junction. As you saw, we removed some of the paint in the area of the distressed liner.

MR. SIESS: Excuse me. What do you mean by delaminated?

MR. HORSCHEL: Through the wall thickness, the concrete wall itself, I believe there was some delamination.

MR. SIESS: In the concrete?

MR. HORSCHEL: In the concrete itself.

MR. SIESS: What makes you think that?

MR. HORSCHEL: Because it was so much cooler due to the nitrogen. The nitrogen actually came out adjacent to that penetration and traveled somewhat.

MR. SIESS: Why couldn't it have traveled between the liner and the concrete?

MR. HORSCHEL: It certainly could have, but that wall was cooler there and also, when you knock on that wall in that area, it does give a different sound; it does sound hollow. It sounds like it's just a concrete shell, say, two inches thick. It doesn't sound like it does a few feet away.

MR. SIESS: That's the best test, yeah.

MR. HORSCHEL: Yeah. As we also noted in our tour, there are several small tears. And as I just showed you, we measured the leak rates from many of those tears, as it was able to give us some idea of when those tears developed in the test.

MR. SIESS: Is all the rebar welded including the diaganol rebar?

MR. HORSCHEL: Let me defer that again to Randy.

MR. WEATHERBY: The original bars were setting right at the yield point, 145; the hoop bars were yielded.

MR. SIESS: What about these?

MR. WEATHERBY: The diaganol bars were yielded as well.

MR. SIESS: They were yielded, too. Okay.

MR. HORSCHEL: One other thing we noted during post test inspection, that each tear or neck region that we found was associated with the study.

MR. SIESS: You said "adjacent to," that would mean to one side to me. You mean "at the tear."

MR. HORSCHEL: Well, I guess if you look at where it tore, it goes right down the edge of it; that's why I said "adjacent."

MR. SIESS: I see what you mean.

MR. MARK: You spoke of some patches being cooler, and you deduced that the nitrogen was oozing through that region.

MR. HORSCHEL: This is in the area of our major tear and it's actually to the wall.

MR. MARK: Yes. Do you mean that you or somebody walked up to that damn thing and held your hand against it?

MR. HORSCHEL: Yes, sir. After we decreased the pressure to 20 psi, we went up to it.

MR. MARK: Oh, after you dropped the pressure. Thank you.

MR. HORSCHEL: The project is important to me, but not that important.

Here just a stretch out of the liner; you can see some of the distress as well as the tears. I want to make

a -- point that out that this shows both the tears and the distressed areas.

Here, of course, is our major tear. We had a tear next to this personnel airlock. We had two tears here that actually showed signs of leakage. Two ones below equipment Hatch A, they look like a tear to me, but with our little flow heater system, you couldn't get an even flow of nitrogen to these two lower tears.

MR. SIESS: So you went through the wall, then.

MR. HORSCHEL: In fact, we might not even be through the liner, is that what you meant?

MR. SIESS: I mean not through the liner.

MR. HORSCHEL: Visually, just looking at it, without any magnification, to my eye, it looked like they washed it.

Here we had some neck areas next to this that were very analogous to torn regions in this -- next to this insert plate. We had a slight denecked area next to this personnel airlock. And we had the two studs tear through this Equipment Hatch B. This is one of our large bore piping penetrations, scale large bore piping penetrations.

We actually found some distress there that wasn't too significant. I probably have some plots and I'll show you later that show some of the strength concentrations that

are associated with the stud and that insert plate.

MR. SIESS: All of those are at studs?

MR. HORSCHEL: All of those are adjacent to studs.

To equip the REA model, this is Equipment Hatch A, this is our one instrumentation penetration where next to it we had the stud tear and the liner tear.

A couple of views from the outside. Close up, you can see how the concrete is cracked just slightly more. We do see some places were the conrete was removed. But this general area in here is the area that was cooler to the touch and also sounds different when you knock on it.

MR. SIESS: Were those the little spauls?

MR. HORSCHEL: Yes. Right here.

MR. SISESS: Were they there before you took the pressure off, because sometimes you get that when it recovers.

MR. HORSCHEL: That I couldn't tell you. I really don't know. It doesn't show up well in this picture but there is some motion between like a block of concrete here and this other concrete over here. And there was some difference in radial motion.

MR. SIESS: Afterwards?

MR. HORSCHEL: Yes. This is all post-testing inspection. Let me show you one construction photo before

the concrete and rebar was in place. Here you can clearly see the insert plate, you can actually see the liner studs where we had the major tear.

MR. SIESS: That's the first line of studs outside the break?

MR. HORSCHEL: Yes.

MR. SIESS: What was that distance?

MR. HORSCHEL: Approximately an inch.

MR. WEATHERBY: About an inch.

MR. EBERSOLE: Tell me, don't you think the attachment of the studs translates the stress to the steel and causes it to fail and if they were not there, a failure would occur later?

MR. SIESS: Uh-huh.

MR. HORSCHEL: There's certainly an interaction going on. We'll get to some of that later when we talk about the analysis.

Here's an inside view of that major tear. One thing that actually and probably should look better in this photo than it does in real life, you can actually see some of the shadows where the studs are located.

Again, there is a small degree of displacement between the insert plates and the other side of the liner where the tear was located.

There's one thing I pointed out during the tour

but wasn't obviously evident, is the equipment hatch wasn't there. Here you can see some of that ovalization. You can see the seal is slightly exposed, and here's that one tear near Equipment Hatch A. I have closer views of each of these shots, each of those locations.

Here's the tear. You can almost see the outline of the base of the stud in here. The stud is located right there due to --

MR. WARD: That's not through the wall?

MR. HORSCHEL: That one is.

Actually, when we took these shots, we didn't even realize those two lower ones were there and we don't have any current photos of thsoe lower ones, to my eye, they are obviously smaller than this. But it did look like it was through the liner and its neck that far.

Here's a closeup view of the 1CL. You can see the gumdrop seal being exposed. Keep in mind there is another gumdrop seal that is still in place.

MR. SIESS: Let me get oriented on that. That's the hatch cover to the right.

MR. HORSCHEL: This is the rate for the hatch cover; this is the sealing surface of the sleeve here at the edge of the sleeve.

MR. SIESS: And what's the seal?

MR. HORSCHEL: The red is the foam rubber;

that's the gumdrop seal.

MR. WARD: Why do you call it a gumdrop seal?

MR. HORSCHEL: If you look at it in cross-section, it looks like a gumdrop.

MR. SHEWMON: Are these wider than O rings or -- one of the seals -- one of the penetrations -- let's see, one of the -- what do they call these big cylinders that go through entrance hatches overlies more than the other?

MR. HORSCHEL: Yes. This is Equipment Hatch A, which only had the one cover. We saw more ovalization there.

MR. SHEWMON: You're suggesting, okay, that the reason was because of the cover or the lack of covers or the design of the reinforcing around it?

MR. HORSCHEL: To some degree, you have to speculate on that. But let me tell you what my opinion is: No. 1, the reinforcing is identical, the primary reinforcing on each one of those bosses is the same. Local reinforcing is somewhat different. I think the major reason -- a couple of major reasons they're different is, No. 1, the second equipment hatch actually has a softening, if you will, of the liner. It's got that tapered in section to the boss face. You saw that when you pulled on that, you actually tore some of the stud in that soft -- it's hard to transfer

the load into the sleeve barreling. I think that is one of the most important.

MR. SIESS: This one had no thickened wall?

MR. HORSCHEL: Right. So a liner -- let me go back to that last.

MR. SIESS: It's a steel frame, essentially the same around both edges?

MR. HORSCHEL: The primary reinforcing is the same boss around --

MR. SIESS: By reinforcement, you mean the structural frame?

MR. HORSCHEL: Reinforcing steel.

MR. SIESS: No. I'm talking about the structural frame; the hatch is a steel insert, isn't it?

MR. HORSCHEL: Yes.

MR. SIESS: That is identical to the tube?

MR. HORSCHEL: There's differences between the two just because the one had an inboard hatch cover and an outboard hatch cover. It has to be thickened for the steel. The nominal thickness where it actually penetrates the concrete as it goes through the wall is, I believe, the same thickness.

Here you can see the insert plate around Equipment Hatch A. This is in the plane of the cylinder wall, as is this (indicating). You have a direct loading right there

and you can transfer more load into it. With the other one, where it's thick in both inward and outward, you have that tapered section that you have to go through, which makes it so much softer and much more difficult to get the O ring into that sleeve through the liner.

Also, in code, you can't allow the liner to take the radial load on the sleeve. You have to have a thickened ring behind that. This hatch, because we only had the one cover, has one collar around it and you can't see the (cough). The other equipment hatch has two collars, which also makes it stiffer.

MR. SIESS: Which one oval, the one with the single seal?

MR. HORSCHEL: The one with the single equipment hatch cover.

MR. SIESS: Of course, the one with the double seal has got the thick wall, doesn't it? Which one has the thickened wall?

MR. HORSCHEL: Both of them have thickened walls. One goes entirely to the outside of; one is centered in the wall and goes both inward and outwward.

MR. SIESS: But the thickness is the same?

MR. HORSCHEL: Approximately, yes.

MR. SIESS: And the reinforcment is the same in the concrete?

MR. HORSCHEL: The primary reinforcing is the same. This local reinforcing defines your -- the boss base.

MR. SIESS: Now, there is a steel insert in there.

MR. HORSCHEL: The sleeve itself. Is that what you're talking about?

MR. SIESS: That was the same?

MR. HORSCHEL: The sleeve is slightly different because of the equipment hatch, sealing surfaces and --

MR. SIESS: Is there any chance that the steel with the collar was stiffer than one of the other?

MR. HORSCHEL: Randy, did you want to make a comment?

MR. WEATHERBY: You said that the thickness was the same. Were you talking about the two bosses together on Equipment Hatch B?

MR. SIESS: While I'm thinking about the piece of steel, the steel cylinder constitutes the hatch. And on one end of that is a flange to which you attach a cover. And that flange is what elongates and what ovals. Right?

MR. HORSCHEL: Right. The sleeve itself.

MR. SIESS: Now, it ovals because it's got more load on it in this direction and that direction; but

that flange physically, is it the same stiffness and strength of the two hatches. If I make that flange stiff enough, it doesn't make any difference what load I put on it, it's not going to oval.

MR. HORSCHEL: They are approximately the same.

MR. SIESS: Okay.

MR. WARD: Well, let's see, would you conclude that one of these design styles is superior to the other then, and I guess B is superior to A, or at least as far as resisting this high pressure?

MR. HORSCHEL: If you're worried about ovalization, I would say that B is superior just because you have that transition region from the cylinder plane back to the inside base of the boss.

MR. WARD: Yeah.

MR. HORSCHEL: And that transition actually makes that much softer so it's very difficult to get both back into that sleeve.

MR. SIESS: The transition you're talking about is, what, the thickening of the concrete?

MR. HORSCHEL: Well, it's the thickening of the concrete but really what I'm referring to is that liner; the liner is really loading the sleeve.

MR. SIESS: Rebar is not attached to the

sleeve?

MR. HORSCHEL: That's right. The rebar is not attached to the sleeve.

MR. SIESS: And the transition you meant was where the liner bent.

MR. HORSCHEL: Right. And that's where you had the two studs critical. As long as you -- see, those studs are much more critical in that design as opposed to the other.

MR. SIESS: If you really want to worry about designing containment for severe accidents, you could probably design a containment sleeve that is a lot stiffer than the ones we've designed for LOCA, could avoid some of it.

MR. WARD: It's just making the steel sleeve.

MR. SIESS: Make the steel sleeve stiffer; I mean, to do it, make it easier if it's 14 to 20 feet down here.

MR. HORSCHEL: But it's also conceivable if you make the sleeve stiffer, thenyou can induce boss out here in your liner.

MR. SIESS: Oh, yeah.

MR. HORSCHEL: So, I don't know if you want us to comment on the design and how appropriate they are for --

MR. SIESS: Ideally, you would like to be able to reinforce a hole in there so that everything outside the hole was the same as it was before.

MR. WARD: But didn't the B type design do that better than the A type design?

MR. SIESS: It probably did, but then it did other things that were worse; pulled out the studs.

MR. HORSCHEL: Let's jump over to B.

Here's our cylinder wall. This is what I found in the transition. Here's the interface of the boss.

Here we had two small stud tears. As I pointed out during the tour, this angle at first was very sharp, due to the loading of the containment, it's actually curled and it's now very rounded in here; and that's why I'm only giving you this mud flow back into your sleeve, it just tends to soften that load.

MR. SIESS: Now, you told me that the prototype design where we made the square corner there.

MR. HORSCHEL: Yes. Stone & Webster, I believe generally uses the square right hand junction. We were worried about placing the concrete underneath it, especially at the top area and didn't have the access to use in a full size containment, so we actually tapered this.

Here's one of those stud (inaudible) Equipment Hatch B in that transition ring. You can see that this is a

different type of failure than we experienced in other areas. There is a direct (inaudible) to a shear type loading on the liner.

MR. EBERSOLE: Would you kind of draw a general overall conclusion that the studs do more damage than they do good from this experience?

MR. SIESS: You can't conclude that. We know what damage they do, but we don't know what good they will do.

MR. HORSCHEL: They are there for a thermal purposes.

MR. SIESS: I know they're there for thermal reasons.

MR. HORSCHEL: And if you look at the magnitude of pressure during this testing, this 3.15 times the design pressure, if you take care of this, are you sure something else isn't going to happen at 3.16.

MR. SIESS: Or 3.14.

MR. EBERSOLE: Would buckling be all that damaging? Would buckling cause leaks?

MR. HORSCHEL: Due to a large --

MR. BENDER: Well, I imagine you would want to have some type of controlled information and that's what the studs do.

MR. EBERSOLE: In the buckling mode, but you

could do it in other ways. You could even fold it.

MR. BENDER: There are lots of ways to skin a cat, but this is the way they chose to get it. A better question is --

MR. SIESS: Jesse, we have some experience with buckling. We have some actual experience with buckling. Indian Point feedwater line failure, it occurred right at the face of the inside wall of the containment through 180 degree through-wall crack and it sprayed hot feedwater -- and I don't know how hot the feedwater is -- up the wall above the pipe. And it buckled out a section of liner, as I recall, about 10 to 12 feet wide, circumquenchly and about 30 feet vertically.

It pulled the liner away from the angles. It didn't have studs, but there were circumquenchal angles. Actually sheared off -- broke the weld, left the angles embedded in the concrete and broke the welds. It did not cause a leak. They were cracks formed where the channels were welded, the vertical channels, at the edge of the buckle, but they weren't through wall, but that might have been pure chance. So if you had nothing, you could probably buckle a pretty good section in and maybe not rupture the liner. But then, I don't know what I would do if I had 140 psi acting at the same time and a few other things.

MR. HORSCHEL: It is one conclusion that we

haven't tested or the ankor system in any containment is going to play an important part in the liner integrity.

MR. SIESS: What is __

MR. HORSCHEL: All capacity is really unknown. It depends on what type of situation you have. Here we have some studs. Does a full size stud behave the same way? They also have angle sections attached to it. But do those engage the same way.

MR. EBERSOLE: In the ice condensor like at Sequoyah, that was a major problem.

MR. SIESS: That's just a steel containment.

MR. EBERSOLE: I know, but --

MR. SIESS: We tested those two years ago; they acted differently.

MR. EBERSOLE: Yeah. We really haven't come to a --

MR. SIESS: Got a better reason for putting a concrete shell around it?

MR. BENDER: -- judgment yet as to whether the steel is hydrocracked and is bad or good; all we've established so far is --

MR. HORSCHEL: It's going to be important for the liner. It's something that we feel -- you will see later when you start talking about the analysis. But something that you have to include in here now, whether it's

going to fail the liner or not, is going to depend on any particular situation that you're going to look at. Maybe you'll have it all, maybe not. We need to learn more with our analysis and extrapolate with this model as with a full size plant.

MR. BENDER: Are you, later on, going to try to answer a few questions, like what were we trying to find out and how we could use the results? Are you going to try to answer those questions?

MR. HORSCHEL: Specifically, in this presentation, no. If you want to ask questions at any time through here, I'll try and go through it; I'll try to sum it up.

MR. SIESS: Let's save those near the end of this, Mike.

MR. BENDER: I wasn't planning to depose him now. I just asked whether he was going to be available.

MR. SIESS: They are going to get asked; whether they answer them or not, I don't know.

Dan, a lot of the earlier containments -- and I can't remember how long they kept it up -- they always welded leak channels on the back of the welds.

MR. HORSCHEL: Near the backup bars. Right? Especially --

MR. SIESS: Every weld in the containment had

a channel welded on the concrete side of it for leak testing. Not only did you have the weld in the liner, you had two more welds from the channel welded on there. Is that being done any more?

MR. HORSCHEL: I'm not familiar with it. Since you're not making any containments any more, I guess, they . . .

MR. SIESS: I know they were doing it for a long time because the specs don't call for 100 percent leak testing of liner welds; it only calls for a 10 percent sampling, 2 percent sampling, I think. So they have these leak channels; they could, you know, measure the leakage through the welds by adding more welds. I never quite understood it, but . . .

Okay. Go ahead.

MR. HORSCHEL: We considered about getting those in this containment model. But we saw that same problem and decided to go without.

What we did do was 100 percent vacuum boxes. We actually put a box on there tc pull a vacuum, after you soap solution the weld.

MR. SIESS: You did 100 percent weld test.

MR. HORSCHEL: We needed 100 percent vacuum box.

There's only one representation of a personnel

airlock. We had a monitor on , outside over here. You can actually see the studs marked in this figure. On the opposite side is a close up view of that tear. This is what we found on our post test inspection. We do have a gage located right here.

MR. SIESS: You're sure you don't have one under the gage?

MR. HORSCHEL: I'll get to that one.

Here again, you can kind of see the base of the stud.

MR. SIESS: I always figured strain gages were reinforcements.

MR. EBERSOLE: Isn't that a fundamental; you can't measure anything?

MR. HORSCHEL: Here post testing you can see the effects of the basemat evidence from my work map. As I said before, that's probably more visable in our video film than it is in this photo, but at least you can see that it did happen. It was a fairly uniform around the containment. Obviously, some variation in the amount, but it was still fairly even, about three-eigths of an inch.

Let's get to the one gage that we did have over a stud. And I also want to point out that this is where -- our structural integrity test is close to the high pressure test. We have a strip gage, which is a group of ten gauges

attached to a common backing. And this case, we only wired seven of those ten gages which you could actually see the strain concentration due to the stud. In effect, in the SIT, where we only went up to about 53 psi gage, you can see that we had strains on the order of .55 percent.

MR. SIESS: Each one of these is a different gage.

MR. HORSCHEL: Yes. I --

MR. SIESS: And four of them are closer to the stud, and the other three are out further.

MR. HORSCHEL: Yes. The total length of this gage I believe is like one and an eighth inch long, very, very closely --

MR. SIESS: What was the spacing in the strip.

MR. HORSCHEL: Ten gages on a strip; total strip length is about an inch and an eighth.

MR. SIESS: I mean, were they like this or like this (indicating)?

MR. HORSCHEL: In this case, they're all horizontal and they're measured in a hoop direction sprains.

MR. SIESS: Yeah, but are they in series or parallel, I guess that's what . . .

MR. HORSCHEL: It's in a series.

MR. SIESS: The series are getting --

MR. HORSCHEL: The strip is in the horizontal direction and each gage on the strip is measured horizontally.

MR. SIESS: Okay. I see. And what's the yield strain of that material?

MR. HORSCHEL: Randy, you're going to have to answer that.

MR. WEATHERBY: It's going to be around .2 percent or so.

MR. SIESS: So the liner runs that high?

MR. WEATHERBY: Let's see, it will be about 50.2 KSI over --

MR. SIESS: Leave that up just a minute.

MR. EBERSOLE: These all show permanent views.

MR. HORSCHEL: Yes. That's my next point.

MR. SIESS: That was a tremendous historesis.

MR. HORSCHEL: We continued on in the high pressure test. This is the same strip. Of course, you only -- in my era, I only got six of the seven gages (inaudible).

But we zeroed that. Any plastic strain that was there was zeroed in the audit at the beginning of the test. You can see that we probably had about 25 percent strain, plastic set. That was zeroed. And then when we conducted

the high pressure test, we started from zero and continued on and had some substantial strains, over three and a half percent.

MR. SIESS: Have we got that slide?

MR. HORSCHEL: Yes, you should have both of these -- the back of the packet and it belongs to you.

MR. WARD: I think it's in this other --

MR. HORSCHEL: It's in both of the reports.

MR. SIESS: Oh, it's in one of the reports. Okay.

MR. HORSCHEL: It's also in the viewgraphs.

MR. SIESS: I just couldn't read the scale, that's why I was asking. That's all right. Just put the last one up for a minute, will you?

MR. HORSCHEL: Just the last one.

MR. SIESS: You got up to about three or 4 percent then. What's the -- I can't read the arc real -- pressure scale, 30, 60 . . .

MR. HORSCHEL: Yeah, 30, 60, 90, 120. Pressure is 145 and they said we lost some pressure during that last leak measurement. That brought us up to about 140. We actually did another scan. You see we had some significant straining; we were actually trying to maintain pressure and were actually losing it slightly.

MR. SIESS: Which of those correspond to the

four that were very high in the previous plot?

MR. HORSCHEL: I guess I would have to look at the individual plots. I do have them listed but I don't know them off the top of my head.

MR. SIESS: So there is less difference here --

MR. HORSCHEL: We've got a strained difference here.

MR. SIESS: Huh?

MR. HORSCHEL: There is 1 percent difference between the first and last.

MR. SIESS: Yeah, but there was a 10-to-1 difference on the others. Five, six-to-one difference on those.

MR. HORSCHEL: Okay.

MR. SIESS: The three on the left are a fraction of the four on the right. As you moved away from the stud, it really went down. But on the other figure that is --

MR. HORSCHEL: That's fairly consistent, which means you're really loading your stud and you probably have some classic defemation in your stud.

MR. CLAUSE: I guess that's kind of typical. What you see in an analysis, though is that drain concentration associated with discontinuity like the studs

tend to be higher when you're elastic, or they're low (inaudible). As he strains, enough power strains the concentrations actually decreases on a relative basis.

MR. SIESS: What kind of stress/strain curve do you have for the liner? Have you got a plastic region on it?

MR. WEATHERBY: Yes, it does.

MR. SIESS: When does it start strain augmented?

MR. WEATHERBY: I guess I would have to look at that. I don't remember off the top of my head.

MR. SIESS: Your old model steel went outt about one and a half, 2 percent.

MR. WEATHERBY: I believe that s around 1.7; 1.8 percent.

MR. SIESS: Thank you.

So you were one and a half -- one, one and a half percent in the strain hardening here. You're out beyond that plastic.

Okay. Thank you.

MR. HORSCHEL: And finally, I want to discuss the future options we have of the model.

First, of course, is the nondestructive test of the containment model. We have several things that we will be doing. Vacuum boxing some of the liner sections that

were in critical areas and including some down, for example, in the basemat showing wall region, it's under water. It's not showing visual signs of the stress, which we like to check on.

We also will possibly do some x-raying of critical areas, specifically in the areas of major tear. We'll probably end up removing concrete in that area just to understand more of what happened there, how much the delamination was, and some of the dislocation motions in there just to better understand what happened.

MR. SHEWMON: X-ray is to radiography for loss of density or what do you mean X-ray?

MR. HORSCHEL: Just to see the placement of rebars, how things move, see if we can determine any slippage, things such as that.

MR. SIESS: When you remove the concrete, and particularly, you're worried about the delamination, do you expect to mark it in any way before you remove it?

MR. HORSCHEL: It's something that we need to talk about before we do it. And that's why we haven't --

MR. SIESS: I've had that problem of having cracks and didn't know whether they went this way or this way in a particular thing. And we used something that would be very crude compared to what Sandia can think of, I'm sure. I went out and got a gallon of wood stain and poured

it in the crack. And when I picked the thing up, I could tell where the cracks were. I'm sure you could find a better system of injecting some sort of a dye.

Your transverse cracks are likely to be, you know, just straight through the wall. If you look at that delamination, it would be worthwhile to try to inject some sort of a dye that -- because you probably pick up delamination by just the concrete coming apart? But you are thinking about some way of looking at some of this stuff?

MR. HORSCHEL: We will be, yes. We appreciate your input into that and want as many opinions as we can get before we commit ourselves. Once you start cutting it apart, that's it. You want to document it well before --

MR. SIESS: Some people do worry about this plate-out business. I'm just not sure the model was designed for that and how good it would be. But one possibility would be to inject something from the inside through the crack to see what path it took, although liquid and a gas wouldn't necessarily take the same path.

MR. WARD: Well, that's his air -- he's got that down there is very slow --

MR. HORSCHEL: We also wanted to measure the thickness of the liner in those areas to make sure (inaudible) or do you want how much flaw that was just not

visually perceptible in the post testing inspection of the containment model.

At this point, after we conclude nondestructive test, we have a couple of paths that we could take, which are some I've mentioned exclusively.

First, the sectioning of the containment model. We can actually inspect these areas with greater frequency and look at some of these and see if we can learn more about what happened during this test or, again, we can retest the containment model and have several options there.

The first one that comes to mind, of course, is, in carrying this test out with the use of a bladder. We can also continue on with doing something with the liner repair to see if we can develop some other type of repair.

We also have dynamic options, both external explosions and external explosions, and also things such as terrorist attack and small munitions, and also aerosol testing of the containment.

Right now, we're probably leaning more towards this area, but we are keeping an open mind in trying to determine what will be the best route with the dollars that we have in the interest of the NRC.

MR. SIESS: Let me raise a couple of comments on these possibilities.

MR. HORSCHEL: Certainly.

MR. SIESS: The bladder would be interesting, I think, because you find out when your large hatch is ovaled enough, and you might manage to get it up to a knuckle failure.

Why the repair? I can't see any advantage. You've got enough tears in that liner that you repair these and you'll just be 2 psi up and get some more. That's just my feeling.

The dynamic would be interesting except that unless you repair the liner, you've already got the cracks started and you would have a real question about that. And even if you repaired it, I'm not sure, although that would be interesting.

What's the external like?

MR. HORSCHEL: Just an external blast to see how the container behaves.

MR. SIESS: The only plant we ever worried about -- let's see. We worried about that on two plants, if I'm not mistaken. We worry a lot more about missiles.

MR. EBERSOLE: If you put a bladder in it and you're saying retesting so the bladder will treck the defamation of the concrete, won't you eventually then catastrophically fail the concrete?

MR. SIESS: I could use water.

MR. HORSCHEL: And we could tear the bladder

and lose whatever pressuring of the water.

MR. SIESS: You could use water like they did --

MR. EBERSOLE: I'm talking about if you used air. It would lead to a catastrophic --

MR. HORSCHEL: Certainly you would expect some type of rebar failure. And what that led to, it was really hard to speculate that. It's hard to conceive of anything happening like with our steel model test where pieces actually came off of the containment model.

Keep in mind they actually severe a chunk of this concrete out -- a large chunk. They're serving at least eight layers of reinforcing bars all around the whole mercury of that chalk.

MR. EBERSOLE: Well, it would search and find a weak place, though, wouldn't it?

MR. SIESS: If you test with the bladder, I'll take bets on what fails -- the bladder.

MR. EBERSOLE: The bladder?

MR. MICHELSON: Well, it seems to me before you do anything -- begin to think about what kind of accidents go with this kind of overpressurization. And it seems to me the only kind that can go with them are those that involved very high temperature gases. The core has to heat up a lot to generate the pressure.

MR. HORSCHEL: Very high temperatures in the large part of the annular in the order of 360 degrees Fahrenheit. Is that correct?

MR. MICHELSON: Well, I suppose it's of that order. I've not really tried to postulate the number.

MR. SIESS: Okay.

MR. MICHELSON: But, whatever it is, I think that that particular accident is one which could lead you to want to know certain kinds of things.

First of all, is the pressure self-limiting because of the crack? Does it leak out fast enough, provide some kind of plateau.

And secondly, I think you would want to think again about those temperatures as to how they might affect that gaskets or the O rings or whatever it is you've got in there. There's another distortion here now. So maybe only one of the O rings is affected, and it's the inner one, I guess.

So I think without trying to make any judgments about what's important, that you ought to try to correlate the results with accident phenomena.

MR. SIESS: Well, if you're talking about slowing the pressure, severe accident, you know, builds up over a period of several days. I remember the figures -- I thought you fellows had made some calculations earlier in

the game. I remember for a full-sized containment, a three-inch diameter opening was self-limiting in a slow overpressure case. And for this containment, that's, you know, 1/36th of that, three-inch -- 12-inch diameter. So I don't think there's any question about it being self-limiting.

MR. HORSCHEL: I guess the most recent stuff I heard on self-limiting holes to limit the pressure in the containment, is varied and it really depends on the accident. I've heard some things quoted where the full-sized containment of having holes in there as far as seven-foot squares or seven square feet. I can't remember.

MR. SIESS: No. That's their assumption for -- that's just a pure flat assumption for what it constitutes fully.

MR. CLAUSS: Seven square feet corresponds to rupture and (inaudible). It's more on the order of 10 square inches (inaudible) self-limiting for a slow pressurization.

MR. SIESS: And this is nothing but decay heat and no heat removal?

If there's core concrete interaction would be generating more heat, I guess. It's small for this thing.

MR. EBERSOLE: Have you-all done any testing on the electrical penetrations?

MR. SIESS: Yes.

MR. HORSCHEL: And in other divisions of Sandia, yes.

MR. EBERSOLE: Well, long before you get -- as Mike says, the pressure is due to temperature, and the temperature is going to carry away the guts of the penetrations because there has been a compromising (inaudible) that's unsuitable, I think.

MR. SIESS: Yes, and we're going to hear about that later.

MR. EBERSOLE: You know Charlie Wallis?

MR. SIESS: Yes.

MR. EBERSOLE: Okay.

MR. SIESS: Well, they've got tests on that.

MR. HORSCHEL: I guess we really don't have anything on electrical penetration.

MR. CLAUSS: Well, we don't have anything to present so maybe it's appropriate to attribute common knowledge (inaudible). What we found in our EPA testing is that because the length of the assemblies is so long that significant aftergrading is from the inside to the outside. You're right, the inside seals do degrade, but there's a small enough path that you don't even get that much leakage past the inner seals.

In any event, regardless of what happens to the

inner seals, the outer seals never see the full temperature of the accident. In fact, nothing even close to it. And those outer seals prevent any leakage to the environment through --

MR. EBERSOLE: They never do heat up that much?

MR. CLAUSS: No.

MR. HORSCHEL: And the only caveat that I would like to point out here is that we tested three types of electrical penetrations. And I guess there's many more out there. Some of them were actually built in the fields. But we don't have access to all of them. We're going to have to look at each specific one. But the three that we tested, what they said is correct.

MR. SIESS: But most of them do have an inner and outer seal and there is some temperature difference. Now, the glass ones didn't give any problem at all.

Look, have you made any analyses yet with temperature --

MR. HORSCHEL: For a different type of things, yes, we have. We've looked at, for example, large drywell heads and --

MR. SIESS: No. I mean, for the containment.

MR. CLAUSS: For the reinforced concrete, no.

MR. SIESS: Temperature should help. It puts

the liner in compression. Does it do anything bad? Well, this program that came in ought to help. Right?

MR. CLAUSS: I think it's really hard to say that without giving some analysis because there is a competing phenomena going on. You're right, thermal expansion, differential thermal expansion puts the liner into compression. But at the same time, the elevated temperatures reduce yield strength for that liner.

MR. SIESS: How much does it reduce the 400?

MR. CLAUSS: About 20 percent I would think.

MR. HORSCHEL: But does activity also increase?

MR. CLAUSS: That's what I said.

MR. SIESS: That is the one thing that's missing from this test. The severe accident which will produce 145 psi is also going to produce 300, 400 degree Fahrenheit temperature. And that's a question somebody will eventually ask. And at least for this type of failure, that's one advantage of testing the thing and knowing how it fails. Now you can go back and analyze whether the effective temperature is good or bad.

MR. CLAUSS: And that's what we intend to do. I mean, the long-range plan is first to finish our comparisons with this test and convince ourselves that we can do a good job of analyzing for static pressure alone.

And then analytically we'll go back and add temperature.

Well, if there seems to be a lot of effects on the order of confidence, then we may try to design separate effects tests to validate our model that includes thermal (inaudible).

Before you go too much further, I would just like to make one comment on the testing with the bladder.

We did do a quick analysis to see what the capacity might be if we test with the bladder. You have to keep in mind that the liner has significant tears in it. And before the test, we had calculated that the ultimate capacity where you get rebar failure in the range of 180 to 190 psig. The liner, however, is a significant contribution to the overall strength in the order of 15 to 20 percent.

MR. SIESS: 15 to 20?

MR. CLAUSS: And if you just do a simple back-of-the-envelope type calculation, take the liner essentially out of the ultimate strength, you're right down around 145, 150 for the failure pressure. So you're not going to get much further out of the pressure range that you retest the bladders. The only advantage would be that you might get some information on other failure or, i.e., catastrophic failure.

MR. SIESS: Yeah.

MR. WARD: Can you describe real quickly what

retesting with the bladder means. It's on your checknotes, but I don't know what it means.

MR. SIESS: No, I don't know any more than you do.

MR. HORSCHEL: Instead of using a liner as a pressure gird, we have to put some type of rubber membrane on the inside and use that as our pressure barrier. Keep in mind in our current configuration that's quite difficult to do. It's not a small task.

MR. SIESS: That's why I said, I bet the bladder fails.

MR. HORSCHEL: We'll probably have some (inaudible) in this place. We have all our lead wires running through, which you would also have to run through that bladder somehow. So it's a big complex thing.

MR. SIESS: Well, Murray did that at Alberta. You're familiar those tests?

MR. HORSCHEL: Yes.

MR. SIESS: I'm not sure what his details were.

MR. HORSCHEL: I don't think they had anything on them at all except the bladder and the water.

MR. SIESS: Yeah, I don't think he had any penetrations, you know. He got it to fail just beautiful. Water poured out everywhere.

MR. HORSCHEL: And also, one point about the dynamic testing is, our current data acquisition system is set up for a static test so that would necessitate redoing our data acquisition system for a dynamic test, which would also be -- have a significant cost attached to it.

Continuing on with separate effects tests, the first one here is equipment hatch leakage. We figure we could actually use some stuff on the containment model with those two equipment hatch covers. We can more or less seal one off and see if we can get that unseating of the pressure, unseating equipment hatch, what type of leakage we would get through there with certain displacements and things such as that. They're fairly inexpensive tests.

MR. SIESS: How close were you to complete unseating of that (inaudible).

MR. HORSCHEL: Again, we had to use some opinion in that. My opinion, looking at what we did with leak rate tests after the conclusion of the test and what we measured throughout the test, I suspect that we did have some unseating of Equipment Hatch B with some very slight leakage there that does jibe with what we actually calculated. We would see those at --

MR. CLAUSS: I guess we definitely had an unseating, if you mean by unseating, separation of the metal-sealing surfaces. There's no question about that.

What the question is, whether or not we had enough unseating that you exceeded the springback capability of the seal and thus produced leakage.

MR. SIESS: Now, that's scaled properly?

MR. HORSCHEL: Properly is hard to say because --

MR. CLAUSS: Geometrically scaled.

MR. HORSCHEL: Geometrically scaled accurately describes --

MR. SIESS: Whether they're proper or not, you know, that's a difficult thing to scale.

MR. HORSCHEL: Yeah, we never felt we could really scale the leak rate coming from the containment.

Obviously, some other candidates for separate effects tests that we've listed here is an insert plate in the liner section, similar to what we had in our containment, kind of looking at the strain concentration there and what effects that had. We could continue on with this for concrete and studs and see how those things interact, see if we can actually duplicate the type of tears that we saw on our containment.

Also the knuckle region in our containment is still of some interest. We want to know why we didn't get a shear failure or better understand what happened in this region.

Also, we saw some punching shear of the bosses, some dislocation motion there. Those are also candidates for separate effects test.

And lastly, as you saw out on the site, we have some reinforced concrete panels that are typical of the model that we would like to test to see if panel testing is a good idea to extrapolate the full-sized containments and actually a pressure vessel as opposed to a flat panel.

MR. SIESS: This business with the insert plate, I would think that if it was just a question of the stresses and strains, you would find that element of analysis ought to be pretty darn good for that. It may be a simple test to verify it. But the real problem is going to be handling what the stud does to the material properties and local enforces put on there by the stud which, I think, is completely independent of the insert plate. The insert plate might have changed the stress field in their. But the failure was local to that stud for that stress field.

MR. HORSCHEL: Okay.

MR. SIESS: If there hadn't been studs in there, do you think it would have -- what do you think would have happened to this thing with no studs?

MR. HORSCHEL: I don't think we would have seen those tears there. But I also think that you you need that insert plate. It's a combination of things. It's not

one or the other. I think you need that insert plate in the studs there.

MR. CLAUSS: I think Randy will show some analysis of which we essentially look at the effect of the insert plate. And the effect of the insert plate is to cause a very high slip area immediately adjacent to that insert plate.

MR. SIESS: By "slip area," you mean relative movement to (inaudible) concrete?

MR. CLAUSS: You bet. And the stud, of course, tries to constrain that movement. And that is what develops.

MR. SIESS: I suspect that no matter how you do it, the stud gets (inaudible), period. The stud cannot take that slip without yielding.

MR. CLAUSS: But the point is, there is no slip without an insert plate or something that's been changed -- to move away from the uniform (inaudible).

MR. SIESS: And once that stud has got a load on it of a certain amount, that's all that you can put into the plate. By shear and bending and the load remedy, the shear you can put on that stud before it (inaudible) the concrete depending on, take it are both finite.

Thank you, Dan. That was a very good presentation.

MR. HORSCHEL: Thank you.

MR. COSTELLO: To put things in the perspective of the four bullets in number 3, in the interest of expedienc, Dan.

MR. SIESS: Are we through with No. 2?

MR. COSTELLO: We went through No. 2. I'm sorry, sir.

MR. SIESS: We haven't heard anything about the British yet.

MR. COSTELLO: Okay. If you want to look at this agenda --

MR. SIESS: Yeah, but I want to know where we are on that agenda.

MR. COSTELLO: Okay. On this agenda, sir, we're still talking about comparison to predictions.

MR. SIESS: Okay. We're still in that -- we're in the second bullet. I just --

MR. COSTELLO: And we have two presentations on the predictions versus populations.

The first one will be by Dave Clauss, who has organized and orchestrated a very interesting undertaking of which there are 10 organizations who made the blind pretest predictions which were published in the NUREG (inaudible) on that part to offer speculations as to the --

MR. SIESS: Let me remind -- I should mention

that the Subcommittee numbers received copies of that NUREG. I doubt if anybody has read it. It's a little bit thick. But maybe they looked at the Executive Summary.

MR. COSTELLO: It has predictions. And again, the predictors met here in Albuquerque in November, had an initial meeting and an initial retrospective. They are then going to go back and do a little further work and have a final comparison and assessment of fidelity of predictive methods, which will be presented on or about the same time as our workshop in June.

MR. SIESS: Now, what are they supposed to do next if they're going -- obviously, they can all calculate now at what pressure failed. We know that.

Are they going to now try to explain why?

MR. COSTELLO: Yes, and they're going to concentrate -- Dave will talk about this. But essentially, they're going to concentrate on what they felt were the shortcomings of their modeling on what they would have to approve to get there.

MR. SIESS: Why they were wrong?

MR. COSTELLO: Yes, essentially. And Dave will talk about that and will be followed by a presentation from Randy Weatherby, who had done some scoping claculations using different kinds of concrete modeling techniques.

Dave Clauss will be first. Mr. Clauss?

MR. SIESS: You know, what I found fascinating was how much difference there was of prediction of displacement.

'Blind' Predictions by 10 Organizations

MR. CLAUSS: Okay. I'm David Clauss for Sandia. I'm going to be talking about the group efforts, if you will, on the analysis of the 1:6 scale model. And I really wasn't planning on going into great detail. I just wanted to give you a feel for what the activity was there and what the future plans are. If you want more detail, you'll just have to ask the questions and I'll try to answer them the best I can.

First of all, the objectives of the analysis. One of the first objectives in doing pre-test analysis is to get some insight that can be used both on the instrumentation and conducting the test. And I guess I feel like we were pretty successful in that, especially given some of the things that we captured during the test with instrumentation. We had a lot of strain gages in and around the penetrations where actually it would capture a lot of the high-stream concentrations.

Then the second objective, of course, is that did they have a blind prediction that you can use to equivalent

your analysis methods.

Okay. And then after the test, what we wanted to do with the analysis. The first thing, of course, is to understand the limits taken that control the model failure. And that's something that we're really just getting into right now, as well as the other groups. So it's really not a final conclusion now.

The second objective is to recommend additional tests, to investigate other potential limit states in the containments. For instance, we would like to look into shear failures. That is something that we still don't have a lot of confidence in predicting. It wasn't a mental state that we realized in the model, but it's certainly a possibility in the actual containment under actual severe accident loads.

And then the ultimate objective, as I think Dan has already pointed out, is to propose a comprehensive approach that can be used to evaluate performance of containments in the event of a severe accident.

Okay. I really wasn't sure exactly how familiar you all were with this effort that we have and who was participating. I just wanted to give you an idea of who is involved. And it is a fairly diverse group representing people anywhere from utilities to regulators to national laboratories, and pure research organizations.

In the U.S., there were four groups, three national labs, Brookhaven, Sandia, and Cargon National Laboratories. EPRI also participated. The work for EPRI was done by ANATECH.

In France, there was CEA participating; United Kingdom, we had Nuclear Installations Inspector, central electricity generating toward, and the safety and reliability aspects. In France -- I said France earlier.

In Italy, we had the regulatory agency there, ENEA. I'm never going to try to tell you what that stands for. It would take about five minutes.

In West Germany, there was GRS.

MR. SHEWMON: What's the SRD in the U.K.?

MR. CLAUSS: That's the Safety and Reliability Directorate.

MR. SIESS: They're a regulatory.

MR. SHEWMON: What's NII?

MR. CLAUSS: That's regulatory, isn't it?

MR. SHEWMON: So what's the SRD?

MR. WARD: I think that's part of the UK AEA.

MR. CLAUSS: They're not a regulatory.

MR. SIESS: No, they're not. They're more DOE.

MR. WARD: Sort of DOE or AIC.

MR. CLAUSS: I guess I should point out, too,

that most of these groups -- in fact, all of them essentially did this using their own instrumentation except for Sandia and Brookhaven. They were funded through agencies other than the NRC.

So essentially, it was on a voluntary basis. Our only cost was that of supplying the materials to do the analysis.

Some of the benefits of doing this, we got as much -- instead of having just Sandia analyze the problem,, they had 10 groups. And by doing that, you get a much greater awareness of what the potential failure modes in the containment are. And that gave us, I think, a better ability to plan the tests.

It was a way of establishing uncertainty in pre-test predicions. You would take 10 people who are all competent and you let them do analysis and they came up with great answers. And that's a measure in a sense of the uncertainty in the state-of-the-art capabilities.

MR. SIESS: Were they all -- I'm sure they were all experts with analysis. Were they all equally expert in the behavior of reinforced concrete?

MR. CLAUSS: I would say that in the U.S. all four groups definitely had a lot of experience in reinforced concrete. France, the CEA group certainly has extensive studies in that area.

I'm really not sure about all the British groups. CEGB, I'm not sure. But we certainly have a lot of (cough) reinforced concrete containments. But I'm not sure --

MR. HORSCHEL: One thing there, Dave. The CEGB went with Taylor-Woodrow that has this type of experience.

MR. SIESS: Well, Taylor-Woodrow has done some testing. I'm not sure CEA has --

MR. COSTELLO: CEA has done some test on some pre-test stresses.

MR. CLAUSS: Testing and analysis, yeah. They have done a lot of work.

GRS was relatively new in the reinforced concete effect. And, as you know, Germany, for the most part, concentrates on steel containments.

MR. MARK: When did you have the round-robin predictions available?

MR. CLAUSS: Well, we reportedly --

MR. COSTELLO: Go to the next slide.

MR. CLAUSS: Let me finish this slide first.

It was before the test, but I'll go into that a little bit more.

Another advantage was that we were able to evaluate the ability of different codes to correctly calculate response. And they have a list there of different

codes that were applied by the various groups.

And finally, I think it's been a very beneficial experience for all of us in that we've been able to exchange and share information about all of these tests but on other programs of interest in containment integrity.

Okay. The report which summarized the pre-test predictions was issued before the test. I think it was issued in May. The test was in July. And I believe some of you at least have received a copy of that report.

What it boils down to is separate sections giving details of each participant's analysis. And then we at Sandi ttempted to summarize those analyses as well, give appropriate introductory material.

Another feature of that report was that we asked all the groups to give us essentially standard plots which represented pressure history of strain and displacement at locations that were instrumented in the model. So by taking that approach, it allowed us to make not only comparisons between the difference analyses, but also a pretty immediate comparison between analyses and experiment.

There was quite a range, obviously, in that the capacities was anywhere from 130 to 190. I would say, though, the main reason for the difference was not at the calculations are responsible (inaudible), but that there was a much -- there is a lot of difference in the way that it

was interpreted in the criteria that were applied.

MR. SIESS: I have to say I'm disappointed. I suggested at one time that you ask these 10 people to predict a pressure at which they had fairly high confidence it would not fail.

MR. CLAUSS: Well, sir, I didn't want to disappoint you and we, in fact, asked that question. And it's really -- I don't have a viewgraph on it, but it is in the report. I think I brought a copy with me.

MR. SIESS: I didn't see it in the summary.

MR. CLAUSS: It's in the Executive Summary. I can look at it right now and give you the numbers if you would like.

MR. SIESS: No. Wait until you go through the rest of the stuff.

MR. CLAUSS: Okay.

MR. SIESS: I just missed it somewhere.

MR. WARD: Well, are you going to explain what you meant by different interpretations of failures? Are you going to get into that a little bit more?

MR. CLAUSS: I could try to describe that a little bit with this slide, which shows the predicted capacity as reported in this pre-test report by the various organizations, and also describes the limit mechanism phase that they thought would be critical.

I wasn't really planning on going into this in a lot of detail. Let me just kind of make some general comments on the differences between the criteria.

The people that are near the high end of the range in the 180s, 190s, those people didn't really try to analyze strain concentration in the liner. They did an axisymmetric analyses and they wanted to know what the ultimate structural capacity of the model was.

Now, given that, there's really no way we can say these numbers were wrong. If the liner hadn't failed, we would have gone all the way out to structural failure. I actually believe that the 180 number is probably pretty reasonable.

MR. SIESS: So they did not interpret failure as leakage?

MR. CLAUSS: Well, the ground rules, I must say, were to evaluate exact leaks when the model failed. Instead it was to find accidental leaks.

But, in reality, their interpretation was that the liner would not tear; and therefore, they took it all the way out to this --

MR. SIESS: If you can't answer the hard questions, it's always best to go ahead and answer the easy one.

MR. CLAUSS: Well, I guess I have to admit

that Sandia took that same approach. We felt there was enough flexibility in the liner that even given strained concentrations on the penetrations probably wouldn't be a problem. So we went out further.

MR. SIESS: Let the studs off, you probably let the -- without the studs, you were probably right.

MR. WARD: Okay. So the local liner tear failure was unexpected by most analysts?

MR. CLAUSS: I don't know. I mean, it wasn't that it was overlooked. It was just undetected. You thought it was going to be ductal and --

MR. SIESS: Now, one -- the PCA/CTL EPRI stuff, I guess, predicted a liner tear, but at the knuckle. Does it need a test of a knuckle or the (inaudible).

MR. CLAUSS: You know, I don't know if it's really fair to say it was unexpected. It was certainly a possibility that we have admitted. But we also didn't really have the capabilities in our analyses to go in and do a very rigorous analyses of those strained concentration areas. And to be honest, I'm not sure we do right now. The stud interaction is a very difficult problem to follow.

MR. SIESS: Now, in that respect, I see complete parallels between this and the steel containment.

MR. CLAUSS: In your inability to amend all the --

MR. SIESS: Well, you have the situation there, you recognized a potential concentration sort of that, well maybe ductility will take care of it. You couldn't really handle it anyway, so you went on, and that's where it failed.

MR. SHEWMON: But in the absence of vented containment, this is the -- I would argue that this is the best kind of a weak to have because it eliminates gross failure. It gives you a lot of paths where you're going to deposit aerosals instead of letting them out into the atmosphere.

MR. SIESS: Yeah, it's great.

MR. SHEWMON: So I'm not sure it ever did fail.

MR. SIESS: We ought to build them when new weak spots. You know, take the grinding wheel and go in and put a few around it.

MR. EBERSOLE: It looks like you have --

MR. WARD: I think this argument about depositing aerosals in effect is a little -- it's kind of a hopeful comment. I'm not sure we've got much evidence, but that's going to happen in a reliable and effective way.

MR. SHEWMON: Well, I think you ought to look back at the evidence because I've been told -- I can't give you chapter and verse, but I've been told it by enough

people that I suspect that chapter and verse is there. But partly, these things have to -- it's colder out there.

MR. SIESS: But, Paul, as I recall, for the slow overpressure case, loss of containment cooling. And they figure most of the aerosals are going to be settled out by the time we get to this. And if it's not an explosive release which could lift them off, you really don't have much aerosal in there. You don't have much of anything in there.

MR. SHEWMON: Only noble gas primarily?

MR. SIESS: That's what they say. For the slow overpressure, if the containment does fail, so what? The releases are very small.

On the other hand, if a containment ruptures explosively, like the steel one did, that itself is a catastrophe as far as the immediate concern, whether it released anything or not.

MR. WARD: So all the filtered vents that the Europeans are putting in aren't protected against us, that's a no, never mind anyway.

MR. SIESS: Well, it could be earlier. They're talking about venting a lot earlier. They're talking about venting of one to one half design pressures. And that isn't four days out, either.

MR. WARD: Yeah, but the capacity of those

vents is associated with slow overpressure from the --

MR. SHEWMON: It's a modest capacity, as I recall.

MR. SIESS: It's not as slow as I'm thinking about.

MR. WARD: Well, it's pretty small. Most of them are.

MR. EBERSOLE: Well, hasn't this test really disclosed that in a fortuitous matter, you've got an ordered failure, which is desirable?

MR. SIESS: That's what we're --

MR. CLAUSS: But we don't want to make that general conclusion yet. The one thing we're having to include is the effect of temperature, which we've already said, we'll put the liner in compression and we could delay this liner (inaudible) relative to other possible failures.

So you can't just make a general conclusion pertaining to all the containments out there. Aside from the temperatures is heated also, obviously designed it, the ankor details are different. So you really have to look at the containment on a case-by-case basis.

MR. SIESS: But it's awfully hard to hold the pressure on this system with any kind of leaks. And that is what you showed. You had an ability to pressurize one-third of the volume for a minute. Right?

MR. CLAUSS: Uh-huh.

MR. SIESS: For at least 4,000 cubic feet per minute --

MR. CLAUSS: Yes. In slow pressurization basis scenarios, there is no doubt this would have precluded further pressurization. But there are scenarios where you have very rapid pressure.

MR. SIESS: Oh, if you're talking about a hydrogen burn or a hydrogen detination, that's something else.

MR. CLAUSS: Okay. As far as talking a little bit more about the uncertainties (inaudible) criteria, the people on the low end tended to associate failure with general yielding. And that's sort of the opposite approach that's going all the way out in ultimate failure. Ultimate failure is not conservative and is very optimistic. Narrow yield is very pessimistic. What you're saying is that once you start being moderate displacements, you don't have confidence in your prediction and because of this, that's the failure pressure.

MR. SIESS: As I recall, there was some predictions that were pretty close on the pressure and there were other predictions that at least predicted liner tearing, but they were not the same.

MR. CLAUSS: Well, I think the only group

that explicitly predicted liner tearing was EPRI, which is actually ANATECH. And one of the British ones had predicted liner tearing at 190 or something like that, I recall.

MR. CLAUSS: That's -- I would have to go back and double-check. That's not really what I recall.

MR. WARD: Well, in this table, it says "NII, 130, local tearing around the studs."

MR. CLAUSS: Okay. They said that there was some possibility, but that basic --

MR. WARD: Okay. That was sort of an asterisk on their thing?

MR. CLAUSS: Yeah, right.

MR. WARD: Okay. I see.

MR. CLAUSS: The one that they believe most likely will transfer back.

MR. BENDER: Well, as it turned out, none of them addressed the failure mode that you observed.

MR. CLAUSS: Who was that?

MR. BENDER: No one was 100 percent correct. They all admitted the potential, but they didn't really -- all the analysis up there is focused on a different part of the structure.

MR. CLAUSS: Uh-huh, sure.

MR. BENDER: And so the only relevance of it has to do with, well, how close to that failure mode were

you in that line? And at the moment, I don't think we have an answer to that.

MR. CLAUSS: I agree. I guess that's one of the things I was trying to count on. The ultimate capacity, 180, that might be a good number. We don't know that since we didn't reach that point.

MR. BENDER: On the other hand, we don't really know yet what the purpose of the analysis ought to be. The NII uses its analysis mainly to evaluate whether the peole that are submitting their safety documents have a reasonable basis. I don't think they -- most of them even looked into the basis on which these pressures could occur. And so I'm not prepared to say that what they did makes much sense as an analytical method to -- for the purpose of evaluating this structure.

But it was an interesting exercise. I did scan it fairly carefully and what the various analytical methods are and what they can accomplish. And just from the standpoint of comparing the simplistic approaches with the more sophisticated ones, I conclude that it doesn't make much difference which ones you use. You get about the same answer anyhow. But for the purpose of this --

MR. CLAUSS: Tell me, which were the simple methods that were used? They were all fairly sophisticated approaches.

MR. BENDER: Well, they used -- all of them used computer codes, but some tried even to do a 3D analysis and some did 2D analysis, and some ignored some of the complex structural areas because their codes could only deal with axisymmetric conditions. And there is a whole litany of things that determine whether the method was sophisticated or not. And actually my impression was that aside from the EPRI analysis, most of them were very simplistic. That's my perception from maybe looking at the report for a day.

MR. CLAUSS: Well, I think that's true that most of the people did only an axisymmetric analysis. And relative to the 3D analysis, it gets simpler, but it's still a very sophisticated (inaudible).

MR. BENDER: Well, I didn't -- this is a long-standing argument. It's wasted it here.

MR. WARD: Dave, you thought -- I think you said you thought maybe the 180/190 or what they're predicting wasn't far off. But I thought I heard in the discussion earlier that if the liner hadn't -- when there was some conjecture, discussions about the failure that if the thing were retested with the bladder, I guess, in that discussion, I thought the opinion was it probably wouldn't go much beyond the --

MR. CLAUSS: What I guess the point I was

trying to make was that if the liner had not developed at their rate, that the 180 would be a reasonable number. When you tear the liner, the load --

MR. WARD: Oh, you're losing that strength through (talking over each other). Okay, I understand. But this reminds me a little of, you know, large break LOCA calculations where three or four people come in with different versions of the codes and they all calculate about the same peak clad temperature except some are during blowdown and some are during reflood. And so I don't know -- complicated business.

MR. SIESS: You know what this reminds me of? My daughter worked for an agricultural economist once. And his advice was, you could predict the price for a commodity or you could predict a date, but don't ever predict both.

But, you know, the outstanding things here is the number of people that thought that the wall base intersection was going to be the weak spot. And EPRI, who had actually made tests of that thing, thought it was going to be the weak spot. And it wasn't.

MR. CLAUSS: Okay. That's one of the things that was discussed in detail in the meeting that we had in November that Jim mentioned earlier. And there were several possible reasons why that area wasn't important. It all comes down to things that were done in the modeling of the

basemat. A lot of people didn't include the soil. And there's some evidence to test that by any introducing a flexible soil, they actually get most different shears and (inaudible) at the wall basemat juncture. That's one element that's not consistent with that analysis.

MR. SIESS: What is the support of the model?

MR. CLAUSS: I'm sorry. Say, again.

MR. SIESS: What's the model sitting on?

MR. CLAUS: What's the statement? It's sitting on a mud --

MR. SIESS: Is it on soil?

MR. CLAUSS: Pardon me?

MR. SIESS: Is it on soil?

MR. CLAUSS: Yes. Dan, you could probably describe the soil a little better than I can.

MR. HORSCHEL: Maybe he didn't catch the point. We have a working man underneath the foundation mat for the containment. That working mat is about six inches thick. And, of course, underneath that, we have soil. Some of that soil was imported and impacted for that containment model. That layer is probably about 15 inches thick. And then we would just go back to the natural soils which were there in place.

MR. SIESS: So for practical purposes, your mud (inaudible) would be the contact?

MR. HORSCHEL: Yes. And we do have a bond breaker in that we mean that we place the foundation mat on the (inaudible).

MR. SIESS: Do you have any idea what the separate modulus is for that soil under it?

MR. HORSCHEL: We did some plate berry tests, like you do for a spread for ASTM standards. We have those calculations. But we didn't really project the stiffness of the soil or separate modulus or anything like that. We have the data and we kept it there and we let the analysts use that in the way they felt more comfortable with that.

MR. SIESS: And they think that was the factor?

MR. CLAUSS: Yes. As I was going to say, the number of differences in the way the basemat was modeled. Some people chose the treatments for those ridges, some people modeled flexibility, some people didn't include the mud of the (inaudible). There's also layers of concrete, protective cores, and then a fill slab above the liner that there were a lot of differences the way people modeled. And some people thought it wouldn't be important, others did.

And you see a big difference in the results for shears and (inaudible) as well as basemat uplift, depending on how those details are modeled.

So we need to converge on that and find out which

are the important details and how that affects the model.

MR. SIESS: What's the worst condition, a rigid face or a flexible soil?

MR. CLAUSS: I guess I don't want to make a conclusion on that. But my opinion is, from what I've seen is that the flexible soil is actually the worst condition the reports shown of the wall.

Now, the reason for that is that when you introduce flexibility of soil, you get more bending in the basemat or more flotation in the basemat is a better way to put it. And the cylinder essentially has to conform to the curvature of the basemat at that intersection.

So by introducing flexible soil, they're getting greater curvature at the base of the wall which leads to higher shears and runs within the wall.

MR. SIESS: I guess I'm having a problem. It's just hard for me to believe that the rotation of that big basemat was significant compared to the rotation of the base of the wall, the wall itself, which is much, much thinner. But go ahead. I'll worry about it. I don't know whether that's real not, if that could be calculations.

MR. CLAUSS: Well, I've got a number of viewgraphs here that show various comparisons between the analyses and the final results. I guess I'm only going to put a few of them up. And really -- the point I'm going to

make and they're making some disagremeent, there may be some disagreement from the Committee. But in my mind, I felt that the agreement between the analyses for global measures of the response, like radial displacement to midheight was really very good.

MR. SIESS: This is radial displacement at midheight?

MR. CLAUSS: This is radial displacement of --

MR. SIESS: Who is that character with the plus signs?

MR. CLAUSS: The plus sign is the experimental data. That's the actual measured response.

MR. SIESS: Okay.

MR. CLAUSS: And these are the -- I don't have identifiers on here -- and I apologize for that. But in the report, you can find them. But each different line type represents different analyses by a different group.

So I guess the only point I was going to try to make here today was that --

MR. SIESS: I'm fascinated that they absolutely consistently underestimated the displacement of the range.

MR. SHEWMON: Now, that's after -- something's gone plastic. Was that after the rebars?

MR. SIESS: No, it's after the concrete cracked, is what it is. I'll be willing to bet you.

MR. EBERSOLE: You mean that verticle like is whether you lost the concrete tensile strength?

MR. SIESS: No, that's where this line yields --

MR. CLAUSS: This is clarifying yielding the steel.

MR. SIESS: That's where the lineman yielded.

MR. CLAUSS: What I think Chet has learned is, you don't ever see a jump in the curve, the measured curve corresponding to the loss of the concrete --

MR. SIESS: So they're all viewing at any spec[illegible] loss of displacement that was observed.

MR. CLAUSS: It's not too small.

I would like to defer this to Randy because he's done a lot of investigation as to what exactly happened.

MR. SIESS: No, that's all right. I'm just -- it's just that consistency.

MR. CLAUSS: You'll see that throughout the data. And what we've found, in fact, is that for the membrane response of the cylinder, a no tension model for the concrete seems to compare much better with experimental results than --

MR. SIESS: Well, obviously. That's the only

kind I would ever make. Heck, I did these kinds of --

MR. CLAUSS: Well, I wish we had the benefit of that wisdom a year earlier.

MR. SIESS: And did they actually assume concrete stiffening?

MR. WEATHERBY: Not on this. And for the radial displacements, the hoop strength of the concrete is where --

MR. SIESS: Hoop strength?

MR. WEATHERBY: The hoop strength. It's where the concrete cracks due to hoop stress.

MR. SIESS: Yeah, but you've got cracks at such close spacing that your stiffening between cracks is going to be negligible.

MR. CLAUSS: Well, apparently, you didn't need the results of the test.

MR. SIESS: Well, I've tested a lot of concrete, you know. It took the Los Alamos guys three years to be convinced of that.

MR. WEATHERBY: In all of these calculations, though, about 110 psi say that the concrete will not be buried in the hoop.

MR. SIESS: But they must have assumed some stiffening.

MR. WEATHERBY: No, not by -- by 110, even

the people that assumed that would --

MR. SIESS: Well, then how did we get that much difference?

MR. WEATHERBY: Well, the only thing I can think of is maybe the seismic bars aren't carrying as much load as anticipated.

MR. SIESS: Okay. The question of --

MR. CLAUSS: The afternoon is going to supply that.

MR. SIESS: That's an interesting subject, gentlemen.

MR. CLAUSS: Let me just kind of move towards the end of this discussion because Randy is going to be talking about the analysis in a lot more detail in a minute here. And we have a pretty full schedule this afternoon.

MR. SIESS: That next figure, though, is even more exciting.

MR. CLAUSS: With the vertical displacements, yeah.

MR. SIESS: The vertical displacements.

MR. CLAUSS: Again, let me just defer because I think Randy is going to do that difference between the behavior and (inaudible) direction as opposed to hoop directions.

So let me just kind of wrap up this discussion of

the group effort. A lot of this Jim's already mentioned. After the test, the first thing we did was try to get the data to the various analysts as soon as possible, and that went out in early August, essentially about a week after the test. The testing analysis was featured in SMiRT, got a lot of the attention, generated a lot of interest at that time.

And then as Jim said, we had the entire group here, actually about 30 people in all in early November. And at that time, all these people who that did the analysis were allowed to go out to the test site and inspect the model in detail. We also had essentially two and a half days of in-depth discussions on the differences in the modeling between the various groups and exactly what improvement needed to be made to improve the comparison between analysis and experiment.

There was also some discussion of future plants and what the various groups were going to do. And I believe there were six of the ten groups plan to do additional analyses. And of those six, I believe at least five of them are planning to do what we would call local analyses to try to evaluate this interaction between the studs, liner, and the concrete and to see if they can understand exactly what the limit mechanism was in the model.

We also will -- well, we agreed to produce a second report, essentially the same type of format as the

pre-test report except this report will document comparisons between analysis and experiment and also document any post-test analysis that were done that lead to additional understanding of the model behavior.

And right now, we're kind of shooting for a target of like June 1st for the draft of that report. That may be optimistic because some of our post-test inspections will be done and we will keep us from having to -- occur quite as fast as we might have liked within 90-day information before they can complete their analyses. So it may be later than June.

The next thing I have in there is just sort of a summary of that meeting and you may put that out and you can read that if you're interested. And I guess these are just kind of my obvious conclusions.

There are two things that we're trying to do with the analysis as far as comparisons experiment. One thing is, we want to know how well we can calculate the response. We want to know if we can track displacements, the strains.

Then the second thing that we want to know is, can we interpret failure? And they're really separate issues. We use the calculated response parameters in a failure (inaudible) to interpret failure. And the calculations don't know about that process.

So the first thing I want to make sure we can do

is do the calculations. And my conclusion based on what this group has done in comparison to the experiment, so that would essentially be a good reasonable (inaudible) of predicting global measures of containment response. The difficulty is, the thing that we're working on right now is the localized behavior, behavior of run penetrations, and (inaudible) studs and things like that. And that's something that we need a lot more work on.

MR. EBERSOLE: What you're telling me, that you're concerned with the color keys of design, the fine structure of design is where it is, the fine structure such as run penetrations?

MR. CLAUSS: That's right. If we want to get a better resolution in our analytical capabilities, that's what we need to look at, is all part of the detailed design.

MR. EBERSOLE: In my view, that's true of dynamic systems. It's all in the fine structure.

MR. CLAUSS: That's true.

MR. BENDER: Not clearly to really need to know. If all you're trying to do is find out at what stage you can -- what pressure you can go up to before something fails, if you're going to design for controlled failure, then it may be important to know these things. Somebody needs to make the case for controlled failure in order to --

MR. SIESS: Control nonfailure.

MR. BENDER: What?

MR. SIESS: You said you had the figures on their predictions of no failure?

MR. BENDER: For control --

MR. CLAUSS: Yeah, let me get those.

MR. SIESS: Yeah, I would like to see them.

MR. BENDER: I don't know what that means.

MR. SIESS: I wish we could just --

Jim, can't you convince them to take Item 3 off of there. I hate to think that the NRC spent more than $2 for that.

MR. CLAUSS: What's that again?

MR. SIESS: I would hate to think that the NRC spent more than $2 of our hard-earned money to prove that the concrete is not perfectly bonded.

MR. CLAUSS: Well, first you know it's not perfectly bonded, but it's a question of what assumptions you can make and analysis, an analysis still reasonably tracked. And this is -- you know, as a matter of fact, this is an assumption that most people make in their analysis before they tell us.

MR. SIESS: It's ridiculous. That's because they're analysts. That's what I asked you. They don't know anything about the behavior of concrete and reinforced concrete and steel. And if they did, they would never make

that kind of an assumption.

And I can show them dozens of tests that show that that --

MR. CLAUSS: But can you show them how to model studs and --

MR. SIESS: I'm not interested in how to model it. I'm interested in the behavior. I think if they model the behavior, they shouldn't be making the analyses. Just because they don't know how to model it is no reason for making an absurb assumption. That's solving the easy problem because you can't solve the difficult one. Modeling something that's not true is no help to anybody.

MR. EBERSOLE: Would you agree to change that last sentence to say, "It is relative to assume that it is always smart to try to bond the concrete?"

MR. SIESS: There is a bond between concrete and a plate, but it's not very large and it's very easily broken. And to assume it for something that's going to go up to crack and go to these things, once a crack forms, the bond stress immediately adjacent to the crack is infinite. The concrete is moved and if the steel can't move the same amount of the concrete, the bond is infinite. So it breaks. Dale (inaudible) pointed this out in 1917.

MR. WARD: Well, still --

MR. SHEWMON: You had a point that you wanted

to read there.

MR. WARD: -- I mean, that's a valid lesson from this experiment. And if the particular designers and analysts finally have learned it, that's great.

MR. SIESS: I don't know whether they've learned it.

MR. WARD: I would say it's very --

MR. BENDER: We're just starting the application a little bit. For the purpose that it has been used for in the past it was probably all right. But at this -- for analyzing this failure just doesn't fit because the failure is involved -- involved something where the liner and the concrete do not work.

MR. MARK: Chet, would you enlighten me. What are the studs for?

MR. SIESS: The studs are to prevent buckling under temperature loading.

MR. MARK: Okay. So they do -- are needed for some purpose?

MR. SIESS: No. I'm not sure that the --

MR. MARK: If they were left out --

MR. SHEWMON: I think he wants to answer one of your earlier questions about what they said is reasonable upper limit.

MR. SIESS: Okay.

MR. CLAUSS: We asked them to give us a value at which they had high confidence and low probability (inaudible), which is the number you gave me.

MR. SIESS: Right.

MR. CLAUSS: The estimates ranged from -- only seven people were willing to respond to that, first of all, seven of the ten. Those values ranged from 100 psig to 160. I will say that only one was above the actual failure pressure. The actual amounts were 100 -- 100 twice, basically -- 105, 127, 135, 138, and then the 160.

MR. HORSCHEL: It is in the summary of the report, but it's not in tabular form.

MR. SIESS: Yeah, I just missed it.

MR. BENDER: Who predicted 160?

MR. CLAUSS: GRS from Germany.

MR. BENDER: What was their range for failure?

MR. CLAUSS: They're range for failure was 167 to 189. So they had a lot of confidence in their analysis that probably wasn't justified.

MR. SIESS: Yeah, they're a prediction of high confidence.

Let's see. You had 120, you said, to 160?

MR. CLAUSS: 100, 100, 105, 127, 135, 138, and 160.

MR. SIESS: And the range on failure loads went from a low of 128 to a high of 190, I guess. It's a little smaller range.

MR. WARD: What do you include --

MR. SIESS: They did a lot better on that.

MR. CLAUSS: Well, that's a little easier number. It's much easier to predict what the containment will not fail.

MR. SIESS: Much more useful number.

MR. CLAUSS: But I think that's one of the things that maybe I should have made a point about, is that when we do analysis -- and our objective of the analysis is actually to address several concerns. And one of them is the concerns you-all share, which seems to be when the containment will not fail. You can address that concern with fairly simple analysis method. And I think we're -- state-of-the-art is there right now. That leaves a lot of extra work that needs to be done to assess that particular number when a containment will not --

MR. SIESS: After this test?

MR. CLAUSS: After this test, right. There is a lot of uncertainty, and I'm not sure we'll ever be able to resolve it to determine a problem in predicting actual failure. Now, I realize you don't believe that's important, but there are people out there that are asking us for that

number. So we're trying to do --

MR. SIESS: Who's asking you for that? Is NRR asking for that?

MR. CLAUSS: I'll defer to Jim on that one.

MR. SIESS: Is NRR asking for actual failure loads?

MR. COSTELLO: It's hard telling what NRR is anymore.

It is -- in response to your question, the generic issue and all that business is all rolled in now to the research office. But the people who are looking at the severe accident applicability question are into that, yes.

MR. SIESS: Well, some of the PRA people would like to know a number like that. And some of the PRA people, I thought at one time, were convinced that what they would really like to have would be a probability of leakage as a function of pressure and a range of uncertainty.

You know, [illegible]ally they didn't assume anything about leakage. They assumed that they didn't leak at all and the pressure just went up. Then they began to realize that it might leak at lower pressures if the pressures wouldn't get that high. And I don't think they're ever going to get either one right.

MR. COSTELLO: Well, I think -- my own perception is that there is a little more modesty in the

demand for what one has to know to make to be useful for these PRAs and risk-based assessments. But the fact of the matter is that in the regulatory, you know, sense, severe accident considerations are treated on risk, on risk based in their models.

Now, I think people, as they are being more modest and all, they feel they have to know about containment failure models to come up with an attractable method for regulatory purposes. But there is also something about the failure mode, knowing the failure mode that enhances your confidence about your lower bound prediction; that is, if you're working and you're not -- if there's a significant possibility of a failure mode racking around there that you never accounted for, you, by definition, don't have the lower bound.

MR. WARD: But look -- yeah, but look at -- Chet, you know, I really disagree with you on the importance of knowing failure loads and failure modes. I mean, I think that the move toward regulating the risk to the extent that it's possible and you've got the basis for doing it is good. And I think you're sort of taking the approach that you accuse some of these analysts of things in the forum in that if you can't provide the hard answers, you're going to give me the easy answer. And, you know, that's great. But --

MR. SIESS: No, let me put it differently.

What have we learned from the two tests so far, the steel test and the concrete test? We've learned a lot about failure modes. And learning about failure modes is important.

What have we learned about failure loads? We've learned that failure loads depend on details. Okay?

Now, if somebody tells me I've got to know more about failure loads, we're into a research project of almost infinite magnitude. Because there's about 40 different kinds of containments out there. And even if I take two presumably identical, they're going to differ in details. So I think it's a hopeless effort to try to pin down loads for ultimate structural failure with any degree of confidence that will do anybody any good. We would have to test too damn many things. Modes are important. We learned a lot about modes.

MR. COSTELLO: If you'll bear with me, I think you'll be here this afternoon if we have a chance to have Mr. Clauss come back in and talk about the application of what we've learned from the steel containment as to Sequoyah.

But I think we have a middle ground here that is useful and does take account of what one heard about and what procedures are important. I believe within a year or two, we'll be in the same status with regard to concrete

containments.

You've got particularly Dr. (inaudible) has titled that. There are churning hypotheses about the possibility of local failures.

Again, to say that you're sure something won't fail, you have to be pretty sure that you won't have a local liner failure, and you have to know something about that. I think we're on the road of learning that.

MR. SIESS: Yeah. But if I look at accident management and I've got a containment sitting there and the pressure is building up, and I've got to make a decision and I've got some way of venting it, cool water or any other way. And if I believe that I don't want it to rupture and scare the hell out of people, weather or not it hurts them, I want some idea of when it won't fail. I don't want to wait until 150 or 140 on this one. I'm pretty confident at 120 or 125 it's not going to fail structurally. It may leak. It may leak from the beginning if the valve was open. And everybody that's talking accident management with venting filter, filter vents are talking about one and a half design pressure on most as a maximum. Some of them are talking one design pressure.

I think the Swedes are going to one and a half, the French one, one and a half, the Germans somewhere in that neighborhood. And I'm sure they've got plenty of

confidence that it's not going to rupture at that point. But I'm not going to sit by and wait for that thing to go. I don't want to get anywhere close to that.

I remember back when we first started talking about instrumentation to follow the course of an accident -- what is that? Reg Guide 1.97. We learned that the gauges to measure containment pressure went up to SIT and that's all. They had no way of knowing what the pressure was once it got above -- 15 percent above the design pressure. And we said,

"Yeah, wouldn't you like to know what the pressure is in the containment up to two or three times that?" Why? So we can do something before it went . . .

But my main concern is that if we really think we have to know what the containment failure load is, it's just -- what we've learned from these tests is, is details.

Now, how do you get a generic answer?

MR. COSTELLO: Well, my only response -- and I don't believe it's a generic answer. For those plants, we brought fact-specific detail. Not only the details, but even gross difference in the plant and design for that same type of containment. The classic example is Sequoyah versus WATS bar. The same design pressure, great difference is the plate barriers because of perceptions of economics.

The realization of economics, I guess. But the

failure pressure (inaudibles) are different.

MR. SIESS: Not necessarily. The crack might occur where the plates are the same thickness. You might never get up to the upper part there with it.

MR. SHEWMON: Well, Jim, one of the pieces of paper that I can't lay my hands on but I must have in my room that I've not brought out was something where some member of the Staff, perhaps you, had listed in order five factors, all of which were multiplied to to get --

MR. SIESS: That was (inaudible).

MR. SHEWMON: Roughly how much more than the design pressure one was comfortable at. And the least from those numbers, they all came out about two for steel, concrete --

MR. CLAUSS: Two and a half is closer.

MR. SHEWMON: Well, Dave, they were over two. So in a sense, that's going back to what Chet was saying again, isn't it, that --

MR. COSTELLO: That's what Chet was saying. And people, a couple of times, five years ago attempted to do such a calculation. And it seems that the outcome of these experiments would tend to verify those estimates which were based on essentially estimating the margins in different steps to the design, which is not --

MR. SIESS: What he called a confident lower

bound estimate of functional capability. That meant that he had fairly high confidence that there wouldn't be any rips or tears. I asked him for that. I knew, you know, we were getting these two or three times and I says, "What's -- where's it all coming from?" And he wrote this little thing up.

And I think that confident lower-bound estimate of functional capability is what we want. Now, it depends on how much confidence. As I said before, if you take 115 percent of design pressure, your confidence is fairly high. Right? You tested it to that. You started going up first yield. You may be not quite as confident, but you're still fairly confident that at first yield somewhere in this thing it's not going to do it. General yield, that's what we looked at on the Sequoyah years ago when we were playing around with this thing.

Not quite the same level of confidence, but still fairly high. But the closer you get to this ultimate --

MR. COSTELLO: Do you want to stop a while?

MR. SIESS: -- and we got two tests that tell us that we haven't got any confidence at that level.

Look, I think that concludes the presentation on the tests.

Let me make a couple of comments. I think that this whole program -- and I'm talking now particularly about

the concrete program -- has been carried out in an absolutely superb manner. I can't think of anything that you could have done better or different on it. I think the instrumentation, I think the way you handled the pre-test predictions and got the standard plots and all of that is a tremendous help to everybody. You did a beautiful job all the way through. And I think Sandia is to be congratulated.

MR. CLAUSS: First, I do'nt think I'm quite finished with concrete model. I think --

MR. COSTELLO: Well, I think you had a -- I'll ask this Committe's judgment on this. I think -- we have two more topics on -- two more presentations which speak to your agenda, one of which is was Dr. Weatherby, who is going to outline the differences which he found came from different state-of-the-art concrete modeling techniques, and then Dan Horschel, who is going to give you an update on our interactions from CGEB on their proposed model test.

MR. SIESS: Okay. We're down to the second -- through the bullet over here?

MR. COSTELLO: Yes, sir.

MR. SIESS: Okay. That's a good place to stop. Let's go to lunch and try to be back here by 1:40. Okay?

(The luncheon recess was taken at 12:40 p.m., to reconvene at 1:40 p.m.)

AFTERNOON SESSION

FRIDAY, JANUARY 22, 1988

MR. COSTELLO: This afternoon, we will attempt to try and finish off the scheduled morning presentation.

MR. COSTELLO: The first presentation is going to be by Randy Weatherby from the Applied Mechanics Division, Sandia laboratory. Dr. Weatherby is going to discuss two things with you, one of which, first is the work that he did before the test in the nature of what was scoping calculation, and also to look at differences among axisymmetric modeling technicians which are -- have some current vogue to model reinforced concrete.

Again, the calculations were of the nature in which one does an axisymmetric calculation, predicts strains and displacement, and then attempts to infer from those predicted measurements the behavior of the containment to estimate whether it might fail.

Then finally, he's going to go over with you some initial post-test calculations attempting to look at strain concentration, the (inaudible) strain concentration that one could expect in a location of failure.

Dr. Weatherby?

DISCUSSION OF THE CONCRETE MODEL TEST (continued)
Pretest Analysis and Comparison With Test Results

MR. WEATHERBY: As Jim mentioned, I'll just be discussing calculations that were conducted at Sandia. I'll attempt to describe what the other organizations did in regards to analyzing the 1:6-scale model.

The topics of discussion here will be, first of all, pretest analyses that we've conducted here at Sandia, comparisons of predictions and results, post-test analysis of the piping penetration where we got the large tears you saw this morning, and then a little discussion on future work. And we'll start off with the pretest analyses.

The first thing we did in this project is just look at the section of the cylinder wall during the midheight and do a very simple analysis and get displacements, strains in the liner, reinforcement, concrete as a function of pressure for the midsection of the containment. This just served as more of a bench mark for the finite element calculations and it could give us an idea of what we're looking at.

Then we moved into doing some axisymmetric finite elements analyses of the containment structure, using shell elements. We did a few using continuum elements.

Finally, we did an analysis of Equipment Hatch A, using 3-D shell elements.

Primarily what I'm going to be talking to you today about is the second item here, the axisymmetric finite element using shell elements as far as the response predictions go.

As far as modeling the concrete is concerned, we really looked into two or three different approaches. And the first approach here, this just shows the uniaxial stress-strain curves used to stimulate the concrete is one approach. Here what we have is, we run one analysis where we use an elastic approach for the plastic model for the end points at that time, the central yield strengths of the concrete for tensile fracture strength of the concrete and correctional strength of the concrete. And then we have another calculation where the tensile strength of the concrete seems to be about zero.

What I did was I made two separate analyses of these two different stress strain curves and then pieced them together to obtain the predicted response of the model. So that's Model Type 1. I'll refer to that as a --

MR. SIESS: Excuse me. Don't go too fast on that. I'm still trying to figure it out.

I'm trying to get oriented to the stress-strain curve the way I look at it. Okay.

MR. EBERSOLE: Let me ask you a question that will reflect my ignorance. In looking at this, do you look

at the deformations on the concrete reinforcing rods and take account of the compressive loads applied as you try to stretch the steel?

MR. WEATHERBY: Yes. And the way it models it, the way it's decomposed is, you just add the steel stiffness. You have separate models for the steel and for the concrete. This is what I'm talking about here is just the model for the concrete.

MR. SIESS: You actually -- when you apply that element, your elements have a separate set of properties for the steel and the concrete?

MR. WEATHERBY: Yes, sir.

MR. SIESS: And the steel is in there as layers?

MR. WEATHERBY: Right. It's at appropriate location to properly model the flexoral stiffness of the wall.

MR. SIESS: And what about a bond between them?

MR. WEATHERBY: Okay. In the axisymmetric calculations, it really doesn't -- well, I won't say it doesn't matter whether you assume that there is a perfect bond or not. The only location where you would expect a difference would be at the basemat and cylinder wall junction. Otherwise, the liner is going to be forced to

follow the displacement or the motion of the wall.

MR. SIESS: The difference between the zero and the tensile strength case, those are the ones you got plotted later?

MR. WEATHERBY: Right. Maybe it will be clearer whenever I --

MR. SIESS: Okay. I'm just trying to figure it, yeah.

Has anybody made any test where they simply took a prism of concrete and ran one bar down the middle of it and pulled the two ends of the bar to get the effective stiffness of that element?

MR. WEATHERBY: Not as part of this test. I know that I've seen results from tests like that elsewhere but not for this test.

MR. SIESS: Okay.

MR. WEATHERBY: Really, in using the -- okay. When I use this approach or this model for the concrete, what I was really trying to do was simulate this type of behavior. And we ran a set of calculations assuming this type of behavior for the concrete, and in this case, what I have is, I have a descending branch on a stress-strain curve for the concrete. And the reason in the tension regime and the reason for using this approach is, if you take, let's say, a panel, a reinforced concrete panel and pull on it,

you calculate the stresses in the steel and you calculate the total amount of load carried by the steel. Then you can back calculate from that a stress -- equivalent stress-strain curve for the concrete once you subtracted out the steel properly. And what it looks like in the panel is that the effect of the concrete slowly decreases until you get to about the yield strain of the reinforcement bars. Now, this decaying may be --

MR. SIESS: This is the concrete between cracks?

MR. WEATHERBY: Right.

MR. SIESS: Which is a function of bond and the spacing and all of these characteristics.

MR. WEATHERBY: Right.

MR. SIESS: Okay.

And you've got some curves later on and you characterize them by no-tension model, cracking model, and elastic-plastic model.

MR. WEATHERBY: Right.

MR. SIESS: Could you relate those to the three curves?

MR. WEATHERBY: This is what I refer to as the cracking model.

MR. SIESS: Okay. And the no-tention model is just that and the elastic-plastic was the one 500 psi

tension.

MR. WEATHERBY: The elastic-plastic model consists of piecing two curves together. What I did was I used this model until the strains in the steel got to about half of their yield. And then I said, "Well, the response is going to be closer."

MR. SIESS: Okay.

MR. WEATHERBY: This type of behavior like that.

MR. SIESS: Now, the compression side, is this of any importance at all?

MR. WEATHERBY: There is some compression pumped. The highest compression stress is right around the cylinder basemat junction.

MR. SIESS: Because that's very unrealistic for compression.

MR. WEATHERBY: Right.

Would you take this curve as being more realistic of compression?

MR. SIESS: Yeah. I think it drops off a little fast, but 7,000 pound concrete, maybe not.

Is that first line a straight line? It looks curved on mine. The one from the origin up --

MR. WEATHERBY: This?

MR. SIESS: Yes.

MR. WEATHERBY: It's virtually a straight line.

MR. SIESS: It's supposed to be a straight line.

MR. WEATHERBY: I think it's a straight line there.

MR. SIESS: It's not on the plot.

Is it convenient in your analysis to use three straight lines?

MR. WEATHERBY: Yes.

MR. SIESS: If I use a compressive stress-strain curve, I can express out of the break a little bit more, so it looks like a lot more like the real curve.

MR. WEATHERBY: Well, the way it is, is that these lines linear curve in the model. So you have a --

MR. SIESS: You wouldn't be better off with a continuous curve?

MR. WEATHERBY: Sir?

MR. SIESS: It wouldn't be just as easy to use a continuous expression, just a function of strength?

MR. WEATHERBY: It would be except that's not the way the code was written.

MR. SIESS: Oh. Maybe that's why I could use mine on a (inaudible) 57.

MR. WEATHERBY: Okay. And associated with

the data, the model, is the failure criteria. And these are really the three failure criteria used whenever the model is assumed to fail, whenever the stress in the reinforcing steel exceeds the open strength of the reinforcing bars, or the equivalent strain in the liner exceeds about 15 percent, which is the strain at maximum load, the uniaxial tension stress in the liner material. And the third failure criteria is whenever the transverse shear -- that's the shear that acts across the reinforcement at any location, exceeds the shear strength that was -- comes from a paper based on some Japanese test results. We also compared it to the shear friction model.

MR. SIESS: Is there any reason why you expressed the first one in terms of stress and the second one in terms of strain?

MR. WEATHERBY: No -- well, there is a reason. This 99 ksi came from a test on bars that had splices in them. The actual ultimate strength of this is about 105 or 106 ksi. But in tests of bars with splices, it turns out that the breaking strength, the average breaking strength is -- or rather a lower bound in the breaking strength is more like 99 ksi.

MR. SIESS: Because somebody told me that only about two-thirds of the bars failed in the splice.

MR. WEATHERBY: Right. Still the average of

all the results gave a lower value than tests on the bars without any splices. This is really a slight reduction.

MR. HORSCHEL: Actually, wouldn't that be the other way around, about one-third of the bars failed in the spliced and two-thirds of the bars failed on the bars.

MR. SIESS: But since the stress was completely uniform, I guess you have to take the one (inaudible). Okay.

Let's see. This is grade 60 steel and 90 ksi as specified in . . .

MR. WEATHERBY: I believe that's right.

MR. SIESS: The splice has always exceeded the minimum, yeah.

MR. EBERSOLE: Have you characterized this steel in the quantity context? What's it called?

MR. WEATHERBY: It's A-615 Grade 60.

MR. SIESS: And the rebar?

MR. WEATHERBY: And the liner is 8560.

MR. HORSCHEL: No, no, no.

MR. WEATHERBY: I'm sorry. That's the insert plate. And that's the usual material used in Grade --

MR. HORSCHEL: 414, Grade D.

MR. WEATHERBY: Grade D.

MR. SIESS: What's the yield point?

MR. WEATHERBY: Of the liner?

MR. SIESS: Yeah.

MR. WEATHERBY: About 50 -- a little over 50 ksi.

MR. HORSCHEL: And that's actual.

MR. WEATHERBY: That's actual. The minimum is around 32, I think. 516 is 32.

In all the analysis, in the equipment hatch analyses and all the axisymmetric calculations, my approach was to take these failure criteria and rigorously apply them to the loads and strength, calculate it under the element calculations. When you do that, these are more or less what you arrive at.

MR. SIESS: In your elastic-plastic you had two cases, one with zero tension and one with 500; which is this?

MR. WEATHERBY: Okay. What I did was, I pieced the two together, as I was trying to use two separate analyses to get the overall response. And I pieced the two together at about 114 psig. Below 114 psig, everywhere the concrete had this plateau curve of 500 psi's.

Then above 114 psig, I assume the concrete --

MR. SIESS: So essentially you dropped it there?

MR. WEATHERBY: Right.

MR. SIESS: Dropped it to zero there. Okay.

And since it failed at the bottom, it didn't make any difference?

MR. WEATHERBY: Right.

I interpreted the results to mean that a hedge formed near the basemat at about 170 psig. And this was a limit of load type of situation where increasing pressure just -- you don't even need an increase in pressure to get increasing displacement. When I used the second model, the cracking analysis, predicted failure pressure was 180 psig and the mode was just exceeding the ultimate hoop bars in the cylinder midsection.

MR. SIESS: And I assume the tension on the concrete would be zero at that stage?

MR. WEATHERBY: Right.

MR. SIESS: Both of those then would be no-tension in the concrete.

MR. WEATHERBY: Right.

MR. SIESS: When you got to the failure point.

MR. WEATHERBY: Well, right. By the time you get to 180, that's true.

MR. EBERSOLE: If it had not leaked at that pressure of 180 pounds, but you still retained it by some sort of a bladder concept, would it proceed on to a catastrophic failure concept?

MR. WEATHERBY: Well, if you precluded any type of leaks, then, yes. Well, the other possibility is that the equipment hatch would ovalize and you would have leakage through there. And if you precluded anything -- any sort of leak, then, sure, probably you would get a catastrophic -- or you would get a --

MR. EBERSOLE: A tear?

MR. WEATHERBY: Not necessarily at 180 psig ⌣ the liner. That calculation assumes a liner is intact.

As Dave pointed out earlier, if tears form the liner, then the liner seems to be affected at higher pressures.

Difficulties in failure prediction: These are really pretty obvious. The axisymmetric models neglected strain concentrations caused by both the penetrations and the liner anchorage system.

The 3-D equipment hatch model that I have, that was a three-dimensional model. It did gives us some of the strain concentrations around the equipment hatch, but it neglect the line anchorage system and slippage between the concrete wall and liner.

The reason for doing that isn't because -- really wasn't because I didn't think it was important. It was just -- this was just the first step. And it's all I had time to do.

MR. SIESS: For the first case you've got up there, you would have gotten the same answer for an axisymmetric model with no penetrations, wouldn't you?

MR. WEATHERBY: For this case here? Right.

MR. SIESS: Just a simple membrane analysis would have given you that --

MR. WEATHERBY: Of 180?

MR. SIESS: 180.

MR. WEATHERBY: You're right.

MR. SIESS: In fact, I think you did that, didn't you?

MR. WEATHERBY: Right. I compared the two.

MR. SIESS: In one of the earlier versions I saw it.

MR. WEATHERBY: Right.

The only thing you did -- the way I look at it is, if that doesn't apply, you shouldn't do an axisymmetric analysis. That just says that fortunately, the weak link is in the midsection.

MR. SIESS: Yeah.

MR. WEATHERBY: If the weak link had been somewhere else, then, that simple calculation might not have led to the same result as the full axisymmetric calculation.

MR. SIESS: What circumstances could you think of which would put the weak link somewhere else for

hoop stress?

MR. WEATHERBY: Well, for one thing, if you increased the hoop reinforcement, you know --

MR. SIESS: If you had designed it different.

MR. WEATHERBY: Designed it differently.

If there was some sort of a failure associated with --

MR. SIESS: If you varied the hoop stress either way from midheight, you could have gotten --

MR. WEATHERBY: Right. Any place with (cough) would have influenced the deformation of the model.

MR. SIESS: What's the practice in design? Obviously if you make an axisymmetric model, you'll find a lower hoop stress near the bottom and near the top. Right? Does anybody ever vary the reinforcement that way?

MR. WEATHERBY: I think the vertical bars certainly do change in density as --

MR. SIESS: What about the hoop bars?

MR. WEATHERBY: But the hoop bars, as far as I know, are usually constant.

MR. SIESS: Engineers don't generally like to design one horse shades.

MR. WEATHERBY: The third point that I want to emphasis is that even if you predicted strength concentrations, there is still some uncertainty in what

you -- you get strains in the liner. You can accurately calculate those. But at what point does the liner actually tear? There's not a really generally accepted theory for doing that. And plus you're complicating the issue by the fact that the studs are welded to the liner. The welding process itself may degrade the liner properties flat against the weld line.

MR. SIESS: The liner itself has welds in it?

MR. WEATHERBY: Even the liner itself has welds. Although interestingly enough, most of the failures -- well, they were away from the welds because of the studs.

MR. SIESS: But don't we have a decent theory of failure that divides the attention just for a plate like this?

MR. WEATHERBY: What generally -- I have a viewgraph that pertains to that. It's a common problem in metal-forming to be able to predict when tears will occur in plates under (inaudible) loading.

Generally what you see, you run tests where you vary the ratio of the major strain to the minor principle strain. Generally what you get are curves that look like this, so that the worst case is a case where one of the strains is zero.

MR. SIESS: Biaxial increases them?

MR. WEATHERBY: Right. But then it increases again.

MR. SIESS: Now, where are we here? What's the ratio here? I can't -- two or half? I don't know.

MR. WEATHERBY: Let's see. For this case -- in certain areas in the containment, you're at this location.

MR. SIESS: Oh, well, that's in the midheight three field. It's 2-to-1, but I can't read the scale. I don't know which way it's --

MR. WEATHERBY: Okay. This would be --

MR. SIESS: Where would 2-to-1 be?

MR. WEATHERBY: Well, of course, it's not exactly 2-to-1 because the reinforcement is at --

MR. SHEWMON: Where would 2-to-1 be?

MR. WEATHERBY: Where would 2-to-1 be? It would be a line of the slope of one-half right up here. What would that be? There is a 20 -- well, that's 1-to-1. I'm sorry. 2-to-1.

MR. HORSCHEL: You would go the other way.

MR. SIESS: Right.

MR. WEATHERBY: Well, no --yeah. So it would be up in here.

MR. SIESS: And, of course, you've got more reinforced (inaudible) than you have vertical center line,

contributes less.

MR. CLAUSS: Actually, you're pretty much in a (inaudible), in which you have a large plastic strain in the whole cylinder because the loop strains -- you yield hoop direction first, and loop strains go very large where your (inaudible) strains are still essentially elastic. So you have, say,
2 percent loop straining near the failure point for our field and .2 percent in (inaudible). (Inaudible), which is about the worst condition you can realize.

MR. SIESS: Okay.

MR. WEATHERBY: Yeah, because the uniaxial test is over here. It's basically the highest number on this chart.

MR. SIESS: Uniaxial is?

MR. WEATHERBY: A uniaxial test has a slope of minus a half on here because the strains in the loading direction are twice the strain in the direction where there's no stress in all directions.

MR. SIESS: Twice? I thought I saw a ratio of .5.

MR. WEATHERBY: Right, in the plastic range where --

MR. SIESS: Oh, in the plastic range. Okay. Count the volumes.

MR. CLAUSS: (Inaudible) to be measured at that material of interrupting 20 or 21 percent for this particular material in uniaxial test. In the strain condition, we have -- in the containment model, you're going to have much plastic floating capacity in the axial. I think it's like a factor of two, isn't it almost?

MR. SIESS: I believe 10 percent.

MR. CLAUSS: Ten percent, that's right. So one thing we noticed right away when we started doing these things at the test, this 15 percent criteria we used starting out is probably not good. It should be more like 10 percent.

MR. WEATHERBY: Right.

MR. SIESS: But the test says we can use two.

MR. CLAUSS: We measured strains higher than 2. I'm sure there was (inaudible) in the model they were higher than what we measured.

MR. SIESS: I used 2 as an average.

MR. CLAUSS: Yeah, right.

MR. WEATHERBY: 2 percent strain?

MR. SIESS: Yeah. That would predict the test.

MR. CLAUSS: For this particular combination of strain concentrate, that's true.

MR. WEATHERBY: That works very well.

MR. SIESS: Now, the question is, how much can we refine that?

MR. WEATHERBY: Can we always depend on that for any increases or any set of strain concentration?

MR. SIESS: What kind of test can we make to say it's different and how much different?

MR. WEATHERBY: I'm going to compare these models to some experimental results. We really have some interesting things that you can see. At least one of the plots on that, as you noted earlier, (inaudible) model.

And the reason I've included this in here is to show how much the properties are dominated by the steel. And in fact, in some cases that model hit the best results of any model attempted.

And all these calculations are just plotted for this axisymmetric (inaudible) using different material models for the concrete. We'll start off at the -- in the dome area.

MR. SIESS: Let's see. From that, the strains that will be average over seven inches. That was your --

MR. WEATHERBY: Right. The strength of the others showed seven inches.

MR. SIESS: Which is not too far from the cracked (inaudible).

MR. WEATHERBY: Well, if you -- the cracked facing would be even smaller, about maybe four and a half or five inches.

And what we have here is -- the green line here is the no-tension model for the concrete still in the model. But in the dome, of course, there is no compression so the concrete contributes nothing there.

This blue line is two elastic-plastic analyses pieced together here.

MR. SIESS: And that's where it cracked?

MR. WEATHERBY: 14 and 115. I forget exactly the pressure.

MR. SIESS: That's where the concrete yields?

MR. WEATHERBY: That's where I assumed it vanished.

MR. SIESS: Oh, that's where you assumed it vanished.

MR. WEATHERBY: And I base that on looking at the strain.

MR. SIESS: And it gets back on the other curve -- no, it doesn't get back on the no-tension curve.

MR. WEATHERBY: No, it doesn't because it's still assuming that there's still 10 psi yield strength there. And it's following some sort of yield surface. And it doesn't exactly match with the no-tension results. It's

closer to the no-tension results than the cracking model is.

MR. SIESS: Okay.

MR. WEATHERBY: Okay. The cracking model looks pretty good, but in this area, it appears that the real response in the real case, the load carried by the concrete drops off faster than what I've assumed.

MR. SIESS: What strain are you plotting that? What measured strain are you plotting?

MR. WEATHERBY: These are --

MR. SIESS: No. What measured strains are you plotting?

MR. WEATHERBY: I'm sorry.

MR. SIESS: Strain gage on where?

MR. WEATHERBY: It's in the inside vertical bar in the dome. It's about midway between springline and the apex.

MR. SIESS: And you don't know whether it's at a crack or between cracks?

MR. WEATHERBY: Right. There's no way of knowing that information.

MR. SIESS: If there's really no bond it doesn't make any difference.

MR. WEATHERBY: Right.

In some cases, I'll show you how to compare the output from several gages that are in similar locations that

leads to the same elevations. They're just located at different (inaudible) anglesaround the model.

MR. SIESS: What's the probability that the gages aren't cracked? Assuming that the cracks occur along the bars, are the gages always located between two cross-bars?

It would be logical since the bars are touching each other and you've only got four inches between them.

MR. HORSCHEL: The gages are positioned at different areas, you know, around meridional bars would need to be (inaudible), as you viewed it, left to right, front or back.

MR. SIESS: No, but I'm talking about, here's a bar and here are the bars -- now, here's the bar here and here's the bar as we (cough). Is it midway between those usually?

MR. HORSCHEL: Usually, yes. But one thing I would like to point out which may add to this discussion. And that is, in protecting our gages during construction, we actually plotted that whole thing in an apoxy. So that, to some degree, would destroy the bond between the gage and the concrete up in that area.

MR. SIESS: So it were midway between the two cross-bars and the cracks formed at the cross-bars you had the bond destroyed for two inches in the middle, it's not

likely to be much bond on that bar?

MR. HORSCHEL: Right.

MR. SIESS: Okay. Thank you.

MR. WEATHERBY: Okay. I guess -- I'm not sure that that would -- even if just one bond were debonded, then adjacent bonds would pick up the load there. I'm trying to think of --

MR. SIESS: What?

MR. WEATHERBY: What I'm trying to think of is, if it were divided at that location, would you always expect just to see the steel properties? Is that what --

MR. SIESS: Let's put it this way. Suppose the adjacent bar was not being bonded --

MR. WEATHERBY: Right.

MR. SIESS: It might have a smaller stray than the one you measured.

MR. WEATHERBY: Right.

MR. SIESS: Certainly not larger?

MR. WEATHERBY: Right.

MR. SIESS: What you're getting is the strained -- very typical strain at a crack. It's conceivable, between cracks the strain would be a little lower.

MR. WEATHERBY: Okay.

MR. SIESS: On the bar they couldn't have

them. Since you obviously can't know just what --

MR. HORSCHEL: One other thing that may play some factor in this discussion, is also, the cracking in the dome region is very small. We never noticed large cracks throughout the test. And after depressurizing the model and inspecting those cracks, they're very difficult to see. You almost have to wet the surface, they're that small. You don't have any large cracks in the (inaudible) in the wall.

MR. WEATHERBY: Probably the reason for that is that we're only at about half of the yield strength for the bar, so we're not getting a lot of cracked openings or cracks opening a whole lot.

This plot shows the radial displacement at the spring line. We can see that the no-tension model overpredicts the displacement somewhat, joins in at higher pressures like you would expect. And the cracking model seems to track the results pretty well. In this model, the elastic-plastic model isn't too bad, either.

MR. SIESS: It looks pretty good until you get up to the very large displacements.

MR. WEATHERBY: It should be. Here's what we see for the radial displacement at the cylinder midheight. And this was one of the ones that we were talking about earlier. You do see an upturn in the data sooner than you would expect based on even the no-tension model where the

concrete carries nothing. And the only real explanation that I can think of for this is that the seismic bars don't pick up as much load as we're expecting them to. Because in this sort of calculation, what is assumed is that the size of the straining seismic bars will be equal to one-half of the vertical strain and the hoop strain. It's just a simple more circle transformation.

That assumes that you have perfect bond and that things don't slip relative to one another. And it's possible that we're getting some slippage in the seismic bars and that they're not picking up as much load as we anticipated.

MR. SIESS: Are are familiar with some of the stuff that's been done talking about kinking of bars across a crack at an angle?

MR. WEATHERBY: I've seen a little bit of it.

MR. SIESS: Dr. Sozen commented on this to you at all? He did quite a bit of work on that.

MR. WEATHERBY: He didn't comment specifically with relations to --

MR. SIESS: You had two kinds of assumptions you can make about the deformation of bar across the crack. And they lead to quite different results. And I think that's what your problem is there.

MR. WEATHERBY: Okay.

MR. SIESS: There's no assumption that you can make -- simple assumption you can make that is correct.

MR. WEATHERBY: It's a no-win situation is that it doesn't decrease the --

MR. SIESS: I forget which way it goes now. He did a lot of work on it in slabs reinforced group, as far as in different directions. And he went into that with (inaudible). Do you have the report? I don't think it will help you very much, though. He just might have found it somehow for you.

MR. WEATHERBY: Well, here's where things that, I think, get interesting.

This is really the area where all of the other calculations have the most difficulty; and that is, in predicting vertical displacements in the cylinder, predicting vertical strains in the rebar. And what you see is, if you look at the cracking model, this cracking model has directional properties associated with it. In other words, the concrete could crack due to hoop stresses but still carry load in the vertical direction.

While you look at these results, the cracking model says all the way out here to 118 or so. There shouldn't be any cracks in that stretch. And, yet, if you look at the data, they really closely track this no-tension model and the elastic-plastic model which doesn't have the

directionality associated with it that this cracking model does.

And so I looked at that and I said "Well, what happens if we look at all the gages that are in similar locations?" So I made a plot of those. There are about four different gages here that are plotted.

What you see is that even down here at real low pressures, that all these gages track this no-tension model. Even though the stresses that you would predict from any kind of hand calculation, (inaudible) calculation, would say that you had exceeded the tensile strength of the concrete.

MR. SIESS: Excuse me a minute. Was this cracked in the structural integrity test?

MR. WEATHERBY: Not in the vertical direction.

MR. SIESS: How do you know?

MR. WEATHERBY: Okay. Let's put it this way. It shouldn't have been. That's what I'm going to show on the next slide.

MR. SIESS: When somebody tells me something is not cracked, I want to know how they know.

MR. CLAUSS: No visible cracks.

MR. SIESS: Well, that doesn't mean it's not cracked.

MR. CLAUSS: I know. That's why I'm making

the statement. But we couldn't see --

MR. SIESS: The easiest exploration for that curve is that it was cracked.

MR. CLAUSS: Sure. That's right.

MR. SIESS: Now, whether you can see them or not --

MR. CLAUSS: Yeah.

MR. SIESS: Go ahead.

MR. WEATHERBY: So now we're back to the structural integrity test.

MR. SIESS: Same thing.

MR. WEATHERBY: It's the same thing. Even way down here -- well, by the time you're up to 28 psi, pretty much on this curve.

MR. EBERSOLE: Chet, are you saying that no-tension curve is actually cracked?

MR. SIESS: Well, if you will look at the structural integrity tests, there were obviously some places where there were no cracks up to about 20, 25 psi. There were some that looked like they were cracked at ten, the squares. But once they made that test, you see, that's what these figures show, is what I'm saying; it got cracked in the structural integrity test, period.

Never assume concrete is uncracked.

MR. WEATHERBY: I had a little --

MR. SIESS: Unless that's a conservative assumption.

MR. WEATHERBY: I was wondering. This is just some -- an idea I had that possibly what you have is you have reinforcements running in both directions. Then you have stress applying in two directions. And when you develop two cracks, which can develop down here in this range -- let me say vertical cracks due to hoop stresses -- they can form along these vertical bars and cause the bars to debond from the concrete so that the concrete doesn't add any stiffness in the vertical direction of this (inaudible) as well.

MR. SIESS: I think that's an excellent conclusion. And I can cite you other tests that would be (inaudible) improvement.

MR. WEATHERBY: That would prove this?

MR. SIESS: I don't know whether you're familiar with footing design, but we always permitted a lower bond stress in the footings, two-way footings because the cracks always formed along the bonds and reduced the bond strength along the bar. And I've tested slabs that I can go ahead and check my reinforcement spacing by just looking at the cracks. And you can see the effects the same way. I think that probably is a good explanation of it.

MR. WEATHERBY: I was wondering if this would

show up in any of the SIT tests on the full-scale containments, or if :t's a function of the (inaudible) as well, even though they're No. 4 bars. And I wasn't sure if the No. 18 bar would be less likely to show that much sensitivity to cracking by the hoop stresses.

MR. SEISS: That would be very difficult. The (inaudible) would scale, but then your deformation size -- the opening in the crack -- if you've scaled you a method where the crack should open the same amount in relation to the deformation rights, the deformation rights on bars that related to bar diameter action scale, I think it's all linear.

MR. EBERSOLE: Do the cracks consistently form near the proximity of the rods?

MR. SIESS: Yes, at least for the ones that are on the outside bars.

MR. WEATHERBY: Right. In some cases, you see the cracks following the seismic reinforcements.

MR. SIESS: The hoop bars -- the seismic bars are on the outside.

MR. EBERSOLE: That's the bracing, 45 degree. Right?

MR. WEATHERBY: 45 degrees.

MR. SIESS: I don't know whether there's been any -- in the full-scale SIT tests, I don't know whether

they've measured any vertical displacements. They've measured vertical displacement at the peak of the dome, but that's not a good measure to do (inaudible).

MR. WEATHERBY: You need to stretch the cylinder part.

MR. SIESS: I think that's a good explanation for why -- I think it's obvious the cracks are there. And I think you've got a good reason why.

MR. WEATHERBY: But then what that raises is the question of -- really, you need to account for that in the modeling. And to me, this idea of directional cracking -- when you have crack initiators like reinforcements isn't a very good approach to the problem. I don't know how you incorporated this affect into the model unless you just (cough) for the concrete.

MR. SIESS: Just pay no attention to the concrete in the beginning.

MR. WEATHERBY: Well, I have another plot that suggests that that doesn't work in certain locations.

MR. SIESS: It all depends on what we're looking for and what's important.

MR. WEATHERBY: That's what the next slide shows.

This is the vertical displacement of the basemat at the cylinder wall junction. And this shows the

calculated displacement is a function of pressure whenever you neglect the stiffness of the concrete.

These two other results show what happens when you leave something in for the concrete.

MR. SIESS: Now, this is bathing in the basemat?

MR. WEATHERBY: Right.

MR. SIESS: And it's very thick basemat.

MR. WEATHERBY: Very thick.

MR. SIESS: And it probably hasn't got much cracking due to rebar.

MR. WEATHERBY: Right.

And this is really important. If you think that this is what -- how the basemat curls up, then that's really going to affect the stresses and strains and the liner at the intersection of the cylinder wall with the basemat.

And I think this is one reason why a lot of people were predicting failure in the -- at that juncture. Not because they used the no-tension curve, but because they used their cracking model where they calculated the stresses in the concrete. And once they exceeded the strength of the concrete, they just threw away the concrete.

And so what you see in those curves, you see come along, you know, fairly a line like this. And suddenly you will see a big jump in displacement.

MR. SIESS: The strain in the line, are you talking about is simply a geometric strain due to the limitation?

MR. WEATHERBY: Right.

MR. SIESS: Because this lifts up, that's moving out, it increases that angle?

MR. WEATHERBY: Right. It increases the shear force and everything in -- or it increases the bending moment at that location as well.

MR. SIESS: And you're not for sure?

MR. WEATHERBY: Not for sure.

So that's -- but these models turned out to be pretty stiff. So the comparison is interesting, but I'm not sure what the answer is to resolve the problem.

MR. SIESS: The liner down at that junction, if it doesn't have studs welded to it -- it probably doesn't make any difference. You get it to yield its yield. It's really 15 percent -- it's probably 20 percent there. Not much stress in the other direction.

MR. WEATHERBY: That's right.

MR. SIESS: Really, that's (inaudible) as long as you don't put a curve on it.

MR. WEATHERBY: And the last plot I've got shows the (inaudible) inside vertical bars just above the cylinder basemat junction. This just shows that if you use

the cracking model here, it tracks the flexoral -- the strain resulting from the pressure at that location pretty well. And then the no-tension model is considerably larger because you have more rotation due to the less stiff -- the lower stiffness of the slab on the basemat.

MR. SIESS: But that's a nonlocal effect. That's an effect coming in from the basemat.

MR. WEATHERBY: Right.

MR. SIESS: Okay.

MR. WEATHERBY: And in summary, at most locations, the response is dominated by the steel. The only exceptions would be when the compressive side of the basemat and the basemat itself seems to -- the stiffness of the basemat seems to be strongly dependent on the concrete.

But elsewhere, the concrete contributes little to the stiffness of the model, even in extreme low pressures.

In the cylinder midsection, the vertical load carried by the concrete disappears much sooner than you would expect, just based on the -- without considering the effect of the rebar's cracked (inaudible) .

And the basemat uplift again is significantly over-predicted when you neglect the tensile strength of the concrete.

Unless there are any other questions, I'll move on to another section.

This is the analysis of the piping penetration where we got the large tear in this location right here.

And what I wanted to look at was what would happen if you ignore the anchorage system and ask, "What are the strain concentrations in the liner that result from the transition from a thin lining material to a thicker insert plate." And by looking at this example, that will tell us whether the studs were the primary cause of the liner tear or whether the studs are just an initiator for a crack. So you could say that maybe the straw that broke the camel's back.

So what I did was I constructed just a plane stress finite element model that has both the thickened and and the thinner region model. And in the first cut, I neglected the anchoring system. And I assume there is no friction between the concrete and the liner.

MR. SIESS: What do you mean by "slip freely"? You don't assume the studs are not there?

MR. WEATHERBY: Right.

MR. SIESS: Assume the studs are there?

MR. WEATHERBY: I assume the studs aren't there. And what I'm asking is, do you get enough strain concentration even without the studs?

MR. SIESS: Okay. So there's no studs in this case?

MR. WEATHERBY: Right.

MR. SIESS: And no bond.

MR. WEATHERBY: No bond.

MR. SIESS: And no friction.

MR. WEATHERBY: No friction.

MR. SIESS: Okay.

MR. WEATHERBY: And then the boundary conditions are, you get away from the penetrations and you apply the membrane solution.

What this is is a plot of the relative slip between -- or at least the magnitude of the relative slip between the liner and the concrete wall. What you see is, effect, near this thickened plate, this region right here, has a thickness of 3/16ths of an inch while this region right here has a thickness of 1/16th of an inch. And so what the effect is, it's like you put a patch on an innertube, more or less, and with the innertube idea being that the liners, you like blow up a baloon up inside the cylinder expanding.

Now, right around here, these are where pipes pass through the wall. So I would say there is no slip along the piping capacitors here. And then there is the no horizontal slip that's specified along this boundary and no vertical slip specified along that boundary.

Now, it would be this slippage that would

produce -- that would cause loads to develop in the study, if they were there.

MR. SIESS: Wait a minute. Don't go away. I'm still trying to figure that one out.

M^r WEATHERBY: Okay.

MR. SIESS: At the boundaries, they are together?

MR. WEATHERBY: At the boundaries?

MR. SIESS: The concrete and the steel are -- moved together, have they not?

MR. WEATHERBY: Al ıg here. At least in the vertical direction, they moved together.

MR. SIESS: All those slip lines are positive.

MR. WEATHERBY: Right.

MR. SIESS: What does that mean?

MR. WEATHERBY: Well, first of all --

MR. SIESS: If you add them up, I start at one side over on the left. And when I get to Line C, I'm over at the left and moving over. I've jumped to the third line where it's uniform.

MR. WEATHERBY: Okay.

MR. SIESS: What does that mean? That means that -- which is moved relative to which 600 or something -- 600ths of an inch, or does it make any difference?

MR. WEATHERBY: By 600ths of an inch. Okay. That says that -- well, let me first of all make another comment. This is -- in this model, this point is fixed, every displacement is measured relative to this point.

MR. SIESS: Which point?

MR. WEATHERBY: Well, where these two intersect, would be the --

MR. SIESS: Okay. That's the fixed point.

MR. WEATHERBY: That's the fixed point on the structure. What I've done is I've plotted the magnitude of the slip effect so it has a vertical component, a horizontal component. But it's almost all horizontal, so you could interpret these all to be the horizontal component.

MR. SIESS: Now, go up from your fixed point. Aren't those all fixed points of the steel containment boundary they were the same, or did you, up that line? Now, move up the line from there.

MR. WEATHERBY: Right.

MR. CLAUSS: You're extending in the horizontal direction, but not in the vertical direction.

MR. SIESS: No, I'm just interested in the horizontal direction.

MR. WEATHERBY: Okay. You get a contour here; the contour is here because there is a vertical slip. I should have brought this, the horizontal slip. It

wouldn't have been as confusing.

In that case, you wouldn't see a contour change up here. All these would have zero displacement.

MR. SIESS: All right. Then as I move over, the concrete is moving to the left relative to the liner?

MR. WEATHERBY: Right.

MR. SIESS: Because the liner is stiffer and the concrete up in the free field is tending to move over.

MR. WEATHERBY: Right.

MR. SIESS: But by the time I get over to the left edge, they're the same again?

MR. WEATHERBY: Right. By the time you get over to the left edge, they're moving with the same displacement.

MR. SIESS: They're moving together?

MR. WEATHERBY: They're moving together.

MR. SIESS: So it doesn't have to go minus to get a net zero. The displacements -- we're talking about a slip -- okay.

I think if I drew (cough) integrated and a differentiated -- I drew the curve and integrated, that's what I should get, shouldn't I, or differentiate?

MR. WEATHERBY: Go ahead.

MR. SIESS: I think I understand.

MR. WEATHERBY: Okay. You do get the maximum

slip, of course, at the edge of this insert.

MR. SIESS: Would you trace out line E for me so I can see where it goes.

MR. WEATHERBY: Okay.

MR. SIESS: Just use your pointer. Does it just loop up to that top E and come back down?

MR. WEATHERBY: Right. It loops here.

MR. SIESS: Okay. Fine.

MR. WEATHERBY: And incidentally, these displacements are an estimate, complete with fail or stay. We have some stud tests, and I know the displacements wouldn't be the same if I included them down at the bottom, which I'm going to do next.

But still, at least it tells you that the slippage is enough to fill up significant loads in the studies.

MR. SIESS: A slip of .1 there on Line E means .1 movement?

MR. WEATHERBY: Right. That means the wall has moved over a tenth of an inch more than the one after that.

MR. SIESS: Measure back to this point. At that point, there should have been a theoretical one-tenth of an inch displacement.

MR. WEATHERBY: Relative displacement.

MR. SIESS: Between the two.

MR. WEATHERBY: Right.

MR. SIESS: If I had a whole layer, I could look down and I could see those (inaudible).

MR. WEATHERBY: Right.

MR. SIESS: Okay. Pretty close. That's what the stud has to resist?

MR. WEATHERBY: Right, it's the shearing displacement on the stud.

Well, without -- what do the strains look like? Are they of a sufficient magnitude to cause failure without any studs in the model?

So these -- I plotted (inaudible) plastic strains. For all intents and purposes, it's the hoop strain. And you get a maximum here at the corner of the insert plate there. It's this rounded edge. But the maximum is only 2.8 percent strain, which the strain in max load and uniaxial tension is 15 percent. And even if you take a reduction for a load (inaudible), you can't really suspect failure in this location.

MR. SIESS: What's the direction of principal strains?

MR. WEATHERBY: It's roughly pretty much the hoop direction. And I'll show you the next slide which will give you a little bit more information.

It doesn't tell you the direction.

MR. SIESS: I see.

MR. WEATHERBY: But the direction is predominantly in the hoop direction.

The other interesting thing that I saw in this location is that this is the ratio of the maximum principal strain to the intermediate principal strain. This is what I was talking about before. I didn't think you-all had this plot.

MR. SIESS: The other strain, zero.

MR. WEATHERBY: Far away, in the free field, it's going to be more like .14. So that tells you that the hoop strain -- the vertical strain is about equal to about 14 percent of the hoop strain.

And as you get in close to this insert plate, it gets much worse, so that right around this location, you're almost to the point of strain conditions, where the only strains are through the thickness of the sheets and in the hoop direction.

MR. SIESS: Is this the last analysis?

MR. WEATHERBY: This is the plastic.

MR. SIESS: Elastic-plastic?

MR. WEATHERBY: Uh-huh.

MR. SIESS: At what stage of elastic-plastic?

MR. WEATHERBY: Okay.

MR. SIESS: How far out on a plastic region

are we?

MR. WEATHERBY: Let's let this plot pattern tell us. It tells you that we have -- that the strain is about 3 percent.

MR. SIESS: Oh, okay. That's the equivalent plastic strain? I got you.

MR. WEATHERBY: So you can look on a uniaxial curve and roughly go over two and a half, 3 percent.

MR. SIESS: And this level is what, 145 psi. Right?

MR. WEATHERBY: Right.

MR. SIESS: Okay. Is that what time down there is?

MR. WEATHERBY: Yeah. We raised it one psi per second. It does all sorts of funny things.

And based on this set of calculations, what I had an obvious conclusion now, was when the stud anchors are neglected, the strains are too small to suggest liner tearing. So what that tells me is that the primary (inaudible) in the liner was the strain concentration caused by the stud anchors. And I have some pictures here. Dan showed them a little earlier. But I think the photographs bring out the -- are a little easier to examine. I only have four pictures so I'll just kind of scatter them around.

What you can see in those pictures -- this is after the test, obviously. But you can actually see the dimples where the studs are placed at two inches. And you can see how this relative slip has been a blow to the studs. And that's what I -- I really believe now that the studs are the primary cause of the failure.

MR. EBERSOLE: You would have guessed that by inspection, wouldn't you?

MR. WEATHERBY: Not necessarily.

MR. EBERSOLE: No?

MR. WEATHERBY: The problem is that the first time we walked into the model, we didn't have all this on (inaudible), flashing in and out. When you look at the tears, you don't really see the studs. And that's when I started this project. When the photographs came back and the lighting was at the right angle --

MR. EBERSOLE: You could see the shadows where the studs are trying to hold it.

MR. SIESS: Randy, looking back at your slip plot, that slip is very uniform, all down that vertical edge.

MR. WEATHERBY: Right. There's the secret.

MR. SIESS: Now, suppose there were no studs in there, do you think you could predict where the liner would go? After all, you've still got a weld over there.

It's not too far away. We can't change the material properties.

MR. WEATHERBY: I don't think I could without more test data.

MR. SIESS: Of course, would it make much difference whether you were one at 10 percent strain or 12 percent strain or 14 in terms of pressure?

MR. WEATHERBY: Not at that high a level.

MR. SIESS: What's the curve look like up there? It's pretty flat, isn't it?

MR. WEATHERBY: Yeah. In fact, the rebar probably can't handle more than about 5 percent strain or so. But I think then you would be competing with --

MR. SIESS: That low?

MR. WEATHERBY: That's whenever a significant number of the studs would probably break. It would be about 6 percent, is my guess, based on the (inaudible).

MR. CLAUSS: You mean the splices, not the studs.

MR. SIESS: Splices only get around 5 percent strain.

MR. CLAUSS: Five or 6 percent strain, yeah.

MR. WEATHERBY: At least that would be the strain in the bars.

MR. HORSCHEL: Maximum loaded bars occurs

about 6 or 7 percent, maybe a little bit more. (Inaudible) than that, you reach your maximum load fairly early on about 6 or 7 percent. And the splices are reducing that slightly to your 5 and 6 percent.

MR. SIESS: And an unspliced bar is still only about 6 to 7 percent?

MR. HORSCHEL: An unspliced bar with a maximum load at about 6 to 7 percent strain. It will strain -- ultimate strain is probably about what, 15 percent, or something like that?

MR. WEATHERBY: Right. But by then, you're over the hump and things would be happening fast.

MR. SIESS: Okay.

MR. WEATHERBY: But what I intend to do with this, I'm going to try another calculation where I put in some strains to model the studs and to calculate loads on the studs. That's the next step.

We have some data from stud pull test where we have a stud embedded in the concrete and then a liner plate welded to a stud. And we've pulled on the liner plate and measured the force of the (inaudible) curve for the effect of that curve, and that we put in the calculations to calculate this loading on the studs and see how close we are to causing shear failure or --

MR. SIESS: When you made that test, did you

carry for failure?

MR. HORSCHEL: Failure for studs, yes.

MR. SIESS: What happened?

MR. WEATHERBY: Mixed results.

MR. HORSCHEL: It varied. As I mentioned, we had some with just one stud on it, some with three and some with four. And we had various failures. Some failed in the weld itself, some actually sheared the shank of the stud. And then in the back were one of the four of the specimens, we had one that failed -- two studs failed by weld failure, two failed by the shearing of the shank.

MR. SIESS: When you put more than one stud on they don't share the load. They don't share the loads, do they?

MR. HORSCHEL: From what we found, you can actually average that ultimate load, and it does average out to the --

MR. SIESS: Yield horizontal.

MR. HORSCHEL: But indeed, we could even see some rotation on the stud from time to time so we could send (inaudible) to get you off one side and not go in a straight line.

MR. SIESS: Did you detect separation between the plate and the concrete?

MR. HORSCHEL: We did do some, and I guess I

would have to go look at the report to be sure. But we did measure(inaudible) without of plane displacement motion. And then there was some, but it was obviously quite small.

MR. SIESS: And that could be very hard.

MR. HORSCHEL: Yeah. It took about (inaudible), we're aware of that. But that's all we had to go with right now for our analysis, and we think it's a reasonable start. There is obviously some (inaudible) the analysis to design some better tests and maybe we can further this along a little more. But right now, I don't think we're there. I think we'll need to do some analyses with the tests that we have at hand and see where it can go from there.

MR. EBERSOLE: Chet, can I ask a question?

MR. SIESS: Sure.

MR. EBERSOLE: In the long-run, before you get into refining all these things, it's a little bit like the thermal hydraulics. What's the goal? Is it to ensure you'll have a program failure, as you wish it, or to have a failure like the wonderful one-horse (inaudible), all over the place at once? I think it's the former, which you got accidental.

MR. WEATHERBY: Well, from my -- go ahead.

MR. COSTELLO: Well, I was going to say -- just a thought.

I was going to suggest that in the long-haul, I think that the goal is more like the former, that if you're sure that you know how far you can go from (inaudible) or anything, then it's like any such strategy developed around that or of which that factor (inaudible). Then it would make sure we understand why these things happen and how they happen.

MR. EBERSOLE: You can be sure of that.

MR. COSTELLO: So you can't project them to different full-sized containments.

MR. EBERSOLE: Yeah, so you can be sure really of the former objective.

MR. COSTELLO: Yes.

MR. EBERSOLE: Okay.

MR. COSTELLO: As long as you've got a reliable method.

MR. WEATHERBY: The last slide. This is what I anticipate doing in answering the questions people ask me. The way I look at it at this point, I would focus all my efforts in the future -- most of my efforts on trying to characterize leakage due to liner tearing because that looks like a very important problem to consider now.

And in doing that, I think you have to distinguish between two things. One is the initiation of the tears in the liner. And that involves being able to predict stresses

and studs and very local details. But if you do initiate a liner tear, then you have to know how that tear -- how does it grow. In some cases in the model, it appears that the tears didn't grow at all, like around the equipment hatches. They've just -- well, they grew in small (inaudible) and quit. The matter of the tear around the insert plate grew a long distance. And that's related -- bably related to the fact that there's a uniformed condition along the edge of that insert plate.

So it seems to me that one could distinguish between different types of liner tears, depending on the locations where the tear occurs.

There is some that are likely to grow and others that are likely to stay as pin holes once they form. Both of these problems are very complicated. It's not an easy answer or an easy puzzle to solve it because any approach is going to require a lot of testing to really validate the technique.

MR. SIESS: Randy, since the initiation is going to occur presumably where there's a high stress, what's the probability that it will propogate into a region of lower stress?

MR. WEATHERBY: If there's a large stress or strain gradient -- you know, stress changes very rapidly with position. And it could be that the tear will just grow

a very small density. It may take a lot of pressure to make it grow any substantially.

MR. SIESS: Because everything you've done now suggests that. You get these high stress concentrations. They're not very -- they're high, but they're not very wide.

MR. WEATHERBY: Right. That's the liner -- you're talking about the stretched concentrations caused by the stud?

MR. SIESS: Well, even in the liner those things were not very broad. And that wasn't enough to do it. So to get the initiation, you've got to put the stud in?

MR. WEATHERBY: Right.

MR. SIESS: And that's got to be very local.

MR. WEATHERBY: Right.

MR. SIESS: And once you get away from the stud, you're going to be back into that lower stressed area. And unless you've got a very poor ductility material, it really shouldn't propogate.

MR. WEATHERBY: But it looks like, at least the way I interpret the test and based on the leak measurements and what not, that we did reach a critical condition in the test, so we had a tear that formed -- maybe more than one tear -- along the edge of that -- on the edge

of the piping penetration.

MR. SIESS: Yeah, but I --

MR. WEATHERBY: But there was a certain pressure at which this tear suddenly grew from a fairly small tear to a very long tear that ran along the edge of that insert plate.

MR. SIESS: Well, why wasn't it one, two, three, four, five, six, seven, eight tears (cough) stud? That E line, that whole area is uniform and slipped. All right. And all the studs would be getting about the same load. And it wouldn't be surprising at all with the same stress probably, and the same stud loads that you would get five or six tears to connect it up.

I think what you're saying -- and I'm not saying you shouldn't do what you're doing -- but I just think the probably, the dynamic crack propogation, and that's good. You know, once you get away from that (inaudible) plate, you really drop off.

MR. WEATHERBY: Right. I guess what I'm thinking of is more, when will the crack that's in this stress-field caused by the stiffner plate we want to (inaudible) go to something that's on the same length as a stiffner plate.

MR. SIESS: Gets away from the stiffner plate.

MR. WEATHERBY: If I had a small tear due to a stud --

MR. SIESS: Yes.

MR. WEATHERBY: -- (inaudible) a day. Then if I increase the pressure, at what point will that small tear
is -- or is there a pressure level where that small tear suddenly takes off and grows along the edge of this stiffner plate and (inaudible) in the stiffner plate.

MR. SIESS: You know, I would suspect from the leak rates, the different pressures, that you might have had seven small tears here that gradually grew and connected up.

MR. HORSCHEL: Yeah, if you look at the plate below it, I certainly fear that there is at least two that look like they're ready to capture the liner. And, you know, certainly you can have both of those start at the same time and eventually join up.

Another thing I find interesting if, if we look at the shape of that E line that he has drawn up twice, it's almost analagous the way that tear kind of rounded the corner of that insert plate, kind of showing that, in my mind, the crack was going from a high slip area to a low slip area.

MR. SIESS: And you would really like for it

to run a little bit. You might want it to run far enough to get the heat rate up to pressure.

MR. WEATHERBY: But that was -- we have made this place larger to go to.

MR. SIESS: We can put the studs in at an inch apart (cough) a nice long one that would reduce the pressure.

MR. WEATHERBY: What kind of tests point did I have in mind.

MR. BENDER: That's the other slide.

MR. WEATHERBY: It depends on which problem we are talking about.

MR. BENDER: I'm just looking at the future work. Let's talk about the first one, predicting the initiation of liner tears. What sort of thing is the initiation of liner tears?

MR. WEATHERBY: Well, first of all, we haven't really formulated the specific (cough).

MR. BENDER: So it needs to be the developed?

MR. WEATHERBY: Right. These are merely categories. I can think of some (inaudible) that I would like to follow up on. But what we need to do is plot some designs, some tests to go along with those.

What I have in mind as far as the (inaudible). You then look at the problem of the stud in the liner

because I think that's where the strain concentration comes from. And once you get that forces on the stud, then you can do -- probably look through the thickness of the liner and you can look at the strain concentrations due to bending and shearing of the studs.

MR. SIESS: You've got a problem there, though. You can take your pull-out or push-out tests, as it was, you know, the tests you made and you get some idea of a load slip characteristic from the stud, and that will give you the shear forces of the stud.

MR. WEATHERBY: Right.

MR. SIESS: You're going to have a little more difficulty trying to find out how much (inaudible) is on the line.

MR. WEATHERBY: That's the problem.

MR. SIESS: Because that shear force is going to be concentrated very near the bottom of the stud. It's a flexible -- it's a dial action.

So it will be down near the bottom, and if it's a tenth of an inch off or two-tenths of an inch off, makes the factor 2.

As it starts, you can simply assume the plastic moment of the stud going into the plate.

MR. WEATHERBY: Okay.

MR. SIESS: That's not an upper bound,

though, because there is a weld on the bottom of that that gives you a little extra area.

But you can take the plastic off of the stud just above the weld, and that would be a place to start. That's the only way I would know to do it. And I've tested my channel shape-shear connectors with strain gages on them, but I don't know how to put -- you can't do it on the studs. It won't work.

MR. BENDER: Let's talk about the second item up there, propagation.

What do you have in mind there?

MR. WEATHERBY: Okay. First of all, this is the (inaudible). And in a field fraction again, is still very -- it's not that well developed. And the first thing we would have to do is, we would have to go back in and try to get fracture (inaudible) for the material which we don't have right now, the liner material. That would be a set of tests that we would have to do.

Then your approach here would be to assume the surface wall size that might be scaled by stud diameter. So you would assume that a very small tear develops due to the stud, if that tear is going to extend, just outside of the strain field or the stud -- or the area where the stud influences the strain, and then you ask yourself at what pressure will that size of flaw grow or begin to grow into a

larger flaw, whether it will grow stably slower or faster.

MR. BENDER: Is there any background data that you can use as a frame of reference?

MR. WEATHERBY: There are some cracks that we found (inaudible).

MR. EBERSOLE: Isn't it the fourth bullet, which is, if you arrest pressure buildup by establishing a leak, well at what point would it reclose?

MR. SHEWMON: The steel will never reclose.

MR. EBERSOLE: Not the steel, but the concrete structure reclose to a reasonably tight containment.

MR. WEATHERBY: I don't know that it really ever would.

MR. EBERSOLE: I've heard it claimed that it would.

MR. BENDER: Well, if the (inaudible) is going up through the concrete structure itself, that might be an interesting question.

MR. SHEWMON: I have another question.

MR. BENDER: The pressure doesn't necessarily (inaudible) of job.

MR. EBERSOLE: That's where it goes after the first of the liner.

MR. SHEWMON: That last point is that there

is no pull resistance to the concrete, isn't it?

MR. EBERSOLE: No. It's a natural quench.

MR. WEATHERBY: That would be something I would say that the flow resistance of the concrete would not be that important in the calculation, just as a first guess. I would be trying to calculate the open area to the crack.

MR. EBERSOLE: Is there enough resiliance in the rebars after it's been strained to suggest that we reclose the weld in the concrete.

MR. WEATHERBY: You can go out to the (cough) and look at the structure today and you can see, in fact, in the pictures that you have of the tear, you can see that there is quite a gap in this liner.

MR. SHEWMON: But that also buckles. The question is what gap is there in the concrete?

MR. WEATHERBY: Yes. Well, you can see quite large cracks in the concrete.

MR. SHEWMON: If you want to move away gas.

MR. WEATHERBY: Well, I don't know how to estimate that.

MR. EBERSOLE: In actual concrete pressure vessels like they have on (inaudible), that's the, I think one retrinsic claim, isn't it.

MR. BENDER: That's a prestressed concrete.

MR. EBERSOLE: That's true; that's true.

MR. BENDER: And this is designed to be held closed by the --

MR. EBERSOLE: That may be the best kind to build.

MR. BENDER: Well, that's a relevant argument. Whether the prestress containment may have some different kind of --

MR. WEATHERBY: Well, I think in the prestress containments, I just got through looking at one for another exercise; there, the prestressing, once you overcome the prestressing, it's just like reinforcement in many respects. By prestressing you really don't gain any ultimate capacity.

MR. BENDER: If you take the concrete out of the plastic, what have you got if the concrete remains in compression, up to the point of line, then you could consider.

MR. COSTELLO: But typically, Mike, about 125 percent or 140 percent of design pressure is enough to unload --

MR. WEATHERBY: Right.

MR. BENDER: I don't disagree with that.

MR. COSTELLO: So you know, when we're talking bubble design pressure, we're already in the reinforce mode again.

MR. BENDER: It's a fairly complicated subject, but my reason for that question has more to do with just trying to get some impression of what the ongoing program might be, how long would the test program be, how much effort might be involved, and what do we do with the information after we've got it?

MR. COSTELLO: Let me try and (inaudible) for a second because there is more, we're not alone in this undertaking, allowing the EPRI sponsor -- most of the EPRI sponsor's work at ANATECH is concentrated on minor (inaudible). They are currently working on a policy of -- set of hypotheses which we hope to have available in the spring, and taking together, Randy has attempted to get some of the scoping packages with the calculations, independent calculations for (inaudible) company, and then we would be able to better formulate some test program to check the hypotheses.

But, again, the question of prestressing, I don't want to be premature, but even for those reinforced containments which have contiguous anchorage, there is reason to speculate that if the stud here is the greater of two factors the concentrations, the more ridged anchorage would do a better job of concentration than with the stud and the embedded (inaudible) will pin that piece of liner even better.

MR. BENDER: Based on what I saw of the test out there, I could easily envision looking at different kinds of stud patterns for new container designs. But for existing containers, I don't know that I know what to do.

MR. COSTELLO: I think we also have to -- another thing we have to look at in the pattern of our test program, is the population of anchorage designs. (Inaudible) to arrange for first we have to have some sort of that would hypothesize what is important. You know this kind of anchorage system should be more sensitive than it is or this should be very concentrated.

MR. BENDER: Is EPRI trying to do that? It seems to me that's what they ought to be doing.

MR. COSTELLO: Their ANATECH work is being focused on theories for minor tear initiation. And even, I believe, some propagation, I think, the focus is fairly on initiation. It may well, I hope, suffice the purpose. It seems that a small delta p is required to get you going and then the initiation will suffice.

Thank you, Mr. Weatherby.

To finish off the presentation on concrete containments, we have a short presentation from Dan Horschel, who has been interacting in our behalf with the Central Interstate Charity Generating Board on their proposed model test for a model of the Size Well B

containment, which is a great similarity to a Stumps designs in the U.S..

To give you a little history on it, you have a back and forths about whether the vailidity of K about whether has a (inaudible) or not. The side that decided, which did not include the CEGB, but decided that is was prudent to do a test on the model containment prevailed. CEGB acquiesces its condition of license, but then faced two possibilities; one, do a test which would meet the minimum commitment that they have which is to show the strength of the shell to be at least twice the design pressure to be consistent with the PCRB requirements -- this was the licensing requirements they have on themselves -- or to do a little more and put a liner in and actually look at failure modes. After some going back and forth, it begins to look as if they will concentrate on the minimum test. That being the case, we don't see as being a payoff for us in participating. However, we are likely to participate in their undertaking to about the same level as they participated in the concrete model.

MR. BENDER: That will be a prestress containment.

MR. COSTELLO: It will be a prestress containment, unless they change their minds on the liner; unless they change their mind again on the liner.

MR. SIESS: Who's going to design it?

MR. COSTELLO: The Taylor Woodrow/McApline combination. Dan's been over there interacting with them recently, last fall, and he's going again shortly. He will give you what he knows today.

MR. SHEWMON: What is "PCRV"?

MR. COSTELLO: PCRV, prestress --

MR. SIESS: Fort St Vrain.

MR. COSTELLO: Or more precisely, British gas reactors. And the Minimum 2 Factor was attached to the design of both the British prestress reactor vessels and they all were tested, model tested, all the designs have model tests to show that. The licensing argument was will, youve got to do the same for a prestress containment for light water reactor.

MR. SIESS: Have they got a rationale for not putting the liner in?

MR. HORSCHEL: It really goes back to what Jim was saying about the strength. Since the liner is included in the design as a strength member, they say it (cough) from the test. That's usually their --

MR. SIESS: They are going to build them with a liner, but not test them with the liner?

MR. HORSCHEL: Exactly. Because when you put the strength of the containment, the code doesn't allow you

to use the liner as a strength bearing member.

MR. SIESS: Then -- their code or our code? And they are only interested in strength, not leakage?

MR. HORSCHEL: In fact, it seems like a large (inaudible) this is attached just to manifest that they can go two times beyond the design pressure without that liner. They also, of course, do want to get help (cough). At first, they had two times of design pressure and that seemed to be very important to them.

MR. SHEWMON: Two times beyond or two kinds --

MR. HORSCHEL: Two times design pressure.

MR. SIESS: What was it? What did you change there?

MR. HORSCHEL: I said two times beyond but I meant two times --

MR. EBERSOLE: You said without the liner. Right?

MR. HORSCHEL: Right.

MR. EBERSOLE: They don't allow the liner to contribute to the load.

MR. SIESS: We don't either.

MR. HORSCHEL: That's the same as it is in the United States. The liner is considered the normal load bearing member.

MR. SIESS: That's part of our conservatism.

MR. HORSCHEL: That's considered the natural containment. We're talking about --

MR. EBERSOLE: How do they propose to seal the concrete?

MR. HORSCHEL: They'll be using a rubber bladder and they'll be pressurizing using a hydrogen -- water.

MR. EBERSOLE: That has it's advantages, you know.

MR. HERSCHEL: H2O.

MR. EBERSOLE: They won't use rubber in real life, surely. What will they use?

MR. SIESS: They'll use a liner.

MR. EBERSOLE: Oh, okay.

FUTURE WORK ON REINFORCED AND PRESTRESSED CONTAINMENTS
Prestressed Containments

MR. HORSCHEL: We'll get into this point a little bit later.

So why don't I just start. First off, you always hear of the CEGB, that's Central Electric Generating Board. They are sponsoring this test. It's designed by Nuclear Design Associates and it's a joint venture between Taylor Woodrow and McAlpine. The construction and testing of the

prestress model will be done by Taylor Woodrow.

They do have a peer review group. This is a list of those people. Carl Lomas, who's the chairman of the Sizewell "B" Positive Management Team, PMT.

There's also Peter George, he's part of the Sizewell "B" Positive Management Team.

There's Dave Philips from the UKAEA; James Irving from the Independent Inspection Agency.

Paul Divjak, the Project Manager from Bechtel, Sizewell "B" Project Management team.

There's also Dick Crowder from the Nuclear Design Associates.

And there's myself, from Sandia National Labs.

Some of the features of the containment model: It is a scale model of Sizewell "B".

Along those lines, they also felt that scaling the Sizewell "B" was more important than following codes, so if there is any overlap of those two things, they'll probably lean toward the scaling of the Sizewell "B".

It has one equipment hatch, two personnel airlocks. Both of those are over-designed penetration covers, so that they will not leak. Keep in mind, there is no liner in here, so it's just the (cough) concrete and they have some type of bearing plate around those to hold them into place.

There is no external or internal structures represented.

They use scaled steel and force rather than using a 1:1 bar replacement and tendon replacement.

The tests will be conducted hydrostatically and they, of course, have thickened the basemat due to the (inaudible) malfimestamia of the gravitational forces and not including the internal force, the internal structures.

Due to the scale, which is 1:10, they are mixing the model with concrete.

MR. SHEWMON: What is "scaled steel areas" mean?

MR. SIESS: It's really force.

MR. HORSCHEL: 10 inches is a full-size model on the scale. This scale doesn't have one, it has one square inch. Okay. Say, on a full-size containment, you might have 10 bars and the model might have two bars. So they would have 10 bars and each one is a square inch; it's not on this model.

MR. EBERSOLE: So that cracks won't scale. How tall will this thing be?

MR. HORSCHEL: Overall height is about 23 feet not including the (inaudible).

MR. EBERSOLE: So the hydraulic oil is inconsequential.

MR. HORSCHEL: (Inaudible) pressure fails, the higher pressure that you go, it doesn't (someone talking at same time). Your hydrostatic head will give you approximately 10 psi from top to bottom.

MR. EBERSOLE: That's not enough.

MR. HORSCHEL: It depends on where it fails. If you were at 150 psi, 10 psi difference isn't too much, where if it fails at 50 psi, 10 psi is more significant.

Here's the overall dimensions of the containment model. Here you can see the basemat. They do have it sitting on top of the pedastal so they can get to the original (inaudible) test is so slow.

Here's one of the personnel airlocks.

Nominal wall thickness is .13 meters in the cylinder wall and .1 is your dome.

Inside diameter is seven and a half feet; using radius seven and a half feet.

The top view of the containment, here you can see the three buttresses from the prestressed. How they come up, again, you can see the buttresses, but also the two personnel airlocks and the one equipment hatch. This is a stretchout of the cylinder. Again, the two personnel airlocks, the equipment hatch, and the layout of the three buttresses.

One thing that I think is important is whether

they decided to connect the reinforcing steel. The Sizewell "B" uses cold swaged splices much as we used in our scale model, reinforced concrete model.

For this model, however, they will weld reinforcing steel together, rather than using the splices. Sinse this is an ultimate strength test, we've recommended that they reconsider the cold swaged splices, but it seems like they will weld them.

MR. SIESS: Dan, how much does the rebar contribute to the (cough).

MR. HORSCHEL: I guess, off the top of my head, I really don't know.

One of the things that I was somewhat surprised at is when you lose the preload. Preload only takes you to about one to one -- excuse me -- somewhere around 1.2 to 1.5 of the design pressure, so you're really relying quite a bit on the reinforcement to get you up to that two times --

MR. SIESS: Well, we do that, too. Our prestress usually is designed to get you up to the SIT without cracking.

MR. HORSCHEL: That's true. This is very typical of a Bechtel design and --

MR. SIESS: Rebar doesn't usually contribute that much, 10 or 15 percent maybe, I think.

MR. HORSCHEL: Okay.

MR. SIESS: So really, it's 90,000 stuff, whether it makes 90,000 or 95.

MR. HORSCHEL: But my understanding is, there's always been a problem with welding high yield reinforcing steel.

MR. SIESS: We could get grade 60 in this country that is well able. I mean, I assume they could get some, too. We got an awful way of attaching two bars together. They don't like welding, but butt welding and so forth.

What are they going to learn from this that you couldn't learn from the Alberta test?

MR. HORSCHEL: I have to speak somewhat from the cuff on that. I believe in the Alberta test, they didn't do a very good job of handling things. For example, the walls were disproportionately thick and things such as that.

MR. SIESS: I know, but you can analyze it.

MR. HORSCHEL: Yes, but here --

MR. SIESS: This isn't a prototype, this isn't a proper model anyway. It's a simulation. You have to analyze it. Doesn't have a liner; doesn't have the steel in the right place.

MR. CLAUSS: I think the major question was that there was very little instrumentation of the test in

Canada.

MR. HORSCHEL: That's not really true. They had quite a bit on that. It was probably about 300 gages in the wall, such as that, that's close to what we had planned on this.

But I think the real question, in answer to your question, goes back to what Jim said when he introduced the top of this list; it's almost imposed by the NII, not the CEGB to test the model.

MR. SIESS: Since the VCRBs had to do it, this has to do it.

MR. COSTELLO: That's it.

MR. SIESS: But then they couldn't look at Canadian and the Colony and you know.

MR. COSTELLO: CEGB's first argument was, it was proven technology from the states. Ergo, they don't need to do the same kind of test as probably for VCRV's.

(All talking at once.)

MR. HORSCHEL: If you have a fullsize Sizewell "B," they use the channels instead of the liners.

MR. SIESS: I'm sorry. That's a ridiculous test.

MR. WARD: How are going to get license?

MR. SIESS: We aren't going to learn a thing, unless something peculiar happens.

MR. COSTELLO: I guess I can speculate that what we will get out of it is a chance to get out of it, absent surprises. Absent surprises that are not a result of something straining in this, peculiar to the model.

MR. SIESS: Is this a Bechtel prestressing scheme?

MR. HORSCHEL: Yes.

MR. SIESS: What model, frozen type?

MR. COSTELLO: SNPPS.

MR. SIESS: I'm not sure. Which was SNPPS?

MR. COSTELLO: Callaway, for sure. There was supposed to be five of them.

MR. SIESS: No --

MR. WARD: In Texas.

MR. SIESS: Yes. But three buttresses, what about over the top?

MR. HORSCHEL: I think that's just a (inaudible) thing. They don't need them all the way to the top.

MR. SIESS: No, no. The early designs had a ring gear that was a separate set of tensile bands, a verticle tensile anchored at the top.

MR. HORSCHEL: The reason those tendons go all the way across the top --

MR. SIESS: That's the (inaudible) design

then?

MR. COSTELLO: You have to speculate that, you know, if they go through the tests at this form about it, all we'll get out of it is the ability to see how well, you know, the method that have been shown in the SIT's to TRAC and unloading well enough to 115 percent.

MR. SIESS: Are they don't scale the steel properly in the buttresses, or are they going to mess around with this?

MR. HORSCHEL: They're trying to scale Sizewell "B."

MR. SIESS: I know, but in terms of bars and number of bars and so forth.

MR. HORSCHEL: It won't be the same number of bars. You just --

MR. SIESS: The Canadian model failed to buttress in the anchorage.

MR. HORSCHEL: And that's one thing we want to instrument a little heavier on this model, is to --

MR. SIESS: Looks like it would be sens'tive to have the steel (inaudible) was made.

MR. HORSCHEL: I agree.

MR. SIESS: Interesting.

MR. SHEWMON: Their sealing for the cross-sectional area of the steel will be the same?

MR. HORSCHEL: Yes.

MR. SHEWMON: Not the surface area?

MR. SIESS: Right.

MR. SHEWMON: They hope by next section to yield --

MR. SIESS: They're scaling on forces. The forces of will be right. How they put them in there --

MR. SHEWMON: My questions was it fails by net-section collapsed, not by pulling out?

MR. SIESS: Yeah. There shouldn't be any pulling out.

MR. BENDER: Not figuring on anything failing, that's just pretty much saying whether it takes (inaudible).

MR. SIESS: It's only going to go up to the design level and quit.

MR. HORSCHEL: Well, they do find one until they get some type of failure in your own model.

MR. SIESS: Okay. They do have a war.

MR. HORSCHEL: It will depend heavily on the (inaudible).

MR. BENDER: Right. Hydrostatic.

MR. SIESS: You're going to get 10 people to predict it.

MR. HORSCHEL: I don't think they have been

quite as successful as we have.

MR. WARD: Are you going to predict it?

MR. HORSCHEL: We're going to try and locate it, but people, I guess we're going to try to have that select an analyst, so I'm not sure when this will occur, as far as this program is concerned in their schedule. We'll be relying on some predictions, whether we pretest or or post-test remains to be seen. We would like to do pretest but it's getting people to do that.

MR. BENDER: Excuse me. One other question about the size. Is this designed to the ASME code?

MR. HORSCHEL: Well, it's actually the British standard, but a large part of that is the ASME code.

MR. SIESS: Is 1.5 load factors and --

MR. HORSCHEL: So I'm not that familiar with theirs. I know they adopt the ASME code for the most part. I think they have their own little quirk to take care of there.

MR. COSTELLO: And one of those quirks is an additional requirement of minimum twice pressure.

MR. SIESS: Minimum of what?

MR. COSTELLO: Minimum of two times the time pressure.

MR. BENDER: As opposed to one and a half.

MR. COSTELLO: Right. The ultimate

requirement, ultimate strength of the containment which is twice the design.

MR. SIESS: We have a little more than one and a half, in fact, one and a half divided by 29 gives you 165, I guess, not counting the seismic. They won't have the seismic margin in it. I assume they don't have a seismic.

MR. COSTELLO: It's just that that (inaudible) does have a slippage statement about ultimate -- which is what they are being --

MR. SIESS: Oh, boy. In other words, they are wasting money.

MR. HORSCHEL: Going on to the concrete used in this model, it will be mixed at the site and they are looking both for tensile and compressive properties and will try to match those as best as they can.

MR. EBERSOLE: What is "micro-concrete"?

MR. HORSCHEL: That's where you really don't have a full-size aggregate. It's really more of (inauduible). It's the more that you have in your mix.

MR. EBERSOLE: No superfine aggregate, is it?

MR. SIESS: No. You don't scale the aggrergrate (inaudible). We used it for years before anybody named it. We used to call it mortar.

MR. HORSCHEL: Mortar mix is very common.

MR. SIESS: Deep level concrete.

MR. HORSCHEL: They will take the cylinders for ASTM pressure testing and some of you may even be aware of the British (inaudible); they will be taking some samples from us and and testing those for our analyses.

MR. SIESS: They still make some split cylinder tests?

MR. HORSCHEL: Yes. They plan on doing that.

MR. SIESS: That's the way the buttresses go.

MR. HORSCHEL: They will also do some beam bending tests (inaudible).

Some specimens will be temperature-matched cured. Before we began building this style of the model, at Taylor Woodrow, they do have a fairly advanced concrete testing laboratory there. They may ask you to cut into your copy of the model used on the heaters or whatever is required to move temperature.

MR. EBERSOLE: What part of Q/A do they use to get a consistent repeatable mix all the time, as they make it?

MR. SIESS: Good standard practice, Jesse.

MR. HORSCHEL: That's really it. They've written up some specs from the model and they are trying to minimize any parameter that would affect the strength of the concrete, or really what it is, is just (inaudible) building containment.

MR. EBERSOLE: There is nothing funny about them testing the batches for --

MR. HORSCHEL: Not as a slump (inaudible). If you're at work, you're going to have to test the first part of the concrete also.

Whenever you work with a scale model, even when you don't have 1:1 bar replacement, some areas get very, very dense with all your tendons running through, all your reinforcing steel, plus instrumentation of things like that. It's very hard to properly place the concrete, so slump is very important; as will be the ability to consolidate the mix well and not have it segregating when you start vibrating it to consolidate it.

Again, along those lines, if you plan on using some mockups of congested sections, just to make sure they can place the mix properly as (inaudible) concrete, that's obviously going to be very important. So you're going to find out (inaudible) if they start prestressing it if they didn't (inaudible) follow the concrete.

As far as the specimens are concerned, I guess there is always some controversy when working with the scale model. Should you use scale specimen or full-size specimens, particularly when you're talking about wall sets that are thin as these. You weren't allowed to have a cylinder -- some people argue that you weren't allowed to

have a cylinder bigger than the three sections of the wall. So I have bolted six by 12 in the cylinder and some micro-specimens will be used.

MR. SIESS: How small is the micro-specimen, two by four?

MR. HORSCHEL: We haven't really decided on the size yet.

MR. SIESS: That's what we always used to do it when I did it.

MR. HORSCHEL: That does seem appropriate for the wall thickness that we have and the size that we have.

The last viewgraph: They really started the design about October of last year.

Supposedly, last I heard, they were starting the support tests in November.

I've just received the design packet from them in January, which means that if the construction should follow through, it would be after about February. And the rest of the schedule there, August should be the start of testing; and December, complete testing and report the results.

It's obviously a very optimistic schedule; whether they meet it remains to be seen.

I'll be impressed if they actually get that model built and start testing in the February to August time frame. I think that is the most critical section.

MR. SIESS: Are you afraid they'll show you up?

MR. HORSCHEL: Yeah.

(Laughter)

No, we always have (inaudible) getting up here. We have so much more instrumentation and things like that. But, it's an impressive schedule.

MR. SIESS: That's what you can do when you go after a private contractor.

MR. HORSCHEL: I'm not supposed to respond to that.

(Laughter)

In all honesty, I guess I should say a couple of things about that. As far as design specs and things like that, before we can pour a concrete model, we had to do all that from scratch. We've actually given them a lot of the information that came from our testing and it has helped them to some degree. I looked at the specs that they wrote for this and you can see that they mirror a lot of the things that we have, our testing plant, and things like that. So we really helped them cut down, at least to some degree, beforehand. We've made a lot of the trial and errors, if you will, of instrumentation. We've given them suggestions for what we've used and what worked well for us. So it really has helped them to some degree shorten it up

their schedule and minimize some of that. Hopefully, they won't use it without any thought and hopefully they will cover the bases that we didn't cover that are in their model, but they did gain a benefit from our testing program here.

MR. SIESS: Thank you.

MR. COSTELLO: Okay. I guess I would like to have the committee, in the sense to view their wishes on what to do between now and what you perceive to be a time to (inaudible).

It is -- the remaining presentation on the work on (inaudible) of Sequoyah of closing out the steel containment question in the sense of doing an application to an actual plant and follow-up by some presentations on the ongoing work on penetrations. We could quite easily defer the discussion of the (inaudible) study on seismic capacity of containments because, quite frankly, given our '88 budget reductions, we are not going to proceed at the pace we thought we were going to be; and one of the things that gets pushed off downstream is any question on seismic capacity.

MR. SIESS: I thought the analyses showed that seismic capacity was awfully high.

MR. COSTELLO: Generally speaking, yes.

MR. SIESS: What about the Japanese test, did they put theirs on the shake table yet?

MR. COSTELLO: No, but they had have some (inaudible) analysis, some (inaudible) loads on good size models.

MR. SIESS: I thought they were going to build a pretty good size model and put it on the shake table.

MR. COSTELLO: They have had one steel containment model on the shake table.

MR. SIESS: Steel.

MR. COSTELLO: Steel.

MR. SHEWMON: That's a racking --

MR. COSTELLO: Model.

MR. SHEWMON: One diminsional shape, or just squeeze it against it?

MR. COSTELLO: Most likely to give you the lateral load effect of the --

MR. SIESS: Is that a simulated seismic with the automatic equipment that --

MR. COSTELLO: Yeah.

MR. SIESS: They've got a -- make a static test that has feedback that simulates the input from an earthquake.

MR. SHEWMON: They rock it in some way.

MR. SIESS: No, no, just lateral loading.

MR. SHEWMON: So they push on it on one side?

MR. SIESS: Push/pull.

MR. SHEWMON: And it's held on the bottom by friction or something. That was what I was trying to ask.

MR. SIESS: They do it in a programmable system that stimulates the seismic (inaudible) and then (inaudible) depends on the response for the two cycles.

MR. WARD: Why is that? Because we'll be involved in inertia if things aren't right, or what?

MR. SIESS: Well, you just don't want to push it statically. You want to represent the --

MR. WARD: I mean, why isn't it just done on a shake field?

MR. SIESS: Well, it was a lot cheaper to put it on a shake table, but this is a hell of a lot bigger one. I believe they're testing 6x30 building with this system.

MR. COSTELLO: But if that's your preference, we can do that.

MR. SIESS: I think just put the seismic last any way. I have to leave. I would think that what I would put next on priority would be the study on penetration. We haven't heard much about that at all.

MR. COSTELLO: Okay. That's fine, if that is your preference.

MR. SIESS: And then I sort of leave it to you as to whether we take up this steel containment stuff

after that, finish up on this steel containment.

MR. COSTELLO: Thank you.

MR. SIESS: We haven't heard much on the airlock and seal bellows --

MR. COSTELLO: On containment penetration activities, we have first Dave Clauss, talking about Personnel Airlock test.

MR. SIESS: This question on applicability of model tests that's on typical full scale containments, where did you put that in your list?

MR. COSTELLO: Well, I sort of thought that's what we were touching all along, but let me look . . .

When we're done, have your schedule presentation speak some more to it.

MR. SIESS: That's the 64-dollar question, of course, that we did talk about it all along, but let's come back to it before we're through.

MR. COSTELLO: Okay.

MR. SIESS: And before we go any further what are you going to do with that?

MR. CLAUSS: I need to use the slide projector.

MR. SIESS: Would anybody like a short break before we . . .

MR. CLAUSS: That's probably appropriate.

It's going to take me a few minutes to get the slides ready.

MR. SIESS: Let's take 10 minutes here.

(Off the record)

CONTAINMENT PENETRATIONS

Investigation Of The Leakage Potential

of a Personnel Airlock

MR. CLAUSS: I'm going to be talking about recently concluded experimental program that we had that was designed to look into leak potential of personnel airlock.

The reason that we want to look at the airlock, it has been identified in previous studies the potential -- okay. Thank you.

(Fixing the projector)

MR. SIESS: It's cockeyed, Paul, but it's still all right.

MR. SHEWMON: You can tilt your head that much.

MR. SIESS: Yes.

MR. CLAUSS: The airlock has been identified as a potential failure mode for containment. And as part of this overall objective then to analyze the performance of containment, you need to be able to evaluate each potential failure mode containment to determine whether or not it's

controlling. So that's the basic objective in looking at the airlock.

There are a few studies that singled out the airlock. The first was INEL, internal adminstrative study that was documented NUREG-1037 which tried to predict leak areas from various penetrations. The specific example here, in Zion, the estimated leak area was 5.36 square inches at 134 psig.

MR. SIESS: That was for something that started off tight?

MR. CLAUSS: That's for something that started off tight; and what they were saying, there was enough separations on the ceiling surfaces on the airlock to produce a leakage.

MR. EBERSOLE: Well, tell me, some of them are sealed and pneumatically inflated.

MR. CLAUSS: Right. I guess I should have said that at the very outset. The airlock classes that I want to focus on here are those organic seals which rely on pressure seals rather than inflatable seals. Brad Parks will be talking a little bit about the inflatable seals for test programs we have that will be, the value and potential and those types of thing.

The second study, the Oregon survey, Oregon did a comprehensive survey of the penetrations that were in

various containment in the U.S. And they went through a fairly rigorous figure of merit type analysis trying to evaluate the potential of different penetrations. And I think in their list, (inaudible) No. 1, and this airlock was like No. 2.

So again, to address the concern that the airlock as a potential leaking pattern.

We have heard that the full size airlock canceled nuclear unit and tested to hypothetical severe accident environments.

Okay. So the airlock is essentially one that was built for service in the nuclear plant. I believe it was Callaway Unit 1 or Unit 2 it was originally intended for, that plant was supposed to be canceled.

There were no (inaudible) made on the block for testing and that's what you're seeing here, these various penetrations are for man way, man-way access and instrumentation to pass through, and so forth.

But the dimensions of the airlock are basically 10 feet in diameter. And the overall plant is about 20 feet. The next slide has more: Ten foot in diameter, 20 feet long. The sleeve thickness is about one inch thick near inner door, and that tapers down to 5/8 inch for most of the length and towards the outer door.

The bulkheads are stiffened flat circular plates,

of course, cut out for the door. The plate thickness is about one inch on the bulkhead.

The door size is 42 inches by 80 inches, and there is a real stiff frame around the door which consists of webs and flanges; these plates are quite hefty, as you can see there.

The doors themselves are also flat plate with grooves for the double, grooves for the double body to be on. And then, again, that's the door itself with the stiffeners.

MR. SIESS: What kind of pressures are these things designed for?

MR. CLAUSS: This particular airlock is designed for 56.

MR. SIESS: 56.

MR. CLAUSS: I'm sorry. 60.

This is just an outer view of the airlock and here it's being loaded into the test chamber.

Go ahead to the next. This again is the unit test chamber, it just gives you an idea of the size of the door opening there. Go ahead. Okay.

What we had here was a high temperature/high pressure test. And the objective of the test again, as in all our tests, is to generate data that we can use to validate analytical methods.

In this particular test, I put a strong emphasis on trying to produce conditions necessary to generate leakage. So we got some information on what the appropriate limit state criteria were. Again, this is a caveat I think we have in all our tests. We're not trying to take this test in the design of airlocks. This is a relatively recent vintage airlock and it has (inaudible) of different stiffener details. So, with the results I'm going to show you, shouldn't be taken and used generically. It really has to be a case-by-case basis.

MR. SIESS: Are you pressurizing the inside of this, which would be the outside of the natural containment, just the door you're testing. Right?

MR. CLAUSS: No, we didn't test -- I think I've got a slide that may show the test arrangement. Well, let me go ahead and describe it real briefly.

There were a number of allocations made for the block, cover for testing; we did pressurize the inner door on the surface that we would see pressure in an actual containment accident. The way we did that was to build a pressure vessel that we attached to the inside edge of the sleeve so that there is essentially about a foot long length cylinder and then a hemispherical dome, which was used to pressurize the inner door. There is also (inaudible) was to add a leak-tight bottom chamber. And this facilitated the

measurement of leakage passed the outer door. Leakage would go into that chamber and then reroute it through piping past (inaudible) so we could measure leakage in the event we saw any.

There is also similar leakage in the inner door where if there is any leakage passed the inner door, there is some type of shroud that gathers that leakage over the door frame and the leak is directed through some piping, through flow meters, so that we can measure the actual leak rate.

This is just one of the things that we did try to increase, if you will, the potential for leakage. We wanted to age the seals. If the seals were not aged, they've got so much springback that even with large deformation you would never see leakage.

And the seals are subject to some high temperature and radiation aging of the surface. We did attempt a very severe condition, it's the IEEE specs, which is based on a 40-year surface life. In reality, these seals have just been changed, about e·ery five years.

So this is a very severe condition that perhaps would never be seen in actual containment.

MR. MARK: What's the basis of saying that 30 degrees Fahrenheit equals 200 Mrads?

MR. CLAUSS: Well, we did an analysis and

based on some data from (inaudible), and we looked at the parameter seal performace called compression set retention, which is basically just a measure of how much the seal can spring back after being deformed and subject to these types of conditions. So you deform the seal a certain amount and you subject it to 300 degree Fahrenheit and 200 megarads, which is the IEEE specs, then you can measure a disclaimer called compressive set retention.

MR. MARK: You're merely measuring something analogous to embrittlement.

MR. CLAUSS: Pardon me?

MR. MARK: You're measuring something analogous to embrittlement.

MR. CLAUSS: Yes, it's analogous.

MR. WARD: Loss of --

MR. CLAUSS: It's analogous to that.

So at any rate, we've --

MR. SHEWMON: The way that's worded; it sounds like you aged with high temperatures during radiation. Is that right?

MR. MARK: No, they didn't do any radiation.

MR. CLAUSS: Let me start over.

The IEEE specs are the 40-year --

MR. SHEWMON: I understood that.

MR. CLAUSS: Okay. I'm just trying to lay it

out from the start. Excuse me.

The IEEE specs call for both thermal and radiation aging. In our test, we could only do thermal aging because of the size of the seal and the size of the fixture, it was impossible to do radiation.

What we wanted to do was come up with a thermal aging that was equivalent to the radiation, they were aging in the IEEE spec.

Now, to do that, you have to decide what is the parameter I'm going to try to make equivalent. And the parameter that seemed to affect leakage most highly is compression set retention.

So, we had, from compressed rate data, a pretty good idea of what the compression set retention is for this type of material when it's subject to the IEEE specs. So what we wanted to do and then was to achieve that same compression set retention and thermal aging and what we found through some -- there is a modem called (inaudible) equation that was used for this. It's not that great of a model, but it was the best that is available. Using that model, we came up with 330 degrees Fahrenheit.

MR. EBERSOLE: I thought you just said a 30 degree difference was the radiation effect.

MR. CLAUSS: That is true, but it gives you the same compression set retention as both thermal and

radiation aging.

MR. SIESS: That's the same kind of degradation?

MR. CLAUSS: That's right.

Okay. There was significant damage to the seal. This is a shot of the seal before the aging; the double "dogears" (inaudible), and dogears refers to these. It's essentially a cross-section, they look like little ears from that. Those actually provide the ceiling.

Now the cross-section was significantly changed in the photograph after aging. Essentially these dogears were extruted and the seal became essentially a gasket. You can see there is this lip between (inaudible) that scooted out and that's what is going to form the ceiling seemingly at this point.

Now, it's a pretty gross change but we decided this was a worst case scenario and we wanted to see what would happen to the seal. I think there are some more photographs; you can see it. You can see the kind of damage that this created when the bulkhead was opened or the door was opened causing the bulkhead damage and this material here.

MR. SIESS: That test was intended to find -- reproduce the degradation that would occur following an accident. Right?

MR. CLAUSS: Yeah. That includes the 40 years service life plus LOCA if there was some accident.

MR. SIESS: If it were just normal service, wouldn't you expect the seal to be replaced when it looked like the one you had?

MR. CLAUSS: If it was just normal service, I would take it into account.

MR. SIESS: Well, you showed a considerable change of shape of the seal.

MR. CLAUSS: Right.

MR. SIESS: Now, that could be taken care of by maintenance and replacement.

MR. CLAUSS: Sure.

MR. SIESS: I don't think you would leave a seal in there if it looked like that, so it would have to be --

MR. CLAUSS: Oh, of course you wouldn't leave the seal in if it looked like that.

MR. SIESS: 40 years is really not as important as the accident.

MR. CLAUSS: Right.

Okay. Some of the instrumentation that we had was mild. We were very concerned to handle the behavior of the sealing surfaces. And we wanted to know that it was -- what sort of deformation would go on at the bulkhead. We had a

large number of capacity-type high resolution displacement transducers. These tranducers were capable of measuring deformations on the order of a mill with good accuracy. They also -- well, basically, in high temperature range that we're looking at. Here is a photograph of some of the displacement transducers. You can just see that as we moved around the connectors, we had a number of these displacement transducers. These two (inaudible) bulkhead annotation of the door relative to the bulkhead.

We also had a lot of strain gages. The (inaudible) of the model was 123, with a high temperature weldable gages; I have a photograph of that also. You can see, again, a significant amount of instrumentation is on the door. So we were trying to measure the gages through the bends in the door.

In addition to that, we're also trying to measure the temperature distribution in the lock. We had a total of 112 thermocouples that were located in the environment as well as on the doors and the bulkhead and down the sweep into the airlock. We measured leakage using specific orifice plates which we named with differential pressure transducers. Running it through some calibrations, we were able to correlate the differential pressure with the heat rate and there was switching circuitry to go from a small orifice to a large orifice as the heat rate increased. Also

six pressure transducers to measure the pressure in (inaudible). The test chamber, pressurizing the inner door. Also pressure in the chamber between the two doors in the event that we got (inaudible) into that area. There are pressure transducers in the (inaudible).

I'm going to start to skip to the analysis and just gives you the results of the test programs, we are running late. Let me just get the next couple of slides out and skip right over those. I think I've talked about the analysis enough.

The first series of tests were done at ambient temperatures, pressurizing the 1.15 times design. That was basically just to convince ourselves that we could meet a leak rate test specification without any seals.

We first tested without any seals installed, and that's a test of 1B we conducted on the inner and the outer door. We found that metal to metal contact by itself was not enough to preclude fairly significant leakage. This corresponds to around 10 to 15 percent per day from 1 million cubic foot container.

Then the next two tests we did we had the eight seals installed and again that was just to verify that the seal, even though they are badly aged and deteriorated, were capable of preventing leakage at near design pressures and SIT pressures.

Okay. In the major test was this high pressure/high temperature test. It was conducted basically in three stages. The first stage at 100 degrees Fahrenheit. The objective there was to look at the seal -- let me back up just a little bit. One of the things we were trying to do was use some of the data that we generated from our seal test. And some of the basic conclusions that we had found from the seal test was that the seals degraded at temperatures in the average of 500 to 650 degrees Fahrenheit depending upon the sealing material.

Before it was degraded, leakage was mostly dependent upon compression set retention. Okay. So for this particular material, which is EPPM, degradation doesn't become severe until about 650 degrees Fahrenheit. So we wanted to do one stage of this test at what I would call moderate temperatures which represents the stage in which the seal isn't badly degraded and the compression set retention really dictates whether or not you get leakage. So that was the first stage, and with the chamber heated at 400 degrees Fahrenheit, we pressurized in steps taking data for each step, grade it, analogous --

We pressurized at 300 psig and we did not detect any leakage. I think it was on a earlier slide, I didn't say it, we were capable of finding leaks as small as .5 to 200 scfm range, which corresponds to only about 22 percent

mass today.

So there was no significant leakage, even at 400 degrees Fahrenheit and 300 psig.

Okay. Then the second stage was to heat this to very hot temperatures, 800 degrees Fahrenheit. Okay. We depressurized the model first and then we heated it and let it soak over night so it was sitting at 800 degrees Fahrenheit for about eight hours.

Then the next morning, we began to pressurize flow increments. And beginning at about 60, 70 psig, we noticed a very small leakage, like a .2 standard cubic feet per minute. And that leakage slowly increased with pressure until at 150, we had a heat rate of about two standard cubic feet per minute. Took the data at that pressure level and prepared to take the next pressure step.

The leak rate -- as we started increasing pressure, the heat rate suddenly grew very rapidly, well over 200 standard cubic feet per minute, which was really the limit of our heat rate capability.

So effectively, the inner door was bypassed, and at that point now, we're starting to pressurize the chamber between the two doors. So that's the third stage.

Now, we're actualy putting the loading on the outer door that corresponds to this (inaudible) where you've got leak capacity (inaudible).

As I say, the inner door is bypassed from the pressure of two chambers. And I guess I'm not sure what happened to that slide, but Chamber V-1 refers to the chamber which pressurizes the inner door. And Chamber PL-1 refers to the chamber between the two doors.

At any rate, that pressure is essentially equalized now in this stage of the test because you bypass the inner door.

Then, at that point, then we presumed incrementing the pressure up. The pressure of both Chambers was about 300 psig, and no leakage was measured from the outer door at all. And we did hold the 300 psig for some time.

Okay. Basic conclusions: The relative displacement of the sealing surfaces throughout this test, even up to 300, was very small. And that was expected from the analysis that that heat change may be calculated.

And even given the bad leak seal, there's enough springback there that it can prevent a leak or small deformations. So that basically (inaudible) any leak in the first stage of high pressure test.

In the second stage, when we got up to 800 degrees Fahrenheit, this seal was badly degraded. And you already have that lift there which prevented metal-to-metal contact. At high enough pressure, the sealed material in that gap between the metal surfaces was essentially injected. You

know, the seal was so badly deteriorated that it just doesn't have the strength to just lift that type of pressure.

MR. SIESS: Now, the moderate temperature in water, that's the 400 F?

MR. CLAUSS: Right.

MR. SIESS: Okay.

MR. CLAUSS: Okay. And then another thing that I haven't mentioned yet, but as we did observe when we (inaudible) was that the sealed temperature got significantly less than the air temperatures. When the air temperature of 800 degrees Fahrenheit, the seal on the inner door, up until the point that we got leakage was only about 650, 660 degrees Fahrenheit. And there is significant temperature gradient along the line for the airlock. This is very similar to what was done in the EPAs and it's something that shouldn't surprise anybody.

The outer door only had temperatures in the neighborhood of three or 400 degrees Fahrenheit during this test 2C.

So the reason we never got leakage from the inner door, obviously, is that we never got temperatures sufficient to break the seal.

I guess that's essentially what I've been saying here.

MR. SIESS: That last statement is an artifact of your test story, isn't it? The pressure is equalized.

MR. CLAUSS: I'm sorry?

MR. SIESS: You say that probably -- because once the pressure is equalized, no further mixing took place.

MR. CLAUSS: Yeah. If you have a lot of interaction between the air at 800 degrees Fahrenheit and air in the chamber, eventually that air would get up to 800 degrees Fahrenheit.

MR. SIESS: And even when a million cubic feet inside, that would still be true?

MR. CLAUSS: Yeah, because there's nothing --

MR. SIESS: No pressure to drive it?

MR. CLAUSS: No pressure to drive it.

MR. MARK: In real life, though, that airlock would have been insulated passing through a wall or something.

MR. CLAUSS: Right. That's just going to reinforce the conclusion that the outer door will not see temperatures sufficient to degrade the seal.

MR. MARK: I was thinking the temperature difference end to end would be a different number. Might

be.

MR. WARD: It would test -- you get heat loss out the cylinder between the doors.

MR. SIESS: How much of the airlock is outside the containment?

MR. CLAUSS: In actual containments?

MR. SIESS: Yes.

MR. CLAUSS: It would depend on the type of containments there. If it's in the steel containment -- well, typically, the airlock is routed into the inside of the containment near the inner door. So everything beyond the inner door is outside containment or in the containment wall. And a steel containment, obviously, the thickness of the containment wall is small, so most of it is outside the containment. The reinforced concrete wall is four foot thick, so it's only going to be about five or ten feet of the airlock that's located outside the wall.

MR. SIESS: This says that there's really nothing that happens in a LOCA that would cause extensive leakage through this kind of a lock. Right?

MR. CLAUSS: Yeah.

MR. SIESS: And yet when I see leak rate tests, very frequently, they can't pass the heat rate test because the heat gets through the first airlock. And you go back and you find that somebody banged into a seal the last

time they hauled something through it.

Now, would that be this kind of seal or one of the other types?

MR. CLAUSS: No, it could be this type of a seal. But there's a big difference in what we consider a significant leakge from a severe accident. You have a small level that are allowable in an IRT.

MR. SIESS: I know. But --

MR. CLAUSS: But mainly our redegrading system wasn't capable of measuring leaks that --

MR. SIESS: No. But what I'm saying is, that although -- when the airlock gets in decent shape before the LOCA wil survive, that's what these tests tell me. It won't fail as a result of the LOCA.

MR. CLAUSS: Right.

MR. SIESS: But if they do fail, it will be because they weren't tight to begin with.

I mean, an airlock that won't withstand the 60 psig test without excessive leakage, do you think it's going to get better in that 300 psi?

MR. CLAUSS: Yeah, as a matter of fact I do because -- okay. A 60 psig, you haven't necessarily fully compressed that seal. And you --

MR. SIESS: Suppose the seal isn't there for six inches. That's the kind of cases I'm talking about.

MR. CLAUSS: The seal isn't there?

MR. SIESS: In cases where they try to run their heat rate test and they can't get any pressure in it. They go around and check, and here's a chunk out of the seal on the airlock, either somebody carried something --

MR. EBERSOLE: When he says a chunk, he means a chunk --

MR. CLAUSS: Yeah, it's not going to get any better if that's the case.

MR. SIESS: Okay. That tells us if we want to control that source of leakage, which is potentially a big one -- and I think if you look back at LERs in the heat rate test, you will find it's a frequent source. It's got to be done by inspection maintenance and procedures.

MR. EBERSOLE: Can't you put a pressure inside between the doors and sort of verify the condition?

MR. SIESS: Sometimes they do this testing beforehand. You know, this might not be an integrated leak rate test within the --

MR. EBERSOLE: Sure, just a pretest.

MR. SIESS: It's (inaudible) reservations. And you frequently see it happen. This gives me comfort. And if it's in good shape before, you know --

MR. EBERSOLE: It looks like a nice function of the outer door is simply insulation, thermal insulation.

MR. SIESS: Well, the NRC had some criterion that you had to test the airlock seals every time you went through it. And this was rather difficult to do because it always left somebody inside. You know, you had to do it by pressurizing between the two doors, which meant to put a strong back on the inner door and then somebody had to get out. And so they did let them test it by pressurizing between the seals.

MR. CLAUSS: That's right.

MR. SIESS: But this has been a continuing problem, but it's administrative procedure-type thing. And I think they recognize that.

And what about your last line up there?

MR. CLAUSS: Well, okay. This is a very recently completed test. I don't have the report ordered to do that. I don't have all of the data at this point. So that's one of the things that needs to be done yet, is to thoroughly look through all the data and get it reduced to the appropriate units and so forth to get in the report.

But once I have that report -- incidentally, I should have said a long time ago, this work was contracted out of the (inaudible) in (inaudible).

But once I have that report, then Sandia will start picking this up again and we'll be doing some post-test comparisons with the analysis. And I skipped over

the analysis, but I'm pretty sure that I have showed you that analysis.

MR. SIESS: But your analysis showed that there was very little separation due to pressure.

MR. CLAUSS: That's right. And that seems --

MR. SIESS: And so if the seals were even (inaudible) in there, they'll seal it?

MR. CLAUSS: Yeah, that's what the analysis said. And the part --

MR. SIESS: That's a function of the physical design of the door. It was that stiff.

MR. CLAUSS: That's right. It's going to be dependent on stiffeners.

MR. EBERSOLE: Chet, let me ask you: Is the test performed with both doors closed? I thought the original purpose of the doors -- the two doors was to cover for that small fraction of total time that a door would be opened when you had (inaudible).

MR. SIESS: That's right.

MR. EBERSOLE: But -- and I also thought that was really silly because that's such a small fraction of time. But then I find the real value currently is in the thermal protection you get from the outer door.

MR. BENDER: Two kinds of arguments. One is the argument that has to do with the fact that the

likelihood -- there are two premises involved. One is the likelihood that the severe accident will occur. And when that occurs, there is no reason to argue that simultaneously both doors will be open.

The other is the deal with the other kinds of LOCAs which may occur that may not lead to a severe accident where you want to have just the integrity of the containment.

MR. SIESS: Dave, the analysis predicted the very small openings. You haven't looked at the test data yet, have you?

MR. CLAUSS: I haven't been able to look at it in any detail, but I do know the gaps were small, on the same order of magnitude, at least. That was what we predicted in the analysis. Whether they track it with pressure very accurately, I don't know yet.

MR. SIESS: That's probably the main thing you're going to be looking at, isn't it?

MR. CLAUSS: Yes.

MR. SIESS: Now, was the seal material typical? I know it's typical to some -- how many different seal materials do they use on these doors?

MR. CLAUSS: I believe that most of the airlocks have the same sealing material. But there are differences in the cross-sections, the seals that are used.

They're not (inaudible). I think there are some with gumdrop seals.

MR. HORSCHEL: I guess when they first started making the double dogear seal, it was generally out of silicone rubber. But through time, they've changed them to the EPDM. I think almost all of them, through normal replacement of them, are now using the EPDM on the double dogear configuration.

MR. CLAUSS: At one point, there were some with silicone in them. But I'm fairly confident that there aren't --

MR. SIESS: Is the seal attached to the door or the frame?

MR. CLAUSS: Pardon me?

MR. SIESS: Is the seal attached to the door or to the frame?

MR. CLAUSS: The seal is in a group that's in the door.

MR. SIESS: In the door.

MR. CLAUSS: Uh-huh.

MR. SIESS: Any other questions?

MR. CLAUSS: Let me just say one other thing as far as analysis.

We did predict a small deformation, and given those small deformations, I think we would never expect

leakage out of the moderate temperatures. But there wasn't -- you know, we produced leakage here, and even that might be something you would never see in natural containment because by aging those seals so badly actually excluded this lift, if you will, into the gap between the door and the bulkhead.

Now, the double dogear seal is actually designed to fully compress within the groove. And if you didn't have this lift, and as you pressurized, before you compressed the seal into that group, then you would have good memorable contact. And even at very high temperatures, it's possible that you might not get leakage if that was allowed to happen.

MR. SIESS: What's planned for the future, any other types?

MR. CLAUSS: Well --

MR. SIESS: If the analysis works out, you're going on with the analysis?

MR. CLAUSS: That's right. And we will try it with analysis at other designs of a significant difference.

MR. COSTELLO: There is a population that came up a couple of years ago.

MR. SIESS: How good do you think the analysis has to predict that gap to be able to use it to

look at other types?

MR. CLAUSS: It depends --

MR. SIESS: You said an order of magnitude before, and I know you didn't mean that.

MR. CLAUSS: Pardon me?

MR. SIESS: You said an order of magnitude before, and I know you didn't mean that. That's a factor of 10.

MR. CLAUSS: Yeah.

It really depends on how large deformations are. But deformations are less than 10 mils, was what the analysis predicted here. The difference between one mil and four mil or five mil isn't all that significant. But when you start getting up to deformations on the order of the springback in the seal, obviously, it becomes very critical and you have to have very good accuracy.

Now, that only addresses (cough).

In the high temperature scenario, any small deformation will probably leave leakage. You don't have good metal-to-metal contact.

MR. SIESS: Well, now, you've made that analysis, and let's say that analysis predicts that the opening to be two mils at 300 psi. And when you get to looking at the test results, you find that it was 10 mils at 300 psi. Is that analysis good enough to use to look at

other airlocks?

MR. CLAUSS: No, it's not. We would have to go back and try to explain the difference.

MR. SIESS: You've got some idea of what's good enough?

MR. CLAUSS: Well, I would like to see at 10, 20, 20 percent or so.

MR. SIESS: Okay. Thank you.

MR. EBERSOLE: What percent of the seals are compressed? That's a fixed percentage compression, isn't it?.

MR. CLAUSS: No -- well, under what conditions?

MR. EBERSOLE: Under when you close the door. You compress the COA fixed percentage of its normal dimension.

MR. CLAUSS: No, that's really not true because the -- the seal is employed to cross into that groove just by closing the door. There's a latch --

MR. EBERSOLE: Well, isn't there a metal stop that says, "I want 25 percent compression," or something?

MR. CLAUSS: There are limits as far as what you can get. But when the door is simply closed, there's a latch that's compressant sealed. But it doesn't compress it because you get metal-to-metal contact.

MR. EBERSOLE: So you can oversweep it, or can you. Not overload it the next time you won't (inaudible) that tightly. What's the uniformity of compression; just a variable?

MR. CLAUSS: Well, we measured that during -- as part of this exercise. We started out with the seal before aging, closed it with the latch and measured the compression of various points along this privileged seal, and it varied. It varied anywhere from, I think, a tenth of an inch to about an eighth of an inch. That's not a big difference, but it does vary.

MR. SIESS: I think you said, though, that there was no seal. You got metal-to-metal contact. So there is no mechanical stop. Am I right?

MR. CLAUSS: That's right.

MR. EBERSOLE: Well, there's no reproducability of closure. I thought a good seal had to come to a positive stop with a known percentage of compression.

MR. CLAUSS: That's not the way it works. I think I would tend to agree with you, you should always have the same compression. But that's not the way it works.

MR. EBERSOLE: The operators, you know -- I don't know. Maybe a muscular man will do more than that --

MR. CLAUSS: Well, you know, the latch is --

MR. EBERSOLE: It looks electric.

MR. CLAUSS: Well, it's a roller with a (inaudible). It's not l. e the operator is going to buy this one off the floor. She needs to slide this --

MR. EBERSOLE: Okay.

MR. CLAUSS: So you should get uniformed core.

MR. CLAUSS: Fine, okay.

MR. EBERSOLE: But if you closed that latch without a seal in there, you said you would get metal to metal.

MR. CLAUSS: Well, the -- you will with pressure because the pressure is pressuring the door.

MR. EBERSOLE: After you took the pressure --

MR. COSTELLO: Right.

MR. EBERSOLE: Okay. Thank you.

MR. COSTELLO: Okay. The next presentation speaks more to work that's ongoing and projected for next year. It turns out together, we're going to -- one of the things we're going to have to defer until next year because of budget is work on inflatable seals, those other types of airlock seals.

And we're going to this year concentrate on scoping activities about, is there a potential failure modes for bellows.

Fred Parks has been working on this and will speak about it. Dr. Parks is -- you'll learn when I say here, I guess you haven't seen him before.

Seals And Gaskets, Inflatable Seals, and Bellows

MR. PARKS: As Jim mentioned, my name is Brad Parks. There are three topics that I'll be talking about briefly, all concerning the containment penetration work. The first is the seals and gaskets study. This study is pretty well completed at this time, but the testing has been done and a final report has been written.

The second topic says inflatable seals. We've done pretty much all of the preliminary planning and fabrication of test fixtures for the inflatable seals. There are things pretty well set to start testing and exact timing of when we do the test will depend on our future planning.

And the final topic is that of bellows. Bellows, we are just getting started looking at the problem of seeing if there is a probablem with bellows.

MR. EBERSOLE: Is this big structural bellows like they have in the boilers on the downcomers or.

MR. PARKS: Yes, for the Mark-I.

MR. EBERSOLE: Yeah.

MR. PARKS: That type of bellows, yes.

MR. EBERSOLE: All of those.

MR. PARKS: Right.

First, I would like to discuss a little bit about the seals and gasket tests. These tests were performed at two different locations, one being Sandia National Laboratory and the other one Idaho National Engineering Laboratory. And it just shows here, 22 tests were done at each location for a total of 44 different field and gasket tests. That means that all the testing has been completed. The final report from the Idaho tests were published in July of '87, and the final report for the Sandia test, which also includes a summry of the Idaho test has been completed and is currently under final review process.

Just to give you an idea of what types of things were investigated in the seals and gasket test, I listed the various test parameters that were looked at during the testing. The reason for selecting these particular type of materials and also these type of seals is that a study was done at Argon National Labs in which they surveyed many different types of containments and they found these materials to be the most prevalent, and in a vast variety of different contairments.

The three materials that were tested, the first one is the EPDM material, silicone rubber and the neoprene.

Four different types of seals were tested; the double O-rings, the double gum drop, the double dog ear and the double tongue-and-groove.

They also looked at three different types of aging on these seals. The first type of aging, if you want to call it that is this testing of the seals in the unaged condition; in other words, no artificial aging was applied to these materials.

The second group of aging on the materials was this thermal aging only, where they aged it at 300 degrees Fahrenheit for a week.

And then the final category of aging was thermal and radiation aging, where they aged it at 300 degrees Fahrenheit for a week and then 200 after that at (inaudible) per hour. Here again, we're just trying to simulate a 40-year life in the seals.

Two different types of environments were used during the seals and gasket test. First being steam. They also used -- some of the tests, they used heated, dry air.

The final topic parameter is the amount of squeeze in the seal itself during the test. This is a measure of how much the seal is compressed on the side of the test feature. The actual quantitative measure is the percentage of the original thickness of the seals that is compressed during the test. And it varied from 9 percent of the

original thickness to as much as what was necessary to get metal to metal contact. For metal to metal contact, it was about 25 percent.

MR. EBERSOLE: And the 9 percent, was that just established by dimension or by the mechanical load imposed -- you know, the type of pressure.

MR. PARKS: They have a test fixture (inaudible) so that they'll have a fixed gap in the test fixture and then write down the key plants.

MR. EBERSOLE: In actual practice, do they close seals that way or do they close them to some sort of static load level by adjustment of the cams?

MR. PARKS: That's one question I can't answer.

MR. EBERSOLE: Okay.

MR. PARKS: Okay. I'm going to hit the highlights of the test results.

All the tested seals failed at more severe pressure and temperature conditions than were predicted for the severe accidents of PWR and MK-III type containments.

Some of the tested seals failed at lower temperatures than predicted for the Mark-I and Mark-II containments.

Most of the failures occured at a temperature range of areound 500 to 650 degrees, which is haflway

between the predicted severe accident scenarios for the PWR Mark-III and that of the Mark-I and Mark-II.

From the results of these tests they couldn't find significant effect that aging had on the failure of temperatures of the seals. And finally, some of the Idaho tests were made in which one of mating (inaudible) services, at a angle to simulate flange rotation that might actually occur on -- due to rotation of the sleeve. This rotation varied from zero to 12 degrees. And the results of these tests show that the flange rotations didn't have much affect on the failure of temperature on the seals, but it did affect the amount of seepage before failure.

MR. MARK: You say they all failed under some conditions. Was it apparent that one of the materials was favorable compared to the others or one of the seal types was favorable, or did they all fail at exactly the same temperature?

MR. PARKS: No. As a general rule, the EPDM materials tested around 600 to 650 degrees.

MR. MARK: But you had three materials.

MR. PARKS: Yes, sir. (inaudible) materials around.

MR. MARK: Was one better than the others?

MR. PARKS: The failure temperature on the silicone was normally less than that of the EPDM. Again,

that depends on the environment. And the steam environment of the silicone is affected detrimentally, quite a bit. And the steam environment for the silicone seal has normally been around 500 degrees Fahrenheit. If you tested the same silicone seal in air, it would probably fail around 650 degrees Fahrenheit.

As a general ranking, EPDM was better than silicone and neoprene I think was down there around the same sort of behavior.

MR. MARK: Neoprene is the least favored.

MR. PARKS: I would think so, yes.

MR. MARK: And now, the dog ears and the other things, is one of them better than the other?

MR. PARKS: I don't remember seeing any trend saying that one was definitely better than the other.

MR. SIESS: Where are these used? Where are these seals used in a nuclear power plant?

MR. PARKS: This testing was a generic type testing, just testing the sealed materials.

MR. SIESS: I know.

MR. PARKS: As specifically used as the penetration or the --

MR. CLAUSS: To be used, like the tongue-and-groove and gum drop are designed to test and see if the dry wall (inaudible). You see gum drop, O-ring, I

think I even see some tongue-and-groove.

MR. SIESS: And hatch closures or mechanical penetrations.

MR. CLAUSS: This is for operable penetrations, I believe.

MR. SIESS: Operable penetration.

MR. CLAUSS: Includes hatches and air locks.

MR. SIESS: So there would be no problem of replacing a particular pipe if it turned out to be pretty bad.

MR. CLAUSS: In fact, that's already been done. There has already been several utilities that replaced silicone seals with EPDM.

MR. EBERSOLE: Are these used in the atmospheric relief and ventilation valves and monsters, you know, that are supposed to close but were found not to be able to if there was a LOCA there?

MR. HORSCHEL: The (inaudible) I'm not sure. Part of the electrical penetration, you see a lot more sealing compound or the (inaudible) is used. They have epoxy, they have flaps and electrical penetrations. I guess I'm really not that familiar with that phase.

MR. EBERSOLE: Well, you remember the big flack when we found out that these valves were running open all the time, yet they had no forcing functions obtained

closure if there was a preexisting LOCA because of the dynamic loads that would be obtained in that trenching and that's been fixed by partly closing the big ones and putting more muscle on the others. But they have to have some kind of seal -- I think these are butterflies, that will go through a real awkward sealing function.

MR. PARKS: I'm not familiar.

MR. HORSCHEL: As far as valves are concerned, the NRC does have a program looking at that. I believe that's --

MR. COSTELLO: Yeah, I was going to say that now I'm trying to recall the result of some tests we had done primarily by Idaho National Laboratory. We've had a series of tests that all the LOCA temperatures have been extended, as have accident temperatures. If you're interested, I could --

MR. EBERSOLE: No. I'm talking about the actual physical process of closing in the presence of violent winds which might even blow the seal out.

MR. SIESS: That's not a seal problem.

MR. EBERSOLE: Well, it might even blow the seal off, as far as I know.

MR. SIESS: This says seals and gaskets. Does this cover the gaskets that are used in mechanical penetrations? Are these kind of things we're talking about

here used to close mechanical penetrations?

MR. HORSCHEL: What is your definition of "mechanical penetration"? Personnel airlocks, putting the hatches down?

MR. EBERSOLE: Only those things, only the movable openable things.

MR. HORSCHEL: What if there isn't (inaudible), though?

MR. SIESS: You've asked a question they can't answer, but they'll look it up when we -- Jim will find out what the --

MR. COSTELLO: We'll have some tests on it -- I should have brought a copy of the --

MR. SIESS: I don't think those tests are on butterfly valves, though, which is what Jesse's talking about, three foot butterfly.

MR. COSTELLO: Yeah, three foot butterflies.

MR. SIESS: Are these the kind of seals that you use on the equipment hatch that you have in your model of the airlock and dry water closures?

MR. COSTELLO: The emphasis on the --

MR. SIESS: Dry wall heads, you said.

MR. COSTELLO: The emphasis on the operable penetrations changed several years ago, if you'll recall, and many arguments were made that began to point out, as you

tried to sort through the great population out there, that indeed operable penetrations were the most (inaudible) leak sources.

MR. SIESS: Okay. Very good.

MR. PARKS: Okay.

The second topic that I talk about is this inflatable seals. I suppose you could think of it as a special case of one of the steals that we've already talked about. To our knowledge, there's not any test data out there for inflatable seals, at least not in the bulk of the (inaudible).

There are normally used to prevent leakage around the personnel and the escape lock doors. They are currently installed, or at least planned for 13 different commercial nuclear power plants. All the installations are in either PWR or Mark III type containments.

MR. EBERSOLE: Can I ask a question?

MR. PARKS: Sure.

MR. EBERSOLE: These are part of a common general problem in that they are tied to a nonsafety grade air system unless there just models, which only have the capacity to fail in air pressure but can deliver contaminants, water, rust, junk and all sorts of things --

MR. PARKS: Inside.

MR. EBERSOLE: -- inside the steel. And

that's the common most failure that gets everything inside these air systems and it's under consideration, not physically, but for all the controllers, as being of serious deficiency in general design practice.

Did you look at the aspect of failure by the mode of delivering the wrong kind of air, or not enough air, or moderate air pressure, try not to seal, our whatever? In other words, did you look at the effects of failures in the air supply in both the positive and negative direction?

MR. PARKS: We have not started these steps yet.

MR. EBERSOLE: Okay.

MR. SIESS: That's not within your scope --

MR. PARKS: That is the --

MR. SIESS: -- to measure mechanical behavior of the seal itself?

MR. PARKS: That is within the scope and that's the primary objective of just looking into the canceled behavior of the seal and how it behaves for different internal air pressures.

MR. EBERSOLE: And air qualities.

MR. PARKS: Air quality, we haven't planned to look into, but maybe that is something we should look into.

MR. SIESS: Okay.

MR. EBERSOLE: Well, there is just still one air system up with 200 gallons of water.

MR. SIESS: Can you relate those 14 plants to either vendors or IME's?

MR. PARKS: I can relate them -- I can tell you what plan it is.

MR. SIESS: Yeah, but I'm wondering why only 14 out of 120 plants have chosen this particular system. It's all the Mark-III's.

MR. PARKS: Mark-IIIs and PWRs.

MR. SIESS: It's only two pairs of PWRs and the two ice -- two of the ice condensors.

MR. PARKS: Right offhand, I can't tell you why --

MR. SIESS: This is an A/E responsibility and not a vendor responsibility, isn't it?

MR. COSTELLO: Yeah.

MR. WARD: But they are not all the same A/Es because you've got Duke and TBA in there.

MR. SIESS: I know it. South Texas, that's an interesting decision.

MR. EBERSOLE: Are you going to look at this in the context of this system interactive effects from the air system?

MR. PARKS: Yes. I'll discuss this next one,

we've been on this one too long.

SOMEONE: This next framework is --

MR. PARKS: Just for your general information to let you know where these seals are located and what plants they're located in.

There is a total of 26 different locks out there so that it be personnel or escape locks, or they use inflatable --

MR. SIESS: Where they have used inflatable seals, they have used them for all the locks?

MR. PARKS: As far as I know. I don't know that -- I can't make that conclusion.

Let's look at a typical application of inflatable seals and a personnel airlock. Here are your two doors, here and here, for the personnel airlock. There is normally two different -- or two seals side by side; would be one here and one here, anything one the innerdoor lock.

A typical size for these airlock doors is about six and a half feet high by about three and a half feet wide. If you take those dimensions and calculate the total parameter around the door, it's about 240 inches. So we have a potentially large leak area around the door there.

There is a door stop along the two vertical edges of the door, on this edge here. You can see it better in the lower figure. It's about four and a half feet long on

each side. Here we can see the inflatable seals a bit better. We have leakage pass both inflatable seals, this one and this one (indicating), I suppose this door stop would inhibit some leakage from going past the second seal.

MR. EBERSOLE: Can I ask you a question. I have a strong suspicion these ought to all be outlawed, and I'll tell you why. I'm going to guess that you're going to find it's been postulated that the air supply, because of the disruption in the systems, would have been lost when you have an emergency, like a typical case is. So you will find a standby bottle on the side with a new check valve. It says, "Oh, now, go back on the bottle," but while you were normally running, these things are sustained even large (inaudible) by the capacity of the normal air system that will not sustain (inaudible) on a bottle. It will just go right away. And yet there is no monitoring of the reliability of the checks.

MR. SIESS: Jesse, if we could get them outlawed, we would save the money on this research.

MR. EBERSOLE: Might be the cheapest thing.

(Laughter)

Maybe we could get rid of PWRs, Chet.

MR. PARKS: Okay. The next point I want to make is what happens if we lose pressure inside of the seal?

MR. SIESS: How much pressure is there inside

the seal?

MR. PARKS: Typically, in a PWR, it's around 60 psi. That's the information that I get, yes.

MR. SIESS: And BWRs?

MR. PARKS: I said 60 psi. It's 90 psi inside the seal for the PWR.

For the BWR, I don't have that information right now.

MR. SIESS: So pressure inside of the seal is a great deal less than the outside pressure?

MR. PARKS: The severe accident pressure, yes.

MR. SIESS: And probably, the seal was designed for LOCA.

MR. PARKS: That I can only speculate; I would think so, yes.

MR. EBERSOLE: Well, then they are going to surely leak.

MR. SIESS: 140 on the other side of that seal.

MR. PARKS: Real quickly, a couple of other details. I'm still looking at this same door, this part of the detailed infraction and the section at the top of the door. There is nothing back here to prevent any leakage past the second seal.

So if we -- in the best case, we think this 240 inch parameter around the door, and say that the door stops are going to stop (inaudible), about 100 inches roughly, we've still got roughly 140 inches of the parameter that looks like this. If we lose pressure in the seal, we may have as much as three-eighths of an inch of gap between the seal and the door frame all the way around perimeter of the door.

MR. SIESS: That's a big leak.

MR. PARKS: That's my point. If we multiply this three-eighths times the uninhibited perimeter, we'll get about 50 square inch leak area. That's if we lose, totally lose air pressure in the seal.

MR. SIESS: Do the two seals have a separate air supply?

MR. PARKS: That's another question I haven't been able to give an answer to. I have asked that question, but I . . .

MR. EBERSOLE: Well, even if they do, they've got a check valve in the system that may leak, that separates them.

MR. PARKS: Again, for your information, to give you a feel for what these seals look like. You've normally got five and a half -- five and three-eighths inches wide, about one inch here, about an inch an a

quarter. The actual inflatable bars -- there are two bars in the field -- about three and a half inches by about an inch. All the seals that I know of anyway are made of EPDM material.

As I mentioned earlier, most of the pretest were kept. We have had a -- or actually two test fixtures fabricated to test the inflatable seals in. The figure that is shown here is the actual overall test chamber and this thing here is the actual test picture in place inside the test chamber. The shape of the test ficture is that of a short length of cylinder; it's about 36 inches on the outer diameter, and it's about 13 inches long this way.

The way this particular fixture works is pressure enters the fixture through 32, five-eighth inch diameter holes that are equally spaced around the bottom of the fixture. Pressure enters here (indicating). Once we get leakage past the first seal, we can measure that for leakage ports in here that are located between the two seals, at the --

MR. SIESS: Why don't you use the next slide? I think it's a lot easier to see.

MR. PARKS: Okay. There is just a section through the actual, through the test fixture. Again, this is where the pressure enters the test fixture. We could measure leakage between the seals here, and if we get a leak

past the safety seal, we can measure it and measure these supports around the perimeter in the upper end there.

MR. BENDER: What is it we're going to do with this data when we get it?

MR. SIESS: We're going to find his own work is (inaudible), Jesse says.

MR. BENDER: I asked a serious question. What are we going to do with it?

MR. PARKS: The main thing that we want to look at, and again I'll get to that in couple of slides, is to see how much pressure we need inside the seals so that they don't leak for a given amount of external pressure out here. See if we've got 90 psi in the seal, how high could this external pressure be before we get leakage? Okay? That's the main number that we're looking at.

If it's possible, in the event of a severe accident -- and again I say, if it's possible, it might be that we're being -- this is being very optimistic, I think. If we could increase the seal pressure and the seals to make it 150 psi, we might could withstand or withstand the pressure -- this pressure boundary in the event of a severe accident. Whereas, to leave it at 90 psi and the external pressure during the severe accident reaches 120, we might --

MR. EBERSOLE: Who set that as an objective,

did you all or did NRC? My objective would be to throw them all away on the face of what they are.

MR. SIESS: You're not in research.

MR. EBERSOLE: Well, I guess that's one of the reasons that I don't want to be in research.

MR. BENDER: What is your basis for the design principle to design something like this, to design a seal that had to maintain a certain differential within the internal pressure and the seal in research. And I'm wondering if something like that was behind this.

MR. COSTELLO: Well, Mr. Ebersole has a legitimate point of view in question, but I guess, you know, we got to this after we got, you know, to the (inaudible) because these are identified as having potential when subjected to --

MR. EBERSOLE: Even under the best of operational conditions.

MR. COSTELLO: And of course, you know who's doing it, but that's the decision about whether we (inaudible) or should have a particular (inaudible) having a regulatory decision and we feel could stand problems. What is learned in the research? I don't know exactly how I feel about it, but that just as (inaudible) to some of the (inaudible) structural design of some parts of the containments which are tolerant of overpressure,

overtemperature, so well may be these. But I don't think you know well enough to say how much.

MR. SIESS: Well, if you find out by increasing the pressure inside to 180 psi, say, you could withstand the same pressure that the containment would take under 140, 150. This is something regulatory people could decide to require, assuming that that seal can take that pressure and the system can be used to supply it.

If you find that it ain't going to leak in a severe accident no matter what you do, then it's a question for regulatory to decide that something has to be done about it. But if you're concerned, as Jesse is, with the reliability of the air system, either normal operation or following a severe accident, that is a very good question; unfortunately, it's not one that the structural engineering branch is going to be able to answer.

MR. COSTELLO: Yes, sir. But --

MR. SIESS: Maybe it should have been asked before you started this stuff. But --

MR. COSTELLO: Well, again, there is a certain presumption that no idea have even be taken for conditions for which there are no regulatory structure -- i.e., LOCA conditions -- then it would be there, all you would need to do is build it.

MR. EBERSOLE: It's a classic example of

compartmentalization of function.

MR. COSTELLO: I guess, you know, I would certainly go along --

MR. SIESS: Jesse, what should be done as part of the IPE, the individual plant examination, is reliability of the air system for pressurizing these seals should be a pa-t of the picture. And if they can't prove that is reliable, or if these seals at 60 psi are not going to take accident pressure, that's an IEEE vulnerability to one of the mitigating --

MR. EBERSOLE: Isn't it true that the seals will have to exceed containment pressure in any case?

MR. SIESS: That's what --

MR. PARKS: That is the feeling right now.

MR. WARD: No, not necessarily. It could extrude up there and seal, couldn't it.

MR. EBERSOLE: Well, anyway, I was about to say to foreshorten all this work, you could just first pump the seals up to the pressure you need to hold them at the maximum containment pressure and see if they burst and I suspect they will.

MR. PARKS: The design pressure is 150 psi. The internal pressure --

MR. EBERSOLE: Is 150.

MR. PARKS: Yes, sir.

MR. WARD: Who is the manufacturer of these seals, Brad?

MR. PARKS: Press Ray.

MR. WARD: Well, what do they have to say about it; do they have a body, a testator?

MR. SIESS: What did you just correct to say 150 psi?

MR. PARKS: The internal design, the internal design pressure or internal pressure that these seals are designed for is 150.

MR. SIESS: That is what they are designed for?

MR. PARKS: That's what they're designed for, I don't know --

MR. SIESS: No margin or is that with margin?

MR. PARKS: That is what they tell me it's about.

MR. SIESS: The manufacturer says you can operate them at 150?

MR. PARKS: Yes. He says this is the design pressure.

MR. SIESS: But they don't --

MR. PARKS: I don't know.

MR. SIESS: Plants don't use 150.

MR. PARKS: No.

MR. SIESS: Okay. So if you found out that you had to go to 150 to keep these things from leaking, the manufacturer says you could do it?

MR. PARKS: That's the understanding I have, yes.

MR. SIESS: Now, whether you get the 150 from a reliable supply is another type of question.

MR. PARKS: We talked about this a little bit and I guess we'll go back through it. Okay.

The primary objective as we see it right now, at least from the adequacy of the seals, I guess, is to determine the required difference in the seal and containment pressure, called delta here, to prevent leakage.

(Inaudible) the on psi, (inaudible) seals how high can the containment pressure go on before you get leakage. That's objective No. 1.

The second objective is to get some sort of measure of. We have for a smaller difference in heat pressure.

Finally, this is a very (inaudible) test matrix that we have right now. The plant analysis, there are four different inflatable seal tests. Two using unaged seals and two using aged seals.

The test number of the aging that will be applied

to the seals is still a little bit up in the air.

MR. SHEWMON: You measure the lobide pressure or what?

MR. PARKS: We will measure those -- run a pressure check to see amount of leakage for a certain given differential pressure between the seal and the external.

MR. SIESS: Always with air?

MR. PARKS: That's the current test plan, yes. The reason for going with air instead of, I suppose you're thinking steam?

MR. SIESS: Whatever might be in the containment, yeah.

MR. PARKS: The EPDM materials placed on the seals and gaskets have simply limited, similar behavior, whether it's air or steam.

MR. SIESS: Even in its response to temperature?

MR. PARKS: Yes.

MR. EBERSOLE: What does EPDM mean?

MR. PARKS: Ethylene Propylene Rubber, or whatever.

MR. SIESS: Go back about four slides and spell it out.

MR. EBERSOLE: That's okay. I missed that.

MR. SHEWMON: You've told him all he wanted

to know.

MR. PARKS: The first two tests would be done using air pressure and temperature; then the second two tests would be done using the same sort of steam as the first two tests except the temperature would be increased to 400 degrees Fahrenheit.

MR. SIESS: What duration?

MR. PARKS: The duration of the test?

MR. SIESS: Yeah.

MR. PARKS: It would be as short as possible. We planned to do certain gyrations for the internal steel pressure and the external pressure.

MR. SIESS: Well, 400 degrees, if it's only on for 10 minutes, does it really make that much difference? You won't get the degradation -- oh, some of them will already be aged.

MR. PARKS: Yes, sir.

MR. SIESS: Then what's the reason for the temperature difference if they are already aged and they won't really age any more during the test?

MR. PARKS: The thing is, is the material may soften, mechanical, computerial properties may soften somewhat due to the operating temperature and that may implant them --

MR. SIESS: See if you can determine that

without the benefit of this test period and could do it in a testing machine?

MR. PARKS: We could determine the amount of salt but I don't know if we could determine how that infringes the leakage though.

MR. SIESS: Doesn't the aging involve temperature?

MR. PARKS: Yes.

MR. SIESS: If you age them for 168 hours at so many degrees temperature in the amount of 15 minutes makes any difference or . . .

MR. WARD: No, that's not his point. It's just a seal temperature, whether it's been aged or not, is going to have different properties.

MR. SIESS: Might have a different.

MR. WARD: The last one is 400 degrees F.

MR. EBERSOLE: On the matter of properties, this stuff is what you call tightchested; that is they batch test a batch of this and make a bunch of seals. Do you go back and determine the Q/A on the batch-testing process? You don't do the reliability of the recipe?

MR. PARKS: No.

The last --

MR. SIESS: What's the status of those tests?

MR. PARKS: Okay. All the preliminary test

planning fabrication of test fixtures have been done. I guess the current status is they are on hold pending additional funding.

MR. SIESS: Oh, they have, that's for our '88 budget?

MR. COSTELLO: No, sir. We're not overbudget. This is for '89.

MR. PARKS: Okay. The last topic is that of bellows. As I mentioned earlier, we have just begun looking at bellows. At this point we have done a preliminary fairly comprehensive, I guess, of available technical literature on bellows. From our surveys we've done, it seems like there is two main categories of bellows that occurred at containment penetration. The first type is a vent line bellows, only under Mark-I containments. These bellows are anywhere from 65 to 90 inches in diameter, a very huge structure.

The other common type of bellows, what I already categorized as process piping bellows, they are normally from 18 to 36 inches in diameter and normally occur at the penetration for this steam and feedwater lines.

MR. SIESS: What material are these?

MR. PARKS: Stainless steel.

Take a look at a typical Mark-I containment (inaudible), each of those past bellows that I've described.

The vent line bellows occur at the actual penetration of the vent lines into the suppression chambers. And that would be in this area here, over here and there is normally eight vent line bellows around the perimeter of the containment.

Also, as I mentioned, you also have bellows at these penetrations for the feedwater and mainstream bellows.

Here is a typical detail of a vent line bellow which penetrates the suppression chamber at around 55 to 90 inches in diameter, normally, two bellows, side by side here.

MR. SHEWMON: Why isn't that symmetric top to bottom? That's that top horizontal member outside?

MR. PARKS: This (indicating)?

MR. SHEWMON: The straight line, right by your fourth finger up there.

MR. SIESS: Right under expansion bellows.

MR. PARKS: Right here?

That's an error in the drawing. It should be --

MR. SIESS: You've got the same error four times on the next one.

MR. WARD: That's a sleeve around the bellow section?

MR. PARKS: This is the projected shroud around the perimeter of the bellows.

MR. MARK: Is that bellowed material metal or

something of plastic?

MR. PARKS: Stainless steel metal.

MR. EBERSOLE: There is nothing to keep flying articles from coming from the inside of the suppression chamber and coming down and intercepting the bellows, is there?

MR. PARKS: Not that I'm aware of.

MR. EBERSOLE: If I drop a wrench down there, I'm not going to get normal bellows performance, am I?

MR. PARKS: There's nothing to stop it from going downward.

MR. EBERSOLE: You have other protective guards there? It is a downward tilt, and the first thing that would stop it would be a convolution.

MR. PARKS: Probably.

This is the other type of bellows, as typically found in penetrations, that is the process pipe inside metal. Here they have a protected shroud between the process pipe and the bellows just in case the process piping burst, so that you don't damage the bellows here. Then you see the outside or external (inaudible) goes around the perimeter of the process pipe.

MR. SIESS: Both of those are BWRs. Do you have similar things like this for PWRs?

MR. EBERSOLE: Well, the difference there is

that one is going to move a lot, isn't it, in contrast to the suppression group bel.ows; that is going to move with up and down temperature of the steam pipe? That's what it's there for, yes. But the other one is just there; but this one is going to work.

MR. PARKS: Well, in the event of a severe accident, the other one --

MR. EBERSOLE: But that's a one-time shot.

MR. SPARKS: (Inaudible) during normal operation.

MR. EBERSOLE: So what's the fatigue line for the convoluted?

How do you monitor it?

MR. PARKS: That's a good question.

MR. EBERSOLE: You have got covers on it.

MR. SHEWMON: Is that containment?

MR. SIESS: Yes, sir.

MR. SHEWMON: I don't see containment leaks up there.

MR. PARKS: Finally, if we look at this, this is a very tentative type of test sequence that we may or may not employ, depending on (inaudible) future work. We may, for the future, with additional investigation, determine that a test is not (cough). If we do a test, follow this sort of sequence here, start off with axial compression of

the bellows, with axial compression only, apply internal pressure only or bending the bellows. Also lateral deflection. Each one of these independent of the other. And at each point, compare what the results of our test data with the results (inaudible).

MR. SIESS: Will these be cycle loads, fatigue loading or is this just . . .

MR. PARKS: What the plans are now is it would be either -- no it would not be a cyclic load, it would be to apply a given amount of deformation and measure the strength compile more deformation and do the same thing.

MR. SIESS: Just measure the load deformation characteristics onto those different loads?

MR. PARKS: Yes.

MR. SIESS: What is that doing -- oh, just to check out your analysis?

MR. PARKS: Right.

MR. SIESS: But then that has not failed. How does the bellows fail? That's what we are interested in, isn't it, the failure of the bellows?

MR. PARKS: That's exactly right.

MR. SIESS: How do bellows fail?

MR. SPARKS: By any of these --

MR. SIESS: How do they fail? Back off from the slide and tell me how they fail? What happens to them?

MR. PARKS: That depends on the loading. An axial compression, it may be by bending of the external convolutions.

MR. SIESS: Push it together till it convolutes and cracks.

MR. PARKS: Right. By internal pressure, it's -- the bellows are expanding and again you would get a rupture most likely of the internal convolutions from buckling and cracking.

MR. SIESS: Each one will be tested for failure of that mode?

MR. PARKS: Well, again, if I may be very candid, that's not the current plan. This is to check out our analysis, mostly. We're still trying to stay elastic.

MR. SIESS: But your bellows is then, what, trying to produce deformations, not failure?

MR. SPARKS: At this point, yes. If we move down to Item E here, then we would apply some normal design combinations of A, B, C, and D. Okay. And again check out, we wouldn't expect failure there to check out our analysis. And then finally, at Item F here, we would apply a severe accident combination of the first four items. Exactly what amount of actual deformation internal pressure that we have in a severe accident, we would have to determine over another global analysis of the overall containment, I would

think.

MR. SIESS: So you're going to do an elastic analysis and validate it by making an elastic tests, tests within the elastic range.

MR. PARKS: Basically, yeah.

MR. SIESS: Solely until the analysis, modify the analysis, predict the elastic behavior; and then you're going to test it for failure, and then go run the analysis then because obviously it doesn't fail elastically, does it?

MR. SPARKS: No, I wouldn't anticipate elastically.

MR. EBERSOLE: Let ask you --

MR. SIESS: I don't see where you are going?

MR. EBERSOLE: -- about, in view of the fact we still live right now with pipe failures, if you have a mainsteam failure or people with pipe failure, these things don't take any (inaudible) to speak of. Are there other supports that constrain the motion of the pipe so you don't have to ruin these?

MR. PARKS: Not that I'm aware of, no.

MR. EBERSOLE: Well, what happens to these delicate bellows if you have a pipe break? Does it destroy the containment and who cares? I suspect that's the case, isn't it.

MR. COSTELLO: That's the reason of the guard

pipes.

MR. EBERSOLE: Now, that doesn't take the load off. I'm looking at the guard pipes here, the load, the motion would be seen by the bellows; the guard pipe just fits the water back into the containment, it doesn't carry a load other than that. If you look at the fourth lines, it's straight through the bellows. But there may external brackets out here, or ears on the pipe to minimize axial motion. I don't know. Do you look at that sort of thing, performance of the bellows in emergency modes like that?

MR. PARKS: We will, but sometimes we --

MR. EBERSOLE: We just got started.

MR. COSTELLO: Well, he's a little premature to put the --

MR. SIESS: What Jesse is talking about is loading. You come down to severe accident combinations, you know, what is the loading in a severe accident. It's not just pressure.

MR. EBERSOLE: Really not a severe accident, Chet, it's just a routine common design.

MR. BENDER: I think it would be a good idea for you to talk to one or more of the architect/engineers about how they will use these. Most of these arguments that are coming up here are not new questions. They have been around for a long time. And there are some design

principles involved in how the bellows are installed. And also some looking has been done, hard pressure releases into these small areas where the bellows fit around the guard pipe. So, you might be well advised to get that information before you take off on your own.

MR. EBERSOLE: In regard to BWR in particular, these are stainless steel, you said?

MR. SPARKS: Yes.

MR. EBERSOLE: So the old stress corrosion matter now is enhanced because of the thin cross-section.

MR. SIESS: I think you have two things there. You also had some low cycle fatigue on these things as they operate. They may be degrading.

Under normal design combinations, that's not just normal temperature, that would include a pipe break like this (inaudible).

I mean, pipe breaks come with a normal design. Plants are designed for pipe breaks all over the place; up to and included through the double ended (inaudible).

MR. EBERSOLE: What keeps this fluid being -- will keep the steam off of the bellows.

MR. COSTELLO: I guess the point that drives all this is that there is a concern that all bellows are a (inaudible) after all. Axial motion that some large deformation associated with severe accidents, loading, may

well impose transfer shear and the --

MR. SIESS: Suppose we could design a containment that would take 4 or 5 percent strain, does that have to be absorbed in these bellows?

MR. COSTELLO: Yeah.

MR. SIESS: If this things moves out a foot?

MR. COSTELLO: Yeah.

MR. HORSCHEL: Was it solely the bellows; didn't it also have a convoluted pipe test --

MR. COSTELLO: Right. A little bend of a pipe.

MR. SIESS: And some of them have a pipe restraint, too right around the bend.

MR. HORSCHEL: That is going to really vary from containment to containment. It's not a generic thing; you're going to have to --

MR. SIESS: But you could get very large axial deformations under a severe accident condition. You can get very large lateral deformation from the broken pipe conditions, I guess.

Now a pipe break, if it doesn't lead to a severe accident, we're not all that worried, not that much radioactivity.

MR. COSTELLO: I guess you're aware, as far as dog ears taken out, they begin, (inaudible) I guess where

to point out a number of things, (inaudible) into it, trying to get into the process of what these things are actually designed to tolerate. And then, if there is a question about whether an analysis can take you from what they are designed to tolerate to what exactly they want to (inaudible), then, if the analysis won't carry it, we may have to do a test.

MR. SIESS: Now, if that penetration is a pipe through a PWR, through a containment, does that pipe have an isolation valve? Doesn't it have to have an isolation valve?

MR. EBERSOLE: Yes, they've got isolation valves. It occured to me when I look at this, if you have a failure upstream on the mainsteam isolation valve, you still may tear a hole out of the containment.

MR. SIESS: On the upstream pressure?

MR. EBERSOLE: Unless you fix the pipe so it won't move, tear out the bellows.

MR. SIESS: Then you have a downstream isolation value; but if the bellows fails, it doesn't do any good. Right? The only way you can isolate a bellows failure is with the upstream valve.

MR. EBERSOLE: Well, Chet, if the failure varies in front of the upstream valve, you will get an axial load on these bellows unless it's fixed by mechanical lugs

or stopped somewhere besides the bellows. Otherwise, it's got to take the thrust. And I suspect it will not do it.

There must be anchors. I don't know that, but . . .

I can't imagine --

MR. SIESS: He's figuring out what the scenario is for this because, you know, let's face it, if the steamline (cough) BWR, that's really not a severe accident yet.

MR. WARD: But I think Mike's got a point, there probably has been some looking at scenarios and against the performance of the bellows, and you might not have to start from the ground zero to figure this out.

MR. SIESS: I'm not sure you're going to find different mechanical scenarios for a severe accident or for a normal accident, except if you can let your containment move a lot. Now, we limit it to a 2 percent strain and we took the studs out, it might go to 6 percent strain, and that's a pretty good piece.

And you're limited by the yield strength to the pipe, aren't you?

That's pretty large compared to the bellows, isn't it? If you straighten those bellows out, you're not going to take the yield strength of the pipe.

Has there ever been a bellows failure?

MR. COSTELLO: Not a bellows.

MR. WARD: Of any sort?

MR. SIESS: Well, we had some in the condensor system, but I thought those were some sort of (inaudible), not metal.

MR. BENDER: Research water system in the condensor water system.

MR. SIESS: Yeah. Was that metal?

MR. BENDER: No, it was rubber.

MR. WARD: In other industries, there have been failures of stainless steel bellows. I don't know about any of this size, but 16 inch.

MR. COSTELLO: That was one thing we would like to talk with you because one of the paths was (inaudible). One of the paths that (inaudible) is experiencing in the process industries. And it's a good source of work for (inaudible), of that kind are recorded, or has it just arrived?

MR. WARD: I don't think there's any second opinion of good sources.

You know Rodebaugh is -- you know him?

MR. COSTELLO: Yeah.

MR. SIESS: He's probably got a handle on more of that sort of stuff than anybody else.

I think that this covers everything you had had on

your list except the steel containment and seismic capacity. Am I right?

MR. COSTELLO: Yes, sir.

MR. SIESS: And as you pointed out, we had talked from time to time about this item that appeared on our agenda of the applicability of moc∍ling and testing for full-scale containments.

MR. COSTELLO: Yes, sir. I had prepared -- well, there are two aspects we were going to speak to that. One was to have Mr. Clauss talk about what has transpired in the application of Sequoyah, which is something that is, in fact, done. And then you're going to be treated. That's not why we're leaving a track of how it was going to work just as well for concrete containments. Sometimes if you would care to see some facts or some (inaudible), we would be pleased to accommodate you.

MR. SIESS: I think it's getting pretty late to talk about that now. Maybe we can have a meeting sometime in Washington?

MR. COSTELLO: Yes, sir. I would be glad to.

MR. SIESS: It seems to me that it's still not entirely clear what the real questions are that we're trying to get at here. And how are we going to use this stuff for managing severe accidents.

Obviously, we're not going to use to prevent any

accidents. Use it for venting releases hopefully, but not for preventing severe accidents. And I hear of different things from different people. And as you say, NRR doesn't really know what it wants.

MR. COSTELLO: I didn't say that, sir. I said it's hard to know who NRR is because the generic issue of NRR is now exposed.

MR. SIESS: I think that there's going to be a real effort made by the research to get together with whatever levels are necessary, including NRR to see just what you're going to be able to do with the information you've got or what kind of information they need. I think they need to manage severe accidents. And if the PRA people since they want to know the ultimate, structural capacity with uncertainties, you know, that's almost impossible to answer.

MR. COSTELLO: I would say, to answer in the way a structural engineer like myself would like to answer a question. However, when you have a caveat with uncertainties in there, our recent experience and interactions on the 1150 exercise has shown that the -- an answer which is -- well, it could be Mode A or Mode B with ranges of P plus or minus 20 psi, and some weighting for Mode A versus Mode B based on the calculations that you feel you can do, suffices, it seems, for the risk analysis

purposes because that's enough -- that fits in the grinder and it seems to suffice. So that's comforting.

MR. SIESS: But, Jim, I think any competent structural engineer could look at what you've done analytically, what you've done experimentally, and maybe what the British have done and Canadians have done, and to give a number with a third degree of confidence with a good lower bound figure, like -- you know, pick 125 off of the curves or whatever. But I don't think any structural engineer, average design-type structural engineer could give with any confidence the level at which it will fail for the structural integrity will be lost.

MR. COSTELLO: That's true, but that's not the question that has been asked by the PRA people. They don't want you to --

MR. SIESS: I think that's what ought to be re-examined at NRC because if they start going into a research program that wants to establish the mode at which structural integrity will be lost, this is a program which, in my mind, will approach that we spent on LOCA ECCS.

MR. COSTELLO: What do you think they're asking for?

MR. SIESS: That's what he thinks they're asking for.

MR. CLAUSS: There are two people we're

trying to address ourselves. And from what I hear from this group today, generally, what you see as real value in this program is addressed to the management (inaudible). And for that, we need to know with confidence the pressure at which the containment will not fail. And that's all we need to know. And that's a pretty simple problem. But we're trying to go beyond that to address ourselves as a group. You're supposed to do this in PRA. And right now (inaudible) about 1150. And we have been asked to help those people and we have interacted with them.

They want to know a lot more than (inaudible). They need to know a range of failure pressure, they need to know what their mode is. In a BWR, for instance, it's important to know whether the failure involved bypass or not because that has a source term. So they need to know a location of failure, they need to know what the range of pressures is that that failure might occur over, and they would like some failure size.

MR. SIESS: Well, let's take those questions, and what the next step should be in having you research is to look at what research would have to be done to answer those questions.

MR. CLAUSS: Okay. You want to answer the questions with certainty.

MR. SIESS: What's the probability in trying

to search for these successful and what's the cost?

And my feeling is that somebody has got to -- somebody else has to do it.

The cost will be high to get that probability of success (inaudible) at an acceptable level. It may be much too high to justify.

Now, this is an approach I've seen other people use. They've looked at the problem, laid out a research program, evaluated the probability of that program succeeding and estimated the cost. I think that lowering the level of -- raise the level of confidence to something that's meaningful, it would be a tremendous (inaudible) because that depends on details. And there are too many differences of details in the containments around here now.

MR. CLAUSS: I think we've already raised it to a level such as (inaudible). For instance, you know, you need to make a qualitative judgment. We're not asking for a determinant number of, tell me exactly what the pressure in itself. That's not what they need. They need value judgment. They need to know, is there a chance that equipment that has leakage could occur before close rupture of the shell.

MR. SIESS: Sure, there's a chance.

MR. CLAUSS: Well, we could have found out there was no chance. We could have found out that these

structures fail always at 1 percent strain. You don't develop large enough deformation to (inaudible) go ahead and find that.

MR. SIESS: Of course, you're talking about when it doesn't fail.

MR. CLAUSS: No, I'm not.

MR. SIESS: You're on my side.

MR. COSTELLO: Well, as I say, the one thing will surely happen before the other. You could say that with confidence. Now, I think -- let's look back on the scale. I don't want to burden the Subcommitee too much, but as far as the research -- as far as the steel part of it is done -- I say done is done. I say we have demonstrated that as far as the PRA kind of questions that are being asked now, we've learned enough to be able to satisfactorily provide input for that mil. And I think (inaudible) the other side of town earlier this week with the exercise numerous experts who used what they learned from the tests, applied their judgment for certain specific plants in the exercise.

MR. SIESS: What could you say about when a steel containment will fail?

MR. COSTELLO: What can we say?

MR. SIESS: Yeah.

MR. COSTELLO: We can say that if you've done

an axisymmetric analysis and if you examine the details and found any weak links, you can check the weak link against --

MR. SIESS: Being a (inaudible) weak link?

MR. COSTELLO: Sure. A nice healthy strain concentrator, and we know what to look for by now.

MR. SIESS: Now, having found that strain concentration, you can predict what it will fail at?

MR. COSTELLO: You can have a pretty good choice of chasing that to see if something else will fail before that. You can also have a pretty good idea how -- to though check whether you would, it would at least allow you or not.

And I think -- it's not something that can be done on the back of an envelope. But neither in the likely use in the IPEs, should be back of the envelope. Utilities are going to reasonably be asked to look at their own (cough). The tools are out there so that it can be done.

MR. SIESS: Do you think you can give them a list of things to look for?

MR. COSTELLO: Oh, I think those who have been aware of what is going on and have followed results have figured it out pretty well themselves.

MR. WARD: Well, can you give them a better list than you could have five years ago.

MR. COSTELLO: Yes, sir. And we're not quite

there at that stage on the concrete containments, but I hope we will be.

MR. SIESS: In the IPE, they're going to predict a failure load.

MR. COSTELLO: Or a nonfailure load.

MR. SIESS: Are you accepting either one?

MR. COSTELLO: The good part about the nonfailure load, if you know more is that you have more confidence that you haven't missed a failure load.

MR. SIESS: With respect to the failure load down to zero and the level of confidence leaving out open valves and things of that sort, the higher you go, the broader the band becomes.

MR. COSTELLO: The band doesn't come at a broad -- they don't -- it's not a continuous function from zero. In fact, you start getting certain jumps in there. And when you get out well in the inelastic range (inaudible) to widen.

MR. SIESS: That's the part I'm concerned about.

MR. COSTELLO: But --

MR. SIESS: My idea is that you stop at the point where the band begins to get large, when new confidence begins to get lower. That's what I'm talking about, high confidence and no failure.

MR. COSTELLO: I think for the (inaudible) management, that's exactly what you want.

MR. SIESS: Yes.

MR. COSTELLO: But, again --

MR. SIESS: But when are you going to give the PRA? Are you talking about IPE, down at PRA.

MR. COSTELLO: Uh-huh.

MR. SIESS: Are you using those interchangeably?

MR. COSTELLO: I believe the IPE --

MR. WARD: That's what it is.

MR. COSTELLO: Yeah. We'll be a (inaudible) exercise. And I think, again, I've -- I'm not going to stand here in warranty that the IPE activity will be that -- will be the salvation of the republic.

MR. SIESS: What would you like? How would you calculate the failure pressure of a containment that had channels and angles instead of studs?

MR. COSTELLO: Well, see me next year. But I do believe -- and the (inaudible) will swear on a stack of bibles -- that the channels and angles are -- would give you the machine failure mode even better than a stud.

MR. SIESS: What's the machine failure mode?

MR. COSTELLO: (Inaudible) You know, these are the ones that's been arguing for years. His argument is

that rigid -- more rigid embedment will lock that end of t plane even tighter than (inaudible) concentration. That's his argument that remains to be seen, and there has to be some more analyses on the checking of his calculations.

MR. SIESS: Want to block it in the (inaudible) row of studs of two inches? I doubt if they block it much more than that (inaudible) two inches, it produced a tear.

MR. COSTELLO: I simply respond to your question than that. But I think we're sorting out.

MR. SIESS: So if I had an angle like the location of that row of studs, you would say, what, 2 percent straining it will fail?

MR. COSTELLO: I wouldn't be too premature until I saw some calculations, but I believe that's one of the things we need pursuing.

MR. CLAUSS: If you give a range, though, you're central (inaudible) around 2 percent straining, that pressure correspondences to 2 percent strain in the field. But you don't have confidence in that, to say that's the number.

It could go to 4 percent (inaudible) or it might go at a half a percent.

MR. SIESS: You know, most engineers when they run into a situation where they have low confidence,

they try to get around it by doing something --

MR. CLAUSS: If you're denying structure, yes. But that's not what we're trying to do. We're not trying to design.

MR. SIESS: I'm trying to look at the containment capacity and saying I have no confidence in the ultimate capacity, ultimate structural integrity level, so I'm going to try to pick a number that I do have confidence in and learn to live with it.

MR. CLAUSS: For (inaudible).

MR. SIESS: No, again. (Inaudible) it in deciding on whether plants are safe, deciding on whether they need the (inaudible) program, and then I'm going to look at a high confidence, low probability of failure and base my regulatory approach on that.

MR. WARD: In deciding when a plant is safe, you need to know about the failure modes. What's safe?

MR. SIESS: But if I know the failure mode with a very high uncertainty, what do I know?

MR. CLAUSS: If you're going to -- you know more than you did before you did that.

MR. SIESS: No, I don't. You just think you do.

Give me a (inaudible) up here with a spread on it like that at a confidence level of 20 percent, (inaudible).

You're just kidding yourself. You've got a number, but low confidence. That's no number.

MR. CLAUSS: Chet, you (inaudible) have to know something about the failure mode. Wouldn't you agree that --

MR. SIESS: There's no failure and level; they're two different things.

MR. CLAUSS: Okay. Let's talk about the failure mode then. How do you approach that?

To understand the failure mode, you have to make some assumptions about the failure level.

MR. SIESS: No. I look at only the mode. If I have confidence that the failure in the mode is going to be a leak before break, I might be willing to raise -- lower my confidence level on my value I want to look at. And if I think it's going to be explosive, I'm going to work a lot lower than that, higher confidence at a lot lower level. That's how the mode is affected, I think.

MR. COSTELLO: I think there ought to be --

MR. SIESS: Well, not structurally, but regulatory.

MR. CLAUSS: If you do that in a safety context where you're trying to evaluate safety, you're really biasing your results. Because if you do that in a safety context, you're biasing your results completely.

If you think it's going to be ruptured, the ability of failure is ruptured, and you give yourself that much more modeling, well, you may be asking that --

MR. SIESS: But we're doing this all the time. We let an uncertainty dominate the whole system. If it's a high uncertainty, we're more conservative. We're looking at a NUREG-150 with uncertainty bands like this and (inaudible). But they don't know. They look at the bottom. Watch them.

MR. COSTELLO: I guess the only suggestion I'm going to have is again my suggestion that the venting scheme is being developed in Germany where they're proposing letting go at not much more than design pressure, notwithstanding the fact that 150 percent of the design pressure is given.

MR. SIESS: And they don't seem to need to do research.

MR. COSTELLO: They have.

MR. SIESS: They got our high confidence.

MR. COSTELLO: No. In fact, I'll make this statement. The principal beneficiaries of the USNRC and research program on steel containments were the Germans.

MR. SIESS: Sure.

MR. COSTELLO: They were the principal beneficiaires because they put their own resources in to

apply what the test showed to their only containments. And they put the money in and did the work as opposed to standing and waiting.

MR. SIESS: We did some tests on the (inaudible) site that showed that when the thing went out a foot or two and hit something and it ruptured. But there are regulatory philosophies that severe accidents are residual risks. Let's do what's reasonable to fix it. We've put in vents with filters and we've vented one and a half times the design pressure (inaudible).

MR. WARD: Those vents are with (inaudible) they are putting in our --

And you know they are?

MR. WIESS: Not quite.

MR. WARD: They have no basis --

MR. SIESS: We've been into that situation where it's been vented through the filters and the consequences won't be too bad. And since they take that approach --

MR. WARD: I sure don't want to see cosmetic filtered vents on 120 U.S. reactors. What a waste of research.

MR. SIESS: You think filters are cosmetics?

MR. WARD: I think the sort of filtered vent that they're putting on is cosmetic.

MR. SIESS: What about the French?

MR. WARD: It's even worse.

MR. COSTELLO: I guess I would have to say that when it comes down to it, I think I said this seven or eight years ago in perhaps a different room and a different hotel in Albuquerque, the ultimate user of calculational techniques will be utilities or (inaudible) doing something for the utilities to look at that utility's containment. The insight that should come from the test and the understanding that comes from the test, is something the NRC could use themselves. But the next step, to take something and do something for a given containment, requires an investment and the talents of people that can do it. A&Es are quite capable of (inaudible) steel containments.

MR. SIESS: I wish you had that much confidence that you know all -- that they're going to find all those little places where the stress concentration is high.

MR. COSTELLO: Well, I think (inaudible). We could (inaudible) past in Sequoyah and came away feeling pretty good.

MR. SIESS: What did you learn in Sequoyah?

MR. COSTELLO: Okay. Do you want to give me two minutes?

MR. SIESS: Just tell me the answer.

I've got a report somewhere that we've got three estimates, four estimates that were made by different people about ten years ago --

MR. COSTELLO: Ranging from from 10 psi to 100.

MR. SIESS: No, this is the one that came before out Subcommittee, and we weeded them down to some reasonable numbers. Ames did some and somebody else. And we wrote a letter on it suggesting what we thought was a reasonable capacity. And I would just like to compare that with what you've got.

MR. CLAUSS: I'm not going to give you a capacity because it's an uncertain number. I will give you a range. The range is anywhere from 60, which is your downyield pressure. And I'm sure you've seen it. But I would say that is the lower bound. And the upper bound is probably somewhere in the neighborhood of 80.

MR. SIESS: 60 to 80.

MR. CLAUSS: 80 corresponds to about 4 percent of (inaudible). There's a lot of things that can happen between 60 and 80, though. You also have an anchorage to come into play at (inaudible).

MR. SIESS: How does it fail?

MR. CLAUSS: Again, there is at least three significant possibilities along that pressure. There is

general shell failure. You have equipment naturally, that you do over in that range. And (inaudible) the increase.

MR. SIESS: Do you have one like they had out in the canyon?

MR. CLAUSS: Certainly.

MR. SIESS: At what pressure?

MR. CLAUSS: Between 60 and 80.

MR. SIESS: Between 60 and 80.

MR. BENDER: Is this information you're talking about what you've learned by interpreting your (inaudible) or what you learned by looking at the containment?

MR. CLAUSS: Well, both. Let me first say that after we have our break, I'm going back to really barely scratch the surface of what we would like to do (inaudible). What we plan to do is analyze Sequoyah for three different load scenarios, the one we started on is going to be temperature and pressure. We're doing that one as a bench mark and a reference (inaudible) to that were done. We also are going to use that as a test case for plant analysis with thermal loads.

MR. BENDER: But (inaudible) stop. That's what I want to know.

MR. CLAUSS: Okay. Well, very simply, first of all, we've learned something about modeling techniq 's

for steel containments. We think we have a pretty good feel for what details are important in cause of strain concentrations. And we've used that to identify those areas that we feel are important, which penetrations and which (inaudible) and those are the ones we select for analysis rather than trying to analyze the entire containment, which is (inaudible). So that's one thing we've learned; modeling techniques and what's important.

Another thing we've learned is some idea as far as what failure criteria might be good. And that allows you to kind of weigh the different failures, pressures that you calculate. For instance, I have very high confidence that the containment will at least get to the yield pressure, which is 60. So I would say (inaudible), even though the (inaudible) is very low, (inaudible) 1 percent.

Now, I also learned something, as far as what the average number (inaudible). And that's more two and a half to three that much higher to correspond to that.

Similarly, I can go up to higher and higher pressures. I have essentially (inaudible).

MR. SIESS: What would you estimate the (inaudible) for what (inaudible).

MR. CLAUSS: I honestly can't tell you off the top of my head.

MR. SIESS: The general yield would be

higher.

MR. CLAUSS: Much higher.

MR. SIESS: But the stress concentration in the lower part where it's the same thickness would be higher.

MR. CLAUSS: The lower half of the containment doesn't (inaudible) below by elevations of 120 feet.

MR. SIESS: That's for Sequoyah. Now, the Watts Bar has got the same thickness all the way up and what's one for the (inaudible) concentrations start. A general view of the Watts Bar should be considerably higher than Sequoyah.

The same thickness all the way up. Right.

MR. CLAUSS: Yes.

MR. SIESS: Maybe twice as high, 50 percent higher?

MR. CLAUSS: 50 percent higher, probably.

MR. SIESS: And I'll put the stress concentration in. Would that be --

MR. CLAUSS: You know, I can make it --

MR. COSTELLO: I think one of things you learn from the steel containment test was not to speculate without seeing the details.

MR. SIESS: Let's put the same details in

this Watts Bar as you've done in Sequoyah, but just keep the thickness the same.

MR. COSTELLO: If they were there. Hypothesize the same details are in.

MR. CLAUSS: Well, first of all, all I'm giving you right now is generalizations (inaudible) that we haven't gotten to the point where we've included the penetrations that we feel are important. But I can't tell you what the specific strain concentrations are in Sequoyah at this point.

But when I give a number like two and a half percent, that's just a broad generalization based on the scale model test. I believe that probably our (inaudible) to 9 or 10 percent, which I believe is probably the level at which you really will fail, 10 percent strain.

MR. BENDER: Do you have the evaluation approach written down somewhere?

MR. CLAUSS: No. What do you mean exactly?

MR. BENDER: -- sort of a discussion on how you are going to do the evaluation (inaudible).

MR. SIESS: We're going to have to adjourn this meeting. People have to leave. And I think we've gone far enough to lay the groundwork for another very interesting meeting. Once -- I think the Sequoyah Watts Bar or whatever Dave is working on would be an excellent thing

to discuss at some future time along with some of these other things. And I'm certainly open to be convinced. I would like to be.

And I think that if we have another meeting, we'll try to have it in Washington and we'll get some more regulatory people.

MR. COSTELLO: That would be a most enjoyable enterprise, yes.

(The meeting was adjourned at 5:45 p.m.)

C E R T I F I C A T E

THE STATE OF TEXAS)
COUNTY OF TRAVIS)

I, THE UNDERSIGNED NOTARY PUBLIC in and for the State of Texas do hereby certify that the above-mentioned matter occurred as hereinbefore set out.

I FURTHER CERTIFY THAT the proceedings of such were reported by me, later reduced to typewritten form under my supervision and control and that the foregoing pages are a full, true, and correct transcription of my original notes.

GIVEN UNDER MY HAND and the Official Seal of my office at Austin, Texas, this 25th day of January, 1988.

William C. Beardmore

WILLIAM C. BEARDMORE
Notary Public in and for
the State of Texas

CSR No. 91?

My Commission expires 7-1-89.