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SUBCOMMITTEE ON STRUCTURAL ENGINEERING

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NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON STRUCTURAL ENGINEERING

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Gran Quivera Room
Amfac Hotel
Albuquerque, New Mexico

Monday, March 22, 1982

The meeting of the Subcommittee on Structural Engineering was convened at 8:30 a.m.

PRESENT FOR THE ACRS:

C. SIESS, Chairman
J. EBERSOLE, Member
H. ETHERINGTON, Member
P. SHEWMON, Member
D. WARD, Member

DESIGNATED FEDERAL EMPLOYEE:

J. McKinley

ACRS FELLOW:

T. MC KONE

ACRS CONSULTANTS:

Z. ZUDANS
T. PICKEL

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P R O C E E D I N G S

MR. SIESS: The meeting will come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards Subcommittee on Structural Engineering.

I am Chester Siess, Chairman of the subcommitee, and we have several other ACRS members present today. I am not sure how many of them are members of the subcommittee; but Mr. Etherington, Mr. Shewmon, Mr. Ward, Mr. Ebersole, all members of the ACRS. We have two consultants to the committee, Mr. Pickel and Mr. Zudans. And the designated federal employee sitting at the end of the table is Mr. McKinley. And then we have one of our ACRS fellows present, Mr. McKone. I think that takes care of everybody.

The purpose of the meeting is to review the NRC research program on containment capacity that is being carried out at Sandia Laboratories under the sponsorship of the NRC Office of Nuclear Regulatory Research.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the government in the Sunshine Act. The rules for participation in the meeting have been announced as part of the Federal Register notice. A transcript is being kept, so the speakers will please give their names when they first speak so that the Reporter can get that down. And we have no microphones so just speak loudly. And if the Reporter cannot hear you, he will so indicate.

We have received no requests for oral statements from members of the public, and we have received no written statements from members of the public.

We have an agenda. It is labeled a tentative schedule. It calls for the subcommittee meeting proper this morning with adjournment sometime around noon, and after lunch those present will visit the Sandia Laboratories to look at some of the work that is going on. And that visit, of course, is not a part of the public subcommittee meeting.

We had a meeting on this matter in July of 1981. Members of the ACRS present at that meeting were Kerr, Bender and Mark, none of whom are present today. We have a new shift in, and so those making presentations should keep that in mind, that everybody here is not say as familiar with the program as I am, which may not be too high a level either.

I think Dr. Zudans was at the meeting in July. Pickel was not. Is that right?

MR. ZUDANS: I was at the meeting.

MR. SIESS: Very well. So we might just as well sort of start from scratch. I am speaking now both to Jim Costello and the people from Sandia.

The agenda calls for an opening statement by the Chairman, and I do not think I want to say a great deal now. I could try to summarize what the objective of the research program is, but I think that more properly should come from a representative of the Office of Nuclear Regulatory Research; and I suspect the objectives have been modified somewhat since my last formal connection with it.

So unless there are some questions from members of the subcommittee, I think we might start off with Jim Costello.

Any questions?

(No response.)

There will be plenty of time for questions later on.

Jim, do you want to open things up? I should mention that in addition to Jim Costello from the NRC staff we have Franz Schauer here, who is Chief of the Structural Engineering Branch, Office of Nuclear Regulatory Research -- I forget what division -- and Pete Williams, who is Research Coordination Branch.

MR. WILLIAMS: That is right. Standards and Research Coordination.

MR. COSTELLO: Good morning. My name is James Costello, NRC staff.

I thought for purposes of continuity I would put on the first slide.

(Slide.)

It is something we had seen at the meeting in July, indicating that at least some things remain the same.

Walter Von Riessemann from Sandia, who will be talking to you a great deal later today, is still here. I am still here. And the principal question is still the same; that is, how will containment structures fail and at what loads, or put another way, what is the containment's capacity?

For a little bit of a background you will note that the title currently is "Containment Integrity." Initially the title was "Containment Safety Margins." I think that tells the story that will fill in the gaps.

Back in the fall of 1978 when the program in structural engineering research was just beginning to get organized there was a great deal of discussion and interaction between the research staff and members of the Office of Nuclear Reactor Regulation. Here we are back in the fall of 1978, and the discussions ensued with members of the staff from the Offices of Nuclear Reactor Regulation and the then Office of Standards Development, and ideas began to be focused on what sort of structural research programs were needed.

At that time one of the topics that was identified was what was then called containment safety marg. that is, it was recognized at that time that containments could sustain loadings outside the envelope of the design basis, but because of the way the technology had developed, there was no real consensus or basis for consensus on estimating how much. And the feeling was then it would be a good idea to try to get a handle on the ability of containments to withstand loadings beyond those for which they were designed. And at that time it was felt to be, while one of the highest priority issues, immediacy and urgency were not perceived.

And I guess you are well aware of a long tale of research programs and budgets, a program that was thought about in the fall of '78. It really would not start until fiscal '80. And then somewhere in the spring of 1979 a little bit of immediacy and urgency was perceived, and there was an attempt to get started as quickly as we could in response to the Three Mile Island incident.

The approach that was taken virtually from the outset, although others were explored, was grounded in the observation that there are great differences from a structural engineering perspective among the population of containments, and that prototypical or I should say scale model testing, i.e., tests of a given containment type at reduced scale, in a basically experimental program was out of the question. And there was also a simple observation that that would probably be an improper taking for the NRC as well to model somebody else's containment for him.

The approach was to develop a sufficient experimental data base to allow discriminating judgment to be applied to methods used by agents of applicants, or applicants or licensees to make their estimates for their containments. And that is about the way we have been going for about two years now.

Now, let me move on a little bit.

(Slide.)

About a year and a half ago or so when it came to the question of priorities and allocation of resources there was a fair bit of interaction with the NRR staff and with a review panel which has been constituted to provide peer review for this program.

The interaction goes more or less like this. The questions of priorities and needs are fairly well based on staff perceptions. The review of programs proposed to meet those needs as far as technical adequacy are reviewed by the review panel.

Members of the review panel are not here today, but I thought I would give you a rundown of their names. I think some of the names will be familiar to you: Tom Ahl from Chicago Bridge and Iron -- his main area of expertise is design of steel containments and liners; Bill Baker from Southwest Research has a long history in experimental mechanics; Pete Cybulskis from Battelle-Columbus has been added recently to the panel in response to observations both by the ACRS Subcommittee and other sources that the panel had previously lacked someone with a great deal of conversance in accident scenarios; Asa Hadjian from Bechtel has principal areas of expertise in seismic design and containment analysis; Mete Sozen from the University of Illinois has main areas of expertise in experimental design and testing of concrete structures; John Stevenson of Stevenson and Associates and Joe Ucciferro of United Engineers have containment design experience; Ian Wall from EPRI is on the panel to participate mainly in interactions with probabilistic risk analysis studies; Professor Dick White from Cornell is a nationally recognized expert in experimental work on structures.

As a result of the interaction with the staff about a year and a half ago it was decided to go with first priority on steel containments and deferring work on concrete containments until later.

MR. ZUDANS: Could I ask you one question?

MR. COSTELLO: Sure.

MR. ZUDANS: In the objectives in the previous slide one of the approaches would be to assess selected predictable numerical methods, is that right?

MR. COSTELLO: That is correct.

MR. ZUDANS: I know Sandia has their own in-house expertise on that. Which one of these review panel members comes closest to expert in that field in numerical methodology itself?

MR. COSTELLO: In numerical methodology itself I do not think we have anyone who is especially strong, but Ucciferro is probably the closest.

MR. ZUDANS: He is just a user.

MR. COSTELLO: That is correct. That is correct.

As I say, as a result of the decision to go forward with steel first and follow in with concrete, this decision being dominated principally by resources, a plan was worked out and reviewed by the panel whose names you have just seen. Redon reiterated and is now on the verge of publication. Some elements of if I am sure you may have seen in very brief form in our draft, our most recent draft of the long-range research plan. You will hear more about it today.

The work proposed currently for concrete containments has not had the benefit of review by this peer review panel as yet. So to the extent that we do talk about initial thoughts on how to proceed on concrete containments, please bear in mind that that has not been thoroughly cycled through our review process. Okay.

(Slide.)

I guess this is my half a page prospective vu-graph, where we stand today. This is where we say two major areas of utilization. I think we are beginning to see more in the second area in later times than we saw before. I think we always recognized that there were two areas but did not perhaps foresee what we now see as a growth in demand for our end product.

Where we stand right now, in summary form, with current resources is that static pressure tests -- that is, tests up to failure under monotonic increasing pressure -- will be going on in fiscal years '82 through '84, and details of that will be gone into today.

Current thinking is that what was sometimes called dynamic pressure tests we are currently visualizing for '85 to '87, and these are, of course, tests which will be necessary to gain some credibility for responses to rapid deflagrations and other accidents.

I intentionally changed the slide to reflect my own bias. My own thinking is that when the accident scenario business boils down a little bit from a structural engineering viewpoint of containment, the important part about the unsymmetric dynamic loads will be for containment purposes. They will not be terribly dynamic. And the lack of symmetry will be the significant aspect, not the transient nature. And we visualize looking at seismic effects out toward the end of the decade.

We are a little less clear on what kinds of experiments will be needed to bridge the gap from the basic building blocks. My own personal feeling is that if we can satisfy ourselves with the basic building block, i.e., that there is a general ability in the construction engineering community to predict just what will happen to a given containment under a monotonic decreasing --

MR. ETHERINGTON: Does that long time schedule reflect a low level of funding?

MR. COSTELLO: Yes, sir. I guess that is a short answer.

MR. ETHERINGTON: That is good enough.

MR. COSTELLO: To give you a little bit of history on that, we started out with something, $50,000 in 1979 to get the ball rolling. I have managed to, collecting leftovers from 1980 and '81, managed to piece together, oh, about a million and half dollars for this year.

We visualize being able to corral two to two and a half million in the out years. That is the number we are looking at. We have not been able -- management feeling back in NRC is we have grown in the program as fast as we can within the NRC budgetary constraints.

Another response to your question is we may see something different in scheduling as a result of the poring over of proposed severe accident plans. Some comments on the proposed severe accident research plan indicated that in the view of the commenters progress on containments should be accelerated.

MR. ZUDANS: Jim, I have two questions.

Will someone later attempt to define what is meant by capacity?

MR. COSTELLO: Yes.

MR. SIESS: Not later.

MR. ZUDANS: And the other question, a similar definition for what is meant as input for risk analysis.

MR. COSTELLO: Let me --

MR. SIESS: Wait a minute, Jim. Before you answer that, put your first slide back on; and I want to say a few words to at least try to set the stage for some of this meeting.

(Slide.)

The ACRS has the responsibility, as you know, to review the NRC safety research program; and in our review of that program we usually focus at the broad level of what is being done and why it is being done, and not in all cases do we get down to the level of looking at how it is being done or how well it is being done.

Now, this meeting has both objectives, I think. The question of what and why is something that we can in this meeting explore with the representatives from NRC. Our presentations and interactions with the people from Sandia will be chiefly devoted to how it is being done and how well. So that is the separation that I think is important. So I want to come back now to the first part and spend a little bit of time on that.

Your principal questions, there is the word "failure." This is not a new question to you. What do you mean by "failure," and what does Sandia understand what you mean by "failure?"

MR. COSTELLO: Well, I wish I could say with certainty that we were a great deal further along the road than we were when I talked to you in July. We still have the perception that failure, containment failure is related to leak rate, and that an unacceptable leak rate is failure.

Now, the quantification of that is to my mind inextricably entwined with consequence modeling. The best we can do is to try to assure that what we are doing will provide a suitable piece when that puzzle is put together. And that is the reason why we have gone to the added expense of the -- you know, our feeling that leak rate is a significant failure parameter is in fact one of the reasons we have hewed to the course of compressed gas as opposed to hydrostatic gas.

MR. SIESS: Well, it seems to me -- and I have not heard Sandia's presentation yet; it may get changed -- but from what I have heard so far, not here but before, that the project objective is still very strongly focused on structural capacity and structural failure. It is structurally oriented. For example, this is suggested by the proposed order of testing where you start off with a plain shell without any openings in it at all, which is the opposite end of the spectrum from where leaks are going to be.

And I am becoming increasingly concerned that we may spend several millions of dollars and get several years down the road before we find out that we have been trying to answer or in fact have answered the wrong question.

Now, the question is not when the containment fails but when the containment begins to leak excessively, and that is the only question. The only reason that containment is there, except possible shielding and missile protection, is leakage. Its primary purpose is leakage. It has been pointed out that it is the only engineered safety feature that is there only to protect the public and not to protect the plant. Okay.

Now, I have been thinking about this a little, and I have come up with the thought that there are four ways we can have excessive leakage of a containment. The first is that it fails to isolate; and I think there is a fair amount of evidence around that that is a high probability condition. It took them 12 days at Zion to get the containment tight enough to make an integrated leak rate test. There was a paper published in Nuclear Safety a year or so ago estimating the reliability of isolation, and they came out with not very good figures in terms of what people use in risk analysis.

The second possibility for leakage is the failure of a penetration or an isolation device because of the effect of pressure or temperature following an accident. For example, in the NUREG/CR-2182, the Oak Ridge study on the Browns Ferry blackout, after the batteries failed, in about four hours, three or four hours, you had a core melt, and when the core went through the vessel, the temperature in the containment went up, the pressure went up. And at a pressure of maybe 120 psi gauge, which is, I think, well within the capacity of that containment -- it was designed for 56 -- there was a temperature in the range of 400 to 500 degrees Farenheit, and the people that made that analysis decided that the electrical penetration assemblies, the elastomeric seals would fail and they would blow out. At that point there was no pressure in the containment, but the leakage was, of course, excessive. The pressure dropped to zero.

The third possibility is that there would be a failure of the penetration, or a hatch, or a door as a result of deformation of the basic containment under the pressure/temperature following a severe accident.

And the fourth possibility is there is a rupture of the containment structure itself, which of course leaked excessive leakage.

Now, I expect the probability of those four modes of leakage -- and they could be subdivided -- are about on the order I gave them. I think the first one is the most probable and the last one is the least probable. And I have gotten the impression, which I would be very happy to have corrected, that the emphasis in this program is on the fourth mode, structural failure of containment, and to some extent on the third mode, which is the penetration failure resulting from overpressure/overtemperature of the containment itself.

And we might solve that problem with a few million dollars in a few years and find out that it is a negligible contributor to risk. Now, I do not think that we can afford to do that.

MR. COSTELLO: May I respond a little bit?

MR. SIESS: Yes.

MR. COSTELLO: I guess you expected me to.

MR. SIESS: You and Pete and Franz and any of you that wish to respond at this point.

MR. COSTELLO: Well, I guess, let me work my way backwards through your observations and comments which I think are most helpful.

From my viewpoint the emphasis is about equally on your items three and four; that is, leakage around penetrations caused by deformations.

MR. SIESS: Let me clarify now one reason I put the order I did, is that as I see this program for about the first three years anyway, the next three years it is going to be on number four; and if three gets in there, it is going to come in toward the end.

Am I wrong?

MR. COSTELLO: No. I believe you are correct. But if you will allow me to continue backing up, I will get back to my observation.

Our feeling is that the emphasis is on three and four about equally. The question is how do we get there, and I guess I will have to observe that the first thing in our approach bullet, the first of our approach bullets is effectively what is driving it. We have to have, at least in my perception, a belief that analytical methods suitably calibrated can indeed reliably predict what is going to happen first and where; that is, will it be around a penetration in your items three and four, or will the penetrations hold and will something happen out in the membrane area.

The fact that there are so many varieties of containment leads us to this reliance on analytical methods. And by that I do not mean strictly analytical methods developed as part of this effort. There is a great hope, and I think there will be realization that people who are now trying to make whatever predictions they can will be most pleased to avail themselves of the data to improve their predictions.

But I think that is simply why we are going the way we are going. And I think your observation that you will not get two things with penetrations in them until later in the sequence as outlined by Walter --

MR. SIESS: There is a lot of merit in going from the simple to the complex, even if you know that the complex is really what you want to know. But going from the simple to the complex in this deliberate fashion seems to me to ignore the time constraints that are here.

I keep hearing people on the staff talking about severe accident rulemaking which is now out, but severe accident program, talking about a time scale that I think is quite a bit shorter than this program is going to require.

Now, I can visualize other approaches. I can visualize one where I start off with the most complicated thing I can think of, and the best analysis I can think of, and the best tests I can make and see how good it is, and then find out where it does not work and then take those questions and try to answer them. That builds up from the simple to the complex.

Now, experimental people, that will work for them. It has worked in the past. To some extent it is a gamble. You may end up writing it so complex that you have to go back and start over at the beginning again. On the other hand, sometimes you are able to skip two or three steps and get to an answer, or you might hit it lucky and find out that it works the first time, in which case you are home free.

I do not know how inclined Research is to gamble, but it seems to me that the desirability of looking at that kind of an approach is something that needs to be explored with the licensing people or whoever is trying to make the decision. I do not know whether it is the Commission or somebody else.

MR. COSTELLO: I guess I can offer -- may I speak?

MR. ZUDANS: I just wanted to add to Chet's comment, if I may. I think this is a very interesting breakdown in these four items.

Chet, if I am not wrong, item number one, they could not really do anything.

MR. SIESS: No. And I notice in the long-range research plan they dismiss that. They say failure to isolate is something we take care of by quality assurance and in service inspection; and they do not have the slightest idea of how good their isolation reliability is, but it is dismissed. That, I admit, has nothing to do with this program.

MR. ZUDANS: The second point that you brought out I think is extremely interesting. I am wondering whether this program intended to do anything like it. I think that is a significant point.

I see Walter shaking his head. That will be right?

MR. VON RIESEMANN: We did, but the limitation on the budget --

MR. COSTELLO: Yes, I guess. May I first answer your question and then go back and discuss --

MR. ZUDANS: I did not really ask a question. Go ahead.

MR. COSTELLO: Okay. I am still on the question of why we are going the way we are going, and I have a sense from listening to Professor Siess' comments, I have a sense of deja vu.

On our advisory panel there was a good deal of discussion along these lines. Professor Siess suggests that perhaps extreme prudence on the part of the NRC Research staff in wanting to keep up on the problem -- and I guess I am principally responsible for that. Maybe it is extraordinary prudence, maybe it is simply cowardice, but I think we have to be realistic in our experimental effort. We need a certain amount of batting practice before we can convince ourselves that we do have experiments in which we are measuring what we want to measure and the like. The initial effort on the clean shells, well, I am not so sure I even consider that batting practice; more like leg stretchers maybe, or warmups. They will provide something that will be useful in the large deformation of code prediction for a clean shell, but that is perhaps a side benefit.

The first two tests are really there to shake down -- perhaps it is imprudent to publicize them as tests and just treat them as something that is internal housekeeping. However, I feel we are spending the money. We will say what we are doing to get to items one and two.

Yes.

MR. SIESS: Then we have the question of time. Have you got the luxury of taking the time to do that?

MR. WARD: Let me see, Jim. It seemed to me that there is maybe -- the chart you had up, the last chart you had up you said there are two -- you talk about two areas of use.

(Slide.)

One was judging, to permit the staff to judge the credibility of capacity estimates that the licensees will make. The second is to provide input to risk analyses. Those are two different tasks.

I wonder if the breakdown of Chet's four items is somehow related to these.

MR. COSTELLO: Well, I guess --

MR. WARD: And which of these purposes is driving you in the near term.

MR. COSTELLO: Perhaps first I can respond to your question and then Dr. Zudans' together, and I think it comes down to this. As Professor Siess points out, as he enumerated four areas, the isolation question is clearly beyond the scope of our program. It may well be historically demonstrated to be perhaps the one of greatest concern.

The failure of small penetrations we have always perceived as an issue that is there. In an earlier, more grandiose scheme that extended the resources available, Dr. Von Riesemann and his colleagues proposed a much more elaborate testing program on environmental qualification of penetrations.

Another out we have on that is gee, that belongs in the electrical branch. Now, I will not push that too far today, but I think that is why we have always viewed one as being way beyond our scope, two as being tangential, and I think we have reason to believe that the effort on two will be picked up but perhaps some place else.

But on items three and four, even in the input to risk analysis -- I think Dr. Zudans asked what I meant by that 15 or 20 minutes ago -- it will be in categories three and four probably; that is, when will a containment fail and where. And questions like this seem, in recent go-rounds, to be of extreme interest in risk analysis and consequence analysis.

MR. SIESS: But, Jim, you are addressing this as maybe appropriate from the standpoint of the structural engineering section in the Mechanical Engineering Structural Research branch. That is legitimate to say that my mode two is not your job; it is somebody else's. But it is the job of someone in Research to look at this and say well, maybe mode two is going to be the principal contributor to risk, and we ought to put $10 million into it, and we should not put money into containment capacity, and not at the same rate.

And, you see, we do not have those people here. We do not have any people except a bunch of structural people here, except Pete, and Pete may be able to address this if we give him a chance, if he raises his hand.

MR. COSTELLO: Nobody here but us Indians.

MR. SIESS: As far as Sandia is concerned, if I were the principal investigator on this I would be very reluctant to propose the gamble type of approach I suggested where we start in, test something with a lot of penetrations, and work backwards; and maybe it works and maybe we start over.

As a researcher I would be more inclined to take the step-by-step, A-B-C-D approach. But it is appropriate for the sponsoring agency to make the decision that if we need an answer in the three years, let's take a gamble on some other approach, and it is our responsibility.

So again we have a division of responsibility here, the whole research thing, the structural part. They responded to your RFP, and it is not quite that cold. And these are strategy type decisions, and I really do not expect to get them settled here; but I want to raise it. Maybe Pete --

MR. COSTELLO: Let me make a quick response.

MR. SIESS: And then I will give Pete a chance.

MR. COSTELLO: Of the penetration turning to taffy prediction from Oak Ridge, in fact, I had a couple of discussions with Bill Anderson, who was Chief of the Mechanical Structural Engineering branch, in the last couple of weeks about where does this fit vis-a-vis what we are doing. I guess I can assure you that at least at a somewhat slightly elevated management level there is discussion going on about what some of the things that may be outside the box that are important, you know, where some of these areas may be.

I guess you know much more about the structure than I do, and I think we will not see -- I would cheerfully submit I do not think you will see any major reallocation of resources among programs lumped together into severe accident plan without significant input from the outside and to senior management.

MR. SIESS: Well, it may get it. You see, I can look at it very --

MR. COSTELLO: They might welcome it, too, I think.

MR. SIESS: If I were convinced that modes one and two were much more likely to contribute to modes three and four, and that somebody in NRC was really going to look at modes one and two -- that is, failure to isolate and containment failure or penetration failure -- and really get going on it, then I could be a lot more relaxed about modes three and four. I might be a lot happier by taking it step by step and spending five or six years to look at containment capacity in case it comes up in the future and get that one out of the way.

But if I am looking at containment leakage as a problem in severe accident phenomena, then I guess I have to ask NRC Research whether they are working on the most important parts. Now, whether they end up taking money away from you is something else.

MR. COSTELLO: May I add one more --

MR. SIESS: Okay.

MR. COSTELLO: You jogged me with one more I had on my list that I did not get back to. But I seem to recall discussing in July -- obviously it did not take or it did not permeate the discussion today; my feeling is perhaps I did it in an aside -- as to whether what you have identified as modes three or four are important to us.

If you look at it from -- suppose you identified this as the weak link, what do you do aspect, the valve closing, the most probable, is also the most fixable, at least in principle. The leakage around small penetrations due to environmental effects is also perhaps a little less fixable than procedures, is probably -- I may be getting on thin ice here about my knowledge of high temperature materials -- but I think it is probably fixable at a relatively small expense of time and money if a premature failure of penetration would be at half of what you would expect otherwise.

When we get to mode three, a retrofit of a large penetration, if it turned out that that penetration capacity, the capacity to withstand that penetration limited your overall containment capacity, would be possible but a major undertaking. And when you get down to the membrane failure itself, of course there is no effects.

MR. SIESS: Yes, there is. There is one fix for both modes three and four, and that is the vented filter. You just do not let the pressure get up. That is not an original thought with me, as you probably know.

MR. COSTELLO: Okay.

MR. SIESS: But there are ways of keeping the containment vents from being overpressured, and I guess putting a couple of 36-inch holes in a containment for piping may not be a minor backfit, but it is probably not much worse than trying to fix up equipment hatches that are going to leak.

Okay. Let's see what --

MR. WARD: Chet, could I ask one further questions?

Let's see, you know, the containment failure as you started out, containment failure is an unacceptable leak rate. Somewhere that gets defined, given the situation, quantitatively. I guess my question, going back to Chet's four items here, is it clear that one and two will give you leak rates which are beyond this threshold of unacceptable. I guess it is clear that four and probably three will.

MR. COSTELLO: Yes, sir, I think so.

MR. SIESS: The Oak Ridge study blew out all the electrical penetrations. The pressure dropped to zero instantanteously.

MR. WARD: Okay. I guess you have answered my question. Are the leak rates for those two failures -- they are clearly beyond the threshold of acceptability.

MR. COSTELLO: To my understanding of the readings of WASH-1400 and the Oak Ridge study, the answer is yes.

MR. SIESS: Has anybody ever taken -- for example, there are a number of accident scenarios obviously, and right now people are using the MARCH code, MARCH-CORRAL or something, which I know has a lot of bugs in it; but it will predict certain rates of increase in pressure, right?

MR. COSTELLO: Yes.

MR. SIESS: Has anybody taken that and looked at what size opening or what leak rate -- and I would prefer to think as a structural engineer in terms of the size of opening -- what size of opening would essentially stop that pressure increase. The pressure is going up 10 psi an hour. How big a hole do you have to have where it does not go up any more?

MR. COSTELLO: I am sorry. I cannot answer that question. I have a vague inkling of seeing some calculations associated with holes, but I do not remember the details, and perhaps --

MR. SHEWMON: Sandia has a comment that we will hear later.

MR. COSTELLO: If we can defer until then, we can do that.

MR. SIESS: That is what Dave Ward's question is essentially. I suspect if you get a six or eight inch or twelve inch hole that your pressure will just not go up any more; it will go down.

Okay. Do you want to let Pete --

MR. COSTELLO: Or I could -- if you want to talk about what I have on the last slide for a few minutes, then I could let Pete come on.

MR. WILLIAMS: I will be very brief. I can tell you that there are essentially --

MR. SIESS: You will probably be back, Jim.

MR. WILLIAMS: I am Pete Williams from the NRC staff. I can direct this to two items which are going on which I think address the concerns you have raised at this time.

One is that there has been a long-range plan for severe accidents. It was called NUREG-0900. And in our NRR staff we did have the opportunity to comment on it about a month ago, and in the comments that we offered on containment integrity we did not address mode one, the isolation problem.

I think most of us felt that that was something within our own domain, and we did not foresee any research that would help us in that area. However, in mode two we did insert comments into our comments on NUREG-0900 --

MR. SIESS: Wait a minute. The NUREG-0900, it's the severe accident --

MR. WILLIAMS: Program plan.

MR. SIESS: Research plan, isn't it? It is not the long-range research plan.

MR. WILLIAMS: That is right. And you may have seen our comments on that.

MR. SIESS: Yes, I have.

MR. WILLIAMS: Okay. I cannot remember them all. I know we did ask Research to give us a definition of what is meant by containment failure, and I know we did ask Research to consider penetration failures in their research activities, and we did put an emphasis on time.

We told Research in those comments that we wanted answers by fiscal '84, we want to make decisions during fiscal '84, and that the research should be planned well enough so that the research to be performed after fiscal '84 would really be complementary and supportive of the decisions. That would be what is called good research planning.

Our comments were reviewed by the highest of Research management, and we held a meeting just last week where many of our comments were discussed with high-level management, and they, in particular on containment -- we did not go into it deeply, but they felt that our comments were well taken and that the programs would be directed along that line.

MR. SIESS: Pete, you said you wanted answers by a certain date. Do you really have to have answers, or do you want good questions by that time? It seems to me that with the variety of containments, the variety of penetrations, and within any single plant there is a variety of penetrations, and go to another plant, they are different.

MR. WILLIAMS: I think we want both. The answer that we want, though, is to make a decision on additional engineered safeguards, whether or not vented filter containment should be a device to be backfitted to existing plants. We would like to make a decision on that in the next two years.

MR. SIESS: I do not see how you can get answers that quickly, because if the penetrations are a weak spot, the only way you are going to get answers is to ask every applicant to tell you how good his penetrations are. There is no way the NRC Research, Licensing or their contractors are going to be able to answer the question as to what is the pressure and temperature capability of every possible penetration over six inches in diameter or over three inches in diameter. And if you are going to be able to know whether those penetrations can withstand a severe accident, you are going to have to have a set of questions to ask the licensees, and for him to go out and get the answers now.

MR. WILLIAMS: You are going to have to know what the environmental conditions are going to be that these penetrations will see. I do not think we have anything now that is less than just an estimate. We hope, though, in the next two years to have a better estimate of what the temperature, pressure, humidity and that sort of thing will be in containments.

One more thing I would like to say. As you know, we introduced a new concept of accident management. We do not know where this is going to take us. NRR has endorsed quite strongly the proposal to consider that as an important addition to both accident prevention strategies and accident mitigation strategies; and this may lead us into --

MR. SIESS: How does containment leakage enter into a strategy of accident management? That is your last engineered barrier to radioactivity reaching the environment. After it gets out of containment all you have left is evacuation, sheltering and potassium iodide.

MR. WILLIAMS: Well, you hope, I believe, to keep melted fuel, if it does melt, within the reactor vessel.

MR. SIESS: That is prevention, yes.

MR. WILLIAMS: Okay. Now I think we are into semantics. You might say that TMI-2 was a managed -- accident management took over after the initial event, and it was sufficiently managed to keep the core from degrading further.

MR. SIESS: Well, sure, everybody wants to keep the accident from progressing that far, but that does not mean the NRC is going to let somebody stop at that point. We are not going to take containments out. We are not going to tolerate a leaky containment. What TMI taught us, if it taught us anything, that the containment worked.

MR. SHEWMON: But for scenarios which do not lead to containment problems until you have done nothing for 40 days, they may now change the scenarios, where before that is what they ended up with.

MR. WILLIAMS: You might say penetration design is still dependent on the scenario development. We have not reached the end of the road of what scenarios will be considered.

MR. SIESS: That is, you might consider some scenarios so incredible that they do not have to be designed for.

MR. WILLIAMS: That may be the result in two years. I think that is why there is --

MR. SIESS: Like a double-ended pipe break.

MR. WILLIAMS: That is right.

MR. SIESS: If we are going to continue to postulate a double-ended pipe break, I do not see how we are not going to postulate radioactivity in the containment.

MR. WILLIAMS: Oh, I am sure we will continue to postulate some radioactivity in containment. I think it is the temperature and pressure that the containment will see is what is unknown.

MR. SIESS: Okay. I get your point. You think you might be able to decide that the temperature and pressure level gets so high that you have to worry much about modes three and four.

MR. WILLIAMS: In the sense that we would require containment venting, a vented filter system. That is one of the decisions that people are saying that we want to make in the next two years. And that is, as far as I know, that is an agreed upon strategy, at least at the level of the Office of Research and the Office of Nuclear Reactor Regulation.

MR. SIESS: I really cannot build up a scenario, I guess. If I did research on penetrations and found that there was some penetrations used, probably electrical, that could not withstand 300 degrees Farenheit in combination say with 15 percent over design pressure -- they have been tested to 15 percent over design pressure but not at 300 degrees Farenheit, right?

If I found they could not do that, do you think there is some way I can not worry about those? I am on mode two.

MR. WILLIAMS: Let me go on to -- I wanted to say I had two areas to report. The second area is we are developing a containment user request, an NRR user request, and included in that request I think will be a discussion of our needs in terms of research on containment penetrations and supporting research and scenario development to determine the environment the penetrations will see.

MR. SIESS: Well, I am still convinced that NRR does not know what questions to ask yet from the licensees, and I do not see the research here dealing with modes one and two, and I still think they are probably more important than modes three and four.

MR. WILLIAMS: All I can say is that we do recognize the problem. One of the reasons for our participation in this committee's meeting today is to develop background to develop our user need.

MR. ETHERINGTON: Does the program as outlined place emphasis on what happens to the containment or equally on what the consequences are?

MR. WILLIAMS: You are speaking of the severe accident research plan?

MR. ETHERINGTON: Yes. No. This program.

MR. WILLIAMS: Jim Costello's program.

MR. SIESS: Yes.

MR. ETHERINGTON: Yes.

MR. WILLIAMS: I will have to ask Jim to answer that.

MR. ETHERINGTON: Okay.

MR. EBERSOLE: May I ask another question? There was another elaborate program going on which is called environmental qualification of safety equipment inside containment. That would include electrical penetrations, I believe.

MR. WILLIAMS: It should.

MR. EBERSOLE: I believe that is oriented toward defining the qualifications of such equipment at fixed or identified levels of exposure to temperature and pressure, and it does not include any establishment of margins.

Are you interfaced with that program to understand what conservatisms may or may not be in it?

MR. WILLIAMS: We are working to interface on that. We recognize some lack of coordination in that area, but we will pursue that in this containment user --

MR. EBERSOLE: If I recall, it will be 1989. It will be 1989 before NRC finally verifies that they have got qualified equipment.

MR. WILLIAMS: I hope it is not.

MR. EBERSOLE: That is what the program calendar is now, way out there, too far in the future.

MR. SIESS: But the present environmental qualification program is mostly for DBE's, design basis events, or design basis accidents, but not for severe accidents.

MR. EBERSOLE: Right.

MR. SIESS: Steam line break, double-ended LOCA.

MR. WILLIAMS: And the reason for this is we are not really fixed on what the scenarios should be. You might even say it looks like we will have to come up with DBE types of accident. Maybe that is what we will ultimately come up with is another step, another fallback on some form of more severe design basis accidents.

As you may know, there is a program under way at several labs called SASA, severe accident sequence analysis. I think this is a very healthy program to get a handle on this. It is not a probabilistic risk assessment, but it is an analysis that uses event trees and allows for operator intervention. I think that we have a lot to look forward to from these analyses.

MR. SIESS: Any other questions for Pete?

(No response.)

MR. ZUDANS: I have one for Jim.

MR. SIESS: Jim, you have one more slide.

MR. SHEWMON: Then do we hear from Sandia?

MR. COSTELLO: Yes, I have one more slide.

MR. SIESS: Zenons has a question for you.

MR. ZUDANS: I just want to make sure that my understanding is correct, because all of a sudden I begin to feel comfortable with this program, which was not the case ten minutes ago.

If we now analyze the situation in terms of Chet's defined four items, item one, failure to isolate, certainly is out of the scope of this program. It can be handled from the basis of whatever experience can be collected.

The failure of penetrations or isolation devices due to the environment that might occur in a severe accident does not have to be done in this scale. Experiments can be done on a small device which is really not a structural problem, so therefore it makes sense not to have it in this program. So therefore, the remaining two items that are being addressed indeed fill in the gap that is there.

MR. COSTELLO: I guess I feel pleased that you are assured about what we are doing.

MR. ZUDANS: I am not assured, no.

MR. COSTELLO: My problem is I am unable from my perspective to reassure Professor Siess or members of the subcommittee about progress on modes one or two. I have the feeling that something is going on, but I do not know it for a fact. It is beyond my expertise and competence.

MR. SIESS: One and two are clearly outside the scope of your section. I am not sure they are outside the scope of the containment structure engineering problem. And it bothers me because I am not sure that anybody knows whose scope they are in. I think they are in --

MR. ZUDANS: I want to add one more thing which was mentioned by someone here already. Testing of the penetrations in the environment that is created by a design basis accident is definitely not the whole story. Somehow you have to establish what margin they are or else you have no conclusions to make from these tests with respect to how these devices will behave in a severe accident environment.

Is there anything done in that direction to define the margins, to go as high as they can before they fail?

MR. COSTELLO: Do you mean in the --

MR. ZUDANS: In the penetration areas.

MR. COSTELLO: Pressure/temperature, I have to say that is beyond my understanding. I do not know.

MR. ZUDANS: If you do not do that, you cannot do anything with severe accidents at all.

MR. COSTELLO: Again, I cannot speak authoritatively to that. I understand the concern. I can say that it is also a concern to Dr. Anderson to make sure that things do not fall in the crack and that nothing flops over on his side of the line.

MR. ZUDANS: Okay. Thank you.

MR. SIESS: Now, you are going to talk about what other people are doing, right?

MR. COSTELLO: Yes. Do you want my observations on the IDCOR?

MR. SIESS: This slide, you are going to talk about what other programs are relating to what you are doing.

MR. COSTELLO: On the next slide I will.

MR. SIESS: Before you start, let me make a couple of comments. In the draft I saw of the long-range research plan I thought that what the structural engineering group had done in terms of relating their work to programs being done by others -- DOE, industry, and other countries -- was probably one of the best jobs in that plan. Congratulations.

Second, when you address this, to the extent you can indicate whether any of these other people are thinking about any of these other modes we talked about.

MR. COSTELLO: Okay. I will do that.

MR. SIESS: Or whether you have been interfacing only on the structural point.

MR. COSTELLO: No. I have done a little bit more than that. Again, I would like to wrap up this slide, because this is the one I thought had the substance of today's presentation.

In response to Dave Ward's question about the two areas I thought I should note that, because I guess if we perceived only one or the other as the are of utilization, I think we would have the program configured a little differently.

We are trying to do something which will be useful for both and not get into the worst elements of a compromise. As far as the current schedule, hat is what we see. As Pete Williams and others have suggested, we may want -- it may be wise to speed up the schedule, and it may be that the next time we talk to you there may have been taken a management decision to do something along those lines.

(Slide.)

And my final summary is of ongoing activities, some done by myself, some done mainly by Walter Von Riesemann, and some by us in concert. We have attempted to get the benefit of the predictions made of containment capacity for individual containments made for different utilities and reported as part of the IDCOR Task 10.

My own observation from seeing the presentations but not yet seeing the written reports is that what people are doing is generally what you would expect as a responsible structural engineering attempt at a first cut; that is, people are, generally speaking, taking the models they used for containment design, putting on some sort of bi-linear patch, and attempting to follow radial displacement versus pressure, and then generally come to a point where deflections are beginning to grow according to the model they are using, and then say gee, I am more or less without confidence in the modeling beyond this point. Up to this point I feel pretty good. Therefore, since you asked me to make this prediction of what the containment could take beyond design basis, something which no one had thought about doing before, and allow me the whole of six weeks for it, I think this is as far as I can honestly go.

But I can say that the number I give you is one in which I feel confident is a lower bound. Then come certain caveats about penetrations and a different bunch of people within the A&E firms or utilities from those who reported the structural calculations are vaguely quoted as saying not to worry about the penetrations, except with the single exception of UA from Oak Ridge.

And the IDCOR program I believe is progressing. As to whether a second calculational cut will be taken as part of the IDCOR effort I just do not know.

MR. ZUDANS: Could you spell out IDCOR, what it means?

MR. COSTELLO: Industry degraded core rulemaking response or something. It is an activity sponsored by utilities and a little bit by some of the major A&E's. I think the total package is about $10 million to be spent over two years.

MR. ZUDANS: Who is doing the work?

MR. COSTELLO: The principal contractor and manager is the firm in Knoxville, Tony Buell's firm, Technology for Energy Corporation. And there are a number of subcontractors working around.

MR. SIESS: They are looking at the whole degraded core situation.

MR. COSTELLO: That is correct. But one of the tasks was the so-called Task 10 on containments.

The DOE effort is related, as I have cited here, is related to their response to the public law which requires that DOE respond by this summer about their plans to develop a program to enhance the safety of light-water reactors. It is a funny animal, since there is a great deal of planning going on but nothing in the '83 DOE budget to sustain.

The logic is that should something worthwhile be developed that Congress will see fit to increment the funds. No comment. But as part of this study effort, there is a draft report in circulation right now which tends to look at a wide range of questions, structural capacity only being one of them. And among items that were cited as priority A in that list are questions about leak rate, amount of filtration through concrete and consequence-related questions. The DOE approach is heavily consequence related.

MR. SIESS: Jim, there is an activity which may be more a source of questions than of answers, but a lot of people now are making analyses of containment capacity or containment leakage, the Zion PRA. Other people are going to be doing PRA's, Indian Point and so forth. Zion is under peer review.

Are you or Sandia or somebody following all of these things that are being done to see what kind of issues they raise, either in the original or in the peer review?

MR. COSTELLO: The answer is yes.

MR. SIESS: I thought that the Argonne peer review of Zion had some very interesting points on the containment question, leakage. It raised a few oddball questions that I had not thought of, and I am not sure all are important; but these are the kinds of things you are looking at.

MR. COSTELLO: Yes, sir. That is correct.

And finally, the interaction with foreign programs, we have three things listed. One is past history. The first one is the test done at the University of Alberta on a model of a Gentilly containment, and that effort we hope will be of some use to us in the planning and carrying out of concrete containment experiments.

MR. SIESS: Who was doing that?

MR. COSTELLO: It is done by MacGregor,

Simons, Dave Murray. In fact, they have just about wrapped it up.

The second one is something current. It is our understanding that there will be undertaken a test of a prestressed model similar to a SNUPPS containment, but from our context similar to a SNUPPS containment, from the British containment a model of their containment for their proposed PWR.

This effort is thought perhaps to take place in the next year or so. We hope to gain the benefit from that. We have current interaction trying to get scheduling and coordination.

MR. SIESS: Now, are they thinking leakage?

MR. COSTELLO: I think not.

MR. SIESS: And the Canadian test?

MR. COSTELLO: Leakage. And we still have on the agenda when we get around to thinking about what kinds of seismic issues are relevant and of significance for containments, we will have our eye on some sort of cooperation with the Japanese on the large shake table. No agreement has yet been reached, and we are still hopeful. We still have time.

And I trust I have given you enough of or perhaps more than you really wanted of the background and status of where we are. I guess I will next turn the proceedings over to Dr. Walter Von Riesemann from Sandia Laboratories who is the overall program manager for Sandia and who will coordinate Sandia's presentation.

I think for purposes of efficiency I might, since as questions begin to flow they may be better answered by some other person from Sandia, so I might just identify them as they sit over here on the Sandia side of the house.

Dr. Von Riesemann I think you all know. Wayne Sebrell, also from Sandia, concentrates a lot on the budget and management end of the program. Dr. Tom Blejwas does a great deal of the analytical work. Next to him is Dan Horschel, who also works mainly on the analysis effort. Al Dennis, on the end of the aisle, is responsible for overall planning of the -- planning and scheduling of the effort. And Dr. Ron Woodfin, in the back, is pretty much in charge of the experimental effort.

So if you would like to start now with Dr. Von Riesemann.

MR. SIESS: Franz, did you have anything you would like to put in at this stage of the game?

MR. SCHAUER: No, I do not, sir. I think I would say I think the comments that you indicated are right on. The emphasis seems to be, on this particular program, is on structural capacity; and we have wrestled with the leakage question at a very low level of research effort for many, many years.

I think that we do need to put some time into at least some limited testing to assure that our indications on increased pressure do develop the margins that we are going to get on this test.

MR. SIESS: Thank you.

I think this has set the stage for the Sandia presentation. It is pretty clear, I believe, that it is directed at modes three and four of the four modes I listed. We will decide from the discussion how it relates to three and four, and I think for some of this other discussion we need to get to the attention of some other people in Research, and we will see that they get the minutes and maybe see that they get a copy of the transcript. And I suspect that we will have some comments on the long-range research plan that may relate to this to get some focus on this, because it is too compartmentalized.

Okay, Walt.

(Slide.)

MR. VON RIESEMANN: My name is Walter Von Riesemann.

I feel that before I begin the formal

presentation, Dr. Siess, there have been a lot of questions raised, and I have a few comments in regard to some of them.

On one, the corsequence of a leakage will depend upon the timing, he mode and the location within the containment structure, i.e., a failure late in the accident might be less severe than one early in the accident.

Now, this program does not try to attempt to determine what time the accident or failure will occur, but it is of importance. Also, we realize the problem with isolating the containment. We have unofficial reports that tests on containments before the requalification -- in other words, to see what the leak rate is -- the leak rate is on the order of 10 percent of the volume per day in contradiction or correspondence to the .1 percent volume per day which is the standard for most plants.

MR. EBERSOLE: Would you clarify something for me? You made a comment, leak later on, the evolution might be less consequential. There used to be a design called the bridge containment which deliberately leaked in the first stages when there was virtually no radioactive fission products present but you could get rid of the mechanical load. Subsequently, as I recall, the consequence was less because you had less differential for leakage.

MR. VON RIESEMANN: You are reducing the hazard, so to speak, there by the pressure. The other thing, though, is if you do not do that, if there is plate out of the inventory that occurs, the timing is important again.

MR. EBERSOLE: There is a front and back end to this.

MR. VON RIESEMANN: Right. It is very much scenario dependent and site dependent and containment dependent. In fact, the GE Mark III, which is a freestanding steel containment, the GE people are hypothesizing that if failure does occur, it will be after the material goes through the suppression pool, and the torispherical dome might fail, and only be a small failure and hence not of much consequence.

That is one of the questions that this program can answer. The other thing is in dynamic loading.

MR. SHEWMON: It seems to me you have postulated a scenario I did not hear.

MR. VON RIESEMANN: Will the torispherical dome leak only a small bit or will it be a catastrophic failure? They are hypothesizing a small hole.

MR. WARD: To go back to your first statement, what you said about typical leak rate, is 10 percent a day --

MR. VON RIESEMANN: I do not want to say typical. I said it was unofficially reported on a containment that some tests were done before requalification.

MR. WARD: On a containment?

MR. VON RIESEMANN: On a containment design. There is a lot of discussion on leak rate, obviously, and since the last meeting in July we have talked to people at Battelle-Columbus, for example -- Rich Denning, Pete Cybulskis, Ian Wall -- just what do you need to know in doing a probabilistic study.

If you look at WASH-1400, in many cases if the leak rate was less than 100 percent of the volume per day, it did not mean any difference on the consequence. And 100 percent per day is a fairly large hole. Is it four inch? It is a fairly large size hole anyhow.

MR. SIESS: You mean under 100 percent per day, from 1 to 100 you had the same consequences?

MR. VON RIESEMANN: Right.

MR. SIESS: But you did not know --

MR. VON RIESEMANN: Did not overpressurize to cause catastrophic failure.

MR. SIESS: How high did you get the pressure on the containment with 100 percent per day leak rate?

MR. VON RIESEMANN: It depends on how fast the loading is increasing, obviously.

MR. SIESS: There are lots of scenarios. Did anybody look at them? Zero, 50 percent -- you cannot build up the pressure any faster than that to take it all the way to capacity. In other words, it leaks faster than the pressure can build up.

MR. VON RIESEMANN: I am jumping way ahead, but this June -- and Wayne Sebrell will talk about this -- we are putting on a workshop in Washington on containment integrity. One of the topics in fact is looking at leak rate, the entire question, measurement of it and what is important.

MR. SIESS: I am looking forward to it.

MR. VON RIESEMANN: So I might add that -- you asked the question -- the Canadian tests were essentially structural tests. They are on line. They have, what is it, neoprine mylar inside. They had a lot of trouble, in fact, doing the experiment containing pressure. They were hydrostatic. They had no penetrations. And the leak rate determinations were done separately on specimens of concrete.

The U.K. test, the proposed test, is also structural, and I am not sure whether they are going to include penetration or not It is too early to tell.

MR. SIESS: Pneumatic or hydraulic?

MR. VON RIESEMANN: They will be hydraulic also.

MR. SIESS: I do not think you can get a catastrophic failure of a containment with that load.

MR. VON RIESEMANN: They got a section out of the Canadian containment but not catastrophic in the sense that -- yes, that is one of the problems we are facing in our program using pneumatic pressurization for safety concerns. And you will see why we are going out to where we are going this afternoon.

The other thing was on electrical penetrations. There is contradictory information on that. I think Yankee Rowe did an analysis -- and Wayne will talk about that later -- that their penetrations were all right for certain conditions. But I have also talked to Bill Farmer in the electrical engineering branch of Research, and we at Sandia have the qualification testing evaluation program of the components, and they are going to do some testing on electrical penetrations, at least some preliminary testing under pressure, as this question always does come about.

MR. SHEWMON: Pressure and temperature?

MR. VON RIESEMANN: Pressure and moderate temperature.

MR. SIESS: The Browns Ferry failure was attributed entirely to temperature.

MR. VON RIESEMANN: One of the problems we are having is one of the test apparatus is set up for a LOCA condition, and so the temperature is up to 340, somewhere in that Farenheit, and maybe not high enough.

The other problem people should also address is aging effects on these materials.

Finally, the question of penetrations, originally we looked at a parallel effort in the program where we would do, if you will, "structural activities" and doing separate effects tests on penetrations, but to do budget restrictions they were delayed. And so we do not have that in our program.

With that I would like to begin the presentation.

(Slide.)

What I would like to cover this morning is a brief overview by myself of the program; then the program planning activity from a management viewpoint of looking at resources and time by Al Dennis; and then the question was raised last time, gee, why don't we use some existing facilities -- Al Dennis will address that

-- and also the fabrication and design of containment models. Then Tom Blejwas and Dan Horschel will talk about the analyses that have been done, and Ron Woodfin will talk about the experimental program. And lastly, Wayne Sebrell will talk about the program schedule and related activities, including some of the IDCOR activities, the foreign activities, and the workshop.

And I was very optimistic and had lunch at 11:30. I think we can ignore that point right now.

Next vu-graph, please.

(Slide.)

I think we really covered this to a large extent already, but why are we interested at all in containment strength; and we have already discussed the reason it is needed in risk studies. If you look at the filtered vented containment system for accident mitigation, you need to know the containment strength in order to design that event. You also need to know the strength in fact if that equipment is necessary.

One of the problems with the filtered vente containment is of course dynamic loading: will it be able to vent quickly enough. And as was previously mentioned, the knowledge is also important for the severe accident mitigation strategies, what should be done next in the accident, what is the strength of the containment, and then finally for planning emergency preparedness.

MR. SIESS: Walt, I think we mentioned at the July meeting last year that some of the designs that some of the people are talking about, filtered vented containment, all they felt they needed to know was essentially a fairly reliable lower bound of containment capacity; that they were not about to design their vents, you know, for the load that would rupture the containment. They just wanted to vent before there was any chance that the containment would go. And establishing a reasonably reliable lower bound is a lot simpler than trying to find out when and how it actually fails. They are almost two separate problems.

MR. VON RIESEMANN: But for the risk studies you want to know your distribution of failure.

MR. SIESS: But the distribution of that lower bound would be pretty narrow.

MR. VON RIESEMANN: For the design of the events, yes.

MR. SIESS: The distribution of the 100 percent a day leak rate or 1000 percent a day leak rate, that is going to scatter anyway. They are two different questions. There are going to be two different ways of getting answers for them.

MR. VON RIESEMANN: One you are coming from one direction, one from the other almost.

MR. SIESS: That is right. Now, if you are not doing anything about it and the pressure is sitting there increasing and you want to know what to do about people, then you really are going to be looking at your uncertainty bounds, not at the level.

MR. VON RIESEMANN: Right, yes.

MR. SIESS: And I am not so sure that the bottom end of that is not going to be about the same place no matter how you come out, the actual level.

(Slide.)

MR. VON RIESEMANN: Again, this is a restatement of the program objectives. We are looking at only light-water reactor containments. We are looking under severe accident conditions and severe environments, and I will describe those in a minute.

We are also going to assess selected predictive numerical methods, and Tom Blejwas will discuss that in quite a bit of detail.

(Slide.)

The containments we are going to look at and the loadings are picturized on this vu-graph. We are obviously going to look at static pressure. We are not too concerned where that really comes from. That is not the big problem here.

The way to simulate that, we can either use hydrostatic or pneumatic loading. In the early tests we are going to use pneumatic loading.

Dynamic pressure, that comes about largely from the hydrogen concerns. Whether in fact there will be hydrogen detonations is a question yet to be answered. They would cause spatially varying loads and loads that are unsymmetric, and unfortunately, most likely an infinite number of varieties where you would have to rely heavily then on analysis capability.

Also, in the case of dynamic pressure some of your isolation valves might not even isolate within that time span, and you might get the loading right on the structure, so you might even have a failure potentially before the isolation valve, even if it was open.

Lateral loadings, we are thinking primarily here of earthquakes. On the right hand side we have shown the containment types. What is missing is a BWR-I and BWR type II containments within the context of this program. There is just too many out there.

Of the steel we are looking at primarily what we call a freestanding, some people call a hybrid, and then obviously reinforced concrete and a prestressed concrete.

(Slide.)

The approach that is being used is a combined --

MR. SIESS: The prestressed --

MR. VON RIESEMANN: We get into a semantics problem here.

MR. SIESS: Some of the hybrid has a steel bottom.

MR. VON RIESEMANN: The pure steel, if you will, we are not looking at, or we might look at for another reason. There are a lot of questions, too, by the way, about the base mat, the strength of that. In fact, the more we present this program to the various groups, the more questions that seem to be raised on uncertainties within the design.

We are using a combined approach here where we do scale model experiments and also, of course, analysis. The analyses are used both before the experiments, and then the results are used to benchmark the codes. And the end product we see is this actual data base that can be used either for our numerical efforts or someone else's. We will have analytical methods that we have assessed. And then finally we hope to have a reliable method for assessing the capabilities of these containments.

(Slide.)

MR. SIESS: Let me get one point clear. Your experiments, your model tests are intended to be used with the analyses, to validate the analyses, right?

MR. VON RIESEMANN: Yes.

MR. SIESS: You will analyze the model itself?

MR. VON RIESEMANN: Originally we were looking at --

MR. SIESS: I just wanted to be sure.

MR. VON RIESEMANN: Let me -- that will be answered perhaps in three vu-graphs, but also we were originally looking at replica modeling, but that is too expensive, so we are going to prototypical models, and we are going to, in essence, use the results to evaluate the codes.

A background study was performed, and I went into great detail on this last time. I will just go over it very briefly this time.

We looked at the types of containments that are out there, and we were amazed at the different varieties even at a given site. Indian Point 2 and 3 are different. The stiffening is different. The penetrations are different. There is no such thing as a given type containment.

We also looked at the requirements in the

ASME/ACI code, and obviously they are for design and not for calculating failure, but there are some differences between the two also.

We looked at the previous tests, and very few have done. Ones that I mentioned in Canada. There has been one in Poland and a few in Japan, but none on the steel containments that we could find.

MR. SIESS: What about tests on steel vessels rather than nuclear power plants?

MR. VON RIESEMANN: Yes. There have been quite a few to failure, but in some of those the ratio of the radius of thickness was quite different, and the materials are different. But we have looked at some.

MR. SIESS: These are more like tanks.

MR. VON RIESEMANN: Real pressure vessels, if you will, than the containment buildings. We have a radius of thickness on the order of 500, which is a fairly thin structure. If you are thinking something 150 feet in diameter, minimum plate thickness one-half inch, that is fairly thin.

MR. ZUDANS: Are these aerospace vessels much thinner than containment?

MR. VON RIESEMANN: They are --

MR. ZUDANS: They are also tested?

MR. VON RIESEMANN: They have some there, yes.

MR. ZUDANS: But you have not looked at them?

MR. VON RIESEMANN: But they do not use normally these materials that we are using in containment.

MR. BLEJWAS: These structures are quite often different.

MR. VON RIESEMANN: I think the basic structure is often different, too, the construction.

MR. ZUDANS: Yes.

MR. VON RIESEMANN: We also looked at what is required to do scale modeling, particularly in the problems with doing dynamic testing because the earthquake that can -- well, you have to worry about what earthquake are you going to model and what technique do you have to put on the load, and that implies then the load simulation; static test, again not much problem; dynamic, we might even have to use hydrogen gas or HE. We are not sure which technique we will use.

MR. SIESS: If you are using the test simply to validate an analysis, it does not have to be an exact simulation.

MR. VON RIESEMANN: No, as long as your time history is not so different that you are introducing a new behavior in your model. You do not have the need of a one-to-one correspondence at all, likewise obviously an earthquake analysis.

MR. SIESS: In order to get the same mode of failure.

MR. VON RIESEMANN: Right.

With that background study we also used the input from the advisory group, and with that we came up with a program plan. At the end of the meeting today you will get a preliminary copy of that.

We went through, as you can imagine, several iterations on that one.

Dr. Shewmon.

MR. SHEWMON: On the seismic, the seismic is the superimposition of some static in the seismic load, and you end up with some sort of a space or where --

MR. VON RIESEMANN: On the seismic loading the Japanese, for example, are just putting on a time history onto the containment without any internal pressurization loads in one test. In another test they are going to put on internal pressurization loads and the earthquake, and they will measure leak rate before and after the test, obviously not during the test.

Did I answer your question?

MR. SHEWMON: Yes. That is something else that this group gets into often.

MR. VON RIESEMANN: The combination of loads, LOCA plus SSE?

MR. SHEWMON: We have a new one, a pressure vessel a week after a LOCA which undergoes an earthquake. I wondered if that is where you are going?

MR. VON RIESEMANN: That is one of the places where we can go. We are not going to do that in our program because it gets too complicated.

MR. SHEWMON: Good.

MR. VON RIESEMANN: You know, if you do one thing, you can later on do that final analysis.

The advisory group, the next one I think has already been shown to you. I will not go over that again.

(Slide.)

MR. SIESS: What is Tom Ahl's background?

MR. VON RIESEMANN: He is in charge of the nuclear group there doing the analysis or design for containments. I also have Rich Denning on the chart. As you might know, Rich and Pete worked very closely at Battelle-Columbus.

The evolution of the current program plan, we had a first version which of course was preceded by many preliminary versions.

(Slide.)

And then there should be many dots in between, and we finally have a version now, and we had to change some of the activities because of time restraints and budget limitations. We were first considering using replica models to check the scaling among other things. Also, we were going to conduct parallel activities, but in the final version that we have now we are going to look at protypical models, and this will be discussed in detail later. And we are delaying to some extent the dynamic and seismic activities, and we are also hoping heavily to count on work that is being done in other countries, to interact with them, and of course interact with the DOE if they have a program, and IDCOR.

MR. SIESS: What did you mean by replica models?

MR. VON RIESEMANN: Where you use your laws of scaling, if you will, okay.

MR. SIESS: All right.

MR. VON RIESEMANN: And they become, as you know, expensive.

MR. SIESS: Prototypical means it looks like it but you just analyze what you have.

MR. VON RIESEMANN: You can take certain, if you will, liberties. It looks very similar, okay, but maybe the thickness has changed. For example, it is not pure scaling on thickness for ease in fabrication.

MR. PICKEL: In the large diameter thickness ratio category, localized fabrication construction problems may play a fairly major role. Have those been considered in your modeling and program planning?

MR. VON RIESEMANN: Okay. In the consideration, if you will, of buckling where initial imperfections would say play a strong role, we are dealing primarily with internal pressurization which will not have that large an effect -- initial imperfections will not have that large an effect on it.

We are looking at building -- and this will be discussed later -- a tenth scale, for example, steel model, picking a tenth scale to pick up actual construction practices. So we are considering it, and the advisory group strongly recommended that we consider this.

MR. ZUDANS: The other models are what scale?

MR. VON RIESEMANN: Thirty-second.

MR. ZUDANS: This works out to be a fabricated shell or machined?

MR. VON RIESEMANN: Machined. Rolled and welded, not hollowed out, if you will, from a piece of material. The overall program plan is to look at the loadings, and this sequence again, the static, dynamic and earthquake; and we have switched the containments on the earthquake loading simply because we feel there is more that has to be known on the reinforced concrete than the hybrid steel, more questions.

We picked the hybrid steel because of their low pressure, lower requirements on design on some of them. The ice condensers in some of the Mark IIIs are design pressures of the order of 10 to 15 psi gauge where the large dry, the prestressed concrete is on the order of 60 psi gauge.

It is obviously not clear until the hydrogen program really settles down whether in fact there would be detonation loads from hydrogen.

(Slide.)

But even if there is not, there might be some quasi-dynamic loads that might be asymmetric that we might have to consider.

MR. ETHERINGTON: I missed the significance of hybrid.

MR. VON RIESEMANN: Excuse me. This again is a steel containment on top with a concrete base mat. The ice condenser is typical of this.

MR. SIESS: Jim Costello in his schedule used category static pressure, unsymmetric pressure and seismic effects. Is your dynamic the same as unsymmetric?

MR. VON RIESEMANN: Dynamic is the same as unsymmetric because a lot of the dynamic loading will in fact be unsymmetric. It will be almost, you know, impossible to get a hydrogen detonation that produces a nice symmetric load.

MR. SIESS: That is scheduled under his schedule for FY 85-87. Just a suggestion. I think we could almost defer much discussion on anything but static at this meeting.

MR. VON RIESEMANN: That is what we are planning on doing. That is where the emphasis on the program has in fact been, except, Professor Siess, some of the planning to look ahead. You know, if we want to decide to do seismic tomorrow, you just do not do it that quickly. We are trying to see what the Japanese are doing, for example.

MR. ZUDANS: One question, Walter, on this dynamic internal overpressurization which would result in nonsymmetrical loading. Do you plan to collect the data on precise surface pressure history over the entire structure so that you can certainly relate the analysis, the results of your tests?

MR. VON RIESEMANN: You have to know what loading you had on the structure. Otherwise it is just a go/no go test. So we do plan on doing that.

MR. SIESS: That is why I want to limit it to static, because I think we could devote two days to the dynamic unsymmetrical and seismic.

MR. VON RIESEMANN: Yes. But, Professor Siess, that is where it is interesting.

MR. ZUDANS: That is where it is interesting, because as far as I am concerned, static testing is not interesting.

MR. EBERSOLE: Are the seismic loads assumed to coincide with classical LOCA loads?

MR. VON RIESEMANN: No. As far as I know at this point we are going to decouple that.

MR. EBERSOLE: Decouple it.

MR. VON RIESEMANN: Again, it depends on funds, complexity and where at that point in time we re going. There is also some studies -- perhaps the NRC people can answer this, if they are the right group here -- but studies being done by Lawrence Livermore Labs on load combinations and potentially the fact that they may be decoupled.

MR. SIESS: Gentlemen, I am going to ask you to limit this discussion to the static internal overpressurization which is the next three years of this program at this meeting, and I think we will have plenty to talk about on that. You will have another meeting to talk about the other two phases of it if and when we get to them.

(Slide.)

MR. VON RIESEMANN: Now, if we look -- this will be discussed again later, but the timing scale for the program. And this is for only static loadings, looking only at the steel models. Again, I use the term steel and hybrid steel and freestanding interchangeably.

Looking at 1/32 steel scale models, 1/10, and then also looking at reinforced concrete. I guess I have to take back a word I just said. There is some dynamic pressure loading work, lateral loading. It is just the feasibility of doing it and what should be done but not building models, because there might be a long lead time necessary for those items.

(Slide.)

Now, the steel models that we are looking at, these are 1/32 size. They do not model any particular containment. We are going to be doing two tests, and you will see at least the beginnings of the fabrication of these models this afternoon. We have a hemispherical dome, cylindrical shell, and then we add the horizontal stiffeners, and then we add the penetrations and stiffeners to that.

MR. SIESS: What is the streak down the left side?

MR. VON RIESEMANN: That is the artist's rendering of showing it is a cylinder. Hopefully it is not imperfection.

MR. SIESS: I thought maybe that was after the test.

MR. VON RIESEMANN: After the test there will be pieces all over the canyon.

We had a lot of discussion with the advisory group on what type of head should we put on, and either we go hemispherical or the torispherical ellipsoidal -- they use both -- and the questions then come about, you know, the cost again.

MR. SIESS: All the steel ones do not have the hemispherical one on top?

MR. VON RIESEMANN: No, no. Life is not very simple at all.

MR. SIESS: If you can analyze one, you can analyze the other.

MR. VON RIESEMANN: I guess what Professor Zudans is getting at is the problem with the torispherical is the buckling in the knuckle. Under some loadings they might buckle, well, at a given load, and the behavior beyond that buckling load is not known.

Now, buckling to me is not necessarily failure, and people have done experiments, and the ASME have been surprised at the buckling requirements.

MR. SIESS: But you do not have any penetrations up in the head, or not very many, do you?

MR. VON RIESEMANN: There are penetrations, but we are not putting any in there. To my knowledge not too many.

MR. ZUDANS: How about the three-foot hole?

MR. VON RIESEMANN: That is something new. There are some 500 penetrations in the shell of all different types.

(Slide.)

The program status as of now, I mentioned the 1/32 size steel molds are being fabricated. You will see where we are doing the experiments this afternoon. The analyses are well under way -- you will hear about that in a few moments -- and we hope to begin testing the steel models in the third quarter of this calendar year.

(Slide.)

And, in summary, what we hope to accomplish with this program is using the experiments and analysis to come up with a reliable capability for predicting the capacity of light-water reactor containments.

Dr. Shewmon.

MR. SHEWMON: You are going to fabricate a 1/32 scale, 360 or whatever you want to do for 3-D models, is that right?

MR. VON RIESEMANN: Yes, sir.

MR. SHEWMON: What was the argument for doing that instead of a one-fourth scale, 60 degree segment or something which would go with something that had penetrations and welds, and where I would guess or I think from what I have heard most people would guess the failures you might encompass.

MR. VON RIESEMANN: One of the problems is the boundary conditions, and Dr. Blejwas later on will show some of the analyses done on that. Unfortunately, the large effect that we have, it seems, around the circumference on a penetration we also felt that getting those boundary conditions on it can be done. It would be just as expensive as building the full model.

MR. SHEWMON: When you say boundary conditions are you talking about the computer analysis or actually building?

MR. VON RIESEMANN: Building. Building, sir. In that case it is easier to do it in a computer than it is to do it. Sometimes the reverse is true.

MR. SHEWMON: Yes.

MR. SIESS: Go ahead. I have a general question I suspect that may come up later, but it is philosophical. It is my understanding that you are going to test replica models of these 1/32 scale steel vessels, is that right?

MR. VON RIESEMANN: Well, let's call them prototypical.

MR. SIESS: I mean you are going to test two.

MR. VON RIESEMANN: Yes. Two. Two without --

MR. SIESS: You are going to test a pair of each.

MR. VON RIESEMANN: A pair of each.

MR. SIESS: Why?

MR. VON RIESEMANN: Well, would you feel confident that one test would give you the result?

MR. SIESS: Absolutely. I have an analysis I instrumented taking advantage of symmetry so that no bad reading cannot be checked against another reading in analysis. What do you do if you get different answers on the two? Then you have to go to a third. That is two out of three logic.

I have never heard good arguments for duplicate specimens on any basis. If you do not have an analysis you expect to test the specimen and scale it up to a prototype. Two is not enough. It may be if they are the same, but I have never seen an instance where I had an analysis and was trying to validate it that I could not do it with a single test with enough instrumentation taking advantage of symmetry. And you have symmetry. You have radial symmetry on the first two models. On the third one you can at least get symmetry about a diameter if you wanted to. And if something really terrible went wrong, poor fabrication or something of that sort, then you could always test another one.

MR. VON RIESEMANN: We have the option of not performing two tests if we see downstream things are going very well.

MR. SIESS: If you start out with the idea of two and you do one kind of instrumentation, if you are planning to use only one you would take more advantage of symmetry; that is, you essentially make two specimens in terms of instrumentation, not three or four.

What has your experience been in this, or other people's?

MR. VON RIESEMANN: On taking more than one model?

MR. SIESS: Yes.

MR. VON RIESEMANN: We normally -- well, for example, let me think about the testing we did on turbine missiles impacting the concrete panels of a containment building. We did a test with a given missile at a given velocity, and then we took another panel and we changed the velocity.

MR. SIESS: That is not the identical test.

MR. VON RIESEMANN: I know. We did make those changes, but these were backed up by scale model teting at SRI to show that things were all right, plus the fact that the loading conditions were not much different from one test to the next. You could perceive there were large differences.

MR. SIESS: I started with the first specimen, a simple shell, a hemispherical dome. An analysis on that, you know, does not have any great big open questions in it that I have to validate experimentally.

MR. VON RIESEMANN: It does, sir, if you go way beyond --

MR. ZUDANS: The biggest problem, from what is published in the literature, arises from lack of precise knowledge of boundary conditions in these large deformation tests. Therefore, you do not know how to set up the computer run. In your case you will pay extreme attention to precisely defining and in fact precisely measuring your boundary conditions. So therefore, your analytical model, except for material property prediction, should give very good results. If it does not, then it is more of a failure of techniques used for analysis.

I tend to agree with Chet that really unless you have fabrication defects, you find in the process of testing you do not need a second model.

MR. SIESS: Then you are almost approaching statistical -- two is not very many.

MR. ZUDANS: There is no statistical basis for this at all, not with two pieces.

MR. SIESS: You see, it is a deterministic type of test, a deterministic type of analysis. And once I go beyond one specimen I do not know where to stop.

MR. ZUDANS: Well --

MR. SIESS: I have been through this years ago in other fields and decided that one properly instrumented is better than two.

MR. VON RIESEMANN: Than two properly instrumented?

MR. SIESS: I cannot see any gain beyond the one. And we never had an indication where we needed another one. I was just wondering if you had some experience where with a single test there were so many questions that came up that you had to go out and make another one, and if so, how much time do you save by fabricating that one rather than planning in advance?

MR. ZUDANS: I would see a need for more than one in cases where you have great asymmetries and where your attempt is to produce ultimate capacity in terms of some specific mode of failure. For example, you reinforce your penetrations in one prototypical model, and you run the test and it did not fail around the penetration but failed in the main shell. So you have no information with respect to capability of that penetration because you cannot rerun that test.

Now, I can see that you would want to make another model with different sizing of the penetrations and find out that mode of failure to have some information on that, but not for two identical models.

MR. VON RIESEMANN: But the literature is full of experimental results, for example, on pressure vessels with holes in them where the results are quite different from one test to the next on identical, supposedly identical --

MR. SIESS: Now, if that is true, then I cannot stop with two.

MR. VON RIESEMANN: Unless suppose the two agree?

MR. SIESS: That does not prove a thing.

MR. VON RIESEMANN: Where do you stop then?

MR. SIESS: That is my problem.

MR. VON RIESEMANN: Don't you have --

MR. SIESS: If it is random variations, if they are really there, you have to have a lot of specimens.

MR. ZUDANS: I do not know that --

MR. SIESS: Two identical specimens that give identical answers would suggest that they are not random variations, but not with a very high confidence level depending on the probability. You see, it is always possible to make a second test, but as Zenons was talking about, I can visualize dividing the thing up into quarters and putting two penetrations in of one strength and two penetrations of another. That gives me some redundancy right there.

The other procedure would be to just divide it in half, put strong penetrations on one side and weak on the other, but that would not give you any checking, you see. But with radial symmetry you can do a lot of things, and you can always go to another specimen. But the idea of starting out with pairs --

MR. WOODFIN: You asked whether we had some bad experience. My name is Ron Woodfin, Sandia. I can tell you the turbine missile concrete impact experiments for EPRI, and under EPRI's direction we had only four tests, each of which was considerably different. And the bottom line is we got no answers at all out of that because it raised more questions than it answered because they tested at the wrong conditions.

MR. SIESS: But four is not enough.

MR. WOODFIN: Okay. These are four separate experiments trying to look at four different sorts of things. Each one raised enough questions that we only have hints that we know any more about the program, the process than we did at the beginning.

MR. SIESS: You see, that is a very good argument for testing more than one specimen but not for testing companion specimens on a straight schedule. That is an argument for testing one.

Now, if you get some interesting answers and you have things you cannot explain, you think about now what can I do about it on the next one to answer that.

MR. WOODFIN: The problem that came about -- at any case, that is true, what you are saying, but in this particular case we had originally suggested three of each one so that we would have a very minimal statistical base. In this case that was cut back strictly through economic considerations. However, we do believe that it is necessary to show that you can repeat and do the same thing twice. And I guess I basically do not agree with your assessment that one is enough.

MR. SIESS: What is necessary is to show that you analysis will reasonably predict what happens, and if it does it on one I have a certain level of confidence for something as simple, say, as that first shell --

MR. SHEWMON: Before Ron leaves I would be interested in those tests, whether there was a variation in the orientation of the projectile that was doing this, or was the projectile spinning? Did you just have it on a slab? Did you hit it? All of them were on the same --

MR. WOODFIN: We did two orientations, and by a cursory examination of the data it appeared that the piercing orientation caused more severe back face damage. However, in a closer look at the data it seems to indicate that maybe the reverse is true, that the actual back face kinematics were more severe in the case of the blunt orientation.

This is something that we now only have a hint at, because we do not know if there is a random process here or not since we only did one. If we would even just repeat the two experiments that we did exactly as we had done them before then we would have an idea as to how much was deterministic and how much was random, and we do not know that now. And I am submitting that we can get the same in this program if we only did one.

MR. SHEWMON: You did concrete, is that right?

MR. WOODFIN: That is correct.

MR. SHEWMON: Everyone knows you should build out of steel anyway.

(Laughter.)

MR. SIESS: Walt, assuming you have six specimens, A-1 and A-2, B-1 and 2, and C-1 and 2, what is your order of testing?

MR. VON RIESEMANN: Doing the clean vessel first without anything on it.

MR. SIESS: Both clean vessels.

MR. VON RIESEMANN: Right.

MR. SIESS: Again, I would -- I am thinking about time and money, and I do not think we have all that much time, although maybe we do, on this structural integrity failure. Penetrations become the big deal, and I am not quite sure how much money we are going to have over the next four or five years the way things are going. And I would be inclined to say I would test A-1 and B-1 and C-1 and then I would decide whether I wanted four more Cs or wanted to go over and do that second clean one. That is just a suggestion worth thinking about.

Going to two I can see some logic, but I cannot see the logic of stopping at two.

MR. WARD: May I ask a question, Walt? Do you expect your model to be very directly predictive of the failures? I got the impression from something you said earlier that you are really expecting to normalize the model.

MR. VON RIESEMANN: I guess I am missing what you mean by "model." The computer model?

MR. WARD: The computer model. Do you expect your analytical model to be very directly predictive of failure of your experimental model?

MR. VON RIESEMANN: I will let Blejwas answer that later, but I think on the steel, yes, we do feel that. Reinforced concrete, prestressed we would have less confidence at this point in time.

Dr. Zudans had a --

MR. ZUDANS: I just wanted to bring back this question of single vessel testing. If you do the first clean model, which is really rolled and welded so it is fabricated, I assume the hemispherical head is a pretty accurate piece.

MR. VON RIESEMANN: Yes.

MR. ZUDANS: And then you would tune up your analytical model to this first test within linear range which you can repeat hundreds of times until you reach the point that the analysis agrees exactly with your test. You know, you have adjusted your materials properties properly, and then when you do go with a test beyond the linear range it is impossible to produce two identical models. They will behave differently. The local strain distributions will not be the same. They will very ever so little. But it affects the range dramatically. So when you finish testing the two clean shells I am wondering whether you will be able to make any more judgment than you do on one. I am wondering. I think Chet's recommendation is an interesting one.

MR. SIESS: It is not a recommendation. It is a question.

MR. ZUDANS: Well, that is all right.

MR. SIESS: Have you finished your part?

MR. VON RIESEMANN: Yes.

MR. SIESS: I want to take a short break.

Oh, Harold has a question.

MR. ETHERINGTON: For failure of the same pressure is there any feature of the containment that would not be modeled linearly?

MR. VON RIESEMANN: I am afraid I do not

follow you.

MR. ETHERINGTON: The thickness is 1/32. That is fine. The reinforcing ring spacing is also linear. Is that true all over?

MR. VON RIESEMANN: No. We do not have a perfect replica model, sir, of the hole size. For one thing, we did not try to model a given containment.

MR. ETHERINGTON: No. I know that. But is there any region where it would not be linear?

MR. VON RIESEMANN: Only where fabrication difficulties come about.

MR. ETHERINGTON: Yes. I was talking about from a stress point of view.

MR. VON RIESEMANN: We are trying to keep from a stress viewpoint the scaling --

MR. ETHERINGTON: From the stress point of view it should be scale in every respect, is that right?

MR. VON RIESEMANN: That is right. In the static dynamic those are the problems.

MR. ETHERINGTON: Yes, I understand.

MR. VON RIESEMANN: I would like to ask one question before we take a break.

Due to lack of time, if it is all right with you, we might skip the survey of existing facilities if the membership agrees that it is not appropriate to try to test an existing containment.

MR. SIESS: I think that is worthwhile. What I was going to ask you --

MR. SHEWMON: You mean worthwhile to skip.

MR. SIESS: Yes. You have allowed three hours from the time we leave here until the time we get back for the tour. Would there be any problem if we ran over this morning some and maybe went until 1:00, if necessary, for the meeting and then started the tour at 2:00 and got back here at 5:00? Would this present you with any problems?

MR. VON RIESEMANN: There is no problem.

MR. SIESS: In that case I think we will try to just go straight through this morning and get everything in we can and go to lunch and then go out there.

MR. VON RIESEMANN: Okay.

MR. SIESS: Okay. The committee will be working until 5:00 today for which they should feel fortunate. Is anybody leaving early on a plane?

(No response.)

I know a lot of them are staying over for a meeting tomorrow. Okay.

MR. VON RIESEMANN: There is also a question about cameras, binoculars, tape recorders, et cetera.

They will not be allowed on the trip.

MR. SIESS: Cannot take a camera?

MR. VON RIESEMANN: No, sir.

MR. SIESS: Hardly worth going. Last time we went out there, to save all the clearance hullabaloo we just had a guard go with us.

MR. VON RIESEMANN: You all have clearances now, I believe.

MR. SIESS: We decided we would just go as visitors. They sent a guard along, and it worked out fine.

Okay. We will take about ten minutes.

(Recess.)

MR. SIESS: You may proceed.

MR. DENNIS: Good morning. I am Al Dennis of Sandia Laboratories.

(Slide.)

And the first topic I am going to discuss with you is our program planning. On this the planning is limited to the small scale model tests under static pressure.

(Slide.)

Sandia uses a program called PPARS for program planning. This was developed by NASA, and it allows input from all people.

(Slide.)

It allows input from all the participants in the program on how long it will take them in calendar time and in what resources they will require as far as money, man-hours and facilities to accomplish their tasks.

We combine all this information, and the planning output gives us a calendar scheduling of each event. It identifies our critical paths so that we can take appropriate action to make sure that the proper materials are on hand at the right point in time and get schedules; and it gives us a cumulative summary of resources versus time.

(Slide.)

So we can check our financial needs on the program and also our manpower needs on the program.

(Slide.)

Some examples of where we have used this. With small scale models we have used it to develop a network for the shops, and I have included that at the end of your handout there. It is a three-page network that shows all of the work that our shops will be doing in the fabrication of the six small scale models. A similar task has been done for the theoretical analysis so that we can coordinate analysis with the other working programs; we can feed in material properties to the analysis people at the proper points in time. We have worked it in with the experimental portion of the program to make available models for the experimental people at the times they need them and to coordinate their needs. And then we prepared an overview network which looks at the total small scale model testing and lays out times and costs on that. It is a basic activity, but it does put everything in their proper place and allows good scheduling and good budgeting on the program.

Are there any questions on this particular phase of it?

(No response.)

Then we can pass on to the containment models.

(Slide.)

The next topic on Walt's schedule was our survey of existing facilities, and I will have the handouts on this given to you; but we will not go into that topic now to save time.

MR. SIESS: I just glanced through the handout, and I think it would be quite informative for us to read it, but I agree that we can save the time. I just leave the thought with you, I had a professor once, talking about making tests on full scale structures, he said tests on actual structures never answer questions; they just ask them. I think we have asked enough questions here already, I guess.

(Laughter.)

MR. DENNIS: In our search of full scale programs we did not feel we found any that would fill the program. Either they were unavailable to us or they would have required a long NRC program for license modification.

Let's go on to the small scale models.

(Slide.)

Within the modeling program here we will be talking about the small scale steel models, the large scale steel model, and a brief discussion of the concrete models. Much of this has already been gone over by speakers before me, so we will pass through it quickly.

I call your attention on this that what we are looking for is to establish credibility in the post-yield range on structures, credibility of our prediction methods.

(Slide.)

Our hypothesis is that we can get sufficient information from a limited number of tests, and this hypothesis, like others, will only be borne out by our testing program. If we get good correlation we will feel that we have -- the hypothesis was justified. If not then we will have to recommend that we do into a more extensive program somewhat similar to the initial recommendation for this program.

(Slide.)

There was some talk about what a small prototypical structure is, and we have defined a small prototypical structure to be one that has the same characteristics as the prototype, utilizes similar materials but is not a direct replica of the prototype. For similitude relationships to apply we need to go to replica scaling on these, and that turns out to be an extremely expensive proposition.

MR. ZUDANS: If your objectives are as you stated, why do you have to go 1/32? Why not 1/320?

MR. SIESS: Fabrication.

MR. DENNIS: I guess we go into a little bit of history on this one. Initially we were looking at replica scale models about this. We had a contract with Southwest Research who has a good bit of experience in this area to look into the replica scaling of containment models. They went through the failure modes study on the models and determined what components we could and what components we could not get failure modes on at different scales. The 1/32 scale turned out to be the smallest scale where we could replicate a number of failure modes. In order to keep that failure mode replication we kept the 1/32 scale on the models.

MR. ZUDANS: Yes, but if you are talking about multiple or distinctly different failure modes, then you cannot really do that on a two on one model. Each model can carry only a single failure mode because it is going to fail in some single way.

MR. DENNIS: Yes.

MR. ZUDANS: So which specific mode do you have in mind when you make the pick of 1/32 scale? Certainly not the overall shell failure. That you could do without a test.

MR. DENNIS: What we did, we were looking at a particular reactor containment at the time, and our 1/32 prototypical model now maintains many of the essential features of this containment building. We have eliminated some of the areas that we felt we could eliminate safely and bring down costs, but have kept the areas such as the size of the spacing of the ring stiffeners, the relative thickness to diameter ratios on the major penetrations, and that is similar to this particular containment building.

MR. SIESS: It seems to me that your

requirement for your prototypical model is that it represents an adequate challenge to the analysis. You are using it to evaluate the analysis.

MR. DENNIS: Yes, sir.

MR. SIESS: So that is really what you have to compare it against, much more than the prototype.

MR. DENNIS: Yes, sir.

MR. SIESS: It does not follow at all that the smaller the model, the easier it is to make it and test it, or the cheaper it is either.

MR. DENNIS: No. This gives us a rather -- the 1/32 size gives us a rather convenient size test.

MR. SIESS: How big is it actually?

MR. DENNIS: Well --

MR. SIESS: Are you coming to it? That is all right. I will wait.

MR. DENNIS: All right.

MR. WARD: Bigger than a breadbox?

MR. DENNIS: It is about 4 1/2 feet in diameter and about 5 feet high. It is large enough for a man to get on in the inside and apply instrumentation.

MR. ZUDANS: I guess that last statement is really a better reason than anything else for having it that size, so you can really apply your instruments properly, gauges properly; because if you have to reach and it cannot reach, it is difficult, and the cost of fabrication is not that much greater.

MR. DENNIS: No. Even on this size we are manpower intensive, not material intensive on these things, and as we go smaller the manpower requirements actually go up for the precision machining.

MR. ZUDANS: Okay. Enough.

MR. DENNIS: Let's go to the next one.

(Slide.)

We established three guidelines for the models and the experiments. First, economy, and that has been a driving thing for us. The second is reproducibility of results, and we have just gotten into a discussion on that. We felt that with two models if we got similar data, we could accept that as reproducible; if the data was not similar then we would have to go to a third or fourth model. And that has always been a contingency in our plans, and we wanted to have one experiment performed in a model that was large enough to replicate some of the typical construction techniques.

For the steel models this means to get into some of the actual welding processes that are used on the large steel model on a large containment. On the 1/32 model our welding process is not similar to that that is used on the large model. It is under far more control. It has to be for these thin sheets.

We also wanted to be able to use multiple panels within the wall of the containment where now we are using a single sheet that has one weld seam on it. We want multiple weld seams.

(Slide.)

I think this is the results of our study with Southwest. We found that for steel the minimum size we could go to was a thirty-second, and for conventional construction techniques a tenth; for reinforced concrete that should read 1/16 for the smallest size we would need and 1/10 for the conventional construction.

MR. SIESS: It seems to me you were looking to see how small you could make them. Is that right?

MR. DENNIS: Well, we equated smallness with economy at one point in time, but it turned out not to be the case.

MR. SIESS: The better rule is when you are testing models you make them as large as you can accommodate and afford.

MR. DENNIS: That is where we have wound up.

(Slide.)

This shows the test matrix for the steel containment models. The first three groups in the 1/32 I like to view as our experiments. The final one at the

1/10 scale I like to view as a demonstration test, that we will have developed confidence in our ability to predict what is going to happen, and then with the final model we will show those predictions and also the effects of conventional construction techniques.

(Slide.)

Now, this is a look at the three models. You have seen this before in several forms. The interior diameter on the model is approximately 44 inches, and the height is about 66 inches, so it is a nice size structure. It is going to be big enough to get into to do a good instrumentation job and to work with well.

(Slide.)

This is a conceptual sketch of the seventh model. We do not have it designed yet. You will notice that one of the large changes in it is going to an ellipsoidal head on the base. That was primarily an economy on this one to avoid having to build a concrete or other steel system to mount it.

MR. SIESS: That is the way some of them are built.

MR. DENNIS: Yes. This is actually quite close to the Praire Island-Kiwaneh-St. Lucie type of designs.

MR. ZUDANS: It has the concrete insert.

MR. DENNIS: Right.

MR. ZUDANS: You will do that, too.

MR. DENNIS: We can. We have not decided yet. We were looking to put a manway in the base for interior access on it. If we put a replica in the sense of size and equipment hatch in there, it winds up to be about 25 inches in diameter and rather far off what would be our floor, so it is inconvenient for being out of the model to do instrumentation on it.

MR. SIESS: Now, the hatch closure will be simulated as well as the reinforcement around the opening?

MR. DENNIS: On the 1/10 scale we plan to have an opening hatch on that one, so we will have it gasketed and sealed, and we will use one similar probably to one of the plants I just mentioned that uses this overall design characteristic.

We are looking at building this vessel in accordance with the ASME code and to at least have it where we could get an N-stamp on it if we wanted to.

(Slide.)

Here is our matrix we are currently looking at for reinforced concrete designs. At this point in time we do not have any conceptual design on it, but rather we just laid them out. And as you can see, once again it starts simple and goes to the complex.

MR. SIESS: The seismic seal you refer to would be the in plate steel or the --

MR. DENNIS: The 45 degree steel that they put around the lower half of most of the containments.

MR. SIESS: What about the through-wall bars that are in almost all the plants?

MR. DENNIS: We would plan to put shear steel in these, yes.

MR. PICKEL: What is the thickness of the shells on those?

MR. DENNIS: Well, on a full scale plant you run them about 150 to 160 feet in diameter with walls that run a nominal four to four and a half feet thick. On a tenth scale we would probably have to go to a thicker wall in order to keep our construction economy within reason. So we have not settled on either the wall yet or the placement of the steel. That is the next task coming up is to look at a design for these models.

MR. PICKEL: So the diameter-thickness ratio may have to be altered here.

MR. DENNIS: Yes. I would expect it would. Also on the tenth scale steel, I did not mention that, but the wall thickness to diameter ratio will have to be altered on that, and that is in order to allow us to take advantage of commercially available heads and plate.

MR. ZUDANS: I would have expected on that one you would have picked a scale where you can maintain that ratio with the thicknesses that you can find in fabricated plate. That issue is more critical than anything else.

MR. DENNIS: Originally we did, and we were going to build a scale model of that using 3/16, SA 5/16 steel. Three-sixteenths is the only commercially available. It turned out that under those conditions we would have had to pay for tooling for the dome and the base. Tooling costs on those are excessive, and so we went to the thinnest we can buy sections that are commercially available for a hemispherical dome or an ellipsoidal base. Either one is 3/8 inches. That's why we went to --

MR. ZUDANS: How much thicker is it?

MR. SIESS: A ratio of 8 to 10.

MR. ZUDANS: Eight to 10.

MR. DENNIS: Initially we are dealing in about an 800, R/T of 800. We will be down to an R/T of 600 or something like that.

MR. ZUDANS: One more question that pertains to this steel and the phases of your tests and analysis that will go beyond your linear range.

MR. DENNIS: Yes.

MR. ZUDANS: Do you plan to instrument adequately to measure the deformed shape continuously, the entire measured shape all around the circumference and up and down?

MR. DENNIS: Yes, we do. And Ron Woodfin will go into detail on that.

MR. ZUDANS: Good. I will ask the question then.

MR. SIESS: We are going to have a separate presentation on instrumentation measurements.

MR. DENNIS: Yes. Yes, sir.

MR. WARD: What sort of yield or failure pressures are you expecting over this range of models?

MR. DENNIS: We are expecting -- Tom, catch me if I am wrong here -- but I think it is 150 to 200 psi for the steel model, and the yield was -- what was that?

MR. BLEJWAS: It depends on if you mean membrane modeling.

MR. DENNIS: Maybe we had best defer that until Tom gets up.

Okay. If there are no further questions -- wait, we have one more vu-graph.

(Slide.)

And this once again is our current scheduling on the activities. We expect the experimental program in steel in FY 83. We expect the analysis methods and evaluation to be finished in FY 84. We expect the concrete testing to begin in very late '83 and to be finished up in '84, and the analysis in '85.

We are looking to obtain information from the tests to be run in the United Kingdom on prestressed concrete to do that portion of our program. Once again, that was a financial consideration that we eliminated that portion of the program and will rely on others for it.

MR. WARD: Was the Canadian test, the Gentilly, was that prestressed concrete?

MR. DENNIS: That was prestressed concrete.

MR. WARD: It apparently leaked a lot.

MR. DENNIS: I would say that the test history they use --

MR. SIESS: The Canadians did not have a steel liner.

MR. DENNIS: No.

MR. SIESS: But the U.K. did use a steel liner.

MR. VON RIESEMANN: It is not clear on the U.K. whether they will use steel or copper on the liner.

MR. SIESS: The Canadians do not use any

liner. They use epoxy coating.

MR. DENNIS: In the test they used a vinyl liner, and they experienced some liner difficulties in that test, so they went through several loading cycles before they got a liner that took them to failure.

MR. ZUDANS: Without a liner --

MR. DENNIS: And then the Canadians are great proponents of a leak before break concept on their containments.

MR. WARD: And that was water?

MR. DENNIS: They used water as a pressurizer fluid, and they did compare it with an analytical method. BOZAR-V was the code they used, and they got good results between the code and their model after they had changed the material models.

(Slide.)

MR. BLEJWAS: I am Tom Blejwas, and this is Tom Horschel from Sandia, and I am going to talk about the analysis that we have been conducting and plan to conduct for these containment models. I would first like to put the analysis into the context of the overall program, so on your first vu-graph, the second vu-graph rather --

(Slide.)

-- I wo d like to reiterate that part of our objective is to try to qualify methods of evaluating ultimate capacity. So the way we see our task, we will first proceed in designing and building the experimental models which you have just heard discussed. Next, while this is going on we are doing pretest analysis, so that by the time we actually conduct the experiments we will have predictions that we feel are the best predictions we can make at that time. Then we will conduct the experiments, compare our res.its, and at that time refine our analysis.

After we have refined the analysis to a point we feel it is reasonable, then we will present our comparisons for others to view and to make a determination as to the quality or qualification of the analytical procedures.

(Slide.)

Now you have heard we are looking at three different types of contanment models: steel, reinforced concrete and prestressed in three different loadings. Most of what I am going to discuss now will primarily be the analysis of steel containments to static internal pressurization, but I will also touch briefly on the analysis of the concrete containment models.

(Slide.)

Currently, we are in the process of conducting analysis of the steel containments in two dimensions primarily, and we have a smaller task to do some three-dimensional analysis, particularly of penetrations. Those are -- the 2D analysis is primarily what I am going to present today. However, we are also in the process of starting on some concrete analysis doing things selecting codes and looking at what is available around the country for analyzing concrete containments.

In tha regard, we are having a lot of interaction with Los Alamos. They are already doing a lot of reinforced concrete analysis. Also, we do have to do some side calculations for the support structures to be sure when we conduct our test it is the model that ruptures and not the support structures, so that we have a good test.

(Slide.)

Okay, we looked at what was available in the way of computer codes when we started doing analysis about a year and a half ago. We did a survey and these are features that I selected as being desirable for a code for use on the analysis of the steel containments, both for static and dynamic pressurization.

I will not go through all of these, but I think everybody has their own favorite little features that they like to see in the code, and I think most people would agree that these are somewhat desirable.

(Slide.)

I did want to emphasize, though, that there is one feature there that we thought was particularly important for our task, and that is number four, looking at a code that has large displacement and finite strain plasticity capabilities, since we do expect to go well past the linear range.

(Slide.)

Of the codes that we looked at, this is a small sampling. We looked at a lot of different codes, most of them very briefly, because there are so many that you cannot spend a lot of time and use a lot of assistance from people who have already used some of the codes.

Basically, we came up with three potential codes that did not have any important features missing; at least in the documentation it did not appear that they were missing. These were ADINA, ANSYS and MARC. Since then, I am more led to believe that ANSYS does not have actual large stream capabilities but more medium type of strains in the analytical capabilities.

Since we did this search, there is another code that has come on the scene. That is the one on the bottom, ABAQUS, and that has recently had non-linear material properties and non-linear geometry added to its capabilities, and we think this may be a very good code for the future.

Of these codes, ADINA is used a lot at Sandia. It is also used at Los Alamos. ANSYS is used by Lowell Greiman at ames for his analysis of steel containments. MARC was not being used by anybody that we know of that was trying to analyze steel containments, and we did some experiments using it.

So of the three codes, we selected MARC partly because of the experience we had with it and partly because it also satisfied our objectives.

(Slide.)

Now, our analytical effort is somewhat

paralleling the experimental effort in that we see different cases of analysis that are similar to the three types of models we are going to be conducting experiments on. So that we are starting out with just a clean shell analysis, and this will help us to predict the response of the first two tests.

We are also going to analyze or have analyzed ring-stiffened shells and these will help us with the second two tests. And we are also looking at ways of analyzing the penetrations, and that is perhaps the most difficult part of the analytical effort.

I would like to mention that we did all of our analysis that I am going to present using the MARC code, that we used all of their large displacement finite strain options in the analysis, and these are analysis of the models and you will see pressures put up there for yield or ultimate capacity. These are for our models, and I do not think you can infer from these what the capacity of an actual containment is.

(Slide.)

Okay. As I mentioned, we start off with a clean shell. This is actually the deformed shape at yield, but with a magnification factor of one so that you cannot really see the displacements on the plot. Thi analysis was done using shell elements, high quarter shell elements for all areas except near the base where we switched to using solid-ring elements to try to get a better definition of the three-dimensional stress data at the base. I will talk about that three-dimensional stress data a little bit further later on.

The yield pressure is 64 pounds per square inch gauge and that did occur at the base. That was the first area that would yield, as you might guess.

MR. SIESS: What conditions do you assume at the base? Are you going to get into that?

MR. BLEJWAS: The third viewgraph will have a sketch to show boundary conditions.

Okay, if we amplify the yield condition, we see a deformed shape that agrees fairly well with the type of deformed shape you would expect if you did a linear elastic analysis.

(Slide.)

In fact, if this were the aim of our program, this is nothing special. A lot of people could do this quite easily and conveniently. However, I do want to emphasize that for this linear elastic analysis, the areas of concern are near the base where you see a great deal of bending going on and the areas near the juncture of the cylinder and the shell, the hemispherical dome where also there is a great deal of bending. Now, as we increase pressure, the plastic flow occurs and, as you might expect, you do get a smoothing out of the shape.

(Slide.)

And now, most of the bending or all of the bending that occurs is concentrated right at the base. And if you look at an output of what the stresses are throughout the shell, you find that bending is not significant throughout the entire shell. This was at 179 pounds per square inch. We would expect a membrane ultimate condition to occur within 5 to 10 psi higher than this level. I did not have a plot of that particular condition.

Okay now, the area I want to emphasize or discuss further is in the lower lefthand corner. I have circled the area and I will show you the boundary conditions that we selected. These were selected to be conservative because our model does not end right at its base; it continues down into a ring.

(Slide.)

And rather than -- and also, let me just step up here for a moment. This area right here (indicating) I modeled this as being a straight edge or sharp corner. In our models, it is actually rounded so that some of the stress concentration there will not be as severe as in the actual models.

Now, what we are looking at here are stress contours of mises stress, and this is at the very base. We get what we probably would have guessed in advance, that the area of highest stress is in this lower righthand corner in the inside of the model where we have a combination of both bending and tension due to the upward forces on the model.

MR. SIESS: What stiffness and deformation do you assume for the base? Does the concrete expand?

MR. BLEJWAS: Now we have assumed here rigid conditions. Okay now, figuring that that would be a lower bound --

MR. SIESS: Your model would have rigid conditions?

MR. BLEJWAS: Well, this area in the model, the wall continues down into the model and right here we have a very heavy ring (indicating). Okay, it is not perfectly rigid but by comparison with the model it is rigid. And it is curved here (indicating).

Now, we could have analyzed that condition, although it would have been very difficult, particularly with the non-linear boundary conditions going around the curved corner. And we felt that if we could analyze --

MR. SIESS: I am not really concerned that much with how you are modeling the steel shell, how you are modeling that concrete base that it is attached to.

MR. BLEJWAS: I missed the point of your question.

MR. SIESS: Do you assume it is rigid?

MR. BLEJWAS: Yes.

MR. SIESS: And you are analyzing your model then. It will be attached to a rigid base.

MR. BLEJWAS: That is correct.

MR. SIESS: Now, to apply this to the structure, you would have to make some assumptions differently about that concrete basemat.

MR. BLEJWAS: That is correct. We have not, in our experimental models, tried to replicate the concrete base. But we have tried to make it rigid, and we believe that the concrete base in an actual containment would be close enough to being rigid that our experiments are valid.

MR. ZUDANS: This is a steel model, right?

MR. BLEJWAS: This is steel, that is correct.

MR. ZUDANS: And do I conclude correctly that you use the four elements through the thickness?

MR. BLEJWAS: I use four elements through the thickness with four integration points in each element. They are high order elements.

MR. ZUDANS: Yes, I know the elements. Okay.

The other question is it is good for exercise, but you know, those where you have those support dots indicated, it assumes infinite rigidity of support which is not the reality, and the deformations will go much deeper into your material. And I assume that in a real analysis you will consider what is going beyond that point or not.

MR. BLEJWAS: Okay, the basis for doing this analysis was that the conditions you just described are less severe.

MR. ZUDANS: It does not matter. They are more realistic because you do not have in actual containment any such stiff ring around. You will keep this point you have in the corner where it is.

MR. BLEJWAS: That is correct.

MR. SIESS: If it fails at the base, they will go back and do some more analysis.

MR. BLEJWAS: In other words, I am trying to analyze my model. I can conclude from what I have done that the model is not going to fail at the base; that it is going to fail in the membrane region on the side of the model. So if I do additional analysis, all that is going to provide me is differences in strains and stresses that may be helpful for comparisons but I may not need them if I am trying to predict ultimate capacity.

MR. ZUDANS: Let us say that it may be the case that your highest strains are not at the base. That is what you found from this analysis.

MR. BLEJWAS: Actually what I found is they are about the same as in the membrane region, and since my boundary conditions are so conservative, I expect that what is going to happen is that it is going to fail in the membrance region on the side.

MR. SIESS: Let me suggest something.

MR. BLEJWAS: Yes.

MR. SIESS: To go back to that mode three, which was penetrations induced or affected by the behavior of the containment itself, what you really are going to be interested in there is not necessarily stresses in that shell, but deformations in the shell. It is things that do not affect where the stresses peak. It may or may not have some effect on overall larger deformations.

MR. BLEJWAS: Yes.

MR. SIESS: You keep that in mind, don't you?

MR. BLEJWAS: Yes.

(Slide.)

Okay. From this analysis, we go to the analysis of a ring-stiffened shell, and the first viewgraph shows the deformed shape at yield, but with a very high magnification factor on displacements you will notice that the factor is 106, so that again, you would not be able to see the deformations if it were not magnified. The model here was constructed using all shell elements for economy. We have not included the solid elements at the base partly because we did not see any difference in the ultimate behavior or the non-linear behavior in the gross sense when we switch from all shell elements to shell and solid elements.

This was the additional factor that we were trying to --

MR. ZUDANS: They do have that assumption --

MR. BLEJWAS: Right. That was the reason we went to the solid elements in the previous example.

MR. ZUDANS: Yes, you are right.

MR. BLEJWAS: We performed this analysis at yield and on the next viewgraph you see what it would look like if we did not magnify the deformed shape.

(Slide.)

Again, this looks just like our model. Now, as we increase pressure again, the pattern of variation between the ring stiffeners is not obvious. As you get to much higher stress levels, again, there is a general smoothing of the shell. Something that we at first did not believe or doubted the correctness of was that the rings yield shortly after the walls of the shell yield, and that you get a general plastic flow outward, and there are not a great deal of variations in deformation through the panel.

(Slide.)

MR. SIESS: No vertical stiffeners on your model?

MR. BLEJWAS: That is correct, not in the ring-stiffened model. We do include a few small vertical stiffeners when we get to the penetrations.

MR. ZUDANS: What kind of a strength hardening did you have on that material?

MR. BLEJWAS: Okay. We had an ultimate capacity or ultimate stress of the material of 85,000 and a yield of about 55,000, and the ultimate condition occurred at about 15% strain.

(Slide.)

Here is a viewgraph that shows our approximation. We based this on some data we have from the lab on A 5/16 steel. We are going to, in the future, update our analysis to replicate the material properties of the material that is actually in the models.

We thought that our material was going to be very close to A 5/16; we now see there are differences, and we are going to go back and redo our analysis.

MR. ZUDANS: So for the range you achieved in this analysis, it is essentially -- how far did you go with strain?

MR. BLEJWAS: We went all the way out to 15%. In fact, some of it goes further than that, where the 15% did not occur. In an area that would cause a rupture -- in other words, we would not have a runaway condition -- we continued the analysis further.

MR. ZUDANS: Okay.

(Slide.)

MR. BLEJWAS: Okay. Now what we also did was, since we had modeled our ring-stiffened shell with all shell elements, we went back and took a section of that shell element and assumed that we had evenly-spaced rings on an infinite shell. Then we can take some -- take advantage of some symmetry and if you look at the right side of that, the last line indicates a region that we can then take out, apply appropriate boundary conditions to and analyze in more detail. And so, we have looked at this region with three different types of measures and our results are all very similar and show about the same kind of thing that we saw with all the shell elements.

Primarily, what we were interested in was whether or not the stress concentration that occurs in the plastic range at the juncture of the ring and the shell wall, whether or not that would be significant as we got into the plastic range, and indeed, it is not in any of our analysis.

(Slide.)

Now we know that we have in our model penetrations in the side of the cylinder. In fact, our model has three different penetrations; the fifth and sixth tests have three different penetrations, an equipment hatch and two personnel lock models. Doing an analysis of these precisely would a three-dimensional analysis so we have done what a lot of people in the industry have done when they analyze penetrations. We said well, we think there is some similarity between the way a penetration behaves in a cylinder to the way a penetration would behave in a sphere.

And so, if we analyzed the penetration in the sphere, we can take advantage of axial symmetry and do a two-dimensional analysis. So we have done -- what I am going to show you is some models for two-dimensional analysis.

(Slide.)

We are also doing three-dimensional analysis, but of course, that is slower and more expensive.

MR. SIESS: It seems to me there is a basic error in doing that because in a sphere, a circular penetration remains circular.

MR. BLEJWAS: Yes.

MR. SIESS: And in a cylinder, it will not --

MR. BLEJWAS: That is right.

MR. SIESS: And if I am worrying about seals on an equipment hatch, it seems to me the shape of that hole is going to be a big factor.

MR. BLEJWAS: I agree with you. I think there is --

MR. SIESS: Now, the manway is usually just a cylinder set into that wall. The doors are set into the cylinder, right?

MR. BLEJWAS: That is correct.

MR. SIESS: The equipment hatch, you know, which is an awfully big thing, would have a different shape in a cylinder than in a sphere.

MR. BLEJWAS: That is correct, and that is the reason we are not doing just one or the other; we are doing both.

MR. ZUDANS: One more question. The ring-stiffened shell analysis was done by using the MARK shell elements.

MR. BLEJWAS: That is correct.

MR. ZUDANS: When you did this picture, you came to the conclusion it would be appropriate to -- it is a simplistic, simpleminded analysis, a back of the envelope -- that is good enough.

MR. BLEJWAS: As near as we can tell, that is correct.

MR. ZUDANS: Good confirmation.

MR. BLEJWAS: That is a good point. I wanted to make that one, myself, but we are doing --

MR. ZUDANS: I am sorry I stole it.

MR. BLEJWAS: We are doing a lot of fancy analysis, but we are not losing sight of the fact that we may be able to encourage people to do very simplistic things when we are finished.

MR. SIESS: Inelasticity is a great thing.

MR. BLEJWAS: Okay now. The question was raised before of taking out a part of the side of a containment vessel and putting penetrations in that and just testing just part of the wall, like a 1/6 segment or something like that. Unfortunately, I did not bring it with me, but we have done a quite a bit of analysis where we have taken out a part of the wall and done an analysis on that, choosing appropriate boundary conditions.

At least I thought they were appropriate. And what we found is that you needed to take a real lot of the shell in order to smear out the deformation effects. What we think we got when we did that was that the distortions were concentrated in the penetration because, as the shell wanted to grow outward, the penetration was the hard point. And so it forced that penetration to distort more than we think it really would have.

So what we have now done in our analysis is something like you see here. There is an axisymmetry there. If you look in the upper righthand corner, that is the penetration we are really interested in. The rest of it is a shell with just a few shell elements, like on the order of 7 or 8 shell elements, all the way around until we get close to the penetration. Then we use smaller shell elements, and in some of our models we even use solid ring elements, but we have found that that is not necessary.

At least from preliminary results of this analysis, we believe that our hypothesis was correct that these results are significantly different than when we just chose a segment, and that is part of the reason that I hesitate, when somebody suggests just taking a segment out of the shell and testing penetrations with just that segment. You have to be very careful when you get into the ultimate range, because you are talking about the material yielding and growing greatly. And if you somehow inhibit this growing, you are going to change the actual characteristics of what is going on.

So, I think that is a worthwhile result from what we did with our analysis.

MR. SHEWMON: Are the welds in these butt welds or did they lay something over the joint when they get in field erection?

MR. BLEJWAS: All these are actually being built in. It is more like a shop condition, not actually in the field.

MR. SHEWMON: So the real structures are straight butt welds, is that right?

MR. VON RIESEMANN: That is correct.

MR. EBERSOLE: The penetrations you get, are they fixed with respect to space in a differential fashion? Do they move outward?

MR. BLEJWAS: They move outward.

MR. EBERSOLE: What about the big 28-inch pipe penetrations? They do not move as freely, do they?

MR. BLEJWAS: That is correct. That is something that in these early models, we are not including.

MR. EBERSOLE: Oh?

MR. BLEJWAS: And that may be a very good topic for a tangent to this program or a separate program, because I think that is a very important area.

(Slide.)

In addition to -- the previous slide showed a personnel lock. This is a three-dimensional look at what our model looks like for an equipment hatch, and I just wanted to show you this to just give you an idea of the geometry of the equipment hatch.

We have a concave dish that closes off the penetration, and we will show you another figure that shows it from a side view, or simplistically, --

MR. ZUDANS: Why didn't you do this in a cylinder with those elements?

MR. BLEJWAS: I was afraid that, -- again, we did not do that analysis -- that was just a pictorial rendition of what we were doing. We realized it would be hard to visualize this, so we tried to come up with a way of visualizing it.

MR. SIESS: Did you model the connection between the hatch cover and the hatch boundary, or did you just assume it was rigid, a fixed connection?

MR. BLEJWAS: We did it the way it is in our model, which is a welded joint, so we assumed a fixed connection.

MR. SIESS: The actual structure, it is bolted?

MR. BLEJWAS: Yes.

MR. ZUDANS: I think that this would be the most significant piece of information if you could provide for that sealed surface and allow it differential deformation, both in the analysis and the test, because this would answer the question whether or not this will fail to seal the containment before you reach the ultimate capacity. Whatever you want to call the ultimate capacity.

MR. SIESS: In your 1/10 steel model, will you model that hatch any differently than you do in your 32nd?

MR. BLEJWAS: Yes. I was just going to say that we have not done that analysis. We are a long way off from doing the tenth scale model.

MR. ZUDANS: They could do it in here, too. I feel that in here, it is the same thing. In your 1/32 model you should not weld this thing; you should put it on so it can lift off if deformation of the boundary takes place.

MR. BLEJWAS: I think we are getting into an area that I cannot answer. The fabrication difficulties were the reason we decided not to do that. This is a fairly small and thin piece of struture that we are trying to bolt on.

MR. HORSCHEL It is --

MR. BLEJWAS: It is only 23 mils thick, so we are actually looking at this particular structure in great detail because we are concerned that the cover will buckle. And if you look at classical solutions for buckling pressure, it is in the range where it could conceivably buckle.

MR. ZUDANS: Your conclusions on buckling will not be transportable to real structures because the boundary conditions are completely different.

MR. BLEJWAS: That is correct, and what we are trying to do is decide whether or not this cover needs to be redesigned so that our model does not fail prematurely in that mode.

MR. ZUDANS: Yours will not fail, whereas the real one is free to slide upward.

MR. BLEJWAS: That is right.

MR. ZUDANS: Therefore, it is not as -- it is simply supported. Yours is built in the structure.

MR. BLEJWAS: According to the work I looked at, some experiments reported in the Japanese handbook of structural stability, it turns out thick in this region for the particular parameters we have chosen here. The fixed-in conditions are very, very similar.

In fact, they show the curves crossing, so that the simply-supported has a higher buckling pressure than the fixed. I cannot explain that, but that is what their handbook shows.

I agree with you that we should try to represent the boundary conditions as accurately as possible. However, what I have been trying to do to this point is to do an analysis of our model. The 1/10 scale model will include the features you are discussing.

MR. ZUDANS: Oh, it will.

MR. BLEJWAS: There will be a bolted connection there and we will try to model that to make predictions of whether or not it will lift off and potentially leak when it does lift off.

MR. ZUDANS: My feeling is that is what it will be, hopefully.

MR. BLEJWAS: There is a strong possibility.

(Slide.)

In summary, let me put up what our best guess at this time is what will happen with our models. I would like to emphasize that we will actually try to document our predictions before we do any tests to leave ourselves open for a lot of criticism afterward. But in the clean shell where we get the kind of failure that we expect to occur near mid-height, we will get a meridianal tear. That is our expectation.

We do expect that to occur at a pressure that may be significantly higher than what you would calculate for first yield. We are talking about first yield occurring in our analytical calculations at a pressure of like 65 psi. Ultimate conditions somewhere around 180 or 185 membrane yield occur somewhere around 120 pounds per square inch. But still, the ultimate condition I think is significantly higher than the yield condition.

MR. ZUDANS: But your material curve tells you -- the curve that you use for materials has exactly --

MR. BLEJWAS: That is right, there are some other things that enter in -- the geometric non-linearities help you so you get a higher pressure level than you would predict from the back of the envelope, because the structure changes from being a cylinder to being more like a sphere, and so hence, you can take pressure in two directions rather than primarily supporting it in the circumferential direction, so you do get an extra 15 to 20 pounds per square inch that way.

MR. SIESS: When you move to the actual structures, somebody is going to have to worry about when it touches that shield building.

MR. BLEJWAS: That is correct.

MR. SIESS: It is only six feet out there. Most of them.

MR. BLEJWAS: That is right.

MR. EBERSOLE: Will you expect it to tear to a very large extent in being catastrophic or just relieve itself?

MR. BLEJWAS: I expect it to be catastrophic. There is no mechanism, using pneumatic testing, for the pressure to be relieved very quickly, so we expect when it ruptures we will get many pieces -- maybe not many pieces, but you will see a big gaping hole. You will get a big gaping hole out of it. We do not expect just a simple tear.

We will have some heat-treated areas or heated areas near the welds that probably will precipitate the failure to begin there.

MR. SCHAUER: Aren't there a lot of pipe runs that are anchored off at the shield building?

MR. BLEJWAS: Yes.

MR. SCHAUER: Then they will form very hard hot spots which will prevent this particular vessel from growing more than a few inches.

MR. BLEJWAS: I do not think you can say a few inches. I expect it can grow quite a bit more than that.

MR. SCHAUER: I am looking at a containment and I am looking at the shield building in back of it. I am looking at a pipe that goes through the shield building, through the containment, and is anchored off at the shield building. And the difference there is maybe an order of -- between the shield building and the containment door -- of a couple of feet.

MR. VON RIESEMANN: Five feet.

MR. SIESS: Five to seven feet.

MR. SCHAUER: Five to seven feet. Now I have this hard spot that is going to be created as this containment expands.

MR. SIESS: You have some point loads on the outside.

MR. BLEJWAS: That is correct.

MR. SIESS: Lots of point loads.

MR. BLEJWAS: Lots of point loads, but I also think -- thinking of that as a totally hard spot I do not think is accurate. The piping I have .te.. goes out and typically bends, and then goes out through the shield wall.

MR. SCHAUER: I see.

MR. BLEJWAS: So you can conceivably get bending in the piping. I think that is an interactive situation that is beyond what we have looked at at this point.

MR. SIESS: There are some straight runs through the shield wall because I know they have collection pipes around them at some points.

MR. SCHAUER: Before you get too excited about all this strain being actually available to you, you have to consider these point loads.

MR. ZUDANS: That is correct.

MR. BLEJWAS: Yes, I agree. But also, that strain is what is going to help you with these hard points. The fact that the material will be able to strain and go plastic and distort significantly before you get a rupture will help you in analyzing these hard points.

MR. ZUDANS: Of course, you have to keep in mind right now you are analyzing your model, and you are testing your analysis against the model. The thing you need to keep in mind is that when you start to analyze real structures, there may be features that have to be built into the model that have not been tested in your model, and I think that is what you have to be careful about.

MR. BLEJWAS: Yes.

MR. SIESS: That you are testing the analysis under certain conditions. There will be others for which it is not tested. Now, you may have that confidence.

MR. EBERSOLE: One of the features is simply the secondary containment, is it not? If this catastrophically blows, won't the secondary containment tend to at least partially confine it in the beginning?

MR. BLEJWAS: If it actually blows with the kind of rupture we expect from the models, I doubt it will do anything except spread debris around the countryside. It is designed for very low pressure levels, from what I have heard. I mean, there is no --

MR. EBERSOLE: No pressure capability in secondary containment.

MR. BLEJWAS: Not a significant one.

MR. SIESS: It has three psi at least, but what it is designed for and what it is good for are not necessarily the same thing. I do not think anybody looked at what they are good for. They are not good for 160 psi or what it would be reduced to with the additional volume.

MR. EBERSOLE: They are missile sources to the nearby unit.

MR. BLEJWAS: Perhaps.

MR. ZUDANS: One more question. If you perceive the way you describe which is very interesting and good, what you will come up is "ultimate load capability of containment", probably much higher than those that create non-isolation of the containment during this loading process. What the net result will be is that you do not have to worry about containment ultimate capability; you have to worry about hard spots. Those will be the ones that will fail first and open the path to the outside.

MR. BLEJWAS: But it might give us more confidence in trying to analyze the hard points.

MR. ZUDANS: I do not know how that confidence comes about. I do not see that. Where do you get that confidence? You may have to design supports for those penetrations different, knowing that they will be the first ones to fail because they cannot accommodate six or even deformations. The shied shell may well deform in a clean section.

MR. BLEJWAS: Yes, that is true.

MR. ZUDANS: So what you really will find out is that what you do is good and whatnot, but not necessarily really in the ultimate end. It gives you some good reasons how to design better elsewhere.

MR. BLEJWAS: Yes. It may be that -- maybe to paraphrase what you are saying, that the ultimate condition we will be looking at is of academic important primarily.

MR. ZUDANS: Right.

MR. BLEJWAS: Okay, let me just quickly finish up with these viewgraphs. The ring-stiffened shells we expect a failure that is similar with the clean shell except that we expect it at a slightly higher pressure, and we do expect the rings to go plastic as we have predicted and other people have predicted in analyses.

For the penetrations, we are still in the process of doing analyses, and I would not want to make a prediction there until we have looked at some 3D analyses, and we do not have those completed yet.

MR. ZUDANS: I guess the rings go plastic because the cylindrical pieces the way they are arranged become double curvature shells, and then all of a sudden they create higher load carrying capability. And the full rings do not have that kind of a feature; they end up going the same way.

MR. BLEJWAS: Yes.

MR. ZUDANS: I think it is a good conclusion.

MR. BLEJWAS: Yes.

(Slide.)

Finally, as I mentioned, we do plan on continuing with the analysis, doing a 3D analysis of the penetrations in the steel models. We are going to update our material properties as soon as we have good information on the materials in the models. That has been slow in coming, and we are going to eventually, after our tests are finished and we have some idea of how good our analyses were, try to use other codes to do the same kind of analysis and see if there are difficulties with those codes.

Also, we are at the beginning doing analysis of concrete containments, as I mentioned, and sometime in the next fiscal year we plan on doing dynamic analyses.

MR. ZUDANS: Are you going to do axisymmetric dynamic analysis?

MR. BLEJWAS: Initially, that is all we are going to do. What we would like to do is just do a simple scoping study to see what type of pressures we need in an axisymmetric model before we have serious problems. This is just sort of back of the envelope type analyses to give us a ballpark figure for what kind of pressure we need to be looking at and what type of post-wit impulses we need before we have serious problems.

MR. ZUDANS: What kind of pressurized time?

MR. SIESS: I said dynamics is out of the scope of this meeting.

MR. ZUDANS: Oh.

MR. SIESS: Two years from now we will talk about that.

MR. ETHERINGTON: I would like to know whether the containment heads are usually torospherical or semi-eliptical.

MR. BLEJWAS: Well, Mr. Dennis has surveyed that.

MR. ETHERINGTON: And the pros and cons; that is.

MR. DENNIS: On steel containments, to date no torospherical heads have been built at the top. Prairie Island, some of those have a torospherical head at the bottom. The heads are all hemispherical at the top of the steel containments. GE proposes on a number of their MARK III containments using steel to use torospherical heads. However, the first plant that is coming along in that family is Allens Creek and they have recently switched from a torospherical head to a hemispherical head. Pre-stressed concrete, the shallow dome, is quite common.

MR. ETHERINGTON: Is torospherical better or just easier to make?

MR. DENNIS: It is easier to make.

MR. ETHERINGTON: To design -- from the

designer's point of view there is no problem, but if you get beyond the yield point, don't you have more of a problem with the torospherical? You have a sharper radius.

MR. DENNIS: The primary problem I think that occurs with torospherical is, as you get up in pressure, it has a tendency to buckle.

MR. ETHERINGTON: Yes, that is the point, that is the point.

MR. DENNIS: It offers some advantages in interior layout of the containment building.

MR. ETHERINGTON: If you design it for the kind of conditions we are talking about for ultimate capacity, would you be inclined to use the semi-eliptical instead?

MR. DENNIS: No, I would use a hemispherical head.

MR. ETHERINGTON: You would keep it hemispherical, yes, that is best of all, of course. All right.

MR. ZUDANS: May I remark? For ultimate capacity, it makes no difference; it becomes spherical anyway.

MR. ETHERINGTON: Not quite; it will try to.

MR. ZUDANS: I think they are both MARK III's;

one in Spain, one in Taiwan, are torospherical, right?

MR. DENNIS: I am not familiar with that.

MR. SIESS: They are all hemispherical now.

MR. DENNIS: They are all hemispherical now.

MR. SIESS: On top.

MR. ETHERINGTON: Is it your intention at any time to make design recommendation?

MR. VON RIESEMANN: I think the NRC could answer that question. Are we going to make any design recommendations? Is that our intent?

MR. COSTELLO: The research program directly, sir? No. I do not -- it is hard even to conceive of design recommendations issuing directly from a research program.

MR. SIESS: I think if designers of new plants are told that they have to be able to have the capability to withstand 150 psi or something like that, they will make their own decisions.

MR. ETHERINGTON: The code will probably pick it up, too.

(Slide.)

MR. WOOFIN: I am Ron Woodfin, also from Sandia. My discussion today is going to be relative to the experimental program for the small-scale steel models. The experimental program for the large-scale steel model and the reinforced concrete models has not yet been fully developed to the point that it is really something we can discuss yet.

As we go through it, what I would like to do is take a fairly broad brush approach because I did not anticipate that any of you gentlemen were interested in the extreme details of instrumentation, but I would be glad to answer any questions of that nature if you want to ask them.

MR. SIESS: We are not supposed to be, but it is hard to keep us off the subject.

[Laughter.]

MR. WOODFIN: One thing I would like to remind you of is that the instrumentation and the procedures for this set of experiments is directed toward our objective, which is to validate the existing analytical techniques, and so we have directed our procedures and our measurements toward that end, rather than, say, documenting the dynamics of the failure mode as it occurs or something of that nature.

(Slide.)

We have, as a reminder, the three basic experiments, each of which has two tests associated with it. This was what was discussed earlier. Now I will mention later in our loading procedures the possibility of a slight variation from that position, but I will refer to tests one and two, three and four, five and six, and this is where they come from.

(Slide.)

The technique which we use will be, because of safety considerations, remote and outdoors and this afternoon we will take you to the place where we are going to do that and I will show you on a map a little later where that is going to be so you will have some idea of where you are going and what is involved. I will also give some diagrams of the site and whatnot.

We have opted to use a computer-controlled technique for pressure control and for data acquisition. There are several reasons that will come out during the presentation to show you why that is the case. It will be interactive in the sense that the load steps will depend upon measured data, so that the engineer in charge of conducting the experiment can vary the load steps to get them finer in the regions where more interesting events are occurring, such as around yield, and we reach a plateau where we think we are getting close to runaway. Then we can reduce the size of the load steps and change the procedures in that manner.

(Slide.)

The loading will be pneumatic, using bottled dry nitrogen. That saves us several problems by using the bottled gas and the dry gas so that when we do look at the state of the gas contained within the volume, we do not have to concern ourselves with vapor pressures and such things as that.

We also do not involve the equipment problems associated with moisture and oil that come from compressors and those sorts of things. The pneumatic, of course, gives us some more realistic failure conditions, but forces us into certain procedures required to maintain safety. Consequently, it is going to cost a certain amount of money in addition to do it that way.

We have chosen to use a quasistatic loading technique which involves a series of step-wise increases, so we would increase the pressure during a relatively short period of time, on the order of 15 to 20 seconds, let the deformation stabilize for the period of time required for gathering the data, and then proceed onto another increase. In general, we will proceed in a monotonic manner so that once we go past yield, we would not back off again.

However, this is one of the possible variations for having the second model, a second test available in each experiment. If we have satisfactory replication of the first test, during the second one up to a point well in excess of yield and into the strain hardening region, we can then decide to do a non-monotonic loading for the last part of that -- for the last part of that loading procedure. That is, we can reduce the loading, the pressure, by a certain amount and then reload to see what effect might be introduced and whether the computations can, in fact, follow that type of loading path.

But, of course, that requires that we have convinced ourselves that we have been able to replicate the previous results, at least up to that point.

MR. ZUDANS: I do not quite understand your reason for not cycling on all of the models.

MR. WOODFIN: Well, our original decision for not cycling was that we would take the easiest analytical problem first. Now, if we are convinced that we can do a good job of that, we may then introduce a loading program which is cyclic on all of them. However, the scenarios that we looked at did not seem to have much cycling in them, and it appeared we would not be too far off on this by going to monotonic loading in the first place.

However, that is a complication we will

introduce if we find that we are capable of doing a good job with the cyclic condition.

MR. SHEWMON: Zenons, what would you get beside fatigue effects?

MR. ZUDANS: Not fatigue effects. I am looking for, when you unload, it should go linearly back to a certain point. Where you reload again, if things behave properly and you have good material stabilization you would get back to the same point linearly before you stopped. But if some other change takes place, you may not do that. This is what the challenge to analytical tools is. Not in proportional loading. In proportional loading, you can do the simplest of models.

MR. SIESS: Are you talking about cycling in the elastic range?

MR. ZUDANS: That is right, only in the elastic range. They could not do it the other way around because it is only unloading they do. You load it in the inelastic range; it unloads elastically; you reload again back to the same and you should get to the same point.

MR. WOODFIN: Okay.

MR. ZUDANS: Or else you should get some kind of -- this is where the analytical difficulties come.

MR. WOODFIN: That is correct, and we decided first of all not to put the harder problem to the code until we convinced ourselves that it would handle the easier one.

MR. SHEWMON: I realize people climb mountains because they are there, but I do not quite see the relevance of this to the problem.

MR. VON RIESEMANN: Realistic loading conditions -- the loading is increasing; it does not cycleup and down, even though it is tougher analytically to handle it. We are not modeling any scenario. We expect the pressure to go up and maybe cycle a small amount but not go down to zero and then go back up again.

MR. ZUDANS: I did not say to zero.

MR. VON RIESEMANN: Well, even to unload half of it, say.

MR. SHEWMON: All right.

MR. WOODFIN: And we will continue loading until failure is achieved.

Now, there are two possibilities for failure. One is that we cannot put enough gas into the model to keep the pressure increasing because of some leakage. The other possibility is that it fails in a way in which we can actually see that there is no point in trying. So the second would be what you would call catastrophic. That is if it spits wide open or the top comes off or it flies into a bunch of little pieces, that is clearly failure. It is not quite as clear as what you would call failure when you are simply unable to make up the gas. But we will ensure that we are able at least to present a sufficient amount of gas to have 100% per day leak rate.

MR. SIESS: You are not sure you can define failure, but you will know it when you see it?

MR. WOODFIN: I think that is probably a good way to say it.

(Laughter.)

MR. ETHERINGTON: Is the use of bottled nitrogen going to give you enough of a temperature difference to be a nuisance?

MR. WOODFIN: We are going to do these tests at constant temperature. We will heat it as it goes in and we will have a heater in the model so that what we will do is take the Weather Service predicted high for the day on which we do the test, add about 10 or 15 degrees to that, depending on time of year and wind we expect, and set the temperature at the beginning to that level and keep it that way throughout the experiment.

MR. ETHERINGTON: It will be at about atmospheric temperature, then.

MR. WOODFIN: A little bit warmer than the day in question, at atmosphere temperature.

MR. ETHERINGTON: You will have to heat it, will you?

MR. WOODFIN: Yes. Just to avoid the possibility of having it cool something. It is much more complicated to cool something than heat it.

MR. PICKEL: If your guess is wrong and you do get a nice, long tear where you may leak as fast as you are feeding in rather than a catstrophic type failure, will you monitor your --

MR. WOODFIN: I will get to that in some detail in just a minute.

(Slide.)

We have a data system designed primarily to take structural data strains and displacement and I will go into those in some measure of detail in a moment. Also, we will take auxiliary data just toward what you are asking for pressure and temperature, to be able to establish the state of the gas within the containment at a given time.

Beyond that, I will show you how we go about getting an inference of leak rate. Now, to do this we will use two instrumentation trailers; one for signal conditioning and one for command and control functions. The signal conditioning trailer will not be manned and the command trailer will be, and they will be separated by a distance of about a half a kilometer.

(Slide.)

The structural data strains will be the primary information in the elastic and low plastic regime, compared to the analytical results, but we will measure strains up to 20 percent. Now, in order to be able to do that, you will recognize that that is a rather formidable task to reliably measure strains at that sort of level, so we have done some preliminary work to qualify both our application techniques and adhesives and coatings to be used in these gauges. We will show you some of those results as you take the tour this afternoon.

We have done some tensile specimens using realistic gauges and coatings. We have not done the coatings yet. We are in the process of doing that now, but we will show you results for that.

MR. SHEWMON: Are you at all interested in where it yields first, or just where it yields last? That is where it breaks.

MR. WOODFIN: It is always a problem to try to put a transducer where it yields first because almost invariably, that will be in a very minute area, and in this case that is no exception. Since he is expecting to get yields around the base first, what I plan to do is to put a gradient strip of ten gauges at a point on the base, so that we can have some hope of catching at least within the gauge spacing in that gradient strip, that first yield. So we will look at that.

We will also look in detail at the belt line and the spring line.

MR. SHEWMON: Spring line?

MR. WOODFIN: Spring line is our term for the attachment between the cylinder and the dome. We got that from UC&I, didn't we?

MR. ZUDANS: Same thickness for dome and --

MR. WOODFIN: That is correct.

MR. ZUDANS: That is not represented in the real system, that is not real. They do not use the same thickness in the dome.

MR. WOODFIN: I will let Al speak to that. That is a pretty complicated question. Unless you just want me to generalize on it. But what it amounts to -- it is pretty hard to make one at this kind of scale with that variation in thickness.

MR. ZUDANS: You do it because of matching thicknesses. If you reduce the thickness you do not have that distance because the half-thickness shell deforms the cylinder at that point.

MR. DENNIS: There are a couple of things to consider at this point. One of them is that most of the shells I have looked at, they do not go to half-thickness on the dome.

MR. ZUDANS: That is right, they do not.

MR. DENNIS: They typically just drop maybe 80% of the thickness of the shell in that area. The other thing is, quite often there is a good bit of construction around the inside of the upper end of the dome such as a crane girder or something l'ke that, that would lend additional stiffness to the shell.

We are not including that in our models, and that came after -- the decision came after several conversations with our advisory committee on the benefit of including or not including that. If you look at the ability to replicate test data and get benchmarks by computer methods or analytical methods and get benchmark data, I think having the same thickness of the dome and the cylinder is a reasonable thing.

If you look at it from a fabrication standpoint, we are working with a point of 047 steel, so you have a 47 mil thick wall. And trying to do it to a 30 mil wall there gets into problems of construction.

MR. WOODFIN: The displacements will be our primary indicator of structural performance in the large plastic regime, if you like -- something as the strains get larger and as the deformations get larger. So we are going to use three different techniques to assess displacements.

The CDS technique -- I will explain what these revisions mean on the next slide. It is an optical device. The LVDT technique is a direct measurement, and the photographic technique, of course, is an optical device.

(Slide.)

The first thing I will do is talk about strain gauges.

MR. SIESS: Why don't you skip that?

MR. WOODFIN: No one is interested in these things?

MR. SIESS: Too many might be.

(Laughter.)

MR. WOODFIN: All right.

(Slide.)

The CDS stands for coordinate determination system. This is basically a surveying type system which is computer controlled with high resolution used, in many cases, for setting tooling, say, for fabrication of ships and airplanes and such things as that. Two operators focus the light on the same spot and the system calculates the coordinates of that point.

Now, we have taken that system, modified it slightly or are working to modify it slightly, so we can use a three-light system so we can look all the way around the model and then we will take various points. But this is manually-gathered data, so we will not do that at every pressure loading step. We will do it only, say, every third step or something of that nature because it takes a while. And I will explain that again in a moment.

The LVDT is a standard displacement-measuring device. These will be spring-loaded ones mounted on the inside and, of course, the photographic involves basically a steel camera system.

MR. SIESS: What does an LVDT cost you?

MR. WOODFIN: Around $450.

MR. SIESS: How many do you expect to use in the tests?

MR. WOODFIN: I -- probably one. Maybe none.

MR. SIESS: I notice you had quite a small number. I was wondering whether you would find a --

MR. WOODFIN: The reason for the small number, we originally planned to have something on the order of 60 or 70 of them, but when we began to look at the cost-benefits of using this new CDS system instead of

LVDT's, it turns out even though the capital expense of the CDS system was considerable, the time involved in mounting and aligning the LVDT's for each of the experiments when you go through the whole series of 13 that we have planned, was on the order of twice as much as using the coordinate determination system.

MR. SIESS: So you will depend on the CDS for most of your deformation data?

MR. WOODFIN: There is a transition period. If you look on the next slide here, I think you will see what that transition is.

(Slide.)

The CDS resolution you see is somewhat limited, so it will not be of great value to us at the very low strains. Say less than one shell thickness. However, the LVDT resolution is infinite. Now, on the other end of the scale when we get to the large concrete and large steel model, the biggest LVDT's you can buy have a six-inch throw, and that is not enough.

MR. SIESS: Six-inch what?

MR. WOODFIN: Six-inch maximum displacement. Throw, I said. So that is not enough.

So what we are doing, we would have to get some sort of metric device or something like that. The LVDT's which we are using to measure six inches are about almost three feet long by the time you put the spring-loading capability in them and that sort of thing. And that is required because you either have to have them on a pivot and use two to measure the displacement of one point, or you measure the motion of the shell itself, rather than the material, the motion of the material point.

MR. SIESS: These displacements you are measuring are chiefly going to be radial displacements?

MR. WOODFIN: In the case of the LVDT's they will be radial displacements because the LVDT will be mounted on a radius and will be in contact with the shell at that radius. So that is why I say it is the motion of the shell.

The CDS, however, actually tracks the motion of a point. You mark a point on the body and track the motion of that, so that will be wherever that point wants to go.

MR. SIESS: You could use it actually to integrate strains.

MR. WOODFIN: Yes. As a matter of fact, I intend to integrate all these, not for strains but for calculation of volume because if you go to the inference of leak rate, it is necessary to know that the volume does not change during the period of measurement.

MR. SIESS: What kind of deformation measurements would you be making around the penetration areas?

MR. WOODFIN: Okay. When we get to those, we will arrange our photographic setup so that we get a silhouette of it so that we will have what would be a cut through the plane that includes the deformation. We will also measure both tangential and radial deformations with LVDT's. Those deformations perhaps in the tangential direction may not be very large, so we can use a different type of LVDT which does not have to be three feet long to do that.

And then we will also put some of the CDS targets around that area where we cannot get to it with anything else. So I have adopted a philosophy both of redundancy in kind as well as the peculiar features of each of these three types of displacement measurements.

To try to optimize the amount of data that I am able to get from each region I have considered originally using a brittle lacquer technique or photo-elastic coating, but we cannot get up close to these things to examine them.

MR. ETHERINGTON: But you could pick up the regions where it yielded after --

MR. WOODFIN: It will all be yielded. A

brittle lacquer is so sensitive that there would not be a millimeter, a square millimeter, that was not cracked. It would not tell you anything apparently when you got through, and the difficulty of trying to remotely examine a britte lacquer, say, through a television system or photographic system, is very, very formidable because the angle by which you view it and the angle of which you eliminated is critical to being able to see those cracks.

So, --

MR. ZUDANS: Two other things occur in my mind. Of interest would be to know how much the circumference grows as a total -- I could imagine -- now I am designing, so please bear with me. I run a rope around it, a thin line, coil it; as it grows, it will just coil off and you can measure that very accurately.

The other thing is you could draw lines on it.

MR. WOODFIN: Yes, we will have some of those things used in the photographic -- which will be visible in the photographs. The photographic thing, unfortunately, is delayed. You do not get that right away. And we looked into the possibility of using some sort of television type data, but the resolution is not satisfactory to do you any good. So you are caught between waiting for data of sufficient resolution to be useful or having it immediately and only having a general picture of what is going on.

So we have decided to go for the high resolution data and wait on it, because we have two types of data which will be available to us during the experiment. Okay.

(Slide.)

This shows a little bit about placement of data -- placement of transducers in order to gain the data. We will use 70 to 150-strain gauges, depending on the complexity of the model. We will start off on meridians and parallels. We will then check our symmetry in each case, so we -- and then we will look at details such as I described earler using the gradient strips, and in each case these will be rosettes, so that we can check to determine whether the principal strains are in the directions that we expect them to be and that sort of thing.

The LVDT's will be principally along the meridians and details with only one or two horizontal planes verified for symmetry. The CDS targets, we can use more of them. We will get both meridians and details and we will have at least three meridians measured, in which case we will automatically have a symmetry check. And then we will measure only one merdian with the photographic system, but that will be a continuous silhouette and not something that we have to infer from a series of point measurements, as the others are.

(Slide.)

Actually, I guess I should ask do you have any further questions about those sorts of measurements. I am now talking about auxiliary data.

We will have three independent pressure transducers, actually independent in the sense that they are of different manufacture and a different style. Two of them at least will be strain-gauge transducers. The other one is coming with the pressure control system and that is out for bid and I am not sure exactly what it will be until that comes back in.

They will act as confirmation, both for confirmation of the control system and necessary data for inference of leaks. I will explain in a minute why I hesitate to use the term "measurement of leaks." We will have temperature measurements with thermocouples at each strain gauge so that we can make the appropriate compensation to strain gauges. Then we will also use some strain gauges -- or I beg your pardon -- some thermocouples to measure the temperature of the air or of the gas inside. We will also use resistance temperature detectors.

Now, the primary reason I have used the resistant temperature detectors is because there seems to be considerable interest in using them for the temperature-measuring devices in the normal ANSI standard which is being proposed for measurement of leak rate. And so that if you do an integrated leak rate test on containment, your typical test nowadays is using resistance temperature detectors as post-thermocouples for the temperature in that test, and so I am using both.

The correction of strain gauges is a primary purpose for the thermocouples attached to the shell. We will confirm that the temperature control system is operating correctly and then we will have data for leak inference.

(Slide.)

MR. ETHERINGTON: A leak would hav to be rather large before you could infer that there was a leak, wouldn't it?

MR. WOODFIN: Yes. I will get into that in detail in just a minute.

Now I say I put "measurements" in quotation marks because you can really only -- unless you know where the leak is and you can get it to run through a flow meter, and I do not expect our leaks to be that cooperative -- we can only infer them from measurements of state variables.

MR. SIESS: Put a little Zenon in there.

MR. WOODFIN: On the next slide you will see that we have several techniques for trying to determine where they are. There are no -- in the small-scale steel models, as we already discussed, there are no modeled leak paths. However, it is possible that we might find some leak path in the process of proceeding to failure. If we find a crack that does not run in the way that we expect it to, then we would, in fact, have a leak occurring and we will attempt to detect those sort of things, and I will go through how we are doing it.

But primarily in the small models, this is a way of developing techniques so that when we get to the larger models where we are modeling leak paths in the concrete and the large-scale steel models, we will have confidence in our ability to handle this sort of measurement and computation, and it is rather involved.

So the first thing we are trying to do is learn to detect and locate the leaks.

(Slide.)

I have five techniques. If you have another suggestion perhaps I can throw that in there. Also for trying to detect and localize the leaks, we will use an ultrasonic detector which is a device that is marketed for finding leaks in pipes and pressurized conduit and such things as that, which operates in the 40 kilohertz region. That is down to the normal audible range. We will use acoustic emissions transducers placed on the model and on the fixturing to try to -- and we have been assured by the gentleman who is an expert in acoustic emissions that he can tell whether the leak is in the fixture or the model that way, so are going to pursue that. Yes?

MR. SHEWMON: I take it you will have several acoustic emission transducers fixed around the unit. Where will your ultrasonic transducer be, or what is this?

MR. WOODFIN: Okay. That basically is a little microphone, a sniffer is the common term that is used for them. It is the sort of thing that your telephone repairman uses, your lineman uses, when he goes out and looks for where is this pressurized line leaking. And he points it up at the high line and --

MR. SHEWMON: You would expect somebody to be walking around?

MR. WOODFIN: I will show you in a minute. There will be a fellow out there who has access to that. We will really just put that on one side. Now, if it looks like that is going to be the technique that is going to work, then I will get a couple more.

But I would say the first cut at it with these first couple of models I am going to see which one of these will work. Maybe none of them, but I am trying everything I can think of. So we will also look at the pressure system duty cycle, the inflow -- we will have an inflow indicator that will tell us if gas is coming in when it is not commanded to do so. And we will look at our gas consumption as opposed to the predicted gas consumption.

MR. SHEWMON: What in the system does these cycle means?

MR. WOODFIN: This will be an on/off control system. I will show you a schematic in the a moment. What I am looking at is if I am putting pressure into the model when I should be stabilized, this will give me an indication that it is getting -- that there is gas getting out somewhere.

MR. ZUDANS: It may also not indicate that. You may reach some kind of an equilibrium condition where the vessel grows and you need more fluid to maintain the same pressure.

MR. WOODFIN: That is correct, and I will be checking that because I will have a dynamic output.

That is one of the reasons for having a realtime display of the information. Each time I go to a pressure step I will generate a time history which shows, as a function of time, the pressure against the commanded pressure, and selected strains and displacement.

And if those things keep growing after the pressure has stabilized, then I know that I am on the threshold of a runaway condition, in which case I have a contingency in the computer program. I hit one switch and it goes into an automatic mode for collecting as much data as possible in a short-cycle time.

MR. ZUDANS: You may get to a situation whereby you put in pressure, you stop your gas inflow and you observe the instruments and the history, and the pressure may start falling.

MR. WOODFIN: No. No, the pressure -- if the pressure starts falling, that is a malfunction in my pressure control.

MR. ZUDANS: Or it is a leak, a small leak.

MR. WOODFIN: No, it would have to be a large leak because my pressure control will sense that and not let the pressure fall.

MR. ZUDANS: That is relative. It is sensitive to --

MR. WOODFIN: It is a tenth of a psi.

MR. SIESS: If you are at, say, 125 psi in your tests and you have an indication of a leak, have you considered stopping the test, taking it down to zero or five psi and going out and looking at it?

MR. WOODFIN: Yes. My first question is where is the leak, and if I can convince myself that the leak is, in fact, in the model, then we will have to make the decision as to whether that is more beneficial or whether we should continue monotonic loading. That is a dynamic decision that has to be made during the test.

If, however, I can detect a leak and convince myself it is in fixturing which has many more leak paths available to it than the model, then I will continue on. So that is the reason I have the detection and localization problem.

MR. ZUDANS: Supposing you reach the failure mode you describe as a leak and it does not grow, as you mentioned before. You have several choices, either to finish your test -- that is the end of it -- or you depressurize it, reweld it and continue until you reach the ultimate load.

MR. WOODFIN: There is another possibility which I expect we would take. That is, we stop and measure that leak by inference -- that is, what we will do is we will put the pressure control system into a locked-valve condition for a period of eight to 12 hours or perhaps longer, because that sort of period is required in order to make that kind of measurement. I have this data later in the viewgraphs here. And then we would make the state variable measurements necessary to infer the leak during that period of time.

Now, let me go on to the next one and I will show you what we have been asked to provide.

(Slide.)

We have been asked to provide a curve that looks like this thing here (indicating). Now, NRC does not have enough money to pay for that, is what it amounts to, because this would take weeks to generate this sort of thing for any one experiment, because each datapoint requires a minimun of six to eight hours measurement in order to find the sorts of leaks necessary and bring them up out of the noise.

MR. PICKEL: What is the units on that?

MR. SIESS: I am sorry, I do not understand.

MR. WOODFIN: This is leak rate -- this should have been marked out. I did not want you to have any numbers on that at all. Ignore the numbers, that should have been marked out completely. This is a conceptual -- this is pressure, this is leak rate.

MR. SIESS: I do not understand the pressure. You said I should ignore these numbers --

MR. WOODFIN: Ignore the numbers on both axes.

MR. SIESS: This is the change in leak rate as you increase the pressure?

MR. WOODFIN: Yes.

MR. SIESS: Okay.

MR. WOODFIN: This is a sort of function that the risk analysis people would like to have.

Now, what I am saying is for our small-scale steel models, I expect a step function like this (indicating), that we will come to a catastrophic point and then it will break and you have an undefinable leak rate, or an infinite leak rate.

However, I may be able to get a couple of points on that curve with considerable error bands for these small models. But that is probably the best we can hope for on the small models.

Now, as we go to the larger models, where we have longer test time available to us, we get more points on this curve and can begin to generate something of that nature.

MR. SIESS: But you are not going to get a curve like that unless you have an opening of some kind from the beginning.

MR. WOODFIN: That is correct, that is correct. It will start off -- it will come up from zero somewhere, and this shows some non-zero opening here. But it also shows -- you can view this as not zero pressure. This is just designed to show conceptually the sort of thing that was requested.

MR. SIESS: What you expect to get on the small model is a perfect job and we get that square line over there.

MR. WOODFIN: However, it may be if we have flaws in the model then I can say that it is somewhere inbetween these things, that if I can merely detect a leak prior to failure, prior to catastrophic failure and say for sure it is in the model, that there is an indication that it was not that step function. And that tells me that I need to concentrate more on the later ones in trying to determine what that function might look like.

MR. EBERSOLE: You said you were going to dry this thing with the heater, going to keep it warm?

MR. WOODFIN: Keep it at constant temperature, yes.

MR. EBERSOLE: You are going to override the solar input?

MR. WOODFIN: We are going to shade it. That was one of the reasons, because in this area that is significant, and it would also make significant axisymmetries in the problem, so we decided to build a little patio cover over the thing. Now, that is a secondary function. It will contain in it some chain mail so if this thing fails, -- as the safety people say, the worst case might be -- and you have a crack running around at the base and it takes off like a mortar shell, we hope it will not shoot down any airplanes.

(Laughter.)

MR. ETHERINGTON: Is your gas supply going to be large enough so you can override any sizeable split that does not cause a complete rupture?

MR. WOODFIN: We have sized the gas supply so we can handle at least a 100% per day leak rate.

MR. ETHERINGTON: That is rather a small split, isn't it?

MR. WOODFIN: Yes, it is. I think I can handle probably 1000% percent per day.

MR. ETHERINGTON: If you get a small split, that is the end of your test run.

MR. WOODFIN: Yes, if you get one of significant size.

(Slide.)

Okay. This next curve shows you some of the problems associated with measuring or trying to infer leak rates. This particular one is just concerned with uncertainties in volume, but you can have the same kind of nomograph generated for uncertainties; uncertainties in pressure, temperature measurements or anything else you want. And suppose I demand that I know to within a tenth of a percent per day the leak rate, or that I detect a leak rate of a tenth of a percent per day with an error of only 1% in the leak rate. Or, that I have only 1% uncertainty in the volume. Then my error in the leak rate is 1000%.

If, in fact, I am willing to settle for something more modest, a more modest goal and say I can detect a 10% per day leak rate with a tenth of a percent increase uncertainty in the volume -- this is a tenth of a percent here (indicating) -- then I can achieve 1% accuracy in my statement of the leak rate. So this tells me how well I have to know the volume in order to be able to make a certain statement, a statement of leak rate, with a given accuracy. And as I say, I use this volume uncertainty as an example, but I can do the same sort of graph for knowledge of the pressure, knowledge of the temperature.

(Slide.)

You asked about the size of holes. This you may find to be a useful little thing. It involves a pretty simple assumption; namely, that the flow is a choked flow condition. That is, that the flow comes to sonic condition at a hole, and it assumes a clean circular hole. So you know it is only a very rough estimate as to what size clean circular hole would be required to give you this sort of performance, and it also does not include friction in the channels.

So if we are talking about -- as in the case of the large containments -- a four-inch thick steel plate with a one-tenth inch diameter hole in it, this is not going to be an accurate statement but this will give you an idea of the kind of size of hole.

MR. SIESS: How big a steel plate?

MR. WOODFIN: One and a half inches, but it still was a tenth of an inch hole that has friction in the channel which is not included in this statement.

MR. SIESS: Concrete when it is quarter-inch plate.

MR. WOODFIN: Yes.

MR. SIESS: Plus some concrete.

MR. WOODFIN: Yes. So here we have --

MR. SIESS: Can you explain the 100 degrees, 600 degrees on the left?

MR. WOODFIN: That is total temperature, and that shows the size of the holes are very --

MR. SIESS: I do not understand. I have 100 degrees with arrows pointing to a line, 600 degrees the same line.

MR. WOODFIN: On this side, each one of these bands is representative of 100^{0} F condition; on this side of the band it is representative of a 600^{o} F condition. So it shows the hole size is a very weak function of pressure because pressure is included in the statement of total temperature.

MR. SIESS: So if I have a two million cubic foot containment, one-tenth of a percent per day leak rate corresponds to a tenth of an inch diameter hole.

MR. WOODFIN: Yes.

MR. SIESS: Ain't very big, is it?

MR. WOODFIN: No.

MR. SIESS: And 100% per day would be about three inches.

MR. WARD: This is at what sort of pressure?

MR. WOODFIN: This is at any pressure which you can generate which will give you total stagnation temperature between 100 and 600 ^{o}F, which corresponds to any of the realistic pressures that you might encounter.

(Slide.)

One of the problems -- this just illustrates one of the problems associated with trying to infer leak rates, and this is why it takes the six to eight hours of measurements I was talking about because you have a certain amount of noise and measurement uncertainty, and you have to wait for that leak rate to grow to produce enough leaks, so it grows up out of the noise. That is basically what happens. And so, the minimum time required is when the leak pokes its head up out of the noise.

(Slide.)

Our pressure system will look like this, conceptually. I have guessed who would get the bid, so that model 3500 program pressure generator may not actually be that, it may be somebody else's, but it looked like of the people who came in with bids, they are the most likely to get it. That is the Schwien.

We have the two trailers, we have the signal conditioning trailer shown in the big block and the command trailers shown in the little one. There is a fiber optic link which gives commands from the computer to the pressure controller, the nitrogen bottles then through the associated safety piping and such things as that go out, supply the gas and the pressure controller then anticipates so that we get a very small overshoot. I think I spec'ed that at something on the order of one-hundredth of a psi overshoot.

So, it has a control function which does anticipate and gives you the stepwise pressure that you order.

(Slide.)

MR. ZUDANS: How is the vessel supported?

MR. WOODFIN: I did not indicate that in this particular case. There is a heavy structure made of ASME boiler components which formed at the base for the model and the bottom pressure seal. I can show you some of those components when we go through the tour. I did not present them here.

Now, the site arrangement looks like this. If you will notice the distances, this is on the order of

100 feet (indicating). Now, each of these, the Autolite station is a manned station and the fellow pokes his theodolites up through a hole in the pressure barrier. You can see the model and tape data when it is ordered. There will be no one in this trailer; this is all just equipment.

The command function would be down here. Thi is the fiber optics data link; this is on the order of 300 yards, and there there is a safety observer up on the hill who can see everything, and I will point out these things when we get out to the site as to where everything is going to be located. But that is the general arrangement.

(Slide.)

Now, in order to ensure safety we have to design three zones. There is the red zone where you have to take some special procedures to make sure that you are not introducing any gas into the model anytime that anybody is inside that zone, and basically you only let people go in there when you have to do some repair or something of that nature, and even then you do not go up next to the model unless you depressurize.

Okay. In this blue zone, people will be manning stations in there and they can have controlled entrance and egress, but not during a pressure loading step, or until the model has stabilized after a pressure loading step.

The green zone is a free access zone, and if any of you want to come out and watch, well we will give you a place in the green zone from which to observe.

MR. ETHERINGTON: How far will this thing travel under the worst kind of rupture that you envisage?

MR. WOODFIN: We have a good-sized argument with the safety people about that.

MR. ETHERINGTON: Half a mile at least, won't it?

MR. WOODFIN: If everything worked perfectly, in order to make a projectile of it, perhaps that is the case. Let me show you this. This is a map of Bernalillo County. We are currently right here, we are at the airport or near the airport When we take the tour we will go down here under the base -- this is the Sandia technical area near where our offices and our laboratory are. Then we will drive up behind Mosano base, which is this mountain that is all fenced in, to the test site right on the edge of the national forest just before you get into the national forest. That is where our test site will be, so we are in a considerably remote location. And that, of course, adds to the expense of this sort of testing.

That concludes what I had prepared to say. I would be glad to answer any further questions.

MR. EBERSOLE: Do you have -- I am trying to remember. Do you have to completely depressurize before you approach it, or just down a certain percent from where you were?

MR. WOODFIN: It depends on which zone you are going to and how far into the zone. If you only need to get into the single conditioning trailer, we can enter that from the opposite end. That is shielded, so then we only have to stop introducing pressure.

MR. EBERSOLE: Oh?

MR. WOODFIN: And hold it at that level with locked valves, which means basically, you go to the condition that you would go to for measuring the leak function, which is a stable condition where the pressure controller cannot introduce anymore in.

In the other case, that is a procedural matter of you need to go into the blue zone, to make sure that you do not introduce any pressure while someone is going or coming to one of the barriers, and that whoever is in the barrier has his head down. But as far as going up next to the model, we would then depressurize, not completely but we would depressurize a degree so that we are well below, say, 10% below the current level.

Something of that nature.

MR. EBERSOLE: Step it down 10% from where you were?

MR. WOODFIN: Something on that order. It depends on what region the stress strain curve were in. If we were really close to that runaway area, I would step it down further than if I were in an area where I had some strength hardening to go to.

MR. ZUDANS: And would you make all the measurements for this stepdown?

MR. WOODFIN: Yes, we will take data.

MR. ZUDANS: That would be unloading essentially?

MR. WOODFIN: I do not anticipate doing that, but I am prepared to take data if I have to do it.

MR. ZUDANS: Good, good, good.

MR. SIESS: Thank you. Where are we now, Walt?

MR. VON RIESEMANN: Just a few more final wrap-ups.

MR. ZUDANS: What is the time duration of one test like this here?

MR. WOODFIN: I am trying to make this so that I can get a test with no leak measurements in one day, dawn to dusk.

MR. ZUDANS: With the same shift of people?

MR. WOODFIN: With the same shift of people. However, if we have to make leak measurements, that will carry it over a day at least for every datapoint that we are looking for.

(Slide.)

MR. SEBRELL: My name is Wayne Sebrell, Sandia Labs, and I am part of the program overhead.

(Laughter.)

I mainly wanted to touch on some of the other activities because as you have seen from the analsyis and the fabrication of the test models and the test site, there is a lot of preparation going on, but there is also some peripheral activities that I just wanted to touch base with so that you are aware of them.

Primarily, these are in the technical information and interchanges.

(Slide.)

We have had some interaction with the advisory group. We interact with them not on a set basis but whenever we need to get some -- part of the program reviewed we meet with them. We also have a variety of visitors coming in. We have a workshop that I will discuss and also I will touch base on some of the Sandia programs which I think will answer some of the questions you had earlier, and also on some related programs.

(Slide.)

On the next viewgraph, the advisory groups meets periodically, and what we have done since the last meeting with the ACRS is to add somewhat with a systems background.

(Slide.)

And the next viewgraph is a partial list of some of the people who have visited us, and also that we have interchanged information with. Lowell Greiman from Iowa University, the General Electric people came in and presented a degraded core study which was equivalent to the severe accident sequence analysis. George Howard, Jack Burns, John Stevenson and Bill Baker, and we also had visitors from other countries.

We had Dr. Lemar Wolf from the FDR project. We had four Japanese who were in from the structural dynamic committee; Professor Helmut Karwat from Munich, and two people from the United Kingdom. And just last Friday we had the siting survey mission, which consisted of a group of 15 Japanese.

(Slide.)

One of the other activities were are involved in primarily is in looking at the structural part of the program. It is just a very small part or maybe a small part of what is considered containment integrity, and to get a broader feeling for this, a broader scope of it, we are in the process of setting up a workshop which is June 7, 8 and 9, and the information is on the yellow sheet that is being handed out.

But primarily, it is to provide a broader description of concerns of containment integrity, beyond just the ultimate capacity of the building itself, and to improve our understanding of potential sources and location of leak paths, and providing means of interchange of ideas. We do not profess to know all the ideas that are involved in containment integrity, but we hope that we get a very good representation of people there.

The major topics, as you can see, is that we have the actual failure pressure of containment buildings.

(Slide.)

It is to look at current programs, both in terms of analysis and experiments, operational concerns which deals with the leak rate, leak integrity, load characterization which involves both seismic internal pressurization and also, hydrogen loading. The other related programs that would feed into it -- and I will touch on those in just a minute.

(Slide.)

On the next viewgraph, the Sandia programs that are related that we have some interchange in, and the first one is the hydrogen program in which the hydrogen people are trying to assess what the hydrogen problem is and what the magnitude of it is.

We are also interfacing with the Sandia study that is being funded by the risk part of the house at NRC on the SASA studies, and I can go into more detail if you would like to know more about that program, but right now, it is more in a feasibility study or proposed study part of the program. And also, we are interfacing with the PRA studies.

All our programs are being monitored by a Sandia QA/QC activity which is the quality assurance and quality control, and it influences the program. You have seen the part that Al Dennis touched on, which was the program management. That is part of the quality assurance to make sure we have some method of recording what is happening, to make sure we follow the steps and make sure that hopefully, nothing falls through the cracks at least on the fabrication, the testing and the analysis part of the program, which we are primarily deling with.

(Slide.)

On the next viewgraph are some other related programs or meetings, workshops that we have encountered, and one is the ASME subcommittee on containments; that is, the steel portion. There is another one on reinforced concrete. We have the DOE containment program and we have the ninth light water research information meeting, which is a week-long interchange of all the different areas of research that are going on in the NRC, and probably many of you attend that meeting.

Ron Woodfin attended the integrated leak rate testing workshop out at San Diego and they touched on all the various aspects of how do you measure leak rate and the accuracy of it, so there is a lot of concerns on that. We have been talking to the people at Los Alamos on their structures program, and they are doing a very good job in my opinion on the parts of the structures that they are addressing.

We have had some discussions with EPRI and just recently -- and I think Jim Costello touched on that briefly -- on the IDCOR containment structural capability workshop.

(Slide.)

On the next viewgraph, the next three viewgraphs, is some of the highlights of the IDCOR containment structural capability committee, and their objective is trying to find the failure conditions, modes, loadings and the probability of the containment building itself.

MR. SIESS: Failure meaning?

MR. SEBRELL: Maybe the next couple of viewgraphs will show that, but one of the things --

MR. SIESS: I would just like to know if somebody has defined failure. I keep seeing the word. Is there a single accepted definition of failure that all people are using when they talk about this?

MR. SHEWMON: Loss of integrity came up earlier.

MR. WARD: Unacceptable leak rate.

MR. SEBRELL: Unacceptable leak rate.

MR. SIESS: We all have our definitions. I just wondered if --

MR. ZUDANS: I do not think that can be resolved.

(Slide.)

MR. SEBRELL: When I looked at some of the things that have gone on in some of the analyses, I have tried to look for a definition of failure and my only conclusion was that there was not any real clear definition of a failure.

MR. SIESS: Is there any possible definition of interest except an unacceptable leak rate, leaving unacceptable to be defined by somebody else.

MR. SEBRELL: That is really one of our primary objectives of having the workshop -- is to address that subject and talk it over and see if we can come into a closer agreement on what it is.

MR. SIESS: I have had the feeling -- and some of the earlier discussion touched on it -- that this approach -- in your experience, looking at all of these different groups, is anybody looking at the whole picture?

MR. SEBRELL: The severe accident sequence analysis?

MR. SIESS: No, I am talking about containment leakage, the relation between containment leakage, what goes on inside the containment and the probabilities that are involved.

MR. SEBRELL: I have not seen that myself. Of course, I have only been on the program about four or five months, so I am relatively new.

The highlights of the IDCOR workshop -- let me just go over those briefly -- is that they were trying to use a generic approach for the analysis of the ultimate capacity of containment buildings. They looked at a large variety of containment buildings, like 9 in all. The majority assumed yield stress as failure. In other words, they just really took what they had already analyzed on a design basis, which is in the elastic range, and you could extrapolate up to a yield point. And there was actually some reluctance to go beyond the yield point in trying to determine exactly what failure is.

Most of the people who are doing the analysis look to the penetrations, equipment hatches, personnel locks, piping penetrations, liner interactions, base mat and cylinder junctions. And out of those 9 containment buildings, there is only I think one or two that showed that the penetrations were the weak spot, and the two that they had were just very simple fixes to make them stronger. And so, the conclusion was that these types of penetrations were, for the particular accident scenarios, at least as strong as the ultimate capacity of the building or beyond.

Electrical penetrations were addressed. One particular reactor building they actually did some testing of the electrical penetrations. They tested them up to 100 psi, and I think it is 325°F and this was beyond what they figured the worst accident would be, the IDCOR results.

On the ultimate capacity of the containment buildings, the whole thing is divided into 24 tasks and this is only one task out of the 24, and this is task 10, and it inputs into this last item down here, which is the thermal hydraulic containment response in looking at the probabilities and consequences of the accidents.

(Slide.)

On the next viewgraph my observation -- let us skip that one.

(Slide.)

The IDCOR workshop, there were some remaining questions. There was no clear definition of a failure. There was a good feeling that many of the containment buildings could at least get to the yield of the structure, and then as you get beyond yield, there was some question about whether the building would displace too much or you would have too much strain or there would be some other type of interaction. And so, that was not completely defined.

Also, there seems to be the IDCOR people believe that there is not any need to consider dynamic pressures. They are not looking at hydrogen detonation; they are looking at rapid burn, but in the response times of the containment buildings these are essentally a quasi-static load. So they are only addressing the ultimate capacity from a limited standpoint. They are not considering variations in material properties.

But my own feeling is that if a containment building is going to be in existence for 30 or 40 years, that you would expect to see some differences in the material properties than when you originally started. There is very little experimental evidence that went into their study. They are using primarily analytical stuff.

However, Professor Murray from the Canadian experiments presented his information which was very, very well received. The Canadian experiments -- again, I would like to remind you -- did not have the penetrations. It was a pre-stressed containment building, one-fourteenth sale model. And there were a lot of discussions on penetrations, electrical penetrations.

The individuals who were working on that particular area did not feel there was -- you could address penetrations in a generic sense. In other words, there were so many penetrations, a very large variety of them, that you just could not come out and say all penetrations are going to be okay.

I think I will skip the next. Let us see, on the background study I will just touch on the schedule. You have seen that before.

(Slide.)

And let us see. Our schedule shows -- this is kind of like where we are going and where we are right now. We are just finishing up the background study. It has actually been finished for a period. So I would like to stop at this particular point.

MR. SIESS: Thank you very much.

MR. VON RIESEMANN: Dr. Siess, that ends our formal remarks.

MR. SIESS: Thank you, Walt. You have done an excellent job. I know you condensed some things, we appreciate it. The meeting will be adjourned shortly and we will go to lunch. And I assume we will all go together, so as soon as we can get back, -- are you going to be staying here with us until we leave?

MR. VON RIESEMANN: Yes, most of us will be.

MR. SIESS: I think 2:00 o'clock may be a little bit optimistic, but let us see if we can try to gather in the lobby at somewhere around 2:15.

(Whereupon, at 1:08 p.m. the meeting was recessed for lunch, to reconvene at 2:15 p.m. the same day.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

____

in the matter of: ACRS/Subcommittee on Structural Engineering

Date of Proceeding: March 22, 1982

Docket Number: ____________________

Place of Proceeding: Albuquerque, New Mexico

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

David S. Parker

Official Reporter (Typed)

(SIGNATURE OF REPORTER)

STRUCTURAL SAFETY MARGINS FOR CONTAINMENTS

SANDIA NATIONAL LABORATORIES
ALBUQUERQUE, NM

WALT VON RIESEMANN
TOM BLEJWAS
AL DENNIS
DAN HORSCHEL
WAYNE SEBRELL
RON WOODFIN

PRESENTATION TO
ACRS SUBCOMMITTEE
ALBUQUERQUE, NM
MARCH 22, 1982

OUTLINE OF PRESENTATION

ORGANIZATION OF PROGRAM	WALT VON RIESEMANN
PROGRAM PLANNING	AL DENNIS
SURVEY OF EXISTING FACILITIES	AL DENNIS
CONTAINMENT MODELS	
ANALYSES	TOM BLEJWAS, DAN HORSCHEL
EXPERIMENTAL PROGRAM	RON WOODFIN
PROGRAM SCHEDULE	WAYNE SEBRELL
RELATED ACTIVITIES	
LUNCH	11:30 TO 1:00
TOUR OF FACILITIES AT SANDIA	1:00 TO 3:30
RETURN TO AMFAC	4:00

KNOWING THE STRUCTURAL BEHAVIOUR OF THE CONTAINMENT BEYOND ITS DESIGN REQUIREMENTS AND KNOWLEDGE OF FAILURE MODES IS REQUIRED FOR.

- RISK STUDIES
- DESIGN OF ACCIDENT MITIGATION EQUIPMENT
- SEVERE ACCIDENT MITIGATION STRATEGIES
- EMERGENCY PREPAREDNESS PLANNING

NUREG-0772

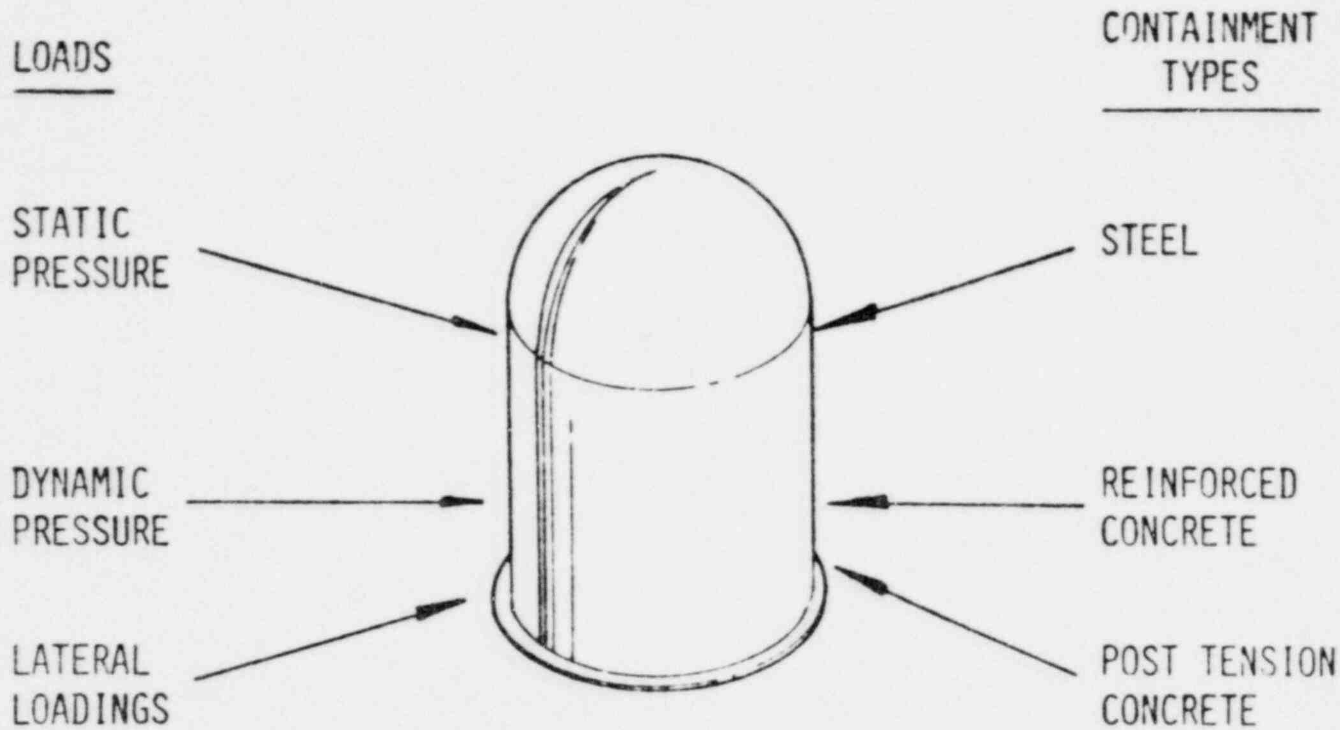
TECHNICAL BASES FOR ESTIMATING FISSION PRODUCT BEHAVIOUR DURING LWR ACCIDENTS

PAGE iii

"ONE OF THE LARGEST UNCERTAINTIES ASSOCIATED WITH PREDICTING THE AMOUNT OF RADIONUCLIDES RELEASED ... RESULTS FROM LIMITATIONS IN THE ABILITY TO PREDICT THE TIMING, MODE, AND LOCATION OF CONTAINMENT FAILURE."

OBJECTIVES

- GENERATE THE DATA BASE NEEDED TO ASSESS METHODS FOR PREDICTING ULTIMATE LOAD CAPACITY AND FAILURE MODES OF LWR CONTAINMENTS UNDER ACCIDENT CONDITIONS AND SEVERE ENVIRONMENTS
- ASSESS SELECTED PREDICTIVE NUMERICAL METHODS


LOADS
STATIC PRESSURE
DYNAMIC PRESSURE
LATERAL LOADINGS
CONTAINMENT TYPES
STEEL
REINFORCED CONCRETE
POST TENSION CONCRETE

APPROACH

- EXPERIMENTAL PROGRAM (UTILIZING STRUCTURAL MODELS) SUPPORTED BY A STRONG ANALYTICAL EFFORT

END PRODUCTS

- BENCH MARK DATA BASE
- ASSESSED ANALYTICAL METHODS
- RELIABLE METHOD FOR ASSESSING THE CAPABILITIES AT LIMIT STATES SIGNIFICANT TO PUBLIC HEALTH AND SAFETY

BACKGROUND STUDY

TYPES OF CONTAINMENTS

CODE (ASME/ACI)
REQUIREMENTS

PREVIOUS TESTS

ANALYSIS METHODS

SCALE MODELING
(SIMILITUDE)

LOAD SIMULATION
- STATIC
- DYNAMIC
- SEISMIC

PROGRAM
PLAN

ADVISORY
GROUP

ADVISORY GROUP

TOM AHL - CHICAGO BRIDGE AND IRON

BILL BAKER - SOUTHWEST RESEARCH INSTITUTE

RICH DENNING - BATELLE COLUMBUS LABS

ASA HADJIAN - BECHTEL

GEORGE HOWARD - ANCO

METE SOZEN - UNIVERSITY OF ILLINOIS

JOHN STEVENSON - JOHN STEVENSON AND ASSOCIATES

JOE UCCIFERRO - UNITED ENGINEERS

IAN WALL - EPRI

DICK WHITE - CORNELL UNIVERSITY

EVOLUTION OF CURRENT PROGRAM PLAN

FIRST VERSION

- REPLICA MODELS
- CONDUCT PARALLEL ACTIVITIES

↓

FINAL VERSION

- PROTOTYPICAL MODELS
- DELAY DYNAMIC AND SEISMIC ACTIVITIES

OVERALL PROGRAM PLAN

STATIC INTERNAL OVERPRESSURIZATION

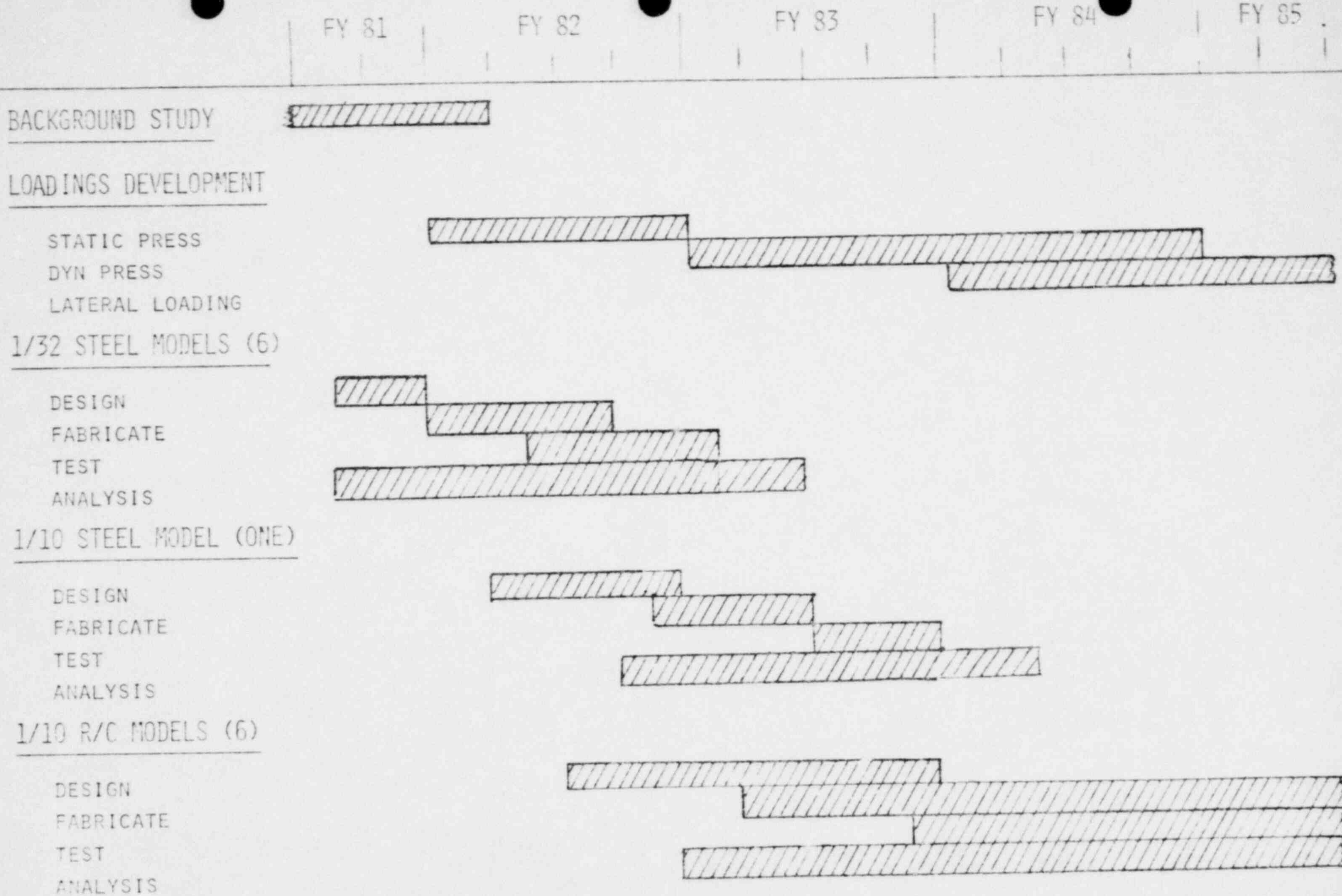
- HYBRID STEEL
- REINFORCED CONCRETE
- PRESTRESSED CONCRETE

DYNAMIC INTERNAL OVERPRESSURIZATION

- SAME SEQUENCE

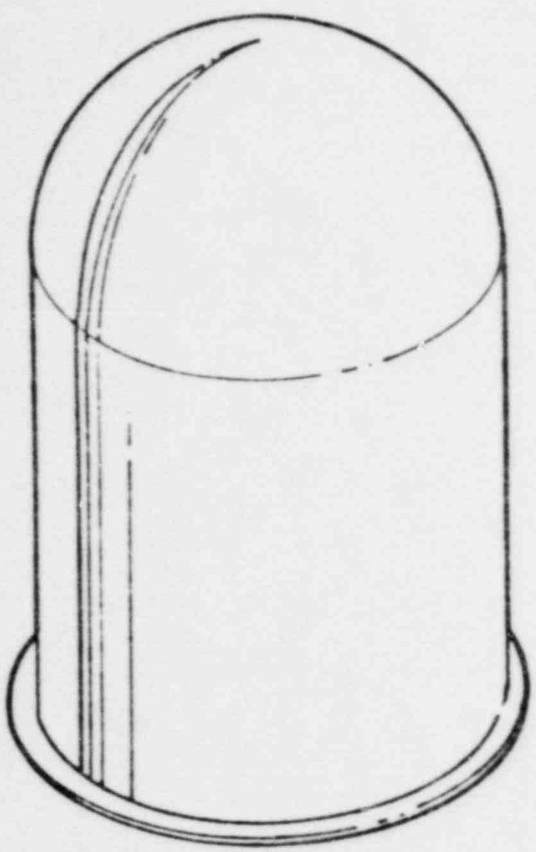
EARTHQUAKE LOADING

- REINFORCED CONCRETE
- PRESTRESSED CONCRETE
- HYBRID STEEL

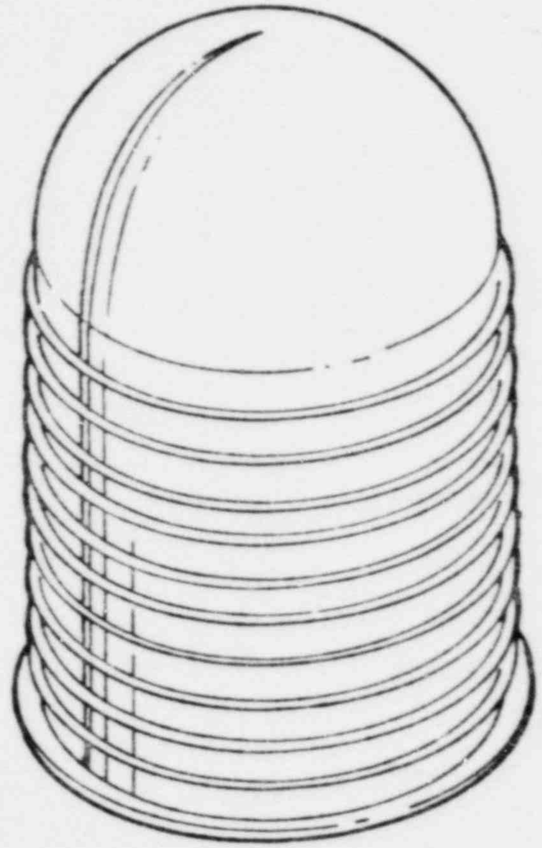

FY 81
FY 82
FY 83
FY 84
FY 85
BACKGROUND STUDY
LOADINGS DEVELOPMENT
STATIC PRESS
DYN PRESS
LATERAL LOADING
1/32 STEEL MODELS (6)
DESIGN
FABRICATE
TEST
ANALYSIS
1/10 STEEL MODEL (ONE)
DESIGN
FABRICATE
TEST
ANALYSIS
1/10 R/C MODELS (6)
DESIGN
FABRICATE
TEST
ANALYSIS

STEEL MODELS

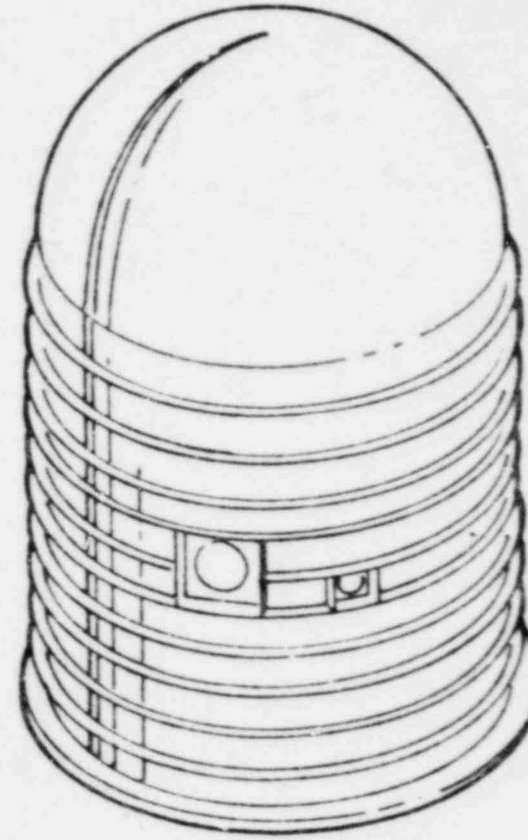
1/32 Scale



BASELINE
SHELL



SHELL WITH
HORIZONTAL STIFFENERS



PENETRATIONS AND
STIFFENERS

PROGRAM STATUS

- 1/32 SIZE HYBRID STEEL MODELS ARE BEING FABRICATED
- EXPERIMENTAL FACILITY IS IN PREPARATION
- ANALYSES ARE UNDER WAY
- TESTING TO BEGIN IN THIRD QUARTER OF CY '82

SUMMARY

STRUCTURAL SAFETY MARGINS OF CONTAINMENTS PROGRAM WILL PROVIDE, THROUGH A COMBINED EXPERIMENTAL AND ANALYTICAL EFFORT, A RELIABLE CAPABILITY TO EVALUATE PREDICTIONS OF THE ULTIMATE CAPACITY OF LWR CONTAINMENTS WHEN SUBJECTED TO STATIC AND DYNAMIC INTERNAL OVERPRESSURIZATION LOADS AND TO EARTHQUAKE LOADINGS

LAYIN 3/22

CONTAINMENT INTEGRITY

CONTRACTOR: SANDIA NATIONAL LABORATORY

PRINCIPAL INVESTIGATOR: WALTER VON RIESEMANN

NRC PROGRAM MANAGER: JAMES F. COSTELLO

PRINCIPAL QUESTIONS: WHAT ARE THE FAILURE MODES AND ASSOCIATED LOAD LEVELS FOR CONTAINMENT STRUCTURES?

APPROACH:

- THE GENERATION OF THE DATA BASE NEEDED TO ASSESS METHODS FOR PREDICTING THE BEHAVIOR OF LWR CONTAINMENTS UNDER ACCIDENT AND SEVERE ENVIRONMENTS BEYOND CURRENT DESIGN REQUIREMENTS

- THE ASSESSMENT OF SELECTED PREDICTIVE NUMERICAL METHODS

- THE IMPROVEMENT OF PREDICTIVE NUMERICAL METHODS AS NECESSARY

REVIEW PANEL MEMBERS

TOM AHL - CHICAGO BRIDGE & IRON

BILL BAKER - SOUTHWEST RESEARCH INSTITUTE

PETE CYBULSKIS - BATTELLE (COLUMBUS)

ASA HADJIAN - BECHTEL (LOS ANGELES)

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JOHN STEVENSON - STEVENSON AND ASSOCIATES

JOE UCCIFERRO - UNITED ENGINEERS

IAN WALL - EPRI

DICK WHITE - CORNELL UNIVERSITY

CONTAINMENT INTEGRITY

AREAS OF UTILIZATION

- JUDGING CREDIBILITY OF CAPACITY ESTIMATES MADE ON BEHALF OF LICENSEES AND APPLICANTS
- INPUT FOR RISK ANALYSES PERFORMED AS PART OF THE SEVERE ACCIDENT RESEARCH PLAN

CURRENT SCHEDULE

- STATIC PRESSURE FY 82-84
- UNSYMMETRIC PRESSURE FY 85-87
- SEISMIC EFFECTS FY 88-90

CONTAINMENT INTEGRITY

INTERACTION WITH U.S. PROGRAMS

- IDCOR - STATE-OF-THE-ART PREDICTIONS OF CAPACITY
- D°E - LEAK RATE AND FILTRATION QUESTIONS RELATED TO "FAILURE"

INTERACTION WITH FOREIGN PROGRAMS

- CANADA - GENTILLY CONTAINMENT DATA
- UK - POSSIBLE SNUPPS TEST
- JAPAN - LARGE SHAKE TABLE

LAyin

PROGRAM PLANNING

AL DENNIS

PRESENTED TO ACRS SUBCOMMITTEE
ALBUQUERQUE, NM
MARCH 22, 1982

SANDIA LABORATORIES

USES THE

PROJECT PLANNING AND ANALYSIS REPORTING SYSTEM

(PPARS)

DEVELOPED BY NASA

PLANNING INPUT

- EACH INDIVIDUAL TASK IS IDENTIFIED
- THE CALENDAR TIME REQUIRED FOR ACCOMPLISHMENT OF EACH TASK IS DETERMINED
- THE RESOURCE REQUIREMENTS FOR EACH TASK ARE ESTABLISHED (MANPOWER, MONEY, FACILITIES, ETC.)

PLANNING OUTPUT

- CALENDAR SCHEDULING OF EACH EVENT
- IDENTIFICATION OF THE CRITICAL PATH
- CUMULATIVE SUMMARIES OF RESOURCES VS. TIME

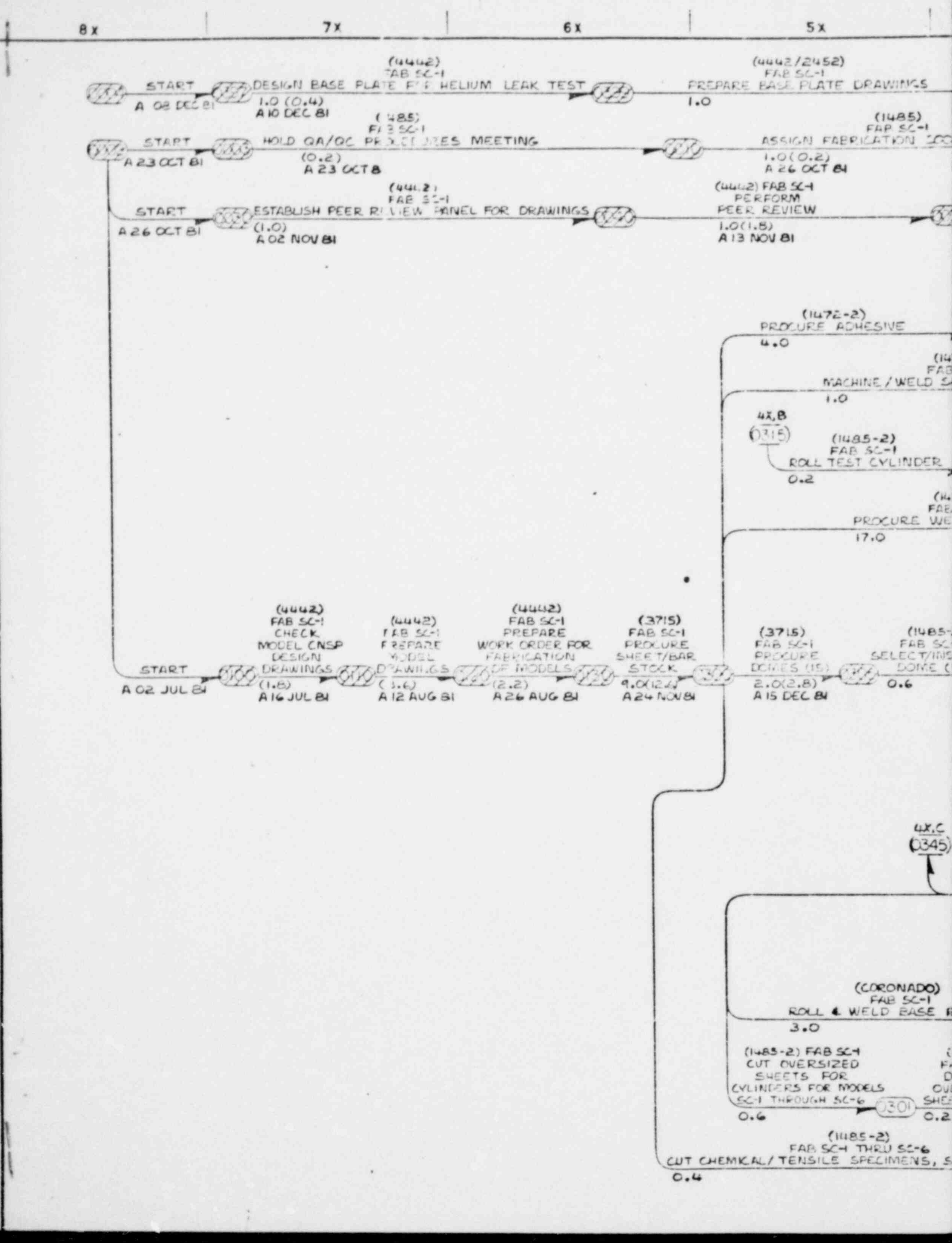
EXAMPLES - SMALL SCALE MODELS

SHOP NETWORK

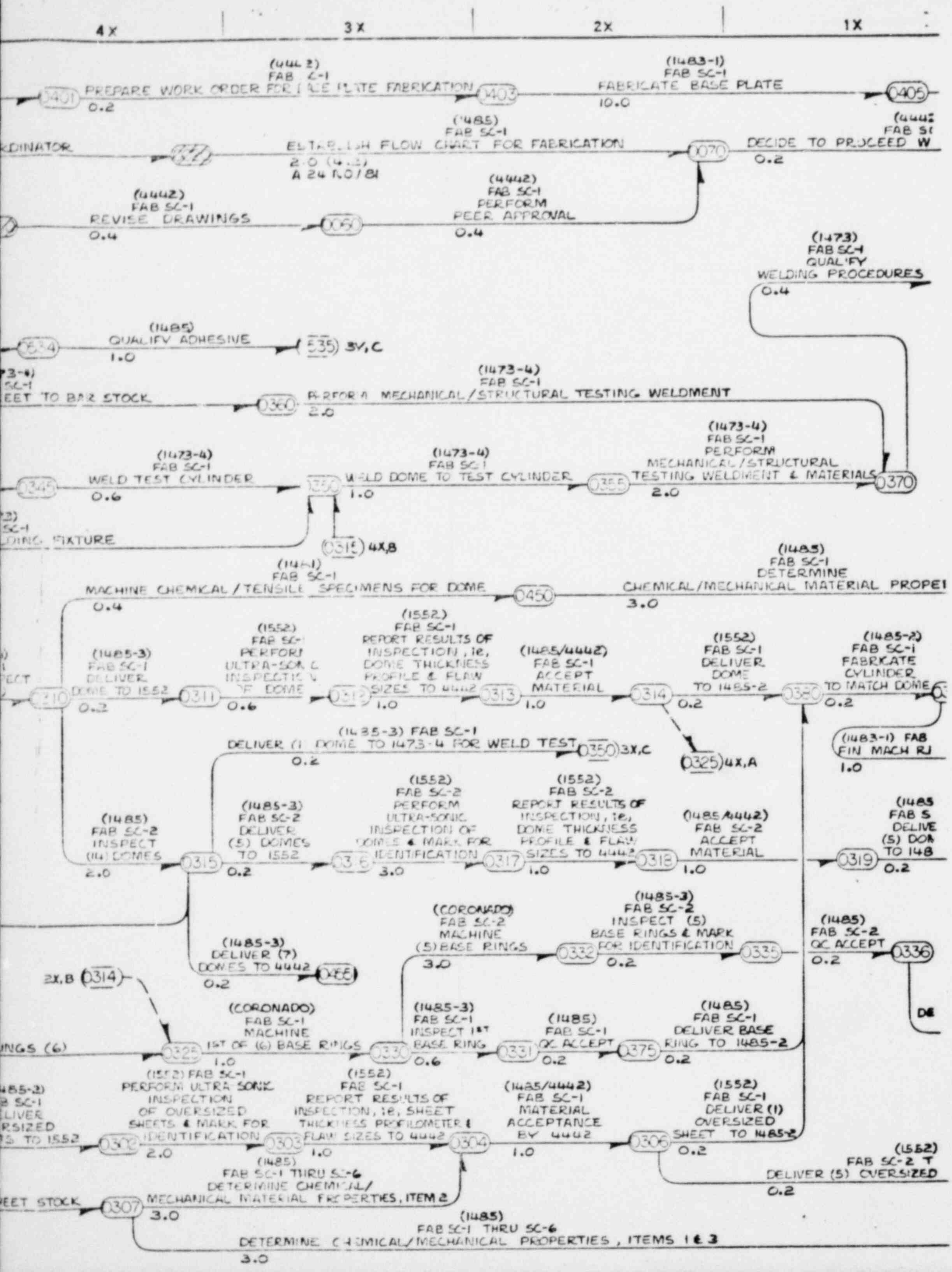
THEORETICAL ANALYSIS NETWORK

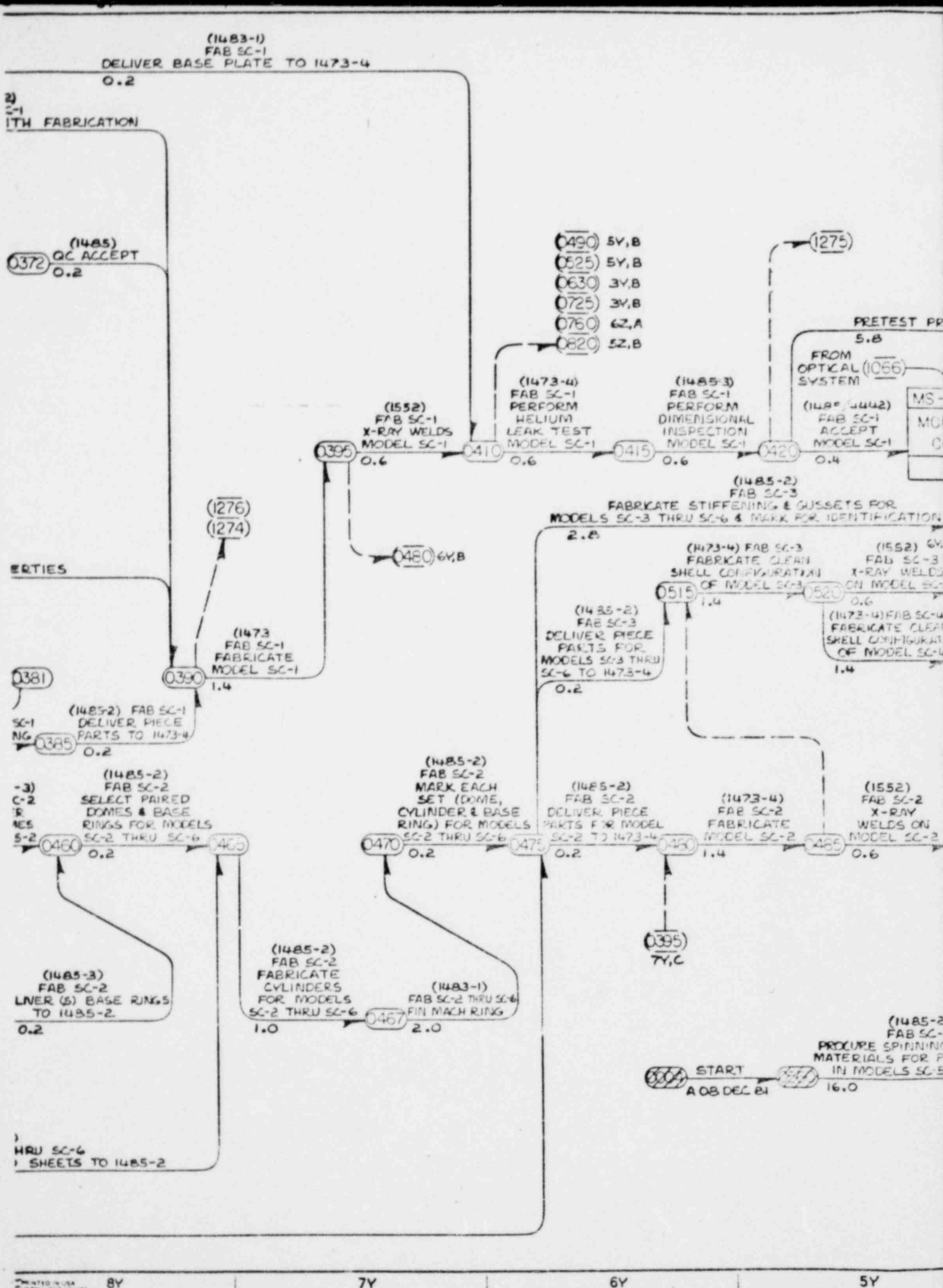
TYPICAL EXPERIMENT NETWORK

OVER-VIEW NETWORK

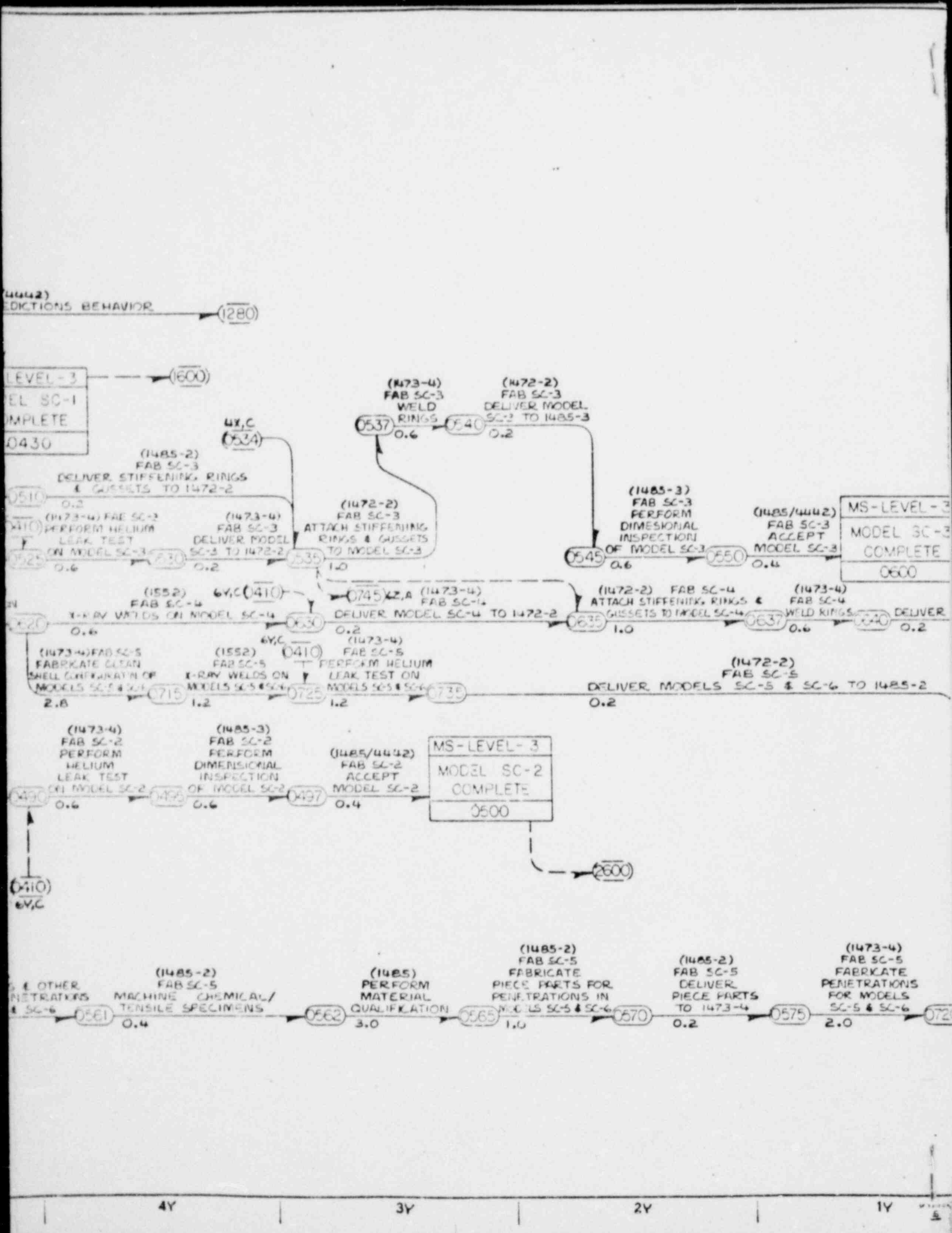


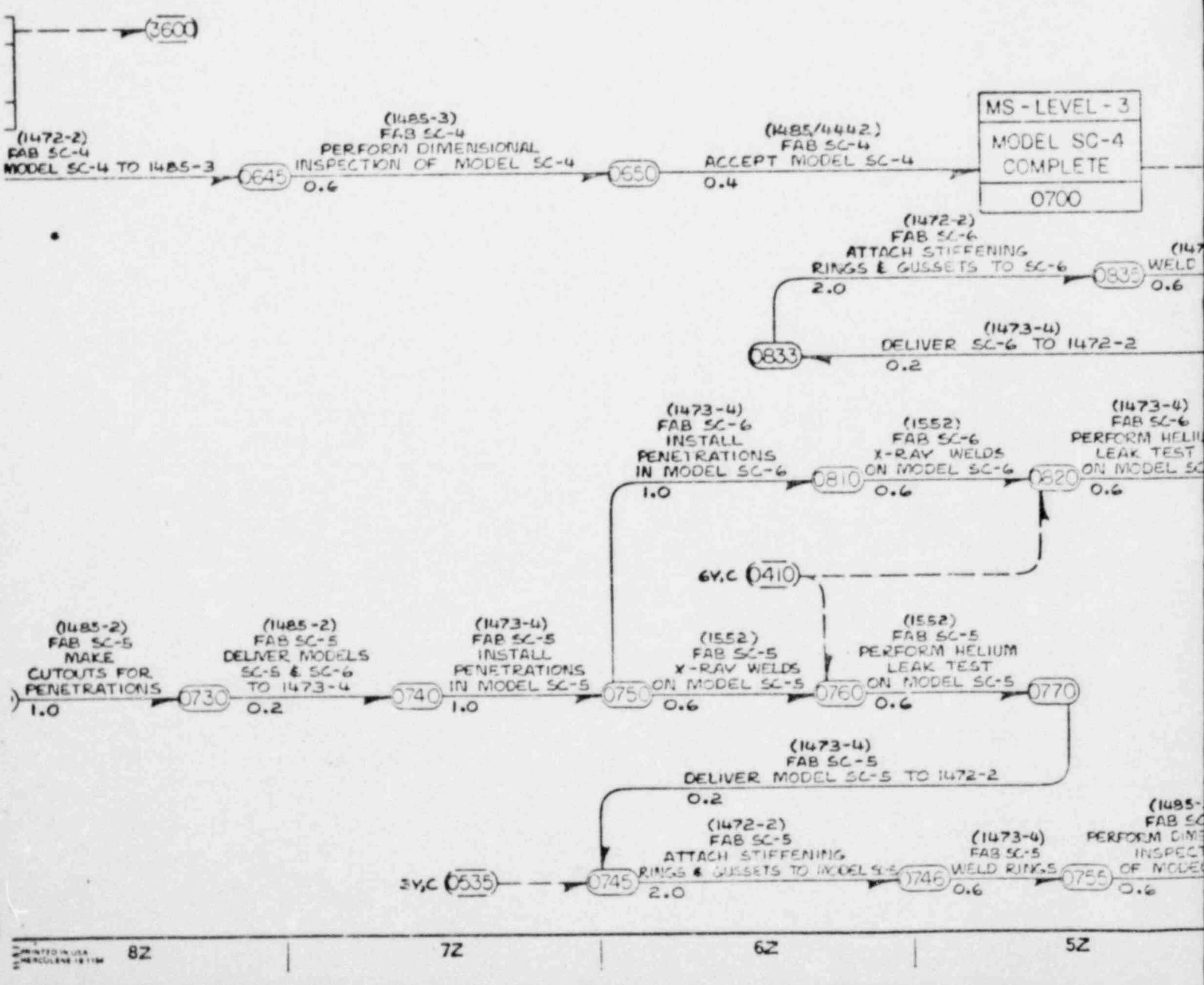
8X
7X
6X
5X
(4442)
FAB SC-1
START
DESIGN BASE PLATE F'R HELIUM LEAK TEST
A 08 DEC 81
1.0 (0.4)
A 10 DEC 81
(4442/2452)
FAB SC-1
PREPARE BASE PLATE DRAWINGS
1.0
(1485)
FAB SC-1
START
HOLD QA/QC PROCEDURES MEETING
A 23 OCT 81
(0.2)
A 23 OCT 8
(1485)
FAB SC-1
ASSIGN FABRICATION
1.0 (0.2)
A 26 OCT 81
(4442)
FAB SC-1
START
ESTABLISH PEER REVIEW PANEL FOR DRAWINGS
A 26 OCT 81
(1.0)
A 02 NOV 81
(4442) FAB SC-1
PERFORM
PEER REVIEW
1.0 (1.8)
A 13 NOV 81
(1472-2)
PROCURE ADHESIVE
4.0
MACHINE / WELD
1.0
4X.B
0315
(1485-2)
FAB SC-1
ROLL TEST CYLINDER
0.2
PROCURE WE
17.0
(4442)
FAB SC-1
CHECK
MODEL CNSP
DESIGN
DRAWINGS
(4442)
FAB SC-1
PREPARE
MODEL
DRAWINGS
(4442)
FAB SC-1
PREPARE
WORK ORDER FOR
FABRICATION
OF MODELS
(3715)
FAB SC-1
PROCURE
SHEET/BAR
STOCK
(3715)
FAB SC-1
PROCURE
DOMES (15)
FAB SC-
SELECT/INS
DOME
START
A 02 JUL 81
(1.8)
A 16 JUL 81
(3.6)
A 12 AUG 81
(2.2)
A 26 AUG 81
9.0 (12.4)
A 24 NOV 81
2.0 (2.8)
A 15 DEC 81
0.6
4X.C
0345
(CORONADO)
FAB SC-1
ROLL & WELD BASE
3.0
(1485-2) FAB SC-1
CUT OVERSIZED
SHEETS FOR
CYLINDERS FOR MODELS
SC-1 THROUGH SC-6
0.6
0301
0.2
(1485-2)
FAB SC-1 THRU SC-6
CUT CHEMICAL/TENSILE SPECIMENS,
0.4


4X
3X
2X
1X
(4442) FAB SC-1
0401 PREPARE WORK ORDER FOR BASE PLATE FABRICATION 0.2
0403
(1483-1) FAB SC-1
FABRICATE BASE PLATE 10.0
0405
(1485) FAB SC-1
ESTABLISH FLOW CHART FOR FABRICATION 2.0 (4.3) A 24 NO/81
0070
DECIDE TO PROCEED W 0.2
(4442) FAB SC-1 REVISE DRAWINGS 0.4
0060
(4442) FAB SC-1 PERFORM PEER APPROVAL 0.4
(1473) FAB SC-1 QUALIFY WELDING PROCEDURES 0.4
(1485) QUALIFY ADHESIVE 1.0
0534
0535 3Y,C
SHEET TO BAR STOCK
0360
(1473-4) FAB SC-1 PERFORM MECHANICAL/STRUCTURAL TESTING WELDMENT 2.0
(1473-4) FAB SC-1 WELD TEST CYLINDER 0.6
0345
0350
(1473-4) FAB SC-1 WELD DOME TO TEST CYLINDER 1.0
0355
(1473-4) FAB SC-1 PERFORM MECHANICAL/STRUCTURAL TESTING WELDMENT & MATERIALS 2.0
0370
WELDING FIXTURE
0315 4X,B
(1461) FAB SC-1 MACHINE CHEMICAL/TENSILE SPECIMENS FOR DOME 0.4
0450
(1485) FAB SC-1 DETERMINE CHEMICAL/MECHANICAL MATERIAL PROPE 3.0
0310
(1485-3) FAB SC-1 DELIVER DOME TO 1552 0.2
0311
(1552) FAB SC-1 PERFORM ULTRA-SONIC INSPECTION OF DOME 0.6
0312
(1552) FAB SC-1 REPORT RESULTS OF INSPECTION, IE, DOME THICKNESS PROFILE & FLAW SIZES TO 4442 1.0
0313
(1485/4442) FAB SC-1 ACCEPT MATERIAL 1.0
0314
(1552) FAB SC-1 DELIVER DOME TO 1485-2 0.2
0380
(1485-2) FAB SC-1 FABRICATE CYLINDER TO MATCH DOME 0.2
(1483-1) FAB FIN MACH RI 1.0
(1485-3) FAB SC-1 DELIVER (1) DOME TO 1473-4 FOR WELD TEST 0.2
0350 3X,C
0325 4X,A
(1485) FAB SC-2 INSPECT (14) DOMES 2.0
0315
(1485-3) FAB SC-2 DELIVER (5) DOMES TO 1552 0.2
0316
(1552) FAB SC-2 PERFORM ULTRA-SONIC INSPECTION OF DOMES & MARK FOR IDENTIFICATION 3.0
0317
(1552) FAB SC-2 REPORT RESULTS OF INSPECTION, IE, DOME THICKNESS PROFILE & FLAW SIZES TO 4442 1.0
0318
(1485/4442) FAB SC-2 ACCEPT MATERIAL 1.0
0319
(1485 FAB S DELIVE (5) DON TO 148 0.2
(1485-3) DELIVER (7) DOMES TO 4442 0.2
0488
(CORONADO) FAB SC-2 MACHINE (5) BASE RINGS 3.0
0332
(1485-3) FAB SC-2 INSPECT (5) BASE RINGS & MARK FOR IDENTIFICATION 0.2
0335
(1485) FAB SC-2 QC ACCEPT 0.2
0336
DE
2X,B 0314
(CORONADO) FAB SC-1 MACHINE 1ST OF (6) BASE RINGS 1.0
RINGS (6)
0325
0330
(1485-3) FAB SC-1 INSPECT 1ST BASE RING 0.6
0331
(1485) FAB SC-1 QC ACCEPT 0.2
0375
(1485) FAB SC-1 DELIVER BASE RING TO 1485-2 0.2
(1485-2) FAB SC-1 DELIVER OVERSIZED SHEETS TO 1552
0302
(1552) FAB SC-1 PERFORM ULTRA SONIC INSPECTION OF OVERSIZED SHEETS & MARK FOR IDENTIFICATION 2.0
0303
(1552) FAB SC-1 REPORT RESULTS OF INSPECTION, IE, SHEET THICKNESS PROFILOMETER & FLAW SIZES TO 4442 1.0
0304
(1485/4442) FAB SC-1 MATERIAL ACCEPTANCE BY 4442 1.0
0306
(1552) FAB SC-1 DELIVER (1) OVERSIZED SHEET TO 1485-2 0.2
(1552) FAB SC-2 T DELIVER (5) OVERSIZED 0.2
SHEET STOCK
0307
(1485) FAB SC-1 THRU SC-6 DETERMINE CHEMICAL/MECHANICAL PROPERTIES, ITEM 2 3.0
(1485) FAB SC-1 THRU SC-6 DETERMINE CHEMICAL/MECHANICAL PROPERTIES, ITEMS 1 & 3 3.0



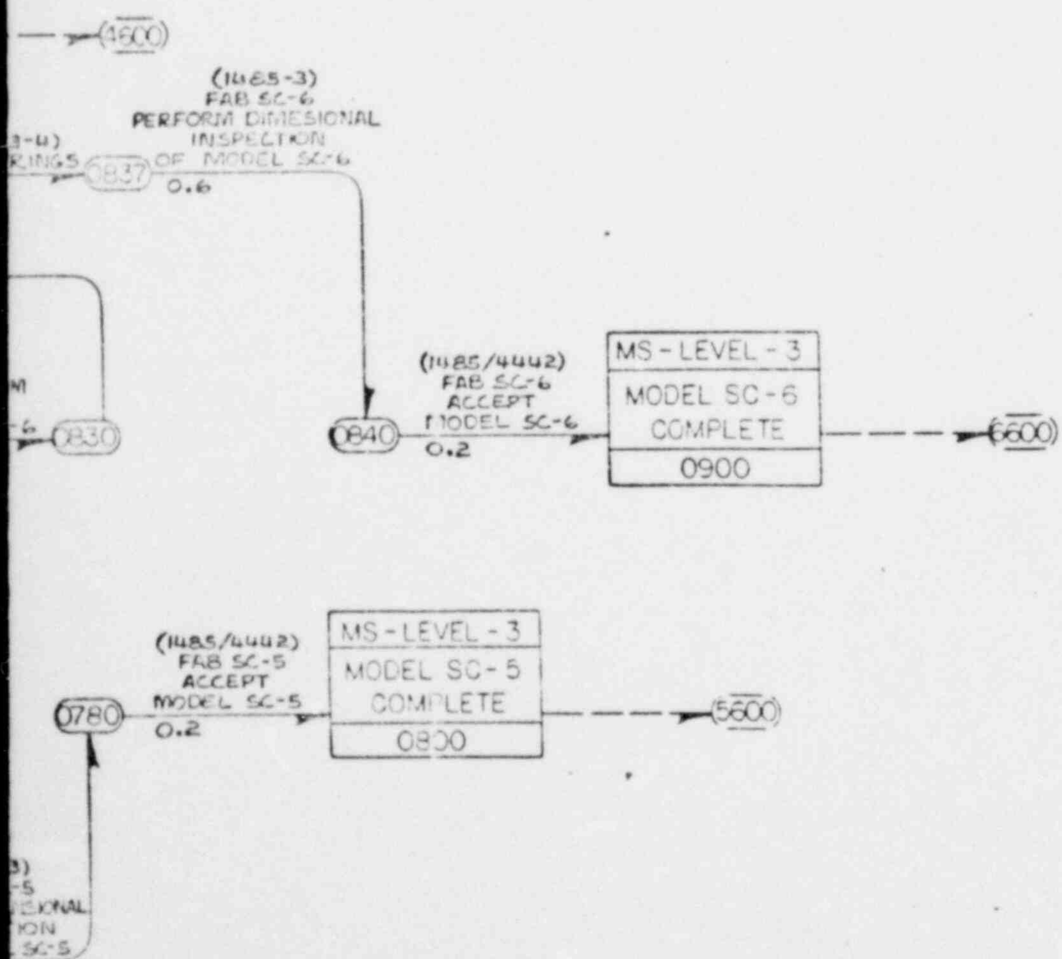
(1483-1) FAB SC-1 DELIVER BASE PLATE TO 1473-4 0.2
ITH FABRICATION
(1485) QC ACCEPT 0.2
0372
0490 5Y,B
0525 5Y,B
0630 3Y,B
0725 3Y,B
0760 6Z,A
0820 5Z,B
1275
PRETEST PR
5.8
FROM OPTICAL SYSTEM 1066
(1552) FAB SC-1 X-RAY WELDS MODEL SC-1 0.6
(1473-4) FAB SC-1 PERFORM HELIUM LEAK TEST MODEL SC-1 0.6
(1485-3) FAB SC-1 PERFORM DIMENSIONAL INSPECTION MODEL SC-1 0.6
ACCEPT MODEL SC-1 0.4
0395
0410
0415
0420
(1485-2) FAB SC-3 FABRICATE STIFFENING & GUSSETS FOR MODELS SC-3 THRU SC-6 & MARK FOR IDENTIFICATION 2.8
1276
1274
0480 6Y,B
ERTIES
(1473-4) FAB SC-3 FABRICATE CLEAN SHELL CONFIGURATION OF MODEL SC-3 1.4
(1552) FAB SC-3 X-RAY WELDS ON MODEL SC-3 0.6
(1473-4) FAB SC-4 FABRICATE CLEAN SHELL CONFIGURATION OF MODEL SC-4 1.4
0515
0520
(1485-2) FAB SC-3 DELIVER PIECE PARTS FOR MODELS SC-3 THRU SC-6 TO 1473-4 0.2
(1473 FAB SC-1 FABRICATE MODEL SC-1 1.4
0381
0390
(1485-2) FAB SC-1 DELIVER PIECE PARTS TO 1473-4 0.2
0385
(1485-2) FAB SC-2 SELECT PAIRED DOMES & BASE RINGS FOR MODELS SC-2 THRU SC-6 0.2
0460
0465
(1485-2) FAB SC-2 MARK EACH SET (DOME, CYLINDER & BASE RING) FOR MODELS SC-2 THRU SC-6 0.2
0470
0475
(1485-2) FAB SC-2 DELIVER PIECE PARTS FOR MODEL SC-2 TO 1473-4 0.2
0480
(1473-4) FAB SC-2 FABRICATE MODEL SC-2 1.4
0485
(1552) FAB SC-2 X-RAY WELDS ON MODEL SC-2 0.6
0395 7Y,C
(1483-3) FAB SC-2 LIVER (6) BASE RINGS TO 1485-2 0.2
(1485-2) FAB SC-2 FABRICATE CYLINDERS FOR MODELS SC-2 THRU SC-6 1.0
0467
(1483-1) FAB SC-2 THRU SC-6 FIN MACH RING 2.0
(1485-2) FAB SC-5 PROCURE SPINNING MATERIALS FOR PE IN MODELS SC-5 16.0
START A 08 DEC 81
HRU SC-6 SHEETS TO 1485-2
8Y
7Y
6Y
5Y


(4442)
EDICTIONS BEHAVIOR
1280
LEVEL-3
EL SC-1
MPLETE
0430
1600
(1473-4)
FAB SC-3
WELD
RINGS
0537
0.6
0540
(1472-2)
FAB SC-3
DELIVER MODEL
SC-3 TO 1485-3
0.2
4X,C
0534
(1485-2)
FAB SC-3
DELIVER STIFFENING RINGS
& GUSSETS TO 1472-2
0510
0.2
0410
(1473-4) FAB SC-3
PERFORM HELIUM
LEAK TEST
ON MODEL SC-3
0525
0.6
0530
(1473-4)
FAB SC-3
DELIVER MODEL
SC-3 TO 1472-2
0.2
(1472-2)
FAB SC-3
ATTACH STIFFENING
RINGS & GUSSETS
TO MODEL SC-3
0535
1.0
(1485-3)
FAB SC-3
PERFORM
DIMESIONAL
INSPECTION
OF MODEL SC-3
0545
0.6
0550
(1485/4442)
FAB SC-3
ACCEPT
MODEL SC-3
0.4
MS-LEVEL-3
MODEL SC-3
COMPLETE
0600
6Y,C
0410
0745
6Z,A
(1552)
FAB SC-4
X-RAY WELDS ON MODEL SC-4
0620
0.6
0630
(1473-4)
FAB SC-4
DELIVER MODEL SC-4 TO 1472-2
0.2
(1472-2) FAB SC-4
ATTACH STIFFENING RINGS &
GUSSETS TO MODEL SC-4
0635
1.0
0637
(1473-4)
FAB SC-4
WELD RINGS
0.6
0640
DELIVER
0.2
(1473-4) FAB SC-5
FABRICATE CLEAN
SHELL CONFIGURATION OF
MODELS SC-5 & SC-6
2.8
0715
(1552)
FAB SC-5
X-RAY WELDS ON
MODELS SC-5 & SC-6
1.2
6Y,C
0410
0725
(1473-4)
FAB SC-5
PERFORM HELIUM
LEAK TEST ON
MODELS SC-5 & SC-6
1.2
0735
(1472-2)
FAB SC-5
DELIVER MODELS SC-5 & SC-6 TO 1485-2
0.2
(1473-4)
FAB SC-2
PERFORM
HELIUM
LEAK TEST
ON MODEL SC-2
0490
0.6
0495
(1485-3)
FAB SC-2
PERFORM
DIMENSIONAL
INSPECTION
OF MODEL SC-2
0.6
0497
(1485/4442)
FAB SC-2
ACCEPT
MODEL SC-2
0.4
MS-LEVEL-3
MODEL SC-2
COMPLETE
0500
2600
0410
6Y,C
S & OTHER
NETRATIONS
& SC-6
0561
(1485-2)
FAB SC-5
MACHINE CHEMICAL/
TENSILE SPECIMENS
0.4
0562
(1485)
PERFORM
MATERIAL
QUALIFICATION
3.0
0565
(1485-2)
FAB SC-5
FABRICATE
PIECE PARTS FOR
PENETRATIONS IN
MODELS SC-5 & SC-6
1.0
0570
(1485-2)
FAB SC-5
DELIVER
PIECE PARTS
TO 1473-4
0.2
0575
(1473-4)
FAB SC-5
FABRICATE
PENETRATIONS
FOR MODELS
SC-5 & SC-6
2.0
072
4Y
3Y
2Y
1Y



3600
(1472-2)
FAB SC-4
MODEL SC-4 TO 1485-3
0645
(1485-3)
FAB SC-4
PERFORM DIMENSIONAL
INSPECTION OF MODEL SC-4
0.6
0650
(1485/4442)
FAB SC-4
ACCEPT MODEL SC-4
0.4
MS - LEVEL - 3
MODEL SC-4
COMPLETE
0700
(1472-2)
FAB SC-6
ATTACH STIFFENING
RINGS & GUSSETS TO SC-6
2.0
0835
WELD
0.6
(1473-4)
DELIVER SC-6 TO 1472-2
0.2
0833
(1473-4)
FAB SC-6
INSTALL
PENETRATIONS
IN MODEL SC-6
1.0
0810
(1552)
FAB SC-6
X-RAY WELDS
ON MODEL SC-6
0.6
0820
(1473-4)
FAB SC-6
PERFORM HELI
LEAK TEST
ON MODEL SC
0.6
6V,C 0410
(1485-2)
FAB SC-5
MAKE
CUTOUTS FOR
PENETRATIONS
1.0
0730
(1485-2)
FAB SC-5
DELIVER MODELS
SC-5 & SC-6
TO 1473-4
0.2
0740
(1473-4)
FAB SC-5
INSTALL
PENETRATIONS
IN MODEL SC-5
1.0
0750
(1552)
FAB SC-5
X-RAY WELDS
ON MODEL SC-5
0.6
0760
(1552)
FAB SC-5
PERFORM HELIUM
LEAK TEST
ON MODEL SC-5
0.6
0770
(1473-4)
FAB SC-5
DELIVER MODEL SC-5 TO 1472-2
0.2
(1472-2)
FAB SC-5
ATTACH STIFFENING
RINGS & GUSSETS TO MODEL SC-5
2.0
3V,C 0535
0745
0746
(1473-4)
FAB SC-5
WELD RINGS
0.6
0755
(1485-
FAB SC
PERFORM DIME
INSPECT
OF MODEL
0.6
8Z
7Z
6Z
5Z

ISSUE	DESCRIPTION		DATE
0	ORIGINAL ISSUE	RRD	12-4-81
1	REV & UPDATE	RRD	



Sandia

1047.011

ISSUE	0	1

SMC MOD-FAB

SIZE	CODE IDENT NO.	DWG NUMBER
D	14213	PART 3 OF 3 (X,Y,Z)

SCALE SHEET OF

4Z 3Z 2Z 1Z

LAYIN

SURVEY OF EXISTING FACILITIES

REACTOR CONTAINMENTS

AND

CONTAINMENT LIKE STRUCTURES

AL DENNIS

PRESENTED TO ACRS SUBCOMMITTEE
ALBUQUERQUE, NM
MARCH 22, 1982

REACTOR CONTAINMENTS

(POWER AND RESEARCH REACTORS)

NOT IN USE

DECOMMISSIONED

MOTHBALLED

ENTOMBED

NOT IN USE

INDIAN POINT 1
HUMBOLT BAY
DRESDEN 1

SHUT DOWN INDEFINITELY.
FUTURE UNCERTAIN

CP-5 REACTOR, ARGONNE
UTREX REACTOR, LOS ALAMOS
PLUTONIUM RECYCLE TEST REACTOR, HANFORD

NOT OPERATIONAL

EXPERIMENTAL GAS COOLED REACTOR FACILITY, OAK RIDGE

NOT PLACED IN SERVICE

DECOMMISSIONED

EXPERIMENTAL BOILING WATER REACTOR,
ARGONNE

MOTHBALLED

- CAROLINAS-VIRGINIA TUBE, PARR, SC
- GE EVESR, ALAMEDA, CA
- FERMI 1, LAGOONA BEACH, MI
- PATHFINDER, SIOUX FALLS, SD
- PEACH BOTTOM, PEACH BOTTOM, PA
- PLUM BROOK, SANDUSKY, OH
- SAXTON, SAXTON, PA
- SEFOR, STRICKLER, AR
- VALLECITOS BOILING WATER REACTOR,
  PLEASANTON, CA
- WESTINGHOUSE TEST REACTOR,
  WALTZ MILL, PA

ENTOMBED

BONUS, RICON, PUERTO RICO

HALLAM, HALLAM, NE

PIQUA, PIQUA, OH

ALL EXISTING REACTOR FACILITIES HAVE BEEN REJECTED FOR ONE OR MORE OF THE FOLLOWING REASONS.

THE REACTOR FACILITY IS UNAVAILABLE FOR DESTRUCTIVE TESTING. IT IS CURRENTLY BEING UTILIZED AS OFFICE AND/OR LABORATORY SPACE.

SANDIA LABORATORIES MUST OBTAIN A MODIFIED LICENSE FOR THE FACILITY IN ACCORDANCE WITH 10 CFR 30 OR 10 CFR 50, DEPENDING ON THE FACILITY'S CURRENT LICENSE. BOTH AN ENVIRONMENTAL IMPACT STATEMENT AND PUBLIC HEARINGS WOULD BE REQUIRED.

THE FACILITY WOULD REQUIRE EXTENSIVE REPAIR. THE COSTS OF SUCH REPAIRS WOULD SUBSTANTIALLY EXCEED THE BUDGET FOR MODEL FABRICATION BASED ON THE COST OF AN INDIVIDUAL 1/10 SCALE MODEL.

CONTAINMENT LIKE STRUCTURES

BUILDINGS, DOMES, AND IGLOOS

SURPLUS PRESSURE VESSELS AND TANKS

BUILDINGS, DOMES,
AND
IGLOOS

CANDIDATE STRUCTURES WERE LOCATED AT

HANFORD, WA
NEVADA TEST SITE
WHITE SANDS, NM

NONE OF THESE STRUCTURES WAS DESIGNED FOR INTERNAL PRESSURE LOADING , NOR ARE THEY PRESSURE TIGHT.

SURPLUS PRESSURE VESSELS
AND
TANKS

NO SUITABLE UNITS HAVE BEEN LOCATED

SUMMARY

USE OF EXISTING STRUCTURES DOES NOT APPEAR TO BE ADVANTAGEOUS. BOTH DOLLAR COSTS AND TIME REQUIREMENTS APPEAR TO BE EXCESSIVE WHEN COMPARED TO USE OF MODELS.

LAYIN

CONTAINMENT MODELS

AL DENNIS

PRESENTED TO ACRS SUBCOMMITTEE
ALBUQUERQUE, NM
MARCH 22, 1982

OBJECTIVE

THE GOAL OF THIS PROGRAM IS TO ESTABLISH THE CREDIBILITY OF POST-YIELD STRUCTURAL PREDICTIONS OBTAINED THROUGH THE USE OF AVAILABLE NUMERICAL METHODS.

HYPOTHESIS

DATA, SUFFICIENT TO ESTABLISH THE CREDIBILITY OF AVAILABLE NUMERICAL METHODS IN THE POST-YIELD RANGE, CAN BE OBTAINED FROM A LIMITED NUMBER OF EXPERIMENTS PERFORMED ON SMALL PROTO-TYPICAL STRUCTURES.

SMALL PROTOTYPICAL STRUCTURE

A SMALL PROTOTYPICAL STRUCTURE IS A STRUCTURE WHICH HAS THE SAME STRUCTURAL CHARACTERISTICS AS THE PROTOTYPE AND WHICH UTILIZES SIMILAR MATERIALS TO THOSE USED IN THE PROTOTYPE, BUT WHOSE STRUCTURAL BEHAVIOUR IS NOT DIRECTLY RELATED TO THE PROTOTYPE BY SIMILITUDE RELATIONSHIPS.

GUIDELINES FOR EXPERIMENTAL PLANNING

EXPERIMENTS SHALL BE PERFORMED ON THE MOST ECONOMICAL SMALL PROTOTYPICAL STRUCTURES COMPATIBLE WITH THE PROGRAM OBJECTIVES

ALL EXPERIMENTAL RESULTS UTILIZED FOR EVALUATION OF AVAILABLE NUMERICAL METHODS SHALL BE SHOWN TO BE REPRODUCIBLE.

AT LEAST ONE EXPERIMENT FOR EACH CONSTRUCTION TYPE SHALL BE PERFORMED USING A PROTOTYPICAL STRUCTURE THAT UTILIZES CONVENTIONAL CONSTRUCTION PRACTICES.

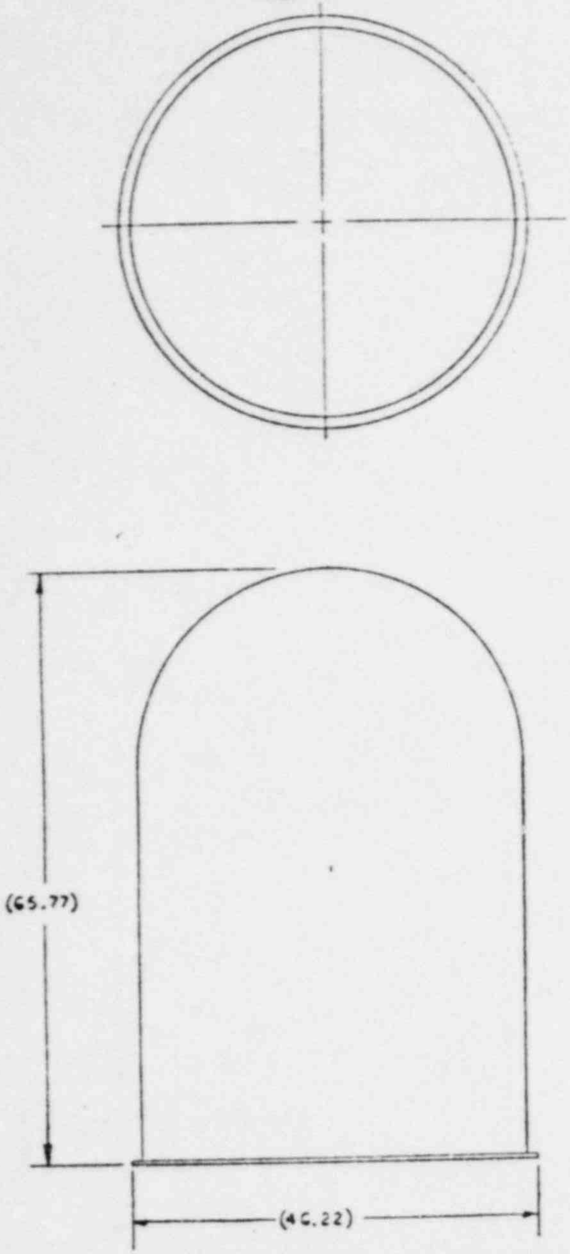
SELECTION OF SIZE OF PROTOTYPICAL STRUCTURE

SIZE SELECTION WAS BASED UPON A STUDY PREPARED BY SOUTHWEST RESEARCH INSTITUTE UNDER CONTRACT TO SANDIA NATIONAL LABORATORIES.

CONTAINMENT CONSTRUCTION	MINIMUM SIZE FOR NEEDED DATA	MINIMUM SIZE FOR CONVENTIONAL CONSTRUCTION
STEEL	1/32	1/10
REINFORCED CONCRETE	1/16	1/10

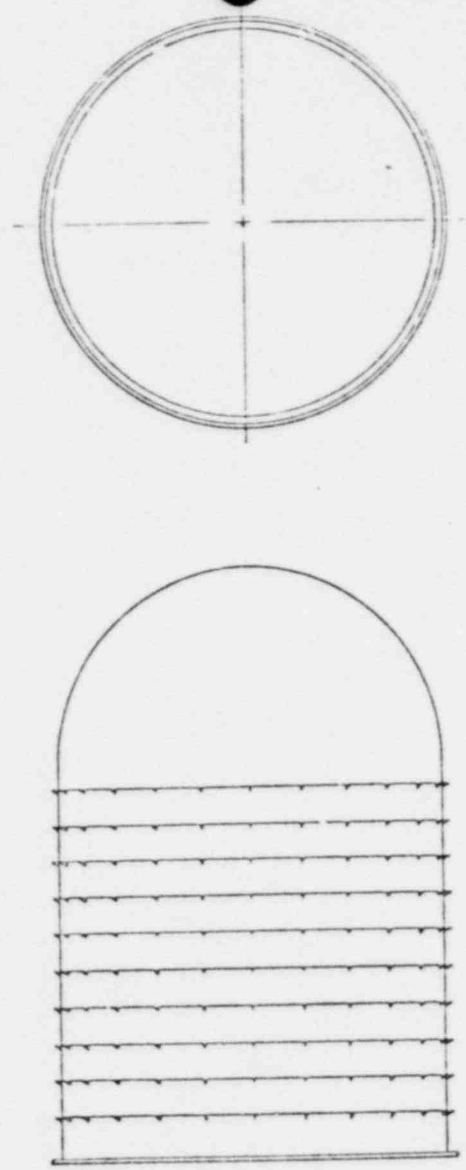
STEEL CONTAINMENT MODELS

EXPERIMENT	SIZE	DESCRIPTION
SC-1 SC-2	1/32	• CLEAN SHELL EXPERIMENT • SERVES AS THE "CONTROL" • PROVIDES DATA FOR BASIC 2-D POST-YIELD METHOD EVALUATION
SC-3 SC-4	1/32	• RING STIFFENED SHELL • PROVIDES ADDITIONAL POST-YIELD METHOD EVALUATION DATA • STRUCTURAL EFFECTS DATA
SC-5 SC-6	1/32	• RING STIFFENED SHELL WITH PRIMARY PENETRATIONS • PROVIDES DATA FOR 3-D POST-YIELD METHOD EVALUATION
SC-7	1/10	• RING STIFFENED SHELL WITH PENETRATIONS • UTILIZES CONVENTIONAL CONSTRUCTION METHODS



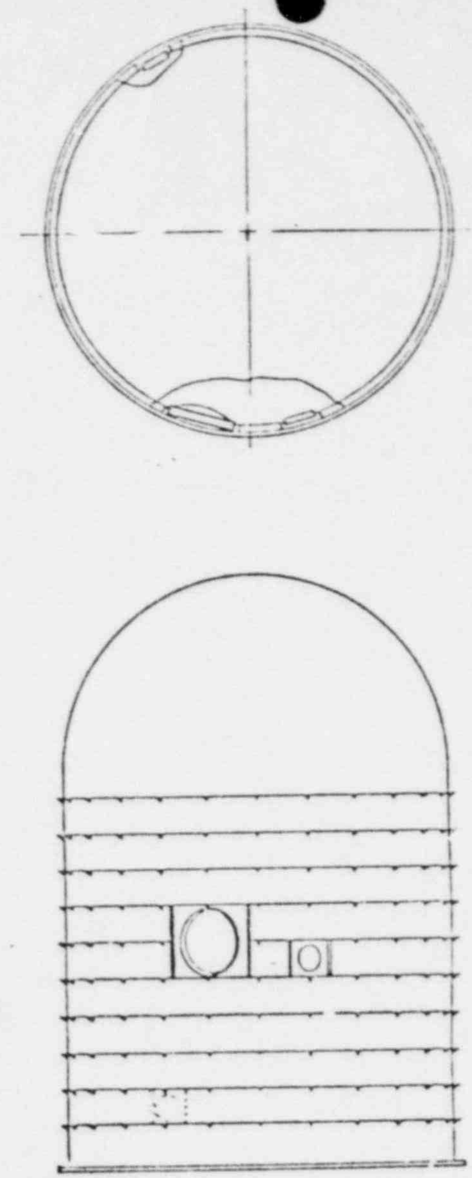
CLEAN SHEEL

SC 1 & 2



RING REINFORCED SHELL

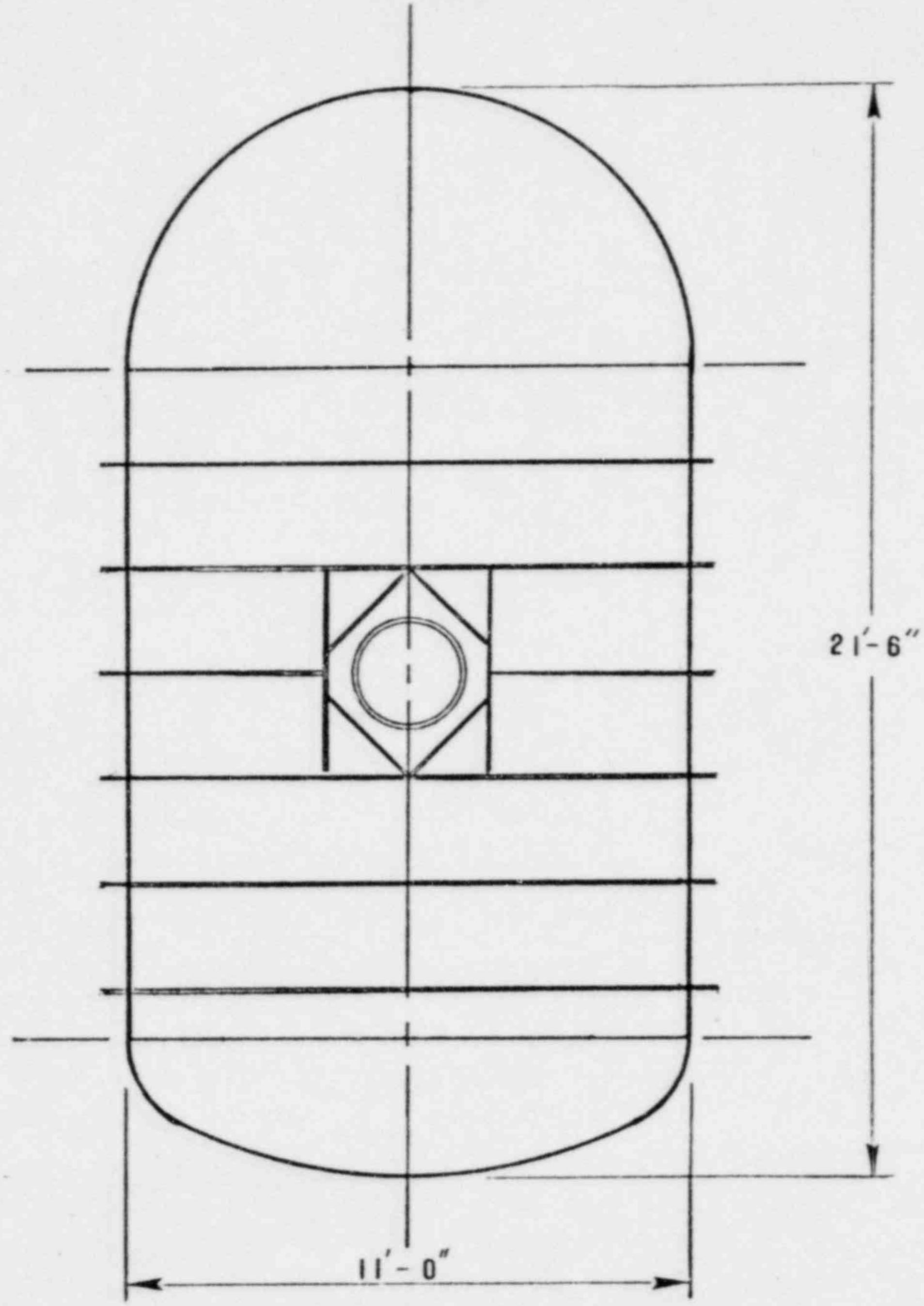
SC 3 & 4



RING REINFORCED SHELL WITH PENETRATIONS

SC 5 & 6

MODEL SC 7



3-18-82

REINFORCED CONCRETE STEEL LINED CONTAINMENT MODELS

EXPERIMENT	SIZE	DESCRIPTION
RCC-1 RCC-2	1/10	• NO SEISMIC STEEL • NO PENETRATIONS • THESE EXPERIMENTS WILL SERVE AS THE "CONTROL" AND WILL PROVIDE DATA FOR BASIC 2-D POST-YIELD METHOD EVALUATION
RCC-3 RCC-4	1/10	• SEISMIC STEEL • NO PENETRATIONS • THESE EXPERIMENTS WILL PROVIDE ADDITIONAL 2-D DATA AND STRUCTURAL EFFECTS DATA
RCC-5 RCC-6	1/10	• SEISMIC STEEL • PRIMARY PENETRATIONS • THESE EXPERIMENTS WILL PROVIDE DATA FOR 3-D METHOD EVALUATION

EXPECTED RESULTS

COMPLETION OF	CONTAINMENT CONSTRUCTION TYPE		
	STEEL	REINFORCED CONCRETE	PRESTRESSED CONCRFTE *
EXPERIMENTAL PROGRAM	FY83	FY84	FY83
EVALUATION OF NUMERICAL METHODS	FY84	FY85	FY84

* CONTINGENT UPON OBTAINING DATA FROM CANADA AND THE UNITED KINGDOM

Layin

SAFETY MARGINS OF CONTAINMENTS

ANALYSIS OF CONTAINMENT MODELS

TOM BLEJWAS
DAN HORSCHEL

OBJECTIVE:

QUALIFY METHODS OF EVALUATING ULTIMATE CAPACITY

PROCEDURE:

1. DESIGN AND BUILD EXPERIMENTAL MODELS
2. ANALYIZE MODELS (PRE-TEST)
3. CONDUCT EXPERIMENTS
4. COMPARE RESULTS
5. REFINE ANALYSES
6. PRESENT COMPARISONS

CONTAINMENT TYPES:

- STEEL
- REINFORCED CONCRETE
- PRESTRESSED CONCRETE

LOADINGS:

- STATIC INTERNAL PRESSURIZATION
- DYNAMIC INTERNAL PRESSURIZATION
- SEISMIC

CURRENT ANALYTICAL EFFORTS

STEEL CONTAINMENTS - 2D

STEEL CONTAINMENTS - 3D

CONCRETE CONTAINMENTS

SUPPORT STRUCTURES

DESIRABLE CODE FEATURES

1. ELASTO-PLASTIC MATERIAL
2. STATIC AND DYNAMIC LOADINGS
3. SURFACE AND POINT LOADS
4. LARGE DISPLACEMENT, LARGE STRAIN PLASTICITY
5. AXISYMMETRIC AND 3D SHELL AND SOLID ELEMENTS
6. FULLY OPERATIONAL ON CDC
7. AUTOMATIC MESH GENERATION
8. GRAPHICAL OUTPUT
9. EXTENSIVE DIAGNOSTICS AND DOCUMENTATION

POSSIBLE CODES

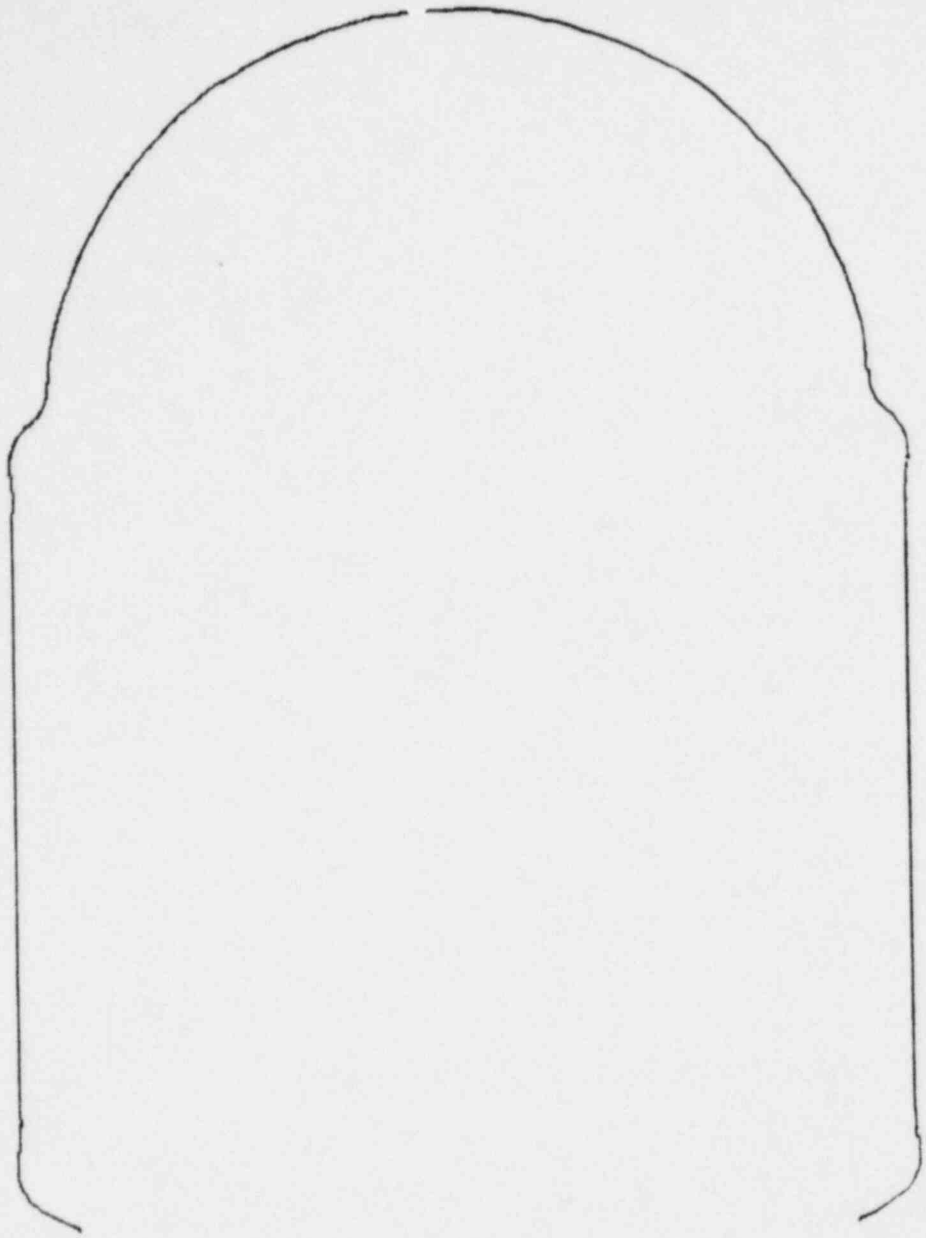
ADINA	
ANSYS	
CEASEMT	FRENCH CODE, NOT ON CDC
CNATS	ONLY AXISYMMETRIC
	NO DIAGNOSTICS
	STATIC LOADING ONLY
HONDO	NO POINT LOADS
	ONLY AXISYMMETRIC ELEMENTS
	MINIMAL DIAGNOSTICS
MARC	
NISA	NO DOCUMENTATION OF THEORY
PAM-NL	NO DOCUMENTATION OF THEORY
	NO SAMPLE PROBLEMS
	FRENCH CODE
UNIKA	NO SURFACE LOADS
	NOT ON CDC
	GERMAN CODE
ABAQUS	

STEEL CONTAINMENTS

CLEAN SHELL

RING STIFFENED SHELL

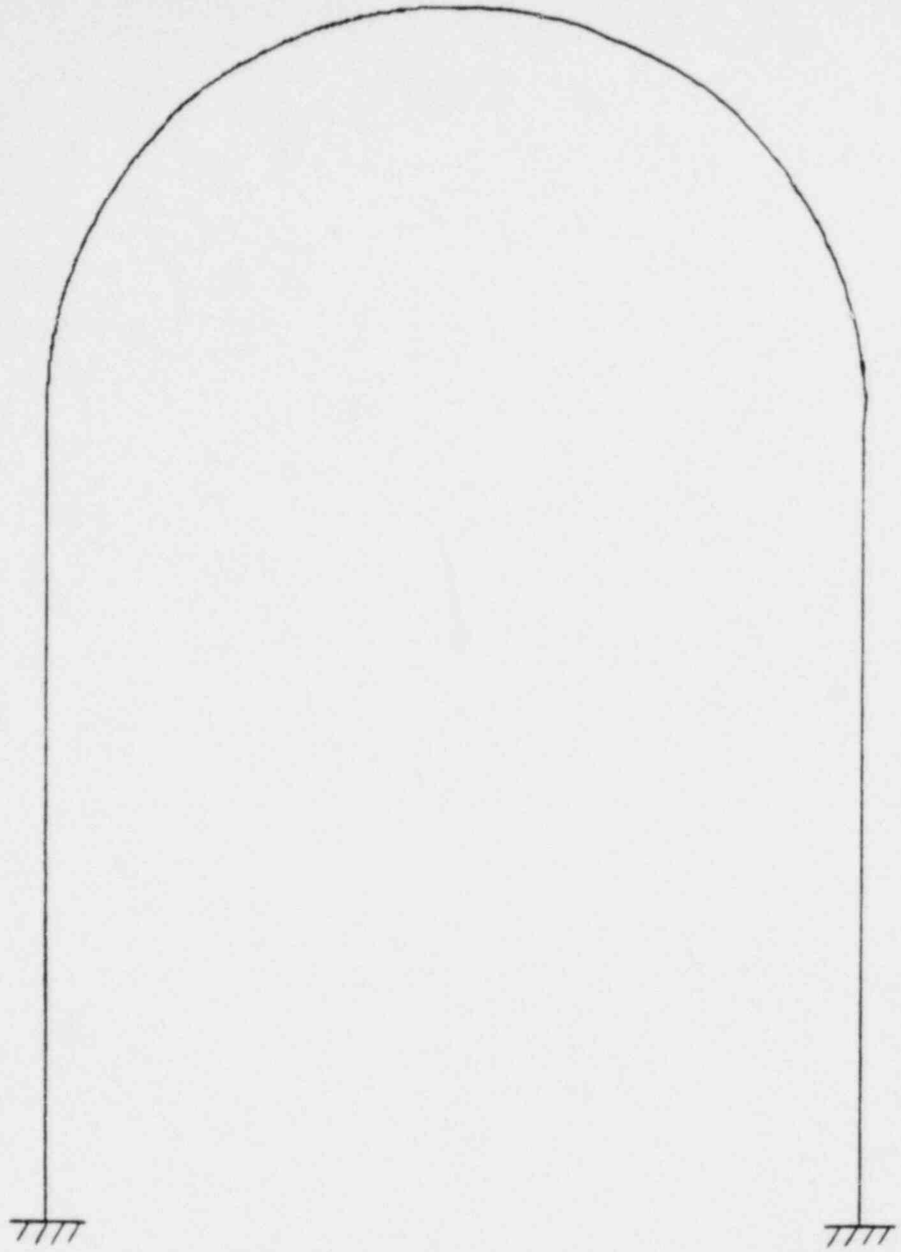
PENETRATIONS



CLEAN SHELL

YIELD PRESSURE = 64 PSIG

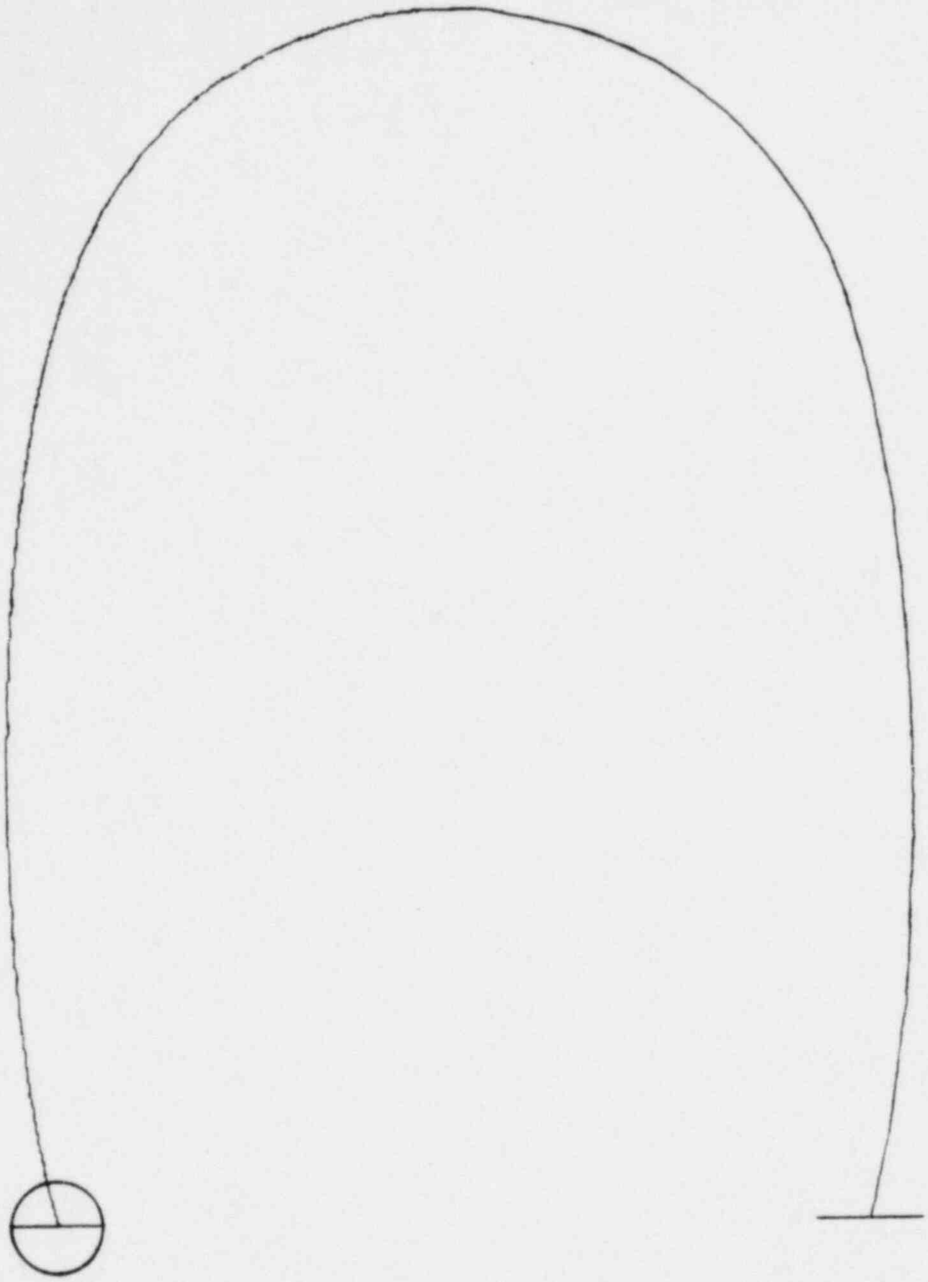
MAG. FACTOR = 160



CLEAN SHELL

YIELD PRESSURE = 64 PSIG

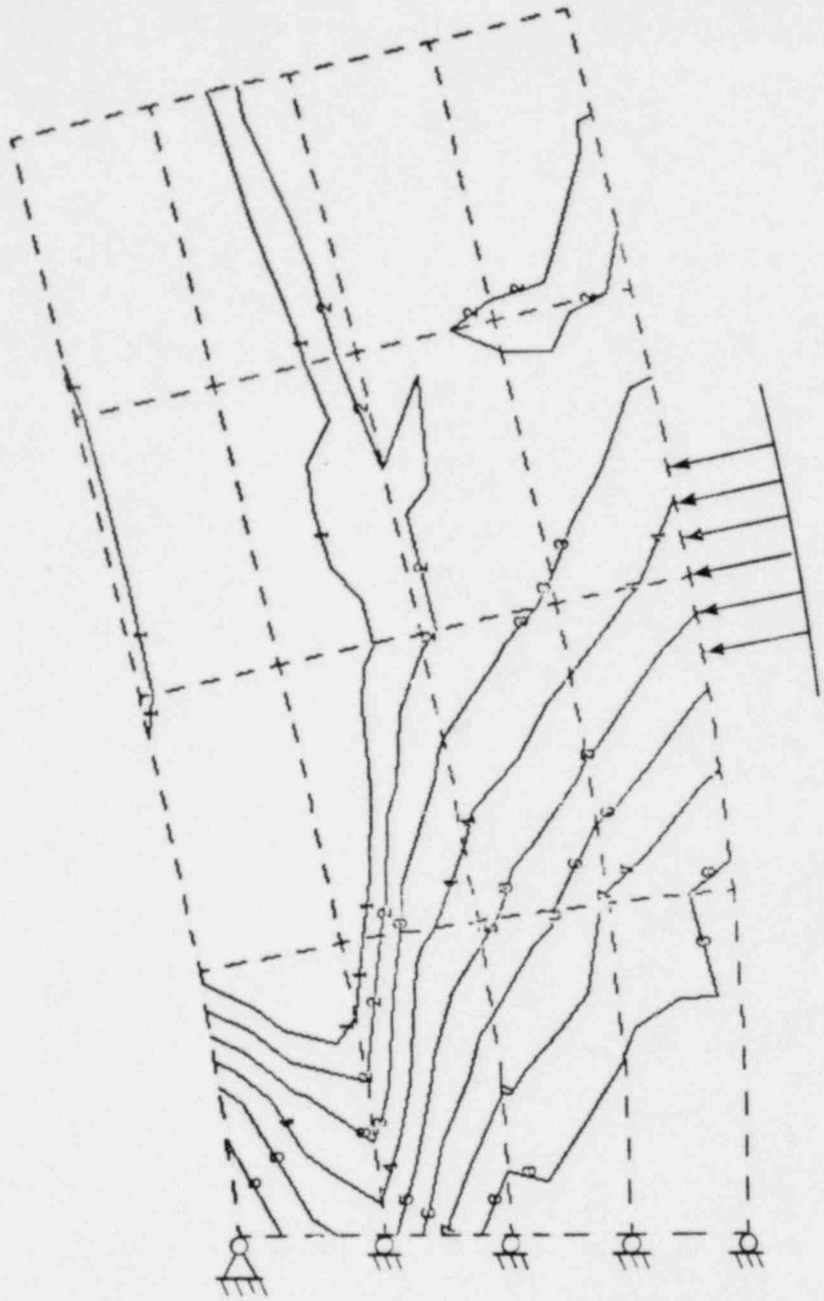
MAG. FACTOR = 1



CLEAN SHELL

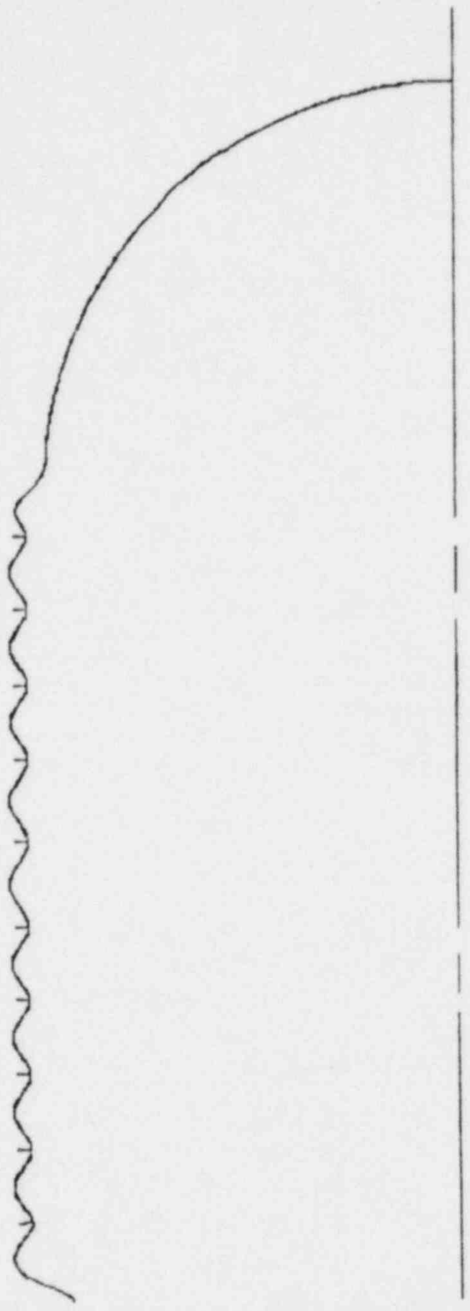
PRESSURE = 179 PSIG

MAG. FACTOR = 1



CLEAN SHELL

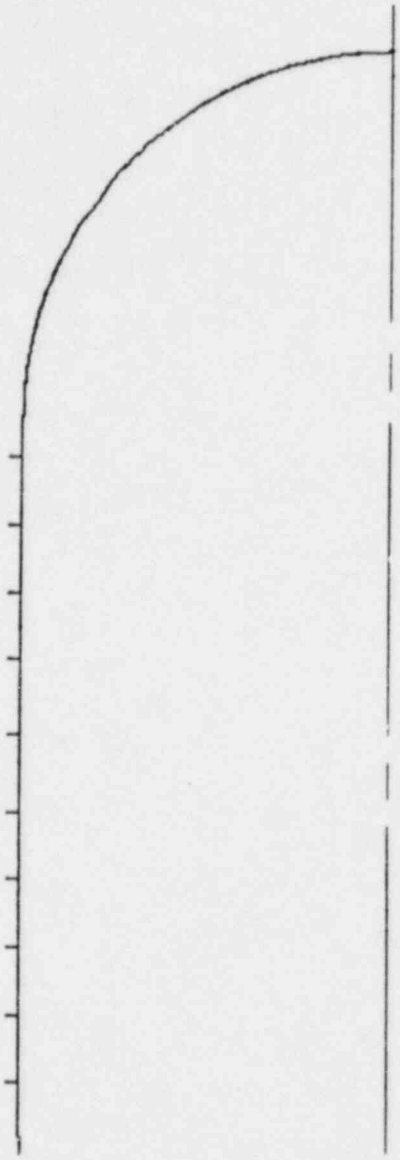
DEFORMED SHAPE AND MISES STRESS CONTOURS AT BASE CONNECTION



RING STIFFENED SHELL

DEFORMED SHAPE AT FIRST YIELD

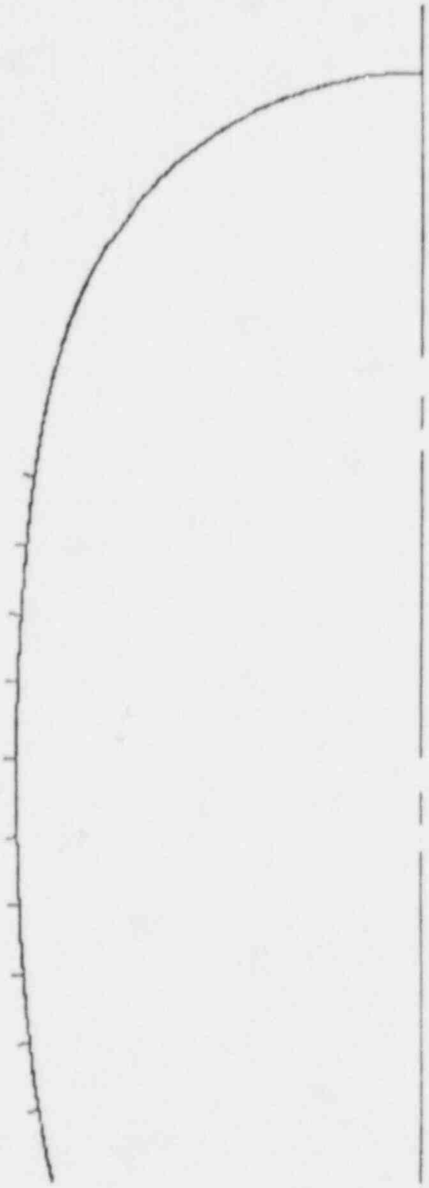
P = 105 PSIG   MAG. FACTOR = 106



RING STIFFENED SHELL

DEFORMED SHAPE AT FIRST YIELD

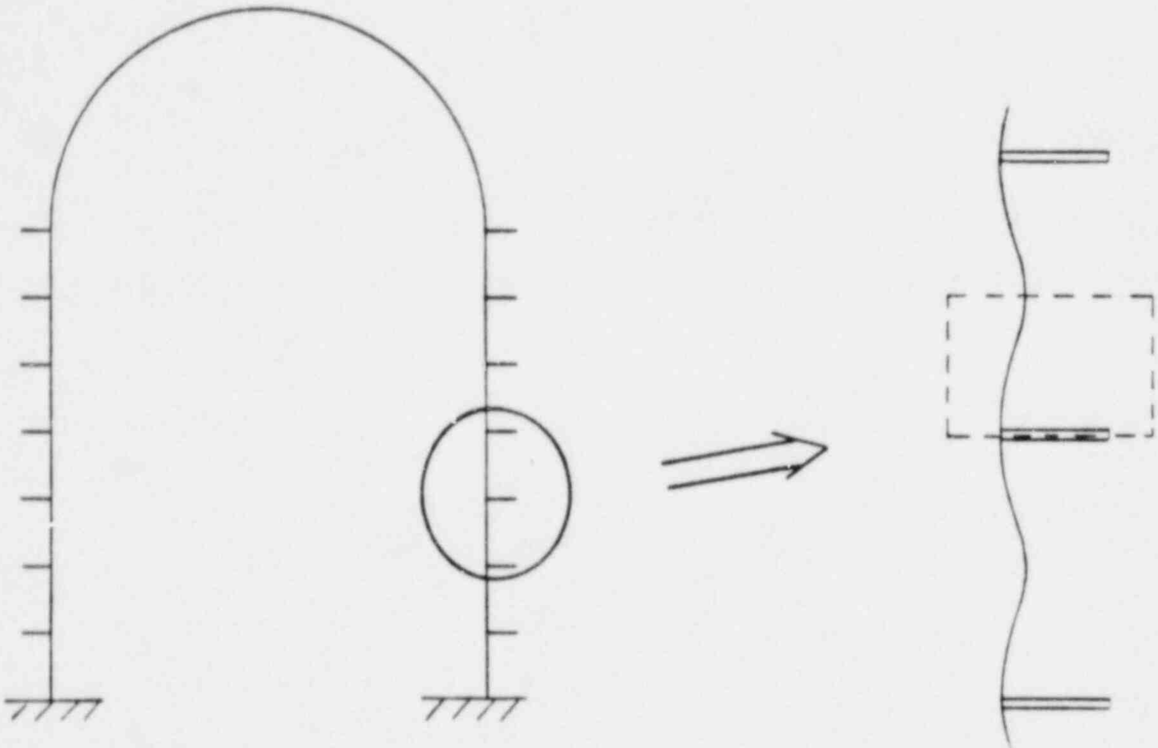
P = 105 PSIG   MAG. FACTOR = 1



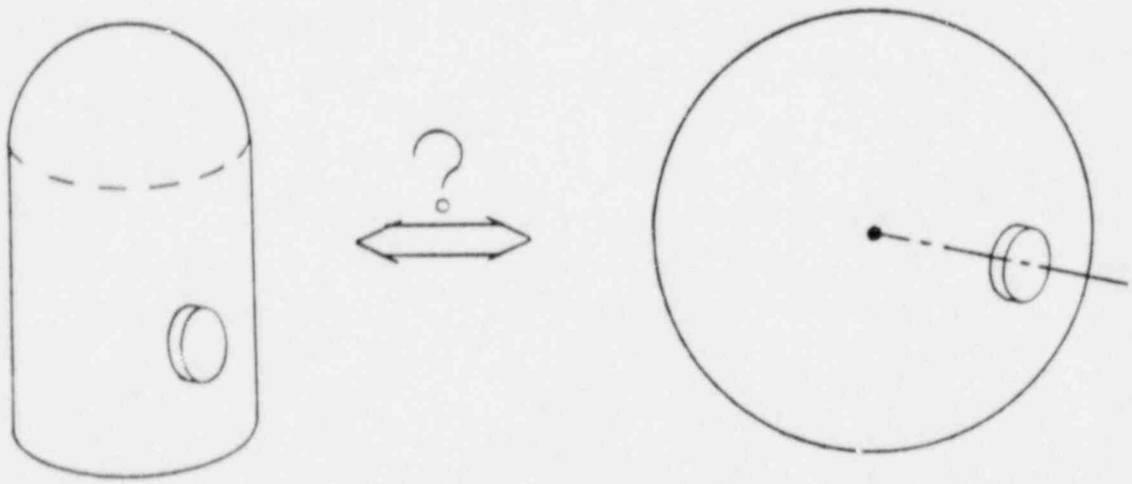
RING STIFFENED SHELL

DEFORMED SHAPE AT 199 PSIG

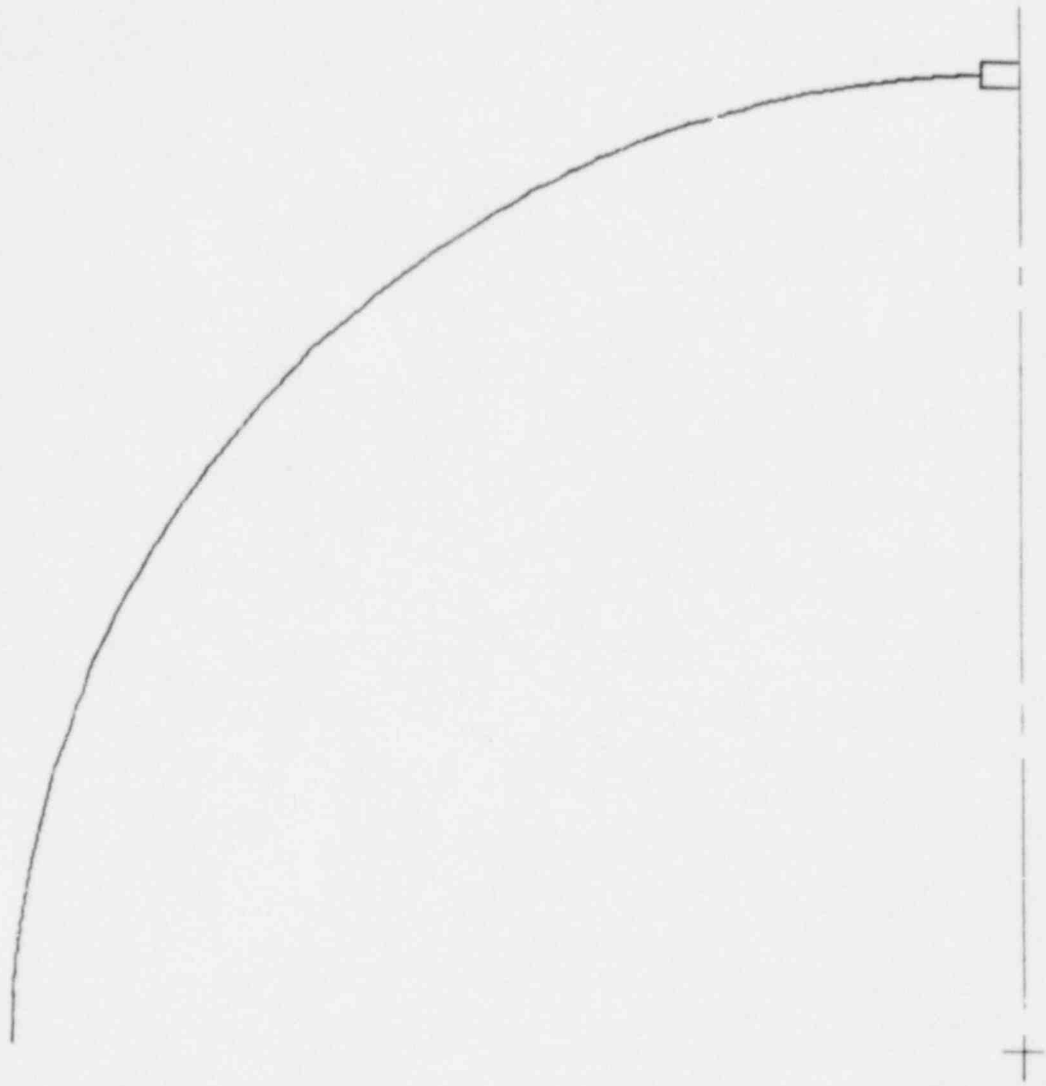
MAG. FACTOR = 1



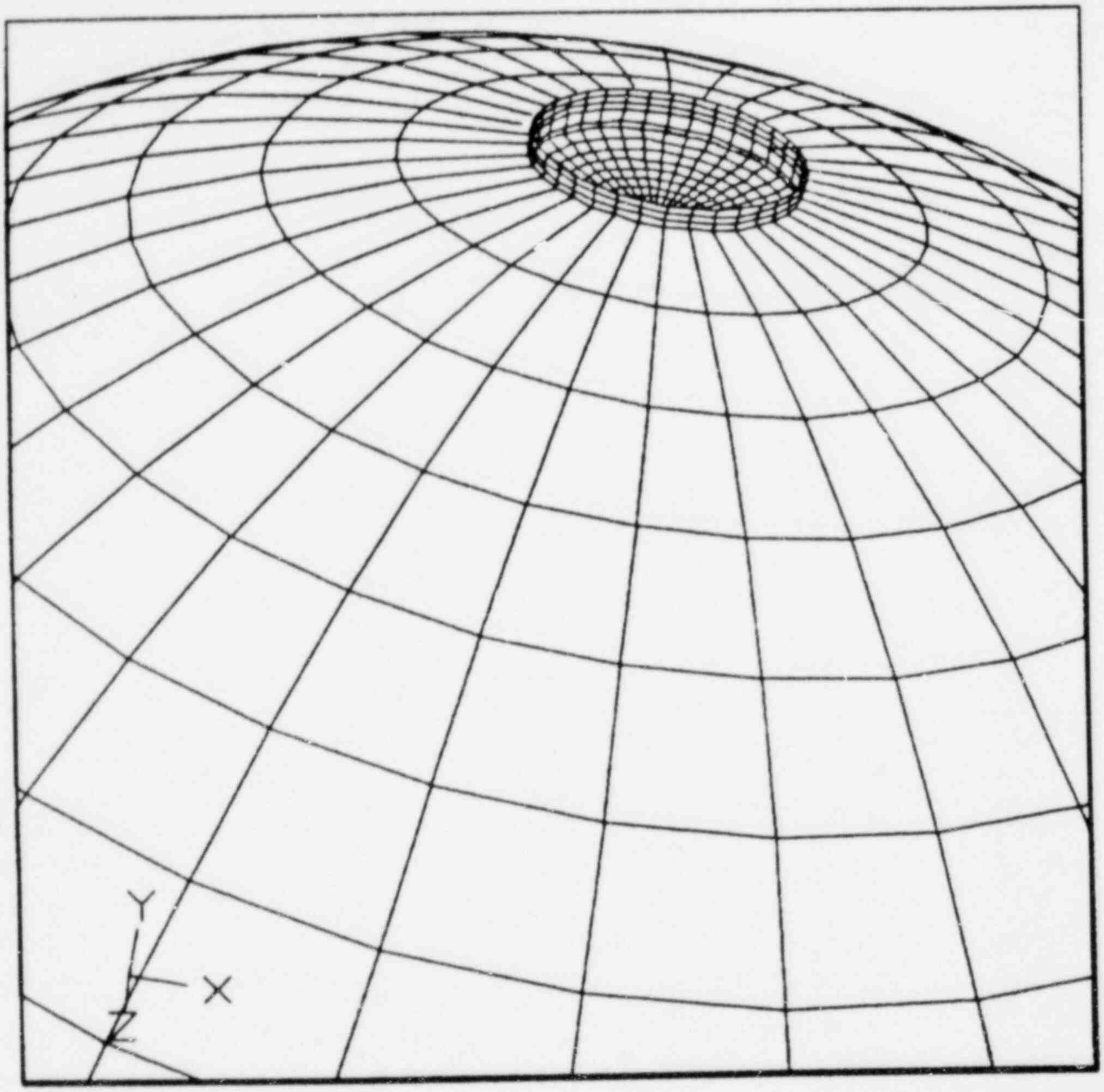
INFINITE SHELL APPROXIMATION



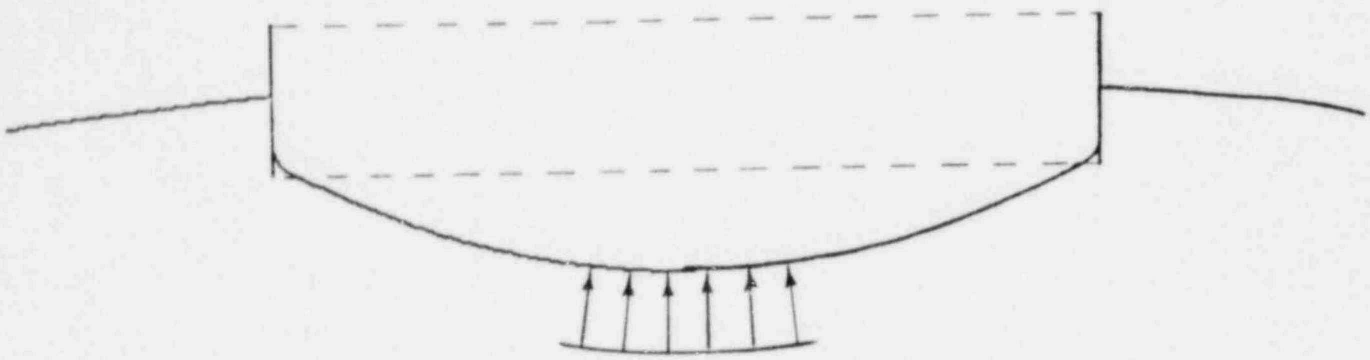
PENETRATIONS IN CYLINDER AND SPHERE



PERSONNEL LOCK IN A
SPHERICAL SHELL



EQUIPMENT HATCH WITH
TOROSPHERICAL CAP



EQUIPMENT HATCH WITH
TOROSPHERICAL CAP

WHAT WILL HAPPEN?

CLEAN SHELL

- MERIDIONAL TEAR AT MID-HEIGHT
- PROBABLY NEAR WELD
- PRESSURE SIGNIFICANTLY HIGHER THAN FIRST-YIELD

RING STIFFENED SHELL

- SIMILAR FAILURE AT SLIGHTLY HIGHER PRESSURE THAN CLEAN SHELL
- RINGS WILL GO PLASTIC

RING STIFFENED SHELL WITH PENETRATIONS

- TO BE DETERMINED

CONTINUING/PLANNED EFFORTS

3-D ANALYSIS OF PENETRATIONS

UPDATE OF MATERIAL PROPERTIES

ANALYSIS WITH OTHER CODES

ANALYSIS OF CONCRETE CONTAINMENTS

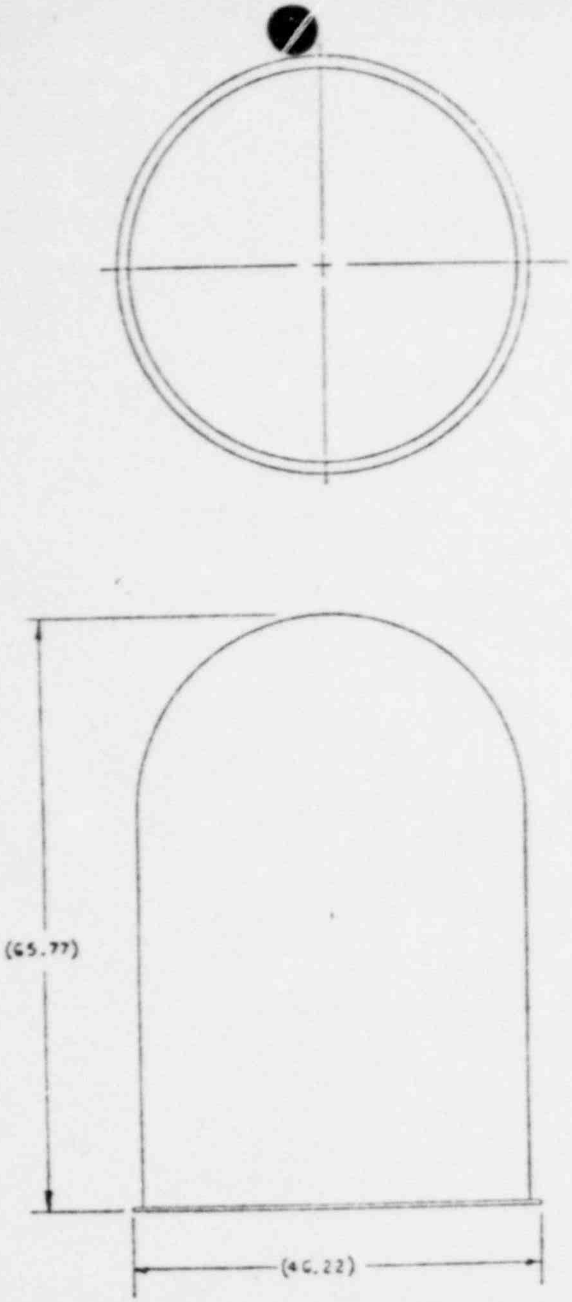
DYNAMIC ANALYSIS

LAYIN

SAFETY MARGINS OF CONTAINMENTS

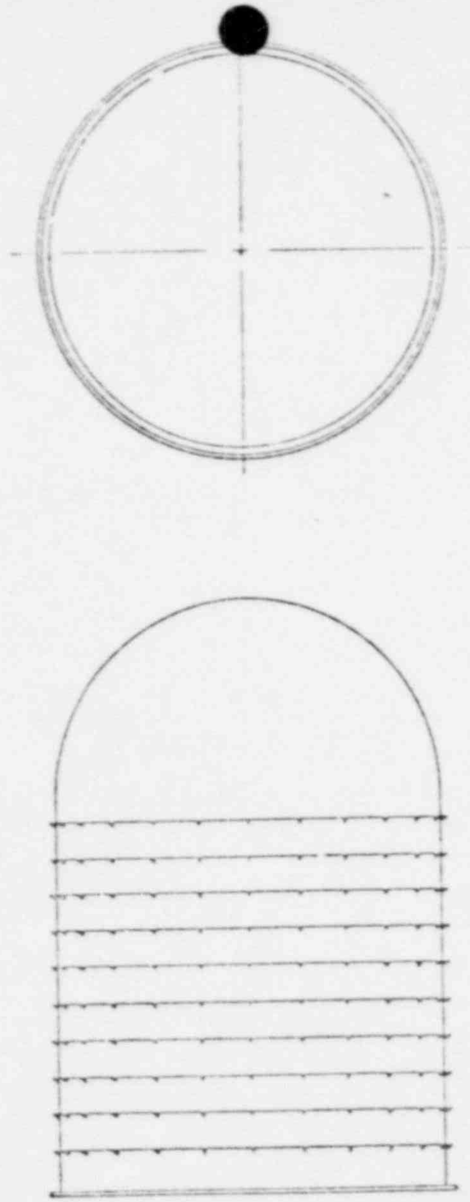
EXPERIMENTAL PROGRAM SMALL SCALE STEEL MODELS

RON WOODFIN



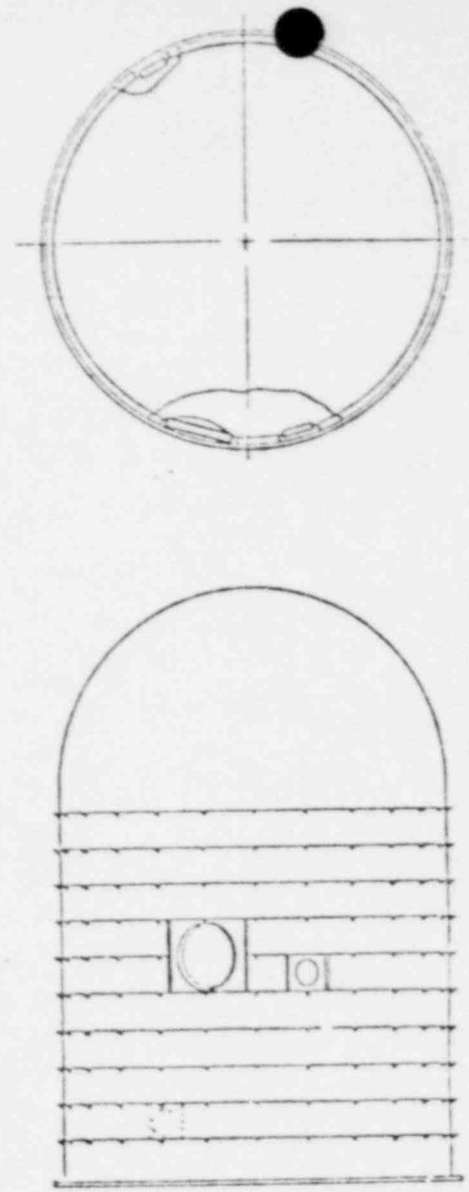
CLEAN SHELL

SC 1 & 2



RING REINFORCED SHELL

SC 3 & 4



RING REINFORCED SHELL WITH PENETRATIONS

SC 5 & 6

TECHNIQUES

- **REMOTE**
  - OUTDOOR
- **COMPUTER CONTROLLED**
  - PRESSURE CONTROL
  - DATA ACQUISITION
- **INTERACTIVE**
  - LOAD STEPS DEPEND ON MEASURED DATA

LOADING

- **PNEUMATIC**
  - BOTTLED N_2
- **QUASISTATIC**
  - STEPWISE INCREASES
- **MONOTONIC**
  - POSSIBLE VARIATIONS
- **FAILURE**

DATA SYSTEM

- STRUCTURAL DATA
  - STRAINS
  - DISPLACEMENTS
- AUXILIARY DATA
  - PRESSURE
  - TEMPERATURE
- TWO TRAILERS
  - SIGNAL CONDITIONING
  - COMMAND

STRUCTURAL DATA

- **STRAINS**
  - PRIMARY IN ELASTIC AND LOW PLASTIC REGIME
  - UP TO 20%
  - PRELIMINARY WORK TO QUALIFY TECHNIQUE
- **DISPLACEMENTS**
  - PRIMARY IN PLASTIC REGIME
  - THREE METHODS
    - CDS (OPTICAL)
    - LVDT (DIRECT)
    - PHOTOGRAPHIC

STRAIN GAGES

- **GAGES**
  - ANNEALED CONSTANTAN
  - UP TO 20%
  - ROSETTES AND GRADIENT STRIPS
- **ADHESIVES**
  - QUALIFIED
  - TESTS FOR TECHNIQUE
- **CORRECTIONS**
  - NON-LINEAR WHEATSTONE BRIDGE
  - TEMPERATURE
  - CROSSAXIS SENSITIVITY
  - LEADWIRE RESISTANCE

DISPLACEMENTS

- **CDS**

  COORDINATE DETERMINATION SYSTEM

- **LVDT**

  LINEAR VARIABLE DIFFERENTIAL TRANSFORMER

- **PHOTOGRAPHIC**

- **CDS**
  - THEODOLITES
  - MOTION OF POINT
  - RESOLUTION ≤ 0.05 IN
  - ≈ 15 MIN / DATA SET
- **LVDT'S**
  - CONTACT
  - MOTION OF SHELL
  - RESOLUTION ≤ 10^{-6} IN
  - ≈ 1 MIN / DATA SET
- **PHOTOGRAPHIC**
  - SILHOUETTE OF DEFORMED SHELL
  - RESOLUTION ≤ 0.03 IN
  - DATA DELAYED

TRANSDUCER UTILIZATION

- **STRAIN GAGES**
  - 70 TO 150 GAGES
  - MERIDIANS AND PARALLELS
  - SYMMETRY CHECK
- **LVDT'S**
  - 10 TO 25
  - MERIDIANS AND DETAILS
  - SYMMETRY CHECK
- **CDS TARGETS**
  - 30 TO 60
  - MERIDIANS AND DETAILS
- **PHOTOGRAPHIC**
  - ONE MERIDIAN
  - CONTINUOUS SILHOUETTE

AUXILIARY DATA

- **PRESSURE**
  - THREE INDEPENDENT TRANSDUCERS
  - CONFIRMATION OF CONTROL SYSTEM
  - LEAK INFERENCE
- **TEMPERATURE**
  - THERMOCOUPLES
  - RESISTANCE TEMPERATURE DETECTORS
  - CORRECTION OF STRAIN GAGES
  - CONFIRMATION OF CONTROL SYSTEM
  - LEAK INFERENCE

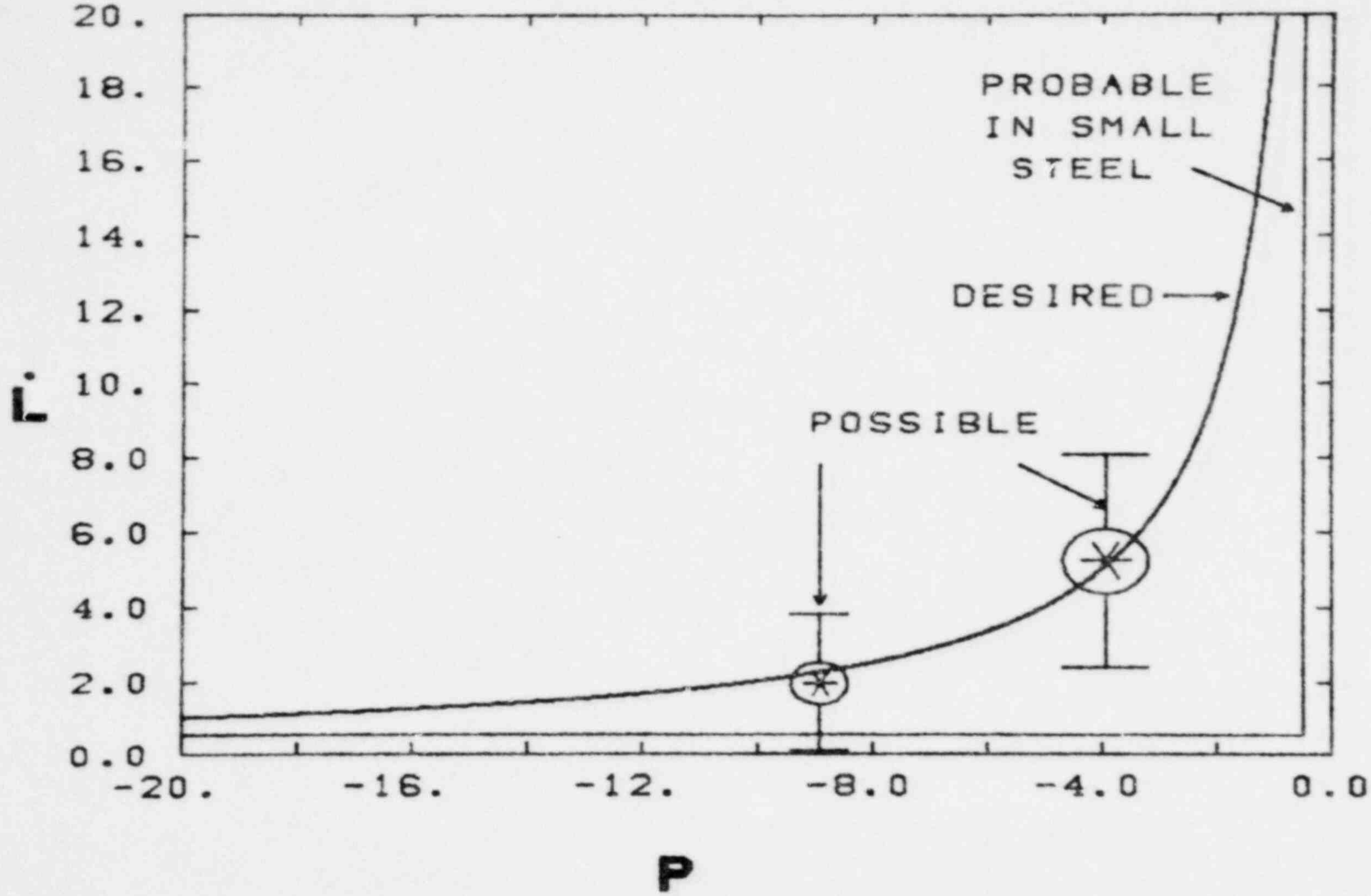
LEAK "MEASUREMENTS"

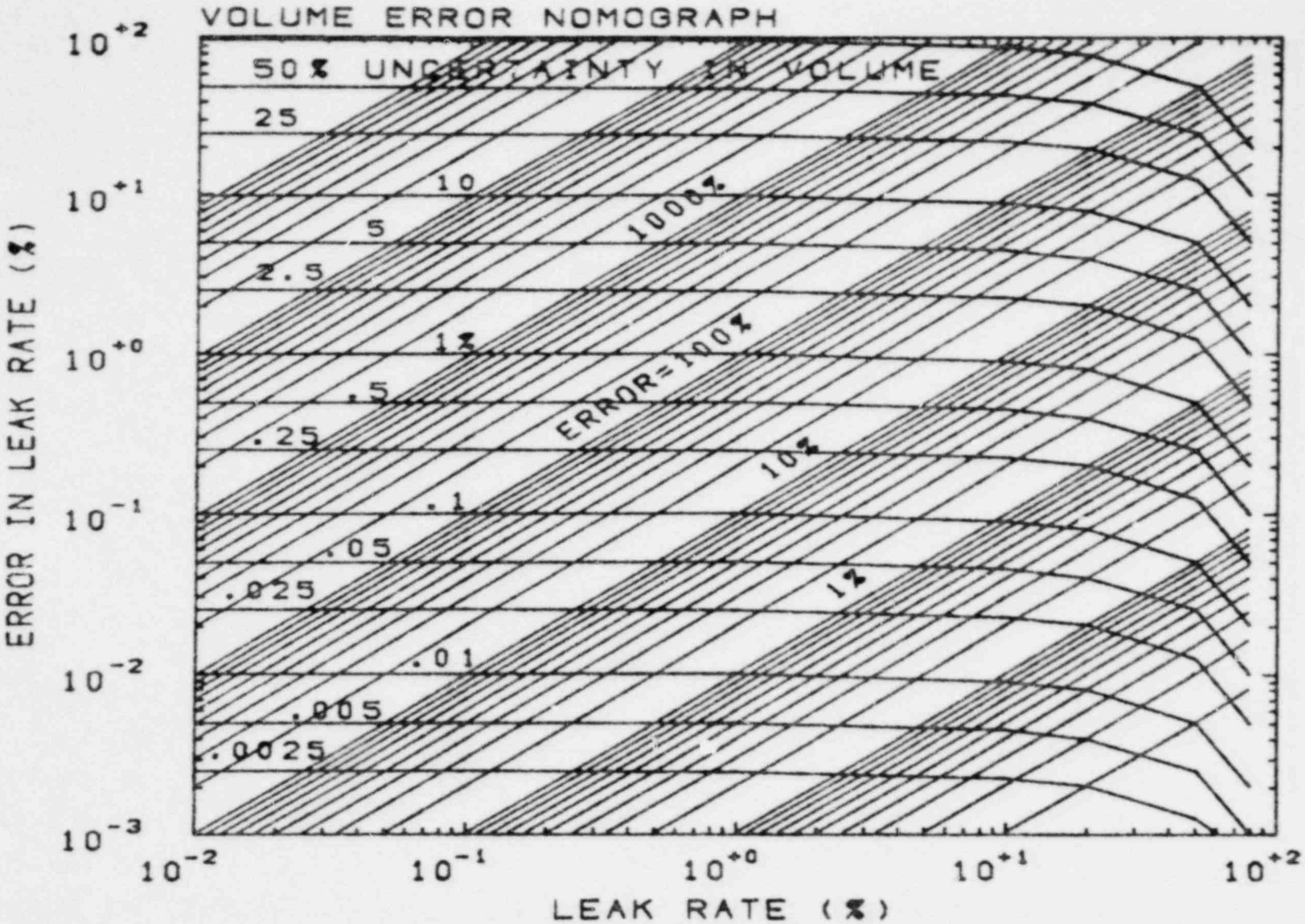
- INFERRED FROM STATE VARIABLES
- NO MODELED LEAK PATHS IN SMALL STEEL MODELS
- DEVELOPING TECHNIQUES
  - DETECT AND LOCATE

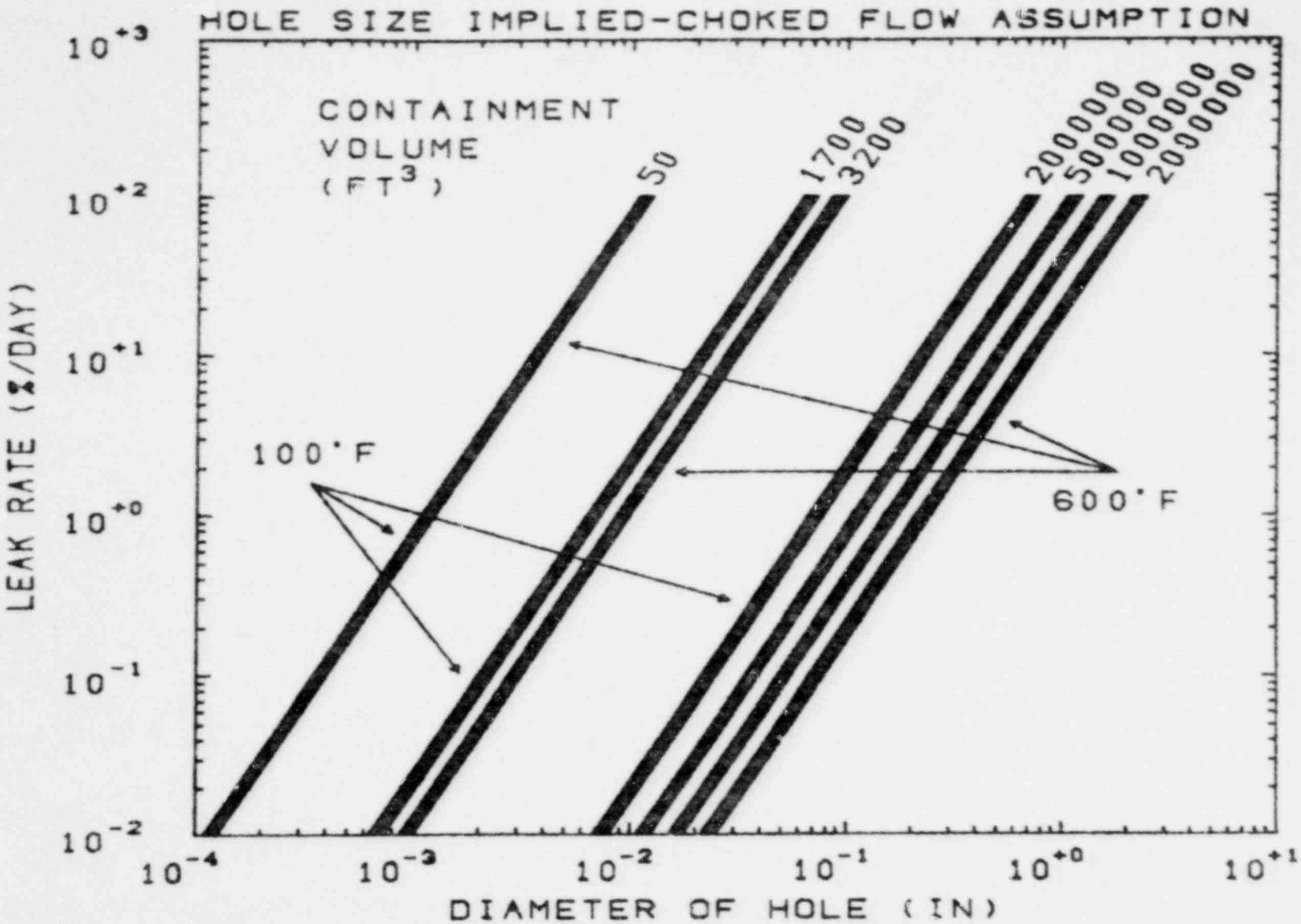
LEAK DETECTION AND LOCALIZATION

- ULTRASONIC
- ACOUSTIC EMISSIONS
- PRESSURE SYSTEM DUTY CYCLE
- IN-FLOW INDICATOR
- GAS CONSUMPTION

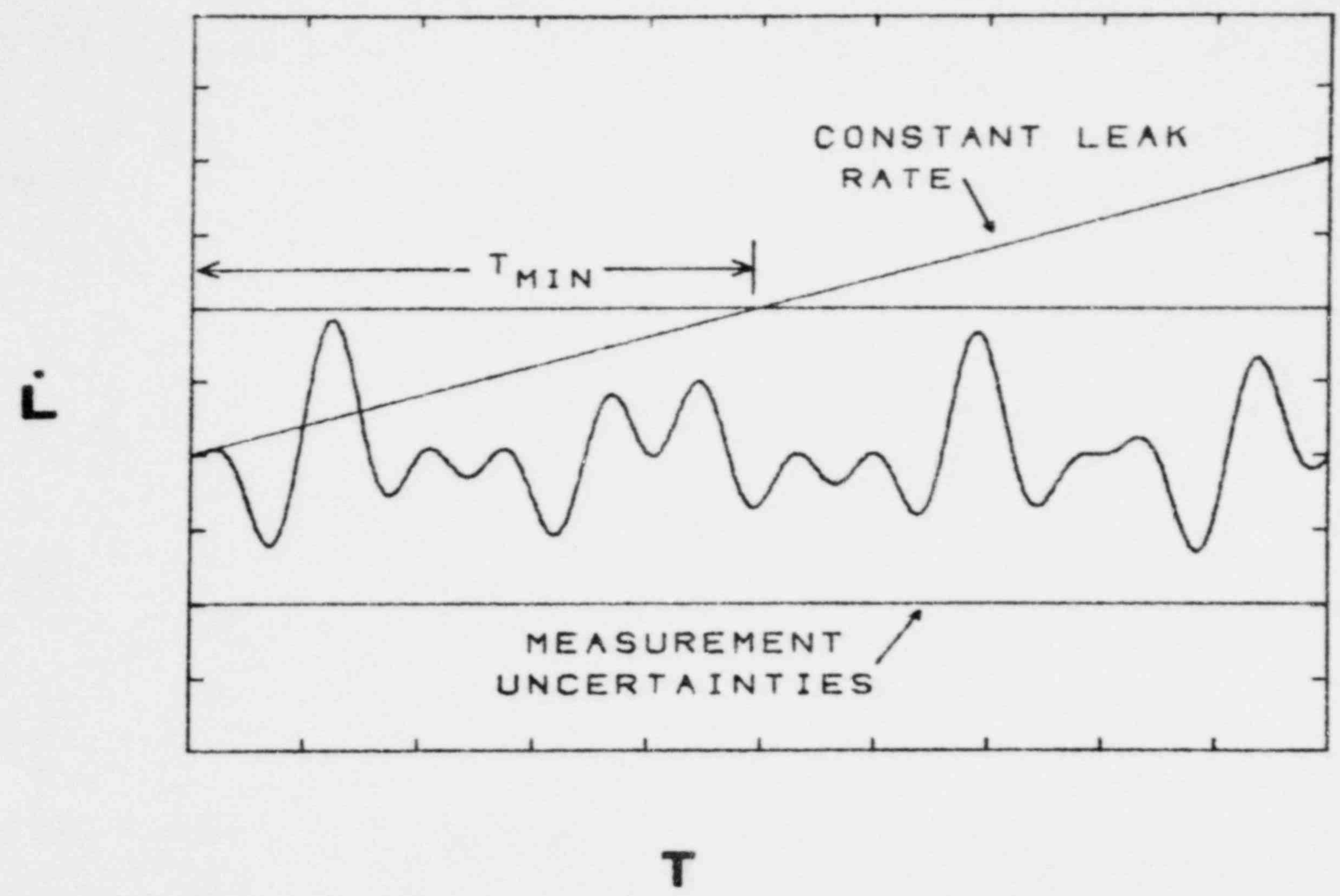
LEAK-PRESSURE FUNCTION



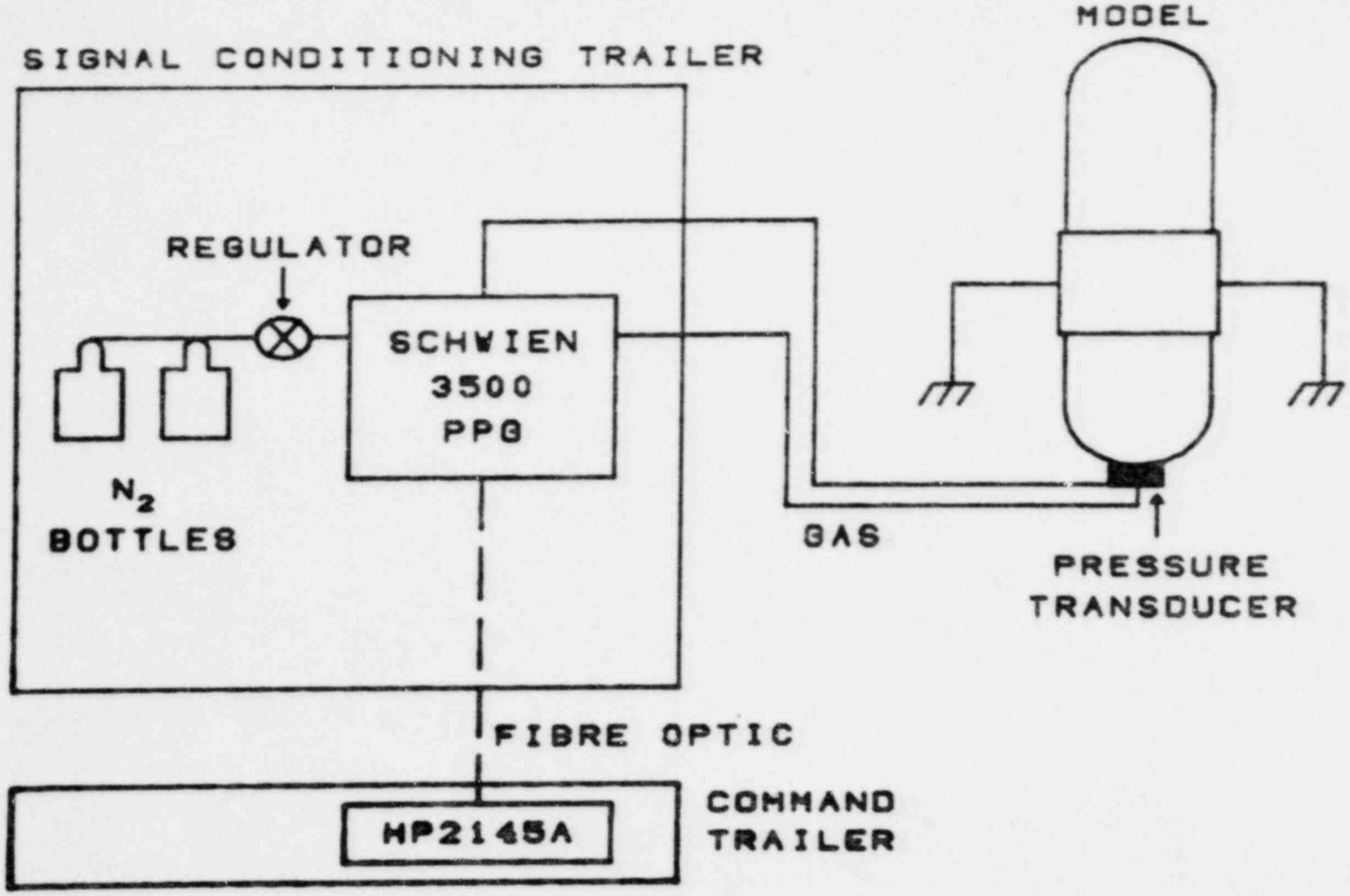

VOLUME ERROR NOMOGRAPH
50% UNCERTAINTY IN VOLUME
25
10
5
2.5
1%
.5
.25
.1
.05
.025
.01
.005
.0025
1000%
ERROR=100%
10%
1%
ERROR IN LEAK RATE (%)
LEAK RATE (%)
10^{+2}
10^{+1}
10^{+0}
10^{-1}
10^{-2}
10^{-3}


HOLE SIZE IMPLIED-CHOKED FLOW ASSUMPTION
CONTAINMENT VOLUME (FT^3)
50
1700
3200
200000
500000
1000000
2000000
100°F
600°F
LEAK RATE (%/DAY)
DIAMETER OF HOLE (IN)
10^-2
10^-1
10^+0
10^+1
10^+2
10^+3
10^-4
10^-3

TIME REQUIRED



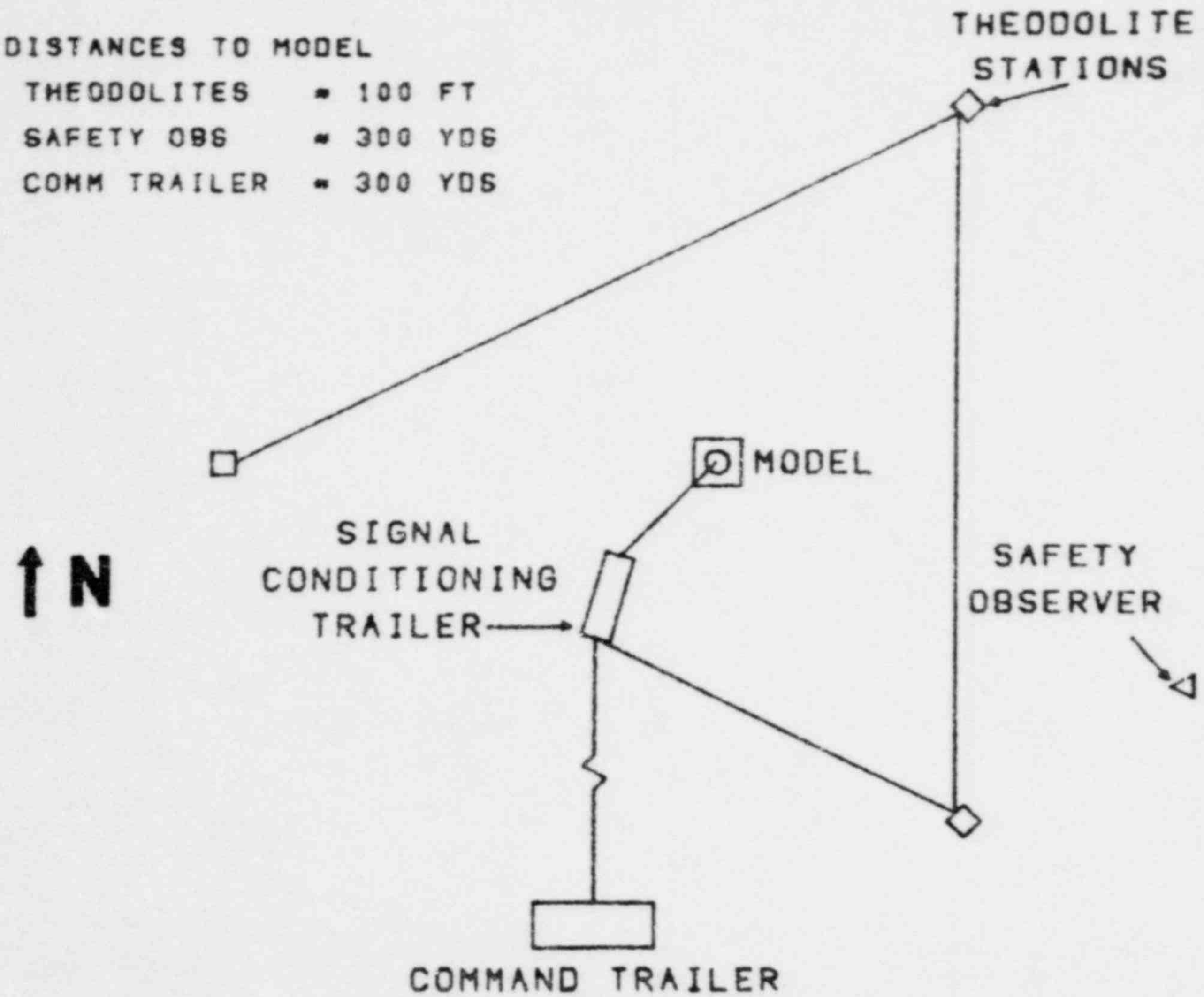
PRESSURE SYSTEM



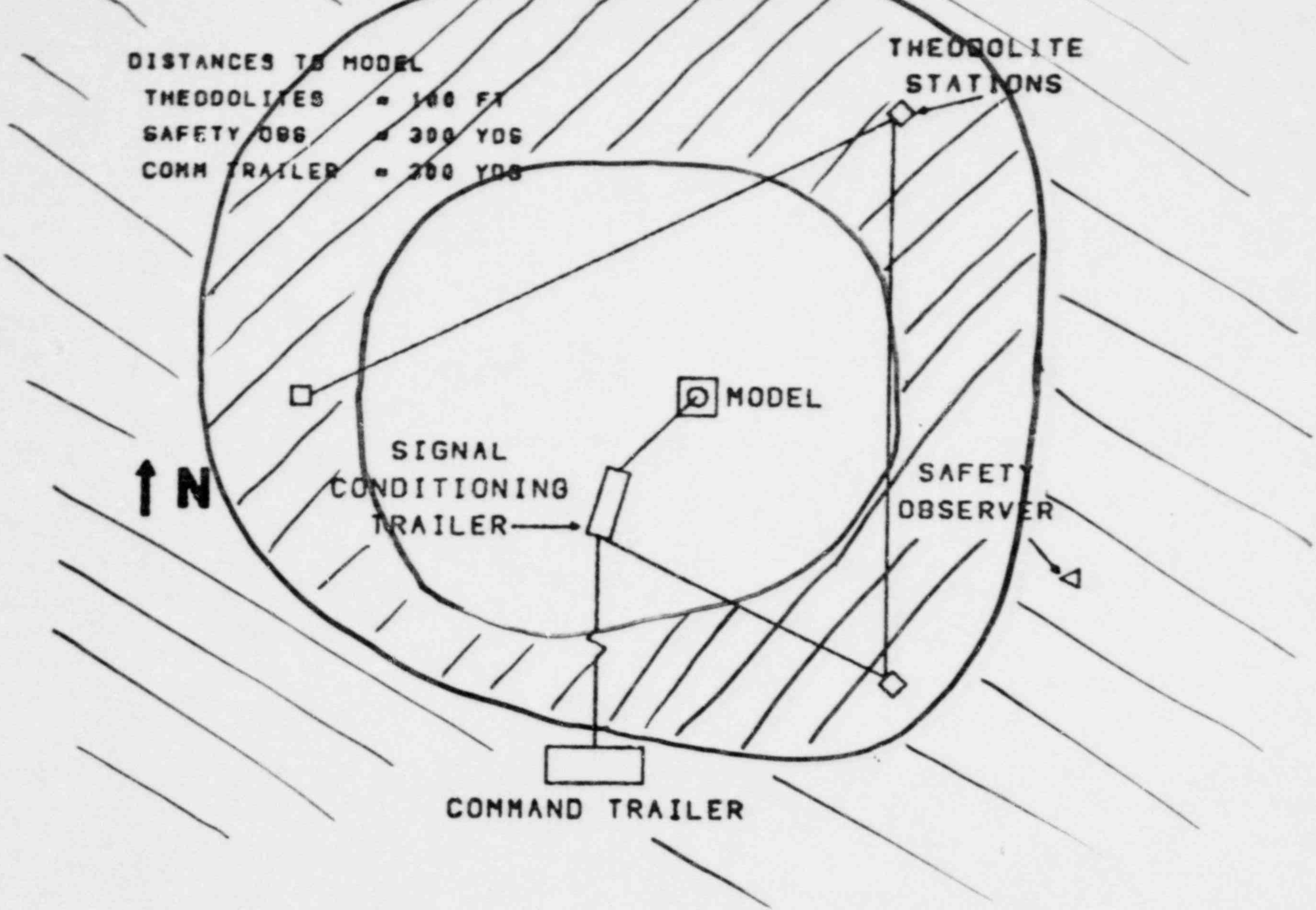
SITE ARRANGEMENT

DISTANCES TO MODEL

THEODOLITES	= 100 FT
SAFETY OBS	= 300 YDS
COMM TRAILER	= 300 YDS



SITE ARRANGEMENT



Lay in

ACRS SUBCOMMITTEE MEETING

MARCH 22, 1982

W. A. SEDRELL

- OTHER ACTIVITIES

- PROGRAM SCHEDULE

TECHNICAL INFORMATION INTERCHANGES

AND/OR REVIEWS

- ADVISORY GROUP
- VISITORS/TRIPS
- WORKSHOP
- SANDIA PROGRAMS
- RELATED PROGRAMS

ADVISORY GROUP

- MEET PERIODICALLY

- ADDED SYSTEMS BACKGROUND PEOPLE TO GROUP

INFORMATION EXCHANGE
(PARTIAL LIST)

USA

- LOWELL GREIMAN, IOWA UNIVERSITY
- GENERAL ELECTRIC-DEGRADED CORE STUDIES
- GEORGE HOWARD, ANCO ENGINEERS
- JACK BURNS, NRC
- JOHN D. STEVENSON, STRUCTURAL MECH. ASS
- W. E. BAKER, SOUTHWEST RESEARCH INST.
- HOWARD LEVINE, WEIDLINGER ASSOC.
- EPRI
- BCL

OTHER COUNTRIES

- DR. LEMAR WOLF, FDR PROJECT, GERMANY
- PROF ANDO (AND 4 JAPANESE), STRUCTURAL DYNAMIC COMMITTEE
- PROF. HELMUT KARWAT, MUNICH, GERMANY
- R.CROWDER, UK
- A.CHALMERS, UK
- JAPANESE (GROUP OF 15)-SITING SURVEY

CONTAINMENT INTEGRITY

WORKSHOP

- PROVIDE A BROADER SCOPE TO CONCERNS IN CONTAINMENT INTEGRITY

- IMPROVE UNDERSTANDING TO POTENTIAL SOURCES AND LOCATION OF LEAK PATHS

- PROVIDE A MEANS FOR INTERCHANGE OF IDEAS

MAJOR TOPICS

I) ACTUAL FAILURE PRESSURE OF CONTAINMENT BUILDINGS

II) CURRENT PROGRAMS
- ANALYSIS
- EXPERIMENTS

III) OPERATIONAL CONCERNS

IV) LOAD CHARACTERIZATION

V) RELATED PROGRAMS/ACTIVITIES

SANDIA PROGRAMS

- HYDROGEN PROGRAM
- SASA STUDIES
- PRA STUDIES

ALL PROGRAMS ARE MONITORED
BY A SANDIA QA/QC ACTIVITY

RELATED PROGRAMS/MEETINGS/WORKSHOPS

- ASME SUBCOMMITTEE ON CONTAINMENTS (STEEL)
- DOE CONTAINMENT PROGRAM
- 9TH LWR RESEARCH INFORMATION MEETING
- INTEGRATED LEAK RATE TESTING WORKSHOP
- LANL-STRUCTURES PROGRAM
- EPRI
- IDCOR CONTAINMENT STRUCTURAL CAPABILITY WORKSHOP

IDCOR CONTAINMENT STRUCTURAL CAPABILITY COMMITTEE

- FAILURE CONDITIONS
- FAILURE MODES
- FAILURE LOADING
- FAILURE PROBABILITY

HIGHLIGHTS-IDCOR WORKSHOP

(2/24-25, 1982)

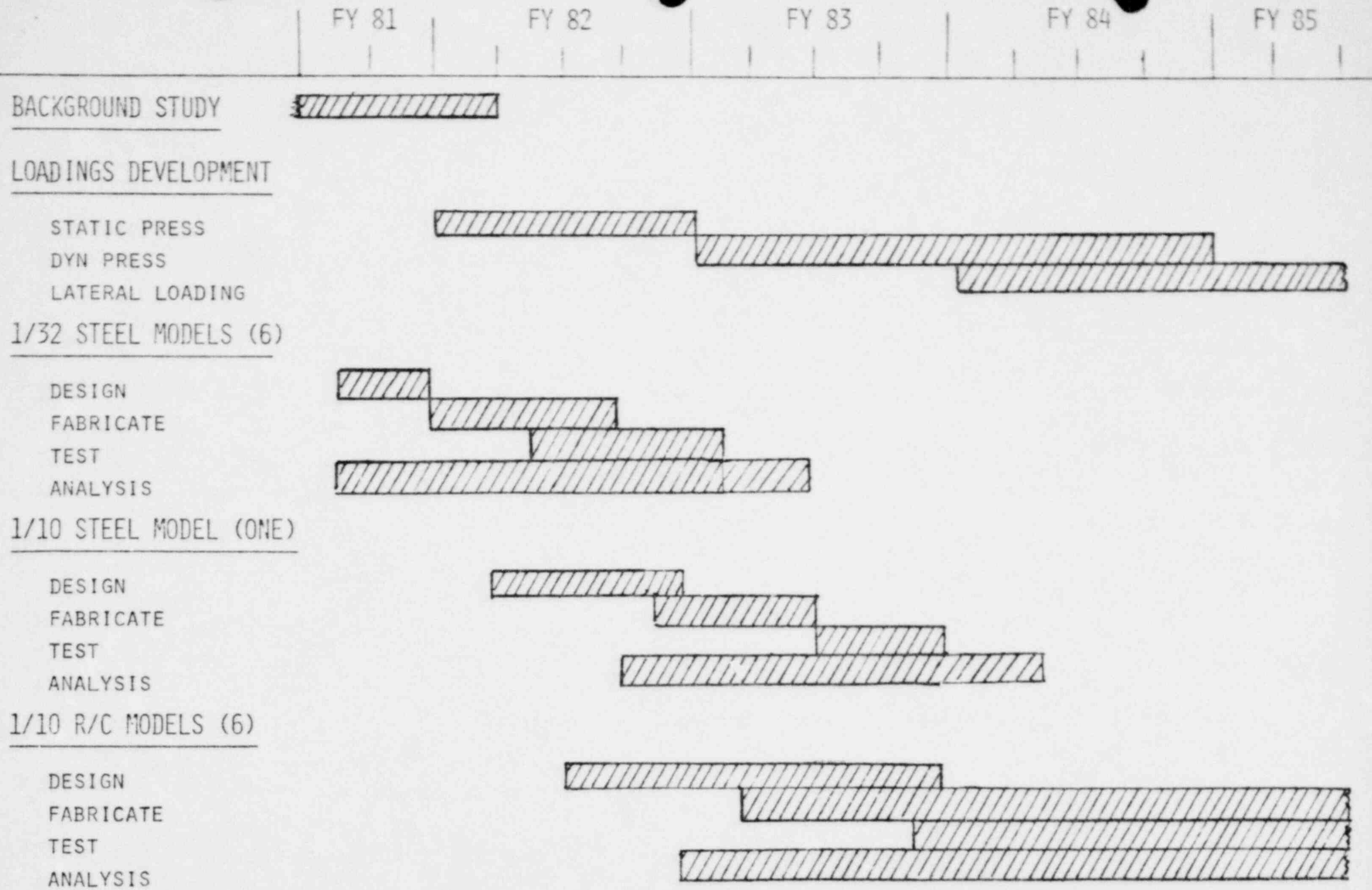
- GENERIC APPROACH

- LARGE VARIETY OF CONTAINMENT BUILDINGS
- MAJORITY ASSUMED YIELD STRESS AS FAILURE
- MOST LOOKED AT
  - EQUIPMENT HATCHES
  - PERSONNEL LOCKS
  - PIPING PENETRATION
  - LINER INTERACTIONS
  - BASE MAT/CYLINDER JUNCTION
- ELECTRICAL PENETRATIONS ADDRESSED
  - SOME ACTUALLY TESTED
- RESULTS VITAL TO REALISTIC ANALYSES OF CONTAINMENT RESPONSE

IDCOR WORKSHOP

REMAINING QUESTIONS?

- NO CLEAR DEFINITION OF FAILURE
- SHOULD DYNAMIC PRESSURES BE CONSIDERED
- MATERIAL PROPERTIES
- LITTLE EXPERIMENTAL EVIDENCE FOR ULTIMATE CAPACITY PRESSURES FOR CONTAINMENTS
- UNABLE TO ADDRESS PENETRATIONS GENERICALLY


FY 81
FY 82
FY 83
FY 84
FY 85
BACKGROUND STUDY
LOADINGS DEVELOPMENT
STATIC PRESS
DYN PRESS
LATERAL LOADING
1/32 STEEL MODELS (6)
DESIGN
FABRICATE
TEST
ANALYSIS
1/10 STEEL MODEL (ONE)
DESIGN
FABRICATE
TEST
ANALYSIS
1/10 R/C MODELS (6)
DESIGN
FABRICATE
TEST
ANALYSIS

SUMMARY

STRUCTURAL MARGINS OF CONTAINMENTS PROGRAM WILL PROVIDE

THROUGH A COMBINED EXPERIMENTAL AND ANALYTICAL EFFORT A RELIABLE CAPABILITY

TO EVALUATE PREDICTIONS OF ULTIMATE CAPACITY OF LWR CONTAINMENTS FOR

- STATIC PRESSURES
- DYNAMIC PRESSURES
- LATERAL LOADINGS