THIS DOCUMENT CONTAINS POOR QUALITY PAGES

NUCLEAR REGULATORY COMMISSION ORIGINAL

T-0782

In the Matter of:

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

245TH MEETING

NRC PDR

DATE: September 4, 1980 PAGES: 1 thru 308

AT: Washington, D. C.

ALDERSON REPORTING

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

Telephone: (202) 554-2345

8009080385

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

- - -

245TH MEETING

- - -

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

Thursday, September 4, 1980

The 245th meeting of the Advisory Committee was convened, pursuant to notice, at 8:30 a.m.

MEMBERS PRESENT:

M. PLESSET, Chairman, presiding
J. C. MARK, Vice chairman
C. P. SIESS
S. LAWROSKI
M. BENDER
D. W. MOELLER
W. KERR
M. CARBON
H. ETHERINGTON
W. M. MATHIS
J. C. EBERSOLE
H. W. LEWIS
P. G. SHEWMON
D. OKRENT
J. J. RAY

DESIGNATED FEDERAL EMPLOYEE:
R. F. FRALEY, Executive Director

ALSO PRESENT:
J. M. JACOBS, Secretary

P R O C E E D I N G S

MR. PLESSET: The meeting will come to order.

This is the 245th meeting of the Advisory Committee on Reactor Safeguards. During this meeting the Committee will hold discussions on the following: BWR scram systems, quantitative risk criteria, Sequoyah Nuclear Power Plant, Units 1 and 2, hydrogen control, NRC regulatory requirements and reevaluation of operating nuclear plants, and revised clad swelling and rupture model.

We shall also discuss other topics as well. The specific items on the agenda today include the BWR scram systems, a brief session on quantitative risk criteria, and the Sequoyah Nuclear Plant.

The meeting is being conducted in accordance with the Federal Advisory Committee Act and the Government in the Sunshine Act. Mr. Raymond Fraley is the designated Federal employee for this portion of the meeting.

A transcript of the meeting is being kept, and it is requested that each speaker first identify himself and speak with sufficient clarity and volume so that he or she can be readily heard.

We have received no request for statements from members of the public for this meeting.

(The Chairman's report follows.)

MR. PLESSET: We will go into open session.

I would like to ask you to look at Tab 1.2. We are going to discuss the BWR hydraulic scram system.

As you know, we had one brief discussion of this already to the full committee which was not very informative, although it is a rather complicated problem. It is not clear yet just what all went wrong on Brown's Ferry. There are certainly some hydraulic problems, but there are other problems, as well.

There was a subcommittee meeting on August 19 and 20 on this question, and we had a rather lengthy discussion of the whole problem. It is quite remarkable, the asymmetry in the scram systems, as you will recall.

MR. SIESS: At Brown's Ferry?

MR. PLESSET: All of them.

MR. SIESS: I thought some of them had separate --

MR. PLESSET: Do they? Some of the very new ones?

MR. SIESS: Peach Bottom.

MR. PLESSET: I thought they all -- aren't they all -- they have separate -- some of them have long lines, don't they? Some of them have very long lines from one point to the other in the reactor. There is a lot of variability. This is something that is an example of the complications you get into when it is a balance of plant question, even though this is an extremely imp--tant safety system.

The so-called hockey stick has been mentioned. There are problems with the vent valves and the drain valves in the systems, and presumably the severe hydraulic water hammer could be experienced as a result of the behavior of the drain valves. And you will hear about that, so let me, unless there are some general questions, to go the agenda that we have in that tab.

I should say that our presentation that Gary Young made and Ms. Zukor made were excellent. You will get an abbreviated version today. So let me call on -- Gary begins. I see. Okay. Gentlemen first.

MR. YOUNG: I would like to describe briefly, with the help of Dorothy Zukor, the report that the ACRS staff fellows prepared on the recent malfunctions of the BWR scram systems. What we tried to do in this report was present briefly a description of the scram systems and kind of in a generic way how they operate, and then go through each one of the events that occurred at Brown's Ferry, at Hatch and at Brunswick, as well as some of the other occurrences that were found during the test program following the Brown's Ferry event.

We also had a discussion of the NRC Office of Analysis and Evaluation of Operational Data Report on the Brown's Ferry event. They went through and visited the

Brown's Ferry site and made some recommendations. Also we did a comparison with WASH-1400 what they listed as some possible causes of failure of the scram system, and we compared that with what actually happened.

So, first I would like to start by showing some slides.

(Slide)

This is the scram system, and I will briefly go through how it is supposed to work and what happened, the reason it did not work at Brown's Ferry.

MR. PLESSET: Can you raise it up a little bit, Gary? Fine. That is good.

MR. YOUNG: This is a schematic drawing of a control rod drive mechanism, and this is the drive piston that pushes the control rod into the fuel assembly area. The way it is supposed to work is the high pressure water is applied to this side of the line, which exerts force on the bottom of the drive piston, pushing it up into the vessel, and then the back side of the drive piston, this area here, is vented via this line to the scram discharge volume, and this is essentially an atmosphere pressure, and this is approximately 1500 psi. So it is a differential system that drives the rod into the core.

(Slide)

This is a schematic of the hydraulic system used to push the control rod into the core. This is the scram accumulator. That is the safety grade water supply that that 1500 psi is used to exert the pressure on the drive piston. It is normally charged during power operation, and it is charged by this drive water pump here.

That is in operation all the time to supply water for normal drive motion and for cooling water.

These are the scram valves here. They are normally closed and held closed by air pressure maintained on the valves.

The back side of the drive piston is vented via this path to the scram discharge volume, and then the scram discharge volume has an instrument volume attached to it. At Browns Ferry, this was a two-inch pipe that connected it to an independent tank. At Brunswick the instrument volume is an integral part of the scram discharge volume.

Normally the scram discharge volume and instrument volume are vented and drained. These valves are opened such that any water that might accumulate during normal operation would flow right on through. During a scram these valves close to bottle up that scram discharge volume, and these valves open during a scram.

So the design numbers that are given by G.E. in designing the system, the scram discharge volume is sized to have a capacity of 3.3 gallons per rod. There are 185 control rods in each of the plants, and this is 3.3 gallons capacity for each one of the rods.

So at Browns Ferry they had a total of aboaut 750 gallons capacity, which was in excess of this number. This number comes from the volume on the back side of the piston when it goes from its fully withdrawn to its fully inserted position. It displaces about 3/4 of a gallon of water. Then in addition to that, there is an allowance for seal leakage from this side over to the back side of the piston of 10 gallons per minute, lasting for 10 seconds, and that is where they come up with this number.

There is also some conservatism in there because those two numbers add up to less than 3.3 gallons.

MR. LEWIS: I am sure I am the only one who does not understand the plumbing, but just to be quite sure, this picture is a little bit different from the previous one in that in the previous one the discharge water comes down the center of the piston tube and exits from the bottom, whereas this shows it exiting from above the piston.

Is the first one right and this one wrong?

MR. YOUNG: This is to show schematically how it works. Although the other one is also schematic, it has a little bit better view of the flow pattern.

MR. LEWIS: Better schematic.

MR. PLESSET: Before you take the slide away, do you want to make any comment about Michaelson's question regarding the loss of air and what it would do to the scram inlet and outlet valve? Do you want to make a comment on that?

MR. YOUNG: Okay. This is the air supply that holds these valves shut during normal operation, and the AEOD group wrote a letter, Mr. Michaelson wrote a letter that said that if this air supply were gradually cut off, your normal mechanism is to rapidly vent this air supply so that these valves fly open very quickly.

He said that if this air supply was gradually lost due to some failure in the air system which is non safety grade, that this valves would drift open very slowly, and with this drifting you would get water leakage from the control rod drive system into the scram discharge volume. His concern was that this volume could quickly fill up with water before the operator would initiate a scram, a manual scram signal.

So basically, it is based on an event that happened at Browns Ferry just recently where they had an air compressor fail and it was a catastrophic failure, so the air supply bled off rather quickly and the operator noted that some of his rods were drifting into the core.

The reason for that is that this valve was drifting open. He had high pressure water here and it was causing the rods to drift in. If he had not initiated the scram -- at the same time the water was going to the scram discharge volume filling it up.

.o his concern is there is a possibility that your scram discharge volume will fill with water before you initiate a scram, and then when you try to initiate a scram, you cannot do it, because you have to have a vent path to vent this water off before the rods will go into the core.

MR. EBERSOLE: There is some follow on to that. The monitoring at Browns Ferry was said to be continuous; however, when one went into questioning and detail, it turns out it was continuously recorded but only periodically read, which led to a case of looking at, I think, it 1 minute every 30 minutes, so you had 29 minutes out of 30 for this to presumably start filling.

You are exposed for that tremendously large fraction of time. I understand that that has been fixed.

MR. PLESSET: Yes.

MR. EBERSOLE: There is another aspect of this that came up just the other day, and that is, of course this fail..re of the service system not to perform abruptly as it was designed to do but progressively as it should not do applies generically to pressure and air and AC and DC systems and all over the place; but I think in this case it may also affect the SDV vents and drains in a manner to cause them to open slowly as well.

So what one has is an open system; however, it is inadequate to drain. So this volume would still fill up. But it leads to a case where if that condition goes on, it appears to offer an open path to the atmosphere, which is a radiation problem which Carl has pointed out, and it may be significant in that it offers a boron leakage path for which you have an ATWS mitigating system, only one shot of boron, with a present zero allowance for any liquid leakage.

That is an adjunct matter to be looked at.

MR. PLESSET: I think that is a very good point that Carl and you have made. You remember we raised the question about the quality of this whole system. They assured us it was great but they stopped at the valves and they did not think what would happen if these valves did not do what they were supposed to do, as Carl and you are pointing out.

So it is a question of how far do they go when they consider the safety features of a system like this. They stopped.

MR. EBERSOLE: I think at this time I am going to make just a fragment of that little statement I made at the meeting about the curious rationale on which this is built. You notice that it is built conceptually on the thesis whenever you have a potential for radiation leakage present, by all means close everything up tight.

So what this really does, one can argue it takes the primary function, which is get the rods scrammed, and it preconditions the dump volume so it is closed and therefore cannot open except to the extent its volume has been defined and it is loose in the system, whereas it could have been designed to be an open discharge into a sump or into the containment or someplace with subsequent action to take care of the relatively minor problem of radioactive water leakage after you have sealed in and locked the rods in place.

So the roots of this rationale here like in a rather vague theory that it is more important to keep a little bit of leakage from occurring than it is to scram the reactor, which I think is inverse logic.

MR. BENDER: My intuitive logic says that may be one of the regulatory influences that is adverse to safety. The fact of the matter is we put so much emphasis on not letting any radioactivity get out, that the vendors have chosen this avenue.

MR. EBERSOLE: Right.

VOICE: Could staff make a clarification on the vent and drain valve?

MR. PLESSET: Briefly, yes.

VOICE: The vent and drain valves still are closed on loss of air.

MR. EBERSOLE: That would act as a seal. So they are different from a scram dump valve.

VOICE: That is correct.

MR. YOUNG: They are both serviced by the same air supply, and they do act somewhat slower. So I think your point that these could be open, as well as these could exist if you had a slow loss of air pressure, but these do close on loss of air. But if it were a gradual loss, they would gradually close.

MR. EBERSOLE: There is a fundamental problem of not looking at the gradual -- it is presumed there are abrupt changes in the barometers. It is a problem everywhere you look.

MR. YOUNG: Another point, too, that was in the Michaelson letter is there is an alarm to the operator to tell him that he is losing air pressure. So he is not flying blind. He knows that he is losing air pressure. It is not a safety grade alarm, but it is in the control room.

Based on that, I think the recommendation was that as soon as he sees he is losing air pressure, he will scram to prevent filling this with water. So he has a period of time between the alarm and the time these valves start drifting open to take his action.

MR. EBERSOLE: Again pointing to the fact that the operator looks at an indicator and thinks he is looking at the barometers. He is looking at a general air pressure but may not be looking at this leg of the system. He may be losing air but not know it.

MR. YOUNG: To talk about the features of the scram system and how they are related, the scram discharge volume's purpose is to allow scram to progress to depletion and then to isolate the water leakage from the reactor vessel following the scram.

The instrument volume is there to alert the operator to the fact that he has water in the scram system prior to a scram, so the instruments here are to tell him he is getting water in there that should not be there and that he needs to take some action -- either initiate a scram or get the water out of there by some method.

(Slide.)

This next slide shows a little closer view of the Browns Ferry design on the instrument volume. This again is just a schematic, but the way the system is set up, the operator's first indication is he gets an alarm there is water in the bottom of his instrument volume, because again, the drain line is open during normal operation. So if water were to build up, he would get an alarm.

As water builds up higher, if he does not take any action, he gets a rod block level which prevents him from withdrawing the rods from the core any more than they already are. And then if the water level continues to build up, at this point he gets an automatic scram signal. And the purpose of this is as water gets to this elevation, it is starting to get up into a point where it can get into the scram discharge volume, the 3.3 gallon capacity; so this scrams him when he still has plenty of capacity to allow a scram.

MR. SHEWMON: At Browns Ferry none of these alarms alarmed.

MR. YOUNG: At Browns Ferry none of these came in.

MR. MOELLER: Excuse me. Was this system not at Browns Ferry?

MR. YOUNG: This is Browns Ferry.

MR. MOELLER: But it failed to alarm.

MR. YOUNG: I will show you. They were operating, the instruments were operating, but I will show you a little later why they did not work.

(Slide.)

Now, I would like to go into the Browns Ferry event. This happened on June 28 this year, and the operators were bringing Unit 3 down for a scheduled maintenance, and as part of the procedure for bringing the plant down they initiated a manual scram at approximately 30 percent power to shut it completely down.

When the operator did this, he got indication that all of his scram inlet and outlet valves had opened because he has position indicators in the control room; but he noted on his rod position indicators that the rods had not all inserted, and they had stopped moving. And this is a picture of the rod pattern --

MR. MOELLER: Excuse me a moment. You were at 30 percent power when he started this. When had he brought it from presumably 100 percent down to 30 percent, and had everything worked all right then?

MR. YOUNG: Yes. It was a normal, routine shutdown.

MR. MOELLER: And how far previously?

MR. SIESS: Not a scram.

MR. YOUNG: It was not a scram. It was a manual decrease in power using the rods.

MR. MOELLER: Okay. Then how much prior to the problem had this been done?

MR. YOUNG: It was a consecutive thing. He was just bringing the plant gradually down, and when he reached the 30 percent power level he initiated --

MR. SHEWMON: Over a period of minutes, hours, or days?

MR. YOUNG: Minutes.

MR. MOELLER: Thank you.

MR. YOUNG: Okay. This was at 1:30 in the morning. Also, it was on Saturday morning when it occurred. All of the zero positions shown here indicate that the rods were fully inserted into the core. The 48 position indicate the rods were fully withdrawn from the core, and this is a view of the west side of the core. There are 92 rods on the west side, and on the east side there are 93 rods.

This was the pattern that they recorded. On the west side there was one rod that did not fully insert, but it was at the 02 position, which from a neutron absorption viewpoint is essentially full in. But one of those 76 rods was on the west side. All of the others were on the east side. And there was one rod up here that was at the 46 position.

As I mentioned before, the 48 position is fully withdrawn, so it barely went in at all. The ones I have marked here in blue can be counted as being essentially in because they were far enough in that they had all of their neutron absorption capabilities. So if you count the rods that were effectively not inserted, there were only about 70 rods that did not go in.

You will notice here the strange rod pattern on the east side. The rods started to insert and then suddenly stopped, and they stopped at different positions. GE and TVA have done some analysis and have been able to confirm that the amount of position change that occurred during this first scram is directly related to the scram speed that they got during tests.

And what that means is that, for example, this rod up here had a very slow scram speed when they ran the test, indicating that it had some seal leakage in excess of some of the other rods or a little more friction, and therefore, it had a slow scram time relative to the others. So it moved for a little while and then stopped in this position, whereas other rods moved considerably further; and that was based on rods that had very quick scram times during their test program prior to this event.

MR. LEWIS: So the inference is they all stopped at the same time.

MR. YOUNG: They all started at the same time or they all stopped at the same time. The difference in movement was based on friction and other losses.

MR. EBERSOLE: Something that happened here is where they are pointing out that is the operator's response. He was doing two things. One, he was faced with a half-ATWS, or he thought he was, so he was going to try to get the rods in, which he did, but he did another thing, but not intentionally or obviously intentionally but by instinct or prior training. He did not intercept the steam flow path.

Had he done that then the course of events would have been somewhat different. He would have collapsed the voids. We would have had a spike in power to some magnitude. But from prior experience he thoughtfully allowed steam to continue to flow. Therefore, he did not have any real power spiking problem, but he could have had if he had reacted erroneously and closed the turbine valves, but he did not do it.

MR. PLESSET: Yes, Max.

MR. CARBON: Gary, what was the power level of the reactor after this much rod insertion? Was the left side still critical?

MR. YOUNG: The indication that the operator had, he had a power range monitor still watching the core, and it indicated less than two percent power. That monitor, the down-scale is two percent power, so he only knew he was less than two percent power. He did not have the instruments inserted to measure the low power levels, so he only knew that he had gone from 30 percent power to less than two percent power. And at this point all of the reports simply say that. They do not say that he was subcritical, but they imply that he probably was.

MR. KERR: Decay heat would be one or two percent power at this point, wouldn't it?

MR. YOUNG: The two percent was neutron power.

MR. PLESSET: This would be above --

MR. BENDER: Gary, I guess I was not clear on the decision process that occurred. He had reduced the power to 30 percent, and then the decision was made to shut down completely.

MR. YOUNG: Yes. That is the routine.

MR. KERR: That was probably achieved by asserting recirc flow, wasn't it?

MR. YOUNG: Yes.

MR. BENDER: At 30 percent the normal procedure is just to scram the reactor by pressing the scram button?

MR. YOUNG: For TVA at Browns Ferry that was their normal procedure.

MR. BENDER: Okay. And at that point all the rods on one side went in, and they observed evidently the rods weren't going in on the other side.

MR. YOUNG: Right.

MR. BENDER: Did you look at the procedures enough to know what instructions the operator has during this part of the operation? What is he supposed to be doing? Is he supposed to be looking at the rod action or what?

MR. YOUNG: Yes, yes. He verifies that all of the scram valves operate and that the rods are fully inserted.

MR. BENDER: What do the procedures tell him to do if the rods are not going fully in? Were you able to find out?

MR. YOUNG: I think the NRC staff has that information. I do not directly know.

MR. BENDER: I think it would be interesting to know whether there is some kind of procedures.

MR. YOUNG: There are procedures, but I could not tell you exactly what they are.

MR. BENDER: Jesse's point, whether the steam valves should have been closed or not closed and whether that was done by intuitive judgment or not is an interesting part of the whole operational planning question.

MR. YOUNG: TVA mentioned at an earlier meeting that it was intentionally -- it was the operator's intention to continue steam flow simply because he has to make sure he has a heat sink.

Now, if anything had gone wrong, any other events had occurred, he could very easily have lost that steam flow, but he does routinely maintain steam flow just because he needs that heat sink.

MR. BENDER: I guess I was interested in knowing just what his training instincts would tell him to do.

MR. YOUNG: Right.

MR. LEWIS: One of the other questions that I just --

MR. PLESSET: Could you use the mike, Hal?

MR. LEWIS: One of the other questions to which I never knew the answer was what do the calculations show the power level would have been if this had happened from full power instead of 30 percent?

MR. PLESSET: We are going to get to that.

MR. LEWIS: Forgive me.

MR. YOUNG: This was the configuration after the first manual scram, and in addition, the operator noted that the instrumentation on the instrument volume, the scram discharge instrument volume, gave him an alarm or gave him an automatic scram signal at 19 seconds into the event.

The normal fill time for that scram discharge volume takes between 45 to 60 seconds to fill up and give him this alarm. It took only 19 seconds, so that was an indication that there was water already in the scram discharge volume that he did not know about prior to the scram event.

MR. BENDER: Is he conscious of that incremental time? It seems to me that is expecting an operator to know a lot, to be able to judge the difference between 19 seconds and 45 seconds or whatever.

MR. YOUNG: I don't know if that was a later finding or if the operator knew it right at the instant. It was recorded that way.

MR. BENDER: Thank you.

MR. OKRENT: I would guess he does not normally look at that. Were these rod positions on the computer?

MR. YOUNG: Yes.

The next action taken by the operator was to clear the scram signal to put the mode switch in shutdown which allows him to bypass the automatic scram signal he has from the instrument volume. By doing that he can reset the scram signal, allow the scram volume to start to drain, and then six minutes after the first scram he initiated a second manual scram.

(Slide.)

This was the rod configuration after the second manual scram. Again, the rods moved in slightly. Each rod moved in a little bit more than it was before, but it did not go all the way. It moved and stopped.

All of the rods on the west side were now in. The ones circled in blue were effectively pulled in.

Okay. Again, he reset the scram signal. He allowed a little bit of time for the scram volume to drain, and two minutes later he initiated the third manual scram.

(Slide.)

And this was the rod configuration after the third one. There were 36 rods out and of those 36 several of them were effectively full in. He at this point again reset the scram signal, allowed some drain time on the scram volume, and at this point he took the bypass switch on the automatic scram signal coming from the level instrumentation, took it back to the automatic position which is, as I understand it, is a routine thing to do.

When he did that there was still water in the instrument volume, and that initiated the final automatic scram; and this was six minutes after the third scram. At that point all the rods went completely into the core, and based on some looks back at the situation, it did not really matter whether it was an automatic or a manual scram at this point.

There was enough space available in the scram discharge volume to allow the full scram, so it is really incidental that it was an automatic scram rather than a manual scram.

MR. EBERSOLE: That sounds as though he knew what was happening, and he was draining as he knew that that was what the problem was.

MR. YOUNG: He had indication that that was a problem. He had indication that each time he allowed it to drain, he got the rods a little further in. So the final complete scram occurred 14 minutes after the first scram.

(Slide.)

Based on the studies that were done later, they determined that the scram discharge volume did have water in it on the east side, and that is what prevented the rods from fully inserting on the east side.

(Slide.)

To understand why that occurred a little better, you need to look at the layout, and I have a drawing here of Peach Bottom, but it is very, very similar for Browns Ferry. This is a plan view looking down from above the reactor vessel, and off to the side here are the hydraulic control units for each one of the rods.

They basically split it up so that 92 of the hydraulic systems are serviced by a hydraulic control unit on the west side, and then 93 are serviced by control units on the east side.

This is the scram discharge volume. It is a six-inch pipe header arrangement. It has in the neighborhood of 300 gallons capacity, somewhat more than 3.3 gallons times 92 rods. A similar arrangement on the east side, and then each one has a two-inch drain line connected to the instrument volume which has all the instrumentation to tell the operator he has water in the scram volume.

MR. PLESSET: About how long is that line?

MR. YOUNG: About 150 feet on the east and on the west side about 20 feet long. So there was a considerable difference in the drain line.

I have an isometric view of the Browns Ferry layout.

(Slide.)

These are the headers for the scram discharge volume on the west side and the east side, and these are the two-inch drain lines that connect to the instrument volume here. And you will notice that this one is 150 feet long, this is only 20 feet long, and this is the expansion loop.

MR. SIESS: This is Browns Ferry?

MR. YOUNG: Yes. Browns Ferry Unit 3. This is the expansion loop that a lot of people thought was the problem originally. It turned out probably it was not. It is a horizontal expansion. Earlier it was reported it might have acted as a trap and prevented water from draining, but it is horizontal, and it would not act as a trap.

(Slide.)

Another view of Browns Ferry design is this one. It shows an elevation view, and these are the volumes, the header arrangement here and here. This is the instrument volume, and these are the hydraulic control units that have the accumulator, and all the scram valves, and everything associated with the scram system.

It is interesting to note here that GE is a design member for the slope of the drain lines in the system of an eighth of an inch per foot. And you can see here that on the west side they had approximately a one inch per foot slope down to the instrument volume, but on the east side they had about a .13 inch per foot, so they just met the one-eighth inch per foot slope on this side. It is 150 versus 20.

You will notice here that the vent lines off of the scram discharge volume tie into a drain header right here, and they rely on air being in this vent header for their vent capability. The drain line off the instrument volume also ties into a common drain header, and this feeds into a sump, and the pipe is under water at this point.

This is a very large header system, and there are a lot of open drains in this system, so you would expect air to be in that header system, but not necessarily so.

Another point is that from this valve on and from this valve on down is a non-safety grade part of the system. That piping is not seismically qualified, and necessarily it does not need all of the specifications of a safety-grade system. And of course this drain header --

MR. KERR: Beginning where does it not meet safety --

MR. YOUNG: From this drain valve down and from this vent valve down. All of the instrument volume and the header arrangement up here are safety grade up to the first valve, which is here.

MR. KERR: But if you did -- let's see. If you had an incident that would open it, it would not prevent function; but if you had an incident that blocked it, it would be serious.

MR. YOUNG: That is true, because the safety function of these valves is to close, so if you --

MR. KERR: The safety function is to close, but if you have them open it does not prevent scram, does it? It just prevents containing the water.

MR. EBERSOLE: Safety function in the context of stopping radiation leakage. If they were open and delayed in closing, then the rods would have drifted in without closing, not at the desired rate, but they would have gone in.

MR. YOUNG: Right. If it was caused by the air supply which connected that with the scram valves.

MR. EBERSOLE: If you left them open until you ascertained scram, they would have gone in but not very fast.

MR. BENDER: What is it that is non-safety grade about it?

MR. YOUNG: The piping is not analyzed seismically, supported seismically not necessarily. That does not exclude them from doing that, but they do not have to do that. And the only other thing is the certification of the pipe material, which is really not that relevant. It is usually the same pipe, but it has a little more documentation on it.

MR. BENDER: If one looked at it in terms of pipe break, that would not prevent the scram system from working.

MR. YOUNG: The pipe would not have any effect on this. The problem is that simply prior to a scram you assume that this system is draining completely because this valve is open and this valve is open; but since this is non-safety grade or even if it were safety-grade, it does not matter.

If this were plugged by water, trash, whatever, it can prevent this from draining into the instrument volume, and therefore defeat a safety function.

MR. KERR: What is the pressure buildup in that system if you do not have drainage? Can the pressure build up to 1500 psi?

MR. YOUNG: Prior to a scram?

MR. KERR: During an effort to scram.

MR. YOUNG: During a scram these valves close. The water comes into the system, and it does pressurize to full primary system pressure. That is its design.

MR. KERR: It will only go to primary system pressure, or will it go to the 1500 psi that is indicated?

MR. YOUNG: The 1500 psi is the accumulator pressure. If you let the scram valves completely open, that water, that 1500 psi water would bypass the seals and go into the reactor vessel, so that pressure would have to pressurize the vessel as well as this to get the 1500.

MR. KERR: But you can go to vessel pressure.

MR. YOUNG: Yes.

MR. BENDER: Gary, would deformation of the header have the indication of perhaps preventing the system from draining properly and be a way of --

MR. YOUNG: Yes, sir. If this pipe were crimped or bent or whatever, or this pip_, you could prevent drainage.

MR. BENDER: Thank you.

MR. EBERSOLE: Successive scrams after a first attempt are done by non-safety grade evolutions, because if there has been some sort of stoppage of if these non-safety grade valves have stopped so they cannot be opened, then you do not have the privilege or a safety-grade second or third or any other shot at the scram. It is designed for a one-shot scram, not for successive scrams in the context of having safety evolutions to permit secondary scrams after the first attempt.

So this whole shutdown was accomplished on non-safety grade evolutions, evolutions of non-safety grade equipment which was cyclically opening these valves which have no part_cular pedigree. You were not entitled to that second drain.

Of course, you would not be if you crimped it. You would never get it, even if the valves would not work.

MR. SHEWMON: You have to bend the pipe a fair ways to completely shut it off.

MR. BENDER: I guess what I had in mind, it would have to be pretty substantial displacement of the header system. It is going to put its force right on that small line. Just by bending it a little bit you can restrict it, so the flow would be somewhat limited; and I can see that as a way of eliminating the effectiveness of the whole drain system.

MR. YOUNG: The way the system is set up, it was intended to be set up if anything occurred that prevented draining, the instrumentation in this would catch the water buildup and cause a scram before that situation was a problem.

But what happened at Browns Ferry simply was that this header filled with water, and as it ran into the instrument volume it was not picked up by the instruments because -- well, I will show you. I have another drawing that shows that a little better.

(Slide)

The design was there to prevent that problem, but it did not work at Browns Ferry. This was a test after the event to find out what could have caused the problem. They filled the east header with water to simulate a blocked vent line condition, and then they closed the drain valve here so they could measure the water accumulation to measure flow rate.

Based on that test, they found that this header drained at the rate of .6 gallon per minute into the instrument volume, which was good because that indicated this thing is self-venting. If you close the vent, it still will drain but it is a very slow drain.

(Slide)

They ran another test on the west side, and got again a self-venting condition. But it drained at 3.2 gallons per minute with this vent valve closed, which was much better but still not very good.

(Slide)

MR. SHEWMON: If I have a bottle of Coca-Cola and pour it out, is that self-venting?

MR. YOUNG: Yes.

MR. SHEWMON: Thank you.

MR. YOUNG: This is a test to simulate both vent valves closed or plugged, and both headers will fill with water, and they got a combined flow rate this time of about .6 gallons per minute. Then a final test was done to simulate the normal condition.

(Slide)

This is the condition that would exist after a scram normally. Everything is filled with water, and you open the vents and the drain, and using some ultrasonic indicators on these headers, they measure the amount of time it took to empty them. They took 9.5 minutes to empty the west side, 25 minutes to empty the east side, and the instrument volume was completely clear.

The level alarm was cleared in 11.5 minutes, so this indicated the problem, which is that at Browns Ferry when this header empties it is draining at about 34 gallons per minute. This level just continues to drop in the instrument volume because it has a flow rate of 36 gallons per minute.

Therefore, the water coming in from the east header comes in at 11 gallons per minute normally. So it just runs right through the instrument volume and the instruments do not pick up anything, and yet there is still water up here in this header.

Now, if you blocked this at all, this drain line or the vent line, you will not get 11.6; you will get something approaching .6 gallons per minute. So that explains how the water was held up in this volume simply by the fact that some -- they don't know how it got there. There are several possible mechanisms for getting the water in there.

The normal leakage from the scram discharge valves, they leak a little bit. It is very small. They measure a .03 gallon per minute leakage from all 185 valves after the Browns Ferry event. It is a small leakage rate, but if this were not draining properly, that would eventually fill up this header and the operator would get no indication over here.

MR. LEWIS: I did not understand one thing. When they did the simulated test with both the east and west header vents blocked, the flow rate was .6 gallons per minute. But that was much less than the west alone was. Why did blockage of the east one slow down the burbling of the west one?

MR. YOUNG: I wondered that, too.

MR. LEWIS: I see. Thank you.

MR. YOUNG: When they filled both headers, it allowed them to drain into the instrument volume. It settled down to about a .6 gallon per minute drain rate.

MR. LEWIS: Is there any common section of pipe between those two 2-inch lines before they go into the instrument volume?

MR. YOUNG: No, there was not. They separately go into the --

MR. LEWIS: They obviously interact somehow.

MR. EBERSOLE: Isn't there some -- the air flow path --

MR. YOUNG: That may be. That may be.

MR. MOELLER: In the test you say they blocked the vents from each side. Why did they do that? Did they have a reason to believe the vents had been blocked?

MR. YOUNG: They suspected there was inadequate venting that caused the problem, so they closed the vent valves to simulate that condition. They had attached a vacuum pump just after the event to the vent headers, and at one point they were able to pull a slight vacuum with that pump on the east side. They were never able to repeat that experiment, but it implied there may have been some type of blockage.

So that is the description of the Browns Ferry event.

MR. LAWROSKI: Are the other Browns Ferry units different from this?

MR. YOUNG: They are similar to this. There is just minor piping variations.

MR. FRALEY: Maybe you ought to recap what the staff has told the utility to do as a result of this.

MR. YOUNG: Okay. Right after the event, some other problems occurred and they came up with a recommendation on how to allow continued operation, and that was to install some ultrasonic level indicators on these headers directly rather than relying on this instrument volume to tell the operator that he has water in the header.

In addition to that, they had they open this vent line here to the atmosphere in the building so that when this valve is opened, it is directly open to the building atmosphere rather than relying on the string header arrangement. So those were the two things that they primarily did to allow continued operation.

MR. BENDER: There has not been any proposal to subdivide those string headers further.

MR. YOUNG: Yes. The final solution that is being recommended by G.E., and the utility is basically going along, is to eliminate that drain line. I think Dot will show in her presentation that the Brunswick design does eliminate that drain line and it is a better arrangement.

MR. BENDER: I had in mind to have more separate drain lines. This thing is divided into two halves, and one might want to divide it into quarters or even a greater subdivision than that.

MR. YOUNG: I see. I don't --

MR. BENDER: Commonality continues to be a matter of concern, and as long as G.E. is going to make the argument that they have independence within their scram units, I think they ought to make damn sure they are really independent.

MR. PLESSET: Well, does that finish your presentation?

MR. CARBON: I have a question. It was my impression from what you said that the problem perhaps stemmed in part from plugged vents. From what you say, the staff had those vents opened to the atmosphere to bypass part of the line.

Did they do anything in terms of the valves, the vents themselves?

MR. YOUNG: They have done some testing to make sure the valves are operating properly, and they have done some tests to make sure the system is venting properly when the valves do open. The valves themselves have not been changed. They are the same valves.

MR. PLESSET: Thank you, Gary.

Is there another question?

MR. LEWIS: A trivial question. In the real world, the header pipes each have 90-odd T's in them, each for the separate rod discharges, or do the rod discharges come together before they go to the header or what?

MR. YOUNG: They are separate. They have 185 separate hydraulic lines that go from the rod drive mechanism all the way and tie into the header.

MR. LEWIS: There are 95 T's in these pipes.

MR. YOUNG: Yes. These are small pipes and they come up here into the header. This shows one of the 185 hydraulic control units. You have two banks on each side.

MR. LEWIS: Okay, thank you.

MR. PLESSET: Thank you, Gary.

I think Dorothy is next.

MR. YOUNG: Yes. She will talk about the Brunswick and Hatch events.

(Slide)

MS. ZUKOR: What I am going to cover are the Hatch and Brunswick events. I will go through the Brunswick event first, mainly because I have more information.

I would like to preface this with the fact that we are still gathering information and that any conclusions are not necessarily final. There is information coming in, and our conjectures and judgments on what has happened and what may have to be done will probably change as more information becomes available.

On October 19, 1979, the Brunswick unit underwent a scram from full power. Following that scram, they noticed damaged pipe supports along the drain line below the scram discharge and instrument volume.

Now, this is a portion of the scram discharge volume. This is the instrument volume at Brunswick. Notice that there is no small two-inch line connecting the two. Brunswick is one of the new BWRs and that line has been eliminated in the BWR-6 design.

The pipe was damaged primarily on the south side, although the north side did undergo some damage and some of the switches. Essentially, the rod block and the high level alarm switch were damaged on Brunswick. This damage is believed to have been the result of a water hammer.

Now, this water hammer is not your classic steam or steam collapse water hammer. It is mainly a hydraulic event which we believe occurred when the system went solid and the vent valve and drain valve failed to close in a reasonable amount of time. These valves were normally closing between 25 and 35 seconds, and at this particular time the solenoid which controls both of those valves -- there is only one solenoid -- failed to close these valves in a reasonable amount of time. So it was taking about 5 minutes for these valves to close.

At this point this entire system could have gone solid and been that way when those vent and drain valves failed to close. After the event, the licensee sent an LER to NRC, and the NRC suggested that the licensee examine the swtiches because the damage to the drain line was of such an extent that it pulled some of the piping right out of the wall.

So they suspected that maybe the switches had undergone some damage. Now, the licensee had inspected the switches, but from a visual inspection, not an inspection where these switches were disassembled and radiographed. So while a new valve or new solenoid was ordered, the plant resumed operation with the vent and drain valve closed.

The idea behind this was that if it took too long for the vent and drain valve to close and that was an unsafe condition, then a very safe condition would be to have them close from the beginning so you would not have to worry about that problem. The idea was to open the drain and drain the system every hour.

Well, the drain valve happens to be in an extremely inaccessible place, so the operators decided that they would wait for the high level alarm switch, and when this switch came on, then they would drain the system. This proceeded until November 14th, where a scram was obtained on the high level scram switches, and none of the lower level switches had alarmed.

Neither the high level alarm switch had sent any indication nor the rod block switch. Upon investigation it was found that both of those switches were damaged to such an extent that they were inoperable.

(Slide)

Now, at this point since you still have fresh in your mind what Browns Ferry looks like, I would like to go through and show you the difference between the instrument volume on these two plants. Notice that the scram level switches come off, the scram level and the rod block level, come off the instrument volume itself. The alarm level is connected to the drain line.

This differs slightly from Browns Ferry where all of the switches are in line and all are connected to the drain line. We are not sure whether this is really a significant point or not, but it is something to watch out for. It is possible that the connection to the drain line may be a source of some of these problems.

(Slide)

This is a partial isometric of the Brunswick system. Notice that this is the alarm switch, this is the rod block withdrawal switch, and these are the scram switches. Again, this is the portion that is significantly different from Browns Ferry.

MR. LAWROSKI: Do you know when those switches were damaged?

MS. ZUKOR: We think it was October 19th. I don't think anyone would want to swear to that because they had not been disassembled and inspected to verify that they were undamaged previous to that.

(Slide)

Notice the difference between Browns Ferry and Brunswick. This is the long line. This line is completely missing in Brunswick. Also, Browns Ferry only has one instrument volume. Brunswick has one on each header.

(Slide)

This slide simply shows the initial configuration before scram so that your vent and drain valves are open, your system is empty, assuming it is draining properly.

(Slide)

At scram your vent and drain valves will close. The system will fill with water. Now, this is the portion that we believe caused the problem for Brunswick. In other words, these valves should have closed much more rapidly than they did.

In closing, on the order of 5 minutes it allowed this entire system to fill up. Now, this water in here is now at reactor pressure, so these valves when they finally do close are closing against full reactor pressure, which can create a transient that could go back and damage these switches.

MR. EBERSOLE: I am under the impression that the Browns Ferry valves closed instantaneously, more or less. Is that correct?

MS. ZUKOR: They closed, I believe, in about 15 seconds.

MR. EBERSOLE: They are deliberately time delayed. Is that correct?

MS. ZUKOR: I don't think so.

MR. EBERSOLE: They are accidently time delayed.

MS. ZUKOR: Yes. It takes that long for the air to bleed off the valves.

MR. EBERSOLE: That is a variable you can control any way you want.

MS. ZUKOR: And in some cases you cannot control it.

(Slide)

These are the switches that were damaged, and this is just a slight schematic that indicates roughly how they were damaged. These switches are hydraulically tested before they bleed the plant or their place of manufacture to 1625 psi. But this is a static test, basically. It is not done under dynamic conditions.

Again, notice that although there were no damaged switches on the south side, the rod block switch and the high level switch are not present on the south side. That is not to say that if they had been there, they would not have been damaged.

(Slide)

Upon investigation it was found that neither the drain piping nor the vent piping had been analyzed for any type of load. As a result of that, the systems had to be strengthened.

What I would like to show you now are some slides that indicate how those systems really look and how they are oriented and what are some of the adjustments that have been made in light of this event. What you see here is the header that comes down from the scram discharge volume into the instrument volume. Notice that the scram discharge volume and the instrument volume are basically one pipe which is simply increased in diameter as it goes into the instrument volume.

There is no clearcut cutoff between the scram discharge volume and the instrument volume. What you see off to the right here is one of the two scram switches.

(Slide)

This is the other scram switch. Some of this instrumentation is the instrumentation that was on the switches to do the NRC test to make sure that they were operating properly. This is the rod block withdrawal switch. This line will go into the scram discharge instrument volume.

(Slide)

Again, this is the rod block withdrawal switch going into the instrument volume and also connected to the alarm switch which is off the drain line.

(Slide)

This is the drain line coming out of the scram discharge instrument volume. No, this is the line which underwent some of the damage and which had pipe supports pulled off the walls.

(Slide)

This is what the floats looked like that had been taken out of the rod block withdrawal and the alarm switch. I cannot tell you which one, but they were equally destroyed. So I don't believe it makes a whole lot of difference.

(Slide)

As you can see, there is a rod that goes into the ball and it is welded at the ball. These balls are 304 stainless steel. There is no damage on the bottom or on the sides to indicate that there was any other event other than pushing the rod into the ball that damaged these floats.

(Slide)

Again, you can see that the float was damaged, and in some cases it was damaged enough to rip the float itself.

(Slide)

Now, I have seen some pictures of floats from Hatch, and they look very similar to this. They were damaged in essentially the same way.

(Slide)

This is a picture of a vent valve. Now, this line goes back into the vent header, which is one of the lines that Gary spoke about, which is a vent and a drain line. You can expect water to be in the line. You would not expect it to be full. This is where it ties in.

(Slide)

This goes to the reactor equipment drain tank.

(Slide)

This is a picture of pipe supports. This is where one of the previous pipe supports came out of the wall. This is a picture of the new pipe support. It is difficult to see because it is the same color, but I have another picture which will make it more clear.

(Slide)

Here is a picture of them side by side. Notice that the base plate is the primary change between the pipe supports. The licensee has sent the NRC a memo and indicated that should additional strengthening be required because of further analysis, they will perform that additional strengthening.

(Slide)

What I would like to do is go through the Hatch event a little bit. There is much less information. It is mostly data.

MR. BENDER: Before you go on, could you recap a couple of things? I guess I was not too clear on how the water hammer was initiated in the first place. Could you clear that up?

MS. ZUKOR: This is not cast in stone. This is how we believe it was initiated. The vent and the drain valve delayed open, so that normally you would expect to have in this system when this vent and drain valve closed. By delaying the opening of these two valves, it allowed this entire system to fill with water before this vent and drain valve slammed shut.

When it finally did shut, they were closing against not air but water that was at reactor pressure. So it is possible that by closing against that force, it created an impulse back into the system.

MR. BENDER: How many times has the opportunity for that event to occur occurred? Is there any feeling for that?

MS. ZUKOR: I cannot give you a number, but it has occurred more frequently than we originally suspected because upon looking back into the computer printouts and some of the LERs, we found that these vent and drain valves had failed on different occasions to fail in their expected time, and there have been damaged floats found.

MR. BENDER: Is it several times a year or several times a month or what?

MS. ZUKOR: Not even that. It is on the order of four events in the LERs that we have managed to look at so far.

Are there any other questions on Brunswick?

MR. EBERSOLE: Concerning the closing mode, what you suggest is these things approach closure and then they rather suddenly snap shut; they do not progressively close and thus diminish or break the acceleration of the water; is that correct?

MS. ZUKOR: We have not been able to get the complete characteristics of the valve yet, but we believe that is what it does.

MR. EBERSOLE: Thank you.

MR. LEWIS: Is there obvious rationale for not having a high level switch on the south instruments?

MS. ZUKOR: Not that I can find, no.

MR. LEWIS: I see.

MR. FRALEY: I think that is considered an alarm and not a safety function, so they don't duplicate it.

MR. LEWIS: From the north but not from the south.

MR. FRALEY: That is correct.

MS. ZUKOR: The alarms are for the convenience of the operator rather than for safety.

(Slide)

In Hatch-2 -- I am sorry, Hatch-1, on June 13th the Hatch-1 plant was shut down for refueling. At that time, two out of four scram switches were found to be inoperable, and this inoperability was believed to be due to an adjustment and maintenance which was done on the switches earlier.

Up until this time the switches had been operating properly and they had passed all their surveillance testing. However, when the LERs were examined and the events that involved the vent and drain valves, it had been found that on the 5th of May in 1980, the vent and drain valves had failed to close on trip and the damage was found on the vent line in May. So there are events -- these are not related because the shutdown was mainly for refueling, and Brunswick, as you know, underwent a scram and that is why they noted they effects.

But there are more events than originally thought of occurring on this particular system.

Now, I am not going to make any conjectures about Unit 1 having a drain system that looks like this because I found out that every one is different and the best thing you can do is not assume anything about them until you see the drawings.

MR. EBERSOLE: Could you throw the previous slide up for just a minute?

MR. PLESSET: Use your mike, Jesse.

MR. EBERSOLE: The one that shows the signal circuitry.

(Slide)

The question was asked about having only on one leg the annunciator. I think there is a generic thing here that ought to be called out. This is a classic example of the fact that operator information fed to the operator is single track and non safety grade, and a low grade in general, not in a class with automatic safety functions, which you notice in the scram systems are duplicated.

There is the one shot pitch to the operator that this level exists. The other case is duplicated, and that is typical of most all information fed to operators on which they base their safety actions, which is one of the generic problems we have.

Heretofore, we have always considered operator information non-safety grade in character. This is just an example of that.

(Slide.)

MS. ZUKOR: Now, Hatch-2 has undergone similar damage to Brunswick. I have seen pictures of the floats and they are partially collapsed. In fact, they look very similar to the Brunswick floats that were damaged.

However, the damage that occurred sort of precludes the assumption that it is all connected with the drain line, because one of the floats that was damaged was on the north bank and one was on the south bank; and these were the scram switches, as opposed to the rod block withdrawal or the alarm switches. So as of yet, we are still not sure exactly what the mechanism is that is damaging these plates.

MR. SHEWMON: Normally, one of the two should see the same pressure there.

MS. ZUKOR: Correct.

MR. BENDER: This was learned by physical inspection as opposed to symptomatic indications?

MS. ZUKOR: The float damage?

MR. BENDER: Yes.

MS. ZUKOR: Yes, because the floats are in a housing. As you saw, you cannot see any of the floats from the outside. You have to either disassemble the system or radiograph it, and I get the impression that even radiography does not show it. You almost have to disassemble them to see that they are okay.

MR. BENDER: And this was learned as a result of the NRC all points bulletin or whatever it was?

MS. ZUKOR: Correct.

MR. BENDER: Correct.

MR. FRALEY: Which scram switch was damaged?

MS. ZUKOR: I don't know precisely which one. One on this side and one on this side.

MR. FRALEY: One on each side.

MS. ZUKOR: And right now that is all the information we have available to us on Hatch, and more information is expected to come in.

MR. SIESS: Dot, near the beginning you said something about the design, with the two scram discharge instrument volumes, that GE had had a new design for BWR-6's, and I think that was the term you used.

MS. ZUKOR: Gary is going to go through that.

MR. SIESS: I got the impression earlier that these systems really were not designed by GE; that they just specified volume and it was up to the AE or subcontractor. Does that mean that GE is telling people what they want?

MS. ZUKOR: Yes. They have a suggested design.

They cannot require their licensees to use the suggested design.

MR. SIESS: Did they have one before?

MS. ZUKOR: I do not believe so. As a result, they had a subcontractor --

MR. SIESS: It would be interesting to know why they have now suggested something that is good, where before they did not suggest anything, including what was not good.

MR. PLESSET: There was one outfit which designed these.

MR. SIESS: I know that. But Dot says GE now has a suggested design, and I got the impression that previously they did not even have a suggested design. They just said, you want 3.3 gallons, they said, that is right. I wondered what they learned or what happened to cause them to change their policy and suggest a design which is a good design, it looks like, in terms of the volumes and the hydraulics.

MS. ZUKOR: I do not think they had a suggested design. The scram discharge volumes themselves are very similar. Where they tend to differentiate is below the scram discharge volume into the instrument volume.

MR. EBERSOLE: I think they found that general criteria and specifications were insufficient.

MR. SIESS: If that were true, this would be reportable, because it would be a safety deficiency on all the previous plants, wouldn't it? I am not a lawyer. Is there one present? They seem to decide those things.

MR. BENDER: I recall a generic item that referred to interfacial relationships between balance of plant and the primary -- the nuclear system. And we took it out because the regulatory staff said they had now established a requirement for checking those interfaces.

MR. SIESS: We only got concerned about interfaces with standard plants, and I don't think any of these are standard designs. These are all just so-called "custom designs." Custom designs are made by different people. We never talked about interfaces on custom designs.

MR. FRALEY: Apparently, somehow GE did get the word, as history moved forward, to put in the hockey stick versus the drain line and to put in DP cells versus the floats, which were evolutions in this system design, which did get passed on to the AE's, but we are not quite sure how. Because you can see this evolution over the years.

MR. SIESS: What I am interested in is what -- did something happen that made them realize this was an improvement or did somebody just sit down and think of it and say, gee, this is an improvement? I don't think anybody has found any previous incidents of flow blockage.

MS. ZUKOR: No, no.

MR. SIESS: And did they occur and not be --

MS. ZUKOR: I believe that question was asked of GE and there was no real response that said why they changed.

MR. SIESS: Here we have some bad systems and some good systems. There was a reason for the good systems. Apparently they were not completely accidental. And I would sort of like to know why we get good things sometimes, as well as why we get bad ones.

MR. PLESSET: Thank you, Dot.

Let's ,o back to Gary

MR. YOUNG: Okay. I would like to briefly go to the recommendations that were made by the NRC Office of Analysis and Evaluation of Operating Data. They reviewed the Browns Ferry 3 event specifically and came to some -- had some findings and some recommendations. The findings are listed in the handout. I will not go through those. But the final recommendations are shown here.

(Slide).

And the first recommendation is that the operability of the instrument volume high-level trip or scram system should be independent of venting and draining requirements. And this goes back to the fact that at Browns Ferry the drain problem presented in the instrument volume from seeing the water and initiating a scram when they have water there.

So he is recommending that the operability of this system be independent of venting and draining. I will show later how that can be accomplished.

They recommend that the instrument volume instruments be redundant and diverse. And the reason for that is obviously that there are problems with the instrumentation, such as was found at Brunswick and Hatch, and that you cannot always rely on this instrumentation, on these floats, to work properly. So they are recommending that they be -- they already are redundant, but they are not diverse.

MR. BENDER: Gary, if you satisfy one, is two important?

MR. YOUNG: Yes.

MR. BENDER: Why?

MR. YOUNG: Because the instruments -- if you just satisfy one, if you made a system where the instruments will see the water if there is water there, but if they are unreliable they may not cause a scram and, number two, make sure that they are reliable, diverse, and redundant, they will initiate a scram if water is present. So they really are directly connected.

MR. BENDER: Thank you.

MR. YOUNG: The third item is that all the vent and drain paths from the scram discharge volume should be redundant automatic isolation valves. Right now there is one vent valve on each of the headers and one drain valve on the instrument volume.

The concern there was the single failure of any one of those valves would result in a leakage of primary system water following a scram. So they are recommending that they become redundant valves instead of just a single valve that they have now. And this is to assure that the system is bottled up following a scram.

The fourth item concerns operating procedures to be followed during the complete or partial scram failure. Apparently, at Browns Ferry 3 there was concern that the operator did not initiate the standby liquid control system or the boron injection system, and he would have needed some upper management approval to do so, when in fact the decision probably should have been made much quicker.

So this is not true at all plants, but apparently at Browns Ferry that was a problem.

And then the fifth item is a recommendation -- it is not really safety-related. It says: Consider modifying the vent drain arrangement to improve drain reliability. It goes back to the problem of having a vent drain tied into a drain system that might not be able to adequately vent.

So this was more, as I said -- as I say, consider it. It is not a necessity if you do all the other things.

GE has made a recommendation, preliminary recommendation, on a configuration that would meet all of the items that I just discussed.

(Slide).

This is the hockey stick arrangement that exists on the newer plants. This is the scram volume, and then the instrument volum. is an integral part. It has no drain line between the two. Therefore, any water that gets in the system, because it is slow, will drain into the instrument volume and therefore you cannot have the situation where there is water up here and not down here.

And that would be independent of a vent or a drain on the system. It would not matter if the vent drain worked; water would still fall to the lowest point, and then the instrumentation they showed here has been slightly modified to not connect with the drain line any more. It connects directly with the instrument volume.

And this was, I think, an attempt to correct the water hammer problems at Brunswick and Hatch. It is not a definite solution because no one really knows what caused the problem at this point.

MR. BENDER: Was this possibility considered in regard to --

MR. YOUNG: I believe the NRC staff is looking into -- this is just a recommendation that they -- that GE has made. And I believe during their presentation the staff will discuss some of that, the other possibilities.

MR. BENDER: Thank you.

MR. PLESSET: I have to make a remark. We let Gary pass over pretty easily the remark about the operator needing upper management approval for boron injection. It depends on what kind of situation you are in as to whether one can do it or not without management intercession.

MR. SHEWMON: Is management the SRO on duty or do they have to call up the front office or something?

MR. YOUNG: It is the on-plant management, the shift supervisor.

MR. SHEWMON: But that person is not necessarily in the control room. He is available someplace, hopefully, in the plant.

MR. YOUNG: Yes, he is definitely in the plant. And I think normally he would be very close to the control room.

MR. EBERSOLE: It requires twice as many instruments to accomplish the same function as the old design did. Therefore, it is half as reliable.

MR. YOUNG: It requires twice as many instruments, but I believe -- for example, if water got into this header, it would scram the plant. So it is really kind of redundant.

MR. EBERSOLE: Wait a minute. These hav e to be coincident on both sides.

MR. YOUNG: No, sir. Any water in either header would scram the plant.

MR. EBERSOLE: They have rigged it for A and B channels?

MR. YOUNG: It is similar to the design that already exists at Brunswick. It is really the same type of configuration.

MR. EBERSOLE: You will still get scram?

MR. YOUNG: I don't believe that is true. I believe you've got water in just one of these headers; then you would scram the plant.

MR. KERR: Can't we find out that -- don't you get scram, supposedly, while you still have enough volume left to produce scram?

MR. EBERSOLE: If you have scram and you fill it up, then you don't have a second option.

MR. YOUNG: The half-scram does not cause a rod -- the rods don't move at all on a half-scram.

MR. EBERSOLE: I mean a half-stroke.

MR. YOUNG: A half-stroke, I see, okay.

If there are not any more questions, Dot Zukor will talk a little bit about the WASH-1400 comparisons of these events.

(Slide).

MS. ZUKOR: As a result of analyzing the scram systems, we have more or less generally concluded that the BWR scram systems are not as reliable as originally expected and thought.

(Laughter).

And as a result, some of the calculations done in WASH-1400 are a little bit overly conservative that the system will work. Now, what I have here is a fault tree for the analysis done for a failure to complete a successful scram for Peach Bottom. This is similar to the Browns Ferry plant in that it has that two-inch drain line. At least now it does. Soon it will be changed. But it still has the two-inch drain line.

Now, previously this was the only mechanism thought of that would block the system, and you usually have water entering the header, as Gary mentioned, because you have some leakage into the system. Once that satisfies that end gate, it can continue to go up to the top, where you have a failure to successfully scram.

This number here is on the order of 10^{-7}, which is extremely small, which you would usually expect for a dead leg pipe that is basically a passive component.

(Slide).

Now, what I have done with this system is to include the fact that if this header fails to drain -- and it can fail to drain for more than one reason, not necessarily that particular two-inch line. It can fail to drain for a number of reasons:

One is if the scram discharge instrument volume fails to drain, and that can be because the drain valve fails to operate or the level instruments fail to function, which has been found to be not an uncommon occurrence.

(Slide).

The trip header vent line can fail to vent, again for a similar reason. You have your vent valve failing to operate.

Now, what this would do for a plant like Browns Ferry is simply slow the rate of drainage down. But it may be significant enough to prevent the system from draining properly, or your vent line itself is plugged. Again, you have the original vent which was on the original WASH-1400 fault tree, which says, okay, the drain line is blocked.

Now, what reduces this probability of failure -- and you have it on your handouts; I did not write it on the transparency -- is that the vent valve and the drain valve, because of their active compatibility, have a much higher probability of failing than, say, this class of component here.

So your trip header failing to drain becomes something on the order of 10^{-3} or 10^{-4} rather than

-7

10 . Notice, however, that at Browns Ferry there is the possibility that that was precisely what happened, even though it is a very low probability event. It could have been that that system was drained.

Now, after the event, the whole scram discharge instrument volume at Browns Ferry was cleaned very completely. So it is possible that we will not find out where that plug was, or if in fact there was one.

MR. SHEWMON: Did they analyze what came out of it besides water?

MS. ZUKOR: Yes. They found some crud, but nothing that was significant to make them think it would have blocked the system. And at places where there were bends and turns, they cut the turns out and checked the system to make sure that there were no plugs in there. And they were not able to find any.

MR. FRALEY: There was one other thing. Dot did not include in here the probability of a design deficiency which would increase the probability of such a failure, and that in effect is what they have concluded happened at Browns Ferry. This long run of pipe that would not drain, you know, that let the SDIV drain faster than the scram discharge volume, which was really a design deficiency, which would have been another leg to this fault tree.

MR. LEWIS: I don't entirely understand the definition of "probability" here, Dot, because 10 to the minus -- normally, the probability is that when you call on something it will not perform, whereas here a blockage or a filling of a header can be a long-term affair. So the probability is that when you look for a scram, you will find that it actually was blocked. That depends on how often you look for a scram.

MS. ZUKOR: Yes.

If there are no more questions, I have completed my presentation.

MR. PLESSET: Any other questions?

MR. LEWIS: I would like the record to show that the Chairman promised me the answer to a question earlier.

MR. PLESSET: We will still get that answer for you. Don't worry. Just stick around.

MR. LEWIS: Is that the price I have to pay?

MR. PLESSET: That is the price.

Well, I want to thank both Gary and Dot for their presentations, which I think were rather models of clarity. And the Committee rarely has that kind of an opportunity to be exposed to clarity. Let me put it that way.

Well, let's take a ten-minute recess.

(Recess.)

MR. PLESSET: Let's reconvene.

It would be moderately appropriate since Gary is an ex-ACRS fellow and Dot is still an ACRS fellow, to introduce new ones to the committee, and maybe I can ask them to appear.

Stu Biehl, would you stand up? The new ACRS fellow. Thank you. And Bill Baldowitz up there. Thank you.

So now the committee members will be getting onto you pretty quick for all kinds of interesting tasks.

Well, let's go on -- we are running a little late -- the staff presentation. And I think that Mr. Speis is going to take over. Would you go ahead?

MR. SPEIS: Thank you, Mr. Chairman.

I am Thomas Speis of the Office of Nuclear Reactor Regulation. Before I make some opening remarks I would like to congratulate the fellows for doing such an outstanding job, and our people will attempt to be as clear as possible.

We have a number of items to go over today, but we also have in mind to go through the description of the event and the description of the design, but that part of the presentation will be abbreviated since it has been given already by the -- by your fellows.

We will concentrate basically on the actions that the NRC has taken since the events, of both short-term actions taken and short-term actions underway, as well as longer term actions. Also, we will cover analyses covering "what if" type of questions, assuming -- superimposed -- if you had some limiting transients. And also we will say a few things about the implications for the ATWS position.

I would like to ask you if you think -- by the way, our presentation is probably an hour and a half with a moderate amount of questions. Mr. Thadani will be making the presentation on the implications of ATWS, but he has to leave here about ten after 12:00 to go to a Commission meeting. So if you think our presentations will go beyond a quarter after 12:00, we would like to get him after Mr. Mills makes his initial presentation.

MR. PLESSET: That is fine. Let's do that.

Now, you remember we promised Mr. Lewis some --

MR. SPEIS: Is this the analysis of the event?

MR. PLESSET: Yes.

MR. SPEIS: This will be presented today also. We have a number of staff members, and they will be introducing themselves as they make their presentations. In addition to myself, there is Ed Jordan from the Office of Inspection. He will help orchestrate the staff's presentation.

So with no further remarks, Mr. Mills will start the presentations.

MR. PLESSET: Thank you.

MR. MILLS: My name is Bill Mills. I am a member of the IE staff.

(Slide.)

Since Gary and Dorothy have discussed the partial scram at Browns Ferry-3 and the Hatch and Brunswick events, I will briefly summarize those and primarily discuss the concerns of the Browns Ferry-3 event as raised within the staff, the short-term actions we have taken through Bulletin 80-17 and related ongoing short-term actions, and our conclusions on the Browns Ferry-3 event.

(Slide.)

This slide shows a simplified diagram of the scram discharge volume at Browns Ferry, and as previously stated this morning, the function of the -- the function of the scram discharge volume is to receive exhaust water from the control rod drives during reactor scram.

During normal operation prior to scram, scram discharge volume is empty, the vent and drain valves are open, and leakage into the system drains into the instrument volume of the drain line. Level switches are provided on the instrument volume to detect an accumulation of water.

In the Browns Ferry-3 partial scram event, an accumulation of water in the east scram discharge volume caused a failure of the rods to scram. Even though the exact cause for that accumulation of water is not known, the basic problem with the system is the poor communication between the scram discharge volume and the instrument volume.

These two volumes are connected, as previously stated, by approximately 150 feet of two-inch piping. This makes it possible under certain conditions to accumulate water in the scram discharge volume without detection in the instrument volume.

As I will discuss later, we have taken corrective actions to ensure that this does not happen again. I will discuss them in more detail later. But briefly, we have required that the vent and drain valves be operable, and that has to be verified, and verification that the vent lines are free of obstruction, that this system is free to drain. And also we require direct monitoring of the scram discharge volume, besides the normal instrumentation provided here.

We have also required that following each scram, all of these level switches be functionally tested with the injection of water to make sure that they are operable. This is the result of the Hatch and the Brunswick concerns with the damaged lines.

MR. BENDER: Why are you insisting on the operability of the vents?

MR. MILLS: If you have an ineffective vent, then the draining of the scram discharge volume will be degraded.

MR. BENDER: Will it be ineffective?

MR. MILLS: It can be, yes. It depends on the leak rate into the scram discharge volume, and there are two cases. One would be following a scram in which you started with the system full, and you would get a slow draining rate. But during normal operation with the system empty you would get a better draining rate even if the vent were plugged.

MR. CARBON: How sensitive are your devices on the scram discharge volume? Will they pick up the thing half full, ten percent full?

MR. MILLS: Within about one inch.

MR. CARBON: How big is the volume?

MR. MILLS: The pipe size varies from plant to plant, and they can take measurements on various places. Usually they take it right near the reducer from the six-inch pipe to the two-inch pipe.

At Browns Ferry currently they have a UT monitor there. They believe that the accuracy of that is within, it is either a quarter or a half inch, so that would be out of a six-inch pipe, a quarter or a half inch, so it is less than an inch.

MR. JORDAN: We can add to that that that volume is also sloped, so we are looking at the accumulation in the sloped end of it.

(Slide.)

MR. CARBON: One more question. Are those devices required before the plants can return to operation, or what is the schedule?

MR. MILLS: Yes, it is required following scram before return to operation that each of those switches be operable and functionally tested.

The Browns Ferry-3 event caused the staff to question the reliability of the scram function, and our understanding of the as-built scram discharge volume configuration.

We determined that short-term corrective actions were needed to justify continued operation, and that long-term corrective actions were needed to provide a new scram discharge volume design, improved reliability, and that implementation of ATWS-related procedures and not applications were necessary.

MR. EBERSOLE: All events related to BWRs, isn't there also an implication here that there are subtleties in design detail that may well be present at the PWR design analogous to this one into which we have never looked, such as undervoltage relay functions which may present a problem?

MR. MILLS: That is correct, and I think that is a good point; and that is part of what the NRR task force will consider in their longterm actions.

MR. MOELLER: In terms of the first item, the reliability of the scram function, I am looking at this memorandum of August 22 from Denwood Ross to Harold Denton with an attachment, and it says that "The PAS group conclude that the Browns Ferry experience does not negate the validity of prior probabilistic estimates set forth in WASH-1400."

And then the next paragraph says that "It is clear that the occurrence has created considerable concern regarding the reliability of the GE scram system, and that the failure rate or failure estimate has been revised."

Are those two statements compatible, and could you explain them in relation to what -- to this aspect of the problem?

MR. PLESSET: I think Speis wants to make a response.

MR. SPEIS: What are you reading from?

MR. MOELLER: I am reading from the attachment to this memorandum of August 22, 1980. I can show it to you.

MR. PLESSET: That is a memo from Denny Ross.

MR. MOELLER: Page 3, item 5 and 6.

MR. SPEIS: We will address it later on.

MR. PLESSET: Okay, fine. Why don't you go on then?

(Slide.)

MR. MILLS: The staff took immediate corrective action following the Browns Ferry-3 event to ensure that an event of that type did not reoccur. Actions were taken both through an 80-17 -- the requirements of that bulletin are listed here.

For all BWRs it was required that within three days they verify that the scram discharge volume is empty and operable. By "operable" we mean that the vent and drain valves are operable, that the vent line is free from obstruction, and that the system is empty and draining -- and will drain.

We also required scram tests to be performed to confirm that no significant problems existed and to provide data on the operation of the scram discharge volume and its draining characteristics.

We required that the scram discharge volume be verified empty after the scram tests and after all other scrams that occurred. We also required procedures for monitoring the scram discharge volume daily for an accumulation of water.

They were required to have emergency operating procedures to ensure that operator actions were adequate for an event of the Browns Ferry-3 type.

In addition, actions were specified in the bulletin to be taken to mitigate the consequences of an ATWS event.

(Slide.)

After issuance of the original bulletin 80-17, Supplement 1 was issued. This supplement was issued following further staff review, and the information that was received on as-built configurations.

Supplement 1 required continuous monitoring of the scram discharge volume to be installed by September 1 or additional actions had to be taken. A design review of the vent system to find ways to improve the venting of the scram discharge volume. Procedural controls for the use of standby liquid control, we required that the key be maintained in the control room, and the operator be provided with procedures and criteria such that he could make the decision without management approval, that he could initiate standby liquid control if it were deemed necessary.

We also required a verification of as-built drawings for the scram discharge volume, and in particular, interties with the vents and drains to identify any deficiencies.

MR. EBERSOLE: We asked TVA in the L.A. meeting about the continuous monitoring. Subsequently we found out there was a control that ran by every thirty minutes that looked at a continously monitored signal but was viewed by operators only once every half hour, so this left a vast exposure.

If an accidental fill takes place in the 29 minutes that the operator was not looking at it -- was that not subsequently corrected?

MR. MILLS: I will be discussing that.

MR. EBERSOLE: Okay.

MR. MILLS: You are correct. They have a continous monitor there, and they have one individual who is assigned to go from one unit to the other and look at those; but I will get into that a little later.

MR. BENDER: I heard some words like requiring the key, I think you said key to the standby liquid control system, the actuator, I guess, be in the control room.

MR. MILLS: Correct.

MR. BENDER: That tells me something that I guess I was not aware of. Is there the possibility that some guy could take the key and walk out and there would be no way to actuate the liquid control system because it is locked up?

MR. MILLS: I do not think that that is a realistic possibility for a couple of reasons. They also require it be in a designated location.

MR. BENDER: I don't care about the key. If the key is not there, can you actuate the liquid control system?

MR. MILLS: No. You need a key in the switch in order to actuate. It has to be a deliberate action.

MR. EBERSOLE: This will bring up a fundamental issue.

MR. BENDER: Well, we can think about that some.

MR. EBERSOLE: I think it is a good time to bring up a fundamental issue, and that is whether one should use keys or frangible seals like lead-sealed wires. And in my own view, a key is infinitely worse than a frangible seal, which is clearly indicative of having been pre -- is a clear preventitive for any inadvertent action.

I wish we would get rid of the keys and put in frangible seals.

MR. MILLS: As you are aware, we have positions for installation of equipment to mitigate ATWS, and in there we are looking for automatic initiation of a standby liquid control. So there are actions going on that would eliminate that problem in the future.

MR. PLESSET: That might be a long time coming.

MR. MILLS: That is true.

(Slide.)

Supplement 2 to Bulletin 80-17 was issued after testing at Browns Ferry-1 and Dresden-3. It highlighted the importance of the event on a scram discharge volume. This supplement required that for each BWR, the vent for the scram discharge volume rely on no component other than the scram -- excuse me, other than the vent valve, and that the vent must be positive in its function at all times, directly connected to reactor building atmosphere.

If the vent configuration did not conform with that requirement, they were to make modifications within 48 hours. As a result, approximately 15 plants modified their vent system.

(Slide.)

MR. LEWIS: What does the term "positive in its function at all times" mean?

MR. MILLS: That it be open.

MR. LEWIS: It means that there is no valve in --

MR. MILLS: There is no closed valve, and no component has to operate to provide the function, and that the line be free and clear to the atmosphere.

MR. LEWIS: If there is a valve that can fail, of course -- I'm trying to understand the "at all times" -- that means there is literally nothing in the line.

MR. MILLS: Right. It is open. It is free to communicate with the atmosphere.

MR. LEWIS: Okay.

MR. MILLS: Supplement 3 was issued following a concern that was raised by Mr. Michaelson of AEOD that loss of air pressure in the scram discharge volume could result in filling the scram discharge volume prior to reactor scram.

The bulletin required procedures to ensure that operator action took place so an event of this type would not occur, and it required immediate manual scram of the reactor on low air pressure or other signals that would be indicative of a loss of air pressure in the CRD system.

And I would point out that there are alarms directly on the CRD system downstream of the pressure regulator for the CRD system, so that that pressure is monitored directly to the scram valves.

MR. KERR: What does "low" mean?

MR. MILLS: In this case it means 10 pounds. There is a margin of 10 pounds above the pressure at which the valves would start to open.

We also required that the functional tests of all level switches be performed following each scram and prior to reactor startup because of the problems we have observed with those switches.

MR. PLESSET: Have you made any suggestions regarding the kinds of switches they might use?

MR. JORDAN: In terms of the level indicating switches?

MR. PLESSET: Yes.

MR. JORDAN: The next fix or the measurement on the volume after September 1 is generally going to be a UT-type level fix.

MR. PLESSET: That is general now. I knew TVA was going to do that. Now you are making that a general.

MR. JORDAN: We did not specify. We were not prescriptive as to what type switch. We were looking for diversity between these ball-type float switches and the utilities in general, and through GE selected the UT level measurement.

MR. PLESSET: Okay. Thank you.

MR. MILLS: Our status so far, all plants have responded to both 80-17 through Supplement 2. Scram tests have been completed on all plants except Brunswick-2 which is currently shut down for an outage.

Our review of the responses is ongoing. However, our screening and information so far has identified some scram deficiencies which I will discuss in a minute.

Responses to 80-17 through Supplement 2 have been satisfactory for all plants. They have implemented procedure changes, modification to the vent as necessary, and in general the requirements in the bulletin.

There is one exception and as we have discussed, we required continuous monitoring be installed by September 1 on the scram discharge volume for an accumulation of water or that additional actions had to be taken.

In the responses we found out that most plants will not have the continous monitoring installed by September 1. By "continuous" we mean continuously recorded in the control room and alarmed in the control room.

So getting back to your point, the current Browns Ferry configuration would not satisfy this requirement.

MR. EBERSOLE: How long will that be allowed to persist?

MR. MILLS: I will cover that in just a minute.

The additional actions that were required in the bulletin were that if they could not have it in by September 1, give us a detailed explanation as to why it could not be in, provide a firm schedule for installation of the continuous monitoring, and increase the frequency at which it is monitored.

Right now it is on a daily frequency, and we were looking for something shorter between the tests.

(Slide.)

This slide shows the scram system deficiencies which have turned up so far. You notice the first two and the last one are concerned with the float damage that has been observed at Brunswick and Hatch.

I would like to point out that our involvement with the float damage at Hatch and Brunswick and prior to the Browns Ferry-3 event, the Operational Events Evaluation Branch was established in I&E after the Three Mile Island event. However, it was established after the Brunswick and the Hatch events had occurred.

But during our review of operating experience, besides keeping up with the current operational problems, we went back into past problems as time permitted. That is when we uncovered these events, recognized the significance of the events, had discussions with NRR that resulted in the issuance of Bulletin 80-14 prior to the Browns Ferry-3 event.

The other scram deficiencies identified here, I do not plan on going through each one. These have been discussed briefly in the bulletin or one of the supplements previously, except for the Fitzpatrick problem which was found most recently when they returned from a refueling outage and did their 80-17 scram tests. They found they had a small loop seal in the drain piping from the discharge volume to the instrument volume.

The system did drain properly except a small amount of water was retained in that loop seal.

MR. MOELLER: You were not going to discuss them, but can you give us a rough date for the Hatch-2 event?

MR. MILLS: I can give you an exact date.

MR. ZUKOR: I think it was June the 26th.

MR. MOELLER: I wanted to mention that because the emphasis in the presentation has been on Bulletin 80-17. On June the 12th they issued Bulletin 80-14, and you had had failure in Hatch-1, and this was issued then on June the 12th. And if on June the 26th, then two weeks later, Hatch-2 has a problem, I am surprised somewhat.

What did Bulletin 80-14 accomplish? Here we have not only the same utility but the same station in presumably a duplicate unit having a problem that had occurred in the other unit and which was the subject of a bulletin.

MR. MILLS: The failures at Hatch-2 were discovered by July 26th.

MR. MOELLER: July, that is even worse. So it is six weeks.

MR. MILLS: And they were found as a result of testing from Bulletin 80-17, not Bulletin 80-14.

MR. MOELLER: What did Bulletin 80-14 accomplish then? What was its intent?

MR. MILLS: This particular unit right here was the only unit that was not injecting water directly into the float chamber to test the operation of the float. They were doing their functional testing just by manually actuating the electrical portion of the switch, and when they responded to Bulletin 80-14, they did not recognize that that is how they were doing their tests. So as a result, their failures went undetected, and it was subsequent in the 80-17 testing that they realized the problem that they had.

MR. MOELLER: That helps some. Thank you.

MR. EBERSOLE: Did they just take the panel off and move the panel up and down?

MR. MILLS: I think all they did was actuate the contacts on the switch, close them, and saw that they got the proper response in the protective system.

MR. ETHERINGTON: Was that a reasonable misunderstanding or should they have understood what was intended?

MR. MILLS: The reason that they did that is that they have the standard tech spec, versus other plants which have the older form of tech spec. And it was the new tech spec that led them to do this type of test the way the functional test is described in the standard tech spec. The frequency of the functional test was increased at Hatch-2 by the standard tech spec. It was moved from quarterly to monthly, so they did it more often. But they did not do it with the injection of water.

MR. JORDAN: I will answer that. It was a very unreasonable response on their part, based on the intent of the bulletin.

MR. KERR: I cannot understand the response of the presenter, because he seemed to imply that they did a test specified by tech specs, and you are saying --

MR. JORDAN: I would like to separate the bulletin from the tech specs, and say that the bullet was identifying a problem to them and they clearly should have responded to that particular problem, that there was damage occurring to the floats, and they should test them in a direct manner, not an indirect.

MR. KERR: I don't understand the tech spec comment. What was it meant to be?

MR. JORDAN: That was the licensee's excuse, not the staff's position at all.

MR. KERR: I guess I do not understand the comment. What was his comment meant to tell us?

MR. MILLS: The plant definitely made a mistake, and they did respond when they found their mistake and reported that they had made an error in their response to Bulletin 80-14.

MR. KERR: What did the standard tech spec versus a non-standard tech spec have to do with it? I am curious. I still do not understand.

MR. THATCHER: The standard tech specs have defined channel functional tests, and they defined it for both an analogue channel, where you are using some device, like the transmitter, that puts out an analogue signal and from that you get an actuation signal; and they also define a channel function test for what they call a bistable channel.

But there is potential for someone reading those and misinterpreting exactly what is being said. The words in those definitions are to the effect that one should inject a signal as close to the monitored parameter as possible. So if someone chose to, they could misinterpret those words and change the tests, which as I understood, they did start out with a channel test, channel functional test, that included adding water. Is that right, at Hatch-2? At Hatch-1 they were doing that.

MR. MILLS: I am not sure on Hatch-2.

MR. ETHERINGTON: I think they chose to misunderstand. I think that is what you are saying.

MR. THATCHER: I think if you read documents like IEEE 279 and so forth, I don't think you could misinterpret those statements. However, the words in the tech specs are not --

MR. KERR: Has any thought been given to the possibility that maybe the wording in the tech specs could be changed so they could not be misinterpreted?

MR. THATCHER: I personally don't know if they are thinking about doing that.

MR. MILLS: The tech specs --

MR. KERR: Since these are standard tech specs --

MR. MILLS: There were previously deficiencies in the tech specs, and we required that the tech specs be modified to have functional tests that directly inject water into those float chambers.

MR. KERR: Okay. So the tech specs now could not be misunderstood, even deliberately.

(Laughter).

MR. THATCHER: No, I would not say that.

MR. OKRENT: Let's see --

MR. MILLS: The requirements --

MR. OKRENT: Are you suggesting they be prescriptive?

(Laughter).

MR. KERR: I don't see how one avoids being prescriptive in tech specs. That is what they are meant to do.

MR. OKRENT: Well, but I think it would still follow the same idea, that what you are trying to have the licensee do is test the things in an adequate fashion, at a certain frequency. And that would in principle say, we are now not being prescriptive; we are telling the licensee that there is a certain period in which he should test and --

MR. KERR: I am suggesting they be as unambiguous as possible, then.

MR. OKRENT: I would say unambiguous, but not prescriptive.

MR. KERR: It prescribes -- well, I agree with you.

MR. SIESS: Would you buy that standardized tech specs have to be prescriptive?

MR. OKRENT: We frequently pick on the staff because they are too prescriptive, and --

MR. KERR: Tech specs by definition tell one specifically the operating conditions. They give things like power level, temperatures, and they use numbers. And they in that sense have to be prescriptive.

MR. MILLS: I would like to point out that our bulletin was very non-ambiguous as to what they had to do with those switches; that they had to inject water and test them with the use of water; and also that the tech specs had to reflect that. And sample tech specs were sent out and new tech specs will be coming in to reflect the bulletio.

(Slide).

Our short-term actions are ongoing. We are reviewing responses to 80-17. We have an ongoing review to identify the need for improvement in other areas of the control rod drive system. And we are considering further actions to require installation of continuous monitoring of the scram discharge volume by December of 1980, monitoring once per shift in the interim.

MR. EBERSOLE: How do you arrive at the adequacy of a once per shift, rather than once every three days or once every five minutes or once every ten seconds? What is the basis of your decision?

MR. MILLS: I personally feel that once per day, as currently required, would detect the accumulation of water in the scram discharge volume.

MR. EBERSOLE: At what rate of introduction, and what is the basis for that?

MR. MILLS: The basis is that if it were to come in -- if it were to come in at a rate faster, such that it would fill up in less than 24 hours, we would get indications either from rod drift, rod temperature alarm, or low air pressure, and that action would be taken from those other considerations.

MR. EBERSOLE: Has that relationship been established by calculation?

MR. MILLS: I am not sure what kind of calculations you are referring to.

MR. EBERSOLE: The degree of leakage per unit time, according to the condition of valves or the condition of the air pressure or whatever. I mean, it seems to be a quantitative problem.

MR. MILLS: It is, and I think all the mechanisms that we have identified so far for getting water into the scram discharge valve we feel are -- we feel that it is appropriate for those leak rates that could exist into the volume and out of the discharge volume, as well as not being able to detect it in the instrument volume.

MR. EBERSOLE: That would include a stuck rod valve, rod jump valve?

MR. MILLS: Yes, because if it were partially stuck open, if the value were low enough that it did not give an indication on the temperature alarm or a rod drift, and it was just one of them, then that would be a low enough volume that the scram discharge volume would not fill up within the 24 hours.

MR. ETHERINGTON: Couldn't you get leakage from the cooling water without actuating the temperature alarm?

MR. EBERSOLE: The water coming in is from the cool water side. There is no mixing. It progresses up from the bottom to the top.

MR. MILLS: There is a low enough value at which you could get, let's say, less than .1 gpm; you could get leakage out of that drive such that you did not actuate the temperature alarm. But for the drive the way it is constructed, if the leakage gets to approximately .1 gpm you will get a temperature alarm.

MR. BENDER: Who is responsible for the second item up there?

MR. MILLS: This is independent of the long-term effort that is going on in NRR. This is part of an I&E

review that we do of operational events and BWR operating experience.

MR. BENDER: I am not really clear on what was meant by that. I would presume that somebody is looking at the need for alterations in the design of some sort. Is anything intended more than an LER review?

MR. JORDAN: Not for that item. That was on the short-term actions, and so we were talking about, you know, immediately related to this event. We are still looking at this event and the facets of it that could contribute further. We are still talking short-term. So the design changes you are talking about are long-term and they will be discussed separately.

MR. BENDER: Okay. How many LERs exist that relate to this item right now?

MR. JORDAN: I don't know off the top of my head.

MR. BENDER: Is there any group looking at the LERs?

MR. PLESSET: Dorothy confided in me that she thinks there are something like 12.

MR. BENDER: I understand Dorothy's review. Dorothy is on the ACRS staff. I want to know what the NRC regulatory staff is doing.

MR. JORDAN: There are two groups that are doing it, both AEOD and NRR, you know, that are doing systematic reviews of that data now.

I will say that the I&E review group is more on real time, the events of today, and we are only looking back a very short time. So we are not doing a constant review of all LERs in a systematic way. We are looking at the current events, their relationship to recollected events, and then the short-term actions. So that's how our office is interacting.

MR. BENDER: I heard what you said. But why aren't the LERs being looked at in a systematic way?

MR. JORDAN: They are, but not by I&E.

MR. BENDER: Who is doing it?

MR. JORDAN: AEOD and NRR. That was my first answer.

MR. BENDER: Is there a name?

MR. SPEIS: It is the NRR responsibility to review LERs.

MR. BENDER: If I wanted the guy's telephone number, what would it be?

MR. SPEIS: Excuse me?

MR. BENDER: What man would I call?

MR. LEWIS: Looking at LERs means looking at all LERs that relate to scram systems, to the entire reactor protective system. Is that what the job is?

MR. SPEIS: The job is to review all the LERs for all reactors.

MR. LEWIS: All LERs, everything?

MR. SPEIS: Yes.

MR. LEWIS: Oh. Thank you.

MR. EBERSOLE: May I have a clarification on some relationships? There was an instruction to provide continuous monitoring, which was refused by the applicants and has been set aside until December, I believe.

MR. MILLS: It will be installed on a schedule -- some plants wil have it in earlier because of availability of equipment. We are looking for the last one to be in in December.

MR. EBERSOLE: So four months from now we are going to be dependent upon two parameters that determine we are not filling the scram volume, that is, rod drift and temperature; is that correct?

MR. MILLS: Those alarms, the daily monitoring. We still would have that. The daily --

MR. EBERSOLE: I am not counting that as being worth anything, because of the possible flow rates. If the rod drift and the temperature monitoring are unreliable -- if I declare they are unreliablel arbitrarily, then I can easily fill up in far less than a day with some leakage. Now, then I am going to get back to my credibility of rod drift and temperature.

Following Mr. Etherington's question, I don't yet see how you cannot have cool water emerging from the rod drive system and flowing up into the rod and thence through a leaky valve to fill the volume in a time far shorter than the periodic interval of inspection. I don't think temperature will show. I am not sure about drift.

In any case, both drift and temperature are non-safety grade systems.

MR. MILLS: They are non-safety grade systems, and I don't think I said that we would not have relatively cool water. I think what I said, to the best of my understanding, it is correct that, for the drive, the way it is constructed and operated, if the valve does leak when you do get to around .1 gpm, the temperature alarm will come in if it works.

And if you question the operation of the temperature alarms and the drift alarms, yes, I have to agree then that you could get water in the discharge volume without alerting the operator. I think the likelihood of that is quite low, that you are going to have a significant sudden degradation of those scram valves without getting some annunciation to the operator.

MR. EBERSOLE: This .1 gpm to the valve, is that not made up from water from the drive system, not water from the reactor?

MR. MILLS: I see it has been a combination, because it goes into -- it is a very small amount, on the order of .1 to .2 gpm per rod; and it goes into the drive and it mixes with reactor water that is already in the drive.

MR. ETHERINGTON: It should not mix; it should drive the reactor water out. The pressure is 25 psi above the reactor pressure.

MR. MILLS: Right. But I think it has to mix in the drive as it flows up along --

MR. ETHERINGTON: It will mix if it is full of reactor water to begin with.

MR. EBERSOLE: The credibility of temperature is worth a re-examination. Evidently, that is all we are riding on, since you do not seem to mention drift.

MR. MILLS: We can look at it in more detail and we will in response to your comment. But my understanding is it cannot get directly from the cooling water into the drive -- to the scram outlet, without a flow path through the drive.

MR. PLESSET: I think GE told us in Los Angeles that the leakage was not the coolant water, but the reactor water.

MR. EBERSOLE: That was due to a peculiarity the system.

MR. PLESSET: Right.

MR. EBERSOLE: How reliable that peculiarity is I don't have a feel for.

MR. JORDAN: Dr. Ebersole, when the scram discharge valve begins to leak, then the 25 psi differential you have is decreased in that drive. The drives were manifolded separately, so that when you do get an increasing amount of reactor water that is ming through the seals and going back out --

MR. EBERSOLE: What you are riding on, in addition to this, is a full control mechanism that pinches off the cold water flow.

MR. JORDAN: If you have the scram discharge valve open, you no longer have the 25 psi on that particular drive as being a differential. You are dropping the pressure.

MR. EBERSOLE: I see. Okay. Thank you.

MR. OKRENT: I wonder if the staff has a response to this question, the one Dr. Moeller raised earlier about the seeming discrepancy between the analysis of the PAS staff and then the succeeding statement. And also, is there something in writing which documents the analysis of the PAS staff?

MR. SPEIS: Yes, there is something in writing. There is an internal memo.

MR. OKRENT: I would like to request that I obtain a copy of it; if I can do it this way, unless you are going to make me use the Freedom of Information Act.

MR. SPEIS: No.

MR. OKRENT: Thank you. But could you tell us a little bit about that seeming discrepancy? Do you have any comments?

MR. SPEIS: Donnie will discuss it.

MR. OKRENT: Yesterday in a Subcommittee meeting on a range of questions, we were talking to Mr. Stello and Mr. Denton, and we were trying to ascertain how the staff decided that after the partial failure to scram had occurred, it was or was not okay for all the BWRs or some of the BWRs to continue to operate. And I would say we got a sort of judgmental answer.

Now, I think one thing we did here was that there was as estimate made by Mr. Thadani or somebody that you take the BWRS as a class and they just put this piece of information in. The scram unreliability was on the order of -- unreliability, one in ten per demand, and that is not radically different, as I recall, from what the ACRS fellows got by looking at fault trees, that it is a factor of ten, that sort of thing.

So I am wondering if in fact staff thought the unreliability was on the order of one in ten at that point. And now, let me think in terms of the points that do not have recirc pump trips; how it was decided that this was an acceptable mode for continued operation. I am trying to see for myself, when the staff decides something needs to be done in a day or a month or a year or just, we will study it, and so forth. And this seems to me to provide a test case.

Can we get any comment from I&E?

MR. JORDAN: Dr. Okrent, that was an I&E, NRR concern, that we did not have the reliability in those systems that we understood or had previously understood. The basis for continued operations was that the actions we were prescribing in this case for the licensees we felt would return those systems back to that level, let's say, of reliability which we previously understood they had.

And so those were essentially immediate actions we took with the affected licensees.

MR. OKRENT: It is not clear to me that you knew enough shortly after this occurred about the designs of each system, in fact, to know where there might be weak spots or single failure points or whatever; that from a fault tree analysis, in addition to just, you know, an empirical look at the data, it might indicate that the unreliability was pretty low.

It is also not clear to me that at the time that you indicated, if the licensee should do certain tests, there was any basis for knowing that these would really remedy the situation.

So I hear you, but I really question that you had the same depth of knowledge, let's say, that we have today. So I am trying to understand this and you know, compared to other things, like what was done after Three Mile Island to certain B&W plants. Let me suggest that had this event been a full ATWS at full power and the pressure pool had gotten very hot, your actions may have been different, although it would not have changed the unreliability of all the other BWR systems. They would be where they were.

Do I make the point? I am trying to understand the rationale. I think in fact the staff does exercise judgment in situations these days where the estimated probability of an event is thought to be substantial, whatever that means, and I become increasingly interested in knowing how and on what basis you do it.

You know, you do not have quantitative criteria. There is nothing in fact -- they are decisions made that end up having quantitative applications.

MR. JORDAN: It was clearly a judgment based on the knowledge that the staff had, and I don't think I can go further than that, based on the discussions you had yesterday already.

MR. LEWIS: I want to associate myself with the point Dave is making because I agree that the general pattern one perceives is that the squeaky axle gets the grease, and the amount of grease depends upon the loudness of the squeak rather than the implications of the need for grease. That is a very important point.

But on another point closely related -- and I also would like to see the PAS analysis that shows that WASH-1400 was vindicated because I thought that there was no part of WASH-1400 more discredited than then calculation of the probability to scram for a BWR. I thought that was something we had put behind us a long time ago, and that seems not to have been the case.

So under your Freedom of Information Act request, I want to look at the same thing.

There is one other point, if I may, Mr. Chairman, and that is that obviously this Browns Ferry event and Hatch and Brunswick before are going to raise a whole host of deep philosophical problems about the reliability of scram. As I look at the WASH-1400 diagram that I guess Dot passed out, there is a left side, which is electrical, and a right side, which is hydraulic.

All our conversation has been on the hydraulic side. But my memory, such as it is, was that G.E. always estimated that the prime vulnerabilities were on the electric side, which we seem not to talk about. Again, squeaky axle.

I wonder if as we reassess the implications of the event we might take a somewhat broader view than just blockages in two-inch lines.

MR. PLESSET: I am very glad you made your comments because it reminds me of another point. I am going to ask you and Dave to give me some paragraphs for our letteron this thing. Since you have spoken so eloquently, I expect these paragraphs both from you and Dave to be eloquent.

MR. LEWIS: I will write the even words if you write the odd ones.

(Laughter.)

MR. EBERSOLE: Since he got into the electrical area, he does not need to bother with the hydraulic area.

MR. PLESSET: We are going to ask you also, Jesse, to give us some paragraphs.

(Laughter.)

MR. PLESSET: And Harold has not said much, but we are going to ask Harold to contribute.

I wonder if you have much more, because we do want to get Mr. Thadani up here as soon as possible because he has to leave early.

MR. MILLS: This is the last slide I have here.

Our conclusion on Browns Ferry 3 is that the corrective actions being taken ensure that the scram discharge volume is empty during power operation. If the discharge volume is empty, the scram will work when called upon. These corrective actions are necessary and they are sufficient to justify continued operation.

If the scram discharge volume is empty, the scram function will work. Long-term corrective action is necessary and is under way. This is headed up by the Task Force, which will be discussed later.

MR. PLESSET: Okay. Now, we will certainly remember the requests that were made by Lewis and Okrent. I am sure we will not forget.

Yes.

MR. MOELLER: Just a quick question without even the answer now. But I have been asking what was the purpose and objectives of this Bulletin 80-14. I notice in the same memo that I referenced earlier, it says in response to Bulletin 80-14, "some BWRs were performing SDV level instrument funnctional tests after every scram."

You see, that leaves me confused. Were they doing it out of the goodness of their heart or were they doing it because they chose to interpret Bulletin 80-14 in a manner so as to require such tests? I would hope that someone later this morning could clarify that for me.

MR. PLESSET: All right. Let's come back to that question after, unless you have a yes or no quick answer, because I do want to get --

MR. SPEIS: I would like to make two points before Mr. Thadani speaks. First of all, we have not excluded for the long term problems associated with the electrical part of the system. I would like to make that clear immediately. We did focus on the electrical part and we got some satisfaction. I cannot quantify that. We should pay more attention to the hydraulic part but we have not excluded the electrical for the long term.

Also I would like to make the point that in answer to Dr. Okrent's point -- we were concerned with these plants, and one of the questions we raised with licensees in Bulletin 80-17 was to perform analysis without RPT showing us what kind of degrading had to be performed in order not to exceed their pressure and temperature limits -- the pressure limits.

MR. OKRENT: I am familiar with the fact that you asked for this and there was at least one response that I have seen, but that is only part of the decision-making process. At the moment I am not saying that what you did was right or wrong. I am interested in understanding the decision process in some depth because, as I say, I think we are getting involved in decisions that are on the borderline of which way to go.

I think the auxiliary feed-water systems which are not seismically qualified is another example of one which in my mind is not on the borderline. It is not sitting clearly in a situation where everything is all right for the indefinite future, or let's say we cannot run another minute, so if we are encountering such, I think we had better start understanding it.

I do not think the answer "it was our best judgment" can continue to be an adequate approach even if that is all you can do on Saturday. Maybe by the following Saturday you ought to have reevaluated it and decided yes, this is why it still remains our best judgment, and we can tell you.

MR. PLESSET: Let me come back to Jesse and to Paul after Thadani. His time is rapidly disappearing.

MR. SHEWMON: Let me ask a short question of the last speaker. If one Duane Arnold finds an SDV drain valve installed backwards, does that get reported as an LER?

MR. MILLS: It would now because of our requirements on the vent drain valves previously. It would not necessarily have been reported.

MR. SHEWMON: Thank you.

MR. PLESSET: We will come back to you Jesse, I promise.

MR. THADANI: I am a member of the NRR staff. In at least partial response to your question, it is a difficult question in terms of the type of considerations one goes through in making any decision. Indeed, when we put together some of the requirements for licensees in Bulletin 80-17, some thought was given to those plants which do not yet have ATWS-related recirculation pumping solved.

The rationale was to try to assess what could be done at these plants to minimize the risk from such events while we were still trying to find out what had actually happened at Browns Ferry, and what, if anything, could be done to improve the situation.

If you remember, at an earlier meeting we had given you a schedule of implementation of recirculation pump trip in all plants. My understanding is that unless things have changed, that all operating boiling water reactors were to have recirculation pump trip implemented by the end of this year, and this is the ATWS-related recirculation pump trip and not the end-of-cycle recirculation pump trip.

I do believe in the decisions that were made following the Browns Ferry event. This was taken into account, as well as the actions that were being taken.

As you know, we have had the ATWS issue for many years, and we have at least taken the position that the risk from an ATWS event, if for a moment we leave Browns Ferry aside, risk from ATWS events, while it is high, it is not so high so as to shut the plants down; that improvements have to be implemented at some schedule, hopefully a fairly rapid schedule.

I think the same kind of thinking was involved in the decisions that were made following the Browns Ferry event. As you remember, back in March of this year we had a meeting with the ACRS at which we discussed our recommendations on this issue, the alternatives that we had considered. In April we received your letter giving us your advice as to which way to proceed, and the care we were to apply in implementing significant design modifications in terms of schedules that were proposed in our earlier report.

We were taking into consideration your letter as well as the industry comments in preparing what we called Commission Policy Paper on this issue when the Browns Ferry event occurred. As soon as the event occurred or soon after that, we sat back and said what does it mean in terms of what we have been doing in this paper that we are writing for Commission consideration? What lessons had we learned, if any?

The first lesson, I think, that we learned was indeed the reliability of the scram system is not as high as had been planned by certain sectors of industry. Indeed, the overemphasis -- what I would call overemphasis on the electrical portions might have been the reason for having missed the seriousness of the type of Browns Ferry scenario, the concern that certain failures are difficult to recognize, especially for highly reliable systems, or that if a failure is recognized, sometimes there might be a tendency, based on some sort of judgment, to say the likelihood of this kind of a failure mode is very low.

Indeed, that is what happened in terms of the type of failure we saw at Browns Ferry and earlier industry approaches, especially as they related to boiling water reactors.

Another important lesson I think we learned was the recommendation that we had made that was termed Alternative 2A in our report, improvements in the reactor scram system to reduce its likelihood of failure. If they had been implemented, they would not have prevented the Browns Ferry event at all, because again, the concentrated effort in terms of improvements was in the electrical portion.

The proposed diversity was to be incorporated in the logic portion, and the sensors in the instrument portion. You have heard a lot of discussions in terms of the problems that these sensors have experienced, and that was one of the reasons for our insisting that we ought to provide some diverse means of detecting the amount of water in the instrument volume.

But that would not have done anything in terms of the water in the scram discharge volume. So it is my belief that the proposed modifications would not have prevented the Browns Ferry-3 event.

On the other hand, perhaps another important lesson we learned was that mitigation, again, might be the most appropriate means of taking care of types of failures which have very low likelihood of occurrence, ones that are difficult to predict. It has reemphasized, in my opinion, the need to do, obviously, as good a job as one can do in terms of design of the system, sit back and learn from these experiences.

We are bound to make mistakes. We are bound to miss certain aspects of the systems, and subject to the consequences from those potential scenarios, one ought to seriously consider ways that would be diverse and would, indeed, mitigate the consequences of both events. We had, as you know, taken the course over the last few years.

In my opinion, the Browns Ferry event further emphasized that that was the way to go. I took a very quick look to see how we might impact the frequencies and so forth of ATWS events that we have discussed with you over the last few months, at least. You, as well as we, recognize the uncertainties and so on in these estimates.

I did what I would call a conservative assessment, assuming that the Browns Ferry event is indeed a legitimate failure to scram event -- and I believe Marvin is going to discuss with you the potential consequences of the Browns Ferry event if it had occurred at 100 percent power along with an anticipated transient, or, for that matter, if you had half a scram at 100 percent power along with an anticipated transient.

(Slide)

If you will just accept for the moment that for those scenarios subject to certain operator action fairly early in the event -- when I talk about early, I am talking about very, very, very few minutes -- the consequences certainly would be quite serious.

Just to summarize some of the things we have done in the past, the first row, which says Prior to Browns Ferry-3, are estimated frequency of a severe ATWS event was on the order of 2 times 10^{-4}, and I am only addressing boiling water reactors now. If we assess that if the type of modifications described under what we called Alternative 3A had been imlemented, this frequency of severe ATWS events would be further reduced on the order of 10^{-5} per reactor year.

The types of changes under Alternative 3A were recirculation, pump trip, automating the standby liquid control system, as well as making sure that the poison system would inject simultaneously. Currently most of the plants cannot do that because of piping design limitations.

Alternative 4A basically went further and said we ought to also be able to handle a single failure in any of the mitigating systems. And for what it is worth, I am not going to try to justify the 10^{-6} number, but rather to point out if you go to the event tree approach and for the moment accept that there is not a common mode failure that would disable scram as well as the mitigating system, which has a frequency higher than 10^{-6}, indeed that is what you would come up with roughly.

Now, the Browns Ferry event occurred, and what does that mean? We took a look at the experience in terms of the total number of scrams that have occurred at boiling water reactors. It is approximately, I believe, on the order of 5000 or so. And a fairly simple point estimate would indicate that failure to scram probability would be on the order of 10^{-4}, combined with the recurrence frequency of anticipated transients.

The estimate of ATWS frequency would be on the order of 10^{-3}.

MR. KERR: Do you have a confidence level associated with that?

MR. THADANI: No. It is a point estimate. It is basically 50 percent.

MR. KERR: Okay.

MR. THADANI: Having said that, certainly most of us recognize that as a result of this event, certain actions have been taken. All these actions are directed towards reducing the likelihood of this kind of an event. My belief is that the true frequency today would be lower than 10^{-3} per reactor here. And whether it is 2 times 10^{-4} or 10^{-3} per reactor year, I do not know.

I tend to think that as a result of the actions taken, the system is better than it was, and we believe in making these rough estimates in terms of frequency of events, I would expect the frequency of an ATWS in the revised system would probably be closer to 2 times 10^{-4} than 10^{-3}. But again, this is a very subjective personal opinion.

I do think improvements have been made to reduce the occurrence rate of this kind of a failure mode. So what that meant -- basically there is a difference of a factor of 5 between considering this event or excluding this event from consideration. You can translate this factor of 5 all the way across, depending on whatever alternatives are being considered.

Another approach was to say this is yet another example of the type of failures that are likely to occur that for whatever reasons we are not going to be able to predict, and that there are certain recurrence frequencies for these types of failures.

Instead of looking at this in isolation, if you will, in terms of just being applicable to boiling water reactors, maybe we ought to sit back and say, okay, what if we were to say that this is representative of a frequency, if you will, of a failure which could disable part or all of the scram system?

What might be the impact on PWRs as well as BWRs? Again, it is a fairly simple approach. You see the difference once you account for pressurized water reactors. The numbers are not significantly different. There is an increase in frequency estimate by a factor of 2 to 3. Again, as I said earlier, because of the improvements, I think there are certain elements of rectification.

I use that term "rectification" with some fear because of the type of reaction I might get, but I think there has been a certain amount of rectification. I agree with the statement that Dr. Kerr made yesterday that this does not preclude recurrence of this failure mode but rather reduces the likelihood of this type of failure.

MR. SHEWMON: Would you tell me where in there the reactor pump trip comes in in this framework?

MR. THADANI: Okay. Basically, if you have alternative 3A implemented, that includes recirculation pump trip, includes the improvement in scram systems, again, only in the electrical portion, and it includes alternating standby liquid control systems.

MR. KERR: Mr. Thadani, I think if you strictly refer to ATWS frequency, then pump trip does not come in at all because pump trip does not prevent an ATWS. What it does is mitigate it.

MR. THADANI: That is right.

MR. KERR: So what you have there is a frequency of ATWS whose consequences are unacceptable, I think.

MR. THADANI: That is exactly correct. I noticed that on the slide. That is why when I started out, I so characterized this slide. Estimated ATWS frequency is somewhat higher, but this is an estimate of ATWS frequency which could result in severe consequences.

MR. BENDER: My recollection of some of these numbers is a little vague, but in developing those numbers, especially when G.E. did it, they took the position that there was a lot of mechanical independence between the drives, and the commonality, if it existed, was in the electrical part of the system.

Now, I don't recall any discussion of commonality relationships that might exist in the hydraulics. How are they expressed up there in those numbers?

MR. THADANI: Well, if I may characterize G.E. analysis, they did have fault-tree analysis of the scram system. The failure mode was indeed there. It was characterized as -- you have to have multiple failures. You have to have plugging of the system as well as failure of the sensors in the instrument volume, and the combination was thought to be for very, very low probability.

If I can quote numbers, it was on the order of 10^{-8}. I would say the mode was recognized, but how you got there in terms of likelihood and so on was certainly underestimated.

MR. BENDER: Is that mode in those numbers up there -- those are not G.E.'s numbers. Those are yours.

MR. THADANI: These are our numbers, and these numbers are based on what we call a systems model. It is basically an exponential model which says we have had so many reactor years of experience, we have such and such frequency of testing, we have had so many failures, and I can make an assessment of the likelihood of this kind of an event without going to the so-called fault tree approach, which fault tree, as well as synthesis models which have indeed been used in the past, both by the industry as well as WASH-1400 --

MR. BENDER: There are only a couple of events, as I recall, that represented experience.

MR. KERR: Let me comment on that. The first number, 2×10^{-4}, I think comes from a consideration of experience and some estimate of reactor years in which there is either one or zero failures, depending on interpretation, and is calculated about at a 95 or 90 percent confidence level.

MR. THADANI: No, I am sorry, Dr. Kerr. The first number, the mechanism is correct, indeed, but it is based on single failure. We through out the N-reactor because we felt it was not applicable to commercial designs. We looked at the reactor years of experience, looked at the frequency of testing, recognized the true testing frequency was somewhat higher than that required by the technical specifications, and that some credit was given for the concept of rectification.

MR. KERR: But the basic number which you took and then modified by giving credit for testing was a number based on experience.

MR. THADANI: It is based on experience but it is not a 90 to 95 percent confidence number. It is a 50 percent confidence number.

MR. KERR: You get about 10^{-4} if you use the raw data, with somewhere around 90 to 95 percent confidence, as I remember. You get another factor of 10 by putting in testing and rectification. So you get 10^{-4} or 10^{-5} and then you put in the rate of anticipated transients, which is about ten per year, and that is the way you get to 10^{-4}.

MR. THADANI: I can tell you, I recollect some of the numbers, the exact numbers come out at 95 percent confidence. Sometime back it was 1.1×10^{-4} for failure to scram. That, when combined wit a recurrence frequency of transients, will give you on the order of 10^{-3} and not 2×10^{-4}.

MR. KERR: If you have put in your rectification and testing, you get to the 10^{-4}.

MR. THADANI: Even if I assume zero failures, I go to 95 percent confidence and I would come up with a number like this, zero failures.

MR. LEWIS: I don't understand how zero failures can give you anything but an upper limit.

MR. THADANI: Right.

MR. LEWIS: There is no 95 percent confidence associated with an upper limit.

MR. THADANI: I can go to the upper limit and that upper limit would be this kind of a number if I throw out that failure that at least we think was a failure.

MR. LEWIS: Which one?

MR. THADANI: The call.

MR. BENDER: Let me see if I can pursue the question I was trying to develop. Given that those numbers are based on what amounts to the operational experience and ow I want to crank in the observation that there were a number of hydraulic faults in the systems that have been investigated as a result of this one event, which might have suggested that those systems would not have scrammed either, how might they influence those numbers up there?

MR. THADANI: Mr. Bender, would you go through that one more time?

MR. BENDER: We found out that Hatch had some problems and Brunswick had some problems. As a matter of fact, I suspect most of the BWR systems had some problems and they were all on the hydraulic side of the system. With my limited knowledge I would have to suspect that some fraction of those would have experienced the same kind circumstances as Browns Ferry if they had been called upon to work.

How would I go about interpreting those numbers that you have up there in light of the observation about the faults in those systems.

MR. THADANI: One way would be to go back and look at fault trees and recognize, possibly, that one could have failure of half the system by possible plugging of the vent line. There would be some other factors involved in terms of timing, what can drain out and so on. But it seems to me that one could go to data and make an estimate as to the contribution.

MR. KERR: It seems to me that number in the upper left is based on experience. Then you cannot attribute anything to that number on the basis of what has been found out because you simply would use data, and the data, in a sense, took into account --

MR. BENDER: Except --

MR. KERR: It does not have anything to do with --

MR. THADANI: No, no. I think Dr. Kerr said it quite correctly. Implicit in this are two failures that we have experienced, Browns Ferry and Call (phonetic), the last two rows, basically. The judgment that we ought not to pay as much attention to synthesis models in terms of trying to show how unlikely the failure rate was was arrived at on the basis of our having seen some of the partial failures in, as you point out, the hydraulic system.

But this did not include quantification. It is very difficult to quantify when you have had only partial failures and move on and try to come to a conclusion as to how likely it is for certain combinations of failures to occur.

MR. KERR: It may be a very fine point, but if one is really interpreting these numbers, the left-hand column, at least the modification due to Browns Ferry 3, does not refer to an anticipated transient without acceptable consequences, fortunately. So that is a modification which really deals with the probability of scram failure and your interpretation as to what effect that might have on the other number.

MR. THADANI: You are quite right. It impacts only one-half or at least the failure to scram portion. We had made an assumption that anticipated transient as well as failure to scram are independent events. I think at least in my opinion, Browns Ferry raises a concern as to whether that is a fair assumption, especially if possible failure of the scram system as related to the demand which might -- here is a scenario, if you will.

I have a scram today. Whatever reason the scram took place, I got a lot of water in my discharge volume and I have another anticipated transient tomorrow, and I have not drained my system. I think it raises a question in my mind and it is a fair assumption to say that the two events are independent. I don't think they are quite independent. That is an assumption in the estimates that you see up there.

MR. PLESSET: Is that all?

MR. THADANI: I am sorry that Dr. Moeller is not here. I would have been glad to have addressed -- I hope I have addressed his question. I tried to in the way I discussed this. I did not address part of what he said, which was reference to the PAS and the conclusion that there is no significant change in terms of conclusions in WASH-1400.

I think that was intended to mean not ATWS-related differences but overall core melt probabilities. I do not want to speak for PAS, but at least I have talked to PAS in terms of frequency, if you will, of the likelihood of failure to scram on demand. Until changes are made, they are in agreement that the failure probability is on the order of 10^{-4}, and the reduction in terms of improvements we just don't know.

But that would then imply that with the kinds of transient frequencies that we see in these plants, that ATWS frequency is somewhat higher than what we had stated earlier.

MR. LEWIS: We have the PAS note here, and it does what it says. In effect, there is nothing in the experience of failure to scram to negate the WASH-1400 numbers for failure to scram, however ill-derived they were. It does not have that last phrase in it, of course.

May I ask just one trivial question? Why in September of 1980 are there still BWRs that do not have RCP trip?

MR. THADANI: There is a commitment to implement that by the end of the year.

MR. LEWIS: By the end of this year. I will ask the same question January 1st.

(Laughter.)

MR. THADANI: I hope the answer is they all have recirculation pump trip.

If you will excuse me, Dr. Okrent, you said you have a copy of a memorandum. Was that a memorandum from Jim Pittman?

MR. LEWIS: Yes.

MR. THADANI: Let me make a correction. On the second page of that it talks about 5×10^{-4} per reactor year based on 2000 scrams and so on. It is the last page, I am sorry. That should be per demand. If you take that per demand and include the transient frequency, you would come up with numbers which are pretty close to what I am saying.

MR. LEWIS: This is a very rudimentary calculation.

MR. THADANI: It is.

MR. OKRENT: Before you leave, I wonder if you could tell me whether there are any problems or conflicts of information requirements or procedural requirements in implementing Alternative 3A. As you know, we sent some questions to the staff and the people who were at the subcommittee meeting did not answer them, and they said Mr. Thadani would be your source of wisdom here.

I am afraid our source is going to run upstairs before I know whether Alternative 3A has practical problems or not.

MR. THADANI: Let me give you a quick answer to that. If you would like a longer discussion, I can come back later in the afternoon. I think that the concern you had related to ADS. You quite correctly pointed out ADS is actuated on high -- there are a combination of signals that have to be present for ADS actuation. Indeed that is true.

One of the concerns we had was the G.E analyses showed that for full ATWS events, you do not get to the point where ADS would be actuated. I suspect, and I have asked G.E. this question and I have not really gotten an answer, but I suspect the main reason is because of the timer, the two minute timer in the ADS actuation. I think this is why they do not actuate ADS.

Your thought that ADS could make consequences for ATWS worse is indeed correct, I believe. It could cause more problems. But current analyses do not challenge ADS.

The recommended change in NUREG 0626 that you referred to in fact would help because it says that you go to ADS only if a high pressure core spray or the reactor core isolation cooling system fails.

Under Alternative 3A the implicit assumption is that basically all systems are available, including high pressure core spray or high pressure coolant injection. So, if that system is available, I think it would give me more confidence that ADS would not be actuated in the event of an ATWS.

MR. OKRENT: Well, I guess it is very hard to discuss the topic in this particular forum, so I don't know whether the chairman is going to be able to put something in on ATWS later today. I have a feeling he is going to need all the time he has on Sequoyah. If sometime at this meeting, even if it means Saturday morning, that we get Mr. Thadani or whatever, I think it could be useful because there is also the question Mr. Ebersole has that I have not really heard answered about can you lose the boron under some circumstances, and a few things that are not unimportant in connection with this.

Let me just leave that as a scheduling question.

MR. MARK: Let me introduce the situation to the chairman. There is a question which relates to this current status of implementing the ATWS proposal, certain features which Thadani thinks require more discussion than he could give to it now. He said he could come back this afternoon if we would like to go back to ATWS this afternoon.

MR. PLESSET: I don't think we can. We will have to have a less rushed discussion of this.

MR. OKRENT: This is what I suggested.

MR. PLESSET: This afternoon would be too --

MR. OKRENT: I said maybe Saturday morning or something, unless we can find time on Friday.

MR. PLESSET: Saturday morning would be all right.

MR. THADANI: I might make one comment.

(Laughter.)

MR. PLESSET: Maybe we should plan it for Saturday morning. How does that fit your schedule?

MR. THADANI: It will have to fit my schedule.

(Laughter.)

MR. PLESSET: I have a question. I would like you to explain one thing to me. There is this note from Georgia Power. Have you seen that?

MR. THADANI: I don't know. I may have seen it.

MR. PLESSET: It says -- we all have a copy -- that the operation of standby liquid control system with both pumps -- it is a question regarding that. And their evaluation indicates that the system as designed cannot be operated safely with both pumps running and the reactor at maximum ATWS pressure of 1297 psi.

Tell me what that means.

MR. THADANI: I don't know.

MR. PLESSET: Okay. Then Saturday morning you will tell me.

MR. OKRENT: I think it is relevant.

MR. PLESSET: Oh, yes, I think so too, Dave.

MR. EBERSOLE: A related question. Mr. Thadani has mentioned the RPT as a system of some importance in this and the boron injection. This is part of a somewhat larger problem. The RPT system is like getting out of an airplane that is burning, and then there is a long way to the ground, and starting the boron injection system is like pulling the ripcord. There are lots of things that still have to happen.

One of these is the reliability of the HPCI system, which now must face a pressure transient, and it always tries to overspeed trip anyway. And now it is faced with an initiating pressure substantially higher than the ordinary pressure at which we use it and test it. I would be inclined to guess that the first thing it will be is overspeed and lockout.

Another thing is, having gotten the boron system running, if you can, in fact, the ECCS sytems are exuberantly trying to flush all the water out of the systems. There are various valves which might be stuck open. The operator must, if he gets the boron in, keep it there because he has only a one shot opportunity and there is hardly enough to leak any of it whatsoever.

MR. THADANI: Well, the first part, certainly I completely agree with you in terms of the reliability of high pressure coolant injection system. In theory the later plants are supposed to have better systems, the high pressure core injection systems. That was one of the big differences between what I call Alternate 3A and Alternate 4A.

We were concerned about just that aspect of high pressure coolant injection systems, and we felt quite strongly that total reliance on that system seemed inappropriate. I am sorry I can't discuss what we are recommending to the Commission in an open meeting, but I can point to the suggestion that we have to include the reliability of that system as to its potential for failure under high pressure conditions.

I have to go by my memory. I think that the data dump seemed to indicate that.

MR. KERR: Mr. Thadani, we had one of our fellows look at this to some extent, and I believe some of the experiences seem to indicate that although there is a considerable failure to start for those high pressure injection pumps, that it is not difficult to restart them.

I don't know.

It is not a failure in the sense that they are automatically unavailable once you get a failure to start. They may start and trip out, but they can be started again fairly readily. So I am not sure -- I mean it certainly would be preferable that they not have this feature. But I think the experience has to be interpreted in the light of the use that you have for them.

MR. THADANI: I could not -- pardon me. I certainly agree with you, Dr. Kerr, that it has to be. On the other hand, ATWS is, relatively speaking, a fairly fast transient, a fast accident. You need high pressure coolant injection systems fairly quickly, and turbine-driven pumps and controls associated with those systems and the types of failures that have been experienced.

One has to then go back and determine how likely is it that the operator is going to indeed notice that the system did not work and fix it in time and so on

MR. OKRENT: You would be requiring the operator to look at a very excited control room panel.

MR. KERR: My impression also is that there had been some fixes for some of these. But I think you are quite right. It is a problem that has to be looked at.

MR. EBERSOLE: One other comment. You characterized the unmitigated accident as having severe proportions. I think that is very optimistic. It carries with it the connotations -- I think that is hardly an appropriate word, "severe," which connotes something that is fairly manageable and we can cope with the emergency preparedness systems and so forth.

MR. THADANI: Sometimes one gets accused of going overboard. I certainly have been accused. I have chosen the word "severe" for one major reason. Some substates of these events were perceived -- at least I am fairly sure in my own mind not all of them will proceed to core melt.

There are some events where you may have a condenser available to you, and if you have a recirculation pump trip, if you have a very smart operator and if you have an event which occurred at fairly low power level -- it has to be fairly low -- then there is a chance that he can protect the plant. Because this frequency includes events like that, I characterize it that way.

MR. EBERSOLE: You are weighting the presence of the condenser in a manner which rather obscures the worst end of the spectrum. If you weighed it another way, it looks worse.

MR. THADANI: I did not mean to obscure it. In past discussions I have gone into some detail as to why we think these events will proceed to core melt, and then one can go to WASH-1400 and determine how quickly one could get into serious trouble. Indeed, this is considered by PAS as the most significant contributor to risk in boiling water reactors.

MR. PLESSET: Let me ask, Mr. Thadani. You have to leave at 12:30, is that correct?

MR. THADANI: I have to be there at 1 o'clock.

MR. PLESSET: There is no rush. I misunderstood.

MR. THADANI: I was going to grab a bite, but I have no problems. Let me just touch on the second part of the question.

MR. OKRENT: Before he goes on, I think sometime at this meeting, in closed session, if necessary, we should hear what it is the staff is going to tell the Commission unless when they tell it it will be in open session, and after that they can tell it to us in open session. I think we ought to hear what the new staff position is in this meeting unless we are not going to write something.

MR. PLESSET: We are going to try to. I was going to ask Mr. Thadani and the Committee how they felt about having him come back at 9:30 Saturday.

MR. THADANI: I will be here.

MR. PLESSET: Should we plan a closed session at that time? That is what Dr. Okrent was suggesting.

MR. THADANI: I would hope that it is a closed session because then I would feel more comfortable telling you what steps we have taken.

MR. PLESSET: Can we do that?

MR. FRALEY: I will check and see if we can or not.

MR. PLESSET: Let's plan on that unless we get some contrary indication.

MR. BENDER: Are you planning to discuss the matter with the Commissioners in closed session?

MR. THADANI: No. I understand and ELD tells me that a document becomes public on the day of the briefing to the Commission. And tentatively, as you heard yesterday Harold Denton describe, in the middle of this month -- I have my doubts. I think it will be later.

MR. BENDER: You have answered all I wanted to know.

MR. PLESSET: Go ahead. I am sorry. I just wanted to settle that. You were discussing. And then Okrent has a question.

MR. KERR: I don't think we can hold a closed session on this subject.

MR. PLESSET: Well, Ray is going to find out.

MR. FRALEY: I will look into it.

MR. PLESSET: If not, we will have him in open session, and that will limit the discussion, presumably.

MR. FRALEY: I think Mr. Thadani could talk to us about current staff thinking without talking about the staff paper and not violate any confidences.

MR. THADANI: In terms of the considerations for a standby liquid control system, or poison, if you will, yes, that is a concern. We did the usual kind of calculations quite some time ago for normal leakage and so on, and we found that there were several days available -- several days available before one would worry about possible power going off. But that is really, at this stage, it is very difficult to answer that question, whether it is today or Saturday, simply because I think it is very plant specific.

In some cases you might need 28 percent of the volume of the standby liquid control system. In another case you might need 35 percent and so on. So there may be some differences in terms of how much leakage.

One of the requirements that we would place on this would be to address that issue, an I think at this stage that is really all we can do. I just don't see how in a generic sort of way we can resolve that problem. That might give away something of what I think the paper is going to do, but I don't believe there are certain parts that can be handled generically.

MR. EBERSOLE: That's just the high pressure, pre-borated feedwater system.

MR. THADANI: There was a suggestion that we made a few years ago for high pressure standby backup feedwater systems and so on.

MR. EBERSOLE: Borated.

MR. THADANI: Unfortunately, the cost associated with it was so high that we looked around for alternate ways to get high reliability where the cost would also be reduced. And the alternate that was proposed was to make the standby liquid control system a much bigger system so that it has a certain amount of redundancy, it is indeed automatic, that it serves two functions. Not only does it serve the function of reducing power by inserting negative reactivity as well as keeping the -- at least contributing to keeping the core covered along with other systems, even if we assume high pressure coolant injection or high pressure core spray fails. And that was the thinking that was reflected in alternative 4A.

MR. PLESSET: Dave.

MR. OKRENT: I was just going to add a comment to a point Ebersole was mentioning earlier; that for one class of ATWS in PWRs we don't have very much time. That is again the plants without recirc pump trip. And I must say when I look at a number like 10 to the -3 as a possible number, best estimate or whatever, I don't know, and if I couple this -- I don't really care whether it is that or 10 to the -4 -- with an event that is a quick release event, overpressurizing the primary system, and if I see a possible avenue, for example, by reducing power to 80 percent until you put in a recirc pump trip, that seems to avert possibly some of the concerns in the short timeframe and put you into the compression pool overheating scheme.

It would seem to me that that is a decisionmaking process that should be consciously one through, and I would not lump all the BWRs into one group in that decisionmaking process. That is what I am saying.

Now, what we heard is they did a study -- the applicant did a study on the point whether pressure was below an awkward pressure. Well, I just wanted to put this on the table.

MR. KERR: How many do we have that do not now have operating pump trips?

MR. THADANI: Eight I think is the number.

MR. KERR: Eight out of?

MR. THADANI: Eight out of 26.

Again, Dr. Okrent, just to comment that 10 to the -3, as I hopefully characterized the things that have been done, whether the real number is still 10 to the -3 or higher or lower I do not know. But if you look at the difference, we are talking about a difference of a factor of three or four between five -- between our earlier estimate and our current estimate, with the Browns Ferry event --

MR. OKRENT: Let me put it this way. As you may have guessed, I think the plants have been running without recirculation pump trip for too long.

MR. PLESSET: You are not alone in that, Dave.

MR. OKRENT: And maybe with a larger number you might well have considered whether because of the nature of the event and how it differs from the longer overheating one, something that is past due. But I would say the recent experience makes the interest more acute.

MR. THADANI: I think I agree with you. The seriousness certainly has increased.

MR. PLESSET: Well, yes?

MR. LAWROSKI: The 80 percent figure that you gave, does that take care of all of them?

MR. THADANI: I beg your pardon.

MR. OKRENT: I saw an analysis for one, Quad Cities or Dresden. I suspect it is roughly the range. It may have been 85 percent. I don't remember. In that ballpark.

What it does is reduce the peak pressure, but it still leaves you in an overheating situation. But you would have more time.

MR. THADANI: I might just make a comment that that would probably be very plant specific, because as you know, different plants have different safety relief capacities and -- as well as the containment pressure considerations now becoming more acute, I think, because the energy you are going to dump is going to be somewhat higher than the current calculations would show.

MR. KERR: Does each of these eight plants have an approved system that has been analyzed and approved by the staff, or is there still some approval process that would have to be gone through?

MR. THADANI: I cannot answer that. Perhaps Tom Toledo can.

MR. KERR: Does each of the eight plants that does not yet have a pump trip installed have an approved system? Have they proposed a system and had it approved by the staff so that the only thing left to do is to install it?

MR. TOLEDO: That is correct and that is by Commission order. In other words, these eight plants must have an RPT --

MR. KERR: My question is have they submitted and had approved by you a pump trip scheme which they only now to install?

MR. TOLEDO: I was about to tell you that the order that went out said you have two designs which are acceptable to the staff. If you put in one of the two designs, you need not come to us for approval.

MR. KERR: Okay.

MR. TOLEDO: And these must be installed this year. They cannot operate in 1981 without an RPT installed.

Some will be installed in September, some in October. I think three plants are due to put them in in December.

MR. OKRENT: I would say it is not unlike dropping the water level in a dam when you get nervous about it.

(Laughter.)

MR. PLESSET: Well, can we let Mr. Thadani go until Saturday morning? I think he would appreciate a little break at this time, right?

MR. THADANI: I certainly could use one. I've got twenty minutes still.

MR. PLESSET: Well, let's continue then, because I think we have a relatively short --

MR. SPEIS: I think we can finish by 1:00.

MR. PLESSET: Why don't we go ahead? I hope it is not too troublesome Saturday morning.

MR. MENDANCA: The first question which is generally asked is what kind of power levels would be expected if you were to have a scram failure similar to the one at Browns Ferry. For a case where you would have a scram failure approximating that of Browns Ferry with the recirculation pump trip, your equilibrium power after your first spike would be around 10 percent. For the bounding case where you would have half rods in and half rods out, that number goes to around 20 percent. That is still with an RPT.

In the case where you do not have the RPT and you still have the rods out on one half side of the core, your terminal power is 40 percent.

MR. EBERSOLE: Before you go further --

MR. CARBON: Excuse me. Let me ask do we have a handout of that, because I could not see that.

MR. MENDANCA: You do not. I will give it to the appropriate people.

MR. CARBON: Then after Mr. Ebersole would you mind repeating what you said because I was hunting for the paper.

MR. EBERSOLE: You qualified that only as an RPT. Is this for a full closure?

MR. MENDANCA: This is just the terminal power that would be achieved. This is equilibrium power. This is a physics calculation

MR. EBERSOLE: Is it under the conditions that the MSIV is closed?

MR. MENDANCA: Yes.

MR. EBERSOLE: That means you are at the pressure of the relief set valves.

MR. MENDANCA: I believe so.

MR. EBERSOLE: So the voids are collapsed, the voids are collapsed.

MR. MENDANCA: Partly. To repeat again, the terminal power is the parameter that this vu-graph is trying to show, and it is trying to show it from 100 percent rod configuration for conditions where you would have scram similar to that of Browns Ferry.

MR. CARBON: From 100 percent power.

MR. MENDANCA: From 100 percent power, yes, sir.

The first figure of 10 percent is for a rod motion pattern in one-half the core similar to what was observed at Browns Ferry, trying to approximate that negative reactivity insertion.

MR. EBERSOLE: You're sure the reactor is at safety set pressure? I think that is an important aspect.

MR. MENDANCA: I believe so. I believe these numbers come from the calculation from the MSIV closure.

MR. KERR: This includes the injection of the standby liquid control?

MR. MENDANCA: No, sir. This is just the power level that would be reached after your initial transient.

MR. KERR: Within minutes, seconds?

MR. MENDANCA: Forty seconds. That number comes to mind.

MR. PLESSET: Jesse, we asked for the calculation with MSIV closure. I presume that is what they did, because we were very explicit about that as I recall.

MR. MENDANCA: It is. The second figure of 20 percent power is for the limiting case of the possible rod insertion from the scram discharge phenomena that was observed at Browns Ferry. That is where you have half the rods on one side of your core sticking out. And that is again with the recirculation pump trip with the RPT, and that number was 20 percent.

The final number is for the case similar but without the RPT, and that was a number of 40 percent.

Now, there are two criteria that we generally look at in ATWS mitigation, and that is to make sure that you have a pressure boundary and to make sure that you can remove your heat from your core, your generated heat under ATWS situations.

We asked immediately or in one of our bulletins, I believe -- we asked for a calculation of what would be the effect of a half scram on vessel boundary criteria, what would your peak pressure be. We asked this of the plants with no recirculation pump trip, and that is because they are the most limited in that feature.

They did perform a generic calculation, and came up with a figure for a half scram; that is, with one side of the core not inserting, one half of the rods not inserting on one half, a max pressure of 1460 psig.

As Dr. Okrent and others have already alluded to, we did also ask for further calculations for full ATWS; that is, no scram. And they did show that in order to meet the emergency pressure level, 1500 psig criteria, you would have to restrict power to somewhere around 80 percent. I think it was 81.5 in their generic calculation, to be specific.

MR. ETHERINGTON: Do you have a figure for 100 percent?

MR. MENDANCA: What the pressure would have been for 100 percent? I don't believe we have that calculation. We have an extrapolated curve.

MR. EBERSOLE: Exponential upward?

MR. MENDANCA: In the curve I saw it seemed to be linear.

MR. EBERSOLE: I don't think it is.

MR. ETHERINGTON: As long as plants are operating at 100 percent, I think we should have that number.

MR. MENDANCA: Yes. The calculations that we asked for or that we received are for the half scram. That answered the immediate question of the scram discharge volume. Perhaps you are correct.

The second criteria which we generally look at in ATWS mitigation is the heat removal capability, and that is generally limited by your suppression pool temperature. We have various calculations in that area.

The first preliminary results which we received informally from General Electric and are specific to Browns

Ferry, the first calculation at this point is again for MSIV closure from 100 percent power, and this is for the plants with the RPT.

MR. ETHERINGTON: Is there a steam bypass there open?

MR. MENDANCA: No, sir. This is for suppression pool temperature. The assumptions here are the 1 RHR at 30 minutes and standby liquid control at 30 minutes, and a temperature of 190 degrees Farenheit in the suppression pool.

The second calculation was for the half rods out, half rods in configuration. It is a bit different assumption, and it shows you have to initiate the standby liquid control system at an earlier time in order to meet the 200 degree limit which is applicable when you have quenchers.

MR. OKRENT: How many have quenchers, by the way?

MR. MENDANCA: I don't believe any do. There may be one that does, but I am not certain on that right now.

MR. OKRENT: They are currently running without quenchers.

MR. MENDANCA: That is correct.

MR. EBERSOLE: There is something missing from that that I think is most important, and that is aux feedwater slow or HPCI or RCS, one or all. It is not there.

MR. MENDANCA: It is assumed to work on its automatic signals. I know, I think it would be informative to have it, and the requirements on it.

The final list under the generic BWR calculations are the ones we have received formerly from General Electric. The first is a Browns Ferry-3 type scram, and it assumes actuation of your emergency systems in ten minutes, which is more in line with our current licensing basis type assumptions, I would say.

Your temperature maximum there is 153 degrees Farenheit. If one postulates, which we did require, that General Electric postulate a longer time in initiating their standby liquid control than 30 minute initiation, you come to the next calculation which indicates somewhere around 190 degrees, 186 degrees Farenheit.

These are for scrams similar to the Browns Ferry. That is approximating the Browns Ferry type scram. For conditions where you have half the rods not going in, again the limiting case from our observation of the phenomena at Browns Ferry-3 and initiating your RHR at 10 minutes, you would require that you would initiate your standby liquid control system at a fairly early time, about five minutes.

GE feels this is an adequate time, and they indicated so in their letter to me, less than 200 degrees.

I wanted to present these consequences. They are important in our evaluation of what needs to be done and what has been done.

I think I would like to have Vince answer --

MR. PLESSET: When are the installations supposed to be completed? Is there a schedule?

MR. TOLEDO: I think we will be going before the Commission with a proposed Action Plan, and I think either this week or next week. That is about the closest we can come to it. I cannot tell you what the thinking is, because we have not presented it to the Commission yet. It will be some time.

MR. SPEIS: We will attempt to provide this information via Thadani.

MR. EBERSOLE: Four thousand pounds at 100 percent. That is just a hand estimate. It may be a little higher.

MR. PLESSET: Any other question?

Well, back to you.

MR. SPEIS: I think in ten more minutes we can finish our presentation. Vince Panciena will talk about the long-term implications.

MR. PANCIENA: About the middle of July a review group was formed within the Division of System Integration to develop a plan of action to resolve this problem. About that same time within the staff there became a concern that we were not sure what the as-built condition configuration of the plants, the operating plants were.

As a result of that, we set up a series of NRC regional meetings. These meetings took place the third -- the last week of July. The meetings were held at Chicago, Atlanta, and Philadelphia. During these meetings -- the objective of the meetings was to obtain an in depth understanding of the as-built condition of the scram discharge volume, the instrumented volume, and the interconnecting piping, vent and drain systems.

The general areas covered during these meetings were we looked at the general layout of the plant, looked at the general layout of the systems. We actually received from most of the licensees as-built drawings showing the as-built conditions of the systems.

We discussed system design requirements, the system interties, primarily the vent and drain systems, what systems intertie into those systems. We discussed the NSSS-AE interface, because we were not quite sure just what kind of interfaces existed.

We also discussed recent tests involving valve opening and closing tests on the vent and drain valves, and the drain tests themselves. These tests were simulated tests where they -- where the licensee filed the headers and then timed the amount of time it would take to drain the system.

Lastly, we discussed emergency procedures primarily dealing with verification that the operator did have authority to actuate standby liquid level control.

MR. MOELLER: These were, as you say, regional meetings. What was the purpose of doing it regionally?

MR. PANCIENA: The purpose was to, in a short period of time, get an understanding of just what the system configuration looked like, because there was some concern that developed that we found problems where valves were installed backwards; we found problems where there was some concern that there were slopes, that the lines leading from the scram discharge volume to the instrument volume were not sloped correctly.

And so the purpose of the meeting was to quickly get out there and have a face-to-face shiftsleeve meeting with each licensee and to obtain that information quickly.

MR. MOELLER: You did it regionally and covered the plants in that region only.

MR. PANCIENA: In Region III we did cover some of the plants that were in the regions further west. We did not cover Humboldt Bay, because Humboldt Bay is in cold shutdown.

To gain a little bit of perspective, I would like to discuss some general results that we obtained from these regional meetings.

(Slide.)

Basically there are two basic configurations. The first configuration, we have a single instrumented volume. In the second configuration we have an instrument volume of each scram discharge header. I have shown these schematically as such.

(Slide.)

At the same time I would like to discuss the vent and drain configurations that go along with this general layout.

All plants, with the exception of the newer plants -- Brunswick, Hatch, and Duane Arnold -- have this kind of configuration where you have two headers, usually an east and west or north and south scram discharge volume header, both feeding to an instrumented volume.

In all cases the line connecting the scram discharge volume to the instrumented volume is two-inch pipe. Similarly, the drain line coming off the instrumented volume is also two-inch pipe.

All plants have a vent system composed of one-inch pipe connected to a vent valve. In this kind of configuration where you have one instrumented volume, there is one plant, Nine Mile Point, that has a vent configuration where there is only one vent valve for both instrumented volumes.

The second configuration is found on the newer plants.

(Slide.)

And in this case here you have the scram discharge volume. You have a large connecting eight-inch pipe that connects into the ten-inch instrumented volume. You have the drain line from each instrumented volume going into a single "T," this being two inches, this also being two inches, going into a two-inch drain valve.

Similarly, most plants have a single vent valve of each scram discharge volume header. There are two plants, Duane Arnold and Brunswick, that have the single valve configuration.

I'd like to say something about what we found as far as the vent systems themselves. We found a wide spectrum of configurations. We found some plants -- for example, Monticello -- that had both a dedicated vent and a dedicated drain system.

By "dedicated" I mean that they only serve that one purpose. We found most plants had interties where they intertied with other systems. In the extreme case -- and I think one of the extreme cases being Browns Ferry -- there was something like on the order of -- it went into a common clean rad waste header, and there was something like over 16 interties.

We also found in some cases where the vent systems actually went down -- and I think this was pointed out this morning by the fellows -- they went down and actually tie into a drain system.

(Slide.)

I would like to spend a little bit of time on design interfaces, because I think there was some confusion in our mind, and I think the regional meetings helped us to understand the situation.

This system was -- in one case where GE was the turnkey contractor, this system was procured by General Electric Company. However, the General Electric Company supplied the major components -- the vent valve, the drain valve ...strumented -- the float instruments.

GE in turn then subcontracted this work to an outfit called Reactor Controls located in California. Reactor Controls did all of the design work and provided the fabrication and built the system. So that was one situation. GE acting as a prime contractor subcontracted this work.

In the other situation where GE did not have a turnkey contract, a very similar path was followed in that GE provided a functional specification for the system, which included such things as slopes of lines, size of the volume to be provided in the scram discharge volume. And GE provided that specification to the licensee. The licensee in turn either subcontracted the work to Reactor Controls themselves -- Reactor Controls directly or through the licensee's AE.

So basically this system was basically designed by one outfit, Reactor Controls, under a subcontract, with major controls such as the vent and drain valves being supplied by General Electric.

MR. SIESS: Did they design any other systems?

MR. PANCIENA: I don't know that.

MR. SIESS: Has anybody tried to find out? It seems to me that after Three Mile Island, we discovered that auxiliary feedwater systems have a great variety of designs, and consequently a great number of reliabilities. I think that another system has the great deal of design reliability.

Which is going to be the next one? Can we think about it and maybe find it out before it happens this time?

MR. PANCIENA: I think that is a good point.

MR. LEWIS: We won't do anything if we do.

MR. EBERSOLE: What is the basis here? Is it inadequacy in the degrees of specification in detail, this horrible thing we call specificity, or is it prescriptive? Is it inadequate prescriptiveness, inadequate specifications? Surely the generic aspects of getting a product like this out of narrative instructions has got to be looked at.

MR. PANCIENA: Yes, sir. My general feeling is that this system was relegated to a secondary position.

MR. EBERSOLE: It moves into the field of what should be a standard requirement which would avoid these things occurring.

MR. PANCIENA: We also found -- I will go back to this.

(Slide)

We find varying lengths here. The long side could vary up to -- the longest the found was about 180 feet, down to something like 100 feet. The short side would be more like 20 feet. So we found variations in the piping dimensions, both in the vent drain and in the piping connecting the scram discharge volume with the instrument volume.

So basically we found nothing that really approached even the standard design.

MR. ETHERINGTON: Are these systems designed for reactor pressure over 15 psi?

MR. PANCIENA: They are designed for reactor pressure, I know that.

MR. BENDER: These systems that you mentioned being designed by Reactor Controls Corporation or whoever they were, that was under subcontract to G.E.?

MR. PANCIENA: Subcontracted to G.E., in the case where G.E. was a turnkey contractor.

MR. BENDER: I see.

MR. PANCIENA: In the case where G.E. was not a turnkey contractor, then this system was subcontracted by the AE to Reactor Controls or by the licensee directly.

MR. BENDER: When G.E. was a turnkey contractor, did they review these designs?

MR. PANCIENA: That I am just not sure of.

MR. BENDER: Are you looking into that aspect?

MR. PANCIENA: We are trying to look into that aspect.

MR. BENDER: Thank you, sir.

MR. EBERSOLE: Are you aware of a case where an architect engineer did the balance of plant but Control Engineering did this part?

MR. PANCIENA: Yes, sir.

MR. EBERSOLE: You would not call that a turnkey.

MR. PANCIENA: The difference between turnkey and nonturnkey is that G.E. was the turnkey in those major contracts. Let me get on to long-term actions. We are in the process of reviewing the responses to the bulletins. We have started on the responses to 80-14, I&E Bulletin 80-14, and we are starting a review of the responses to at least supplements 1 and 2 to I&E 80-17.

So we hope to complete this in a very short period of time because I think we really have to understand what we are being told by the licensee to do, this adequate job of long-term corrective action. We are reviewing the as-built systems as a result of the I&E bulletins and the information we got during our regional meetings.

We have started to develop a matrix that tries to lay out what are the basic characteristics of the system. I think the key to the long-term actions, though, is we have encouraged the owner group participation, and we have encouraged the owners to organize a subgroup to develop design and performance criteria for the scram system. I would like to maybe generalize this a little bit more.

It is not just the SDV, but it is going to include more of the hydraulic systems and possibly some of the auxiliary systems, so it is just not going to be limited to the scram discharge volume. I would like to show this flow chart.

(Slide)

Our plan is to get the owners group to develop or to propose design and performance criteria. We will take into consideration the Michaelson effort as well as comments we received from the ACRS, our bulletin response and review of the as-built drawings.

We are in the process of coming up with what I feel is at least a minimum acceptable list of requirements. We intend to work with the owners to develop or to understand what is needed in the way of criteria. Our plan is that we will meet with the owners as needed in a short period of time to come up with agreed upon criteria which would allow the owners then to implement on a plant-specific basis those changes that are necessary for each plant to meet the criteria.

Mention was made this morning about the G.E. recommendations which have been developed. These G.E. recommendations will be factored into the effort that we are currently undergoing. At the present time a subgroup has been formed. The chairman of that subgroup is a man by the name of Tom Dente, who is from Northeast Utilities, and they are currently meeting.

They met yesterday and they met today to at least start this work going.

(Slide)

The upcoming action at the present time. As I mentioned, there is a meeting with the owners subgroup today and yesterday. We expect to receive the results next Monday. Now, I do not know how final these will be. I presume they will be preliminary results. The staff will have a meeting with the subgroup during the week of the 15th.

I expect that by the third week in October we will have approved design and performance criteria in place. That is the kind of time frame we are workign on. We are asking each licensee to provide schedules for plant-specific modifications by December 15. That is my letter. That concludes my presentation.

MR. PLESSET: Any other point?

Denny?

MR. ROSS: I had two remarks. First, within both I&E and NRR, this problem -- and it is a problem -- has fairly high priority. We have several men assigned to it, senior staff, and it is their number one assignment.

The other thing is the Committee will be seeing this again. It is the first OL matter, which will be LaSalle. It is on the Committee's agenda this fall sometime, so in addition to whatever comments you might want to make on operating reactors, you will get another bite on this subject on the next OL.

MR. PLESSET: Okay, thank you.

MR. EBERSOLE: One way to look at this event is the more general context than just boilers. This should be a precursor as to what you find when you look at a system more deeply. I presume certainly it will be considered as such a precursor to the extent that we look at what might be the equivalent of a scram dump volume in the PWR field.

MR. PLESSET: Any other comment?

If not, we will recess for lunch and return in one hour.

(Whereupon, at 1:15 p.m., the Committee recessed, to reconvene at 2:15 p.m. the same day.)

AFTERNOON SESSION

(2:15 p.m.)

MR. PLESSET: Let's reconvene and go to our next item on the agenda, the report of the ACRS Subcommittee on Quantitative Risk Criteria.

Dave.

MR. OKRENT: Yesterday, if you noticed it when you were picking up your folders, there was a package of three memoranda which you should have received, in white, relating to the subject of quantitative risk assessment criteria. Today while you were having lunch --

MR. KERR: What is your confidence level that we received that?

MR. PLESSET: It is very high.

MR. OKRENT: I don't know that you have it because --

MR. KERR: I don't think I have it. Are there any more?

MR. QUITTSCHREIBER: This was passed out last night about 6 o'clock.

MR. OKRENT: If you do not have the three memoranda, one of which is marked Part I, one of which is marked Part II, and the third of which is not marked Part III but is by Johnson and Kastenberg, tell Gary Quittschreiber and he will know what you need.

Then this noon while you were at lunch you were given a short green draft letter. Let me, then, having mentioned that, summarize where we are and where we are supposed to go.

You will recall in May 1979 we recommended that the Commission work on development of quantitative risk goals for safety, and a couple of months later Chairman Ahearn asked could we provide some specific input into this, and we said we would in about a year. We are just about a month behind our year right now.

More recently, in some of the stuff being written by the Office of Policy Evaluation -- I think that is the name -- they are sort of casting into concrete that we are going to try to provide something in about a year. They are going to try to get something up to the Commission.

So the Subcommittee on Reliability and Probabilistic Assessment or whatever its name is has been working in the area, and we had a briefing on the general subject at a full committee meeting some time ago trying to give you an idea of the kind of approach we had in mind. Last month we sort of had a freewheeling discussion with knowledgeable people, let's say, from the outside coming in, and speaking, if you remember.

The approach we have taken is to prepare three memoranda. The first one is intended to serve as a review of some of the proposals which had previously been made for quantitative safety goals, not exclusively as applied to nuclear power reactors. Many were, but not exclusively. It was not intended to include all that had been made, but what we thought were an interesting selection. And that was called Part I.

My guess is that that document is in fairly good shape. I hope we find it that way. I suspect it is not a particularly controversial kind of a document since what we tried to do is reflect what other people have proposed. But you will have to read it if you have not.

Then the second document, Part II, is entitled Rulemaking on -- it is a specific proposal prepared by ACRS fellow Peismeyer (phonetic) and myself, trying to take advantage of the inputs we had in the subcommittee meetings with consultants and so forth, and would, in effect, represent what you might call the new proposal that exists in this packet of information. I hope it is in reasonably good shape. I will have to wait until I hear what you have to say.

This, in my opinion, is certainly the document on which you should focus primary attention. The third document, which is by Kastenberg and Johnson, which is entitled A Study of the Application of Risk Assessment Criteria, is an effort to look at a few technologies in terms of risk assessment criteria, somewhat like what is in the second document, but they did not have the second document. They had to work with a previous draft, so it is not -- there is not now a one-to-one correspondence -- we can make it that but there is not such a one -- and to see what would happen for a limited number of technologies and technological situations.

Just to give one a little bit of a feeling, that document has one or two areas where we know we want to modify it somewhat. For example, in L&G, there is a numerical difference between what they obtained versus something that is in the report, and we don't know whose arithmetic was right.

And we have one more technology which we always intended to try to include, which we weren't able to pick up, which one of our new ACRS fellows is going to try to do on an accelerated time scale, and it may or may not be -- on dams -- and we are going to try to see there if we can look at empirical information on dams and devise certain kinds of risk numbers and then do a risk study kind of thing on dams and see how they look, and then how both of these compare against the criteria.

MR. PLESSET: You had better put that in.

MR. OKRENT: That will be in Part III.

So, by way of introduction, that is what these three memoranda are. Again I want to repeat my firm opinion is the most important one to look at is Part II. After that, I would say, is the summary in Part III, but there are several appendices in Part III which, hopefully, are accurately reflected in the executive summary.

My guess is that Part I represents an area that is the least controversial.

Now, the way we have in mind trying to proceed, as you have probably noticed, is that tomorrow morning there is a block of time, roughly 3-1/2 hours, for the Committee discussion of this. Actually, four hours is shown. I think it is intended that a half-hour of that be used by Mr. Stegy to tell us what the Commission has in mind from the point of view of proceeding on development of quantitative safety goals.

They made recommendations to the Commission for a program aimed at developing something on a fairly short time scale. I am not sure whether he will be on first in the morning or last, but my preference would be that he be last. He would be sort of the dessert, something to look forward to if you are good boys and finish in time.

If you look at the letter hurriedly, you will see that the first page is boilerplate and the second page is boilerplate, until about the last paragraph, which says that the Committee hopes that this set of memoranda will contribute to the process of the development of quantitative criteria. And up on the top it says that this is expected only to be a first step in an interim process.

So the intent is not that this is -- whatever it is the Committee sends, if it sends something, it is not supposed to be the be all and the end all, but one specific thing that people might talk about and criticize. So it is intended not that this be something that the Committee thinks is the way to go, but it is in good enough shape to serve as something to discuss.

Now, I can see three or four possible ends of what we do this month and/or next month. We might decide it is not ready to go anywhere, so back to the drawing board. We might talk about it at this time and get subsequent comments that you want included by next month, and hopefully we can finish it by next month.

We might look at it and say, well, it seems pretty good, leave it to the subcommittee chairmen and the fellows to work on editorial parts and members would give specific editorial recommendations or so forth as to where they think there should be changes, but there is nothing that looks like it just has to be changed, an important number or concept or whatever it is.

In other words, the Committee might say we are happy as of this month, but clean up the editorial part and get it out by October, but we don't have to have it back on the agenda. And you can probably think of one or two other variants on that. I am not going to try to guess in advance where we are likely to end up.

Now, my request, and it is a firm request for the discussion tomorrow, is as a minimum you have carefully read Part II, that you have made your comments and you have them well articulated, if you can, written out or whatever. If you have editorial kinds of comments, write them into the copy. That is the easiest way to handle it.

You should not try to handle anything that is even semi-editorial, semi-technical. I think we ought to at least initially see what are the important questions, and then I hope we could look at what I call the summary of Part III. We can also talk about Part I and that would be good, but, you know, you ought to know what is in Part I and Part III so you know what is the subject of discussion.

But Part II is where there is a specific proposal, and it is somewhat different than what you heard a couple of months ago because this is a version that Reismeyer and I prepared on Labor Day. I want you to know ACRS fellows work even on Labor Day.

That is by way of introduction. I don't know what further discussion you would like now or whether members have some specific questions.

MR. PLESSET: There may be some questions. I think Paul had a question.

MR. SHEWMON: I think I would be very unenthusiastic about trying to get a letter out at this meeting. Some people read fast and late at night. I don't read well late at night or particularly fast. I think to say all comments must be in writing by tomorrow so we can write a letter on Saturday is just rushing.

MR. PLESSET: Comment, Dave?

MR. OKRENT: I had assumed myself we would need October. I said --

MR. PLESSET: That is what I thought, too.

MR. OKRENT: But I did not want to preempt the Committee from the possibility of throwing up its hands 00

(Laughter.)

MR. PLESSET: Do you mean washing its hands?

MR. OKRENT: Or whatever. But let me say I think, though, nevertheless, whether Paul reads slow or fast, he should stay up long enough to read Part II.

(Laughter.)

MR. PLESSET: Okay. I think that helps Paul. It certainly clarifies the point.

Any other question of Dave? Yes, Mike?

MR. BENDER: I only had a chance to skim the document, Dave, so I am really not trying to comment on it in any substantive way, but it seems to emphasize primarily reactors. Is that the charter which you envisioned?

MR. OKRENT: Well, the Committee recommended that the NRC develop quantitative goals for reactors. We did say in our original letter that the NRC should advise the Congress of this and ask the Congress for its opinion in a broader context. In fact, the draft letter mentions this. But nevertheless, Part II is in terms of reactors. In Part III, as I indicated, a look is taken at what this same set of criteria, though, would mean for some other system.

So it is there. But Part III is not an exhaustive look. There just is not time.

MR. BENDER: I had in mind addressing the whole fuel cycle.

MR. OKRENT: I am sorry. Again, we did not look at the rest of the fuel cycle here.

Now, EPA in effect has gotten some criteria for the whole fuel cycle and so forth, but you are correct. The original proposal was for quantitative safety goals for nuclear power reactors.

MR. PLESSET: Any other question of Dave now that you know your evening reading is prescribed?

MR. OKRENT: I should note one other thing. There were a few typographical errors in Part 2. We will have, rather than read them to you actually --

MR. SHEWMON: Let us find them for ourselves.

(Laughter.)

MR. OKRENT: I'll let you find others, but the secretary did a remarkable job considering she had to type from Reismeyer's scrawl and mine superimposed on his in single space writing.

(Laughter.)

In fact, some of the things were not even her fault. We will have a new Part 2 for you before the end of the day. And will it be green?

MR. REISMEYER: Yes.

MR. OKRENT: So you should get one Part 2 in green, and we will have not a large number of corrections, but some of them are important.

MR. PLESSET: Thank you, Dave.

MR. OKRENT: We missed the reactor scam. How did that happen?

MR. PLESSET: Anyway -- well, I appreciate your brevity, Dave, I really do, because it has helped us a lot in our schedule. And I think we will proceed to our next item regarding the Sequoyah Nuclear Power Plant.

And before I call on the subcommittee chairmen to report, I was told before our recess for lunch that they had brought a model of the containment and of the globe igniters, is that correct?

They were going to put them on the table, but we thought that somebody might come in and take them out. So any time you want to wander over there, take a look, without disrupting the meeting too much. Feel free to do so.

MR. LEWIS: Are they going to inflate them and we find out what the pressure limit is?

(Laughter.)

MR. BENDER: We used to get some zinc batteries and hydrochloric acid to fill balloons with. Could we try that technique today and see what happens?

(Laughter.)

MR. PLESSET: I don't know whether we want to. I will take it under advisement, Mike.

Now, the items that should be pertinent to our discussion were given to you mostly separately, right?

MR. QUITTSCHREIBER: Separate bundle.

MR. PLESSET: In the front of your notebook. So you might want to make sure you have those in hand. And as I mentioned to you, we had two subcommittee meetings on this topic, and we should first get reports from them. And even before that I think that the Sequoyah subcommittee chairman, Dr. Mark, wants to make a brief comment regarding some of the specific features of Sequoyah and results of the augmented low power tests. So let's do that before we go to the subcommittee reports.

Carson, would you go ahead?

MR. MARK: I would hope I could have my little say here.

You will remember in July the committee wrote what it thought was its letter on Sequoyah. Commissioner Gilinsky was interested in the question of hydrogen control. He sent back a letter on August 7th asking for further clarification of the committee's position on hydrogen control. He had two reasonably simply stated questions.

I don't have the letter in front of me. One had to do with -- does anyone have that letter? Do you have the questions? It is in my bundle somewhere.

The questions I am thinking of are the ones of the date August 7. "Does the committee believe that additional hydrogen control measures are necessary for ice condenser containments" is one question; and the other, "Is the committee reasonably persuaded of the effectiveness of distributed igniters in ice condenser containments? Can they be counted on to keep pressure increases caused by hydrogen burn at suitably low levels, which I would define as design pressures, during accident sequences involving

TMI-like quantities of hydrogen?"

He goes on to say that, "We are not talking of dealing with remotely hypothetical events, but protecting against what was experienced last year."

I think in one form today's discussion is supposed to provide the basis for answers or comments on those questions. In a different form the Commission is going to be reviewing the staff's current -- yesterday's proposal that a full power operating license be issued for Sequoyah tomorrow afternoon; and they very much want comments from the committee on, I believe, the general subject that touches things most particularly related to those questions.

Going back just for a moment, you have in your hands an SER and the staff's submission to the Commission in connection with the license that just came up. The low power tests have been completed. I think there will be some discussion of them in the course of the presentations made today. I won't say anything on that.

Last time we talked about a question as to whether signficant information had been withheld. It may come out today, but I believe the situation is now at the point that I&E has concluded there was no intent of that sort on the part of the TVA.

The general status of Sequoyah is that all non-TMI and I guess even TMI-related issues other than hydrogen control are either resolved or a solution has been either implemented or scheduled, and assumes further questions are a formality.

Maybe the staff or TVA will want to comment on that situation.

TVA thinks of itself as ready to start its power ascension progress as soon as they should receive a license. And I believe that is all I have to report except for the fact that the two subcommittees are much more pertinent to today's discussion -- the Class 9 subcommittee and the Subcommittee on Structural Features, which met yesterday or the day before. Those are not Sequoyah subcommittees, but they have everything to do with what is up for discussion on Sequoyah at this stage and --

MR. PLESSET: I think that Carson described the situation. Let me amplify or repeat. The hope of the Commission is that we will have a fairly finalized version of our comments on the questions raised by Commissioner Gilinsky, namely the question of hydrogen generation and control, which was examined in detail by Dr. Kerr's subcommittee, and then there were some differences in the evaluation of the containment capability, which was examined by Dr. Siess' subcommittee.

These were two questions that were raised and which we have looked into in some depth through these two subcommittees. So I would like you to keep in mind our need to get some formulation for a letter. This will be to the Commission -- that is, Chairman Ahearne -- on this whole matter.

So in order to get on with this, let me first call on Bill Kerr. Would you want to give us your subcommittee report?

MR. KERR: You have a one-page memorandum which attempts to summarize that part of the meeting that was relevant to Sequoyah. We saw results of calculations which had to do with assumptions about hydrogen generation that were deemed appropriate to the kind of incident that occurred at TMI-2.

And the consensus of these calculations, using the MARCH code and TVA -- I guess it was a Westinghouse-developed code that TVA used -- is that one can, by using an appropriate ignition system, keep the containment pressure below about 30 psia absolute by operating the igniter system to burn hydrogen in the containment air up to an amount of hydrogen equal to about 70 percent of that, which would be produced by a total zirconium-water reaction of all the zirconium available.

This assumption does not involve any detailed look at the way in which the igniters operate. It is an assumption which is based on putting into the computer the fact that the igniters begin burning at a certain concentration of hydrogen, and that the burning continued to the point at which one expected burning to stop. Given a concentration of hydrogen and that given this evolution rate of hydrogen, burning starts again.

But it does not model in any detail the performance of the individual igniters. So TVA and the NRC staff independently are carrying out experiments which they expect will give additional information on the performance of igniters in systems involving air, hydrogen, and steam of appropriate concentrations.

The committee also heard a short presentation by Dr. Hubbard of R&D Associates, since R&D had made some comments on hydrogen handling in the Sequoyah containment. Dr. Hubbard emphasized that their treatment had been brief and not very involved and not very detailed.

He did recommend inerting. Their recommendation was not based on any detailed consideration of the problems that might be associated with inerting, but rather on the fact that they did not, on the basis of their analysis, have any indication that the igniters would necessarily work. Appropriately and hence were recommending some other approach be used, because they thought it would be more nearly appropriate.

This deals with part of the Sequoyah problem.

Even if one assumes that the igniter system will work and will keep the containment pressure below some number such that the containment will indeed contain, there is a further question of what to do about operation of Sequoyah between now and the time at which the igniter system will be sufficiently developed and sufficient experimental evidence will exist so that the staff and TVA will agree that it should be operated.

The subcommittee and the consultants I believe reached a consensus, with the exception of one consultant, that probably the igniter system looked promising, and subject to further information was an appropriate at least interim approach to handling the hydrogen.

There was one consultant who had some questions about it based on the fact that it was relatively untried and also on some information that was provided in a Brookhaven report, copies of which I think you have. This had to do primarily with the effect of hydrogen burn on some of the components of the ice condenser.

I did not urge the subcommittee members there present or the consultants to try to pass on the question of what should occur between now and the time of igniter operation. That I think is something that -- with which the committee needs to deal, and it has to be based on taking into account a number of considerations.

I have in the memorandum given my personal recommendation. That is all I have to say.

MR. PLESSET: Thank you, Bill.

I don't know if you want to comment on a point that came up about detonations. I know that you and other members of the subcommittee did consider this problem with some care.

Do you want to comment on that?

MR. KERR: Well, I think much of the consideration of that question has been done by Carson, and unless Carson is unwilling, I would ask him to comment on it.

MR. PLESSET: All right. Do you want to do that, Carson, briefly?

MR. MARK: Let me say just a word or two then. In the subcommittee meeting that Bill is referring to, Dr. Hubbard had in particular -- he was not the only one, I think -- raised the subject of detonation in quite alarmist terms, that the distribution of hydrogen in the containment might not be even, and there might be detonable pockets. And if one of those got ignited, there might be a detonation. And left it to the reader to assume that that is the end of Sequoyah.

That raises a question as to whether it is really true or not. As you know, in WASH-1400 for a large dry containment, the argument is gone through at considerable length that a hydrogen detonation in contact with the wall will not disturb the wall.

I had a question then as to whether that situation might not also apply to Sequoyah. And with the help of Paul Shewmon in particular, my own conviction that you may have a hydrogen detonation in the Sequoyah plant without doing damage to the wall. The consideration will be how much hydrogen is involved and what will be the quasi-equilibrium pressure afterwards for which the containment capability -- Chet will comment on that, I think -- becomes the only important consideration.

There is a draft of a possible letter to Gilinsky which I think will be easier to discuss after we have had the presentations. Attached to that is the argument that Paul put together on this subject. There are also in here references to a study by TVA which brings them to the same conclusion; but I think it is nothing as good as Paul's discussion is that --

MR. PLESSET: Fine. I have not seen the TVA one, but I am familiar with Paul's. I know it is here.

MR. MARK: You won't see it by the time you finish this, but they did it, or the staff says they did it.

MR. PLESSET: Do you want to comment, Paul?

MR. SHEWMON: No. I had a question.

MR. PLESSET: Let me make a comment on the thing that you and Carson did. I thought it was very nice. I think it is on the conservative side myself. I think you overestimate the load a little bit. You would not want to argue with me about that, would you?

MR. SHEWMON: Ten percent of the yield? I do not care.

MR. PLESSET: Okay. Go ahead with your question.

MR. SHEWMON: Bill, will we -- the question of mixing, the degree to which that came in, whether it is bad or good and what happens comes up in your summary. And perhaps you are getting to this in the presentations, but the other is whether or not the igniter system will work, and what is meant by work I guess is something I would like to hear more about.

Will that also come up in the discussions?

MR. KERR: I would hope that both of these come up in discussion. The assumption is that mixing is fairly good.

MR. SHEWMON: It is relatively complete or relatively beneficial.

MR. KERR: That it is relatively complete and that one does not get significant amounts of pocketing. The pocketing question has not been looked at in detail as far as I know.

Your second question was will the igniters work, and I would say that further experimental evidence will add to our knowledge of that question. I don't know with what confidence one can finally demonstrate that they will or will not work. I would doubt if it would be with 100 percent confidence, but there will be experiments in an attempt to simulate the conditions that one might expect in containment, and the igniters will be in place and will attempt to ignite.

Both Livermore and a laboratory which TVA is working with will run experiments which are expected to give information about this. I think some comments will be made about that during the presentations.

MR. SHEWMON: And it is basically whether they will ignite mixtures which are ignitable, is that right?

MR. KERR: I think one could put it that way. One also might say that they will attempt to find out what mixtures are ignitable.

MR. PLESSET: Any other question of Dr. Kerr? If not, I would like to call on Dr. Siess to give a report of his Structural Engineering Subcommittee.

MR. SIESS: I would like to report in two parts. I would like to make a brief report at this point and then when the time comes for presentations by the staff and consultants, etcetera, later on, I would like to have a few minutes to give some introductory material in the hope that it will keep them from each repeating the same introductory material.

The committee met on Tuesday. We reviewed the various analyses that had been made of the capacity of the containment to resist uniform static pressure. We did not take into account the dynamic effects.

We heard from six people, I guess, with seven different analyses based on different assumptions. This is summarized to some extent in the written committee report which is in that same package as the other stuff with a rubber band around it, which I suggest you read at your leisure.

We arrived at a committee recommendation, a committee judgment as to the pressure. It is not one number. We will give you several to choose from.

Thirty-pounds per square inch we consider a conservative lower bound based on neglecting the effects of the stiffening longitudinal members. A reasonable value for a first yielding other than very local bending we considered to be somewhere in the neighborhood of 46 pounds per square inch. A reasonable lower bound or limit capacity based on general yielding but still at very limited deformations, less than an inch or so, we considered to be about 50 pounds per square inch. And then a best estimate of a limit capacity, and I would express that as what I would expect to see if we actually made a test, is somewhere around 55 pounds per square inch.

Now, in all of those numbers I can put a plus or minus 10 percent on them.

We have a range of values from a very conservative lower bound to a best estimate. We will have more information on that and the other assumptions which those are based on later on.

MR. SHEWMON: Is the limit somewhat related to where you think the rupture might occur?

MR. SIESS: There's a general yielding. Rupture is ruled out. Rupture would involve such large deformation, they cannot even be considered. The limit here really comes from elasto-plastic analysis, the real strength hardening material. There would be some higher capacity.

MR. SHEWMON: What is the definition of "limit"?

MR. SIESS: Just what I said. From elasto-plastic analysis, it is the best you can get, beyond which you cannot go.

MR. SHEWMON: It blows up like a balloon.

MR. SIESS: Yes. 20 percent strain, this would look a lot like a balloon in this region, if you go out that far. But those are very small deformations, probably less than one percent strain anywhere, except local bending.

Later on I can give you a little more introduction as to how we got here. You will not hear all seven analyses.

MR. PLESSET: You can spare us that.

MR. SIESS: It was narrowed down. In the meantime, I would like you to read the report, because it lists eight assumptions that were made to get here. You might want to question some of them.

MR. PLESSET: Any other question of Chet before we go forward? I guess not. Thank you, Chet.

I think we will now go to the staff. I think that, Carl, will you be --

MR. STAHL: Yes. My name is Carl Stahl. I am the project manager for the NRC in the Sequoyah plant. My status is brief here. We have provided you two packages, the first of which is the package that has been submitted to the Commissioners, and it contains all the pertinent information for licensing the Sequoyah Plant Unit No. 1 for full power operations.

Of particular interest to you is supplement number 2, which covers all the non-TMI and TMI issues that have been previously discussed with you at the July sessions, and including others before that. Included in the package is supplement number 3. That is a draft version which we will publish shortly. That contains a great deal more information on the hydrogen control issue.

Included in that supplement 3 is the current information on the testing of igniters, that I think will be of particular interest to you. I also want to report that I have received a letter from Region II dated September 3. They have concluded that no further items of significance require resolution prior to issuance of a full power license on Unit No. 1. From their point of view, the plant is ready for power ascension and full power operations.

I wish to point out one additional item that has not been discussed with the Committee or the Subcommitte members, that quite recently, within the past two to three weeks, a member of the Brookhaven National Laboratory staff identified a concern that he had with respect to the insulating material that is inside the Sequoyah containment.

This matter is undergoing review with the staff. We have provided you today recent information from TVA that provides a discussion of this matter. It deals with the subject of the flammability of this particular insulation and the possible impact that this may have in the event of hydrogen burn taking place within the containment itself.

MR. SHEWMON: Where have you give us that?

MR. STAHL: That was distributed, I believe, to you in a separate package.

MR. PLESSET: It looks like this, Paul -- oh, it is coming to you right now.

MR. STAHL: It is coming to you, sir.

MR. SHEWMON: Two classes of citizens.

(Laughter).

MR. STAHL: That material is quite recent, like today. I apologize for the quality of the reproduced material. This came in by Telefax.

TVA is present. If there is any clarity needed, I'm sure they can provide it.

This actually concludes my brief status report. We can proceed to the agenda item if there are no further questions for me.

MR. SHEWMON: Let me ask a simple, non-technical question. Is the composition of this foam significantly different from the foam cup I have in my hand?

MR. STAHL: I will let TVA answer the chemical composition. Polyurethane is the material -- an insulating material. That is not polyurethane, I think. It certainly has different flammability characteristics. The characteristics of the material identified in the material we have submitted -- I think that is a better point of reference than my comments.

MR. PLESSET: Carl, if there are no more questions on that point, I would like to handle the containment discussion through Dr. Siess. So if you would let him take the show, it would be very acceptable to us.

MR. STAHL: Yes, sir.

MR. PLESSET: If that is all right with you.

MR. STAHL: It certainly is.

MR. PLESSET: All right, Chet.

MR. SIESS: They will get their chance.

MR. PLESSET: Yes. You will get your chance. I am not ruling anybody out. But I think Dr. Siess has been through all these various things and might be able to do it expeditiously, and that is what a Chairman always likes.

MR. SIESS: Okay. I'm glad they brought a model. I have a slide I would like to put up there. Can you put that slide up, please. You can look at the model.

The Sequoyah containment consists of a steel cylinder 115 feet in diameter. It has a hemispherical dome on the top and it is attached through bolts to about a ten-foot reinforced concrete slab at the bottom. The thickness of the cylindrical shell is an inch and three-eighths at the bottom one one-half inch up at the top. And that is obviously the weak spot up at the top.

There are meridianal stiffeners running up and down the thing at four-foot spacing; and then there are ring girders going all the way around at nine and a half foot spacing through most of the shell and closer spacing right up around the spring line.

The material that this thing is made of us steel SA 516 grade 60, which has a nominal specified yield strength of 32,000 pounds per square inch and a test yield strength of around [illegible],000. It has an ultimate strength of 60,000 nominal and about 65 ultimate. The ultimate strength occurs at about 20 to 25 percent strain.

And we ruled out any calculations based on ultimate, since 20 percent strain would mean something like ten-foot increase in the radius. That did not seem very reasonable.

In the cylindrical portion of the shell, which is the critical portion, the hoop stress governs. It is about twice the meridianal stress. If there is just simply a cylindrical shell, then the calculation of the stress is very straightforward, and the only questions that are involved in determining the capacity is what you equate the stress to.

What is the uni-axial strength from tests is one question; what is the bi-axial strength is another question. And that is a fairly straightforward calculation. The stiffeners, however, do affect the capacity, and additional complications are introduced into the calculation.

If we only had the ring stiffeners and they are spaced about nine feet apart, they would have little effect upon the capacity, since the region of the shell in between the ring stiffeners would govern. What we have in addition to the ring stiffeners, we have the longitudinal or meridianal stiffeners that are spaced about four feet apart around the periphery, and these have a clear strengthening effect.

Now, there have been analyses made neglecting the stiffeners. Those are perfectly straightforward. The only thing you have to decide is what is your yield strength and yield criteria.

There have been various analyses made trying to take into account the effect of the stiffeners, with quite a range of degrees of sophistication. The Ames Laboratory tests were the first ones made for the staff. R&DA made some analyses, relatively approximate ones, on the behavior with stiffeners. And Dr. Bagchi, Chief of NRC Research, made some back of the envelope calculations taking into account the effect of the stiffeners. These are quite approximate analyses.

There have also been some fairly sophisticated analyses made taking into account the stiffeners.

All analyses involve some portion of the shell. Ames Laboratory has made some finite element analyses. Dr. Udens has made some, and TVA -- actually, Offshore Power Systems for TVA has made a finite element analysis.

The results that I mentioned earlier, the area stress levels, the 38,000 to the square inch, is a simple shell analysis -- no stiffeners -- using a test yield strength and the Von Mises criterion. The others are all analyses involving the stiffeners.

The 46 psi basically comes from Dr. Zudans' analysis, which is strictly elastic. It does not go into the inelastic range. The two upper values that come out of the OPS-TVA analysis, which involve elastic-plastic behavior model the panel.

What I am proposing that we do here is hear not from everyone -- I don't know whether the staff wants us to hear a presentation from Ames. We decided in the Subcommittee we would not. The staff position is not using the Ames analysis which was made for them. The Ames analysis tends to give somewhat higher values than the TVA analysis, which I think is probably a little more correct. And since it does not really enter into our recommendations, we did not think there was too much point in hearing from them.

The analysis by the TVA staff and OPS I do want you to hear, because there is more than one part to it. They have looked at penetrations and hold-down bolts and other things that could affect the strength, and I would like you to see the scope of their review. And that will probably be in two parts: first, the TVA presentation; and then Dick Orr from OPS will make his presentation.

Then I would like to ask Dr. Zudans to tell us what he did to sort of evaluate for us. He is our consultant, and he is an expert in his own right here. And then we can ask the staff to present the basis for their position. They had a very, very short presentation at the Subcommittee meeting, but we definitely want to hear that.

And then R&D Associates -- I believe Mr. Parry is here -- indicated at the Subcommittee meeting that they felt the final TVA analysis was the direction they thought we should go. And I would like for them to comment on where they -- what they think about what we got.

MR. PLESSET: It sounds --

MR. SIESS: We will start with TVA, I believe, and then we will have some initial studies. And then -- I think that --

(Slide).

MR. MOELLER: The vertical, whatever word you use for them, the strengtheners, the stiffeners, do they tie into the dome or do they go on up over the dome and back down?

MR. SIESS: TVA, can you answer that?

MR. MILLS: I think Mr. Don Denton can answer that as soon as he takes the stand.

MR. PLESSET: Let's let him make his presentation, then.

MR. MILLS: Dr. Plesset, we will have Don Denton from our engineering design organization make a very brief statement, and we will follow that by Mr. Richard Orr from

OPS.

MR. DENTON: Would you be kind enough to get my handouts out of my briefcase back there and hand them out to the people, please.

I will answer the question about stiffeners. The stiffeners taper down. The vertical stiffeners taper down at this point here, just beyond the top circumferential stiffener.

MR. SIESS: I thought the model would have the stiffeners on it. I am disappointed.

(Laughter).

MR. LEWIS: I will lend you my knife. You can whittle some.

(Laughter).

MR. PLESSET: Why don't you go ahead.

MR. DENTON: This is my first overlay that I wanted to present to you, and I think you have already seen it. It is just a general description of the vessel. Professor Siess has already given you a description, and I will pass this one.

(Slide).

The next overlay that we have here represents a summary of the evaluation that TVA did in connection with the hydrogen question, and the supportive data backing up these numbers were presented Tuesday to the ACRS

Subcommittee on structures.

Let me just briefly go through the numbers and comment on them as I go. And then I will turn it over to Mr. Orr of Offshore Power Systems. The things we looked at: of course, we looked at the anchorage. That represents first yield of the anchor bolts. The value there is 68.4 psig, the value of the pressure in the containment that it would take to fail.

The equipment hatch is a spherical structure that is for equipment, and it has a capacity of 73 psig. This value, the personnel locks, I have 42 psig there. And we had difficulty locating our stress proportion and our material data on that.

This represents the point at which the end bulkhead stiffeners would at first experience yield. Examining a little closer as to why that is such a low value, I found from our design folks that did the evaluation they assumed that the stiffeners were simply supported, and then they looked at what would be the simple moment right in the middle there. And whenever the first value went to -- when the fiber went to yield, they said, that is 42 psi and that is the strength of the thing.

I checked that out, and just a small amount of the end fixative would run this value considerably above that. The span is only 35 inches, and the plate is half-inch material. The stiffener behind it is four inches by a half-inch.

Let me assure you that this will not be the limiting load on the vessel.

MR. EBERSOLE: Don't you have some very large purge valves?

MR. DENTON: Purge valves, I am not --

MR. EBERSOLE: These are butterfly valves for purging operations. The reason I remember this sort of thing is there was some difficulty in guaranteeing closure under LOCA pressures.

MR. DENTON: To tell you the truth, I am not familiar with the penetration part of that.

MR. EBERSOLE: Since you mentioned vacuum relief, that is why I wanted to --

MR. DENTON: I am getting that out, yes, sir. There are some isolation valves inboard of the vacuum relief valve, and this value here does represent the ultimate strength of the vacuum relief valve.

Those butterfly valves, if that is what you are referring to as purge valves, have a capacity of 150 psi, and they are inboard of it. So that is the qualification.

MR. MOELLER: You said that the modifications for the personnel lock would increase the 42 to what? Is it ten percent or would it triple it or what? Could you ballpark the number for us?

MR. DENTON: To be honest with you, I think this is simply sharpening our pencils. This will show that the end part there will take substantially more than this. I was just saying that whatever we have to do to raise that value to above the value of the vessel, we will do, even if we have to put another small stiffener in that area.

MR. SIESS: What about the vacuum relief valve, that 48, that is below the maximum?

MR. DENTON: Since the butterfly valves are inboard of that and they have a capacity of 150 psi, I think the failure of this, since this is the sort of accident that we are dealing with, a Class 9 accident, I guess we are not limiting ourselves to a double failure-proof system at this point.

MR. EBERSOLE: When you say "inboard," does this mean that there is a fairly large part that goes inside the containment, into which you insert this valve?

MR. SIESS: How large is the pipe? I think Mr. Ebersole is thinking about these 36-inch purge lines.

MR. EBERSOLE: Mainly the pipe that you have them tied into and the buckling mode on that.

MR. DENTON: I don't have the drawings. I was going to say 20 inches, but --

MR. LAU: Those vacuum relief valves are 24 inches in diameter.

MR. SIESS: Do you know anything about the buckling capacity of that pipe under external pressure?

MR. LAU: No, sir.

MR. SIESS: If that pipe failed, you still have the butterfly valves.

MR. LAU: Outboard.

MR. DENTON: Without having run out the numbers, I think the buckling capacity of a 24-inch valve would be substantially larger than the numbers we are talking about.

MR. LAU: The physical arrangement of these valves are that both valves physically are located in the end of this area. The butterfly valves are inboard of the vacuum relief valve.

MR. EBERSOLE: The pipe projection is outside the shell.

MR. DENTON: That is right.

MR. PLESSET: Go ahead.

MR. DENTON: We looked at a number of penetrations. Electrical penetrations were tested to 100 psi. The bellows have a yield strength of 109 psi. We have some spare penetrations that we looked at. The bolted head penetrations, 1300, and the weld penetrations are three. There were other things we looked at those, and presented those as being representative of the scope of our review of the containment.

Now, the critical section of the vessel -- I am going to skip through this, because Mr. Orr is going to go into a little more detail, but the values you see here representing first yield, the differences in the numbers represent the factors that you apply or the considerations that you include, and as Professor Seiss has already mentioned this, Von Mises versus shear, actual test versus ACI Code minimum, 40 percent. The difference between considering the stringers and not considering the stringers represents approximately 35 percent, so you really step up here, and this value, 50.8, is the value that we feel is the reasonable number for which this vessel should be qualified.

This is the best estimate that Professor Siess mentioned.

MR. LEWIS: Will I come out of this understanding --

MR. SIESS: That is not my best estimate. That is my reasonable limit value.

MR. DENTON: I will change my terminology, whatever that means.

MR. LEWIS: Will I come out of this understanding why there is any difference between maximum sheer criterion and the Von Mises criterion?

MR. SIESS: Maximum shear strain.

MR. LEWIS: Is it because it is three-dimensional?

MR. SIESS: Maximum shear stress is just the difference between the maximum and minimum principal stress.

MR. LEWIS: That is because you can make a rotation.

MR. SIESS: The Von Mises is the square root of the difference of the squares.

MR. LEWIS: I understand that. They come out the same. We can talk about it later.

MR. DENTON: I think this will probably be cleared up in Mr. Orr's presentation, in which he is going to amplify on the source of this value here.

At this time, I would like to turn it over to Mr. Orr, if you have no further questions.

MR. ETHERINGTON: I am still not clear.

MR. SIESS: Harold, this is by axial stress.

MR. LEWIS: It is because it is by axial. It is because of that you have two, one, and zero.

MR. SIESS: This is for biaxial stress.

MR. LEWIS: In two-dimensional stress, it would be the same.

MR. SIESS: Yes.

MR. LEWIS: Okay, now I understand. Thank you.

MR. ETHERINGTON: I am still not clear how we can talk about 38 or 40 psi on the first yield basis for the plate and still have the same number essentially as the ultimate for the vacuum relief valves. We cannot take credit for that high yield of the plate. Can you clarify that for me?

MR. DENTON: Wang, could you address the situation in which the butterfly valve --

MR. SIESS: No, put your slide back up and I will explain it. In your second column you have indicated for the vacuum relief valve that that is based on ultimate, and Mr. Etherington is wondering how you can utilize ultimate for that analysis and utilizing yield for the other or the comparable values.

MR. DENTON: There is a small membrane inside that vacuum relief valve which is the weak link in the valve itself, and if I had a cross-section of that thing, you could see it, but it is a real thin membrane, and it is that thing which has -- that is the ultimate value.

MR. ETHERINGTON: I am not disagreeing with your number at all, but I am saying, if this is the number, then we can hardly go up to pressures as high as that with the same factor of safety as you have in the plate. You were talking 26.8, weren't you?

MR. DENTON: No, sir.

MR. ETHERINGTON: Weren't you? You are talking a 50.8 number now?

MR. DENTON: That is the number which we would like Mr. Orr to justify by his presentation.

MR. ETHERINGTON: If this can be justified on the basis that you are not going to exceed the limit -- you are going to exceed it -- you have a weak link in this vacuum relief valve. You are right up there with no factor of safety.

MR. DENTON: I think the answer to that question is, it is more of an operational question than a containment capacity question, and I think that is where I need some help. The butterfly valve, which is the isolation valve, is in board of that, and that thing is closed.

MR. SIESS: There are two valves in series.

MR. DENTON: Two valves in series, and the first one has a capacity of 150 psi. The second one, which is the vacuum relief, has a capacity of 47.8.

MR. SIESS: Why did you choose to use the second valve?

MR. DENTON: Well, the thought was that this would be something that everyone would -- a lot of people might have a question about, strength.

MR. SIESS: They did.

MR. DENTON: So that is the reason. I did not mean to be confusing by including it.

MR. ETHERINGTON: The redundancy then --

MR. DENTON: That is correct. That is correct.

MR. BENDER: Is the relief valve considered to be a backup to the other valve?

MR. DENTON: Wang, could you comment on the operation?

MR. LAU: As far as containment isolation valve is concerned, it is true. As far as the system operation of the vacuum relief valve is concerned, the butterfly valve is the backup of the vacuum relief valve.

MR. ETHERINGTON: We have to assume we have lost our backup.

MR. SIESS: You have two valves to meet the general design criterion on containment, right?

MR. LAU: Yes, that is correct, but the vacuum relief valve can be in the second boundary.

MR. BENDER: In the past, when we have considered these double valve situations, there has always been the question of whether we should always assume the valve facing the worst operating condition is always closed.

Many people have said that dual valves are put in because one of those valves may not operate when demanded to operate, and while I am not -- I am not trying to challenge the reliability of the valves, but I think it is well to understand that the primary boundary may be the vacuum relief valve.

Mr. Etherington's question was, I think, is legitimate to assume that that valve is acceptable, measuring it on the basis of ultimate strength, when you are measuring everything else on the basis of yield strength, and my question now is, when that valve reaches ultimate strength in the particular point that it sees that load, what happens to the valve?

MR. SIESS: The membrane fails, and it leaks. The membrane in the valve is the governing factor. I am sure this would not be acceptable on a design basis.

MR. LAU: The vacuum relief valve is a 24-inch spring-loaded check valve. If I recall correctly, the failure of this membrane would not cause gross failure of the valve.

MR. ETHERINGTON: It could cause leakage, though.

MR. LAU: That is correct.

MR. SIESS: How much leakage?

MR. MYERS: We have not studied that at all at this point. Let me point out the reason the vacuum relief valve was studied in some detail was because our nuclear safety review staff identified it as a potential weak link, because of the nature of the valve, and so the detailed studies were made on that early in the game, when we were talking about much lower pressures.

If that membrane fails, we do not get gross structural failure that would lead to immediate type releases, but there would be basically a breaching of the containment into the annulus area.

MR. SIESS: Would it be more than the annulus could handle?

MR. MYERS: The annulus is exhausted by the emergency gas treatment system, and as a minimum, the flow would be at the full capacity of the emergency gas treatment system, so that you would not have negative pressure maintained in that annulus. It might go positive, but the EGTS can handle quite sizeable flow rates.

MR. SIESS: I assume if you had an accident in the plant where there was some possibility that you might generate hydrogen and create unusual pressures in the containment, that you would start that emergency gas treatment system and pull your vacuum on the annulus and not wait.

MR. MYERS: The annulus it maintained constantly, and the fans start automatically to maintain a negative pressure, and it raises slightly when the s.ell expands due to temperature and pressure, but it is still kept negative through the bulk of the accident.

MR. SIESS: We did not try to address how much leakage would actually take to get out, since there is that annulus, but I think it is an interesting question if you start thinking about small leaks. It does not take a very big leak, I think, to overcome that annulus.

MR. ETHERINGTON: If the valve would reseat after a momentary high pressure pulse, not much damage would be done, but I don't know if we can assure ourselves if it would reseat. What is your opinion based on the construction of the valve?

MR. DENTON: I am sorry, I really cannot answer that question.

MR. SIESS: Can anyone?

MR. MYERS: No, sir.

MR. BENDER: What is the nature of this load? Is it presumed this is a sharp peak type load that will go up for an instant and then fall off quickly?

MR. SIESS: We did not. We assumed essentially a static load, or what Dr. Mark referred to as quasi-static.

MR. BENDER: For analysis purposes, I am sure you did that, but just from the standpoint of the kind of loading it might be, is it likely to be a sustained load as opposed to -- it does influence --

MR. KERR: I think it would show a curve against pressure when measured against time, Mike.

MR. LAU: This valve has no structural problem. This valve is open -- this check valve opens inward, so there is a pressure from the inside of containment. It has a tendency to force the things to close.

MR. DENTON: I think really to answer these questions we would have to have a cutaway of that thing to explain it, and to be honest with you, I really cannot explain the thing here. We did not consider it to be the cause of the butterfly valve inside. We have 150 on the inside, and -- and I think if this is a weak point we want to look at this thing more carefully.

MR. PLESSET: What does the membrane look like? What is its thickness? Can anybody tell us? Do you have any idea, Chet?

MR. SIESS: No.

MR. DENTON: I saw the picture about two months ago, and -- so I know could not describe it. As some

IMAGE EVALUATION
TEST TARGET (MT-3)

6″

MICROCOPY RESOLUTION TEST CHART

IMAGE EVALUATION
TEST TARGET (MT-3)

6″

MICROCOPY RESOLUTION TEST CHART

mechanical drawings are, they are a little bit confused. However, evaluating the thing, I looked at it and looked at the calculations that accompanied the evaluations.

MR. SHEWMON: I thought the TVA man said it was designed for significantly higher pressures than we are talking about. Don't we believe him, or didn't I hear him right?

MR. SIESS: Are you confusing the butterfly valve, which is good for 150?

MR. PLESSET: It is a different valve, I think, Paul.

MR. SIESS: One is a butterfly valve, and the other is the check valve, which is good for only 48.

MR. SHEWMON: All right.

MR. SIESS: Which is downstream from the butterfly valve.

MR. BENDER: Could I ask another question about --

MR. SIESS: Which we have now postulated to be open.

MR. BENDER: -- about the closures? I suspect that they are all sealed by some kind of rubber gasket of some sort around the perimeter. Is that the general principle on which they are sealed?

MR. SIESS: You said closures?

MR. BENDER: Hatches and the valves. I guess they are all closed by some kind of a rubber seal. Is that correct?

MR. DENTON: Could one of you all answer that?

MR. DILWORTH: Mr. Bender, as we have seen here, we do not have the valve expert, unfortunately, with us today.

MR. BENDER: Let me just ask my question, without meaning it to be more than a question. Have you looked at the definition under these very high loads to know that the valve -- that the seals themselves will take deformation and then hold when the pressure decays?

MR. MYERS: Early in our study, in about the spring of this year, we looked at resilient seating type affairs, and we looked at the O-ring type seals, because that had been raised. At that time we were looking at pressures in the 20 and lower 30-pound range. The preliminary results were that that would not be a problem area, and therefore we did not pursue it at that time any further.

As we go through in defining an exact pressure that one might use as a design basis -- I hate that term, but as a basis for this system, we would look at those in detail again.

MR. SIESS: Mr. Denton, would you put the first slide back up?

As far as the hatches are concerned, they are located in regions of considerably thicker plate, and under these pressures there would probably be no yielding whatsoever. You said deformation.

MR. BENDER: I am talking about deformation of the seals, Chet, and although sometimes people find that those seals get overcompressed, they don't respond after the loading, and you may have a seal over only part of the surface, and I think that needs to be looked at. I don't say it is necessarily a problem.

MR. ORR: I would like to present to you today some results of a finite element analysis that was performed on the TVA Sequoyah containment to determine its limit capability. Don has been through the critical areas, and the area of shell that is thinnest and is most critical is the one-half inch course immediately below the spring line, and what we looked at in our analysis was a single panel between the hoop stiffener at elevation 788 and the hoop stiffener at elevation 778.5.

We did not consider the change in plate thickness of half-inch to five-eighths. We just assumed half-inch throughout.

(Slide.)

MR. ORR: So, this represents a typical panel that we looked at. This is a half-inch plate, hoop stiffeners top and bottom, nine foot sis apart, vertical stringers nine and a half inches by one-half inch, four degrees apart. In order to do the analysis, we assumed that this was one of many panels in a cylinder, and hence we assumed symmetric boundary conditions at each of these rings, also at the center of the panel so that we were able in practice to analyze a quarter of a panel.

(Slide.)

MR. ORR: The quarter of the panel tends two degrees up at the top and is four foot nine inches high. It has half of a hoop stiffener up at the top, and it has half of an axial stiffener on one side. We developed a finite element model of this quarter panel.

(Slide.)

MR. ORR: And we analyzed it on the ANSIS computer program. Each of these elements are plastic triangular elements. The panel itself is divided into elements that are about six inches on a side, and there are four elements in one direction and about ten in the vertical direction. The stiffeners are also modeled using finite elements, the hoop stiffener, and the longitudinal stiffener.

The boundary conditions, we assumed symmetry on all boundaries, so on the vertical boundaries the tangential displacement is zero. This allows the whole panel to move out radially, and the rotation about the vertical axis is zero, and the rotation about a horizontal tangent is zero.

On the boundaries at the top and the bottom, it was assumed again as symmetric boundaries. The vertical displacements were all imposed to be equal, but a force was imposed in the vertical direction to account for the pressure in the vertical direction. Again, two rotations, zero, one being the rotation about the horizontal tangent, the other being the rotation about the vertical axis.

We did a check case first of all for an internal pressure on an elastic analysis, and then we extended it to a non-linear large displacement analysis considering elastic plastic behavior. In the plasticity in the yield criteria, the Von Mises yield criterion is used. We assumed no strain hardening, so in other words it is elastic, perfectly plastic, with a yield stress, uni-axial yield stress at 45,000 psi.

(Slide.)

MR. ORR: This next plot shows the results of radial displacement at each of the four quarters of the quarter panel as a function of pressure. What we see is linear results or relatively linear results extending up to 46 psi. In practice, there is some local yield that commences at about 30 to 32 psi, but the gross yield does not start occurring until 46 psi, and then we extended the analysis on up to 48 psi and to 50 psi, and got valid results there.

At 52, we could not get results, and we would not expect to get results above a certain pressure, because with an elastic, perfectly plastic analysis, the displacements would be infinite. What the results show is, Points A and B were shown on the original quarter panel.

(Slide.)

MR. ORR: Points A and B are two points on the hoop stiffener, one at the intersection with the longitudinal stringer, one at mid-span between longitudinal stringers, and these two points move radially outward to reach the same magnitude.

Point C and Point D -- C is the center of the panel. D is the mid-span of the longitudinal stringer.

What the results show is that early all four points are moving out radially about the same amount at 46 psi, and we are talking a displacement of just about one inch. The mid-span of the stringer, which is Point D, moves out a little bit more than the hoop stiffeners, and the mid-point of the panel moves out a little bit more than that.

The difference between C and D represents the local deflection of the plate relative to the adjacent stringers and the deflection of D relative to A and B is the mid-span deflection of the stringer relative to the hoop stiffeners.

The next vu-graph I will show you summarizes the stresses at certain locations of the shell at 46 psi.

(Slide.)

MR. ORR: At this stage, all of the stresses in the shell itself in the panel are at or close to yield. The ring has hoop stresses between 37 and 39 ksi. Yield would be at 45, so the ring is getting close to yield, but is not there yet. The axial stringer has much lower stresses, and in this case you can see that it is bending.

If you look at mid-span, the longitudinal stress on the inner edge is 47 ksi; on the outer edge 22 ksi. That is the end span at the ring stringer junction. The inner edge is 18 ksi tension, and the outer edge is 10 ksi compression.

The conclusion from this is that the mechanism of the limit load is that the ring goes to yield, but the stringer remains elastic, so basically the whole show is going to be moving out radially at the same time. At 50 psi, which was the top point that we went to in our analysis, we looked at some of the strains that were shown in the analysis. The maximum strain is about .3 percent. This is about equal to twice the yield strength.

So, we are still talking very low magnitudes of stress.

MR. SIESS: This was at what level?

MR. ORR: This was at 50 psi. The maximum strain, and that is due to both bending and membrane, is just less than three-tenths of 1 percent, and occurs at the location of the mid-span of the longitudinal stiffener.

MR. BENDER: How are their stiffeners attached to the shell? Are they full penetration weld?

MR. ORR: I believe they are double fillet welds.

MR. DENTON: That is correct.

MR. ORR: The final vu-graph I would like to show you is just an attempt to summarize some of the simplified analyses and hand-type calculations that people did, and then compare it with the results that we had on the finite elmement analysis.

The initial number is, as Dr. Siess quoted, for half-inch plate using nominal yield, 32 ksi. A pressure that gives that stress is 23.2 ksi. And this is about twice the pressure that the containment was designed for. That factor of two is consistent with ASME requirements.

Then, a factor F1 is the ratio of actual yield to nominal yield, in this case based on test reports. It was 45 ksi divided by 32 ksi. So, this is a 41 percent increase. Factor 2, if you use Von Mises, which is a continuous curve and typically matches better than the Tresca criteria, the ratio is 1.15, about the maximum, and perhaps 1.1 -- 1.11 is a better number to use. This is the number that Dr. Siess has been using.

Then, coming to one of the concerns that came up in the R&D Associates comments, they did not feel that the smearing of the stiffeners was valid. This shows the effect of smearing of the stiffeners, taking the hoop stiffener, which is 16 inches by one and a quarter, the one almost at the spring line, and smearing that over a length of shell, nine foot six by half an inch, it represents a factor of 1.35. So, the hoop stiffeners contribute 35 percent.

I will skip this line first. The finite element model that I did the analysis on was indeed consistent with this stiffening configuration of two hoop stiffeners, one above -- half at the top and half at the bottom of the nine, six panel.

If I take the product of all of these factors, 23.2, 1.41 for the 45 ksi yield, 1.15 for the Von Mises, 1.35 for the shear values, I come up with 50.8 psi. The analysis I showed you, we have results up to 50. We could not get results to 52.

Going back now to this line, the actual configuration of the containment is not perfectly symmetric, and in fact just about the half-inch panel, there is another hoop stiffener three feet six away.

(Slide.)

MR. ORR: So, if instead of taking a hoop stiffener and smearing it over nine foot six, I take this hoop stiffener and smear it from midway between these hoop stiffeners to midway between these two, then that becomes a six foot six height, and then I can come up with another equivalent smearing. Let me find the right vu-graph.

(Slide.)

MR. ORR: That gives me an effect of the hoop stiffeners of 1.51 instead of 1.35, and I believe it is fairly representative of the way the shell will actually behave. So, with the panel at the spring line, we say the capability is 23.2 psi times 1.41 for actual yield times 1.15 for Von Mises times 1.51 for the smearing, which comes up with 56.8 psi.

That concludes my presentation. Any questions?

MR. SIESS: This was the last analysis made, or the most recent, and it essentially verifies the first analysis made. Maybe we should stop here.

MR. PLESSET: Is that progress, Chet?

MR. SIESS: Yes, that is progress. Nobody believed the first one.

MR. PLESSET: No questions.

MR. SIESS: I would like to call on Dr. Zudans.

MR. MOELLER: The original design was at what, 20, 15?

MR. SIESS: The original design? Twelve psi.

MR. MOELLER: Twelve. Okay, thank you.

MR. SIESS: Zenon, I would like for you to present not only the results of your analysis, but also to relate them, if you can, to Dick's and to any of the others that you know of, if you would.

MR. ZUDANS: I will do that.

We had quite a discussion on Tuesday, and I am not going to repeat everything I said. It might not be so easy. But first, to the best I can show so far -- in the subcommittee -- Dick's analysis -- Richard -- he does not like to be called Dick -- is the right way to go, and the only concern I might have with it is that it takes a very small section near the spring line where there are other discontinuities, but I think it is a nice piece of work, and I think it shows how the shell will behave, and it also confirms what I found. Even the numbers are very similar.

I did not do inelastic analysis, but since this material has very little strength hardening, inelastic analysis essentially does not have to be carried very far to come up with a final limit load. Now, the other analysis, as was stated before by Chet, that by Ames, used a method of smearing out rings and stiffeners, and it fails to be rigorous, even in a simple sense, except for the calculation of simple membrane, which is PR/T, known to everybody, and that is what the 23.2 psi is in this calculation.

Now, one must give the credit in doing a job as they did, because I am told afterwards that they only had been given, say, four days to give an answer. If you give a four-day answer, that is what you get, a four-day answer. You have to give a guy a chance to read his own work, at least, before he puts his name to it.

So, I was more critical on Tuesday -- I am not as critical today, because I don't think it concerns anybody any more, because we now have a set of answers that we all can believe in the sense that if anybody did the most precise analysis, they would not find any different answers.

The other analysis, the R&D Associates, they are kind of artistic. It is beautiful. However, unfortunately, not to the problem that we are dealing with right now. So, they are only as good as the initial assumptions are. So, to my amazement, though, Mr. Bagchi's calculation, which, unfortunately, I did not read before I wrote my report, came out with a correct answer on a back of the envelope type calculation.

The reason for that was, he simply gave it a little bit more thought of how the stringers interact, and drew up equations for it, and got the answer that is very close to the real answer, after all those sophisticated calculations.

I was not going to do any analysis. I only was asked to review two reports, but after I reviewed those two reports, it kind of angered me, because I could not write and give Chet any answers as to what to expect, so I performed some analyses myself.

First, I analyzed two different locations. One is from -- halfway from this point over two rings, halfway to this point. This was the area where the Ames report claimed the weakest spot was, and the reason they arrived at that conclusion was because the way they smeared out the stiffeners in the rings, it looked like the weakest section.

Then, afterwards, finding that this was not really the critical section, I went back to this area where the half-inch plate joins the five-eighths inch plate, and of necessity this is a critical point regardless of what the internal pressure is, because there is a significant discontinuity.

First of all, the inside surfaces match exactly. The reference lines or the center lines of these two sections mismatch, and there is a bending introduced into that section. I would like to see locations like that always reinforced, but this is not the case here, so that was a logical candidate for the weakest spot.

Here I took a model from this location here, went over one of these rings, over this five-eighths inch plate, over the half-inch plate, over this ring, this piece of plate, this ring, and halfway about the spring line, so I had three rings at several spans.

The reason for going that far with the model was because I had the same doubts Richard stated, that these are discontinued in here. It is very difficult to justify symmetry boundary conditions at those points, so if I move from what I call the critical region far away enough and impose the symmetry there by the simple rule of decay of the boundary perturbations towards this section of interest, I could reason that this would be not shadowed over with any false results.

Now, the first analysis that I did then addressed the other section, and that is what the revolution model looked like. The problem used here -- from here to there is able to represent the rings as radial plates in linear elasticity. It is exact. It represents the cylinders and the stringers. These are the stringers that are welded to the shell and the rings. They are represented in a smeared out fashion, but in such a way that their bending stiffness and meridinal stiffness are exactly represented.

So, what it really does is, it should overstress the stringer a little bit more and understress the shell a little bit more, because they are forced to deflect together.

Normally, as you saw already from Richard's model, the center portion of the span will deflect slightly more than the stringer will.

Now, to prove some points, in the other analysis, I did one without the stringers, and this is the distribution without the stringers. You see the hoop stress at the rings reduces essentially to zero, and between the rings it reaches the full hoop stress as if the rings were not there. It indicates that without the rings, the analysis resulted in 23.2 psi. It would have been exactly correct.

As you introduce the stiffeners, the picture changes completely.

(Slide.)

MR. ZUDANS: This is the figure that shows how the hoop stress varied in the lower portion of five-eighths inch plate, and you can distinguish the location of rings, but not particularly being too different. In other words, the entire shell tends to be within 10 percent or so one from the other.

Now, to make sure that this shell type analysis is justifiable, I also made up a finite element model.

(Slide.)

MR. ZUDANS: It is in essence similar to what Bob did. However, Iput the ring in the middle of the element because I felt that the easiest way to justify symmetry boundary conditions is halfway between the rings and halfway through the rings here. So, this analysis was on exactly the same model as the previous one, and I used the results of these two analyses to gauge the quality of the shell revolution type of analysis.

There was a statement, I guess, to show how much in error the statement that the stiffeners carried the same load the entire lengths -- I plotted here the load contribution by the stiffeners. You see, it essentially reduces the load-carrying capability to zero, and increases this value, so the assumption of some uniform load along the stiffener is only good if you can talk about an infinite cylinder that is completely free to expand.

Then the solution would be exact.

Okay, now the analysis that represents the weakest spot is done on this model, which is the three rings and the span in between.

(Slide.)

MR. ZUDANS: If you recall, Richard analyzed the space between these two rings, a quarter of it. This carries the analysis beyond to include these two rings that are quite close together and certainly provide significant stiffeners. Here on this model the symmetry boundary conditions are here. This is fixed axially, and, of course, can grow freely in the radial direction. The thickness change takes place here, from five-eighths to half an inch.

(Slide.)

MR. ZUDANS: The results of this analysis are shown on this slide. This is the physical bound between the two stiffeners. You can see here where the half-inch plate joins the five-eighths inch. There is a significant increase in hoop stress and also a significant jump in the axial stress.

Now, this is the extent of the analysis I have done. If I use these results shown in this area, then I come to the conclusion that based on the shell analysis, I would -- and on Von Mises criteria, I could yield this cross-section -- Let me show you.

(Slide.)

MR. ZUDANS: It is this cross-section here, at 51 and a half psi, gauged by the finite element comparison to the shell revolution in the other area. I had to reduce this to about 48.9 psi, so it will fully yield here.

But the most interesting thing is that the entire panel is so close to that, if you compute the Von Mises effect, it would be about 17,000 -- 15,000, which really means within about four psi or so the entire panel will yield, and the rings, if this panel is able to continue carrying load without ballooning out, the rings would start going at 61 psi, this ring first, and those would be much higher.

This is about 77, so the structure is very, very well balanced in a way. If it starts yielding at 60 psi, you could probably find several panels going, so to speak.

That is about all I have to say. Yes, Dave?

MR. OKRENT: There is some probability that there are welds in this structure which are imperfect in the sense that they were able to withstand the pressure test that was done, which was a much lower pressure.

MR. ZUDANS: Thirteen and a half.

MR. OKRENT: But might nevertheless have substantial flaws, and flaws of the type that would, let's say, give at some higher pressure. How would you factor this kind of thing into your thinking about what constitutes a pressure with some degree of confidence that you know one should count on, or whatever are the right set of words?

MR. ZUDANS: By the code requirements, all the shell welds, both longitudinal and vertical, have to meet the code, so it is not likely you would find any difference beyond the code size.

MR. OKRENT: They are not 100 percent --

MR. SIESS: This is -- Is this 100 percent weld inspected?

MR. DENTON: The welds are 100 percent radiographed for all pressure boundary welds.

MR. ETHERINGTON: That does not include the welds for the stiffeners?

MR. DENTON: There is a magnetic particle examination, I believe, on those.

MR. OKRENT: Let me again understand what you are telling me. I thought there was some thickness below which only --

MR. SIESS: You don't need stress relievers. You are thinking about the concrete containment liners, and they are only partially inspected, because they are not a pressure boundary. They are only a leak boundary.

MR. OKRENT: But only a 100 percent weld inspection.

MR. SIESS: That is what he says, and that sounds right. This is a pressure boundary.

MR. OKRENT: That would, I think, reduce -- not eliminate, but it certainly would reduce the probability of a large flaw.

MR. SIESS: It is very ductile material.

MR. BENDER: Generally speaking, the welds provide enough stiffness so you can be comfortable that there is no concern there about those giving way under the loading. The fillet welds.

MR. SIESS: The fillet welds are not that important, since the plate is on the inside of the rings, the only purpose those fillet welds have is for a little composite stiffening between the ring girder and the plate, you know, shear transfer, and that is not going to be very much of a load or stress.

MR. ZUDANS: Your statement is absolutely correct. The shield transfer capacity here is rather minimal. The bending is not significant. So you could say, all of this weld, that and that, are not really significant, but when you come to this weld, it is very significant. These would be very significant, and of course this is extremely significant because it happens to be sitting right where the critical section is, but these continuous fillet welds, the stringer attachment and the ring attachment are not significant. There is very little bending there.

MR. SIESS: We did not cover that at the meeting, but I checked on it in between.

MR. ZUDANS: I figured you would.

(General laughter.)

MR. ZUDANS: So what it really means is, the kind of strains you would have about a one-inch deformation. There is not much to go beyond that point.

MR. SIESS: I think at this point to get the analyses completed we ought to hear from the staff. They have heard all of this before, and they have a position which I have already indicated earlier, but I see no reason why we should not hear it.

MR. STAHLE: Dr. Tan will give the presentation.

MR. OKRENT: If I could pursue my question just a little bit to Chet and Zenon, how big a flaw would there have to be to bother you at the kinds of pressures you are talking about?

MR. ZUDANS: Me?

MR. OKRENT: If it is a half-inch thickness flaw, it would not disturb you.

MR. ZUDANS: No, not if it is only that long.

MR. SIESS: Paul might have some idea.

MR. SHEWMON: I have no complaints with that answer.

MR. SIESS: The weld material is usually stronger than the plate material.

MR. OKRENT: I just wanted to see if I should worry. At that size, I think it is unlikely.

MR. SHEWMON: It is not unlike a notch in a piece of copper.

MR. TAN: I want summarize the staff's review and evaluation of the various analyses and to present to you our conclusion.

Our evaluation consists of the original Ames Laboratory analysis, R&D Associates, and Bagchi's analysis, and OPS, and Dr. Zudans', because after last Tuesday's subcommittee meeting, we feel we have to take all analyses into consideration and to finalize our position.

(Slide.)

MR. TAN: These are the analyses done by TVA. It shows the various -- various assumptions with respect to the ring stiffeners and the stringers. This is the value reached by TVA, 38, and OPS comes to 50 and 57, just as we sent it by Richard Orr. This is the Ames Laboratory, after the revised analysis. This is R&D Associates' results of analysis. This is Mr. Bagchi's, and that is ours.

These are the finite element analyses from Ames Lab. It is as high as 60 psi, and Dr. Zudans is 47. One is at yield, one is at limit. Now, from this we have to -- Dr. Zudans said the Ames Laboratory analysis may not be as good as OPS, but after the revision -- they revise their value, it is comparable to the OPS.

Basically, OPS I think also used the smear technique. There is not much difference.

Now, as to the value by Bagchi and Zudans, I have more confidence in Dr. Zudans' because he is my former boss.

(General laughter.)

MR. TAN: And I know he is competent. But Dr. Bagchi's number, you know, if you look very carefully, it can be manipulated. There are many assumptions there. He hit the jackpot. That is all.

(General laughter.)

MR. TAN: If you look very carefully, you know, his analysis, you know, so this is a number -- so after looking over all the values at the subcommittee meeting, we recommended 33 psi, but after looking over all of these results and hearing the experts' opinion, we finally -- we said, okay, we agree with using the Von Mises criterion, because the original 33 is on the Tresca criterion.

MR. MOELLER: Could you refresh me on the factor 45 over 32?

MR. TAN: This is the actual mill test result. The 32 is the code specified value. Besides TVA, we also used 32.

MR. BENDER: That is ultimate strength?

MR. TAN: Yield strength. Yield strength. So our conclusion is -- on the basis of results of TVA, staff's consultant, and others, and taking into consideration all the factors, the staff calculated that the value of 38 psi at yield is reasonable and therefore acceptable as the Sequoyah containment limiting static internal pressure.

The basic problem we have to remember for containment, it is necessary but not sufficient because of the leakage problem. We feel 38 psi can be used.

MR. SIESS: I think you heard Mr. Orr say that this maximum strain was about three-tenths percent, which is just about twice the yield. Are you concerned with an additional -- that much strain?

MR. TAN: The problem -- we tested the containment to about 15 psi, so from 15 psi to 38 already we don't know very much how much the leakage would be, but I think we have some confidence, you know, before reaching the yield, that the leakage should not be too much, but after the yield is reached, you know, nobody can say how much the leakage would be.

MR. SIESS: So the staff's position is that as long as you stay elastic -- I have to put elastic in quotes, because there is local bending, but essentially membrane elastic, as long as you stay elastic, you feel confident about it.

MR. TAN: Yes. Otherwise, if we don't have the leakage problem, we can go to 50 or 60 as our analysis shows. The leakage -- I don't know -- under the seals in the penetrations how they deform, because, you see, if the containment -- the containment penetrations, you can build a very leak-tight structure, but with all the seals and all the penetrations, I am not confident --

MR. SIESS: You are saying all these penetrations. Most of the penetrations are down in the thicker plate, and at 50 psi versus 38, I doubt if even the five-eighths inch plate or the next panel down would be yielded.

TVA has got -- I checked with them, and they have found one string of penetrations up in the region we are concerned with. There are four in a row backed by an inch and a half plate, I believe. It is a string, right? So, most of the penetrations are not going to be at yield, even at 60 or 70 psi.

MR. TAN: Okay. All right. By the personnel lock -- the seals in the personnel lock and the equipment hatch, you would have those. I am not concerned about those seals.

MR. SIESS: You are not concerned about overall deformation?

MR. TAN: I don't believe there is a problem.

MR. SIESS: So you are concerned about the fact that higher stresses might limit -- take some of the penetrations into yield, and the penetration hardware would be at yield?

MR. TAN: That is our concern. Otherwise, we can relax.

MR. SIESS: I just wanted to --

MR. TAN: The 38 is the lower bound. If you want to go higher then it is a matter of judgment how much higher we can go until the seal will not have a leakage problem.

MR. PLESSET: Chet, you indicated perhaps RDA might want to make a comment.

MR. SIESS: Any further questions for Mr. Tan?

(No response.)

MR. SIESS: Okay, thank you very much.

R&DA was involved in this by reviewing the initial Ames Laboratory Studies, and pointing out some disagreements and deficiencies, and they raised the question of the panel capacity, which I feel quite sure led to some of these other more detailed analyses.

I think it is appropriate to ask Mr. Parry from R&DA if he would like to summarize their current feelings about what you have heard and if you have any general feelings about what you think would be an acceptable pressure, we sure would like to hear those.

MR. PARRY: Yes, and I have three vu-graphs.

MR. SIESS: Okay, fine.

(Slide.)

MR. PARRY: The presentation that I gave on Tuesday was dated August, 1980. You will notice this one is dated September, 1980.

(General laughter.)

(Slide.)

MR. PARRY: Now, a few general comments. All the prior analyses that we had seen until Tuesday we considered to be limited in scope mainly because of time and funding. We had about four days to do this analysis. Very clearly, all those results were very dependent on the initial assumptions. What were the ring stringer effects? And we very quickly concluded that the precise stiffener plate interactions are a very complex thing, and we sa'd the only true solution requires a detailed finite element code analysis.

Fortunately, TVA picked that up and did a very good analysis, and we essentially agree with what TVA and Offhsore Power Services say. We tried to establish some bounds, knowing the problem, and also, I might say, having been involved in some things that have gone wrong in simple analysis before we qualified our analysis by that statement requiring a more detailed finite element analysis.

I know some of these simple analyses go wrong, and people have been bitten by simple analyses. You are all aware of that. So we looked at it. The encastre plate, suppose the plate section were held rigidly on all sides, and we looked at it as a membrane aircraft fusilage type analysis.

We realize our analysis is conservative. We don't have a jaundiced view of the world, and we are looking at something which should be conservative. Our analysis was conservative, and as I say, the offshore power systems analysis was done with realistic results. I suppose I have a slight disagreement with the interpretation of the OPS analysis. I will talk about it in a minute.

Now, the other thing that happened was -- and we assumed this. Now everybody was in the same boat. We originally assumed 32 ksi as the strength tests have shown. It is actually 45 psi. And since we are not setting criteria, we are finding out what pressure this thing should go. We should use the test results. If we are setting criteria, then we have to put some safety factors in, and so forth. That is a different matter.

(Slide.)

MR. PARRY: Now, very briefly, we believe that the qualitative summary of what happens is something like this. Panel quilting takes place, and this again is well known in aircraft fusilage type analysis, because the panel is, because rings and stringers, it tends to bow, and Richard's analysis clearly shows that.

The first big stress point is at the midpoints of the long sides, and as he said, if you take that analysis up and gradually increase the pressure, then fiber yield will be reached at about 31 psi at these points, and eventually as you raise this thing up, then we finally get to our simple analysis of 38 psi.

The detailed analysis of TVA shows 46 psi, which we accept. Now, that is where the thing starts going non-linear rapidly and becomes plastic. If you did the smearing analysis, as Richard did, you get 50 psi. So, what this really says is, in the linear portion, and all the stuff that was done before was always linear, nobody until this recent analysis did any non-linear analysis. It says indeed this double stiffening, rings and stiffeners goes two-thirds of the way towards the smear, 50 psi.

Well, we accept that because it has been done and now done properly.

(Slide.)

MR. PARRY: So, to summarize -- you people have been through this before. We said simple analysis was 27, but the 45 ksi, so the 32 ksi raised that to 38. The OPS finite element says 46. There is still something to be gained by going somewhat plastic, so the 46 in our opinion now is a conservative estimate. We still, however, will be concerned about the midpoints of the sides. After all, they are close to welds. If the welds are well inspected, there should be no problem, but after all, on the midpoints of those sides, there are 180 points around that circumference, so they should be well inspected there.

There is another saving grace here which has also been alluded to. This 46 psi is based on a complete half-inch panel, and it is not a half-inch panel. Two-thirds of it is half-inch, and one-third is five-eighths, so there is some help from there.

So, having requested this analysis, we now accept that result. It is as simple as that.

MR. SIESS: Thank you very much.

MR. PARRY: I think the staff are now being a little conservative like we were originally.

MR. MOELLER: Would you comment again for me on the two values that are much lower than the 38, the boiler code, and the Sandia analysis? How do you evaluate those, or how do you cross them off, so to speak?

MR. PARRY: The boiler code analysis uses maximum shear energy. Everybody else is now using the Von Mises, which gives a 15 percent difference, which is more realistic in a practical case. The boiler code is used because it is very simple to use, and it is conservative, and after all, the code has to be conservative.

MR. MOELLER: And the Sandia?

MR. PARRY: I don't know anything about the Sandia analysis. I have only seen a result.

MR. SIESS: It was not Sandia. It was Battelle.

MR. PARRY: Our report said Sandia.

MR. SIESS: It is not based on actual strength. I don't think it should be in that column.

MR. PARRY: Okay.

MR. SIESS: It was based on 32,000 yield, so it belongs over in the other.

MR. PARRY: It is.

MR. SIESS: I see. Yes.

MR. PARRY: I have two columns there.

MR. SIESS: We know too little about it to know how they got it.

MR. MOELLER: But you have reasons not to view it with alarm?

MR. SIESS: We have reasons to believe the ones we have seen which agree remarkably --

MR. MOELLER: Yes.

MR. SIESS: -- I think there have been about four approaches that come within, I would say, plus or minus 10 percent of the same value. That is almost unbelievable.

MR. PARRY: I think there is pretty general agreement now that the analysis has been done correctly.

MR. SIESS: Actually, as I read the Battelle report -- No, I will take that back. I don't know what the figure is.

MR. PARRY: The thing I saw, they had two values, 24 psi, and I am talking about this 32 ksi yield and 30 psi, and they said, well, it is 27 psi plus or minus three.

MR. SIESS: Twenty-four yield and ultimate of 30.

MR. PARRY: Yes, and they add them together, divide it by two, and said plus or minus three.

MR. SIESS: Yes. I don't trust that.

MR. PLESSET: Does that cover it, Chet?

MR. PARRY: Mr. Chairman, I would like to say something else. I see that RDA is down in the hydrogen discussion. I believe there is no one here from RDA to discuss that. I am only peripherally involved in that.

MR. PLESSET: All right. All right. I guess that completes this part of our discussion. I think we are going to have a discussion from TVA. I would like to suggest a ten-minute recess. In fact, let's take it.

(Whereupon, a brief recess was taken.)

MR. PLESSET: Let's reconvene and turn the meeting over to representatives from TVA. As I mentioned to you before, they have a model which, among other things, shows the location of t[illegible] igniters, and it would be worthwhile if the committee members would take a look at it. Maybe they can do it after their presentation is complete, but it will be of some interest for you to do that, so why don't you go ahead with the presentation? We will look at the model later, after you are all finished.

MR. MILLS: Yes, sir.

We will have Mr. Wang Lau from our engineering design organization to lead off this presentation, and he will be followed by Mr. Dave Gasser from Westinghouse, who will talk some about the vent wall testing that is going on.

MR. LAU: I am Wang Lau, TVA.

I plan to spend about five minutes to give brief statements on nine items. The statements are supported by the handouts and the documents that we have submitted to the

NRC.

Item Number One: Based on our original Sequoyah design and the post-TMI modifications we have made on hardware and operating procedures, we believe that the probability of an accident at Sequoyah which will result in a degraded core is no higher than other plants. We believe that no additional hydrogen mitigation system is required for full power operation.

Our analysis indicates that with a reasonable design limit of 57 psi gauge for thecontainment, Sequoyah can take about 700 pounds of hydrogen in an adiabatic burn. This means that Sequoyah can accommodate an event similar to TMI even without the benefit of an ice condenser. The results of calculations including the ice condenser provide significantly more capability.

Item Two: We believe that an interim distributed ignition system has good potential for obtaining additional protection. We have not identified any negative effect due to controlled ignition as opposed to uncontrolled ignition. On the other hand, we have seen from my analysis that controlled ignition has a positive effect in mitigating an assumed sequence of accident events.

Three, based on our limited research and limited testing at Singleton Lab, we believe that the interim distributed ignition system we have designed and installed show promise in burning up hydrogen at relatively low concentrations. We intend to obtain firsthand knowledge. While testing at Fenwall Lab through Westinghouse, we will perform ignition tests at various hydrogen concentrations, steam flow, and water droplet environments. This will be discussed by Westinghouse later.

We have 31 thermal glow igniters in Sequoyah. We use diesel engine glow plugs. These can be seen from the models we have brought here. The containment model has a scale of one-eighth of an inch to one foot. The igniters are spaced with the intent of burning bulk hydrogen. We have decided to delay the use of spot igniters until we are sure of the effects of electromagnetic interference. We are working on it with the help of consultants.

Item Five: We have studied in considerable detail all the possibilities such as nitrogen inerting and filtered vented containment. We believe that distributed ignition systems is the preferred interim measure for obtaining additional protection, although there was no absolute need for this or other systems, as I stated earlier.

Item Six: We have joined with American Electric Power to contract with Atlantic Research Corporation to do work on halon. Work has started, and we expect the report in about four months.

Seven, we have also started our investigation on catalytic converters.

Eight, in other areas, TVA's degraded core task force intends to, A, study system transient and accidents to arrive at a fair range of probability for hydrogen generation and identify critical sequences. Operating procedures and maintenance procedures will be examined to determine their contribution to risk. Work is in progress.

B, through computer and system analysis, obtain a regional hydrogen generation rate. Work has started.

C, obtain the computer code for containment response analysis. This will be similar to the present CLASIX code by Westinghouse, but will include internal heat sink, initial water droplets, et cetera, to remove certain conservatism and obtain more realistic results.

C, through a series of tests, determine if the assumptions and parameters used in the present analysis are consistent with the test data. This work is in progress at Fenwall Lab.

In summary, A, we believe that Sequoyah is safe to operate at full power. B, we believe that we have reasonable assurance that the interim distributor ignition system will be effective and offer the best potential for obtaining additional protection. C, we are moving on many fronts to study degraded core related subjects.

I would be glad to answer your questions.

MR. PLESSET: Let me first ask what kind of a schedule do you have on the glow plug study? I mean, there is a program, your own program, right, and the staff has a program at Lawrence Livermore Lab, but you have your own program.

MR. LAU: Yes, we have two different programs. One is at Singleton Lab. Basically this will be a quality assurance type sampling test. We have procured about 600 different types of igniters. We will just go through the sampling and arrive at some kind of confidence limit type thing.

Another type of test we are doing would be covered by Westinghouse later, about the Fenwall tests.

MR. BENDER: I believe you mentioned 31 igniters. How did you decide on the number and distribution?

MR. LAU: Okay. The igniters are intended to burn up bulk hydrogen. Mr. Myers wants to add additional comments.

MR. MYERS: That, of course, was asked of us earlier. In the interim system, the objective is to burn bulk quantities of hydrogen, that is, over long spaces basically, all the upper compartment or lower compartment, and maybe propagation in between. Therefore, as far as number of igniters, what we were after was to ignite or provide reasonable assurance of igniting a concentration if the bulk average in those compartments got high, rather than in some local area.

As far as the number, based upon all the studies we have done in the past on subcompartment analysis for LOCA studies and what have you, we have a pretty good understanding of the distribution in the areas. We thought at that time that on the order of 20 to 30 would probably be required.

When we got down to the detailed location, which included sticking them near the top, if there were compartments that were relatively wide in comparison to their heights, then we put several in those compartments, more than one. When we got done, we had about 31 igniters located.

The critical dimensions, really, of the upper compartment burn to the lower compartment burn is a rough radius or diameter of 75 to 100 feet, and we tried to make sure that the igniters were not located that far apart.

MR. BENDER: Are there any compartments that do not have igniters?

MR. MYERS: Inside the reactor vessel cavity I do not believe currently has an igniter. There is a chase for the in core instrument tubes that do not have -- does not have an igniter.

MR. LAU: But they will be under water.

MR. MYERS: The dog houses of the steam generators and pre ..urizers do not have igniters up in them, but they do have igniters near the entrance.

MR. BENDER: Where would the igniter be that is nearest to the vent valve - is that the word I want to use?

MR. MYERS: You are speaking of the reactor system -- vent system to be added.

MR. BENDER: Yes.

MR. MYERS: The exact location of that vent release has not been set. We are currently reconsidering and looking at that, but there is an igniter in e area of the pressurizer compartment near the ceiling that would be in the plume from those kinds of releases, and also from the pressurizer relief tank.

MR. BENDER: How about the vessel downstream. If that igniter did not ignite, and the hydrogen moved to the next point --

MR. MYERS: The probable flow path in that lower compartment is up to the ceiling, because it is hot air, and then there are igniters along the ceiling, so the igniters are around the circumference of the ceiling, if you will, and with the ice condenser, of course, it is a reasonably short distance.

MR. OKRENT: Were the comments you just made all addressed specifically to hydrogen control, or were they intended to cover other kinds of questions that arise when you think about degraded core or core melt accidents?

MR. LAU: Well, we interpret the phrase in the agenda, "additional hydrogen control measures," to be interpreted as over and beyond interim distributed ignition systems. That would include inerting, vented containment, et cetera, halon, catalytic converters.

MR. OKRENT: I see. So you are looking, if I understood what you just said correctly, at other kinds of things in addition to hydrogen control with regard to degraded core or core melt accidents.

MR. LAU: Yes, sir.

MR. OKRENT: What is the staff's position for ice condenser type containments? Is it coming, you know, with regard to the question of, is there something that they think they should look at or licensees should look at for ice condensers?

I probably should remember this.

MR. BUTLER: Let me try to just clarify. I think there was a little mixup with respect to the understanding on the scope. The understanding with respect to today's agenda in our area here is limited to those measures needed to deal with hydrogen generation. When you go beyond that -- I have the feeling that your question was directed at other measures beyond hydrogen control. Do I understand that correctly?

MR. OKRENT: Yes. Well, I thought the man from TVA just said that he had some things like that in mind in what he was just discussing.

MR. BUTLER: But he clarified that by saying that the other things he had in mind were things beyond the igniters, such as halon control, halon systems, and fogging systems, not necessarily vented containments or core catchers or things like that.

MR. OKRENT: I am not going to try to put words into his mouth, but I want to know -- let me tell you why I think it is relevant to the discussions.

Commissioner Gilinsky has posed some specific questions in terms of hydrogen control, but it seems to me if one is going to try to develop an answer to a question, are measures to deal with hydrogen appropriate on some time scale, or prior to something, or whatever is the way it is phrased, and if we are talking about a lot of hydrogen -- I don't mean like in the rulemaking hearing on ECCS -- one might well think about this in a broader context.

In other words, that becomes part of a spectrum of accidents, and you ask yourself, is there a reason to stop with hydrogen? That may be. Or should one look at other things? Possibly. If so, why, and on what time scale, and so forth?

Now, I am asking now what the staff position is with regard to ice condenser containments, because that is the one we are talking about now. With regard to not only hydrogen control but other things, regarding core melt accidents.

MR. ROSS: First, with respect to the hydrogen management, it is the staff's position that the ice condenser owners, like all other PWR owners, should provide some hydrogen control measure analysis information on a time scale somewhat less than a year. We have expressed our position in the interim rule, which the Commission just a few minutes ago authorized us to issue comment.

When the rule becomes effective, it would require in this case TVA within six months to file some analysis information on a number of things related to hydrogen control, including scenarios that lead to burning, effectiveness of halon, water fogging, and so on.

What we would do with that information is not defined. Presumably, we could become alarmed and decide that something better needs to be done, or that we have become satisfied, and decide everything is real fine. I don't know, but as far as hydrogen control, that is our position, and we have not gone -- we have not done anything that may be idle speculation beyond the seat of the information.

Now, with respect to other things -- I am now going beyond the design basis -- there are a number of activities. We talked yesterday about the advance notice for rulemaking, which goes into these more elegant molten core response features, such as filter vented containment core retention devices.

I don't know what the Commission is going to do. They were going to have an affirmation session maybe today, and authorize issuance of that Federal Register notice, which would trigger a rather long rulemaking process, years long. It turned out not to be true, but if an ice condenser plant happened to be in what we call a high population zone, then it might have additional studies as to further design features to reduce risk like I think Mr. Denton talked to the subcommittee yesterday.

The emergency planning rulemaking -- and there is another rulemaking being considered by uniform or minimum engineered safety features standards. All could impact the ice condenser likely with everything else. I think that is pretty much the ongoing activities going beyond the design basis.

MR. OKRENT: Let's see. Is there somewhere where you think I could read why it is -- namely the logic -- why it is that the staff is now recommending that all BWR's be inerted, or I can read what they specifically think should be done as far as study on ice condensers, and why the things that they are not recommending now, they are not recommending be studied or done?

MR. TEDESCO: We have talked about the proposed interim amendments that relate to hydrogen control.

MR. OKRENT: Have they changed from those that you had in previous documents on hydrogen control?

MR. ROSS: No, no, they are unchanged. There is a SECY-80-107 series, and then the logic is the same. They should be available to the committee, but if they are not, I can see that they will be sent.

MR. OKRENT: I have seen the earlier ones.

MR. ROSS: There is no change.

MR. LAWROSKI: When you answered Mr. Bender's question about the basis of your locations of the igniters, what you said did not include what was referred to last week. That is, you have some constraints on you in the Sequoyah design as to where you can put the igniters that we heard about. You did not say anything about that now.

MR. MYERS: I think what you were told in the subcommittee meeting is that the igniters are located where there used to be emergency lighting fixtures. That is true, but we are not constrained to that. If we find the need to have it somewhere else, then we will move it to that, but basically the kind of coverage criteria our igniters needed were roughly the same as those lights needed, and so they came out very nicely in roughly the same kind of places, considering our criteria is not such that we need an igniter right here instead of six inches away.

We did not use all those lighting fixtures. We only used part of them.

MR. LAWROSKI: Unfortunately, what you just said --

MR. MYERS: We are not constrained.

MR. LAWROSKI: It is a concern.

MR. MYERS: We are willing to move it somewhere else if we find a basis for that.

MR. MARK: Something Mr. Ross said. You use the phrase "design basis" as if hydrogen control in TMI amounts was sometimes part of the design basis, is that correct?

MR. ROSS: No, I was using that design basis on the terms of what is required by today's regulations. Obviously, TMI-2 exceeded the design basis amount of hydrogen. I don't know if it exceeded the core coolability or not. I guess not by definition since it was cooled. It is a toss-up.

MR. PLESSET: Paul, and then Jesse.

MR. SHEWMON: I am disoriented here. Was that an introduction to what is coming, or is that the summary.

MR. PLESSET: They have more but they got interrupted.

MR. SHEWMON: He got up and he went way, but I find a bunch of handouts have come out of heaven onto my desk.

MR. PLESSET: We will try to fix that, Paul.

MR. SHEWMON: Fine. What comes next?

MR. PLESSET: I think Jesse seems to be anxious to ask a question.

MR. OKRENT: Could you tell me what the TVA presentation is? Is that it? Are there five talks or what?

MR. PLESSET: They have more. Why don't you tell us what we want to do?

MR. MILLS: Our next presentation will be from a Westinghouse gentleman, Mr. Dave Gasser, who will talk about the Fenwall test which we are undergoing at this time on the igniters. After that we don't have anything else planned. We will try to answer any questions that you might have.

MR. SHEWMON: I would like to see a copy of what he handed out. My total recall failed me this afternoon.

MR. MILLS: I am sorry, sir.

MR. SHEWMON: I would like to see a copy of some of the assertions he made, of which he had two pages, so I might question him on some of them or decide if I want to.

MR. PLESSET: Do you have a copy?

MR. MILLS: Yes, sir. They should be there.

MR. SHEWMON: There is a bunch of figures that talk about the tests that you conducted. Now, where do I find it?

MR. LAU: Sir, the reason the presentation was so short was we were given 20 minutes and that includes TVA's presentation and Westinghouse's presentation. So I used a very brief position statement and then used the handout to support those statements.

MR. SHEWMON: Okay. If I wanted to find su;port for y.ur statements in the handout, where should I look?

MR. LAU: Sir, that depends on the subject that you want to locate.

(Laughter.)

MR. SHEWMON: Let's talk about why you think your containment would take 700 pounds of hydrogen without any cooling at all.

MR. LAU: That is based on the adiabatic burn. That particular statement, unfortunately, is not in the handout at all.

MR. SHEWMON: You asserted also that things would be even better in an ice condenser. Where is that supported?

MR. LAU: That is going to be presented by the Westinghouse presentation next. Obviously --

MR. SHEWMON: There is a lot of information there and it is not very useful or germane to what you said. Okay, I will wait.

MR. PLESSET: Jesse.

MR. EBERSOLE: I just wanted to ask --

MR. BENDER: Can I just ask quickly. The summary statement that you presented, Mr. Lau, have you got it so that it can be reproduced? You were reading from it, weren't you?

MR. LAU: Yes, sir.

MR. BENDER: Why don't you let one of our staff reproduce the few pages and pass them out. It would probably save a lot of time.

MR. LAU: I would be glad to.

MR. PLESSET: Jesse, you had a question.

MR. EBERSOLE: There is not any information available now, I take it, concerning your understanding of the functionability of this glow plug under various environments. I am particularly thinking of a deluge which would depress the temperature until you don't get any temperature, and maybe accumulate -- probably evaporate compounds on the surface.

It is a shielded plug, isn't it? The conductor is under a cladding of some sort.

MR. MYERS: That is correct.

MR. EBERSOLE: It can take a lot of beatings.

MR. MYERS: We believe it can take a lot of beating. That is the reason we selected it.

MR. EBERSOLE: Are you going to hit it with intermittent sprays which will put it out and turn it on and so forth?

MR. MYERS: The Fenwall test program has in it at this point some humidity tests. We expect to try to run some actual spray tests and some flow tests in the same area, and that will be covered in the later presentation.

MR. EBERSOLE: Aren't you going to be dealing with a tremendous variation in cooling on the surface?

MR. MYERS: Yes, and we are doing analytical studies at this time to try to estimate that as a basis for figuring out what kind of testing might be appropriate. There is also the question, of course, of the different environment that it will be in, the moisture fraction in the air, and we are trying to test and understand that because that is different than it was originally designed for.

MR. EBERSOLE: Thank you.

MR. PLESSET: Okay. Why don't you go ahead with the next presentation?

MR. GASSER: Dave Gasser, Westinghouse.

As at the Subcommittee meeting, the set of presentations, in order to minimize the total time taken, there will be a run-through of the transient analyses that support the statement that Wang Lau made earlier about having more capability than what he presented using the CLASIX code, and that is scheduled to be done by the NRC staff subsequent to our presentations.

I would suggest that we defer it until then, although if you would like, I also have some slides here that I can present. I was thinking perhaps for the Committee's time it would be better to just compress it, go through the hydrogen test program and then follow the presentation with the staff presentation in total.

As Wang mentioned, as a part of the total program going on with respect to the hydrogen control, we are doing testing work at Fenwall Labs in Ashland, Massachusetts. We have started this test work. The TVA igniter grooming has been started, and we have now run five tests within the six-plus foot diameter vessel that they have up there, and those tests have been run at 8, 9, two at 10 percent, and one at 12 percent hydrogen concentrations in dry air at room temperature.

We are scheduled right now to take delivery of the heaters for this chamber at the end of this week. Following the installation of the heaters, an initial grooming of the facility with the heaters on, we would run through the sequence of some 13 tests that had been defined -- and I will discuss those in a little bit more detail in just a minute -- and complete that effort in the first series of tests by approximately the 1st of October, which relates to a question earlier in terms of schedule.

What I have also shown on this Vu-graph, within the facility grooming, the igniter, based on the results at Singleton, heats to a full temperature in some 30 seconds. In our tests we have seen ignition at 17, 16, 16 and 15, and I cannot tell you what the fifth one is other than it is between 15 and 17. We just ran that yesterday. I am saying 15 to 17 based on just having watched my watch as opposed to having gone through the actual reduction of the data.

This shows the peak pressure that was reached within the chamber for each of the burns that the igniter started. Note with this ignition time we are seeing ignition sooner than we thought.

MR. EBERSOLE: Don't your times just define the heat at the dry igniter and had nothing to do with the combustion rate?

MR. GASSER: I did not mean to give the impression they had anything to do with combustion rate. This is when ignition occurred. And it is the time --

MR. EBERSOLE: Was the igniter preheated?

MR. GASSER: The igniter was off. We filled the vessel and then stirred the vessel, allowed it to sit and become quiescent, and then turned on the igniter. The igniter characteristic that has been measured at Singleton shows that it goes from turn-on to the time it reaches its maximum temperature in the range of 1700 degrees Fahrenheit in approximately 30 seconds.

Now, we are seeing the ignition here in something like 15 seconds, which means -- the meaning I take out of that is we are getting ignition in the dry air at a temperature less than the maximum of the igniter. The actual burn, looking at the traces -- and the equipment up there has the capability of running at 64 inches a second -- we see about one second or less to go from zero to approximately 10 pounds in this 10 percent case, and you have fully gone through the pressure transient to the peak in less than 5 seconds.

MR. EBERSOLE: What is the time response of your pressure measuring equipment?

MR. GASSER: The pressure recording equipment and pressure sensing equipment that they have up there is designed to be able to pick up detonation pressures. It is extremely high frequency. The actual charts run out, and we are running them on one of the slower speeds at 4 inches a second. So we get 5 feet of paper before we even start anything, and then have 100 millisecond tiny marks on it.

So we don't expect definition.

MR. EBERSOLE: What is the size of your vessel?

MR. GASSER: It is a six-foot diameter vessel, 130 some-odd cubic feet sphere.

MR. EBERSOLE: Is there variability on peak pressures as a function of volume at your content of the device due to self-compression?

MR. GASSER: There is some. In particular there is a change in the velocity of burn that you get.

MR. EBERSOLE: What is that sort of function, volume versus pressure peak?

MR. GASSER: I would have to go back and get David to respond to that because I cannot give you a fair answer. We have information on it.

MR. EBERSOLE: It is a needed piece of information, isn't it?

MR. GASSER: For this type of burn, as long as you are into a burn phenomenon, I don't think you will see a variation in the peak pressure as against the volume. The peak pressure now, you will see a change in the time you get to that pressure, but I don't believe you will see a change in the peak pressure until you move into things like very, very turbulent deflagrations or detonations.

MR. SHEWMON: What is the temperature at which yoiu think ignition is occurring?

MR. GASSER: We have not checked back yet.

MR. RAY: How many igniters were there in this vessel?

MR. GASSER: One.

MR. PLESSET: Go ahead.

MR. GASSER: The initial test --

MR. MOELLER: This is in air.

MR. GASSER: Yes. We don't have the heaters on yet. The initial test with the heaters will be aimed at a very rapid, establishing initially the igniter performance.

What we are looking to do here is pick up concentrations of hydrogen at two sides, one of which we get a complete conversion into pressure somewhere in the range of 10 to 12 percent based on the current data if you think of it in terms of dry air.

The other one on the other side, consistent with that 8 percent that we saw there, where you get a considerable amount of burning but you never get up high in pressure, and during the course of the actual tests we will take samples, having charged the chamber. We will then take a sample before and after ignition and go through a chemical analysis of that to find out just what we did to get a burn to correlate with the pressure conditions.

We will be looking at a sequence of tests that have 100 percent humidity conditions. We have varying pressures corresponding to, in a judgment sense, the kind of pressures that we might be seeing to initiate burns based on a preliminary analysis. We will be doing both static and flowing gas streams past the igniter within the test at pressurs that are the same.

So we would have one ignition in a given set of conditions with zero velocity. We will also then have the capability with the fan to put approximately 5 and 10-foot per second velocities past that igniter and the one you are looking at back there, blowing that stream right past the igniter and checking on any differences if they should show up in the ignition characteristic.

MR. LAWROSKI: One hundred percent humidity?

MR. GASSER: Having set the pressure that we want to run at, we have established an introduction of steam or water such that we would be at effective 100 percent humidity or saturation condition and then set the temperature of the vessel. We will hold constant temperature on these tests prior to starting.

MR. LAWROSKI: Relative humidity.

MR. GASSER: Yes.

MR. ETHERINGTON: Is the peak pressure reached a little later than the ignition time?

MR. GASSER: Yes, for the 10 percent case. And from zero to 10 pounds on that with 750 milliseconds. And as I recall, and I will have to confirm this number, the 50 pounds was reached about 1.1 seconds.

MR. ETHERINGTON: About 1.1 after ignition.

MR. GASSER: After ignition. The later tests that we have scheduled for the Fenwall facility on the TVA igniter work are aimed at further performance confirmation. What we want to do is take these initial results plus some things that we are seeing from the analyses and further go through characterization of performance of these devices in the kinds of atmospheres that we may see.

We will do additional hydrogen concentration tests. We have the capability within the facility to inject steam and hydrogen in varying mixes, starting from any condition that we chose, but at least theoretically starting from a condition of air in the igniter, inject steam and hydrogen and varying flow rates into the chamber with the igniter turned on, and see if we get the kinds of repetitive burns within that if the igniter is capable of doing that at the concentrations we will see.

The final one that we are currently contemplating if we can run the test is to look at the effects of sprays. I might note that the igniter itself is designed with that thing that comes out over the top, and it is a flash shield to prevent direct spray inpingement onto the igniter, the heated element itself.

And finally -- and Don, you might bring that up -- the last of the Vu-graphs with respect to the testing at Fenwall. We have run five burns. The igniter that we used up there was not sealed, and it continues to perform, or at least it did until I took it on an airplane last night, and I am not sure what it would do right now.

There are no major signs of external damage. There are some indications of minor penetration into the inside without apparent performance degradation. This now is the device that was put in. This one was not painted orange and white. This was put in and run through those five burns.

The inside, if you choose to look, with the transformer is apparently in mint condition. Even the outside other than some marks that have apparently, I think, occurred in transit, has no apparent blackening or degradation even though we measured using just taped on for the grooming. You can see the place where the tape is and some blackening at the tape.

We did measure temperatures in excess of 600 degrees in the gas stream that was coming through. We see some evidence here of penetration apparently inward during the course of the test, and we don't know whether it happened in the first or the last test or anywhere in between, but nothing showing up inside in terms of degradation.

So initially, at least, the igniter is -- and this is the actual igniter itself. We have taken it off the front so people can look at it and potentially even compare it back there. The igniter does seem to be capable of functioning within the environment that would be created in a hydrogen burn, a single burn.

Now, we remain to characterize whether it will ignite in the kinds of conditions and temperatures and steam and pressure that it would see.

MR. MOELLER: The igniter that was in this box was actually the one that caused the hydrogen to burn. It was not just placed there in another igniter.

MR. GASSER: The whole box you are looking at right now was inside that six foot vessel for five burns.

MR. MOELLER: And the igniter inside -- there was an igniter inside this box which did the ignition?

MR. GASSER: It is actually sticking outside, as you can see back there, and this is the one that came off of that.

MR. PLESSET: Let me ask you a question regarding this. Are you going to try to get into the range of detonation?

MR. GASSER: Initially, no.

MR. PLESSET: Will you try to do it eventually?

MR. GASSER: I think the only way that we would, at least I would think right now, that we would move into this is if we found in analyses that are currently contemplated for looking at the distributional effects that there was a potential for reaching that kind of condition. Then we may well go into that in the test program.

But Jack Myers might like to add something to that.

MR. PLESSET: What is the vessel like?

MR. GASSER: Six foot diameter, 2-inch thick. It is a coated vessel.

MR. PLESSET: Two-inch what?

MR. GASSER: Two-inch steel.

MR. PLESSET: Okay.

MR. GASSER: Two hundred or five hundred pounds. I don't recall.

MR. PLESSET: Why don't you try to go into the detonation range. That won't hurt that thing. You might get some interesting results.

MR. MYERS: The first phase of the testing, as he indicated, gets the baseline data to check with the data that is in the literature and used as a basis to go to Phase II where we are going to do more detailed studies. One of the possibilities in that phase, if it appears it can be safely done with the apparatus, is explore where the detonation limits are with the kind of environmental conditions such as water vapor that we are exploring for the flammability limits.

MR. PLESSET: I am trying to urge the staff to ask you to run the detonation range. Maybe they will.

MR. BENDER: What are your plans for the testability of this thing?

MR. GASSER: I could not hear all the question.

MR. BENDER: What are your plans for determining the testability of the igniter? Are you planning to install in such a way that it can be turned on every day or every few days to see whether it will heat up in its post heat-up?

MR. GASSER: I will defer that to TVA.

MR. MYERS: We proposed to the NRC a surveillance program for those igniters. And the current plan is they are on circuits and we are going to run basically a pre-op test or a baseline test to see what the current trial of those circuits is with the igniters working.

We will do some surface temperature checking at that point, and then we can go back in surveillance tests, turn them on and heat them up. And if there is a significant change in the current characteristics, we can go and check out why.

MR. BENDER: With regard to your test program, I could envision a circumstance where the igniter that was nearest to the high concentration of hydrogen was not the one which ignited the hydrogen. It might ignite it at low concentration and then propagate the deflagration or whatever it is into a higher concentration area.

Does your test program allow for that kind of circumstance?

MR. GASSER: In a single six-foot sphere, I do not believe we can get two different, significantly different initial distributions of hydrogen. As I mentioned, one of the things that we are going to try to do is, within that sphere, the igniter itself is located on a stand effectively in the middle, here. One of the ports that allows for filling is here, and angled up towards it.

We can run a test and come up with some reasonable data -- we can put in steam and hydrogen mixtures from here into potentially either an atmospheric no-hydrogen mix or no-hydrogen concentration and get test results that I am not sure that they do exactly what you want, but they begin to move in that direction.

MR. BENDER: Okay. It is just a matter of -- it is just being sure that we don't start the reaction in the wrong place and then get the detonation that we really did not want.

MR. MOELLER: How many different igniters did you look at and how did you choose this one?

MR. GASSER: I defer to TVA in terms of the test work on that.

MR. MYERS: We set out looking at a wide variety, and let me go through the process and you will see why I have some difficulties giving you an exact number. We knew roughly that we needed something that would give us a surface temperature in the range of 1500 degrees F. So we went looking for devices that would do that, and we came up with and actually got examples of a large number of devices that were alleged to do that or specified to do that.

And they ranged from things that were a foot or more long, with large coils, to a little bitty thing they use in model airplanes, which is a very small resistance fire that is open to the air.

And we looked at those, first of all, to see whether they were specifying. We checked back on a limited number of those that looked useful; I would say about six that we dug into in some detail. And some of those were supposedly going to do the job, but when we actually turned them on they could not cut it. When we turned them on, heated them up, they failed.

The diesel glow plugs, we have looked at the 7-G's, we have looked at another kind from General Motors. We looked at about -- and I believe -- at those that appear to function at the kind of temperatures that we are after. And the Bosch and the 7-G plug have been run for some time. So we have some confidence that it can maintain that temperature.

And so we are doing now screening analyses, sample analyses, to see what the relative reliability and ambient conditions are. So we have looked at a large number and slowly culled down until we get down to the 7-G plugs being our primary. But we are looking at backups in case we find some fault with the 7-Gs.

MR. MOELLER: What would the source of power be for this in an emergency?

MR. MYERS: They are run off the diesel generators.

MR. EBERSOLE: They call this a distributed ignition system, so it must be important to have distribution of some sort. Is there any carefully worked out plan about the degree of distribution?

MR. MYERS: Okay. The mechanism -- the interim system, okay. What we have designed right now is to be used on -- I would like to call bulk combustion. That is where the flame propagation is, basically, in all directions. So therefore, in a given volume, if the flame starts it will progress unless it runs into a rarefied atmosphere or one that is non-combustible.

That is the basis for the interim system.

MR. EBERSOLE: As it runs forward, it compresses the unburned gases in front of it and is approached by another wave, so that you do have autocatalytic compression.

MR. MYERS: At the velocities of wave front we are talking about, there is nothing in the sense of a wave front, if you will. The pressure is relatively uniform.

But there is a difference in the composition and there is experimental data to show some difference in flame speeds. And of course, there is the heating going on in that compressed gas. There is some data to give us confidence in what is going on there.

MR. EBERSOLE: I remember the delay tanks at the Browns Ferry stack discharge, that we were forced to go to 900 psi piping because of potential detonation effects there. There is a lot of data on hydrogen combustion.

MR. MYERS: Yes, sir. We are using that data. We are very familiar with that.

MR. ETHERINGTON: Your first slide showed eight percent hydrogen and pressure that looks like 3.2; is that right?

MR. GASSER: That is correct, 3.2. Perhaps it is worth putting this up, since it is available. I don't think it is worth spending a whole lot of time on, since it is only indicative of what we have. But what is shown here is the data from 1971, 12-foot diameter sphere, room temperature conditions, empirically testing what the pressure rise, the delta p, was from atmospheric, given varying concentrations of hydrogen.

You are looking at 4, 8, 12 percent. You see there data down in the range of 4 to 8 percent; there was a very low pressure rise. There was none in the area from 8 into the range of 12, where this data peaks up very steeply to what is the calculated delta p on a pure theoretical basis for that.

And the x's that are shown here just represent where we have been now with those four grooming runs, and we were not intending to reproduce this curve. That curve is in the literature and well done. We were just using it as an indication of how well our apparatus is in fact performing.

From this data, one would expect that somewhere in the range of 8 to 9 percent you are coming off of this curve very steeply and down. In that 8 percent we are showing it. In fact, we are just right about there, with a couple of psi rise.

MR. ETHERINGTON: With your igniter on, will that hydrogen eventually burn?

MR. GASSER: We will find out how much burns within the test program. We are planning to run, as I mentioned, 8 percent tests, having taken a sample before and a sample after the burn, and then doing measurements on that and correlating against what we saw as pressure and finding out whether or not all of it burns slowly and the effects of the heat sink of just building up temperature in the vessel.

We are not planning to attempt to glean that data from the test program, even in this initial phase.

MR. MARK: It is certainly believed and frequently said that the bottom of that can burn a very small percent of the hydrogen present.

MR. GASSER: That is right.

MR. OKRENT: I was wondering, can someone remind me of the relationship between the numbers like 50 psi that we saw at 10 percent hydrogen here and the 38 psi that we heard the staff and Chet and others talking about before?

MR. GASSER: Perhaps the easiest thing that I might do here, Dr. Okrent, is show -- and this is coming a little bit ahead --

MR. OKRENT: If someone is going to do it, I will wait.

MR. GASSER: To direct your attention to it, this is the transient run made igniting a 10 volume percent any time that it came, any time that we reached that concentration, wihin the CLASIX code, and then burning all the hydrogen that was available. And what is shown here is you come up and you peak not quite at 27 psi, come back down, back up, and there is a series of nine burns that take place.

And basically what is happening is, you have the hydrogen introduced, it burns, it exhausts through the ice condenser, and there is more air coming back in from the air return fans. And you also have the hydrogen coming back in from the presumed introduction using the MARCH. It becomes another burn, and you continue to do that.

And this happens also to be the basis, then, for the statement that Dr. Lau made a little bit earlier, in that this total transient includes the introduction of 70 percent of the core hydrogen potential, or some 1500 plus pounds of hydrogen into the containment, and then tracks that on through its burn. And in fact, you burn some 950 pounds, and there is some 600 left at the end.

That is going a little bit ahead into what Charlie Tinkler will be presenting. That is, in effect, a direct relationship in terms of what we calculate and then what we are looking at in the test program in terms of trying to determine the hydrogen at 10 percent is converted, and it will be ignited in the kinds of conditions that we see.

MR. OKRENT: Let me see. Just to test what I am hearing, if I reach, say, 10 percent in the upper compartment and ignite it there, what pressure should I expect, and is it okay?

MR. GASSER: If you reach 10 percent in the upper compartment, I cannot give you a figure off the top of my head for what 10 volume percent is in the upper compartment. If I go back, we know that if you take the 700 pounds and burn it uniformly you get up to the 57.

But this now, as you move into this and the calculations that are being done with plastics, you are looking at a coupled transient situation, and I do not have the number off the top of my head for what a 10 volume percent burn in the upper compartment means.

MR. OKRENT: Let's see.

MR. GASSER: You cannot use that pressure as the pressure that one would reach in that scenario that you are postulating. Those peak pressures merely show what happens within that chamber and the effects show completion of the burn or noncompletion of the burn. But those cannot be related to a situation within the containment.

MR. SHEWMON: The difference between what you do in your chamber and the question that Okrent is asking is that he -- you could relieve your pressure by expanding the gas into parts of the vessel -- parts of the containment which do not have hydrogen in them, just air.

MR. GASSER: Basically, you have a situation in the containment in which you have the lower compartment, the ice condenser, the upper compartment will get this -- this is part of the code -- with these things connected directly, in the sense of coming up this way; and also with the fan return and other things.

Now, as you track the accident scenario, where is the hydrogen introduced and how does it go, given the assumptions of mixing within these.

MR. SHEWMON: I thin you can answer my question by saying yes. But since you have not, let me ask it again.

The premise was that if the hydrogen is only in the upper containment -- compartment -- that it is the 10 percent and it burns. If the pressure does not relieve itself during the burn, you get 50 pounds. You assert you do not get 50 pounds. So you must relieve it somehow.

MR. GASSER: You will not necessarily get 50 pounds. You will get a delta p above what the atmospheric --

MR. SHEWMON: You get 50 pounds in your six-foot containment?

MR. GASSER: Yes.

MR. SHEWMON: There is no way that that --

MR. GASSER: And the initial conditions are standard. You get 50 pounds.

MR. SHEWMON: Let me ask again. Given 10 percent in only the upper compartment and it flashes, the pressure rise will be much less than 50 pounds, and the reason is that there is intercommunication between those regions.

MR. GASSER: Go ahead, Chet.

MR. MYERS: That is correct. There is another factor, though. First you have the communication through the open spaces between -- that go through the deck, if you will.

The second thing is, there is water spray in the atmosphere of the -- in the atmosphere of the upper containment. If the sprays are on -- and it looks like they should be at this time -- and the heat of evaporation eats up a lot of the pressure energy.

And the third thing is, if it is a reasonably slow burn, as we would expect at these concentrations, there is actually water put in during the burn process to take up a little more heat.

MR. OKRENT: The last time we met I think there was somebody from Westinghouse that indicated that there was a difference between a burn in the lower part of the containment and the upper part.

MR. GASSER: There is.

MR. OKRENT: And I guess first let me ask: How much is the difference and what is it attributed to? In other words, let me just for the moment postulate that we burn only in the lower part, starting with 10 percent, or we burn only in the upper part, starting with 10 percent. And let's leave the core sprays out of this -- containment sprays out of this, as if they were not on.

Is there a difference in expected pressure, in how much, and what can it be attributed to?

MR. GASSER: Yes, there is a difference in expected pressure. I cannot give you the answer because I don't know the number for the top. One of the major differences is you can relieve significantly through the lower compartment into the ice, because that is the way the flow is. From the upper side those doors will tend to close, and you cannot have the same size relief path as you do from the lower compartment.

MR. OKRENT: This is what I assumed. I just did not know how big the effect is. What I want to know is, is it urgent that you not ignite ir the upper part?

MR. EBERSOLE: Are you thinking about collapsing the intermediate --

MR. OKRENT: I am not thinking about anything now. I am trying to understand whether they really cannot afford to have it burn in the upper compartment. I would like them to tell me yes or no.

MR. MYERS: We have a capability for much more hydrogen burning in the lower compartment than the upper compartment. So a burn in the upper compartment -- a lower concentration of hydrogen will challenge the containment first.

MR. OKRENT: Do you know what concentration?

MR. MYERS: We only know it is bigger than 10 percent.

MR. EBERSOLE: Is the weak point the upper?

MR. MYERS: We don't think so, no, sir.

MR. EBERSOLE: Do you know accurately the rate of pressure rise?

MR. MYERS: We do not know that accurately, because that depends on flame speed and the flame speed is dependent on environmental conditions that we are trying to test.

MR. EBERSOLE: That can become very critical.

MR. MYERS: Yes, sir.

MR. OKRENT: Again, I would like to know a little bit more about the upper part. You said you know 10 percent hydrogen is okay -- I think you said that a moment ago -- in the upper part. That means you have done a calculation of some kind assuming 10 percent burn in the hydrogen and the ignition is taking place; is that correct?

MR. MYERS: Yes, sir. The very simple, straightforward, adiabatic combustion problem.

MR. OKRENT: What humidity did you assume?

MR. MYERS: 75 percent humidity at 90 degrees Fahrenheit.

MR. OKRENT: All right. And no water drops and so forth?

MR. MYERS: That is right. I also checked it at a slightly higher water load.

MR. OKRENT: What delta p did you get?

MR. MYERS: About 50 pounds, plus or minus a pound. I don't remember.

MR. OKRENT: I don't know why that is okay, because earlier I heard the staff say 38, if I understood correctly.

MR. MYERS: You must remember, I heard that first this afternoon and do not necessarily agree, you know, until I look at the basis they gave.

MR. OKRENT: I don't think I heard -- I am not sure what Dr. Siess was saying.

MR. PLESSET: The staff can tell you.

MR. OKRENT: It seems at the moment that we do not quite know whether -- whether you would be willing to turn those igniters on deliberately if you knew there was 10 percent in the upper part or you'd say, maybe I'd better look and see if there is something else I can do, burn in the bottom after I get it down.

Let me just assume a scenario. Hydrogen is generated and it is lighter than air and it moves to the upper part. Okay. In fact, there is some tendency sometimes of that to be in the upper part of the building. And you find it up there and you measure 10 percent up there. And it is 4 percent in the lower part. Would you in fact be willing to turn the igniters on, confident that your containment can take it? That is the question I am asking.

MR. MYERS: By the time one reaches that, I really do not know what kind of decisionmaking process I would go through. I think I would have to turn them on earlier than that, and I have not -- I have not gone through the decisionmaking logic that says whether I turn those on or not.

I would have reasonable confidence if it were below 10, based on the analysis we have seen. I would be very concerned if we were significantly above 10.

MR. OKRENT: It would be nice if it started burning at 5.

MR. MYERS: Yes, sir. What we are hoping the test data will show, because the literature shows that you get some partial burns; so we will get some removal, if you will, at lower concentrations as a benefit. But we are not relying on it.

MR. OKRENT: If I understand what I remember, most of your igniters are in the lower part.

MR. MYERS: Yes, sir.

MR. OKRENT: The place we want to be sure that we get ignition below 10 percent is in the upper part. I sort of have seen inverted logic in that.

MR. MYERS: The reason for the number is that there are a lot more compartments on the lower level. That is the reason.

MR. OKRENT: I understand. But you need a rather high degree of confidence in that upper part that you ignite before you get to --

MR. GASSER: One of the things to remember about the location: there are a number that are coming directly at the exit of the ice chest as it moves in. Those add to the ones that are in the upper compartment itself.

MR. SHEWMON: I am afraid I heard more about the strength of containments than I care to know, I thought. But I read Dr. Lau's thing here and it says, our analysis indicates that with a reasonable design limit of 57 psig for the containment. As I understand it, the design limit was 12 or 13 psig. The yielding limit is somewhere between 38 and 50, depending on who you talk to.

So did I slip a gear someplace or what does that statement -- whose reasonable design limit for the containment is 57 psig?

MR. LAU: Perhaps I did not choose the right word. But in this case what I meant is that the 57 is the number that we have been talking about earlier this afternoon. And the design is indicated with this particular series of events.

MR. SHEWMON: It is your ford hope and maybe professional belief that it would not rupture at 57 psi; is that what you are saying?

MR. LAU: It is not mine; it is our structural people's opinion.

MR. MILLS: There is one question. A gentleman at Brookhaven raised a question about ignition temperatures with regard to the foam insulation around our ice condenser. We submitted a response to the staff today, NRC staff, about six pages long. I would like to summarize that in about two sentences, if we could.

I will ask Mr. Don Williams of engineering design to address it.

MR. WILLIAMS: For the purposes of this meeting, I am going to read from the letter of September 4 to Mr. Tedesco of the NRC.

"The containment wall was insulated with a rigid urethane foam which is poured in place behind the air handling ducts. The ducts run the full circumference of the height of the ice condenser containment wall interface, and are steel panels which provide an effective thickness of at least one-quarter of an inch of galvanized steel.

"The foam insulation after installation is sealed at the top and the bottom to form an airtight compartment."

That is the end of the text in the letter.

Just in summary, Westinghouse has test data on the foam insulation which shows that the foam encased -- when the foam is encased, it is open at the top. It leaves a readily available source of air to burn. The foam will not burn by itself. It needs air, and our fire protection engineers have also looked at the flammability of this foam, and in summary we are convinced that the foam insulation in the ice condenser walls is sufficiently sealed to preclude any burning.

MR. PLESSET: Did you have any other comments you wanted to make?

MR. MILLS: No, sir. I did want to say particularly Dr. Lau rushed through his talk. We thought maybe it would save some time, and I realize it was rather skimpy in parts, and we would be willing to expand on any portion that any member would be interested in, or to answer any questions along those lines.

MR. PLESSET: Well --

MR. MILLS: Whatever your desire is, sir.

MR. PLESSET: Let me ask you another question about the status of the review of the vent pipe repair. Have you got a comment for us on that brief comment, or were you prepared for that? Staff is going to tell us about that.

MR. STAHLE: We are prepared to discuss the I&E inquiry with respect to the repair. As far as repair of the weld, we believe that is complete, and of course it is discussed in our SER Supplement Number 2.

MR. PLESSET: Okay.

MR. STAHLE: Would you like --

MR. OKRENT: That is the hydrogen issue?

MR. PLESSET: Do you want some more? Let him finish this.

MR. OKRENT: I assumed we have not finished this.

MR. PLESSET: On hydrogen or on this?

MR. BENDER: Just a short question. It won't take long. With regard to the elastic seals, are you planning to do anything further about the elastic seals than you have done?

MR. MYERS: At this time we have not chosen a course of action, but I would say that that action would lead one of two ways, one, either to be able to show by analysis and available data that the seals would not suffer the kind of deformation that would open them up when the pressure was released, or to actually do some testing ourselves under those pressures. That would be our intent.

MR. BENDER: That is a good answer. Thank you.

MR. PLESSET: Let me clarify one thing, Dave. You were asking about these pressure limits, and Dr. Zudans says he would feel comfortable with 50 psi, just to give you a number to cherish and remember.

MR. SHEWMON: Fifty psi for what?

MR. PLESSET: For containment pressure to yielding.

MR. SHEWMON: How about between floors of the containment?

MR. PLESSET: You did not go into that, right?

MR. ZUDANS: Right.

MR. PLESSET: He is not worried about that, I gather.

MR. EBERSOLE: Mr. Chairman, are we going to talk about survivability of apparatus inside, or is that a separate business?

MR. SHEWMON: Telephones don't do very well.

MR. EBERSOLE: There are many things in there that will give you trouble. I don't know if that was within the scope.

MR. PLESSET: It was not originally planned, but if you have a need --

MR. EBERSOLE: I can think of a good many needs, like where we have imploding of apparatus not ready to receive this rather high spike pressure, which may or may not be fitting.

MR. PLESSET: That is a legitimate question.

MR. EBERSOLE: It is interesting to note that you may have a gale through your air return fans, which will either stall them or make generators out of them and make them go ten times their normal capacity.

MR. MYERS: Let me tell the committee where we stand on that issue. We have first of all gone through and identified equipment which we have determined to be critical. That is the kind of stuff that you either should

.ave, have to have, or would like to have post the kind of events we are talking about. We are now in the process of going through and finding out what our paper says about that equipment.

Let me take pressure, for instance, static pressure. We have been through the bulk of those components, and most of the components in an ice condenser are qualified for pressure, the same as a dry containment, from 55 to 75 pounds, so the ice condenser has that margin already built in.

There are a few components we found which were qualified to about ten pounds, but each of those that I looked at so far were things that we did not need, and if they were to respond in a very bad manner because of the high pressure, would not cause us any pain.

I think the committee is probably aware that we some years ago took off the little push buttons on the wall that you operate valves and motors with, and use disconnects, which are supervised circuits in the control room, so the pressure pulse could not push those buttons. Pressure, we feel quite comfortable with at this time. We may find a few instruments that we would like to have that do not have the qualification, and those may need to be changed out, but we have not found any yet.

As far as pressure pulse, the kind of flame rates we are talking about so far they really aren't a problem from a pressure pulse point of view, so until we get into much faster burn times, that is sitting on the back burner, if you will. The temperature question is one where our codes are grossly conservative, in that they do not account for heat sinks, and so at this point we do not have a real firm handle. We have a very conservative handle, I guess, on the temperature transient things we go through.

You have seen the igniter that has been throug some of these transients, and we are going to in later tests probably test some material to see what kind of -- what this transient temperature does to them.

As far as the dynamic effects, such as flow through the air return fans, the air return fans are one of the critical components, and that is something we have to look at both when it is assisting and when it is blowing back against you.

MR. EBERSOLE: Yes.

MR. OKRENT: I have a question of the staff. In their SER supplement, they mentioned that they have a report from their consultant at Brookhaven National Laboratory. They do not discuss it very much, unless I missed where it is discussed, but I think it falls in the category of what Paul Shewmon calls fast reading.

(General laughter.)

MR. OKRENT: But he did not complain about this package. I do not know why. In any event --

MR. SHEWMON: Nobody promised me a quiz on that tomorrow.

MR. OKRENT: I would like to ask the staff whether in the letter of August 8, 1980, from Barry to Ross, there are any reservations that have been raised there that stem either -- either now look troublesome based on what they know or they would consider potentially troublesome, you know, or do they think these are likely all to wash out.

MR. KERR: Isn't a copy of that letter included with one of the SECY papers? I have seen a copy. You perhaps probably have one somewhere, Dave, if you knew where to look.

MR. OKRENT: I have the letter from Barry to Ross.

MR. KERR: Okay, you have the letter.

MR. OKRENT: It is an appendix in Supplement Number 2 to the SER.

MR. KERR: Okay, you have the letter. I am sorry.

MR. STAHLE: We do have a vu-graph on this matter, if you would like to have the staff discuss it in a little more detail. This may answer your question.

MR. OKRENT: It would be relevant, Mr. Chairman, since we do have these questions posed. A couple of them have been discussed already, but not all

MR. STAHLE: I will have Mr. Tinkler go into this subject.

MR. PLESSET: Harold, did you have a question?

MR. ETHERINGTON: Yes.

MR. PLESSET: We will come back to it, unless it is on this.

MR. ETHERINGTON: There must be some concern whether the 10 percent of water, steam, or whatever it is at 90 degrees plus the possibility of sprays is going to raise that region of non-combustion from 8 percent into maybe the 10 or more percent. Then we have to be really concerned about the containment capability. Are we going to have answers to those questions before they desire to go to power? Or do we have answers now on a theoretical basis?

MR. PLESSET: I don't think your question got across quite. Could you repeat it?

MR. ETHERINGTON: We have a region of non-combustion of hydrogen, around 8 percent dry air. In the presence of steam and particularly sprays, that surely is going to be higher. Is that correct? And that is pushing us into a region where we have to worry about our peak pressure in view of our containment limit being under 50 psi.

My question is, do you have any discussion of that, or do you expect to have answers before you go to power?

MR. PLESSET: That is a question they will have to consider. Maybe we will come back to that, if you can hold it for just a minute.

MR. ETHERINGTON: Yes, surely.

MR. PLESSET: All right, go ahead.

MR. TINKLER: Okay. Regarding the comments in the report -- here is the Brookhaven letter, Possible Objections to Igniters. One of the comments was, "Ignition may occur when some regions are detonable."

Our general response to that is, one of the functions of the igniters is to prevent large detonable admixtures from accumulating. Igniters will be turned on early, be effective as early as possible so we can burn the smallest possible quantities of hydrogen at a time. The fans do provide active mixing to promote small gradients within the various regions of the containment, and it is based on our preliminary information.

We believe that the containment shell and interior walls could survive small local detonations. There remains to be more work done on the last item, but that in essence is our basis for believing that that is not a substantial concern. Detonable regions may also occur if we do not have distributed ignition systems. Random ignition could cause local detonation, as well as an intentional ignition system.

MR. OKRENT: Those statements are true, except presumably some people are thinking about alternatives to ingnition systems, and it is not ignition system or nothing.

MR. TINKLER: It would have to be a suppressant.

MR. OKRENT: All right. So, I am not sure you have given all the alternatives that are potentially possible in giving your arguments, and you might have --

MR. TINKLER: If you inerted the atmosphere, you would prevent that sort of mixture. Okay? That is the only other way I foresee that you would guarantee you would not have detonable mixtures.

MR. OKRENT: I have seen other things that may or may not do it also, but I just wanted to note that you have a somewhat incomplete stage on which you are presenting your players.

MR. TINKLER: Assuming we go with ignition control, those would be our options.

MR. SHEWMON: So what if you get detonation? I mean, ignition may occur in some regions that are detonable. Why don't you just say, so what?

MR. TINKLER: Well, we cannot say so what right now.

MR. SHEWMON: Why can't you?

MR. TINKLER: Because we have not demonstrated, at least not to my knowledge, that the detonations will be --

that all the possible detonations will be sufficiently small that you could never rule out the problem. You could never rule out the concern for problems during detonations, problems such as pressure loading, shock effects on equipment.

We do feel --

MR. SHEWMON: Pressure loadings on what?

MR. TINKLER: On the shell. TVA has examined it briefly by reference to other work that has been done in the area, I believe, and has concluded that the effects of detonation on the containment shell are minimal.

MR. SHEWMON: If you can find any references that that is not true, we would very much appreciate seeing them.

MR. TINKLER: I do not know -- and I am not aware of any references that say that is not true.

MR. SHEWMON: I will provide you one before you go home.

MR. TINKLER: Okay. If we could guarantee that detonation poses no threat to the containment shell, we would be glad to state that as a reason for not being concerned with it.

MR. PLESSET: I think Dr. Shewmon is aware of some analysis that maybe you are not aware of for which he is partly responsible.

MR. EBERSOLE: I get a little uneasy about people not looking on the slab between the upper and lower compartment that holds the air return fans and a lot of other stuff as being a significant structural member, which, if damaged or destroyed, may cascade into other equipment failures, even though the shell is good.

So, the slab has some defined psi loading, and it is rate sensitive, and if you get the detonation rates, I wonder if it will relieve fast enough.

MR. TINKLER: Are you talking about the effects of actual detonation loading or equilibrium pressure that would result after detonation?

MR. EBERSOLE: Whatever the effect of shock load on the slab is. I suspect it is critical upward as well as downward. I am not sure. You don't know what the loadings are, do you? It is a rate thing, because it has appertures in it.

MR. TINKLER: There is some limit above which you cannot tolerate deflagration of hydrogen in the upper compartment, let alone detonation. I doubt very seriously -- We would have a lot of problems if you had a deflagration of 12 percent of the upper compartment, let alone a detonation.

Another point of the Brookhaven memo was, "Focusing effects can develop detonations." This is a valid concern as far as we know. The problem should be addressed with or without igniters, assuming you do not suppress the burn in some other fashion. But again, in general, early ignition we feel is favorable, burning the hydrogen at lower concentrations or as low a concentration as possible.

One of the comments was regarding combustion. "associated pressure and temperature histories unknown. These have not been calculated." That is not true. It should say, these have been calculated, and have been presented before the subcommittee. These results are based on the CLASIX code, the MARCH code, and various adiabatic calculations.

The Brookhaven memo expressed concern over lower compartment ignition that would progagate to the upper compartment. We have done analysis where we propagated burns to the upper compartment, and the results of those analyses have demonstrated the pressures are below yield.

Again it is true that the flexibility is more limited when you burn in the upper compartment, but to date, based on the analyses that have been performed, we have seen that given the distributions that are calculated in the codes due to the mixing of the fans, that the concentrations are such that even with burning in the upper compartment, the pressures were tolerable.

MR. OKRENT: What enrichment do you think can be burned in the upper compartment acceptably?

MR. TINKLER: If we judge it by 38 psig, I would say that 10 percent is getting close to the limit, or maybe at limit. It would be simple to do an adiabatic calculation of 10 percent. If you want to ignore the sprays, which I do not think is appropriate, but if you did ignore them, it would be simple to do the hand calculation, but just as a guess, I would have to say probably around 10 percent.

The question regarding insulation has been addressed already, and there is a question regarding reliance on the air return fan system. Unless the air return fans are made inoperable by hydrogen burning, we see no reason to assume all the air return fans are --

MR. OKRENT: That is just the question they posed. Your answer that it is safety grade --

MR. TINKLER: The system is safety grade. That just addresses the fact that other than the effects of hydrogen burning, we see no reason to assume that fans are not operating.

MR. OKRENT: Unless you do not have AC power, which is a frequently touted cause of the original event.

MR. TINKLER: Yes, but I would point out that the effects of hydrogen burning on the air return fan system is an area that we intend to pursue. There is not much else we can say about it right now.

MR. BENDER: The purpose of deciding whether and when to turn on the igniters, how much sampling do you need to have?

MR. TINKLER: Well, the TVA position right now is that the sampling system that was originally designed for the plant is adequate for interim operation. They have indicated that they will continue to study the need for upgrading sampling. We feel that may be prudent, that they increase sampling so they know concentrations in local regions or in subregions or some portions of the containment. Right now, most of their sampling is based on a sample of gas that is mixed before it goes to the analyzer.

MR. BENDER: I would think sampling ought to be on that list, then, somewhere.

MR. TINKLER: Well, this is the Brookhaven list. We have a list of topics for further review, and that is one of them.

MR. OKRENT: Do you envisage that the igniters would be turned on when you reach the right concentration, or they would be left on early if you thought there was a chance of building up hydrogen?

MR. TINKLER: Right now I believe the igniter actuation is a step in the emergency procedures after which certain other actions are taken. If we demonstrate that igniters do not pose a threat, or do not result in a loss of safety, it would presumably be beneficial to turn them on as early as possible.

MR. OKRENT: Does TVA have a position on this? Do they think they should not be turned on early for some reason?

MR. MYERS: Let me give you just the picture here. TVA committed in their nuclear program review to study and backfit if possible. We decided this interim system was feasible to do, and so the first thing we checked was to see if we could find any safety drawbacks, assuming no benefit, were there any safety drawbacks.

We did not find anything significant, and that was largely due to the fact that we believed there was a large number of ignition sources there already. They may not be as reliable as ignition sources as we would like, so our policy for the interim system is to turn them on as soon as we know we have a LOCA at this point.

That is what the procedure is based upon, and the rationale for that was that we -- one of the ways you can get into degraded core conditions is the operator screws up and does not make a decision at the right time, and to turn on the igniters is just another decision. We don't have the monitoring at this point to justify that.

TVA is committed to upgrading the monitoring system, the hydrogen monitoring system at all our plants We are going to add additional monitors and additional monitoring locations. We have already started on that.

MR. SHEWMON: Let me make a statement and then, I guess, ask both of these people to reply. I have in front of me a figure here. In my speed reading, I got halfway through this. Anyway, it is a report diagram quoted in WASH 1400 on combustion and flammability of hydrogen-steam-air mixtures. It shows the flammability limit being some place at 5 percent and below, rather independent of the amount of steam that surrounds it.

The question is, is there any reason that either the staff or TVA has to believe that this is not applicable, and thus the gas mixture would not start burning at 5 percent?

MR. TINKLER: Okay. As we reported in the subcommittee meeting, we have obtained the assistance of Lawrence Livermore in order to test the igniters, and they will test the igniters in varying atmospheric conditions of steam, air, and hydrogen, in order to expand that portion of the curve so we have a better understanding of the effects of steam at low hydrogen concentrations.

The slope is so steep there it is very difficult to see the exact effect.

MR. SHEWMON: What slope?

MR. TINKLER: Of the curve there. The effects of steam on a flammability limit. It appears to be very small.

MR. SHEWMON: There is virtually no effect of steam up to about 40 percent.

MR. TINKLER: The Livermore test will be run with steam concentrations all the way up to 70 percent. Okay? So we should have an assessment of the effect of steam on the flammability limits, and I believe that Fenwall test programs will eventually include those effects, too.

What you saw were shakedown tests where they just run it on dry air.

MR. OKRENT: I asked if you believe this curve. You said yes, but you are getting it reconfirmed.

MR. TINKLER: We want to learn more about it.

MR. MYERS: I am not an expert on hydrogen combustion, but my people have been going through the available literature and are working with a test program. So what I have to say just basically reflects what we have found to date in the literature. Ignoring the steam component initially, about 4 percent you can ignite hydrogen in a quiescent stream or it will burn upward basically.

Somewhere in the range, depending upon the source, of 6 to 8 to 9, it starts moving sideways, and then it will actually propagate in all directions. So in actual fact, the physical arrangement of the ignition source, if you will, to physically what is going on around it may change. What you call you flammbility limit, that is, hydrogen may burn at 4 percent but it will not propagate in all directions, so you may have to get higher than that to get three dimensional propagation.

The literature does indicate that steam moves this lower limit the equivalent of 4 percent. It does affect that. At this point we don't have any data that indicates to us how much it burns at that very steep slope on the curve that was shown a while ago.

MR. SHEWMON: You were trying to convince us that you had complete mixing in any part -- if you are in here trying to show us you have complete mixing in any part of that thing, we would hoot you out of the room.

MR. MYERS: We are quite sure we have inhomogeneities between the different compartments already. We know that.

MR. SHEWMON: That is a plus, because when one starts to go, you have the rest of your system.

MR. MYERS: Yes, sir.

MR. SHEWMON: Okay.

MR. BENDER: I would not be all that enthusiastic. I think the ignition might start in the wrong place.

MR. SHEWMON: I don't understand what you are talking about then or now. Can we do it over supper?

(Laughter.)

MR. EBERSOLE: Mr. Chairman. Chuck, in the consideration of alternatives, the question has come up of how many man hours do you spend in Sequoyah per year inside the containment and what for, and ultimately with all this array of junk inside the containment that you have to go in and tend versus the Browns Ferry case where you don't have any.

MR. MYERS: I cannot give you exact numbers, but they are not a few percentage points difference between the entry requirements at Browns Ferry with this inerted containment, very small, and Sequoyah, which has a lot of complex equipment.

We have gone through on the inerting question -- and I don't have those results with me today -- and looked at our best estimate working with the operating people, what equipment is in there and how frequently it actually requires maintenance as opposed to a designer's idea of how often it would be required, and then looked at moving those things outside, if possible.

We were able to reduce significantly the entries, which were several times a week at this point. We hope they will get less than that.

MR. EBERSOLE: Would it be in hundreds of man hours per year?

MR. MYERS: Yes, sir.

MR. EBERSOLE: How many hundreds?

MR. MYERS: I don't know. But when you go in for planning purposes, two or three men more than once a week, it will build up pretty fast.

MR. EBERSOLE: Thank you.

MR. OKRENT: We talked around Harold Etherington's question, but I don't think we have heard TVA or the staff directly address it.

MR. PLESSET: Would you repeat your question, Harold, since we may have forgotten it?

MR. ETHERINGTON: The premise was that in the presence of hydrogen or sprays, the composition of the noncombustion was 8 percent in air, and it is likely to be higher and push us up into the 10 or 11 percent, and then we are beyond the peak pressure -- the pressure is beyond the containment capability.

My question was whether we are going to have answers to this question either on a theoretical or experimental basis for the plant goes to power.

MR. MYERS: The question of when the plant goes to power is outside of my area where I can do anything about it. We are ready at this point and we do not expect to have the kind of data you are talking about and the kind of data you want before the next several months, two to three months.

MR. ETHERINGTON: You would like to go to power without it.

MR. MYERS: Yes, sir. We do not know theoretically how it would move, but there is good reason to believe that it would go up. There is another effect that cannot be forgotten. Ten percent hydrogen is not the same no matter what kind of water content you have. As the vapor fraction goes up, that has a beneficial effect on you -- not real strong, but it is beneficial.

MR. OKRENT: One can think of a scenario possibly where you have a lot of steam and that suppresses the combustion. If you have a lot of spray, it is hard to see how you could have too much steam. Let me for the moment suggest you have hydrogen steam and you need those sprays and so you turn it on. Because they have been on long enough, you turn them off. And there you are.

MR. MYERS: One has a little difficulty, when one assumes several hundred pounds of hydrogen and choosing its location, ruining most containments, including the ice condenser.

MR. OKRENT: This is not completely out of the realm of situations that could occur.

MR. MARK: Could I mention, Harold, perhaps this curve which has the fuzzy zone, as we produce frequently, is not the only curve on flammability limits. There is a curve at 300 degrees Fahrenheit with experimental points all the way up to 50 percent steam for flammability limits. Those numbers are known.

MR. OKRENT: I think what you are interested in here is that ratio where it will burn horizontally and downwards. You may not get enough combustion.

MR. MARK: If it is close to the flammability limits, fine. If you run far beyond them, it is quite different.

MR. OKRENT: Mr. Chairman.

MR. PLESSET: Yes.

MR. OKRENT: I would like to get back to the staff a little bit.

MR. PLESSET: On what subject?

MR. OKRENT: Hydrogen.

MR. PLESSET: Okay.

MR. PLESSET: But in a broader conte. . In this document entitled Hydrogen Control for Sequoyah, prepared by the staff, they discuss, I guess, what their plan is with regard to the interim rule, and what they say is that this interim rule design analyses would be performed for all other plants to evaluate measures that can be taken to mitigate the consequences of large amounts of hydrogen generated within eight hours after onset of an accident.

Design analyses would be filed six months after the effective date of the rule, or by the date of filing of the application for the operating license, whichever is later. I assume that is similar to what they are proposing or have just gotten approved.

MR. STAHLE: That is correct.

MR. OKRENT: As I indicated earlier, in my own mind the generation of large amounts of hydrogen is sometimes associated with an event which stops, like TMI, but perhaps with at least equal probability with an event that goes further.

At the moment it is not clear to me by what rationale the staff, for example, has decided that they should ask for design analyses for measures that can be taken to mitigate the consequences of large amounts of hydrogen, but not ask for design analyses for other measures.

The ACRS has recommended once or twice -- I cannot remember -- that the Commission ask for a more broad kind of analysis. And since this is still low pressure containment compared to most, although people have moved it up to 12-1/2 or some pressure like 38 or something where it might fail, it is more subject to overpressurization in a range of transients than the large dry containment.

I think at the moment I find it a little bit hard to rationalize the staff position, and in a sense the Sequoyah review is in the context of the staff's proposal for what I guess they call an interim rule. So somehow it seems to me in what we say to the Commission on this, either we reiterate our previous recommendation and say don't do just design studies on hydrogen control but do it in a broader way -- and this is certainly applicable to Sequoyah -- or we would be -- I don't know -- in some ill-defined situation or position with regard to what the ACRS thinking was.

I want to call that to your attention. We discussed this obliquely at the last meeting. I think the applicant says he was going to do some kind of study, not in what we have seen here. But in any event, I have a problem with the staff at the moment aside from the applicant because I do not understand their logic.

Let me go one step further because I think it is relevant. We were earlier talking about failure to scram and what was the probability of a seriuous accident there. I don't know what the real numbers are, but some of the numbers we saw for BWRs possibly are significant when you look at the probability per year.

There is a document that the staff has just made available to us either last week or today -- I am not sure, it may be both -- in which somebody has looked at auxiliary feedwater systems that are nonseismically qualified, and they arrived at the judgment that maybe this might lead to a probability of 10^{-4} core melt, and they would look at WASH-1400, and they say if we let this run three years while we figure it out, this will only double the probability.

Now, I find that these are getting to be big numbers. In fact, I am not saying that these numbers are unrealistic. I think when you start looking at all the avenues and all the experience that we have had, we had better assume, at least for purposes of planning, that the numbers might be pretty big.

In other words, I don't see any basis for being very confident that they are small or even within the band of WASH-1400. If, in fact, they are fairly large, I would say that is all the more reason for the Commission not dragging for several years through some kind of ill-defined rulemaking on degraded cores and core melts before it does something

MR. PLESSET: Well, I think that was a nice summary.

MR. BUTLER: May I react, Mr. Chairman, with respect to the first part of Dr. Okrent's question?

MR. PLESSET: I think that is fine, but I thought you might want to think it over.

MR. BUTLER: If we have that opportunity, we will read the transcripts and provide a response later.

MR. PLESSET: Which would you prefer, Dave?

MR. OKRENT: I would be interested in hearing any comment they have now and later.

MR. PLESSET: Both. All right, go ahead.

(Laughter.)

MR. BENDER: Why don't we encourage them to think it over some so we just get one answer.

MR. PLESSET: All right. Will you accept that? You will not be unhappy with that. We will take one answer later.

Any other questions on these items? I want to give the NRC staff an opportunity to make a few comments, and I think we are running a little behind schedule, not surprisingly. So could we go to the staff?

MR. STAHLE: Mr. Butler will make a few comments.

MR. BUTLER: I intend to rely on only the last four or five sheets of that handout. This will be just a very brief statement on how the staff would respond to Commissioner Gilinsky's two questions. There is an extra slide there, I think, that should not be in there, and it will be clear shortly.

The first question: Does the staff believe additional hydrogen control measures are necessary for ice condenser containment? It is the staff's view, in response, that hydrogen control measures beyond those prescribed in 10 CFR, Section 5044 are required for ice condenser plants but that a reasonable period of time, that is, within a year, may be allowed for implementation of these measures so that appropriate studies and tests can be completed.

The staff's bases for this view are: one, TMI Short-term Lessons Learned items have been implemented, placing Sequoyah in the same risk space as Surry and Peach Bottom; two, aggressive applicant and staff programs are in place to improve the hydrogen management capability of Sequoyah with a time frame of the next four months.

Preliminary work shows the interim distributed ignition system to be a very promising approach. Four, backup programs are in place should the IDIS prove unacceptable. And finally, operations at full power do not foreclose later adoption of alternative measures.

(Slide)

The next slide is a restatement of Commissioner Gilinsky's question number 2, and I have just directed it to the staff. Is the staff reasonably persuaded of the effectiveness of distributed igniters in ice condenser containments? It goes on. The response is the longer one, A.2.

The staff's view is that the distributed igniter system appears very promising as an additional hydrogen control measure. Analyses with the CLASIX and MARCH codes have shown the system can substantially improve capability of an ice condenser containment to accommodate the hydrogen releases from the degraded core accident.

However, further analyses and tests need to be conducted to determine its range of efficacy and to assure that overall safety is not degraded. The work is now under way and should be completed by December 1980. Pending the results of further work, the staff's view is that use of an igniter system during accident sequences involving TMI-like quantities of hydrogen would be the containment pressures that exceed design pressures but that are less than containment failure or yield pressures.

We find these features of the IDIS acceptable pending the rulemaking proceeding on much the same bases indicated in our response to the first question. Those are the staff's views on how it would respond to the two questions.

MR. PLESSET: Okay, thank you.

Any question?

MR. MOELLER: I guess I have a question. This addresses Sequoyah. Where does D.C. Cook fall in all of this? I mean I am not prolonging it, but here are measures, and you are saying it is okay for Sequoyah to operate maybe a year or four months or whatever. We have D.C. Cook in operation and I wondered where it fits in.

MR. PLESSET: D.C. Cook 1 and 2.

MR. MOELLER: Yes. It is a two-unit station.

MR. BUTLER: The staff's view on that was presented during the subcommittee meeting, and it goes as follows. Once a mitigated device or system is found suitable and acceptable for the Sequoyah station, the staff will then make that kind of a system required for all ice condenser plants.

MR. MOELLER: Thank you.

MR. PLESSET: Any other questions of either the staff or TVA people?

If not, let me mention again an announcement that I made this morning. The Committee is meeting with the

Commissioners tomorrow at 1:30 to discuss this item, the Sequoyah license item. I believe it will be upstairs. So that any of you who are interested might want to come at that time. It will be an open meeting.

There will be part of it that precedes that relating to quite different items that are closed, but after the closed session we will go into open session on the Sequoyah matter. So those of you who would like to participate and listen should be aware of that.

MR. KERR: When you say those of you who would like to participate, you are talking about members of the Committee.

MR. PLESSET: Beg your pardon? Well, I will get that later.

We will go into recess for ten minutes.

(Whereupon, at 6:45 p.m., the Committee recessed, to reconvene in executive session.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

____

in the matter of: ADVISORY COMMITTEE ON REACTOR SAFEGUARDS - 245th Meeting

Date of Proceeding: September 4, 1980

Docket Number: ______________________

Place of Proceeding: Washington, D. C.

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]

(SIGNATURE OF REPORTER)

CURRENT ASSESSMENT
ROLE AND EFFECTIVENESS OF IGNITERS

. IGNITERS ARE NEEDED TO ASSURE THAT CONTAINMENT PRESSURES DO NOT EXCEED FAILURE PRESSURES FOR A SUBSTANTIAL FRACTION OF DEGRADED CORE ACCIDENT SCENARIOS.

. THE LIKELIHOOD THAT THE IGNITERS WILL BE CALLED INTO SERVICE HAS BEEN MADE VERY REMOTE BY IMPLEMENTATION OF STLL MEASURES, I.E., THE STLL ITEMS HAVE PLACED SEQUOYAH IN THE SAME RISK SPACE AS SURRY AND PEACH BOTTOM.

. PRELIMINARY RESULTS FROM ANALYSES WITH THE CLASIX AND MARCH CODES SHOW THAT IGNITERS CAN KEEP HYDROGEN BURN PRESSURES BELOW ABOUT 26 PSIG FOR CASES INVOLVING SUBSTANTIAL AMOUNTS AND RATES OF HYDROGEN RELEASE.

EFFECTIVENESS OF IGNITERS

PRELIMINARY ANALYSES OF HYDROGEN BURNS DURING THE S_2D SEQUENCE - WITH CLASIX/MARCH INDICATE THAT:

- PEAK CONTAINMENT PRESSURE WILL BE ABOUT 26 PSIG IF ICE DOES NOT EXIST DURING THE HYDROGEN BURNS

- PEAK CONTAINMENT PRESSURE WILL BE ABOUT 13 PSIG IF ICE EXISTS DURING THE HYDROGEN BURNS

ROLE OF IGNITERS

1. PERMITS THE CONTROLLED BURNING OF LEAN MIXTURES (8-12%).

2. ALLOWS ICE BEDS, SPRAYS, AND HEAT SINKS TO ABSORB THE BURN ENERGY.

3. PROMOTES BURNING IN LOWER COMPARTMENT.

4. PREVENTS DEVELOPMENT OF DETONABLE MIXTURES IN LARGE VOLUMES.

ASSESSMENT OF THE NEED FOR ADDITIONAL HYDROGEN CONTROL MEASURES

THE STAFF'S VIEW IS THAT HYDROGEN CONTROL MEASURES BEYOND THOSE PRESCRIBED IN 10 CFR SECTION 50.44 ARE REQUIRED FOR ICE CONDENSER PLANTS, BUT THAT A REASONABLE PERIOD OF TIME, I.E., WITHIN A YEAR, MAY BE ALLOWED FOR IMPLEMENTATION OF THESE MEASURES SO THAT APPROPRIATE STUDIES AND TESTS CAN BE COMPLETED.

THE STAFF'S BASES FOR THIS VIEW ARE:

- TMI STLL ITEMS HAVE ALREADY BEEN IMPLEMENTED PLACING SEQUOYAH IN THE SAME RISK SPACE AS SURRY AND PEACH BOTTOM.

- AGGRESSIVE APPLICANT AND STAFF PROGRAMS ARE IN PLACE TO IMPROVE THE HYDROGEN MANAGEMENT CAPABILITY AT SEQUOYAH (TIME FRAME: 4 MONTHS)

- PRELIMINARY WORK SHOWS THE IDIS TO BE A VERY PROMISING APPROACH.

- BACKUP PROGRAMS ARE IN PLACE SHOULD THE IDIS PROVE UNACCEPTABLE.

- OPERATIONS AT FULL POWER DO NOT FORECLOSE LATER ADOPTION OF ALTERNATIVE MEASURES.

Q - 1. DOES THE STAFF BELIEVE ADDITIONAL HYDROGEN CONTROL MEASURES ARE NECESSARY FOR ICE CONDENSER CONTAINMENTS?

A - 1. THE STAFF'S VIEW IS THAT HYDROGEN CONTROL MEASURES BEYOND THOSE PRESCRIBED IN 10 CFR SECTION 50.44 ARE REQUIRED FOR ICE CONDENSER PLANTS, BUT THAT A REASONABLE PERIOD OF TIME, I.E., WITHIN A YEAR, MAY BE ALLOWED FOR IMPLEMENTATION OF THESE MEASURES SO THAT THE APPROPRIATE STUDIES AND TESTS CAN BE COMPLETED.

THE STAFF'S BASES FOR THIS VIEW ARE:

. TMI STLL ITEMS HAVE BEEN IMPLEMENTED PLACING SEQUOYAH IN SAME RISK SPACE AS SURRY AND PEACH BOTTOM.

. AGGRESSIVE APPLICANT AND STAFF PROGRAMS ARE IN PLACE TO IMPROVE THE HYDROGEN MANAGEMENT CAPABILITY AT SEQUOYAH (TIME FRAME: 4 MONTHS),

. PRELIMINARY WORK SHOWS THE IDIS TO BE A VERY PROMISING APPROACH,

. BACKUP PROGRAMS ARE IN PLACE, SHOULD THE IDIS PROVE UNACCEPTABLE

. OPERATIONS AT FULL POWER DO NOT FORECLOSE LATER ADOPTION OF ALTERNATIVE MEASURES

Q - 1. DOES THE STAFF BELIEVE ADDITIONAL HYDROGEN CONTROL MEASURES ARE NECESSARY FOR ICE CONDENSER CONTAINMENTS?

A - 1. THE STAFF'S VIEW IS THAT HYDROGEN CONTROL MEASURES BEYOND THOSE PRESCRIBED IN 10 CFR SECTION 50.44 ARE NOT REQUIRED IN THE NEAR TERM FOR FULL POWER OPERATIONS AT ICE CONDENSER PLANTS. BY NEAR TERM WE MEAN WITHIN THE NEXT YEAR.

Q - 2. IS THE STAFF REASONABLY PERSUADED OF THE EFFECTIVENESS OF DISTRIBUTED IGNITERS IN ICE CONDENSER CONTAINMENTS? CAN SUCH IGNITERS BE COUNTED ON TO KEEP PRESSURE INCREASES CAUSED BY HYDROGEN BURNS AT SUITABLY LOW VALUES -- WHICH I WOULD DEFINE AS DESIGN PRESSURES -- DURING ACCIDENT SEQUENCES INVOLVING TMI-LIKE QUANTITIES OF HYDROGEN?

A - 2. THE STAFF'S VIEW IS THAT THE DISTRIBUTED IGNITER SYSTEM APPEARS VERY PROMISING AS AN ADDITIONAL HYDROGEN CONTROL MEASURE. ANALYSES WITH THE CLASIX AND MARCH CODES HAVE SHOWN THAT THE SYSTEM CAN SUBSTANTIALLY IMPROVE THE CAPABILITY OF AN ICE CONDENSER CONTAINMENT TO ACCOMMODATE THE HYDROGEN RELEASES FROM A DEGRADED CORE ACCIDENT.

HOWEVER, FURTHER ANALYSES AND TESTS NEEDS TO BE CONDUCTED TO DETERMINE ITS RANGE OF EFFICACY AND TO ASSURE THAT OVERALL SAFETY IS NOT DEGRADED. THIS WORK IS NOW UNDERWAY AND SHOULD BE COMPLETED BY DECEMBER 1980.

PENDING THE RESULTS OF FURTHER WORK, THE STAFF'S VIEW IS THAT USE OF AN IGNITER SYSTEM DURING ACCIDENT SEQUENCES INVOLVING TMI-LIKE QUANTITIES OF HYDROGEN WOULD LEAD TO CONTAINMENT PRESSURES THAT EXCEED DESIGN PRESSURES BUT THAT ARE LESS THAN CONTAINMENT FAILURE OR YIELD PRESSURES.

WE FIND THESE FEATURES ACCEPTABLE PENDING THE RULEMAKING PROCEEDING ON MUCH THE SAME BASES INDICATED FOR THE FIRST QUESTION.

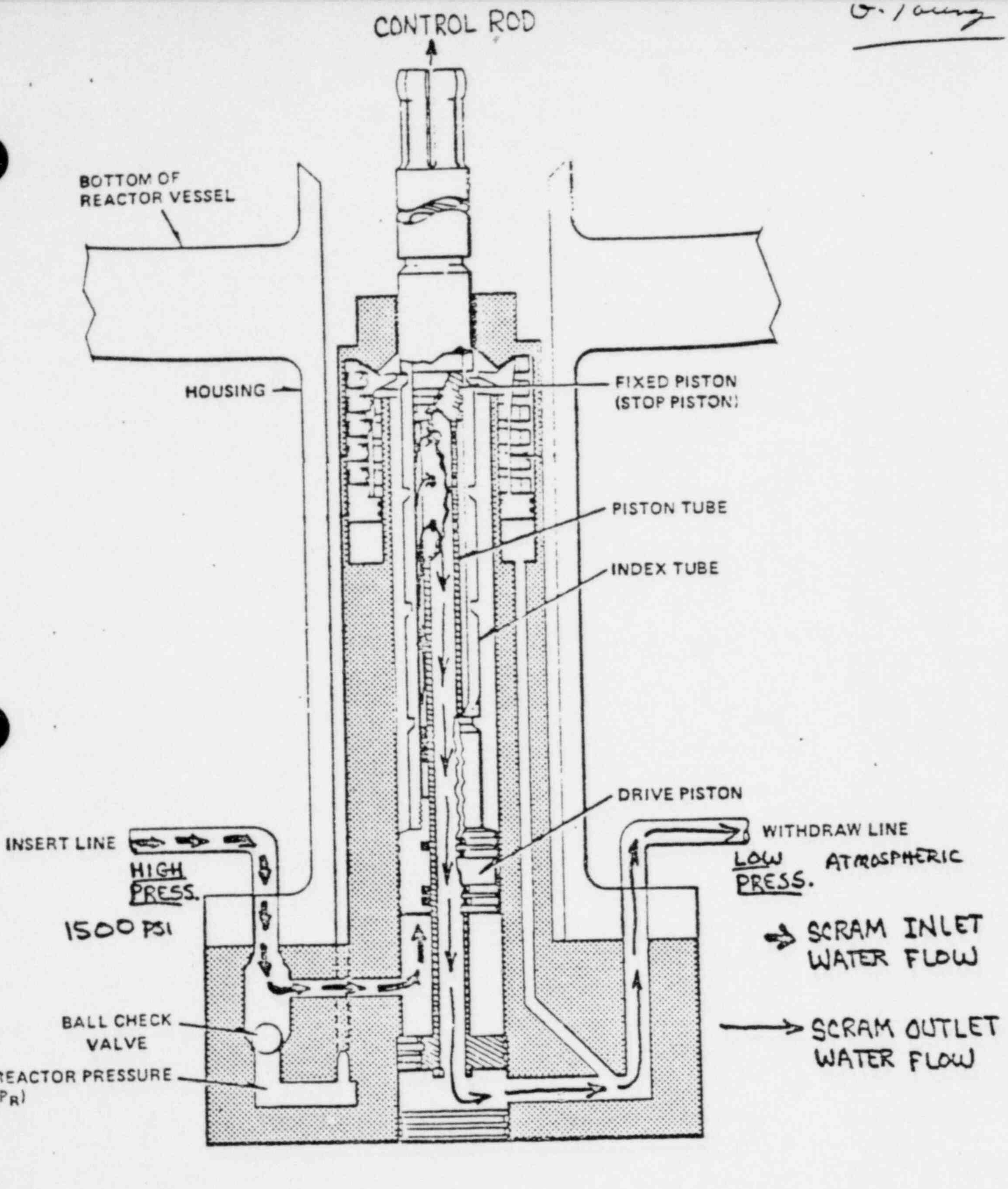


FIGURE 2.2 BWR CONTROL ROD DRIVE MECHANISM (SIMPLIFIED SKETCH)

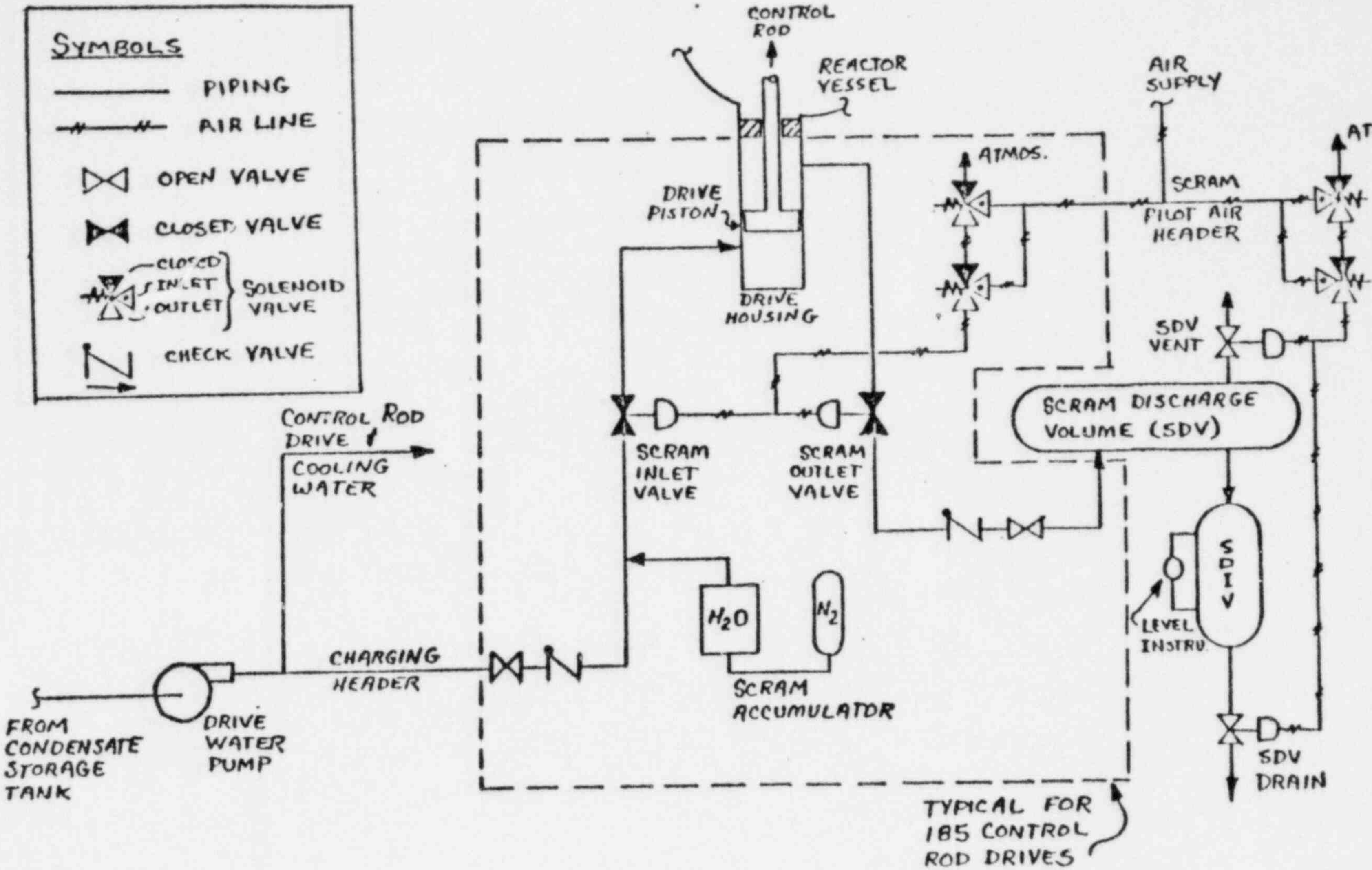


FIGURE 2.1 BWR SCRAM HYDRAULIC SYSTEM (NORMAL VALVE LINEUP, PRE-SCRAM)

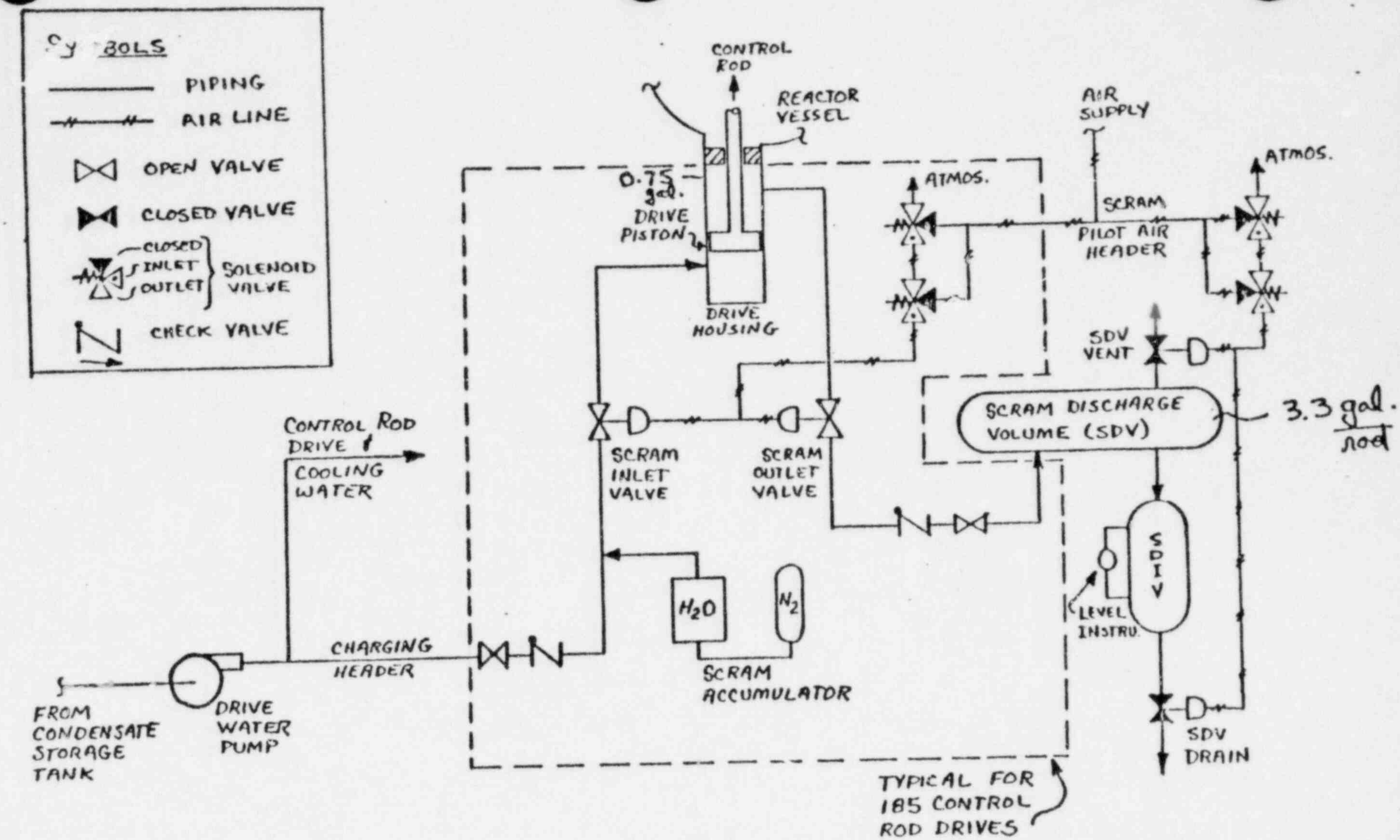


FIGURE 2.3 BWR SCRAM HYDRAULIC SYSTEM (SCRAMMED VALVE LINEUP)

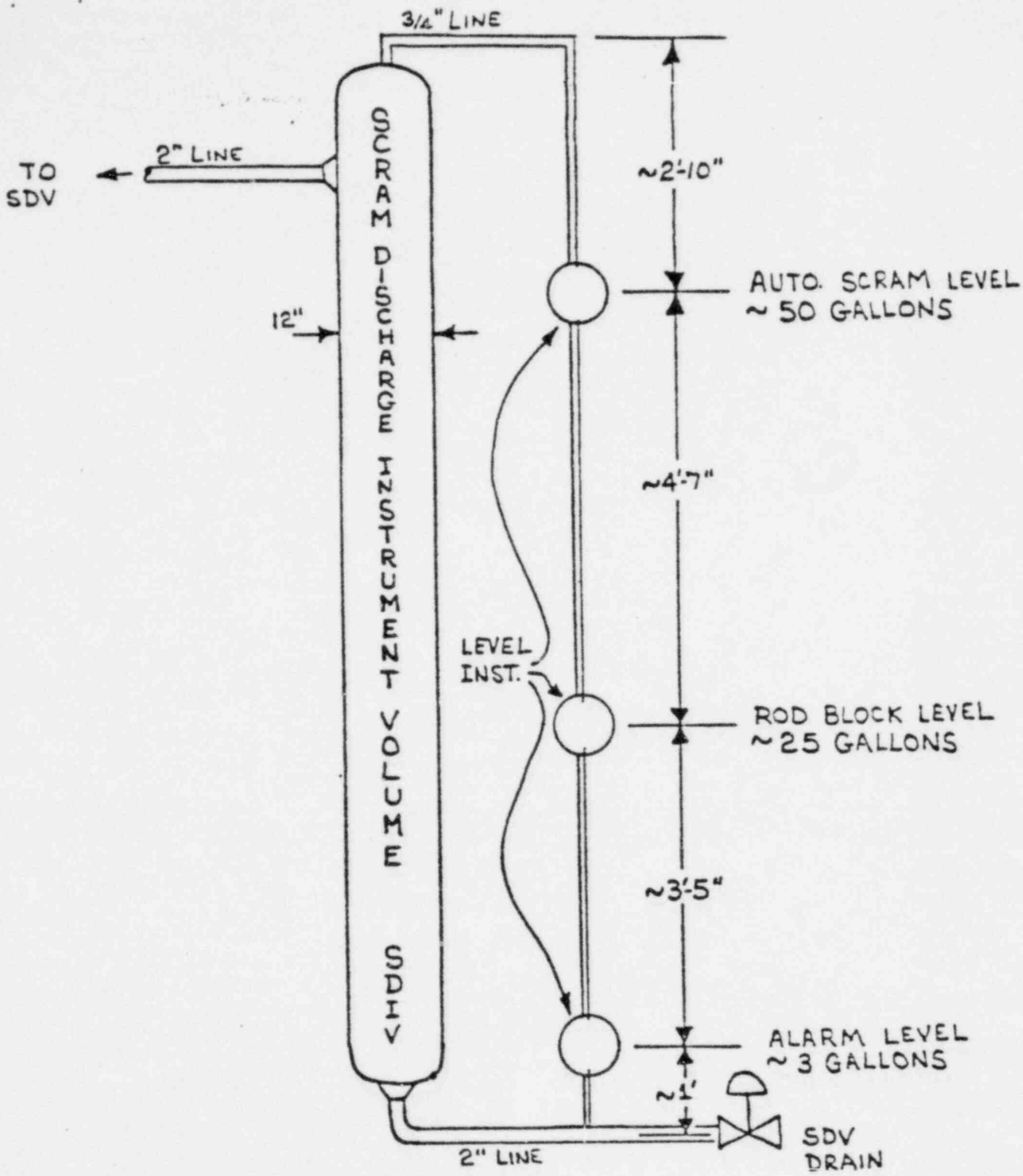


FIGURE 2.4 BROWNS FERRY SCRAM DISCHARGE INSTRUMENT VOLUME

(93 RODS) EAST | WEST (92 RODS)

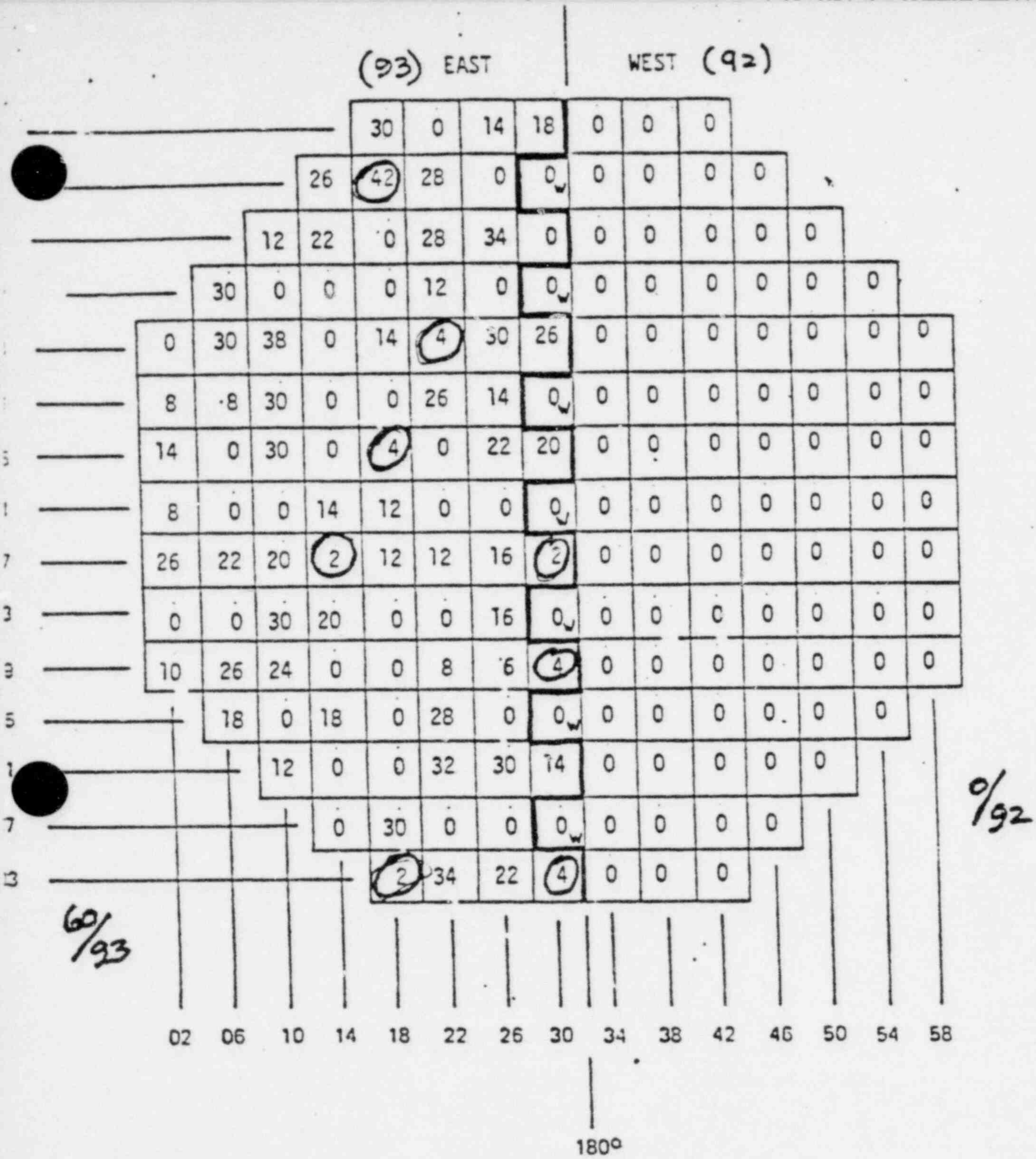
02	06	10	14	18	22	26	30	34	38	42	46	50	54	58
				40	4	36	42	0	0	0				
			36	46	38	2	0_w	0	0	0	0			
		36	34	0	36	42	10	0	0	0	0	0		
	42	0	20	0	24	0	0_w	0	0	0	0	0	0	
12	40	44	0	30	28	40	38	0	0	0	0	0	0	0
24	0	40	0	0	34	32	0_w	0	0	0	0	0	0	0
30	4	38	0	18	8	34	30	0	0	0	0	0	0	0
22	10	0	34	28	2	0	0_w	0	0	0	0	0	0	0
42	36	30	14	26	28	36	24	0	0	0	0	0	0	0
8	0	38	32	0	12	36	2_w	0	0	0	0	0	0	0
34	42	34	20	10	28	22	26	0	0	0	0	0	0	0
	40	0	36	0	38	0	0_w	0	0	0	0	0	0	
		26	6	14	40	40	24	0	0	0	0	0		
			0	40	2	0	0_w	0	0	0	0			
				28	42	42	30	0	0	0				

75/93 1/92

180°

Control Rod Scram Group Assignment

Figure 3.1 Control Rod Positions After First Scram



Control Rod Scram Group Assignment

Figure 3.2 Control Rod Positions After Second Scram

(93) EAST | WEST (92)

	02	06	10	14	18	22	26	30	34	38	42	46	50	54	58
					24	0	0	0	0	0	0				
				20	40	22	0	0w	0	0	0	0			
			0	16	0	24	30	0	0	0	0	0	0		
		26	0	0	0	6	0	0w	0	0	0	0	0	0	
	0	24	36	0	2	0	24	18	0	0	0	0	0	0	0
	2	0	24	0	0	20	0	0w	0	0	0	0	0	0	0
	6	0	26	0	0	0	14	16	0	0	0	0	0	0	0
	0	0	0	2	4	0	0	0w	0	0	0	0	0	0	0
	18	16	14	2	4	6	2	0	0	0	0	0	0	0	0
	0	0	26	12	0	0	2	0w	0	0	0	0	0	0	0
	0	16	18	0	0	0	2	2	0	0	0	0	0	0	0
15		4	0	10	0	0	0	0w	0	0	0	0	0	0	
			6	0	0	28	26	8	0	0	0	0	0		
07				0	24	0	0	0w	0	0	0	0			
03					0	30	10	0	0	0	0				

46/93

0/92

180°

Control Rod Scram Group Assignment

Figure 3.3 Control Rod Positions After Third Scram

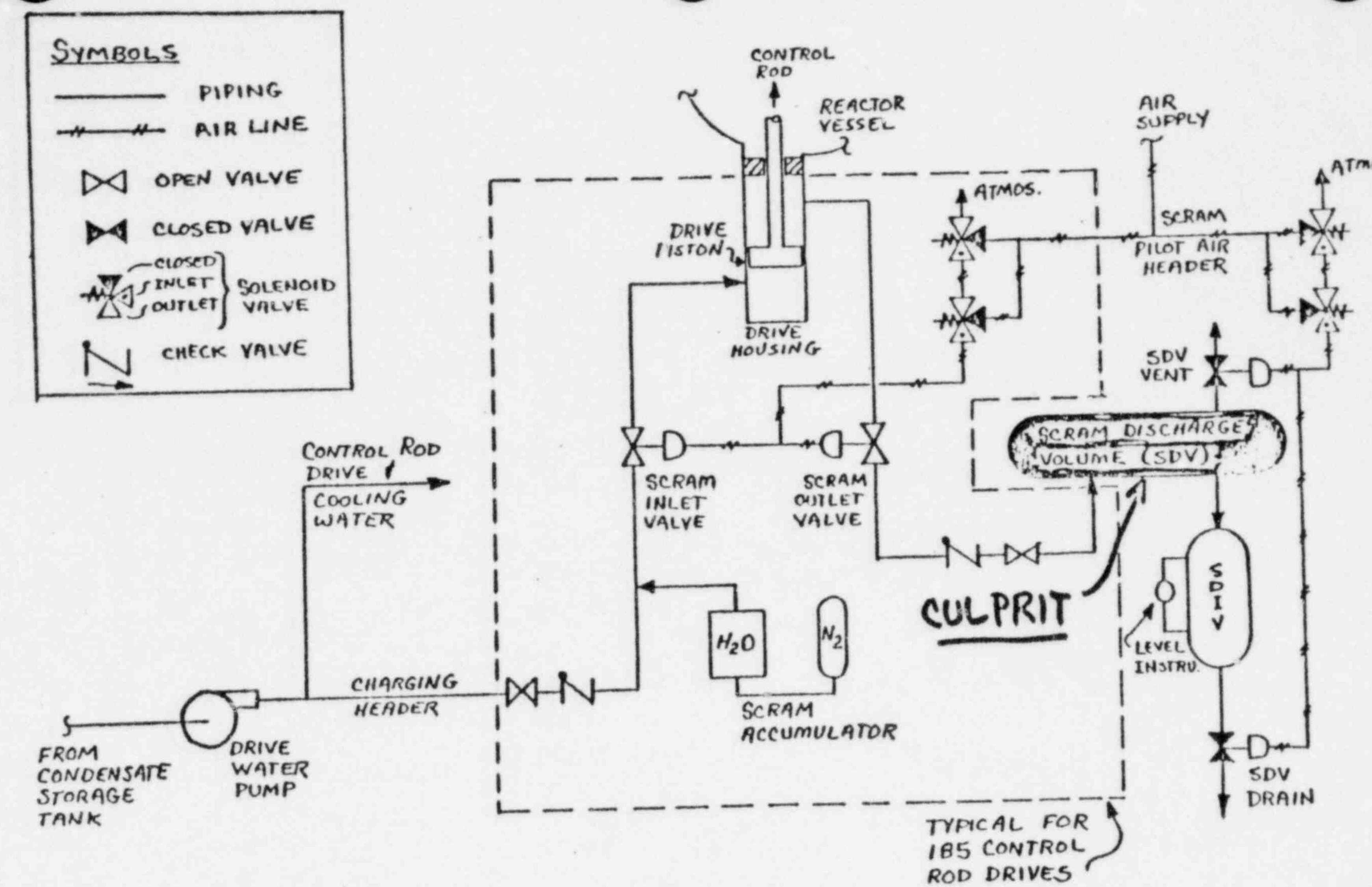
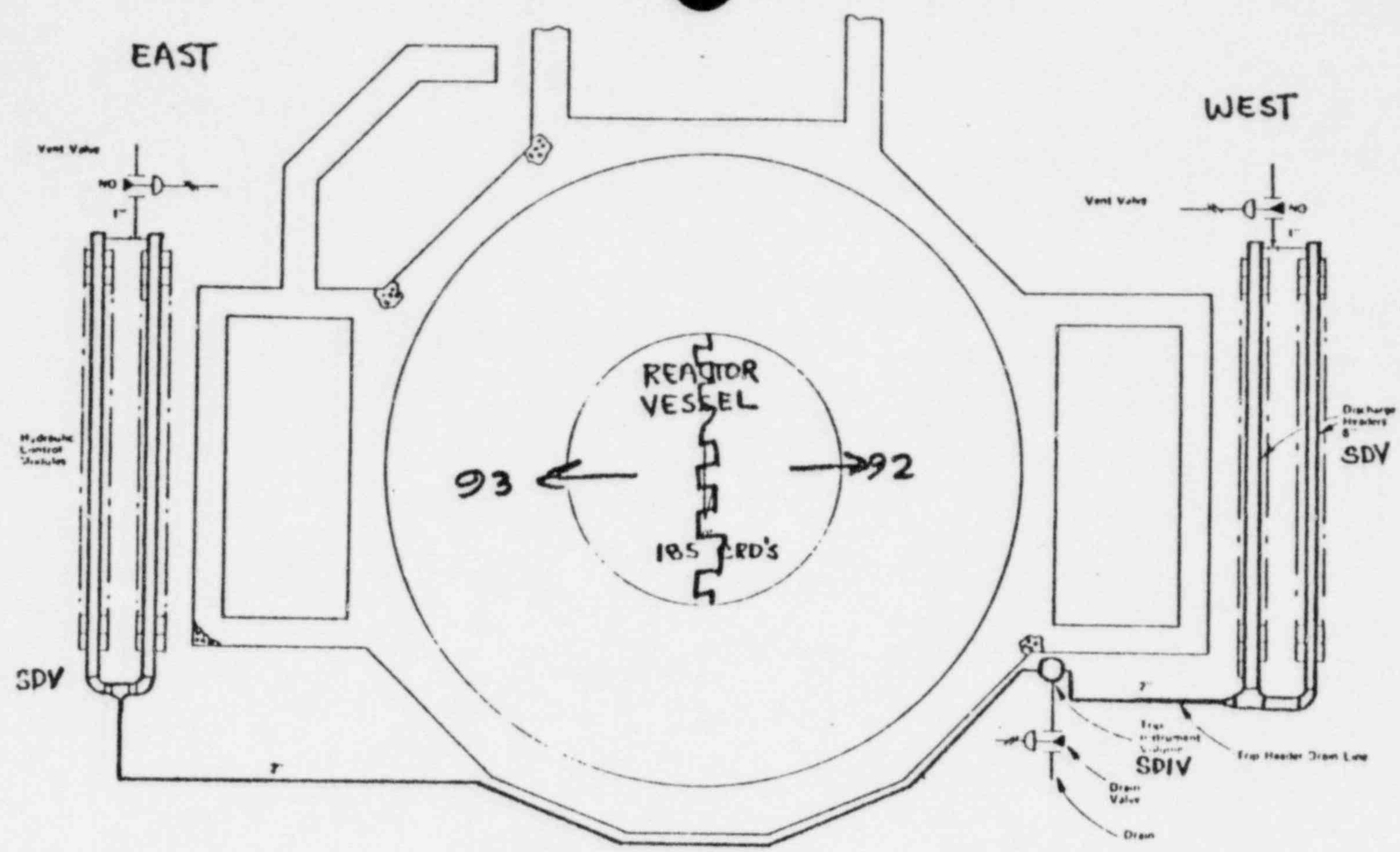


FIGURE 2.3 BWR SCRAM HYDRAULIC SYSTEM (SCRAMMED VALVE LINEUP)



PEACH BOTTOM Reactor Protection System Trip Discharge Headers

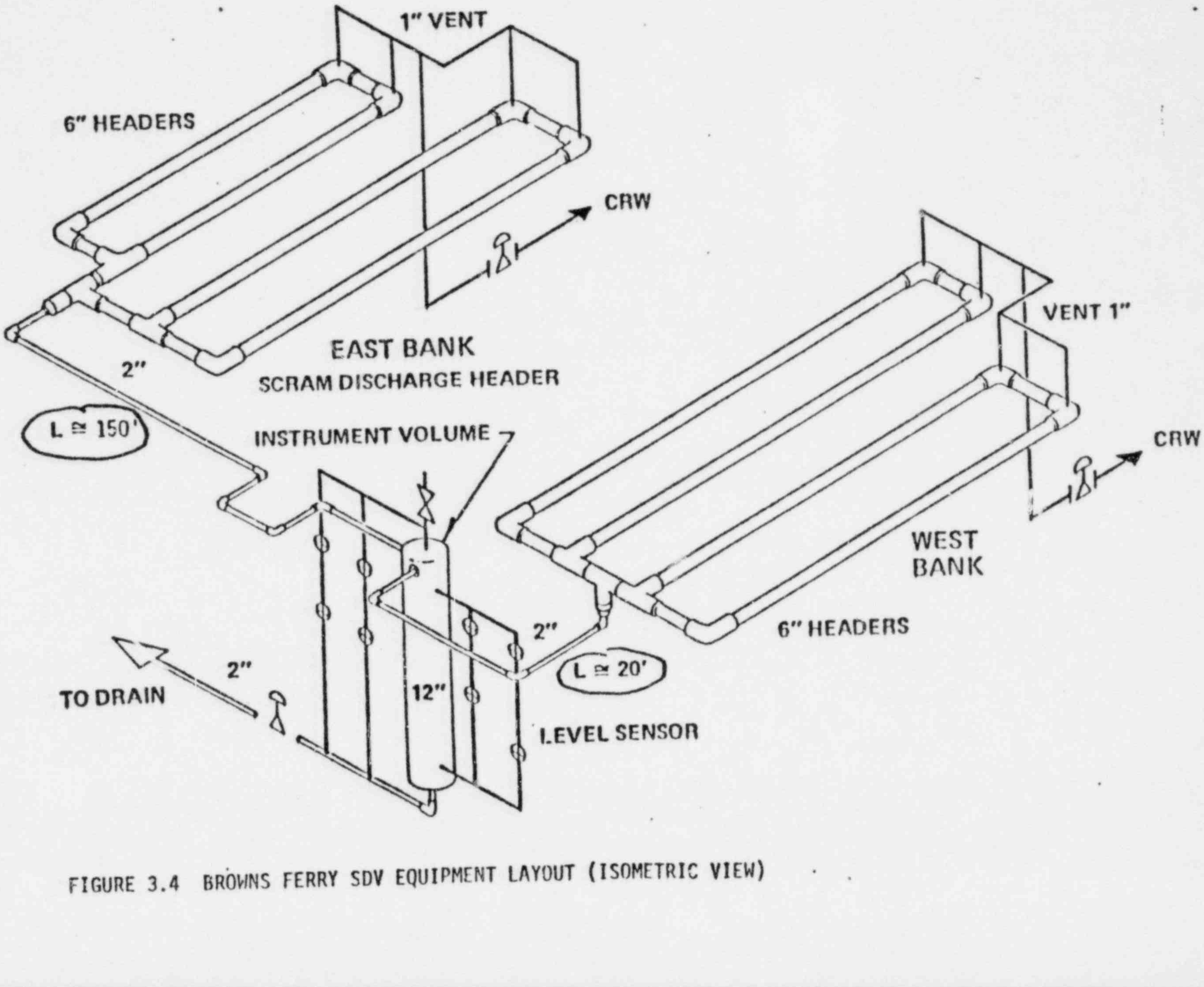


FIGURE 3.4 BROWNS FERRY SDV EQUIPMENT LAYOUT (ISOMETRIC VIEW)

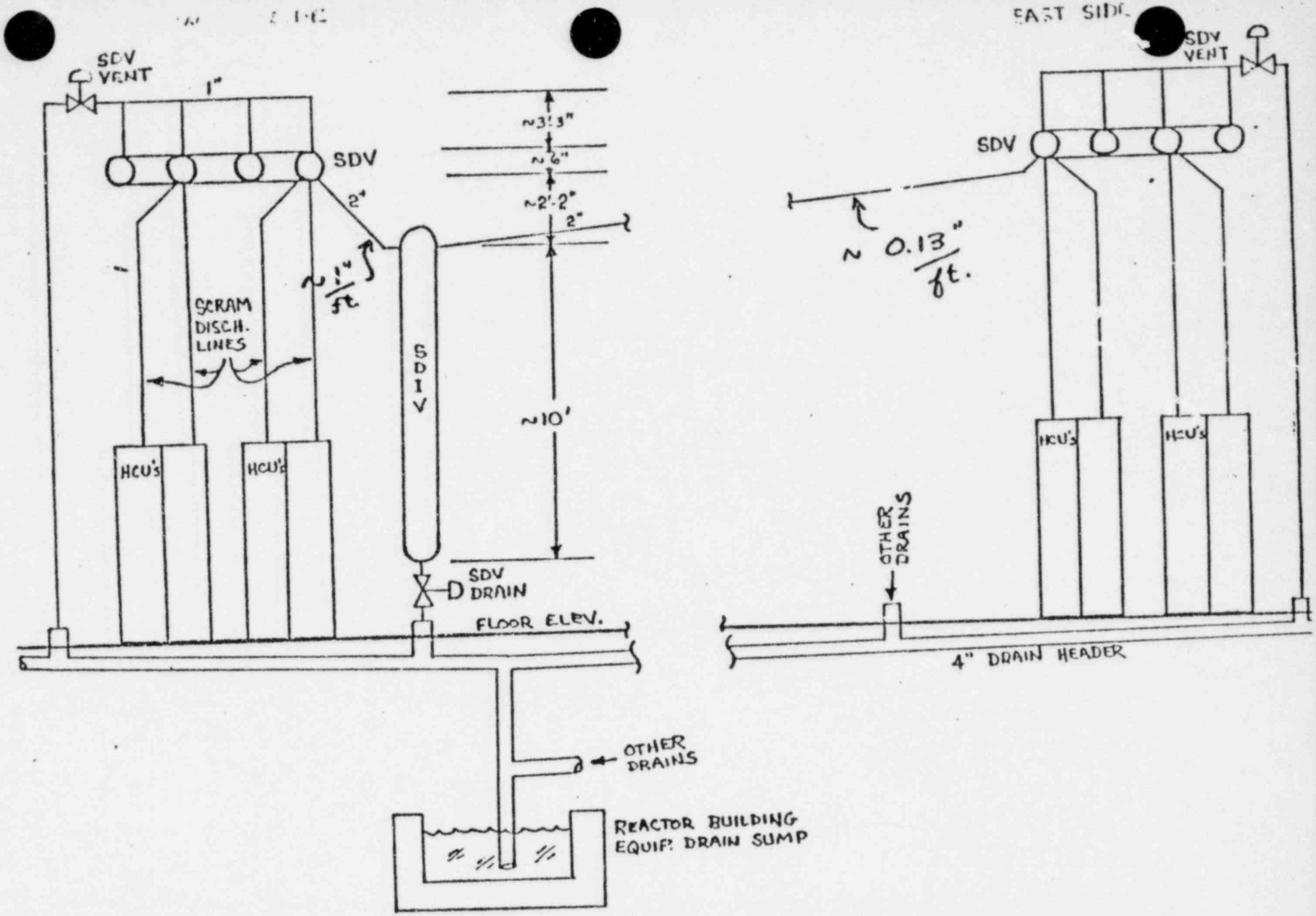


FIGURE 3.5 SKETCH OF BROWNS FERRY SDV EQUIPMENT LAYOUT (ELEVATION)

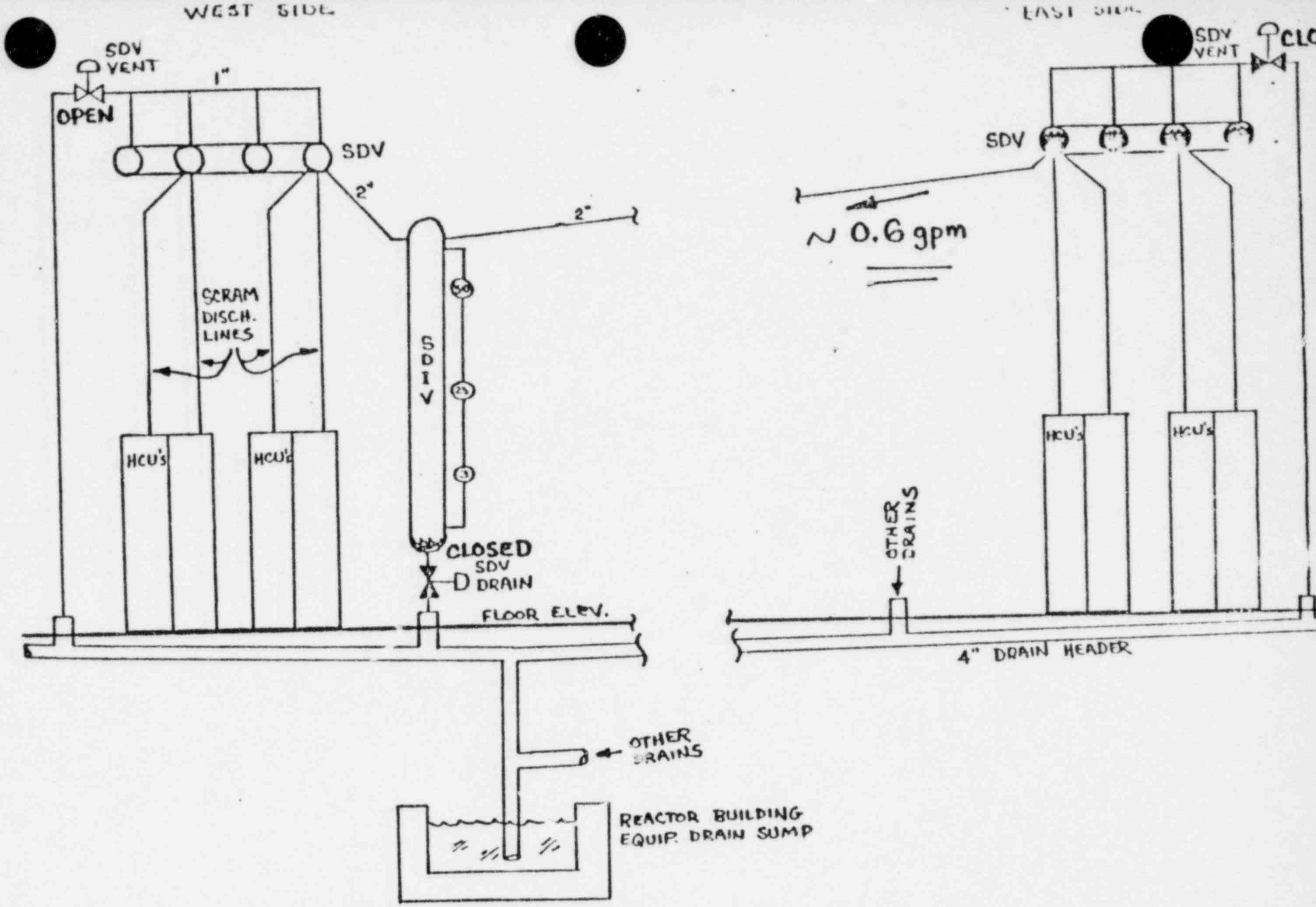


FIGURE 3.6 SIMULATED EAST SDV VENT LINE BLOCKAGE TEST

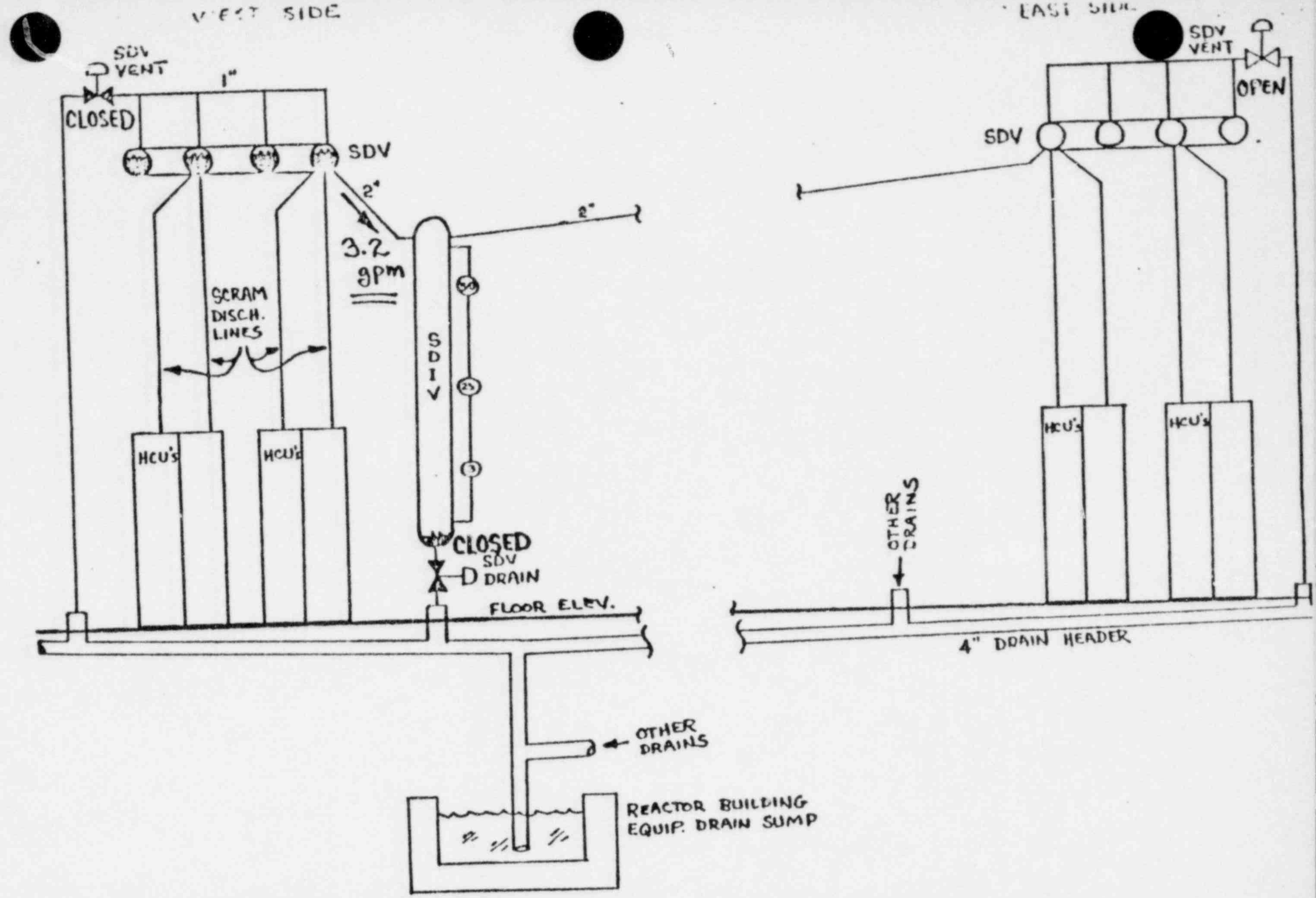


FIGURE 3.7 SIMULATED WEST SDV VENT LINE BLOCKAGE TEST

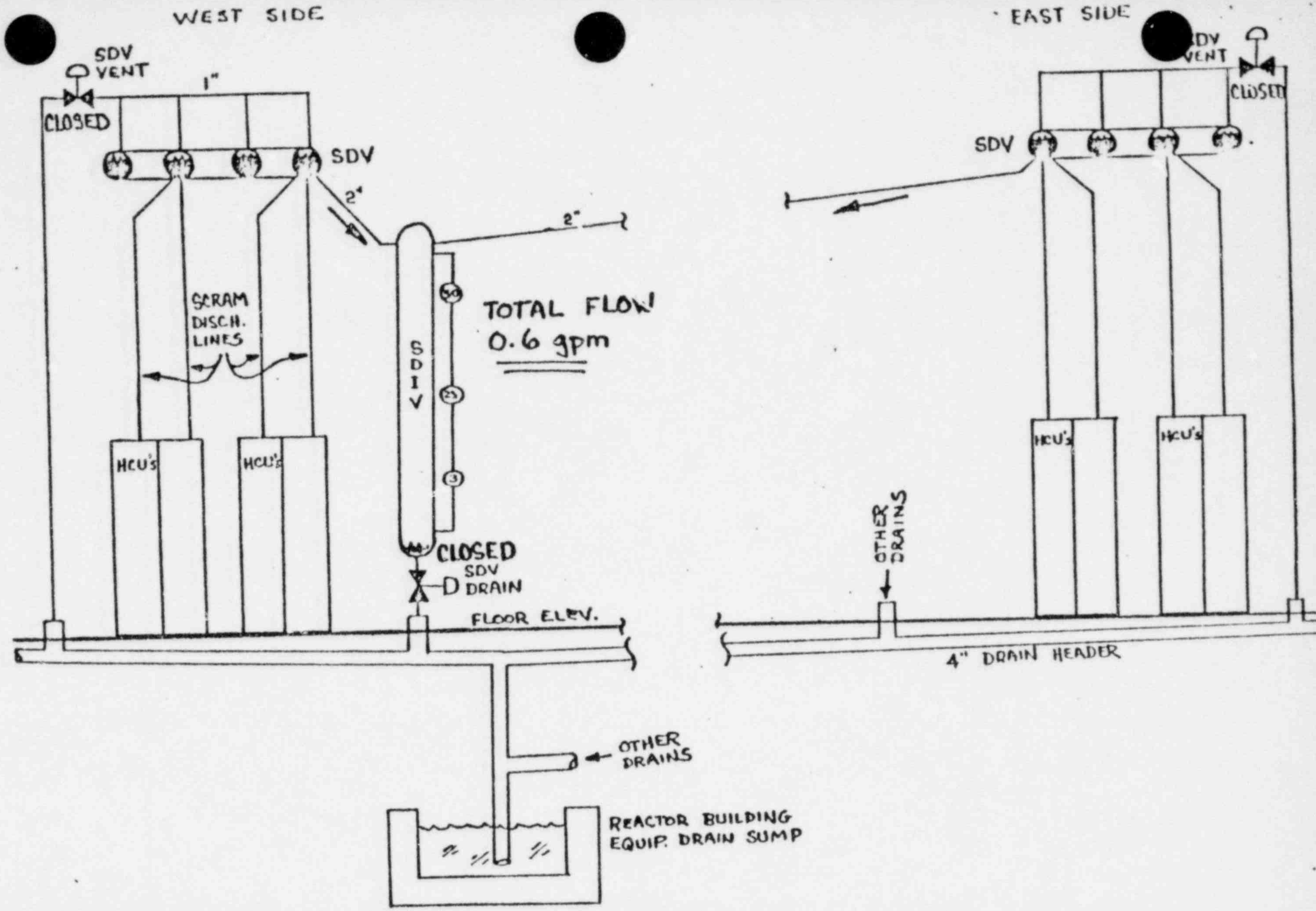


FIGURE 3.8 SIMULATED EAST AND WEST SDV VENT LINE BLOCKAGE TEST

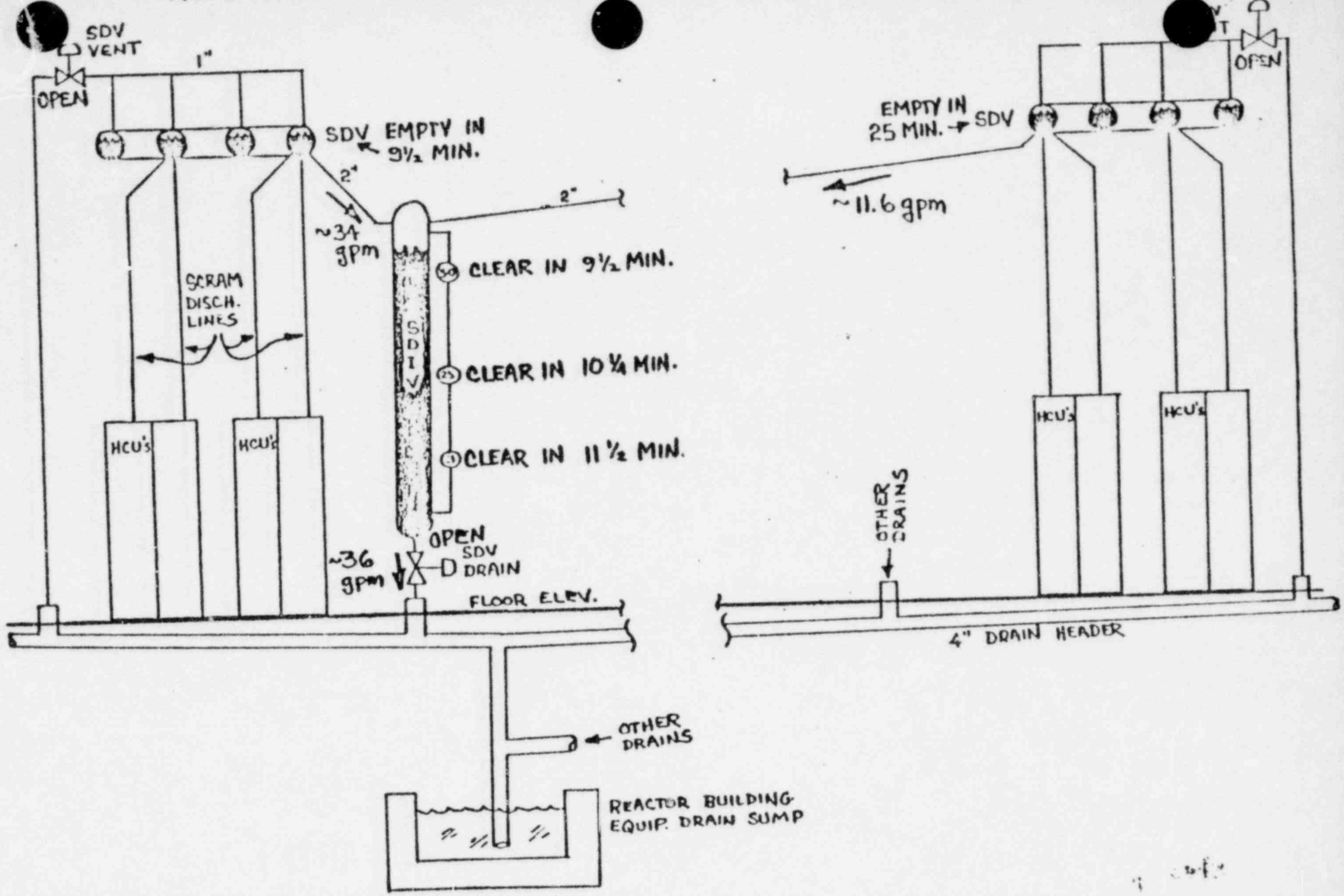
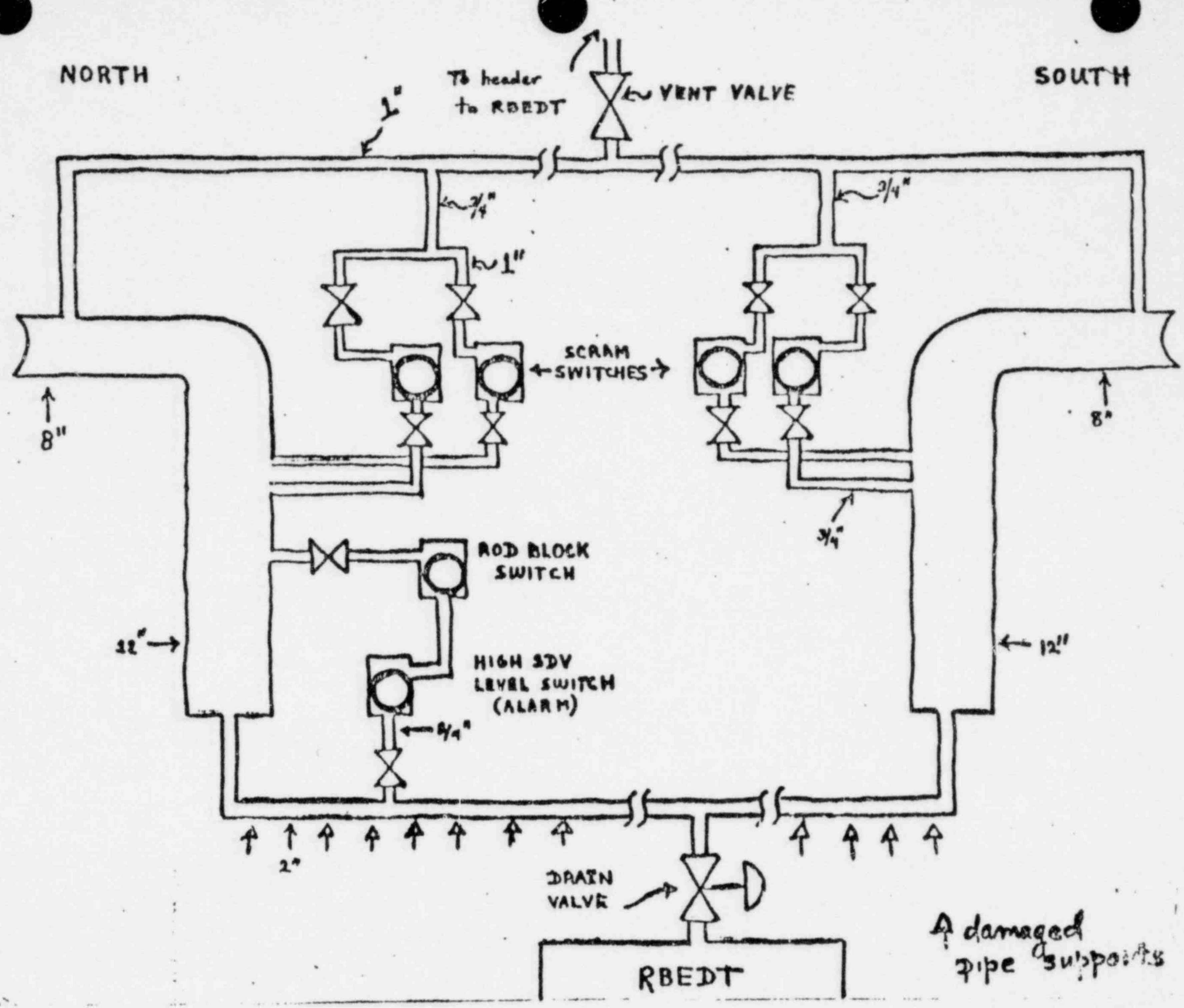
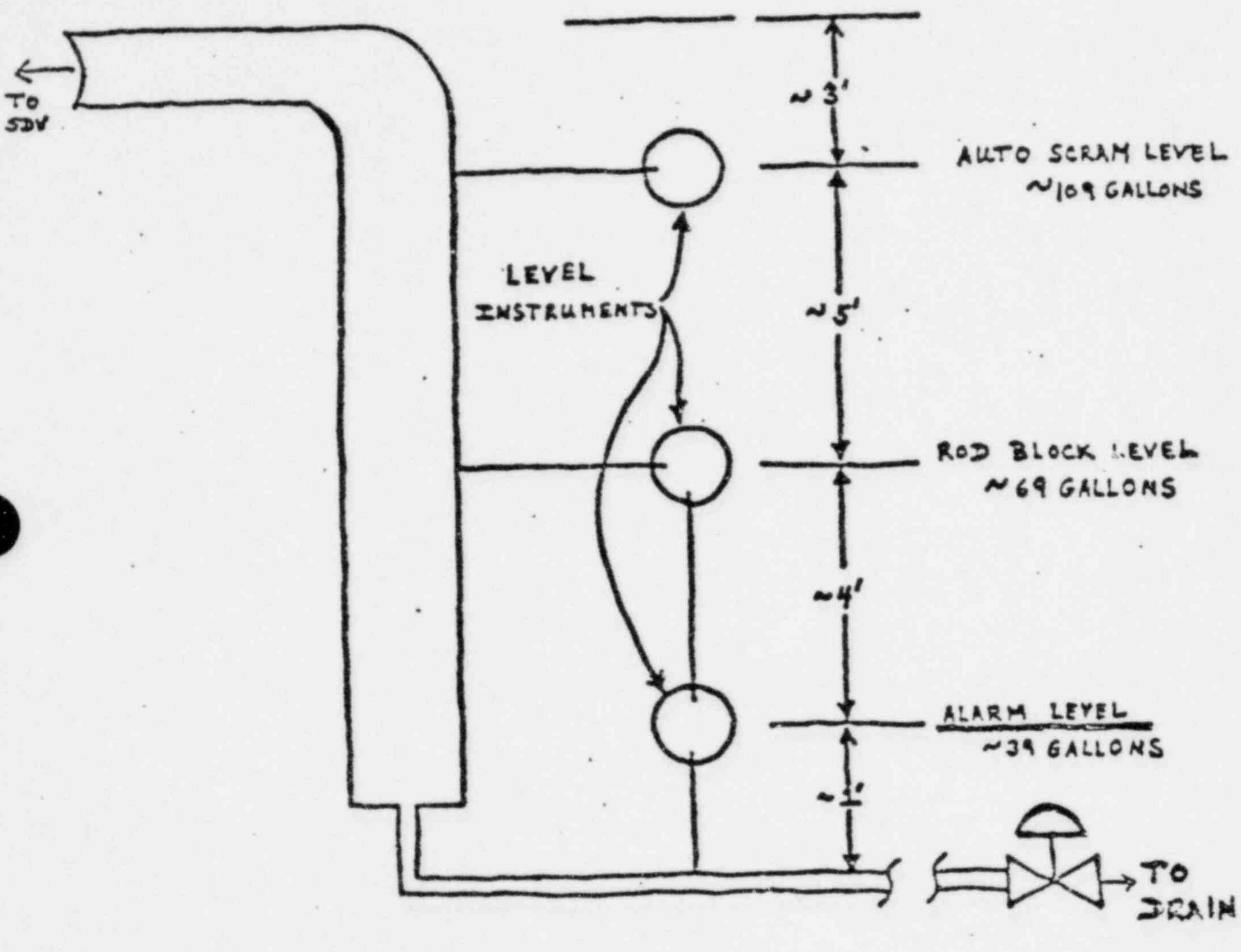


FIGURE 3.9 NORMAL DRAIN TEST

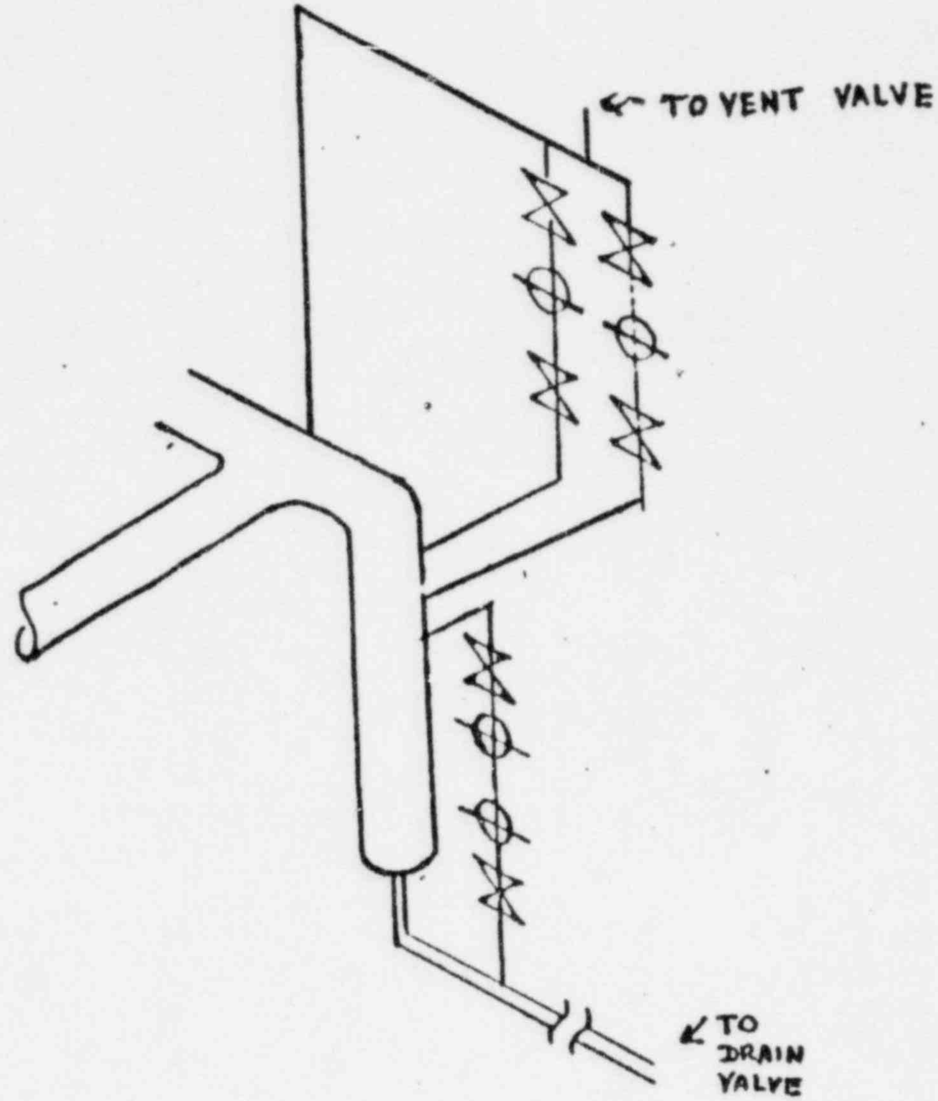
1

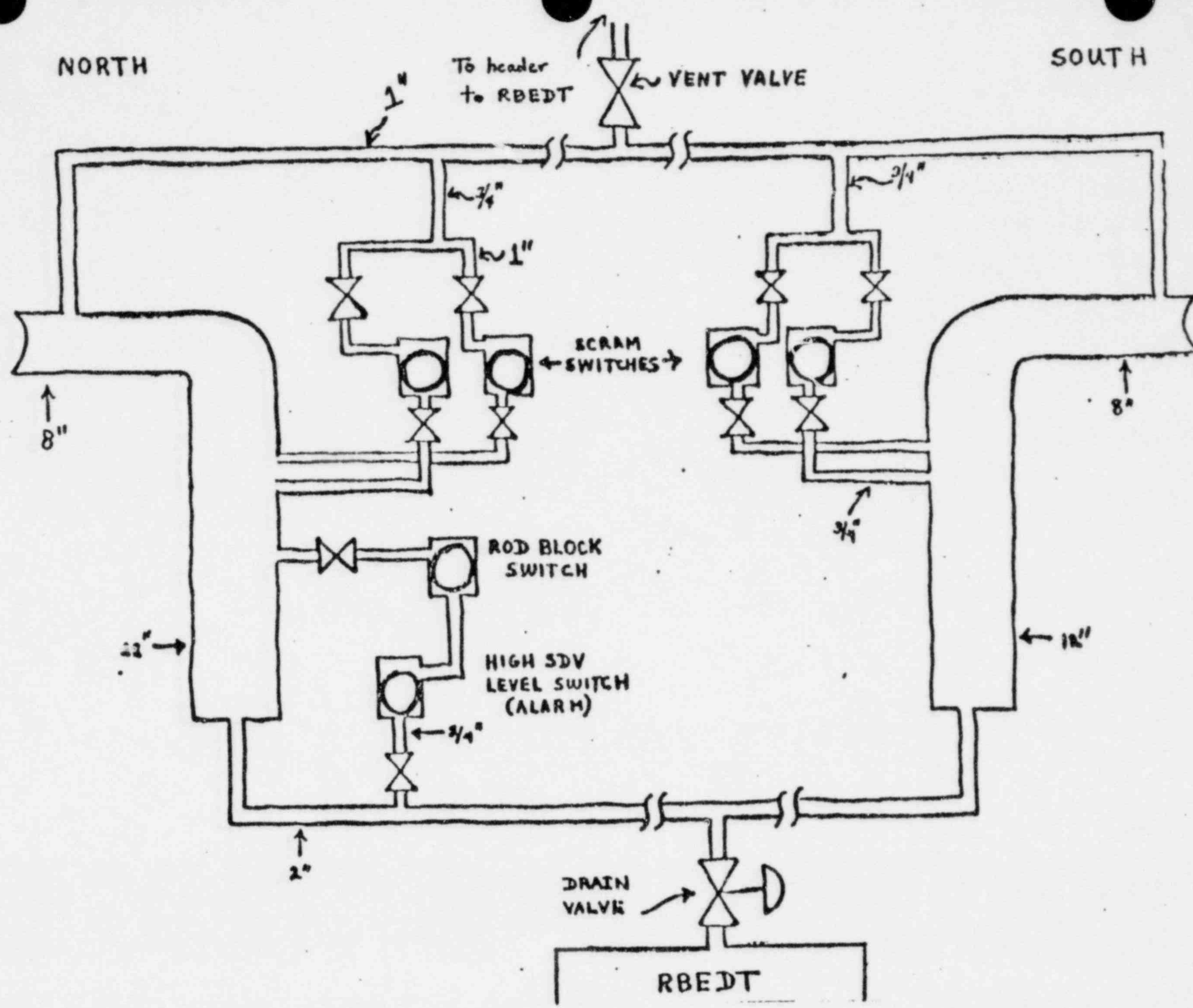

NORTH
SOUTH
To header to RBEDT
VENT VALVE
1"
3/4"
1"
3/4"
SCRAM SWITCHES
8"
8"
3/4"
ROD BLOCK SWITCH
12"
12"
HIGH SDV LEVEL SWITCH (ALARM)
3/4"
2"
DRAIN VALVE
RBEDT
damaged pipe supports

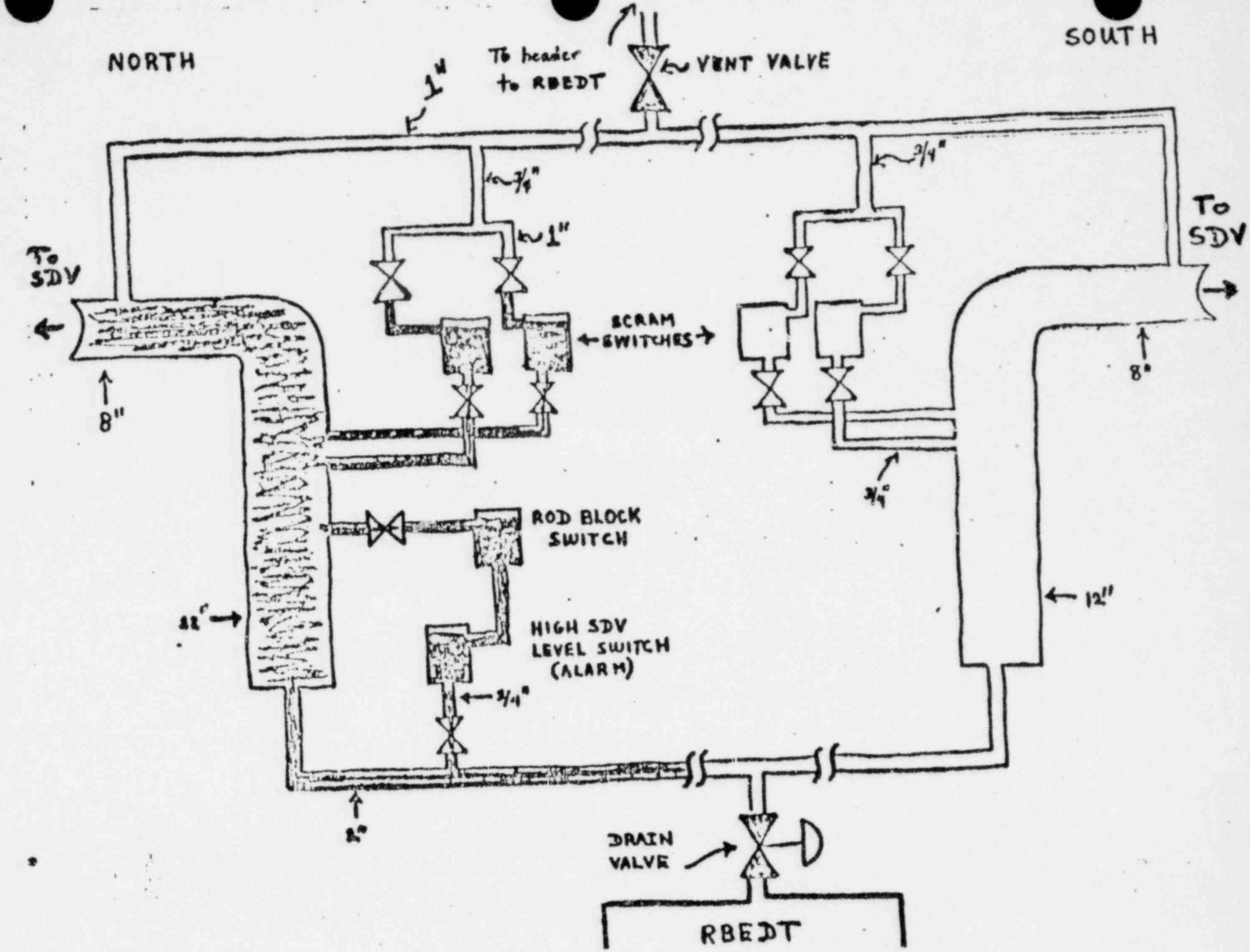


TO
SDV
~3'
AUTO SCRAM LEVEL
~109 GALLONS
LEVEL
INSTRUMENTS
~5'
ROD BLOCK LEVEL
~69 GALLONS
~4'
ALARM LEVEL
~39 GALLONS
~1'
TO
DRAIN

3

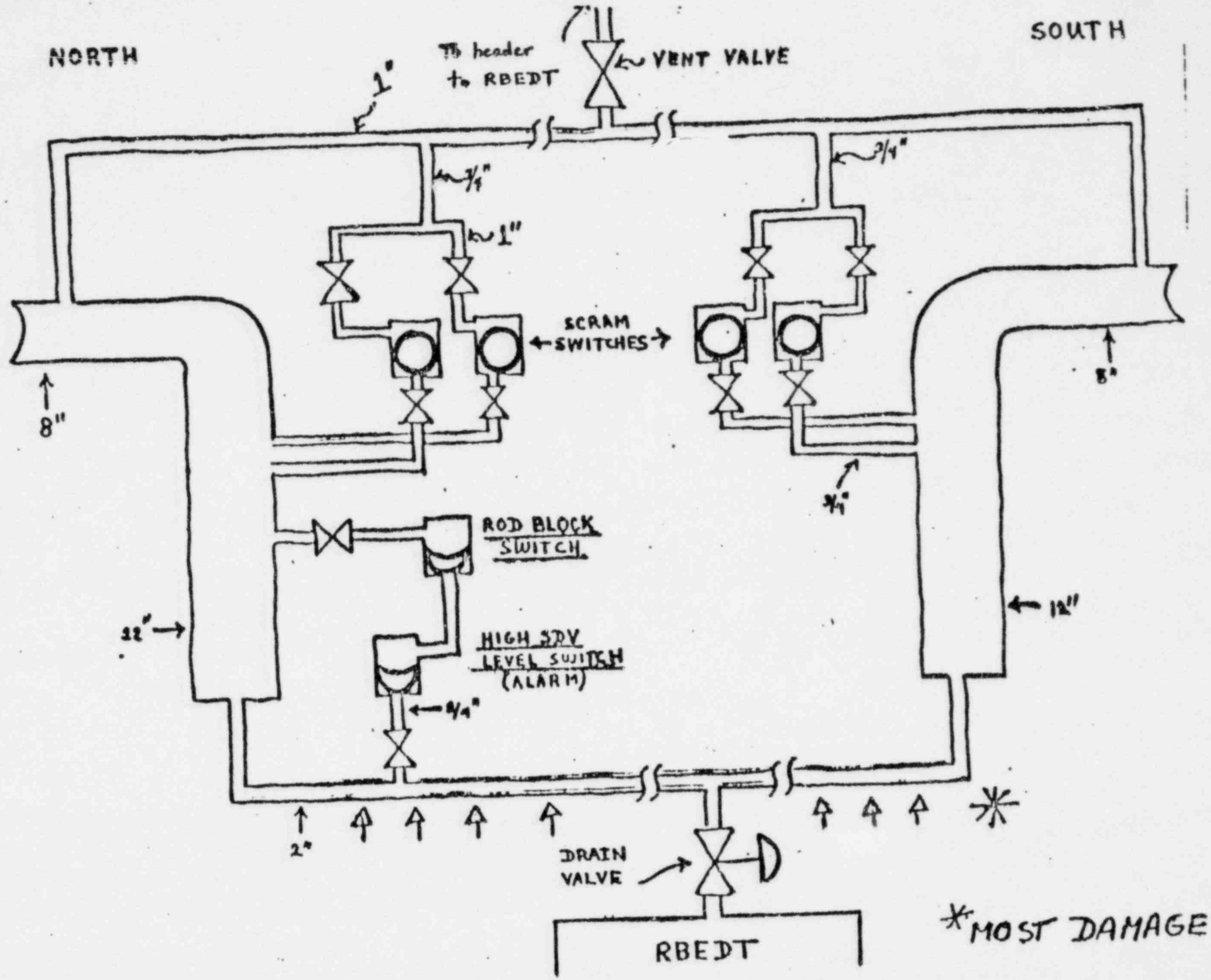



NORTH
SOUTH
To header to RBEDT
VENT VALVE
1"
3/4"
1"
3/4"
SCRAM SWITCHES
8"
8"
3/4"
ROD BLOCK SWITCH
22"
12"
HIGH SDV LEVEL SWITCH (ALARM)
3/4"
2"
DRAIN VALVE
RBEDT



NORTH
SOUTH
To header
to RBEDT
VENT VALVE
1"
3/4"
1"
To
SDV
8"
SCRAM
SWITCHES
3/4"
ROD BLOCK
SWITCH
HIGH SDV
LEVEL SWITCH
(ALARM)
3/4"
12"
DRAIN
VALVE
RBEDT

6.


NORTH
SOUTH
To header
to RBEDT
VENT VALVE
1"
3/4"
1"
3/4"
SCRAM
SWITCHES
8"
8"
3/4"
ROD BLOCK
SWITCH
HIGH SDV
LEVEL SWITCH
(ALARM)
12"
12"
3/4"
2"
DRAIN
VALVE
RBEDT
*MOST DAMAGE

AEOD FINDINGS

1. The cause of the partial scram failure was water accumulation in the east SDV.
2. The SDIV "High Water Level Trip" did not and does not provide protection against filling the east SDV even for normal venting and draining conditions.
3. A single failure (e.g., west side SDV vent or drain line blockage) can completely disable the SDIV "High Water Level Trip" installed to protect against loss of scram capability for the control rods.
4. With the present SDV/SDIV layout, a single failure (blockage) of an SDV vent or drain path can cause a partial loss of scram capability.
5. There are numerous actual and potential mechanisms for introducing and accumulating water in the SDV's with no accumulation in the SDIV.
6. The current SDV/SDIV layout results in the automatic "Hi Water Level Trip" safety function being directly dependent on the non-safety related reactor building waste drain system.
7. The float-type water level monitoring instruments on the SDIV have a significant degree of unreliability.
8. The current BWR RPS logic does not allow scram reset to attempt a re-scram if certain automatic scram signals are present.
9. Failure to close of a single SDV vent or drain valve during a reactor scram can result in a unisolatable release of reactor coolant outside the primary containment into the secondary containment.
10. The emergency operating instructions at Browns Ferry did not cover a partial or total scram failure event.

AEOD RECOMMENDATIONS

1. The Operability of the SDIV "Hi Water Level Trip" should be independent of the venting and draining requirements.

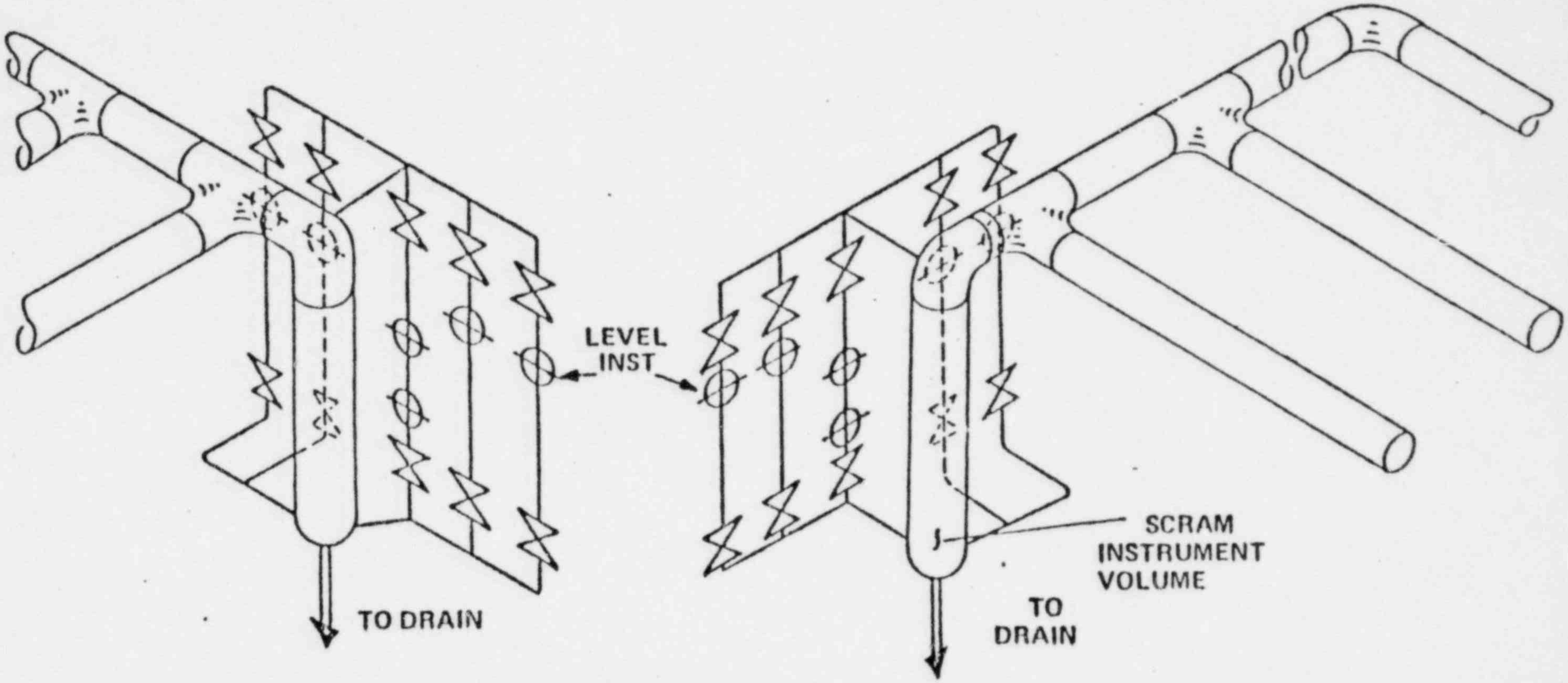
2. SDIV instruments should be both redundant and diverse.

3. All vent and drain paths from the SDV should have redundant automatic isolation valves.

4. Emergency operating procedures and operator training should be provided for complete and partial scram failure conditions.

5. Consider modifying the SDV vent and drain arrangement to improve drain reliability.

GE RECOMMENDED SDV CONFIGURATION



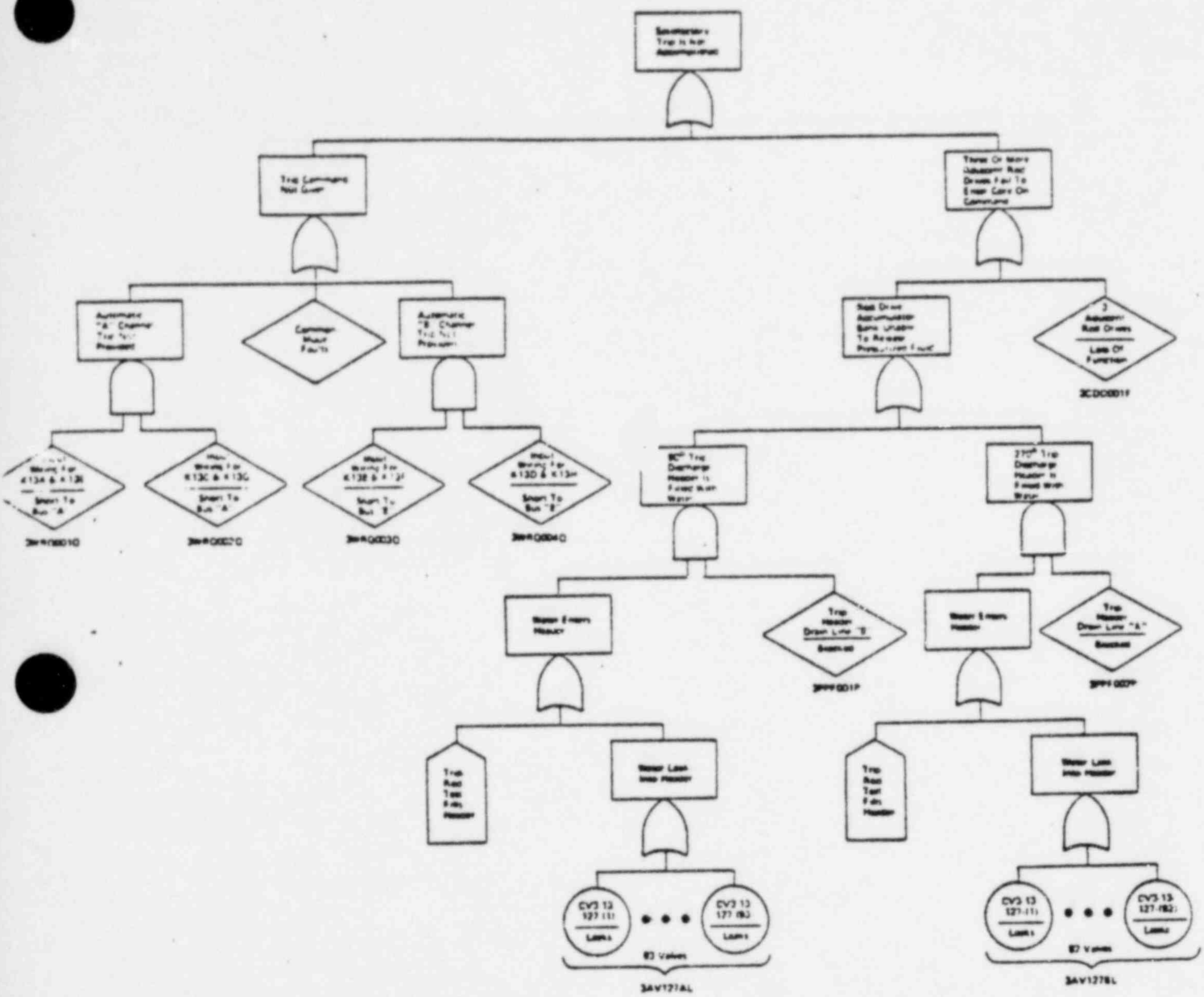


FIGURE II 6-16 Reactor Protection System Reduced Fault Tree

APPENDIX II - WASH-1400

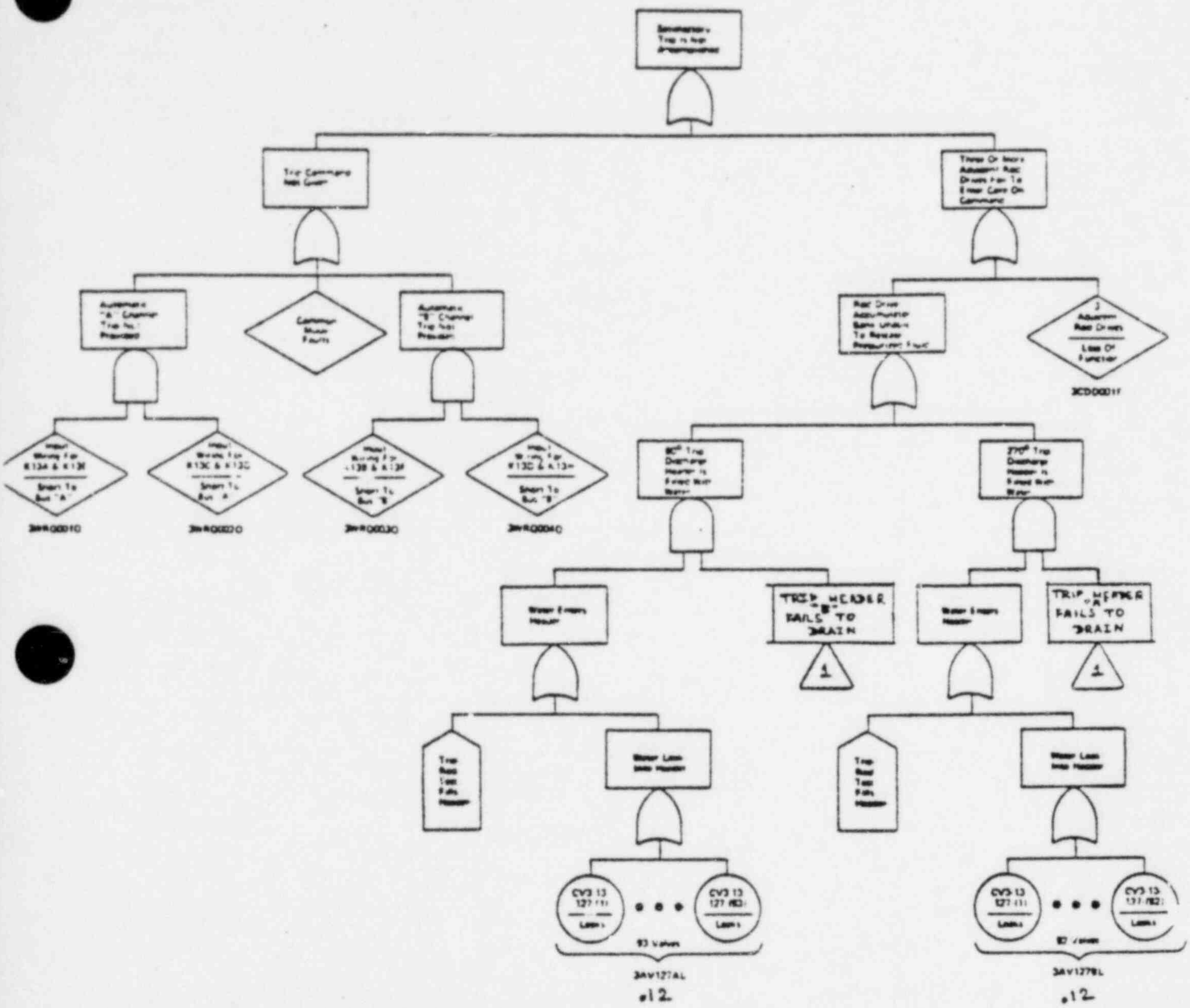
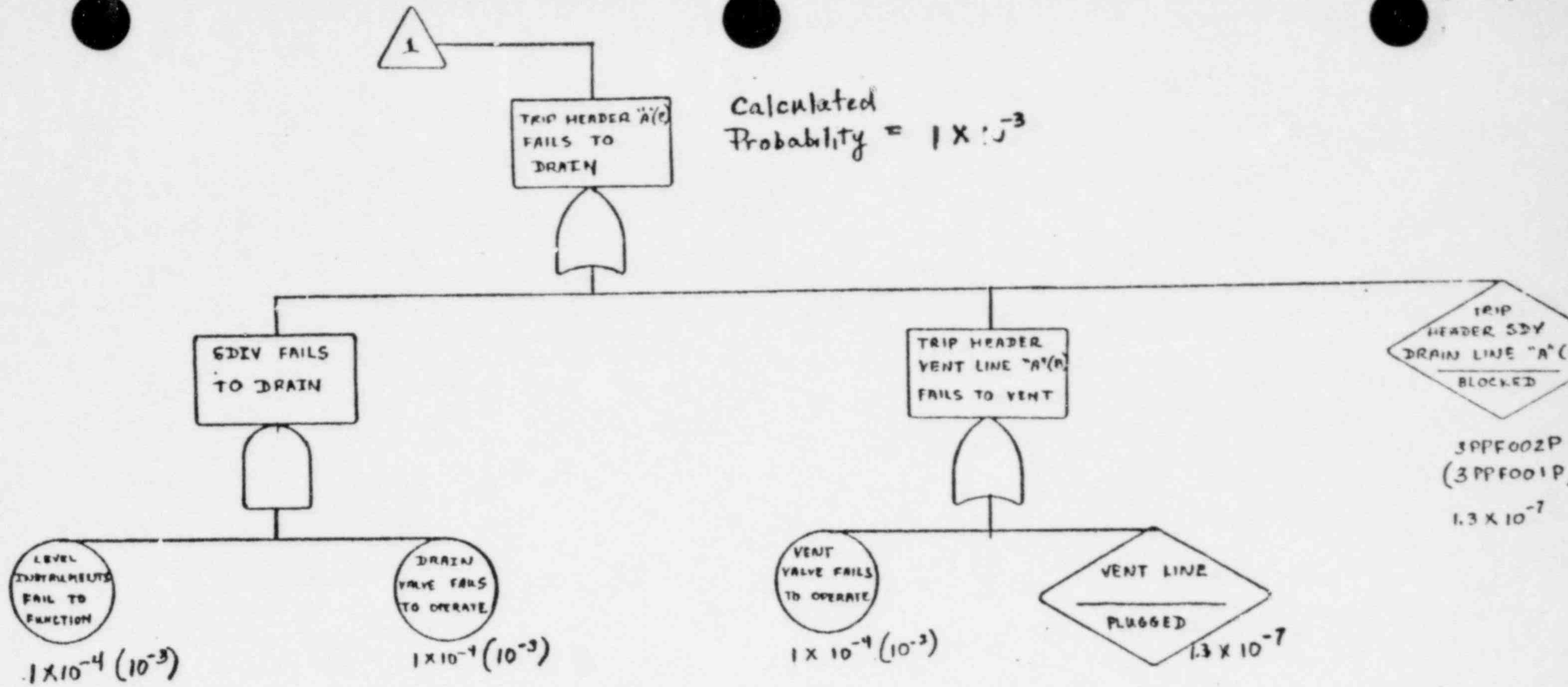


FIGURE II 6-16 Reactor Protection System Reduced Fault Tree

Adapted from Appendix II WASH-1400



Calculation: $(1\times10^{-3})(1\times10^{-3}) + [1\times10^{-3} + 1.3\times10^{-7}] + 1.3\times10^{-7} = 1\times10^{-3}$ Upper boun

$(1\times10^{-4})(1\times10^{-4}) + [1\times10^{-4} + 1.3\times10^{-7}] + 1.3\times10^{-7} = 1\times10^{-4}$ Lower Pr

BWR HYDRAULIC SCRAM SYSTEM

NRC STAFF REPORT

1. BROWNS FERRY EVENT AND ASSOCIATED EVENTS - W. MILLS.

2. NRC SHORT TERM RESPONSE - W. MILLS.

3. NRC REGIONAL MEETINGS - V. PANCIERA.

4. PLAN OF ACTION TO RESOLVE PROBLEM - V. PANCIERA.

5. RESULTS OF ACCIDENT ANALYSES - M. MENDONCA.

6. IMPLICATIONS FOR ATWS - A. THADANI

NRC REGIONAL MEETING

OBJECTIVE - IN-DEPTH UNDERSTANDING OF AS-BUILT CONDITIONS IN SDV INSTRUMENTED VOLUME, INTERCONNECTING PIPING AND VENT & DRAIN SYSTEMS

1. GENERAL AREAS COVERED

   A. SYSTEM CONFIGURATIONS
      (A) GENERAL LAYOUT
      (B) SYSTEM DESIGN REQU.REMENT
      (C) SYSTEM INTERTIES
      (D) NSSS - AE INTERFACE
   B. RECENT TEST RESULTS
      (A) VALVE OPEN/CLOSE TESTS
      (B) DRAIN TESTS
   C. EMERGENCY PROCEDURE VERIFICATION

SYSTEM CONFIGURATIONS

I. SDV - IV HYDRAULIC COUPLING

   A) TWO BASIC CONFIGURATIONS

      1. SINGLE IV

      2. IV OFF EACH SDV HEADER

II. SDV VENT SYSTEM

   A) LARGE VARIABILITY IN DESIGN

      1. DEDICATED VENT PIPING

      2. CROSS-TIE BETWEEN SDV HEADERS

      3. INTERTIES WITH OTHER SYSTEMS

III. SDV DRAIN SYSTEMS

   A. BASIC CONFIGURATIONS

   B. INTERTIES WITH OTHER SYSTEMS

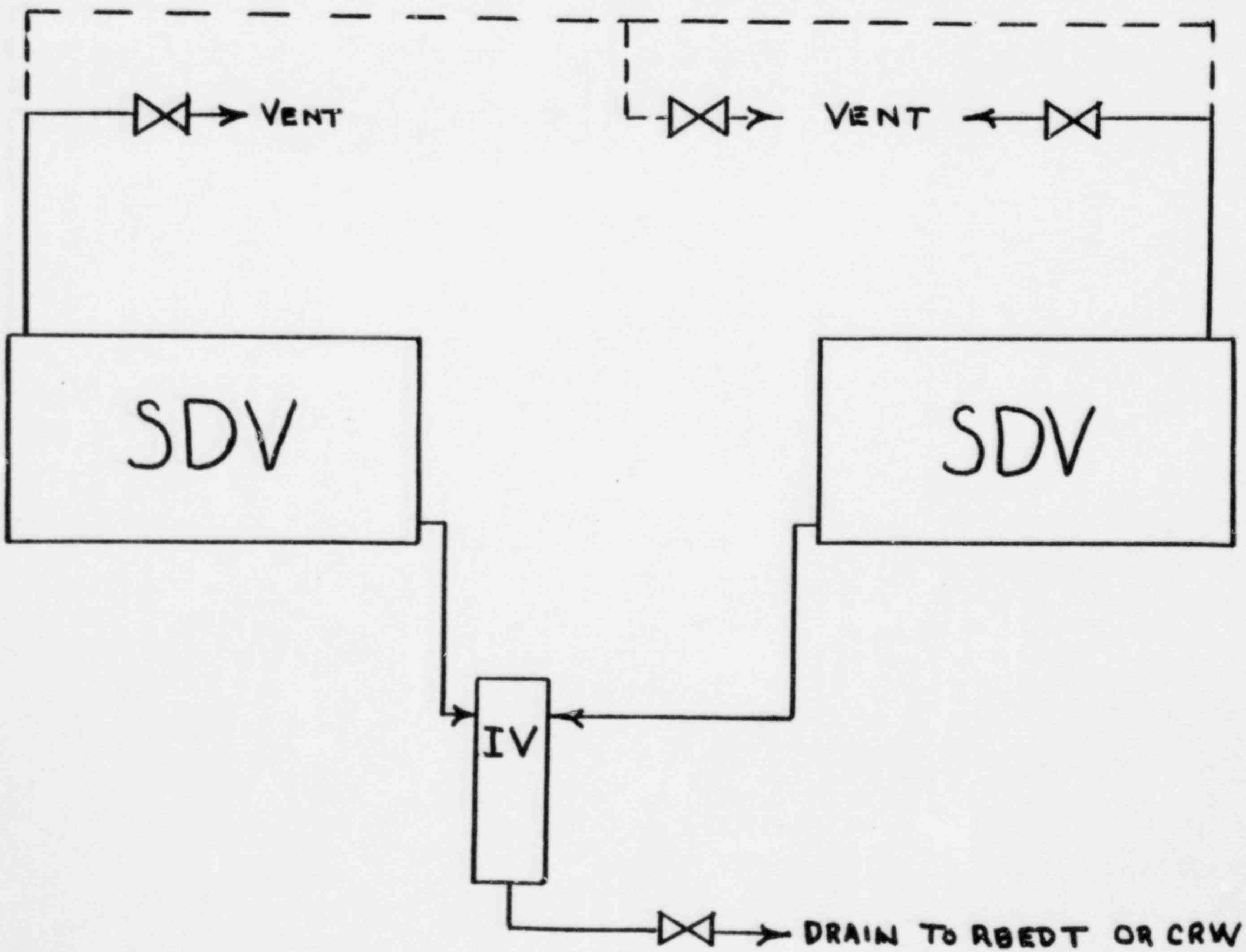
IV. DESIGN REQUIREMENTS

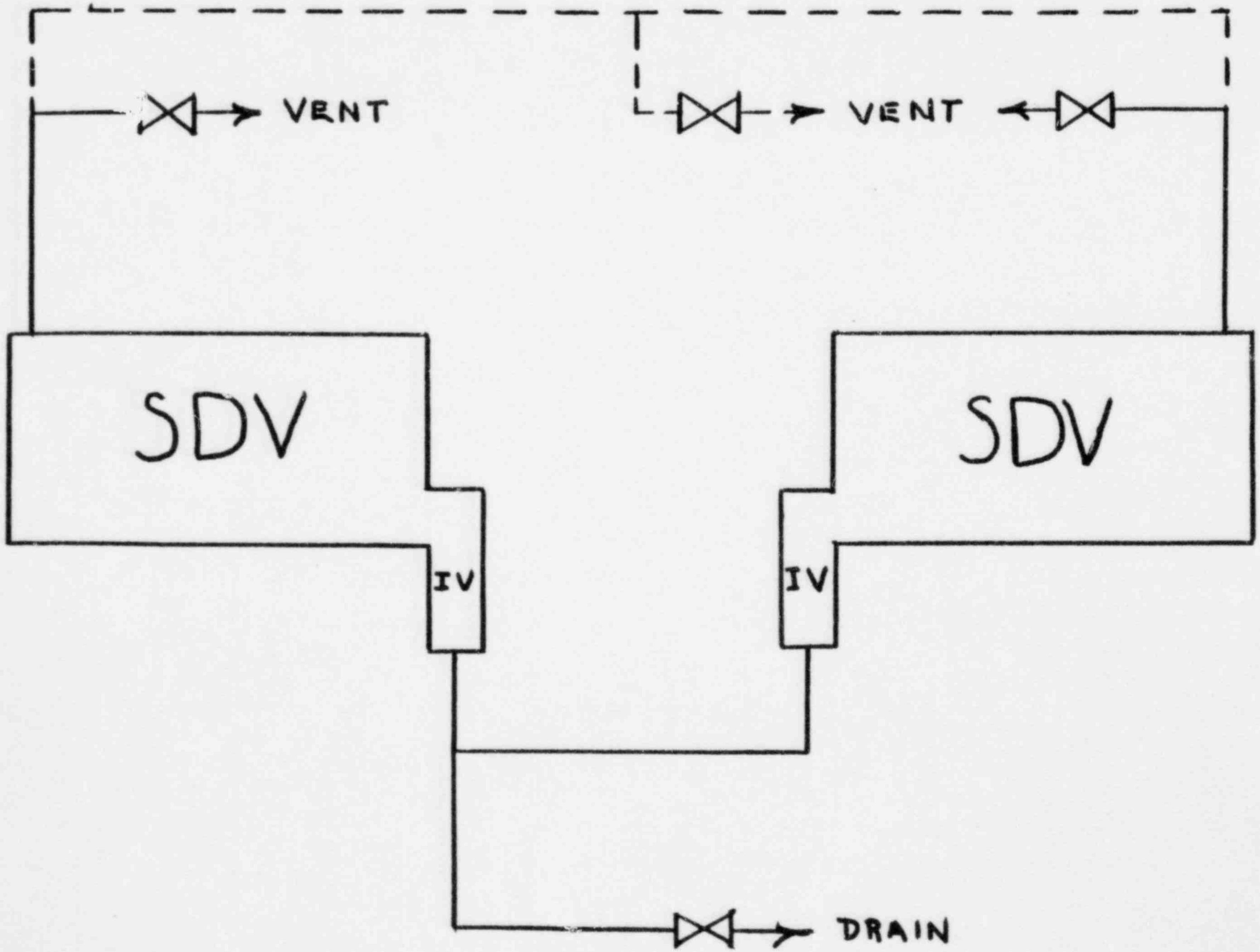
   A. SDV VOLUME REQUIREMENT

   B. PIPING SLOPES

   C. DYNAMIC LOADS

   D. DESIGN INTERFACES


VENT
VENT
SDV
SDV
IV
DRAIN TO RBEDT OR CRW


VENT
VENT
SDV
SDV
IV
IV
DRAIN

LONG TERM ACTIONS

1. COMPLETE REVIEW OF BULLETIN RESPONSES.
2. REVIEW OF AS-BUILT SYSTEM CONFIGURATIONS.
3. ENCOURAGE OWNERS GROUP PARTICIPATION
   a. SUBGROUP TO DEVELOP DESIGN & PERFORMANCE CRITERIA FOR SDV.
4. APPROVE CRITERIA.
5. IMPLEMENT PLANT MODIFICATION IN CONFORMANCE WITH CRITERIA.
6. AUDIT PLANT MODIFICATIONS.

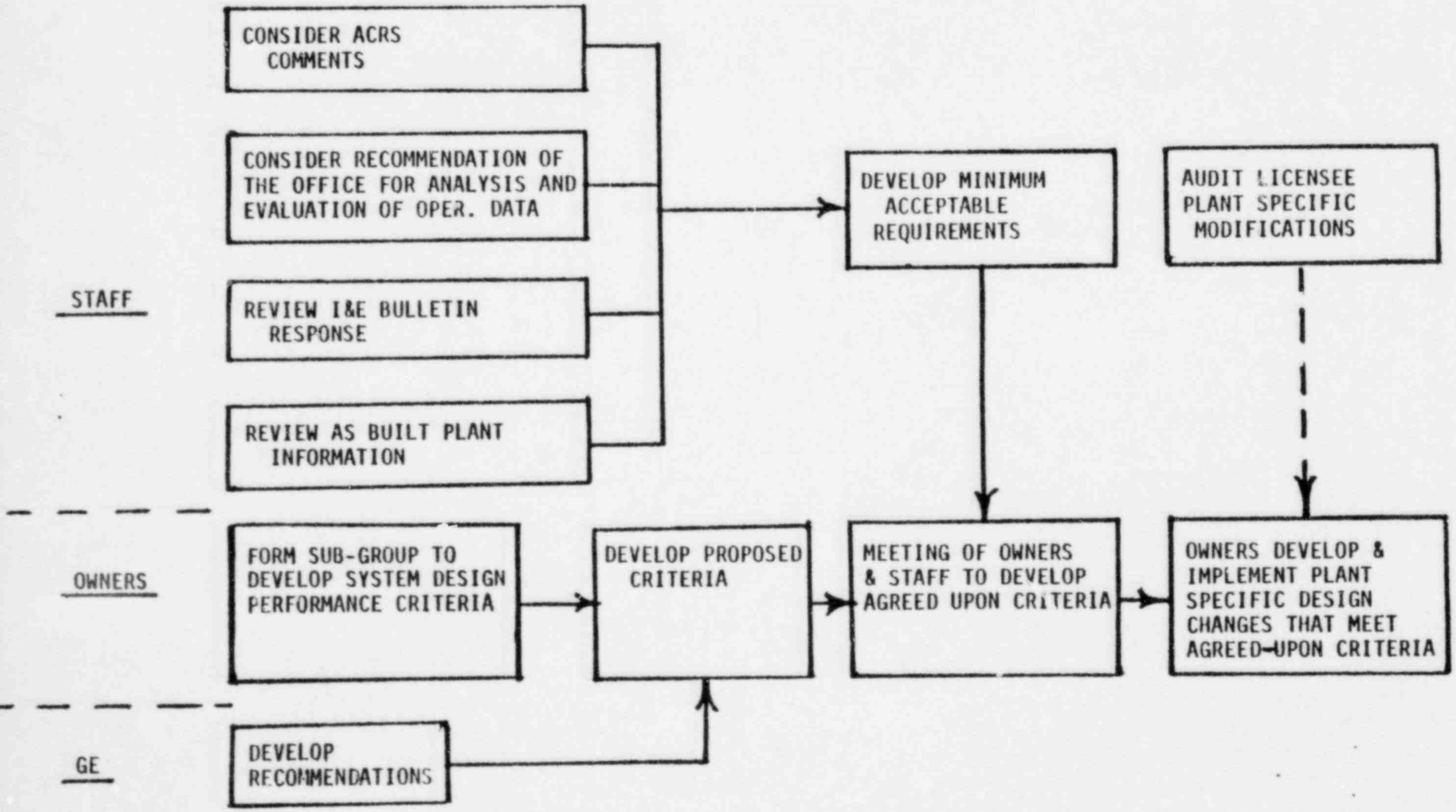

STAFF
OWNERS
GE
CONSIDER ACRS COMMENTS
CONSIDER RECOMMENDATION OF THE OFFICE FOR ANALYSIS AND EVALUATION OF OPER. DATA
REVIEW I&E BULLETIN RESPONSE
REVIEW AS BUILT PLANT INFORMATION
DEVELOP MINIMUM ACCEPTABLE REQUIREMENTS
AUDIT LICENSEE PLANT SPECIFIC MODIFICATIONS
FORM SUB-GROUP TO DEVELOP SYSTEM DESIGN PERFORMANCE CRITERIA
DEVELOP PROPOSED CRITERIA
MEETING OF OWNERS & STAFF TO DEVELOP AGREED UPON CRITERIA
OWNERS DEVELOP & IMPLEMENT PLANT SPECIFIC DESIGN CHANGES THAT MEET AGREED-UPON CRITERIA
DEVELOP RECOMMENDATIONS

FIGURE 1

UPCOMING ACTIONS

1. MEETING OF OWNER'S SUBGROUP - SEPT. 3 & 4, 1980.
2. RESULTS OF SUBGROUP MEETING TO STAFF - SEPT. 8, 1980.
3. STAFF - SUBGROUP MEETING - WEEK OF SEPT. 15, 1980.
4. APPROVED DESIGN AND PERFORMANCE CRITERIA IN PLACE - OCTOBER 17, 1980.
5. SCHEDULES FOR PLANT-SPECIFIC MODIFICATIONS - DECEMBER 15, 1980.

MSIV CLOSURE
PRESSURE VESSEL CRITERION*

• GENERIC BWR
NO RPT
EOC

P_{MAX}=1460 PSIG

*PRESSURE VESSEL LEVEL C SERVICE LIMIT = 1500 PSIG

MSIV CLOSURE FROM 100% POWER
FOR PLANTS WITH RPT
SUPPRESSION POOL TEMPERATURE LIMIT*

BROWNS FERRY PLANT(PRELIMINARY FROM GE)

- BF3 TYPE SCRAM
  1 RHR @ 30 MINUTES
  1 SLCS @ 30 MINUTES
  T_{MAX}=190°F

- RODS ON ONE HALF OF CORE STAY OUT
  2 RHR @ 30 MINUTES
  1 SLCS @ ABOUT 8 MINUTES
  T_{MAX}=200°F

GENERIC BWR

- BF3 TYPE SCRAM
  2 RHR @ 10 MINUTES
  1 SLCS @ 10 MINUTES
  T_{MAX}=153°F

- WITH 1 SLCS @ 30 MINUTES
  T_{MAX}=186°F

- RODS ON ONE HALF CORE STAY OUT
  2 RHR @ 10 MINUTES
  1 SLCS @ A BIT GREATER THAN 5 MINUTES
  T_{MAX}=LESS THAN 200°F

*SUPPRESSION POOL TEMPERATURE LIMIT =160°F w/o QUENCHERS AND 200°F w QUENCHERS

BF-3 EVENT IMPACT ON ATWS

BWR ESTIMATED ATWS FREQUENCY

	CURRENT	ALT. 3A	ALT. 4A
PRIOR TO BF-3	$\sim 2 \times 10^{-4}$/RY	$\sim 1 \times 10^{-5}$/FY	$\lesssim 1 \times 10^{-6}$/RY
BF-3 (BWR'S ONLY)	$\sim 1 \times 10^{-3}$/RY	$\sim 5 \times 10^{-5}$/RY	$\lesssim 5 \times 10^{-6}$/RY
LWR'S	$\sim 5 \times 10^{-4}$/RY	$\sim 2.5 \times 10^{-5}$/RY	$\lesssim 2.5 \times 10^{-6}$/RY

EFFECTS OF BF3 EVENT FROM 100% POWER

FROM RODS OUT, ROD MOVEMENT EQUAL TO BF3 EVENT AND RPT
POWER = 10%

HALF RODS IN OTHER HALF OUT WITH RPT
POWER = 20%

HALF RODS IN OTHER HALF OUT NO RPT
POWER = 40%

BROWNS FERRY 3 EVENT (PARTIAL SCRAM)

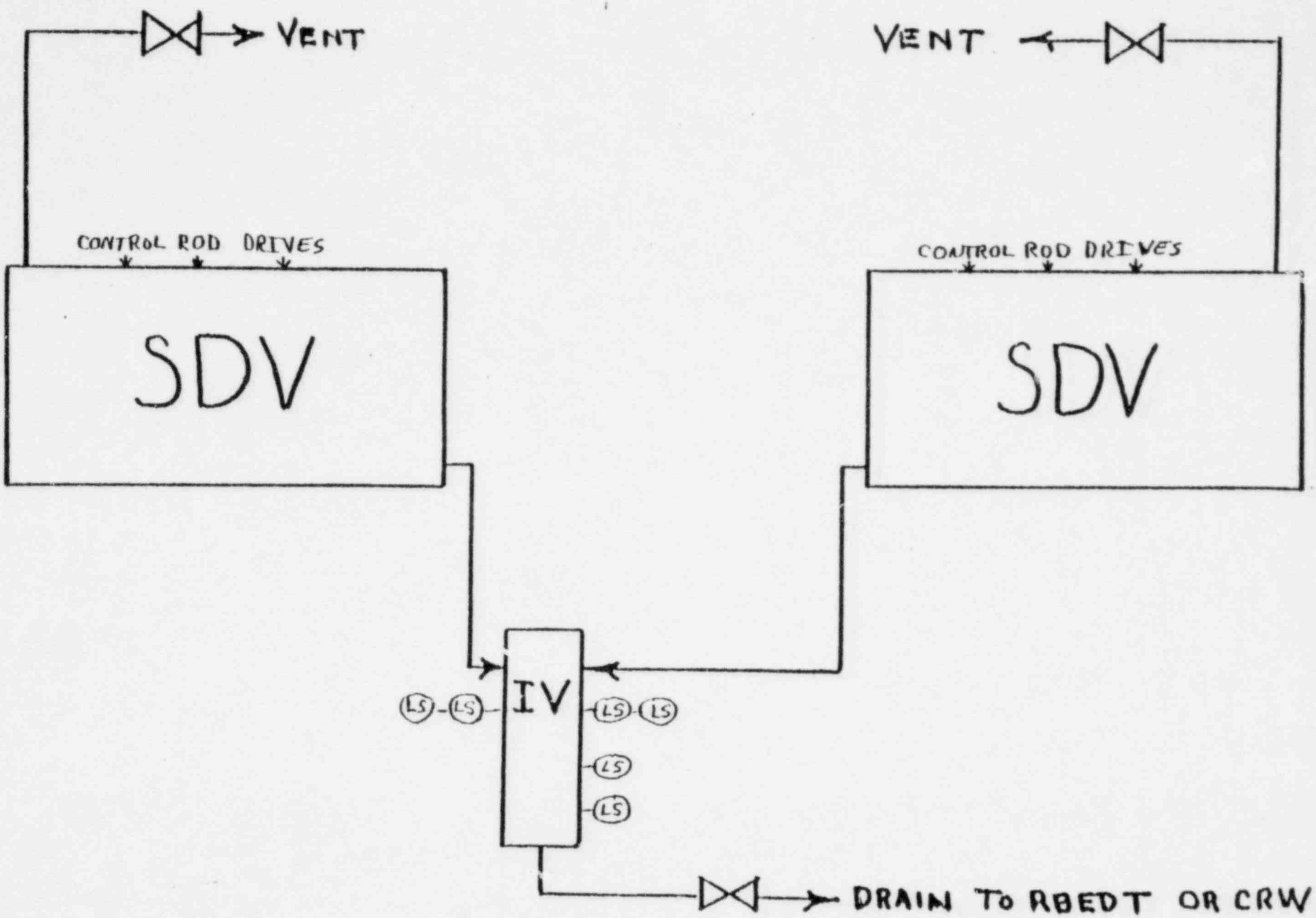
I. CONCERNS RAISED

II. SHORT-TERM ACTIONS TAKEN - BULLETIN 80-17

- 80-17 REQUIREMENTS
- 80-17 FINDINGS

III. SHORT-TERM ACTIONS ONGOING

IV. CONCLUSIONS


VENT
VENT
CONTROL ROD DRIVES
CONTROL ROD DRIVES
SDV
SDV
IV
LS
LS
LS
LS
LS
LS
DRAIN TO RBEDT OR CRW

BROWNS FERRY 3 EVENT

CONCERNS RAISED

- -- RELIABILITY OF SCRAM FUNCTION
- -- UNDERSTANDING OF THE AS-BUILT SDV CONFIGURATION
- -- SHORT-TERM ACTIONS NEEDED TO JUSTIFY CONTINUED OPERATION
- -- LONG-TERM ACTIONS NEEDED TO PROVIDE:

    SDV DESIGN WITH IMPROVED RELIABILITY

    ATWS RELATED PROCEDURES AND MODIFICATIONS

BROWNS FERRY 3 EVENT

1. WITHIN THREE DAYS VERIFY SDV EMPTY, VERIFY SDV OPERABLE
2. WITHIN 20 DAYS PERFORM ONE AUTOMATIC AND ONE MANUAL SCRAM TEST AT NORMAL OPERATING TEMPERATURE AND PRESSURE
3. AFTER SCRAM TESTS VERIFY SDV EMPTY AND OPERABLE
4. DEVELOP PROCEDURES TO MONITOR THE SDV DAILY FOR WATER ACCUMULATION
5. REVIEW EMERGENCY OPERATING PROCEDURES AND ENSURE THAT REQUIRED OPERATOR ACTIONS ARE ADEQUATE FOR BF-3 TYPE EVENT
6. TAKE ACTIONS SPECIFIED TO MITIGATE THE CONSEQUENCES OF AN ATWS EVENT
7. WITHIN FIVE DAYS OF PERFORMANCE OF EACH SCRAM TEST, SUBMIT RESULTS TO NRC
8. BWRs CURRENTLY SHUTDOWN, PERFORM THE SCRAM TESTS PRIOR TO POWER OPERATION

BULLETIN 80-17, SUPPLEMENT 1

REQUIRES:

- -- CONTINUOUS ALARM MONITORING BY SEPTEMBER 1
- -- DESIGN REVIEW OF VENT SYSTEM BY SEPTEMBER 1
- -- PROCEDURAL CONTROLS FOR AVAILABILITY AND USE OF STANDBY LIQUID CONTROL SYSTEM
- -- VERIFICATION OF AS-BUILT DRAWINGS OF SDV AND DESCRIPTION OF VENTS AND DRAIN

BULLETIN 80-17, SUPPLEMENT 2

REQUIRES:

EACH BWR WITH SDV VENT SYSTEM THAT DEPENDS ON ANY COMPONENT OTHER THAN THE VENT VALVE ALONE FOR PROPER VENTING MUST PROVIDE AN ALTERNATE VENT PATH CONTINUOUSLY OPEN TO BUILDING ATMOSPHERE WITHIN 48 HOURS OF NOTIFICATION TO CONTINUE OPERATION. IT MUST BE POSITIVE IN ITS FUNCTION AT ALL TIMES.

BULLETIN 80-17, SUPPLEMENT 3

REQUIRES:

-- IMMEDIATE MANUAL SCRAM ON LOW CRDS AIR PRESSURE

-- IMMEDIATE MANUAL SCRAM ON EITHER MULTIPLE ROD DRIFT ALARMS OR MAKRED INCREASE IN CRD HIGH TEMPERATURE ALARMS

-- FUNCTIONAL TEST OF ALL SDIV LEVEL SWITCHES FOLLOWING EACH SCRAM, PRIOR TO REACTOR STARTUP

80-17 STATUS AND FINDINGS

STATUS:

- -- ALL PLANTS HAVE RESPONDED TO 80-17, SUPPLEMENTS 1, 2
- -- SCRAM TESTS HAVE BEEN COMPLETED (EXCEPT BRUNSWICK 2, S/D)
- -- STAFF REVIEW OF RESPONSES IS ONGOING
- -- RESPONSES TO SUPPLEMENT 3 DUE SEPTEMBER 2, 1980

FINDINGS:

- -- SOME SCRAM SYSTEM DEFICIENCIES HAVE BEEN FOUND
- -- RESPONSES TO BULLETIN 80-17, SUPPLEMENTS 1, 2 HAVE BEEN SATISFACTORY FOR ALL PLANTS,
  - -EMPTY AND OPERABLE SDV VERIFIED
  - -DATA ON SDV DESIGN/OPERATION OBTAINED
  - -DAILY MONITORING FOR WATER IN SDV ESTABLISHED
  - -PROCEDURES FOR SBLC INITIATION PROVIDED
  - -ATWS ANALYSES PROVIDED TO THE STAFF

EXCEPT,

-- CONTINUOUS MONITORING FOR WATER IN THE SDV NOT INSTALLED FOR MOST PLANTS BY SEPTEMBER 1, 1980, AND INCREASED FREQUENCY OF DAILY SURVEILLANCE NOT PROPOSED IN THE INTERIM

SCRAM SYSTEM DEFICIENCIES

BRUNSWICK UNIT 1 - CRUSHED FLOATS IN HIGH LEVEL ALARM AND ROD BLOCK INSTRUMENTS. DRAIN PIPING SUPPORTS DAMAGED.

HATCH UNIT 1 - TWO HIGH LEVEL SCRAM INSTRUMENTS FOUND INOPERABLE DUE TO BENT FLOAT STEMS

DRESDEN UNIT 3 - SDV HEADER DID NOT DRAIN PROPERLY

BROWNS FERRY UNIT 1 - SDIV DID NOT DRAIN PROPERLY

MILLSTONE UNIT 1 - 10-SECOND DELAY RELAY WAS NOT CONNECTED ELECTRICALLY

DUANE ARNOLD - SDV DRAIN VALVE WAS INSTALLED BACKWARDS

PEACH BOTTOM UNITS 2 & 3 - BACKUP SCRAM VALVE SOLENOIDS CONNECTED TO INCORRECT ELECTRICAL SOURCE

NINE MILE POINT UNIT 1 - ONE ROD FAILED TO SCRAM DURING MANUAL SCRAM TEST - SCRAM PILOT VALVE FAILURE

FITZPATRICK - LOOP SEAL FOUND IN INSTALLED SDV DRAIN PIPING

HATCH 2 - TWO SDV HIGH LEVEL SCRAM FLOATS FOUND CRUSHED

SHORT-TERM ACTIONS ONGOING

-- ONGOING REVIEW OF 80-17 RESPONSES

-- ONGOING REVIEW TO IDENTIFY NEED FOR IMPROVEMENT IN OTHER, RELATED AREAS

-- CONSIDERING FURTHER STAFF ACTIONS TO REQUIRE INSTALLATION OF CONTINUOUS SDV MONITORING BY DECEMBER 1980, WITH MONITORING ONCE PER SHIFT IN THE INTERIM

CONCLUSIONS

-- CORRECTIVE ACTIONS BEING TAKEN ENSURE THAT THE SDV IS EMPTY DURING POWER OPERATION

-- THESE CORRECTIVE ACTIONS ARE NECESSARY AND SUFFICIENT TO JUSTIFY CONTINUED OPERATION

-- LONG-TERM ACTION IS NECESSARY AND IS UNDERWAY

- NRR TASK FORCE

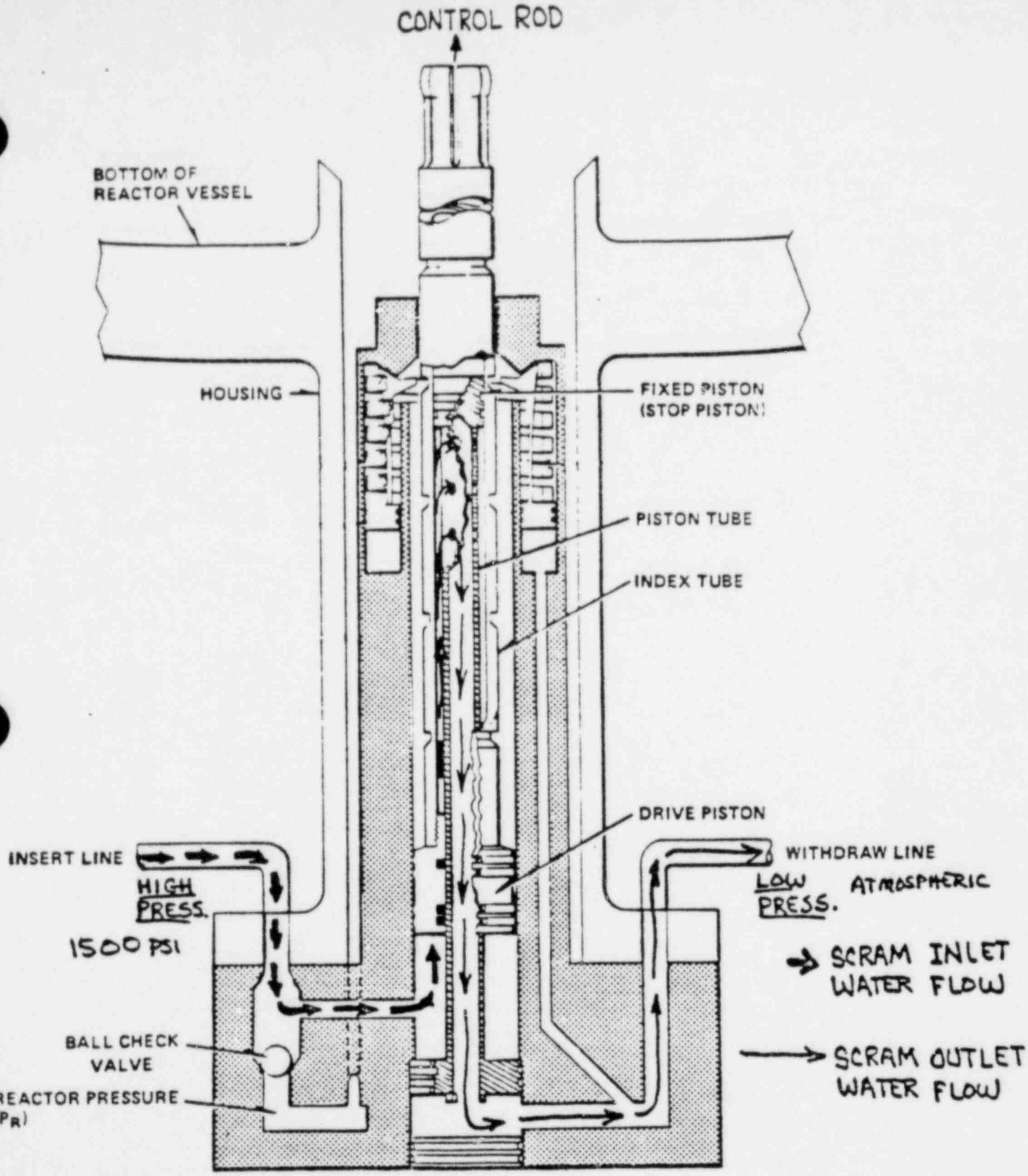


FIGURE 2.2 BWR CONTROL ROD DRIVE MECHANISM (SIMPLIFIED SKETCH)

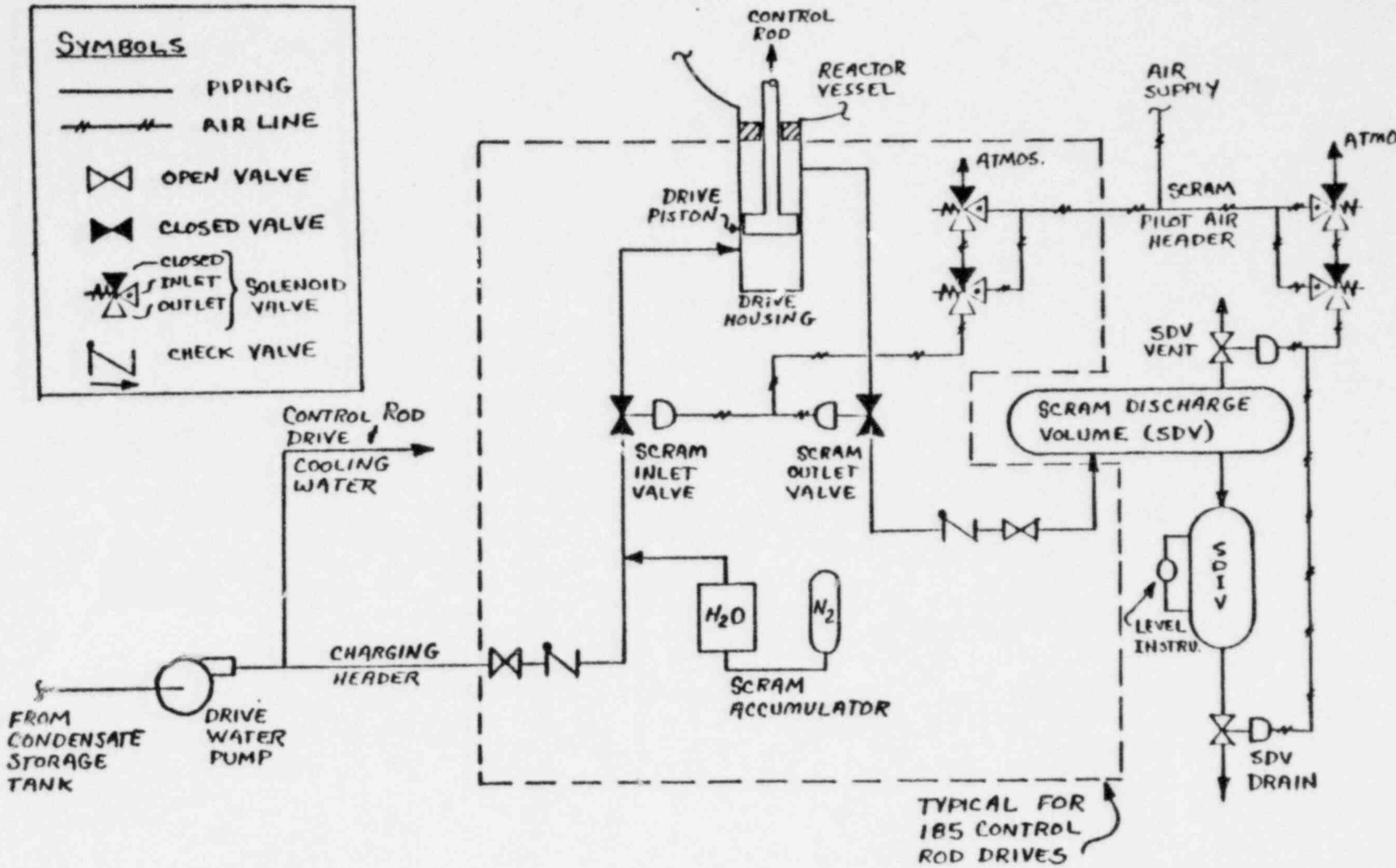


FIGURE 2.1 BWR SCRAM HYDRAULIC SYSTEM (NORMAL VALVE LINEUP, PRE-SCRAM)

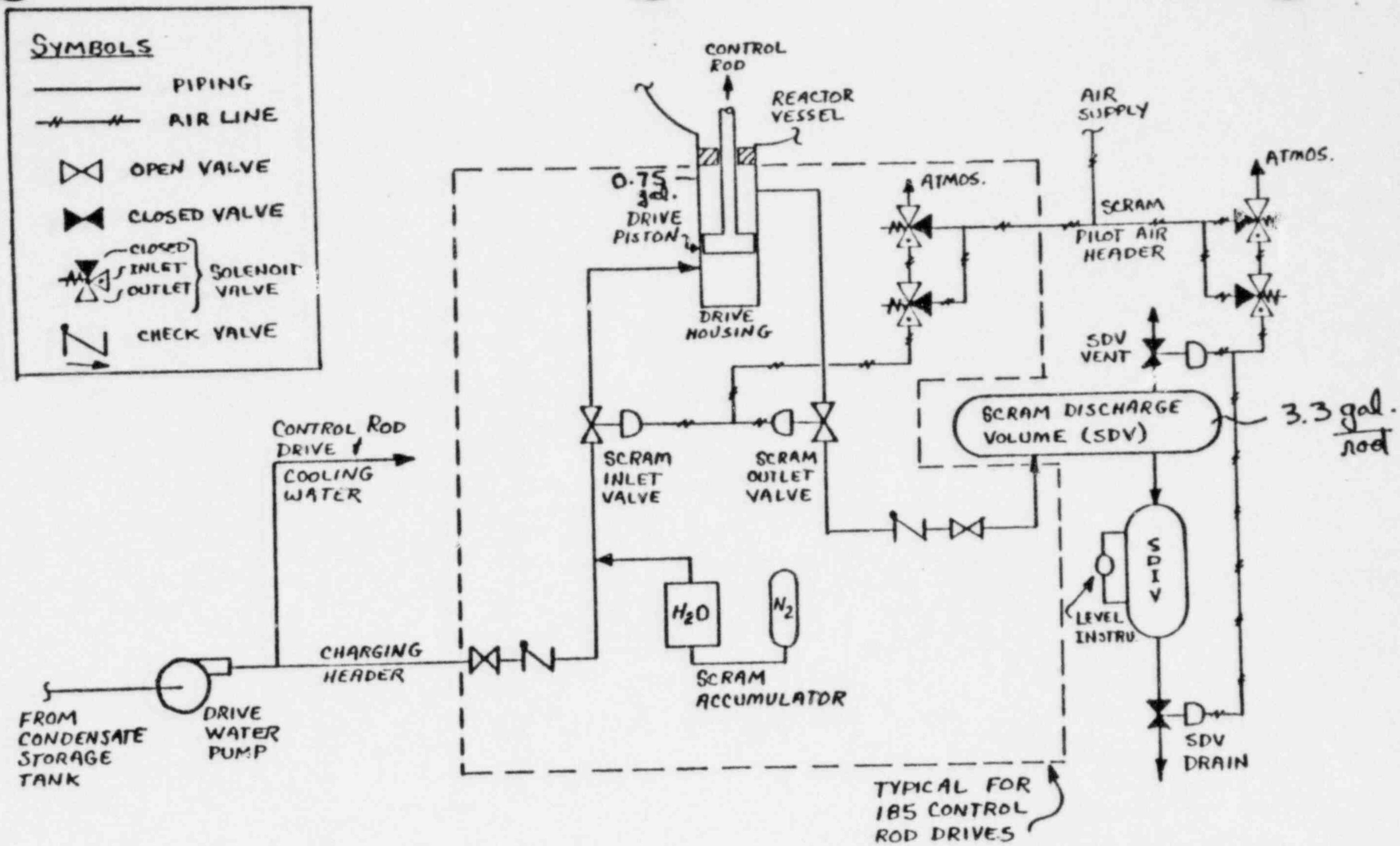


FIGURE 2.3 BWR SCRAM HYDRAULIC SYSTEM (SCRAMMED VALVE LINEUP)

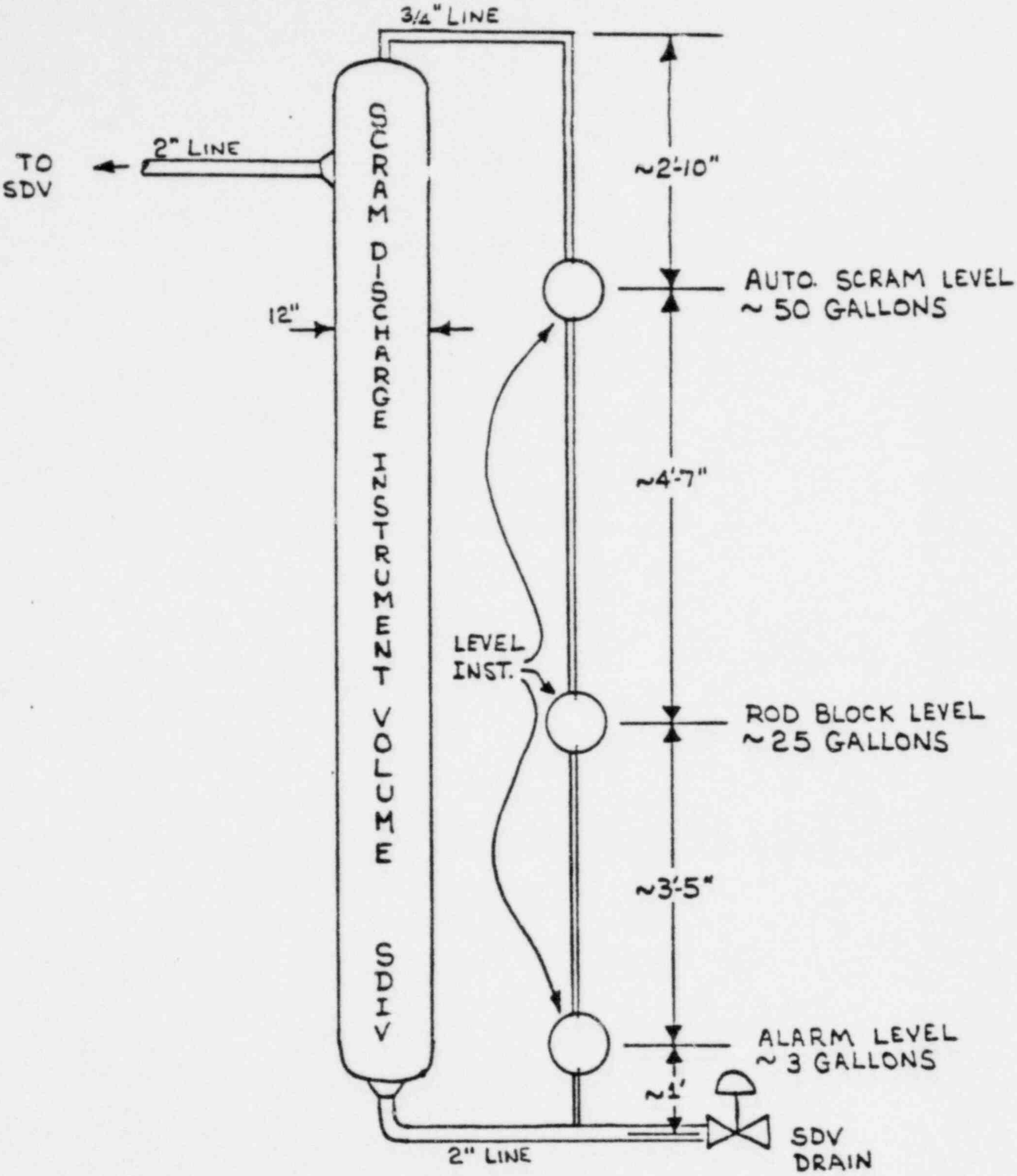


FIGURE 2.4 BROWNS FERRY SCRAM DISCHARGE INSTRUMENT VOLUME

(93 RODS) EAST | WEST (92 RODS)

	02	06	10	14	18	22	26	30	34	38	42	46	50	54	58
5					40	4	36	42	0	0	0				
5				36	46	38	2	0_W	0	0	0	0			
1			36	34	0	36	42	10	0	0	0	0	0		
17		42	0	20	0	24	0	0_W	0	0	0	0	0	0	
13	12	40	44	0	30	28	40	38	0	0	0	0	0	0	0
39	24	0	40	0	0	34	32	0_W	0	0	0	0	0	0	0
35	30	4	38	0	18	8	34	30	0	0	0	0	0	0	0
31	22	10	0	34	28	2	0	0_W	0	0	0	0	0	0	0
27	42	36	30	14	26	28	36	24	0	0	0	0	0	0	0
23	8	0	38	32	0	12	36	2_W	0	0	0	0	0	0	0
19	34	42	34	20	10	28	22	26	0	0	0	0	0	0	0
15		40	0	36	0	38	0	0_W	0	0	0	0	0	0	
11			26	6	14	40	40	24	0	0	0	0	0		
07				0	40	2	0	0_W	0	0	0	0			
03					28	42	42	30	0	0	0				

75/93

1/92

180°

Control Rod Scram Group Assignment

Figure 3.1 Control Rod Positions After First Scram

(93) EAST | WEST (92)

	02	06	10	14	18	22	26	30	34	38	42	46	50	54	58
					30	0	14	18	0	0	0				
				26	42	28	0	0w	0	0	0	0			
1			12	22	0	28	34	0	0	0	0	0	0		
7		30	0	0	0	12	0	0w	0	0	0	0	0	0	
3	0	30	38	0	14	4	30	26	0	0	0	0	0	0	0
9	8	8	30	0	0	26	14	0w	0	0	0	0	0	0	0
5	14	0	30	0	4	0	22	20	0	0	0	0	0	0	0
1	8	0	0	14	12	0	0	0w	0	0	0	0	0	0	0
7	26	22	20	2	12	12	16	2	0	0	0	0	0	0	0
3	0	0	30	20	0	0	16	0w	0	0	0	0	0	0	0
9	10	26	24	0	0	8	6	4	0	0	0	0	0	0	0
5		18	0	18	0	28	0	0w	0	0	0	0	0	0	
			12	0	0	32	30	14	0	0	0	0	0		
07				0	30	0	0	0w	0	0	0	0			
03					2	34	22	4	0	0	0				

60/93

0/92

180°

Control Rod Scram Group Assignment

Figure 3.2 Control Rod Positions After Second Scram

0°

(93) EAST | WEST (92)

	02	06	10	14	18	22	26	30	34	38	42	46	50	54	58
					24	0	0	0	0	0	0				
				20	40	22	0	0	0	0	0	0			
			0	16	0	24	30	0	0	0	0	0	0		
		26	0	0	0	6	0	0	0	0	0	0	0	0	
3	0	24	36	0	2	0	24	18	0	0	0	0	0	0	0
9	2	0	24	0	0	20	0	0	0	0	0	0	0	0	0
5	6	0	26	0	0	0	14	16	0	0	0	0	0	0	0
1	0	0	0	2	4	0	0	0	0	0	0	0	0	0	0
7	18	16	14	2	4	6	2	0	0	0	0	0	0	0	0
3	0	0	26	12	0	0	2	0	0	0	0	0	0	0	0
9	0	16	18	0	0	0	2	2	0	0	0	0	0	0	0
5		4	0	10	0	0	0	0	0	0	0	0	0	0	
1			6	0	0	28	26	8	0	0	0	0	0		
7				0	24	0	0	0	0	0	0	0			
3					0	30	10	0	0	0	0				

46/93

0/92

180°

Control Rod Scram Group Assignment

Figure 3.3 Control Rod Positions After Third Scram

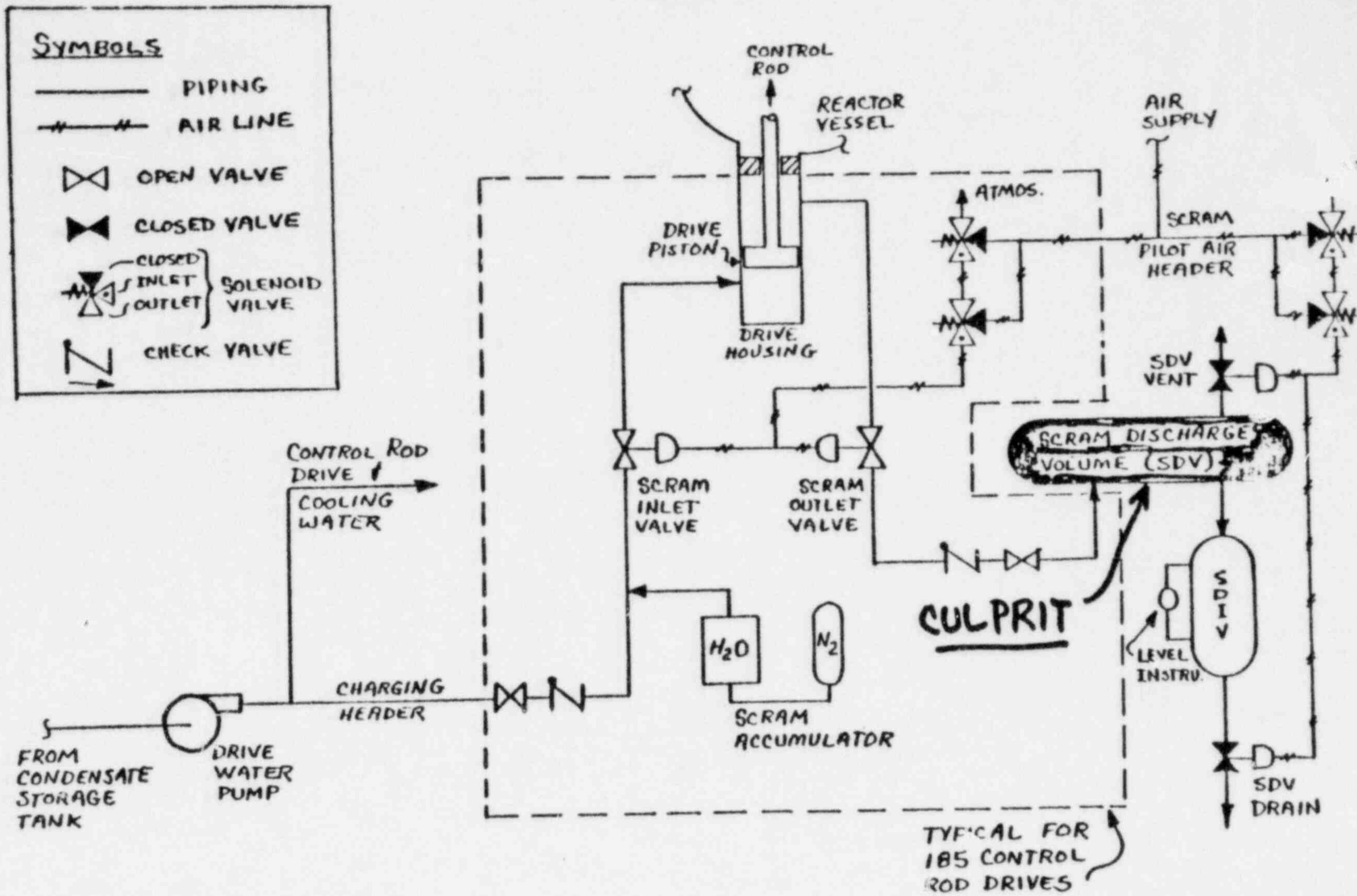
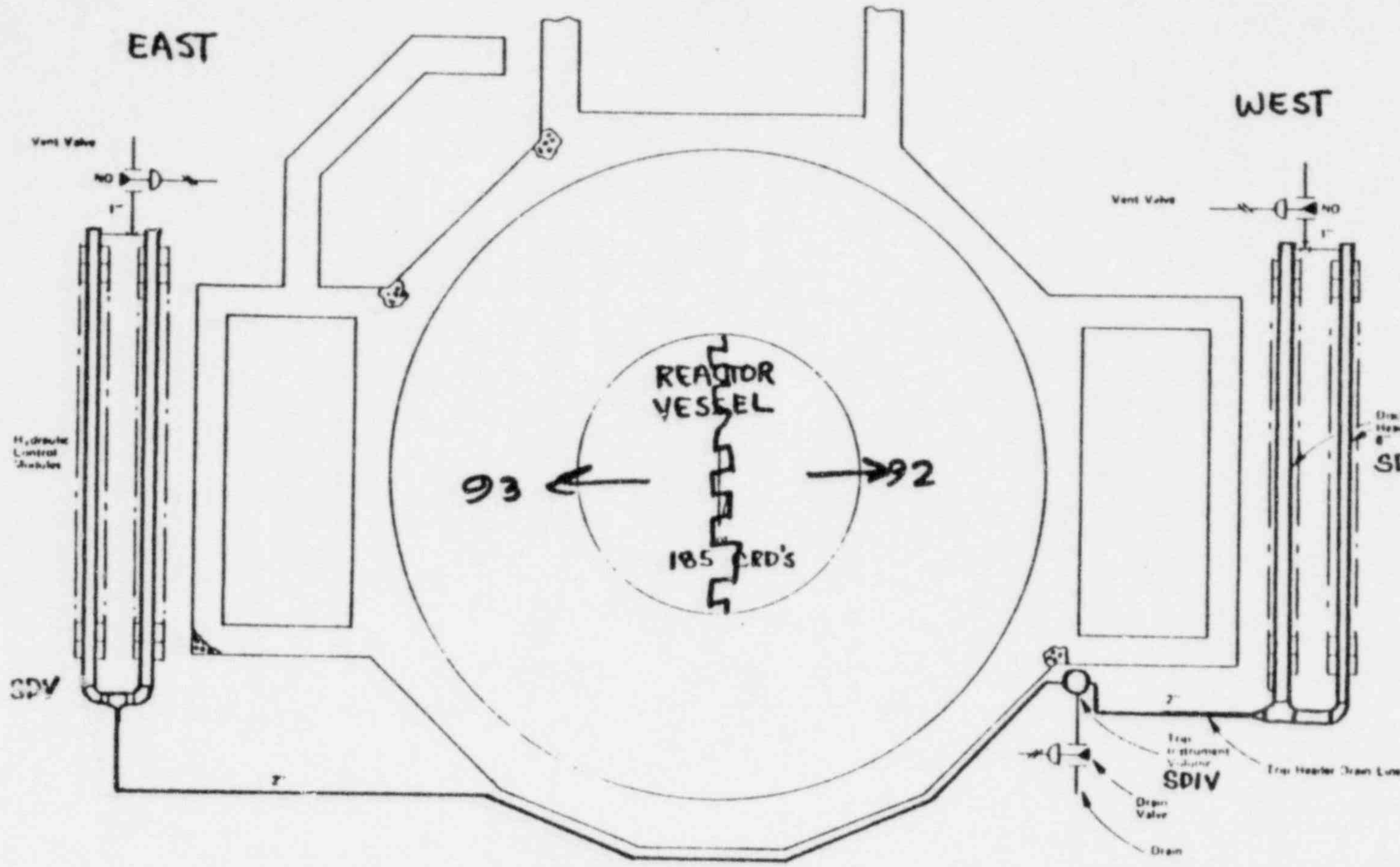


FIGURE 2.3 BWR SCRAM HYDRAULIC SYSTEM (SCRAMMED VALVE LINEUP)



PEACH BOTTOM Reactor Protection System Trip Discharge Headers

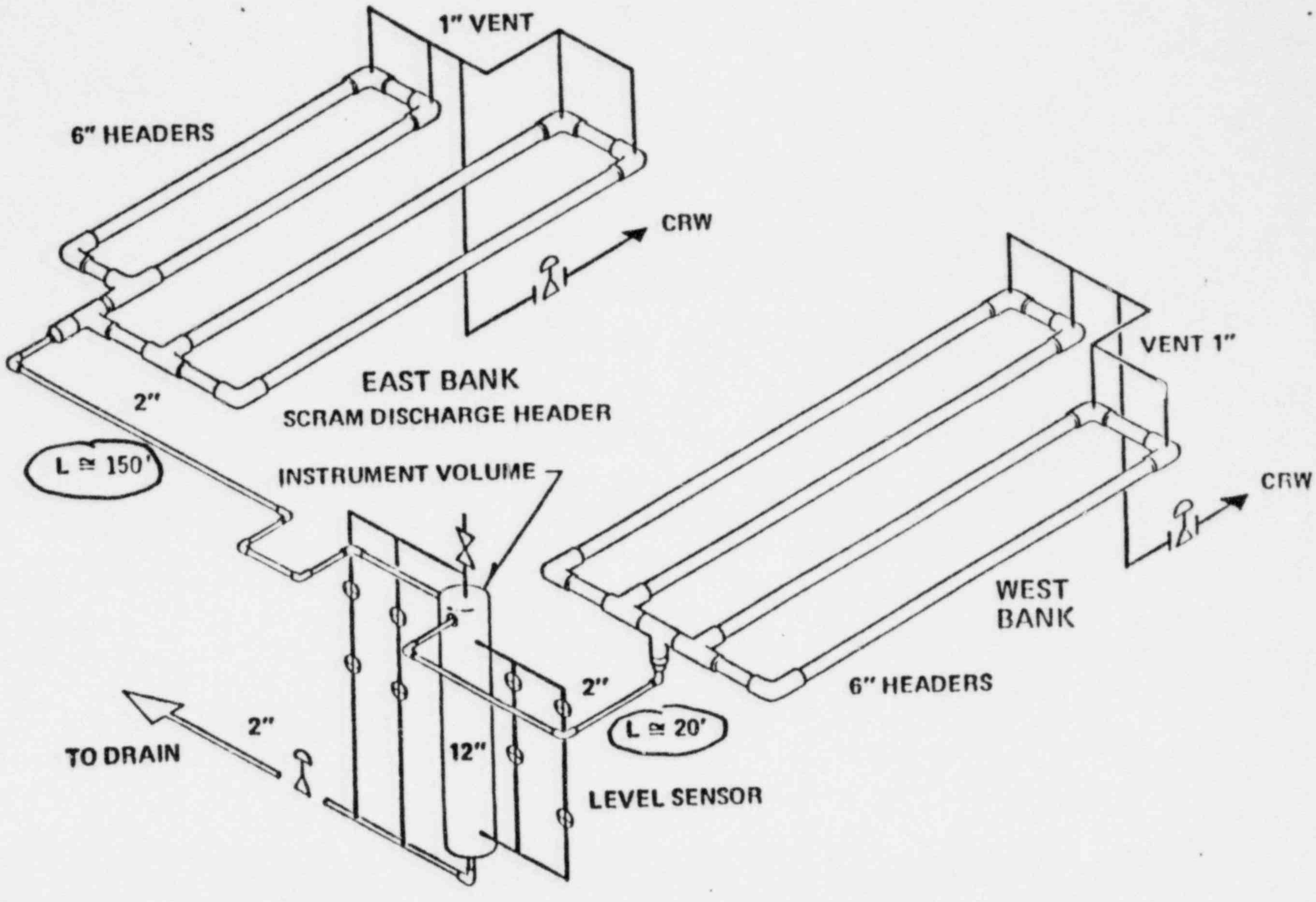


FIGURE 3.4 BROWNS FERRY SDV EQUIPMENT LAYOUT (ISOMETRIC VIEW)

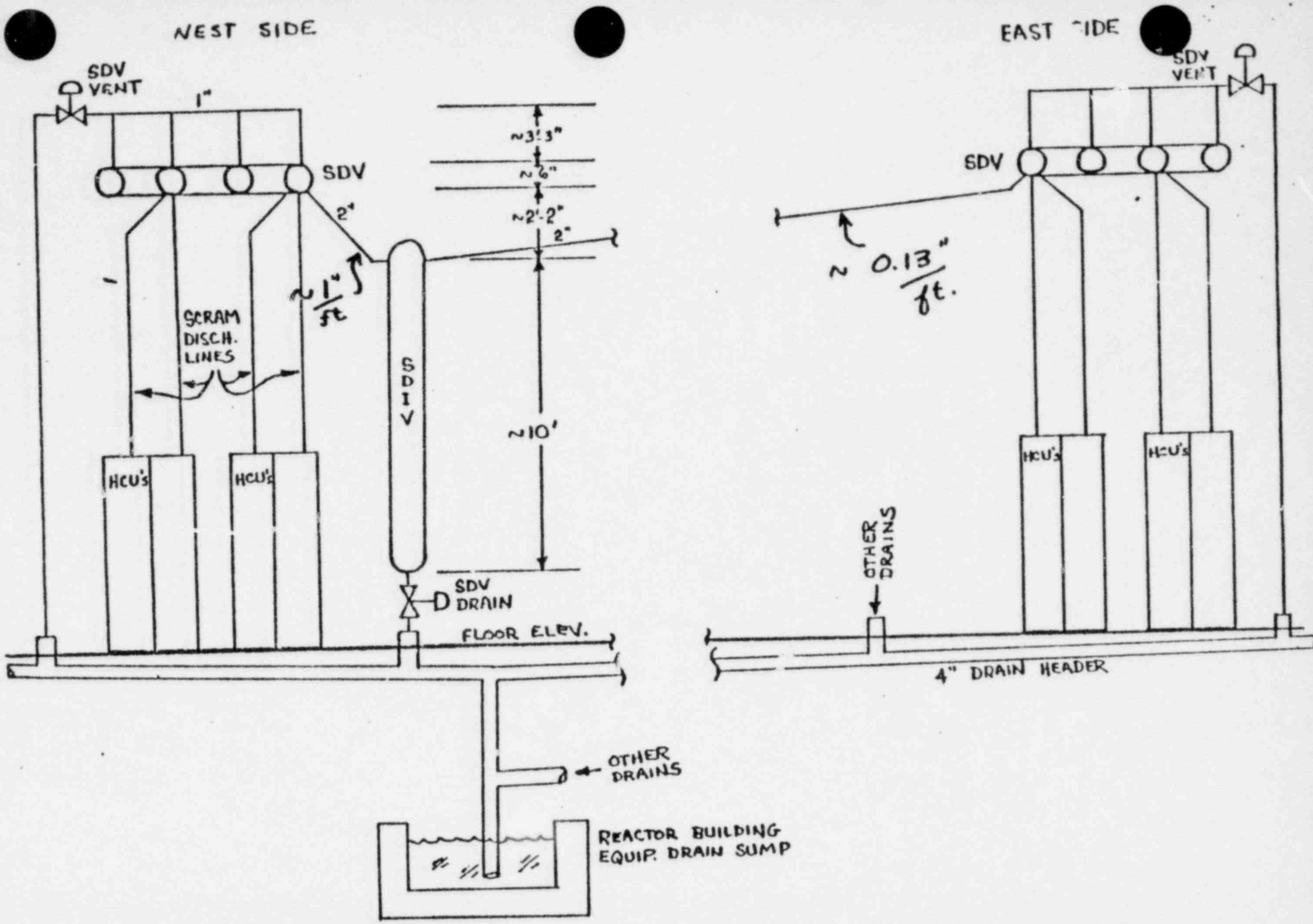


FIGURE 3.5 SKETCH OF BROWNS FERRY SDV EQUIPMENT LAYOUT (ELEVATION)

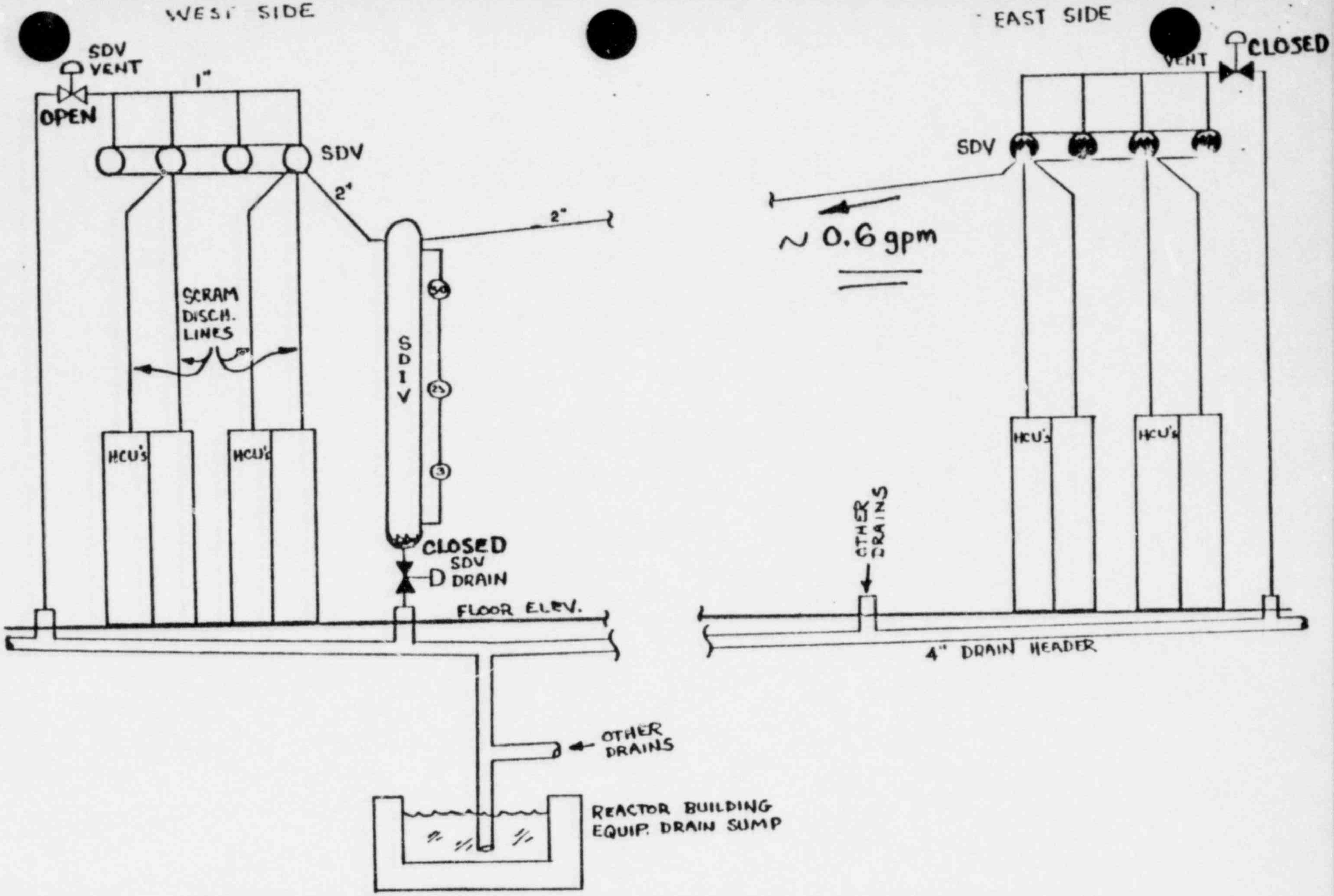


FIGURE 3.6 SIMULATED EAST SDV VENT LINE BLOCKAGE TEST

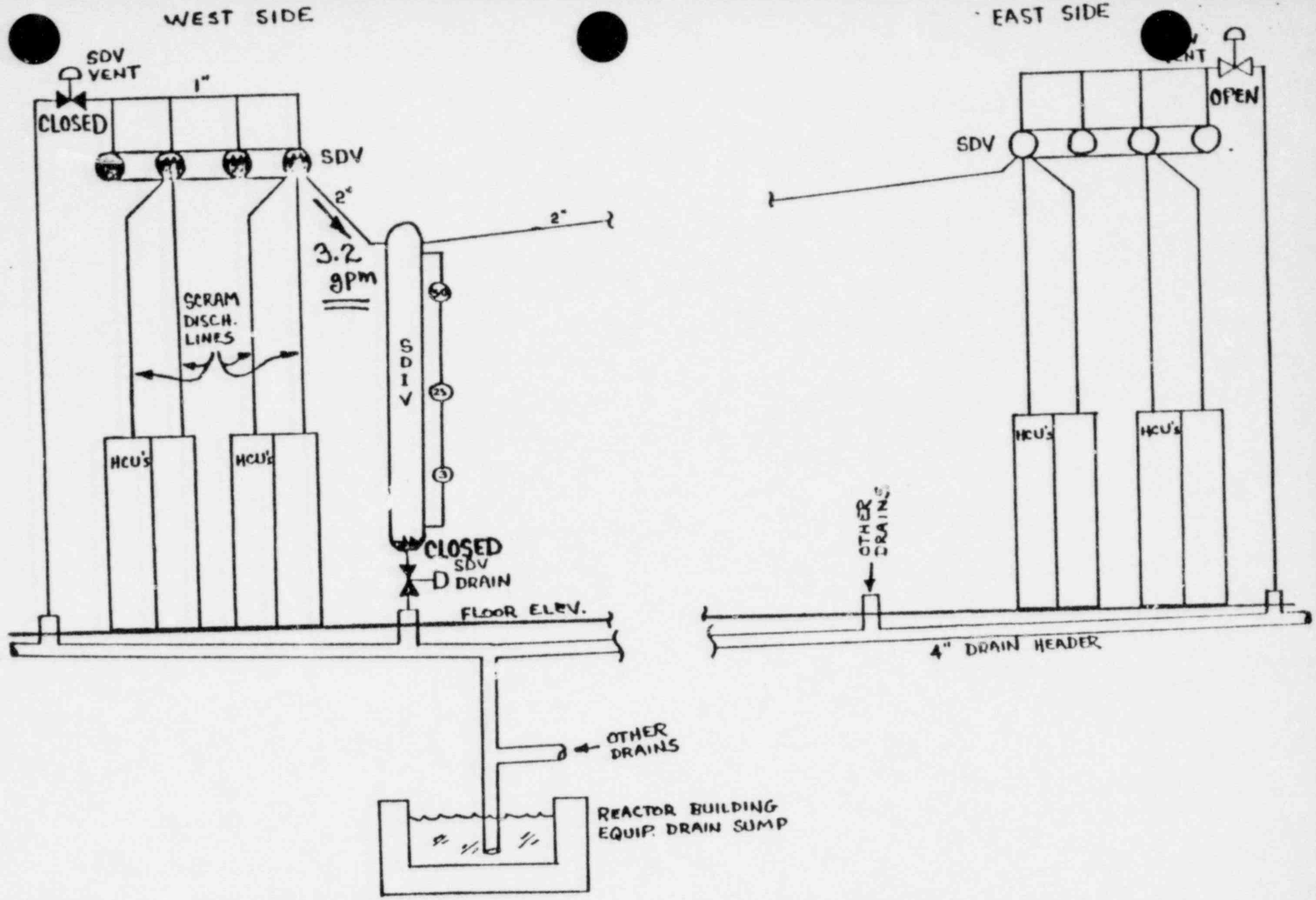


FIGURE 3.7 SIMULATED WEST SDV VENT LINE BLOCKAGE TEST

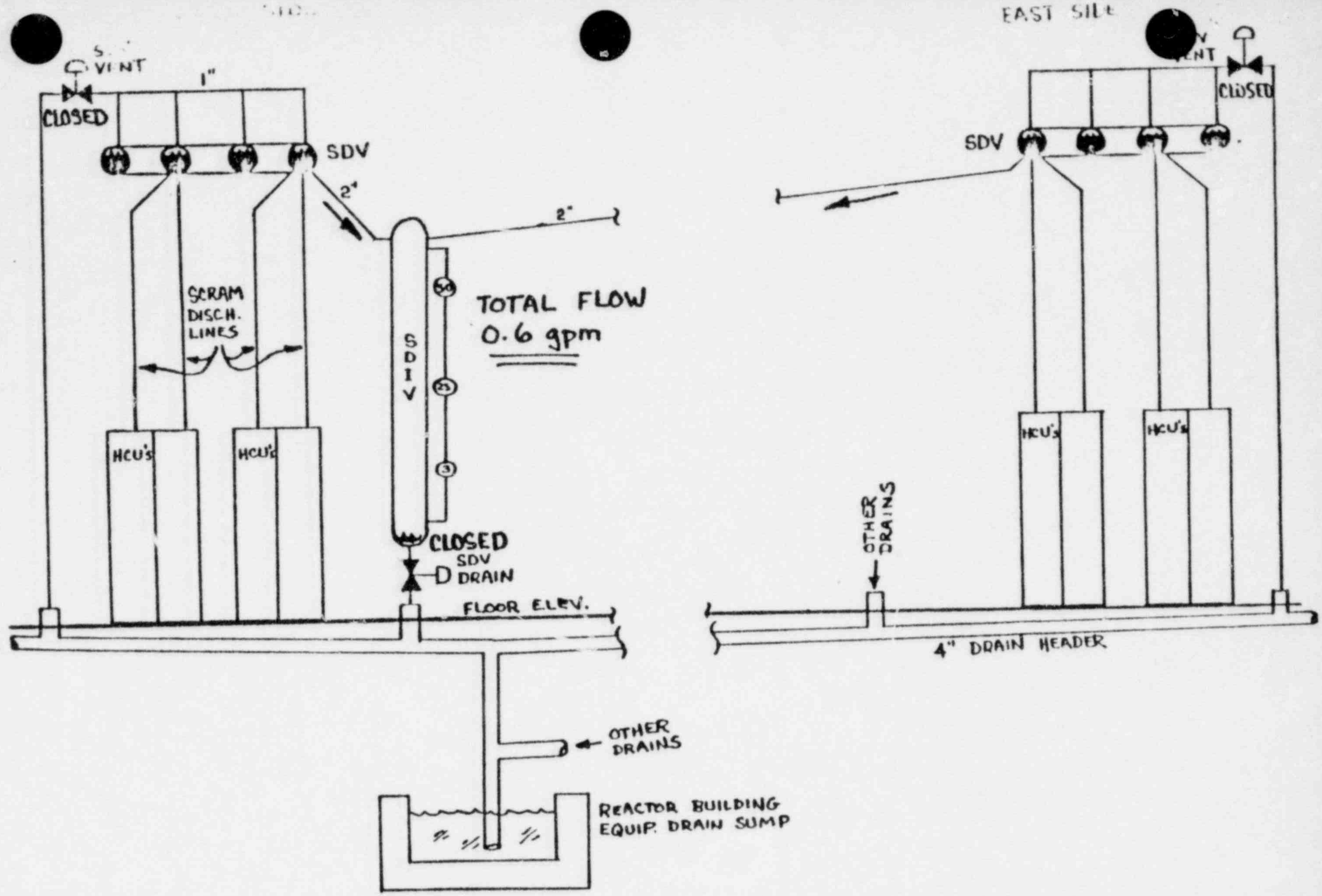


FIGURE 3.8 SIMULATED EAST AND WEST SDV VENT LINE BLOCKAGE TEST

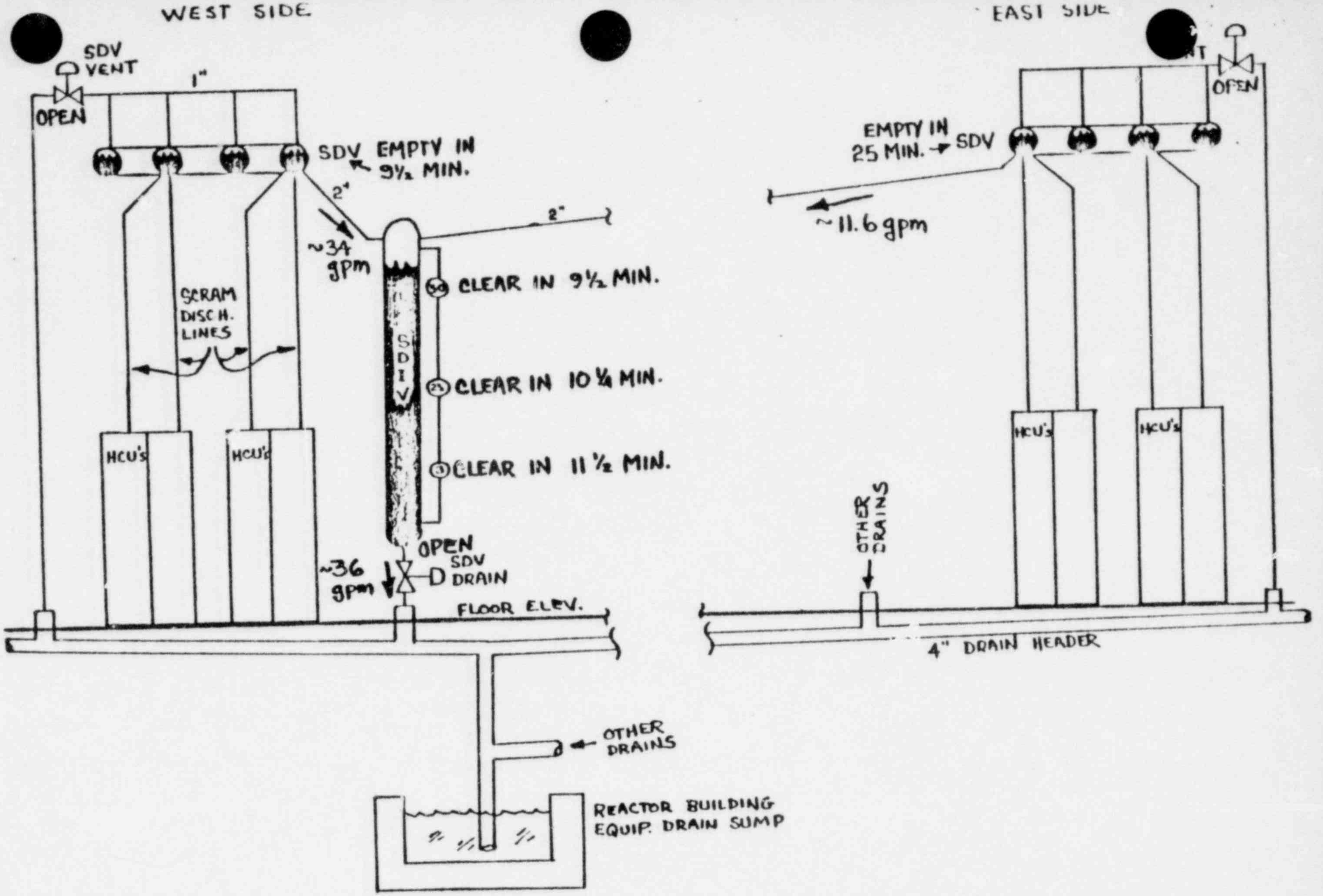
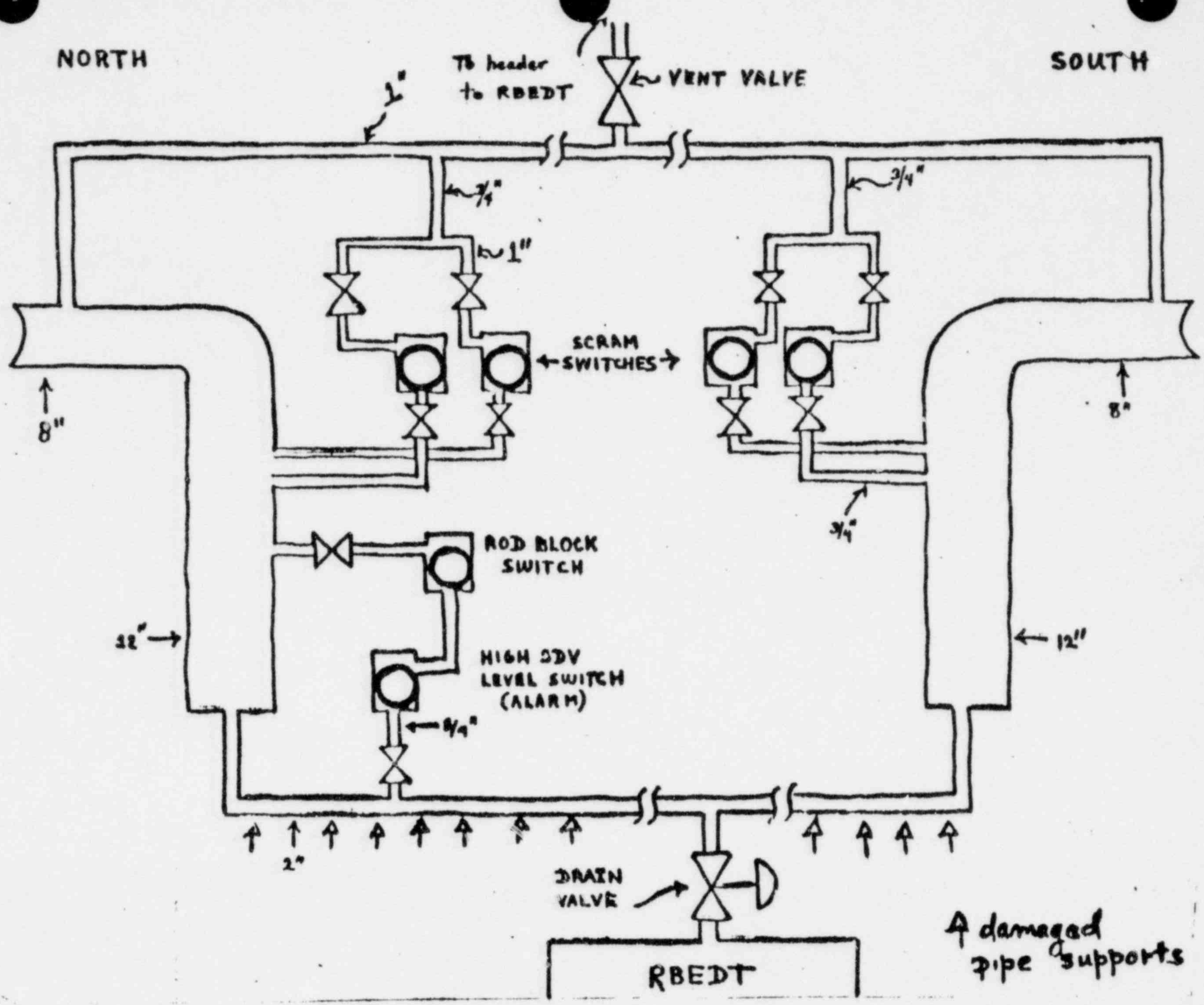
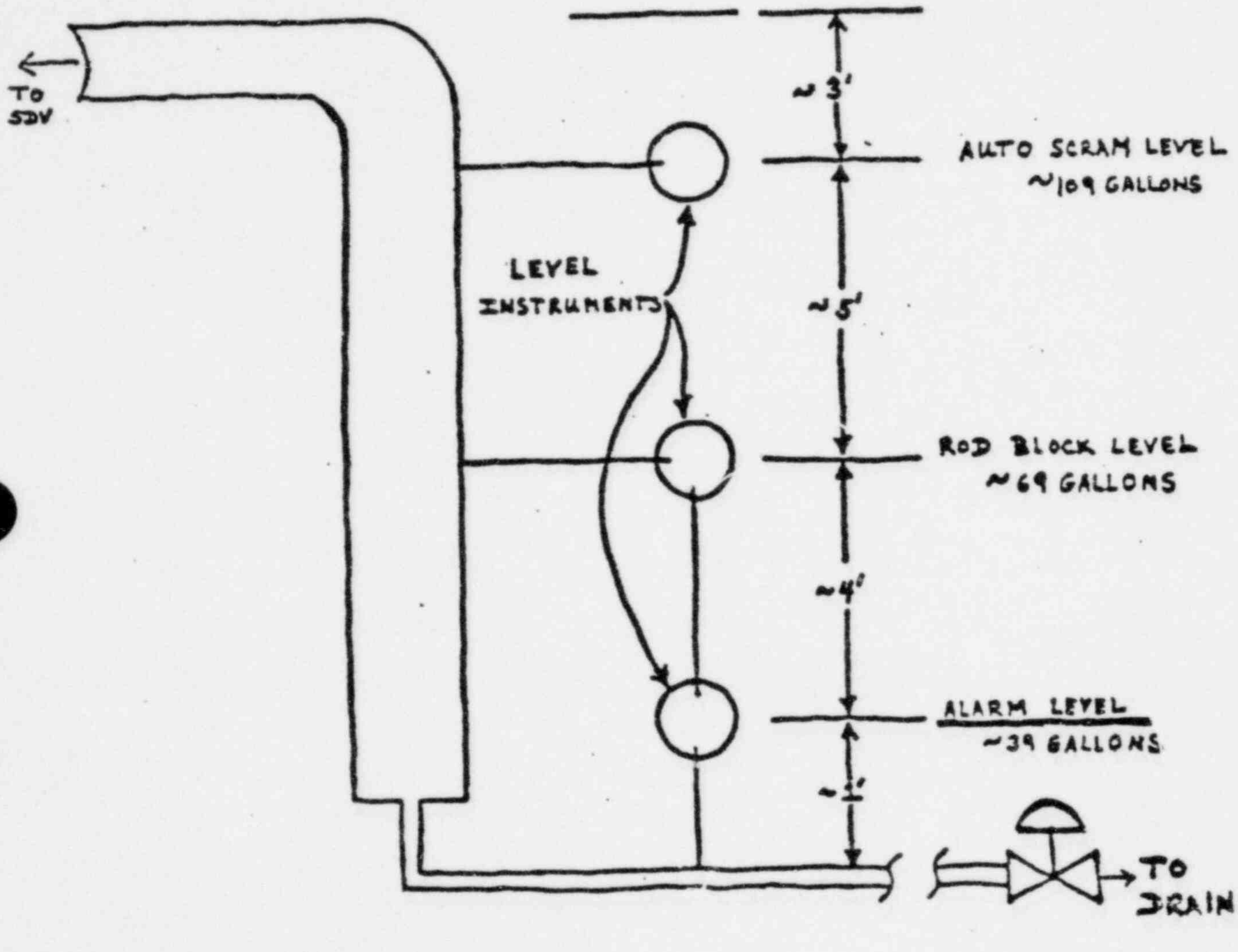


FIGURE 3.9 NORMAL DRAIN TEST

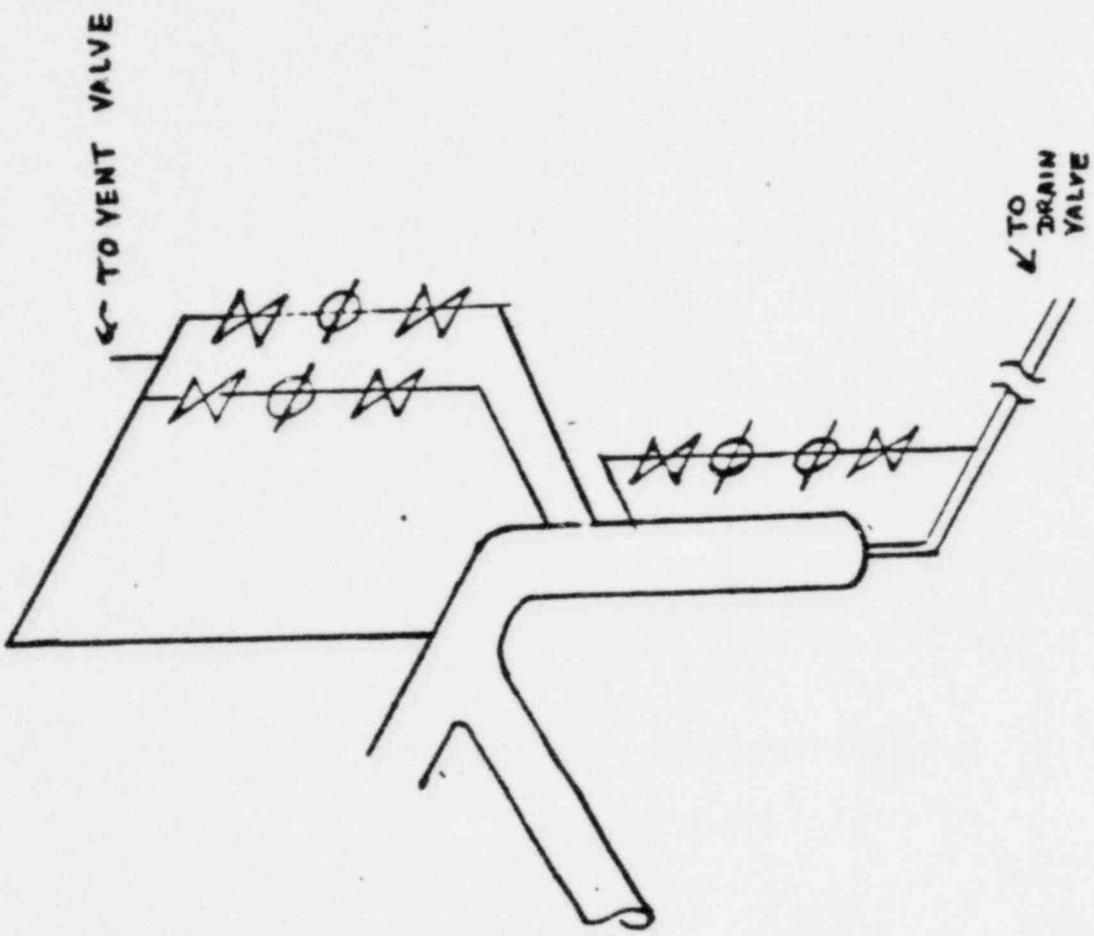
1


NORTH
SOUTH
To header to RBEDT
VENT VALVE
1"
3/4"
1"
3/4"
SCRAM SWITCHES
8"
8"
3/4"
ROD BLOCK SWITCH
12"
12"
HIGH SDV LEVEL SWITCH (ALARM)
3/4"
2"
DRAIN VALVE
RBEDT
A damaged pipe supports

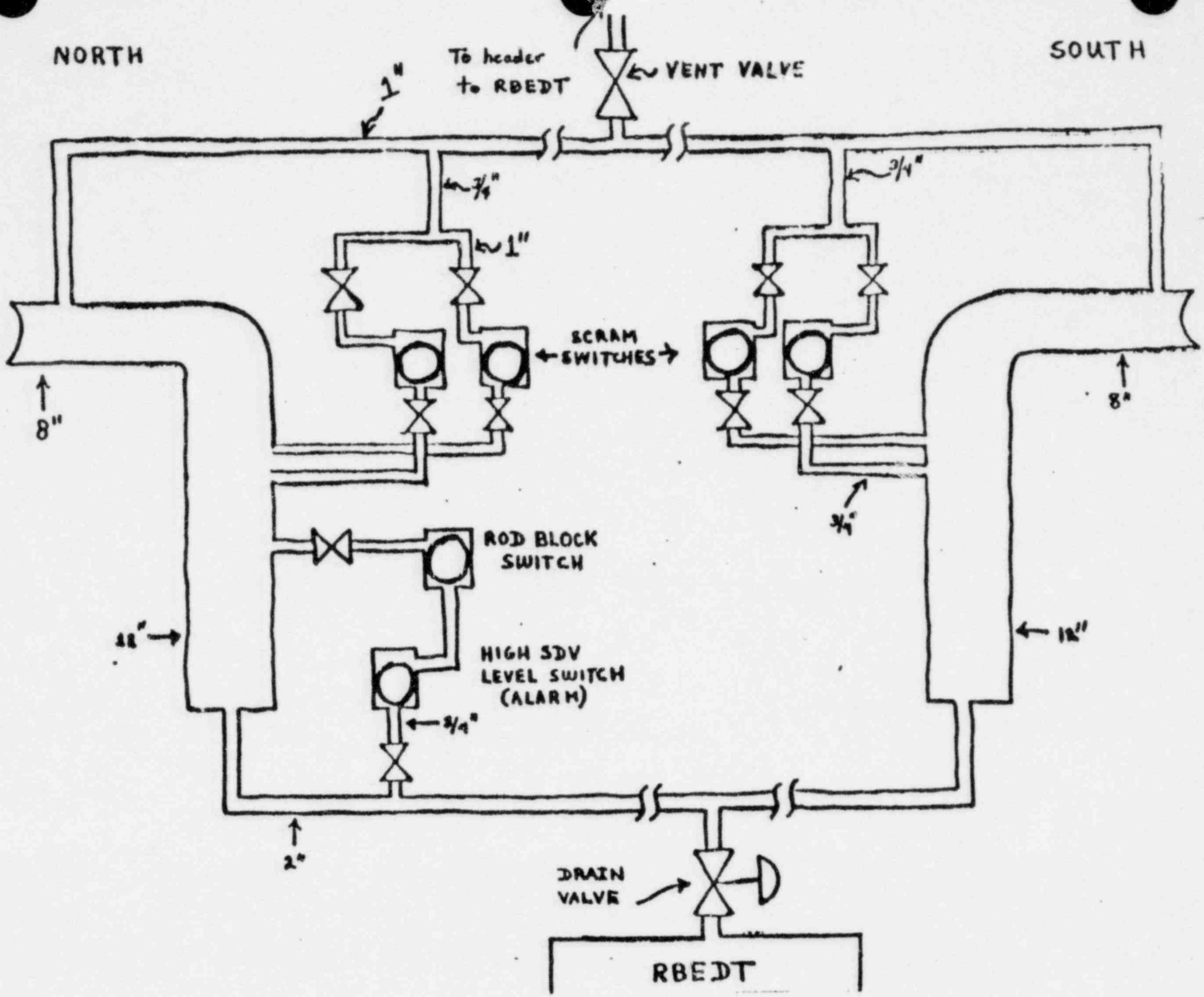


TO SDV
~3'
AUTO SCRAM LEVEL
~109 GALLONS
LEVEL INSTRUMENTS
~5'
ROD BLOCK LEVEL
~69 GALLONS
~4'
ALARM LEVEL
~39 GALLONS
~1'
TO DRAIN

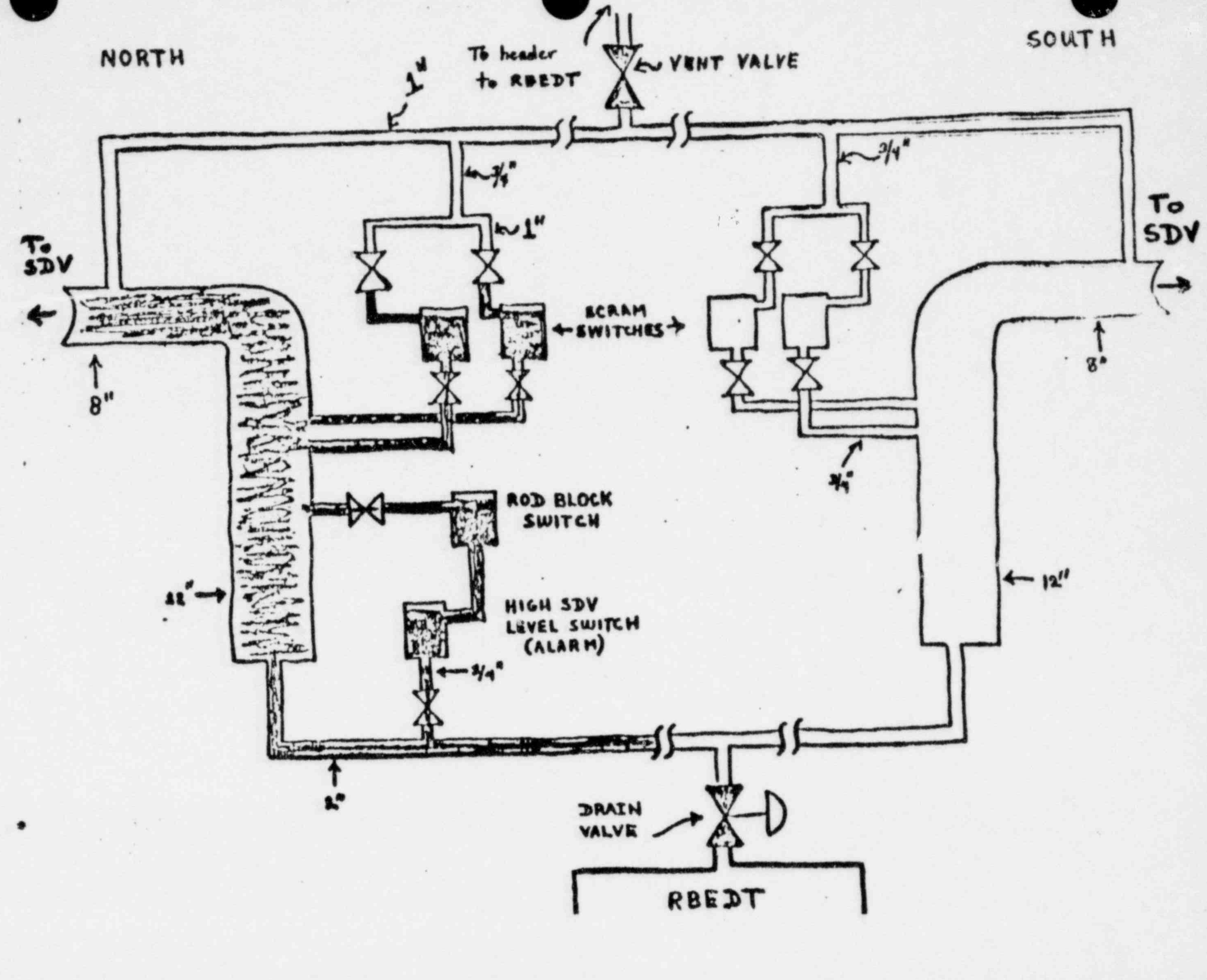
3



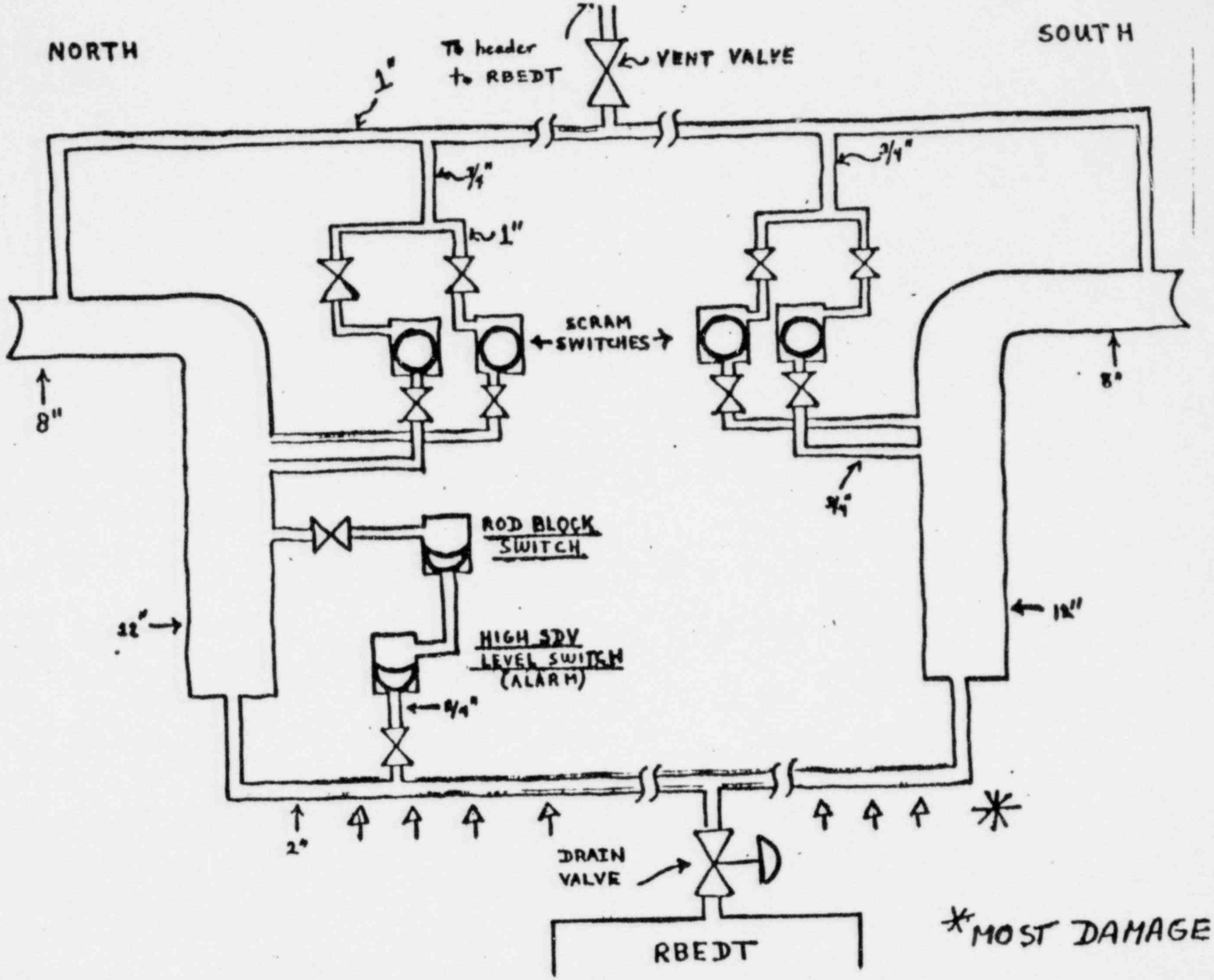
4



NORTH
SOUTH
To header
to RBEDT
VENT VALVE
1"
3/4"
1"
3/4"
SCRAM
SWITCHES
8"
8"
3/4"
ROD BLOCK
SWITCH
12"
12"
HIGH SDV
LEVEL SWITCH
(ALARM)
3/4"
2"
DRAIN
VALVE
RBEDT


NORTH
SOUTH
To header to RBEDT
VENT VALVE
1"
3/4"
1"
3/4"
To SDV
To SDV
8"
8"
SCRAM SWITCHES
3/4"
ROD BLOCK SWITCH
HIGH SDV LEVEL SWITCH (ALARM)
3/4"
12"
12"
2"
DRAIN VALVE
RBEDT

6.


NORTH
SOUTH
To header
to RBEDT
VENT VALVE
1"
3/4"
1"
3/4"
SCRAM
SWITCHES
8"
8"
3/4"
ROD BLOCK
SWITCH
HIGH SDV
LEVEL SWITCH
(ALARM)
3/4"
12"
12"
2"
DRAIN
VALVE
RBEDT
*MOST DAMAGE

AEOD FINDINGS

1. THE CAUSE OF THE PARTIAL SCRAM FAILURE WAS WATER ACCUMULATION IN THE EAST SDV.
2. THE SDIV "HIGH WATER LEVEL TRIP" DID NOT AND DOES NOT PROVIDE PROTECTION AGAINST FILLING THE EAST SDV EVEN FOR NORMAL VENTING AND DRAINING CONDITIONS.
3. A SINGLE FAILURE (E.G., WEST SIDE SDV VENT OR DRAIN LINE BLOCKAGE) CAN COMPLETELY DISABLE THE SDIV "HIGH WATER LEVEL TRIP" INSTALLED TO PROTECT AGAINST LOSS OF SCRAM CAPABILITY FOR THE CONTROL RODS.
4. WITH THE PRESENT SDV/SDIV LAYOUT, A SINGLE FAILURE (BLOCKAGE) OF AN SDV VENT OR DRAIN PATH CAN CAUSE A PARTIAL LOSS OF SCRAM CAPABILITY.
5. THERE ARE NUMEROUS ACTUAL AND POTENTIAL MECHANISMS FOR INTRODUCING AND ACCUMULATING WATER IN THE SDV'S WITH NO ACCUMULATION IN THE SDIV.
6. THE CURRENT SDV/SDIV LAYOUT RESULTS IN THE AUTOMATIC "HI WATER LEVEL TRIP" SAFETY FUNCTION BEING DIRECTLY DEPENDENT ON THE NON-SAFETY RELATED REACTOR BUILDING WASTE DRAIN SYSTEM.
7. THE FLOAT-TYPE WATER LEVEL MONITORING INSTRUMENTS ON THE SDIV HAVE A SIGNIFICANT DEGREE OF UNRELIABILITY.
8. THE CURRENT BWR RPS LOGIC DOES NOT ALLOW SCRAM RESET TO ATTEMPT A RE-SCRAM IF CERTAIN AUTOMATIC SCRAM SIGNALS ARE PRESENT.
9. FAILURE TO CLOSE OF A SINGLE SDV VENT OR DRAIN VALVE DURING A REACTOR SCRAM CAN RESULT IN A UNISOLATABLE RELEASE OF REACTOR COOLANT OUTSIDE THE PRIMARY CONTAINMENT INTO THE SECONDARY CONTAINMENT.
10. THE EMERGENCY OPERATING INSTRUCTIONS AT BROWNS FERRY DID NOT COVER A PARTIAL OR TOTAL SCRAM FAILURE EVENT.

AEOD RECOMMENDATIONS

1. THE OPERABILITY OF THE SDIV "HI WATER LEVEL TRIP" SHOULD BE INDEPENDENT OF THE VENTING AND DRAINING REQUIREMENTS.

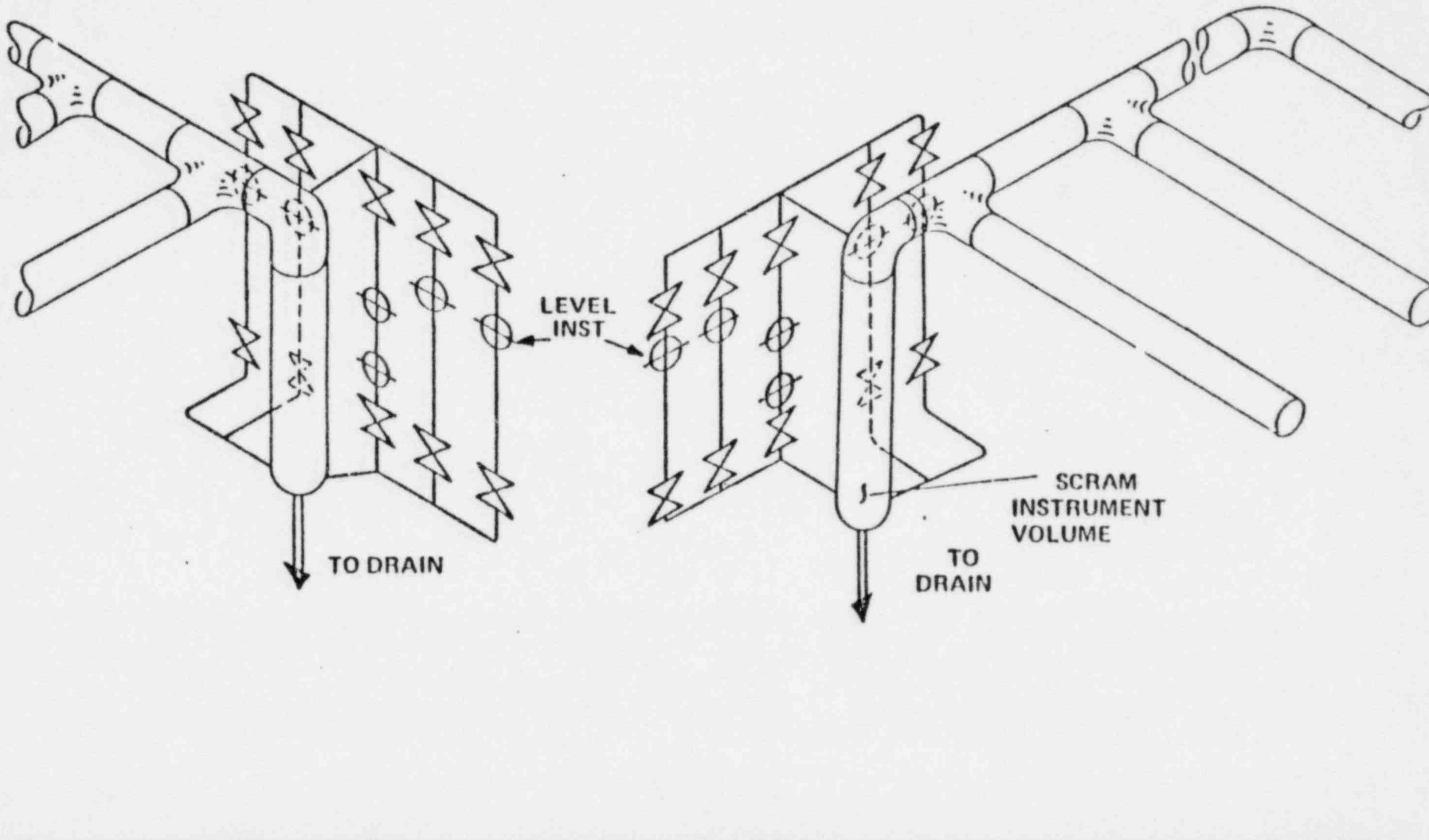
2. SDIV INSTRUMENTS SHOULD BE BOTH REDUNDANT AND DIVERSE.

3. ALL VENT AND DRAIN PATHS FROM THE SDV SHOULD HAVE REDUNDANT AUTOMATIC ISOLATION VALVES.

4. EMERGENCY OPERATING PROCEDURES AND OPERATOR TRAINING SHOULD BE PROVIDED FOR COMPLETE AND PARTIAL SCRAM FAILURE CONDITIONS.

5. CONSIDER MODIFYING THE SDV VENT AND DRAIN ARRANGEMENT TO IMPROVE DRAIN RELIABILITY.

GE RECOMMENDED SDV CONFIGURATION



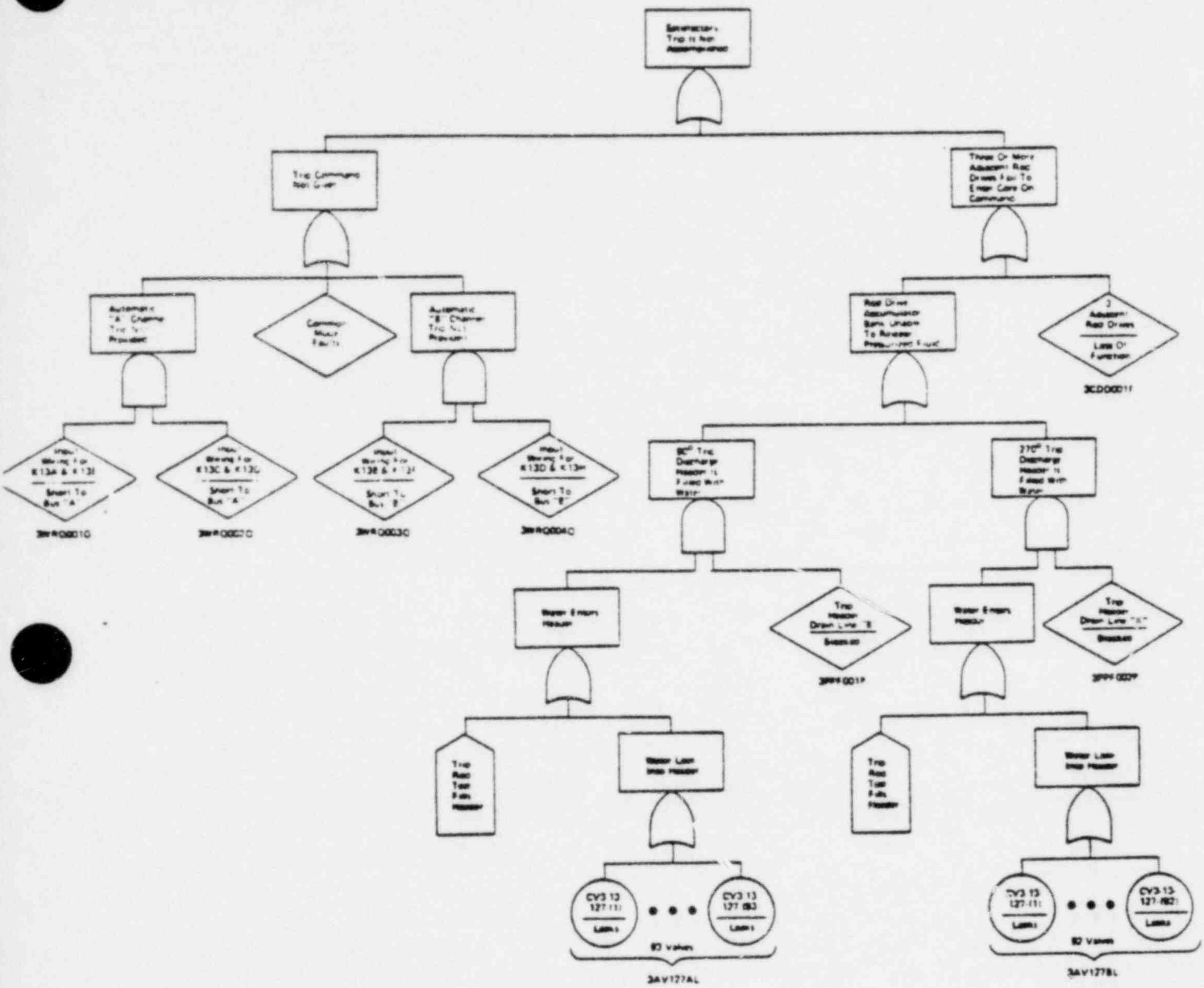


FIGURE II 6-16 Reactor Protection System Reduced Fault Tree

APPENDIX II - WASH-1400

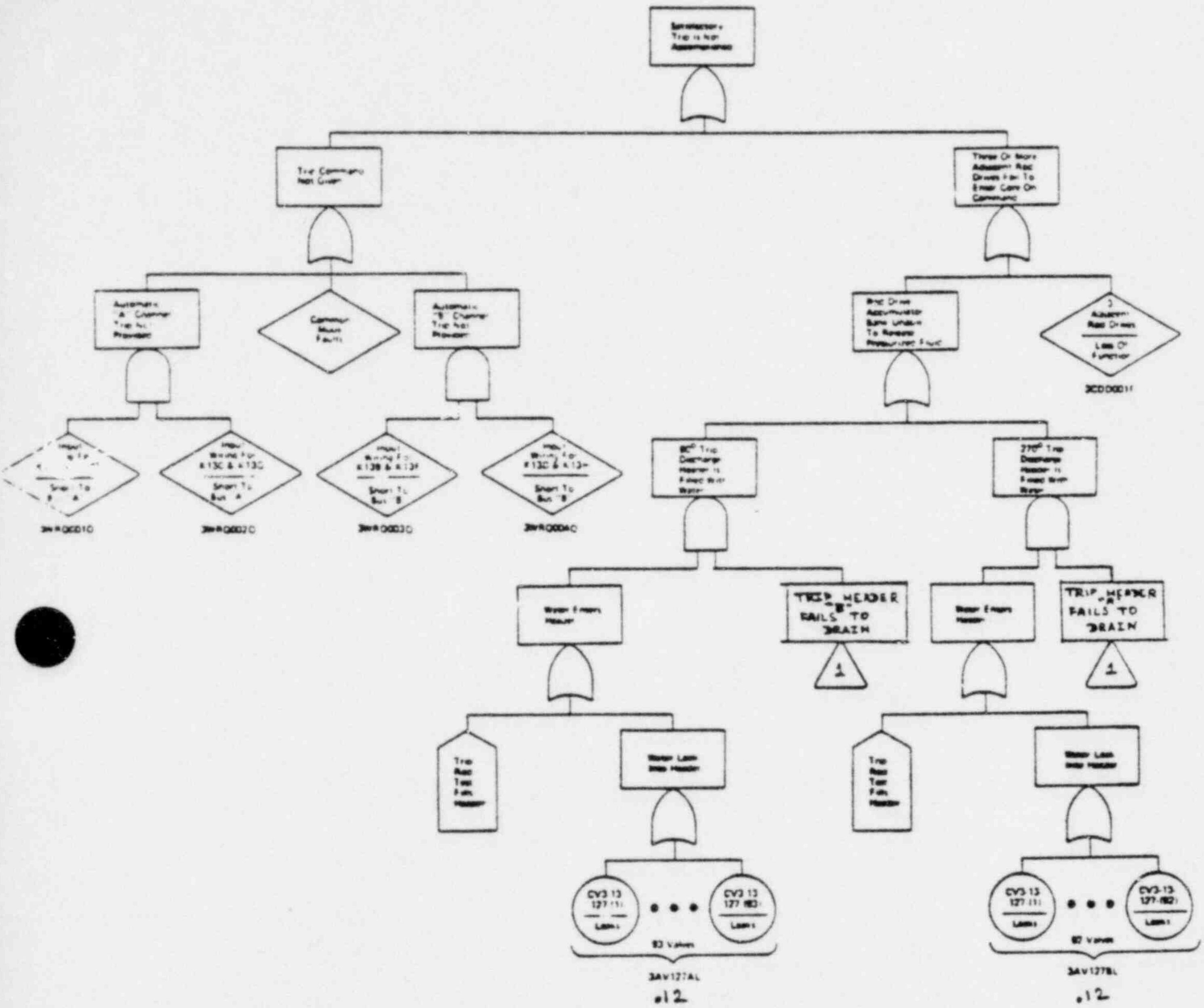
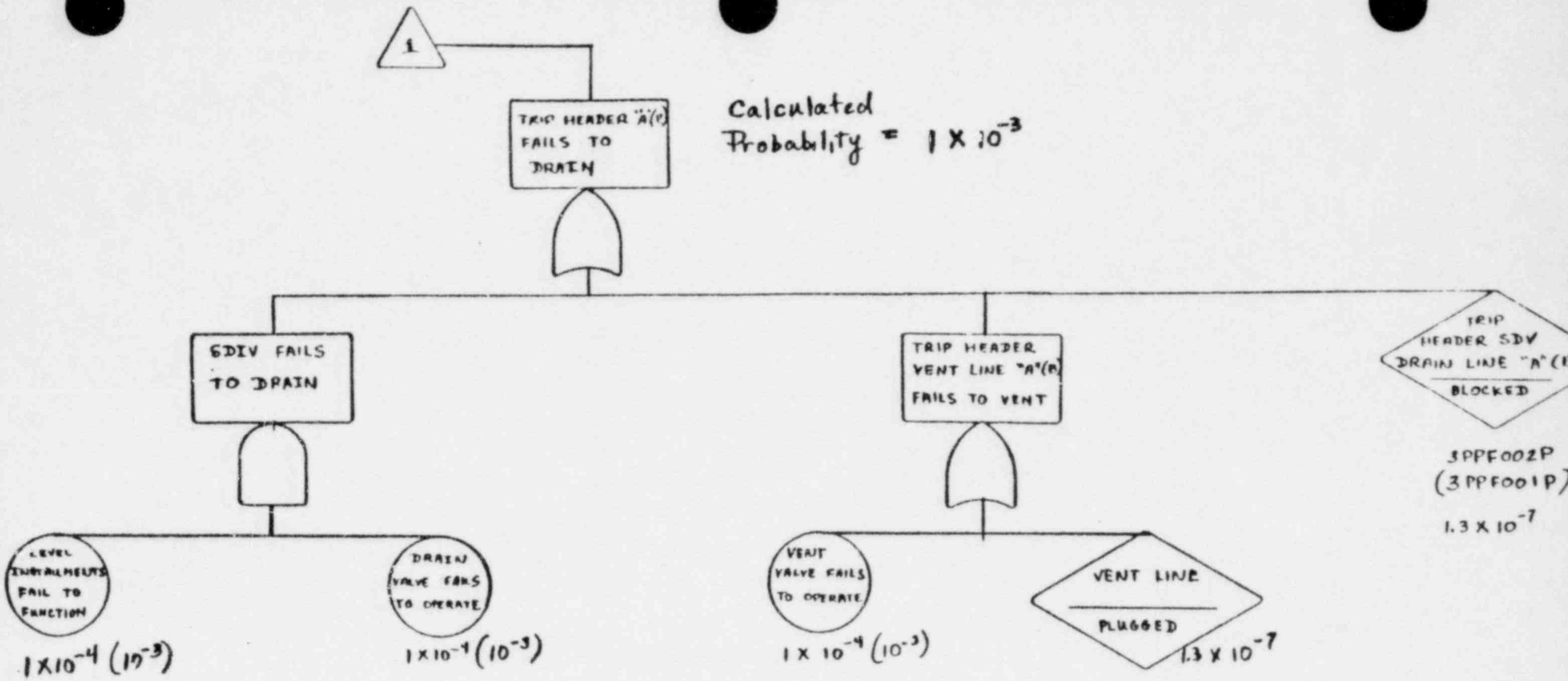


FIGURE II 6-16 Reactor Protection System Reduced Fault Tree

Adapted from Appendix II WASH-1400



Calculation: $(1\times10^{-3})(1\times10^{-3}) + [1\times10^{-3} + 1.3\times10^{-7}] + 1.3\times10^{-7} = 1\times10^{-3}$ Upper bound

$(1\times10^{-4})(1\times10^{-4}) + [1\times10^{-4} + 1.3\times10^{-7}] + 1.3\times10^{-7} = 1\times10^{-4}$ Lower bound

SHORT TERM REVIEW PROGRAM

OBJECTIVES

- EVALUATE FUNCTIONABILITY AND DURABILITY OF THE GLOW PLUG IGNITER
- EVALUATE EFFICACY OF THE PROPOSED IDIS IN IMPROVING HYDROGEN CONTROL CAPABILITY
- ASSURE NO SIGNIFICANT LOSS IN SAFETY BY USE OF THE PROPOSED IDIS
- INVESTIGATE H_2 CONTROL FEATURES ALTERNATIVE TO IDIS

APPROACH TO ESTABLISH
FUNCTIONABILITY/DURABILITY OF IGNITERS

NRC

- TESTING OF THE GLOW PLUG AT LIVERMORE LABORATORY ADDRESSING OPERABILITY IN VARIOUS ENVIRONMENTS
- COMPLETED OCTOBER 31

TVA

- DURABILITY TESTING AT TVA SINGLETON LABORATORY
  - 148 HR CONTINUOUS TEST HAS BEEN SUCCESSFULLY COMPLETED
  - OPERABILITY ESTABLISHED WITH 1720°F AT 14V
- FUNCTIONABILITY TESTING AT SINGLETON LABORATORY
  - SERIES OF TESTS DEMONSTRATED COMPLETE COMBUSTION OF H_2 IN 12-14% MIXTURES
- TESTING OF THE IGNITER UNIT AT FENWALL LABORATORY
  - TEST MATRIX OF VARYING ATMOSPHERE COMPOSITIONS AND TURBULENCE
  - COMPLETED OCTOBER 1
  - FURTHER TESTING WILL CONTINUE INCLUDING EFFECTS OF SPRAYS

RESULT: NRC WILL EVALUATE IGNITER PERFORMANCE BY REVIEW OF INDIVIDUAL TEST DATA AND COLLECTIVE ASSESSMENT OF RESULTS.

APPROACH TO EVALUATE EFFICACY OF IDIS IN IMPROVING H_2 CONTROL CAPABILITY

- GENERAL APPROACH IS FOUNDED ON EVALUATION OF CONTAINMENT TRANSIENT ANALYSIS FOR DEGRADED CORE ACCIDENTS.

- RANGE OF DEGRADED CORE ACCIDENTS TO CONSIDER WILL BE SELECTED BY TVA AND REVIEWED FOR ACCEPTABILITY BY THE STAFF.

- TVA WILL PERFORM ANALYSIS (USING CLASIX) FOR THESE ACCIDENTS DEMONSTRATING THAT CONTAINMENT PRESSURE IS LESS THAN A PRESELECTED VALUE.
  - YIELD STRENGTH/ULTIMATE STRENGTH
  - IMPUT TO THE CALCULATION WILL BE IGNITER PERFORMANCE PARAMETERS VERIFIED BY TESTING

- TVA WILL COMPLETE INITIAL VERIFICATION OF CLASIX CODE AND REPORT RESULTS.

- NRC WILL PERFORM CONFIRMATORY ANALYSES THROUGH BCL USING MARCH.

RESULT: NRC WILL EVALUATE CLASIX CODE AND THE ANALYSIS RESULTS.

APPROACH TO ASSURE NO SIGNIFICANT LOSS IN SAFETY BY USE OF THE IDIS

TVA

- TVA WILL EVALUATE RANDOM IGNITION SOURCES FOR COMPARISON TO IDIS.
- LOCATION OF IGNITERS WILL BE EVALUATED TO SEE THAT BURNING IN IMMEDIATE AREA WILL NOT IMPAIR ESSENTIAL EQUIPMENT.
- POTENTIAL FOR AND CONSEQUENCES OF LOCAL DETONATIONS WILL BE ASSESSED.

STAFF WILL REVIEW TVA SUBMITTALS AND WILL INDEPENDENTLY ASSESS EFFECTS OF LOCAL DETONATIONS.

H_2 RELATED ACTIVITIES

1. TECHNICAL ASSISTANCE CONTRACT A0249
2. TECHNICAL ASSISTANCE CONTRACT
3. TECHNICAL ASSISTANCE CONTRACT
4. SHORT TERM RESEARCH ON H_2 CONTROL
5. RESEARCH ON DEGRADED MELTED CORE ACCIDENTS
6. ZION/INDIAN POINT STUDIES (I)
7. ZION/INDIAN POINT STUDIES (II)
8. ZION/INDIAN POINT STUDIES (III) TECHNICAL ASSISTANCE
9. IGNITER TESTING
10. H_2 CONTROL
11. H_2 GENERATION AND CONTROL

TABLE 1. PRELIMINARY CONTAINMENT ANALYSIS SENSITIVITY STUDIES

	TOTAL H_2 BURNED (LB)	PEAK TEMP. (^oF) LOWER COMPARTMENT	ICE BED	UPPER COMP.	PEAK PRESS (PSIA) LOWER COMP.	UPPER COMP.
1. BASE CASE	900	2200	1200	150	26.5	28.5
2. H_2 IGNITION AND PROPAGATION @ 8%	1050	1200	700	260	28.5	30.5
3. 1 AIR FAN	900	2200	1350	160	26.5	29.5
4. NO ICE*	850	2400	2000	270	41	41
5. NO AIR FANS	1200	2370	2580	1090	46.4	92.4

* ICE EXISTS ONLY FOR THE FIRST TWO OF 7 BURNING CYCLES.

SUMMARY OF RESULTS

Case	Ignition Point v/o H_2	Burn Limit v/o H_2	Burn Time, sec	Flame Propagation 1 ⇄ 2	PT psia (1)	PNEW psia (2)	PMAX psia (3)
1	10	0	1	no	23	58	141
2	10	0	5	no	23	58	136
3	10	0	25	no	22	58	131
1X	10	0	1	yes	44	58	126
1X (5)	10	0	1	yes	53	66	150
4	10	4	1	no	22	44	122
5	10	4	25	no	22	44	114
6	12	0	1	no	24	64	141
7	12	0	25	no	23	64	137
6X (5)	12	0	1	yes	60	71	181
8	8	0	1	no	22	51	132
9	8	0	25	no	22	51	127
10	8	4	1	no	22	36	120
11	8	4	25	no	21	36	110
10X	8	4	1	yes	27	36	112
18	4	0	1	yes	24	41	111
17 (4)	10	0	1	no	31	79	146
18 (4,5)	10	0	1	no	35	68	223
19 (4,5)	10	0	1	yes	50	66	149

(1) Peak pressure prior to core slump
(2) Peak adiabatic H_2 burn pressure prior to core slump
(3) Peak containment pressure after head failure
(4) Ice melt complete at 21 minutes
(5) Modified treatment of suspended water droplets

VERIFICATION OF CLASIX

- INITIAL ASSESSMENT COMPLETED.
- BASED ON COMPARISON OF RESULTS WITH DESIGN CODES
  - COCO (COCOCLASS 9) - DRY CONTAINMENT CODE USED TO MODEL H_2 BURNING IN ZION/INDIAN POINT STUDY
  - TMD - SHORT TERM TRANSIENT ICE CONDENSER CODE
- CLOSE AGREEMENT OF CLASIX VS TMD RESULTS AND CLASIX VS COCOCLASS 9 RESULTS
- CLASIX RESULTS ARE REASONABLE/CONSERVATIVE.
- FUTURE WORK
  - COMPARISON OF RESULTS WITH LOTIC CODE (LONG TERM TRANSIENT ICE CONDENSER CODE)
  - COMPARISON WITH TEST DATA (FENWALL)

SHAKEDOWN TESTING OF FENWAL LABORATORY TEST FACILITY WITH SEQUOYAH INTERIM DISTRIBUTED IGNITION SYSTEM IGNITER UNIT

TEST NO.[1]	HYDROGEN CONCENTRATION (V/O)	IGNITION TIME (SEC)[2]	PEAK PRESSURE (PSIG)	TIME TO PEAK PRESSURE (SEC)	PEAK TEMPERATURE OF VESSEL[3]	PEAK TEMPERATURE OF IGNITER UNIT OUTER SURFACES[3]
1	8	17	3.2	33	NO MEASURABLE INCREASE	310
2	9	16	41	3.1	425	600+[4]
3	10	16	50[5]	1.075	620	930
4[6]	10	16	48	1.05	630	850
5	12	15	67	0.5		

NOTE: POST-BURN ANALYSIS OF VESSEL ATMOSPHERE NOT DONE FOR TEST FACILITY SHAKEDOWN TESTING, TO DETERMINE CONSTITUENT CONCENTRATIONS.

(1) HYDROGEN/AIR MIXTURES AT AMBIENT PRESSURE AND TEMPERATURE.

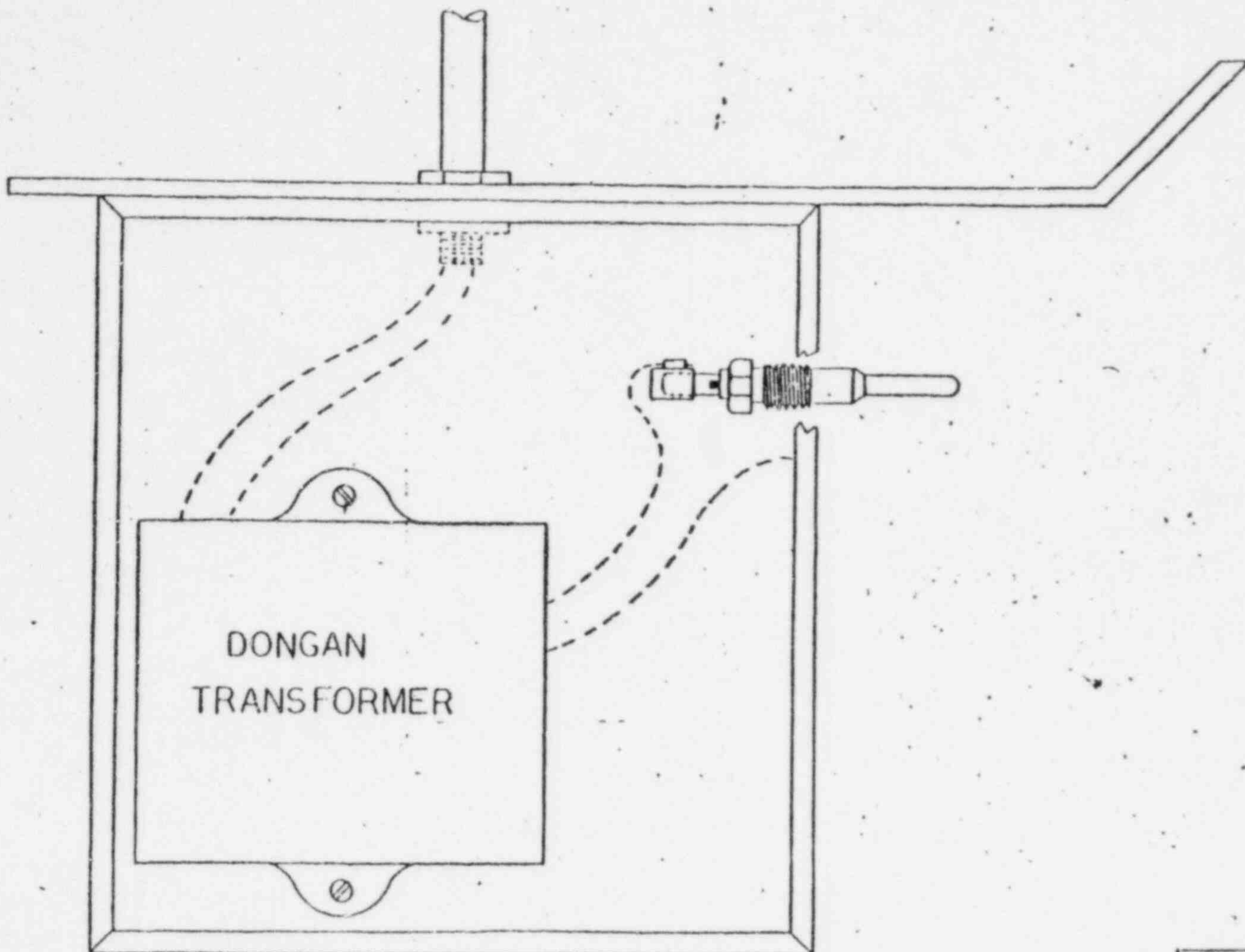
(2) ELAPSED TIME FROM ENERGIZING GLOW PLUG TO FIRST INDICATION OF PRESSURE RISE.

(3) IGNITER AND VESSEL SURFACE TEMPERATURES MAY NOT BE TOO MEANINGFUL SINCE THERMOCOUPLES ARE PRESENTLY TAPED TO SURFACES.

(4) SPECULATED THAT THERMOCOUPLE LIFTED FROM SURFACE SLIGHTLY AND MEASURED VESSEL ATMOSPHERE TEMPERATURE SINCE THE TEMPERATURE READING JUMPED TO 1260°F.

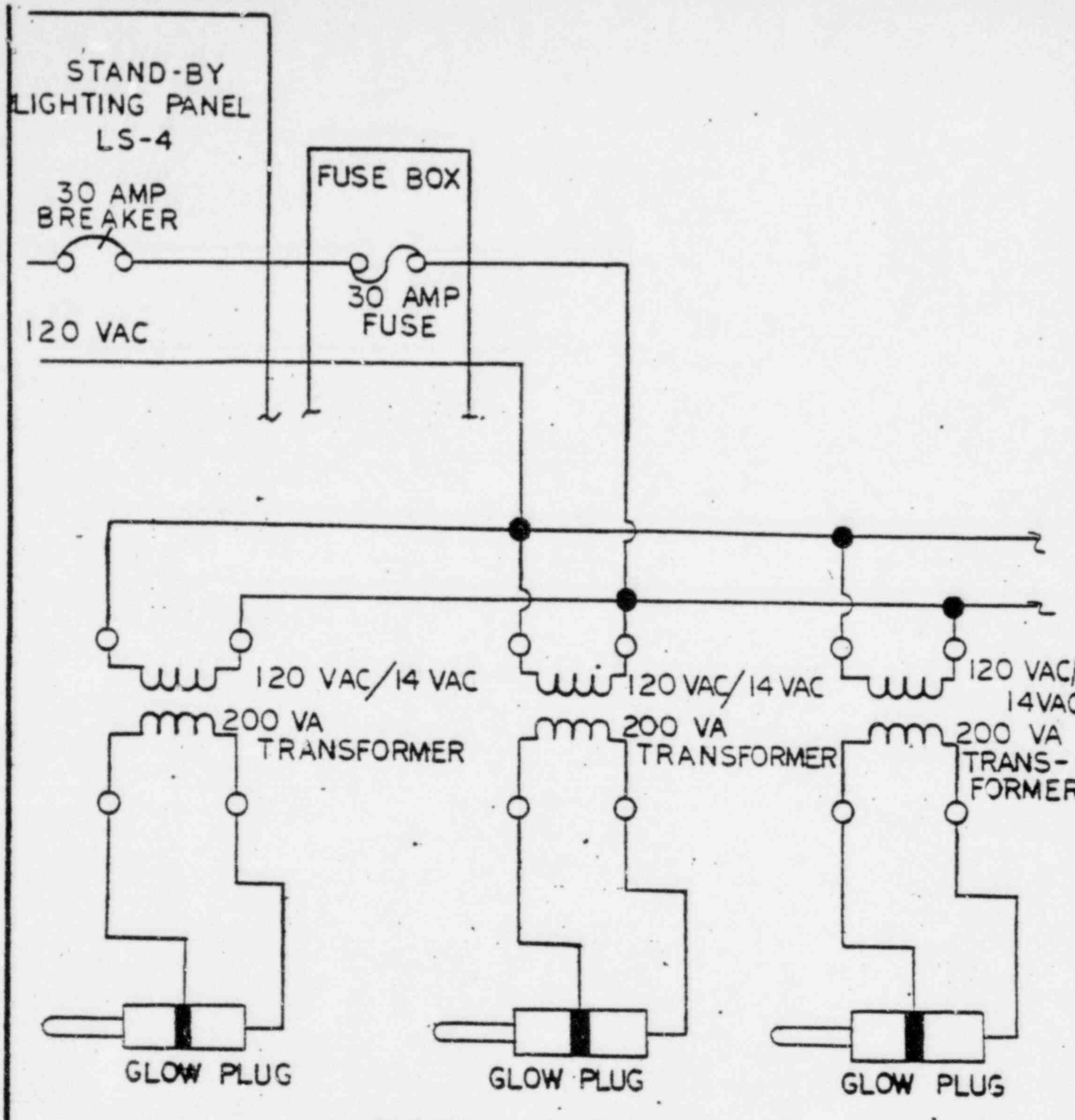
(5) PRESSURE DECAYED TO 25 PSIG IN 3.2 SEC; EQUILIBRIUM PRESSURE WAS 10 PSIG.

(6) RESULTS WERE ESSENTIALLY IDENTICAL TO TEST NO. 3.



SEQUOYAH NUCLEAR PLANT
FINAL SAFETY
ANALYSIS REPORT

PRELIMINARY PHASE I
MOUNTING DETAIL
FIGURE 6.2-147



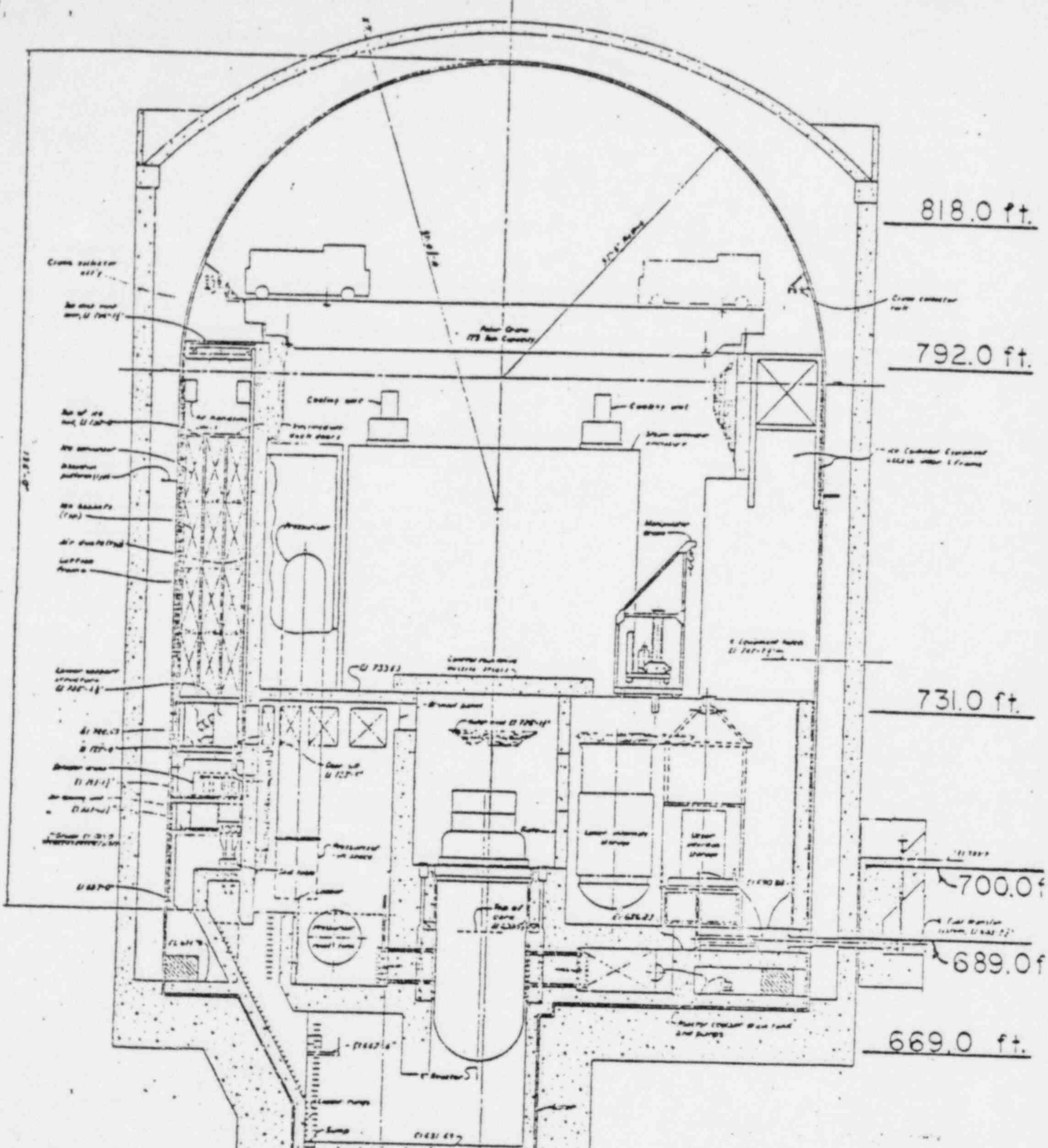
- TYPICAL OF ONE CIRCUIT

(NUMBER OF GLOW PLUGS PER CIRCUIT VARIES FROM ONE TO EIGHT)

ELECTRICAL SCHEMATIC
FIGURE 15

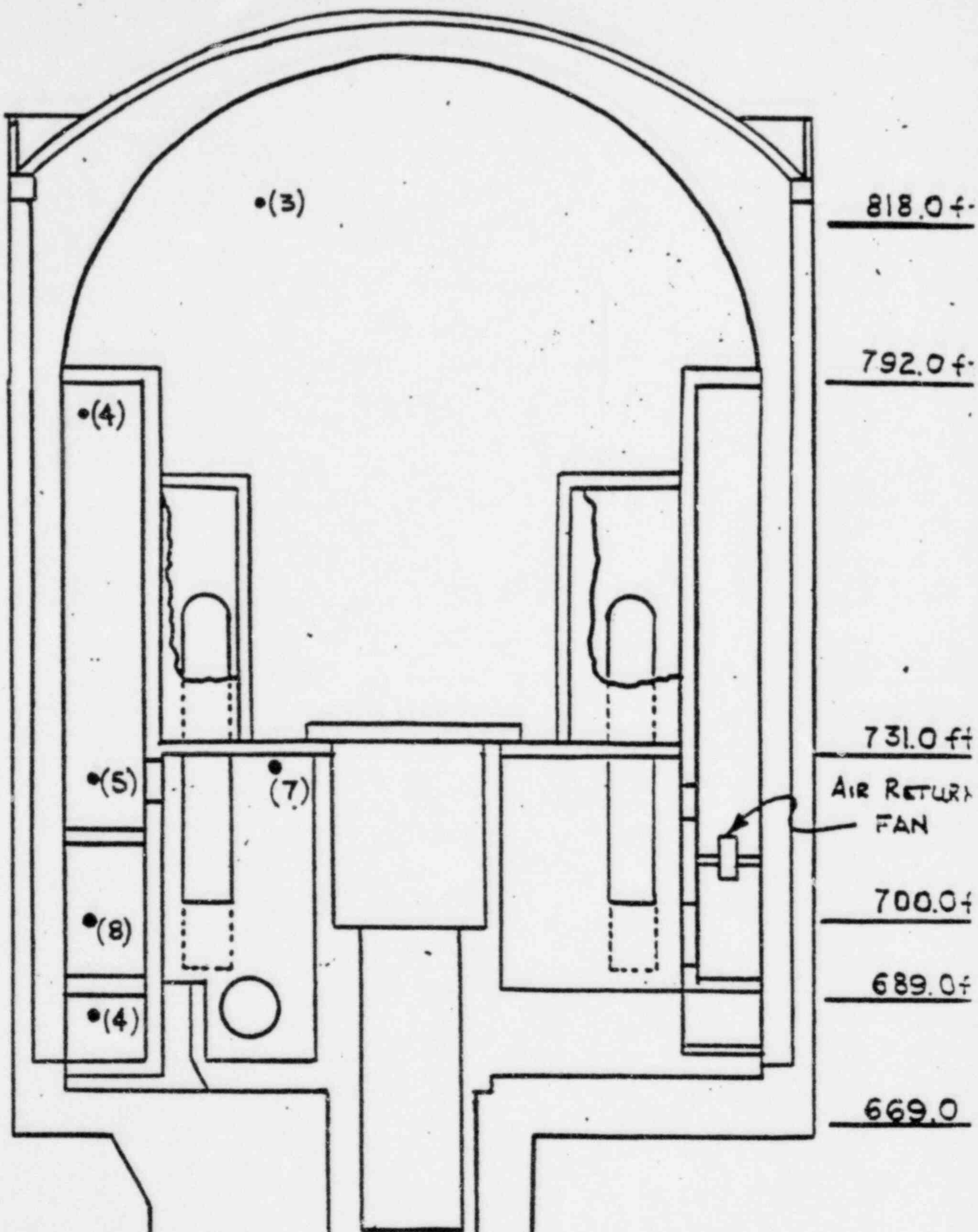
TENNESSEE VALLEY AUTHORITY
DIVISION OF ENGINEERING DESIGN

SUBMITTED	RECOMMENDED	APPROVED

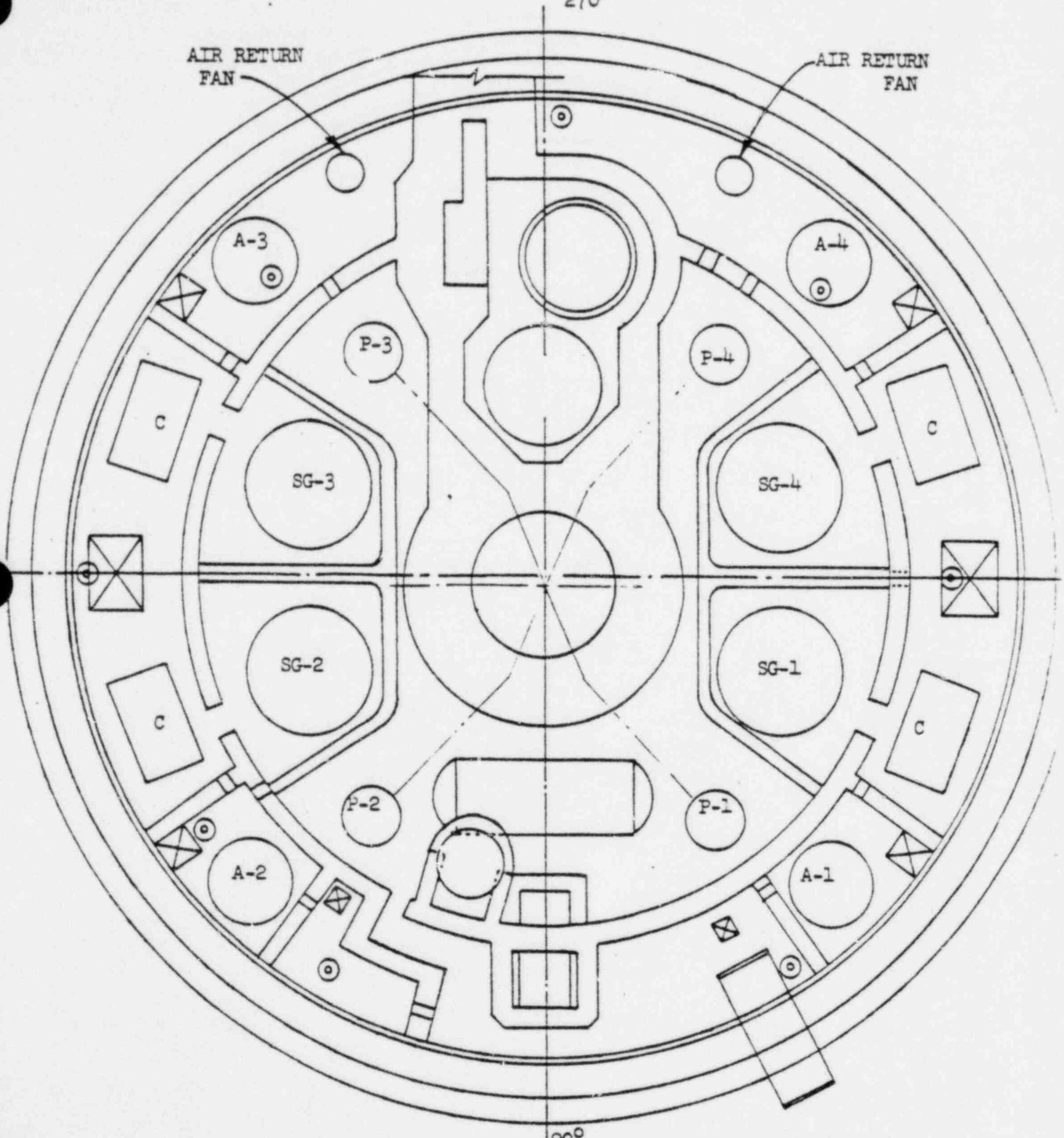


SEQUOYAH NUCLEAR PLANT
FINAL SAFETY
ANALYSIS REPORT

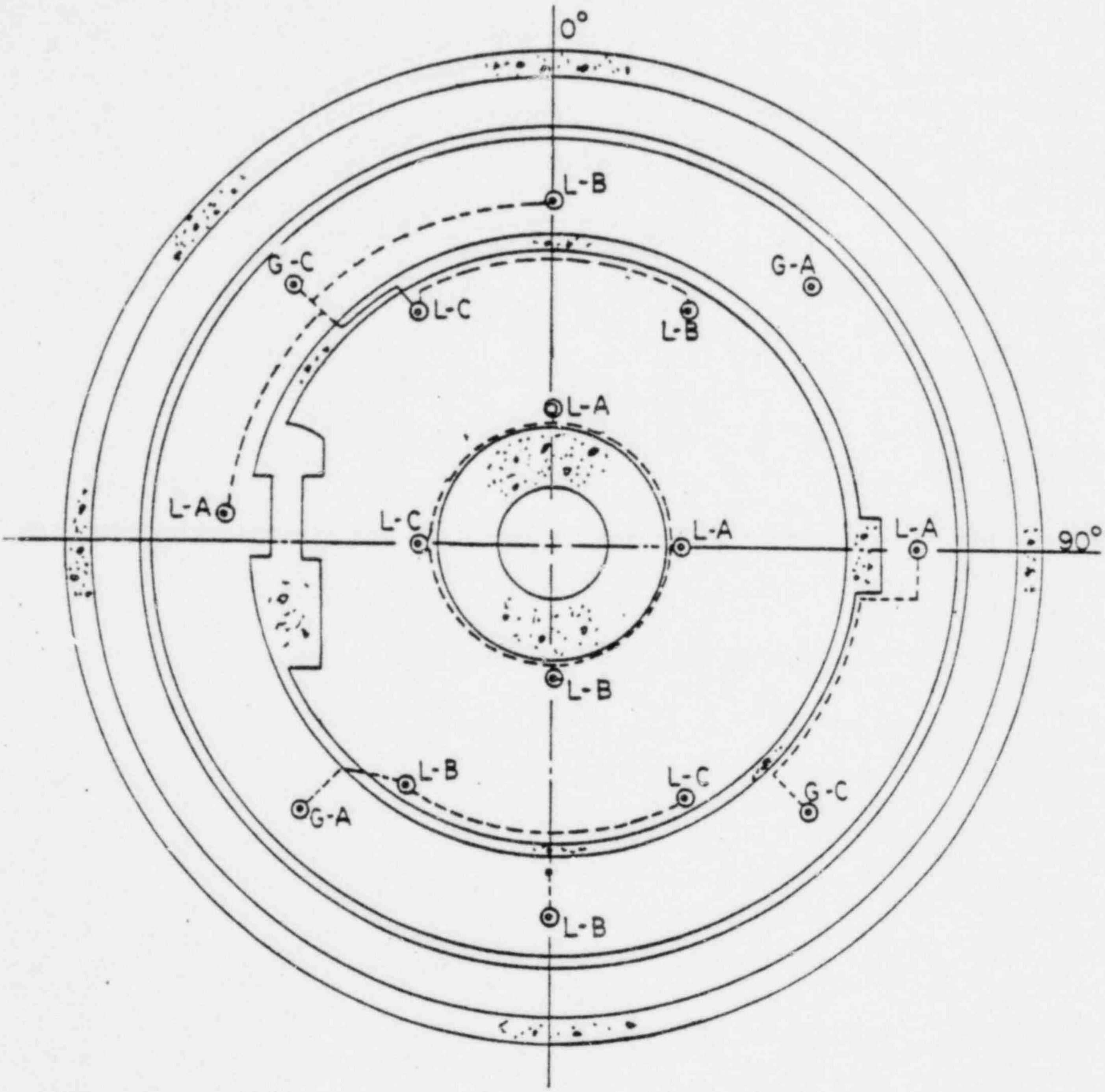
CONTAINMENT LIGHTING
FIXTURE ELEVATION
FIGURE 6.2-141



•(3)
818.0 ft
792.0 ft
•(4)
731.0 ft
•(5)
(7)
AIR RETURN
FAN
700.0 ft
•(8)
689.0 ft
•(4)
669.0

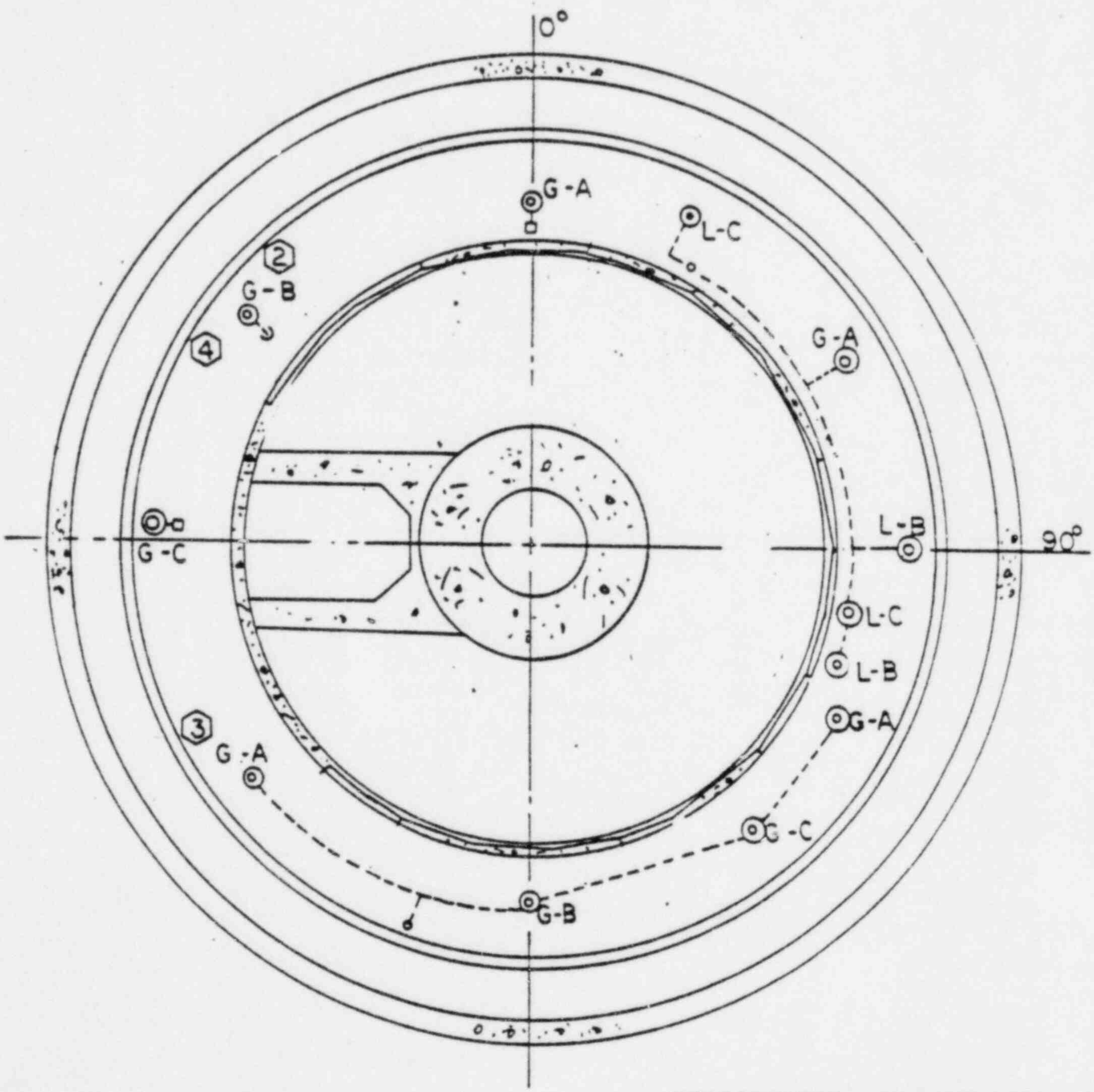


A= ACCUMULATOR
C= COOLING AIR UNIT
P= REACTOR COOLANT PUMP
SG= STEAM GENERATOR
⊠ = HATCH OPENINGS
◎ = IGNITER LOCATIONS



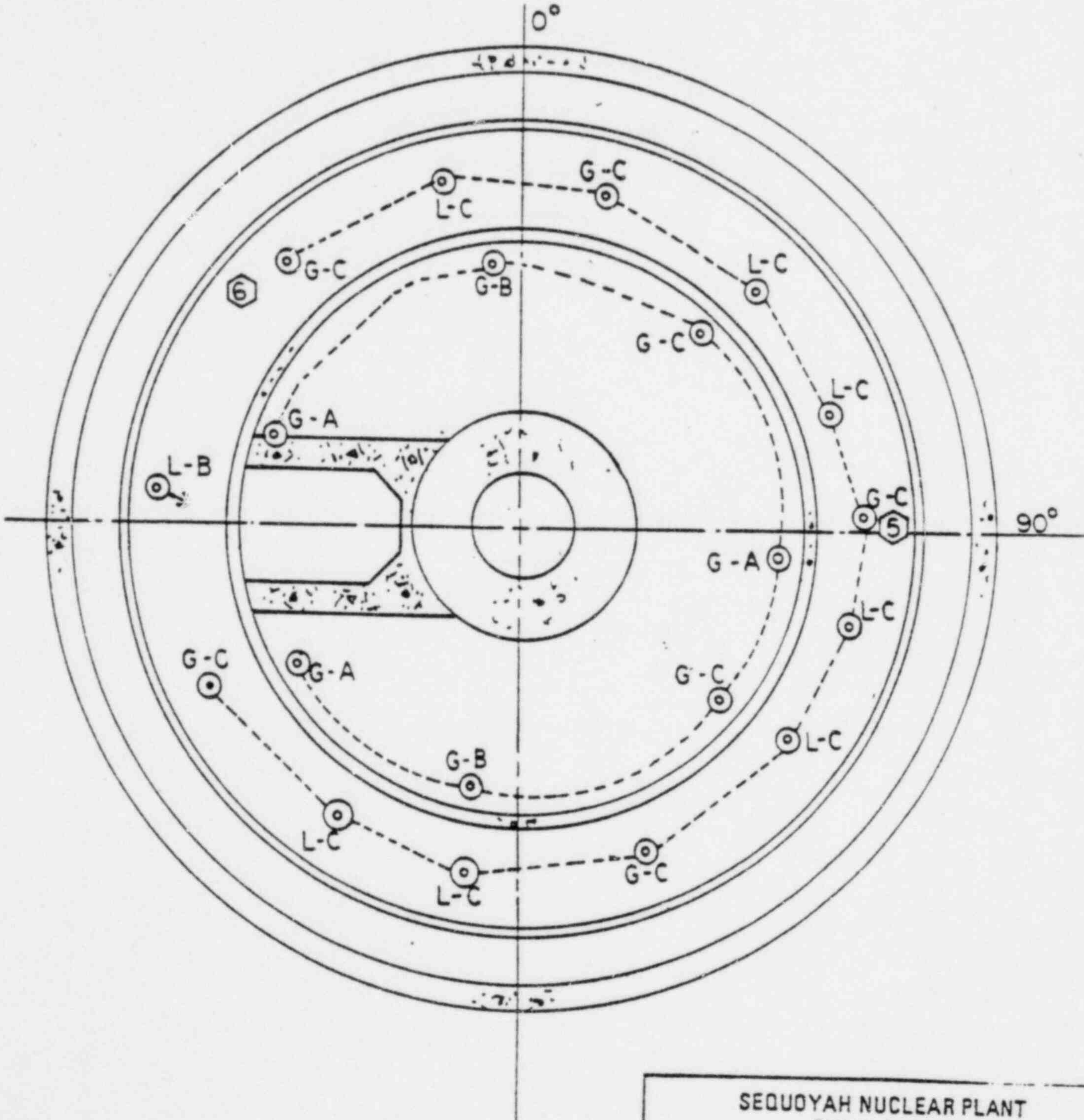
SEQUOYAH NUCLEAR PLANT
FINAL SAFETY
ANALYSIS REPORT

CONTAINMENT LIGHTING FIXTURES
EL. 689.0'
FIGURE 6.2-142



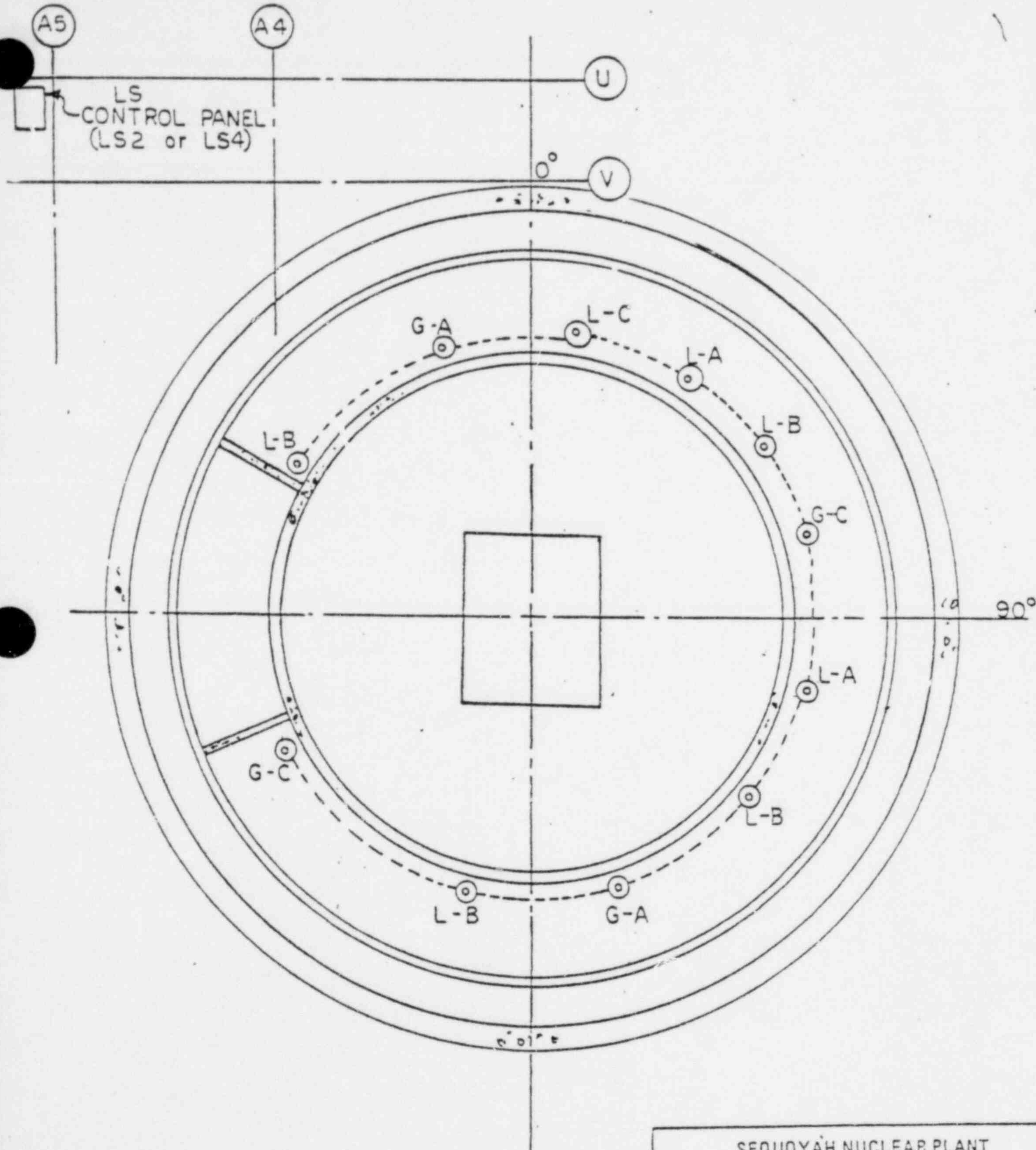
SEQUOYAH NUCLEAR PLANT
FINAL SAFETY
ANALYSIS REPORT

CONTAINMENT LIGHTING FIXTURES
EL. 700.3
FIGURE 6.2-143



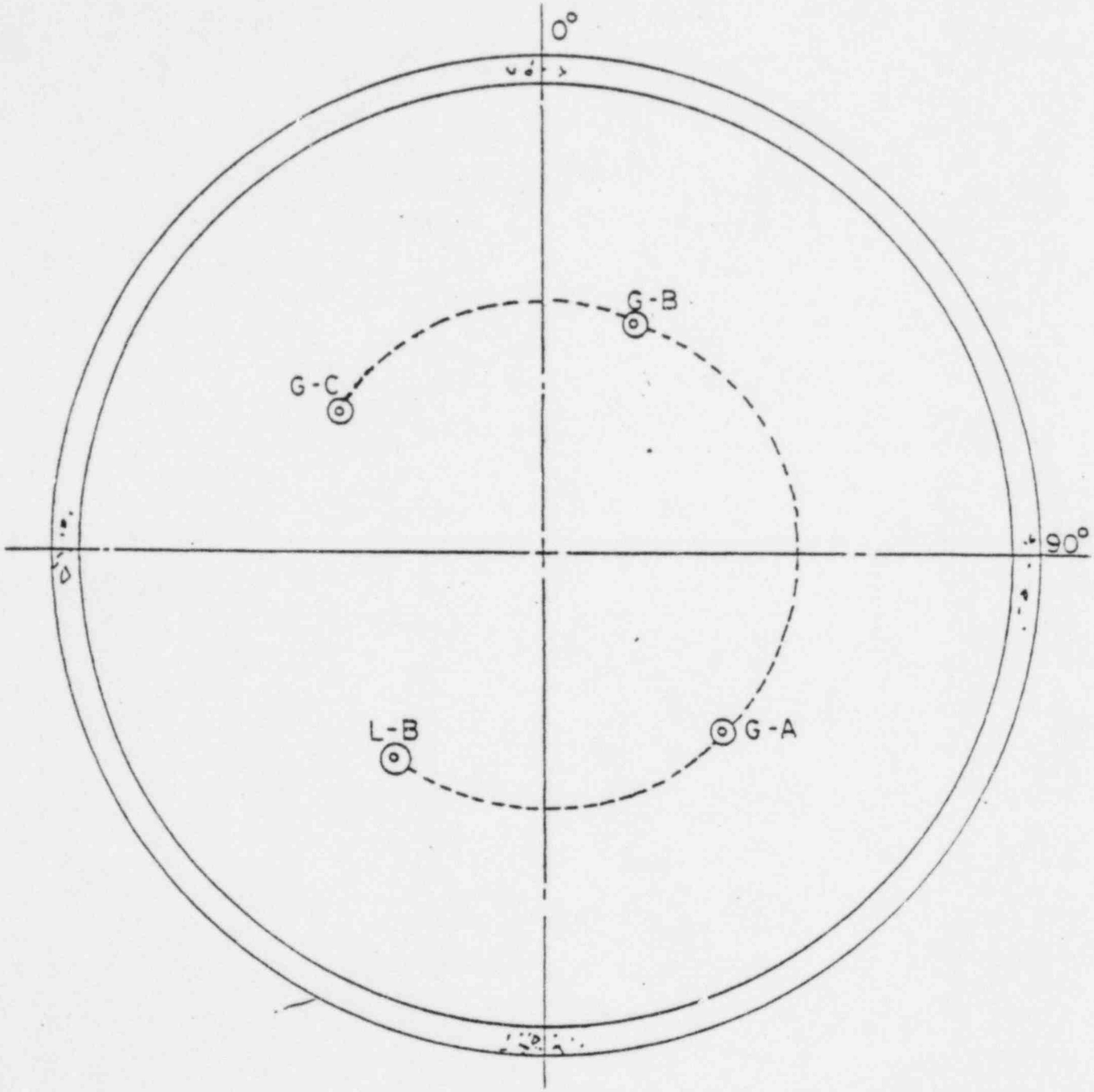
SEQUOYAH NUCLEAR PLANT
FINAL SAFETY
ANALYSIS REPORT

CONTAINMENT LIGHTING FIXTURES
EL 731.0'
FIGURE 6.2-144



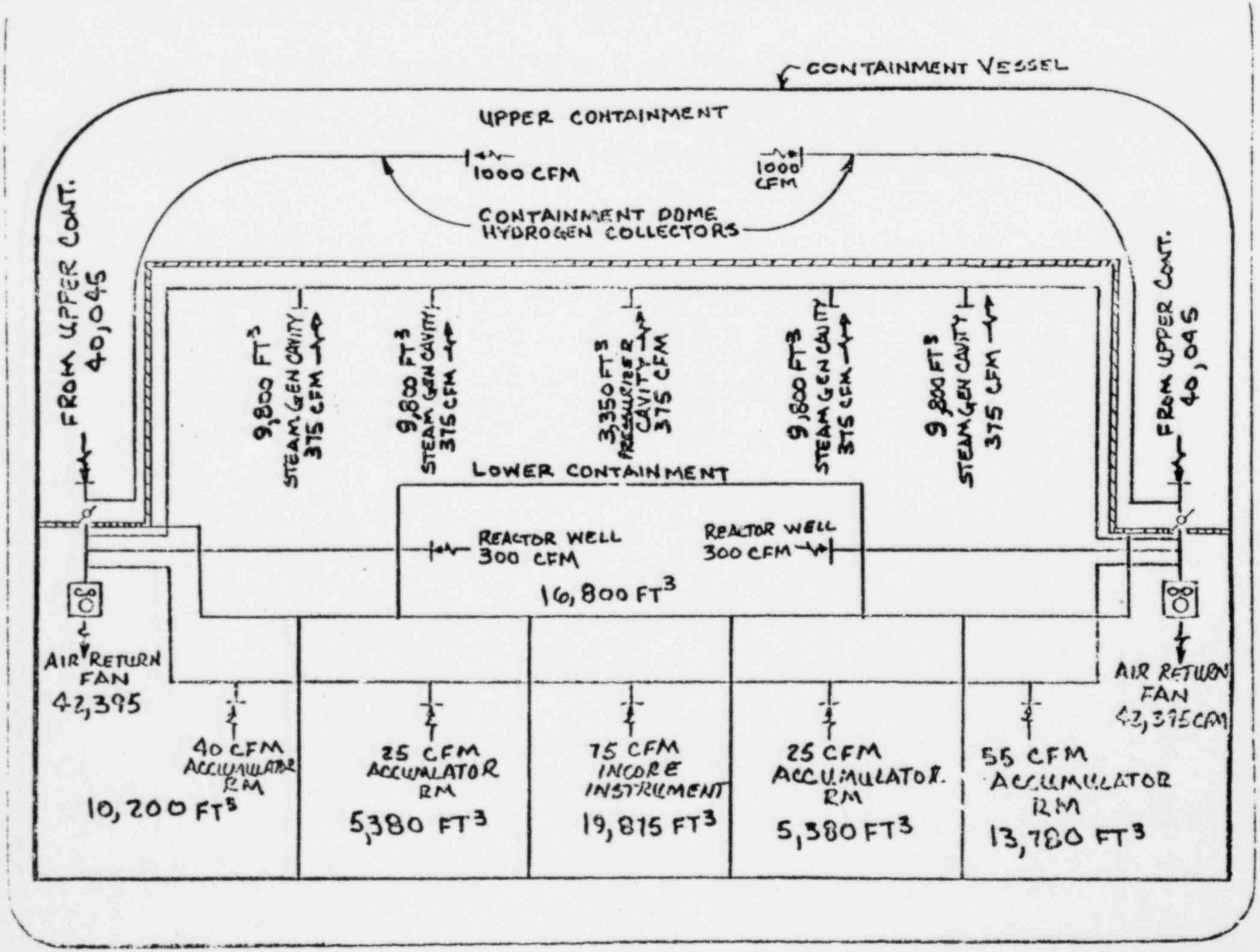
SEQUOYAH NUCLEAR PLANT
FINAL SAFETY
ANALYSIS REPORT

CONTAINMENT LIGHTING FIXTURES
EL 792.0'
FIGURE 6.2-145



SEQUOYAH NUCLEAR PLANT
FINAL SAFETY
ANALYSIS REPORT

CONTAINMENT LIGHTING FIXTURES
EL 818.0'
FIGURE 6.2-146



CONTAINMENT VESSEL
UPPER CONTAINMENT
1000 CFM
1000 CFM
CONTAINMENT DOME HYDROGEN COLLECTORS
FROM UPPER CONT. 40,045
FROM UPPER CONT. 40,045
9,800 FT3 STEAM GEN CAVITY 375 CFM
9,800 FT3 STEAM GEN CAVITY 375 CFM
3,350 FT3 PRESSURIZER CAVITY 375 CFM
9,800 FT3 STEAM GEN CAVITY 375 CFM
9,800 FT3 STEAM GEN CAVITY 375 CFM
LOWER CONTAINMENT
REACTOR WELL 300 CFM
REACTOR WELL 300 CFM
16,800 FT3
AIR RETURN FAN 42,395
AIR RETURN FAN 42,395 CFM
40 CFM ACCUMULATOR RM
10,200 FT3
25 CFM ACCUMULATOR RM
5,380 FT3
75 CFM INCORE INSTRUMENT
19,815 FT3
25 CFM ACCUMULATOR RM
5,380 FT3
55 CFM ACCUMULATOR RM
13,780 FT3

PRELIMINARY TESTING TO IDENTIFY

COMMERCIALLY AVAILABLE IGNITERS

TESTING CONDUCTED AT TVA'S

SINGLETON LABORATORIES

1.0 Introduction

TVA has a testing program which is being conducted at TVA's Singleton Laboratory to obtain preliminary information about the performance of commercially available igniters. The purpose of these tests was to screen alternative igniters and to gain a degree of confidence that the igniters could ignite hydrogen. The tests were not run under normal laboratory test conditions since the objective was to identify which igniters, if any, were most promising as subjects for more detailed testing and evaluation. Nontheless, TVA gained considerble information and assurance that commercially available igniters could ignite hydrogen.

2.0 Preliminary Screening

A number of igniter types were evaluated, ranging from high energy spark igniters to large diameter (1-1/2" I.D.) heater coils. Although the spark plug type igniter was considered an excellent candidate for this application, it was rejected prior to preliminary testing due to potential problems with electromagnetic interference (EMI) with critical instrumentation. TVA's Electrical Engineering Branch is researching the problems associated with EMI generators, and spark type igniters may be considered at a later date for use in Sequoyah unit 2 or Watts Bar.

Two other potential candidates, both coil heaters, were rejected after the first one, a large diameter (1-1/2" I.D.) coil, could not reach sufficient surface temperature, and the second one failed at the connector in less than five minutes. Therefore, testing was restricted to diesel engine glow plugs, since they were known to be capable of achieving the 1500°F minimum surface temperature desired by TVA and because of their rugged design.

TVA determined that at 12 volts ac, acceptable surface temperatures could be achieved but that considering line losses, variances in system voltages, possible plug cooling due to high humidity, and other effects, TVA would need to operate the plugs at 13 volts ac ± 1 volt.

Since the possibility existed that TVA could overstress the plugs by overvoltage, TVA consulted glow plug manufacturers and identified two types of failure modes which could be expected. The first type of failure caused by overstressing would be the failure of the heater wire within the glow plug sheath. This type of failure due to the breaking of the circuit would outwardly cause the plug to discontinue glowing. The second type of failure caused by overstressing would involve offgassing of the glow plug tip. Unlike the first type of failure after offgassing, the glow plug may continue to glow; however, the surface temperature would drop significantly.

3.0 Description of Glow Plugs

Glow plugs manufactured by three different companies have been levels both on the primary and secondary side and at the plug were measured by a digital voltmeter (Fluke model number 8024A), and the current levles were measured by an amp meter (Triplett model number 10 type 2). The surface temperature of each of the glow plugs was measured by either a thermocouple (type S) connected to a potentiometer (Leeds and Northrop model number 8690-2) in contact with the surface of the plug or by an optical pyrometer (Pyro model number 85). A total of 12 plugs have been tested to date.

4.2 Surface Temperature

A GMAC model 7G plug was operated at 12, 14, and 16 volts ac. Surface temperatures as measured by the thermocouple were 1480, 1550, and 1650°F, respectively. Since the thermocouple would be expected to increase local heat loss and hence reduce the measured local surface temperature of the thin-walled plug sheath, these valves were probably somewhat lower than actual surface temperatures. This conclusion was supported by later readings with the pyrometer while testing another GMAC model 7G at 14 volts ac and getting 1720 ± 15°F.

A Bosch plug has been tested at 13 volts ac. It produced a surface temperature of 1700°F as measured by an optical pyrometer. Based on these results, TVA concluded that the diesel glow plugs could produce the desired surface temperatures.

4.3 Voltage Tests

Voltage tests have been completed on only the GMAC model 7G plugs. Based on tests on 5 GMAC 7G plugs, reliable operation at 14 volts was confirmed by two other 7G plugs failed at 16 volts ac after a few minutes.

Inconclusive testing on 2 Bosch plugs resulted in failure when operated at 14 volts ac; however, one Bosch plug operated satisfactorily at 13 volts ac. One Isusi plug was tested at 14 volts ac but lasted for only 30 minutes.

4.4 Extended Operation

Endurance tests have been performed on only two plugs for extended periods of time. A GMAC model 7G plug was operated continuously for 148 hours without failure and was later used in the hydrogen burning tests. A Bosch 10.5 volt plug was operated at 13 volts for 90 hours, then cooled down for two hours and turned back on. It has been running continuously after being reenergized since August 20, 1980, at 10 a.m.

5.0 Hydrogen Testing

One igniter (AC 7G) was installed in a "PARR" (229HC6-T316-031579-

5142) pressure vessel in order to determine feasibility of igniting hydrogen in a sealed container. The vessel lid has a silicone rubber sealed gas injection sampling port. Hydrogen concentrations in the vapor phase were determined before and after ignition intervals. An ignition interval is the time current flows through the igniter circuit. The hydrogen was measured by a Perkin-Elmer gas chromatograph equipped with 3920 thermal conductivity dector and an M-2 integrator. The chromatic graph was standardized with hydrogen and air mixtures prepared from research grade hydrogen and laboratory air.

Temperature measurements were made with a mercury and glass (484635, ASTM 9C) thermometer. Temperatures reported are ambient for tests 1 through 3. Prior to tests 4-10, 100 grams of water was added to the vessel. The vessel was heated by a temperature adjustable hot plate to saturation temperature of the water and maintained throughout the test. The reported temperature is the water temperture after completion of the test. Results of the 10 ignition tests are given in table 1.

6.0 Future Tests at Fenwall Laboratories

TVA and Westinghouse have contracted with Fenwall Laboratories to perform hydrogen burn testing on the AC igniter and its mounting enclosure in an enclosed vessel. Attachment 1 is the proposed Test Plan for the testing. The final test plan is being prepared by Fenwall and should be available in the near future. These tests are designed to prove the effectiveness of this igniter assembly to burn a volumetric quantity of hydrogen in environmental conditions which approximate postulated accident conditions inside containment.

7.0 Conclusions and Summary

The purpose of these tests at Singleton was to select a commercially available igniter that was capable of igniting hydrogen. From the results obtained, the GMAC model 7G glow plug produces more than adequate temperatures at a range of voltages that can be provided inside the Sequoyah containment.

In addition, the plug seems capable of extended operation at high temperatures and has been shown in small tests to be able to ignite 12 percent and lower volumetric quantities of hydrogen. Although it has not been tested as thoroughly, the Bosch plug appears like it may also be an optional igniter.

TABLE 1

HYROGEN IGNITION TESTS

Test No.	Vessel Contents	Temp. (^{o}F)	Initial Hyd. Conc. (% Hyd.)	Final Hyd. Conc. (% Hyd.)	Ignition Intervals (Min.)
1	Hyd., Air	90	12.5	0.1	5
2		80	7	0.1	5
3		80	3.5	0.1	5
4	Hyd. Air, Water	120	12	0.1	3
5		180	14	0.5	3
6		180	4	2.5	1
7		180	2.5	1.5	1
8		180	1.5	1.3	1
9		180	11	5	1
10		180	5	2	1.3

Vessel Volume 1.1 dm^3 (0.039 ft^3)

Operating Voltage 12V dc

E50239.07

ATTACHMENT 1

SUMMARY

SEQUOYAH PLANT

HYDROGEN IGNITER TEST PLAN

1. Introduction

The following describes tests to be conducted on a type of hydrogen igniter to be installed in the Sequoyah Nuclear Plant. The igniter consists of a "glow plug" as used in diesel engines, the surface of which exceeds 1500° F and serves as a hot surface to initiate hydrogen burning, and a power transformer and an enclosure for the unit. The function of the igniters in the nuclear power plant containment is to burn hydrogen, in accidents where it could be released, when it reaches a burnable concentration thereby precluding its buildup to high concentration levels. The tests will be conducted by Fenwall, Incorporated, at their facilities in Ashland, Massachusetts. The unit, consisting of glow plug and enclosed transformer, will be placed in a test vessel and subjected to a range of environmental conditions (including hydrogen concentration, temperature, pressure, and steam), and its hydrogen ignition performance monitored.

1.1 Purpose of Tests

The primary purpose of the tests is to demonstrate that the igniter will initiate a volumetric burn of the hydrogen for the specified environmental conditions (pressure, temperature, water vapor). A secondary objective of the tests is to narrow down the hydrogen concentration range for which a volumetric burn of hydrogen will be initiated.

1.2 Acceptance Criteria

For the initial set of tests, the following acceptance criteria will be used:

1. Data generated are internally consistent (i.e., ignition at 8% consistently produces low pressure rise).

2. Data gathered confirm theoretical predictions.

3. Igniters reliably ignite mixtures at high (12%) concentration and provide relatively complete combustion.

2. Description of Igniter

The igniter is a General Motors Ac Division Model 7G glow plug (thermal resistive heating element) requiring 14V ac supply at a maximum of 8-1/2 amps. The surface temperature of the plug as measured by an optical pyrometer should be a minimum of 1500° F. TVA has measured 1720° F surface temperature on one of the glow plugs at their facilities. The igniter is powered by 120V ac stepped down to 14V ac. The power transformer is a Dongan Electric, Incorporated, Model 52-20-187 specially wound transformer having the following characteristics:

120V	RMS	AC	on primary side
14V	RMS	AC	on secondary side
200V	A	Min.	

Class H (High temperature insulation)
Open style with 18" flexible leads
Certified capability that transformer will operate at 220° C.

The igniter and transformer are mounted as a unit as shown in Figure 1 with the glow plug extending from the side. The unit is encased in a 1/8-inch steel plate box type casing and sealed with a rubber seal for water tightness.

3. Description of Test Facility

The tests will be conducted by Fenwall, Incorporated, at their facilities in Ashland, Massachusetts.

3.1 Test Vessel

The igniter unit will be tested in a spherical vessel in excess of six feet in diameter. The internal volume of the test vessel is 1000 gallons (134 ft^2). The vessel is constructed of carbon steel (exterior) and is lined with stainless steel. The vessel is designed for a working pressure of 500 lb/ft^2. The vessel is equipped with five diameter access ports (four on circumference, 90° apart,

and fifth at the top), one of which is drilled to attach to a manifold with valves and connecting lines to air, steam, and hydrogen makeup sources.

The vessel is heated externally via electrical heaters. The vessel will be equipped internally with a fan to promote mixing and also to create a draft at the igniter heating surface during testing when desired.

3.2 Instrumentation and Measurements

The vessel is instrumented with two pressure transducers to monitor the pressure including the pressure transient during the hydrogen burn. The output is carrier amplified and feeds to an oscillograph device. Thermocouples are provided which will monitor vessel atmosphere temperature prior to and after a burn. In addition, a thermocouple will be used to measure the temperature of its heated surface. Gas mixtures will be formed using pressure instrumentation and a partial pressure method in which a given gas is added until the appropriate partial pressure is indicated. Sampling capability exists via a 1-inch by 1-foot lecture bottle. Hydrogen and oxygen analyzers will be provided to measure pre- and post-burn concentrations of these gases:

	O_2 Analyzer	H_2 Analyzer
Manufacturer	Hays Republic	Hays Republic
Model	A 00632	SH-A-00643D
Range	0-5%/0-20%	0-5%/0-20%
Accuracy	± 1% F.S.	± 2% F.S.

4. Test Plan

4.1 Identification of Tests

The unit consisting of the glow plug and encased transformer will be positioned in the test vessel (via 18-inch port) with the glow plug heating surface located near the center of the test vessel. Various mixtures of H_2, steam, and air will be adjusted with pressure and temperature as specified and then the igniter turned on. The pressure transient will be recorded and the mixture analyzed for H_2 and O_2 content prior to and after the burn. The test matrix for the first series of 12 tests is shown in Table 1. Initial total pressures of 15, 21, and 27 lb/ft^2 a will be covered at hydrogen concentrations of 8 and 12-volume percent. Initial temperature wil vary from 180° F (dry case) to 350° F (superheated steam) with most of the tests being conducted at saturation temperature corresponding to the pressure to be tested. In addition, a fan will be located in the test vessel to provide drafts of 5 and 10 FPS in the vicinity of the glow plug to simulate turbulence which may be developed in the vicinity of the igniters.

Further testing will be developed based on the outcome of test series #1, and may include addition of an instrumented transmitter and steel or concrete surfaces with thermocouples attached to measure temperature response on hydrogen burn. In addition, means to simulate spray droplet entrainment in the atmosphere are under investigation.

4.2 Test Procedure

The basic procedure is to adjust mixture concentration temperature and pressure, then energize glow plug and record the pressure and temperature transient. Hydrogen concentration after the burn will be measured to assess completeness of burn. The steps for the different tests are as indicated in Table 2. In one of the tests with a steam environment, the glow plug will be energized after the steam, pressure, and temperature environment conditions are reached, but before hydrogen is added, and allowed to stand for two hours. Then the glow plug will be deenergized, hydrogen adjusted, and then the glow plug energized. The purpose of this is to allow for preburn exposure to the environment.

4.3 Test Schedule

The test schedule is tentatively planned as follows:

Facility Preparation	8/18 through 8/29
Test Series No. 1	9/1 through 9/5
Subsequent Tests	9/8 through 9/12
Test Evaluation	9/15 through 9/19

DE01;SQNHYD.AA

TABLE 1

TEST SERIES NO. 1

Test	Temp (^{o}F)	Total Pressure* (Gauge)	Hydrogen Concentration (Volume Percentage)	Fan Induced Flow Speed (fps)
1	180	0	12	0
2	180	0	8	0
3	Sat temp	6	12	0
4	Sat temp	6	8	0
5	Sat temp	12	12	0
6	Sat temp	12	8	0
7	Sat temp	6	12	5
8	Sat temp	6	8	5
9	Sat temp	6	8	10
10	Sat temp	6	12	10
11	350	12	12	0
12	350	12	12	10

*This is the total pressure due to air, hydrogen, and steam. For tests 1 and 2, the pressure will be higher than 0 due to the added hydrogen partial pressure and the evaluated temperature.

PROPERTIES OF HALON 1301

- LOW BOILING POINT (-72°F)
- LOW TOXICITY (UL GROUP 6)
- INSOLUBLE IN WATER (0.0095 W/O)
- INERT
- LOW RADIOLYTIC DECOMPOSITION (0.00023 g/d/R/h)
- NO LONG TERM ACTIVE MIXING REQUIRED AFTER INJECTION

SUITABILITY OF HALON 1301

- o PREVENTS HYDROGEN IGNITION AT SUFFICIENT CONCENTRATIONS.
- o ATLANTIC RESEARCH CORPORATION REPORT SHOWED HALON 1301 SUITABLE FOR USE IN A MARITIME REACTOR CONTAINMENT.
- o INITIAL STUDY BY ARC FOR AEP/DUKE/TVA INDICATES HALON 1301 SUITABLE FOR USE IN ICE CONDENSER CONTAINMENT.

AREAS OF FURTHER STUDY BY ARC FOR AEP/DUKE/TVA

ON HALON 1301

- EFFECT OF HALON 1301 AND ITS DECOMPOSITION PRODUCTS ON CONTAINMENT MATERIALS
- TEMPERATURE AND PRESSURE EFFECTS ON CONTAINMENT DUE TO INADVERTENT ACTUATION
- POTENTIAL PROBLEMS ON LONG TERM ACCIDENT RECOVERY
- EFFECT OF HIGH CORE TEMPERATURES ON HALON 1301 DECOMPOSITION
- POTENTIAL FOR NON-INERTED HYDROGEN POCKET DETONATION TO INITIATE COMBUSTION IN INERTED MIXTURES
- PERSONNEL HAZARD DUE TO INADVERTENT OPERATION
- SYSTEM DESIGN AND INCORPORATION

Degraded Core

- TVA is following the state-of-the-art developments at national laboratories (Battelle, Columbus, Brookhaven, Oak Ridge), AIF, EPRI, etc.

- TVA is building the capability to use MARCH as a starting point.

- MARCH is not intended for design.

- TVA has set a goal to obtain a hydrogen generation rate curve (into the containment) for a fair range of core damage accidents.

STATEMENT IN RESPONSE TO QUESTION -

DO ICE CONDENSERS NEED ADDITIONAL HYDROGEN MITIGATION SYSTEMS?

The nuclear power industry and NRC have identified many lessons in the TMI-2 event. As a result of studies by the staff, the Kemeny Commission, consultants, ACRS, and others, including TVA's own Nuclear Program Review, a large number of changes have been identified. Some were implemented almost immediately, some are in various stages of implementation, and others are the subject of intensive study or planned rulemaking. The issue of the effects of hydrogen generation from degraded cores was considered by many, including TVA, as one of the more important raised.

We are addressing all of our containment designs; while the lower design pressure is a disadvantage for this issue, the ice condenser containment also has definite advantages, including a large, passive heat removal capability.

As a result of its Nuclear Program Review, TVA committed to:

Study ways to contain larger amounts of hydrogen and to backfit feasible features into the Sequoyah design. (TVA Nuclear Program

Review: Sequoyah Nuclear Plant and the report of the President's Commission on the accident at Three Mile Island, November 1979.)

TVA moved immediately to fulfill that commitment by committing significant resources to the issue. That effort continues and at this point has resulted in a significant study of degraded core accidents and their mitigation, in a long range plan to further study and act on the recommendations of that study, and in installation of an interim distributed ignition system. We feel that the steps taken and planned to reduce the likelihood and minimize the extent of core damage events, when coupled with the plants' inherent capability to withstand substantial core damage (about 25% metal-water reaction), would provide a sufficient degree of safety for the short term until TVA's and others' studies could be completed. However, since TVA is committed to make feasible improvements in the safety of our plants, we proceeded to install the interim distributed ignition system once we were convinced that it would not reduce plant safety and had the promise of increasing the amount of metal-water reduction that the plant could withstand. Our efforts are being placed on determining how much increase in capability the interim system affords and on our long term program which addresses other alternative measures in addition to controlled ignition.

TVA believes: that Sequoyah can be safely operated at least in the short term until our studies can be completed; that the plant already has significant capabilities to withstand a range of core damage events; and that the interim distributed ignition system increases this range of capability. We are firmly committed by policy, by staff opinion, and by actual work to take the lead on this safety issue.

E50238.02

TVA IGNITOR TEST FACILITY GROOMING STARTED

H_2 %	IGNITION (SEC)	PEAK PRESSURE PSIG
8	17	32
9	16	41
10*	16	50
12	15	67

*2 TESTS

INITIAL TESTS AIMED AT ESTABLISHING IGNITOR PERFORMANCE

CONCENTRATIONS FOR COMPLETE AND LOW PRESSURE CONVERSIONS

100% HUMIDITY CONDITIONS

STATIC AND FLOWING GAS STREAMS

VARYING PRESSURES

LATER TESTS SCHEDULED FOR FURTHER PERFORMANCE CONFIRMATION

- ADDITIONAL H_2 CONCENTRATIONS
- MULTIPLE BURN IN TRANSIENT CONCENTRATIONS
- SPRAY EFFECTS

AFTER FIVE BURNS, UNSEALED IGNITOR CONTINUES TO PERFORM

NO MAJOR EXTERNAL DAMAGE SIGNS

INDICATIONS OF MINOR PENETRATION WITHOUT
APPARENT PERFORMANCE EFFECT