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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

274TH GENERAL MEETING
ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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

Friday, February 11, 1983

The Advisory Committee on Reactor Safeguards met, pursuant to notice, at 8:30 a.m., Jeremiah J. Ray, Chairman, presiding.

ACRS MEMBERS PRESENT:

JEREMIAH J. RAY, Chairman
JESSE C. EBERSOLE, Vice Chairman
PAUL G. SHEWMON
CARSON MARK
CHESTER P. SIESS
ROBERT C. AXTMANN
DADE W. MOELLER
MYER BENDER
WILLIAM KERR
MAX W. CARBON
HAROLD ETHERINGTON
FORREST J. REMICK
DAVID A. WARD
DAVID OKRENT

ALSO PRESENT:

PAUL BOEHNERT,
Designed Federal Employee

P R O C E E D I N G S

MR. RAY: The meeting will now come to order.

This is the second day of the 274th meeting of the Advisory Committee on Reactor Safeguards. During today's meeting the Committee will hear reports on and discuss the Clinch River breeder reactor project. The items scheduled for discussion on Saturday are listed in the schedule for this meeting which is posted on the bulletin board at the back of the room and on the outside of the room.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act, and the Government in the Sunshine Act. Mr. Paul Boehnert on my right is the Designated Federal Employee for this portion of the meeting.

Portions of this meeting will be closed as necessary to discuss proprietary information applicable to this project.

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

We have received no written statements or requests to make oral statements from members of the public regarding today's meeting.

The only item on today's schedule is the Clinch River breeder reactor project, and Dr. Carbon will begin this discussion. It's yours, Max.

MR. CARBON: This is obviously a continuation of the review that has been in progress for some time. I would just review briefly, that in July we discussed site suitability, and in December we had a plant overview, and in January we had heat removal systems.

Now today we look at the plant seismic margin, and part of this is a review of material that was covered last July, but part of it was not. And in addition, today there is the mechanical nuclear and thermal hydraulic design with the reactor core, the internals of the reactor vessel.

This is to include some discussion at least on instrumentation, and it is I believe -- I hope to include a discussion of the maximum chilling transfer for the core, which Mr. Ebersole wanted or raised a question about last time.

In addition, there will be some discussion on the pooled circuitry and the interfaces of steam-water -- I'm sorry, sodium-water, sodium-air, and so on. And there will be a discussion on the materials and a discussion on the steam generator accidents and consequences.

Very briefly, for next month we have scheduled reactor shutdown and control, and a very extensive discussion on the containment philosophy and design, which will cover both DBA's and beyond the DBA, CDA energetics, structural margins for energetics, and non-energetic CDA's, including meltdown and thermal and pressure margins for those non-energetic CDA's.

And then in April there will be a mix of topics: reliability, in-service inspection, human factors, accident contingency, probably some QA/QC discussion, and I'm not sure but perhaps something on sabotage, depending upon your interest.

As sort of a second major item, let me call your attention to how the Staff will be participating today, which is different than it has been in the past. As you are aware, we have been carrying on a review sort of in parallel with the Staff, which we have had extensive discussions with the Applicant at the same time that the Staff has been.

Heretofore, the Staff has not had positions on most of the topics that we have taken up. But in contrast, today on these topics the Staff does have and will present actually its final position, I believe, on several topics. We don't have the SER yet. I believe it is scheduled out about March 1st, and so we can reasonably raise any kinds of questions later that we want to after we get the SER.

But on the other hand, the more that we can finish up as we go along, the less we will have to duplicate and perhaps the less inefficient use of time.

As a third general topic here, let me call your attention to an article on LMFBR's which was passed out in the loose material that should be inside your folder. It's title is "The State of the Art for Fast Reactors." It is by a former Director of the U.K. fast reactor program, and based to a considerable extent on his interpretation of some of the discussion of the material at the fast reactor safety conference in Lyons, France, last July.

Dave Okrent and I both attended that. I thought the article would provide good background. As I commented in the cover letter, there are differences in approaches among the French and the British and ourselves that we take to fast reactor safety, and certainly there is no reason to expect that everyone is going to take the same features or anything like that.

It is simply that I wanted you to be aware that there are differences and to give all of us the opportunity to explore these differences when they seem worth exploring. Some of the particular ones which have been mentioned in the past -- and again, we use a heterogeneous core, which our people in the U.S. feel offers some definite safety advantages. None of the other three groups, the French or British or Germans, do use such a core.

In contrast, the French and British at least make more extensive use of in-core thermocouples, and I believe the British have ultrasonic equipment for under-sodium testing. I think that the British and French are both using core catchers in their large prototype size reactors, several times the size of our CRBR.

Going on to item number 4, I would call your attention again to the fact that everyone I think knows that we're scheduled to run to 8:00 o'clock tonight, and that is all right. But I guess I would sort of suggest we try and stay on the relevant topics, so as not to extend it too far beyond that.

Finally, let me call on the other working group chairman and Subcommittee members for any additional comments they might wish to make before we start. Bill, do you have any comments?

MR. KERR: No.

MR. CARBON: Bob Axtmann?

MR. AXTMANN: No.

MR. CARBON: And Carson?

MR. MARK: No, thank you.

MR. CARBON: Paul?

MR. SHEWMON: No.

MR. CARBON: Does anyone else care to make comments or suggestions?

MR. RAY: I would like to suggest that serious consideration of your last remark about going beyond 8:00 p.m. be dominant, in order not to dilute our considerations.

MR. CARBON: I don't think there will be much of a difference of opinion on that.

Well, with that, then, let us proceed with the agenda, and it calls for an introduction by Mr. Stark of the NRC.

MR. CROSS: Mr. Chairman, this is Peter Gross from DOE. In the interest of making sure we meet the 8:00 o'clock deadline for completing this meeting, due to the weather two of our presenters today will not be able to make it. Their planes have been cancelled. And this is Griffin and Mallett from Westinghouse, and unfortunately their flight has been cancelled, and we don't have anyone here who can make those presentations. It is approximately 45 minutes of the schedule.

MR. CARBON: Well, if we have to pass we certainly will. On the other hand, the more things that pile up the more difficult it makes it down the road.

MR. SHEWMON: If we have to break at 7:15 tonight, we might be able to.

(Laughter.)

MR. BENDER: As an ex-Chairman, you sound very tractable.

(Laughter.)

MR. STARK: Good morning. I expect all of our reviewers will be present today at the required time.

As Dr. Carbon did indicate, the reviewers have completed their SER sections and our SER will be out on March 4th, so the positions you'll be hearing today will be final positions of the Staff. And I guess that is really all I need to say about that.

What I would like to then do is go into the next subject, which is the external phenomenon. And what I intend to do here is several things.

(Slide.)

Most of that information, in fact all of it, is contained in chapter 2, so I thought I would give you a summary of chapter 2 and then we would discuss one or two items in detail.

The chapter 2 items were a part of the site suitability report, which was, as Dr. Carbon indicated, was discussed last summer. Since that time the Staff has also completed their SER review for chapter 2. There are two items that have changed since last summer.

One is in the meteorology review. You may recall last summer that the meteorological dispersion model then did not comply with Reg Guide 1.145. It now does. The SER reflects that. The results are acceptable. They were acceptable before, but that is just a minor change.

The other item is in seismology. The Staff has since received an SER from USGS, who is an advisor to the Staff, and Bob Rothman, who will follow me, will discuss how that particular SER has been factored into our SER. And since there has been a lot of interest in the seismic activity, Bob is going to present in a little more detail the results of the seismic review, kind of a refresher plus an update.

The one thing I do want to point out in this particular area in chapter 2 is that the standard review plan does apply and gives us good guidance, and therefore the review on chapter 2 was done in accordance with the standard review plan, and chapter 2 has no open items in it right now.

MR. BENDER: I don't even know if this is the right place to raise this question, but I'm going to raise it anyhow. The previous practice in licensing, particularly with respect to hydraulics, has really been related mainly to the question of having to do with small releases of radionuclides.

We really haven't tried, as I understand it, to spend much time worrying about very large accidents and how the hydrology of the region might relate to such things. This is only one reactor, so maybe this isn't important.

MR. STARK: Well, the environmental statement, the Staff's final environmental statement, does address that, and we looked at the impact on ground water, and I will have to look at that number again, but it is in the order of 12 years or 8 or 9 years until the radioisotopes are found in ground water. That is what you're referring to, due to major accidents.

MR. BENDER: Do we have enough information in the way of analysis to support that position? That is, do we understand the mechanism of the accidents well enough to argue that case?

MR. STARK: I don't know if I can personally answer that, but I can look at the final environmental statement, see how it is addressed, and give that information, because it is somethi g that we analyze and do predict numbers for. And as I said, they are required as a part of the environmental review.

MR. BENDER: This may not be the right place to raise that question.

MR. STARK: Let me get the information. Then perhaps if we could look at your question from there. I have a hydrologist here. Maybe he would have the answer to that. Dick Codell from the Hydrology Branch.

MR. CODELL: Richard Codell from the Hydrologic Engineering Section.

We have in this case presented a small analysis for ground water contamination of the Clinch River. That is in the environmental statement. What we have been doing on all environmental statements is an analysis in which we compare the potential ground water releases and realizing that this problem is, as severe as it might be, is much less of a problem than atmospheric releases, we are able to draw conclusions, and we have done so in the Clinch River case, on two examples of light water reactors in recent times.

We have also analyzed the contamination of water supplies, surface water supplies, as a result of atmospheric fallout from large atmospheric releases.

Those are Fermi 2 and Indian Point. Those cases are being done ad hoc. We haven't gotten into a large program of doing them on every site.

But the results further confirm that the liquid pathway consequences and risks are far less than the atmospheric risks.

MR. BENDER: I think that maybe you didn't address the accident concepts in the right way. I really don't have much of a feeling for the surface contamination from an accident that largely involves airborne contamination. I had more in mind those kinds of accidents that postulate penetration of the containment and subsequently they represent a path, liquid pathways from that kind of an accident through the ground system.

MR. CODELL: That is exactly the situation we looked at in the Clinch River case, and we considered the penetration of the basemat and subsequent transport of dissolved radionuclides in the ground water to the Clinch River. I don't have the figures in front of me. I am thumbing through the FES. I don't recall that it was any particular problem. That is, no worse than the large majority of light water reactor sites that we have studied.

MR. BENDER: What are we presuming that we know about the subsurface structure?

MR. CODELL: Well, there are certain basic hydrologic factors that have been studied on the site, such as the permeability, porosity, and there are certain inferences you can make about the chemical behavior of radionuclides in the soil.

We also in our branch have been studying methods by which these types of releases could be stopped before they ever reach surface water.

MR. BENDER: Could you just provide us a copy of the analysis?

MR. CODELL: Do you mean beyond that which is in the FES?

MR. BENDER: I don't think the FES has enough substance to it to be able to analyze it. It has general statements.

MR. CODELL: I would be glad to work a writeup for our analysis which would explain in a little more detail, although I believe in the Clinch River case we did not spend very much time on it.

MR. AXTMANN: As I recall, a week or so ago the project announced the basemat would never be penetrated. But you assume that it would.

MR. CODELL: That's right, we assume that it would. I wasn't aware of what you just said.

MR. BENDER: It certainly all right to make assumptions about that. I'm not trying to argue with you. I just wanted to see what your analysis was.

MR. CODELL: Certainly I would be glad to work something up on that. I could have it done in a few weeks.

MR. OKRENT: On the same question, while you're standing, there is some part of the NRC that considers high-level wastes and how long it should stay put if you put it in the ground. And to them 12 years is not a long time. In fact, it is almost like a day.

Is there someone within the NRC that looks at how that problem is being approached? Of course, there is a different probability of something being in the ground if in one case you put it in the ground and in the other case it takes an accident, so that would have to be factored into the consideration.

But nevertheless, I am interested in knowing whether somewhere within the NRC or within your branch a look at both of these topics has been taken and somehow they are put into a harmonious position.

MR. CODELL: I think I understand what you're getting at. As far as I know, no one has ever looked at long-term storage of accidental releases in the ground. I think the assumption we have always gone under is that you would deal with it in the short term, but in the long term you would not allow any kind of accidental releases to stay in place. You would do whatever you had to do after the event, and I can't really go beyond that.

I think it would be prudent not to leave any kind of high-level accidental releases in the ground, and I think you would probably excavate and dispose of this material somewhere else.

MR. BENDER: You see how easy it is?

(Laughter.)

MR. OKRENT: I'm impressed.

MR. RAY: Go on.

MR. STARK: That's all I really had to say. But what I would like to do is now bring Bob Rothman up and have Bob give you an overview and an update of the seismic, the Staff seismic evaluation.

MR. OKRENT: Just one quick question. This reactor is designed for the standard tornado in the Reg Guide?

MR. STARK: Yes, I think they predicted 73 mile an hour winds, so I think it is designed for 90 miles.

MR. OKRENT: Now, this is tornado, I said. It's just that the Staff is playing with some new method of analysis and I just wanted to find out whether that in some way had drifted into this design.

MR. GROSS: Our presenter will cover that in detail when he gets up.

MR. OKRENT: Okay.

MR. STARK: I will try to look it up, also.

MR. ROTHMAN: I'm Robert Rothman in the Seismology and Geosciences Branch.

We completed our review of the geology and the seismology of the Clinch River breeder reactor SEP and the USGS has acted as advisors to the Staff. The conclusions reached in the SER are that the faults at the site and the site region are not capable of controlling an earthquake. The design is a recurrence of the 1897 Giles County maximum modified intensity 8 event, the SSC of 8.25g anchoring a Regulatory Guide 1.6 spectrum. It is possible to account for the occurrence of this in the site vicinity.

There are no known capable faults in the Southeastern United States, but as a further confirmatory study of the non-capability of the local faults, the Staff recommends a study to investigate the relationships of the Pleistocene deposits in the local faults in the Clinch River area --

MR. KERR: I'm sorry, would you repeat that last statement.

MR. ROTHMAN: I said we know of no --

MR. KERR: I understood that, but then you said the Staff recommends something.

MR. ROTHMAN: The Staff recommends as a confirmatory item a study of the relationship between the Pleistocene river terrace deposits and the local faults in the site region.

MR. KERR: And how long would you anticipate such a study would take?

MR. ROTHMAN: We're not putting any time limit on this. It is not even being put out as an open item. We're just recommending that it would be prudent for the Applicant to look at these and report on them, possibly in the FSAR. We have no evidence that there are capable sites, but this is something that was done for other sites in the region, such as Watts Bar and Phipps Bend, and the geologists thought such a study could be done but they didn't want to put a licensing condition on it.

MR. KERR: I guess I don't understand the significance of the recommendation, then. But maybe I don't need to.

Go ahead.

MR. ROTHMAN: What they recommend is that we look at where these terrace deposits overlie local faults, that they look and see if there have been displacements of the terrace deposits which would indicate recent movement of the faults or possibility of the faults.

MR. MOELLER: If that was done for these other sites, would the data there not apply?

MR. ROTHMAN: Well, that's right, and the Staff has based its conclusion on knowledge of work that was done for the other sites. There are, however, from what I understand -- and I'm not a geologist and I haven't looked at this -- that there are river deposits or terrace deposits in the vicinity of Clinch River, along the Clinch River, that have not been looked at. They were looked at, as I said, in the Watts Bar region and in the Phipps Bend region.

The existence of a possible seismogenic zone has been postulated in eastern Tennessee. The evidence for such a zone does not warrant its consideration as a capable fault within the meaning of Appendix A to Part 100 of 10 CFR.

The results of seismological research in the region, including the data from a well distributed network of seismic stations, will be monitored to further address this postulation. A probabilistic analysis was performed by the USGS and it indicates an order of magnitude difference in the recurrence of the SSE acceleration if this seismogenic zone is considered to exist, as opposed to the diffuse seismicity in the southern Appalachian region.

And that is basically where we stand.

MR. OKRENT: What would the numbers be for each of the two assumptions, then?

MR. ROTHMAN: For the diffuse zone, the numbers are on the order of two times 10^{-4}, and you have about an order of magnitude higher if you assume this. And this zone was assumed to be 140 kilometers long, capable of an earthquake on the order of magnitude 7 or such, and it passes within 15 kilometers of the Clinch River site. And it would be about -- the excedance was about an order of magnitude.

MR. OKRENT: So depending upon which postulate you make, two times 10^{-3}, two times 10^{-4}, is that what you're saying?

MR. ROTHMAN: Well, the diffuse zone is not the one end of it and this zone the other. There are other models that could be looked at, like requiring the Giles County earthquake to be confined within the Giles County seismic zone, which would then decrease your recurrence.

So in other words, we're not looking at two end members. These are just two things that the USGS looked at.

MR. OKRENT: I don't recall. If the USGS recent considerations on the Charleston earthquake were applied, would they move that into a zone which encompassed Clinch River?

MR. ROTHMAN: We are not very clear on that, on just how far west under -- you know, there are several hypotheses on how the Charleston earthquake could reoccur, and in maybe one or two of those it might be, have to be considered, but not in all of them. And this has not been addressed.

As you know, there is a planned program for addressing the Charleston event, and part of that is going to be a probabilistic study that will be performed. So it will be addressed as far as the eastern plants are concerned.

I don't know what the USGS would say, if they would say that they would consider Clinch River as part of their eastern seaboard in their recent Charleston.

MR. OKRENT: I guess I'm a little surprised that the project office hasn't tried to find out just where this particular reactor fits with regard to that question. I don't think it is going to be a major consideration, let me say, but nevertheless I would have thought you would have tried to get that particular point nailed down.

MR. ROTHMAN: It is addressed in the SER, in the geology section of the SER.

MR. BROCHAM: This is Steve Brocham with the Geology Section.

In the position the USGS sent to us, they defined the eastern system as the coastal plain and the Piedmont. Taking that definition strictly, Clinch River is west of their definition of the eastern seaboard.

MR. OKRENT: Thank you. That is a direct answer, I think.

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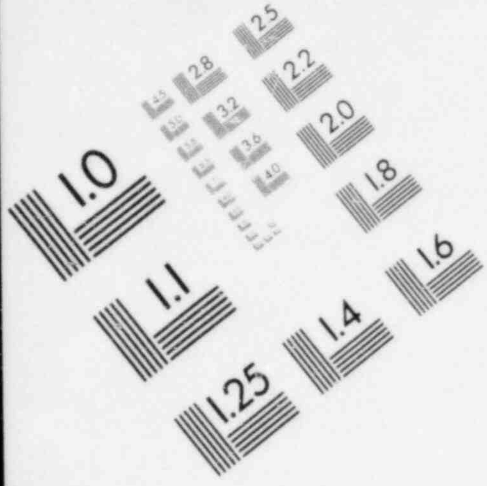
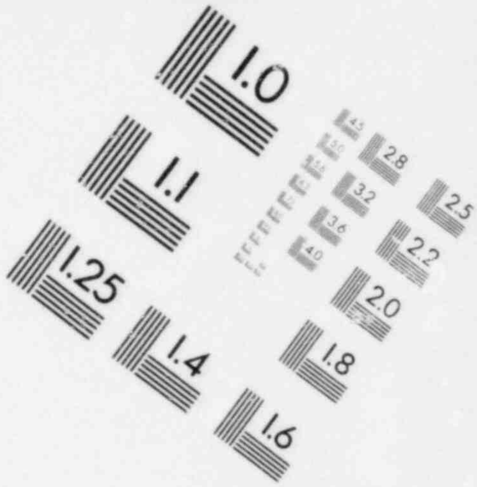
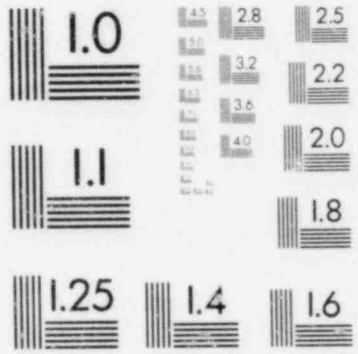

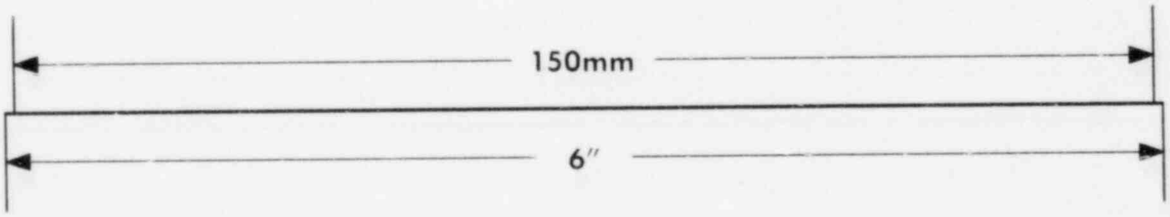

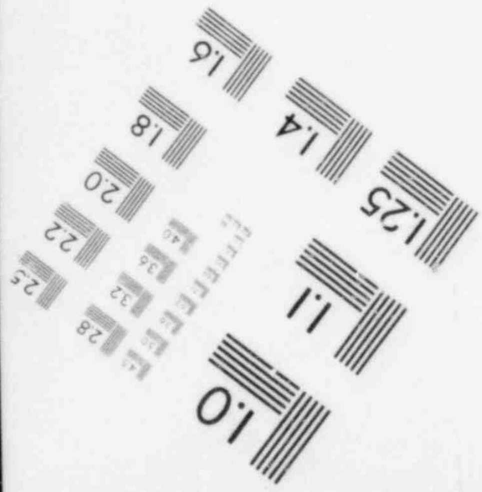
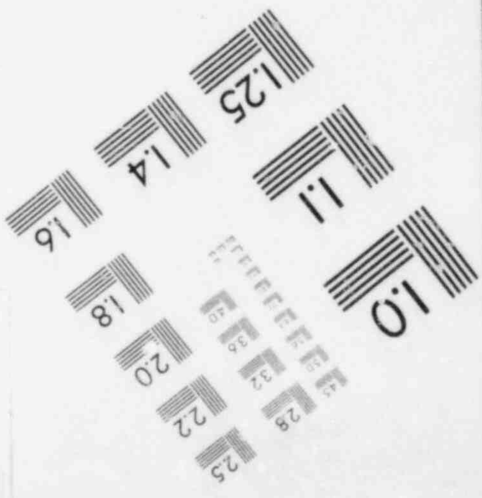

MR. CARBON: Any other questions?

(No response.)

MR. CARBON: Continue.

MR. ROTHMAN: That's all I have.

MR. CARBON: Good. Thank you.

I guess that concludes the Staff presentation. Let's move on then to the Applicant.

MR. MOELLER: Could you clarify for me the topics on tornadoes also, what were tornadoes? Oh, this is the tornadoes.

MR. CARBON: Yes. I wondered if there were any questions of the Staff.

MR. STARK: I was doing some looking after Dr. Okrent asked me a question, and I guess I will touch on both tornadoes and on hurricanes in the SER section that I'm reading.

It said between 1953 and 1974 54 tornadoes within a 10,000 square mile area containing the site. This results in a mean annual tornado frequency of 2.5, and a recurrence interval for a tornado at the plant site of 1,450 years. The design basis tornado characteristics selected by the Applicant conform to the recommendations of Reg Guide 1.76, which is design basis tornado for nuclear power plants.

It gives the characteristics: rotational speed, 290 miles per mile; translational speed, 270 miles; and at 3 psi a pressure drop occurring at a rate of 2 1/2 psi per second.

It goes on to say that remnants of hurricanes and tropical storms occasionally affect the area, and during the period of 1871 to 1973, nine tropical storms or hurricanes passed through the region. And it said the fastest mile of wind recorded in this area has been 73 miles per hour at Knoxville in July of 1961.

That's all I have to say right now.

MR. CARBON: Are there any more questions of the Staff?

(No response.)

MR. CARBON: Go ahead, Mr. Palm.

MR. PALM: My name is Bob Palm from Burns and Rowe, the AE for the project. What I intend to cover very briefly is a summary of how the Clinch River design accommodates the effects of various natural phenomena that could occur at the Clinch River site.

(Slide.)

These include the tornado, which Mr. Stark just talked about; maximum precipitation effects at the site, specifically at the Clinch River site; also, flood effects at the Clinch River site; and earthquake conditions.

(Slide.)

As far as the design basis tornado is concerned, the safety-related structures are designed in accordance with the regulatory guide requirements, and we have established a design basis tornado in accordance with Reg Guide 1.76.

Under Region I -- eastern Tennessee does fall under Region I which is the maximum region in the eastern part of the country which is subject to the maximum tornado wind velocities -- the rotational velocity of 290, a translational of 70 for a total wind velocity of 360 miles per hour.

As Rich pointed out, we have a pressure drop of 3.0 psi which is accommodated by the exterior envelopes of all the safety-related structures. The calculated velocity pressures are distributed in accordance with wind distribution formulas which are included in the ANSI standard. These are well known and pretty much followed on all nuclear power plants.

(Slide.)

In line with tornado protective design we also have generated a large spectrum of missiles which are reported in the PSAR, and based upon the design to protect the safety-related structures from these missiles, we have a minimum thickness of concrete of 2 foot 3 inches, and this is somewhat larger than the required minimum for the current standard review plan.

We also do provide at openings, whether they be vent openings or exhaust openings or inlet openings at roofs or walls, we have protective structures to prevent any path of a missile entering inside the buildings.

The method of analysis is reported in a lot of detail in Section 3.5 of the PSAR, and this includes formulas and design approach for protection of steel, protection of vital equipment housed in either steel structures or concrete structures. This includes penetration, potential scabbing or generation of secondary missiles from the impact on the outside face of the structure.

Also, the design includes overall and localized stability of the structure to assure that the protective envelope is not overstressed either locally or in a general way.

MR. SHEWMON: If you go back to your previous slide, you refer to this as being Region I.

(Slide.)

Is that more or less severe than out in the plains where tornadoes are common?

MR. PALM: Region I, I don't know of the

specific extent of it. I do believe it does go out into the midwest belt, and it comes as far east as the Appalachians.

MR. SHEWMON: When was the last time there was a tornado in Knoxville?

MR. PALM: When is the last time? I'm sorry. I don't know. I believe it was fairly recent, within the last ten years.

MR. EBERSOLE: I lived there. It was a few years ago. It took out a dock and left a path through the woods down there of torn-out trees. And I think it is probably about six or seven years ago.

MR. SHEWMON: They do come through periodically then?

MR. EBERSOLE: Yes.

MR. SHEWMON: Thank you.

MR. PALM: That's right. There is a report on the history of tornadoes in the Clinch River site in the PSAR.

(Slide.)

As far as the maximum precipitation is concerned --

MR. MOELLER: Excuse me. On the slide you had shown just before this one you pointed out that your concrete is 2 feet 3 inches versus 2 feet. How much more protection does that give you? Is there a way to quantify that? I mean can you tell me a percentage?

MR. PALM: Well, it is pretty much --

MR. MOELLER: Is it linear?

MR. PALM: It is pretty much linear for half the depth, because the rule says you design the missile penetration or you calculate the missile penetration, then you double the thickness of the wall. So, in essence, we have about -- if you divide the 2 foot 3 in half, you've got 1 foot, 1 1/2 versus the 1 foot, and that is kind of a straight line factor of safety above the minimum requirement.

MR. MOELLER: Thank you.

MR. EBERSOLE: May I ask a question? The most obvious result of a tornado would be to tear out the normal power system and leave you riding on the diesels for some long time. Have you put any particular design protection for the diesel plants themselves to accommodate that situation?

MR. PALM: Yes, sir.

MR. EBERSOLE: How quickly do you need power? For how long an interval can you be in blackout?

MR. PALM: I can't answer that question. I don't know if anybody from the project could.

MR. EBERSOLE: I take it you could be in AC blackout for some period of time?

MR. CLARE: Yes. This is George Clare from Westinghouse. As we have presented to some of the subcommittees in the past, we do have the capability to sustain a complete station blackout for some period of time. We don't consider that to be a design basis for the plant, but we do have that capability.

We also have the capability, a tornado protective capability for all three of our diesel generators to run many days without even any fuel being brought on site.

MR. EBERSOLE: Thank you.

MR. CARBON: What have you done, as an example, to especially tornado-harden the diesel sites? You answered Mr. Ebersole's question that you had taken extra efforts. What have you done on it?

MR. PALM: The diesel generators are housed in a concrete tornado-hardened enclosure, seismic Category I setting on subsurface material to account for potential instability from tornado winds or the earthquake. In general, that is what we're doing.

MR. CARBON: I presume this is the same as would be done at any plant.

MR. PALM: That is correct, sir. That's right.

(Slide.)

Now, as far as the floods are concerned, Mr. Newton, who will be following me, is from TVA, and he will cover the basis for the flood condition at the Clinch River site in more detail. However, we have determined or calculated the PMF -- that is, the probable maximum flood -- at a maximum elevation of about 780, 780 feet, including a 40-mile per hour wind velocity and resultant wave runup. However, the maximum flood at the site is governed by an upstream dam failure, that being the Norris dam. And this dam failure is combined with one-half of the PMF; and as I said, the details of the basis for this elevation, the analysis for this elevation, will be covered by Mr. Newton.

In any case, the plant grade is established at elevation 815. It is well above this maximum hypothesized calculated water level. And further, the safety-related structures, including the diesel generator building, emergency cooling water facility are designed for assuming that the flood level -- rather, the groundwater level, reaches the same level as the flood, 809 feet.

We have accounted for hydrostatic effects and included water tightness features in any of the design of these structures below grade or below this flood elevation.

(Slide.)

The earthquake design --

MR. CARBON: Excuse me. Was it your intention to skip over the slide on the maximum precipitation?

MR. PALM: I'm sorry. That was not my intent. Thank you.

(Slide.)

The maximum precipitation again at the site -- and I'm talking about the area or the region of calculation for determining the maximum flood level of the river, but this is specifically at the local area of the site -- we have first of all designed the drainage facilities for a 100-year storm, which is a maximum 3 1/2 inches of rain per hour.

The design has further been evaluated for a maximum potential storm based upon probable maximum precipitation, better known as PMP, where we have examined the conditions at the site based on 14 inches of rain in an hour and almost 30 inches in an 8-hour period. And based upon this quantity of water inundating the site, we have determined that we can allow a 6-inch maximum of flooding in the plant area.

To account for this, we have building entries 12 inches above grade, and we have allowed a maximum of

8 inches of ponding on the safety-related roof areas. And if there is excessive water, it is discharged to grade. We also do provide curbs, I believe, that are 18 inches or 2 feet in height around any openings in the safety-related building roofs.

And equivalent to this PMP we have a calculated 80-inch snowfall, and this is equivalent to a 40-pound roof load which is also included in the design. Actually, the 40-pound roof load is covered well by the tornado design requirements, so the capacity is well above what both the water and the potential collected snow on the roof.

MR. CARBON: The 100-year storm itself is, I believe, taken to be 3 1/2 inches per hour maximum, is that correct?

MR. PALM: Yes. This is from the records that we have included in the SER. I don't know for how long a period those records are.

Don Newton, do you have any idea on the rainfall records?

MR. NEWTON: I think that all comes -- this is Donald Newton, TVA, Flood Hazard Analysis. I assume that you're using the National Weather Service CP-40 rainfall, and the years of record that are in that, I'm not really sure. That is a fairly old document, but it is still the standard.

MR. PALM: The origin of all of this data is from the National Weather Service.

MR. MOELLER: I don't understand why you designed for drainage at 3 1/2 inches per hour, but you allow the rain to fall at 14 inches per hour.

MR. PALM: Primarily because this is a maximum probable event, and as long as the site can tolerate some overcapacity of the system design, then we don't see that there is any safety problem. The reason why is primarily economics insofar as the drainage system is concerned. We would have to go to say from a 42-inch drain pipe to something like maybe 8 feet.

MR. MOELLER: I see. That is helpful.

MR. PALM: And we have accounted for this in the local topography and the slopes, et cetera.

MR. OKRENT: If I could come back to wind for one minute, does the tornado design provision cover steady wind speeds far greater than the wind speeds for which you've designed the plant, or if there were greater wind speeds, steady wind speeds than you designed for could that create problems for some components or systems, and if so, how?

MR. PALM: Wind speeds greater than the design tornado velocity, you're talking winds in general?

MR. OKRENT: No. You design, if it understand it correctly, for tornado wind speeds, but also there is some kind of what I would call steady wind speed.

MR. PALM: I had intended to cover this very briefly, but because of time or whatever, our design is basically for an equivalent 90-mile per hour wind.

MR. OKRENT: Suppose it were 120 miles per hour, would that create a problem, or does your tornado design cover it?

MR. PALM: The tornado design would cover it, that is right.

MR. OKRENT: Would it cover 150? That is a pretty high wind speed. I'm just trying to understand.

MR. PALM: To understand it, we, as far as the design is concerned, there is no time element involved in the velocity pressure distribution or design pressures.

MR. OKRENT: Okay. That answers it.

MR. EBERSOLE: Let me ask a question in this matter. The negative pressure in tornadoes, there's two ways to go at this. You can either build a building to sustain it, or you can fit it. Which did you do?

MR. PALM: We have a combination actually. The structures, the envelopes are actually designed for the 3 psi. There are certain systems that do allow for venting certain compartments because of --

MR. EBERSOLE: But you have, in general, you have put an envelope around the whole thing for 3 psi?

MR. PALM: That's right.

MR. EBERSOLE: If that is bridged, do you then have a flow path inside that will prevent excessive pressure in compartments?

MR. PALM: It won't be breached. The design will not allow it.

MR. EBERSOLE: You're counting on the perimeter. You don't have a bleed-down path from compartment to compartment inside.

MR. PALM: I believe we do, yes. Through some of the openings and so forth there is a bleed-down path, and we do have.

MR. EBERSOLE: And that assumes you have a hole on the perimeter?

MR. PALM: That's right. I understand your question now. You have to hypothesize that you do indeed have a certain compartmentalization where you do have bleed-in of the pressure reduction.

MR. EBERSOLE: So you use both approaches.

MR. PALM: That is correct.

MR. EBERSOLE: Thank you.

(Slide.)

MR. PALM: On the earthquake design this is a review of basically what was covered at the summer of '82 meeting, pointing out the major parameters for establishing the safe shutdown earthquake for Clinch River using the tectonic approach, the Southern Valley and Ridge province, in accordance with the regulation.

We have identified the largest historical earthquake in the province and Charles County, Virginia; and NRC had classified this earthquake as an intensity 8. And based upon these requirements, we correlated the intensity to acceleration using the most conservative intensity correlation relationships recognized by NRC, and on the basis of that we came up with a .25 g zero period acceleration. And this large earthquake at Giles was assumed to occur at the site, at the Clinch River site.

We have used a one-half SSE as the OBE for Clinch River. This is also included in the design. And all the design, including damping factors, generation of the ground motion input, modeling techniques, method of analysis and so forth, follows the recognized and accepted light-water practice that is identified in the standard review plan and the regulatory guides.

So that is nothing unique about our approach. And Mr. Morrone of Westinghouse will get into the details insofar as the available margin that we have through this design approach that we have taken.

MR. CARBON: Have you gotten into different kinds of earthquake design problems, this being a liquid-metal reactor, things like sloshing in the pool? What are the different problems that you face on thin wall pipes instead of thick? Can you comment on some of those things?

MR. PALK: Well, most specific analyses were done by Westinghouse. We provided all of the input to them insofar as the response spectra and time histories. And perhaps Paul Dickson or George Clare might want to comment on that.

MR. DICKSON: This is Paul Dickson of Westinghouse.

Yes, there are some differences, and they are accounted for in the analyses.

MR. CARBON: Can you summarize the differences and say something about how significant they have been? What has been the net effect?

MR. DICKSON: One of the differences you alluded to was the fact that we have a large surface in the pool, and there is some sloshing of sodium which is different than a water system, at least in the water vessel for a light-water reactor. And then the thinner pipes, the seismic pipe at least in the primary system tends to control the piping design for the location of snubbers and hangers, as opposed to say a light-water reactor where the seismic design is probably less controlling because the pressure is more of a consideration. In the intermediate system the sodium-water reaction is more controlling than the seismic design.

Did that answer your question?

MR. CARBON: Yes. I think so.

MR. PALM: I would say, just to make a general comment further to what Dr. Dickson said, is that there is a lot more detailed interplay between the structural building design and the systems and component design for liquid-metal components, very much so.

(Slide.)

In summary, based upon potential natural phenomena that has been identified that could occur at the Clinch River site, we have established conservative design bases to, in the Clinch River design, to accommodate the loads effects generated from these phenomena. And we have essentially completed the Clinch River design to show that indeed the design does cover these design bases and resultant conditions.

Mr. Newton of TVA will now continue on the flood analysis.

Are there any further questions on this?

MR. DICKSON: While Mr. Newton is coming up, if I could add just one more thing. One other difference in order to accommodate our guard vessel concept, most of our large components such as the reactor vessel must be supported from the top, and that enters into the seismic design analysis capability significantly.

MR. NEWTON: I'm Donald W. Newton. I head the Flood Hydrology section of the Flood Hazard Analysis branch of the Tennessee Valley Authority, and what I am going to try to do is to briefly summarize the determination of the design basis flood level for the breeder site.

The handout that I provided, I have cut some of that material out in an effort to shorten the talk; so if you have any questions why, we can come back to it.

Our determinations, though made in the early '70s, are in accordance with the current Regulatory Guide 1.59 and the ANSI documents, and may be a little safer than are required.

(Slide.)

This shows you the site, the location. We have roughly outlined the watersheds, the site being here, which is directly on the Clinch River, with drainage area some 3400 square miles. The site is actually also on an arm of the Watts Bar reservoir, so the watershed in yellow, which is the watershed above Watts Bar dam, can also influence the site somewhat. That drainage area is -- the total in yellow is some 17,310 square miles. This dam is some 55 miles below the site.

(Slide.)

This is a diagram that maybe will show you a little bit better what is involved in the determination. You can see the site here between mile 16 and 18 on the Clinch River. Upstream where you have the Norris dam, which really ends up being the controlling feature in terms of flood levels at the site, the small dam, Melton Hill, downstream, and then Watts Bar dam downstream on the Tennessee River, also a potential source of flooding, with Tellico and Fort Louden dams here, Douglas and Cherokee and Fontana. And there being the site.

(Slide.)

MR. CARBON: Are you going to get into which of those dams can be assumed to fail and what this does to the flood level?

MR. NEWTON: Yes. Very briefly.

MR. MOELLER: Would you repeat the difference in the red and the yellow? The yellow drains towards the Watts Bar dam?

MR. NEWTON: Yes. The yellow is the total drainage area above Watts Bar dam.

(Slide.)

MR. MOELLER: And then the red is above what?

MR. NEWTON: Above Clinch River. That is the Clinch River; that long, skinny watershed is your Clinch River. And we are, of course, on the Clinch River below Norris dam, which is up in here, and then this is that total drainage area above Watts Bar which doesn't prove to be controlling, but it has an influence so you have to take a look at it.

MR. MOELLER: Thank you.

(Slide.)

MR. NEWTON: Your potential sources of flooding at the site are storms, storms which produce the probable maximum precipitation on the Clinch River, and that I showed you in red, or on the Tennessee River shown in yellow, or some combination thereof; floods that we call the maximum probable flood are the definition of the upper limit of flooding; and seismic-induced floods from seismically-induced dam failures. And this proves to be the controlling event.

We didn't examine in detail, but you need to consider snowmelt or ice jams. We are in a temperate climate, and that is not controlling. Or landslides, and there is a small slide volume potential, but they are limited. So the controlling event is the seismic-induced dam failure.

(Slide.)

I'm going to skip over the determination of the PMF, the most severe flood that can reasonably be predicted because it is not controlling by a significant amount. In other words, we examined four different storms, a number of different storm patterns, different seasons, to try and determine the most critical flood centering.

There were some dam failures. Fort Louden and Tellico upstream would fail. Part of Melton Hill would fail. But all of this together produced a design flood level or a flood level of, I will show you later, which is much less than the controlling event. So let's not -- I will skip over that unless you have some questions later.

MR. CARBON: I have one question there. You say some of those dams would fail. It's my impression that your knowledge or our knowledge in general of which dams will fail from seismic events is really not very good. Is your knowledge of failure from flooding on a much firmer basis?

MR. NEWTON: Yes. In this case we are having water overtopping the earth embankments, so it is a question of will it overtop an earth embankment, a road and fail, and we have enough information about that. And the depth and duration of overtopping was such that we are quite clear that the earth portions of the dam would fail.

In the case of Melton Hill it was a concrete dam, and the structural analysis would say yes, it would fail.

MR. CARBON: And you're also on firm ground on the ones that won't fail.

MR. NEWTON: Yes. And it depends upon the storm center where the largest floods occur. So we are pretty clear on that analysis.

In refreshing your memory now when we are looking at floods resulting from seismic events, obviously I'm saying if a dam should fail from a seismic event, we are talking about the two situations that we examined: a failure caused by the safe shutdown, the largest earthquake, and that is assumed to occur coincident with a 25-year flood with your reservoir at that maximum level during that flood event, or a smaller earthquake, OBE, and a larger flood, one-half of this probable maximum flood. Again, with the reservoir at the maximum level during that storm, the dam is assumed to fail or at least is examined at that level.

MR. SIESS: Well, those two combinations must be chosen on some kind of a probabilistic basis, am I correct? That is, the larger earthquake and the 25-year flood and the smaller earthquake I assume at half the PMF is a greater return period than 25 years, is that right?

MR. NEWTON: Yes. Much greater than that, yes. It is an attempt -- I don't know the details of how that particular standard came up with, but this would be your upper limit on your seismic, your SSE.

MR. SIESS: But not zero probability.

MR. NEWTON: I don't believe so, and that is not my field. Somebody else would have to tell you what that probability is.

MR. SIESS: Is it intended that somebody will address the probabilities associated with those?

MR. NEWTON: I'm not so sure. I would make the point that what we are showing is that even those were to occur, the plant is safe against that, so we're really talking about something above what flood levels this would cause. I believe the probability maybe on the order of what, 1 in 10,000 for the SSE. I am not really sure. That is not my field. I think I've heard that number.

MR. DICKSON: That we will be addressing later.

MR. SIESS: Thank you.

MR. EBERSOLE: Let me ask a question. Did you say that if you have a PMF, that is, 100 percent PMF, that you do or do not have to consider the dam failure?

MR. NEWTON: In the storm, the probable maximum precipitation which occurs over the watershed and translating that flood into flood flows and flood hydrographs, we do have to evaluate the upstream dams to make sure that they can contain that flood.

If it cannot contain it, if it would be over top, then the question is if it is over top, is it sufficient to fail the dam. And in our determination of the PMF for the breeder reactor site there were, in the critical storm combination situations, there was overtopping and failure of some upstream dams.

MR. EBERSOLE: Then the worst order would come from 100 percent PMF or maximum possible flood, which would produce the dam failure, right? Not these here.

MR. NEWTON: These are the controlling events. The PMF is a much lower flood level.

MR. EBERSOLE: Is it?

MR. NEWTON: Yes, much lower, as I will show you later on. I'm skipping over it because it is non-controlling.

MR. EBERSOLE: I see.

MR. NEWTON: It comes to elevation 778.

MR. EBERSOLE: The PMF? Now, you're talking about the PMF on the Norris Reservoir?

MR. NEWTON: In this case, the storm which was found to produce the maximum flood elevation at the site was not centered above Norris Reservoir; it was 50 miles North-Northeast of Knoxville, up at Bulls Gap. But that is a little different.

MR. EBERSOLE: Let me get back to the crux of it. On the Norris Reservoir the maximum possible flood level, it's considered to fail Norris Dam?

MR. NEWTON: No, no way. It doesn't even come close.

MR. EBERSOLE: So you don't have to deal with that.

MR. NEWTON: You don't even have to deal with that.

MR. EBERSOLE: If you did, that would be the worst case, wouldn't it?

MR. NEWTON: Yes, it would probably would be. Depending upon whether or not it overtops and Norris Dam would fail, and it is a concrete dam and structurally it might contain it.

MR. EBERSOLE: Are there any earthen fills up away from Norris dam that come down? Any saddle dams?

MR. NEWTON: Not that I'm aware of offhand.

That was never an issue, so we didn't look at it in detail.

MR. MOELLER: Do I understand correctly that these two alternatives are sort of standard? I mean, do you always use these?

MR. NEWTON: These are the standards specified by NRC.

MR. MOELLER: Thank you.

MR. NEWTON: And that is why NRC criteria -- they have specified that we shall examine these, and we have.

(Slide.)

Norris Dam we actually examined. Now, there were some 10 dams and combinations of dams that could fail in various combinations or centerings of these various earthquakes, and we examined all of those and what you boil down to, which is pretty obvious, is that Norris Dam upstream, if it were to fail, becomes the controlling events.

So the others were all looked at. You can forget about them. We come back to Norris, and let's take a look at Norris.

Now, we examined the dam. We concluded actually that it would not fail in an SSE or an OBE. The factor of safety would not be less than one, but because of the uncertainties in such estimates, we went ahead and did determine -- well, let's postulate it would fail -- and determine what is the most likely mode of failure should it occur.

So we have bypassed the argument -- well, would it or wouldn't it fail; we don't think it would. And then we got into a discussion of well, given that you have decided that it would fail, what is the most likely mode of failure. And let me simply show the two modes of failure.

(Slide.)

This is in the operational basis earthquake, which is the smaller earthquake. It is the larger flood and therefore, the highest head water levels, and therefore, it becomes controlling. And this is a brief outline of what we postulated would happen in terms of the mode of failure we're talking about. A concrete gravity dam which is some 1860 feet across the top. It is some 285 feet high. We have a spillway section in here. We have the powerhouse section in here (indicating). And for the OBE we postulated that these taller blocks would simply overturn like this. That would produce an opening here of some 665 feet wide.

The engineers estimated that if it did happen, most likely mode of failure would be the overturning of these blocks like this, and then the base of the dam would create a debris level here, about elevation 970. This would all come into this valley such as this (indicating). Note that it doesn't completely valley and there is actually significant end-around flow. But what you have created, in a sense, is a weir here, 665 feet wide, with an elevation of 970 for the top. But there is end-around flow.

This would be judged to be the likely situation in an OBE.

(Slide.)

In the SSE, we have a wider section. The analysis indicated that there would be more of the blocks that would fail, creating an 833 feet wide weir. The top of the concrete would be controlled by the width of the base and again, would be at about elevation 970.

In this case, there is a wider section that one nearly fills the valley and we have less end-around flow, given the mode of failure. The next step is the determination of the downstream flood levels.

(Slide.)

And the major elements of that kind of a determination, of course, is to determine the flows in the reservoir and in the streams and in the system for this 25-year flood, and we have a watershed model which was used to develop the PMF calibrated against zero flood, which was used to determine these flows.

We are concerned about the outflow from a breached Norris Dam. This is a question of the rating curves at the dam. Then we're talking about combined flows at the site. The flow from the dam out-breach and flow from the surrounding watersheds.

The unsteady flow models were used to determine the flows at the site. This is at present the most sophisticated procedure for analyzing that. The rating curves -- and that is the next slide --

(Slide.)

which establish the discharge from the failed dam are shown here. These were developed from a 1 to 150 scale hydraulics model study, and verified very closely by hydraulic analysis. I won a $10 bet. This is the rating curve for the OBE, the 665-foot section, and to everyone's surprise initially, you get a little more outflow because of that end-around flow around the concrete sections. And this would be the rating curve for the SSE where you got more debris, more in the downstream channel.

So the discharge from the dam is really more nearly controlled by the debris, in this case, than it is the width of the weir.

(Slide.)

In the OBE, what you would have -- the dam was postulated to fail at the peak of the flood. You would have this very sharply rising outflow hydrograph with 1,960,000 cubic feet per second coming out of the dam. The dam would drain relatively rapidly like this if it failed at elevation 1035.

MR. REMICK: What is the importance of the March date at the bottom?

MR. NEWTON: The March date ties into the date of the probable maximum flood. You not only need to look at the time and aerial distribution of the storms, but what time of year meteorologically. You can get large area storms in our region in March; you get smaller area storms and your maximum PMP tends to come in July, and we had a fairy large 8000 square-mile storm which produced PMP on 8000 square miles. So that is a March storm. Most likely, it would occur in mid-March.

This is a one-half PMF, and you assume it fails at the peak of it, and that gets you to March.

MR. REMICK: March 26th.

(Laughter.)

(Slide.)

MR. NEWTON: This does create some interest. Down at the site, then, we have reduced from 1.9 million down to about 900,000 cfs, and this would be the hydrograph at the site at mile 18 upstream, reaching an elevation of 804 with plant grade up here at 815 or downstream a little bit lower, 798. And that would be your elevations at the site.

(Slide.)

So that from the standpoint of the analysis, then, we have these flood elevations, the PMF which I didn't discuss but which was evaluated in detail with a maximum flood level of 777.8. The OBE with a one-half PMF, a controlling event. Now, this is without wind waves; just the still water level. We would have reached elevation 804.3 or for the SSE failure in a smaller flood, 796.

So then, then, becomes our controlling event. And according to NRC criteria, if we are above that elevation including wind waves, then we are safe.

Now, there has been some question about how sensitive our conclusions are on that elevation 804, regarding the modes of failure, the debris level and the width of that failure. We made an engineering judgment, we made a study. We said hey, this is the most likely mode of failure.

(Slide.)

But obviously, you could postulate somewhat different modes, so we looked at a number of possibilities just arbitrarily. You make an engineering judgment, and this one looks like it makes sense but let's try and bracket it. So we took three blocks, 168 feet. And this is all in the OBE, by the way. We simply concentrated on that because that is controlling.

168 feet wide in contrast to 665 -- let's take it all the way down. Let's assume that debris is out of the way. We would have reached 808 here at mile 18, 808.4, somewhat higher. Let's assume more blocks fail, 370 foot wide. Let's assume that the debris is a lot lower. We don't think it would be completely washed out, but maybe it is lower. You would have 925.

Pick a number. Well, you would reach 811.9. You are somewhat higher. Let's take the same 665-foot wide sectionthat we postulated but let's lower the debris level from 970 to 945. Well, we would get 808.9. Let's give a dimension to the whole problem. Let's see what the upper limit is.

Well, the upper limit would be assume that the dam completely and instantaneously disappeared. It is unrealistic but it certainly screens the upper limit, and you have got elevation 818, which is some three feet above. But using this as a device to see what is the upper limit, -- and obviously, there is going to be debris -- we think it adds credibility to these kinds of numbers.

We assume that we are not sensitive -- we think our basic conclusion that it will not exceed 815 is not sensitive to these levels of debris levels.

MR. REMICK: Maybe you explained this and I missed it, but the significance of mile 16 and mile 18 -- why two, and measured from what? From the dam?

MR. NEWTON: The miles are measured from the mount of the Clinch River, so you are coming upstream. And remember that we are on an arm like this of the Clinch River, so mile 18 is the upper side, and mile 16 is on downstream. There is some slope plus some storage. So in a sense, it becomes the controlling point. It is the first point of attack to the site, and therefore, that is what we looked at.

MR. CARBON: Just for understanding, what is the significance of 815 again?

MR. NEWTON: This is the plant grade. This is the one that we want to make sure that the plant grade is above any maximum flood level. So this 815 is the discussion we had previously. Everything -- all safety-related structures are above that; at or above that. So if we can show that that is above any flood level, then we are safe.

There is one other argument in here that we could make. That is, that we don't think that the dam would fail in an OBE. It might be more likely to fail in an SSE, which is a smaller flood, less head water levels. And although we didn't compute it, if you instantly vanish the dam, we estimate that the elevation would be on the order of 810.

So we think that any way you look at it, from a practical standpoint, 815. At that plant site level there is no flood level; you are well above any flood level.

MR. EBERSOLE: Don, could I ask you a question? That old Dam Norris used to have gasoline engine generators with which they intend to control their gates and sluices in the event of a loss of power. Are all of your analysis based upon any idea that those gates will be closed under these maximum flood conditions? You know, I'm talking about spillway gates.

MR. NEWTON: We're talking about the spillways, and in our flood studies -- and these, of course, are even the 25-year -- we operated the dams as we would in that kind of a flood, and the gates were assumed operable.

MR. EBERSOLE: But those engines were taken away about 30 years ago because they wouldn't work, anyway. So do you not have to make your analyses on the basis that the gates are down and the power is lost?

MR. NEWTON: We don't believe so. In our current dam safety program and inspection and the rest of it, we feel sure that -- George, do you know what the auxiliary backup is for power for gate operation at Norris?

MR. McCANON: Jess, I'm sorry, I do not know the answer to your question.

MR. EBERSOLE: Would you identify yourself?

MR. McCANON: I'm George McCanon, Civil Engineer in TVA's Hydro-Design Project.

MR. EBERSOLE: Well, I remember that about 25 years ago they decided to throw those old engines away because they never could get them started in the first place. But that left a residual question of whether, in a terrible storm condition you, in fact, had to lift the gates. A storm which would have taken down the transmission lines.

MR. KERR: I would have thought there would be a connection to the hydro generator station itself. I don't know.

MR. EBERSOLE: The engines were put there for a purpose, Bill.

MR. KERR: That was put there in the days when they thought there might be droughts and they thought the reservoir might be empty.

(Laughter.)

MR. NEWTON: Let me answer it this way. We're right now involved in a complete review of older dams, dam safety, what have you. As a part of that rule, we are specifically examining the operability of the gates' inflood, such as what we're talking about. This is a part of what we are doing. I don't now remember the details of Norris. I could of some others, but Norris I don't because we haven't gotten to it.

MR. EBERSOLE: But your basic --

MR. NEWTON: Our basic answer is that we will be able to operate those gates by some means in floods of this type. This is specifically what we're looking at.

MR. EBERSOLE: You might have to put some new diesels there.

MR. NEWTON: We might have to. We might have 100 people standing up there.

(Laughter.)

Whatever it is, we will have some means to operate those gates. We've got to.

MR. EBERSOLE: I'm not sure that the people here appreciate the implications of having a bad flood with those gates down. Would you explain that to them?

MR. NEWTON: Well, having the flood with the gates down would be advantageous to us.

MR. EBERSOLE: Would it?

MR. NEWTON: Yes, in the sense that if those Norris gates -- you can raise to control storage, to operate them. If they are down, then you have got your spillway capacity with no stoppage at all.

MR. EBERSOLE: But you have an overtopping of the gates to deal with.

MR. NEWTON: Well, the gates are down into the spillway crest itself, so that then you have --

MR. EBERSOLE: Oh, I'm sorry. I really mean when the gates are up. I'm 180 degrees away.

MR. NEWTON: But the point is if the gates are up, then you would have flow over the top of the gates. But what we're doing is operating those gates in floods of this sort with our control downstream at Chattanooga. We would operate those gates as we would during a flood of that type, and the details of the operations we assume -- frankly, I don't remember.

MR. SIESS: Does it take power to operate those gates, to lower them?

MR. EBERSOLE: Yes.

MR. NEWTON: I think so.

MR. McCANON: Yes. And our policy is that we will have backup capabilities of operating all of our gates.

MR. SIESS: How long do you have to lower the gates once one of these huge storms starts? Does it have to be done in minutes, hours or days?

MR. NEWTON: You are now getting into the details of this particular operation, and as I say, we made these basic studies back in the 70s. Depending on our dam we have anywhere -- some of them are remotely operated and we can operate them right away. Some require people going there and opening them, and then after you've opened them there is a certain amount of time.

MR. SIESS: I'm sorry, I'm assuming -- I'm going along with Mr. Ebersole's assumption that for some reason you've lost transmission lines and you -- how much time do you have to do something in the way of repairs or getting other sources of power?

MR. NEWTON: We have, in the first place there is some warning that a flood of this sort is happening and is occurring. Because we're talking about a storm -- well, one of the slides we have which I didn't show you talked about a storm where we had a three-day antecedent storm. A three-day dry period and then a three-day main storm.

What we do is we think through, when do you have to operate those gates, as a part of our analysis, and I don't remember the details. And we have available auxiliary power or some means to operate those gates so that we operate them the way we want to operate that dam to efficiently control downstream flooding. And we've thought through and worked out the timing and the rest of it. I just don't know the details and I can get it for you if you like.

MR. SIESS: And when you efficiently control downstream flooding, that's giving due consideration to Clinch River?

MR. NEWTON: Yes.

MR. SIESS: Not to Knoxville?

MR. NEWTON: As a matter of fact, if we were to operate strictly for the site we would have not let the elevation get to head water elevation, 1035. We actually could have let it go lower. What we did was we operated the dam as we would in a flood like that. We did reach elevation 1035 and we showed that if the dam did fail in that situation, you were still below the plant site.

MR. SIESS: Where is 1035 in relation to the spillway?

MR. NEWTON: I don't recall offhand. I would have to look that up. I've got a book back here. Do you remember, George? Do you have it handy. I think it's 1024, isn't it?

MR. McCANON: The crest is 1020.

MR. SIESS: The crest of the spillway, so this is with gates down and 14 feet ahead over the spillway.

MR. NEWTON: Yes. As I say, it has been some years, and if you want those details we will get them for you.

MR. EBERSOLE: Don, in general but specifically with respect to Norris, is the safety concept of this sort of thing dependent on having guaranteed operation of the spillway gates?

MR. NEWTON: No, I don't think so. Do you mean the safety of the dam or the safety of the site?

MR. EBERSOLE: Well, the safety of the flooding problem I'm talking about. Are you depending upon, in fact, getting the gates down? That's all I am after.

MR. McCANON: Are you talking about the Clinch River reactor? We don't believe the operation of the gates has any effect.

MR. NEWTON: If we couldn't operate the gates and couldn't raise them and couldn't force the headwaters that high --

MR. EBERSOLE: So you could take the problem as if you had a full maximum flood with the gates stuck in the up position?

MR. NEWTON: I can't answer that.

MR. EBERSOLE: Well, they can stick in either direction if you haven't got power.

MR. NEWTON: I don't know the answer to whether it was stuck up or not. I don't know.

MR. EBERSOLE: That is why I said assuming they were down.

MR. NEWTON: I just don't now know that answer.

MR. SIESS: I think the case that Mr. Ebersole is postulating would be gates up, half the PMF. Now, I assume that would put the water level up above 1035.

MR. NEWTON: I don't know.

MR. SIESS: And then fail the dam with an earthquake?

MR. EBERSOLE: No, I'm not on the earthquake; I'm just on the flood. But I have stuck the gates.

MR. SIESS: Well, why would you limit it to the flood case and not the earthquake case?

MR. EBERSOLE: Well, I had a concept that if the gates were stuck in the up position and due to this terrible storm you had no AC power to operate the gates and you would overtop the gates with them in the up position and that would lead to degradation of the dam in the worst possible way.

MR. NEWTON: I don't know.

MR. SIESS: Overtopping a concrete dam is not nearly as automatic a failure as overtopping an earth dam.

MR. EBERSOLE: I don't know if there are other aspects.

MR. NEWTON: No, there are not. We did not go through and say hey, suppose the gates are up. And I don't know when you would put them up in your flood operations. You have to lift them in a flood operation. We did not go through -- and you would have to postulate at what time they were up.

The Norris Dam, it was pointed out, is a concrete dam, and you are beginning to pile unlikely on unlikely on unlikely now.

MR. EBERSOLE: So you're ready to take the maximum possible flood with the gates up? Do I hear that?

MR. NEWTON: We don't think that is a logical prudent combination. That is, we don't believe the gates will be stuck up. We didn't examine that to determine. If it was true, you would have to raise the gates at the worst possible time and then assume they are stuck. And that just doesn't seem to be engineeringly sound.

MR. EBERSOLE: I think it focuses on the reliability of power for the gates.

MR. NEWTON: The reliability of the ability to operate the gates -- and we believe that with our inspection and maintenance system, with our backup systems and our constant checks, that we can, in truth, operate those gates. There is considerable free board -- I know this -- above the PMF, as we did compute it, but before you get to the top of the dam. I guess it would be that even if you assumed everything worst, the worst storms at the worst time, the worst possible storm at the worst possible time, and all your auxiliary power operations and everything goes out and the gates are stuck at the worst, at the top --

MR. EBERSOLE: Why were those engines put there originally?

MR. NEWTON: Pardon?

MR. EBERSOLE: Why were those engines put there originally?

MR. NEWTON: Jess, you always try to have a backup system. That is what we're talking about, is the reliability of the backup system if power is gone. In a storm like, it is very well you would stop generating. As a matter of fact, we postulate that the turbines are not operating in a storm of this storm, and that is not available tous, and that you do have to have some means of opening those gates with the power not available at the site. This is standard operating procedure. And I apologize for not remembering all of those details.

MR. SIESS: I would like to change gears and get back to the slide you have on the screen, which is the OBE with one-half the PMF in a sensitivity study that is based upon reservoir elevation of 1035.

MR. NEWTON: That's right.

MR. SIESS: All of those examples, right?

MR. NEWTON: Correct.

MR. SIESS: And that's 14 feet of flow over the down gates.

MR. NEWTON: Correct.

MR. SIESS: It seems to me that it would be helpful as a sensitivity study to see what you would get there if the gates were up. Would it be overtopped with one-half the PMF? That is, would it be 1035?

MR. NEWTON: I would have to go back and examine -- and as I say, it has been some time since we did these flood routings -- as to specifically what our operations are. The bottom line would be that if you could reasonably postulate -- well, not reasonably because we think we have got one reasonable. But if you were to say everything went bad and you did have a higher elevation than 1035, in truth you would have some higher elevations down at the site.

Once again, assuming that the dam fails, which we don't think it will.

MR. SIESS: But again, for a sensitivity study, that would seem to be an appropriate parameter. I mean, right now I get a certain amount of comfort out of looking at 818 there for just wiping the dam out. In other words, you varied the conditions of the dam in that figure, and it seems to me that varying the elevation of the reservoir would be another basis for a sensitivity study that might come out still comfortable.

MR. NEWTON: It could be it would be lower or higher. We would also have to go lower. I would also point out that another sensitivity is around the earthquake. We don't think the dam will fail in an OBE. It would be more likely to fail in an SSE, and that was the elevation 810 that I gave, in contrast to the 818. So if you slice it that way, you come up with 818 down to 810. If you do simply debris levels you get this, and as you say, whether the 1035 varied up or down, you would come around it.

But I would think that we might want to go below 1035, too. I would have to review the actual details of our assumed operation. And I wish I could remember that because I have a feeling that that is your answer. I think the gates probably got down pretty early in the flood, and it may well have been right at the start. I'm not really sure.

MR. MOELLER: In your opening remarks you said you examined Norris dam in order to postulate whether it might fail, and you concluded that it probably would not, but nonetheless you went ahead and did your analysis.

Now, what do you mean by saying that you examined Norris dam? Did you look at the design, original design, or did you go out? Do they do annual inspections, and you looked at that?

MR. NEWTON: Let me call upon Joe Hunt to answer that.

MR. HUNT: Joe Hunt, civil engineer in our Civil Engineering branch.

We did some analyses of our dam, earthquake analyses. We have looked at the performance of concrete gravity dams during past earthquakes. We have regular inspection programs where we go out and inspect our dams. Based upon all those things we feel very confident that the dam will not fail.

We have a present dam safety evaluation study under way where we have evaluated the dam for earthquakes, and the earthquakes we are looking at are larger than the OBE. They are not as large as the SSE, but they are larger than the OBE. And in today's evaluation the dam would not fail for an OBE.

MR. MOELLER: Okay. That is helpful. Thank you.

MR. NEWTON: I think the point, in answer to the sensitivity, I think we have gone overboard to try and make sure that the site was well above any flood level. And I'm not just sure where you quit asking these "what ifs," not that they aren't real.

MR. CARBON: Chet, is your question resolved?

MR. SIESS: I would still like to see the sensitivity study on the reservoir elevation, as long as Jesse has the question about the gates. It may not be a reasonable assumption, but we are not noted for reasonable assumptions.

MR. NEWTON: We can look at that. I think I would first like to review and have available the information about what our actual gate operations assumptions were in that flood. We can certainly provide that. Our current workload would make that on down the road some place, but depending upon how important it was, it certainly can be done. And I would have to ask somebody else as to their feelings as to whether you want to get us to get into that.

MR. GROSS: If it is important, we can provide that in a relatively short period of time, probably before the next full committee meeting. That would be one month.

MR. NEWTON: How much time?

MR. GROSS: About one month.

MR. NEWTON: I'm not sure whether physically we've got the manpower that we could put off our other jobs. We will have to argue about that.

MR. GROSS: If I understand the discussion --

MR. NEWTON: We can give the discussion about the gate operations and the rest of the assumptions to test in truth to see about the 1035, but to do a series of runs postulating 1035 or 1040 or whatever the different elevations are, and then the downstream flood levels, you're talking about a healthy amount of work. And whether that could be done in a month or not I don't know.

MR. CARBON: I guess there would be no great need before the April meeting, so a couple of months is all right. But we would like it then.

MR. GROSS: Okay.

MR. CARBON: I have one more question there. What role does the Melton Hill dam play?

MR. NEWTON: Very little. It is not important to the final conclusion. That is assumed to fail.

MR. CARBON: The dams of the other branch past Tellico and so on, do they play a role in terms of if

they go out and you have flooding below Kingston, that
this backs up and affects CRBR?

MR. NEWTON: These were all examined, and they are noncontrolling. That is in the SAR. I don't remember the exact flood levels, but they were all investigated, and they are lower flood levels.

MR. CARBON: But they don't in any way cause backup combined with Norris that would change the values you have given us here?

MR. NEWTON: You postulate a centering of an earthquake, and we looked at an earthquake centered such that Norris would fail. We looked at the forces on the surrounding dams, and there would be no failure. So what we have done is a whole series of movements of earthquakes throughout the region looking for combinations of possible failures and this kind of a thing. And there are no other failures coupled with Norris. There are other failures of other dams in which Norris is not impacted and doesn't fail, so it is not a reasonable or a logical combination. We have looked at those logical combinations.

MR. CARBON: Any further questions?

(No response.)

MR. CARBON: I guess that's it then. Thank you, Mr. Newton.

We have a break scheduled. I guess we might as well take it now.

(Recess.)

MR. CARBON: Let's continue.

MR. MORRONE: Good morning. My name is Tony Morrone. I'm with Westinghouse Advanced Reactors division.

(Slide.)

This presentation is on reserve seismic margins available beyond the .25g SSE of CRBR. The evaluation was made based upon a generic basis with ratios and extrapolations.

As I was saying, the evaluation was made on a generic basis with ratios and extrapolations from linear elastic analysis. We have not performed nonlinear inelastic analysis, nor have we made a statistical evaluation.

The margins thus determined are applied to the CRBR SSE to determine a maximum ground acceleration or a reserve margin earthquake at which systems, structures and components begin to fail.

MR. OKRENT: What does "generic" mean as you used it?

MR. MORRONE: We did not look at every piece of equipment. This, I believe, will become evident as I show how some of these margins were obtained.

(Slide.)

First of all, I would like to define some terms so we can understand what the reserve seismic margin is; and that is, the seismic reserve strength or capability available when the calculated effects stress functional performance due to all loadings equal allowable limits, like code or performance.

In some specific evaluation we also considered a design margin, which is the ratio of the allowable over the calculated. I would also like to define a nominal margin. This is discussed in the Battelle Labs report on realistic seismic margins by Rutabo and Desei. And this is the ratio of the ultimate strength over the allowable stress when the allowable stress is equal to the calculated stresses; and that is the seismic stress and the nonseismic stress.

MR. OKRENT: What has that got to do with reserve margins, in your opinion?

MR. MORRONE: The reserve margin will be based on a nominal margin, and I will show you that after this nominal margin is determined, that I will convert this nominal margin into a seismic only margin just using the seismic portion of the load.

MR. OKRENT: But I'm trying to understand, are you suggesting that one can go to ultimate before failure of all of the components?

MR. MORRONE: I am going up to failure, and I am using ultimate.

MR. SHEWMON: Let me ask a different question. I was going to ask you if all of these stresses were elastic.

MR. MORRONE: All elastic.

MR. SHEWMON: Say they assume it's a piece of glass. The ultimate is still elastic, I guess.

MR. MORRONE: The ultimate given by the ASME code, yes, sir.

MR. SHEWMON: So what is the ultimate given by the ASME code? How is it established? Physically what happens to the material at that point?

MR. MORRONE: Well, I would say that deformations become so large that there is structural damage at that point.

MR. SHEWMON: Now, you have run a stress-strain curve. Where is it? Is it at the ultimate stress or at the elastic limit?

MR. MORRONE: At the ultimate stress.

MR. SHEWMON: So it is not elastic.

MR. MORRONE: Correct. But as I mentioned, we based this evaluation on ratios to ultimate.

MR. SHEWMON: So what you have is some stress, what the elastic strain would be to give you the stress that would correspond to the ultimate, or is it the strains which would correspond?

MR. MORRONE: It is the stress which would correspond to the ultimate. I do have a vu-graph here that will show the margin that we get to ultimate. Perhaps that might clear it up.

MR. OKRENT: I would like to understand why it is whatever you're using is the right thing to use. It's not clear to me whether you're talking about many oscillations or a single applied load, for example, in deciding what the failure point is. And also, I don't know why for some components it is suitable to go to failure, because something may need to function or it may need to form and not allow something to function or whatever.

What you're doing may be fine, but I am unable to relate it to the application.

MR. MORRONE: If you will bear with me, perhaps I might answer your concerns as I go through the presentation.

MR. EBERSOLE: Pardon me. I guess I can't believe that that is a conservative way to calculate margin when you use S_u as the numerator. I got the impression that S_u was when you are really gone.

MR. BENDER: Why don't we wait and hear his story and then challenge it?

MR. MORRONE: There will be a lot of calculations that you may want to challenge. But anyway, to continue, the reserve margin earthquake then is the .25g SSE at Clinch River times this reserve seismic margin.

The sources are the conservative predictions of building and equipment response and conservative definitions of structural and functional performance limits.

(Slide.)

Okay. I would like to show you a system evaluation procedure. We have this diagram with two branches. The righthand addresses the reserve seismic capability of buildings and structures, which is given by the product of the structural strength reserve capability and the seismic restraint conservatism.

The lefthand branch addresses the reserve seismic capability of system equipment which is limited by either the structural reserve capability or the equipment functional reserve capability. So we consider the lower of the two to arrive at the equipment capability.

The equipment structural reserve capability is given by the product of the structural strength reserve capability and seismic response conservatism. Similarly, the equipment functional reserve capability is given by the seismic response conservatism and the system functional reserve capability. All of these blocks contribute to these margins. We will go through all of them.

MR. WARD: Since you may be talking about -- I gather you're going to be talking about strains beyond elastic, isn't there some interaction or potential for interaction between those two legs of the system as you've described it there.

MR. MORRONE: In the equipment?

MR. WARD: I mean in the building and structure. If it is deformed beyond, can't that have some effect on the capability of the system contained within the building?

MR. MORRONE: Yes.

MR. WARD: But that is treated in your analysis.

MR. MORRONE: To a small extent. For example, when the building -- when the reinforced concrete cracks, the reinforcing steel would yield, we do have lower responses for the building. So the equipment flow response spectra would pertain to this, so it would have a positive effect on the capability of the equipment.

MR. WARD: Positive in that case. Is it positive in all cases?

MR. MORRONE: In this example it would be positive in all cases, because most of our equipment is designed with response spectra techniques, and we use elastically-derived spectra.

(Slide.)

This vu-graph shows the equipment structural strength reserve capability. The first is given by the material minimum strength assumptions. The ASME code dictates that minimum strength values be used to derive allowable stresses. However, for seismic capability, a reserve margin earthquake, it is appropriate to use average strengths.

From a study of ultimate strength curves published by ASME, we found that the ratio of average to minimum ultimate strength is 1.25. However, in this report by Rutabo and Desei, they mentioned a ratio of 1.2, so we used those lower ratios here.

MR. OKRENT: Before you go on, I guess, I am trying to understand why it is appropriate to use average, since if some components or some parts of a component are stronger than average and some components or some parts of a component are weaker than average but always above code minimum, using the average will certainly underestimate the capability of the stronger part, but it will overestimate the capability of the weaker part. And it is the weak point that is of interest.

Could you help me?

MR. MORRONE: Well, for design purposes I agree with you. We should not use average. But remember, what we are trying to do here, we are trying to determine the largest earthquake that this plant could take before the system starts to fail. And I believe in this case using an average value is more appropriate than using a minimum value.

MR. OKRENT: I would agree if somehow you have eight columns, and they share the load in such a way that if one gives, the other picks it up and so forth, and in some way it is the average strength that works. But on the other hand, if I postulate two pipes, each of which you need one of them to have to serve a vital function, and one of them is above average and one below average, using the average doesn't tell you what will happen.

MR. DICKSON: This is Paul Dickson of Westinghouse.

I think we're missing a little point here. This plant is designed to an SSE of .25 g's for a zero period of ground acceleration, and it is designed to all the code rules.

Now, all the components that must be Category I will survive in the most conservative of analytical methods as well as the use of minimum straight properties. What we have been asked is since there is a concern that the probability of an earthquake larger than .25 is not absolutely zero, what is likely to be the size of the earthquake where you really have failures? And that is what we were trying to look at.

We are not pretending that this is an absolute guarantee that you can go beyond that. We guarantee .25 g's ground acceleration. We're just trying to make a reasonable view at what is the size of the earthquake where you would expect failures to occur. And I believe that if you will let Mr. Morrone continue through this a little bit, we might get to that point.

MR. OKRENT: Well, he is proceeding along making certain assumptions. I would like to understand the basis for the assumptions. And by the way, we might as well go back to a point that was a little bit obscure, I think, when the Staff made their presentations, obscure to me compared to what I see in their evaluation.

What I read from the USGS is a somewhat indefinite position concerning what they would choose as even the SSE for the site, let alone what the frequency of the SSE is that he mentions.

So I think in the first place it would be awkward for everybody, for example, if this plant were built to a certain basis and had no reserve margin really, and three years later, as happened in other plants, there is a re-evaluation for some reason.

Well, whatever basis you're using, it seems to me you should be able to say why it is relevant; and I'm trying to understand why in this particular case average is relevant rather than what you think is the point above minimum that you are pretty sure will occur.

MR. DICKSON: And basically that is what Mr. Morrone is trying to address when he gets to the end. He doesn't take the best of all of these margins. He takes where he thinks the mean is. But when he gets to his final number, it is not a number that if the SSE were changed to we could immediately accept it.

MR. WARD: This is sort of a best estimate reserve margin?

MR. DICKSON: That's correct. That is what it is, and we want to present it on that basis.

MR. MORRONE: For a design basis earthquake we have to remove the conservatism. The conservatism is a big source. Certainly the other area of structural strength reserve capability is the code design stress limits for service limit level B, which is the faulted condition.

The components, of course, are designed for an allowable main tensile strength stress of 70 percent of the ultimate. Therefore, we obtain a ratio of ultimate strength to allowable stress of 1.43, which is just the reciprocal of .7. And the total structural strength nominal margin is 1.72. And I sort of underline here "nominal," because the seismic only margin given by only the portion of the loading that is seismic will be higher than this.

(Slide.)

Now we come to the system seismic response conservatism. This consists of five items. First is the system damping assumptions. The damping values used in CRBR are consistent with Regulatory Guide 1.61, and they are conservative, especially the 3 percent of damping value for equipment and large piping.

From a Westinghouse report on damping values of nuclear power plant components it was shown that a 4 percent damping value was conservative, and we design our PWRs with this 4 percent value.

Now, considering the fact that for this evaluation we are at stress levels at or near ultimate, a realistic damping value would be at least 5 percent from floor response spectra for Clinch River, a typical reduction in going from 3 percent to 5 percent is 1.2.

The other source is the development of the ground accelerogram. In generating a motion time history for the ground, we envelop the NRC criteria response spectra very conservatively. It is shown in the handouts. And also, we do not take advantage of the standard review plan rule that we can be below the criteria response spectra by five points.

We have estimated the conservatism here as being 5 percent. Also, there is a reduction of floor response spectra due to the inelastic action of the building. This will be discussed in more detail when we come to the building margins. We only take a 5 percent factor for this reduction.

We developed design response spectra with computer-generated spectra by enveloping the upper and lower bounds of soil moduli by widening the peaks and smoothing the spectra to eliminate valleys and spectral fluctuations. In addition, the flow response spectra are conservative when applied to a piece of equipment uncoupled from the building.

Newmark showed that there is a limit on the amplification above the design response spectrum and floor response. Our floor response spectra exceed this limit. So for this total conservatism it's within 10 percent.

MR. BENDER: I just wanted to establish a frame of reference. This kind of analysis is evidently being done to show that the pressure maintaining capability of the system has some margin. If you're referring to the Westinghouse work, for example, that was mainly on piping systems, as I recall.

MR. MORRONE: Not piping systems, no, sir.

MR. BENDER: It was not?

MR. MORRONE: Do you mean the report on damping values?

MR. BENDER: Yes.

MR. MORRONE: No. This considered all equipment. I happen to be the author of that report, and I had Japanese test data, the Indian Point No. 2 results.

MR. BENDER: But it was all based upon a level of stress applied under pressure loads, wasn't it?

MR. MORRONE: I don't think pressure loadings even came into it. It was with small excitations. Some of these tests and structures, they came from glass tests, some laboratory tests. Pressure did not enter into this.

MR. BENDER: That is helpful.

Is there any question of displacement in this analysis, deformations of any sort?

MR. MORRONE: That comes in in the functional capability where some equipment is limited to a yield criterion such that they will not deform excessively. So the functional performance is ensured by limiting the stresses at or near normal yield. And I will show an example of one of these components that had that kind of functional limitation.

MR. BENDER: Okay. Thank you.

MR. MORRONE: For time history analyses we modified this for the possible frequency variations of the building. One way would be to vary the time interval at which the acclerations are given analogous to compressing and expanding the time history.

The other method that we use on CRBR is to develop spectra consistent histories; that is, we modify the original motion and ensure that the response spectrum of this original motion fully envelops the design response spectrum. So we should have at least a 10 percent conservatism in here.

So the total system seismic response conservatism is the product, of course, of these first three factors, and just once, the 1.1, depending upon which type of analysis we perform.

(Slide.)

Okay. Now we come to the seismic only margin. The equipment structural reserve seismic margin is the product of the structural strength reserve seismic margin and the seismic response conservatism.

Now, recall that we have had the nominal margin of 1.72 for structural strengths. Using this report by Kutabo and Desei, which is NUREG/CR-2137 and using their terminology, we define a seismic only margin with either of these two equations.

The question here is by what amount can the seismic load only be increased to yield a nominal margin of 1 instead of 1.72. So with a conservative assumption that the portion of the seismic load -- and our equipment varies from 60 to 90 percent -- substituting in this formula we obtain a structural strength reserve seismic margin of 2.2 where K equals 60 percent, 1.8 where K equals 90 percent. Therefore, multiplying by the seismic response conservatism, we obtain equipment structural reserve seismic margin from 2.6 to 3.2.

MR. OKRENT: Is this where I should understand why ultimate is the right thing to use when you may have some hundred or so oscillations? In other words, I'm trying to understand are you picturing where you're going through significant plastic deformation but not reaching ultimate on each of these oscillations and therefore not failing.

What is the picture you have of what's going on?

MR. MORRONE: The picture is that how far can I load this equipment before it starts to fail.

MR. OKRENT: But there is a difference between loading once to near ultimate and loading 100 cycles to near ultimate. Do you agree?

MR. MORRONE: Are you talking about fatigue?

MR. OKRENT: I am talking about multiple large plastic deformations, that you can a failure mode due to that without ever having nominally reached ultimate low cycle fatigue.

Now, I'm just trying to understand whether you think in what you've done you've incorporated that, and if so, how; or if you think in fact you're not getting to the region of low cycle fatigue because of what you're doing. I'm trying to understand your picture.

MR. DICKSON: Excuse me.

Tony, how many cycles do we take in SSE and how many of them are close to reaching the .25g acceleration?

MR. MORRONE: Well, first of all, fatigue is not evaluated for a faulted condition, but the SSE, of course, it is one SSE, and there are ten cycles of motion in the SSE. So we're talking about ten cycles.

MR. OKRENT: Yes, but we're also talking about larger earthquakes which are longer, which may have more cycles.

MR. MORRONE: Peak cycles. I'm talking about peak cycles.

MR. OKRENT: So am I. Again, I'm trying to understand in what you've done whether you've included an allowance for low cycle fatigue or you don't expect to get into that region or just what.

MR. MORRONE: Fatigue, in my estimation, is not a consideration for the SSE. It is for the OBE.

MR. OKRENT: I'm sorry. You're trying to show us how much margin you have, and you are using ultimate as a measure. If you're going to ultimate, you are going beyond the point at which you normally design for the SSE. And you tell me fatigue is no consideration for the SSE, and I agree.

MR. MORRONE: Due to the small number of cycles.

MR. OKRENT: I would like to know whether this has been factored at all into your choice of ultimate.

MR. MORRONE: No, sir.

MR. OKRENT: It has not?

MR. MORRONE: It has not been.

MR. OKRENT: Okay. Then that is something to be looked at possibly.

MR. EBERSOLE: Are you going beyond the point where you get permanent deformation just a little bit? All right. Then if you have ten cycles, why aren't these just arithmetically added, because I don't see any reason for the equipment to return to its original position.

MR. MORRONE: Okay. But remember this evaluation is not based on strain levels.

MR. EBERSOLE: Well, I think that is one of the problems. Later on I'm going to ask you how you're going to design a battery rack, and I'm going to do so for a very good reason: the belief that you must consider strain, because it will depend upon the level of movement that is permitted in such a design as to whether or not you succeed in shutting the plant down.

MR. MORRONE: Is that usually analyzed or is it tested?

MR. EBERSOLE: I think it is analyzed from the standpoint of the straps that you put around the battery. Now, on a modular basis they may shake an individual battery, but I don't think anyone shakes an entire set of cells.

What you do is you bolt these rigid and inelastic cells together with hard copper bus bars, and you create a system which is highly susceptible to mincr deflections.

MR. OKRENT: By the way, is there some report I should have read that gives all of this in much more detail?

MR. MORRONE: This report here is a good reference.

MR. OKRENT: No. But that describes what you are presenting here, your analysis and so forth. Has that all been documented?

MR. MORRONE: There is an older report where we presented a seismic capability not going to ultimate. Okay. We just considered basically the conservatism we had in our design, and those were the margins that we had at which the plant could operate without any problem.

I understood that we wanted to look at the margins beyond the SSE, and this is my attempt to show margins beyond the SSE.

MR. BENDER: It's been a while since I looked at this report of Rutabo and his associates, but I think this was sort of an arbitrary definition that he used to develop his margin of conservatism.

But really to do this right -- and I'm not sure you need to do it right -- but it seems to me that if you were trying to be rigorous about it, you would be trying to look at what the deformations were as a function of the load. In a seismic event the loading has a very short peak period. Even if it is a long period, the time at peak load is very short; and so the time at which you would see the forces representing ultimate strength would be short.

And it seems to me to look at it you would want to have some kind of a stress-strain relationship that is a function of time. Have you tried to look at anything like that?

MR. MORRONE: No. You would do this for all equipment?

MR. BENDER: No. I would try to do it for some typical equipment just like you're talking about doing. I looked at a few typical applications so I could get some feeling for what the relationship is between the loading that might represent peak conditions and what your nominal design loading is.

MR. MORRONE: And what would you consider as failure then due to strength?

MR. BENDER: Well, I wouldn't really try to get to failure. I would try to look at something a little bit less than failure, because failure is a very hard thing to define.

MR. SHEWMON: But that's what he's asking you to design. Granted everything you've said, you have to take some condition.

MR. BENDER: Well, I don't want to find the margins of failure. I want to find out whether there is some margin, and I can define it anywhere I want to. I can define it at 90 percent, which is not a bad way to do it.

MR. PALM: I think perhaps to alleviate the committee's concern, there is additional margin beyond what Mr. Morrone has identified as the seismic capability limit. And that is about in the order of 70 to 80 percent of the whole plastic strain range of the material. When you are talking ultimate strength as your maximum capability, well, you're just up at the front end of the flat part of the curve on a stress-strain curve. And you do have a significant plastic range beyond that.

MR. BENDER: For a period of time.

MR. PALM: For a period of time.

Now, the important point is that under dynamic type loads, particularly for the safe shutdown earthquake, the structure or the component isn't going to feel it once it gets out in that plastic range. It will feel it to a certain point, but it will not be a sustained type of load where the strain will keep increasing until you do reach a rupture limit.

So that is a very important fundamental to understand, that we are nowhere near that point. We are up at what we consider the ultimate. You do have a lot more to go before you even would start to worry about rupture.

MR. WARD: I think that may be true for a system where you worry about rupture, but here we're talking about not just structures but equipment. And in many cases you are concerned with loss of function somewhere. So I think maybe it's a little too optimistic to say you have that long part of the curve available.

MR. MORRONE: For functional requirements we do limit the stresses to the upper level.

MR. SHEWMON: Tell me, is it inherent in all earthquake situations that one goes through several cycles and ultimately then builds up enough amplitude to take it to yield?

MR. MORRONE: Well, the cycles of the earthquake are a direct function of the amplification.

MR. SHEWMON: Okay. But what you -- you see where I'm coming from is that these damping values, though the engineering profession is unwilling or incapable of giving much credit for it, the damping values rise precipitously as you approach the yield; and if you try to take an harmonic oscillator and oscillate it at its yield strength, you've got a hundred percent damping.

MR. MORRONE: They do increase, I agree.

MR. SHEWMON: So if one was or I was convinced that indeed all of these things had to oscillate and build up in amplitude until they got to the yield, then I would take much more comfort out of the assumption of 5 percent or 3 percent damping because I know that is extraordinarily conservative as you approach yield.

But I am not familiar enough with the things you are treating to know whether or not indeed it takes several cycles for all of these things to build up to that load or not.

MR. MORRONE: Well, certainly if we had an earthquake with a single spot there would be no buildup, so it is a function of the DBA and the number of cycles in a motion which builds up. Of course, when we say yield we do mean that the state of stress in the component is yield.

MR. SHEWMON: But that state of stress is critically dependent on the damping and how much energy has been dissipated in the cycles before it gets to yield.

MR. MORRONE: Yes. With an assumption of a certain damping value when we determine the stress. Now, you may say well, but with this stress you could use some more damping; so if you put more damping, then the response is lower. So you could go in circles that way, but they are interrelated.

MR. SHEWMON: I think that is probably the main source of the conservatism, though it gets complicated enough that it is very difficult to take any credit for it.

MR. MORRONE: Well, I feel quite comfortable with 5 percent damping to ultimate.

MR. PALM: Would the committee like to see a diagram on the blackboard of perhaps the conditions of conservatism in this assessment?

MR. WARD: Yes, sure, if that's all right.

MR. MORRONE: I do have near the end a list of additional conservatisms which we have not addressed.

MR. PALM: It is a function of strain and stress. You draw a simple stress-strain diagram of steel material. Normal allowable stresses are basically tied in with this point here on the curve below the yield limit -- the yield limit being basically about 7/10 of -- allowable limit being about 7/10 of yield. You get up to yield at this point on the curve, and beyond this we reach a point which is identified as the ultimate limit.

Now, the margin that Mr. Morrone is talking about is basically the difference between the allowable and the ultimate. The question was raised about additional capability to sustain cyclic effects, dynamic loads beyond the design basis earthquake that we have identified. This is what you do have available, and this is a very significant road out on the plastic range.

Now, I'm talking generically. We can be relating this to a structure. We can be relating it to a piece of pipe or a reactor vesssel.

MR. MORRONE: There's a large amount of energy absorbed.

MR. EBERSOLE: Can you relate it to a piece of equipment which is sensitive to displacement, like a shaft, running in a shaft bearing, or I mentioned earlier a brittle set of batteries contained in a bunch of iron straps.

MR. PALM: Yes, this could be displacement oriented. It is directly correlatable to displacement. But when you're talking displacement you have to get into specifics, whether you're talking about a shaft or a vessel.

MR. EBERSOLE: What I'm really saying is much equipment is going to be displacement sensitive and not strain sensitive.

MR. PALM: I understand that.

MR. MORRONE: Well, for that the limit is yield, so it is elastic.

MR. EBERSOLE: No. I'm saying if you go beyond a certain level of amplitude of movement that the machine quits even though it is still in the elastic regime.

MR. MORRONE: But the stress limit will prevent that from happening.

MR. PALM: That is all I wanted to say. If you wanted me to expand on this, or if Tony wanted to.

MR. WARD: Tony, I guess what I haven't understood yet is you have said that pieces of equipment that perform, they are functionally limited. You estimate they could just go to the elastic limit.

I don't see where that comes into your

equations here. I guess you're treating it generically.

MR. MORRONE: Well, I do have another slide that will show that.

(Slide.)

I just wanted to show you an example of a specific component, in this case the containment vessel. From a buckling analysis based upon ASME code case and 284, we obtained a nominal margin of buckling of 1.9. Then again, a seismic only margin when plugged into this equation with an actual ratio of seismic to total loadings of 70 percent comes out to be 2.29 along with the system response, seismic response conservatism. The equipment structural reserve seismic margin would be 3.3 as limited by the containment vessel.

Paul Falk this evening will give you an example of another specific component -- the steam generator.

Now, we come to the functional reserve capability. As you saw from the system evaluation table --

(Slide.)

-- The equipment capability is also affected by functional requirements, and these requirements are most important for the shutdown system and the shutdown heat removal system.

Now, the question here is not stress but the design intended function. The shutdown system components must remain structurally intact with no excessive deformation. Also, the effect of seismic excitation on scram insertion rates must be shown to be acceptable.

This vu-graph shows the margin that we have in our scram calculations. The scram analysis is performed with various retardation forces, including impact forces given by the earthquake. These impact forces are multiplied by a certain coefficient of friction to obtain equivalent vertical frictional forces which retard the scram time.

The first item of conservatism was that the scram insertion performance was evaluated by factoring the impact forces to .33 g's instead of .25; therefore, we have an initial margin of 1.32. Then for reactivity insertion the assumption was made of the worst case rod position and minimum rod worth rather than maximum rod worth and associated rod position; and this was calculated to be equivalent to an increase in the earthquake of 10 percent.

The other item is friction coefficient. We used a value of 1.0 from tests on the primary control rod system. We obtained a mean sliding coefficient of friction of .45, so the ratio of those two gives us 2.2. Also, we did not use any impact damping. And a sensitivity calculation with a 10 percent impact damping value based upon 50 percent coefficient of restitution showed us that the impact forces would be reduced from 7 to 10 percent, which was a factor of 1.07 here. Therefore, the functional reserves seismic margin is the product of those four, and multiplied by the seismic response we have a seismic margin of 5.0 for scram calculations.

MR. OKRENT: Before you leave that, in that friction coefficient what goes into the 0.45, which I guess is the number you think is better. How well is this ascertained, and does this allow for stainless steel swelling and flowing or whatever kind of distortion might occur?

MR. MORRONE: They are actually tests that we perform at ARD with a full-size control rod drive.

MR. OKRENT: But that would not be on a core which has been subjected to distortion over life.

MR. MORRONE: Well, this friction is between the drive line and the guide tubes.

MR. OKRENT: But does it depend at all on alignment questions or things of this sort? I'm just trying to understand.

MR. MORRONE: I don't think so.

MR. SHEWMON: The normal force would. He's talking about a friction coefficient which is the ratio of the normal force to the sliding force. And I think you are saying he may not know the normal force very well, and he is saying but he measured the ratios. I think you're talking about different things.

MR. DICKSON: Yes. Let me add to that. That 1.0 coefficient, friction coefficient was used in the analysis. That assumed the worst possible boiling in a destroyed core and the worst possible offset of the upper internals to the core itself.

MR. OKRENT: Yes, but you're now assuming .45 in this analysis which related to, if I understand correctly, a situation -- well, I'm just trying to understand.

One other small point: in the 1.45 number, part of this came from using 5 percent versus 3 percent damping.

MR. MORRONE: Yes, sir.

MR. OKRENT: Now, does that change indicate something that you feel should apply for all systems or for piping systems? Is it something that originates in the building itself?

MR. MORRONE: There would be additional

equivalent damping given because of the cracking of the concrete in the building. We have not considered that. This 5 percent is for equipment and large diameter piping, all equipment in large diameter piping that were allowed to use 3 percent for design purposes.

I'm saying that the close to ultimate, there is so much energy dissipation in --

MR. OKRENT: Well, I don't know what the valid response is, but it is conceivable to me that you're building to stay elastic and not cracking or whatever, and so transmit forces with sort of normal kind of damping and control rods are not the large piping. They might get whatever it is is transmitted via the floor which is maybe very strong for whatever reasons or whatever. But I'm just trying to understand now whether that factor of 1.45 is good for all equipment of what.

MR. MORRONE: I believe it is good for all equipment. We're not stipulating that the building would remain elastic and the equipment would remain elastic. We're saying for stress levels at or near ultimate what would a realistic damping value be.

MR. OKRENT: Okay.

(Slide.

MR. MORRONE: For the shutdown heat removal we considered two systems, or rather we evaluated two systems: the normal system and a backup system. The normal system capability is limited by the rupture discs that are in the sodium piping between the IHX and the steam generator.

The evaluation was based upon the worst loop. The other two loops experienced lower seismic excitations, but we did not have the calculations for those. They would have been more appropriate to use for this evaluation.

And you can see from here how we arrive at the seismic response conservatism, how we have this grading of 339, and then we subtract five-year aging effects and come up with 296 psi. Then we subtracted the steady state operating pressure, normally 77 psi for seismic. The calculations with the .25g SSE give us a 45 psi pressure; therefore, the rupture disc functional reserve seismic margin, that is 1 plus the 32 or 45 or some place about 32 divided by 45.

Now, you see, the seismic response conservatism here has decreased. It is no longer 1.45, because in the analysis of the piping systems, the time histories were used directly as provided by the architect engineer rather than develop the design margin. So I took that margin out, and we have a functional reserve margin of 2.26 for the rupture discs.

MR. EBERSOLE: How do you test the rupture discs?

MR. MORRONE: How do we test the rupture discs?

MR. EBERSOLE: Yes. It is just like a fuse, isn't it? You can't test it.

VOICE: Infrequently.

MR. EBERSOLE: Infrequently somebody said. So what is your level of confidence that you in fact obtain on the ruptured stress that you intend to get? Do you do a PRA on that or something?

MR. MORRONE: I don't know.

MR. CLARE: George Clare from Westinghouse.

We have not, of course, tested the rupture disc units that will be used in the plant. What we have done is to build identical units from the identical material that will be used in the plant and tested them, both in the sense of a static test and also in the sense of putting them in sodium systems and testing them with various types of loading there to understand their failure or their behavior as they rupture.

MR. EBERSOLE: How thin is the material?

MR. CLARE: I can look that up for you and when we get to the steam generator presentation later today I will give you that number.

MR. EBERSOLE: Thank you.

(Slide.)

MR. MORRONE: The backup system for heat removal is the direct heat removal service, DHRS. And here is a list of the major components of the system. We evaluated all of them, and they showed very large margins except for the electromagnetic pumps which were limited by functional criteria by keeping the stresses at or below yield.

The evaluation of these pumps is shown here.

(Slide.)

Again, the calculated design margin based upon yield criterion of 1.01. That is, we were this much below yield than with the material minimum yield strength assumption. We obtained the structural strength functional margin of 1.21. The actual ratio of the seismic to the total loading is 32 percent. Using these values in the equation we have seen previously, we have obtained a reserve seismic margin of 1.66, which when multipled by the system seismic response conservatism gives us an equipment functional reserve seismic margin of 2.41.

By the way, this is the limiting margin that I will show you in our conclusions.

MR. EBERSOLE: May I ask a question? Do you need DC, direct current, to shut this plant down and monitor the success of the shutdown? Do you need the DC power supply?

MR. DICKSON: Not to shut it down.

MR. EBERSOLE: I mean to remove decay heat.

MR. DICKSON: You don't need it to remove decay heat. You need it to monitor decay heat, and you need it to control the water level in the steam drum.

MR. EBERSOLE: What about the EM pumps?

MR. DICKSON: You do not need the EM pumps to shut this plant down.

MR. EBERSOLE: You're telling me you can really go completely blind on DC power?

MR. DICKSON: Those EM pumps are strictly backup.

MR. EBERSOLE: So the awkwardness would be in not knowing what you're doing, right?

MR. DICKSON: It is conceivable that you could send two operators; one to operate a valve and another one to look at the site class in the steam drum, and they could do that. Obviously, that couldn't be done instantaneously.

MR. EBERSOLE: Are there any environmental heat-up problems that would result from that, a failure of DC, because it automatically means you would use the AC cooler?

MR. DICKSON: We have evaluated that and there are some equipment losses we sustained. If you have a sustained loss of all AC power to provide cooling, you could still successfully shut the plant down.

MR. EBERSOLE: Thank you.

MR. MORRONE: These, by the way, are tested.

MR. DICKSON: The only things that need DC power for safe heat removal is some valves that provide the aux feedwater to the steam drums.

MR. MOELLER: Are the approaches that you are using in calculating these reserve, or the margins, are these standard procedures? Why I ask, like you say, the scram insertion performance was evaluation to .33 g instead of .25, so you divide .25 into .33 and you get 1.32.

Now, is that linear? You're treating it linearly?

MR. MORRONE: Certainly.

MR. MOELLER: There's no question but what a .33g is exactly 1.32 times as bad as a .25.

MR. MORRONE: Well, the impact forces do come from a non-linear analysis of the drive line. But the inputs to this non-linear analysis come from a reactor systems analysis. So the input to the non-linear analysis would be, of course, just the ratio of the maximum accelerations.

MR. DICKSON: If I could add just a little bit, in some cases the results of the analysis is non-linear, but when he gets down what he is looking at is what is the size of the earthquake that this plant can really stand and has margin to stand. And since that shutdown system was evaluated specifically for a .33g earthquake, that clearly is one place where you could use the ratio of the two.

MR. MOELLER: Okay, thank you.

MR. MORRONE: Before concluding the presentation on equipment, I would like to show examples of the reserve seismic margin for equipment, which is qualified by tests rather than by analysis.

(Slide.)

This equipment consists mostly of instrumentation and control equipment of the plant protection systems. The qualification is to IEEE standard 344. We used two types of test motion; single frequency tests at resonance, regardless of what the natural frequency of the building is. We subject equipment with a motion which has the same frequency as the equipment. Also, we use multiple frequency tests with random motion.

The criterion here is a full enveloping of the required response spectrum with the test respnse spectrum. The required response spectrum is the spectrum calculated at the mounting of the equipment. Test response spectrum is the response spectrum of the shake table motion.

(Slide.)

To give you typical examples of some of the equipment that was tested, this is a typical comparison of the RRS and TRS. This is the test response. You can see that the enveloping is very conservative; way above the requirements, to both ZPA values and maximum peak.

We have a similar comparison for the vertical direction. That happened to be for the horizontal direction.

(Slide.)

And this is even better. Based upon these two viewgraphs, --

(Slide.)

then I have a typical example of the reserve seismic margins. For tested equipment, the seismic response conservatism here includes a 10 percent margin required by IEEE 323, but not the margin due to damping because damping is inherent in the equipment. Therefore, the seismic response conservatism is 1.33; then if we determine a margin on the ratios of the ZPAs we would get a reserve seismic margin of 2.85 times 1.23, of 3.79 times the margin to fragility.

This equipment is not tested to fragility. So we have a reserve margin to fragility before the equipment would fail. Based upon the peak we have 2.05 margin to fragility.

MR. OKRENT: Has this equipment all been tested or is it going to be tested?

MR. MORRONE: A lot of it has already been tested. There is other equipment that has not yet been tested.

MR. MOELLER: On this chart now, it is similar to some of the other ones that you presented, you will do certain things to two dozen places and other things to one. Is that because of a less certainty in the development of the required response spectrum? For example, on the shutdown margin, the friction coefficient, you could have divided and gotten 2.22 but you stopped at 2.2. I guess I just need to be educated.

MR. MORRONE: It may be sloppiness, but there is no hidden meaning in the decimal places. The conclusion goes to one place, anyway.

MR. MOELLER: Okay, thank you. Of course, if you stop at one place on the ground and accelerate, what is that accelerogram?

MR. MORRONE: Yes. We could not take advantage of the 5 percent. Sometimes we have to go to two places.

(Slide.)

We now come to the buildings and structures' structural strength reserve, seismic margins. This evaluation was made for shear walls, since most of the load at shear walls are seismic and the structures are designed by strength methods with load factors and strength reduction factors. And the OBE load examination -- and OBE is a service load and, by the way, the design is controlled by service load, the load factor for the OBE is 1.9; for the SSE it is 1.0.

Even though the OBE is one-half of the SSE for maximum ground acceleration, actually the OBE produces much more than one-half the SSE loading because smaller damping values are used for the OBE. And if we conservatively assume that this ratio is 55 percent, then we obtain a 5 percent margin, .55 times 1.9, for the strength reduction factors, the ACI code limits, the allowable below ultimate capacity and reduction factors are applied, which range from .75 to .90. Using the most conservative factor taking the reciprocal of .90, we get a margin of 11 percent.

Also, we have minimum strength assumptions analogous to the equipment, so the reinforcing steel yield -- we estimate that the yield strength is 15 percent higher than specified. The concrete design is based upon 28-day strength. However, the plant will not go into operation until a year or more after the concrete is poured, and we estimate that the aging effects of concrete result in a 25 percent increase in ultimate strength. Since the shear strength varies proportional with the square root of ultimate strength, we obtained a factor of 1.22.

The last item here is the redundant path loads that we have in buildings. Since these buildings are interconnected in a common foundation map -- they are multiple, inter-connected cells -- the failure of one would be picked up by the other. We will only assign a 5 percent margin here, so the total structural strength reserve for seismic margins comes out to be 1.37.

MR. BENDER: It is not clear to me why the concrete and reinforcing steel margins are additive. What is that?

MR. MORRONE: It's not clear why they are?

MR. BENDER: Why they are additive? Where did we get 1.37?

MR. MORRONE: 1.37 is the product of all four, but 1.2 is the square root of 1.25. Now, I did not add the 5 to 15 on top of the concrete aging.

MR. BENDER: Never mind, it's not important.

MR. SIESS: There are some things you've done now that I'm sure are strictly correct, but what you have done I think has been done reasonably conservatively. The 1.37 you come up with is a bottom line and does not look unreasonable. But you must realize that that is an average value.

All of the parameters you've been messing with up there are variables. They have some distribution. Some are low, some are high. And what you have been dealing with pretty much are averages and not with extremes. So on the average, you have a margin of something like that, maybe a little larger.

MR. MORRONE: But on the conservative side; average on the conservative side.

MR. SIESS: I don't know what you mean by conservative side, if I'm talking about a margin.

MR. MORRONE: Well, for example, we use .90 here instead of .75. That is on the conservative side.

MR. SIESS: But the .90 is in there to take account of low yield strength steel, so you've moved it over from the lower side of the distribution to the mean, and then in the next one you've moved it somewhat to the right of the distribution to date. You can't take account of the fact that the average strength of steel is usually higher than the specified strength. The minimum strength is usually lower; the average strength is usually higher. So you've messed with some of the distributions and I could say you counted things twice.

MR. MORRONE: Well in this case, you understand I only counted the concrete reserve strength.

MR. SIESS: Yes, but that only affects shear, and any connections are much more sensitive to flection rather than shear, especially under repeated loads. So the 1.37 is maybe conservative but it is an average and there could be parts of the structure where it is less 1.37.

MR. WARD: But yet, he agreed at the beginning of that that what he is attempting to do is to assess sort of a best estimate of seismic.

MR. MORRONE: A statistical study would be required. This is not a statistical study.

MR. ETHERINGTON: I don't know if this is a best estimate. I don't really think so, because it doesn't take into account things that we have observed. We have observed deficiencies in engineering. We have observed deficiencies of construction, materials and none of these are factored in.

Of course, I would think if someone would try to conduct all of the bad things that they could think of, they could very well come up with a negative margin if they neglected the good things.

MR. SIESS: That is the point I was trying to make; that the worst case is likely to be below 1 and the best case is likely to be well above the 1.37. So if you take the 1.37 as about an average margin, I think it is not unreasonable. And that is what he means by best estimate.

MR. MORRONE: Remember, the purpose of this evaluation.

MR. OKRENT: Well, we will get back to the purpose when you're done.

MR. SIESS: I don't recall that anywhere you factored into the structural strength the fact that all of it is not contributed by earthquake.

MR. MORRONE: At the beginning, when I started discussing the buildings I stated that this considers shear walls.

MR. SIESS: I'm sorry.

(Slide.)

MR. MORRONE: Okay. A second viewgraph for buildings and structures considers the seismic response conservatisms, which is a bit different from equipment we still have, the 1.05 for the development of the ground accelerogram. However, here we considered the reduction of the response spectrum due to the inelastic action of the buildings.

As you know, there is a substantial reserve strength in the inelastic range with energy absorption due to cracking of the concrete and yielding of the reinforced steel. Newmark discusses an inelastic design response spectrum in this document here, NUREG/CR-0098, where the spectral accelerations are reduced for all frequencies below 33 hz. This reduction is a function of the ductility factor and the frequency.

And the suggestion for the ductility factor of structures' housing plus Class 1 equipment was 2 and 3. Also, the constant production in the design spectrum from 2 to 8 hertz was given by this equation 1 over the square root of 2U minus 1. And using four ductility factors of 2 and 3 in this equation, we come up with elastic accelerations reduced by 45 to 58 percent, which results in the reserve margin of 1.7 to 2.2.

Multipled by the 1.05, we get building and structures' seismic response conservatism of 1.8 to 2.4.

MR. SIESS: Are there any cases in the design of this plant where failure would be defined in terms of excessive deformation rather than inadequate resistance?

MR. MORRONE: Probably with the EM pumps. That is why we had the yield criterion.

MR. SIESS: I'm talking about structures now in relation to this slide.

MR. MORRONE: Bob, can you help?

MR. PALM: The answer is no.

MR. OKRENT: How about penetrations in structures?

MR. PALM: I'm sorry?

MR. OKRENT: Where there are penetrations in structures.

MR. PALM: I'm not sure how that relates to Dr. Siess's question.

MR. SIESS: You see, the point is that some of the conservatisms you've taken advantage of here in terms of inelastic behavior will lead to increased strength, but also, to increased deformation, and that is not the conservative direction if deformation is a governing factor.

MR. PALM: Yes, I understand that.

I am not quite sure I understand your question, Dr. Okrent.

MR. OKRENT: Well, I would have to assume that the reliability of some kinds of penetrations in an earthquake would depend upon the amount of deformation. If I am wrong, correct me.

MR. PALM: You are right.

MR. OKRENT: All right, then. I will add my question to Dr. Siess's and ask whether, in fact, that something that one should read about -- it is not in the list of viewgraphs.

MR. PALM: I understand your question now, and that is considered as part of the design process between the architect engineer and the system designer where we do, in addition to providing responses, we do give displacements -- time history displacements, and the design of penetrations that are liked to the structure are also included in the loads and the displacements translated between, let's say, a fixed point on the structure and a connecting system. Does that answer your question?

MR. OKRENT: Let me pursue this just one minute. I would have to assume that you do this at the SSE level. They also did this at the SSE level at Indian Point 2, but when they looked beyond the SSE level, as I am sure you are aware, the clearances were not sufficient. In this case, the deformations might be awkward.

Well, what I find is of interest, but I can't tell --

MR. PALM: I have your question now, and I think the answer is, at least from my end, that we have not checked displacements at penetrations or at similar type of locations.

MR. OKRENT: Well, this is a problem with what you call a generic look, because if when one does a generic look,you are not sufficiently complete, it may be deceptive.

(Slide.)

MR. MORRONE: Here is the summary of the reserve seismic margins. You see that the system reserve seismic margin is 2.4, which is really controlled by the functional reserve seismic margin. This was the DHRS functional margin. The structural reserve margin is 2.6 to 3.2, which is similar to that of the buildings and structures.

MR. DICKSON: Tony, I think it's worth noting here that all of the argument earlier about whether you should go to ultimate does not become a controlling one. It is thrown out with the 2.4 functional, which is based upon yield.

MR. MORRONE: Good point.

MR. OKRENT: Well, that is for some applications; not for all.

MR. WARD: It is based upon yield for that one particular piece of equipment.

MR. DICKSON: Well, that's right, but you see, that is what is the controlling number. That goes right on up to the top which limits the reserve seismic capability for the whole plant.

MR. MORRONE: So we're saying let's take this down to 2.4, then, and that would not affect this reserve seismic capability.

MR. DICKSON: In that chart, only the minimum is allowed to proceed on to the top; whatever is limiting.

MR. OKRENT: Okay. Within that context. But you have left an impression that there are some larger margins on other things based upon the analysis.

MR. WARD: I guess the point is if one of the other systems that was analyzed to ultimate, if it had been analyzed to yield, it might have come out lower than 2.4.

MR. DICKSON: That is possible. What he looked at to yield were those that required dimensionality in order to work, and the two he used here were the EM pumps and the control rods.

MR. SIESS: Let me postulate something, the answer to which might help me a little bit. Suppose now you went about the design of this plant for seismic excitation 2.4 times what it has been designed for in the present SSE, but you allowed a criterion of no loss of function. Would you make any changes in the design?

MR. OKRENT: Or in the tests.

MR. MORRONE: Now, as far as giving the qualification by test, no. Changes in the design, I would not think so. But remember, that we can do that because of code requirements.

MR. SIESS: That was a postulation. It was a way of looking at it. You are satisfied yourself now that you can take 2.4 times the SSE with no loss of function?

MR. MORRONE: No, I'm saying 2.4 times the SSE, we will get malfunction, or it will start there.

MR. SIESS: But at 2.39 there would be no loss of function?

MR. MORRONE: That is the bottom line.

MR. DICKSON: In all likelihood.

MR. MORRONE: I do believe that we have even more than this because of this list of conservatisms.

(Slide.)

I will not go through all of this and take the time. You have it in your handouts. I would just like to highlight that the design of most equipment is controlled by the OBE and not SSE. And again, the OBE loads are greater than one-half of the SSE load. So really, the margin that we had before, the reciprocal of 1.47, that is too low because under the SSE design, we're below the code allowables, quite a bit below because they were controlled by the OBE. And we have all of these margins that really have not been quantified.

So the 2.4 I believe is a good estimate and not necessarily optimistic.

(Slide.)

So in conclusion, we have the CRBRP system. For system equipment we have a structural reserve seismic margin of 2.6 to 3.2, which when multiplied by .25g gives us a reserve margin earthquake from .65g to .8g. However, the functional reserve seismic margin is 2.4; therefore, the reserve margin earthquake is .6g.

For CRBRP buildings and structures, the reserve seismic margin is 2.5 to 3.2, and the equivalent reserve margin earthquake is from .62 to .80. The conclusion, the bottom line, is that the CRBRP seismic capability is at least .6g.

MR. OKRENT: Well, in the letter that the ACRS wrote dated July 13, 1982, there, the last two paragraphs read as follows, and I will just read them and then comment.

"With regard to the seismic design of this plant we believe it is important that the combination of seismic design basis and margins and the seismic design be such that this accident source represents the load contributions to the overall loads of the plant. We believe this matter will warrant detailed examination at the construction permit stage to insure that necessary margins are available for all important systems and components.

"The NRC staff has concluded that the CRBR plant can be designed and constructed in such a manner that it will no greater risk to the health and safety of the public than an LWR plant using current safety criteria. We agree the proposed site is suitable for such a plant."

Okay. Reading that leads me to request the applicant and to the staff -- not to be answered today, but I think, although your presentation is interesting, I don't find myself able to digest it all, nor can I take the set of viewgraphs and send it to appropriate consultants and ask for a review as to its adequacy. So in my opinion, this is an area that should be documented, and with a chance for the ACRS to have it looked at hard as to why what you say is adequate and that your generic sampling is adequate as well as your methodology and so forth for the purpose.

And again, I'm giving an individual opinion, reading this letter and interpreting it I think in what to me is a sort of straightforward way, and I think from the staff at some point we need to hear why, in view of what they now deem to be the margins for LWRs, in view of what they can tell either qualitatively or quantitatively, that the SSMRB program or so forth -- why, in this regard, Clinch River is equivalent to an LWR, since in some of the recent LWR studies the seismic is turning up as a non-unimportant contributor to risk.

And as an afterthought, it seems to me it would be of some interest to see whether, -- without asking the SSMRP program to try to analyze this, which would be another two-year project or something -- whether what they have learned from what they have done introduces any questions to be looked at for CRBR, that perhaps you wouldn't look at without the benefit of whatever comments they have.

MR. WARD: Is there a topical report on the material which Mr. Morrone has covered?

MR. MORRONE: Not yet. We do have no report. We have a 1977 report that discusses margin, but not beyond the SSE.

MR. WARD: Is that report available, the 77 report?

MR. MORRONE: We've given it to the staff.

MR. DICKSON: Yes, the 77 report is available, and basically, it describes the methodology and some of the designs have changed.

MR. CARBON: Let me ask the applicant if it clear to you what Dr. Okrent has asked and requesed.

MR. GROSS: Well, let me try and take a stab at that. I believe that what Dr. Okrent requested was a report that describes in a little greater detail the material which Mr. Morrone presented here today so that he and some consultants could review it.

MR. OKRENT: Well, it is for you to judge what you consider to be adequate to be responsive to the issue. I wouldn't say specifically, it was a report which covered only what was here, because that may not be adequate for answering the question. The material presented here.

In other words, as I indicated earlier, the generic sampling itself has to be thought about and one has to make a case for why it's adequate to the purpose.

MR. BENDER: Could I ask the staff whether it agrees with the conclusions that have been drawn by the applicant?

MR. STARK: I would try to answer that indirectly. I was going to discuss this at the end of this presentation anyway. But ACRS prepared a letter on January 11th which the staff is reviewing right now concerning this particular subject. And the staff has prepared a formal response for it. I had an opportunity to look at it briefly yesterday and it says basically, they agree with the need to assess or produce additional information in this area.

The staff, as far as Clinch River is concerned -- I am going to give a little bit of an overview and then at the end talk about some specifics for Clinch River. The response that the staff has prepared for this letter is saying that the staff and researchers are proposing a seismic research plan to work on this particular item.

In addition to this, the staff is talking about the SSMRP program to see if it can be used or modified to support this particular program. But in general, the staff feels for Clinch River that the structural and equipment design margins can be handled on a generic basis, but it appears that the piping design margins can't be handled generically. And it is the piping design margins that we are looking at and incorporating into our mechanical and materials review for Clinch River.

So, the way I have answered it, I guess to both of you but to Dr. Okrent, is that we hope we can handle a large portion of it generically, but the piping we feel we can't.

MR. OKRENT: Well, I am willing myself to have someone make a case that there are some parts of this plant that look enough like other plants that have been analyzed, even though they are LWRs that you can draw some general conclusions and so forth. And maybe it is only the piping that is enough different. The piping is clearly different, being thin-walled.

I myself haven't tried to look at other parts to see whether there should be a few additional things or many additional things that need their own look. Maybe the piping may be able to stand up, but it should be a considered judgment.

MR. EBERSOLE: Can I ask Ken a question? A while ago I was searching for what I guessed might be the Achilles heel of the shutdown heat removal process, and I picked the batteries because that is where it is in LWRs. In essence, the whole shutdown heat removal process is propped up by the batteries.

I got the impression that you're better off than they are. But I wonder, since I saw a few things here and you mentioned about looking at the gauge glass and I thought about what is getting the water to the gauge and is it continuing to run, so unless you can really show that you can, without benefit of DC, continue the shutdown heat removal process, then we get back to questioning what ought not to be as difficult a design problem: the batteries in the DC systems.

There was an LER not long ago that said in some plant and maybe more plants these critical cells were simply spontaneously cracking. They didn't need an earthquake; they were cracking while they were sitting there. And the reason for that was that the structure of the entire integrated cell was, of course, different from that of an individual -- the entire battery, rather, was different than an individual cell and they were bolted together with rigid copper bus bars, and a minute amount of deflection in these would simply crack these brittle plastic or other brittle material cells, and promptly create an open or short circuit and you would lose the DC.

I only mention this as a point of fine detail where unless you really cover it carefully, the whole show is lost. So I will ask you again to either defend that you can get along without the DC system or show that you will always have it. And I suspect the latter is going to be the case.

MR. DICKSON: I'm sorry, sir, you suspect the latter is the case? That you would have to have the batteries?

MR. EBERSOLE: I don't think you can get along without it.

MR. DICKSON: For all design basis events we have assumed the batteries are available. We have considered what would happen if you lost the batteries, and it can be operated manually.

Now, we have not done a detailed review to determine just how long you have and what are the resultant temperatures. But as was presented earlier, the sodium in the system is fairly forgiving and it absorbs a significant amount of heat. At one of the earlier meetings you saw that we have considerably more time than the light water reactor to respond so while we have not done an analysis to look at it, I am reasonably confident that if we did, we could show that two operators could operate a shutdown heat removal system without batteries, except in their flashlights and in their walkie-talkies.

MR. EBERSOLE: I think that would be a major showing if you could do that.

MR. REMICK: I have a question of the staff. Are there, in the Commission's rules and regulations, any requirement for design margin beyond the SSE for CRBR?

MR. STARK: There are no regulations right now beyond the SSE for any plant.

MR. REMICK: Light water, also?

MR. STARK: That's correct.

MR. REMICK: And so I'm not sure what this exercise, other than being of interest in knowing, but from a licensing standpoint, the importance of it if that shutdown margin was 1.2 or 1; having certificates from the licensee on the plant?

MR. STARK: As it stands right now, according to the requirements it doesn't, but ACRS sent a letter last month to the Commissioners who in turn sent it on to the staff and we are assessing that and trying to initiate new research programs and modify existing research programs to look at this and attempt to address it. But there are no regulations right now.

MR. REMICK: To try to get an understanding of what shutdown margins might be?

MR. STARK: That's correct.

MR. OKRENT: If the staff stays with its current approach -- in a sense one might say it's the legal approach -- that they are faced with this big problem of lots of reactors potentially being in the same tectonic region that contained the Charleston earthquake. I don't think they can live with the past.

MR. STARK: We also don't currently have a good method to evaluate this particular margin, and know that it is conservative and know that it is well tested and well founded, also.

MR. SIESS: If the staff stays with the same legal approach, the remedy is to raise the SSE to a lower probability earthquake.

MR. OKRENT: But we've got a lot of reactors that are sitting there.

MR. BENDER: I think the point I was going to make is simply this. I'm not sure I would agree with Dr. Okrent that we need to change the SSE, but even if we did, it is not necessary to show that you can stand a .6g.

MR. OKRENT: I wasn't suggesting we need to change the SSE. From a safety point of view, I was saying if you say there are certain legal requirements, as in Appendix A, that they go by tectonic regions and use the largest historic one and so on, it creates some problems.

MR. BENDER: Well, it bothers me some that we are trying to -- we have gotten ourselves wrapped up in a legal question. And this is something that ought to be dealt with in a technical way. And really, if the staff has looked at the site enough to draw the conclusion that there is no reason to change the SSE, I think I would have to agree with Dr. Remick that we surely ought not to waste any time trying. But if you haven't reached that position, then we would like to know what it is you are going to go to.

And that's the main reason, as I understood it, that we put something in the letter to deal with margins beyond the SSE. If you can really establish that you've got the right number, it doesn't make sense to start fighting for a bigger number. That is a personal opinion.

MR. OKRENT: Well, Mike, I think there's another interpretation to the letter, and in fact, it doesn't say that the SSE should be larger. I think the question is when you design for the SSE, whatever it is, is the contribution to risk from earthquakes an acceptably low portion of the overall risk and so forth. That is not the same as saying designed for a larger earthquake. And all of the rules and so forth.

MR. BENDER: I don't know what the statement meant, and right now I guess I would like to know what it is we're trying to get at. What do we want the staff to do?

MR. STARK: Let me try it again. The staff believes we would license the plant at 2.5g without a requirement for additional seismic margins. We are, however, openminded to the comment of ACRS, and ACRS has been asking this question for a long time for a large number of plants. And therefore, we are honestly looking i to this particular area for additional information to see what we should do with it.

MR. SIESS: I don't see how the staff can talk about additional seismic margin when they don't know what the seismic margin is now. That is, you don't know the return period of the SSE.

MR. KERR: Well, it might be that they have to do this in order to carry on a conversation with the ACRS, who keeps talking about additional seismic margins.

(Laughter.)

MR. CARBON: I wonder if we could move on. Mr. Clare?

MR. CLARE: Yes.

(Slide.)

I'm George Clare with the Westinghouse Advance Reactors Division. I do have a few comments to make about seismic margins. Perhaps in light of the ongoing discussion this is more of a footnote than anything of a great amount of significance, but let me go ahead anyway. It may help perhaps put a cap on some of the discussion.

(Slide.)

I am not here to give you the results of any independent assessment of seismic margins. The most detailed look that we have is the one that Tony Morrone just presented to you. However, we do have the benefit of a compilation of studies that have been done over the last 10 years or so of earthquakes in the Oak Ridge area, and these have been plotted up on a particular graph by some gentlemen at Oak Ridge National Laboratory. Mr. Beavers, Mr. Manrod and Mr. Stoddart.

And without going into any detail, what you can see from this particular viewgraph is that the range of the studies over the last 10 years is consistent with the conclusion that was drawn by USGS and cited by Mr. Rothman earlier this morning that the .25g SSE chosen by CRBRP would have a recurrence frequency on the order of every 1000 to 10,000 years.

Now, the question is how does this relate to what Mr. Morrone said a few minutes ago. And what he said is that he thought, without being too detailed, that the plant would be able to withstand an earthquake on the order of two times the SSE peak ground acceleration, and that would take us well beyond the point where these curves are drawn, and it would take us well above the once in every 10,000 years.

(Slide.)

The conclusions that we draw from that rather simplistic look at things are, as I said, the recurrence frequency of our SSE is 10^{-3} to 10^{-4} per year. The earthquake with an acceleration twice that of the SSE would have a recurrence frequency considerably less than 10^{-4} per year. And therefore, we think that is a good kind of intuitive feeling for the risk we get from the plant.

Now, in light of the questions that were raised a few minutes ago, I can reflect a little bit on studies that were done by the project several years ago. And we did do some early risk assessments. They were for a design other than the current design of the plant, however, very similar designs.

And what we found, using the analysis methodology that was used in WASH-1400 -- and we are aware that there are some criticisms of that assessment technology, but that was what we used. And in fact, the likelihoods of earthquakes that were used in that study were of the order of the midpoint of the spread of data. What we found was that the risk from large earthquakes was not insignificant, but was not the dominant factor in the risk from the plant. It was a sigificant contributor, but I believe the contribution was less than 50 percent of the overall risk, and, of course, that was broken into several categories and so that is a little bit of a vague statement.

We know more about the plant today, both from studies similar to the ones that Tony said, and most particularly I would point out his comments on the rupture discs in the intermediate heat transport system, where we know a lot more today than we did back in 1976 when the earlier assessment was done. And in fact, that would lead us to the conclusion that the seismic risk is less than what was estimated in the earlier assessment.

The only other point is, of course, that we are doing a PRA at this point in time. Really, we're just getting started on it. It will be concluded sometime in the next couple of years, and that assessment will specifically include an assessment of seismic risk. It will include the consideration of the likelihood of the larger earthquakes as well as the fragility of the equipment for such events. And that is the situation we find ourselves in at this point in time.

MR. SIESS: Could you tell me who Wiggins is on that chart?

MR. CLARE: J.H. Wiggins.

MR. SIESS: From California?

MR. CLARE: Yes. That's it.

MR. CARBON: Any further questions?

MR. OKRENT: Well, again, although I think it will be helpful when the PRA is available, the design of the plant can't be changed very readily as a result of the PRA. I don't think it will be helpful to anyone if a plant is built and there happen to be one or two places in it that are less capable to withstand seismic events than what has been estimated here. These turn out to be rather important.

I might note a number like something smaller than 10^{-4} per year is a pretty small number in some contexts, but it is a pretty large number if you envisage that you don't have containment integrity and you hav a serious accident. So one has to, again, look more deeply into these numbers to see what their context is.

I really think it is better to know more now than to invite the kind of troubles that keep cropping up in this area later.

MR. CARBON: Any other comments or questions?

If not, thank you, Mr. Clare. Let's move right on.

MR. GROSS: Before we move to the next subject, I wonder if we could ask for Mr. Newton of TVA to come back. He has gotten some information which responds to earlier questions.

(Slide.)

MR. NEWTON: The question that was raised was around the question of how high was the head water level and the sensitivity of our conclusions about the maximum flood level at the site, and let me go through that. The answer lies in the operations that we assumed.

MR. CARBON: Excuse me, just a second. Was that in reference to Dr. Siess' question?

MR. NEWTON: No, it was in reference to your question and Jesse Ebersole's question that raised a question about the maximum flood level at the site. There were two questions. It was the operability of the gates, and Mr. Buchanan, in a minute, will tell you what the backup system is.

The answer really lies in how we assume the operation. And I know this is an extremely tall slide, but basically, these are hydraulic lift gates so normally they are in a down position. So that we have an OG overflow crest and these hydraulic lift gates are in the down position in a flood operation. And to operate the gates we open a valve, run the water in and lift these gates up. And we can lift them from a crest elevation of 1020, so that it is up in a full position with the top crest elevation at 1034. We raise the gates.

In our flood operations in both the PMF and in the one-half PMF -- remember, the one-half PMF goes with the seismic OBE failure -- we have lifted the gates deliberately to force the flood levels to the maximum heights possible, because what we are doing is we are utilizing the storage that we have. Norris Dam has a tremendous amount of storage; it is safe against a PMF so we are using it in these big floods, or we would use it. They haven't happened yet.

We would use it in these big floods to minimize the downstream impacts. We've got that storage. So what we did postulate was an opening of the gates, and we forced the heat water levels to the maximum that they could be forced to. We couldn't get it any higher than the 1035 that we use in our OBE operations.

So the answer to your question is there is no other operation. If the gates were not to operate, we would assume they would be down in the closed position and we would have passed more water and we wouldn't have gotten that high. If they were to be opened and not be operable, it doesn't make any difference because in effect, that is what we have already assumed. We have assumed that they are up. So that we do have the maximum head water level in the PMF and in the OBEs, and we think we are fully covered.

Now, I will let Mr. Buchanan answer the backup system. Suppose we had to operate the gates --

MR. CARBON: Hold up a minute. Chet, does that help you?

MR. SIESS: Well, it was Jesse's question.

MR. EBERSOLE: Yes, you answered my question.

MR. NEWTON: I just didn't remember those details. These are our normal operating procedures which we assumed, and that is what we assumed.

MR. EBERSOLE: Well, that's the most pessimistic assumption you could make.

MR. NEWTON: That's right.

MR. MOELLER: Since you said the water raises the gates, then these engines that Jesse was talking about, are they to lower the gates?

MR. NEWTON: There is a hydraulic lift gate, and we have to open up valves and let water in, and that water lifts the gates.

MR. MOELLER: So is the power to open the valves?

MR. NEWTON: It is the power to open them. Mr. Buchanan can tell you about that and the backup system.

MR. CARBON: Is there no further interest in this?

MR. EBERSOLE: I have none. If he is already in the worst configuration, I guess we can close it on that.

MR. CARBON: Fine, thank you.

MR. NEWTON: We've got a backup system. It is manual.

(Laughter.)

MR. CARBON: Thank you, Mr. Newton. Let's go ahead, then. Mr. King?

MR. KING: I'm Tom King of the staff. We actually have three speakers for Chapter 4. I will introduce them. The first one is Ralph Baars of Los Alamos National Laboratory and he will talk about the mechanical design and the fuel blanket and control assemblies. He was the primary reviewer and worked under the direction of Mike Tokar of the Core

Performance Branch.

Following Ralph, Walter Brooks from the Core Performance Branch will present our evaluation of the electronics design, and following Walter I will present the evaluation of the thermal hydraulic design.

We will start off with Ralph Baars.

(Slide.)

MR. BAARS: My name is Ralph Baars from Los Alamos National Laboratory. As indicated by the last bullet on the viewgraph, I was the reviewer of the fuel system. This first viewgraph where we are enumerating the scope of the review that was conducted, we looked at the mechanical design of the fuel blanket and control pins and assemblies, including the design criteria limits, the design methods, the steady state conditions and the transient conditions.

We reviewed the development testing plans, including in-reactor, ex-reactor, steady state and transient conditions. Can you hear me okay?

MR. SHEWMON: Sir, in your presentation someplace will you get to a comparison between this and the FFTF fuel or what the experience base of this fuel is in use or would be in use?

MR. BAARS: I touch on it very briefly. I didn't have anything specifically prepared as a direct comparison. They are very similar systems.

MR. SHEWMON: Well, it seems to me that if one takes an empirical approach, the proof is in the burning or the heating or whatever the simile should be, and therefore, I would be interested in hearing not only what you did in your ab initio mechanical parts, but indeed, how much you think we can rely on the experience that has been developed in other people's radiation experiments.

MR. BAARS: I think I will get into that later on. I don't know whether what I have got is specific enough for your satisfaction, but I will try to address it.

MR. SHEWMON: Fine.

MR. SCHWALLIE: Sam Schwallie from Westinghouse. In my presentation a little bit later I can get into just exactly what he's talking about in terms of geometric comparisons with FFTF as well as the data base.

MR. BAARS: We do think that this is a rather powerful favorable factor in favor of the CRBR system.

(Slide.)

For the means of guiding ourselves as to adequacy of the fuel system, we adopted these acceptance criteria. We looked at the conformance with the general design criteria as they were modified to four LMFBRs, this having been done in the first review on the CRBR. In particular in this review, we were concerned with what is the general design criterion 8 for reactor design, and this was the one that specified acceptable fuel design limits must be established and are to have an adequate margin.

Secondly, we spent some time reviewing 4.2 of the standard review plan for guidance in reviewing the fuel system design, and we tried to stay fairly close to the intent of that document. That, of course, is rather specifically oriented toward LWRs, but we attempted to comply with what we perceived as the intent.

Thirdly, we looked at the completeness and adequacy of the applicant's design criteria limits, the design methods and the conceptual design, and we reviewed the development testing to support these criteria limits and methods.

(Slide.)

This viewgraph identifies some of the favorable factors that we see for the CRBR fuel system. First of all, there is a massive test program that has been conducted and is ongoing now and will continue. We think that the results from these tests have shown that we can expect there to be rather few failures to the

CRBR core exposure, which is what we've considered here.

I don't mean to say that there have been no fuel failures in the test program. What I do mean here is that the failures that have occurred have been almost all related to factors that have nothing to do with CRBR. In most cases, they are related to reconstitution of test assemblies.

Secondly, the operation is far from coolant saturation lessening the chance of cooling discontinuities. Thirdly, the proposed scram trip settings terminate abnormal occurrences far short of significant fuel damage or disruption. Fourthly, we feel that the relatively low smear density design of CRBR fuel, 85 percent, 85 1/2 percent to be precise, is about twice -- provides about twice the relative volume to accommodate radial expansion, as is the case with LWR fuel design.

MR. WARD: What is the definition of smear density?

MR. BAARS: Smear density is simply what the definition of the fuel would be if it was smeared out completely throughout the volume available inside the cladding.

MR. SHEWMON: In cross section, usually, is it not?

MR. BAARS: Yes, that is correct. One minus that number, or 100 minus that number would give you the percent of the volume inside the cladding. That is not actually dense fuel.

MR. WARD: So you're saying that it is a similar number for LWR fuel, 93 percent?

MR. BAARS: Something like that. There are fall back positions of reduced power exposure and operating temperature available in the event of significant problems involved in operation.

And finally, one of the more important factors here, we are now beginning to accumulate data and will have a substantial amount of data available on a very similar system; namely, the FFTF fuel pin. It has the same fuel density and has slightly less pellet density, slightly smaller cladding gap and does not have axial blankets. Apart from that, it is very nearly identical.

(Slide.)

We have identified some issues, and the first one here is related to the criteria concerning the coolable geometry limits. Before launching into the details of this, I want to make sure to try to put this in the best perspective that I can.

The fuel designs with the current scram trip settings do not being to approach any challenge to coolable geometry. We are concerned here strictly with whether the coolable geometry limits themselves as proposed by the applicant would do the job if they were ever approached.

The first problem that we have here is cladding melting and appears to be the basis for assuring -- a lack of cladding melting appears to be the basis for insuring against coolable geometry. Now, I've called this a limit here; it is not, in fact, a limit. As the applicant treats it, he, up until now, has not been willing to regard that as a limit. That is a somewhat abstruse point at this time because if we have a limit, we have a problem with it.

First of all, we find no good basis provided in the PSAR as to why simply avoidance of cladding melting would insure coolable geometry. We did look at some data, a small amount that was relevant and that we had time to look at. We did not find evidence there that either a ballooning or a gross slumping was likely before you got right up to melting.

Nevertheless, we are dubious that any large portion of the core could withstand extreme temperatures of this sort without some impact on coolable geometry. We strongly recommend that the applicant adopt a firm cladding temperature as a coolable geometry limit, one that would be well below melting, and for which he has data that he can show that coolable geometry definitely would not be affected, if that were observed.

MR. WARD: Is there any -- you say that they have a cladding limit. Is there any monitoring of core temperatures which will be helpful in precluding the problem you're talking about?

MR. BAARS: There is outlet temperature monitoring. I am not extremely familiar with it, but I think it probably will be treated better in the later presentations. The presentation on thermal hydraulics. And possibly, the applicant will have some more information on that.

Secondly, to assist in assuring the cladding melting will not be reached, a no-boiling guideline is used. This is a violable limit that is that boiling can be exceeded in essentially screening criteria where above boiling you would analyze further to determine whether the cooling geometry would be compromised.

The operable thing here is the word "viable" and we are concerned because we've got no information as to how such cases would be evaluated or what sort of criteria they would use to judge whether coolable geometry would be compromised or not, and whether they would address all the relevant phenomena.

These first two points I think primarily relate to other cooling type transients. The third point is that we don't believe that either cladding or coolant temperature base limits are adequate of themselves to guard against molten fuel expulsion, when overpower conditions are present.

We feel that some limit more directly related to overpower should be named here. We feel this has some basis in the testing program in that the unterminated transient overpower tests that have been conducted, those in which molten fuel expulsion occurred, almost all of them occurred with coolant temperatures below boiling. And sometimes, at cladding conditions that were within some of the cladding temperature guidelines.

MR. CARBON: Excuse me, Mr. Baars. We seem to be falling further and further behind, and I wonder if you could speed up any.

MR. BAARS: Sure. At any rate, the applicants here have committed to address all of these issues and document a comprehensive basis for coolable geometry limits by the time the FSAR is submitted.

(Slide.)

MR. CARBON: If you could wind up in five minutes or something it would be very helpful.

MR. BAARS: I will do my best. This enumerates, identifies the issues on methods that we have. The applicant uses two models to evaluate fuel performance. The cumulative damage function model, commonly referred to as CDF, and the ductility limited strain or DLS model.

The first one, the CDF model, is the more sophisticated of the two. It uses realistic properties and addresses things that are generally in a more mechanistic fashion. The ductility limited strain model is very much an empirical model. The problem we have with the cumulative damage function is we feel the model should be qualified to integral rod test data so as to be sure to pick up any mechanisms that might be operating that are modeled in the procedures for the model.

Secondly, the model does not address the fuel adjacency effect. I don't want to get into that. It occurs when the cladding next to the fuel appears to be more degraded than at the molten fuel. We feel at the present time the model does not address that; at least, the information available to us. And we feel that this should be addressed. It is an important fact.

Finally, the statistical base does not cover the data variance. This stems primarily because the applicant wishes not to include the part of data variance that is due to error in measurement, inability to run identical tests, et cetera. We don't disagree with this, but we feel the applicant should provide a firm supportable basis for that amount of the data variance, and to date he has not done so.

The ductility limited strain model is very similar to the FFTF design procedure. There have been some changes in the model, and we feel that it should be requalified to the integral rod test data. The margin to failure with this model we don't believe has really been established. Some sort of means to quantify what margin there is between what this model predicts and what actually occurs we think should be enumerated.

The applicant has committed to address the CDF issues by submittal of the FSAR. We have not specifically asked him to sign on the dotted line as far as the DLS model is concerned. We do say in the SER that if he wishes to use the model for the FSAR, that we think these issues should be addressed.

(Slide.)

This addresses some of the issues we have identified for data base issues. This covers the atypical factors, the coverage for operating range and then some data in the cladding area. Again, I want to put this in perspective. Many of these factors the applicant agrees with. He has ongoing programs addressing them. In some cases he has the data in hand, although we have not seen it.

What we have attempted to do here primarily is to provide a snapshot as to our view of what the status of data is as of the information that was available to us when we made the review. In particular, the item here under cooling at the end of life and the response to high fluence and high temperature, I believe he has that in hand but has not relayed it to us. The atypical factors are something that have hung around for a long time and we feel they should be addressed so that the perceived relevance of the data is not marred.

Under the coverage area there, virtually all of the data base is 25 percent plutonium, and the CRBR design value of plutonium content is 32 percent. So there is some data available in this area that indicate there aren't any great cliffs out there, but we feel that a firmer data base is desirable.

Blanket rods -- we think there is not much data, or there are not many tests that have been run. There is no data that is available to us. There is an extensive program plan to address this area, particularly with the radiations in the FFTF.

Slow over power -- again, in the range from the power to melt test up to the 50 cent per second crack test, there is virtually no data at all as to the mechanisms involved in a cladding breach. We feel quite strongly about this, and the applicant does have an ambitious program running in EBR-2 in a transient mode to address this area.

Finally, on cladding, the fuel adjacency effect, how much data is actually needed here I guess at this point is problematical. There is a lot of information available. I think it needs to be integrated and put together, and perhaps at that time, additional data might be needed.

(Slide.)

Our conclusion on the fuel system is that we believe that prospects for success of the CRBR fuel system justify issuance of a construction permit. We do have a caveat here that, however, we feel that the ability to clearly demonstrate acceptability of the system for an operating license without resorting to fallback positions depends upon addressing the identified issues.

(Slide.)

Our basis for these conclusions includes the previously-enumerated favorable factors. I think that was about the third viewgraph I showed. Secondly, all of the foregoing issues are primarily relevant to the ability to evaluate fuel performance, and not to fuel performance itself.

Thirdly, the programs are underway or have been committed by the applicant to resolve the issues by FSAR submittal, and the final point is we believe that the availability of fallback positions allows deferral of resolution to the FSAR. And that concludes my prepared presentation.

MR. SHEWMON: Before you got into controlled ramps, when you talked about failure you were talking then about rupture of the cladding during normal operation. Is that what you were judging against when you talked about failure margins or probabilities or whatever?

MR. BAARS: That was with the CDF model under the fuel evaluation models.

MR. SHEWMON: When you talked about failure I didn't know what you were talking about and I'm asking what you meant.

MR. BAARS: I'm talking about failure of the cladding.

MR. SHEWMON: So it is rupture of the cladding.

MR. BAARS: That's correct.

MR. SHEWMON: What is the predicted or expected life that you judged against? There was something in here.

MR. BAARS: What was the goal that I judged it against? 80,000 megawatt days per ton.

MR. SHEWMON: Okay. And the control material which you mentioned -- I didn't hear it -- is boron carbide? Pellets and stainless steel?

MR. BENDER: That's correct. It is in pellets. They're in stainless steel, relatively a half an inch in diameter or larger.

MR. SHEWMON: And the blanket is UO2 unenriched?

MR. BAARS: That is correct. Those are also relatively large pins. There are 61 pin bundles. I can show some of these.

MR. SHEWMON: I will take your word for it.

MR. BAARS: Yes, they are 61-pin bundles. They are in exactly the same duct as the fuel assemblies are installed in. The fuel assemblies are 217-pin. The absorber assemblies, the primary controls, are, I believe, 39-pin assemblies, and the secondary control systems are 31-pin assemblies.

MR. SHEWMON: Fine, thank you.

MR. CARBON: Other questions?

(No response.)

Thank you, Mr. Baars. Let's move on, then. Mr. Brooks?

MR. BROOKS: My name is Walter Brooks and I am with the Core Performance Branch at NRC. And I will discuss --

(Slide.)

--the section 4.3, which is nuclear design of the PSAR, or the SER. And I will discuss the design bases including development of the principal design criteria, the design description which I will describe only very briefly because it will be described in considerable detail later by the applicant. I will talk a little bit about the reactivity coefficients and, as you see, power distributions, reactivity coefficients, et cetera.

I should say that the nuclear design portion of the safety evaluation report was based on a technical evaluation that was prepared by Los Alamos Laboratories, and one of the preparers of that report, Mr. Kinman, is with us today in case you have detailed technical questions, the answers to which I might not know. He is prepared to answer questions.

The Core Performance Branch then took that technical evaluation report and prepared the safety evaluation report from it, putting it in the terms of the various requirements and meeting those requirements.

(Slide.)

The next slide shows the principal design criteria affecting the core, and some of these, of course, you recognize. The PDC-8 is the one that was just talked about with the fuel and specified acceptable design limits. And the rest of these -- that is the list. I will talk about them independently as I go down through the subjects. We don't need to spend anymore time on this, since we are in a hurry.

(Slide.)

What I will show next is a core layout, and this, again, will be presented and discussed in considerable detail by the applicant. I would just like to point out the control systems. There are two control systems, two scram system. Both a primary control system and a secondary control system. The primary control system consists of these three assemblies in what is called row 4 on the corners, and the six assemblies on the corners of row 7.

So there are a total of 9 control assemblies that make up the primary control system. There are 6 which are on the flat of row 7. These make up the secondary control system. And there is another interesting part in here; there are 3 more -- I'm sorry, there are 5 more. There are 6 assemblies here that start life as blanket assemblies and stay in the reactor for a year as blanket assemblies and then are transferred like Cinderella's pumpkin into a fuel assembly to extend their life for another year.

MR. BENDER: Have you prescribed any mode of control here? Is there a modal control specified?

MR. BROOKS: A mode of control?

MR. BENDER: Yes. That rods have to move collectively or separately or some such thing as that.

MR. BROOKS: That has been prescribed, and I think the applicant will probably discuss that. But let me say what is. The secondary scram rods are removed from the core. The three primary scram rods in the center of the core. The row 4 rods are also parked above the core during operation. Operational control of the plant is then obtained from the six other primary control rods which are partially in the core during operation.

These are operated, as I understand the system, they are each operated separately but they are constrained to be within a very short distance of each other. That is to say, they do not get lifted as a bank, but they must be banked.

MR. BENDER: guess I was leading into a question. Does the staff try to put any limits on variations in the rod behavior within that bank, or are you relying on the applicant to tell you what he's doing?

MR. BROOKS: We would have no restrictions. Well, let me say it a different way. We would permit anything as long as he didn't violate his fuel limit, His linear heat generation limits.

MR. BENDER: So you would correlate this with the previous information regarding the thermal performance of the fuel? Is that what we are hearing?

MR. BROOKS: What we will come down to in a bit --

MR. BENDER: Maybe I ought t wait, I'm sorry.

MR. BROOKS: Well, I just wanted to point out the control systems, and there will be considerably more detailed information later.

(Slide.)

Now, let me talk a little bit about the relevant criteria to the reactivity control system. These are, as you see, 23, 24, 25, 57 and 58. Now, 23 is the criterion which states that SAFCO shall not be exceeded for the reactivity control system. This is the so-called single failure; that is, any single malfunction.

24, which requires two independent systems, that one has to protect against SAFCOs in normal operation, and anticipated operational occurrences. The other of which must protect, must assure the ability to cool the core in the event of such an occurrence.

These systems, both systems, have to have sufficient reactivity worth in order to hold the core subcritical at the hot shutdown state, with a stuck rod. Each system must independently be able to do this in the absence of the other system, and in the presence of a stuck rod in the system. And then, one of the systems has to be capable of taking it all away to refueling temperatures.

PDC-25 requires both systems to maintain the capability to cool the core under accident conditions. And also, you assume a stuck rod for that case, also. And PDC-57 requires that the reactivity control system be designed so that reactivity insertior rates and amounts are limited -- the amounts are limited to preclude significant damage to the core boundary or loss of ability to cool the core.

Criterion 58 requires that the systems be designed to be highly reliable. Mostly, that is -- that criterion will be discussed further when we get around to talk about the control systems themselves, and I won't say anymore about it.

The design bases that the Applicant derived from these criteria will be discussed later by the Applicant, and I will merely say that it is the Staff judgment that the design bases that were chosen by the Applicant meet these criteria.

Now, the rod worths that were calculated by two-dimensional diffusion theory code, which actually all of the effects were treated by use of the buckling turn, and this is the sort of procedure that is standard and is acceptable if properly verified. The Applicants have verified these calculations against a number of critical experiments in the ZPTR series.

We have some concern that the experiments for which we so far have data have not been terribly close, and a number of them are homogenous core experiments, and there has been one pre-engineering mockup. So we have some concern about the fine-tuning the methods, but the Applicant has committed to perform experiments and I think probably has already performed them, but to document the results for the FSAR review.

As far as the results of the calculations are concerned, the results in the PSAR showed that in fact the systems do meet the shutdown requirements with an additional margin of more than a dollar at the worst case.

MR. OKRENT: That's not very much reactivity.

MR. BROOKS: No. It is not one percent, but it is like half a percent.

MR. OKRENT: Is there any U-275 in this?

MR. BROOKS: There is very little. The beta is 0034.

MR. RAY: Is this in 1967 or in 1983 dollars?

(Laughter.)

MR. OKRENT: It is a U-238 dollar.

(Laughter.)

MR. BROOKS: With regard to PDC-57 which requires that the reactivity insertion limits be limited, the Applicant has looked at the case where the rods go out the fastest they can; that is to say, they go out at some, as I recall at 35 or 37 inches per second or something of that nature when the roller nuts spring off by centrifugal force and drop the rods, so that's as fast as you can move them. And have shown that they still have plenty of margin to core coolability.

MR. CARBON: If we could try and wind up in about five minutes.

MR. BROOKS: Yes. The power distributions are used here --

Well, let me then take the next slide.

(Slide.)

Here the power distribution and instrumentation, the relevant criteria with respect to these things are 8, 11 and 18. Criterion 8 requires that the reactor and associated coolant and protection system be designed to assure that SAFCOs are not exceeded during normal operation or anticipated operational occurrences.

Now, the design basis that has been used to meet this requirement is that incipient fuel melting shall not occur under these conditions -- that is to say, normal operation -- and anticipated operational occurrences. And specifically what they have done is to take 15 percent over power conditions and include three signal uncertainties.

Now, as you have heard earlier, incipient, because this coolant is so far from saturation, incipient fuel melting is the thing you get first. So this is an appropriate criterion to use here for SAFCOs.

The Applicant has set design limits, linear heat generation design limits of 16 kilowatts per foot on the fuel and 20 kilowatts per foot in the blanket, and the arrangement of the fuel and the blanket assemblies has been chosen to meet these conditions. So that as the plant progresses through life with different control rod positions and so forth, you always need the 16b kilowatts per foot; and they have shown, given part of the distribution values for this six cycles, and shown that they meet these limits.

Now, the prior distributions again are performed by the 2D-2D synthesis method, which is again a technique which is used for light-water reactors and is acceptable if it is properly verified. And here again, they have verification that has been performed of the synthesis, but here again not for the engineering mockup critical. And again, the Applicant has committed to perform these tests and document them for the FSAR review.

Our Criterion 11 requires that instrumentation be provided to monitor the core variables over the range of operation for normal operation occurrences, normal operation anticipated occurrences and postulated accidents in order to assure adequate safety.

MR. CARBON: Mr. Brooks, could you simply go to your results and conclusions? Just take up your results and conclusions on these.

MR. BROOKS: All right.

Then let me the make an overall conclusion. I won't go through the rest of these individual subjects. Let me make an overall conclusion.

Our overall conclusion is that we believe that the nuclear design of the core meets the requirements of the criteria that are listed here. What we will require and the Applicants have committed to do is more verification of their methods and some more documentation of their methods so that these can be reviewed in the FSAR. That is the thrust.

MR. CARBON: But you anticipate no particular problems?

MR. BROOKS: We anticipate no particular problems. We think that there may be small differences as a result of the new critical experiments that will be performed, but we don't anticipate any problems.

MR. CARBON: Fine.

Are there questions?

(No response.)

MR. CARBON: Thank you.

Let's move on to Mr. King.

And, Mr. King, could you try and wind up by 1:15?

MR. KING: I will try and wind up before that.

(Slide.)

My name is Tom King, and I am with the Program Office in NRR, and I will just run through quickly the scope of our review.

What it involved was a thermal hydraulic design of all the in-vessel components. We looked at the criteria, the design methods for both steady state and transient conditions. We looked at the development testing done and startup testing planned, and we had help. We had Brookhaven and Argonne National Laboratories do some independent calculations for us to overcheck some of the Applicant's analysis, and we had Wolfgang Barthold of Barthold and Associates do an independent review of the thermal hydraulic design.

(Slide.)

Briefly, our acceptance criteria were conformance with two principal design criteria: the one on reactor design and the one on flow blockage; conformance with the applicable sections of the standard review plan on thermal hyraulic design.

We looked generally at the completeness and adequacy of the Applicant's design criteria methods and design, and the same thing for his development testing. And as I mentioned, we did some confirmatory analysis to overcheck the results, the Applicant's results, in what we felt were some of the critical areas of thermal hydraulic design. And I will talk about those in a little bit more detail.

(Slide.)

The major safety features that we considered the design has is it provides for decay heat removal via natural circulation. It prevents gas entrainment in the reactor vessel by venting potential collection areas and providing a suppressor plate at the top of the upper plenum. It minimizes the potential for flow blockage via several features incorporated in the design: multiple flow paths at the inlet strainer, wide openings at the bottom of the assembly to allow for any axial motion of the assembly. It provides monitoring instrumentation for core assembly outlet temperatures.

Someone had a question on that earlier. That is not safety grade instrumentation. It is not part of the plant protection system, but it does provide pretty thorough coverage of the core outlet temperatures.

(Slide.)

MR. WARD: Does that monitoring permit, for example, recognition that a fuel element might be swelling; there might be clad ballooning before there is failure of the clad?

MR. KING: Only if there was swelling due to overtemperature. If the swelling was not resulting in some out of normal temperature, it would not tell you that.

MR. WARD: But I mean will the swelling

introduce enough change, the swelling itself be a cause of change in coolant temperatures?

MR. KING: Theoretically, if you get enough swelling and it started to cut off flow through the assembly, you would notice the outlet temperature drop.

MR. WARD: That is what I mean. Does that system -- can it be expected to detect serious fuel swelling before there is failure of the cladding in an individual fuel element?

MR. KING: If you had serious fuel swelling that was causing flow starvation, it would tell you that that was happening. Whether or not you would get cladding failure before you got to the point where the thermocouples were detected, I don't know.

MR. CARBON: Dave, there are not thermocouples on all channels.

MR. KING: There are on most channels. For the fuel and inner blanket there are only about 12 positions out of about 170 that do not have thermocouples.

This is the second half of that slide on what we consider important features. The design requirements require prevention of control rod flotation during refueling due to inadvertent start of the primary pumps. The design requirements for the thermal hydraulic design requirements are based upon being able to meet all of the structural temperature requirements of the in-vessel components: the fuel blankets, the permanent core support structure, upper internal structure, pipe components, and the new orificing design of the fuel and blanket control assemblies provides shielding to the core support structure.

(Slide.)

Resulting from our review were four areas that we had some concern in that we consider to be acceptable for resolution during final design, and these are the margin of flotation on the primary control rods. We're not satisfied that there is enough margin. Since it is a replaceable component, we consider resolution of that issue as part of final design acceptable.

In the FFTF operation, cycle 1, they observed a delta P increase of approximately 4. We don't know what the cause of that was or what the implications are on CRBR design, but that will be something that will have to be resolved as part of the final design and factored into the design.

MR. OKRENT: How much?

MR. KING: Sixteen psi.

MR. OKRENT: Out of what?

MR. KING: Out of roughly 120. Again, we

consider that adequate for resolution during final design because there are fallbacks to reduce power, reduce flow, reduce burnup.

The latest power-to-melt data from FFTF testing needs to be factored into the final design, and we had some questions on the application of hot channel factors, primarily in -- well, one in the natural circulation area for which high channel factors should be used, and then in the normal steady state operation.

The uncertainty is on the hot channel factors and the fact that they're being applied in a linear fashion. Again, we feel the Applicant has committed to address our concerns in that area as part of final design, and we feel that is adequate.

(Slide.)

I mentioned our overcheck calculations done by Brookhaven and Argonne. Just briefly what was done is we have done overchecks of steady state full power core conditions, the natural circulation condition, the DHRS flow, which is the direct heat removal system. Our concern there was what the in-vessel thermal hydraulics were like. And we are continuing even beyond the CP stage to do some more work in that area, independently looking at primarily sensitivity of these various calculations to changes in what we consider key parameters, trying to find out really what are the key parameters.

The results of our independent calculations, we didn't look for exact agreement with the Applicant, but we wanted to see if we independently calculated the design criteria he met and see if we came up with the same trend for the transient that the Applicant did.

(Slide.)

And this is our overcheck using the supersystems code by Brookhaven on the natural circulation case station blackout, and we got very good agreement between SSC and what the Applicant was proposing.

MR. CARBON: Have both those codes been benchmarked against the FFTF results such that you would expect them to agree?

MR. KING: They have been checked against FFTF. They have not been changed to exactly fit FFTF, but they were checked to verify that they predict actual in-reactor data.

MR. CARBON: Were they changed to some extent?

MR. KING: I will ask. Jim Guppie is here from Brookhaven. I will ask him to comment on that since he's the one that did the calculations.

MR. GUPPIE: I'm Jim Guppie from Brookhaven

National Labs talking for the SSC calculations.

We have not fully completed our FFTV validation studies, but we have done analyses comparisons out to say five or six minutes into the various transients at 175 percent, 35 and 5 percent power. What is done is the modeling is not changed. However, the input, the geometric description for SSC is then appropriately changed to simulate the FFTF plant.

And when we did that and ran comparisons, having then known after the fact what the power levels were and the power history so that you can better determine what the decay heating is and so on and so forth, our comparisons were fairly reasonable with the plant data.

MR. CARBON: All right.

(Slide.)

MR. KING: We had similar calculations done by the COMMIX code at Argonne, and this is just one example to give you a feel for the kind of comparison we're getting between COMMIX and the Applicant, which is called ARD-308 here.

(Slide.)

I will skip some of these in the interest of time.

On the DHRS operation we had concern about the in-vessel thermal hydraulics; that since the inlet and outlet lines of the DHRS were basically the same elevation and were not too far apart, were we going to get any short-circuiting.

(Slide.)

And not remove heat but have incoming flow go right out the outlet line.

We had run a calculation using the same assumptions the Applicant had used on short-circuiting and came up with this comparison of prediction of what's going to happen to in-vessel temperatures under a 20 percent short-circuiting, and then it showed the SSC predicts fairly well what the Applicant predicts.

(Slide.)

We also did a little sensitivity calculation to show if the short-circuiting were higher or lower, how that would affect temperatures. The middle curve is the nominal case, the one the Applicant used, and that corresponds to 20 percent short-circuiting. And as you get more and more short-circuiting, your temperatures go up.

COMMIX is a very detailed in-vessel thermal hydraulics code, and we did a calculation to try to get a better handle on what kind of short-circuiting did we really have, and we just got some results this week, and it really indicates the short-circuiting is very close to zero, and it's going to be very close to what is represented by the bottom line.

(Slide.)

Our conclusion basically is that the design has a high probability of meeting the design criteria. There are fallbacks of reduced power, flow or burnup if complications arise during final design. We consider the design acceptable for ACP, and the basis for these conclusions --

(Slide.)

-- Are what we consider the incorporation of the significant safety features, the fact that the Applicant has significant test program, development test program to support the thermal hydraulic design, both full-scale tests, sodium and water, on assemblies and scale models of in-vessel geometry.

The FFTF fuel design and the in-vessel thermal hydraulic design is similar to CRBR, and we would consider continued FFTF operations certainly would give us useful information and would apply to CRBR. And we consider the independent calculations that we have done so far confirm that the Applicant's design will meet the design criteria. And the Applicant has committed to testing during initial startup to confirm natural circulation and to confirm DHRS performance, and to measure in-vessel temperatures and vibrations as part of the startup program.

And the preliminary safety analysis we feel has been done with all or a lot of conservatisms that result in conservative predictions.

MR. BENDER: Have you tried to compare what has been done for CRBR with what was done for PHOENIX?

MR. KING: In terms of the development testing?

MR. BENDER: Was the same kind of confirmation program used of the design?

MR. KING: I have not tried to make that comparison.

MR. BENDER: Just as a frame of reference, it seems to me like there would be some advantage in seeing what a successful reactor system has used, if that one is said to be successful, because we don't have a lot of experience.

MR. KING: It is very similar to FFTF. I have made that comparison. It is very similar in terms of the analysis method, the development testing, and the design; and in fact, there is probably more being done in Clinch River than there was in FFTF.

MR. BENDER: Well, that is a good frame of reference.

MR. EBERSOLE: Is the FFTF cooled by liquid water in the final sense?

MR. KING: FFTF is sodium cooled, and the heat is dumped directly from an intermediate sodium system to the air through an air blast heat exchanger.

MR. EBERSOLE: I was really getting around to this. What is the practical consequence of rupturing a steam tube in this plant? Well, I guess one way to express it, how long a shutdown would that produce if you burst a steam tube in the steam generator?

MR. KING: I can't answer that.

MR. EBERSOLE: Would it be a year for heaven's sakes?

MR. KING: I would have to ask the Applicant.

MR. DICKSON: We're going to go into that later in the presentation.

MR. KING: That concludes what I had to say.

MR. CARBON: Are there other questions?

(No response.)

MR. CARBON: Fine. Thank you, Mr. King.

Before turning the session back to the Chairman, let me introduce Dr. Grace, who is with us today, Dr. Nelson Grace, who is replacing Paul Check as the director of the CRBR Project Office.

I might just comment that he used to be with

FFTF and then strayed into the fusion field and is now back home.

Mr. Chairman, it's back to you.

MR. RAY: I would adjourn now for lunch and with the understanding we will be back at 2:15.

(Whereupon, at 1:5 p.m., the meeting was recessed for lunch, to be reconvened at 2:15 p.m., the same day.)

AFTERNOON SESSION

(2:15 p.m.)

MR. RAY: The meeting will resume, and Dr. Carbon will chair it.

MR. CARBON: And we will move on with Mr. Baloh.

MR. VIJUK: I'm Bob Vijuk rather than Frank Baloh. The reactor portion of the agenda will be covered as shown on this vu-graph. Frank Baloh will talk about the reactor vessel and internals -- that is, everything other than the reactor core -- and then we will discuss the reactor core in three parts: the nuclear design, thermal hydraulic design, and finally the mechanical and structural design and the fuel and blanket assemblies.

Our plan is to spend about a half hour in each area unless you would like us to alter it. We intend to tell you -- to describe the design to you and tell you the basis for the design. We intend to describe the design and tell you the basis for the design, but we want to focus on the development testing that we have done to either demonstrate features of the design or the performance of the total design.

You will see that the state we are in is very different as we move through here. Most of this part of the reactor is either totally fabricated or well along in the fabrication. Then in the core areas you will see that all of the development programs are essentially done, and we will present you some data that is not in the PSAR on criticals, but work has been completed. So the Staff did not have the benefit of that in their review.

In the thermal hydraulic area we are almost done with testing, and we will show you some -- what that program looked like. In the fuel and blanket area there is much testing that is still in progress, largely in FFTF, and you will be told what that is going to be. So our focus is to show you how we demonstrate or will demonstrate that the design will do its job.

With that, unless you want us to revise our plan, we will move right into reactor internals.

(Slide.)

MR. BALOH: Okay. As Bob has said, what I am going to be doing is giving you a brief description of the reactor enclosure.

MR. CARBON: Mr. Baloh, could you sort of stand aside if possible?

MR. BALOH: I'm going to be describing the reactor enclosure which includes the enclosure head, the vessel, the lower internals, which includes all of the permanent structures other than the core removal components which will be discussed later, and then finally the upper internal structure itself.

(Slide.)

This is an isometric of the reactor vessel closure head. It is approximately 21 feet in diameter by about 13 feet high, and in its normal operating condition weighs about a million pounds. When it's configured for refueling it will be up to about a million and a half.

Characteristics that distinguish it are the three rotating plugs. That configuration is to support the refueling operation. The in-vessel handling machine would be mounted on the nozzle you see here on the small rotating plug, and by rotating the intermediate plug you can envision being able to locate over any radial location of the core. The core is directly below these control rod drive mechanism nozzles that you see here; so with the rotation of the IRP one would be able to locate this radially over any location, and then by rotation of the large plug hit any of these circumventional locations at that radius.

These operations would be done in parallel. It wouldn't be necessary to sequence one to the other. The nozzles you see here are support nozzles for the upper internal structures which I will describe briefly later. The head structure itself is mainly carbon steel. These 22-inch plugs that you see on the top, there are three plugs that are nested. They are effectively the same composition all the way down, but the are separated obviously so that they can rotate.

Each of the plugs are suspended from what we call riser assemblies. These are cylindrical shells that attach to the plugs and then have a phlange on the top to which a bearing assembly which has an integral bull gear attached to it driven by the plug drives for the refueling operation.

The temperatures of the plugs themselves are kept uniformly at 400 degrees throughout both operation and refueling to assure consistent alignment at all times. The main function of the risers is to reduce that temperature down to a maximum of 125 up in this area where there are some elastimer seals and the bearing.

The primary separator for the cover gas is a dip seal. That exists between each of the plugs and also between the outer plug and the phlange of the vessel.

MR. CARBON: Is the entire cooling by gas?

MR. BALOH: Cooling of what?

MR. CARBON: The what.

MR. BALOH: No. There are head heaters. As you can see here, during normal operation approximately half the heat is supplied from the reactor itself, the other half from these electrically controlled -- they are on a control system. There is also a forced circulation so that the entire head access area is available for manned access at all times. We predict less than 5 MR per hour during operation in this area. The design requirement is 25 MR per hour in that area, and it has got a nominal temperature of about 80 degrees in that location.

MR. EBERSOLE: We were talking earlier about the loss of power. Are there any cases where loss of power would affect these artificial heating systems that keep the sodium liquid below the dip seal sodium -- any sodium anywhere, including impulse lines or whatever?

MR. BALOH: I don't think there is any concern. If the reactor was critical, obviously loss of power would shut you down. You would then have natural circulation set up.

MR. EBERSOLE: No. I don't mean that. I mean are there other places in the circuit where one must fit in artificial heat at all times to keep it liquid?

MR. BALOH: No. In fact, the heating of the head itself is to maintain consistent alignment. It is not directly a safety consideration. And you could lose all of these heaters, and it would not prevent shutdown or anything of that nature.

MR. CARBON: To understand this, does the reactor vessel support ring more or less rest on the platform, the floor?

MR. BALOH: The outer -- you'll see a little bit later when I show an isometric of the vessel there is a ledge here that the outer ring is mounted to, and then it goes up through the outer assembly, and that is --

MR. CARBON: Well, in effect, the head assembly sits in.

MR. BALOH: That's right. There's an annulus here between the riser assembly and the vessel phlange. There are three shield plates that are between nine and ten inches depending upon which one in thickness down to this location. We're talking all carbon steel material. On the lower shield plate we have suspended -- and this picture doesn't show it -- these are subassemblies of reflector plates that provide reflected insulation. There is somewhat over 300 degree temperature.

MR. ETHERINGTON: I'm missing something

somewhere. You mentioned heating. Isn't this cooling during operation?

MR. BALOH: No. We're heating to maintain uniformity of temperature.

MR. ETHERINGTON: But why did it need to be heated? Is it losing temperature below? There would be a gradient across it if we did not heat it, and there would be gradients set up. What we're doing is maintaining uniformity. We're actually maintaining within about 15 degrees plus or minus with a design requirement around 50.

MR. REMICK: Frank, I believe EBR-2 had problems with the rotating plugs freezing and binding, or I shouldn't say binding. But are there any special provisions to prevent that here?

MR. BALOH: Well, first off, we have had considerable contact with the EBR-2 people. They've been involved in our design as consultants from its inception, and we believe we have addressed all of their problems.

First off, they used a bismuth-based tective which they have frozen and into the blade section of the dip seal they heated. The problem that they run into is they had a very legal seal system, and oxygen was able to get down into the dip seal area and cause what they called clinkers that actually froze things up, and they did not have the access for cleaning which we have also incorporated.

So, yes, we were aware of their problems. We have addressed them. And we use sodium which is always liquid. And the seal system that we have has argon over it, and it is a multiple barrier seal system. In fact, the seal systems up in this portion, the primary function is to maintain the purity. The radioactive cover gas interface is right here, so the remainder of the seal system is to keep that environment pure so that we don't have those problems.

MR. REMICK: Thank you.

MR. BALOH: The last portion I will describe here --

MR. SHEWMON: Before you leave that subject, you said you do have ports for cleaning.

MR. BALOH: Yes.

MR. SHEWMON: How do you expect to clean it?

MR. BALOH: Well, first of all we don't have a freeze-thaw cycle, so that the approach that is taken is that we would put in a scoop-type device, slowly rotate the plug, and we can actually through a glove box mechanism take the crud that would accumulate on the surface of the sodium dip seals.

MR. SHEWMON: And you think you could sweep it up to a given point?

MR. BALOH: Yes. There has been testing done of that operation. The lower portion of the head has the suppressor plates. These are also suspended from the bottom of the lowest shield plate, and their function is to assure that we do not get gas entrapment into the melted sodium.

And one of the areas where the core-to-scale tests that were done on the outlet plenum indicated that initially this design had the suppressor plates approximately two feet below the sodium. They are now three feet. Testing has confirmed that that is where they should be to do the job.

The closure head itself is assembled and has gone through considerable functional testing of the plug-drive systems, the elastimer seals, leakage rates, and effectively it has been confirmed to do all the job that it has been set out to do. Postioning accuracy came well within all of our requirements and guidelines.

(Slide.)

The reactor vessel is primarily a 304 stainless steel structure. It's 59 feet high by approximately 20 feet in diameter. The top phlange you can see here. The phlange where the closure head sits you can see schematically the area where the trough or the dip seal would be. This phlange is carbon steel. It thermally matches the expansion characteristics of the closure head, and it is also maintained at the same 400 degree temperature.

There is a transition joint in the vessel of inconel-600 that takes us from the carbon steel down into the 304 stainless structure. This transition joint is low temperature. The top is around 400; the bottom around 600. And it is a couple of feet above the sodium level, so it doesn't see sodium.

Inside the vessel is a 316 liner. Its function is to shield the 304 primary structure of the vessel from the hot outlet plenum. It effectively keeps the entire vessel then outside of the creek regime, and that was its function. It comes in at around a maximum of 870 degrees. It is mounted on a forging at this elevation.

Now, there are three outlet nozzles, three three-foot diameter outlet nozzles, and this is one of the areas where special consideration had to be given. Because of the feature that we added, there is some differential expansion between the vessel liner now and the vessel itself.

The design accommodates that and can prevent leakage of the bypass coolant which goes behind the liner and prevents it from mixing with the hot outlet plenum coolant.

We have done extensive testing in that area, both to demonstrate that the seal can accommodate the differential expansion and the interfaces for wear couples and so on.

At the lower portion of the vessel there is another forging. It has a support cone which on this picture you can see how it supports the core support plate itself; and I will address that a little bit further later.

The inlet nozzles, there are three of those two feet in diameter again. The deflection of the incoming sodium downward is a result of inlet plenum testing, core-to-scale testing again, and to assure that the flow does not impinge directly on some of the lower internal components, as you will see in one of the next vu-graphs.

MR. WARD: At the end of life will the neutron irradiation down the core area significantly affect the stainless steel properties?

MR. BALOH: No. This is going to be well down below any threshold for loss of ductility in this area.

MR. CARBON: What is your philosophy on why you have the inlets down below the level of the core rather than up above?

MR. BALOH: Philosophy? I would think from a natural circulation point of view that this would be the ideal location.

Paul, do you want to add anything to that?

MR. DICKSON: No. What Max is referring to is the idea of bringing in the inlet nozzle at a higher elevation rather than the downcomer down inside the vessel. It obviously makes for a much larger vessel. And you realize it is inside a guard vessel.

MR. BALOH: I didn't mention that, but there is a guard vessel that goes above the minimum safe level with a three to six-inch annulus all the way around this vessel.

MR. CARBON: But the inlet pipes nevertheless go through the guard vessel and out, I guess.

MR. DICKSON: They actually are an integral part of the guard vessel.

MR. CARBON: All right. To be sure, they turn up just as soon as they go outside?

MR. DICKSON: There is no piping in the primary system. That is outside the guard vessel. That is not an elevated pipe. That is the rule. And it applies to this as well.

(Slide.)

MR. BALOH: This is a picture of the completed reactor vessel prior to its shipment to storage, which it is in now.

MR. BENDER: Could I ask about that transition weld from carbon steel to inconel-600 to stainless? Is that the combination?

MR. BALOH: That's right.

MR. BENDER: Are there any metallurgical pitfalls in that thing that you have to watch for?

MR. BALOH: Yes. And we did considerable addressing of that. In fact, in Appendix G of the PSAR there is a discussion of that very question. One of the things in our research that we found was that the primary cause whereby metallic welds do fail occurred above 800 degrees. We were virtually unable to find any failures in fossil fueled vessels or elsewhere at lower temperatures, and especially when a high nickel filler is used for making the weld to cut down the carbon migration and factors such as that.

This entire concern has been addressed and has been documented in that section of the PSAR.

MR. BENDER: Is any surveillance required?

MR. BALOH: Right now the surveillance is a VTM-2 type inspection for Section 11 of the code. The initial radiography and such was wide spectrum x-ray double angle, and the normal PT and so forth to go through it.

MR. BENDER: I wasn't sure what operating history might do to you.

MR. BALOH: Again, the temperatures I think are the key. The actual ferritic weld at the top is just a little over 400, about 450.

MR. SHEWMON: How many cycles of temperature do you expect to go through up there?

MR. BALOH: Well, again, that's very near the area where we control temperature with the head heaters and so on. Internal cycling is extremely small. There are very small temperature changes up there and very long in nature, nothing very rapid.

MR. CARBON: We speak once in a while and ask questions about the core support structure. It's highly unlikely to break, of course, but with the vessel wall itself and all the weight that it is supporting and the sodium and so on is that an equal improbability to the core support?

MR. DICKSON: We consider both of those well beyond any design basis.

MR. CARBON: I know you do. But again, we frequently ask to discuss questions about core support.

MR. DICKSON: We're going to give you a complete rundown of that at some later meeting.

MR. CARBON: And my question is, improbable as it is, is the possibility of the pressure or the reactor vessel itself in the core equally --

MR. BALOH: In this particular case I think the key is that we would never expect -- and I would just use the actual picture --

(Slide.)

-- A rapid failure. This whole area is going to be monitored for any leaks. And so that if there is any cracking or anything, which we certainly don't expect, that would be monitored, and then the VTM-2, the visual inspection that would take place in this annulus, would also be another means of identifying any potential initiation of flaws.

(Slide.)

This is a picture of the lower internals. It is slightly enlarged, but basically it is the area you see here. And starting at the bottom end we have the core support structure itself, the plate, which is two-feet thick. We have a number of channels for feeding sodium to the bypass, which goes into this annulus to feed the liner and some of the other peripheral assemblies which I will discuss.

The core support itself is welded to this core support cone which I showed you on the vessel vu-graph earlier. Also attached to the core support base is a 16-foot but 2-inch thick structure with two forgings at the elevation where the load paths of the core assemblies are reacted.

The base of the core support has what we call module liners. These are cylindrical capsules which fit into the base of the core support. They provide the blockage features, and if you can imagine when all of these assemblies are in, there would be another barrier here. And testing again has confirmed that you could take essentially complete blockage of several of these and see almost no change in the outlet temperature because of the bypass feed that can come in from various other locations.

MR. SHEWMON: Halfway up that drawing I see something called core former structures. Do those move in and out?

MR. BALOH: This is a passive core restraint system. Let me work my way up. I was going to say a few words on those.

MR. SHEWMON: Fine.

MR. BALOH: Into the module liners we insert what we call lower inlet modules, LIMs; and what these are are assemblies having a body and a stem which you can't see because they are down into the module liners. But seven assemblies insert into each of these. They can be control rod assemblies, blanket assemblies or what have you.

For the blanket and control rod assemblies there are orifice cartridges in here that meter the flow. The orificing for the fuel itself is contained within the fuel assemblies and in some of the peripheral LIMs. There are also cartridges in the stem which provide flow to the outer shield assemblies through these bypass flow modules, which there are six of around the base on the outside of the lower inlet modules.

There is a seven-inch thick shield surrounding the core area on top of which is the core former itself. These are forged streams which have six segments pinned in place at the predetermined radius to give the proper gaps for containment of the assemblies that go into the core. And I'm not sure if they will be talking about this further, but it is mainly a matter of setting these gaps in the band between acceptable refueling loads to pull them out and compactness of the core to control reactivity during normal operation.

But these are preset at installation. What is set is the actual orientation and their location relative to the center of the core.

MR. SHEWMON: Your feeling is that with the 316 you won't have enough swelling to actually bind up your core at full burnup, is that it?

MR. BALOH: You mean swelling of the fuel assemblies in the core itself?

MR. SHEWMON: Yes.

MR. BALOH: Yes. I would have to defer that question to the people who will be talking about assembly design, but yes.

MR. SHEWMON: Well, the reason for them is so that you could get the fuel out after.

MR. BALOH: But even an FFTF where the design was started with both passive and activated core or core former capability, at the end it was a pin and set operation. That is now also a passive system, and I believe it has been confirmed to show acceptable operation.

MR. SHEWMON: I doubt if they have full burnup on the core yet. Okay.

MR. BALOH: The last of the items I will discuss is the horizontal baffle. You can see it in better perspective here. It provides the boundary between the hot outlet plenum sodium and the inlet plenum sodium which is bypassed; about 2 percent of the flow is bypassed up through and behind the liner to provide the cooling and the lower temperature on the vessel itself.

There are five fuel transfer ports in the horizontal baffle assembly itself. This is where when the in-vessel transfer machine temporarily parks assemblies for pickout by the ex-vessel transfer machine.

(Slide.)

This is a picture of the upper internals structure. It is primarily a 316 stainless steel structure, box structure, 3-inch plate top, 4-inch plate in the bottom. The whole assembly is suspended from the intermediate rotating plug of the closure head via 14-inch columns that are one-inch thick.

The lower portion -- and I will give you just a little more detail after I go through the general -- is a mixing chamber basically where the effluent from the core is mixed. The delta t's from varying assemblies can run 250, 260 degrees, and so this chamber in this location serves to mix and then pass up through these chimneys which also show a little more detail on.

Beyond that, the control rods, 15 control rod drive mechanisms mounted on the IRP, the drive lines pass through the shrouds which are made of inconel, and the shrouds then connect to -- not the shroud but the drive lines which pass through them connect to the control assemblies themselves.

Because the upper internal structure is mounted on the closure head, the fuel assemblies and such are on the core support. There are alignment keys that the UIS inserts into during normal operation. For refueling operation there is a jacking mechanism on top which jacks this whole assembly about 9 1/2 inches up and out, and then it rotates with the refueling equipment.

(Slide.)

Just to give you a brief runthrough on what the functions are, mainly I've already said that it provides the control rod alignment with the core via the shroud tubes and the alignment features where it goes into the core former at that elevation. It also protects against cross-flow as it comes out into the outlet plenum region, so that the concerns for vibration and such of the drive lines is minimized.

It mixes the core outlet flow.

(Slide.)

As the flow comes out, as I said, there is a relatively large delta t, and there are a number of items in this mixing box. The box itself is encased in inconel to withstand the thermal cycling imposed by the striping of these relatively large delta t's of the core effluent.

These items you see here, instrument post, I will speak a little bit about the analysis. The shroud tubes come down through the chimneys and also are in this location, so that as the fluid comes up into this chamber because of the distribution and velocity and so forth, there is a torroreal motion here, and then there is mixing and then passing up through the chimneys. There are 29 of these chimneys. They are also made of inconel. And as the fluid comes up, it then is injected into the higher portions of the outlet plenum. That is one of the next items here, mitigates transients.

During a scram when the effluent from the core would be cold there was concern that there would be short-circuiting. In fact, testing showed there would be short-circuiting. So by having the sodium, the cold sodium injected into the hot outlet plenum, we avoid the short-circuiting effect, and indeed have confirmed that the outlet plenum flow during transients as well as day-to-day operation meets all the design requirements.

(Slide.)

It also serves as an additional backup holddown. The core assemblies themselves are held in place by hydraulic balance. The lower portion -- and I don't have that picture -- the lower portion inside the core support plate itself. There is communication to low pressure plenum. All of the assemblies have direct access to that, so that the pressure underneath all the assemblies have access to the low pressure plenum, and so they are hydraulically stable. However, if for whatever reason hydraulic balance should be lost, all of the core assemblies, blanket assemblies have this secondary feature, and what you are looking at is the bottom portion of an instrument post, which looks like this.

And you are seeing the three pods, or whatever you would call them, that would cover all of the core assemblies, blanket assemblies and such. And in addition, where the instrument posts are not there, the lower portion of the shroud tubes serve as that function.

The overall upper internal structure can exert 166,000 pounds of upward force. If it were required to have the upward force of several assemblies, it would be well within any of that capability.

The instrument posts themselves, these, each of the instrument posts contains a dry well which terminates either at the center or in one of the three outer locations into which a thermocouple is inserted. These are replaceable thermocouples, and they provide then the indication of the temperatures of the effluent coming from the various subassemblies.

They are mounted on the top plate of the mixing chamber, and the leads, one of the concerns again was to assure that when we -- the leads themselves --

(Slide.)

-- Then come up through the support columns and into the head access area.

I think I have probably taken my time. If there aren't any questions, I will let the people who are going to be talking about all of the action in the core itself take over at this point.

(Slide.)

MR. DONCALS: In this part of the reactor discussion I will present an overall overview of the CRBRP nuclear design with special emphasis being given to the experimental support that we have performed to validate our predictions.

As you can see, the outline is as follows. Initially what I will do is present you a very brief discussion of the reactor description and show you some of the design bases that we used in laying out the CRBRP core arrangement.

This will be then followed by an overview of the critical experimental program followed by very speciifc examples of reactor design areas that have been supported by the experimental program. This will give you some insight into our ability to predict the nuclear predictions on CRBRP. I will then have a brief summary.

(Slide.)

The CRBRP reactor has 156 fuel assemblies and 76 inner blanket assemblies arranged in what we call a heterogeneous arrangement. As you can see, in the center of the reactor we have a small island of blanket assemblies, and this island is in turn surrounded by rings of fuel, blanket fuel, blanket fuel assemblies.

This whole heterogeneous arrangement of fuel and internal blanket assemblies are then in t n surrounded by 126 radial blanket assemblies. The design of the radial blanket assembly is identical to that of the inner blanket assembly. In turn, the blanket assemblies are then surrounded by 312 radial shield assemblies.

The core height of CRBRP -- that is, the active core height -- is 36 inches. On the top and bottom of this active height of core we have 14 inches of axial blankets.

Now, the particular arrangement that we have here for the heterogeneous arrangement was basically selected to achieve a breeding ratio of 1.2 in all operating cycles of CRBRP using a fuel assembly very similar to that used in FFTF. We call that the small pin design or .23 pin diameter.

So to achieve a breeding ratio of 1.2, we went to the heterogeneous arrangement. In going to that arrangement we also noted that we have additional benefits such that we reduced the fluences on the fuel assemblies by approximatetly 20 to 30 percent. So that was a significant gain.

In addition, we cut down the sodium void worth. We made a considerable reduction in the sodium void worth in the fuel assemblies in the reactor. So these were positive gains we achieved in going to the heterogeneous configuration.

The next point that I would like to make is that we did a lot of detailed analysis in coming up with this overall arrangement where you see the selective arrangements of fuel and blanket assemblies. And in doing this we achieved a configuration that only has one single fuel enrichment. In other words, we only had one single fuel enrichment in all of these assemblies.

In addition, you can see around the control assemblies we made pockets of fuel assemblies. This was done to enhance our control rod worth, and thus we were able to achieve the requirements that are listed in

General Design Criterion 24.

(Slide.)

The next point that I would like to very briefly cover with you is the CRBRP fuel management scheme because it is quite simplified.

As you can see here, all fuel and inner blanket assemblies are replaced as a batch every two years. In other words, this whole entire fuel and inner blanket assemblies are replaced as a batch with the exception that in alternating years at this location -- we have six of these locations -- we start with internal blanket assemblies, and then at the end of the burn we replace them with six fuel assemblies to give us sufficient excess reactivity to perform the subsequent burn.

In the radial blanket assemblies you can see first the first row because they are in the high flux region. We operate them for four years at continuous operation. We can't keep them in the reactor. And the second row of radial blanket assemblies are maintained within the -- are kept in the reactor five years.

Using this fuel management scheme that I've illustrated here, we are able to keep the fuel peak linear powers in the fuel assemblies below 16 kilowatts per foot on all operating cycles, and in the internal and radial blanket assemblies below 20 kilowatts per foot.

MR. WARD: Is that 1.2 breeding ratio the average in equilibrium?

MR. DONCALS: In fact, it is. Our minimum value is very close to 1.2. We are actually in excess of that in our equilibrium cycle.

MR. SHEWMON: Did you say what your heat per linear foot -- you said it was 20?

MR. DONCALS: In our fuel assemblies -- I will get to that a little later, but in our fuel assemblies the design criteria that we used, it's only a design criteria in laying out this arrangement, was that we would maintain the peak linear power in the fuel below 16 kilowatts per foot, and in the inner blanket assemblies and the radial blankets, 20 kilowatts per foot.

MR. DICKSON: Excuse me, Dick. You should add to that that is with 15 percent overpower.

MR. DONCALS: In the later part of my discussion I will present that. That is the definition of a peak linear power. But in a subsequent part of the discussion I will present what we include, what we define as the peak linear power.

(Slide.)

Very briefly, on the CRBRP control assemblies, just to identify them for you, CRBRP has 15 control assemblies, and they are broken down into two banks. We have two banks -- the secondary control system and the prinmary control system.

The secondary control systems are located in what we call the flat position of the seven row position. These control assemblies are parked at the exit of the core during full power operation; that is, at the exit of the core at the top of the core and the axial blanket interface. They have sufficient reactivity to shut down the reactor from any operating condition with a step rod and any anticipated fault down to refueling conditions. That is the purpose of the secondary system.

The primary system we have nine control assemblies, and you can see there are three in the row 4 position, and they are also removed from the reactor at hot full power conditions and parked at the exit of the core. The remaining six primary rods here at the corner positions are used to maneuver reactivity and supply the necessary reactivity in the operating cycle.

This combined bank here is able to shut the reactor down to hot standby conditions with a stuck rod and also any anticipated fault that we have considered to date.

(Slide.)

Now, the next subject that I would like to briefly go over and really highlight in my discussion here is the CRBRP nuclear experimental program that we have used to support all of our analytical predictions.

Here is a schematic of the various disciplines that either have planned, analyzed or actually performed the experiments. Since we are the lead reactor manufacturer, Westinghouse specified the experimental needs of this design in very general terms.

We then met with Argonne, GE and ourselves in what we call program planning meetings. These occurred very six months, six to eight months. And then after those meetings we then met with the Department of Energy under our base program, Phil Henning's organizations, and our own Clinch River project organization, and we had an agreed plan to perform the subsequent experiments at ANL, ANL or the Argonne National Laboratory who performed these experiments in the ZPPR facility. And then each of the disciplines -- and that is the point I would like to get across; it is not just Westinghouse -- but Argonne would analyze those experiments, Westinghouse and GE. These results were then compared, and Westinghouse would then incorporate the biases and uncertainties in the design. So we have a lot of cross-checks between the various disciplines.

(Slide.)

Just to show you, the experiments were performed at the ZPPR facility or the Argonne on the west site at Idaho Falls, and deep within this massive structure here -- it is really concrete, steel and sand -- we have our ZPPR plutonium critical assembly.

(Slide.)

And as many of you are aware, it is a split bed type assembly in which the halves are shown on the side and also on this side. The CRBRP core was mocked up by taking two-inch drawers with the representative core material and what we call platelet form and inserting them into this matrix, and also in this matrix you can't see here too well, but this was one of our configurations in our experimental program. After the core is configured, these two halves are brought together, and we have our critical assembly.

(Slide.)

Now, the reactor design areas -- and this is the meat of my discussion here -- have been supported by other critical assemblies as are shown. Shown are the power reactor design parameters that the nuclear people basically predict, and the critical experimental data source that has been obtained.

You can see we have a lot of data in support of our fuel enrichments, power distributions, control rod margins, reactivity coefficients, the various ones, and also other parameters such as breeding, temperature defect, ex-core detectors, and fast flux and fluences.

Now, what I would like to do is to highlight various experimental results that we have had obtained in these different design discipline areas and show you how well we do in our analytical predictions.

(Slide.)

The fuel enrichment philosophy in CRBRP, as many of you are aware, is just to guarantee that the reactor can be maintained at hot full power conditions throughout each design burnup cycle. To do this we normally just put enough nominal excess reactivity into the system to do that, plus whatever uncertainty level that we want to accomplish this at.

Now, as you can see, the key under the nominal excess reactivity is the prediction of the cold critical Eigen value, and I brought along some predictions in the ZPPR-7 facility of this value that we have achieved.

(Slide.)

And here you can see -- here are some different configurations in ZPPR-7. Here is our calculated K effective values, our experimental values, and our C/E values. And as you can see, the mean is .989, or we have roughly a 1 percent value in our predictions. Likewise, the standard deviation is very tight on this. It is something on the order of .16 percent. So we do fairly well on our predictions of the Eigen value.

And using information like this plus a lot of other experimental data, we have come up with the CRBRP fuel enrichments are on the order of 33 percent. That's defined as plutonium over plutonium plus uranium. That's the single enrichment for all fuel assemblies. And our beginning of life fissile inventory is on the order of 1500 kilograms.

Another area that we have a lot of experimental data -- and I'm not going to show you too much of it, but I would just like to highlight it to you, and here is what Paul Dickson was alluding to, what we call our peak linear power.

(Slide.)

Really it is like most physicists do. We use the radial power distribution normalization, the axial normalization, and we define it with our 15 percent overpower consideration. Likewise, we use the 3 sigma value when we quote the 16 kilowatts and 20 kilowatts value.

What I would like to do is just briefly highlight how we can come up with this uncertainty in the analytical predictions that we use in our power distributions.

Now, you won't be able to see this too good.

(Slide.)

Here is -- this is the mockup of the ZPPR facility. Here is the blanket fuel, blanket fuel, blanket control assembly locations, and here is the blanket assemblies, radial blankets surrounding the core.

The only point that I would like to point out here is that we have a lot of detail. Shown here is the calculated-to-experimental ratio for the Uranium-235 n fission reaction rate. And the point that I would like to bring out here is we have a lot of radial definition in both the fuel assemblies and the internal blankets as well as the radial blankets, and also in both directions and throughout the reactor.

Now, we have similar data like this for all the major different reaction rates in CRBRP.

(Slide.)

And I have summarized them for you on this sheet here to show you the accuracy that we do predict these various reaction rates.

Here's the reaction rates in the fuel at the beginning of life and at the end of life. We have listed the C/E values so you can see in the plutonium it's very good because that is the most power normalization that we have, and about 2 percent 1 sigma in fission reactions. You can see the various values in the inner blankets.

Now, taking data, this data, and using it with our predictions, we then came up -- we predicted the power distribution in all of the operating cycles.

(Slide.)

Now, this figure here shows you a composite or a snapshot of the maximum powers in the reactor. It is not at any given time, but the maximum power in the fuel assemblies -- these are these locations here -- that occur at the beginning of the cycle, and in the radial blanket assemblies where you see the 20 and these heavy hot line values. It is at the end of cycle 4 where you build up most of your plutonium.

So you can see here at this location this is where we've refueled that blanket with the fuel assembly. At the midterm we have 15.9 kilowatts per foot.

MR. SHEWMON: What is your average linear power then in that? Pick a high number at random and tell me what the average would be.

MR. DONCALS: I can tell you our radial -- well, let me define it this way for you.

(Slide.)

The combined effects of the FNR and FNZs, maybe the FNRs around 20, 25 percent, and the FNZ is around 30 percent. So you've got like 50 percent due to your shape factor.

MR. SHEWMON: Is that max-to-average ratio?

MR. DONCALS: Yes. Say 25 percent for the radial parameter; say 30 percent for the axial. So it is about the 50 percent for those two combined effects.

Does that take care of it?

MR. SHEWMON: No, because I want to know what an average rod power is at the middle, if you wish, and there is also 3 sigma in there plus 15 percent.

MR. DONCALS: The 15 percent, well, we have about 50 or 55 percent for this quantity; we have about 15 percent for that. If you just do not multiply them but add them, you get about 60, 65. This value is not large. It is about 8 percent. So we don't have a lot of gradients in the assembly, so basically you can take that value and divide by that magnitude, and you will have the average value.

But as you can see, the uncertainty is

relatively small in their prediction.

(Slide.)

The next subject that I would like to briefly cover, also to give you some more insight into the experimental results that we have to support our design, is in the area of the control rod worth data in CRBRP.

Now, we have taken a whole host of experimental data, and you can see we have performed various bank worths as illustrated here. We have also looked at asymmetric groupings of rods with different banks in and out. We have looked at pin control rod mockups versus these plates that we have in the ZPPR and bunching experiments where you will take pins in one array and then bunch them or predict those, and we also obtained the axial worth profile.

We also have not in the control area, but we have performed fuel blanket interchange worth experiments where we do interchange those six assemblies at the mid-year cycle.

Now, what I would like to do is show you some of the bias factors that are associated with these various control rod bank worths.

(Slide.)

That will give you a magnitude of how well we do predict these worths. And shown is the control rod worth predictions, the calculated worths and the measured worths. For the beginning of life condition that is identified as ZPPR-11B and end of life as 11C.

And you can see in the row 3 or the inner control. three control rods; these are close. And in the row 7 position, flat positions, about 7 percent and 4 percent. In the corner positions at the end of cycle mockup they were slightly better than that. So the biases are fairly small in these parameters.

Our uncertainty that we apply also is of the order of 12 percent; so in our control rod predictions in Clinch River we apply these biases plus a 3 sigma value, and we quote an additional 12 percent reduction in these worths.

(Slide.)

The last area that I would like to very briefly cover with you is show you some of the experimental results that we have obtained in the reactivity coefficient areas, and this is a significant one here.

This is again the ZPPR-11B configuration. You won't be able to read these different numbers, but what it illustrates is we performed a detailed radial zone voiding map in the ZPPR facility in which we took the sodium void out of selective regions in the reactor; and you can see the highest number is 13. So we really voided this whole reactor in 13 individual steps. This gave us a host of data that we could then go back and look at our analytical predictions and see what the normalization to this would mean in the way of sodium void in CRBRP.

What is shown here we feel is very significant.

(Slide.)

And let me discuss this very briefly. Using ENDF-3 data file one would predict -- and I'm only going to talk about the six-inch fuel height region -- one would predict about $1.15 in sodium void worth for CRBRP. If one would take the ENDF-4 data, which is a later data than we are currently using, one would predict $1.90.

Now, what we did, we then went back to the experimental data that I illustrated that we were taking and got biased factors of what we call our moderation and leakage terms in our perturbation calculation, and we now predict with -- we analyzed the ZPPR facility with ENDF-3, and then we got those biases and applied them to predictions using ENDF-3, and we got $1.50 for the sodium void worth.

We did the same calculation with ENDF-4. We went back to the experimental data, analyzed it with that, brought the appropriate biases, and we predicted $1.49. So we feel fairly confident in the biasing technique that we came up with here, and we got consistent bias sodium void worths in CRBRP.

(Slide.)

The next subject I would very briefly cover is we have also made Uranium-238 small sample Doppler. We have performed a small sample Doppler experiment. Shown is the measured fuel U-238 Doppler in the ZPPR facility. And also we calculated the Doppler, and you can see we got very good agreement. In fact, we underpredicted it by about one or two percent. So it gave us a lot of confidence that the Doppler prediction that we are putting in PSAR and that we are quoting is fairly good.

(Slide.)

In summary, I would like to just say that the bias factors and the uncertainties in the calculated CRBRP nuclear parameters are based on an extensive zero power critical experimental data base. The experiments have been completed. We are now in the process of finishing up the analysis on them and incorporating them in our data.

Thank you.

MR. REMICK: One of the criticisms one hears of the CRBR is that it is an outmoded design. Would you have any reaction to that from a nuclear design standpoint?

MR. DONCALS: Well, myself we have actually written some articles on that particular subject where we have counterargued on it, and we feel that CRBRP is not an outmoded design. It is a very advanced design. We are incorporating the heterogenous configuration which has considerable merits. We find that, as I showed before, that with a small pin similar to FFTF one can achieve high breeding ratios. You can reduce the fluences on the fuel assemblies by 20 to 30 percent, and that has a significant effect on fuel life time. We cut the sodium void by half with the design. And we have breeding ratios in excess of 1.2, and our doubling time is on the order or 30 years, and that is very similar to the large plant design. So we feel it is a very advanced design and not an outmoded design.

MR. BENDER: Excuse me. Can I ask a couple of questions about the evaluation of the core under malfunction conditions? You assume sticking rods, I guess. What does it do to the power distribution?

MR. DONCALS: In CRBRP -- the question came up earlier this morning -- but we keep our bank, we operate in a bank mode, but we keep the bank within an inch and a half. We want all the rods to be within an inch and a half of the average position. And that would result in power perturbations with the extreme one being out relative to the bank on the order, I would estimate, of 4 percent.

Now, that 4 percent uncertainty has been included in when we quote the 15 kilowatts per foot.

MR. BENDER: I don't have any reason to know that it can or cannot happen, but is it possible for one rod to stick in a position that is significantly different from that allowed for inch variation?

MR. DONCALS: There are procedures that if it does stick, there will have to be -- and I believe maybe Paul Dickson could explain it better than I. If a rod would stick in a given position and it gets out of alignment more than an inch and a half, some corrective action must be taken at that point; that is, in our tech specs and in our procedures.

MR. VIJUK: You're really assuming the rod is stuck and doesn't come in. The bank is maintained. It is monitored. There are two kind of displacement transducers on the rod positions, and there are electronic rod blocks on the positions. The controlling bank must be within an inch and a half during operations.

MR. BENDER: And if it isn't, what happens?

Does the reactor scram?

MR. VIJUK: If it isn't, there's a technical spec shutdown.

MR. BENDER: Okay. Let me ask a slightly different question. Since the fuel is moving around a little bit in this reactor, I guess, you are shifting fuel elements occasionally, I guess, or are you?

MR. DONCALS: Do you mean shuffling fuel?

MR. BENDER: Yes.

MR. DONCALS: No. In CRBRP we burn in place and then replace the whole core. We never shuffle assemblies.

MR. BENDER: And how about the blanket?

MR. DONCALS: No. We burn in place and then remove it.

MR. BENDER: So there's no problem of misplacement of fuel.

MR. DONCALS: No. That is one of the things we like about our design, that we are able to, because the power distribution is relatively flat, we achieve uniform burnups, and we don't have to shuffle fuel.

MR. BENDER: That's good. Thank you.

(Slide.)

MR. MARKLEY: I am Bob Markley, and I will be discussing the core thermal and hydraulic analysis and design. I will cover the core T&H description and bases, including flow paths, principal design data, flow allocation; summarize the performance predictions, both steady state and in transients; cover the T&H development test programs, including some examples of data; and conclusions.

(Slide.)

This is a schematic of the reactor itself. The flow comes in through inlet nozzles, mixes in the inlet plenum, is fed into the lower inlet modules via primary or auxiliary ports. The lower inlet module directs the flow to the core assemblies -- and when I say core here I mean fuel blanket control -- and even the radial shield assemblies.

The flow there is predominantly axial. It is just like swirl to the flow and the bundles, the volume in the mixing chamber and through the outer plenum up here.

MR. WARD: Where does the swirl come from? Is that the stable swirl just by the inlet flow?

MR. MARKLEY: We have wire wrap spacer systems, so they are excellent mixing devices.

(Slide.)

The principal core T&H design data, just to review this with you, for fuel and blanket, this is the number of rods per assembly. The rod diameters, as we mentioned, about .230. This is the outside diameter, about a half an inch blanket, the pitch-to-diameter ratio, the wire wrap lead or pitch, and some of the lengths of flows in the bundles themselves.

(Slide.)

This vu-graph summarizes the flow allocations that we have determined to meet design requirements. What I have here are the principal or major flow paths. We have orificing zones as shown here in these assemblies, and then the major constraint, the constraint that has determined that flow basically determined the flow that we have allocated to those assemblies. And let me just go through that a little bit.

We have 66 percent of our flow fed to the fuel assemblies, 16 percent to the inner blankets, 12 percent to the radials, a little over 1 percent to the control assemblies and so forth for removable radial shield, the vessel and leakages and so forth. And the 66 percent flow to the fuel assemblies is metered in 6 orificing zones, 6 different flows.

Naturally, the highest power assembly gets the most flow and so forth. So we can optimize both temperatures and the use of flow. The same thing for the inners, the blanket and the radial blanket. As you notice, there is one zone of 6 here in both. That is the alternating fuel blanket assembly.

MR. BENDER: When are those orifice adjustments made? Are they predesigned into the core?

MR. MARKLEY: Yes.

MR. BENDER: And are they never changed during the life of the core?

MR. MARKLEY: No. The fuel orificing is in the fuel assembly, so they would be designed into the fuel prior to putting it in whenever you replace these assemblies. The same thing for the inner blankets. A portion of the radial blankets is in the assembly and some in place. So you have -- you can control the flow basically as you put the core in as you design it.

MR. BENDER: How do you check to be sure you've got the right flows?

MR. MARKLEY: We have outlet thermocouples. That is certainly one check.

MR. SCHWALLIE: We have a mechanical discrimination system; that is one-way discrimination such that any assembly that goes into its right place, and if it's in the wrong place it's always overcooled.

MR. DICKSON: You're not relying upon thermocouples to tell you have the right flow distribution, the mechanical discriminators that he just spoke of.

MR. BENDER: That doesn't tell you whether you've got too much. It just keeps you from getting too little.

MR. DICKSON: That's correct.

MR. SHEWMON: Why don't you two hold a public discussion?

(Laughter.)

MR. WARD: They can't all be overcooled. You couldn't get them all in.

MR. DICKSON: That's correct.

MR. MARKLEY: We certainly have flow checks to check the flows so we know what the flows are.

MR. DICKSON: Prior to inserting them in the reactor.

MR. MARKLEY: Within a very close amount.

MR. MOELLER: On the last item on the right there you have what, 1.4 percent. I can understand bypass. How much is actually leakage, or are you referring to leakage as sort of another way of saying bypass?

MR. MARKLEY: As you know, we have piston rings in our assemblies, and we have run a considerable number of tests on the piston rings. We have them characterized, and that is what we mean by data and calculations. And again, this is a maximum value of leakage through the various paths of that sort.

MR. MOELLER: It is leakage through the paths. It is not leakage as you would think of leakage out of the primary system or something like that.

MR. MARKLEY: No.

Just to go through one of these, and certainly the basis for our allocation of the fuel assemblies are the pin lifetime and in transient considerations, the striping and the assembly outlet temperatures, and we consider all of those a priori in setting the flows.

(Slide.)

This vu-graph summarizes the principal core T&H performance data. These are the design conditions for the plant. I think you have seen those before. This is the pressure drop from nozzle to nozzle in the reactor, and by fuel inner blanket and radial blanket. I have already mentioned the number of flow zones, orificing zones. This gives you a feel for the range of the hot spots.

This is the cladding ID temperature tha we see in the fuel, and the maximum temperature in the fuel in the inner blankets and the radial blankets. These are the values of the fission gas pressure buildup through burnup, maximum value of the fuel around a thousand, for the inner blankets 2 to 300 psi. And those temperatures and pressures meet the lifetime requirements that Mr. Schwallie will discuss later.

These are the type of velocitie we see in the bundles of the fuel and blanket assemblies; and again, that is well below the limit of about 30 feet per second which is a very conservative limit that we've set.

Also, the maximum mix mean outlet temperatures on a nominal basis here for the various assemblies, and the magnitude of the temperature gradients, the maximum temperature gradient we will see between a fuel and a radial blanket assembly.

MR. WARD: What is the basis for the 30 feet per second limit?

MR. MARKLEY: We have looked at capitation erosion-corrosion considerations. We feel 30 feet per second is even no problem, but we set up a limit of 30 feet per second for that. We also have limits in the orificing the same way, but again, we feel they are very conservative for those considerations.

MR. MOELLER: Help me with understanding this pressure of the fission gas. What is the cladding designed to withstand?

MR. MARKLEY: Well, it is a function of

temperature, of course, but it is -- well, it is certainly above that.

MR. MOELLER: And does that assume some leakage rate?

MR. MARKLEY: No. This is the gas that is captured in your pins.

MR. MOELLER: This is the pressure inside the pin itself?

MR. MARKLEY: Right.

MR. MOELLER: Okay. Not the pressure being exerted on the fission gas.

MR. MARKLEY: It's the internal fission, your fission gases due to burnup which you accumulate inside the pin.

MR. MOELLER: Okay.

MR. AXTMANN: Have your fuel elements been tested for four years in a reactor?

MR. MARKLEY: Ambrose. I think Ambrose Schwallie knows that history very well.

MR. SCHWALLIE: This is Ambrose Schwallie.

We have run pins in EBR-2 of this very similar design except for overall length considerations, certainly these pressures and beyond those times for years. The EBR-2 data base I'm going to call the reference pin design which is similar to Clinch River, and that of FFTV has shown that 80,000 burnup is very easy to accommodate in all stainless steels, and we've achieved burnups of around 140,000 and 160,000 in a few experimental pins.

MR. AXTMANN: Let me propose a scenario that the wire wrap is -- how is it held to the fuel element?

MR. MARKLEY: It's captured at each end and wrapped around the rod.

MR. AXTMANN: Suppose in two years that the wire wrap starts to degrade. Somebody used the wrong weld to attach it to the fuel element, so that you start getting little fragment bits as a progressive disease. And this is imaginary. How would you find out about that as wire wrap starts to fragment?

MR. MARKLEY: First of all, as you know, there is a lot of experience with wire wrap bundles. In irradiation experiments we have not seen that kind of performance.

MR. AXTMANN: I suppose you haven't, but I'm postulating now --

MR. MARKLEY: I think you might get -- again, as you know, the wire wrap does give you a slight local hot spot. If the wire wrap accumulated, you would still have that hot spot. It has to be right at the top of the fuel to give you any real hot spot problem. I don't think it would give you much of a problem.

MR. VIJUK: You would pick that up through your operating fuel monitoring system. If you got this hypothetical degradation, eventually you would get some breached cladding. We have a tech spec on the operations. We would detect failures as they occur where the activity in the cover gas at the top of the vessel. We would then have delayed neutron detectors on the primary pipes and so forth, and then we have a tech spec limit on how many failures we can operate the reactor with.

MR. MARKLEY: I don't even --

MR. VIJUK: That's the only control on it. There was no other way that you would be monitoring for such a phenomenon taking placing. You just look for breaches of the water.

MR. WARD: How do you locate the fuel pin or the assembly that fails?

MR. VIJUK: We have a discrete tag gas. Each assembly contains a tag gas that you can pick up.

MR. AXTMANN: Thank you.

(Slide.)

MR. MARKLEY: Okay. This is a map of the core assembly mixed mean outlet temperatures. This is at the beginning of cycle 1. What I was showing here, this is the center of the core, and this is a one-sixth sector of the core. Your radial blanket, two rows of radial blankets out here.

The control assemblies -- and this is the assembly number, the nominal mix in the outlet temperature, and this is the outlet temperature on a 3 sigma basis. The darkened-in assemblies here are just the ones where we look at every assembly, we look at every pin and analyze it and determine where the hottest pin and where the peak pins are.

And these are -- this is a peak fuel pin that is located in this assembly, the hottest fuel pin in this assembly, the hottest and peak inner blanket assembly in this assembly, and the hottest -- I'm sorry -- the peak radial blanket pin in this assembly and the hottest pin. And those pins we look at in great detail and do a lot of structural analyses on those to determine that they are the limiting pins and characterize them.

MR. WARD: During operation you will have a single thermocouple at the outlet of each one of those assemblies?

MR. MARKLEY: That's right. Except I think there are eight fuel and a few radial blankets that don't have it.

MR. WARD: During operation how do you expect you will deal with failures of those thermocouples? Will you allow operation if a certain number of them fail?

MR. MARKLEY: Yes. We will allow operation and with a certain number of failures, and that will be determined.

MR. CARBON: What is the magnitude of the percentage that you will allow?

MR. MARKLEY: I don't believe we've come up with that number unless Paul Dickson has.

MR. DICKSON: Yes. As Mr. Vijuk stated earlier, we are looking for failures for monitoring the activity of the cover gas, the tag gases appearing in the cover gas, or if the failure results in the breach of the fuel region, the delayed neutron detectors. Those are our primary means of detecting failures rather than thermocouples.

The only requirement to continue operation of the plant as far as thermocouples are concerned are those that we discussed last which are the control thermocouples.

MR. MARKLEY: And there are 30 of those.

MR. SHEWMON: Replacing a thermocouple isn't too hard, or is it major exercise even when you're changing fuel?

MR. MARKLEY: You can replace the thermocouples. You may elect to wait until the end of the cycle when you are shut down to do that.

MR. SHEWMON: That wasn't my question. The question was how much extra work is it when you are shut down at the end of a cycle?

MR. MARKLEY: I will have to ask someone else to answer that.

MR. SHEWMON: Is it harder than putting in another fuel rod?

MR. BALOH: This is Frank Baloh.

You would only replace thermocouples when you were shut down, and it is an extensive process. It is not like putting in another assembly. You would not just do it in a very quick manner like a few minutes or even an hour.

MR. SHEWMON: Thank you.

MR. EBERSOLE: When one of these fuel pins bursts for whatever reason, does that produce any sort of immediate reaction?

MR. MARKLEY: Yes. You release cover gas where we detect that.

MR. EBERSOLE: In the course of dumping the 962 psi gas load into the core, in the worst place does that produce a minor power pulse of any sort?

MR. MARKLEY: No. We would not see that in this power.

MR. EBERSOLE: Thank you.

MR. ETHERINGTON: Do you have a procedure for locating failed fuel pins?

MR. MARKLEY: Yes, we do. We have tag gas which is a different type of concentration.

Ambrose, do you want to answer that?

MR. SCHWALLIE: Yes. This is Ambrose Schwallie.

Each assembly has a tag gas which is a xenon-krypton mixture and those isotopes. And through mass spec dialysis or diagnosis of the cover gas we could determine which assembly it is.

MR. WARD: I guess I missed the significance of the 30 control thermocouples.

MR. MARKLEY: Throughout this core we have 30 thermocouples that are used for control, and they are, if you average them, they are very close to the value of the average.

MR. WARD: They represent the core average?

MR. MARKLEY: Right. There are 30, and they are symmetrically located, et cetera, so that we get a good average condition of the core, and they are also over even the maximum temperature assemblies and so forth, too.

MR. REMICK: Can you select a different 30 from time to time? If one fails, can you select another?

MR. MARKLEY: Yes, you can hook up others. You could actually lose some of those and still have a very close average. And over a lifetime of a core those average out very close to staying constant, within a few degrees.

MR. DICKSON: The 30 includes an allowance for failures. The 30 includes spares.

(Slide.)

MR. MARKLEY: The next page in your handout also shows a similar map for the hot spot temperatures also. I didn't intend to show that. You can look at it in your handout.

(Slide.)

The next page in your handout also shows a typical cladding and temperature-pressure history showing how the hot spot temperature, the hottest pin in that assembly, the hot spot temperature changes over two cycles and how the pressure will build up as you typically see. And this type of information is given to the structural analyst, and they use those to determine the integrity and the lifetime of the assemblies themselves.

(Slide.)

The next area I would like to cover or at least summarize are the design transients, and they fall into two categories, undercooling and overpower type events. And what I'm going to show you here is our worst case undercooling event, the results of it.

This is the three-loop natural circulation transient which I believe you had presented to you before. I will summarize the maximum cladding. This was the hottest spot in the core in each of these assemblies that we predict and the time of occurrence.

These results are presented in detail in this report. What I have listed here is the assembly, the fuel assembly where you see the hottest pin inner blanket assembly and the radial blanket assembly. These are the nominal temperatures, and they are 1300 below, and in a 3 sigma worst case with a lot of conservatisms, we calculate temperatures as high as 50 and 65. Our acceptance criteria is boiling, less than boiling, and boiling at these locations would be 1720 or greater because that is based certainly -- the hottest spot is at the top of the fuel. It is also based upon these conservative conditions of zero flow, zero cover gas pressure, and the minimum cool level.

And as you can see, we have even with the 3 sigma conditions, we have greater than 150 degrees margin on a 3 sigma worst case basis, and like greater than 400 degrees margin on the basis of a nominal calculation.

(Slide.)

And the next vu-graph shows you one of the reasons why we feel these are very conservative calculations. And I have not listed all of the conservatisms here because there is a long list of them, but I think this puts it all together in perspective.

What I am showing here is we have done some pre- and post-test analyses of the FFTF natural circulation tests, and they ran their tests from 100 percent power and 100 percent flow, and what I'm showing here is the measure and the predicted sodium temperatures.

This is at the top of the fuel section where you get the maximum temperatures. It is this particular thermocouple located in the fuel, and it is for the row 2 photo in the FFTF test.

This shows the sodium temperature versus time, and here we have the measured data as measured from that test. These were the pre-test predictions and the post-test predictions of our best expected values, and they are pretty close to nominal. Actually, if anything they are a little conservative because I think the designers always want to make sure they have some conservatisms in there. But they are pretty close to nominal-type calculations. They are best expected.

The difference here is that when we put the actual operating conditions in that test, the decay powers and so forth, we got a little different conditions but still quite close to what we expected.

Now, if you go to the same basis as the 3 sigma worst case temperatures that I just presented to you, this is this prediction, predictions from the current CRBR assessment approach, you would predict a temperature like this, and that is about 3 to 400 degrees above what you expect.

We also feel there is a lot of buoyancy help in these kind of low flow conditions and inner assembly heat transfer. These things, your hot spots, are always going to be helped by those self-compensating effects. And there's even about a hundred degrees conservatism in that. And this is the prediction with accounting for what we call inter- and intra-assembly flow and heat redistribution. Basically it is buoyancy and heat transfer between the assemblies, as you will see, in a low flow condition.

MR. CARBON: On the measured data curve are there quite adequate points to really shape that curve?

MR. MARKLEY: Yes. There is a lot of intermediate points, though it would get too busy if I put them all on.

MR. MOELLER: Excuse me. Could you go back two slides where you were showing the pressure buildup with time in the plenum?

(Slide.)

What I need clarified is across the abscissa you show CY-3 and CY-4. Is that -- no, go back one more on the graph, the graph before this chart -- you show -- is that the third and fourth year?

MR. MARKLEY: Yes. This just happens to be one typical example I show you for the third and fourth cycle. It is typical of the temperature and pressure histories that we see in a fuel pin.

MR. MOELLER: Well, what ab ut the first and second year?

MR. MARKLEY: It's similar. Just not as long.

MR. MOELLER: I'm not with you.

MR. DICKSON: Bob, he's missing the point. Each assembly is only in there two years. This assembly was brand-new at the beginning.

MR. MOELLER: I'm sorry. I thought they were in there for four years.

MR. DICKSON: Some of the blankets are in there for four years, the radial blankets, and some of the radial blankets are in there for five years; but everything within the core is two years.

MR. MOELLER: Thank you.

(Slide.)

MR. MARKLEY: Again, because of the press of time I'm summarizing our transient area. And this just summarizes.

We certainly have established the proper interface requirements such as the PTS settings, flow coastdowns and other things that affect your transients. We have compatible steady state operating conditions, and this was greatly influenced by our orificing, by flattening and reducing maximum temperatures, by optimizing the flow to the various core assemblies.

And all design basis accidents, both overpower and undercooling, have been evaluated on a conservative basis and meet the guidelines of no boiling, no clad melting, and acceptable lifetime structural integrity.

(Slide.)

Okay. The next area I would like to cover then is the T&H development testing. The last time I was here I presented the example on the list of the fuel assembly development area, so I will go through quickly some of the development testing for blanket assemblies that we've conducted.

(Slide.)

We have conducted or performed a full-scale 61-rod heat transfer test in sodium -- this is a prototypic blanket bundle -- looking at the very detailed temperature distributions. We had around 700 thermocouples in this. We got electrically heated rod. We not only looked -- we covered a very wide range of operating conditions, and we also included transients, natural circulation, and so forth, and that program is about completed. And I will just show you a few examples of that and try to give you a feel for what we have done there.

(Slide.)

These are the wide range of test parameters that we have covered. As far as flows, well below anything we expect in natural circulation. The temperature range, we have looked at flat power skews all the way up to 4.6 to 1, very steep power skews. We have simulated our adiabatic boundaries. We have auxiliary reduction inside, so we can look at inter-assembly heat transfer. You can cool these ducts or you can heat them. And we have covered a lot of data in that area. We also have a lot of transient and natural circulation data, too.

MR. CARBON: These are electrically heated rods?

MR. MARKLEY: Yes, they are. Let me just show you one or two pieces of data from that.

(Slide.)

This is a plot of normalized temperature across this 61-rod bundle, and these are the typical sets of data that we have obtained. This is at the core midplane, the heated zone midplane. This is a plot at the outlet of the heated zone, and of course there is where you see your maximum temperature. And this is a plot of an elevation 25 inches up into the bundle.

We have several test series here, and every time we would run a test series with a gradient of this sort, which is 2 to 1 across the bundle, we would then reverse it and check the test again. And you can see excellent duplication of test results.

We also made code predictions, and you can see our subchannel code predictions versus that. And naturally, one of the important things we look at are the peak predicted hot spot versus the measured hot spot.

MR. WARD: Did you actually make those as predictions before you ran the test?

MR. MARKLEY: Yes. And after, too.

MR. WARD: That's easier, isn't it?

(Laughter.)

(Slide.)

MR. MARKLEY: And then you would take those hot spots you measured and predicted data for all of the varying conditions that you looked at, plot them, and we would do a statistical analysis of that, get the direct bias and the statistical variations, and that determines the uncertainty with which you can predict that temperature. And you can see we have a lot of data points for a prototypic bundle there

(Slide.)

MIT has done a lot of work, fundamental work, research work in these areas. We also had a 5 to 1 scale air flow test where we learned what these flows really looked like because you have got to understand them, and it is not easy. And with a large bundle like this we could map in very great detail the cross-flows and the axial flows.

Pressure drop tests -- and let me show you an example of those.

(Slide.)

Here is over 200 data points of a friction factor versus the rod bundle Reynolds number for the blanket bundles. There is air data, there is water data, and there is sodium data here, and there is also some other correlations by other people up here at the higher flow zones. But you see very good correspondence and no instabilities in that kind of a rackup of test data. And that is actually used in all of our -- that is the basis for our analysis.

MR. WARD: What sort of Reynolds numbers do you have with the plant operation?

MR. MARKLEY: If you're at a hundred percent flow, you would be here 10 percent, 1 percent. We have gone down to almost 2/10 of a percent. We expect maybe 3 percent as the lowest in our reactor.

And then just to summarize one area again --

(Slide.)

-- I have tried to summarize all of our core pressure drop testing in this sheet, and I've lifted the components, the component parts of those components that you would test, how much they mean in the pressure drop -- this is an approximate value -- the range that we have covered. And in all cases we have gone 100 percent or more down to again much lower than we expect in natural circulation for at least all of the high heat generating type assemblies.

The others are either out of the core or very low, but we have still covered a good range there. But we have not gone quite as far, and you can see the status. They are practically all completed except one or two there which will be completed, and certainly this data will be factored into the FSAR work.

(Slide.)

I then put some more examples in the handout of friction factor data for our fuel bundles.

(Slide.)

Pressure drop data for the overall fuel assembly. So that you can take these component parts and add them up. They should check to the total pressure drop for a prototypic bundle -- I'm sorry -- a prototypic assembly.

Pressure drop data for our control assemblies where we again have over 50 points that we have taken, and I have included that in there, and also some pressure drop data of some of our orifice shield assemblies; but they are all tested and characterized by flow tests.

MR. CARBON: Would you comment on what we heard this morning about the pressure drop in FFTF went up 15 percent or something like that? Why was that?

MR. MARKLEY: We don't know yet, and they don't either. We are looking at it, and we will certainly try to factor that into our design. We do have fallback methods in case that is a reality.

MR. CARBON: Did it occur right after startup or over a period of time?

MR. MARKLEY: It would build up slightly, I believe, like a psi per day until it got up to the 10 to 15 percent.

MR. CARBON: And then just sat there?

MR. MARKLEY: Then it would peak out as far as we can see. When it shuts down you would partially lose it, and then it will build a little bit.

MR. CARBON: Is it consistent throughout all the fuel or just sporadic?

MR. MARKLEY: I think we are working with them trying to get the answers to that at this time, Dr. Carbon, and I'm not sure we have it yet.

(Slide.)

In conclusion, on --

MR. MOELLER: Excuse me. Has that been the experience in France or any of the others?

MR. MARKLEY: It has been experienced in a few out-of-pile loops where you had sodium endurance testing in a few cases. At the time we thought it was bearing scoring or something, and we were really trying to decide what it is.

MR. CARBON: Excuse me. Still in answer to his question, have they experienced anything like this in Phenix or PFR?

MR. MARKLEY: The EBR-2 people were not aware of this and do not think they have experienced it.

MR. CARBON: What about the British and the French?

MR. MARKLEY: When I talked to the British three or four years ago they did not know of anything of this sort. I talked to the French, and I do not know their experience in that.

In conclusion, in the core T&H development testing area we do have a large T&H data base available already. The data is on all components over a wide range of operation, and I tried to illustrate that with our pressure drop testing and our heat transfer data.

The uncertainties that we use are based upon this data and have been factored into the PSAR, and all the data will be factored into the FSAR, what little we don't have remaining.

(Slide.)

And in final conclusion, the reactor flow distributions do meet the component design requirements. The cooling flow paths are well characterized. They are controlled by orifices which have been tested over a very wide range of conditions and are factored into our analyses.

We have a large component T&H development base already available. We have a comprehensive design where you look at every assembly, every pin based upon conservative yet realistic limits; and the analysis methods are verified with a large data base, as I showed you one or two examples.

MR. BENDER: You have clearly done all you could, all you think you ought to do prior to building the reactor and putting it into operation. What do you envision as other kinds of confirmatory tests that might be needed when the plant starts to go to power or prior to going to power?

MR. MARKLEY: We will be running a natural circulation confirmatory test. There are system flow tests, as you know, many of them co[illegible] and at heated conditions during startup.

MR. BENDER: I'm thinking in terms of just monitoring the flow distribution over the core. You've got the thermocouples which will tell you what is happening when you are at power, but is there anything prior to that that should be done?

MR. MARKLEY: I think the testing that we have done certainly gives you an excellent feel for the pressure drop. These are well characterized.

MR. BENDER: Well, what I'm trying to find out now is how to look for surprises like the 15 percent change in flow that was observed at FFTF. Would I know about that by something other than thermocouple indications?

MR. MARKLEY: Yes. Your flow meters, your pump. You certainly would have a pump that would be affected by that, and your thermocouples.

MR. DICKSON: That is not a 15 percent change in flow. That was a 15 percent increase in pressure drop, and it was picked up because the pump did not take care of it.

MR. BENDER: Thank you, Paul.

MR. MARKLEY: Any other questions?

MR. DICKSON: Bob, before you go away, I would like to note one thing that you didn't mention. The Japanese have also seen an out-of-pile test at pressure drop increase in fuel assemblies but not in-pile. But I would like to follow up with Dr. Remick's question that he asked of Dick Doncals.

I don't want you to take too much time, but if you can just take a minute to describe where your analytical capability is today compared with where the analytical capability was when Clinch River was started, in other words, that using an FFTF. And also, since you have been a member of the technical interchange with foreign countries, if you could compare our analytical capability, particularly that that the French would like to have, with some of the foreign capability.

MR. MARKLEY: Okay. First of all, certainly we also designed FFTF at Westinghouse ARD. A lot of our tools were extensions of the tools used for FFTF core design. I think we have developed them to a much greater extent, and with a lot of data to back up those analytical methods.

In summary, I could get into any detail, but I don't think we want to. But we are using a lot of the -- at least in the T&H area and physics and so forth, a lot of the methodology we used to design the FFTF reactor, and we are using that or improvements on it now for Clinch River. This is the same people.

MR. DICKSON: I thought you would note that in the FFTF the orificing was done to equilibrate temperatures at the beginning of life. A major advance was thought to have been achieved when we equilibrated temperatures at the end of life, and we are now orificing to maximize lifetime within constraints set by delta t's between assemblies and all transient and safety analyses.

MR. MARKLEY: We factored them all in to a prediction of the lifetime, so our flows are determined by optimizing lifetime throughout the plant. We do the entire calculation of that. We have been around that cycle several times, so we feel we can do it.

As far as foreign, foreign methodology, yes, I have talked to several -- the Japanese, the Germans and the British. I think they're way ahead in methodology and in the T&H area. That is all I can speak for. I think we in this country have put a lot more into developments of methods and also in testing. We have performed a lot more testing in the core area I believe than those countries.

I cannot speak for the French. We were not able -- I have never been able to talk with the French very freely.

MR. DICKSON: Is it a fair statement to say that they put in more instrumentation in order to measure temperatures and flows because they have a less degree of certainty in calculating them?

MR. MARKLEY: I could only guess on that, and that might be unwise.

MR. WARD: Since you --

MR. BENDER: That is a two-sided coin. You've got a lot of faith in your computations and are arguing that you don't need this much measurement capability because of it. But I think your case is somewhat self-serving as you presented it.

MR. DICKSON: We tend to put forward self-serving cases, but we heartily agree with it.

(Laughter.)

MR. WARD: Since the assemblies are individually designed or the flow orifice designed and you don't have a thermocouple, let's say, at the outlet of each assembly, it is conceivable that you could get one in. There are controls, mechanical or administrative controls, what have you. You could get one in and operate the reactor, and you would have a much higher sodium outlet temperature there.

Now, is there a safety issue there?

MR. MARKLEY: We have discriminator posts that you just can't put that assembly in the wrong position. He just can't put it in there.

MR. WARD: What if you've got the wrong piece of hardware? I don't know what your orifice plates are, but it is some piece of hardware. What if the incorrect piece of hardware gets in that assembly?

MR. MARKLEY: I believe there is very close surveillance and QA for that, so I don't think we expect it. That is very unlikely. It would be very unlikely maybe that that particular assembly didn't have a thermocouple.

As far as locations, we do have symmetry in the core, so you do have symmetrical sectors at least covered. If one assembly is not covered here, you have five other assemblies just like it that will have thermocouples over it. So there is a lot of redundancy and duplication from that standpoint.

MR. WARD: If the assembly was operating with a low flow and it was undetected, is there a safety issue there?

MR. MARKLEY: Certainly if you got low enough flow you would have failures, and then we would have DND systems and cover gas systems that detect that.

MR. WARD: And you see that as just an operational issue, or is there a safety issue?

MR. MARKLEY: I think it's an operational issue, because we do not see propagation in any sort of these assemblies. Even in the unlikely happening that they would get into trouble, our calculations say they do not. I think Fermi proved that. They had a couple of assemblies where they shut off all the flow, and only those assemblies failed, and it did not propagate beyond that.

MR. KERR: But Fermi was operating at fairly low power at the time, as I remember.

MR. MARKLEY: It did not propagate at all either, but you are right.

MR. CARBON: I would like to add to that that some of us would argue that Fermi, the statistics there don't prove anything. And it is the project's intention that there would not be propagation from some small something up to a major event. But the French and British looked at that quite differently, and it used to be a concern here in the United States that propagation would be something to be quite concerned about. And we have asked them to present a more extensive argument on this in about two or three weeks.

MR. WARD: Does this include, for example, can a pin failure -- maybe Mr. Schwallie will be addressing this -- but if there is an individual pin failure, is there a sodium oxide reaction?

MR. MARKLEY: Again, we do not think -- and there's a lot of evidence based upon our Argonne National Laboratory work -- that these do not propagate if you have different kinds of failures, gas release or whatever. But I gather that is the subject of another meeting.

MR. CARBON: It will be a more extensive

meeting.

MR. MARKLEY: And incidentally, knowing my discussions with the British, I'm very surprised that they feel that way, because several years ago they did not. They agreed wi h us on propagation.

MR. CARBON: Dave, I didn't want to stop you from asking questions. I simply wanted to tell you that there will be more discussion later on this.

MR. MARKLEY: Thank you.

MR. SCHWALLIE: My name is Ambrose Schwallie, and I ask you to bear with my head cold today. I apologize for that.

(Slide.)

The areas that I'm going to talk about in the next half hour is try to give you some confidence in the limits that we talked about and were pointed out by Mr. Baars this morning, to try to tie that back to what they try to serve in terms of what the design can accommodate, and relate that somewhat to the safety situation.

I would point out some of the things that we've talked about just briefly in the design description and give you some feel for some of the more pertinent evaluations in terms of the margins as we see them today as contrasted against those design limits and the status of the testing programs. And I'm not going to put too much emphasis on what we have completed but what is ongoing to solidify the design a little bit more.

(Slide.)

I think at a previous meeting Dr. Dickson talked about core design criteria a little bit. The point I want to make here is that from the RDT standard in terms of the damage severity limits for the core through unlikely events -- and sometimes I will use the terminology "normal upset" and "emergency upset," referring to anticipated events, and "unlikely" and "emergency" are synonymous.

But through this point we tried to design the core such that we preclude failure from any mechanistic phenomena that we would understand. Okay. And in the case of the fuel rod and the blanket rod, we use two techniques.

The ductility limited strain criteria is more of a recipe-type criteria that is not so phenomenological in nature but accounts for certain pertinent aspects, namely thermal creep and plasticity, and was derived primarily in the FFTF days, and it has been modified by us to account for a somewhat little more understanding about fuel. But it's basically a recipe that is a designer-oriented quick tool for assessing the design.

We have gone to the cumulative damage function technique which is more predictive in its nature and tries to dynamically track the materials properties through time, the fuel performance with time, irradiation effects, fluence effects, hardening, what have you.

This technique is being integrated very heavily right now in the national fuel programs in terms of the LIF code and the generation of the data base to try to qualify that technique.

(Slide.)

In terms of the assembly, what I thought I might do is just walk you through and point out the pertinent features. From the outside of the assembly starting from the bottom to the top this lower inlet nozzle region which extends about in this area, this interfaces with the lower inlet modules. There are two piston rings to try to prevent excess leakage flow from going past the assemblies, up the outside, through the interstitial region of the core. Really, these elongated holes in the inlet slot are such that if the assembly moves up and down from its full seated position to a secondary holddown position, reduced flow would not result.

This discriminator post is designed such that if you go into the wrong core position, it will not seat, and this is in a core position that would result in the assembly having less flow than it is supposed to have such that this length is long enough the refueling machine would not let go, it wouldn't seat down, and the interlocks would say I'm locked in a position; you can't let go.

This is totally a stainless steel structure 316. The duct region itself which is from here to the top of the handling socket is 20 percent cold core material.

Now, a little while ago somebody was asking about the core restraint aspects and do we get core compaction against the core thermal rings to do the swelling. This load pad is located well up above the axial blanket region out of the high fuelant region such that we don't get any swelling in stainless steel at that axial elevation above the core.

The fluences up in this region are low, only 2 times 10^{22}.

MR. SHEWMON: Where are there discontinuities in the length of that then? Where is your fuel?

MR. SCHWALLIE: The fuel would -- okay, the fuel region itself, the core region, is about at this elevation down to here. I've got this cut in two here. But this load pad is about four inches above -- the bottom of it is about four inches above the top of the core itself. So we don't really get any gross deformation of the assembly here, and any deformation of the duct that we take down in this region is accommodated by the diameter difference of the duct and the load path.

The outlet nozzle of the assembly is welded to the duct. It has a load pad, and it has these sloping features on the top so that that feature, along with the transition from the round to the hex here, provides the camming and gearing so that the assemblies as they come in a little bit misalign from the refueling machine, straighten themselves up and slip into the core. And the inside of the nozzle has a ledge feature for the refueling machine to grab a hold of, and then this scalloped region on the top is put in there so that it is small enough such that if you had a refueling accidentand tried to put an assembly down where there was one, it would not go down in and damage the rod bundle, and the scallops would still provide flow access through the assembly.

MR. WARD: How do you get -- you have to have rotational orientation. How do you get --

MR. SCHWALLIE: If you're just a little bit off-line, the assemblies will clear themselves in such that the load paths line up correctly by slipping past these camming features here. And there is a similar feature here in the transition from round to hex.

MR. CARBON: Do you have the same orifice size in each of the fuel assemblies?

MR. SCHWALLIE: No. Both the number of plates and the hole diameters themselves vary from each orifice zone to the next.

MR. CARBON: How do you know? How can you be certain you've got the right orifice?

MR. SCHWALLIE: It is primarily administrative QA control and fabrication.

Now, we do do an air flow test when we fabricate each assembly, and that gives us a final confirmation that we have the right orificing with the right assembly and its identification system, which is a notched system which is read both administratively and by the refueling machine.

So after fabrication we get a confirmation that the right orifice is in there through that air flow test.

MR. CARBON: How sensitive is that test?

MR. SCHWALLIE: On FFTF where we have had three discriminator zones it has been very good. As a matter of fact, we have never had a case where we had the wrong orificing in, but I think the air flow testing is within 15 percent of differentiation of flow rates between different orificing zones itself.

MR. WARD: And then the size of that discriminator post is tied to that?

MR. SCHWALLIE: That's right. That's tied also.

MR. WARD: But down in the reactor internals then you've got something that receives that. Is that changeable, or is that fixed with the reactor?

MR. SCHWALLIE: You can change LIMs, but suppose we go in with just A-1 and we never decide to change it. That always stays, each zone stays permanent, and it is the female matchup of this male insert.

MR. WARD: But if after your first cycle you decide to change the orificing for some design, in your next cycle you have to go in and change those female?

MR. SCHWALLIE: No. If I want to allocate flow a little bit differently from zone to zone, I can change --

MR. WARD: What if you want to make another zone I guess is what I'm asking.

MR. SCHWALLIE: Then you would have to change LIMs.

MR. CARBON: What is the maximum flow difference if you have the worst wrong orifice size? What is the flow difference from the biggest orifices to the smallest orifices?

MR. MARKLEY: Roughly you have about 150,000 pounds per hour in a minimum flow assembly and around 200,000 pounds per hour in a maximum flow assembly -- fuel assemblies.

MR. CARBON: Your delta t is what, 300 degrees or something, so this gives you another hundred degrees, and thus, it doesn't really change things too much?

MR. MARKLEY: That prorating or allocating of flow helps your temperature because you put the higher flows in the higher heat generating assemblies and reduce --

MR. CARBON: But I mean if you made a mistake and the minimum flow orifices were in the hottest assembly.

MR. MARKLEY: Yes. That would give you higher temperatures.

MR. CARBON: But only by about 100 degrees.

MR. DICKSON: I don't think we would want to make a claim that they would survive. I think we would assume that it would run to overtemperature and would fail sometime during life.

MR. WARD: What is the delta t in your air flow tests?

MR. SCHWALLIE: I can't be too definitive on the numbers. I'm not sure exactly of the total pressure drop they have been running on FFTF. The air flow testing and water flow testing correlation has been very good in EBR-2 also.

MR. CARBON: Excuse me a second. Can I ask the Staff if you looked into the -- explored the quality control aspects of this?

MR. STARK: Are you referring to the analysis or the manufacturing itself?

MR. CARBON: Everything connected with it.

MR. STARK: Well, to date what we have been looking at is largely the analysis of the overall QA program. We have been looking at some components that are manufactured, but I don't think that we have looked at any fuel assemblies yet.

MR. CARBON: Have you adopted some sort of position on being satisfied with the procedures that will be followed on that?

MR. KING: We have gotten a commitment that this air flow test will be done as one of the last steps of fabrication. We have not reviewed in detail or in fact I think to any extent the fabrication process itself, the QA administrative controls, that kind of thing, other than that we did get the commitment to do this air flow test. I think we consider that more of an OL-type item than a CP item.

MR. CARBON: Okay.

MR. WARD: That may be true unless you find out at the OL stage that you wished they had more thermocouples in the reactor.

MR. SCHWALLIE: Okay. In the bottom of the assembly just above the inlet region or the orifice plates, and then there's this very bulky region here that provides shielding for the permanent reactor structures, nothing too complicated about this. Then just above that is the initiation of the rod bundle region, and there is a key-way assembly design, a rail attachment assembly design. That actually restrains the fuel rods from any axial movement. And then the tube bundle itself is 270 pins with a wire wrap spacing on it.

Now, the previous vu-graph I showed you just gave you an example of the kinds of quantified design limits that we used for the fuel rods. In terms of the other assembly structures, the ducts, the outlet nozzles and inlet regions and so forth, we not only look at ductile fracture modes and fatigue effects and brittle effects due to irradiation hardening. That really is only a consequence of the duct, because everything from here down and up above is a very low fluence type situation where you don't lose ductility.

But we also worry about the functional aspects of fit, form and function, both in reactor from a core restraint point of view in terms of what that translates into refueling loads, and then the configuration of the assembly in terms of bulging and residual, and transferring the assembly out of reactor. And we worry about that aspect also.

(Slide.)

In terms of how do we compare with FFTF on an assembly basis, we have six discriminating zones in the fuel; FFTF has three. And that is just primarily because of our core arrangement and the larger core size. In terms of lower shielding, we are about the same. They were a little bit more than us because of the requirement at one time to have the closed loops in FFTF for specialized testing.

We have a little bit bigger outside dimension across the load pad, and that is primarily because we need a little more meat for the larger core and the seismic load kilo-carrying capability. The load pad thickness being a little bit thicker gives a little bit more room for the ducts to bulge outward.

We have a little more axial room at the top of our tube bundle between the top of it and the handling socket than FFTF does, and that is just anticipation of a little bit more higher burnup eventually.

We have a fixed load pad at the top. FFTF had a floating color and that was primarily put in back in the days of the evolution of the interation, or the understanding of creep and swelling, the core restraint and pipe calculations. We don't need that and they're not going to have it in their next build. And we have got a little bit more misalignment capability than they have in terms of the refueling grappler mismatch. But that is just to allow for a larger core.

So, all other aspects in terms of the wire wrap lead and the pin pitch to diameter ratio and that kind of thing and the inside -- the cross-dimension of the duct and the wall thickness of the duct and so forth, we are identical to them.

(Slide.)

In terms of our fuel rod, we've talked about most of the features and most of this has been mentioned today, but very simply, it is a hermetically sealed component, welded with tubing and cladding 15 mil wall thickness as it is welded to a top and bottom end cap. The wire wrap is welded at each end. From the bottom end cap you've got 14 inches of blanket material with three foot of actual active core, another 14 inches of axial blankets, a spring to keep everything tight, closed pack during shipping. Just a tube to transmit the forced to the top end caps, and then this tag gas capsule assembly that upon welding or sealing of the pin we rupture the diaphragm and release the xenon-crypton tag gas mixture into the pin and it becomes part of the bond gas, and then each assembly has a unique tag gas composition.

Our fuel is a dished fuel such that when the fuel comes to hot conditions, there is no doming of the fuel to elongate the stack. We have a 91.3 percent theoretical density with about a five to six and a half mil gap. That is a diametral gap between the pellets and the cladding. The blanket pellets above and below are 96 percent density with a 10 mil gap. The 10 mil gap is primarily there to allow for the migration of cesium fission products in the upper region of the fuel column. Everything else, I think, is pretty self-explanatory. The cladding is 20 percent CW 316.

MR. BENDER: Can I ask my question now?

MR. SCHWALLIE: Sure.

MR. BENDER: Do you pre-pressurize the fuel?

MR. SCHWALLIE: No, we don't.

MR. BENDER: There is nothing in there that is under-documented?

MR. SCHWALLIE: Our glove box is welded at atmospheric pressure.

MR. BENDER: And they contain what, a mixture of argonne?

MR. SCHWALLIE: The fuel gas is helium.

MR. BENDER: What happens with burnup to the gap? Does it change with time?

MR. SCHWALLIE: Yes.

MR. BENDER: Can you tell us a little bit about its behavior?

MR. SCHWALLIE: Sure. What happens is in Clinch River, first of all, from a design standpoint, smear density is the controlling parameter in terms of overall fuel performance in a global nature. And smear density as we talked about it this morning is the total void available inside the pin for a swelling combination. What happens is on your ascent to power, the fuel will thermally expand out toward the cladding, but you do not close the gap until you restructure the void deployment that you build into the fuel and create a central void.

Now, you can do that within two to three days of full power operation, and then as you burn up and, say, get the 2 percent burnup, you will have pretty much fully restructured the fuel. You've got about a 20 to 30 mils central void in the center. And the fuel will be in contact with the cladding at that point in time.

And then from that point on, you start getting fuel clad and mechanical interaction until about 5 to 6 percent burnup. The cladding reaches a fluence, then it starts to swell a little bit.

So that is the relieving mechanism plus a radiation creep, which is very good for us in a relieving sense from the secondary stress levels. That off-balances the differential growth between the fuel cladding and time, and we would predict that we would maintain fuel-clad contact over three-quarters of the fuel height throughout lifetime to the end of life from about 2 percent burnup, on.

MR. BENDER: Now, the fission gas pressure builds up in the system, does it not?

MR. SCHWALLIE: That's right.

MR. BENDER: Does it exert enough external pressure or internal pressure to cause the gap to open up?

MR. SCHWALLIE: No. The stainless steel is very stable. We don't have any of the zircalloy type irradiation creep instability problems that you might have in another reactor.

MR. BENDER: And the cladding is strong enough to hold the pressure?

MR. SCHWALLIE: That's right. One of our

design-imposed constraints is that that balance of fission gas pressure under steady state operation, we never want the stress level greater than the proportional elastic limit. That kind of keeps us clean from a lot of considerations.

What we are off-balancing is the stress rupture capability of the cladding in time under that internal pressurization.

MR. BENDER: How about the ratcheting phenomena? Are any of those of concern in this particular fuel?

MR. SCHWALLIE: We haven't seen it in our testing programs. Just one test we did to try to address that about a year and a half ago is we took some pins of varying burnups in EBR-2, cycled the pins from about 100 to 150 percent over power for 54 cycles. And we got -- we couldn't measure the strain difference prior to and after testing.

MR. BENDER: Thank you.

MR. SHEWMON: Staying on those two subjects for a minute, you've never seen any fragmentation of your pellets?

MR. SCHWALLIE: Yes. When we do destructive exam, we crack the pellets very severely.

MR. SHEWMON: And they never get out of whack and out of orientation and give a local strain to the cladding?

MR. SCHWALLIE: The only time we ever -- not an oxide fuel. We have never been able to see localized stress concentrations other than in some very early days in carbide fuel when we had sodium-bonded pins and we had very large fuel clad gaps, totaling an applicable situation to here where you've got a big pellet chip in that annulus and carbide fuel being very, very hard and very, very stiff, did give rise to a breach mechanism of the cladding.

In oxide fuel we have never seen fuel fragments or anything like that, and we have tests where we have known that we have -- in the radiation program we have rogue pellets, so to speak. We speak of an off-normal pellet as a rogue, and what you get is you tend to get a lot of elasticity of the fuel itself. The fuel has pretty good radiation creep characteristics to allow it to hot press, so to speak.

MR. SHEWMON: A different question. You've got a quarter of an inch of stagnant sodium between your sub-assemblies, apparently, that being something like the offset of your subassembly shims up top. Has that caused any sort of problem or extra transient when you change power? Or if the temperature doubles?

MR. SCHWALLIE: We refer to that as the interstitial flow throughout the core region, and we modeled that and we predict its behavior, both where it goes and what its temperature seeks, and what you find is flow is -- first of all, leakage flow is very low, and what you find is wherever it goes it takes on the temperature of the duct structure that is next to you.

MR. SHEWMON: So that is basically stagnant sodium convecting?

MR. SCHWALLIE: It kind of perks its way up very slowly.

MR. SHEWMON: Okay.

(Slide.)

MR. SCHWALLIE: Okay, in terms of rod internals in FFTF -- we have a different plutonium concentration than in FFTF, and our EBR-2 data base is primarily on 25 percent plutonium, and I will come back to that a little bit later. We have a little bit higher pellet density than FFTF. We found that it was a little bit better to put our void in the side of the plenum rather than in the pellet itself, so we translated it to a little bit smaller diameter than FFTF. The combination of the two gives you the same smear density.

We have axial blankets. FFTF only had a couple insulated pellets to make the damage transition from the top fuel pellet to an inconel reflector that they have on the top of their pin, above and below. That is reflected here (indicating). We have a bigger fission gas plenum than FFTF, and of course, our rod is longer.

MR. WARD: The fission gas plenum, does that count as part of the smear void?

MR. SCHWALLIE: No, the smear is totally an area concept, not a volume concept.

(Slide.)

Okay. In terms of the blanket assembly, --

MR. WARD: It is a cross-sectional area?

MR. SCHWALLIE: Yes, a cross-sectional area. Now, in terms of the blanket assembly, externally they look the same. The inlet nozzle is just a little bit different. The orifice plates are in the bottom of the shield region. The primary difference is in the tube bundle itself.

(Slide.)

The blanket rods, there are 61 of them in the assembly. They are 506 in diameter, 15 mil wall cladding. The big feature is a four-inch axial pitch on the wire wrap, and that is primarily due to a desire on the part of mitigating the gradients of cross-radial blanket assemblies. The four-inch wire wrap pitch gives you very good distribution across the assembly so that in the outer rows you try to get the hot and cold flow communicating so the temperature gradient across it from a bowing standpoint is mitigated.

The pin pitch to diameter ratio is very much tighter. This is a 33-mil wire wrap as compared to 56 mils for the fuel. Again, though, just a stack of pellets with a spring and a spacer, and these also have tag gases in them. So that if they fail, we can detect them, also.

MR. EBERSOLE: Does that wire wrap form the actual spatial separation itself?

MR. SCHWALLIE: Yes.

MR. EBERSOLE: Is there any potential for gnawing over the years, or chewing up that 15 mil cover?

MR. SCHWALLIE: Yes, we learned our lessons there. In the early days of EBR-2 when we went to 61-pin bundles we always had a concern of introducing too much bundle duct into action. In other words, where you wanted to make loose bundles so that when we got a lot of swelling we didn't pinch the pins too much.

What we found is we made them too loose and we did get vibration and wear and we chewed some cladding up. The fix is if you stay, as a rule of thumb, below 6 mils per ring of fuel total porosity, you can preclude that, and we have done that in EBR-2 and the bundles have worked very well.

We have looked at bundles out of FFTF; one of the lead assemblies that was in there that has had over 100 days of full flow conditions on it, and it is very clean. And, of course, the French experience has been very good that way, also. So we have got that problem licked, but we did have it at one time.

MR. WARD: You seem to be able to recount French experience on some issues and not on other issues.

MR. SCHWALLIE: Well, I think that comes primarily from in the fuel business we have quite a bit of interaction with them from time to time. Different disciplines interact a little bit differently in terms of the interaction of the base programs.

(Slide.)

I thought I would just show you what a shield assembly looks like and what its functions are. Actually, it is the shield of permanent reactor structures. They are very simple in external appearance, just like a fuel and a blanket. They have for a tube bundle just solid rods, just for shielding purposes, and we double-decked these rods in certain locations, 14 of them on two sides of the core protect some baffle welds.

And then the other function is that we have the center of several of these assemblies dedicated to having a tube on the inside. That contains surveillance specimens to get lead information on ductility and so forth of the permanent reactor structures. So it is basically a shielding and surveillance function, and also, to transmit the forces of core restraint loads out to the core former rings.

(Slide.)

In terms of some design evaluations, in terms of the cladding on the fuel for the two limits that I talked about for steady state operation we've got about a 35 percent margin on the CDF technique, 75 percent on the constrained technique including transients through the unlikely events. We have got 2 percent margin on the ductility limit. This should be CDF and this is ductility-limited strain.

We analyzed the wire wrap through time, and we have two concerns; one is we don't stretch it too much, and the other is that it doesn't get too loose versus time. So we talk in terms of keeping the stress levels pretty low, and also, its ductility-limited strain. And then we don't want any wire slackening such that you get wire movement up and down the pin.

MR. BENDER: Excuse me. Have you had any

experience with wires breaking?

MR. SCHWALLIE: We have never been able to -- there are two cases that I'm aware of. When we took the bundle apart we have broken wire; one was on the fuel and one was on a blanket. But my opinion is that we broke them when we were taking them off because we were trying to strip the duct off the tube bundle. When we -- let me back up a second.

When we take these assemblies out of the reactor, we do six-position neutron radiography of the assemblies, and you can see the outside rows of the assemblies very plainly, or the fuel pins. We go to pull the duct off and it is tight, and we have actually had to slit -- and both of these bundles we had to end up slitting the duct to get it off, and after the fact we found a wire on an outside pin, but we could never see it broken on the radiographs and I think we broke them when we were trying to strip the duct off.

MR. BENDER: If wires did break, what would be their behavior in the flowing sodium? Would they just stay where they are?

MR. SCHWALLIE: I think certainly, after we got into where we had a bundle, a positive bundle interaction effect with the multiple contact -- you have contact with the pin continuously along that wire's length. I wouldn't project that it would move. I think it might move if it broke, and if there was a little pre-stress left in it you might expect it to give a little bit. But it is sort of like a spring; it has a permanent set and as you wrap it I wouldn't expect it to move much. The ones that were broke, they just stayed there; they didn't coil up or anything.

MR. BENDER: There are advance fuels coming down the pike. What is the nature of the things you can see further down the line?

MR. SCHWALLIE: Well, from a fuel lifetime point of view, my biggest concern has been the deformation in that in the three 16 cladding, higher fluences will swell a lot, and the advance alloys, alloys that are lower swelling will certainly mitigate that problem. And that the wire wrap will probably work fine as long as we can keep the overall deformation of the assembly below, say, two wire diameters. That is, the interaction between the bundle and the duct.

We do have fallback positions in the program. We do have derivative assemblies in FFTF right now that could eventually be utilized as an alternate spacing mechanism.

MR. BENDER: Well, I've heard talk of thinner cladding. Is that real or just one part of the passing parade?

MR. SCHWALLIE: I don't think personally it is too real. I think the way to go is we might increase the smear density in the fuel a little bit more, or we find that smear density would go a little lower but thinning the cladding below 15 mils doesn't have a lot of practicality in my opinion, primarily from a fabrication point of view and the defects that you get.

MR. BENDER: Thank you.

MR. WARD: One other question. What are the heat transfer characteristics of that wire wrap? Does it act as a fin, or does it cause a hot spot on the cladding?

MR. SCHWALLIE: What you get is if you can imagine an azimuthal profile of the temperature as you go around the pin and you start under the wire, you will get roughly a 50 to 100 degree, depending upon where you are in the pin and what the heat flux is at that point, you will get a 50 to 100 degree hot spot. Then as you go away from it and you start dropping around, you will get in the middle of the subchannel and so forth. So you do get this cusp type temperature distribution around the pin.

MR. SCHWALLIE: All of the hot spot temperatures that Mr. Markley discussed earlier were under the wire wrap.

(Slide.)

In terms of bundle duct interaction, we don't want to get too loose, we don't want to get too tight. This is the limit that we learned out of EBR-2 that was good to stay away from, from a looseness point of view. And we tried to stay within one wire wrap diameter on bundle tightness.

(Slide.)

I have similar information for the blanket assemblies but I think I will skip it because of the time.

(Slide.)

In terms of the development programs, these are in a global nature the status of the activities that have occurred up to now. Some of these tests were listed by Bob Markley. I list them because I am interested from, not a hydraulic point of view, for example, on these flow and vibration tests, but I'm interested in vibration characteristics of the assemblies. And again, that goes to the amount of porosity you put into the bundle.

All of our out of reactor testing is done from our vantage point. We would consider our steady state irradiation program in EBR-2 on reference fuel to be complete. Of course, we will be getting FFTF data and we have completed the transient testing on TREAT reactor on the other two reference pins.

We have got a lot of information on both the mechanical properties and their dependency with time and irradiation effects, and we have correlations that describe the swelling in reactor deformation and post-irradiation properties of different heats of steel so that we can encompass what our steel is going to behave like.

And we have also got quite a bit of experience now on run beyond breach experience in EBR-2.

MR. WARD: How long -- let's say the cladding breach occurs and we have this system for detecting it. How long is it before you know you have it, and what sort of damage can you do to the fuel pin in the meantime?

MR. SCHWALLIE: There are two aspects. If we have had plenum breaches not in the fuel region itself in EBR-2 our experience has been that they do not result in fuel-sodium contact leakers of any kind. We have logged some sodium in some pins but nothing happens; we get no DND signal release out of them at all. The gas comes out, the covered gas system says yes, we let it run and they are very stable.

The other thing, if we get breaches in the fuel region itself, what we find in oxide fuel is that you generally get a very thin, intergranular crack through the cladding. Now, if you are not in a region where you had clad swelling so that the fuel is still pushing out and it is the driving force, we can run about 20 to 25 days in EBR-2 and we get a DN signal, provided we haven't had a shutdown during that time.

If we have a shutdown, we find that going down and logging sodium and coming back to power with some fuel-sodium reaction we open up the breach and we get a bigger DN signal than we had when we shut down. There are pins in EBR-2 that have not had shutdowns, and we can usually run 25 days with confidence. And we have had a couple of subassemblies that have been in a high swelling regime that have run 96 days. So we are kind of optmistic.

We're also finding that the kinetics of fuel-sodium reaction are pretty slow, and its burnup is dependent upon the amount of free oxygen that is available on the pin surface at that time. But it is not accelerated; it doesn't just happen in a matter of hours. It takes a couple of days.

Okay, I have a similar kind of slide on the blanket development testing.

(Slide.)

Just a couple to point out because blankets were so much different and FFTF didn't have them. We went ahead and did some of this bundle compaction testing to try to determine the stiffness of that bundle and how it might interact from a pinch plane point of view of the wire and pin against the duct.

That kind of testing was done for the fuel in FFTF. We have also done flow and vibration testing on the blanket since it was different. We have done some duct load pad crush tests to see how the bundles react to it.

And we have done some cladding rupture testing because of the larger diameter cladding, and this test here was an irradiated duct out of EBR-2 to try to get -- for a long time people thought that there was no ductility left in the irradiated material and we demonstrated that there was plenty of ductility left to handle any kind of deformation that we got from our seismic loadings.

(Slide.)

The emphasis today in the testing program of the fuels program is to try to link our EBR-2 understanding to FFTF to account for the things that now we have got long pins instead of short pins, we've got different fluence to burnup ratios, different chemistry and so forth. We have three tests in FFTF; two of them are in there now, this one is going in in May that has axial blankets and 33 percent fuel. So that through these three tests we can directly relate our plutonium difference to the 25 percent data base that we have. These two assemblies will also give us information in that they have axial blankets, also.

We have some experience to date -- there was a test done in EBR-2 that had both 30 and 40 percent plutonium in there, and the destructive examination data on that to date says that within the scatter statistical behavior of each of the kinds of phenomenon that are sensitive to plutonium concentration, there is no significant difference.

Again, these tests will address that. And then the reload fuel for FFTF is about 30 percent plutonium, so we're getting a large amount of data to extrapolate off of that.

In terms of this linkability here, two things are going on there. And this is primarily not only steady state. We are reproducing a lot of the fluence to burnup tests that were done in EBR-2 but this is also testing FFTF rods at comparable ramp rates as they are done for EBR-2 rods to see if long fuel columns behave any differently than short fuel columns.

We do have somewhat of a lack of overpower transient data for slow overpowers, and this is below 10 cents per second. I described this test to you. This was a blanket test.

MR. CARBON: Excuse me, Mr. Schwallie, could you wind up rather quickly?

MR. SCHWALLIE: Yes, no problem. This operational reliability testing program was to get from .1 to 10 percent ramp rate data to go along with the 50 cent up to $3.00 per second ramp rate data we have. Two tests were done last week and were taken to 60 percent overpower in EBR-2 and did not fail, so we got a factor of 4 on a 14 percent overpower capability at Clinch River.

This FCCT testing is fuel cladding transient testing; that is, ex-reactor testing of cladding that has been irradiated both next to fuel and not next to fuel so we can get the fuel adjacency effect that Mr. Baars talked about this morning. And we will be doing three tests at 10 cents a second to see how it correlates with the EBR-2 data.

We have an active RBCB program in EBR-2, and we have three blanket tests for FFTF. These two are in the reactor, and this is an instrumented assembly that will be going in about a year or year and a half from now, and we will do a natural circulation simulation on a blanket assembly in FFTF. It is a fully instrumented assembly.

(Slide.)

The next slide just summarizes that testing program. I don't think we need to spend much time on it, but everything is in place to answer the questions and address the concerns that the staff had.

(Slide.)

In terms of my overall conclusions, we do have a design basis that is relatable back to the regulatory guides and the damage severity limits that the core has to survive under. The anlysis and testing that has been done to date shows that there's a very high probability of this core performing acceptably and meeting its goal lifetimes, and that the testing programs are in place to gain the understanding that some people think we might not have.

MR. REMICK: What are you trying to accomplish in the RBCB test in EBR-2?

MR. SCHWALLIE: One of the big things we're trying to do is from an operational point of view, we would like to use the DND system, the delayed neutron detection system, as a diagnostic to give us information on that breach in the core.

What we've found so far is that we can relate DN signal strength to breach size. The amount of exposed fuel that is available to coolant.

MR. REMICK: Are you looking at propagation or is that pretty well settled?

MR. SCHWALLIE: No. I think propagation, as far as I'm concerned, is a dead issue. We are primarily just wanting to get experience on the kinetics of that fuel-sodium reaction and how it translates into diagnostic information.

This also has very important application for the reactor maintenance. We are also trying to see if we actually do put plutonium into the system. That is another positive thing that has come out of the RBCB program in EBR-2; that we have yet to elevate the plutonium level in EBR-2 with all of the tests that we have done. We don't apparently create a maintenance problem with RBCB type operations.

MR. WARD: The reason, I guess, propagation you say is a dead issue -- is that mainly because the failed rod doesn't swell up and interfere with the flow in the rest of the assembly?

MR. SCHWALLIE: That is primarily it. Plus, you tend to -- first of all, the nature of the breaches we get, we don't get rapid gas ejection. If you fail in a plenum region it is generally a micro-fissure inter-granular type thing, and the gas just comes blowing down very slowly. If you fail in the fuel region, it is generally because you've got fuel-cladding interaction and the fuel is plugging up the breach, so to speak, and the gas doesn't come out quick, either.

MR. WARD: How much of the favorable overall characteristics is due to your helical flow? I think you mentioned you have got a backup fuel design that would have not wire wraps but some other type.

MR. SCHWALLIE: Grids.

MR. WARD: Should we be concerned about whether there might be more of a failure of flow reduction in propagation reaction?

MR. SCHWALLIE: I think grids have a little bit more concern to me from a blockage, debris retention, point of view than the wire wrap does.

MR. CARBON: Any other questions?

(No response.)

Let us take a break, then.

(A short recess was taken.)

MR. CARBON: Let's go on with the meeting. I've been requested to announce that if we go beyond 7:00 o'clock we should warn people in the garage that the garage downstairs closes at 7:00. But if we are to stay on schedule, we will be done before 7:00.

MR. CLARE: I will do my very best. The subject I would like to address at this point is what I have called fluid-system interfaces, which is alternatively on the agenda called fluid circuitry interfaces. I am not certain that there is any real difference.

What I will do is to step briefly through each of the major fluid systems in the plant and identify the interfaces that that fluid system has with other fluids, including gases, environments, et cetera, and identify in a general sense -- and I believe that is all that time would allow for -- the kind of approach, the features that we have to assure that whatever the interactions might be at that interface, will be acceptable.

(Slide.)

And the first system that I will address is the primary sodium coolant system. And it has three other fluids that it has interfaces with. And I might note that I will address the argonne cover gas last, so it will not show up until we get to the last viewgraph.

Of course, a principal interface with the primary coolant system is the sodium coolant system, and that interface is through the passive boundary of the intermediate heat exchanger.

Now, one of the principal things we do on that interface is to maintain the pressure of the intermediate heat transport system greater than the pressure of the primary heat transport system, which means that if a leak should develop, any leakage would be from the non-radioactive sodium system into the radioactive sodium system, thereby reducing any consequences in terms of leakage of radioactive material out towards the environment.

We do have leakage detection that will tell us when any significant volume of IHTS sodium has leaked into the primary heat transport system, and there is a very considerable volume beyond the detection capability to accommodate that in terms of an expansion of the volume ofthe PHTS. So that there is no immediate hazard from whatever leakage might occur.

MR. MOELLER: Excuse me, when you say that is a passive boundary, what do you mean by that?

MR. CLARE: I mean it is a tube, a solid steel tube. There are no valves, for example, leakage paths from the original design standpoint. It would have to be a structural failure of some sort in order for the fluids to intermix.

MR. MOELLER: Okay.

MR. CLARE: We do have also on the primary sodium coolant system an interface with Nak, and before I go further, let me talk briefly about Nak. A question that has come up a couple of times in prior meetings. Nak is a eutectic mixture of sodium and potassium.

(Slide.)

22 percent sodium, 78 percent potassium by weight. The melting temperature is 9 degrees Fahrenheit, the boiling temperature is just a bit below the boiling temperature of sodium. The Nak boiling temperature being 18 degrees F. Any mixing of sodium and Nak would not result in a chemical reaction, would not result in an adverse effect on the process equipment.

There would be an increase in the Nak melting temperature as we moved away from the eutectic point. Similarly, there would be a decrease in the sodium melting temperature as we add Nak.

Now, the fact of the matter is that when we look at our systems there is very, very little Nak compared to the sodium systems that it interfaces with. So there would, in fact, be very little effect in terms of a decrease in the sodium melting temperature. There might be some increase in Nak melting temperature, but even that, within the volume that could be accommodated, would be essentially insignificant.

MR. SHEWMON: Why did you use Nak?

MR. CLARE: We used Nak principally because of this temperature.

MR. EBERSOLE: I figured you did. Now, when you mix sodium with it, that temperature is going to go up, isn't it?

MR. CLARE: That is correct.

MR. EBERSOLE: Will that cause some problems in the instrumentation?

MR. CLARE: No, it would not. The statement about adverse effect on process equipment is essentially true. Now, if there were large amounts -- let's assume I approach pure sodium. That theoretically could become a problem. However, one would detect any leakage before any significant percentage increase in the sodium content, and you might increase this to 20 degrees Fahrenheit, but there would be no significant difference.

MR. SHEWMON: The Nak is what you cool your cold trap with? Is that right?

MR. CLARE: The Nak is what we cool our cold trap with. It is also the secondary coolant in what we call our direct heat removal service. The interface there is in a heat exchanger; shell and tube heat exchanger. We call it the overflow heat exchanger.

MR. MOELLER What is the sodium melting temperature?

MR. CLARE: For pure sodium, I believe that's 218 degrees.

MR. REMICK: What are the limitations on the amount of Nak you can get into sodium and still operate? Is it an activation problem? Primarily, a long-term activation problem?

MR. CLARE: Getting Nak into sodium would result in some activation products we would not otherwise expect in any significant quantity. Is that a problem? That would be a slight operational problem and it wouldn't even be a very significant one from that standpoint. So it is not even clear that for the small amount of Nak that one might expect to leak that it is a problem at all.

MR. REMICK: So there is no limitation on the leakage of Nak in the sodium?

MR. CLARE: Well again, we will detect it, and we can detect it by detecting the levels in the expansion tanks on the Nak system. And we would expect to detect very few gallons going into a million gallons of primary sodium.

MR. SHEWMON: Is there any straightforward way you could get sodium out of Nak, or I'm sorry, sodium out of the Nak, or potassium out of sodium?

MR. DICKSON: Distillation is the only way. You might have noticed that the Nak systems are all at higher pressure, so you would almost never expect sodium to leak into the Nak, raising the boiling point. Any inteface would leak Nak in, and the quantity of primary sodium is so vast compared with the quantity of Nak that you would never get into any activation problem that you would notice in consideration of the design basis amount of fission products that you assume is in the sodium.

MR. BENDER: Does the potassium influence the corrosion characteristics of the sodium at elevated temperatures at all?

MR. CLARE: To my knowledge, it does not. There have been successful operations of Nak-cooled reactors before, with no specific problems.

MR. BENDER: I know, but I wasn't sure what the temperature was.

MR. CLARE: I don't know the exact temperature, but to my knowledge, there are no such effects.

MR. SHEWMON: I would be willing to bet you even more money than a dime that it's just about like sodium, and once you get the oxygen out, why, you're in good shape.

MR. BENDER: You're a good enough authority for me.

MR. CLARE: Now, the third interface that I have identified here is that of the nitrogen environment in the RCB cells, and all of our p imary sodium, piping and components are located in inerted cells within the reactor containment building. And, of course, the piping and the components provide a passive boundary to those cells.

There is sodium leakage detection in each of the cells to tell us should there even be a tiny leak of sodium, and in addition, the inerted environment in the cell, along with the liner, a carbon steel liner which completely surrounds the cell or completely lines the cell I should say, completely surrounds the primary heat transport system equipment, avoids any sodium-concrete reaction.

This type of provision I assume we will discuss in detail next month when we discuss the containment philosophy.

Now, the other point I have identified for this interface has to do with the separation of any leaked sodium in that cell from any cooling water that is separated by yet another passive boundary. And just to note quickly without going into detail that we have the additional protection of being able to detect, isolate and drain away any water that might have leaked from the water cooler for that particular cell for the remote case that we would even have any sodium leakage into the cell.

(Slide.)

Now, the second fluid system that I will address is the intermediate sodium cooling system. Now again, following out the heat transport system, the principal interface is with our steam water system, through the passive boundary of the steam generator modules. I will discuss at length in my next presentation the features we have for leak detection and leakage accommodations which include rupture discs, reaction production separation and collection system, and a dump system and safety relief systems to relieve the water on the water side.

Now, some of the intermediate sodium piping runs in the same inerted cells in the containment people as the primary heat transport system, and we have the identical protection against any leakage into that cell for the intermediate that we do for the primary.

In addition, a major portion of the intermediate sodium equipment is located in an air environment in the steam generator building cells. Now, the key difference between these cells and these cells (indicating) is that the sodium out in the air environment is not radioactive, and therefore, there is no particular need to absolutely prevent the release of any sodium-sodium reaction products from those cells.

The equipment, of course, provides a passive boundary, and in the intermediate building we also provide very sensitive leak detection. As leakage accommodation, we have catch pans in the bottom of the cells to collect any leakage and protect any concrete below the equipment. On some of the catch pans where there would be the greatest accumulation of sodium, we have fire suppression decks which are -- think of them as a sheet metal cover over the catch pan, which will act, by reducing the amount of air access to the sodium, to reduce both the amount of burning and the duration of burning for the pool fire that would result from the collection of sodium in the catch pan.

Another important feature we have is loop separation. Each of the three heat transport loops is located in a completely environmentally separated set of cells from the other heat transport loops. So a sodium fire that results from leakage in one loop would not carry over in any direct way and affect the other loop.

However, we do relieve the pressure in any one of those cells which can release aerosols from the building, and of course, depending upon meteorological conditions, there could be some aerosol carryover into some other portion of the building. And we have qualified our equipment, we have specified that the equipment will be qualified as necessary to operate in that sodium aerosol environment.

Now, another sodium coolant system that we have in the plant is the sodium system that cools the ex-vessel storage tank, and that is our version of a spent fuel pool that you would have in a light water plant. It is a tank on the outside. It appears much like our reactor vessel, and in it we store any spent fuel which has been discharged from the reactor.

Because we are dealing with radioactive sodium in this case, we will bring radioactive sodium into this vessel as a result of refueling. We contain that sodium system in the same type of lined inerted cells that we have for the primary coolant system, and all of the provisions are identical.

MR. WARD: Why don't you use Nak in the ex-vessel storage tank? I should think there might be some advantage to that in freezing temperatures.

MR. CLARE: One could, but the way we transfer both new and spent fuel is we carry the fuel assembly in a little pot of sodium, and since we're going to be interchanging it with the reactor, it makes more sense to keep this sodium.

Now, the EVST sodium coolant system is itself cooled by three Nak coolant system, and these Nak coolant systems have the same sorts of protection from the radioactive system as does the intermediate heat transport system from the primary heat transport system. Pressures are maintained so leakage will be towards the radioactive source.

Leakage detection, leakage accommodation -- the Nak itself is located in a combination of nitrogen environment and air environment, and when it's in a nitrogen environment, it has the same sort of protection as the sodium does; when it's in an air environment it has essentially the same protection as the intermediate heat transport system sodium.

MR. AXTMANN: On the previous slide you listed an aerosol mitigation system. Could you describe that in a sentence or two?

(Slide.)

MR. CLARE: The aerosol mitigation system per se is merely a system which we provide while providing some cell pressure relief and we don't design the building as a high pressure containment sort of building so we do have to vent the building. At the same time, we want to limit the amount of venting and limit the amount of aerosol that may be released to the environment which can then carry over into other parts of the plant.

So, it principally will consist of some combinations of louvers, dampers, et cetera, which can be closed at some appropriate time after the pressure has been relieved.

MR. EBERSOLE: But your rupture discs, they discharge into some space, which I take it is of limited volume.

MR. CLARE: Yes, they discharge into the reaction product separation tank, and I will be covering that in another schematic later.

MR. MOELLER: On the right hand, what did RCB stand for?

MR. CLARE: Reactor containment building.

MR. MOELLER: Thank you.

(Slide.)

MR. CLARE: The final point I will cover is relative to the argonne cover gas system, and all of our liquid-metal systems are capped with their free surfaces exposed to an argonne atmosphere. So we have an argonne cover gas on the primary and the intermediate system.

The EVST sodium coolant system I have left off for purposes of space. The EVST Nak coolant system. But it would fall into the same situation.

We also have argonne in our fuels handling cell, and this is because in the fuel handling cell we would be handling spent fuel assemblies which are essentially covered with a film of sodium. So we want to maintain that as an inert environment.

There, of course, is a direct interface with the coolant, the liquid-metal coolant, in every case. And, of course, the pressure is the same in the cover gas as it is in the sodium. Most of the systems are at about one atmosphere.

The key difference being that in the intermediate sodium coolant system you will recall I said we wanted to keep the intermediate pressure higher than the primary pressure in the intermediate heat exchanger, and we do that by pressurizing the cover gas. So it is significantly greater than one atmosphere. In fact, our nominal setpoint for that would be 93 psi.

In essentially all cases, we monitor the purity of the argonne cover gas, principally from the standpoint of radioactivity. Also, we look at such things as oxygen content, water vapor. We can sample the cover gas, an then we have various means of processing the cover gas, depending on what it might be postulated to contain.

In the case of the primary system, as we discussed a few minutes ago, there could be leaking in fuel pins which would result in fission gas bubbling up through the sodium and entering the argonne cover gas. Therefore, we have a radioactive argonne processing system that will remove those fission gases. And that is by use of a cryogenic still.

MR. REMICK: Question. What do you do with the gases after you take them out through the cryogenic still? Do you release them to atmosphere, or are you going to bottle them up and store them in perpetuity?

MR. CLARE: We bottle them up and store them. but not for perpetuity. What we do is we accumulate them for about a period of a year. After a year, we drain the still bottle into what we call the noble gas storage vessel. Throughout the following year, we release that radioactive gas through another rad waste system, which we call CAPS, the cell atmosphere processing system. That system contains cryogenic charcoal debris beds which will provide some additional holdup, and then they are vented to atmosphere.

MR. REMICK: And what comes out the venting?

Is it mostly cesium?

MR. CLARE: No, these are noble gases. It would be crypton and xenon.

MR. REMICK: What is the longest term of the half life?

MR. CLARE: I believe the xenon is the longest-lived isotope or has the longest lived isotope, but I would have to check the numbers to be sure.

MR. REMICK: Well, am I correct cesium comes out of the cover gas also, or am I incorrect in that?

MR. CLARE: Cesium will not come out of the cover gas. The cesium will stay with the sodium and it will be plated out. Most of it, we would assume, is going to be plated out in our cold trap, or at various cold surfaces in the system. We would not expect that with the cover gas.

I mentioned the cold trap briefly before, and it is just a situation that -- it is a component where we cool the sodium down and trap out any impurities that might be in it.

Note that for the intermediate sodium system, the cover gas, the argonne, is non-radioactive, so that is simply a once-through system. We shove the argonne in, and if we need to vent it we just vent it to the environment.

The EVST coolant system -- we do monitor that system but by and large, that is a once-through system. We vent it to the environment through the cryogenic charcoal beds of CAPS that I mentioned a few minutes ago.

The fuel handling cell is a rather large cell, and it has a lot of penetrations. So there is some potential for ingressive oxygen and water, and we have a special atmosphere purification unit that cryogenically removes oxygen and water from the argonne atmosphere there.

MR. WARD: The argonne doesn't get exposed to neutrons anywhere? You don't get any argonne 41?

MR. CLARE: We do get a small amount of argonne 41, but it is not a large amount. And to the extent that it exists, it will just stick with the argonne itself. It just stays in the system.

MR. REMICK: Do you happen to know what the gaseous effluent from CRBR would be compared with a current BWR?

MR. CLARE: I know that the doses are very much smaller, principally because our iodine will hold up in our cold trap just like the cesium. It is very tightly tied to the sodium, and also, tritium releases are very low for this plant. So from an overall radiological standpoint, this plant has a much lower normal off-site dose than a light water plant. I can't give you a quantification of that.

That, then, completes the overview of the fluid systems interfaces.

MR. CARBON: Any further questions?

(No response.)

Thank you, Mr. Clare. The next topic on plant materials, none of the people scheduled to speak are here, so we will completely drop it and move on then to the final topic, the steam generator accidents and consequences. And I understand Mr. Stark will speak for Mr. Beckner.

MR. STARK: This is Richard Stark again. Unfortunately, Dick Beckner couldn't make it down in the snow. I, nevertheless, met with Dick yesterday when we had a kind of dry run, and I'm going to attempt to summarize what I believe are the bottom line items of the staff findings.

I would like to start off by saying first of all, I want to compare the safety function of the Clinch River Breeder Reactor steam generators to those of a light water plant. A PWR steam generator typically provides three safety functions. One is decay heat removal, one is it plays a significant role in the steam line break accident, and the third one is if you do have an accident that involves the steam generator, usually you have radioactive isotopes, so you have a radioactive penalty to pay with it.

Looking at each one of these three items, the Clinch River Breeder Reactor steam generator is used for decay heat removal, so in that respect it is similar to a light water plant. The second item on the steam break accident -- it is not similar. The steam line break accident in this particular plant is just a very minor accident. As a matter of fact, it's an extremely small accident for two principal reasons.

One is this particular reactor has a very small negative temperature coefficient as far as reactivity is concerned, and that alone would do it. Another item is with the intermediate loop and the primary loop and the steam generator being the third loop, the loop times along, there is well over a 100-second delay time from the time you have this rapid cooldown until it is sensed in the core.

The rods or a scram -- the PBS system would scram the reactor in three to four seconds based upon a steam flow mismatch, so for several reasons this is not a significant concern.

The last item I was talking about the radioactive consequences. Aside from a small amount of tritiu , the intermediate loop is non-radioactive. So therefore, any leak that would go from the water side into the sodium side, while it would be a violent chemical action, is essentially non -- has no radioactive consequences.

MR. MOELLER: You mentioned the tritium. Does the tritium permeate?

MR. STARK: Through the I jets, that is correct. The staff nevertheless looked at some accidents and we analyzed small leaks and large leaks, and the bottom line on small leaks is they are picked up by hydrogen and oxygen monitors in the intermediate loop and the consequences are insignificant.

The large leaks -- there is a rupture disc into a steam to water reaction product system; I think it is called SWRPS or something, and that handles and deals with the reaction product.

It appears from what we have looked at that the pressure pulse is such that the intermediate loop is not challenged. There are rupture discs on the evaporators and the superheater, and it looks like there is also capability in the expansion ta. There is a lot of buffers there and a lot of relief mechanisms that will protect the IHX, which is the primary boundary between the intermediate loop and the primary loop.

The IHX design pressure is on the order of 700 psi. All of these rupture discs rupture in the order of 300 to 325 psi, I believe.

In addition to that, it locks -- this is another matter, but we have looked at the test plan and it is a pretty good test plan. The steam generators in the past have had not a very good history, but it looks like the applicant is certainly trying to test it and trying to get a lot of test history.

In addition to that, we do have an in-service inspection program that is tied to this, monitoring the steam generator. And there are a few other design features which I'm sure the applicant will tell you about.

In a PWR the water -- the steam comes in on the shell side and evaporates on the shell side. All of the good steam is boiled off and anything that precipitates out falls down to the tube sheet, and therefore, it collects all of the precipitates. I guess they are all bad.

This particular steam generator is different in that the water flows through the tubes, and the sodium goes in the shell side. And all the separation tends to take place in another tank, which is the steam drum. So, therefore, the steam generator should be spared from that particular item.

But in general, we think that from a safety standpoint it looks as though the accidents involving the steam generator have acceptable consequences. It looks as though the applicant is trying to make a reliable steam generator just from a commercial operation standpoint, looking at the history that PWR has. And we find from a safety standpoint that it is acceptable, and I hope from a commercial standpoint that he has good success.

MR. SHEWMON: What is the allowable leak rate, or what happens if a leak does come in one steam generator?

MR. STARK: That is the small break accident. As I said, there are some sensors. Unfortunately, I don't have all of the details, but the Applicant senses for the presence of hydrogen and oxygen in the intermediate loop and then takes the appropriate action, shuts the plant down and fixes that particular leak. He doesn't continue it. And I guess he will address this in greater detail.

As I said, unfortunately our reviewer is not here, and I'm trying to just give you the bottom line.

MR. EBERSOLE: In a bad failure what keeps the failure from progressing to a failure when you might generate a fire in the vicinity of the first failure?

MR. STARK: I'm not sure I can answer that precisely, but we have looked at accidents that have happened and have gone back to Fermi, and looking at the whole history of leaks that we have seen. The Applicant proposes three complete breaks in tubes, one followed by another and then followed by another one. And at least from an experience standpoint we feel that this envelopes all of the experience -- more than envelopes.

What has been experienced in the past -- and I am going to have to ask the Applicant to address that -- the real reason.

MR. EBERSOLE: It looks like it ought to be autocatalytic and could just progress to something very bad indeed. I mean having broken one and now two.

MR. STARK: It doesn't, but I don't know why.

MR. MOELLER: Recently I saw a news release or something of a leak in the French Phenix. Was that in a steam generator?

MR. STARK: Yes, it was. I think it happened in December.

MR. MOELLER: You've looked at that, or will someone tell us why we won't have one here?

MR. STARK: The Applicant's going to do a better job than I have. What we have looked at, the steam generators have had a lot of these little problems; and as I indicated, from a reliability standpoint it is something we are trying to look at and make sure it is factored in. It has no safety implications. And Phenix fixed it, and they are back up again and running, and they were down for just a short period of time, a couple of weeks, I imagine.

I know people are looking at steam generators both from a safety standpoint and from a commercial reliability standpoint. From a safety standpoint we feel very good, and from a reliability standpoint it appears that the Applicant is really trying to test this particular steam generator as much as he can; and I certainly hope he is successful in it because it will help to demonstrate a good plant later on from a commercial standpoint.

MR. CARBON: Just as a point of information, have you looked at the several leaks that have taken place in the PFR steam generator from the safety standpoint?

MR. STARK: We have been aware of them, and we have looked at the consequences, and we have looked at the impact on Clinch River; and as I have indicated, we don't see a safety problem.

MR. CARBON: Any further questions?

MR. STARK: That is all we have, and what I would like to do is I would like to excuse ourselves since we are all Washington-based now, and try to get home.

MR. SHEWMON: One more question. If I wanted to find a description of the neutronic safety-related control system, where would I find it?

MR. DICKSON: Chapter 15 of the PSAR.

MR. STARK: He's talking about neutronic control.

MR. DICKSON: Related to this accident?

MR. SHEWMON: Related to neutronics.

MR. DICKSON: Chapter 7.

MR. SHEWMON: Chapter 7 doesn't do much, and it refers me to 3.2, and 3.2 is a misprint. So maybe before you quit you can show me where in 7.

MR. CLARE: We can try to do that. The control would be divided into two sections. In Chapter 7 you will find a discussion of the instrumentation of control equipment itself, wires, transistors and those kinds of things. In Chapter 4, 4.2.3, you will find the discussion of the physical control rod and those kinds of things.

MR. CARBON: Does anyone have any other questions to address to the Staff?

(No response.)

MR. STARK: Thank you.

MR. CLARE: I hope I can deliver on all the promises that I and others have made for what this presentation will contain.

The subject is steam generator leaks.

(Slide.)

What do they look like, and when they happen how do we accommodate them.

To begin with, I thought we could look just conceptually at what are the kinds of problems that one might get into as a result of steam generator tube leaks. And Mr. Stark touched on them briefly.

The first is that if you do have a steam generator leak, you would be in a situation where you were shutting down the reactor with less shutdown heat removal capacity than you might otherwise have.

The second is that because of the vigorous reaction between water and sodium, mechanical loadings on the primary and intermediate coolant boundaries could be generated, and specifically, the intermediate heat exchanger, the separation between the two boundaries is of most interest. And then as a by-product of the chemical reaction, hydrogen is generated, so that reaction product needs to be dealt with in order to avoid any adverse consequences.

Now, to address the first of these potential indirect effects just simply and directly, we have a multiple heat transport system, heat removal pads, and operator flexibility to isolate, repair or replace a leaking steam generator, as well as the direct heat removal service which is a heat removal path from the reactor which is totally independent of the steam generators, all of which serves to mitigate the effects of any steam generator tube leak on shutdown heat removal system capacity.

Now, I will talk in a little more detail about the latter two effects, and I will note for your reference that this was discussed in somewhat more detail by Paul Dickson in a June 25, 1982 meeting of the CRBRP subcommittee.

(Slide.)

Now, we have three levels of protection provided against the effects of steam generator tube leaks. The first of these is leak detection from which the operator can detect a leak and manually shut down the reactor.

(Slide.)

And what I've drawn over on this other vu-graph is a part of our heat transport system, a part of one of our heat transport loops. What we have is an intermediate heat transport system, sodium, that comes into the superheater and comes down through the superheater, and then there are actually two evaporators. I've only shown one to keep things simplified on the drawing.

The sodium goes through the evaporator and back to the intermediate heat exchanger. We have leak detectors on the piping, both exiting the superheater and exiting the evaporator. In addition, we have vents at the top of each of these units that vents a small flow of sodium from the unit, collecting any gas accumulation that might happen at the top of the heat exchanger during normal operation, and we monitor those areas. And what we do is to look for either oxygen or hydrogen which would be the products of any sodium-water reaction, and take the appropriate operator reaction in response to it.

Now, there was a question -- and I think Dr. Shewmon asked about it -- about what would be an acceptable leak. There are three sorts of alarms, if you will, that will tell the operator to do different things. And I might note that these are preliminary kind of procedure thoughts that we have at this point in time.

The first alarm that we get in the system will come at a level of about 2 times 10^{-5} pounds of water per second -- a very, very small leakage rate. That is not even a pinhole. At that point the operator will not shut down the reactor. He will proceed to try to understand what is going on using his various leak detectors and try to be sure he knows exactly where the leak is.

MR. SHEWMON: That was 10^{-5} gallons per minute?

MR. CLARE: 10^{-5} pounds per second.

MR. SHEWMON: I will be glad when SI units come to this field, but go ahead.

(Laughter.)

MR. CLARE: 10^{-2} grams per second. How's that?

MR. MOELLER: Now, you say you will pinpoint where the leak is. How does he do that?

MR. CLARE: He will not pinpoint it in terms of a particular tube or even where within a unit, but he will make certain that he knows which module it is in by looking at which detector is giving him the strongest signal.

Now, at a leakage rate between 10^{-2} and 10 -- and I'm sorry, it is pounds per second, and I don't have a quick conversion to grams -- the operator will begin to take this leak very seriously, and once he has confirmed the leak, he will prepare to and go ahead and shut down the reactor. So that would be at about the limit at which we would operate the plant without immediately scramming.

MR. SHEWMON: You have done enough work so that you feel that the growth of the void is still slow up beyond that limit, or how do you pick that as an action point?

MR. DICKSON: I want to add, one of the problems with experience with little leaks is when you shut down and close up, you can't find them at all. So it isn't a matter of operating them and saying it's no problem. Below this you know you can't detect it, so all you do is shut down and then come back up again. So you have to wait until it has gotten large enough to do something.

MR. SHEWMON: But this is a singularly unspecific detection system, and all it does is to say there's a hole in the steam generator some place. And you can then go back and do what?

MR. DICKSON: Which is why you wait until the leak is large enough to be able to detect it, after you have taken the water out and can go inside and inspect it.

MR. SHEWMON: Do you detect with an eddy current?

MR. DICKSON: Eddy current and pressure. Both are means of detecting the leak.

MR. SHEWMON: So you're looking through 10,000 tubes with a probe to try to find something, is that it?

MR. CLARE: We are pressurizing the tubes, yes. And that is why we at the very least want to know what module it is in. We wouldn't want to go through all three of them in a particular loop.

Now, the second level of protection that we have is an expansion tank rupture disc on each loop with automatic water dump.

Now, what this is is a rupture disc which looks at the cover gas pressure in the expansion tank, and just a few minutes ago I mentioned a 93 psi cover gas, and that is what is contained in that tank, and the tank level is used as a way to control volume as the sodium heats up and expands and cools down and contracts.

If the pressure in the system reaches 150 psi, a good 50 psi greater than the normal pressure here, this rupture disc will burst, and it merely vents gas to something called a sodium dump tank which I haven't even shown here, and it's just a big, empty tank filled with nitrogen which will then come back and equalize with the argon.

At that point there will be a signal which automatically dum s the water in the evaporators into the evapora dump tank, which again is just an empty tank which is there to receive the water from the evaporator, and that relieves the driving pressure to push the water into the sodium, and reduces the reaction to a lower level, and would in the long run, if the operator didn't ramp the plant down, very quickly result in a scram. So, in effect, the plant will scram in the event this rupture disc bursts from a slow buildup of the pressure in the system to about 150 psi.

And we would expect that to occur relatively quickly if you were to get a leak of the size of approaching a pound per second, to give you a feeling for what kind of a leak that is.

Now, the third level of protection is what we call the main rupture discs, the result of the bursting of which is that we have an automatic reactor shutdown tied into our plant protection system, and also the same kind of water dump I've talked about before.

What those rupture discs are are pairs of reverse buckling rupture discs at the outlet -- excuse me -- at the inlet of the superheater and at the outlet of each of the evaporators. And this is actually not a properly drawn figure, because the rupture disc is actually on the side of the T that looks directly into the shell side of the steam generator module, so the pressure wave will come out through the sodium, directly strike the rupture disc, and then the reaction products from the reaction are vented into this reaction product separator tank.

The gaseous reaction products then are separated and just taken out of the steam generator building through a vent. There is a small rupture disc with a low set point, a few psi, on that line which merely protects the inert environment that we maintain in this tank prior to any accident occurring.

Again, should any one of these three sets of rupture discs on any loop be ruptured, there would be an automatic plant scram, an automatic dump of the water side in that particular loop.

This collection of equipment that I have just discussed is what we call the sodium-water reaction pressure relief system or SWRPRS, sometimes abbreviated SWRPS.

(Slide.)

Now, the important question then is how big do you have to make the system; what is the phenomenology of the leak; how do you evaluate whether or not you've got the right system. And what we do is to define a design basis accident for the SWRPRS which serves also as the design basis accident for the primary and intermediate coolant boundaries in the mechanical loading. And we suggested that using engineering judgment, considering reactor experience, which is admittedly limited, a very extensive experimental data base, and some analysis results. And I will run through some of that with you, summarize it briefly. And again, I would refer you to the June 25th transcript for more detail.

The important parameters of the design basis accident is the size of the leak, how much water is leaking per second into the sodium, and that tells you how much energy will be given off; and then, of course, if there is more than one tube leaking, how many of them leak; and then perhaps a not so obvious parameter is the timing. And the reason that the timing is important is because only extremely rapid events and the propagation in terms of enlarging the water flow rate is important, only that very short propagation is important because of the rapid pressure relief.

Now, let me give you a feeling for that. If I were to get a complete double end rupture of one tube, the pressure pulse would arrive at the rupture disc, and the rupture disc would be burst. To the extent that the pressure is relieved down so that it stays around the 300 to 400 psi range in a matter of tens of milliseconds; so anything that occurs beyond seconds is clearly out of the range of interest. So let's take a look at the phenomenology.

What I have tried to draw here is a cross-section of a steam generator tube. And as a bit of a reminder, each steam generator tube is 5/8 of an inch in diameter, the wall is about 109 mills as fabricated. We have allowances for degradation over life, and what we count on is 77 -- there should be a zero in there -- 77 mills of wall thickness at end of life.

Now, the types of failures that one would expect to occur will be a small pinhole type failure that originally occurs, and the smallest people have measured any particular leak is down in the order of just where the sensitivity of a leak detection system begins, this 10^{-5} pounds per second.

At that point we would expect plugging to be experienced. We would see a leak for a little while, but then the reaction products and the sodium and what not would plug up that crack, and it would stop leaking, and we might see it again. However, after a while there would be some reaction products, a reaction wastage erosion, et cetera, and it would begin to eat away at that leak site until at some point we had a fairly large area of erosion around the leak site.

Now, the process to come from this point down to this point would be on the order of hours to days to months, and it is uncertain to that extent how long it would take. It depends upon the configuration of the original leak and what the conditions happen to be. For example, the conditions in a superheater where you have less dense water coming through your leak is -- well, I should say steam leaking through the leak -- is less of a severe situation than where you actually have subcooled water coming through if it were an evaporator.

Now, what happens when you get sufficient degradation in the area of the tube is you get a stress rupture when that wall gets so thin that it no longer can contain the say 1500 psi that it sees on the inside of the tube, 1500 psi or greater, and then what you get is a blowout of the bottom of that crater. And from experiments we know that that is on the order of 50 mills through the orifice with the degraded area being some three times that in diameter.

A leak of that size in the evaporator where the subcooled water is inside the tube would be on the order of 15 grams per second. Now, that is what we call a precursor failure. That is not an event which is large enough to lead to an immediate burst of a rupture disc. It would gradually begin to pressurize the system as we introduced hydrogen.

(Slide.)

However, this is the kind of thing where, as Mr. Ebersole pointed out, we would have a reaction near the adjacent tubes. So what kinds of mechanisms then are there that could cause tube-to-tube failure propagation.

We believe there are three important ones to be considered. Wastage and corrosion of the tube, that breaks down the material or takes it away. Now, we have done numerous experiments in this country and abroad on that question, and indeed wastage and corrosion will take place. Wastage we find to be most rapid, and it will occur on the order of 1 to 5 mills per second maximum. And if you think about our 100 mill wall thickness, it takes tens of seconds to do significant degradation to that tube, and that then gets beyond the range of consideration where it's important for the sizing of our reaction product system.

The third mechanism is the one we think is most important to look at, and it is stress rupture. And when we say stress rupture what we really mean is we're going to overheat a tube to the point at which its material property is degraded so that it can no longer contain the stress, the pressure internal to the tube, and it would rupture in a tensile sort of way.

Experimentally we have observed that stress ruptures on the order of 10 seconds -- actually I believe the shortest has actually been 16 seconds -- after the precursor leak has begun. We have done a bounding analysis, and in the bounding analysis we assumed we had a piece of the tube wall. We assumed it was an adiabactic piece of steel, no heat being removed from it, and we put it in instantaneous continuous flame on one side of it. That was at the temperature that one would get if one had a stochastic mixture of water and sodium. And that temperature is 2700 degrees Farenheit. And given the thickness of the steel, it would take on the order of 1 second to heat that wall up to the point where it would no longer contain the water pressure.

So the experiment says this is indeed a bounding analysis in the sense -- and it is much shorter by an order of magnitude compared to anything we have seen in real life.

Now, I also mentioned that size was important, and what we have found from experiment -- and it made sense -- is that stress rupture failures are limited in size, and when we get one of these failures it is a splitting of the tube. The tube will split open, a fish-mouth, if you will, from the overheating. Typically we will see a 45 degree opening in the tube. And perhaps the most important thing is that the extent of that in an axial direction along the tube is only about an inch and a half at maximum. And this is the part that makes sense logically.

The reaction zone which is heating the tube up is only present in one particular spot at one particular point in time, and indeed, the material is cooler when you get away from it. It is cooler not only because the flame front isn't there; it is being continuously cooled by sodium flowing up around the tube, at least until the bubble pushes the sodium away, and it's also being cooled by the water of the steam on the inside.

In any case, we have never experienced in any of our tests a leak greater than the equivalent of 50 percent of a double-ended guillotine rupture. The water flow from the tube has never been greater than 50 percent.

(Slide.)

So if we just put a picture up of what I've just talked about --

MR. MOELLER: Excuse me. It's never been greater than 50 percent? What is typical? I mean has it --

MR. CLARE: Typical is more between 10 and 30 percent.

MR. MOELLER: Thank you.

MR. CLARE: Although there haven't been that many, I should point out.

The kind of situation then we're talking about is where we have this precursor failure where we have hundreds or tenths of a pound per second of water impinging with its reaction zone on some adjacent tube, a relatively small reaction. Then if we were to get one of these stress ruptures, we would relieve a much greater amount of water into the sodium, and we would get a large reaction zone around that failure.

It would be a very dynamic environment. We have flowing sodium on the outside. We have the reaction zone. There would not be a stable situation. However, we have said let's assume that before we can vent the water down, and let's assume before the bubble pushes all the sodium away, that the reaction indeed overheats some other tube -- and let me pick this one over here -- and this one fails, and we call that a secondary failure. And then we could continue going from there.

(Slide.)

However, we again drop back --

MR. CARBON: Would you leave that up just a second? I'm not sure of your sequence.

(Slide.)

Is P your precursor failure?

MR. CLARE: Yes.

MR. CARBON: And then you have a primary failure on a different tube? I'm not sure what you're saying.

MR. CLARE: That is correct. The water leaking out of this tube creates a reaction which overheats the adjacent tube. That adjacent tube sustains the stress rupture that I talked about, and then it creates a larger reaction. Then if one goes ahead and postulates the additional failure of another tube, then one would get a secondary failure.

MR. CARBON: But the primary tube which had the leak in the first place isn't the one that failed?

MR. CLARE: Well, it has the original pinhole type leak. If you were to consider a weld defect, for example, where there was a small leak or some other material, a stochastic failure type of thing, this is the precursor.

MR. DICKSON: It failed up to the point of the blowout that he described. That raised the leak rate up to the 15 grams per second.

MR. CARBON: And then you assume from there on a bigger failure takes place?

MR. DICKSON: Yes. By virtue of the flame front on it heating.

(Slide.)

MR. CLARE: This is the precursor here, a 50 mill hole roughly.

MR. CARBON: But then you get a bigger failure in F_t.

MR. CLARE: Yes. This is the tens of a percent of a double-ended rupture typically.

MR. MOELLER: Now, if it fails, if it causes a secondary failure two or three tubes away, why doesn't it just cause a propagation of failures in a whole cluster of tubes?

MR. CLARE: In a theoretical sense it can do that, and the question then comes down to a sense of timing and what one actually experiences in life. And indeed, because there was a question about that, there has been a vast experimental program, not necessarily a well-integrated one, however, in the past.

MR. DICKSON: Could I add something to that? In that very much disrupted zone the mechanism that was going on is the erosion and corrosion that he showed takes tens of seconds, and we're assuming here that the next rupture is another stress rupture, and that can only occur out at the flame front because there is no high temperature well inside the disrupted zone. It's only a flame front where the two products are interacting.

MR. MOELLER: Thank you.

MR. WARD: But it still could. It might very well involve several tubes.

MR. CLARE: That is conceivable. But again, let's look at the --

MR. WARD: In fact, it's almost inconceivable that it would involve just one, isn't it?

MR. CLARE: The effects will certainly be felt by more than one tube. The question is from the standpoint of timing and the simultaneous nature that is needed in order for this to be a significant problem. And I will go back to the point that what we are really interested in is something probably less than a second, but let's be generous and expand our horizon to several seconds.

MR. MOELLER: On these experiments will you please tell us how they were done or how many tubes there were? Were they at temperature and so forth?

MR. CLARE: I will try to do that, and if you will allow me, I will focus on the U.S. tests. I have other information and tables that I could get to on the foreign tests, but I would have to dig that out.

Now, there have been some 63 tests related to sodium-water reactions throughout the world in the last 20 years or so. Now, I will point out that that is a different number from what Paul Dickson presented in June of last year.

(Slide.)

There are two reasons for that. One is I have been somewhat more generous to myself and included tests that he may not have considered large sodium-water leak tests, and an example would be where we have injected inert gas into a sodium-filled circuit to try to understand the behavior of a rupture disc. Also, there have been additional tests done since June.

Now, an important point about that is that there have been secondary failures in only four tests out of all 63. Specifically in the U.S. there have been nine tests performed in large leak test rate out in California, those tests specifically designed to be prototypic of CRBRP; and we had two tests there that produced secondary failures.

Now, let me tell you about those tests. There were two series of LLTR tests. The first series used the test article, which was originally built by Atomic International, and I believe it began operation in 1968. The modular steam generator, which is a 158-tube steam generator, otherwise prototypic of our steam generator, the sizing of the tubes and the material, et cetera.

The way the test was conducted there was the water tubes were capped off at the bottom, and a plenum of water was put essentially beyond the upper tube sheet, so you did have water in all of your tubes except in one location where you had the stagnant water in the one location principally coming from the bottom, and at least in some of the tests in some cases it came from the top.

We put in the tube which one used to simulate the original leak, so there was a special tube run in so that you could turn on the water and dump it into the sodium and let it run. On the sodium side we just had stagnant sodium in there to a reservoir and also to a rupture disc system. That didn't relieve the pressure.

So then using various tests at various conditions, and the conditions did simulate CRBRP operating conditions up to close to 1000 degrees as low temperature, as I believe 500 degrees were so with water pressures on the order of 2000 psi which we might see in various degrees of subcooling, steam superheat, et cetera -- the whole range of conditions.

The second series of large leak tests was done in a similar manner, but they used a special steam generator unit for the purpose which was half the height of the current CRBRP steam generator but otherwise the same, about 750 tubes four feet in diameter -- very similar from that standpoint.

Now, there were two tests in the series that produced secondary failures. The secondary failures occurred in tens of seconds. Again, the shortest one occurred in 16 seconds. And indeed, in one of these tests there were additional failures. They occurred well beyond that, and in fact, the next failure beyond this, beyond the secondary failure in this test occurred as an additional failure in the tube that was put into purposely leak.

Now, in every one of these tests you have to put one in that you make leak when you want it to leak, and that tube was the source of the third failure.

But again, the important point is --

MR. WARD: That is interesting because that is apparently right next to -- obviously right next to the primary failure.

MR. CLARE: That is correct.

MR. WARD: And this argument about it is more likely that the secondary failures are going to be out at some distance where the flame front is doesn't hold.

MR. DICKSON: That argument applies to a quick rupture which would occur from the stress rupture effect. To have a leak within that erosion zone after tens of seconds corresponds to an erosion-corrosion type of rupture which could occur anywhere, and you would expect it to be fairly close.

MR. WARD: And the secondary failures weren't stress failures.

MR. CLAPE: The secondary failures in all cases where stress failures. But the point is, number one, they were not full double-ended ruptures; they were more like 10 percent, 20 percent of a rupture.

MR. WARD: Well, if they are stress failures, don't they involve the flame front then?

MR. DICKSON: No. You could erode the wall away until it could no longer handle the stress.

MR. CLARE: In tens of seconds corrosion can be involved. If you will recall, I talked about the timing in tens of seconds that could occur, and that is the point Paul is making, is that given that you extend that time frame, then that could have been the mechanism. Indeed, wastage when you go long enough is often combined with stress rupture. When you waste away enough of the tube, you then get the stress rupture.

MR. MOELLER: How did you fix up the precursor tube to be sure it leaked and initiated the event?

MR. CLARE: The way it was done -- and I don't have the exact details with me -- we actually drilled a

50 mill hole in the tube and then otherwise covered it up, prevented the water from getting to it, and then allowed the water to come flying in through the hole.

MR. MOELLER: Okay. Now, with the other tubes around it, was there any care -- well, I'm sure there was care -- but did you try to select tubes that were brand new or tubes that had been used for some purpose for a while or what?

MR. CLARE: I don't know that there was any particular attempt to choose a tube in any condition or another. It was a typical tube. The one thing that was done is the orientation of that leak was chosen to optimize based upon earlier laboratory scale tests the impingement of the reaction zone on the target tube. And, in fact, I believe in many cases the bench test, the laboratory scale tests said that this is not the worst configuration.

(Slide.)

In fact, the worst configuration is when you are impinging over here on this tube. So the target tube was selected to be some adjacent tube where we could optimize that reaction time.

MR. DICKSON: And if I could add, although they were new tubes, they were minimum tube wall as simulating an end-of-life condition.

MR. MOELLER: That is helpful.

(Slide.)

MR. CLARE: So now if we consider the experimental results, the analytical results, the understanding of the phenomenology, the kind of failure that we might expect to happen in this plant -- and this has been borne out by the in-reactor events that have occurred, and there have been some -- we would expect a small leak, likely a detectable leak. But if you assume the leak was not detected, you would assume a gradual rise, possibly a fraction of an EDEG secondary failure after which you would burst the expansion tank rupture disc, the water side would be vented, and you would never see a burst of the main rupture disc and SWRPRS.

On the other hand, if you assume that some secondary failure occurred with a large enough water release, you could get a rupture of your main rupture discs with that initial failure after your precursor, and you would expect that to occur in tens of seconds, indeed probably minutes after the blowout on the precursor.

(Slide.)

However, to be conservative what we have done is to define a design basis sodium-water reaction event which includes a precursor, and I will come back to the pressure in a minute, followed by a primary failure not equivalent to 10 percent or 50 percent of a double-ended rupture, but one equivalent to a double-ended rupture -- we will call that time zero -- followed by a secondary failure at 1 second, followed by a tertiary failure at 2 seconds. So that's pop, pop, pop on double-ended ruptures.

Now, we define this precursor in such a way to maximize the mechanical loadings on the system, and we have done sensitivity studies to assure ourselves that this is the case. And what we do is we assume that this precursor is just right so that we raise the system pressure to 325 psig, which is just at the bursting point of the rupture disc, the main rupture discs, but we do it so rapidly that the expansion tank rupture disc does not rupture. And therefore, we do have this overpressure condition at the time this double-ended rupture occurs. And that is our design basis leak.

(Slide.)

Now, just for some perspective --

MR. EBERSOLE: Is that conservative, or could you argue that it is just lifting the set point to the point where you will get an immediate response of the rupture disc?

MR. CLARE: That was a question we asked

ourselves, and I said we did sensitivity studies. We wanted to evaluate that. I will talk about our evaluation techniques in a few minutes.

But having done the sensitivity studies, we find that this indeed gives us the highest pressure downstream. It turns out that from the double-ended rupture you get such a rapid peak anyway that this doesn't affect the rupture time.

Schematically, if you think of this as the water flow rate and think of the double-ended rupture as coming very rapidly, we do have the precursor leak which may take tens of seconds, one double-ended rupture followed by a second, followed by a third. This is contrasted with a more plausible leak where you might get a single fraction of a double-ended rupture. Very likely you would rupture one of your rupture discs here, but based upon the test data you say well, maybe you would have another one. In fact, all of these have happened, a minute or two minutes past the point of the initial rupture in our experiments.

(Slide.)

Let's compare this design basis event with the foreign events that have been considered in the design of those reactors, and we find that that comparison is very favorable.

The U.K. uses essentially an identical design basis accident as we use. In Germany and France they use only one tube rupture as a design basis. Japan we don't quite understand. We know they use one tube for the licensing process, but we know that they otherwise consider one followed by three others. And although we don't know the exact timing between those others, we know they are not considered to be simultaneous.

And then, of course, what we have is three tubes at a one-second interval.

(Slide.)

Taking that then as our design basis accident, we evaluate the effects on the system. We do that using a computer code called TRANSWRAP -- transient sodium-water reaction analysis program. For the purpose of the evaluation we select the worst leak location and the worst initial conditions for the leak based upon the sensitivity studies. And the results of those sensitivity studies are that the leak is in the evaporator where the water is subcooled, and that, therefore, gives us the highest mass flow rate for any particular leak size of the tube. And we find that the highest, that the worst combination of pressure and subcoolant occurs at some slight delay after loss of offsite power where we have already started a transient in the steam generator system.

The actual leak rate of the water through the tube, the failed tube, again is assumed to be a double-ended rupture. This is established using the RELAP code. Of course, that is a well-validated GE code used in the water reactor business.

The phenomenology that is assumed in TRANSWRAP is that there is a hydrogen yield of 65 percent, and what we typically find in the experiments is that that yield will be somewhere between 35 and 50 percent. I believe the British used 55 percent in their calculations, and the Germans perhaps used 60. We are at the high end of that range.

Of course, the hydrogen yield is only one part of the story. If you're trying to figure out how fast this bubble is expanding and what the pressures are down in the system, we need to know what temperature the hydrogen is at, and we assume 1700 degrees which bounds our experimental results. And this combination does do a good job of predicting the events that we have seen in the experiments. It bounds those experimental results.

We model the rupture disc with dynamic elastic plastic rupture disc behavior. We actually model in a stress-strain sort of sense the behavior of the two in series rupture discs. As the stress increases, they begin to buckle backwards. They contact the knife edge, which will eventually tear the disc open, and then after some hold time tears the rupture disc open allowing the sodium to pass through. That is all discretely modeled in the TRANSWRAP code, and we have validated that model against the experimental results in the large leak test facility.

(Slide.)

The mechanical loadings from the event are also predicting using TRANSWRAP, and TRANSWRAP actually uses the water hammer model that comes from the computer code HITRAN, which is an old standby developed by the Army Corps of Engineers.

Sodium is treated as a compressible fluid. It is a one-dimensional code. For sodium hammer the friction effects of the fluid are modeled. The strain, which would dissipate the energy as the wave propagates down the piping, is not accounted for; so by the time the pressures get down to the intermediate heat exchanger, which again is the critical boundary, we have very conservatively treated it, and much of the energy which would have been dissipated is still contained in our predicted pressure pulse.

Again, the TRANSWRAP results are validated as being conservative, using the data from the large leak test facility which did have rupture discs and did have runs of sodium piping to try to be sure we can predict this type of behavior.

(Slide.)

So then as a summary, we have conservatively selected a sodium-water reaction event as our design basis, and we have a validated computer code which we used to conservatively model the consequences of that design basis event.

Now, I have just a few words to try to address the additional questions that were raised in the request that we come here and speak on this subject. And a key one of those questions was what about steam generator reliability.

(Slide.)

We have looked at that for the purpose of doing a number of these sensitivity studies we have done on the plant on reliability, availability, et cetera; but because the steam generator modules are first of a kind components, there has not been extensive operational or testing data in terms of many, many years of actual operations; so we don't have a statistical data base. However, what we have done is to survey various steam generators and fossil fuel plants, LWR plants, and what data does exist for steam generators for both thermal and fast reactor units and test units.

And I might note that there have been some 18 years of testing with steam generators similar to the one we're going to have in the plant. The modular steam generator that I mentioned earlier was first put into testing by AI in 1968. It will be 20 years before we put that unit in operation in the plant. We haven't accumulated 20 years yet. We will before we start up the plant.

Engineering judgment was applied to all of this data to derive failure parameters that we used in our reliability studies, and this next vu-graph gives those.

(Slide.)

This is a very small leak, one which could be detected prior to the bursting of the expansion tank rupture disc. The medium leak is one that would result in rupturing of the intermediate -- excuse me -- of the expansion tank rupture disc, and the larger leak is one which we are sure would rupture the main rupture discs and relieve the system.

Now, if you are interested in how you convert 10^{-6} hours per module to years in a plant with nine modules, let me do that for you. This is about once every two years, this is about once every ten years, and this is about once every 40 years.

Our understanding of this is that it is kind of a lower limit of a conservative limit of the probability of leaks based upon the data we have available. I might note that the Phenix experiment suggests that this is indeed conservative. They've been operating that plant since 1974, and to the best of my knowledge they have sustained two steam generator leaks.

MR. SHEWMON: The British, on the other hand, had a fair amount of trouble on this.

MR. CLARE: That's right. And the Staff said I would address this, didn't they? PFR had a much different steam generator than the one we have, and they have indeed had considerable problems with it. To begin with, they built their units out of stainless steel -- I forget the number -- 221 or 321; and they found that their welding process was not necessarily very reliable. And we have taken the specific actions of developing the new weld process, and I think you have probably seen some discussion of that in one of the prior meetings with your working group, and we are using ferritic 2 1/4 chrome one moly material, which based upon our experience, as well as others, is probably the best material to use.

MR. SHEWMON: Well, your reason why you think you've got a better design in the system than the British is that you aren't using welded 321?

MR. CLARE: Well, it is that we have specifically put together a weld configuration which is highly reliable. These are the principal differences, and I don't know all of the details between the units. And we are using a material which would be less subject to attack from reaction products.

MR. SHEWMON: What did the French use in their material, do you know?

MR. CLARE: They have used a couple of different materials. They have some stainless steel and some ferritic, and I don't have all of the details.

MR. DICKSON: I wanted to add one thing. The British experience is that most of their leaks occurred in their welds, and those were tube-to-tube sheet welds, and that seemed to be where the problem existed. We have eliminated that, not just as a weld technique but as a design concept.

MR. LONGENECKER: The French used 316 in the superheater, and the used 2 1/4 one moly in the evaporator, and they have had leaks in the ferritic units; and they think their problem is they didn't stress relieve the welds.

The British problem, the same configuration, they've got stainless steel units. In the superheater and the reheater they didn't stress relieve their U-tube ferritic unit. When they developed the first leaks they weren't very big, and they got some constant sodium hydroxide. They shut down. By the time they did that the caustic had been transported over to the other units, and now what they're getting is just a continual succession of very small leaks in the austenitic units. So what they are doing is they are building new ones out of 9 chrome one moly.

They did three things wrong which we have corrected. One is they used some bad materials. They used some dirty 2 1/4 chrome one moly. When they tested they didn't stress relieve nor did they have a volumetric inspection of the tube-to-tube sheet weld. And the third one is they didn't really do any testing for any of the operating phenomena like flow-induced vibration.

So what we have done is go to the vacuum arc remelt forgings and very pure tubing, and do the rod anode inspection on the internal bore weld that we have. And we have told you today about some of the testing.

The testing that we still have to do besides the prototype is the full-scale flow-induced vibration model in water to make sure we have taken care of all three.

MR. SHEWMON: When you say dirty you mean the vacuum arc? You're thinking of oxide occlusions primarily?

MR. LONGENECKER: Yes.

MR. CARBON: Put this in context, would you, please, John. It is my impression that the British have some pretty capable technical people, and yet -- and they were trying to build this steam generator for the big unit. They thought they had all of their problems solved and ran into all kinds of difficulties.

Is this because of a normal learning cycle, or did they goof, or did they not test enough?

MR. LONGENECKER: I think the principal -- it is hard to tell back that far because everyone has different views on what their hindsight was. But I think the general consensus is that they thought at the time that they could dump before they got very much caustic in the unit so that they could use stainless steel. They didn't know -- the use of those ferritics was so new at the time, they really didn't have a very good data base on the stress relief. And we have done a pretty extensive program, like about seven or eight years' worth, through Oak Ridge National Lab and Combustion Engineering on our 2 1/4 one moly learning all of the phenomena.

The other was that they didn't do enough preservice inspection to determine whether they had any leaks. Had they done the kind of tests that we do to pressure, I think they would have found that they probably did -- the Russians did; they put the units into service anyway, the ones that they had leaking.

What that led to, the leaks happened so early in the service that they weren't really accustomed to reading their hydrogen and oxygen monitors, and so when they started getting the leaks, they ran for about six weeks seeing the leaks. By the time they found out how many they had, they had formed caustic, and it didn't take much to get over to the reheaters.

The Russians, on the other hand, did dump on the order of 400 pounds of reaction products into the sodium side. They had ferritic units 2 1/4 chrome one moly, cold-trapped them out and fixed the units and went back up. So primarily it was just bad judgment in using an austenitic material.

MR. CLARE: I might note in all of those events we're talking about we did not or they did not get the kind of energetic reaction in terms of a very rapid millisecond type of propagation that would give you any problem in terms of pressure pulses on your current boundaries.

MR. MOELLER: Earlier, a couple of slides back, you went past rather fast for me. This TRANSWRAP computer code was well proven?

MR. CLARE: Yes.

MR. MOELLER: And that we could have faith in it? Now, why should we have a warm feeling about it?

MR. CLARE: It has been validated by using it to predict both before and after the leak tests that have been performed in this large leak test rig that I talked about.

MR. MOELLER: In the sodium-water?

MR. CLARE: In the sodium-water both for smaller leaks and for larger leaks.

MR. MOELLER: And it has been shown to yield pretty good correlations?

MR. CLARE: That's correct in terms of, for example, rupture discs, burst times, burst pressures, peak pressures, on down the line.

MR. MOELLER: All right. Thank you.

MR. DICKSON: It is conservative in some areas. There is a loss around every elbow that is not predictable and not in the code. But there is some loss that varies depending upon a variety of conditions, and that is not in the code at all. So when he says it is validated, it is validated to the extent it does not underpredict.

MR. CARBON: Were there further questions? If not, thank you, Mr. Clare, and thank all of you for your fine effort and presentations today.

Mr. Chairman, I turn it back to you.

MR. RAY: Thank you, Max.

I might comment that while grueling, it was a very interesting presentation.

We will recess now until 8:30 tomorrow morning.

(Whereupon, at 6:30 p.m., the meeting was recessed, to be reconvened at 8:30 a.m., the following day, Saturday, February 12, 1983.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

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in the matter of: ACRS/274th General Meeting

Date of Proceeding: February 11, 1983

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.

Ray Heer

Official Reporter (Typed)

Ray Heer

Official Reporter (Signature)

STAFF REVIEW OF PSAR CHAPTER 2

- o STAFF DISCUSSED HYDROLOGY, GEOLOGY, SEISMOLOGY, GEOGRAPHY IN NUREG-0786 SITE SUITABILITY REPORT FOR THE CLINCH RIVER BREEDER REACTOR PLANT.

- o STAFF REVIEWERS HAVE COMPLETED SER SECTION FOR THE CHAPTER 2 REVIEW.

- o THE SRP IS APPLICABLE TO CRBR FOR THE CHAPTER 2 REVIEW.

- o STAFF CONCLUSION - NO OPEN ITEMS.

CRBRP BRIEFING FOR
ADVISORY COMMITTEE ON
REACTOR SAFEGUARDS (ACRS)

ACCOMMODATION OF NATURAL PHENOMENA IN CRBRP DESIGN

Presented by

ROBERT E. PALM
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BURNS AND ROE, INC.
ORADELL, NEW JERSEY

FEBRUARY 11, 1983

NATURAL PHENOMENA

- TORNADO
- PRECIPITATION
- FLOOD
- EARTHQUAKE

DESIGN BASIS TORNADO

- SAFETY RELATED STRUCTURES DESIGN TO WITHSTAND TORNADO EFFECTS
- DESIGN TORNADO IN ACCORDANCE WITH REGULATORY GUIDE 1.76 - REGION I
- TORNADO EFFECTS COMBINED WITH OTHER LOADS
- WIND VELOCITY = 360 MPH
    - ROTATIONAL VELOCITY = 290 MPH
    - TRANSLATIONAL VELOCITY = 70 MPH
- PRESSURE DROP = 3.0 PSI
- VELOCITY PRESSURES DETERMINED IN ACCORDANCE WITH ANSI A58.1

TORNADO MISSILE PROTECTIVE DESIGN

- SPECTRUM OF MISSILES IDENTIFIED
- MINIMUM THICKNESS OF EXTERIOR CONCRETE = 2'-3"
  - GREATER THAN MINIMUM 2'-0" REQUIRED BY SRP
- MISSILE PROTECTIVE STRUCTURES PROVIDED AT CRITICAL OPENINGS
- METHOD OF ANALYSIS DESCRIBED IN PSAR SECTION 3.5
  - PENETRATION INTO STEEL AND CONCRETE STRUCTURES
  - PREVENTION OF SCABBING IN CRITICAL AREAS
  - OVERALL AND LOCALIZED STRUCTURAL RESPONSE EVALUATED TO ASSURE STRUCTURAL INTEGRITY

DESIGN FOR MAXIMUM PRECIPITATION

- DRAINAGE FACILITIES DESIGN FOR 100 YEAR STORM
  - 3.5 INCHES PER HOUR MAXIMUM
- CRBRP DESIGN EVALUATED FOR EFFECTS OF PROBABLE MAXIMUM PRECIPITATION (PMP)
  - MOST CRITICAL STORM FOR LOCAL SITE CONDITIONS
  - 14 INCHES PER HOUR MAXIMUM
  - 29.5 INCHES IN 8 HOURS
- 6 INCH MAXIMUM LOCAL FLOODING ALLOWED IN PLANT AREA
  - BUILDING ENTRIES 12 INCHES ABOVE GRADE
- 8 INCH MAXIMUM PONDING ON ROOFS
  - EXCESS DISCHARGED BY OVERFLOWS TO GRADE
  - CURBS PROVIDED AROUND ROOF OPENINGS
- EQUIVALENT 80 INCH SNOWFALL DEPTH ACCOMMODATED IN DESIGN
  - 40 PSF ROOF LOAD

DESIGN FOR FLOODS

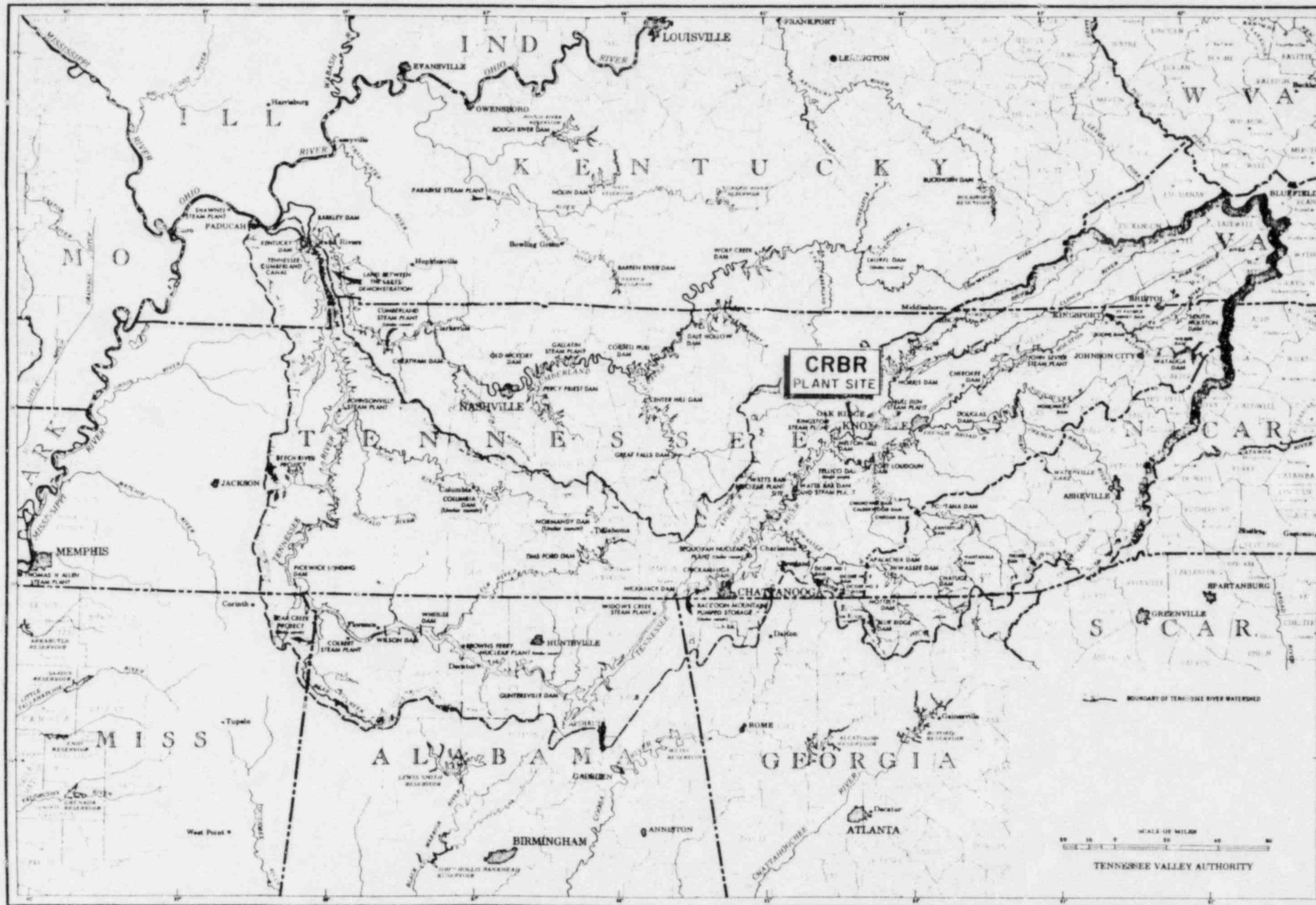
- PROBABLE MAXIMUM FLOOD (PMF)
    - MAXIMUM ELEVATION = 779.8 FT. INCLUDES 40 MPH WIND AND WAVE RUNUP
- MAXIMUM FLOOD LEVEL AT SITE = 809.2 FT.
    - BASED ON UPSTREAM DAM FAILURE COMBINED WITH 1/2 PMF
    - (DETAILS TO BE PRESENTED LATER BY TVA)
- PLANT GRADE AT ELEVATION 815 FT.
- STRUCTURES DESIGNED FOR MAXIMUM GROUNDWATER LEVEL OF 809 FT.
    - HYDROSTATIC EFFECTS
    - WATERTIGHTNESS

EARTHQUAKE DESIGN

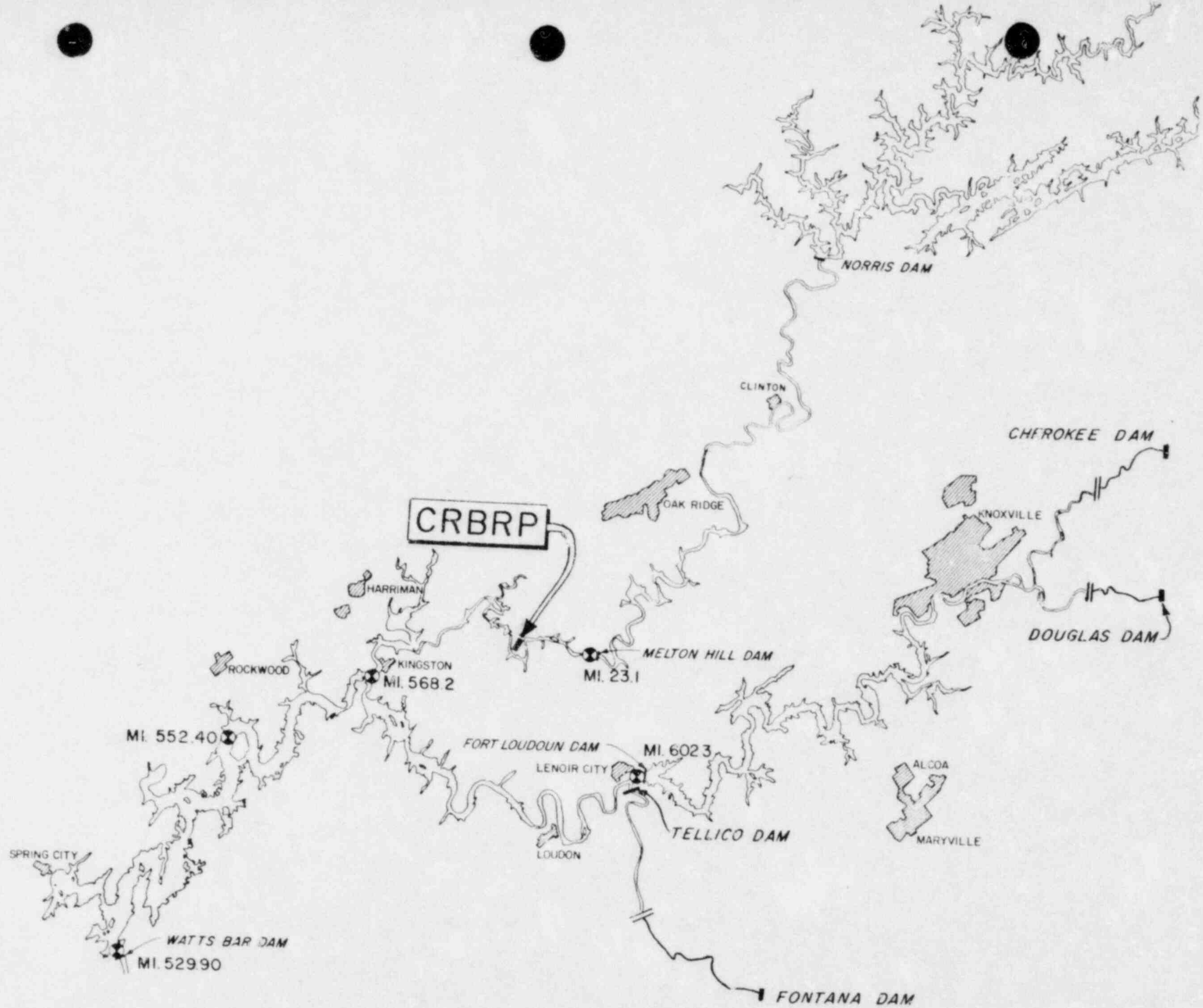
- TECTONIC PROVINCE APPROACH FOR DETERMINATION OF SSE
    - IN ACCORDANCE WITH 10CFR100, APPENDIX A
- LARGEST HISTORICAL EARTHQUAKE IDENTIFIED AS GILES COUNTY, VIRGINA, 1897
    - NRC CLASSIFIED THIS EARTHQUAKE AS INTENSITY VIII
- CORRELATION OF INTENSITY TO ACCELERATION RESULTS IN SSE = 0.25 G
- EARTHQUAKE ASSUMED TO OCCUR AT THE CRBRP SITE
- OBE = 1/2 SSE = 0.125 G
- SEISMIC DESIGN OF STRUCTURES, COMPONENTS AND SYSTEMS IN ACCORDANCE WITH APPLICABLE CODES, REGULATORY GUIDES AND SRP

SUMMARY AND CONCLUSIONS

- CONSERVATIVE DESIGN BASES HAVE BEEN ESTABLISHED FOR POTENTIAL EVENTS FROM NATURAL PHENOMENA.
- THE CRBRP DESIGN ACCOMMODATES EACH OF THESE EVENTS



Tennessee Valley Region


NORRIS DAM
CLINTON
CHEROKEE DAM
OAK RIDGE
CRBRP
KNOXVILLE
HARRIMAN
DOUGLAS DAM
ROCKWOOD
KINGSTON
MI. 568.2
MELTON HILL DAM
MI. 23.1
MI. 552.40
FORT LOUDOUN DAM
MI. 602.3
LENOIR CITY
ALCOA
TELLICO DAM
MARYVILLE
LOUDON
SPRING CITY
WATTS BAR DAM
MI. 529.90
FONTANA DAM

WATERSHED

POTENTIAL SOURCES OF FLOODING

EXAMINED IN DETAIL

- STORMS -

  PRIMARY WATERCOURSE - CLINCH RIVER

  ADJACENT WATERCOURSE - TENNESSEE RIVER

- SEISMIC-INDUCED DAM FAILURE -

NOT EXAMINED IN DETAIL

- SNOW MELT/ICE JAMS

  -- TEMPERATE CLIMATE --

- LAND SLIDES

  -- SLIDE VOLUME POTENTIAL LIMITED --

DEFINITIONS

PMP - RAINFALL DEPTH

FOR A PARTICULAR SIZE BASIN

APPROACHES THE UPPER LIMIT

FOR A SPECIFIED DURATION

PRESENT CLIMATE CAN PRODUCE

PMF - MOST SEVERE FLOOD

CAN REASONABLY BE PREDICTED

OCCUR FROM HYDROMETEOROLOGICAL CONDITIONS

ASSUMES

OCCURRENCE OF PMP CRITICALLY CENTERED

SEQUENCE OF RELATED METEOROLOGIC AND HYDROLOGIC FACTORS

TYPICAL OF EXTREME STORMS

PMP

9 DAY STORM

*3-DAY ANTECEDENT STORM	6.9 INCHES
*3-DAY DRY PERIOD	0
*3-DAY MAIN STORM	17.2 INCHES
*TOTAL	24.1 INCHES

*AVERAGE ON 17,310 SQUARE-MILE WATERSHED ABOVE WATTS BAR DAM

NRC CRITERIA

FLOODS FROM SEISMIC EVENTS

ALTERNATIVE 1 - DAM FAILURE CAUSED BY SAFE SHUTDOWN EARTHQUAKE (SSE)

COINCIDENT WITH 25-YEAR FLOOD

ALTERNATIVE 2 - DAM FAILURE CAUSED BY OPERATING BASIS EARTHQUAKE (OBE)

COINCIDENT WITH ½ PMF

NORRIS BACKGROUND INFORMATION

CONCRETE GRAVITY DAM
COMPLETED IN 1936
LENGTH - 1860 FEET
HEIGHT - 265 FEET

OVERFLOW SPILLWAY
SLUICES
NONOVERFLOW SECTIONS ON EACH SIDE

ORIGINALLY DESIGNED FOR AN EARTHQUAKE ACCELERATION OF 0.1G
THROUGHOUT ITS HEIGHT

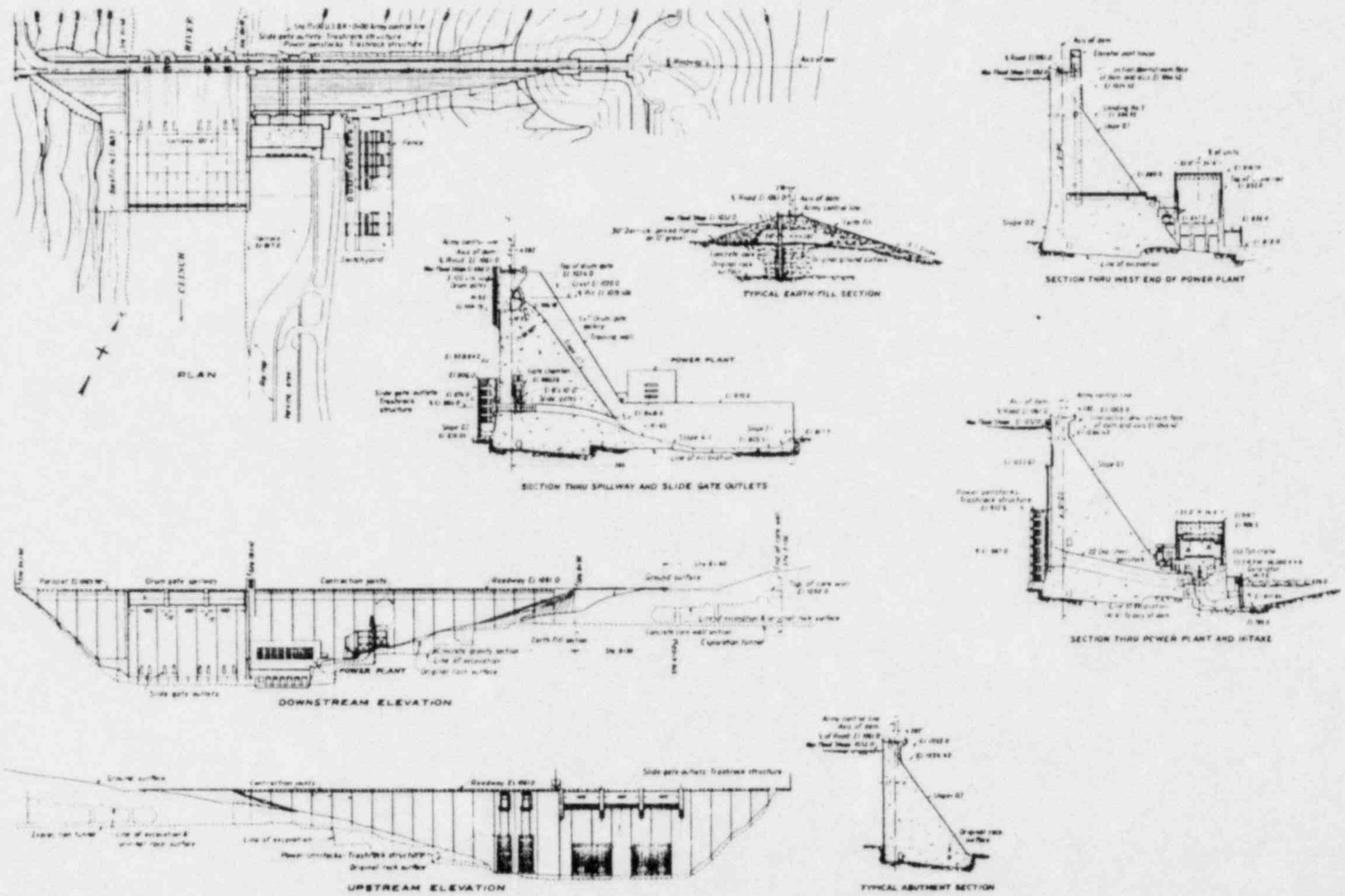


Figure 2.4-28. Norris Dam - Plan Elevations and Sections

6650-39

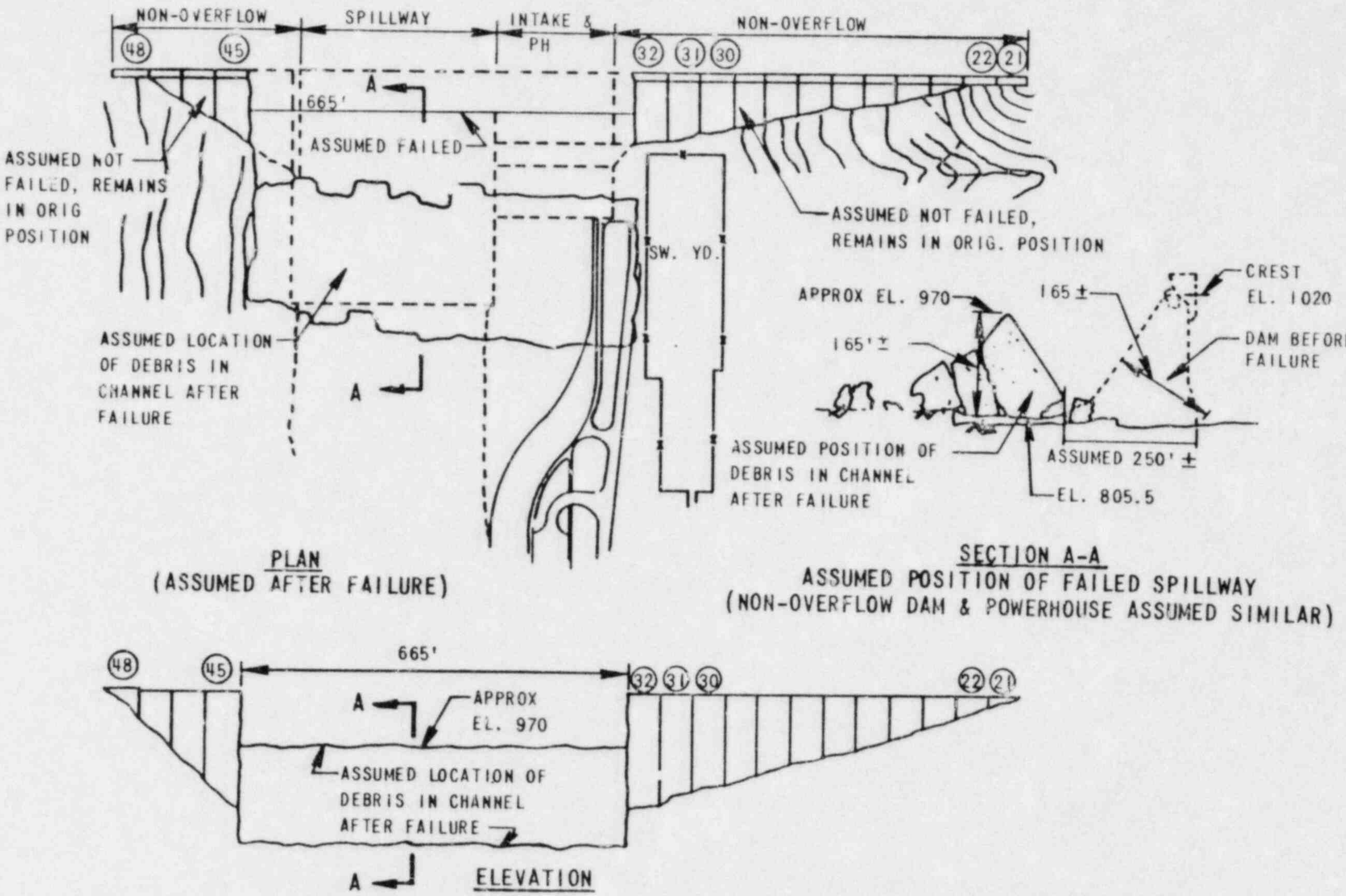


Figure 2.4-30 Norris Dam - Analysis for OBE & One Half PMF-Assumed Condition of Dam After Failure

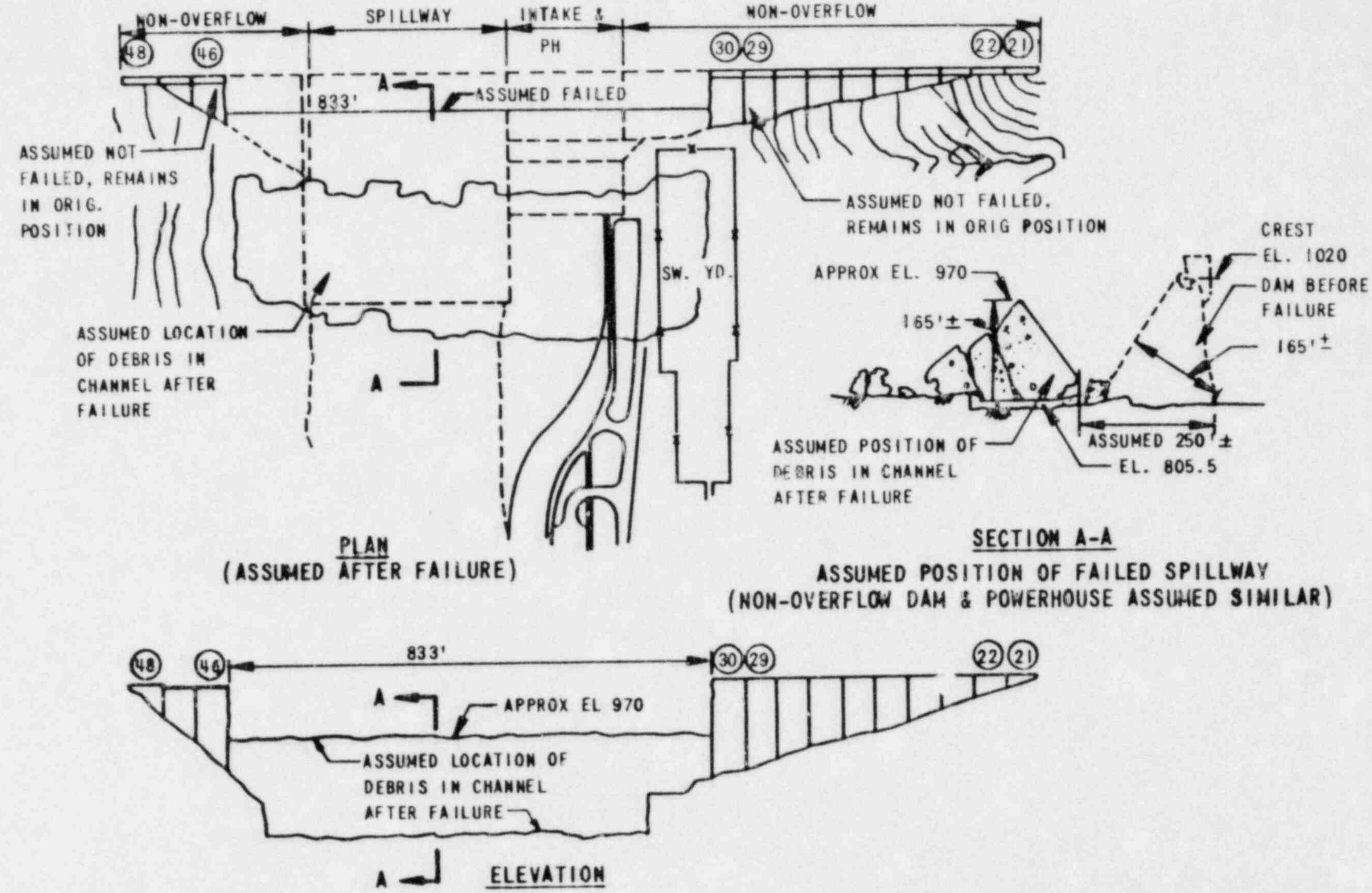


Figure 2.4-31 Norris Dam-SSE + 25 Year Flood Judged Condition of Dam After Failure

MAJOR ELEMENTS

NORRIS FAILURE FLOOD ANALYSIS

WATERSHED FLOWS IN ½ PMF OR 25-YEAR FLOOD

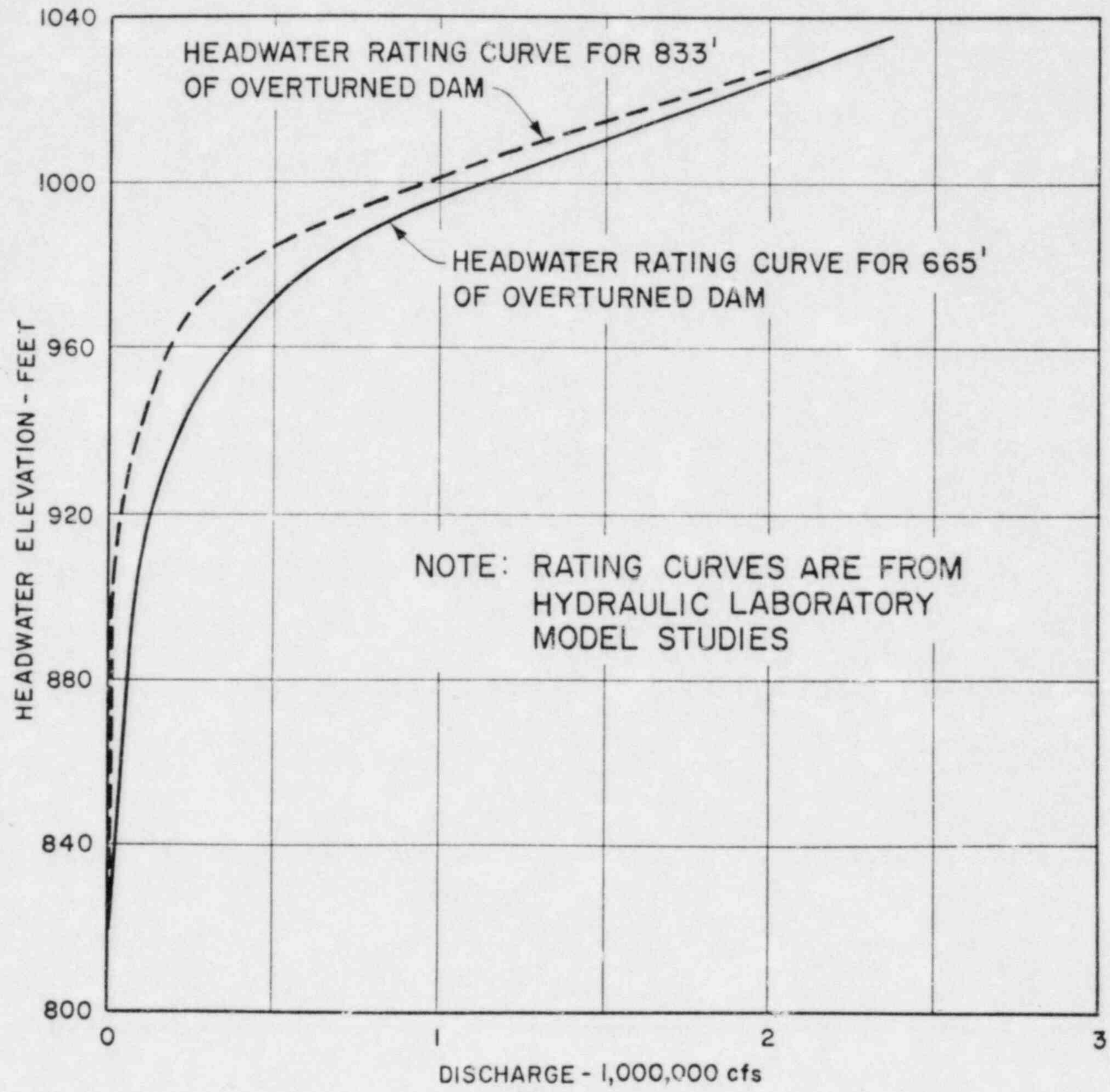
-- WATERSHED MODEL --

OUTFLOW FROM BREACHED NORRIS DAM

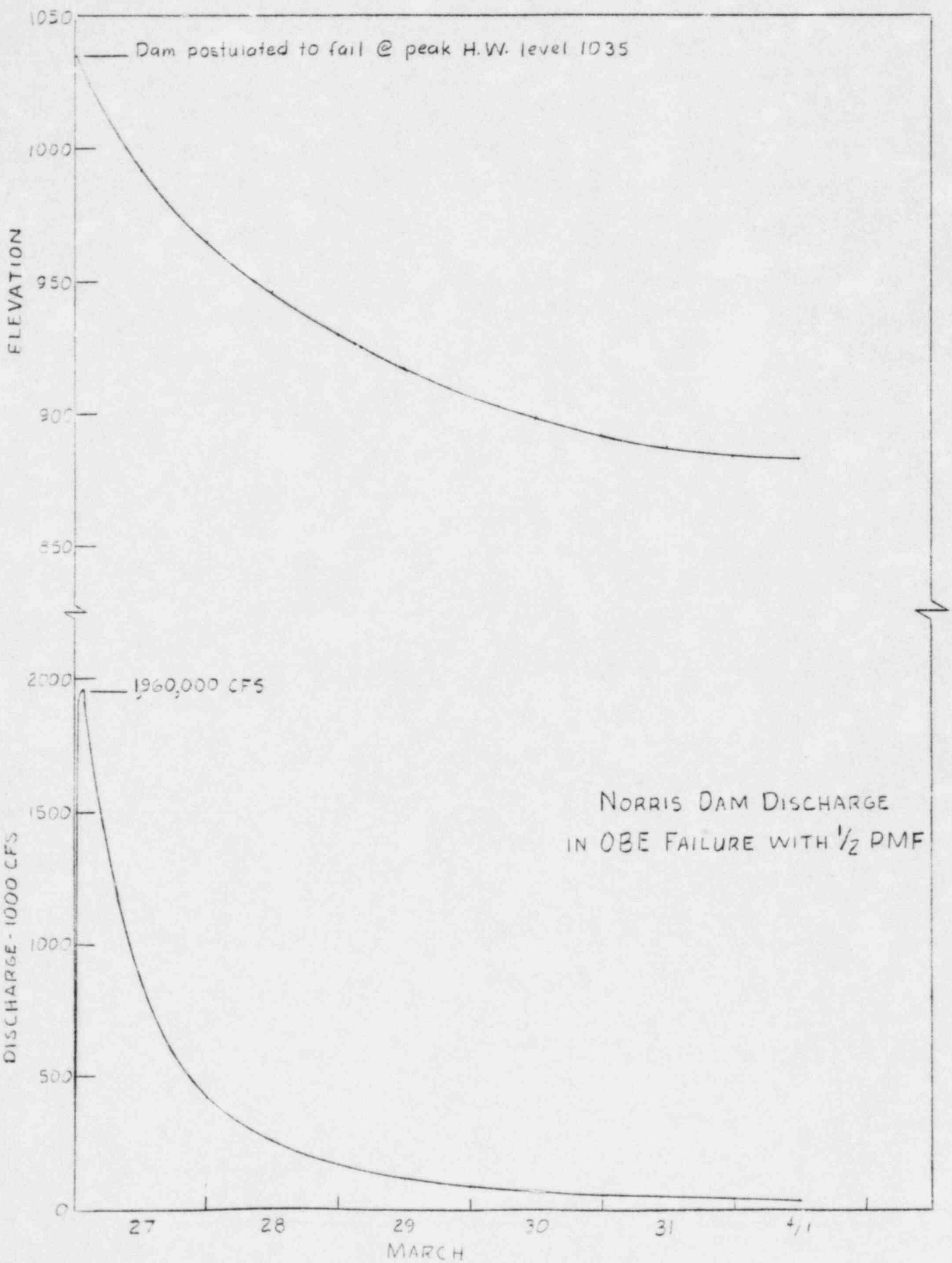
-- RATING CURVES --

COMBINED FLOWS AT SITE

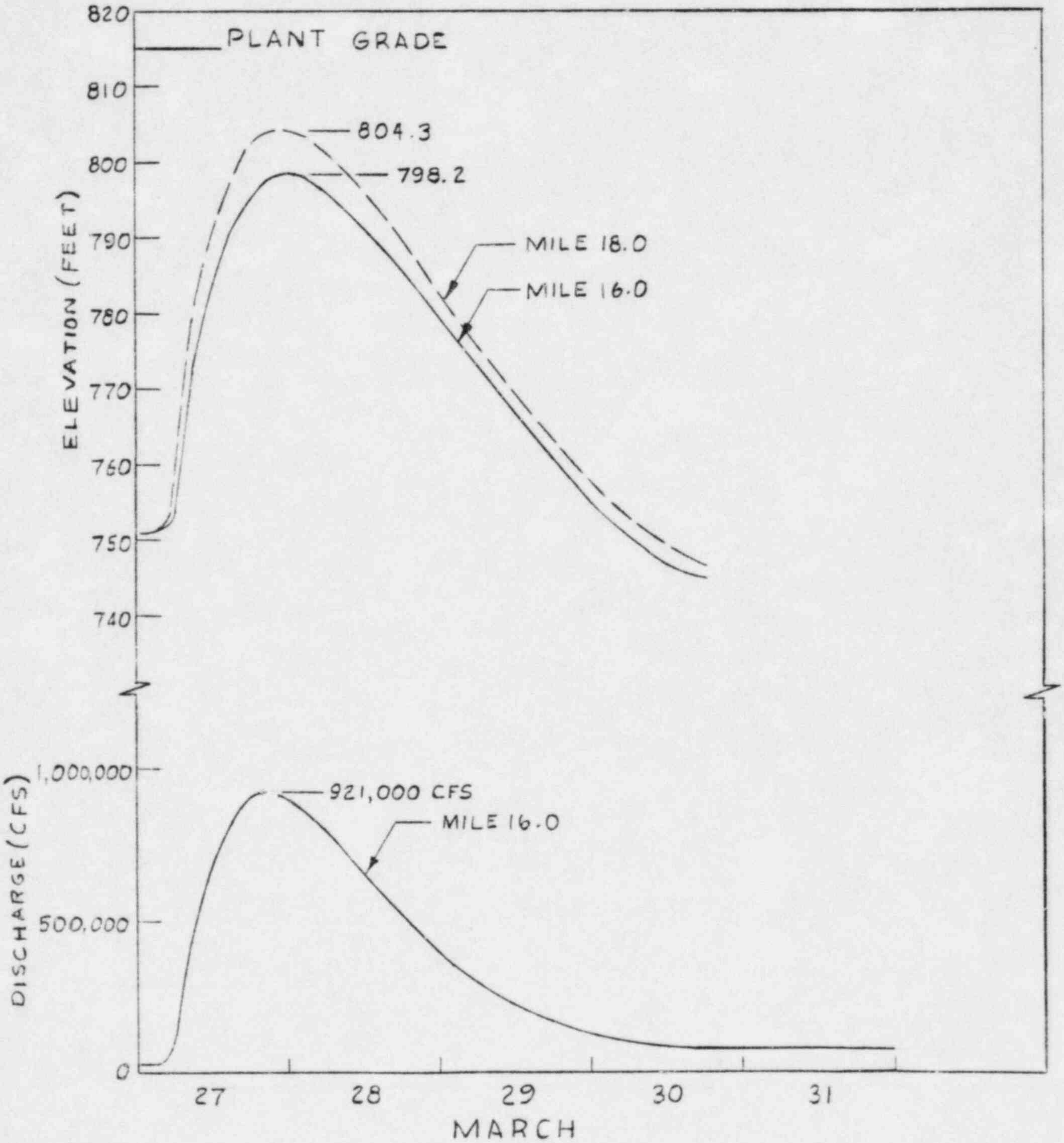
-- UNSTEADY FLOW ANALYSIS --



Headwater Rating Curves - Norris Dam



NORRIS DAM DISCHARGE
IN OBE FAILURE WITH 1/2 PMF



CONTROLLING EVENT

OBE + ½ PMF

FLOOD ELEVATIONS

PLANT GRADE ELEVATION = 815

EVENT	CRBR ELEVATION MILE 16	CRBR ELEVATION MILE 18
PMF	777.2	778.8
OBE FAILURE WITH ½ PMF	798.2	804.3
SSE FAILURE WITH 25-YEAR FLOOD	790.5	796.3

SENSITIVITY ANALYSIS

POSTULATED FAILURE MODE	CRBR ELEVATION MILE 16	CRBR ELEVATION MILE 18
OBE CONDITIONS WITH ½ PMF		
VANISHMENT OF 3 BLOCKS (38-40) TO GROUND LEVEL (168-FOOT WIDTH)	802.2	808.4
OVERTURNING OF BLOCKS 37-43 (370-FOOT WIDTH) WITH 925 DEBRIS LEVEL	805.3	811.9
OVERTURNING OF BLOCKS 33-44 (665-FOOT WIDTH) WITH 945 DEBRIS LEVEL	802.6	808.9
INSTANT VANISHMENT OF ENTIRE DAM (NO DEBRIS)	811.0	818.0

CRBRP RESERVE SEISMIC MARGINS

ADVANCED REACTORS DIVISION
WESTINGHOUSE ELECTRIC CORPORATION
MADISON, PENNSYLVANIA 15663-0158

February 11, 1983

ARD

8228-16

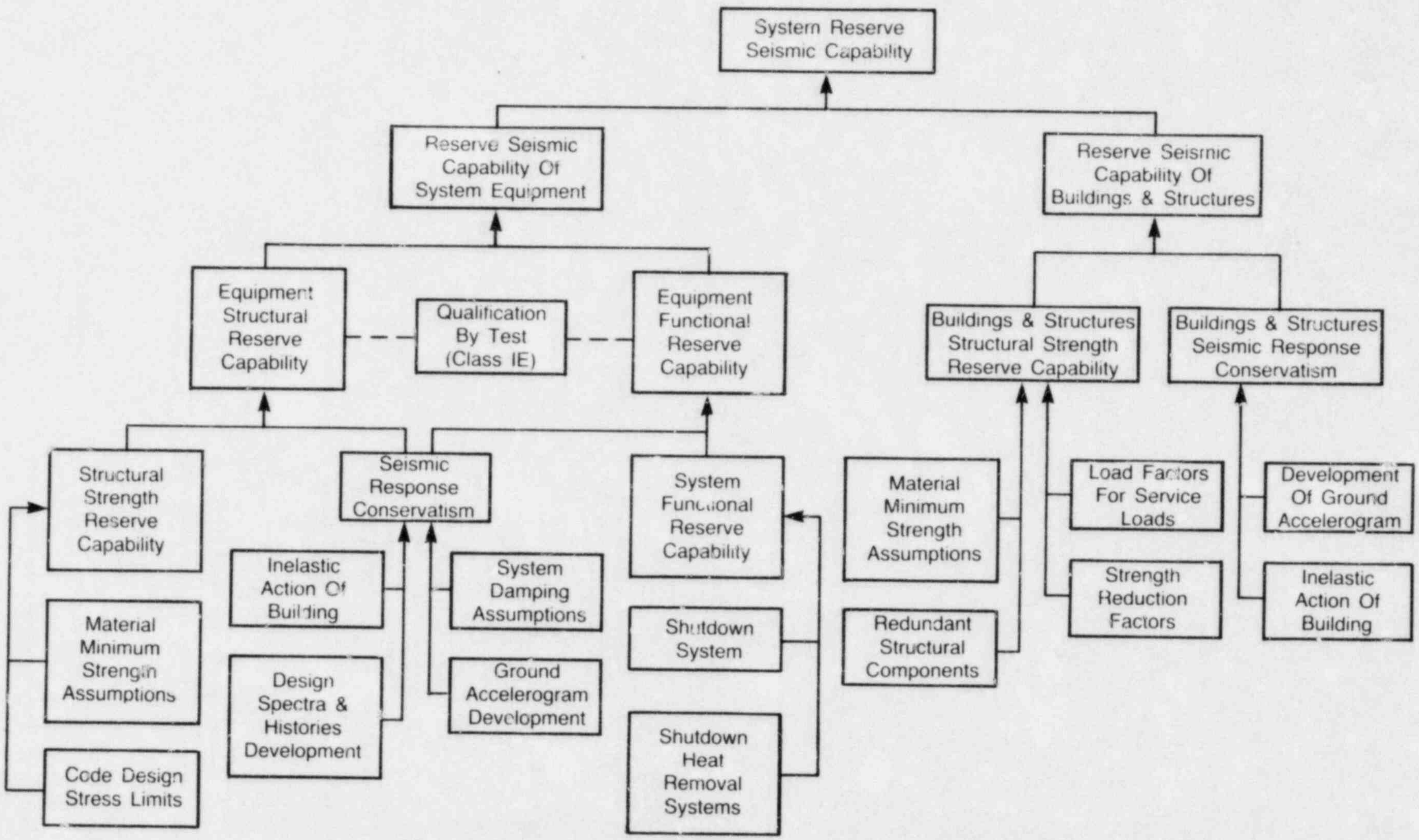
CRBRP RESERVE SEISMIC MARGINS

- Reserve seismic margin: seismic reserve strength/capability available when calculated effects (stress, functional performance) due to all loadings equal allowable limits (code, performance)
- Nominal margin: S_u/S_a when $S_a = \sigma_s + \sigma_n$
- Reserve margin earthquake: 0.25g x reserve seismic margin

Sources:

- Conservative predictions of building and equipment response
- Conservative definitions of structural and functional performance limits

RESERVE SEISMIC CAPABILITY OF CRBRP SYSTEM EVALUATION PROCEDURE



ⓌARD

EQUIPMENT STRUCTURAL RESERVE CAPABILITY
STRUCTURAL STRENGTH RESERVE CAPABILITY

- Material minimum strength assumptions:
  - Code minimum strength
  - Average strength for seismic
  - Ratio of average to minimum 1.20
- Code design stress limits
  - Service Limit Level D allowable membrane tensile stress = 0.7 S_u
  - Ratio of ultimate strength to allowable stress 1.43

STRUCTURAL STRENGTH NOMINAL MARGIN = 1.72

EQUIPMENT STRUCTURAL RESERVE CAPABILITY
SYSTEM SEISMIC RESPONSE CONSERVATISM

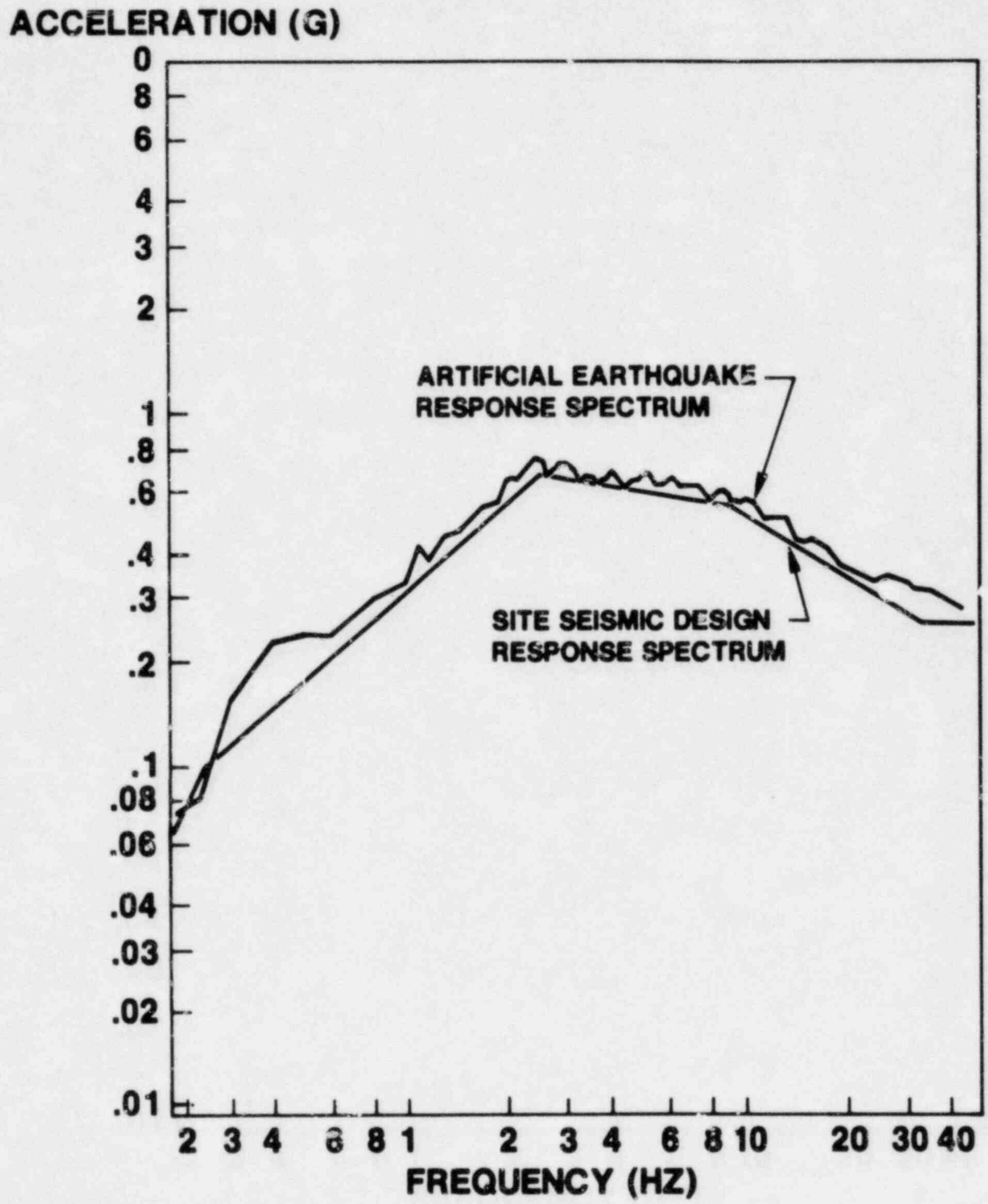
- System damping assumptions:
  - R.G. 1.61 3% damping value
  - Test results, 5% damping value
  - Peak response ratio 3% versus 5% — 1.2
- Development of ground accelerogram:
  - NRC SRP rule on spectra enveloping
  - Artificial response spectra conservatism — 1.05
- Reduction of floor response spectra due to inelastic action of building — 1.05
- Development of design response spectra:
  - Envelop upper and lower bounds of soil moduli
  - Peaks widened and higher due to uncoupling
  - Spectra smoothed to eliminate valleys and spectral fluctuations — 1.1
- Development of design histories:
  - Possible frequency variations of building
  - Vary Δt, compress and expand history
  - Develop spectra-consistent histories — 1.1

SYSTEM SEISMIC RESPONSE CONSERVATISM:

$(1.2)(1.05)^2(1.1) = \underline{1.45}$

ⓦARD

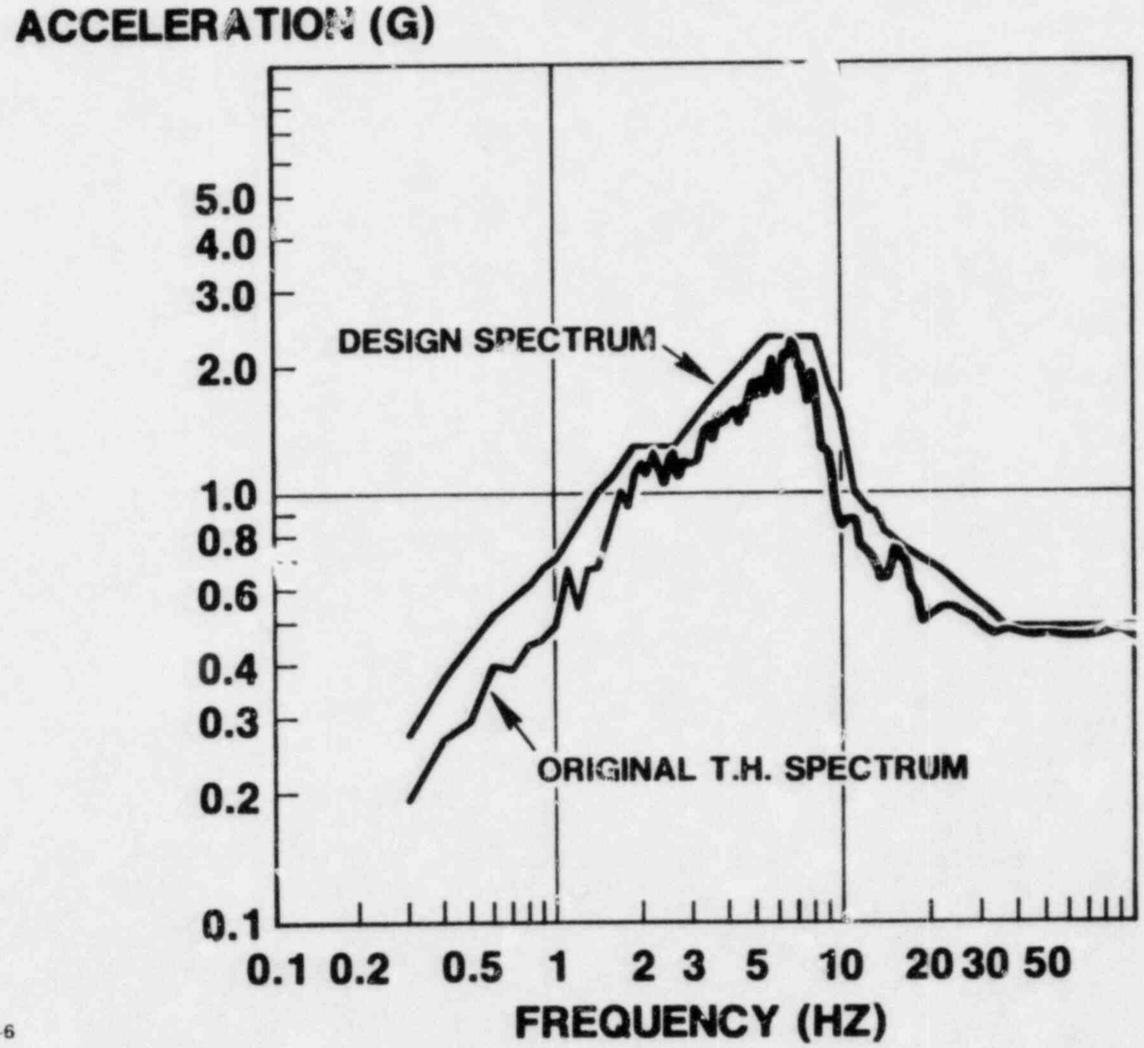
CRITERIA RESPONSE SPECTRUM ENVELOPING WITH HORIZONTAL E-W MOTION, SSE-7% DAMPING



Ⓦ ARD

SSE E-W HORIZONTAL + TORSIONAL COMBINED—DESIGN AND ORIGINAL T.H. RESPONSE SPECTRA AT R.V. SUPPORTS, EL. 800 FT.

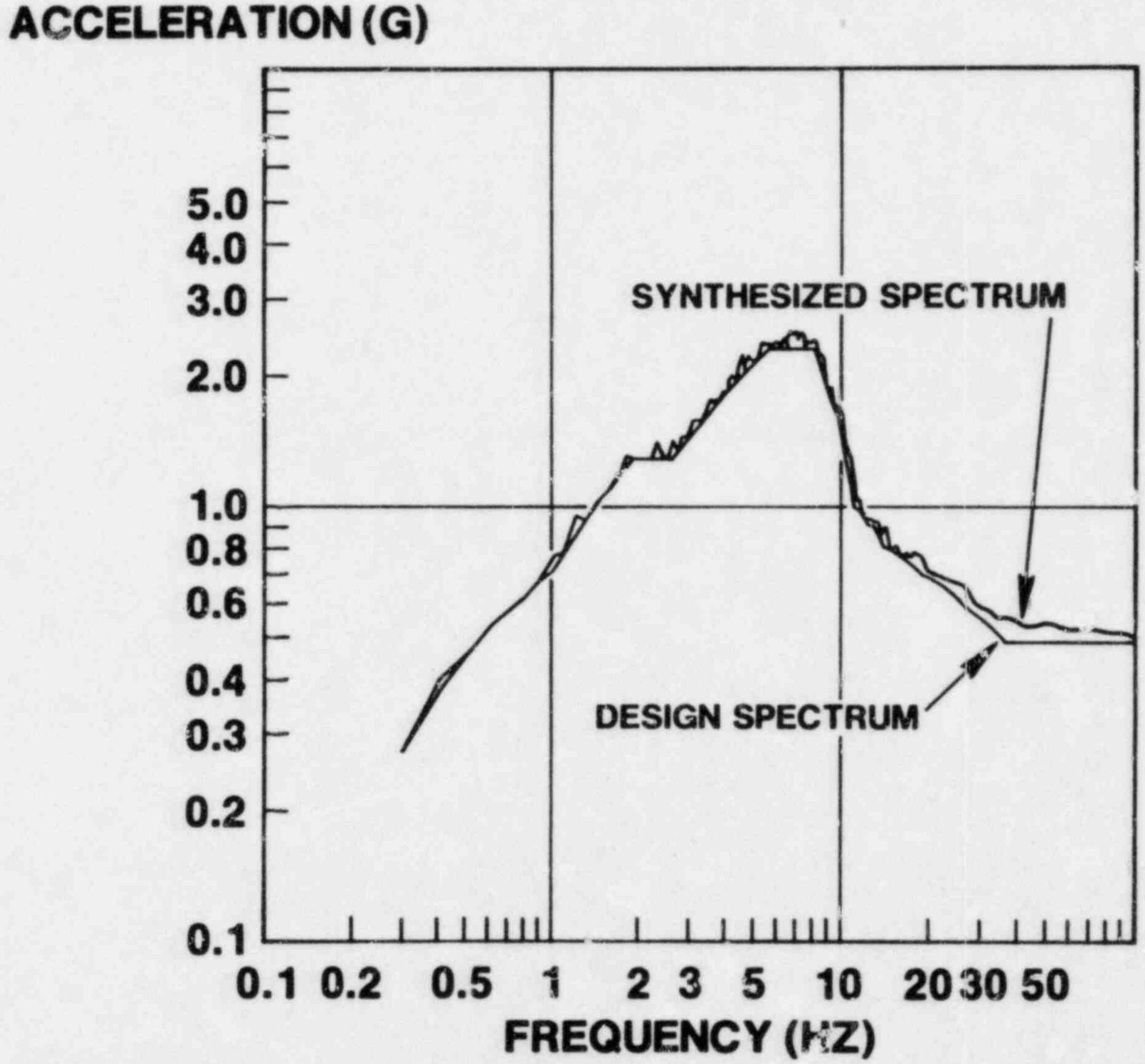
(3% CRITICAL DAMPING)



9522-6

SSE EW COMBINED HORIZONTAL AND TORSION-DESIGN AND SYNTHESIZED RESPONSE SPECTRA

(3% CRITICAL DAMPING)



9522-7

EQUIPMENT STRUCTURAL RESERVE CAPABILITY
STRUCTURAL RESERVE SEISMIC MARGIN

- NUREG/CR-2137:
  - Nominal margin (NM) $= S_u/S_a = S_u/\sigma_s + \sigma_n$
  - Seismic - only margin $= M_s$
  - $M_s = (S_u - S_a)/\sigma_s + 1 = 1/k\ (NM-1) + 1$
- Structural strength nominal margin = 1.72
- Conservative assumption of $k = \sigma_s/S_a$ = 60% to 90%
- Structural strength reserve seismic margin: 1/0.6 (1.72-1) + 1 = 2.2 for k = 60%; 1/0.9 (1.72-1) + 1 = 1.8 for k = 90%
- Seismic response conservatism = 1.45

EQUIPMENT STRUCTURAL RESERVE SEISMIC MARGIN
= <u>2.61 to 3.19</u>

ⓌARD

8228-4

EQUIPMENT STRUCTURAL RESERVE CAPABILITY CONTAINMENT STRUCTURAL RESERVE SEISMIC MARGIN

- Nominal margin (NM) on buckling = 1.9
- Seismic - only margin, M_s = 1/k (NM-1) + 1
- Ratio of seismic to total loadings, k = 70%
- Containment buckling strength reserve seismic margin = 1/0.7 (1.9-1) + 1 = 2.29
- System seismic response conservatism = 1.45

EQUIPMENT STRUCTURAL RESERVE
SEISMIC MARGIN = 3.32

ⓦARD

8228-5

EQUIPMENT FUNCTIONAL RESERVE CAPABILITY SHUTDOWN SYSTEM FUNCTIONAL RESERVE SEISMIC MARGIN

• Design capacity in excess of requirements:	
• Scram insertion performance evaluated for SSE of 0.33g	1.32
• Conservative system response requirements:	
• Worst case rod positions and minimum rod worths	1.10
• Friction coefficient (1.0 versus 0.45)	2.2
• Impact damping	1.07

- Shutdown system functional reserve seismic margin = 3.42
- System seismic response conservatism = 1.45

EQUIPMENT FUNCTIONAL RESERVE
SEISMIC MARGIN = 5.0

ARD

8228-6

EQUIPMENT FUNCTIONAL RESERVE CAPABILITY
RUPTURE DISCS FUNCTIONAL RESERVE SEISMIC MARGIN

- Evaluation based on worst loop
- Zero time rupture disc rating = 339 psi
- Five year aging effects (creep, corrosion, stress relieving) = 43 psi
- Rupture disc rating after five years = 296 psi
- Steady-state operating pressure = 219 psi
- Allowable pressure for seismic = 77 psi
- Pressure due to 0.25g SSE = 45 psi
- Rupture discs functional reserve seismic margin = 1 + 32/45 = 1.71
- Seismic response conservatism = $1.2\ (1.05)^2 = 1.32$

EQUIPMENT FUNCTIONAL RESERVE SEISMIC MARGIN = <u>2.26</u>

EQUIPMENT FUNCTIONAL RESERVE CAPABILITY DIRECT HEAT REMOVAL SERVICE (DHRS) COMPONENTS

- Overflow Heat Exchanger
- EVS (Ex-Vessel Storage Sodium Cooler)
- Air Blast Heat Exchanger
- EM Pumps*
- NaK Expansion Tank
- Sodium Piping
- Critical valves (evaluation in progress)

*Limiting component

WARD

8228-14

EQUIPMENT FUNCTIONAL RESERVE CAPABILITY
DHRS EM PUMPS FUNCTIONAL RESERVE SEISMIC MARGIN

• Calculated design margin based on yield criterion	1.01
• Material minimum strength assumptions	1.20

- Structural strength functional margin
- = 1.01 (1.2) = 1.21
- Ratio of seismic to total loadings, k = 32%
- EM Pumps functional reserve seismic margin
- = 1/0.32 (1.21-1) + 1 = 1.66
- System seismic response conservatism = 1.45

EQUIPMENT FUNCTIONAL RESERVE SEISMIC MARGIN = 2.41

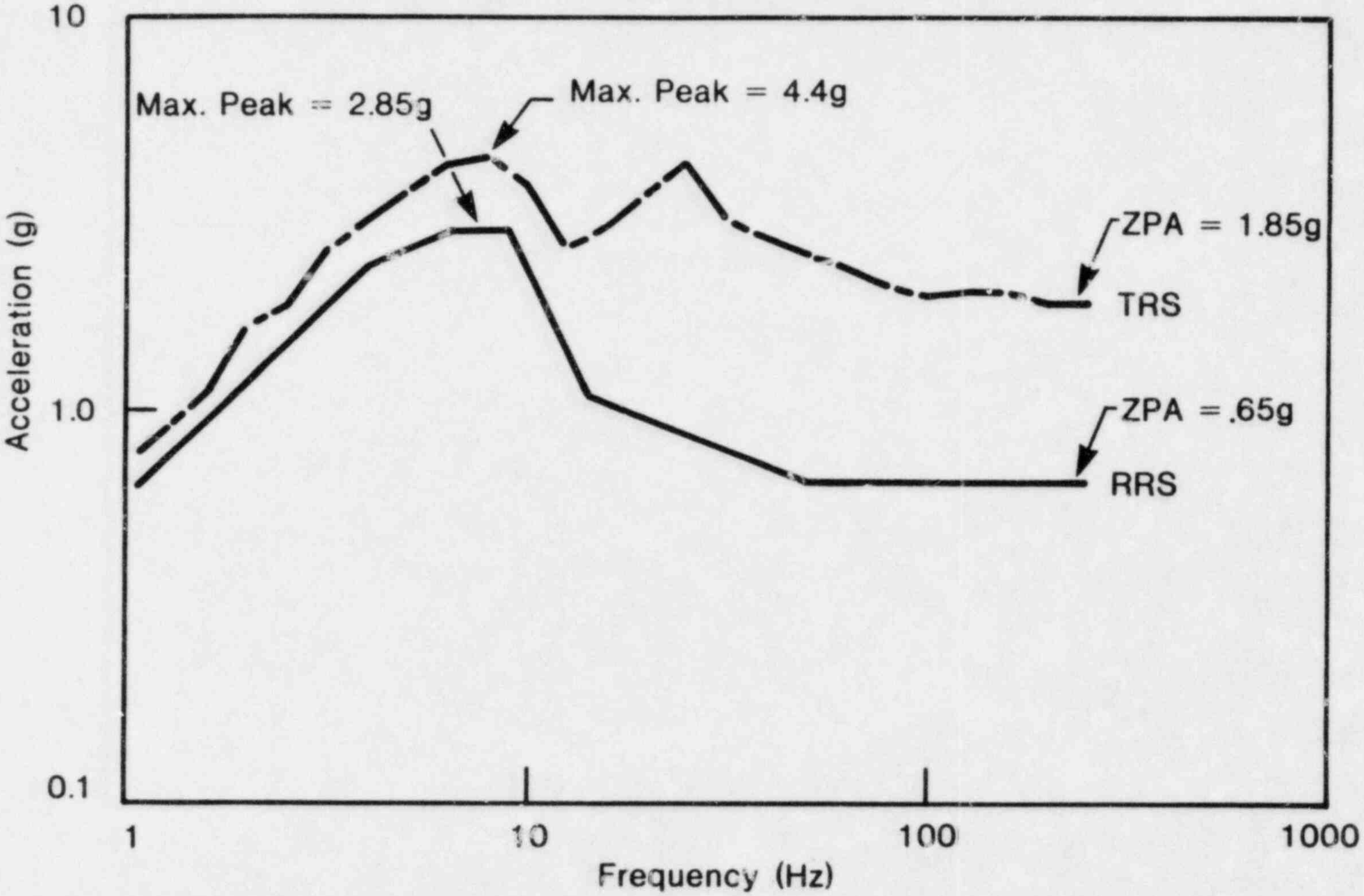
SEISMIC TESTING FOR CLASS 1E EQUIPMENT

- Qualify to IEEE std. 344-1975
- Single frequency tests
- Multiple frequency tests
- Single frequency plus multiple frequency
- Multiple frequency and recommended single frequency

EXAMPLE OF COMPARISON OF TRS/RRS FOR TESTED EQUIPMENT

- Reactor shutdown and isolation equipment
- Housed in cabinets and whole cabinet shake table tested
- Both sine beat unidirectional and multiple frequency biaxial motion
- Cabinet rotated 90°
- Functioned properly during and after testing
- TRS conservatively enveloped RRS
- Additional conservatism by enveloping horizontal RRS, 10% IEEE-323 margin and use of design spectra

PRIMARY REACTOR SHUTDOWN SYSTEM COMPARATOR/BUFFER CABINET AND LOGIC CABINET ASSEMBLIES

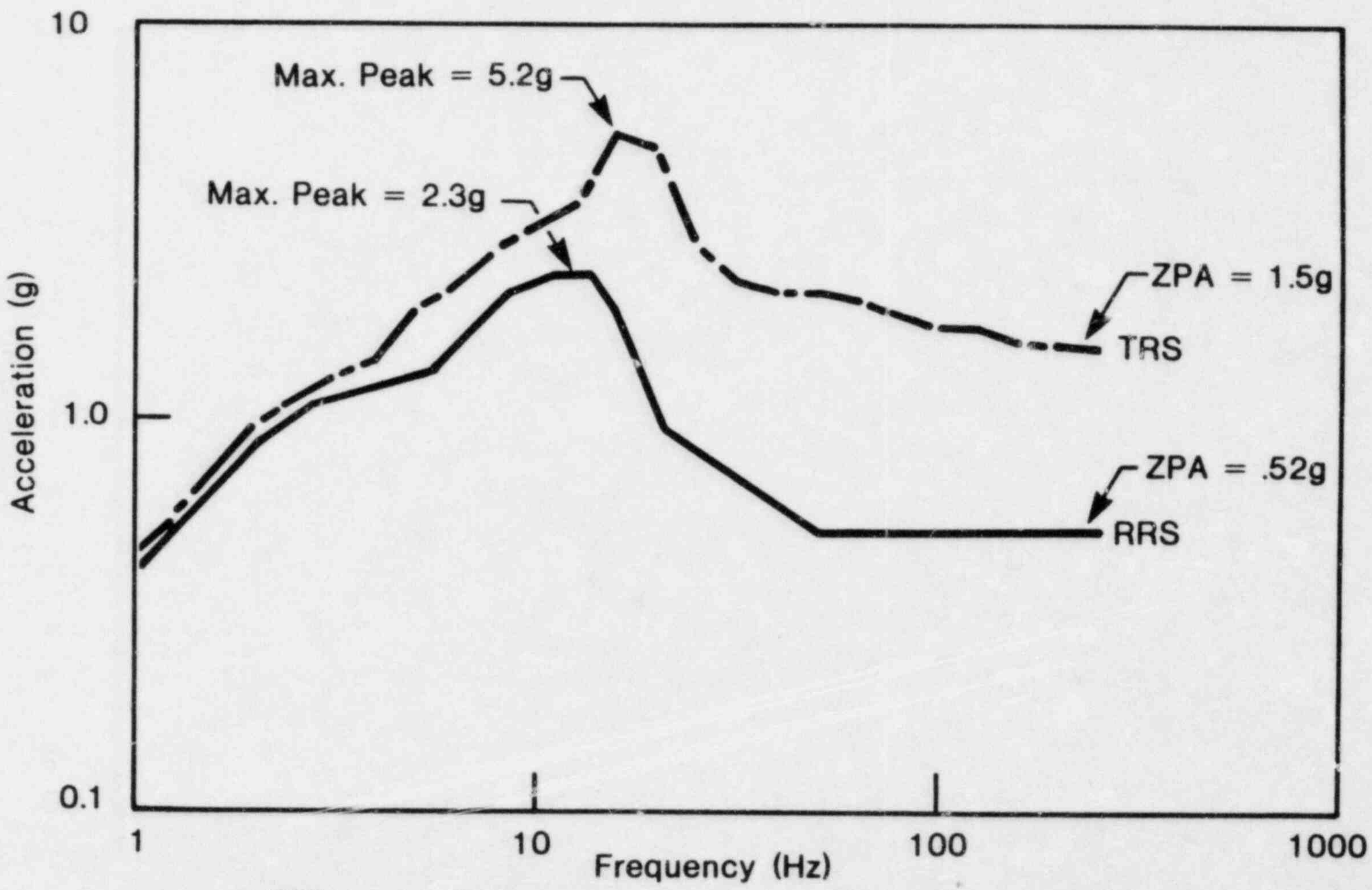


Damping 5%

SSE - Horizontal

7230-1

PRIMARY REACTOR SHUTDOWN SYSTEM COMPARATOR/BUFFER CABINET AND LOGIC CABINET ASSEMBLIES



Damping 5%

SSE - Vertical

7230-2

EXAMPLE OF RESERVE SEISMIC MARGIN FOR TESTED EQUIPMENT

- Seismic response conservatism for testing:
  - Development of ground accelerogram 1.05
  - Reduction of floor response spectrum 1.05
  - Development of required response spectra 1.1
  - IEEE-323 margin 1.1
- Total seismic response conservatism = 1.33
- Margin from TRS/RRS enveloping:
  - Ratio of ZPA = 2.85
  - Ratio of maximum peak = 1.54

RESERVE SEISMIC MARGIN on ZPA = 3.79 x margin to fragility

RESERVE SEISMIC MARGIN on peak = 2.05 x margin to fragility

ⓌARD

8228-7

BUILDINGS AND STRUCTURES RESERVE SEISMIC CAPABILITY
STRUCTURAL STRENGTH RESERVE SEISMIC MARGIN

- Load factors for service loads (OBE):
  - OBE load factor in load combination = 1.9 (1.0 for SSE)
  - Design controlled by service loads
  - Loading produced by OBE > 50% SSE
  - Result in reserve strength at least — 1.05
- Strength reduction factors:
  - ACI Code limits below ultimate capacity
  - Reduction factors range from 0.75 to 0.90
  - Result in reserve strength at least — 1.11
- Material minimum strength assumptions:
  - Reinforcing steel yield strength 5% to 15% higher than specified
  - Concrete design based on 28 day strength
  - 25% concrete strength increase due to aging in one year
  - Result in reserve strength at least — 1.12
- Redundant structural components:
  - Interconnected buildings on common foundation mat
  - Multiple interconnected cells
  - Estimated margin due to redundant path load — 1.05

STRUCTURAL STRENGTH RESERVE SEISMIC MARGIN = <u>1.37</u>

WARD

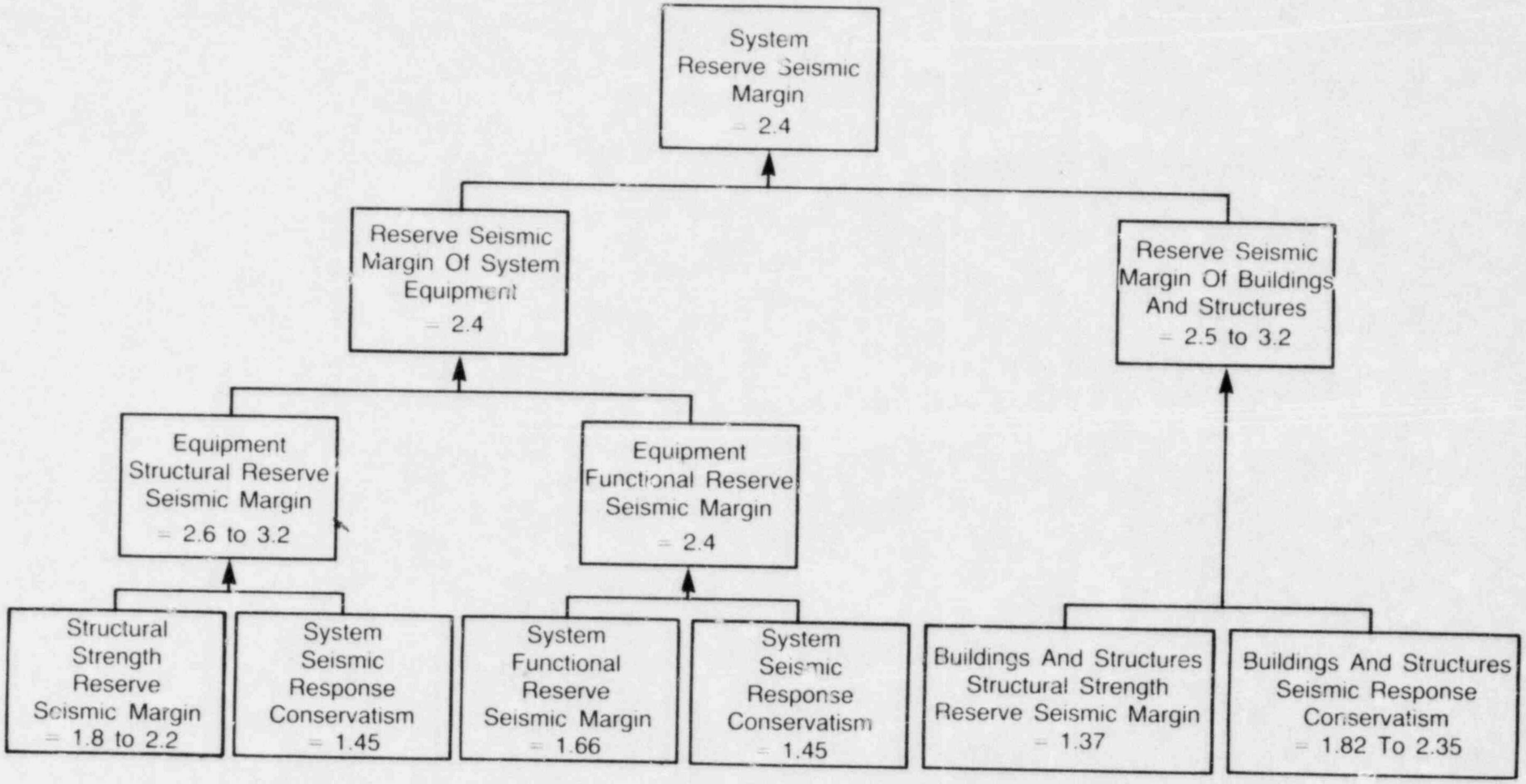
8228-9

BUILDINGS AND STRUCTURES RESERVE SEISMIC CAPABILITY
SEISMIC RESPONSE CONSERVATISM

- Development of ground accelerogram 1.05
- Reduction of response specrum due to inelastic action:
  - Substantial reserve strength in inelastic range
  - Energy absorption due to concrete cracking and yielding of reinforcing steel
  - Newmark's inelastic design spectra (NUREG/CR-0098)
  - Reduction of spectral accelerations below 33 Hz
  - Reduction is function of ductility factor and frequency
  - NUREG/CR-0098 ductility factor (μ) between 2 and 3 for structures housing Class I equipment
  - Reduction for 2Hz to 8Hz range $= 1/(2\mu-1)^{1/2}$
  - Elastic input accelerations reduced by 45% to 58%
  - Results in reserve margin of 1.73 to 2.24

BUILDINGS AND STRUCTURES SEISMIC RESPONSE CONSERVATISM = <u>1.82 to 2.35</u>

RESERVE SEISMIC CAPABILITY OF CRBRP
RESERVE SEISMIC MARGINS



WARD

8228-8

CONSERVATISM IN RESERVE SEISMIC CAPABILITY

- Assumption of calculated stress equal to allowable stress for equipment
- Design of most equipment controlled by OBE
- OBE = 50% SSE but OBE equipment loads > 50% SSE
- Use of linear-elastic dynamic and stress analyses
- Reduction of floor response spectra due to inelastic action of building
- Reduction for ductility factor of equipment
- Envelope spectra for multiple-support system
- Response spectrum versus time history analysis
- Exclusion of non-structural elements
- Redundance of structural elements
- Ground response spectra with high amplifications
- Absolute combination of seismic loads with other loads
- Conservatism by designer action and duplication for design simplification
- Load factors on dead and live loads for buildings
- Building serviceability requirements (shielding, stiffness, TMBDB, tornado missile)

CONCLUSIONS

- Reserve Seismic Capability of CRBRP System Equipment:
  - Structural Reserve Seismic Margin = 2.6 to 3.2
  - Reserve Margin Earthquake = 0.65g to 0.80g
  - Functional Reserve Seismic Margin = 2.4
  - Reserve Margin Earthquake = 0.60g
- Reserve Seismic Capability of CRBRP Buildings and Structures:
  - Reserve Seismic Margin = 2.5 to 3.2
  - Reserve Margin Earthquake = 0.62g to 0.80g

CRBRP SEISMIC CAPABILITY = AT LEAST 0.60g

CLINCH RIVER BREEDER REACTOR PLANT

BRIEFING FOR:

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS) FULL COMMITTEE

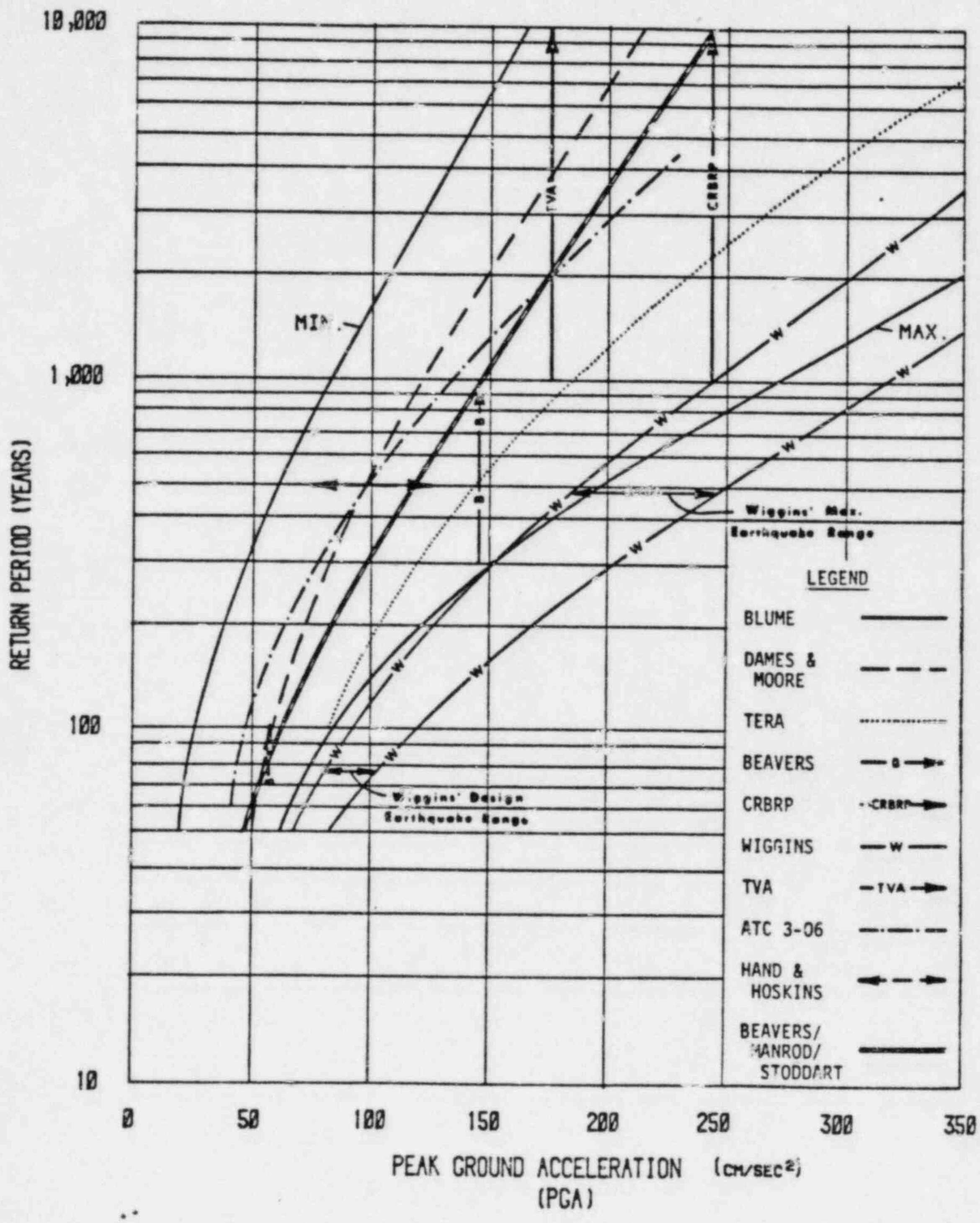
SEISMIC MARGIN

PRESENTED BY:

G. H. CLARE
LICENSING MANAGER,
CRBRP PROJECT
WESTINGHOUSE
ADVANCED REACTORS DIVISION
OAK RIDGE SITE

FEBRUARY 11, 1983

2-83-3229-18



SUPERIMPOSED RESULTS FOR OAK RIDGE

FIGURE 10

SEISMIC MARGIN

- ESTIMATES OF 0.25g SSE RECURRENCE FREQUENCY RANGE FROM 10^{-3} TO 10^{-4} PER YEAR
- AN EARTHQUAKE WITH ACCELERATION TWICE THAT OF THE SSE WOULD HAVE A RECURRENCE FREQUENCY SIGNIFICANTLY LESS THAN 10^{-4} PER YEAR
- MARGIN ASSESSMENTS INDICATE CRBRP COULD BE SHUTDOWN AND MAINTAINED IN A SAFE CONDITION FOR EARTHQUAKES WITH A RECURRENCE FREQUENCY SIGNIFICANTLY LESS THAN 10^{-4} PER YEAR

CRBR FUEL, BLANKET AND CONTROL ASSEMBLY MECHANICAL DESIGN

PRESENTATION TO ACRS - 2/11/83

BY - M. TOKAR - NRC
- R. BAARS - LANL

SCOPE OF STAFF REVIEW

- o MECHANICAL DESIGN OF FUEL, BLANKET AND CONTROL PINS AND ASSEMBLIES, INCLUDING:
  - DESIGN CRITERIA/LIMITS
  - DESIGN METHODS
  - STEADY STATE CONDITIONS
  - TRANSIENT CONDITIONS
- o DEVELOPMENT TESTING, INCLUDING
  - IN-REACTOR
  - EX-REACTOR
  - STEADY STATE
  - TRANSIENT
- o REVIEWED BY LANL

ACCEPTANCE CRITERIA

- o CONFORMANCE WITH CRBR PRINCIPAL DESIGN CRITERIA:
    - #8 - REACTOR DESIGN
- o CONFORMANCE WITH INTENT OF SRP 4.2 "FUEL SYSTEM DESIGN"
- o COMPLETENESS AND ADEQUACY (BASIS) OF APPLICANTS':
    - DESIGN CRITERIA/LIMITS
    - DESIGN METHODS
    - CONCEPTUAL DESIGN
- o ADEQUATE DEVELOPMENT TESTING TO SUPPORT THE DESIGN/CRITERIA/LIMITS/METHODS

FAVORABLE FACTORS
FOR SUCCESS OF SYSTEM

- o MASSIVE LMFBR TEST PROGRAM SHOWS MIXED OXIDE FAILURES VERY RARE FOR CRBR GOAL EXPOSURE

- o OPERATION IS FAR FROM COOLANT SATURATION, LESSENING THE CHANCE OF COOLING DISCONTINUITY

- o PROPOSED SCRAM TRIP SETTINGS TERMINATE ABNORMAL OCCURRENCES FAR SHORT OF SIGNIFICANT FUEL DAMAGE OR DISRUPTION

- o LOW SMEAR DENSITY OF FUEL (85%) - ABOUT TWICE THE RELATIVE VOLUME TO ACCOMMODATE RADIAL EXPANSION AS LWR FUELS

- o FALLBACK POSITIONS OF REDUCED POWER, EXPOSURE AND OPERATING TEMPERATURE ARE AVAILABLE

- o OPERATING DATA ON SIMILAR (FFTF) SYSTEM AVAILABLE BY FSAR

CRITERIA ISSUES

COOLABLE GEOMETRY LIMITS

- NO BASIS PROVIDED TO SUPPORT CLADDING MELTING LIMIT FOR ENSURING COOLABLE GEOMETRY

- VIOLABLE NO-BOILING GUIDELINE INADEQUATE - NO INFORMATION AS TO HOW CASES INVOLVING BOILING WOULD BE EVALUATED

- NEITHER CLADDING NOR COOLANT TEMPERATURE BASED LIMITS ADEQUATELY GUARD AGAINST MOLTEN FUEL EXPULSION FOR OVER POWER CONDITIONS

RESOLUTION: APPICANTS HAVE COMMITTED TO ADDRESS ALL OF THESE ISSUES AND DOCUMENT A COMPREHENSIVE BASIS FOR COOLABLE GEOMETRY LIMITS FOR REVIEW BY THE STAFF PRIOR TO FSAR SUBMITTAL.

METHODS ISSUES

FUEL EVALUATION MODELS

- o CUMULATIVE DAMAGE FUNCTION MODEL
  - MODEL HAS NOT BEEN QUALIFIED TO INTEGRAL ROD TEST DATA
  - MODEL DOES NOT ADDRESS FUEL ADJACENCY EFFECT
  - STATISTICAL APPROACH DOES NOT COVER DATA VARIANCE

- o DUCTILITY LIMITED STRAIN MODEL
  - MODEL SHOULD BE REQUALIFIED TO INTEGRAL ROD TEST DATA
  - MARGIN TO FAILURE NOT ESTABLISHED
  - MODEL UNCERTAINTIES NOT ESTABLISHED

RESOLUTION: APPLICANT HAS COMMITTED TO ADDRESS CDF ISSUES BY SUBMITTAL OF THE FSAR.

DATA BASE ISSUES

- o ATYPICAL FACTORS
  - FLUENCE/BURNUP
  - SHORT RODS
  - TRANSIENT TEST RADIAL POWER DEPRESSION
  - NO PRECONDITIONING IN TRANSIENT TESTS

- o COVERAGE
  - 32% PLUTONIUM
  - BLANKET RODS
  - SLOW OVERPOWER
  - UNDERCOOLING AT END-OF-LIFE

- o CLADDING
  - FUEL ADJACENCY EFFECT
  - RESPONSE AT HIGH FLUENCE AND HIGH TEMPERATURE

RESOLUTION: APPLICANT HAS ACTIVE COMPREHENSIVE PROGRAM TO ADDRESS THESE ISSUES. THESE ISSUES ARE ENUMERATED AS THE PRESENT STATUS OF THE DATA BASE FOR WHICH WE HAVE DOCUMENTATION.

CONCLUSION

PROSPECTS FOR SUCCESS OF THE CRBR FUEL SYSTEM JUSTIFY ISSUANCE OF CONSTRUCTION PERMIT.

HOWEVER, ABILITY TO CLEARLY DEMONSTRATE ACCEPTABILITY OF THE SYSTEM FOR AN OPERATING LICENSE WITHOUT RESORTING TO FALLBACK POSITIONS DEPENDS ON ADDRESSING IDENTIFIED ISSUES.

BASIS FOR CONCLUSIONS

- PREVIOUSLY ENUMERATED FAVORABLE FACTORS.
- ALL OF THE FOREGOING ISSUES ARE PRIMARILY RELEVANT TO THE ABILITY TO EVALUATE FUEL PERFORMANCE, NOT TO FUEL PERFORMANCE ITSELF.
- PROGRAMS ARE UNDER WAY, OR HAVE BEEN COMMITTED BY THE APPLICANT, TO RESOLVE THE ISSUES BY FSAR SUBMITTAL.
- THE AVAILABILITY OF FALLBACK POSITIONS ALLOWS DEFERRAL OF RESOLUTION TO THE FSAR.

CRBR NUCLEAR DESIGN

SER SECTION 4.3

ACRS MEETING

FEBRUARY 11, 1983

W. L. BROOKS

CORE PERFORMANCE BRANCH

USNRC

SAFETY EVALUATION REPORT

SECTION 4.3

DESIGN BASES

- PRINCIPAL DESIGN CRITERIA

DESIGN DESCRIPTION

REACTIVITY CONTROL SYSTEM

POWER DISTRIBUTIONS

REACTIVITY COEFFICIENTS

INSTRUMENTATION

CORE STABILITY

ANALYTICAL METHODS

PRINCIPAL DESIGN CRITERIA AFFECTING THE CORE

PDC 8 - REACTOR DESIGN

PDC 9 - INHERENT REACTOR PROTECTION

PDC 10 - SUPPRESSION OF REACTOR POWER OSCILLATIONS

PDC 11 - INSTRUMENTATION AND CONTROLS

PDC 18 - PROTECTION SYSTEM FUNCTION

PDC 23 - PROTECTION SYSTEM REQUIREMENTS FOR REACTIVITY CONTROL MALFUNCTIONS

PDC 24 - REACTIVITY CONTROL SYSTEM REDUNDANCY AND CAPABILITY

PDC 25 - COMBINED REACTIVITY CONTROL SYSTEMS CAPABILITY

PDC 57 - REACTIVITY LIMITS

PDC 58 - PROTECTION AGAINST ANTICIPATED OPERATIONAL OCCURRENCES

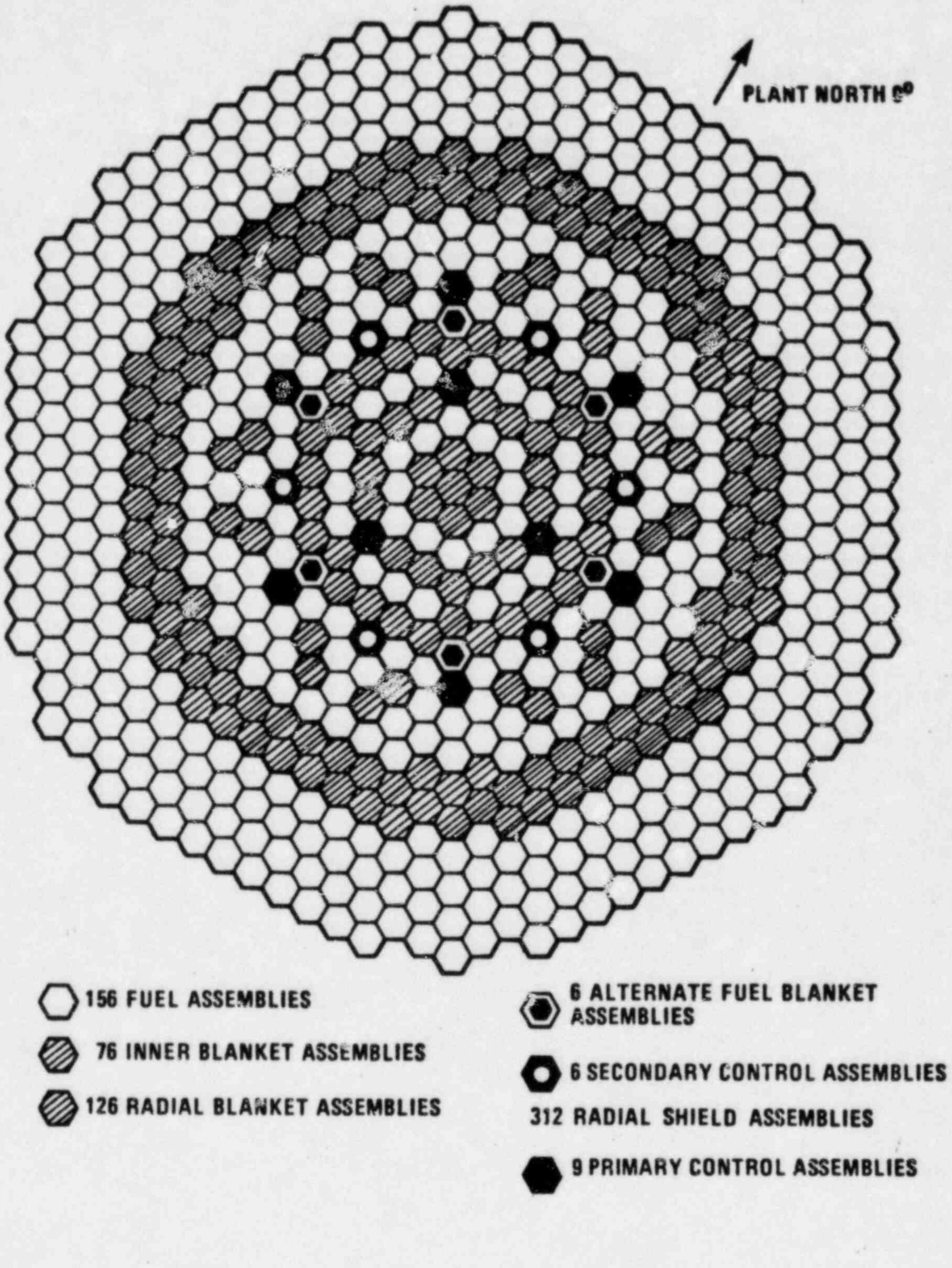


FIGURE 4.3-1. Clinch River Breeder Reactor Core Layout

REACTIVITY CONTROL SYSTEM

RELEVANT CRITERIA

- 23
- 24
- 25
- 57
- 58

DESIGN BASES

CALCULATION METHODS

RESULTS AND CONCLUSION

POWER DISTRIBUTIONS AND INSTRUMENTATION

RELEVANT CRITERIA

- 8
- 11
- 18

DESIGN BASES

CALCULATION METHODS

RESULTS AND CONCLUSIONS

REACTIVITY COEFFICIENTS AND CORE STABILITY

RELEVANT CRITERIA

- 9
- 10

DOPPLER COEFFICIENT

OTHER COEFFICIENTS

CORE STABILITY

CALCULATION METHODS

EFFECT OF BOWING

ANALYTICAL METHODS

SUMMARY OF METHODS

COMMITMENTS

CRBR REACTOR THERMAL - HYDRAULIC DESIGN

PRESENTATION TO ACRS - 2/11/83

BY T. KING - NRC

SCOPE OF STAFF REVIEW

1) THERMAL/HYDRAULIC DESIGN OF IN-VESSEL COMPONENTS, INCLUDING:
    - DESIGN CRITERIA/LIMITS
    - DESIGN METHODS
    - STEADY STATE CONDITIONS
    - TRANSIENT CONDITIONS

2) DEVELOPMENT AND STARTUP TESTING
    - STEADY STATE
    - TRANSIENT

3) REVIEW AND INDEPENDENT ANALYSIS BY:
    - BNL
    - ANL
    - BARTHOLD & ASSOC.

ACCEPTANCE CRITERIA

- CONFORMANCE WITH CRBR PRINCIPAL DESIGN CRITERIA:
  - #8 - REACTOR DESIGN
  - #60- FLOW BLOCKAGE

- CONFORMANCE WITH SRP SECTION 4.4 "THERMAL HYDRAULIC DESIGN"

- COMPLETENESS AND ADEQUACY OF APPLICANTS':
  - o DESIGN CRITERIA/LIMITS
  - o DESIGN METHODS
  - o CONCEPTUAL DESIGN

- ADEQUATE DEVELOPMENT TESTING TO SUPPORT THE DESIGN/CRITERIA/LIMITS/METHODS

- CONFIRMATION OF APPLICANTS' ANALYSIS BY SELECTED INDEPENDENT OVERCHECKS

MAJOR SAFETY FEATURES OF DESIGN

- PROVIDE FOR DECAY HEAT REMOVAL VIA NATURAL CIRCULATION
- PREVENTS SIGNIFICANT GAS ENTRAINMENT BY:
  - VENTING POTENTIAL GAS COLLECTION AREAS
  - SUPPRESSING VORTEX FORMATION AND TURBULANCE IN THE UPPER PLENUM
- MINIMIZES THE POTENTIAL FOR FLOW BLOCKAGE BY:
  - PROVIDING DISCRIMINATION FEATURES TO PREVENT ASSEMBLY PLACEMENT IN A CORE LOCATION OF HIGHER POWER THAN WHAT IT IS ORIFICED FOR.
  - PROVIDING MULTIPLE FLOW PATHS TO THE ASSEMBLY INLET NOZZLES.
  - PROVIDES CORE INLET STRAINERS WHICH WILL FILTER OUT PARTICLES LARGER THAN 1/4 INCH.
  - PROVIDING INLET NOZZLE OPENINGS WHICH ALLOW ASSEMBLY VERTICLE MOTION WITHOUT CUTTING OFF FLOW.
- PROVIDES MONITORING INSTRUMENTATION FOR CORE ASSEMBLY OUTLET TEMPERATURES.

- REQUIRES PREVENTION OF INADVERTENT CONTROL ROD FLOATATION DURING REFUELING.

- REQUIRES SUFFICIENT FLOW TO ALL PERMANENT AND REMOVEABLE CORE COMPONENTS TO MAINTAIN THEM WITHIN THEIR STRUCTURAL LIMITS FOR ALL STEADY STATE AND TRANSIENT OPERATION

- ORIFICING DESIGN WHICH PROVIDES SHIELDING TO CORE SUPPORT STRUCTURE.

ITEMS TO BE RESOLVED AS PART OF FINAL DESIGN

- PRIMARY CONTROL ROD FLOATATION
- AFFECT OF OBSERVED FFTF CORE ΔP INCREASE ON CRBR DESIGN
- AFFECT OF LATEST POWER TO MELT DATA ON CRBR FUEL DESIGN
- CORRECT METHODOLOGY FOR APPLICATION OF HCF's.

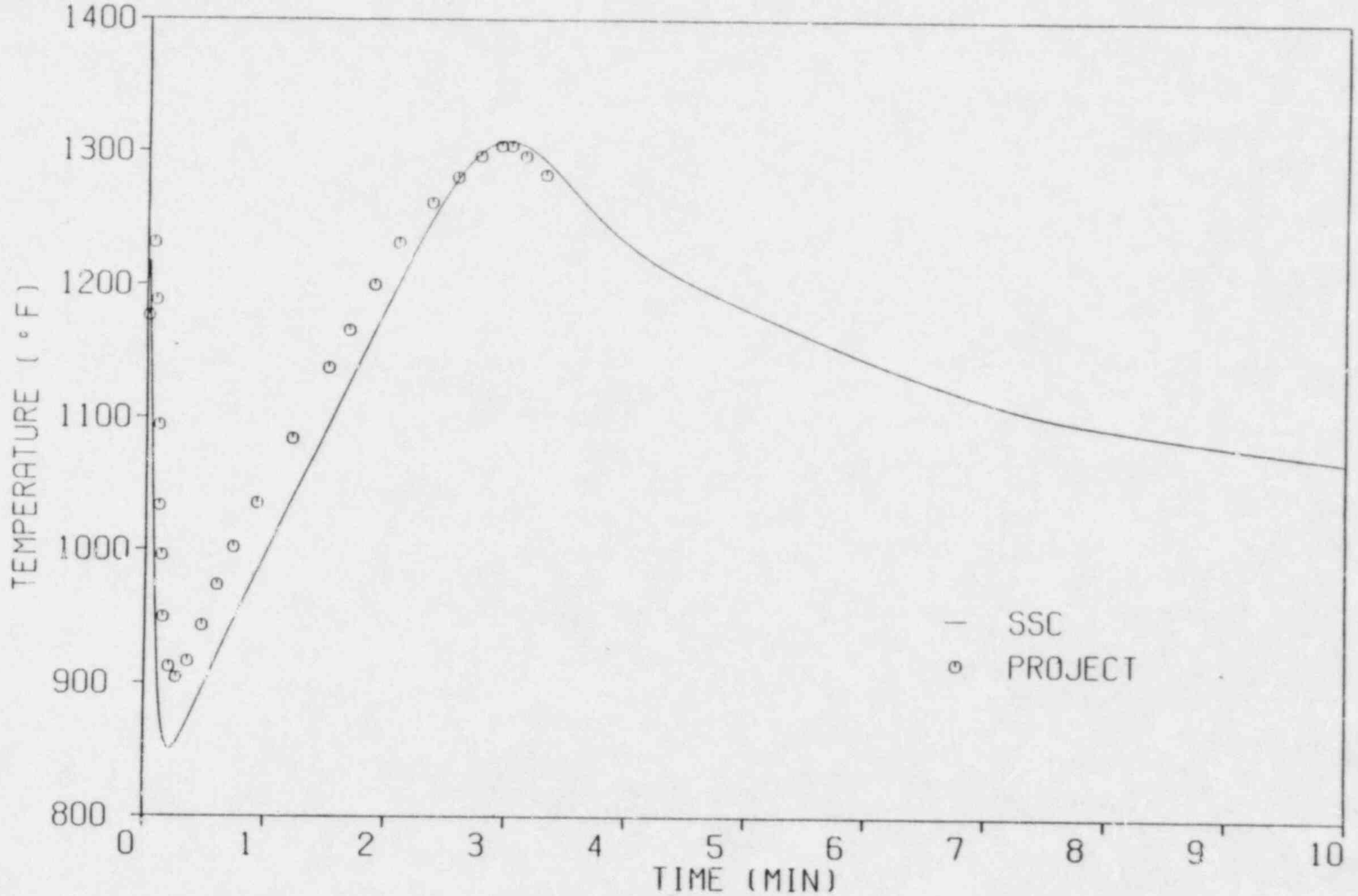
INDEPENDENT OVERCHECK ANALYSIS

- o STEADY STATE, FULL POWER CORE CONDITIONS

- o NATURAL CIRCULATION:
  - COMPARISON WITH APPLICANTS' BASE CASE
  - COMPARISON WITH FFTF RESULTS

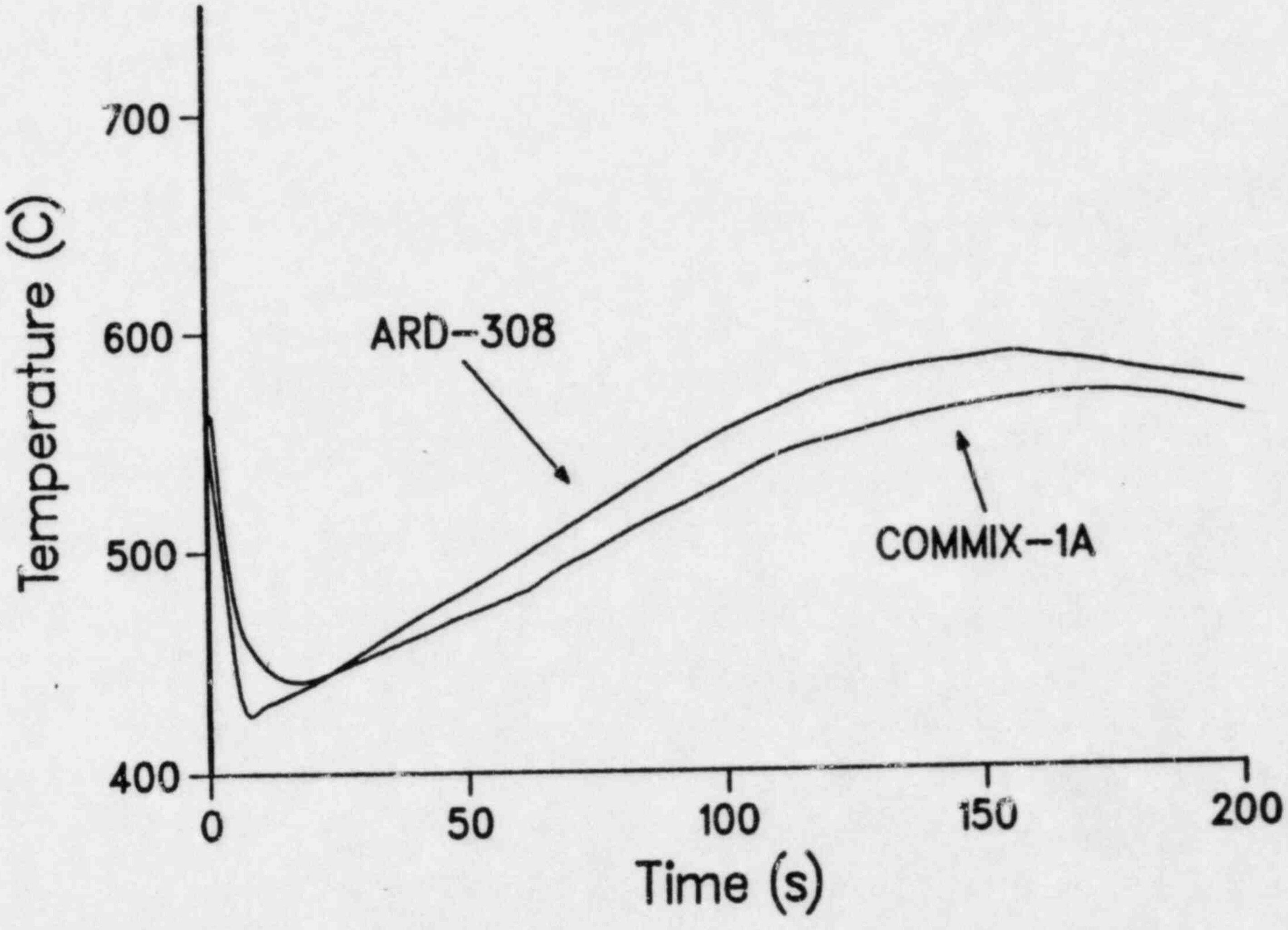
- o DHRS:
  - COMPARISON WITH APPLICANTS' BASE CASE
  - COMPARISON WITH FFTF DATA

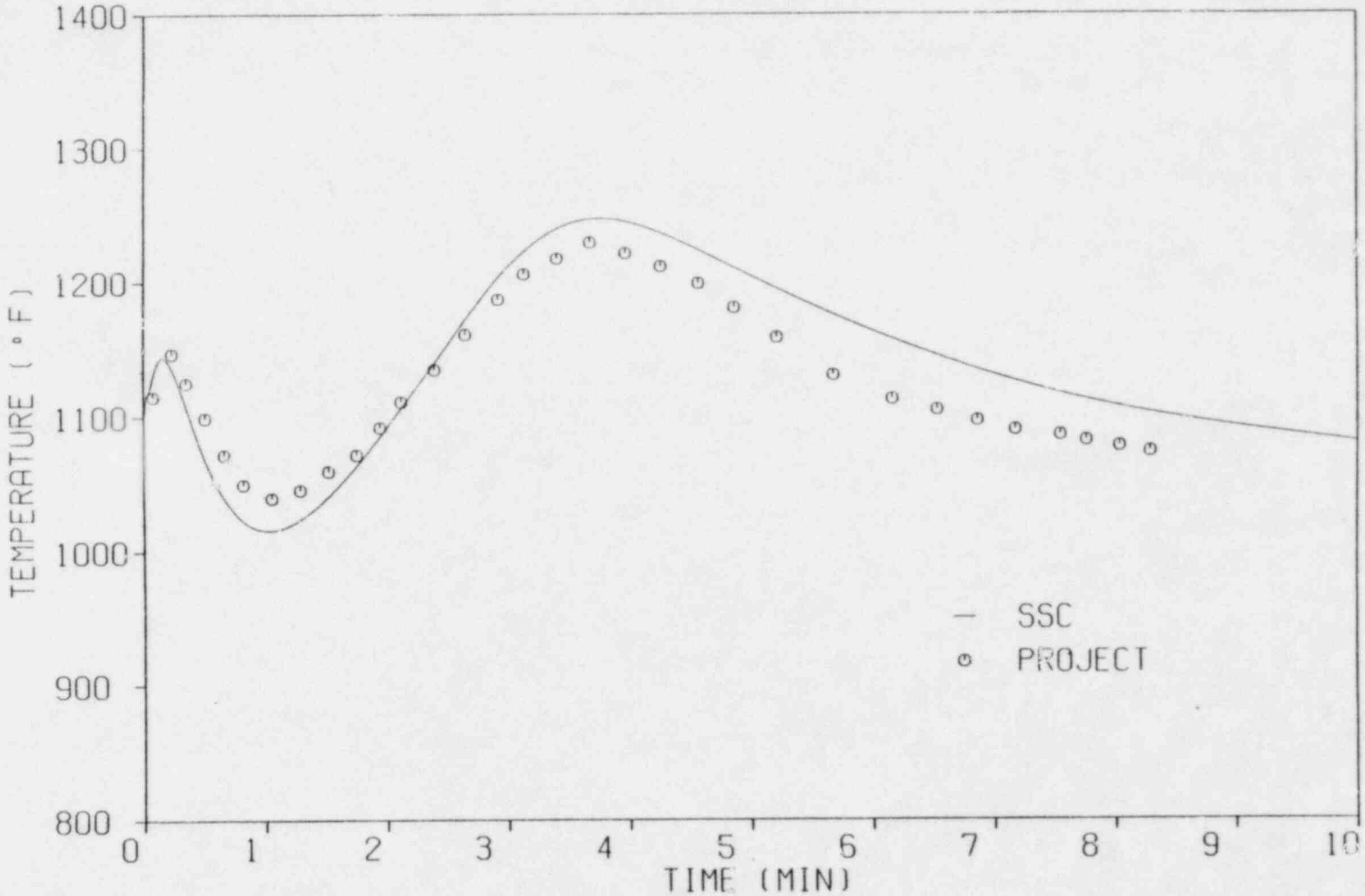
- o FOLLOW ON WORK IN SUPPORT OF OL REVIEW:
  - SENSITIVITY STUDY ON BASE CASE NATURAL CIRCULATION CALCULATIONS
  - SENSITIVITY STUDY ON BASE CASE DHRS CALCULATIONS
  - ANALYSIS OF NATURAL CIRCULATION TRANSIENT FROM REFUELING CONDITIONS



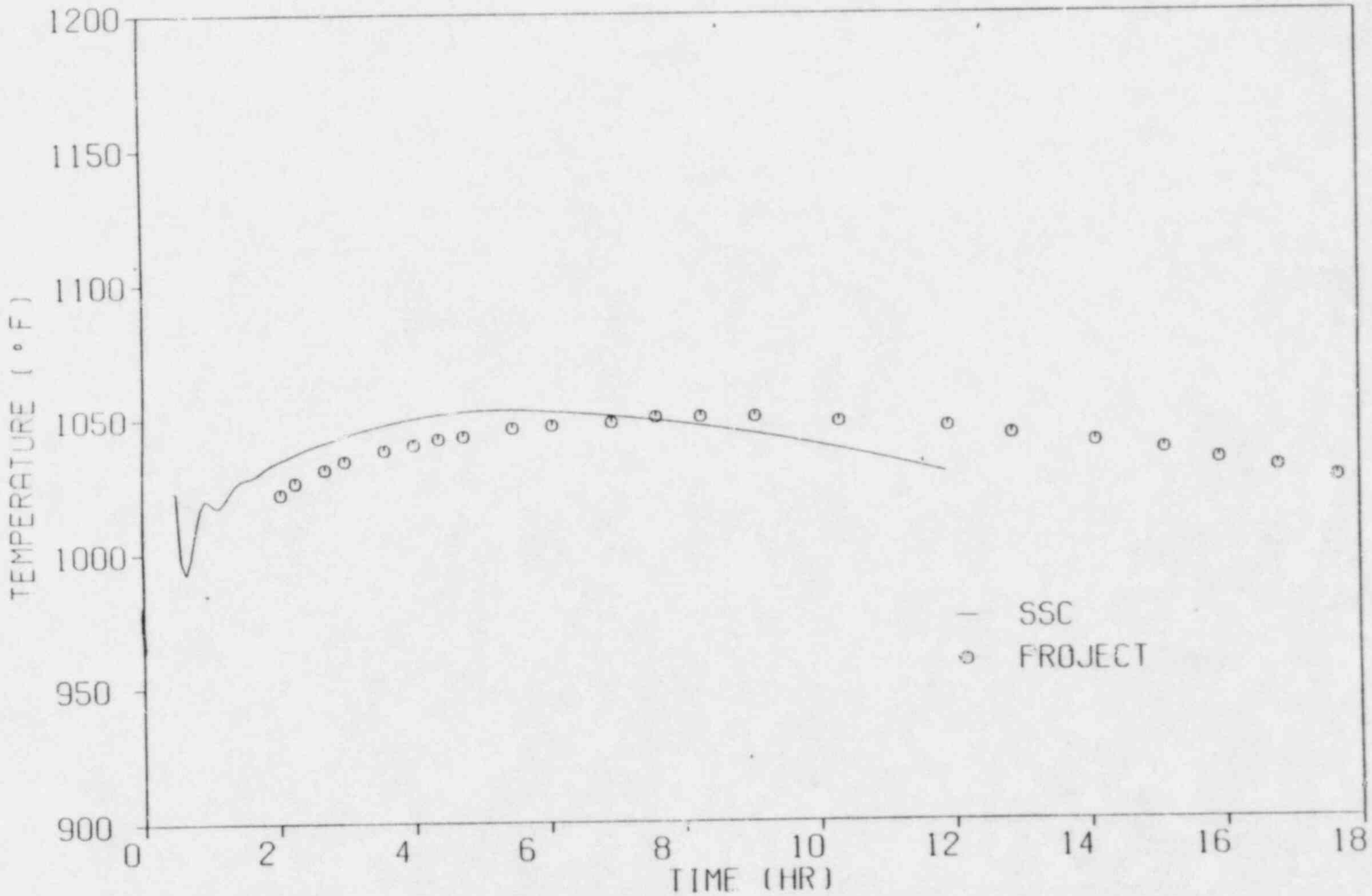
STATION BLACKOUT - COOLANT TEMPERATURE FOR HOT FUEL PIN

Fuel Assembly Outlet Temperature

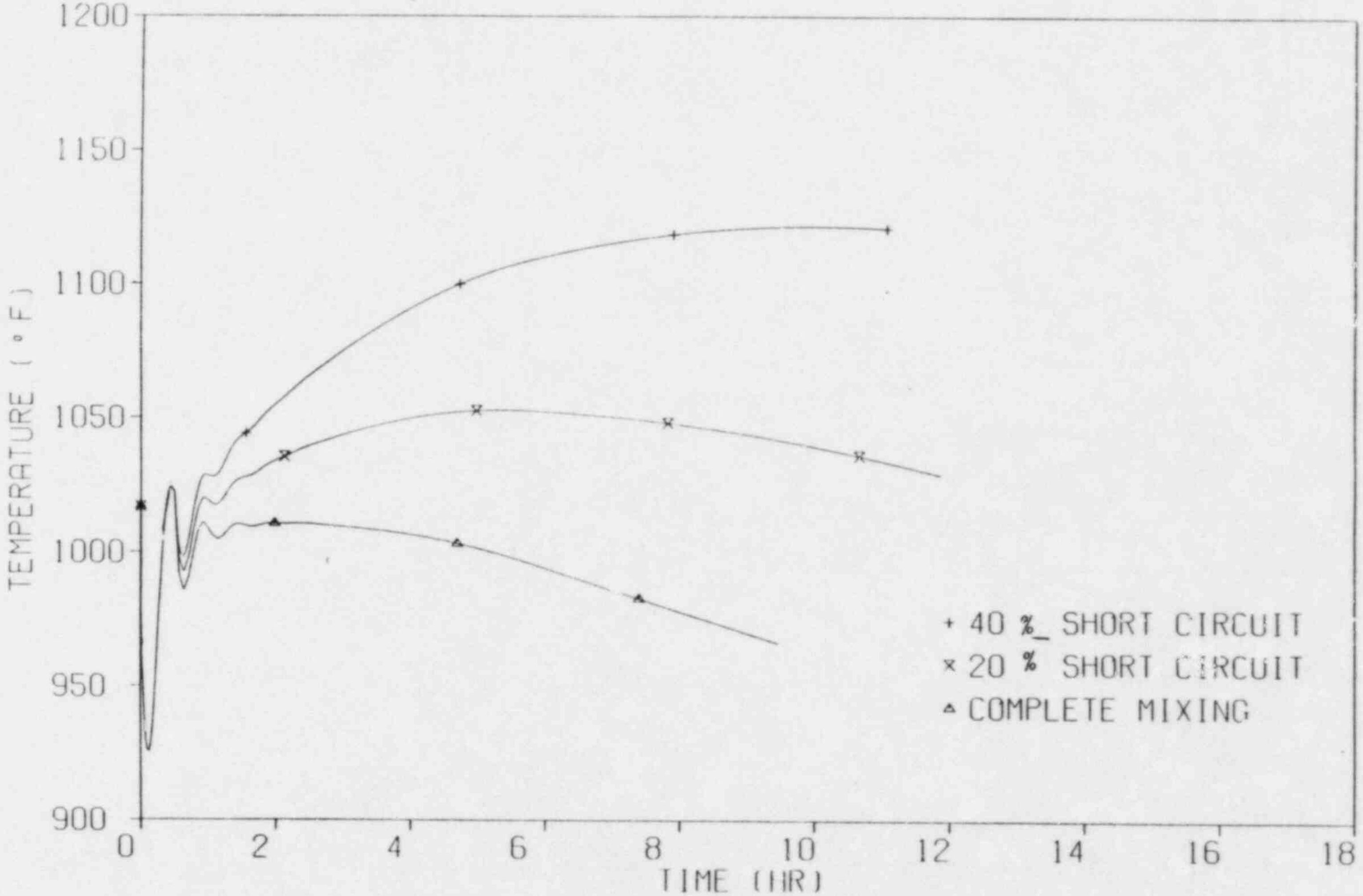




STATION BLACKOUT - COOLANT TEMPERATURE FOR HOT INNER BLANKET PIN



DHRS DESIGN BASIS ACCIDENT - BULK UPPER PLENUM SODIUM TEMPERATURE



DHRS EVENT - SENSITIVITY OF BULK UPPER PLENUM SODIUM TEMPERATURE TO VARIATIONS IN "SHORT CIRCUIT" FLOW FRACTION

CONCLUSION

- DESIGN HAS HIGH PROBABILITY MEETING CRITERIA.
- FALLBACK OF REDUCED POWER, FLOW OR BURNUP EXIST IF COMPLICATIONS ARISE DURING FINAL DESIGN.
- THEREFORE, STAFF CONSIDERS DESIGN ACCEPTABLE FOR CP.

FACTORS WHICH INDICATE DESIGN IS ACCEPTABLE FOR CP

- o INCORPORATES FEATURES TO:
  - MINIMIZE THE POTENTIAL FOR FLOW BLOCKAGE
  - PREVENT SIGNIFICANT GAS ENTRAINMENT
  - MONITOR ASSEMBLY OUTLET TEMPERATURES
  - ALLOWS DECAY HEAT REMOVAL VIA NATURAL CIRCULATION
- o FLOW DISTRIBUTION, ΔP's FRICTION FACTORS, GAS ENTRAINMENT ARE SUPPORTED BY EXTENSIVE WATER AND SODIUM DEVELOPMENT TESTING.
- o FFTF FUEL DESIGN AND IN-VESSEL THERMAL HYDRAULICS DESIGN IS SIMILAR TO CRBR AND CONTINUED FFTF OPERATION WILL PROVIDE DATA DIRECTLY APPLICABLE TO CRBR.
- o STAFF's INDEPENDENT CALCULATIONS INDICATE THAT APPLICANTs' DESIGN METHODS PROVIDE A REASONABLE ESTIMATE OF SYSTEM PERFORMANCE.
- o APPLICANT HAS COMMITTED TO TESTING DURING INITIAL STARTUP TO:
  - CONFIRM NATURAL CIRCULATION PREDICTION
  - CONFIRM DHRS PERFORMANCE
  - MEASURE SELECTED IN-VESSEL TEMPERATURE AND VIBRATIONS.
- o PRELIMINARY SAFETY ANALYSIS HAS BEEN DONE WITH CONSERVATIVE ASSUMPTIONS:

- ADDITIONAL DECAY HEAT
- HCF
- ADDITIONAL ΔP
- THDV + 20 F CONDITIONS FOR PERMANENT COMPONENTS
- UPPER BOUND ON PHYSICAL PROPERTIES
- ETC.

CRBRP REACTOR DESIGN

Presented To ACRS
Washington, DC
February 11, 1983

Westinghouse Electric Corporation
Advanced Reactors Division
Madison, PA 15663

ARD

8254-1

REACTOR TOPICS

1.	Introduction	Mr. Robert M. Vijuk Manager, Nuclear Systems Engineering
2.	Reactor Vessel and Internals Design	Dr. Frank J. Baloh Manager, Reactor Enclosure and Lower Internals
3.	Core Nuclear Design	Mr. Richard A. Doncals Manager, Nuclear Analysis
4.	Core Thermal and Hydraulic Design	Mr. Robert A. Markley Manager, Thermal and Fluid System Engineering
5.	Fuel and Blanket Design	Mr. Ambrose L. Schwallie Manager, Fuel and Removable Assembly Design

REACTOR VESSEL AND INTERNALS DESIGN

WARD

8254-3

PRESENTATION OUTLINE

0 GENERAL OVERVIEW

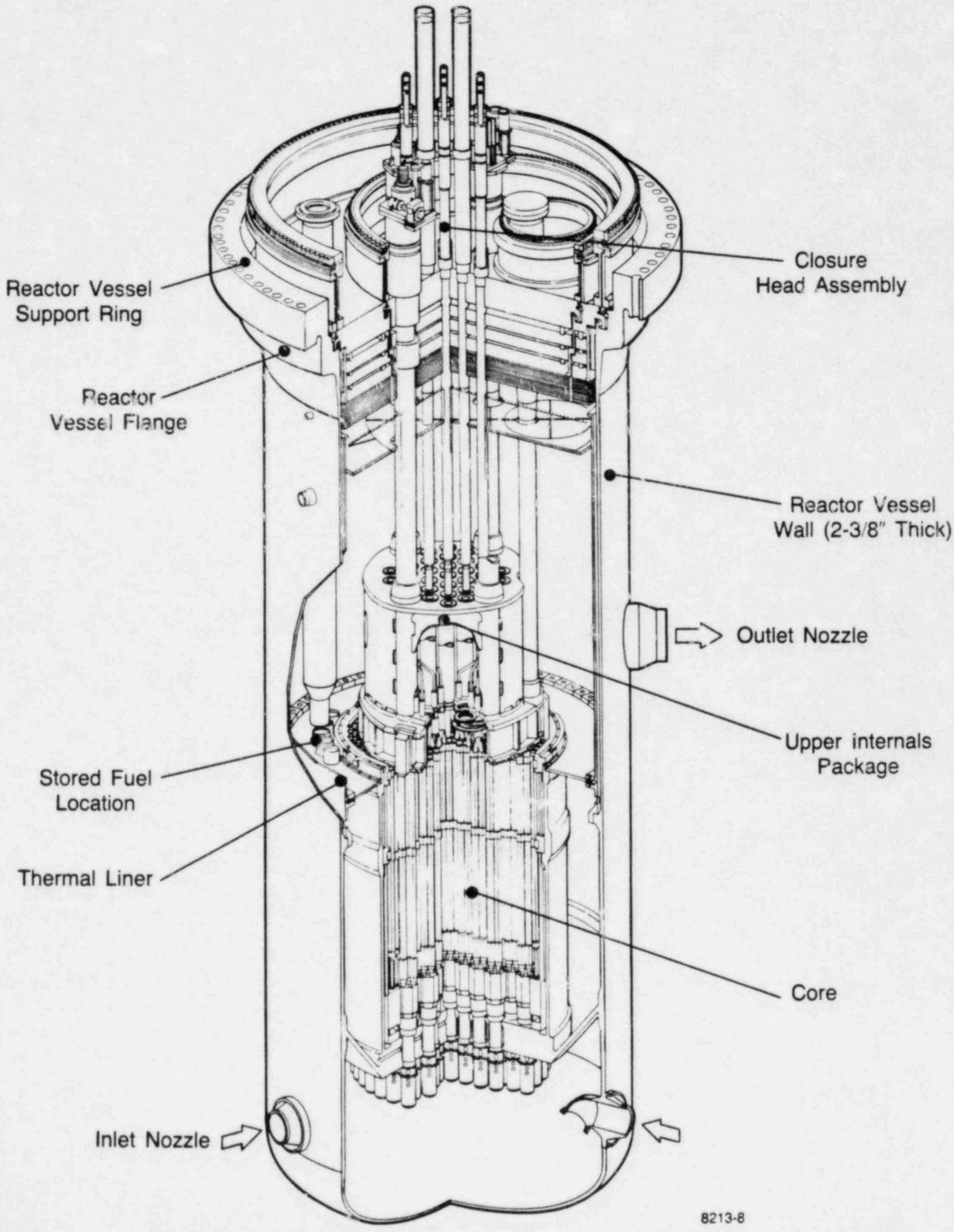
- CLOSURE HEAD (5 MIN)
- REACTOR VESSEL (5 MIN)
- LOWER INTERNALS (5 MIN)

0 UIS DESIGN (15 MIN)

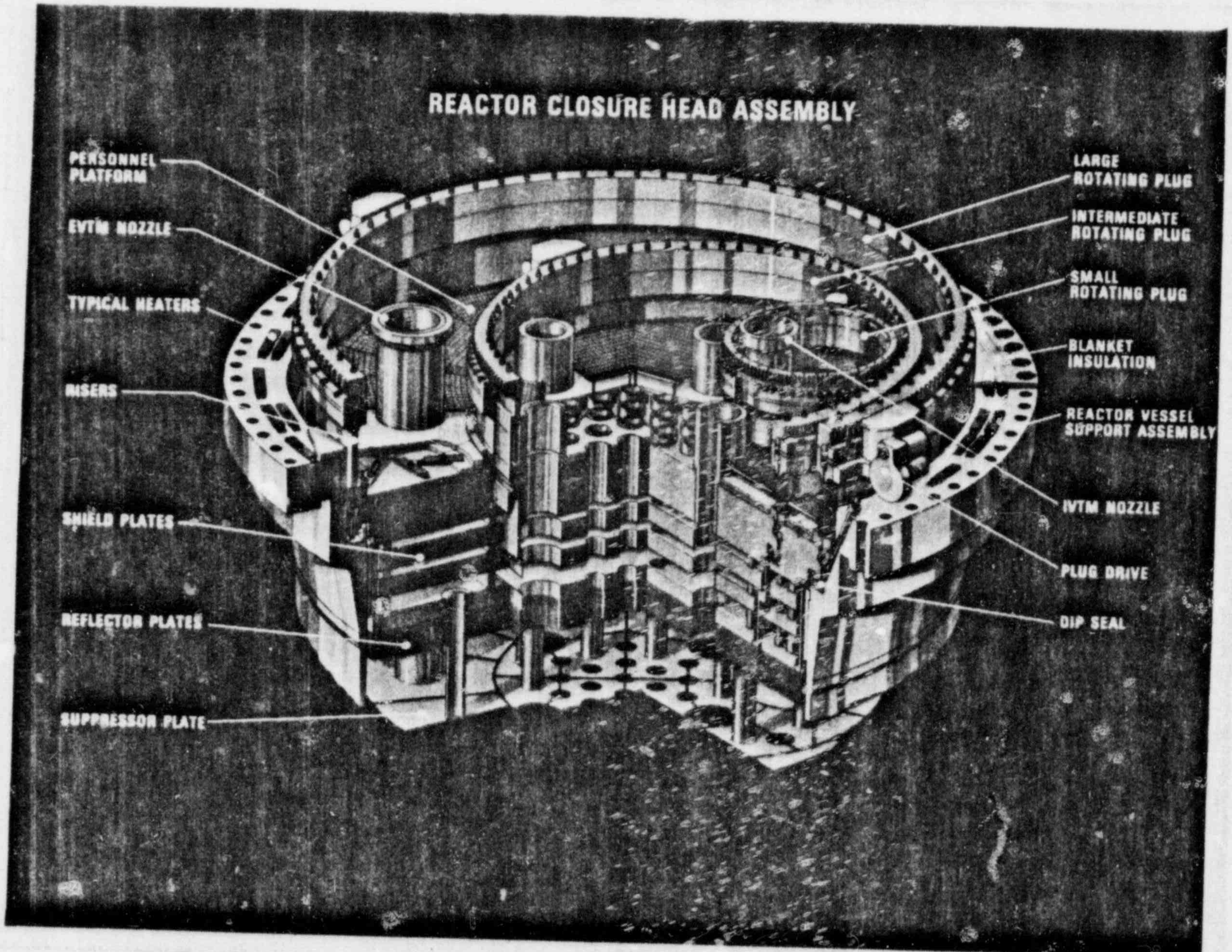
- FUNCTIONAL REQUIREMENTS
- DESIGN DESCRIPTION
- PERTINENT DESIGN CONSIDERATIONS

ⓌARD

CRBRP REACTOR VESSEL

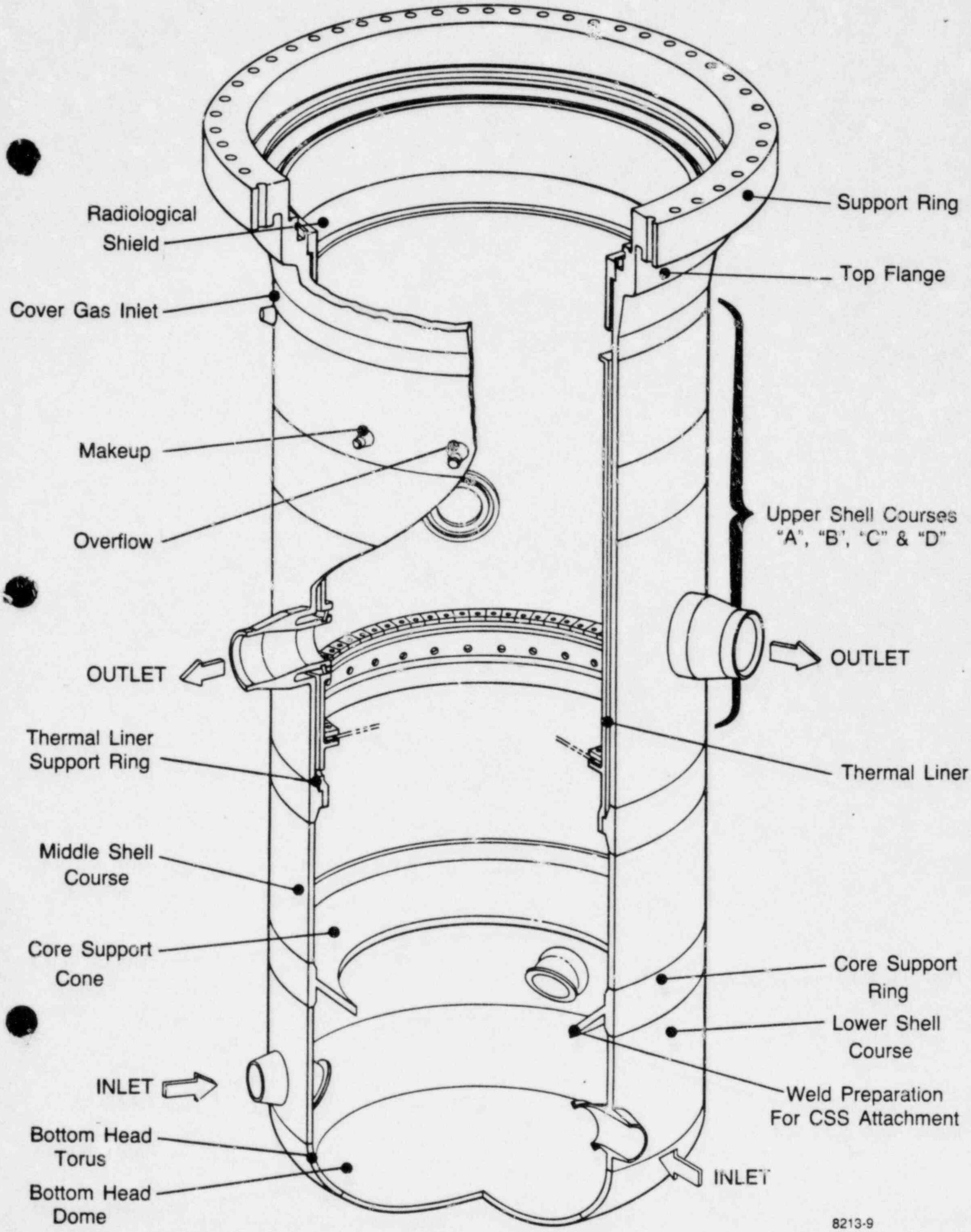


8213-8


REACTOR CLOSURE HEAD ASSEMBLY
PERSONNEL PLATFORM
EVTM NOZZLE
TYPICAL HEATERS
RISERS
SHIELD PLATES
REFLECTOR PLATES
SUPPRESSOR PLATE
LARGE ROTATING PLUG
INTERMEDIATE ROTATING PLUG
SMALL ROTATING PLUG
BLANKET INSULATION
REACTOR VESSEL SUPPORT ASSEMBLY
IVTM NOZZLE
PLUG DRIVE
DIP SEAL

REACTOR VESSEL

ⓌARD



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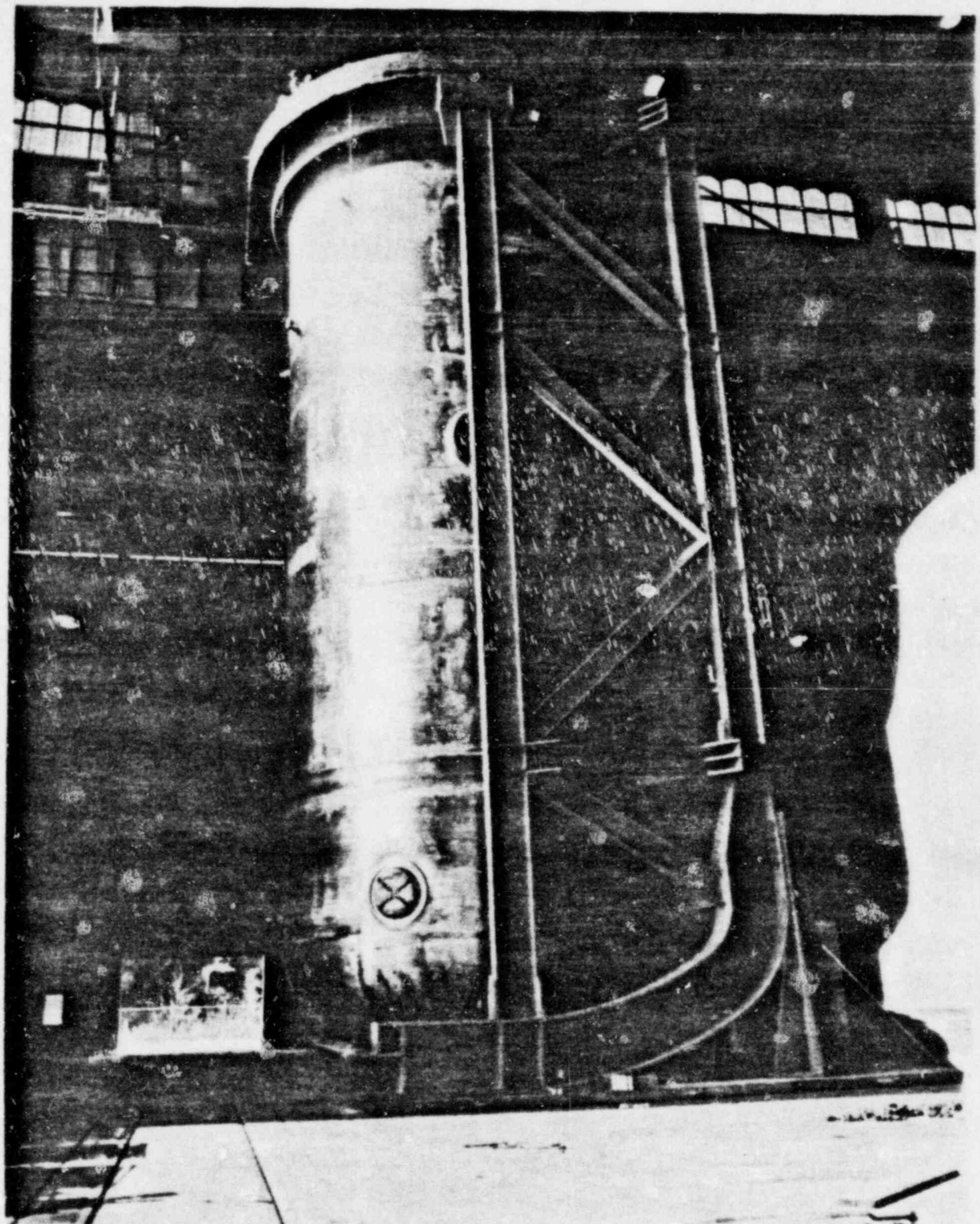
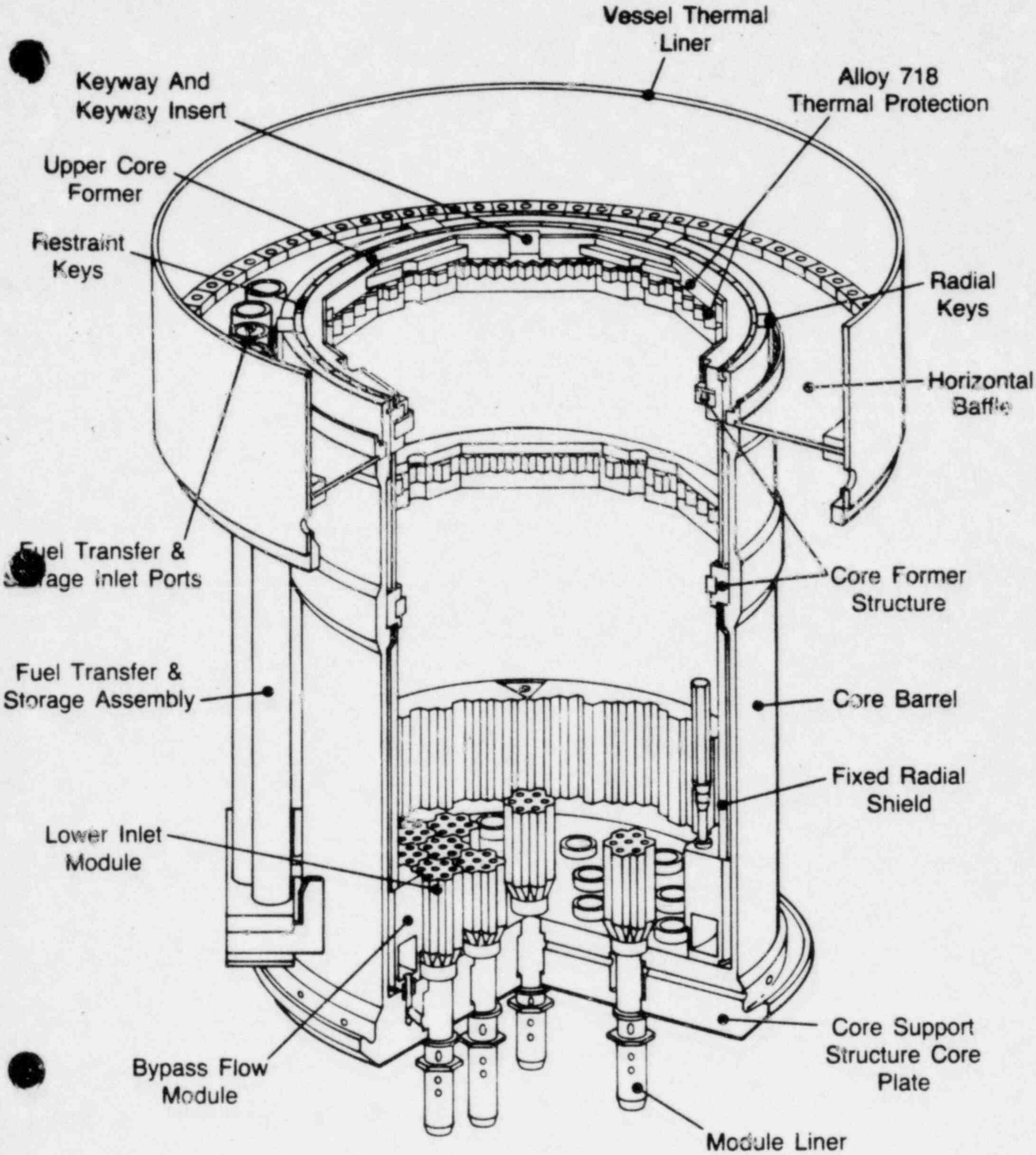


LOWER INTERNALS SYSTEM CORE SUPPORT STRUCTURE

ARD

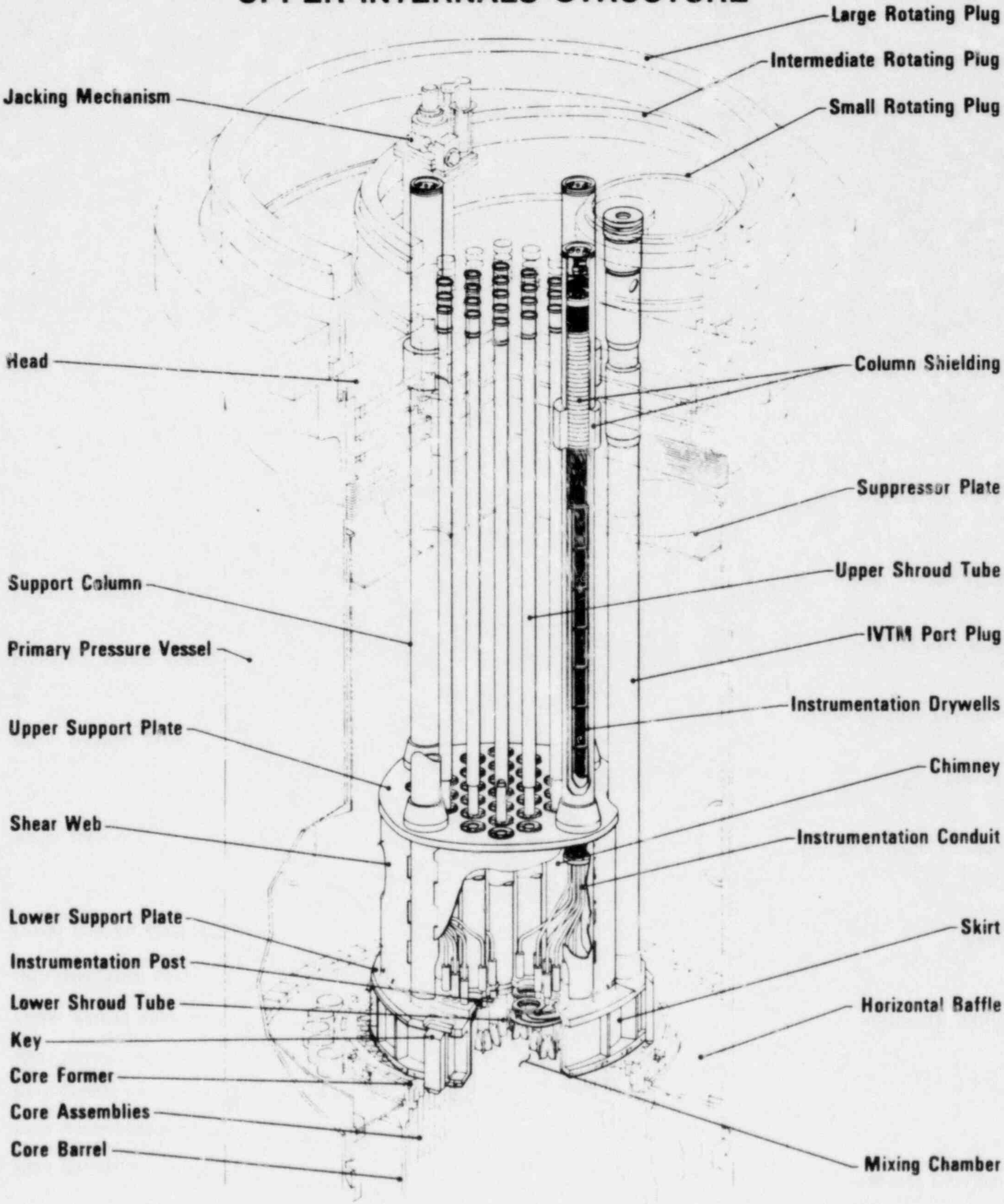


8213-7

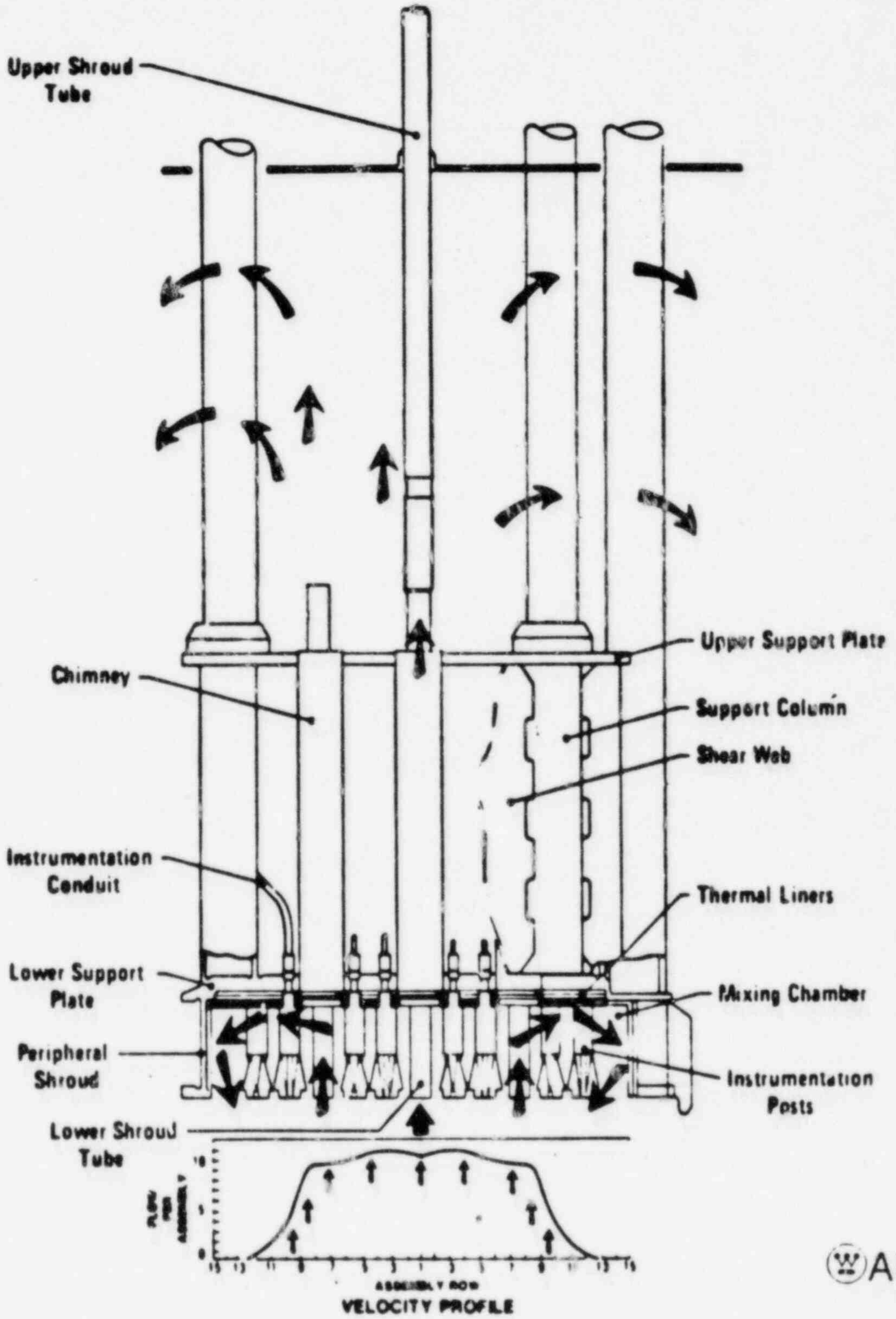
WHAT DOES IT DO?
UIS FUNCTIONAL REQUIREMENTS

- Maintain control rod alignment with core
- Provide cross flow protection for control rod drivelines
- Mix core outlet flow
- Mitigates transients in PHTS hotleg
- Provide secondary core holddown
- Position and support above-core instrumentation

UPPER INTERNALS STRUCTURE



ELEVATION SCHEMATIC OF THE UPPER INTERNALS STRUCTURE



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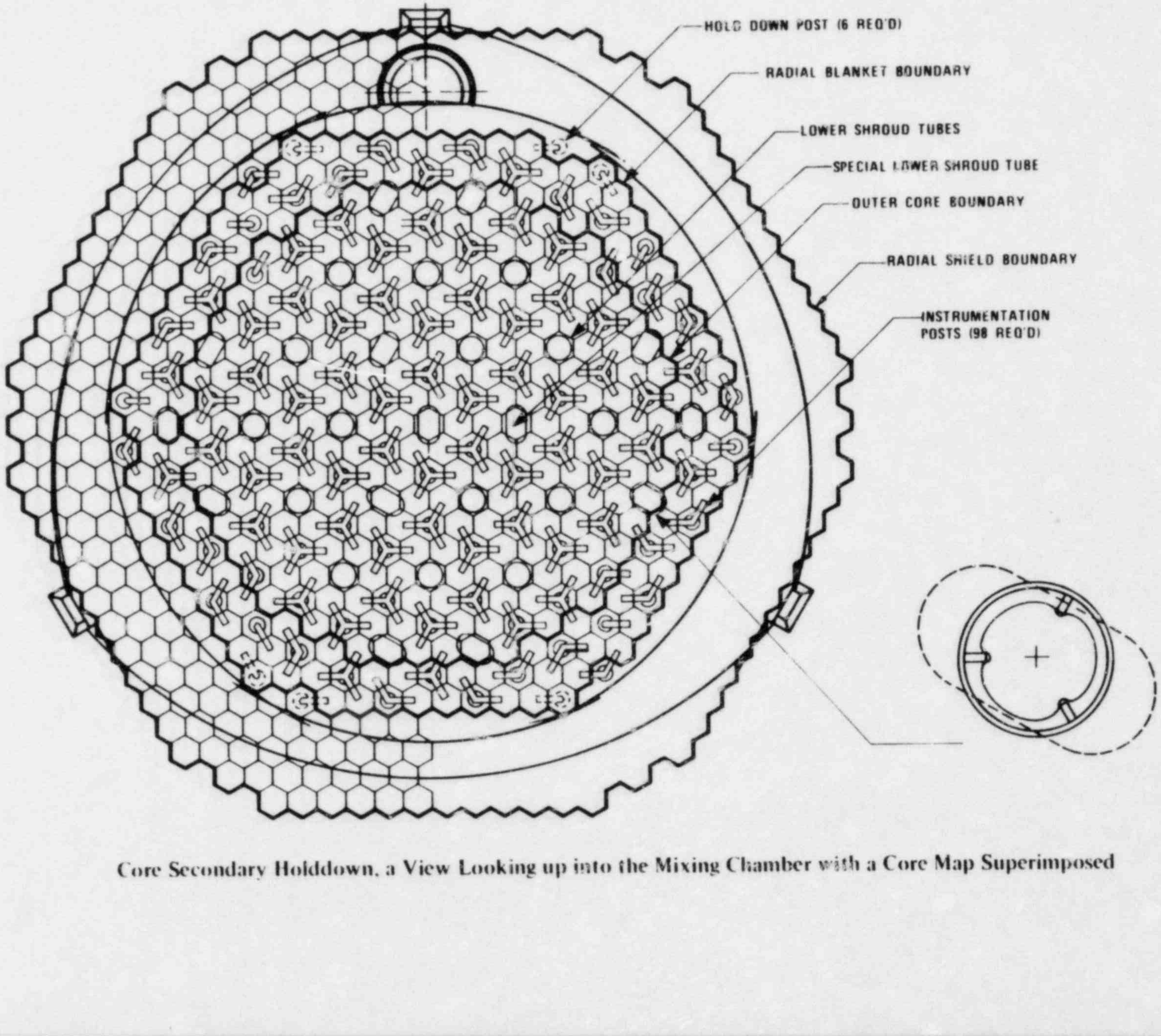
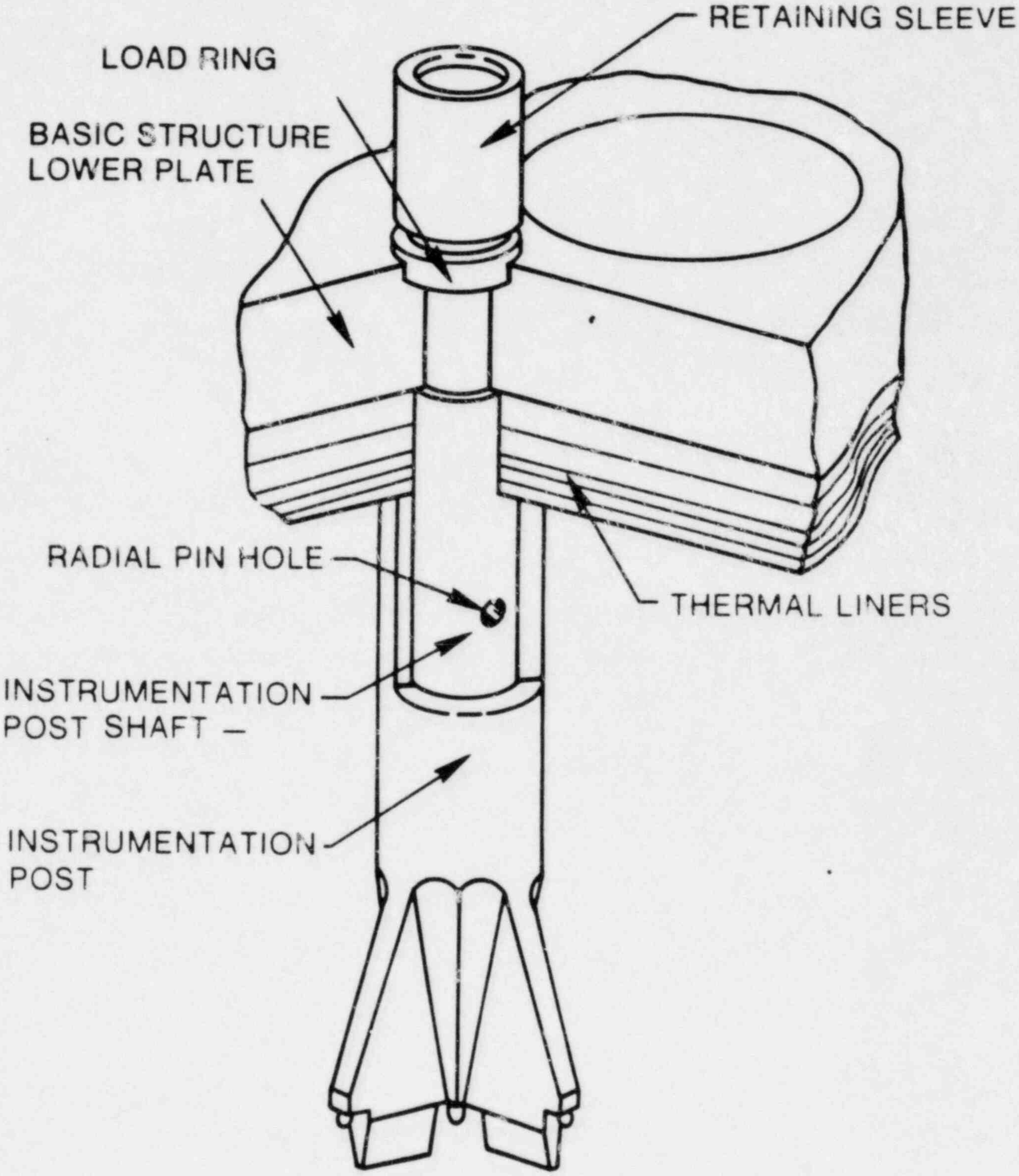
9299

WARD



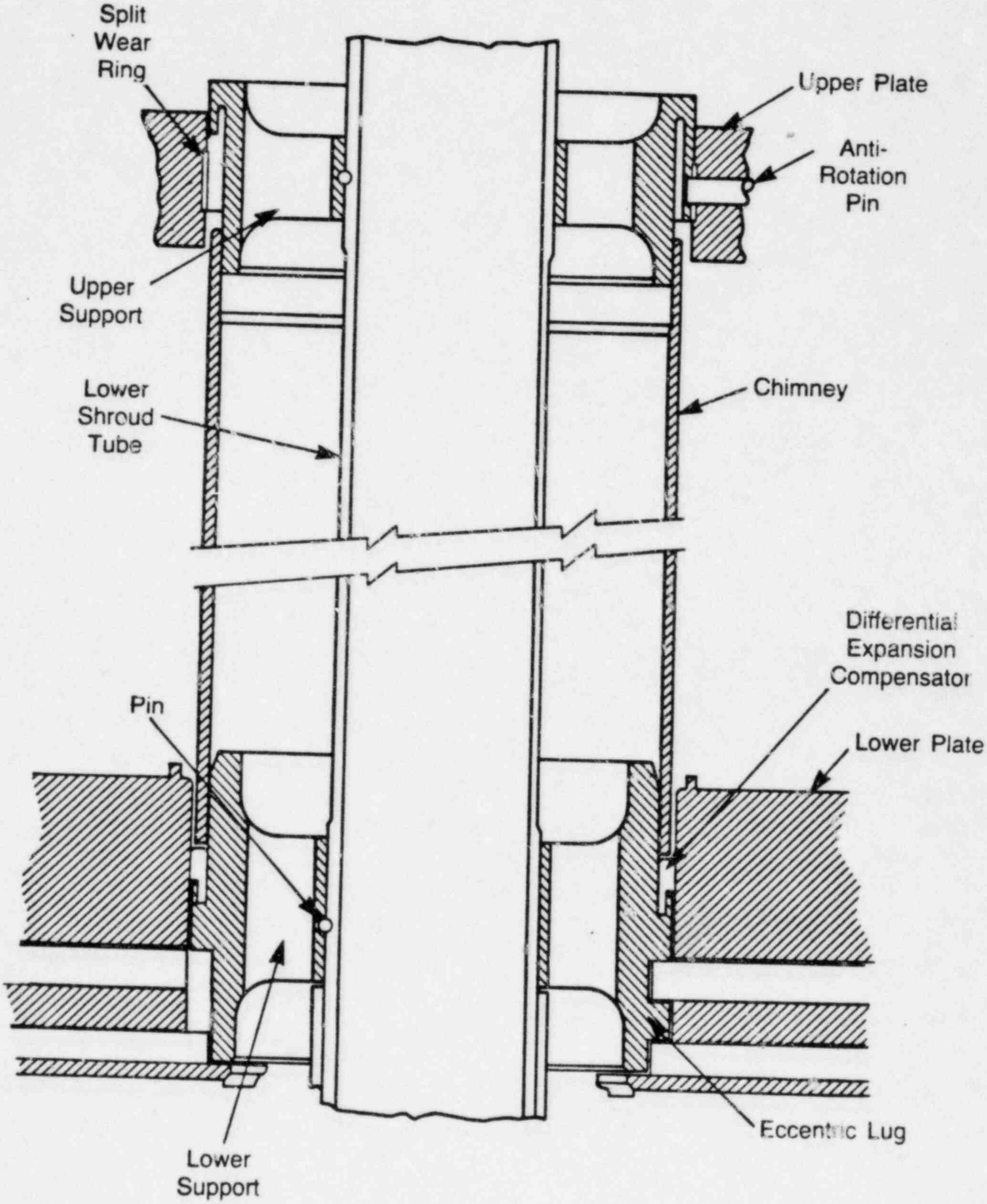
Core Secondary Holddown, a View Looking up into the Mixing Chamber with a Core Map Superimposed

3854-7


RETAINING SLEEVE
LOAD RING
BASIC STRUCTURE LOWER PLATE
RADIAL PIN HOLE
THERMAL LINERS
INSTRUMENTATION POST SHAFT
INSTRUMENTATION POST

UIS CHIMNEY MECHANICAL SUPPORT BETWEEN THE LOWER AND UPPER PLATES

ⓌARD



8213-4

Figure 10

UIS MATERIALS SELECTION

ⓌARD

- 316 stainless steel
  1. Compatibility with liquid sodium
  2. Well developed fabrication technology
  3. Creep rupture allowable superior to that for 304 SS
- Alloy 718
  1. Compatibility with liquid sodium
  2. High fatigue strength at high cycles (10^8 to 10^9) at high temperature
  3. Commercially available ;in product forms required
  4. Essentially no cobalt
  5. Adequate material property data base
  6. Fabricable into required configurations

8213-5

HANDOUT

MATERIAL

ONLY

REACTOR T&H DEVELOPMENT TESTING

INLET REGION

TEST TITLE	SUPPORTING INFORMATION	STATUS
• HEDL Inlet Plenum Feature Test - 1/4 Scale	Characterization of Inlet Plenum and Lower Internals T&H Performance	Completed
• HEDL Inlet Plenum Feature Model Particle Mobility and Bubble Dispersion Tests	Particle Transport and Bubble Breakup Characteristics	Completed
• WARD Inlet Plenum Feature Test - 1/21 Scale	Visualization of Inlet Plenum Flow Patterns - Determination of Mixing and Transport Times	Completed
• HEDL Piston Ring Leakage Tests	Piston Ring Leakage Rates	Completed
• WARD LIM Orificing Tests	Flow Control to Blanket Assemblies, Removable Radial Shields and Bypass	80% Completed
• WARD LIM Characterization Tests	Flow Distribution and Pressure Drop in LIM	Completed
• WARD LIM Orifice Life Test	Orifice Lifetime Characteristics	Completed
• HEDL RRSA Orifice Tests in Water	Characterization of Orifice Plates	Completed

REACTOR T&H DEVELOPMENT TESTING

OUTLET REGION

TEST TITLE	SUPPORTING INFORMATION	STATUS
• HEDL Integral Reactor Flow Model, Outlet Plenum Feature Flow and Vibration Test - Phase I Testing	Plenum Velocity Patterns, Mixing and ΔP Characteristics, Vibration, Gas Entrainment and Striping	Completed
• HEDL Integral Reactor Flow Model, Outlet Plenum Feature Flow and Vibration Test - Phase II Testing	Hydraulic and Vibration Characteristics of Upper Internals	Hydraulic Completed Vibration In Fabrication
• BCL Outlet Plenum Stratification Test	Flow Distribution and Temperature Response to Transient Operation	Completed
• ANL 1/10 Scale Outlet Plenum Tests	Temperature Distribution and Response at Steady State and Transient Operation	Completed
• ANL 1/15 Scale Outlet Plenum Tests	Transient Tests in Water and Sodium	Completed
• ANL Chimney Vibro-Impact Tests	Full-Scale Flow Induced Vibration of UIS Chimney	80% Completed
• HEDL Fuel Transfer and Storage Assembly	Heat Transfer Characteristics of Stored Fuel Assembly	Completed

REACTOR T&H DEVELOPMENT TESTING

STRIPING TESTS

TEST TITLE	SUPPORTING INFORMATION	STATUS
• HEDL IRFM Striping Tests	Striping Data on: Chimney and Instrument Post, Control Rod Shroud Tube, Upper Internals Structure and Bypass, Removable Radial Shield, Blanket and Fuel Nozzles, Core Barrel, Former Rings, Horizontal Baffle, Liner and Suppressor Plate, Outlet Nozzles, Etc.	Completed
• ANL Striping Tests	Striping Data on: Mixing Tees,	Completed
	Seven Nozzle Assembly with Portion of Upper Internals	In Progress
• WARD Striping Tests	Striping Data on: Seven Assembly Outlet Nozzle Feature Test,	Completed
	Seven Assembly Outlet Nozzles Test,	80% Completed
	Local Interstitial Flow Striping Test,	Completed
	Interstitial Flow-Water Table Tests,	Completed
	Thermal Striping Tests in Sodium - Dunk and Rotating Cylinder	Completed

CORE NUCLEAR DESIGN

ⓌARD

8254-4

CRBRP NUCLEAR DESIGN

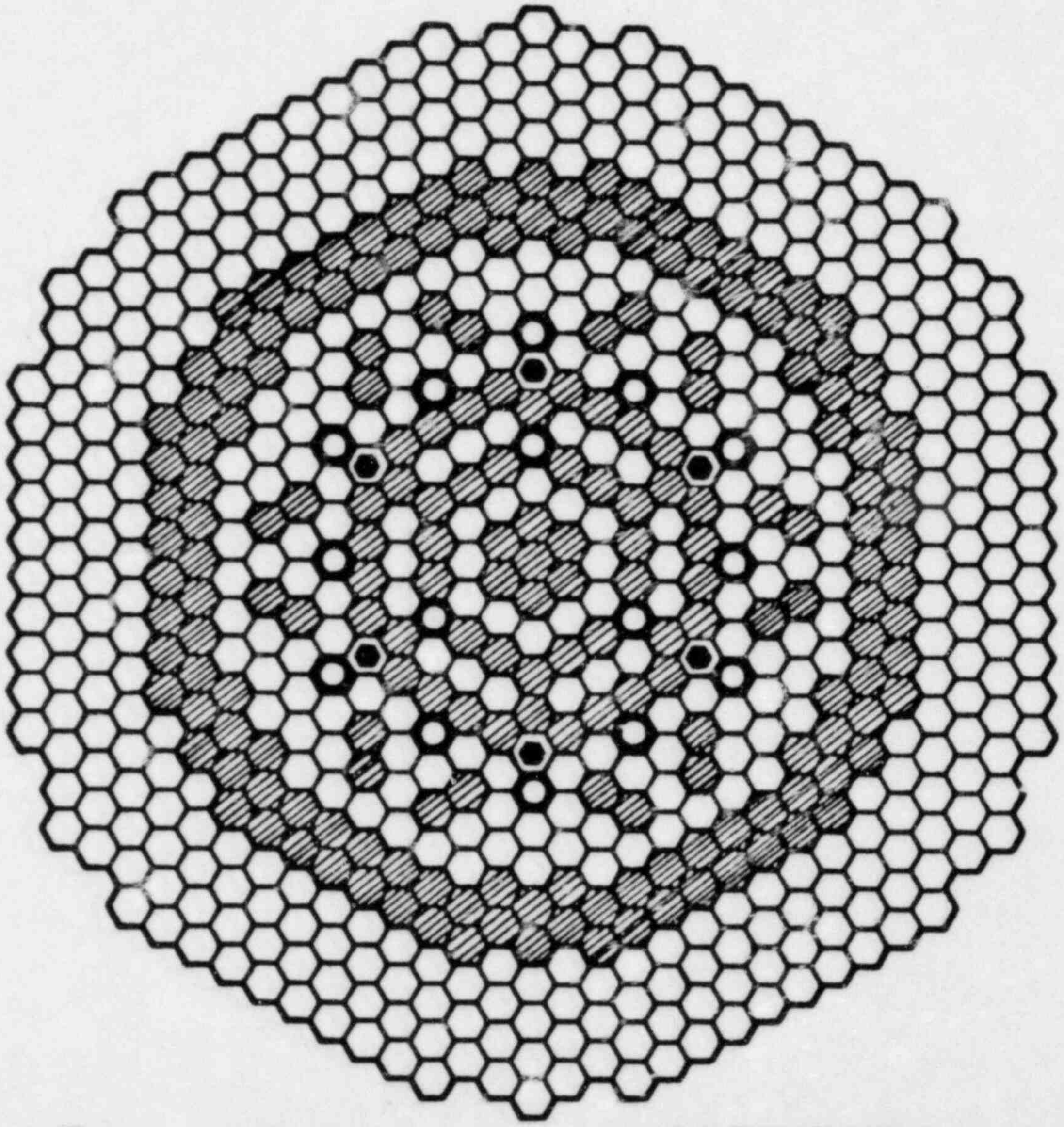
Outline

- Reactor description and design basis
- Critical experimental program
- Reactor design areas supported by critical experiments
- Summary

8255-2

CLINCH RIVER BREEDER REACTOR CORE LAYOUT

WARD



156 FUEL ASSEMBLIES

76 INNER BLANKET ASSEMBLIES

126 RADIAL BLANKET ASSEMBLIES

6 ALTERNATE FUEL BLANKET ASSEMBLIES

15 CONTROL ASSEMBLIES

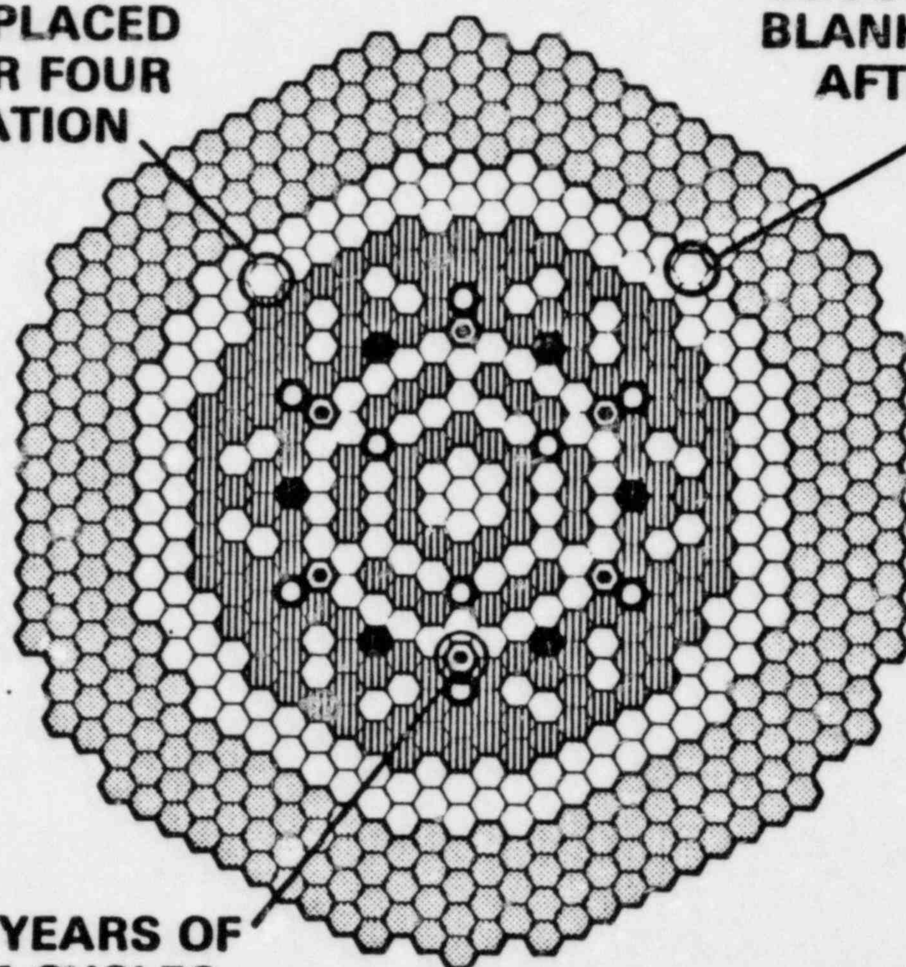
312 RADIAL SHIELD ASSEMBLIES

CRBRP FUEL MANAGEMENT

WARD

FIRST ROW OF RADIAL BLANKETS ARE REPLACED AS A BATCH AFTER FOUR YEARS OF OPERATION

SECOND ROW OF RADIAL BLANKETS ARE REPLACED AFTER FIVE YEARS OF OPERATION



IN ALTERNATING YEARS OF THE EQUILIBRIUM CYCLES, THESE SIX INNER BLANKET ASSEMBLIES ARE REPLACED WITH SIX FRESH FUEL ASSEMBLIES

ALL FUEL AND INNER BLANKET ASSEMBLIES REPLACED AS A BATCH AT TWO YEAR INTERVALS

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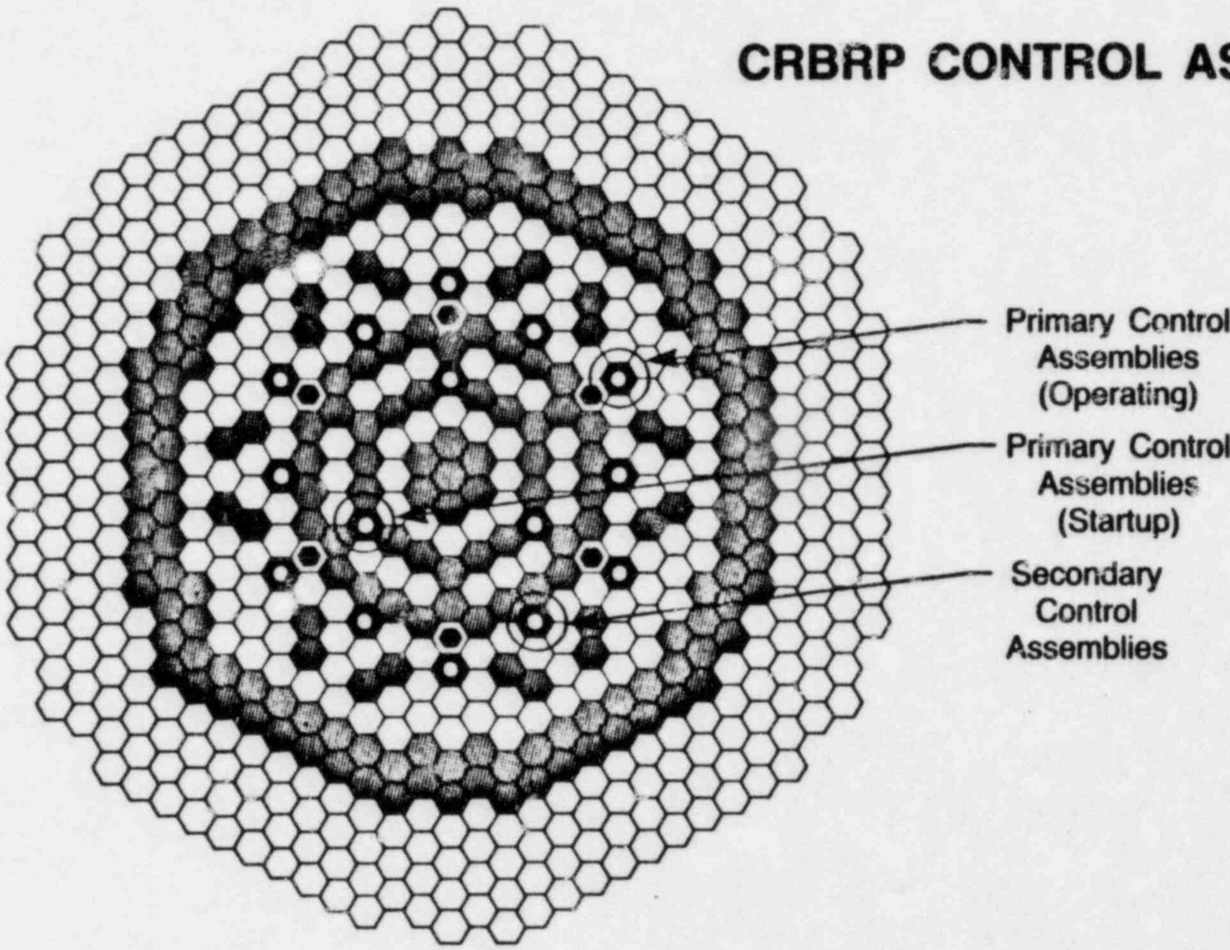
CRBRP CONTROL ASSEMBLIES



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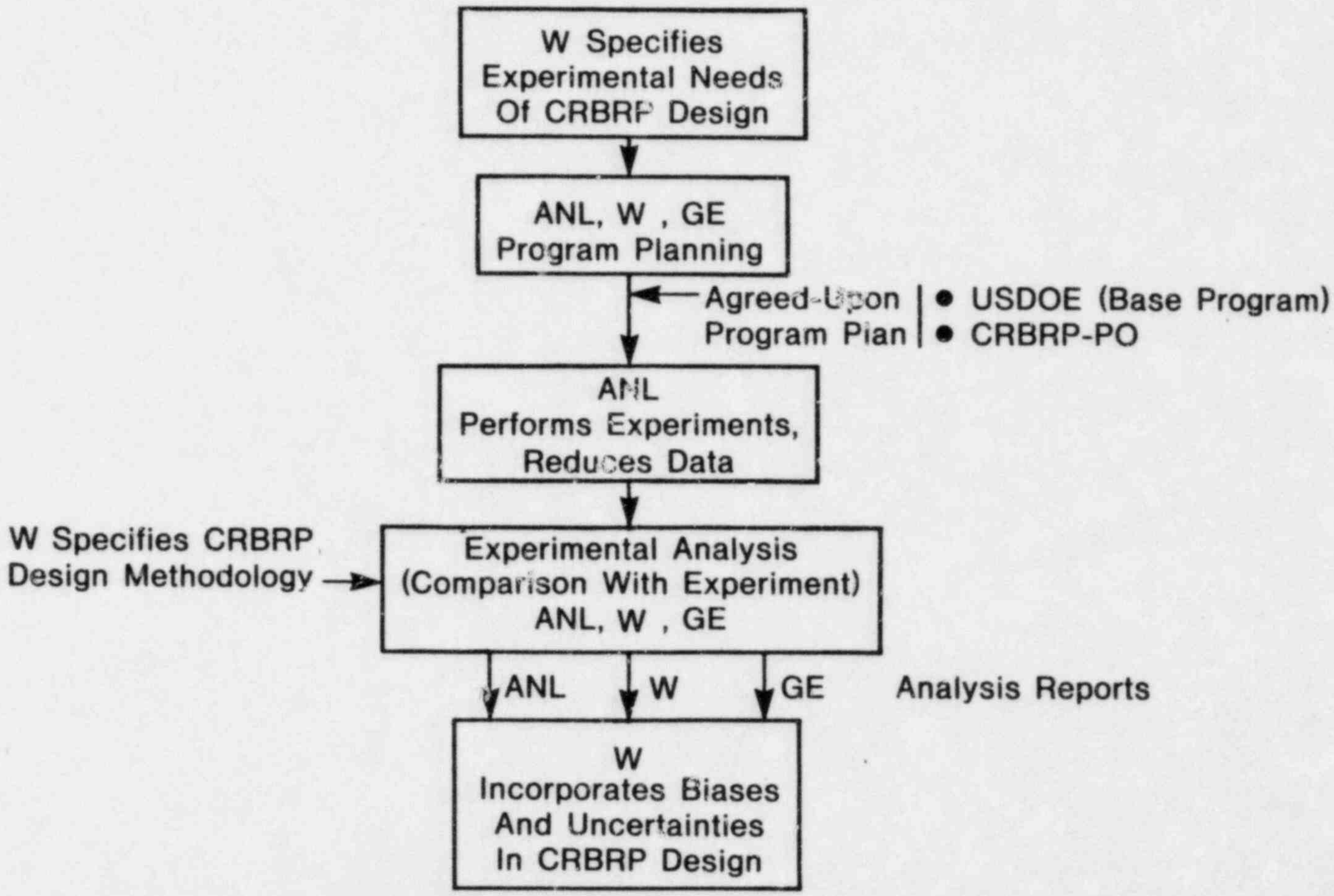
CRBRP NUCLEAR EXPERIMENTAL PROGRAM



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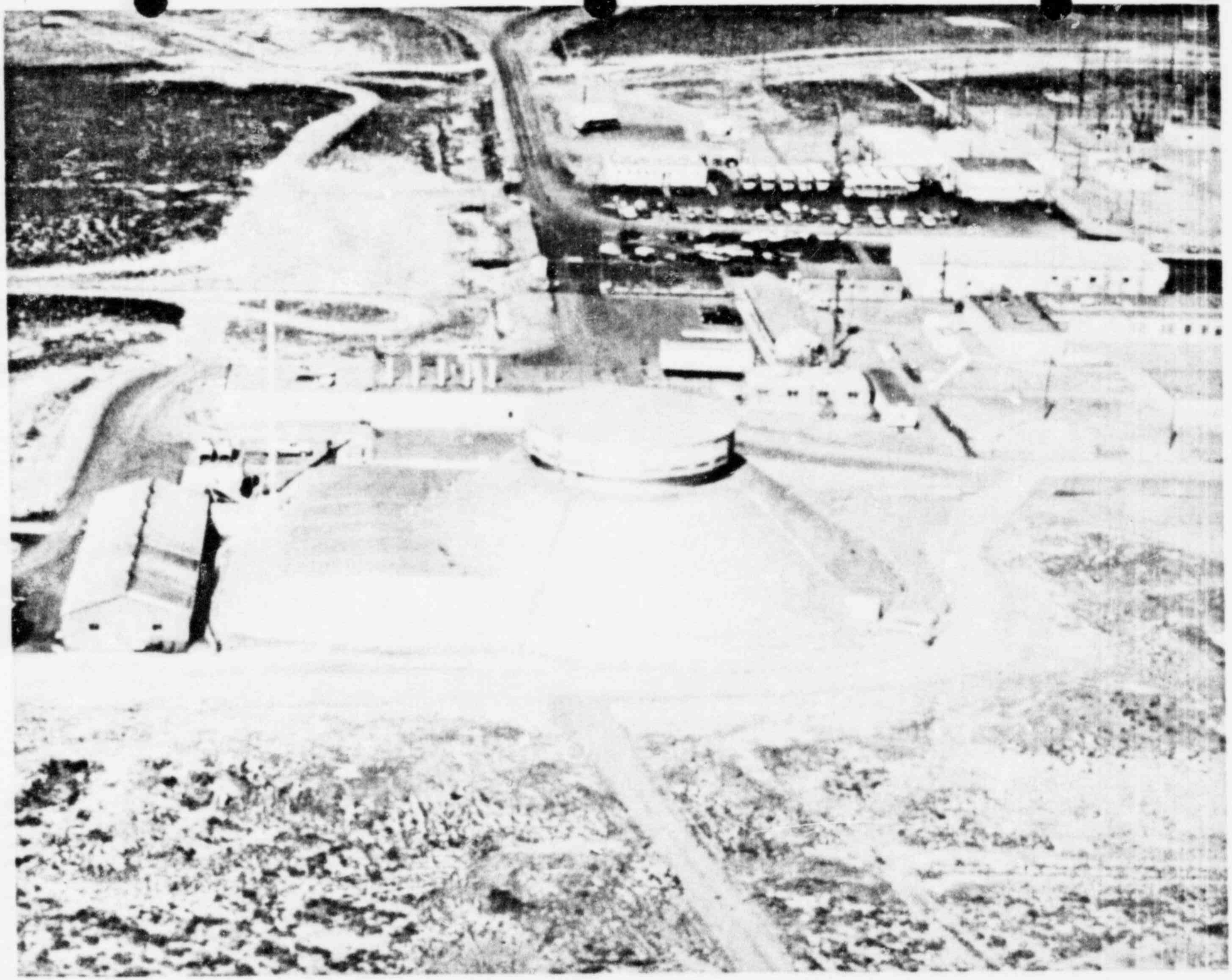


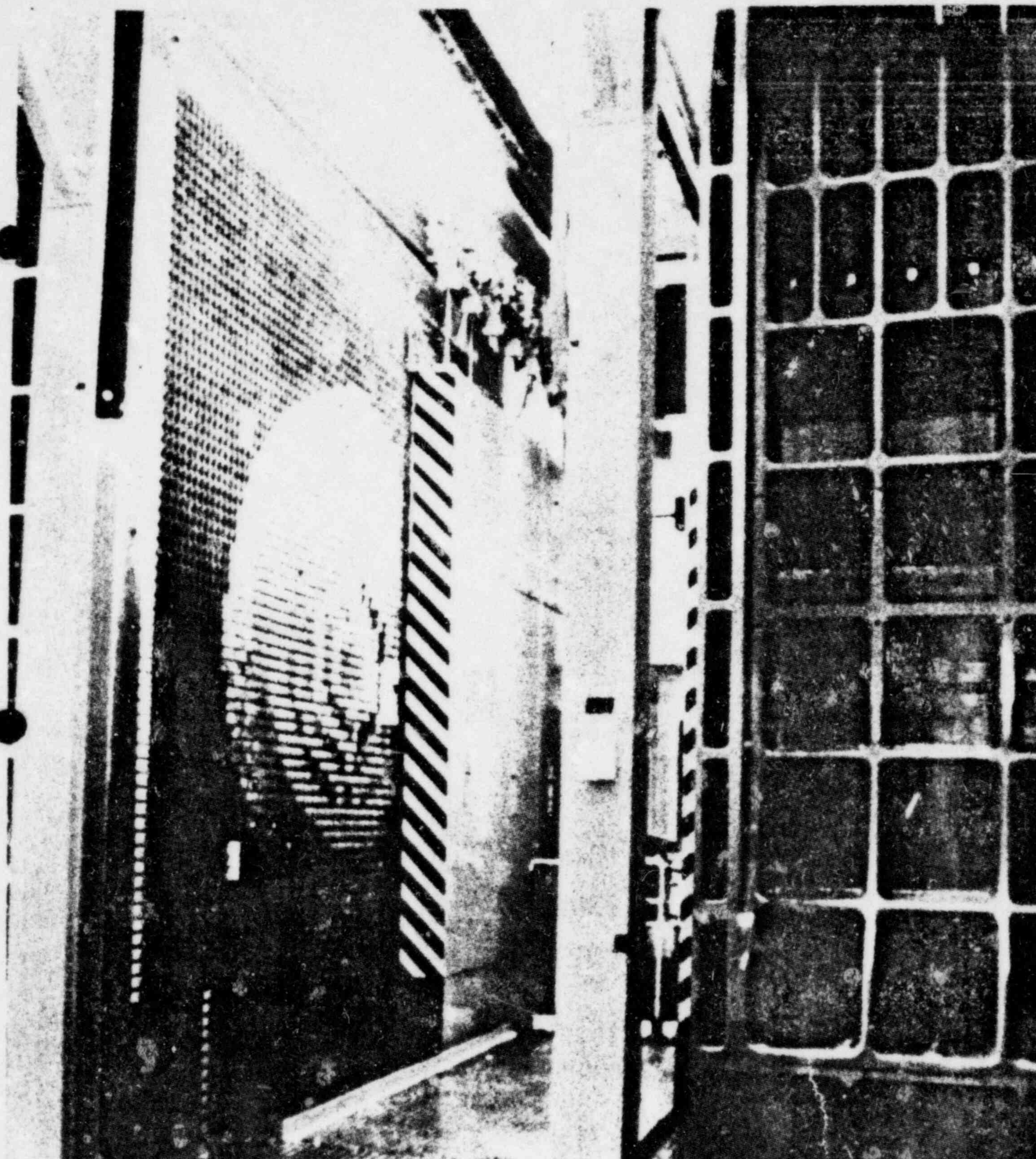


Figure 14 Zero Power Plutonium Reactor

REACTOR DESIGN AREAS SUPPORTED BY CRITICAL EXPERIMENTS

Power Reactor Design Parameter	Critical Experiment Data Source
• Fuel enrichment	Critical fuel loading, Doppler and core expansion worth, core conversion ratio
• Power distribution	Isotopic fission and capture rate distributions, gamma heating, blanket spiking studies
• Control rod margin	Control rod subcritical reactivity worth
• Reactivity coefficient	
Doppler	Small heated-sample U^{238} Doppler worth
Sodium void	Large zone-voiding reactivity worth
Core restraint (expansion)	Small-sample worth distributions
CDA-related	Sodium void worth, fuel and steel slumping worth
• Other performance data	
Breeding ratio	C^{238}/F^{239}
Temperature defect	Doppler worth, core expansion worth
Ex-core detector capability	Control rod worth with ex-core detectors
Fast flux/fluence	Neutron energy spectrum, spectral indices

FUEL ENRICHMENT PHILOSOPHY

Guarantee that the reactor can be maintained at hot-full-power conditions throughout each design burnup cycle

Nominal excess reactivity:

- Cold-critical eigenvalue, K_{EFF}
- Cold-to-hot temperature defect
- Fuel burnup reactivity deficit
- Mid-term refueling reactivity addition

Uncertainties:

- Criticality prediction
- Fuel burnup reactivity swing
- Temperature defect
- Fissile loading and core geometry tolerances
- Impurities
- Refueling worth

CRITICAL EIGENVALUE PREDICTIONS VERSUS EXPERIMENTAL VALUES

	Calculated k_{eff}	Experimental k_{eff}	C/E
ZPPR-7A	0.99019	1.00045	0.9897
ZPPR-7B	0.98924	1.00083	0.9884
ZPPR-7C	0.99089	1.00161	0.9893
ZPPR-7D	0.99347	1.00110	0.9924
ZPPR-7F	0.98873	1.00079	0.9880
ZPPR-7G	0.98858	1.00075	0.9878
ZPPR-8F	0.99156	1.00090	0.9907

Mean = 0.9895
$1\sigma = \pm\ 0.0016$

6110-24

CRBRP FUEL ENRICHMENTS

Cycles	Pu/(Pu + U)
1 & 2	32.8
Equilibrium	33.0

Beginning of Life Fissile Plutonium Inventory, 1498 kg

ARD

6110-12

POWER CALCULATION COMPONENTS

$$\text{Linear power (kW/ft.)} = \frac{\text{Reactor power (kW)} \cdot \text{region power fraction}}{\text{no. of rods} \cdot \text{length of rod (ft.)}} \cdot F^N_R \cdot F^N_Z \cdot 1.15 \cdot (1 + 3\sigma)$$

Where F^N_R is the normalized radial power distribution

F^N_Z is the normalized axial power distribution

1.15 is a 15% overpower-margin multiplier

$1 + 3\sigma$ represents the 3σ power envelope

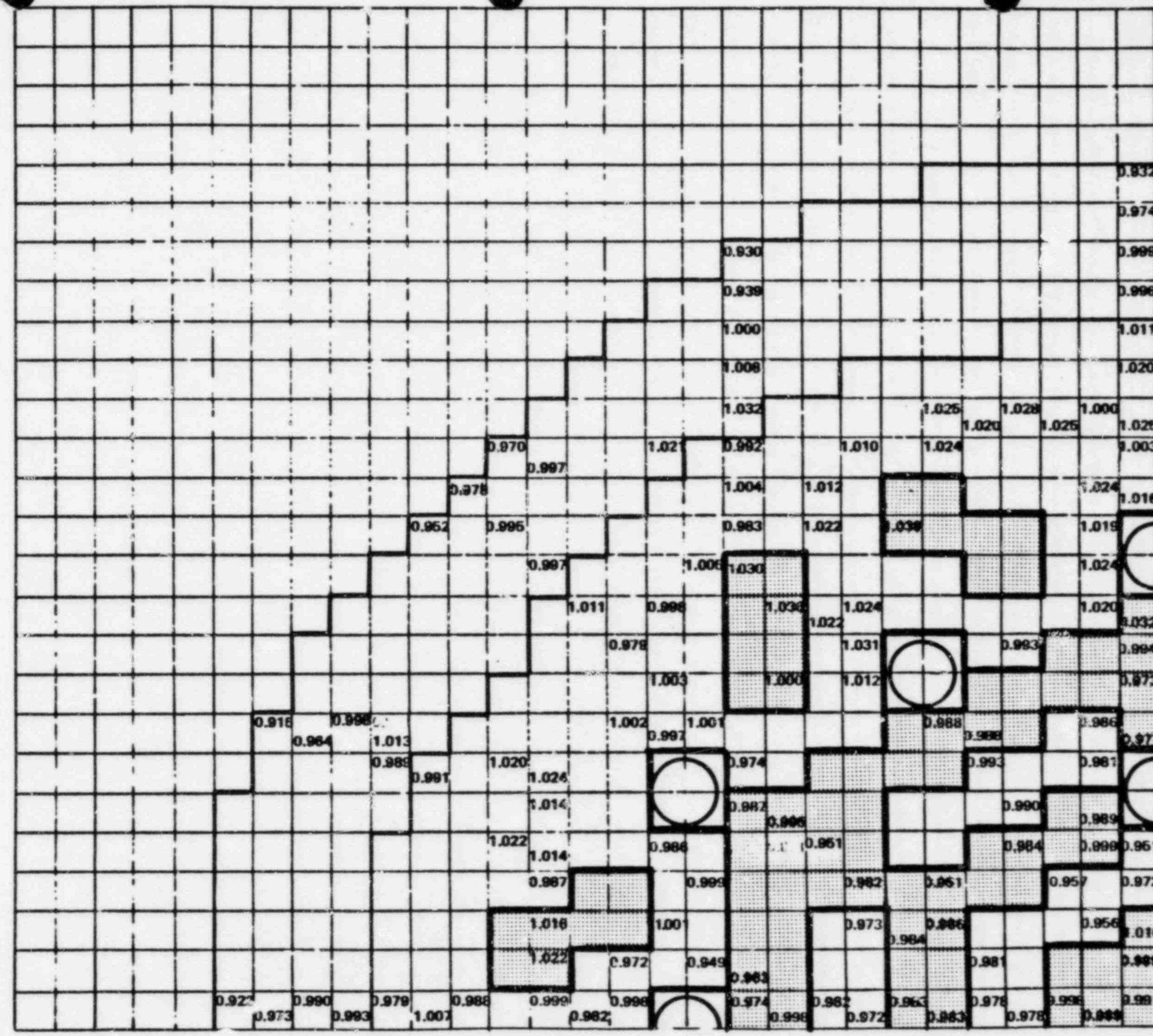


Figure 6.4. ZPPR-11 Phase B Midplane Calculation-to-Experimental Ratio for 235 U (n, f)

WARD

REACTION RATE CALCULATION TO EXPERIMENT RATIOS

Reaction	ZPPR-11B Beginning Of Life C/E ± 1σ	ZPPR-11C End Of Life C/E ± 1σ
Fuel		
Pu^{239}(n,f)	1.000 ± .019*	1.000 ± .019
U^{235}(n,f)	1.057 ± .026	1.043 ± .026
U^{238}(n,f)	0.879 ± .034	0.922 ± .034
Inner Blanket		
Pu^{239}(n,f)	1.014 ± .023	0.989 ± .023
U^{235}(n,f)	1.050 ± .026	1.022 ± .026
U^{238}(n,f)	1.093 ± .041	0.983 ± .032
U^{238}(n,capt)	1.055 ± .025	1.088 ± .025

*Uncertainty includes statistical deviation in foil C/E's plus estimated systematic uncertainty in measurement

7571-5

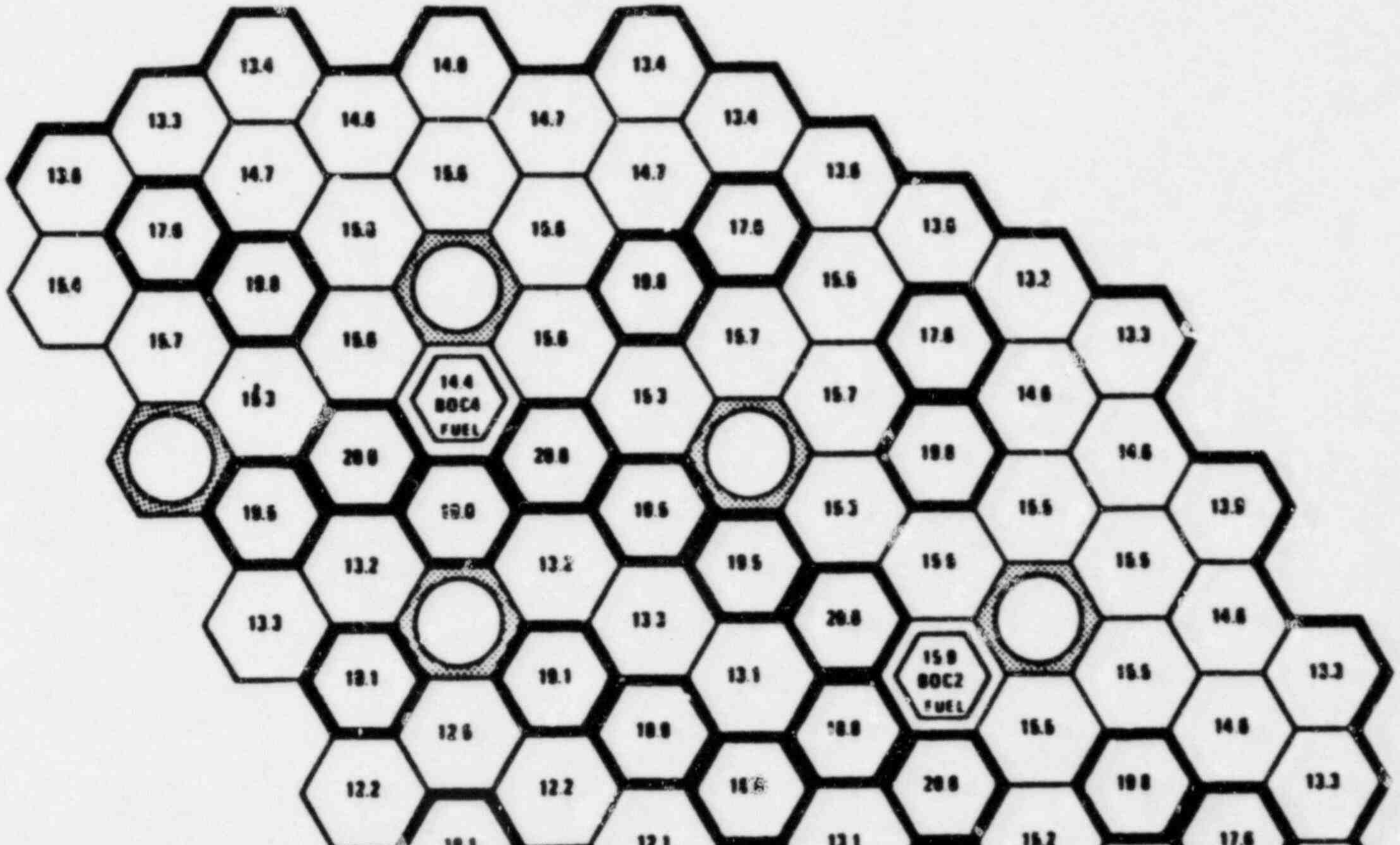
FUEL REGION POWER UNCERTAINTY FROM REACTION RATE UNCERTAINTIES

Component	1σ(%)	Power Fraction
Pu^{239} fission	$\pm$1.9%	.765
U^{235} fission	2.6	.005
U^{238} fission	3.4	.065
Other fission	5.0	.065
Gamma heating	8.0	.10

Resulting 3σ uncertainty is $\pm$5.5%

ⓌARD

PEAK LINEAR POWER DISTRIBUTION (KW/FT)
(3σ + 15% OVERPOWER CONDITIONS) FUEL AT BOCI
(EXCEPT REFUEL CHANNELS) INNER BLANKETS AT EOC4
(NOTE: THESE VALUES DO NOT OCCUR SIMULTANEOUSLY)



6110-28

SUMMARY OF USE OF ZPPR CONTROL ROD WORTH DATA IN CRBRP DESIGN

Experiment	Application
3R4, 6R7F, 6R7C bank worths in ZPPR-11B	Bias factors
Asymmetric-group rod worths	Verify that control rod worth bias is not substantially different in faulted (stuck rod) shutdown configuration
Pin control rod mockup	Pin versus plate extrapolation effects, evaluate B^{10} enrichment effects
Pin bunching	Evaluate capability of relatively simple central-rod calculational model to account for control rod worth reduction associated with absorber-pin bunching
Axial worth profile	Verify RZ calculations and chopped cosine approximation
Fuel/blanket interchange worth	Assess CRBRP mid-term refueling worth uncertainty

ⓦARD

8255-4

ZPPR-11 CONTROL ROD BANK WORTHS

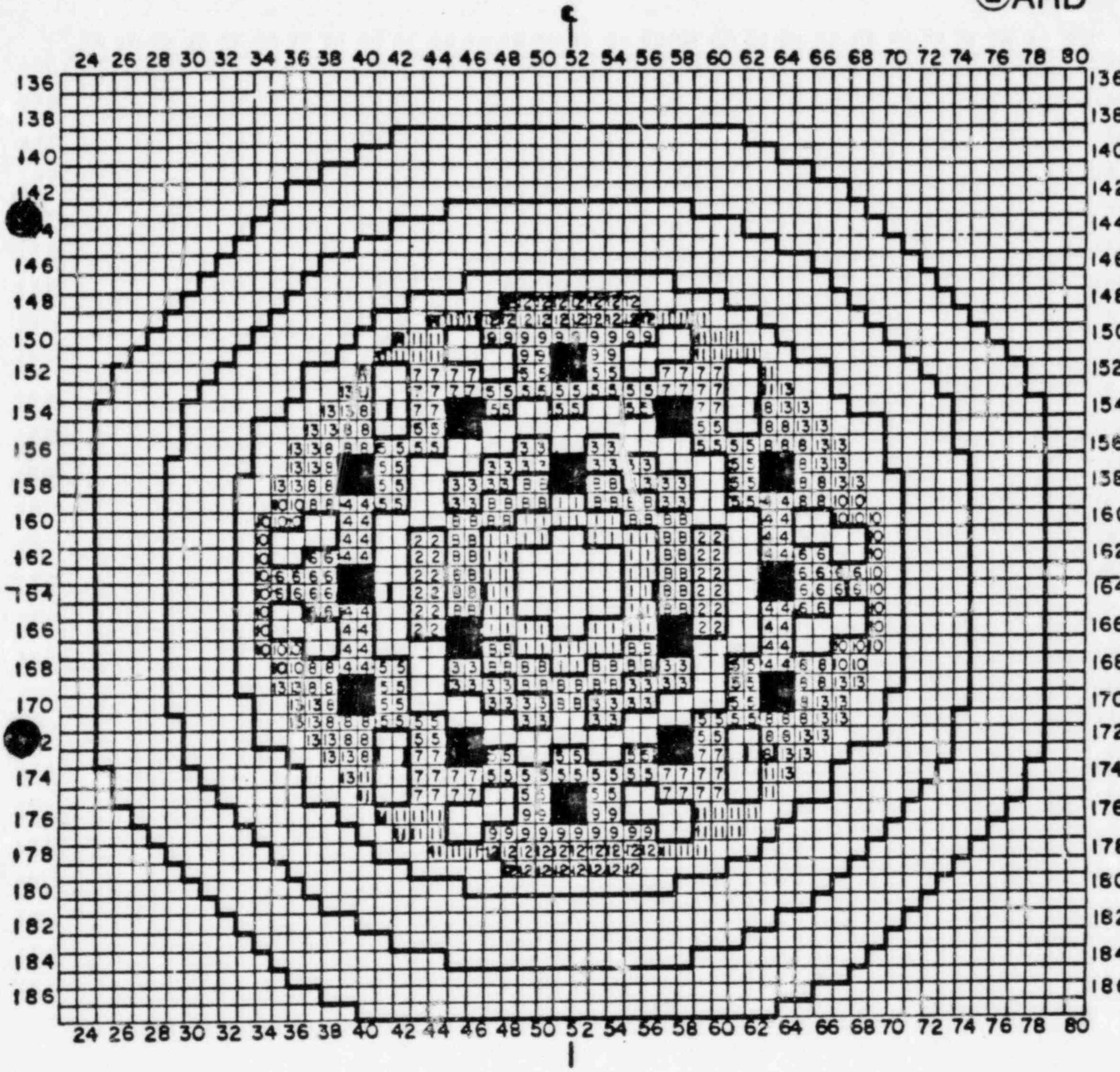
	ZPPR-11B Beginning Of Life			ZPPR-11C End Of Life		
	Calculated* Worth $	Measured Worth $	C/E	Calculated* Worth $	Measured Worth $	C/E
3R4	3.33	3.34	0.997	6.17	6.27	0.984
6R7F	12.97	12.42	1.044	15.81	15.36	1.029
6R7C	16.95	16.28	1.041	16.71	16.19	1.032

*Calculations with 4 mesh per ZPPR-drawer

Ⓦβ_{EFF} = 0.003426 (ZPPR-11B)
0.003540 (ZPPR-11C)

ⓌARD

8255-5



1-13 Zone 1 through Zone 13

B Blanket Ring 1

Fig. 3.1 ZPPR-11E Radial Sodium Void Zones

HETEROGENEOUS CRBRP SODIUM VOID REACTIVITY ($)
END OF CYCLE FOUR

FLOWING SODIUM ONLY (APPROXIMATELY 82% OF THE TOTAL)

	ENDF/B-3	ENDF/B-3 BIASED	ENDF/B-4	ENDF/B-4 BIASED	UNCERTAINTY ±1 σ
36- inch fuel	1.15$	1.50	1.90	1.49	±.28
Lower axial blanket	-.17	-.19	-.15	-.14	±.03
Upper axial blanket	-.17	-.19	-.16	-.16	±.03
Total	.81	1.12	1.59	1.19	

7918-8

ZPPR-11B FUEL U^{238} DOPPLER CONSTANT
-T dk/dT

Measured fuel U^{238} Doppler	-.00332
Calculated Doppler	-.00327
C/E	0.986

SUMMARY

Bias factors and uncertainties in calculated CRBRP nuclear parameters are based on an extensive zero power critical experimental data base

Experiments include:

- Critical fuel loading
- Power distribution parameters
- Control rod worth characteristics
- Reactivity feedback effects

CORE THERMAL AND HYDRAULIC DESIGN

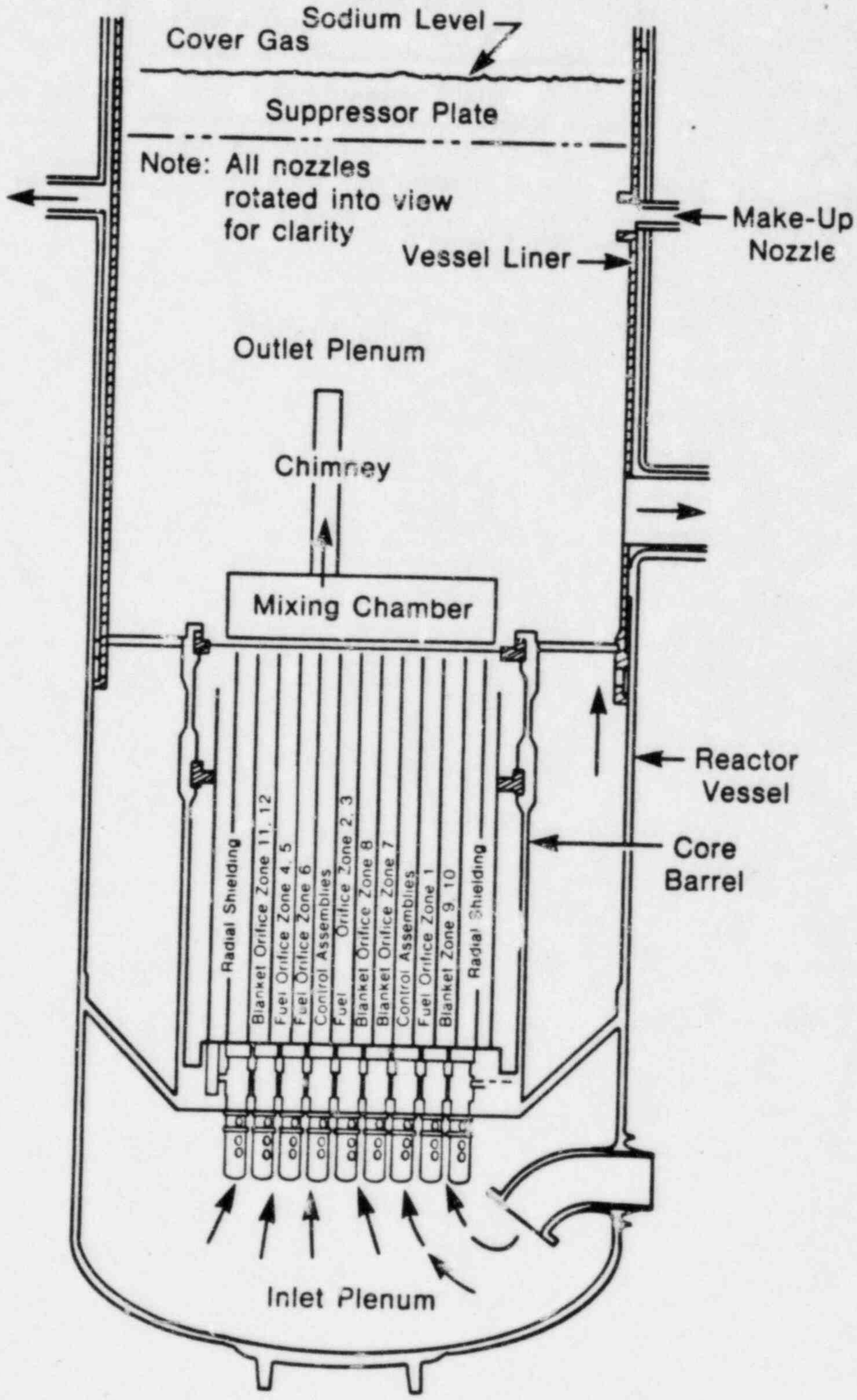
ⓦARD

8254-5

CRBRP CORE T&H ANALYSIS AND DESIGN OUTLINE

- Core T&H Description and Bases
  - -- Flow Paths
  - -- Design Data
  - -- Flow Allocations
- Performance Predictions
  - -- Steady State
  - -- Design Transients
- T&H Development Test Programs/Data
- Conclusions

CRBRP SCHEMATIC FLOW PATHS



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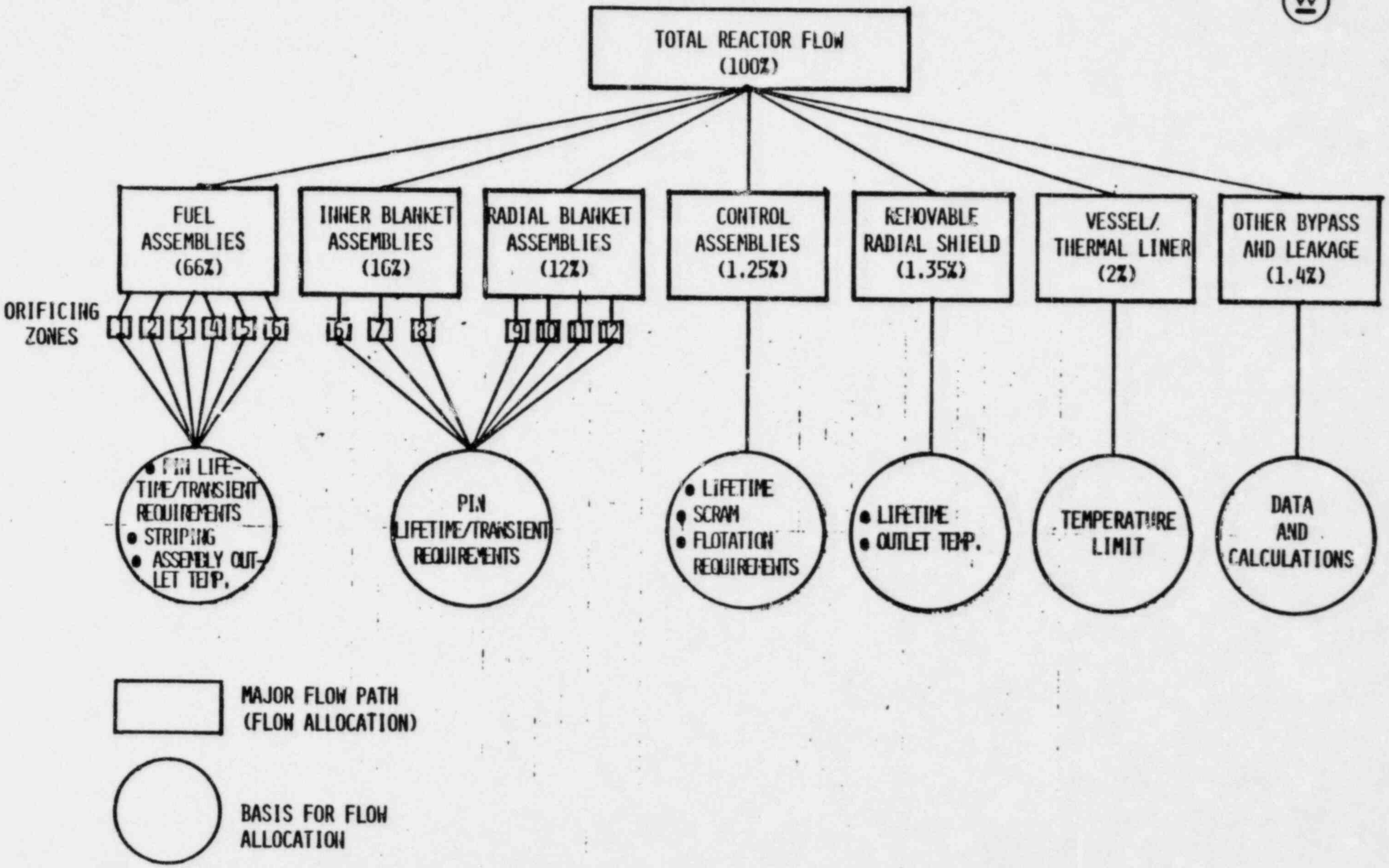
CRBR PRINCIPAL CORE T&H DESIGN DATA

	Fuel	Blanket
Rods per assembly	217	61
Rod diameter (in.)	0.230	0.506
Pitch-to-diameter ratio	1.25	1.07
Wire wrap axial pitch (in.)	12	4
Axial lengths (in.):		
Lower axial blanket	14	
Active core	36	64
Upper axial blanket	14	
Fission gas plenum	48	48

7038-2

CRBRP FLOW ALLOCATION

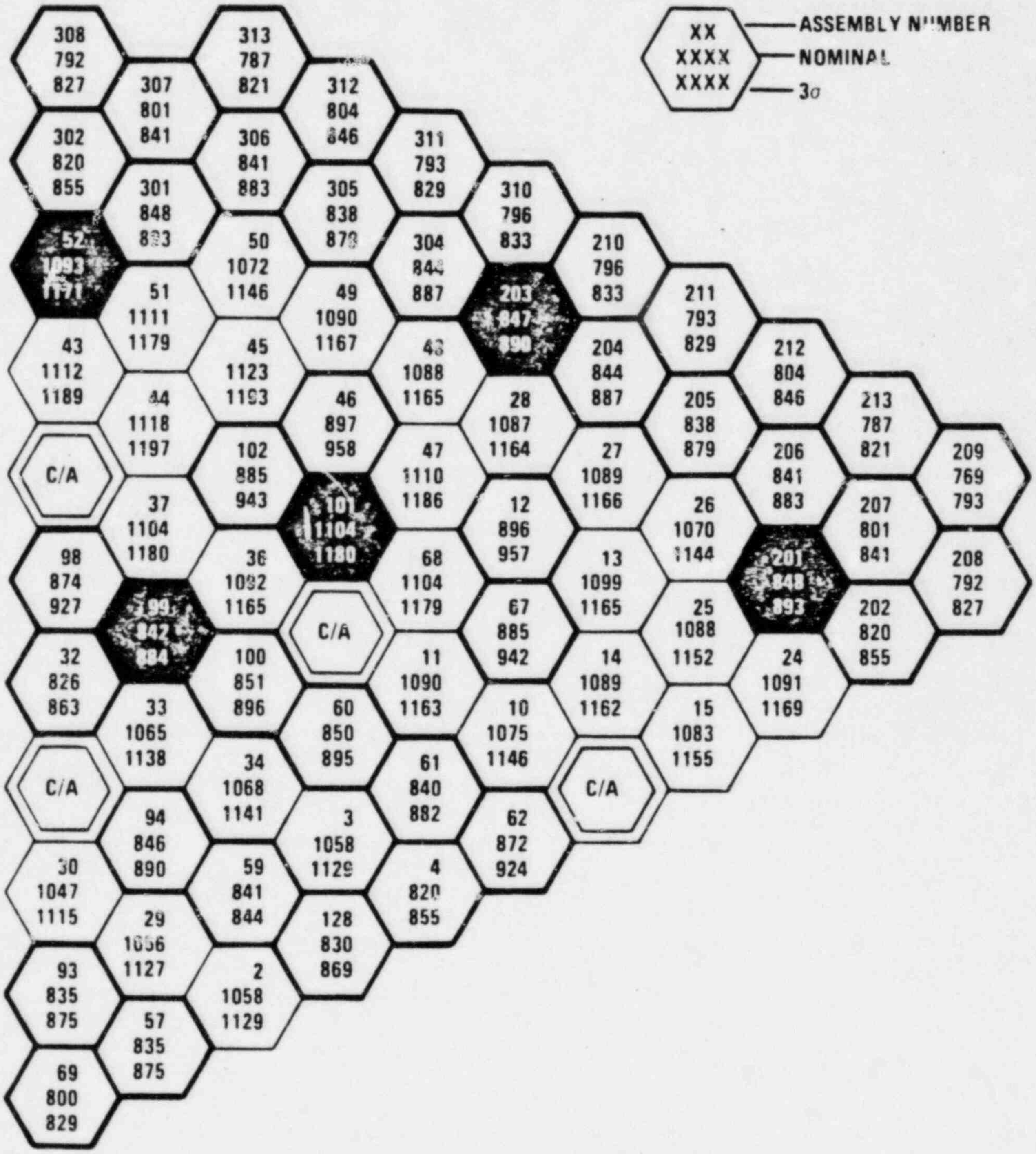


CRBR PRINCIPAL CORE T&H PERFORMANCE DATA

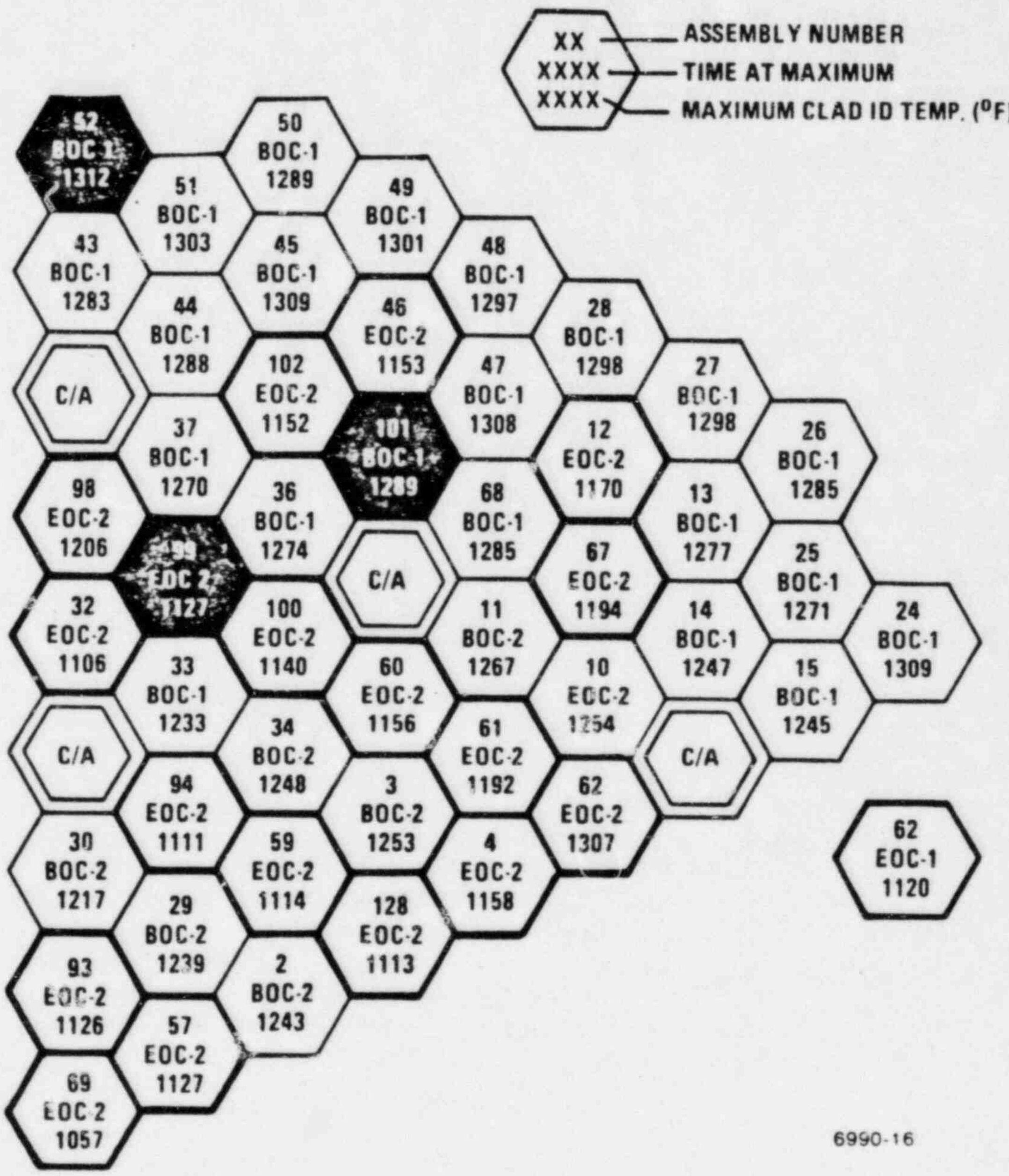
Reactor Inlet Temperature	730°F
Reactor Outlet Temperature	995°
Reactor Design Flow	41.446×10^6 lb/hr
Reactor Vessel Nozzle-to-Nozzle Pressure Drop	123 psi

	Fuel	Inner Blanket	Radial Blanket
Number of Orificing Zones	5 - 6	3 - 2	4
Range of Maximum Hot Rod Cladding Temperatures (2σ), °F	1201 - 1312	1057 - 1262	989 - 1228
Maximum Fission Gas Pressure (2σ), Psia	962	249	273
Maximum Flow Velocity in Bundle (Ft/Sec)	23	18	13
Maximum Mixed Mean Exit Temperature (Nominal), °F	1123	1029	1003
Maximum Temperature Gradient (Nominal), °F	273 (fuel/radial blanket)		

CORE ASSEMBLIES MIXED MEAN OUTLET TEMPERATURES - BOC1 (THDV - °F)

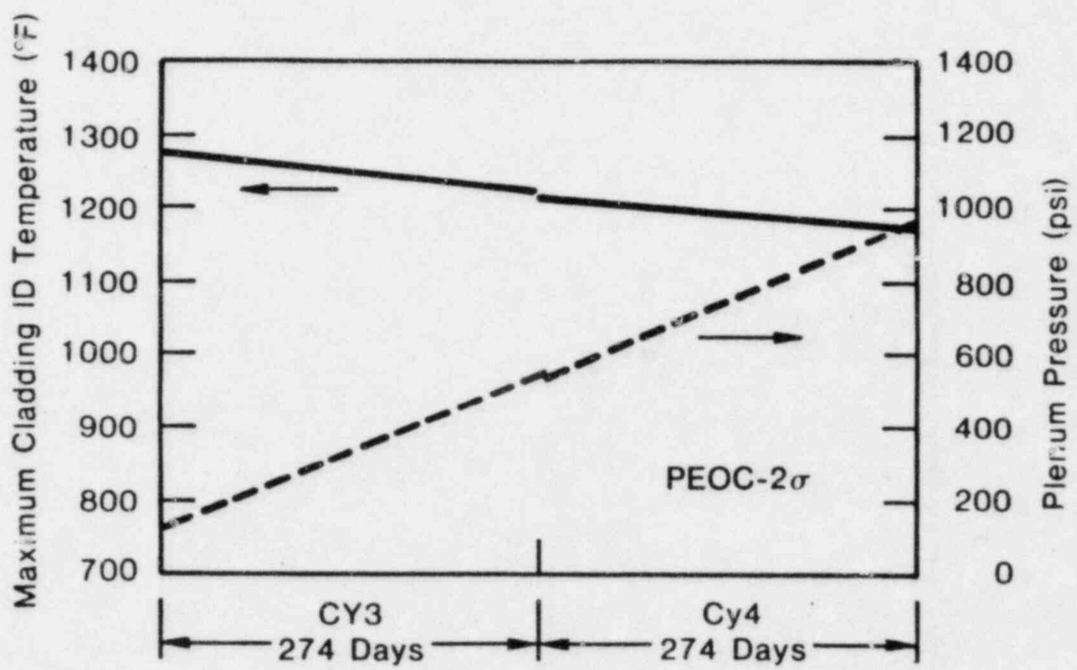


ENVELOPE OF FUEL, INNER BLANKET MAXIMUM CLADDING ID TEMPERATURES FOR FIRST CORE (PEOC-2σ)



6990-16

CLADDING TEMPERATURE/PRESSURE HISTORY IN F/A #101, O. Z. 1



6215-11

DESIGN TRANSIENTS

WORST CASE UNDERCOOLING EVENT

CRBRP THREE-LOOP NATURAL CIRCULATION

TRANSIENT - MAXIMUM CLADDING/COOLANT TEMPERATURE (°F)

AND TIME OF OCCURRENCE (SEC.)

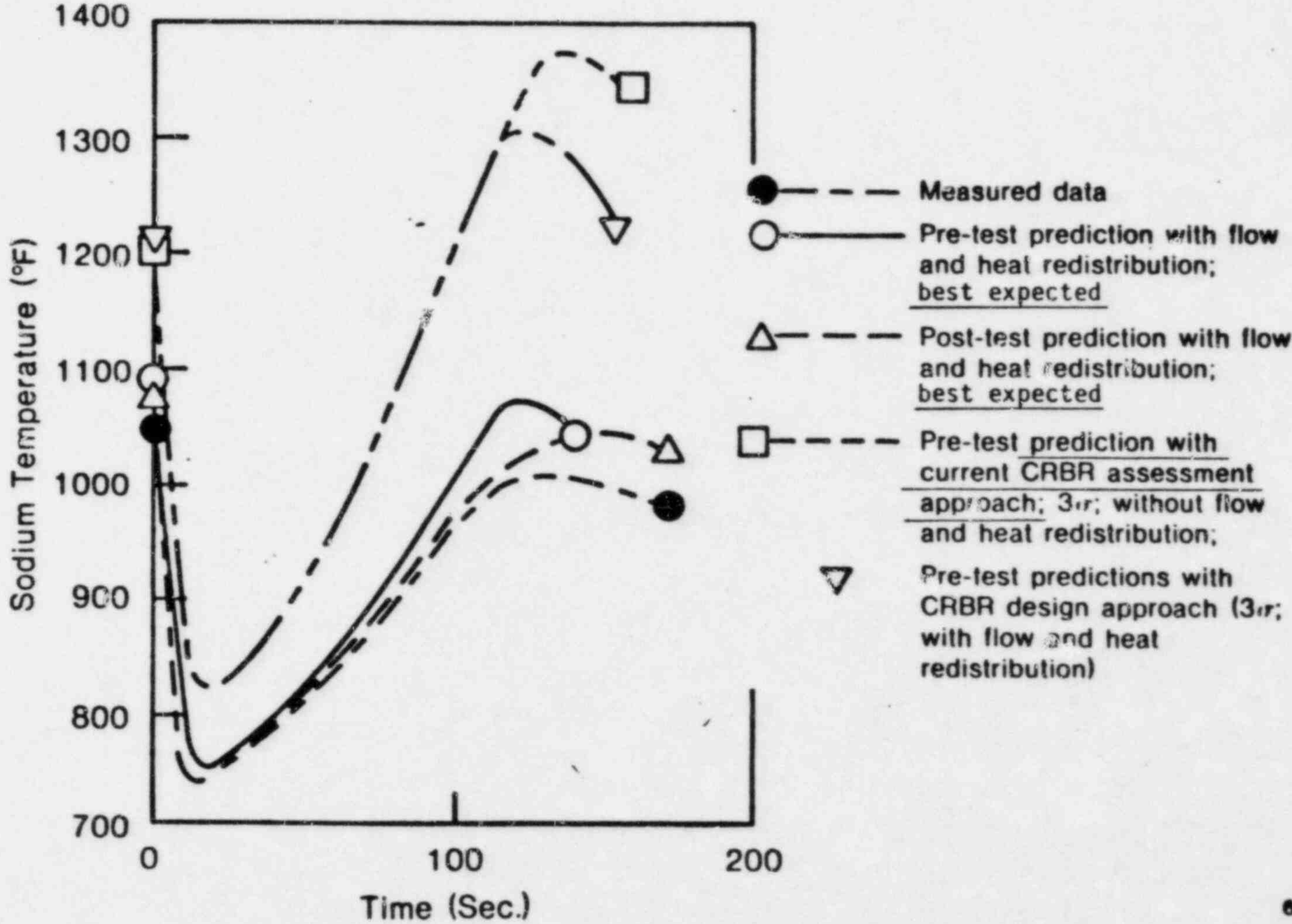
- PRESENTED IN CRBRP-ARD-0308

ASSEMBLY	NOMINAL		3σ	
	TEMP.	TIME	TEMP.	TIME
FA-52	1299	178	1565	180
IB-99	1229	222	1544	239
RB-203	1279	275	1556	389

ACCEPTANCE CRITERION: T_{MAX} < BOILING

$T_{SAT.}$ = 1720°F $\left\{ \begin{array}{l} \text{AT TOP OF FUEL ACTIVE REGION} \\ \text{ZERO FLOW} \\ \text{ZERO COVER GAS PRESSURE} \\ \text{MINIMUM OPERATION POOL LEVEL} \end{array} \right.$

MEASURED AND PREDICTED SODIUM TEMPERATURES AT TOP OF THE FUEL SECTION, TX1016, FOR ROW 2 FCTA-FFTF
(Test Initiated From 100% Power/100% Flow)



6720-19

TRANSIENT REACTOR/CORE DESIGN REQUIREMENTS SUMMARY

- Proper Interface Requirements Have Been Established
- Compatible Steady State Operating Conditions Have Been Established (e.g., Through Orificing)
- All Design Basis Accidents (Overpower and Undercooling) Have Been Evaluated on a Conservative Basis and Meet the Design Guidelines of:

  -- No Boiling

  -- No Clad Melting

  -- Acceptable Lifetime/Structural Integrity

CORE T&H DEVELOPMENT TEST PROGRAMS

- Fuel Assembly
- Blanket Assembly
- Core Pressure Drop
- Examples of Data

OUT-OF-PILE T&H DEVELOPMENT TESTING FOR FUEL ASSEMBLIES

TEST TITLE	SUPPORTING INFORMATION	STATUS
• ORNL 19 and 61-Rod Bundle Heat Transfer - Sodium	W/W Bundle Temperature Distribution Over Wide Operating Range, Including Transients	Completed
• HEDL 217-Rod Low Flow Heat Transfer - Sodium	Low Flow Bundle Temperature Distribution	Completed
• HEDL 217-Rod Bundle Mixing - H_2O	Detailed Bundle Mixing	Completed
• ANL 91-Rod Bundles Mixing - H_2O	Bundle Swirl and Mixing	Completed
• MIT Fuel Bundle T&H	Flow Split, ΔP, Flow Distribution and Mixing	In Progress
• WARD 11:1 Scale Wire Wrap Bundle Air Flow	Detailed S/C Axial and Cross Flow Characterization and Mixing	Completed
• HEDL CRBR Assembly Flow and Vibration	Verification of Flow and Vibration Characteristics	Completed
• HEDL FFTF Assembly/Bundle Flow	Bundle Pressure Drop	Completed
• HEDL Inlet/Outlet Nozzle and Orifice Flow	Cavitation and ΔP Characterization	90% Completed
• EBR-II Orifice Cavitation Proof Test	Flow Control Orifice Lifetime/Cavitation	In Progress
• HEDL Assembly Outlet Nozzle Instrumentation	Correlate T/C Outlet Temperature Measurements	Testing Complete

OUT-OF-PILE T&H DEVELOPMENT TESTING

FOR BLANKET ASSEMBLIES

TEST TITLE	SUPPORTING INFORMATION	STATUS
• WARD Full Scale 61-Rod Assembly Heat Transfer - Sodium	W/W Bundle Temperature Distribution Over Wide Operating Range, Including Transients	95% Completed
• MIT Blanket Bundle T&H - H_2O	Flow Split, ΔP, Flow Distribution and Mixing	In Progress
• WARD 5:1 Scale Wire Wrap Bundle Air Flow	Detailed S/C Axial and Cross Flow Characterization	Completed
• HEDL Assembly Flow and Vibration - H_2O	Verification of ΔP and Vibration Characteristics	Completed
• WARD Full Scale Bundle Pressure Drop - Sodium and Water	Bundle ΔP Over Wide Flow Range	Completed
• WARD Blanket Flow Orificing Characterization	Pressure Drop Characterization	Planned
• HEDL Assembly Outlet Nozzle Characterization	Correlate T/C Outlet Temperature Measurement	Testing Complete

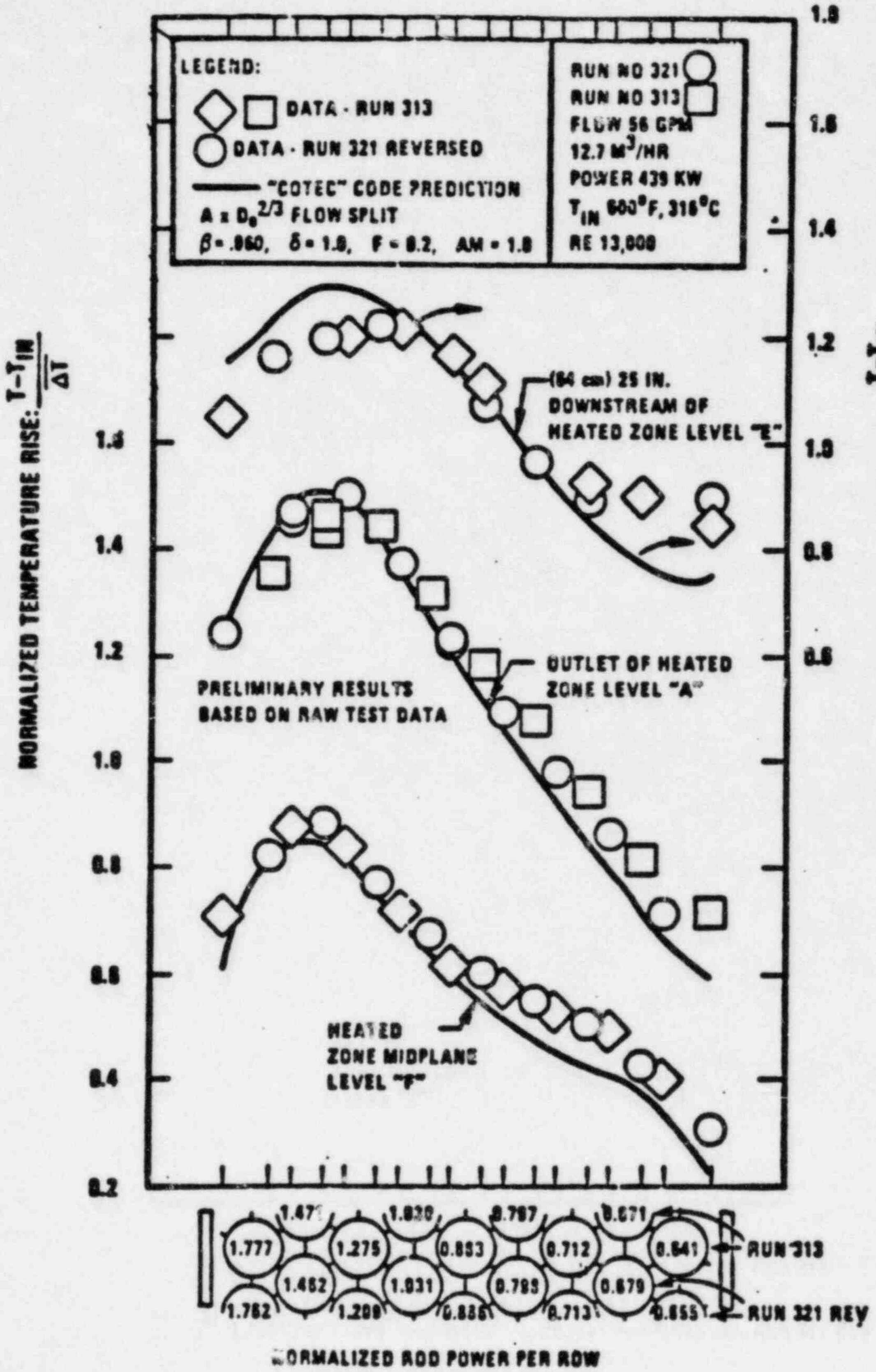
WARD BLANKET ASSEMBLY HEAT TRANSFER TEST

RANGE OF TEST PARAMETERS

• Power Input	17 to 880 Kw
• Flow	2 to 140 Gpm
• Reynolds Number	500 to 26000
• Power-to-Flow Ratio	100 to 300°F
• Power Input Gradient	1:1 to 4.6:1 (Max:Min)

CONDITIONS SIMULATED

- Adiabatic Boundaries
- Inter-Assembly Heat Transfer Effects:
  - -- Auxiliary Cooling
  - -- Auxiliary Heating
- Transient and Natural Circulation

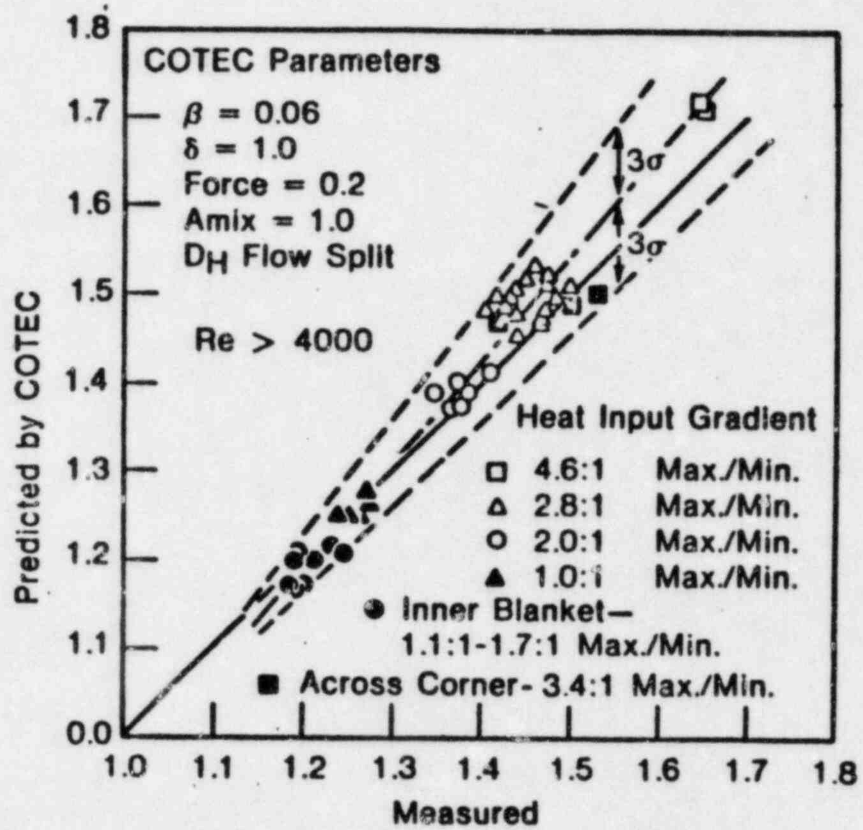


Predicted Vs. Measured Temperature Profiles – Input 2.8/1 Gradient – 440 KW
Re 13,000

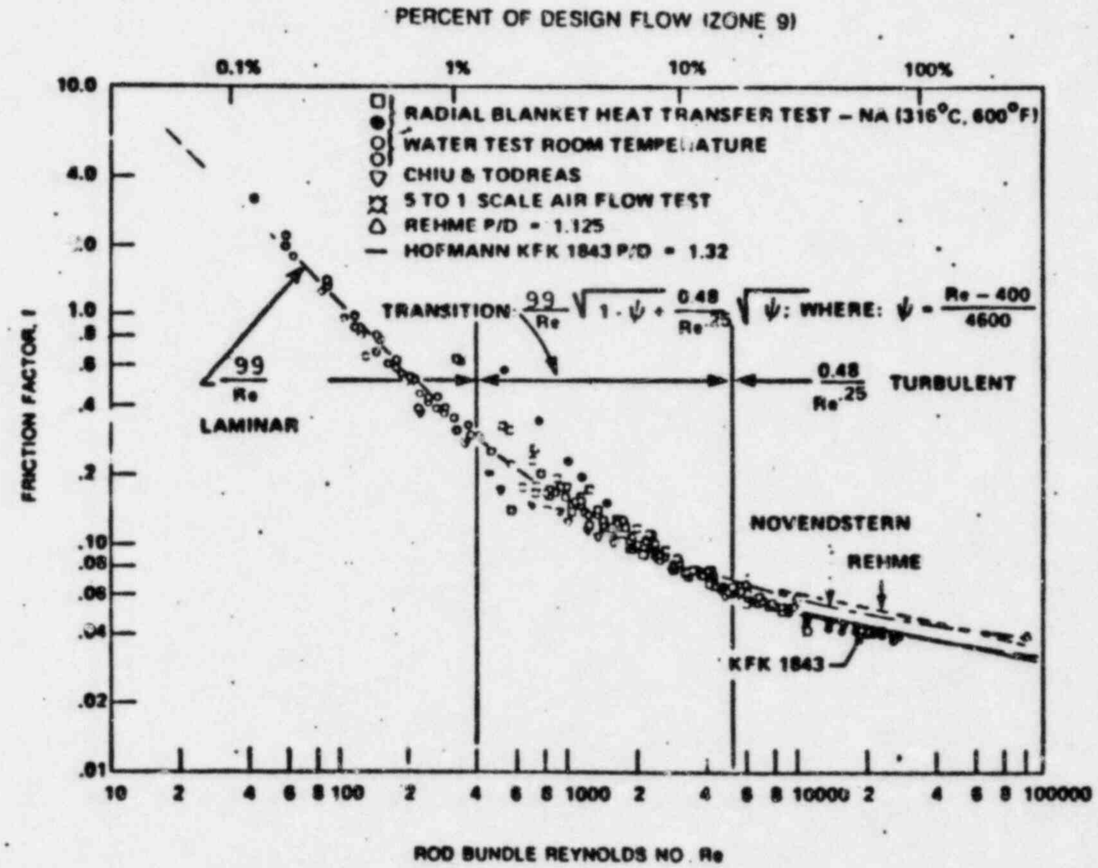
1825-1

BLANKET HEAT TRANSFER TEST

PREDICTED VS. MEASURED PEAK NORMALIZED TEMPERATURE RISE



FRICTION FACTOR TEST DATA FOR TIGHT PITCH TO DIAMETER ROD BUNDLES WITH 4 IN. WIRE WRAP SPACER LEAD



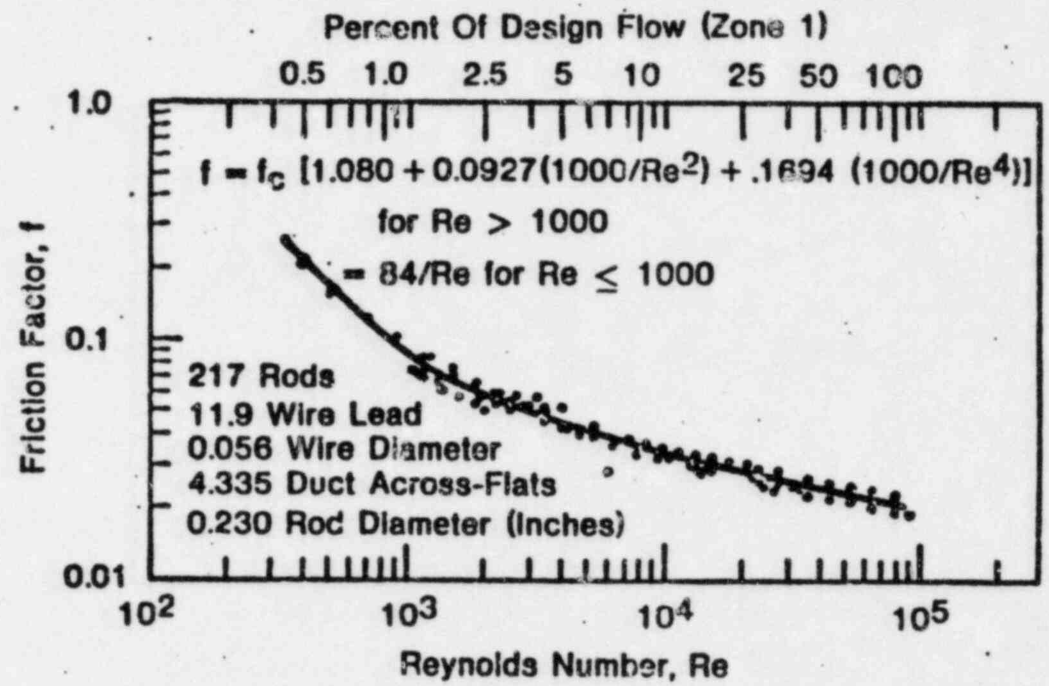
CORE PRESSURE DROP TEST RESULTS

- RANGE OF DATA AND STATUS
- TYPICAL EXAMPLES OF TEST DATA/CORRELATIONS/RESULTS

CORE PRESSURE DROP TESTING - STATUS

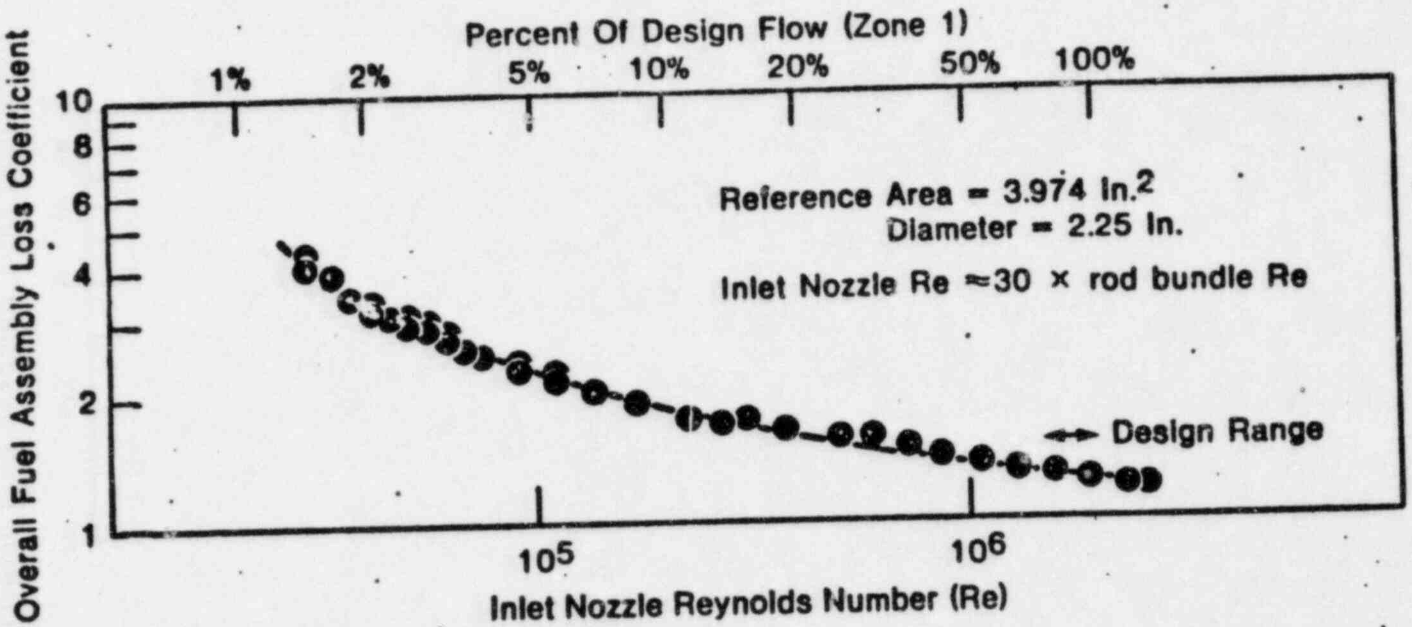
COMPONENT	~ΔP AT 100% FLOW (PSI)	RANGE OF TEST DATA (%)	TEST STATUS
CORE			
FUEL: INLET-ORIFICE-SHIELD	35.5	1.5 - 120	COMPLETE
ROD BUNDLE	58.4	0.5 - 120	COMPLETE
ROD BUNDLE INLET AND OUTLET	1.8	1.5 - 120	COMPLETE
OUTLET NOZZLE	1.8	1.5 - 120	COMPLETE
INNER BLANKET: INLET-ORIFICE-SHIELD	37.7	2 - 120	PLANNED
ROD BUNDLE	60.4	0.2 - 100	COMPLETE
ROD BUNDLE INLET AND OUTLET	1.4	2 - 120	COMPLETE
OUTLET NOZZLE	0.9	2 - 120	COMPLETE
RADIAL BLANKET: INLET-ORIFICE-SHIELD	63.5	2 - 120	PLANNED
ROD BUNDLE	32.6	0.15 - 135	COMPLETE
ROD BUNDLE INLET AND OUTLET	0.7	2 - 120	COMPLETE
OUTLET NOZZLE	0.4	2 - 120	COMPLETE
PRIMARY CONTROL: INLET-ORIFICE-SHIELD	94	2 - 200	COMPLETE
ROD BUNDLE	3.0	2 - 200	COMPLETE
ROD BUNDLE INLET AND OUTLET	0.9	2 - 200	COMPLETE
OUTLET NOZZLE	7.1	2 - 200	COMPLETE
SECONDARY CONTROL: INLET-ORIFICE-SHIELD	75	18 - 125	COMPLETE
ROD BUNDLE	2.5	18 - 125	COMPLETE
OUTLET	28	18 - 125	COMPLETE
REMOVABLE RADIAL SHIELD: OVERALL	30	30 - 120	COMPLETE
PISTON RINGS	100	30 - 120	COMPLETE

FRICTION FACTOR DATA AND CORRELATION FOR 217 PIN WIRE WRAP SPACED FUEL ASSEMBLY



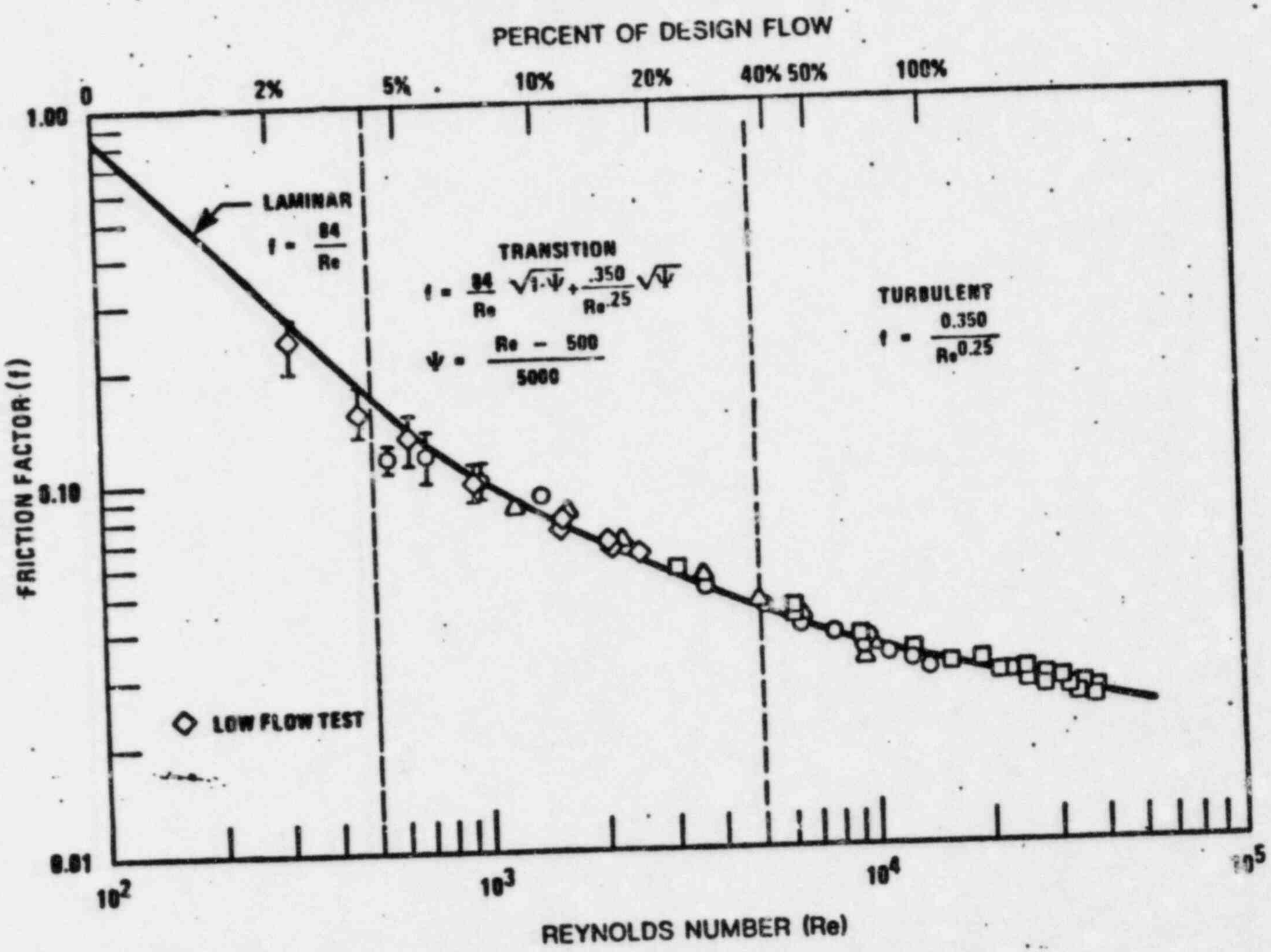
0000-9

OVERALL FUEL ASSEMBLY LOSS COEFFICIENT AS A FUNCTION OF REYNOLDS NUMBER FROM CRBRP FUEL ASSEMBLY FLOW AND VIBRATION TEST

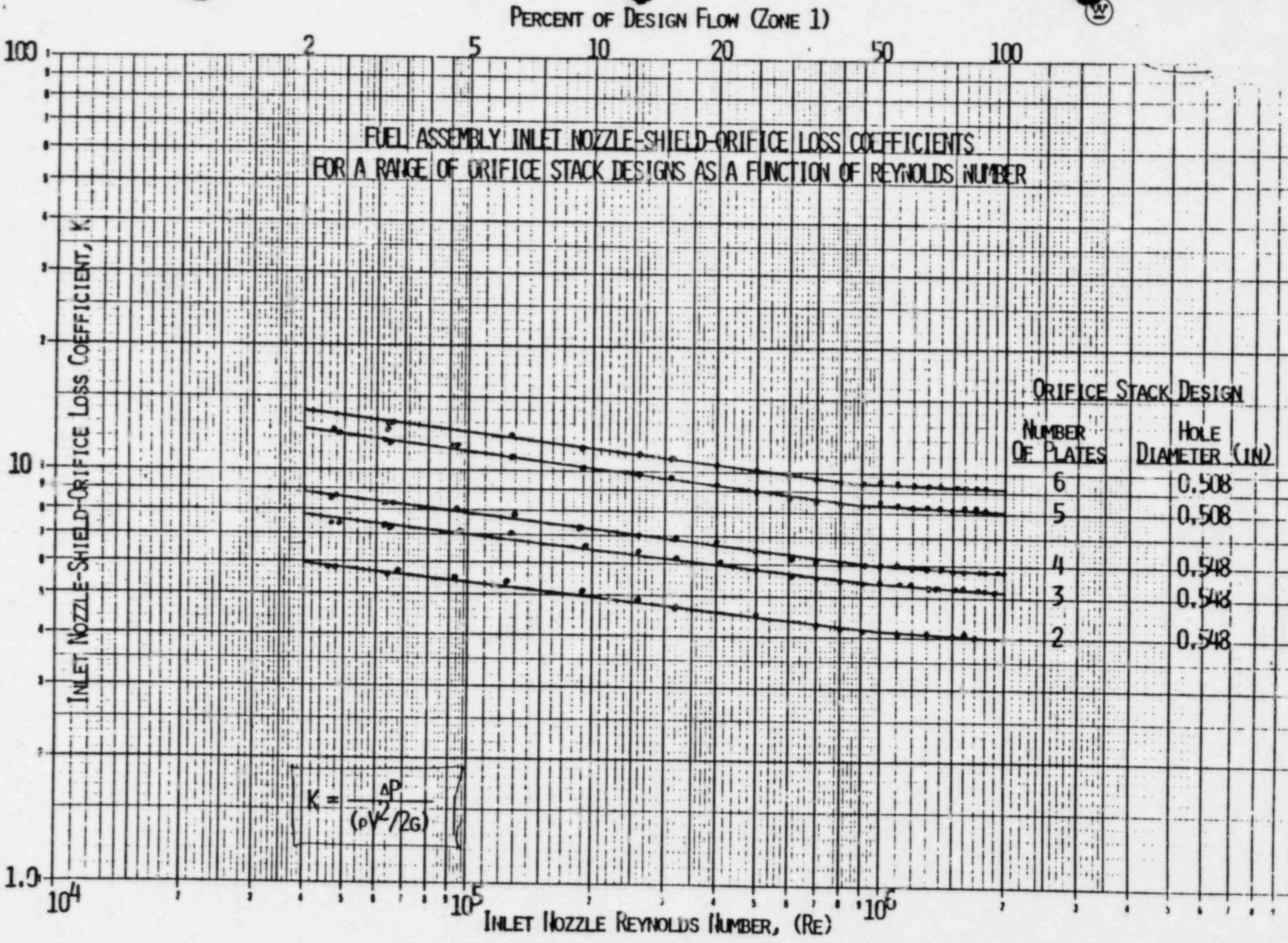


6990-14

PRIMARY CONTROL ASSEMBLY ROD BUNDLE FRICTION FACTOR



1-0999



PERCENT OF DESIGN FLOW (ZONE 1)
FUEL ASSEMBLY INLET NOZZLE-SHIELD-ORIFICE LOSS COEFFICIENTS
FOR A RANGE OF ORIFICE STACK DESIGNS AS A FUNCTION OF REYNOLDS NUMBER
INLET NOZZLE-SHIELD-ORIFICE LOSS COEFFICIENT, K
ORIFICE STACK DESIGN
NUMBER OF PLATES
HOLE DIAMETER (IN)
6 0.508
5 0.508
4 0.548
3 0.548
2 0.548
$K = \frac{\Delta P}{(\rho V^2/2G)}$
INLET NOZZLE REYNOLDS NUMBER, (RE)
2
5
10
20
50
100
1.0
10
100
10^4
10^5
10^6

CORE T&H DEVELOPMENT TESTING

CONCLUSIONS

1) Large Core T&H Data Base Available

2) Data On All Reactor Components - Over Wide Range of Operation, e.g., ΔP, Heat Transfer Data

3) Uncertainties Used for PSAR Based on Available Experimental Data

4) All Data Will Be Factored into FSAR Input

CONCLUSIONS

- Reactor Flow Distribution Meets Component Design Requirements
- Cooling Flow Paths Well Characterized, Orifice Controlled, Tested, Modeled
- Large Components T&H Development Data Base
- Comprehensive Design with Conservative, Yet Realistic, Limits
- Analytical Methods Verified with Large Data Base

FUEL AND BLANKET DESIGN

WARD

8254-6

CRBRP CORE MECHANICAL DESIGN
FUEL, BLANKET, SHIELD

- Bases
- Description
- Evaluations
- Testing programs

ⓦARD

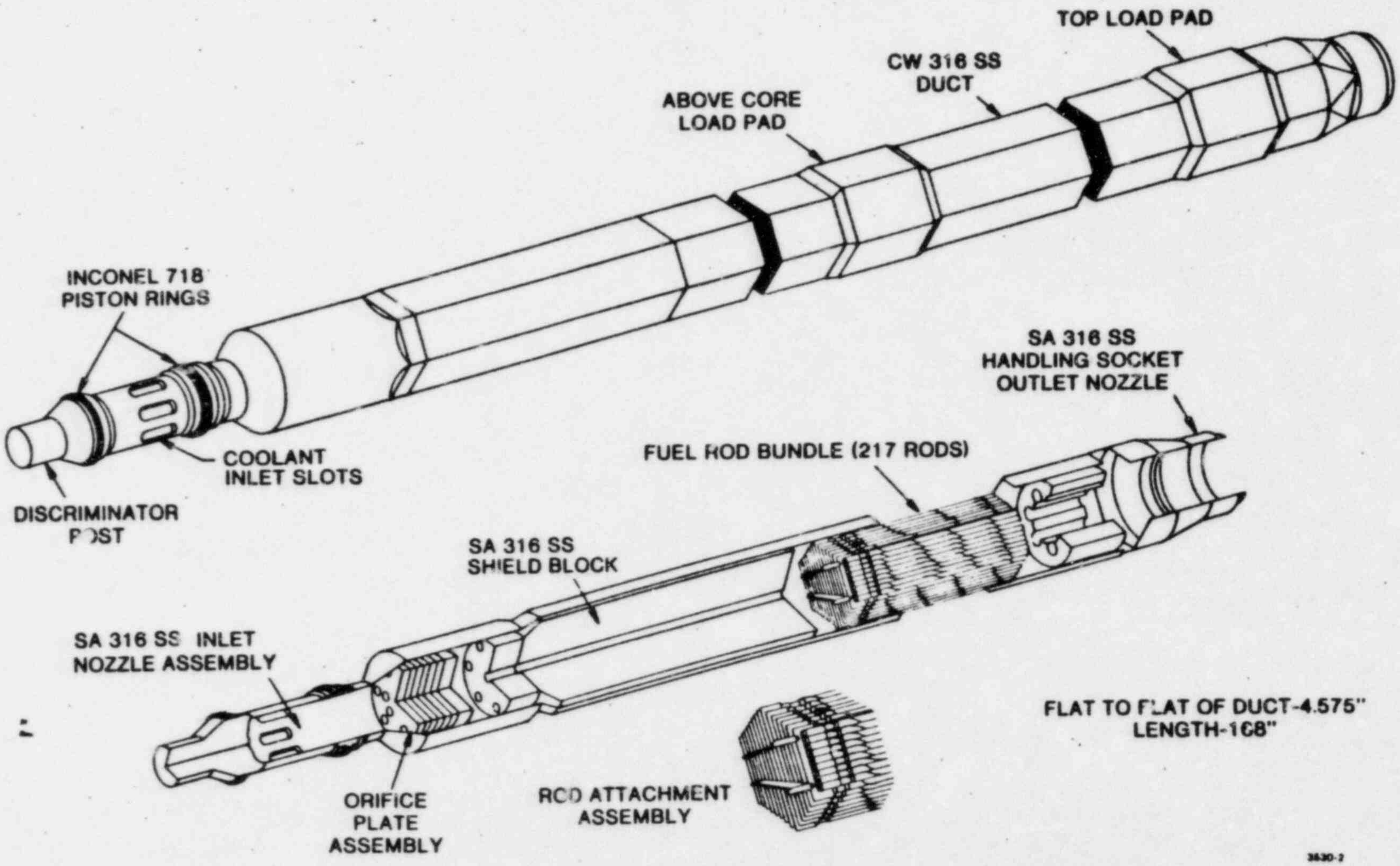
8253-1

DAMAGE SEVERITY LIMITS

Event Category	Damage Severity Level (RDT C-16-1)	Design Limit
Normal operation	No significant loss of effective lifetime	Ductility limited strain ≤ 0.2% (normal creep & plasticity) Power-to-melt Proportional elastic limit One wire diameter-flow channel closure
Anticipated events (Upset)	No reduction of effective lifetime below the design values	Ductility limited strain ≤ 0.3%
Unlikely events (Emergency)	A general reduction in the fuel burnup capability and, at most, a small fraction of fuel rod cladding failures	Cumulative damage function ≤ 1.0 (creep rupture, plasticity, fatigue damage)
Extremely unlikely events (Faulted)	Maintain coolable configuration	Cladding solidus, no Na boiling*

*PSAR guideline

CRBRP FUEL ASSEMBLY



FLAT TO FLAT OF DUCT-4.575"
LENGTH-168"

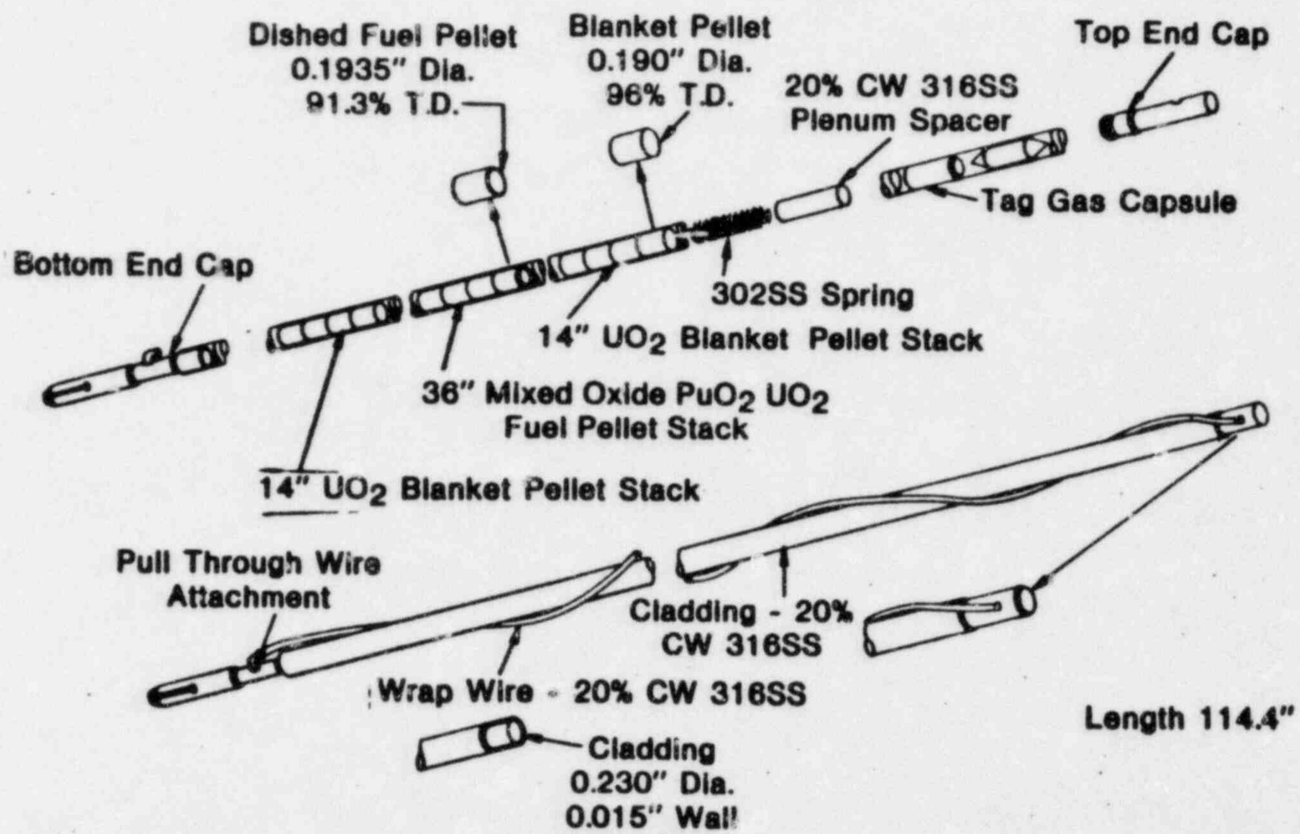
3630-2

CRBRP FUEL ASSEMBLY COMPARISON WITH FFTF

Design Parameter	CRBRP Value	FFTF Value	Reason for Difference
Types of descriminators (orificing zones)	6	3	Core arrangement and core size
Lower shielding length (inches)	20.0 (1 piece)	21.5 (3 piece)	FFTF closed loop cooling not required in CRBRP
Duct load pad (inches):			
—Outside dimension	4.745	4.715	Accommodate larger seismic loads in larger core
—Wall thickness	0.205	0.190	Provide more space for irradiation induced deformation in higher burnup reloads
Fuel rod growth clearance (inches)	2.10	1.00	Provide more space for irradiation induced deformation in higher burnup reloads
Type of top load pad (outlet nozzle)	Fixed	Floating collar	Evolution of creep and swelling equations for core restraint
Misaligned grapple pickup capability (inches)	1.75	1.25.	Allow for more tolerance stackup in larger CRBRP core

6185-15

CRBRP FUEL ROD



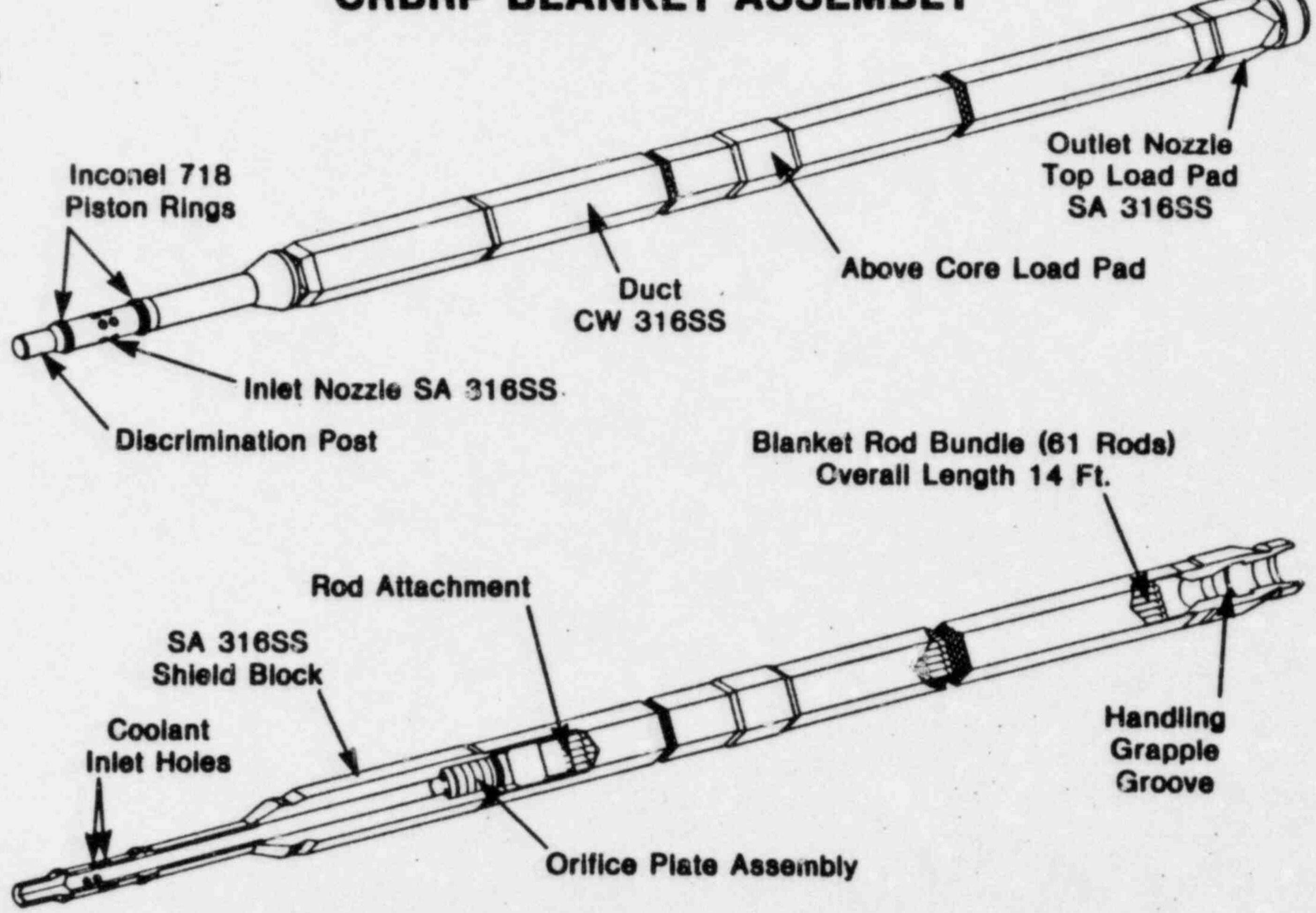
6115-1

CRBRP FUEL ROD COMPARISON WITH FFTF

Design Parameter	CRBRP Value	FFTF Value	Reason for Difference
Pellet PuO_2 content	0.33	0.225/0.275	More power per assembly in CRBRP heterogeneous core
Pellet density (percent of theoretical)	91.3	90.4	Reduced FCMI for same smeared density
Pellet diameter (inch)	0.1935	0.1945	
Axial blanket stack lengths (inch)	14.0	0.8	Breeding requirements of CRBRP
Inconel reflector lengths (inch)	0.0	5.7	Shielding provided by axial blankets
Fission gas plenum length (inch)	48.0	42.0	Provide more space for accomodation of fission gas in higher burnup reloads
Overall rod length (inch)	114.4	93.4	As above

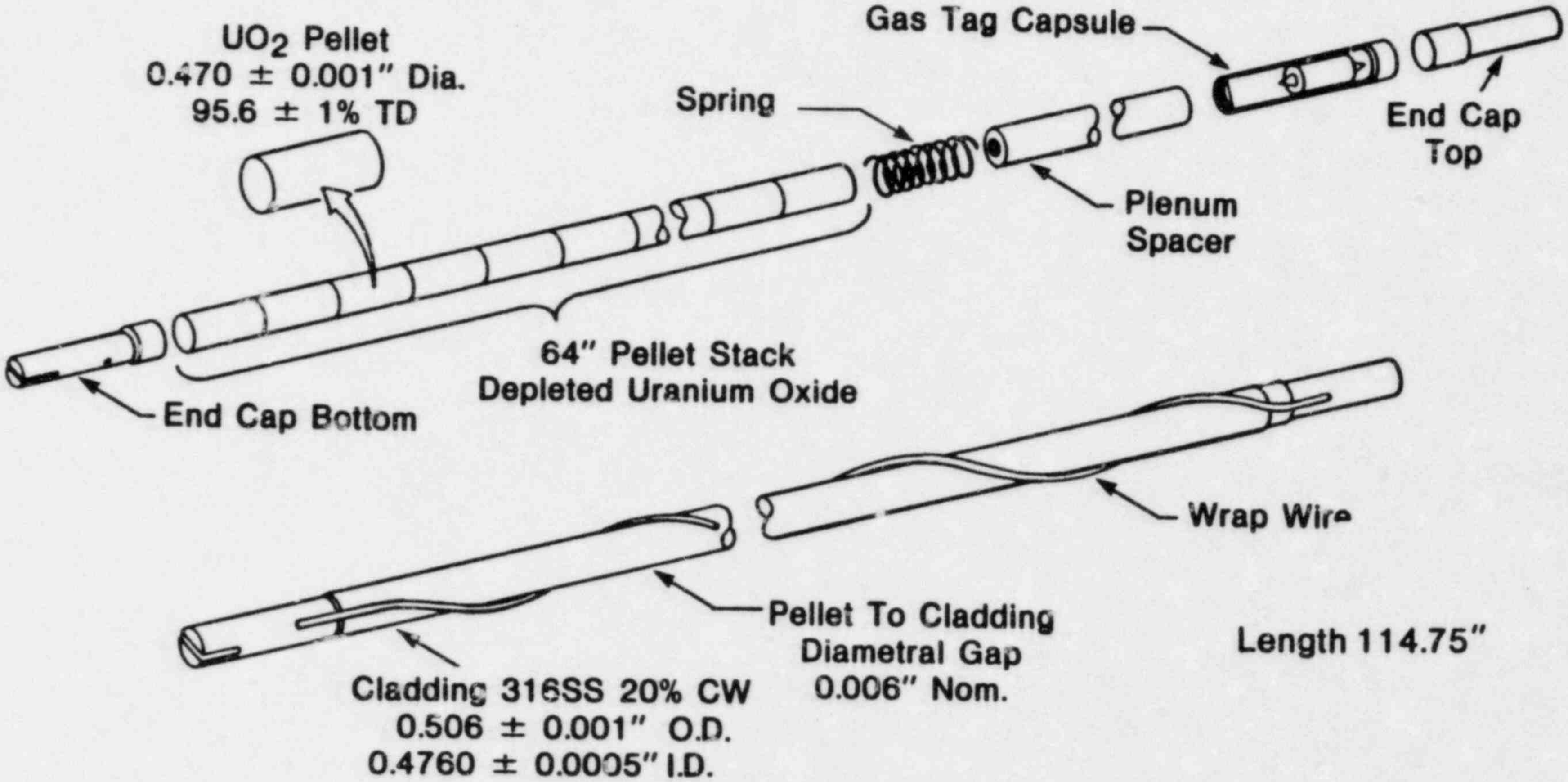
6185-16

CRBRP BLANKET ASSEMBLY



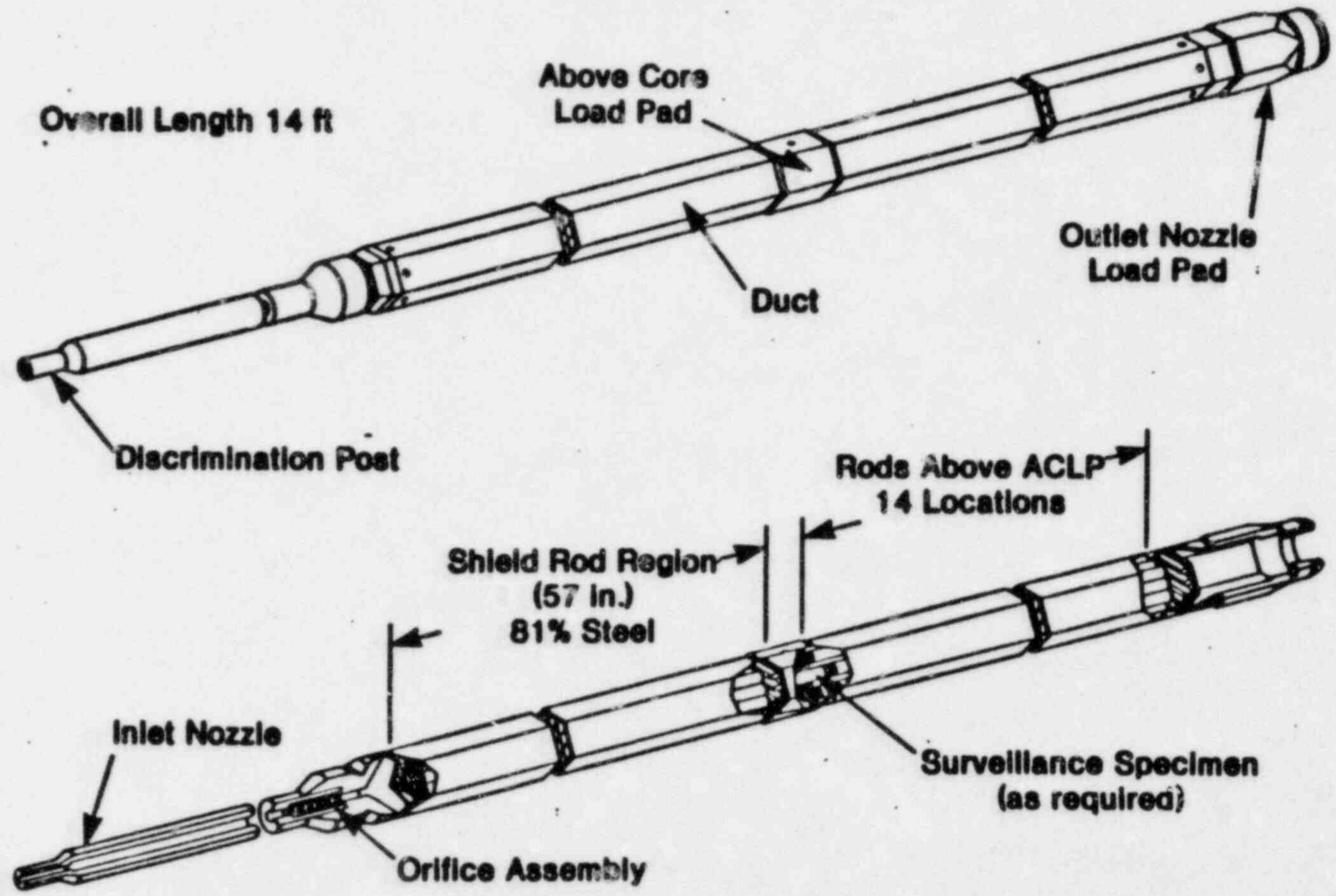
6115-5

CRBR BLANKET ROD



6112-28

REMOVABLE RADIAL SHIELD ASSEMBLY



6115-3

DESIGN EVALUATIONS - KEY FUEL RESULTS

Cladding damage

- Cladding damage is within design limits
  - 35 percent margin on steady-state cumulative damage
  - 75 percent margin on steady-state ductility limited strain
  - 2 percent margin on steady-state and transient ductility limited strain
  - 8 percent margin on steady-state and transient ductility limited strain

Wire wrap

- Maximum wire wrap stress and strain are below limits of 21 ksi and 0.6%
- Maximum wire slack is 0.1 inch; acceptable

Bundle/duct interaction

- Maximum bundle/duct interference of 0.020 inches below limit of 0.056 inches
- The maximum bundle/duct clearance of 0.04 inches is less than the 0.054 inches (6 mils/ring) limit

Duct dilation

- The maximum duct dilation is ~ 80 mils which is less than the limit of 108 mils

DESIGN EVALUATIONS - KEY BLANKET RESULTS

Cladding damage

- Cladding damage is within design limits
  - 68 percent margin on steady-state cumulative damage (Radial)
  - 250 percent margin on steady-state ductility limited strain (Radial)
  - 9 percent margin on steady-state and transient ductility limited strain (Inner)
  - 600 percent margin on steady-state and transient ductility limited strain (Radial)
  - Margins are not reduced due to FCMI from a mid-life power jump

Wire wrap

- Maximum wire wrap stress and strain are below limits of 21 ksi and 0.3%
- Maximum wire slack is $<$ 0.1 inch

Bundle/duct interaction

- Maximum bundle duct interference of ~ 0.013 inches is below the design guideline 0.033 inch
- Maximum bundle/duct clearance of ~ 0.065 mils. Adequate based on testing
- Adequacy of design due to unique blanket features (stiffness) to be obtained from EBR-II and FFTF irradiation testing (WBA-40, 41, 45)

Duct dilation

- The maximum duct dilation is 67 mils for the IBA and 82 mils for the RBA which is less than the limit of 108 mils

STATUS OF DEVELOPMENT TESTING FOR BLANKET SUPPORT

Title	Supporting Information	Status
R.B. heat transfer test	Verification of heat transfer behavior	Testing > 90% complete
Blanket rod irradiation testing in EBR-II	Verification of steady-state performance	Two tests complete, post-test evaluations complete
Blanket assembly irradiation testing in FFTF	Verification of steady-state performance	Two experiments in FFTF, instrumented blanket test being fabricated
Blanket flow control testing	Provide orificing data	Testing complete
Blanket bundle compaction test	Verification of rod bundle behavior	Testing complete
Blanket mechanical testing	Verification of design adequacy	Testing complete
Blanket assembly flow and vibration testing	Verification of flow vibration characteristics	Testing complete
Duct load pad strength and bending stiffness test	Verification of duct behavior	Testing 80% complete
Cladding rupture test	Verification of cladding behavior	Testing complete
EBR-II duct crushing test	Verification of irradiated duct behavior	Testing complete

KEY FUEL AND BLANKET ONGOING DEVELOPMENT TESTING

- Effects of axial blankets on fuel pins
  - CRBR-1, CRBR-3, CRBR-5, D9-4, AB-1
- 33% Pu content in CRBRP fuel
  - PIE of ANL-08 (30-40% Pu)
  - CRBR-3 and CRBR-5 experiments
  - FFTF reload fuel ~ 30% Pu
- Link FFTF data base to EBR-II data base
- Slow overpower transient response
  - WSA-10 and WBA-24 tests completed
  - Operational reliability testing program in EBR-II
  - Slow ramp rate FCTT testing
  - TREAT transient testing
- RBCB testing in EBR-II
- FFTF blanket confirmatory testing
  - WBA-40, WBA-41, WBA-45/46

SUMMARY OF DEVELOPMENT PROGRAMS

- EBR-II fuel and blanket steady-state testing completed
- TREAT testing of reference EBR-II fuel rods completed
- Major FCTT testing completed - testing to link different heats of materials is ongoing
- Slow overpower and RBCB testing in EBR-II (ORT) ongoing
- Future TREAT testing of FFTF and CRBRP prototype rods is planned and ongoing

DEVELOPMENT TESTING FOR FUEL SUPPORT

Title	Supporting Information	Status
Assembly flow and vibration	Verification of flow, vibration characteristics	Complete
Inlet/outlet nozzle feature tests	Verification of design adequacy	Complete
Fuel transient performance	Verification of transient performance	EBR-II/TREAT testing partially completed, FFTF and CRBRP testing to be done in EBR-II and TREAT
Fuel steady-state irradiation	Verification of steady-state performance	EBR-II testing complete, FFTF testing initiated
Reference cladding/duct material	Irradiation induced swelling, in-reactor deformation, post-irradiation tensile properties, post-irradiation fracture, cumulative damage	EBR-II testing essentially complete; FFTF testing planned
Run beyond cladding breach	Establish feasibility and allowable operating time of breached rods/assemblies	EBR-II irradiations in progress

CONCLUSION

- The fuel and blanket design limits have been derived from damage severity limits
- Analysis and testing to date have shown that core design limits are met
- Major testing programs are complete. Extension of the EBR-II and TREAT data base to CRBRP specific design is ongoing

CLINCH RIVER BREEDER REACTOR PLANT

BRIEFING FOR:

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS) FULL COMMITTEE

FLUID SYSTEM INTERFACES

PRESENTED BY:

G. H. CLARE
LICENSING MANAGER,
CRBRP PROJECT
WESTINGHOUSE
ADVANCED REACTORS DIVISION
OAK RIDGE SITE

FEBRUARY 11, 1983

2-83-3229-17

PRIMARY SODIUM COOLANT SYSTEM

N_2 ENVIRONMENT IN RCB CELLS

- PASSIVE BOUNDARY
- Na LEAKAGE DETECTION
- LEAK ACCOMMODATION
- N_2 ISOLATED FROM COOLING WATER BY
  - PASSIVE BOUNDARY
  - ISOLATION VALVES
  - H_2O LEAKAGE DETECTION (MOISTURE DETECTOR AND LEVEL DETECTOR)
  - LEAKAGE COLLECTION (DRAIN LINES FROM COOLERS)

INTERMEDIATE SODIUM COOLANT SYSTEM

- PASSIVE BOUNDARY (IHX)
- (P)IHTS > (P)PHTS
- LEAK DETECTION
- LEAK ACCOMMODATION

NaK COOLANT SYSTEMS - DHRS AND COLD TRAPS

- PASSIVE BOUNDARY (OHX AND COLD TRAPS)
- (P)NaK > (P)Na
- LEAK DETECTION
- COLD TRAP NaK ISOLATED FROM COOLING WATER BY DOWTHERM COOLANT LOOP

SODIUM - NaK COMPATIBILITY

- NaK IS 22 W/O Na AND 78 W/O K (EUTECTIC MIXTURE)
  - MELTING TEMPERATURE ~9°F
  - BOILING TEMPERATURE (1 ATM) ~1518°F
- MIXING OF Na AND NaK WOULD RESULT IN
  - NO CHEMICAL REACTION
  - NO ADVERSE EFFECT ON PROCESS EQUIPMENT
  - INCREASE IN NaK MELTING TEMPERATURE
  - DECREASE IN Na MELTING TEMPERATURE

2-83-3229-2

INTERMEDIATE SODIUM COOLANT SYSTEM

AIR ENVIRONMENT IN SGB CELLS

- PASSIVE BOUNDARY
- LEAK DETECTION
- CATCH PANS
- FIRE SUPPRESSION DECKS
- LOOP SEPARATION
- CELL PRESSURE RELIEF
- AEROSOL MITIGATION
  - EQUIPMENT QUALIFICATION

STEAM/WATER SYSTEM

- PASSIVE BOUNDARY (SG MODULES)
- LEAK DETECTION
- LEAKAGE ACCOMMODATION
  - EXPANSION TANK RUPTURE DISKS
  - MAIN RUPTURE DISKS
  - REACTION PRODUCT SEPARATION SYSTEM
  - WATER DUMP SYSTEM
  - SAFETY RELIEF VALVES

N_2 ENVIRONMENT IN RCB CELLS

- PASSIVE BOUNDARY (PIPING)
- Na LEAKAGE DETECTION
- LEAK ACCOMODATION
- N_2 ISOLATED FROM COOLING WATER BY
  - PASSIVE BOUNDARY
  - ISOLATION VALVES
  - H_2O LEAKAGE DETECTION (MOISTURE DETECTOR AND LEVEL DETECTOR)
  - LEAKAGE COLLECTION (DRAIN LINES FROM COOLERS)

2-83-3229-4

EVST SODIUM COOLANT SYSTEM

N_2 ENVIRONMENT IN RCB CELLS

- PASSIVE BOUNDARY
- Na LEAKAGE DETECTION
- LEAK ACCOMADATION
- N_2 ISOLATED FROM COOLING WATER BY
  - PASSIVE BOUNDARY
  - LEAKAGE DETECTION (MOISTURE DETECTOR AND LEVEL DETECTOR)
  - LEAKAGE COLLECTION (DRAIN LINES FROM COOLERS)

NaK COOLANT SYSTEMS

- PASSIVE BOUNDARY (SODIUM COOLERS)
- (P)NaK > (P)Na
- LEAK DETECTION
- LEAK ACCOMMODATION
- NaK ISOLATED FROM N_2 ENVIRONMENT BY
  - PASSIVE BOUNDARY
  - LEAK DETECTION
  - LEAK ACCOMODATION
- NaK ISOLATED FROM AIR ENVIRONMENT BY
  - PASSIVE BOUNDARY
  - LEAK DETECTION
  - LEAK ACCOMMODATION

ARGON COVER GAS

PRIMARY SODIUM COOLANT SYSTEM

- DIRECT INTERFACE WITH Na COOLANT FREE SURFACE
- (P)ARGON > 1 ATM (EQUAL TO THE SODIUM PRESSURE AT FREE SURFACE)
- PURITY MONITORING
- RADIOACTIVE ARGON PROCESSED TO REMOVE FISSION GAS

INTERMEDIATE SODIUM COOLANT SYSTEM

- DIRECT INTERFACE WITH Na COOLANT FREE SURFACE
- (P)ARGON > 1 ATM (EQUAL TO THE SODIUM PRESSURE AT FREE SURFACE)
- PURITY MONITORING
- NON-RADIOACTIVE

FUEL HANDLING CELL

- DIRECT INTERFACE WITH Na COOLANT FREE SURFACE
- (P)ARGON > 1 ATM
- ATMOSPHERE PURIFICATION UNIT REMOVES O_2 AND H_2O

EVST SODIUM COOLANT SYSTEM

- DIRECT INTERFACE WITH Na COOLANT FREE SURFACE
- (P)ARGON > 1 ATM (EQUAL TO THE SODIUM PRESSURE AT FREE SURFACE)
- PURITY MONITORING

2-83-3229-3

CLINCH RIVER BREEDER REACTOR PLANT

BRIEFING FOR:

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS (ACRS) FULL COMMITTEE

STEAM GENERATOR LEAKS

PRESENTED BY:

G. H. CLARE
LICENSING MANAGER,
CRBRP PROJECT
WESTINGHOUSE
ADVANCED REACTORS DIVISION
OAK RIDGE SITE

FEBRUARY 11, 1983

2-83-3229-19

THE INDIRECT EFFECTS OF STEAM GENERATOR TUBE LEAKS COULD POTENTIALLY IMPACT SAFETY

- REACTOR SHUTDOWN WITH LESS SHUTDOWN HEAT REMOVAL CAPACITY
- MECHANICAL LOADINGS ON THE PRIMARY AND INTERMEDIATE COOLANT BOUNDARIES
- HYDROGEN GENERATION

MULTIPLE HTS HEAT REMOVAL PATHS AND OPERATOR FLEXIBILITY TO ISOLATE, REPAIR, OR REPLACE A LEAKING STEAM GENERATOR MODULE AND THE DHRS (INDEPENDENT OF STEAM GENERATORS) MITIGATE THE EFFECTS OF SG TUBE LEAKS ON SHRS CAPABILITY.

2-83-3229-9

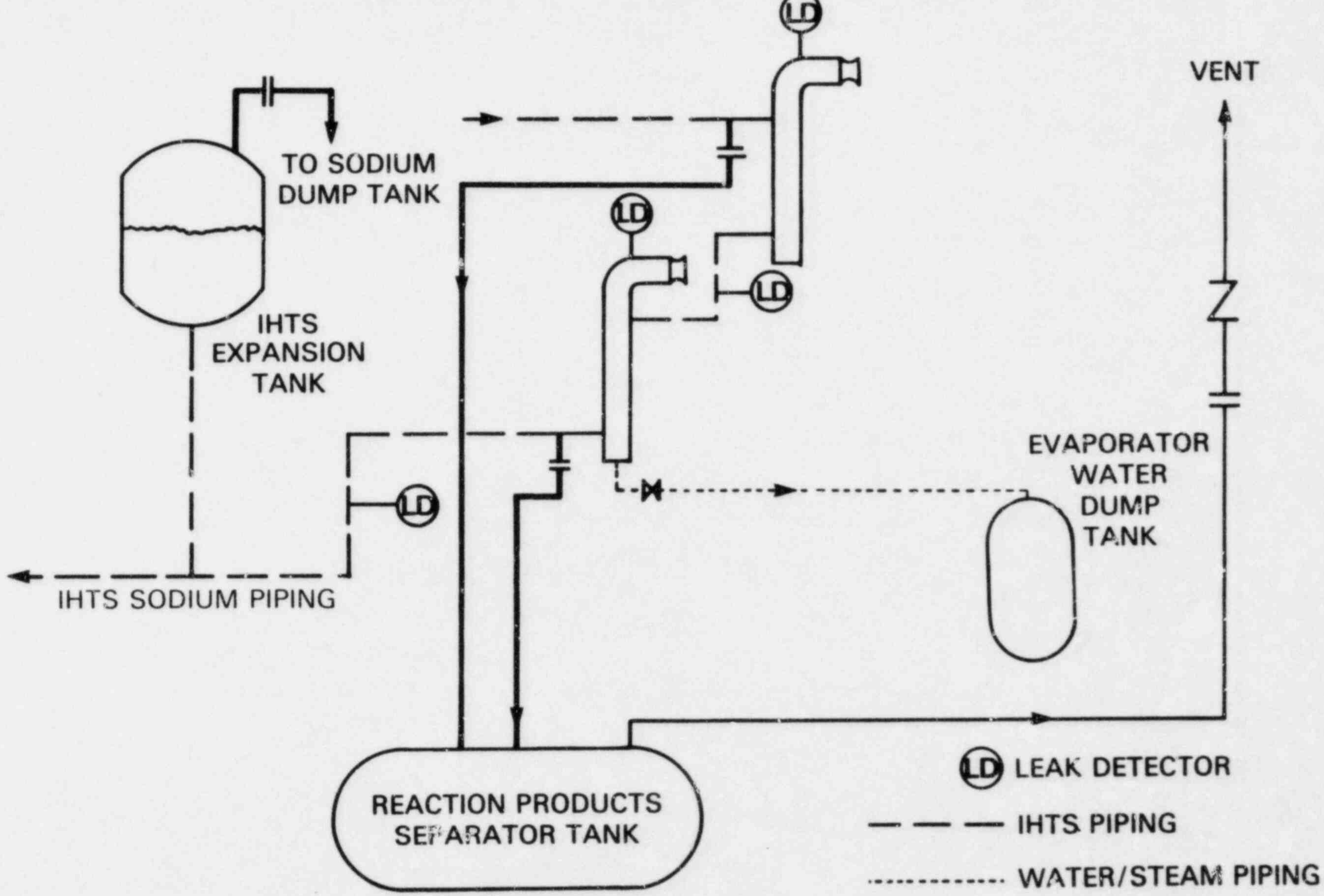
THREE LEVELS OF PROTECTION ARE PROVIDED AGAINST THE EFFECTS OF SG TUBE LEAKS

- LEAK DETECTION WITH MANUAL REACTOR SHUTDOWN
- EXPANSION TANK RUPTURE DISKS WITH AUTOMATIC WATER DUMP
- MAIN RUPTURE DISKS WITH AUTOMATIC REACTOR SHUTDOWN AND WATER DUMP

SODIUM WATER REACTION PRESSURE RELIEF SYSTEM (SWRPRS)

2-83-3229-6

CRBRP SODIUM WATER REACTION PRESSURE RELIEF SYSTEM



2 83 WL2520 30(OV3229 7)

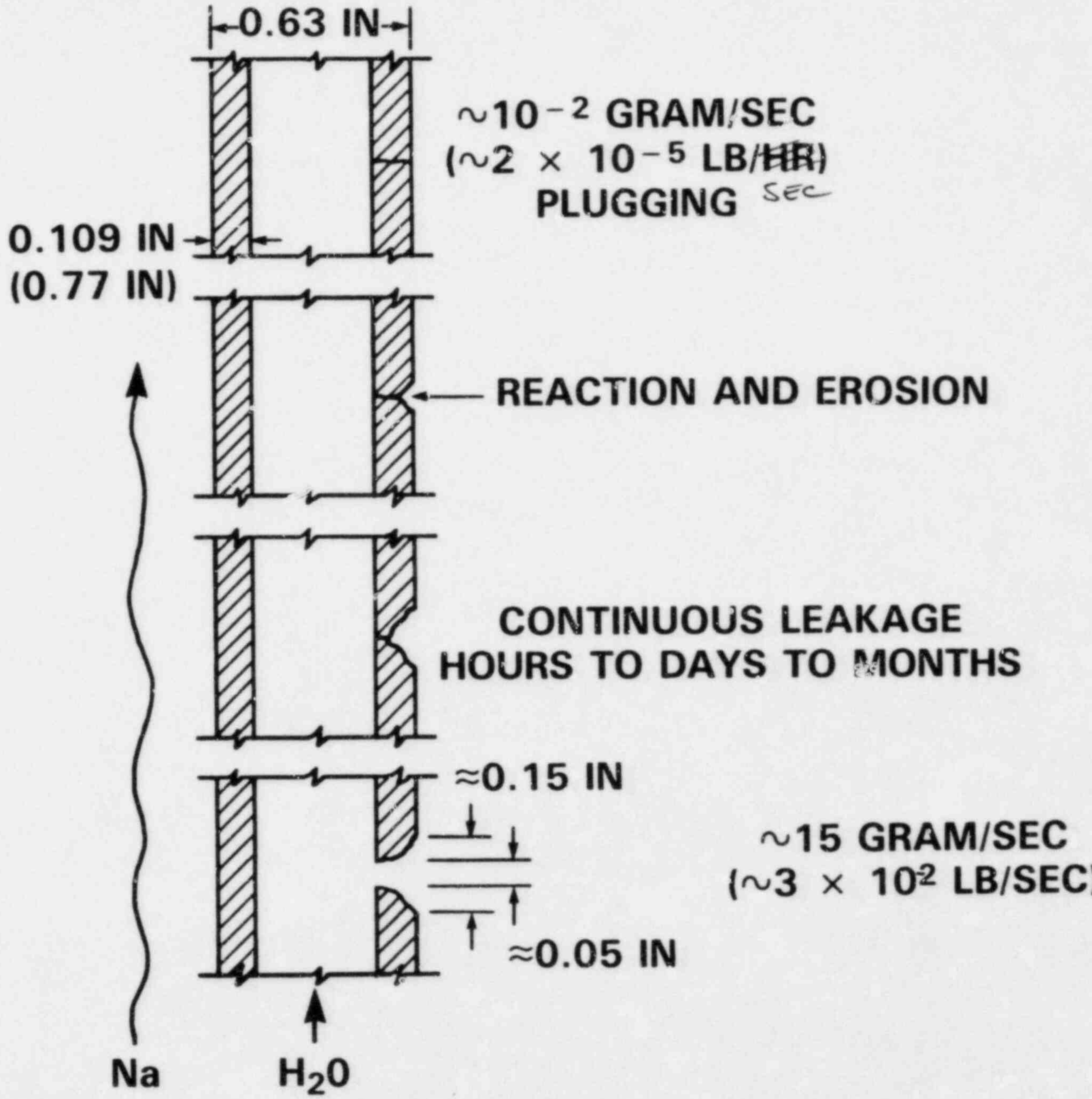
THE DESIGN BASIS ACCIDENT FOR SWRPRS AND THE PRIMARY AND INTERMEDIATE COOLANT BOUNDARIES WAS SELECTED USING CONSERVATIVE ENGINEERING JUDGEMENT CONSIDERING REACTOR EXPERIENCE, EXPERIMENTAL DATA, AND ANALYSIS RESULTS

- SIZE OF LEAK(S)
- NUMBER OF LEAKS
- TIMING

ONLY EXTREMELY RAPID EVENT PROPAGATION IS PERTINENT DUE TO RAPID PRESSURE RELIEF (FEW SECONDS)

2-83-3229-8

DEVELOPMENT OF SWR PRECURSOR



2-83-3229-10

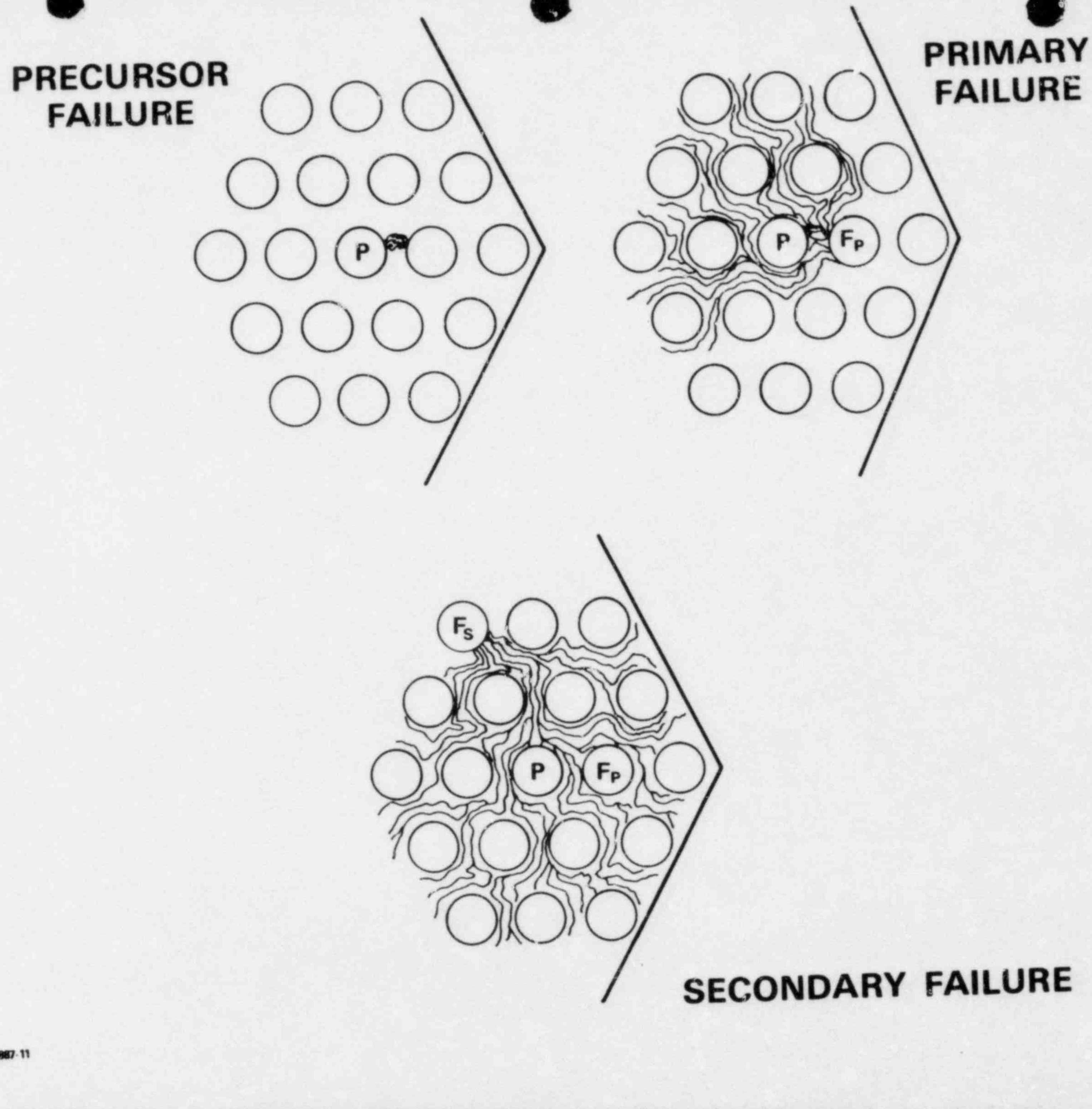
THREE MECHANISMS CAN CAUSE TUBE-TO-TUBE FAILURE PROPAGATION

- WASTAGE
- CORROSION

  } – EXPERIMENTAL – TENS OF SECONDS

- STRESS RUPTURE (OVERHEATED TUBE)
  - EXPERIMENTAL –≈10 SECONDS
  - BOUNDING ANALYSIS–≈1 SECOND

STRESS RUPTURE FAILURES ARE LIMITED IN SIZE: 45° GAP, 1 1/2 INCHES LONG, LESS THAN 50% DEG.


PRECURSOR FAILURE
P
PRIMARY FAILURE
P
F_P
SECONDARY FAILURE
F_S
P
F_P

4-82-2887-11

SWR EXPERIMENT SUMMARY

- 63 LARGE LEAK TESTS
- SECONDARY FAILURE IN 4 TESTS ONLY
- NINE U.S. TESTS (LLTR) SPECIFICALLY CRBRP PROTOTYPIC—TWO TESTS PRODUCED SECONDARY FAILURES
- SECONDARY FAILURES OCCURRED IN TENS OF SECONDS

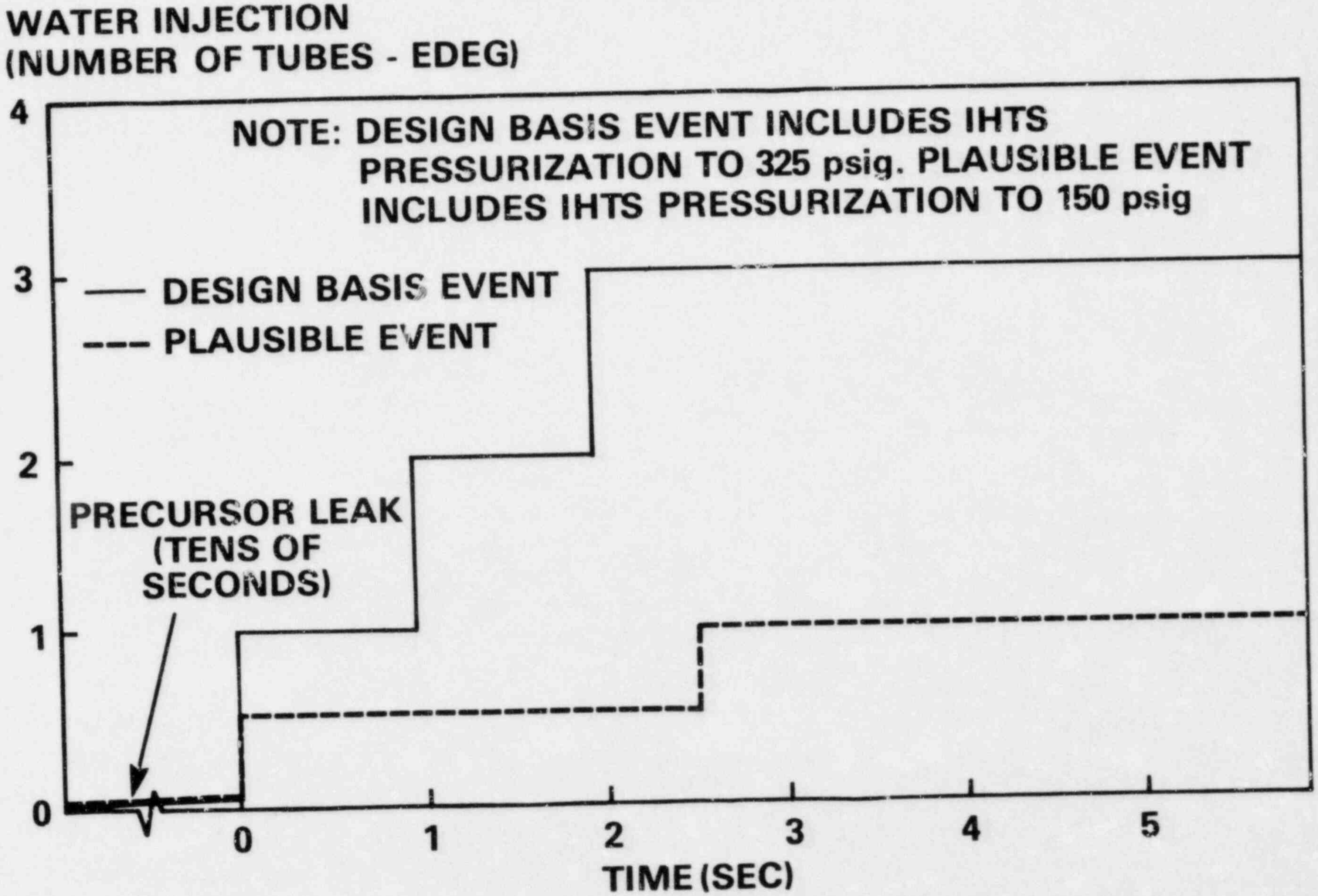
2-83-2887-13-1

DESIGN BASIS SODIUM WATER REACTION EVENT

- PRECURSOR - SODIUM PRESSURE - 325 PSIG
- PRIMARY FAILURE - 1 EDEG @ T = 0
- SECONDARY FAILURE - 1 EDEG @ 1 SEC.
- TERTIARY FAILURE - 1 EDEG @ 2 SEC.

6-82-2887-17

PLAUSIBLE EVENT VS DESIGN BASIS EVENT



8-82-2807-16

COMPARISON WITH FOREIGN SWR DESIGN EVENTS

COUNTRY	FAILURE SIZE	NUMBER OF FAILURES	INTERVAL BETWEEN FAILURES
• UK	1 EDEG	3*	1 SEC
• GERMANY	1 EDEG	1	NA
• FRANCE	1 EDEG	1	NA
• JAPAN	1 EDEG	4**	UNKNOWN
• US	1 EDEG	3	1 SEC

*NOT A LICENSING DESIGN BASIS ACCIDENT

**ONLY (1) ONE FOR LICENSING PURPOSES

6-82-2887-19

LARGE SWR EVENTS ARE CONSERVATIVELY EVALUATED USING THE TRANSWRAP COMPUTER CODE

- WORST LEAK LOCATION AND INITIAL CONDITIONS BASED ON SENSITIVITY STUDIES (EVAPORATOR AFTER LOSS OF OFFSITE POWER)
- LEAK RATES ESTABLISHED USING RELAP 4/MOD 5
- ASSUMED HYDROGEN YIELD OF 65% AND 1700°F REACTION ZONE TEMPERATURE WHICH BOUNDS EXPERIMENTAL RESULTS
- DYNAMIC ELASTIC-PLASTIC RUPTURE DISK RESPONSE MODEL BASED ON EXPERIMENTAL RESULTS

2-83-3229-12

MECHANICAL LOADINGS FROM SWR EVENTS ARE CONSERVATIVELY PREDICTED USING TRANSWRAP

- SODIUM COMPRESSIBILITY MODEL
- ONE-DIMENSIONAL "SODIUM HAMMER" MODEL
- FRICTION EFFECTS MODEL
- ENERGY CONSUMED IN PIPING STRAIN IS NOT ACCOUNTED FOR

VALIDATED USING EXPERIMENTAL DATA FROM THE LARGE LEAK TEST RIG PROGRAM.

2-83-3229-13

SUMMARY

- THE DESIGN BASIS SWR EVENT IS CONSERVATIVE RELATIVE TO EXPERIMENTAL & ANALYTICAL EVIDENCE
  - PRECURSOR PRESSURE
  - SIZE OF FIRST FAILURE
  - TIMING AND SIZE OF SECOND FAILURE
  - EXISTENCE OF THIRD FAILURE
  - COMPARED WITH FOREIGN DESIGN BASIS EVENTS
- THE TRANSWRAP COMPUTER CODE IS USED TO CONSERVATIVELY MODEL THE CONSEQUENCES OF THE DESIGN BASIS SWR EVENT
  - LEAK RATE
  - REACTION PRODUCTS
  - MECHANICAL LOADS

2-83-3229-14

STEAM GENERATOR MODULE FAILURE RATE ESTIMATES

- THE STEAM GENERATOR MODULE ARE FIRST-OF-A-KIND COMPONENTS, AND THERE IS NOT EXTENSIVE OPERATIONAL NOR TESTING DATA FROM SIMILAR COMPONENTS IN SIMILAR ENVIRONMENTS.

BASIS

- THE FOLLOWING TYPES OF UNITS WERE INCLUDED IN THE REVIEW OF HISTORICAL EXPERIENCE OF TUBE FAILURES:
  - FOSSIL-FUELED PLANTS
  - LWR PLANTS
  - SODIUM-HEATED STEAM GENERATORS, INCLUDING BOTH THERMAL AND FAST-REACTOR POWERED UNITS, AND VARIOUS TEST UNITS.
- ENGINEERING JUDGEMENT WAS USED TO DERIVE THE CRBRP STEAM GENERATOR FAILURE PARAMETERS FROM THE HISTORICAL DATA.

CRBRP STEAM GENERATOR MODULE FAILURE RATE ESTIMATES FOR RELIABILITY STUDIES

- WATER-TO-SODIUM LEAKAGE
  - SMALL LEAK: $\lambda = 7.0 \times 10^{-6}$ HR-MODULE; LEAK RATE LESS THAN .01 LB/SEC
  - MEDIUM LEAK: $\lambda = 1.4 \times 10^{-6}$ HR-MODULE; LEAK RATE BETWEEN .01 AND 5 LB/SEC
  - LARGE LEAK: $\lambda = 0.28 \times 10^{-6}$ HR-MODULE; LEAK RATE GREATER THAN 5 LB/SEC