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UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

Combined Meeting
CRBR Subcommittee/Structures and Materials
Working Group

Room 1046
1717 H Street NW
Washington, D.C.
Wednesday, March 16, 1983

The combined subcommittees met at 8:30 a.m., pursuant to notice, Max Carbon presiding.

Present for the ACRS:

M. Carbon, Member
P. Shewmon "
J. Mark "
S. Bush, Consultant
W. Lipinski, "
Z. Zudans, "

P. Boehnert, DFE

Present for the Applicant:

R. Dickson
R. Gross
G. Nickodemus
W. O'Bryant
L. Strawbridge
W. Pennell
P. Planchon
R. Palm
S. Niemcyzk
R. Markley
A. Schwallie
R. Tilbrook
R. Tinder

Present for the Staff:
R. Stark
T. King
H. Holtz
T. Butler

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delete B. White

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P R O C E E D I N G S

MR. CARBON: The meeting will now come to order. This is a combined meeting with the Advisory Committee on Reactor Safeguards Subcommittee or the CRBR and the Structures and Materials Working Group. I am Dr. Carbon, subcommittee chairman. The other ACRS members present today are: Drs. Shewmon and Mark.

We also have present ACRS consultants: Drs. Bush, Lipinski, and Zudans.

The purpose of this meeting is to continue review of the DOE CP application for CRBR. Addressed will be topics of fuel failure propagation, accident recovery and emergency planning, and items from the Structures and Materials Working Group review, including in-service inspection, core support structure integrity, loose-parts monitoring.

This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the Government in the Sunshine Act. Paul Boehnert is the Designated Federal Employee for the meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on Wednesday, March 2, 1983. A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice. It is requested that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard.

We have received no written statements from members of the public. We have received no requests for time to make oral statements from members of the public.

Before we begin the meeting, I would call upon Dr. Shewmon. Do you have any comments, Paul, to make in the way of introduction to in-service inspection?

MR. SHEWMON: I don't think so.

MR. CARBON: I have no particular comments either.

On item number 5, accident recovery, I would emphasize that that item is recovery after a hypothetical accident, and it is an item which Mike Bender is particularly interested in. With regard to local fuel failure presentation in the afternoon, the last presentation today, I would comment that this is a topic that used to be of considerable concern but it apparently is of much less concern at this time. And we will be looking forward to the project presentation, and it looks as if the Staff has arrived. And does anyone else, any of the consultants or, Carson, do you have any comments or questions? Are you ready?

Let us proceed with the meeting. I will call on Mr. Stark of the NRC Staff.

MR. SHEWMON: Who hits the ground running.

MR. STARK: Good morning. My name is Richard Stark, for the NRC Staff. I would like in the first presentation to discuss the objectives and findings of our in-service and pre-service inspection review. And I want to give you some ideas here on the first slide, which I will come back to on later slides.

(Slide)

We will discuss what we thought were reasonable objectives. We wanted to make sure that fabrications examination for vessels or piping or whatever had the best available base or would yield the best available line data. So we concentrated heavily on looking at heat exchanger vessels, piping, and RFER, particularly in Chapter 4 and Chapter 5 is the details of the pre-service examinations of interest.

The other thing that is kind of a parallel to this, we are also requiring that if for any reason you have to get into the plant and make a modification, reweld a given area or put a modification in particular piping, we want to make sure that that baseline information is again achieved, so we are saying that the examination similar to what was done in fabrication be considered in the plant design and that anytime you do a repair or modification, that you again try to make sure that you get good baseline data, you are sure that you don't have large flaws or large cracks.

The second part is concentrated on -- we will also look at the type of in-service inspection equipment that is available, and we want to talk about future ISI or planned ISI, as you probably know. Ultrasonic in-service inspection at 400 degrees or above right now is not now an extremely reliable device, but our thought is we wanted to keep provisions in it flexible for in-service inspection or throughout the whole construction phase, for the following reasons: If someone were to develop a better UT probe that would work, a more reliable probe, or we want to make sure that that piping insulation had been removed -- because if we could achieve better in-service inspection, we would do so.

MR. SHEWMON: Are you going to talk about what criteria you will use for deciding what needed to be inspected and what didn't?

MR. STARK: I guess I will give some examples. For pre-service inspection we are requiring that they fully inspect all welds, all vessels, tanks, liners, piping and --

MR. SHEWMON: Internal brackets and studs or just pressure valves? I can ask Martin.

Why don't you use the mike?

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MR. HUM: While he is focusing on the inspection, we want to be able to get the best possible examination and we are requiring inpsections on the piping system and the vessels plus a UT examination of the piping system. There has been no special additional requirements on bolts, other than what would be --

MR. SHEWMON: I was thinking in terms of welding.

MR. HUM: Well, I was thinking about the closure and things like that. In this position on the special requirements for the internals, however, the Applicant is addressing the appropriate surveillance requirement for the reactor vessel internals.

MR. SHEWMON: You didn't really care whether they inspected the core support or not?

MR. HUM: No, sir.

MR. SHEWMON: Are you sure you didn't care?

MR. STARK: We required them to do it.

MR. SHEWMON: What I'm trying to get at, anything that was welded, you would have to inspect, is that the criteria?

MR. STARK: Well, we looked at pressure boundaries primarily, and after that we looked at other key areas inside. We looked at in-service inspection for the IHX. We also looked at the supports for the internals for the reactor vessel. We will be addressing that.

MR. SHEWMON: Is there a list of those things in the SER that you consider important and check?

MR. STARK: Yes. As I said, I can pick the sections that will show that to you. I don't know if the list is going to be so exhaustive that it is going to tell you everything, but we try to look at every component and try to look at both the pressure boundary integrity and we try to look, if it's a heat exchanger or vessel, what was inside, and if some of those ought to be addressed.

MR. MARK: I'm sure you won't be able to answer this, but I would like to raise the question, and probably will raise it again:

In connection with in-service inspection, where you said you are going to press for all you can get, with every reactor in-service inspection, there is some number of manrem received with probability one.

MR. STARK: Yes.

MR. MARK: And it might save some manrem with a probability less than one and perhaps a great deal less than one if something weren't turned up by such inspection. Do you ever consider trying to balance those against each other?

MR. STARK: We do. As a matter of fact, one of the slides that will come up will address that item. I'll give you the answer right now. On the primary loops, we are only going to do this in-service inspection -- if I go back one particular slide, we are only going to ask them to reconfirm or reverify if they have to be down for maintenance. So if they are down and they are doing a repair or modification and they have paid the ALARA penalty, then we will ask them to go ahead and do that particular in-service inspection or fabrication examination. But on the intermediate heat exchanger, we are planning that they look generically at welds in key areas, and the intermediate loop is not a radioactive loop. So I think the question you are asking, are we considering trying to get this, and also the person is being exposed to perhaps more exposure than he needs, I think you will see in the next couple of slides we are trying to do a little bit of both -- get a generic understanding of looking at these high temperature welds without burning up people at the same time.

MR. MARK: Well, it is just that point that I think has not always been given as much attention as it should, and if you are giving it more than it has formerly received, I am very pleased.

MR. STARK: Okay.

This slide, Martin just discussed the three items here, so I'll go on to the next slide and not repeat what is on this particular slide. I should have put the next slide up here.

Again, this is a point that I was just trying to make right now. As far as I understand, you are saying that you should do examinations and this is on the primary loop we are talking about -- if you have to do a required maintenance, the only time we will be doing it is during required maintenance and not whenever maintenance isn't required, so that we can reduce the operational exposure, and also plant outages.

The temperatures are the same. Sodium is flowing through both loops. The materials are the same. We think there will be generic information gathered from the IHX.

This particular bullet doesn't really add much to the ISS story.

Some of the topics that are still being reviewed by the Applicant -- and I think this is what Martin was referring to and it kind of reflects an answer to Dr. Shewmon's earlier question -- we, in talking to the Applicant, were discussing inspection on -- periodic inspection or verification of the internals. In addition, we are talking about the intermediate. If you have a leak, how do you know you have a leak, how do you fix it, and how do you inspect it. This is an item that we have been discussing with the Applicant, and we have some techniques.

What we have been doing in this area is, what is this component, how do you inspect it, what will you do if you get in this particular situation.

Now in this item, right after I finish my discussion, the Applicant is going to give you a report on the surveillance procedures that exist for the reactor internals. The Staff requires periodic verification or inspection, whatever the correct words are, for reactor internals, and in addition we are requiring that they have the capability to inspect IHX. If you ever have to come down for maintenance, we would like to know what the status of the other tubes is before you start back up.

I talked about the operating license review and our desire to incorporate a strong ISI program, especially if an ISI program matures and develops into something that is very useful. So we think that an item that we have addressed in our ISI, in our review consideration, is that we want the Staff and the Applicant to look very carefully at the status of the nondestructive technology at that particular point to see to what extent we can use it, to see what happens to it for in-service inspection.

Also, during the OL review, we will designate the specific locations and the methods and the frequency for in-service inspections.

MR. BUSH: Could I ask -- in that respect, I would classify the usage from sometimes fair to poor.

MR. STARK: I think we agree.

MR. BUSH: Certainly with the existing techniques. There are two options: You can modify the techniques or you can go to semiautomatic or automatic procedures, which takes the water out of the loop, which is a plus.

Are you considering that aspect, as well as what I would call the calbiration evaluation aspect? Because I think it is amply proven in these sections that calibrating on a flat bottom doesn't tell you anything about the reliability of detection of cracks, what you are concerned with.

MR. STARK: I think we were considering that. I think we were saying what if the French two years from now developed a technique that worked or a transducer that worked at this high temperature. We would then feel an obligation to try to make sure that this device was incorporated. So what we have done, we have required that the Applicant maintain the provisions for it. It doesn't even have to be in-service inspection. I don't know how radiography would work. If there were a volumetric inspection technique that were promising and reliable, that we have concentrated on as far as the criteria is concerned on maintaining flexibility to go in and provide that particular function later on, access provisions for removable piping, insulation, anything that would lead to keeping that flexibility as long as possible. Certainly throughout the crucial phase so that during our OL review we would evaluate where the technology exists. We don't want to be closed-minded on this because we would agree that it is between lousy and poor, or whatever words you used. But we want to maintain that flexibility as long as we can, and therefore we are keeping that flexibility as a criterion.

I have one more slide now.

(Slide.)

Basically what the Staff has done and what RSR shows is that we are requiring that adequate -- and we find that adequate examinations will be maintained through fabrication PS inspection, and gives us confidence that there are no significant flaws.

I indicated we will get the baseline data from this particular investigation. Any time that I come down to do a maintenance, whether it is a year from now or 10 years from now, we want to make sure that the NDT techniques be used to give you this same confirmation of the baseline data and this assuarnce that we have in the original fabrication, and that we want to look at in-service inspection on a generic basis.

I talked about the intermediate loop, and I also indicated our flexibility and our strong desire to continue to look at ISI over the next -- through the operating license review and to see if that holds any more promise, and that is where we stand.

So, if there are no other questions, I am finished.

MR. CARBON: There is not.

MR. SHEWMON: Spence, the bottom line on this seems to be will we make sure there aren't any in the first place, and we don't know of any reason they should grow in the second place, and if they do, we will look for them. How strong do you look for them when they first go through this, before you start it up?

MR. BUSH: You are talking about using double angle RT or something of this nature?

MR. SHEWMON: Yes.

MR. BUSH: I guess I'm not extremely optimistic about RT and finding tracks. Double angle is much better than zero degree. I think there are other ways I think one might go in, a pancake coil or eddy current, anything of that nature, and probably get a fair feel. There are techniques. I doubt at this stage that either RT or UT would give you the reliability that you want.

I would classify the reliability probably as below 50 percent.

MR. SHEWMON: The welding materials are reasonably well established.

MR. BUSH: I'm not that worried about small cracks and things in here. I would be much more worried by the unpredictable, which is not a severe problem in the liquid metal system. But you can't completely rule out something of that nature.

MR. SHEWMON: Okay. Thanks.

MR. CARBON: Go ahead.

MR. NICKODEMUS: Good morning. I'm Glenn Nickodemus from Westinghouse here today to present the discussion of the high level of assured structural integrity of the reactor vessel core support cone welds, rather than the in-service inspection, as Rich had mentioned.

(Slide.)

Here is a brief summary of what I am about to cover.

Core support cone welds have a high level of assurance for the designed lifetime of the plant. In making that statement, we have considered the following areas:

The cone and the structure is designed, constructed and inspected to rigid ASME code requirements. The welds have an operating temperature of 750 degrees fahrenheit, located in a benign environment. The sodium and thermal aging effects on the material of this location are negligible, and the radiation effects are negligible at these locations. The welds have been purposely located away from geometric discontinuities in most stress regions.

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They meet all the design limits of the ASME code with very substantial margins, and we have done calculations that show a high degree of tolerance to crack growth and instability.

(Slide.)

The new viewgraph is a little bit further idea of the discussion areas. The lower weld located between the cone and the core structure I'll be referring to as a section AA. The upper weld connecting the cone to the reactor vessel is a Section BB. And I'll also in particular be discussing areas around two gas vent holes located 180 degrees apart from the cone.

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MR. SHEWMON: There is a solid membrane that must be -- to adjust fluid flow where it should be.

MR. NICKODEMUS: I beg your pardon?

MR. SHEWMON: Is that a continuous membrane?

MR. NICKODEMUS: This one?

MR. SHEWMON: Yes.

MR. NICKODEMUS: Yes. Not the ASME pressure boundary. It is a boundary between the high-pressure inlet and the low-pressure outlet.

MR. SHEWMON: And the only perforations through it are the gas vents that you are going to talk about?

MR. NICKODEMUS: Yes. And particular concern was paid to the welding and inspection of these welds. It is the weld joints that were inspected prior to welding. Welding procedures and welders themselves were qualified. During the process, each bead was brushed. All starts and stops and each weld were ground. After the interface welding was completed, the welds were surface-ground and visually and liquid-penetrant inspected. The route area of the welds were back-grooved and cleaned and the back side radio area was visually and liquid-penetrant inspected.

The core support was then radiographed for information in the partially completed stage. The outer face welding of both welds was completed. Again, ground and visually and liquid-penetrant radiographed. The welds were surface-finished to the final configuration and a final visual, a liquid-penetrant radiograph inspection was performed.

Very high-quality welding came from the procedures. There were no repairs required in the core support structure weld and only two repairs in the vessel weld, and this was only 3 percent of the circumference. After final repairs of those welds, both welds were cleared for X-ray.

(Slide)

Discuss a bit further some of the conditions existing at the welds: Corrosion is nonexistent because of the low temperature of 750 degrees Fahenheit. Erosion in these areas is negligible because we have very low flow. Carbonization and aging are both negligible. Irradiation is negligible. The embrittlement due to the lower power and temperature is negligible.

MR. BUSH: With regard to aging, you may be right. But I think there is the necessity to at least ask questions. The fact that we have been looking at the case of the pressurized water reactors which are a couple hundred degrees lower than you have here in the stainless-steel, and I believe there is evidence at least of some embrittlement in the 550 to 600 degree regime after about 100,000 hours.

MR. NICKODEMUS: I suspect that the level of embrittlement may not be excessive, but I would say that the statement without technical backup is probably subject to some consideration.

MR. BUSH: I guess the question is, do you have evidence at least of 10 or 20 thousand hours that would validate your statement? I recognize that the delta ferroid vessels isn't extremely high, but there is a reasonable amount.

MR. GRIFFIN: With regard to the question relative to again, the ASME code limits that were used to compare these results to -- do include the effects of aging. It is small. We will address it but it's also included and not only is it negligible, but included in the design limits.

MR. SHEWMON: Is this 304 or 316?

MR. GRIFFIN: 304.

MR. SHEWMON: For the welds or 304 face?

MR. BUSH: We are not concerned with the base metal. I agree with you completely with regard to the base metal. But if I am running high in delta ferroid, I am not sure it's valid.

MR. GRIFFIN: It's not specifically true for the weld metal, but the weld metals have been compared. The limits apply to the base metal, have been compared to weld metal and weldness and everything we have has shown that the weld and the weld metal are covered by the limits.

MR. BUSH: That's what I am asking. What do you have that confirms that? That is the only question. I suspect you are right, but I have yet to see any definitive evidence that that is true.

MR. GRIFFIN: We can present some of those. I referred to them last Thursday in the document that I referred to. That includes the materials data base.

MR. SHEWMON: How does that go out on the welds? Do you know?

MR. GRIFFIN: I can't say offhand. We can get an answer later if you like.

(Slide)

MR. NICKODEMUS: To give you a brief description of some of the structural analyses of some of these welds. This model was used for the analysis of the lower welds, also used for the analysis of the remaining hardware. The upper weld was analyzed by a vendor with a different model.

Overall, they have the vessel, the long shell here. The boundary conditions here. And they are responsible for the analysis of this joint.

(Slide)

This sketch gives you a better idea of the kind of detail included in the local area. These two section lines go right through the area of the weld and the vent hole. Some of the predominant loading conditions on the structures are the thermal transients. One of the worst is the uncontrolled rod withdrawal from full power. The temperature starting at 750, increasing slightly, and then dropping as we scram.

The max thermal stress for this event occurs at about

1,000 seconds, at which point in time the pressure has dropped, was initially at this level, had dropped considerably and then remained down for the duration of the transient, and the pressure is lower --

MR. SHEWMON: This is a scram; is that right?

MR. NICKODEMUS: This is a scram right here.

MR. SHEWMON: Why do you call it rod withdrawal? These aren't fuel control rods, are they?

MR. NICKODEMUS: This is a rod withdrawal for the early part.

MR. SHEWMON: I see. And that precipitates a scram?

MR. DICKSON: Paul Dickson, Westinghouse. That assumes that the reactor has run up in power to just under the 115 percent EPS trip point. There is the controls that are assumed to have failed in -- we assume that the reactor sits there just under the trip point of 115 percent for 5 minutes. Then the reactor operator notices it and hits the scram button and that defines our most serious event.

MR. BUSH: Why did you establish the spectrum of normal and upset loads that you would use in the design? In a light-water reactor you go through a process and you modify the number of heat ops and shutdowns, et cetera. Here you have less of an information base. I presume that you built on the experience of others. Is that how you did this one, or are these arbitrary designs?

MR. DICKSON: They are arbitrary to the extent that they have been expanded in number and intensity.

MR. SHEWMON: The pressure on there is the pressure difference across the membrane, or is it consistent pressure?

MR. NICKODEMUS: I just spotted that when I put it up. It looks low. The pressure is about 170-130.

MR. SHEWMON: And you also talk about psi.

MR. NICKODEMUS: So this would have been to the difference on that.

MR. SHEWMON: Everybody but me knows the pressure is in what?

MR. NICKODEMUS: Pounds per square inch.

MR. SHEWMON: All right.

MR. ZUDANS: This is the bulk sodium temperature?

MR. NICKODEMUS: This is the inlet plenum.

MR. ZUDANS: Did you have some preliminary inspection on skirt support on both sides?

MR. NICKODEMUS: I don't have them with me. This transient would be used for the thermal analysis of the structure, would be applied with the film coefficient to the bottom side of the cone. Somewhat reduced transient because the flow has to go through the core support structure to get through the top side of the cone, would be applied at the top side of the cone, and then a thermal temperature analysis would be performed. That would then be reviewed and the times of critical temperature distribution evaluated for stress.

MR. ZUDANS: You mentioned that the worst time point is 100 seconds?

MR. NICKODEMUS: A thousand seconds. Over here.

MR. ZUDANS: There is another critical point where your skin effects show up. Where is that point? Or is that point considered in the analysis?

MR. NICKODEMUS: Yes, it is. This is just one of the events. This event would normally be looked at at this point here, which would be coming up to one of the higher temperature cases and also at a low point, and then it would be reviewed at various points in between to make sure there are no other locations that are more severe. This would be done for all the transients.

MR. ZUDANS: This is the fastest temperature drop that exists in the anticipated transient?

MR. NICKODEMUS: In the inlet plenum, I would believe it is. It's relatively benign thermal conditions compared to the outlet plenum --

MR. ZUDANS: Okay.

MR. NICKODEMUS: Structural analyses were performed to the ASME code.

(Slide)

Elevated temperature code case 1592. Even though the cone itself only exceeds 80 degrees for a period of approximately 10 hours during the entire 30-year lifetime and this temperature even in those cases remains below about 850. So we are using the elevated temperature code case criteria in the fairly severe case. The structure does not really require. Results of the analysis indicate significant margins of safety. The primary membrane stress has a stress of 6,000 and allowable of almost 15,000. So it is 137 percent margin. Membrane plus bending, it is a margin of 1.27.

The secondary membrane plus bending including -- has a margin of 0.19 and a fatigue damage has a margin or a fatigue damage summation of 0.06 with an allowable of 0.9.

Folded events do not push the cone towards its limit. The primary membrane has a margin of 2. Membrane plus bending has a margin of 1.3.

This is at Section A-A or the lower weld structure.

MR. ZUDANS: I guess you didn't have any calculable creep damage?

MR. NICKODEMUS: Only of about 10 hours above 800 degrees.

MR. ZUDANS: And this limit is essentially reached in your case?

MR. NICKODEMUS: This one?

MR. ZUDANS: Yes. That is which transient does it correspond to? Which of your loading --

MR. NICKODEMUS: I am not sure I can answer that. It may well be a combination of this transient for the down cycle and another transient for the up cycle.

MR. ZUDANS: I see. So if you did perform a random combination of different ones and not just specific transients?

MR. NICKODEMUS: That's right.

MR. ZUDANS: So that's what's required?

MR. NICKODEMUS: Yes.

MR. ZUDANS: Did you look at the same time also at the stress levels in the vessel welds that are located at essentially the same --

MR. NICKODEMUS: Yes, we did. Abve the cone attachment.

MR. ZUDANS: Yes. And below it?

MR. NICKODEMUS: Yes. I don't know the results offhand, but they were evaluated and did meet code requirements.

MR. ZUDANS: In that case, a case of a section at the core support, very close to this attachment.

MR. NICKODEMUS: Yes.

MR. ZUDANS: That did not show any higher stresses?

MR. NICKODEMUS: No more critical in this area. The thickness also increased in there with a taper.

MR. ZUDANS: And this was linear elastic analysis?

MR. NICKODEMUS: Yes.

MR. ZUDANS: Did you vary the temperature properties with the temperature?

MR. NICKODEMUS: Yes. Material properties would be inputted as it is temperature-dependent.

MR. ZUDANS: I am wondering, this computation of linear analysis covers the range of temperatures of 200 degrees radiation?

MR. NICKODEMUS: Yes.

MR. ZUDANS: The way I remember the answer -- maybe it has been changed -- you could not assume some average property and perform the circle step calculation? Which was was it done? Or you are not sure?

MR. NICKODEMUS: You can input a temperature, a temperature-dependent property. It would be input in equation form or in tabular form that would be used in the analysis.

MR. ZUDANS: But you would hav to perform an analysis step by step for 500 seconds, and I am wondering whether that was done or a single-step calculation was done.

MR. NICKODEMUS: I am not sure I understand what you are asking. Your transient would be reviewed to determine the peak surface to remain temperature difference, the difference between average temperature of one area and another, and would be evaluated at those points in time. The temperature dependence of the material properties in a 200-degree swing is not very high. I think what you are asking me is did we evaluate the stresses at many times to get the effect of the temperature dependence?

MR. ZUDANS: Yes. That is the question. Did you or did you not?

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MR. NICKODEMUS: No. It would not have been evaluated at more than what was judged to be the critical points in time.

MR. ZUDANS: Okay

MR. GRIFFIN: Griffin, Westinghouse. We are below 800 degrees Fahrehneit, so temperature really doesn't have a very significant effect. Is that what you mean?

MR. ZUDANS: Well, in stainless steel, steel properties change from 600 to 800.

MR. GRIFFIN: Well --

MR. ZUDANS: You see where I am coming from? You can take a set of properties and perform single-step analyses or you could proceed in a time history and change properties with each step corresponding to the temperatures that existed. That is what you would have to do with it.

MR. GRIFFIN: This is elastic. I can't say for sure, but I can almost guarantee it wasn't done.

MR. ZUDANS: Sure.

MR. GRIFFIN: Still basically a linear elastic analysis.

(Slide)

MR. NICKODEMUS: The results of that analysis at the upper weld are similar. The normal and upset primary membrane stress intensity margin is 1.3. Membrane plus bending is 0.39. The range of secondary stress intensity is considerably reduced and the margin of the -- in this case is 0.81. The fatigue damage is 0.02 with an allowable of 0.9. The fatigue damage including the local stress concentration effects at the vent holes is approximately 0.03 with an allowable of 0.9.

MR. SHEWMON: Sir, why don't we state that we will take your word that it is designed very conservatively and according to the best engineering practice, and what we are going to spend a fair amount of time discussing today is, gee whiz, what if? for unlikley things.

MR. NICKODEMUS: Okay.

MR. SHEWMON: And the reason this came up in the first place was you hadn't designed it right or made it out of tough material. It was, gee whiz, if that weld started to have a flaw in it or if it should actually fail, it would be very awkward because you don't get your control rods in and out. It might become uncontrollable.

MR. NICKODEMUS: Yes.

MR. SHEWMON: So if we talked about what if a crack started to form and grow around there, don't ask us where it came from. It's just an essential weld, absolutely essential in its integrity. What sort of things would happen or what defense do we have there?

MR. NICKODEMUS: I have about two vuegraphs on crack propagation studies. Would you like to cover those or turn it over to Paul?

MR. SHEWMON: I think I want to get off of this analysis.

MR. NICKODEMUS: Fine.

(Laughter)

MR. SHEWMON: I trust you would have done it well.

(Slide)

Paul just happens to have a few slides along, I see.

(Laughter)

MR. DICKSON: Just happens to have a few along. I had the feeling someone was going to ask that and had the feeling his name might be Dr. Shewmon. So what was magic about that? Well, we will look at that.

(Slide)

To orient you, when the reactor is at full power, then that force on this cone here is upward. The upwward force overrides the weigh. That stays true on the support cone at all operating conditions, even down below the 40 percent flow case. If you took a break right here at the outer break, Section BB, then this net support force would be upward from 40 percent to 100 percent. If you imagine the break at the inner weld, Section AA in the analysis, then the net upward force would not be greater than the downward force until you reached about 50 percent flow. So for the most part, the core support cone supported during all operating pressures, all operating flows. Only for a very small range of the operating flow regime would you be below -- have a net downward force with a break at that point.

Now, the core is shown right here, 3-foot high. When you are shut down, the secondary control rods are all the way in and the latch that is broken is about 3 foot above this core. So it is still well down inside the ducts. With the secondary rods retracted, then of course the bottom part is at the top and the latch is about 6 feet farther up, still within the boundaries of the ASME. The primary controls would also be in, but their latch is much further up, except during refueling. They all have their latch broken at the top of the core.

(Slide)

So we are going to imagine a worst case. This thing has broken right here and fallen into the lower plenum. The reason this was shown tipped off at the side a little bit was to show at the worst it can't lay over flat, it will still be more or less upright and sitting there in a bath of sodium at whatever temperature. It will allow natural circulation through the core.

MR. BUSH: And the rods are where in that case?

MR. DICKSON: The secondary rods are still right where they were during the shutdown. The primary rods are dangling from here, and in this case where it tipped over, they would be absolutely into these assemblies. But you don't have enough strength really to force anything.

MR. CARBON: Excuse me. Is that a tilting -- if a tilting took place in operation, that the rods would still go in?

MR. DICKSON: Yes, sir. Operation -- I am sorry, let me go back then. If this falls in its 40 percent range you are talking about?

MR. CARBON: Full operation.

end3B MR. DICKSON: We have looked at that.

MR. DICKSON: It won't fall. Force would keep it up.

MR. CARBON: Would keep it from tipping as long as --

MR. DICKSON: If it were to break completely in that manner, it could go up. It could go up two inches.

MR. CARBON: It couldn't tip sideways with the force below it? I would appreciate force holding it up.

MR. DICKSON: But it can move only two inches in the top of the assembly without encountering the instrumentation.

MR. CARBON: What I'm saying, could it start moving up and start canting to the side such that even with a force under it, it could still tilt?

MR. DICKSON: It could tilt by as much as two inches over that span of about 15 feet. It is a very small tilt.

MR. CARBON: What would that do to the operation of the rods?

MR. DICKSON: The primary rods would probably have difficulty. The intermediate rods would not because they would simply tilt with it. Even the primary rods have a universal joint to enable them to withstand a certain amount of tilting. But whether it is that much or not, I don't know. They do have a universal joint that will account for some warping of the -- can you hear me without the microphone? The upper tunnel structure is keyed into the support structure, and that will retain the upward motions and also maintains the upper internal structure, and thereby aligns the rods.

MR. CARBON: Thank you.

MR. DICKSON: Those are located right up at the edges here with the upper internal structure. There are three keys at three points.

MR. CARBON: The right-hand sketch there shows lots of freedom for free convection cooling. Is that sketch to scale, so to speak?

MR. DICKSON: Yes, sir.

MR. CARBON: The one on the right?

MR. DICKSON: Yes, sir. Those are 33 inches from that point to the bottom of it.

MR. ZUDANS: On that assumption, would you not get a flow distribution such that you might lose a fraction of that uplift, and therefore the power levels at which it will stay up might increase?

MR. DICKSON: Yes, sir. I'm going to go to that.

MR. ZUDANS: Thank you.

MR. DICKSON: We will take the postulated failure occurs right after shutdown, and the reactor remains subcritical. The reactor remains subcritical due to secondary rises in the flow distribution. It remains subcritical at its operating temperature, and with a 730 degree inward flow.

Now, as the temperature of the inward flow reduces, it removes the upper feedback. The reactor tends toward critical. So upon falling, the reactor is subcritical, but it will begin to approach criticality again during some certain parts of its cycle.

For almost all of the operation, it will remain subcritical, even cooled down to 600 degrees, but for the first few days of the cycle 3 and 4, the reactor would go to criticality, and as the decay power reduces to 4 percent, that decay power will reduce to 4 percent in about 100 seconds, and that is at 730 degrees F.

Now, the system tries to bring inlet temperature on down to 600 degrees, and decay power continues to fall, of course. The net result is that nuclear power has to increase to maintain the temperature to keep the upper feedback balanced so that the reactor achieves a steady state, so that the reactor power will increase, and as its system temperature gets to 600, the reactor is operating at 90 megawatts at the very beginning of cycle 3.

What we are seeing is a burnout of the fuel, and I might note all of these are based on nominal calculations, and because this is beyond the design basis case.

The cycle 3 and 4, after 44 power days -- in fact, it either works out to 37 days or 39-172 days. You will not reachieve criticality even if you cool down to 600 degrees. For those same three cyles, you can cool all the way down to 400 degrees. Should the reactor operator decide he wanted to bring it down to the 400 degrees, he has to take action to do that. The 600 degrees is an automatic system to which the operator takes action to bring it down to 400 degrees. He would achieve criticality again between 40 days and 75 days, but after 75 days, it will sit on the bottom of the reactor vessel with six secondary rods inserted and not achieve criticality even at 400 degrees, and at cycles 1 and 2, it would never reachieve criticality with the six secondary rods.

MR. ZUDANS: Paul, if you assume before these conditions you next shut down the reactor --

MR. DICKSON: Yes.

MR. ZUDANS: That means the secondary rods were in and engaged?

MR. DICKSON: Yes, sir.

MR. ZUDANS: And therefore if it falls they would remain --

MR. DICKSON: Yes.

MR. ZUDANS: Would it matter if it fell a little bit further down? Because you show in your generic specs that it was sitting on the rods.

MR. DICKSON: No credit whatsoever.

MR. ZUDANS: Then you wouldn't have access as you have now?

MR. DICKSON: Yes, sir. If the thing were sitting straight up, as you could see, this would be a little lower down. This would be higher up, but basically 33 inches at the shortest point on each side, and about another foot because of the curvature around here would leave you plenty of room for flow.

MR. SHEWMON: What are the principals?

MR. DICKSON: These are.

MR. ZUDANS: And there are lots of them? They are all across the --

MR. SHEWMON: And they are pretty firm or husky stuff?

MR. DICKSON: We have not stressed it out. We knew you would bring it up, so we brought a picture of what it would be like. I think they are roughly an inch thick.

Do you know, Bill?

MR. PANNELL: Yes.

MR. DICKSON: Bill Pannell says yes, that it is an inch thick.

33 inches long, and as you can see, they are rather substantial members. It's almost impossible to visualize

this falling through eight feet of sodium and having any crushing effect on these whatsoever.

MR. ZUDANS: On the head, for that matter.

MR. DICKSON: Yes.

4-B

MR. SHEWMON: While you are there, can you tell us what the structures are that are around some of them?

MR. DICKSON: Those are frost-fixed, so that this one is being inserted. You can see this being pushed up, and these have been supercooled so that they can be slipped in and then expanded, and these were put in a while ago, and these are -- well, you can see this one is up to here. This is just frost. Basically, for the worst time in life cases, if the operator doesn't intervene, the inlet temperature will stabilize at about 600 degrees, the reactor power is about 90 megawatts, reactor outlet bulk temperature is about 1200 degrees Fahrenheit, and reactor peak outlet temperature is 1350, and the hotleg temperature is about 890.

Most of the flow that is being driven by the pony motor will bypass because it has a simple loop and the [illegible] driving its overflow through it to heat up the hotlegs, so it does that to the hotleg temperature.

This is a little higher than normal operating conditions, and you don't expect much fuel life, but no problem. You know, we are nowhere near melting and, in fact, if you got fuel filters, that would allow some of the fuel to escape. That's probably a plus.

MR. CARBON: So this is strictly cooling by natural circulation?

MR. DICKSON: Strictly natural circulation cooling.

MR. LIPINSKI: Now, this is after the shutdown?

MR. DICKSON: Yes, sir, right after the shutdown.

MR. LIPINSKI: Is the structure still buoyant at 40 percent?

MR. DICKSON: The structure is buoyant. If you failed out at the outer weld, it is not buoyant at the inner weld.

MR. LIPINSKI: What happens if you get a 40 percent --

MR. DICKSON: We knew you would ask that, so we analyzed that, too. You get a 30 second spike. The system begins to fall. The primary rods are coming out, and in .03 seconds there is enough reactivity put in in about a 10 second spike insertion with a fall of 2/10ths of an inch. You get a trip signal. Then both the secondaries and the primaries begin to come in, and since they are driven, the primaries and the secondaries with a hydraulic lift, they will overrun the falling fuel assemblies for the first few inches, put in enough reactivity that they turn that spike around in less than a half second.

You have a small 30 second spike, the primaries come in, the secondaries come in, and then as it continues to fall, the primaries come back out, but the secondaries are fully inserted.

MR. LIPINSKI: What happens if that scenario slows down and the barrel moves down slowly, not abruptly, and you are under power control with your automatic system?

MR. DICKSON: If it moves down, it has to move exceedingly slowly so that the power control would try to follow it. As I mentioned it, a 2/10ths of an inch downward will trigger the scram system.

In fact, I think a 10th of an inch will.

MR. LIPINSKI: Not if you are under control.

MR. DICKSON: Yes, sir, because that is more of a step. See, the control rods only want the step .025 inches at a time, and only one at a time. So you have just taken all four steps at once.

MR. LIPINSKI: If I take this barrel and I make that thing creep away, not abruptly, but with a slow change that you are not aware of, and you are under automatic control, there is going to be some limiting rate. This thing will start moving down very slowly, and how does your control system have enough intelligence that this is happening as opposed to --

MR. DICKSON: If you conjecture that it is moving down very slowly, and I don't know how it does that in that situation, but if I do imagine that, it wants to slide very, very slowly, obviously the control rods would continue to follow it in, and I suspect the reactor operator would be suspicious when he saw the lights come on telling him that his primary control rods were all bottomed, and the reactor was making 40 percent power. At that point I suspect he would scram, and if he didn't, and it moved farther, it would then scram at that point, because the primary control rods can't follow this beyond the bottom of their travel, and so it would scram at that point.

MR. ZUDANS: One more question. There is a positive upward force, say to 40 to 100 percent operation. Is there any structure that would prevent the core from moving up during that time?

MR. DICKSON: There are two structures, as Bill mentioned. There is the key loads that prevent it from tipping, but one of the design requirements of the instrument post that sit not more than two inches above the core is that they be able to withstand the upward force, not picturing a failure of a core support cone, the loss of hydraulic holddown that will push the fuel assemblies up, and they are designed to hold that down through the hydraulic force.

Our bottom line is that only for a small fraction of CRBRP operating life would even a recriticality occur, and I might mention in this regard that I said cycle 3 and 4. If you take the present nuclear analysis, they say that the cycle or cycles repeat, but as soon as we get operating data out of cycle 1, a large amount of our uncertainty that we put into our loading would go away, and we could then specify the fuel loading and not have quite this much access again.

For cycles 3 and 4, you won't have time. They will already be being built. The fuel for cycles 3 and 4 will already be fabricated before you have operative data from cycles 1 and 2, but certainly for cycles 5 and 6, you can.

So it's only for a small fraction of life, and no power excursion would occur, as I mentioned, other than a 30 second spike in a very unusual case.

For the most part, recriticality is achieved gradually, and as you know, reactors that operate on thermal control are exceedingly stable systems.

4-C

Now what if we are wrong, so that the core cooling is significantly less than predicted. Dave isn't here. He would have said what caused this is a seismic event.

(Laughter.)

A partial meltdown of the core could result, and that would take the reactor to subcritical and not be a problem even with the long term decay heat cooling, and at the worst of all possible scenarios, if you lost all core cooling, you can get a total core meltdown, but the total core meltdown would take longer to evolve than what has presented earlier, so that in all likelihood, this core support cone wouldn't fail.

If it does fail, for most of the operating life, nothing will happen. For a very small fraction of the operating life, you do get some additional power, and in the worst possible scenarios, we are no worse off than our normal TMBDB scenario that was presented earlier which we have designs to accommodate.

MR. LIPINSKI: Instead of having put the structure in tension, why isn't it supported from compression in numbers from the bottom?

MR. DICKSON: It is supported in compression for most of its life.

MR. LIPINSKI: That cone is in tension?

MR. DICKSON: No, sir, the cone is in compression.

MR. LIPINSKI: Okay. When you are at flow. But when you are not at flow.

MR. DICKSON: We are designing this reactor to be at power most of its life.

MR. LIPINSKI: Let's assume you are not 40 percent; you are 100 percent.

MR. DICKSON: Even at 40 percent, it's in compression. Yes, Bill.

MR. PENNELL: For the selection of that configuration so that there's continuity with the shell was satisfactorily removed, the discontinuity stresses associated with the inlet -- if you turn it the other way around --

MR. DICKSON: That was another reason, but didn't you also want to keep it in compression during the bulk of its life?

MR. LIPSINKI: Well, you could have boots and suspenders.

MR. PENNELL: We don't want redundant structures in there. The environment characterized -- the last thing you want to do is introduce redundant --

MR. DICKSON: He would also appreciate the fact that that sodium that is up in that area tends to change temperature much more rapidly than if the cone was inverted because obviously, the flow isn't right along. It tends to be a dead area.

MR. LIPINSKI: But you don't have to have ridges? You have slipping columns that would take care of thermal expansion. The two points would not be fighting each other, but you could give yourself some margin of drop, having dropped that amount. Then you enounter --

MR. DICKSON: We are not going to claim that the design we have couldn't have been redesigned in another manner and even exceed to perhaps a better design. What we will claim is our design is quite adequate with significant margin.

MR. BUSH: With that geometry, if you want to control your continuity stress, --

MR. PENNELL: Well, basically, it is a geometry consideration. We separated them by three wavelengths so we don't get super position effects. We did consider the issue of crack propagation, and one point may be relevant here that wasn't brought up earlier. Nothing that we have seen in actual practice has ever given an instantaneous 360 degree circumferential failure.

Paul mentioned we had substantial loads acting either up or down. Now if it occurs, this post- late failure during normal operation, it would tilt upwards, and that opens a low impedance flow path from the inlet to the outlet, and of course, it shows up instantaneously. That you have in the upper internal structure so you do have multiple signals that something has happened in that core if it ever proceeds to the short condition that Paul showed, but if it ever proceeds to any significant circumstance -- .

MR. DICKSON: If there are no further questions, let's move on.

MR. KING: My name is Tom King. I'm with the Clinch River Program office. I'm going to give a very brief summary of our review of the proposed no loose parts monitoring system. Initially, in the review the applicant didn't propose notice for the monitoring system for the CRBR. We couldn't find in our review any compelling reason for not having one. We didn't see any significant differences between the LWR and the CRBR in terms of ways to generate loose parts, so we considered that such a system should be applied to Clinch River and the applicant is now committed to design and install a loose parts monitoring system for the requirements of Reg Guide 1.133. The major design criteria are included in Reg Guide 1.33.

The applicant is committed to install sensor locations on CRBR in the reactor vessel area, the primary and intermediate heat transfer system pumps, the IHXs, the generators, the natural collection points of the system. These are also committed to do component noise and vibration measurements to look for degradation.

A lot of the LWR experience with the loose parts monitoring system I think will apply to Clinch River. There are some differences. You just can't take an LWR off of a shelf and stick it on Clinch River. Clinch River has a higher temperature which will affect the lifetime of the accelerometers. The guard vessels along the main components complicate installation and calibration of sensors. There are some differences that have to be accounted for. In the LMFBR program, there is an experience data base on loose parts monitoring for LMFBRs and PBR-2s not tested at high temperature in sodium microphones, and those microphones have been installed in a loose parts monitoring system in the FFTF reactor vessel.

MR. MARK: Excuse me. A loose parts monitor picks up the vibrations from something bumping on something else, a piece of metal and banging against the side?

MR. KING: Impact, right.

MR. MARK: How bit a piece? How far away is it, compared to -- is it the size of a marble, or does it have to be as big as a football, or what?

MR. KING: For an LWR, it's in the neighborhood of several pounds.

MR. MARK: Several pounds of steel, a foot or two away?

MR. KING: More than a foot or two away. There has to be enough sensors so that you can detect that impacting anywhere within the vessel and the system that you are looking at.

MR. MARK: It won't pick up a thimble?

MR. KING: No, I don't think it would pick up a thimble. I think those sizes for the LWR were determined based upon looking at what size of a loose part would cause damage to the internal structure, and that same kind of consideration is going to have to be done at Clinch River. We haven't arrived at what is the minimum size particle we want to pick up.

MR. MARK: This loose particle of several pounds won't go through many of the inlet holes through which coolant is supposed to flow?

MR. KING: No.

MR. MARK: But it indicates that something somewhere is broken?

MR. KING: Yes. The concern for Clinch River is not flow blockage; it is concerned with banging and causing further degradation of something else that is in there.

MR. MARK: Well, you've already got some degradation that you would like to know about.

MR. SHEWMON: Is the attenuation much less in sodium microphones and stainless steel as opposed to just finding them on the outside of the vessel?

MR. KING: There are two things, I believe. One is the attenuation of sound in stainless steel is more damping than there is carbon steel. Too, I think for an LMFBR with the guard vessel around the reactor vessel and the pump and the IHX, we can start locating sensors on the outside. Those components found in that region, if they fail, you can't replace them, and you want to calibrate them.

MR. CARBON: This indicates that you have done this EBR-2? What sort of magnitude particle size can they pick up there?

MR. KING: I'm not sure that has been determined yet. Primarily, it was put in EBR to look for a system background noise and to see that if you had so much background noise, that you couldn't use these at all, and to look at lifetime calibrations.

MR. SHEWMON: Something came loose in the IHX five or ten years ago and that was used one time. There were no vibrations, so it may not be just loose parts. It may be some other incipient failure.

MR. KING: What they did do in EBR was to tap an instrument probe that came down into the pool, and you could hear that very plainly. They weren't up there with a sledgehammer, but making a reasonable tap and you could hear it. I have heard it, and I've listened to the FFTF system and you can hear it fairly clearly.

MR. MARK: The background noise is higher or lower than the LWR?

MR. KING: I haven't listened to an LWR so I really can't say. I've listened to two LMFBRs and it was pretty low.

MR. MARK: Wouldn't it be -- given a conclusion that the background noise was down, you could see more things easily at a given level?

MR. KING: Yes.

MR. MARK: And I suspect that this background noise is down compared to an LWR in normal behavior, but I don't know.

MR. KING: Certainly from a PWR you would expect the background noise to be down. Your statement is true, the lower the background noise, the more sensitive your system would be to pick up.

MR. ZUDANS: Are there any plans for CRBR to observe the gross characteristics of the system and the components not, say, vibration moves, but if you would have a signature in the beginning, of how the system responds and what is its natural flexibility and observe the shift in it? Would you conclude there is some major structure modification, or a failure has occurred? Are there any such in this particular plant?

MR. KING: As part of the overall diagnostic noise in noise vibration areas, there are periodic vibration monitoring in plants. I'm not sure of the details or whether that is permanent or temporary. Maybe it is installed. You can ask the Applicants that question. As part of developing this program, that includes loose parts monitoring.

To sum up our conclusion, it is a commitment by Applicants to design, install and operate a loose parts monitoring system for CRBR in accordance with Reg Guide 1.33 is acceptable for CP.

MR. O'BRYANT: I am Wendell O'Bryant, manager of maintenance and test for Westinghouse.

(Slide.)

As an introduction, we are committed to the loose parts monitoring system consistent with the LWR technology, modified as necessary, as Mr. King explained, for the CRBRP plant environment.

General design criteria was mutually agreed between NRC and the Applicant in a meeting on November of last year.

I am prepared to answer some of the questions that were asked of Mr. King, a couple of them that -- our general design criteria that we came up with, I will discuss them briefly, although I think a lot of the questions have been answered by Mr. King.

Sensitivity. We feel that we can definitely detect a half a footpound impact, and probably a lot more sensitive than that and, yes, the liquid sodium should have less background noise than a water plant, due to the low noise that we have.

We are normally talking about noise of less than 25 foot per second equal to or less, and in the inlet plenum for the vessel, for instance, and some of the natural collection points, are much lower than that, as low as four and five feet per second.

MR. SHEWMON: We jumped to you last. I noticed you are not talking about going to sodium, but you are going out of sodium. Is this because you have a pipe plant and therefore you can get at the important vessels?

MR. O'BRYANT: Talking about the sensor locations, and let me briefly show you where we will put the sensors.

MR. ZUDANS: The input was not on your list.

MR. O'BRYANT: In the reactor vessel we are now determining the best place to put those sensors, whether it's on the head to detect noises up in the upper region, or down at the top of the core, and we have access to those, and we are right now in the process of determining where the best location would be, but we have the ability to put detectors up in the upper region.

Now, as he mentioned earlier, in cases where you have the guard vessels, you've got to place the detectors out of the guard vessel in order to be able to get to them to maintain them, and replace them, but we have run some preliminary tests that indicate the attenuation is very small with the length of pipes that we are dealing with. So basically we'd be monitoring the inlet portion of the vessel by the detector placed upon the piping outside of the guard vessel. We intended to have the detector on the primary pump again outside of the guard vessels, but you monitor the noises and the IHX again on the down --

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fols.

MR. ZUDANS: I would like to repeat the question that I asked the Staff here. Here I see your monitors that are identified by the asterisks or stars.

MR. O'BRYANT: Yes.

MR. ZUDANS: If you would want to monitor the deterioration of the system rather than just a loose part, the gross behavior of the system, you would have to put such devices on -- well, essentially it moves the rigid body and if you would analyze the significance, it would contain its natural frequencies of motion.

Now, if you store that when you did the per-service inspection and periodically examined the response of that system and noted the displacement of your natural frequencies, it's a gross monitoring. It's a direct indication that something went wrong in a system. Either the support broke or a piece of support broke or something happened, and I am kind of surprised that you don't have any such monitoring indicated. Certainly, that is not a loose part on it; that is monitoring of the integrity of the gross system.

MR. O'BRYANT: We go through a program earlier in the plant life. You are talking about flow-induced vibration and so on. The pumps have permanent monitors on them so you can monitor it earlier; early in the life you have accelerometers in the core that you verify the lack of vibration early in the core in the startup process. And the same thing through all of the process.

In the startup program we monitor at all the flow levels in the flow-induced vibration of those components.

MR. ZUDANS: I understand. But the purpose is completely different. I saw the Germans made a presentation in one of the conferences and later the French made their system where they used such motion monitoring system. You don't care about noise, the component of it. You more or less care about gross motion of the component. And if the system is healthy, it just stays. You know, it keeps where the natural frequencies show up on the analyzed record. They remain fixed. As soon as something different bgins, those begin to move around and you are then able to identify that there is something wrong and go in and fix it.

I think this is the ideal place to use such a system. The other thing is that the way I understand the previous presentation by Paul is that the only way to observe the motion if it begins to creep one way or another where you could have similar results from this type of reading or large displacement reading, that would range the characteristics of it, and even if the break was inside the reactor, it would respond to a different frequency.

MR. O'BRYANT: Mr. Ziegler?

MR. ZIEGLER: The present system as it is conceived will be capable of doing what you are saying. In fact, the present LWR systems are capable -- mind you, you are not measuring the actual displacement of the surface, you are actually monitoring the frequency of those structures as they are propagated through the acoustical path. They have seen variations before, and we don't expect to have any difference whatsoever. and not seeing the variation in the Clinch River reactor. You will have these peaks, as you say.

MR. ZUDANS: You plan to use this system for that purpose?

MR. O'BRYANT: Yes, I believe that is right. We will take a noise signature on the thing, and we will definitely be able to identify any changes to that and therefore any degradation or whatever you -- changes in the background noises.

MR. ZUDANS: It's not the noise component that I am concerned with, it's the gross behavior that is a better indicator for structural state, but I understand from you that you do have the capability, whether or not it must -- it was specifically planned to be used for that purpose -- that is another question.

MR. DICKSON: Dickson of Westinghouse. Our plans on monitoring this reactor aren't fully worked out, so we don't want to say it's not planned. We will take full advantage of any information we have to diagnose the behavior of this reactor. If that information is available and intelligible, we certainly will use it to ensure the safety of the plants.

MR. ZUDANS: Thank you.

MR. LIPINSKI: You have got to be careful with respect to the first response. It's a question of where the sensor is located. If I tie it to the reactor vessel, I don't know anything about pump behavior, so where the sensor is placed is very important. If that sensor is too far from the pump, you don't get information.

MR. O'BRYANT: On the case of the pump, I believe we have other sensors that are not part of the system that are there for that purpose.

MR. LIPINSKI: That is probably important to answer Dr. Zudans' question.

MR. CARBON: Mr. O'Bryant, your handouts here are quite clear. Why don't you jump to your conclusions and let us simply ask questions.

MR. O'BRYANT: Very good.

MR. CARBON: Okay.

MR. O'BRYANT: Conclusions that we reached in performing the studies.

(Slide)

We performed these. We could find no potential loose parts that could degrade the ability of the CRBRP program. We took credit for redundancy, but we couldn't find a real safety reason for doing it, but concluded that it would be a very valuable diagnostic tool for the plant. And as such, we are going to install it and use it to the extent of its capabilities.

MR.LIPINSKI: Using the parts that have been left in the --

MR. O'BRYANT: We considered that. Parts can be left in or break loose from various components, but generally speaking, due to our low flows, if they are carried at all, transported at all, they are transported into the natural collection points in the bottom of the IXY in the bottom of the reactor, the reactor vessel, and will remain there and in the reactor vesssel itself, the lower inlet nozzles will filter anything greater than a quarter of an inch, preventing it from moving up into the core, and so the low flow runs down there.

If we had parts left in or came loose from a component, it would naturally collect there and would not be moved beyond that point.

MR. CARBON: Any other questions?

(No response.)

MR. CARBON: Fine. Thank you, Mr. O'Bryant.

Let's take a 10-minute break.

(Brief recess.)

MR. CARBON: Let us continue with the meeting.

MR. STARK: This is Richard Stark from the Staff. I would like to get a brief project manager's review of what -- I would like to pose and answer two questions for you -- what is the SMBDB; and the second is why did the Staff require it? The answer to the first question is: structure margin beyond the design basis is basically the assessmsent of the ability of the containment to tolerate a postulated core-destructive accident. And the second question, why did the Staff require the applicant and the Staff to evaluate this particular accident -- and I am going to read directly from a May '68 letter sent from the Staff to the applicant. It said, "We will therefore not consider CDAs as design-basis accidents. Nevertheless, because of the differences in the state of technology and experience between LMFBRs and LWRs, the consequent inability to evaluate the safety of the CRBR design as precisely as can be done with LWRs, and the absence of a quantitative risk assessment based on experience and data such as the reactor safety study for LWRs, prudence dictates that additional measures be taken to limit consequences and reduce residual risks from potential CRBR accidents having a lower probability than design-basis accidents to ensure that the public health and safety is adequately protected. The basic approach should be to protect the cotainment system from the unique effects of CRBR core-disruptive accidents."

Tha is the end of the quote. So what you are going to hear now is -- you will hear what the Staff is going to and has done to satisfy the requirement of the Staff of 1976, and I will turn it over to Howard Holtz.

MR. HOLTZ: I might elaborate on that. As far as I know, there are two sets of people that are going to discuss the subject today, and Westinghouse, since they will undoubtedly present a lot more graphic material regarding the reactor, I thought I would limit myself to this part and Mr. Tom Butler, who is a consultant to us, has done a great deal of work related to the scale module testing and the structural criteria that we use, and he will speak to that after these first vuegraphs. And I am coming back and will tell you our summary and concluusions.

The purpose of the SMBDB -- and it's the margin beyond the design basis -- and its purpose is to assure that during a CDA and immediately following the reactor closure head and head-mounted components will not challenge containment of the operating floor. These challenges to containment from SMBDB are missile generation from control rods or any other penetrations that go into the core, and secondly, above the operating floor if there is leakage from components or from heads itself, that this spray be confined and contained within the head access area so that it doesn't have an opportunity to spray directly in the containment. On that basis --

MR. CARBON: I presume not only sodium but CO2 and hydrogen?

MR. HOLTZ: That is the TDB scenario. We are working with the reactor and the core. There is a core-disruptive event which sends a sodium slug to impact the head of the reactor or closure head, which consists of three rotating plugs. I am sure you see a lot of pictures on that from Westinghouse.

Our evaluation, the applicant and the Staff had a number of working meetings and several hundred questions which are a matter of record. And these documents, some of them, are historic, such as this one, and the most important new element that didn't exist in '76-'77 was the scale model tests. And a lot of new information has been extracted from those, and I will return a little bit later to give a conclusionary vuegraph.

But these two letters of December 9th and February 14th from the applicant gives us a real basis to make the statements that we do. I would like now at this time if there are no questions, to introduce Mr. Butler.

MR. ZUDANS: I just wanted to make sure that you are limiting this structural design basis to the missile generated by potential --

MR. HOLTZ: Not component.

MR. ZUDANS: And that is the limit of it?

MR. HOLTZ: No, it is the head itself.

MR. ZUDANS: Anything that comes from the initial --

MR. HOLTZ: Right. So it's the reactor closure head. Let me see if I got a picture here.

MR. ZUDANS: That is all right. I understand. I don't need a picture.

MR. HOLTZ: You might need a picture. That happens to be the intermediate rotating plug. The top section of it happens to be about 2 feet thick, and all of this stuff down in here is the radiation -- thermal radiation reflectors, neutron shields, et cetera, and gas entrailment things, so this entire area is what we are going to be talking about in this subject.

MR. ZUDANS: And it's all as a consequence of CDA? It's initiated by a core-disruptive accident, isn't it? Isn't that a question related by margins? I guess it would not be beyond design basis but within the design basis. In all of these components, they have very careful design --

MR. HOLTZ: Yes.

MR. ZUDANS: And unlike an LWR design, in this design you are to use the design capability by performing more precise analyses and doing -- and that leaves the question open as to once you have satisfied all the requirements, what is the design-basis margin still available? Have you had anything in that nature?

MR. HOLTZ: Yes. Actually, we both follow the ASME code, Appendix F, Level D, and there are some modifications that we allow for this event that would push materials and structures a little harder than they would force those events that are in the design basis. Mr. Butler can speak a little more on this subject if you -- I am sure Westinghouse can too.

MR. MARK: So you in a word say what is the mystery, the sodium slug? What is the sodium slug?

MR. HOLTZ: The sodium slug is that material that lies between the upper internal support structure and the bottom of the head that gets accelerated from a core-disruptive event. You will hear more about that tomorrow because Mr. Theofanous and Mr. Bell will speak to that.

MR. MARK: It is imagined that all the flat of sodium gets uniform acceleration and is thrown up?

MR. HOLTZ: At this point in time, that is the way we are treating it.

MR.MARK: So you don't treat it as it would almost be in reality, some sort of a geyser?

MR. HOLTZ: You are correct. It doesn't look like a fountain, but in reality it probably does.

MR. BUTLER: My name is Tom Butler. I am from Los Alamos,and we have been contracted by the program office to help them review the SMBDB event and particularly the structural part of this. Now here I have said that we will talk about the primary system response to SMBDB. In fact, mostly we will address the head, although there are requirements for the total primary system, address briefly today these areas, the first two briefly and the third in more detail.

(Slide)

The criteria used in evaluating the primary system boundary and the loads experienced by the primary system and this talks a little bit about the sodium slug which was mentioned earlier, and then the conclusions that we have drawn from the scale model tests that have been performed and the analyses that have been performed.

We performed some limited independent analyses,but mostly our review consists of in-depth review of the tests and analyses that the applicant had performed by way of the evaluation criteria for the analysis performed on the primary system. There is provided a membrane strain limit to protect against plastic instability. This limit goes along with the spirit we say of Appendix F, Section 3 of the ASME code. The limit is based on the work of Hillyer and it protects against plastic instability.

This is a limit provided to protect against local ductile rupture and that is based on the work of McClintock. In most of these, the applicant presented substiating data to show that the limits are of appropriate conservative nature. We have looked at that substantiating data in depth also and have come to an agreement with the applicant that these are appropriately conservative.

There are also stress limits that are used for some of the elastic analyses that they do where they can get away with that.

(Slide)

The load requirements that the primary system has to take. They are basically based on Rexco Hep calculations. Now the Rexco Hep calculations have been verified with the scale model tests that were run at Stanford Research Institute and have been found to be conservative in predicting the loads for the applicant's baseline case.

Now, tomorrow Charlie Bell and Professor Theofanous will talk about how they think the loads they predict compare with the applicant's baseline case.

Now, out of the requirements that presently exists in the documentation that Howard mentioned, the vessel head is required to accommodate a sodium head with 75 megajoules of kinetic energy, and there is a pressure head that it has to take which is consistent with 75 megajoules.

MR. ZUDANS: How do the 75 megajoules relate to total exclusion during the CDA? Is it 600 or 1200 or what?

MR. BUTLER: I think it would probably be best to delay that until tomorrow because Charlie Bell has a long talk about that, I am sure.

MR. ZUDANS: All right. Good.

MR. BUTLER: But it is felt that even though this was prescribed by the applicant in their documentation, that that does involve what Professsor Theofanous and Charlie Bell have come up with in their calculations.

MR. BUSH: On the last item I would have thought that in a slugging condition like that; that since your piping is relatively thin-walled, that the refracted wave would almost certainly make a failure of the primary piping, which I haven't heard about. If I have enough energy to lift the slugs out, I would suspect I had more than enough energy to split the piping.

MR. BUTLER: The evaluation at this point indicates that is not so, that the piping would indeed remain intact and these strain criteria that we have presented apply to the piping as well as to the vessel, to the whole primary system.

Now, that kinetic energy of the 75 megajoules is oriented upwards, is the overpressure that after the impact is what goes down the piping system.

MR. SHEWMON: When you have left the slug on top, is it assumed to be unconnected toanything the way it is in the light-water business, these days or some days?

MR. BUTLER: No. We look at it as the total structure. The head connected to the vessel flange.

MR. SHEWMON: And it's deformation as well as the energy required to lift the head goes into the calculation?

MR. BUTLER: Right. Now, the 75 megajoules I will address here in a minute, how we estimate how much of that energy is transferred to the head itself. And I think probably Westinghouse will address that in more depth in their talk.

MR. SHEWMON: This has been very interesting, but to answer my question, you do when you calculate energy, that is absorbed in the deformation, consider it as a connnected --

MR. BUTLER: Right. Right.

MR. SHEWMON: Right.

MR. BUTLER: The primary, which we determine that a vessel head has modified, would absorb a sufficient amount of energy, is through plastic strain of the head as it bends when it is held on the boundary.

MR. ZUDANS: The head bolts?

MR. BUTLER: No, we have checked the whole load path to assure that it can take these loads, but we have not taken into account the energy absorbed by the bolts in assessing the system in a conservative manner.

In other words, we are requiring the vessel head itself to absorb all the requird energy.

MR. ZUDANS: Well, that is not what will happen in reality. I mean the bolts are at least, I remember from previous discussions, they were supposed to stretch significantly and absorb most of the energy.

VOICE: We will be showing that if that will help you.

MR. ZUDANS: I think it will be necessary to see how the energy is partitioned between the different components and why.

MR. BUSH: I am not convinced.

MR. BUTLER: I will wait for Westinghouse on the last item because you have seen instances where the energies were far below 75 megajoules where it managed to fail piping thicker than you have here. So I guess I need to be convinced.

(Slide)

Most of our review centered on the scale model tests, and I will give you a quick summary here of the te~ts that have been run to date and the conclusions that we have drawn from these tests.

The tests to date have been designated SM-1 through 8. Three of thse were hydrostatic of the head only. That is SM-1, 7, and 8. The others were dynamic tests that include, in the case of SM-2 and 3, a simplified head.

In the way of configuration, SM-1 was a head with no shields plate attached. It was hydrostatically tested to failure. After that test, simplified head was used on SM-2 and 3. The difference between that test was that SM-2 had no upper internal structure and SM-3 had the upper internal structure, and in SM-2 we were talking about the slug and how it comes up.

In SM-2 we had no upper internal structure. In SM-3, this was done because of the presence of the upper internal structure, which disrupts the coherence of this sodium slug that rises above the core.

SM-4 and 5 were the complete vessel with the internal nonprototype head. This was nonprototypic in the way the shielding plates were attached to the head. As a follow-up to this, two more hydrostatic tests were run. The first had nonprototypic head with the shielding plates attached as in the tests SM-4 and 5, and then in SM-8 it was run with the shielding plates in the prototypic -- more prototypic of the design.

The difference between these two tests was that SM-8 absorbed considerably less energy than nonprototypic model, as we expected SM-7. Based on this, we did a more thorough review and found that we would estimate -- I might put up the conclusions -- that the vessel head as presently designed cannot be shown to accommodate 75 megajoule kinetic energy slug of sodium, and further from SM-1 and the other two hydrostatic tests, that failure would be kinematic disengagement of head intermediate rotating plugs. It is something that I am sure Westinghouse will go over in more detail in a few minutes.

This is important because this allows more modification of the head design and some relatively simple manner to allow for more deformation before this disengagement appears and more energy can be absorbed in plastic deformation, and I have noted that the remainder of the load path will accommodate these loads resulting from impact of a 75 megajoule slug.

Unless there are any questions, I guess that is it.

MR. ZUDANS: Will Westinghouse show -- can we see the kinematic disengagement of intermediate rotating plug?

MR. BUTLER: Yes.

MR. ZUDANS: Thank you.

MR. ALLEN: Can I ask a few questions? I am Allen with NRC Staff. Iwould like to make a comment with regard to the question about pipe failure. I am not questioning the possibility of afailure. That is not my field. But CDAs are divided into two main aspects. The potential early failures from energetic releases like the slug impact againt the head and the long-term consequences that you heard last week from John Long, which involved a rather slower phenomena where the debris falls into the cavity and the sodium winds up in the cavity and the debris winds up there.

If the pipe should fail, the scenario would progress, I believe, in an SMBDB type scenario. The concern here is with failure of the barrier between the core and the containment earlier in the transient, and that is why the focus on the integrity of the head. I don't know if that helps.

MR. ZUDANS: There is,however, a different consequence. If it were the failure in the system, it goes through the reactor vessel and collects in the pit. I think it's not so simple. Now the other issue is that the impact is only the head. The pipe will see the pressure history, which is completely different, and unless there is a reflection from this impact, the pipe won't see any such impact because the pipe outlet is many feet below the head and -- I don't know.

MR. STRAWBRIDGE: I will be addressing that in my presentation.

MR. HOLTZ: This is a summary of what's in our SER, and I believe I am correct in stating that the applicant is aware of a lot of this, and it's been discussed with the applicant. And if they do not agree with it, I am sure Paul Dickson will let me know.

Well, one of the fallouts of this intensive review, which began in December of 1982 and followed on into 1983, which was this kinematic interaction of the plug behavior that Tom Butler mentioned, and so as a result of this, so we are in agreement that additional tests are needed. We are identifying these as SM-9 and SM-10. They will be scale model tests similar to what was done before, but with the inclusion at least in SM-10 for dynamic tests showing how it behaves with the modifications incorporated in it. We consider this a confirmatory test because we do believe that the plug can be made to arc in such a manner that we can absorb the energy that will go to the plug, which we estimate currently is around 40 megajoules.

We are holding the applicant to this 75 megajoules or kinetic energy in a sodium slug at the bottom of the plug at the time of impact. And we agree with them that the modifications that they have discussed with us will probably allow them to reach this value.

Well, it's stronger than that. We believe that they will meet that value. I mentioned the SM-9 and 10 tests, and I have an A and B option, which is in the SER, and we feel it would be highly desirable to have an analytical model that transcends the problem in its simplest form to show the relationship between the scale models and the full-scale models.

At the present time we both have spent bundles of money on it, and we don't really have one, but we would certainly like to take another look at that. It still, however, requires that SM-9 and 10 be done, and in the B option we have recommended that at least a spare model of SM-10, which is the dynamic model, be manufactured at the same time they are manufacturing their first one, because if they lose their data, we would be blind, and we feel that this is a prudent and reasonable thing to do because it's their intent at this time not to go back into the mathematical area so much as going from conditions in the scale models to the full-scale models. And in their letter of transmittal to us of February 14, there is a package addressing higher sodium releases than 1000 pounds. We admit that we could handle higher sodium releases than 1000 pounds if they are controlled and preferably remained in the head access area as a pool fire, as a spray fire. They are hesitant.

We don't really consider this a CP kind of a problem, and we have added one additional requirement which we have been told about, and we would like to see them carry the load past not from the vessel flange but down to and including its distribution in the concrete structure of the cavity.

Now, for Dr. Zudans' benefit, FFTF was designed differently in that it had an under-the-shield fuel-handling system, and it did have stainless-steel stretch bolts that a lot of energy was taken out with. This machine does not. It has high-strength steel bolting, and according to calculations, we don't believe that the head is going to fail in that regime. And I think that Westinghouse will discuss that.

MR. ZUDANS: If you replace the FFTF steel welds, I can't really understand the rationale.

MR. STRAWBRIDGE: We hope that Westinghouse can enlighten you on that. I will point out the difference with a picture.

endT.5A

startT.5B

MR. ZUDANS: A picture isn't required.

MR. SHEWMON: Why don't you wait until we get to that?

MR. CARBON: Fine. Thank you, Mr. Holtz.

MR. MARK: The decision to change to high strength was made on the part of the applicant or the Staff?

MR. HOLTZ: The applicant.

MR. STRAWBRIDGE: I am Lee Strawbridge.

(Slide)

I am from Westinghouse. I am going to be discussing the structure margin beyond the design basis that we have taken with respect to those structural margins.

First, let me say that just in general what we are dealing with is very low probability events. And as we have discussed in prior meetings there are design features that are there to prevent FCDs and to make them in fact appropriate to be considered as beyond the design basis. Nevertheless, we do include and have included from the very beginning of the project prudent margins beyond the design base to further reduce the public risk.

These margins are of two types, which we have SMBDB, for short. They arestructural margin beyond design base. PMBDB for short. You have heard in previous discussions the thermal margin beyond the design base. Basically, that is dealing with the coremelt aspects. That is not the scope of today's meeting. Today's meeting is limited to the structural margin beyond the design base, which is dealing with the potential accommodation, the accommodation of the potential energetics that could result from core-disruptive accidents. So the purpose of having such requirements is to accommodate dynamic loads that could be associated with HCDAs.

(Slide)

We do that by -- we want to do that so that we will avoid any large release of vaporized fuel or fission products through the reactor head into containment or any large release of sodium into directly the containment.

By doing that, we will prevent any short-term challenge to the containment and consequently avoid any large radiological releases from the containment.

(Slide)

The form of the SMBDB requirements that we have identified and imposed on the design are indicated here. There are basically three types of requirements. The first type is denominated dynamic load requirements, and this is to maintain the short-term integrity of the reactor coolant boundary. This is more general. It is not just a head for dynamic loads that could be associated with HCDAs.

We also have leakage requirements to avoid leakage releases that could get in the containment and challenge the containment integrity and certain geometric requirements that provide certain clearances between components to avoid any unacceptable interactions during the HCDAs themselves.

(Slide)

MR. MARK: You had a statement there of 101 megajoules. How do you get that down to three figures? It escapes me.

MR. STRAWBRIDGE: Sorry about that.

MR. MARK: Anyway, total energy release, that is energy release. That is not kinetic?

MR. STRAWBRIDGE: Yes. And I will have a later vuegraph to show that.

MR. MARK: So show us how we get that number.

MR. STRAWBRIDGE: Yes. Moving in now to the start of this discussion, the derivation of the loadings that we have used for the structural margin beyond the design base, I point out here that these loads were originally derived in about 1975. The significance of that is that is about the time when we were placing some of the first orders for the major components, so we wanted to be sure to identify certain structural loading requirements on that hardware as the orders were placed.

Now, at that time we, of course, did not have the kind of base of calculations for HCDAs that we have today. We have preliminary analysis and extensive analysis that had been completed. Even when pessimistic assumptions were made in terms of average fuel vapor temperatures, it was less than 4300 degrees Kelvin. In choosing a method for calculating such loads, we indicated an average vapor temperature of 4800 degrees Kelvin, and the purpose of that was to insure that we did provide some margin to accommodate on the data and the models as well as margin to accommodate design evolution.

Back in '75, we realized we had not reached final design, and there could be some evolution that could have some impact. In fact, the main point that has occurred since that time has been the change from the homogenous type core to the heterogeneous type core, and in fact, that is it in the diretion of being a favorable influence with respect to HCDA energetics.

MR. SHEWMON: Don't remove it.

MR. MARK: What is the melting point and the boiling point that you use for fuel?

MR. STRAWBRIDGE: Fuel melting point is about 500 degrees F. I don't remember it in K., just in units.

MR. MARK: And boiling point?

MR. STRAWBRIDGE: Somebody in the audience has the boiling?

VOICE: 9800 degrees C.

MR. SHEWMON: Now, all you have said, we collapse the core. What are you doing here to start this?

MR. STRAWBRIDGE: This is not a mechanistic calculation of one sequence. What I was referring to up here was a series of analyses that had been done looking at specific sequences where you looked at the things like a loss of power -- and with the assumption that none of the shutdown systems operate, and so you go into an excursion on that basis. We looked at transient overpower type.

MR. SHEWMON: None of the control rods go in?

MR. STRAWBRIDGE: None of the control rods go in.

MR. SHEWMON: Okay.

MR. STRAWBRIDGE: And transient overpower -- with none of the control rods going in. A whole range of things like that were analyzed and led to conclusions of these lower type fuel vapor temperatures. However, in choosing this number, there was not a mechanistic calculation. We said, well, put margin on this number to define the loads and recognize that that is going to provide some extensive additional capability compared to that fuel vapor temperature. And there is a large difference in energy content between that temperature, 4300 Kelvin, and the 4800 degree Kelvin.

MR. SHEWMON: So what you do is take an arbitrary, semi-arbitrary, insertion of activity, and this was for the homogenous coure. And now we have to go to the inhomogenous core? And why is that? To reduce the energy you get from one of these?

MR. STRAWBRIDGE: The initial reason for the change was to achieve -- be sure that we approached the breeding goal of the plant. This is a higher breeding again, so with the -- another factor that goes along with that change, though, is a reduced sodium void coefficient, a less positive sodium void coefficient, and that has an influence when you go through the sequence of things.

MR. SHEWMON: But you said --

MR. STRAWBRIDGE: So it is a positive effect from that standpoint.

MR. SHEWMON: But you have not reduced the bottom line because of that, because you felt you could cope with the bottom line, you could argue?

MR. STRAWBRIDGE: That's right.

MR. SHEWMON: Okay. Thank you.

(Slide)

MR. ZUDANS: Dr. Fauske just said that boiling temperature is 7200.

VOICE: 3200.

MR. ZUDANS: Okay. Sorry.

MR. SHEWMON: He is such a quiet lad, we can't hear him.

MR. STRAWBRIDGE: Now, initial fuel vapor temperature condition. That corresponds to an average of 4800 degrees Kelvin defines a pressure volume relationship which is used in calculations that I will be describing in a moment where the initial pressure here is some 270 bars.

(Slide)

MR. SHEWMON: Do you vaporize 1 cc of the core or the whole core? It seems to me the volume of this gas also is germane.

MR. STRAWBRIDGE: The volume is germane and the volume that is used is the volume of the whole core.

MR. SHEWMON: That also came out of your consideration of failure to insert rods or it was just a problem you could solve?

MR. STRAWBRIDGE: Well, like mechanistically, one would not predict in most sequences these are going to vaporize all the core. In the next vuegraph, in fact, it shows that the initial fuel volume that we use --

MR. SHEWMON: I guess I would kind of like to see some aspect of it brought through the, I hope, the SER when we get into the things, and as a practical engineer I would say they are nonsensical, but maybe I don't understand the situation. Maybe the incredibility of it ought to be on the record if we are making them jump through these hoops or spending a lot of time as you have and they have and now we have too.

MR. STRAWBRIDGE: All right. A few other characteristics that go along with that particular condition that we used as a starting point are shown here. The average temperature was 4800 degrees Kelvin. It was not a uniform, it was a uniform with a peak up around 6000, starting from an initial pressure of 273 bars. That is the core volume. Now, if that is that pressure volume curve is expanded to the volume that results in impact of the sodium that is above the core with the underside of the head shielding, that would result in 101 megajoules of work energy released to that point.

If one theoretically -- which is about the only way you can do it -- were expanded to one atmospheric, then that would force the 661 megajoule number that you have heard about, and if we talk about 661 megajoule event, that is what we are referring to.

Now, I might point out here also that the number 75 megajoules was mentioned in the NRC presentation you just had.

The 75 megajoules is upward, total upward kinetic energy out of the same calculation that led to -- that I will be describing in the case here that led to numbers such as 101 megajoules at the time of slug impact. Total up part kinetic energy of the slug was 75 megajoules. So that is how the 75 megajoules ties into these numbers.

They are assuming the same initial starting condition of this average fuel vapor temperature that I mentioned.

(Slide)

The mold that we have used to predict dynamic loads was the Rexco Hep code and the high-pressure initial starting condition is placed in the core region and around the core region we have the various structures, including the core support structure and the barrel, the reactor vessel.

We have the head representation up here along with some hold-down representation here of getting the loads into the ground. We also have sodium represented through the vessel and one significant thing which is not represented in this figure of mechanical structure is the upper internal structure. It is physically in this region above the core assembly. It is not included in this analytic model that we used.

At the time the loadings were generated, there was not a real capability to handle analytically the calculation of that effect. And we recognized that it would be a major conservatism to ignore it, but we did ignore it in the calculations leading to the loads that I will be describing.

MR. ZUDANS: On this model, how was the core support structure represented? I don't see anything.

MR. STRAWBRIDGE: Basically, you see --

MR. ZUDANS: If you would replace that section, if you would replace that section with the actual design as it exists now, wouldn't that in essence represent the weak link?

MR. STRAWBRIDGE: No.

MR. ZUDANS: And would the expansion forces push the core down rather than cause the most damage at the reactor vessel head?

MR. STRAWBRIDGE: There is forces on the core support structure. We are using in fact this calculation only to develop loadings on the structures, and then we go into more detailed models using the loadings to actually assess the integrity. We do not use this model to assess the integrity of the core support structure.

MR. ZUDANS: And the conclusion is that the main thrust is upward and not downward?

MR. STRAWBRIDGE: Well, for every action there is a reaction. The importance there is a difference in the upward direction, and that results from the difference that you have in cover gas space represented by this area right here unlabeled, and that cover gas space requires room for acceleration of this liquid material to take place. And this can result in a considerable impact loading with the head, which I will be showing in just a moment here.

MR. ZUDANS: What you are telling me is that the support as modeled in here was fairly rigid but you took loads resulting from that calculation and applied to the existing support skirt and it did not break it?

MR. STRAWBRIDGE: That's exactly right.

MR. ZUDANS: You are saying it was the gas compressed on top of sodium? How did you generate the impact? What happened to the gas?

MR. STRAWBRIDGE: The gas -- until you got to the point of the plates, shield plates beneath the head, compressed in calculation.

MR. ZUDANS: And after that? Oh, where did it go?

MR. STRAWBRIDGE: This is some amount of gas that is actually between these shield plates and so on, which in fact could not physically be compressed. So you don't compress it to zero volume, you compress it to some lower limit volume.

MR. ZUDANS: Was the effect calculated assuming that the gas did not provide any cushioning?

MR. STRAWBRIDGE: The compression of the gas was taken into account in the calculation.

MR. ZUDANS: But I don't see any model there. The two additional models. Was that volume put in the calculation in any way at all?

MR. STRAWBRIDGE: Yes. And maybe that will be a little clearer from this next vuegraph. I am not sure. Let me describe. This is just some of the sequence of calculations that we go through using the Rexco Hep code where initially you are putting the high-pressure material in this core region and as time progresses, that core region is expanding due to the high pressure now expanding and pushing materials in all directions and causing expansion of that. And note that this cover gas space here, the head is above this point, not shown on this figure, but the cover gas space is this part here.

(Slide)

And in fact, the 60-millisecond time has decreased to almost contact. We predict contact with the lower -- so that has progressed as the calculation proceeds.

MR. ZUDANS: Unless you have a way for the gas to disappear, you will never have an impact on the total surface. The gas has to remain someplace because of the pressure that is generated. Its volume is not zero.

MR. STRAWBRIDGE: No. There is some portion of the cover gas volume that is in the upper shield plate region which cannot be compressed, and so we are not taking it to zero compression.

Allen Christy, do you have something to add?

MR. CHRISTY: Allen Christy, Westinghouse. I believe in the model actually the cover gas as such was not really present. It was really a volume, but that corresponded fairly closely to the real situation because the volume of gas above the shielding is considerable and as the sodium comes up, it compresses the gas in the lower area into those spaces above the shielding, and the final gas pressure isn't that significant as impact is occurring.

MR. ZUDANS: First of all, impact did not occur in the head.

MR. CHRISTY: It occurrs on the lower side of the shielding.

MR. ZUDANS: And you cannot remove the cushioning effect. I remember that in the calculations. So that means it is an extreme calculation.

MR. CHRISTY: It is more conservative.

MR. SHEWMON: The gas has zero compressibility in this calculation.

MR. ZUDANS: Yes. That is it.

MR. MARK: What is the material that runs vertically?

MR. STRAWBRIDGE: That is the core barrel. It's on that graph there.

(Slide)

This is the reactor vessel here. The core barrel is actually in here.

MR. MARK: You are computing everything inside the reactor vessel?

MR. STRAWBRIDGE: Yes.

MR. MARK: And the wall is a couple of inches thick?

MR. STRAWBRIDGE: Yes, sir. In that order.

MR. MARK: And the pressure that you are applying to that is 4200 or so psi. That does nothing. There is no disportion of that that makes a difference.

MR. STRAWBRIDGE: The reactor vessel does experience some strain, some deformation, a few percent of permanent strain is predicted, well below 10 percent or so. That would be in the area of where you would have any concern or failure in the vessel.

MR. MARK: It does enlarge the area in which the sodium sits.

MR. STRAWBRIDGE: Yes.

MR. MARK: That is not taking into account --

MR. STRAWBRIDGE: No. It does take into account the flexibility of that reactor vessel.

(Slide)

Now, the head load predicted from this calculation that we are talking about is this load that goes up to a peak of 160 million pounds. This is force as a function of time, and I mentioned in the previous vuegraph impactoccurs at 70 milliseconds, and that is this big spike, and some of the dropoff that is experienced here is in fact due to vessel expanding and creating more volume. So that is taken into account in the calculation. And there is wave reflections that are responsible for some of these longer-term effects here.

MR. SHEWMON: And when you get your 75 millijoules or whatever, it is for how long? Just the initial spike or after --

MR. STRAWBRIDGE: No. It's not just the initial spike.

MR. SHEWMON: And you point to something there? Is it off the scale or 10 percent of that or what?

MR. STRAWBRIDGE: We are using this as the actual loading requirement on the underside of the head. So we have to take this load over the whole time shown here. That is our load requirement.

MR. SHEWMON: Thank you.

MR. CARBON: I still don't understand. Can you relate this to 75 millijoules for me, or am I on the wrong --

MR. STRAWBRIDGE: Yes. Let me try. Presumably, it is the interval under that curve.

MR. CARBON: Is it a lot more than that?

MR. STRAWBRIDGE: That is essentially it. It is not quite that.

MR. CARBON: Excuse me. The 75 megajoules is essentially the interval under this curve?

MR. DICKSON: No, it is not the integral under this curve. Of that 75 megajoules it hits, of course, it must be distributed. Some going to the head and some going elsewhere. Our next speaker will address that split of energy.

If you take the strain that that force produces so that you get the strain energy, then that represents the fraction of the 75 megajoules that does appear in the head but the whole 75 megajoules does not turn into energy in the head.

(Slide)

From this calculation, this one looks at the point in time of slug impact, which is on it here, but 70 milliseconds or so, and all the points where material is moving in this calculation at that point, upward, the upward kinetic energy is in fact the 75 megajoule number.

MR. ZUDANS: One more question, or a couple more. This space above the sodium in the reactor is connected to color gas monitoring systems. It's also connected to some overflow system which functions later on as the independent heat removal system. Not in this calculation, but in the later review would they be broken off?

MR. STRAWBRIDGE: No. I will get to that in about two vuegraphs.

MR. ZUDANS: All right.

MR. STRAWBRIDGE: I have inserted one vuegraph here to respond to a question raised earlier, to heat transport systems and would the same events leading to the head loads cause failure of the transport system piping? In the Rexco Hep calculations that I showed, we do look at the pressure loading at the location of the nozzles, reactor vessel nozzles. Here is the outlet nozzle pressure and in fact, as you can see here, there is a pressure loading prior to impact as the pressure wave gets up to the location of the nozzle before it gets on and the slug impacts the head. You have some earlier pressure peak into that, and later on you see peaks due to reflected waves after you have the impact with the head. But what we have done is to use this loading requirement at the outlet nozzle, another similar loading requirement calculated from Rexco at the inlet nozzle, and then used another calculational tool to predict loadings around the whole system, not only the piping but the components in the primary system and these pressures as you can see here, which would be pressures at the nozzle location and generally around the system, they are generally lower than this but are running 400 to 500 psi range.

And those pressures are not sufficient to fail the primary heat transport system piping.

MR. ZUDANS: Where was the outlet?

MR. STRAWBRIDGE: If this was the most limiting pressure along that piping, then it was used. I can't recall if this was simply used or not, but we looked at the pressures all along the section and chose the most limiting condition for it.

MR. ZUDANS: This time scale in milliseconds interests me. I expected it would be in microsecond range in this incident in the beginning. Are you sure there are some peaks there that are not shown in here?

MR. STRAWBRIDGE: Perhaps the difference here is that what you are thinking of shorter time frames, you are thinking of what is going on in the core. That could be some of the conditions when you have gone super-prompt critical.

MR. ZUDANS: If something happens, how much time does it take for the pressure pulse to propagate to this location? Is that micro or milliseconds?

MR. STRAWBRIDGE: That is milliseconds, yes, sir.

MR. ZUDANS: Okay.

MR. MARK: You will avoid a fair amount of -- this outlet nozzle is 3 feet across.

MR. STRAWBRIDGE: Yes.

MR. MARK: So sodium will be running out as fast as the 400 psi pressure will move it, which is as fast as it will move it upwards too. That is not in the picture I guess. You will lose a fraction of your sodium slug out these pipes.

MR. STRAWBRIDGE: Yes. That is true. But that pushing sodium into the pipe is in fact one of the aspects that is pushing pressure pulse around the system.

MR. MARK: I am just saying it will subtract from your -- of what goes on with the pump.

MR. STRAWBRIDGE: Oh, okay. Now, in fact, the question was raised, are we raising other areas?

(Slide)

In fact, this is the list of areas that we have applied structural margin beyond the design basis loads, broken it into two sets. One is basic loads which can be derived from the Rexco type model that I showed you. And that's the various components in the vessel and under the heads and so on. Then you have to take the Part B point loads from Rexco Hep and apply it in some other model and get to the point of these components. That is this list of components: reactor cover gas system, impurity monitoring, and analysis system. So we

So we are taking other systems into account here and specifying dynamic loads.

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MR. ZUDANS: One more question. With respect to the model, at least the picture you showed did not indicate any deformation on the reactor previously. Was it subsequently analyzed for pressure histories derived in this analysis?

MR.STRAWBRIDGE: No. That Rexco calculation included a flexible row in that reactor vessel.

MR. ZUDANS: It was just small enough not to be noticed?

MR. STRAWBRIDGE: Yes.

MR. CARBON: I have no knowledge of Rexco code. Is it a highly complex one? And if so, is it well grounded, well checked, lots of tests against the code to be sure that it's giving you good results; dependable?

MR. STRAWBRIDGE: Yes. I consider it to be a well-verified code. It's been tested against different experiments. Not only some of the experiments that I have been showing,but also, FFTF and various kinds of experiments, and I think it's a recognized, well-verified code.

MR. CARBON: Against experiments?

MR. STRAWBRIDGE: Yes, against experiments.

Now, when we performed our analysis of the components, there is a special aspect of margin beyond the design base analyses.

(Slide.)

These are given design base analysis. I would like to note those load requirements are not part of the ASME code. So they are not included in the stress report showing code compliance. However, we prepare separate reports, stress reports. We may use recent acceptance criteria compared to ASME codes, provided functional requirements are met.

SMBDB loads are not combined with seismic loads. I think NRC mentioned there is some relaxation of the acceptance criteria compared to the ASME code, but the basic spirit of the code is maintained. We do not combine seismic loads, for example, with the structural margin within the design base loads.

Let me just summarize now what we consider to be the conservatisms -- summarize this portion in what we consider to be the conservatives in the SMBDB loads that we have specified.

In the first place, we are looking at a Class 9 accident condition, a very low probability condition that is beyond the design base and doing something very special for that condition. The loads that we have derived assume that an HCDA could be an energetic event, even though that is not our best prediction. In fact, we have to get quite pessimistic in order to predict that.

Nevertheless, we derived loads on the basis of an energetic event.

Isentropic fuel vapor expansion. We have also ignored the upper internal structure, and that is another conservatism.

I mentioned at the time that we developed the loads we had a homogeneous core, and now we have a heterogeneous core. But the fact that we could not change load requirements at that time -- we have provided another level of conservatism by still using these earlier homogeneous type loads.

(Slide.)

MR. MARK: Well, you showed us a graph of the load on the head?

MR.STRAWBRIDGE: Yes.

MR. MARK: So many million pounds over so many acres. You didn't show us a pressure history at that point between sodium, but you have it, of course.

MR. STRAWBRIDGE: Yes, sir.

MR. MARK: How do you take the sodium -- is it still an incompressible fluid or is it put in there so that it really vaporizes?

MR. STRAWBRIDGE: The sodium compressibility is treated in the Rexco code. The vaporization you are talking about.

MR. MARK: It is going to have a tremendously strong reflected strong pressure wave coming back down the sodium, and it might or might not vaporize it.

MR. STRAWBRIDGE: Cavitation pipe mechanism.

MR. MARK: It will dissipate energy in the sodium, heating it, possibly vaporizing it, certainly compressing it, and that energy is not going to be available for your load. And I am wondering what you do about the equation of state and heat content of sodium under this increase reflected in pressure wave.

MR. STRAWBRIDGE: Well, the Rexco calculation includes internal energy in the sodium, and that turns into -- much of it becomes heat energy.

MR. MARK: Okay. You have a reasonable equation of state for the sodium when this pressure step comes back?

MR. STRAWBRIDGE: Yes, I think so.

MR. DICKSON: Excuse me, Lee. Dr. Carbon, if you would indulge us, I would like him to go back to the two viewgraphs that showed that loadings on the nozzle, since Dr. Bush just returned and he missed that and he asked a specific question relative to those points.

MR. MARK: This is for you, Mr. Bush.

(Slide.)

MR. STRAWBRIDGE: The question has been raised on if we have a condition in which you can have a very high head load, can that same condition result in failure of the heat transfer system piping. I pointed out that we have considered that aspect, and we have imposed loading requirements on the piping, and this is the calculation of what the pressure versus time history is at the location of the outlet nozzle from the same calculation that gave us a certain high loading on the head that we used for head requirements. This pressure history, one for the outlet nozzle and another for the inlet nozzle, is used in the separate calculation to track the pulses around the piping and to develop dynamic loadings for the piping in the primary heat transfer system.

The point is, however,that I wanted to say thatthe kind of peak pressures that we see here are in the range of 400 to 500 psi. They are not extremely high, and they can be readily accomodated by the piping without failure. The difference is that the head load is seeing an impact due to a liquid sludge of material having traversed a certain distance before it impacts the head. You don't see any equivalent impact here, but the total overall pressure effect is something that is quite tolerable, and the piping can, in fact, take it and is required to take it.

(Slide.)

Now, as shown on this viewgraph, we have identified suchload requirements on other connected systems such as the overflow and makeup system, reactor cover gas system, the impurity monitoring and analysis system, so they are all taken into account, and the equivalent type loads are derived for all of the systems. They are all identified in our basic document.

MR. BUSH: This says you didn't get a slug. You are assuming a full system on the piping?

MR. STRAWBRIDGE: Yes.

MR. BUSH: The second thing is, of course, is whether there is a wave that could -- what you are telling me there is that the maximum value is only about 500 psi. I guess I am surprised at that, but most of my experience is in LMFBR, so it's much greater.

MR. STRAWBRIDGE: Right, so we have accounted for that.

(Slide.)

Let me move on just briefly to the assessments that we have made in the structural margin beyond design base area.

MR. CARBON: You said that the SMBDB loads are not combined with the seismic loads. It would seem that a seismic event would be as good an initiator as you could think of for an protected loss of flow. Is that not so? Or what is your rationale for not combining the two?

MR. STRAWBRIDGE: The rationale is that we are designing the entire plant, including all the shutdown systems for the SSE type event, and in fact, making sure it will perform under those conditions. So we do not think the SSE is a particularly appropriate initiator for the kind of conditions that we are talking about.

MR. CARBON: Well, certainly, there is no high probability initiator, but it could seem that a seismic event might be as high or higher than any other.

MR. STRAWBRIDGE: Well, a seismic event is an initiator of lack of power. However, that seismic event I would not expect to be an initiator for the other part that is required, which is the failure of the scram.

MR. CARBON: But having part of it taking place, the other part of your concern is that the other part does take place when something does cause loss of flow?

MR.STRAWBRIDGE: The philosophy that we have used in developing the analysis and choosing what analyses are appropriate to consider is that we would combine reasonably appropriate events, though these we call anticipated that would happen once or so over the plant lifetime. We would combine that range of events with failure on scram with something like an SSE. That is extremely improbable by itself. We would not comvinw that with failure to scram because we would be combining two extremely improbable events as opposed to one more likely and one more improbable.

MR. CARBON: But it seems there is possible common mode aspects to this; at least, all the seismic events would seem as likely a cause of loss of flow as anything else that strikes me at the moment for the loss of flow portion, but we are admitting loss of flow is a relatively probable condition.

MR. STRAWBRIDGE: That is anticipated, so we will take that as being probable, from whatever the cause might be. It's the failure to scram, the very, very impossible event, and we don't see that being caused by the seismic event.

MR. ZUDANS: Could I ask for clarification? What is the timescale? Supposing you had an SSE, what is the time range between that point and the time that you would experience this load?

MR. STRAWBRIDGE: Typically, if the initiator we're talking about, an SSE, this could lead to a loss of power. We are talking about on the order of 15 seconds or so to get to the point of sodium boiling in the core and then beyond that to get to the point of any possible condition of energetics, so you would have that kind of timespan at least before you could have additional dynamic loads to energy HCDA.

MR.ZUDANS: So they may come on before but not during? That is the way I take it.

MR. STRAWBRIDGE: I agree with that.

MR. CARBON: Just for curiosity, if they did, would it exceed the acceptable levels?

MR. STRAWBRIDGE: I really don't know. I don't think we have analyzed any combination of seismic along with SMBDB loads.

MR.DICKSON: Dr. Holtz reminded me that there have been analyses that combined some seismic acceleration with this event, and would you say again, what was the level that it would take?

MR. HOLTZ: Howard Holtz, NRC. I didn't say it that way. What I said was that there has been a separate analysis done on seismic margins by Dr. Mallet,which indicated that the heat transfer system piping would take up to a half of a g, which I think there is margin for seismic events.

MR. MARK: It seems to me there might be a seismic reverberation conceivably going on, but it wouldn't affect this. It would be shaking pipes, and might affect other systems but it wouldn't affect what was going on in that -- that wouldn't look very serious inside the sodium.

MR.STRAWBRIDGE: That's a good point.

Dr. Carbon, if it's satisfactory with you, I will propose the next three viewgraphs talk about structural criteria where I did not plan to go into detail. Is that all right with you?

MR. CARBON: Fine.

(Slide.)

MR. STRAWBRIDGE: I will move on to scale model testing which has been talked about some by NRC and I will try not to repeat what they have said. The objectiveswe have set up for the scale model tests that we have run are first, to assess the ability of such models to withstand HCDA loads, provide an understanding of response and interaction of reactor components,provide information to support methods of verification.

To date we have run a threee static test and four dynamic tests performed with scaled SMBDB pressure-volume source that I showed you earlier. That was the one that was used to develope the head loads and the other loads around the system. The scaled bassed on one-twentieth scale model test size, which we have used.

I would like to show one of those static tests which this one happens to be called SM7, which had a head and shield plate under the load similar to one of our tests that I will talk about in a moment. This shows the view here which indicats the three rotating plugs and those plugs testin in a test fixture.

The device is pressurized by a fluid from the underside and this is a static test, just looking at the effect as you jack up the pressure under the head, how does the head deform, and what is its failure mode when it finally reaches a failure point? So the results that we get from a test such as that are indicate here.

(Slide)

Looking at the deformation profile across the head, the three rotating plugs shown across here and for different static pressures under the head plotter here were at a pressure of about 2600 psi in this case. We reached the point where there was disengagement between the large and intermediate rotating plug at this particular location, and you can see things moving apart there, and at that point we were unable to hold pressure in the test any more, and so that was considered equivalent to a failure condition.

MR. ZUDANS: This -- this -- does this model represent the actual arrangement in disengagement?

MR. STRAWBRIDGE: It was intended to do that. You are going to hear more in the next presentation of the details of that and it turns out that there were some non-prototypicalities that will be considered in a future test that will also be described.

(Slide)

The dynamic tests that we ran are shown here. The first two dynamic tests were fairly simple. Not showing all the internals.

(Pause)

What we found in comparing the results of these models is that the kinetic energy of the slug at the time of impact -- at the time of impact with the head in the two cases was done by a factor of two in this case which had the USI and that was one of the things we were looking for out of these first tests.

MR. MARK: What was the motivating mechanism here? To get a high pressure somewhere that made the sodium move?

MR. STRAWBRIDGE: We placed a special shaped type

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in the core region, the region in this model, which would represent the scale core region, and did the extensive calibration test to insure that we were representing a pressure volume relationship that we wanted to get and so putting off that charge in this region then, moved the materials around, gave you the impact with the head and so.

MR. BUSH: That was fluid filled.

MR. STRAWBRIDGE: This is fluid filled. The scale model tests were done with water, not sodium. Now, in the later tests we were more prototypic. You can see it starts looking like the real thing. The USI is there. The head has the three rotating plugs here. It did not have in these first two early models, so we are getting into more detail, and I would like to show just a little better view of this last test.

(Slide)

This will indicate the kind of instrumentation that we had available in this test. Just to point out, all the P's are pressure gauges. SG's are all of the places for strainign gauges. The A's are accelometers, and WS is water surface level gauges moving the motion upward of the slug so that we could find out the velocity of it at impact with the head so this is a fairly detailed representation of our actual condition, and we did have various extensive instrumentation on it.

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(Slide)

The conclusions that we have reached from the scale model tests are shown here. We generally confirmed the conservatism of methods used to predict dynamic loads. We made comparisons of things like strains in the reactor vessel experimentally and compared to predicted. The vesel and core barrel strains well below failure strains. The response of upper internals structure did not jeopardize boundary integrity. The upper internals structure did deform some, but did not do anything that would jeopardize integrity. We showed that in fact the UIS is very important in mitigating the head loads. We determined that the head failure was by disengagement at interface between large and intermediate plugs. We determined that the head response is in fact sensitive to the representation of the under head shielding. The details of that are, in fact, important as we showed in comparing a couple of the tests where we had different types of under head shielding.

Now, although the dynamics tests showed that the head plugs and margin rings remained elastic, capability for SMBDB head load was not proved by the tests. We do not consider that the tests we have run have proved the capability for the head to take the required load, that high load with the 160 million pound spike in it, for example, because of two things. There were nonprototypic

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things in the one I mentioned; and, secondly, the head load was lower than required load because of mitigation by UIS. Now, the head never saw nearly so high a load as what our design requirement head load is. It has not been the project's intent to demonstrate experimentally the head capability for this required SMBDB head load. Since, as I explained, that head was in fact derived from a calculation that totally ignored the UIS, which is in the design. That is an important mitigating effect in the desing, and when we performed the scale model tests, we considered that it was quite important to take that into account because in the tests even though we couldn't do it very well analytically, we could exclude the UIS and determine directly its influence, so we did include its effect in the scale model test. However, now this overall approach that we have used up to now has been modified fairly recently as a result of three different things that have occurred. The very complex head model that we have applied -- and we have tried to match that against some experiments. There were difficulties in getting an appropriate match there, and secondly there was another situation -- you will hear more about that in the next presentation.

The third aspect is that NRC has recently required that the head capability must be demonstrated experimentally before the actual head requirements, which

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assuming there is no UIS present, so these three factors have lead us to a revised approach that has been agreed with the NRc.

Now, the next speaker, Mr. Pannell, will discuss this approach that we are taking in detail, and he will show that in fact it is feasible -- clearly is feasible to design the head to take the required loading. To do that the approach that we have used is to go back to that original REXCO calculation that I showed earlier and to look at the upward kinetic energy at the time of slug impact and that turned out to be the 75 megajoule number that has been mentioned to date, so we have assumed for purposes of doing this study that that 75 megajoules is all in fact in a slug of sodium that impacts the head, and that is the kinetic energy at the time, so the feasibility studies to be presented by Mr. Pannell will show that the head be able to accommodate energy absorption required to be associated with that 75 megajoules.

MR. MARK: Do I have it correct? The 75 is based on the original estimate of some kind which even went back I guess to a homogenous core and is free of recognizing the upper internals structure?

MR. STRAWBRIDGE: That's correct.

MR. MARK: So you cut it in half if you put in the upper internal structure?

MR. STRAWBRIDGE: That'w what we found from comparing SM2 and 3 tests. That is the only difference.

MR. MARK: The real machine will have an upward internal structure, so you take it out, move the sodium to get 75, and the head will stand it? That is your next experiment?

MR. STRAWBRIDGE: That's right.

MR. MARK: Can you say in the dynamic tests that will be done how that calculated 75 compared with what in fact appeared in that slug? It's one of your things which had no internal structure.

MR. STRAWBRIDGE: Maybe Alan Christy can recall details of those calculations.

MR. CHRISTY: Alan Christy, Westinghouse. The upward kinetic energy of the slug from the Test SM2 compared very accurately with full scale REXCO calculations from which the SMBDB loads were derived

MR. MARK: So that gives you confidence that the 75 would appear in such a system?

MR. CHRISTY: Yes.

MR. ZUDANS: I have a couple of questions if I may ask. You said that you had some difficulties demonstrating the results that you get in a head of this more complex model testing. Did I understand you correctly?

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MR. STRAWBRIDGE: Yes. Yes. It is a very complex model.

MR. ZUDANS: I understand. There is something else that I would like to put in proper perspective. If the plates are reduced at impact on the head, the energy had to go some place else, and therefore I would expect that your pressure history would be less advantangeous in the rest of the system. The tests indicate that in case of the presence for a similar test with shields and without shields, you would have higher peak pressures in the reactor vessel.

MR. STRAWBRIDGE: No, we didn't run a test that would show that. The tests -- the dynamic tests tht we ran had the shields represented, so we didn't have two tests to compare one against the other to look at that effect.

MR. ZUDANS: But, anyway, because you close the windows, and whether or not there would be a detrimental effect, I don't know.

MR. STRAWBRIDGE: Well, as the next speaker will show --

MR. ZUDANS: Well, I'm probably going further back. Initially when you had that vessel, the code was used without shield plates. That means that you had a free expansion.

MR. STRAWBRIDGE: Once you look at the real model where you do have upward -- and you have a shield plate, the computed pressure history may not be conservative. The shield plates, in fact, were representative of REXCO.

MR. ZUDANS: But the --

MR. STRAWBRIDGE: The upper internal structure was not --

MR. ZUDANS: And I understood that that represents significant continuation of the load that --

MR. STRAWBRIDGE: Yes. That is true.

MR. ZUDANS: Then if that is the case, then they tell me that the pressure would increase in the rest of the system as compared to the one that you calculated?

MR. STRAWBRIDGE: The calculations did in fact have some shield plates there.

MR. BUSH: Let me pursue Dr. Zudans' comments from a different angle. We have had one classic case of a nonhypothetical core disruptive accident, SL-1. You probably get -- you certainly get a severe pressure pulse.

Now, the head took that, but the circumference section of the vessel and it was thicker -- in other words, the thickness to diameter ratio was substantially higher than this one. It looked like a rather corpulent individual who had a very large bulge around it which I suspect is the condition here where I get a slugging and the pressure pulse, because I have a thickness to diameter ratio, I have a very large number and -- and reverse number, I guess you would say, and I would have certainly expected under these circumstances that I would dissipate enough energy in that direction that I would get substantial bulging of the vessel.

MR. STRAWBRIDGE: I will try to put it in perspective. The solid line is data right out of the SM-4 test which was physically the same as SM-5, which I showed you detail on, and this is the plot of strain along the vessel wall, along this vessel wall, and also strains of the core barrel, this line being the physically measured strain out of the test, and the peak strain is less than 2 percent. This is plotted, of course, not to scale. So it looks like a big bulge here, but it is only a 2 percent strain peak, and 1 plus something percent strain in the core barrel, so we are -- yes, getting some bulging, but it is not dramatic bulging.

MR. ZUDANS: I looked at this picture before. What is that double wall there?

MR. STRAWBRIDGE: Here?

MR. ZUDANS: Yes.

MR. STRAWBRIDGE: The vessel thermal line.

MR. ZUDANS: That is a double wall like that?

MR. STRAWBRIDGE: Yes, sir.

MR. MARK: If you had some magic mechanism, and since we are dealing with magic potions here, anyway, to vent the gas and then there would be no concern about this whole business at all. If I fill the space with sodium while the pressure is developing down in the core?

MR. STRAWBRIDGE: If you had a solid sodium system?

MR. MARK: Yes.

MR. STRAWBRIDGE: You would not generate the slug impact loads. Would you generate fissures through the system?

MR. ZUDANS: How about 273 atmosphere or less? That was peak pressure in the table you gave us.

MR. STRAWBRIDGE: Yes. That is a high number, though.

MR. MARK? That is 4200 psi?

MR. ZUDANS: No. I don't think so.

MR. MARK: All right. That is peak pressure. All right.

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MR. ZUDANS: Speaking about pressures, I have another question that kept on bothering me. In the pressure history as computed by Rexco it shows that it's over in a few milliseconds. I am just wondering why is it that fast? Is it out through nozzles big enough to shoot the rest of the volume out?

MR. STRAWBRIDGE: No. The outflow through the nozzle is not taken into account through the Rexco.

MR. ZUDANS: No?

MR. STRAWBRIDGE: No. What is happening, though, is the vessel is in fact expanding some. That is happening on a time scale that causes the pressure to drop down. That is one of the important effects.

MR. ZUDANS: If that is the case, it would, of course contract?

MR. STRAWBRIDGE: Well, it can't contract where you had two percent strain, but it will contract it about one percent strain. You are talking about the vessel can move?

MR. ZUDANS: Yes.

MR. STRAWBRIDGE: And that is one of the things. There is some of that happening in the calculation.

MR. ZUDANS: I guess even with the argument that you brought up, I am not sure that the Rexco-computed pressure history is conservative. I mean the total HCDA load is conservative. I am not concerned about it, but that the representation by the Rexco code may not be because of the absence of load restrictions for the energy escaped to the head. In a vessel or in the rest of the volume, it may be --

MR. STRAWBRIDGE: If there is any fluid going out through the piping and so on that is not accounted for, that is a mitigating effect.

MR. ZUDANS: But if is not going out through the system and maybe much cannot be out, but because of the resistance of the flow upwards toward the head is less than the real one, the pressure buildup may be higher, and the rest of the vessel --

MR. STRAWBRIDGE: We have considered -- I think what you are getting it, if I could restate the area of concern, is by not having the UIS there, can we be developing lower pressure, if the effect of the UIS causes an increase in vessel pressures which we did not consider in the Rexco?

MR. ZUDANS: That is what I am saying, except that I don't know what the quantitative numbers are.

MR. STRAWBRIDGE: We have information on that from the comparison of the SM-2 and 3 tests. In fact, we saw some slight increase in vessel wall pressure at certain locations in the case where we had the UIS. They were not large increases, and they did not occur over the whole range of the vessel, just at certain specific locations.

Paul Dickson, I believe, has a comment.

MR. DICKSON: Yes. In an Argonne paper in 1972, a pyramidic analysis was done. They let the bolt stretch and varied the thickness of the wall. That gets the same effect you are talking about, and it does make an effect in partition of energy, but it's not gross. Over a large range of wall thickness you are talking maybe 30 percent more going to the head or to the wall.

MR. ZUDANS: It would be qualitatively the same but not to the same time frame, because before the effect can dissipate you have to reach head first. UIS would create high pressures before the head is reached, so it is not quite the same.

MR. DICKSON: It's not exactly the same, but it's as close as we can relate to right now.

(Slide.)

MR. STRAWBRIDGE: One reason I am not terribly concerned about that is the fact that going back to this one again where we are saying strains less than two percent -- remember, that any failure is out here at ten percent or something -- so even if loading did go up, there is still the capability in the system.

MR. CARBON: We have to move on quickly.

MR. ZUDANS: Just one question. Do you have a slide that shows the tests that compared with and without UIS strain?

MR. STRAWBRIDGE: I don't have it handy right here. I may have -- that could be dug out to show it to you perhaps later.

MR. ZUDANS: Okay.

MR. CARBON: Let's move on.

MR. BUSH: Well, I can -- I am not getting an answer to this, I don't think. I am not sure that the energy distributes itself fairly uniformly. In SL-1 it acted as if the energy was in a relatively narrow band as if you sheared across like this, and that says that if I have a certain amount, it is going to be a completely different way than if I sheared it along the wall as contrasted if I put it in narrow bands.

I think we are talking about highly hypothetical things, but I am -- I think I have the same concern as you do. I don't -- I am not convinced that the Rexco code models it as I suspect it might.

MR. DICKSON: Rexco did give a conservative prediction for the amount of strain as compared to --

MR. STRAWBRIDGE: The energy is not distributed uniformly. It bulges more at the top, and there was 1.4 percent strain in SM-2 and 2.8 percent in SM-3.

MR. CARBON: Still a large margin to a ten percent or so strain.

Move as rapidly as you can.

MR. PENNELL: I will be as brief as I can.

MR. CARBON: Go ahead then.

MR. PENNELL: Bill Pennell, Westinghouse, Falls Mills site.

I will go through this rapidly. Here is the material I plan to cover. First of all, I will give you a brief walkthrough of what the closure head looks like and point out the elements of it that are key to the discussion that will follow. I will then look at the evaluation of the test results and show you how we diagnosed the actual mechanism of failure and thereby determine what we had to do to correct that, put the failure pressure up to a higher level. I will then show you how modifications -- and we will then see energy absorption capability that is predicted for the modified head.

There is very little of it will be based on the Rexco analysis. Then I will show you the tests that we planned to back up the presentations, and then I will summarize anticipated performances as it exists today and as it will exist when we make the modifications.

Here is the closure head.

(Slide.)

Note that here we have the actual structural plugs. This is the small rotating plug. We will say nothing more about that.

The intermediate rotating plug and the large one. Note that they are joined together by these risers -- the riser assemblies. The riser assemblies have significant shear thickness, and they were not represented in the SM-8 tests. Under the head you have three heavy shield plates, and these shield plates we know from test performance in a manner that is exactly similar to the deformation that occurs in the large rotating plug. I am talking about the static pressure tests here where the pressure was applied to the underside of these plates. You find that the large rotating plug is the only member and the associated shield plates the only ones that undergo any significant plastic deformation.

The intermediate rotating plug is almost a rigid mode relative to the deformation. That occurred here. These elements are almost unstrained after the test. There is a slight curvature, but it is very modest.

The other point here -- and here are the margin shearings. The margin shearings were present in the test, the SM-8 test. There is on top of the margin shearing -- and you will see a larger scale picture -- there is a keeper ring, a continuous keeper ring. In the context of the discussion we will have today it's important that it fills up the space between the intermediate rotating plug and the large rotating plug, and you will see the failure mechanism was in large measure due to the fact that the IRP was able to slide laterally and thereby the margin shearing was allowed to come out of the engagement. I will show you that in more detail in a moment.

of excess material here, it wouldn't have been necessary. The calculated defects of this -- actually, it's a two-plug, segmented plug. The calculated plastic defect capability is more than adequate to deal with the 75 megajoule slug.

The fact that there was a previously undetected canomatic interaction at the lower edge would still have made the disengagement earlier, but we know how to correct that.

MR. ZUDANS: You don't have a cross-section of where the head connects to the vessel?

MR. PENNELL: Yes. You have a better picture than I have in your handout. In fact --

(Slide.)

-- I will use my backup vu-graph to show you this. Here is the SM-8 test results. We used these test results because the configuration of the under and head shielding was close to prototypic. Here you see a series of profiles corresponding to different static pressures in the region below the head. The pressure was applied in the small cavity and the bladder sealed this annulus.

One point to note is that the annulus over which the pressure was felt in the model was only up to this radius. In the actual prototypic head it would be up to this radius, and you will see more about this later on. So as the pressure built up here, the underside of these plates felt the pressure, The other point to mention relative to the observation that was made earlier about the role of the bolts is that this design is quite different from the AFPF design in one important effect. The periphery of the AFPF closure head -- that was a fuse in the system.

Basically, what you have here is another margin shearing at the outer periphery of the large rotating plug, and that is the reason we don't get a large amount of strain energy in the bolts in this design.

There are bolts that attach to the system to the steel structure here, and they have a modest amount of energy in them, and I can show you the calculations of the energy that is deposited in them, but it's a relatively modest amount. That is a big difference between the AFPF system and this system.

The triple rotating plug wipes this entire area, and there is nothing I can bolt this to. I must have a rotating joint tip now. The load path, if you imagine yourself, impulsive loading goes downwards and upwards, and there is downward impulse which is counteracting that, and there isn't a large flexible element between the head and the vessel.

On FFTF that wasn't true. It was designed to be the major flexible element in the system. It was not deemed to be necessary on this design to do that. This is the D ring. I think that you will see that, but for a little bit and we know from test efforts that that is the way the pressure is applied, and basically the entire head deformed upwards. Although it doesn't appear here, when you look at the actual model head, this intermediate rotating plug is almost a flat disc. However, there is marking coning -- permanent coning deformation on the large plug; and if you now look at that large rotating plug and compared it with what is happening to the intermediate rotating plug, you see that at this interface a large mismatch. Whereas --

Now, you have to remember this is a small rotating type, so theother interface between the large rotating plug and the intermediate plug -- and here you see the same direction they are going. There isn't a slope mismatch. That is important in that what it did was to permit the interfaces to remain approximately the same in terms of relative geometry at this interface, but there was a gross slope discontinuity at that face, and we had the head contacting at this bottom corner, which I hope I can show you in the next figure.

Your figure is better than the one I will use, but I don't believe the one you have would have projected correctly, so if you excuse my use of this, here again is the large rotating plug.

(Slide.)

Red is the intermediate rotating plug, and as you got up to the condition that corresponded to the point of disengagement, and in fact at some point before it -- this point here, which corresponds to a real point, the geometry is not exactly correct.

This point came into contact here. Because there was no riser represented here, and once this comes into contact here, the further hinging -- since there would be no displacement from one relative to the other here, related in the overall red -- this would simply be pushed bodily sideways inside the large rotating plug, and that continued until such time as it simply was pushed out of the engagement at this elevation.

See, it's pivoting about that, and you are pulling away here from the margin shearing. Now, the action of the risers would have prevented that or would have tried to prevent it. And furthermore, had there been the correct margin shearing, keeper rings here, they would also have prevented that lateral motion to a degree, but those elements were missing.

Of course, the big thing that should have been missing was that interference. If we had just had a little more clearance there, we would never have gotten in effect -- in fact, that is the essence of the modification. It just takes a little relief machining there, and we don't get the reaction that led to the failure in the first place.

You see two layouts here. From the information we had previously, it wasn't clear what the layouts were made -- when the plug could slip all the way over here or just as far as -- in that particular layout it indicated it had to go. This was the minimum it could go. This was the maximum it could go. The significance is if it was only going this minimum amount to shear off the corner of the margin shearing. If it was going the maximum amount, it would actually clear the margin shearing, but we since got the head model and we examined it, and we know the lower picture is more correct. In fact, I will show you photographs taken of it, and you find that at the point of disengagement there was very little scraping away of material at that interface. It moved over far enough that it could move out with actual clearance.

Let me show you the photographs. This is a composite of the photograph of the models.

(Slide.)

This is the SM-8 model, and you are looking down on the top surface of it. Recall at the start of the test this annular gap and this annular gap were the same width. You can't close them completely because the abutment down below is the thing that contacts, but you can see that the IRP has moved bodily over some.

This gap is closed as much as it can, and this is wide up, and you can see the margin shearing sitting there. It's a segmented ring, and you can see the clearance. The plug comes right up by it so the mechanism was a rather straightforward canomatic mechanism.

Were it not that we finished the machining of the head, there would be no difficulty in doing it at all, but having determined, we understood this failure mechanism and just --

(Slide.)

Just for the record, here is the conceptual sort of modification we will be making. Basically, we have to take a skim cut here and skim cut on the shield plates here. They give the interface the clearance required to hinge through the required deflection without getting the interaction, without pushing the plug sideways.

MR. SHEWMON: From the figure that we have, it would seem to me that it would rupture at the other side or separate from the other side of the I guess third interface in. I guess between red and blue on what you have there.

MR. PENNELL: Yes, I know what you are referring to, and it reflects the fact that -- and I apologize for this -- but at the time the drawings were made, we didn't have an adequate understanding of that interface. You actually have to see the three-dimensional -- and you are referring -- I don't know if this will show it.

(Slide.)

MR. SHEWMON: But I misread from this --

MR. PENNELL: If I remember, I said disregard the small rotating plug, the small rotating plug which appears to be almost on a point of being able to come out. The small rotating plus is not accurately represented. We have looked at models, and the small rotating plug is firmly engaged, but the IRP is of concern.

The IRP modification is the one we are concentrating our attention on. Now, this --

MR. ZUDANS: Could I ask a question with respect to this? Although it's clear that machining this point -- you would not have this disengagement effect locally. If you are not changing anything, then the physical possibility for the entire intermediate part to move in your picture to the right -- still there?

MR. PENNELL: No. You remember I emphasized in the model, the risers were not present. Now, the risers constitute normally stiff short beams. For this red plug to move laterally relative to the green plug, you have to shear that riser, and that riser is very stiff. It's not as stiff as the reactor vessel, but you are talking about that order of magnitude.

MR. ZUDANS: I don't see where the riser is connected.

MR. PENNELL: Well, okay. Let me put back up the picture of the closure head.

(Slide.)

Now, the closure head, here is the intermediate plug, and there is a cylinder that is bolted to the intermediate rotating plug and another cylinder here. At the top ends they terminate in fairly heavy forged rings, and then the rings are connected by a large bearing.

MR. ZUDANS: Okay. That is where the bearing is located.

MR. PENNELL: That is where the bearing is located. And you have a sinusoidal shear distribution from the IRP to this cylinder. It goes up to the top at the bearings. Since you are only transmitting normal loads, and you will get 90 degrees interface, so that angle enters there, and then the outer space, and then it comes back down as a sine direction to the large rotating plug, and that was absent.

Once you take away the forcing function, that will keep the plugs centered.

Now, the additional centering mechanism -- it's a -- it is the presence of the shearing keeper ring which in the context of this discussion you can simply regard as a radial packer. The plugs can't move laterally one relative to another more than the amount of gap between the margin shearing keeper ring and the plug.

Does that answer the question?

MR. ZUDANS: Yes.

MR. PENNELL: Now, having determined what the mechanism was, we concluded we knew how to make that go away.

The next problem was to determine what the energy absorption capability of the head would be once we made it go away.

(Slide.)

We determined that we would rely on test data and basically here you see the test data from the SM-8 test. These are two points on either side of the plug, IRP and the LRP, where the failure occurred.

See, you have here the nonlinear tail-end section. If you look at the test results about here, disengagement between the plugs started to occur, so that is what this represents.

Now, in order to extrapolate these concerns, we have to do a number of checks. First, we checked the elements with straining plastically. We found it was only the large rotating plug, and therefore, we had a basis for extrapolating. We checked the strains that we get in the larger rotating plug. If we extrapolated that part of the curve -- the strains that we got when we extrapolated up to 3,000 -- and the significance of that will become apparent later -- we have the order of 1.2 percent.

The uniform elongation at the operating temperature is about 7 percent, so we were nowhere close to the limited uniform elongation, so we determined that we were in a region where stable post-yield of the large rotating plug would continue, and we extrapolated the curves, and I will show you a typical extrapolated curve.

(Slide.)

In this manner we went up to 3,000 psi because in separate analyses we surveyed the limiting strengths features on the head and determined that the strengths of the joint between the large rotating plug and the vessel phlange was the next limiting factor.

Now, it's important to reference that we have gone from a defect-limited problem to a strength-limited problem. Three thousand psi corresponds to the failure mode of that joint. I have data to show where that was derived, if anybody is interested; but I don't plan to present it otherwise.

Basically, our acceptance criteria limit us to 90 percent of the load. We are using collapse criteria. The failure load is built on the actual as-built properties. We are allowed to go up to 2,700 psi.

Now, basically what you will see in the next curve --

MR. ZUDANS: I would like to dwell on that curve.

MR. PENNELL: Surely.

MR. ZUDANS: I can't quite perceive that you can do it -- since you are now in the inter -- I can't see how you can extrapolate. My feeling is that there is a point that it will be asymptotic.

MR. PENNELL: It's continuing to rise in a very stable manner, at 1.4 percent, another .2 percent strain. At this point we would predict about between 1.1 to 2.4. I agree with you. It will become asymptotic, but that is at the point of uniform elongation, and that is about 7 percent, so my reason for saying extrapolation is valid is that I am so far away from that point.

MR. ZUDANS: Talking about here a relatively narrow ring with variable widths.

MR. PENNELL: That's correct. That's correct.

MR. ZUDANS: And this ring produces -- once you reach certain deformation, it will just flip over?

MR. PENNELL: We are very far from the stability limit. If you look at the geometry, we are talking about -- I should have emphasized the point, but this geometry here -- this is -- you have an accurate layout of it in your hands, but this actually does represent the deflection that we are anticipating going up to. You can see you are very far from the stability limit on that.

MR. ZUDANS: Now, this is actual deformation?

MR. PENNELL: This is obtained using the extrapolation technique I just described, but basically the deflections up to this point are germane to the concern about a stability limit.

I agree with you. If I tried to go up further -- if I tried to go to 7 percent, I am sure I would get into instability, but we satisfied ourselves we were far away from it.

MR. ZUDANS: And that is --

MR. PENNELL: You remember 3,000 was what I said was the limit of the load. We are only allowed to go to 90 percent of it, so that is -- that is the flexion. That is the deflection that corresponds to that.

MR. ZUDANS: It is far away from any trouble?

MR. PENNELL: Yes.

MR. ZUDANS: The small ring section, I guess it is made rigid by other elements.

MR. PENNELL: There is one effect I ought to emphasize here; the effect of the margin shearing keeper ring is coming into play here. Remember, this wasn't present in the original test, but you can see now that that ring has rolled to the point where it has come into hard contact with this. That prevents further rolling of it.

MR. ZUDANS: Since all of that load has to really go through that last shearing?

MR. PENNELL: Through here, yes.

MR. ZUDANS: If you check that cross-section.

MR. PENNELL: Yes. That's where my 3,000 psi came from.

(Slide.)

I think this picture will illustrate the area you are concerned about. The margin shearing is in here, but, yes, we checked the strengths of this joint. The actual limiting strength was governed by the shear out of this material here. Unfortunately, the cutout isn't shown here, but that was the weakest piece of material. The shear out here plus the strength of these bolts. These bolts added about -- the bolts account for about 270 psi, and the shear -- the margin shear ring actually shearing out of the material in the vessel phlange accounts for the balance.

MR. ZUDANS: To the right?

MR. PENNEL: This is the vessel phlange, and this is a plug, and the artist stopped short of showing the margin shearing, but that is -- the abutment begins here, which the shear ring rests.

MR. ZUDANS: And this is divided?

MR. PENNELL: It is not a single piece. Yes, but it has a continuous keeper ring.

MR. ZUDANS: What is that?

MR. PENNELL: You have to put the margin shear ring in after the head is loaded. You put it in and slide it into the grooves that are not shown, and then you bring down a continuous ring that goes into this opening.

MR. ZUDANS: And the shear ring position -- where -- it is selected so that it is --

MR. PENNELL: The shear ring has no toroidal bending in it. It's driven into the receptacle that it sits in, and just pure compressive load is all. In the tests the keeper ring wasn't even present, which is the only thing that -- there was no toroidal bending. There was no toroidal bending resisting capability.

MR. ZUDANS: I guess that is a typical location anyway.

MR. PENNELL: Yes. We treated it as such, and our assessment was that it was the thing that next limited the strengths of the head and the 2,700 psi was devoid from an analysis of that joint. That used actual archive materials, test data, and the limited analysis.

MR. ZUDANS: Thank you.

MR. PENNELL: Okay. Now, moving onto the result of that extrapolation.

(Slide.)

In the SRI report there was a pressure deflection curve, a volume change. This is, if you will, the swept volume under the domed head. Here you see it for the SM-8 test. This represents about the limit of capability of the head as it exists then. It's 90 percent of the maximum load, 2,010 psi, that the head was available to withstand; so if you were to do an energy absorption capability of the head as it existed then, you would integrate the area under this curve up to this point, line 6, with extrapolation of the deflection curves using the straight line extrapolation technique that I just outlined. Failure occurred at 3,000 psi.

Whether we will get nonlinear disengagement with the modification I can't tell you. It is not germane because we are only allowed to use 90 percent of that pressure, and so we can integrate now up to this line marked 4. So basically with the modification in place and the elimination of the canomatic disengagement mechanism, this area represents the energy that you can absorb prior to that from the existing test data. This represents the energy that you can absorb.

I am sure it's clear from the discussion up to this point, but there is the potential for error in this. A number of the interferences that I looked at were determined by graphical layout. There is always a potential for them being in error. The reason I don't get too excited about that, the area under this curve is substantially in excess of that which we need, and I will show you those numbers in a moment.

MR. ZUDANS: I guess it has been pointed out that this is how much energy you can absorb, but that is not the energy that you can dissipate.

MR. PENNELL: I haven't got to that point. I agree with you. This is how much energy we can absorb, and I will give you actual numbers momentarily, but the thing I believe that I have to address is how much energy we can actually deposit in the head. Before I get to it, I will pick up a point that came up earlier on. Here are some additional sources of energy absorption capabilities that are additional to those that I described up to this point. You remember earlier on I showed you that the model area was a slight underestimate of the pressure area that existed in the real closure head. This is the correction for that. I am using engineering -- mechanical engineering rather than megajoules. I will convert it back later, but I think it's 8 1/2 times 10^6.

Here you see that area correction gave us 36 times 10^6 pound inches. Simply raising the head through the domed elevation that we anticipate that it will go to -- it weighs about one million pounds -- gives us 5 times 10^6 pound inches.

The reactor support strained energy, which was the subject of the earlier discussion, attributes about 11 times 10^6 pound inches, and adding those together gives me 52 times 10^5 pound inches.

When I compared that to the numbers I am getting from the head, you will see they are very small. Don't react to the 22 megajoules. I will give you that explanation next.

Dr. Zudans, I can go through the derivation of this. I know you were concerned that that seemed like a prime energy absorption location, and it's a rather low number in my record.

MR. ZUDANS: Well, the way it's designed, it's not so.

MR. PENNELL: Well, then I won't go through that if that's all right.

(Slide.)

This is the calculation that determined the amount of energy that you were going to deposit in the head. It's a very simple momentum calculation. What we have is the mass of the slug over the mass of the head plus slug combined giving us a velocity of that combined slug after impact. This assumes an instantaneous transfer of momentum. The kinetic energy is pressed here, and following the algebra through, you got the kinetic energy that will be deposited in the head -- and assuming inelastic collision -- and I will come back to that in a minute -- has been deposited in the head being 1.95 megajoules out of what started out at 75 megajoule slug.

Where is the rest of the energy? A quick water hammer type calculation shows you that about 40 megajoules at the instant following impact is potential energy right there in the slug. It goes on to strain vessels, and we believe to create violent turbulence in the slug. It is a rather simple calculation, and we'll show you that is the home of more than 50 percent of the energy at the instant immediately following impact; but this level of energy we still believe to be significantly on the conservative side. I base that statement on the results from the Rexco analysis produced by ANL where they would be getting something like half of this number.

Perhaps we are a bit less than that. So that then is the amount of energy that a 75 megajoule slug may deposit in the head, and I believe it's a conservative estimate of it; and I will compare the energy absorption capability of the modified head that I just showed you with this energy -- and again, we will be working in pound inches, so 173 times 10^6 pound inches is the amount of energy I need to absorb, and we will now look at what I can absorb.

(Slide.)

This curve represents nothing more than the results obtained from integrating under the prior curve. Remember, I had two balloons on the prior curve -- one corresponding to the usable energy in the head as it existed, and here you see it, and it's about 94 times 10^6 pound inches. I need to be able to accommodate 173, so we are short. With the modification in place, the head alone can accommodate 308 times 10^6.

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to see whether there is toroidal bending.

MR. ZUDANS: Since that is a critical area, I think that you probably, hopefully, will find it unique.

MR. PENNELL: I think that I just got a strong head.

MR. DICKSON: Dr. Griffin of Westinghouse will respond to a question asked earlier.

MR. GRIFFIN: Dr. Bush asked a question about the duration of weld data that we have. The data that I was referring to in the range of 900 to 1000 degrees and all the data I could dig up in the last couple hours is also in the range of 900 to 1000 degrees. There is no significant effect of the welds. Now, the data I have in the range of 900 to 1000 degrees Fahrenheit is in the range of pre-exposure. Well, it's -- this sum after 1000 and a few points after 2000. In all of these data they show that the various effects are negligible.

I don't have specific data at the moment. I presume it exists for less than 800 degrees.

Did you have some specific data that you were interested in?

MR. BUSH: I have looked at the casting. You get some embrittlement in the 600 Fahrenheit range.

MR. PENNELL: The amount is not significant.

MR. BUSH: I simply do not like to see the statement that there is no problem without some backup on it.

information I want you to hear. There is something about the testing that you need to be aware of. The yield strength of materials was tailored as best we could to match the hot yield strength of the material in operation. You can't do an exact job and it turned out the yield strength was a little bit higher. That means that for the head as it exists today, the predicted energy absorption is a little higher because it was a deflection limit, if you will. The kinematic interaction was -- and if you change from that and you go to a stress-limited situation, which we now have, what happens is when you extrapolate the curves, you slightly underestimate the energy-absorption capability because to get to the same load, the head will dome a little bit more and neither effects are very significant, but basically they are present.

MR. ZUDANS: The previous model that you showed in the head cross-section didn't seem to resemble head closure. Were these tests 9, 10 and 11, is that the head closure and the vessel flange?

MR. PENNELL: We are still doing analyses to see whether that is necessary. The margin keeper rings. They are all represented.

MR. ZUDANS: Whether it's strong enough or not will determine what --

MR. PENNELL: It's going to be prototypic in the local area. What we are doing is -- we are doing the analysis

Here is an analytical model that was used to assess load transfer under dynamic loading from the vessel and the head not shown here. Through the vessel flange down through the spring, which represents -- you can see the bolts here and down through a spring that represents the steel support. That load transmission -- that load was subjected to combined loading -- you see here, the pressure -- the pressure history acting downwards through the core support structure, the vessel wall and acting downwards, you see here, the upward pressure history which is the same that Lee Strawbridge shows, and this combined history was imposed on that model. The resulting deflection and load transfer is shown here.

Here you see the vertical displacement. Here you see the force. This gives us the 11 -- I think it was 11 times 10^6 pound inches, but you can see the topping out. You are yielding those bolts a little bit, but there is not a lot of deflection here.

MR. ZUDANS: Yes, you gave me more detail than I wanted to hear, but that is fine. The weakest link in the head structure is really now not a large part, not the head itself, but the joint between the head and --

MR. PENNELL: Yes.

MR. ZUDANS: And that does not compromise the 150?

MR. PENNELL: No, it does not compromise the 150. No, it does not compromise it. There is another piece of had we noticed this effect that Mr. Pennell described, we would have been able to take 75 megajoules now, and we may be able to when the test is done properly.

MR. MARK: All right. And the upper head is good enough to take that 75 back down to 50 or below in real life?

MR. DICKSON: If we have to, we will shave the head.

MR. MARK: But all of this will be gone through tomorrow?

MR. DICKSON: Yes, sir.

MR. MARK: Thank you.

MR. ZUDANS: That's a figure to show the load capacity of the shield and the other rings?

MR. PENNELL: I will explain about that. The joint between the shear ring and the vessel itself is designed in a rather unusual manner. What is required is that the fabricator of the attachment bolts have a bolt yield strength for the material. The as-delivered certification. Then the actual diameter is dimensioned such that the throat area of the bolt is tailored with that yield strength to limit the load transmission capability of the bolts to the ledge to 50 millipounds. So you can't get significantly higher load being transmitted there. Now, you might ask is that going to yield -- and I think I should show these very quickly because it will clear this point up.

(Slide)

apologize. It was good for 75 with a high level of competence.

MR. MARK: What is the 150?

MR. PENNELL: That was to limit why I had a high level of confidence. You are basing it on graphical extrapolations.

MR. MARK: The points at 150 which you don't want to lean on, of course, at all?

MR. PENNELL: That's correct. That's correct.

MR. MARK: Which would, of course, point at the same level that the Staff's old 1000-plus megajoules would point at.

MR. PENNELL: I hear a ratchet clicking.

MR. MARK: No. No. It will come up tomorrow, and we will learn the current status.

MR.DICKSON: If I can understand where you are leading, I don't think it is quite like that. When we did our original analysis, we did it on a conservative basis, as Mr. Strawbridge explained, and did not include the mitigating effects of the upper internals, and we required our head to take that kind of load. What the NRC is now saying to us, since you designed for it, we don't want you to back off on it and take advantage of the mitigating features. It's not quite the same as ratcheting.

MR. MARK: So you are telling us that you think you are okay at 75 megajules?

MR. DICKSON:That is what we designed for, and tubes. They absorbed negligible energy. They trimmed the spike of the pressure curve. They were a load spreader, if you will. It turned out on this head when the designers went through it that they found it was more than trimmed out by the nature of the large under-the-head shield plugs and they didn't need the crushed material.

MR. CARBON: Any other questions?

MR. MARK: There was a time about five years ago, maybe seven -- you had 661 megajoules.

MR. PENNELL: Yes.

MR. MARK: The Staff at the same time was saying something approximately twice that was necessary. Have they now joined you in saying that the 661, which is also what translated equal to your 75 -- that that is a satisfactory level to work against?

MR. PENNELL: I am going to ask someone else to answer that.

MR. DICKSON: That is on tomorrow's agenda.

MR. MARK: Of course, we can --

MR. PENNELL: My understanding is that we don't get anything like that.

MR. MARK: I think so, too. Then in addition you say you are prepared, on paper, anyway, to say that your design ought to be good for 150, which is twice that?

MR. PENNELL: If I gave you that impression, I will have two static tests. Depending on what those static tests tell us, we will run a 75 megajoule scaled appropriately slug test with no UPS inside the vessel, and that will be the final demonstration that we have a head that can take that slug.

Now, bear in mind I had capability to take substantially greater than that on paper, and that gives us the assurance we need to feel confident that that test will be a success. We don't have to be correct in all the elements of the analysis I showed you. Here is the summary of what we believe.

(Slide)

Here is the existing head. There is an increment of additional energy absorption that can go in there that I haven't added in, but basically it has 94 x 10^6 pound inches. If you conduct it through the conversion from slug energy to energy deposited in the head, corresponds to a slug energy of 40.8 megajoules, which I think is the number Howard mentioned earlier. It can go up to -- we only need 75, and we don't plan to test it beyond that. We are confident that the head can accommodate the energy delivered to it, and that completes what I have to say.

MR. ZUDANS: Did you at any time during the design process -- did you consider the pressure material?

MR. PENNELL: It was done on the FFTF reactor. I was directly involved with the design of that. We had crushed

The increment due to the corrections and the small motion of the support gives me an additional 52, giving me a total of 360 times 10^6. Now, it's true to say that I would have an increment that I could add to this energy absorption capability too. It wouldn't be quite as large as that, but nevertheless, it would fall way short of the 173.

Now, I must emphasize when I say that there is in this test some very important features that would have increased the strength of the head and therefore its energy-absorption capability was missing from the test, so we do plan to rerun that test. The SMA test. With those features in the model -- and we will be revising that number, and depending on what the test tells us, we either will or won't need to modify the head configuration, and that is why I see in the table of tests that you have here one more test that Howard Holtz mentioned, and my numbers are slightly out of sync.

(Slide)

Here you see the repeat of the static pressure test and the model is the existing head geometry, and it has prototypic representation of the riser, the keeper rings, and the shield plate support cylinders, so those additional items will be in that test. We plan a static test which is going to be on a geometry which is identical with this, apart from the fact that the machining relief that we identified as being necessary to take away the kinematic interaction will be present, so we

T.5F(9)

MR. GRIFFIN: According to some data by Wilder, it runs out to 67,000 hours at 900 degrees Fahrenheit. He found none. Apparently the delta feroid would not change.

MR. BUSH: You have three embrittlement mechanisms in this range, and I expect -- what I would represent -- not about 4 or 5 percent delta feroid. I wouldn't expect anything.

MR. GRIFFIN: If we can pursue it further.

MR. BUSH: No.

MR. GRIFFIN: Thank you.

MR. CARBON: Any other questions from anyone?

Let's break, then, and come back about 5 till 2:00.

(Whereupon, the meeting was recessed, to reconvene at 1:55 p.m. the same day.)

End T.5F
End
5 series.

6 series
fols.

T 1
Riley
fls R&L
Rabb

AFTERNOON SESSION

(1:55 p.m.)

MR. PLANCHON: I am Pete Planchon. I work for Westinghouse on the Clinch River project and I work out of Oak Ridge. I will talk about features in the Clinch River design that allow us or the plant operators to cope with emergencies that might happen in the plant.

(Slide)

Now, this is an outline of the topics that I will discuss in my presentation. I will discuss our on-site emergency control centers. Those are principally our control room and the technical support center. Also, I want to mention right now that we do have an operational support center in the plant. I won't spend a great deal of time discussing it, but it is an area to put emergency personnel and equipment for responding to emergencies.

In discussing the control room and the technical support center, I will discuss a large set of instrumentation and data systems for getting information to the control room operators and supervisory personnel in the control room and technical support center so they can make the correct decisions and carry out emergency response and emergency recovery procedures.

Now, after I put together the presentation, I was relayed a set of questions about emergency response that dealt with sodium leaks and how we would deal with those. I intend to work most of that discussion in with the presentation and then wrap up and address any residual concerns at the end of the presentation.

(Slide)

This is an artist's concept of the Clinch River control room, and it is taken from the perspective of the technical support center. This person right here could represent the supervisor in the technical support center. This person right here would be the control room supervisor, and this person right here would be the unit operator.

I will talk about the details of this layout, the instrumentation and controls available for dealing with emergencies in more detail, but first I would like to talk about one of the major elements of philosophy that has led us to that particular design.

(Slide)

MR. ZUDANS: Could I ask a very quick question? I see that poor guy sitting there, and he has to use his keyboard on the CRT, right? Where is he going to put his legs?

MR. PLANCHION: This person right here. Where is he going to --

MR. ZUDANS: -- put his legs when he wants to use his CRT keyboard.

MR. PLANCHION: He will be able to reach the keyboard from where he is sitting right here.

MR. ZUDANS: Have you ever tried to do that?

MR. DICKSON: That is an artist's concept. We have a mockup of it, and I guarantee you can sit at it.

MR. ZUDANS: Good, because I use it and I know --

MR. PLANCHON: One of the major elements or one of the major considerations that led us to this design is the consideration of command and control of the whole plant operations. We think it is important to provide the control room supervisor information that will support him in a cognitive behavior mode, that he is in a mode that he can access information, take that information and make decisions from it, direct actions both inside and outside the control room and throughout the plant and to deal with normal operations as well as emergency operations and then have information available to see that his directions have the desired effect in plant operations.

The other consideration is that we want to provide the unit operator, this person at the board that operates the various pieces of equipment in the plant with information to support him in a real-based and skill-based mode. A real-based mode would be where he would follow procedures step by step and carry out a prescribed action. Skill-based would involve the skill of manipulating the controls and getting feedback from the indications to control a plant evolution or control

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a piece of equipment at a plant.

Now, the main thrust of what we have proposed to provide with these two concerns or design requirements is to provide a state of the art plant computer system where the supervisor -- the unit operator will use it too, but it is primarily for the supervisor to provide him information. We have output terminals here and you can see those on the layout of the board, and we provide a board that is laid out that has a number of human factors considerations factored into this layout to help the unit operator in actually directly controlling the plant process.

(Slide)

Now, I want to talk a little bit more about the details of our emergency control center, and the first one is the control room. The picture that we are looking at right now, we call this arrangement an overview arrangement. It provides an overview from the supervisor's perspective so that he has a direct view of the actions of the unit operator and also he has a wide view and can assimilate information that he receives from the control board.

The layout on the control board -- it is laid out to follow an energy flow scheme. The safety controls are down here. These include the containment isolation system. The reactor controls are in this area right here. Included in that would be rod control, overall plant control and readings of

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reactor flux. The energy then flows from the reactor through the primary heat transport system section, intermediate heat transport system section. Steam generator controls and indications are located here. The balance of plant, main steam, feedwater condensate are in this area, turbine control, and finally controls and indications for the generator.

We have incorporated into this layout our accident monitoring instrumentation. This is instrumentation that is consistent with the guidance that is given in Reg Guide 1.97 as we have applied them to an LMFBR system. The key parameters, and for an example, one of those key parameters would be measurement of reactor flux, are Class 1E safety-related instrumentation, redundant, seismically qualified and environmentally qualified and integrated into the main control panel in this energy layout scheme so they make sense to this unit operator relationship-wise.

Their location is fixed so that it facilitates control of the reactor.

Perhaps I should say a few words more about accident monitoring instrumentation. I mentioned that we had incorporated accident monitoring instrumentation into the design of our plant using the guidance in Reg Guide 1.97. Basically that means that we have instrumentation that corresponds to the types that are described and perform the functions that are described in Reg Guide 1.97. As you know, that Reg

Guide is fairly specific for water reactors, but in functional terms we have found it to be applicable to our plant. That is, we provide information for the operator to make decisions about the actuation of safety systems.

We provide instrumentation from which he can assess whether or not the basic safety functions -- reactor shutdown, decay heat removal and containment isolation -- are being performed. We provide information from which the operator and supervisor can assess --

MR. CARBON: Let me interrupt a moment, Mr. Planchon. We have had somewhat of a mixup here. The topic on the agenda here was aimed at Mike Bender's question the other day, which had more to do with the recovery from an accident rather than the emergency handling at the time. This stemmed in considerable part from Mike's concern that we had an accident at TMI and 3-1/2 years or 4 years later it is still sitting there and we don't have it cleaned up or anything like that. And part of this discussion, at least, was intended to say: suppose we had an accident at CRBR, could we recover from it, and how might the design be changed if need be to enhance recovery?

Maybe nothing needs to be done, but have you considered that aspect of it? I don't know whether you are prepared to discuss that at all here today or not. What you are saying is good material but it isn't exactly what we are aiming at.

MR. PLANCHON: Okay. A good portion of my presentation does have to do with managing an emergency up to the point of recovery. However, I have included some discussion of recovery, particularly in areas that involve sodium spills.

MR. CARBON: Would you, to the extent practical, emphasize recovery in contrast with up to the time of the accident or up to recovery?

MR. PLANCHON: All right, yes, sir, I will. I will try to be very brief about the other one.

MR. CARBON: Okay.

MR. PLANCHON: Now, let me just be very brief and say that our plant computer system supports a safety perimeter display system, an integrated set of graphics from which decisions can be made and emergency response and recovery procedures or strategies can be applied.

I guess one other point I want to make with respect to accident monitoring is that we have an extensive capability for detecting sodium leaks to be able to determine that we do have a sodium leak and be able to determine it in very low levels of sodium leakage. We can determine the location of that leak with a fair amount of accuracy.

As you know, our plant is divided up into cells, so the consequences of a leak are basically restrained to that cell.

I guess the other point is that, given a sodium leak, our design is such that we can continue to carry out the safety

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functions of cooling the core and we have the capability with our instrumentation to monitor that.

MR. ZUDANS: On that slide that you are showing here, could you show where the instrumentation of controls of direct heat removal system are located?

MR. PLANCHON: The direct heat removal system controls are on a panel that is not visible from this view. It is out in this area right here, and that is basically at right angles to this edge of this panel. The panel has the K-frame shape and has a layout similar to the main control panel that you see here.

MR. SHEWMON: Sir, you talked about Reg Guide 1.97 and emphasized how nicely it allowed the operator to see when you were getting into trouble. One of the main thrusts of that was to cover -- also have instruments with range adequate to be able to give reliable proportional indications even when you were in bad trouble, and this got into inordinate ranges.

To what extent have you also got that aspect in your instrumentation?

MR. PLANCHON: We have addressed that and let me see if I can give an example of that. I guess the quickest example to give would be instrumentation that would monitor pressure in containment, in the containment shell to make sure that -- well, first to be able to determine the status of our containment. We have arranged that not according to what our design

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basis pressures are for containment, but we have arranged that instrumentation so that it goes up to where the containment is actually being structurally challenged.

MR. SHEWMON: What about sodium temperatures up to the boiling point?

MR. PLANCHON: We have provided, where the thermocouples above the core -- their range does go up -- I believe that is up to the boiling point. Could you confirm that, Bob Tinder?

MR. TINDER: Yes.

MR. SHEWMON: What about radiation level in containment? That is three orders of magnitude or four higher than operating level but comparable to what you might have in a bad accident.

MR. PLANCHON: There was consideration of that. I don't recall the exact numbers for the containment radiation ranges, but they are ranged well in excess of what one would expect with a design basis event.

MR. SHEWMON: Fine. Thank you.

MR. LIPINSKI: Could we go back to the thermocouples if we are going up to the sodium boiling point? What are the materials?

MR. PLANCHON: I don't recall. Could you help me out?

MR. TINDER: Chromel alumel.

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MR. LIPINSKI: It doesn't go up to that point. I thought you were talking about tungsten or something like that. That is why I asked the question.

MR. SHEWMON: Tungsten boils at 2200 K. was it, or C.?

MR. LIPINSKI: 3200 C.

MR. TINDER: I don't know what the theoretical limit on it is.

MR. SHEWMON: Walt, sodium boils well before 1100 C. Chromel alumel will go up to that.

MR. LIPINSKI: It is not guaranteed to one percent but it will go up there.

MR. SHEWMON: By the time we get up to 1100 C., one percent may not bother us.

MR. CARBON: Go ahead.

MR. PLANCHON: Let's see. I believe I made the point that we have an extensive set of instrumentation to detect and locate and allow us to confine and deal with sodium leaks. Perhaps now would be a good point to discuss recovery from a sodium leak. One of the things we have done --

MR. CARBON: Could you go further and discuss recovery for the big accident?

MR. SHEWMON: Have your sodium be well contaminated.

MR. PLANCHON: Let me bite off one challenge at a time and then talk about sodium, please. One of the things that we have done in our design and in the design of the control room is to review the control room and its adequacy and the adequacy of our procedures and provisions for dealing with emergencies. We have conducted a sodium leak and sodium fire review and they looked at our capability for recovery from sodium leaks in the various cells, in cells that are air-filled, which would systems in it which would not contain radioactive sodium,in cells that are inerted and lined that would contain sodium that would be radioactive.

The conclusion of that task force was our approach to cleaning up a sodium leak or sodium spill would be essentially the same used in other sodium facilities, and in particular in test facilities.

Now, to give you some idea of what this would involve, we can talk about some of the experience in the containment system test facility at the Hanford Lab where they have actually tested the cell liners and they have also tested some of the catch pans that were typical of those used in FFTF and Clinch River.

In one of these tests an area of sodium, I believe it was 110 square feet, hot sodium was put onto a test pan. It was in an air-filled cell. The test pan was filled up to a depth of about six inches, so it was a sizable sodium spill.

They found that about 10 percent of the sodium burned before the oxygen in the cell had depleted, and the cleanup

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after the test had been run and the capabilities of the test pan had been determined -- they spread Metal-X on top of the sodium and they put a covering of light turbine oil on top of the solidified sodium and actually dug it out with mechanical shovels.

I understand that it was dug out in squares about 6 inches thick and 8 inches by 8 inches, put into 55-gallon drums. These drums were backfilled with argon and put in storage. There are similar experiences for sodium fires in inerted cells. However, there you don't have the burning phenomenon and you don't have the oxides to deal with.

I guess the conclusion is that the approach for cleaning up one of these spills that we would use in the event of a major sodium spill would follow the experience and be similar to that that was used in these test facilities.

Now, we also looked at a typical radiation level that one might expect, say, in a radioactive cell after sodium leak. If one does a fairly simple calculation where some of the fuel is spilled and the sodium leaks out and forms a planar surface the thickness of about a foot, the calculations are that after about a ten-day period after shutdown, the radiation levels will be on the order of 50 to 250 millirem, and the cleanup effort would have to deal with radiation levels in this area.

Now, if one had damage to the core that was more extensive, then the cleanup would have to deal with higher radiation levels.

END T 1
Riley
Rabb

T 2
Riley
Rabb

MR. SHEWMON: Is the application of the Metal-X and the oil automatic or does someone have to go in there with a 5-gallon can and spread it around?

MR. PLANCHON: In this experiment that I related to you, they put on protective clothing and went into the cell and de-interted the cell, opened it up, ventilated it and spread around the Metal-X and the turbine oil.

MR. SHEWMON: You don't have anything that would automatically spread either in your cells?

MR. PLANCHON: No, we don't. I would like to talk about the other emergency controls that are in our TSC, or technical support center. This is is the layout of the TSC.

(Slide)

MR. CARBON: Excuse me. Is that the final topic of your talk on the technical support center?

MR. PLANCHON Yes. We had one other thing we wanted to talk about in response to the questions that were asked, and it was to discuss some aspects of the Fermi incident and how it would relate to Clinch River.

MR. CARBON: Why don't you just skip the support center and go on to that.

MR. PLANCHON: Okay.

(Slide)

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Now, George Clare was going to talk for a short time on Fermi and how that would apply to our particular situation.

MR. CLARE: If I could just do that briefly. We were not certain exactly what you would be interested in, although we did see that Fermi might come up. I did go back and check over the Fermi-1 incident, and without going back to the very beginning of the incident and what was involved, there was a considerable amount of fuel damage to two of the assemblies, what you might describe as a complete meltdown of two sub-assemblies. The plant was shut down after several days to allow the power in the core to decay. The undamaged fuel assemblies were removed. The sodium was essentially drained down. The decay power in the fuel at that point in time for tbe remaining two damaged assemblies was not so great that it had to be kept in the sodium.

They were able to go in with the periscopes and determine what the status of that fuel was, and they found that the two sub-assemblies were self-welded together, essentially. Melted together is perhaps a better term. They again remotely down through the reactor vessel head chiseled the assemblies apart and then removed those damaged assemblies.

They were sent off to Savannah River to be reprocessed along with the rest of the spent fuel from Fermi. We would anticipate with any reasonable low amount of damage to

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any of our spent fuel assemblies that we would handle them in a very similar manner. Now, it is conceivable that in some situation we might not be able to drain the sodium out, so you would have not only the difficulty of remote handling but also of blind handling, if I can call it that.

A recent instance at EBR-2 perhaps is a good example of the kind of thing that could be done in that situation. They dropped a new fuel assembly, dropped it off of one of their refueling machines and it built in to a location at the bottom of the reactor vessel. Once they figured out where it was, they just kind of scratched their heads and figured what could have happened to it, and then I think they sent a grapple down to see if they could touch it, and indeed they found it.

They then fashioned a wire rope snare for the fuel assembly -- that is my understanding of the description for it -- and again completely blind, they did not even utilize under-sodium viewing equipment, to the best of my knowledge, although it is available, they went down, snared the fuel assembly, and it turns out that they took it back up to their fuel-handling cell, inspected it, found out it was okay, stuck it back in the reactor, and today it is in there operating.

We could use similar removal techniques for loose parts, pieces, whatever damage might have been caused in the core in Clinch River should that be the case. Continuing along

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with that kind of a concept, of course the problem at Fermi-1 that caused the fuel-melting incident was some loose parts in the inlet plenum of the reactor vessel. Before they could restart the plant, they had to go in and remove those loose parts. They did that by cutting into an elbow of the primary heat transport system piping.

Again, after allowing the sodium to be drained off of what would be the equivalent of our reactor cavity, they pushed a probe -- again, maybe it was a snare, maybe that is the best word for it -- into the reactor vessel inlet plenum, and at the same time went down through the reactor vessel head in the core support structure in a manner similar to what we might be able to do. So they had one arm coming in from the top and one arm coming in from the side, and they were able to manipulate the loose plate that was down in the inlet plenum and pull it out. And furthermore to avoid any of the other similar plates in the inlet plenum from coming out, they were able to chisel off and, in fact, do some torch cutting of some rivets, I think they were, that were holding the other plates on and pouring all that debris out of the reactor vessel.

We again would be able to use similar techniques on Clinch River. Insofar as the cleanup of the sodium was concerned, whether the sodium was in the guard vessel having spilled out of the piping, we could heat it up, process it through our system just as normal sodium, clean it up with a cold trap, clean it up with filtration devices similar to what we expect to put in the core positions prior to plant startup, what we refer to as our core special assemblies.

Insofar as the disposal of whatever debris there was in the core -- excuse me, in the primary system, it would be handled again as Fermi did the same way we would handle any other spent fuel, kept cool in sodium as long as need be and then reprocessed or whatever the situation was at that point in time.

The sodium would not be so sufficiently contaminated that it would be unusable once it is processed through cold traps and filters. Indeed, the Fermi-1 sodium was put back into their primary system, the plant was restarted with the same sodium, and indeed with all likelihood Clinch River will use the same Fermi-1 sodium in our primary transport system.

Now, we have not done detailed scenario-by-scenario studies as to exactly what machines would be fashioned and exactly what procedures would be followed to clean up from an event. We think we have surveyed the technology to be confident that we could clean up from any of the types of scenarios that we have talked about and that we would certainly have sufficient time once the incident occurred to develop the appropriate machines and procedures to do that in a safe manner.

MR. CARBON: Let me ask again. Suppose they had a serious accident in which damage took place to the core and

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contaminated sodium was spread around the containment, a Three Mile Island kind of thing. Have you looked at the problems that would be encountered in recovering from something like that and explored or thought about what kind of changes you might make in your initial design to accommodate some such accident?

MR. CLARE: I am not aware of anything we have specifically done to accommodate an accident severe as the one you are postulating where we are thrusting sodium into the area up above containment. Certainly the very extensive cell liner system that we have in the plant is specifically designed to minimize the consequences of the sodium fires.

The guard vessels are a similar example where we would minimize the effects of a fire. Our emphasis is to prevent the sodium from getting above the operating floor.

MR. LIPINSKI: You mentioned that Fermi had two subassemblies fused together. If that happened in Clinch River, could you handle it?

MR. CLARE: We would handle them the same way Fermi did, break them apart and then handle them one at a time.

MR. LIPINSKI: You would have to go down through the cover and work through the sodium in the site where you are going to chisel and break them apart. You wouldn't be able to see where they were fused together.

MR. DIXON: You possibly could if you waited a long

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enough period of time so that they would not be overheated in air and did not need the sodium coolant. Then, of course, you could go in there with a tank drain.

MR. LIPINSKI: But we are talking about having a full core in place. You would have to decide which subassemblies you could unload and leave the fused ones last.

MR. DICKSON: That is exactly what Fermi did.

MR. LIPINSKI: Do you have a tank that will take your entire sodium supply if you had to move it from the primary system to somewhere else? Do you have a tank that will accept it?

MR. CLARE: We have a combination of four tanks, but yes, they will do that. That was a design requirement for those tanks.

MR. DIXON: I think it is worth noting, even though these were not engineered with the thought of what you do for a long term after an accident, there are cell liners, and every cell of the primary system is separated so that anything spilled in them is isolated from the remainder of the system, isolating any radioactive matter.

MR. CLARE: That perhaps is a key point I forgot to bring up earlier. One of the things we do is compare what might happen in our plant to what happened at TMI. Any incident involving the primary coolant boundary for Clinch River, save a highly energetic HCA that could be postulated to push sodium

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out through the reactor head, would be contained in a single inerted blind cell. You would have no trouble with access to containment. Insofar as the bus is above the operating floor, whether the sodium is inside or outside the pipe is not important. That certainly makes things a lot easier to deal with than what they are dealing with at TMI today.

MR. CARBON: Except if you have any sort of core melt, it certainly won't be isolated.

MR. CLARE: I believe it would be.

MR. CARBON: Well, we have had all the discussions on TMBDB and aerosols.

MR. CLARE: With something approaching 100 percent of the core down through the reactor vessel, that is certainly true. It is much different than TMI.

MR. CARBON: But it is not apparent to me that it would take anything close to 100 percent to keep from having radioactive aerosols of all kinds coming out into the upper part of containment.

MR. CLARE: So long as you are able to avoid, again, any highly energetic event --

MR. CARBON: No energetic.

MR. CLARE: We haven't drawn a line, but depending on the particular situation and whatever the decay power might be, any event which does not lead to a rapid penetration of the primary coolant boundary we would expect to be contained below

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the operating floor.

Hans, why don't you come up and make this point.

MR. FAUSKE: Hans Fauske.

I think to differentiate between TMI and a complete core melt accident, in the case of a TMI-type accident, the core ultimately would be coolable and all the fission product released from the fuel would be contained in the primary sodium. There would be no contamination in the containment itself.

MR. CARBON: Well, really I am talking about -- we didn't anticipate TMI and we didn't know what was going to happen, and it isn't all that clear that we know what is going to happen at CRBR, and I don't think it is out of the realm of possibility that we will have some kind of core melt type accident which would lead to considerable radioactive material getting out somewhere, possibly into the upper part of the containment. I don't really try to tie it to Three Mile Island as such except to say we were surprised there: are we maybe going to be surprised at CRBR or have we tried to look ahead and see what might happen and try to allow for it.

MR. DICKSON: I guess the answer to that is yes, we have tried to look ahead. In the cases we are talking about, SMBDB and TMBDB, we have certainly taken the complete extreme of the spectrum of accidents, and apparently we have conveyed the impression that a partial core meltdown leads to TMBDB. That certainly is not the case. TMBDB is a complete 100

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percent core meltdown right after operation at full power instantly, not any delay or any partial meltdown. Partial meltdowns that did not penetrate the reactor vessel or, if it did, didn't penetrate through the guard vessel, would stay within containment -- or rather stay within the primary sodium and would not involve radiation getting into the access portions of the containment system.

MR. CARBON: Well, if you follow through it mechanistically, I guess I have to agree. I think if we follow through the TMI accident prior to it happening, it didn't do anything either. There wasn't any problem.

MR. DIXON: Well, it didn't do much except that it did contaminate the containment area. It is virtually impossible to visualize every possible thing that could go wrong that leads to something beyond your design basis. As I said, we have taken the worst case. It can't get any worse than that. Anything smaller than that certainly is going to be tractable. Most of the types of accidents you can imagine will retain their radionuclides within the primary sodium, or if not within the primary sodium, at least within an inerted cell.

MR. CARBON: Well, I think you have answered the question that we wanted to explore on what sort of thinking you have done and what sort of possibilities exist. Let's leave it for the present and go on to the next topic.

END T 2
Riley/Rabb

MR. LIPINSKI: I have one more Max. Even though they say that the fission products are going to be contained in that primary system, the gaseous ones are going to mix with the cover gas, and as the concentration goes up and you have got any leakage from that cover gas some fraction appears inside containment.

If I recall the EPR 2 experiments with the failed pins, as they release the gas into their cover gas, then they have got some fraction that they measured within their containment, and if you have a whole core go and an appreciable number of pins and you get your entire gas inventory into the cover gas, it is not clear what is going to appear inside containment and your leakage rates.

MR. CLARE: If I could address that just very briefly, we or course do have a complete clean-up system on our cover gas system that would over a period of time completely clean up even a hundred percent release of the fission gas into the cover gas.

Further, the cells in which that equipment is located where one might expect some leakage to valves, compressor seals, et cetera, are fairly much closed cells and we have the ability to process those cells and the atomosphere in those cells with holdup prior to release to atmosphere. Again, these would be largely nobel gases and eventually, yes, indeed, with no more operation they would be released to the environment and subsequent to any venting, we again would have access to containment.

MR. CARBON: Let me move on to Mr. Palm.

(Slide presentation)

MR. PALM: I guess based on the past discussion, this is sort of a continuation on one of the detailed evuations that has been performed by Clinch River considering a core melt through the reactor cavity and into the foundation mat.

Specifically what I will be covering is in more detail from what I presented at the committee meeting last week, the evaluation of the nuclear island foundation mat due to the effects of a core melt through the cavity and into the foundation mat.

This is specifically the area that I will be talking about very briefly.

The analysis that has been performed basically was a thermal stress analysis of the mat in the local region below the reactor cavity considering the worst of all worlds with the fuel melt occurring rapidly, the accident times zero, the floor liner failing and considering various realms of penetration of the sodium fuel mass into the concrete above this containment liner and then further on beyond the point of sodium boil dry whether or not the foundation mat would be able to maintain its integrity for a very long period of time such that the containment, which is of course supported off the foundation mat, its integrity would not be compromised

The analysis that we have done is basically a thermal stress analysis using finite element elastic-plastic techniques, using the computer program ANSYS, the temperature profiles that were developed by Westinghouse for up to a period of 8,000 hours, or approximately a year after the initiation of the accident.

Material properties are the same properties that I had mentioned last week. They are an outfall of a very comprehensive high-temperature test program that was performed by the project on prototypic concrete to be used in the Clinch River plants.

The material properties are non-linear and are high temperature dependent.

The analysis that we performed is very similar to the analysis that we performed on the internal structures and the confinement structure, that is it is a step-by-step process where we consider conditions at various time increments after the accident. This is done with iterative procedures and we do account for the change in the material characteristics from the high temperatures, both the steels and concrete that is in the models, and we also account for change in the cracking of compressive crushing of the concrete at the sperious time steps.

The specific model that we used for the foundation mat is basically, it is a half exisymmetric model from the centerline of the reactor on out to this edge of the founction -- not the edge, but the portion of the nuclear island foundation mat.

The nuclear island foundation mat actually continues on out. It is approximately 350 by 400 feet in dimension. This particular section here is about 190 feet in diameter.

The model is, as I said, a finite element model. What we have done based on the output from Westinghouse is plotted the various temperatures with time through the accident, and two things are occurring.

One is that the melt front is continuing down through the thickness of the mat and the temperature increase is progressing outward from the center of the reactor cavity.

This particulat plot here that you have in your handout shows the conditions after one year for the 8,000 period.

This particular analysis is for the base case where the melt front of 2200 degrees, this is the melting temperature of concrete, that is Fahrenheit, progressed down to about five to six feet above the bottom of the mat.

Under the margin assessment case, this melt front is further down about one or two feet above the bottom of the mat, what we consider the worst of all worlds insofar as the worst maximum penetration that could occur with an accident of this type.

Now, the important thing from this plot here is that when we perform the thermal stress analysis, we found that we were really concerned about was whether the stress conditions out in this region here where are counting on this section of the mat to support the containment vessel and the confinement structure.

As you can see, the temperatures out here are relatively small, 150 degrees or 100 degrees. Under the margin assessment case these temperatures are essentially the same. There is really no difference as you get out beyond about this point here.

The results of the evaluation to the time of sodium boil dry, and I should mention this, it is very important, and that is that the temperatures above the foundation mat, above the containment liner are approximately about 100 degrees. There really is no influence of any significance up to the point of sodium boil dry, and that under the base case is 132 hours, and under the margin assessment case it is about 50 hours.

The conditions at 8,000 hours based on the temperature plots that I showed on the model measuring out from the centerline of the reactor cavity, we do have concrete that is completely degraded and crushed.

Let me show you another viewgraph while I am talking here.

This illustrates what I am point out, that in the center zone, and again this is the center of the cavity or reactor building, that for a radius of 40 feet from that center we have concrete that is degraded and crushed from the high compressive stresses.

Perhaps I should explain very briefly what really happens with this mat. When you heat the center section of this mat it tends to want to grow outward obviously, and the surrounding section of the mat, whether it be the outer area of the mat or strained bedrock, will tend to restrain this thermal growth and that promotes very high compressive stresses in the mat section.

Now the reason that this degrades are two reasons.

One is that it is getting very hot in this area, and two is that we have the core melts. We have the degredation from the core melts advancing through the concrete. So it is stress degredation and the core melt degredation.

MR. BUSH: I would think you would get severe spillation as a consequence.

MR. PALM: Spillation on the surface as you are going down, that is right. And when you get beyond 40 feet, roughly from 40 to 60 feet we have partial degredation and crushing. Again, this is of no sequence. That is this zone right here because this is still well away from this area out here where we need the support.

Beyond the 60 foot radius the concrete remains structurally sound, as I said, because the temperatures beyond this point are 200 degrees and less. So the temperature of the heat up of the concrete is relatively minor.

MR. ZUDANS: Bob the analysis results don't show any significant mat rotation where the containment and shield building are connected to it?

MR. PALM: We looked at that and and mostly we had shifted the thermal moments of cost in addition to the actual heat up force, and the answer is no.

MR. ZUDANS: Now you had another semi-CDA type of accident where you had a large sodium spill in another cell that was essentially right next to the containment. You will speak about that, too?

MR. PALM: I hadn't intended to.

MR. SUDANS: I remember, because last week --

MR. PALM: Are you talking about this cell, the tank cell?

MR. ZUDANS: Yes.

MR. PALM: That is a design basis accident cell.

MR. ZUDANS: And it didn't damage the containment?

MR. PALM: No. We had done a very detailed analysis of this structure above the mat and including the mat interface and determined the influence of the cell accident condition, as well as the containment DBA on the containment vessel that is embedded in the concrete from the mat up to the operating floor level. That is a separate analysis.

We did do an analysis due to the temperature outgrowth through the reactor cavity to the PHDS cell to this outer wall. We did also do that. That was part of our DMB evaluation.

MR. ZUDANS: Now in your model I saw you showed that the mat ends with the shield building. In actuality I guess mat goes beyond that. At least this slide shows it goes beyond this.

MR. PALM: Yes, it does but what we did is we simulated either embedment in the rock or the continuation of this very massive mat with a fixed condition.

MR. ZUDANS: Oh, you assumed fixed condition at that line?

MR. PALM: That is right.

MR. ZUDANS: So that is actually conservative. So you are conservative with respect to the center portion of a mat because you created a larger constraint, but you are not conservative with respect to containment building connection because it doesn't allow rotation.

MR. PALM: We have insofar as this particular area, we have looked at this in some detail.

MR. ZUDANS: And you have no problems with that?

MR. PALM: Actually it was kind of surprising the fact that the growth here, the thermal growth here and the thermal growth here is pretty much the same.

MR. ZUDANS: Of course, if you have a 80 foot diameter hole in a mat, I don't think you are greatly concerned about that.

MR. MARK: Could I ask, you assume concrete is all right up to some temperature. You said 200 degrees. Is that Fahrenheit?

MR. PALM: Actually the temperature is good up until anywhere about 700 degrees Fahrenheit. That is what we assume.

MR. MARK: Now something will happen to it at 100 degrees Centigrade, the water will come out.

MR.PALM: Yes.

MR.MARK: Or at least half of the water, not the crystalization water but the free water.

MR. PALM: That is right.

MR. MARK: That will come out at 100 Centigrade.

MR. PALM: That is right.

MR. MARK: Then when is it that you really begin to damage concrete? You say 700 Fahrenheit?

MR. ZUDANS: No. I just can't believe it will ever get that high.

MR. PALM: Based on a test, we have run out to about 1600 degrees Fahrenheit, from relatively 70 degrees on out to 1600. Roughly above 700 to say 800 degrees the amount of residual strength in the concrete is in the order of about 20 percent or so. At 100C up to say 400C we still have strength in the concrete, although a major portion of the water has been forced out.

MR. MARK: Well, that is what I was supposing of course.

MR. PALM: And this has been through the experiments that we had performed.

So in conclusion through this evaluation of the mat, we find that there is adequate support provided for the peripheral wall in containment vessel and the confinement structure and that the containment confinement integrity will be maintained for at least 8,000 hours after the accident.

That is all

MR. CARBON: Any questions.

MR. ZUDANS: I would like to clarify a little bit about concrete strength because we might be left with a false impression. You do have some strength that you can measure in the compressive direction, but you wouldn't have any strength in the tensile direction.

MR. PALM: We don't count on tensile strength.

MR. ZUDANS: So wherever your calculations of tension, which it will wherever the temperatures are lower, you have to assume that it is none existing or cracked or otherwise.

MR. PALM: I don't think that your conclusion of this 40 or 60 radius is bad. It is all right, because you had temperatures there of around 500 degrees.

MR. BUSH: That is why I raised the question on spillation, because that is usually where you get your tension interface and then it just doesn't support it very well.

MR. CARBON: Thank you Mr. Palm.

Let's move on to the next one.

(Slide presentation.)

MS. NIEMDZYK: My name is Sue Niemczyk. I work at Oak Ridge National Lab. The question I have been asked to address is assuming that the basemat melts through, what kind of a problem is posed by the radioactivity released to the hydrosphere in case of a severe accident in the Clinch River plant.

To address that problem I think we really need to consider two basic issues. First of all, what is the problem, what is the source term that would be released, what is the radioactivity that would be released to the subsurface and what would be the consequences resulting from that. In other words, is there really a need to do anything about those releases and, second of all, what can be done about those releases.

There hasn't really been much done at all for the breeder reactor itself. There was, however, a study done at Sandia that looked at the same problem for all light water reactors, and it is that that I will use as basis for the discussion today.

In that study, what they did was consider all the operating and under-construction light water reactors plus a number of proposed ones.

First of all, they estimated the consequences, assuming that no actions were taken. Then they went back and put in various types of interdictive procedures.

In addition, they considered both the feasibility and effectiveness of various types of interdictive procedures that could be taken, and they considered both source interdictive procedures which are ones located at the site of the reactor and pathway interdictive ones.

Perhaps you wouldn't want to take action within the immediate vicinity of a plant but you would wait until the radioactivity got out into the environment. So that is a distinction between source and pathway.

According to that study for light water reactors if basemat melt-through occurs, even if you don't take any actions the consequence of releases to the hydrosphere are not expected to be significant for many sites.

However, for some sites the consequences are such that releases may be significant unless adequate mitigating procedures are taken.

Although adequate mitigating procedures can probably be taken at most sites, it is not obvious that they can be taken at all sites. Only mitigating actions taken close to the source of the accident can be very effective in reducing the radiation dose to the population.

So keeping that in mind, we will just discuss source interdictive procedures today.

The emphasis in my talk will be to discuss what is the case of light water reactors and to put the Clinch River plant into appropriate perspective and show how it compares to the light water plants.

First of all, we have to consider the source term, and this is obviously a gutted out PWR but I think the melt-through release is about the same for both, at least the basic of it.

What would happen in the melt-down accident is you would have your core eventually melt through the basemat. At least that is the assumption we are making here.

As the core melted its way down through the concrete it would pick up the residual of the concrete and it would pick up the soil as it was going. So you would get a very large mass.

As it was moving on down, you would have gases boiling off, you would have your carbonates in your limestone and your carbonates in your combination water and concrete.

So what you would end up with eventually, it would take presumably longer with the breeder reactor but many days or weeks for the light water reactor for the basemat to melt through and for your melt mass to come to rest.

Because of all the bubbling that was going on you would probably have a rather porous mass. As the mass cooled it would crack substantially. So if you had any ground water under there presumably it would into very good contact with it and leach out the materials relatively rapidly. Also the mass would be warm for a period of years, very war. So you would have accelerated leaching rates.

What would work in your favor in general is if you would have a kind of a vapor shield floating around the mass. As your melt came to rest in the soil and this ground water tried to contact it, there would be a vapor shield formed around it. This would form in effect a barrier.

The calculations estimate that for light water reactors that barrier would be effective in preventing radioactivity from contacting the ground water for a period of about six months to two years. So presumably you have at least six months to two years to get in there before the radioactivity got into the ground water.

For a light water reactor you have not only the melt debris, but you have opened up a hole here in the bottom of your basemat. In a lot of your accidents you could have large releases of very contaminated water. For example, if your containment didn't fail and your sprays worked, you would have a large fraction of your more volitile radionuclides contained in that spray water.

There are many cases in which that spray water could presumably be dumped down the hole here and go into the ground. That release is much different in character than the melt-through release for two reasons. No. 1, it would be mostly more volatile radionuclides, and, No. 2, it would be a much faster release. You would have your radionuclides already entrained and you wouldn't have to worry about leaching or about vapor shields or about anything else.

There is not that kind of a relase at a breeder reactor. So at a breeder you have gotten rid of one of your worse types of releases, and you just don't have it present.

Just summarizing the points, in the light water reactor you have two basic types of releases and in the breeder reactor you only have your melt debris release.

The nice point about the melt debris release is it would generally take a long time initially for ground water to contact the radioactivity and then for the material to be transported. In the sump water release that you don't have with the breeder reactor, you have got the potential for a relatively rapid release and that makes it very hard to interdict that type of release.

Comparing the Clinch River source term and the typical light water source term, in general for most of the LWRs that were looked at in the Sandia study, the Clinch River plant has a much thicker basemat under the reactor cavity. Therefore, you basemat melt-through is less likely. You have a smaller core in the Clinch River plant and there again the basemat melt-through is less likely.

In addition, there is less fission product activity available for releasing at the Cinch River plant

No. 3, there would be no major releases of highly contaminated water from the Clinch River plant.

In addition, it is the sump water or the suppression pool water that would contain the volative radionuclides. So you would have a whole class of radioactivity that wouldn't be involved in your breeder reactor releases.

No. 4, not in favor of the Clinch River plant, is you would have more plutonium in your releases. but here you will see that that does not dominate the releases or the consequences.

MR. MARK: This point you make about the smaller core and less fission fragments is really only comparing with the 3,000 megawatt thermal reactor. If you compare it with a reactor of the same power, you have got just as much fission products.

MS. NIEMCZYK: Right.

Here by long-term, I mean greater than a period of about 300 years.

Overall, what do the consequences of the releases depend on? Well, first of all, it is initially related to rate of releasees to the hydrosphere.

First of all, you have the delay until ground water contacts the debris and then you have your initial rates of leaching.

Second of all, once the material gets into the ground water you have got to transport it to the nearest surface water, which in the case of Clinch River is the Watts Bar Reservoir.

You can characterize that really by two times. The first is the time it takes for ground water to get from beneath the plant to the nearest surface water, and we call that the ground water travel time, and that is the minimum time it would take radioactivity to get from a plant to the nearest surface water.

The radionuclides in general move at a slower rate than the groundwater. They absorb the soil by ion exchange, the precipitate out and they do a lot of other things. So they can take many more years to get to the surface water than the ground water itself.

And last affecting the consequences you have the various exposure pathways you have to consider, your populations at risk and their characteristics.

MR: LIPINSKI: Could we talk about that Clinch River site water. It sits on a peninsula and the river has to run around the site. The borings showed that the rock strata had cracks and crevices. What can you say in terms of what the structure is like at river water level beneath the plant grade?

MR. NIEMCZYK: We will get to that in a few minutes.

First of all, from the Sandia study, if you look at the ground water travel time, and this is the time it takes for the ground water to get from the plant to the nearest surface at all light water reactors, and here they were lumped according to all the plants currently operating and this is the number of containments versus the time it takes for the water to get to the surface water.

This is the total number. They considered a lot of proposed and under construction.sites.

If you look at where Clinch River fits in, it is over here ---

MR. SHEWMON: We would like to look at where Clinch River is but it is very difficult. Could you move over and take the pointer there and point at the screen maybe.

MS. NIEMCZYK: It is right here at 10 to the 4th

MR. SHEWMON: Thank you.

MS. NIEMCZYK: If you do the approximations in the same way they were done in this study. So it compares very favorably.

MR. CARBON: Excuse me. Tell us again what that means, that ground water travel time.

MR. NIEMCZYK: This is the minimum time it takes for groundwater from beneath the plant to get to the nearest surface water.

MR. CARBON: Which you said was the Watts Reservoir or something.

MS. NIEMCZYK: Right. So presumably you have at least that much time to get in there and do something about your source term.

MR. BUSH: So that is from time zero after it has benetrated the mat; is that correct?

MS. NIEMCZYK: Right.

MR. MARK: And this is water travel time.

MS. NIEMCZYK: This is the water travel time. The radioactivity would be traveling more slowly.

MR. ZUDANS: It bothers me you are using a statement that you have time to do something about your source term. Once it is in there what can you do about it?

MS. NIEMCZYK: We will get to that too.

MR. CARBON. Once again this is saying that it is ten to the fourth days before any water becomes contaminated.

MS. NIEMCZYK: The groundwater would be contaminated but the river wouldn't.

MR. CARBON: Before it gets to where it would influence population?

MS NIEMCZYK: Right the idea being that once it gets to the river, the problem kind of gets away from you.

MR. CARBON: But up until then it is really not harming anyone.

MR. NIEMCZYK: As long as it is still in the ground you have got some control over it.

Well, what difference does the ground water travel time make in terms of population doses? Well, in the Sandia study the large leg is Lake Michigan and they looked at a series of water bodies.

The figure for the Clinch River, Tennessee, Ohio and Mississippi system is just about like this, but I don't have it. You know, if you look at the total population dose that is possible from the releases, and this is for an LWR, and look at it as a function of groundwater travel time. This is what I was talking about before.

There is a period where they are fairly significant out to about a hundred days. As soon as you get beyond a hundred days, the population dose, if you don't do anything, this is if you don't do anything about your source term, falls off very rapidly, and Clinch River was over a ten to the fourth.

What about the Clinch River site in particular? Well for those of you who may not be familiar, the Clinch River site is right here, and this is the Watts Bar Reservoir that goes all around it.

This is kind of narrowing in on it. Here is the plant itself. Here is the containment building. In the topography here there is rise right through here. There is silt layer right through here that is relatively weathered, and there is a valley that runs down this way and over this way.

Where your groundwater would flow is it would take the shortest path through here to the surface water. Both the layer here and the layer here are limestone. You have got silt stone layer that is rather impermeable and then two limestone layers.

MR. SHEWMON: Can you read that scale in the upper right-hand corner and tell me how far is it to the nearest body of water?

MS. NIEMCZYK: I know that from here to about here it is about 60 to 100 feet, taking the shortest estimates, which is this case is inappropriate because it doesn't look like that.

This is the site before construction and then when the building is put in there. What is under here again is silt stone. It is a rather impermeable layer. It is probably one of the best places you could have put a reactor. It is really impermeable. There would be a very low level of ground water. It is very hard for ground water to get into it.

The construction at the site is to go down to this level. With a large number of the LWRs what they do is go through and dig down a ways and put in underdrains and other systems to disrupt your subsurface. There are no plans to do that at Clinch River. What you would have is your melt debris sitting down here in what really would be a very nice repository on a long term basis.

Looking at this section, it is perpendicular to the last one.

Remember, I mentioned these two limestone areas on either side and then again there is the silt stone.

Here again this distance is well over 145 feet in thickness. So you have a lot ways to go before your melt debris can get out of the silt stone.

What about the potential pathways for escaping and reaching the accessible environment? Well, the usual postulated path is it would escape through the silt stone. In this case it is not too likely because the silt stone is so impermeable. It could escape via construction channels. That is not very likely at this site either because they are not putting in any spare channels. It could escape through fissures, at least the ones on the surface grouting through. Then last, it could escape through the degraded silt stone. If you have a very harsh core melt in there for a long period of time, it is going to cause changes to the silt stone and I am not really informed about what they are. So your last two sources here would probably be your main escape routes.

What that means is that you don't have a major flow of groundwater through your melt, you don,t have a massive transport problem and you have got escape only through the leak paths. You have got a very long time to get in there and you have got very low, slow releases.

The way the material could get to the river, it would transport through the unwetted silt stone or it might move over to a limestone layer or it could move up and move through the weathered layers of the surface where it would move a little bit more rapidly to get to the river which would decrease the ground water travel time than what I stated before.

MR. SHEWMON: Chemically what is silt in this case and what are the exchange characteristics for the fission products likely to be?

MS. NIEMCZYK: The silt stone would have fairly good absorption characteristics. Is that what you are asking?

MR. SHEWMON: Yes.

MS. NIEMCZYK: The radioactivity would tend to exchange very strongly in the silt stone. It has enough, and I can't think of a good term for it, but it has enough stuff in it that exchanges very well with materials. If the radioactivity moved over into a limestone layer you wouldn't have that. Limestone does not exchange at all very well and you have much more rapid movement.

MR. LIPINSKI: What was the deepest site boring compared to the river level? Did the borings go below the river level?

MS. NIEMCZYK: I believe river level is 740, if I recall correctly, and that is right here. So your basemat is beneath the river level. Is that correct?

MR. PALM: Yes. We ran several borings, most in the order of 200 feet below grade and grade about 800 in the plant area.

MR. SHEWMON: Where is that relative to the water level in the reservoir?

MR. PALM: About 140 feet below.

MR. SHEWMON: All the borings and nice and dry indicating there is no groundwater transportation?

MS. NIEMCZYK: That is right. They have done all these indicated borings that they have taken, and when they look at the permeabilities of the rock, the rock is very impermeable.

MR. PALM: That was one of the criteria we had insofar as locating the foundation level for the nuclear island was to get essentially into a homogeneous rock below clay lenses and cracks and so forth.

MS. NIEMCZYK: In other words, you have what looks naturally like a very good site. You might decide that it didn't look good enough. You might decide that you were worried about your leak pathways, you might decide that there was great public pressure and you wanted to do something about it. Well, what could you do about it? And here by "temporary" I mean what could you do on a hundred year basis. because presumably what you would want to do is go in and temporarily isolate the melt debris and then ultimately you would want to remove it to a repository. But what could you do on a sort-term basis? Well, you could put in some dewatering wells around the site and you could pump the water out and lower the watertable. Because you are below the river that is not all that good an idea. I am assuming if you stop pumping your water level would rise again, and also in your very impermeable rock that is difficult to do. You could inject water, and again that isn't a very good idea. You could say, okay we will just let the groundwater get contaminated, there is not that much of it and we will pump it out it is contaminated and purify it, but that just displaces the problem and then you would have to worry about all the radio activity taken out and the contaminated resins or whatever.

You might go in and freeze the ground and that is obviously very temporary and very expensive. You might put in a slurry trench and dig down to an impermeable layer and fill it up with concrete to stop the flow of groundwater. That is really just not all that good a solution because quite often you can get seepage under that.

Probably the best solution is putting in some kind of an isolating grouting and I will describe that a little bit more.

MR. ZUDANS: What would water injection do?

MS. NIEMCZYK: You can divert the flow of water quite often. It does cite it wouldn't buy you much at all. It takes a tremendous amount of water. Essentially what you would be trying to do is to keep the water from coming down the hill in this case.

MR. ZUDANS: And the ground freezing, I guess you would go away from the heat source and maybe there is a hope to freeze.

MR. NIEMCZYK: Where ground freezing would be good is if you had a site that had pretty permeable soil and you were just worried about early releases and you could very quickly go in and freeze the ground and sort of isolate it.

MR. ZUDANS: Can you imagine doing anything very quickly on that size?

MR. NIEMCZYK: The advantage of the Clinch River site is you don't have to do anything for a long time. It is naturally a good site. If you were to plan a reactor on the basis of the releases to the hydrosphere, what you would do is you would plan to put your reactor on an impermeable layer.

MR. ZUDANS: So these are the interdictive measures that you mentioned before which you promised to tell me more about.

MS. NIEMCZYK: If for some reason the site isn't good enough, then what else could you do?

MR. ZUDANS: I am not very well sold on any of these.

MS NIEMCZYK: The last one is the only one I would recommend and that is a grout curtain.

MR. ZUDQNS: I thought you meant the No. 7.

(Laughter.)

MS. NIEMCZYK: What I note is that any time you are dealing with a subsurface is that you are dealing with a very uncertain situation and you have to have an extensive monitoring program on the site because you have always got a potential for leakage or some kind of escape path.

This is a very idealized melt debris sitting under your plant. The idea is to go in and completely isolate the thing with a grout curtain. Obviously you would have to do it farther out than what is indicated in this figure because of thermal problems. You can have serious thermal degradation in your grouting and that wouldn't really help you. So you would have to be out far enough that you wouldn't have that.

What kind of an effort would be required? What kind of a time scale and cost? Well, first of all, you have to convene your experts and really formulate a solid plan to do it.

Then if there have been any releases to the atmosphere, you have a contaminated site and you have to get in and clean up your site. The you have to determine the subsurface conditions. At Clinch River, you already know what those are. Especially at some of the old light water reactors you don't always know well what is under there. You would have to go in there and first determine what was there before you could plan a mode of operation.

Now you might decide to wipe out a couple of miscellaneous buildings at your station because they just get in the way of your grouting platforms and drilling equipment. Then you would have to assemble your drills and your teams and your grout. You would require very experienced personnel to do it because you would be slant drilling. It would require very careful preparation of your grout. It would be a very sophisticated operation.

Once you had all this done, you would try to completely isolate your station.

MR. SHEWMON: What material have you selected for the grout that has superior properties to the rock that is already there?

MS. NIEMCZYK: It would depend on whether you wanted to do extensive cracking of the rock that was there. If you went in and took an option of extensive fracturing then you could go with a cement or bentonite grout.

If however, you decided to grout in the situation that is there, about the only thing you could use is some kind of chemical grout because of the low permeability of the rock or low porosity.

MR. ZUDANS: It just doesn't make any sense because if you took care of concrete and took care of silt what do you have to put in there?

MS. NIEMCZYK: I beg your pardon?

MR. ZUDANS: I mean it already went through the concrete and it is going through the silt.

MS. NIEMCZYK: It goesn't keep going forever.

MR. ZUDANS: I understand that, but this idea of saying that you can inject something or grout something, it sounds kind of childish.

MS. NIEMCZYK: Well, you can always eventually grout. It is not a rapid procedure. At some sites it would be your only viable procedure. There isn't really a problem, but you are doing it in addition to it if there is enough public concern. What you would initially do is form a group curtain that would encircle the whole plant and initially what you would want to do is form a single group barrier.

MR.SHEWMON: That has to be one continuous barrier. When you showed your cone, you would have to generate the surface of that cone underneath the entire mess.

MS. NIEMCZYK: That is right. Obviously you want to ensure the grouting conditions. What we were told you need to do is to drill holes about every five feet around the perimeter.

MR. SHEWMON: Well, holes every five feet won't do. It has got to be a continuous wall, the surface of a cone, otherwise I will get past any columns I put in and go in between those columns.

MR. BUSH: You are drilling at an angle though. So five feet at the surface doesn't mean anything down below.

MR. SHEWMON: Oh, I see what you are saying.

MS. NIEMDZYK: The preferred procedure that we were given was to go in and actually go down, if your rock isn't fractured enough, you go down and actually fracture it so you would have a complete penetration.

What you do in an idea situation is first you form an initial grout curtain. You form your single cone. Then you go around and form another cone. Then you go back between those two cones and put in another even dense layer. We are assured that probably within three layers you could do it but it is just the time and effort required to do it very carefully, which obviously would take some effort. It is a difficult site to do the grouting. It would probably take at least two years to perform the job, but then you have that time because of your vapor shield and you will probably have a lot more time than that because you are stilling on silt stone.

It is really questionable whether you even need to do it because of the silt stone under your basemat.

MR. MARK: It is really scarsely even questionable. You are going to do all this, and by these estimates and of course I don't believe them at all, to avoid 10 cubed man-rem. You are allowed to spend up to a million to do that and you can't even draw the blueprints for the grout for less than that.

MS. NIEMCZYK: Well that is the thing. Most of those doses that population dose, this is primarily a very low dose to a very large number of people. These are doses that you normally wouldn't get in and do anything about.

MR. SHEWMON: She prefaced this by saying of popular demand and said just don't stand there, do something and this would be something, as I understood her.

MR. MARK: Yes, you could clear the dump.

(Laughter.)

MR. SHEWMON: That would guarantee you a few votes.

(Laughter.)

MS. NIEMCZYK: You can't say that we do what is necessarily logical.

As far as the permanent solution, what would you do after your first hundred years? Well, you could reinforce the grouting, but that is probably not all that good an idea. You are surrounded by limestone layers. The layers currently don't have solutioning, but eventually most limestone layers do get solutioning. So if you are talking about tens or hundreds or thousands of years, eventually you get solutioning around your grouting cavity.

You could also get settling of the ground because your groundwater table is going to change over a period of time. Therefore they will be put a stress on your grouting, and the grouting in this reinforcement could ultimately deteriorate. So you will probably have to do something about it.

What you would have to do is then move the debris to a permanent repository. It is mostly not any technical problems that prevent you from doing something about it after it has cooled down sufficiently, but it is mostly legal problems.

The main thing you would have to worry about is preventing any egress of the debris to the environment as you were doing it.

The good features of the Clinch River plant and site as far as releasees to the hydrosphere is that the building would have a relatively thick basemat. Also, the core is relatively small. So for both points melt-through would probably not be likely.

In addition, you would have relatively small amounts of volatile radionuclides being released to the hydrosphere. You would have no significant releases of highly contaminated water which are the hard ones to control.

If basemat melt-through occurred the melt would reside in siltstone and therefore there would only limited water available for leaching.

The bedrock beneath the plant would be relatively undisturbed by construction. Therefore, again you would have only limited water avaialable for leaching. You have to get the material out and into the water before it can go anywhere and this site really doesn't permit that.

If everything I have said is incorrect up to this point, we still have the fact that the site is a long way from the river. 1600 is a long way compared to most LWR sites. Therefore if your initial efforts to contain the radioactivity, you would have time to do something else.

In summary, if the basemat melt-through and no interdictive actions are taken, it is estimated that the resulting consequences to the public will be relatively small.

If it is decided that mitigating measures are indicated, then it should be possible at the Clinch River site to almost completely prevent any radiation dose to the public by appropriate source interdictive procedures.

MR. SHEWMON: Is that all?

MS. NIEMCZYK: Yes.

MR. SHEWMON: Thank you very much.

The Chairman said we had earned a 10-minute break. So why don't we take one.

(Whereupon, a short recess was taken.)

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MR. CARBON: Let's go ahead, Mr. King.

MR. KING: My name is Tom King, I'm with the staff, and I'm going to summarize the results of the staff review on local failures, and in particular, how local failures affect failure propagation.

First slide really just gives you an idea of the scope of the staff review, which really paralleled the applicant's work. There was a review of experimental and analytical data on local failures and blockages; that included those things for fuel assemblies, blanket assemblies and control assemblies.

We did a review of the detection systems primarily from the standpoint of detection that would be acceptable for terminating failure propagation.

We looked at the work that was still planned, both analytical and experimental, in this area, and we had quite a bit of assistance from Los Alamos National Laboratory in this area. They were our primary reviewer and consultant.

The first thing I want to emphasize is we think the potential for failure propagation is very small. This viewgraph just lists some of the major features which we feel help prevent failure propagation. The fact that we used ducted assemblies that have about a 120 mil thick hex can around the pin bundle which tends to retard any propagation from one assembly to another. There's flow blockage prevention devices andthe zion gas entrainment prevention devices, features to prevent misloading errors, extensive QA and inspection during fuel blanket assembly control, assembly fabrication. And there's instrumentation. Delayed neutron detection, fission gas detection, core exit thermocouple systems in the plant to look for effects of local disturbances.

Types of failure or causes of failure that were looked at were stochiastic pin failure -- two different defects or welds or cladding. Failures due to insufficient heat transfer, due to low flow or excess power.

MR. SHEWMON: Sir, this has been a problem for longer than many of us have been around. Is there any -- or a hypothetical problem. Is there any evidence that it can occur or under what conditions might it occur?

MR. KING: I'm going to get into that in a little bit.

MR. SHEWMON: As quickly as possible.

MR. KING: To give a little more breakdown on the types of effects that were evaluated, fission gas releases within several different scenarios , follow that. Fuel release, excess power events,low flow events, changes in heat transfer, fuel performance, and fuel performance after a cladding breach, and I won't talk about each of these. The main thing I wanted to show you was I put an asterisk next tc those events where there was not only analytical data but experimental data to back up the conclusions that were reached for each of these. And as you can see, there's quite a few asterisks that show up on here since there's quite a bit of experimental support to back up the conclusions of the analysis.

MR. LIPINSKI: That must pertain to individual pins as opposed to the whole core.

MR. KING: Yes, individual pins. Local events.

MR. SHEWMON: But presumably, he's still talking about propagation.

MR.KING: The intent is to keep local events from becoming whole core events.

The conclusions that we reached from our review are, one, there's been no observed fuel failure propagation in any operating LMFR and that includes the United States and foreign plants. Most of the failures have been small pinhole cladding breaches that release fission gas. All analysis and experimental data indicate that fission gas releases, small local blockages do not lead to additional pin failures. It takes large flow blockages or expulsion of molten fuel to cause additional pin failures. The time required for additional pin failures to occur, at the earliest is on the order of minutes, and depending on the event can extend out into hours.

Fuel will be exposed to flowing sodium in a situation that leads to additional pin failures.

MR. MARK: Excuse me, could you interpret that for me? Do you mean that if there are additional pin failures, that things will still be cool because the sodium is flowing? Or the flowing sodium causes the additional pin failures?

MR. KING: What I meant by that is that it takes an event that causes either the injection of molten fuel or flow blockage severe enough where you have cladding melting and physical motion of fuel to affect the pins around it and to cause additional failures. In which case you will have fuel exposed to flowing sodium.

MR. MARK: Okay. So that may lead to some processes like fuel cladding interaction and dissolving and stuff.

MR. KING: Right. Which has been -- part of the evaluation is one of the things they looked at was fuel cooling interaction.

MR. SHEWMON: One could also conclude that it would quench the fuel and it wouldn't travel so far.

MR. KING. For small amounts of fuel released the tendency has been the coolant sweeps it right out of the core.

In looking at detection needs, we concluded that we need to absolutely do a reasonable job of saying we can detect and prevent failure propagation, covering the range of types of things that can occur. We need a fast-acting system to detect it.

And we've looked at the core exit thermocouples, core exit flow meters and the DND systems, and we've concluded that the DND system has the highest probability of giving us that detection capability because it will detect fuel exposed to flowing sodium, which we've concluded is the condition required to have propagation.

MR. LIPINSKI: Is it a fast-acting system or is fast enough on the basis of the timescale in which it takes place?

MR. KING: In Clinch River, it's in the order of about a minute to get a signal on the DND, and we feel that that's fast enough. Whether that needs to be tied into the plant protection system or not is something we're going to evaluate at the OL stage and the next slide will get into this.

Basically, our bottom line is that in installing the DND system, we don't want to preclude being able to tie that into the plant protection system, the scram system, at this stage. Whether that needs to be done or not will be part of the OL review.

There's additional data or additional testing still going on in this area, evaluation. We feel that we would like to have that completed. I havelisted the three items that we feel fall in this category: completion of the run beyond cladding breach program. This country has not generated much data on the performance of LMFBR fuel after there's a cladding breach, and this program at EBR-2 is underway to do that and we feel completion of that program is a requirement.

Completion of the confirmatory FFTF testing of the assemblies that are prototypical of Clinch River. That includes assemblies up to cladding breach. And completion of the examination and evaluation of the P-4 test which was a test run in the SLSF facility in Idaho that actually injected molten fuel into a -- I think it was a 37-pin bundle for any worst case site conditions, looking for propagation. And that test is under examination right now.

And the summary of our position at this stage is the DND system should be installed so as not to preclude connection to the scram system. A final decision on the need for that connection will be made as part of the OL review. At this stage, our position is if there's failed fuel, if there's a fuel failure in the plant, it's detected, that should be removed at the next shutdown, whether that's planned or unplanned shutdown. And if the DND signals sees a pre-determined level, that will be a special shutdown to take that out.

And a complete P-4 tech examination, a rundown cladding breach, the FFTF testing programs and consider it's acceptable for a CP with the above conditions met.

That concludes what I wanted to say.

MR. CARBON: Any questions?

MR. LIPINSKI: In connection with the RSS, have you concluded whether it has to be both primary and secondary, or are you not considering that at this point?

MR. KING: We haven't considered that. We haven't

MR. ZUDANS: Mr. Chairman, I have one residual question from the previous Westinghouse presentation related to this load-carrying capability of the top head, and I'd like to ask it and maybe the answer could come later.

This is the question. The head and rotating blocks, from what we were shown, they form non-uniformly around the circumference. As a consequence of that, the shear teeths that transfer that load to the vessel flange will be loaded non-uniformly. And I'm wondering whether the non-uniformity of the shear key loads has been factored into the load capacity that was associated with these shear teeth.

That's the question, and the answer can come later.

MR. CARBON: Unfortunately,the individual that answers the questions has now left, he's driving back to Pennsylvania so we will have to get that answer to you next time.

MR. LIPINSKI: This is a residual question from that basemat issue. We've talked about instrumentation following the course of an accident. Has anybody considered laying thermocouples underneath that basemat before it's poured?

MR. SCHWALLIE: If I could answer that, the answer is no.

MR. CARBON: Move on, Mr. Schwallie.

MR. SCHWALLIE: My name is Ambrose Schwallie from Westinghouse. The next series of presentations that you're going to hear will be an attempt to summarize the technical information that exists today on local faults. To get to that end, what we'll do is provide a description of the fuel design so that we have a terminology that's consistent among us so that the follow-on presentations are on a common base. We'll review for you the pertinent data base relative to local faults, conclusions on fuel failure propagation and the potential for it. The kinds of local faults that we'll be talking about basically are operation with breached fuel, the breach itself and follow-on operation after that time, and blockage potential, both inlet blockage as well as incore blockage.

We'll describe to you and give you some idea of the sensitivities and what we can see with the detection of the locations systems that are built into the facility. And this area, as Mr. King described, is the cover gas monitoring system, failed fuel location system, the delayed neutron monitoring system, and we also have the capability to take cover gas samples as well as sodium samples. We'll describe to you the intended use and utility of the core outlet thermocouples and what their sensitivity and intended purpose for incorporation into the plant is, and then we'll close off with a discussion of the foreign and domestic experience on PPS instrumentation and the usage of that relevant to local faults.

MR. SHEWMON: In view of the fact that we're now an hour behind in schedule and that the committee isn't too shy here, would you tend to skip over things, and if we have questions we'll ask them.

MR. SCHWALLIE: Okay. What I'll do, then, is basically state our summary on technical and local faults, and this is where we want to get to, to this understanding at the end of the day. The position is that rapid rod drop failure propagation due to local faults leading to a cause of loss of coolant geometry is incredible. What I mean by incredible is there's no realistic mechanisms envisioned to do that on a rapid basis. And it's not occurred in any of the experience to date.

WE do have instrumentation to alert the operator to local fault conditions, and considering the long time intervals necessary for any postulated propagation, we can provide the operator with sufficient warning well in advance to take any kinds of corrective actions necessary.

So since rapid rod-drop propagation is incredible, we do not believe that the requirements for local fault detection and protection against any kinds of propagation is considered necessary for incorporation into the PPS.

Most of you I think are familiar with the design and I'll save some time here. All I wanted to do was to get some terminology straight between us. The presentation following me by Mr. Markley will discuss the potential for this area of the fuel assembly to undergo a blockage situation, from the lower inlet nozzle up through and including into the rod bundle itself. Mr. Tilbrook will talk about incore blocking, this heat generating type blockage. Just to remind you, we do have a discriminator to protect against sub-assembly misplacement in the core. We have one enrichment zone so we do not have an over-power situation in the core. We do have protection on the top end of the assembly to preclude any kind of blockage of a misplaced assembly coming into the core through this feature.

The thing that might be of concern is blocking from the lower end up. In terms of the fuel rod itself, we'll talk to you today about breaches, cladding breaches. The rod, as you're familiar, has, from the bottom up, 64 inches of pellets, the active fuel region surrounded by depleted uranium oxide, and then a fission gas plenum on the top.

Now, we'll talk about incore sodium contact breaches. That means a breach in the cladding next to the active fuel, where a fission gas leak could be from anywhere up in this region of the pin; not adjacent to fuel, but could be a region whereby sodium could ingress into the pin and go down next to the fuel.

Blanket assemblies lock just like fuel assemblies and all the pertinent features relative to blockage and local fault considerations. Same kind of inlet region, orifice plate design up to the rod bundle; both the fuel and blanket assemblies use a wire wrap to space the fuel pins in the bundlet relative to each other, which is a fantastic design feature for providing swirling, mixing and cross-flow between pins relative to any kind of blockage situation. You can see that wire surrounds and wraps up around the pin.

MR.LIPINSKI: On that gas capsule, is that gas open to the full plenum, or is it contained in the capsule?

MR. SCHWALLIE: Yes. What happens is after we weld the pin shut and it's hermetically sealed, we rupture a diaphragm with this penetrator and the tag gas permeates itself throughout the pin so it's available wherever the breach would occur.

MR. LIPINSKI: That would occur at construction.

MR. SCHWALLIE: That's right.

MR. SHEWMON: You have one set of isotopes per subassembly?

MR. SCHWALLIE: That's right. Each assembly has a unique xenon-crypton mixture.

The only other point Iwant to leave you with at this point is the design philosophy kind of thing which is kind of important because it has some connotation to the kinds of numbers and how many you expect to have.

The bottom line of this viewgraph is that all the way through normal, anticipated and unlikely events adesign basis for the plant is that we've established design limits that preclude mechanistic age-old failures from happening. And that's through normal anticipated events and the worst unlikely event including uncertainties. Such that we're trying to do the job from the standpoint that what failures we have will probably be stochiastic in nature, and of the infant mortality type. And LWR experience, if we can get to be that good, indicates that that's about 01 to 02 percent. The number of rods and thrusts. That's in the range of 5 to 10. So we don't expect a large number of failures to have to deal with.

Now, unless you have some specific questions of me at this time, I would close at this point to save time.

MR. CARBON: Fine, thank you.

MR. SCHWALLIE: Mr. Markley will now talk to you about in-core blockages.

Tp 4 starts

MR. MARKLEY: I'm going to be discussing the CRBR inlet blockage consideration. My name is Bob Markley.

These are the two viewgraphs that summarized my last presentation in February, just to bring you to that speed again. The summary inlet in-module blockages are highly improbable because of the built in-flow blockage prevention features which provide much flow path redundancy. And let me just review those.

First of all, we have multiple primary ports; six in each of the inlet modules, which would be certainly very difficult to block. Further, we have radial and axial debris barriers which would prevent any large object getting up into this region (indicating). And then we have the auxiliary flow ports where flow can also enter the modules. It seems just impossible to block any of that region.

We have a strainer that strains all the flow that enters all these holes, and a wide open area for flow and also, multiple slots where the flow enters all the core assemblies.

MR. SHEWMON: What's the shape of your strainers?

MR. MARKLEY: They are quarter inch diameter holes.

MR.SHEWMON: What is the shape? Where are they?

MR. MARKLEY: The shape is a sleeve on the inside here attached to the module and the flow --

MR. SHEWMON: So it's a cylinder, not a flat filter.

MR. MARKLEY: It's a cylinder. The strainer prevents debris larger than a quarter inch from entering the modules. The orifice plates provide another level of screening up above in the unlikely event that anything would even be in there. And debris not strained by the fuel rod support keys and the unheated rod bundle entrance will pass through the rod bundle, and my next viewgraph will elaborate on that.

And again, even 50 percent aerial blockage is here causes very small decreases in temperature. So we have a lot of margin in these areas.

Now let me go up into the rod bundle inlet region. This is a picture of that region. This is the bottom of the rod bundle, the flow enters from below. These are the attachment railes; they are attached to these support bars which are attached to our shield box. So the flow is entering in this direction, and let me just go through the rationale on the screening effect of this. First of all, there's certainly a very low probability of having objects in the system. We have a QA program comparable to the FFTF program which was very successful. They had very little debris in that system.

We have core support -- core special assemblies during pre-operational testing which will filter anything out of the system greater than 4 mils.

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MR. MARKLEY: And in the unlikely event that you would have particles in this particular matter from all the evidence we have seen, we would expect very slow build-up. In your test of path entrainment, and even if you put a large amount of particles into this system, it's widely distributed. We never saw a preferential type of flow that would just go to one assembly. It's very widely distributed in all of those tests. We saw that in all cases and, of course, particles of concern would have to be exactly in this size range and at this entrance region.

There are more than 400 interconnected flow channels. What I mean by that, all these channels are interconnected and you would get very rapid flow recovery even behind any kind of blockage that you could conceive.

In addition, the margins to accommodate blockages are large. We have, as I mentioned, redundant flow paths in the subchannels. They are all connected, and there is a large amount of cross flow between any of these subchannels.

For instance, in one foot in the fuel assembly, you have about 200 percent cross flow vs. your axial flow, very large amounts of cross flow. There is a lot of flow coming in and out of these channels. That's based on the tests that we ran, air flow tests, studied those in great detail.

MR. CARBON: What does that lead to? 200 percent velocity or quantity?

MR. MARKLEY: If you take the average axial flow, mass flow, the amount of cross flow in and out of that channel it is 200 percent of that.

In addition to that, there is also a cascade strainer effect here again in the event that you would get something, anything larger than about 150 mills would stop right here at the leading edge of these rails. Something between 150 and 100 mills might get into where the rods start, and then further anything smaller than that, about 56 mills might get a few inches up here, but again that's a foot below any of the heated portion of the rod, so there is again sufficient time for any flow recovery. It's just hard to concede that you would take tremendous amounts of particles to cause a flow reduction, and that's again what I am saying here. Actually because of some of this cascading straining effect, it would take even larger than a 50 percent blockage to get that kind of a temperature increase.

MR. MARK: That 56 mills is magic because anything smaller just goes on through.

Does that back up the picture you are describing here?

MR. MARKLEY: Yes.

FFTF is almost identical to our fuel bundle.

There's a little bit of difference in length.

MR. MARK: I don't know how long it's run, but there is no blockage experience.

MR. MARKLEY: No, in fact, if their CSA, if they did run, showed very little particulate matter. They have a little few particles, but not very much.

MR. ZUDANS: When you made this presentation last time, I had some problems believing that you didn't have accumulation potential right where the entire wiring begins.

Now, I understand last time you explained it, you would need tremendous amounts of particles, and you simply didn't have them.

MR. MARKLEY: That's correct. And further, all of those channels are interconnected and highly interconnected.

MR. ZUDANS: I'm saying they are connecting about that point. If you would block the entrance, you could either connect them a foot higher or --

MR. MARKLEY: Yes. It's interconnected to full length.

MR. ZUDANS: Is that only within a single channel? There is no interconnection between adjacent channels?

MR. MARKLEY: Yes. It's just within one of these 217 rods and also the 400 subchannels. They are connected with a large communication of flow between them.

MR. ZUDANS: What we see here is --

MR. MARKLEY: This is the bottom of the inlet to the rod bundle and one subassembly.

MR. ZUDANS: And this is in case of a duct?

MR. MARKLEY: Right. The duct is round in circumference.

MR. ZUDANS: If you blocked off this one, there is no communication?

MR. MARKLEY: There is no communication between this assembly and the adjacent one. They are closed ducts.

MR. DICKSON: However, if I could refer you back to where those were all initially fed, there is significant interconnection in all of these, such that anything larger than the quarter of an inch that would be screened out and did block one hole, would not block a subassembly.

MR. ZUDANS: Well, I understand that question. The only thing is if I blocked it beyond those rails, then there is no recourse completely. I would have to have lots of particles to block this beyond the rails, because your smallest cross section begins where the pins begin.

MR. MARKLEY: Remember, we put assemblies in here, our core special assemblies that filter the whole system down to like 4 mills before we start --

MR. ZUDANS: Yes, I think you are convincing.

The only thing -- something still lingers in my mind in that. I don't like the place where they got stopped. It is my feeling they should have been stopped before, but that's okay.

MR. CARBON: Let's move on to Mr. Tilbrook.

MR. TILBROOK: My name is Roger Tilbrook, from the Westinghouse Advanced Reactors Division.

Some of what I am going to tell you, you have seen before, so I'll move through as quickly as I can, and tell me if there is anything you want to discuss, if I left some of the details out.

Basically I am going to cover what is in 15.4 in the failure event. A failure which is initiated within a single fuel assembly within a bundle. The details of design and the filtering process was addressed earlier.

I am going to look at what happens in the bundle. Stoichastic rod failure postulated local blockage in the assembly, a bubble going through the core, and finally because it's addressed in the PSAR, molten fuel consequences.

This here is stoichastic rod failures.

(Slide.)

I'm going to touch very quickly on fission gas release, fuel particle release, and operation with failed fuel.

Fission gas release. We consider several effects.

The initial thermal effect of the gas jet impinging on a pin. We look at rod gas blanketing, which is a more microscopic effect, and we address transient mechanical loading within the PSAR.

This first one was addressed very extensively by a series of tests at the Argonne National Lab, and blanketed all the core design conditions significantly on either side, and we apply those to conditions within the core on a 3 sigma pin, and we find that we can reach, if you assume a steady state jet of the worst type of 1600 degrees Fahrenheit.

On the other hand, we find the blowdown is relatively now out of the fuel pin, if it's in a plenum in which there is no thermal effect on the neighboring pin, because there is no heat generated, and if it's being released, there is two orders of magnitude of levels on which you would see any significant thermal consequences.

7-B

So this is not a problem within the scope that we are looking at. With the thermal blanketing effect, we find that this is postulated within the PSAR. The condition we fail, on 217 pins, and we find that we get perhaps 150 degree temperature increase maximum for 3 sigma, 115 percent of a power pin.

On the other hand, if we consider a single rod failing in the core, the consequences are negligible. The gas is dispersed throughout the assembly, shown by tests, and they are of no consequence within the bundle at all.

In terms of mechanical loading on the ducts -- well, we look at the mechanical loading on the rod and the ducts. In terms of the rod, we find that we have a very high sonic velocity in sodium, and you cannot sustain a pressure if there is a cross pin sufficient to cause damage. If you look at a duct, we find the duct pressures are limited for analysis and experiment to be about 30 percent of the fission gas pressure. These conditions give us about 200 psi maximum local pressure, and this is well within the range that we have as the capability within the bundle.

This was addressed as part of the OECD international agreement, and the reports, CSNI report 40, and this basically says there is no past, no record propagation, and this was agreed to in the ACRS Subcommittee on Advanced Reactors report that is being reviewed.

There is also some experience in this area, as much as we have never seen fission gas related propagation that I mentioned, and experienced.

As we consider particle release within a bundle, operating experience with failed fuel indicates little or no washout from fuel pins.

Gregory in the United Kingdom issued a paper. Leon discussed this in some detail and said particle release ar8

is very rare, and there is no evidence that is caused by slow consequences at all.

He also indicated that it takes place over a considerable period of time, and the particle sizes, 45 percent of them are below about 10 mills, and 75 percent are less than 40 mills, which is well within the range addressed earlier for things being swept through the bundle.

7-C

Heat generating blockages associated with assumed particle retention in the bundle are discussed later.

However, in the mode they are postulated, because the tests performed at UCLA that I mentioned last time, wouldn't show any evidence of blockage generation at all. Operation with failed fuel. We find that you can look at fission product leaking into the system. This is cleaned up by the sodium clean-up system and does not represent a safety hazard to the reactor. If you consider how you get sodium ingress into the pins such as occurred during power cycling, the pressure that may be postulated, if you get vaporization of sodium, it's relieved through the initiating breach.

GE did a test where they shot a fuel pin which actually had a plug of sodium on the fuel centerline without any consequences at all.

Finally, you can look at fuel-sodium chemical reaction. This was addressed a little earlier, and the next line addressed that.

In terms of operating with failed fuel, let's consider what experience around the world has been, and if you look at the molten test, they had grossly destructed fuel.

MR. CARBON: What test?

MR. TILBROOK: A series of tests done by the Germans, Belgians, and Dutch, and the PR-2 reactor. There is a series of five. The first two had stainless blockages around the fuel. It was operated in one case for 48 minutes without any effect.

After the test had been done, they used the fuel blockage, they ran for five days without any consequences to the system at all, and that's a bounding type consideration for failed fuel operation.

The fuel-sodium, the reaction is part of the concern that we have with local fault and why we take particular interest in it. It is different. One of the main characteristics with light water reactors, you can't get a uranium-plutonium form in the presence of free oxygen. You can get some fuel swelling through the breach. Under the worst case conditions at the end of life, where the oxygen potential in the fuel is maximized by the deficiency during life, and you assume all the free oxygen is available at the one breach site.

We still only have 1.7 percent expansion, which has negligible effect on the flow, the rest of the circuit, and the heat transfer in the rest of the bundle.

Kinetic data indicates about two to six days to reach equilibrium, which is a relatively slow process. This is based on stabilization of the DN signals in which they fail fuel.

MR. SHEWMON: Are you saying if all the excess oxygen reacted, there would only be enough uranium to give that much expansion, or what?

MR. TILBROOK: Yes, if you assume all the free oxygen goes to that one site, that is the assumption.

Experience indicates sodium-fuel reaction does not lead to failure propagation. There have been several tests, DFR, Rapsodie, and several other reactors, where they run with no fuel and even 100 days, and there is no consequence in the fuel bundle at all by way of propagation.

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MR. LIPINSKI: Before you take that off, on the sodium, MO_4, what does that mean?

MR. TILBROOK: It stands for uranium. It is used for the heavy elements.

MR. MARK: Well, the temperatures that are applied in the DFR, Rapsodie, are all the same temperatures?

MR. TILBROOK: They are all in the same ballpark. They may be slightly cooler in some cases, but they are all basically in the same regions.

MR. MARK: I imagine what you might ever go on could be sharply dependent on temperature.

MR. TILBROOK: That's probably true.

The next item is local flow blockages within the bundle. These were addressed at the last meeting when we talked about this back in February. All I will do is put a summary sheet up here. The six-channel in-core passive planar blockage, based on the experiments that were run at Oak Ridge, indicate that the worst consequences will be a reduction of fuel lifetime.

The formation of heat-generating blockages, which could occur by fuel being relocated within a fuel bundle, we would know about that during the relocation due to -- first, we know we would have a failed fuel pin from the cover gas monitors, and then we would be able to track any expansion in the breach size with the DND system, so we are not going to create a heat-generating blockage that we don't know about and that we cannot monitor to maintain within the criteria that is to be established for the plant operation.

The DND system is capable of detecting HGB smaller than that which could propagate damage.

MR. MARK: I think the last speaker showed us pictures of the fuel pins with a mysterious gas tag capsule.

MR. TILBROOK: Yes.

MR. MARK: Now, does that not belong on this page that we are looking at as another thing you could be watching for?

MR. TILBROOK: No. The gas tag capsule is inside the fuel pin envelope. There is one per fuel pin and it is within the envelope of the fission gas --

MR. MARK: But you have got here -- it just tells you that you have got something to look at.

MR. TILBROOK: No, I understand the gas tag is just a location device. You don't monitor the gas tags continuously. You monitor the cover gas constantly, and if you can detect a failure, then you will dip into the gas tag for a sample for the gas tag system to identify the failure location.

MR. MARK: Now, I wanted a word or two about that gas tagging. It is a mixture of what? Is it gases or isotopes?

MR. TILBROOK: Gases. I am not the best person to j-3

End 7D

talk about the gas system.

This is one of the potential causes of failure within the fuel bundle, the small bubbles passing through the core. It has been taken within the system to design so that potential gas pockets accumulate gas in a significant volume that could go through and get into the core, and back in February we demonstrated how bubbles were -- any large bubbles were broken up and dispersed, so there is no chance of getting a large bubble, for example, of this type as being postulated here.

Part of Section 15.2, the reactivity -- as a consequence of it, there are some heat transfers, along with reactivity preservation, something of this size, and we get 68 degrees Fahrenheit.

The reactivity consequences, the small bubbles, are negligible, a 5-inch bubble going through a subassembly is worth, the maximum location, around about 1 cent or something of that order.

So the preservation is due to negligible and undersized, is 25 degrees Fahrenheit maximum increase in temperature of the exit of these conditions, and we see no propagative consequences due to bubbles in the system.

I'm going to go through a very quick summary of molten fuel issues. In the PSAR they are addressed as potential hypothetical postulated consequences and how a lot of research has gone on to address the issues. We know from our analysis that molten fuel will not occur. They occur during a molten event, but not during any other, and in these circumstances is a scram transient.

Molten fuel is not formed behind passive planar blockages that could be postulated. Boiling should produce clad failure, if you postulate a large enough blockage for it, a sufficient size, but it won't give molten fuel. Molten fuel just isn't formed in heat generating blocks which are nondetectable. The sensitivity of the DN system will be addressed later, about a factor of 3, smaller than the smallest blockage that we have identified that could potentially cause 1600 degree Fahrenheit on the cladding.

The D-1 and D-2 tests show transient operation with molten fuel. There is about 79 milliseconds. There are no consequences in these. However, P-1, they generated molten fuel in there and operated with it for the equivalent of about a day. It was not all in one operation. They melted and remelted the casting and remelted it, and there was no consequences out of that, either, and went on and performed the test as originally planned.

Worldwide experience with liquid metal oxide fuel plants is extensive and all the evidence supports the facts, that there are no consequences from fuel failure. We have a quick review of what we know of some of the failures around the world. Many of these plants, whether identified like Phenix, PFR, where these are operating plants, they are also being used as a radiation type facility as well. Many of these failures are in precisely these types of assemblies where you are testing a new design, pushing a design harder, when this failure here, FFTF, was precisely one of those types of situations with its advanced design for blanket pins, and this is what failed in its early life.

end 7-E

In some cases this large number here on DFRs is because of a downflow plant and you don't get the normal gravity of the bubbles, so that is not really representative of situations we are looking at. Several of these other areas like Rapsodie, most of those failures are way out beyond the design conditions that you would expect in our plant and are representative, again, of fuel design prototype tests, new design proof tests and things of that nature.

In no case is this propagation being seen from one pin to another, so we conclude for local faults that local failure rates in normal driver fuel, stochastic fuel rod failures doesn't lead to failure propagation. Failed fuel is detectable first by the cover gas. You can then locate it and you can monitor it, progress, if you choose, to leave in the core after a DN signal has been established by the DN system.

Degradation processes are slow and can be monitored with removal at predetermined operating limits as appropriate, and they may change during plant design.

Finally, the consequences of failed fuel are benign. The processes are well controlled. They are so slow that EBR-II even took a DN system out that they had at one time. Even though they are a test reactor, they remove it from the system.

Any questions?

MR. CARBON: Yes, a general question. At one time there was considerable concern about possible rapid and pin-to-pin propagation. Are you saying that now it is essentially a complete consensus in the United States that that is just no longer?

MR. TILBROOK: I would say that is complete consensus among the international community. The only way you can get a rapid failure propagation is at time of failure, is what you are postulating, and that means it is associating with fission gas release and CSNI-40 Reports had a committee which included the Japanese, the English, the Germans, and the French, all in working on the report. They came to a consensus that fission gas release by itself is not, of course, a propagation.

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MR. CARBON: Fission gas release, but there are other potential initiator events, and my question was, insofar as the United States is concerned, is there a consensus that none of these potential initiating events will lead to rapid propagation?

MR. TILBROOK: Fission gas is your only rapid propagator that you have at this time.

MR. CARBON: You say fission gas release?

MR. TILBROOK: Yes, sir. That is the only thing that occurs when stoichastic pin failure happens, that is, release of fission gas of perhaps a particle, that is swept right out with it, and the only consequences in the neighboring pin by propagation could be the gas release effect.

MR. SHEWMON: And you are saying that the only possible one, and that doesn't seem to work that way?

MR. TILBROOK: There is a postulate for rapid propagation which is defined within the industry of something that occurs at the time of stoichastic pin failure, a pin failure itself, gas release, and that is the only thing that can occur at that time, and it isn't a propagative mode.

MR. LIPINSKI: Could I react to the question and leave the word "rapid" out?

MR. CARBON: I'm still somewhat unclear. You cite European agreements and so on, but I can also cite European publications where they don't agree, so that's where the source of my first question on consensus in the United States came from, and I guess you are saying --

MR. TILBROOK: In the United States there is consensus.

MR. CARBON: That there is no concern about the rapid propagation due to failure of the pin itself, whether it be fission gas or anything that --

MR. TILBROOK: At the time of failure, it is not a concern within the United States.

MR. CARBON: And then you are saying further that that same consensus holds internationally?

MR. TILBROOK: Yes.

MR. CARBON: I guess Item No. 6 here has to do with that discussion internationally. If that is so, I'll raise this question. I can wait, or I guess I still can go back to where I can cite data that not all people seem to be inagreement with that. I don't know whether to ask you.

MR. DICKSON: Roger, you did look into some of the reports that Jack Moore was paraphrasing.

MR. TILBROOK: We adjusted the reports in the paper.

MR. CARBON: That's what I'm referring to.

MR. TILBROOK: None of those reports are concerned with rapid propagation. The three reports from local failure that went to the rest of the Leo meeting also have supported that, because you address the French experience and the papers that you cite in that were cited in Jack Moore's paper. Gregory is very strongly of the opinion that those things are so robust you can almost do anything to them and you will will still survive.

The foreign paper, which is a source of figures with a branched table, that leads you through to some of that, the requirements are not concerned with rapid failure. They are concerned with postulated flow failure and sort of conditions that he is using as part of his argument or needs as part of his argument, way beyond the bounds of acceptance, as demonstrated by experience, and in the Gregory paper, which is also presented, addressed some of those concerns and indicates you'd have to have about three kilograms of free fuel in a relocated assembly to form a blockage sufficient to give you the sort of conditions that are alluded to, and there is no way you could get that sort of blockage without being well aware of its existence within the assembly from the DN detection.

MR. CARBON: Perhaps my final question is why did they feel they needed rapid-acting rapid propagation.

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MR. TILBROOK: I don't know. I can see no logical reason for them doing that. When I look at some of the other European information, like the French, I talked to them just yesterday, they stated to me that for Super PHENIX they would be quite happy to reduce their thermocouple to one-third coverage. They are looking for global effects. They are not looking for local.

MR. CARBON: That doesn't seem to tie together.

MR. SHEWMON: Do you have evidence that in the next generation the decisions that they are making now, that they will use thermocouples? Why don't we ask them next week?

MR. DICKSON: Perhaps we should defer this questioning to Lee Strawbridge when he gets back on?

MR. SHEWMON: You will recall, Max, that you in the paper went through a rationale. We don't see any reason why they name thermocouples. Nevertheless, we use them.

MR. CARBON: Yes. Pretty much so. But he also said that the British are doing the same thing.

MR. TILBROOK: I was talking to some of the people over there, and there are several variations in viewpoint even within the British organization. The closer you get to the people who were working on DFR and were involved in it -- were involved in the DFR internal tests, the more firmly you find people of the opinion that they aren't going to do you any because you don't need that type of response.

The French use thermal monitoring and they put them in the PPS because they happen to have them available

MR. CARBON: Next is number 40 again.

MR. DICKSON: Since we got your passouts out of order, I notice that the second talk follows his first talk just 11 pages into the passout.

MR. SCHWALLIE: Okay. At this point, then, what I would like to describe to you is the fuel failure monitoring system that we have in the facility, and basically I think to give you some indication of the extensive tests of the system, what we can see and when we can see it.

When we talk in terms of fuel failure monitoring, the cover gas monitoring system is a continuing operating system. Its intention is to detect, locate and characterize fuel and blanket rod failures and provide the operator with information about a failure, a situation that is in the core that needs some action to be taken.

He can turn on the fuel location system, which has a location system to identify which fuel or blanket assembly does have a breach in it, and then through cover gas analysis both the tag gas that in his opinion -- which is the 9-krypton gas mixture, 135,137-krypton, and emission products can characterize the extent.

MR. MARK: 9-krypton. So then having put in a tidy mixture, and you are going to have what, a couple hundred

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of these?

MR. SCHWALLIE: Different tags, yes.

MR. MARK: As many as you have assemblies?

MR. SCHWALLIE: That's right. 156 fuels and a couple hundred blankets.

MR. MARK: So different tags. 300. Now, you have fission gas injected into your well-selected gas mixtures, so that each of these will change, and they will change at different rates and you are going to have to be discriminating.

MR. SCHWALLIE: Right. The tag composition, the dilution effect first is time has to be dealt with.

MR. MARK: It sounds pretty hairy. You pull these out of the cover gas. It has argon and then you have to separate and you have what? Not a chemical way of distinguishing those gases.

MR. SCHWALLIE: Mass spectrometry.

MR. MARK: And does it take you less than a week?

MR. SCHWALLIE: It takes eight hours to collect a sample, refine it, trap it, get enough of it to make a positive identification.

MR. MARK: And you have confidence you can say, well, this is sample number -- this is tag number 311 and not 312 or 313?

MR. SCHWALLIE: That's right. And we can -- what we have got is we think we unreasonably -- well, the dilution

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effect versus time and the fission products that produce in the range of the tag. Now, clearly early in life that is not a problem. It gets into a problem later in life with the dilution of the krypton.

MR. MARK: The dilution of krypton?

MR. SCHWALLIE: Yes. The krypton.

MR. MARK: Yes. All right.

MR. SCHWALLIE: Now, tags are put in about 75 percent composition of xenon and 25 krypton. We are primarily looking for xenon, and the xenons that are produced from fission are pretty short decay, so 135,137 is pretty robust and it stays there for a long time.

Now, experience with the same type of tagging technique is sometimes pretty good. Sometimes for old failure we have had some concern finding, narrowing down the two to three assemblies once you get that amount of dilution. But in the range of 7, 8, 9 percent burnup we are looking at in Clinch River, I think we can distinguish the assemblies very well.

MR. MARK: So they aren't depleted by neutron flexions much?

MR. SCHWALLIE: No.

MR. MARK: Well, it sounds great. Fine.

MR. SCHWALLIE: I kind of looked at it the other way. Finding the early-in-life stuff, very early, one or two days of operation as opposed to old stuff.

MR. MARK: Thank you.

MR. SCHWALLIE: If you narrow it down to the probability of two to three, then you might pull some.

MR. MARK: Of course, if you don't narrow it down at all, you might pull them all.

MR. SCHWALLIE: Then the third system we have is the delayed neutron-monitoring system, and the primary function there is to look at precursors coming out of the fuel to give you some idea of the extent and characterization of in-core sodium content. Relative to your concerns about how long does it take the systems to work, the cover gas monitoring system, I am showing a range of time for detection of 15 to 90 minutes. The 15 minutes is basically a bursted pin. Fairly large breach. They get a blowdown, burst, and the 90 minutes is the type where they are looking at a pin that has a very small leak and leaking one percent of its inventory for a day, very low escape of the gas out of the pin.

Okay. So the cover gas monitoring system within an hour and a half will tell you that something is going on.

MR. MARK: Is that a continuous monitoring?

MR. SCHWALLIE: Yes, continual all the time. This system here normally is not continuous. We could run it once a day or whatever, but normally we would only turn this system on once this system says there is some activity there.

Now, here what we are talking about is once we get

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an indication of breach, it takes about eight hours total to get a positive identification through the mass spec technique of an indication of who that assembly is. Now, the DND system, we kind of worry about two extremes. Full flow 100 percent. Here we are talking about less than a minute for transient time from the core breach out to the detector. This is less than two minutes. There should be a "less" sign there, and the count time, depending on the background that you have in the system, is about one to three minutes.

MR. MARK: That is also running continuously?

MR. SCHWALLIE: Yes. That is a continuous system. There is one on each loop. Three detectors per loop.

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Now, it's 10^{-12} cc of fission gas per cc of cover gas in the system. Just to give you an idea, to peg that back, we could see fuel failures at that sensitivity at a one percent leak rate per day within two to three days.

Now, we have a program startup at Clinch River, so we fully restructure it. Within the next day we could see a fuel failure.

For example, the ETTF claims they could see something on the order of 02 to 05 ppb with a very clean cover gas system, and our system is very similar to theirs. The experience that we have had already in finding an infant mortality in FTF -- it was very good in terms of positive identification of who was with very little tag coming out of the pin. Okay. The delayed neutron monitoring system. The sensitivity is set such that we preclude the rods getting too big due to fuel swelling and enlarging the cladding at the breach site that we don't get any kind of flow characterization or closure down, and we also want to protect against these postulated porous heat-generating situations, and this is certainly the most restrictive because it takes huge blockages to get boiling.

So the sensitivity that we have specified is 1.5 square centimeters of exposed fire by recoil. That 1.5 square centimeters by geometric exposure of fuel to sodium.

MR. LIPINSKI: How does the first one compare? Would it have been necessarily a break?

(Slide)

MR. SCHWALLIE: I am not sure I understand your connection there.

MR. LIPINSKI: You will have surface contamination, and you won't need a break in your elements.

MR. SCHWALLIE: That is a consideration of the design of the system.

MR. LIPINSKI: Isn't it also on the upper one, too, for cover gas?

MR. SCHWALLIE: No, because really what we are looking for here is xenon-133, but definitely yes, that is

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taking into the foreground, background considerations all kinds of things.

MR. LIPINSKI: It will give off gas as well as give you the recoils? It's going to do both?

MR. MARK: One last question on that gas. If you have the miserable luck that assembly 13 and 213 should both start to release gas at once, then there would be a chance that you would point at assembly number 111 because you now got two of your type gases.

MR. SCHWALLIE: There is always that problem.

MR. MARK: That is the compounding of the improbabilities.

MR. SCHWALLIE: But what we believe we can do is in the presence of two, find the third with greater than 95 percent probability.

(Laughter)

MR. SCHWALLIE: How is that?

MR. MARK: That is fine.

MR. SCHWALLIE: Like I say, we have to do that in view of economics.

MR. DICKSON: Ambrose, could I add something to that? I am almost as skeptical as you are that they are that good. They said that all the ratios so that no two look like a third. That is tough, but apparently it can be done, and then when they get a leak in the FFTF, they went right to it with very little gas coming out. They did not claim it was located 100 percent. I believe it was 99.9 percent assurance that they had the right one, and they did.

(Laughter)

MR. SCHWALLIE: Now, just to try to tie together a little bit about what we have talked about, we have sort of said that blockages are incredible and from the bottom of the assembly up, and that from a safety consideration you can have huge blockages and large gas bubbles and blowdown pins and so forth, and that is not a problem, so the intent of having the system in there with its sensitivity set at such values is that it protects you from getting up to local faults having the sizes that Mr. Tilbrook talked about , the whole idea of why that system is there, to run the fuel from an economic point of view beyond breach to a convenient shutdown time.

Our intent to use that is to operate with gas leakers until we get a DND indication to some specified level of fuel exposure and then shut down and take everything that is either leaking or has a fuel-sodium contact out of the core. Okay. So the systems are tied more to the economics of fuel utilization, of economics and plant utilization as opposed to safety.

Now, in terms of specific experience in the U.S. with breached fuel, the conclusion that we would arrive at out of the data base that we have coming out of EBR-2 is that the gas leakers absolutely present no problem. They just blow down

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at whatever rate they choose in terms of the nature of the breach and continue running.

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Out of the assemblies where we have had fuel sodium contact in core, we have run confidently 22 days with like three-quarters of a square centimeter breach effect, and everything is benign. There is one assembly that has gone 96 days. The nice thing that we have found is that the DND system is much more sensitive from a operating rod point of view than we ever designed for. We are finding that the DND emission over recoil extremely sensitive. We have seen factors of 20 up to 200 enhancement of the signal, so us designing the system means that in actuality, operating rods give you a much more sensitive signal.

MR. MARK: I am not clear on your word "recoil" here.

MR. SCHWALLIE: I am saying what are the precursors that will come on the surface of the file into the sodium.

MR. MARK: So the cladding is missing. You have --

MR. SCHWALLIE: Now, what we are finding -- and you have an operating rod that has a severe temperature gradient and it is acting like a pump and pushing more precursors out of the breach than we have ever imagined it would.

MR. MARK: Right.

MR. SCHWALLIE: So the rods are pumping these things out. This is kind of handy because it gives us capability to determine if we have a DN signal when we have an operating rod or a blockage because we dropped the power and see how the DN signal changes because blockages only react as recoil. So that gives us an operating flexibility here and a little more capability to understand what we have. In terms of transient operation we have logged rods and Roger talked on this, the GE test. The original breach site is more than capable of taking care of the sodium pressures that are built up inside the pin. And the other concern there is if you have a breached mode, and you have a fuel-sodium contact, you are in a layer there.

What does that do to the thermal conductivity of the fuel? And the bottom line is not much. We do have some development to take care of. We have to make sure we understand the diameter increase versus the amount of exposed fuel so that we can clarify that down and make sure that 1 1/2 square centimeters is adequate. We do have to make sure that we get more data on transients over power.

(Slide.)

The next vu-graph just gives you specific tests if you want to read -- that you could -- that have been done in the planned testing program. We started out with pre-defective pins, then went to naturally breached pins, then ran natural breaches to get multiple failures. This is the same kind of conclusion I had before. However, in this RCBC test we did get a followon breach a short while after the first breach. Pin-to-pin contact there. We put together and we had bowing. Metallographic examination indicates the hot spots, about what we would expect. And if you analyzed that pin with that hot spot, it would have predicted it to fail about that time.

So it is not in relation to the first breach per se. It was a bad design. Okay. There is some 13 or 14 tests that are either ongoing or planned relative to developing this data base. They are including both fuel variables as well as blanket variables. Plutonium concentration, plenum defects. Unless you have any questions, I won't go into that any more than that at this time.

MR. MARK: You have pointed at the number of experiments that are in prospect which will expect to be run through that list in what, in a year or three years or what?

MR. SCHWALLIE: This program down at the bottom here is aimed for completion in early '86.

MR. MARK: Three years?

MR. SCHWALLIE: Yes. That is it then.

MR. CARBON: Thank you, Mr. Schwallie.

Walt, I didn't mean to cut off your question a while ago of the low propagation. Did you get your answer?

MR. LIPINSKI: Yes. There is nothing further.

MR. SCHWALLIE: At this time we will have Bob Markley talk about the use of thermocouples.

MR. MARKLEY: I am Bob Markley, and I would discuss the core exit thermocouples. If you found that in your handout. And Bob Bender will then discuss how the signals from this then displayed to the operator after I complete my part of the talk.

The CRBRP core exit thermocouples have two functions. One is to control the reactor, and we will talk more about that later. And the second is for design verification, basically to verify our margins, power distribution around the core, any symmetry or lack of that would be there. And further, we monitor the fuel rod lifetime. The signal from the thermocouples is fed to and on the computer and at quite regular intervals the lifetime, the CDF of the fuel is calculated, predicted. They will also look at any -- see any operational disturbances that might affect the outlet temperatures such as a change in a power distribution, shift in the power flow. Also to assess our uncertainty factors that we have factored into the design predictions, and further, it will be an actual look, an actual measurement of temperatures throughout the lifetime so that we can factor this into post-irradiation measurements.

MR. LIPINSKI: Last time we talked about this I think there was a problem about it.

MR. MARKLEY: I believe Bob Tinder will cover how the operator sees the signals, so we will leave that for him.

This vu-graph shows the core exit thermocouple coverage.

(Slide.)

Practically all of the locations are covered with thermocouples. The X is for design verification thermocouples. The C indicates the control thermocouples. As far as the coverage, 148 of 156 fuel assemblies -- there are some more located above them. Interblanket 72 of 76. All the alternating assemblies and 112 of 126 radial blanket assemblies giving a total of 338 out of 364 of the locations covered.

Before I go on, I will just show you the control thermocouples are in three sectors. You will see ten of them are in a sector here, ten in another sector, and ten in another sector. They are symmetrically located. It helps to see that here.

(Slide.)

The thermocouples that we use for automatic control are required to maintain a steady state outlet temperature within a fairly narrow band, within a few degrees. We don't want great variations over a lifetime on those, and also to minimize any temperature overshoot that would occur.

As I mentioned, there are 30 positions of these in 390 degree sectors. There are ten in each of three sectors. The rationale for that is we want to closely approach the core mixed mean temperature and follow the cycle swing as things change somewhat in the core. They are fairly well distributed within a 30 degree sector and with the three sectors that we cover we could have redundancy and could go to another sector in case we want to place any other thermocouples. They are of the dry well type, and their time constant is less than 10 seconds.

MR. MARK: Can one thermocouple running very hot affect the control system?

MR. MARKLEY: They are -- if it would -- you mean if one went way out bounds?

MR. MARK: Yes.

MR. MARKLEY: I gather they don't go in that direction. Usually they are in the other direction. They are monitored and they also look at a max and a min to determine --

MR. MARK: If you take an average, then one or two indicating trouble in their spot wouldn't affect the control system?

MR. MARKLEY: I am sorry. Will you cover that, Bob?

VOICE: Yes.

MR. MARKLEY: Let Bob go into that in detail.

MR. MARK: Yes.

MR. MARKLEY: The temperature measurement uncertainties over the fuel assemblies will range from about eight degrees F. This would be a center of a core -- with sixfold coverage by symmetry to as much as 25 degrees of a peripheral location.

As far as the inner blankets, those uncertainties range from 15 degrees, again in a high flow, symmetrical position, 40 degrees in a low flow, single coverage location. And these are the reasons why the thermocouples are not hooked to our safety system or not safety-related.

As far as local faults are concerned, things like blockages, high heat flux, first of all, there is only a very limited range in which the thermocouples would detect things that occurred. They are insensitive to very low faults. You would have gross blockages to 50, 60 percent range before they would be detect that sort of an occurrence.

Further, on the high side, the thermocouples are blinded when you get very large flow blockages such as 90 or 100 percent. With no flow coming up, the flow from adjacent assemblies would determine what temperatures and this in that range or above. You just would not get an adequate signal.

Further, there certainly is a low probability of occurrence of these type of things. Basically saying that you don't need the thermocouples, and if a local temperature increase would cause a cladding failure, this would be detected by the failed fuel monitoring system which Ambrose Schwallie discussed.

We do scan a limited number of clad failures. They are not safety concerned because of the fact that rapid propagation will not occur, as Roger Tilbrook covered for you. That's the conclusion to my part of the talk. Now Bob Tinder. If there are no questions, he can talk about how the operator sees the response and interprets these signals.

(Slide.)

MR. TINDER: To answer your question on the thermocouples, one use for using 30 is that we bring all of those into an average and reject any one of the 30 that is outside of the band, 5 or 10 percent band. It is rejected and not included in the average.

MR. MARK: Okay. So it's telling you that this one is for some reason or other running 60 degrees too hot, and you don't believe it?

MR. TINDER: Throw it away, right. For the control purpose.

MR. MARK: Sure.

MR. TINDER: You will see as I go on it is monitored. All 338 fuel and blanket thermocouples are in the reactor vessel. Each of those is wired to the plant data handling display system. In the data handling and display system, each one of those is compared with an algorithm that takes in the history. That is in the core. We then compute an alert when it is outside of a tolerance band. An alert types on an alarm typewriter. That is a typewriter sitting to the left of the chief operator there. An alert also appears on an alarm CRT. Normally, the operator would have one of the CRTs in the main control room dedicated to alarms to where it would appear on that one all the time. It would also -- the last three lines on all CRTs will have the last three things to alarm.

There are 5,000 channels in the data-handling system, and probably half of them do have alarms associated with them. The last three would appear on all CRTs. The operator can request thermocouple reading at any time. He can ask for it to come out on a CRT or on a hard copy that he can get his hands on and deliver to somebody. And as for the trend of any one of the thermocouples, we take 30 of those 338 and we use these for control in the average and reject circuit that I answered a question on at the beginning.

Now, when we have less than 25 out of those 30 are being averaged, that is alarmed to the operator on the plant's main alarm system. You know, those human factors and what the operator needs will be under study for the next three or four years at least.

MR. ZUDANS: That would appear to me at least as an impressive, alive, dynamic information.

MR. TINDER: You may be right. There is a red and green and orange, and there are a lot of ways. You can use candlesticks at the core.

MR. MARK: When you say you can hook and take a look a a trend, across what sort of time? Would it be 3 milliseconds or 3 hours or what?

MR. TINDER: It would be days. The computer system has a trend file established in it where -- and I said this is just starting to be done now, the programming and so forth of the computer, to have it set so that if the thermocouple moves over three degrees, that point is put in the trend file, and do that for X number of days and then have it to be a circular file, and it will be at least days. Then the data is put on magnetic tape for permanent storage use by the engineering department.

MR. MARK: That is for use after an accident if you ever have one?

MR. TINDER: (Nods affirmatively.)

That's all I planned for the display to the operator.

MR. STRAWBRIDGE: I am Lee Strawbridge, Westinghouse, Waltz Mill site, and I will be discussing the review that we have made of the worldwide application of local fault in response to earlier questions raised by Dr. Carbon. As part of that we did review the Leon papers that -- the ones that you gave us and the other references in that paper, so that formed part of the base for what we did. We went beyond that, and we looked for data from other places as well. We performed the survey which included looking for information on 17 sodium-cooled fast reactors in order to see if we could make some sense out of the data that we got.

As the end result of that, we looked at it from the standpoint of certain major plant design variations such as are there trends that exist with respect to loop versus pool considerations? Are there trends that exist for power versus test? Are there trends for small versus large reactor size, and in terms of the fuel design characteristics?

The two principal characteristics were how about oxide or metal fuel, and wire wrapper versus grid spacers. The information that I have in -- by the 17 reactors that cover those areas is on the two vu-graphs combined, and perhaps I can just use both vu-graphs. I will put them on at one time, two views.

(Two slides.)

I had two blanks that you have in the passout, and just as a result of a meeting yesterday, I was able to complete a little more information, and I filled in a couple of blanks on this one on the screen which is not in your passout copy. The one we have done is list across the top of the 17 different reactors considering the various foreign countries and finally the U.S. reactors on the end here and down the side we considered these characteristics that I just mentioned -- the loop or pool, power or test, fuel, spacer concept, and then asked the question does the design include core exit thermocouples, and are there thermocouples in the PPS that are there for purposes of local fault monitoring?

The overall results are tabulated here on these two vu-graphs. What we found was when we looked for trends with respect to the different either plant or fuel design characteristics, one place where we did find what I considered a very significant trend was in the area of loop versus pool concept, and those results are tabulated on this next vu-graph.

I apologize for the correction. This was due to adding that new information on the previous two vu-graphs and the two that were in the unknown column before. One got added into the yes column and one into the no column, but the strongest correlation was for pool versus loop, and in effect, for the 11 loop concepts, only 8 we know the answer to the question of are thermocouples in the PPS for local fault monitoring.

Of those 8, the answer for 6 of them is no. Six out of 8 do not have them in the PPS for local fault monitoring. If you look at the pool reactors, 4 out of 5 in fact have outlet thermocouples in the PPS for local fault monitoring.

Now, it is not entirely obvious why that kind of a substantial trend does exist. It is individual designers in countries taking certain position, as I think you heard before from Mr. Tilbrook, even in a country such as the U.S.

There are strong differences of opinion when you talk to certain parties in the U.K. versus other parties in the U.K. about their effectiveness and whether it makes sense to put them in the PPS or not. The one thing that I point out is that, with the pool concept, is that a delayed neutron monitoring system would be less sensitive and involve longer time delays than would the system in the loop reactors, so that is a possible explanation for that, at least intuitively.

MR. LIPINSKI: If you sum them, you end up with six and seven.

MR. STRAWBRIDGE: Yes.

MR. ZUDANS: Does the age of the loop plants where you have six of them give some kind of a clue as compared to the two remaining ones, or the design rather than age?

MR. STRAWBRIDGE: I guess I do not know the answer to that. I would have to go back and look at the two previous graphs, and one could look at that for five minutes and probably answered the question. I had not looked at it from that standpoint.

MR. ZUDANS: All right.

MR. STRAWBRIDGE: So overall, what I would like to say is that there is no universal agreement on approach.

(Slide)

It is certainly a true fact that for core outlet thermocouples usually included in PPS in pool reactors, usually not included in loop reactors -- including the Clinch River, which, of course, is a loop reactor. The application of the instrumentation is consistent with the worldwide trends that were noted.

I would like to turn now to a few conclusions from this whole series of presentations we heard on the general subject of local faults and just try to tie these different presentations together, if I could.

(Slide)

First of all, the first section, that there should be a low probability of loading defective fuel, that is, defective in the first place, and that includes loading fuel into other locations besides where it was designed to be loaded because of the features that we have. The design features that were described to you and the operational requirements prevent ex-core blockages.

The in-core blockage expected based on operation, tests and analysis. We do not expect in-core blockages, and that conclusion is based on operation, tests, as well as analysis that has been performed. We certainly do expect some fuel failures during operation. They are anticipated and they will be detected.

We do have extensive experience that shows that when such fuel failures do occur, they will not propagate.

On the question of thermocouples, it is our belief that thermocouples do not significantly improve the margin of safety for local faults. They are not very effective in showing the effect of small blockages, for example. Systems such as DNDs are more sensitive for detecting failure conditions from a safety standpoint, and we do use them.

You include the Clinch River design being a loop reactor and not including them in the PPS is consistent with the majority of the worldwide experience.

MR. ZUDANS: That is a little too strong because four of the six are U.S. plants. You know, look at your list. Four of the six that you have identified.

MR. STRAWBRIDGE: Yes.

MR. ZUDANS: They are U.S. plants.

MR. DICKSON: Are we not part of the world? I would like to think we can be included in the worldwide experience.

MR. ZUDANS: I don't know that they are strictly worldwide because there are a couple of them that we don't know, one in Germany and one in Tokyo.

MR. STRAWBRIDGE: Well, the Russians are the ones we don't have information on, but I think it is appropriate to include earlier U.S. experience when we are looking at what I am defining as worldwide. I certainly would not intend to exclude U.S. as being part of that worldwide experience, and the final and perhaps overriding conclusion is that instrumentation for local fault monitoring is not needed in the PPS because there are no faults that could propagate on a time scale that does require PPS.

MR. CARBON: I guess you can make anything out of numbers that you wish. You can find even a stronger correlation is that the U.S. does not put them in, other nations do.

MR. LIPINSKI: I was going to put my observations in that if I look at the French, the British and the Japanese, I see all yeses, with some unknown for the Russians. So the rest of the world, I would conclude, is yes, and the U.S. is no.

MR. STRAWBRIDGE: I don't think it quite that simple, but I agree. One can look at the information and come up with different ways of interpreting it. I looked at it from the standpoint of those various design concepts, and the only place where I found what I would call quite a strong trend was the loop versus pool.

MR. LIPINSKI: But is it a question of national licensing policies in being conservative from a safety standpoint?

MR. STRAWBRIDGE: Did you have a comment to make?

MR. SCHWALLIE: Some of those people use grids and so forth, so you can temper that with the design feature that goes in that. If we had grids, we might change our position.

You understand?

MR. CARBON: I don't think it is a very productive argument. The grid and wire wraps don't make much difference in that chart either.

MR. STRAWBRIDGE: The thing that stands out most is the thing Walt was pointing out. That is more of a country-by-country type. I don't say that you make anything out of it, but that is what seems to stand out.

MR. LIPINSKI: You have indicated that SNR300 is an alarm. The last I knew, it was a trip. You have verified that it was an alarm?

VOICE: Which country was that?

MR. LIPINSKI: Germany.

VOICE: Leon confirmed yesterday that they do not have a trip on that. They have an alarm to the operator and it goes into a normal shutdown sequence. They don't expose the plant to a scram, and the licensing authorities, having reviewed the information, they have not requested that it be a trip.

MR. LIPINSKI: Your information is later than mine.

MR. STRAWBRIDGE: The question involved is the delay times for some condition to exist before thermocouples could tell the PPS to do something. It would be a short time after the signal reaches the PPS.

MR. MARK: But this is instantaneous once you have j-6-8B

said?

MR. STRAWBRIDGE: Essentially, yes.

MR. MARK: Now, the neutron detectors that tell you you have fuel that just appeared, that is about a 15-minute wait?

VOICE: No, 40, 50 seconds, two minutes.

MR. MARK: One or two plus another one or two. Very-few-minute warning that there is fuel failure somewhere. And then the other thing you are pointing at is that the evidence for spreading or enlarging of some failure somewhere can be from hours to days?

MR. STRAWBRIDGE: That's right. Up in the days or beyond the time frame as you see it, yes, sir.

MR. MARK: So it's in that context that you say we don't really want something that happens in a very, very short time after this temperature is on the average raising a question?

MR. STRAWBRIDGE: That's right. We see no need for a response on that short time span.

MR. MARK: But you would expect to have a warning tied into this? The temperature has passed some line which you could have predetermined?

MR. STRAWBRIDGE: Yes. The operator would be aware of those conditions in a hurry.

MR. ZUDANS: Excuse me. Now, I think I got confused. You said the thermocouple would be slower and the DNDs would be faster?

MR. STRAWBRIDGE: The thermocouple response time was about ten seconds, less than ten seconds.

MR. CLARE: I think we have a semantics problem. If the temperature goes up --

MR. ZUDANS: Oh, no, no.

MR. CLARE: -- then the thermocouple will respond quickly. We might expect it to occur over the life. You wouldn't even see it.

MR. ZUDANS: That's correct.

MR. CLARE: Correct.

MR. ZUDANS: So therefore that was the main reason why you would rely on that?

MR. CLARE: If you want any response at all, you can rely on thermocouples.

(Pause)

MR. CARBON: This concludes it then, Mr. Strawbridge?

MR. STRAWBRIDGE: Yes.

MR. CARBON: Any questions that didn't get answered? We did skip your ten-minute presentation at 2:15 following the accident recovery planning. Did you have comments to make there?

MR. STARK: We didn't have anything significant to add, and it was kind of three subjects under one heading, so I will skip it.

MR. CARBON: If there are no more questions or if no one has anything to add, we will recess until tomorrow morning.

(Whereupon, at 5:35 p.m. the committee recessed, to reconvene the following day, Thursday, March 17, 1983, at 8:30 a.m.)

End
8B

CERTIFICATE OF REPORTER

I hereby certify that the transcript of the proceedings taken before:

THE ADVISORY COMMITTEE ON REACTOR SAFEGUARDS, CRBR SUBCOMMITTEE/STRUCTURES AND MATERIALS WORKING GROUP

DATE: March 16, 1983

PLACE: Washington, D.C.

TIME: 8:30 a.m.

is a true and correct transcript of the proceedings.

Ronald Graham
Ronald Graham

Leslie Burroughs
Leslie Burroughs

Ann Riley
Ann Riley

Mary Simon
Mary Simons