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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

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SUBCOMMITTEE ON THERMAL HYDRAULIC PHENOMENA

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OPEN SESSION

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WEDNESDAY

MAY 9, 2012

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B1, 11545 Rockville Pike, at 8:30 a.m., Sanjoy Banerjee, Chairman, presiding.

SUBCOMMITTEE MEMBERS PRESENT:

SANJOY BANERJEE, Chairman

SAID ABDEL-KHALIK

J. SAM ARMIJO

DENNIS C. BLEY

MICHAEL CORRADINI (via telephone)

HAROLD B. RAY
JOY REMPE
MICHAEL T. RYAN
STEPHEN P. SCHULTZ
WILLIAM J. SHACK
GORDON R. SKILLMAN
JOHN W. STETKAR

CONSULTANTS TO THE SUBCOMMITTEE PRESENT:
JOHN FLACK
THOMAS S. KRESS
GRAHAM B. WALLIS

NRC STAFF PRESENT:
ANTONIO DIAS, Designated Federal Official
WILLIAM RULAND
STEWART BAILEY
ERV GEIGER
PAUL KLEIN
STEVE SMITH
MIKE SNODDERLY

ALSO PRESENT:

MICHAEL MURRAY
ERNIE KEE
TIM SANDE
DAVID JOHNSON
BRUCE LETELLIER
RODOLFO VAGHETTO
YASSIN HASSAN
GIL ZIGLER
KERRY HOWE

C-O-N-T-E-N-T-S

C-O-N-T-E-N-T-S (CONTINUED)

P R O C E E D I N G S

8:30

CHAIR BANERJEE: [presiding] The meeting will now come to order.

This is a meeting of the Advisory Committee on Reactor Safeguards, the Subcommittee on Thermal Hydraulics Phenomena. I am Sanjoy Banerjee, Chairman of the Subcommittee.

Members currently in attendance are Steve Schultz, Dick Skillman -- Dennis Bley will join us in the afternoon -- Harold Ray, Sam Armijo, Michael Ryan, Said Abdel-Khalik, Bill Shack, Joy Rempe, and John Stetkar. We are also supported by our consultants, former ACRS members Graham Wallis and Tom Kress. Mike Corradini is also on the phone line.

This is the second day of a two-day meeting to hold discussions with NRC staff and industry representatives on WCAP-16793-NP Revision 2, Evaluation of Long-Term Cooling Considering Particulate, Fibrous, and Chemical Debris in the Recirculating Fluid, including the associated models and test data that support the report.

Today's session will focus on the staff's Draft Safety Evaluation Report on WCAP-16793-NP. The SERs support resolution of Generic Safety Issue GSI-191,

Assessment of Debris Accumulation on PWR Sump Performance.

Additionally, the Subcommittee will be briefed by representatives from the South Texas Project on a risk-informed approach to resolution of GSI-191. This will probably happen in the afternoon, and this is only for informational purposes.

The Subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the full Committee.

Antonio Dias is the Designated Federal Official 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 April 25th, 2012.

A transcript of the meeting is being kept and will be made available as stated in The Federal Register notice. It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

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

We will now proceed with the meeting. There has been a slight sort of confusion on the agenda. So, we have a period when we can have a Subcommittee discussion before the staff is ready to present, I think.

MR. BAILEY: One of the discussions we had late yesterday related to the South Texas presentation this afternoon. There was some question of whether that was going to be for information only or whether you wanted them to follow up with the full Committee. Did you want to have that discussion at this time?

CHAIR BANERJEE: Right. I think at the end of the discussions today the Subcommittee will consider whether we want them to brief the full Committee as well or not. If we decide that they should brief the full Committee, then we will request that.

I think the full Committee letter at the moment -- correct me, Sam or John -- is due for July, right?

CONSULTANT FLACK: July, yes.

CHAIR BANERJEE: Yes. So, the WCAP matter at the moment is slated for July. That would probably be the time when we would ask for the briefing as well.

Now if there is a feeling in this Subcommittee that we would want to brief the full Committee at some other time, we can decide that as well.

So, let's hold that --

MEMBER SHACK: When is the options paper going up?

MR. BAILEY: The options paper is going up in June.

CHAIR BANERJEE: Right.

MR. BAILEY: The options paper is going up in June.

Do we have people on the phone? But I will use the microphone.

CHAIR BANERJEE: Yes, Mike is on the line.

Yes, but at the moment we are scheduled to write our letter in July. I guess the issue here is also this is still a Draft SER that we have. If there are any changes in between July, then we will certainly consider that, I would imagine.

MR. BAILEY: Between now and July? Yes, it is undergoing some minor editing, but no substantive changes to it.

CHAIR BANERJEE: Okay. So, we can handle that.

We had a very good meeting with people from the PWR Owners' Group yesterday, and there are several members here who wanted the meeting. We spent most of the afternoon looking at data which was interesting,

though Harold managed to do other things, but he was here, or he left a little early. But, nonetheless, it was a very enlightening meeting.

As you will see, the staff's position will consider some data that we did not really consider at the meeting yesterday because it was primarily data that was taken at Westinghouse. So, there will be some other data which was taken by AREVA which is part of the staff's consideration, and a very important part, that we will talk about more today in closed session. So, after a brief introduction, we will close everything. Depending on who has access to which data, we will have to clear the room appropriately, and the staff will look at that.

To summarize yesterday's discussions, the data that was presented during the meeting -- I think I am allowed to say in an open meeting -- indicated that there were pretty similar delta Ps or pressure losses over a wide range of fiber loadings, which was sort of a little bit unexpected. But a wide range of conditions, we got similar pressure losses.

There was some effect of particle fiber loading ratios. I am not going to go into any details here. Of course, the position right now is that we are trying to consider what are really bounding estimates.

I think that is what we will hear from the staff.

They have a taken a position where we are fairly comfortable that it looks like a bounding estimate of pressure losses. They will explain this position further.

So, I think is, more or less, my summary of yesterday's discussion in a very brief. If any of the other members have things to add, please feel free to do so, who were there. Bill was there and Said was there and Sam, and, of course, our consultants were there as well.

So, Graham, do you have, in particular, anything to add to that?

CONSULTANT WALLIS: No. I am looking at the slides and planning ahead.

(Laughter.)

CHAIR BANERJEE: All right. Okay.

MR. RULAND: Mr. Chairman?

CHAIR BANERJEE: Yes?

MR. RULAND: We are ready to start whenever you are.

CHAIR BANERJEE: Okay. So, Bill, do you want to make a few remarks.

MR. RULAND: Actually, Stu would like to start out.

CHAIR BANERJEE: All right.

MR. BAILEY: Okay. And let me start out first, when we talk about proprietary here, as they are setting up, I will explain this. We put that on the first slide of the package so you would realize that this is the proprietary set of slides. The majority of the slides are not proprietary. The ones that do contain proprietary data in terms of pressure drops or other actual numerical information, I believe are notated at the top.

So, I believe everybody at the table has a proprietary version. I think the ones out in the audience are likely the non-proprietary version. When it comes down to getting into those proprietary slides, we will make the decision at that point on the best way to proceed; who is privy to the information or who may have to step out until the next break.

CHAIR BANERJEE: So, we leave it in your hands.

MR. BAILEY: Okay, and then, we will have to leave it up to the information owners to keep us straight on who is allowed to see that information and who is not.

CHAIR BANERJEE: So, just warn us when it is coming, and then we will --

MR. BAILEY: Okay. I will do that. So, hopefully, I am not taking too much of what Erv wanted to say.

We are here to present our safety evaluation.

CHAIR BANERJEE: Right.

MR. BAILEY: I also wanted to give some level of thanks to the PWR Owners' Group for the presentation yesterday and the work that they have done. This has been a difficult path for the Owners' Group over the last four-plus years, looking at sometimes conflicting or unexplained test data in the face of large uncertainties.

As you can imply by yesterday's presentation and the test matrices that you looked at, there have been a number of challenges and it has been somewhat difficult to explain the behavior at the level that most of us might like. This has not been an exact science.

In the staff's review, we have, similar to you, had to infer what the actual phenomena occurring is based on the results of the test and based on the test observations. We have been out to observe a number of the tests, and Erv gave you some of those test reports yesterday or the trip reports yesterday.

But the WCAP came in, and the staff was sensitive to the fact that it needed to provide guidance to industries such that they could close GSI-191. As we had discussed yesterday, there is some amount of legacy information in the Topical Report. There is also some level of statements that are not thoroughly supported. But, nevertheless, the staff performed its evaluation and came up with what it believes are defensible limits for plants to use in closing out their Generic Safety Issue 191.

So, with that, I will leave it to Erv.

MEMBER SHACK: Just to interrupt for a second, John and Tony, Mike is looking for slides. Do we have a copy that we can send him?

CHAIR BANERJEE: So, at the moment, the meeting is open?

MR. GEIGER: At the moment, yes. When we get to the slides, there are about five slides that have information that is proprietary.

MR. KLEIN: The first 20 slides are non-proprietary.

CHAIR BANERJEE: Okay. Fine.

MR. GEIGER: Halfway into some of the testing data has some --

CONSULTANT WALLIS: Well, there is nothing

on the slides themselves that indicates.

MR. GEIGER: Actually, in the heading of the slide, it says "proprietary".

CHAIR BANERJEE: The whole bunch is. At least, let's treat it as proprietary.

MR. GEIGER: Actually, that is interesting. I put headers on them yesterday. Slide 21, there is a big "proprietary". So, every one I tried to put "proprietary" on top of the slides and title.

CONSULTANT WALLIS: Does that mean that we can't make comments on them for the public record?

CHAIR BANERJEE: No, we can for the closed session.

CONSULTANT WALLIS: Yes, but for now? This isn't a closed session. We can't say anything until we get to the closed session?

CHAIR BANERJEE: You can say --

MR. BAILEY: I would defer discussion of the proprietary slides until the closed session. If our pace goes right, hopefully, we will be doing that this morning, and by the time of the first break we will be able to open the meeting back up again.

CONSULTANT WALLIS: So, we can't comment on any of the experimental results until we go into closed session?

MR. BAILEY: That is correct.

CONSULTANT WALLIS: Thank you. That is very useful.

MR. GEIGER: Good morning.

My name is Ervin Geiger.

With me is Steve Smith and Paul Klein. We will be describing the staff safety evaluation of the WCAP-16793-NP.

Yesterday, the PWROG presented quite a long discussion on this. I guess some of our slides cover the same areas. So, we are going to try to just go over them very quickly.

Where is PageDown? I'm sorry. PageDown, there we go.

So, basically, our discussion will cover the items in here. We will give a brief history, although Westinghouse gave a pretty good history of the events. So, we may fill in a couple of dates. And then, an overview, and then we will present our Technical Evaluation.

Next slide.

This is a timeline of the WCAP-16793 evolution. It started with the Generic Letter 2004-002. In response to that, the PWROG prepared its WCAP to allow licensees to evaluate downstream effects.

Rev 0 was submitted. Staff reviewed it and presented at ACRS, and there were many comments that came out of that presentation.

In response to that, the Owners' Group went off and did a lot of testing and some analyses to answer the question. One question was, what happens if you get a uniform blockage across the bottom, and so on? And the other was to address chemicals.

So, PWROG, through Westinghouse, did all of that and came back with a Revision 1 to the WCAP, which included a lot of test results and things. In reviewing some of those results, we had additional RAIs, and a whole other effort ensued, which, then, some additional testing was done. Now we are at Rev 2 of the WCAP.

CHAIR BANERJEE: And just for clarification, Ervin, this WCAP-16793 is a non-proprietary version.

MR. GEIGER: Yes.

CHAIR BANERJEE: But it refers to two reports, 17057 and the AREVA test report, which are proprietary.

MR. GEIGER: Yes.

CHAIR BANERJEE: So, just to let the Committee know this.

MR. GEIGER: And that is, when Revision 1

of the WCAP came in, actually, there was no real acceptance criteria in it. It all referenced these reports. There are a lot of pressure drops across the difference parts of the vessel which apparently were proprietary.

So, those reports could not be issued. Staff reviewed those reports, but they were non-public, and we based our evaluation on all those reports.

So, now WCAP Rev 2 actually has the acceptance criteria, but, still, there is no data in there for what the test results were, and so on. So, we still have to rely on those two reports. They are in ADAMS, but they are non-public.

CHAIR BANERJEE: So, to support the conclusions, we have to rely on the proprietary data?

MR. GEIGER: Yes, yes.

CONSULTANT WALLIS: Can we ask you right now, do you think that the three years that have elapsed since the last meeting you have answered the questions we raised in 2008?

MR. GEIGER: Well, we answered the first question about the uniform blockage, which, of course, at this point we are really not relying anymore. We will get into it in the slide.

Through all the additional testing, yes, we

did. We tested a lot of chemicals and things. So, we had a much better understanding of how all this, but there is still --

CONSULTANT WALLIS: So, they tested them? You didn't test anything?

MR. GEIGER: Well, they tested, yes. They tested. We observed tested. So, they tested.

CONSULTANT WALLIS: So, they answered the questions that we asked. The testing answered the questions that we asked in 2008?

MR. KLEIN: I think the testing actually probably raised more questions instead of answering the questions that you had.

CONSULTANT WALLIS: I think that is correct. Now I am just wondering. That is why I asked you this question.

MR. KLEIN: I think our overall approach is we are at a point where we are comfortable accepting a fiber limit that we don't think will build a filtering bed within a fuel assembly. And beyond that, we have a number of unanswered questions.

CONSULTANT WALLIS: Yes, I think that is a true statement. Thank you very much.

MR. GEIGER: Okay. Slide 4, this is just an additional overview. Right now, what the WCAP does,

the intent of the WCAP is it basically sets the limits on the temperature of the fuel that will not result in degradation oxidation over 30 days, as claimed by Westinghouse yesterday.

It sets an upper limit on the quantity of debris that could go into the reactor vessel, be transported to the four inlets, and not --

(Interruption by phone line noise.)

CHAIR BANERJEE: Let's just get this hung up.

MR. GEIGER: Sorry.

CHAIR BANERJEE: Go ahead.

MR. GEIGER: So, it sets limits on the debris that could go to the core inlets that will not block adequate flow to make up for -- well, in this case, what it does is for a cold leg break, we require the full flow because the decision was made by the Owners' Group and us to avoid answering all the questions about what happens if you get spillover.

So, if you get a constant input with either two RHR pumps or one RHR, or whatever, that that full flow will go through the vessel with the head you have, based on the --

CHAIR BANERJEE: For the hot leg?

MR. GEIGER: For the hot leg, yes. For the

hot leg. For the cold-leg break, it was basically just the pressure available in the downcomer to get flow through. So, there are two different flow values. Initially, there were two different tests, the cold-leg/hot-leg test. And then, as we went back and forth with the acceptance criteria, things changed. Steve will cover a lot of that.

Slide 5, this is a tool for licensees to use just to evaluate their capability to get coolant into the core and to make sure that, due to the deposits on the cladding, the temperature will not exceed 800 degree F that was set as an acceptance criteria.

MEMBER SKILLMAN: Ervin, my name is Dick Skillman.

Let me ask you this question.

MR. GEIGER: Sure.

MEMBER SKILLMAN: You just mentioned RHR pumps, pumps in participation to get water to the core.

MR. GEIGER: Uh-hum.

MEMBER SKILLMAN: How do you know that the pumps will survive the fiber that you are predicting on the core, please?

MR. GEIGER: There is another WCAP-16406-P, proprietary, that the licensees are also implementing to evaluate the effects of the downstream

debris on pumps and valves and clogging and spray nozzles, and all of that, and instruments, too. So, there is 16406-P.

CHAIR BANERJEE: And we reviewed that and approved it?

MR. GEIGER: Yes, quite a long time ago, yes.

CHAIR BANERJEE: The staff approved it a long time ago.

MR. GEIGER: Yes, and that is based on testing that was done by different entities on pumps and things.

MEMBER SKILLMAN: Thank you, Ervin.

MR. GEIGER: So, there is criteria in there about how much debris you can have and the wear rates and all that are in there.

MEMBER SKILLMAN: Thank you. Okay.

MR. GEIGER: And also, for those plants that cannot meet the stringent acceptance limits in this WCAP, it makes some suggestions on alternates, you know, avenues you can pursue to perhaps increase that limit. And those would be subject to staff review.

MEMBER REMPE: Excuse me. I apologize for missing yesterday morning, and I know there was a discussion, I guess, about the thermal conductivity, the

fuel that was assumed to come up with a constant heat flux that was assumed for these analyses.

MR. GEIGER: Uh-hum.

MEMBER REMPE: Did you consider thermal conductivity degradation in this constant heat flux. It would be important if you did.

MR. GEIGER: I didn't --

MR. BAILEY: To answer your question, no, thermal conductivity degradation was not included. Actually, the analyses that we were talking about really do date back to 2008. What it looks at is it looks at maximum deposits, maximum crude, maximum oxidation that you would expect over the course of the event, and performs essentially steady-state heat transfer coefficients, assuming those maximums, to show what is the maximum clad temperature you would get.

And they showed a temperature -- they showed that they stayed within their limit, which was 800 degrees F. That 800-degree-F temperature limit is based on autoclave testing that they did, looking at the behavior of the cladding that has already been heated and quenched. That is one of the reasons it sets a lower limit than the 2200 that you are looking for, your typical 50.46 analysis.

CHAIR BANERJEE: Is your point, also, Joy,

that the effect of thermal conductivity degradation may affect the blowdown debris generation? Or what is it?

MEMBER REMPE: No, it is just that --

CHAIR BANERJEE: Just on this, for the long-term?

MEMBER REMPE: Is there enough conservatism that it won't be important? I think what I am hearing is, yes, there is enough conservatism --

CHAIR BANERJEE: Yes, this is a steady-state.

CONSULTANT WALLIS: I think that the temperature of the cladding is independent of the conductivity of the fuel, once you get the long-term cooling --

CHAIR BANERJEE: Speak up, Graham. He can't hear you.

CONSULTANT WALLIS: -- once you get to long-term cooling. Because it is governed by a given amount of heat coming out and the resistance to the outside world. That is what matters. And what happens in the fuel doesn't affect.

CHAIR BANERJEE: It will affect the early stages of the blowdown, obviously. Stored energy will be different.

MR. GEIGER: We are talking over the

duration of the accident.

CHAIR BANERJEE: Yes. So, this is a long time.

MR. GEIGER: We have got longer than that.

CHAIR BANERJEE: Way past, yes.

Do you expect it will have any effect on debris generation or is it still insignificant?

MR. BAILEY: Thermal conductivity degradation?

CHAIR BANERJEE: Yes, in terms of stored energy and things like that.

MR. BAILEY: No, we are not --

CHAIR BANERJEE: Or it is a very small effect?

MR. BAILEY: No. The debris generation that you are looking at is really based on simply the blowdown of the RCS.

CHAIR BANERJEE: Right.

MR. BAILEY: And really, it is the initial portions and the large mass in energy release. If there is some additional stored energy that, then, remains in the fuel during the quench period -- but most of the dynamics are significantly down at this point, as far as GSI-191 is concerned.

CHAIR BANERJEE: Yes. I think that it is

a small effect, but the amount of stored energy that you have to get rid of during the blowdown phase will change because you have more stored energy.

MR. BAILEY: You know, I believe that that is exactly right.

CHAIR BANERJEE: Yes.

MR. BAILEY: What you are looking at here is, at a minimum, 20 minutes into the event. That is the soonest we get onto recirc and start worrying about issues like GSI-191. In reality, it takes time for the debris bits to build up and time for the debris to transport or get transferred into the core. A reasonable minimum time, you are looking at here is more like 45 minutes to an hour, I think, in order to see any significant effects. At that point, you are in a, more or less, steady-state boiloff condition.

CHAIR BANERJEE: Right. I think the point is the jets, the eroding of the -- but I do think it is a second-order effect. But at some point you might look at it, just to see if it affects the duration of the jets. At the moment, it is so empirical --

MEMBER SHACK: It is the way we treat jet impact, anyway.

CHAIR BANERJEE: Yes. I don't think it will matter. So, okay.

MR. GEIGER: For clarification, are we talking about crud and things blown out of the vessels or --

CHAIR BANERJEE: No, we are only talking about the impingement, the duration of impingement of the jets on insulation and other things.

MR. GEIGER: Okay. Outside sources?

CHAIR BANERJEE: And the question of whether this stored energy affects the generation term. Do we have more fiber?

MR. GEIGER: Okay. Now this prolonged jet, in other words, targets more. Okay.

CHAIR BANERJEE: But I think it is a second-order effect.

MR. GEIGER: Yes, I think we have already basically cleaned everything out --

CHAIR BANERJEE: Yes, yes.

MR. GEIGER: -- with the initial jet from what is required to be taken as debris generation.

CHAIR BANERJEE: Sorry.

MR. GEIGER: I can always learn something here, you know.

So, yes, the staff evaluates each of these points.

So, slide 6, our regulatory evaluation is

basically based on 10 CFR 50.56(b)(5) criteria. That is to get enough coolant into the core for long-term core cooling, in this case with debris. So, what we are looking at is the effect debris has on getting coolant flowing to the core. So, I am not trying to revisit all the criteria, just that.

CHAIR BANERJEE: So, this limit is set as 800 degrees Fahrenheit right now, right?

MR. GEIGER: Yes, that was set at 800 degrees after the quench because of the test results that we received that showed that up to 800 degrees there are no problems. We don't have any data right now to show that there is no degradation above 800, so we set it at 800. You know, if there is more data available later on, that could be raised.

CHAIR BANERJEE: At the moment, that is your --

MR. GEIGER: At the moment, it is 800 degrees, yes.

CHAIR BANERJEE: Okay.

MR. GEIGER: And we will go into a little later about how those analyses were performed to show that.

CHAIR BANERJEE: Is there any limitation, also, on -- the reason I am asking this is, when we

considered one of the new reactor concepts, we sort of accepted a 50 percent exit quality as being a reasonable limit from the viewpoint of a lot of things, including deposition and stuff like this.

Have you looked at that limit and seen what the rationale for that was? Because this Committee reviewed that and we agreed to it.

MR. BAILEY: The answer to that, I think as you identified in yesterday's presentation, some of the analysis is building on very simple boiloff legacy analysis for long-tern core cooling. And so, it did not go into great detail over all the dynamics you would expect in the reactor coolant system in terms of setting some of the pressure drops that they were using as criteria for their tests.

As you will see when we get into the data, at the 15-gram level, the differential pressures are very, very low with a lot of margin. So, I believe such issues become less important.

But, to back up to the 800 degrees, just in order that we have the right perspective on that, that was an industry-proposed criteria that they validated through the autoclave testing. To my mind, there is no reason to believe that temperatures would not be acceptable, but at the moment those are the temperatures

that have been justified by the licensees.

CHAIR BANERJEE: Okay.

MR. GEIGER: Okay. On to slide 7. So, with respect to GSI-191 and the Generic Letter 2004-002, licensees are required to demonstrate that, if you add adequate coolant into the core with the debris limits -- what the licensees have to show is that, currently, under this WCAP, their debris that bypasses or passes through the strainer and ends up at the core inlet is less than what was qualified in this testing. They have to perform some calculations to show that they comply with this 800-degree analysis -- and Paul will go further into some of those analyses -- and, also, to calculate the deposit thickness based on this. We set a maximum of .050 inches, as they described yesterday, to limit/prevent touching of two rods or filling that gap. Then, the heat transfer mode changes and, also, then, the fill patterns could be altered.

MEMBER ABDEL-KHALIK: Why isn't there a limit, also, on the length of fibers that pass through to the core, given the fact that the results do depends on the length of fibers and the experiments were limited to fibers less than 2 to 3 millimeters?

MR. SMITH: The basis for the fiber length -- and we will have a slide that shows what the

fiber lengths used were -- it was based on actual testing of what bypassed strainers during bypass testing, to determine what the downstream source-term would be.

So, I think we took more of a realistic approach to what would actually be in the core. I am not sure if larger fibers would give you a higher head loss, but we don't expect a significant amount of larger fibers in the core.

MEMBER ABDEL-KHALIK: But this set of criteria stands on its own, and it pertains to how the core will behave. And the experiments were done with a given amount of debris, a given set of debris characteristics. The results of the data, the results of the experiments, are valid only within that range of parameters. Then, it would seem appropriate that, whether it is redundant or not, to also include that criteria, a limit on the fiber length.

MR. SMITH: That would be a difficult limit to enforce. I don't know you could possibly --

MEMBER ABDEL-KHALIK: Well, I mean, presumably, that came about as a result of testing of the strainers.

MR. SMITH: That is how we determined the size distribution or how the testers determined the size distribution.

MEMBER ABDEL-KHALIK: And presumably, whatever debris mass limit that will come out of these results will be shown through testing of the strainers.

MR. SMITH: That's correct.

MEMBER ABDEL-KHALIK: So, those same tests can produce not only the mass that will pass through, but also the size characteristics of the fibers. This set of data or this set of limits has to stand on its own.

MR. GEIGER: I would tend to agree with you, sir, yes.

MR. BAILEY: You are correct, there is an embedded assumption about the distribution of fibers that enters the core. The values that we took were taken to be prototypical values based on the strainer bypass testing. That was determined to be valid for the operating fleet that this WCAP is intended to be used for.

Again, this is not really a parameter that a licensee controls directly.

MEMBER SHACK: You have condition 14 that asks them to demonstrate that this bypass beats these limits. I don't see any reason you can't expand that condition to say that he verifies the distribution.

MR. GEIGER: I think that is reasonable.

MR. BAILEY: We will consider that. I would say at the moment that we do not have a series of tests that is running various size distributions.

MEMBER SHACK: No, I don't think we are asking that. It is just that, when you look at the debris that passes through the strainer, you verify that it is less than 15 grams and it has a size distribution that is consistent with the tests.

MR. GEIGER: Well, at least the range of the particles, I mean, in the fiber is not way out one way or the other, yes.

CHAIR BANERJEE: So, at the moment, Stu, they are taking some grab samples to look at how much is bypassing, correct? Is that how they are demonstrating that you have adequate performance, or they will demonstrate?

MR. BAILEY: On the bypass testing, yes. Well, they will be doing full capture of the flow-through.

CHAIR BANERJEE: Full capture?

MR. BAILEY: The guidance that we just issued supports full capture of the fiber that makes it through the strainer.

MEMBER ABDEL-KHALIK: And if that is the case, you should be able to do sizing.

MR. BAILEY: They should be able to do a comparison.

CHAIR BANERJEE: Why don't we defer this question to --

MR. KLEIN: I think it might be a reasonable expectation that the licensees verify their plant-specific bypass has representative fiber lengths through the testing. Our expectation is that will be the case, though, since even though there are multiple designs, there are limited sizes of perforation.

CHAIR BANERJEE: Yes. So, can you now or later tell us what sort of typical distribution, because yesterday we heard about this?

MR. BAILEY: We will. Yes, we have that as our slides, I believe.

CHAIR BANERJEE: Okay.

CONSULTANT WALLIS: Can we pursue this a bit more? Said annunciated a principle that the results of the test apply to the conditions of the test, and he talked about fibers. But the tests were done with silicon carbide with a certain size range. As far as I know, there is no silicon carbide in containment.

You heard yesterday that silicon carbide probably interacts with the chemicals in some unusual way which changes the results and explains some of the

characteristics of the results. We learned about the size range of silicon carbide, which is not typical of sizes in the containment. And we talked about the fact that the particles seemed to have a very limited size range. So, they are rather like gravel, and we know that water goes through gravel. But if you put fines in the gravel, then it blocks the holes in the gravel and water doesn't go through it.

So, there is a bigger question in my mind about the extension of tests with silicon carbide for the very limited size range and using it to explain what happens in the reactor, whatever debris it is that comes in as particle size from containment. I have no idea what that is.

MEMBER ARMIJO: I agree with Graham there. I am more familiar with BWRs. But if you go to a blowdown of a BWR, you are going to have a lot of iron oxide floating around, crud floating around. To me, that would be a distribution of very fine particles of iron oxide, and that is what would be the thing that is interacting with fibers as well as other material that is in containment.

But there were no tests that I saw -- maybe they have been done in the past -- that says, yes, this is how much iron oxide we would expect to be floating

around in a PWR or a BWR later, but that wasn't done. I just worry that this silicon carbide isn't really representative of the particles. Well, maybe the particles aren't as important as the fibers, and we are concentrating on the fibers. That may be the answer.

But I think there are a lot of things that we haven't done, particularly the chemistry of this thing. We haven't considered the fact of what happens when this aluminum oxyhydroxide goes through the core, the intense gamma radiation and interacting with that material, whether it aids or makes the bonding worse. And I use the word "bonding" loosely because I don't know how this aluminum oxyhydroxide actually interacts with the bad fibers and the particles that create the blockage.

So, there is a lot of stretch in this thing, and maybe you are looking for some sort of an empirical limit that says, hey, no matter what happens, we can survive this kind of phenomenon.

MR. SMITH: Yes, there were quite a few points made. So, I don't know if I will remember or be able to respond to all of them. I will start out.

A couple of things. The basis for the surrogates that we use for head-loss testing, and we expanded that to this testing also, is that theory and

practice show that the smaller particulate creates higher head losses. So, we try to choose a surrogate that is on the smaller end of the size range because, in general, that is going to create higher head loss.

The other point I heard was about the iron oxide in BWRs. Now we are not dealing with BWRs here. that has been evaluated for BWRs. They track how much iron oxide they think -- they predict how much they are going to get in the torus or in the suppression pool. They control that. They are supposed to clean it out.

I am not as familiar with BWRs.

MEMBER ARMIJO: Well, it is on the fuel. It is not just in the torus.

MR. SMITH: Okay. I thought --

MEMBER ARMIJO: I mean, there is crud every fuel rod.

MR. SMITH: Okay. I thought the majority was generated from the torus, and there were a lot of programs done where the toruses were cleaned up and coated and things like that. And now, they actually track how much is in there. So, I am not aware of the ones, of the fuel rod issue.

But I am going to let Paul talk about a couple of --

CHAIR BANERJEE: We are actually jumping

ahead to sort of slide 15 or 14. But, if you like, we can deal with it, and we will go through those slides a little faster. If you want to defer the discussion, we can do it. It is up to you.

MR. KLEIN: We would like a chance to respond since there were a number of questions raised. And then, we will maybe move through the slides a little bit faster when we get to them.

CHAIR BANERJEE: Okay.

MR. KLEIN: But I guess, from our viewpoint, we don't see the particulate in these tests or the strainer tests as the critical thing. There was a little bit of a misunderstanding yesterday, I believe, when the particulate was shown as 10 microns plus or minus 2 microns. That was the specification for the nominal size of the particulate. If you actually look at the distribution that is in the proprietary reports, there is quite a range of sizes within that silicon carbide that was tested.

In general, the silicon carbide and the fiber by itself is not driving the head loss at all. It is really, particularly when you get down to the very low fiber loads, almost all of the head loss comes from the chemical precipitate.

MEMBER ARMIJO: And the fiber.

MR. KLEIN: And the fiber, I mean, yes. So, we don't think that if we change to a different type of particulate that it would have a significant effect. There have been a lot of strainer tests done with all different types of particulate, and that doesn't seem to be the controlling thing when it comes to the head loss unless you get into some of the problematic materials that Steve mentioned yesterday, like Microtherm and cal-sil. They act a little bit differently, but just a classical particle doesn't tend to drive head loss.

CONSULTANT WALLIS: These are hard particles. You know the size. You can calculate from first principles what the pressure drop would be through a bed of certain thickness if they are all the same size. It is just simply a --

CHAIR BANERJEE: But he is saying they are not. They are actually --

CONSULTANT WALLIS: But their size range isn't enough to fill in all the nooks and crannies in the strainer.

CHAIR BANERJEE: Well, we should look at that when we come to the proprietary.

MR. KLEIN: Yes, when you put chemical precipitate on it, with that size range, it clearly fills

in all the nooks and crannies because you get blockage.

CONSULTANT WALLIS: But not if there are extra particles. With extra particles, the chemicals have no effect. That is one of the things we heard about yesterday.

MR. KLEIN: Well, and that is one of the things I don't think we truly understand. It may be related somehow to compressibility of the bed and whether, if you lock the complete thickness of the fiber bed up with the silicon carbide, whether you possibly can't get the bed compression that you normally see with a fiber-only layer with chemical precipitate on top of it. But it is a question that we don't have a complete answer to.

CONSULTANT WALLIS: That is answered by measuring the compression, not by speculating.

CHAIR BANERJEE: So, what is the typical size of the chemical precipitate? Is it colloidal? Or is it really -- do we have an idea?

MR. KLEIN: Well, that is a good question.

CHAIR BANERJEE: We asked whether it was dendritic yesterday.

MR. KLEIN: One of the things that Argonne National Lab did with the WCAP, not the staff from the ICET tests, but they measured, as prepared, particulate

size before and after ultrasonic deflocculation, and they saw that the ultrasonic deflocculation had quite an effect on measure particle size. It looked like it cut the average particle size almost in half. It went from 12.5 microns down to 7.5.

CHAIR BANERJEE: This is the particles, not the --

MR. KLEIN: These are the WCAP --

CHAIR BANERJEE: Targets?

MR. KLEIN: Surrogates, yes. That size, I thought that is what you had asked.

CHAIR BANERJEE: Yes, that is what I asked, yes.

MR. KLEIN: It looks like that range is from 1 micron up to maybe about 30 microns.

CHAIR BANERJEE: Okay. So, you have got a fairly wide range of sizes.

MR. KLEIN: And I think under flow you might get even perhaps a wider range. Because we know it agglomerates in some cases in a bed, and in other cases I don't think it has a lot of shear strength. So, you could have quite a fine particle size.

I know some of the early work at the University of New Mexico and by LANL measured very small sizes of aluminum-hydroxide-type precipitates.

CHAIR BANERJEE: Okay. I think you have answered the question.

MEMBER ARMIJO: But, Paul, just for your reference, you know, the iron oxide that is in the reactor is submicron; it is really small stuff.

MR. KLEIN: Yes.

MEMBER ARMIJO: Ten microns would be huge for an iron oxide that is on the surface of a fuel element.

MR. KLEIN: They were measuring nanometer size particulate out at LANL, I think, with some of the early precipitates. So, I think it has to agglomerate before you even see a measured effect in some cases. Because I believe in very smaller size, it will just pass through a bed undetected.

MEMBER SHACK: And that is one thing we found at Argonne, is that we had other surrogates that were very fine that didn't give the head loss. The WCAP --

MEMBER ARMIJO: If they are too fine, they don't get through. I mean they get through easily. If they are some optimum size, they interact --

MR. KLEIN: In some of the vertical-loop tests at Argonne there was head loss without any visible precipitate layer. So, in there, they were

smaller-sized particles.

CHAIR BANERJEE: So, let's move on, and I am sure there will be more questions about particles.

MR. GEIGER: Okay. Slide 8. These were covered in the March '08 meeting. I am just going to touch on them briefly. Okay? There was a lot of all-day discussion on these analyses back then, which was the COBRA/TRAC analyses to show about the 99.4 percent blockage. You can get enough flow into the core.

And then, the question came, but how much of a uniform blockage you would need. So, then, Westinghouse went back and did a calculation modifying, putting a constant CD and increasing it to C1; you didn't get enough flow to make up for a boiloff.

However, the staff is not relying on any of these analyses for justifying the adequate flow to the core since we have demonstrated that you can, within a material actually less than what is in those calculations, due to the chemical precipitate because chemical precipitate is difficult to model into these because it is not linear or anything.

CHAIR BANERJEE: Well, in principle, what was done was to change the K factors --

MR. GEIGER: Yes.

CHAIR BANERJEE: -- of the inlets until you got inadequate cooling. So, you can find out what that K factor and the associated pressure drop was.

MR. GEIGER: And also, Research did some analyses for us to verify, do some calcs. They came up with a much thicker thickness than what we found in these tests. So, it sort of showed that the fuel could handle quite a bit of fiber until you got the chemicals, and that sort of changed everything. So, based on that, staff decided not to rely on these analyses at all.

CONSULTANT WALLIS: Can I say, does this mean the staff has changed its mind? Four years ago or something, we had a presentation and staff seemed to agree that, no matter what the debris bed did, because of this, everything was okay, as I remember.

CHAIR BANERJEE: That is when we asked those questions about what happens with more uniform blockage and things like that.

CONSULTANT WALLIS: We raised a question. So, you backed off from endorsing this approach?

MR. GEIGER: We are not endorsing it, but if you just think for a minute, what the calculations do show, that is, if you had a little bit of a flow path, you get water. So, now you have to have this leap of faith that you are going to have some area open. Now

do we think that this stuff is going to totally block everything out with all the dynamics that occur? It probably is not realistic, right? But we haven't gotten to the point where we can show -- well, we have these paths. We have these analyses and things and show that, yes, everything is okay. But, as it is, we are all saying that somehow -- and that is the plants may have options to pursue these other avenues to show other flow paths into the core, if our acceptance criteria becomes too burdensome to them.

So, I think, if you use reasoning versus what we have done, maybe we have kind of gone extreme. But how do you show? Because our job is to show adequate coolant flow.

MEMBER CORRADINI: May I ask a question at this point?

CHAIR BANERJEE: Sure. Go ahead, Mike.

MEMBER CORRADINI: So, the staff, the way you just described it, I think, at least seems reasonable to me. So, if I could turn this around, the staff is open to what I will call -- let's use the term that we are going to hear later today -- a risk-informed approach. But a risk-informed approach will require more data to support any sort of reduction or alleviation of what you have as your minimum acceptable criteria?

MR. BAILEY: This is Stewart Bailey.

I would say, yes, that is definitely the case. Another way to put this is we have taken a simplified test methodology which we believe to be the worst case, and we are open to refinements to that methodology. I see a lot of TH experts in the room. So, I think some of you understand how difficult it may be to actually make some of those refinements.

But there are arguments to be made that would lead to an uneven buildup of debris core or even clear spots, as was originally modeled.

MEMBER CORRADINI: Right, right. So, I mean, it could be a combination fo timing of when things occur, appropriate heat fluxes, water levels, flows, but you are open to all this. It just is going to have to have what I guess, from what I heard yesterday, a broader set of data that you can rely on to see whatever relaxation of constraints you have put on it?

MR. BAILEY: Yes, I would say that is true, and the justification behind it, the analyses to support the new set of assumptions.

MEMBER CORRADINI: All right. Thank you.

CONSULTANT WALLIS: Maybe we have that already. I mean, yesterday we heard that when the plate gets blocked, the flow goes around in the bypass between

the fuel assemblies. We had a lot of arguments about how this explained the data. So, there is a huge flow area. I mean, it is much bigger than 1 percent.

MR. GEIGER: Right. We may get into that a little later because there are some surprises there.

MEMBER CORRADINI: So, Graham, if I might jump in one last time, I agree with you. But it seems, then, you would have to do a test that you would show a scale effect where you went from one assembly to four assemblies, to such a level that you would show the bypass effect is maintained.

CONSULTANT WALLIS: I am not sure about that. If the bypass never gets blocked by fibers, then it is always there.

MEMBER CORRADINI: Right. But if you were in a meeting and they would claim that, you would want to see --

CONSULTANT WALLIS: I am not sure I would.

MEMBER CORRADINI: I would bet on it. You would like to see some of scale test that shows on one assembly you would see in four, et cetera.

CONSULTANT WALLIS: No, I don't think so. I think if you showed in the unit cell that the bypass got blocked, I would believe it wouldn't get blocked in another unit cell.

MEMBER CORRADINI: Okay. All right.

MR. KLEIN: I think we have observed tests where the whole assembly has been blocked, including the gaps.

CONSULTANT WALLIS: So, you clarified this question?

MR. GEIGER: Yes, we have.

CHAIR BANERJEE: So, let's keep that for later.

MR. GEIGER: I think we will touch upon that, too, in Steve's presentation.

Okay. Slide 9. Again, these are also covered with the heat transfer calculations for clad heatup for the rod and for blockage in the spacer grids. So, I don't know if anybody has any need to discuss. It was discussed in quite detail at the last meeting.

I am sort of going through this because I know the testing is the discussion that most interesting to everybody.

So, with that, I am going to turn it over to Steve, who is going to describe the testing that was done and the evaluation of it.

CHAIR BANERJEE: Do you want to close the meeting now?

MR. SMITH: We can read a few more slides,

if you want.

CHAIR BANERJEE: Okay.

MR. GEIGER: We are down to slide 26 --

MR. SMITH: Twenty-one.

MR. GEIGER: Twenty-one is the first --

CHAIR BANERJEE: The only problem is when you get questions which talk about proprietary information, you will need to segregate them and hold them then until that time.

MR. SMITH: That's fine. We did a similar thing yesterday where they went through a general description, and then we tried to hold questions.

CHAIR BANERJEE: Yes.

MR. GEIGER: If it comes to that, we will close it.

CHAIR BANERJEE: Yes. Go ahead.

MR. SMITH: Okay. What I was going to say is just a lot of the first slides -- I know you guys want to get to the data, right?

(Laughter.)

So, a lot of the first slides, the first several slides I have were discussed in relative detail yesterday. I know some people weren't here. So, I am just going to go through them quickly.

CHAIR BANERJEE: Yes, go ahead and do it.

Sure.

MR. SMITH: And let me know if you want me to dwell on anything.

This slide just is a list of what we are going to talk about.

This is a picture of a fuel assembly in the test rig, similar to what we saw yesterday. I actually think the picture they had yesterday was a little better because you could see the full assembly. I am not going to dwell on it unless somebody wants me to.

CHAIR BANERJEE: Well, I think maybe to point out a couple of things here --

MR. SMITH: Okay.

CHAIR BANERJEE: -- Steve, would be helpful to people.

MR. SMITH: Do you want me to point out --

CHAIR BANERJEE: Well, yes.

MR. GEIGER: This was a Westinghouse test assembly here.

CHAIR BANERJEE: Yes. Let me see if you have a --

MEMBER SHACK: The next slide, 12, probably has something you can work from.

MR. SMITH: I will just quickly show what this is. This is the plexiglass column. It was

plexiglass so you could see kind of what was going on in there during the testing.

Of course, this is the fuel assembly. These are spacer grids or mixing grids used to perform mixing under normal operating conditions.

Over here is a large mixing tank where the debris was added in there, and it is stirred by pump recirculation to keep the debris in suspension so that it all eventually can have an opportunity to transport --

MEMBER RYAN: It is going to help if you talk to the microphone.

MR. SMITH: Okay.

MEMBER STETKAR: Yes, you can use the mouse to point. It works pretty well.

MR. SMITH: Oh, okay. Thanks.

And just some others. These are how the water is transferred into and out of the fuel assembly. This would normally be the exit from the fuel assembly if it was flowing from the bottom up, as it was in most tests.

And then, you can see there are several spacer, I mean, pressure taps here where they could take differential pressure. These red tubes, there is a small valve. So, they can valve the differential

pressure in and out and take pressures at different locations across the grid.

CONSULTANT WALLIS: How big is that? Forty-seven gallons per minute through that little piece of plastic pipe is a pretty high velocity.

MR. SMITH: It is a pretty high velocity.

CONSULTANT WALLIS: It really is.

MEMBER ARMIJO: I think it was about 44.7, right, or 45.

CONSULTANT WALLIS: Forty-five, something like that. But it is really wiping through there.

MR. SMITH: I think it was about 2-inch plexiglass.

CONSULTANT WALLIS: Oh, it is 2-inch? Okay. It looks smaller than that. If it is 2-inch, it is --

MR. SMITH: This is 2-inch flexible, yes.

CONSULTANT WALLIS: It looked like 1-inch to me, but it is 2 inches?

CHAIR BANERJEE: There are a couple of points for the people who were not here yesterday. It is important whether the recirculation line is submerged -- apparently important -- or above the water level when the water returns to that tank. That is one thing.

Initially, this was above and then it was later submerged?

MR. SMITH: That is correct.

CHAIR BANERJEE: Correct?

MR. GEIGER: There were some modifications made to make one of the cross-tests, and then the tests came out different, and then there was a question to raise the line, and so on, yes.

CHAIR BANERJEE: So, there were some issues whether the inlet was submerged or not, the full configuration for the entrance for how the material was kept suspended, and the quality of the water. All of those were important.

MR. SMITH: The water-quality issue, we were sort of on the side aware that they were looking at doing some testing. The tests that you saw yesterday afternoon, we didn't really know that those tests were going on, and we have never seen the results of those before yesterday. So, that is something that we didn't have a chance to evaluate.

But we were aware of the submergence of the return line issue. That is something that testing was done on, you know, that we got results for.

Let me go to the next.

MEMBER SKILLMAN: What is the quality of

that water?

MR. SMITH: It is tap water.

MEMBER SKILLMAN: That is tap water?

MR. SMITH: Yes.

MEMBER SKILLMAN: At approximately room temperature?

MR. SMITH: In general, room temperature. Some tests were done up to 130 degrees.

MEMBER SKILLMAN: Had that water been sitting there for months and months before it was pumped through that assembly or was that relatively-fresh tap water?

MR. SMITH: In general, I think it was pretty fresh. I am not positive of this, but they were doing tests relatively quickly. So, they would drain it out and then put new water in it, when they were going to do the next test. So, it was probably a day or two generally between tests.

MEMBER SKILLMAN: Thank you.

MR. SMITH: Okay.

CHAIR BANERJEE: Also, it mattered what tap water it was.

MR. SMITH: The question was where the tap water came from. There was some correlation found between tap water from New Jersey versus tap water from

the Pittsburgh area. So, yes.

CHAIR BANERJEE: And deionized water. But, to some extent, this is to be expected because many things will change in these fine particle systems that, as were discussing the zeta potential has changed, depending on the water.

CONSULTANT WALLIS: Can you remind us about how big this is or you are not allowed to say? I mean, in terms of the cross-section of the --

MR. KLEIN: It is a full-scale cross section. I don't know if the cross-section is proprietary or not.

CONSULTANT WALLIS: Roughly, how big is it, just to get a feel for it?

MR. SMITH: It is about 8-and-a-half inches square.

CONSULTANT WALLIS: Just about 8-and-a-half inches square.

MR. GEIGER: A 17x17 fuel assembly.

CHAIR BANERJEE: I think the broad strategy that the staff had followed is to have a bounding situation. So, even though all these details, that is, the effects of tap water and this stuff, it doesn't matter to the first approximation because you have sufficient margin. At least that is how I see it.

MR. SMITH: That is what we are going to, hopefully --

MEMBER ARMIJO: That is the goal.

CHAIR BANERJEE: Whether you convince us or not, but that is the strategy? That is what I am saying.

MR. SMITH: That is our strategy.

MR. KLEIN: I think it has evolved into that strategy.

(Laughter.)

CHAIR BANERJEE: Maybe it wasn't that way to start with. Okay.

MEMBER ARMIJO: But, you know, the question is, what reactor water are you representing, whether you used deionized or tap water from this city or tap water from that city or reactor water from one plant or another plant. I don't have any good feeling that you have got a representative water, much less silicon carbide for iron oxide and aluminum oxyhydroxide, or something else.

MR. SMITH: Following an accident, basically, you are going to start out with borated water for all of the plants, right. And then, they have different buffers. And then, following an accident, they are all going to have different debris. And this stuff is all going to get mixed up, and some of it is going to go into solution. So, it is impossible to -- I

won't say it is impossible -- but it would be very difficult to test all the different possibilities.

MEMBER ARMIJO: Yes. So, in that situation, you look for something that is bounding or limiting or worst case or something.

MR. SMITH: Or you attempt to set an acceptance criteria where you believe that it is not going to have a large enough influence to prevent you from getting cooling.

CHAIR BANERJEE: New Jersey tap water.

MEMBER ARMIJO: What could be worse?

(Laughter.)

MR. BAILEY: The in-vessel test that we saw yesterday with different water qualities, that is the first time we have seen that done for in-vessel effects. But there are studies out there that have looked at the effect on strainer head loss.

CHAIR BANERJEE: And what did you find there? Did you find a significant effect?

MR. BAILEY: There was an effect, and there are certain water types that behave more similar to either deionized or buffered borated, deionized water. I don't have all the details here. I guess maybe I should not have opened that up, not having all the details in my pocket.

CHAIR BANERJEE: No, but that is interesting if you have some guidance from your head-loss testing for the sump screens, right?

MR. BAILEY: We do, and we try to factor in all of the information when we go and make our decisions.

CHAIR BANERJEE: Right.

MR. KLEIN: I would say it is a fair characterization we were somewhat surprised in some of the strainer tests that the water quality had the effect that it did. We thought with just tap water and particulate and chemicals that that would be a good representation in some cases.

It appears that the water quality can affect how the particulate and fiber interact. And so, we are still learning in that area.

MEMBER SKILLMAN: It seems like there is some information that would be worthwhile to be considered. At TMI, had we gone post-LOCA recirc, we would have had a venomous brew of coliform because the water that was in the containment was Susquehanna River at the end of a runoff spring with all of the upstream farm runoff.

When the plant goes on post-LOCO recirc, what is in that sump is not pristine water. It is not tap water. It is not drinking water. It is not

deionized water. It is filled with all of the filth that is in the building, the sawdust that was left by the carpenters when they were putting up scaffolding and everything else that can be, if you will, washed across the floor.

So, it seems that a representative water condition would be water that is somewhat like creek water or water that is truly industrially-dirty, not necessarily biologically-dirty, but industrially-dirty. What you have here is a fairly pristine test. While it gives data, if there is the hint that the water quality, the pH -- I will give an example. The water out of Lake Erie at Davis-Besse is a very different quality than the water out of the major rivers, and the hardness is radically-different. If you check the steam generator health, you will find that in many cases the steam generator health is related to what the raw water supply is for that particular plant.

So, if you theorize that the water quality has an effect on the capability of the debris to plug the assembly, then the source of the water needs to be looked at very carefully because it will not be clean.

MEMBER STETKAR: Dick, I guess I am a little confused about your question because I am not sure how we got river water in the containment here. I mean, I

do understand there is a variability in the quality of the RWST water and the reactor coolant system water from plant to plant. But, just for the record, I am not sure that we should try to be inferring that we have got river/lake water recirculating in the containment.

MEMBER SKILLMAN: I will make my point. What was in the basement of TMI was the result of a relief valve that failed to open that put Susquehanna River in the sump. Had we gone on recirc, then the water that would have been presented to the fuel assemblies would have been a large proportion of Susquehanna River water.

My point is what is in the reactor building sump when you go on recirc is not pristine water. It can be filthy.

MEMBER STETKAR: Well, that is why they are adding all of the guck -- I will use the technical term -- to it.

(Laughter.)

MEMBER SKILLMAN: And the gentleman said, "Hey, we understand that the water quality has an effect on the delta P." And I am saying maybe there needs to be a very clear understanding of what the range of quality may be, such that there may be some surprises if there is, if you will, an organic content to that water.

CHAIR BANERJEE: Yes. So, clearly, the water quality has, let's say the composition of the water has had an effect on the sump screen testing, that there is some information, as Stu was pointing out.

I don't know if that was used for guidance with regard to these tests or not. But I have encountered problems like this with a different situation, which is oil-water emulsions. What that water is is very, very important. You have to actually have a reference water to do experiments because it completely changes the behavior of the emulsion, depending on what the water is. So, it can be expected because you get absorption onto interfaces and things from the water, and it just needs very little.

But I think what we can do is we can come back to this, but I want to get through the process because, ultimately, as you say, maybe you have to have a strategy which is very bounding. But, hopefully, what you could show at some point is that the approach you are taking today takes care of a lot of these uncertainties. To the extent you can, that is really where we want to go.

MR. KLEIN: If I could just make a quick comment? The data that Stu was referring to, actually, on the strainer side became available to us after this

test program was quite mature. So, we did pose that question to the Owners' Group, how water quality might affect their results.

The data they presented yesterday, we saw for the first time yesterday, since it was relatively new. So, I think it is something we will be sensitive to, but I don't know that it would change our conclusion if our premise is that you don't build a filtering bed by limiting the amount of fiber.

So, I think it is something the staff needs to think about some more since we just saw the data. But, in the end, if the limiting fiber amount that is acceptable is such that you don't build a filtering bed and you don't believe water quality would affect that, then the conclusion may still stand.

CHAIR BANERJEE: Please go ahead.

MR. GEIGER: All right, the next one. Okay, 12.

MR. SMITH: All right. This just gives some information about how the tests were run. I don't know if you want to spend any time going over this.

CHAIR BANERJEE: I think the one thing, Steve, that came up yesterday was the order in which things were added. So, if you could just speak to that? Because in these tests, the particulates were added

first and then the fiber and then the chemical. Just sort of explain this a little bit and how they arrived at this.

MR. SMITH: Okay. The way that we arrived at that was we started out using the same addition order that we had used for strainer testing. That was based on previous strainer testing and, also, vertical-loop testing where this was seen to be the most conservative way of adding the debris. You ended up with the highest head losses if debris was added in this order.

We stuck with this. However, there were some tests done where some problematic debris was added after the bed was built and the chemicals were added. When that problematic debris, either cal-sil or Microtherm, was added, there was no significant additional head loss seen from that. So, that is the only additional information I can give you.

CONSULTANT WALLIS: Can I pick up on that? When the tests were done with these loops for the strainers, a very small amount of cal-sil had a huge effect on pressure drop. When you do these tests, it has no effect. It was even beneficial.

MR. SMITH: In one case beneficial.

CONSULTANT WALLIS: It seems to me you can't extrapolate experience from these strainer tests

to these tests. It is different. And yet, you are arguing that you can refuse to do any tests with a different debris order than this, simply on the basis of a couple of strainer tests. That is a very weak argument. It is counter to experience with cal-sil. Cal-sil doesn't reproduce the same effects in this test as in the strainer tests. Why would you think the order should be the same?

MR. SMITH: Well, I think that, realistically --

CONSULTANT WALLIS: We know order makes a difference in the other tests.

MR. SMITH: Order has been shown to make a difference in the other tests. Realistically, what is going to happen is that the debris is going to arrive in some sort of random and probably a mixed order, and that is --

CONSULTANT WALLIS: We know by the physics of the blowdown.

MR. SMITH: The blowdown or how things mix in the pool and then get pumped around. So, that is going to be random and it is going depend on where the break is, things like this.

So, we also accepted that sort of a homogenous debris addition for some of the strainer

tests because we felt it would be considered realistic. Now what happens when you put all the particulate in and then that is circulating, and then you add some fiber? You end up with a similar thing, although the particulate is always just available to be taken out by the fiber.

CONSULTANT WALLIS: We can come back to this later. I want to come back to this.

But it does seem to me that, if you are going to eventually come up with a criterion based on 50 tests which have a lot of whimsical characteristics, you cannot rely on a couple of tests at this minimum fiber thing which you are talking about. At this one you are going to decree is acceptable, you have to investigate these whimsical effects. You cannot rely on one or two tests to say it is okay to have "X" amount of fiber or less.

You have to, then, say, okay, we know water quality has an effect. We know water has an effect. We know all these things have effects. We have got to explore what effects they might have at that "X" value of fiber.

CHAIR BANERJEE: Are you saying, Graham, that let's say that at 15 grams per assembly they are not able to form a fiber bed?

CONSULTANT WALLIS: Well, we don't know

that under all conditions, do we?

CHAIR BANERJEE: Yes. So, you are saying that the order of addition may have an effect on whether you form a fiber bed or not?

CONSULTANT WALLIS: The water quality and effect of the chemicals will be different depending on how you put them in. We have learned all those things in these tests, and it has been very, very useful.

CHAIR BANERJEE: Yes, but I think --

CONSULTANT WALLIS: Since it has been a learning experience, now you have got to concentrate on validating your criterion.

CHAIR BANERJEE: So, we were not able to talk about the 15-grams-per-assembly tests yesterday because they were primarily done by AREVA. So, we don't know the nature of the beast that was formed at that. So, I think we will hold these questions until we see that, until we get to closed session and you can show us what it looked like under those conditions.

If you couldn't form a bed because there was not sufficient fiber, the order may not matter that much. So, as Graham says, it may be worth exploring. But you would convince me, if you can't form a bed, then --

CONSULTANT WALLIS: Well, they couldn't form a bed? Is that what you are going to say?

MR. BAILEY: I think we get into that in more detail when we can look at the actual test data in closed session.

CHAIR BANERJEE: Yes. Yes, let's hold it until then. That is what I am saying.

MR. BAILEY: Yes.

CHAIR BANERJEE: Right.

MEMBER ABDEL-KHALIK: I have a question about the fourth bullet.

MR. SMITH: Okay.

MEMBER ABDEL-KHALIK: Flow rate reduced if head loss approaches the facility limits. I understand that this is sort of a safety to protect the housing, which is plexiglass housing, and that limit is about one bar, roughly.

MR. SMITH: Yes.

MEMBER ABDEL-KHALIK: The question is, have you done any calculations to see how the housing bulges at one bar and how the change, the deformation in the housing at that pressure compare to the half inter-assembly gap that is, presumably, maintained?

MR. SMITH: No, we didn't. We haven't done any of those kinds of calculations.

MEMBER ABDEL-KHALIK: Do you have any rough idea how much bulging happens for a plexiglass housing

of this size at a pressure of one bar gauge?

MR. SMITH: I think that what would happen is, because of the way it is constructed, if it had any kind of significant bulging, you would start seeing leakage at the joints because it is glued and bolted together. But I can't quantify the amount of bulging that might occur for this. I mean, they did --

MEMBER ABDEL-KHALIK: A great deal of discussion took place yesterday regarding the role of that gap and how precisely they maintained the gap and centered the assembly within the housing, and made sure that that gap is half the distance between two neighboring assemblies. But the question is, how precise is that?

MR. SMITH: The main place where that gap is measured is at the bottom of the assembly. I think it is relatively well-reinforced at the bottom, right where the gap occurs because it is where the --

MEMBER ABDEL-KHALIK: Does Westinghouse have an answer to this?

MR. KLEIN: I would like to respond to the gap question. One test that was done when AREVA early on had complete blockage, including the gap, they did a test to double the gap. So, in other words, instead of having a half-gap around the periphery, they actually

blocked the periphery and then they put four corners of an assembly together, so that the gap was actually cross-shaped in the center of the assembly, but the width of the gap was twice the nominal gap in this type of test. They blocked that as well. It seems like a small change in the gap did not really affect the result in that case.

CONSULTANT WALLIS: Well, we can get to it. Maybe we need to talk about it in a closed session.

We had evidence that the gap was open, that the stuff went through it, I think yesterday.

MR. BAILEY: But two additional points there. I mean, the arrow is up at the top, but the maximum pressure, of course, is at the bottom. You can see that it is -- I am not aware of an analysis, but you can see that it is supported. The arrow is at the top still. If you put the arrow at the bottom of the assembly, that is where the maximum pressure drop would be, and you can see that it is supported by that ring.

But you will also see that, when we get into the test data, I think it will alleviate some of the concerns when you look at the --

MEMBER ABDEL-KHALIK: These are not rings. These are strips that are holding --

MR. GEIGER: Well, these are plastic --

MEMBER ABDEL-KHALIK: They are spheres.

MR. GEIGER: A sphere. In other words, yes, it was a sphere with a whole square cut out and split, and then there is a steel band around it.

MEMBER ABDEL-KHALIK: Right.

MR. GEIGER: Okay. That is the stiffener support.

Now the CDI, they made a --

MEMBER ABDEL-KHALIK: I am just curious if Westinghouse had done that calculation when they designed the facility.

MR. SMITH: There were problems with the facility, and I think they had to add things to strengthen it to present such failures as you were talking about.

CHAIR BANERJEE: Well, he is not talking of failure necessarily. He is saying that between the edges there can be bulging due to the pressure.

Now you have got this whole thing sitting within the cut-out circle, right, more or less? And then, you have got a steel band around it?

But somebody can do a calculation with those points of support and see if there was --

MR. GEIGER: But you would not actually see -- the spacer grids are pretty much where these reinforcing rings are.

CHAIR BANERJEE: Right.

MR. GEIGER: You know, I am sure the deflection is very small.

CHAIR BANERJEE: That part is constrained, yes.

MR. GEIGER: Where the spacer grids are is where the rings are. So, you are not going to have -- we didn't take measurements, but I can't imagine --

MR. KLEIN: I don't think we can answer that question, but if the question is getting at, is it possible as you get high dPs in these test assemblies, you get a lot of bulging and, therefore, bypass around the blockage, we didn't observe that because we saw a test where the flow assembly blocked, and they had to reduce flow almost down to zero in order to protect the fixture. I would think if there was a significant amount of bypass due to bulging in that case, that you would have seen a different type of behavior.

CONSULTANT WALLIS: They had to reduce the flow rate down zero?

MR. SMITH: Near zero in some of the tests.

CONSULTANT WALLIS: So, this is the penalty you might pay in a reactor case?

MR. SMITH: Well, we are trying to stay away from that.

(Laughter.)

CONSULTANT WALLIS: Did it suddenly happen that they had to reduce the flow rate to zero.

MR. SMITH: Relatively suddenly when chemicals were added.

MR. KLEIN: In some of the tests, as you add chemicals, you start approaching a dP that is beyond the capacity of the assembly.

CONSULTANT WALLIS: That is why it went down to zero. It didn't go down to zero because of the resistance.

MR. GEIGER: Well, it was 1450 psi, so it is not like they had much margin left.

CONSULTANT WALLIS: That is why a lot of the tests stop at a certain pressure drop.

MR. KLEIN: Yes.

CONSULTANT WALLIS: And then, that doesn't tell you how high they would have gone.

CHAIR BANERJEE: We don't want to know.

(Laughter.)

CONSULTANT WALLIS: The problem is the pressure drop is not very different from what is available in the hot leg break.

MR. KLEIN: That would be an unacceptable result, clearly.

CHAIR BANERJEE: Now let me change the subject. With this higher temperature test, were there also some tests done with head loss for the sump screens which indicated the effect of temperature? Do you have such data?

MR. SMITH: There were some tests done at a constant temperature of about 120 degrees. One vendor does tests like that.

The way that the bed was characterized, the flow through the bed was characterized, was actually by performing flow sweeps. You know, they decreased and increased flow to see what the characteristics of the bed were, to see if you could do a viscosity correction or not. So, head-loss testing, yes. And they did similar flow sweeps during a lot of these tests also to characterize the flow through the bed.

CHAIR BANERJEE: Because one of the surprising results that came out yesterday, we thought that there would be some advantage to the higher temperatures. In the experiments that were discussed yesterday, we didn't see any of these advantages due to reduced viscosity.

MEMBER CORRADINI: Sanjoy, can you say that louder? I didn't hear you.

CHAIR BANERJEE: At least to my

recollection yesterday, we did not see a reduced head loss at higher temperatures, which is what we would have expected to see if there was a reduced-viscosity effect.

CONSULTANT WALLIS: If the head loss were governed by the bed and not by the bypass.

CHAIR BANERJEE: Well, whatever. We didn't see this to the extent that I expected to see it.

CONSULTANT WALLIS: Moreover, we were told yesterday that the temperature of the sump water at the start of this process is about the saturation temperature of the containment, which is significantly larger than both of these values you have mentioned here.

MR. SMITH: That is correct.

CONSULTANT WALLIS: And we don't quite know what effect this might have on some of these whimsical effects we observed in the tests.

CHAIR BANERJEE: Now what Graham is saying is right, that if it is primarily controlled by the pressure loss in the bypass, then that is not as laminar as through the bed. So, the first approximation, Darcy's Law wouldn't apply. So, the viscosity effect would be much weaker. But it puzzled me yesterday.

MR. SMITH: I think that as far as the temperature effect, there was not a good study done. I think probably the best way to do that study would be

to start the test at one temperature, build the bed, and then change the temperature and see what happened. That way, you could get a true idea.

CONSULTANT WALLIS: Are you going to require that be done or are you going to have faith that what you test at 70 degrees F can in some way be used to protect what would happen at the containment saturation temperature?

MR. SMITH: The SE we are writing now is not requiring the test be done.

CONSULTANT WALLIS: Well, you seem to have a lot of faith that these things which are different about the real situation won't have an effect.

MR. SMITH: We have some knowledge about these things. You know, we don't have perfect knowledge. There are a lot of questions, we agree. I think that is explained in --

CONSULTANT WALLIS: The question that I am sort of raising in my mind when I write my report is, how certain do you need to be that your criterion X is going to work? It seems to me that there are lots of uncertainties. If I did some kind of an assessment, there wouldn't be a great deal of certainty that you would have with what you know now.

MR. SMITH: I think we need to be very

certain that our criteria is good and --

CONSULTANT WALLIS: Well, if you want 95/95, you ought to do 59 tests.

(Laughter.)

MR. BAILEY: One of the criterion that we are looking at here, as Paul has said several times, is ensuring that there is insufficient fiber, at least for the basis of this SE, due to the limitations you are talking about and due to our observations and some of the scatter in the data, the staff's decision or the staff's finding is based on there not being enough fiber to develop a filtering bed.

CONSULTANT WALLIS: You saw the fiber I brought in yesterday?

MR. BAILEY: That would really make a lot of these issues much more important.

CONSULTANT WALLIS: You saw the pile of fiber I brought in yesterday? I will bring it in again.

MR. GEIGER: Here, we have ours.

CONSULTANT WALLIS: Well, okay. I will bring in mine.

(Laughter.)

This is a thing like this.

MR. BAILEY: Yes, it is right there.

CONSULTANT WALLIS: I find it difficult to

believe that under all conditions you would never get this covered by this fiber.

MR. BAILEY: That is roughly 18 grams. It is not chopped as fine as the fiber that we used.

CONSULTANT WALLIS: It is not dispersed; it is kind of clumpy.

MR. BAILEY: That is actual baked Nukon.

MR. GEIGER: And to answer your temperature question, though --

CHAIR BANERJEE: That is a good thing to pass around. You should. Okay.

MR. GEIGER: The temperature down in the steam of the RHR is not as hot as in the sump, except for maybe the CE plants, right? So, you also have to consider that your temperature --

CHAIR BANERJEE: So, the reason that I was asking about the temperature is they did one test, if you recall. It was the second-to-the-last test or something, which was in that table that was shown. So, since it is proprietary, I am not going to talk about it in more detail.

But it seemed to show a similar pressure loss to the one at lower temperatures.

CONSULTANT WALLIS: The one on slide 21.

CHAIR BANERJEE: Whatever it was.

MR. GEIGER: You have to realize, what the effort was is, when the tests were at the point where the fiber loads were very low, and they were trying to take some of the conservatisms out of the test by testing at a higher temperature to get credit for that, maybe they had some tests that showed that boric acid or some of those things helped. In the end, it didn't prove out to be much of a benefit because the chemicals behaved so much different than anything else, that I think basically that is what --

CHAIR BANERJEE: And then, Graham raised the concern yesterday that, if you stop these things at a high temperature, you might actually make it into felt, which is a horrible thought.

MR. GEIGER: We did see felt.

CHAIR BANERJEE: Yes.

CONSULTANT WALLIS: You didn't do tests at high temperatures, so you don't know.

MR. GEIGER: I don't think you need high temperature, but --

MR. RULAND: Just one general comment. The staff's criteria is reasonable assurance of adequate protection. So, the staff doesn't have to have absolute assurance that this is going to work. The staff has a lot of practice and use in using that criteria.

Now if you go look at the administrative law, you are not going to find it defined anywhere, right, what is reasonable assurance. But we have a lot of practice. This is what we do all the time. So, our technical staff is applying that criteria when they make their technical judgments, and it is not perfect.

CONSULTANT WALLIS: This is why you allowed them to put those very, very small strainers in all those reactors?

MR. BAILEY: To get back to the question of the temperature, you're right, this is a test that they ran to try to recover some margin by running at a higher temperature, with the expectation that the viscosity would make a significant difference.

Note that there were also other changes to the loop made at the same time, as we had discussed yesterday. So, I am not sure that we have a direct one-to-one on different fiber levels with the exact same loop at different temperatures.

But you're right in your observation. They were attempting to show a significant benefit, and it did not materialize.

CHAIR BANERJEE: Yes, I was puzzled because, if it was laminar flow, it should show a benefit.

CONSULTANT WALLIS: Well, this just shows how uncertain the whole thing is. I mean, you are doing your experiment and you expect to see something, and you see something else. So, I don't know how sure you can be about the conclusion you are reaching based on a couple of tests.

CHAIR BANERJEE: Okay. Let's --

CONSULTANT WALLIS: Let's leave that for now.

CHAIR BANERJEE: Yes.

MR. GEIGER: Just to remind you about something, the question came up yesterday about the test at CDI where we blocked off with tape the bottom.

CHAIR BANERJEE: Right, right.

MR. GEIGER: I have those values for you, if you are interested. Okay?

CHAIR BANERJEE: Okay. We shouldn't show them now, I think. Or do you want to?

MR. GEIGER: Well, it was just that I don't have a slide of it, actually. I have an email that I dug up yesterday.

CHAIR BANERJEE: All right.

MR. BAILEY: Let's be careful about going into any numerical values until closed session. Okay?

MR. GEIGER: But those are not proprietary.

MR. SMITH: This was done by a plant, and I think they are not proprietary.

MR. GEIGER: Yes. These were done by Diablo Canyon. Okay?

At 5 gpm, with it blocked off, they had .4 psi. And when they ran it, they couldn't run it up to the full 120 inches because their tank was limiting, but at 37 gpm they had 4.3 psi. Okay? That was just flow around the point, the gap.

CHAIR BANERJEE: And this was just water?

MR. GEIGER: This was just water, yes. And then, they took the tape off and then they ran what --

CONSULTANT WALLIS: What was the pressure drop at 24.7?

MR. GEIGER: Well, it was 4.3 psi at 37 gpm.

CONSULTANT WALLIS: Thirty-seven gpm.

MR. GEIGER: They had to back it down because they couldn't --

CONSULTANT WALLIS: We can scale that up to whatever it would be at 45.

CHAIR BANERJEE: It gives you an idea. That is really all I wanted.

Okay. Let's go on.

MR. SMITH: All right. The next slide, we are on slide 13.

This just talks about the flow rates that were used during the testing.

CONSULTANT WALLIS: Are we going to accept that all flow goes through the core?

MR. GEIGER: All flow for the hot leg break.

CONSULTANT WALLIS: It maximizes the debris that goes through the core. But, then, as the debris builds up, the flow can be decreased.

MR. GEIGER: The flow may decrease.

CONSULTANT WALLIS: Yes, it keeps on decreasing until it gets to the boiloff maybe.

MR. GEIGER: However, with the way that the limits for the test were set, it was set before you would --

CONSULTANT WALLIS: I understand that, but --

MR. GEIGER: Okay.

CONSULTANT WALLIS: And if you want to cool the core, you only need 3 gpm.

MR. GEIGER: Are we talking cold leg or hot leg break?

CONSULTANT WALLIS: We are talking about hot leg. We would have to cool the core, no matter where it comes from.

MR. GEIGER: But we wanted to avoid -- but,

then, you have to spill over to tubes.

CONSULTANT WALLIS: Yes.

MR. GEIGER: And one criteria was not to spill over to tubes.

CONSULTANT WALLIS: Why not? There is not harm done if you spill over to the tube. It just comes around to the top of the reactor and cools it from the top.

MR. SMITH: Because it adds more -- although we already have a lot of uncertainty, we didn't want to add more.

CONSULTANT WALLIS: But here you have got a factor of 15 over what you need. You can accept it. It is fine. It is just I am puzzled by --

MR. BAILEY: The answer to that is a little bit more complex. You're right, in order to absolutely cool the core, you need about 3 gpm. That is based on a 20-minute decay heat and it goes down steadily -- well, not steadily, exponentially.

But having the flow over the tubes raises several questions. One is, does it actually make it to the core? That depends which steam generator it preferentially spills over and which hot leg the break happens to be in.

CONSULTANT WALLIS: It doesn't matter if it

gets there or not; that's true.

MR. BAILEY: The other problem with reducing yourself to a boiloff condition is, then, at that point you start to precipitate boric acid. In order to keep the two technical issues separated, which was the early intention of this project, we avoided getting ourselves into situations that left us in a pure boiloff situation.

CONSULTANT WALLIS: With the cold leg, you do have boiloff.

CHAIR BANERJEE: But, Stu, this is what I was saying about the 50 percent exit quality criterion that we accepted or discussed at length. Then, I think it was demonstrated that the boric acid precipitation problem was not limiting of that condition. I don't know, were you here for those discussions?

MR. BAILEY: I was not, and I would be interested in hearing that.

CHAIR BANERJEE: Yes.

MR. BAILEY: As we go forward, you will be hearing that we are looking to recouple these questions because they need to be. In most of the somewhat simplified analysis that is done to date, there is not, to my knowledge --

CHAIR BANERJEE: But it is a good leverage

to have.

MR. BAILEY: -- there is not credit for boric acid being taken out by exit quality.

CHAIR BANERJEE: Yes. Yes. But we could revisit it. At the moment, we understand the reason for your criterion. And I do agree that you don't know which steam generator it will spill over. So, it could preferentially spill over and just go out of the hot leg, which is where the lowest pressure point is likely to be. So, that is the steam generator which is most likely to spill over.

MR. BAILEY: So, this is an acknowledged conservatism.

CHAIR BANERJEE: Yes. So, okay. I think it is whatever you have done is reasonable there.

MR. SMITH: I think the point of this slide was to show that the hot leg break maximizes the amount of debris entering the core. There were other flow rates tested besides the 45 or 44.7 gpm, and you can see what those were.

And then, the next slide talks about the cold leg break, which is the boiloff condition that you were talking about.

And then, for either break, the hot leg or a cold leg break, all of the currently-operating PWRs

will go to hot leg injection. That is initiated at some range of time after the event occurs. That is to prevent boric acid precipitation.

CONSULTANT WALLIS: So, this excess flow spilling out the break is just equivalent to excess flows spilling out into the steam generator really. It is the same sort of thing.

But maybe we should just move -- you have done it, so let's stick with it.

MR. SMITH: Okay. We will stick with it.

I don't have a lot more to say about that unless there are questions on that.

CHAIR BANERJEE: Yes, but with the cold leg, I guess the thing that we should note is that you are taking a full split, so that only a portion of the debris is coming to the core, right?

MR. SMITH: That is correct, and it depends on the plant design what that split is. Usually, the CE plants have the lowest ECCS flow. So, they get the largest percentage of debris into the core. And then, plants that have a very large ECCS flow rate get a very small percentage of debris into the core, and more of it goes out the break.

CHAIR BANERJEE: Is the assumption that, when you recirculate back, is some of the debris taken

out successively by the screens?

MR. SMITH: Some of the debris is either taken out -- it has an opportunity to either settle or be taken out by the screens. You would think, depending on the efficiency of the filtering, it is going to take several pool turnovers to actually clean it, you know, before you are not getting any more of this recirculating debris.

CHAIR BANERJEE: And what is the typical turnover time --

MR. SMITH: For?

CHAIR BANERJEE: -- for the system, yes?

MR. SMITH: I think it is on the order of a couple of hours. It depends on the flow rate and the size. You know, for a CE plant, it would probably be much longer than that. But for a plant with a larger ECCS flow rate, one to two hours maybe.

CHAIR BANERJEE: Okay. All right.

MR. SMITH: Okay. This talks --

MR. GEIGER: Slide 15.

MR. SMITH: Slide 15 -- I'm sorry -- talks about the debris types. I think that was relatively well-covered yesterday and also this morning. I am not going to add anything to that.

Go to the next.

CHAIR BANERJEE: We would be here until tomorrow otherwise.

(Laughter.)

MEMBER ARMIJO: But it is silicone carbide. It is silicone --

CHAIR BANERJEE: It is not silicone.

MR. SMITH: That's a typo.

(Laughter.)

MR. SMITH: Oh, silicone carbide? Yes, sorry. Sorry about that.

Okay. This does show the target and the range of fiber sizes that were used during the test. The target was based on a mean of what had been collected downstream of strainers during fiber bypass testing.

MEMBER SKILLMAN: In the discussions yesterday with the PWR Owners, how confident are the owners that the debris type and fiber lengths are representative of their actual as-operating containments?

MR. SMITH: We talked about that a little bit this morning. I guess the fiber lengths, that was something that we thought that maybe we could add as a condition/limitation that would have them validate that their actual bypass size is close enough or equivalent to the bypass size that was used during the testing or

the fiber size that was used during the testing.

As far as the fiber type, Nukon is what was used during the testing. That is what the majority of the plants have in them. Some plants have other types of fiber which are similar, but not made by the same company.

MEMBER SKILLMAN: To what extent is the plant cleanliness discussed as part of the debris size?

MR. SMITH: I don't know that it is discussed at all as part of the debris size. The plant cleanliness is something that is important, as to how much debris is going to arrive at the strainer and then get past the strainer. But the sizing is not considered in the plant cleanliness.

MEMBER SKILLMAN: So, why would one conclude that the debris type that you have shown on page 15, on slide 15, is representative of a typical PWR containment?

MR. SMITH: I am not sure I understand the question. What was shown on slide 15 or 16?

MEMBER SKILLMAN: Well, 15 shows the types of debris, the fibrous debris, particulate debris, the problematic debris, and chemical debris. I guess I would offer that there is probably other debris in the PWR containments in this country that is not represented

on this slide.

And so, if the real goal here is to demonstrate that, through this WCAP And through the testing, that PWRs have really been covered, then the cleanliness of those containments that are in this target area needs to be a discussion item. As I said earlier, containments can have sawdust, raw dirt, leftover newspapers, magazines, notebooks, and anything else that gets carried into containment and isn't removed on closeout prior to restart. So, how is that addressed?

I am particularly concerned or particularly focused on sawdust because it is cellulose. When it compacts, it is like the felt that Dr. Banerjee reacted to. And I know this is very commonly left in containment because there isn't a scrubbing process on restart. It is a general cleanup, closeout, pick up the wires and the fuses and things that have dropped on the floor. But there isn't necessarily a mopping operation to make sure that what will be the exposed surfaces are clean.

MR. KLEIN: I guess I will start, and maybe you can jump in, Steve.

I think there is a range of cleanliness across the fleet, but, clearly, some plants do wash down containments each outage and maintain higher levels of

cleanliness than other plants.

I know that latent debris that is used in this strainer test I believe was based upon plant samples that LANL analyzed. I don't know if Bruce Letellier can comment on whether there was sawdust or something similar present in any of those samples.

Why we think this is representative, because I think it is not possible to test every combination of materials, as you pointed out, but probably the predominant fibrous insulation in the fleet is Nukon. They also tested cal-sil and Microtherm, which are other commonly-used materials. The chemical surrogate, although there can be a wide range of precipitates that form, we think they used the most conservative, and that is based on testing that Argonne National Lab performed for us.

The particulate, we agree that silicon carbide is probably not going to be found in containment, but the 10-micron size and the distribution was intended to try to model the inorganic zinc that was shown to fail at a 10-micron size particulate. So, there was some thought into trying to get representative samples here, although you can't test every material.

I think if we had tried to vary the materials, I am not so sure we would have seen

significantly-different results because in a lot of the controlling cases most of the head loss comes from the chemical precipitate. I think, provided you have a fiber bed, you will still see that same response.

CHAIR BANERJEE: I think one thing you should say as well, Paul, is that most of the debris that these ponds have to deal with is LOCA-generated by the jets. So, they come off the insulation, and the insulation which is most likely to cause plugging at least of the screens is Nukon. So, it is really erosion of the Nukon insulation and its fragmentation in the LOCA jets, as well as formation of these particulates from various sources.

CONSULTANT WALLIS: Does the insulation accumulate particulates over time when it is in containment for years?

MR. SMITH: The insulation is --

CONSULTANT WALLIS: It is jacketed, but is some of it not jacketed? It is all jacketed?

CHAIR BANERJEE: What I am saying is not true --

CONSULTANT WALLIS: So, it is just the superficial dust which would be collected on the pipes?

CHAIR BANERJEE: -- for containments which have different types of insulation, which some of the

new plants have moved towards, where containment cleanliness becomes a very large issue there because the insulation sources of debris have been cut down.

So, for example, if we go to some of the new reactors or some of the very clean reactors where they don't have Nukon, then the containment latent debris is very important. For example, we have had to put some limits on the latent debris that can be there in certain containments because of that reason. But most of the existing fleet doesn't have that issue. There may be a few reactors. Am I correct on that one?

MR. SMITH: Some of the reactors are limiting their latent debris --

CHAIR BANERJEE: Okay.

MR. SMITH: -- in order to meet the stringent requirements for the fuel. They are planning on doing that. You know, we don't have any input. Some have claimed a lower amount of latent debris, and they have programs in place in order to ensure that they maintain that cleanliness level. Some just assumed the bounding, or not the bounding, but the typical amount.

MR. GEIGER: They are all basically 200 pounds.

CONSULTANT WALLIS: When they do the strainers test, they put the particles in first?

MR. SMITH: Yes.

CONSULTANT WALLIS: Then, they all get bypassed?

MR. SMITH: The particles all bypass, that is correct.

CONSULTANT WALLIS: So, the bypass test has got to be different from that?

MR. KLEIN: The bypass tests typically don't include particulates.

CONSULTANT WALLIS: They don't?

MR. KLEIN: Yes.

CONSULTANT WALLIS: Why not?

MR. KLEIN: I think the logic was that, if you put a lot of particulate in there, you would clog your downstream filter with particulate before you grab the amount of fiber that might come through the strainer.

CONSULTANT WALLIS: Assume it doesn't affect in any way what the fibers do?

MR. KLEIN: We think it could have an effect, but we think that testing without particulate is likely conservative for most cases.

CHAIR BANERJEE: Well, I think there is a practical reason, which is that you can weigh the fiber and you know how much has passed through. But when you have particulates, then you don't know how much is

particulates and how much is fiber.

MEMBER ARMIJO: I think it is conservative, too, because the particulates seem to tie up the chemical.

CONSULTANT WALLIS: But think of the reactor case. Think of the reactor case. I mean, if the particulates arrive at the strainer first, they don't get filtered. They go right through. They go to the core. Then, they are available to stick to the rods. I don't know whether they make a bed or not.

CHAIR BANERJEE: They may or may not stick on the rods when they pass through.

CONSULTANT WALLIS: It is important, the sequence in which these things arrive. If the particulates are more easily transported than the fiber, they might get to the strainer first and they will go through. So, what do they do now downstream? Do they just go around and come back again? Or do they stick to the rods in some way because they are hot? It is boiling. We were told that, when there is boiling on the rods, the material sticks to them.

MR. KLEIN: I think that that is an assumption. I don't believe that that is based on running heated rod tests and seeing particulates stick to the rods. Our observation from the --

CONSULTANT WALLIS: Boiling. Boiling on the rods.

MR. KLEIN: Yes.

CONSULTANT WALLIS: The assumption was it was boiling. When the vapor evaporates, the liquid goes and the stuff stays behind.

MR. KLEIN: That is correct. We haven't seen evidence of particulates sticking prior to fiber being added to the test and starting to filter out particulates.

CHAIR BANERJEE: Well, I think we have a question with regard to how typical this distribution is. And what you have observed is that, from your strainer bypass test, that this is fairly typical of the size distribution that gets through, right?

MR. SMITH: That's right.

CHAIR BANERJEE: No matter what amount gets through, this is roughly --

MR. SMITH: Right. The amount is going to vary on how fast you put it in and the amount that you put in.

CHAIR BANERJEE: Right.

MR. SMITH: This is independent of that.

CHAIR BANERJEE: Yes. So, this is based on actual data?

MR. SMITH: Yes.

CHAIR BANERJEE: From your strainer tests or your sump screen tests?

MR. GEIGER: We didn't think of sawdust and things, but I guess these strainers are all totally submerged. Sawdust I guess would float, would it not?

MEMBER SKILLMAN: If it is wet, it will flow as a fluid. No, it won't float. It will be intermixed in the fluid, yes.

Mr. Chairman, I think the issue of latent debris is as much a part of this riddle as the test fiber distribution, and the latent debris ought to be mentioned in the Safety Evaluation. If some PWR owners have established a limit of "X" number of pounds, or whomever, or whatever --

CHAIR BANERJEE: Or very clean plants.

MEMBER SKILLMAN: -- it becomes significant. But that ought to be part of the closeout of this issue for us.

CHAIR BANERJEE: Yes, we have dealt with latent debris in the past in the similar way. Now whether it is correct or not -- we have dealt with it as fiber and in some cases we have even looked at hair.

MR. RULAND: We have had a number of discussions with the industry about latent debris, in

particular. As you can imagine, the industry is particularly worried about this latent debris because they are waiting for the inspector to show up during a post-refueling outage containment closeout and the inspector finds what have you. So, the industry is particular nervous about this matter.

What I can tell you is or assure you is the industry is laser-like focused on trying to figure out how to deal with this issue. So, how it actually is going to be solved or addressed, we already have limits about latent debris, but I can assure you that, based on our conversation with the industry, plant cleanliness is a result of GSI-191. They all recognized there is going to be what I would call post-closeout licensing basis maintenance issues that everybody is going to have deal with.

So, I would just leave that for your information.

CHAIR BANERJEE: I think Dick's question is, how do you deal with it in the testing? In the past, we have considered latent debris primarily through the latent debris which is affecting both in-core and out-of-core head losses as being fibrous of this type.

Now what you are saying that there is latent debris which are not typically fibrous, like sawdust and

things --

MEMBER SKILLMAN: Yes, sir.

CHAIR BANERJEE: -- but may become --

MEMBER SKILLMAN: Can become.

CHAIR BANERJEE: -- become fibrous. I think when it is fibrous, what we have found in the past is that is when it does the most damage in terms of head loss. Things which are fibrous, because they have this sort of behavior where they can get hung up and all tangled in knots and form beds, that is probably the worst.

If you have debris which is non-fibrous, it tends to have an effect, then, when it actually gets caught in these beds, and then, on top of that, when you have chemical precipitates. So, the sort of basis for this is almost like a worst case, trying to say that all of this or a significant portion of this is fibrous. So, it is like setting a limit.

I have forgotten exactly how we treated latent debris, what fraction was fibrous and what wasn't. It is back in my memory.

MR. SMITH: The assumption is 15 percent fibrous.

CHAIR BANERJEE: Okay.

MR. SMITH: If you go by the assumed value.

CHAIR BANERJEE: Yes.

MR. BAILEY: And the answer to that is, it is not specifically mentioned in the WCAP, but the latent debris, fibrous and otherwise, is evaluated in a licensee's transport calculations, what makes it to the strainer and then what makes it through the strainer and into the reactor vessel. So, the total strainer bypass is a value that the licensees would go and evaluate to show that they are within the bounds of the Topical Report.

MEMBER SKILLMAN: Thank you.

MR. GEIGER: It is in NEI 04-07.

CHAIR BANERJEE: All right.

MR. SMITH: All right. This is a picture you guys didn't see yesterday. So, it is relatively similar. It is a little bit more complicated than the one that was shown yesterday for the Westinghouse. This is the CDI test facility.

So, the one thing that is not shown here that they had -- and I am not going to turn around and talk away from the microphone -- is in the mixing tank they also have a propeller to keep the debris suspended.

MR. GEIGER: And also, a correction to I think what was discussed yesterday or a clarification. The way flow is controlled through the fuel assembly in

this test is there was a manually-operated valve that controlled how much went into the fuel and how much went back to the tank. So, it was like a three-way valve.

So, based on what the pressure drop was over the flow rate, to make N a constant flow, that was as the pressure went up across the assembly, they had to open that valve more to get more pressure on that side.

CHAIR BANERJEE: Now, you know, the system I saw at Westinghouse, when I visited, also had a bypass which wasn't shown yesterday. But I think what they said is that they only used that for the last few tests. Right?

MR. GEIGER: And I think they probably put a cold leg test initially when they had less flow, because it was a constant --

CONSULTANT WALLIS: Could we look at slide 17?

MR. GEIGER: Seventeen?

CONSULTANT WALLIS: It says, "Large openers, hot leg injection". Is hot leg injection upper plenum injection?

MR. SMITH: Hot leg injection would be similar to upper plenum injection, yes.

CONSULTANT WALLIS: Why? In all the pictures we have seen, it comes through the bottom. It

is driven by the steam generator head and it comes through the bottom of the core, and you test the bottom of the core. These arrows I think are wrong.

CHAIR BANERJEE: No, no, you can also do top or bottom.

CONSULTANT WALLIS: It could be, but what you have shown here is really upper head injection, not hot leg injection.

CHAIR BANERJEE: Yes, the small arrows are the bottom.

CONSULTANT WALLIS: That is cold leg injection.

CHAIR BANERJEE: Yes, the small arrows show you the --

CONSULTANT WALLIS: Because in hot leg injection, it normally comes through the bottom of the core as well. That is where all the tests have been.

MR. GEIGER: No, for when you hot leg recirc, you know, when you switchover.

CONSULTANT WALLIS: That is upper head injection?

MR. SMITH: No, for a hot leg break or a cold leg break you are correct. Both injections come through the bottom of the core.

CONSULTANT WALLIS: Oh, I see, it is the

injection; it is not the break you are talking about.

MR. SMITH: That is correct.

CONSULTANT WALLIS: I'm sorry. I understand that.

MR. SMITH: Later hot leg injection --

CONSULTANT WALLIS: Okay. That's okay. Thank you.

MR. SMITH: Okay.

CHAIR BANERJEE: The arrows simply show there are two directions you can --

MR. SMITH: And someone said that the arrows actually showed couplings or something in the construction, which didn't make sense to me. I think they actually do show hot and cold leg injections.

CHAIR BANERJEE: The small arrows, in any case, is the general --

MR. SMITH: That is the typical, the small arrows are the typical flow path.

CHAIR BANERJEE: Yes.

MEMBER ABDEL-KHALIK: So, the pump in this case is not a variable-speed pump, is that correct?

MR. GEIGER: Correct.

MEMBER ABDEL-KHALIK: And the flow is controlled manually?

MR. GEIGER: Manually.

MEMBER ABDEL-KHALIK: Somebody is watching the pressure drop --

MR. GEIGER: Yes.

MEMBER ABDEL-KHALIK: -- and the flow meter?

MR. GEIGER: That is how CDI did that.

MEMBER ABDEL-KHALIK: And they just adjust this bypass valve?

MR. SMITH: That is correct.

CHAIR BANERJEE: We were told yesterday it was a variable-speed pump.

MR. GEIGER: Yes. That is why --

MR. SMITH: I don't know if it is a variable-speed pump or not. But, reading the test report, they did control manually the flow rate.

MR. GEIGER: There is nothing to vary the speed of the pump.

MR. SMITH: And then, the other picture, the next slide is slide 18, for Mike's benefit. It is the Westinghouse test facility which was shown yesterday. It is probably the same picture. It is very similar.

The next slide, slide 19, just a repeat of how the tests were done, approximately how many tests were done. Over 60 tests were done. We have already

talked over a lot of this.

CONSULTANT WALLIS: Could you be clear, please, for the Committee here, about what you mean by p/f ratio? It seems to me that there is a particle flow rate and then the fiber flow amount, the particle amount and the fiber amount. There are two variables.

When you talk about p/f ratios, in almost every case you are keeping f constant and varying p. You are keeping the amount of fiber constant and you are varying p in almost all of these cases. That should be clear, that you are taking a cut across those two-dimensional surfaces.

MR. SMITH: There were several --

CONSULTANT WALLIS: No, but when you plot in these diagrams versus p/f ratio, you are really plotting versus p because you are keeping f constant.

MR. SMITH: That's correct.

CONSULTANT WALLIS: And that should be clear.

MR. SMITH: That's correct.

CONSULTANT WALLIS: Otherwise, it could be confusing.

MR. SMITH: I will make it clear when we look at those.

CONSULTANT WALLIS: Yes, I think we should.

MR. SMITH: There was a reason why --

CONSULTANT WALLIS: I think we should be clear about that because, really, what you are varying is f and p, and the p/f ratio is just some kind of a cut across there, right?

MR. SMITH: It is a way of looking at things.

CONSULTANT WALLIS: But it tends to get misleading when you talk about a limiting p/f ratio --

MR. SMITH: Right.

CONSULTANT WALLIS: -- which is only true for a certain f.

MR. SMITH: And it is only also true for a range of flow rates. You know, we saw the flow rate had an effect on that.

CONSULTANT WALLIS: All right. So, there tends to be this --

MR. SMITH: There are a lot of variables.

CONSULTANT WALLIS: There is a magic number of p/f ratio of 1, but it is not a universal thing because p and f both are varied. As long as that is clear --

MR. SMITH: All right. Let's go to the next slide No. 20. This is a little bit more talk about particulate-to-fiber ratios and sort of how we progressed from high particulate-to-fiber ratios down

to lower particulate-to-fiber ratios or at least searching for a different particulate-to-fiber ratio that would give us the most limiting head losses. It has been discussed a lot already.

This is really the last slide that we can show without closing our meeting.

CONSULTANT WALLIS: Now this evidence of 1:1 being limiting, I don't know if we got into the evidence, but since f can also be varied, it would be interesting to know how independent it is of f. 1:1 isn't a magic that is always true. Once you say hot leg, limiting flow rate here versus 1:1, it implies some kind of universality independent of how you vary other things.

MR. SMITH: I think it shows, what we will be able to see is that we saw a trend, and we tried to zero-in on what that trend would tell us.

CONSULTANT WALLIS: That helps, yes.

MR. SMITH: And it may not be absolute at every fiber load, but it is probably a pretty good, we think it is probably a pretty good indicator.

CONSULTANT WALLIS: Well, it is obvious, since it is 45-to-1 the cold leg, there is something that changes it, right?

CHAIR BANERJEE: I think the concern

yesterday was that that was shown to be limiting 1 is to 1 at relatively-high fiber loadings.

CONSULTANT WALLIS: Right.

CHAIR BANERJEE: Now whether it was limiting at low fiber loadings was the issue, I think. So, we can visit that once we get to the data.

I think this is a good time to take a break. We will come back and close the meeting. So, let's say we will take a 15-minute break.

MR. GEIGER: One clarification, though. The data we are showing is generated by Westinghouse, so it is proprietary to Westinghouse. I just want to clarify that.

MR. SMITH: That is on the next slide.

MR. GEIGER: Yes.

MR. SMITH: And then, in a couple of slides, it will be both data.

MR. GEIGER: Oh, both data?

MR. SMITH: Yes.

MR. GEIGER: Okay. Sorry.

CHAIR BANERJEE: When we close it and we come back, you give us instructions as to who should be in the room and who shouldn't.

MR. GEIGER: Okay.

CHAIR BANERJEE: Okay?

All right. So, we are off the record now.

(Whereupon, the foregoing matter went off the record at 10:29 a.m. and went back on the record in Closed Session at 10:49 a.m.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

2:02 p.m.

CHAIR BANERJEE: We are back now in open session.

I am going to have a few minutes of discussion in open session of the results that we have seen with regard to WCAP-16793. And then, we will go on to having a discussion which will be a presentation by STP on their risk-informed approach.

And then, I am going to after that close the meeting completely with only the staff and have a meeting in which we will discuss some other results that we have for load fiber-loadings, which is not available to anybody else than us and the staff.

MEMBER SHACK: Why are we going to comment before we see this wonderful information? It could change our minds.

CHAIR BANERJEE: Well, would you like to comment after?

MEMBER SHACK: I would rather see everything first.

CHAIR BANERJEE: Well, we could close it and then open it again. Would you rather do that?

MEMBER SHACK: All right.

CHAIR BANERJEE: Because the discussion we

are going to have has to be completely closed for everybody other than the staff and us. Okay?

In that case, I will do it the other way around. We will have STP come now, speak to us about their risk-informed approach. Then I will close the meeting completely, and then I will open it again for a discussion of the whole day, the whole situation, but mainly about what we are talking about, WCAP-16-whatever-it-is. I was going to have it now, but we will just defer it to the last item in the day. And hopefully, we will end by 5:30. All right?

Okay. So, if STP is ready, we will take you now. Thank you for coming to talk to us. We are looking forward to this.

Is it Michael who will be leading the discussion?

MR. MURPHY: Mike will begin, right.

CHAIR BANERJEE: Okay.

MR. MURPHY: I am going to start up our meeting and then turn it over to the different presenters, so that the folks that are technically responsible for the different areas can give you the best perspective.

CHAIR BANERJEE: Okay.

MR. MURPHY: That is our plan today.

CHAIR BANERJEE: This is an informational meeting only.

Thank you. Go ahead.

MR. MURPHY: Okay. I would like to thank you for the opportunity for South Texas Project. I am Mike Murray. I am the Regulatory Affairs Manager at South Texas. I have been at South Texas since the startup of both units, through commercial startup and all through my career, most of my career at South Texas Project.

I have met a number of you with different roles that I have had for South Texas Project, and it is a pleasure to see you again that I have met. So, thank you.

I would like to get into desired outcomes. What we did is we went ahead and set up desired outcomes. We thought it was important to understand what we wanted to accomplish and, hopefully, get alignment that that is where you would like to be also in accomplishment.

We are going to show how we are integrating a deterministic and a probabilistic model to assess the risk of fibrous insulation in containment. So, that is our desired outcome.

We also want to solicit, collect input, insights, consider the feedback that we can get from this

Subcommittee as we move forward with developing this process and project.

I will have to add that we have got real good feedback from the NRC already. We have got questions and comments for our use as we develop our processes as well, and we certainly appreciate that. So, it is going to be a valuable for us today and, hopefully, for you.

Our agenda, we are going to provide an overview and background, context, deterministic and risk-informed closure efforts.

Yes?

MEMBER STETKAR: You mentioned you had feedback from the NRC. Are you going to be submitting a Topical Report on this that the staff will perform a Safety Evaluation now? Or do you know what is going on in regulatory space?

MR. MURPHY: We currently are looking at a license amendment with that process.

MEMBER STETKAR: Okay. Thanks.

MR. MURPHY: Yes, sir.

Also, our plan is to provide a high-level project elements, physical models, and how we integrate that into the probabilistic risk assessment.

The speakers, I will be speaking to start with. And then, each speaker will introduce themselves

in more detail on their background and credentials as we go. It will be Ernie Kee, Bruce Letellier, Roldolfo Vaghetto, and Tim Sande, Gil Zigler, and Kerry Howe, and David Johnson will help us with the presentations.

Also with us we have a number of our team members here. We have John Crenshaw. He is our executive sponsor at the South Texas Project. He is Vice President of Projects and he is our executive sponsor.

We also have Steve Blossom, Scott Head, Craig Murry, West Schulz, Craig Sellers. We have Zahra Mohaghegh and Yassin Hassan. And we also have Alex Galenko with us as well. So, that is our team. And Steve Frantz is with us. So, that is our team members that are here with us today.

Next slide.

Okay. So, what we want to do is go through -- what we have given you with this slide is a preview of what we will be covering -- go back, please -- a preview of what we are going to be covering, so that you will see that we will hit your topic as we go through it. Just, if you will, keep focused and understand where we will hit the different areas.

So, we will be going over the background and overview. Ernie will be providing that. Bruce will be

giving us the integrative framework, thermal hydraulics. Rodolfo will give us that. LOCA frequency, Bruce will cover that for us as well. Debris generation and transport, Tim Sande will help us with that. Then, we will have strainer head loss. Gil will help us with that. Then, we will have the chemical effects. Kerry will be presenting that. Downstream effects, Tim will be helping us with that. And probabilistic risk assessment, David Johnson will help us with that.

I am not going to spend a lot of time on this slide. The importance of this slide and the next slide is to show you the team, the makeup of the team that we have got developed to work with this process. In this slide, if you will, notice the depth, the experience, and also the academic experience we have with the team in the different areas we are focused on.

Next slide.

I will give you a moment to look at it and digest it. Okay. Thanks.

So, with that, I will turn it over to Ernie Kee. He will give you an overview of the process as we are continuing to work through it.

MR. KEE: Thank you for the opportunity to speak to the Subcommittee.

I do have a protocol question. Do we have a time when we are going to complete this, just so people have an idea of the pace?

CHAIR BANERJEE: How many slides do you have?

MR. MURPHY: Fifty-four I think it is. Fifty-seven.

MR. KEE: Some of them are just pictures.

CHAIR BANERJEE: I think if we shoot for a couple of hours, that should be good.

MR. KEE: Thank you.

So, just a brief overview --

CHAIR BANERJEE: Maybe a 15-minute break in between.

MR. KEE: Thank you.

So, I am Ernie Kee. I, too, have been working at STP with the same timeframe as Mike, but I have done a lot of other things before that. My most recent experience there is in the Probabilistic Risk Assessment Group. My group develops, designs, and deploys all the risk applications at the plant. So, we are kind of the business end of the PRA.

In this GSI-191 project, I pretty much identified the resources, defined the scope of work, coordinate the work in the PRA, thermal hydraulics,

uncertainty quantification, the experiment program, and worked with the oversight group.

STP, I will just talk about STP real briefly. It is dual-unit station, a large Westinghouse PWR, large dry containment. We have independent ECCS trains. That is a little bit different than most plants you are probably familiar with. Our primary insulation is fiberglass, and we buffer the water in the sump with trisodium phosphate solid baskets.

We haven't been completely idle in response to GSI-191. We have installed very large sump strainers that are like 10 times larger than the ones that were originally installed in the plant.

One of the big advantages of this size strainer is the approach velocity is extremely low. It is like .01 feet per second.

We also, when we have success with all trains of ECCS, we terminate one train of containment spray that is analyzed. We do that as an continuous action step in our EOP.

CONSULTANT WALLIS: Excuse me. You said your primary insulation was fiberglass?

MR. KEE: Yes, sir, Nukon.

CONSULTANT WALLIS: How much of it is released in a large-break LOCA?

MR. KEE: We have that number. We have a sphere of influence.

MR. SANDE: In the zone of influence, it could be 15 to 18 hundred cubic feet.

CONSULTANT WALLIS: Eighteen hundred cubic feet? So, it is a large amount. Thank you.

MR. KEE: Yes, depending upon where it is.

CONSULTANT WALLIS: And what is the area of the strainer?

MR. KEE: Eighteen hundred square feet per train.

CONSULTANT WALLIS: So, there are several of these?

MR. KEE: Yes, sir. Three independent trains, yes, sir.

CHAIR BANERJEE: If it a whole 1800 cubic feet on that, it is a fairly thick layer of fiber, isn't it?

MR. KEE: We compute that.

MR. LETELLIER: Yes, it can be, right, depending on how many trains are running, if the thickness changes.

MR. GEIGER: And we have removed the cal-sil, probably the most offensive insulation, from the containment.

There is discussion about post-cleanup after maintenance in the containment refueling. We are very mindful of that at South Texas Project, and we have a whole crew go over the containment top to bottom every outage.

So, I think this has been hashed pretty well. It is a longstanding issue of GSI-191, starting back in the eighties. But I would just call your attention to the last two bullets, where in late 2010 the Commissioners issued the memo that appeared to indicate interest in a risk-informed approach to solve this and close this problem out.

So, STP likes that kind of initiative. By March of 2011, we completed assembling this team that was introduced earlier. The view was to assess the risk of the as-built, as-operated plant against an ideal plant and to continue by assessing that risk and understanding, use it in the risk-informed regulatory actions that we have done in the past that were successful.

CONSULTANT WALLIS: What is the metric?

MR. KEE: Yes, sir. Those are called out in the regulations, Regulatory Guide 1.1 --

CONSULTANT WALLIS: It is still Core Damage Frequency, isn't it?

MR. KEE: It is difference in core damage frequency and core damage frequency, and delta change in large early-release frequency and large early-release frequency. So, you look at the plant average values that, of course, have to meet limits and the change for what we are doing here.

CONSULTANT WALLIS: How is core damage defined in the long-term cooling? Is it a certain temperature, like 800 degrees, for a long period of time or something? What is it? How is it defined?

MR. GEIGER: Yes, we will talk about that, I believe, in the next slides. But, generally, we use success criteria that are guided by the deterministic experiments right now. For instance --

MR. SANDE: We are actually trying to use precursors to those fuel failure conditions as our thresholds of concern. So, we are not trying to shave the line and proceed to any sort of fuel damage at all.

MEMBER SKILLMAN: What is a precursor in that context, please?

MR. SANDE: We will look at some various performance measures, but a precursor to concern is challenging the net positive suction head that is required. A precursor to concern is accumulating fiber within a fuel channel, et cetera. There are several

generic things that you wish to avoid. As we look at the spectrum of scenarios, we can compile a range of those parameters for each one.

MEMBER SKILLMAN: Thank you.

MR. KEE: So, our view is that there has been a lot of good work done. You have heard some of that work that has been done over the last day and a half by the Owners' Group. But they are primarily deterministically-based. And as has been said in here, they involve conservative assumptions. And those conservative assumptions in this particular issue have proved to be very difficult to overcome. We would also like to understand the risk and the uncertainty in a quantified way.

I just listed some kind of highlights out of the regulation, 10 CFR 50.46, where what it is asking for is to look at the whole spectrum of LOCA, ensure that the most severe cases are included in that spectrum, use realistic kind of models to describe the behavior of the reactor system while accounting for uncertainties. And comparisons to applicable experimental data and uncertainties in the analysis methods and inputs, we need to be mindful of that, so that the uncertainty in the calculated results can be estimated. We show there is a high level of probability that the criteria set

forth will not be exceeded.

So, what we like about the risk-informed approach is it tends to address those kinds of concerns that we agree with, basically. We look at the full spectrum, and we will talk about this, of LOCA events, all of them that we can think about of different sizes and locations throughout the space in containment.

We are trying to model the physical processes as realistically as practical or possible. We are quantifying the probabilities and the frequencies associated with all these events.

CONSULTANT WALLIS: What is the difference between a probability and a frequency in the way you define them?

MR. KEE: So, right. We look at, for example, in LOCA, we look at the likelihood, given that we have an event. So, in the PRA you have initiating events, like a large-break LOCA. Given that we have a large-break LOCA, what is the likelihood at any given location throughout the plant that it will occur? What is the probability and the size?

CONSULTANT WALLIS: By frequency, you mean the same thing?

MR. KEE: It can be interpreted that way.

CONSULTANT WALLIS: Because core damage

frequency is really a core damage probability. It is your assessment, but --

MR. KEE: Sure. Yes, sir. Yes, sir.

MR. JOHNSON: Just to clarify, the two terms are very different. Frequency has terms of units per year, right? Probability is a chance or likelihood that a particular event will occur.

CONSULTANT WALLIS: I think they are usually interpreted in the same way.

MR. JOHNSON: And that is not correct.

MR. KEE: So, the regulation -- oh, I'm sorry.

MEMBER STETKAR: For the record, identify yourself.

MR. JOHNSON: David Johnson. Sorry.

MEMBER STETKAR: Thanks.

MR. KEE: Yes, sir. So, the regulation actually asks for frequency, change in frequency, in Reg Guide 1.174.

CONSULTANT WALLIS: If you have a probability of 10 to the minus 6, then the expected frequency is pretty darn small.

MR. KEE: Yes. Yes, sir.

CONSULTANT WALLIS: But if you have more reactors, you would say there is more frequency. Is

that what you would say, then? What would you say? How would you make a distinction? If you say the CDF is 10 to the minus 6 in the conventional way, how would you define frequency?

MR. JOHNSON: This is David Johnson again.

I am not sure I really understand the --

CONSULTANT WALLIS: Well, you said one was different, one was units per year and one was probability.

MR. JOHNSON: Right. I mean, a scenario you might thing of as a frequency: an initiating event occurs with a certain likelihood, a certain frequency, number of events per year. And then, we ask other questions to build the scenario. It might be the conditional likelihood that the break location is a particular location, oriented in a particular direction, generating a ZOI of a particular size. But, all together, the scenario is described in terms of a frequency, but the individual components you can think of in terms of likelihood or probability.

MR. KEE: I like to think of it as how many times you roll the dice. So, one pass through the PRA is one initiating event. How many times does that occur per year?

MEMBER STETKAR: Graham, think of the

simple thing of driving, you know, leaving your house. You might leave your house 15 times a day. That is a frequency.

CONSULTANT WALLIS: That's right.

MEMBER STETKAR: A conditional probability that you whack a car is a probability.

CONSULTANT WALLIS: I understand that. I understand that perfectly well.

MEMBER STETKAR: And there is an uncertainty about both of those.

CONSULTANT WALLIS: If you are going to assess how likely it is that I will leave my house today, that is a probability.

(Laughter.)

MEMBER STETKAR: Yes.

CONSULTANT WALLIS: But if you measure how many times I actually do, that is a frequency.

MEMBER STETKAR: That is correct.

CONSULTANT WALLIS: It is quite different. One is a state of knowledge and one is a measurement.

It seems to me all of this is frequencies, is a probability, unless you have a frequency which is actually measured, isn't that true?

MR. LETELLIER: So, as a point of clarification, there is, indeed, an unfortunate dual

usage for this word.

CONSULTANT WALLIS: That's right.

MR. LETELLIER: In general, we use time rate frequencies to talk about the initiating event in terms of the number of times per year. Whether it is normalized to a single plant or a population of plants, it is an annual time rate.

Almost everything else in a PRA, and, indeed, within our analysis, is really a conditional probability.

(Interruption by noise on phone line.)

If I could repeat a little bit of that? Generally, the initiating event is described in terms of its time rate of frequency, number of occurrences per year. Everything else is generally a conditional probability, which is simply a proportionality, a fraction of occurrences on a relative basis.

And I specifically use the word "conditional probability" because that presumes an initial plant state; for example, like the size of the LOCA. A medium break has occurred, and conditioned on that, follows the events scenario.

CONSULTANT WALLIS: But if I toss a coin a lot of times, I get a frequency. From that, I estimate a probability? Is that what you are saying? Because

you would have to do the experiment to make the frequency. But I won't quibble about it, but it seems to me that there is an overlap between the way you are defining things here that is very fuzzy and it doesn't really matter.

MR. LETELLIER: It is. It is a traditional debate.

CONSULTANT WALLIS: Yes.

MR. KEE: So, anyway, in the risk-informed approach, we quantify the uncertainties. We are mindful in our work here particularly to include the possibility of extreme events, and the uncertainty and experimental and operational data -- we are developing that even as we speak now -- are used directly in our quantification, our uncertainty quantification, to characterize the uncertainty.

And guidance for these levels of acceptable levels and ways to deal with them are given in, as I mentioned, Reg Guide 1.174. And the risk goes from unacceptable to very small. That is defined and methods to evaluate uncertainty and --

CONSULTANT WALLIS: Did you hear the discussion we had this morning? Does characterizing the uncertainty apply to the kind of stuff, the kind of data we heard about this morning, the kind of stuff

about sump blocking with various amounts of debris? You can't even predict the mean without worrying about the uncertainty. There is no analytical method that predicts this blockage of a sump screen or the core. There is no analytical method that is accepted by the NRC. So, how are you going to get this prediction and uncertainty?

MR. KEE: I think as we walk through the different models and physical processes that we have modeled in our uncertainty quantification, that may be helpful. I think we will discuss that and just --

CONSULTANT WALLIS: I will wait for that.

MR. KEE: Yes, because we don't have much time.

Our project objectives are using a risk-informed approach to provide technical basis to close the safety issues related to this GSI-191 by the end of next year.

We want to analyze and implement the necessary licensing requirements needed to support an exemption. So, what we have drafted so far is the exemption from certain requirements of 10 CFR 50.46.

I have said already that we are using Regulatory Guide 1.174 as the basis for making these quantitative judgments. And I have already mentioned

how we are going to look at what we are going to look at in terms of the delta CDF and the delta LERF.

I would like to now turn over the presentation to Bruce Letellier, who is going to introduce the framework that we are working in, and so forth.

MEMBER SKILLMAN: Before we do that, please, may I ask this question? To those two bullets, do you see or envisage that you will be required to make plant modification in order to reach your goal of closing your response to 191 by the end of 2013?

MR. KEE: I would say we would, when we have our methodology in place, we will have a mechanism whereby we can evaluate the risk/benefit for making plant modifications. There are some things that are pretty clear that we would probably undertake at this time or at sometime in the near future that would help reduce, say, chemicals, aluminum oxyhydroxide, the presence of that, if it shows up.

But we don't know that. We don't have the framework in place to evaluate what kinds of modifications make sense in terms of benefit to the plant.

MEMBER SKILLMAN: I think you are saying it is likely that you will be making some, but you have not

identified specifically what they are at this time?

MR. MURPHY: The process will take us through to those decisions, I think is another way of saying it --

MEMBER SKILLMAN: Okay. Thank you.

MR. MURPHY: -- as we go through it.

MEMBER SKILLMAN: Okay. So, this will not just be an analytical drill. You may end up with hardware changes or chemistry changes or other such things?

MR. KEE: Operational, yes.

MEMBER SKILLMAN: Thank you. Okay. Thanks.

MR. LETELLIER: So, as Ernie said, my name is Bruce Letellier. I work at Los Alamos National Lab. For many years, I had the pleasure of working with the staff on both regulatory and research issues related to GSI-191, and now I have joined the industry, hopefully, to proceed with the novel closure opportunity. I am pleased to be back before the ACRS Subcommittee again.

Today we would like to emphasize the high-level framework of what we are trying to accomplish because there are some relatively-new features to a risk-informed closure that you may not be familiar with from the deterministic approach.

This figure on slide No. 13 breaks down the two approaches into two parts. As Dr. Wallis asked, the ultimate performance criteria are changes to core damage frequency and large early-release frequency. Those are traditionally quantified by an existing plant PRA. We are fully intending to interface with the existing PRA with relatively minor modifications.

So, the purpose of the supporting uncertainty assessment, which is shown on the lower box, is as a modules. We are essentially populating the sump availability criteria.

You can see conceptually how the information flows from detailed assumptions about the plant physics and the scenarios involved with the LOCA to populate the branch fractions related to the PRA. We have done this intentionally so that we can take advantage of existing plant analysis tools with relatively few modifications and, also, maintain transparency.

So, although this box that is labeled "CASA Grande" may become relatively complex, it collects all of the information that subject matter experts familiar with GSI-191 would be commonly familiar with. In essence, the CASA Grande module functions like a fault tree in the PRA context.

CONSULTANT WALLIS: But what do you put in these boxes? You have to put some knowledge into these boxes.

(Laughter.)

MR. LETELLIER: Indeed.

CHAIR BANERJEE: Or lack thereof.

(Laughter.)

MR. LETELLIER: Indeed. So, the overall objective is to combine frequency -- and by that, I mean an annualized rate -- combined with the uncertainty about what the outcome of each of these modules is, and folding that towards a prediction of the performance metrics, which is a quantifiable measurable value such as delta P across the strainer.

So, these are all relatively-new approaches to a risk-informed process.

CHAIR BANERJEE: How are you going to establish all those distributions and things?

MR. LETELLIER: So, traditionally, it is done through formalized uncertainty quantification. In some cases, you do an expert elicitation. In some cases, you appeal to very minimal available data. In some cases, you use non-informative priors in a Bayesian context, which I think Dr. Wallis --

CONSULTANT WALLIS: There is no way you are

going to predict the pressure drop on the in-core debris by expert elicitation or by one of these other methods.

CHAIR BANERJEE: Even Dr. Wallis, who is an expert, I don't think would tender an opinion on this.

(Laughter.)

Or if he would, he is not the Dr. Wallis I know.

(Laughter.)

CONSULTANT WALLIS: Well, I give an opinion with a confidence.

(Laughter.)

MR. LETELLIER: That is the point of having the framework, in fact, is to accommodate the uncertainty in your confidence and propagate that through in a formalized manner, so that you can diagnose what the principal issues are, what the driving factors truly are.

The advantage of the risk-informed process is to put things in a balance, in a relative perspective. So, in fact, if it turns out that small breaks have a much, much higher frequency that are causing some particular concern, they could be a higher-risk contribution than the double-ended guillotine break with a very, very low annualized frequency.

CHAIR BANERJEE: So, we have a feeling that

15 grams of fiber at 20 you block the core. I mean, almost any break is going to produce enough fiber. So, what are you going to learn from that?

MR. LETELLIER: I am not sure that is exactly true. If we look at some of the details over thousands of break scenarios, you can see that there is a very large spectrum of latitude.

CHAIR BANERJEE: But there are only two or three data points. How are you going to use that over thousands of break scenarios?

MR. LETELLIER: The way that it is implemented right now is on the basis of a decision criteria. So, this morning, in fact, we have seen a range of evidence supporting or refuting 15 grams, 25 grams, or what have you. That becomes a threshold of a concern, as I mentioned before. If there is continual debate upon what that value should be, then those uncertainties are folded into the probability evaluation. And I can show an example of that, if you like.

MEMBER ARMIJO: Is that CASA Grande model, is this it or are there other boxes that are going to be added?

MR. LETELLIER: There may well be additional boxes. This is just a cartoon --

MEMBER ARMIJO: Okay.

MR. LETELLIER: -- to show how the information flows through the process.

MEMBER ARMIJO: But, for example, chemical effects, you are missing the box of the state of the core and the reactor right after the LOCA, all the particulates in there, the chemistry, and all those things changing. You are going to put that in?

MR. LETELLIER: There is a more detailed version of this that we can discuss --

MEMBER ARMIJO: Okay.

MR. LETELLIER: -- if time permits.

MEMBER ARMIJO: All right.

MEMBER BLEY: Bruce, you make this sound like it is a code or something. Is that what it is? Or is this just a structure that you are laying out for us about where you will search for evidence and bring experts to bring their judgments with a confidence, as Graham says, to the table?

MR. LETELLIER: No, indeed, it has become a utility. It is an operable code.

MEMBER BLEY: It is a real thing right now?

MR. LETELLIER: It is. It is not just vaporware. It is more than an acronym.

Essentially, the purpose of CASA Grande is

to automate the hand calculation that any plant engineer would have to do now. You have to make assumptions about the location of the break, the amount of debris, the transport factors, et cetera. And this enables the rapid evaluation of many thousands of scenarios to support the statistics evaluation. And as Ernie said, it will also support the diagnostic exercise of prioritizing our response.

MEMBER BLEY: Are you going to get into the structure of this as you go on?

MR. LETELLIER: I hope so.

MEMBER BLEY: You will? Okay, I will wait.

MEMBER SKILLMAN: But let me ask this: how can this model be used, for instance, by a simple plan like Prairie Island, two loops, two reactor coolant pumps, 126 fuel assemblies, versus a large machine like you have at South Texas Project 1 and 2?

MR. LETELLIER: Well, we are intending to build this on the most generic basis possible to accommodate everyone's interests. So, there may be a few plants where this is impractical, perhaps because they don't have a fully-mature PRA, perhaps because they have yet to construct an as-built CAD model. There are certain elements that facilitate this analysis that some plants may be more or less prepared to accommodate.

MEMBER SKILLMAN: So, it is more a function of, if you will, the owner-operator's maturity in its data versus the size of the plant?

MR. LETELLIER: I don't think it has anything to do with the size of the plant because the assumptions that have to be made for each of these boxes are very generic. They already have plant licensing submittals.

I thought you were asking about the flexibility of the tool for accommodating the broader interests?

MEMBER SKILLMAN: You have answered my question. Thank you.

CONSULTANT WALLIS: So, you start with LOCA break frequency. Maybe there is some hope of predicting debris generation from knowledge of experiments of jets hitting pipes, and so on. Debris transfer down to wherever it is going is a pretty iffy thing. And then, you have got to proceed through the other parts of this thing. I am just wondering how big this Grande is, how "grande" it is, you know.

(Laughter.)

It looks like an enormous task to really do.

MR. LETELLIER: But you should recognize, the Subcommittee should recognize that decisions

regarding each of these steps are presently being made in hand calculations for licensing purposes.

CONSULTANT WALLIS: I think that the NRC has allowed various conservative assumptions.

MR. LETELLIER: That's correct.

CONSULTANT WALLIS: That is very different from realistic analysis.

MR. LETELLIER: So, one of our intentions is to describe the spectrum of uncertainties which accommodate the deterministic assumptions. Both the best estimate and the extreme tails should all be in the distribution of uncertainty. If we do the statistics properly, it will be propagated without bias and it will include the full spectrum of outcomes.

CONSULTANT WALLIS: Extreme tails are very difficult. The probability of all the debris generated going into the sump, for instance, it is not zero --

MR. LETELLIER: That's true.

CONSULTANT WALLIS: -- but it is pretty minute.

MR. LETELLIER: Indeed.

CONSULTANT WALLIS: And you tell me what it is.

MR. LETELLIER: We have consciously constructed a non-uniform sampling scheme that

emphasizes the tails. I have an example in the package, in case we don't get there.

CONSULTANT WALLIS: Because the 15 grams will be a tail, presumably, in more ways than one.

MR. LETELLIER: You can define uncertainties into broad categories for various convenient purposes. We have notionally divided that issue into a threshold of concern. So, if there is continuing debate about what the value should be, that should be introduced as a probability distribution on your acceptance criteria, not necessarily on the physics of the event. All of the physical variability, the uncertainty about the phenomenology gets folded into the range of performance metrics.

CONSULTANT KRESS: Now NUREG-1150 quantified uncertainties by using expert judgment to get the distributions. Is that what you have in mind here?

MR. LETELLIER: In some cases. The very first example is the LOCA frequency --

CONSULTANT KRESS: Yes.

MR. LETELLIER: -- which we will discuss. That is a very good example of expert opinion.

MEMBER SCHULTZ: Now the boxes you have shown here or the diagrams you have shown here, all the arrows go in one direction. In other words, feedback

effects are not yet accounted for, and in some cases you might consider those in the modeling, if you get into the detail modeling of some of what you have described?

MR. LETELLIER: Oh, absolutely. If we are aware of those complications, they should be factored in. At the moment, in its current implementation, the parameters are largely assumed to be independent, with a couple of notable exceptions. There are explicit dependencies on the size of the LOCA, small, medium, and large, for obvious reasons. The plant responds differently to each magnitude of event. You have different safety systems.

Likewise, you will have different ranges of phenomenology that interplay. For example, the sprays at South Texas are not expected to operate during a small break, and that becomes one of these interactions that affects chemistry and sump pool temperature.

CONSULTANT WALLIS: Debris transport and all sorts of things.

MR. LETELLIER: Indeed, indeed. That's right. So, any of these parameters can be specific to the break size category.

So, I think we have covered the analytic objectives on slide 15. Let's move on to 16.

I haven't yet defined the acronym. CASA

Grande stands for Containment Accident Stochastic Analysis.

CONSULTANT WALLIS: Where did the "Grande" come from?

(Laughter.)

MR. LETELLIER: Because it sounds better than CASITA.

(Laughter.)

But, honestly, a large dry is about the biggest large house that you can imagine.

(Laughter.)

Being from New Mexico, it is in deference to our local culture.

So, the objectives of this utility function are to propagate the uncertainty in physical parameters from the break initiation all the way to potential core damage precursors. And specifically, we are looking at these four. It is not an exhaustive list, but it is a relevant list to current debate.

The strainer head loss was the initial scope of GSI-191. It has sense then been generalized to look at core blockage in terms of grams per fuel channel of fiber. It now includes boron precipitation thresholds, and it includes air ingestion from a degasification, dissolved air being released at the sump screen.

CONSULTANT WALLIS: So, this implementation in a computer model, when you have only got a few experiments, you have to somehow construct some analytical form to put in the computer, don't you?

MR. LETELLIER: That is one approach to abstract the data. The other approach is to have either one or more than one physical model, like a head-loss correlation, which you can compare and contrast across the range of input variability.

CONSULTANT WALLIS: If such a thing can be derived from the kind of data we have seen. If you have sparse data, it is difficult to get these models, isn't it?

MR. LETELLIER: That is true. Yes, in the common jargon, UQ, Uncertainty Quantification, 90 percent or more of the work is specifying the input distributions. The mechanics, the statistics of propagating the distribution, that is the fun part, and that is what CASA Grande does right now.

CONSULTANT WALLIS: But, then, what tends to happen is people say, "We have no idea what the distribution is. So, we will just assume it is uniform," or something, which doesn't really put in any information at all.

MR. LETELLIER: But you are very familiar

with maximum entropy constraints --

CONSULTANT WALLIS: Yes, I am.

MR. LETELLIER: -- that provide information even if there is no data.

(Laughter.)

If it has to be a positive value, for example, there are always constraints on the information --

CONSULTANT WALLIS: Is there life on Mars? Do you know that? No.

(Laughter.)

There is no data, but I want an assessment.

(Laughter.)

MR. LETELLIER: That is the life of a contractor.

(Laughter.)

CONSULTANT KRESS: These damage precursors, are you going to have limits to those, set values on them?

MR. LETELLIER: Indeed. The best example is a strainer head loss.

CONSULTANT WALLIS: Let me continue with life on Mars. It is either there or it isn't. So, they are equally-likely outcomes, and there is a 50 percent chance there is life on Mars, as a maximum entry

distribution.

MR. LETELLIER: But opinions vary.

(Laughter.)

So, when you are setting up the probability, you will get a range of answers.

So, to answer your question, Dr. Kress, the strainer head loss is a very good example. Every plant has a net positive suction head that is required.

CONSULTANT KRESS: Right.

MR. LETELLIER: Now, if you exceed that head loss, it doesn't immediately lead to core damage, but it is an important threshold of concern. We will be selecting that as our threshold. Anything that exceeds that will be assigned to failure. And similarly, we need a similar perspective on each of these performance metrics.

We have talked about the objectives of having a diagnostic platform. Essentially, my personal goal is to put in context all the information that we do have available, so that we can interrogate it in a systematic way to help prioritize our response actions, whether that is a hardware change to the plant or simply a defense-in-depth action that we take, because we have learned it is a good idea.

MEMBER SKILLMAN: Excuse me. Are those

four items that are on the first bullet the only four precursors that you are talking about?

MR. LETELLIER: At the moment, that is correct.

MEMBER SKILLMAN: Okay. Thank you.

MR. LETELLIER: Okay? Recall that we have finished an initial quantification with very rudimentary tools. Year one was spent in methods development. Year two is being devoted to research efforts to fill in the blanks. And this is a just a status of where we are at right now.

CHAIR BANERJEE: So, let me understand core blockage. If, as the staff maintains, that there is a cliff at 15 grams per assembly, that means that if you have more than 15 grams of fiber per assembly, you say the core is going to fail with 100 percent probability?

MR. LETELLIER: It that, indeed, is the threshold we choose, then that is the implication.

CHAIR BANERJEE: Well, the threshold you choose has to also be the threshold which the staff agrees to, right?

MR. LETELLIER: Yes.

CHAIR BANERJEE: Or are you going to argue this 15 grams away in some way? I am trying to understand what your strategy is, actually.

CONSULTANT KRESS: You have the probability or a distribution in that.

CHAIR BANERJEE: So, we are going to say that maybe 30 grams will work? Is that --

MR. LETELLIER: Let's look at it from this perspective. Given the spectrum of break locations in containment, I can itemize, systematically itemize, how many are on the hot leg and what size distribution that they have. And we can fractionate the spectrum of concern, so that we are debating 15 grams relative to the appropriate portion of the accident space.

CHAIR BANERJEE: Yes, but let's talk about hot leg right now. Okay? Fifteen grams per assembly is sort of a cliff, as far as we can see right now. It is my word "cliff". The staff avoids using that. But, nonetheless, that is what it is, from all the evidence we have seen.

(Laughter.)

So, it doesn't matter how the 15 grams gets there. I am not interested in that. If it gets there, are you going to assign 100 percent probability that the core will be damaged?

MR. LETELLIER: As I said, if, indeed, that is the threshold, then if it is exceeded, we would assign that to failure. However, let me observe, I wasn't

privy to all of the discussion this morning, but much of the staff's concern about 15 grams is predicated upon the instant arrival of chemical debris. In order to avoid the formation of a filtration bed, 15 grams seems to avoid --

CHAIR BANERJEE: It is not instant arrival. So, I don't know; it can be over a long period of time. It is any arrival of chemical --

MR. LETELLIER: But we have an aggressive chemical effects test plan that we are prepared to share with you, to try to demonstrate that it may not be as arduous as the WCAP formula implies, at least not at South Texas.

CHAIR BANERJEE: So, you will show how these tests are wrong?

MR. LETELLIER: I am not debating whether they are wrong or right; they are just simply different.

CHAIR BANERJEE: You are going to have your own set of tests that replace all this?

MR. BAILEY: I think in the context of the in-vessel effects, they are trying to do more to look at the range of RCS conditions that you would look at and the range of timing in terms of cooling down and in terms of getting the chemical effects into the core.

Remember that they do have a very large

hot-side injection capability at South Texas, two trains of it, in fact. That provides more than ample flow to the core if it is initiated. And so, if that is initiated before the chemical effects are in place, it may well be feasible that they can handle higher debris limits.

CHAIR BANERJEE: Right, if there is no chemical --

MR. BAILEY: If there was a delay.

CHAIR BANERJEE: Yes.

MR. BAILEY: If the timing works in their favor. And so, potential changes to the plant as opposed to large-scale removal of insulation may involve timing of certain actions that they would take.

CHAIR BANERJEE: Okay. So, what you are really saying is you are going to find a way not to allow chemicals to get to the core?

MR. SANDE: This is Tim Sande.

We are going to be talking about that a little bit later in the presentation. Just to give a little preview, basically, our approach is, rather than looking at the bounding conservative approach that the Owners' Group has taken to try to solve this issue for everyone, we want to look specifically for South Texas conditions, and then we want to look at specific

scenarios and look at realistically what is happening in those scenarios, and evaluate what kind of fiber loads can we withstand for a particular scenario.

So, not all scenarios -- 15 grams for certain scenarios may be the limit. Other scenarios, it may be something completely different.

As I mentioned, we will be talking about that later. Maybe if we could put that off until later in the day, it would be easier to --

CHAIR BANERJEE: Okay. But I am trying to understand what that two means or the one there. Are you going to access the available database or you are developing your own database? What is happening? How is that going to be QAed? Who is going to take a look at it?

MR. LETELLIER: We want to take advantage of what has been done so far. But they are things that we are looking at that may very well require additional analysis and additional testing. So, those plans, I mean, we are still developing our approach, but there is a good possibility that we may do additional fuel testing to support the risk-informed analysis.

CHAIR BANERJEE: Okay.

MEMBER SCHULTZ: But you are looking to develop a plant-specific input dataset --

MR. SANDE: Right.

MEMBER SCHULTZ: -- that covers all of this --

MR. SANDE: Right.

MEMBER SCHULTZ: -- that we described on the previous slide? It is through identifying those specifics that you will work to determine what is influencing the risk?

MR. SANDE: Correct.

MR. LETELLIER: I think that having a systematic evaluation of all possible break scenarios -- I say that loosely; it is a systematic interrogation of many, many possible scenarios -- will change our perspective about what we are worried about most acutely. That is part of the benefit of having this formalized approach.

The next slide on page 17 is perhaps the best opportunity to talk about the mechanics of CASA Grande. I will like to inform the Subcommittee that you will be given backup slides to look at, examine at your convenience. And there are numerous details on every topic that we were prepared to discuss and just don't have time today.

CONSULTANT WALLIS: Why is there an arrow from debris generation to chemical concentration?

MR. LETELLIER: I'm sorry, please repeat the question.

CONSULTANT WALLIS: I see there is an arrow from debris generation to chemical concentration. But I thought that what we are concerned about with chemicals was oxides of aluminum which come from things like ladders which happen to be in the sump, which have nothing to do with debris generation.

MR. LETELLIER: Dissolved silicon from the fiberglass is also a concern.

CONSULTANT WALLIS: But that is not so important as aluminum oxide, is it?

MR. LETELLIER: We are actually investigating the contra-corrosion or competing-corrosion effects of silicon versus aluminum.

CONSULTANT WALLIS: You are going to do more with the chemistry --

MR. LETELLIER: Indeed.

CONSULTANT WALLIS: -- than just assume it is only aluminum?

MR. LETELLIER: Indeed. That's right.

So, this is a more detailed flowchart. I think the primary message is in two parts. The righthand side is the in-core effects; the lefthand side is the strainer effects, and there are some

opportunities for crossover, most notably debris bypass in the middle.

Now, if you were to walk from the bottom to the top and try to come up with a hand calculation, you would have to make assumptions about very specific details. First of all, the location of the break relative to its annualized frequency across the whole spectrum; you need a very specific XYZ location. In addition, you need the size. If we had the luxury of a directional jet, you would need an azimuthal angle.

All of these things are important because of the relative geometry of your break, your sources versus your targets. That explicitly determines the composition of the debris that you are worried about.

CONSULTANT WALLIS: So, how are you going to do this? I mean, all these arrows carry parameters, variables to physical or chemical parameters, and so on.

MR. LETELLIER: Yes.

CONSULTANT WALLIS: So, if you are going to calculate strainer debris total head loss from chemical precipitate and all of the various debris, you have to carry into this thing "X" grams of fiber or "Y" grams of so-and-so, and so much chemical.

MR. LETELLIER: That's right.

CONSULTANT WALLIS: Which is a continuum.

And then, you are going to have to predict a head loss.

MR. LETELLIER: That's right.

CONSULTANT WALLIS: We don't have any way of doing that yet.

MR. LETELLIER: We have addressed that question before. There are a couple of alternative approaches. One is to take a very abstract view about the likelihood of the extremes. The other approach, which we are adopting, is to look at existing head-loss correlations and to interrogate the input parameters, so that we can generate a spectrum of possible results.

CONSULTANT WALLIS: Oh, you are going to go back to try to take something like this data we saw this morning and fit it with some kind of an empirical analytical model theory, or whatever you want to call it?

MR. LETELLIER: That is the hope. That's right.

CHAIR BANERJEE: But one of the difficulties, clearly, is that there isn't one that we have come across that works, any correlation. That is why we sort of take a bounding approach.

MR. LETELLIER: Am I correct in assuming your comment is regarding the generality of any single correlation?

CHAIR BANERJEE: Yes. If you try to look at all this data and develop a correlation, it is multi-dimensional space, clearly, which involves a lot of variables. And nobody has really been successful in doing that, which is why one sets a boundary, because there are so many uncertainties, so many unknowns.

I mean, you do an experiment in one facility and they do it another, and the difference in pressure losses is a factor of two, three, or four. So, clearly, these are classically-imposed problems. As I said, very small changes make a very different, large change to the outcome --

MR. LETELLIER: Right.

CHAIR BANERJEE: -- which is why one has taken up to this point a bounding approach, saying that, if we do this, we are sure that we will get the head loss less than 14 psi, or something.

MR. LETELLIER: So, two responses to the observation. First of all, South Texas has a very specific combination of debris types that we are concerned about. And indeed, it has a much more favorable flow velocity than they did 10 years ago. So, we are looking at very, very low approach velocities which perhaps minimize the concern over bed compression. They have fibrous debris mats of well-characterized

Nukon fiberglass in combination with perhaps high loadings of particulate, primarily from assumed failure of unqualified coatings. So, there is a relatively-small parameter space of debris combinations.

The other comment about sensitivity, the sensitivity of a model does not necessarily preclude the characterization of that sensitivity. And that is something that has been perhaps missing from our work with head-loss correlations in the past.

CHAIR BANERJEE: What do you mean by "preclude the characterization of the sensitivity"? I don't understand what it means.

MR. LETELLIER: So, in a deterministic method, we hope for a predictive accuracy that is within some acceptable limit. But in the risk-informed approach, a factor of two is simply a wider tail than before. So, we can still sample that range of predictability and propagate through the possible combinations.

A very good example in our existing analysis is the bypass fraction. We have very, very limited data for how much fiber passes through the strainer. And so, at the moment, it can range anywhere from 100 percent bypass to something smaller that is constrained by the

information.

CONSULTANT KRESS: But you have to put a probability on those.

MR. LETELLIER: You do, yes.

CONSULTANT KRESS: And that is where your expert elicitation --

MR. LETELLIER: In some cases, that's right.

CONSULTANT WALLIS: The difference between zero and 100 percent, that is a pretty broad --

(Laughter.)

MR. LETELLIER: Indeed. Probabilities are like that. They are always constrained by zero and one.

CONSULTANT WALLIS: That is right. So, you haven't added any information by saying that. So, that means that your task is difficult if you get something which varies so much, so broadly.

MR. LETELLIER: Absolutely. If there is no information in any of the parameters, then your result is a questionable valuable. However, I think we can do much better than that in most cases.

CONSULTANT WALLIS: If it is something between one fiber and 20 truckloads of fiber, that is a pretty broad spectrum to cover.

MR. MURPHY: I would like to help us focus on getting through the desired outcomes, to make sure we hit the big picture on the entire scope and expertise we have with the project, and meet your time goals.

MR. LETELLIER: Let me just make one comment about the mechanics before we move on to the next topic. All of the statistical sampling is being performed in what I call a non-uniform Latin hypercube sampling structure. So, it is non-uniform weighting. So, indeed, we can carry the extreme tails without bias, and it is a traditional Latin hypercube design because there are multiple parameters that have to be thoroughly sampled.

Now there are many dozens of parameters and growing. So, adequate sampling is of vital interest. Right now, we are running replicates, replicates of batches of independent scenarios in order to track the convergence on our performance metrics.

MEMBER STETKAR: How are you handling correlations? You said this is a time evolution --

MR. LETELLIER: It is.

MEMBER STETKAR: -- you know, as you propagate the uncertainties through. So, I have confidence that you are handling that. You glibly mentioned very early that you are treating all of these

as independent, but that can't be the case, I hope.

MR. LETELLIER: Right. So, there is a very explicit dependency correlation between the size of the event. If I postulate a small break, then all of the plant response is commensurate with a small break.

MEMBER STETKAR: I got that.

MR. LETELLIER: Okay. Right. Regarding the time dependence -- and this is, I think, of academic interest, and I welcome your feedback -- we are treating event trip times, for example, turning on and off the spray, operator actions, as probabilistic values. If the notional time to turn off a train is "X" minutes, there is a distribution about that time. In essence, we are randomizing the event sequence. The event tree is a randomized quantity that we interrogate along with all of the other physical parameters.

MEMBER STETKAR: In the interest of time, I guess I won't pursue that. But there may be some subtle correlations if, indeed, there are dependencies on operator actions. If an operator action occurs -- pick a number -- 30 minutes into a sequence, then another operator action can't happen 14 minutes into a sequence, even though the distributions might cover both of those things. When you sample from those, you need to account for that timing-dependence. And I

don't know what other phenomena -- because I am not a chemist, or whatever -- I don't know what other types of phenomenological dependencies there may be that require kind of careful sampling.

MR. LETELLIER: Yes, those are important observations. Indeed, correlations do exist. There is nothing about the methodology that precludes our incorporation of those correlations. The challenge is usual --

MEMBER STETKAR: Except that you set up now a computer model, and sometimes people set up computer models that can't handle the notion of correlated uncertainties. So, I would hope that whatever sampling routines you have don't preclude that.

MR. LETELLIER: It is intended to be robust.

MEMBER SCHULTZ: And I would suggest you might include that on the list, but you might want to prioritize where your key areas of investigation are. You have got some other real challenges to handle before you add that into the list. You may certainly conclude it. It sounds interesting. But you have got some real tough challenges with just the other mechanics and the phenomenological evaluation.

MR. LETELLIER: So, my personal role in

this project is to assemble the framework and to anticipate the mechanics of those details. We are relying on other members of the team to populate the scientific phenomenology portions and feed the information through the system.

CONSULTANT WALLIS: I am wondering, when you submit this CASA Grande to us or the NRC, and it has all these assumptions and everything in it that produces some numbers as an output, how is anyone ever to tell whether it is believable or not?

CONSULTANT KRESS: That is why your uncertainty analysis --

CONSULTANT WALLIS: Would we have to look at everything, have to look at every uncertainty, every assumption? How do we know how to assess the validity of it?

MR. LETELLIER: So, if you are talking about a verification --

CONSULTANT WALLIS: Yes.

MR. LETELLIER: -- effort, you can always collapse these distributions to sharp values or mean values in order to assure that it is functioning the way that you expect. And you can play. You can shift those --

CONSULTANT WALLIS: Do testing on the thing

itself --

MR. LETELLIER: Yes.

CONSULTANT WALLIS: -- by restricting some things?

MR. LETELLIER: Yes. That's true. And you could compare that to deterministic analyses that are very familiar.

CONSULTANT WALLIS: Aha. Which, of course, we have great confidence in.

(Laughter.)

MR. LETELLIER: I won't respond to that.

I think it would be useful if we moved on to the next topic. I realize you will have many questions about every topic that we discuss. This is an introductory conversation, and we actually welcome your feedback.

Recognizing that this is the Thermal Hydraulic Subgroup and that having thermal hydraulics models is a cornerstone of much of our understanding about plan analyses, we have Rodolfo Vaghetto from Texas A&M to talk about RELAP5 modeling.

CHAIR BANERJEE: So, can you tell us, roughly, what topics you will cover, so we cant take a break at the appropriate time?

MEMBER BLEY: Slide 5.

CHAIR BANERJEE: So, we are at slide 5 right now?

MR. MURPHY: No. We have just gone through the first three topics, the integrative framework.

CHAIR BANERJEE: And then, we have --

MR. MURPHY: Thermal hydraulics and then LOCA frequency.

CHAIR BANERJEE: So, why don't we take the thermal hydraulics, and then we will take a break after this?

MR. VAGHETTO: Thank you.

My name is Rodolfo Vaghetto. I got a master's in nuclear engineering back in 2000. I have approximately 10 years of experience in the engineering field. I had the chance to join Thermal Hydraulic RELAP5 team at Idaho National Laboratory. I had experience in the past years using RELAP5 and alternate system codes. Today, I am working on my PhD at Texas A&M University in nuclear engineering. Dr. Yassin Hassan is my advisor.

What we are doing at Texas A&M, we are working to develop the models to study the thermal hydraulic response of the reactor system and containment required for this project.

RELAP5-3D is a system code that we are using

to perform all the thermal hydraulic calculations for the reactor system, and MELCOR is in use to predict the containment response, in particular, the temperature of the water in the sump, and, also, to provide the boundary condition for the RELAP5 simulations.

At Texas A&M, we have also coupled RELAP5-3D with DAKOTA. This is to facilitate the sensitivity analysis that we are performing to get the confidence of our RELAP5 model and have a better understanding of the system response.

The code, in just a few words, is a software that has been developed by Sandia and it has been conceived to facilitate sensitivity analysis, uncertainty quantification, and design optimization. So, it has been coupled with CFD codes, and we have coupled with RELAP5-3D. So, it is the first time it has been coupled to system codes.

The RELAP5-3D model, we are currently focusing our attention to two different models. One, which we call 3D Vessel 1D Core -- RELAP5-3D originates from the old RELAP5 Mode 3 code, which has only 1D capability. This new software, this new code, which is not new, brand-new, has a 3D capabilities.

So, in our first model, we have decided to have some of the most important components of the vessel

to be modeled with a 3D component. And we will see later in the presentation there is the nodalization for the 3D components that we used in the vessel. The core is still 1D, two channels.

The reason why we have this model is because it runs relatively fast. So, we can have, in an order of hours, we can have a complete simulation that goes from the break opening up to 24 hours after the sample switchover.

CONSULTANT WALLIS: Can I ask how this is integrated with the ballistic safety analysis, the PRA? Does that mean that every time you do one of these thousands of different scenarios, you run RELAP5 to model that scenario? This isn't the way PRAs work now. It would be nice if the code could be in the PRA, but it usually isn't.

MR. LETELLIER: And indeed, it is not yet practical to couple them in that fashion. What we are doing is running cases in RELAP5, first of all, to find the nominal behavior of pool temperature, time to switchover, et cetera, for the major break classes, the small, medium, and large.

We are also running enough cases to look at variations on those primary performance metrics. And then, we are doing secondary, performing secondary

sampling on those distributions that we generate from the code.

CONSULTANT WALLIS: So, you are sort of condensing the output into a form which will fit into the PRA?

MR. LETELLIER: That is true.

MEMBER SCHULTZ: And that is where DAKOTA comes in? Or is DAKOTA used in --

MR. LETELLIER: Yes, it can be used to drive reduced-order models from the synthetic data. By reduced-order model, I simply mean a correlation or an abstraction that is easy to evaluate. It fits the calculations with some desired precision within the range of the parameter space.

MR. VAGHETTO: Yes, in our case, DAKOTA is helpful because it helps for analyzing the cases. So, if you have a sensitivity analysis where you have to run like several cases for each parameter, you can run in multiple processors, and you have one sensitivity study that can run with the same speed of one case.

CONSULTANT WALLIS: When you run 50.46 deterministic LOCA analyses now, don't you do the same thing? You run the same code? Or is it something different?

MR. KEE: Not RELAP5, no, sir.

CONSULTANT WALLIS: You use a different code?

MR. KEE: Yes, sir. Westinghouse codes are used.

CONSULTANT WALLIS: For the other one? And you use an ASTRUM method. So, that is something, too. You can't just adapt that right away to squeeze into the code, the PRA, somehow?

MR. KEE: Right. So, those codes are not, in general, the ones we use for safety analysis aren't capable, for example, of 3D analysis that we know.

CONSULTANT WALLIS: This is more sophisticated and more accurate? Is that it?

MR. KEE: It is maybe more sophisticated.

CONSULTANT WALLIS: It gives the same sort of outputs in the end, doesn't it? It gives outputs which you can, then, put into the PRA?

MR. KEE: Sure.

CONSULTANT WALLIS: Just the way that the existing code does?

MR. KEE: Sure, yes, and I think that opportunity is there to use a different model that is appropriate and gets the --

MR. LETELLIER: Indeed, in our initial evaluation, we did use the Facility Safety Analysis

Report for our initial distributions. But the model that Rodolfo will describe has tremendous flexibility for answering questions about the RCS response.

CHAIR BANERJEE: So, other people have used things like COBRA/TRAC. Why did you choose something different from that?

MR. VAGHETTO: Well, we at Texas A&M in RELAP5 personally, and Dr. Hassan, in particular, whenever we run cases that maybe have not been run in the past, we have very good support from people at Idaho National Laboratory, which has experts. Whenever you run cases, most of the time you have to understand what the code does. In some cases, you need to involve some people to put their hands in the code to see exactly what it is doing.

So, we feel personally, I think, at Texas A&M University we feel comfortable using RELAP5-3D system code, which has been anyway largely used in lightwater reactor simulations.

CHAIR BANERJEE: So, let me understand. This system that you are talking about is going to have various components which have different codes and things. Are these going to be codes which are either accepted or approved by the NRC or are they going to be things which the NRC will have to look at and approve

or --

MR. SANDE: I can try to answer.

CHAIR BANERJEE: -- to provide true applicability? There are a whole of things.

And then, before we could use the code, you have to go through a process, right?

MR. SANDE: Yes. Let me try to answer that question.

CASA Grande is a code that is being developed on this project specific for implementing the risk-informed approach. Now things like RELAP and MELCOR and other software that we are using for various aspects on the project are the choices of this team.

Somebody else that wants to come in and implement the risk-informed approach -- for example, we are using MELCOR to get pool temperature data to implement into CASA Grande because the time-dependent pool temperature is a very important input. Now someone else that wants to do the same approach doesn't have to use MELCOR just because we used it, but the analysis needs to be a robust analysis.

So, I think the NRC has to be confident that a robust analysis is being done, but it is not that they have to approve the use of MELCOR with CASA Grande. The choice of code is --

CHAIR BANERJEE: But they have to accept your results at some point.

MR. LETELLIER: Right, they would.

CHAIR BANERJEE: So, how do they accept your results if they haven't accepted or approved the code? I am just curious.

MR. LETELLIER: Yes. Anticipating problems? Yes.

CHAIR BANERJEE: Yes.

MR. LETELLIER: These are the two phenomenology codes that we have introduced. With the possible exception of some equilibrium chemistry models for our guiding our experimentation, these are the only two external codes that we are relying on for systems information. We haven't gotten explicit feedback from the staff on the implementation of these two, but, indeed, we do need to have that conversation and make sure they are being used appropriate to the purpose.

CHAIR BANERJEE: So, for other applications that we have seen who have used COBRA/TRAC or some versions of it, the staff has accepted it, and so on. It has been through a long process of verification and validation, or whatever, maybe not for this application exactly, but in the past we have accepted it.

Now, if you have a code where you can, as you said, put your fingers on it and keep changing it, that is not exactly what we -- you know, if you want to use this code ultimately for licensing actions, it has to be frozen and approved at some point, right?

MR. MURPHY: So, this would be one of those insights that we will note and make sure that we work with --

CHAIR BANERJEE: Well, I don't know; maybe we should ask the staff.

MR. BAILEY: We don't have all the answers on that at the moment. We recognize the challenge. I don't think that we had seen that they were using these codes until just recently. So, we realize that we need to have further discussions with South Texas to see how do we go about our acceptance of these codes and our review of these codes.

But, fundamentally, what you are saying is exactly right, that we would need to look at how these codes are used and have confidence in the results that they are obtaining.

MR. SNODDERLY: Yes, this is Mike Snodderly from the staff.

I think, typically, in risk-informed applications, they could use codes like RELAP, and the

codes and their input assumptions would be onsite for the staff to inspect, if we would like. And we would probably do some spot-sampling with the Office of Research and their ability to run RELAP, some sequences, to give us that confidence.

But we do not anticipate its being a design-basis-type calculation where they submit a code for approval by the staff for this risk-informed application.

CHAIR BANERJEE: So, it means that you can go out and use the code to do things like thermal hydraulics calculations --

MR. SNODDERLY: For Reg Guide 1.174 risk-informed applications, that has typically been the approach.

CHAIR BANERJEE: That's interesting.

CONSULTANT WALLIS: Now these codes have a history of use. I mean, they have been accepted for all kinds of purposes. And so, we can sort of say they are probably okay.

But, then, there are other codes like CFD which the staff I think will accept for modeling how debris flows around the containment, and does it settle out, and where does it go on the strainers, and all that. Those would be where the uncertainties really are, it

seems to me. Are you proposing to use codes for that?

MR. SANDE: Yes and no. We have done deterministic transport analysis for South Texas in the past that has used CFD modeling. We are planning to incorporate that into the risk-informed framework.

CONSULTANT WALLIS: You are planning to use codes for other purposes than just these traditional sort of LOCA-type analyses?

CHAIR BANERJEE: The problem, as you know, of course, is settling in turbulent fluids. So, CFD calculations of the mean field, that doesn't tell you very much about settling. If you even try to predict homogeneous turbulence settling and that, you get a draw.

MR. LETELLIER: Yes, to my knowledge, CFD codes have never been used in a predictive method for debris transport because --

CONSULTANT WALLIS: How would you do it? I mean, you can't ask an expert, I mean, "I have got this geometry here. How far do you think this debris is going to go along this strainer?" I mean, I would say, "I haven't a clue." You know, until you have some kind of analytical model or something to explain it, the expert has no opinion.

MR. LETELLIER: The traditional approach

has been based on the empirical evidence of threshold velocities to see when, in fact, you exceed those settling thresholds, and applying conservative application of that information, they are typically assigned zones where it can be resuspended and zones where it will be trapped and permanently sequestered, depending on its size.

In addition, there is compensation made for degradation of large pieces into small pieces. All of these assumptions are very familiar to the staff from deterministic calculations.

CHAIR BANERJEE: Right. I think the staff and we are in sync on this, that it is very, very difficult to give any credit for settling in a turbulent fluid of fibrous or particulate debris, simply because it is not well understood at all. Secondly, all the evidence is that settling is hindered in turbulence. So, it is not like Stokesian settling or something changes.

MR. SANDE: For fine debris, that is true. In our deterministic analysis that we did for South Texas, we didn't credit any settling of fine debris. For small and large pieces of debris, the methods that are used to predict whether that debris would settle or transport are more well-defined and the approaches have

been accepted by the NRC.

CHAIR BANERJEE: So, you are saying big chunks, macroscopic?

MR. SANDE: Yes, like a 1-inch clump of fiber or a 6-inch piece of fiberglass.

CHAIR BANERJEE: Right. Okay. So, that is what you are applying it for?

MR. SANDE: That is correct. Yes, the CFD model focused on that.

CHAIR BANERJEE: So, anything which is fine, you are going to have to transport? There is no way out.

MR. LETELLIER: And presently, we are using very coarse definitions of size, 60 percent, small; 40 percent, large. The 60 percent is fully suspended and completely transportable. So, there are some very crude cut sets here that define our state of knowledge.

CHAIR BANERJEE: Yes, because it frightens me when you say the approach velocity is very low because at that point I get the feeling that somebody is going to claim credit for settling there. It is not because the Reynolds' numbers can still be very high.

MR. SANDE: Yes, the approach velocity through the strainer is important for head loss. It is not the same as the bulk flow velocities and the pool

coming toward the strainer, which is important for transport. So, we are not taking credit for what has been called near-term settling effects.

CHAIR BANERJEE: Good. Okay. At least that removes one concern.

CONSULTANT WALLIS: So, your analysis is not really best estimate? I mean, your RELAP analysis is, presumably, a best-estimate type?

MR. SANDE: Yes.

CONSULTANT WALLIS: But when you are dealing with some of these other things like settling, you are being conservative?

MR. SANDE: That is correct. To the extent possible, we want to be realistic and we want to evaluate the range. But in some areas where we just don't have good information, we will take a common conservative assumption as long as that assumption doesn't skew the results significantly.

CONSULTANT WALLIS: And there is no uncertainty? There is no uncertainty attached to that? I mean, you are not propagating uncertainty in the assumption. You are saying we neglect something or something like that?

MR. SANDE: Well, I would say on any assumption that we pick you could assign an uncertainty

to it and have a probability distribution. Some things that is very important and it has a bit impact on the results. Other things are relatively unimportant.

And it turns out that debris transport is not actually all that important to the outcome of the risk-informed approach. We can get into that a little bit more later.

CHAIR BANERJEE: Now, with the thermal hydraulics, where are you using it exactly? Is it to find out the flow structure in the vessel and in the core for long-term cooling? Or is it through the blowdown phase? What is this being used for?

MR. VAGHETTO: Yes, that is basically correct. Not only we are --

CHAIR BANERJEE: What is correct?

MR. VAGHETTO: Which means like we are studying all the phase, like starting from the blowdown and the long-term cooling. But we are actually like focusing our attention to the long-term cooling phases. So, whatever happens after the --

CHAIR BANERJEE: So, you are using this to look at mixing in the lower plenum?

MR. VAGHETTO: What do you mean "mixing in the lower plenum"?

CHAIR BANERJEE: This code. Are you

actually looking at, say, debris transport?

MR. VAGHETTO: Oh, no, no, no.

CHAIR BANERJEE: Oh, okay.

MR. VAGHETTO: Okay. The system code is able to predict only the fluid behavior and we cannot with RELAP5 through these tables predict any --

CHAIR BANERJEE: Right. So, you are just using a lumped parameter? Or what are you doing?

Yes, please, go ahead.

MR. HASSAN: Yes, Yassin Hassan, Texas A&M.

RELAP5 is a system code. We predict the thermal hydraulic, not transport of the debris at all. So, we use it for a blowdown and, also, the longer-term cooling with operations of the operators. In a sense, we use 3N, right, of the cooling for hot leg injections and switching to the cold leg, and so on. So, we see the behavior of the core.

CHAIR BANERJEE: No, it just frightened me when I saw 3D vessel; what did that mean?

MR. HASSAN: No, no. It is exactly like TRACE code. RELAP5, as you know, is an NRC code --

CHAIR BANERJEE: Right.

MR. HASSAN: -- adding all these 3-dimensional physical, which is very, very -- calls some issues out, turbulence modeling. It is like TRACE.

It is a system code.

CHAIR BANERJEE: All right. Thanks.

MEMBER SKILLMAN: Rodolfo, for clarification, I interpreted from what you said earlier when you talked about others looking at the results, that you weren't going to be modifying RELAP5-3D.

MR. VAGHETTO: No, the software.

MEMBER SKILLMAN: Rather, for verification/validation, that you are deriving input and then results that make sense --

MR. VAGHETTO: That's correct.

MEMBER SKILLMAN: -- that you are having experts look over your shoulder for the work that you are doing?

MR. VAGHETTO: Yes. I wanted to clarify that point because like I can communicate with the system through the input file. The user is asked to select the right physical model, depending on what you expect the system to behave in specific components.

Then, when I ask the experts, it is because I maybe get the results and I want to make sure that the results are correct sometimes. And they help me to understand what is the best selection in the input file to make the RELAP5 to work as we expect in the best way.

CHAIR BANERJEE: So, the innovation here is

really to connect it to DAKOTA for the uncertainty? DAKOTA has existed for quite a while. I guess Idaho did some work on trying to connect it to RELAP codes at some point. I vaguely remember.

MR. VAGHETTO: And we did it at Texas A&M as well. So, our RELAP5-3D that we have installed is coupled with DAKOTA. We are actually working to run sensitivity analysis at the moment, which is the first step required to have the confidence of the model itself.

CHAIR BANERJEE: Okay. Fair enough. Let's go on.

MR. VAGHETTO: Yes. Okay. So, you were asking, also, about like the thermal hydraulic parameter that we asked RELAP5 to calculate. I showed here a couple of examples. So, we asked RELAP5 parameters like break flow, like the flow through the core or through the downcomer, flow through the full steam generators, for example, during the phases of the accident.

MEMBER REMPE: Before you leave that slide, how do you interface the RELAP and the MELCOR? I am puzzled with that.

MR. VAGHETTO: Yes, I wanted to talk a little bit later on, but I can anticipate. We are actually planning in the near future to work using a coupled version of RELAP5-3D and MELCOR. Sandia has

already worked on this coupled version.

MEMBER REMPE: So, this is planned? This isn't already --

MR. VAGHETTO: It is not --

MEMBER REMPE: This is used, but it is not yet --

MR. VAGHETTO: MELCOR at the moment is used as an independent code. What we do at the moment, we run RELAP5 simulations. We get the information from the break, like the mass flow rate, the enthalpy flow, which is the energy to support the containment. And then, we feed them manually in the input file of MELCOR, those two parameters, and we run the MELCOR to analyze the containment.

MEMBER REMPE: You don't use any vessel models in MELCOR? You just are using the containment model?

MR. VAGHETTO: Correct.

MEMBER REMPE: Okay. Got it.

MR. VAGHETTO: So, the model of MELCOR contains only the containment and the features. And we will discuss later about the nodalization. The primary system and everything related to the reactor is modeled in RELAP5-3D.

MEMBER REMPE: Okay.

CHAIR BANERJEE: So, I notice that we have got quite a lot of detail here. It is unlikely that we can finish even in an hour.

Why don't we do this: maybe we need to take a break, too. But we might want to take a break and maybe you can organize it so that you hit only the main points. We have the slide deck now. But when we go through it, maybe we can't cover all of the slides even in another hour. So, hit the main slides, the main points you want to make, and we will try to get through it by about 4:30, if we can, then. Okay?

We will take a 10-minute break. So, we will reconvene at 20 to 4:00, approximately.

Okay. So, we are off the record now.

(Whereupon, the foregoing matter went off the record at 3:30 p.m. and went back on the record at 3:46 p.m.)

CHAIR BANERJEE: We are back in session.

MR. MURPHY: So, as we continue to move through, what we are going to do is we are going to hit some of the high points, as you suggested, Mr. Chairman.

CHAIR BANERJEE: That's great.

MR. MURPHY: And we want to make sure that we have a chance to describe our testing that we plan to do as well as the overall big picture with that.

We are going to play musical chairs here in a minute because we want the presenters to be up here, so we can have good eye contact and good discussion.

CHAIR BANERJEE: Good.

MR. LETELLIER: So, as we reconvene, let me draw your attention to slide No. 18. It was a transition slide that was inadvertently omitted. I just want to clarify where the thermal hydraulics calculations are helping to inform the process. So, it is the flowchart. You should have a large-format version. It is slide No. 18.

CONSULTANT WALLIS: We don't have any numbers on our slides. We don't have any numbers on the slides.

MR. LETELLIER: The only thing is to say that there are modules in CASA Grande that depend on the TH calculation.

MR. VAGHETTO: Yes, this is Rodolfo Vaghetto again.

I will spend just a few more minutes focusing on the nodalization of the power plant for the RELAP5. This is the nodalization of the power plant. As you can see, we have four independent loops, three independent injection trains. We have implemented some plant operating procedures to turn on and off the trains

during the LOCA accident.

There was a lot of discussion yesterday regarding the flow of the water through the steam generators. So, this model is able to simulate and predict the flow during the phases of the accident.

And actually, we have some preliminary results that basically see how important is the model of the steam generator and the water flowing back to the reactor vessel during the phases --

CONSULTANT WALLIS: But when you have the limiting condition of overheating the core, do you assume it is exit quality of one when you do RELAP? Or what do you assume? Do you assume 50 percent or what?

MR. VAGHETTO: In RELAP5, there are not actually assumptions on that in the sense that you could calculate --

CONSULTANT WALLIS: You have got to have some criteria --

CHAIR BANERJEE: The 800 degrees Fahrenheit criteria.

CONSULTANT WALLIS: You calculate the temperature of the cladding?

MR. VAGHETTO: Of the cladding, yes.

CONSULTANT WALLIS: And then, you use 800 or something? What do you use?

MR. VAGHETTO: Well, at the moment in these preliminary results, we are not using any limiting case to stop the calculations. I wanted to just to see what is the behavior. So, RELAP5 can go even higher than 2200. But, I mean, I am not limiting the calculations at this time. It is just preliminary results to show the behavior of the system.

CHAIR BANERJEE: Do you have the facility to put an inlet resistance of the core?

MR. VAGHETTO: Yes, and that is what I was going to talk in the next nodalization. So, if you go to this slide, this is basically the 3D nodalization. So, RELAP5 allows the user to define the K loss coefficient and the junction between volumes. With this nodalization, we did it already and we have some preliminary results for the 1D core.

We made a simulation imposing a very large K loss coefficient at the bottom of the core. We ran cases of different break sizes and different break locations to see what is the behavior of the system during the phases of the accident.

But, in this particular case, when we have a 3D core, the 293 fuel assemblies are independently modeled and there is crossflow. So, we can assume a core blockage, a partial and full core blockage at the bottom

or at any location of the fuel assembly. In that case, we have a better estimation of the fuel flow inside the core and inside the vessel.

CONSULTANT WALLIS: It is 193 --

MR. VAGHETTO: Fuel assemblies.

CHAIR BANERJEE: Can you look at the effect of bypass as well?

MR. VAGHETTO: What bypass are you talking about?

CHAIR BANERJEE: Well, one would be, of course, not just between the assemblies, but from the --

MR. VAGHETTO: Yes. If you see, like on the left, there is the core bypass --

CHAIR BANERJEE: Right.

MR. VAGHETTO: -- which is the channel 551.

CHAIR BANERJEE: Okay.

MR. VAGHETTO: That bypass is basically the region between the baffle and the barrel. We have that region modeled. So, we can actually like predict also the -- and we have seen from preliminary results that that, for some phases of the LOCA, can play an important role.

CHAIR BANERJEE: Yes, there is one thing that perhaps you should know. It may be interesting to have the facility to be able to model the core in terms

of dryout and heating because you may gain some advantage that way, by having a limit something like 50 percent quality rather than requiring all water to come out. I mean, all water is a good limit, but we also know that cores will stay under this 800-degrees criteria, possibly with --

CONSULTANT WALLIS: What do you mean by "all water" then?

CHAIR BANERJEE: Sorry?

CONSULTANT WALLIS: All water? Why all water?

CHAIR BANERJEE: Well, at the moment, for the hot leg break that is what the assumption is, right?

CONSULTANT WALLIS: It is?

CHAIR BANERJEE: I think that is where you get your 44 gallons per minute.

CONSULTANT WALLIS: You can't have all water if it is coming in at saturated temperature and being heated.

CHAIR BANERJEE: Okay. Essentially, the quality is close to zero.

CONSULTANT WALLIS: I think it is the cold leg break you worry about. We get a lot of --

CHAIR BANERJEE: Cold leg break is not the case. It is the hot leg break.

CONSULTANT WALLIS: And you don't worry at all. You have got a lot of water coming out at the top and there is no problem at all.

CHAIR BANERJEE: Yes. Yes, but that may be an overly-restrictive criteria.

CONSULTANT WALLIS: It doesn't get to 50 percent quality.

CHAIR BANERJEE: Yes.

CONSULTANT WALLIS: With 45 gallons per minute.

CHAIR BANERJEE: However, you don't need 45 gallons, you may not need 45. All I am saying is maybe it would rethink some of the criteria.

MEMBER ABDEL-KHALIK: How and where do you model crud deposition?

MR. VAGHETTO: RELAP5-3D is not able to model the crud deposition. It is a system code.

MEMBER ABDEL-KHALIK: I understand, but, you know, how do you account for that phenomena?

MR. VAGHETTO: Okay.

MR. HASSAN: This is Yassin Hassan again.

If you look here to the core, that is why we did it at 3D. I mean, we can put a loss coefficient at the first grid. In that case, we know if it deposits there, so how much flow will go through the core.

As a matter of fact, the loss coefficient is distributed in a certain way to accommodate the bypass between the 193 --

MEMBER ABDEL-KHALIK: Well, the staff was talking about a phenomenological, or Westinghouse was talking about a phenomenological model to account for crud deposition on the fuel and limiting the thickness of that layer to some value. And you intend to just sort of forget about that physics and include some kind of modification to an input loss coefficient at the entrance to the channels?

MR. HASSAN: At the entrance to the core, that is our intention. With respect to the deposition of the crud on the fuel elements, we have to have experimental data to tell me where is that and its thickness. And we don't have that right now.

MEMBER ABDEL-KHALIK: Okay.

MR. HASSAN: Okay? Or RELAP5 cannot do it.

CHAIR BANERJEE: Okay. Carry on.

MR. VAGHETTO: So, the next topic will be -- you will have like the slides. So, I just selected the most important slides for the thermal hydraulic. The next topic will be covered by Tim Sande.

MR. LETELLIER: So, this flowchart is on slide 26. It is being used as a transition to show you

the phenomenological topics. We will try to skip through these with about five minutes per topic, allowing for your questions, starting with the beginning. That is the initiating event itself, which requires a LOCA frequency.

The fundamental requirement for all of our LOCA frequencies is that whatever we use in CASA for the relative scenarios has to be consistent with what the PRA uses for the annualized frequencies of the events. At the end of the day, we pass this information back to the PRA in consistent units of small, medium, large LOCA, with the conditional effects that we have analyzed in the phenomenology.

The best information that we have to use is NUREG-1829, which is primarily based an expert elicitation for a plant average behavior. So, there are a number of challenges and deficiencies associated with using that information. This is maybe the best figure to speak to some of those challenges.

This is a graphical representation of a table that basically shows the exceedance of all breaks greater than or equal to a given size. So, if you look at the far left, these one, two, three, these four opinions, if you will, represent the range of confidence in the assessment of annual breaks of all sizes, anything

larger than half an inch.

Similarly, as you proceed to larger sizes, in the middle this would be the fifth percentile, the median, the mean, and the 95th percentile of confidence level for all breaks larger than approximately 14 inches.

So, it is very important to recognize that these are presented as an exceedance function, very close cousin to a complementary cumulative distribution function, with the exception that this information is not normalized from zero to one. This is normalized to the total annual frequency.

But it is tabular in nature. So, a couple of our challenges are, how do we sample this is a continuum basis, so that we can carry forward all of the uncertainties as well as the physical variability that is shown across the range of sizes?

MEMBER BLEY: From your introduction, is this what was used in your PRA? Or is this something new you are developing?

MR. LETELLIER: Let me ask David Johnson to answer that.

MR. JOHNSON: This is David Johnson.

The PRA is currently undergoing a major update, a new model or revision, if you will, which I

think is going to be released this month. They are transitioning to using an 1829-based LOCA characterization.

MEMBER BLEY: And this is that characterization?

MR. JOHNSON: This is one characterization. There are many characterizations that are possible.

The detailed answer to your question is no, but we are both based on 1829. There are some reasons why we are a bit different, but we have the same foundation.

CONSULTANT WALLIS: So, when you sample these breaks, the probability of a large break is so tiny, it will never happen unless you have a lot of sample. Well, not never happen; it is very unlikely to happen.

MR. LETELLIER: Right, and that is the reason for the non-uniform sampling strategy. I have a figure to explain that, if we have time.

Very quickly, I will throw out the topics of how we have to manipulate this information. You can question me on any details that we need to revisit.

So, the first step is to actually have a fit, some kind of smooth representation of the uncertainty

at any given size. To do that, we are using an optimized fit of a bounded Johnson probability distribution. It is unimodal, and we can fit this with very high accuracy to recreate, to be faithful to the expert opinion.

The next step is that we have to be able to interpolate between sizes. To do that, we are assuming that the underlying density function that supports this is uniform. So, there is an equal probability of incurring a break anywhere in the range of 7 to 14 inches and, similarly, for any interval.

Keep in mind, this is the underlying density. All right. So, if you accept that assumption, that implies a linear --

CONSULTANT WALLIS: You can go through it and differentiate it, couldn't you? You are just doing a stepwide thing here.

MR. LETELLIER: Essentially, but you have to know where your starting point is, and that is the density representation. So we are using a linear interpolation between these data points in order to get the probability of breaks in any size --

CONSULTANT WALLIS: Linear? You said it was uniform. So, it is linear on this graph? Okay.

MR. LETELLIER: I actually have not shown the interpolation between size bins, for that very

reason. I wanted to avoid the confusion.

All right. So, the next problem that we have, the next challenge that we have is that this information is not location-specific. It does not specifically apply to given reactor systems or given piping sizes. But, yet, there is a growing body of evidence on degradation mechanisms and failure initiation rates.

One of our early attempts was a bottom-up approach where we attempted to predict from in-service inspection data what the proportion of breaks in each size range would be. We were not successful in replicating this information.

So, the alternative approach is the top-down, to start with this global, cumulative viewpoint and redistribute, remap the information onto the plant locations. We are currently working on a compromise between these two extremes.

We have not shown explicitly, but we have an as-built CAD model that supports all of the geometry, all of the mechanics of postulating breaks anywhere in containment.

CONSULTANT WALLIS: Well, another approach would be to simply say we will look at the large break and we will vary the input, as shown here. And we will

do a whole run just for large break.

So, then, we get some CDF large-break-dependent. And you can do it for the other break sizes, rather than trying to model them all at once. Then, you get sort of a picture of how it varies. This might show up whether or not the large break or the small break are most effective or which is the worse, and so on.

MR. LETELLIER: We are essentially doing that. However, it was simply convenient to do the whole spectrum all at once. And then, we always present the information by category.

CONSULTANT WALLIS: How do you do that and make sure you don't get all small breaks?

MR. LETELLIER: I will show you.

CONSULTANT WALLIS: But, then, it is not fair. If you artificially increase the probability of large breaks, then you are not being quite fair.

MR. LETELLIER: We would never bias the distribution. That is where the non-uniform weighting comes in.

Here it is. Okay. So, the break frequency sampling, if you look at an exceedance function for a given type/size, and this one terminates at 18-inch pipe, an 18-inch pipe can actually experience breaks in

the large category, in the medium category, and the small.

So, we are currently dividing this. This is all notional. These are all free parameters that can be refined.

We are actually forcing the double-ended guillotine condition to always be present in every sample. Furthermore, in this example we are randomly sampling one, two, three, four additional breaks in the large category at this location. We are forcing two medium breaks at this location. And we only care about perhaps one representative small break.

Now the unbiased weighting comes by carrying the probability weight, and that is the differential probability that you were thinking of before. So, in our sampling strategy of every parameter, you always carry the value of the parameter and its associated weight. This should be very familiar to anyone from radiation-transport theory. This is the essence of Monte Carlo sampling for a phenomenological model.

There are additional details in the backup information. I welcome further conversations. But we need to move ahead to the next step in the sequence, which is debris generation and transport.

MEMBER SHACK: First, just on this thing, to be specific, I mean, are you somehow weighting these things towards, say, welds with fatigue or somehow associating with the degradation mechanism? Or is it any weld is random?

MR. LETELLIER: We are presently fully interrogating every weld location in containment. We are assuming there are multiple breaks at every weld. Now that begs the question, why not the through-wall conditions? And it comes back to the discretization of the parameter space.

What we believe presently is that there are sufficient number of welds to fully interrogate the combination of debris compositions, that there are enough welds to capture all of the interesting combinations of fiberglass and latent debris, et cetera.

MEMBER SHACK: But you are assuming they are at random, rather than the way the NRC does, with a high-energy line break where you --

MR. LETELLIER: That comes back to what I mentioned, the compromise between that uniform assumption, which is very simplistic, and the bottom-up approach, which requires some predictive capability for break phenomena. We haven't fully achieved that. So, we are working on a compromise that retains that

information for reactor type, energy of the line, erosion, histories, et cetera.

MR. MURPHY: So, let us do our musical chairs, and then Tim will take us to the next section.

MR. SANDE: As we are moving around, I will just give a brief background for myself. My name is Tim Sande. I have doing deterministic GSI-191 analyses for the last eight years, and I have supported most of the plants, most of the PWRs in the U.S., as well as some international plants and a few BWRs also.

So, I will be talking about a few different areas. We will try to move through these topics pretty quickly.

But my role in the South Texas work is basically to take the models that have been developed for the deterministic analyses and help develop the methodology for implementing them into the risk-informed framework.

So, right now, we are going to be talking about debris generation and transport and the accumulation on the strainers. This is a figure of this South Texas CAD model that we have got.

Basically, what we are doing is we are taking the data from the CAD model and we are importing

it into CASA Grande. So, we have all the insulation modeled on piping and equipment. We have got the concrete walls modeled as robust barriers that would limit the extent of the zone of influence, and we have got the weld locations modeled. So, we have the X, Y, Z coordinates for each weld on the RCS piping.

So, once we get that data into CASA Grande, now you have got a 3-dimensional representation. We are assuming the standard deterministic assumption of a spherical zone of influence, or ZOI, for a double-ended guillotine break. For a break on the side of a pipe, something less than the double-ended break, we are using the hemispherical ZOI. And those are very common deterministic assumptions.

And then, the size of the ZOI is dependent on the type of insulation you are looking at. For Nukon insulation, you have got a 17D ZOI as a standard assumption, and then it is scaled based on the break size. So, for instance, if you have a 2-inch break, you would have 17 times two would be a 34-inch radius ZOI. If you have got a 31-inch pipe break, then you have got 17 times time approximately 3 feet, and you have got a very large ZOI that takes up nearly half of containment.

So, Bruce just talked about the LOCA frequencies. Each one of those breaks has its own

frequency associated with it. Those double-ended guillotine breaks are very, very low frequency, but we are still incorporating that in the debris generation analysis to say how much debris gets generated for that particular case, and then walking through the rest of the analysis for transport and head loss, et cetera.

So, CASA Grande, it is very easy to put a sphere or a hemisphere, or any other shape of a ZOI you want, into the model. You can automatically calculate the insulation quantity for a particular case that you have selected.

Now we have got, as Bruce showed, multiple break sizes at each location. We have got hundreds of potential locations, hundreds of welds in containment. And then, if it is something less than the double-ended guillotine break, the direction of the jet matters, too, because that hemisphere will be pointed in whichever direction the hole is on the pipe. So, there are many, many different scenarios that we have to sample from, and we end up running thousands of different cases to look at --

CONSULTANT WALLIS: Now the ZOI calculation is deterministic? There is no uncertainty in the ZOI itself?

MR. SANDE: Currently, we are using the

standard deterministic approach.

CONSULTANT WALLIS: Just deterministic, right.

MR. SANDE: Right. Which is conservative. The way that the ZOI was determined has conservatisms built into it. However, it is very easy for us to say, well, what is our confidence in that 17D ZOI? What if it was a 7D ZOI or something less than 17?

CONSULTANT WALLIS: Well, then, you would have to sample that, too.

MR. SANDE: Sure.

CONSULTANT WALLIS: So, you have got the thousands of breaks, and then you have got sort of a number of uncertainties in the ZOIs. Okay.

MR. SANDE: Absolutely.

CONSULTANT WALLIS: It all multiplies as you go down the road, doesn't it?

MR. SANDE: Absolutely. And another variable is the shape of the ZOI. Realistically, it is not going to be a sphere. It will be some sort of a jet shape. So, we can modify that very easily and look at how a different shape of the ZOI might affect the results.

CONSULTANT WALLIS: You are saying it is very easy?

MR. SANDE: I mean, if you define the shape -- maybe Bruce can speak to what --

CONSULTANT WALLIS: Adjust the shape, if you adjust the shape, you can produce something?

MR. SANDE: Exactly.

CONSULTANT WALLIS: Right. Okay. That's all.

MR. SANDE: So, what I am trying to say is it is easy to do sensitivity studies to see how much it matters.

CONSULTANT WALLIS: It is not based on physics. So, it is just saying you adjust the shape and what does it do.

MR. SANDE: Sure.

CONSULTANT WALLIS: Okay.

MR. SANDE: So, one of the things that we actually discovered as a part of our initial quantification was that using that 17D ZOI isn't as important as we thought at first. It is very important if you are doing a deterministic analysis looking at the maximum quantity of debris that could get generated. But if you look at the realistic analysis saying that 31-inch double-ended guillotine break is a very, very low-probability event, and the stuff that is much more significant probabilities or frequencies are those

small breaks, well, the amount of debris you get generated from a 2-inch break times a 17D ZOI is pretty minimal. So, it turns out that the ZOI size itself is not as important in the risk-informed evaluation as it is in the deterministic evaluation.

CONSULTANT WALLIS: Unless it is only the large break that produces more than 15 grams.

MR. SANDE: Well, latent debris alone can produce more than 15 grams.

CONSULTANT WALLIS: Yes. Okay.

MR. SANDE: Yes.

CONSULTANT WALLIS: That is of fiber?

MR. SANDE: Yes.

CONSULTANT WALLIS: Per element?

MR. SANDE: Right. Yes.

CONSULTANT WALLIS: Three kilograms in total, right?

MR. SANDE: Yes. Generally, the commonly-assumed amount of latent debris is 200 pounds total with 30 pounds of --

CONSULTANT WALLIS: So, it doesn't produce the 3 kilograms of fiber that you need to bypass the screens to block everything in the core, if you are talking about downstream effect?

MR. ZIGLER: Yes.

MR. SANDE: Yes.

CONSULTANT WALLIS: It doesn't?

MR. ZIGLER: We will talk about downstream later on.

MR. SANDE: Yes.

MEMBER SKILLMAN: Please repeat, Tim, your comment about the latent debris amount.

MR. SANDE: A generally-assumed quantity or kind of a standard assumed quantity is 200 pounds total where 15 percent of that is assumed to be fiber. So, it would be 30 pounds of fiber, latent fiber, which using a density of around 2.4 pounds per cubic feet gives you 12 or 20, 12.5 cubic feet of fiber. If you convert that to grams per fuel assembly, it is well over 15 grams.

CONSULTANT WALLIS: It is a lot, yes.

MEMBER SKILLMAN: Thank you.

MEMBER ABDEL-KHALIK: Yes, since you are using a deterministic criterion for the zone of influence, why not do this as a lookup table where you do all these calculations ahead of time?

MR. SANDE: Bruce, would you like to answer that?

MR. LETELLIER: We are looking for opportunities to improve the numerical efficiency, and that is certainly one option. Because we have a finite

number of break locations, those become attributes, if you will. And, yes, we are trying to pre-calculate as much as possible. As we add layers of uncertainty, it becomes more of a computational burden.

CONSULTANT WALLIS: So, if 30 pounds of fiber is the standard left in containment -- before you break any insulation, right; that is just the residue that is left in there? -- you only need 6 pounds, or something like that, to get this 15 grams per assembly.

MR. SANDE: That sounds about right.

CONSULTANT WALLIS: This means that you have to somehow predict how much of this 30 pounds actually makes it to the assemblies?

MR. SANDE: Yes, but we also need to look at -- the 15 grams may be outflow for certain situations; it is not necessarily outflow for all scenarios. So, let's differ that --

CONSULTANT WALLIS: Well, suppose it were. You would still have to predict how much of this 30 pounds actually gets to the core?

MR. SANDE: Absolutely, yes.

CONSULTANT WALLIS: And that means you have to know something about how long the fibers are in containment and all that kind of stuff then?

MR. SANDE: Yes.

CONSULTANT WALLIS: Okay.

MR. SANDE: Okay. I will try to move through this.

CONSULTANT WALLIS: So, you have to figure out how it does this washing around in the next slide, too?

(Laughter.)

MR. SANDE: Sure. Yes. We need to figure out how much is generated, how much is transported, how much bypasses the strainer, how much accumulates in the core. All of those things are models that we have to --

CONSULTANT WALLIS: So, you need a magic wand to do that, but okay.

(Laughter.)

Well, you can do it.

MR. SANDE: What we are hoping to do, we don't have a lot of time today, but what we are hoping to do is show you that we have got robust methods for either conservatively or realistically assessing each one of those factors, so that the end result is something that is a reliable result.

CONSULTANT WALLIS: You use a kind of CFD that is illustrated in this picture here?

MR. SANDE: Yes. Let's go ahead and skip to that slide. So, this is actually from the

deterministic analysis that we did for South Texas a few years ago.

CHAIR BANERJEE: I remember that picture.

(Laughter.)

MR. SANDE: So, the actual transport fractions, we are not building a CFD code in CASA Grande. CASA will not automatically run a CFD model to predict what transport would be in the pool.

What we are doing for transport is, basically, taking the deterministic methods and analyzing what blowdown, washdown, pool fill, recirculation, and erosion would be. It might be different for different scenarios. A small break isn't going to have the same transport as a large break because the flow rates are much different and the water level may be different.

So, what we will do is we will develop transport fractions that apply to certain groups of scenarios. So, maybe for all small breaks we will have one set of transport fractions, and a different for medium, and a different for large.

CONSULTANT WALLIS: Are you using containment sprays during this period, too?

MR. SANDE: In this particular simulation, this is for a large break. And, yes, containment sprays

were on and incorporated.

CONSULTANT WALLIS: The washdown is also caused by that?

MR. SANDE: Right.

CONSULTANT WALLIS: Yes. Okay.

MR. SANDE: Yes, for small breaks, containment sprays wouldn't be initiated. And so, you would have less washdown in a small break than you would in a large break.

Another thing is the location of the break may play a role. So, a break in the reactor cavity may have a different set of transport fractions than a break in the steam generator compartments.

But the key that I am trying to point out here is that that will be done outside of CASA, just like the RELAP modeling is being done outside of CASA. And the outputs of that analysis are being plugged in as inputs to CASA to help determine --

CONSULTANT WALLIS: Have you got the time-dependent arrival of debris on the strainer?

MR. SANDE: Right.

CONSULTANT WALLIS: Now that means you have to know how the morphology of the layer develops depending on how things arrive at what time?

MR. SANDE: Yes, the time dependence is

very important. So, the stuff we have talked about up to here is very similar to the deterministic analyses.

CONSULTANT WALLIS: There haven't been experiments done with all kinds of different time dependencies of arrival. I don't quite know how you predict anything from that.

MR. SANDE: You are talking about the head loss itself?

CONSULTANT WALLIS: Yes.

MR. SANDE: Yes, let me defer that question to Gil's presentation.

(Laughter.)

You're welcome, Gil. Good luck with it.

CHAIR BANERJEE: Let me try to understand how this works. You are going to run a series of CFD calculations or something like that for bins of break sizes and locations, or something? How are you going to get the velocity field to get the transport?

MR. SANDE: Well, we may end up doing that or we may end up using the currency of D results. And I am saying that, for recirculation, this large-break result is conservative for all breaks.

CHAIR BANERJEE: Oh, okay.

MR. SANDE: And the washdown may be different for small breaks. So, we may make simplifying

assumptions to --

CHAIR BANERJEE: But you just describe a velocity field?

MR. SANDE: Well, yes.

CHAIR BANERJEE: Based on these calculations?

MR. SANDE: Right.

CHAIR BANERJEE: And then, you just look at the transport, given a source, any arbitrary source there, the particle paths?

MR. SANDE: Well, yes, built into the current transport analysis, the deterministic analysis, is an analysis of where that debris would be, based on whether it is blown down to the pool initially or if it washes down later in the event, and things like that. There is a very mature methodology that has been developed for doing those calculations deterministically.

CHAIR BANERJEE: Let's assume you know where the debris sources are. Some of it washed down. And now you have got to get it to the screens, right?

MR. SANDE: Uh-hum.

CHAIR BANERJEE: So, you, then, follow the particle plots in some way in that velocity field?

MR. SANDE: In the CFD, basically, the CFD

is used to determine regions where you would have transport or regions where you wouldn't. So, for example, on this picture here, this is analyzing two sump operations. So, you have one sump there and one sump up here. And you can see the red areas are regions where the velocity of the floor exceeds the tumbling velocity for a particular type of debris. I think this is RMI debris.

So, if you look at this, this region here all shows that the velocity is high enough to move the debris to the strainer. So, let's just say the distribution was uniform over all of containment. Then, anything over in this region would not be transporting because you don't have the high velocities.

CHAIR BANERJEE: But that is for the RMI, clearly. But if you had fiber or you had fine particulates, everything transports, right?

MR. SANDE: Yes. Yes, this would be for pieces of debris --

CHAIR BANERJEE: Right.

MR. SANDE: -- pieces of fiberglass or pieces of RMI or for fiber fines, like the individual fibers or particulate debris; that is if we assume all will transport, anything in the --

CHAIR BANERJEE: All will transport, but

the timing is what you are looking for, right, when it gets to the screen? Or you just assume it all gets to the screen?

MR. SANDE: The CFD is only used to figure out what is the total amount that gets to the screen. Now the timing is addressed by this last bullet. For recirculation, the stuff that is in the pool, you can look at the pool turnover, and there is a first-order equation that you can solve to say what is the transport over time.

CHAIR BANERJEE: So, that assumes a well-mixed pool or what?

MR. SANDE: Right. Yes. And fine debris would be well-mixed in the pool.

CHAIR BANERJEE: But the pool itself is not well-mixed, right?

CONSULTANT WALLIS: No.

CHAIR BANERJEE: There are plateau regions and there are recirculation regions.

MR. SANDE: Yes, initial distribution for fine debris, I mean, generally, you can assume uniform distribution because the fine debris that is generated in the ZOI is going to be blown throughout containment, and it is going to --

CHAIR BANERJEE: Well, that is your

assumption.

MR. SANDE: Right. Yes.

CHAIR BANERJEE: You assume that initial conditions are all uniform for the fine debris.

MR. SANDE: Right.

CONSULTANT WALLIS: Doesn't it matter about how it gets to the floor? Doesn't it go down stairwells and things? And doesn't that make a difference?

MR. ZIGLER: Bruce wants to answer here.

MR. LETELLIER: If I could clarify, we are assuming a conservatively-high fraction of fines, and we are further assuming that it is homogeneously-mixed and available for transport at all times.

CHAIR BANERJEE: And then, you just have a stirred-tank reactor sort of thing, well-mixed system. So, it decays exponentially.

MR. LETELLIER: Right.

CHAIR BANERJEE: Do you have a distribution of residence times? You assume some distribution of residence times?

MR. LETELLIER: We can in --

CHAIR BANERJEE: Like with CSDR or something?

MR. LETELLIER: We could do that for

different debris sources. In particular, we are curious about the chemical products. We are curious about the time rate of introduction for failed coatings.

CHAIR BANERJEE: Right.

MR. LETELLIER: And, of course, we are interested in the point-of-origin particulates and fibers. We tend to put those in at time zero; everything that is created in the ZOI that is available for transport appears in the pool.

CHAIR BANERJEE: But the reason I am asking this in more detail is, clearly, the devil here is in the details.

MR. LETELLIER: Uh-hum.

CHAIR BANERJEE: Because it is going to be crucial to you when the chemical stuff arrives at the strainers and the core.

MR. LETELLIER: Yes.

CHAIR BANERJEE: And as well as the fibers and the particulates. So, that seems to me that you need to follow the particle paths in some way to do that, given a certain velocity distribution. That is not a very difficult calculation to do. It takes a little bit of computation, but you can probably write that program in a week or something.

MR. SANDE: Yes, and I think that --

CHAIR BANERJEE: Do you follow what I am saying?

MR. SANDE: I think I understand.

CHAIR BANERJEE: If you use it as a well-mixed system, everything is going to, more or less, arrive together, the particles, the fibers, as well as the chemicals.

MR. SANDE: Well --

CHAIR BANERJEE: Other than the dissolution time for the chemicals, you have a source --

MR. SANDE: Yes, that is the key.

CHAIR BANERJEE: Yes. So, you are going to have to make it a little bit more sophisticated, I think.

MR. ZIGLER: I would like to bring the attention to the backup slides. All of this is populated into the debris transport logic trees, which for this program is pretty comprehensive. So, there are debris transport logic trees for each kind of debris and, also, for each size distribution of the debris. And each one of them has its own unique percent transports and the different phases on it. So, there is an underlying third dimension of the transport logic tree which should be representative in your backup slides.

CHAIR BANERJEE: Okay. But, ultimately, you are driving this all with a velocity field. All

right? And your velocity field is something you fixed, essentially? Or you are not recalculating that?

MR. SANDE: I don't know if I completely understand what you are getting at there.

CHAIR BANERJEE: Imagine that I put something in at the yellow point there, right? And it has to get to the sump --

MR. SANDE: Yes.

CHAIR BANERJEE: -- wherever the sumps are. So, the transport time depends on the velocities that you have. So, this moves along some line and ends up at the sump, the mean flow field.

MR. SANDE: Yes.

CHAIR BANERJEE: If it is in one yellow, it takes a longer time than the other yellow, for example. Okay?

So, are you taking that into account, because --

MR. SANDE: Not right now, no.

CHAIR BANERJEE: Not right now? Okay. So, to me, it seemed that the crucial part -- I mean, all this is great, but at the end of the day what is really important is the fact that the chemicals may not arrive for long enough that you don't plug up the core.

MR. SANDE: Yes.

CHAIR BANERJEE: And by then, you have got your hot leg injection on. At the end of all this, that is what really makes some sense.

CONSULTANT WALLIS: Sanjoy, it is not just time. It is also place. I mean, if there were a snowstorm and the drifts formed don't form uniformly everywhere -- they are blown around by the wind, just the way this stuff is blown around by the liquid -- but it is not just the time of arrival. If you followed all those trajectories, you would find that there was more debris in some places than others, wouldn't you?

CHAIR BANERJEE: Well, because you are injecting more -- you know, he has got a distribution of break sizes and break locations.

CONSULTANT WALLIS: No, but I am saying, how is it distributed around?

CHAIR BANERJEE: Plus washdown.

CONSULTANT WALLIS: Are there areas of the strainer with no debris and some other areas with lots of debris? Or do you just have it cover everything uniformly?

MR. SANDE: It depends on the debris type definitely. I like the suggestion you are making. I don't know that it would be practical to implement because we don't know exactly where the debris is. We

have some assumptions that we have to make because you can't predict exactly where a particle of debris is going to transport during blowdown and washdown.

MR. MURPHY: This may be one of those opportunities to take in the item you gave us for consideration and work with it, so we can move on through, if that is okay.

CHAIR BANERJEE: Yes, let's go on. I agree. I was just looking for a clarification as to what you are doing. I think I have got a picture. So, move on.

MR. SANDE: Yes, there are some other things on here, but let's go ahead and move on, so that we have time to get through everything.

MR. ZIGLER: Okay. My name is Gil Zigler.

It is very nice seeing Dick Skillman over here. It is where I started my career, was with B&W. At that time, Dick was actually one of my initial bosses, believe it or not.

Anyway, now I am with Enercon Services, having joined my previous manager of 16 years, Dr. Peter Mast, who is in the audience over here.

Over my career, I have been actively involved in ASME codes and standards. So, you should have seen me involved in here. I am a member-at-large

of the Board of Nuclear Codes and Standards.

I am the founding Vice Chairman of the Committee on Nuclear Risk Management that brought you the PRA standard. Currently, that Committee is chaired by Rick Grantham, who, unfortunately, is not here from South Texas Project. I am also a member, almost founding member of the Operation and Maintenance Committee.

Back in the eighties, in the nineties timeframe, I was working as a manager of the Technical Support Service to the Office of Safety Issue Resolution of the NRC. And in December of 1992, Marty Virgilio tapped me on the shoulder and said, "Let's go over to Sweden because the Swedish regulator is having a meeting to discuss the Barseback event." That happens to have been 20 years ago. So, we are coming right on the 20th anniversary of the Barseback incident at this time.

Since that time when we came back, Marty informed us that what we had was a LOCA without ECCS and proceeded to fund the whole efforts of NUREG-6224 that brought you the first ideas of how to go about doing the debris generation transport head loss calculations. And 6224 was a risk-informed study on it where we did, what we considered at that stage back then as state-of-the-art, a break frequency analysis. The

conclusion was that there was a high probability of having significant core damage, given a break in the primary system of a BWR.

Since those days, I have transitioned over to the commercial side, have been involved in the design and implementation of about 60 percent of the BWR fleet, of their new strainers on it, and transitioned over to the PWR. I was involved in writing, supporting the writeup of NEI 04-07.

And in this function over here, in the South Texas Project, when it came up, Rich Gratham tapped me on the shoulder and said, "Come on, we will need to get this risk-informed GSI-191," which I leaped at the opportunity.

My role over here in this function is to provide overall guidance in all the aspects, ranging from debris generation down through the in-core side of the fence. My particular specialty, subspecialty, on all of this is the conventional head loss side of the fence on it.

We needed for CASA Grande to come up with some sort of a correlation, given a debris quantity of fibers and particulates, to come up with an anticipated head loss from a conventional side of what that head loss would be. We looked at the available correlations and

settled on something that was very familiar to me and to Bruce Letellier, also, with the 6224 head loss correlation.

We fully understand the 6224 head loss correlation has not been validated in the space where we are of South Texas Project. .15 feet per second was the lowest velocity where 6224 was tested. We are in South Texas going all the way down to .01 -- it is really .009 -- but .01 feet per second. We need to verify.

The other thing that is very favorable for the implementation of 6224 for the initial verification was that we are looking at strictly two types of debris, the fiber debris and particulate. Those are very well-known, very well-behaved. There is an extensive database, a worldwide database, for that matter, that can seen in the OECD NEA reliability database for BWRs.

Nevertheless, we have fully understand that we need to have some sort of an assurance that the 6224 head loss correlation would be valid. One of the plans that we are going to be doing in this year is to do quite an extensive amount of vertical head-loss tests to verify the validity of the 6224 correlation, starting off with addressing, first of all, how 6224 was developed with using conventional tap water and using head loss that we can generate 1.5 to 2 feet of water with an eta

of about phi beta being the particulate-to-fiber ratio.

So, we will validate, first, the loop under conditions that we have a good amount of data at the higher-approach velocity. Then, we will start transitioning from there to validate where 6224 works, indeed, into the buffered borated water conditions of the South Texas Project.

Once we have validated that it does, indeed, work under those conditions, then we will transition using the buffered borated water, looking now at the debris loads that we are anticipating from South Texas.

CONSULTANT WALLIS: Gil, can I ask you something here?

MR. ZIGLER: Sure.

CONSULTANT WALLIS: The NRC, in its evaluation of strainers, doesn't accept 6224. It accepts these POOF tests when you do something which is supposed to be prototypic and you measure.

MR. ZIGLER: Uh-hum.

CONSULTANT WALLIS: But can you use those tests to so validate 6224 or at least give some idea of its uncertainties?

MR. ZIGLER: The answer is yes, but let me explain 6224 is a flat-plate correlation. The strainers that we are using at South Texas, fortunately,

they are using a strainer that does, indeed, attempt to have equal velocities on each one of the plates. So, the horizontal plate has a propensity of being more applicable to the South Texas Project-type strainers.

We have done extensive testing on strainers that were designed intentionally for non-uniform debris loadings on it. Those, the correlation really, really does not look good at all. One has to make some bold assumptions about how the non-uniform bed is formed. So, on those cases, the data is less available.

CHAIR BANERJEE: Have you done any testing on these strainers yet?

MR. ZIGLER: The testing has been done on those strainers on it.

CHAIR BANERJEE: Where was it done?

MR. ZIGLER: It was done at Alden Research Labs on it. Some of the data is applicable to our case, and that is what we have used to look at the impact of the --

CHAIR BANERJEE: Plant-specific testing?

MR. ZIGLER: What?

CHAIR BANERJEE: You haven't done any plant-specific tests?

MR. ZIGLER: Those strainers underwent plant-specific testing; that is correct.

CHAIR BANERJEE: Have you submitted those results to the staff?

MR. ZIGLER: Yes, the staff has already those results.

CHAIR BANERJEE: Do they include chemical effects?

MR. ZIGLER: They include chemical effects also.

MR. SANDE: That was part of the deterministic evaluations.

MR. ZIGLER: Right.

CHAIR BANERJEE: Okay.

MR. ZIGLER: But the chemical effects testing that was done used the WCAP precipitate. Kerry here, or Dr. Howe here, will shortly discuss how we firmly believe that the applicability of the WCAP amorphous phase perhaps is not so applicable. It is another reason why we hope, based on the chemical testing that will be undergone at the University of New Mexico, to revalidate again that, yes, we can use 6224 correlation, even extending it into the realm of the chemical side of the fence.

CHAIR BANERJEE: So, testing currently, if I understand your implication, that your strainer designs or the amount of area you are putting in, and

so on, using the staff procedures and guidelines, re showing unacceptable results or acceptable results?

MR. ZIGLER: There are some data points associated with the chemical side of the fence which was pretty high on it. And that is the issue, why we are approaching it that way; that's right.

CHAIR BANERJEE: So, now you are trying to figure out what to do?

MR. ZIGLER: And there are some other issues. Some of the debris simulates that were issued -- and I don't want to go into the trivia, into the details of it, but part of the validation of the correlation is that we will be conducting yet another tank test with those strainers with the typical debris loads that we anticipate.

CHAIR BANERJEE: So, the problem, if you take the current set of tests and the current staff guidelines, your head losses are too high?

MR. ZIGLER: Yes.

CHAIR BANERJEE: And you would either have to remove insulation or you have to go another route? Is that what I understand?

MR. ZIGLER: Yes. Yes, it is what triggered where we are.

CHAIR BANERJEE: So, that is the summary,

and this is a potential path to follow?

MR. ZIGLER: Right. And what we are doing here is that CASA Grande needs to have a robust correlation that we had a high degree of --

CHAIR BANERJEE: What happens if it doesn't work, the correlation? What does CASA Grande do?

MR. ZIGLER: Well --

MR. LETELLIER: If I may, as was mentioned earlier, there is no specific requirement to defend one correlation. We can have alternatives, and we can modify as needed.

MR. ZIGLER: Right.

CHAIR BANERJEE: Because I think there is an extensive study that Professor Wallis did at one point as to what was wrong with the correlation.

MR. LETELLIER: Of course, and there is no reason that we can't compare those alternatives, the Prodiact formulation, 6224, the Wallis new and improved, et cetera.

CONSULTANT WALLIS: Well, isn't there a problem that, even of the correlation works, the effect of chemicals tends to produce a much higher pressure drop? We don't have a prediction for that.

MR. ZIGLER: Yes, Dr. Howe over here will be talking about how we are firm believers that the

chemical precipitate that is for the South Texas conditions are not the amorphous WCAP type of a precipitate, which will have a significant impact on the head loss, a significantly different impact on the head loss.

And I would like to turn it over to him right now at this stage, so we can look at exactly -- to answer your question.

By the way, there are some backup slides on head loss in which I talk exactly, hopefully addressing some of the points of Dr. Wallis' concerns about 6224 and why we think it is applicable to where we are.

CHAIR BANERJEE: Okay. Let's move on.

MR. HOWE: Are we moving on?

CHAIR BANERJEE: Yes.

MR. HOWE: Okay.

CHAIR BANERJEE: Thank you.

MR. HOWE: My name is Kerry Howe. I am at the University of New Mexico. My area of expertise in this project is water treatment and water chemistry.

On an ironic side note, a little over 20 years, I was involved in a pilot study for the process selection for the treatment processes for the Camden, New Jersey water treatment plant.

(Laughter.)

So, it just made me smile this morning.

CHAIR BANERJEE: Is that the water used in Ewing, New Jersey?

(Laughter.)

MR. HOWE: I looked it up on my phone to see how far away it is. So, I don't know if it is the same plant or not.

The point, though, I think is well-taken that water is not just water, that as we go around the country, it depends on whether you are getting your water out of the Potomac River or the Delaware River or Lake Superior. I mean, it is all different. So, the differences that you see in your testing are not surprising to me.

CHAIR BANERJEE: But this is reactor water, which has been in touch with a lot of other things.

MR. HOWE: But they do their head-loss testing in tap water.

MR. ZIGLER: A point of clarification, all of our head-loss testing will be done with demineralized, borated, buffered water. So, we want to eliminate this whole side effect of "What if," "What if," "What if?"

CONSULTANT WALLIS: With realistic temperatures? Do you do it at realistic temperatures?

MR. ZIGLER: We will probably, given your interest in temperature, we will probably run temperature at 195 to revalidate making sure where we are.

CONSULTANT WALLIS: It reduces some of the uncertainties if you do that.

MR. ZIGLER: Absolutely.

CHAIR BANERJEE: Dr. Wallis likes felt.

(Laughter.)

MR. ZIGLER: We are looking forward that Dr. Howe over here will invite Dr. Wallis over there, and we could have so many tests at his facility.

MEMBER ARMIJO: If you would invite me as well, I would very much like to go there because I really think that the only thing unique about this whole thing is the chemical effects. The chemicals that we are talking about have been treated as chemically-inert and non-interacting in any physical way. It is totally artificial. So, somebody has got to really dig into that part of it because that is the weakness.

CHAIR BANERJEE: But, you know, Sam, just --

MEMBER ARMIJO: The thermal hydraulic guys have done their thing, but the chemistry is weak.

CHAIR BANERJEE: In defense of Argonne and

all these other people, I must say that there has been an extensive amount of work done in order to develop the surrogates today.

MEMBER ARMIJO: I am not criticizing the surrogates. I am just saying, going after that, it hasn't really developed. It is just sort of stopped there.

MR. HOWE: Our intention is to build on that existing information.

MEMBER SHACK: We have had all sorts of things and irradiated up to --

(Laughter.)

I mean, borating is easy. Deionizing is easy. After that, it gets a little trickier.

MEMBER ARMIJO: I have some ideas there, but these are bench-top tests.

MR. HOWE: So, you mentioned ICET. I was one of the investigators.

CHAIR BANERJEE: You were involved with ICET?

MR. HOWE: I was one of the investigators on the ICET test where we identified the extent of the chemical effects on GSI-191.

I have got nine slides here in this presentation that relate to chemical effect. I am going

to try to cover two of them.

(Laughter.)

I am going to try to keep my message to two things. So, let's go to slide 38 to initiate this discussion.

So, the first question might be, well, why are we reopening chemical effects? The first point is we are doing more experiments. And the first question is, why?

CHAIR BANERJEE: We like experiments.

(Laughter.)

MR. HOWE: And these guys are willing to pay for it.

(Laughter.)

This slide I think tries to set the stage for why we want to look at things in a little bit different detail. So, when we did the ICET test, we looked at a range of conditions. That and other things led to the WCAP formula for how to do the head-loss testing.

You can see that in a couple of cases, ICET tests 1 and 5, with all the other data that was collected in support of the WCAP, those two tests tend to fit the WCAP equation that is used. And the Y-axis here should be aluminum corrosion rate. Sorry.

CHAIR BANERJEE: You are using TSP as your buffer?

MR. HOWE: In ICET test 2 the TSP was the buffer. The pH was right around 7. And those conditions were fairly similar to the South Texas plant.

Now what is significant in this graph is, again, the Y-axis is on a log scale. So, if we look at the ICET test that was most similar to South Texas, we are looking at an aluminum corrosion rate that is an order of magnitude lower than what the WCAP formulation will predict.

So, if we are going from an industry-bounding situation and a deterministic situation to a plant-specific situation, then it is worth reopening this question and trying to understand a little bit more specifically what was different about ICET test 2 which is most applicable to South Texas.

CHAIR BANERJEE: What was 5 and 1?

MR. HOWE: Five and 1 were the two cal-sil tests. I'm sorry. No, 5 and 1 were the two high-fiber, high-pH tests. Okay?

MEMBER SHACK: I mean, it is the NaOH that really drives the aluminum corrosion.

MR. HOWE: Correct. Right. Correct.

So, we could spend an hour on this one graph

and I could tell you what is happening here.

CHAIR BANERJEE: So, 3 and 4 were what?

MR. HOWE: Three and 4 were the two cal-sil tests.

CHAIR BANERJEE: yes.

MR. HOWE: There was high silicon, which one of the strong hypotheses here is that silicon in the water leads to inhibition of aluminum corrosion. So, the presence of the silicon essentially pacifated the aluminum surfaces and led to less corrosion in tests 3 and 4.

ICET test 2 also had higher silicon, but not as high 3 and 4. But there was also the TSP, the phosphate in the water. In the water treatment industry you use phosphate, again, as a corrosion inhibitor.

So, we have got things like silicon and phosphate that on a plant-specific basis can change the parameters of aluminum corrosion. So, if you look at the potential source-term for the chemical effects on the strainers, we need to investigate what is a situation that is more specific to South Texas.

CHAIR BANERJEE: Kerry --

MR. HOWE: Yes?

CHAIR BANERJEE: As you know, even fairly small amounts of chemical have a very large effect. So,

if you look at the experiments that you have seen, if you are even 1/10th or less, you still see a very large effect.

MR. HOWE: One-tenth of some number.

CHAIR BANERJEE: Let's say for the in-core effects. Typically, if your bounding number is 800 or something grams per assembly, if you go down to 100, you still see a very large effect almost immediately.

MR. HOWE: But do you anticipate that that goes down to zero?

CHAIR BANERJEE: Are you saying that --

MEMBER SHACK: Is STP a relatively-low aluminum plant, for example?

MR. ZIGLER: There is a critical assumption on this discussion here, which is the other hypothesis that Kerry is going to show here is that the formation of what we anticipate to be the structure is crystalline in nature, not amorphous. Yes, we agree the amorphous is a real problematic issue.

I mean, I can show you how, when I first started doing my first head-loss testing on this, when the loop went bingo on us, and we tried to predict some sort of a curve that we could look at it, and it behaved highly non-Newtonian. I mean, the difference between a few more drops in the vertical head-loss loop went from

literally nothing to a formation of a debris bed that was essentially, when we opened the spigot in the bottom, the column of water in the vertical head loss sat there and dripped.

CHAIR BANERJEE: Let him continue.

CONSULTANT WALLIS: Very small change has an enormous effect.

MR. ZIGLER: Highly non-Newtonian in the form of the WCAP precipitate. And Kerry here will be talking about --

CHAIR BANERJEE: We understand what you are going to do, revisit ICET.

MR. HOWE: We are not only going to revisit ICET, but we are going to extend that. And I do want to dwell on the aluminum for a second.

Life is a continuum. So, from zero to some number as a huge problem, there is some point in the middle -- and we don't know where that point is -- where there is a threshold. Are we above or below that threshold is worth investigating.

We do use aluminum precipitation in the water treatment industry. We have been using this process for 100 years to remove particles from water. The water you are drinking today has been treated that way.

CHAIR BANERJEE: So, let me ask you something. You are going to revisit some version of ICET, just to get it clear. And then, what are you going to do after that?

MR. HOWE: So, that is a great question.

CHAIR BANERJEE: Yes.

MR. HOWE: What was done in ICET was to identify whether corrosion is a problem and whether precipitates will form. Those results were taken into the vertical head loss loops, where we discovered that precipitates can cause huge problems.

Those are separate effects, and there is a disconnect there. If I take a large amount of aluminum and dump it into a bucket all at once, I can get it to precipitate rapidly, and it will precipitate as an amorphous phase.

Corrosion is more complex. And so, what is going to happen is aluminum is going to be released in the solution at a slower rate. I am not going to suddenly and instantaneously produce a solution that is over my solubility limit which forces things to precipitate, but I am going to gradually come up to a solubility limit.

And the question is what happens when I gradually raise my aluminum concentration to reach a

precipitate limit in an environment where I am circulating water through a dirty environment. What happens with precipitates is they tend to form -- they don't spontaneously nucleate in a homogeneous solution. There tends to be heterogeneous nucleation, which means it happens on surfaces.

Okay. So, if I am circulating through the debris bed and I am gradually bringing my aluminum concentration up to the solubility limit, there is an expectation that what happens is we start essentially precipitating or plating-out aluminum hydroxide onto the fiberglass or other places in containment as a crystalline phase, because it is finding the nucleation sites, which may have an entirely different head loss behavior than the spontaneous homogeneous precipitation, which is the way the --

CHAIR BANERJEE: Is this a high-aluminum plant?

MR. HOWE: I don't know the comparison.

MEMBER SHACK: Is it like 5,000 square feet?

MR. HOWE: West was 6700 square feet, but I don't know if that is high or not.

CONSULTANT WALLIS: This is very interesting. You are revisiting ICET or revisiting all

the questions we asked at that time, all these questions: what is the form of the precipitate? You are going back to the old correlations for head loss. So, you are redoing the entire research history we have been through for --

CHAIR BANERJEE: But for a specific plant.

CONSULTANT WALLIS: Yes, for a specific plant. It is very interesting. So, you are questioning all the tracks we have been on before.

MR. HOWE: The other slide I am going to try to work from is slide 41, which is try to address our strategy, and maybe that answers your question, Dr. Wallis.

So, we are going to do several different things on this overall test. We are going to revisit ICET in the context of doing some 30-day tests. These will be different because what we are going to do is we are going to be using the ICET tank, but we have retrofitted that where we now have head-loss columns piped into the tank. We will be circulating water out of the tank, through some head-loss columns, and then back into the tank. So, we can have corrosion materials happening in the tank and be feeding that water through our head-loss columns. Some of this was done at Argonne in a little bit different configuration. To where we

can look at what happens from a head loss perspective when the aluminum is released into solution at the rate at which corrosion occurs.

Let's just focus on the two pictures on the right side. The top one is the ICET tank. It is actually SonarLabs, and we have got that up and running again.

The lower picture is the three head-loss columns that we have built. And so, what we are doing is in the interest of having some reproducibility of our data, instead of taking water out of the tank and running it through one column, we have built three in parallel. And so, we will have three separate debris beds there, and we will have each one instrumented with flow rates and differential pressure cells.

MEMBER SHACK: These will be pre-formed fiberglass beds?

MR. HOWE: Yes. Yes. So, the goal here is we will form a debris bed with fiberglass. Once those three beds are formed and are somewhat consistent with one another, then we will pipe in the tank and start the 30-day --

CONSULTANT WALLIS: I thought fiberglass alone didn't produce chemical effects in testing. You have to have some particles in there to catch the

chemical.

MR. HOWE: Yes.

CONSULTANT WALLIS: What are you going to do about the particulates?

MR. HOWE: There will be a representative debris bed that will have fiberglass and particles in it.

CONSULTANT WALLIS: And particles?

MR. HOWE: Yes. The exact form of that we haven't --

CONSULTANT WALLIS: So, you know how to create a representative debris bed?

MR. HOWE: I have students right now working on that.

MR. ZIGLER: It is we are creating a debris bed of fiber and particulate which we have a head-loss measurement on it, which we are going to be using that debris bed as an instrument to detect the formation of precipitates in the debris bed.

MEMBER SKILLMAN: Kerry, the third bullet, realistic temperature is what, please?

MR. HOWE: So, the objective here, one of the issues that we want to look at, which I will touch, is the ICET tests were done under constant temperature for the 30 day, 104 degrees Fahrenheit. What we want

to do is follow the temperature profile of a LOCA. And so, we will start at a high temperature and let the temperature decline over the 30-day period consistent with --

MEMBER SHACK: What will you do to kick up the corrosion for that part of the LOCA that you can't simulate?

MR. HOWE: Our intention is to add extra material during that period of time and then pull it out.

So, our initial temperature on the tank is intended to be 185 degrees Fahrenheit. There will be a short period of time at the beginning --

MEMBER SHACK: Do safety people all like this?

MR. HOWE: We are not boiling. Our new tank has a lot more insulation.

CHAIR BANERJEE: And probably you have easier safety people than Argonne has.

MEMBER ARMIJO: These are glass columns? These are glass columns, not --

MR. HOWE: Those columns are stainless steel and polycarbonate.

CHAIR BANERJEE: Knowing universities, it is a lot easier to get stuff done there.

MEMBER SKILLMAN: Please finish. A

hundred and eighty-five down to --

MR. HOWE: Yes. And so, one of the outputs from some of the other team members will give us a temperature profile over a 30-day period. And so we will allow the temperature in the tank to decline at the rate or consistent with a nominal LOCA. I don't have those numbers yet.

MEMBER SKILLMAN: Okay.

MR. HOWE: But those will come from Adolfo.

MEMBER SKILLMAN: Okay. Got it. Thank you.

MEMBER SHACK: One of the things we had at Argonne when we tried to do this is that that initiation is kind of a random process. You could be sitting there waiting, waiting, waiting, and nothing is happening. And then, all of a sudden, boom, the flow is stopped.

It is hard to know on a 30-day test, for example that you have done it exactly right. But maybe that is your point, that if you can get the 30 days, that is longer than you really need to get past your hot leg switchover.

MEMBER STETKAR: So, Kerry, just one question as far as interface with the PRA. We originally saw everything going up into the PRA model. There is some likelihood in a risk assessment that you

have different temperature profiles.

If I have got three trains of things, running two trains versus one train can affect those temperature profiles. Are you going to take that into consideration? When you said that somebody else is going to give you that temperature profile, are there one or two or three of them?

MR. JOHNSON: Yes, this is David Johnson.

We are still working on that interface.

MEMBER STETKAR: Okay.

MR. JOHNSON: But if we see that the temperature profile is an important issue, then the interface will include how many trains of fan coolers, sprays, et cetera, maybe even seasonal variation of surface water temperatures.

MEMBER STETKAR: Yes.

MR. JOHNSON: But we understand that there are a number of interface issues.

MEMBER STETKAR: Okay.

MR. JOHNSON: I think in the unlikely event I get to my slide, that was what I was going to say.

(Laughter.)

MEMBER STETKAR: I might have saved you some time.

(Laughter.)

CONSULTANT WALLIS: Can I understand what you are doing?

MEMBER ARMIJO: I would like to ask a question here. The particles you are going to work with, is it going to be silicon carbide or is it going to include materials that are already in the reactor? Iron oxides, nickel oxides, these kinds of things, there is a lot of that stuff there. I don't know if it is equivalent in mass to what you generate during the blowdown.

MR. HOWE: Let me answer that question by saying, again, what we are intending to do is for these debris beds to essentially be instruments to demonstrate whether or not chemical effects happen in the same way that they did in the WCAP. So, we want to have a debris bed that we can reproduce that has some nominal head loss and will register a significant increase in head loss if they are exposed to the WCAP goo.

And so, in that context, we are planning on using fiberglass and silicon carbide as kind of a baseline bed.

MEMBER ARMIJO: It is a reference filter.

MR. HOWE: It is a reference filter.

MEMBER ARMIJO: Okay.

MR. HOWE: Before we do our first test, we

will add in the WCAP and make sure that we see an increase in head loss. Okay? If we don't, then we have the wrong kind of bed.

MEMBER ARMIJO: Yes.

MR. HOWE: And if we see an increase in head loss when we have the WCAP goo, we will feel like we have got a representative instrument. Then, we will go to the 30-day test and see if we get the same kind of response with the same kind of aluminum concentration. My hypothesis is we will see a different response.

MEMBER ARMIJO: Okay. Just one other little question, in fact. From the 185 to the lowest temperature you get within 30 days, is that a couple of orders of magnitude reduction in corrosion rate? Or it is pretty flat?

MR. HOWE: It is not real flat. I don't think it is a couple orders of magnitude, either.

MEMBER ARMIJO: Okay.

MR. HOWE: I would have to look up the numbers, but there is a temperature dependence on both the corrosion rate and the solubility limit for precipitation. That is one of the reasons that this temperature profile is important, because at the higher temperatures early on is higher corrosion. So, if we reach a saturation point at a point where the temperature

is still declining, now we have become supersaturated with respect to the precipitate because the precipitation limit is going to be lower later in the 30-day test. When or if that happens is going to be an important question.

MEMBER ARMIJO: Yes. Okay.

MR. ZIGLER: And to answer one of the questions that you may have come up with, Dr. Wallis, the first test that will be conducted, 30-day test, will be strictly the fiberglass debris beds with nothing in the tank, with buffered borated water on it, following the temperature profile.

So, we do two things. We address the temperature effect issue on it, and we also address the long-term degradation of the fiber bed. So, we have that as an underlying database that we clearly understand what the head losses are through those fiber beds.

CONSULTANT WALLIS: So, what I understand is, rather than accepting the Westinghouse surrogate, you are going to make your own precipitate in what you think are more realistic conditions?

MEMBER SHACK: Well, but, then, he is going to assume that his solubility in this test is somehow related to the solubility in the reactor.

CONSULTANT WALLIS: That's right, but he is going to try to be more realistic about what would happen in the sump, rather than accepting --

MR. HOWE: But we want to start with that surrogate as a baseline.

CONSULTANT WALLIS: As a baseline, yes, but then you want to see if what is realistic comes up with the same answer or something better or worse.

MR. HOWE: Better or worse, we want to find the right answer.

MEMBER SHACK: Like ICET, you are going to put in concrete and other materials as well as the aluminum?

MR. HOWE: There are some things from the ICET, for instance, uncoated steel, you know, all the tests that have been done since then have demonstrated that that is not a player. And so, we are not going to put, for instance, the uncoated steel, and the concrete is going in. Anything that was --

MEMBER SHACK: Concrete seems like, yes, one of the more critical ones.

CONSULTANT WALLIS: How much sawdust are you going to put in, to bring back the question that we had earlier?

(Laughter.)

MR. HOWE: I am going to let Bruce Letellier answer the sawdust question. Okay?

MEMBER SKILLMAN: If I could, on your sketch on page 43, please, are the flows vertically through the column upward?

MR. HOWE: Downward.

MEMBER SKILLMAN: They are downward?

MR. HOWE: Yes.

MEMBER SKILLMAN: Okay. Thank you.

Does that make a difference, downward versus upward?

MR. HOWE: The debris bed will be formed on a vertical screen. So, we are going to add the fiberglass and particles, as was mentioned, before we start the chemical test. So, we will let the debris go in the top and form this debris bed. And so, we will have formed debris beds on these three horizontal screens, and then start the chemical tank, adding stuff in. In order to get a consistent debris bed at a velocity of .01 feet per second, I think it had better be horizontal and downward.

CONSULTANT WALLIS: Otherwise, you wouldn't be able to carry it up.

MR. HOWE: Yes. I don't think we would have enough velocity to form a uniform debris bed in any

other orientation.

MEMBER SKILLMAN: Thank you.

MR. ZIGLER: And I would bring to your attention the orientation, the pump is upstream of the debris bed. And that has a reason for its being there. It is to take care of degasification.

CHAIR BANERJEE: Okay. We really must move on now.

(Laughter.)

MR. HOWE: Yes.

MR. MURPHY: We have got two more topics to cover. Tim will take the next one.

CHAIR BANERJEE: You really have a hard stop at 5:15. Five minutes each, and we will stop it.

(Laughter.)

MR. SANDE: We had a day and a half of in-core blockage, and I will see if I can do it in two minutes.

(Laughter.)

Just very briefly, what the industry has done so far haws been looking at the bounding scenarios, conservative assumptions, bounding scenarios, and oftentimes conservatisms lumped on top of each other, which has given what in my opinion is a very conservative result.

What we want to do is approach this problem realistically. Just like we are approaching everything else as realistically as we can, as realistically as practical, we want to approach the in-core issue realistically also.

So, there are four high-level scenarios. You have got cold leg breaks with both cold leg and hot leg injection. Those flow paths make a big difference. And then, you have got hot leg breaks with both cold leg and hot leg injection. So, we will be looking at each of those scenarios.

The switchover time at South Texas to hot leg injection is about five-and-a-half hours after the start of recirculation. Strainer bypass will predominantly occur within five full turnovers, based on what we were talking about earlier with the time it takes for fines to transport to the strainer. Most fines will transport to the strainer within five full turnovers, which is about two hours for a large break or it could be a couple of days for a small break.

And then, chemical precipitation is not likely to occur for several hours or days. So, it is likely that we won't see chemical precipitation until after we switch to hot leg injection. Now that will be confirmed by the chemical effects work that we are doing.

So, this is our high-level plan for how we are addressing core blockage. We are still developing the details of this. But the first thing is to perform an initial evaluation of those different scenarios that we have got, look at things like the Owners' Group has looked at. What is the driving head, the height of the steam generator tubes, and how much head of water will you have for the different scenarios? As well as break sizes, there is a big difference between a small break, where the RCS may be essentially full of water, the water is over the top of the steam generator tubes, compared to a large break where that may not be the case.

We are going to use RELAP5 to simulate full blockage at the bottom of the core. We have actually done some of those initial evaluations already. And we are going to look at the different scenarios and say, what happens if I have got a medium break on the cold leg side and full blockage on the bottom of the core? Does that go to core damage or not?

CONSULTANT WALLIS: Full blockage means no flow at all?

MR. SANDE: Absolutely. You can flow come from the top down, but you can't have flow going from the bottom up.

And so, we have run some of those scenarios.

The initial results show that in some cases, even if you have full blockage, you don't go to core damage because the water can go around the steam generator tubes and cool the core from the top.

Now some of them would go to core damage. So, obviously, full blockage isn't acceptable for all scenarios.

(Laughter.)

So, the cases where full blockage would lead to core damage, we want to look at those cases in more detail. Those are a limited number of cases. So, what we will do there is we will take our time-dependent transport analysis. We haven't really talked about bypass, but we are planning to do bypass testing to get a good determination of what the bypass quantity is under different conditions.

CONSULTANT WALLIS: You mean in the annulus?

MR. SANDE: I am talking about strainer bypass.

CONSULTANT WALLIS: Strainer bypass?

MR. SANDE: How much fiber gets past the strainer.

CONSULTANT WALLIS: Yes.

MR. SANDE: So, we will do testing to

quantify the amount as well as the characteristics. We will look at fiber sizes to determine what the appropriate length of the fiber is. It was a hot topic earlier in the presentations.

And then, the chemical effects testing to determine the time-dependent chemical loads. So, all of that will go into our debris load for those particular cases. Again, we will rely on the RELAP modeling to figure out what is the realistic driving head.

CHAIR BANERJEE: How far is the aluminum from the core? I think this is the key point, whether the chemicals will get there or not in time to block it. How far is it --

MR. KEE: It is in various locations.

CHAIR BANERJEE: Okay.

MR. KEE: That first number I quoted was just the aluminum scaffold boards. But it is spread throughout the containment.

CHAIR BANERJEE: Okay.

MR. KEE: These are just walking boards on scaffolds is what it primarily is, but there is some equipment, also, that is aluminum.

CHAIR BANERJEE: There is aluminum proximity?

MR. KEE: To the core?

CHAIR BANERJEE: Well, in terms of the transport time is what I am looking at.

MR. KEE: We have walked this down. I mean, some of it is probably never even going to get hit by sprays, for example. It is very high above the pool. But we need to analyze all that.

MR. SANDE: The transport time probably is insignificant. What is important is the formation time for chemicals. The transport time may be less than two hours.

MEMBER ARMIJO: That is your rate-determining step, is that formation of the chemical?

MR. SANDE: Right.

CHAIR BANERJEE: Well, this is your crucial issue really. If it takes more than five hours, probably you are okay.

MR. SANDE: Right. Well, we are okay for the cold leg injection period. Now we still have to look at hot leg injection and say, is that case going to --

CHAIR BANERJEE: Is it going to be okay for that?

MR. SANDE: Right.

CHAIR BANERJEE: Sure. Okay.

MR. SANDE: So, in the initial evaluation,

we are looking at driving heads that are rough estimates, and the issue of back pressure came up on certain scenarios. We can use the RELAP5 modeling to tell us what is the realistic driving heads for certain scenarios. And then, also, the close to the core versus what goes past the core in different things.

And then, one of the pieces that we haven't figured out yet is we would have to have some kind of either analytical or test method -- it will probably be a combination -- to determine, given that debris load and those flow conditions, what is the head loss for that scenario? So, we most likely will do fuels testing on this.

CHAIR BANERJEE: Are you going to do realistic bypass in terms of like, you know, when your strainers are not blocked initially, there is more bypass expected?

MR. SANDE: Yes.

CHAIR BANERJEE: As we build up a filter bed --

MR. SANDE: Yes, our bypass testing will focus on what realistically is going on.

CHAIR BANERJEE: On the realistic bypass testing?

MR. SANDE: And I would like to note that

only the largest of the large breaks would generate enough fiber to fully cover the strainer. So, 99 percent of the breaks will not have enough fiber to fully cover the strainer.

CHAIR BANERJEE: Which is the most dangerous situation, of course.

MR. SANDE: Well, for in-core blockage, sure. You can potentially have bypass at any point during the event.

CHAIR BANERJEE: We realize that it is the small breaks which holds the most risk for core blockage.

MR. SANDE: Yes. And if I could, I would like to share some of the preliminary results that Rodolfo got from the RELAP modeling.

MEMBER STETKAR: Notice the rest of us are quiet.

(Laughter.)

MR. SANDE: In 30 seconds, the preliminary results for small breaks indicated that, even with full core blockage, you would still get enough flow coming around for either a hot leg side small break or a cold leg side small break, which to me indicates fairly highly that small breaks aren't going to be a concern at all.

CHAIR BANERJEE: Because you have got recirculating flow?

MR. SANDE: Right. The flow can go the other way around.

CHAIR BANERJEE: We will look at that work.

MR. SANDE: Okay. There are two other topics on here, boron precipitation and air intrusion. I don't know that we need to spend any time on those because you can read the slides on that.

MEMBER REMPE: Have you interacted with the NRC with respect to the University of New Mexico testing about the quality of the data? Do they have to meet NQA1 or anything like that? I know that they said the codes don't have to be NRC-approved. What about the data that you are getting from them?

MR. JOHNSON: They have a robust test plan with independent assurance of the samples, et cetera.

MR. BAILEY: I don't remember a specific discussion on the quality. We have had numerous meetings on the chemical effects testing that they are going to be doing. We have actually gone down to Texas a couple of times to see them in the development of the test protocol and sort of supporting analysis. The RELAP/MELCOR coupled analysis that you are seeing is being used to determine the temperature profile that they will be using in the tank.

Did we get discussion on the QA aspects of

the chemical effects --

MR. KEE: We have a QA plan. Do you want to speak to that, Kerry?

MR. HOWE: I think the specific question was whether NQA1 --

MEMBER REMPE: Yes.

MR. HOWE: And our test plan I think does not meet that standard. So, we do have a QA plan. We will be doing good sampling on -- I'm sorry, I am trying to see you.

MEMBER REMPE: You don't have to see me. talk to the microphone. It is more important.

(Laughter.)

MR. HOWE: Yes. Okay. The short answer is that we do have a QA plan, and that is something that is going to be reviewed with South Texas. It has not been reviewed with the NRC. Our intention was that it was not at that level of standard that you are talking about. I guess I need a response from NRC.

MR. RULAND: We have had some general discussions about the quality assurance features that they are going to use. They understand that it is something we are going to examine, but we have not said that they must comply with certain industry standards. NQA1 is one of them. We understand that this is a

risk-informed submittal. It will likely be an exemption.

Typically, when you do risk-informed submittals, they typically are done in a realistic way. They may or may not comply with a specific quality assurance standard.

MEMBER REMPE: Okay. Thank you.

MR. MURPHY: In meeting the goal, we will have that and we have continuous dialog with the NRC. So, we will continue to work with them.

CHAIR BANERJEE: So, do you want to summarize?

MR. JOHNSON: Let me just try to find some closure here in 45 seconds, and then Mike will summarize.

CHAIR BANERJEE: Okay.

MR. JOHNSON: As we heard earlier, the output from CASA is mapped back to the categories that are looked at in the PRA, the small, medium, and large LOCA. As we said earlier, we are working on defining that interface. If we find that the pool temperature profiles are very important, then we will no resolution in terms of tracking the number of fan coolers, the number of sprays, trains, et cetera, to map that.

What we did -- let me just skip to this slide -- in our first quantification, sort of a test

quantification in 2011, we took the models as they were in their current shape and ran through the whole process and said, "Well, what would 1.174 say?"

With a lot of assumptions and a lot of simplified analyses, we estimated the delta CDF, if you will, just based on looking at the frequency of scenarios that led to core damage. I would like to point out that in that analysis -- and, Bruce, please correct me -- we saw no cases where NPSH at the strainers, where it was lost, where it was threatened. But we did see for some medium LOCAs and, more likely, for some larger LOCAs, we saw some in-core effects, again, with our models that we had in 2011.

So, without looking at the latent-debris-only case, we just estimated the delta CDF as the frequency of those scenarios involving in-core damage. And if we accept that as a first estimate, if you will, of where we would land in the 1.174 matrix, the dot is shown here on this slide.

So, again, very preliminary results. We think that we are in region 3 in the 1.174 world.

I will turn it back over to Mike.

MR. MURPHY: Yes, and I will close this out.

Again, the status is we are the pilot plant for this risk-informed closure program. We have

periodic communications with the NRC. We are staying engaged there.

I will just focus on the last one. Our plan is to submit a license amendment near the end of 2012. So, that is where we are heading with our test plan.

I would like to review desired outcomes very quickly. I hope we met these desired outcomes to show how we are integrating the deterministic and the probabilistic approach. I hope we have a good desired outcome there.

I will say that the next one was solicit, collect, and consider feedback. I think we met that. We got good feedback, a lot of feedback -- and it is certainly appreciated -- from the Subcommittee. It helps us make sure we are focused in the right direction. That is much appreciated.

I would like to close it with thank you for letting us have the opportunity to show off the hard work this team is doing.

CHAIR BANERJEE: Well, thank you very much for taking your time to come and inform us about what is going on. So, we discuss it. If the Committee so desires, we may ask you if you would come to one of our full Committee meetings and briefly brief the full Committee.

MEMBER SHACK: Although we are pretty full.

(Laughter.)

CHAIR BANERJEE: Yes. At the moment, yes. But we will discuss it internally and we will get back to you on that.

MR. MURPHY: Right, and we will work through our contacts and we will support your request.

CHAIR BANERJEE: A lot of the full Committee is here, most of them.

(Laughter.)

MEMBER ARMIJO: Yes, all but two.

CHAIR BANERJEE: Yes.

Okay. So, thanks very much, and we look forward to hearing more about this as things go on.

MR. MURPHY: Thank you.

CHAIR BANERJEE: Thank you.

All right. So, I think right now, if it is agreeable, I would like to close the meeting for a little while and then reopen it again for our discussions.

So, the only people who should stay are the staff right now.

We are going to stay on the record for this discussion. So, I will close it now. We are going to close the meeting after everybody has left the room. We will reopen the meeting again, yes, but right now we are

closing the meeting.

(Whereupon, the foregoing matter went into Closed Session at 5:19 p.m.)

(Whereupon, the foregoing matter went back into Open Session at 5:59 p.m.)

CHAIR BANERJEE: So, we are back into open session, and now we go on the record.

This open session is primarily to get remarks of the Subcommittee members, including our consultants, as to the WCAP that we are supposed to be reviewing the SER for.

We have a full Committee meeting scheduled for July. This is really to give guidance as to what the Subcommittee feels at the moment. "Feels" is a bad word.

(Laughter.)

MEMBER ARMIJO: We don't "believe," either.

(Laughter.)

CHAIR BANERJEE: We don't "believe" and we don't "think".

So, I think the best thing would be to go around, as we usually do, starting at the head of the table there with Graham Wallis and just going in turn, and getting each one's remarks.

CONSULTANT WALLIS: Well, I think this whole experience --

CHAIR BANERJEE: Can you come closer to the microphone?

CONSULTANT WALLIS: -- over the years has been a learning experience. We have learned about all kinds of things that affect all sorts of things. What you have to concentrate on is your rationale for accepting something as a criterion, and whether or not you have enough evidence to support whatever rationale, whatever you are going to accept as that criteria, that set of criteria, as the staff spelled out.

If you want to accept 15 grams, then you have to have sufficient evidence to make your case in light of the uncertainties and, also, for the actual conditions in the reactor, not for something sort of artificial somewhere. So, you have to think very carefully about whether you need more evidence or not.

CHAIR BANERJEE: And that's it? You are done?

CONSULTANT WALLIS: That is it.

CHAIR BANERJEE: Okay. Tom?

CONSULTANT KRESS: I pretty much have, believe it or not, the same view as Graham does. I thought that the evidence that was given to us was not

to make a hypothesis, that their acceptance criteria is probably good, but I don't think we have enough data to be sure of that. I would have said we needed to have more data focusing strictly on the 15 grams and the 1-plus-1 ratio, and vary that ratio a little bit on either side of it, and do a number of identical tests to show that you don't have some problem with running the tests themselves.

I thought they should also vary to some extent the order in which they put things in. I think I would have done some varying in that order.

I think probably the indications are that at 15 grams it doesn't matter how many particulates you have; it doesn't matter how much chemistry you have. You are probably in a coolable geometry. But I don't think we have quite enough evidence to make those conclusions.

That was before I saw this stuff on the AP1000. I haven't had a chance to look at it yet.

I think I would also run some tests -- I didn't see the data on the tests where you completely blocked the inlet, the bypass and the inlet, and just see what delta P you get there, so we can understand some of the other data.

I worry a lot about using silicon carbide

as a simulant for particulates. I think we need to think more about that. I don't think that is a good simulant for the real particulates that you get. So, I would think about trying to think up another type of simulant for that.

That is pretty much my opinion.

CHAIR BANERJEE: Okay.

Steve?

MEMBER SCHULTZ: I fall in line with the previous two comments associated with the 15 grams and the data that has been presented. I did not have the benefit of the discussions yesterday, but understanding what I could from the discussions today, and seeing the difference in results between the two test facilities, I have to express disappointment that the root-cause had not identified clearly what caused the differences in the test results.

Given what has been termed a cliff, or certainly a strong difference between results with very small inputs in experimental input, that really does need to be explained. Bench tests might do that looking at chemical effects. But, in the absence of something like that, additional testing ought to be performed in order to reaffirm the results at 15 grams. I think it can be demonstrated, but I am not yet convinced that it

has been demonstrated.

With regard to the discussion we heard from South Texas on the risk-informed approach, I think that the work that has been done and the work that is going to be done is going to be very useful in doing what risk-informed should do, informing the industry about what is important in these analyses and the effects that need to be investigated. I am looking forward about the results as they develop. It is a very aggressive schedule to make a submittal by the end of the year, but I wish that effort well in achieving it.

CHAIR BANERJEE: Dick?

MEMBER SKILLMAN: I have four comments. First of all, I am comfortable with the 15 grams per fuel assembly. I am concerned about the representativity of the water quality, particularly as it might impact chemical effects. I am concerned that the chemical effects are potentially not fully appreciated.

That is driven by the recognition that the basic water quality, particularly the hardness and the dissolved solids and the organic content, may affect the delta P. And so, I conclude, unless the real water quality for the plants is the same as the test water, the results might not be applicable to the fleet. So, the issue of the hardness of the water and the quality

of the water and how the chemicals react in that water are a critical piece of this riddle. That needs to be addressed.

CHAIR BANERJEE: Okay. Dennis?

MEMBER BLEY: I wasn't here for the Subcommittee meeting except for this afternoon. So, I will just comment on this afternoon.

I was encouraged by what I saw. From previous meetings on this topic and presentations and readings, it has been troublesome. Trying to do the generic case to cover all of these plants leads you to real difficulties, I think, and strong conservatisms. Getting some tests done and analysis done for a plant-specific case, looking for that one plant, a better look at where the debris might come from, where it might go, what might happen, seems a really valuable step forward.

As far as the integrated analysis they are trying to do, the engineering calculations in there I am sure they can tie together and do. It is a very complex thing. Their discussion of how they might test that program, their tying them together, seems like a reasonable approach. That remains to be seen, how all that works.

The place I am nervous, and I would

encourage them and staff later when they look at this, there is a lot of places where they are going to have to put together expert elicitations to represent the ranges of things that could happen based on all the available information. That is the sort of thing that can get done and buried in the analysis and never really looked at hard.

I think that needs to have a really bright light focused on it. Each case where that is done needs to be well-documented at what the information sources were it was based on, what the judgments were and why they are reasonable, and why they cover the full range of possibilities, and why whatever distributions they come up with are at least reasonable.

That is going to be a fairly big package of information and a process that needs to be done really well and carefully and thoroughly documented. It is a place where documentation often falls down and gets buried down so low in the analysis that it is hard to resurface. So, I think that is a place we should look real hard later.

CHAIR BANERJEE: Okay. Thank you.

Sam?

MEMBER ARMIJO: I think more experiments have to be performed to support the 15-gram thing. I

think you are right that it is going to work out, but I think there have been too many surprises, and you trying to use the other data is not the way to nail this thing down. So, I might change my mind by the time of the full Committee, but it just seems to me that two experiments are just not enough.

I was very impressed with the South Texas approach, particularly their attack on the chemistry, trying to use realistic chemistry and not exclusively using surrogates. I think that is important. Particularly the kinetics of the aluminum dissolution, I think it is really important. It will set the critical timeline. Maybe the rate-controlling step in the whole process could be that dissolution rate of that aluminum.

So, anyway, I thought that was excellent work. Whether they can get all that work done by the end of this year, I think it is going to be tough, but I wish them well.

That is all.

CHAIR BANERJEE: Okay. Mike?

MEMBER RYAN: I think, based on the comments of all my colleagues that are much more knowledgeable in this area than I am, I am taken by the array of comments that all suggest the same thing, which is more testing. To me, I heard a number of valid

questions come up that probably make sense. Now what that testing should be and how it should be designed and conducted probably will need some additional thought, and maybe our own thought of what we might recommend. But I think I concur with that view.

CHAIR BANERJEE: Okay.

MEMBER RYAN: Thank you.

CHAIR BANERJEE: Said?

MEMBER ABDEL-KHALIK: Ditto. I agree with the comments regarding the need for additional experiments to confirm the adequacy of the acceptance criterion of 15 grams per assembly at prototypical conditions.

I am glad to see that the staff agreed to include the verification of the length of the fibers that bypass the strainers as part of the confirmation of the applicability of the methodology.

There are two specific concerns that I have about the data. No. 1, I don't really believe the cold leg break experiments. I do not believe that these experiments were correctly done because chemical addition was started before the geometry of the bed reached steady-state conditions. And the geometry of the bed has a direct and critical impact on the ultimate delta P that will be reached. Therefore, if you sort

of start the chemical addition at an unknown geometry, it is probably not going to give you prototypical delta P values that one would expect.

The second concern I have about the experiments is I do believe that the experimental data that were presented really depend on the design of the experiment. Specifically, they depend on the size of the mixing tank because, depending on the size of the mixing tank, that will dictate the concentration. And as far as I can tell, the size of the mixing tank was not scaled based on sump size. And no one has shown any data that would convince me that, for a given flow rate, the ultimate delta P depends on the total inventory of particulates and fibers, total inventory alone, which is matched in the experiments, rather than not just the inventory, but also the concentration of the fibers in the water.

And therefore, until and unless some experiment is done to show or something is extracted from the experiments that have already been done to show that concentration effects are negligible, and that the primary variable is the integrated total amount of both particles and fibers, I just don't know what to believe.

CHAIR BANERJEE: Bill?

MEMBER SHACK: I am comfortable with the

conclusion from the SER with the restrictions they have placed on it that it would provide an adequate assurance of long-term cooling, together with all the other guidance that the staff has provided on the way to address the sump blockage issue.

I echo mostly Dennis' comments on the STP. I think one of the things is just to do a plant-specific analysis, whether you were doing it by risk analysis or deterministic, I think there is a lot to be gained by looking at specific conditions in a plant.

I agree with Sam on the chemistry. I just think that you are always going to come up with enough questions that it is going to be difficult to come to the conclusion that your results in your laboratory experiments preclude the possibility of a precipitate forming in the reactor. That will just have to be a judgment.

Again, the comments of the review committee and all the peer review on all the previous chemistry work will be equally applicable, I think, to the work that is planned here. It is just difficult to address.

CHAIR BANERJEE: Joy?

MEMBER REMPE: I agree with the colleagues who expressed the need for additional data and, hopefully, some understanding of what is going on with

the water chemistry. I am hoping by the time we have a full Committee meeting that there are more data or at least a plan for how the data will be obtained for us.

You had said earlier today we should express some opinion related to the need for South Texas to be presenting at the full Committee meeting.

CHAIR BANERJEE: Right.

MEMBER REMPE: And is it a two-hour meeting that you planned or an hour and a half?

CHAIR BANERJEE: Well, we don't know because we have a very tight schedule. So, even if schedule something, it may not be for July. I mean, it may eventually --

MEMBER SHACK: I don't know that a two-hour presentation of STP will do anybody any good. You know, we will be in the same situation we were here today.

MEMBER REMPE: And so, I would vote for separating the issues or have a 10-minute brief that we are doing this and no details. But I think it doesn't do much good. I agree with Bill on that one, too. I think it needs to be separated.

CHAIR BANERJEE: John?

MEMBER STETKAR: I don't have anything to add on the WCAP or the testing.

With regard to South Texas, it sounds very,

very encouraging. I think that I am a little disappointed we didn't get a chance to hear how it is all going to come together. I hope that they have a little bit more meat behind how it is all going to come together, because I think they have some real challenges putting together that integrated model. All the little bits and pieces that we heard about all sound good, but doing a full integrated model, propagating uncertainties, accounting for the timing phenomena, is going to be a real challenge.

I think, to echo Joy's recommendation, I don't think it would be worthwhile -- I think we have too much on our plate at the full Committee meeting with the WCAP. I would suggest perhaps having --

CHAIR BANERJEE: A separate briefing?

MEMBER STETKAR: -- a separate focused Subcommittee meeting, Thermal Hydraulics/PRA or something like that, to give South Texas much more focused attention and allow us to learn more, a little bit about what they are doing, consistent with their schedule.

I mean, obviously, they have done a lot, but are in a very active phase of their project. I think it would be a lot more useful to us and probably be more useful to them, rather than having them to come back and

prepare for a full Committee meeting, which does take some time.

CHAIR BANERJEE: Stu and Bill, do you have a comment on the South Texas Project? What are your thoughts on a briefing for the full Committee? What do you think?

MR. RULAND: Yes, based on what I heard and based on the Committee's interest, it sounds like, at least from my perspective, a briefing of the full Committee at this short juncture is probably premature.

CHAIR BANERJEE: Yes. So, we can put it off.

MR. RULAND: What I would argue is we will monitor what they are doing. When we think the time is right, we will work with South Texas to find something that is appropriate for your schedule.

CHAIR BANERJEE: And we will then schedule, if you wish, a proper time slot for it.

MR. RULAND: Yes, that would be good, a sufficient time slot so that you could sufficiently explore all the issues that they have out there.

MEMBER ARMIJO: Well, you know, if they are ready and they complete and submit chemistry experiments, which is to me really new stuff, I would like to see it at sort of a Subcommittee where we could

get into some depth, rather than just a full Committee on everything. It is just too much.

CHAIR BANERJEE: So, if you feel at some point that an informational briefing to a joint PRA/Thermal Hydraulics Subcommittee is called for, you can be in touch and we can try to schedule something.

MR. RULAND: Yes, and we will work with South Texas.

CHAIR BANERJEE: Yes. So, let's leave that in your hands.

MR. BAILEY: Okay. Yes, we will look at their integrated schedule.

CHAIR BANERJEE: Yes.

MR. BAILEY: As you got the impression here, there is a lot. It is going to take a good amount of time to really do it justice. We will look at their schedule for completing some of the independent items. Maybe it is best to come back and do piece-parts of their overall analysis.

CHAIR BANERJEE: Right. So, I think let's leave it in your hands.

MR. RULAND: We got it.

CHAIR BANERJEE: The only thing that I want to say about the WCAP is you heard from the Committee, and there seems to be significant unease, I would say,

about not necessarily the criteria or the acceptance criteria because there may even be a sense that these are perfectly adequate, but what is lacking probably is some additional experimental support for that. This seems to be the opinion of the majority of the Subcommittee at the moment.

I don't think that they are asking for, at least the sense of it that I have -- correct me if I am wrong -- they are not asking for anything extensive, but they would like to see some further support for the 15-grams-per-assembly acceptance criterion, and, of course, also related to the length of the fiber and its distribution, or whatever the condition that you put.

I don't think that we are asking, or the Subcommittee -- I am not including myself there -- is asking for an extensive set.

MR. RULAND: I understand.

CHAIR BANERJEE: I think it is up to the staff and the applicant to decide what is appropriate there. It is not up to us to design it. And that is the only guidance that I can give you.

MR. RULAND: I understand. Of course, as you might be aware, the PWR Owners' Group at this point has claimed that 15 grams as a generic limit is unusably conservative. So, how the PWR Owners' Group and their

Executive Committee want to proceed, I know Tim has been here, but, you know, it is really the Executive Committee of the PWR Group that is going to make that call one way or the other, what they are interested in supporting.

CHAIR BANERJEE: Sure. I mean, it is up to them.

MR. RULAND: Correct.

CHAIR BANERJEE: If there is data that they bring forward which supports --

MR. BAILEY: If there is or if there is not. Keep in mind the intention of this WCAP. As it was pointed out, this is being used in concert with the rest of the conservatisms and the overall analysis and, in effect, sets a very, very restrictive in-vessel limit for them to essentially work down to.

It is a somewhat different situation in the new plant where I am designing from scratch with absolutely no fiber. I am looking at plants that are already established, have been running for a long time, and what level of modifications or actions do I have to do in order to answer a generic safety issue, you know, an issue that came up long after these plants were licensed.

So, I think, to some extent, there is a different focus perhaps. We will have discussions with

the Owners' Group and see whether there is a willingness there to further test down in the 15-gram limit, but I think you have seen the focus that they have had since they submitted this WCAP has been towards getting to higher fiber loads. But we will pursue it.

CHAIR BANERJEE: Right. I think I realize, and I think the whole Subcommittee realizes, that at 15 grams we have considerable margins at the moment of what pressure losses are acceptable. But the problem there is simply the paucity of data. There are only two data points. So, based on that, this is what the opinion of the Subcommittee is. It is not necessarily my personal opinion.

Okay. So, I think, with that, I would like to thank everybody for spending their time in informing us, South Texas, Westinghouse, and the NRC. Thank you very much.

I will adjourn the meeting and thank the Subcommittee.

(Whereupon, at 6:26 p.m., the Subcommittee was adjourned.)

WCAP-16793-NP, Rev. 2
NRC Staff Safety Evaluation

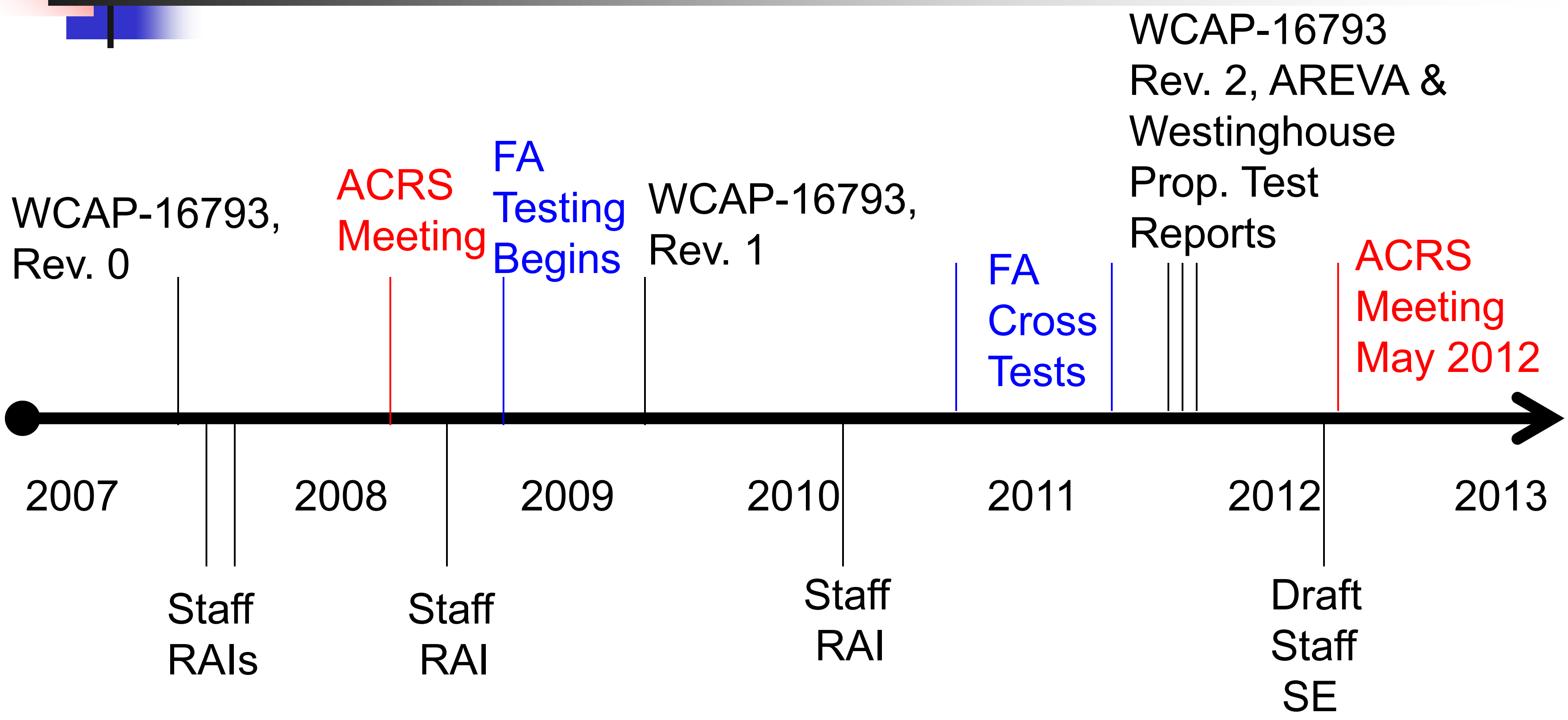
Stephen Smith, Ervin Geiger, Paul Klein
Office of Nuclear Reactor Regulation

Advisory Committee on Reactor Safeguards
Thermal Hydraulics Phenomenon Subcommittee
May 9, 2012

Outline

- Background/History
- Overview
- Regulatory Evaluation Criteria
- Technical Evaluation
  - LTCC Acceptance Basis
  - Analysis
  - Fuel Assembly Testing
  - Chemical Effects
  - Conditions/Limitations

WCAP-16793-NP History



WCAP-16793-NP, Rev. 2- Overview

- With respect to GSI-191 and GL 2004-02, the WCAP presents evaluations and a method licensees can use to address the impact of strainer bypassed debris on core cooling by:
  - Setting a limit on the maximum temperature of fuel clad based upon a conservative value that prevents fuel damage (in accordance with 10CFR50.46)
  - Establishing an upper limit on the quantity of debris that may be transported to the core inlet
  - Demonstrating that fuel clad temperature will not exceed an acceptable limit when debris is deposited on the fuel rods and spacer grids.

WCAP-16793-NP, Rev. 2-Overview (cont'd)

  - Providing a tool for licensees to use to perform plant-specific evaluation for deposit thickness and clad temperature
  - Suggesting options for plant specific testing/analysis to increase the fiber acceptance limit

- **The staff evaluation of each of the above topics is summarized in this presentation**

Regulatory Evaluation Criteria

- 10 CFR 50.46(b)(5) – After any calculated successful initial operation of the ECCS [emergency core cooling system], the calculated core temperature shall be maintained at an acceptably low value and decay heat shall be removed for the extended period of time required by the long-lived radioactivity in the core

LTCC Acceptance Bases

- With respect to GSI-191 and GL 2004-02 evaluations, licensees are considered to have demonstrated adequate core cooling when:
  - Fibrous debris passing to the core is limited to an amount shown to be acceptable by fuel assembly testing
  - Calculated core peak clad temperature does not exceed 800ºF (temperature limit supported by autoclave testing) after the core has been quenched
  - Calculated deposit thickness on fuel rods does not exceed 0.050 inches (to preclude gross blockage of flow channels between fuel rods)

Analysis-Core Inlet Blockage

- The WCAP describes WCOBRA/TRAC analyses showing:
  - A relatively small unobstructed area at the core inlet will allow sufficient flow into the core to match boil-off
  - A very high uniform flow resistance at the core inlet can be tolerated before flow into the core is reduced below that required to match boil-off
- Staff does not rely on these core inlet blockage analyses in the safety evaluation of the WCAP

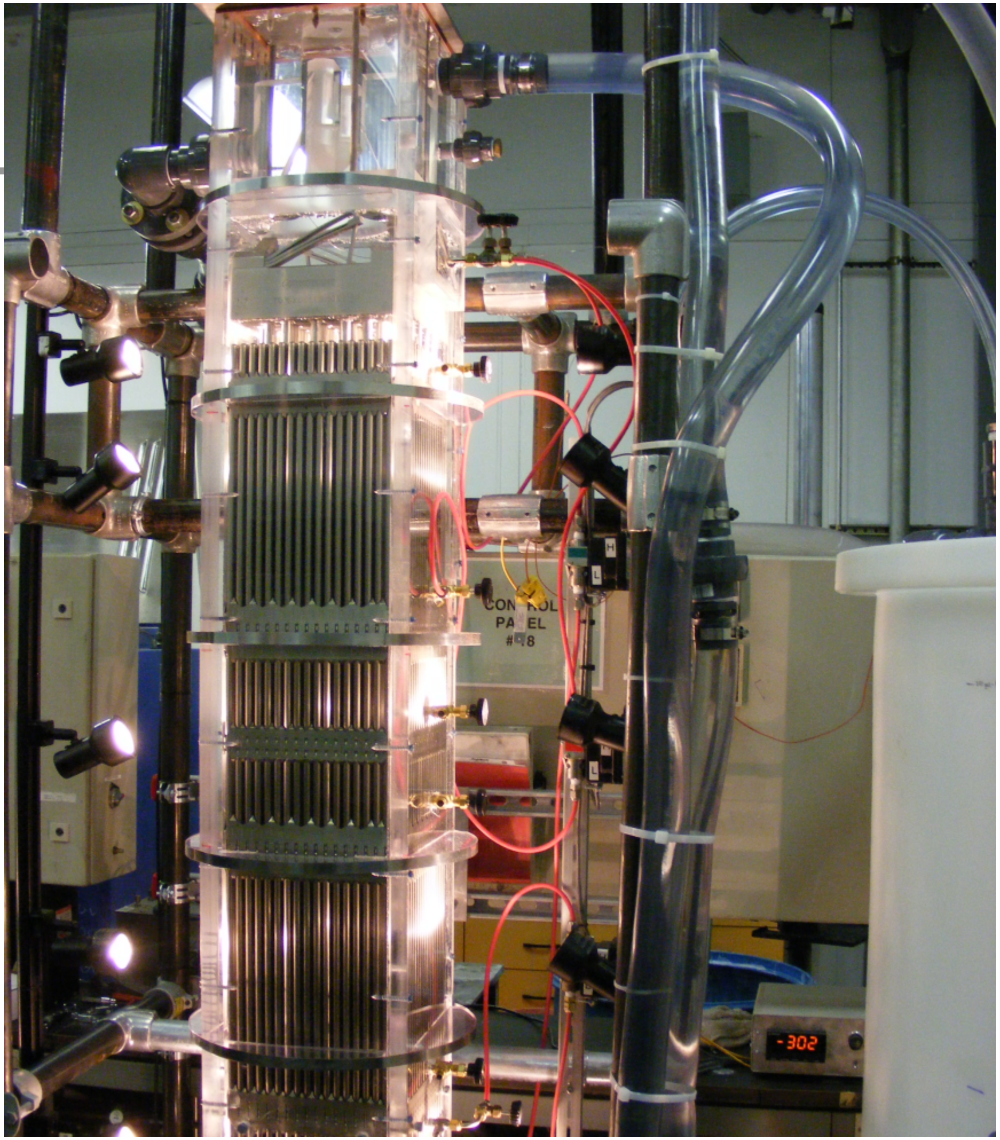
Analysis-Local Heating of Fuel Rods

- Fuel clad temperature should not exceed 800ºF following core quench and re-flood
  - Supported by autoclave test submitted to show corrosion and hydrogen pick-up at 800ºF will not have a significant effect on clad properties over a 30-day period
- WCAP describes analyses using bounding plant conditions to demonstrate that:
  - Deposits on the fuel cladding will not result in fuel clad temperatures exceeding 800 ºF
  - Deposits packed in the spacer grids will not result in fuel clad temperatures exceeding 800 ºF
- The NRC staff accepts these conclusions based on the conservative analyses described in the WCAP

Fuel Assembly Testing

- Test Description
- Test Results Summary
- Fiber Limits
- Hot-Leg vs. Cold-Leg Break Results
- Particulate to Fiber Ratio
- Cross Tests
- Conservatism
- Boric Acid

Fuel Assembly in Test Rig



Test Description

- Partial Height (1/3 height), Full Cross Section Fuel Assembly
- Fluid chemistry – potable water
  - Buffered borated test run – no benefit realized
- Flow rates controlled
- Flow rate reduced if head loss approaches test facility limits
- Measured pressure drop across lower grids and full assembly
- Mixing Tank agitated to suspend debris
- Lower plenum and core support plate modeled
- ½ gap between fuel assemblies modeled by test column walls
- Debris addition order – particulate, fiber, chemical
- Temperature - Nominally Room Temp (about 70 ^{o}F)
  - A few tests as high as 130 ^{o}F

Test Description – Flow Rates

- Hot-Leg break flow rate is about 45 gpm (cold-leg injection)
  - All flow goes through the core
    - Maximizes amount of debris entering core
  - Based on all pumps running, highest pump flow
  - Also tested cases for single pump and lower pump flows
  - Alternate Flows Tested
    - 15.5 gpm - minimum injection with failed loop
      - Requested by staff
    - 17 gpm – UPI
    - 6.25 gpm – CE Plant Westinghouse Fuel
    - 11 gpm – CE Plant Areva Fuel

Test Description – Flow Rates

- Cold-Leg break flow rate is about 3 gpm (cold-leg injection)
  - Based on decay heat at recirculation initiation
  - Matches core boil-off
  - Excess flow spills out the break
  - Flow rate into core decreases with time
- For either break – hot-leg injection initiated within hours from recirculation start
  - Intended to dilute boric acid buildup following a cold-leg break
  - Also provides alternate flow path to core (top of core)
  - May be beneficial for debris blockage at core inlet
  - Hot-leg injection schemes are plant specific
  - Usually initiated within 2 – 12 hours from recirculation start

Test Description - Debris Types

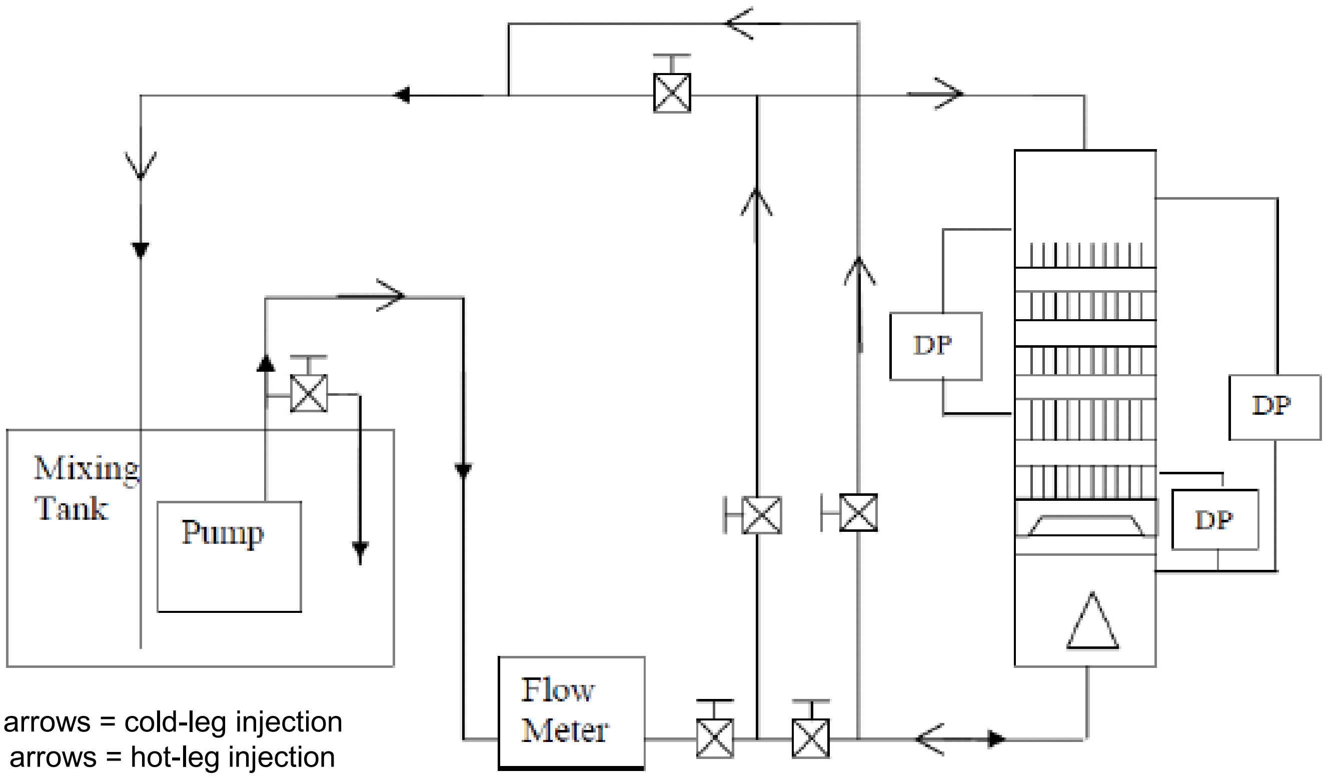
- Fibrous Debris
  - Nukon – sized to match debris from strainer bypass testing
- Particulate Debris
  - Silicone Carbide
  - Sizing same as for strainer tests (10 +/- 2 micron)
- Problematic Debris
  - Cal-Sil
  - Microtherm
- Chemical Debris
  - AlOOH – Prepared using WCAP-16530-NP-A Method
  - Same precipitate used for many strainer tests

Test Description – Fiber Size Distribution

- Fiber length based on strainer bypass test samples

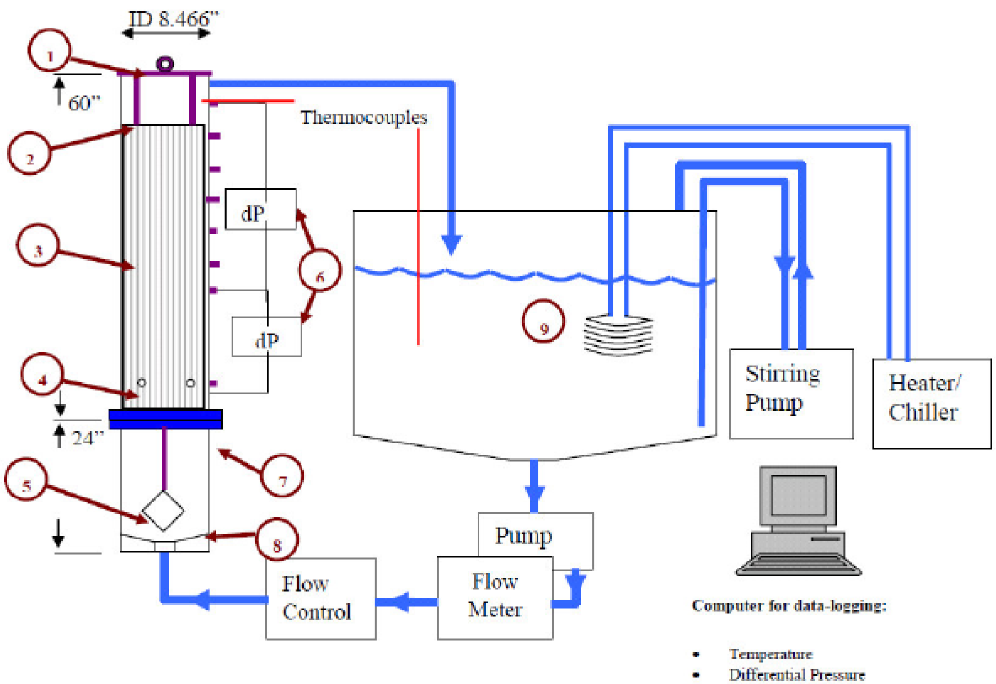
Fiber Length	Target	Range
<500 microns	77%	67-87%
500-1000 microns	18%	8-28%
>1000 microns	5%	0-15%

CDI Test Facility



Small solid arrows = cold-leg injection
Large open arrows = hot-leg injection

Westinghouse Test Facility



1. Stainless steel top plate and lifting ring
2. Stainless steel hold-down bar
3. One-third height fuel assembly
4. Horizontal positioning set screws
5. Flow diverter (cube)
6. Differential pressure gauge
7. Port for measurement of differential pressure
8. Bottom flow cone
9. Temperature-regulation coil

Test Results - Summary

- PWROG sponsored over 60 fuel assembly tests at two similar facilities
  - Westinghouse – Churchill, PA
  - Continuum Dynamics Inc. – Ewing, NJ
- Single fuel assembly tests
- Hot-leg and cold-leg flow rates
  - At limiting p/f ratios for each case fiber limits are similar
- Included problematic debris
  - Debris that is problematic for strainers did not cause high head losses in fuel assembly testing
- Included Chemicals
  - Small amount of WCAP chemical debris results in large head loss increase at limiting p/f ratios

Test Results – Summary

- Initial tests conducted with high p/f ratios
  - Based on strainer test experience
  - Relatively high fiber limits were attained
  - Staff noted a dependence of head loss on p/f ratio
    - Requested additional testing
- Varied particulate to fiber (p/f) ratio
  - High (hot-leg) flow rate limiting p/f ratio is about 1:1
  - Low (cold-leg) flow rate limiting p/f ratio is about 45:1
  - Fiber limits much lower at limiting ratios
  - Little contribution from chemical precipitates for hot leg tests at high p:f ratios

Test Graph – Hot-Leg – 25 Grams Fiber

- Deleted

Test Results – Effects of Flow Rate

- Beds formed at higher flow rates have lower resistance
  - Higher overall head loss due to higher flow rate
    - Higher flow rates are limiting due to higher flow
    - With higher debris limits, cold-leg flow rates may become limiting
      - Significantly lower driving head
- Flow rate affected bed location at the Westinghouse facility
  - At higher flows, beds distribute to multiple spacer grids
  - At lower flows (cold leg) beds formed at the lowest grid
- Flow rate did not affect bed location at the CDI facility at limiting particulate to fiber ratios
  - Both hot and cold-leg cases formed at the first grid
  - At higher p/f ratios beds form at multiple grids – like Westinghouse
- This is another difference between facilities

Test Results - Particulate to Fiber Ratio

- Low p/f ratios limiting for high flow rates (hot-leg break)
  - Results in greatest total head loss when chemicals included
  - Without precipitates, higher p/f ratios are limiting
  - Flows from 15 to 45 gpm tested
- High p/f ratios limiting for low flow rates (cold-leg break)
  - Results in greatest head loss when chemicals included
  - Without precipitates, p/f ratio is less important
    - High p/f ratios are still limiting
  - Driving head for the cold-leg break is much lower
    - About 15 psi for hot-leg break
    - About 1.5 psi for cold-leg break
  - Flows of 3 gpm tested

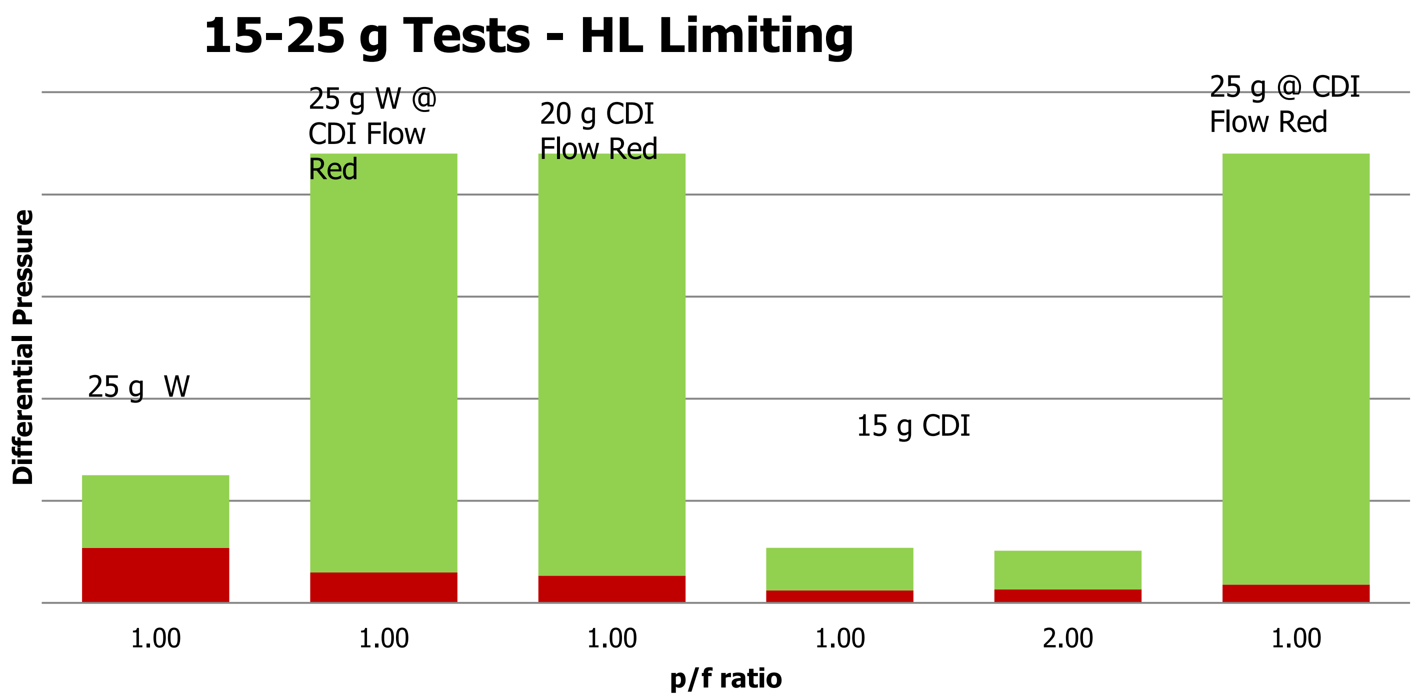
Test Results - Fuel Assembly Fiber Limits

- Fiber limits are based on industry testing
- Staff accepted limits are based on testing at the limiting facility
- Only fiber limits are proposed
  - Tests were performed at varied p/f ratios to determine those most limiting
  - Tests included chemicals (at varied p/f ratios)
  - Debris normally considered to be problematic for strainer tests was determined to behave similarly to particulate in fuel assemblies
  - Small amounts of chemicals resulted in maximum head loss
    - Additional chemical load did not have significant effects

Test Results - Hot-Leg Break Flow

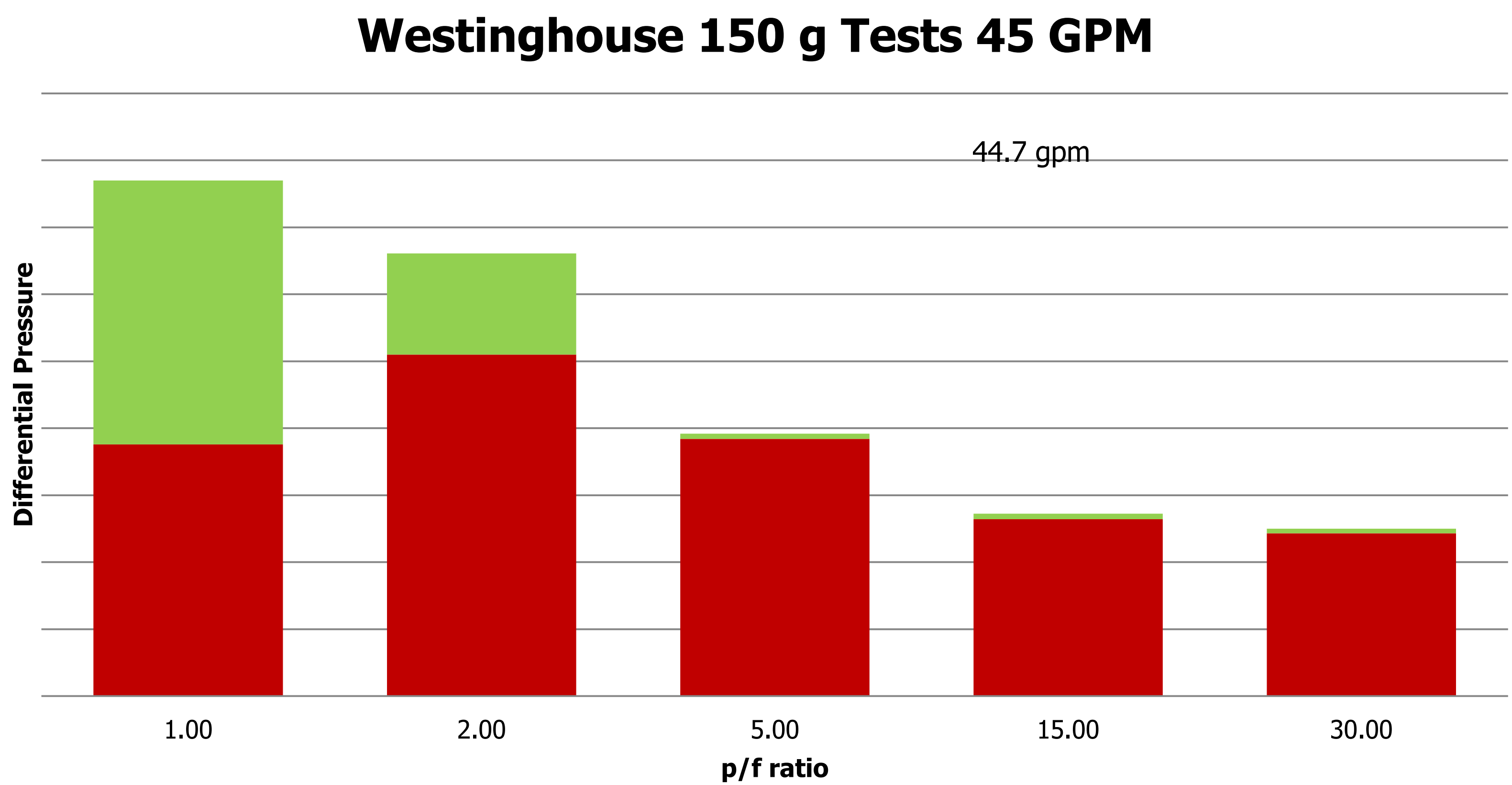
- Hot-leg case debris limits were found to be limiting
  - 15 gram per fuel assembly limit
  - Flow rates were varied and higher flow rates were found limiting
  - Low particulate to fiber ratios are limiting for hot-leg cases
    - 1:1 is limiting
    - Lower ratios not tested for hot-leg case
      - Increased fiber limits may require additional sensitivity testing below p/f = 1:1
  - At 15 grams fiber pressure drops were relatively low
  - Above 15 grams the head loss margin decreases rapidly

Head Loss vs. Fiber Amount

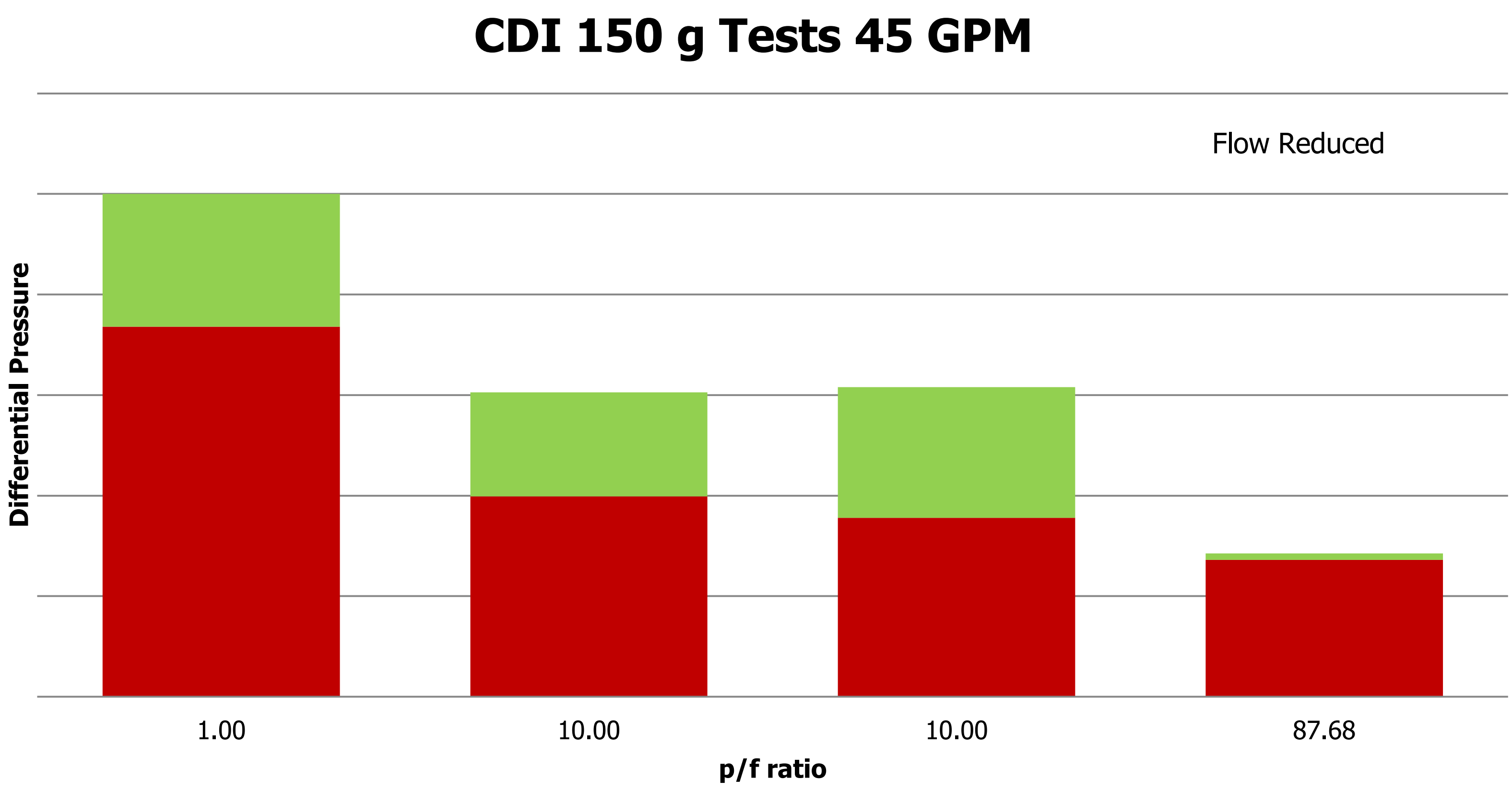


Test with > 20 psi are not actual final values – flow was reduced
Lower colored band is non-chemical head loss
Upper colored band is chemical head loss

Hot Leg p/f Ratio Study – Westinghouse Tests



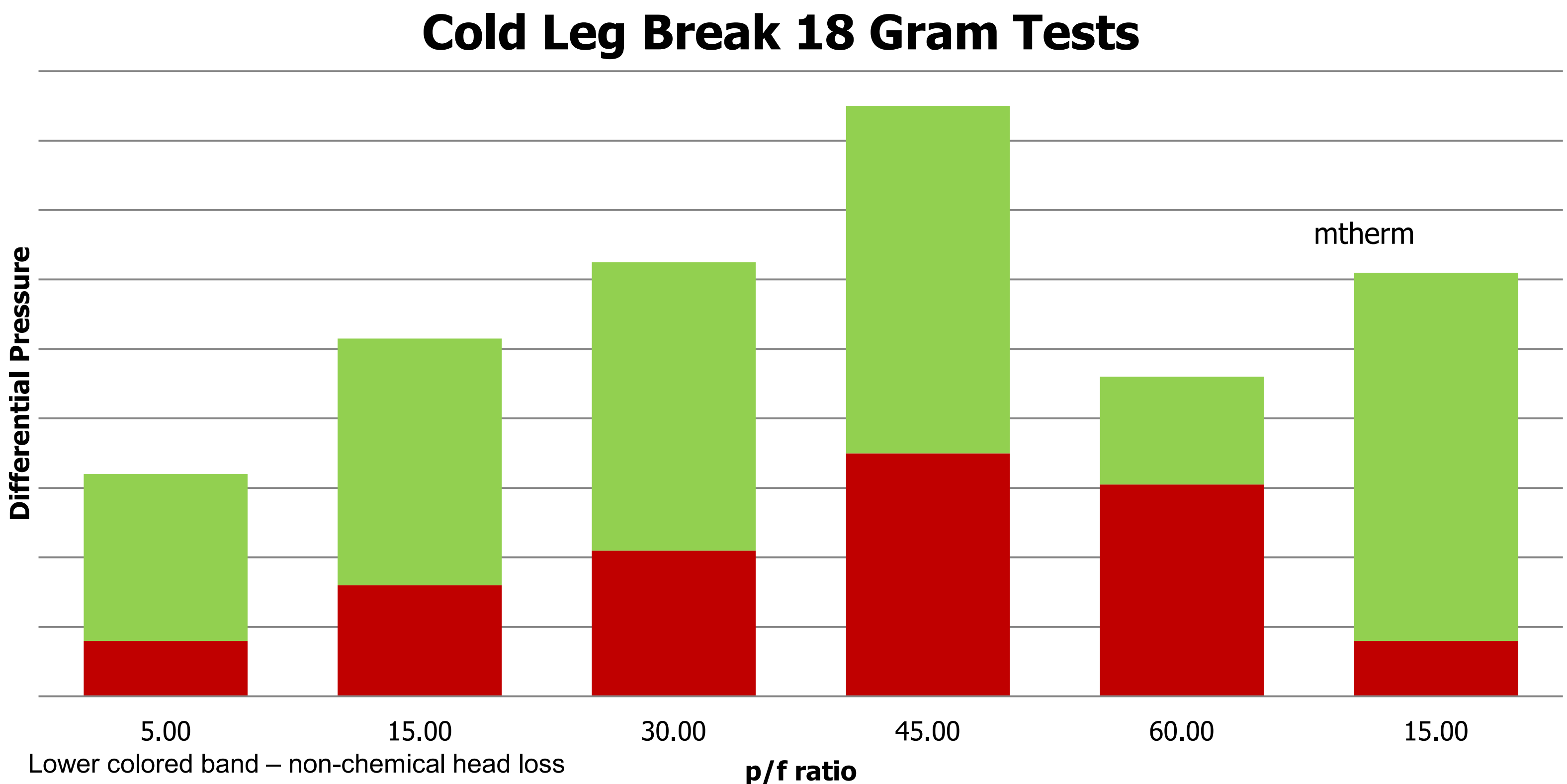
Hot-Leg p/f Study – CDI Tests



Cold-Leg Test Results

- Cold-leg case debris limits are close to hot-leg limits
  - Testing identified an 18 gram limit for cold-leg cases
  - However, less debris reaches the core for a cold-leg break
    - Significant flow out the break
      - Debris flows with the coolant
      - Ratio is plant design dependent
      - Current hot-leg limits ensure low debris load for cold-leg (<15g)
  - Lower flow rates tend to build debris beds at the first grid
  - Higher p/f ratios limiting for cold-leg cases (45:1)
  - Cold-Leg driving head is significantly lower than hot-leg
    - Plant dependent, but roughly 10x smaller
  - Any increase to hot-leg limits will require re-evaluation of cold-leg case

Cold Leg p/f Ratio Study



Lower colored band – non-chemical head loss
Upper colored band – chemical head loss

Fuel/Facility Differences and Cross Tests

- Test results indicated some differences between the test facilities or fuel assemblies
  - Tests of Areva fuel conducted at CDI had more limiting results than tests of Westinghouse fuel conducted at Westinghouse
- Cross test performed to better understand how differences in fuel assembly design and test facilities were affecting results
- Initial cross test involved an Areva assembly tested in the Westinghouse test facility. Later on, a Westinghouse assembly was tested at CDI
  - Westinghouse concluded testing conducted at CDI is more conservative but Westinghouse results are still valid due to conservative test methods
  - Areva concluded the test loops behave differently and that there is no difference in Westinghouse and Areva fuel behavior if tested under similar conditions

Fuel/Facility Differences and Cross Tests (cont'd)

- After initial cross test, the PWROG, Areva, and Westinghouse attempted to determine the cause for the cross test differences
  - No root cause identified
- Westinghouse eventually identified a repeatable effect from submerging the return piping to the mixing tank
  - Higher head losses resulted when return line submerged
  - No phenomenological reason confirmed
    - Theorized that air entrainment may have had some effect
- After both sets of cross tests, staff concluded the most limiting results could be used generically
  - Results from CDI test facility used to set 15 gram fiber value since these results are most limiting

Conservatisms – Fuel Assembly Testing

- <u>WCAP-16793 states</u>:
  - Tests recirculated debris with no chance for settling or filtering by a strainer
  - Tests conducted at limiting p/f ratios
  - Tests conducted at constant flow rates
    - Flow could decrease allowing adequate cooling if head loss increases
  - Turbulence within the core will prevent coplanar blockage of the core or disrupt debris beds
  - Tests assume uniform core blockage
  - Mixing tanks agitated to ensure debris suspension
  - Alternate flow paths not credited

Conservatisms – Staff Evaluation

- Not all claimed conservatisms have been demonstrated
- There are unknowns regarding behavior of fuel
- Some conservatisms are apparent
    - p/f ratio
    - No filtering by strainer in fuel tests
    - Tests were fully stirred to ensure transport
    - Flow rates decrease if head loss increases
        - May provide little margin
    - Debris will deposit non-uniformly
        - Extent unknown
    - Alternate flow paths exist
        - Some may be significant
- Other conservatisms not demonstrated or significant (e.g. fuel bowing)

Boric Acid

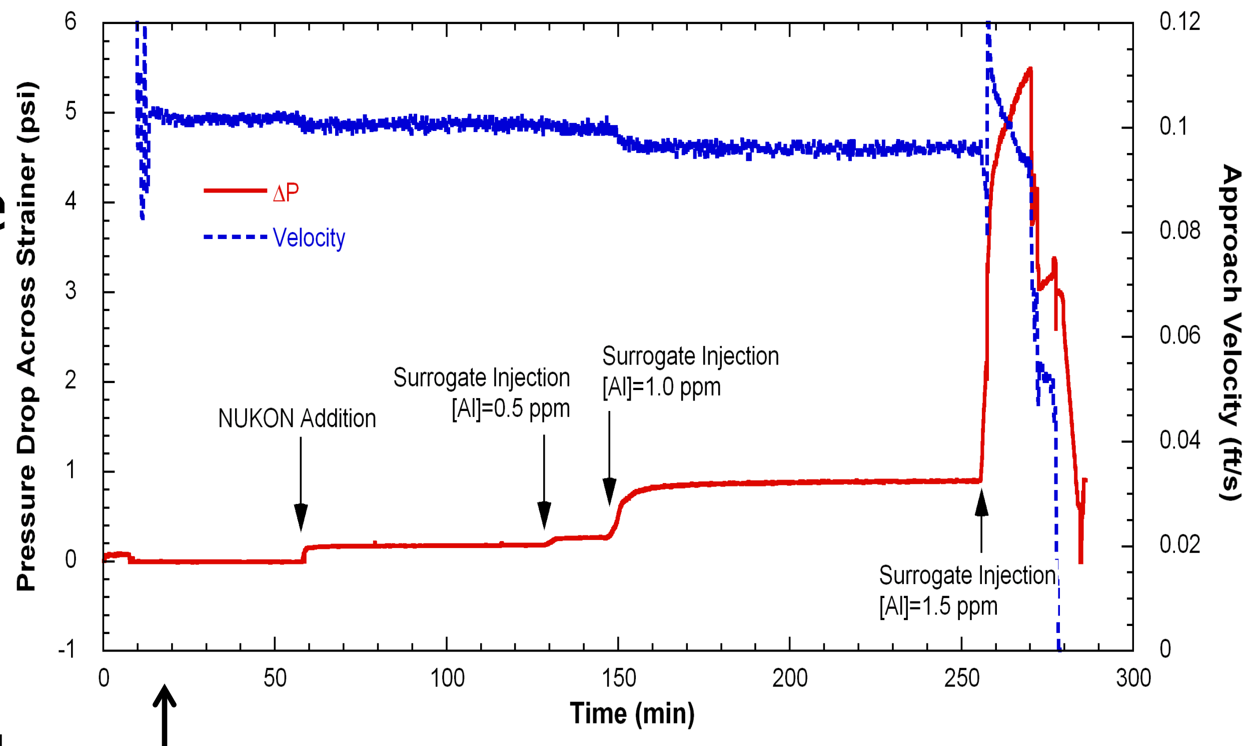
- NRC and the nuclear industry had agreed to evaluate boric acid precipitation issues in a separate PWROG program
- For cold-leg breaks boric acid precipitation is a concern
- Debris could affect ability of coolant in core to mix with lower plenum
- Debris is not accounted for in current boric acid evaluations and boric acid is not considered in GL 04-02 in-vessel evaluations
- With small fiber loads mixing will not be affected
  - A 15 gram fiber limit to the core for a hot-leg break results in less debris ($\leq$50%) in the core for a cold-leg break. That is at 15 grams there is not significant head loss, so at one half that amount mixing will not be affected.
- Moving forward, plants seeking a debris limit greater than 15 grams will be required to evaluate debris effects on boric acid precipitation
- A separate boric acid program will evaluate the effects of debris, even at lower loads

WCAP-16793-NP, Rev. 2 "In-Vessel" Chemical Effects

- Fuel Assembly Tests
  - AlOOH precipitate was added after fiber and particulate
- Fuel Rod Deposition
  - Chemical source term calculated using WCAP 16530-NP-A, "Evaluation of Post-Accident Chemical Effects in Containment Sump Fluids To Support GSI-191,"
  - Deposits on fuel – LOCADM analysis

Fuel Assembly Tests – AlOOH Precipitate, WCAP-16530 Recipe

- ANL vertical loop tests indicate AlOOH precipitate produces the highest pressure drop across a debris bed
- Typical FA test add 20 gals, however, test with 2% of typical load- same result



ANL Vertical Loop Test - AlOOH Precipitate

WCAP-16793-NP – LOCADM

- Inputs from core design parameters such as:
  1. Decay heat
  2. Fuel surface area
  3. Maximum zirconium oxide thickness
  4. Crud thickness based on fuel age
  5. Thermal conductivity values for crud and oxide
  6. Depth in the core and
  7. Fuel element power factor

- Maximum deposition rate occurs when local node conditions predict boiling

LOCADM – Chemical Source Term Assumptions

- WCAP-16793 uses the data for total dissolved materials and precipitated chemicals from WCAP-16530 as the starting point for all ionic materials that can be deposited on the fuel
- Deposition of species on the fuel increases the dissolution rate outside the reactor since the overall solution concentrations are lowered
- No deposition occurs on system surfaces outside the reactor core. All material that is transported to the fuel clad surfaces during boiling is deposited
- Once formed, deposits are not thinned by flow attrition, dissolution, or any other means

LOCA-DM Chemical Deposit

- Two thermodynamic programs (OLI StreamAnalyzer and HSC Chemistry) predictions guided selection of a bounding chemical deposit thermal conductivity
- A lower bound value of 0.11 BTU/(hr-ft-ºF) is used, from the lower bound value for a sodium aluminum silicate deposit

Example Thermal Conductivity Values BTU/(hr-ft- ^{o}F)

fiberglass (dry to water/steam mix) composite foam insulation	.05 to .6 .09 to .10
sodium aluminum silicate	.12 to .23
calcium carbonate	.34 to .52
calcium sulfate	.46 to 1.6
glass	.50 to .80

Staff Rationale For Accepting WCAP-16793-NP Chemical Effects

- WCAP-16793 uses the data for total dissolved materials and precipitated chemicals from WCAP-16530 and assumes all ionic material is available to be deposited on the fuel. This provides a high degree of conservatism given that precipitates may settle on the containment floor, be captured in a sump strainer debris bed, or attach to other system surfaces such as in heat exchangers

- The assumed LOCADM chemical deposit thermal conductivity value 0.11 BTU/(hr-ft-^{o}F) is judged to be conservative

Rationale for Accepting Chemical Effects Evaluation (cont'd)

- Westinghouse calculations showed the following conditions would not cause peak clad surface temperature to reach 800 F:
  - the highest power fuel rod
  - decay heat level at the time switchover to recirculation
  - 100 micron zirconium oxide layer, 100 micron crud layer
  - 50 mils chemical deposit, 0.1 BTU/(hr-ft-ºF)
  - Assuming no axial heat conduction occurs

Rationale for Accepting Chemical Effects Evaluation (cont'd)

- LOCADM calculations for a sample high-fiber plant, 7000 cubic feet of fiberglass debris and 80 cubic feet of calcium-silicate debris, yielded 10 mils maximum chemical deposit thickness

- Therefore, the NRC staff concludes there is a large margin between the chemical deposit predicted for a high-fiber plant with large amounts of calcium silicate insulation and the amount of deposit that would cause the maximum peak clad temperature to exceed the acceptance criteria

Safety Evaluation
Conditions and Limitations - General

- Licensees shall confirm that their plants are covered by the PWROG sponsored fuel assembly tests
- Licensee's GL 2004-02 submittal shall report:
  - The quantity of strainer bypassed fiber
  - The available driving head used in the evaluation
  - The calculated pressure drop
  - The peak cladding temperature predicted by the LOCADM analysis
- Prior to use of new fuel designs, licensees should evaluate their affects on acceptable debris loads

Conditions and Limitations – Plant Specific Evaluations

- Plants that establish higher debris limits shall submit tests and analyses supporting those limits to the NRC
- Plants that credit alternate flow paths shall demonstrate that the flow paths would be effective
- Licensees shall show that core inlet blockage will not invalidate existing post-LOCA boric acid dilution analyses

Conditions and Limitations - Chemical Effects

- Any plant-specific refinements to the WCAP-16530-NP-A base model to reduce the chemical source term need to be justified
- Default crud thickness input for LOCADM shall be 127 microns
- Licensees shall provide a technical justification for use of a chemical deposit thermal conductivity value greater than
  0.11 BTU/(hr-ft-^{o}F)
- Licensees shall accelerate the aluminum release rate by a factor of 2 until the WCAP-16530-NP predicted total aluminum amount is reached

Conclusion

- The staff concludes that applying the procedures, methods, and debris limits contained in WCAP-16793-NP, Rev. 2, as qualified by the NRC staff SE, will result in an acceptable plant-specific evaluation to resolve the issues associated with GL 2004-02

Path Forward

- The WCAP-16793-NP, Rev. 2 fiber limits do not bound a significant number of plants
- Therefore, the PWROG has indicated that plant specific options may be pursued to demonstrate adequate core cooling can be maintained with vessel fiber loading greater than 15 grams/fuel assembly
- NRC staff will be reviewing any plant specific testing to determine if greater fiber limits are justified

ACRS Thermal-Hydraulics Subcommittee Meeting on Proposed Risk-Informed Resolution of GSI-191
9 May 2012

Agenda

- Desired Outcomes
  - Show how we are integrating deterministic and probabilistic models to assess the risk of fibrous insulation in containment
  - Solicit, collect, and consider feedback from the Subcommittee on the risk-informed approach described in this presentation
- Agenda
  - Provide an overview of the background and context of deterministic and risk-Informed closure efforts
  - High level view of the project elements, physical models and the probabilistic risk assessment

Introductions and Agenda

- Introductions, Speakers
  - Mike Murray
  - Ernie Kee
  - Bruce Letellier
  - Rodolfo Vaghetto
  - Tim Sande
  - Gil Zigler
  - Kerry Howe
  - David Johnson

Introductions and Agenda

- Additional STPNOC Attendees

John Crenshaw	Vice President, Projects, Outages & IT, STPNOC
Steve Blossom	Manager, IT Support & Tech, & GSI-191, STPNOC
Scott Head	Manager, Regulatory Affairs, STP 3 & 4
Craig Murry	Manager, Safety Review Project, STPNOC
Wes Schulz	Design Engineer, STPNOC
Craig Sellers	Alion Science & Technology
Zahra Mohaghegh	Technical Oversight, Soteria Consultant
Yassin Hassan	Texas A&M University
Alex Galenko	The University of Texas at Austin
Steve Frantz	Morgan Lewis

Agenda, continued

- Presentation content
  - Review technical team, Mike Murray
  - Background, overview, Ernie Kee
  - Integrated framework, Bruce Letellier
  - Thermal-hydraulics, Rodolfo Vaghetto
  - LOCA Frequency, Bruce Letellier
  - Debris generation and transport, Tim Sande
  - Strainer head loss, Gil Zigler
  - Chemical effects, Kerry Howe
  - Downstream effects (bypass, incore blockage, boron precipitation, air ingestion), Tim Sande
  - Probabilistic Risk Assessment, David Johnson

Risk-Informed GSI-191 Team

- The South Texas Project
  - Steve Blossom, Project Manager
  - Rick Grantom, Industry & Regulatory Coordination Lead
  - Ernie Kee, Technical Team Lead
  - Jamie Paul, Licensing Lead
  - Wes Schulz, Design Engineering Lead
- GSI-191 Analysis & Methodology Implementation (GAMI), Alion Science and Technology
  - Tim Sande
  - Gil Zigler
  - Craig Sellers
- Corrosion/Head Loss Experiments (CHLE), University of New Mexico
  - Kerry Howe, PhD
  - Janet Leavitt, PhD

Risk-Informed GSI-191 Team

- Containment Accident Stochastic Analysis (CASA) Grande, Los Alamos National Laboratory
  - Bruce Letellier, PhD
- Oversight, Soteria Consultants
  - Zahra Mohaghegh, PhD
  - Seyed Reihani, PhD
- Thermal Hydraulics (TH), Texas A&M University
  - Yassin Hassan, PhD
  - Rodolfo Vaghetto
- Uncertainty Quantification (UQ), The University of Texas at Austin
  - Elmira Popova, PhD
  - Alex Galenko, PhD
- Probabilistic Risk Assessment (PRA), ABS Consulting
  - David Johnson, ScD
  - Don Wakefield
- Location-Specific Failure Behavior (DM), Knf Consulting Services, LLC
  - Karl Fleming
  - Bengt Lydell (ScandPower)

STP Nuclear Power Station

- Basic Description
  - Dual Unit, 3853 MWth, four Loop Westinghouse PWR NSSS, large, dry containment, independent ECCS trains (no cross connection headers)
  - Primary insulation fiberglass, Trisodium phosphate buffer
- GSI-191 Response
  - Uniform-loading ECCS strainers installed (approximately factor of ten increased strainer flow area)
  - Early termination of Containment Spray
  - Marinite (Calcium Silicate) insulation removed
  - Post-maintenance containment cleanup and Inspection

Historical Perspective

- The assurance of long-term core cooling in PWRs following a LOCA has a long history dating back to the NRC studies of the mid 1980s associated with Unresolved Safety Issue (USI) A-43
- Results of the NRC research on boiling water reactor (BWR) ECCS suction strainer blockage of the early 1990s identified new phenomena and failure modes that were not considered in the resolution of USI A-43
- As a result of these concerns, Generic Safety Issue (GSI) 191 was identified in September 1996 related to debris clogging of the ECCS sump suction strainers at PWRs
- December 2010 the Commissioners Issued Staff Requirements Memorandum for SECY-10-0113 – Closure Options for Generic Safety Issue - 191, Assessment of Debris Accumulation on Pressurized Water Reactor Sump Performance
- By March 2011, STP had completed fully assembling a team having as its objective assessing the risk of the issues raised in GSI-191 in the as built, as operated plant, with the view to continue with previously successful risk-informed regulatory actions

Past Closure Efforts and Requirements

- Despite significant work, GSI-191 remains open
  - Analysis efforts have been deterministically-based
  - Conservative assumptions are used to avoid straightforward uncertainty quantification
  - Risk and uncertainty are unquantified
- Specific considerations regarding long-term core cooling analysis
  - Several postulated LOCAs of different sizes, locations, and other properties are required to ensure that the most severe cases are included
  - The analytical technique must realistically describe the behavior of the reactor system while accounting for uncertainties
  - Comparisons to applicable experimental data and uncertainties in the analysis methods and inputs are essential so that the uncertainty in the calculated results can be estimated
  - The calculation shows that there is a high level of probability that the criteria set forth will not be exceeded

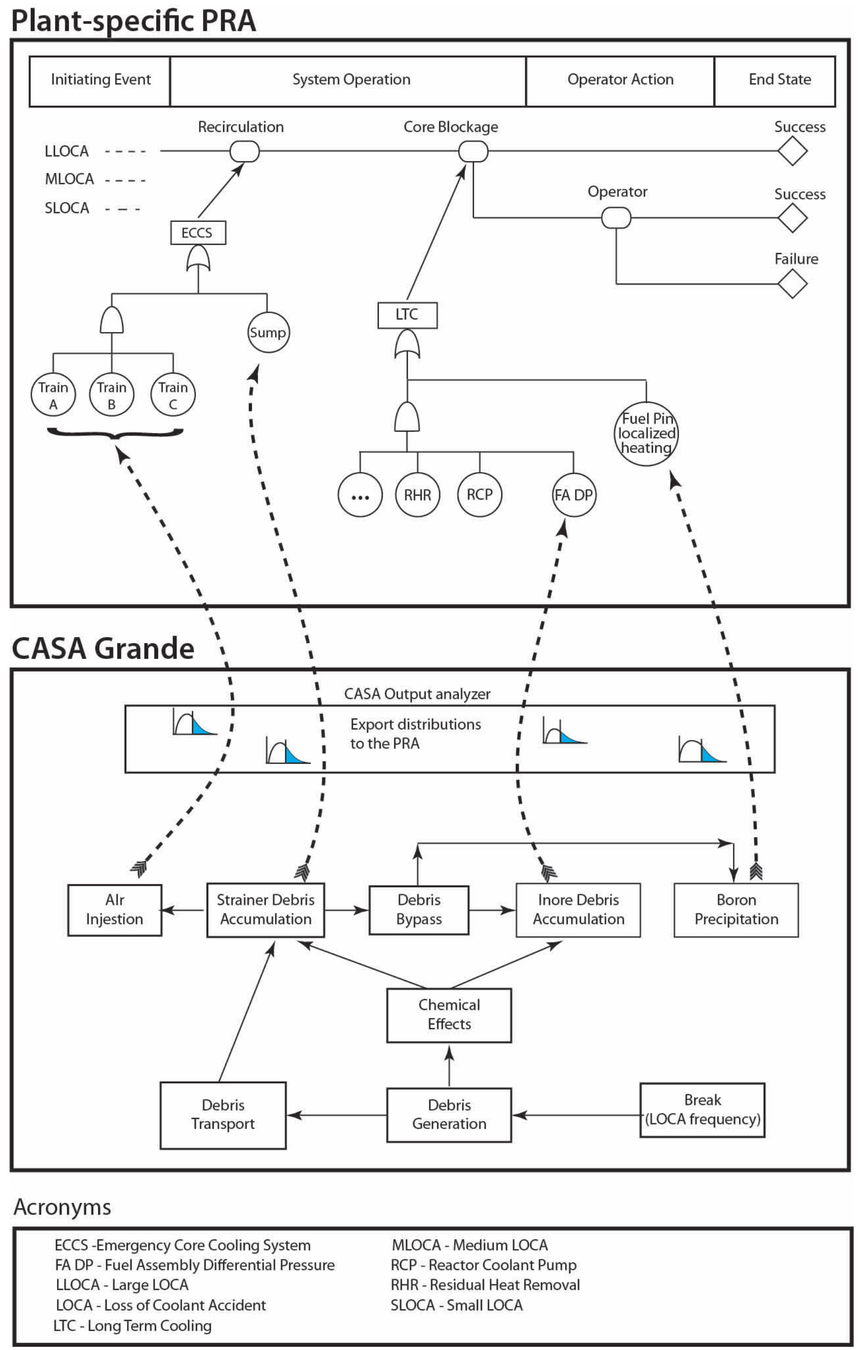
Risk-Informed Approach

- Advantages of the risk-informed approach
  - The full spectrum of postulated LOCA events is analyzed
  - The ensuing physical processes are modeled as realistically as possible
  - Probabilities and frequencies are quantified appropriately
  - Uncertainties are quantified to include the possibility of extreme events which have not been contemplated in traditional deterministic analysis
  - Uncertainty in experimental and operational data are used directly to quantify and characterize the uncertainty
- Guidance for “high level of probability” has been provided in RG 1.174
  - Risks from “unacceptable" to “very small” are defined
  - Methods to evaluate risk and uncertainty are required

Project Objectives

- Through a risk-informed approach, provide the necessary technical basis for the NRC to close the safety issues related to GSI-191 by the end of 2013
- Analyze and implement the necessary licensing requirements needed to support an exemption from certain requirements of 10 CFR 50.46
  - The licensing approach is based on Regulatory Guide 1.174
  - Decision making is based on the difference in risk between a "perfect" design and the existing design

- The methodology is implemented in two main components that utilize the existing PRA LOCA logic
- Basic event distributions are propagated through underlying physical models to the plant-specific PRA
- The methodology is designed to facilitate implementation at other plants



Uncertainty Quantification in the GSI-191 Computer Model, CASA Grande

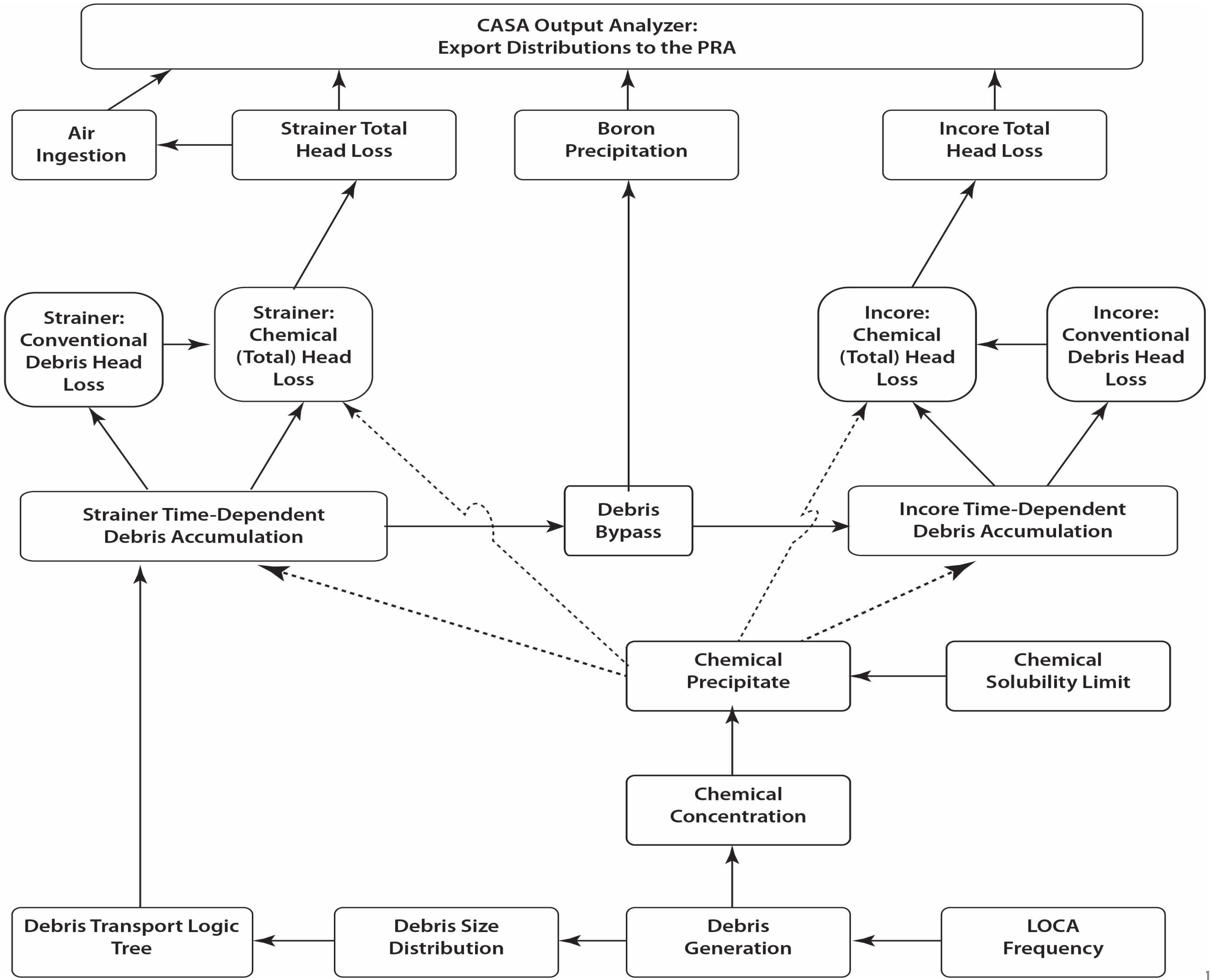
- Modeling and propagation of uncertainties for the GSI-191 project involves several steps
  - Uncertainty models for the input parameters
  - Proper sampling strategies of the input
  - Implementation in a computer model
  - Output analysis
- Methodologies, some new to traditional risk analysis, are required
  - Parametric (or non-parametric) fits of data may require expert elicitation
  - Traditional Monte Carlo sampling may need to be supplemented with other strategies
  - Propagation of uncertainties includes time-dependencies

Analytical Objectives

- Develop tools to populate sump availability and core blockage for the PRA
- Inform risk mitigation strategies and defense in depth
- Add resolution to phenomenological models
  - Time dependent scenario evolution
  - Accounting for the frequency of occurrence
- Uncertainty quantification and propagation

CASA Grande Objectives (Containment Accident Stochastic Analysis)

- Propagate uncertainty in physical parameters from break initiation to potential core damage precursors (1) strainer head loss, (2) core blockage, (3) boron precipitation, (4) air ingestion
- Fold uncertainties into plant performance metrics to support Risk Based Decision making
  - Diagnostic platform for parameter studies, research prioritization, sensitivity analysis, comparison of physical approximations
  - Risk Mitigation and Defense in Depth
- Properly weight the relative frequency of many thousands of accident sequences
  - Statistically sample and combine parameter variations (both physical and decisional) in unbiased distributions of possible outcome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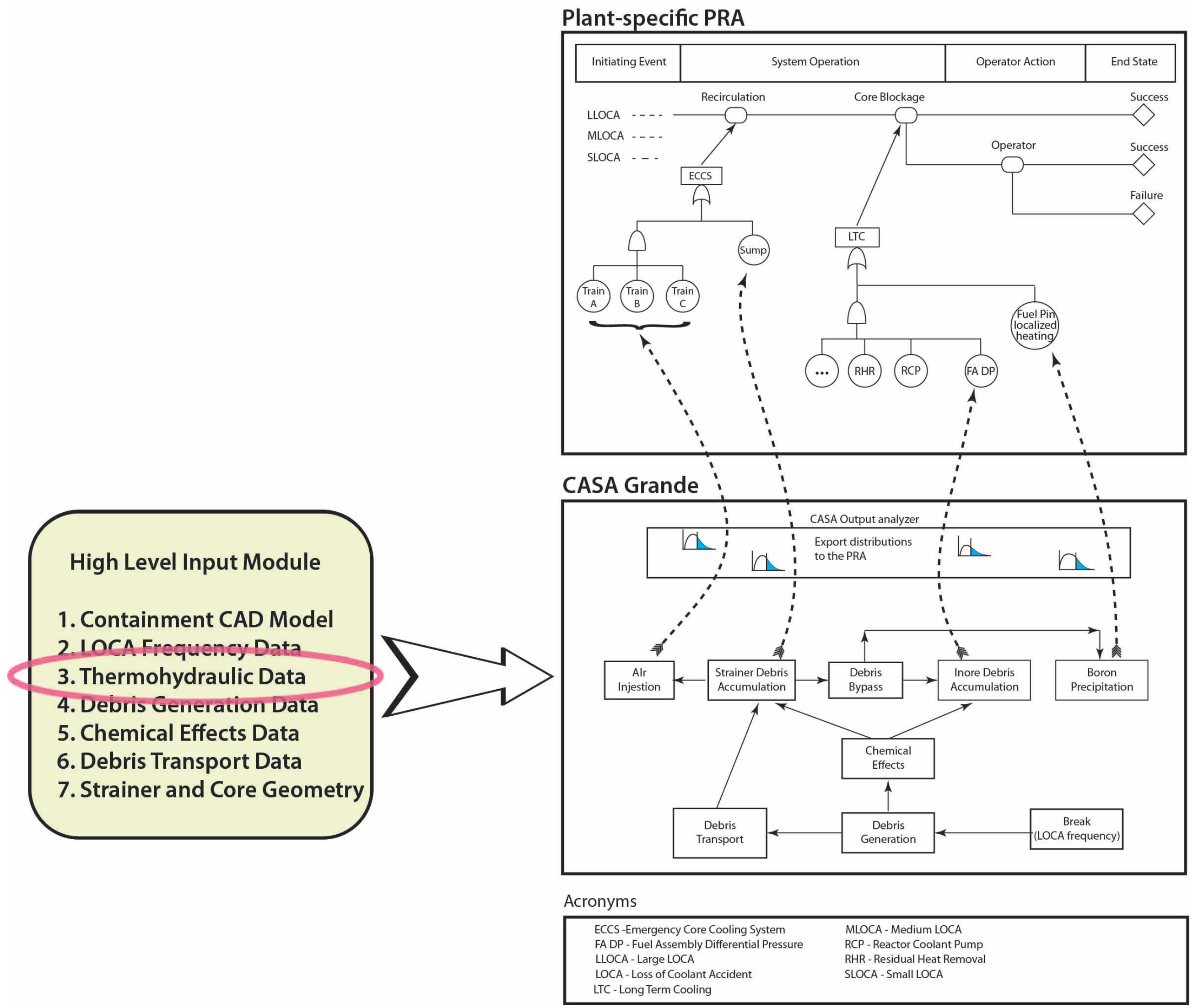
Thermal-Hydraulics



Thermal-Hydraulics

- RELAP5-3D is used to perform the thermal-hydraulic calculations of the reactor system during the phases of the accident
  - 3D vessel, 1D core model used for system response (break flow, mass through core, inlet and outlet temperatures) and containment boundary conditions
  - 3D vessel, 3D core used for detailed core thermal-hydraulic response
- MELCOR is used to evaluate the reactor containment response (sump temperature) and RCS boundary conditions
- RELAP5-3D is coupled with DAKOTA (optimization and uncertainty analysis Sandia computer code) to perform sensitivity analysis

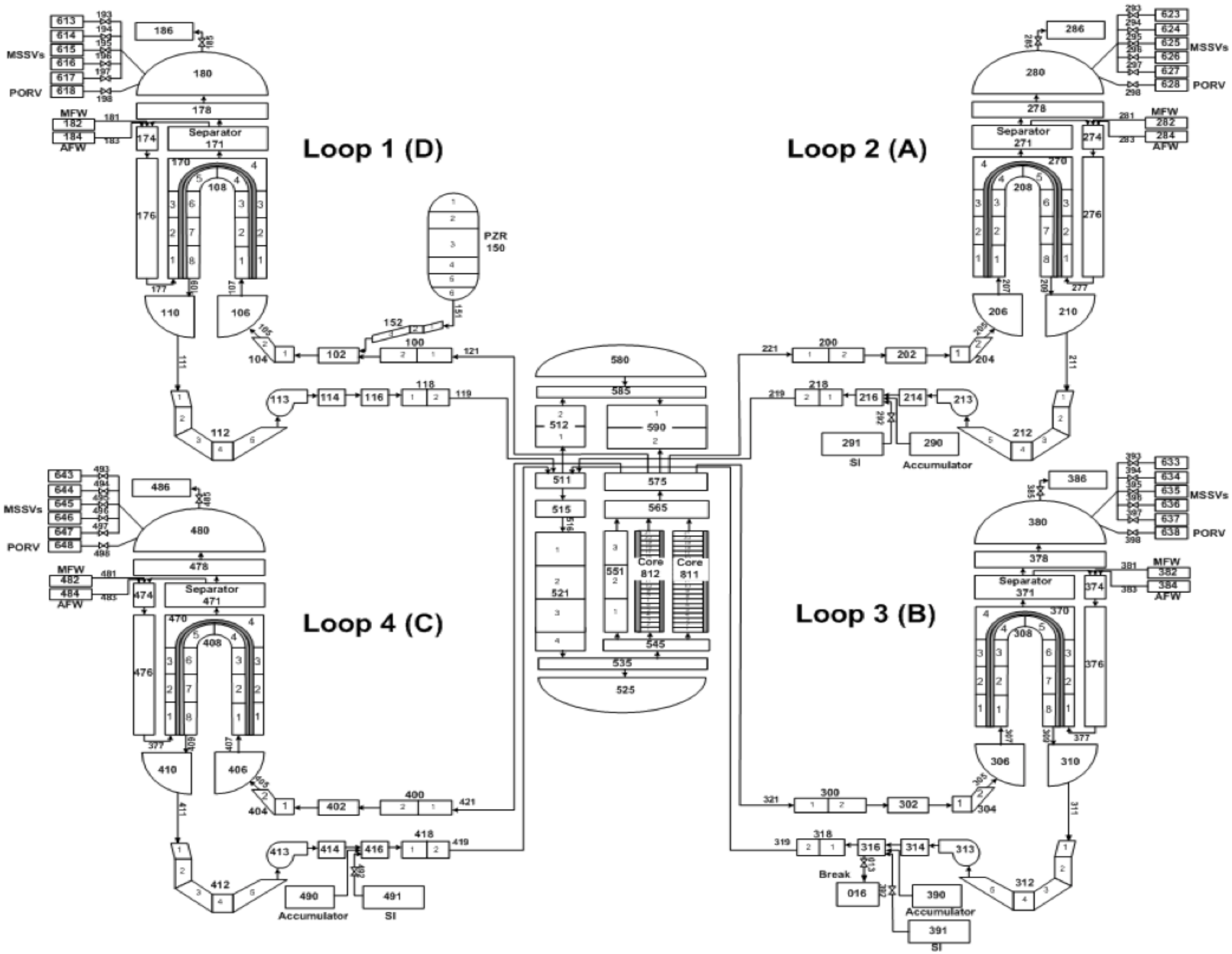
Thermal-Hydraulics, RELAP5

- Data collection
  - STP UFSAR
  - Certified inputs (RETRAN, MAAP, Gothic, Contempt)
  - CAD drawings
  - STP simulator results
- Model and input preparation
- Input review and documentation
- Case execution

Thermal-Hydraulics, RELAP5 Nodalization

- Four independent loops to account for flow asymmetries during blowdown and long-term cooling phases
- Separate average and hot channels modeled with 21 axial subvolumes
- Independent SI trains
- Main Plant Operation Procedures (POP) implemented
- Long-term cooling operations included (RWST isolation, hot leg injection)
- Different break sizes
- Different break locations

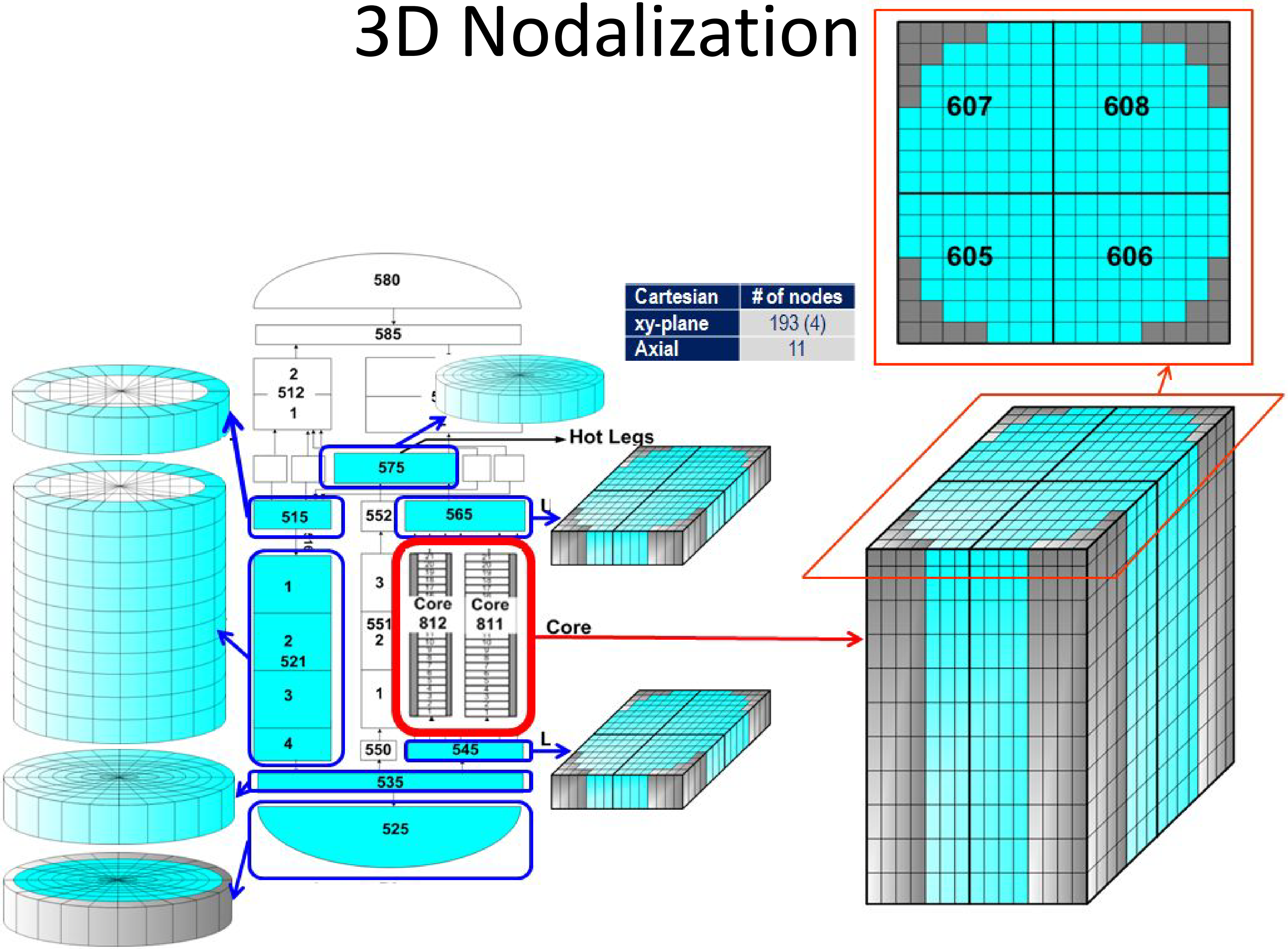
Thermal-Hydraulics, RELAP5 1D Nodalization



Thermal-Hydraulics, RELAP5 3D Nodalization

- Main vessel components (downcomer, lower plenum, lower/upper core plates, reactor core) modeled with 3D components
- Reactor core modeled with 193 channels with cross junctions
- New vessel nodalization integrated into the model

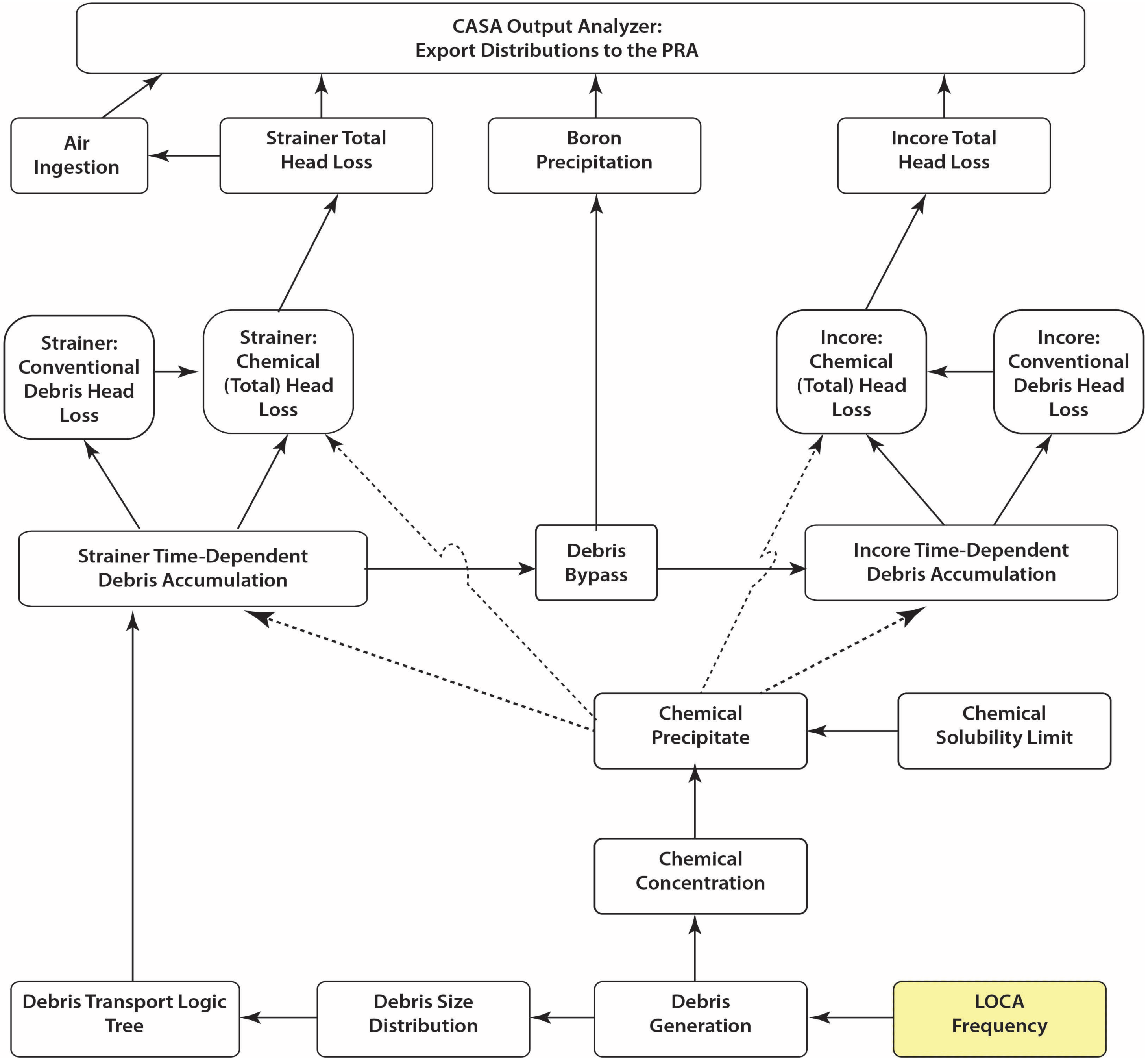
Thermal-Hydraulics, RELAP5 3D Nodalization



Cartesian	# of nodes
xy-plane	193 (4)
Axial	11

Thermal-Hydraulics, MELCOR

- Containment consists of six control volumes (CVs)
- Flow paths model pathways between containment CVs
- Heat structures for containment walls, floors, etc.
- Includes input for engineered safety features
  - Fan coolers for containment cooling and air circulation
  - Containment sprays in upper containment
  - Mass and enthalpy source from RELAP5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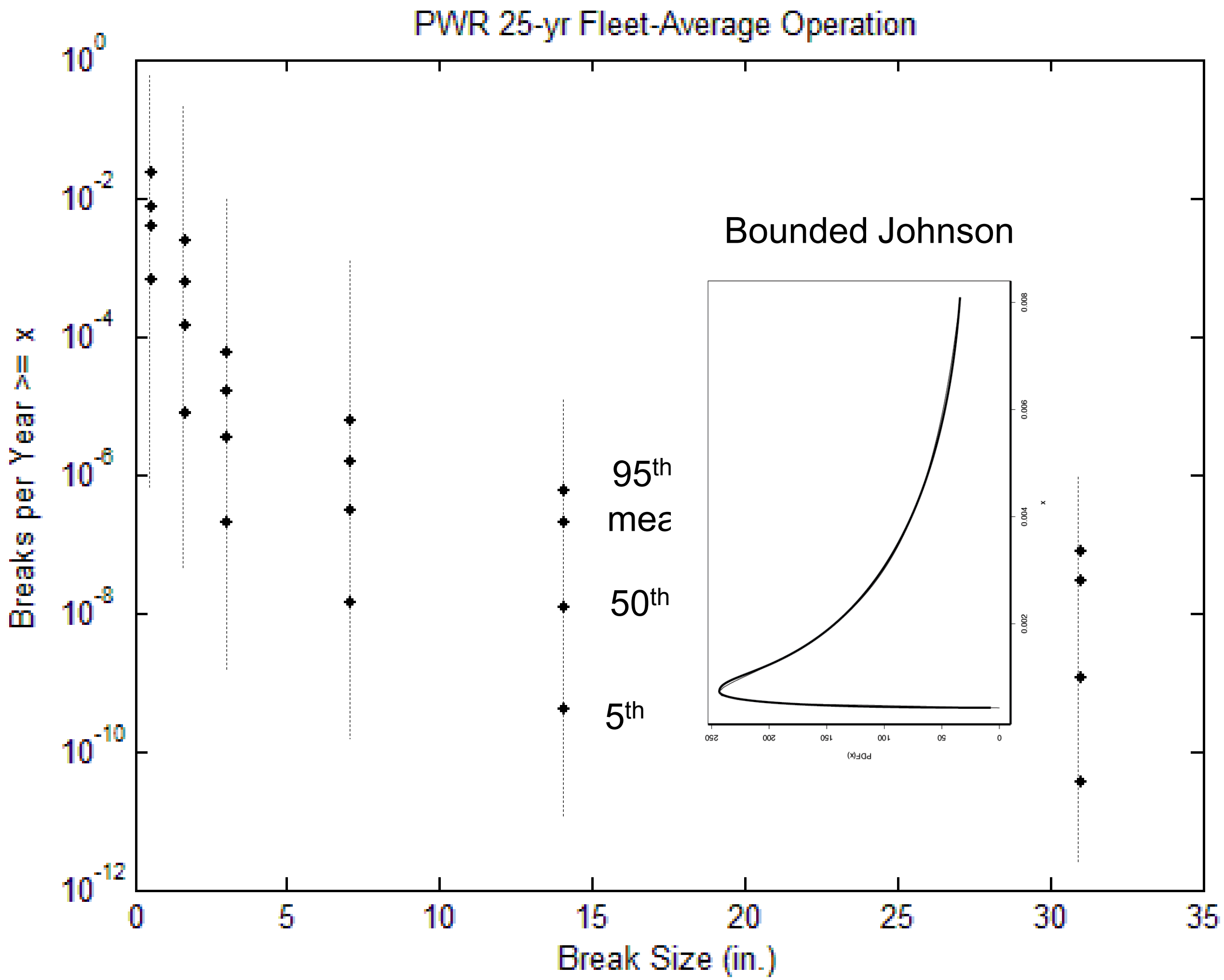
LOCA Model is Consistent with PRA

- Desire to maintain consistency between the PRA logic and CASA Grande
  - The PRA and CASA Grande both begin with NUREG 1829 estimates
  - NUREG 1829 provides four distributional characteristics for each break size category: mean, median, 5th and 95th percentiles. We would like to sample from distributions having a bell-shaped probability distribution function (PDF) that match these values as close as possible
  - The LOCA frequencies are not plant specific or plant-location specific, degradation mechanisms need to be included
  - In 2011, we investigated degradation mechanisms based on the Risk-Informed ISI methodology
  - In our analysis we need to "initiate" a break at a random location (weld) inside STP
- We conserve the values from NUREG 1829 and use them as input to both CASA Grande and PRA analysis

NUREG 1829 Break Frequency Illustration

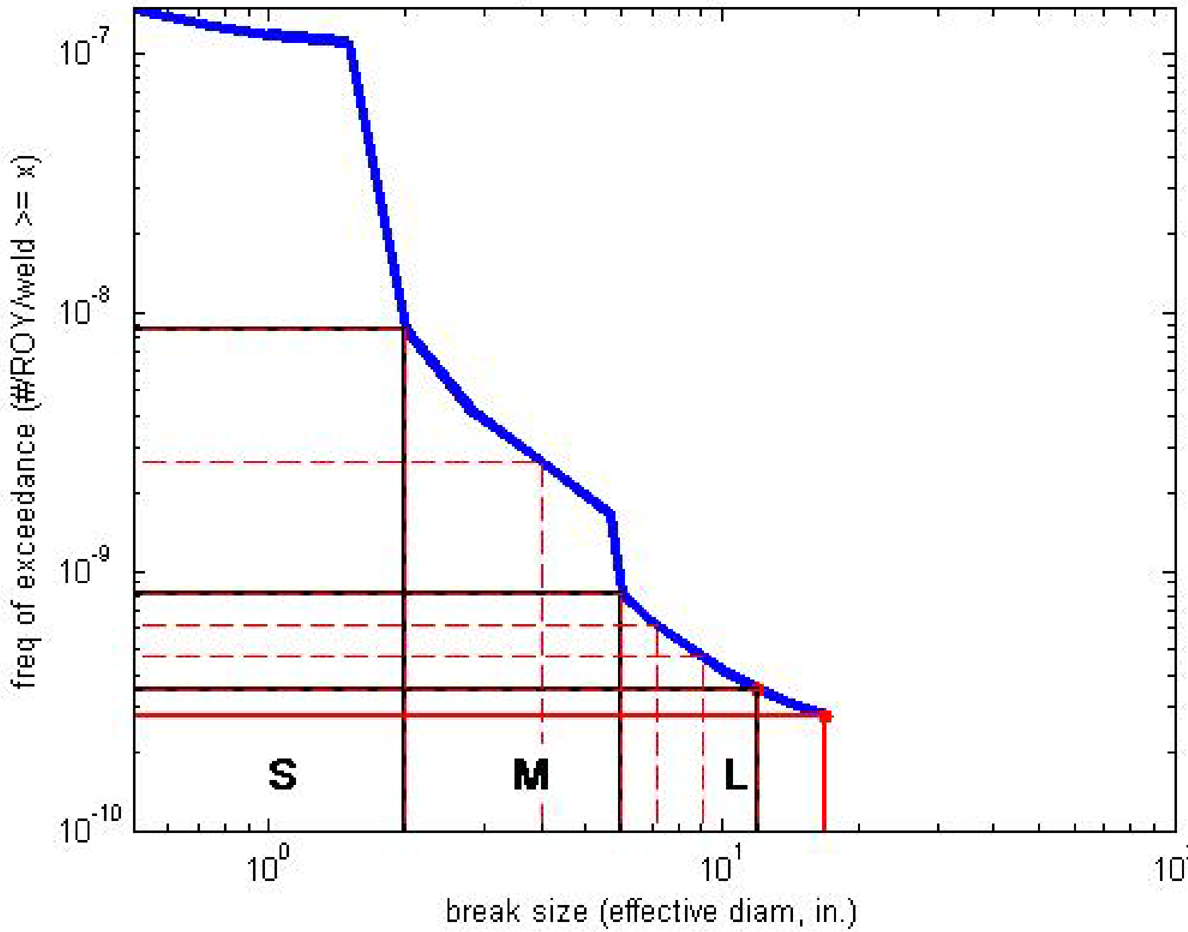


LOCA Model Uncertainty Distribution is Consistent with NUREG 1829

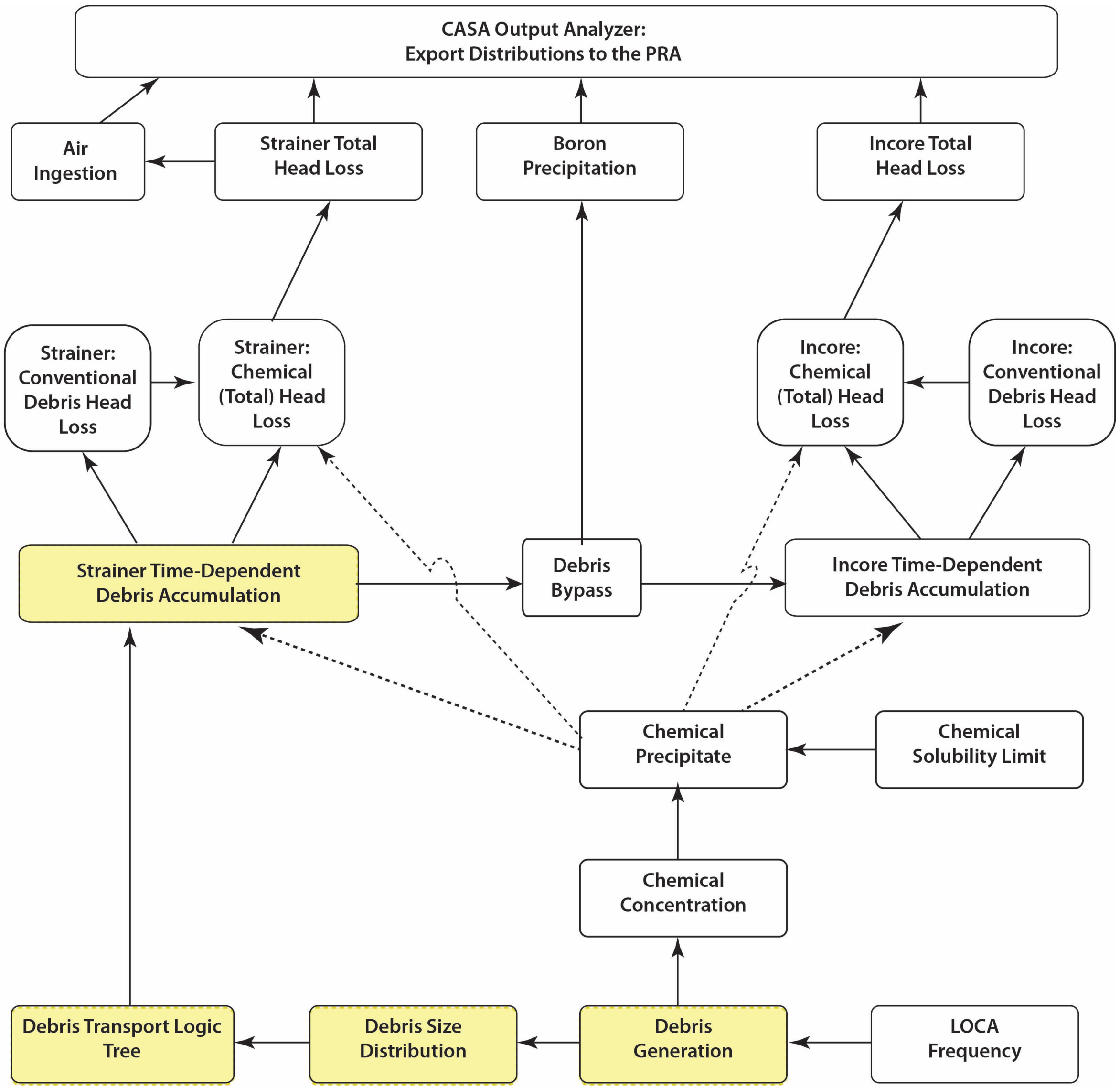
- Parametric fits to the four parameters of the distributions
  - NUREG 1829 used two split Lognormal distributions
  - NUREG/CR 6828 used a Gamma distribution
  - Although there are an infinite number of distributions one can partially fit, we are looking for the one that matches the four characteristics as closely as possible. Therefore, a parametric distribution with four parameters is required
  - The bounded Johnson distribution fits this requirement
- Bounded Johnson optimized fitting process
  - For each break size category, solve a nonlinear optimization problem having as the objective function the weighted squared error. The highest weights are put on the median and 95th percentile.
  - Consider six constraints that correspond to matching each of the NUREG 1829 characteristics: mean, median, 5th, and 95th percentiles plus two constraints regarding the form of the pdf curve and a shift to preserve the 5th percentile

Break Frequency Sampling Strategy

- Nonuniform probability sampling ensures that DEGB is included for *every* weld location.
- Example:
  - Applies to all welds of a particular type
  - 3 bins for LLOCA plus DEGB
  - 2 bins for MLOCA
  - 1 bin for SLOCA

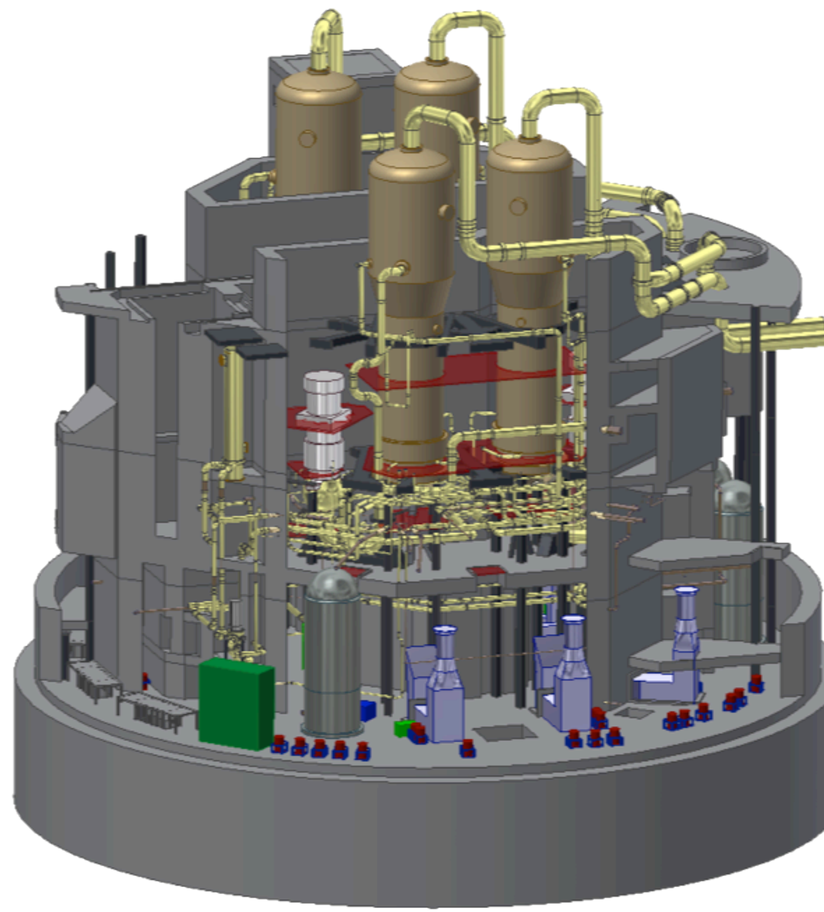


Debris Generation to Accumulation



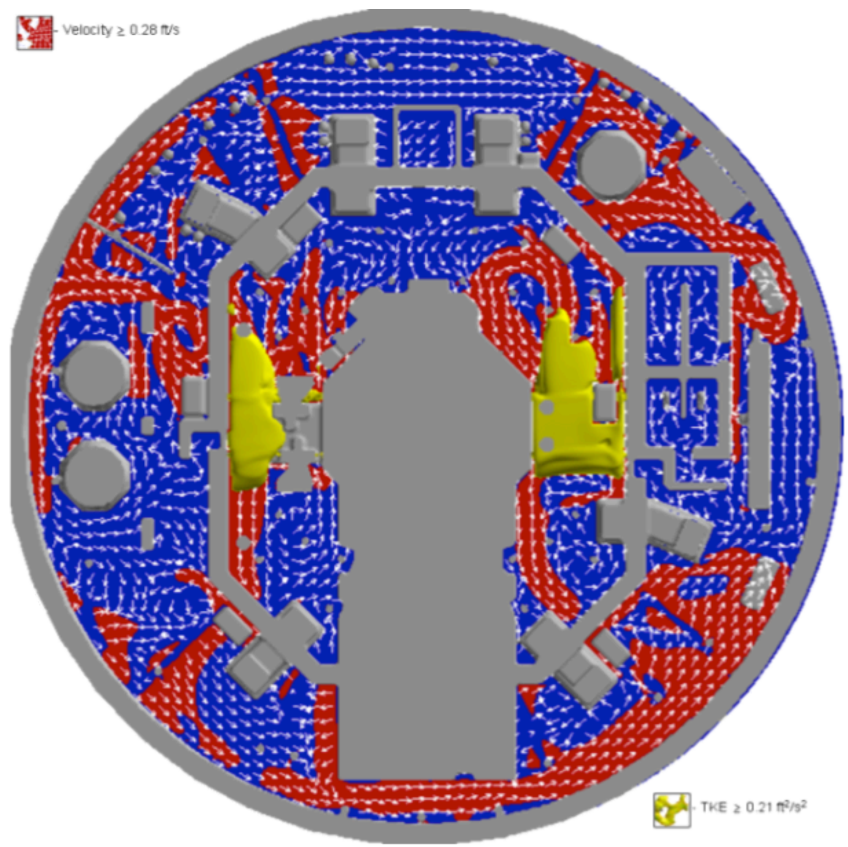
Debris Generation to Accumulation, continued

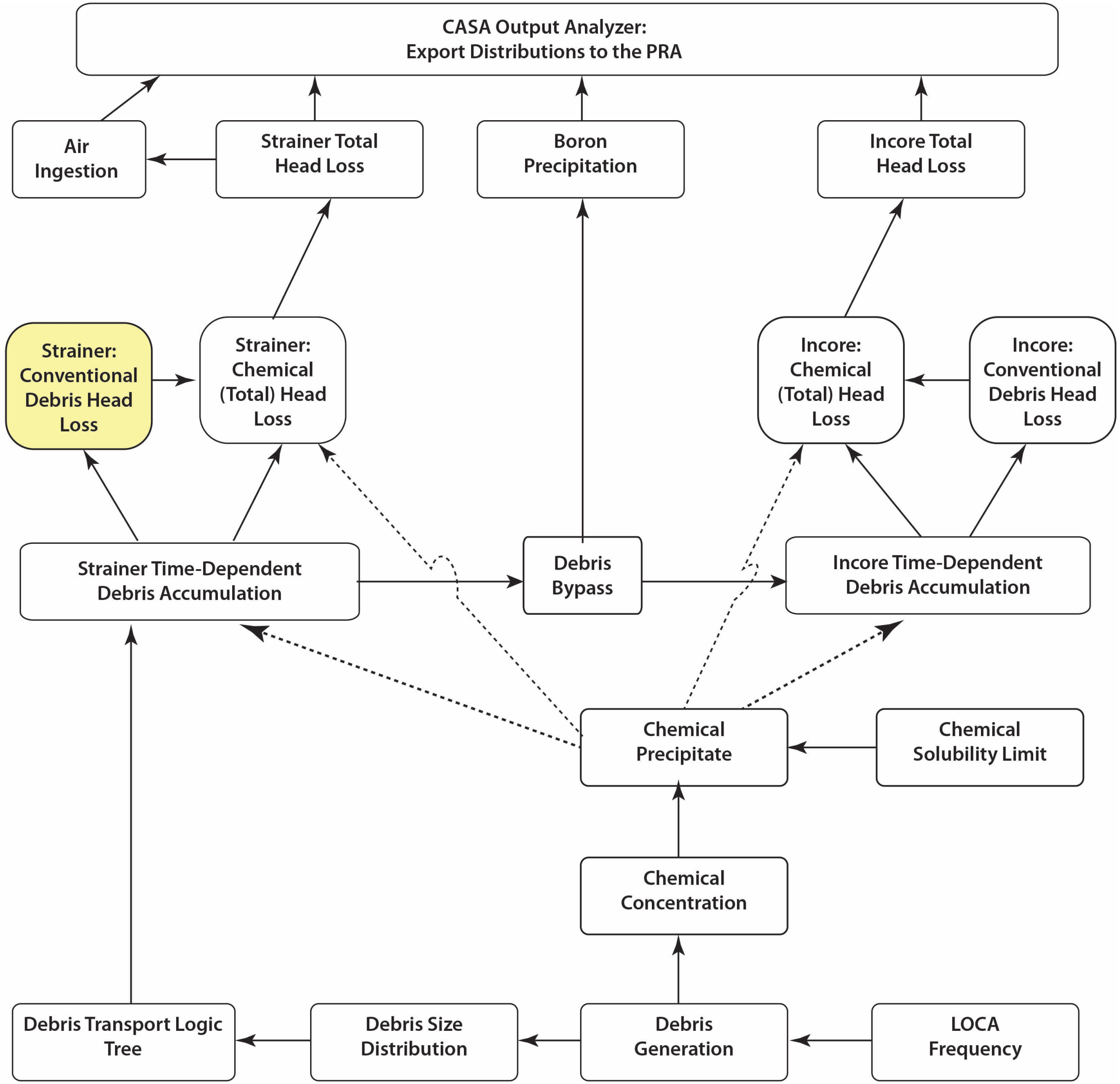
- Adapt deterministic GSI-191 methodology to the risk-informed approach
- Import CAD data into CASA Grande
- Determine ZOI based on insulation destruction pressure and break size
- Automatically calculate insulation debris quantities for thousands of break scenarios in CASA Grande
- Input coatings and latent debris quantities and debris characteristics



Debris Generation to Accumulation, continued

- Input blowdown, washdown, pool fill, recirculation, and erosion transport fractions
- Determine time–dependent arrival of debris at the strainer based on time-dependent variables (unqualified coatings failure, washdown transport, recirculation transport, fiberglass erosion, chemical precipitation, etc.)

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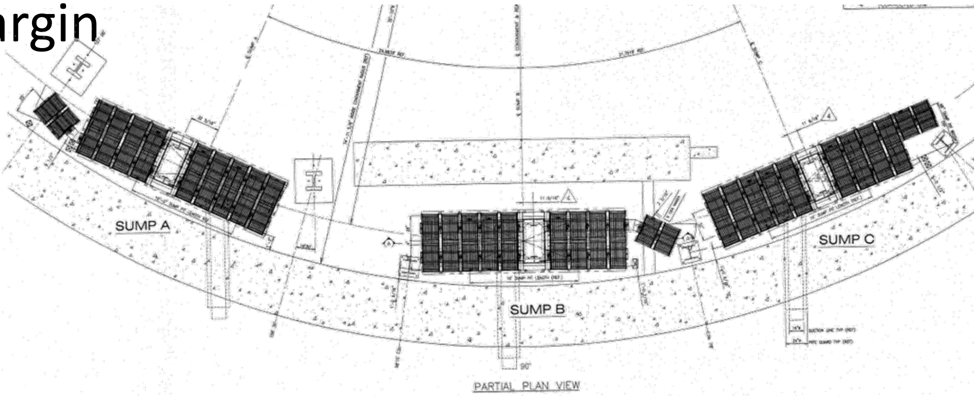
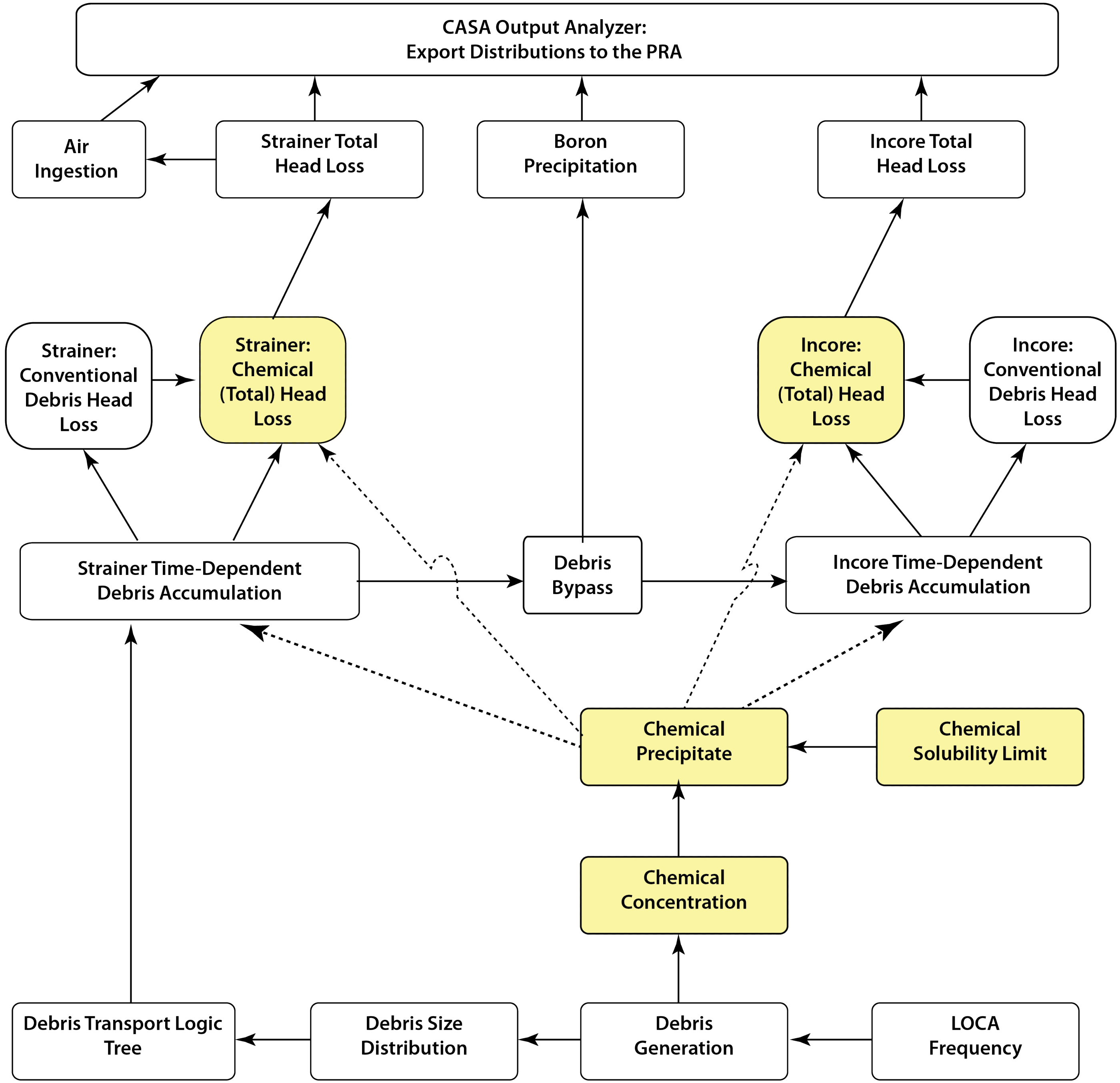
Strainer Head Loss, Conventional

- Time-dependent head loss will be calculated based on debris arrival time, flow rate, and temperature
- NUREG/CR-6224 head loss correlation was used for initial quantification
- Correlation will be verified or modified as necessary for STP-specific conditions based on vertical loop head loss testing
- Acceptance criteria are that strainer head loss must be less than NPSH margin and structural margin





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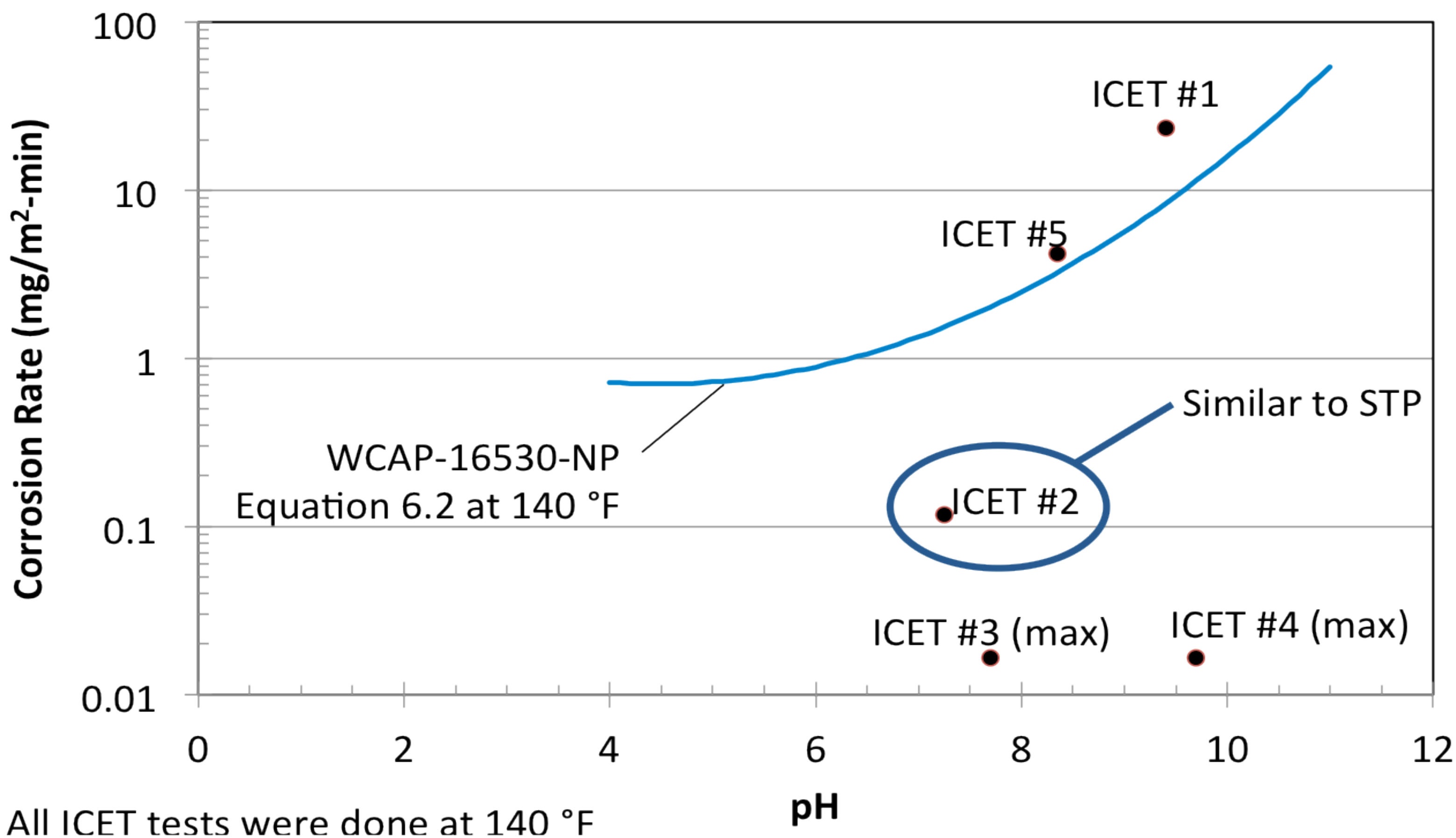
Chemical Effects, Issues

- STP strainer head loss testing showed that head loss roughly doubled upon introduction of WCAP-16530-NP precipitates
- ICET tests similar to STP had less corrosion than predicted by the WCAP-16530-NP equation and no precipitate formation

Chemical Effects, Realistic Experiments Compared to Deterministic Experiments



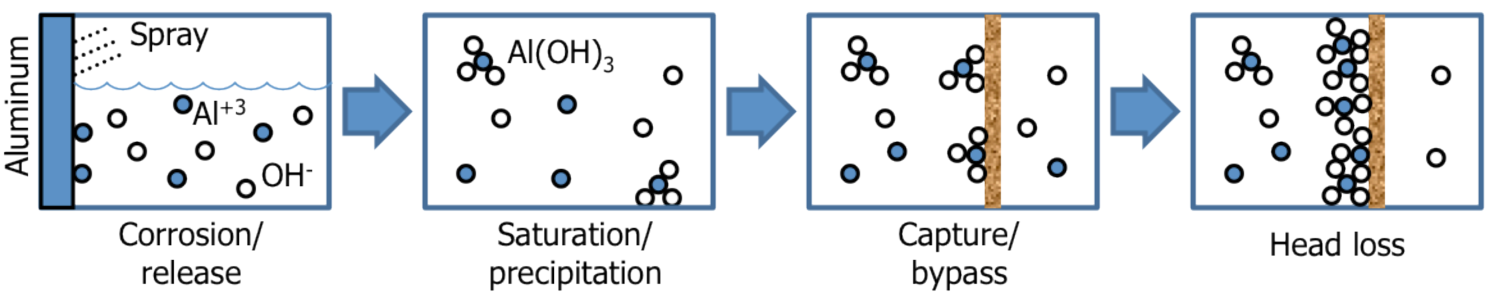
ICET results from NUREG/CR-6914 (2006)

Chemical Effects, Objectives

- Support the risk-informed resolution of GSI-191
- Improve understanding of how corrosion processes contribute to head loss
- Develop data and models for input to CASA Grande to calculate time-dependent chemical concentrations, solubility limits, precipitate formation, and precipitate contribution to head loss at the strainer and in the core
- Use bench-top testing to investigate potential variations in plant-specific conditions

Chemical Effects, Hypothesis

- Constituents in solution (phosphate, silicon) can lead to passivation of metal surfaces, reducing corrosion
- The pre-formed precipitates used in the WCAP-16530-NP protocol can cause higher head loss than when metal ions are released slowly into solution via corrosion



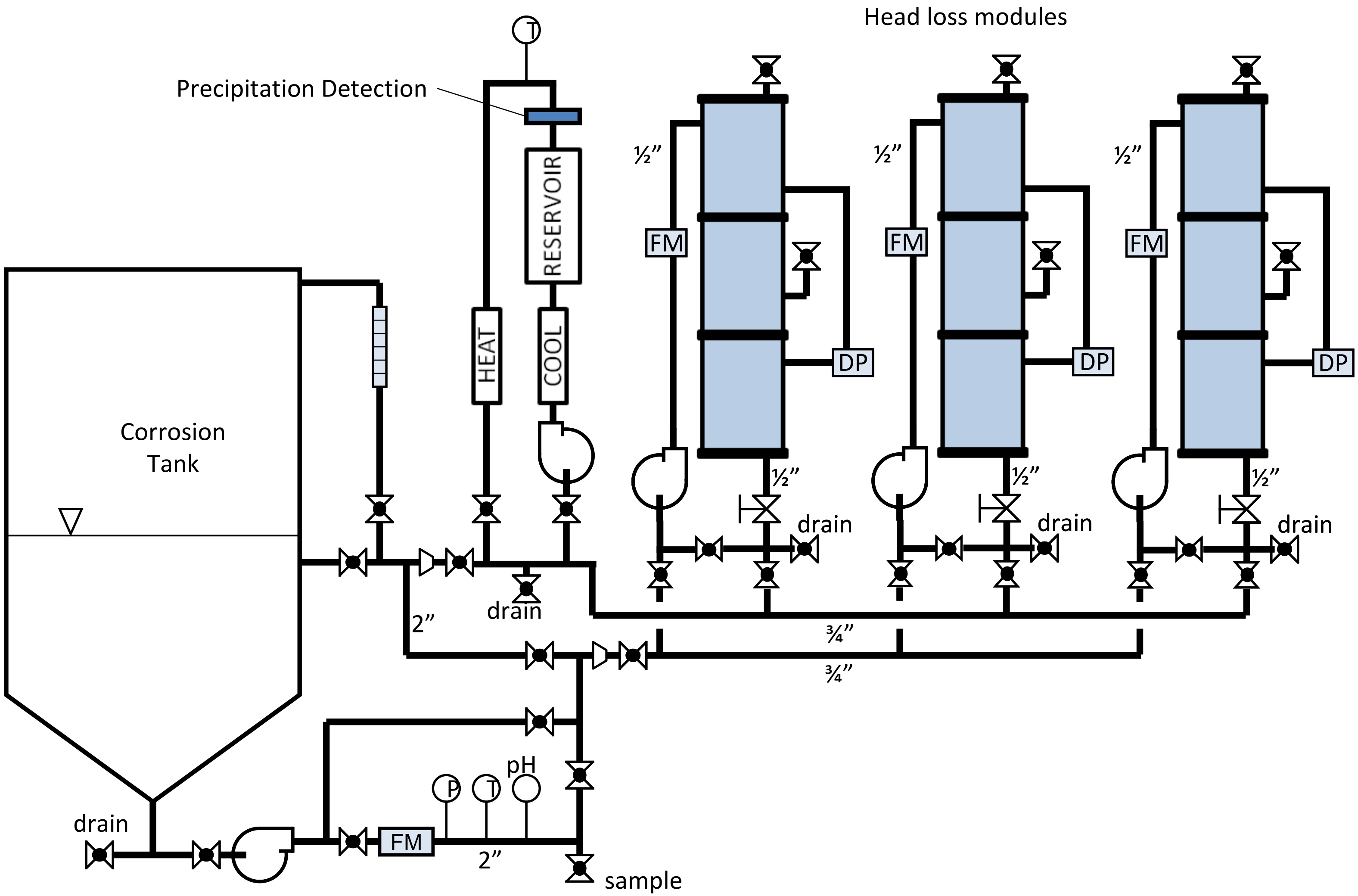
Chemical Effects, Comprehensive Experimental Strategy

- 30-day corrosion/head loss experiment (CHLE) tests
  - Investigate relationship between corrosion, release, and head loss for small, medium, and large LOCA conditions
- Bench-scale tests
  - Investigate factors that affect corrosion and inhibition
  - Investigate the composition of precipitates that are formed
- Shorter-term CHLE tests
  - Generate head loss data for additional conditions to populate input to CASA Grande

Chemical Effects, Apparatus and Design

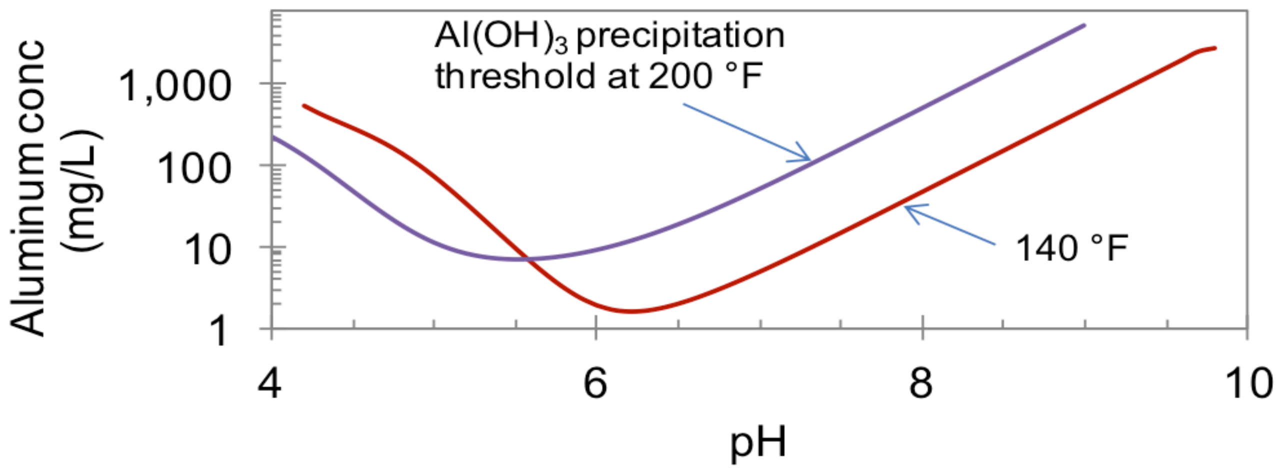
- Corrosion testing integrated with head loss testing
- Relevant materials scaled to quantities in STP containment
- Realistic temperature, pH, chemicals, materials, and flow rate
- Three parallel head loss modules with representative debris bed for repeatability

Chemical Effects

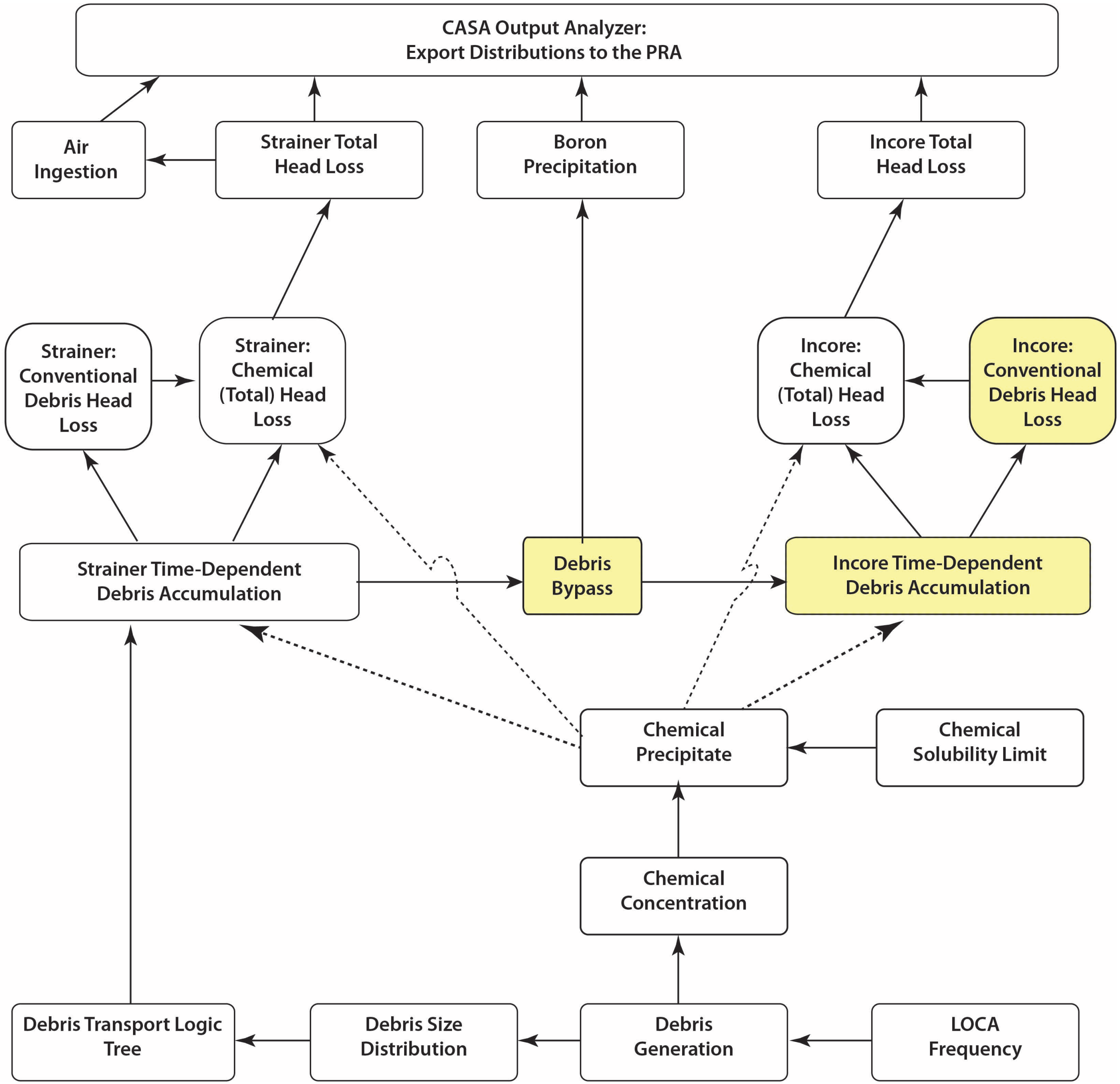


Chemical Effects

- CASA Grande module to predict time-dependent concentration of materials in solution as function of materials, chemicals, pH, temperature
- Solubility calculated from solution chemistry, pH, and temperature
- CASA Grande module to predict time-dependent head loss from chemicals as function of temperature, debris bed, and concentrations exceeding solubility limits



Al(OH)3 saturation data calculated with Visual MINTEQ ver 3.0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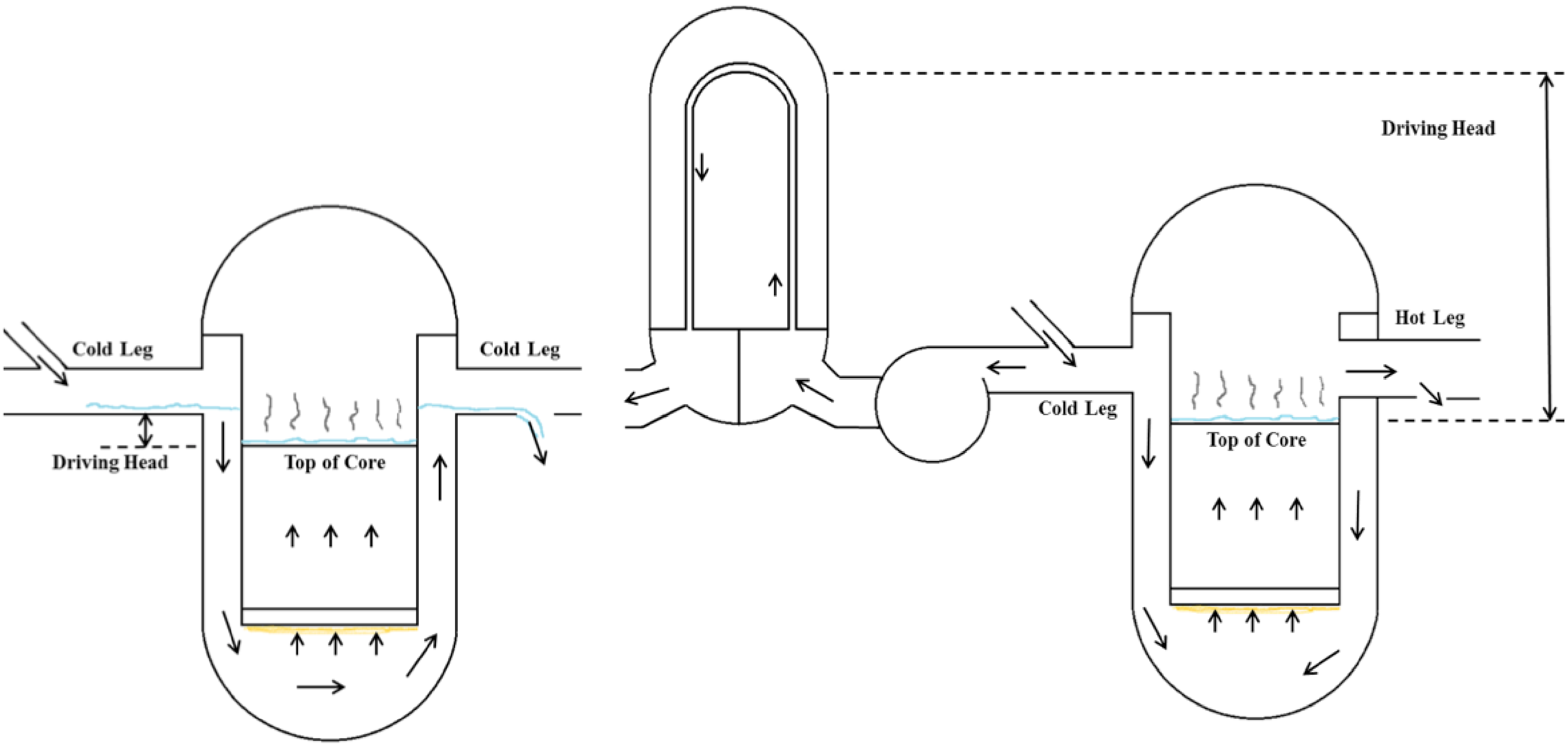
Incore Blockage Considerations

- Core blockage is highly dependent on break location and injection flow path
  - Cold leg break with cold leg injection
  - Cold leg break with hot leg injection
  - Hot leg break with cold leg injection
  - Hot leg break with hot leg injection
- Switchover to hot leg injection occurs 5.5 hours after start of recirculation
- Strainer bypass would predominantly occur within 5 pool turnovers (less than 2 hours for an LBLOCA and over 2 days for an SBLOCA)
- Chemical precipitation is not likely to occur for several hours or days (i.e. after switchover to hot leg injection)

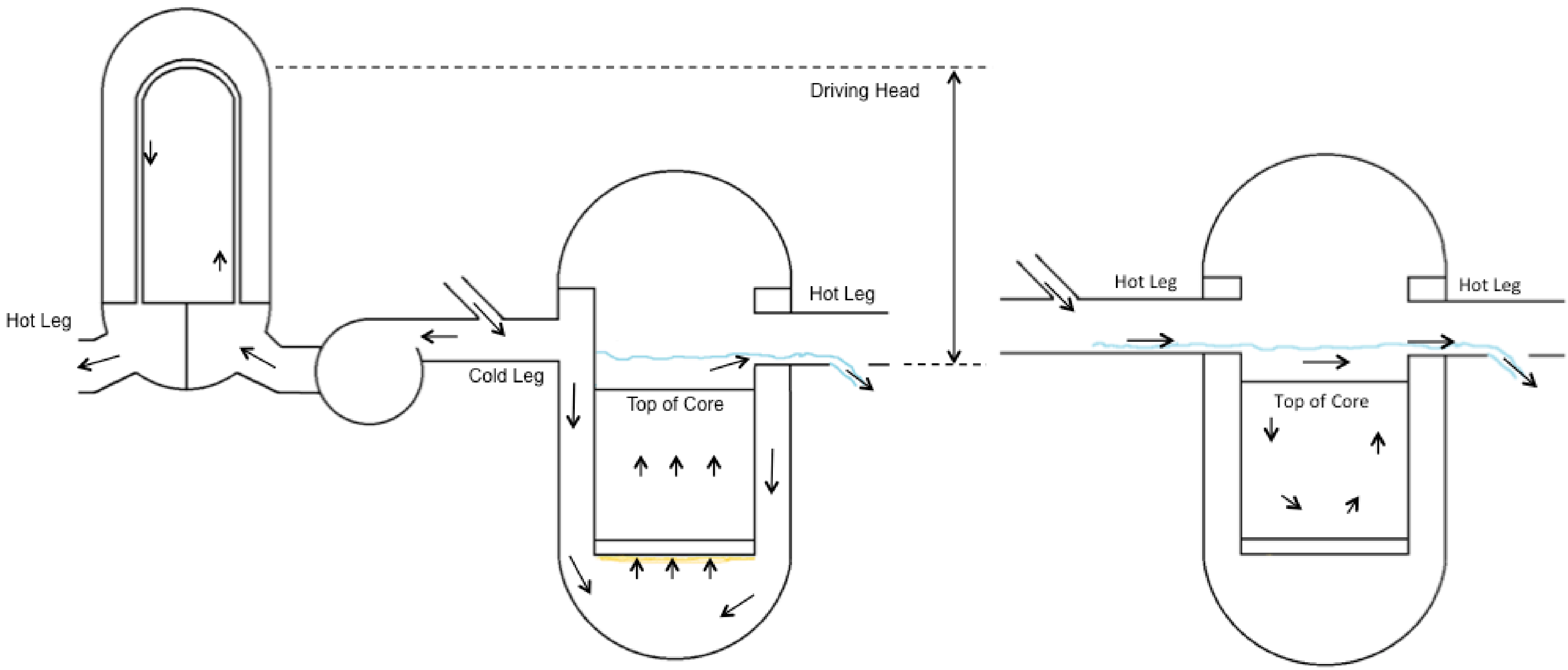
Plan for Addressing Core Blockage

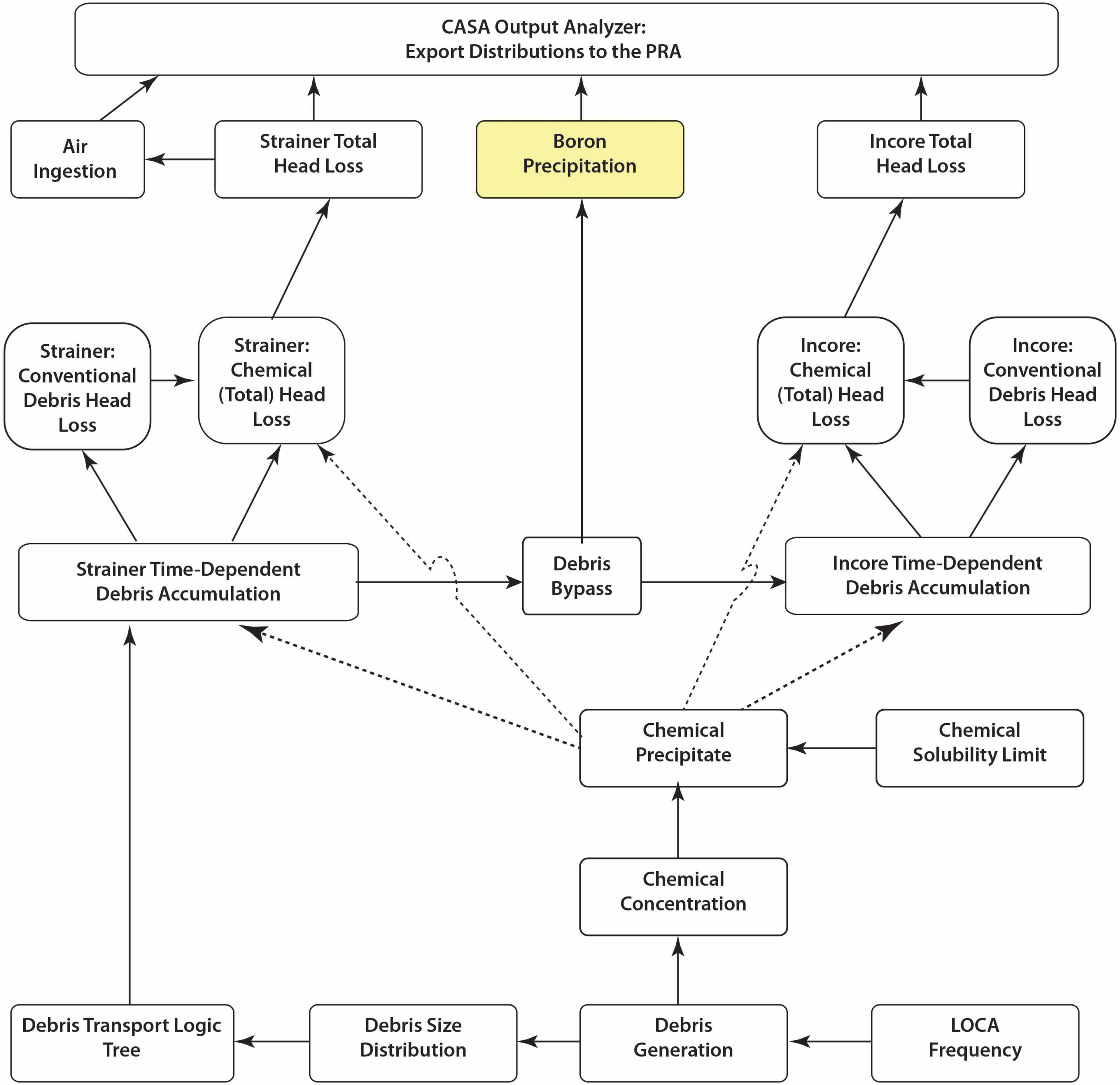
- Perform initial evaluation of expected incore conditions for range of break sizes and break locations
- Use RELAP5 to simulate full blockage at the bottom of the core to identify scenarios that would not lead to core damage
- For scenarios where full core blockage could lead to core damage:
  - Use results of time-dependent transport analysis, bypass testing, and chemical effects testing to determine the time dependent debris loads in the core
  - Use RELAP5 simulations to define the driving head and required core flow for scenarios of concern
  - Develop analytical and/or test methods to determine incore head loss
  - Compare head loss (realistic blockage) at required core flow to available driving head to find the scenarios (if any) that lead to core damage

Large Cold Leg Break



Large Hot Leg Break

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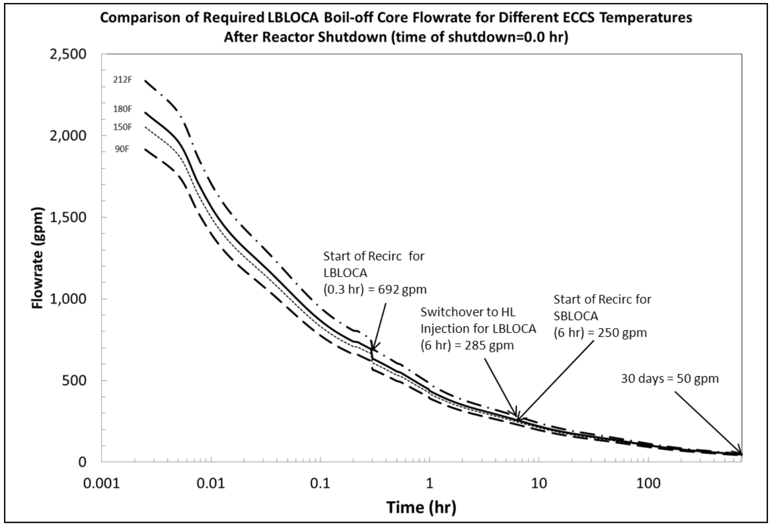
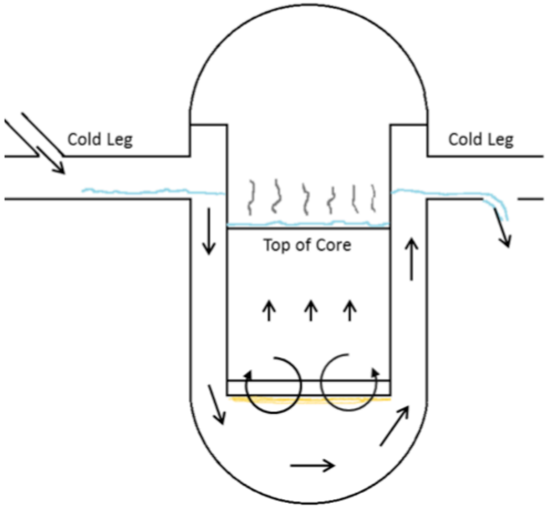
Incore: Conventional Debris Head Loss
Strainer Time-Dependent Debris Accumulation
Debris Bypass
Incore Time-Dependent Debris Accumulation
Chemical Precipitate
Chemical Solubility Limit
Chemical Concentration
Debris Transport Logic Tree
Debris Size Distribution
Debris Generation
LOCA Frequency

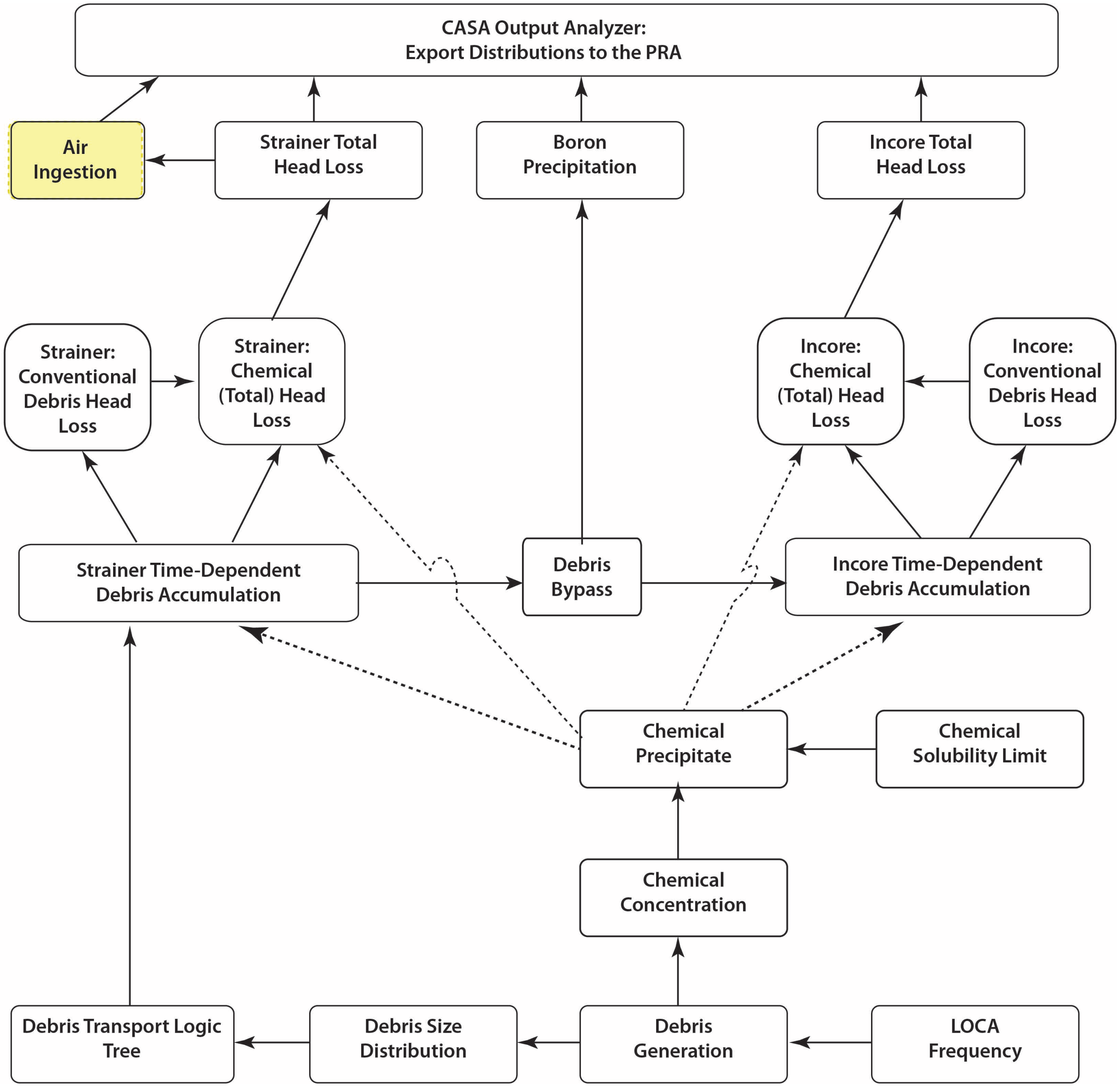
Boron Precipitation

- Boron precipitation is primarily a concern for large cold leg breaks during cold leg injection
- Boiloff rate is approximately 692 gpm at the start of recirculation for a large break giving a flow split of 5% to core
- Low debris loads will allow mixing with lower plenum
- STP has combined hot and cold leg injection following hot leg switchover at ~5.5 hours
- Further evaluation will be done this year



Injection flow for LBLOCA (3 train operation) is 13,260 gpm




CASA Output Analyzer:
Export Distributions to the PRA
Air
Ingestion
Strainer Total
Head Loss
Boron
Precipitation
Incore Total
Head Loss
Strainer:
Conventional
Debris Head
Loss
Strainer:
Chemical
(Total) Head
Loss
Incore:
Chemical
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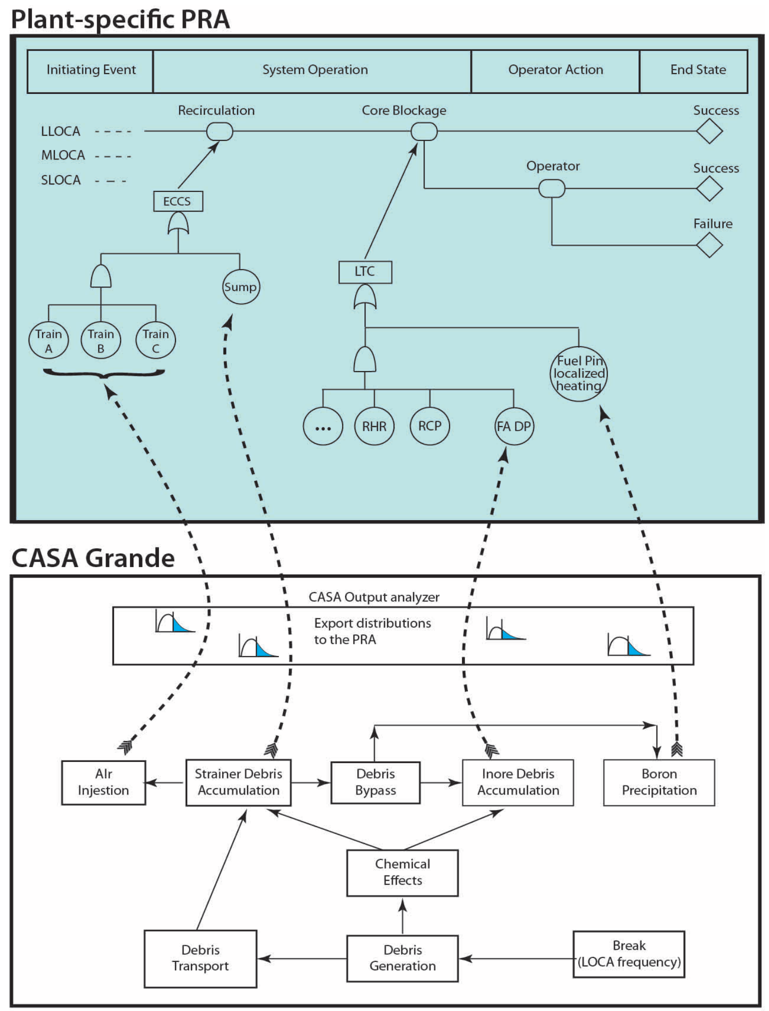
Debris Accumulation
Chemical
Precipitate
Chemical
Solubility Limit
Chemical
Concentration
Debris Transport Logic
Tree
Debris Size
Distribution
Debris
Generation
LOCA
Frequency

Air Ingestion

- No vortex formation based on STP strainer design
- Pressure drop across strainer may release air bubbles from solution
  - Assume dissolved gas in containment pool is at equilibrium conditions based on Henry's Law (function of containment pressure and temperature)
  - Calculate quantity of air released based on difference in pressure downstream of the strainer and the pool surface (if strainer head loss is greater than strainer submergence, air will be released)
- Compare void fraction at pump inlet to pump acceptance criteria (2%)
- Adjust pump NPSH margin as described in RG 1.82 Rev 4

Probabilistic Risk Assessment



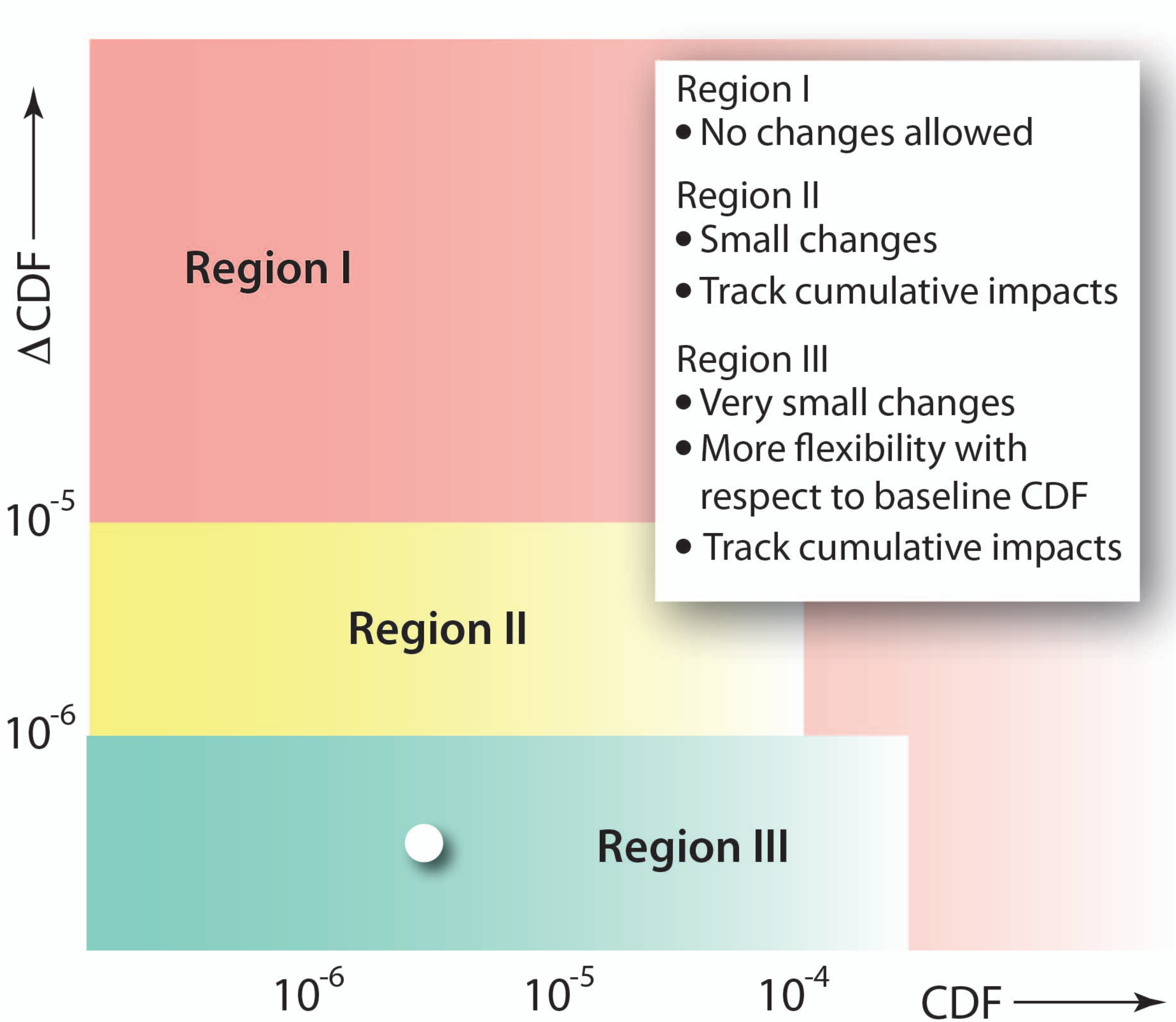
Acronyms

ECCS -Emergency Core Cooling System
FA DP - Fuel Assembly Differential Pressure
LLOCA - Large LOCA
LOCA - Loss of Coolant Accident
LTC - Long Term Cooling
MLOCA - Medium LOCA
RCP - Reactor Coolant Pump
RHR - Residual Heat Removal
SLOCA - Small LOCA

Probabilistic Risk Assessment

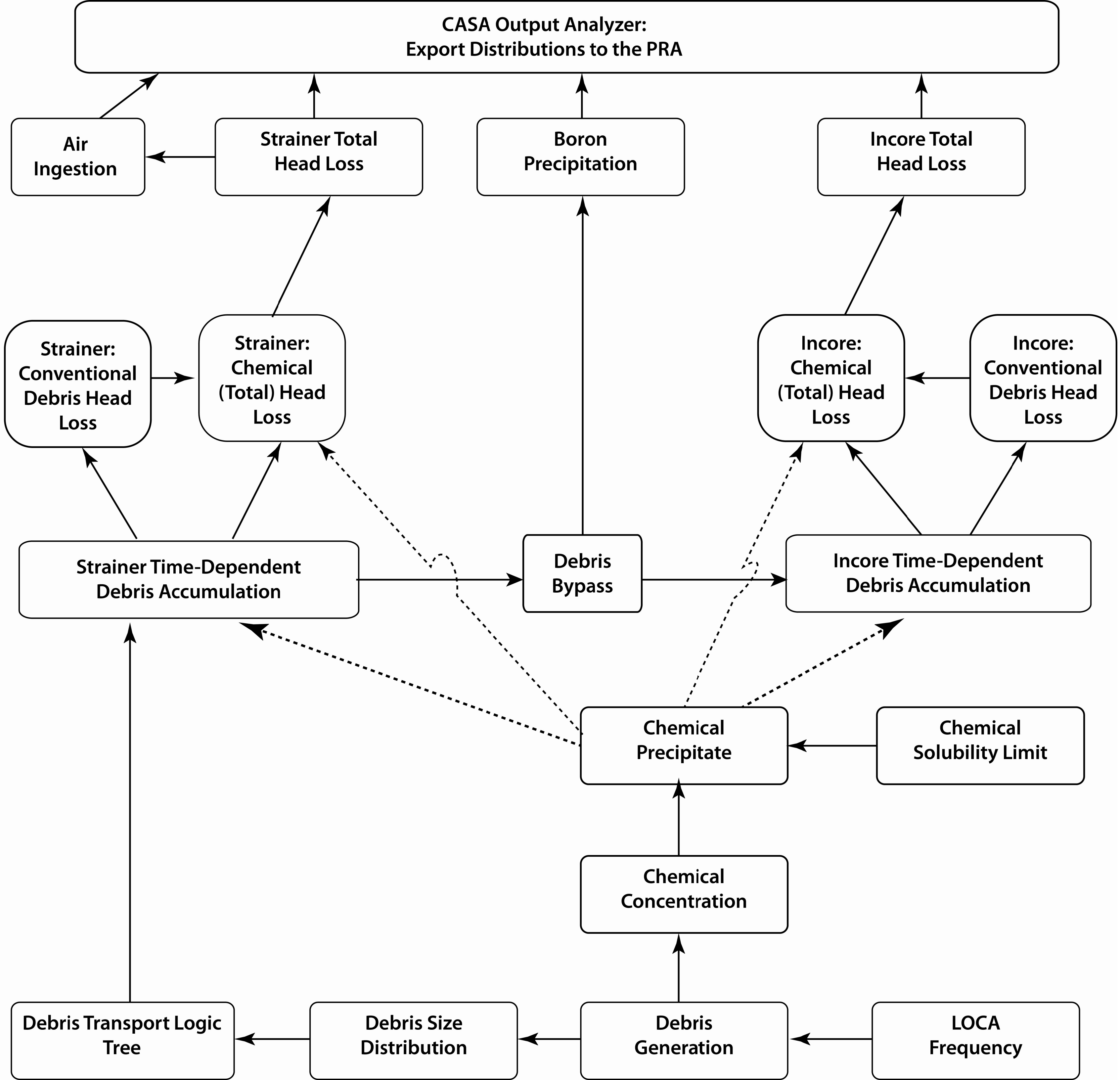
- Initial quantification provided mean ECCS failure probability (and std dev) for L, M, S LOCA categories under conditions 1, 2, and 3 trains fully operable
- ΔCDF and ΔLERF will be calculated by comparing "as built" plant to "ideal" plant
  - ΔCDF will be bounded by sum of frequencies of CDF sequences involving loss of NPSH, fuel channel blockage and boron precipitation
  - ΔLERF will be bounded by sum of frequencies of LERF sequences involving loss of NPSH, fuel channel blockage and boron precipitation
- Contributions due to individual phenomena will be separately identified
- Contributions from risk-important break locations will be identified
- Importance measures (e.g., FV, RRW, RAW, Birnbaum) will be identified for phenomena of interest

PRA, Illustration of Results CY2011



Status

- STP is the industry pilot for a risk-informed closure program
- There is periodic communication with the NRC staff on status and methods proposed
- STP understands the NRC desire to close out GSI-191 and is working to an aggressive closure schedule
- A major milestone was reached in calendar year 2011, an initial risk-informed quantification
- Plan to submit a license amendment request last quarter of 2012



CASA Output Analyzer:
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Debris Transport Logic Tree
Debris Size Distribution
Debris Generation
LOCA Frequency

South Texas Project Containment Accident Stochastic Analysis - CASA Grande -

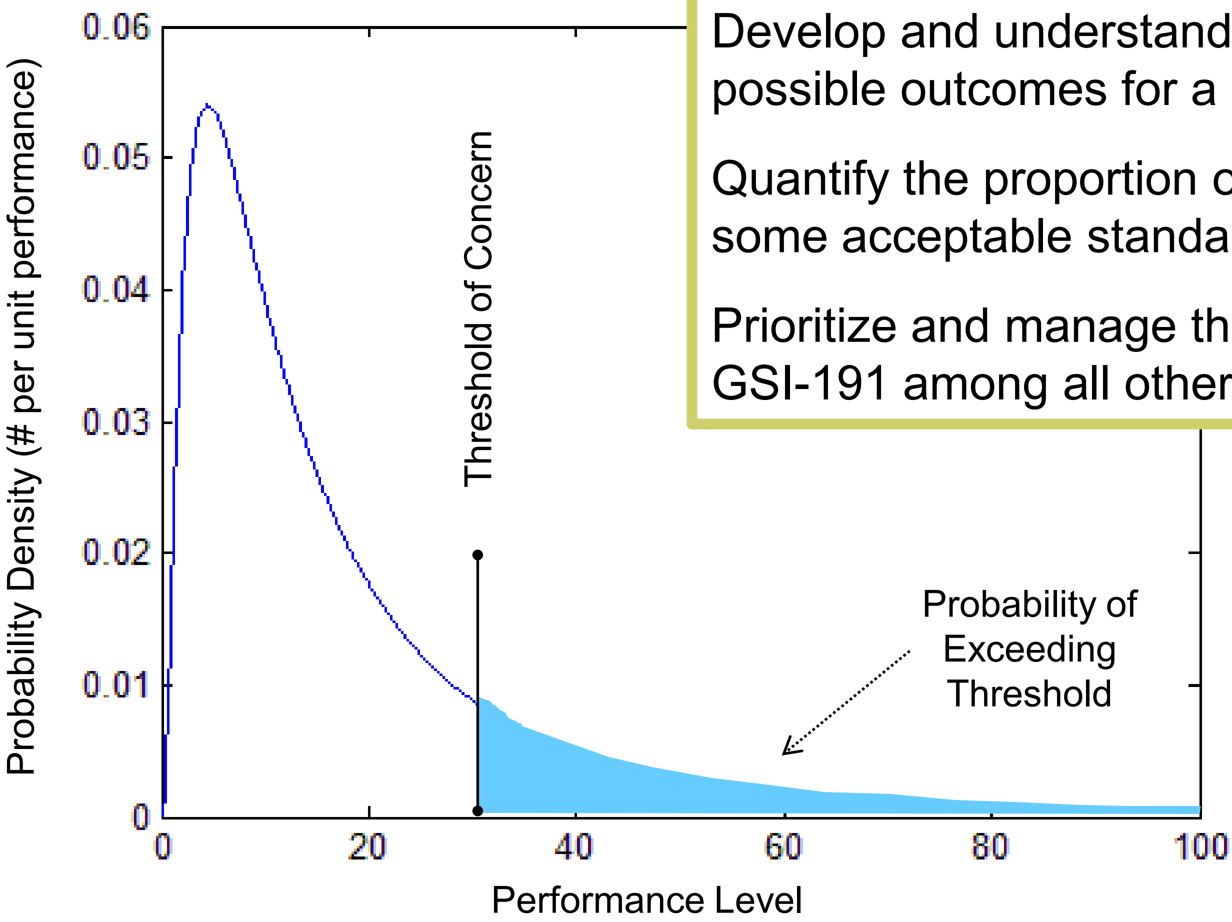
Bruce Letellier
Los Alamos National Laboratory
Los Alamos, NM

ACRS Thermal Hydraulics Subcommittee
Rockville, MD
Wednesday, May 9, 2012

Communication Goals

- **Containment Accident Stochastic Analysis (CASA)**
  - Objectives
    - Automate scenario evaluation to enable ensemble probability est.
    - Quantify probability of ECCS availability for plant PRA
    - Support risk mitigation strategies through sensitivity studies
  - Utility / Flexibility
  - Implementation
    - Physical Approximation
    - Uncertainty Quantification / Propagation
    - Failure Integration
- **Overview**
  - Emphasize present status of work in progress
  - Collect feedback on development questions/concerns

Risk Assessment Philosophy



Develop and understand the entire distribution of possible outcomes for a performance metric

Quantify the proportion of scenarios that exceed some acceptable standard (failure)

Prioritize and manage the risk of failure posed by GSI-191 among all other risk contributors

CASA Grande Objectives

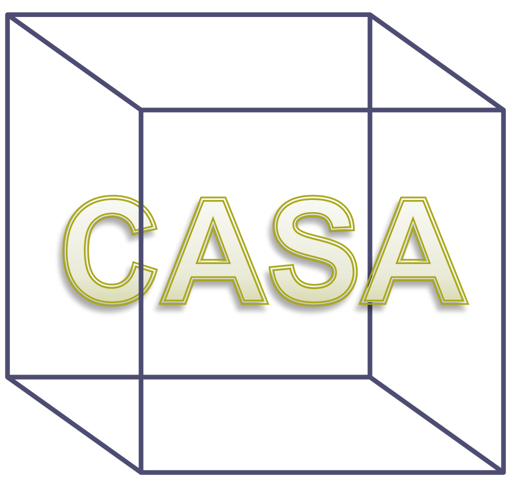
- **Propagate uncertainty in physical parameters from break initiation to potential core damage precursors (1) strainer head loss, (2) boron precipitation, (3) core blockage, (4) air ingestions**
- **Fold uncertainties into plant performance metrics to support Risk Based Decision making**
  - Diagnostic platform for parameter studies, research prioritization, sensitivity analysis, comparison of physical approximations
  - Risk Mitigation and Defense in Depth
- **Properly weight the relative frequency of many thousands of accident sequences**
  - Statistically sample and combine parameter variations (both physical and decisional) in unbiased distributions of possible outcome
- **Introduce relative time-dependence of plant response and debris impact**
- **Populate PRA branch fractions for S,M,L sump availability**
- **Extensibility for any random variables, alternative plant analyses**

CASA Evaluates Numerous Scenarios

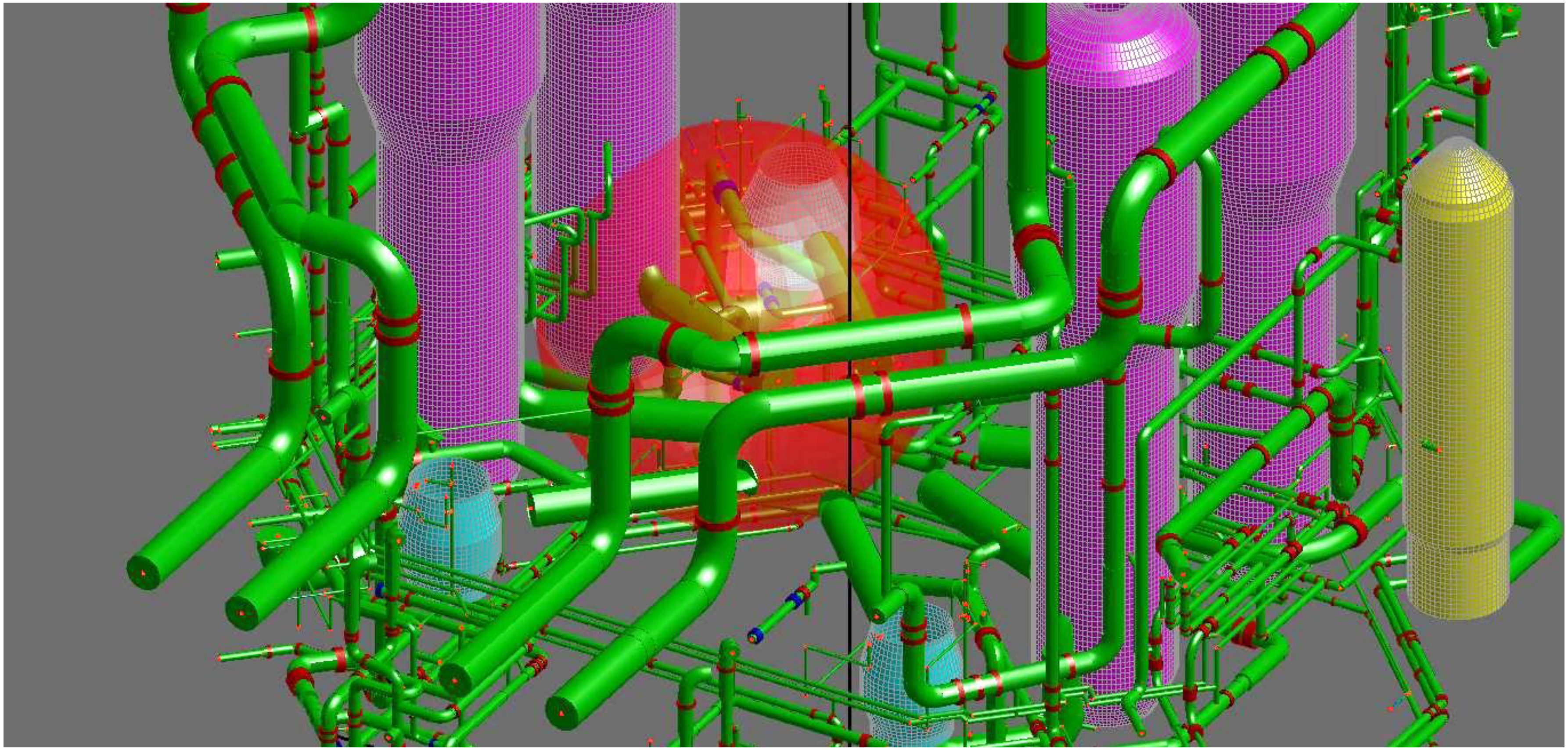
- **Normal hand calculations trace one accident scenario from:**
  - break location => debris generation => debris transport => debris accumulation (ΔP) => debris bypass (core blockage)
- **CASA automates calculation of performance impacts to enable statistical evaluation of 1000s of accident scenarios**
  - Postulates multiple breaks at every weld (including DEGB)
- **Nonuniform LHS sampling**
  - Randomly combines unique parameter values for each scenario
  - Prevents biasing of final distributions
  - Ensures inclusion of DEGB endstate
- **Replicate batches used to track convergent sampling density**

"Glass Box" Development Philosophy

- Same physical parameters and assumptions as hand calcs
- No embedded simulation or high-fidelity physics
- Input-driven analysis parameters
- Assembles GSI-191 phenomenology for SME scrutiny outside of PRA



CAD Data Imported for ZOI Calculations



CASA Random Variable Definition

- **Any scenario parameter *can* be treated as random**
- **Explicit correlations**
  - Physical parameters on LOCA size
  - Break location/freq/size
- **Pump trip times sampled to create randomized event sequences**
- **Any parameter treated as a random variable is defined by:**
  1. Mean
  2. Standard Deviation
  3. Lower Limit
  4. Upper Limit
  5. 2-parameter family
  6. Direction of Conservatism
  7. Logarithmic sample base

CASA Sampling Steps (1)

- **Point of Origin**
  - Pick weld class (relative freq between weld classes)
  - Pick spatial location (equal prob among all members in CAD)
  - Pick break size (relative freq within the weld class)
  - Pick azimuthal jet direction perpendicular to pipe run (uniform)
    - Hemispheres for breaks < pipe diam, Spheres for DEGB
  - Pick damage radius for each target type (user dist)
  - Pick Large and Fine fractions (user dist, complements, S,M,L dep.)
- **Use these values to calculate debris volume for one scenario (repeat to generate debris distribution)**

CASA Sampling Steps (2)

- **Debris Transport (follow Large and Fines separately)**
  - Pick washdown fraction (user dist, Lrge or Fine, LOCA dep., could easily be elevation dependent)
  - Pick Large fiber erosion factor (user dist)
  - Set fillup transport fractions to recirc sumps and dead volumes (currently user specified constants)
- **EOP progression**
  - Pick time to recirc (user dist, LOCA dep.)
  - Pick time to spray off (user dist, LOCA dep.)
  - Pick time to train off (user dist, LOCA dep.)
  - Pick time to LPSI suction (user dist, LOCA dep.)

CASA Sampling Steps (3)

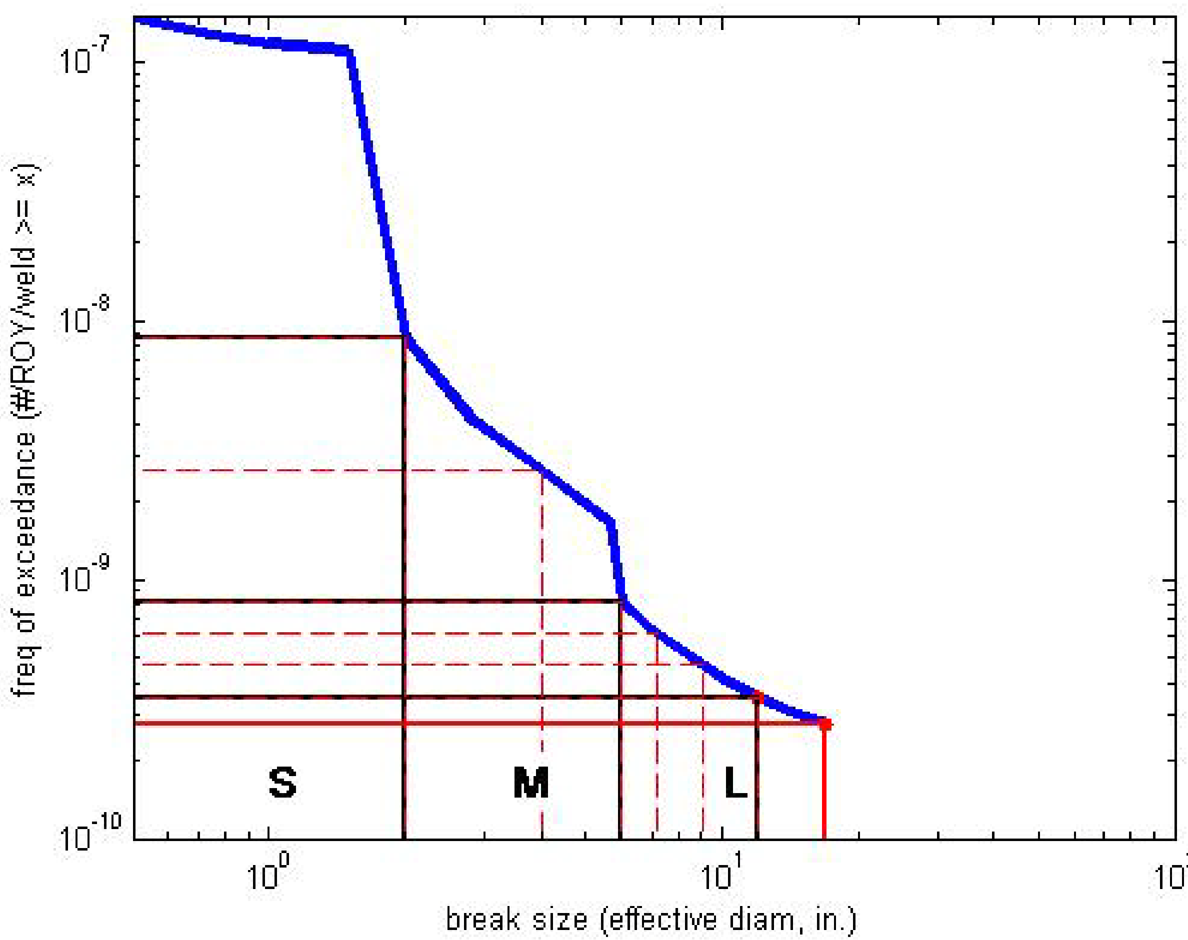
- **Performance Criteria**
  - Pick limiting NPSHmargin (user dist, LOCA dep.)
  - Pick debris bypass threshold (user dist, LOCA dep.)
- **Pool Conditions**
  - Pick nominal pool temperature (user dist, LOCA dep.)
  - Set pump flow rates (assumed runout)
  - Max # of trains operable (for PRA branches)
  - Set available volume

CASA Sampling Steps (4)

- **Additional Debris**
  - Pick misc debris area (user dist)
  - Set overlap ratio
  - Define latent debris fiber / particulate quantities
  - Define failed coatings type, quantity, time-dependent rate
  - Define chemical debris, quantity, time-dependent rate

Break Frequency Sampling Strategy

- Nonuniform probability sampling ensures that DEGB is included for *every* weld location.
- Example:
  - Applies to all welds of a particular type
  - 3 bins for LLOCA plus DEGB
  - 2 bins for MLOCA
  - 1 bin for SLOCA

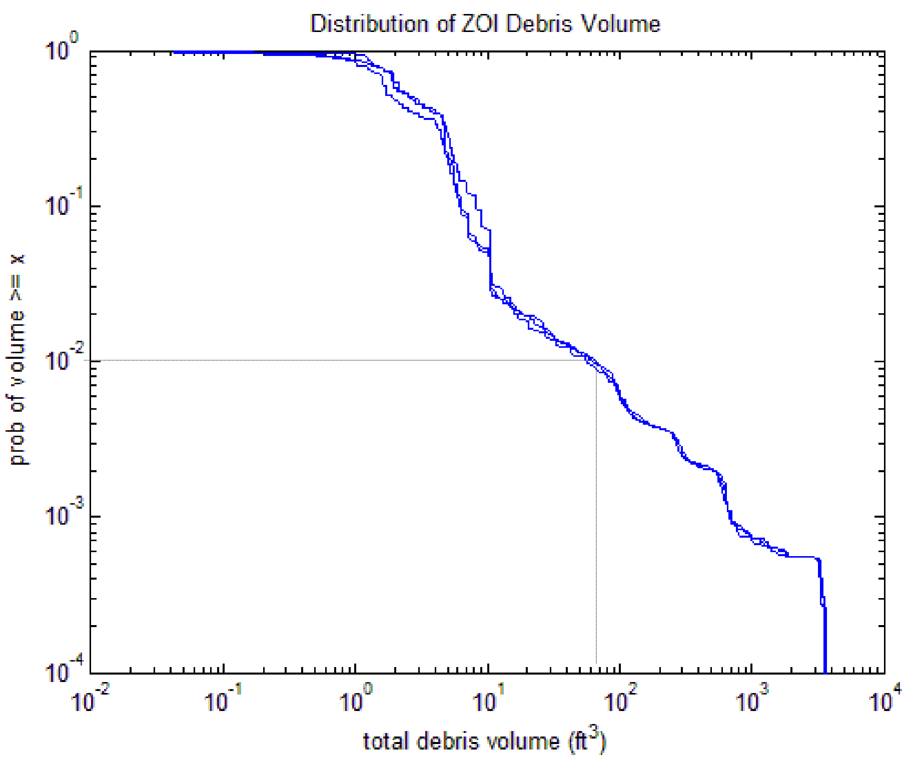


Preliminary Analysis Assumptions

- **Physical Approx**
  - No concrete
  - Approx SG and RCP geometry
  - Homogeneous mixed pool
  - No chemical products
  - 17 L/D spherical ZOI for NUKON
  - Paint debris introduced at 24 hours
  - Tight std dev on all variables *EXCEPT* bypass
  - GL values for latent debris coating failure, etc.
  - No containment overpressure

- **Random Variables**
  - Break size (~10 per weld)
  - ZOI L/D each insulation
  - Large vs Fine debris fractions
  - Washdown fractions
  - Fiber erosion
  - Pool temp
  - Misc debris (area equivalent)
  - EOP Conditions
    - Trecirc, Tspray_off, Ttrn_off,
  - Action Levels
    - Limiting NPSH_margin
    - Threshold bypass (g/FA)

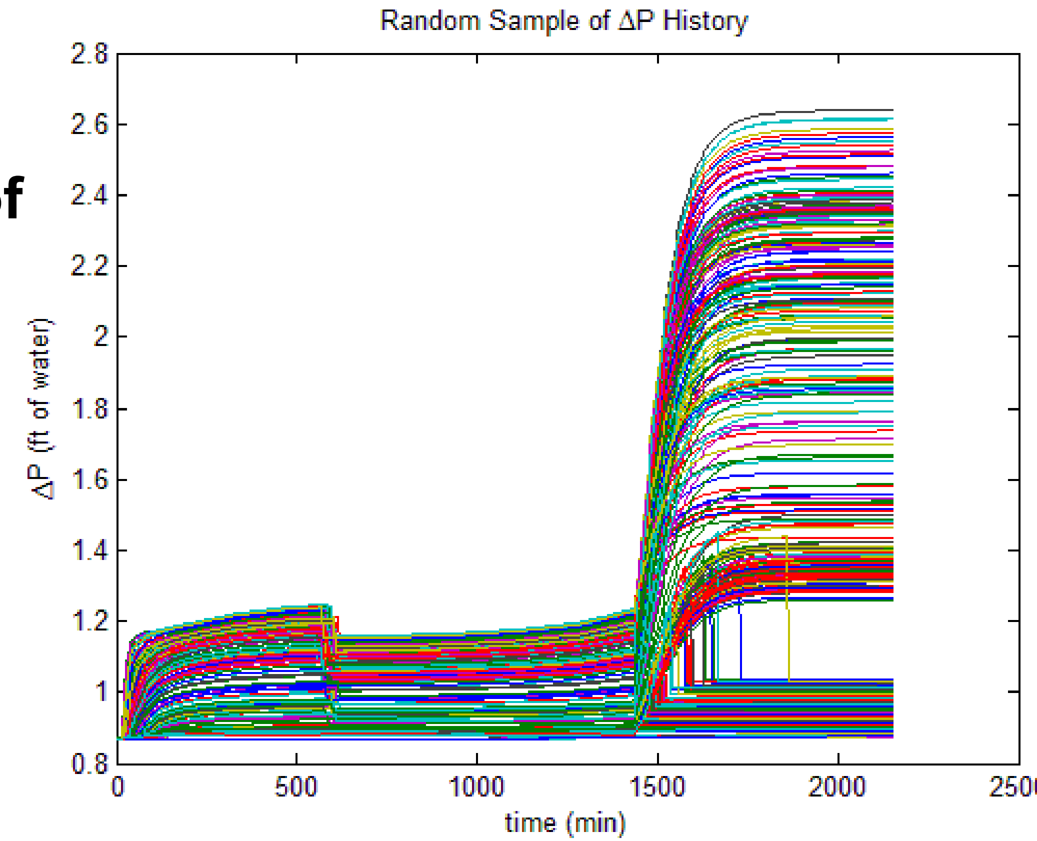
Distribution of Initial Debris Volume



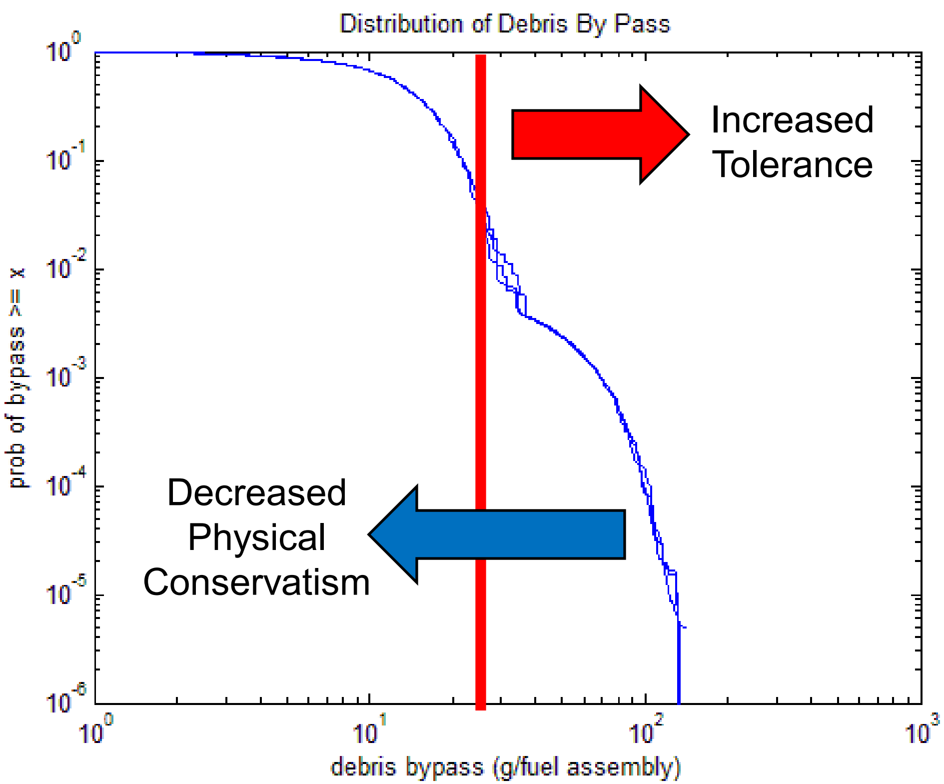
- Example: approximately 3000 breaks sampled per 3 replicates
- Relative initiating event frequencies included
- Under conservative assumptions, max debris event would exceed 3000 ft^3 of fiber
- 99% of cases are less than 70 ft^3 fiber

Prototypical Head Loss Histories

- Steep increase indicates arrival of coating debris
- No cases found in parameter space that exceed limiting NPSH of 18 ft H_2O
- Factor of 2x increase applied for chemicals when fiber bed is contiguous

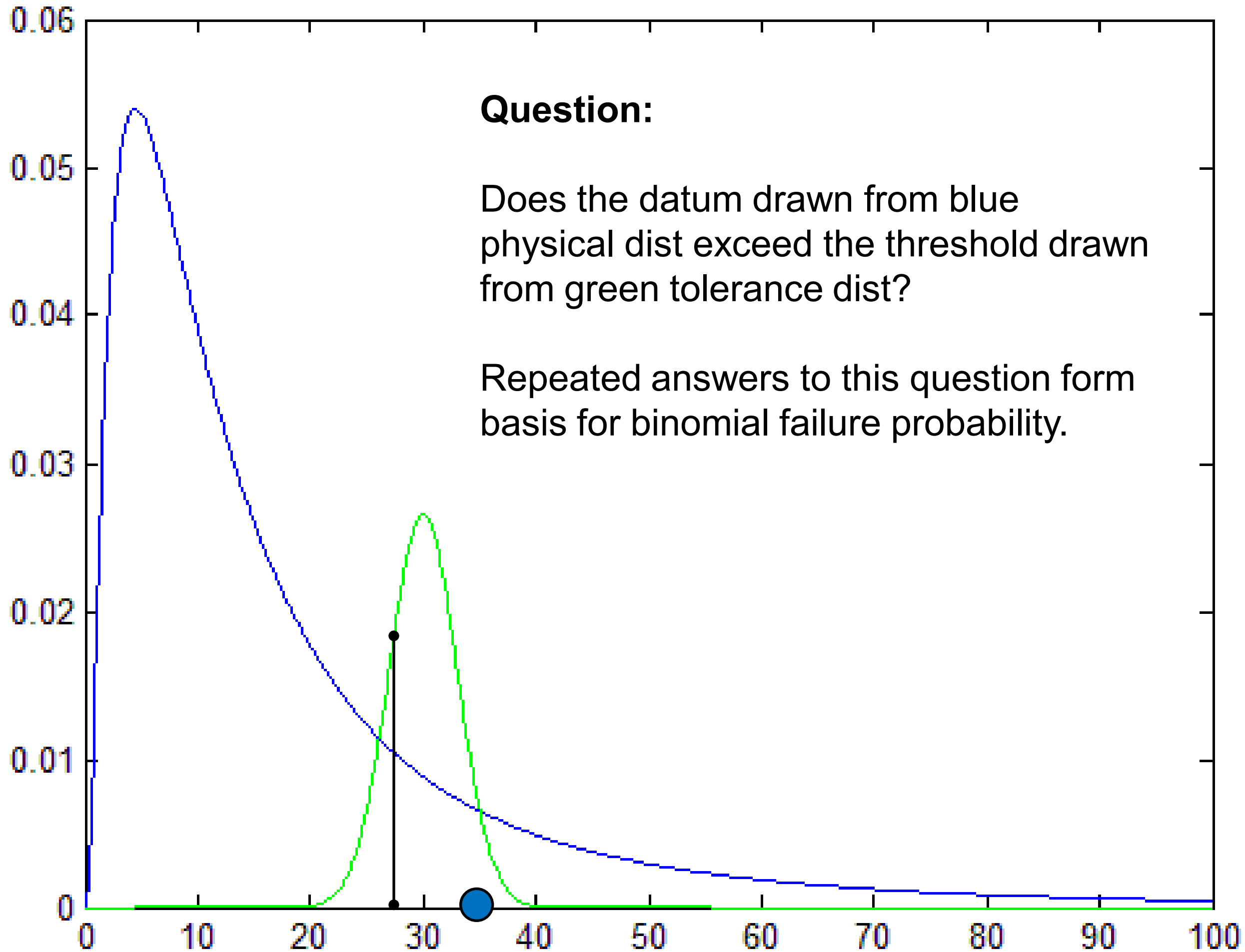


Distribution of Debris By Pass (g/FA)



- Bypass correlation only admits changes in flow rate
  - Two families w/spray (M and L) and without spray (SLOCA)
- Approximately 7% probability of exceeding 25 g/FA
  - Cannot read this result from fig
  - Comes from combining this physical dist with uncertainty on 25g threshold of concern
- Steep decline indicates rapid improvement with increased tolerance for bypass

Uncertainty in Decision Thresholds



Question:

Does the datum drawn from blue physical dist exceed the threshold drawn from green tolerance dist?

Repeated answers to this question form basis for binomial failure probability.

Initial PRA Interface

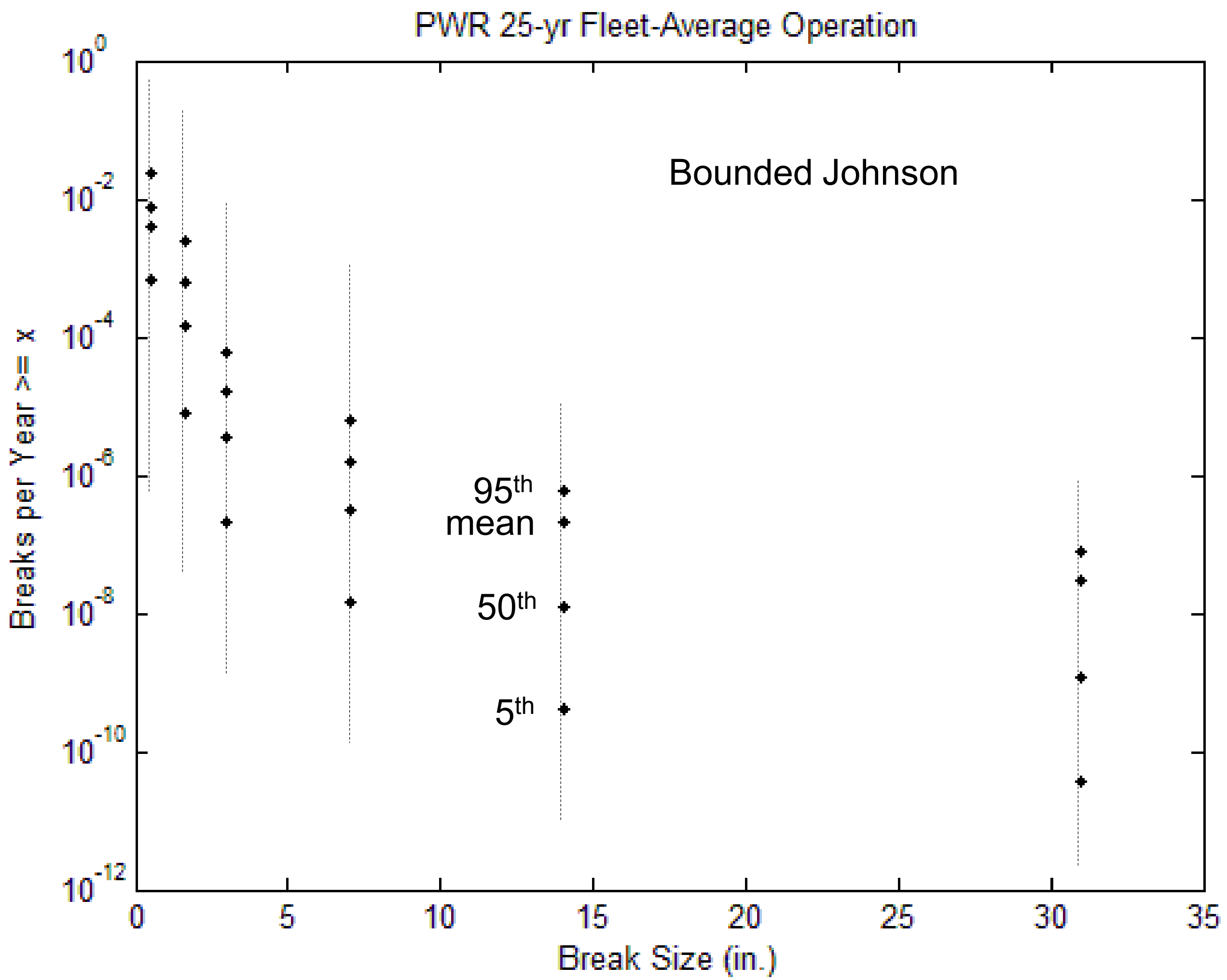
- Initial quantification provided mean ECCS failure probability (and std dev) for L, M, S LOCA categories under conditions 1, 2, and 3 trains fully operable
- Table for 75 g/FA sharp core blockage threshold
- Errors based on 25 replicates of ~2800 breaks
- Break frequencies based on arithmetic mean of NUREG 1829 break frequency envelope

Ntrain = 3				
overall	small	med	large	
3.5588e-013	0	1.7878e-002	2.4545e-001	mean
9.1785e-015	0	5.8282e-004	1.2996e-003	std dev
2.5791e+000	NA	3.2600e+000	5.2948e-001	std err (%)
Ntrain = 2				
overall	small	med	large	
1.1148e-013	0	4.8313e-003	1.1726e-001	mean
7.1540e-015	0	4.4504e-004	1.7875e-003	std dev
6.4173e+000	NA	9.2116e+000	1.5244e+000	std err (%)
Ntrain = 1				
overall	small	med	large	
3.5000e-016	0	0	1.1649e-003	mean
7.2124e-017	0	0	2.4005e-004	std dev
2.0607e+001	NA	NA	2.0607e+001	std err (%)

Year 2 Improvements

- **Variability on additional factors**
- **Concrete truncation of ZOI**
- **Time-dependent pool temp (MELCOR)**
- **Hot Leg / Cold Leg designation for sorting core blockage scenarios**
- **Containment transport fractions by elevation**
  - Easy to implement, not obvious how to quantify
- **Directed jet geometry**
  - Improved ANSI or CFD supported by test data
- **Chemical product calculator**

NUREG 1829 Break Frequency Illustration



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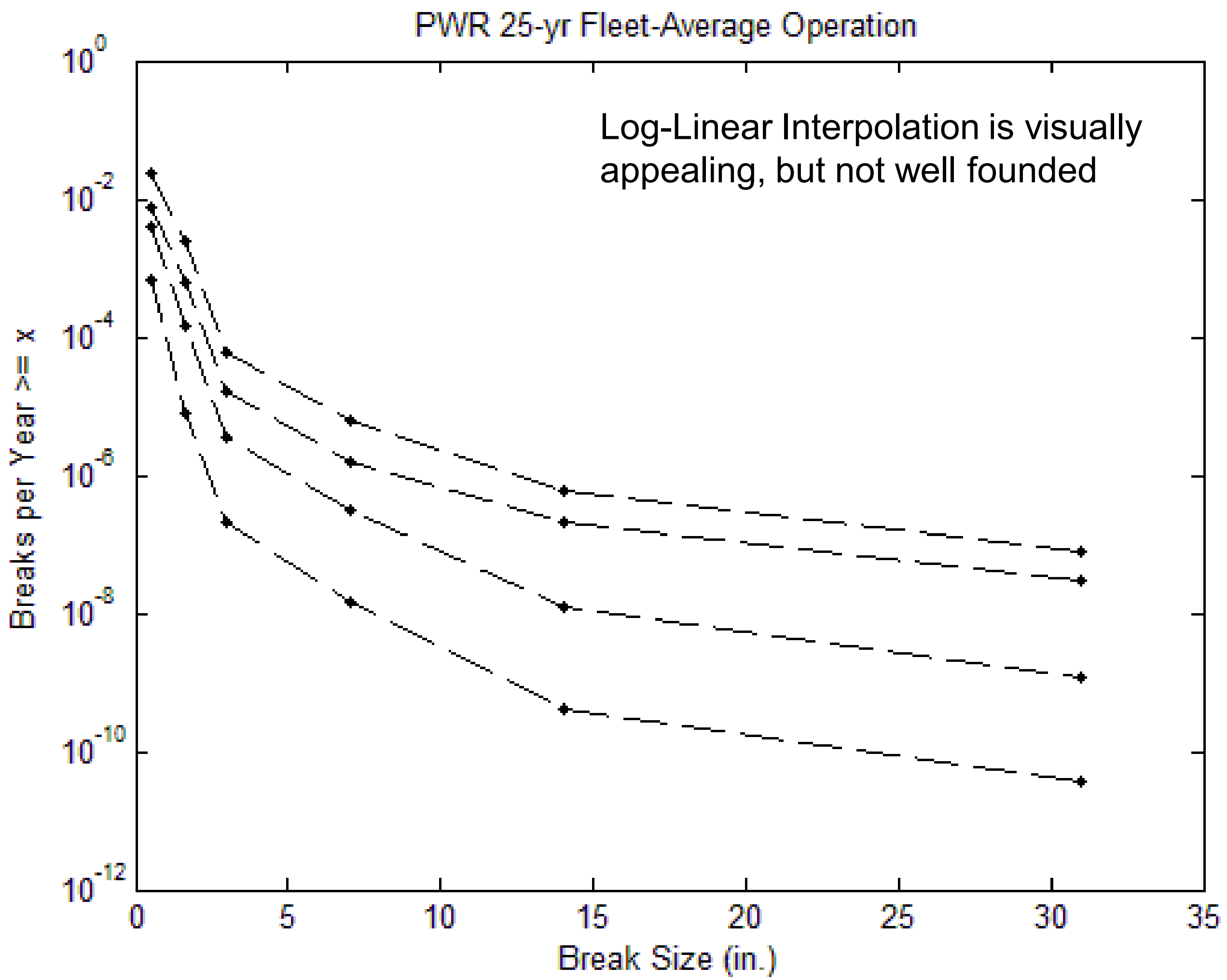
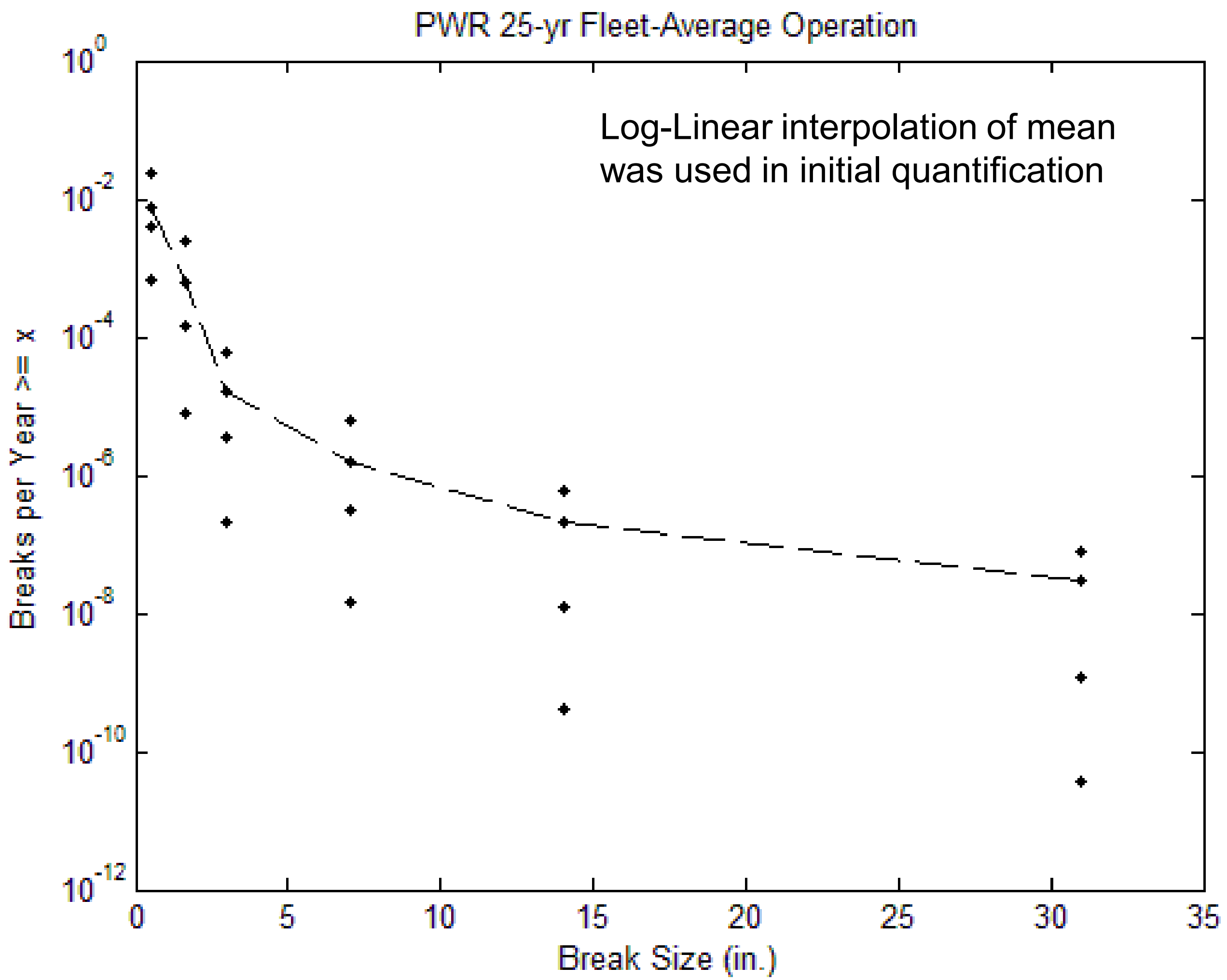


Los Alamos NATIONAL LABORATORY EST. 1943

Operated by Los Alamos National Security, LLC for the U.S. Department of Energy's NNSA

NUREG 1829 Break Frequency Illustration



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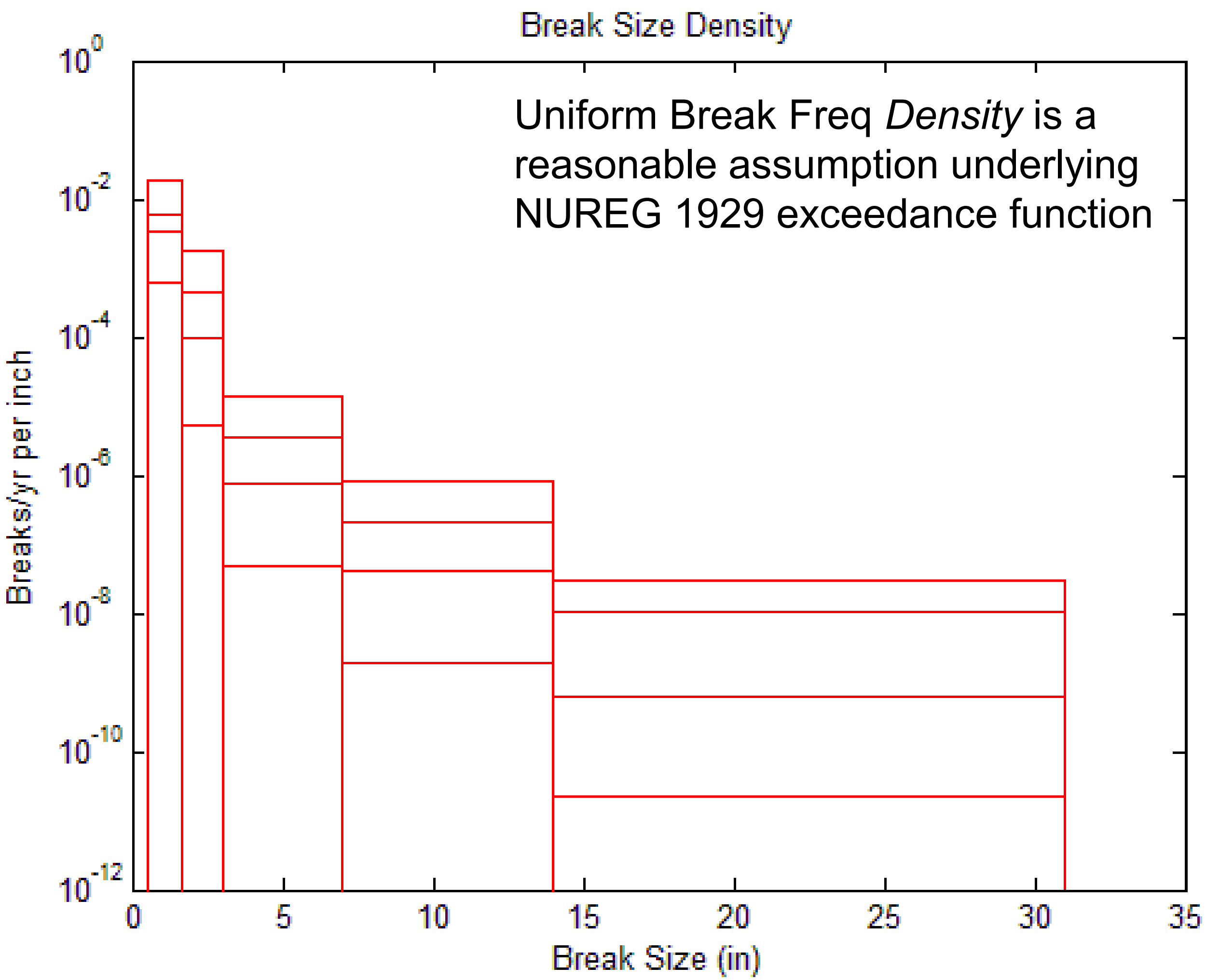

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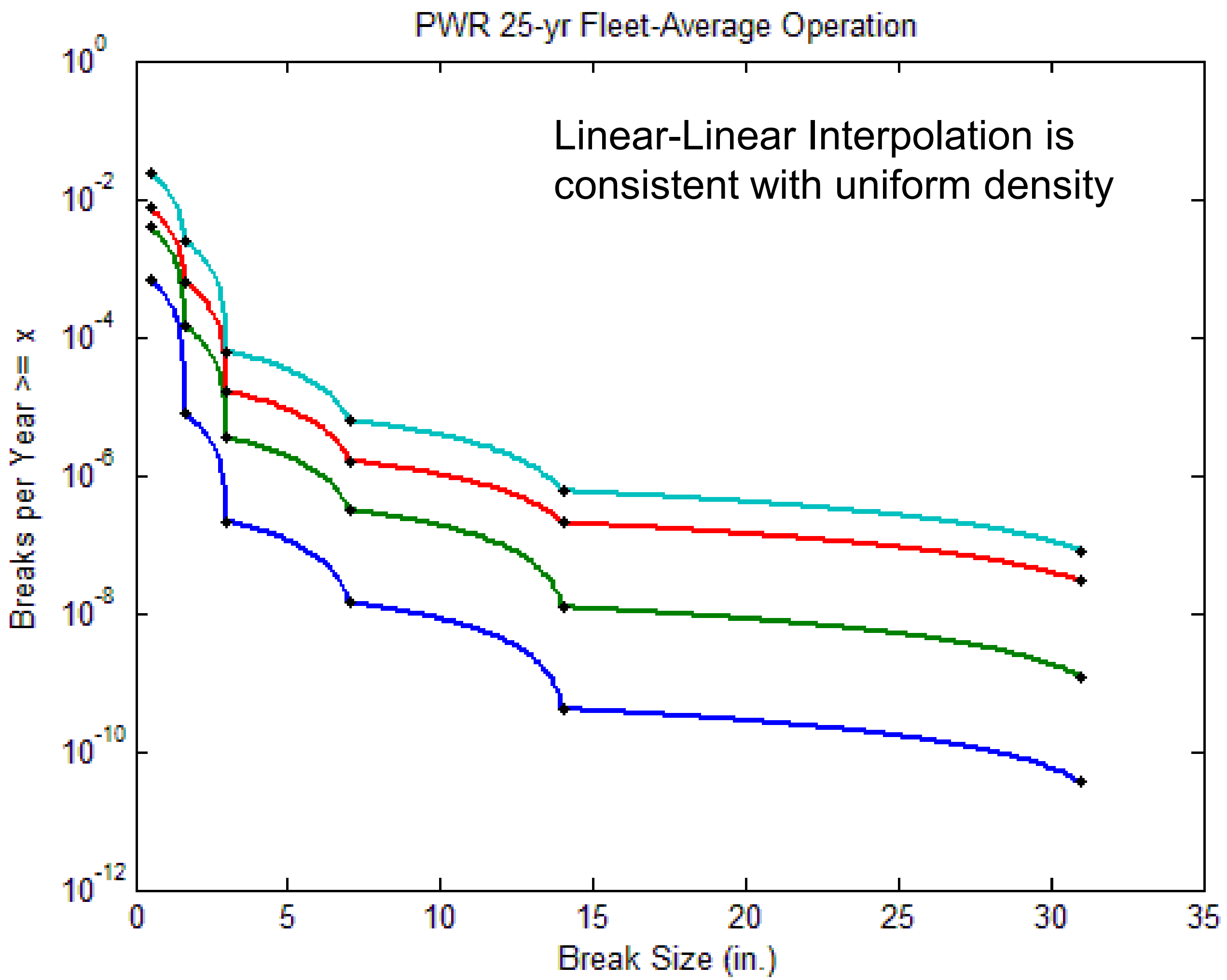
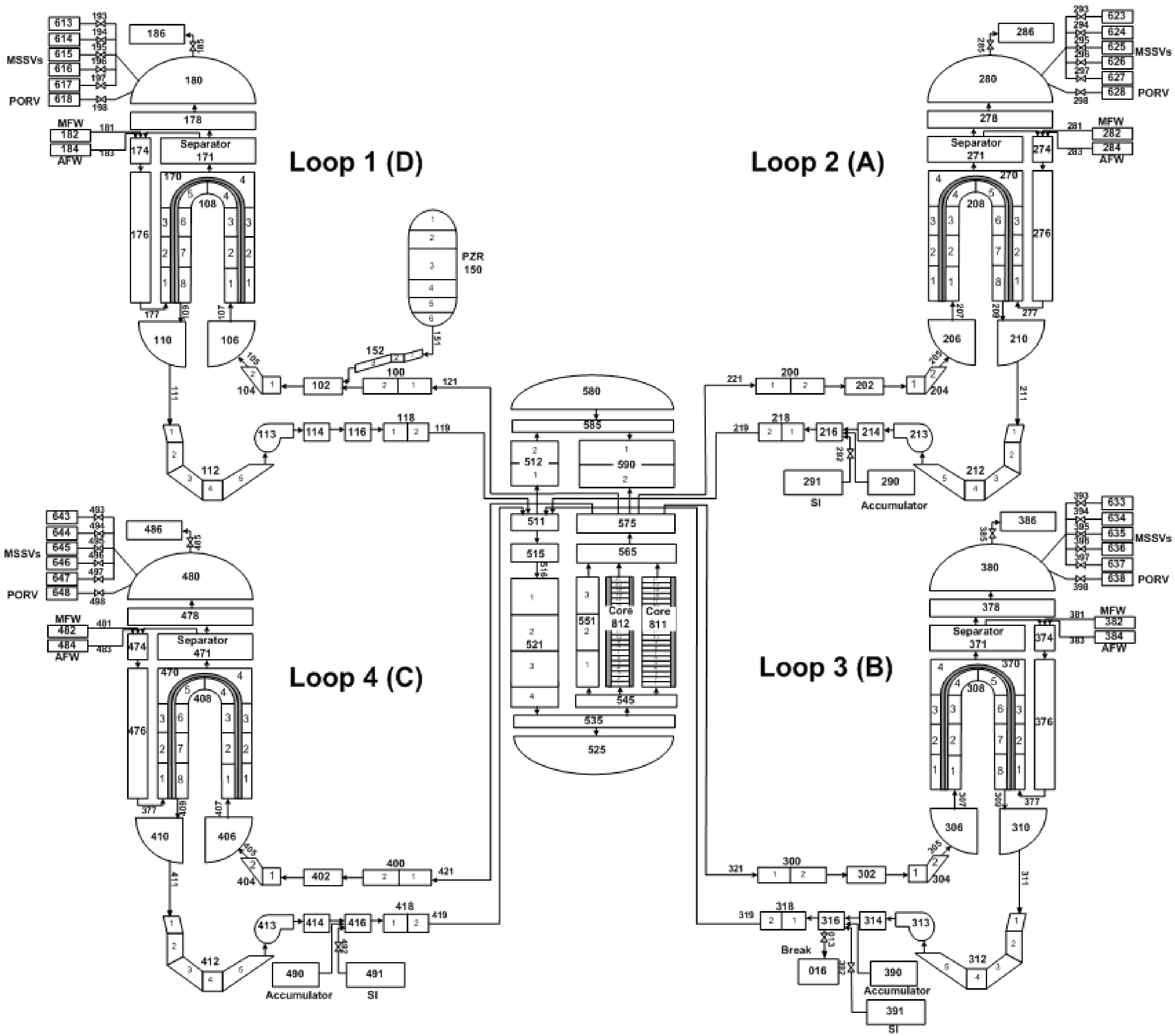
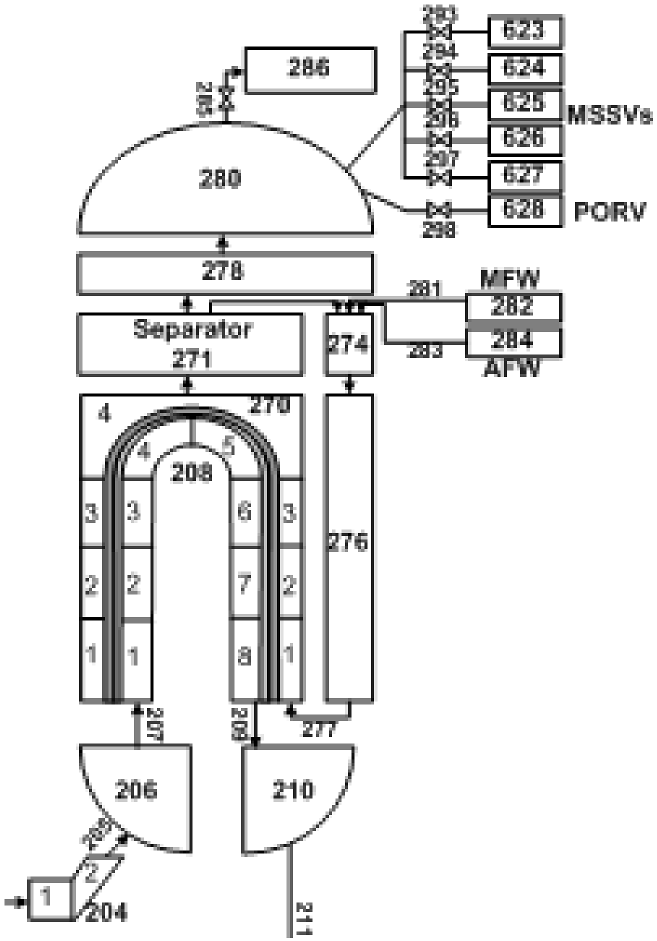


Thermal-Hydraulic Backup Slides

RELAP5-3D Full 1D Nodalization Diagram - Overview



RELAP5-3D Full 1D Nodalization Diagram – Steam Generators



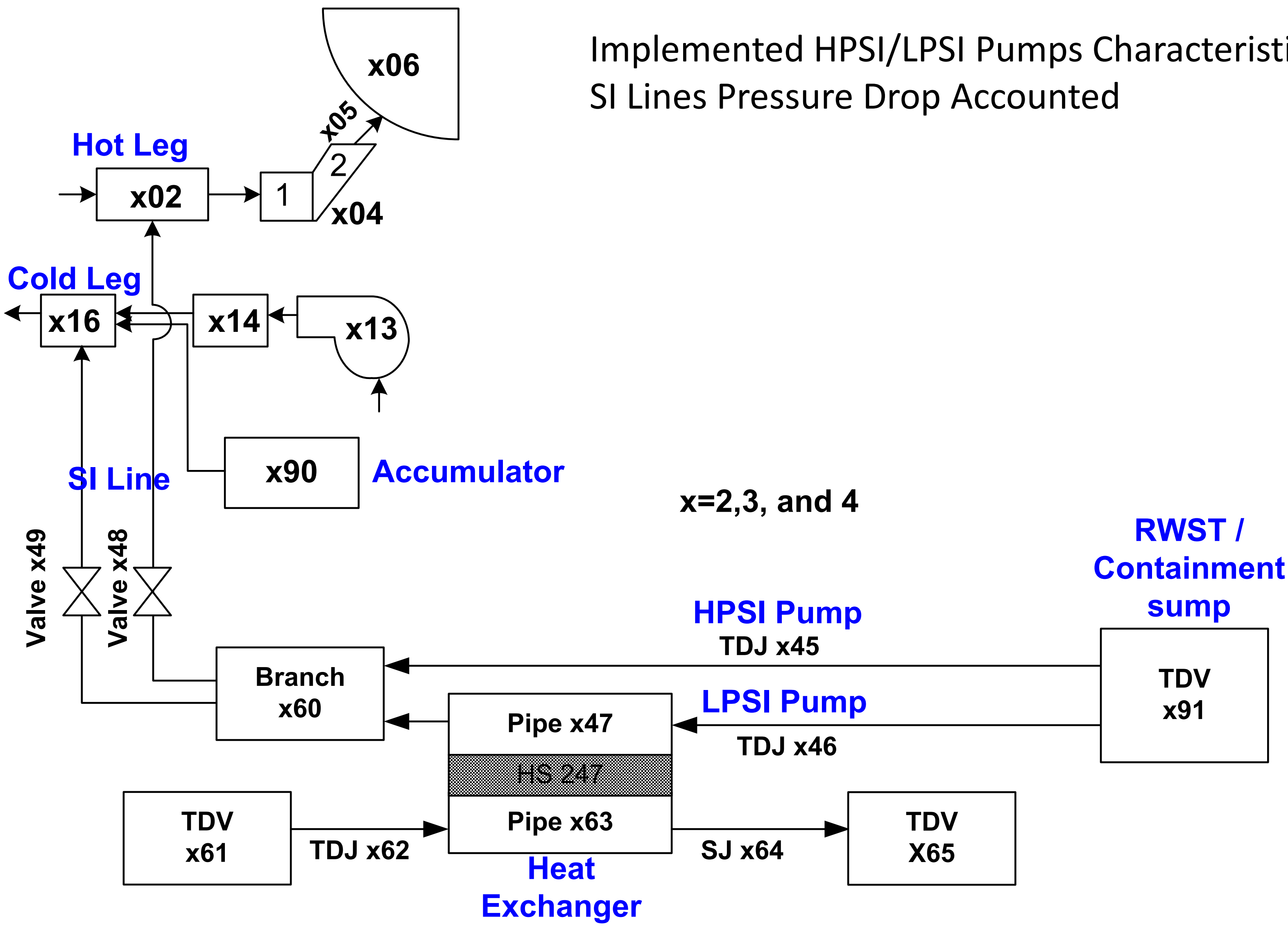
Countercurrent Flow Limiting (CCFL) Model (Wallis correlation)

- Reflux cooling mode
- Liquid holdup

RELAP5-3D Full 1D Nodalization Diagram – SI System

Implemented HPSI/LPSI Pumps Characteristics

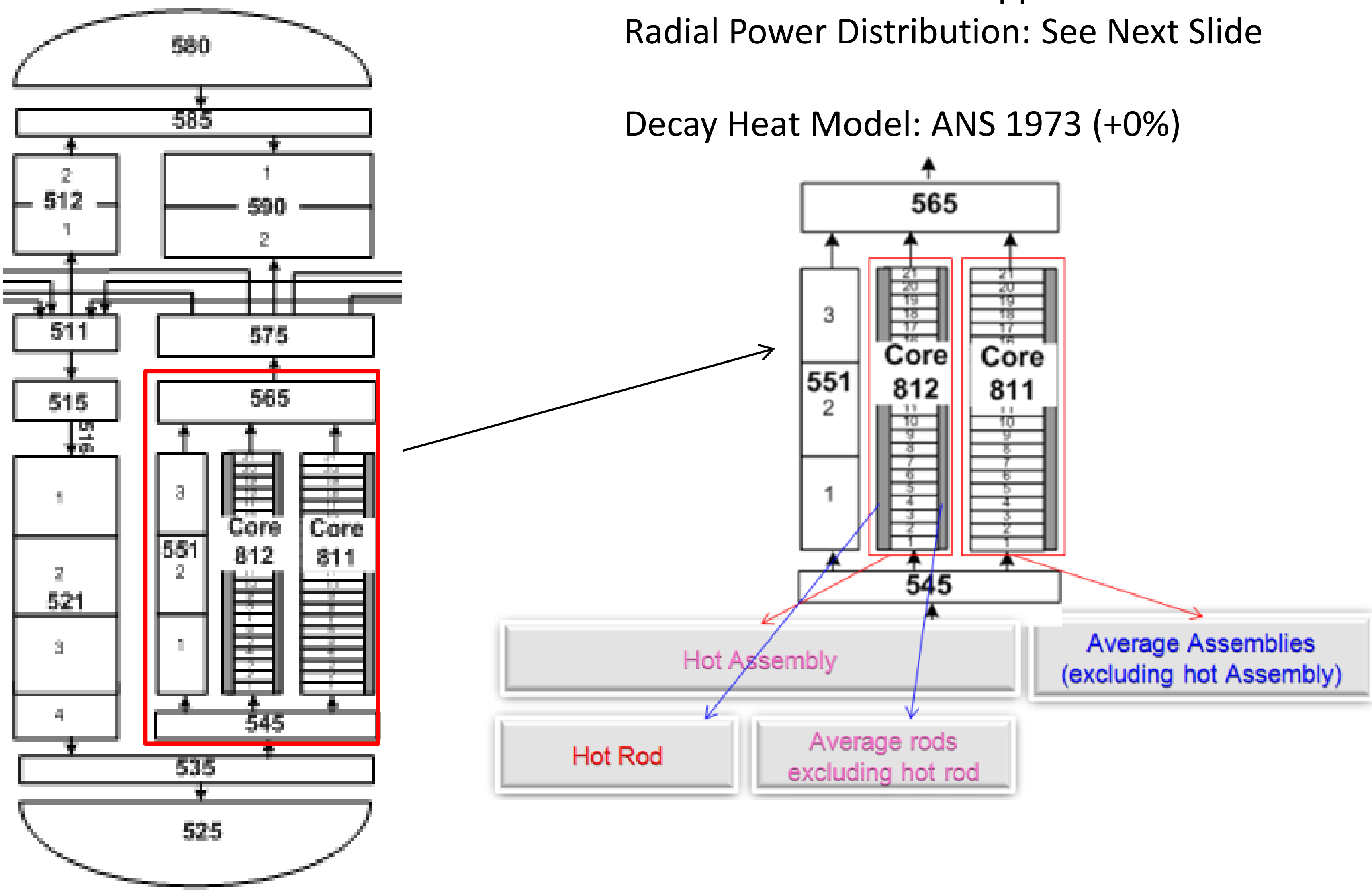
SI Lines Pressure Drop Accounted



RELAP5-3D Full 1D Nodalization Diagram – Vessel and Core

Axial Power Profile: Chopped Cosine
Radial Power Distribution: See Next Slide

Decay Heat Model: ANS 1973 (+0%)



Radial Power Distribution

Core Parameters

N_A	# of Assembly	193
$N_{fr/A}$	# of fuel rod per an assembly	264
F_{DH}	Hot channel enthalpy rise peaking factor	1.62
P_{HA}	Hot Assembly Peaking factor	1.443
γ	Power ratio deposited in the fuel	0.974

Normalized Power Distribution

$$1$$

$$\frac{P_{HA}}{N_A} \qquad \left(1-\frac{P_{HA}}{N_A}\right)$$

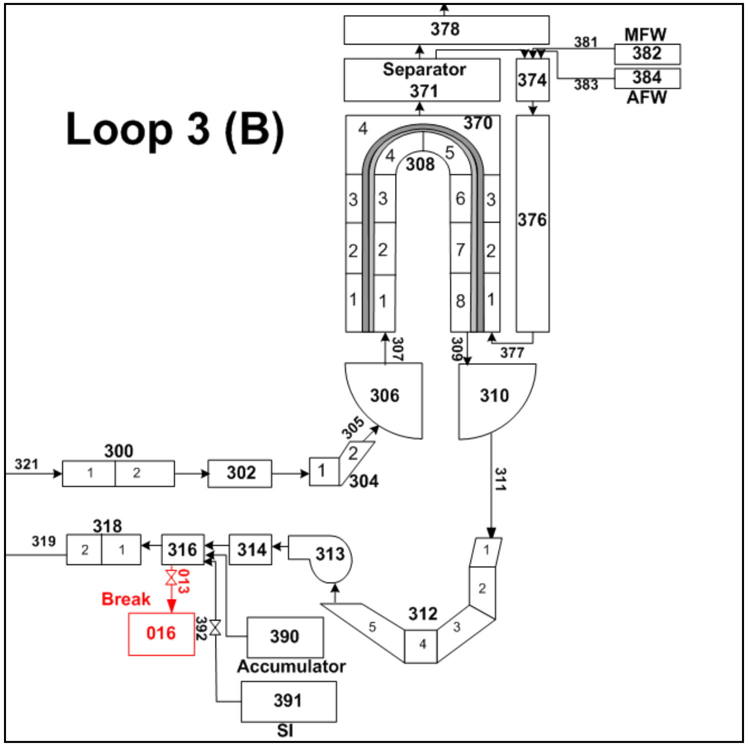
$$\frac{F_{DH}}{N_A N_{fr/A}} \qquad \frac{P_{HA}}{N_A}-\frac{F_{DH}}{N_A N_{fr/A}} \qquad \gamma\left(1-\frac{P_{HA}}{N_A}\right) \qquad (1-\gamma)\left(1-\frac{P_{HA}}{N_A}\right)$$

$$\gamma\frac{F_{DH}}{N_A N_{fr/A}} \qquad (1-\gamma)\frac{F_{DH}}{N_A N_{fr/A}} \qquad \gamma\left(\frac{P_{HA}}{N_A}-\frac{F_{DH}}{N_A N_{fr/A}}\right) \qquad (1-\gamma)\left(\frac{P_{HA}}{N_A}-\frac{F_{DH}}{N_A N_{fr/A}}\right)$$

Fuel	Moderator	Fuel	Moderator	Fuel	Moderator

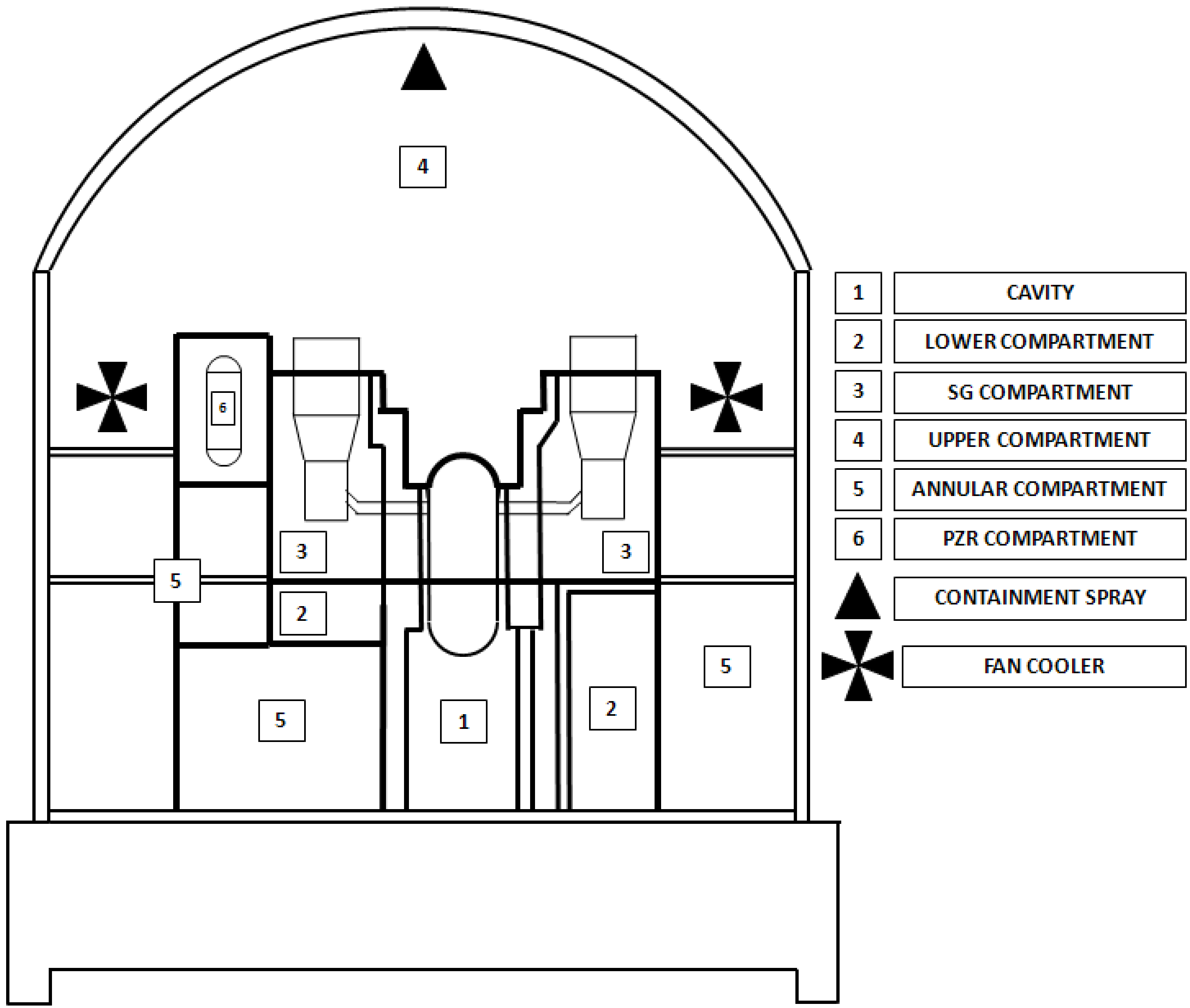
Hot Rod	Hot Channel (excluding hot rod)	Average Channel (excluding hot channel)

RELAP5-3D Break Modeling



- ✓ Break assumed to open Instantaneously (trip Valve)
- ✓ Vertical Stratification Model (Top, Lateral, Bottom)
  - To account for Entrainment
- ✓ Abrupt Area Change Enabled at the break Junction
  - To account for combined flow losses associated with the sharp-edged area reduction and expansion
- ✓ Ransom and Trapp Chocked Flow Model
  - Sensitivity on Discharge Coefficients (nominal = 1.0)

✓ Containment Pressure Response accounted in the Discharge TMDPVOL


4
6
3
3
5
2
2
5
1
5
1
CAVITY
2
LOWER COMPARTMENT
3
SG COMPARTMENT
4
UPPER COMPARTMENT
5
ANNULAR COMPARTMENT
6
PZR COMPARTMENT
CONTAINMENT SPRAY
FAN COOLER

- MELCOR nodalization constructed with reference to MAAP model
  - 6 control volumes (called compartments in MAAP)
  - 9 flow paths (called junctions in MAAP)

FL #	"FROM" CV	"TO" CV	ORIENTATION
1	CAVITY	SG COMP	HORIZONTAL
2	CAVITY	LOWER COMP	HORIZONTAL
3	LOWER COMP	ANNULAR COMP	HORIZONTAL
4	LOWER COMP	SG COMP	VERTICAL
5	SG COMP	UPPER COMP	VERTICAL
6	ANNULAR COMP	UPPER COMP	VERTICAL
7	CAVITY	LOWER COMP	HORIZONTAL
8	ANNULAR COMP	PZR COMP	VERTICAL
9	PZR COMP	UPPER COMP	HORIZONTAL

  - 49 heat structures (called distributed heat sinks in MAAP)
    - Various floors, ceilings, and walls for all compartments
    - Condensation surface area available
  - Engineered safety features actuating on containment pressure set-points
    - Containment Sprays
    - Fan Coolers

Head Loss Back-up Slides

Head Loss Calculations

- CASA Grande requires a correlation to determine head loss over the range of relevant conditions:
  - Debris loads
    - Fiber
    - Particulate
    - Microporous
    - Chemical
  - Flow rate
  - Temperature
  - NPSH margin

Porous Media Head Loss Correlations

- Porous media head loss correlations follows the classical porous media flow equations:

$$dP = [\ a*U + b*U^2]*dL$$

where:

a = coefficient for viscous term

b = coefficient for inertia term

- NUREG/CR-6224 correlation is a semi-theoretical correlation developed based on flat plate vertical loop head loss testing with
  - Nukon fiberglass fibers - nominally 7 micron diameter, and
  - BWR suppression pool sludge (iron oxide) – nominally 10 micron diameter

- NUREG/CR-6224 experimental data were performed at
  - fluid temperatures ranging from 60^oF to 125^oF,
  - debris bed thicknesses ranging from 0.125 in to 4 in and
  - approach velocities ranging from 0.15 ft/s to 1.5 ft/s

NUREG/CR-6224 Correlation

$$\Delta H = \Lambda \left[3.5\, S_V^2\, \alpha_m^{1.5} \left(1 + 57\, \alpha_m^3\right) \mu \mathrm{U} + 0.66 S_V \frac{\alpha_m}{(1-\alpha_m)} \rho \mathrm{U}^2 \right] \Delta L_m$$

Where:

- ΔH = head loss (ft-water)
- S_v = surface-to-volume ratio of the debris (ft^2/ft^3)
- μ = dynamic viscosity of water (lbm/ft/sec)
- U = fluid approach velocity (ft/sec)
- ρ = density of water (lbm/ft^3)
- α_m = mixed debris bed solidity (one minus the porosity)
- ΔL_m = actual mixed debris bed thickness (in)
- Λ = 4.1528×10^{-5} (ft-water/in)/($lbm/ft^2/sec^2$); conversion factor for English units

NUREG/CR-6224 Correlation

- The very low approach velocity at STP (~ 0.01 ft/sec) suggests that the head loss will be dominated by the viscous term.

- The viscous term of the NUREG/CR-6224 correlation is based on experimental work by C. N. Davies, “Proceedings of Institute of Mechanical Engineers,” (London), B1, p. 185, 1952.

- For STP conditions the NUREG/CR-6224 correlation could be simplified to:

$$\Delta H = \Lambda \left[3.5\, S_V^2\, \alpha_m^{1.5} \left(1 + 57\, \alpha_m^3 \right) \mu \mathrm{U} \right] \Delta L_m$$

NUREG/CR-6224 Correlation

Supporting Compression Equation

- NUREG/CR-6224:

Based on: W. L. Ingmanson, et. al., "Internal Pressure Distribution in Compressible Mats Under Fluid Stress,' TAPI Journal, Vol., 42, No. 10,1959.

$$\frac{\Delta L_0}{\Delta L_m} = \alpha \left[\frac{\Delta H}{\Delta L}\right]^{\gamma}$$

Where α and ϒ are empirically based. Currently: α = 1.3 and ϒ = 0.38

- Alternative (Clint Shaffer, 2005) [1]:

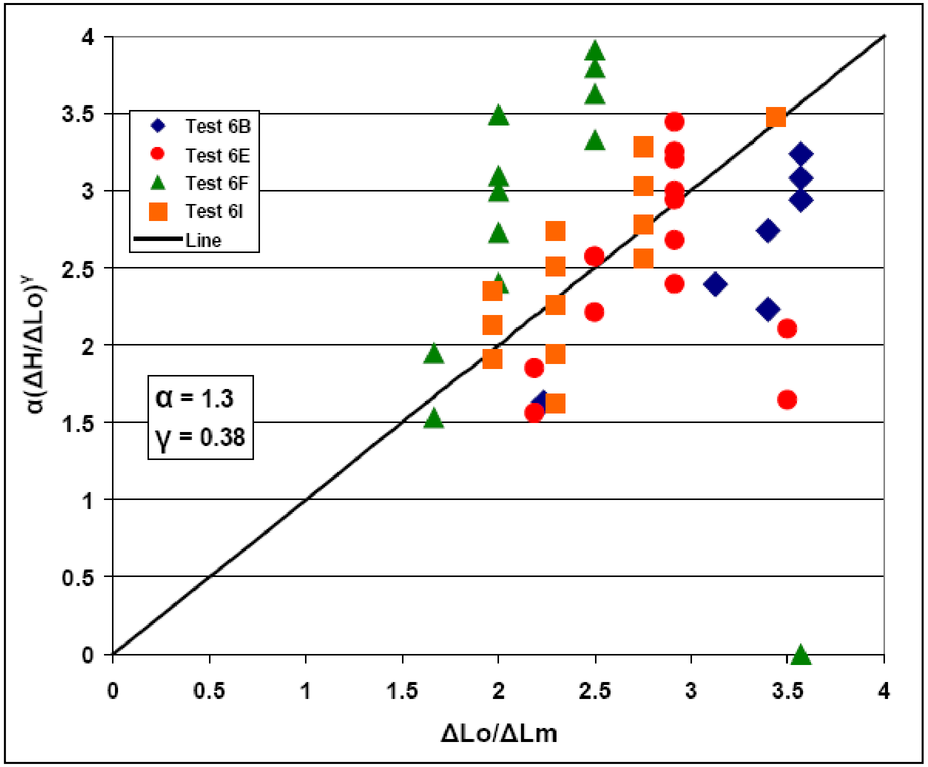
$$\frac{\Delta L_0}{\Delta L_m} = 1 + \alpha \Delta L_0^{\phi} \Delta H^{\gamma}$$

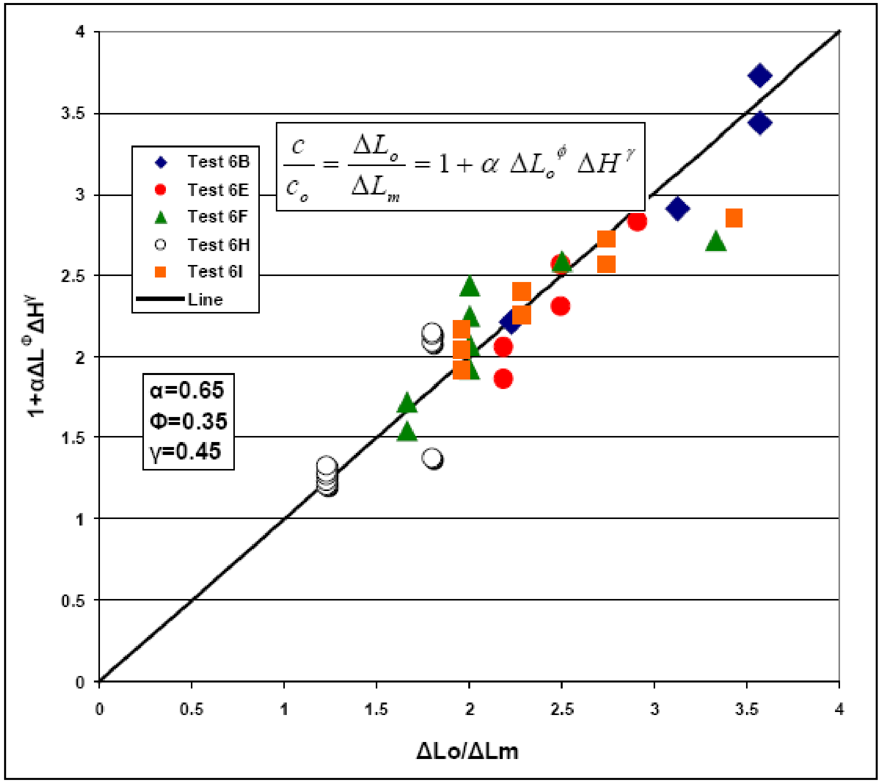
Where α ,ϒ , and Φ are empirically based. Currently: α = 0.65, ϒ = 0.38, and Φ = 0.35

1) "6224 Correlation Training Session", NRC Headquarters, April 12, 2005

Shaffer Compression Alternative [1]

$$\frac{\Delta L_o}{\Delta L_m} = \alpha \left[\frac{\Delta H}{\Delta L_o} \right]^{\gamma} \qquad \frac{c}{c_o} = \frac{\Delta L_o}{\Delta L_m} = 1 + \alpha \, \Delta L_o^{\phi} \, \Delta H^{\gamma}$$





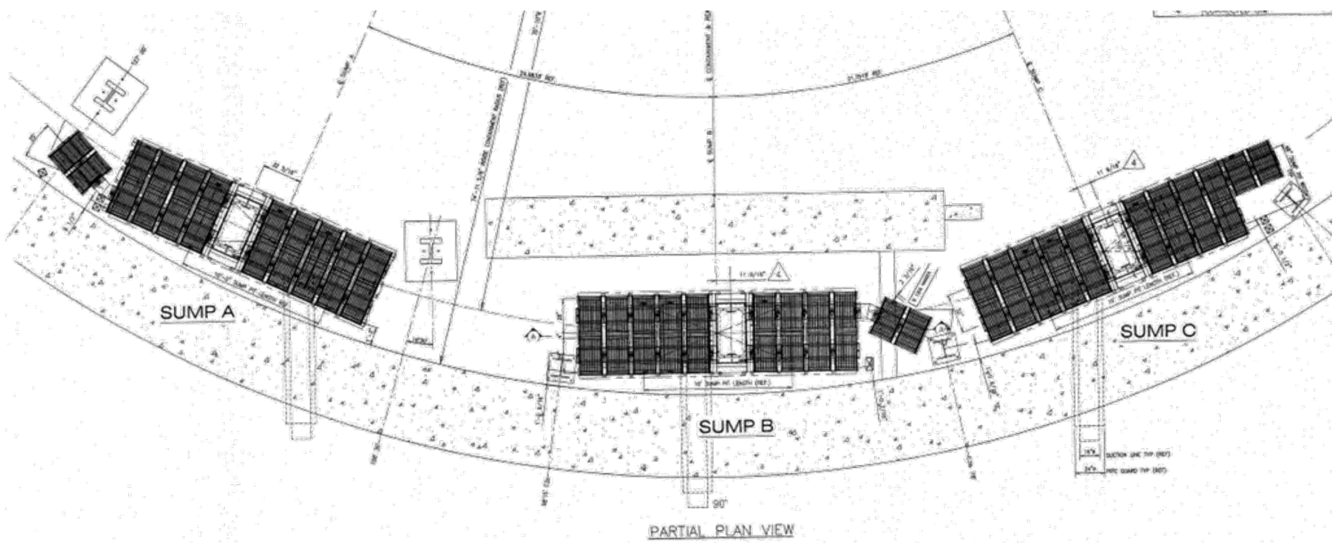
1) "6224 Correlation Training Session", NRC Headquarters, April 12, 2005

Head Loss Correlation Refinements

- Perform vertical loop testing to acquire head loss data at STP specific conditions, i.e. low approach velocity and representative fiber + particulate loadings.
- Adjust correlation supporting equations to best fit the experimental results.
- Determine from integrated chemical effects tests the impact of chemical precipitates on head loss.

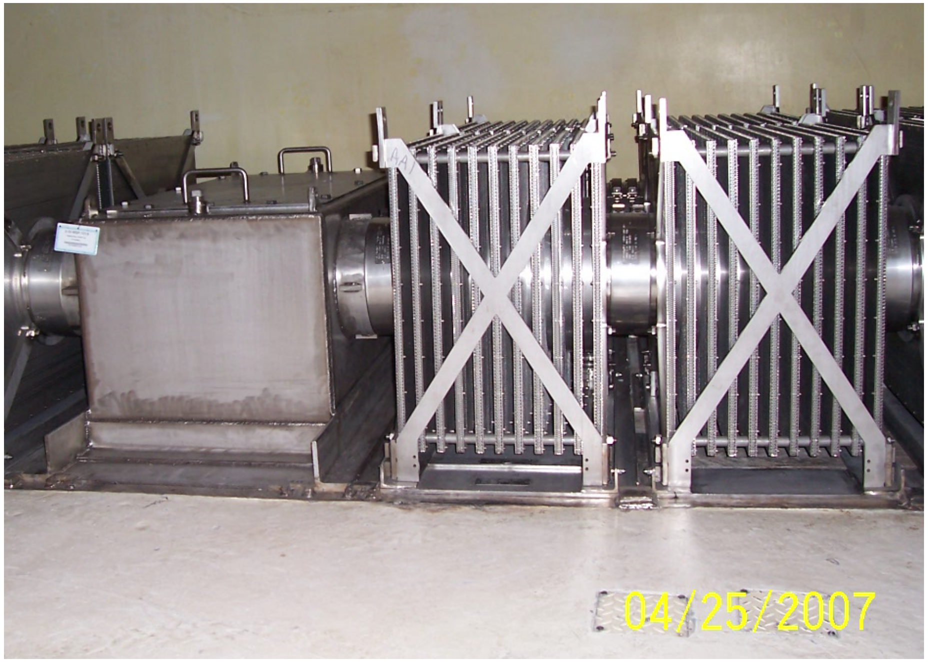
Strainer Geometry

- Calculate strainer area and gap dimensions based on strainer drawings
- Calculate average approach velocity based on total strainer area
- Calculate interstitial volume based on gap dimensions
- Calculate increased approach velocity for large debris loads based on circumscribed strainer area



Strainer Dimensions

- Strainer area (per train) = 1,818.5 ft^2
- Circumscribed area (per train) = 419.0 ft^2
- Interstitial Volume (per train) = 81.8 ft^3



Photos of STP PCI strainer

Flow Rate and Temperature

- Input total flow rate through each ECCS strainer for the specific case analyzed (maximum of 7,020 gpm per train at STP based on 1,620 gpm per HHSI pump, 2,800 gpm per LHSI pump, and 2,600 gpm per CS pump)
- Calculate debris accumulation on each strainer based on relative flow split
- Calculate pool fluid density and viscosity for a given pool temperature

NPSH Margin

- Input NPSH margin for each safety injection and containment spray pump
- Compare calculated debris bed head loss to the pump NPSH margin to determine whether the pump would fail
- NPSH Required
  - LHSI Pumps = 16.5 ft
  - HHSI Pumps = 16.1 ft
  - CS Pumps = 16.4 ft
- NPSH Available (excluding clean strainer and debris losses)
  - Start of Recirculation (267 ˚F) = 22 ft
  - 24 hours (171 ˚F) = 42 ft
  - 30 days (128 ˚F) = 51 ft

NUREG/CR-6224 Head Loss Correlation
Mixed Debris Bed Solidity

- The mixed debris bed solidity (α_m) is given by:

$$\alpha_m = \left(1 + \frac{\rho_f}{\rho_p} \eta \right) \alpha_o c$$

where:

α_o = the solidity of the original fiber blanket (i.e., the "as fabricated" solidity)

$\eta = m_p/m_f$, the particulate-to-fiber mass ratio in the debris bed

$m = \Sigma m_i$ is the total particulate mass (lbm)

ρ_f = the fiber density (lbm/ft^3)

ρ_p = the average particulate material density (lbm/ft^3) = $\Sigma \rho iVi / \Sigma Vi$

c = the head-loss-induced volumetric compression of the debris (inches/inch).

NUREG/CR-6224 Head Loss Correlation
S_v Averaging for Mixed Debris Bed

- The averaged surface to volume ratio for a mixed debris bed is given by:

$$S_v = SQRT\ [\Sigma(S_{Vn}{}^2 * v_n)/\Sigma(v_n)],$$

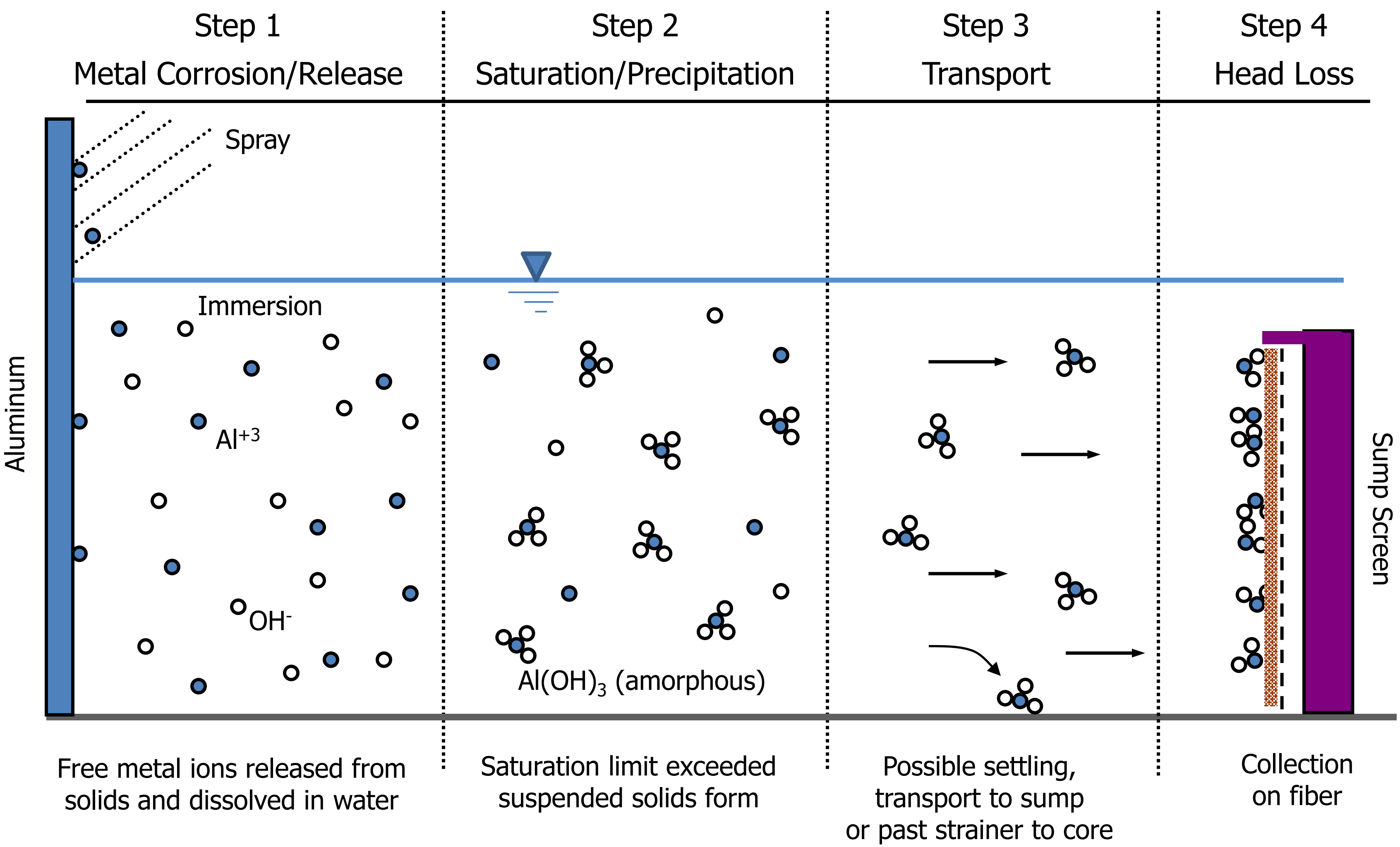
where:

S_{vn} = S_v of the nth constituent

V_n = Volume of the nth constituent

Backup Slides for Chemical Effects

Corrosion/precipitation scenario



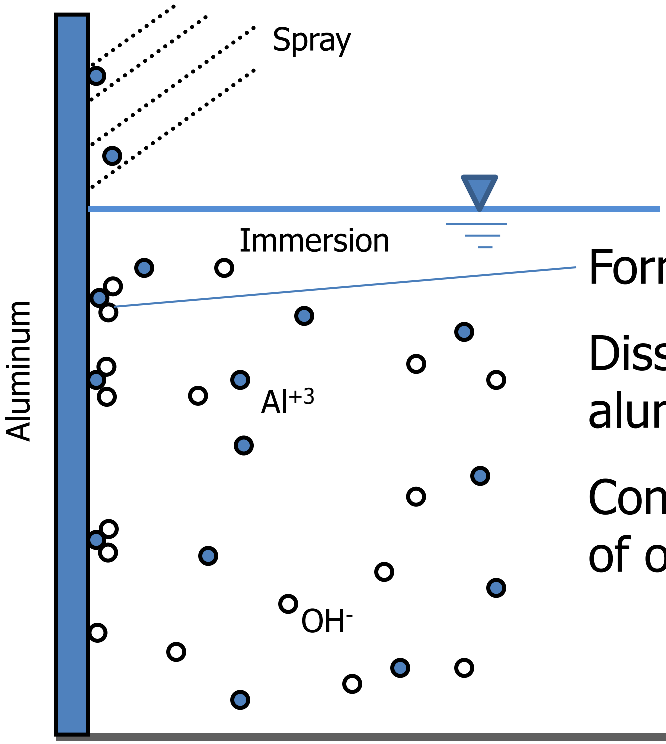
Corrosion and release may be overestimated

- Corrosion rates were determined in studies of relatively short duration
  - Over longer time, base metal corrodes but oxide layer forms at surface, limiting release of corrosion products into solution
- Passivation of surface by silicon and phosphate
- Contribution of soluble aluminum from un-submerged (sprayed) sources vs submerged sources
- Results in conservative estimate of soluble metal concentration

Corrosion/release mechanism

$$Al + 2H_2O + OH^- \rightarrow H_2 + Al(OH)_3$$

$$Al(OH)_3 + OH^- \leftrightarrow Al(OH)_4^-$$



Formation of oxide/hydroxide layer on surface

Dissolution of oxide/hydroxide layer releases aluminate ions into solution

Concentration in solution is limited to the solubility of oxide/hydroxide layer

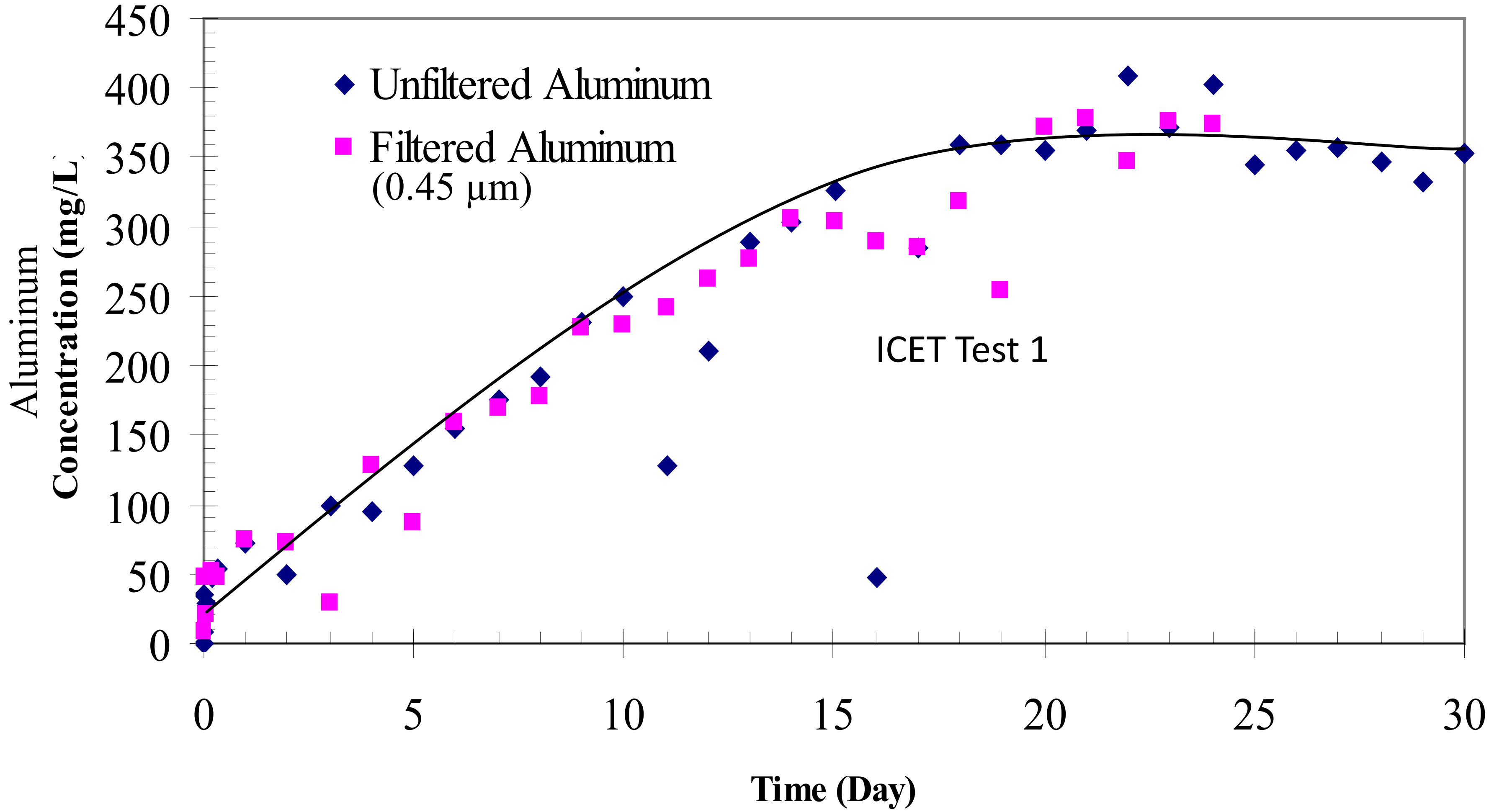
ICET Test Overview

ICET Test Number	Buffer and pH adjustment chemical	Insulation (%)		Measured pH	Chemical byproducts	
		Fiberglass	Cal-Sil		Visible at test temperature	Visible upon cooling
1	Borate (NaOH)	100	0	9.3 - 9.5	No	Yes
2	Phosphate (TSP)	100	0	7.1 - 7.4	No	No
3	Phosphate (TSP)	20	80	7.3 - 8.1	Yes, only during first few hours	No
4	Borate (NaOH)	20	80	9.5 - 9.9	No	No
5	Borate (Borax)	100	0	8.2 - 8.5	No	Yes

Test Duration: 30 days
Test Temperature: 140°F
Spray Duration: 4 hr
Water Chemistry: Boron, lithium hydroxide, hydrochloric acid

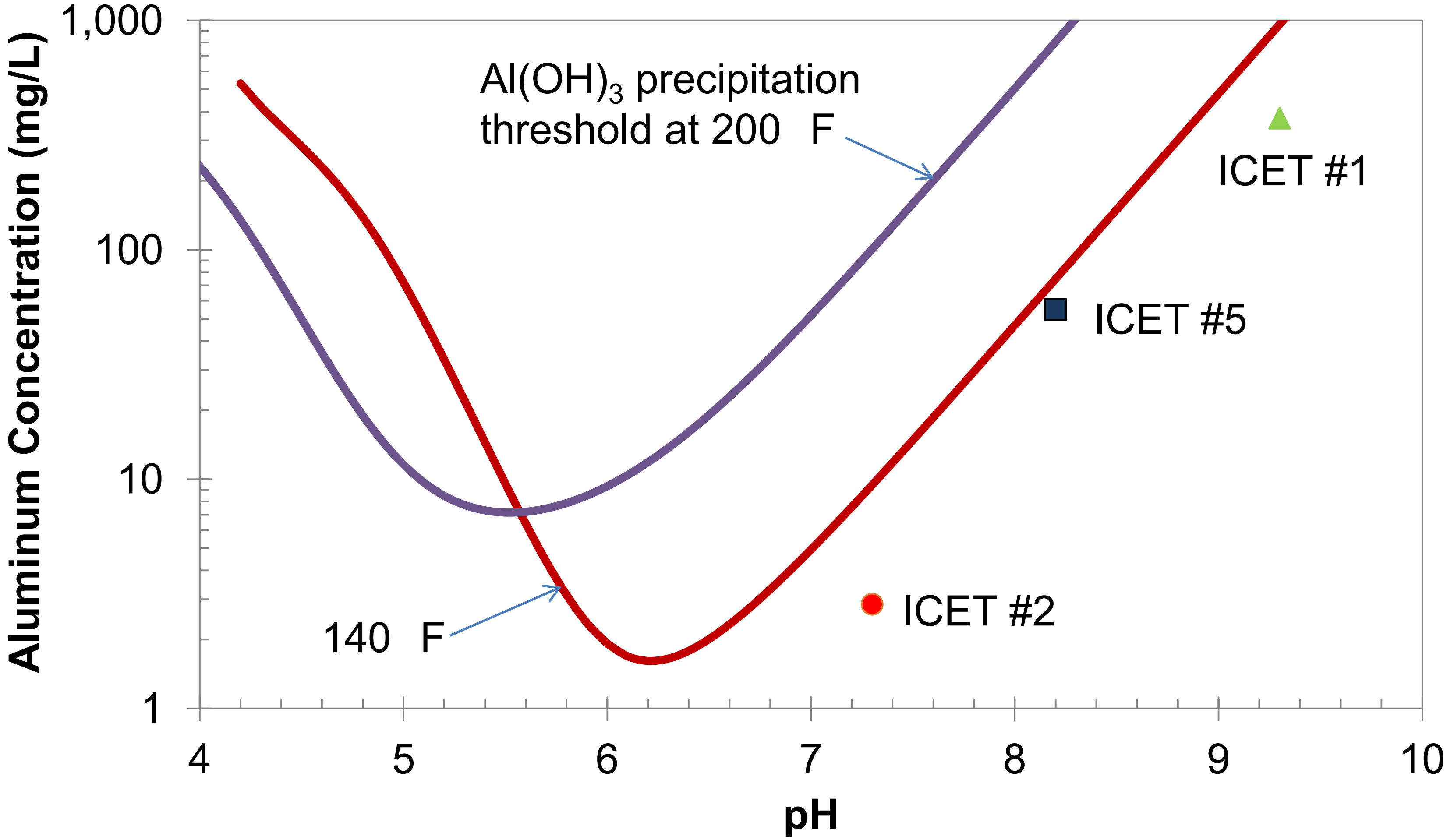
Data and results summarized from NUREG/CR-6914 (2006)

Aluminum release into solution in ICET



ICET results from NUREG/CR-6914 (2006)

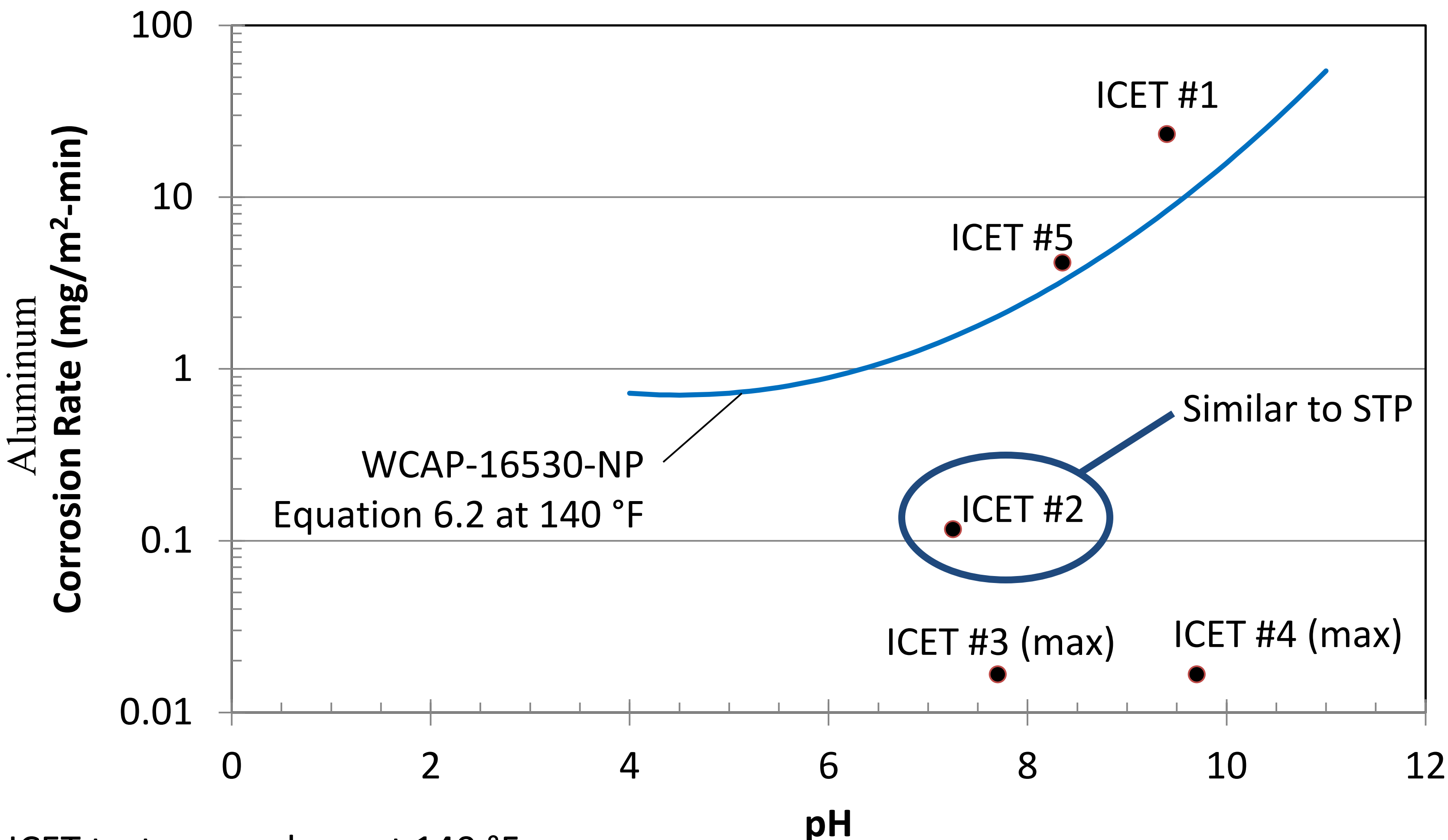
$Al(OH)_3$ solubility vs. Al concentration



ICET results from NUREG/CR-6914 (2006)

$Al(OH)_3$ saturation data calculated with Visual MINTEQ ver 3.0

WCAP-16530-NP vs ICET Tests



All ICET tests were done at 140 °F

ICET results from NUREG/CR-6914 (2006)

Passivation of Al corrosion in ICET Tests

ICET Test	pH	Al (mg/L)	Si (mg/L)
1	9.3-9.5	360	7
2	7.1-7.4	BD	45
3	7.3-8.1	BD	45
4	9.5-9.9	BD	82
5	8.2-8.5	50	4

- BD is below instrument detection limit
- Approximate concentrations at day 30 of testing

Results summarized from NUREG/CR-6914 (2006)

Precipitation may be overestimated

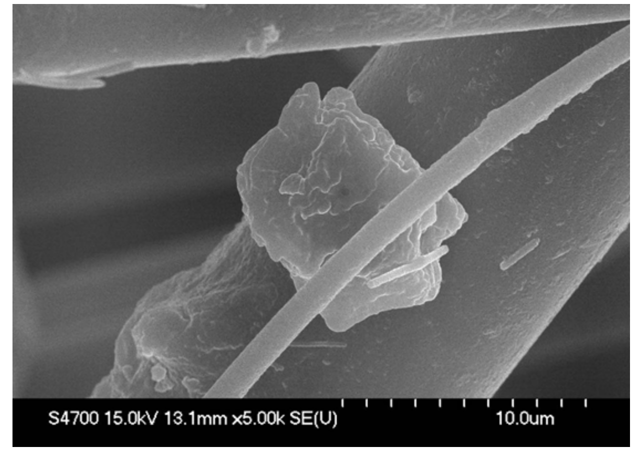
- The head loss from chemical effects may be less significant than determined using the WCAP methodology.
  - Lower aluminum release into solution
  - Not all aluminum released into solution results in precipitates
  - Different mechanism for precipitation/transport of solids to debris beds
  - Different speciation and/or morphology of solids

Form of precipitate may be more benign

- Amorphous phase precipitate assumed by WCAP-16530-NP
  - Occurs in solution
  - Transported to screen
  - Greater head loss ?
- Mineral (crystal) phase precipitate
  - Occurs on surfaces
  - Not transported
  - Occurred during VUEZ chemical effects tests
  - Less head loss ?

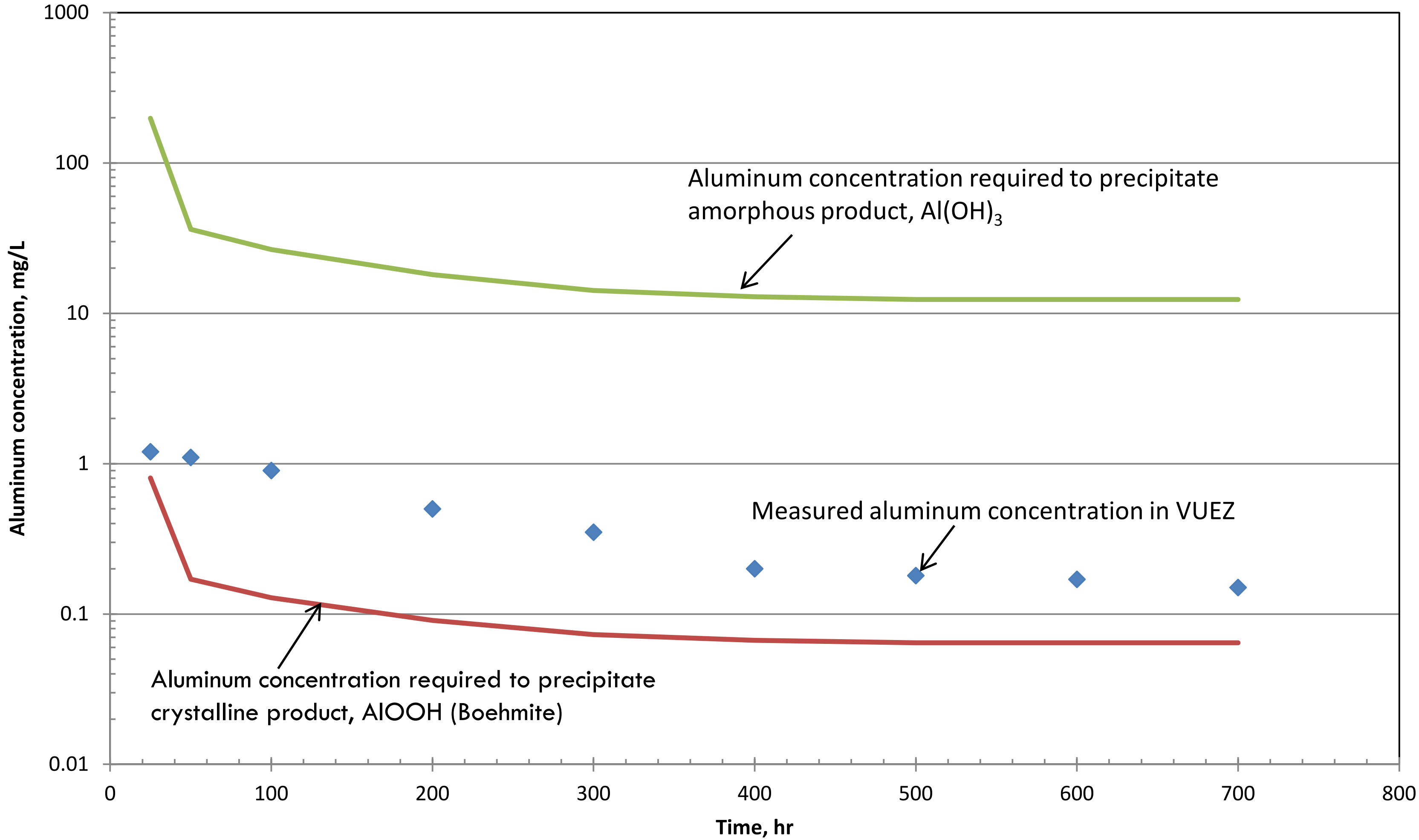
Test Characterization of Precipitates

- VUEZ tests indicate precipitates form as crystals, with nucleation on fiberglass fibers, rather than amorphous precipitates forming in the pool as predicted by WCAP-16530NP



Photos from Mattei, et al., “Experimental Program on Chemical Effects and Head Loss Modeling,” 2012.

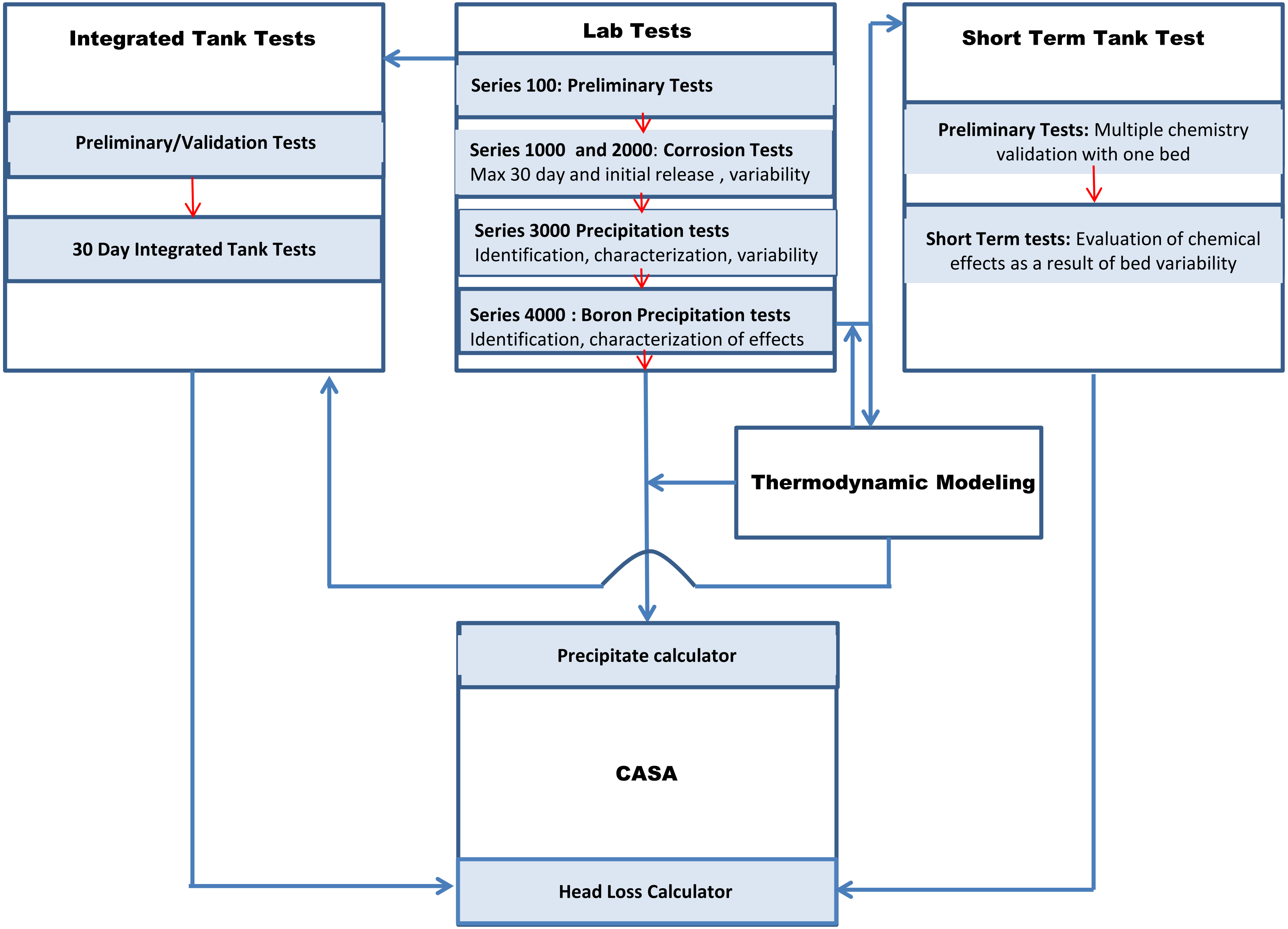
Amorphous vs crystalline phases



Data from Alion Report ALION-REP-GENE-4777-100.
Al calculated with Visual MINTEQ ver 3.0 based on temperature at time of measurement

General Experimental Strategy

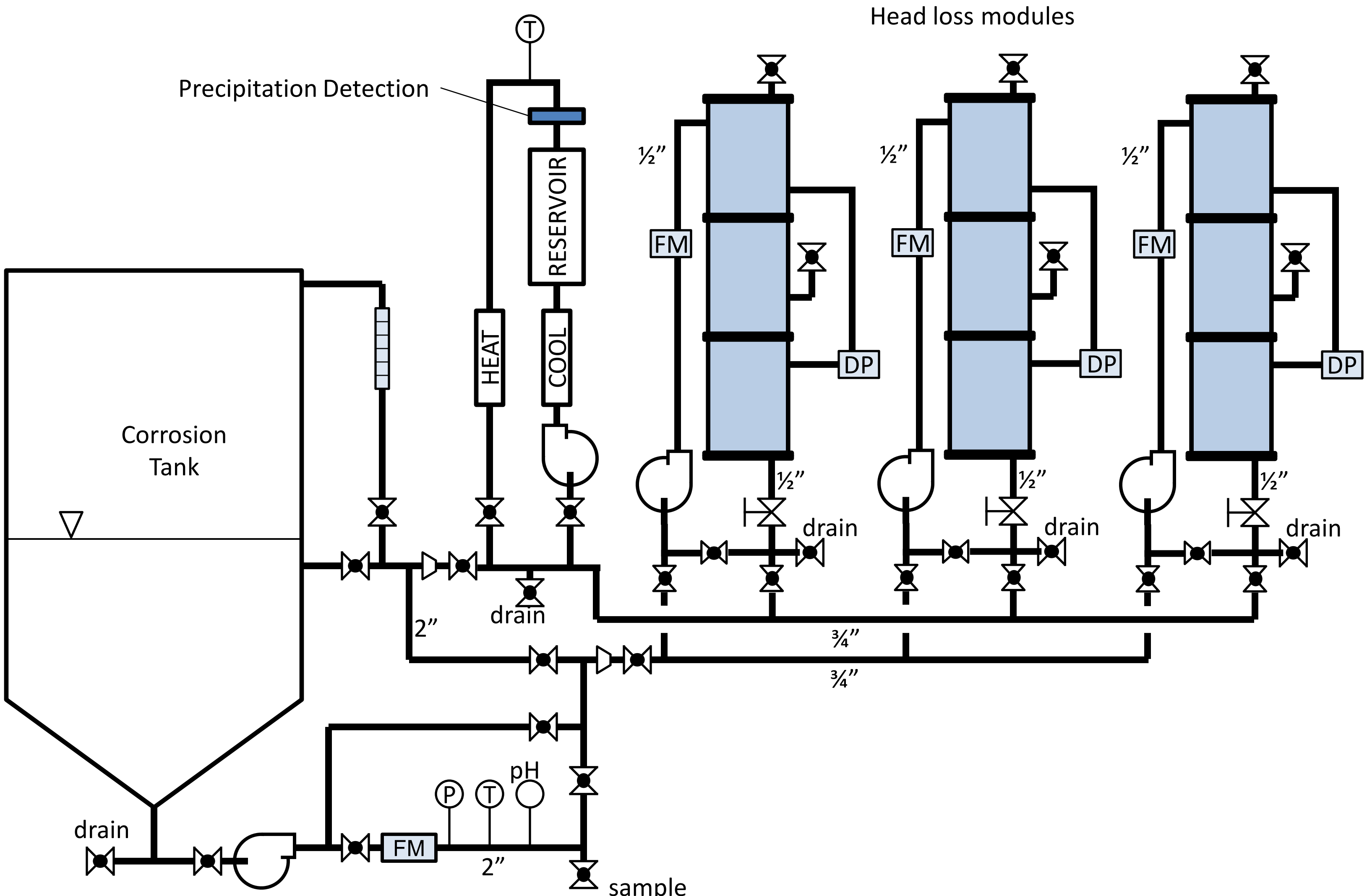
- 30-day corrosion/head loss experiment (CHLE) tests
  - Investigate relationship between corrosion, release, and head loss under 3 primary break conditions
- Bench-scale tests
  - Investigate factors that affect corrosion and inhibition
  - Investigate composition of precipitates that form
- Shorter-term CHLE tests
  - Generate head loss data for additional conditions to populate input to CASA


Integrated Tank Tests
Preliminary/Validation Tests
30 Day Integrated Tank Tests
Lab Tests
Series 100: Preliminary Tests
Series 1000 and 2000: Corrosion Tests
Max 30 day and initial release , variability
Series 3000 Precipitation tests
Identification, characterization, variability
Series 4000 : Boron Precipitation tests
Identification, characterization of effects
Short Term Tank Test
Preliminary Tests: Multiple chemistry validation with one bed
Short Term tests: Evaluation of chemical effects as a result of bed variability
Thermodynamic Modeling
Precipitate calculator
CASA
Head Loss Calculator

Corrosion/Head Loss Experiment (CHLE) Equipment Configuration

- 30-day corrosion tests integrated with head loss testing
- Materials in corrosion tank scaled to quantities in STP containment
- Three parallel head loss modules with representative debris bed for repeatability
- Small, medium, and large LOCAs tested
- Realistic temperature, pH, chemicals, materials, and flow rate
- Declining temperature profile similar to LOCA

CHLE Equipment Process Flow Diagram

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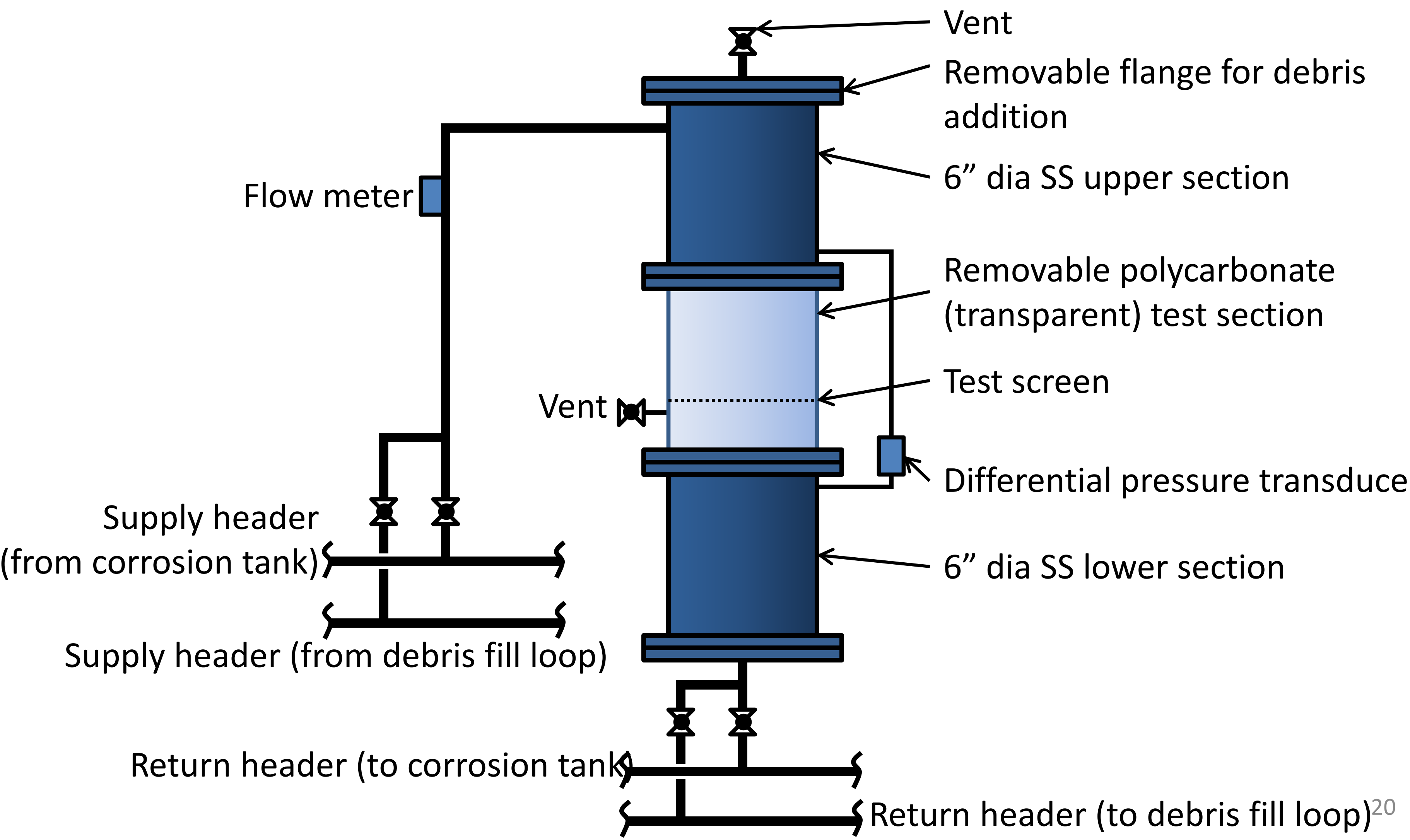
STANLEY

Range of Plant-Specific Conditions

- Some input parameters may have variations based on ranges for normal operating conditions and differences in accident scenarios and plant responses.
- Variable parameters significant to chemical effects include:
  - Boron concentration (RWST varies within acceptable operating ranges, and RCS varies over the fuel cycle)
  - Water volume (RWST varies within acceptable operating ranges)
  - Pool temperature (time-dependent parameter that varies depending on break size and plant response)
  - Debris quantity (varies depending on break size and location)
  - pH (time-dependent parameter that varies depending on buffer quantity, boron concentration, and long-term acid formation)

Head loss assembly



Experimental conditions for STP experiments

- Solution chemistry representative of normal containment pool conditions
  - Contributions from RCS, RWST, accumulator
- Materials added to corrosion tank to maintain ratio of (material quantity)/(solution volume)
  - Metals, concrete, and coatings = ft^2/ft^3
  - Insulation debris = ft^3/ft^3
- Velocity through debris bed representative of strainer design

Basis for exp. conditions (continued)

- Temperature drop through heat exchanger representative of ECCS heat exchanger
- Hold time at lower temperature before passage through debris bed representative of travel time between heat exchangers and reactor core

Temperature strategy

- Max. temperature in test system ~ 185 ºF.
- Corrosion due to higher temperatures accommodated with temporary inclusion of additional materials
- Amount of materials to add based on literature corrosion rates, verified with bench testing

Debris bed formation strategy

- Particles and fiber in 5:1 mass ratio
  - NUKON Fiber prepared in accordance with the NEI fiber debris preparation protocols
  - 10 μm diameter silicon carbide particles
- Independent recirculation loop allows debris bed to be formed without debris circulating through corrosion tank
- Once debris beds are formed, uniform, and consistent, circulation of fluid from corrosion tank begins

Bench test description (1)

- Objective: Evaluate whether existing corrosion literature can be used to correlate quantity of extra aluminum and fiberglass to be included in 30-day test to account for corrosion due to temperatures above 185 °F
- System: Autoclave for high temperature testing
  - T >185 °F
  - Quiescent conditions
  - Aluminum and fiberglass present

Bench test description (2)

- Objective: Evaluate the effects on the rate of aluminum corrosion due to fiberglass dissolution, phosphate, and oxide layer formation
- Test conditions:
  – Aluminum/silicon interactions: vary the relative amounts of aluminum and fiberglass in the system, adjust pH
  – Phosphate inhibition: run tests at same pH with and without TSP (use NaOH for pH adjustment)
  – Effect of oxide layer formation and solubility limitations: vary pH and evaluate corrosion rates and maximum Al in solution relative to solubility

Short-term CHLE Test Objectives

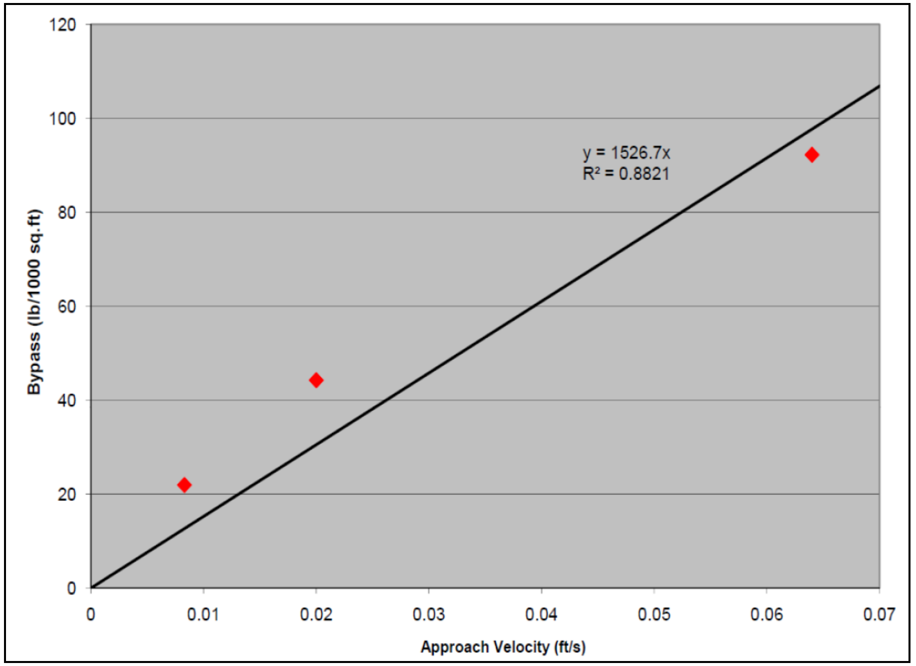
- Objective
  - Supplement head loss data needed for CASA inputs
- Test Description
  - Use integrated corrosion/head loss test equipment
  - Representative debris beds from STP vertical loop head loss testing
- Head loss measured in 3 parallel loops

Backup Slides for Debris Bypass, Incore Blockage, and Boron Precipitation

Debris Bypass

- Currently using a simplistic correlation for fiber bypass (based on a very limited data set):

  BP_{total} (g) = 1.538 * Q (gpm)
- Testing will be conducted to more accurately determine fiber debris bypass over the range of conditions applicable to STP



Gilbert Zigler, “Quantification and Characterization of By-pass Fiber Debris” Revision 2, Alion Science and Technology, January 18, 2011

Debris Bypass Test Plans

- New NEI debris preparation protocol will be used to more realistically prepare fine debris (reduces the artificial generation of fiber shards due to mechanical shredding)
- Testing will be performed in a large tank with a prototype strainer module
- Testing will be conducted with fiber only (up to 100% bypass will be assumed for particulate and precipitates)
- Filters downstream of the strainer will be used for 100% capture of bypass fibers
- Important variables that will be investigated in the testing include fiber concentration, total fiber quantity (or bed thickness), and strainer approach velocity

Bypass Implementation in CASA Grande

- Based on the bypass test results and the time-dependent arrival of debris at the strainer, CASA Grande will calculate a time-dependent bypass quantity as a function of the strainer flow rate, strainer coverage, and concentration of debris in the pool
- Fiber transport to the reactor vessel will be determined based on the flow split between the CS and SI pumps
- Fiber transport to the core will be determined based on the fraction of SI flow that passes through the core
- Fiber that bypasses the core and re-enters the containment pool will be re-evaluated for potential transport and bypass through the strainer a second time

Example Debris Bypass Calculations

- Fiber bypass fraction is calculated using the correlation: BP_{total} (g) = 1.538 * Q (gpm)
- Total sump flow rate assuming two train operation is 14,040 gpm with 8,840 gpm through the SI pumps and 5,200 gpm through the CS pumps
- STP reactor vessel: 193 fuel assemblies
- BP_{total} = 1.538 * 14,040 gpm = 21,600 g (47.6 lb_m; 19.8 ft^3)
  - Split to SI pumps: 21,600 g * (8,840 /14,040) = 13,600 g
  - Split to CS pumps: 21,600 g * (5,200 / 14,040) = 8,000 g
- Incore fiberglass debris load: 70 g / fuel assembly

Debris Bypass vs. SI Flow Rate

Scenario	Nominal Safety Injection Flow Rate (gpm)	Debris Bypass to Reactor (g/FA)
LBLOCA – 3 Train Operation	13,260	106
LBLOCA – 2 Train Operation	8,840	70
LBLOCA – 1 Train Operation	4,420	35
MBLOCA (6 inch break)	9,000	72
SBLOCA (1.5 inch break)	4000	32

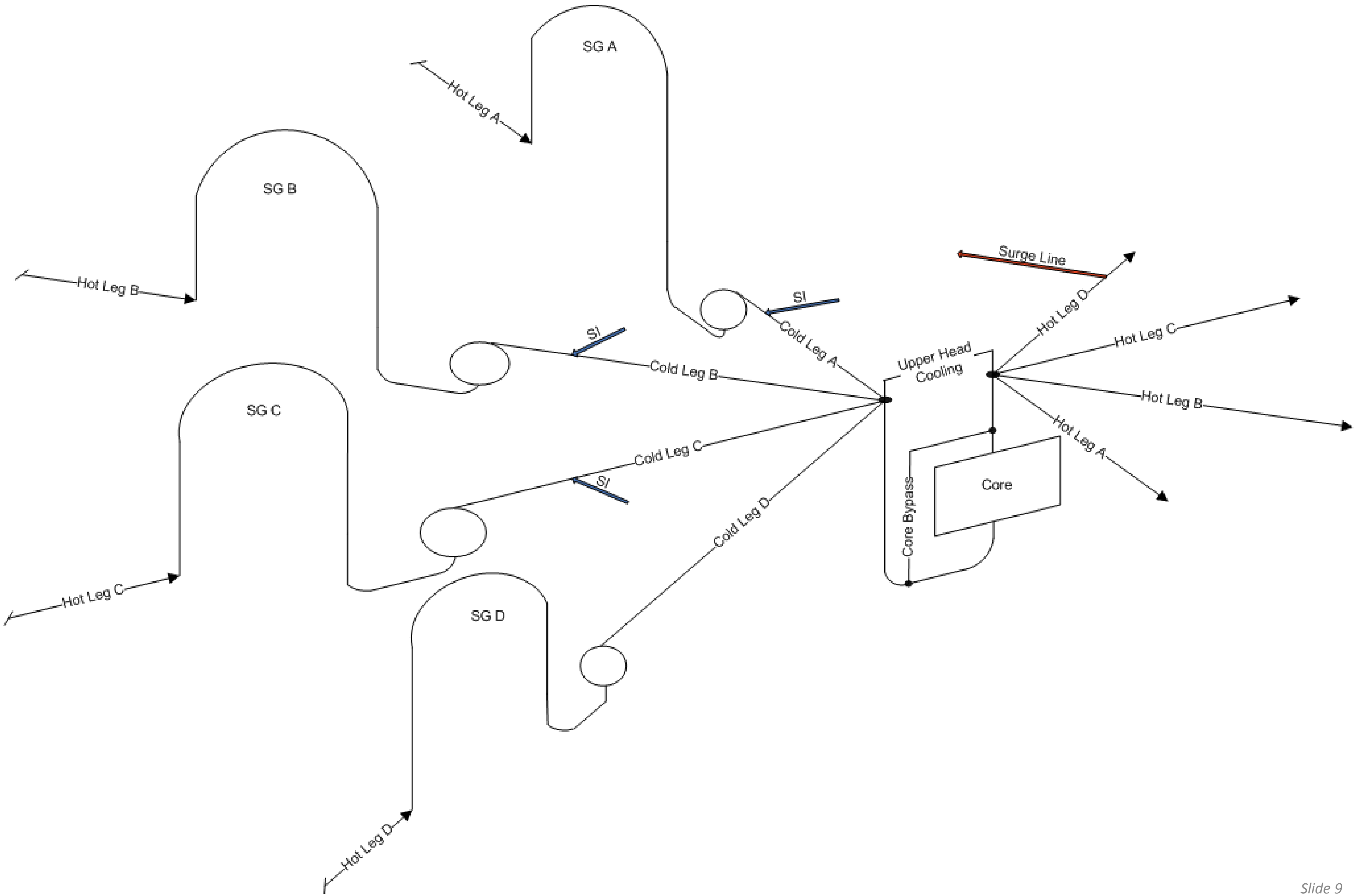
Plan for Addressing Core Blockage

- Perform initial evaluation of expected incore conditions for range of break sizes and break locations
- Use RELAP5 simulations to refine initial evaluations and identify scenarios where full blockage at the bottom of the core would not lead to core damage
- Use results of time-dependent transport analysis, bypass testing, and chemical effects testing to determine the time dependent debris loads in the core for scenarios that could lead to core damage
- Use RELAP5 simulations to define the driving head and required core flow for scenarios of concern
- Develop analytical and/or test methods to determine incore head loss and identify any scenarios that would result in core damage

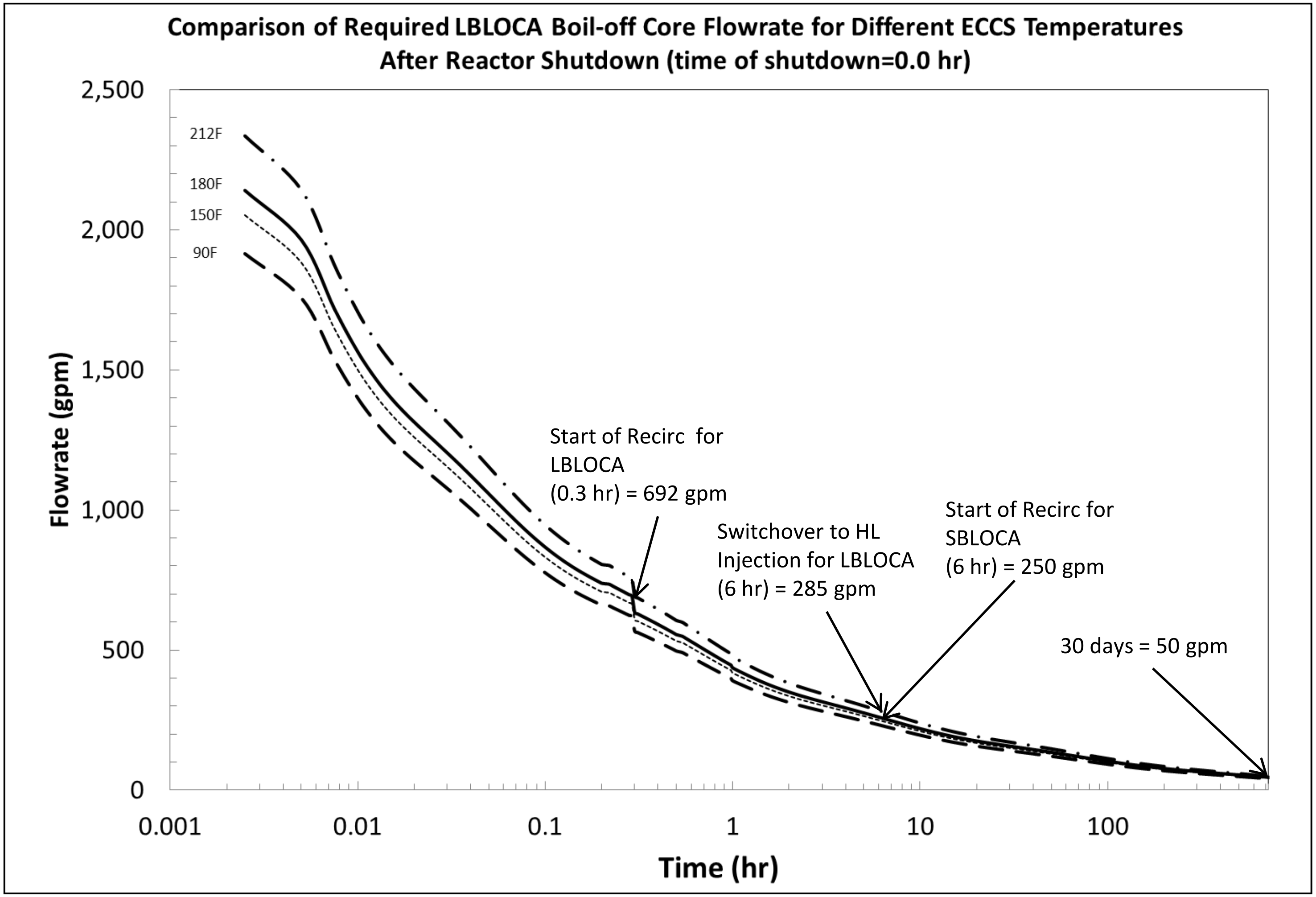
Incore Blockage Considerations

- Core blockage is highly dependent on break location and injection flow path
  - Cold leg break with cold leg injection
  - Cold leg break with hot leg injection
  - Hot leg break with cold leg injection
  - Hot leg break with hot leg injection
- Switchover to hot leg injection occurs 5.5 hours after start of recirculation
- Strainer bypass would predominantly occur within 5 pool turnovers (less than 2 hours for an LBLOCA and over 2 days for an SBLOCA)
- Chemical precipitation would likely take several hours (or days)

Illustration of RCS at STP



Required Minimum Core Flow



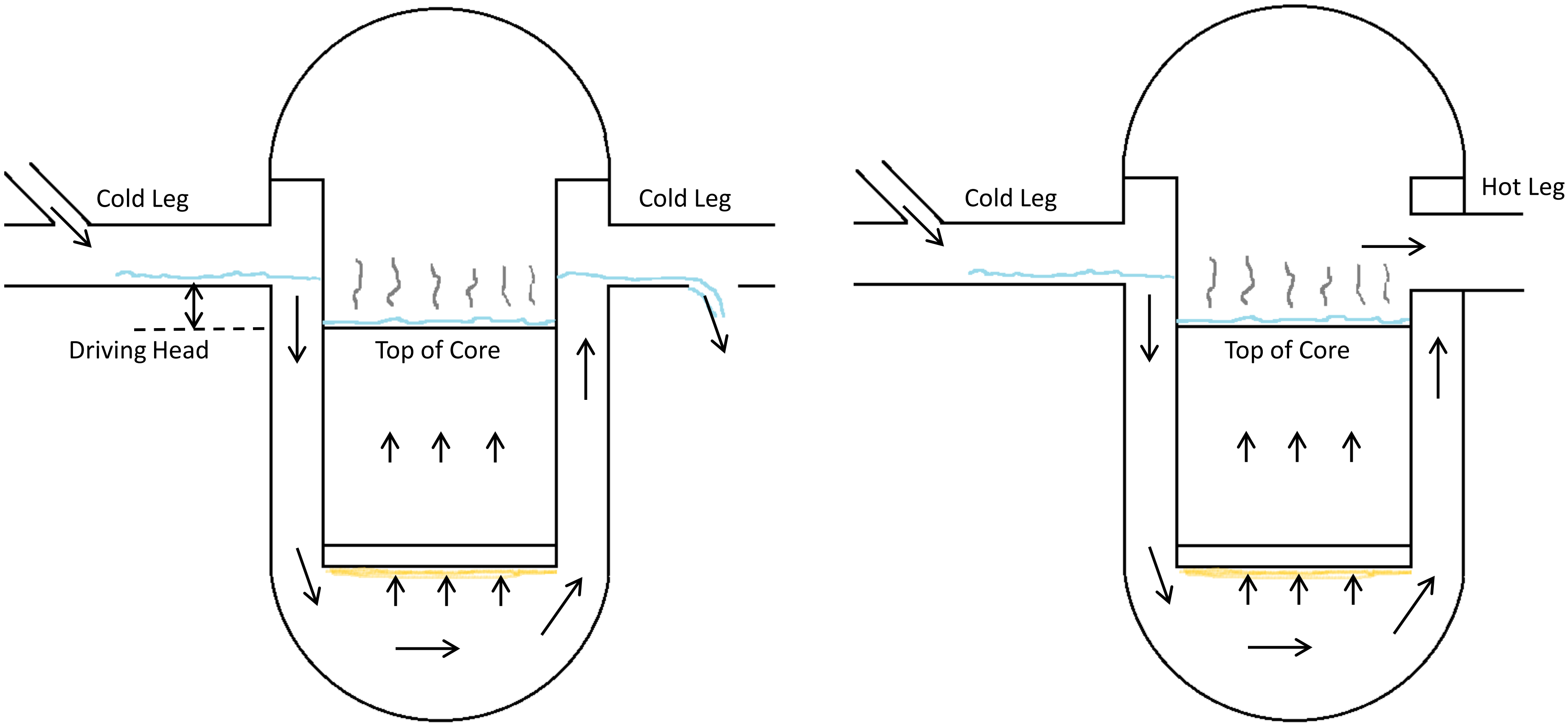
Injection flow for LBLOCA (3 train operation) is 13,260 gpm

Injection flow for SBLOCA (1.5 inch break) is 4,000 gpm

Incore Acceptance Criteria (LBLOCA)

- Cold Leg Injection Acceptance Criteria:
  - Flow rate through the core must be at least equal to the boil-off rate to keep the core cool
  - Head loss at the minimum required flow rate cannot exceed the driving head
- Hot Leg Injection Acceptance Criteria
  - Flow rate through the core must be greater than the boil-off rate to prevent boron precipitation
  - Head loss at the minimum required flow rate cannot exceed the driving head

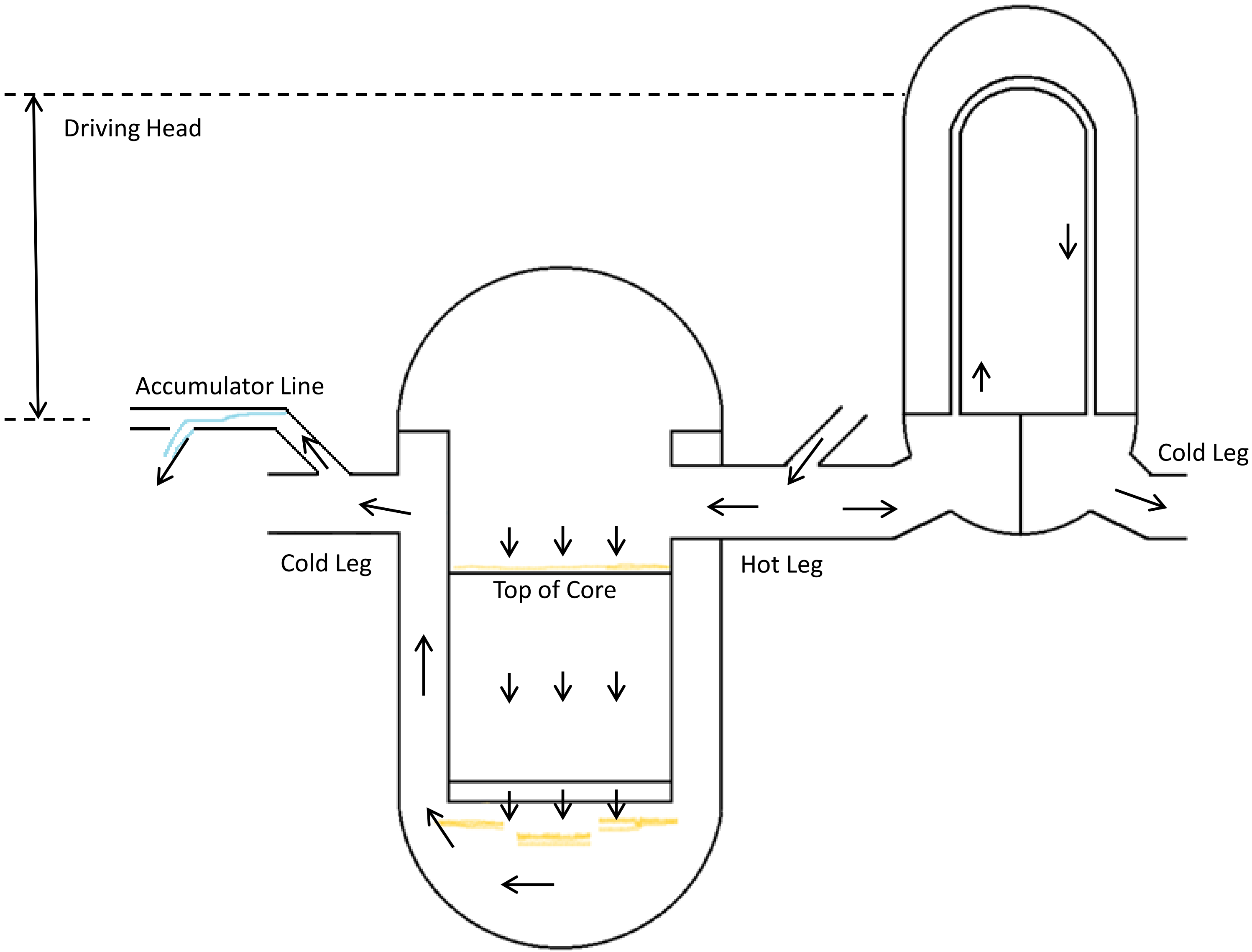
CL Break with CL Injection (LBLOCA)



CL Break with CL Injection (LBLOCA)

- Driving head: 31.1 ft (base of cold leg) – 26.9 ft (top of active fuel) = 4.2 ft
- Bypass debris will be split between the core and transported out the cold leg break
- Max transport to core for 3 train operation: 692 gpm / 13,260 gpm = 5%
- Assuming 106 g/FA bypasses strainer: 106 g/FA x 5% = 5 g/FA
- Chemical effects not expected this early in event
- Flow through core will continue to decline until switchover to hot leg injection

CL Break with HL Injection (LBLOCA)

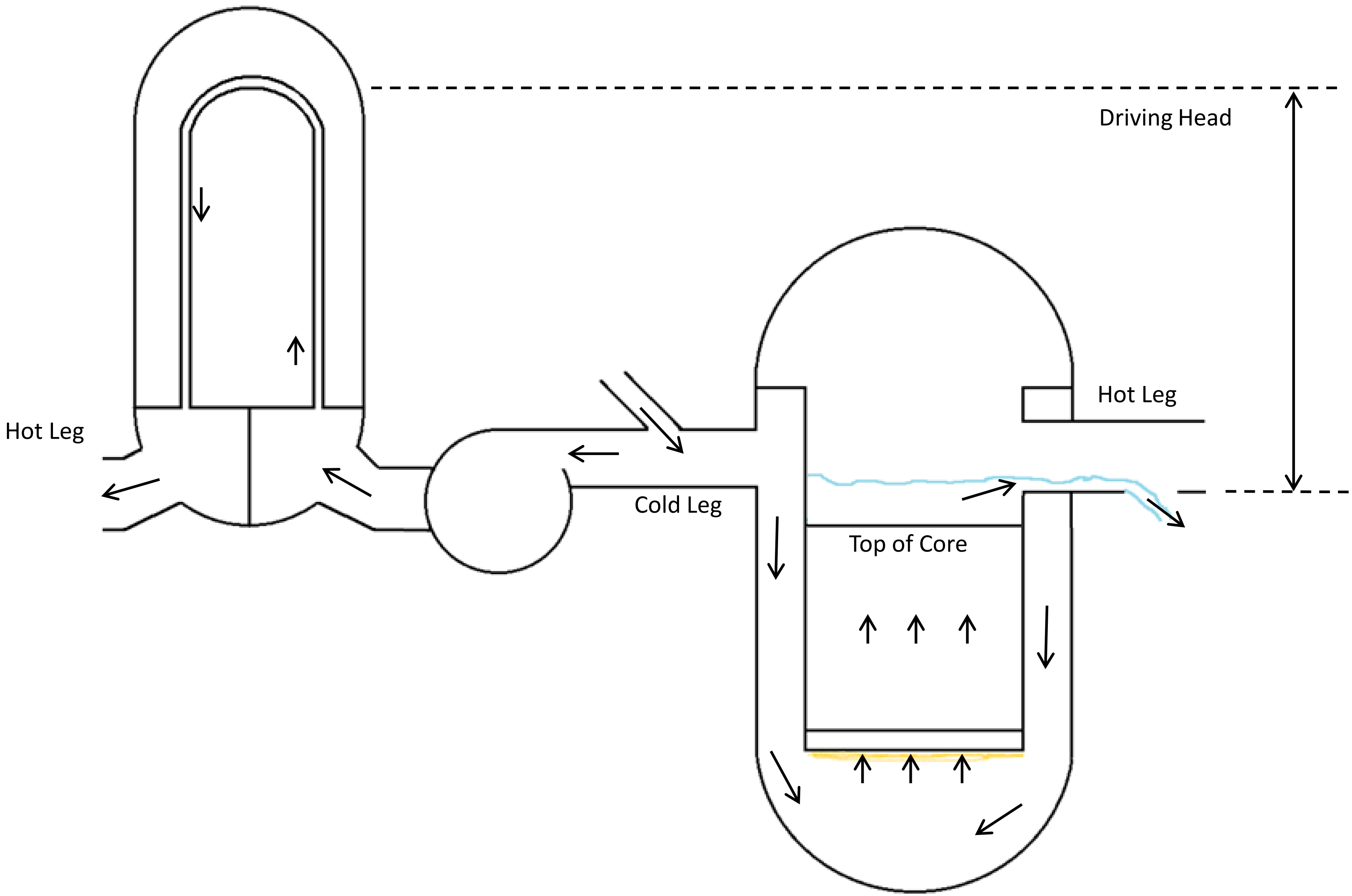


CL Break with HL Injection (LBLOCA)

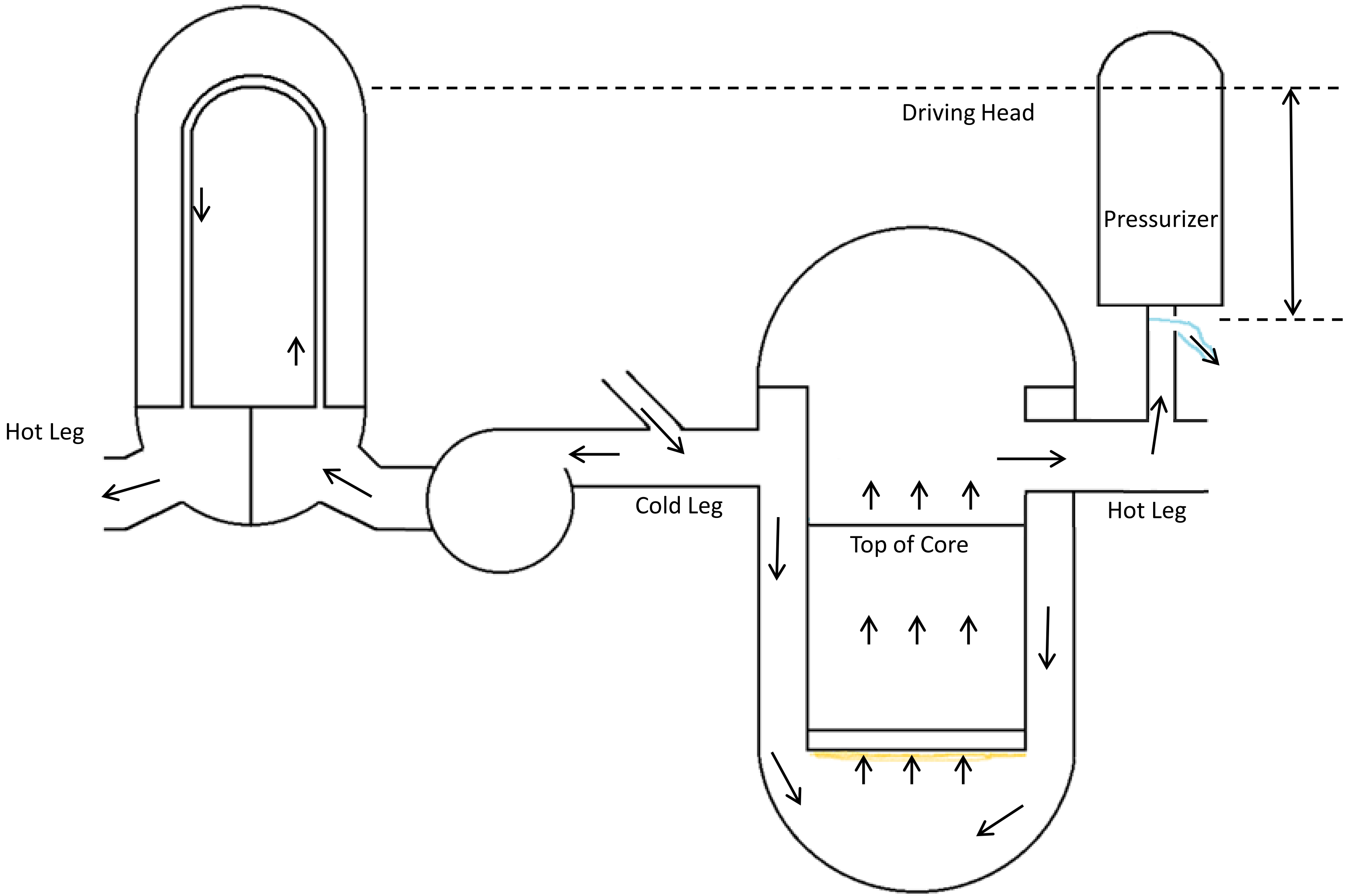
- Driving head: 72.3 ft (SG tube spillover) – 35.0 ft (highest elevation for RCS CL side break larger than 6”) = 37.3 ft
- Debris in core will be back-flushed toward lower plenum
- Max required flow = 285 gpm + 10% to prevent boron precipitation[1] = 314 gpm (will continue to decline)
- Debris bypass expected to be reduced after switchover to hot leg injection since most debris will have already accumulated on strainer; potential sources of long-term bypass include:
  - Delayed fiber erosion
  - Delayed washdown from upper containment
  - Bypass debris circulated through containment sprays back to the pool
  - Bypass debris back-flushed out of the core on switchover to hot leg injection

[1] WCAP-16793-NP, Revision 2, October 2011

HL Break with CL Injection (LBLOCA)



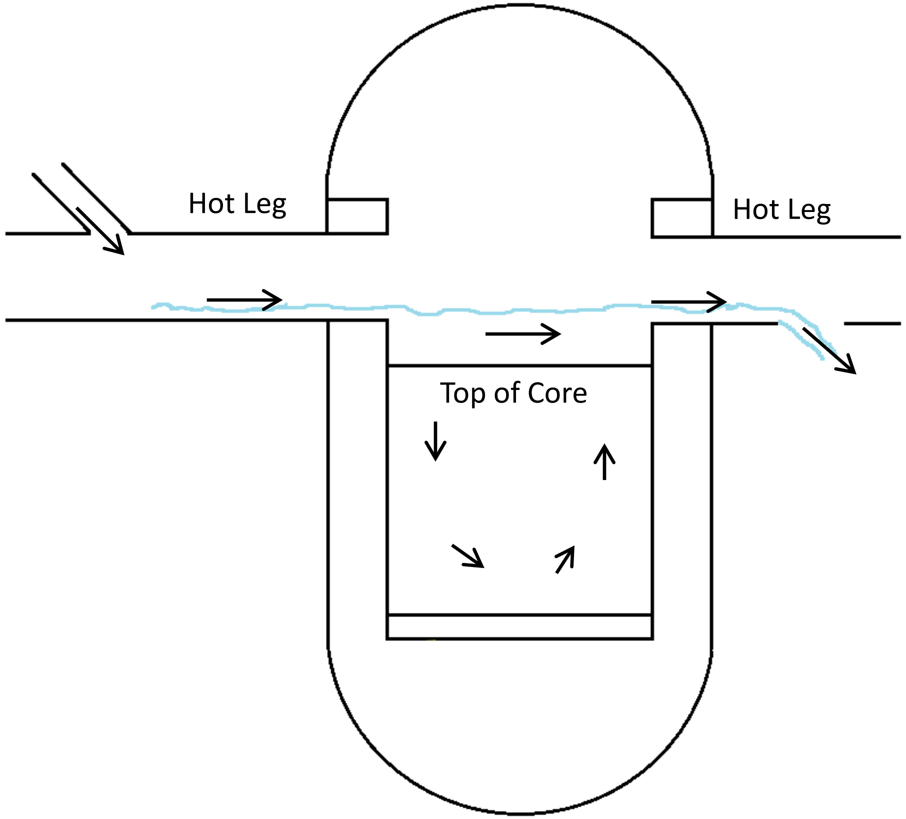
Surge Line Break with CL Injection (LBLOCA)

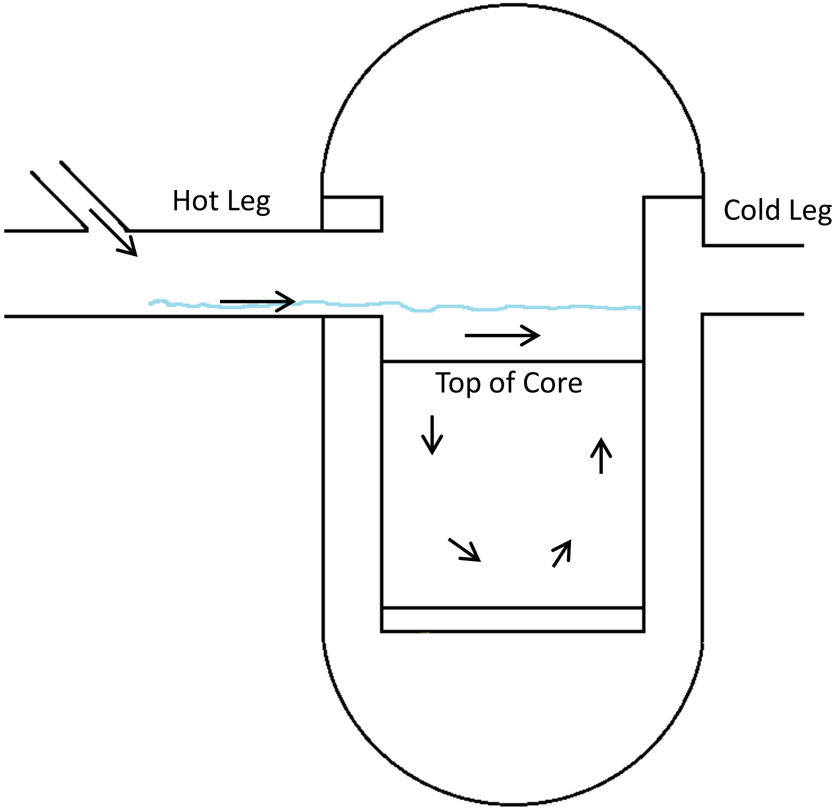


HL Break with CL Injection (LBLOCA)

- Driving head (hot leg): 72.3 ft (SG tube spillover) – 31.0 ft (base of hot leg) = 41.3 ft
- Driving head (surge line): 72.3 ft (SG tube spillover) – 47.9 ft (base of pressurizer) = 24.4 ft
- Potential for 100% of bypass debris to accumulate in the core
- Chemical effects not expected this early in event
- Max required flow = 692 gpm + 10% to prevent boron precipitation = 761 gpm (will continue to decline until switchover to hot leg injection)
- For higher elevation breaks, water would still be able to enter the top of the core even if there is full blockage at the bottom of the core

HL Break with HL Injection (LBLOCA)





HL Break with HL Injection (LBLOCA)

- Flow essentially passes straight through top of reactor vessel keeping the core cool
- Required flow through core will continue to decline
- Debris bypass expected to be reduced after switchover to hot leg injection since most debris will have already accumulated on strainer; potential sources of long-term bypass include:
  - Delayed fiber erosion
  - Delayed washdown from upper containment
  - Bypass debris circulated through containment sprays back to the pool
  - Bypass debris back-flushed out of the core on switchover to hot leg injection

Incore Blockage Summary (LBLOCA)

Scenario	Required Flow / FA (gpm)	Fiber Load / FA (g)	Chemical Load	Driving Head (ft)
CL Break, CL Injection	3.6 → 1.5	5	-	4.2
CL Break, HL Injection	1.6 → 0.3	<100	Present	37.3
HL Break, CL Injection	3.9 → 1.6	100	-	41.3
HL Break, HL Injection	1.6 → 0.3	-	Present	-

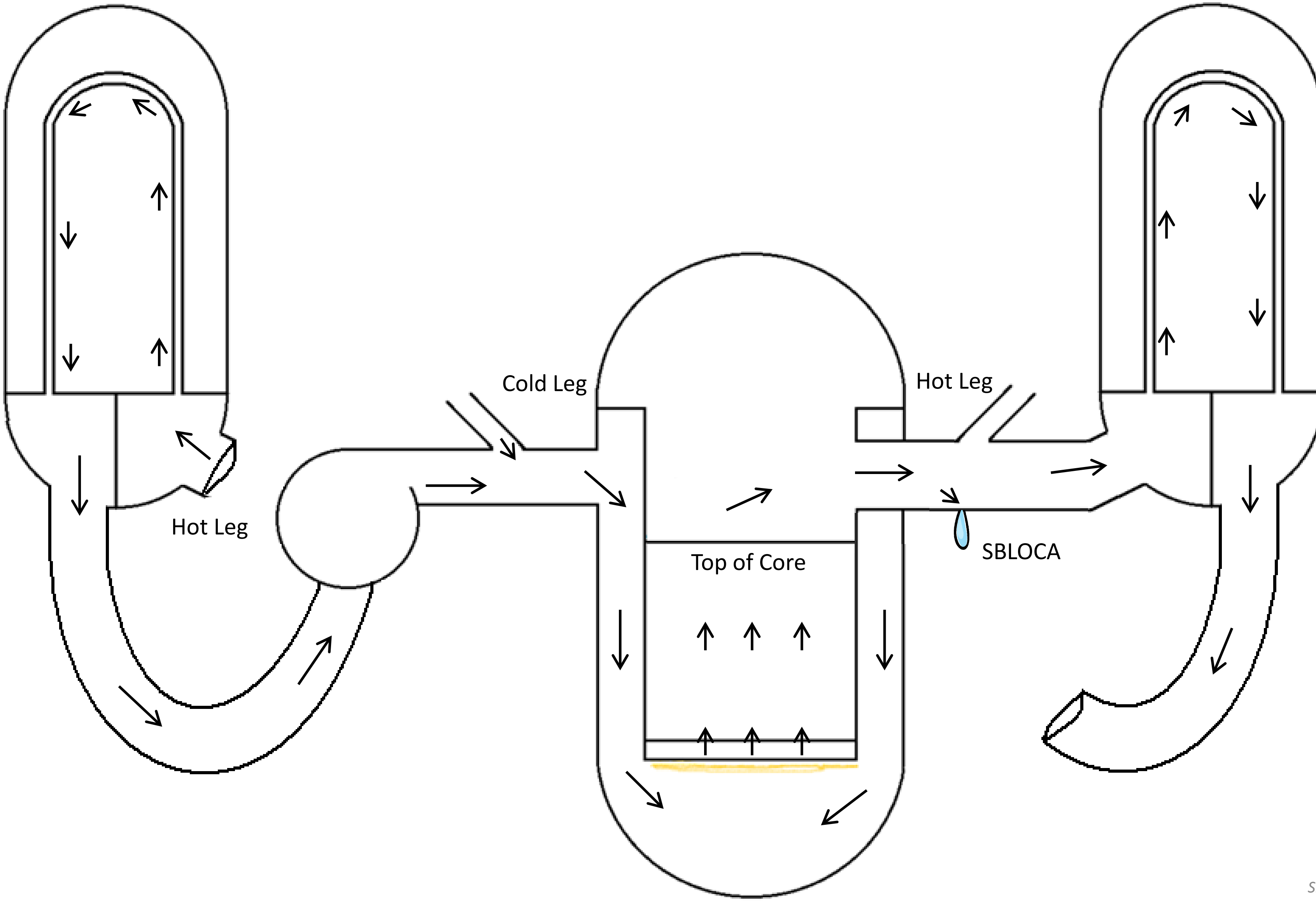
Assuming:

- Fiber bypass of approximately 100 g/FA
- Strainer is not fully covered allowing long-term fiber bypass
- Chemical precipitation occurs after switchover to hot leg injection
- Hot leg break with hot leg injection has no significant blockage points

Considerations for SBLOCAs

- Injection flow alone may not be sufficient to cool the core early in the event
- The steam generators would quickly fill with water allowing natural circulation to cool the core
- Switchover to recirculation would occur 6+ hours into the event
- Debris bypass quantities are likely to be very low due to the low sump flow rates
- If core blockage does occur, the injection flow should still enter through the top of the core

Natural Circulation for an SBLOCA



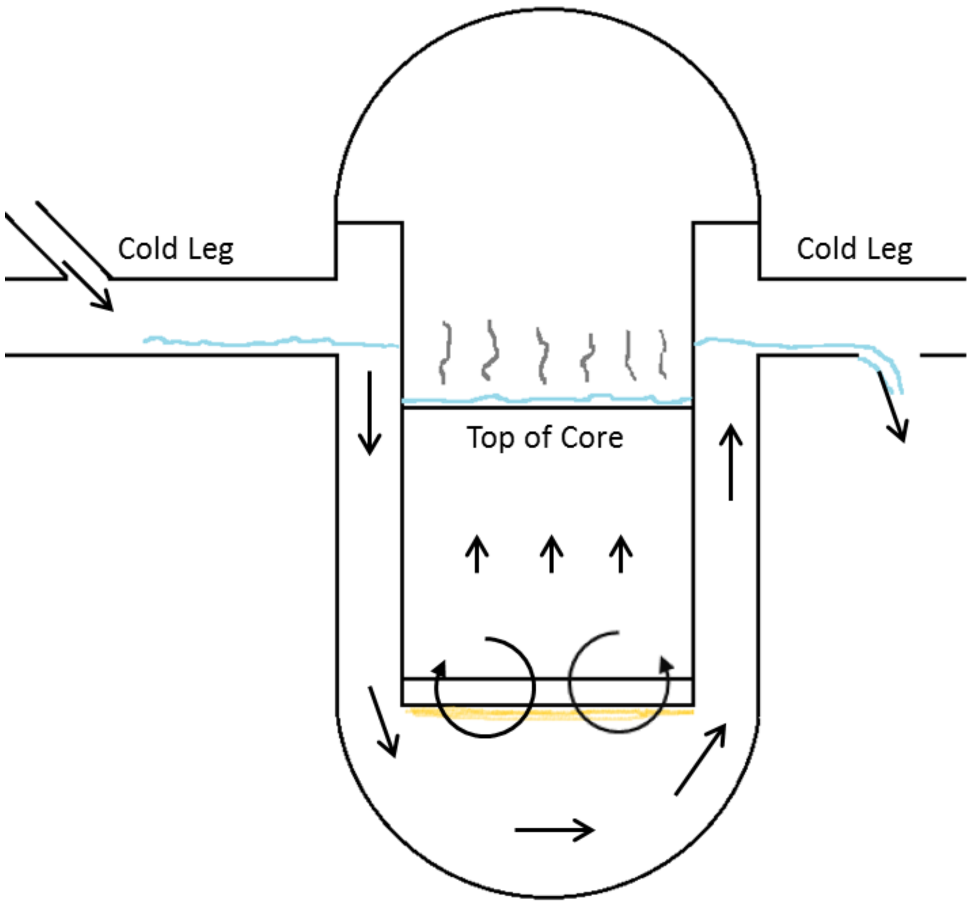
Plan for Addressing Boron Precipitation

- Perform initial evaluation of expected incore conditions for scenario of concern (large cold leg break during cold leg injection)
- Conservatively calculate boron concentration during the cold leg injection period to determine whether boron precipitation could occur before switchover to hot leg injection
- If necessary, use PWROG test results or develop new test methods to determine whether boron precipitation would occur

Boron Precipitation Considerations

- Boron precipitation is primarily a concern for cold leg breaks during cold leg injection
- It is only a concern for large breaks where boiling would occur
- Plants are designed to switch to HL injection before boron precipitation can occur, but fiber beds may inhibit diffusion in the lower plenum and accelerate the onset of boron precipitation
- As discussed previously, only 5 g of fiber per fuel assembly is expected to transport to the core for this scenario
- Low fiber loads would not be likely to significantly inhibit the natural diffusion of boron into the lower plenum
- STP has combined hot and cold leg injection following hot leg switchover ~5.5 hours after the start of recirculation or ~6 hours into the event

Boron Precipitation for CL Break with CL Injection (LBLOCA)



Backup Slides: PRA

ACRS Subcommittee Meeting

May 9, 2012

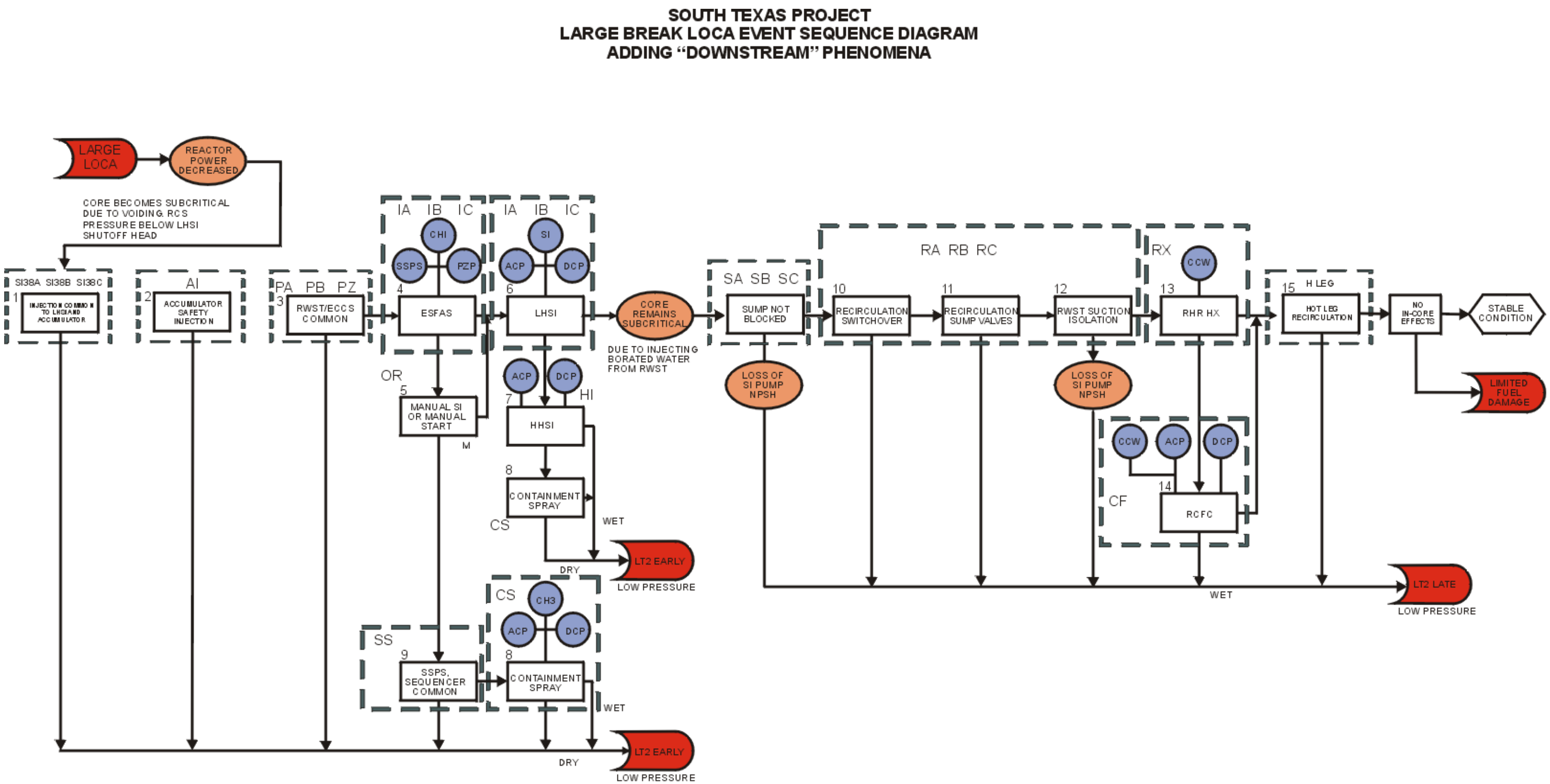
Risk Model builds off Model of Record

- 2011: reference model was STP_Rev6
- 2012: New Model of Record to be completed in May 2012 (STP_Rev7)
- 2012 GSI PRA model will build off STP_Rev7
- 2012 effort will be subject to 10CFR50 Appendix B procedures

Differences between MOR and GSI 191 PRA Models

- Sump blockage basic event moved from recirculation fault tree to unique top event
- Model structure changes
  - To reflect different perspective (e.g., success criteria for containment heat removal in MOR asks if at least minimum equipment is available; GSI 191 model considers number of fan coolers operating, etc)
  - Sequences added to represent in-core fuel blockage; boron dilution, air ingress
- MOR uses 'generic' sump blockage likelihood; GSI 191 Model uses detailed plant-specific evaluation (CASA GRANDE)

Modified Large LOCA ESD



RG 1.174 Considerations

- Comparison between as-is and RMI
  - Can bound delta CDF for STP by considering 'downstream' scenarios
  - This ignores any downstream impact from latent debris
- For at-power conditions only, in Region III
  - 2011 Uncertainty analysis suggests 95% confident in Region III
- Need to consider low-power conditions
- Need to consider DID

Results for Model of Record

MOR: STP_Rev 6 with truncation 1E-14					
Initiator Category	Sump Blockage Likelihood	Fuel Element Blockage Likelihood	CDF w/o Fuel Element Blockage	Frequency of Fuel Element Blockage	CDF w/ Fuel Element Blockage
RCP Seal LOCA	1E-5	0	1.55E-07	0	1.55E-07
Non-Isolable Small LOCA	1E-5	0	2.76E-08	0	2.76E-08
Isolable Small LOCA	1E-5	0	2.87E-08	0	2.87E-08
Medium LOCA	1E-5	0	1.09E-08	0	1.09E-08
Large LOCA	1E-5	0	9.86E-09	0	9.86E-09
Open SRV (one)	1E-5	0	4.00E-10	0	4.00E-10
Open SRV (two or more)	1E-5	0	7.62E-11	0	7.62E-11
All other initiators	1E-5	0	6.23E-06	0	6.23E-06
Total			6.45E-06		6.45E-06

Results for Model Supporting 191 Resolution

Current Model with truncation 1E-14					
Initiator Category	Sump Blockage Likelihood	Fuel Element Blockage Likelihood vs Number of operating ECCS Trains; 1 train/ 2 trains/ 3 trains	CDF w/o Fuel Element Blockage	Frequency of Fuel Element Blockage	CDF w/ Fuel Element Blockage
RCP Seal LOCA	0	0/0/0	1.33E-07	0	1.33E-07
Non-Isolable Small LOCA	0	0/0/0	2.68E-08	0	2.68E-08
Isolable Small LOCA	0	0/0/0	2.87E-08	0	2.87E-08
Medium LOCA	0	0/2.42E-3/8.94E-3	1.07E-08	2.20E-07	2.30E-07
Large LOCA	0	5.82E-4/5.91E-2/1.25E-1	9.82E-09	2.97E-07	3.06E-07
Open SRV (one)	0	0/0/0	3.89E-10	0	3.89E-10
Open SRV (two or more)	0	0/0/0	6.65E-11	0	6.65E-11
All other initiators	0	0/0/0	6.22E-06	0	6.22E-06
Total			6.95E-06		6.95E-06

Results of 2011 Quantification