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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

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REGULATORY POLICIES AND PROCEDURES SUBCOMMITTEE

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WEDNESDAY

APRIL 25, 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., William Shack, Chairman, presiding.

SUBCOMMITTEE MEMBERS PRESENT:

WILLIAM J. SHACK, Chairman

SAID ABDEL-KHALIK

J. SAM ARMIJO

DENNIS C. BLEY

CHARLES H. BROWN, JR.

MICHAEL CORRADINI (via telephone)

HAROLD B. RAY

JOY REMPE
MICHAEL T. RYAN
STEPHEN P. SCHULTZ
GORDON R. SKILLMAN
JOHN W. STETKAR

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

NRC STAFF PRESENT:
HOSSEIN NOURBAKSH, Designated Federal Official
KATHY H. GIBSON
RICHARD CHANG
JASON SCHAPEROW
RANDY SULLIVAN
TINA GHOSH
PATRICIA SANTIAGO
MARTY STUTZKE

ALSO PRESENT:
MARK LEONARD
RANDY GAUNTT
JEFF GABOR
PATRICK MATTIE
NATE BIXLER
ED LYMAN

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P R O C E E D I N G S

8:29 a.m.

CHAIR SHACK: The meeting will now come to order. This is the meeting of the ACRS Subcommittee on Regulatory Policies and Practices. I'm Bill Shack, chairman of this Subcommittee. Members in attendance are Said Abdel-Khalik, Sam Armijo, Dennis Bley, Charlie Brown, who will be here in a few minutes; he's been delayed somewhat, Harold Ray, Joy Rempe, Mike Ryan, Steve Schultz, Dick Skillman and John Stetkar.

Mike Carradini is also participating through the bridge line, and hopefully it will work better. We apparently have a new phone, so we'll see if that cuts down the noise. Also in attendance are ACRS consultants Tom Kress and Graham Wallis.

The purpose of this meeting is to discuss the state of the art reactor consequence analysis SOARCA project. The Subcommittee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate for deliberation by the full committee. Dr. Hossein Nourbaksh is the designated federal official for this meeting.

The rules for participation in today's meeting have been announced as part of a notice of

this meeting previously published in the *Federal Register* on March 28, 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, use one of the microphones, and speak with sufficient clarity and volume so that they can be readily heard. We have received no written comments or request for time to make oral statements from members of the public regarding today's meeting.

This happens to be the second consequences analysis that we've been discussing in the thing. We did spent fuel pool last time, and reactor consequences, severe accident consequences, not for the reactor. I just sort of want to reiterate, John Stetkar's motive.

It is what it is. So we're looking at the consequences of some selected sequences. We do want to look at the way those sequences were selected, that choice of scenarios, and again, I think another interesting conflict is what we mean by best estimate, in terms of something that has very large uncertainties, and hopefully we'll get some insight from that today.

We have a lot of material to get through

today, so I'm going to just keep these remarks short and what the staff did. Kathy Gibson at the Office of Nuclear Reactor Research, I think, wants to provide an opening statement.

MS. GIBSON: Hi. I'm Kathy Gibson. I'm the Division Director for the Division of Systems Analysis and Research, and my division was the lead for this project, although we've had many people from the NRC and also our colleagues at Sandia, Dycoda and other offices within Research that helped us with it.

Today, we plan to present work to you that has been done on SOARCA since we last met with you in June of 2010, and I think we've met with you, I don't know, about a handful of times over the course of this project. So we intend to cover what we've done since we last met with you.

Since that time, we completed a peer review, and we've published a variety of NUREGs documenting the SOARCA work for public comment. We recently conducted public meetings at the two plant sites, in Pennsylvania and Virginia, the Peach Bottom and Surry plants.

Over the past year, we've completed reanalysis based on input from the peer reviewers and also from the licensee. We interacted with our

external peer review committee and we incorporated NRC staff comments from throughout the NRC on the documentation.

Additionally, substantial work has been performed on the SOARCA uncertainty analysis, and I know you have great interest in that topic. In that one of the plants that we analyzed in SOARCA, the Peach Bottom plant, is similar in design to Fukushima, there's been a lot of interest in comparisons between what we predicted in SOARCA for Peach Bottom station blackout and the Fukushima site.

So we have done some comparisons there, and our analysis and Fukushima shows that severe reactor accidents, even when unmitigated, they proceed more slowly, and radiological releases are smaller than what we previously thought.

So there's more time for evacuation, and as a result, there were no early fatalities predicted in SOARCA or at Fukushima. SOARCA predicted the individual risk of latent cancer fatalities to be millions of times lower than the general U.S. cancer fatality risk, and well within the Commission's safety goals, and only time will tell, of course, with Fukushima.

Finally, SOARCA analysis shows that

successful mitigation will prevent core damage, or delay or reduce radiological releases. You will hear details about these activities today from NRC staff, our colleagues at Sandia and Dycoda, and a representative from the external peer review committee, and we look forward to your questions and comments.

I'll turn the floor over to Richard Chang, who's our project manager for this --

CHAIR SHACK: Richard, just before you start, Mike can you hear us? Can we hear you?

MEMBER CORRADINI: Are you talking to me, Bill?

CHAIR SHACK: Yes, talking to you. Okay, we hear you.

MEMBER CORRADINI: Yes, I can hear you, yes. There's a lot of paper shuffling, but that's pretty normal.

CHAIR SHACK: Okay. Well, you're not making a whole lot of noise so --

MALE PARTICIPANT: Yes, it's a big improvement.

CHAIR SHACK: Well, you told me to be quiet, so I'm trying to do my best.

(Laughter.)

MR. CHANG: Thank you. Hello. I'm Richard Chang. I'm the project manager for the SOARCA project, and one of the folks working as a program manager for it. I'll be providing an overview of some of the previous SOARCA briefings to the ACRS.

I will also be discussing the SOARCA conclusions for Peach Bottom and Surry, as well as providing an update on the project status from when we briefed you last, in the summer of 2010.

In 2006, NRC briefed the ACRS on the objective of SOARCA, which is to develop a body of knowledge on the realistic outcomes of severe reactor accidents.

We also briefed the ACRS on our approach, which was to use detailed integrated modeling of accident progression within MELCOR, and the modeling of off-site consequences within MACCS, to predict the likely outcomes for a significant but still remote core melt accidents.

In the 2007 time frame, NRC staff briefed the ACRS on scenario selection, MELCOR and MACCS improvements as part of SOARCA, SOARCA insights, as well as mitigative measures, and then began the process of the peer review in the late 2008 and 2009 time frame.

In June 2010, NRC staff updated the ACRS on the status of the peer review committee interactions, as well as the status of the licensee fact checks and NRC staff comment resolutions.

I guess with the first bullet, SOARCA demonstrates the potential benefits of employing 10 C.F.R. 5054 mitigation for the scenarios analyzed. When successful mitigation is assumed, major core damage is averted for all scenarios, but the Surry short-term station blackout and its variant, the short-term station blackout with thermally induced steam generator and tube rupture.

For the mitigated short-term station blackout at Surry, containment failure is delayed by an additional 41 hours, and for the mitigated thermally-induced steam generator tube rupture at Surry, containment failure is delayed by an additional 46 hours.

However, since this is a bypass scenario, you have small releases beginning at approximately 3-1/2 hours. In addition, the SOARCA analyses indicate that all modeled accident scenarios, even if operators are unsuccessful in mitigating the accident, progress more slowly and release much smaller amounts of radioactive material than early studies.

The earlier study that I'm referencing is the Sandia siting study, mainly Sandia siting study Source Term 1. As a result, public health consequences from severe nuclear plant accidents are smaller than previously calculated.

CONSULTANT KRESS: Is this primarily due to the change in the rate that things get released, or is there some difference in the source term other than being slowed down?

MR. CHANG: Sure, and part of it is because, and I'll go into it in the next slide, if you'll allow me. Sorry. I guess ultimately, to answer the question, SST-1, the release begins at approximately an hour and a half, whereas aside from the thermally-induced steam generator tube rupture, which begins at around 3-1/2 hours, you have the Peach Bottom short-term station blackout, and just to define what SOARCA defines as short-term station blackout as, it's the loss after the seismic initiator. It's the immediate loss of AC and DC.

The next quickest one would be Peach Bottom short-term station blackout, and the release begins at approximately eight hours.

CONSULTANT KRESS: So there's a difference in timing and the amount in the source term?

MR. CHANG: Yes, yes, and just to get into it a little bit, for the unmitigated long-term station blackout at Peach Bottom and Surry, core damage begins at 9 to 16 hours respectively, and releases begin at 20 hours for Peach Bottom and 45 hours for Surry.

Now for the unmitigated short-term station blackout, which is immediate loss of AC and DC, releases begin at eight hours for Peach Bottom and 25 hours for Surry. The ISLOCA releases, which is also a bypass scenario, for the unmitigated scenario, the release begins at approximately 13 hours. Like I stated before, SST-1 begins at one and a half hours.

Now in terms of magnitude, for SST-1 iodine is approximately 45 percent and cesium is approximately 67 percent, whereas for the SOARCA scenarios, it's a maximum of 15 percent for the ISLOCA scenario, ten percent for I have -- for the short-term station blackout for Peach Bottom, and around two percent for the other scenarios, or it may be less.

Now for cesium, they're all approximately two percent or loss for the SOARCA unmitigated scenarios. Related to that, the delayed release is calculated, as Kathy said before, to provide more time for emergency response actions, such as evacuating or sheltering in place.

Due to this, latent cancer fatality risk is generally dominated by the return criteria, which is, I guess, the long-term latent cancer fatality risk.

MEMBER CORRADINI: Can you repeat? I'm sorry. You said the latent cancers are dominated by what again? I'm sorry.

MR. CHANG: By the return criterion or the habitability criterion.

MEMBER CORRADINI: And you'll go back into that later, I assume?

MR. CHANG: Well, I can touch on it right now. What the return criterion, it's set by the state, and hopefully Joe will correct me if I'm wrong.

It's set by the state, and it's a state limit for when folks, after hypothetical severe accident, when folks can return to live on the land, or use it for its original purposes.

MEMBER CORRADINI: So if I -- just let me push the point. So you're saying in Virginia, there's one policy and in Pennsylvania there's another policy?

MR. CHANG: There are actually two separate -- I believe Virginia follows EPA guidelines, which is two rems per year for the first year, and then after that, 500 millirem per year thereafter,

okay, and Pennsylvania has a policy of 500 millirem per year throughout.

MEMBER CORRADINI: Okay, thank you.

CONSULTANT KRESS: Is this associated with the linear no threshold assumption also, in the sense that you're going to calculate the consequences of them being there forever after they come back?

MR. CHANG: I believe so, but hopefully someone else can step in to answer more clearly.

MEMBER SCHULTZ: To ask it differently --

MR. CHANG: Sure.

MEMBER SCHULTZ: You're presenting some conclusions in general qualitative terms?

MR. CHANG: Yes.

MEMBER SCHULTZ: Are those conclusions based on a linear no threshold model, or are they assuming a threshold?

MR. CHANG: They're based on LNT. But we also have a couple of threshold calculations as well within SOARCA.

MEMBER SCHULTZ: And I understand we'll see those later?

MR. CHANG: I believe so, but I could talk through them a little bit now. Sure.

MR. SCHAPEROW: We don't have any

presentation material on that today. We did show that at the last ACRS meeting. The off site consequences are a lot smaller with a threshold model, as expected, up to a factor of 100 smaller, especially because, you know, the threshold model in some cases is above 500 millirem per year.

MR. SULLIVAN: The return criteria, I believe you asked a question -- Randy Sullivan, part of the SOARCA team. You asked about the return criteria. Perhaps there's a nexus to linear no threshold.

I think the state just took a conservative view or what they considered to be a conservative view, and reduced the habitability criteria from the EPA recommendation of two rem in the first year, 500 millirem thereafter, to 500 millirem every year. That's not a decision we were involved in. It just simply is what's written into the state plan.

I suppose there is a nexus too LNT in there somewhere, but it had little to do with SOARCA. We just followed the state plan.

CONSULTANT WALLIS: Can I ask you something about the scenario selection?

MR. CHANG: Sure.

CONSULTANT WALLIS: Earthquakes look

pretty bad. The stuff now on the Internet about huge solar flare-ups, which supposedly can knock out the grid, damage may be to electrical systems in the plant, and maybe worse, knock out all communications and damage electronics on vehicles so people can't evacuate.

Is this a fantastic scenario, or have you somehow evaluated it?

MR. CHANG: As far as I'm aware, I don't think we evaluated solar flares. But for that one, I might have to defer to Marty Stutzke for scenario selection.

MR. SULLIVAN: This is sort of odd. Randy Sullivan, and I happen to be working on a task force at NRC regarding solar flares. We have a petition for rulemaking regarding what to do about solar flares.

The technical evaluations of solar flares are not conclusive as far as we're concerned. We see some agencies saying it's a "oh never mind," and others thinking it's a total disaster.

What we are fairly sure of, and we prefer to have deeper technical work to our standards before we made our mind up totally, is that emergency diesel generators survive through this event, no matter what.

The issue comes down to is their fuel

supplies in a societal degradation, in a state of societal degradation. So we're focusing on, you know, most plants have seven days' worth of fuel supplies and probably more. We're focusing on how to make sure the generators run as long as they need to run, should there be a sustained outage.

CONSULTANT WALLIS: That's very good for the plant, but how about the people? I mean can they learn about it and can they evacuate?

MR. SULLIVAN: They don't have to evacuate if the generators are running.

CONSULTANT WALLIS: No, that's right. But if there are releases --

MR. SULLIVAN: But there's no releases if you have electricity or batteries, for that matter. So that's what we're focusing on, is plant safety.

CONSULTANT WALLIS: But you're looking at it. That's very good, and perhaps it will be an appendix to the SOARCA or something?

MR. SULLIVAN: No, it will not. It's a separate issue.

CONSULTANT WALLIS: It will not? A separate issue.

MR. SULLIVAN: Under a petition for rulemaking, and I don't know if that comes before you

or not. I haven't got that far.

CONSULTANT WALLIS: Well, I can tell people who work, who ask me about this, that you're working on it.

MR. SULLIVAN: We are working on it, and from at least a staff perspective, we consider this to be a potential natural disaster, not as serious as a tsunami, but something that could affect society at large, and we want to make sure that our licensees are prepared.

CONSULTANT WALLIS: Thank you.

MEMBER CORRADINI: I had a question, if I could just go back to Slide No. 5.

MR. CHANG: Sure.

MEMBER CORRADINI: So you in Slide 5, you compare it to SST-1. I'm curious. There are other release categories in the '82 report. So from a comparison standpoint, what do these releases compare to, because clearly they don't compare to the SST-1. They're much lower.

MR. SCHAPEROW: This is Jason Schaperow on the NRC staff. The SST-1 source term in the siting study is a release from a reactor accident where there's no mitigation. It is an accident which basically injection is lost; the core melts and the

release happens.

The other source terms in the siting study have mitigation. They have, for example, the SST-2 has containment sprays. So what we're trying to do is we're trying to compare our calculations without mitigation to what's in the siting study. So that's why we chose SST-1.

MEMBER CORRADINI: And so just to be clear, without mitigation, implying still some sort of containment failure and physical processes are the only thing that make the difference, or are there also -- I thought embedded in the green and the red lines and the orange lines are operator actions?

MR. SCHAPEROW: Yes. There are operator actions in there. These are actions that the operators would take in accordance with their procedures. But these are actions that they would take regardless of what kind of accident it was, but these are not actions that would prevent the release.

These are just actions that they would take under any circumstances. So if we had an accident -- if we were back in 1982 doing this analysis, and we did it in a more detailed fashion, we would include those operator actions as well, I believe.

MR. CHANG: Those are not the --

MEMBER CORRADINI: Okay. I'm just trying to compare apples to apples, and I just want to understand why you chose this as the comparison.

MR. SCHAPEROW: Yeah, and actually as an example, when I get into the ISLOCA discussion a little later this morning, I'll try to make that very clear, as to what we mean by mitigated and unmitigated.

MEMBER CORRADINI: All right, thank you.

CONSULTANT KRESS: That's surprising me, the 1982 reference, because I thought those mitigating features were a result of what you would do in case an airplane crashed into the --

MR. SCHAPEROW: Yes. Some of the mitigating features are just operators, like the pressure of taking the steam generators and using them to start cooling down the plant. Now that's not going to prevent the release, but in the old days, we didn't necessarily take all that stuff into account. So our earlier realistic analysis of a release didn't include those actions.

CONSULTANT KRESS: Mitigating versus unmitigating has to do with the standards that you develop --

MR. SCHAPEROW: Has to do with the -- that's right. Has to do with new stuff, B.5.b, the new accident management guidelines, not the existing procedures from before.

CONSULTANT KRESS: Okay, thank you.

MR. SCHAPEROW: Anyway, I hope to clarify again a little bit more when we talk about the ISLOCA.

MR. CHANG: And related to that, individual early fatality risk is essentially zero for all SOARCA scenarios, and just on this slide, we're on Slide 6, SOARCA's absolute, individual latent cancer fatality risks are millions of times lower than the U.S. average annual cancer fatality risk, from all causes, which is approximately 2 times 10 to the minus 3. The safety goal is shown to provide context for the SOARCA scenarios.

Now I just wanted to provide an overview of a status update from the last time we briefed you, and since the last time we briefed, we have new or revised analyses, based on input from the plant fact check comments, as well as the external peer review committee comments.

We've reanalyzed the short-term station blackout, but at Peach Bottom we've reanalyzed the short-term station blackout, with RCIC blackstart,

assuming blackstart at one hour instead of ten minutes previously.

The licensees came back to us as part of their plant fact checks and said they didn't think ten minutes was a realistic time estimate. Mark Leonard will discuss our additional analyses on SRV failure criteria, based on comments from the peer review committee, as well as traversing in-core probe systems at Peach Bottom.

Randy Gauntt will discuss the distribution of risk to barium and cerium in the short-term station blackout versus long-term station blackout scenarios at Peach Bottom. At Surry, as Jason mentioned before, we reanalyzed the Surry ISLOCA scenario, based on a licensee fact check comment, and Jason Schaperow will discuss this in more detail.

We also adjusted the core pump flow rates in the mitigated long-term station blackout, but ultimately the results for SOARCA do not change. There's no core damage, and that was based on a licensee plant fact check comment.

We also added an appendix, which compares SOARCA to Fukushima on a couple of specific areas, which Randy Gauntt will discuss in more detail this afternoon. We also completed our peer review

committee interactions. Jeff Gabor, who's a peer review committee member, will provide a peer review committee perspective.

We have begun work on the uncertainty analysis, which Tina Ghosh will discuss later today, and Pat Santiago will discuss the public comments we've received on SOARCA, as well as the proposed future SECY paper for the SOARCA project, and I think that was my last slide.

MALE PARTICIPANT: Sorry.

MEMBER REMPE: That's okay. Rather than pick on the people who did the plant-specific analysis presentation, when I was going through the material, those two reports had been issued as, I guess, final documents, whereas the other documents we were given were draft versions.

But when I was going through those documents, there were a lot of things in them that are detracting from the quality of the report. References that are cited but not listed; axes, legends that imply the external vessel temperatures higher than the internal vessel temperature.

There was something about condescending steam, I believe, in one.

(Laughter.)

MEMBER REMPE: But I guess I would like to inquire whether the staff would be willing or consider reissuing those reports and having some cleanup.

MR. CHANG: In regards to Volumes 1 and 2 of the SOARCA reports?

MEMBER REMPE: The Peach Bottom and the Surry-specific analysis.

MR. CHANG: Yes. What we have done, related to that, we got a number of comments from one of the peer review committee members, as well as Karen Vierow. She actually might be listed on this goal, on specific editorial and comments with regard to axes, graphs, as well as things spellcheck didn't catch, that the staff may have missed in its review.

So we've been incorporating those comments, and we do plan on having a Rev 1. So yes.

MEMBER REMPE: I'll make sure the staff gets the same comments to you, some nitpicks that they still --

MR. CHANG: They're important, and we're aware of that, and we have received some comments from the peer review committee members and various members of the public, that we're addressing those comments. So thank you.

MEMBER REMPE: Good.

MEMBER ARMIJO: Once you've done that, will you reissue chapters of the SOARCA report?

MR. CHANG: The intention is what we'll do is, and I think Pat will send it out, but I think the intention is is to be able to issue a Rev. 1 in the near future, at the same time that NUREG-1935 is finalized. So that's the intention.

MR. CHANG: And we've, Mark Leonard and I will run the slides.

MR. LEONARD: My name is Mark Leonard. I've been responsible for the Peach Bottom MELCOR analysis for SOARCA. I'm a contractor for Sandia.

So what I'm going to discuss this morning, if you go to the first slide please, are not the comprehensive front to back analyses or details of the individual calculations, but really focus on three particular items that came up in the last couple of years, as a product of the peer review committee's comments on the work that we have done.

Three areas that they focused on several times actually, that permitted us to reexamine some of the assumptions that went into the development of the MELCOR model itself, the scenario description, and those are itemized here.

I'll talk about the work that we've done

since the peer review comments were received, and what effects they had on the baseline analyses.

The first is the effects of safety relief valve seizure, and when I use that word, I mean sticking in the open position. We will not be talking about failure in the closed position today, primarily because if the lead SRV, the one that has been cycling for most of the early portion of the accident, and for some reason randomly or by other mechanism it closes or refuses to open on a demand, then simply the next relief valve, and the next one and the next one.

There are 13 in total in Peach Bottom. So the closed failure is not one that we've been particularly concerned about. Opening, though, does have an important impact on accident progression and the criteria that we used for deciding when a valve would stick in the open position has been among the comments that the peer review committee provided. So I'll talk about how we've reexamined that.

The second is an item that Bob Henry in particular had mentioned in the peer review comments, which was some observations he had made early on his analysis of TMI, as well as early, I won't necessarily call it an analysis. It's somewhat speculation, but there's also some evidence in terms of activity that's

been measured in various parts of the reactor building at Fukushima, that would suggest that there might have been a containment bypass mechanism that leaked at least some activity to the building prior to the leakage to the environment.

The question is what pathway might that have been. He had a suggestion for that, especially in a BWR where there are -- there's one particular system that is a in-core instrument tube penetration system that I'll talk about, and we've looked at the potential for leakage through that system and changing the results that we've calculated so far in terms of the source term.

The third item is the effect of barium and cerium as particular species that contribute to the offsite releasive activity. Historically, those two species are not major contributors to offsite dose, primarily because their quantities released to the environment have been so small.

They are not the dominant contributor, but they are contributors to dose in the SOARCA calculations. So in particular, their release differs between the short-term and long-term blackout, and because they have a contribution to dose, they have raised some interest in terms of why those releases

appear to be now significant when they've not been in the past. So we'll talk about that as well.

CHAIR SHACK: Let me just ask a question on essentially the verification of the MELCOR model, since all this kind of depends on MELCOR calculations.

When I look at the best practices book and then that report in the 1935, everything was referenced -- a lot of these release things are referenced to PHEBUS-type results, especially FPT1.

Again, these are all PWR kinds of things. I mean, you know, it's PWR control logs. Apparently the control log chemistry seems to have a strong effect on some of the releases. I see very little reference to the FPT3, which has the boron carbide, you know, the PWR-type thing.

What is the assumption that all these release values that we get from the PHEBUS PWR calculations are really relevant to Peach Bottom?

MR. LEONARD: I think I'd defer to Randy Gauntt for that, who did most of the assessment for the release models.

MR. GAUNTT: I guess in addition -- this is Randy Gauntt. In addition to -- we do lean heavily on the PHEBUS test for integral verification of the melt progression modeling and the source term. We

also lean on the -- we go back to the Oak Ridge HI/VI test, and as well as the VERCOR tests.

These are small pellet tests, where the measure the release of different --

CHAIR SHACK: Well, let me just -- the daily report I defined on the FPT3 test indicated a rather quite striking change in the release of gaseous iodine. So I mean there was a marked change.

MR. GAUNTT: I mean that's a good, you raised a good issue, and I'd say FPT3 is, that's the latest of the PHEBUS test. I would describe it as being kind of on the research edge. The belief is that the boron that's released is somehow tying up with cesium, and resulting in less iodine being tied up with cesium.

So at least during the core damage phase, as this iodine's coming out, they were seeing, you know, some larger tens of percents, perhaps, of iodine coming out in gaseous form. I believe we may touch on that a little bit in the uncertainty.

CHAIR SHACK: Yeah. Like I said, now is that addressed in the uncertainty analysis?

MR. GAUNTT: I want to say yes, but I'm not sure I'm recalling that precisely.

MS. GHOSH: Yeah. This is Tina Ghosh. We

did take more of the PHEBUS test into account and coming up with -- we have five separate bins for the chemical forms of iodine and cesium, and we are sampling larger fractions of gaseous iodine, which we didn't in the best estimate. So we're looking at the potential effects of taking that into account.

CHAIR SHACK: And do we have enough results from that series to know that that's the only effect that we have to be concerned with, the FPT3 analysis?

MR. GAUNTT: FPT3?

CHAIR SHACK: Yeah. I could only find a brief summary report. I don't know what the status actually is.

MR. GAUNTT: My recall on FPT3 is the big outlier there is the behavior of that gaseous iodine, and it seems to me something that is slightly transient, in that as this iodine goes into the containment, yeah they are seeing initially 10, 15, 20 percent gaseous iodine.

But in time, that gaseous iodine seems to fall away and approach a kind of steady state level. The belief is there's some chemistry going on within the containment, that is attempting to, you know, produce a more or less steady state in the airborne

iodine.

So that's where the research front is right now, and we do not have models that currently capture that chemistry.

MEMBER ARMIJO: Are you pretty certain about the initial state of iodine at the initial melting?

MR. GAUNTT: From PHEBUS?

MEMBER ARMIJO: Yeah.

MR. GAUNTT: You're probing my memory here, because somebody may -- if somebody knows this better, speak up. But of the airborne iodine, there is elemental, and I think what's coming out is elemental. There also is a hyper-fine particulate of --

MEMBER ARMIJO: Aerosol type.

MR. GAUNTT: --of iodine or something that's hard to differentiate from gases, because it is so small, and then there's the organic.

MEMBER ARMIJO: Are we going to talk at some later point in more detail on that?

MR. GAUNTT: I think in the uncertainty study, we don't differentiate between elemental and organic. But we do, you know, differentiate between particulate and gaseous.

MEMBER REMPE: But in general, the database for BWR melt progression phenomena is considerably less than PWR. It's not just for protecting fission products, but just how the fuel assembly will degrade, how it's held up in the core plate, interactions on the lower head, the drain line as well as instrument tube failures.

So I think that's important to keep in mind. There's a lot more uncertainties. Is it like less than ten BWR-specific test, as opposed to over 40 for BWRs?

MR. GAUNTT: You may have surveyed that number. I know I did one or two myself decades ago. The Germans did a couple of them as well. But we have been pretty PWR centric in our model development. That said, I feel that we have had opportunity to be studying Fukushima. I'll try to say a little bit about Fukushima a little bit later on, and I'm, you know, not at all discouraged with what, you know, how this compares to what we think we know about the --

MEMBER ARMIJO: The reason I brought up the question is, you know, I'm not a severe accident person, but I did quite a bit work on fuel, and what we found in a lot of hot cell examinations of BWR fuel is that the iodine is, with rare exception, totally

consumed as, tied up as cesium iodide.

I don't know if it decomposes when you go through a melting. I don't know any of that stuff. But it seems like if you weren't seeing it in the form of an iodide, how did it get out?

MR. GAUNTT: Our assumption in MELCOR generally is that the iodine will tie up with the cesium, and that's our predominant view of things.

This business in FPT3, you know, is -- I don't know where the chemistries is happening, whether it's happening right at the release site or after it's released from the fuel and sort of mixing up with vapors from the control blade, the boron carbide products.

But the theory is, that's been put forward is that the cesium is preferring to tie up with boron, and therefore leaving iodine without a dance partner, so to speak.

CONSULTANT KRESS: There's not that much boron in there, compared to --

MEMBER ARMIJO: BWR has got a lot of boron carbide. I don't know if that decomposes to --

CONSULTANT KRESS: Didn't VERCOR's program, aren't they looking at this? Did you check that?

MR. GAUNTT: There's a follow-on from VERCOR called VERDON, and you know, it's moving forward. I don't know what's been done there yet. In MPT-3, they were examining the rod-type form of the boron carbon. So it's a little different from the blade and the boiler.

I think it probably encourages more oxidation of boron carbide and evolution of boric acids or whatever it is --

CONSULTANT KRESS: Part of an increased surface area.

MR. GAUNTT: Yeah, and they are -- in the boiler, the boron carbide mixes very intimately with the steel, and it principally, you know, becomes an iron boron liquefied thing that runs away.

Whereas in the PWR, it's actually a pellet stack inside a tubing, and so I think there's potential for that to stand up longer and have protracted exposure to steam, whereby you can get more gaseous forms evolving off. That's my understanding. I don't think the models for this are, you know, are very well developed at this point.

CHAIR SHACK: Just pursuing Joy's question a little bit, again the lower head models are all PWRs, and these lower heads look a little different.

Is that accounted for in the uncertainty analysis?

MR. LEONARD: Not directly. Lower head failure mechanisms are not one of the parameters that we're currently examining in the uncertainty study.

MEMBER CORRADINI: Is there -- may I just ask a question to follow-on? Is there any work that the staff sees necessary to examine that, because the other thing that came out in previous discussions was timing of lower head failure, and whether it be a BWR or a PWR, the timing relative to any sort of mitigative actions can have a large effect.

So does the staff feel that this is an avenue that's worth pursuing, or just too uncertain to even begin, and you're taking a conservative approach?

MR. LEONARD: I would say one of the limitations is an availability of an analytical model. The tool that's available in MELCOR at the moment, excuse me, for representing the mechanism for a penetration of any type failure is entirely parametric.

It does not represent the meaningful heat transfer and material movement processes that we suspect are dominant mechanisms for tube ejection or any of other mechanisms that have been looked at in the past. It's a very parametric model.

So from an uncertainty perspective, it certainly could be examined, but we would be looking at very general global parameters, not varying input parameters at the same level of fidelity that all the other parameters are being buried.

And to some extent that would be speculative, that would not be based on other even external detailed analyses. So it's an area that --

MEMBER CORRADINI: Just to follow up, Mark, if I might. So what you're really -- if I could say it differently, you're saying there no data to hang your hat on, so you install the parametric model, in which you dial in a result and then made sensitivity calculations on that result?

MR. LEONARD: That's what's currently done, that's correct, and we rely in many respects on the limited available experimental information, as well as some of the analytical work that Joy had been involved with in the past, as a basis for making some judgments about what reasonable values for those parameters might be, and trying to provide some informed judgment about whether global creep rupture would --

If we represent lower head failure by calculating global creep rupture in a reasonably

detailed way, that that's adequate, that we would not be far off in a time of debris relocation to the drywell floor.

MEMBER REMPE: Let's talk about your vessel failure prediction. Did you predict an area, so you have a bottom, a hole in the bottom? Did you predict a tear like saw at the Sandia test, that depressurizes?

I never found an area in this report, and was that area based on an amount of vessel steel that was heated up in the relocating debris, or did you just have a default value, this is the area that they altered?

MR. LEONARD: There is a default value.

MEMBER REMPE: But it's not really based on data or analysis.

MR. LEONARD: Well, you're right in the sense that it is the area that is used is not intimately connected to the calculated temperature distribution. So those two things are disconnected, that's right.

The area is based on the geometric distance between neighboring control rod drive assemblies. I mean it has a physical interpretation, but it is not explicitly connected to the temperature

distribution that the code calculates for creep rupture. That's true.

MEMBER ARMIJO: Not the bottom drain line?

MR. LEONARD: And the drain line is not represented at all.

MEMBER REMPE: At all.

MEMBER ARMIJO: Probably the quickest thing to melt down.

MEMBER REMPE: I think someone said that in the previous reports.

CHAIR SHACK: It seems strange to have it disconnected from the temperature distribution.

MR. LEONARD: The temperature distribution, make sure I say this correctly, the temperature distribution is the basis for calculating the global creep of the hemispherical head.

But the initial area that is created is not proportional, for example, to some prescribed temperature profile. The area is specified as part of the model definition, up front.

MEMBER CORRADINI: So if I could just -- I'm sure we're beating up on this, and you've already told us you're not going to do any of the uncertainty. But let me just ask a thought question. If it were in the uncertainty, and you then considered a smaller

hole, let's take for Peach Bottom the drain line, what would you expect to be the qualitative result of that?

In other words, does that change the result significantly? Does it create a larger release, a smaller release, a later release? Where do you go qualitatively with knowing that you've got potential holes that are small and may fail early?

MR. LEONARD: It affects the initial drain rate of debris out of the lower head into the containment floor. The drain rate is proportional to the whole size, but the whole size of blades becomes larger as the molten material pours through it.

Previous parametric work, sensitivity studies have been quite a while ago though, as many as ten years ago, I think, had tried to look at that, in terms of was the, if you will, the time frame from debris relocation from the lower head to the floor substantially changed if you used a small initial area that a blade, rather than a larger area initially.

And the outcome of that parametric study was that the difference was not significant in terms of the amount of time it took, especially for a BWR Mark I, with liner melt-through being the dominant containment failure mechanism.

Did it affect the time at which, or if you

will, the time difference between lower head failure and liner melt-through failure, containment and the beginning of a release to the environment? The conclusion of that early work was that the difference was not significant.

MEMBER REMPE: And they considered material solidifying when it hit the liner and remelting and all that type of phenomenon?

MR. LEONARD: The answer is no, that it's time, perhaps, to revisit that parametric work, because the way in which we represent both the in-vessel material distribution, the debris bed itself in the lower head prior to failure, that material distribution is different now than it was in the way we represent it, even within MELCOR say ten years ago.

And in particular, the method used to calculate lateral debris movement across the drywell floor is very different. It's much more mechanistic now than it was at the time. So it's probably, I wouldn't disagree that it's time to revisit that parametric and see if that's something that we --

CONSULTANT WALLIS: Well falling out on the floor, you do have an analysis. Is there any data, when you talk about liquidus and solidus of concrete, and I'm not sure that's relevant. When

fluid flows along the top of the concrete, does it care what the solid is? I mean I'm not -- is that based on data, on some sort of assumptions that may or may not be appropriate?

MR. LEONARD: This is in terms of the --

CONSULTANT WALLIS: The stuff flowing along the floor and getting to the liner.

MR. LEONARD: Right.

MEMBER CORRADINI: There's data, Graham. There's data from -- the Europeans have done a number of experiments. I can't remember the name of the test series.

They've done a number of experiments in terms of looking at various super heats of melt flowing across a concrete floor, because they were concerned of a similar fashion.

And also in the EPR, they've actually designed it to encourage spreading. So I can't remember the name of the test series. Joy may remember.

MEMBER REMPE: But I would almost think that it would be a direct hit on the liner actually. So I'm a little puzzled. Isn't that what usually they assumed, was that it came out of the vessel and it did attack the liner directly? So it's hitting that --

it's carbon steel, right, the liner?

MR. LEONARD: It is, but the liner is several meters away. You have to --

MEMBER REMPE: It's if you have a bottom hole, and it has to go out --

(Simultaneous speaking.)

MR. LEONARD: No, within the -- for the BWR, the reactor vessel is supported by a cylindrical concrete pedestal that has a doorway in it. So the debris has to accumulate within the pedestal to a depth that now can flow laterally out the doorway, across the floor and then touch the drywell liner.

Under the baseline assumptions that we're using in terms of the criteria for movement, I mean that happens within a matter of minutes, 15 minutes perhaps. That's an average value for us. So it is fairly quick.

MEMBER CORRADINI: I guess, just since I started this digression, if you have a logic that makes sense to you guys, that while you don't want to do this within the uncertainty analysis, it would seem to me, at least in the uncertainty document, you ought to have a discussion that says this is not included and this is why it's not included, and leave it at that.

The reason I brought it up in this regard is I can understand there's many things that you want to do in the uncertainty analysis, and this may be lower priority. But there ought to be at least a short discussion explaining why you think it is, based on either experimental or past parametric analyses.

MR. LEONARD: That's a good suggestion, and repeating what I said earlier, that one of the motivations for not representing it is because we don't have the analytical model foundation for that process, to the same level of fidelity that we have for the other ones that we're representing in the uncertainty study.

So it was difficult to decide how exactly you would define the parameters to vary.

CHAIR SHACK: I mean but that's one of the problems I have, is that you seem to fit the analysis on what you can do, and you know, then you know, okay.

I look at this scenario. I can't do a best estimate analysis of it, so I forget about it and I do this one over here that I can analyze.

Well that doesn't seem much of a criteria for selection.

MR. LEONARD: I don't want to defer, in terms of ejecting this comment away from me, but it

would be helpful --

(Laughter.)

CHAIR SHACK: Oh yes, you do.

MR. LEONARD: When Tina comes up later today, she's going to talk a little bit about the parameter selection process. How did we go about selecting what could and couldn't be done, what should and shouldn't be done. If we can revisit that then, that would probably be helpful, because my guess is that the lower head failure mechanism is not the only one that we could offer. There's other issues that could/should be --

CHAIR SHACK: Well I mean, it is a larger question of how you address model uncertainties, and sometimes you can address model uncertainties in these parametric kind of calculations that you seem to be doing. Other times, you can't, but I want to make sure the uncertainty report doesn't just analyze the uncertainties that I can analyze.

At least if you can't analyze the other uncertainties, you ought to recognize that they're there, and they can in fact be perhaps at least as important as the ones that you are analyzing.

CHAIR SHACK: The only reason I bring up the model or the limitations of the model is that it

was one of the, one of the criteria that was used to decide which parameters could and should be represented, so you didn't unfairly bias the effects of the uncertainty in one parameter against the effects of another.

The only reason why one might appear to be more important is because you're treating one extremely simplistic, with a very wide range of uncertainty, as opposed to one that you have a very detailed mechanistic model for, and you know the variance of that particular input parameter.

That was a concern, especially when it came to models like penetration failure, where the mechanistic model is very, very simple. It's almost --

CHAIR SHACK: I think we can understand that, but in the uncertainty analysis, we ought not give the impression that we've covered all of the things that are important if we haven't, and in fact we ought to at least catalogue them and give some judgments --

MR. LEONARD: And that's a fair summary.

CHAIR SHACK: --about where it could extend.

MR. LEONARD: And I think Tina will talk

about others, that in the process of running the initial spectrum of calculations, we discovered several things that were surprises, things that turned out to be more important than we would have expected.

So some refined analysis will probably fall out of that as well. So yes, it's been a learning process, that's for sure.

MEMBER REMPE: And claiming that the vessel failure is a higher fidelity, when the temperature distribution is not connected to the whole size doesn't quite jibe with me.

MR. LEONARD: Yeah. Okay. So if I've successfully deferred any more questions on uncertainty until later this afternoon, and we'll come back to it. It's a good discussion.

The next slide, please. The first of the three topics that I mentioned earlier is the outcome of the peer review process. It had to do with the criteria for the cycling safety relief valve to stick in the open position.

This failure mechanism, at least in terms of the detailed calculation of axiom progression in a MELCOR context, is something that's been done for quite a while, in terms of the history of NRC analyses of -- especially station blackout, long-term station

blackout and accident progression.

But the criteria for defining when the valve sticks has always been a little soft. It's not clear even what the mechanism should be that could be represented, and how do you mathematically model them within the code. That's been evolving, and it was a good discussion with the peer review, in the sense that we were able to discuss the two primary mechanisms that are currently represented in these calculations.

The first referred to is the stochastic failure model, and that's one in which we simply use a specified failure rate, a random failure rate that comes from classic PRA databases. In the best estimate analysis, we take that failure rate, invert it to define the expected value for the cycle, the average cycle at which the SRV might stick in the open position, and we simply define that number as a number of cycles, cumulative number of cycles at which the SRV would stick.

CONSULTANT WALLIS: Doesn't it depend on for how long it's open, because that determines the heat transfer.

MR. LEONARD: That's the second mechanism.

CONSULTANT WALLIS: Yeah, but I mean

number one, it's not just number of cycles. It's something to do also with the characteristic of the cycle. If it's open/shut, open/shut, it's very different from staying open for a minute and shutting.

MR. LEONARD: That's right. Again, when we talk about the uncertainty, this parameter in particular is one of the major uncertain variables, that when we hear the initial results of the uncertainty analysis, you'll learn how the effects of uncertainty in the failure rate have had a very, very important effect on the outcome.

MEMBER BLEY: Mark, I don't remember details, but back 20 years or more ago, industry did some extensive testing, I thought. Now I don't know if they were done at temperature. I thought they were, and did you guys go back and revisit that data?

MR. LEONARD: There are two databases that we reviewed. One is the NUREG/CR that's the -- Marty Stutzke might remember the number. I don't recall off the top of my head, but it's the generic database for all the components, one of which are the SRVs, and it has the average failure rate for failure to open, failure to close.

We reviewed that, and in fact that is the primary basis for the distribution that's put into the

uncertainty analysis. Another one is a study that was done, that reviewed in more detail than just the global failure rate, which is just -- which doesn't take into account at which cycle did the observed failures actually occur.

Did it fail on the first time, the second time, the 300th time, and it tried to decompose failure rate or correlate failure rate to the cycle at which it failed. Is the failure rate higher or lower as it begins to cycle, or does failure accumulate with time? It attempted to examine that.

The difficulty is that the observations of valve failure, although there are large numbers of them, not many of the LERs actually document at which cycle did the valve fail, so that the available information helps us decompose that failure rate as a function of number cycles is not particularly good.

Although this one study that I believe Idaho did, provided a recommendation for failure rate on the first cycle, the first few cycles, and then a different failure rate when you get into the hundreds. In our case, for the best estimate calculation, we had to choose a failure rate.

So we used a nominal value that came from the plant-specific PRA, and are using that for the

best estimate single calculation. The uncertainty study considers that number to be a very wide distribution. So a full spectrum was considered.

But getting back to the question of temperature, that gets to our second criterion, both of which operate in parallel. So both mechanisms are always available, depending on which one would occur first.

The random failure or stochastic failure is always operative, and in parallel to that, we're calculating the temperature of a representative internal component to the valve. It's primarily the valve stem, the most exposed component, moving component within the valve, and we specify a temperature at which that component would reach, where the thermal expansion of the valve stem relative to the sleeve in which it has to slide is large enough that the gaps close.

The basis for that temperature is a -- go to the next slide, please.

CONSULTANT WALLIS: Isn't it a temperature difference of some sort?

MR. LEONARD: It is, in the sense that what we're looking at on the left, you'll see just a cross-section, a representative cross-section of the

type of valves we're looking at. This is a, this happens to be a two-stage, and Peach Bottom uses three-stage valves.

The only difference in those has to do with components that are far-removed from the central portion of the valve, so we don't need to be concerned about that.

But the main valve stem that you can see in the center, slides left and right here in this picture through a sleeve, and the question is, is the temperature difference in combination with the material property differences, the valve stem is stainless. The sleeve and the valve body are carbon. There are some in-canal components internal to the valve.

So the combination of thermal expansion material properties, temperature differences, do all of those lead to a condition where the valve stem, in combination with the sleeve, the gap closes, the closure -- the clearance between --

CONSULTANT WALLIS: Probably all those things.

(Simultaneous speaking.)

MR. LEONARD: We're talking about all that stuff. When we began the SOARCA calculations,

whatever it's been, five, six years ago, we did some very simple hand calculations for this, because there's no analysis for this that we were able to find anywhere.

So we did some very simple 1(d) hand calculations, to try to understand how much time would it take, over what temperature history, for these components to reach temperatures at which material deformation. For example, if this valve, the valve stem is moving back and forth in a cyclic manner and very abruptly for the Target Rock relief valves.

They're not sliding valves. They slam full open, slam full closed, that that mechanical effect, with degradation of material strength, might lead to deformation, thermal expansion.

MEMBER RAY: Does it matter whether it's relieving steam or solid water or flashing or is that a variable in any of this?

MR. LEONARD: The fluid variables in this case are steam and hydrogen, that when we -- again, getting back to temperature.

MEMBER RAY: Not flashing in the valve body itself.

MR. LEONARD: Or for this condition at least, there's no water that would be moved. The

safety relief valves in the boiler are on the steam line at a very low elevation, but the reactor, you have to enter the steam line. You have to go to the very top of the reactor vessel out the nozzle and down, which is --

MEMBER RAY: The most common cause of one of these failures, in my experience is vibration due to flashing in the valve body. It's not the temperature, that stem galling on the guide because of expansion. Anyway, I just wanted to know if there was what you assumed about the --

MR. LEONARD: Mechanical effects of flashing we have not taken into account.

MEMBER RAY: Well that's usually why a valve sticks open, is it's passing a flashing fluid, and it just causes the stem to vibrate enough to gall in the guide.

MR. LEONARD: If that occurred during some of the early cycles, when there was still plenty of water within the reactor vessel, in principle that would contribute to the database that leads to the stochastic failure probability.

So I can argue, perhaps, that that effect, if it's the dominant mechanism for observed valve failures, would be captured in the database that we're

using for identifying the failure rate.

The thermal mechanism is something different, where unlike the early cycles, where the valve is operating at its rated temperature, that is the steam flow through the valve is within the domain of the rate of values for the valve, once the water level in the reactor vessel decreases below the top of the core, oxidation begins; the temperature of gases emerging from the top of the core exceed, by a large margin, the rated value, the rated temperature of the valve; and the fluid that's being passed is not just steam but also high temperature hydrogen.

The heat transfer to the valve changes considerably, and the valve temperatures increase at a much faster rate. So what we have done to try to --

MEMBER RAY: It would seem like there would be a huge difference between the failure rate of a two-stage valve and a three-stage valve, inasmuch as the pilot for a three-stage valve is a fairly tiny thing, and it would be significantly impacted by the severe transience.

Have you looked at the difference between two-stage and three-stage?

MR. LEONARD: In a minute, let's get to that, because it's a good -- it is one of the

uncertainties in heat transfer through these detailed components, and I'll show you how we've been able to treat that so far. But it's a very good question.

Let's go to the next slide, because the -- what I show there on the right is the 3D finite element model that was built for the valve body and the internal components, retaining material properties of both the cast body as well as the stainless shaft and other internal components.

What's shown here on slide are the boundary conditions to that three-dimensional model. On the left is the time-dependent temperature history of the fluid that has passed through the valve.

It's obtained straight from the MELCOR calculation for the long-term blackout, and on the right is a representative plot of the velocity or flow through the valve, which is really meant to remind us that it is not continuous flow.

It's cyclic flow. The valve is open for a few seconds, and then it's closed for 45 seconds or a minute, and then reopens again. The period is about 45 seconds on average during the time frame that we're concerned about.

These are boundary conditions to that three-dimensional model, in terms of heat transfer

within the central core of the model itself.

Next one. On the upper right is just a sample illustration of the graphical display of the temperature distribution in the valve, approximately at the time that failure is predicted. Failure in this case is when the gap between the moving valve stem and the sleeve that allows it to slide closes to zero.

Red in this case gives you an idea that really it is the valve stem and the components directly connected to it that are primarily absorbing most of the energy, that the thermal inertia of the body is so much larger that it takes a lag time, if you will, for temperature to penetrate most of the valve body.

CONSULTANT WALLIS: Well, symmetrical, cylindrical growth. It touches in one area presumably, doesn't it?

MR. LEONARD: Several actually, yeah.

CONSULTANT WALLIS: Several. But it does not assume any symmetry? You're actually in 3D --

MR. LEONARD: No. There's actually, if I can point to this, there's a sleeve here, which is one sliding component, but behind it is the piston that is the actual surface area that causes the force that

moves that valve stem back and forth.

When it comes to the two versus three stage, among the things that had to be buried, and that's what's represented here by this 5, 10, 20 percent flow, is the amount of flow that would initially come into the valve, but passes through these small ports to the drive mechanism in the back.

Those ports are open. In fact, that's the mechanism by which the piston allows the valve to move back and forth. The amount or fraction of flow, if you will, through the valve that is diverted through those ports is not known.

We don't know what fraction of the total flow that is discharged through the valve during the cycle actually is passed through the small ports, enters into that back stage, that portion where the piston is located, and then returns through a return line that was shown in your previous diagram out here.

The ports enter on one side, change the pressure on the outside of the piston, and then return through another port. The fraction of flow that's diverted in that direction rather than passing straight through the valve is not known. When we spoke to the valve --

MEMBER ADBEL-KHALIK: Well, you do know

the geometry.

MR. LEONARD: I'm sorry?

MEMBER ADBEL-KHALIK: Well, you do know the geometry?

MR. LEONARD: That's right. Unfortunately, you know, in terms of a material response model, though, the material response model has to be provided at boundary condition -- the fluid flow, if you will, is not part of the finite element model of the valve material response. So we had to provide that as a boundary condition.

MEMBER CORRADINI: Can I just ask a question? I think I know where Said's going with this, and I guess I'd agree with him. If this is a major uncertainty, if it is, if it is, it seems to me that you could do a combined analysis, because --

And maybe it's just that the 5, 10 and 20 onset. But such analysis has been done, is done in other circumstances. So I guess I'm curious. The 5, 10 and 20 in your minds bound these various designs, in terms of what you call the bypass flow?

MR. LEONARD: We don't have a basis to judge whether it is bounding or not. So the next step would be to perform a calculation that would try to estimate the diversion flow, compared to the total

value discharge flow. That's not known.

MEMBER CORRADINI: But okay. So but let me ask a different question. If you went to the valve manufacturer, they don't have any indication, under at least normal operating conditions, what the flow splits are?

MR. LEONARD: We asked that question, and did not get a response that was very helpful. It didn't mean that they didn't know; it's just it could be that we weren't able to get through to the people who might know.

MEMBER CORRADINI: Okay, right. I mean --

MEMBER ADBEL-KHALIK: Do you know many plants use two-stage versus three-stage?

MR. LEONARD: We do. We have an inventory of that. My recollection is the majority are three-stage. So the two variables you see in this case are trying to capture the parametric uncertainties that went into the analysis, and that's the fraction of flow that's diverted to the inner stage. That's the 5, 10 and 20 percent diversion flow.

The other is the initial gap between the sleeve and the valve stem itself. That is proprietary information to the valve manufacturer. We weren't aware of it, but we know what typical clearance

capacities are. They're typically rated, and these terms are very loose, versus a free or a close-running fit. It depends primarily on whether it's an internal sliding mechanism or an external mechanism.

And so we looked at all three, to see the extent that the initial gap had an effect on the time or conditions for seizure. So the two variables, flow diversion plus the initial gap size, and what's shown on the left are two representative temperature histories for the dimension of the gap from its initial condition, during valve heating, down to the point where the gap closes to zero.

So approximately 42, 45 hundred seconds in this case. Now this is seconds after initial heating begins, would be the time at which the closure, the gap width closes to zero.

We take that value of time, correlate it back to the temperature of the gas or the temperature of the valve stem itself at which closure occurred, and that becomes the criterion that we use in the MELCOR model for defining when the valve would seize by a thermal mechanism.

MEMBER ADBEL-KHALIK: So there is no way, for example, the bellows in the pilot to deform, that would cause the valve to permanently open?

MR. LEONARD: In the pilot? That's certainly possible. Other mechanisms are being proposed --

MEMBER ADBEL-KHALIK: Higher probability than what you're assuming here?

MR. LEONARD: Even fatigue of the spring is possible if the spring itself is weakened, that it no longer has the strength to be able to drive the valve closed. I mean there are a number of other conceivable mechanisms that we've considered. This one was the one that we can analyze directly.

MR. SCHAPEROW: Also, our sensitivity analyses showed that if the valve stuck open a bit earlier during the core heat-up, it wouldn't make a big difference in the source term and the rest of the accident progression. So we looked at, I think we got, I don't know, maybe a dozen or so cases we ran for --

MR. LEONARD: Let's go to the next slide.

MEMBER ARMIJO: Before you leave that, you didn't analyze the close-running fit. I mean I'm just assuming maybe nobody uses, builds valves that way. So that would cut the time down quite a bit.

MR. LEONARD: We actually ran, I only just for the sake of not pressing your eyes to be able to

see three curves on one page, I only used two representative ones. We did all three.

MEMBER ARMIJO: Okay, because that's a big change from 135 millimeters, .135 millimeters to .09 is a pretty small change. But from .09 to .0275, that's a pretty big change.

MR. LEONARD: That's right.

MEMBER ARMIJO: I just wanted to know what, how much --

MR. LEONARD: Our understanding is that the free-running fit is the most representative of these types of components, that the close-running would be very finely machined components that wouldn't be subjected to the kinds of loads that a big SRV --

MEMBER ARMIJO: This would not be a typical for these valves?

MR. LEONARD: The close-running, the free-running fit, the curve at the bottom is the one that we've been advised is probably the most representative for this type of component.

So we used the criterion that was developed from this analysis, to define the thermal failure criteria for the valve, which operates in parallel to the stochastic criteria, and then we ran the best estimate case, which is represented here in

this shaded color, with the valve stem temperature of about 900 K would be the criterion for thermal seizure.

CONSULTANT WALLIS: Now this stochastic, this five percent to 90 percent is based on some kind of a distribution?

MR. LEONARD: That's right.

CONSULTANT WALLIS: Where does that come from?

MR. LEONARD: The distribution that is, corresponds to the failure rate that we used --

CONSULTANT WALLIS: That's really an uncertain distribution, when you're looking at the tails, like five percent, isn't it?

MR. LEONARD: It's a wide distribution, that's true, and the values of stochastic here are the cumulative failure probabilities. So the expected value that we used corresponds to a cumulative failure probability of about 63 percent.

But what we looked at in addition to the base case, which uses the best estimate value of the valve stem failure temperature or closure criterion if you will, gap closure, the stochastic value that represents the expected value for valve sticking in a random context.

We varied both of those parameters over some reasonable range, both lower and higher temperatures, earlier and later stochastic failure mechanisms, to see the extent to which uncertainty in these parameters really affected the results, and that's what I want to try to summarize here.

Several cases that are worth looking at is this is an interest. First, as you can see, and I'll focus primarily on the long-term blackout. For the base case, the stochastic failure mechanism occurred first in the base case for long term. But in the short term blackout, you can see that the thermal mechanism occurred first.

So difference in terms of the total number of cycles that preceded the challenge, in terms of core uncovery. We focused on the long-term blackout. If you push the stochastic failure criterion all the way out to say its 90 percent cumulative failure probability, that is, you're trying to force the thermal mechanism, the number of cycles increases considerably from the base case 270 up to 470.

The amount of time that it takes for the valve to seize obviously moves out considerably. That change in the failure criterion moves the seizure time out about three hours. What I've shown you in this

column is the creep damage index for the main steam line, which I'll get to in a minute.

This is a series of numbers that tries to represent the time at which the source term to the environment becomes significant, and what I mean by that is when the iodine released to the environment is greater than one percent, that's the threshold that we begin to be concerned about changes in the source term.

This time changes about ten percent over that range. The magnitude, that is, it moves a little bit earlier. The magnitude of the release of iodine and cesium to the environment increases by about a factor of two.

If we vary another factor in the criterion, which is not only that the valve sticks in the open position, but what do we mean by open? Full open, some partial open position?

The idea was this valve is moving through a sliding valve stem. In principle, it could seize at any distance along that valve stem, even though the motion is full open/full close.

If seizure, collapse of the gap is the failure mechanism, then in principle it could seize at any open position. So we looked at some variants of

that. Only a one-tenth open area versus a one-half. These are all full open areas.

And the interesting thing that we observed there is if we used the thermal failure mechanism at a smaller seizure area, that's when we begin to see other aspects of the model begin to pick up its participation. But with only a one-tenth open area, the depressurization rate of the reactor vessel was significantly reduced.

High pressures are sustained for a longer period of time into the process of core damage. The energy that would normally be discharged through that valve to the suppression is now a fuel bottled up inside the reactor vessel.

That energy is transferred in part to the main steam line, main steam line nozzle. Because of the high pressure in combination with high temperature, one here means this is the creep damage index that we calculate for the main steam line piping. The main steam line creep rupture would occur in that case.

It changes the release pathway to the environment, to the containment in a completely different way. Instead of discharge from the reactor vessel through the SRV to the pool to the torus, you

now have a direct and large discharge directly to the drywell, and a load, a mechanical load, a pressure load to the drywell that would not be observed otherwise. The effect of that --

MEMBER RAY: Is the stochastic database just from the nuclear data? For example, look at super-critical fossil plant relief valve performance. Is that part of this database?

MR. LEONARD: When I mentioned the two studies that we reviewed, one of them is nuclear only; the other is more comprehensive than that. It includes fossil-fired plants as well.

MEMBER RAY: Yeah, because those would be more representative, I would think, for the kind of fluid that you're passing here. Man oh man, they sure stick a lot more than that --

(Simultaneous speaking.)

MR. LEONARD: If steam line creep rupture fails, you can see that has a significant effect on the environmental release. Instead of it only being a factor of two or so, you're talking about almost a factor of eight or ten increase in the release to the environment.

MEMBER CORRADINI: And Mark, I guess I didn't appreciate. If I had the creep rupture

failure, it enters drywell, but then it goes through the bellows or through the piping, still back into the wet well, does it not?

MR. LEONARD: It does, but several things to take into account. One is that the pathway for that gas and the aerosol content that's in it to the suppression pool goes through the downcomers into the pool, which are a much larger diameter than the T-quencher small holes that are the discharge point for the SRVs.

So the scrubbing efficiency and the non-condensable gas content is very high, which reduces condensation as an aerosol capture mechanism. The very large flow rate, as well as the large diameter of the bubbles that are created in the pool by the discharge through the bellows is not nearly as effective a scrubbing process as you would get at the SRV tail pipe.

MEMBER CORRADINI: Right. So captured it based on the models, relative to the downcomer versus the actual SRV piping?

MR. LEONARD: And those effects are integral to the MELCOR model. It recognizes those dimensions and the difference in the flow pathways as part of the model. So the scrubbing mechanism is

connected to the flow pathways, and the representative bubble size.

MEMBER CORRADINI: Okay, all right, and then one last thing. I guess I'm reading your table. You assume the creep rupture failure whole size? Where does that fit in here? I missed it.

MR. LEONARD: I didn't mention it. These calculations all assume that if creep rupture occurs, that it's a full diameter break. It is a variable we looked at elsewhere.

MEMBER CORRADINI: Okay, so and that's clearly conservative, but is it the basis is what? I guess I don't appreciate what the basis is.

MR. LEONARD: There's a paper I'd have to -- that we -- and we dug it up as part of the uncertainty, because this is a variable in the uncertainty analysis, is when the -- if the main steam line fails by creep, how large is the opening?

It's a variable that is included in the uncertainty analysis, and we've been struggling to define the distribution for that area, and we're given a reference for mainly a PWR study for hot leg creep rupture, which tended to show that when creep occurred, there's a very strong bias towards large opening areas. It either opened with a big hole or it

didn't open. So that's the lower head.

MEMBER CORRADINI: Okay. That helps me. Then one last quick thing. If I go back to Slide 6, where, I mean maybe you could just point on the pretty color cartoon. Where is the creep rupture likely to occur on the outlet of the flanged area, on the inlet or not even near the valve body?

MR. LEONARD: It wouldn't be anywhere near the SRV. It would be up at the steam nozzle that's attached to the reactor vessel, because that would be the point -- or I should say that main steamline piping that is immediately downstream of the nozzle safe end, because that's the thinnest wall at the highest temperature.

MEMBER CORRADINI: Okay, and so that's upstream of where we're looking in the cartoon?

MR. LEONARD: Quite a bit upstream, yes.

MEMBER CORRADINI: Okay, all right. So almost like the T section coming out of the main steam line?

MR. LEONARD: No. In fact, this is a valve that's connected to the main steam line, tens of meters downstream from where the creep rupture location would occur. So when creep occurs, the SRV is taken completely out of the picture. It's not

longer participating.

MEMBER CORRADINI: Okay, okay. Let me ask one last -- I'm sorry.

MR. LEONARD: Go ahead, Mike.

MEMBER CORRADINI: So let me jump to Fukushima. Is there some way, and I guess I have to get at this.

Is there some way going and saying if I wanted to investigate an observation, because there was a depressurization in all of these units, is there some observable that one would want to look at if one could, to determine a stuck open SRV versus a creep rupture failure upstream of that point, and if so, what would that be?

MR. LEONARD: Containment pressure.

MEMBER CORRADINI: Well, but in both cases, containment pressure. But in all cases, I guess Randy knows this better than anybody, I'm not sure if there was any sort of measurement of pressure.

Is there any sort of observable, other than pressure, or I should say the frequency of pressure measurements, maybe Joy knows this, was not at that time scale, that you could actually catch the difference?

MR. GAUNTT: Mike, this is Randy. Let me

just say what I know about it or what I think I know about it.

MEMBER CORRADINI: I guess Randy, the reason I'm asking this is I'm looking at a way to essentially delineate between the two, and the only thing I can come up with is actually the recent events.

MR. GAUNTT: Yeah, and so let me tell you what we've done, in the context of our MELCOR Fukushima Unit 1 analyses. So we exercised the SOARCA approach in Fukushima, and lo and behold, we failed the -- we'd actually run it several ways. But we fail the steam line in the Fukushima analysis.

But we don't know that for sure. It's a bit of a horse race between whether the SRV would stick open. So we ran that case alternatively. In the case of the SRV seizing open instead of the steam line failing, the vessel blowdown is through the suppression pool. So the steam, the steam energy is deposited in the suppression pool.

Alternatively, if you fail the steam line, the blowdown is into the drywell environment. In both cases, there's a lot of hydrogen, not condensables. So in the case where we vent the steam into the drywell, we see a higher residual drywell pressure

following blowdown than the case where it blows down into the suppression pool, because the heat sink is there.

Just based on those two scenarios that we've run, we get a better match with the long-term drywell pressure that's measured, the technical data for the steam line rupture than we do for the venting into the wet well.

So we kind of put our odds, we put 70 percent of our chips on we think the steam line ruptured, and we're going to put 30 percent on well, on the other hand, it could have seized open the SRV. But we get a better match to the residual drywell pressure in Fukushima, when we rupture the steam line.

MEMBER REMPE: Are you talking Unit 1?

MR. GAUNTT: Unit 1, yeah.

MEMBER REMPE: Okay.

MEMBER ADBEL-KHALIK: On Slide 6.

MR. LEONARD: Okay.

MEMBER ADBEL-KHALIK: I understand that you don't know exactly, or the manufacturer did not give you the information as to how much flow would go to the pilot in the second stage, versus the main flow. But have you done the analysis to see even within these assumed "bypass flows" to the pilot and

second stage, how long would it take for the pilot to reach these limiting temperatures?

MR. LEONARD: Only in the context that I've shown here, which is for those assumed --

MEMBER ADBEL-KHALIK: For the pilot, not for the main disk.

MR. LEONARD: No. The pilot itself has not been included in the model. It sits on top of here.

MEMBER ADBEL-KHALIK: Do you know how small the pilot is?

MR. LEONARD: The pilot is very small in comparison to this.

MEMBER ADBEL-KHALIK: Right. So at the five percent flow, do you think how long it would take for the pilot to reach these limiting temperatures?

MR. LEONARD: Well, the pilot is physically removed, even from the back stage of the main valve. That is, the pilot itself sits on top here, and it's, there's an additional flow diversion from this chamber up into the pilot and back.

So it would be a yet smaller fraction of the total flow that goes through this stage of the valve. The flow rate through here is designed to make sure that the pressure on the back side of the piston

is sufficient to drive the valve closed, whereas the pilot only needs, it has a secondary shaft that moves back and forth, only to make sure of the pressure difference across the piston face itself.

MEMBER ADBEL-KHALIK: You're talking about a pilot for a two-stage, or a pilot for a three-stage valve?

MR. LEONARD: For the three-stage.

MEMBER ADBEL-KHALIK: For the three-stage?

MR. LEONARD: Yes.

MEMBER ADBEL-KHALIK: And you think that that pilot wouldn't reach these limiting temperatures --

MR. LEONARD: You raise a good question.

MEMBER ADBEL-KHALIK: Allow it to deform --

MR. LEONARD: Our judgment, if you will, was that because of the physical separation of the two portions of the valve, the pilot from the back stage of the main valve that we're showing here with the piston.

The fact that the flow rate into that section of the valve was smaller than we've already diverted to the piston and the spring area, meant that its response was going to be slower than what we see

in direct contact with valve stream. That may not be correct, but that was our judgment when the model itself was built.

MEMBER ABDEL-KHALIK: Have you looked at temperature histories of leaking three-stage valves for both the pilot and the second stage?

MR. LEONARD: No.

MEMBER ADBEL-KHALIK: Okay. You would probably see that the temperature of the pilot is higher than the temperature of the second stage.

MR. LEONARD: During a cycle?

MEMBER ADBEL-KHALIK: Correct.

MR. LEONARD: Yeah. Are you aware of data that is available or are available for that?

MEMBER ADBEL-KHALIK: I couldn't give you the data.

MR. LEONARD: I'm interested in -- I'm not aware of any temperature distribution data. There are temperature measurements on the valve itself, but not numerous ones that would provide us with temperature distribution information. But if you're aware of some, that would be very helpful.

We have nothing to benchmark this calculation. Let's put it that way. If there is any information out there, we'd be very appreciative to

learn of it.

MEMBER ADBEL-KHALIK: But if this is such an important calculation, with huge sort of uncertainty, and if there is even the possibility that under these conditions, that this valve would cycle for only once or twice, have you looked at the possibility of what happens if the number of cycles after which this valve was seized is only limited to a few, two?

MR. LEONARD: Go to the next slide again. You can see that in the short-term blackout case, where --

MEMBER ADBEL-KHALIK: It's 15 still.

MR. LEONARD: In this case, where instead of moving upward, moving the stochastic model out, you pull it back and say what if it fails in just the first few cycles? How does that affect things?

That's also been examined in the cases that are part of the uncertainty assessment the team will talk about later, where as I said, rather than a fixed number of cycles as the stochastic criterion, the full distribution of failure rate was considered, which is used then to calculate a smaller and larger number of cycles to failure.

In general, if it fails open, in a very

few number of cycles, these results over here are much improved. Much lower releases on average than you see for the reverse, which is if the reactor vessel remains at high pressure longer, the valve continues to cycle as designed, and you reach a thermal failure criterion.

If you go that far out in time, the releases tend to get much larger, especially if you can move the thermal distribution in the system out to a point where main steam line creep rupture now becomes possible. That's a threshold over which the answer changes quite a bit.

MEMBER ADBEL-KHALIK: Great, thank you.

MR. LEONARD: Well thank you for the comments. I think, especially in terms of flow distribution, that's something we should look into.

MEMBER STETKAR: Mark, I hate to ask, and we're trying to do things real time here, so I'm only partially listening. The stochastic distribution that you did use for the failure rate, where was that derived from finally?

MR. LEONARD: The value that is used in these calculations came directly from the Peach Bottom IPE.

MEMBER STETKAR: That's what I've read.

Where did they get their value?

MR. LEONARD: I don't know the answer to that.

MEMBER STETKAR: Because I know where NUREG/CR whatever the number is, the 6928. I know where that came from.

MR. LEONARD: What you will find is that, and in fact it's part of the document that we put together for Peach Bottom, is that a failure rate, or I should say the number of cycles, the expected value for failure --

MEMBER STETKAR: One over lambda.

MR. LEONARD: One over lambda, if you use the IPe failure rate, corresponds to roughly the 90th percentile of the value for the generic database.

MEMBER STETKAR: The generic database is dominated very strongly by a subjective, non-informed prior probability distribution, because there's only two failures in that generic database.

MEMBER RAY: John, aren't you back on the question, at least it sounds like you're trying to ask it --

MEMBER STETKAR: Well, to some extent. I just want to make sure that since we're going to -- Mark has cleverly deflected the uncertainty to Tina,

I just want to make sure of the uncertainty we're talking about, since he's a valve guy.

MEMBER RAY: Well, let me ask the question, though. Wasn't the answer then that it's not the fossil plant database where valves stick open all the time?

MR. LEONARD: That's correct.

MEMBER RAY: Okay.

MR. LEONARD: As far as I know, unless Peach Bottom somehow used that database to estimate whatever their estimate was derived from.

MEMBER STETKAR: Going to the report, it says that Peach Bottom has changed that value to that.

MR. LEONARD: That's correct. Our understanding is that they've moved to the NUREG/CR value, that's correct.

MEMBER STETKAR: Well, because it's a factor of, you know, four times lower.

MR. LEONARD: Again, we're kind of jumping ahead to what -- I hate to steal Tina's thunder, but there are a couple of problems with simply using the numbers out of that book, or the other book that goes with it.

One of them is that the data are -- the failure, the observed failures are all cases that a

valve failed in one, two, three cycles. We're talking one, two, three hundred cycles.

MEMBER STETKAR: And when you say "all cases," you mean both?

MR. LEONARD: Both of them. And so the question remains what, how does the valve stochastic failure probability change as you move from one to tens to hundreds of cycles? There's no data. There are no data for that.

MEMBER STETKAR: But I think what Dennis was asking about, you know, quite a while ago, didn't -- wasn't there quite a bit of work done on the Target Rock valves back three or four years ago --

(Simultaneous speaking.)

MEMBER BLEY: --when this issue came up after TMI.

MR. LEONARD: The other document, not 6928, if I have the right number, but there's another more recent document, 70-something, did look at not only the observed industry failures, but also the test data.

MEMBER BLEY: Including the industry test data.

MR. LEONARD: Including the industry test data, which did move, cycle the Target Rock valve

several hundred times. If you look at the failure rate that the test data reports, it is virtually identical to the industry-observed data. It's 10 to the minus 4 or something.

MEMBER BLEY: And that was, they did -- well, they didn't do high temperatures like you're talking about.

MR. LEONARD: We've intentionally tried to separate the thermal expansion in the high temperature condition from the stochastic failure.

We're always assuming here that the stochastic failure is applied primarily to thermal conditions within the design range of the valve, so that at least the thermal component is somewhat removed in comparison to all the other mechanical just corrosion and a variety of other effects, the vibration as well. I mean a number of other mechanisms that would be truly stochastic.

CONSULTANT WALLIS: But since the knowledgeable observer is going to be suspicious of the stochastic approach, couldn't you provide more data or some basis for it in your report?

MR. LEONARD: The basis right now that's documented, we could describe the details of the database --

CONSULTANT WALLIS: But if it's based on how many points or something, or something, so that there's a -- it gives the reader more confidence. Otherwise, it looks pretty iffy.

MR. LEONARD: For the moment, we've provided the references for that information.

CONSULTANT WALLIS: That doesn't help.

MEMBER STETKAR: Well, the report, there are three paragraphs, and the report indeed does parrot what Mark said, that they used the failure rate from the Peach Bottom IPE study, period. It does note that that's higher than, you know, this other --

MR. LEONARD: About a decade higher than the generic database would suggest, and I can say there was considerable debate as to whether we should keep the IPE value or move to the generic database, and among the decisions for keeping -- among the reasons for keeping the higher IPE value was the, I'll say qualitative expectation that the failure rate would increase as the number of cycles increased.

There will be more and more likely for failure to occur as you move from ones to tens to hundreds of cycles. Quantitatively, we can't capture that with --

MEMBER BLEY: I don't know why. What were

the arguments for that? I mean when we see, just pick something simple like check valves in a compressed air system that operate a lot, they fail a lot less than check valves that operate very rarely.

So I'm not sure what kind of argument you were basing that idea on, that increasing numbers of operations would increase the failure rate.

MR. LEONARD: I mean the proposed mechanisms were things related to the accumulation of corrosion products --

MEMBER BLEY: But operation tends to clean that stuff off. That's why we exercise.

MEMBER STETKAR: But in some sense, I mean if you think about the way the data are developed, this is not -- in some sense I think they're being misused a bit.

This is not a failure rate per demand in terms of number of cycles; it's sampling of population. One out of 270 valves will stick open.

MEMBER BLEY: Right.

MEMBER STETKAR: The first time it's demanded. That's different than saying it's going to run for 270, a valve will run for 270 times, you know, on average, before it fails.

MEMBER BLEY: It is. But it's typically

used both ways because you don't have enough knowledge to discriminate what you're talking about.

MR. LEONARD: In fairness to the other speakers, I mean we are going to come back to this topic. So I want you to hold these thoughts, because your advice is going to be very helpful to know how to guide the uncertainty assessment of this issue.

MEMBER STETKAR: Just out of curiosity, because you and I have similar blocks in terms of remembering NUREG numbers, and Marty Stutzke doesn't. You got as far as NUREG-70, and I don't have that.

(Laughter.)

MEMBER STETKAR: Are there a couple of other digits on the NUREG, because I don't have it.

MEMBER BLEY: 7037.

MEMBER STETKAR: Okay. If we could get it. Just keep the thing going. If I could get it during the break --

MR. LEONARD: I have it with me. I have it down here, so when I'm finished, I'd be happy to give that information to you.

MEMBER BLEY: Before we leave this, I just wanted to toss one thing on the table, and it probably comes up in the uncertainty analysis. When you were describing your analysis of the temperature changes,

you argued that gee, when there's temperature changes, that could seize us anywhere between 0 and 100 percent. Ten percent is likely as anywhere else.

It doesn't sound physically reasonable to me. If you run a pump without cooling and the wave lengths begin to expand, keep it running and it keeps running, and you turn it off and it won't start again. I mean if you can get it moving, the chance that it stops at ten percent seems pretty unlikely.

MR. LEONARD: And I think that's -- we have learned the same way, that the initial distribution that the measurement presented, in terms of the seizure location, represented effectively complete uncertainty between full open/full close.

MEMBER BLEY: Like equally likely.

MR. LEONARD: Equally likely. What I think we're beginning to move to is one that is far more likely to simply stay open and not move. So there would be a strong bias toward a full open position.

MR. SCHAPEROW: Another argument in that regard is that the thing doesn't have, the stem doesn't have to move too far before you're now at the full open area, and once -- it keeps moving even after that. But it just isn't -- so you're in that zone

very -- the ten percent opens on a very small amount of time as the thing's traveling out. It's just --

CONSULTANT WALLIS: And also, the stem is hotter at one end than the other. So the bit that sticks is the bit when it's full open, isn't it?

MR. LEONARD: That's right. So it's something we've learned along the way, where total uncertainty about an issue that we had no idea what impact we have.

MEMBER BLEY: It's a place to start.

MR. LEONARD: We've now learned that it has a very strong influence on the results, so we have to be more careful.

MEMBER BLEY: I'll just follow up on what Graham said earlier. I can understand brief references to failure rates and that sort of thing in the whole study. But when you've narrowed down on something important, providing more of the details, where that data comes from is really key.

MR. LEONARD: This is the feedback of the uncertainty assessment, in the sense that I don't think we fully appreciated how important the uncertainty about that was, until the uncertainty assessment was done, which now is following the original work.

So we've documented what we've done, and are now discovering new information after that. So if I can, the final conclusions that we've come to, at least in terms of variance in the SRV failure criteria, is that reasonable variations in the failure criteria have a significant effect on in-vessel thermodynamic signatures.

What I mean is that things related to the time at which reactor vessel depressurization occurs, and to a small extent the time at which lower head failure occurs. The effects of those things, although the signature's changed, the cumulative effects in terms of the release to the environment, the time at which release begins is reasonably small, factors of two, not factors of ten.

Provided -- this is a very key, underlined word -- provided that creep rupture of the main steam line is not the outcome of the variance in the SRV failure criteria. When that happens, if that happens, then there's a significant threshold.

It's not a small increment. It's a significant increment in the radiological release to the environment. So that particular issue is receiving a lot of attention in the uncertainty analysis, okay.

A total shift of ideas. That was the summary of our work on SRVs, and as I said, we'll come back to some of that. The second item that was raised during the peer review was the potential for a containment bypass pathway, that can be generated if a particular system, the traversing in-core probe TIP system in a BWR, is operating at the time the accident happens to occur.

So I'll quickly describe for you, for those of you not familiar with TIP, a very brief description of what the mechanical configuration of that system is, what it means in terms of a potential leak pathway, and what the operating practices of the system are, so we have an idea of what is the time window over which this possible leak pathway might occur.

The in-core probe system is used for reactor engineering calibration, power management --

MEMBER STETKAR: Mark, just make sure you stay pretty close to a microphone, so that you're on -- a lot of times it's easier if you use the mouse, if you have a mouse.

MR. LEONARD: The in-core system, the drive mechanism if you will, most of the machinery is in a room. Actually, here's a cross-section of this

floor of the reactor building. It's in a room immediately adjacent to the containment outer wall.

That room contains, if you will, little motors that drive a cable that has a probe at the end of the cable that is driven into the core, and traverses the full axial length of the core to do flux measurements and a variety of other things that reactor engineering means for their work.

Cable runs through a tube that follows a path roughly shown here. It enters the reactor, enters a drywell through a penetration in the wall about mid-height in the spherical region. Enters through a hole through an upper region of the pedestal, and then vertically into the lower head to the core.

So the idea would be that if somehow this tube were open to the reactor building at the time the accident would occur, the difficulty would be that during the process of core damage, what would normally be the sealed end of the tube would now be melted and possibly open.

If the open area of the tube were exposed all the way out to the reactor building, this, the machinery out here is not designed to be leak-tight, at least not leak-tight against the pressure that

would be at reactor vessel pressures. It's designed to be leak-tight at much, much lower atmospheric pressures than that.

So that could be the containment bypass leak pathway that was raised as a question of whether that would significantly impact our results if the system were operating at the time the accident occurred.

To give you an idea of the system configuration, on the left I've shown the reactor vessel would be on the left side, the drive units in the reactor building on the far right. There is a shield wall within the reactor building that separates the chamber where the probe itself is stored during normal operation.

This drive unit out of the reactor building through a cable pushes the probe out of its shield chamber, through the drywell wall, into a device that's referred to as an indexing unit, and the best basically to think about that is it's sort of a random selector or a design selector to allow only three drive mechanisms to be able to penetrate the core in many, many more locations.

So you have three drive mechanisms, any one of which can, through this indexing unit, be moved

to a different radial location in the core, to be able to do measurements across the entire core radius.

Among the other reasons the indexing unit is important is that within it is a relief valve that would lift at relatively low pressures, that's designed primarily to protect the tubing system from the nitrogen purge system that provides a full nitrogen purge within the tube during normal operation.

If for some reason that system were to go haywire and to over-pressurize the tube, the relief valves would open. The relief valves are not designed to relieve, in terms of reactor vessel pressure. That's not the configuration that would be expected.

So the question is if within the reactor vessel you fail these tubes and expose this leak pathway, leakage through the index or the drive units out in the reactor building would provide a containment bypass mechanism. It's important to also recognize there are isolation valves in this system, a ball valve as well as what's referred to as a sheer valve.

The ball valve is the primary isolation mechanism, that when the probe is fully withdrawn it is in the shield, which is most of the operating

period of the reactor. This valve is closed, and the system is completely isolated from the outside world.

MEMBER ARMIJO: Even if the guide tube failed, those valves would stop the pressure? It goes through -- the probe goes through a guide tube, right?

MR. LEONARD: In this case, the guide tube goes through the center of these valves. So for example, that's why the sheer valve is here, that if the probe is inserted at the time an isolation signal were to occur, and the probe cannot, for some reason, be removed, if the drive units fail, for example, if the ball valve cannot close because there's a cable through the middle of it.

That's the purpose of the sheer valve, that they're explosive valves that would drive a knife, if you will, through the cable and sheer the opening to the point where it causes isolation. Both of these valves require electric power. The ball valve is AC; the sheer valve explosive mechanism is discharged by a DC signal.

So in our case, for a full station blackout during core damage, where neither AC nor dc power would be available, if the system were in operation, there's a potential leak pathway through the open tube back to the drive mechanism. The

numbers to keep in mind --

MEMBER STETKAR: And Mark, it's been a while since I looked at these things. Back the previous slide. You briefly mentioned the relief valves on the indexer. You're presuming that the drive mechanism will blow out at a lower pressure than those relief valves will lift, or have you looked at that? Am I stealing your story?

MR. LEONARD: We've bounded the leakage, because the leakage at the drive mechanism through what are effectively mechanical seals that at high temperatures would fail anyway.

MEMBER STETKAR: Okay.

MR. LEONARD: What that leak area would be is not --

MEMBER STETKAR: No. But I mean you told me what I was asking. You presumed that those seals will fail at a lower temperature pressure threshold than the relief valve would. The relief valve's probably --

MR. LEONARD: Because they're designed to be sealed against roughly maybe two bar. I mean a very low pressure relative to what we're talking about.

MEMBER STETKAR: And it's a probably 100

and some odd pound relief valve?

MR. LEONARD: That's right.

MEMBER STETKAR: Okay.

MR. LEONARD: And they're certainly not capable of taking high temperature.

MEMBER STETKAR: Okay, thank you.

MR. LEONARD: Okay. The system's only in operation for about an hour every four months. That's the frequency that the probe measurements are taken, which from an action and frequency point of view represents somewhere in the neighborhood of 4 to the minus 4 per year. You have that on top of an accident sequence of 1E minus 6.

The total frequency of the accident occurring at the time the tip is operating is very, very small. Nevertheless, we wanted to quantitatively measure the effect of the system if that condition happened to occur, and because of uncertainties, as I mentioned earlier in terms of leak areas, we first began by looking at a bounding case.

That is that the normal condition is that all three tubes in fact would be operating simultaneously. All three drives would be working at the same time, and the main reason for that is to get all the measurements done as quickly as possible, to

reduce total exposure.

MEMBER ADBEL-KHALIK: I thought it takes a shift to do this, rather than an hour.

MR. LEONARD: We were informed that it takes an hour to do the total test cycle. This was among the information that we pulled from the fact check, I think, with the licensee.

MR. CHANG: We had a conference call with the TIP system manager over at Peach Bottom, and that's what they told us.

CONSULTANT WALLIS: It seems very quick.

MR. LEONARD: They're able to do -- by running all three tubes for an hour, they can do all the measurement locations fairly quickly, and it's part of their effort to reduce personnel exposure because, you know, this is one of the more active systems, from an activity point of view and an exposure point of view, one of the systems they have to be careful with.

I've already mentioned that the isolation valves require electric power to operate, and at the moment there are no formal provisions. What I mean by that is written procedures to close the valves in the absence of electric power. I think as part of the discussion we've had through this program, that's an

insight that the licensee has considered.

Next. Because of the variables and some uncertainty in the leak area, we first approached a bounding case, which is all three lines are in operation. The isolation valves are full open, and all three probes are fully withdrawn.

What that means is the maximum leak area, full diameter of all three tubes. Now when I say that, that only corresponds to three times about a quarter-inch ID tube. These are very, very small tubes. The cable that drives the probe in is an aircraft cable of roughly a quarter in in diameter.

A much more realistic case would be that all three tubes are in operation, that with the probes inserted, the cable diameter is, as I mentioned there, .258 out of a 2-8 diameter. The gap around that is extremely small. So again, we're looking at the bounding case.

We built a model for the tubes. That is, the tubes themselves are considered the control volume. The leak through those tubes to the reactor building is incorporated into our integral calculation of action and progression, fission product release and transport.

What I've shown here is an overlay of the

baseline results, the best estimate results that are presented in the report, against a sensitivity that includes the maximum bounding leakage through the TIP tube.

On the left is the main bounding case. This is one where the iodine release, in comparison to the base case cesium, compared to the base case and molybdenum, as well noble gas, you can see that the difference is very small, certainly within the uncertainty of the accident signatures that we're generating in the first place.

MEMBER BLEY: Two quick questions, because it's been a long time since I looked at the TIP system. Is that tube typically is not open to the RCS, is it? So it would have to fail, and you're assuming that it fails?

MR. LEONARD: That's right. You're assuming that --

MEMBER BLEY: Because of the core damage.

MR. LEONARD: In this case the tube is assumed closed until the extent of damage in the core reaches approximately midcore height, and when fuel cladding or approximately mid-height of the core fails, based on the all the failure criteria that are normally represented as part of the material

relocation models --, we open this tube. The idea is that you open --

MEMBER BLEY: This calculation doesn't have any frequency in it?

MR. LEONARD: What do you mean?

MEMBER BLEY: In the 10 to the minus 4, the chance that you're in this condition?

MR. LEONARD: This represents a case where that tube is full open. So you are effectively operating during that 10 to the minus 4th period.

MEMBER BLEY: But this is just a consequence. So it doesn't have that 10 to the minus 4?

MR. LEONARD: That's correct. So that the --

MEMBER BLEY: But even so, it's not a big --

MR. LEONARD: This is fraction of inventory released to the environment, and the main curves that you see here are the baseline results that do not take into account this leakage pathway. The lines that are somewhat shorter, and it's only because it takes -- these calculations are very time-consuming because of the small dimensions that the tube represents.

We've been able to show that during the key portion of the release period, the release increases slightly, but not in a substantial enough way --

MEMBER BLEY: This is going to matter. I always kind of question the 10 to the minus 4 because I don't know why you couldn't have accidentally left one of these ball valves open.

MR. LEONARD: That's right.

MEMBER BLEY: It might give you a higher exposure than the 10 to the minus 4.

MEMBER ARMIJO: That's a stainless steel tube?

MR. LEONARD: That's correct.

MEMBER ARMIJO: And it not pressurized against reactor pressure?

MR. LEONARD: That's correct.

MEMBER ARMIJO: So when this, this thing's at high pressure and the fuel's getting very, very hot, the stainless steel is getting into creep rupture, creep collapse. But one of the mechanisms of failure is actually just flattened and squeezing these tubes down?

MR. LEONARD: That's right.

MEMBER ARMIJO: Did you take that into

account, or you just left them geometrically?

MR. LEONARD: It's a good lead-in. Thank you for the question, because the curves you see on the left are --

MEMBER ARMIJO: But that's what will happen.

MR. LEONARD: --are the results if the tube remains fully intact, and there are no adverse effects on the tubes.

To the right, we actually calculate the wall temperature of the tube, and at temperature and pressures that would lead themselves to creep, we assume that the tube ruptures in a full open position, full open area to the drywell, because of the majority of the tube run is through the containment, not outside the building.

The isolation valves are only a few inches from the containment wall, for example. What you can see is rather than the release increasing slightly, it actually decreases slightly, and that's primarily because the leakage is now through the tube to the drywell, which provides additional scrubbing through the suppression pool.

CONSULTANT WALLIS: But not for the iodine. The iodine looks the same in the two cases.

MR. LEONARD: That's right.

CONSULTANT WALLIS: Is that a misprint or is that --

MR. LEONARD: No. They're basically -- you don't see nearly as strong an effect for the iodine as you do for either of the other two species shown here.

MEMBER STETKAR: Mark, the analyses you did on the creep rupture assumes pristine tubes, because there's been experience in these things of, you know, reasonably substantial wall thinning at several plants.

MR. LEONARD: Uh-huh. In this case, we used the design value of the tube wall thickness, but used the standard creep model in terms of stress required for the tube to fail.

MEMBER STETKAR: But I mean you -- thanks.

MEMBER REMPE: Where did you place this tube? In the center of the core, at the peak temperature of the core, wherever that is, and so it was a movable tube to hide the peak cladding temperature?

MR. LEONARD: The only thing that is tied to the core itself, in this case, is the criterion that's used to define when the tube opens, and in that

case, we monitor the clad integrity in the radial center of the core.

I'd have to check to be sure on this, but either at the core midplane or slightly higher, one of the two. Certainly in the upper half of the core, but certainly also core, the center radius.

MEMBER REMPE: So it's at the center of the core? I mean you could tie it to just peak by temperature wherever it is?

MR. LEONARD: We could tie it to anywhere. We had to choose a location.

MEMBER REMPE: You picked a location.

MR. LEONARD: We picked the hottest location. That was the intent.

MEMBER REMPE: So it's a movable tube that's at the hot location, somewhere in the core?

MR. LEONARD: It would depend -- in this case, reality would depend on the indexing unit. If at the time this occurred, the indexing unit had moved the probes to some outer location, then the tube that's in operation would not be in the center.

But the guide tubes would still exist on all other core locations, and they would all go back -- they all go back to that indexing unit. So the connection, the flow connection between the core and

the indexing unit for all radial locations is always open.

It's only from the indexing unit through the machinery, through the drywell wall to the reactor building, that's where you -- everything condenses down to only those three lines.

So in this case, we're assuming that even if the probe was not operating in the center, that the open tube from the center to the indexing unit is still open, and that it would somehow be connected through the tube to the outside world.

So it's bounding in a couple of contexts.

MEMBER REMPE: And then I missed somehow or other. I understand what the creep rupture TIP to this is. Tell me again of the continuous cases? Is that -- what does this say?

MR. LEONARD: The tube remains intact for the entire accident scenario, that the tube is always open and is not -- tube integrity is not in any way compromised by creep or any other mechanism.

MEMBER STETKAR: You blow out the seals in the --

MR. LEONARD: Blow the seals out in the drive mechanism. It's a continuous flow path to the reactor building. Maybe that's a better way to think

of it.

CONSULTANT KRESS: Now this is a flow driven by steam pressure going through this tube, carrying the fission products with it?

MR. LEONARD: That's right.

CONSULTANT KRESS: So you have to know what the fission product concentration in the steam line is and in the core.

MR. LEONARD: That's right.

CONSULTANT KRESS: And how that gets driven into the entrance to the tube, and the pressure drop through there gives you the flow rate, and that ends up giving you that curve?

MR. LEONARD: That's exactly right. So whatever the local fission product concentration is in the core, at the location where the tube fails --

(Simultaneous speaking.)

CONSULTANT KRESS: It's probably about the same -- probably about the same anywhere in the core.

MR. LEONARD: That's right.

CONSULTANT KRESS: Yeah, okay.

MR. LEONARD: Okay. So the conclusions that we came to from this sensitivity were that first of all, the frequency of the situation is very low. Certainly, the combination of the conditional

probability that the system is operating, coupled with the accident sequence frequency is well below the truncation limit that the program was designed for.

CONSULTANT WALLIS: Mark, if I may go back? Why does it blow out? I mean these things that are inserted. Because it's not pressurized, because it's sealed at the end. That's okay.

MR. LEONARD: I don't recall the design pressure for the system, but it's very low.

CONSULTANT WALLIS: So can this blow out the wire?

MR. LEONARD: Which water? I'm sorry.

MEMBER ARMIJO: The wire.

CONSULTANT WALLIS: The wire.

MR. LEONARD: The wire.

CONSULTANT WALLIS: The wire can be blown out by the pressure difference on it. So it's not so unreasonable to assume that the wire is gone.

MEMBER ARMIJO: It's a very long wire. It would probably just jam in there.

MR. LEONARD: It would probably bind up, because the wire is 20 or 30 meters long in the center of the tube, with a very small --

CONSULTANT WALLIS: I know it goes in pretty smoothly, so it comes out pretty smoothly.

MR. LEONARD: But it would also have to be completely open at the drive end for that --

CONSULTANT WALLIS: Well, whatever the mechanism is there, it's not pressure. It doesn't take those pressures.

MR. LEONARD: But it's still connected to the drive mechanism at the other end. That's right. So the frequency of this event that we're looking at is very small.

CONSULTANT WALLIS: Test it.

MR. LEONARD: There will be another test you could do. The leak area is very small. We've examined a bounding one, at roughly eight times what would probably be the more likely area, if the cable were in place.

The possibility of creep was examined, because the tube temperatures would be very high, and the effect in terms of the fission product release to the environment for the offsite consequence calculation was small enough that it did not significantly disrupt the baseline accident sequence. We're very grateful for looking at it.

CONSULTANT KRESS: Did you by any chance go back to TMI and see if these calculations agreed with the kind of iodine you saw?

MR. LEONARD: We have not.

CONSULTANT KRESS: No?

MR. LEONARD: Although we were provided, by the peer review committee, with some information about some activity measurements in the same area, same floor of the building at Fukushima. So it would be interesting down the road to see if this idea could be connected there.

CONSULTANT KRESS: They have these same probes there?

MR. LEONARD: Yes.

MEMBER SKILLMAN: TMI had 52. All were exposed to the complete chemistry and radionuclide inventory. But all are sealed at a seal table in the containment. So there was no backflow of consequence at TMI-2.

CONSULTANT KRESS: This was Henry's reason for --

MR. LEONARD: That was the initial motivation, yeah, that's right. For the last topic, I will pass off to Randy Gauntt, who examined barium and cerium behavior.

MR. GAUNTT: I have like ten slides here explaining some observed, some observations that were made relative to barium, but it seems not quite

commensurate with the whole body of work that was done in SOARCA. But from time to time, MELCOR delivers an unusual result, and then I'll get called in to explain. Is there a bug in the code, or is this a good answer or what-not?

This is one of those instances here. In the case of the short-term station blackout in Peach Bottom, we observed that the barium and cerium were dominating the emergency phase dose, and what we knew was we were releasing to the environment ten percent, in the short term ten percent of barium in the short-term station blackout, and only one percent in the long-term station blackout.

This is at the end of the road. MELCOR delivers you the answer, and now it's a bit of an archaeological dig to find out now why did it release ten percent?

What I'm going to tell you, the sum story is right here on one slide, but I have -- the subsequent slides are basically the body of evidence, if you will, kind of circumstantial evidence that leads us to why we think we're seeing additional barium in the short-term station blackout.

What we do know is it's coming out during the ex-vessel, the core concrete interaction stage. We

can tell that clearly, and that means it's being driven by the CORECON, that's the core concrete interaction, in concert with VANESSA.

VANESSA is Dr. Powers' code for doing the chemical equilibrium on the fission products speciation in the ex-vessel. So unfortunately, VANESSA does not reveal itself very well to the user. We see what's released at the end of the day.

So after digging into this quite a bit, short story is these enhanced releases are due to chemically reducing environment, that is a short period of time during the ex-vessel phase, when there is zirconium mixing with the core concrete.

The zirconium's quickly consumed, and then lesser reactive metals sequentially get consumed. But the zirconium is the strongest reducing agent.

MEMBER ARMIJO: But hold it. The zirconium's already been reacted with water to form hydrogen, and it's an oxide. So how can it be a reducing agent?

MR. GAUNTT: Because you don't oxidize all of it in the in-vessel stage. You drop a lot of zirconium with steel as well, unoxidized.

MEMBER ARMIJO: So you've apportioned the amount of zirconium that is just unreacted?

MR. GAUNTT: Yeah.

MEMBER ARMIJO: And somehow that molten zirconium will reduce barium oxide, cerium oxide?

MR. GAUNTT: I'll step you through, you know, to show you what happens here. I kind of summed it all up here. It's chemically reducing. That favors the higher, the more volatile reduced form of barium.

I'll show you a little bit about that. It is temperature-dependent. We see that in the short-term blackout, there's higher temperature in the MCCI.

That seems to have an effect on the equilibrium, and there's also strung mass transfer due to all of this gas that's percolating through the core concrete. That tends to transport the barium out. So all these things act together, and I'd just note that BWRs have a lot more zirconium in them than PWRs.

So we tend to see this phenomena in our BWR calculations. It's come up in the past. Jocelyn Mitchell used to note from time to time man, I'm seeing a lot of barium in this calculation. I wonder why that is? And we finally dug into it here.

Let's go to the next slide. This kind of shows some of the circumstantial evidence here. I put a lot on this. You'd probably see it better on your

slide. What I've indicated here is the barium release, along with MCCI temperature for the short-term station blackout and for the long-term station blackout.

It's a bit noisy. I apologize, but this is kind of how you have to root through this. What I've shown here, the red curve here is a fuel release. That's probably easiest to follow. Fuel release of barium prior to the ex-vessel. This is in-vessel release down here, around two percent.

Then at eight hours when the head fails, all this material, including unreacted zirconium, falls to the concrete and begins to react. The zirconium reacts quickly. What I've shown in the green shade there is the presence, the time period during which there is unoxidized zirconium.

Outside of this green bar, the zirconium has been consumed. It's all oxide, and so what you see is you see this big burst of release of barium during the time period that unoxidized zirconium is there. When the zirconium goes away, the release stops and the curve kind of goes over here.

We see in the long term blackout a similar period of time here, where there is unreacted zirconium, but a much smaller bump, and so here's some

of the circumstantial evidence. We have higher MCCI temperature. That's this blue line here.

For reference, I'm showing here the boiling point of pure barium metal right here, and you can see that you have lower temperatures here in the long-term station blackout than in the short-term station blackout. So far, this is kind of all circumstantial. This is the trends we're seeing.

Next slide. So why the hotter temperature? I plot here in the top curve the decay heat. So it's earlier in time, and so the short-term station blackout has a higher decay heat that's with the fuel material on that. So there's more power from decay relative to the short term.

This kind of translates over to higher chemical energy in the core concrete interaction. You have more power from the generator than in the core concrete mix in the short term than in the long term.

CONSULTANT WALLIS: So when you do the uncertainty analysis of this temperature, it will be like a switch?

MR. GAUNTT: Right.

CONSULTANT WALLIS: For some scenarios, it doesn't reach the boiling point of barium and for some it does. It would be like a switch.

MR. GAUNTT: I should think we might see that. I mean it does seem to be just right at that transition point.

MEMBER CORRADINI: Hey Randy?

MR. GAUNTT: Yeah.

MEMBER CORRADINI: Randy, can I ask a question? I just want to kind of follow on Graham's point. Is it really a switch, because you've got a partial pressure that's always --, or is there some chemical reaction that until you get it, until you actually get to a boiling point where you're generating essentially its own vapor, that you change the phenomena.

I would just think barium's always going to be outguessed at some partial pressure through the gas bubbles in your MCCI modeling, or am I misunderstanding?

MR. GAUNTT: Let's go to the next slide. I think it's tied in with the Gibbs energy. Now I don't know if there's any chemists here. If Dana was here, he would tell us all about this. Advance it.

MEMBER CORRADINI: I'm sure he would.

MR. GAUNTT: Yeah. The next slide, I guess this is more circumstantial evidence here. We have in the short-term station blackout -- here, I'm

showing gas generation. There's a lot more gas percolating through the MCCI in the short term compared to the long term.

Now let's go to the next slide and try to put all this together. So it's not, Mike, actually as simple as calculating a vapor pressure. I show here on the chart vapor pressure of barium metal over pure barium, and the pressure of barium oxide over pure barium oxide.

Now that's not what's happening in the complex system. I just kind of show this for reference. But you can see that the barium metal is more volatile than the barium oxide, and you know, the difference here is just a few hundred degrees Kelvin that separates these two. So that shows you that it's real sensitive to temperature.

Now what actually happens in VANESSA, it's chemical equilibrium code, and it calculates equilibrium of a multi-component system. A lot of equations are being solved within VANESSA, and basically it's an approach that involves minimizing Gibbs energy, and Gibbs energy is temperature-dependent.

MEMBER CORRADINI: So Randy, Randy if I might just ask a question, and again maybe you guys

are all going to cleverly pass this off to the uncertainty person.

(Laughter.)

MR. GAUNTT: And I'll be catching a flight while you're doing that.

MEMBER CORRADINI: As I said, you're very clever. So I guess my point is if you want to unscramble this, can't you not just interdict into the calculation and just tell MELCOR that there's less zirconium?

In other words, if you think it's a combination of decay heat and zirconium content, and essentially the sparging rate that's affiliated with gas release on the concrete, one could unravel this by strictly changing one of these things from a sensitivity standpoint, reducing zirconium but keeping the higher decay.

So you can actually take what you're saying and see if by some qualitative hypothesis, that this essentially reduces it significantly. Do you see where I'm going?

MR. GAUNTT: Oh yeah, yeah. I should have talked to you sooner. You know, that's something we could definitely -- that's definitely doable.

MEMBER SCHULTZ: But that would address

one of the variables, but there's also --

MR. GAUNTT: It would in temperature as well. It would confirm our circumstantial case here.

MEMBER ARMIJO: You know, I don't know what you've input into the VANESSA code, and I don't know the code either, but there's so many components that are involved here. You've got stainless steel. You've got zirconium in an oxide form and perhaps in a metallic form.

You've got stainless steel in a molten form. At some point in here you've got some concrete. You've got so many variables that go into this thing. It seems to me like you're -- it's oversimplistic to just pick out zirconium plus barium oxide. Oh by the way, you've got --

MR. GAUNTT: Well VANESSA, of course, is handling the full system, and I picked out a couple of the operative reactions, to just illustrate what's going on.

MEMBER ARMIJO: So you input all of these elements into the VANESSA?

MR. GAUNTT: They are in the VANESSA database, and we don't input anything. They're all there. Now MELCOR delivers to VANESSA so many kilograms of zirconium metal, so many kilograms of

zirconium oxide, UO2, stainless steel, and all this mix drops down onto the floor, and then that's when VANESSA takes it on.

MEMBER CORRADINI: I think though Sam, I guess I'm somewhat sympathetic to Randy kind of unraveling it, because I do think, at least in old calculations that I remember, the presence of a lot of zirconium totally changes how the chemistry behaves, because it's stealing oxygen from everything, and it's keeping the temperature very high.

If you add that to the fact that you're early in time with the decay heat, I'm just looking for ways to very simply unravel it. That's the only reason I asked a question about this, because in some sense I think I see where Randy's going with this.

MR. GAUNTT: So the equations that I pulled out here, they're of course reversible. But here we see zirconium reducing barium oxide. Barium oxide would be the, you know, the preferred form otherwise. But zirconium is reactive enough to reduce the barium oxide and produce this more volatile barium metal form.

Additionally, there's hydrogen present in the MCCI because of the zirconium reacting with water. So this presence of hydrogen down here in the third

equation, that's another strong reducing component that's pushing the equation over to produce barium metal.

It is temperature-dependent. You have to have the zirconium there. Without zirconium, you don't see this reduction in the barium, and then lastly, this strong gas sparging is, drives a mass transfer.

So what it does is it takes these volatile forms and sweeps them out, and sort of keeps the equations constantly in disequilibrium, and then VANESSA tries to put it back into equilibrium. So these equations are constantly pushing to the right-hand side, because of the mass transfer.

MEMBER REMPE: When you're relocated and you're doing the MCCI, how much unoxidized zirconium is there roughly? I'm sure it varies by scenario and time, but is it 40 percent, 20 percent, 10 percent?

MR. GAUNTT: Oh my gosh. Mark, do you recall?

MR. LEONARD: Twenty to thirty percent.

MR. GAUNTT: Twenty to thirty percent.

MEMBER ARMIJO: Is that mostly channel material? Is that where it's coming from?

MEMBER STETKAR: Mark, you've got to come

up to the microphone.

MR. LEONARD: I think representative for these cases would be residual unoxidized zirconium in the neighborhood of 30 percent. When it arrives in the lower head, you don't know what, where it originated from. All you know is it's zirconium. Whether it came from unoxidized channel box or a clad, it's debris in a big pile. Where it originated from, you don't know.

MEMBER ARMIJO: And there wasn't enough steam or water to react with it?

MR. GAUNTT: In vessel? It's reacting in the core, and then it's incomplete, and then it runs through its melting point, and the metallic part of it runs down and freezes up. So you always end up with some fraction of unoxidized zirconium, both from the cladding and the channel boxes as well.

MEMBER SCHULTZ: Randy, the last bullet, strong gas sparging, again drives the mass transfer here. So that has a, as you described it, a dramatic effect on the result. Is that an element that's an assumption in the model, or is it well-based upon experimental evidence, the strong gas sparging in the MCCI?

MR. GAUNTT: I think it's well-based in

experimental observation. It's inescapable that you're producing a lot of carbon, in this case carbon monoxide and carbon dioxide, as well as hydrogen. Those are the principle gases that are coming off of the concrete.

MEMBER SCHULTZ: So then it does drive back to what Mike was describing, the content or the amount of zirconium that is in the area at this time in the evaluation?

MR. GAUNTT: Yeah. The principle role of the zirconium is to produce a reducing, sufficiently reducing environment that allows that barium to form in the first place. All of the carbon monoxide and carbon dioxide that's coming out of the concrete, it's just bubbling through this like a witches brew, right.

So it's constantly sweeping the gaseous products out, including the volatile barium, and that keeps the equations sort of unbalanced.

MEMBER SCHULTZ: And it drives the temperature, the zirconium.

MR. GAUNTT: And the zirconium drives the temperature up. So those things are all -- it would be interesting, you know, to carry out Mike's exercise, to see if we could, you know, physically turn some of those knobs and confirm that. That did

not occur.

MEMBER REMPE: Of the assumptions about the unoxidized zirc are again assumptions based on limited data, especially with light phase progression and some of this occurring; correct? Or how strong are the databases to support some of this?

MR. GAUNTT: Yeah. Any of the assumptions you're talking about, they're deeply embedded in the melt progression models, and there are no assumptions that we're --

MEMBER REMPE: Right, I know that. But the models themselves are based on limited data is the point I just wanted to bring up again.

MR. GAUNTT: They're based on the experiment set we have, and we leaned heavily on the PHEBUS test. That's true.

MEMBER ARMIJO: I just wanted to ask you a question. If hydrogen is such an effective reducing agent, assume you reacted all of the zirconium and just made a lot more hydrogen, would this effect still occur?

MR. GAUNTT: If there was no zirconium and if you back up, yeah. As soon as the zirconium goes away, well this doesn't show it. It's one of the other slides, that one.

As soon as the zirconium is consumed, and I was watching this, and this is one thing I can tell. As soon as the zirconium disappears, the barium release shuts off. So it's really tied in with the reducing properties of the zirconium.

MEMBER ARMIJO: So hydrogen alone won't do it.

MR. GAUNTT: So really it's the same -- it's kind of the same story with cerium, cerium and to a lesser extent lanthanum, show these similar trends.

The next slide, I think, shows I just replicated this for the cerium release, and you can see prior to the in-vessel release of cerium was vanishingly small, but again, when it went ex-vessel, and for the period of time that zirconium is unoxidized, you see that bump of cerium here.

A much smaller bump over here for the long term blackout, and again, we think that's temperature dependence of the equilibrium. And finally -- okay, so with cerium, the operative reaction appears to be this one here, where cerium oxide is, goes to the gaseous form.

So it's essentially the same story, and like I said it is circumstantial. I checked this with Dana, and he says this is solid. That's what, this is

what VANESSA should be doing. I think that's all I've got on cerium.

MEMBER ARMIJO: Well, I've got to ask the question, you know. From what you heard, have you gotten anything out of Fukushima that provides any data of barium and cerium bursts?

MR. GAUNTT: You know, I spent a month over there, and I was asking people do you see barium, and you know, they do report -- there are some reports where they show some barium.

But I don't see any smoking gun, in terms of this. I don't have -- I haven't picked up on anything on Fukushima, as far as barium, you know, heightened barium release, although I'm pretty sure Unit 1 went ex-vessel.

MEMBER ARMIJO: That's what most people think, but maybe more than one unit went ex-vessel.

MR. GAUNTT: And maybe Unit 3.

CONSULTANT KRESS: As far as I know -- sorry, Mike. As far as I know, VANESSA has no experimental validation.

MR. GAUNTT: Tom, I don't know where it is. If it's there, I have not seen it.

CONSULTANT KRESS: I haven't seen any. It may be. That's why I'm asking is there any.

MR. GAUNTT: I do not know where it is. If there is a basis, I think it's VANESSA was put together strictly on a chemical basis, and that was some years ago.

CONSULTANT KRESS: My chemistry's little rusty, but I was assuming Gibbs-free energy was applicable only in the gas phase, not the liquid phase. Somebody who's a better chemist than I, maybe they could --

MEMBER ARMIJO: It works. You know first of all, it's hard to think that this is an equilibrium situation here, you know, and you've got --

MR. GAUNTT: I do know that the VANESSA assumption is it takes, it calculates the equilibrium, and assumes it --

MEMBER ARMIJO: Yeah. That's the only way you can use it.

MR. GAUNTT: I don't think there's any rate associated with that. The rate step seems to be the mass transfer from the gas sparging. So there you have it, barium.

CHAIR SHACK: Just before you go, I had one question just on a curious footnote in the Peach Bottom report, and that is that their procedures for using their B.5.b to vent the containment, they set it

up so they vent the drywell rather than the wet well.

That doesn't seem like the greatest idea in the world. Is there some reason they did that? When you talked to the people, I mean it seems a little curious.

MR. CHANG: Well, I guess ultimately, are you talking about the 18 inch line or --

CHAIR SHACK: Well, apparently they're not using the B.5.b to open that line. They just open the drywell. That's what the footnote says.

MR. CHANG: Sure. I guess ultimately, the B.5.b procedures, when we had discussions with them early on the SOARCA project, it was decided that a 16 inch line would be the preferred pathway, to avoid any sort of concerns about habitability within the reactor building itself.

I guess Mark looks like he's got some input into that. Well never mind. So ultimately, what we did, I was just there with Bob Prato this past November, talking to the licensees, walking through procedures and assumptions within SOARCA.

We discussed with them the potential ability to use the B.5.b equipment, the two pony bottles, to be able to open up the 16 inch hardened vent line. They said yes, it is possible to use that.

The one thing to note, though, is that containment venting, at least within SOARCA, I don't think it's needed until about 20 hours into the scenario, at least for the long-term station blackout.

So the intention was, rather than to put a weight towards mitigated and unmitigated, without having it done in HRA, what we did was we analyzed two scenarios. One scenario, which assumes that mitigative actions occur, and included in that, use of a 16 inch hardened vent line, and one where --

CHAIR SHACK: That sorts of contradicts the statement that you only used the things that were in their procedures now.

MR. CHANG: Sure, and I guess --

MR. SCHAPEROW: That's the mitigating case.

CHAIR SHACK: Yeah. But he said in the mitigated case, they used the 16 inch line, but their current procedure said you don't use the 16 inch line. You use the drywell line.

MR. LEONARD: This is Mark Leonard. It depends on when you say "this procedure," which procedure you actually mean. There are several procedures that govern the opening and actuation of containment vent lines, and there is a selection

process of which vent pathway to take.

CHAIR SHACK: Well I mean again, I'm only reading the footnote.

MR. LEONARD: The implementation of B.5.b is that it existed at the time that we were doing the review for containment venting. The B.5.b equipment was incorporated into the vent procedure for the main drywell and I believe the wet well vent, the larger vents, but it had not yet been incorporated into the 16 inch hardened pipe vent pathway.

The fact that the B.5.b specific features had not yet been implemented into that procedure, didn't obviate the selection procedure, which would tend to prefer the hardened vent for reasons that are mentioned in that procedure, that talk about contamination or difficulties in the reactor building of steam filling if that vent path were to be compromised somehow.

So that mixture of input is what led us to choose one, in spite of the fact that strictly speaking, the B.5.b portion of the implementation procedure is not yet incorporated into the hardened pipe vent procedure itself. I hope that helps.

MR. CHANG: And the one thing to note, just we did a couple of back of the envelope

calculations, and we might not have necessarily included it in the report. But the four other vent lines, I think we also looked at a six inch vent line as well, and whether or not that would be sufficient to keep RCIC running and lower containment pressures, and really that was sufficient as well.

So I guess the point for is there are a number of vent pathways available for the licensee.

CHAIR SHACK: Any further questions? I think it's time for a break. Thank you very much for a very interesting presentation. We'll be back in 15 minutes, 11:10.

(Whereupon, a short recess was taken.)

CHAIR SHACK: We will come back into session. Now we got lucky on the first two presentations, where one ran short and the other one ran long. But we may not be so fortunate for the rest of the day, so we might have to try to worry a little bit more about schedule.

MS. GIBSON: We'd be happy to come back a second day if it lasts long.

CHAIR SHACK: No. We have St. Lucie fun tomorrow. We're ready for the updated Surry analysis.

MR. SCHAPEROW: My name is Jason Schaperow. I'm with the Office of Nuclear Regulatory

Research, and I'm going to talk today about the update Surry analysis that we've done since we met with you last in 2010.

This updated analysis came out of what was going on in 2010, which was we were in the middle of a peer review, and we also gave a copy of the Peach Bottom and Surry reports that we had drafted to the two licensees. We said here, take a look at this, and let us know if there's any factual errors, what's called fact check.

Sure enough, Surry came back with one and it was a doozy. So it required us to completely go back and completely revise the ISLOCA analysis, and we actually started on this kind of in earnest in the beginning of 2011. We were banging away at it, and in March, the big accident happened over in Japan, and we got yanked off it again.

So I'd like to think we would have finished -- this took us about a year to go through this reevaluation of the ISLOCA, but we lost a few months because of the Fukushima accident. We were busy in the ops center and on call all kinds of crazy hours.

Anyway, as I said, we did have -- we revised the analysis. The licensee identified that an

important pathway in our model, that is between the Safeguards buildings and the ops building, was not open, as we had assumed it was.

It was a pipe tunnel and they had filled it previously a number of years back with penetration sealant.

CONSULTANT KRESS: Is that typical of other PWRs though?

MR. SCHAPEROW: I can't really -- I don't have an answer for you on that as far as.

CONSULTANT KRESS: They'll probably want to look --

MR. SCHAPEROW: There's other buildings, and they're all connected by pipe tunnels.

CONSULTANT KRESS: Yeah. Probably wanted to look for typical PWRs. I might have assumed that they weren't all filled that way.

MR. SCHAPEROW: Yeah. They said the reason they filled this with penetration sealants is because they had a contamination issue, as far as surface contamination. One area, they didn't want contamination in one area to end up in another area. It was supposed to be a radiation barrier.

MEMBER BLEY: And this was a whole tunnel?

MR. SCHAPEROW: Well, the tunnels got --

you know, these pipes going through -- it's kind of an opening maybe about twice as big as the pipes, and what they did is they'd sprayed, they had sprayed penetration sealants to kind of fill up the open area, so that air would not flow between the two buildings.

MEMBER SKILLMAN: But in many cases, those gaps were filled for fire protection. A lot of that was --

MR. SCHAPEROW: Yeah. We talked about this, and they said it was for radiation.

MEMBER SKILLMAN: So I think it's probably safe to assume that this is typical and not atypical.

CONSULTANT KRESS: That's good to know, yeah.

MR. SCHAPEROW: Actually, when all was said and done, it turned out not to be an issue. There actually is other ways for these two buildings to communicate. The water would flood up in this area and push the sealant out. So it's not a structural thing. It can't take a lot of pressure.

But it turned out that in the final analysis, excuse the pun, it was not a factor. But what it did do is it caused us to go back and re-look at our entire ISLOCA model that we had built. So the original model did depend heavily on deposition in the

aux building. It was where we had most of the deposition going on.

We thought we would lose that in this new model. We think that we'd better take another look at this. The other thing that was, we had gotten from the licensee and the IPE early on was that the lower head safety injection pumps could flood as a result of the ISLOCA, and so when we did the reanalysis, we found out that that might not be the case.

So we went back to the site, again about a year ago. We went, we visited the site. We talked mitigation. We did a walk-down of all these buildings that were just outside of containment where the ISLOCA was happening or would happen.

We developed a detailed model, a nodalization of these areas, the Safeguards area, the containment spray pump area and the main steam valve house. We also developed a nodalization for the ventilation system for this area, and finally, we developed a detailed model of the piping between the cold leg and this building. We didn't have that before.

So we added this new detailed piping model between the cold leg and the Safeguards area, and the reason we did was because we thought that we were

going to see a lot of deposition in this piping, about 180 feet of piping with a five-inch diameter. So it's a relatively modest-sized pipe, very long. We've got aerosols going down it, so we thought that this would be important.

So we added new aerosol models to MELCOR to treat turbulent deposition in straight pipes. We also have quite a number of elbows and other flow regulators in this piping, and we added models for that as well. Then we validated these models against the LACE test, the LACE bypass test in particular, and we also benchmarked it against the VICTORIA models.

MEMBER CORRADINI: So can I ask a question at this point? So you did a hand calculation that indicated that this would be an important effect, and this allows you to essentially improve MELCOR in the process, or what was the motivation for this last one?

MR. SCHAPEROW: Well, actually one of the peer reviewers had asked us why aren't you treating turbulent deposition in the low head safety injection piping? And we responded well, we have a lot of deposition in the Safeguards area, in the aux building. So when we went back to redo the analysis, we took another look at it, based in part on the comment of the peer reviewer, that it might be

important.

Also, I went back and looked at the LACE test, and those tests were specifically run for this scenario, in an attempt to be prototypical, and they found a lot of deposition. The most prototypical LACE test, LA-1, 98 percent deposition in the pipe.

MEMBER CORRADINI: These are the tests at HEDL?

MR. SCHAPEROW: Yes.

MEMBER CORRADINI: At Hanford? Okay.

MR. SCHAPEROW: That's correct. Actually, I think it was greater than 98 percent. That was within the limit of their measurements. I don't think -- their pipe was a little small. I think it was about 2-1/2 inch diameter. I think it was roughly, I don't know, 80 feet long.

But it was enough to make us think that we really should be treating this. If we're going to claim realistic best estimate, or I should say more realistic, you know, best estimate that we can do, we felt that we needed this modeling.

So we took the time. It took us a few months to get it in and get it debugged. It was a heroic effort by the folks at Sandia, but it was well worth it, as you'll see in the analysis that I'm going

to present.

MEMBER REMPE: In general, like in the documents, let's MELCOR 1.8.6 was used, but now this is one example where you've added new models.

How different were the versions of MELCOR used for Surry versus Peach Bottom, and you refer to it as MELCOR 1.8.6 Star or something, since you have -- there were other examples one can cite where model changes were made in the study?

MR. SCHAPEROW: Yes, yes. No, that's true. The project took place over six years, so I think I'd like to characterize model changes made during that time as bug fixes. This is perhaps the one exception, where we actually added a model to the code.

But I don't -- there was no -- the way the project was conceived actually in the very beginning, we knew we were going to have a long campaign of several years between the calculations and eventually we had the peer review.

So the way the project was conceived, we actually had at the very first year, in 2006, we sat with some experts and went through the MELCOR models, to see what, how the code was. Was it up to par? Was it ready for this campaign we were going to do?

So I'd like to think that the code is basically the same as back in 2006. This one --

MEMBER REMPE: Well take this bottom hot leg model as an example that was added.

MR. SCHAPEROW: That's a result of the 2006 review.

MEMBER REMPE: Right.

MR. SCHAPEROW: Actually, the ERIN expert, Jeff Gabor, says you really need to think about the potential for SML creep rupture. So we added modeling to treat that.

MEMBER REMPE: But generally speaking, except for this turbulent deposition, whatever version of MELCOR was used was used for both --

MR. SCHAPEROW: Yes, that's correct. And the reason this wasn't identified in 2006 is because the ISLOCA wasn't one of our original sequences.

We had other sequences for Surry, but this one didn't come up until we went on our first site visit, and we compared PRAs, our SPAR models pointed to your PRA say, and then out came the ISLOCA. So we didn't have the benefit of that earlier review.

But we did have the benefit of the peer review, and when we met with the peer reviewers, they said well, why aren't you doing turbulent deposition

in the piping, and we got into resuspension and all that stuff. But they were right. We did need it. It would certainly improve the realism of our analysis.

So the new analysis came out with regard to start time at core damage. About the same as before. We're now at 13 hours; before we had nine hours.

The reason for this result is the operators do it by procedure. They would stop and isolate both of the low head safety injection pumps, and two of the three high head safety injection pumps. And with regard --

MEMBER STETKAR: Why would they do that? Do they meet the DoD criteria for shutting, resetting safety injection for some of those pump slots, and why do they need them?

MR. SCHAPEROW: The way we did the mitigation evaluation was we met with the operators. We asked them to go through their procedures, and tell us, for this scenario, how they would -- what they would do to mitigate. What actions would they take? They said well, we have procedures for this.

MEMBER STETKAR: Everybody has procedures for everything. I want to understand in this particular scenario, why they would get to the

conditions where their procedures would absolutely instruct them to reset safety injection, and shut off those pumps?

MR. CHANG: Well, in regards to -- this is from memory, but in regards to the high head safety injection pumps, I think it's a specific step as they walk through it.

MEMBER STETKAR: Only when you reset safety injection is my recollection, unless people have really changed the philosophy of their procedures, and to reset safety injection, you can't turn those pumps off, in most plants anyway, unless you reset the Safeguards actuation signal.

You can't procedurally reset the Safeguards actuation signal until you meet criteria about pressure, level, subcooling margin, which you would not necessarily meet, I don't think, in these scenarios. So that's why I'm curious.

MR. CHANG: And from my understanding, which is somewhat limited, I understand that the licensee has developed procedures specifically for ISLOCA events, I think, and I need to dig them up to look at them.

MEMBER STETKAR: Different than the fundamental EOPs that the operators are trained on?

MALE PARTICIPANT: And how would they get to them? How would they know?

MEMBER STETKAR: Philosophically fundamentally different.

MALE PARTICIPANT: Yeah.

MR. CHANG: And I'd have to get back to them, just to look through them. We have a copy. I have a copy at my desk, but --

MEMBER STETKAR: In some sense, it doesn't make too much difference, because there is an unmitigated, I guess, scenario that you look at. But the curiosities that I have here that --

MR. SCHAPEROW: Well but again, the reason we put these in here was because we were attempting to be realistic, you know, what would happen.

And based on our tabletop exercise and discussions, and actually in one case they actually went into the simulator with the procedure and walked through the procedures too, and when they got to these steps, they marked down the time they would get to these steps. That was what our modeling was based on.

MEMBER STETKAR: For a large interfacing system LOCA.

MR. SCHAPEROW: For a rupture, for a LOCA outside containment, correct.

MEMBER STETKAR: A fairly large LOCA outside containment, six inch line.

MR. SCHAPEROW: Yes.

MEMBER STETKAR: That's surprising. Okay. That's surprising.

MR. SCHAPEROW: So with regard to the releases, the environmental releases, we calculated iodine, environmental release of 16 percent offsite, and a cesium release of two percent. Our earlier analysis was a nine percent for both cesium and iodine.

We had calculated significant deposition in the reactor core system, and in the low head safety injection piping. We also saw some iodine revaporization as the low head safety injection piping heated up. Iodine is more involved; so is cesium.

But the iodine did subsequently deposit in the Safeguards area, and also in the filtration system, which was operating. This is not a station blackout; the filtration system would be operating.

MEMBER STETKAR: Actually Richard, if you could just make a note. I'd really be interested in seeing those procedures that you used.

MR. CHANG: Sure.

MEMBER STETKAR: Because you know, this is

something that he needs to check.

CONSULTANT KRESS: Is this cesium iodide that's revaporizing?

MR. SCHAPEROW: That's correct, cesium iodide. We used that vapor pressure in the model. Finally, we used this analysis to identify conservatisms in the PRA model, and additional mitigation measures that are practical.

We noted that about two hours and 40 minutes into this event, the pressure and other parameters are low enough that they could start RHR and terminate the event. This is something that was not part of the PRA model that was our starting point.

MEMBER STETKAR: At, at --

MR. SCHAPEROW: At two hours and 40 minutes.

MEMBER STETKAR: Surry RHR is separate from LHSI?

MR. SCHAPEROW: Correct.

MEMBER STETKAR: Separate pumps?

MR. SCHAPEROW: Separate systems.

MEMBER STETKAR: Good. Not typical of a lot of PWRs.

MEMBER SCHULTZ: Jason, you noted that that could be done, but did not take credit for it --

MR. SCHAPEROW: Yeah. It wasn't taken credit for in the underlying PRA. We said well, why are you going to core damage? Well, you know, we didn't take credit for it. We just assumed that we had this hole. The refueling water storage tank was not refilled, core damage.

So again, that's kind of trying to show the benefits of this more detailed approach using MELCOR, is that once you try to -- when you put everything together in an integrated model like this, these things start becoming obvious. Like wow, you know. They could have used RHR. There's other things they could have done as well to mitigate, you know. I'll get into that later.

MEMBER SCHULTZ: This is fine. Thank you.

MR. SCHAPEROW: Just so to take a step back, I want to talk a little bit about the reanalysis, how we did it. We developed a new full plant model for MELCOR, to do an integrated analysis of ISLOCA. This new model includes detailed modeling of the adjacent buildings.

It includes the Safeguards area, the containment spray pump area, main steam valve house, and the ventilation system for these areas. The one area where we're not fully integrated is the low head

safety injection piping deposition.

The full plant model uses a separate model we built, a smaller model just for the piping, to calculate how much stuff deposits in the piping, and this other model, which is run separately, we take the result from that and we put it into the full plant and we re-run the full plant case.

The new separate effects model includes the new deposition models, turbine deposition, inertial deposition. So what I'm going to do is I'm going to go first through the full plant model, and then I'll just backtrack a little bit, just show you how we came up with the DFs for the piping.

MEMBER STETKAR: Just out of curiosity, because I haven't really studied a lot about all of the nuances here. I think identifying prototypicality issues may be important. Do you have any sense of how the results might change if Surry didn't have the RHR system separate?

MR. SCHAPEROW: For our unmitigated case, we did not credit the RHR system.

MEMBER STETKAR: Okay. Thanks.

MR. SCHAPEROW: So you'll see this -- you'll see basically unmitigated cases. I'll get into it. It only credits the operator reactions involving

basically shutting the pumps off, and one other one.

MEMBER STETKAR: But it does credit that?

MR. SCHAPEROW: Yes, it does, it does.

MEMBER STETKAR: Thanks.

MR. SCHAPEROW: Okay. So the full plant model, I just want to show you a piece of it. This is the piece of -- this is an important piece, because this is where the break is. The pipe break is in these buildings adjacent to containment.

If you look all the way at the left, all the way on the bottom, you'll see a volume labeled A-54. That's the bottom floor of the Safeguards area. That's where the pipe break is. That's the thinnest wall piping, and if you look a little bit more over to the right, you'll see the right half is an elevation view of the buildings.

Again, A-54 is shown there in the bottom of the Safeguards area. The Safeguards area is connected to the pump spray area right next to it, and then next to that it's the valve house, the main steam valve house. Next to that, which is not shown here, is the aux building.

That tunnel that I was talking about, that's now filled with penetration sealant, is all the way at the bottom right-hand of this sketch.

MEMBER BLEY: I'm sorry. These numbers --

MR. SCHAPEROW: Oh, I'm sorry.

MALE PARTICIPANT: Where is that?

MALE PARTICIPANT: It's over here.

MR. SCHAPEROW: Right here, right tunnel of the aux building.

MEMBER BLEY: Oh, okay.

MR. SCHAPEROW: So this is what originally, was the original flow path. The fission products would come in at A-54, move across through this area, and into the aux building.

That was our original thinking a few years back. But again, we did it. We went back to the site. We got better information. We have good information now, and we've got a better model now.

Also, I just wanted to mention briefly that we have the ventilation system model. We have both the supply and the exhaust of the ventilation system now, and of course this ventilation system includes the HEPA filters and the charcoal filters, all the stuff that's in place for accidents.

The next chart is a timing of events table, something we typically put together for these accident calculations. It shows not only the different actions that would be taken as well, but it

also shows just when things happen phenomenologically.

Again, the ISLOCA scenario is a LOCA outside containment, with no refill of the refueling water storage tank. That's the base scenario.

But within that scenario, we wanted to see well, what would take place? What actions would happen? So as I said, to repeat myself, we went to the licensee. We said okay, what would you guys during this type of an event?

They said, well we would secure these pumps, and we'd begin a cool-down with the steam generators. That's what our procedures would direct us to do.

MEMBER BLEY: I hate to ask an almost insulting question, but I am. When you go to the utility and ask them how you do this, who were the people you actually asked?

MR. SCHAPEROW: We met with senior reactor operators. I think that was the main -- we had PRA staff there as well, both PRA staff and senior reactor operators.

MEMBER BLEY: And this was directed at the operators and not the PRA staff?

MR. SCHAPEROW: Yes.

MEMBER BLEY: Okay.

MR. SCHAPEROW: I'm struggling --

MEMBER BLEY: Sometimes you get really different answers, because the PRA staff tells you all the things I could do, where the operators tell you how they really do use their procedures.

MR. SCHAPEROW: Yeah. I'm struggling a little bit about this because the person who was the lead in this area is no longer with us. He passed away a few months ago, Bob Prato. Richard's picked that up, you know. Remember, you weren't part of the ISLOCA stuff too much.

MR. CHANG: A little bit, but --

MR. SCHAPEROW: But yeah. We had, they were all involved in the conversation, because sometimes the PRA staff can help the senior reactor operators understand the event a little bit.

MEMBER BLEY: Most often we find that the operators can help the PRA staff to understand how the plant works.

MR. SCHAPEROW: There's a little back and forth. It's done in a group environment. We're all in the room together.

MEMBER ADBEL-KHALIK: Did you ever want this scenario in a simulator?

MR. CHANG: The licensees offered to run

the at least beginning portion of operator actions early on, to see I guess ultimately what the timing would be.

So they walked through their procedures, at least for the first, I guess, maybe 20 or so minutes, first couple of steps about securing pumps and things like that, and provided us the timing. We actually did not observe them run the simulator.

MR. SCHAPEROW: Actually, we talked to them about that, and we got into the part where we said -- they asked how important is this to you? Should we take time out? Should we put the operators in the simulator and go through this?

So we actually took a few weeks or a month, and we ran some MELCOR calculations, and we came back to them and said look, we really need to know this well. They said okay, fine. We'll go into the simulator. We'll put the operators in the simulator and we'll do it.

That's kind of how it evolved, which was back in the, like July time frame maybe, a couple of months after Fukushima.

MEMBER BLEY: Just a skeptical point of view, because I've worked a lot doing the same kind of thing.

If you lay out the scenario ahead of time and what you're worried about, and then ask them what they'll do, you sometimes get an answer different from setting up the situation, maybe the simulator, with no preconceived idea of what's coming, and seeing what they really respond to it.

So it can give you very optimistic results sometimes. Go ahead, but that's a place I'd be skeptical a bit about.

MEMBER STETKAR: I'd say especially given some of the times that they've violated.

MR. SCHAPEROW: Yes. Results of the calculation. It's a LOCA, and sure enough, you know, when the accident happens, pressure comes down. This just shows just pressure dropping off in the first couple of hours. A little further out on the time line, down around 14, 15 hours, you'll see a couple of pressure bumps there.

This is core damage going on around here. So we have molten core debris falling down into the lower plenum where there's still some water. So we're seeing some pressure rises during that time.

With respect to water level in the vessel, the water level is maintained. They are shutting pumps off, but even one high head safety injection

pump, which they are running, is plenty. They get to around six hours and the refueling water storage tank runs out of water. So then you see the fall off in the water level.

Moving to refueling water storage tank inventory, again this plot shows that the tank is empty at six hours. You'll see a couple of bends in the curve. This is where they're shutting pumps off. So when they shut the first pump off now, the flow rate out of this pipe is a little less and a little less.

With regard to fission products, this next plot shows where the iodine goes. Again, as Tom Kress pointed out, MELCOR models iodine vapor pressure as being cesium iodide. The iodine does end up in many places. Some of it ends up in the very beginning -- when it releases from the core, it ends up in the RCS. Some ends up a little further downstream in the low head safety injection piping.

Then further downstream, some ends up in the Safeguards area, and then some of that is sucked into the filtration system. The net results was a fission product release of roughly 16 percent. That's the red line.

Moving to cesium, cesium is a lower

volatility than iodine, so more of the cesium ends up closer to the core. So you'll see here the main two places where the cesium ends up. It ends up in the RCS and the low head safety injection piping, and relatively small amounts of it end up further downstream in the Safeguards area and the filters.

Okay. Just turning now to the separate effects model that we used, to come up with aerosol deposition to put in, aerosol deposition in the piping put into the full plant model, this shows our nodalization. About in the middle of this is a dashed line, showing the containment boundary.

We have seven nodes inside the containment for this piping, and then one node outside the containment. Then the pipe break is in the thin wall pipe just past this flow element. This is an orifice. It's a seven inch orifice in the line.

Turning to aerosol deposition, MELCOR does have a range of aerosol deposition models in it. It's got what we have typically used over the years for these analyses. It's got settling, it's got thermophoresis, diffusiophoresis. But in the past, when we did the steam generator tube analysis back in the late 90's, we used VICTORIA for the really detailed modeling of the tubes and the Venturis and

the restrictions and the things that would go with that.

So we didn't have that in MELCOR at this stage. So we decided that we really needed to put it in. So we did add new modeling to MELCOR for turbulent flow conditions.

CHAIR SHACK: Remind me. How well did the VICTORIA model compare with the ARTIST experiments that were done later? Were you over-predicting the contamination? You don't remember?

MR. SCHAPEROW: Well, I'm not actually sure we ran VICTORIA for the ARTIST test. I don't know if anybody else here at Sandia, any of our Sandia staff, you know.

MEMBER CORRADINI: Can you give us, just to remind us of the history? We should remember this, but I don't. VICTORIA was disconnected, simply because it was not an integral part of MELCOR analysis. It was run separately? I didn't understand --

MR. SCHAPEROW: Well, the initial work that was done, I guess a lot of this goes back to the 80's. The NRC was developing a set of detailed severe accident models. First and foremost was SCDAP/RELAP5, which treated the core, all the in-vessel stuff, the

RCS. But it didn't do the containment and it didn't do fission product chemistry and aerosol deposition.

So in addition to SCDAP/RELAP5, we had the contain code for the containment part of the analysis, and we also had VICTORIA. VICTORIA was a detailed fission product chemistry and aerosol model. But in parallel with that, we were also developing MELCOR, which is supposed to kind of be integrated. It had everything in it, soup to nuts.

But of course we couldn't have the same level of details we had in SCDAP/RELAP5, or VICTORIA. So back in the 90's, when we were doing the steam generator tube rupture analysis, we ran SCDAP/RELAP5, and then we took the heat transfer fluid flow conditions from that and imported that into VICTORIA, and ran VICTORIA with all of its detailed aerosol models, to figure out what the deposition would be in the steam generator.

MEMBER CORRADINI: Okay. All right, thank you.

CONSULTANT WALLIS: These things deposit. Why do they stick?

MR. SCHAPEROW: They are -- these aerosols are partially molten. They're coming off the core. They've got multi-components. There's a lot of

different --

CONSULTANT WALLIS: They solidify?

MR. SCHAPEROW: Pardon?

CONSULTANT WALLIS: They solidify?

MR. SCHAPEROW: Most of them, the aerosol material, it's got, it's made up of a whole world of chemicals.

CONSULTANT WALLIS: What does it do? Does it produce sort of a fuzzy deposit or smooth deposit, or what does it look like? Is it hairy, sort of things that stick out and --

MR. SCHAPEROW: Yeah. Well --

CONSULTANT WALLIS: I'm just wondering how much you can deposit without getting it reentrained?

MR. SCHAPEROW: Yeah. Well, actually we thought about the reentrainment issue. We talked to the peer review committee about it. I mean I don't mind it getting reentrained as far -- I mean as far as the MELCOR, the question is where does it end up, you know.

CONSULTANT WALLIS: What's it look like? Does it get reentrained? Why does it stick, all kinds of questions?

MR. SCHAPEROW: Sure. There's a lot of issues there. For me, kind of the bottom line issue

was does it become a micron-sized aerosol again? It certainly -- actually, I saw some analysis that was done by industry.

I think it was a Polestar, Applied Technology. They did some analysis for ISLOCAs a few years back, and they concluded that it would kind of spread out and be all along the whole piping length, because it's kind of liquidy, kind of sloshy.

But other people might believe that it tends to be kind of solid. So it's kind of all get stuck up in the initial part of the piping, and not go anywhere. So it is going to get kind of dragged along the pipe? The flow rates are very high.

CONSULTANT WALLIS: And how thick is it? How thick?

MR. SCHAPEROW: Well, if you take the view that it stays where it eventually deposits, it will plug the pipe.

CONSULTANT KRESS: The cesium iodide and the cesium borate solids at these temperatures. So if you're talking about those two, they're not liquid.

CONSULTANT WALLIS: So it looks something like the exhaust pipe from your domestic dryer? It gets all fuzzy with lint and stuff like that? Is that what it looks like?

CONSULTANT KRESS: Those aerosols do look like that, right, the ones I've seen. They agglomerate into chains.

CONSULTANT WALLIS: Not just that, but there must be some capacity to this thing, and maybe there's a limiting thickness or something. No. I'm just asking.

MR. SCHAPEROW: No. Well, as I said, we thought about the issue about, you know, the major issue was would it reentrain. For us, the big issue was would it reentrain back into the aerosol-sized, the micron-sized particles, which is of interest for offsite consequences.

But the other question about where would it end up was kind of important for the point of view of this issue we had with iodine revaporization. If all the aerosols deposited in one spot, like in the beginning of the piping, now you've got a lot of decay heat load concentrated in one area, and that would suggest that you would get iodine revaporization, and that's where our modeling came down.

We actually revaporized a lot of it. You'll see in these slides I'm going to show you. You're going to see the first segment of piping all the iodine deposits, and then it revaporizes.

CONSULTANT WALLIS: And it doesn't redeposit.

MR. SCHAPEROW: No. We're actually not predicting much redeposition. The place where we're seeing most of our deposition, as I'll get into, is down all the way at the very end, in the Safeguards area, where the piping is under water. So the piping is roughly, you know, 300 K. It's cold, and --

MEMBER REMPE: So you do allow lift off?

MR. SCHAPEROW: Well --

MEMBER REMPE: Revaporization.

MR. SCHAPEROW: We're predicting revaporization, because we're having such a concentration of radioactive decay in one area in the piping.

MEMBER REMPE: That's the denature resuspension type of action?

MR. SCHAPEROW: Yeah. This is the first time in my experience with severe accident analysis that we've seen enough fission products deposited in one area, where it heated it up enough to revaporize. When I worked on steam generator tube integrity, we saw that with the steam generator tube bundles, a lot of service area and the fission products tend to deposit throughout the bundle.

So the main thing driving the fission product revaporization in that case is just the hot steam and the hydrogen coming off the core. The fission products themselves are so distributed across the tube bundle they're not really contributing much to the tube heating.

But here's a case where we've managed to concentrate so much radioactive material that we're actually reheating and revaporizing.

MEMBER ARMIJO: What's the chemical form that revaporizes?

MR. SCHAPEROW: Again, we used the vapor pressure for cesium iodide as the vapor pressure for iodine throughout the calculation, so -- and under these conditions, it's relatively volatile.

I want to take you through the results of the separate effects calculation. I'd like to focus here on the time period of roughly 14 to 16 hours. This is the time when we're seeing the core heat up and degradation and fission product release from the core.

The very first node in the separate effects piping is the lowest one here. It's the red curve, and so the flow rates in that area are in the area of 80 to 100 meters per second. And then a

little higher up, the green line is --

CONSULTANT WALLIS: I was going to ask, what's the long-term prospect for this plane? Presumably, there's a long-term cooling that's actually effective eventually?

MR. SCHAPEROW: The pipe is uninsulated in the containment building.

CONSULTANT WALLIS: It's open. You just don't attempt to close it. It's a long pipe which is open to the environment.

MR. SCHAPEROW: Correct.

CONSULTANT WALLIS: What's the long-term prospect for this thing? I mean is there still stuff flowing through it at a late date?

MR. SCHAPEROW: Our analysis has the whole release, the release largely happening over a couple of hours.

CONSULTANT WALLIS: What's the long-term prospect for this pipe full of this stuff? Does it have flow through it over a long period of time later?

MR. SCHAPEROW: There is a little flow over the long term. Actually, you can see some of that down here at 22/24 hours. We've got, you know, flow in the 20 meters per second range down here.

CONSULTANT WALLIS: Vapor flow in a cold

pipe it condenses, it washes this stuff out, or what does it do?

MR. SCHAPEROW: You're talking about long-term revaporization of what's stuck in the pipe?

CONSULTANT WALLIS: If you have this fuzzy stuff on the pipe wall, and you have steam flowing there for a long period of time, condensing, then presumably you have water that comes out eventually, presumably carrying (phone rings)?

MEMBER REMPE: That's Mike.

CHAIR SHACK: Are you there, Mike?

(Off record comments.)

CONSULTANT WALLIS: It all happened when Charlie came in, so --

MEMBER BROWN: I'm sorry, guys.

MR. SCHAPEROW: Basically, the piping in the reactor building, the low head safety injection piping actually comes off the cold leg. It goes down pretty much to the floor. But now you can kind of see in the sketch here. It goes down about 30 feet, 29-1/2 feet, and then it goes back up again. So with regard to water getting out of the piping, I don't think --

CONSULTANT WALLIS: Well, I just wondered what in the long term, there is steam flow through

this pipe for a long time?

MR. SCHAPEROW: Yeah.

CONSULTANT WALLIS: There is?

MR. SCHAPEROW: Steam, hydrogen.

CONSULTANT WALLIS: So it will condense in the pipe, and presumably will wash --

MR. SCHAPEROW: Yeah. I'd have to get back to you.

CONSULTANT WALLIS: Wash stuff off the wall. Well, I mean this isn't where it goes.

MR. SCHAPEROW: Well --

CONSULTANT WALLIS: And you're going to get back to me.

MR. SCHAPEROW: I have to look at elevation, more elevation views of this to see, because this --

CONSULTANT WALLIS: Eventually it will fill up the bottom elbows with water, doesn't it?

MR. SCHAPEROW: That's correct.

CONSULTANT WALLIS: So you're going to get back to me on this one?

MR. SCHAPEROW: Yeah, okay.

CONSULTANT KRESS: It looks like (paper shuffling) in those parts, well above the steam condensation pressure.

CONSULTANT WALLIS: Not forever.

CONSULTANT KRESS: Even after 24 hours.

MR. SCHAPEROW: Well, you know, eventually they're going to flood the containment I mean, if they want to stop this accident. So all this would be under water eventually. But as far as a revaporization release, it would have to get really hot to have revaporization really. Do you want to wait a minute or do you want to keep going?

CHAIR SHACK: Give him a second.

(Off record comments.)

CHAIR SHACK: Mike?

MEMBER CORRADINI: Yep.

CHAIR SHACK: Okay.

MR. SCHAPEROW: So this slide also lists, it lists the length of the piping in the system, 184 feet. 23 elbows, a lot of places for fission products to get trapped. The piping system has a Venturi.

The reason I've got four written here is because we also use the Venturi deposition model to model the two stuck open relief valves in the system, and also the flat-plate orifice that is just inside of the Safeguards area.

Finally, we modeled the necking down from the cold leg into this piping as a sudden contraction.

We validated this against the LACE bypass test and benchmarked it against our VICTORIA modeling. Well, we covered this. Okay. Sorry about that.

So anyway, a time frame of 14 to 16 hours. This is where most of the action is. The fission products -- the core is melting, the fission products are being released. We've got high flow rates in the system.

The next slide looks at temperatures in the system. Again, looking at 14 to 16 hours, you'll see there's a bunch of colored curves up near the top, and then there's a blue one at the bottom. The blue one at the bottom is the piping in the Safeguards area that's under water.

With regard to the colored curves above, the highest one is the --

CONSULTANT WALLIS: Well, how is it heated in the remote, in the far, far distance, the furthest from the vessel? How is it heated the furthest from the primary? How will it be heated, with steam?

MR. SCHAPEROW: There are three sources. One source is -- well, two. Decay heat from the core is coming and going through the piping, and so that --

CONSULTANT WALLIS: No, the heat doesn't. The steam goes through.

MR. SCHAPEROW: Well, super-heated steam and hydrogen are coming out of the core and going through the piping. That heats it, plus there are some fission products deposited down there.

CONSULTANT WALLIS: It stays hot all the way to the end? It doesn't condense?

MR. SCHAPEROW: No, no. These temperatures are -- we're at 900 K. It superheated.

CONSULTANT WALLIS: So it stays hot all the way to the end of the piping?

MR. SCHAPEROW: Sure, yes. The uppermost curve, the red curve is the very first volume off of the cold leg. It gets quite hot. It gets up to 1,200 K, and the reason is is this is where we're depositing most of the fission products.

We actually ran this model turning off decay power in the system, and we found that these curves, (cough) kind of collapse a bit. We don't have this big spike at 1,200 K for the red curve. These different colored curves kind of -- not completely on top of each other, but you don't see this big spread, and actually --

MEMBER CORRADINI: So can you just repeat that Jason? So you basically turned off -- I was going to ask about this. You turned off decay heat.

MR. SCHAPEROW: In the piping. Not in the core; in the piping.

MEMBER CORRADINI: Yeah, I understand.

MR. SCHAPEROW: We've turned off decay heat in the piping, and these --

CONSULTANT WALLIS: Decay heat in the piping, because there are radioactivities occurring in the piping?

MR. SCHAPEROW: That's correct. We have decay in the piping, because for me, a big issue, for us a big issue was are we going to get revaporization of iodine and what's driving it?

Is it the decay heat from the core going, superheating the steam? Is that what's making the stuff come off the wall, or is it the fission products themselves stuck to the wall? Are they heating up the piping?

So we shut off the fission products themselves heating up the piping, and lo and behold, we didn't get the iodine revaporization.

MEMBER CORRADINI: Okay, thank you.

CONSULTANT WALLIS: But doesn't it help if you get vaporization when you -- why did you shut it off? Doesn't the heat help the vaporization?

MR. SCHAPEROW: Yeah, that's right. So if

we shut off the decay heating in the piping, we did not get iodine revaporization. It stayed where it ended up.

CONSULTANT WALLIS: So you're showing that that was the cause of the vaporization?

MR. SCHAPEROW: That's correct.

MEMBER ARMIJO: How much cooler was the piping when you did that?

MR. SCHAPEROW: It was, they all, it all fell down a bit below 1,000 K. In the 900 K, roughly 900 to 1,000 K was about our highest temperature in the piping.

MEMBER ARMIJO: From this 1,200 K level?

MR. SCHAPEROW: Yes, yes. We weren't anywhere near 1,200. We were way down.

MEMBER SKILLMAN: How well do you understand the fission product transport, such that there would be sufficient fission product isotope to give th at quantity of heating? What I think you're saying is the deposition on the pipe has sufficient fission product to give that temperature over (phone ringing) degrees K, over 1,000 degrees K.

MR. SCHAPEROW: Yeah. Well again, these models are -- you know, these are theoretical models that are widely used. I think it's called the Wood

model, I believe, is the name of it, and they were validated against the LACE test.

So the other analysis that I had seen that was done by industry was where they also used similar modeling, but they spread out the fission products along the entire length of the piping.

So they did not predict revaporization of the iodine, because they had more -- the heat transfer favored it staying put, because they had spread it out along the whole length.

MEMBER SKILLMAN: Non-mechanistically, or that's what their model put out?

MR. SCHAPEROW: Well, they suggested that because of the liquid nature or the more liquid nature of the deposits that they would spread out as a result of these high flow rates.

MEMBER SKILLMAN: They would flow along and drag.

CONSULTANT WALLIS: They'd flow along, they'd come out the end?

MR. SCHAPEROW: They would kind of, yeah, dribble out into the Safeguards area.

CONSULTANT WALLIS: Or something --

MR. SCHAPEROW: Yeah, onto the floor or the water. I guess there was probably water there at

the time.

CONSULTANT WALLIS: And they heat themselves and evaporate again.

MR. SCHAPEROW: If they were concentrated, if it was concentrated enough. Okay. So --

CONSULTANT WALLIS: This liquid film transport, was that modeled?

MR. SCHAPEROW: No, not for fission products.

CONSULTANT WALLIS: There is a liquid film, and if there is a high velocity of steam, it will flow along the wall, and eventually it will all come out.

MR. SCHAPEROW: Correct.

CONSULTANT WALLIS: What happens to it then?

MR. SCHAPEROW: That's right. It's on the floor of the Safeguards building.

CONSULTANT WALLIS: Will it do something now? Nothing good to the environment.

MR. SCHAPEROW: Well, it would have to make it through the Safeguards building and pass the filters.

CONSULTANT WALLIS: Maybe it's worthwhile calculating the rate at which that film would flow.

MEMBER ADBEL-KHALIK: If you look at the previous slide, please, 17. When you get a result that looks like this, what is your reaction?

MR. SCHAPEROW: I did not believe that there would be significant fission products in one area to cause it to revaporize the iodine. But you know, we spent a lot of time at this. We spent, I don't know, a couple of months on this.

We really, we turned over a lot of rocks, to use a colloquialism. We tried it with insulation, without insulation. We tried it with radiated heat transfer. We spent a lot of time looking at this issue, and so this is our best shot.

CONSULTANT WALLIS: But somewhere in your calculation, you calculate how much iodine comes out of this pipe?

MR. SCHAPEROW: Yes. That's the Slide 18.

CONSULTANT WALLIS: Come out as a vapor. There's not a liquid film coming out or anything like that.

MR. SCHAPEROW: That's right, a vapor. Well, some of it might be small aerosol particles as well, very small, like the kind that condensed from vapor. Slide 18 has kind of the bottom line, result of the separate effects calculation. The top curves

are the cesium and the bottom curves represent the iodine.

For the cesium, the dotted blue line is how much cesium came off of this cold leg and went into the piping, the low head safety injection piping. The bottom blue line is how much was retained in the piping. So you can see just about all of it was retained in the piping.

We started to see a similar trend for iodine back around 14 hours, but as the piping started to heat up, we saw that about half of the iodine would revaporize in that calculation. We took the DFs from this calculation, basically that the end points of these curves, we put them into the full plant model.

One thing we did not put into the full plant model was the time-dependent nature of the revaporization of the cesium iodide. If we did, we may have seen perhaps some additional deposition in the Safeguards area, because the release would have kind of been later, kind of after the big core degradation that's going on.

Maybe it would be a little quiescent in the Safeguards area, see a little more deposition. The other thing it would have delayed the release some more, providing more time for evacuation and offsite

protection measures.

CONSULTANT WALLIS: So you have 200 kilograms in a pipe, which is how long? How long is it?

MR. SCHAPEROW: 184 feet.

CONSULTANT WALLIS: And what's its diameter?

MR. SCHAPEROW: Internal diameter is 5.2, 5.4.

CONSULTANT WALLIS: Approximately five inches?

MR. SCHAPEROW: It's below 5-1/2 and more than 5. I forget the exact number. We did these analyses a while ago, back in October. We finished up back in October. Actually, there's a lot more than this.

This is just cesium and iodine. We have, you know, we have tin from the cladding. We've got all the other isotopes. We've got -- later in the accident, we're actually going to get concrete aerosols in here from the CPI. This pipe has got a lot of aerosols in it.

CONSULTANT WALLIS: Well, I'm interested in how molten the stuff is on the wall.

MR. SCHAPEROW: Actually, it might vary as

a function of time, as the different things come out of the --

CONSULTANT WALLIS: That's right. I'm just surprised you haven't calculated it.

MR. SCHAPEROW: Actually, this is an issue that we looked at a bit with VICTORIA, again back when we were doing steam pipe tube integrity, where because of VICTORIA's additional capability of doing fission product chemistry, we had or represented, theoretically at least, the chemical reactions that you could have in the RCS.

So we predicted how much of each chemical compound you would have, and we went back and we looked at well, what was the temperature in the steam generator, and what were the compounds that were going through it, and how much -- so what was liquid and what was solid. We actually calculated how many grams liquid, how many grams solid we had.

Actually, Nate Bixler did that work many years ago. So but yeah. Certainly it's an important issue.

Finally, my final chart here is again, more on the cesium iodide in the piping. This chart shows how much cesium iodide is deposited in each of the volumes in the piping, and one thing it shows, if

you look at the red curve, that's the very first volume.

So we see a lot of deposition in the first volume, but then we see it revaporizing. We see similar behavior in all the volumes, except for the very last one, which is CV-301, the blue line. In that one, we don't see revaporization because it's in a much colder area, under water.

A recap of the results, our best estimate calculation now predicts 13 hours to start of core damage, and the reason there is this time lag between the initiator and the start of core damage is that we do model the operator actions that we believe will occur by procedure.

The iodine cesium release is again, we have a much bigger iodine release than a cesium release, again because of this revaporization of iodine.

I'd like to talk for a few minutes now about mitigation. Going back to the scenario definition, the unmitigated interfacing systems LOCA is a failure of the two check valves on the pipe, and no refill of the refueling water storage tank that gets you to core damage.

In our MELCOR calculation, we calculate

that RWST will be empty in six hours, and core damage several hours later. So when we looked at it, we said well, maybe there is enough time to refill the refueling water storage tank. I don't know exactly what assumptions went into the earlier PRAs that we started with here, the earlier PRA like the SPAR model, but six hours is certainly quite a bit of time.

Also, going back to the pumps and the operation of the ejection pumps, we went back to the site in October, just this past October, and we talked to them about this final pump. They said they would keep one pump running, and we said well, you know, what do you mean keep this pump running?

Would you keep it running at its rate of low or would you throttle it? They said oh, "Well, we throttle it. Our procedures would have us throttling the thing."

So we did a calculation where they throttled the pump, just enough to keep the water level above the core kind of thing, and sure enough the water lasted --- the refueling water storage tank lasted an additional 24 hours, because decay power is going down, down, down, and you don't need the flow rate of that pump, 600 gpm, certainly not 24 hours into the event. You only need 100 gpm.

MEMBER BLEY: Is that their practice, or is that actually in their procedures?

MR. SCHAPEROW: Well, they've --

MEMBER BLEY: The ones I've looked at, it's not in the procedure.

MEMBER STETKAR: If it's their procedure, we ought to see what their procedure is.

(Simultaneous speaking.)

MEMBER STETKAR: As Bill said, it is what it is.

MEMBER ARMIJO: Would there be a reason not to put it in procedures?

CONSULTANT WALLIS: Could I go back to this --

MEMBER STETKAR: We discussed that with various folks and --

CONSULTANT WALLIS: -- I'm sorry. You said you've got 180 feet of pipe, and you've got 200 kilograms of only CSI, and you've got some other stuff as well. So roughly you've got about a kilogram per foot of this stuff, plus other stuff.

So I calculate you've got something like a centimeter of stuff all over the wall. If that's a liquid film, it's going to go pretty darn fast with the steam roaring by, and it will all get slushed out.

What happens to 200 kilograms when it dumps out on the floor in the building?

MR. GAUNTT: So Graham, this is Randy Gauntt. We asked all these questions that you're asking right now when we were looking at this, and yeah, the depositions are thick. They're very cakey, as you would calculate them. They do look a lot like what the photos from LACE, where they cut into the pipes and they show extremely thick depositions.

Now we worried about a number of things. We worried about being so much decay heat we could melt the pipe. We looked at that, where we rationalized that if we built up this cake and it was molten, what would happen, because our models -- we don't have a reentrainment treatment.

CONSULTANT WALLIS: Or flow.

MR. GAUNTT: So we said okay, it's going to sluff off and go downstream and maybe hang up in the pipe somewhere else, or ultimately maybe it just dribbles out the end of the pipe. In any case, and Jason, help me remember this right.

In any case, we argued to ourselves that this would not be a resuspension of a very, you know, transportable aerosol. It would be big chunks and blocks and mist, you know. It could be mist, it could

be a puddle, and would not represent a transportable source term offsite.

Now what to do with it, you know, as it's falling out of the -- when it falls out of the pipe, yes. What happens then, yes.

(Simultaneous speaking.)

CONSULTANT WALLIS: Okay, so you have looked at this?

MR. GAUNTT: We worried about it, and you know, this is pushing the front on our ability to model this. We realized from the LACE experiments that we were neglecting turbulent deposition.

CONSULTANT WALLIS: Well, I think a story which simply says it deposits in the pipe, is not the end. I think the story should be continued along the way that they're describing here, because it is a thick deposit. You're saying it's hot enough to revaporize, which means it's probably hot enough to melt.

MEMBER ARMIJO: Well, what is the melting point of cesium iodide?

MR. SCHAPEROW: Just the cesium iodide revaporizes.

CONSULTANT WALLIS: But anyway, I think it would be good to continue the story in the report,

because the intelligent reader maybe is going to have questions.

MR. GAUNTT: Would ask that question, right.

CHAIR SHACK: We're running a bit behind. So if we can finish up in about ten minutes.

MR. SCHAPEROW: Sure. I'm about done. I only have two or three more slides. The other point that I kind of raised earlier was that we noted that core damage could have in fact been averted by starting RHR. You know, the system is a separate system from the low head safety injection system.

They do have power. This is not a station blackout event, and they have to refueling water storage tank until six hours. So they have a few hours to get this started.

Finally, we also looked at what might be done to limit how much radiation is released offsite, basically through the system. We said well, if we're having a bypass event where we've got a hole in this, we've got this pipe bleeding out into the Safeguards building and subsequently went out this pipe, maybe we can open something else up inside containment and have the release going in the containment instead.

So we did a little looking. One potential

thing was the PORVs on the pressurizer. So if they open the PORVs up, they could divert some of the release into the containment.

MEMBER ADBEL-KHALIK: Is that part of their EOPs, in response to this event?

MR. SCHAPEROW: I don't know. This is something that we thought up ourselves.

MEMBER ADBEL-KHALIK: It's not.

MR. SCHAPEROW: This is NRC research. So what have we learned from this detailed integrated analysis? What we learned is it would be really nice if we could do all these calculations and say hey, you know, we think that by changing the procedures thus and such, we'd be better able to mitigate severe accidents. I mean that's one of the ultimate goals of having a big model like this.

MEMBER ADBEL-KHALIK: I didn't think they had manual ways to open up those PORVs.

MR. SCHAPEROW: Ahh, but most of the accidents in SOARCA are station blackouts. This one is not. This one is the lights are on, so to speak.

MEMBER ADBEL-KHALIK: I didn't think you could do it with power.

MR. SCHAPEROW: Oh. Opening the PORVs is not sufficient, because the PORVs dead end in the

pressurizer relief tank. But there are certain points in the accident where the pressure is high enough, that the pressurizer relief tank will open up into the containment. I'm sorry, I'm sorry.

MEMBER ADBEL-KHALIK: I see. That's what you used.

MR. SCHAPEROW: So we actually did two cases, one where they opened the PORVs early, like within the first hour or so of the initiator, and one where they opened the PORVs like around the start of core damage, because we wanted to see --

If they just opened the PORVs at some time, if the pressure doesn't go high enough, you're not going to create that release path through the pressurizer relief tank in the containment.

MEMBER STETKAR: My suspicion is if they open them up early, they're not going to be shutting pumps off.

MR. SCHAPEROW: Yeah. It may be more likely that they'd do that closer to -- you know, when they get into core damage, that may be when they start thinking we better start opening some of these valves up.

MEMBER STETKAR: Particularly the guidance to depressurize is for high pressure melt scenarios,

and this wouldn't be at that time. So it's not -- I mean you could think of anything that you might want them to do.

MR. SCHAPEROW: But the thought is is that, you know, how do you stop a bypass accident? Well, make an opening somewhere else in the RCS. I think this has been thought about, and we saw this in spades in the steam generator tube integrity work, you know.

If you fail the hot leg either before or after tube rupture, that's a good thing, because then you're going to be directing (cough) into the containment and where they'll deposit, as opposed to in the steam generator, where they could revaporize later if it gets hot enough.

Oh, just a word on scenario frequency. We went into this. Our SPAR model scenario frequency was 3 times 10 to the minus 8 per year. This why we didn't talk to the people at the site about this during our initial site visit. But when we went there, they said oh no, 7 times 10 to the minus 7 is what we have for our frequency for this event.

So we initially did include it in SOARCA. Subsequently, and Richard was involved in some of this in the last year or so, we talked to them again and

they said no, no. We didn't mean it was 10 to the minus 7. We thought when it was 10 to the minus 7 if we had one check valve failure plus leakage of the second check valve.

But if you fail both check valves, then it's 10 to the minus 8. I'm like okay, fine. So we explained this, and therefore this is actually a lower frequency event than we had originally expected. We did keep it in the analysis.

MEMBER STETKAR: Since you want to talk about numbers, did they look at the chance that that second check valve didn't close? Do they have positive procedures to know that they were both closed? I mean if you want to talk about numbers. So you didn't investigate that.

MR. CHANG: I'm not sure about that. But at least for the first scenario, with catastrophic failure one check valve and leak by a second check valve, what they have is an MOV at the end, and in theory, it should be able to close off the line in ISLOCA.

MR. SCHAPEROW: At least that's the likelihood.

MR. CHANG: Oh yes, sorry.

MR. SCHAPEROW: Just to kind of wrap up on

bypass accident, since we had another bypass accident and tube rupture, I just wanted to kind of tie that together with the ISLOCA.

I think to sum it up, our analysis of both of these events, we used lowered detailed modeling than we had in the past, both in the area of operator actions as well as, of course, our phenomenological modeling has advanced quite considerably over the years.

Our releases from bypass accidents are smaller and more delayed than earlier studies, and do not result in large early releases. We also have some insights into mitigation as a result of our analysis. That concludes our Surry update.

CHAIR SHACK: Pretty good. Any further questions from the group? Joy.

MEMBER REMPE: Not on what you presented, but in going through the report, several times it talks about hot debris quickly heating the lower head above the melting temperature of stainless steel, 1,700 on the inner surface.

I'm aware there's cladding that's stainless in the inner surface, but the vessel's carbon steel. So I'm always puzzled why one would reference that, and I just wanted to verify, did you

use carbon steel for the vessel material?

MR. SCHAPEROW: I'd like to say yes, but I might not be the most authoritative person on that.

MEMBER REMPE: It's something we're thinking about, because I just --

MR. SCHAPEROW: It may just be a documentation thing --

MEMBER REMPE: Several times throughout the document, and I was just think it's something that ought to be verified. Also, there's again maybe it's a typo or I don't know what, but there's a Figure 5.4, and they have vessel failure at 2100 hours, and the inner surface is like 2,000 K; the outer surface is about --

MALE PARTICIPANT: I'm sorry. Can you say that again? I'm sorry.

MEMBER REMPE: It's Figure 5.4.

MALE PARTICIPANT: I don't have that.

MEMBER REMPE: And the inner surface is 2,000 K; the outer surface of the vessel is about 1,800 K, and you would have melted the vessel through if it was carbon steel. So I would have thought vessel failure would have been earlier.

So maybe it's just the put the line at the wrong place. But I just am kind of wondering about

some of the results I saw in some of the plots, and I think some careful review is needed.

(Off record comments.)

CHAIR SHACK: We're going to recess for lunch, come back at one o'clock.

(Whereupon, at 12:15 p.m., a luncheon recess was taken.)

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

1:04 p.m.

CHAIR SHACK: We're back in session now, Mike.

MEMBER CORRADINI: Okay, great.

CHAIR SHACK: You're on the record. You're on the record. Don't say anything inappropriate, like I just did. Randy, are you going to start?

MR. GAUNTT: Okay, I'll go ahead and start. So Fukushima came along before we were able to get this project closed out, and it raised, I guess, a lot of obvious questions about how our SOARCA calculations were comparing to what happened at Fukushima.

I'll digress briefly and say I was able to spend a month in the embassy in Tokyo shortly after the accident, to kind of follow along. I was slightly embedded with the NRC team who was there in force, and I was sent by Department of Energy.

I have to tell you, I brought along my SOARCA calculation results for Peach Bottom, and one of the first things we did was pull out the SOARCA station blackout charts, and overlaid them on top of the emerging information that was coming out of the

TEPCO database.

We adjusted the time base a little bit, and the comparison of that pressure signature and the response of the reactor was just stunning. So those results were very useful, in terms of having the discussion in Japan on what was happening in these reactors and so forth.

We were sort of warned not to say we had ever calculated Fukushima before, but in my mind we calculated Fukushima before it happened. So anyway, some of the questions that -- go ahead and advance the slide there.

Some of the obvious discussion points or questions about how SOARCA compared was obviously with the RCIC operation. We'll show a slide here that shows extraordinary long operation of RCIC. I'm not sure I understand how it ran so long in Unit 3, I think it was, Unit 2 I think it was. So we'll contrast that a little bit.

I'm hoping to just open the discussion, because I don't have total clarity on all of these issues. So we can maybe just discuss those as we go, and then the impressive hydrogen explosions that everyone's seen on TV. Do we get that in SOARCA, and yeah, we do get hydrogen explosions in SOARCA.

Then there's the issue of our 48 hour truncation in the scope of response to SOARCA, compared to obviously these accidents proceeded for days and day, and may not actually have been over for months, when you think about it that day. And they were multi-units, three reactors simultaneously. So there's a question about multi-unit risk. I think you're going to touch on that.

Oh, there was hysteria over spent fuel pool No. 4. The word was it had uncovered and had caught on fire. We were asking where's the cesium? There would have been cesium release of biblical proportions, if that's a perfect term. Anyway, there is a spent fuel pool study going on right now, I think, and maybe you'll say something like that.

MEMBER ARMIJO: Randy you might, as you look further, you might look at the effectiveness of the mitigation work at the Daini site, 20 kilometers down the road. They weren't unaffected by the tsunami, but they did heroic efforts and a lot of very good application of their EOPs, their severe accident management and great leadership. It would be worth the staff to take a look at that.

MR. GAUNTT: Oh, I think you're right, and in the Fukushima units, we won't have time to talk

about it. But the EOPs let them down. They just got into so much trouble with saturated suppression pools.

MEMBER ARMIJO: Yeah.

MR. GAUNTT: Anyway, the next slide, this is don't -- this is -- there may be some errors on this slide. I've been putting this together, because I love time lines, and this was to just sort of lay out this multi-unit accident, in terms of what was happening when.

The Unit 1 accident was virtually al hands-off short term station blackout, as it developed. Originally, folks thought the isolation condensers were running more than they were running. They did virtually no good, and after the tsunami they lost instrumentation.

That proceeded to core damage pretty quickly. I show on the time line there some events that, you know, anything in blue kind of represents their somehow managing heat sink, getting water into the reactor, or running RCIC or HPCI.

In the case of Unit 1, it's a mystery how the reactor depressurized. When they regained instrumentation, they could see that oh my gosh, the reactor has, it's depressurized. How did that happen?

We think in our analysis they ruptured a

steam line, but there's a lot of -- there are a lot of different possibilities that happened. It seems pretty clear it went ex-vessel in Unit 1. Unit 3 may have gone ex-vessel as well. Unit 3 and Unit 2 both had the operation of RCIC, and Unit 3 had RCIC and HPCI. Unit 2 was running on RCIC for a very long time.

I show then some of the periods of time where they had core damage. At Unit 3, it looked like they almost recovered it and then they lost coolant injection and it's a kind of similar story for Unit 2. So that's kind of time line overview of the multi-unit nature of Fukushima.

Go ahead to the next slide. So in terms of operating of RCIC, Units 2 and 3 were operating heroically for a day to days, and I'm not real sure how they were able to run Unit 2 so long with likely saturated suppression pool. Elevated containment pressure probably helped them to raise the boiling point of the water.

So in contrast, you know, I think before Fukushima, we were probably ringing our hands on how long should we credit battery life and extended operation of the RCIC system. So this just shows you a comparison, where we're certainly far less heroic in

the operation.

Our RCIC operation is basically limited by the battery life time, and of course in Fukushima, they went out to the parking lot to get batteries.

MEMBER ARMIJO: More batteries, yeah.

MR. GAUNTT: And work the system. So that --

MEMBER ADBEL-KHALIK: How do you think RCIC operated for 70 hours?

MR. GAUNTT: How did it?

MEMBER ADBEL-KHALIK: Yeah.

MR. GAUNTT: Like I say, elevated containment pressure, I think, kept the water from flashing. I don't have an analysis that explains how the pump was able to suck near-saturated water and not cavitate, and maybe it was cavitating to some extent.

MEMBER STETKAR: They tend to be -- well, I don't know. This was RCIC, so it's a little more complicated. It's a pretty simple plot.

MEMBER BLEY: That's a Terry, Terry turbine.

MEMBER STETKAR: You know, but I mean it's a centrifugal pump.

MR. GAUNTT: So you know, I don't know how effectively they were running. They did finally give

out, and that's about all I have to say about RCIC, unless there's more discussion that you want to have on the topic.

MEMBER ADBEL-KHALIK: Is it at all reasonable to assume that the pumps would operate beyond the life of the battery? I mean are you trying to draw a lesson from this table?

MR. GAUNTT: Trying to draw a lesson from --

MEMBER ADBEL-KHALIK: From this table?

MR. GAUNTT: From this table. I guess we're just contrasting how we credit RCIC to what was done under more or less heroic measures at Fukushima.

MR. LEONARD: Randy, this is Mark again. We actually do credit RCIC operation beyond the end of batteries. In the long-term blackout, when the batteries die as we're simulating it, the reactor vessel pressure is already very low.

We had previously opened the SRV to depressurize, and we've been told by the system engineer with the licensee for us that under those conditions, if dc power goes away, the governor valve, which actually controls steam inlet flow to the turbine, wouldn't change its position. It would simply remain as it is, because the reactor vessel is

already at low pressure.

If dc power stopped prior to depressurization, then that would not be the case. The governor valve position would change. The valve would go full open. Steam would enter the turbine at a much higher rate, and the system would trip on a turbine overspeed.

For our station blackout scenario, because of the prior depressurization of the vessel, that condition doesn't happen. The overspeed condition cannot occur because the reactor vessel's already depressurized. So in our case we do run the turbine, the pump for a while after dc power, but there's no instrumentation available to guide operators on level control.

So the question is well what flow rate would they attempt to manually manage the system, to "maintain level." The assumption in the SOARCA analysis is that in the absence of instrumentation, they simply let the pump run at the flow rate it had when the batteries died, just keep things as they are.

Unfortunately, what that does, because the loss of dc power also closes the SRV that was open, the leak rate, if you will, the loss of coolant from the reactor vessel to the pool is now terminated. So

you no longer have a loss of coolant but you've got a constant flow of RCIC injection into the reactor vessel.

The vessel level continues to rise to the point where you spill over into the steam lines and the system kills itself, if you will. That's the RCIC scenario in a long-term blackout. So it's useful here in the sense that not only is it a matter of batteries; it's manual management of system flow and the absence of instrumentation or whatever else.

As Randy said, somehow they were able to do that, perhaps because of the electric power through auto batteries they brought into the control room, to be able to more accurately or have some indication of reactor vessel water level.

In our simulation for the long-term blackout, we have no instrumentation, no indication of reactor water level. That's the primary problem.

MEMBER ADBEL-KHALIK: But do we know the operator actions that led to the extended operation of RCIC?

MR. GAUNTT: I don't. I have looked at the TEPCO reports, and that's just not coming through, on precisely what were they doing.

MEMBER CORRADINI: Randy, may I ask, isn't

there going to be follow-on report from the Japanese that will get more indepth about some of the operator actions, or is that -- or am I remembering wrong? I thought something was going to be coming out in the July or June time frame?

MR. GAUNTT: I don't know, Mike. The latest thing I have is dated December 2nd from TEPCO.

MEMBER CORRADINI: Okay, fine.

MR. GAUNTT: So that's RCIC. Next slide. The hydrogen explosions were --

CHAIR SHACK: Oh just on the RCIC, suppose you had the batteries. How long would you be able to run it without, before the suppression pool heated up? What would your calculation say for that?

MR. GAUNTT: I don't know if I've got that on the top of my head.

MEMBER CORRADINI: I was going to ask a different question. I am sorry. I'll wait for you to finish with Bill. I'm sorry.

MR. GAUNTT: Okay. So I don't know. Mark, do you --

MR. LEONARD: I don't recall the specific number either, but it's on the order of 24 hours. I mean it's day-ish, inside 24 hours.

MR. GAUNTT: Yes, it's a day-ish.

MR. LEONARD: It's not 70 certainly.

MR. GAUNTT: Okay. So on the -- Mike, did you have a question?

MEMBER CORRADINI: Yeah. I guess just again on RCIC, I was talking to some system engineers. Let me ask you a question. It's a bit kind of out of the blue.

If one were to put essentially on the RCIC system I'll call it an alternator, that essentially recharged the batteries, so that RCIC not only provides cooling, but also is a way to, for an ongoing long-term station blackout, recharge the batteries.

Has that been considered at some sort of I'll call it alternative thinking about long-term station blackout and the ability to cope with it, because where I guess I'm coming from is this whole discussion you guys were having about how could the RCIC run without the batteries.

My thought is that if one can connect the two systems, such that you're draining the batteries by essentially doing all this control stuff, but if you use this small turbine pump combination to actually have some sort of way to charge back the batteries, now you have essentially a system that can provide long-term cooling for much longer term. You

see my thinking?

MR. GAUNTT: Sounds like a good suggestion. I guess you're ultimately up against the -- but in this case, they're isolated from their heat sink.

MEMBER CORRADINI: Well, I think you're right. Given what we're talking about, now we've lost the heat sink. So we're ultimately going to get, hit the wall by a loss of heat sink or we'll do the saturation in the pumps, etcetera.

But I guess I'm trying to connect it such that if one has this system, which at least on an empirical basis for Units 2 and 3 had shown to be pretty doggone reliable in the face of a pretty dramatic event, what was one of their Achilles heel was there was a disconnect between recharging the batteries and potentially through actually the operation of RCIC.

CHAIR SHACK: That's actually looked at in a number of SAMA analyses for license renewal, but it's always rejected as not cost effective.

MEMBER CORRADINI: Was that Bill that said that?

CHAIR SHACK: Yes.

MEMBER CORRADINI: So maybe we just have

to think about cross-benefit again.

MR. GAUNTT: Good discussion. Okay. On to hydrogen. We've been telling people for years and years nuclear power plants can't blow up, and then we see three, we see three nuclear buildings blown up. So do we predict that in SOARCA? Yes, we do predict hydrogen, significant hydrogen burns.

We don't specifically have a detonation model in MELCOR. The hydrogen burns, according to that turn area flammability chart, depending on steam partial pressure and hydrogen oxygen availability.

One difference, I think, between what happened at least in Fukushima Unit 1 versus our SOARCA analyses, is I am reasonably sure in Unit 1 that they did not fail the drywell liner, at least not early on.

I believe that because of the protracted high containment pressure that was observed. In SOARCA, when we fail the lower head, in Peach Bottom we pretty much always fail the liner, which will then depressurize the system.

Instead in Fukushima, it appears, and I have to qualify everything I say, it's what I believe, that the hydrogen that got into the refueling bay in Fukushima Unit 1 was by virtue of the head flange

leakage at the top of the system, and you can see that the Fukushima drywell pressure comes up to around 110 psi and sits there for like ten hours, almost like it's pressure-regulated at 110 psi.

Our SOARCA model has that leakage mode, and we would predict, then, this constant source of hydrogen and steam going into the refueling bay. Interestingly, our analysis says it was the protracted hydrogen steam going into the refueling bay that kept it from exploding.

It wasn't until they depressurized the containment, that sort of cut off the steam flow, and then in the reactor building, steam began to condense and they approached flammability from that direction.

That's what MELCOR says, and there's other analyses out there. But in the cases where you fail the drywell liner, the hydrogen escape path is now low in the building where the liner rupture is. So that kind of changes the nature of the hydrogen burns. In the Peach Bottom analyses, we would have those burns down in the lower parts of the building, and Mark, were there ever any cases where we burned in the refueling bay?

MR. LEONARD: I think all of the cases eventually burn in the refueling bay, but they begin

down low.

MR. GAUNTT: They begin down low, whereas in Fukushima 1, it looks like it all went up in the refueling bay. Not so clear for Unit 3. The nature of the explosion looks like it could have involved some of the lower floors of the building.

MEMBER ARMIJO: What happens with the nitrogen from the inerted containment during this scenario? Is it purged? Is it purged at least out of the refueling bay building?

MR. GAUNTT: Yeah. It's just purged out into the --

MEMBER ARMIJO: Why didn't the hydrogen leak out also?

MR. GAUNTT: What our calculations tell us, I have to qualify that, is we probably leaked into the refueling bay upwards of a 1,000 kilograms of hydrogen. But if you look at the instantaneous concentration in there, there's only ever about two or three hundred kilograms resident.

So gas is coming in. It's displacing air, and some of it's leaking out of the building. That said, I think analysis of or understanding of hydrogen behavior under these circumstances is something we're probably going to need to study some more.

MEMBER REMPE: And as part of that study, are there any subcompartments? I assume your nodalization diagram for the reactor building indicates the refueling bay is one big volume, right?

MR. GAUNTT: Yes, it is.

MEMBER REMPE: So are there some -- I'm not as familiar with the building up there. There are some subcompartments where hydrogen, it should be monitored in smaller regions?

MR. GAUNTT: Yeah. There's subcompartments in the floors below, and in fact hydrogen that goes into the refueling bay can work its way down, you know, as it --

MEMBER REMPE: And you monitored each of the volumes shown in this report, but are there -- is a way to break up the refueling bay that should be considered too?

MR. GAUNTT: You know, I think this topic is probably needs to be addressed through some CFD kind of studies. We represent this in MELCOR with one big control volume. You've got all the issues of high diffusivity of hydrogen, light gas stratification, thermal stratification and condensation of steam, you know, all ongoing. So it's --

I wouldn't say that we have necessarily

the definitive analysis of that refueling bay, and that's something we ought to be studying. The hydrogen burns in SOARCA were sufficiently large to blow out panels, as we saw in Fukushima.

MEMBER ARMIJO: And that's only two to three hundred kilograms emission to do that?

MR. GAUNTT: Oh, it's interesting. In Fukushima, it was so delayed before they got to flammable conditions. But I think in our Peach Bottom, because it's coming in from lower floors, that you will reach flammability conditions sooner. Am I capturing that right, Mark?

MR. LEONARD: With the diagram you see on the slide, the release pathway in the SOARCA calculations would be roughly at the location of the vent pipes, of course. That's where the debris would contact the liner outside the pedestal. So the leak pathways is into the basement of the reactor building.

That volume is relatively confined, certainly small in comparison to the refueling bay. And depending on the other gases that are being discharged with it, steam nitrogen, that volume in fact may remain inert for a while. It may not be instantaneously flammable in the release.

But when the gas begins to move upward in

the building and it encounters more and more air, then it reaches a flammable mixture fairly quickly, certainly by that first level above the torus itself.

And in each of those floors, as well as well as the rooms and the stairwells, are all represented as separate compartments. So the spatial distribution is represented in a fairly detailed way below the refueling bay.

So the flame, as it begins down low, propagates up high, provided the flammable mixture is available at upper elevations. But in all cases, the initial burn produces an overpressure that's large enough not only to open the blowout panels at the top of the building, but mainly the other doorways that are in the building itself.

MR. GAUNTT: So I think that's it on hydrogen. The 48-hour truncation. So that's -- it was so clear from Fukushima the accident's not over in 48 hours, but the consensus view amongst the SOARCA team and the state and regional people, were that within 24 hours, equipment can be brought to the site, if needed, and response launched and by 48 hours, be able to get on top of the situation, and anyone else is welcome to add to that, who's been involved with the decision.

But that's our story and we're sticking to it. Randy, was there anything you can add to that?

MR. SULLIVAN: We looked at this in some detail, but if you have no questions and wish to accept it, perhaps we don't have to go into it. But we were doing site-specific analyses, right. There are two heavy airlift wings within 150 miles of this site.

We explored the kind of capacity that they have to lift heavy equipment into the site. It is substantial. We explored how much of his heavy lift equipment, helicopters I'm talking about, military, National Guard units in Pennsylvania and Virginia, not to mention the Naval air base or whatever that is at Norfolk.

This stuff can lift 22,000 pounds. There's electrical generators and pumps all over the East Coast. So we put that together as being able to get stuff into the site. Now in the case of Peach Bottom, the roads are available, you know. In Surry, the earthquake is a little more, you know, has more substantial damage to roads, we believe.

So if the roads are available, then you know, you've got an eight or 12 hour response time if you want it. So then we looked at pumping curves and

how much water you have to move and could you flood the torus room to the level of the drywell penetration that's caused by the corium, and the answer to all that was yes.

The PWR is a little more complicated, because you're working against pressure, and that's the kind of analysis we did, you know. So that's where we came down, that there would be a national level response, including people in this building by the way, all looking for assets to help this plant out.

MR. GAUNTT: Multi-unit risk. Clearly, it was a multi-unit accident in Fukushima. We specifically did not collect to look at multi-unit issues in the SOARCA project, but this will be, I understand, addressed in the soon to be underway Level 3 study.

Finally, spent fuel pool risks and the consequences. So we did not include spent fuel pools in the SOARCA analysis either, and again, I think maybe you're going to speak more about this.

MS. SANTIAGO: Just basically to say that there's two ongoing agency projects that you've heard about already, and I think there was a two weeks ago discussion with the subcommittee for the spent fuel

pool scoping study, and of course, a Level 3 PRA. We've been talking about it for a number of months now.

CONSULTANT WALLIS: Shutdown risk is out because of frequency?

MR. CHANG: I think in the beginning chapters of the main report, I guess low power and shutdown risk, it's one of those things we acknowledge is not -- us not examining.

CONSULTANT WALLIS: But the consequences are not trivial.

MS. GIBSON: And that's also going to be covered in the Level 3 PRA.

MS. SANTIAGO: Correct, and I'll go through a whole list of comments and questions that we've gotten from the public, from the peer reviewers, from other stakeholders, that touch on many of these items that we're talking about, that will be looked at closer in other projects.

We had to stay with the scope, SOARCA, because in the last year, as you've listened this morning, we've done a lot in the reanalysis of the SRVs and the ISLOCA. You'll hear from the peer reviewers.

When Fukushima sadly happened, we diverted

some attention to looking at questions for that. So we just had to limit, by direction in part, what we looked at for SOARCA.

But it didn't mean that we didn't hear those different issues. We just put them on a table and said okay, we're going to look at these and other projects, and I'll go through that a little bit later.

MR. GAUNTT: So sorry for the brevity of that, but that's our discussion on Fukushima.

MEMBER ARMIJO: I just want to ask you, do you have any kind of arrangement with the Japanese, when they start getting data on the product released in the environment, to check the barium/cerium issue? Is the staff going to get that information?

MR. GAUNTT: I don't know that we're in specific negotiations on that. But there is a developing relationship with NEA, OECD, the Japanese, NRC, Department of Energy and so forth that will be kind of looking down the road at some of these things.

We have a good idea right now if what cesium is on the ground, and that should be useful to us in terms of validating our atmospheric transport and deposition.

Most of the release went offshore, so there's a big lack of closure on, you know, how much

actually got out. When we run the analysis on these, we predict something like one percent release, with a lot of capture in the suppression pool.

I think that we will be plugged into this for some time, and we're going to get very useful information out of the decommissioning process, that will kind of further help us validate this page. But on the whole, I'm fairly encouraged with how our codes have compared to what we think we know about Fukushima at this point.

MEMBER BLEY: Before we go ahead, I have a question that probably was sort of asked this morning, and I heard some things that address it, but only partially.

When I look through the report, it seems or it states that some human action, some operator actions are included in the unmitigated analysis, and some normal operator actions and EOPs are relegated to the mitigated, on a case-by-case basis.

I didn't see any clear criteria on how you decided which ones to put in which category. Can you tell us about that?

MR. SCHAPEROW: This is Jason Schaperow of the staff. The mitigated case was intended to include everything.

MEMBER BLEY: Okay. It doesn't say that --

MR. SCHAPEROW: The whole nine yards, whatever we thought the licensees were going to do, what they told us they would do, when we looked at the procedures.

The unmitigated cases were intended to be without the B.5.b equipment and procedures, because one of our primary objectives was what are the benefits of B.5.b. So we're trying to -- the reason we had two cases, we were trying to distinguish that.

MEMBER BLEY: Okay. Well, maybe I'll find out when you talk about it, because there are places in the report where it says only some of the non-B.5.b operator actions, and some were reserved for the mitigated case, and it gives a few examples and were scattered in the report.

MR. SCHAPEROW: Our philosophy was that again, the mitigated case should be the whole, whatever exists, whatever's existing. The unmitigated cases --

(Simultaneous speaking.)

MEMBER BLEY: It's easy to say that, but that's not what it says --

MR. SCHAPEROW: That was our overall high

level philosophy.

(Simultaneous speaking.)

MEMBER ARMIJO: The mitigated, they threw everything at you.

MEMBER BLEY: Yeah, but in the end --

MEMBER ARMIJO: That's where the problem is, right?

MEMBER BLEY: That's where what I hear here and I heard earlier this morning doesn't match what I read in the report. I'll try to find a couple of those examples.

MR. GABOR: All right. Good afternoon. My name is Jeff Gabor. I work for ERIN Engineering. I'm one of 11 members of the SOARCA external peer review committee.

Next slide. The presentation I've got today is going to discuss the objectives of the peer review. We're going to highlight some of the individual peer review team members and their areas of expertise, provide an overview of the review process, and then finally get into the individual comments from each of the reviewers.

The main objective for the peer review process was to provide an independent review by each of the review team, of the technical work conducted

within the entire SOARCA project. Our goal was to ensure that the SOARCA provided technically accurate information.

In addition to that, the peer group also offered some feedback on specific issues related to how this material would be presented to the general public, and I'll mention a couple of things there.

The list here shows the committee was made up of a variety of technical experts from industry, with identified areas of expertise from Level 1, 2, 3 PRA, external hazards, emergency planning and health effects.

The peer review committee included the list you see here: Ken Canavan from EPRI; Bernard Clement from IRSN; myself; Bob Henry from Fauske and Associates; Roger Kowieski from Natural and Technical Hazards Management; Dave Leaver from Polestar; Bruce Mrowca from ISL; Kevin O'Kula from URS Safety Management Solutions; John Stevenson from Stevenson Consulting; Karen Vierow from Texas A&M, Karen was our chairperson; and Jacquelyn Yanch from MIT.

Next. The documents that our groups reviewed included the draft SOARCA NUREG documents. It also included the presentation material that was provided to us.

We had a peer review comment resolution document that we worked on, where we would provide some questions, and then the staff and the contractors would get back to us on specific items.

Then anything that we would specifically ask for in terms of supporting information was also included.

The review process benefitted from many active discussions with the staff and their contractors. We had a total of five meetings between July '09 and December 2011; in addition, two teleconferences as well.

Some items that weren't specifically included in our scope of work were the final uncertainty review and the final uncertainty quantification and sensitivity analysis. We're eager to hear the presentation by Tina today, as well as everyone else.

We didn't specifically in our scope attempt to fix the document, editorial review of the document, although I understand that some of the peer review team did provide some comments on editorial things. The proposed uncertainty analysis methodology was presented to our team in October of 2010, and there was a peer review guidance memo that we issued

at the time, providing some feedback on that plan.

The parameters and their associated distributions that were going to be used in the uncertainty analysis, they were presented to us in January of this year, and again, there was considerable feedback between the peer review team and the staff.

MEMBER STETKAR: So Jeff, although you said you didn't -- if I understand, you didn't review the uncertainty quantification because it's not done yet, I guess.

MR. GABOR: Correct.

MEMBER STETKAR: But you did review the input?

MR. GABOR: We did, the parameter selection and the distributions. Next. So as far as the coverage of the various SOARCA topics, I think we felt that the team included the right people to address all the major areas. You can see from the list in front of you accident sequence selection, mostly looked at by Ken Canavan and Bruce Mrowca. Accident progression, also Ken, Bernard from ISRN, myself and Bob Henry. Myself, Bob Henry and Bruce Mrowca also focused in on the mitigative measures.

We had several people looking at the

radiological release results. In addition, Roger Kowieski and Dave Leaver focused a lot of their attention on the offsite emergency planning and response part of the calculation. Offsite radiological consequences, a similar group, including Kevin O'Kula and Jacquelyn Yanch.

Then finally, John Stevenson provided peer review of seismic issues and structural issues; overall PRA application, Ken Canavan and Mrowca, and then specific to severe accident modeling, mostly myself, Bob Henry and Karen Vierow.

MEMBER STETKAR: Jeff, maybe it's here, but was there any specific examination of what I call, you know, putting the pieces together, the integration of kind of the Level 1-2-3 --

MR. GABOR: It probably fits under the PRA application part. I would think Ken Canavan and Bruce Mrowca were mostly looking at that.

MEMBER STETKAR: Did they?

MR. GABOR: I think they have some comments. We'll get to their specific comments. Hopefully that will hit on a couple of those topics. Okay. At this point, I'd like to move into the individual assessments and provide a brief summary of each members' comments.

The summaries of assessments included in the following slides are the final summaries. During the whole process, each reviewer provided comments on the review process, many of which were addressed by the SOARCA team, and I'm not going to present those things here.

The SOARCA analyses were reviewed based on -- were revised based on the comments from the review team, and basically this presentation doesn't identify all of the contributions that the review team made.

I think as stated on this slide, there was no attempt to pursue any kind of a consensus opinion, and as such, the individual comments from each of the team members were not altered by the chairperson, and represent those individual inputs.

MEMBER CORRADINI: Can I ask you a question at this point?

MR. GABOR: Sure.

MEMBER CORRADINI: So was that a going-in part of the scope, that the committee would just provide individual comments and not come up with consensus comments, or was that a decision by the committee?

MR. GABOR: It was a going-in, I believe. It was stated going in that there was no -- the staff

didn't have an interest in pursuing that.

There were a couple of places where the group issued memos, where it wasn't necessarily consensus, but one opinion would be shared by multiple reviewers, and those were documented in a couple of memos that were transmitted.

MEMBER CORRADINI: Okay, thank you.

MEMBER ADBEL-KHALIK: So members did not benefit from the reviews that the specific subgroups focused on in specific areas?

MR. GABOR: They did. We all shared our own individual reviews as they were drafted up, and of course, we all participated jointly in all of the five meetings and the two telephone conferences.

MS. SANTIAGO: And we went through each of the reviewers' questions and the staff responses at those meetings. Some of these meetings were three-day meetings to go through all the comments.

MEMBER ADBEL-KHALIK: But the reports that were prepared as a result of those subreviews were not modified in any way as a result of those discussions. They were just information briefings for you?

MR. GABOR: Maybe I don't understand the question. I mean the final reports that the review team issued were there, were the opinions at the end

of the project. So if there were issues that were raised and resolved, they may not appear in the final document, if they were satisfactory.

That's not quite -- I don't think that was true in each case. I think a lot of the -- we drafted, our initial draft came out quite a bit before the end of the project. So a lot -- and with a lot of us, I know me personally, I tried to keep the original comment, but then capture any resolution from the SOARCA team in my report.

Okay, so onto some specifics, and all I've tried to do is to highlight some of the key issues that each of the reviewers brought up during the process. Ken Canavan from EPRI, he felt that overall, that the SOARCA study met its objectives.

He noted that because SOARCA, since SOARCA was mostly a consequence evaluation, and didn't represent a full-scope PRA, that there could be a chance that some low frequency high consequences events would be missed, might not have been considered.

Next. In addition to that, Canavan noted that some of the SOARCA conclusions could be plant-specific and wouldn't apply to other plants. So that was a theme that you'll read in a lot of the peer

review comments.

Specific to the review of the sequence selection process, Canavan noted that a plant-specific analysis could produce unique sequences, and sequences that weren't necessarily represented by the more generic process that the SOARCA, what was represented by the SOARCA methodology.

Canavan also commented that changes in key phenomenological assumptions could also influence the final results, and I think we heard some of that this morning in the presentations.

Bernard Clement from ISRN agreed that SOARCA overall met its objectives for updating the quantification of offsite consequences. He identified some specific phenomena associated with RPV failure. Again, we talked about that this morning, treatment of iodine release that might influence the overall results.

He also supported the proposed uncertainty analysis, and felt it would increase the confidence in the overall results.

And then specific recommendations that were made by Bernard included (1), to revise the SOARCA study based on any new PRA results or insights; (2), to expand the SOARCA study to address other

plants, again the same theme that came through on many of the reviews.

A third item for Bernard is he commented that benchmarking SOARCA with future MELCOR versions would be recommended, and then specifically he indicated, he felt that there should be a reassessment of the thermally-induced tube rupture analysis with any updated flaw data or material data that might become available in the future.

For my review, I felt that SOARCA represented a major advancement in our understanding of severe accident progression and radionuclide release. Again, since the study was primarily focused in a deterministic evaluation, I cautioned and I felt that there was care needed in communicating these results in the context of a true risk assessment.

Next. My comments also identified some limitations in the deterministic approach taken, and highlighted some differences with a more complete probabilistic approach. Some of the technical areas, specific technical areas of most interest to me were treatment of the SRVs due to high RC primary system temperatures, and the impact that that had on actually reducing the source term.

It turns out opening the SRV, transporting

all the fission products into the pool will overall reduce the amount that could be released once the containment was impaired or failed. I commented that there was, you know, clearly a competition between the seizure of the SRV and the potential for a main steam line rupture. We heard a lot about that earlier today, and that topic did become, as you can tell, a pretty major focus for additional work that was done, the finite element analysis and the additional work that the SOARCA team did.

I made notes while I guess Mark was talking this morning. I had also identified that I felt that they needed to look at lower head penetration failures. In analyses that we have done, we have found -- with industry tools, we have found that a limited penetration, welds might be vulnerable to a local penetration failure.

I also agree with Mark's comment earlier today. It may not make a big difference in the end result, because a smaller penetration failure is going to tend to grow into something larger anyhow.

CONSULTANT WALLIS: Did the staff respond to that? I mean I picked up that too. Did they respond to that issue of the low penetration?

MR. GABOR: The response that we got, well

the first response was it just wasn't a model currently in MELCOR. The other response was because the scenarios that the SOARCA team had looked at were low pressure scenarios, that ejection of an instrument tube or a CRD tube would be less likely at the lower pressure, which I tend to agree with. But there are high pressure scenarios to deal with as well.

The last point I thought I'd make was that I also, in looking and addressing the SRV seizure open scenario, that there could potentially be a release pathway, not unlike the steam line that came as a result of SRV tailpipe failure. The piping downstream of the SRV is thinner-walled piping. It's going to be exposed to pretty high temperatures.

If it would rupture, it would potentially be in the drywell air space, and again, probably much like the scenario that the NRC analyzed with the main steam line creep rupture, saying more about myself, I guess, than anything.

MEMBER REMPE: Just quickly, in your first slide you talk about you really ought to be careful in presenting results of this study against, for example, the safety goal, which you see in several of these documents. Do you think they ought to just take that safety goal line off? Are the caveats they have --

how strongly do you think they ought to be careful about how they present it?

MR. GABOR: I think, and I think they heard a lot of our comments, that the peer review team made, that you have to put it in the right context, that it's a consequence study. It's not a PRA, it's not a Level 3. We've got that coming, and I think the Level 3 project will address a lot of our concerns about doing more of a full-scope, you know, risk analysis instead of a more, I'll call a focused consequence study.

MEMBER REMPE: So they should take that safety goal line off, or I didn't hear the answer to the question.

MR. GABOR: I'm not sure I know the specific safety goal statement you're talking about.

MEMBER REMPE: It was in the presentation chart.

MS. SANTIAGO: The chart that Richard showed at the very beginning, and --

(Simultaneous speaking.)

MEMBER REMPE: They are presenting the results with the safety goal.

MS. GIBSON: Actually, we added that line as a result of a comment from the peer review

committee.

MEMBER REMPE: The peer review was happy about that.

(Laughter.)

MS. GIBSON: You probably noticed that we caveated that.

(Simultaneous speaking.)

MR. GABOR: I guess to answer to your question directly, I don't think it's incorrect to put the results up against the safety result in that context. It's more in terms of, you know, in portraying this as a full Level 3 PRA or as a PRA, they may have.

MEMBER REMPE: And they have a lot of caveats in the report. Okay. Just wondering.

MS. GIBSON: And specifically because of that. We had that large discussion at the peer review committee meeting in December about how to present that, and so --

MEMBER RAY: In that regard, let me just add, I don't quibble with the conclusion that it's a major advancement in our knowledge. It can well be, but then the problem is how do you keep that from implying that it's 75 percent of the answer that one would need to have? How do you put a major

advancement in the context that you just talked about, and that's what I'm debating in my mind right now.

MR. GABOR: Specific to my definition of a major advancement, it's really focused on our ability to analyze the severe accident response.

It's not in sequence selection or -- and even personally, it's not, I didn't really focus th at much attention on the offsite consequences. The others that did also agreed that the modeling did represent state of the art techniques.

But I do think specific to MELCOR and MACCS-2, and the efforts that went in to upgrading the models, and how they were exercised in SOARCA, is state of the art.

MEMBER RAY: Yeah, and I think that's a good answer. It's just that the danger is that people will over-interpret the outcomes as being more conclusive than you intend.

MR. GABOR: Given that the uncertainty analysis wasn't part of our original scope, I had originally made some recommendations to include that in the final analysis, and as we all know, that evolved with the project, and we'll hear -- we're going to hear a lot about it today, I'm sure.

One of the things we did talk about, and

of particular interest to me again on uncertainty analysis related to core melt progress, Level 2 PRA, is it's really difficult, because of how all of these parameters and we had some pretty active discussions on this, but how Level 2 parameters are all correlated to each other.

It's a different beast than doing random sampling on valve failures and things like that, because hydrogen generation, vessel failure, all of those parameters all tend to feed back on themselves and again, finally, my recommendation, no surprise, was that I saw benefit in taking the tools that were created for SOARCA, and extending that into a more complete Level 3 PRA framework.

Okay. I've got to get back on the other folks. Bob Henry felt that the study advanced our knowledge of severe accident progression. Again, I think his comment was in line with mine, that with the advancements and the modeling work that had been done with MELCOR.

He felt that the focus on a station blackout and the containment bypass events was appropriate, and then he felt that Fukushima validated the decision to kind of focus in on those scenarios.

Bob Henry had also described the

importance of RCIC operation, and identified or commented that it was, he felt, that it was conservatively modeled in the SOARCA study, specifically in looking at what happened at Fukushima Unit 2.

Okay. Henry saw that the MELCOR benchmarking activity was a strength. We spent a good part of a meeting going through a lot of the benchmarks. Bob Henry is quite interested in these benchmarks, and I think felt that it added a lot to the value of the end product.

As identified earlier, he felt that the importance of looking at these release pathways from the reactor coolant system initially identified that those hadn't been explored in SOARCA, and in particular, the in-core instrumentation in the TIP system, and how it might create a flow path that could, you know, potentially result in hydrogen and high radiation outside of the reactor vessel.

I think it was mentioned earlier about relating that back to TMI. He's actually done that, and I think the release is, I don't remember the details that he studied. But I believe that he can actually account for hydrogen and radiation levels in containment by looking at that leak pathway in TMI as

well.

Let's see. Again, this release pathway had originally not been included in the SOARCA study. The only comment I made, again this morning listening to the presentation, is that I believe, you know, Mark did a good job showing the sensitivity to having that release pathway or not.

But one thing to keep in mind, and I think Bob Henry had raised this at the end of our process, that there could be other scenarios, that that sneak path or that release path could be a little more significant. That was it.

Next slide. Early on his review, Henry had recommended that the MELCOR best practice document include a more thorough description of the code benchmarking that had been done, and actually how those results from the benchmarks made their way into the SOARCA evaluation.

Okay. The next reviewer, in the review of the emergency response analysis, Roger Kowieski felt that the parameters and the assumptions were reasonable and adequate. He also let that the response time lines were consistent with actual time lines that were used by, you know, a typical offsite organization.

Dave Leaver felt that SOARCA represented a substantive high quality effort, and that it made a significant contribution to the understanding of nuclear plant risk. He saw that the project transparency was a strength, and he felt that the Level 1 PRA work utilized the latest information available.

Leaver did feel that the study of the large seismic event should be a focus of some future evaluations. On the matter of completeness, Leaver believed that the approach was technically sound, but he did some benefits in expanding the scope to address additional sequence, accident sequences.

He also felt that expanding the study to address other plant types would strengthen the completeness argument for the effort. Leaver felt that there was nothing forthcoming from Fukushima that would indicate that SOARCA's sequence selection process had overlooked any important scenarios.

Then on the topic of public risk, Leaver felt that SOARCA provided strong evidence that past risk characterization had been unrealistic and excessively conservative, his words. The 50-50 4HH or the so-called B.5.b measures were found to be important to risk, and he felt that they provided a

margin for some of the uncertainties associated with sequence selection.

Leaver also stated that the SOARCA results demonstrated the importance of emergency response planning and accident management, to reduce overall risk.

Okay, for Bruce Mrowca. He commented that he felt that the objectives were only partially achieved. He stated that SOARCA demonstrated state of the art techniques on the phenomenological modeling, but not on sequence selection process, or modeling of the security-related mitigation improvements.

MEMBER RAY: On that last point, why?

MR. GABOR: I'm going to get there. Mrowca found that the sequence selection process was limited.

He felt it lacked some transparency, and in his write-up he provided some examples of how the process could have been improved, and then I think to your point, relating to the mitigative measures and the associated operator actions, he felt that a more thorough, disciplined human reliability analysis of these key operator actions was needed.

He believed that lacking this HRA analysis really limited the credibility of the benefit that

could be achieved from these actions, these additional actions.

MEMBER RAY: Okay. So it's based on the operator performance, not on the reliability, what do I want to call them, non-compliance-based features and equipment and so on.

MR. GABOR: Yeah. His comments were mostly directed towards the human reliability. Okay. Kevin O'Kula reviewed -- in his review, he stated that he felt the SOARCA study met its goal of applying state of the art techniques to accident phenomena, and more in line with one of this areas of expertise, he felt that it represented state of the art for offsite consequence analysis.

Kevin had recommended some additional work in the areas of straight-line Gaussian plume segment modeling, economic impact modeling, and assumptions that had been made on decontamination and clean-up costs.

MEMBER CORRADINI: Can I just get a clarification? For the economic consequence modeling with older data, meaning the data was old in the sense of out of date in terms of economic values? If I remember correctly, that was his point?

MR. GABOR: I believe it's all of the

economic modeling, and it would include what you just mentioned. It might include underestimating the impact on society in terms of a cost. Others had, a couple of others, you'll see, had similar comments.

MEMBER CORRADINI: Okay.

CONSULTANT WALLIS: That's my comment on the whole SOARCA. I mean the message of the SOARCA seems to be nuclear power's okay, because nobody dies in an accident. That means that the other consequences become the determining factor, right? And it doesn't say much about that at all.

MR. GABOR: The land contamination and cost issues were not part of the SOARCA study.

MS. SANTIAGO: Correct.

MR. GABOR: That comment was raised by most of the peer reviewers.

CONSULTANT WALLIS: But then when you publish a document for the public, a glossy little thing which says it's okay because nobody died, people are going to pick on this, and they're going to say well, is that the only thing we worry about?

MEMBER CORRADINI: Can I ask Graham's question differently? It was not in the scope of the study to report out these measurables or these calculated values; is that correct?

MS. SANTIAGO: That's correct. This is Pat Santiago. That is correct. It was originally part of the plan, and I'll talk about this again in a little bit. But in order not to delay the study more, that piece was carved out and it's being looked at under another project.

I think we're very, very clear on what SOARCA does look at, and it does indicate that it doesn't have a full HRA. It doesn't have the economic consequence and land contamination value. So Dr. Bixler and I see Randy Sullivan standing at the mic.

MR. SULLIVAN: You know, we were asked the same question by Commissioner Apostolakis, and there's nowhere in this study where it says we think it's okay that there's core damage. Our whole regulatory structure is such that we do not accept any of this. So I don't know how we get to this study saying it's okay, because nobody dies, that the plant melts down.

I mean it shows the effectiveness of emergency preparedness and saving lives. It has nothing to do with the NRC accepting these kinds of accidents as an okay thing.

MR. GABOR: Okay. I'll proceed. O'Kula found --

CONSULTANT WALLIS: Be careful you don't

give the impression. I mean I agree with what you're saying, but you've got to be careful when you do something for the public that they don't think that's what you're saying.

MR. GABOR: O'Kula found that the approach taken for the offsite consequence was comprehensive and sufficiently best estimate. He also found value in finalizing the uncertainty analysis, which of course we hadn't be able to see the results of, to determine where the best estimate values would lie relative to the other statistical measures, and we're going to hear more about that.

John Stevenson provided comments related to seismic risk. Early on, he had raised a concern that for the seismic event, the magnitude that it was assuming for Surry, the liquefaction of the soil could lead to displacement of structures and potentially cause containment penetrations to rupture, creating a release pathway.

We heard that comment from John at every peer review meeting. He was very consistent. Stevenson, he had also raised questions regarding the build up of hydrogen and a possible need for procedures and hardware to avoid the potential for deflagration and detonation transition.

His overall impression was that SOARCA made a significant contribution to the understanding of U.S. commercial reactor risk.

MEMBER STETKAR: Jeff, did John comment much on the seismic hazard or not?

MR. GABOR: Usually, he focused more on the liquefaction of the soil. He might have, in his individual comment. I don't remember.

MEMBER STETKAR: I mean I remember -- I wrote some notes to myself. Back in June, kind of we questioned for Surry that if you look at the USGS seismic risk map, you might infer that the frequency of the accelerations that were used could be, you know, a factor of 10 to 100 higher than what was used in the SOARCA project.

I was just curious. I know he's not a seismic hazard guy; that was just since he was your seismic name on the --

MR. GABOR: I don't recall that. If he did, he put it in his report. Okay. Next for Karen Vierow, Karen overall agreed that the approach for modeling severe accident phenomena was valid, and that SOARCA did represent the state of the art regarding severe accident modeling, again a comment similar to others.

Vierow found that benchmarking of melt core was a strength, and that additional sensitivity on certainty analysis would be essential to the overall credibility of the project. Vierow also found useful insights in the study, but cautioned against extrapolating the results to other reactor types, again a pretty common theme.

Then finally, Vierow commented that the current evaluations still retain some conservatisms in the analysis, and that a decision or discussion, excuse me, a discussion of those conservatisms would be helpful, again to enhance the overall credibility of the study.

Then Jacquelyn Yanch looked at the consequence analysis and the associated health effects. She noted that the SOARCA did not consider socioeconomic impacts, kind of my word, but I'm pretty sure that's where she was going, and felt that it was a significant omission in the study.

In addition, Yanch noted that due to the extended accident times, that the public would have a significant amount of time to leave the area, and therefore incur very little dose, which we see in the results.

Most of the dose -- also we see that most

of the dose calculated was incurred when the public returned back home, which was based on the state-established limits that we talked about this morning.

Then Yanch noted that the health effects approach that she felt reflected state of the art, and that it was consistent with approaches that had been taken by the scientific field in general, in those areas.

Then finally to wrap up, the peer review committee provided comments and written documents throughout the process. Specific memos were transmitted to the SOARCA team on key issues, and on the proposed uncertainty quantification and sensitivity analysis project.

Our final report was issued in January of this year, and that's all I have. Thanks for letting me summarize our findings, and I'd be happy to answer any other questions.

MEMBER BLEY: Something you said in the very beginning surprised me. I mean looking at some of the documents we got with the peer review comments, we see the comments from each of you, and a resolution. From what you said, that resolution was your resolution or I thought it was what the staff was saying they've done in response to the comments?

MS. SANTIAGO: The resolution that you saw was what the staff --

MEMBER BLEY: That was the staff's response. Okay, thanks.

MS. SANTIAGO: And in fact, that's what we went through in December at a fairly lengthy meeting, and then we received the final peer review letter in January of this year, as staff said.

So I'm going -- my name is Pat Santiago. I'm the chief of the Accident Analysis Branch in the Division of Systems Analysis in the Office of Research. I'm going to talk a little bit now about the public comments on Commission documents that we are planning.

(Paper shuffling.)

Slide 2, go to the Commission documents. The first will be a Commission memo. I will provide the SOARCA NUREG that 1935 is the name for, and then we'll have the specific analyses for Surry and Peach Bottom, as well as we'll provide the public information brochure that was developed.

The second document that we'll provide to the Commission will be a description of the projects and issues that we've identified throughout the process, that call for follow-on research, and then

we'll sum what the Commission had asked us to do originally, which was to develop a body of knowledge and give them a recommendation on the original scope of the project, which was to do 104 reactors, which was reduced over a period of time in scoping the project. So we'll talk a little bit more about that.

Slide 3 identifies the public meetings that we held. After we issued these NUREGs in January 31st of this year, we set out to provide two public meetings at each or near each location. One was held at Surry, Virginia on February 21st, and the other one was held in Delta, Pennsylvania, which was near the Peach Bottom site.

We provided basically an overview of the project, and we discussed various aspects of scenario selection, offsite consequences, just as you see in the outline of the main report, as well as the public information brochure, and we responded to many questions. Mike.

We had a number of comments that we received, approximately 75 comments. We had many attendees at the Peach Bottom public meeting, which we expected, and we're still going through some of those comments. We're bidding them, and I'll talk a little bit more about that in a second.

MEMBER ARMIJO: Excuse me. Those 75 comments, were they written comments or are they --

MS. SANTIAGO: Correct. They were submitted through the Office of Administration.

(Off record comments.)

MEMBER SKILLMAN: On your Slide 3 please, how well attended were those public meetings please?

MS. SANTIAGO: The public meeting at Surry had a number of state representatives, a number of licensee representatives. We had a few members of the public, a student, I think, that attended. At the Peach Bottom public meeting, we had three groups, public interest groups. One was TMI Alert.

MEMBER SKILLMAN: Beyond Nuclear.

MS. SANTIAGO: Beyond Nuclear, and they actually provided comments at the meeting, and then they also, in addition, submitted them through the public website that we had established here at NRC, for receiving comments on the main report.

We had the representatives from the state of Maryland. We had Dr. Dave Allard from the state of Pennsylvania and his emergency preparedness director. We had several people from the licensee there as well. So that was more well-attended.

CONSULTANT WALLIS: Since they're going to

be the audience, the people that are going to read this, the people it's addressed to, are the public, aren't they, and they're not the internal people like us?

MS. SANTIAGO: Correct.

CONSULTANT WALLIS: Is SOARCA supposed to go out there and influence millions of people?

MS. SANTIAGO: It's to inform and discuss a research project. That was done.

CONSULTANT WALLIS: It's important to get feedback from the people you're trying to influence, and eight people seems a pretty diminutive sample of that.

MS. SANTIAGO: We did the best with advertising. We attend University --

CONSULTANT WALLIS: I think you need to do a better job. Otherwise, you're going to get a report which is written for people like us, which I think it is.

MS. SANTIAGO: We have a variety of reports. We have a highly technical report that Tina writes, which documents this --

CONSULTANT WALLIS: The glossy one too.

MS. SANTIAGO: Yeah, the glossy one was for the public mainly, and we held a conference also

at the -- well, a session at the Regulatory Information Conference in March. We had probably 125 attendees there, several university professors, obviously a lot of folks from the industry.

MR. CHANG: We held a press conference with a number of --

MS. SANTIAGO: Yes. We held a press conference on February 1st, directly after we had the --

MR. CHANG: I'm going to send you the comment of my wife.

MS. SANTIAGO: Okay, on the glossy one? And so we've dealt with those or we're dealing with those comments still, and I'll talk a little bit more about those in a minute. So I guess the Regulatory Information Conference was the last public meeting that we had, and so on Slide 4, I want to just talk very briefly about the different types of public comments that we received.

The staff is currently finalizing the summary of all those comments, and then we're finalizing the responses to those public comments. We right now have binned those 75 comments into five general categories dealing with the scope of the analysis, scenario selection, accident progression,

emergency preparedness and consequence analysis.

On this slide, I talk a little bit of what some of the comments have been. The common one has been, as you heard earlier, that we did not analyze the spent fuel pool, or we didn't consider multi-units. That wasn't in the scope of the plan, and I'll talk again in a few minutes about where we believe the agency will look at those particular issues.

Other comments noted again that SOARCA did not do a full HRA for operator actions, and we talk about the use of mitigated and unmitigated cases. We did, as Jason spoke of earlier, perform tabletop exercises and walkdowns at the sites, and we used that information for this study.

We tried to make sure it was very clear and detailed in the specific site analyses, and we had a summary in the main report of what we did. Another question involved how we reported offsite consequences, and we talk about early fatality risk and the latent cancer fatality risk. This is what we used in part to compare to the NRC safety goal and the 1982 siting study.

Other comments noted that we should have looked at cancer incidence rates, as well as environmental contamination and economic consequences,

and again, I'll talk about those particular issues in a minute.

Many comments came after the unfortunate event at Fukushima, and that stopped us for a little bit, so that we could look at what did we have in our study that we could touch on, at least, to compare.

So we included an appendix in the main report, and Dr. Gauntt went through the five topics that we selected on RCIC, 48-hour truncation, multi-units, that we talked about in very general terms.

MEMBER ARMIJO: In that session, was there a lot of concern about the spent fuel pools draining, burning? You know, was there disinformation still floating out there that there was a catastrophe out there?

MR. CHANG: At the RIC?

MEMBER ARMIJO: No, at your public meeting.

MS. SANTIAGO: Public meetings didn't ask very specific questions, I don't believe, about Fukushima. But we did note what we did include in the main report with regard to that. But we didn't get many questions on that. We got more questions actually on emergency preparedness, and Dr. Allard was ideal to have at the Peach Bottom public meeting.

He was very supportive of how the study was done, and he actually explained what the state procedures were. Then as we go into a discussion on spent fuel pool, and you've heard it, I think the same group heard the discussion a few weeks ago, it talked more about the specifics and the consequences for that, and that study is really going to look into that more deeply than we could ever do in this particular time period.

There were also comments, many comments, in fact, about the codes that we used, the MELCOR code in particular. So we were very methodical about developing a validation and benchmark table for the MELCOR code, so that we could demonstrate how and what studies and what physical tests were done that could, that looked at the different sections of that code.

Another question talked about battery duration assumptions and station blackouts, and again, there were questions about the MACCS code and what the MACCS code does. So we're answering all these questions. Some of them I think we got even during the peer review discussions. So we'll be wrapping that up in the next week or two.

We did try to have that completed for this meeting, but we were focused more on edits to the

NUREGs and also to making sure we had the second appendix that will be included in the main report, which was for the peer review.

Slide 5 talks about the memorandum that I mentioned that we will provide to the Commission, that provides all these NUREG documents, and as I noted, they'll be three appendices included in the main report. It will be the Appendix A, which was on that Fukushima comparison.

Another appendix will be on the peer review comments and its final letter, and that was all provided to the subcommittee two to three weeks ago.

What we'll do there is basically bundle it with a cover sheet that summarizes what was done, and the meetings that were held, and attach the final peer review report as well as that comment resolution document that you received from the staff.

MEMBER REMPE: When is the memorandum supposed to go out?

MS. SANTIAGO: Okay. The memorandum and the Commission paper are due in mid-June, and so we scheduled this meeting and also a full committee meeting in -- well, the full committee is May 10th, so that we could ask for a letter.

MEMBER REMPE: Will you have updated versions of the Volume 1 and 2 for Peach Bottom and Surry by that time?

MS. SANTIAGO: We're hoping to have the updated versions by the end of next week, and so edits received any time now.

MR. CHANG: And we've received some edits, editorial comments from Dave Leaver. But we've been incorporating in, and like I said before, (cough) as well. So yeah.

MS. SANTIAGO: And so we have another person that's also focused on completing the summary of the public comments, binning them and providing responses.

In fact, had a draft available and we're working through the final answers. So by hopefully the end of next week, we'll have that available, the other third appendix.

And then we'll also talk in the memorandum to the Commission about the uncertainty analysis study and the status of it, and primarily identify the plan and the parameters that we're looking at. We expect to complete the uncertainty analysis at the end of November and Tina will talk more about that and the status and where we are.

On page or Slide 6, this is the compilation of many, many comments that we've received from various groups, the peer review, and these are the lists of issues that I've mentioned, some in passing, that we've identified, and where we think other agency projects will look at them.

As you see, the spent fuel pool issues and the multi-unit and site risk will be looked at under the Level 3 PRA, as well as the HRA and the low power shutdown and other modes that we discussed earlier. There also will be a comparison to NUREG 1150. People have asked us about comparing that. That will be done under the Level 3 PRA.

On the economic consequence/land contamination issue, there's currently an Office of Research Commission paper that's under development, plus we have other upgrades that we're looking at as far as economic consequence modeling for MACCS. That will be done in the next six months to a year.

As far as cancer incidence rates, we took the comment and we felt that that cancer risk study would look at that aspect as well. Additional comparisons Fukushima, we have a DOE/NRC group completing a forensic analysis. I don't know that they have identified other issues as yet to look at,

but the five that we chose we felt were the top five that we could at least talk about very quickly, as to why they weren't included and how they will be dealt with under other agency projects, to answer questions on that.

And then in one of th very first Commission papers that we prepared, we talked about developing an integrated predictive computer-based tool. But in Research we currently have various activities that are looking at that. Large seismic events and soil liquefaction, as Dr. Stevenson raised a number of times, will be looked at under the seismic research plan.

Then that brings us back to other plant and containment types, like the ice condenser, the Mark II's, the Mark III's, the CE and B&W.

CONSULTANT WALLIS: Now on this integrated predictive computer-based tool, is that something that we can foresee being electronically available to students and universities?

MS. SANTIAGO: Yeah.

CONSULTANT WALLIS: So we can ask the class to look at various accidents and pursue them when you have comments on them, and maybe you'd like what they think?

MS. GIBSON: That was something, as Pat said, that we had proposed to the Commission in the very first SECY that went up, to get approval to do the SOARCA project, and that was supposed to be part of the project.

As I understand it, the idea was that we would have a, what they call a faster than real-time tool, that the Operations Center and Reactor Safety Team could use to predict how an accident was going to progress.

What we decided over the years is that that alone would have cost as much as the SOARCA project, and probably taken as long. It was prioritized low and cut from the budget.

So we never did that, and what we're going to say in the SECY paper to the Commission is that based on the Fukushima experience, we don't think we need to do anything more on this, because we were able to use, with computers as fast as they are, and with MELCOR and MACCS.

We were able, and how long the accidents take place, as SOARCA showed and Fukushima, there's plenty of time to use the existing MELCOR and MACCS tools to do any predictive calculations that we want to do, and we did in fact do that during Fukushima.

So that's a long answer to your question.

CONSULTANT WALLIS: That's wasn't quite what I asked.

MS. GIBSON: Well, you asked if it was going to be available for students and universities, and the answer is no.

CONSULTANT WALLIS: So much of this stuff is within a very tight community like us.

MS. SANTIAGO: Well MACCS --

CONSULTANT WALLIS: No one outside it can sort of play with it, and investigate and think about it. They're very good if the educational --

MS. SANTIAGO: MACCS is actually used at various universities, and I don't know MELCOR.

MS. GIBSON: So is MELCOR.

MS. SANTIAGO: But I can tell you from distributing MACCS, in my particular group we do it often, and so it is well-known.

CONSULTANT WALLIS: There is a potential.

MS. GIBSON: And we had a user group, and all you have to do is join the user group, and you get MELCOR, and the only thing we ask in return is that if you find any problems with it, you let us know so we can fix it.

MEMBER REMPE: In light of some the

results you've gotten, even though they're clearly not comprehensive, wouldn't a new advanced or revised source term be something that could be -- with more work obviously, and maybe after the Level 3 PRA is done.

But it just seems like the source terms are so much lower. Your state of the art has improved so much that it seems like that would be something that would be a natural progression out of something like this, or it might confuse this, or something that nobody wants to do that for any reason.

MS. GIBSON: There is source term work out of it. It's not as a result of SOARCA. It's a result of the latest PHEBUS test.

MEMBER REMPE: Yeah. It just seems like --

MS. GIBSON: So eventually there will be another source term, but we haven't --

MEMBER REMPE: Aren't we supposed to do an advanced source term every decade or something like that, to stay updated, so that the utility --

MS. GIBSON: And that's in the works.

MEMBER REMPE: It's been quite a few decades since the last one.

MS. GIBSON: Well, with this PHEBUS test,

the final meeting and results are coming out, I think, next month.

MEMBER REMPE: So then at some point in the future, that would be on the docket or the agenda to do. So I don't see it in this list. So it might be something that ought to be added.

MR. GAUNTT: A lot of the, Joy, a lot of the ground work for revising the regulatory source term, if that's what you're talking about, has been underway for several years.

We've been looking at MOX fuel, high burnup fuel, and in the process kind of learned that the, you know, regular source term for non-MOX, non-high burnup is different than the, you know, last installment of NUREG 1465.

I don't know what the schedule is or, you know, drive or doing a rev to that, but a lot of the ground work's been done.

MEMBER REMPE: Just I thought I would discuss it, and I thought I'd bring it up.

MEMBER ARMIJO: Is there any work on your list, maybe it's buried in one of these categories on tsunami, probabilistic tsunami hazard analysis?

MS. GIBSON: I'm going to look at Marty and say is anything going to be done on Level 3 PRA

with regard to tsunamis? I mean we generally had gotten questions from groups on --

(Simultaneous speaking.)

MEMBER ARMIJO: Yeah, we did.

MR. STUTZKE: I'm Marty Stutzke from the Division of Risk Analysis in the Office of Research. As fare as the site Level 3 project, we will look at all the external events that screen in for the site, according to the standard, ASME standard for that. Now whether that specifically includes tsunamis or not, I think that remains to be seen.

MEMBER STETKAR: If you were betting, given the site they've selected --

MR. STUTZKE: Probably not.

(Laughter; simultaneous speaking.)

MR. STUTZKE: Now the site that's been selected is Vogtle Units 1 and 2.

MALE PARTICIPANT: It's a --.

MR. STUTZKE: It is on the river. I tried to screen it out yesterday, and my boss told me no. It's still on the table like this. But that would also include, we're going to look at upstream dam failures at the site, to see whether that's worthwhile.

We'll look at just large precipitation

events, things like that. Of course, hurricane risk like that; probably aircraft crash, things like that.

MEMBER ARMIJO: But not low elevation, coastal sites?

MR. STUTZKE: Well, not yet.

(Simultaneous speaking.)

MR. STUTZKE: The other thing is that within the Division of Engineering over in Research, they do have a tsunami research effort underway. But I don't know. I can't tell you the standard. That's what Annie Kammerer does.

CHAIR SHACK: Okay.

MS. SANTIAGO: Thank you. On Slide 7, I'm just going to talk a little bit more about the second document, the Commission paper, which will be a notation vote. That will be going up again in mid-June as well, and that paper will describe again the objectives, and that the staff believes that the SOARCA project objectives have generally been met, and that we don't believe a SOARCA-type analysis of all eight plant types or 104, as originally was described in the original paper, and now 108 licensed reactors, is needed.

We believe that the SOARCA project, as we present today, provides a large body of knowledge

updating the understanding of severe accident progression, mitigation and method and consequences, rather. We updated and improved the models that we use, MELCOR and MACCS.

MEMBER BLEY: Can I back you up to this one, not that I'm arguing for a --. But maybe somewhere, as part of the uncertainty analysis, some addressing of things that might be quite different if you were looking at different types or plants, might be real helpful for the sites.

MS. SANTIAGO: Okay, and one of the things that we want to conclude is the follow-on research that we'll still recommend to the Commission after this SOARCA project when we send it out, and that's Slide A. We believe that we could benefit by doing the analysis of an ice condenser.

That's a unique containment design. We did begin Sequoyah when we first started this project, so we believe we could gain some cost savings in time, because we did begin the decks for that particular --

MEMBER BLEY: I forget. How many are there, five, four?

MS. SANTIAGO: Ice condensers?

MEMBER BLEY: Yeah.

MS. SANTIAGO: Yeah. I think there's

about --

MR. CHANG: No, I'm not sure -- it's under ten, I think.

MS. SANTIAGO: Yeah, we have a list. Yeah, it's under ten, definitely under ten.

MEMBER BLEY: Well, it's about to get a Part 50 license hearing. Well, not about to, but is still in the queue to get one, Watts Bar II.

MS. SANTIAGO: And whether we'll argue that there's still a good need to look at it because of potential hydrogen vulnerabilities during station blackouts, and we believe if we limit it to just looking at the station blackouts, that we could do it in a relatively short time frame, a year, year and a half.

So we also think that in some of the NTTF recommendations and discussions that we've had, that they could inform some of those recommendations and results. So that's what the staff is drafting at this point in time, to prepare.

I have been talking to Hossein, because we did indicate to the Committee that we would provide drafts of these documents for your review. We just need some more additional management review and possible steering committee review before we present

it to you.

But generally we've given our steering committee and our management these recommendations. So with that --

MEMBER BLEY: But when you spoke of it as documents, is it like a project planned for each of these, something like that or --

MS. SANTIAGO: At this point in time, we would just indicate to, in this Commission paper, correct, what the recommendation is, what the resource needs would be, time lengths, and that would be basically it. And again, it would be very focused with the station blackout. With that, I think we'll turn it over to Tina, since we've -- or do we get a break? We get a break.

CHAIR SHACK: Thank you very much. We will break until three o'clock.

(Whereupon, a short recess was taken.)

CHAIR SHACK: Now we can come back into session. Now the moment we've all been waiting for, the uncertainty analysis.

(Simultaneous speaking.)

MS. GHOSH: Yes. My name is Tina Ghosh. I'm; with the Office of Research. This is Patrick Mattie from Sandia National Lab, who is the lead on

the Sandia side, and I asked Mark to come sit with us too, because he deferred a number of issues to this session.

(Laughter.)

MS. GHOSH: We have a fourth hot seat here, so what -- I'll get into who the team members are later, but I think as is obvious to everybody, SOARCA's a very multidisciplinary project, and certainly the uncertainty analysis, we're looking at a lot of different subject matters. So we have a fairly large group of core team members who are contributing to this effort.

So depending on how the discussion and questions go, we may direct, you know, questions to other team members who are with us today in the audience, and also a couple on the phone bridge, in case we need them.

So we can go to the next slide. This is the first time we're talking to the members of the ACRS about the uncertainty analysis in particular. We are in the middle of it right now, so we don't have final results and we certainly have quite a lot more work to do.

But we thought it's a good time to come talk to you, because at this point, we have a set plan

in terms of the approach that we're talking, the parameters that we're looking at, initial distributions that we assign, and we've gotten some very interesting preliminary insights on what kind of phenomenology is driving results in different regions.

We thought we could talk a little bit about that with you today. We would like to come back to you after we're further along with results, and I'll talk a little bit about that at the end. So this is kind of going to be more of a broad overview of what we're doing in the uncertainty analysis, and we certainly welcome your feedback.

I think you got a preview of a couple of the issues we are struggling with. One of them has to do -- a couple have to do with SRV failures, what we've assigned in terms of distributions and what we're looking at.

If you have insights that could help us in progressing our understanding of what we should be doing, that would be very helpful, and we'll get more specifically to that, again further in the discussion. Next slide, please.

MEMBER STETKAR: Tina, before you start getting into it, I was trying to read ahead here, but and I didn't see it. So I guess I'd just like to get

it on the table first. Could you define for me what the term "best estimate" means?

MS. GHOSH: In terms of the uncertainty analysis, the best estimate is what everybody else has done, who have spoken before me. So it's what is documented in the SOARCA main report, and the detailed analysis for Peach Bottom and Surry.

So that was kind of the team's, you know, best effort at providing a single estimate of, you know, consequences for the scenarios that were analyzed.

MEMBER STETKAR: Could you define for me what that means in the context of a real uncertainty analysis? What's the mode?

MEMBER BLEY: Maybe I could ask it in a little simpler way. I mean many people we talk with from SAP (ph), when they come here, when they speak of best estimate analysis, it's including uncertainty.

This is a point estimate, and is it supposed to be something like a mean? Is it supposed to be the most likely? Is it supposed to be the thing that would happen most often? What do people think about it when they claim they were doing best estimate?

MS. GHOSH: Yeah, and I'll let my team

members jump in after I give an initial answer, but I believe that something like the mode or the most likely outcome is what was the aim of the best estimate, and you know, certainly while the best estimate was a point estimate, there were a number of sensitivity analyses that were done for specific issues.

But you're right, that this is the first integrated uncertainty analysis that we're looking at. But the best estimate was an attempt to create the most likely, and if anybody else wants to --

CONSULTANT WALLIS: Well, in some of your graphs, you show a probabilistic, probability distribution, and the best estimate is at one end or the other. So it's clear it's not the mode.

MS. GHOSH: Well theoretically --

CONSULTANT WALLIS: What you mean is what was assumed in SOARCA. It's not the usual definition.

MEMBER STETKAR: And that's actually the reason for my question, because SOARCA is a public document, and the public understands the term "best estimate" as this is as good as we can do, and we would bet our lives on this. That's the way the public understands that term.

MEMBER ARMIJO: It's a big public, John.

(Simultaneous speaking.)

MEMBER STETKAR: It's a big public, but I would estimate that if a bunch of technical experts came out with a document that said this is our best estimate of what will happen, the average member of the public would say they're willing to bet their lives on this.

MEMBER SCHULTZ: We've heard of experts from the peer review that have said some, many. The models are conservative in these areas, but which may not be the best estimate.

MEMBER STETKAR: That's correct.

MEMBER REMPE: And in fact, Karen Vierow said I think you ought to emphasize more realistic instead of best estimate, and I'm guessing she's even more familiar with it than any of us are, and so maybe she's got a point --

CHAIR SHACK: Yeah, you know, but I think it's more than John said. It's the context of best estimate in something where you've got very broad uncertainties. It becomes, you know, a somewhat less meaningful thing. You know, if the uncertainty was very narrow when you say "best estimate," you really do have something in mind that may represent an expert's best opinion.

When you have distributions as broad as this, I don't know about any public that John knows, but you know, I doubt that very many experts would say that this is really what's going to be. But I'm a little surprised that the mode is the word that you would pick.

I mean every time the NRC picks a metric or something, it's almost always the mean. I could understand the median, but the mode seems the least likely of all those parameters that I'd pick.

MR. LEONARD: In some sense it depends on whether we're talking about a statistical reliability parameter, SRV failure rates, stochastic failure rate. Then we could argue whether mean, mode, median is appropriate. In many cases, though, we're looking at phenomenological modeling uncertainties, not really parameter uncertainty.

Then what mean, median, mode, how you would properly interpret that becomes a little more difficult, more challenging. When is say mode, I would say we would say it's the most likely. It means it's based on the evidence we have from available experimental data, and availability of alternative ways to model the integrated processes.

What's the most likely, best combination

of the mathematical models we have, that seems to reproduce the signatures that we've observed in the experimental data. MELCOR as a code as a whole represents that style of thinking.

The other alternatives are available. Users can pursue the effects of other modeling approaches. But the "best practice" principles tend to use the selected combination of models and their input parameters, that provide the best match against the experimental data we have available. In that context most likely is what we're trying to represent.

MEMBER STETKAR: How do you answer the possibility that we heard from the previous presentation, that the uncertainty analysis report is kind of targeted for let's say, you know, late fall of this year, that quantitative estimates that come out of that uncertainty analysis, whether they're characterized as a median or a mean or something, could be substantially different than the quantitative best estimate that comes out of something that's published now in January-February-March?

How would the NRC staff answer that discrepancy, let's call it, or perhaps perceived discrepancy?

MS. GHOSH: Okay. So I think what I can

tell is we've done preliminary analyses, which we'll talk about. Today, we're only going to talk qualitatively about the results. But I think we have seen enough of a range of results to know kind of where the answers are likely to come out when we do the final set of runs that are going to be documented in the fall, and I think what we can say is that we haven't seen anything that invalidates what was done in the best estimate study.

In terms of the general conclusions, such as, you know, the source term and the health effect results being much, much, much lower than the Sandia siting study, I mean those continue to hold true in a very wide range of conditions that we're looking at.

MR. MATTIE: But I just want to add something here, and I think it's important, because internally we have this discussion almost daily, and it's almost a philosophical discussion of what are you more competent in?

Are you more competent in a mean value that can be heavily skewed by a very high outlier result, or are you more confident in a best estimate result, and a best estimate which in part operator, experience, you know, most likely occurrences, belief on a broad range of uncertainty, of possibilities,

physical possibilities, that the belief, based on operator experience or other intuitive or basis on the phenomenology, that this is the most likely event?

I think you actually put more value in your best estimate, regardless of where it falls in the distribution, because when you do a probabilistic analysis, most of the time what you're saying is your probability of exceeding a certain value. That's more a quantitative measure than the mean, in my opinion.

MEMBER BLEY: But look, most likely nothing's going to happen, okay. But something may happen, and that's what we're interested in. So that, you know, if there's something that has a ten percent chance of happening, and it's nowhere near the most likely, given that you've got an accident going on, but the consequences are much more severe, that could really be where the answer is for the risk.

Now Tina is saying that you haven't seen anything like that yet in the cases you're looking at, but I find that a little surprising from what other people have said about DFs and things like this going on in various scenarios.

MS. GHOSH: Yeah. I think when we get a little bit further into the presentation, that we show -- I'll tell you the three cases that we've run so far

from MELCOR, and I'll give you examples of the results of that we're seeing.

So it will give you some idea of the difference in results, and these are not statistical results. We picked out a couple of individual possibilities that kind of demonstrate when different phenomena dominate. The biggest one is main steam line creep rupture versus having the early stochastic SRV failure, which prevents that.

So you'll get a sense for, you know, the range of results we're seeing. You know, I think the challenge with this uncertainty analysis is that, you know, unlike something like a Level 1 PRA, where we have a lot of data on component failures, we're really looking at a range of phenomena in this uncertainty.

Some of them we have some idea of what the possibilities could be. Other ones, we don't have as much data and information on. So I think, you know, what Pat, again what Patrick was kind of going back to is do you put more -- I think what we, one of the values of doing the study is to see what are the most influential parameters, you know. What are kind of the ranges of parameter combinations that we would worry about, in terms of giving a very different result.

But in terms of putting a lot of stock on the exact results, I mean that's harder to do, just because there's so much uncertainty about even characterizing the uncertainty in some of the phenomenology that we're looking at.

CONSULTANT WALLIS: Like the vessel failure. I mean this morning, I thought we heard there was no analysis and no data. So how can there be epistemic uncertainty?

MEMBER SCHULTZ: It sounds as if you're not doing an uncertainty analysis, as I would see it. You're doing an investigation into uncertainty.

MS. GHOSH: I think that's fair to say. For some of the things we're looking at, I think that's fair to say. You know, it's part of this iterative process. I think for some of the parameters, initially that was it, you know. We were just looking at what effect could this parameter have, looking at a certain thing.

You could almost look at it as a combined sensitivity analysis. So whereas before we were perhaps looking at one issue at a time, now we're taking the great computational power that we have and looking at many, many parameters, many, many different combinations at a time.

So it is almost like an investigation of the effects of the possible uncertainties, and we'll go a little bit more into that. So it's very much an iterative process. Once you figure out which are the ones that are giving you the largest effects on results, you try to go back and scrutinize what you know about that parameter more and so on.

CONSULTANT WALLIS: We heard about the VANESSA code, right. I'm not sure that it's possible to determine the uncertainties in the code, because I'm not sure that there's a way to do that, because you can't untangle what's in the code.

Also, VANESSA's an equilibrium code. So the basic uncertainty is more than the parameters that it predicts or to go into it. It's in whether it's relevant, because great phenomena may be more important. So the epistemic uncertainty is not just determined easily by parameters.

You may have a code like VANESSA, where you can't really figure out what determines the uncertainty in its output, and you may also have a code like VANESSA which may not be the right, modeling the right phenomena.

MS. GHOSH: Yeah, actually and that brings us to, I guess, what the scope and the goals of the

uncertainty analysis were, which is this slide, and you know, it's carefully worded.

The goal of this uncertainty analysis, and I certainly hope it's not the last uncertainty analysis we ever do, because it's a very interesting area, and we still have a lot left to learn. But we wanted to --

MEMBER STETKAR: Excuse me, Tina. You're not going to get out of it quite this quickly yet. This is a question for the whole project.

MS. GHOSH: I haven't gotten out of anything.

(Laughter.)

MEMBER STETKAR: Right. You're trying to get through the things on the slide. Obviously, it's a question for the whole project. It's not just the uncertainty analysis.

There is a lot of benefit to doing some amount of uncertainty analysis, especially considering the fact this is a public, focused toward public information, to illustrate a level of confidence, in terms of the margins that are available.

In other words, we have reasonably high confidence in a lot of the stuff that we've seen. We're not going to characterize anything by a best

estimate, by a number. There's uncertainties; we've examined the uncertainties.

We don't have enough quantitative information to be able to characterize a number with 1, 2 or 12 significant figures as a mean or a median or a mode or whatever you want to call it, of a distribution.

But we've done a lot of -- we've looked a lot at the uncertainties, to characterize the fact that we still have confidence in the margins, confidence in the fact that what we've done gives us information to say that a lot of the earlier studies did have a lot of conservatisms in them for the following reasons.

I think that's a very worthwhile exercise. I think it's a very powerful communications tool, if you cast it that way. On the other hand, developing that story ten months after something that comes out with numbers on it, that are called best estimates, could be a bit of a problem, follow me.

So it's not necessarily a discussion specifically about what's being done or will be done in the uncertainty analysis. It's a way that the whole project characterizes the results, and what people will understand as those best estimates.

MEMBER BLEY: Can I say one more thing on this? Well, I guess am belaboring it. But I don't see anywhere in your reports where this concept is really described.

I see best estimate this, best estimate that, better best estimate this, and proved best estimate that. But nothing about what that really means. I think you're very vague about it.

MR. MATTIE: I've got a -- in my head, you know.

MEMBER BLEY: We all know what it means, but it might be very different to all of us.

MR. MATTIE: I came here with a speech, and I remember very little of it. But I will say --

(Laughter.)

MR. MATTIE: --going back to the goals, I mean realistically maybe we should never compare the uncertainty study with the best estimate, because what the uncertainty study, at least in this first iteration is, what is believed to be important parameters. They've been selected.

It's not a parametric study of everything in the consequence analysis or in the Level 3. It's what we believe, based on our experience in the SOARCA best estimate, what we believe are the most

influential parameters.

We've taken the best available data to populate those distributions. We've run it through the mill and we come and we go wow, maybe we were wrong. This doesn't really affect it, or it affects what we thought it would, or it in fact has a greater effect.

When we revisit those results, we then take a closer look at the data that went into it, and what's our confidence on those extreme values, if that's in fact what's driving some of the unexpected behavior or surprises, and then we look at the model assumptions and how they relate to those ranges of inputs, and then we look in fact at the ability of the code to calculate over those ranges and model assumptions, and that iterates a few times.

I mean we honestly would have to do that extensively, before you could come back and say exactly where your best estimate fits within that distribution. In this first iteration, all we're really getting is an indicator of how much we know about what's driving our system uncertainty.

That's what we're getting in this first iteration of the uncertainty, and maybe that should be explicitly stated in the uncertainty analysis, rather

than a comparison or a validation of the best estimate, because it's not.

CONSULTANT WALLIS: How about operator actions? I mean there are quite a few operator activities involved in this scenario. Are you going to subject them to uncertainty analysis?

MS. GHOSH: Yeah, we know. We talked about this actually with the peer review committee, you know. Several of the members brought up the fact, and this came up earlier. So we're looking at the Peach Bottom unmitigated long-term station blackout scenario. In that unmitigated scenario, there are a handful of operator actions that are credited.

Yeah, so there's two -- well, a third one that had -- okay. So there's two that we are planning to look at in a separate sensitivity analysis, and the primary reason for that is that we did not have an HRA, as you know, in the SOARCA best estimate.

We still don't have -- we don't really have an HRA team for the uncertainty analysis either, and anyway, we want to keep with what the SOARCA best estimate did. Those two actions are manual depressurization at one hour, and taking manual control of RCIC at two hours.

There's -- so we want to do is a

sensitivity analysis that looks at what if those two actions were taken at a different time than what is --

CONSULTANT WALLIS: For actions you anticipate them taking.

MS. GHOSH: Yes.

CONSULTANT WALLIS: At Chernobyl and TMI, there were some things the operators did, which were not anticipated they would do.

MS. GHOSH: Errors of commission, so to say, yeah.

CONSULTANT WALLIS: Commission. I don't know what you do about something like that.

MS. GHOSH: Yeah. You know, I think for the scenario that we're looking at, you know, the station blackout, there's not a whole lot working.

You know, the ones that are credited, we felt had pretty high reliability, because they're described very specifically in EOPs and they're practiced regularly in the training that the operators have.

So we felt pretty confident that those would be taken. What we thought was uncertain was the timing, the precise timing at which those actions would be taken. There is a third action, to block open the doors, I think, to prevent overheating.

MR. LEONARD: Well, there's a general series of actions that sheds loads on emergency buses to extend the battery lifetime. So we're not explicitly representing the actions, but we're representing the effect of the action, in terms of a distribution of the net battery lifetime. So that set of actions, if you will, is captured indirectly, but the other two are not.

MS. GHOSH: Yeah. So those will be dealt with separately, rather than the integrated uncertainty analysis.

MEMBER SCHULTZ: And what you've described is an uncertainty study or sensitivity study to elements that are different than what's been assumed in the analysis that's being published now.

MS. GHOSH: Right, right. Right now there's this --

MEMBER SCHULTZ: And I wouldn't call that an uncertainty analysis. I hate to back up to the beginning, but --

MS. GHOSH: Sure, sure. This is -- all I mean to say is we didn't include those as parameters in the integrated uncertainty analysis, but we're treating those separately. In a sensitivity analysis, that will be documented in the uncertainty --.

MR. LEONARD: In a sense, the best estimate characterization suffers from the absence of an uncertainty analysis ahead of time. Had the, if we did the analysis in the reverse order, then this discussion wouldn't be taking place.

But the prescription was to reproduce a single best estimate characterization of accident progression offsite consequences for the scenarios that survive the screening criterion. The uncertainty analysis was an effort to try to characterize the uncertainty about that best estimate after the fact.

That puts us in a language problem, statistically in a language problem, but as Mr. Stetkar is fond of saying, it is what it is, and that's where we are.

MEMBER STETKAR: Well, but when you characterize what it is, again if it were only the group of us sitting around this table, I think we pretty well understand technically what was done.

MR. LEONARD: That's right, yep. But you asked the question would it change?

MEMBER STETKAR: Huh?

MR. LEONARD: You asked the question would the "best estimate" change. Now that you are in the position that in an idea context you would have been

in the first place. We don't know the answer to that right now.

MEMBER STETKAR: And therefore, as Dennis mentioned, some discussion in today's report, not the November uncertainty report, but the SOARCA report on what is meant or how to interpret those best estimates or how to try to understand what they might mean, in the context of a yet to be published uncertainty study, and I agree with Steve; it's a study of uncertainties.

MEMBER REMPE: And I think that discussion should talk about phenomena especially, because it is difficult to say a best estimate --

MR. LEONARD: It isn't, because they're not only complex in the sense I mentioned earlier, but many of them are non-linear and cumulative and dependent.

So it's very difficult *a priori* to decide which direction the trajectory of any of these signatures we've talked about so far today, which way they will go when you start combining uncertain parameters. That's the purpose, really, of the uncertainty assessment, is to determine that.

MEMBER BLEY: I guess you're right, and the thing you said earlier is really important. Our

letter four years ago kind of said you ought to be looking at uncertainties from the beginning, because that puts it in the context.

As I looked through the two reports you have, there's a lot of discussion of uncertainty in the earlier work, and that there's been research done to address that. There's almost now words addressing the fact that there's uncertainty in the scenario selection, in all of the steps that led to the results of the SOARCA analysis.

Now that's the thing, I guess, that bothers me most. Eventually, I don't think that's fixed, and that's where we're headed. But enough said on that.

CONSULTANT WALLIS: Can I give you an example that's puzzled me, and you may want to explain something here. You're talking about thread and collar friction, the tightening up the bolts on the head, and there's a statement that says "The friction factor varies between .12 and .2." Okay. So we expect a distribution or something.

Then it says "The best estimate value is .09." Now does that mean the best estimate value is the value that was use din the SOARCA, or how does it get to be outside the range of possible values? How

do I interpret a statement like that?

MR. MATTIE: That one parameter, I guess -- which one is that?

CONSULTANT WALLIS: It's on page 429, drywell head flange tightening, where they tighten the bolts.

MR. LEONARD: Everyone's pointing at me.

(Laughter.)

MR. MATTIE: In response to your question, number one is in the report, "best estimate" indicates in the uncertainty draft report that you received, when it says "indicates a best estimate value," that was the value that was used in the best estimate calculation. That's correct.

CONSULTANT WALLIS: I'm thinking it would be much clearer to me if you said that the data shows it goes between .12 and .2. But SOARCA used .09.

MR. MATTIE: Yeah, the SOARCA estimate.

CONSULTANT WALLIS: Because when you say "the best estimate value" --

(Simultaneous speaking.)

MS. GHOSH: Okay, thank you. Yeah.

CONSULTANT WALLIS: I think you have to say "the SOARCA estimate." Then we'd know what you're talking about.

MR. GAUNTT: I wonder if I might just make one comment. I don't know if it will help. This is Randy Gauntt, and I've been struggling with this uncertainty phase and what does the best estimate mean myself.

One thing I know the best estimate means we've evolved this code and invested knowledge in it for like 30 years, and we run it on TMI and we run it on PHEBUS experiments, and we try not to set up anything unique that's only good for this best.

So in a way, our best practices document is our cumulative notes on how we think is the best way to run this code, given the sum total of all the assessment work that we've done. That's what really brought us to the SOARCA study, is that after 30 years of work and assessment, is our best estimate.

And now we're challenged here with putting uncertainty distributions on this, and wondering well what should our cumulative knowledge that led us to this SOARCA platform, is that going to land on the mode or the median, or what does it mean in terms of the distribution?

So I guess I just wanted to make the point that we came up with best estimate based on 30 years of cumulative knowledge invested in the code, and now

at this point we're struggling with what is uncertainty. I don't know if that helps but --

MR. LEONARD: Well it does, in the sense that I think the issues or the examples that Tina will eventually get to, you'll see that the topics that we tend to see as controlling are ones where the parameters are being varied are boundary conditions from outside the code, not parameters that reflect models inside the code.

CONSULTANT WALLIS: Change the term "best estimate" to "SOARCA estimate." Then we'd know what you're talking about, because it's very disconcerting to see a best estimate which is outside the allowable distribution. If you say "SOARCA estimate," then we could say "Aha. They wanted to be conservative, so they overestimated or something."

MEMBER BLEY: And Randy's comments are very reasonable, but you picked a term that means many things in the outside world. Generally, it's not that.

CHAIR SHACK: Tina.

MS. GHOSH: Okay. Getting back the goals of this uncertainty analysis, we wanted to develop insight into the overall sensitivity of the SOARCA results to uncertainty and inputs, in the existing

SOARCA model. So I want to say that up front. We wanted to use the existing SOARCA models; we weren't looking at alternate models.

So using the existing models, we wanted to see well, what are the most influential input parameters for both the releases and consequences. And because we expect that we're going to be doing a lot more of this type of uncertainty analysis in the future on severe accidents, we wanted to demonstrate, you know, how one could go about doing an uncertainty analysis.

If we go to the next slide, as I mentioned the focus is on parameter uncertainty. There was a lot of discussion this morning on issues surrounding model uncertainty, which is an issue that's very near and dear to my heart. I spent some time developing a doctoral thesis, and as an intern with ACNW back when Mike was on the ACNW looking into that.

But for this uncertainty analysis, the extent to which we address model uncertainty, as Mark mentioned, is really where that isi captured in the parameters, in the existing model parameters for MELCOR, and beyond that, we're not looking at alternate models for different phenomena.

And as we already mentioned, we're using

the Peach Bottom unmitigated long-term station blackout kind of as a first step, because we had to scope this uncertainty analysis. Fukushima happened, you know, after we had already -- after we were well into the SOARCA project, and after we had started the uncertainty analysis.

We did not change our scenario definition based on Fukushima, and I think Randy already talked a lot about that, like the RCIC run time and so on. So we kept it to our existing Peach Bottom long-term station blackout.

What we're planning in the uncertainty report is a separate qualitative discussion that talks about comparison with Fukushima. But we didn't change our scenarios to reflect what we saw at Fukushima.

MEMBER BLEY: Don't go on yet. Your second bullet is one that I was kind of hanging on as I tried to read the report, and I've been through it, but not in detail enough to feel comfortable. Model uncertainty is addressed only to the extent that some parameters, that's the thing that hit me.

Everything in here fiddling with parameters and what they could change, and you know, when we have a presentation on a new experiment that sometimes give us surprising results, there's

different chemical processes than people expected.

Are you picking up -- it wasn't clear to me, because that's not what I do much of. Are you able to pick up those different possibilities and processes that might be going on strictly through the parameter uncertainty range? It would seem they have to be groups of parameters that would affect a whole process --

MR. LEONARD: Let me give one example, perhaps, to make it more concrete.

MEMBER BLEY: Yes.

MR. LEONARD: It was mentioned earlier about the speciation of either iodine or cesium or both. Certainly since siting study work was done, even early calculations with MELCOR, the insights from PHEBUS have caused us to change our understanding of the dominant form of the aerosol that cesium is transported.

In the past MELCOR, and before that earlier code calculations represented it as a hydroxide. The PHEBUS work now suggests that it forms a molybdate as the dominant transport form. That's not absolutely certain. The integrated effects of the PHEBUS experiments suggests that's the trend.

But if you do the small individual pellet

release experiments, hydroxide still is, meaning evidence from other experiments. We can't completely eliminate the possibility that it could be an alternate form.

The competition between the two is a detailed chemical process that is not represented in either a deterministic way within MELCOR, but it is a true modeling uncertainty, in terms of what are the governing phenomenon that determine the proper speciation of both cesium and iodine.

MEMBER BLEY: And this change --

(Simultaneous speaking.)

MR. LEONARD: So how do you address that global modeling issue? The way in which we've tried to capture it here is to simply pre-populate the chemical species that MELCOR releases from the fuel and transports downstream into different chemical groups, pre-reacted chemical groups. How much of it is released as hydroxide, how much of it is released as a molybdate or other forms?

And so we do that in a discrete form. We have five different species, combinations of primarily iodine and cesium that can be transported.

Each of them has a relative probability, and so the modeling uncertainty associated with the

chemical reactions that create those is not properly captured as a model uncertainty, but we represent the effects as parameter.

That is, a pre-populated concentration of those individual groups, and we randomly select from those different groups.

MEMBER BLEY: That makes sense to me. When I read this, it wasn't clear that -- it wasn't clear to me that picking those different populations and distributions was accomplishing, was at least trying to be a substitute for doing --

MR. LEONARD: A more comprehensive modeling.

MEMBER BLEY: The modeling. What we're really after here is a model uncertainty, and we're reflecting that through these selections. Does it say that in here? I might have --

MR. LEONARD: It might not say it as clearly as it should.

MEMBER BLEY: It wasn't clear to me. So I was saying gee, I haven't picked up any of the process stuff that's important, but I saw the possibility that you were doing that. I just couldn't tell.

MR. LEONARD: Yes.

MS. GHOSH: Right. Yeah, I appreciate the comment. We're still working on improving the documentation. You know, we're frantically both doing analyses and working on the documentation, and what we gave you is a snapshot in time, and we're certainly working on improving that as we continue our analyses and parallels.

So we'll revisit that, and make sure that we've captured some of that --

CONSULTANT WALLIS: Well, you can do that if you have data for the population, how you populate these various things. You have to use PHEBUS or something.

MR. LEONARD: Yes.

CONSULTANT WALLIS: And in the case when you used VANESSA, you don't have that. It's all model uncertainty.

MR. LEONARD: And the ex-vessel releases at the moment are not on the list. I'm trying to think. I have to check to be sure, but I don't believe that we're varying anything in terms of the ex-vessel contribution of the source term is not part of the list of uncertain parameters.

MS. GHOSH: Okay. So the focus of the uncertainty analysis is the state of knowledge

uncertainty, the epistemic uncertainty and input parameters. We just want to point out the aleatory uncertainty due to weather will continue to be handled in the same way that it was for the SOARCA best estimate study.

Whether uncertainty is modeled in the best estimate, that's the one uncertainty that explicitly is modeled, and we continue to handle it in the same way in the uncertainty analysis. So as you know, we have the MELCOR portion, the MELCOR code to model the releases, and MACCS to take it to the final consequences, and we're looking at the uncertainty and the key model inputs for both of those codes.

MEMBER BLEY: I mean this one does it again. The focus is on uncertainty and input parameters, but it's not really what drove you. It was, I don't think. I think it was trying to pick up some of these modeling uncertainties and using that as a surrogate for it. I hope I'm right. If I'm not, then --

(Simultaneous speaking.)

MR. LEONARD: Randy can probably speak to this better, but in some ways that's natural to MELCOR. You know, the nature of MELCOR trying to calculate everything under the sun means that

compromises have to be made to model detail, and some of the models are more parametric than others, and those parameters represent a combination of governing phenomena that lie behind them. So there's a range that is probably reasonable to pursue.

That's the connection between modeling and parametric uncertainty. Rather than, for example, what is the SRV stochastic failure rate as a number. I mean that's not what we're talking about.

MEMBER BLEY: I think your analysis would be more convincing if it told that story.

MR. LEONARD: Okay.

MEMBER SCHULTZ: And if you describe those things, and if you also describe in the description of what is being done, what is not being done, because you've described it nicely here. But that needs to be documented.

It has to be documented, so that the reader understands right from the first section of the documentation, this is what we are doing, this is what we are not doing, and why we're doing the first part and why we're not doing the second.

MS. GHOSH: Okay.

MEMBER REMPE: So phenomenological uncertainties that you're not capturing should be

identified.

MR. LEONARD: That's right, and I mean Dr. Wallace has already pointed out since we've heard the ex-vessel contributions have some effect, I mean it's a model within MELCOR that we do not have parameters to vary. I mean it's a deeply-seated model within the code, that there's not a tool that we can use to manipulate it that looks at these phenomenological uncertainties. So it's not on the list.

CONSULTANT WALLIS: Do you look for strange numbers? I mean on Figure 4.1.1, there's a SRV closure estimate of 10 to the 7th demands before failure? Does that sound reasonable?

MR. LEONARD: No.

CONSULTANT WALLIS: 10 to the 7th, or maybe I misread the figure. Ten million demands for -- I would cut this probability thing off somewhere.

MR. MATTIE: Well let's -- we can address that. I mean but that directly comes out of the NUREG/CR information that we have, the distribution that we use, right. And I know many of you are familiar with that distribution and it's some form of Bayesian update and you fill in the gaps, you know.

So it's very sparse data. The idea is that you'll never get there, because the thermal

failure happens well before that. So it wasn't a concern at that point.

CONSULTANT WALLIS: It doesn't matter.

MR. MATTIE: And I just -- there was something that was said earlier, and I apologize if maybe I'm out of order here, but I don't want to get in a situation where we promise something we don't deliver, because if you try to document everything you don't include, it could be an extensive documentation.

I'd rather say that we'll make a point of documenting that the analysis, the effects can only measure what we did include in the uncertainty. You can't measure a parameter you didn't make uncertain in the uncertainty analysis. It's constant. You know, you don't see any effect of it because it's constant value.

MEMBER BLEY: Even if you can't model it, if there's something that you know could be important, leaving that out, just leaving it out, I think, creates a misimpression.

MR. MATTIE: Where we could identify, and I think we have made in the past, especially in the conversations in the peer review, we tried to identify areas that we felt, we believe could be important, but we just had no basis for any distribution or to, you

know, data to make it uncertain.

MEMBER BLEY: That's not everything. That's something you thought could be important. It deserves a spot, even if you can't quantify it.

MEMBER STETKAR: That's what I was getting to.

MR. MATTIE: But otherwise, we did try to include everywhere at the first cut we thought was important, based on subject matter expert input, and in some cases use uniform distributions, because we just don't know, to see if it actually has an effect in the analysis.

You know, I should get into the analysis results. That comes out. But I just want to make sure that --

MEMBER SCHULTZ: I've been really meaning to say that you want your documentation to be such that you don't leave the reader with the expectation that you've done everything, and then they can poke holes in the documentation or in the study, because you didn't describe why the entire spectrum wasn't included.

MEMBER REMPE: For example, you have in your main report, you concluded improved phenomenological modeling for things such as reactor

pressure, vessel failure and molten core concrete interactions. Yet different failure modes weren't considered in the uncertainty analysis, ex-vessel phenomena uncertainties weren't considered, right?

MR. LEONARD: Ex-vessel fission product release was not considered uncertain, but lateral debris mobility was. It is treated as an uncertainty.

MEMBER REMPE: And that's the only or the key driving uncertainty you think with respect to ex-vessel phenomena, such as molten core-concrete interaction? There's not other types of phenomena that you might be wanting to consider?

MR. LEONARD: The comment's well-taken, in terms of how we would provide some, the reader some caution, that this is not necessarily comprehensive. That's fair. We'll find someone to do that.

MS. GHOSH: You're talking about -- you're talking about the main report?

MEMBER REMPE: Well, it's giving the reader one perspective, and then if they read the uncertainty analysis, they may think well they have taken -- and everyone has limitations to their work. I'm not trying to be too picky here, but I just think some acknowledgment of it is needed.

CONSULTANT WALLIS: It's very good to try

to study the uncertainties you can characterize, and it's very valuable. But you have to say that there's some that you didn't characterize.

MR. LEONARD: In agreement.

MS. GHOSH: Okay. So this slide starts to describe basically how are we going about the uncertainty analysis. We've identified key uncertain input parameters. Not all uncertain input parameters, because you know as we've been discussing, certainly there are a lot of possibilities. But in expert judgment, what were considered to be the key ones in the model.

Then we propagate the uncertainty in two steps using Monte Carlo technique, get us to a set of source terms from the MELCOR code, and then we feed those in the MACCS code, and the MACCS code uses those source terms, along with the uncertain samples for the MACCS parameters to spit out, in this case up to 300 code runs, which we call realizations.

So if I say "realization," it's essentially, you know, one code run, the output of one code run, and we're looking for basically what is the influence on the outcome. If we go to the next slide.

MEMBER REMPE: Out of curiosity, like how long does a typical uncertainty analysis take speaking

in time, versus a single MELCOR for a similar case? I mean is it --

MR. MATTIE: The same amount of time.

MEMBER REMPE: You can do the analysis in the same amount of the time as the point estimate?

MS. GHOSH: We have Sandia's resources.

MR. MATTIE: Roughly. We have about 300 computers available, and that's what we do.

MEMBER REMPE: Okay.

(Laughter.)

MR. MATTIE: I will say, though, that -- and again, I apologize for maybe being out of order here, but the machine time is the least of your time on these types of analyses. It's the actual close -- you know, you're processing, you're evaluating.

You run your statistical measures, and we'll get into it in this presentation, on systematically, you know, going through the volume of data.

MS. GHOSH: Yeah, but we're essentially people-limited, in terms of how many people and how much time, rather than computational resources.

MEMBER REMPE: Okay.

MEMBER SCHULTZ: It's not out of order and it's not different.

MS. GHOSH: The results we're planning to include in the final report are the cesium and iodine releases over time, the distribution of latent cancer fatality risk, and using the three dose threshold models, the LNT and the other two threshold models that were used in the main SOARCA study.

Really the crux of this is we want to be able to identify, out of the parameters that we've looked at, what are the most influential parameters to the outcomes and kind of why. So get at the phenomenological insights and what are the most important things that we can drive the results in different directions.

The tools we use are standard statistical regression-based methods, as well as scatter plots and looking at individual utilizations of interest to get phenomenological insights.

I think Jeff mentioned we -- as Jeff Gabor mentioned, we did solicit guidance from the SOARCA peer reviewers. The uncertainty analysis was outside of the scope of the SOARCA peer review committee, but we did present the initial approach and plan, and requested a guidance document from them.

We were lucky enough to be able to go back to them with an updated plan of the parameter choices

and distributions, and kind of had a teleconference with them on how we had addressed their comments on what they had seen so far.

MR. MATTIE: They provided an extensive list of comments that we addressed, and quite frankly improved the analysis because of it.

MS. GHOSH: Yeah, and in the final report, we'll have an appendix that has the peer review reports, as well as the staff on responses to the --

CONSULTANT WALLIS: Your final conclusion, what will it look like? I mean SOARCA now says the very low, let's say it says no one will die. Let's say it says that. It seems to say that, but so what?

Your final report, because of all these broad distributions, is going to come out with a distribution that, you know, so much probability of 100 people dying, all of this stuff, and it's going to look very different in terms of public impact, isn't it?

MS. GHOSH: You know, based on our preliminary insights, while we are going to get a distribution, we're still not getting anywhere close to, for example, essentially zero early fatality risk.

Even if the latent cancer risks are higher for cases like main steam line creep rupture, they're

higher and, you know, in quantitative terms, could be quite a bit higher. But in absolute terms, we're still talking very low risk.

CONSULTANT WALLIS: But it doesn't change the conclusions?

MS. GHOSH: No, absolutely not.

MEMBER RYAN: Tina, could you just back up one second to the other slide. You mentioned the three dose threshold miles. Could you talk a bit more about that?

MS. GHOSH: Yeah. I believe maybe on June 2010, the committee was briefed on this. We're doing LNT of course, because that is our regulatory basis, and then the other two we call, for short we call U.S. background, which is actually the average background and medical dose, which is set at 600 and some millirems per year.

So anything under 620 we disregard until you get above that threshold, and then the third model we call for short the Health Physics Society model, which is if you get five rem in any one year or ten rem over a lifetime, if you don't get anything above that, then we disregard those doses. We only calculate the risk once you get past that threshold.

MEMBER RYAN: What risk models are you

using? Are you using linear, sublinear, threshold, for the cancer part of risk?

MS. GHOSH: Yeah. So we're going to eventually exercise all three of those models for our results. To date, we only have LNT results for one of the cases so far.

MEMBER RYAN: Right. But you are? That's kind of a future step?

MS. GHOSH: Yes, yes.

MEMBER RYAN: Thank you.

MS. GHOSH: And you know, we had some debate about whether to combine that health dose model uncertainty -- not uncertainty, but choice with the integrated uncertainty analysis, and the decision was that that's really mixing apples with oranges. So we're keeping that separate from the integrated uncertainty analysis.

MEMBER RYAN: You're talking about almost a deterministic decision to put it this way or that way.

MS. GHOSH: Yeah.

MEMBER RYAN: So it's probably good that you separated it.

MR. MATTIE: Initially, we had it included. That was a peer review input that we

decided to go with, because we agreed.

MS. GHOSH: Yeah, it doesn't make -- yeah. So we took that out.

MEMBER RYAN: Thank you.

MS. GHOSH: Okay. So this is just a conceptual, a cartoon really of what kind of results are we getting. On the left side you have what was done for the best estimate.

You can see that for the MACCS portion, the weather was included as part of -- the weather uncertainty was included as part of the calculations, and what we report is in essence the mean consequence from, you know, all of the different possibilities from the weather bins. What we're adding --

MEMBER BLEY: Can I ask even about that. I mean (cough) in the consequence assessment was really the weather over the data set we got, which is not all that big over the range of what might possibly happen. How many years are in that data set?

MS. GHOSH: Oh, the weather, yeah.

MEMBER BLEY: Twenty years, thirty years, something like that?

MS. GHOSH: No. This issue comes up a lot.

MEMBER BLEY: It does, but if you're going

to do uncertainty, maybe you ought to consider that we could be outside that range. Every year, we get some new weather that's outside the range. So if one looks more broadly, you don't have the data, but at least you can, from anecdotal reports and records, see where, whether weather's done in the past and at least make comments on how it might.

MS. GHOSH: Yeah. What's typical in MACCS applications, you use a full year of weather data.

MEMBER BLEY: It's one year.

MS. GHOSH: One year, and you make enough weather bins --

MEMBER BLEY: I forgot. I thought it was more than that.

MS. GHOSH: No, one year, and you make enough weather bins to have a statistically significant representation of all the different combinations of, you know, of what you could see, and Nate can elaborate.

But based on past studies, if you choose one year versus another, and you know, from the early SAMA analyses that were submitted for license renewal, the staff had asked them, had requests for additional information, asking, you know, what's the difference if you use one year versus another?

And it turns out that it doesn't make a huge difference whether you use -- which year of data you use, as long as you use a complete data set that has all the seasons and so on.

MEMBER STETKAR: Tina, suppose you use 50 years and looked at the variability over that 50-year data set, not Year 1 versus Year 2?

CONSULTANT WALLIS: That's right. That varies a lot.

MR. BIXLER: I would say -- this is Nate Bixler from Sandia Labs and the PI for WINMACCS, MACCS-2 development. Statistically, how many years of weather data you would want or need depends on what kind of statistic you're looking at out of the results.

If you're looking at mean results, what we found is that they depend about ten percent above or ten percent below a typical value, depending on which year of data you use. So a range of plus or minus ten percent. If you were looking at a 95th or 99th percentile result, then of course it's going to matter more which year's worth of data you're looking at.

So since we in the SOARCA analysis we focused on mean results, it wasn't particularly result which result we were -- which year of data we were

looking at.

MS. GHOSH: So the graphs on the right now show the contribution of the uncertainty analysis. Now you get a family of curves rather that one curve, and the spread of curves gives you some indication of the contribution of the epistemic uncertainty to the problem.

So we go to the next slide. I just wanted to quickly list who we have on the team, because I'm next going to go into kind of our process for choosing the uncertain parameters and the distributions, and we relied a lot on expert judgment.

So just so you know who those experts were on the team, one of them is sitting right next to me. We had several people who have a lot of experience with MELCOR, and just on severe accidents in general. I won't read all the names. Randy Gauntt you already heard from this morning. Mark Leonard, sitting next to me and there are others from Sandia.

Ed Fuller, who more recently joined us, our group here at the NRC. Similarly for MACCS, we have folks who have been working with the MACCS code and consequence analysis for many, many years. You have uncertainty analysis methodology folks like Patrick and myself, and for the health effects

modeling portion, and in particular the latent health effects, because that is what drives the risk.

As I said, even with all our variations, we haven't seen any early fatalities. We had some help from Keith Eckerman at Oak Ridge National Laboratory, who had co-authored Federal Guidance Report 13, which has an uncertainty approach for health effects modeling in there, which he helped us implement for the SOARCA, both for the SOARCA main study as well as for the uncertainty analysis.

So the next slide. As I said, we relied heavily on our experts to come up with what should be these key input parameters that we're looking at, and to come up with distributions for them.

Our approach was based on a formalized process, but we didn't actually employ a formal FERC process, and you know, the experts were informed not just by their general knowledge of all the experimental data that's out there and their years of working with MELCOR and MACCS, but also a lot of the work that had been done specifically for SOARCA.

So for example, we had preliminary insights from the sensitivity studies, looking at single issue for SOARCA and so on. So all of this knowledge was brought to bear on coming up with kind

of a list of parameters.

Then we also shared that list with certainly our SOARCA peer review committee, but also with other folks at Sandia and NRC outside of the project, who have expertise in these areas. So to get kind of a check on are we capturing important things. And if we go to the next slide --

MEMBER STETKAR: Tina, before you leave this, the last bullet says it's based on the formalized PIRT process, but you said you didn't really do that.

MS. GHOSH: Yeah.

MEMBER STETKAR: What you're doing is an expert elicitation process, and indeed the agency has written, you know, a reasonable amount of guidance on how to do that. I'm curious why you didn't follow that approach. It's a little bit different than PIRT.

MEMBER BLEY: Or at least some of the structured part, to control biases and make sure you're getting all the information on the table. Now maybe you did some of that, but when you just say it was informal and what you just said. Again, you're not doing that.

There's a lot of sources of bias, even with the best experts, that if you don't control that

and force people to think beyond the place they start, you lose a lot of confidence in what comes out of such a thing. So if you can tell us any more about what you did and how you did it, it would be helpful.

MS. GHOSH: Yeah. I think, you know, to some extent, the uncertainty experts worked hand in hand with the subject matter experts, to kind of draw out those issues, to kind of refine, in particular you know, the distributions.

But because a lot of this we're doing for the first time, in terms of the scope of what we're looking at, and you know, recognizing that our scope doesn't include everything, but it actually includes quite a lot that we're looking at, and we'll get to that a little bit later, about 23 independent MELCOR parameters, the first iteration is almost an investigation into, you know, what could be the possible effects of the uncertainties and the precise distributions.

For a lot of them, we didn't have a lot of data to go on. So at this stage, you know, so I say it's an informal PIRT process. You could call it also informal expert elicitation. I think the types of things that are prescribed to do in a formal process we did, but much more informally, and perhaps not

documented in a way that you would for a formal process.

But I think the kinds of thought processes you're thinking of, I mean we tried to get at. So for example, after Fukushima happened, you know, we certainly revisited what we were looking at, and even though we weren't changing our scenario based on Fukushima, you know, just trying to come up with hazardous accident provided insights that we should now be looking at other things.

In fact, we added the head flange parameters after the Fukushima accident. So we had some group discussions to go through these, you know, thought processes. But it wasn't formal. It was more informal.

MR. MATTIE: I just want to say two things to that, and one is that there's a lot in that bullet, you know. On one hand we say we didn't follow a formalized PIRT, and it goes back an earlier question about we didn't include everything. We know we didn't.

And then the second part is, I don't want to under-value the effort that we actually did put into the parameter selection and distributions. In terms of at least the uncertainty team, the entire

uncertainty team has been through an expert elicitation in a formalized PIRT process many, you know, several times at least before most of us.

So we used that process, although not documented. That's the key here. We didn't document the process, which is what you would really do in a PIRT, you know. Part of it is the transparency and the confidence-building and the distributions and the full evaluation.

Many in most, if not all, though I can't speak for all of them, the subject matter experts have also been through formal expert elicitation and PIRT processes, and they employed those techniques when choosing what they believed to be most important parameters and their distributions and technical basis.

And thirdly, we socialized this, and that's a slight understatement, with the peer review panel, and got extensive comments back, which we then, to our best ability, addressed and I believe we addressed them all to their satisfaction. We had a resolution meeting in January of this year, in terms of those parameters.

And I said thirdly and lastly, but in reality there's one more item. The last item is that

the consequence analysis in WINMACCS especially and MACCS-2 was developed as a probabilistic code to handle uncertainty, and has quite an extensive technical basis for the distributions that we're using in that analysis.

So the majority of our expert elicitation processes focused on the MELCOR side more than the MACCS side. So there's a fairly robust, although not extensive treatment of uncertainty, not a complete treatment. That's what the PIRT-like process but not a PIRT involves.

MS. GHOSH: And if we go to the next slide, you know, we tried to focus on confirming that the parameters representations we came up with appropriately reflect the key sources of uncertainty, and that we have a reasonable and defendable technical basis for them.

So in that Chapter 4, the draft Chapter 4 that we shared with the Committee before this meeting, is our attempt to capture all the thinking that went into, you know, why we think the parameter is important, and you know, basis for the distribution with references and so on.

So that's kind of where we tried to summarize and capture, you know, the thinking that

went into the parameters. If you go to the next slide, and just an overall philosophical point. Again, there are many more parameters than the ones that we included in this uncertainty study.

But the attempt was to obtain contribution from uncertainty across the spectrum of phenomena if you look at different stages of accident progression, and get kind of balanced depth and breadth of coverage, rather than digging very deeply into one area and neglecting another area.

And same as the second bullet. Yeah, that's okay. We can go ahead to the next slide.

MEMBER ADBEL-KHALIK: With regard to the sequence issue, the SRV stochastic failure rate, do you plan to or have you looked at the opposite problem of set point drift? It is certainly not unheard-of for plants to send their SRVs for as-found testing after each outage, and most, if not all SRVs would fail the plus or minus three percent acceptance criteria.

If you look at the opposite problem, I assume that the consequences would be more severe.

MR. LEONARD: For example, if the drift were upward.

MEMBER ADBEL-KHALIK: It's always upward

when I mean that failing the same point.

MR. LEONARD: It moves -- first of all, the absolute value at which, the pressure at which the valve lifts is not the issue. It's whether it opens and stays open or not.

So if it drifts upward a few, even a few tens of psi by the time it opens, then it's not going to affect the accident progression. And if the lead SRV is the one that drifts --

MEMBER ADBEL-KHALIK: If you know that for sure.

MR. LEONARD: Yes, in the sense that we see the effect of --

MEMBER ADBEL-KHALIK: That a ten percent set point drift.

(Simultaneous speaking.)

MR. LEONARD: I can say yes in the sense that as you see the effect of drift in representative, even in long-term station blackout calculations, where because the delta P that is the basis for whether the valve opens is reactor system pressure to drywell, not to the environment, over a long-term blackout the drywell containment pressure begins to slowly increase.

That increase in the base pressure can be

observed at the set point, the absolute value of the reactor vessel pressure at which the relief valve actually lifts.

So if you were to watch the cycles in pressure, over time they actually begin to drift upward. Not because of anything in the set point drift, but because the base pressure is affected.

MEMBER ADBEL-KHALIK: Right. But my concern is the actual set point drift due to corrosion bonding of the valves.

MR. LEONARD: If the lead SRV set point were to drift upward beyond the set point for the next SRV, the other one simply picks up and moves.

MEMBER ADBEL-KHALIK: But if all of them drift high, have you looked at the LERs for plants that send their SRVs for testing?

MR. LEONARD: The effect of drifting all the SRVs upward no, has not been examined.

MEMBER ADBEL-KHALIK: Actually, I've looked at the LER database.

MR. LEONARD: I'm just struggling to understand how even a few tens of psi upward drift would affect the accident progression itself. Certainly, it would affect the signature, in the sense that the absolute value of pressure at which the lift

would occur changes.

But if it's still going to cycle approximately at the same range, and if the rest of the events in terms of the controlling phenomena are unaffected, I'm just trying to think what effect would a -- instead of increasing it from 2250 it goes to 2270.

MEMBER ADBEL-KHALIK: It would likely be a lot higher than 1270. We're talking about a BWR SRV.

MR. MATTIE: It could be a qualitative answer to that if you know what your, you know, a ten percent of 20 percent set point drift and you look at the pressure curve on, let's say, an SRV stochastic failure that has, you know, not occurred.

I mean what kind of temperatures would we have to get before we get something else occur, like a thermal failure or a creep failure. You could qualitatively say at what point does the drift not matter?

MR. LEONARD: Well, except for the first few minutes, only one valve cycles anyway. So you would have to drift all 13 for it to significantly adversely affect the pressure history. Eventually, it would get to whatever the lowest one would be.

MEMBER ADBEL-KHALIK: I mean it is not unheard-of that most, if not all SRVs --

MR. LEONARD: Would drift upwards.

MEMBER ADBEL-KHALIK: For a plant would drift up beyond the plus three percent acceptance criteria.

MR. LEONARD: Okay.

MS. GHOSH: I guess we'll look into that further. This slide lists the MELCOR zirc parameters. I want to point out that some of the bullets actually contain multiple parameters. I think we're looking at 23 independent parameters all together, and the italicized headings are sort of the categories or the phase of the accident progression that we're considering.

So we have sequence issue, the battery duration, the SRV stochastic failure rate, and when stochastic failure happens, whether when it happens, may or may not bring into play if SRVs is going to thermally seize open instead. If the SRV thermally seizes open, then we also sample an open area fraction for the SRV.

We have in-vessel accident progression parameters. I don't need to read all of them. We list them here. There's ex-vessel accident

progression parameters and that's the debris lateral relocation time constants. Then we have some containment and building behavior parameters.

CONSULTANT WALLIS: Where does vessel failure fit into this?

MS. GHOSH: Sorry?

CONSULTANT WALLIS: Where does the failure of the vessel fit into this? I see things happening inside and outside, but I don't see a vessel failure somewhere in here.

MR. MATTIE: Well, I would say Randy could probably better answer that. But the fuel failure criterion, the zircaloy melt breakout temperature, you know, anything in-vessel, is effectively going to control the timing the timing that the core drops into the lower head.

CONSULTANT WALLIS: It takes some time to fill the vessel.

MR. MATTIE: It does take some time, yes.

CONSULTANT WALLIS: So how do you represent that?

MS. GHOSH: That's modeled.

MALE PARTICIPANT: How do you represent --

CONSULTANT WALLIS: Well, that's not in the list here.

MR. LEONARD: As you mentioned earlier, that you know, we've had several comments in the past about the absence of either penetration failure. Right now we're representing global creep rupture of the lower head as the failure mechanism. That's what's calculated, and either the initial opening size that Joy had mentioned earlier, or the possibility of penetration failure --

CONSULTANT WALLIS: What do you use for your uncertain parameter in terms of this creep failure? What's your uncertain parameter that you're going to put in your code, to characterize how long it takes to fail?

MR. LEONARD: The Larson-Miller parameter is the parameter in this case, but it is not on the list of the uncertain variables that we're sampling.

CONSULTANT WALLIS: How do you put it into your analysis? How do you put vessel failure into the uncertainty analysis?

MR. LEONARD: The criteria for lower head failure as a particular phenomenon is not sampled as an uncertainty.

CONSULTANT WALLIS: It has to happen in order for this whole thing to --

MR. LEONARD: We could sample from a

distribution of the Larson-Miller parameter, and have chosen not to, and the rationale for it --

MEMBER REMPE: Or the temperatures, because you've got to have a life fraction rule. There's a lot of things that go into it.

MR. LEONARD: That's right. We considered it in the early selection list, and it fell to the second tier, primarily because when we watched the behavior of the creep model, we raised from a damage index of zero to a damage index of one so quickly that --

CONSULTANT WALLIS: In the vessel.

MR. LEONARD: On the lower head.

MEMBER REMPE: That's due to other things associated with the melt core progression, that you wait until the dryout has occurred before you start to heat up.

MR. LEONARD: Exactly right. So but given that as a generic boundary condition, a phenomenological boundary condition to the way the accident progression takes place, that the thermal challenge to the lower head does not occur until all the water is evaporated, and the debris begins to slowly heat up, with that signature as part of the damage progression.

CONSULTANT WALLIS: Will slowly heat up?

MR. LEONARD: Yes.

CONSULTANT WALLIS: So how hot does it have to get before it does anything?

MR. LEONARD: That's where roughly the melting point of stainless steel comes in. The debris temperature reaches approximately 1,700 K at the time we typically see creep rupture occur in the vessel at low pressure.

MEMBER REMPE: You could, I mean you said you have a stated area that you're assuming, and you could take that area and bury it to accommodate a penetration failure up to whatever that area is, something like that.

MR. LEONARD: That's right.

MEMBER REMPE: But there's some things that could be done. Timing if it was a penetration would occur a lot earlier probably than what you're doing with vessel failure. Again, with the expert elicitation theme that wasn't important. I guess it doesn't matter, but I guess I'm not sure I would have come to the same conclusion.

It seems like some of your peer reviewers did say that it was important parameter.

MR. LEONARD: Yeah. We had comments

several times about penetration, and there are a couple of ways, at least two ways that could be accommodated. They're in the area as a distribution, but using the creep criterion is the trigger for actually invoking failure.

I mean we could put those two things together and perhaps come up with a way that at least influenced the debris drain rate to the containment, and right now there's nothing that influences that. That's true.

CONSULTANT WALLIS: So it happens very quickly. It's sudden, an all or nothing thing? I mean I just think TMI got a blister in its vessel, but it didn't fail.

MR. LEONARD: Right.

CONSULTANT WALLIS: So something stopped it from failing.

MR. LEONARD: Right.

CONSULTANT WALLIS: And you don't seem to have anything in here like that. It has to get hot enough for long enough for something to happen.

MR. GAUNTT: Maybe I can add a few words to that. Randy Gauntt, and I guess our understanding of TMI, we don't have a mechanistic explanation for why the head didn't fail, but there was always water

down in the head. However it happened, it was coolable.

When we take that forward to modeling in the BWR, we have found the 1(d) dryout criteria for debris to be unrealistically aggressive, and kind of taking our cue from TMI, there was water in the lower head of TMI. That was coolable. We steer the code such that you essentially have to dry out the lower head before it begins to really threaten the lower head.

I think also in this calculation, it's going to be at low pressure by the time you get to relocation. So in fact it's not even a creep analysis. What you'll see is node by node, starting from the inside. You're basically melting your way through the vessel until there's no strength left. So it's not really a --

CONSULTANT WALLIS: That sounds good. So why don't you -- can't you model that and figure out how long it takes and put some uncertainty on it?

MR. GAUNTT: We would do that. We would --

CONSULTANT WALLIS: It seems to me an important phenomenon in this whole scenario.

MR. LEONARD: What Randy just described is

modeled and is calculated. That's the baseline approach to the model.

CONSULTANT WALLIS: It should be some line here which says something about progression of failure of lower vessel or something.

MR. LEONARD: What we're saying, though, is the reason it's not on this list is the parameters that govern that model are not treated as uncertain variables.

CONSULTANT WALLIS: Just use a number. You don't vary it at all. There's no uncertainty about when the vessel fails.

MR. LEONARD: In this case, there's no uncertainty about the parameters that calculate when the lower head fails, that's true.

MR. GAUNTT: Our knob on that, were we to put it in, I think would be some kind of heat transfer coefficient from the debris to the head.

MEMBER REMPE: Like a gap.

MR. GAUNTT: Would be --

MEMBER REMPE: That's been observed in some of the --

(Simultaneous speaking.)

MR. GAUNTT: Gap resistance or something, and I'm not sure, but don't we just fail a whole ring

when we --

MR. LEONARD: It fails on a ring by ring basis.

MR. GAUNTT: Fails on a ring by ring basis. So the area turns out to be the --

MR. LEONARD: Effective area of the ring.

MR. GAUNTT: Area of the ring, yeah.

MR. LEONARD: It's reasonably a good start.

MR. GAUNTT: It's a big failure.

MR. LEONARD: It's noted. I mean both the comment from the peer review as well as what we've heard today, that some means of capturing the effect of smaller, slower drain -- smaller hold, slower drain rates --

MEMBER REMPE: The earlier --

(Simultaneous speaking.)

MR. LEONARD: We'll have to consider how we'll do that. This surface coefficient between debris in the lower head is already a parameter that we have some control over. It's the way in which we ensure that the lower head dries out before you begin to thermally attack the vessel. I mean that's one of the parameters that controls that. Okay.

MS. GHOSH: Okay, and then we have fission

product release transfer and deposition parameters. So go to the next slide. There are about 20 independent uncertain parameters in the MACCS portion, but there's actually hundreds of individual parameters, because a lot of these are large groups of parameters. For example, a lot of the latent health effects parameters, there's a lot of them. They're radionuclide-specific.

But here again, we tried to look at all of the different areas that MACCS models. So the atmospheric transfer and deposition, the emergency planning and response, as well as the health effects.

MEMBER RYAN: Just a quick question, Tina, on the cancer mortality risk coefficient. Are you correcting for cancers cured versus cancers developed? What is the cancer mortality risk coefficient telling me?

MS. GHOSH: Yes. What is it telling you?

MEMBER RYAN: Yeah. What does it do? How do you define it?

MR. MATTIE: Defer that one to Nate.

MS. GHOSH: How deadly?

MR. BIXLER: The cancer risk coefficient is just giving a dose to an organ, which the risk of getting a fatal cancer from that dose to the organ.

MEMBER RYAN: Oh, I see, and it's one of the several things you kind of --

(Simultaneous speaking.)

MEMBER RYAN: Sorry. I misunderstood the topic. Thank you.

MS. GHOSH: I am going to actually skip the next couple of slides, because Mark talked extensively about SRVs, and we're going to come back to some, you know, initial insights that we're seeing.

We can go to Slide 17. We don't need to spend a lot of time on this either. This is more just a cartoon of the phenomenology that the parameters that we're varying is meant to capture.

In essence, they're time at temperature models for different phenomena, and you know, the parameters are uncertain. But they're meant to capture the uncertainties in the melt progression stages.

So if we go now to Slide 18, okay. This is another example. Mark talked a little bit about this before. Based on the PHEBUS program findings, you know, cesium, there's a lot of cesium molybdate was observed, whereas traditionally cesium hydroxide is the species that was assumed in past analyses.

So we've created five bins, which we show

on the next slide, and the reason they're important is for example, the vapor pressure varies, depending on which species that the cesium is in.

So we're looking at alternate speciation for both iodine and cesium, and we based the fractions for the bins that you'll see on the next slide on the different PHEBUS tests that are now available, that weren't all available at the time the main SOARCA study was completed.

So if we go to the next slide, you see what our combinations are. I know the writing is kind of small, but I believe you have hard copies in front of you. Combination 5 is what we are taking as most likely -- of all of the cesium and cesium molybdate, and a small fraction of gaseous iodine. But most of the iodine is still in the cesium iodide form.

And for the other bins, we have alternate samplings for cesium hydroxide in different fractions of gaseous iodine, though still small. So the cesium, I think in essence it's less than ten percent as cesium iodide. So the mass balance is retained, and what's left over, over 90 percent of the cesium is split according to these fractions into either all cesium hydroxide, all cesium molybdate or a portion of both.

CONSULTANT WALLIS: You have extraordinary precision in combinations under 3.

MALE PARTICIPANT: Four and five.

MS. GHOSH: Yes. These were based on the PHEBUS experiments. But you're right, that we shouldn't convey false precision, because of course there's a lot of uncertainty around these fractions. That's our interpretation of what PHEBUS is telling us about --

(Simultaneous speaking.)

MS. GHOSH: Yeah, right. Okay. All right, next slide. You know, before I get into discussing the preliminary analyses we've done and what we're seeing, I want to stress again, and we said this a couple of times, that uncertainty analysis is an iterative process.

You know, we've learned some things with the first couple of iterations, and you know, we're revisiting the things that you see in the bottom box, which says "iterate." We want to make sure that we're within the range of validity of what MELCOR is modeling, that the analysis stable.

And then one of the early issues we identified is that we think we're actually looking at two different scenarios, which is whether or not MSL

creep rupture happens, and that really changes, you know, the outcome. And then just checking, you know, whether we've uncovered any bugs or modeling errors.

So we're in the midst of this iterative process, and what I'm going to go through is just give you a status of, you know, what we've done so far and give you some examples of interesting insights that we're getting, and then kind of end with where we're going.

So the next slide. The preliminary MELCOR analyses we've done so far, we've done three cases. So the first case, which we'll call the combined scenario, uses all of the distributions as they're laid out in Chapter 4 of what we gave you from our draft report.

And what we found with that is that, you know, both SRV stochastic and SRV thermal failure parameters were varied, and Mark mentioned that, you know, the stochastic failure rate database is perhaps modeling a different situation than we are expecting to occur in the long-term station blackout scenario, and that that stochastic failure rate, actually most of it lies to the left of, you know, the SOARCA best estimate value that was used.

In that first case that we did, we ended

seeing that an SRV thermal failure actually happens most of the time, and as we were sampling that thermal failure open area as a first cut, we said okay, it's just uniform from zero to one.

But we ended up observing a lot of main steam line creep failures when we have mostly SRV thermal failures happening, and we're varying that open area anywhere from almost fully closed all the way to fully open.

So as a second -- and then we thought, you know, we're really modeling two scenarios here. The best estimate scenario was that we have a relatively early stochastic SRV failure. In this UA case now, we're modeling main steam line creep rupture. It's really a different scenario than what we did for the best estimate.

So the second case we did, kind of as a sensitivity, we had the thought well let's keep the SRV thermal seizure open area fixed at one, which is fully open, to see what -- we're not going to get as much main steam line creep rupture, to see what we get there.

Then the third case, we fixed the SRV stochastic failure rate to be the same as we had in the SOARCA main study, to kind of look at the

uncertainty around, you know, what may be closest to the SOARCA main study.

Then a quick note. We haven't done as much analyses with the MACCS so far. But what we did so far is we used the source terms from that first case, from the combined scenario case using the LNT model, and treating both the aleatory and epistemic uncertainty, as I mentioned before.

So if we go to the next slide. This is a very high level broad-brush kind of qualitative description of summarizing, you know, what we've seen so far. So the cesium release timings are very similar to the SOARCA --

CONSULTANT WALLIS: Excuse me. These are based on some kind of Monte Carlo sampling, are they?

MS. GHOSH: Yes.

CONSULTANT WALLIS: So how many runs did you do?

MS. GHOSH: For each one, we attempted 300.

CONSULTANT WALLIS: 300 runs.

MS. GHOSH: Yeah. So we had three cases of 300 each. In every case, we had a handful of either failed runs or MELCOR runs that were going to take so long that we didn't want to wait for them.

But there are quite a few runs that we've done at this point.

So the time, the cesium release timing is improtant, and the reason I'm concentrating on cesium is because that is the dominant radionuclide for long-term health effects, which is pretty much the only health effects that we're seeing. So we focused a lot on cesium.

The magnitude of release at 48 hours is generally slightly higher than we saw in the best estimate, and again, this is because we were seeing a lot more main steam line creep rupture cases. But it's still far, far, far below the siting study results, just to give you some context.

CONSULTANT WALLIS: You say the magnitude. You mean the mean release, or what do you mean by that, magnitude of the release?

MS. GHOSH: Yeah. Well, you can pick your figure of merit in any of them. Mean, median, 95th percentile.

CONSULTANT WALLIS: All of them. All the choices are generally higher than the SOARCA estimate.

MS. GHOSH: Right, right.

MR. LEONARD: If you remember back to the table that I presented this morning on the SRV topic,

where we had one case that went to main steam line creep rupture, if you look at the release fractions at 48 hours for that case, it was a factor of eight or ten higher.

That's what we're really referring to here, is that number. So it's consistent with the early sensitivities in that sense.

MR. MATTIE: In that case, there's the distribution results, many of which look like the one that Mark just described, the main steam line creep rupture that was presented in that SOARCA best estimate as an alternative sensitivity. Many that look like thermal SRV, and a handful that look like stochastic SRV.

MS. GHOSH: For the MACCS results, again, we only did the first combined case so far, but essentially the distribution of risk results for latent cancer fatality risk looks similar to the best estimate, and the early fatality risk is still essentially zero.

So if we go to the next slide, we are getting interesting phenomenological insights from now, you know, kind of looking at all the results and kind of unraveling what's driving what, when and so on, and we can confirm a lot of the phenomenology that

we had already seen in the SOARCA, the main SOARCA study.

And as I mentioned before, nothing that invalidates what was done in the SOARCA main study. In particular, there's a strong dependence between the number of SRV cycles before failure to reclose, and the probability of thermal SRV failure and/or main steam line creep rupture.

And you know, Mark had already shown this with the sensitivity analyses that he had done, that's documented in the Peach Bottom report. We see that there's a dependence between the area fraction of the SRV thermal failure and the current MSL creep rupture, again something that we had already seen in the sensitivity analyses, and then the chimney effect when the railroad doors fail open is also observed.

CONSULTANT WALLIS: This is where you had to modify MELCOR to put in another halfway or something? Because the chimney effect isn't modeled in MELCOR.

MR. LEONARD: This is, the chimney effect is referring to a flow path through the reactor building after the containment fails. In this case, the railroad doors are the main equipment access doors at grade level in the building, which I don't believe

is on the diagram we showed earlier today.

But because the release path from containment enters the building at a low elevation, the residence time and attenuation of any released aerosol within the building depends a lot upon the extent to which there's an air flow through the building.

So if the building remains bottled up and only opens by the blowout panels at the top of the building, there's still enough circulation and residence time in the building that attenuation is reasonably good. I mean a DF of two or three is better than zero, better than one.

What we find more often, though, is that the hydrogen deflagrations that occur early create a differential pressure that blow those doors open. The question is when that happens, and they're enormous doors, because they're double-hinged doors designed to allow a railroad car to enter the building.

So the question is when they fail, what kind of effective area do you have, and there are two doors in parallel, with a chamber between them. So this parameter is trying to take into account uncertainty on the effective flow area through those doors to the environment, which creates the inlet air

flow that drives that chimney flow up to the upper elevation of the building, and completely destroys the residence time.

So a large area in principle shortens the residence time in aerosols --

CONSULTANT WALLIS: The chimney is modeled without modifying MELCOR?

MR. LEONARD: That's right. I mean the buoyancy from the low elevation to the top is part of the natural calculation. That's right.

MEMBER ARMIJO: Wouldn't that be an easy thing to spot from the Fukushima events, whether the doors were blown open or not? They're accessible.

MR. LEONARD: Again, the damaging hydrogen combustion at Fukushima was high, not low, and so the destruction of the building is primarily you see at the top.

MEMBER ARMIJO: But not in Unit 4. The bottom of that building was blown out.

MR. LEONARD: That's right, that's right, that's right. So the effects, the qualitative of the effects are similar to what we see in these cases, where the building walls remain intact because they're concrete at that lower elevation, and the doors open.

I mean doors to everything. The railroad

doors, the doors to the stairwells, all of those, and each one of those doorways in the building is explicitly modeled in the MELCOR model, with a known differential pressure that has to be generated for them to open.

So it changes the flow paths, plural, through the building when that happens. The most important one is whether you get this inflow of air at grade level that allows you to create the chimney effect through the equipment hatch that passes through all elevations of the building.

That's why it seems to show up. It's a little surprising that it was as important as it is, but there's an explanation for it.

MR. MATTIE: That bullet doesn't rank its importance. It's just it was noted as important.

MS. GHOSH: So the next slide, Slide 25, this is just a quick snapshot, again, of analyses in progress, of what our step-wise rank regression is showing as important for both the iodine and cesium releases at 48 hours.

The step, the rank correlation coefficient is just a very rough, rough, rough measure of how much of the variation in output can be explained by different input parameters that we're varying.

The SRV open area fraction for thermal seizure, not surprisingly, shows up as very important, because that is a controlling feature of whether or not you have MSL creep rupture.

CONSULTANT WALLIS: Well isn't it likely to be fully open?

MS. GHOSH: Yeah, and we need -- this is one of the issues that we are revisiting now, and actually we want to have some discussion with the Committee on, because we are coming back to that. In fact, we talked a little bit already about it earlier today.

MR. MATTIE: In this particular analysis, it's uniform between zero and one.

CONSULTANT WALLIS: That's not likely.

MS. GHOSH: And that's why it's an iterative process. As our first cut, we had assigned a uniform distribution, but now that we're seeing so much main steam line creep rupture, we are revisiting what that distribution should truly be.

MEMBER STETKAR: Tina, way back when we were talking about, you know, expert elicitation or a PIRT process, was that uniform distribution the result of your polling experts on how valves work?

MS. GHOSH: You know, for some of the

parameters, again because we knew this was going to be an iterative process of the first cut, we assigned uniform distributions, you know, between the bounds that we thought were likely and you know, with the thought that depending on --

MEMBER STETKAR: Well, let me stop you there. Did you do that for everything?

MR. MATTIE: Can I answer this?

MEMBER STETKAR: As sort of the baseline state of knowledge?

MR. MATTIE: We did not have an expert elicitation for this particular parameter. We used the subject matter experts in MELCOR modeling, and because we didn't have an expert elicitation, we could only go from zero to one, because we didn't have any basis for doing something different.

MEMBER STETKAR: Well, let me then go. Does that mean that except for the MELCOR modeling, everything else is characterized by a uniform distribution between zero and one?

MS. GHOSH: To answer your question, no. So there are two issues. There's one, you know, what the bound should be and then what should the shape be within those bounds. So for some of the parameters, it was felt that we had enough information that we

thought there was some likely value.

So we, for example, a triangular distribution is what we used, kind of I think for several of the parameters, because we felt that there's more confidence that we knew what would be a more likely value, and that while we recognize that there's uncertainty around that value, we think that it's more likely.

So we picked a mode and then have a triangular distribution around it. For some of the parameters, as a first cut, we assigned the uniform distributions for, you know, the bounds that came up. And another example we'll talk about a little bit later. So this is one of them, the open area fraction for the thermal seizure area was one of them.

Another one was the drywell liner failure area. Just as a first cut, we assigned a uniform distribution. We initially did not think it would be that important. So just to, you know, to throw it into the mix in the combined uncertainty analysis, initially we assigned a uniform distribution, to see, you know, whether it makes a difference and under what conditions and so on.

MR. MATTIE: I want to kind of go back to that. We first started with the subject matter

experts in the different models, MELCOR and MACCS, and they identified the parameters and the modeling areas they thought were important.

The next step was to go in and take the available technical basis of the data that was available, that was at least available to the best of their knowledge, and define distributions.

In some cases, it was well-defined. In many cases for MACCS, there was extensive expert elicitation that had been done previously. There was a number of studies --

CHAIR SHACK: That's probably Corradini.

MR. MATTIE: Is that cutting me off?

(Phone ringing.)

CHAIR SHACK: Mike, is that you?

MEMBER ARMIJO: I guess not.

MEMBER CORRADINI: Yes, this is me.

CHAIR SHACK: Oh. It's a little slow in the take-up.

MEMBER CORRADINI: Sorry.

MR. MATTIE: Okay. We're on the uncertainty analysis.

MEMBER CORRADINI: That's okay.

MR. MATTIE: Slide 25. Okay, just to finish that comment, though, and then where we didn't

have or knowledge of the sufficient technical basis, we proposed distribution values to evaluate.

At that point, we went through the peer review, which greatly improved many of them, and in other areas, we agreed that we didn't know or didn't have a technical basis to do anything different, and evaluated at that point whether we believed that it was sufficient for the first iteration or the iteration of this uncertainty study.

One of the parameters that demonstrates that is the open area fraction on thermal-failed SRV.

CONSULTANT WALLIS: So the railroad door open area is a uniform distribution?

MR. MATTIE: Again, yes.

CONSULTANT WALLIS: It is?

MR. MATTIE: Yes. Another area which we looked --

MS. GHOSH: Yeah. There's two, and in fact --

CONSULTANT WALLIS: It's likely that they'll blow open a crack.

MR. MATTIE: Well, what's interesting is that --

(Laughter.)

MALE PARTICIPANT: It's got bad hinges or

something.

MS. GHOSH: We don't go zero to one, though. The bounds are not zero to one. They're less than that, and also we're sampling the inner door area as well as the outer door area, and there's a set of--

MR. MATTIE: There's two doors, but there's -- I don't want to steal all of the Tina's thunder, but we were working on this for several months, and it's always exciting to share your results. But for instance in that parameter, we found that it wasn't actually the open area fraction that was important. It was whether they're open or closed.

Just the fact that you have that chimney effect is the dominant effect. It doesn't really matter how big the opening is, as long as you get some positive flow through there. In this case, the distributions sufficiently tell us what we need to know.

But for SRV open area fraction when we have a thermal failure SRV, we find that it is extremely sensitive. Tina will get to that, and we need to revisit that because of its importance, that you know, the basis for that distribution is probably not sufficient.

MEMBER BLEY: It's not a throttle valve.

It's like a gate valve. When it's ten percent open, there's still flow. So it's not surprising that you found that.

MR. MATTIE: Fair enough.

MEMBER BLEY: I'm not very surprised.

MR. MATTIE: Fair enough.

MS. GHOSH: So the chemical form I'll talk about too. SRV stochastic failure rate we already discussed, and then it turns out the dynamic agglomeration and shape factors also shows up as important. We talk a little bit about that later.

So Slide 26. Now here we have a scatter plot to show what is the influence of essentially the SRV stochastic failure rate, you know, as the distributions that are laid out in the Chapter 4 that you all have, on whether or not SRV thermal failure happens, and then SRV thermal failure with main steam line creep rupture.

I think, as you can see from this case, almost all of the time in this first case, you're getting SRV thermal failure. It's only for the very high sample values of the stochastic failure rate, where you have SRV stochastic failure.

CONSULTANT WALLIS: I don't understand the axis here. It says "influence of number of cycles,"

and I see 10 to the minus 4. That's not a cycle.

MS. GHOSH: Oh yeah. The cycle, right. It's a one over. So the number of cycles is one over the failure rate.

MR. LEONARD: This is failure rate.

CONSULTANT WALLIS: So you're talking about a million cycles at the bottom end?

MS. GHOSH: At the bottom end?

MR. MATTIE: Yes.

MS. GHOSH: Yes. At the very lower end, yes.

MR. MATTIE: That's what that means.

MS. GHOSH: Right. So 270 was the best estimate value, and as you can see, right now we're taking this lambda distribution from the NUREG/CR report that Mark mentioned, and the best estimate value is fairly high on this distribution of failure rate.

It's one of the issues we're struggling with, that you know, we have this sense that this failure rate distribution isn't completely representative of the situation that we're modeling in the long-term station blackout, but then what else do we use to come up with a distribution for this?

CONSULTANT WALLIS: So this tail is

extrapolated in some way. We shouldn't take the tail seriously, should we, down in the bottom left-hand corner?

MR. LEONARD: This is a beta distribution.

MEMBER STETKAR: It's a beta distribution.

CONSULTANT WALLIS: Extrapolated to infinity.

MR. LEONARD: Well, it's a beta -- there's a non-informative prior, and then they updated it with two failures --

(Simultaneous speaking.)

MR. LEONARD: Tina made the right comment here though. You can see that any failure rate less than 10 to the minus 3 certainly always leads to thermal failure first. So the precise number is not important. It doesn't matter. Where we were in the baseline SOARCA analysis, you can see with that triangle is up in the range where stochastic failures happen to occur first.

You don't have to move very away from that, where the thermal mechanism takes over. I mean that's really the lesson learned here, I think.

MS. GHOSH: Right, and then once the thermal mechanism takes over, we're sampling an open area right now, and we're seeing a lot of main steam

line creep failures, since we're sampling the whole range from zero to one, and okay.

So the next graph on Slide 27 now shows what is the dependence of whether or not you get main steam line creep rupture on that sample value of the SRV thermally failed open area, and as you can see, as you have lower open area fractions, you're much more likely to have MSL creep rupture.

The black line is essentially a cumulative exceedance probability for how likely is it that you'll have MSL creep rupture, if you have a value or exceedance probably if you have that value or less. So as you have larger open area fractions, you're becoming less likely to have MSL creep rupture. Right now, we are sampling uniformly from zero to one.

Mike, did you have a question or comment?

MEMBER STETKAR: He's not shy. He'll chime in.

MS. GHOSH: Okay, and for rather large open areas, there's much less chance of having an MSL creep rupture. If we go now to Slide 28, this is actually --

(Phone ringing.)

MS. GHOSH: I think we lost him. This is just an illustration of the chimney effect. I guess

the question earlier of the railroad doors. The refueling bay panels always open, and then depending on whether or not the railroad doors open, you either have or don't have a chimney effect, and the actual open area is not as important as whether or not the railroad doors are open at all, and this is already noted in the best estimate.

MEMBER ARMIJO: There's no effect of rubble in the building reducing the flow? It's you don't consider that, or you just assume everything geometrically is okay?

MR. LEONARD: In this case, the equipment hatch is really big, so unless you somehow totally block, and when I mean really big, I mean it's ten meters square. Unless that's totally blocked somehow, this chimney effect would not be impeded. I mean it's conceivable, but --

MEMBER ARMIJO: But even if this was cracked open, like leaking through the doors. Are you actually -- I misunderstood you. I thought you said if they were just slightly opened.

MR. LEONARD: You're talking about rubble against the railroad doors?

MEMBER ARMIJO: Rubble within the building, and you know, all the flow paths are

blocked.

MR. LEONARD: Again, because the height of the railroad doors is very tall, I mean I don't know the precise number, but they're 20 or 30 feet tall, and the dimension of the equipment hatch that rises from the grade level up to the top of the building is ten meters square, I mean it's big. It would take a lot of debris of any type to somehow impede that flow. So no, it's neglected.

MS. GHOSH: Okay, Slide 29 now shows one of the new insights that has been quite interesting, and initially surprised us, and we couldn't have had this insight with the main SOARCA study, because it has to do with when we have cesium hydroxide as an option, as a chemical form of cesium, whereas in the main study it's all cesium molybdate.

We found in that first case t hat combined case that a lot of the cesium was actually getting held up by the stuff on top of the core, and it turns out that there's a lot of chemisorption that's happening, and this happens at high temperatures, and you end up losing a lot of the cesium hydroxide onto, you know, stuff getting stuck to the steam dryers and the upper reactor pressure vessel.

So this is a surprising result, I would

say, because the conventional wisdom was that cesium hydroxide would lead to higher releases than cesium molybdate, but actually in this case, it turns out when you have high enough temperatures, it gets held up due to chemisorption.

We did talk to subject matter experts in this area, to confirm that this is a real phenomena that's backed up by data and so on. It turns out that it is. It's modeled in MELCOR and it's backed up by real data. So that was a surprising finding.

This scatter plot just shows that depending on what chem form you're sampling, how much of the fraction is getting chemisorbed. So the chemical bin 1 is where all of it is cesium hydroxide, and you see that you get a significant amount of chemisorption on all of the cesium hydroxide.

Chem form 2 and 4 is a half and half, cesium hydroxide and moly, and you get, you still get a significant chemisorption, less than where it's all -- and then you can see with the molybdate it's not getting held up. So anyway. So this was an interesting finding, and the -- yes.

MR. MATTIE: I just want to -- I only want to point out this, the population that we're plotting is only for the creep rupture cases.

MS. GHOSH: Thank you. If we go to the next slide, just to continue on that line, so as Patrick said, that plot was for the main steam line creep rupture conditions. But for other conditions, cesium hydroxide can lead to slight increase in releases, for I think the reasons that the conventional wisdom told us, has a lower vapor pressure rate.

So for the stochastic failure SRV scenario, the cesium hydroxide leads to higher releases. The MSL creep rupture scenario gets held up and because of the chemisorption, it leads to lower releases.

MR. MATTIE: I would say also this is one of the reasons that we devised the specific cases based on different scenarios, because unless you sample it frequently enough, you can't get these statistical correlations. They won't show up.

MS. GHOSH: Okay. The next slide, Slide 31, I think comes to another surprising result I would say, in the analyses that we've done so far. This drywell liner open area, the distribution, and Mark can speak to this better than I can.

But you know, we initially set up bounds in the distribution to kind of see well, what is the

potential effect, and we find that there is in fact quite an interesting effect that this drywell liner open area has.

What we're plotting here is whether or not there's water in the drywell coming from the wet well. So the green circles are that you have no water in the drywell coming in from the wet well, and all of the red circles, and you can see there's a lot of them, is where wet well water is getting drawn into the drywell.

This is certainly a surprising result, and it turns out that because of the way it's modeled in MELCOR, the open area -- the pressure differential that's created once you have the drywell liner open area is pretty large, and it's large enough that you draw water from the wet well into the drywell.

We didn't see this effect in the best estimate, the main SOARCA calculations. So this is kind of a new phenomena that we're looking at. We're still trying to track down now do we think it's physically realistic to be able to open up, for example, a one square meter hole in such a short time period, which is what is, you know, creating this effect.

So this is one of the things, and as part

of our iterative process now we're trying to track down, because it is showing up as more important than we initially had anticipated. I don't know if you want to add anything to that.

MR. LEONARD: This might be a good example where the insight of the initial uncertainty calculations have led us to question the model itself, not just the value of the parameter.

CONSULTANT WALLIS: Question whether or not there's wet well water in the drywell?

MR. LEONARD: No. When we say the liner open area, what this means is when debris contacts the liner, and the liner fails, creating some opening to the environment, the question is well how big is that opening, and in all previous analyses, that value is a number that is opened instantaneously as the effective open area for leakage out of the containment to the reactor building.

When we start looking at larger areas than we had normally considered in the past, what we did not consider is the impact of the assumption in this model, that the total area opens instantaneously, that it isn't equivalent to a leak before break, or a rip, if you will, that gradually grows, at least grows over a time constant that's compatible with the

depressurization time of the containment.

In this case, because it's instantaneous, when you get to the upper ends of the uncertainty range, the drywell blows down very quickly. The wet well cannot keep up, because the passage of gas from the wet well back up into the drywell is blocked by definition by design with water.

It can't keep up, and the only way that differential pressure can equilibrate is to drywell the water, not all out, some of the water out of the wet well through the vent pipes up into the drywell, relieve that differential pressure, and then the water settles back down again.

So that short time frame differential pressure is what's causing this effect of the water, and the reason the water's important is because all of the early volatile fission products that were released from the core were deposited in the pool through the open SRV.

So when the water comes up and evaporates because there's now debris on the floor of the drywell, the activity associated with that water contributes to the source term. So the cesium iodide content in the water becomes part of the environmental release.

CONSULTANT WALLIS: Now what you're saying is if the hole is bigger than a certain amount, you'd get water in the drywell? That doesn't anything about verifying this curve. So the implication is that you're somehow verifying a probabilistic, a probability distribution.

That has nothing to do with it. I don't know where the distribution comes from, but what you're really saying is that if it's bigger than a certain size hole you get water.

MR. LEONARD: We do, under the assumption that the area opens instantaneously. I guess that's the comment that I'm trying to make is --

CONSULTANT WALLIS: But it's sort of misleading to put it on the probabilistic diagram, because there's nothing about probability in your conclusion.

MS. GHOSH: Yeah, I apologize. This is just a way to show that for the sampled values we have on the X axis of the runs that we do --

CONSULTANT WALLIS: But you get a surprising result physically.

MS. GHOSH: Yes.

MR. LEONARD: The probabilities come in in that these results were based on a linear distribution

of an instantaneous open area between .1 and 1.

CONSULTANT WALLIS: That's a questionable conclusion.

MR. LEONARD: So well there's two questionable ones. You know there are some people who have commented that the upper bound may be too large. That's a fair comment. I think what this observation is teaching us more than that is that it isn't the area; it's how quickly does that area open, that the model itself may need to be manipulated before we make the final calculations.

So this has been an interesting insight, because it's not the number that matters; it's the way in which that number is applied to the calculation.

MR. MATTIE: It's just not one of your obvious, you know, value-added. For most people, when they think of an uncertainty study, this is not one of their primary conclusions that, you know, going into it that we're going to actually validate some of the modeling assumptions here.

We find that this one may break down. But I want to make that point, because you make an interesting one. This cumulative probability on this graph, it's just the sample value. So it's the distributions. So I'm plotting just the sample values

over the distribution for this parameter, and then just color-coding when something occurs.

MS. GHOSH: Right. So the color-coding is what we're trying to convey, not the --

MR. MATTIE: Not the probability of whether that actually occurs.

CONSULTANT WALLIS: But shouldn't they lie exactly on the line?

MR. MATTIE: Well, the black line is the theoretical sample, and then the red dots are the Monte Carlo samples, as interpreted by me as the CDF. So my CDF approximation might be slightly off. So I would determine it to my CDF approximation.

MS. GHOSH: Okay. So the next slide, I guess, is just an introduction to the next two slides. We're now showing probabilistic results at this meeting. As I mentioned before, we would like to come back to the Committee with results when we're further along.

But we wanted to just give you some examples of the different behavior that we're seeing, the different profiles based on what we have done so far. So the first example is where we do have a main steam line creep rupture case, with this water influx to the drywell, and the second one will be

chemisorption example.

So if we go to the next slide. For your reference, on Slide 35 are the profiles for the SOARCA best estimate, which I'm not going to talk about. But that's for your reference, you know, for comparison to what we are showing here.

So you can see here, and the legend, just for your information, the SRV OA frac, this yeah. This is down, great. The blue box on the lower left of the graph, that first parameter, we're telling you what the sample values are of some of the key parameters.

That's the open area fraction for thermal SRV failure. The second one is the chem form. In this case, it was all cesium molybdate. The third one is the drywell liner failure area, so in this case it was .73 meters squared. The DASF is the dynamic agglomeration shape factor, which was sampled as 1.17, and the railroad doors were open in this case, as they are in most cases actually.

So this graph is showing essentially where SRV thermal failure happens, which in this case around 12 hours, and then when we have MSL creep rupture following with the cesium release to the drywell, which looks like it's roughly a little bit before 16

hours.

Then the red arrows is just pointing to where we have, we observed this influx of water from the wet well into the drywell, and eventually the drywell failure and cesium released to the environment. There's a lot of data on this curve. I'm not going to go through all of it, each of the curves. But maybe for your information to look at.

MR. MATTIE: I chuckled a little bit when Randy's presentation, and he only had four or five curves on here. So --

(Off record comments.)

MS. GHOSH: Okay. The next one is an example where we see significant cesium chemisorption. This is from the first probabilistic case, and the dark kind of bluish-purple curve shows, you know, what is chemisorbed onto the steam dryers and deposited on the upper RPV head.

So this is from the same probabilistic case as the one on the previous slide, and you can see the cesium releases are significantly lower, because the cesium is trapped on the steam dryers in the upper RPV.

MEMBER ARMIJO: Some things are saying it's failed on here.

MS. GHOSH: Sorry?

MEMBER ARMIJO: This goes from -- one goes from zero to one; the other one zero to .5.

MR. MATTIE: They're on different scales.

MEMBER ARMIJO: You're just trying to calibrate.

MS. GHOSH: Yeah, thank you.

MEMBER BLEY: So Tina, I'm trying to calibrate myself. We're going through a lot of these and we're seeing very great changes to some of these. Your statements earlier that nothing invalidates the SOARCA calculation. I take it to mean that you find no cases that lead to higher consequences than were modeled in the original analysis; is that what it means, because we're seeing quite a bit of variability?

MS. GHOSH: Yeah. You know, I think part of it comes down to what is the explanation for the variability. I mean right now the variability is coming from what we put into the uncertainty analysis, and I think we pointed out there are several parameters that at this point, the team is uncomfortable with what we put into the analysis on the front end.

I think one is the SRV stochastic failure

rate. We're struggling to come up with a better database, to represent a situation that we think is closer to what we're modeling in the long-term station blackout. I think a second example is the drywell liner failure area, and this issue of the instantaneous pressure differential --

MEMBER BLEY: That's a modeling problem.

MS. GHOSH: Right, that we're trying to figure out, you know, how should we make sure that we're not creating some unrealistic, you know, outcome. There's another one that we'll talk about later.

We're investigating some of the dynamic agglomeration shape factor and particle density, some of the related fission product parameters, to make sure that we have not ignored correlations, that we may be incorrectly independently sampling variable values that should really be correlated, and in fact not even just the two that I mentioned.

But there are other parameters in MELCOR that are probably correlated that we're not even sampling, and we're trying to be smarter about how we do that so again, we don't create physically unrealistic results. So with all of the exploration that we've done, we have seen, you know, significant

variation in, for example, the release amounts.

Similar to what Mark showed, you know, is possible with the SRV sensitivity case studies done. So as we mentioned, maybe ten times higher a cesium release. But they're still way, way, way below the siting study results or say earlier studies or the safety rule or anything like that.

And it also comes down to do we think, you know, main steam line rupture case is more likely than the SRV stochastic case, and that's something we're really struggling with, you know, what to say about that. I think right now, you know, if we don't have any more information than we have today, we'll probably present two cases in the uncertainty report.

One for where we keep the SRV stochastic failure rate fixed, which will be more like the best estimate on the uncertainty around the best estimate, and then the second case where we do vary it, and if we don't have a better distribution than we have today, that's really going to be a main steam line creep rupture case.

And if there's no -- I don't think we have any indication that tells us that our best estimate or main SOARCA scenario should have been a main steam line creep rupture case, because that's really driven

by factors of what we put into the inputs, that we're now questioning whether, what is the basis for those distributions.

The two key lines is the stochastic failure rate that we're struggling with, and also using a uniform distribution for that thermal seizure open area, which may not be appropriate.

MEMBER RAY: Remind me. The thermal failure is later than the stochastic?

MR. MATTIE: Yes.

MR. LEONARD: Always.

MEMBER RAY: Yeah, okay. Well, I'm glad you're still struggling with the stochastic failure.

MR. MATTIE: I think in the grand picture, you know, the variation on source term releases that we're looking at is explainable. In the phenomenology, we're discovering some insights that we had previously not seen before. We're still talking about very small fractions released ultimately to the environment, within a range like, as Mark discussed.

And then consequences that are as expected, based on what we saw in the best estimate. So you know, we have seen nothing from the results so far that could, that will, that have exceeded the threshold for the ability for emergency planning and

evacuation to handle the event, such that we're not getting early fatalities and the long-term consequences are substantially like we've seen in the best estimate.

So you can split the hairs really fine, and want to validate the modeling assumptions and the phenomenology that we see. But we've seen nothing extreme, and going back to your original question, that would lead us to believe that something in the best estimate was done incorrectly or wasn't treated.

MS. GHOSH: Yeah, right. And getting back to your consequence piece, I think we can go ahead and skip ahead to Slide 36, which summarizes the MACCS results essentially, which right now we've only done for that first combined case, which is primarily main steam line creep rupture cases.

What we see here is that the health effect risk varies sublinearly with the source term. So even if you have a larger source term, in essence the habitability criterion, if you have enough time to evacuate and done the habitability criterion, is really a controlling feature of the consequences that you see, because people are not allowed to return back and in essence get dosed, until you've met that habitability criterion.

So that kind of -- that's why the eventual consequence results vary very sublinearly with the source terms, even if the source term --

CONSULTANT WALLIS: They don't go up. They go up slower than linearly.

MS. GHOSH: Much slower, much, much slower, yeah.

CONSULTANT WALLIS: It's not the consequence for the people. I mean they're away from home for twice as long.

MS. GHOSH: Right, and yeah, I guess we talked about that earlier.

CONSULTANT WALLIS: I wouldn't be happy not to die, but to have to go away for all that time.

MS. GHOSH: Sure, sure.

MR. MATTIE: We haven't looked at that one yet.

MS. GHOSH: Yeah. We've limited the consequences that we look at to the same ones that are looking at in the SOARCA study, yes. And then, you know, cesium isotopes always dominate for the long-term risk, which is why we spent so much time looking at the cesium part of the source term.

The barium, tellurium and cerium can dominate over iodine for short-term risk, but these

are very, very, very small risks.

MEMBER RAY: Not too important.

MS. GHOSH: Yeah, I think that's all to say about that. We'll have more MACCS results later. I think at this point we've done more with the source term results. But next time we come talk to you, we'll have more phenomenological insights for that part of it.

Just to give you a very high level view, Slide 37 shows kind of the range of consequences that we're seeing out of MACCS, and the best estimate kind of lies, you know, within the range for the 0 to 10 and 0 to 20 miles. Again, this is for the combined case and using LNT. We haven't done the alternate dose model calculations yet.

CHAIR SHACK: As soon as you bring the frequency in, you sort of raise John's question, of what the seismic frequency is. When I went through these, even if I use the old 1488 values, your values seem low to me, and they're only going to get lower compared to, you know, what we think are more modern estimates.

So just where did these frequencies come from? Are they based on 1488, or are they --

MS. GHOSH: The mean core damage

frequency?

CHAIR SHACK: Yeah.

MS. GHOSH: Of 3E to the minus 6? I think Marty is coming to answer.

MR. STUTZKE: The answer is they're basically based on 1488, but you have to account for the seismic failure that gets you into core damage as well. 1488's merely the occurrence of the initiating event, okay.

CHAIR SHACK: Yeah, but sort of it would fail everything. I was kind of equating the two of them.

MR. STUTZKE: No, because components have seismic resonance fragility that's also included in there.

CHAIR SHACK: Okay. That was good enough. Fair enough, and it's still John's question, for the more substantial one.

MS. GHOSH: Okay, so the next slide. I guess based on what we have looked at with the MACCS analyses, and we did a preliminary habitability criterion, sensitivity analysis. By the way, we decided to treat the habitability criterion separately from the integrated uncertainty analysis, maybe partly for policy reasons.

That criterion is set by the states, and from what I understand, if an actual accident were to happen, they might set a different criterion --

CONSULTANT WALLIS: The criterion is not age dependent? I would think that somebody who's in their 70's would be happy to go back, because by the time they die, they'd be 100 years old. But you don't want children to go back. That would make a big difference.

MS. GHOSH: Right, you know, and actually one of our peer reviewers, Jacquelyn Yanch, you know, brought up the issue of were a real accident to occur, you know, are people really going to be forbidden, and when would different groups be willing to go back.

But we have not explored, you know, these issues as part of SOARCA, and we're relying on the state guidance, what the state says that they would use should an accident occur.

However, if a real accident were to occur, the state is free to set a different criterion than what is on paper. So we wanted to look at the sensitivity, because certainly we know this is a controlling feature of the consequence analysis, and we wanted to see what is the effect.

But we kept it outside the integrated

uncertainty analysis, because we didn't want to mix it up with -- mix essentially a policy decision up with the consequence modeling.

MEMBER RYAN: Tina, I'm guessing in answer to Dr. Wallace's question or proposition is that you really haven't dealt with voluntary or involuntary risks. I can go back into an area where I'm going to get a dose because I chose to, rather than -- I'm guessing that's outside the scope of what it does, is that right?

MS. GHOSH: Yeah. I would say that's correct.

MEMBER RYAN: Not that it's not a good question, but it's not -- I'm sensing it's not within your scope at this point.

MS. GHOSH: Right. It is outside the scope of what we've done, yeah. So not surprisingly, the health effect risk is nearly proportional to the habitability criterion, when you assume an LNT model.

If you then look at alternate dose models, and you're looking at alternate habitability criterion, well if the habitability criterion is lower than your dose truncation model, I mean your health effect drops very dramatically.

But again, that's not surprising. But

we're doing the calculation just to document what the effects could be. Just in terms of, you know, a reality check, you know, as we keep mentioning, uncertainty analysis is an iterative process, and we want to make sure that we've put into it makes sense at the other end.

The preliminary MACCS results, we haven't uncovered anything that says that what we've put into the input parameter distributions are unreasonable. So we're not expecting to change anything on that at this point.

If you go to the next slide, so what do we have left to do? Clearly, we're still in the middle of the uncertainty analysis. We still need to finish our regression analyses using the various statistical measures to identify influential parameters.

We're going to continue investigation into single realizations, to confirm phenomena that we think is important, and also validation of results. We want to get some insights on other source or metrics of interest, such as hydrogen production and iodine, for examples.

We need to do the same analyses for the MACCS results, and we have to finish the separate sensitivity analyses that we have planned, the

habitability criterion, which we've started but haven't finished, and also as we mentioned earlier, the timing of the two operator actions that are credited in the unmitigated long-term station blackout. We want to see the effect of varying the timing for those two actions.

On the next slide, we're planning an appendix with a discussion and qualitative comparison with Fukushima, very similar to what was done for the main study, and we are planning at this point to update some of the parameter distributions, and in the next few slides, I'll just talk a little bit about that.

Yeah, we can go to the next slide. The dynamic agglomeration and shape factors and particle density parameters that we have, they do turn out to be important, and when we went back and revisited the ranges that we were sampling, and the fact that we were sampling them independently, we suspect that we are not doing that properly.

So we're revisiting both the ranges that are feasible to sample, and also you know, to kind of think about the correlation effects that might be important. So that's an FYI. When we come back, we should have the updated information.

The distribution for the drywell liner failure flow area, we talked about that already. We're revisiting, you know, the mechanics of how the model is working, and whether it's realistic or not.

Then the third issue, the uniform distribution of that SRV thermally-failed open area, it very much drives the occurrence of main steam line creep rupture, and we are thinking at this point that a uniform distribution from zero to one is not the most realistic.

CONSULTANT WALLIS: For this failure flow area, it would really help if you had a mechanistic analysis, which could give you some sort of a bounding value, instead of just assuming ten times nominal or something.

If you could show that there is some bounding value from, I don't know, First Law of Thermal Dynamics or something, which says it's not going to fail bigger than so much in the worse possible case or something.

MR. LEONARD: In this case, it was done in the sense that the failure will occur at the contact surface between debris in the wall, and we know what, that the debris movement out of the pedestal to the wall is focused in the direction of the doorway. So

there's going to be a natural, if you will, azimuthal spread, and that upper bound was well if it spreads to its full q quadrant dimensions and the rip is roughly that big, you know, a few centimeters big.

CONSULTANT WALLIS: It doesn't climb the wall. It just spreads along.

MR. LEONARD: It can't really climb the wall, at least openings. So that's what the bounding value was, is if that entire quadrant contacts instantaneously with an open area of approximately a few centimeters, what is that azimuthal dimension? What's that effective area? That's where the number came from.

I think the question now is not so much what that number is, but is it at all reasonable to open it, you know, in one instant, or should it somehow more gradually open from a smaller to a larger area?

CONSULTANT WALLIS: This one sounds incredible.

MR. LEONARD: Yeah, that's right. When it's small, it doesn't matter. But it's turning out to be important now, so we have to upgrade the model.

MR. MATTIE: We've discussed it, and I discussed it with Dana Powers, and he felt that, you

know, it would happen quite quickly, but on the order of seconds, not instantaneously, and so it may be, you know, rather than --

CONSULTANT WALLIS: You mean it spreads all along instantly?

MR. MATTIE: Well no. He meant the penetration.

CONSULTANT WALLIS: Oh, the initial penetration.

MR. MATTIE: The initial contact.

CONSULTANT WALLIS: But that one meter square doesn't happen instantly.

MR. MATTIE: Exactly, exactly, yeah. Let me, in this particular analysis, it's probably more important in the fact that area, at some point the area isn't going to matter. But the rate clearly does matter, yeah.

MS. GHOSH: So we're working on that one, and then the next slide, I think this one we've talked about a lot about too. We're uncomfortable, and we don't know, we're not sure what else is out there to come up with a distribution for the SRV stochastic failure rate, but we do think the database we're relying on is not representative of the situation in our scenario. So we're struggling with that.

If anybody knows of any other information sources, I think we would welcome that. So the schedule. I think as Pat Santiago mentioned, there's a Commission memorandum that's going to go up in June, that forwards the results of the SOARCA main study, and that memorandum will contain a very short discussion on where we are with regard to the uncertainty analysis, the schedule and what our interim conclusions are.

We hope to complete the analyses and documentation around September of this year, and we would like to come back to the ACRS and present the results and the final report, and we would like to get a letter from the ACRS, if you all were willing to do that, on the final uncertainty analysis.

And right now, we are planning to transmit the final uncertainty analysis to the Commission in November of this year. Any questions?

CONSULTANT WALLIS: Well, it seems to me, this isn't a question, it seems to me that this uncertainty and sensitivity study is every bit as important as the SOARCA report, because without it, there are all kinds of questions that remain from the SOARCA.

So I think it's a very important

supplement. Without it, the SOARCA report would be weaker significantly. That's just a personal opinion. But it's very important. I think you're doing a professional job.

MEMBER REMPE: What about doing something for Surry and not just Peach Bottom?

MS. GHOSH: We would love to do that. You know, I keep trying to sell this uncertainty analysis as a first step, you know. I think it's the first time we're really trying to tackle a problem of this magnitude, in terms of the integrated look at so many different parameters using the MELCOR code.

So this is kind of a first step. We're doing, you know, one of the scenarios for Peach Bottom. I think we would really like to do, as a next step, one of the Surry scenarios, and also because I think we'll be helping out with the Level 3 PRA project, which is for PWR.

So you know, in some sense to the extent that we can get insights for PWR, similar to what we're getting for Peach Bottom now, to help us be smarter about the uncertainty analysis when we get to Vogtle, I do think it would be valuable, and we're hoping to get that.

We don't have concrete plans at this

point, but I think that's a natural next step.

CHAIR SHACK: I think we could probably write a letter. I think we could do that.

(Simultaneous speaking.)

CHAIR SHACK: So I think that might well come up in a letter.

MEMBER STETKAR: You're going to discuss the uncertainty analysis when you come to the full Committee in a couple of weeks?

MS. GHOSH: Oh.

MEMBER STETKAR: The question is what do you present to the full Committee?

MS. GHOSH: Yeah. What would you like us to present?

MEMBER STETKAR: I would say the uncertainty analysis, but most of the committee is here. I'm not sure how many more -- well, Sanjoy.

CHAIR SHACK: Sanjoy will be here.

FEMALE PARTICIPANT: Dana.

MEMBER STETKAR: Dana. Oh, Dana has to recuse himself.

CHAIR SHACK: And Jack may not be back. That's about it.

(Simultaneous speaking.)

CHAIR SHACK: We'll have some time to

think about it some more, and so I really, I think, you know, we've been through much of this before. I really think the uncertainty --

MEMBER STETKAR: Not much of that has changed.

CHAIR SHACK: And not much of that has changed. The uncertainty analysis is really -- I mean I'm sort of with Graham, that when you have a best estimate analysis, it doesn't make -- it's not complete until you've got the uncertainty analysis.

So I really think that uncertainty here is just absolutely necessary to complete this, and that's one reason we might write a letter recommending that Surry could be under an uncertainty. But let's plan on just doing the uncertainty analysis as the full Committee presentation.

MEMBER RAY: Well, if you say -- you know, I'm trying to get my mind around what, how do we use the results, who do we apply them to. One of the things that's been mentioned is to validate the viability of the emergency evacuation planning, for example. The time for the release is such that we can have confidence that evacuation can be used as a way of avoiding radiation exposure.

But embedded in the uncertainty analysis,

of course, is at least one variable that has a heck of an effect on the timing, and that is what if that valve sticks open the first time it lifts? You've got -- and I realize these are safety valves, not PORVs. But you've got block valves on PORVs because they do stick open.

I've certainly seen safety valves stick open on fossil plants a lot. So if the question was is there a much earlier release scenario associated, now this is a beyond design basis accident. I understand that. It's long-term SBO.

So you know, I'm just struggling with well, is that even relevant to anything, because it is an accident that's beyond a design basis, and so how important is it to recognize that there's a much earlier, credible, in my mind anyway, release scenario?

All you have to do is have one of these valves stick open the first time it lifts, and you're on your way. And so the bottom line is --

MR. LEONARD: Let me comment on that, because our observations from the sensitivity and I think the uncertainty analysis is confirming it, that the earlier the SRV sticks, the better you are.

MEMBER STETKAR: The better it is.

(Simultaneous speaking.)

MR. LEONARD: Time frames get down. It's when it sustains the cycling for a very long time, that's when most of these other phenomenological --

MEMBER RAY: I forgot. You did say that, but that's just part of the processing I've been doing through. I'm glad you reminded me of it.

CHAIR SHACK: When you lose cores, it's generally a good idea not to have valves stick open. Once they're gone --

(Simultaneous speaking.)

MALE PARTICIPANT: It depends on where the valves are, though.

MEMBER RAY: I guess I was thinking the valve sticks open, you lose your water inventory, the core fails and you have an earlier release, and that's sort of the simplistic way I was thinking about it.

In any event, like I said, that's at least one application that I can draw out of this, that it's where you use the results to say yes, our emergency planning provisions are adequately for even a beyond design event like this.

CHAIR SHACK: Can you hook up Mike again?

MEMBER RAY: All of that was just meant by way of saying I'm trying to grapple myself with how do

you apply the uncertainty, because that presumes you have some vision for how to apply the results themselves.

MEMBER STETKAR: Well first I want to have some confidence in the results.

MEMBER ADBEL-KHALIK: I'm grappling with the same kind of question, and it just -- it is good that you're doing the uncertainty analysis. But my concern is that there are just so many parameters that it doesn't matter what kind of answer you get.

You will always be able to justify the result to yourself, because there are just so many interrelated parameters, and therefore ultimately, the credibility of this study will just depend on whether or not the result passes the smell test. To me, at this stage, it's still an open question.

MS. GHOSH: Sorry. I don't know if I understood what you were saying. You're saying there's so many parameters that could explain variation in results, or I'm sorry. Can you elaborate?

MEMBER ADBEL-KHALIK: There are so many variables that it doesn't matter what answer you get. You will always be able to sort of argue your way through the answer.

MR. MATTIE: I don't know if I'd agree with that statement.

MS. GHOSH: Yeah. You know, the past-done, you know, I guess past uncertainty analyses show that even when you have a lot of uncertain parameters, it's usually just a handful that's driving the results.

(Phone ringing.)

MS. GHOSH: I think we're seeing the same thing here. We have 23 parameters we've put in, but we're really driving the results with a handful of them.

CHAIR SHACK: Hold on, because the -- no. The phone is interfering with the microphones. So until the phone stops, don't talk.

MS. GHOSH: Oh, okay.

(Off record comments.)

MEMBER RAY: But we haven't achieved perfection yet. It's better, but not --

MEMBER ARMIJO: It's a lot better.

MEMBER STETKAR: Apparently Mike doesn't have any final comments.

(Laughter.)

CHAIR SHACK: We can email him.

MS. GHOSH: I'll just repeat my last

sentence for the record, because I think there was some interference. Past uncertainty analyses have generally shown that even when you have a lot of uncertain parameters that you, you know, put into your analysis, it's usually just a handful that are controlling the results.

I think we're finding something similar in this case. We've put in 23 parameters, but it really boils down to a handful that's really driving changes in the results.

MEMBER ADBEL-KHALIK: Assuming that you know what's going on in detail, and you understand all the phenomena, I would agree with you. But I don't think we do.

MR. MATTIE: You always have to consider your state of knowledge in these types of analyses.

MEMBER ADBEL-KHALIK: Yes, and that's why I say ultimately it will just depend on whether or not the final result passes the smell test.

MR. MATTIE: If you make a convincing case that you understand what's an important phenomena, and to that regards, of the 23 parameters that we put in, we only measure their importance of those 23 parameters, and of those 23 parameters that we considered, only a handful of those parameters showed

an important effect.

And when we say "important effect," what we're describing is the -- its contribution, that single parameter's contribution the variants or uncertainty in the expected outcome. So there are parameters that we didn't consider that could also be important.

If you did a parametric study exhaustive on every conceivable input of MELCOR, then you could also come back and say well, what about parameters that aren't in MELCOR? You could, right?

MEMBER ADBEL-KHALIK: Sure.

MR. MATTIE: So but at some point, you would have a level of confidence in your result. I think that's what we're doing, is we're contributing to that level of confidence in the ultimate result.

MEMBER STETKAR: And again, for the purposes of the study, I think you know, go back to it is what it is. It's not what it's not. But given the constraints of what it is, being able to still express some knowledge about, you know, I come back to margins.

I had this discussion with drawing a line with safety goals or whatever, but still having some degree of confidence that within the range of what

we've been able to examine, and some expression over uncertainties over that range, we still have learned something, and we still have some confidence about what that margin is, and I think that's important.

(Dial tone.)

Anybody can always argue that you can look at other things, and indeed you can. And indeed, there's a possibility they might be important. But that's not what it is.

MEMBER BLEY: I guess what I like about this afternoon is seeing that you're finally addressing these issues. What I like is that you've--

MEMBER CORRADINI: I'm here.

MEMBER BLEY: --you've begun, you're trying to explain a variability in your results, and understand them in terms of what's modeled and what might not be in the model, and that side of it.

So this is for me a big step forward in this whole SOARCA exercise, and I look forward to seeing where you get to next. But I think you're on the right track, and that's the first time I've said that.

(Laughter.)

MEMBER SCHULTZ: I agree, and the reason I say or would stress the importance is that we heard

an identification of how many downstream projects, analyses, programs are going to be affected, are going to leave -- the SOARCA analysis will lead toward or lead to follow-on analyses, and that makes this portion of the SOARCA analysis that much more important.

CHAIR SHACK: Around the table, Dick.

MEMBER SKILLMAN: No comments. But thank you for a very informative day. Thank you very much.

CHAIR SHACK: Dennis, anything additional?

MEMBER BLEY: No, except the thing I mentioned earlier, if what everybody told us earlier about what operator actions are where, is the way you said, then get rid of that statement in your report that says we looked at --

CHAIR SHACK: The procedures.

MEMBER BLEY: Others as well. In the mitigated cases, we looked at the actions associated with B.5.b and other modeled human actions separate from that. Just get rid of that; it's not true. If it is true, tell what they are. I think we'd better have Mike's comments before we lose him again.

CHAIR SHACK: Yeah. What's happening to Mike?

MEMBER CORRADINI: Are you making jokes

about me again?

CHAIR SHACK: No. We wondered what --

MEMBER STETKAR: You keep disappearing.

MEMBER BLEY: We want to hear from you before you disappear.

MEMBER CORRADINI: Well, I lost, I basically lost service. I tried to call back and it was busy. So I kept on sending emails, so that's all right. So do you want my comments now?

CHAIR SHACK: Yes, before you get dropped again.

MEMBER CORRADINI: So first of all, I thank the staff. I followed it up through, but not including the uncertainty analysis. That's when I kind of went offline. I think the uncertainty analysis --

CHAIR SHACK: Well, you're going to hear it again at the full Committee.

MEMBER CORRADINI: I guess I would emphasize that in the uncertainty document, what things they cannot vary in a rigorous way or in the way in which they're choosing to do it. That doesn't mean they shouldn't identify that uncertainty.

I'll use the example when Randy was up, and I asked about his -- I asked about his comment

that for short-term station blackout versus long-term station blackout, there was a factor of 10 difference in source term release.

He did a kind of a heuristic thinking process, that he thought it had something to do with early times, high temperatures and a lot of zirconium. So you may not be able to directly do the sort of analysis you're doing on some of the other variables, where you pick a high and a low and you do a distribution.

But that doesn't mean you ignore what you're uncertain about. So I would think the uncertainty analysis ought to be, in some sense, a relatively good listing of the things that you know you might be unsure about, but you may not have the tools to at least investigate it within the structure of how you're doing some of the other calculations.

That's one example. The other example is in relation to the scope. Scope did not cover land contamination. Clearly, we had discussed that, but there was a mention of another ongoing study that would do that.

I've come up with a number of things that popped in my head, but I think the uncertainty analysis at least ought to identify what you think are

the key physical things, that even though you know they're uncertain, you're not able to analyze it in the way you're analyzing the others. But at least it's a place where you can go back to, and do further analysis.

Beyond that, I think in terms of specific accident sequences and melt progression, I actually think it's a very complete report in many ways. So I think that's good. Other than that, that's my major comment, is how I deal with physical processes that don't fit the mold of the uncertainty protocol.

CHAIR SHACK: Harold, anything else?

MEMBER RAY: I've said everything, Bill. Thank you.

CHAIR SHACK: Sam.

MEMBER ARMIJO: Yeah. I think, I mean I'm pretty impressed. I think you've taken on a big job and dealt with it as best anybody could. The thing I think you're still some conservatism in parts of your model, the barium and cerium behavior. I think some of the results you're getting on that may be an artifact resulting from the way you're using the VANESSA code.

It's a thermodynamic code with no kinetics in it. So I'm -- with a heterogeneous debris, with

solid UO2, with barium and cerium buried inside the UO2, things melting. It's very complicated, and so I don't think you can use the thermodynamic reaction to assume that everything reacts, even though it's not in contact.

So but it may not matter. Like you mentioned, you have 20-some parameters and maybe this barium and cerium phenomena just really doesn't impact much. But I think I know someone in Albuquerque who will teach me something about VANESSA, and I'll maybe change my mind later. But that's what I got out of it today. That 's all.

CHAIR SHACK: Said?

MEMBER ADBEL-KHALIK: I have no other comments.

CHAIR SHACK: John.

MEMBER STETKAR: Nothing more, thanks.

CHAIR SHACK: Charlie.

MEMBER BROWN: Nothing else.

CHAIR SHACK: Joy.

MEMBER REMPE: Well, several times I've mentioned vessel failure and drain line, and I think I heard that that will be addressed. I was really glad to hear that they'll be a Rev. 1 of the two plant-specific reports, and again the uncertainty

analysis of documenting what it can and cannot do or how things were applied to accommodate. I think that was important.

I know you can't document that you didn't consider meteor attacks or whatever, but some of the key things I think you can do that. So I think I heard that that would be done, so I was happy.

CHAIR SHACK: Tom.

CONSULTANT KRESS: Well again, I have a number of comments about the whole day, not particularly about the uncertainty.

CHAIR SHACK: Work on the whole day.

CONSULTANT KRESS: Well, in the first place, I do think this was a good study and was a good thing that you're doing. I think it's well worthwhile, and did a good job, and most of it is technically dependable for it is what it is.

Of course you would probably know, I was disappointed that there were no societal risks, or not risks, societal consequences included, but I understand why, and I'm kind of glad to hear that there's a program later on to look at that, and I'd be interested in following that.

But when I get to doing this, consequences and risk even, the societal effects, I think they're

going to need another safety goal. They have to define what societal risk is acceptable to them from the standpoint of the safety code, and I think they need to give that a lot of thought.

I also thought that the mitigation versus non-mitigation needed to be better explained, in the sense of what was left out and what wasn't in those two. I thought it did show that the SAMDAs are really useful things. We probably should have had some of those SAMDAs in there much sooner than this. That's one of the lessons I took away from it.

It seems like the timing of the release is almost as important or moreso than the quantities, and I think there might be some, if I were to look at the uncertainties, that may be an area that I'd think about whether we have that right. I think it could involve things like that valve sticking.

You know, I understand that if it sticks earlier, you're better off. But maybe it sticks later, and I don't know where is the worse place for it to stick, to stick open. I like the comment made by some of the peer reviewers that most of the consequences result from the return criteria.

I think this brings to mind, I think for a long time, the linear no dose assumption has been a

real issue that needs to be re-thought out, particularly if that's going to be the maximum consequence driver.

I guess I'm all right with choosing the accident sequences that has a cutoff frequency value to them, but I would -- at first thought, I was thinking that we'd be better off to select an accident sequence from a LERF value, rather than the CDF, because the LERF value gives you bigger consequences.

It's a measure of the fission product releases, and I think I would have chosen that as my criteria to choose a sequence. This was advertised as state of the art, but I worry about VANESSA in there, and basically for the same reasons Sam mentioned, that I don't think when you have a heterogeneous mixture of things that come out at different times, I don't think you could even do a Gibbs-free energy with those.

VANESSA, I'm sure, assumes equilibrium, a complete mixture of everything, and works it from that point. I don't know if it -- I don't think I've ever seen any validation of it, experimentally or otherwise. It's strictly an analytical model.

So I worry about that part, and I don't know how you would do an uncertainty analysis on that, because the way it's done, it actually maximizes the

consequences, and maybe that's acceptable. So maybe I'm not so worried about it.

One question I had early on was I don't know how plant-typical Surry and Peach Bottom are, for all of the BWRs and PWRs. But and I worried about why aren't we looking at other plants, but we got an answer to those. So I lost, I just marked that one off my list.

I don't think the study in any way does away for the need for Level 3's for actually all the plants. It does provide some good input and some good things that the Level 3's can think about and be part of their study. So I was glad to see that.

One comment somebody made, I forgot here, and I agree we have to, and that is the documentation, I think, it would be nice if it's very clear that they tell what the reasons are for the consequences being so much different than previous studies. It would be nice to have that really made clear.

And I think if not so much related to this study, but when the Level 3's are done, I think there's a real need for us to revisit the consequence models that are in MACCS. I don't think we've ever taken a real serious look at them, and I think it's time that we did that. That's most of the comments I

had.

CHAIR SHACK: Graham.

CONSULTANT WALLIS: Well, I won't add anything now. I'll send you a report with details.

CHAIR SHACK: Any additional comments or questions before we go to the public? Well again, I'd like to thank the staff for a very informative presentation. Any comments or questions from the audience?

MEMBER STETKAR: Public comment?

MR. LYMAN: Thank you. This is Ed Lyman from the Union of Concerned Scientists. I'd just like to characterize what I think this study, where we are today with this study. Despite six years of effort and God knows how much staff time and money, it has essentially confirmed the magnitude of the latent health consequences from the siting study in 1982.

All you have to do is look at Table 7-15, to see that the -- other than the consequences within ten miles, the short-term station blackout unmitigated is within a factor of two of what they got in the 1982 safety study. That factor of two is essentially meaningless, in light of the uncertainty analysis that we've seen, where the fission product releases are at least are variant, at least by a factor of two, and

even if it's sublinear, you can see that it certainly could wash out a lot of that difference.

So I think the study, rather than finding significantly smaller latent health consequences than the siting study, it's essentially confirmed it. Now with regard to early fatalities, it's a tautology. All the study has shown is if you evacuate the public within the emergency evacuation zone before there's a release, that you're not going to have any early fatalities.

Now I think the great insight is that the, at least for the scenarios evaluated, core damage occurs, releases are delayed, relative to the assumptions that were for the SST-1 source term and what are contained in the baseline assumptions for the setting of the emergency evacuation zone.

But again, this does not evaluate -- this was not intended to evaluate the scenario which could lead to the fastest early release. That might be a large break LOCA or a terrorist attack, where you could conceivably have releases which are faster than even the short-term station blackout. So it doesn't really address the issue of whether emergency planning evacuation times are adequate.

And certainly with regard to Peach Bottom

and Surry, these plants are not representative with regard to the evacuation time estimates that are achieved, because they're in relatively rural areas. So they're not representative of large population plants that are in more densely populated areas, where evacuation time estimates may be longer than five or six hours.

So I don't think -- so I think the study's a lot better than when it started, that there are interesting technical insights that have come out of it.

But it should stop pretending that it's proving something which it isn't proving, namely that there's some significant, that older estimates were significantly more conservative, and that somehow nuclear power is much, much safer than people thought in the past.

I think it essentially has highlighted some interesting areas, but essentially confirmed those older studies. Thank you.

CHAIR SHACK: Okay. Any other comments?

(No response.)

CHAIR SHACK: Is there anybody on the bridge line? Is the bridge line open?

(No response.)

CHAIR SHACK: Then I think we can adjourn for the day.

(Whereupon, at 5:47 p.m., the meeting was concluded.)

State-of-the-Art Reactor Consequence Analyses (SOARCA) ACRS Subcommittee Meeting

Richard Chang

April 25, 2012

Agenda

- Previous ACRS Interactions
- Conclusions for SOARCA Pilot Plants
- Status Update

Previous SOARCA Briefings to ACRS

- July 2006- Subcommittee
- September 2006- Full Committee
- December 2006- Full Committee
- July 2007- Subcommittee
- November 2007- ACNW
- November 2007- Subcommittee
- December 2007- Full Committee
- June 2010- Subcommittee

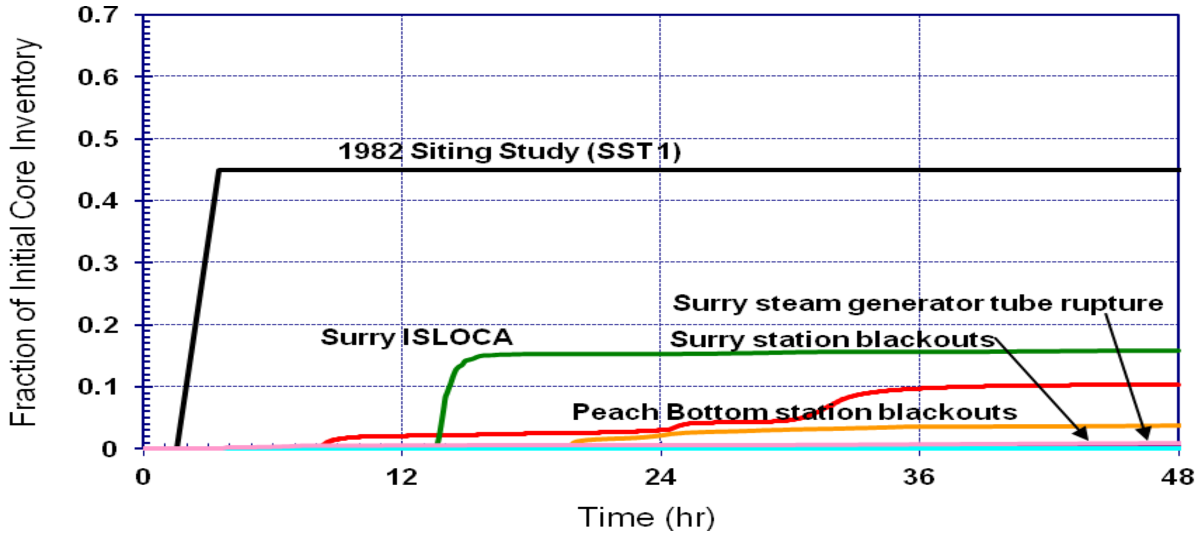
Conclusions for SOARCA Pilot Plants

- When operators are successful in using available onsite equipment during the accidents analyzed in SOARCA, they can prevent the reactor from melting, or delay or reduce releases of radioactive material to the environment.

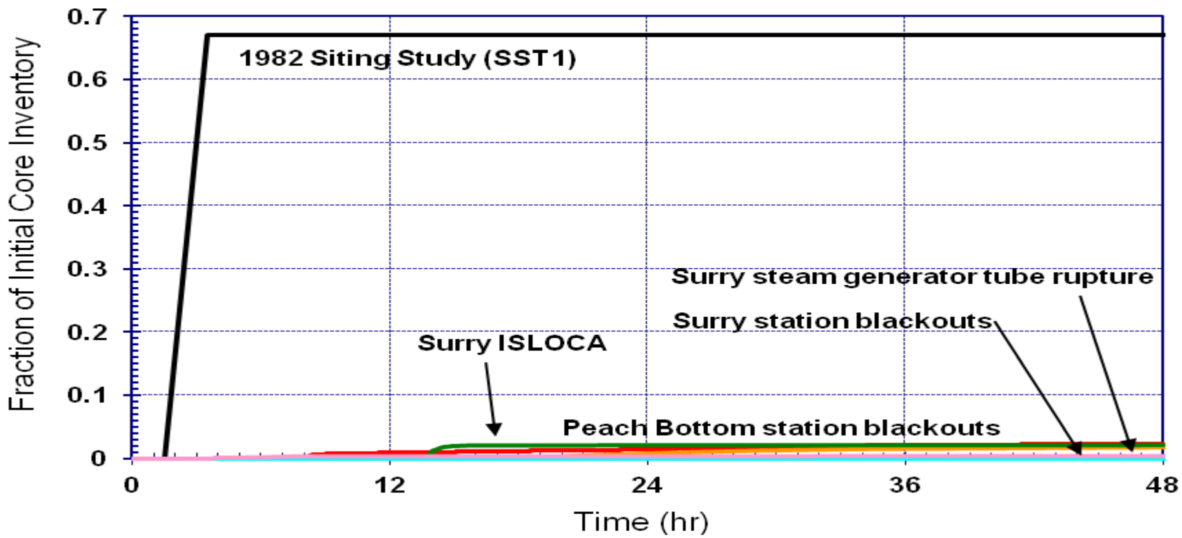
- SOARCA analyses indicate that all modeled accident scenarios, even if operators are unsuccessful in stopping the accident, progress more slowly and release much smaller amounts of radioactive material than calculated in earlier studies.

Conclusions for SOARCA Pilot Plants (cont.)

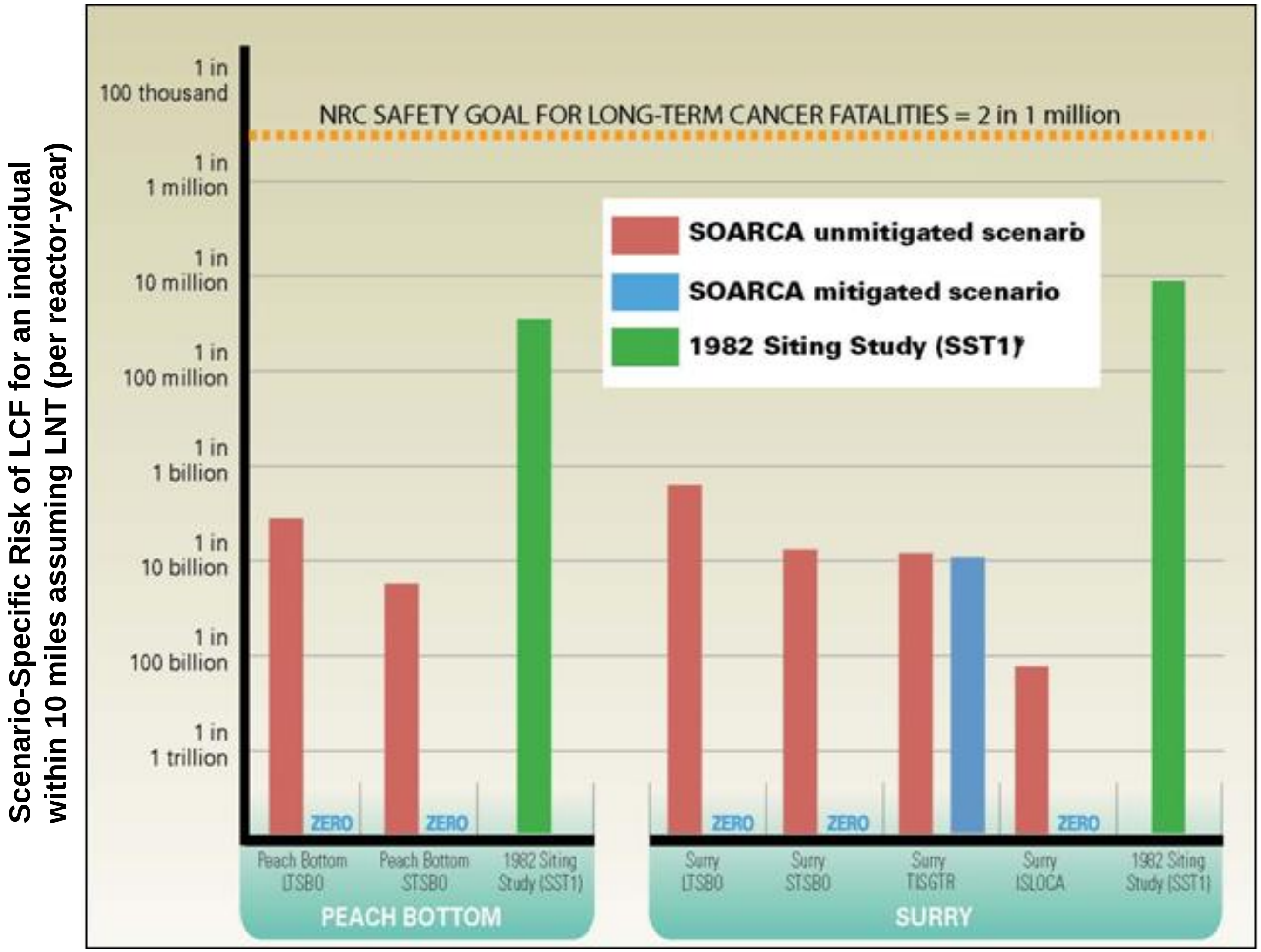
Iodine Release



Cesium Release



Conclusions for SOARCA Pilot Plants (cont.)



Note: Comparisons of SOARCA's calculated long-term cancer fatality risks to the NRC Safety Goal and the average annual U.S. cancer fatality risk from all causes are provided to give context. Relative to the safety goal comparison, the safety goal is intended to encompass all accident scenarios. SOARCA does not examine all scenarios typically considered in PRA. Additionally, estimated risks below 1×10^{-7} per reactor year should be viewed with caution because of the potential impact of events not studied in the analyses and the inherent uncertainty in very small calculated numbers.

* The 1982 Siting Study did not calculate the risk of long-term cancer deaths. Therefore, to compare the 1982 Siting Study SST1 results to SOARCA's results for risk of long-term cancer death, the SST1 release was put into the MACCS2 code files for Peach Bottom and Surry unmitigated STSBO calculations.

Status Update

- New/revised analyses based on plant fact checks/external peer review committee comments
  - PB RCIC start time for STSBO w/blackstart; SRV analyses; TIPS; and Ba/Ce
  - Surry ISLOCA reanalysis; and mitigated LTSBO
- Fukushima Appendix
- SOARCA Peer Review
- Public Comments and Commission Documents
- SOARCA Uncertainty Analysis

Resolution of Peer Review Issues on the Peach Bottom MELCOR Analysis

ACRS Briefing
April 25, 2012

M.T. Leonard
R.O. Gauntt

State-of-the-Art Reactor Consequence Analysis Program

Sandia National Laboratories

Outline

Overview of three topics discussed at length with the SOARCA Peer Review panel:

- Effects of SRV seizure (open) on accident progression and source terms
- Potential for containment bypass and early fission product leakage from containment via failed in-core instrument tubes
- Late environmental release of Barium and Cerium

(1) SRV Failure Mechanisms During LTSBO

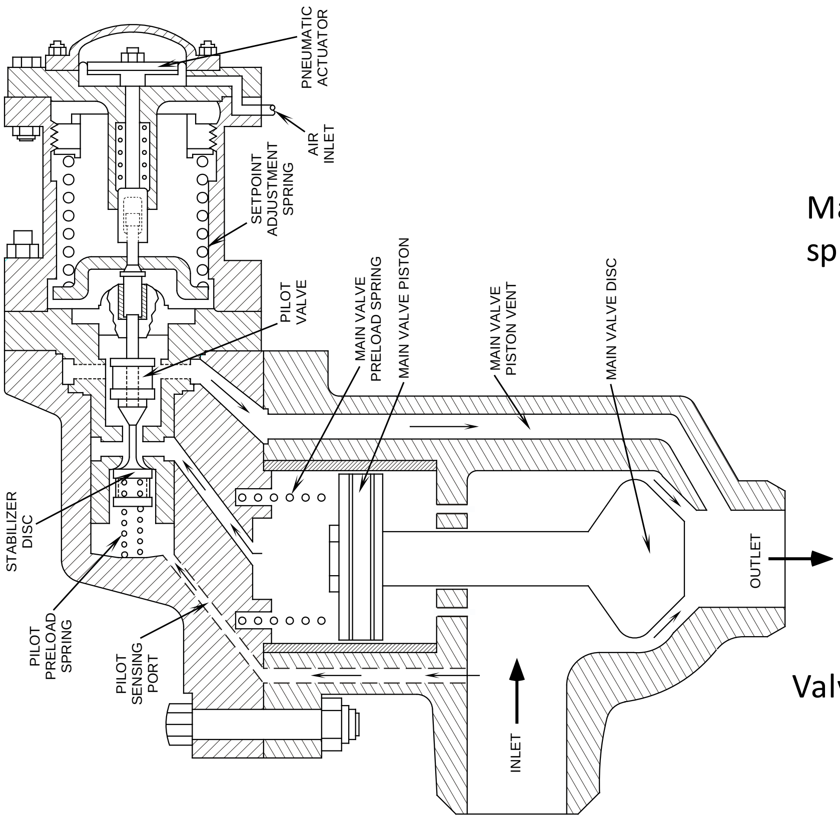
- **Stochastic failure to reclose**
  - Random failure of valve closure mechanism
  - High number of valve cycles

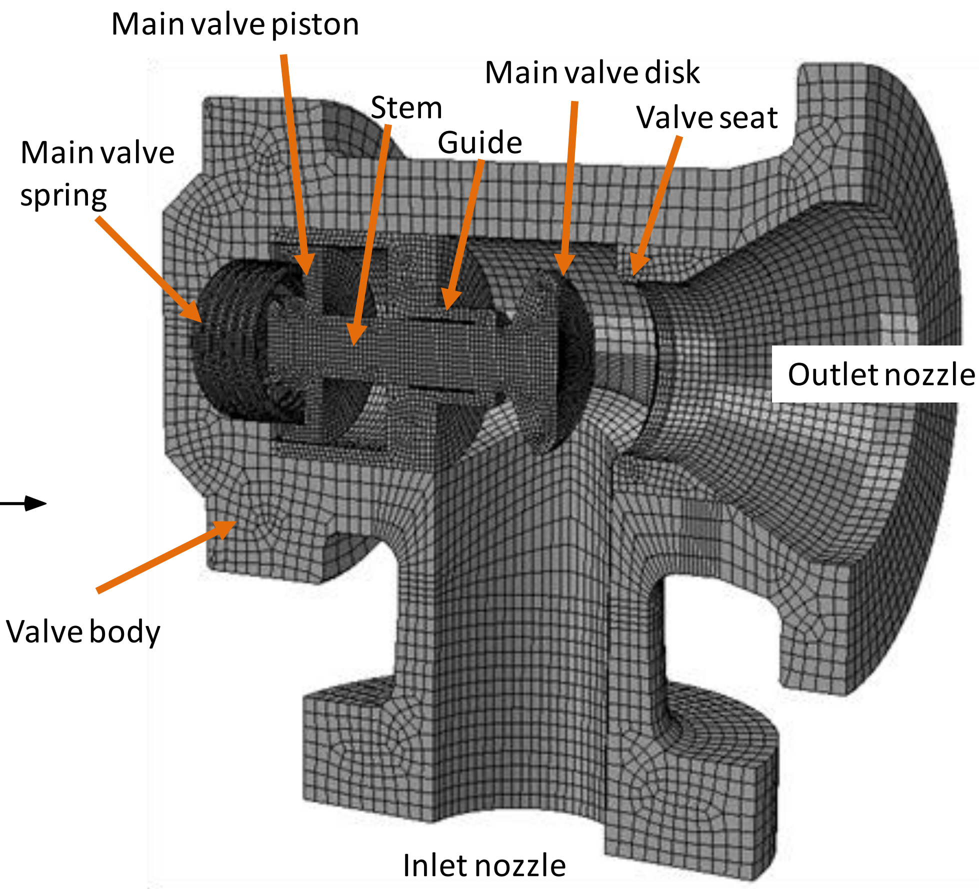
  → Addressed by applying SRV failure rate (λ) from Level 1 PRA data base: expected value (1/λ) for number of successful cycles before failure ~270.

- **Seizure in open position after onset of core damage due to heat transfer from high temperature gas**
  - Thermal expansion of internal valve components likely failure mechanism

  → Addressed by calculating thermal response of sample valve internal component and comparing to seizure criterion based on detailed finite element model of SRV

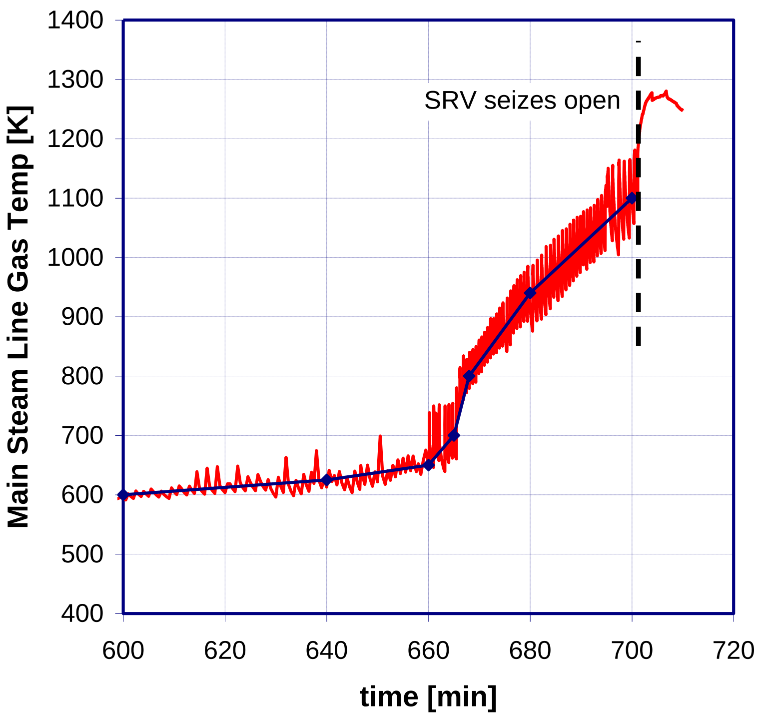
Finite Element Model of Target Rock SRV

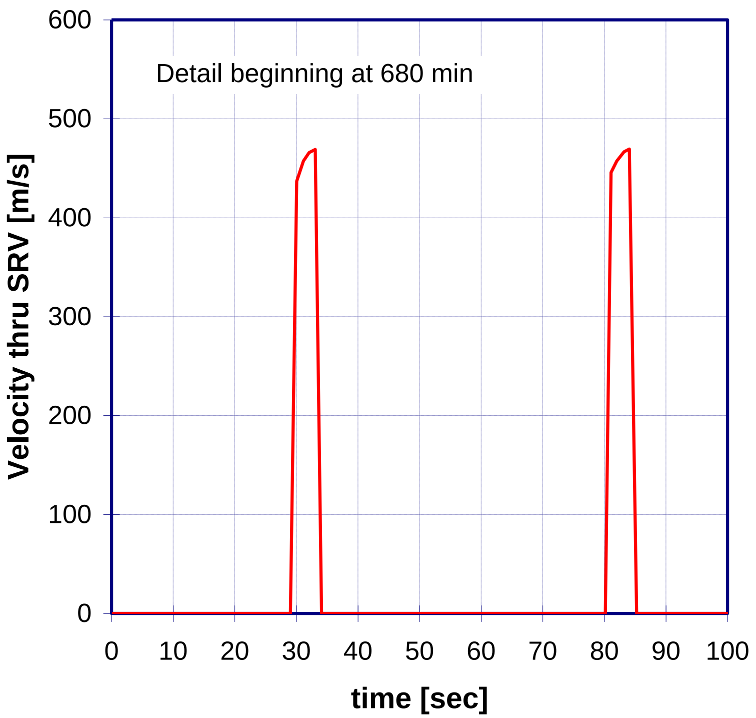




Thermal Analysis of Target Rock SRV (2)

- **Boundary conditions: temperature and flow from MELCOR calculation of LTSBO**





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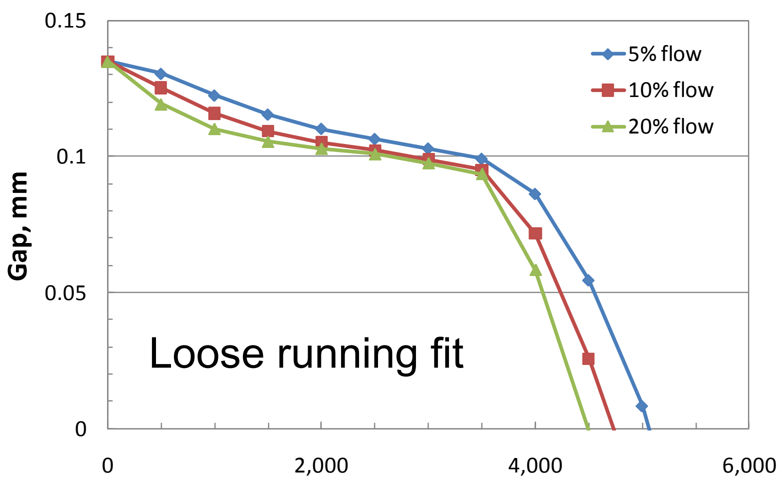
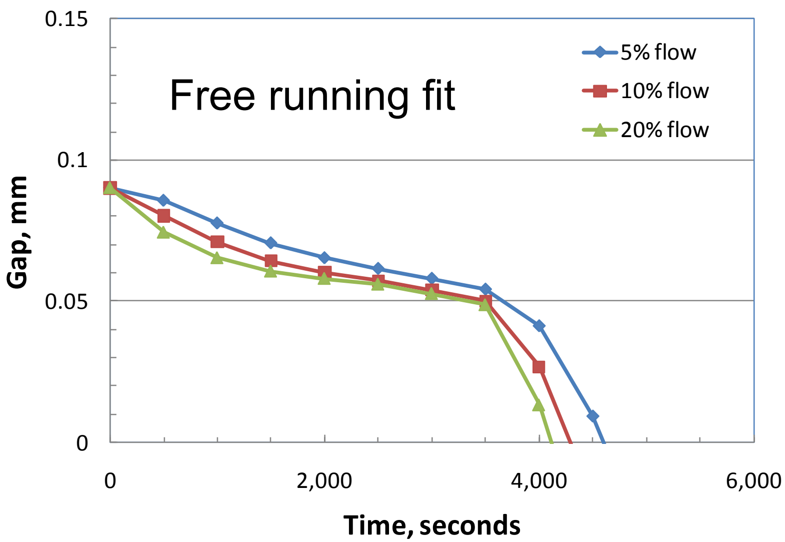
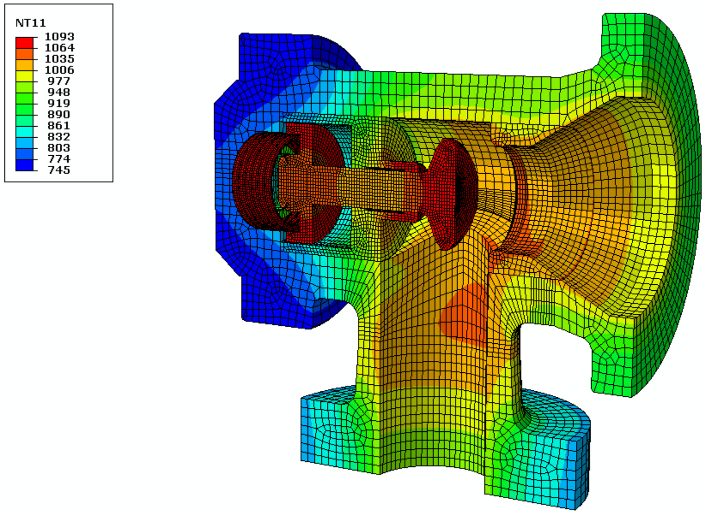
Reduction in Gap Clearance for Valve Stem







Clearance Fit Type	Minimum gap size, mm
Loose running fit	0.135
Free running fit	0.090
Close running fit	0.0275

Full closure of gap, when:
800K < Tgas < 900K

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Impact of Alternate SRV Failure Criteria on Environmental Release

SRV seizure criterion	SRV failure mode	Nr of cycles at Seizure	Time of SRV Seizure (hr)	Max MSL L-M Damage Index	Iodine Release to Env > 1% (hr)	Fraction of Inventory in Environment at 48 hrs: Iodine	Cesium	Ba
LTSBO								
Valve Stem > 900K; Stochastic 63%	S	270	8.2	5.4E-08	23.5	2.0%	0.5%	0.6%
Valve Stem > 811K; Stochastic 90%	T	465	11.2	1.2E-05	21.8	3.2%	3.2%	1.3%
Valve Stem > 900K; Stochastic 90%	T	475	11.4	7.0E-04	21.2	4.8%	1.8%	0.8%
Valve Stem > 900K; Stochastic 90% (1/10 open area)	T	475	11.4	1.0	12.8	17.8%	5.3%	1.5%
Valve Stem > 900K; Stochastic 90% (1/2 open area)	T	475	11.4	0.0290	20.6	3.3%	2.3%	0.8%
Valve Stem > 1100K; Stochastic 90%	T	497	11.7	0.53	21.0	3.5%	1.9%	1.5%

STSBO								
Valve Stem > 900K; Stochastic 5%	S	15	5.5 min	2.4E-06	12.1	1.7%	0.5%	4.3%
Valve Stem > 900K; Stochastic 50%	S	187	1.7	1.6E-05	9.2	11.4%	1.7%	6.3%
Valve Stem > 811K; Stochastic 90%	T	187	1.7	1.6E-05	9.2	11.4%	1.7%	6.3%
Valve Stem > 900K; Stochastic 63%	T	195	1.8	0.001	9.7	11.6%	1.7%	9.6%
Valve Stem > 900K; Stochastic 90% (1/10 open area)	T	195	1.8	1.0	5.5	18.4%	8.0%	4.7%
10 cycles with Gas Temp > 1000K; Stochastic 90%	T	211	2.1	0.133	8.8	11.1%	2.0%	8.4%

Indicates "Best Estimate" Case

S=stochastic
T=Thermal

Grey indicates cases in which MSL creep rupture occurs.

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Conclusions of SRV Sensitivity Analysis

- **Variations in SRV failure criteria have significant effect on in-vessel thermodynamic signatures, but small effect on:**
  - **Time to lower head failure**
  - **Time environmental release begins**
  - **Cumulative release to environment**

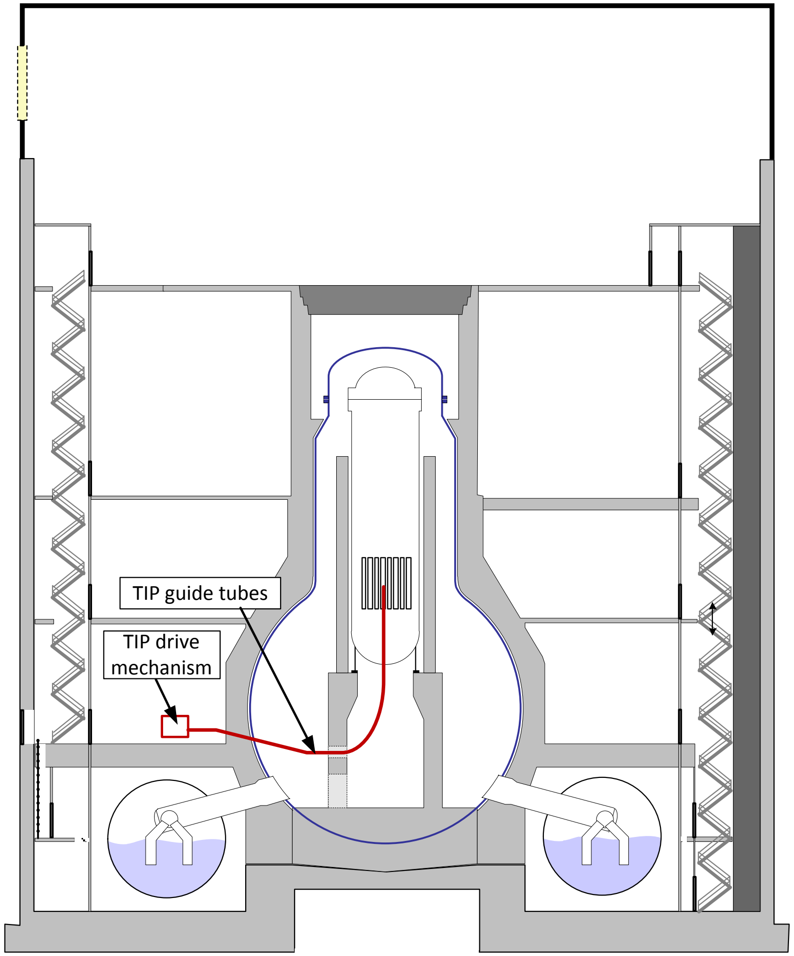
… provided creep rupture of the main steam line (MSL) is averted.

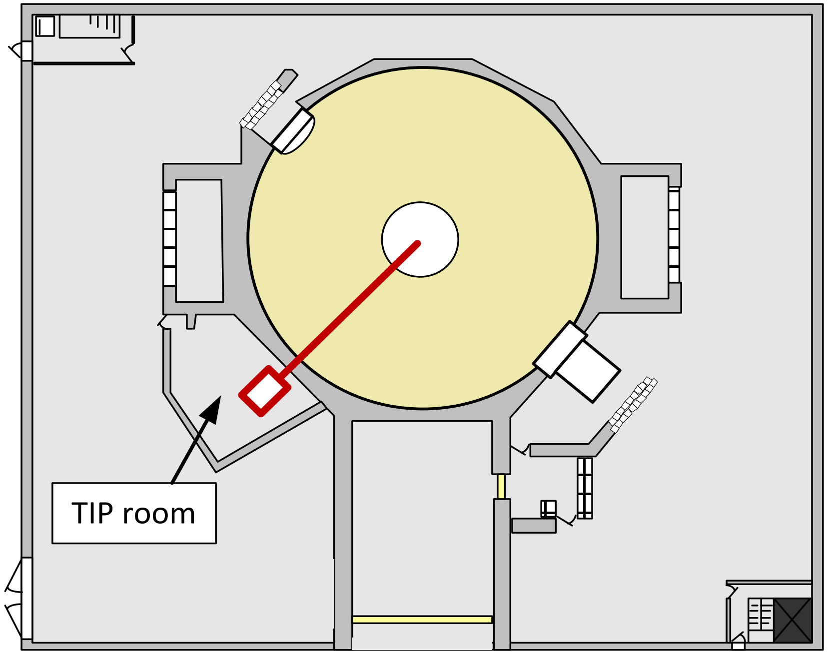
→ Conditions leading to MSL creep rupture to be investigated in detail in SOARCA Uncertainty Analysis

(2) Investigation of Early Fission Product Release through In-core Instrument Tubes

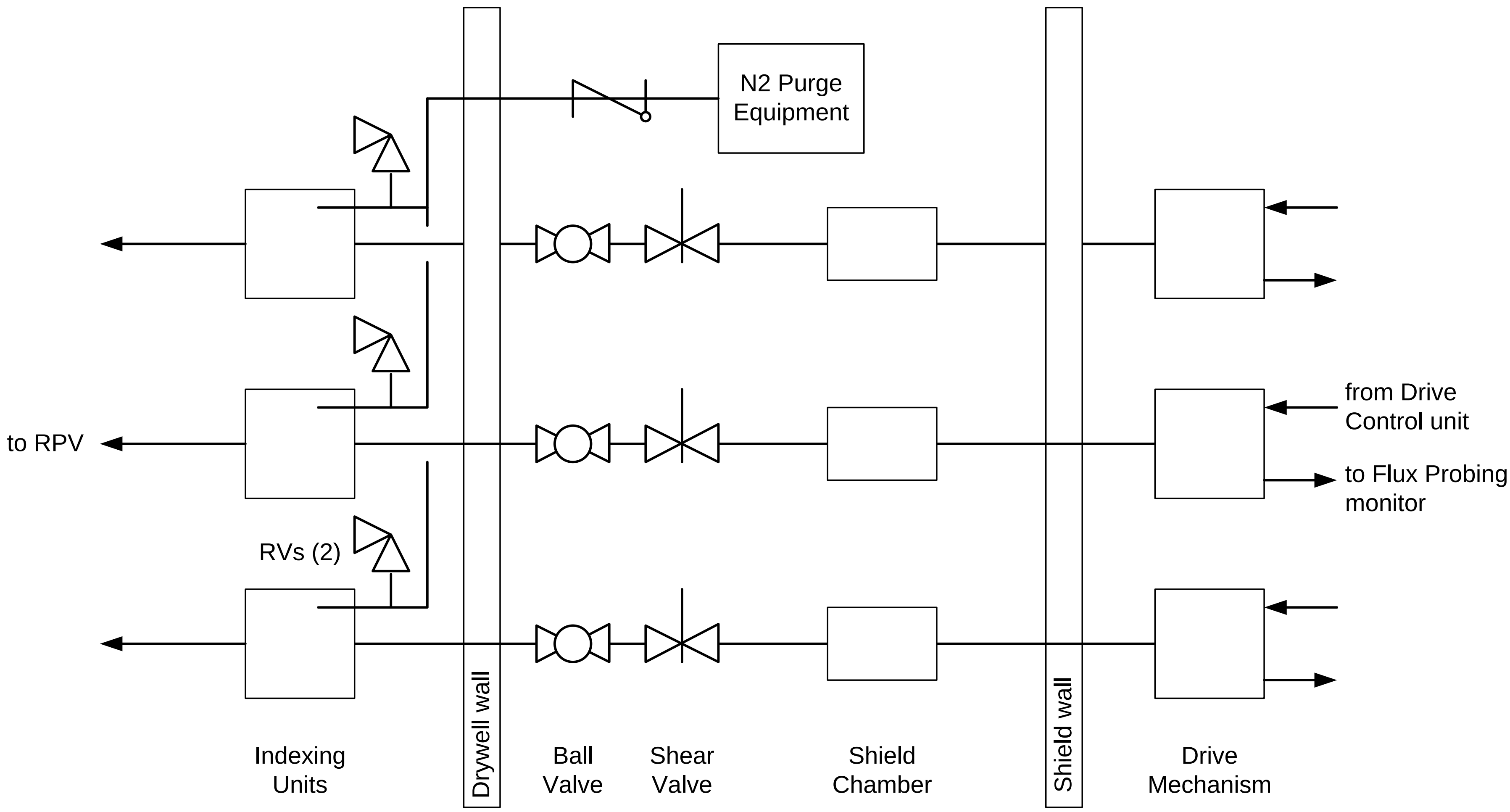
- Description of “Traversing In-core Probe” (TIP) system
  - Mechanical configuration
  - Potential leak pathway(s)
  - Operating practice (time windows for leakage)
- MELCOR model
- Calculated Results for LTSBO

Postulated Containment Bypass Leak Pathway via TIP tubes





TIP Drive Configuration



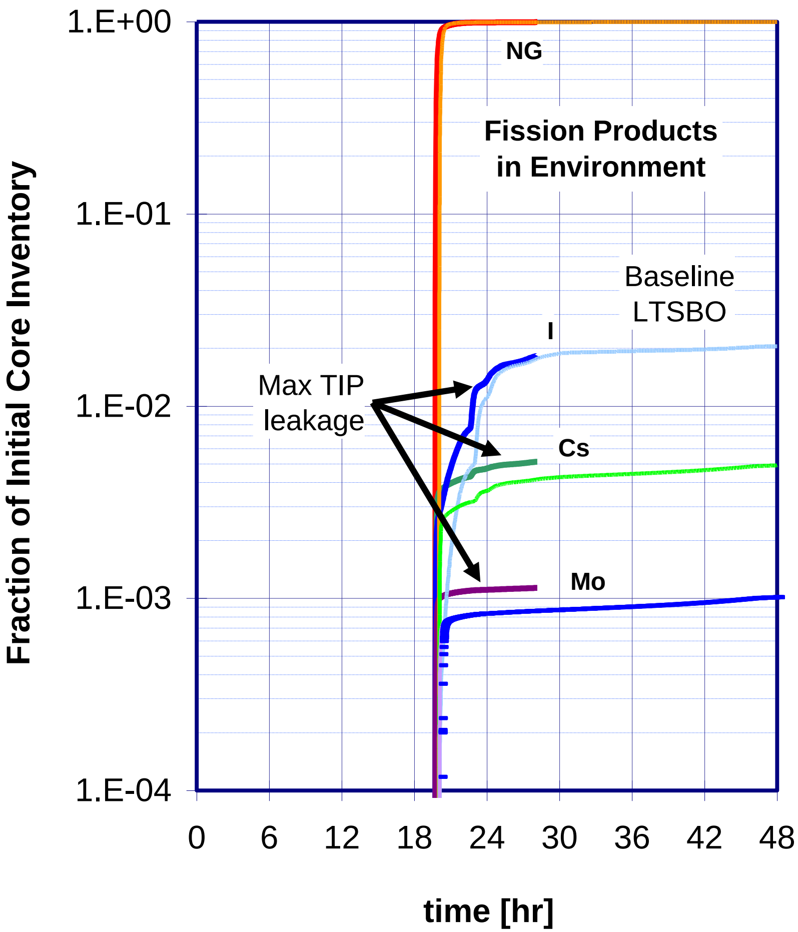
Operating Conditions

- TIP system in operation (ave) 1-hr every 4 months
  - Average conditional frequency = 4.6E-4/yr
- All three tubes in operation simultaneously to minimize overall exposure time
  - Probe inserted (tube cross-section occupied by drive cable) most of operating time period
  - All probes fully-withdrawn unlikely, but possible
- Isolation valves require electric power to operate
  - No formal provisions for actuation if power is unavailable

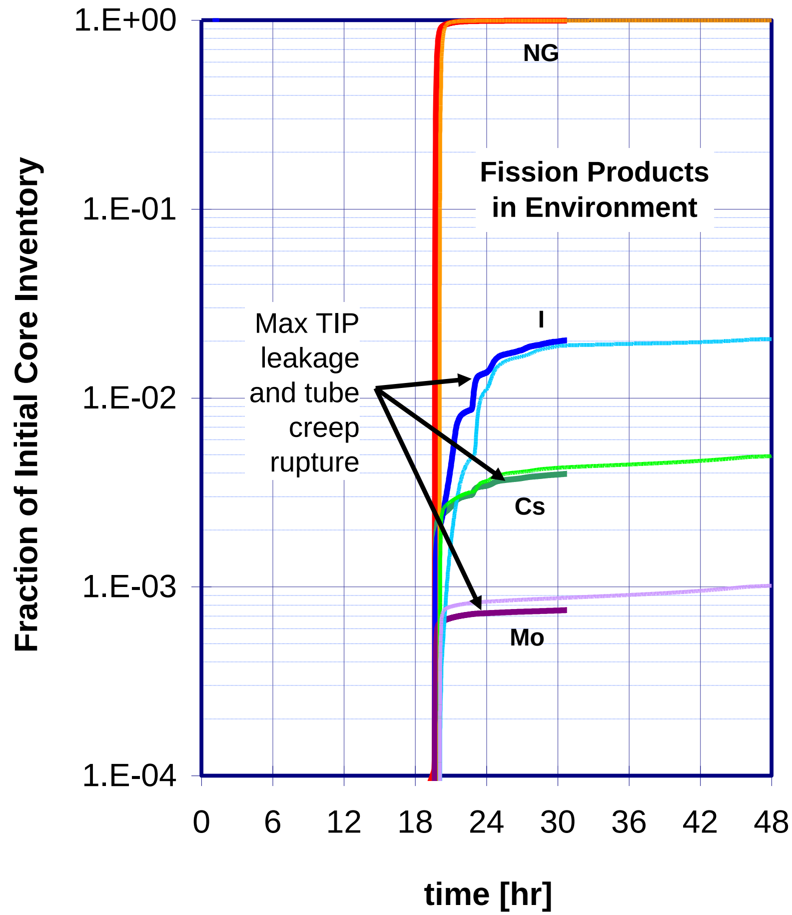
Leakage Potential

- Bounding case
  - 3 lines in operation at a time (isolation valves open)
  - All 3 probes fully withdrawn to shield chamber
  - Area corresponding to 3 x 0.28-in (ID)
- More realistic case
  - 3 lines in operation (isolation valves open)
  - All probes inserted (tubes filled with 0.258-in cable)
- In both cases limited area defined by tube cross-section (RVs and tube end-point areas larger)

Sensitivity Calculations with Bounding TIP Leakage – Source Term



Continuous TIP leakage



Creep Rupture of TIP tube

Conclusions on Effects of TIP Leakage

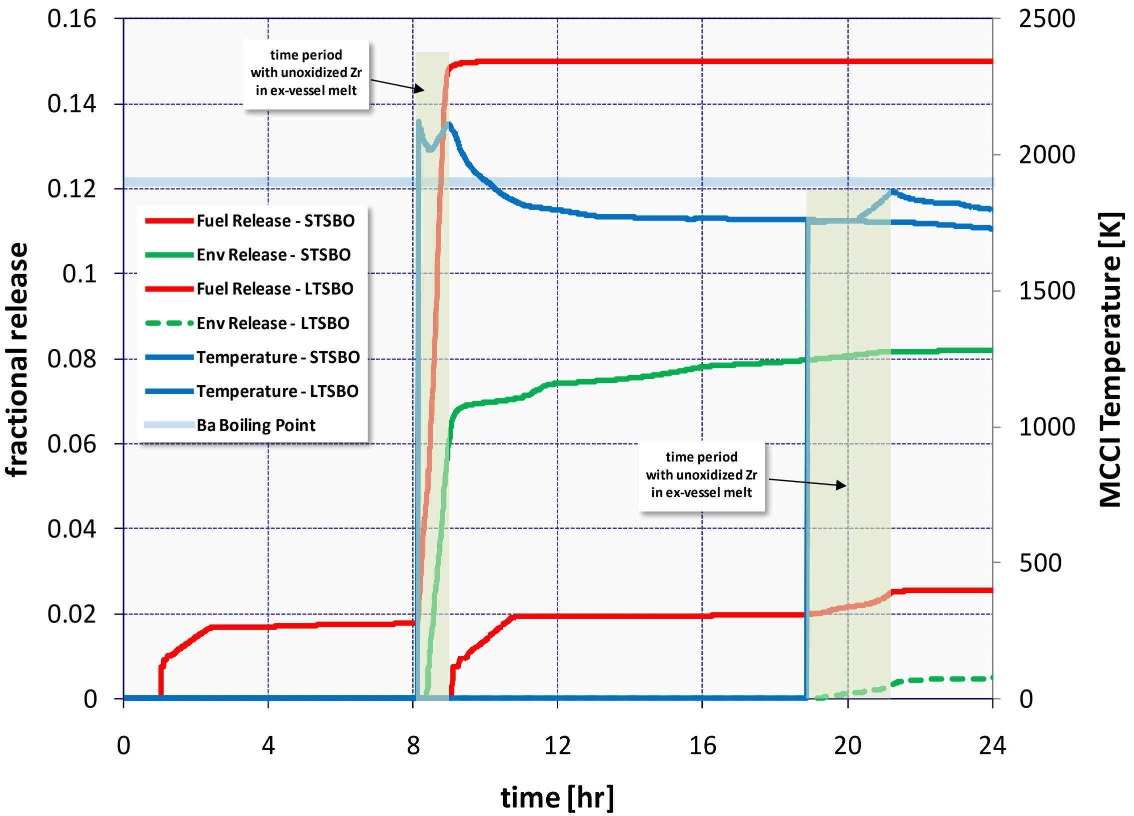
- Frequency of accident sequence occurring at time TIP system in operation well below truncation threshold ($\sim 10^{-9}$ to 10^{-10})
- Most likely leak area less than 0.1 in^2
  - Bounding area ~0.8 in^2
- Possibility of creep rupture of tube in drywell
  - Eliminates direct containment bypass, but retains early containment (drywell) leakage
- Effects on F.P. release are small for bounding (maximum) leak rate

Ba and Ce Behavior Background

- Ba and Ce predicted to dominate "emergency phase" doses for Peach Bottom STSBO
  - ~10% in STSBO versus ~ 1% in LTSBO
- Enhanced released occurs during the ex-vessel MCCI phase
  - CORCON/VANESSA
- Investigation show enhanced release attributable to
  - Chemically reducing environment (*essential*)
  - MCCI temperature (*strong effect*)
  - Strong mass transfer from MCCI gas generation (maintains disequilibrium)
  - BWR's have more Zr in core relative to PWR's

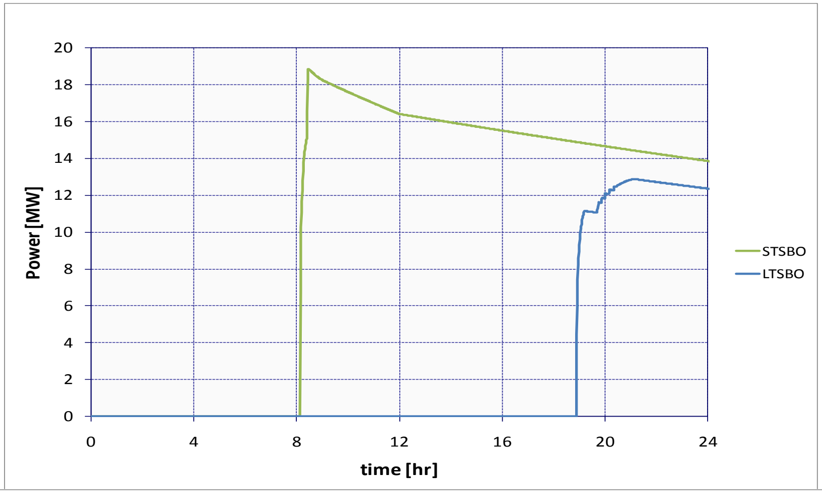
Barium Release

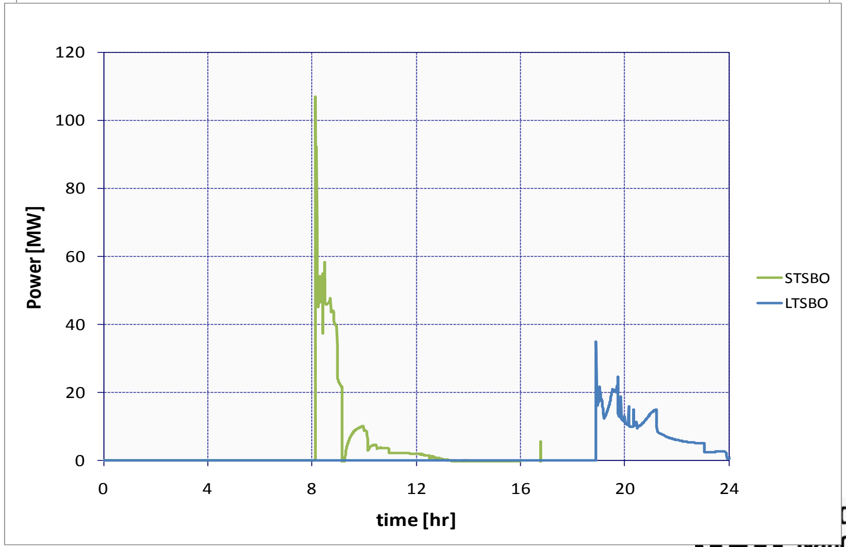
- Considerable variability was observed with respect to Barium releases
  - General trend is that environmental releases for the STSBO are on the order of 8% to 10%
  - The LTSBO, on the order of 0.5% to 1%



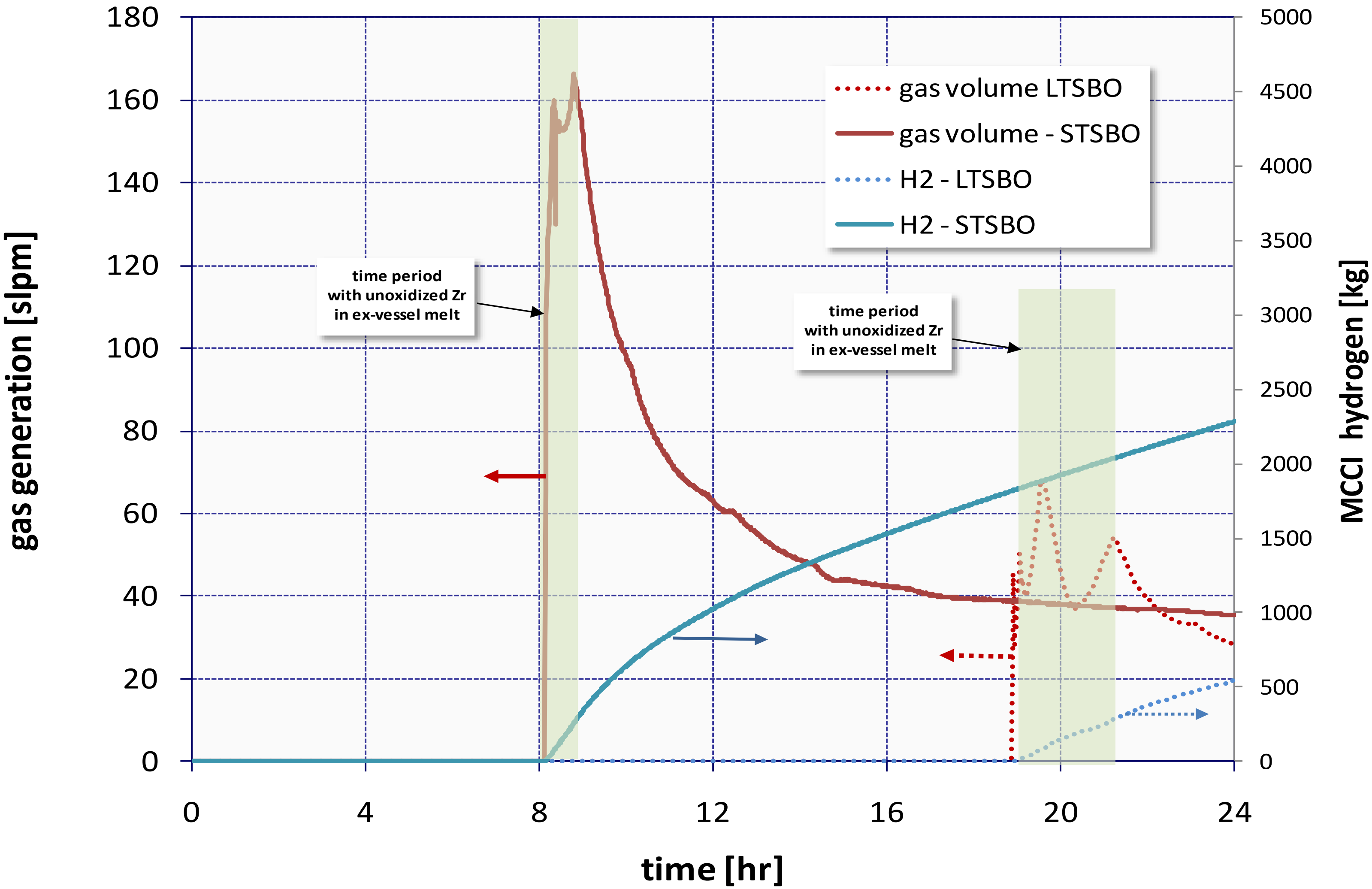
Decay Heat and MCCI Chemical Power

- Decay heat is larger in STSBO
- Temperature is higher in STSBO
- MCCI Chemical heat is larger in STSBO
- Factors combine to maintain higher MCCI temperature
- Gibbs energy is sensitive to temperature
- Net effect is higher partial pressure of Ba and Ce



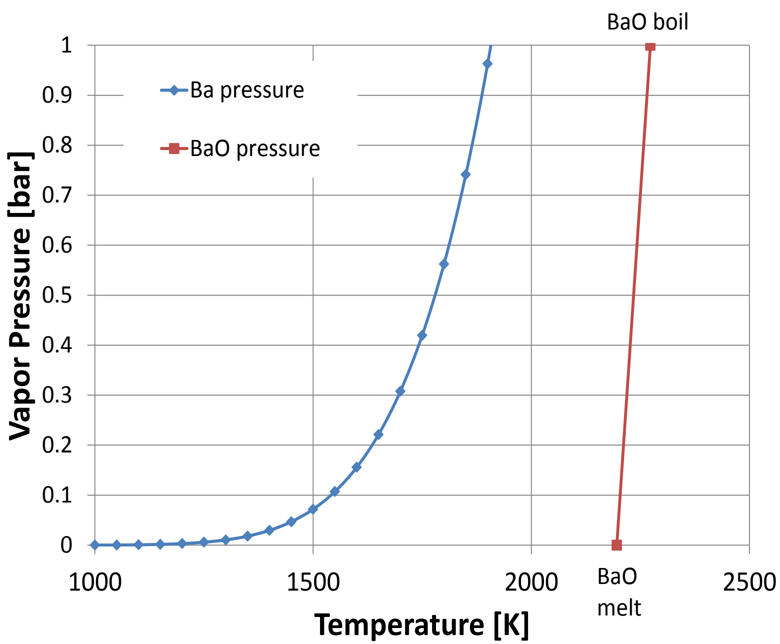


Non-condensable gas and H_2 generation in MCCI stage of STSBO and LTSBO



Barium Volatility Determined by Speciation

- Ba speciation determined by Vanessa code
  - Runs with CORCON in MELCOR
- Vanessa calculates equilibrium of complex chemical system
  - Minimizes Gibbs energy
  - Gibbs energy is temperature dependent G(T)
- Presence of unoxidized Zr permits Ba-metal speciation
- Strong gas sparging in MCCI drives mass transfer
  - Products of reaction swept out of melt
  - Drives reactions to the "right"

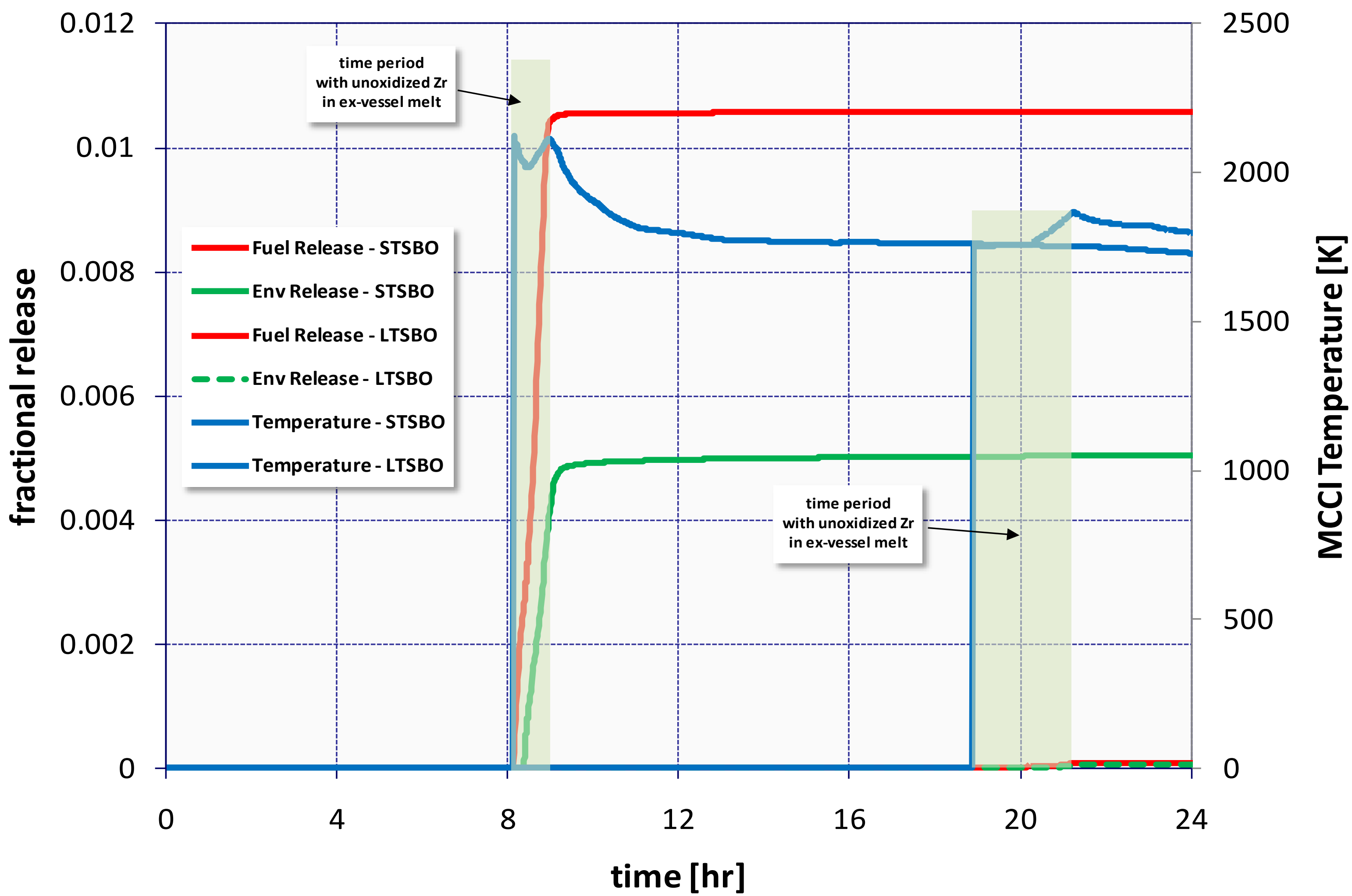


Cerium Release

- Ce and to a lesser extern La show similar trends of enhanced release in the STSBO BWR analysis, again attributable to the intense but brief period of Zr driven chemical reduction in the ex-vessel MCCI release phase

- In the case of Cerium release, the fraction of the inventory that is released in-vessel is vanishingly small and for both STSBO and LTSBO, Ce release essentially occurs in the ex-vessel MCCI phase

Fractional release of Cerium in STSBO



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Cerium Chemical Reaction

- **The net reaction for release of Ce in the ex-vessel stage is:**

[ ] [ ] [ ]

- **Where the metallic Zr ensures highly reducing conditions are maintained and the vigorous gas sparging through the MCCI melt ensures the equilibrium is shifted towards the right hand side of the equation**

Barium and Cerium Conclusions

- **While not readily apparent by MELCOR output, the partial pressure of hydrogen in the MCCI melt for the STSBO is much larger than in the LTSBO due to more rapid Zr oxidation and higher melt temperature, both effects responsible for the much larger predicted Ba and Ce release**

- **Again, as with the Ba release when the metallic zirconium becomes consumed the sparging rate of non-condensable gas decreases and the reduction potential is diminished considerably and the release of Ce falls off**

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Conclusions on Barium / Cerium Release

Summary Discussion

U.S.NRC
UNITED STATES NUCLEAR REGULATORY COMMISSION
Protecting People and the Environment

Updated Surry Analysis ISLOCA Reanalysis

ACRS Subcommittee Meeting

April 25, 2012

Overview

- ISLOCA analysis required complete revision
  - Licensee reviewed SOARCA Surry report in 2010
    - Licensee identified that the release pathway into Aux building which afforded significant deposition is filled with penetration sealant
    - Original ISLOCA analysis had significant deposition in Aux building
    - Original ISLOCA analysis had LHSI pumps flooding in 2 minutes
- Site visit and plant walkdown – January 2011
  - Developed new detailed model of Safeguards Area, Containment Spray Pump Area and Main Steam Valve House
  - Developed detailed ventilation system model
  - Developed LHSI piping model
  - Additional review of mitigation

Overview

- Added new models to MELCOR
  - Turbulent deposition in straight pipes
  - Inertial deposition in elbows and other flow irregularities
  - Validated against LACE tests and benchmarked against VICTORIA
- Results
  - Core damage began in 13 hours (previously estimated 9 hours)
    - Operator actions (by procedure) to stop and isolate both LHSI pumps and to stop 2 of 3 HHSI pumps
    - Operators used Surry plant simulator to estimate times to stop and isolate LHSI pumps
  - Iodine and cesium releases of 16% and 2%, respectively. (previously estimated 9% for both iodine and cesium)
    - Significant deposition in RCS, LHSI piping
    - Iodine revaporization from LHSI piping with subsequent deposition in Safeguards Area and ESF Filtration which serves Safeguards Area

Overview

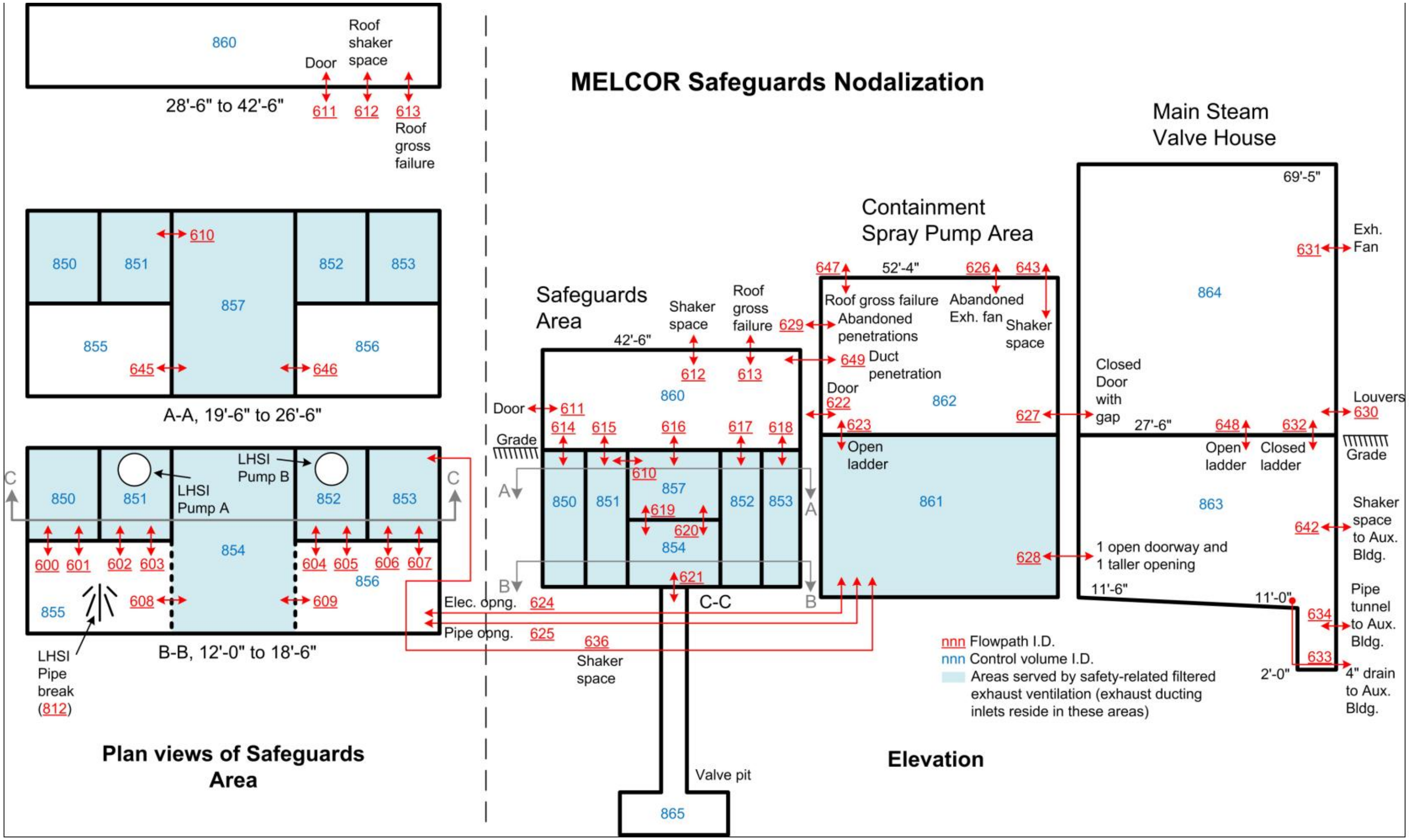
- Used MELCOR analysis to identify conservatisms in PRA model and additional mitigation measures that are practical (e.g., use of RHR prevents core damage)

Approach to ISLOCA Reanalysis

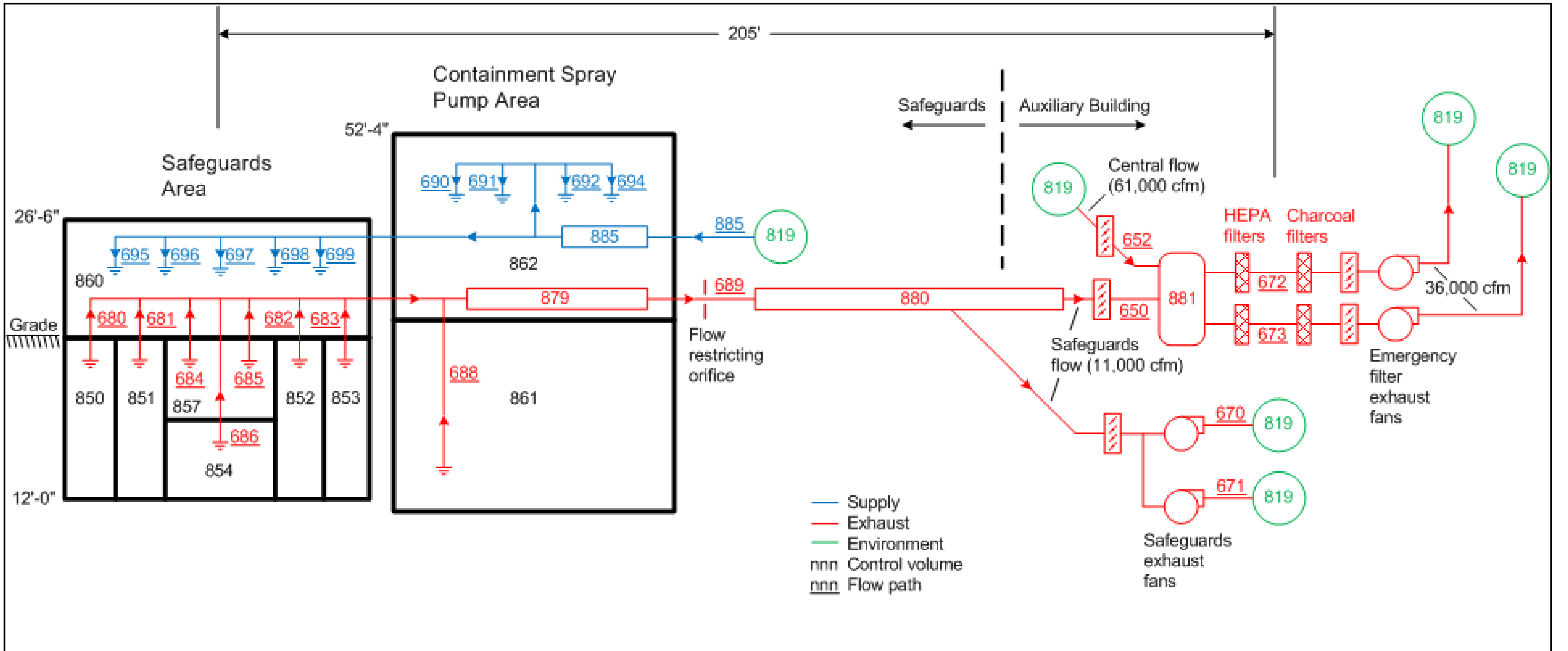
- Developed new full plant MELCOR model for integrated analysis of ISLOCA
  - Includes detailed modeling of Safeguards Area, Containment Spray Pump Area, Main Steam Valve House and ventilation system
  - Uses DFs from separate effects MELCOR model for LHSI piping
    - separate effects model incorporates new deposition models
      - turbulent deposition in straight pipes and inertial deposition in elbows, venturis, sudden contractions

Full Plant MELCOR Model

Safeguards Area, Containment Spray Pump Area, Main Steam Valve House



Full Plant MELCOR Model ESF Ventilation System



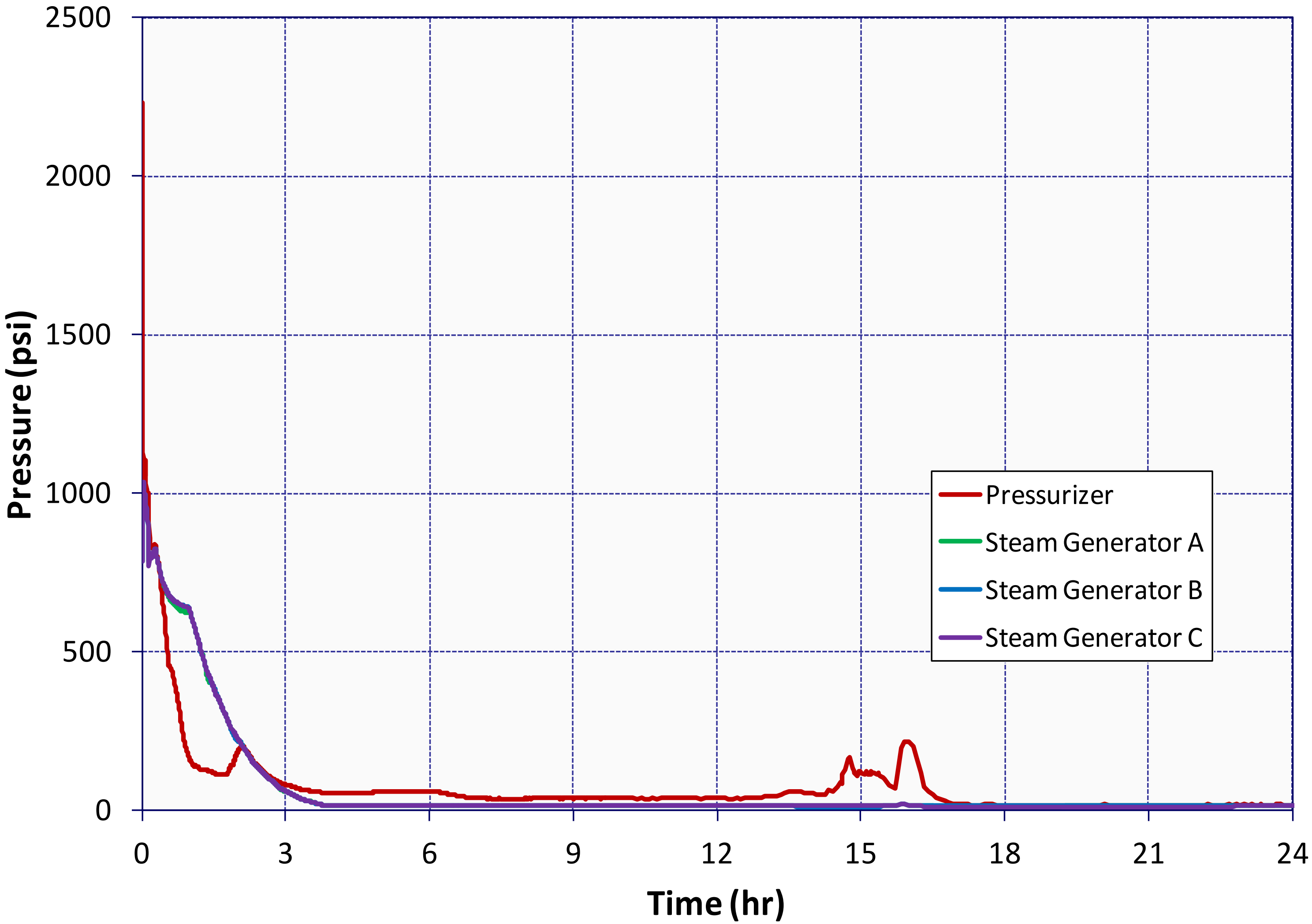
MELCOR Representation of Safeguards Ventilation

Unmitigated ISLOCA – No RWST Refill

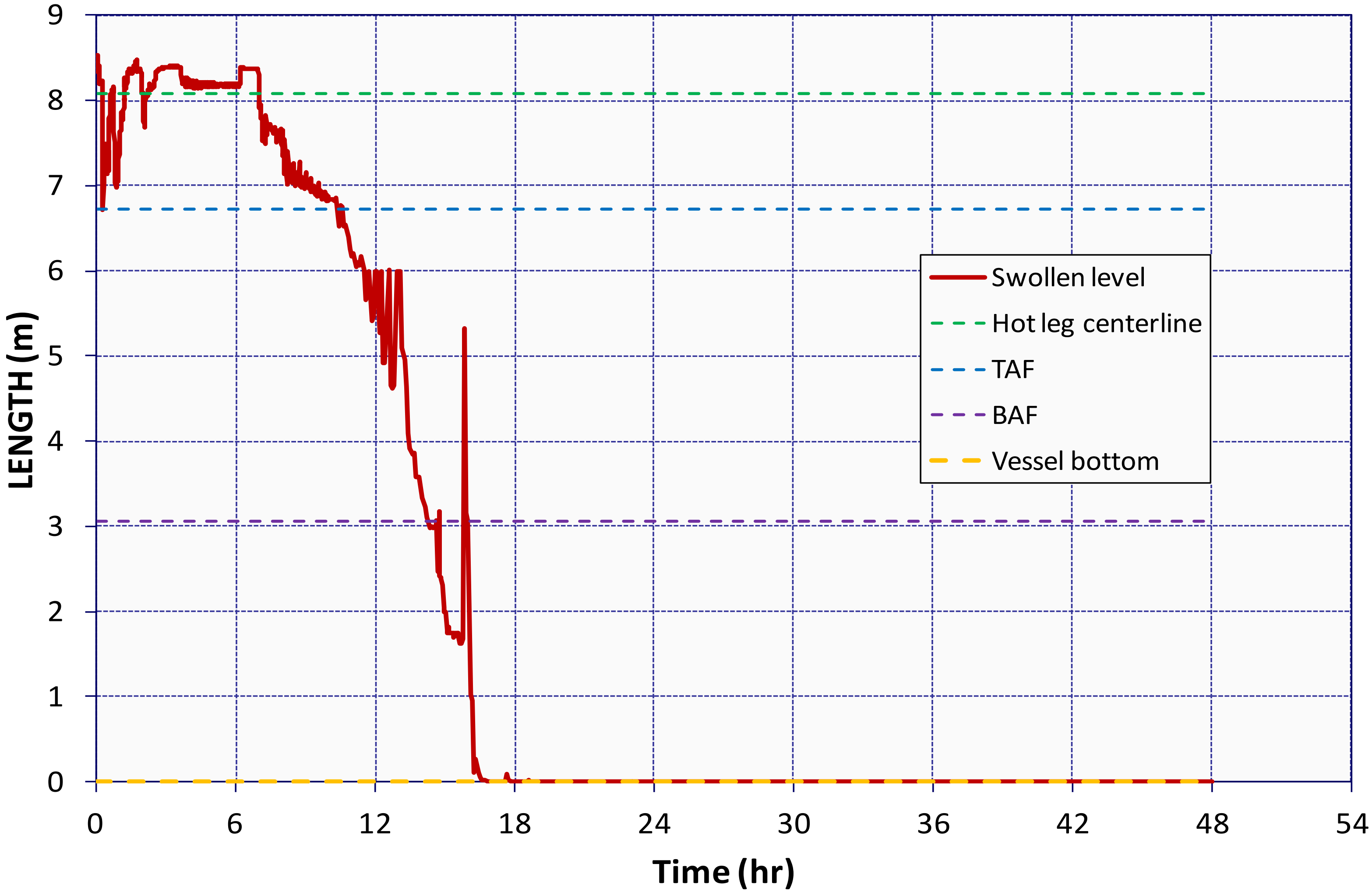
Event Description	Time (hh:mm:ss)
LHSI check valves fail	00:00:00
LHSI piping ruptures in Safeguards Area (outside Containment)	00:00:00+
Safeguards Area personnel door opens	00:00:16
SCRAM	00:00:22
ECCS initiates	00:00:26
Safeguards Area filtered exhaust ventilation system starts	00:00:26
Safeguards Area roof flashing tears	00:00:36
LHSI isolation valve MOV 1890C motor floods (valve inoperable)	00:02:41
MSVH/Aux. Bldg. pipe tunnel opens (penetration sealant dislodges)	00:04:13
Operators stop LHSI Pump A*	00:06:17
Operators secure 1 of 3 HHSI pumps	00:15:00
Operators stop LHSI Pump B*	00:15:44
Operators isolate LHSI pump suctions*	00:16:18
Accumulators begin discharging	00:28:27
Operators begin cooldown	01:00:00
Accumulators exhausted	01:12:00
Operators secure 2 of 3 HHSI pumps	01:45:00
RWST exhausted, HHSI ends	06:12:00
Water level at TAF	10:15:00
First fuel rod gap release	12:49:00
First hydrogen burn	13:29:00
Release of 1% of core inventory of iodine to environment	13:39:00
Safeguards roof fails grossly (from hydrogen burn)	13:54:00
Reactor lower head fails	18:34:00

*Operators used Surry plant simulator to estimate times to stop and isolate LHSI pumps.

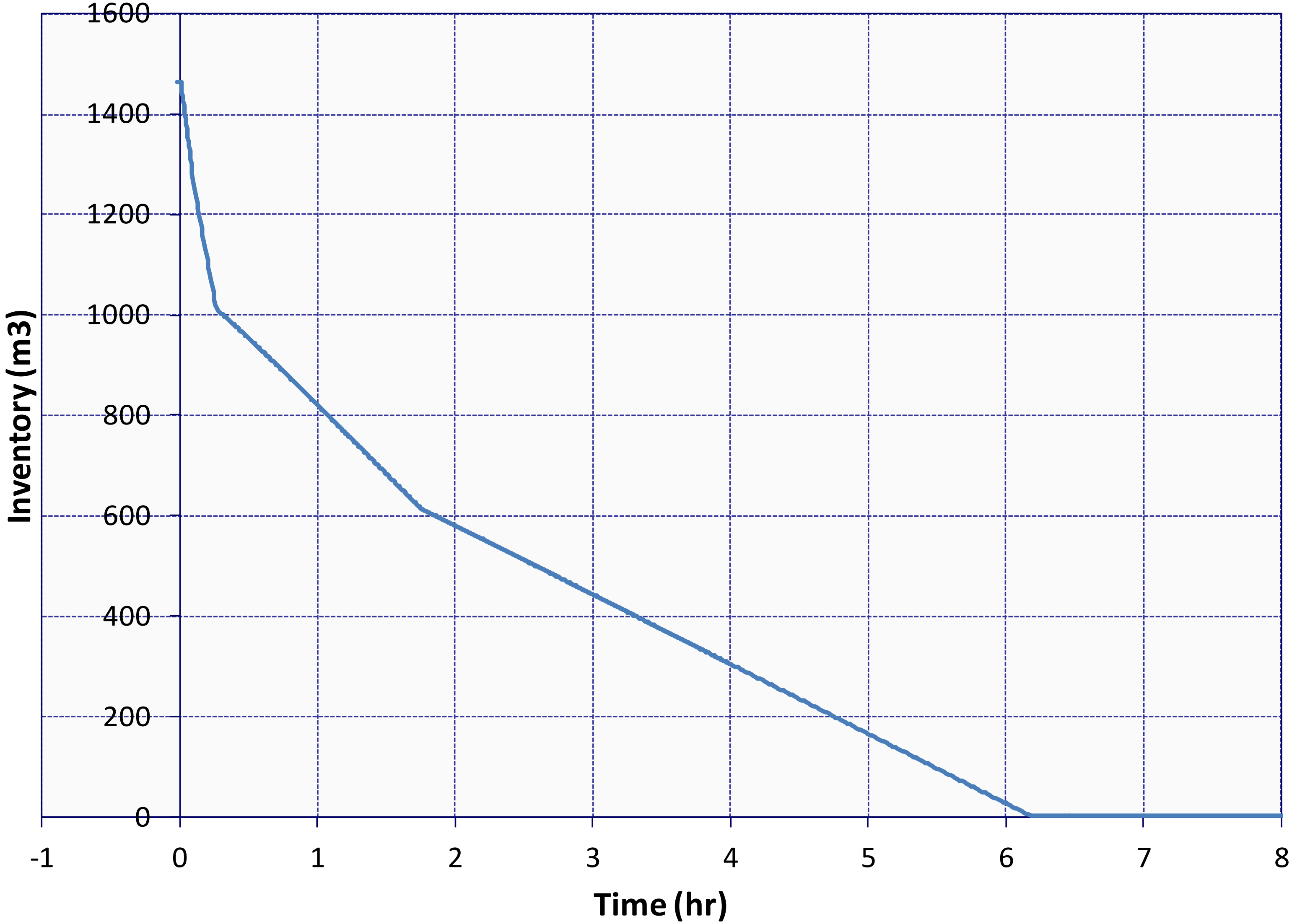
RCS Pressure



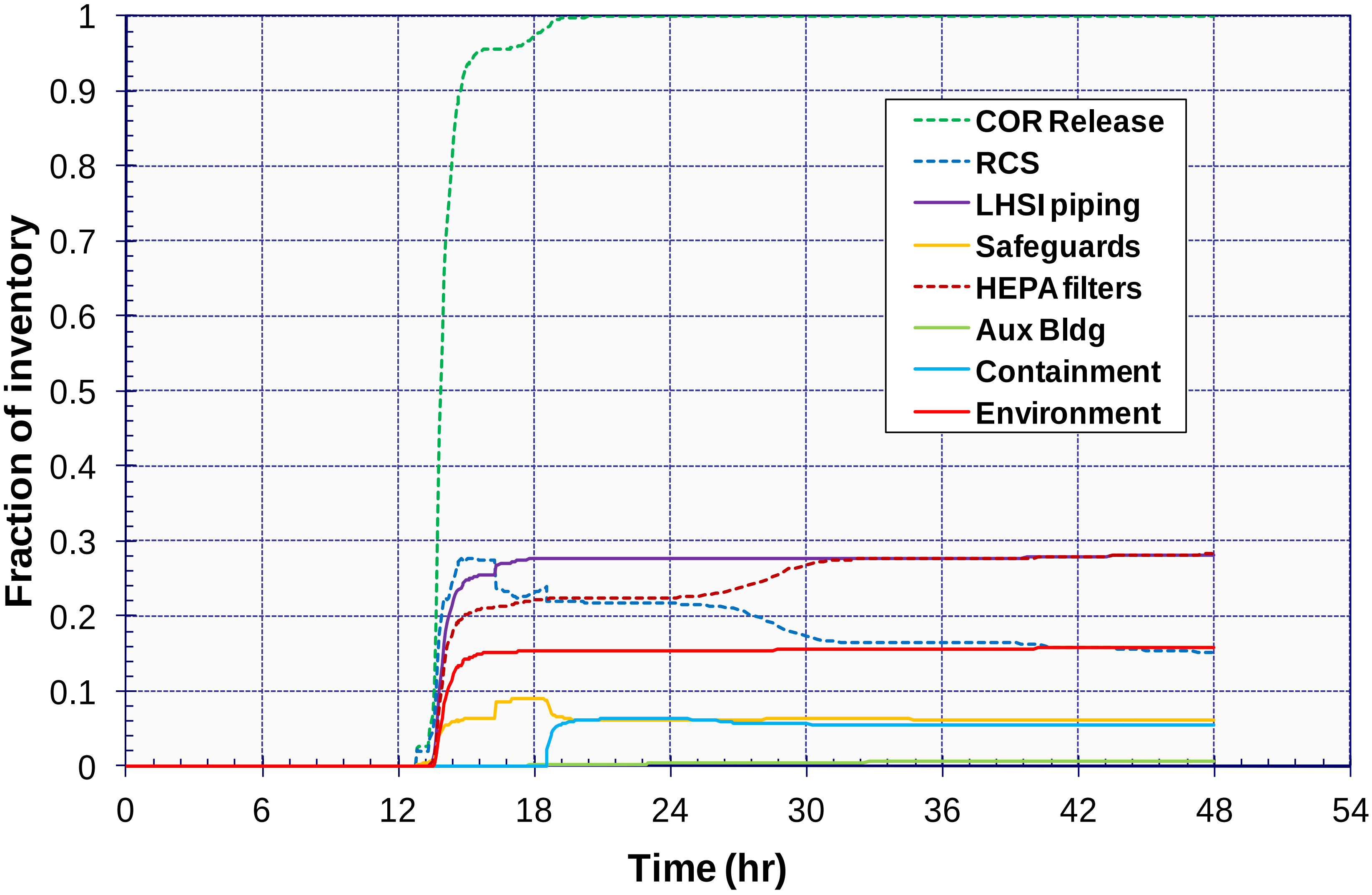
Reactor Vessel Water Level



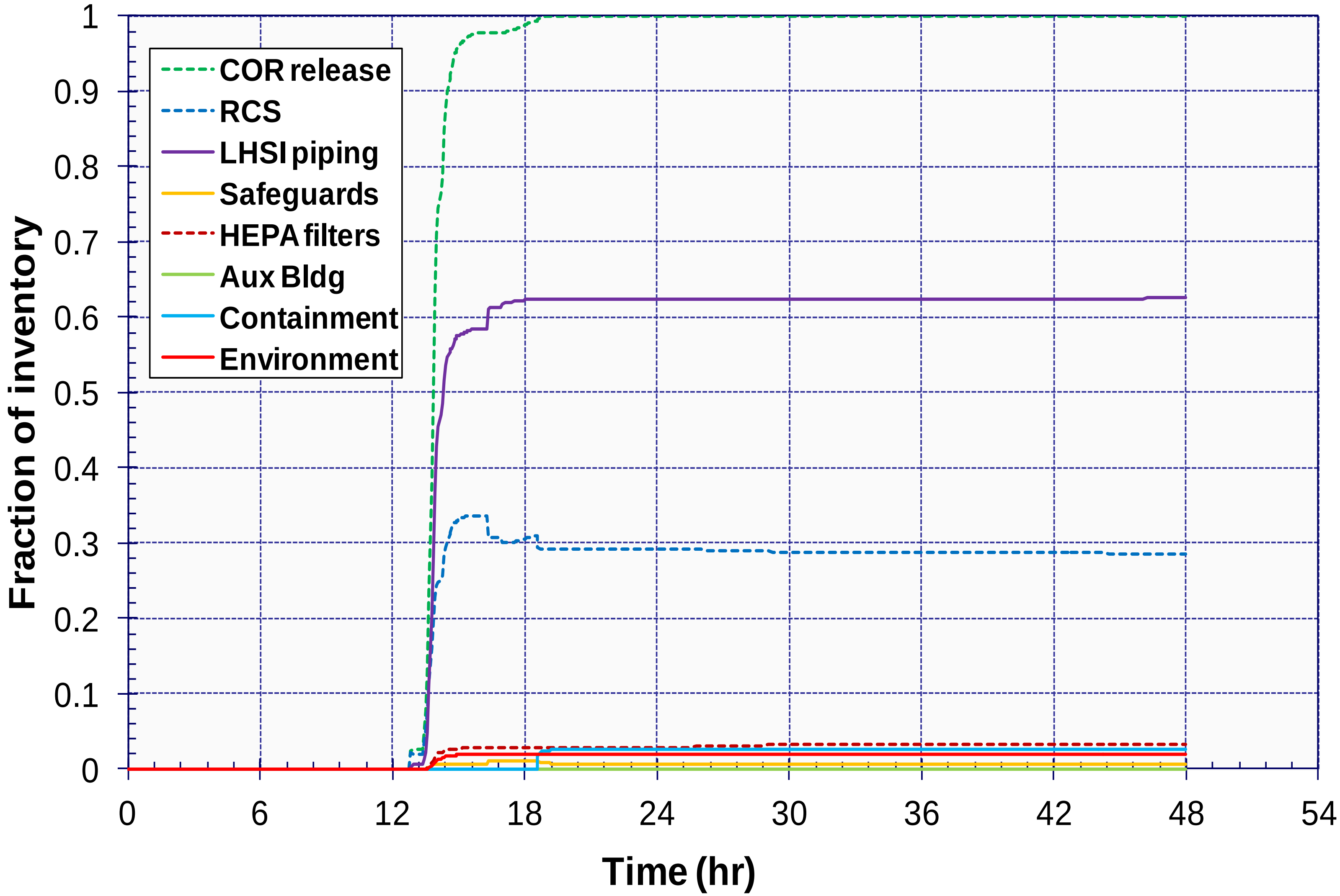
RWST Inventory



Iodine Distribution

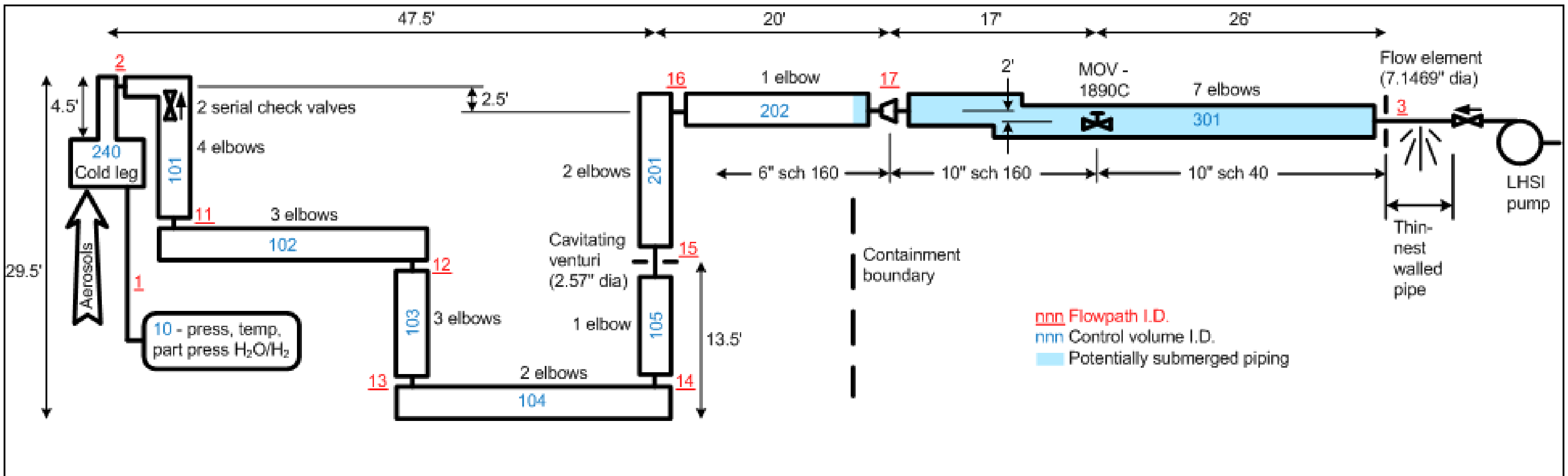


Cesium Distribution



Separate Effects MELCOR Model for LHSI piping

- Separate effects MELCOR model developed to estimate DFs in LHSI piping for full plant MELCOR model

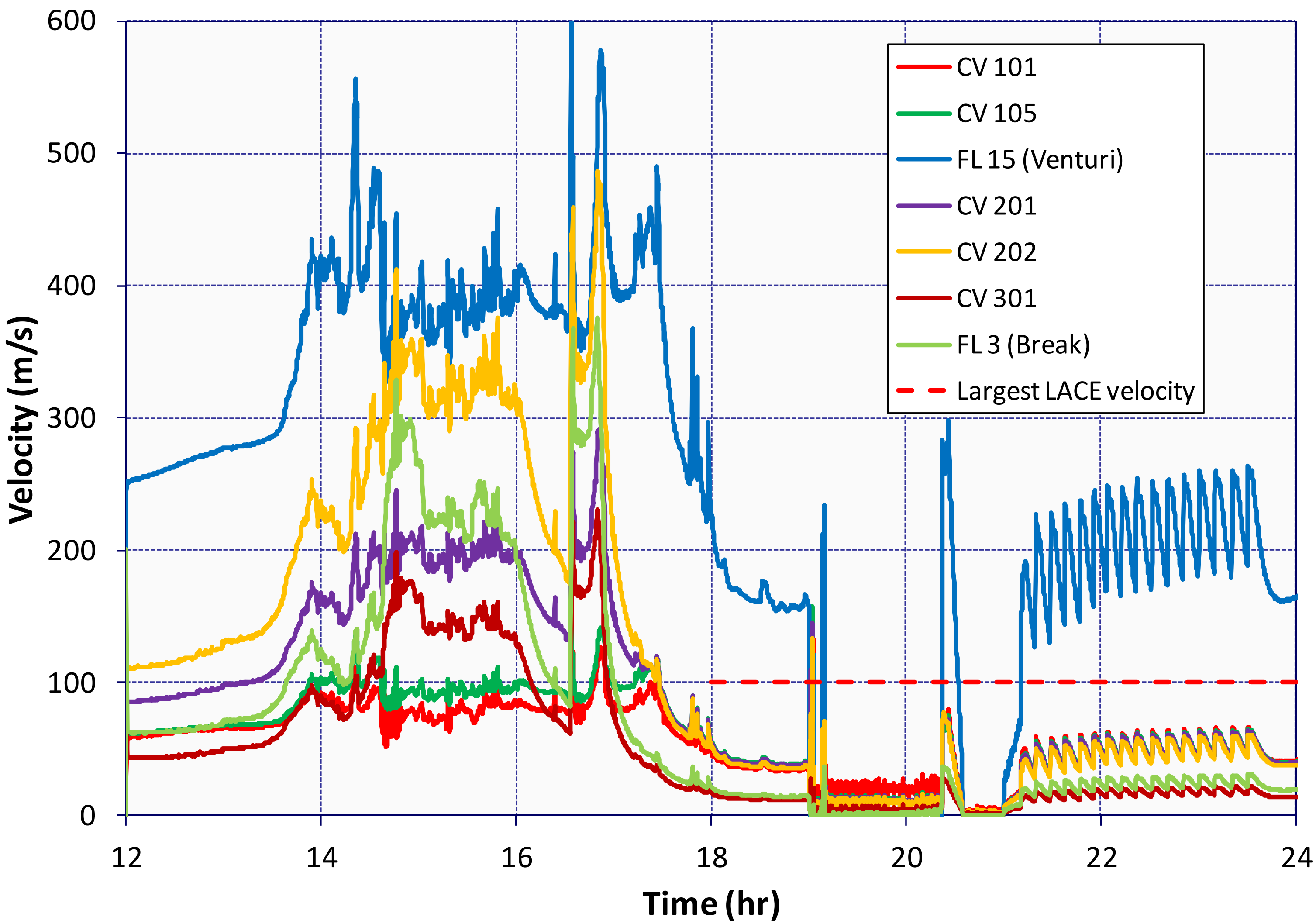


MELCOR Representation of Low Head Safety Injection Line

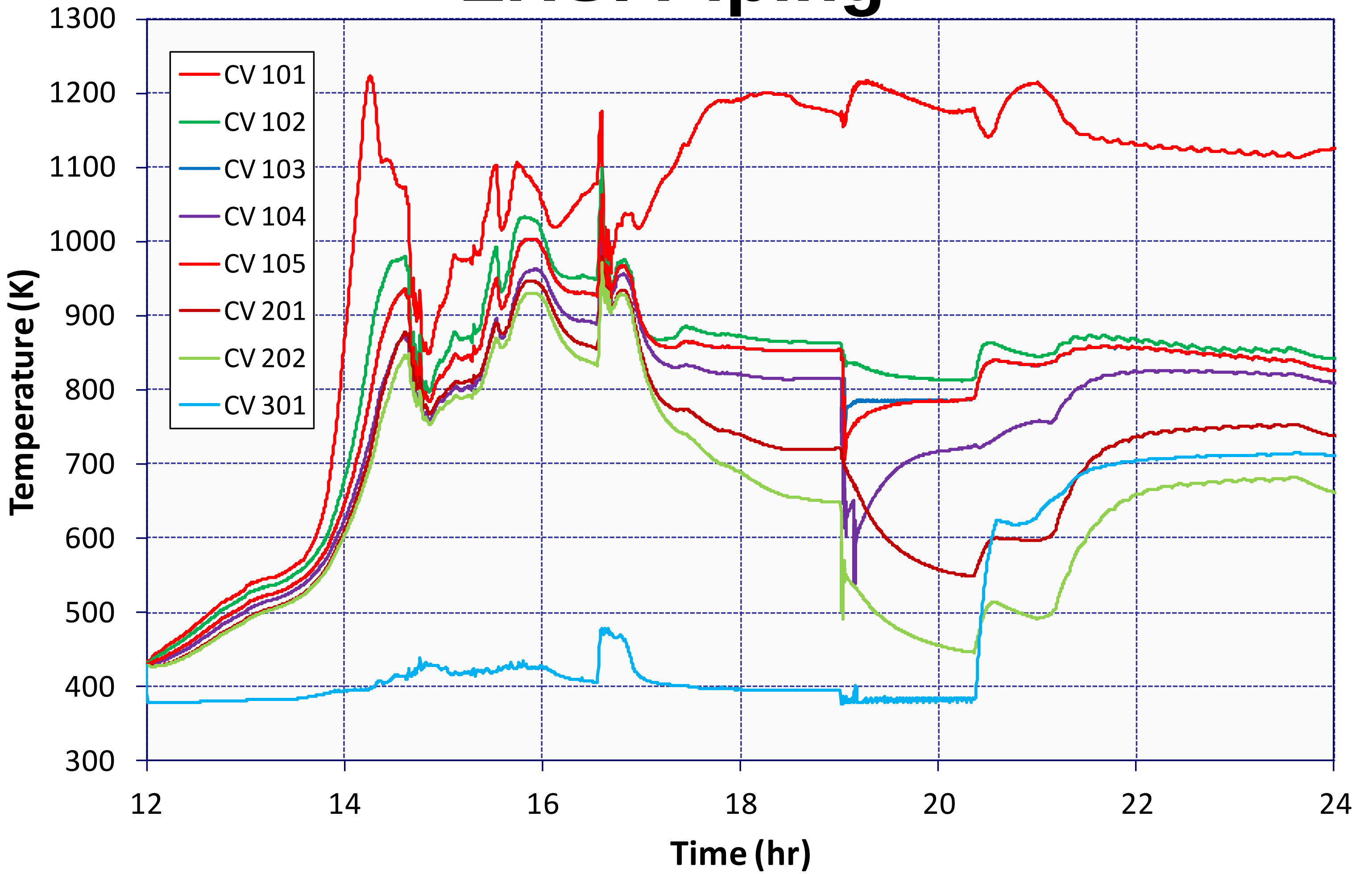
Separate Effects MELCOR Model for LHSI piping

- MELCOR has traditionally modeled aerosol deposition under typical severe accident conditions (VICTORIA used for special conditions)
  - Settling, thermophoresis, diffusiophoresis
- Added new modeling to treat deposition under turbulent flow conditions
  - Turbulent deposition in straight pipes (184 feet)
  - Inertial deposition in elbows (23), venturi (4), sudden contraction (1)
- Validated against LACE test results and benchmarked against VICTORIA modeling

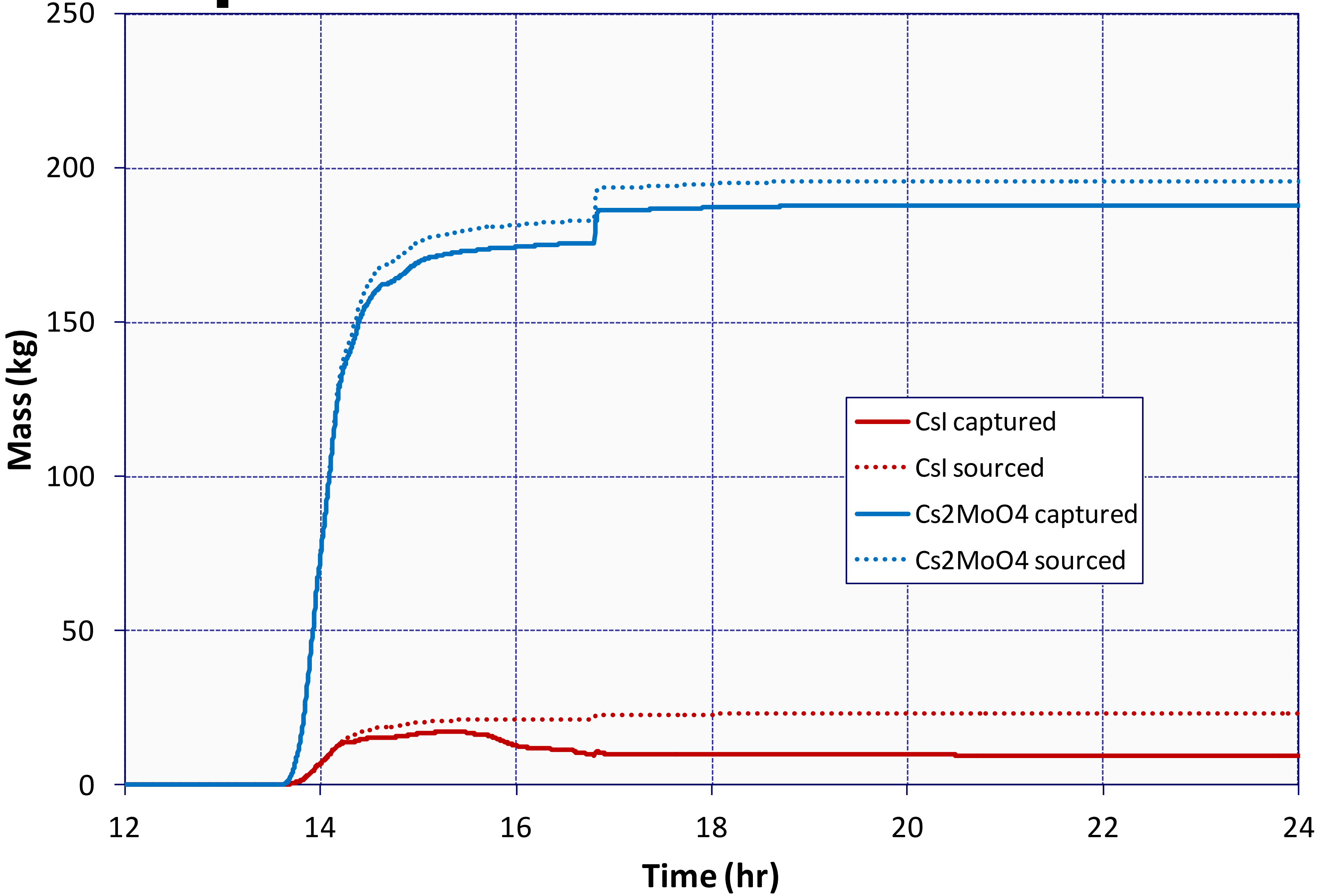
Separate Effects Calculation Vapor Velocity in LHSI Piping



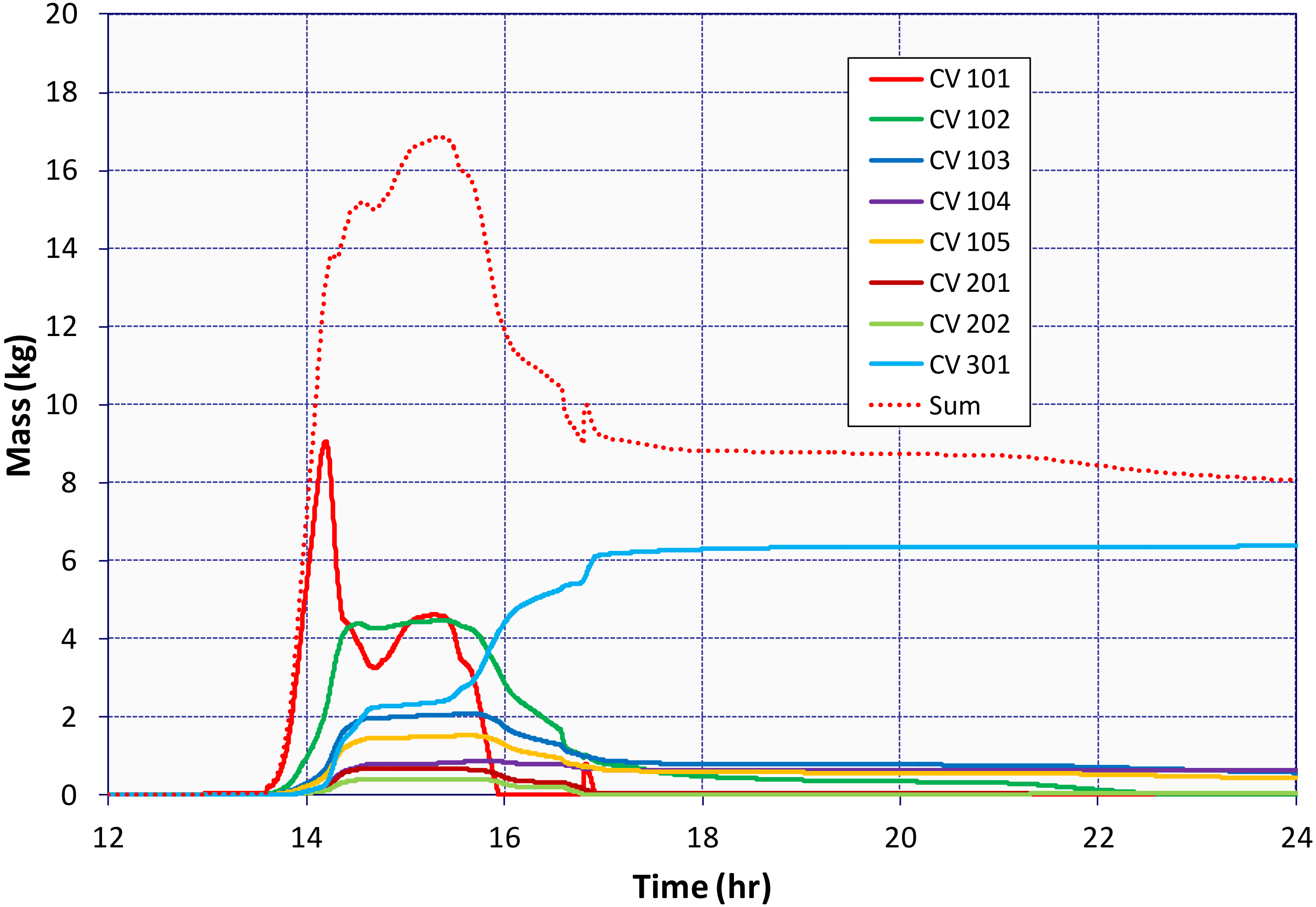
Separate Effects Calculation Inner Wall Temperature in LHSI Piping



Separate Effects Calculation



Separate Effects Calculation CsI Deposition in LHSI Piping



Unmitigated ISLOCA – No RWST

- Results
  - Core damage began in 13 hours (previously estimated 9 hours)
    - Operator actions (by procedure) to stop and isolate both LHSI pumps and to stop 2 of 3 HHSI pumps
    - Operators used Surry plant simulator to estimate times to stop and isolate LHSI pumps
  - Iodine and cesium releases of 16% and 2%, respectively. (previously estimated 9% for both iodine and cesium)
    - Significant deposition in RCS, LHSI piping
    - Iodine revaporization from LHSI piping with subsequent deposition in Safeguards Area and ESF Filtration which serves Safeguards Area

Mitigation of ISLOCA

- Unmitigated ISLOCA – catastrophic failure of 2 check valves and no RWST refill or cross-connect
  - RWST empty at 6 hrs, core damage starts at 13 hrs – ample time to refill RWST
- Unmitigated ISLOCA calculation included operator action (by procedure) to stop and isolate both LHSI pumps and to stop 2 of 3 HHSI pumps
  - During October 2011 site visit, we learned that, by procedure, operators also would throttle the remaining HHSI pump
    - Delays emptying RWST and starting core damage by an additional 24 hours
      - HHSI flow at 6 hours (600 gpm without throttling, 150 gpm needed)
      - HHSI flow at 24 hours (100 gpm needed)

Mitigation of ISLOCA

- Core damage could be averted by starting RHR
  - RHR entry conditions established by 2 hrs and 40 min
  - RWST not empty until 6 hrs (without throttling remaining HHSI pump)
- Opening pressurizer PORVs reduces the fission product release
  - Diverts some fission products to the containment
  - Other RCS-to-containment valves would perform similar function

Frequency of ISLOCA

- SPAR model – 3×10^{-8}/ry
- Licensee PRA model – NRC originally understood to be 7×10^{-7}/ry
- During recent site visits, NRC learned that licensee PRA model had 2 ISLOCA scenarios:
  - Catastrophic failure of 1 check valve and up to 300 gpm leak-by of second check valve – 7×10^{-7}/ry
  - Catastrophic failure of 2 check valves – 3×10^{-8}/ry
- 3×10^{-8} does not meet SOARCA screening criterion of 10^{-7}
  - NRC elected to retain scenario because it has been commonly identified as an important contributor in PRA

Conclusions – Bypass Accidents

- SOARCA analysis of ISLOCA and SGTR used more detailed modeling of operator actions and plant systems and improved phenomenological modeling in MELCOR
- Fission Product Releases
  - Releases from bypass accidents are smaller and more delayed than in earlier studies. Bypass accidents do not result in large early releases.
- Accident Mitigation
  - SOARCA analysis provides insight into conservatisms in PRA model and leads to identification of additional mitigation measures that are practical (e.g., use of RHR prevents core damage)

State-of-the-Art Reactor Consequence Analyses (SOARCA) Project

Comparing SOARCA with the Fukushima Accident

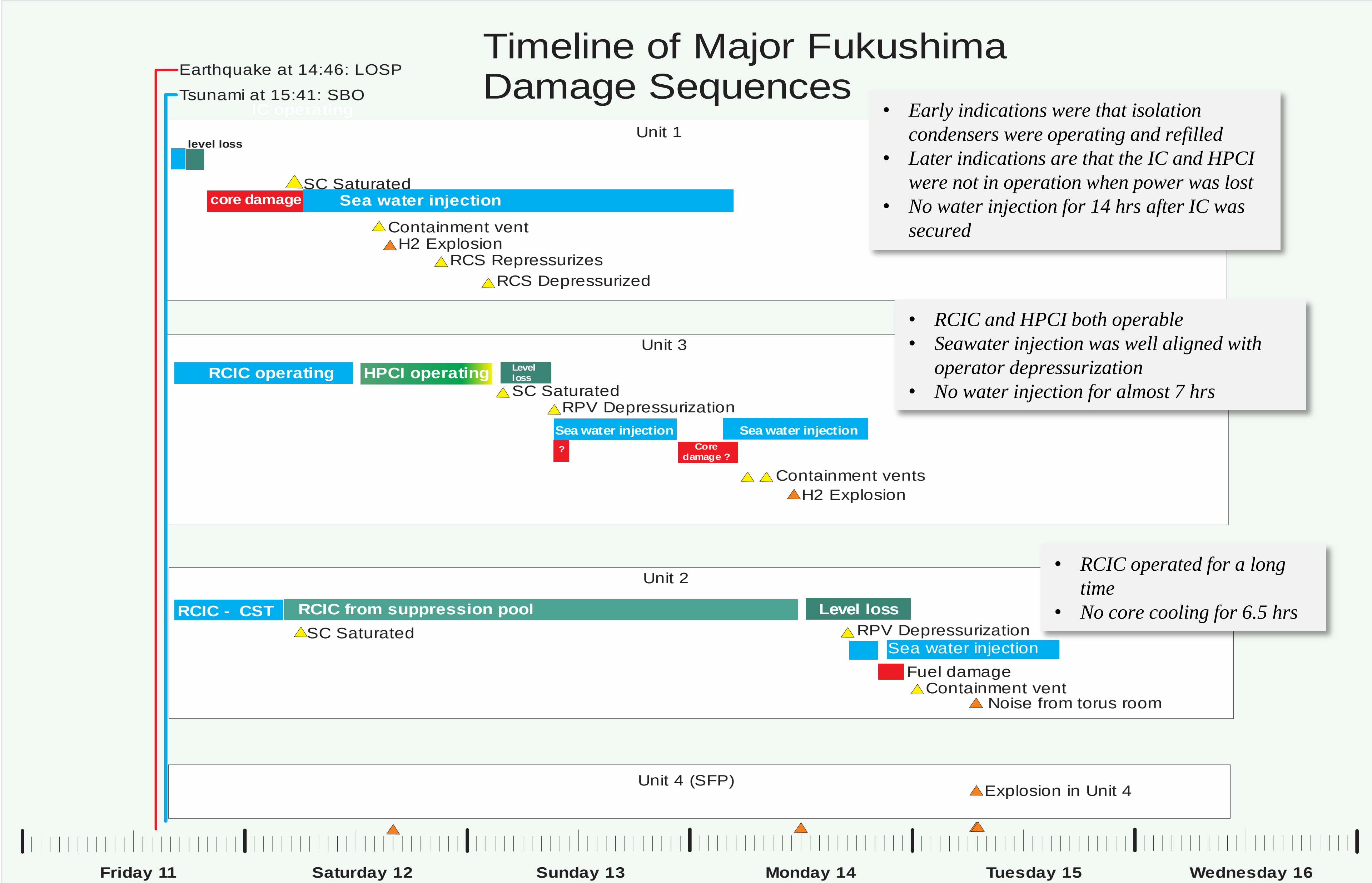
Presented to the
Advisory Committee on Reactor Safeguards
Washington, DC
April 2011

Dr. Randall O. Gauntt, Sandia National Laboratories

Overview

- RCIC operation
- Hydrogen release and combustion
- 48 hour truncation
- Multiunit risk
- Spent fuel pool risk/consequences

Illustration of Major Fukushima Damage Sequences Developed for Discussion Only



Information within this illustration was developed from the INPO 11-005 Special Report and the Report of Japanese Government to IAEA Ministerial Conference on Nuclear Safety – Accident at TEPCO's Fukushima Nuclear Power Stations – Transmitted by Permanent Mission of Japan to IAEA, June 7, 2011

Operation of RCIC

	Start of RCIC operation (hours)	End of RCIC operation (hours)	Duration of RCIC operation (hours)
Fukushima Unit 2	0	70	70
Fukushima Unit 3	0	21	21
Peach Bottom unmitigated LTSBO	0	5	5
Peach Bottom unmitigated STSBO with RCIC blackstart	1	3	2
Peach Bottom unmitigated STSBO without RCIC blackstart	Not modeled	Not modeled	N/A

Hydrogen Release and Combustion

Photo modified from SAND2012-3221C

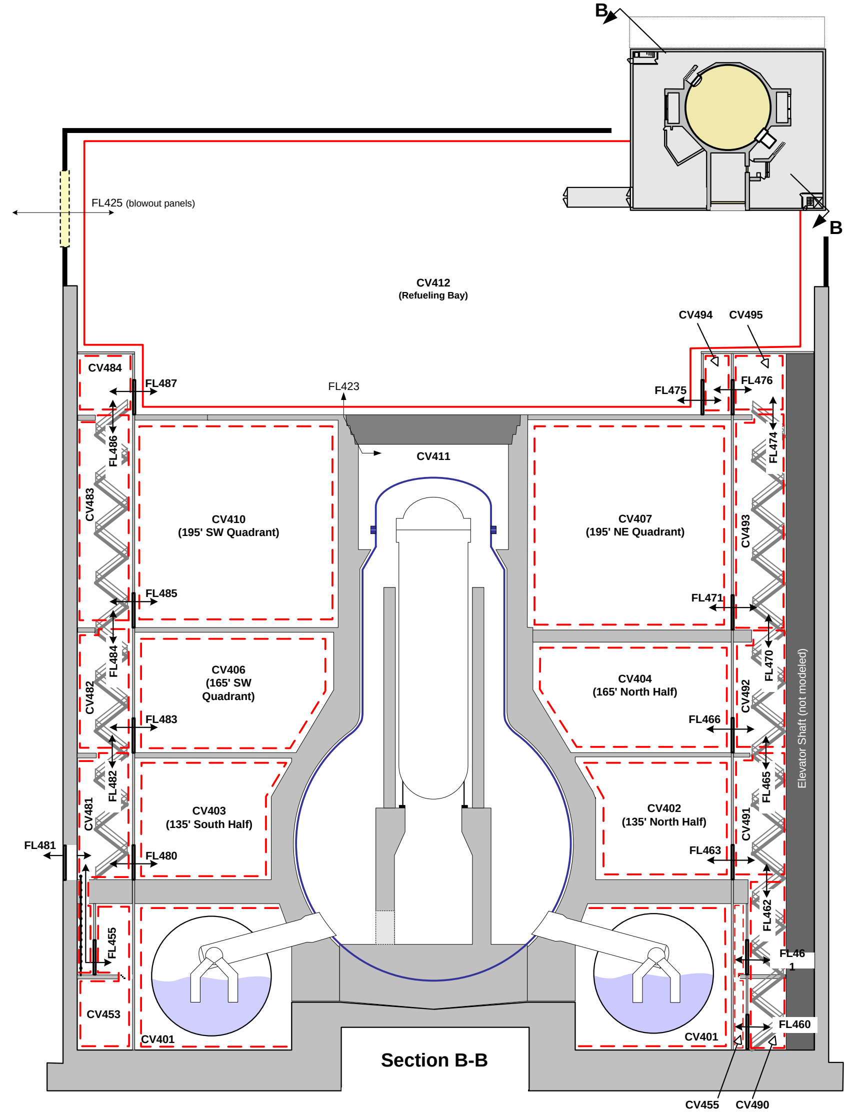
Unit 3

Unit 1

A Government of Japan report suggests the hydrogen release pathway was leakage through the drywell head flange

Peach Bottom LTSBO Hydrogen Concentrations

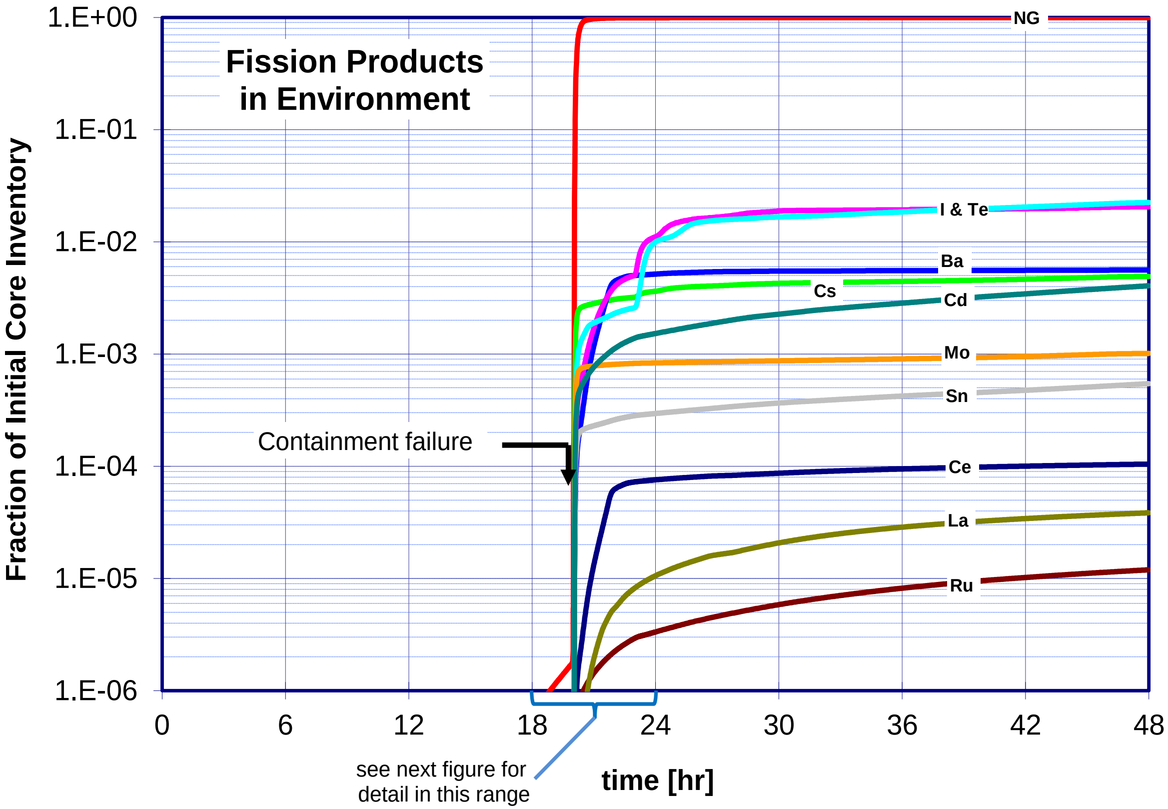
SOARCA models leakage through the drywell head flange



National Nuclear Security Administration

Sandia National Laboratories

48 hour Truncation for Releases



Peach Bottom LTSBO

The 48 hour truncation time for SOARCA was based on the availability of the many resources at the State, regional, and national level to mitigate the accident

- If all other options failed, the ability to flood the reactor containment could occur within 48 hours

Multiunit Risk

- Multiunit risk was an item considered for follow-on research.
- This will be included in the Level 3 PRA

Spent Fuel Pool Risk/Consequences

- Spent fuel pool risk was an item considered for follow-on research.
- This will be included in the Spent Fuel Pool Scoping Study and the Level 3 PRA.

PEER REVIEW OF THE STATE-OF-THE-ART REACTOR CONSEQUENCE ANALYSIS (SOARCA) PROJECT

Jeff Gabor
ERIN Engineering & Research, Inc.

Presented to the Advisory Committee on Reactor Safeguards
April 25, 2012

Overview

- Peer Review Objectives
- Peer Review Committee Members
- Peer Review Process
  - Committee Charter
  - Review Scope
  - Coverage of SOARCA Topics by Committee Members
- Individual Assessments by Peer Reviewers
- Documentation Provided Throughout the Peer Review

Peer Review Objectives

- The main objectives:
  - to provide independent reviews by each Committee member of the technical work conducted within the SOARCA project
  - to assure that the SOARCA study is technically accurate

- Guidance with respect to specific issues and comments on the effectiveness of presentation within the SOARCA NUREG documents to the public have also been offered.

Peer Review Committee Members

- Ken Canavan, Electric Power Research Institute (EPRI)
- Bernard Clément, Institut de Radioprotection et de Sûreté Nucléaire
- Jeff R. Gabor, ERIN Engineering
- Robert E. Henry, Fauske and Associates
- Roger B. Kowieski, Natural and Technological Hazards Management Consulting, Inc. (NTHMC)
- David E. W. Leaver, WorleyParsons Polestar
- Bruce B. Mrowca, Information Systems Laboratories
- Kevin R. O’Kula, URS Safety Management Solutions
- John D. Stevenson, JD Stevenson Consulting Engineer Company
- Karen Vierow, Texas A&M University
- Jacquelyn C. Yanch, Massachusetts Institute of Technology

Review Scope

- The documents reviewed included:
  - draft SOARCA NUREG documents
  - presentation materials provided at Peer Review Committee meetings
  - Peer Review comment resolution documents
  - Uncertainty Analysis Plan
  - Supporting documents that were supplied at the Committee's request.
- Latest draft SOARCA NUREG available to the Committee at the time of preparation of final deliverable:
  - Main Report (Dec. 23, 2011)
  - Volume 1 (Oct. 12, 2011)
  - Volume 2 (Nov. 17, 2011)

Review Scope

- The scope of the review does not include:
  - the Uncertainty Quantification and Sensitivity Analysis
  - editorial review of the SOARCA documents
  - review of the public brochure

Coverage of SOARCA Topics

- Accident sequence selection
  - Ken Canavan
  - Bruce Mrowca
- Accident progression
  - Ken Canavan
  - Bernard Clément
  - Jeff Gabor
  - Robert Henry
- Mitigation measures
  - Jeff Gabor
  - Robert Henry
  - Bruce Mrowca

Coverage of SOARCA Topics

- Radiological release
  - Bernard Clément
  - Jeff Gabor
  - Robert Henry
  - David Leaver
- Off-site emergency planning and response
  - Roger Kowieski
  - David Leaver
- Off-site radiological consequences
  - David Leaver
  - Kevin O’Kula
  - Jacquelyn Yanch

Coverage of SOARCA Topics

- Seismic issues
  - John Stevenson
- Structural issues
  - John Stevenson
- Probabilistic Risk Assessment applications
  - Ken Canavan
  - Bruce Mrowca
- Severe accident modeling
  - Jeff Gabor
  - Robert Henry
  - Karen Vierow

Individual Assessments

- All of the written materials which were provided to the SOARCA team by the reviewers have been assembled by and coordinated through the Peer Review Committee chair.
- Each reviewer’s assessment of SOARCA has been transmitted as received, without editing or other modification by the chair.
- A consensus opinion of the Committee has not been pursued during the review process.

Individual Assessments – Canavan (1/2)

- The SOARCA study has largely met its objectives.
- Consequence Analysis:
  - While effort in SOARCA is expended to attempt to ensure that that the most significant sequences, in terms of both frequency of occurrence and related consequences are chosen, there are always questions as to the rigor of this process in any analyses of this type.
  - There is the possibility that certain accident sequences, while not dominant, may have higher risk than would be indicated by the frequency of the accident sequences due to an increased consequence.

Individual Assessments – Canavan (2/2)

- Plant-specific nature of SOARCA
  - Because these plants do not encompass all of the design, maintenance and operation practices across the nuclear fleet, some conclusions are likely applicable to that site only and the results may not be typical.
- Individual Accident Sequences
  - As part of the SOARCA review, the accident sequences criteria used in the SOARCA study were applied generically to various accident sequences in previously published PRA studies. However, it should be noted that this review was informal and generic. Plant-specific application could produce different results.
  - Changes in assumptions or the state of knowledge of certain phenomena could influence the results of the analysis and further limit the usefulness of the final result.

Individual Assessments – Clement (1/2)

- The SOARCA project succeeded in achieving the objectives of updating quantification of offsite consequences.
- Uncertainties exist concerning the first failure locations. This was not addressed for RPV failure.
- Due to a shortcoming in MELCOR's FP behavior treatment, a gaseous iodine source term was "superimposed" on MELCOR's source term. This might not give consistent results for sequences that were not studied in the SOARCA.
- Addressing the uncertainties issue will increase robustness of the results and confidence in the conclusions.

Individual Assessments – Clement (2/2)

- Recommendations:
  - Revise SOARCA documentation according to new PRA results if their outcomes make it useful.
  - Address other pilot plants using the SOARCA methodology.
  - Benchmark SOARCA evaluations of selected sequences with future versions of MELCOR
  - The likelihood of TI-SGTR for Surry should be re-assessed using update flaw distributions and material changes when such data is available.

Individual Assessments – Gabor (1/3)

- SOARCA represents a major advancement in our understanding of severe accident progression and radionuclide release.
- SOARCA successfully addressed the major objectives of the project related to severe accident progression.
  - However, due to the primarily deterministic approach taken, great care must be taken in communicating these results in a risk context to the public.

Individual Assessments – Gabor (2/3)

- The focus on individual accident progression scenarios in a deterministic framework has limitations.
  - The consequences are a strong function of the path selected.
  - It is not sufficient to characterize the potential consequences of a severe accident scenario using a single accident progression analysis, even if it is the best estimate case.
  - The presentation of risks must be made in a fully probabilistic framework, rather than the SOARCA project's quasi-probabilistic framework.

Individual Assessments – Gabor (3/3)

- The SOARCA project does not adequately address the uncertainty in severe accident phenomena.
  - A full appreciation of the results and uncertainties can only be accommodated in a fully probabilistic assessment addressing the applicable uncertainties.
  - This is being addressed in a separate program.
- It is important that the largely deterministic analytical techniques employed in the SOARCA project be extended into true risk frameworks in order to more completely characterize the results and communicate risks.

Individual Assessments – Henry (1/3)

- The SOARCA activity is a major step forward in developing credible, integral analyses for severe accident sequences to be used in regulatory decision-making.
- Accident sequence selection:
  - The focus on the sequences of Station Blackouts and containment bypass is appropriate
  - The Fukushima accidents validate the selection of the SBO sequences for the SOARCA.
  - Equally important is the role of the RCIC system, that is conservatively modeled in the SOARCA accident response evaluations.

Individual Assessments – Henry (2/3)

- MELCOR Modeling of the Severe Accident Sequences
  - The MELCOR results are reasonable and benchmarked with appropriate phenomenological experiments except for some aspects of the following:
    - Possible challenges to the integrity of the RCS pressure boundary
      - Flow paths along in-core instruments may lead directly to the containment atmosphere.
      - These flow paths were not modeled in the MELCOR analyses.

Individual Assessments – Henry (3/3)

- A review of the MELCOR "best practices" document is an important component of the documentation that should be reviewed along with each MELCOR accident analysis that is used in specific regulatory decisions.
  - It is recommended that the MELCOR "best practices" include the insights developed from an individual benchmark such that these would be implemented in integral plant analyses going forward.
  - Without such a procedural step, it is not clear what is learned and what is used from the benchmarks.

Individual Assessments – Kowieski (1/1)

- Regarding the off-site emergency response sections:
  - The parameters used and assumptions made in the emergency response modeling were reasonable and adequate.
  - The emergency response timelines used in the modeling are consistent with the actual response action timelines by the off-site response organizations.

Individual Assessments – Leaver (1/3)

- The SOARCA is a substantive, high quality effort which makes a significant contribution to the understanding of US commercial reactor risk.
- The technical quality of the SOARCA is high and provides a major advancement in the state-of-the-art of characterization of integrated severe accident risk in Levels 2 and 3.
  - The transparency, which has been and continues to be a key objective, indirectly supports the quality of the SOARCA project.
  - The internal event Level 1 work utilized the latest information available.
  - The selection process for external event sequences was less clear. Large seismic events should be assessed as a part of a separate, future study integrated into the NRC seismic research program.

Individual Assessments – Leaver (2/3)

- On the matter of completeness of scope, the SOARCA project has taken an approach that is technically sound.
  - Those classes of accident events not considered were identified within the SOARCA and are not likely to substantially alter the SOARCA findings on reactor risk.
  - There would be benefits to applying more detailed best estimate, SOARCA-like methods to some of these classes of events.
  - Extension of SOARCA to other LWR plant types would further strengthen the completeness of effort.
- On the matter of completeness of sequence selection, the Level 1 (CDF) screening process is reasonable from a technical standpoint.
- With respect to accident types and offsite health consequences, there is nothing forthcoming from the Fukushima accident that suggests that the SOARCA selection process overlooked important accident scenarios.

Individual Assessments – Leaver (3/3)

- Observations on reactor safety and public health risks associated with US LWR operation based in light of SOARCA findings:
  - The SOARCA project provided very strong, convincing evidence that LWR risk characterization has been unrealistic and excessively conservative.
  - The 50.54(hh) mitigative measures are important not only because of the risk impact but also because of the provided margin for uncertainties in sequence selection and analysis, making the SOARCA risk predictions even more robust.
  - Perhaps the most significant finding from the SOARCA project is the importance of the role of emergency response and accident management in reducing risk.
    - It is now an appropriate time for the nuclear community to begin consideration of how SOARCA methods and results could be used to further improve LWR safety.

Individual Assessments – Mrowca (1/3)

- The SOARCA project objectives stated in Section 1.2 of the Main Report were only partially achieved.
  - The innovative and state-of-the-art techniques used in the SOARCA analysis are focused on the phenomenological modeling and are not used for the identification of sequences to be modeled or for the application of security-related mitigation improvements.
- With regard to the objective of incorporating significant plant improvement and updates, the SOARCA project stated that the team attempted to accurately reflect plant conditions and use the latest Level 1 information concerning initiators derived from plant-specific models. The practice employed appears reasonable.

Individual Assessments – Mrowca (2/3)

- Sequence Selection
  - The lack of a Level 3, internal and external events PRA resulted in the use of a sequence selection and screening process that does not guarantee completeness.
  - This reviewer found weaknesses in the defensibility and transparency of the process.
  - The case for using the selected screening process is not well made.
    - The analysis concludes that the benefits could most efficiently be demonstrated by applying the methods to a set of the more important severe accident sequences. However, the benefits of realistic analysis can be achieved by selecting any relevant set of sequences.
    - The other SOARCA objectives suggest that it is necessary to capture all or a significant portion of the risk. A more comprehensive approach would appear to be called for.

Individual Assessments – Mrowca (3/3)

- Treatment of Mitigation Measures and Operator Actions
  - The failure probabilities for the additional security-related mitigation actions were not determined, preventing the determination of the mitigated sequence frequencies and the full assessment of their impact.
    - The lack of a human reliability assessment severely limits the credibility of the benefit of these additional actions.
    - It also results in incomplete frequency information.
  - The SOARCA Project did not demonstrate through state-of-the-art techniques that the mitigation improvements objective was achieved.

Individual Assessments – O'Kula (1/2)

- Adequacy of the SOARCA Concept
  - The SOARCA study more than met its goal of applying state-of-art, and valid approaches for evaluating severe accident phenomena and ensuing subsequent offsite consequences.
  - However, some aspects of the offsite consequence analyses maintain older models or input data. These are acceptable for achieving the overall goals on the SOARCA project in most cases but merit some thinking in the direction of upgrades or replacement with other options for later work.
    - Straight-line Gaussian plume segment model
    - Economic consequence model with older data
    - Assumptions and input data associated with decontamination and cleanup of economic assets

Individual Assessments – O'Kula (2/2)

- SOARCA Approach
  - The approach taken for the offsite consequence analysis was comprehensive and met expectations for contemporary standards and assumptions.
  - The SOARCA processes were sufficiently best-estimate with respect to the offsite consequence analysis performed.
  - It will be important to finalize the uncertainty analysis to understand parameter impacts and sensitivities, and where the best-estimate values lie relative to other statistical figures-of-merit.

Individual Assessments – Stevenson (1/2)

- I agree from a natural phenomena hazard standpoint, the greatest threat to the loss of electrical power both from sources outside and inside the two NPP's studied resulting in station blackout is a very low probability of occurrence earthquake.
- The concern raised is that such a level of earthquake for the Surry NPP could lead to the soil liquification or consolidation leading to displacement that might cause containment penetrations to rupture and thereby lead to early containment failure which was identified in the SOARCA study.

Individual Assessments – Stevenson (2/2)

- There is a need to assure that:
  - the severe accident management team is aware of the potential for hydrogen build-up in containment to detonation concentration levels.
  - and for them to develop administrative procedures or install hardware to preclude this detonation level build-up from occurring.
- My overall impression of the SOARCA project and associated documentation is that it is a realistic effort to validate the consequences of a complete loss of cooling to a large commercial BWR and PWR power reactor and as such makes a significant contribution to the understanding of U.S. commercial reactor risk.

Individual Assessments – Vierow (1/3)

- Adequacy of the SOARCA Concept
  - The SOARCA approach for modeling severe accident phenomena is valid, in part, because it is a comprehensive and integrated analysis approach.
  - SOARCA is state-of-the-art for analysis of severe accident sequences in that the latest version of MELCOR severe accident modeling has been adopted.
    - Version 1.8.6 was used in SOARCA. The changes from 1.8.6 to 2.1 are for "modernization" to a newer FORTRAN version.
  - The SOARCA approach is adequate because code validation has been conducted in a systematic manner to test code capabilities for key SOARCA phenomena and their interactions.
    - Some analysis aspects remain that require additional sensitivity studies and uncertainty quantification. These studies are essential to the credibility of SOARCA.

Individual Assessments – Vierow (2/3)

- Attainment of SOARCA Objectives
  - The objectives for severe accident analysis have been largely met.
    - Many insights have been gained.
    - However care should be taken in extrapolating results to other reactors as reactor-specific characteristics may exist.

Individual Assessments – Vierow (3/3)

- Presentation of the SOARCA effort as “best-estimate”
  - The current evaluations retain some conservatisms,
  - Care should be taken by the SOARCA team to qualify the claim of best-estimate analyses.
    - “More best-estimate”, or “more realistic” than earlier analyses would be appropriate.
  - A compendium of conservatisms and their justifications would be useful.
    - Discussion of the conservatisms would enhance SOARCA’s credibility by illustrating to the public that the results were not adjusted in an optimistic manner.

Individual Assessments – Yanch (1/2)

- No effects on the population other than radiation-induced health impact are evaluated in the SOARCA study and no attempt has been made to evaluate the consequences of environmental contamination and its impact on human suffering, human health, or the local and national economic consequences of a severe reactor accident.
  - This is a significant and important omission.
- For most of the scenarios addressed in the SOARCA study, the accident proceeds slowly enough that, should it be necessary to give the evacuation order, the public can leave in a timely way so that little to no radiation dose is incurred until the public is permitted to return home.
  - When to return home is determined by return-home dose limits set by individual states.

Individual Assessments – Yanch (2/2)

- SOARCA approach to estimating health impact reflects the state-of-the-art.
  - The strategy for determining the impact of exposure to anthropogenic radiation is broadly consistent with the approach taken by the scientific field in general and by several national and international agencies and committees.
  - Approach: assume a threshold for acute effects, integrate the dose over a 50 year period, assume cancer is the only impact on long-term health, the use of a DDREF of 2.0, and the application of a common risk factor throughout the entire dose range.

Documentation Provided Throughout the Peer Review

- Peer Review Comments following July 2009 Meeting
- Peer Review Comments following September 2009 Meeting
- Comments on SOARCA Document Description following March 2010 Meeting
- Memo Providing Guidance on SOARCA Issues
- Memo on Uncertainty Quantification and Sensitivity Analysis
- Memo Providing Guidance on Uncertainty Analysis Methodology
- Draft Committee report in May 2010
- Final Committee report in January 2012

Backup slides

Committee Meetings

- Rockville, MD, July 28-29, 2009.
  - A draft of the SOARCA NUREG document, dated July 2009, was received for review prior to the meeting.
  - The SOARCA team presented the project to the Committee members
  - Initial comments and questions were discussed verbally.
- Action items:
  - The Committee provided written comments on the SOARCA document and information presented at the two-day meeting

Committee Meetings

- September 15-16, 2009 in Bethesda, MD.
  - Prior to this meeting, supplemental material including reports of MELCOR and MACCS external review committees, the 1982 Sandia Siting Study and a memo from Dana Powers on fission product retention in steam generator tubes were transmitted to the Committee members.
  - The SOARCA team presented the latest results to the Committee members.
- Action Items:
  - The Committee provided written comments on the SOARCA document and information presented at the two-day meeting

Committee Meetings

- March 2-3, 2010 in Rockville, MD
  - A draft of the SOARCA NUREG document, dated February 14, 2010, was received for review prior to the meeting.
  - Presentations by the SOARCA team on the first day focused on comment resolution and plans for Uncertainty Quantification and Sensitivity Analysis.
  - The second day of meetings was primarily for discussions amongst the peer reviewers and small group meetings with members of the SOARCA team, as requested by the peer reviewers.

Committee Meetings

- March 2-3, 2010 in Rockville, MD (cont.)
- Action items:
  - The Committee members were asked to provide written comments on the description of the SOARCA in the draft NUREG.
  - Issues arose for which the SOARCA team requested guidance on a time scale shorter than that for preparation of the Committee's final report. The Committee provided a memo.
  - The Committee members were asked for their insights into the Uncertainty Quantification and Sensitivity Analysis, an issue which several members were interested in but which was determined to be outside of the review scope. The Committee provided a memo.

Committee Meetings

- October 26-27, 2010, Rockville, MD
  - The reviewers received a draft plan for the Uncertainty Analysis, dated Oct. 19, 2010, a draft NUREG/CR on uncertain input parameters for use in off-site consequence analysis codes and an ORNL report documenting an uncertainties in cancer risk coefficients.
  - The SOARCA team discussed the proposed Uncertainty Analysis technical approach and resolution of peer review comments from earlier meetings.
  - On the second day, the peer reviewers discussed completion of the Committee's final report.
- Action items:
  - prepare a list of unresolved review comments
  - draft a guidance memo on the Uncertainty Analysis
  - finalize the Committee's report.

Committee Meetings

- Rockville, MD, Dec. 6-8, 2011
  - The reviewers received the draft SOARCA NUREG document dated Nov. 29, 2011 (Main Report), Oct. 12, 2011 (Volume 1) and Nov. 17, 2011 (Volume 2).
  - The SOARCA team presented changes in SOARCA analyses and results on the first two days.
  - On the third day, a description of the MELCOR code validation effort was provided.
- Action item:
  - Complete the Committee’s report

Committee Meetings

- Sept. 5, 2011 teleconference
  - The SOARCA team provided a written response to reviewer comments to date on July 29, 2011.
  - A teleconference between the SOARCA team and the peer reviewers was conducted to discuss the responses and clarify whether the reviewers had any remaining issues.
- Jan. 5, 2012 teleconference
  - A second teleconference was conducted to explain to the peer reviewers the selection of parameters being used in the Uncertainty Analysis and their distributions.
  - The reviewers had not seen the results or conclusions of the Uncertainty Analysis at the time of preparation of the final report.

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Public Comments and Commission Documents

ACRS SOARCA Briefing

April 25, 2012

Agenda

- Overview of Public Meetings/Comments
- Commission Memorandum
  - SOARCA Documents
- Commission Paper - Notation Vote
  - Issues Considered for Follow-on Research
  - Conclusions
  - Recommend Analysis of Ice Condenser Containment

Overview of Public Meetings

- NRC held 2 public meetings on SOARCA: 2/21- Surry; 2/22- Peach Bottom
- 30-day public comment period
  - ~75 comments received from 8 people
- RIC SOARCA session March 14

Overview of Public Comments

- Scope (battery duration; site-wide risk; need for HRA; low-power and shutdown modes; reporting alternate results)
- Fukushima comparisons (potential release of large amounts of contaminated water)
- Assumptions within MELCOR and MACCS2 (MELCOR validation; gaussian plume model; FGR-13 use; weather data; evacuation timing)

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Commission Memorandum

SOARCA Deliverables

- NUREG-1935, SOARCA Report
- NUREG/CR-7110, Volume 1, Peach Bottom Integrated Analysis
- NUREG/CR-7110, Volume 2, Surry Integrated Analysis
- NUREG/BR-0359, Modeling Potential Reactor Accident Consequences

Commission Paper- Notation Vote

Issues Considered for Follow-on Research

- Spent fuel pool issues (SFPSS, NTTF, Level 3 PRA)
- Multi unit and site risk (Level 3 PRA)
- Low power, shutdown, and other modes (Level 3 PRA)
- Human reliability analysis (Level 3 PRA)
- Comparison to NUREG-1150/historical studies (Level 3 PRA)
- Economic Consequences/Land contamination (RES SECY paper)
- Cancer incidence rates (Cancer Risk Study)
- Additional comparisons to Fukushima (DOE/NRC Forensic Analysis)
- Develop integrated, predictive computer-based tool (NSIR/RES activities)
- Large seismic event and soil liquefaction (seismic research plan)
- Other plant and containment types – eg, Ice condenser, Mark II, III, CE, B&W (open)

Commission Paper-
Notation Vote

Conclusions

- The staff believes that the SOARCA project objectives have generally been met
- Staff believes SOARCA-type analysis of all 8 plant types or 104 reactors as originally described (now 108 licensed reactors) is not necessary
  - Provided body of knowledge updating understanding of severe accident progression, mitigation, and consequences
  - Provided flexible and updated models and methods
  - Level 3 PRA will continue to add to this body of knowledge
- Staff recommends limited follow-on research

Commission Paper- Notation Vote

Recommend Follow-on Research

- Analysis of Ice Condenser Containment during station blackouts
  - Ice condenser is a unique containment design
  - Behavior may be different than Mark I and Large Dry containments analyzed in SOARCA
  - Potential hydrogen vulnerabilities during SBO
  - Potentially different mitigation strategies and timing
  - Supports response to NTTF recommendations and other agency projects and decisions

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Protecting People and the Environment

SOARCA Uncertainty Analysis

Presented by: Tina Ghosh, PhD

ACRS Subcommittee Meeting
April 25, 2012

Outline

- Goals and approach
- Selected parameters and bases
- Preliminary analyses
- Phenomenological insights from MELCOR and MACCS2
- Status and schedule

Goals of the Uncertainty Analysis

- Develop insight into overall sensitivity of SOARCA results to uncertainty in inputs
- Identify most influential input parameters for releases and consequences
- Demonstrate uncertainty analysis methodology

Approach

- Focus is on parameter uncertainty
- Model uncertainty addressed only to the extent that some parameters represent or capture alternate model effects
- Peach Bottom, unmitigated, long-term station blackout scenario chosen
- Scenario definition not changed after Fukushima
  - A separate qualitative discussion planned for an appendix

Approach (continued)

- Focus is on epistemic (state-of-knowledge) uncertainty in input parameters
- Aleatory (random) uncertainty due to weather is handled in the same way as the SOARCA best estimate
- Looking at uncertainty in key model inputs
  - MELCOR parameters
  - MACCS2 parameters

Approach (continued)

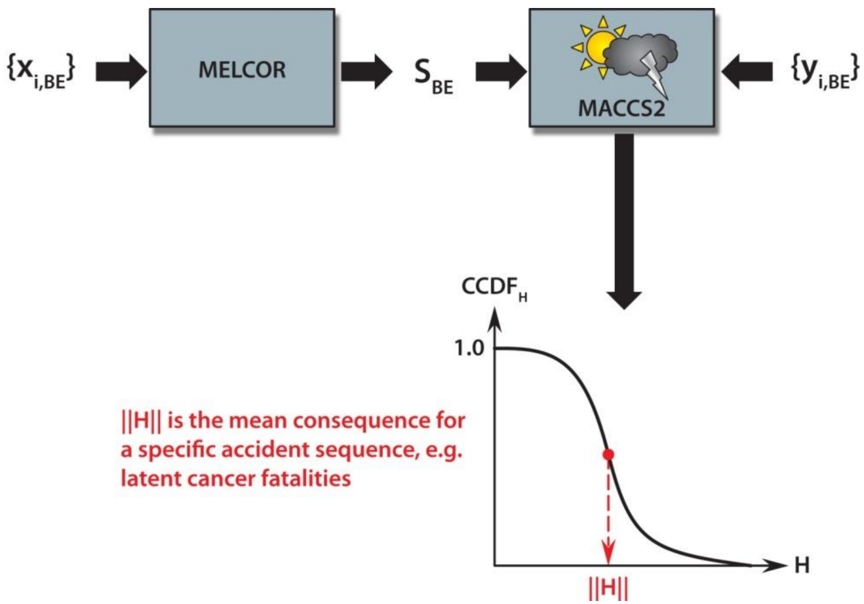
- Key uncertain input parameters were identified
- Uncertainty in these parameters propagated in two steps using Monte Carlo and Latin Hypercube (LHS) sampling:
  - A set of source terms generated using MELCOR model
  - A distribution of consequence results generated using MACCS2 model
- An epistemic sample set of 300 generated to complete a corresponding number of individual code runs (Monte Carlo “realizations”) to evaluate the influence of the uncertainty on the estimated outcome

Approach (continued)

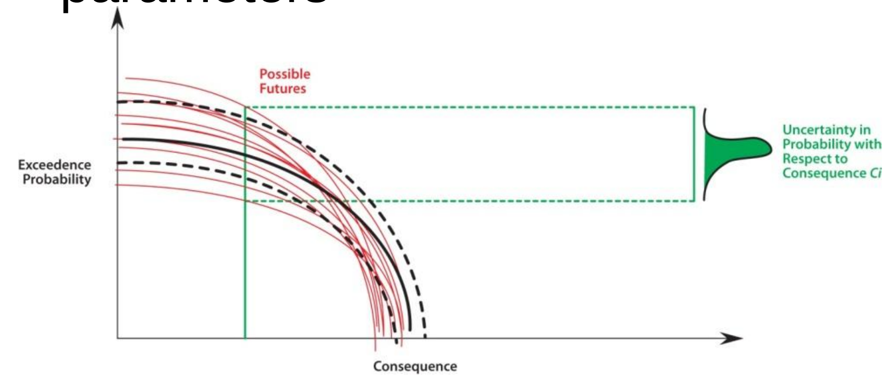
- Results reported will include:
  - Analysis of source term releases including Cesium and Iodine release over time
  - Distribution of latent cancer fatality risk, with three dose threshold models
  - Description of most influential uncertain parameters in study
- Tools used to analyze results include statistical regression-based methods as well as scatter plots and phenomenological investigation of individual realizations of interest
- Guidance solicited from SOARCA peer reviewers on the uncertainty analysis plan documenting the approach, chosen parameters and distributions

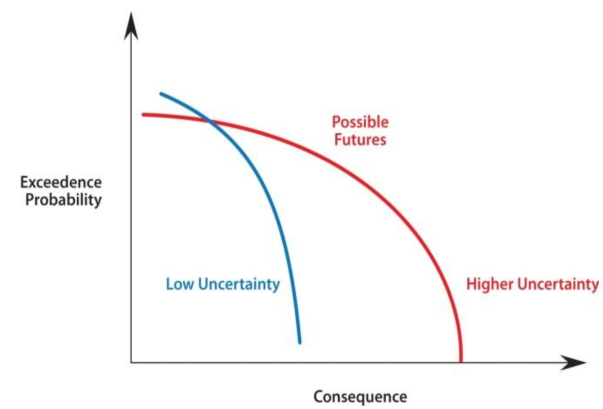
SOARCA Uncertainty Analysis

Best estimate calculations include weather variability in the consequences



Probabilistic UA includes both the weather variability and the uncertainty in the epistemic input parameters





Core Team Members

- MELCOR and severe accident progression: Randy Gauntt, Kyle Ross, Doug Osborn, Don Kalinich (SNL); Mark Leonard (dycoda); Ed Fuller (NRC)
- MACCS2, consequence analysis and emergency response: Nate Bixler, Joe Jones, Doug Osborn (SNL)
- UA methodology: Patrick Mattie, Cedric Sallaberry, Jon Helton (SNL); Tina Ghosh (NRC)
- Consultants for MACCS2 latent health effects modeling: Keith Eckerman (ORNL); Tony Huffert (NRC)

Process for Choosing Parameters and Distributions

- Core team of staff from SNL and NRC with expertise in probability and statistics, uncertainty analysis, and MELCOR and MACCS2 modeling for SOARCA
- Subject matter experts (SMEs) provided support in reviews of data and parameters
- Approach is based on a formalized PIRT (phenomena identification, and ranking table) process.

Process (continued)

- Focus on:
  - confirming that the parameter representations appropriately reflect key sources of uncertainty, and
  - ensuring model parameter representations (i.e., probability distributions) are reasonable and have a defensible technical basis.

Process (continued)

- Attempt to obtain contribution from uncertainty across the spectrum of phenomena operative in the analyses, through a balanced depth and breadth of coverage
- A subset of possible uncertain parameters is proposed that cover the range of phenomena across the stages of a severe accident.

MELCOR Uncertain Parameters

Sequence Issues

- Battery duration
- SRV stochastic failure rate, thermal seizure criteria, and open area fraction

In-Vessel Accident Progression

- Main steam line (MSL) creep rupture open area fraction
- Zircaloy melt breakout temperature
- Molten clad drainage rate
- Fuel failure criterion
- Debris radial relocation time constants

Ex-vessel Accident Progression

- Debris lateral relocation time constants

Containment & building behavior

- Drywell liner failure flow area
- Drywell head flange leakage parameters
- Hydrogen ignition criteria (where flammable)
- Railroad doors open fraction

Fission Product release, transport, and deposition

- Cesium and Iodine chemical forms
- Aerosol deposition parameters

MACCS2 Uncertain Parameters

Atmospheric Transport and Deposition

- Wet deposition model linear coefficient
- Dry deposition velocities
- Dispersion parameters

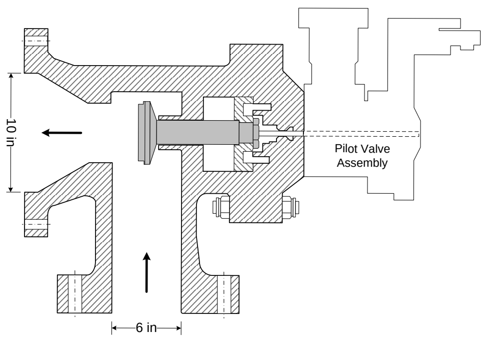
Emergency planning and response

- Shielding factors
- Hotspot and normal relocation
- Evacuation delay and speed

Health Effects

- Early health effects
- Latent health effects
  - Groundshine dose coefficients
  - Dose and dose rate effectiveness factors
  - Inhalation dose coefficients
  - Cancer mortality risk coefficients

BWR SRV Seizure Modeling



Modes of Valve Seizure

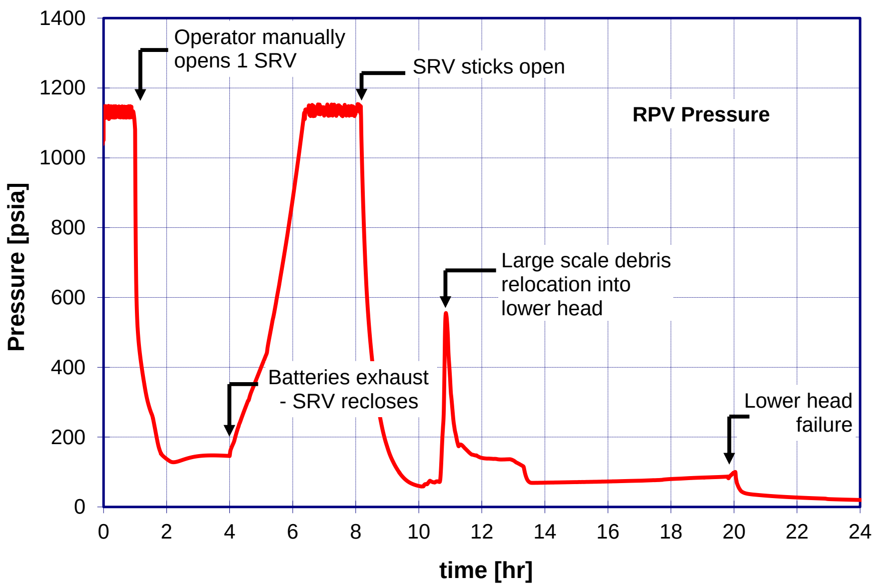
- Excessive cycling
- Differential thermal expansion
- Material deformation

In severe accident conditions, high temperature gases well exceed design conditions

T_{op} ~ 600K

T_{SA} > 800 to 1100K

cycles for hours



Seizure in stuck open eventually occurs

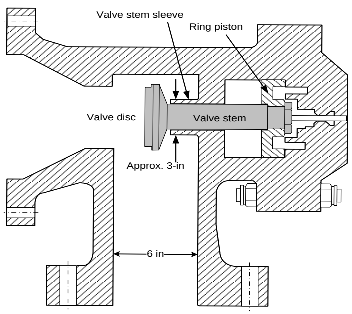
excessive cycling

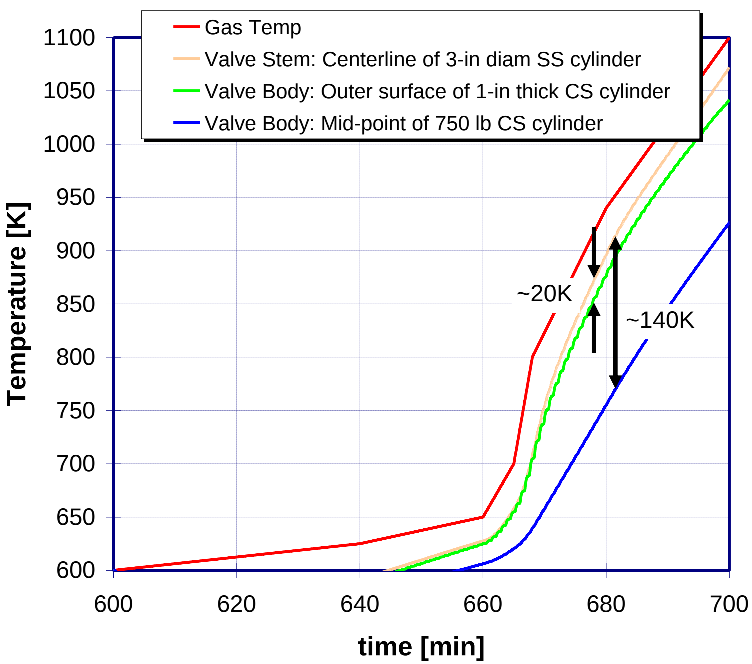
thermal deformation

partial or full open

Valve behavior important to accident progression

SRV Thermal Failure

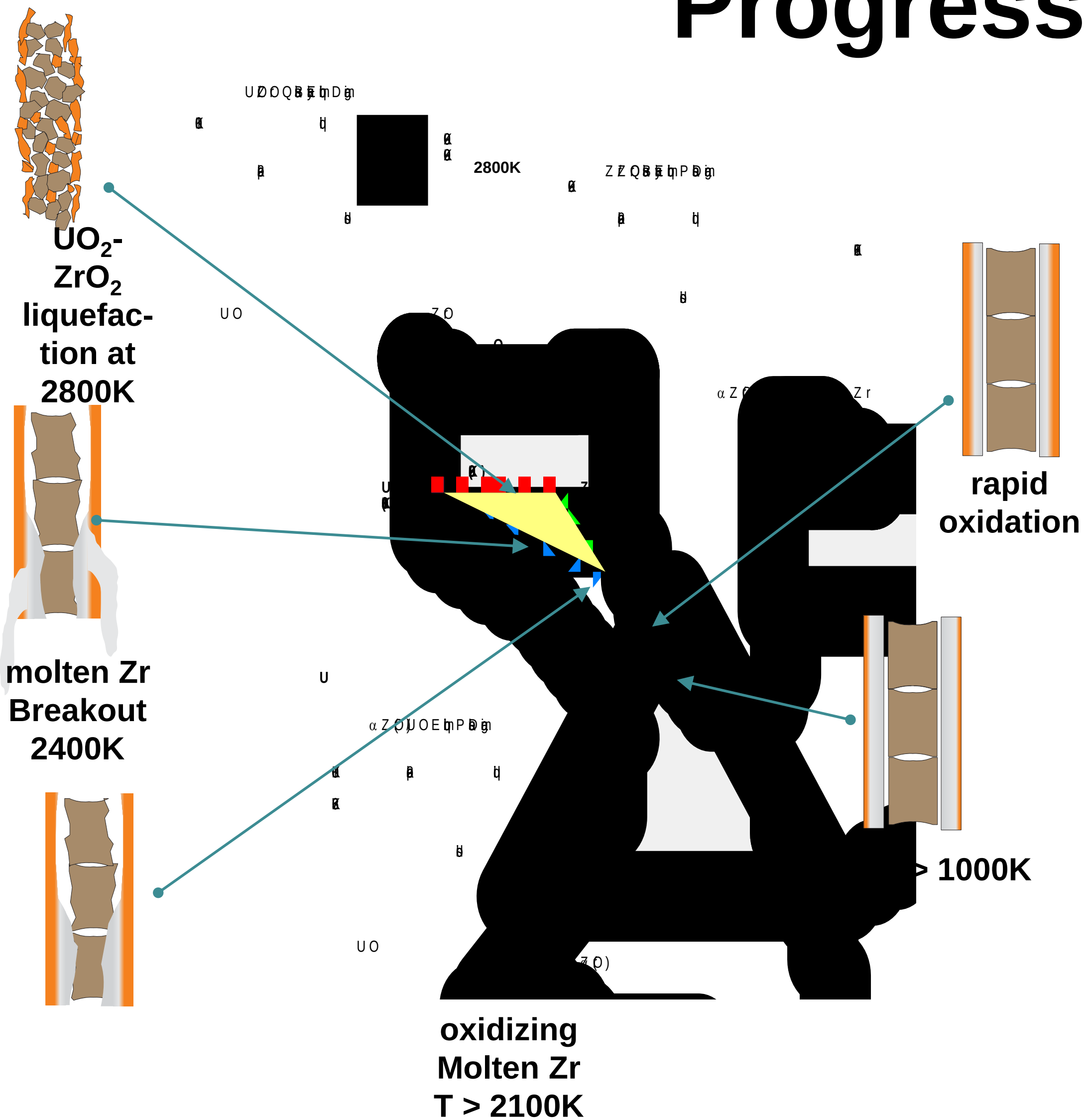




- In severe accident conditions, high temperature gases well exceed design conditions
  - T_{op} ~ 600K
  - T_{SA} > 800 to 1100K
  - cycles for hours

- SOARCA Best Estimate Boundary Conditions:
  - Gas flow through the valve is based on a cycle period of 45 seconds with an open cycle duration of 5 seconds
  - Gas velocity during an open cycle is constant during a single five second cycle, but increases from 420 m/s to 500 m/s as the gas temperature increases from 600 K to 1100 K
  - Gas temperature as a function of time. Approximately 100 K temperature changes between the beginning and end of a cycle were neglected. The mid-point values were used
  - The steam and hydrogen mole fractions were specified as a function time

Modeling Melt Progression Stages



Zr metal melt at 2150K

UO_2 dissolution in Zr metal ~20%

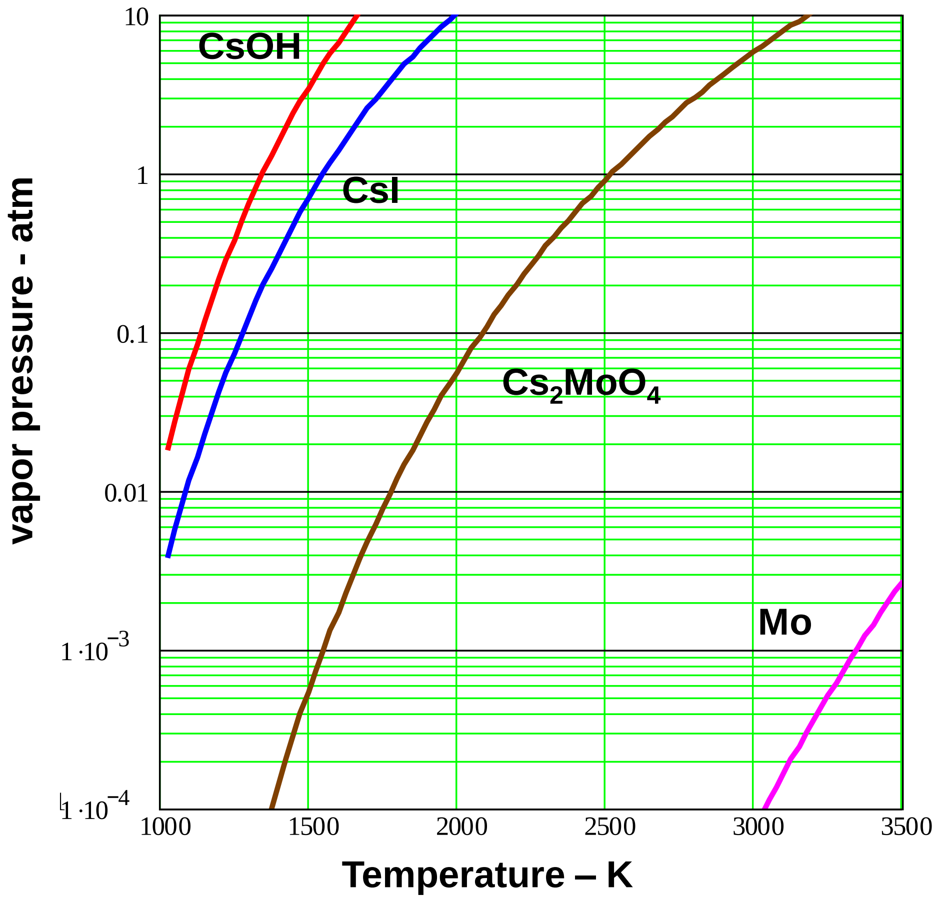
Zr melt breakout ~2400K

Loss of rod geometry ~2600K

UO_2-ZrO_2 liquefaction ~2800K

Parameters uncertain

Speciation of Cesium and Iodine



- Based on Phebus program findings
  - Iodine treated as CsI
  - Cs treated as CsI and Cs_2MoO_4
- Cs_2MoO_4 considerably less volatile than CsOH or CsI
  - Affects retention in RCS and long term revaporization
- Uncertainty analysis exploring alternative balance of speciation
  - I_2, CsOH, CsI and Cs_2MoO_4

MELCOR Parameters
CHEMFORM

Chemical Forms of Iodine and Cesium

Parameter			Distribution		
CHEMFORM: Five alternative combinations of RN classes 2, 4, 16, and 17 (CsOH, I_2, CsI, and Cs_2MoO_4)			Discrete distribution		
			Combination #1 = 0.125		
			Combination #2 = 0.125		
			Combination #3 = 0.125		
			Combination #4 = 0.125		
			Combination #5 = 0.500		
Five Alternatives			Species (MELCOR RN Class)		
		CsOH (2)	I_2 (4)	CsI (16)	Cs_2MoO_4 (17)
Combination #1	fraction iodine	--	0.03	0.97	--
	fraction cesium	1	--	--	0
Combination #2	fraction iodine	--	0.002	0.998	
	fraction cesium	0.5	--	--	0.5
Combination #3	fraction iodine	--	0.00298	0.99702	--
	fraction cesium	0	--	--	1
Combination #4	fraction iodine	--	0.0757	0.9243	--
	fraction cesium	0.5	--	--	0.5
Combination #5	fraction iodine	--	0.0277	0.9723	--
	fraction cesium	0	--	--	1
Best estimate	Fraction iodine	--	0.0	1.0	--
	Fraction cesium	0.0	--	--	1.0

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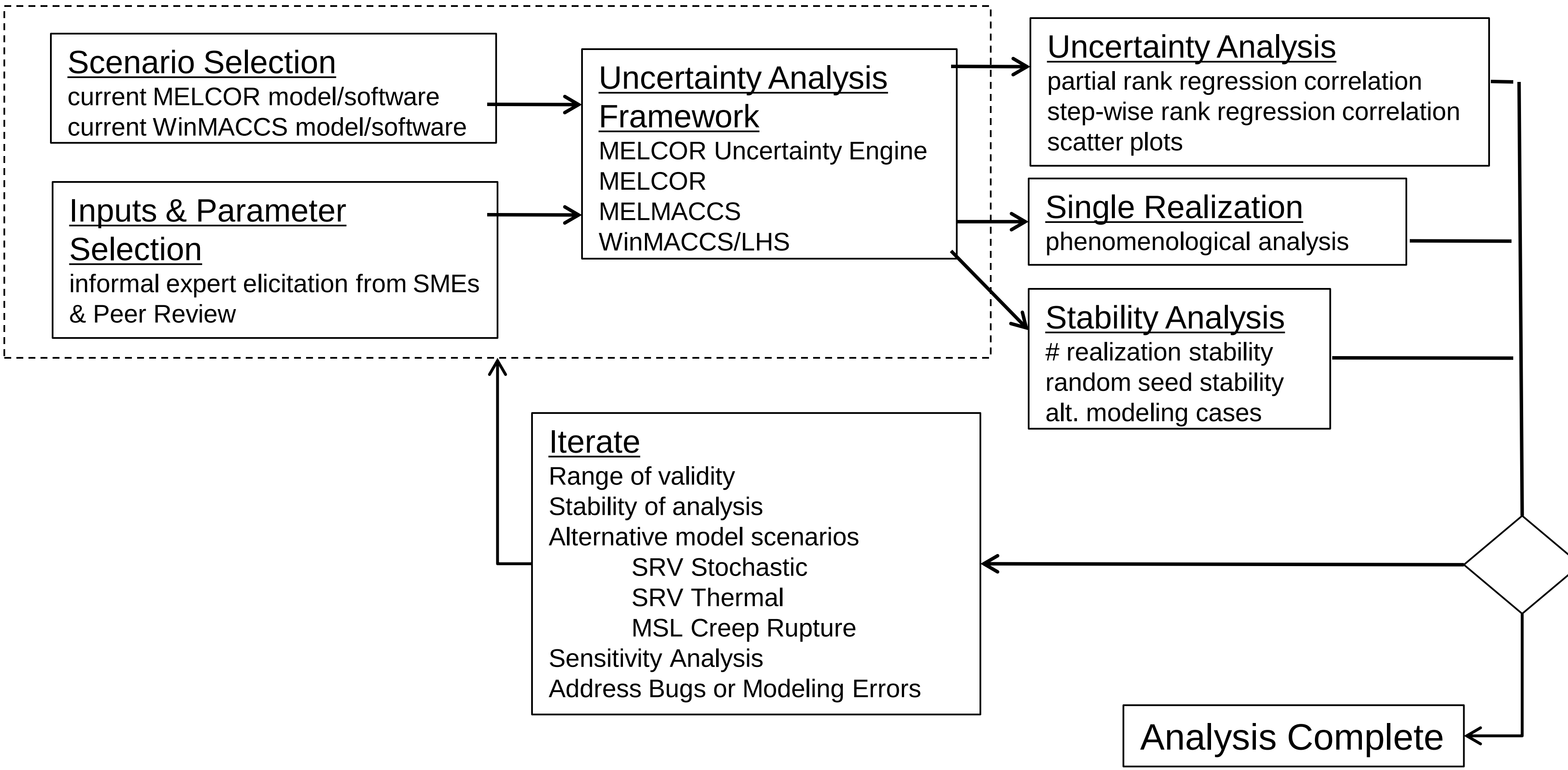
Probabilistic Uncertainty analysis is an iterative process



Preliminary Analyses
Source Term/MELCOR

3 probabilistic cases:

- (1) Combined scenario probabilistic case using distributions as laid out in draft NUREG/CR, chapter 4, with SRV stochastic, SRV thermal, and MSL creep failures occurring.
- (2) SRV Thermal scenario case keeping the SRV thermal seizure open area constant at 1
- (3) SRV stochastic scenario case keeping the SRV stochastic failure rate constant, at the SOARCA best estimate value

Preliminary Analyses
Consequence/MACCS2

- Source terms from combined scenario case
- Using LNT model
- Aleatory & epistemic uncertainty

Preliminary Analyses
Summary Results

- MELCOR: Cesium release timings are similar to SOARCA best estimate, magnitude of release at 48 hours generally slightly higher than best estimate (but still far below Siting Study SST1 results)

- MACCS2: distribution of risk results for latent cancer fatality risk similar to the best estimate, and early fatality risk essentially zero

Phenomenological Insights
MELCOR Analyses

- Confirmation of phenomenology seen in the SOARCA best estimate, and nothing that invalidates the SOARCA best estimate
  - In particular, strong dependence between number of SRV cycles before failure to re-close and probability of thermal SRV and/or MSL creep rupture, and
  - dependence between area fraction of SRV open when failed thermally and occurrences of MSL creep rupture
  - Chimney effect when rail road doors are failed open
- As well as additional new insights

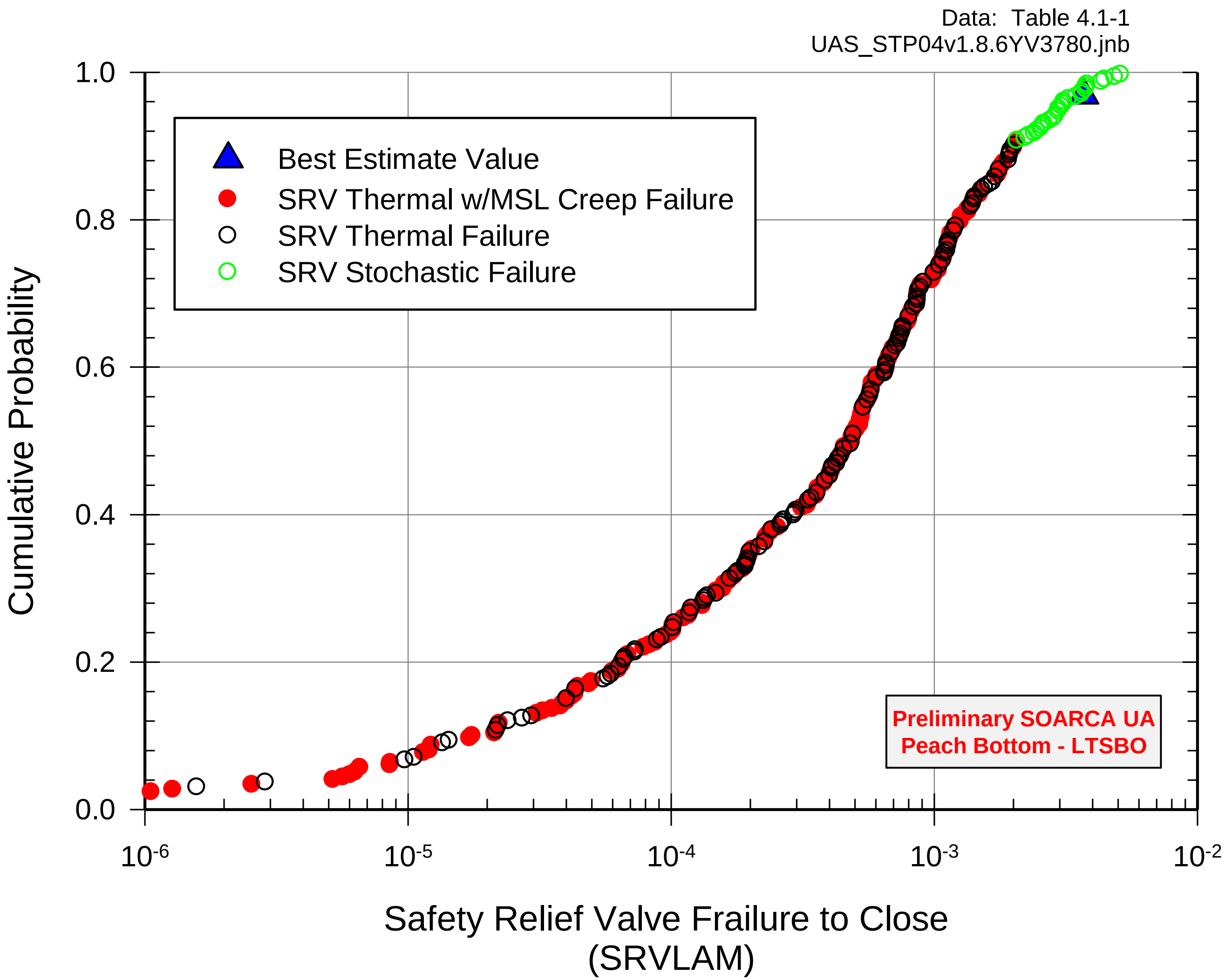
Parameter Sensitivity

Source Term SOARCA UA Peach Bottom LTSBO

Stepwise rank regression on the fractions of cesium and iodine release in 48 hours indicate the following parameters are important:

- SRV open area fraction for thermal seizure (R^2 = 0.29 - 0.30)
- Chemical form (R^2 = 0.06 - 0.09)
- SRV stochastic failure rate (R^2 = 0.04 - 0.06)
- Dynamic agglomeration and shape factors (R^2 = 0.02 - 0.05)
- Railroad door open area (R^2 = 0.03 - 0.04)

Influence of Number of SRV Cycles before Failure to Re-Close



Distribution from NUREG/CR-6928

λ mean value = 7.95 x 10^{-4} per demand (1,258 cycles)

λ best estimate value = 3.7 x 10^{-3} per demand (270 cycles)

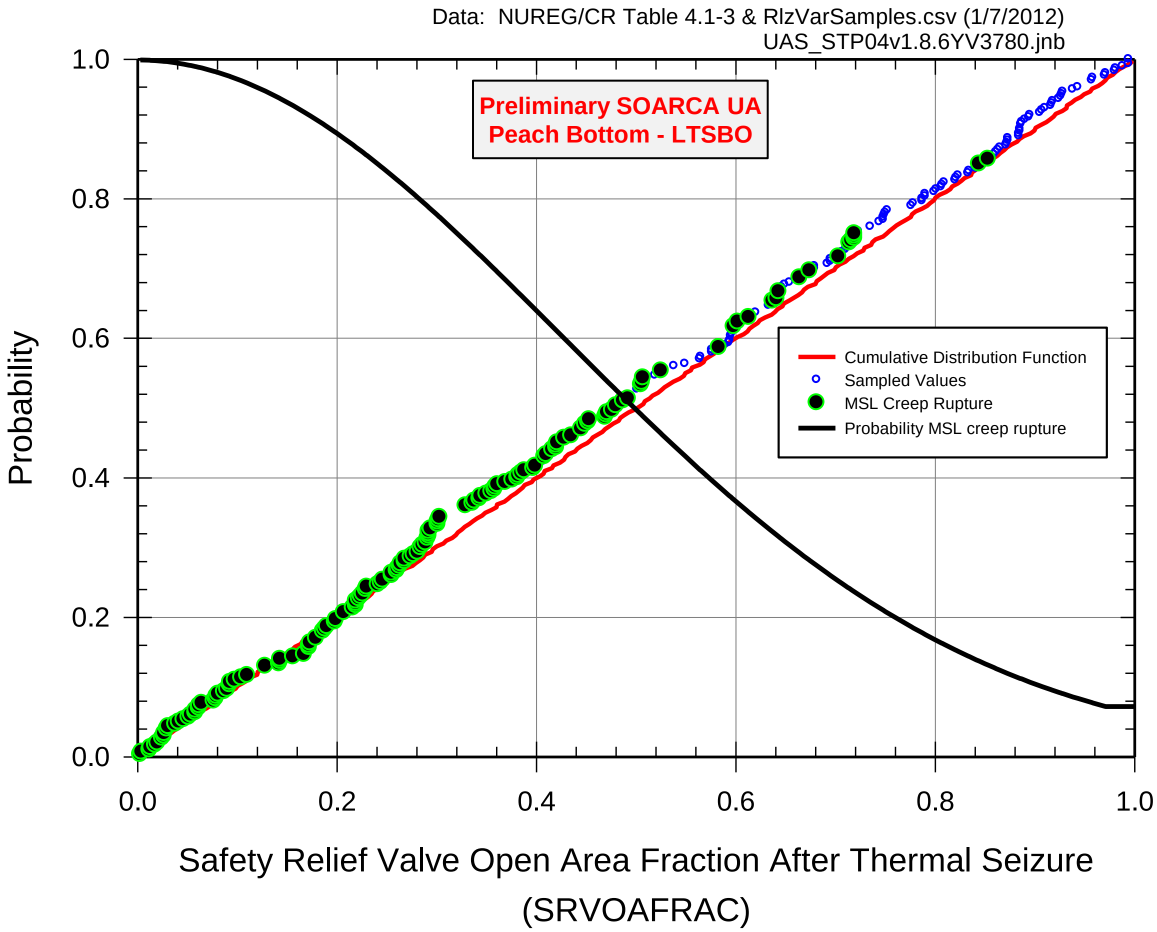
Cited in Peach Bottom IPE

Timing of SRV failure is important

SRV will fail thermally

Potential for MSL creep failure controlled by SRVOAFRAC

Influence of SRV Thermal Failure Open Area when SRV Thermal Failure Occurs



Uniform Distribution [0,1], failed closed to failed fully open

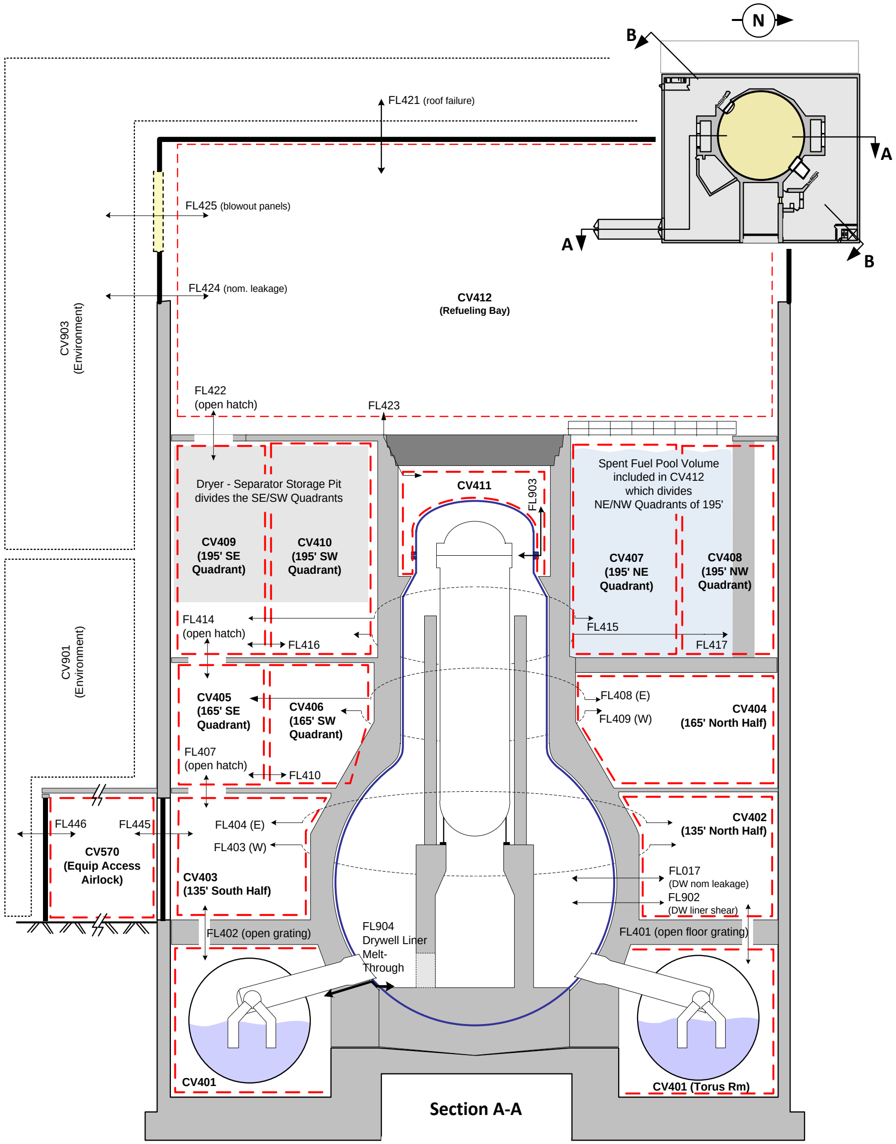
mean value = 0.5

Timing of SRV failure is important

SRV will fail thermally first in all cases

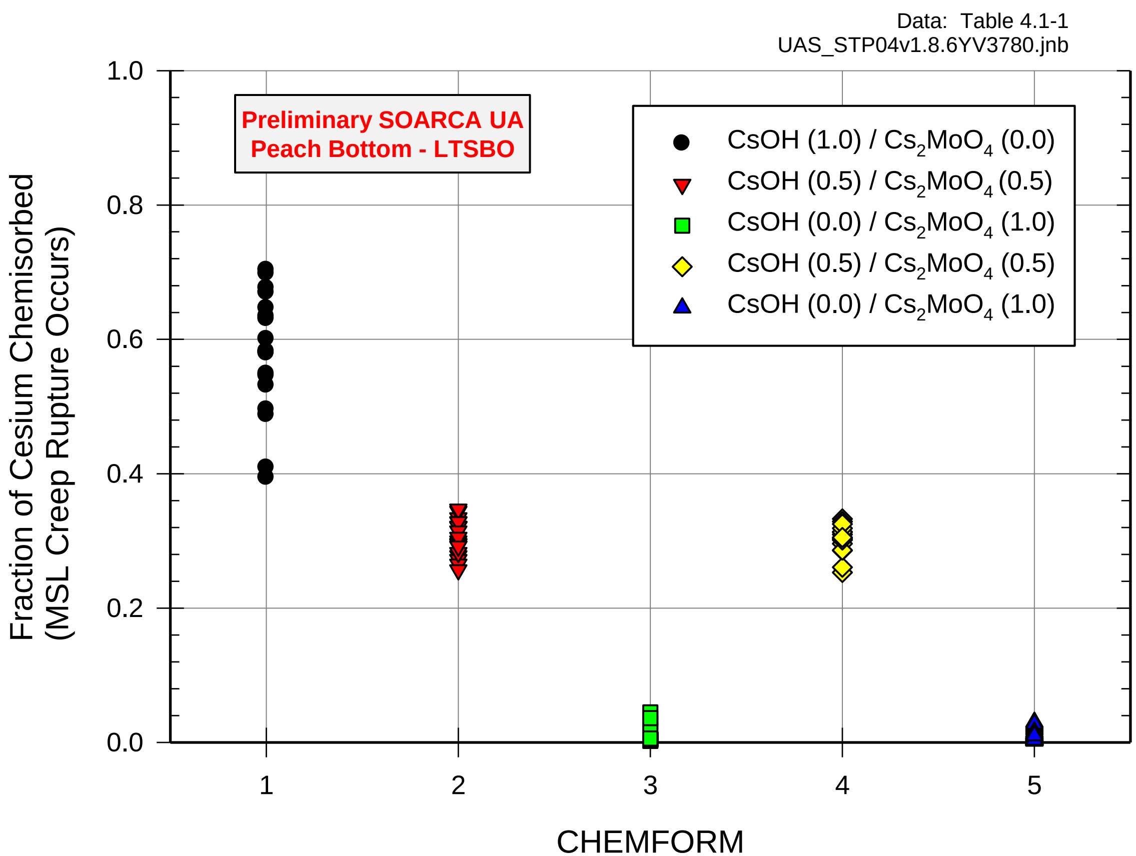
Potential for MSL creep failure controlled by SRVOAFRAC

Railroad Doors



- Railroad doors opening creates a chimney effect leading to increased releases to the environment; actual opening area is not as important as whether they are open or closed
- This effect was noted in the SOARCA best estimate and has been noted in previous MELCOR studies.

Effect of CsOH on Fraction of Cesium Chemisorbed in MSL Creep Rupture Cases



Discrete distribution [0.125,0.125,0.125,0.125,0.5]

Chemisorption on the steam dryers and upper RPV

Limits the mass available for release

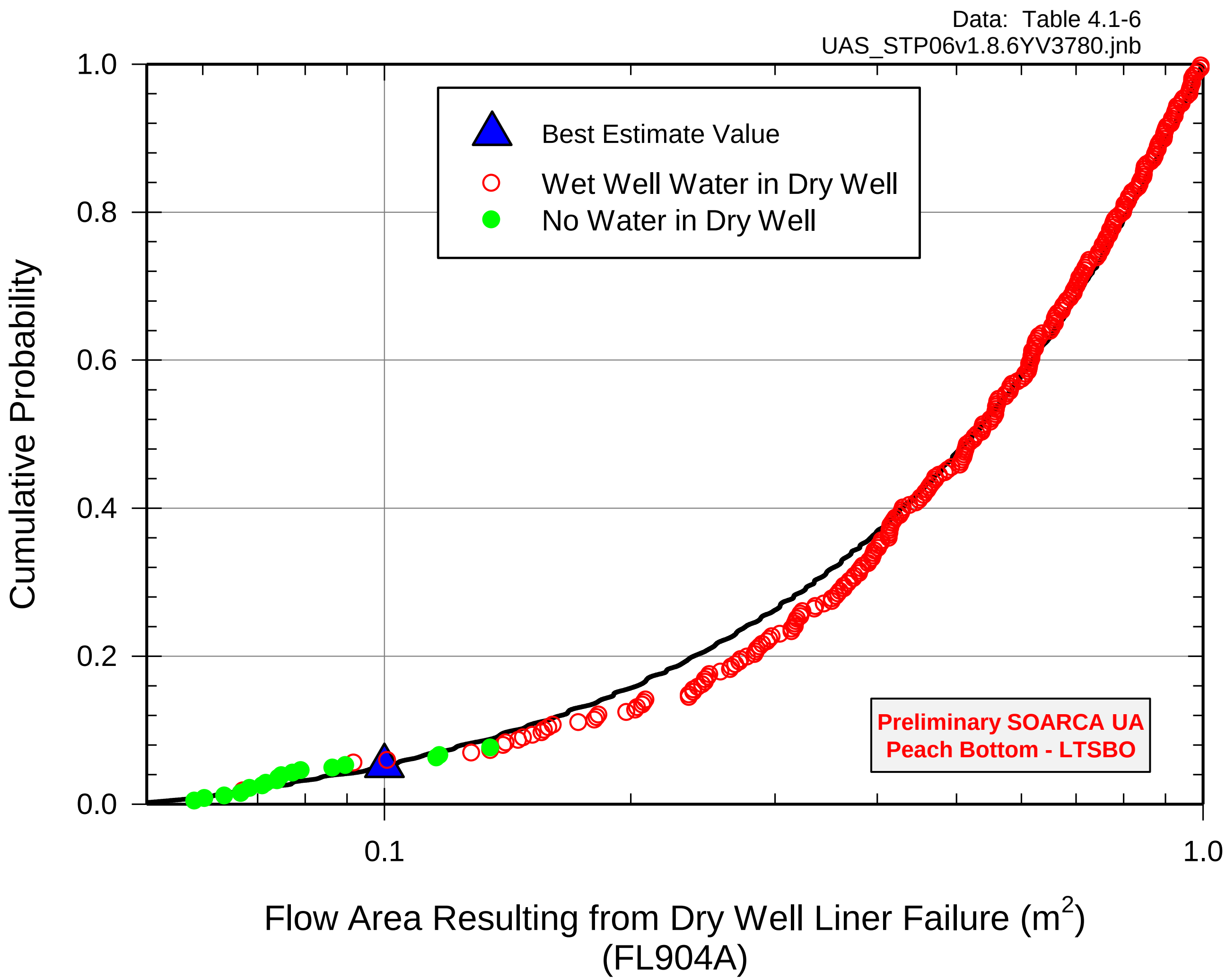
Occurs in higher temperature MSL creep rupture cases

This effect not seen in best estimate calculation (no CsOH in best estimate)

Chemical form of Cesium

- Chemical form of Cs as CsOH (present in bins 1, 2, and 4) can have an important effect limiting the mass available for release to the environment due to chemisorption for high temperature scenarios like MSL creep rupture conditions
- CsOH can slightly increase releases for lower temperature accident progression events like stochastic failure of SRV

Drywell Liner Open Area



Uniform distribution selected from [0.05,1]

Vacuum breakers overwhelmed by pressure vented from dry well

Mass of RN relocated in water flashed into dry well

Mass is deposited on surfaces in dry well and can be slowly released to environment

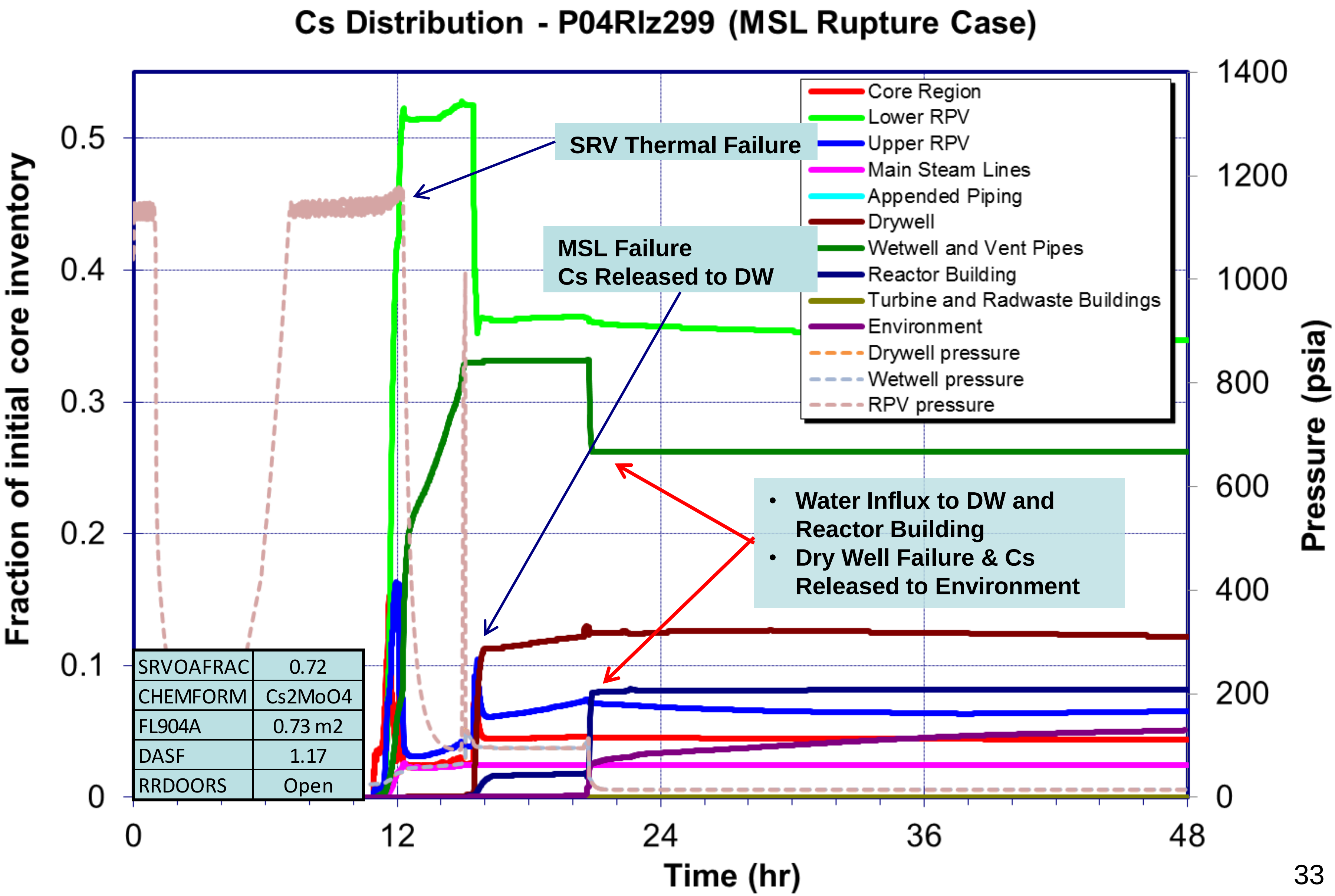
Timing of opening area is important

This effect not seen in best estimate calculation.

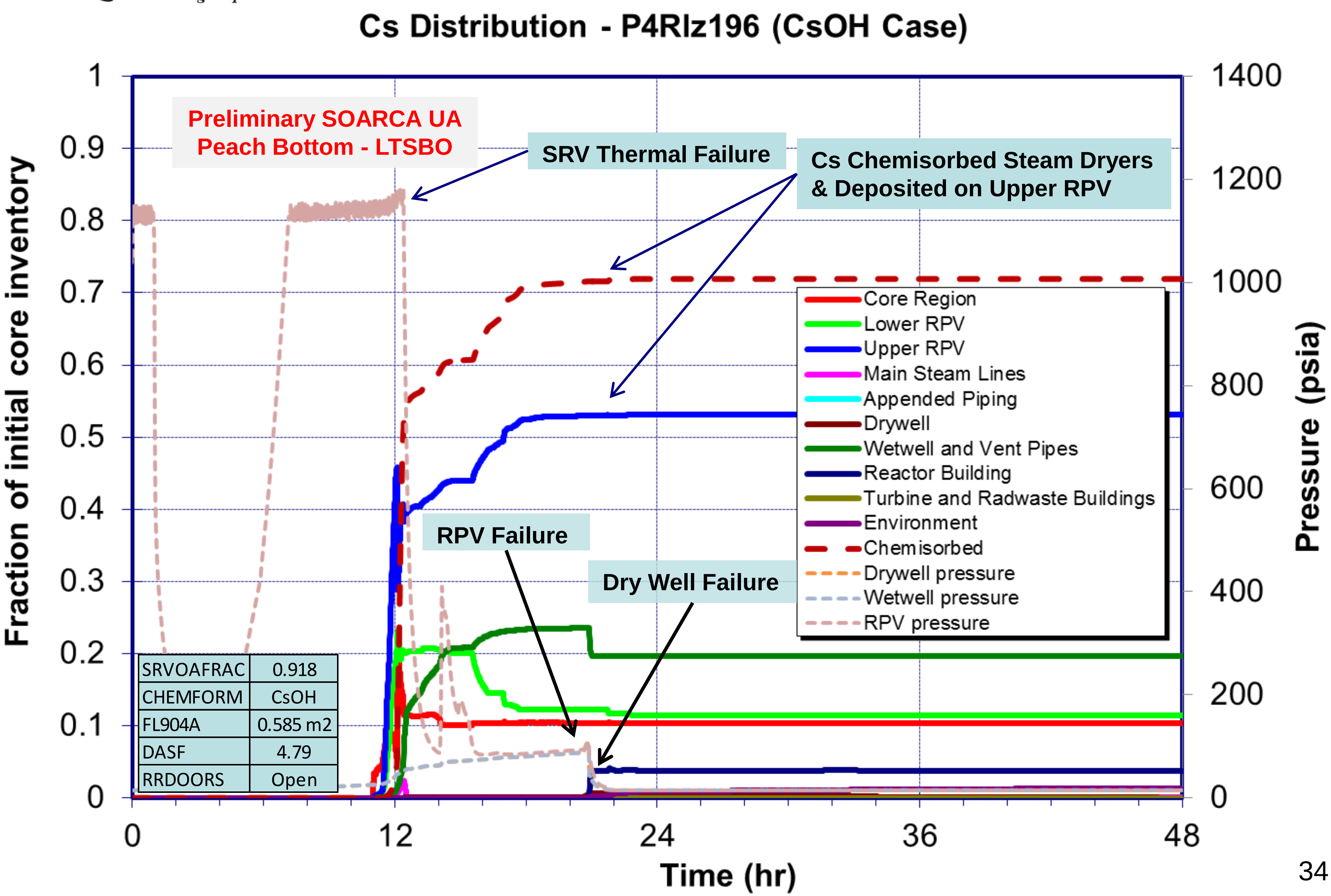
Examples of Single Realizations from the UA Monte Carlo runs

- Examples from SOARCA UA Combined Case for Peach Bottom Unmitigated Long Term Station Blackout, Cesium distribution over time
  1. Main Steam Line Creep Rupture, water influx to drywell
  2. Chemisorption on steam dryers and upper RPV

SOARCA UA Peach Bottom Unmitigated LTSBO -Main Steam Line Creep Rupture



SOARCA UA Peach Bottom Unmitigated LTSBO- SRV Thermal Failure w/CsOH



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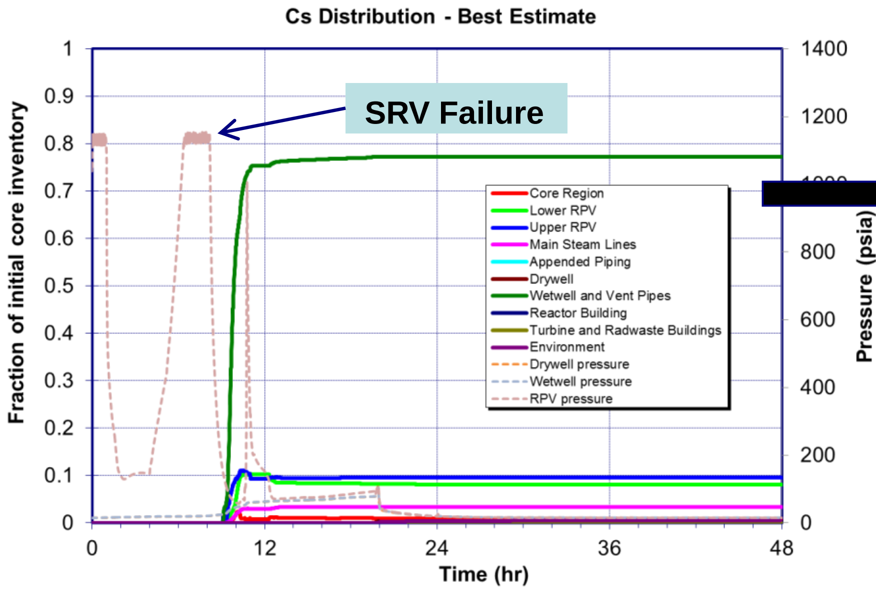

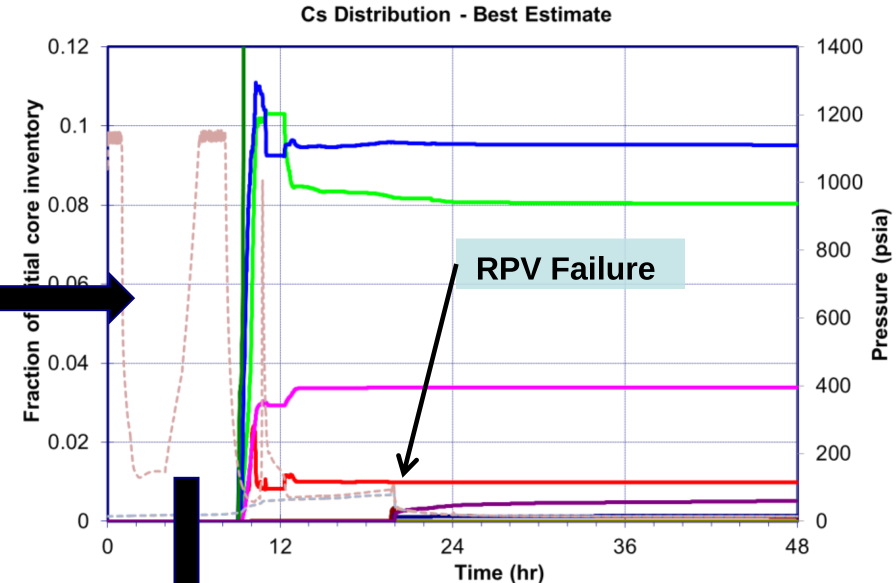

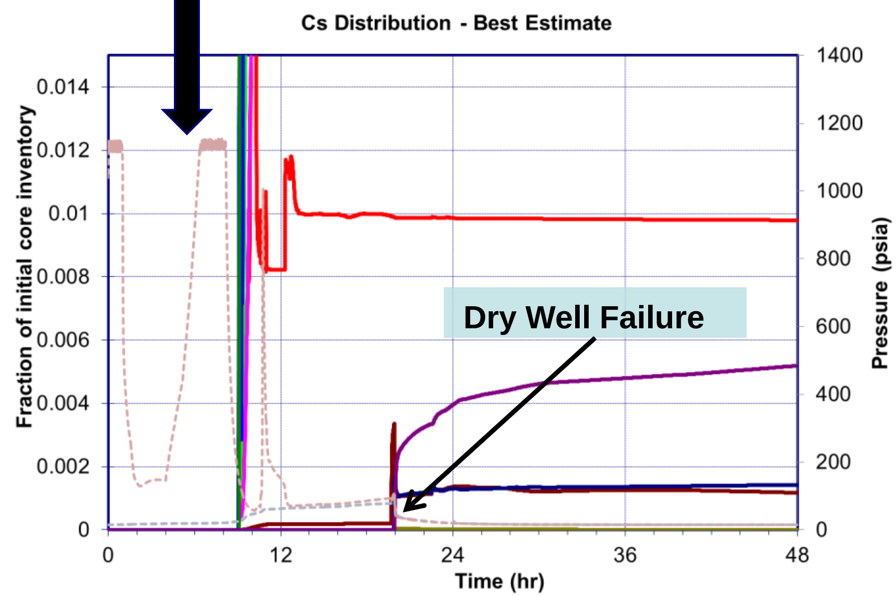

SOARCA Best Estimate Peach Bottom Unmitigated Long Term Station Blackout

Phenomenological Insights
MACCS2 Analyses

UA Combined Scenario Case – Peach Bottom LTSBO

- Unlike older studies, both SOARCA best estimate and preliminary UA results show that the long-term phase dominates health effect risks because emergency response is faster than progression to release
- Health-effect risks vary sublinearly with source term because more remediation is required for larger releases, i.e., people are not allowed to return to their homes until dose is below prescribed level regardless of initial level
- Cesium isotopes always dominate for long-term risks; barium, tellurium, and cerium can dominate over iodine for short-term risks

Preliminary Analyses *consequence results using LNT*

- Individual latent cancer fatality (LCF) risk best estimate falls within the distribution of results from the UA combined case
- Early fatality risk is still essentially zero

Individual Latent Cancer Fatality Conditional Risk (per event) Peach Bottom Unmitigated LTSBO (Mean Core Damage Frequency of 3E-6/reactor year), Combined Case Using LNT		
	0-10 miles	0-20 miles
Minimum	2E-5	1E-5
Maximum	9E-4	6E-4
SOARCA Best Estimate	9E-5	8E-5

Phenomenological Insights
MACCS2 Analyses

UA Combined Scenario Case – Peach Bottom LTSBO

- Based on preliminary habitability criterion sensitivity analysis:
  - Health effect risk is nearly proportional to habitability criterion assuming LNT (related to dominance of long-term phase)
  - Health effect risks drop dramatically if there is a threshold in dose response and habitability is chosen to be below the threshold.
- Preliminary results suggest that distributions of MACCS2 input parameters are reasonable

UA Status

- Finish uncertain parameter sensitivity analyses
  - Regression analyses (e.g., stepwise rank correlation coefficients, scatter plots)
  - Continued investigation of single realizations to confirm physical phenomena and validation of results
- Phenomenological insights for other source term metrics (e.g., iodine, hydrogen production, barium, etc.)
- Additional MACCS2 results and analyses
- Separate sensitivity analyses:
  - Habitability criterion
  - Evaluation of the timing of two operator actions in the unmitigated LTSBO

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UA Status (2)

- An appendix with discussion and qualitative comparison with Fukushima
- Uncertainty analysis is an iterative process requiring feedback from the analyses to validate the robustness of models used to simulate the physical phenomena and the modeling assumptions over the sampled hypercube
- Identified MELCOR parameter distributions that are under consideration for revision
  - e.g., selection of uniform distributions in absence of strong technical basis

Parameter Evaluation and Refinement

Ongoing examination of the technical basis and range of model validity to evaluate potentially non-physical regions of the sample hypercube:

- Dynamic and agglomeration shape factors (i.e., Chi/Gamma) and particle density improperly sampled independently
- Uniform distribution of drywell liner failure flow area (FL904A) up to a value of 10x its nominal value may not properly reflect SME state-of-knowledge and timing of dry well liner failure
- Uniform distribution of SRV open area fraction for SRV thermal failure events. Drives occurrence of MSL creep rupture.

Parameter Evaluation and Refinement (2)

- SRV stochastic failure rate
  - Available database may not be representative of the situation in the SOARCA unmitigated LTSBO scenario, where the SRV fails after repeated cycling and in high-temperature conditions

Schedule

- Commission memorandum forwarding results of the SOARCA pilot plant study will contain short discussion on status of UA and interim conclusions (June 2012)
- Complete analyses and documentation (September 2012)
- Present final results and updated insights to ACRS (Fall 2012)
  - Staff is seeking an ACRS letter on the final report
- Transmittal of final NUREG/CR report to Commission (November 2012)

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Questions?

Acronyms

BWR	Boiling water reactor
DW	Dry well
LCF	Latent cancer fatality
LHS	Latin Hypercube Sampling
LNT	Linear no threshold
LTSBO	Long-term station blackout
MSL	Main steam line
ORNL	Oak Ridge National Laboratory
PIRT	Phenomena Identification and Ranking Table
RN	Radionuclide
RPV	Reactor Pressure Vessel
SME	Subject matter expert
SNL	Sandia National Laboratories
SOARCA	State-of-the-Art Reactor Consequence Analyses
SRV	Safety relief valve
SST1	Siting study source term 1
UA	Uncertainty Analysis