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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

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REGULATORY POLICIES AND PRACTICES SUBCOMMITTEE

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THURSDAY

MAY 19, 2016

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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 8:33 a.m., John W. Stetkar, Chairman, presiding.

COMMITTEE MEMBERS:

JOHN W. STETKAR, Chairman

RONALD G. BALLINGER, Member

DENNIS C. BLEY, Member

MICHAEL L. CORRADINI, Member

JOY REMPE, Member

GORDON R. SKILLMAN, Member

ACRS CONSULTANT:

WILLIAM SHACK

DESIGNATED FEDERAL OFFICIAL:

HOSSEIN P. NOURBAKHSH

ALSO PRESENT:

JON BARR, RES

KEITH COMPTON, RES

HOSSEIN ESMAILI, RES

RANDY GAUNTT, SNL

TINA GHOSH, RES

SALMAN HAQ, RES

WALTER KIRCHNER, Invited Expert

RICHARD LEE, RES

JOSE MARCH-LEUBA, Invited Expert

DOUG OSBORN, SNL

JOSE PIRES, RES

KYLE ROSS, SNL

PATRICIA SANTIAGO, RES

STEVE SCHULTZ, Public Participant *

TODD SMITH, NRC

MATTHEW SUNSERI, Invited Expert

SHANNON THOMPSON, RES

ANDREA VALENTIN, Executive Director, ACRS

*Present via telephone

A-G-E-N-D-A

P R O C E E D I N G S

8:33 a.m.

CHAIRMAN STETKAR: The meeting will now come to order. This is a meeting of the ACRS Subcommittee on Regulatory Policy and Practices.

I'm John Stetkar, chairman of this meeting. ACRS members in attendance are Ron Ballinger, Dick Skillman, Mike Corradini, Dennis Bley and Joy Rempe.

Also in attendance is our esteemed consultant the good Dr. Bill Shack. As opposed to the evil Dr. Bill Shack who couldn't make it today.

The purpose of this meeting is to discuss state of the art reactor consequence analyses project, Sequoyah Integrated Deterministic and Uncertainty Analyses.

The subcommittee will gather information, analyze relevant issues and facts and formulate and propose positions and actions as appropriate for deliberation by the full committee.

Hossein Nourbakhsh is the designated federal official for this meeting.

The entire meeting is open to the public. Rules for the conduct of and participation in the meeting have been published in the Federal

Register as part of the notice for this meeting.

A transcript of the meeting is being kept and will be made available as stated in the Federal Register notice.

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

I'll remind everyone to please check your little communication devices and silence them.

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

However, I understand that there may be folks on the bridge line who are listening in on today's proceedings and we'll open the bridge line at an appropriate time at the end of the day and entertain public comments.

By the way, anybody up front, make sure that you push the little thing down at the base. People call it a push button. It doesn't look like a button but it says push. Turn the green light on when you're talking. Make sure the green light is off when you're not talking so we don't get extra noise.

And with that we'll now proceed with the meeting and I call upon Pat Santiago of the Office of Nuclear Regulatory Research for some introductory comments. Pat?

MS. SANTIAGO: Good morning. My name is Pat Santiago. I'm the chief of the Accident Analysis Branch and I want to thank you for giving us the full day today to make the presentation to you.

The Accident Analysis Branch over the past few years has briefed this subcommittee as well as the full committee several times on the state of the art reactor consequence analysis study.

Initially we briefed in 2012 on the Surry and the Peach Bottom consequence analyses. And we've also briefed on the Peach Bottom and more recently the Surry uncertainty analyses.

We appreciated your feedback during these briefings and we incorporated many of your recommendations into our SOARCA work.

For example, you recommended that we conduct the uncertainty analysis in parallel from the beginning of the project. And for Sequoyah we have done that.

In addition, we improved the severe accident codes and models since the original SOARCA pilot studies in 2012.

These analyses provide valuable knowledge development and experience for staff that cannot be gained through normal training activities.

The staff appreciates your comments and in particular your noted commendation of the SOARCA work as a major step forward in developing more realistic integrated approaches for analyzing important accident sequences in level 2 and level 3 PRAs.

The insights from these analyses can be useful in the regulatory decision-making process.

Most recently Sequoyah was used to inform some information that we provided in a Commission paper to close near-term task force recommendations 5.2 and 6 on other containments and hydrogen.

Overall the SOARCA project has been an important reference point for further intermediate and long-term research, both domestically and internationally. I want to point to these efforts at the end of the briefing.

On the next slide you'll see the main core team members and advisors. Many of them you recognize. We have several NRC staff. Dr. Ghosh has briefed often. We have Jon Barr and we also have Sandia staff Kyle Ross, Dr. Randy Gauntt and we have other Sandia and staff on the line from Dycoda as well.

SOARCA is a multidisciplinary analysis. There are several other NRC and contractor staff that have technical expertise who assist in completing these deterministic and uncertainty analyses.

I want to thank the team and the NRC advisors who contributed and reviewed the documentation we will be presenting on today.

In last year many of the same handful of staff were involved in the development of Peach Bottom and Surry analyses and the uncertainty analysis as well as the spent fuel pool study and more recently the containment protection and release reduction rulemaking technical analysis.

We also want to thank TVA who was very cooperative on doing fact checks for this documentation, allowed us to come down and have some site visits to gain some specific site

information. And also we were able to use their training center to hold the public meeting in April.

On the next slide you'll see the outline of our presentation. And the outline illustrates the complexity in the modeling and the analysis performed for the potential severe accident scenarios at Sequoyah.

We'll discuss what was performed for accident progression, source term, emergency response, dispersion, health effects and uncertainty analyses.

In the briefing we will include the key points ACRS noted which were critical including the understanding of the impact that uncertainties have on the outcomes.

I'd like to now turn over the briefing to Jon Barr who will discuss an overview and discuss accident scenario development. Thank you.

CHAIRMAN STETKAR: Thanks, Pat. I did want to mention, we have a tremendous amount of material to get through today.

And we have also a couple of time constraints because two of our key members are bailing out on us early.

So I want to make sure that we get through the parts of the presentations that are germane to in particular Joy and Mike's areas of expertise which is a lot of the MELCOR analyses I think.

And I only say that that -- I'm not trying to squelch members' questions, but if things start to drag on too long we can come back to other topics in the afternoon after they leave.

But I want to make sure that we have enough time to get to those topics in our agenda.

MS. SANTIAGO: Are you bailing at lunch? Because most of the MELCOR is this morning.

CHAIRMAN STETKAR: No, I'm just worried about two hours of up front high-level discussion.

MR. BARR: Okay. Thanks, Pat. I have just a few slides to give a little introduction of the presentation today.

First, for some background. Back in 2012 after we had completed the best estimate analyses of Peach Bottom and Surry, and the uncertainty analysis for Peach Bottom we submitted a Commission paper in which we recommended some limited additional analysis.

Specifically, there were two

recommendations. One was to conduct an uncertainty analysis for a Surry severe accident scenario to extend the uncertainty insights that we gained from Peach Bottom to a PWR.

And the other was to complete an analysis of an ice condenser containment to explore some of the unique attributes of that containment design.

We had actually started analyzing Sequoyah earlier in the project alongside the Peach Bottom and Surry. But we had decided to pause that in order to focus resources on completing Peach Bottom and Surry. So we had already had some information and a good point from which to continue that.

The Commission approved those recommendations and noted that they should complement and support the Level 3 PRA project as well as some of the lessons learned activities following Fukushima, specifically 5.2 and 6.

5.2 is related to reliable venting for containments other than BWR Mark I and II, and 6 is for hydrogen control and mitigation.

MEMBER BLEY: There aren't any further planned SOARCA analyses after this one, are there?

Or are there?

MR. BARR: Not formally planned.

MEMBER BLEY: How about informally?

MR. BARR: There are -- the idea is for additional analyses, but we'll talk about that.

MEMBER BLEY: We won't forget it. It's on a slide? Okay.

MR. BARR: And on slide 6 these are the five overarching objectives for the entire SOARCA project which apply to the three pilot plants as well as the base case and uncertainty analyses.

I'm not going to go through each one specifically, but I wanted to note that we have two Sequoyah-specific objectives which we'll get to today.

Those are first to expand the body of knowledge on severe accident outcomes specific to the ice condenser containment design. So we'll be looking at features that are unique such as the ice beds, the importance of igniters and hydrogen issues.

And the other was to generate information that supported NTTF 5.2 and 6.

As of March 2016 we submitted a SECY paper recommending closure of 5.2 and 6. And those

materials include some of the figures and technical results that we had generated through the Sequoyah project.

On slide 7 this is to give a little bit more of the background on why the ice condenser containment is a little different from the others.

I'm sure this is all very familiar, but I'll go through it real quickly.

This chart shows typical containment design details for the different containment types in the U.S.

In the gray bar is the design pressure. And you can see that circled in red is the PWR ice condenser which has the lowest design pressure, about 10 or 12 psig, about one-sixth of that of the PWR large dry.

And it's this relatively low design pressure among all the containment types and a smaller volume compared to the PWR large dry which leads to a potential susceptibility to early containment failure from hydrogen combustion in a severe accident such as a station blackout.

As you all know the ice condenser relies on hydrogen igniters to introduce controlled ignition sources to remove hydrogen and oxygen in a

controlled fashion before they can accumulate to levels that could challenge containment integrity.

In GSI-189 NRC studied and analyzed the importance of igniters. And one of the recommendations that came out of that was to have backup power for not only licensees of ice condensers, but also of Mark III plants.

And following that the licensees voluntarily committed to have backup power for the igniters.

And so when we had started this project that was one of the things that was inspected and in place, in contrast to some of the flex mitigation which was not installed and inspected when we were starting the analysis a year or two ago.

And as Tina will talk more about later -- so that's one of the reasons that when we talk about mitigation, our mitigated versions of the scenario is where we're referring to the use of backup power to igniters which is a little bit different than how it was done with Surry and Peach Bottom.

So as I mentioned before we really want to focus on issues unique to the ice condenser

containment. Regarding the approach, we've used the latest versions of the MELCOR and MACCS computer codes, and we've taken advantage of some of the newer modeling features that they include.

In developing the Sequoyah site and plant-specific MELCOR MACCS models we've included a lot of newly available information. We've updated core inventory data. We've used population from the most recent Census. And also some of the emergency response parameters in MACCS are informed by the evacuation time estimate that was submitted in 2012.

MEMBER REMPE: So Jon, before you go on there's some higher level comment. And I think this is the place from browsing through your slides to bring it up.

But I thought that this report, the way you analyzed SOARCA is significantly different from the other plants because you did focus on the things that were related, as you were told to, a limited scope analysis.

But then you see sentences in the executive summary saying that the SOARCA project sought to focus its resources on the more important severe accident scenarios for Sequoyah.

And even though you have focused on things relevant to the ice condenser, it's not clear to me that that's the most important thing for Sequoyah. Is that a true statement? Or should maybe it be modified a bit?

MS. SANTIAGO: We are in the process, by the way, of updating the executive summary. So some of those items have been pointed out to us.

But I still think we may say something to that effect. But we'll also try and explain what we mean by that.

MEMBER REMPE: Okay. Because then also you have a Figure ES-5 that basically you've ignored the effects of steam generator tube rupture, and interfacing systems LOCAs.

And so I think again if I don't know better I'm wondering if SOARCA has a much higher risk, or Sequoyah has a much higher risk than the other plants. Because if you added the others in it seems like you might have a higher number.

And I just wanted to bring that up because, as pointed out, I'm going to be gone later today when we have closing comments. It's editorial, but.

CHAIRMAN STETKAR: No, it's not

editorial, it's important. You ought not to compare them because you're comparing apples and kumquats.

You've made changes to the models enough here. It's a different site. You don't have a complete analysis and drawing any comparisons among the three studies is specious at best. And trying to draw any global risk implications is worse than that. From my opinion.

MEMBER REMPE: I agree, I just was trying to be nicer.

(Laughter)

MEMBER REMPE: I'll quit trying. But anyway, the other thing I was kind of wondering about was when I was reading this there's some higher-level differences that I think existed.

You've considered flex equipment. The other plants didn't which even makes it more like apples and kumquats.

MS. GHOSH: We didn't.

MEMBER REMPE: Oh, I thought some places I've seen in the analysis you've actually considered flex equipment I thought. And I can go through and point out some examples in later sections as we go through it.

MS. GHOSH: Okay. There was originally more qualitative description and discussion of what flex capabilities are there.

I think there's a little bit of discussion in chapter 2. And it's just to say that we didn't credit it. We looked at it. We know that the capability is in the process of being installed, but we don't model it, we don't credit it.

So it's to say so when the rest of the -- when people read the rest of the study it's with the knowledge that we're taking no credit for that.

CHAIRMAN STETKAR: I'm sorry, that's not quite accurate. You do take credit for -- I don't want to give it a name, Jonathan or Jon. You take credit for repowering igniters in a long-term station blackout.

MS. GHOSH: Yes, which is --

CHAIRMAN STETKAR: Well, where does the power come from?

MS. GHOSH: So, I think we'll discuss that in the next talk. That's one mitigated, but that's what Jon mentioned. The only mitigation we consider here is the potential successful powering of the igniters because they have a dedicated power

source just for the igniters, even if everything else goes wrong and nothing works.

Maybe we'll revisit how we talk about flex where we do talk about it. Because we tried to make it clear that we didn't model it. We're just recognizing that it exists and we didn't credit it.

MEMBER REMPE: I read what he read and I thought you did take credit for it. So whatever you did, please be more explicit about it and acknowledge that that's different than the other plants.

And then you did talk about how you tried to combine the uncertainty analysis with the actual evaluation.

And I'm guessing that one example might be that you looked at the mean containment failure was an example of how this approach differed.

But I was left wondering what else was different. How did this new approach affect your analysis? Could you talk a little bit about that? And make sure it's in the summary too.

MS. GHOSH: Yes, I think we will. You'll see when we talk about the unmitigated short-term station blackout, the two variations, we

do the uncertainty analysis at the same time.

So we're not focused on a base case, or a reference case anymore, because we have a whole population of potential outcomes to look at. And we're considering the insights from that whole population.

The first time around we completed kind of the individual deterministic cases and then did the uncertainty, but this time we don't focus as much on that individual initial case.

MEMBER REMPE: So I would enhance and beef up the discussion on that point because I was left wondering, well, you know, trying to think of what I read through and give some examples on how it made your results differ. And I had a short list. So I think it would be good for you guys to acknowledge what happened differently.

MS. GHOSH: But just to be clear, in the unmitigated version we don't credit flex. The backup power to the igniters is not flex, it's the GSI-189.

In reality they may choose to use something different, but that was a commitment that they had made. It was in place even before flex. So they have multiple options.

And we don't comment on how likely it is that it would be successful. We just do a sensitivity case that if they are successful in lining up the igniters' powering time that this is the outcome. It's just a sensitivity case.

MEMBER REMPE: You talk about the flex support instructions were considered in section 2.3.

MS. GHOSH: Right. And then the end of that paragraph is we do not credit anything in the analysis.

MEMBER REMPE: Okay.

MS. GHOSH: We tried to make that more clear. Because it's just to say that we know this capability exists. The first comment from the plan was we just installed all this wonderful stuff and you don't credit it.

So we want to recognize that it's there so it may be less likely we get to this very bad day scenario that we've analyzed, but we didn't evaluate it.

MS. SANTIAGO: We could maybe reorganize how we state.

MS. GHOSH: Yes.

MS. SANTIAGO: Put it right up front.

MS. GHOSH: Yes, I think that's a fair comment. We tried to rewrite it, but we'll try again to make it more clear.

MR. SHACK: Just -- I mean, flex itself sort of emphasizes the backup power if you look at the flex guidance documents.

Did any plants actually change the way they do the backup power when they went to flex? Or is it all with the old 189 resolution?

MS. GHOSH: I mean, they have additional capability now. So there was some discussion about now that they have the additional capability which power source they would use and what they would hook up.

But again, we didn't get into that because --

MR. SHACK: They just postulated it existed and looked at that.

MS. GHOSH: Our basis was really based on the older commitment that was already on the books before flex came online. And now they have new options, but we didn't get into that.

MR. SHACK: Just coming back to that earlier comment from Joy, you know, there's always this problem in SOARCA that you never really are

looking at a balanced risk kind of profile for the plant. So there's always this discussion of how far you are from the acceptance levels for long-term cancer risk.

Would it be helpful to say more up front about what the risks are for particular plants so people could recognize -- no, John doesn't like that idea.

CHAIRMAN STETKAR: Because these are not risks. Any time you use the term "risk" and compare it to anything in society you are leading the public astray because you're trying --

MR. SHACK: Well, they see it already.

CHAIRMAN STETKAR: And they shouldn't. And they shouldn't. Because they make comparative statements between, look, we didn't do a risk assessment, but we have a conditional latent cancer fatality and the frequency of the sequence is 10 to the -6, and look at this compared to the average cancer fatality rate.

Well, that's silly. That's misleading the public.

MEMBER CORRADINI: So what's silly to you is ES-5.

CHAIRMAN STETKAR: ES-5 is silly. It's

misleading the public.

(Simultaneous speaking)

MEMBER CORRADINI: And every SOARCA report.

CHAIRMAN STETKAR: Every -- and we've commented on it before.

(Simultaneous speaking)

CHAIRMAN STETKAR: What's worse is comparing this SOARCA to the other SOARCAs is the assumptions that are built into this SOARCA are different. They have different evacuation models in here. They made different assumptions about a large fraction of the population that leaves very, very early, but they didn't in the other study.

So how am I to believe any of this stuff in terms of the relative effects from early versus late releases and so forth?

MEMBER CORRADINI: I guess if I can jump into the non-modeling discussion.

It strikes me that if I were to write the executive summary I would say it's not a risk study, it's a consequence study.

Since it's a consequence study we're considering specific scenarios. And if you're going to go between Surry and Peach Bottom and

Sequoyah I'd replace ES-5 with something that said for this scenario with this estimated frequency we get this sort of response. I'd have an x-y plot like to like.

Because that's a consequence study. And so now you're going to take some assumed scenario that has some frequency in these three plants, and then a calculated consequence with godawful uncertainties. And I'd plot that.

Otherwise ES-5 does come across as if it's a risk calculation. That would be my way of explaining it to the public.

The other thing too is since we're only talking about appearance and not substance is that when you go through these -- earlier on when you go through these tables I think you do a very nice job with table ES-1 and ES-2 of what you're doing.

But then I immediately started reading the text to say what does that mean. So it would strike me you want to explain what does it mean by no random ignition source, or when a random ignition source.

Or tell them -- I hate to sound like a publication person, but if I write a table and I've got all this stuff in the table I'd have a lot of

subtext that said go look here to explain this thing. Go look there to explain this phrase.

Then the reader who will not read 454 pages but who will just read the 6 pages will know where to go.

It seems to me you need to give a roadmap so that they understand it's a consequence study between three reactors with three assumed scenarios. We're going to tell you with those assumptions what's the mean, median and range of consequences.

Then at least I think that's what you're doing.

CHAIRMAN STETKAR: And actually, when I read the executive summary my notes are until I got to that figure whatever it is, ES-5, it flowed okay.

You're asking for more pointers back in the study, but I at least understood what it was and what it wasn't.

And then suddenly I see this comparison among the three SOARCA studies. And I know they're different.

And I see these statements about, well, look at what the total cancer risk in society is,

and I know that's not an appropriate comparison at all because none of these are risk studies.

So it kind of fell apart when you felt the need to do some of those broad comparisons.

Up until that point it was kind of describing what it was and what it wasn't.

MR. SHACK: But you're misled both ways. I mean, if you put in a conditional probability of a result without a notion of what the frequency of that is then --

CHAIRMAN STETKAR: But Bill, this was never supposed to be a risk assessment.

MEMBER CORRADINI: But if you populated a graph which was just frequency and consequence.

CHAIRMAN STETKAR: No, don't do that, because you don't know the -- they're guessing about a frequency. I will tell you that the frequency of a station blackout might be a lot higher than that from other things that they haven't thought about.

MEMBER CORRADINI: You're saying they don't even know the frequency.

CHAIRMAN STETKAR: They don't know the frequency. They're just -- suppose the frequency was 10 to the -6 ish.

MR. SHACK: Well, they have the frequency for an internal events of PRA of 5 times 10 to the -6.

MEMBER CORRADINI: I'm kind of with Bill on this. You've got to give them some comparison.

CHAIRMAN STETKAR: No, you don't. They're not doing a risk assessment. They are not doing a risk assessment.

A risk assessment has frequency and consequences. They're doing a consequence evaluation.

Which is fine. There's nothing wrong with doing that, but don't try to say you're doing more than you did. Don't try to say it.

MEMBER CORRADINI: I guess I'm with Bill on this though, John. It seems to me you can at least cast it so that on some common basis I know what the frequency is and I put it here, and I know what the consequence is and I put it here.

Now all my effort for 454 pages is to look at how the consequence changes given the assumed sequences of some estimated frequency. That's what I think Bill is just saying.

MR. SHACK: All I want to do is

emphasize that they're looking at a specific narrow set of scenarios in the risk they're talking about.

(Simultaneous speaking)

CHAIRMAN STETKAR: They're not looking at risk. They're looking at consequences.

MR. SHACK: They do turn it into --

CHAIRMAN STETKAR: They try to, and I'm trying to make them not do that because they're not doing that. And it was never intended to do that.

The frequency for what they're doing to demonstrate an integrated what we've learned about modeling accident behavior both sequence analysis, MELCOR containment, MACCS, integrating the two and propagating uncertainties through those things is a very, very valuable piece of work.

And let it be that valuable piece of work. Let the risk assessment people take that knowledge and those capabilities and use them in a real risk assessment where you have a delineation of sequences, a delineation of timing, a delineation of contributors and frequencies associated with them.

Don't try to make it more than it is. That doesn't mean that it's bad. It's great.

MEMBER CORRADINI: Is that on the

record?

MS. GHOSH: Can I just -- this is dangerous, but I just want to point out one small thing.

The figure 5, those are all the conditional outcomes. We didn't multiply the frequencies. We didn't multiply the scenarios.

CHAIRMAN STETKAR: I'm aware of that, and yet if I compare -- as Joy mentioned, this study did not look at consequential tube rupture. Surry did.

MS. GHOSH: Right.

CHAIRMAN STETKAR: So this one is biased low, by how much I don't know because the conclusion for Surry was consequential tube rupture was a really big deal. So this one is biased low because of that.

This one is biased low also because Surry -- we'll get to this later -- did not include the early 50 percent of the population immediately going away. It did not include cohort 4 in the MACCS model. It didn't. So this one's biased low because we're getting a big chunk of the people away out of the EPZ early.

So how can I compare now Surry versus

Sequoyah even on the same sequence? Because I've got a big chunk of the population moving out faster in this one compared to Surry. So how can I compare the two? Even on consequence.

Despite the fact that you're going to say, well, we know that it's all people coming back in.

You know that under the assumptions that have been made. If the early stuff is more important maybe the conclusions would be different. So how can you compare the two?

MEMBER REMPE: Just a last point again at a high level. I think there are a lot of insights embedded as I read through the report just on phenomena and consequence behavior.

As you're updating the executive summary if you could bring some of those nuggets up I think that would be good to again show this is a good piece of work. And that was the other high-level comment I wanted to make sure I communicated today. Thank you.

MS. GHOSH: Thank you.

MEMBER BLEY: I'll throw in one last comment. I don't think it's repeating bits and pieces.

I see no reason why one can't do a study and look at a scenario, and look at frequency and consequences of that scenario. You can. We do it in many different places.

But you can't make comparisons across studies that have very different assumptions. And I think John hit on a couple of the things that would make me very suspicious of these results being comparable. And that's very dangerous.

And any hint overall that we're talking about the overall risks from these plants is clearly not true. It's a consequence study that may have appropriate, maybe sometimes not appropriate, frequencies associated with them.

MR. BARR: So, just to finish the last bullet on slide 8.

(Laughter)

MR. BARR: One of the features of the approach here was to consider uncertainty and accident progression and consequence analysis.

And this time we did it in parallel with the base case analysis instead of as a follow-on as was done earlier.

And now Tina will discuss the accident scenarios and the different variations.

MS. GHOSH: Actually, we already started talking about some of the things I wanted to mention.

And I do want to go through this portion of the talk quickly because we actually don't do anything on that front end frequency part. We're just drawing on information from other studies.

What we did was really on the consequence part. So I think that's more interesting to talk about.

But just to set up, I'll just go through what our front end assumptions were going in.

So I think Jon already --

CHAIRMAN STETKAR: Jon, if you're not talking turn your mike off.

MS. GHOSH: So, I think Jon mentioned this time this was a more limited scope study compared to the first two SOARCAs where they really went through a screening process to decide which scenarios to study.

This was meant to be a more limited scope quicker study that was very focused on the ice condenser containment issues.

And we recommended to the Commission that we do this for station blackouts only and they sent back a staff requirements memorandum that said focus on station blackout.

So we had some initial work back in 2008 when we had started the Sequoyah pilot plant but then paused to look at what kind of station blackouts we might want to look at.

Because we were picking this up years later we had our PRA folks look at the latest scram model that we have for Sequoyah to look at what would be good scenarios to study, and also considered on the site-specific external hazards information for the site to figure out what would be candidate scenarios.

And we've already talked about this, but the approximate core damage frequency contributions are really just provided for contextual information in terms of helping the reader understand how likely or unlikely is this very bad day that we're about to talk about.

So, I guess this is no surprise. The initiating event that this process kind of pointed to would be a good one to look at was a large seismic event. By large I mean beyond design

basis. In this case something greater than a 0.5 g peak ground acceleration.

And with this initiator we looked at two variations of station blackout. Again, no surprises. These are ones, the two categories that traditionally people look at, either a short-term station blackout where you not only have lost all ac power but you don't have your turbine-driven aux feed system working for whatever reason, and we'll talk about that on the next slide.

And this is a less likely more severe version of the accident.

Again, just for context we approximate the CDF contribution to be about 1 in 500,000 years of reactor operation. And because we didn't look at flex we don't credit anything that might change that frequency. Yes.

CHAIRMAN STETKAR: Tina, in the short-term station blackout since you're the scenario person and I like to think about scenarios, do you have dc power available or not?

MS. GHOSH: You know, we tossed this around amongst the team. And we're going to get to that on the next slide, how do you get to the short-term station blackout. Let me get to that on

the next slide.

The other variation we looked at is the long-term. And in this case your batteries did survive and your aux -- you have a water supply to the aux feed system. So your aux feed is working until sometime after the batteries deplete.

And this one is a little bit more likely, less severe accident within this class.

So, the key assumptions for both of these variations of the station blackout.

We assume that the steel containment, the containment isolation systems and the ice condensers have survived in both of these variations of the accident.

But then in the short-term station blackout we assume that the turbine-driven aux feed system is not available and it could be due to one of these three things that we mentioned. Either you don't have dc power batteries, you don't open the steam valve to the turbine-driven aux feed system, or you don't have the water supply. Maybe the CST tank failed. And we don't credit any recovery actions to try to get one of these.

So, it could be the loss of dc power in the larger seismic events. We don't expect that

you would have dc power, but it could also be that you just can't get water to the system. But for whatever reason you just don't have your turbine-driven aux feed system.

CHAIRMAN STETKAR: Okay. Thanks.

MR. SHACK: -- survived the seismic event.

CHAIRMAN STETKAR: No, the reason I bring up is they presume the containment isolation is 100 percent successful at T zero.

MR. SHACK: You assume everything is hanging together after the 0.5 earthquake.

CHAIRMAN STETKAR: Yes. But it's a short-term station blackout. If I have no dc power that's not true. Containment isolation is not successful.

I don't know whether Sequoyah has any ac-operated containment isolation valves. I don't know if it has any dc-operated containment isolation valves. But I doggone well know that if I don't have dc power I don't get Safeguards actuation signals to close those valves.

So, somehow dc power has got to be somehow available if I assume containment isolation is successful.

So, when you develop your scenarios, make sure that you describe precisely what you're assuming. It can't be this wishy-washy, well, you know, there's a whole bunch of different ways that the turbine-driven aux feedwater pump might fail, and one of them might be loss of dc power, because your entire scenario and your risk profile would be much different if you didn't have dc power. So you must have dc power if containment isolation is successful.

MR. SHACK: Or the model's wrong.

CHAIRMAN STETKAR: Or the model's wrong.

MS. GHOSH: I guess we did have the plant look at this and they spent a lot of time on the up-front scenario description. And we didn't get that particular feedback from them.

CHAIRMAN STETKAR: Okay, I'll ask you a question then since you're modeling the Sequoyah plant and had feedback from TVA. Does the containment isolate successfully if you have no dc power? That is a simple question, yes or no.

If you don't know the answer to that question, get feedback from TVA.

MEMBER CORRADINI: Can I try a

different way of asking John's question? Which is are the assumptions here for short-term station blackout the same as the assumptions in Surry?

I don't know enough about the details that John worries about to worry about those details.

My question is is the starting initial and boundary conditions the same between the two.

CHAIRMAN STETKAR: We asked the question on Surry and got the same non-answer.

MEMBER CORRADINI: Well, okay. Regardless.

CHAIRMAN STETKAR: The point is if you're modeling a specific scenario you ought to understand what's in that scenario and what's not for the plant that you're modeling.

Because we're now modeling a plant. It's not a generic plant. It's not an amorphous Gedankenexperiment. You're claiming this is Sequoyah.

MS. GHOSH: Right.

MEMBER BLEY: And you're claiming the consequence results make sense.

CHAIRMAN STETKAR: Right.

MEMBER CORRADINI: So, can I at least

ask my question and get an answer? Were the assumptions for the short-term station blackout and the long-term station blackout identical assumptions as you used in Surry?

MR. ROSS: Yes.

MEMBER CORRADINI: Okay. So, real or unreal, at least they're the same.

MS. GHOSH: Yes.

MR. SHACK: Except they didn't consider tube rupture.

MEMBER CORRADINI: Well, I mean for the scenario, for the particular scenario.

MS. GHOSH: Right. We didn't do tube rupture here.

MR. SHACK: It becomes an assumption.

MS. GHOSH: So, the next slide just shows I guess in a little bit more detail what the boundary conditions are for the MELCOR simulations.

So the earthquake has happened. The reactor trips. You don't have any ac, any pumps that rely on ac don't work. Valves that are designed to fail close to isolate the systems and the containment, including the MSIVs.

The emergency power, nothing else is working, and the turbine-driven aux feed system

isn't working. Your safety valves start opening to relieve pressure. And we do model RCP seal leakage.

And then the --

CHAIRMAN STETKAR: You model RCP seal leakage and I couldn't -- at 21 gpm. I never heard anything more than 21 gpm in terms of a progressive, either in the short-term or the long-term.

Do you model an actual RCP seal leakage model? Because if I look at the level 3 PRA they had a very, very intricate that they've simplified, but different probabilities of seal leakages that get up to I think 480 gpm per pump, like 1,900 and whatever that is, some odd gpm total.

And I didn't see any discussion anywhere other than this notion that you said you have 21 gpm.

MR. ROSS: Right. In Surry we investigated the seal leakage variability as an uncertain parameter. We didn't do that in Sequoyah.

The 21 gpm is at pressure so as pressure comes down the leakage would as well.

CHAIRMAN STETKAR: But if pressure

doesn't come down it will go up.

MR. ROSS: So, if pressure were to go above normal --

CHAIRMAN STETKAR: I'm sorry, if you remain at pressure over time with no cooling the seal leakage rate will expand considerably up to quite the Westinghouse models, 1,900 gpm total. Which is a different -- changes your timing of core damage, changes your core recovery considerably compared to what you've modeled.

MR. ROSS: Yes, we didn't capture that.

CHAIRMAN STETKAR: So that's another difference.

MEMBER CORRADINI: But you can't beat them up in both directions, right? You can't tell them to make it identically similar and then simultaneously say make it --

CHAIRMAN STETKAR: Well, the question is whether they have different seals. That's a possibility.

The 21 gpm for both plants tells me they have the same seals.

MR. SHACK: Well, there's some cryptic note in there that you've turned off a mechanistic model in MELCOR for seal leakage and you use some

Westinghouse tabulated one. But that means just what it says.

CHAIRMAN STETKAR: That's why I had a question about did you model it or not. I think the answer is, as Kyle said, no, they didn't. Other than the 21 gpm.

MR. SHACK: The report says something else.

MS. GHOSH: It doesn't vary with pressure as the simulation goes on.

MR. ROSS: It does vary with pressure, but it doesn't grow.

MS. GHOSH: It doesn't grow. Okay.

CHAIRMAN STETKAR: The difference is -- again, I compare this to another effort that the NRC staff is heavily invested in and that's the level 3 PRA model where their assumption, right, wrong, or indifferent, is that at time T = 13 minutes after T-zero the seal leakage increases and there's a probability anywhere from -- there's some probability that it remains at 21 gpm, but that's small.

And there are different probabilities that it increases anywhere from there up to their discrete values up to 480 gpm per pump. And that

substantially affects the timing of their station blackout scenarios. Substantially.

MR. SHACK: But it says in the report disabling the mechanistic pump seal failure modeling and instead applying discrete leakage values per Westinghouse owners group.

CHAIRMAN STETKAR: Yes. But the only discrete leakage value that I could find was 21.

MR. SHACK: At least I assumed somewhere within the calculation. It started at 21. It could have done something else.

MR. ROSS: Yes, the actual modeling gives 21 gallons per minute at nominal pressure. And then it would reduce as pressure came down. But the leakage area if you will doesn't change or grow.

MR. SHACK: Okay, but does that mean that they really have different seals than Surry does?

MR. ROSS: So we didn't corroborate that.

MR. SHACK: You don't know.

MR. ROSS: Right.

MR. SHACK: Why did you decide to use this model rather than the Surry model? Is that

something the plant told you?

MR. ROSS: No, I think we felt like we had investigated this issue in the Surry analysis and so we were trying to keep things more contained in this analysis.

MR. SHACK: It just seems like this could affect what you were interested in here --

MEMBER CORRADINI: You're not --

MR. ROSS: Yes, the leakage rate could affect the time to the onset of core damage. Yes.

MS. GHOSH: It didn't show up as important for Surry with all the other parameters we had considered. I think that's why we decided to leave it off this time around.

CHAIRMAN STETKAR: I have an impression that I think I know why you did what you did, because you wanted to focus on hydrogen and the size of the Sequoyah containment. And you wanted to test the modeling for those phenomena. That's fine. That's fine.

But it comes back to don't compare it to anything else then.

MEMBER CORRADINI: So, can I try it a different way? Because maybe this won't satisfy Member Stetkar, but I think maybe the executive

summary, maybe you're trying to satisfy too many missions simultaneously. So maybe that's the way of asking you guys. Was the primary mission what he just said?

MS. GHOSH: I think we wanted --

MEMBER CORRADINI: All other things are secondary missions to the work?

MS. GHOSH: We wanted to get unique insights to the ice condenser containment. You know, what would be different because of the containment design.

So I think it's fair to say that that was the main focus of this study.

MEMBER CORRADINI: Okay. So then maybe the way one would at least address the executive summary is this is our main mission. We might have secondary missions, but to do our main mission we simplified in X, Y and Z.

Because I view some of these things as simplifications from what you learned in Surry. And you wanted to focus on essentially hydrogen behavior.

So if that's the case then comparing it between plants becomes less important than essentially trying to determine how the

consequences are affected by hydrogen behavior.

My only point is that if you're trying to address too many things simultaneously then you'll get criticized on all three, whereas your main mission was hydrogen behavior in a unique plant containment design.

And maybe coming up front in the executive summary and explaining it that way would help folks understand.

MEMBER BALLINGER: With maybe some additional words related to the differences between the two and what the impact might be. Differences between this one and the others. A couple of paragraphs or something so it cuts the head off the snake that we've been talking about.

MEMBER SKILLMAN: May I please ask a question?

MS. GHOSH: Yes.

MEMBER SKILLMAN: I understand the physical plant bullets that you present on slide 13.

But as I read the documentation I was asking myself where are the other peculiar assumptions for this analysis.

I'm going to ask a question. Don't

answer. Have you ever been inside an ice containment plant and gone into the areas where the ice is located? Don't answer.

If you have you will find that there are large doors. And it turns out they can move literally at the tap of your finger. I mean, they are activated by the most gentle delta p. That's a good thing.

What's interesting is when you go on the other side here is a maze constituted of many of these baskets. Unless the ice is in the basket and the ice is the correct geometry that has the right area and the right interstitial flow area you won't achieve the heat reduction and absorption and pressure reduction that you're assuming.

So it seems to me then that in addition to these assumptions there needs to be another set of assumptions that say this many grams or this many pounds of ice assuming this specific heat, and this latent heat vaporization, or phase change, and this amount of area because those and the uncertainties around those have major effect on what the pressure response will be.

So, where are those assumptions that are so unique to this containment design?

MR. ROSS: So, I don't believe that we did a good job of getting those into the report. We have tried to verify those numbers well, and certainly they are very important numbers.

We didn't look into the variability of them as part of the uncertainty analysis.

MS. GHOSH: There's some discussion in chapter 2 about what might happen to the baskets, the ice condenser part of the containment.

And in chapter 3 the model description, we explain how much ice we assume to be there. So we didn't vary the amount of ice.

What we did vary was the door position once it blows open because that can help control how much flow is going to actually get through the baskets.

So, there's some discussion in there, but we didn't include the ice mass as one of the uncertainty parameters.

MR. ROSS: No, but I think it's important to realize that the function or the involvement of the ice baskets in a station blackout are quite different than involvement of the ice baskets in a design basis accident for which they were designed for, being a large LOCA.

So there's a lot of ice in a station blackout. Flow moves into it gradually. There is a larger emission of steam when the hot leg fails. But nothing on the order of a large break LOCA.

So, we did look at some variabilities of what happens to the ice condenser doors, but we didn't vary the porosity of the ice or look at what plus or minus mass of ice there might possibly be.

CHAIRMAN STETKAR: We're going to get into the ice condensers later, right?

MS. GHOSH: Yes.

CHAIRMAN STETKAR: Joy has to leave at 11.

MEMBER BLEY: My comment about the frequency before I still agree with, you can look at frequencies.

But if you can't answer the kind of questions John was asking for things that I'm almost certain were modeled in the SPAR model like the reactor coolant pump seal LOCA which must have been modeled for their blackout scenarios then the frequencies don't really apply to the case you're looking at.

All those kind of comparisons are suspect.

MS. GHOSH: Yes, and certainly our PRA folks who are in the audience can tell you they did look at that.

The seal LOCA variation was much less likely, about five times less likely than the other things. So we didn't focus on that.

But that is in the SPAR model and that would just have been a much less assessed CDF contribution if we looked at that variation.

The only thing I want to point out on this slide, the beginning of the LTSBO is exactly the same.

I just want to repeat because this gets confusing. When we say unmitigated we are still crediting a couple of EOP-based human actions. So it's not -- it's different. The SOARCA parlance is different from what you might hear in the PRA world.

So the two actions we credit are nominal dc load shedding as well as secondary depressurization.

Oh sorry, that was short-term. Okay, this is the long-term. Yes, so that's the difference of long-term.

And then you have your aux feed system

working for a while until batteries deplete.

MEMBER CORRADINI: I'm sure Kyle will get to it, but I just view the difference is that once I lose aux feed I just move in time everything that I see.

MS. GHOSH: Yes.

MEMBER CORRADINI: In terms of assumptions. That's it.

MS. GHOSH: Right. So, the only mitigated scenarios we looked at this time, we did a sensitivity case for each of the scenario variations, that the backup power to hydrogen igniters is hooked up successfully and in time.

And for the STSBO we picked a realization from our uncertainty set where we had early containment failure and then we credit the igniters and see what happens.

And with the long-term sensitivity case we used the base case inputs and then made igniters available.

CHAIRMAN STETKAR: Do you know at Sequoyah -- I know what you did on long-term station blackout. When they shed dc loads, do they shed dc loads that would be needed for them to actuate the igniters? You say they have to be

manually actuated from the control room. Somebody has got to switch them on.

Some plants shed dc loads that they think aren't necessary to prevent core damage, but might be necessary to save the containment because they're not necessary to prevent core damage.

MR. ESMAILI: If I understand your question correctly we don't need -- Hossein Esmaili -- we are relying on ac power for igniters.

CHAIRMAN STETKAR: I don't want to waste time on -- if I flip a switch typically the switch has dc power that goes out and actuates a relay. That relay closes a set of contacts which energizes ac power. That's what I'm talking about.

MS. GHOSH: Okay. So, the scenario variations we did look at, we just wanted to provide the summary slides. So it gives a summary of what we did the uncertainty analyses on, which things we only did sensitivity analysis on.

So, for the short-term station blackout we looked at two variations of the unmitigated, which means no igniters are available.

In one Monte Carlo simulation we did not credit the random ignition sources. In one set we did. So there are some ignition sources that

are known. You know, when you have a hot leg or RPV breach those are always modeled.

But then there are unknown sources that could also trigger an ignition. So that's the difference between those two sets.

In the case where we credit the random ignition sources we have both the known sources as well as the potentially unknown sources. And Kyle will get into how we modeled that when he goes through his model description.

And for both of those sets we did a Monte Carlo simulation set separately. So we did an integrated uncertainty analysis on those two variations.

Then for the long-term station blackout the approach we took was we had a base case or a reference case, and then we varied some of the parameters that we know to be important and did a set of sensitivity cases to get a range of outcomes.

And for both the short-term and the long-term station blackout we did sensitivity cases only in terms of crediting the igniters. We didn't do an uncertainty analysis on potential igniter, the igniter scenarios.

MEMBER CORRADINI: Just a clarification. Probably Kyle or somebody is going to get to it.

So, I'm curious how the random ignition sources are correlated with equipment behavior. They're not?

MS. GHOSH: They're not.

MEMBER CORRADINI: Okay, so then I'll have a different question later. Thank you.

MS. GHOSH: Okay, so at this point I'm going to turn it over to Kyle for the MELCOR model overview. The next slide.

MR. ROSS: Very good. So I'll describe the MELCOR model for Sequoyah. I'll try to pass over things that are identical or largely similar to the modeling that was done for Surry. But if there's anything I'm talking too much about please tell me or something I'm not talking enough about please say.

MEMBER REMPE: Kyle, just out of curiosity, when I was reading the report it often refers to the earlier Sequoyah model. What was the earlier Sequoyah model for? Was it the 1150 studies? Where was it developed?

MR. ROSS: Well, this model goes back

probably 10 years or so. It was developed --

MS. SANTIAGO: Back in 2005 Dr. Tenkler wrote a paper and we were going to look at several different designs. And so the initial set was reduced down to eight and then ultimately to three. And they were Peach Bottom, Surry and Sequoyah.

So we started the Sequoyah probably in 2008. Tina mentioned that we paused that analysis because of the complexity for Peach Bottom and Surry just to move those pilot plants forward.

And then when we sent the Commission paper up on the initial pilot studies we did recommend that we take a look at Sequoyah because of the ice condenser containment.

And as you know Fukushima happened as we were completing.

MEMBER REMPE: I think you're not understanding my question. It's a bit different. Where was this Sequoyah model for MELCOR developed? Was it developed for the GSI-189 studies?

MS. SANTIAGO: It was SOARCA.

MEMBER REMPE: It was solely -- so the original model was solely developed for SOARCA and then it was an update of the older one for SOARCA. Okay. I was just curious if it had a different

pedigree.

MR. GAUNTT: This is Randy Gauntt. I probably have some corporate memory here that's helpful.

But this Sequoyah model was used in the 5046 revisiting hydrogen control at least 10 years ago. I forget when we did that with MELCOR 185.

And it was one of our actually first uncertainty applications where we were looking at what kind of spectrum of hydrogen could you expect in station blackout in Sequoyah.

And going further back than that I'm not exactly sure where that Sequoyah model actually originated, but we've had it for quite a long time.

MEMBER REMPE: So my comment in the report is again editorial, but when you talk about the earlier Sequoyah MELCOR model please say what you mean and put a reference of where it was. Okay? Thanks.

MR. ROSS: So, the model is equally representative to our knowledge of either of the Sequoyah units.

It's a 2.1 general purpose model for transient reaction modeling but has only been exercised for station blackout scenarios.

It's current with respect to fission product inventory, consistent with middle of cycle, and it has the current steam generators, the replacement steam generators represented.

MEMBER CORRADINI: Kyle, maybe you're going to get into this. I haven't looked over all your slides.

But when you guys did the Surry uncertainty you had an Appendix A which I thought was very helpful where you said, okay, here's 186, the whatever you call it, baseline best estimate, whatever it is, in SOARCA.

And now we're going to run 2.1. We're going to show you the evolution unmitigated just so we see traces of how things behave.

And they're kind of the same, and when they're not the same we think this is happening or this is happening.

Do you do that here either between this Sequoyah and Surry, or between this Sequoyah and the old Sequoyah?

What I'm trying to get at is some sort of -- I won't use the word benchmarking. Comparison so once you went to a 2.1 model of Sequoyah you knew when you got a different behavior

you could identify it. Did you do that sort of -- like you did in Appendix A of the Surry?

MR. ROSS: We did do that. We did go through those.

MEMBER CORRADINI: Did I miss it?

MR. ROSS: No.

MEMBER CORRADINI: Somewhere in here.

MR. ROSS: So, it sounds like we have not documented.

MEMBER CORRADINI: I couldn't find it. I was looking for the equivalent of Appendix A in the Surry uncertainty where you guys kind of did this one to one.

I think again for the sake of modeling then everybody goes okay, at least we know we're on the same common ground, however realistic.

MR. ROSS: Very good. Sounds like we came up a little bit short in our documentation here.

MEMBER CORRADINI: That's all right, I just wanted to make sure I didn't miss it. Okay. Thank you.

MR. ROSS: Let's see. One of the loops, the loop that the pressurizer is connected to is represented individually. The other three

loops in the Sequoyah model are lumped together. Reactor coolant pump seal leakages is represented at 21 gallons per minute at pressure. And hot leg rupture modeling is included.

The active core is represented with 12 axial core levels, five radial core readings. Those are coupled to CVH volumes that are -- which are five stacks of four volumes.

The nodalization has been developed with the need to support in-vessel natural circulation well. The lower head is distritized into seven segments with 10 nodes through the thickness of the steel.

MEMBER CORRADINI: Again, just to make sure. So this is, in terms of five radial rings downcomer, lower plenum, et cetera, et cetera, this is essentially Surry.

MR. ROSS: Yes, that's right.

MEMBER CORRADINI: 2.1 Surry, sorry.

MR. ROSS: Yes. Surry is a three-loop plant so there's some differences.

MEMBER CORRADINI: Right. Okay.

MR. ROSS: The upper reactor vessel nodalization is detailed again with natural circulation in mind.

The steam generator nodalization has been destructed to support hot leg countercurrent natural circulation. The circulation is managed very proactively with flow path openings and closings, and momentum additions that are necessary in a code like MELCOR, a pipe-type code like MELCOR to generate the kind of flows that have been identified in CFD work done by the NRC and documented in NUREG-1922.

As we talked earlier we did not make the effort to do the best estimate modeling of steam generator tube rupture possibility in the Sequoyah work. So the hot tube modeling that we did in Surry isn't active in the Sequoyah model.

MR. SHACK: One of the things here, you didn't do any uncertainty analysis on the hot leg failure in the natural circulation model which you didn't do that in Surry either but it seems less important there than it does here where that's sort of a major ignition source.

MR. ROSS: Right.

MR. SHACK: What the condition of the containment is when that ignition source goes off. So I mean, you looked at all the variation in the condition of the containment, but you didn't look

at the variation that you could get in the ignition timing.

MR. ROSS: Ignition timing. So with respect to the modeling of the hot leg nozzle failure? Yes.

So, what we did do in Surry was affect -- as an uncertain parameter we affected the strength of the natural circulation. So that played on when the hot leg would fail.

So we felt like there were several parameters that we might have sampled to affect hot leg failure timing, and we chose one as uncertain in the Surry study. And we did not bring that uncertainty into the Sequoyah study.

MEMBER CORRADINI: Can you remind us what that uncertainty was? I've forgotten.

MR. ROSS: Let's see, there's a factor that relates to how hot the hottest tube gets and the uncertainty of that pointed out in the -- it was a non-dimensional parameter. I'm sorry, it's been a few since I've looked at it.

MR. SHACK: Yes, but that was aimed towards the tube rupture problem in Surry whereas here I think the hot leg rupture timing is important to how big that hydrogen detonation.

MR. ROSS: It is. We found that to be quite important.

MR. SHACK: But then you didn't look at that as an uncertainty here, and that seems to me a question of why not. It seems like an important variable for something that you were focused on which is the hydrogen.

MR. ROSS: The hot leg timing is -- we did certainly find that to be quite important.

We -- something important with respect to that is the modeling that we have in place is the Larson Miller tree which if you look at the temperature response of the hot leg there's a very steep rise in temperature.

So what the comparison winds up being for hot leg rupture is the reduced strength of the steel given the severe temperatures compared to the --

MR. SHACK: That could all be affected by the natural circulation. Larson Miller is certainly one thing that could affect the hot leg rupture, but there are certainly others.

MR. ROSS: Right. So that's --

MR. SHACK: I mean, there's some uncertainty in that CFD modeling. We take into

account the uncertainty in the finite element modeling of the containment structural response, but not the CFD natural circulation here.

It seems to me that seemed like the most important parameter that I could see left out of the uncertainty analysis for which you were interested in.

MR. ROSS: Yes, hot leg failure is certainly important. As just to talk for a little bit of defense, we didn't know going in what we were going to find as a result.

It's the same with the safety valves. We didn't realize the dramatic influence that failure of a safety valve would have on whether containment rupture is due to hydrogen burn or not. And so we didn't really know going in what was going to be an issue.

MR. SHACK: That one ends up at the top of your regression analysis whereas there's no regression on any of the parameters that might affect the rupture time when I suspect that might also be high if they had been included. But you never know.

MR. ROSS: Yes, I think it's true.

MR. OSBORN: So, this is Doug Osborn.

We did look at the CFD work for -- back when we were looking at uncertain parameters for Surry.

And what we determined back then was that trying to apply some uncertainty to that CFD model would be very involved, so much so that it would probably be more costly than we wanted to give it benefit so we ignored it and that carried over into the Sequoyah analysis.

And in regards to the Larson Miller uncertainty when that damage factor starts to take off it really goes off on an exponential. So to vary it from a Larson Miller of equal to 1 to like 0.1 that only varies it by seconds or minutes as far as hot leg failure.

MR. SHACK: But I mean, having done a number of those calculations from the CFD things we got heat transfers coefficients that moved locations of failure up and down the piping system.

And whether those are -- the final version is the true and final version.

MR. OSBORN: Right, but the issue is trying to take those CFD models and apply them into a system model like MELCOR is rather involved. And to do that for each variance or uncertainty, to go and vary that within each one of the MELCOR model

realizations can be very time-consuming.

CHAIRMAN STETKAR: This is something I know nothing about, but I am sensitive to people who say they do an uncertainty analysis but don't.

What I hear you saying is there could be substantial uncertainty from that phenomenon.

MR. SHACK: At least I think so. I mean, it could change the timing considerably.

CHAIRMAN STETKAR: And the staff did not examine that.

MR. SHACK: Right.

CHAIRMAN STETKAR: And yet they know now that indeed the timing of that failure could be a substantial effect on the overall results from this study. Right?

MR. SHACK: It seems to me a possibility, yes.

MS. GHOSH: So I think that's a fair comment and thank you for pointing that out. We will take it under advisement going forward in terms of how we talk about it in the report, or if we can do future work.

CHAIRMAN STETKAR: The point is just because it's difficult and time-consuming to do an uncertainty analysis doesn't mean that you ought

not to do it. Because your concept of the amount of time that's required, you could probably fit in -- you've done some very crude -- we'll talk about this later -- characterizations of uncertainties elsewhere. You could have probably done a crude characterization of uncertainty here. That's sort of what I had in mind.

When I say crude I mean intelligent, but not, you know.

(Simultaneous speaking)

MS. SANTIAGO: I think it's fair to say we need to add something to the report that's somewhat along the lines, or do future work like Tina said.

CHAIRMAN STETKAR: Typically a lot of times if you get some initial results they kind of tell you a little bit about what you ought to be thinking about, and maybe refine things a little bit rather than just saying well, these are our results.

MR. ROSS: On the pressurizer safety valves I think here I was just going to briefly describe the system of valves and what we accommodated in the modeling, and then talk about what we assumed failure wise when we talk about in

this parameter description presentation.

So, there are three safety valves atop the pressurizer. They are in parallel. And what happens to the three parallel valves as a system is more important to consider than what happens to any of the valves singly.

There are failure to open and failure to close possibilities. If a failure to open occurs or a failure to close but in a mostly closed position pressure relief would transition from the affected valve to the next set point valve that would be lower oval state 1 to state 2 there.

If failure to close occurs the RCS vents are regulated to containment which for example would be state 2 to state 4 which would be lower left to the right differently colored, yellow colored oval.

If by rare chance all three valves failed to open state 5, the purple oval would be obtained and there would be no pressure relief.

Just to mention that in the sets of MELCOR calculations most often only the lower set point valve wound up being exercised. The other valves didn't come into play.

But there were certainly a significant

number of cases where pressure relief did move up to second and even the third valve.

CHAIRMAN STETKAR: We're going to talk more about the parameters later, but before Joy and Mike leave I wanted to make one comment and get some feedback.

This diagram that you have here, as I understand it these valves are modeled in MELCOR as if they are completely independent. Is that correct?

MR. ROSS: Yes.

CHAIRMAN STETKAR: Okay. Such that I can have a probability P1 that valve V1 fails to close with area A1, and a probability P2 that valve V2 fails to close with area A2, and so forth for valve V3.

That is not correct because if we account for -- this is a state of the art uncertainty analysis, and state of the art uncertainty analyses say that I have the common state of knowledge regarding all three of those valves such that if the world works according to sample 1 from my model of the world then all three valves work the same way.

I don't have independent probabilities

of failure to close, and independent open areas. They are all the same.

MS. GHOSH: Let me clarify.

CHAIRMAN STETKAR: Now wait, let me finish.

MS. GHOSH: Because that's not what we did.

CHAIRMAN STETKAR: That is what you did or your tables are wrong because you have summary tables that show me three separate --

MS. GHOSH: -- let me tell you what we did.

CHAIRMAN STETKAR: Okay, what did you do?

MS. GHOSH: We -- I know this gets confusing because we can talk in terms of a composite probability where we considered the epistemic and the state of knowledge correlation there. And then the stochastic nature of when they actually fail.

If you combine those together versus doing it, thinking of them separately.

So we have a common failure probability across the three valves that's sampled in each Monte Carlo draw. So each realization has one

failure rate for failure to open and failure to close that sample.

But given that failure rate we tried to do the second level of stochastic sampling that given that same failure rate because it's a failure on demand how many cycles did it survive given that failure rate.

So, in the end you're right that when you look at the table what it looks like is that there are three independent -- they're behaving like three independent valves, but the initial first step was to draw one failure probability before we got to the second step of a different number of demands based on that same failure rate.

CHAIRMAN STETKAR: I don't understand what you're doing. Maybe Dennis does. If I look at the summary tables I see three distinct probabilities for failure to close with three distinct open area fractions. And indeed the discussions of the scenarios examine that.

They say hey look, in this scenario valve number 1 failed after two failures and it failed 90 percent open. But valve number 2 was never challenged.

In this other scenario valve number 1

didn't fail until 82 cycles, or let's say 17 cycles, and it only failed 16 percent open. And look, valve number 2 cycled 15 times but that was longer than before we got the hot leg rupture.

That tells me that you're modeling them completely independent. That's not true. That is wrong. I don't care how you set up your sampling algorithms and what you have deluded yourself into mathematics, it is wrong.

If valve 1 fails after seven cycles valve 2 fails after seven cycles and valve 3 fails.

MS. GHOSH: That's a deterministic view of the world.

CHAIRMAN STETKAR: No, it isn't.

MS. GHOSH: The valve has a 50 percent chance of failing on demand it's a constant failure rate across the three valves, but it's still a stochastic phenomenon whether it fails or not when you command the valve to open and close.

CHAIRMAN STETKAR: If you look at the references for treatment of correlated state of knowledge for uncertainty treatment you will find that you are wrong.

It is why the state of the practice in risk assessment, the state of the art if you want

to call it that, samples from any set of valves or pumps or whatever if I use the same -- if I say there's no reason to believe that they're different and I have the same state of knowledge about their behavior when I sample from that state of knowledge they will all behave the same way. They will all behave the same way.

MS. GHOSH: If we flip three quarters are they all going to land on heads exactly the same number of times? Maybe we did it differently than how it's done in PRAs or other cases, but we tried to be more explicit about separating the epistemic and the aleatory parts of the valve modeling.

MEMBER CORRADINI: For the uninitiated since I'll let you guys argue this, can you say again what you did so at least I get it?

MS. GHOSH: So let's pretend each of the valves is a coin. They have a 50 percent chance of failing on each demand.

So, each valve has a constant probability of failing on demand. And then you don't know how many demands there's going to be on it. But every time you demand that valve to open, or I guess close in this case, there's a 50 percent

chance it's going to close.

So there's two ways to input it into the MELCOR deck. You can either specify that it's going to be what we had done in previous analyses is to specify that you're just going to take the mean number of failures given that failure rate, and that it's deterministically going to fail at that number.

So if it's a coin that's 50/50. That's not a great example, but let's say it's going to fail on the first demand. That's the mean number that -- times that you can cycle it before it fails.

But this time we tried to add that aleatory aspect, the random nature of you don't actually know given that failure rate which demand number that valve will actually fail at.

So we tried to impose that second layer of uncertainty modeling on the valve behavior. So each Monte Carlo draw, the three valves had the same failure rate specified.

MEMBER CORRADINI: But then once you've determined that then you have variability on how many times -- when it does fail, is it one, two, three, whatever.

MS. GHOSH: Right, right. So that you don't deterministically decide that all three valves are going to -- given that same failure rate that all three would fail --

MEMBER BLEY: You and John are saying kind of the same thing. I have to go look, but the tables he's saying don't support that. And I have to look at it. I haven't looked at that yet.

MS. GHOSH: Yes, and I'll revisit how we documented it. It does get confusing because we had to do all of this part of the modeling outside of the MELCOR code and then create an input deck to feed into the code.

MEMBER CORRADINI: But I guess at the very least, I guess I see two things that have to be clarified.

One, if the table doesn't express what you just said it's got to be clarified.

And then two, I'll leave you practitioners of failure rate modeling to discuss the next step.

But it seems to me there's two points. One, probably what you wrote doesn't come across as to what you did which is at least the first step.

CHAIRMAN STETKAR: Well, we asked about

this on Surry and they said oh yes, we did it wrong on Surry and there's a workaround that we used, and the workaround didn't work.

And now I'm seeing the same effect on Sequoyah. But I don't know --

MS. GHOSH: No, we did it right this time. So in Surry we didn't accomplish what we intended. This time around we accomplished what we intended.

So the intention was same for both studies. In Surry we found an issue that we had to go back and devise a workaround. But that's why that discussion is not there.

But the Sequoyah analysis was done by the time we had come to you with Surry. The approach is the same. We didn't modify it after the feedback that we got in February. It's exactly the same that we used. It's just that this time we implemented it as we intended from the beginning rather than having to go back and do the workaround later.

The only thing -- and again, I don't want to dwell on it, but the open area fraction we did assume was different because that we have very little information on. And we didn't see that that

was an epistemic uncertainty in terms of how big that flow area would be when the valve failed. So that is modeled independently.

MEMBER CORRADINI: Did you hear that last thing, John?

CHAIRMAN STETKAR: No, I didn't.

MS. GHOSH: I was just pointing out that the open area fraction is modeled independent because that is thought to be a random thing in terms of what position or what flow area the valve fails with.

MEMBER BLEY: And you assume that that's uniform.

MS. GHOSH: Yes, because we have -- we'll get into that.

(Simultaneous speaking)

CHAIRMAN STETKAR: But I wanted to get this while we had more people here about how the uncertainty was treated. Because that's more of a fundamental issue.

Now, if I look at Table 4-14, for example, I am led to believe that in a specific realization each valve has a different number of cycles to failure.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: How was that number of cycles to failure produced?

MS. GHOSH: First a failure rate was sampled from the failure rate distribution. And then given that failure rate outside of MELCOR which is why it has to get summarized as an input to MELCOR there's basically some kind of simulation that says given this failure rate how many, you know, the number captures that would be on aleatory part in that given the failure rate how many cycles did the first one get to before it failed. How many cycles did the second one get to, and so on.

CHAIRMAN STETKAR: But that's not described anywhere in the report. I don't know what you used for that as your characterizing it, an aleatory uncertainty about.

MS. GHOSH: So that was identical to Surry.

CHAIRMAN STETKAR: And I didn't find it there.

MS. GHOSH: Okay. Let me go back and make sure that -- I think in some cases we didn't do as good a job in the stuff that we had covered in Surry. We refer back to that report and we didn't repeat that discussion.

CHAIRMAN STETKAR: But it wasn't clear. We asked the same thing on Surry and you said well, yes, we had to do some sort of workaround. And you had some assumption that because you didn't believe the uncertainty results you were assuming that one of them never failed to open on Surry.

MS. GHOSH: Yes, that was with the open area fraction sampling.

MEMBER BLEY: So to me the results aren't particularly suspect. It depends on what the failure rate was and that they fail at different times seems reasonable. I can calculate that sort of thing, but I'd like to see how you calculate it. And we don't see that.

MS. GHOSH: Yes. So we should beef up the documentation on that. We can do that, yes.

CHAIRMAN STETKAR: I just wanted to make sure that I had a chance for Joy. We'll talk more about the parameter models later.

MR. ROSS: With respect to containment modeling the compartmentalized nature of an ice condenser containment is reflected in Sequoyah model nodalization.

The ice chest was represented with many control volumes as opposed to a few to allow steam

emission to be more localized.

The incomplete sealing by the ice condenser entrance doors are assumed to be in normal condition.

Failure of the ice condenser entrance doors to reclose if they're open fully, if they're forcefully opened fully then we assume that they're not able to fully reclose.

MEMBER CORRADINI: Say that again, please?

MR. ROSS: If an ice condenser door is forcefully opened to its full open position we assume that that affects the door such that it can't resume a normally closed position.

CHAIRMAN STETKAR: But that's not true, or at least in the documentation. In the documentation it says for the lower doors there is an uncertainty distribution that varies uniformly between zero and 1 for the likelihood that it sticks open.

There's words in one place that leads me to believe what you just said, that once the lower door is forced open fully it stays open. But when I look at the discussion of the uncertainty for those doors there is a discussion that says

there's a uniform uncertainty distribution anywhere from zero stuck open which means it's a reversible door to 1 that it's stuck open meaning it stays open. So when I sample from that it could be anything.

MR. ROSS: So what's actually in the model is if the door is forcefully opened fully it is allowed to reclose only to that fractional position that is sampled as an uncertain parameter.

CHAIRMAN STETKAR: So, to make sure I understand that, whether I characterize it as reclosing to a position that's fully closed, or reclosing to a position that's fully open it's anywhere between fully closed and fully open after it's forcefully opened.

MR. ROSS: That's right.

CHAIRMAN STETKAR: Okay. What's the basis for that? Specifically I looked up the reference and it says in the reference that TVA -- I won't quote because we have limited time here, but if you're interested I can quote from the appendix.

It says TVA claimed that they designed the doors such that when they're opened under design basis loads they stay open.

Now, if that's true I don't think it's equally possible that they could not stay open, or stay half open.

MEMBER BLEY: And that's a design basis LOCA.

CHAIRMAN STETKAR: And that's a design basis LOCA.

MEMBER BLEY: Slam them open.

CHAIRMAN STETKAR: Which would -- but that's what he said, when they're forcefully opened.

So if the people who own the doors and design the doors say that they're going to stay open by design under a condition that opens them fully why in this uncertainty analysis are we saying that there's a very, very high probability that they don't? Almost guaranteed that they don't.

It's equally likely that they work perfectly.

MR. ROSS: Is there such a latch that you're aware of?

CHAIRMAN STETKAR: It's not a latch. It says deformation. They're designed to deform. Now, I don't know how these things are designed

because I've never worked on them.

MR. ESMAILI: This is Hossein Esmaili. We look at how these doors behave and we went back to when they were looking at direct containment heating issue.

And at that time they were doing calculations with CONTAIN and also later on with MELCOR. This was the work done by Sandia at the time so they contacted the TVA.

And if you look at that NUREG that I cited, I think it's 5586, I'm not sure.

So there are uncertainties on how these doors behave. I think it was said correctly that these doors open fully under 1 psf. But after that, after that they are supposed to remain open.

But there's a probability that they would also close. And so we took that as an uncertainty that a fraction of these doors would not come back, would not necessarily get crushed open.

In the analysis that we have done, and I think Kyle is going to show that later on, the issue is that these doors do not fully open because we are not looking at the DBA.

It only fully opens and it gets crushed

open when you have a hydrogen burn. So, they look at this as a sensitivity calculation.

(Simultaneous speaking)

MEMBER CORRADINI: So I understand what you're saying. Do we only consider this possibility of stopping somewhere in between fully open and fully closed if we've had a hydrogen burn?

MR. ESMAILI: No. We consider only that when it reaches that 1 psf which is about 46. So once the door fully opens there's a possibility that not all the doors fully open. You know, there are 24 around the circumference of the containment.

If it fully opens, if they fully open we say that, for example, 50 percent of these doors remain open and 50 -- but at that point a large fraction of the doors have remained open. And there is also leakage to the lower plenum doors.

In our uncertainty analysis instead of doing sensitivity analysis that they have done before we assigned a probability that we don't know how these doors really operate. And so some of them can remain open and some of them can be reversible.

For those who remain open then you don't have the pressure for the others to fully

open.

CHAIRMAN STETKAR: Let me read into the record because I did look up the NUREG. And in Appendix A, this is a quote from NUREG/CR-5586. Reference DBA 88 states that once the doors are open during a DBA the door hinge assemblies are designed to deform preventing reclosure.

The pressure required to do this is not given. Door momentum in addition to static pressure is presumably involved in producing this deformation.

It is also indicated that if the doors open under conditions insufficiently severe to deform the hinges they can reclose, at least to the neutral point.

It is not entirely clear whether they're expected to reclose fully in the event that a reverse pressure is reestablished, for example, if the ice condenser cold head is reestablished.

In the present study NUREG/CR-5586, the people who really looked at this, it was assumed that the lower plenum doors normally would reclose fully given the necessary reverse pressure.

The exception was that if the doors were once fully open it was assumed that they would

remain fully open in order to simulate the deformable hinge behavior noted above.

Now, in that study they did three sensitivity cases. They said our best estimate based on the way they're designed is that they're going to stick fully open.

But suppose they were reversible, or suppose 50 percent of them stuck open. They did sensitivity studies. In this case people took those three sensitivity studies and drew a line through them and said well, that's our uncertainty in the way the doors behave.

That's not an uncertainty analysis understanding the way the equipment apparently is designed to work.

There's not a 99.99 percent chance that less than all of them stick open. If they're designed to stay open, and taking arbitrary things from a sensitivity study and characterizing them as your uncertainty analysis about the behavior of a piece of equipment is not a state of the art uncertainty analysis. It's simply playing games with numbers.

MEMBER CORRADINI: Can I clarify one thing, John, that you read?

CHAIRMAN STETKAR: Sure.

MEMBER CORRADINI: The way I read that is under DBA loads. But none of this except for hydrogen burn would create a DBA load.

CHAIRMAN STETKAR: My point is they do have DPs that would open them fully. I don't know how you get them, but in their model they do have DPs. And for those DPs they do have this uncertainty distribution about whether it's characterized as, you know, the fractional area. Whether you characterize it as a percentage of the doors that stick open or whatever.

I don't know what gets those DPs. But all I'm saying is that if you're going to model something and you're going to characterize your uncertainty in the way that that thing behaves characterize your uncertainty in the way that thing behaves based on an understanding of the actual piece of equipment, not an arbitrary sensitivity study that somebody else did, and certainly not something that seems to be contrary to the way the thing is apparently designed to work.

MR. ROSS: I don't believe we were aware. I wasn't aware of the door design feature of staying open.

CHAIRMAN STETKAR: As I said, I have no idea how they designed these doors. I'm just quoting from -- but this study refers back to it.

In one place in the study you just brought it up. You say that they stick open. And then as I was going through the later parts of the study where I found the uncertainty distribution that they pointed to I said, gee, where did they come up with that.

MR. ESMAILI: I think they also acknowledge there's large uncertainties on how these doors behave.

I mean, at that time they say that this is how -- you know, they went back to TVA, the same that you were talking about, and that's why they did this NUREG.

So, we didn't know exactly. But you know, they were looking at the DBA condition. And as I said, you know, we are looking at cases where we have the pressurizer relief tank opening where it does not lead to the fully open position.

And so based on what we saw in NUREG-86 the fact that they are just -- even when they discuss this with TVA that the operation of these doors as designed is supposed to do that, but

they're still not quite sure how they operate. That's why we assigned that probability distribution that we did.

Besides, you know, these doors are opening on very, very low pressure. So, it's true that under hydrogen combustion and when the core goes expressed, some of them get stuck open.

But as long as the pressure is enough they're going to push these doors open. So it doesn't -- so whether 20 percent or 50 percent of them get crushed open, the fact is that given enough pressure the rest of the doors can open also.

CHAIRMAN STETKAR: Hossein, suppose all of them stick open. Suppose you use the model that they used in the study that says if you get up to the design DP they all stick open. What difference would it make in your study?

MR. ESMAILI: I think our studies are showing that it's not very sensitive to this --

CHAIRMAN STETKAR: That's -- it isn't given the uncertainty distribution you have where there's an exceedingly high probability that that doesn't happen.

I'm asking you the other question is

suppose they all did stick open the first time you get up to whatever that pressure that opens them fully. How would the results of the study change?

MR. ROSS: We did have such cases and they didn't stand out.

MEMBER CORRADINI: So can I say it differently just so I get it?

I'm still back to the performance of it. So when the doors open that allows you to see a train of ice to do a cooldown to essentially open up the heat exchanger flow.

But the fact that I have variable areas, don't the basses communicate circumferentially?

MR. ROSS: They do.

MEMBER CORRADINI: So, this is like an ice PWR. If I open one assembly I'm going to go to the other assemblies given I've got enough of a delta p. Am I understanding the behavior?

MR. ROSS: Yes, I believe you're understanding it well.

MEMBER CORRADINI: Okay, fine. Then I think we're arguing about nothing.

CHAIRMAN STETKAR: We may be, but my whole point is that if you're going to do an

uncertainty analysis they selected this as one of the important parameters to develop uncertainty about. And if I have no confidence in the uncertainty for that parameter it's not clear what we're doing.

If it's not important then I would say why didn't they just stick them fully open as the people who did that original study did.

MR. ESMAILI: And so we will have to do sensitivity. The question will have come up and saying, you know, you have to do sensitivity calculation to show what if they don't open. Because in that same NUREG they're saying that it is possible that these doors could be reversible.

So, we would have been not doing uncertainty and relying on some sensitivity analysis.

We tried to do that knowing that there is -- we don't quite understand how these hinges get crushed, et cetera.

CHAIRMAN STETKAR: Let's go on.

MEMBER REMPE: I have a bunch of questions about the containment.

CHAIRMAN STETKAR: Yes, then do that, please.

MEMBER REMPE: Okay. Nodalization. You beefed up the nodalization because of the ice condensers. How do you know it's adequate? Did you do sensitivities to say okay, if I added more nodes that I get the same results?

MR. ROSS: We didn't accomplish sensitivities with the nature. It was more of judgment, thinking about the ingress of the steam into the ice condensers and wanting to have a localized volume as opposed to representing the whole ice condensers with just one volume was a consideration.

But we didn't try going to, say, to say double and see if there would have been a difference.

MEMBER REMPE: I'm just kind of wondering because sometimes we're finding that nodalization may not be adequate in other studies that are going on.

MR. ESMAILI: Sorry, this is Hossein Esmaili. We were informed. The same NUREG that you were talking about, NUREG-5586, they had done sensitivity to nodalization of the ice condenser.

So they started out with the four nodes out, and then went to six nodes out. And so they

looked at what type of distribution they get. So they finally went and this is written that they went to a 26 node ice condenser model.

And this was the original node that was used in calculations in support of GSI-189. And our nodalization is we had changed a little bit, but it is very -- it's similar to the analysis that was done.

So we knew we were not going to do it with four nodes or six nodes, we are going to do enough nodes to capture all the things that they had seen before.

MEMBER REMPE: That's good. So, I would be happier if you could beef up the NUREG or your SOARCA report and acknowledge that because that's a question that I always have when I read something like this.

Also, talk about your assumed or your selected containment failure pressure. It doesn't assume any temperature-induced degradation.

There was a statement in the report that said well, we know it's not important if it's below 300 F. So the question I had was well, I didn't see any temperatures in the containment predicted. Did the containment temperature stay

below 300 F?

MR. ROSS: I don't think I have a plot to fall back on.

MEMBER REMPE: I'll trust you. Do you know kind of what the values are?

MR. ROSS: There is elevated temperatures in containment. The upper containment is where the breach location is. But I have to say I don't think we've considered the temperature of the steel although that work was done.

MR. ESMAILI: So, this is Hossein Esmaili again.

So, we looked at that under 5.26 that you were just briefed I think about a month ago. So, we did provide both the temperatures inside the containment and also the structure temperatures.

MEMBER REMPE: For a different plant, right?

MR. ESMAILI: No, it was Sequoyah.

MEMBER REMPE: For Sequoyah?

MR. ESMAILI: You have the figure. You don't have it in the NUREG, but we were looking at the temperatures that we were observing both for the Mark III and ice condenser. And we plotted out all those structure temperatures.

It is possible that because of hydrogen combustion you can get some momentary spikes in the temperature.

But when we looked at the structure temperatures they were more or less remained around 300 or sometimes even lower than that. And these are in the --

MEMBER REMPE: So tell me again. You're telling me the structure temperatures remained around 300 F?

MR. ESMAILI: Yes.

MEMBER REMPE: Okay, because again in this document it says that temperature won't affect the structure integrity if it stays below 300 F. So, you're saying that you think it's around that temperature a lot of times?

MR. ESMAILI: I can find you the SECY for 5.2 and 6 and show you the temperatures. But this was for a long-term station blackout. But yes.

MEMBER REMPE: So what I'd like to see in the report is that --

MR. ESMAILI: -- entire containment shell. And I did not see temperatures exceeding 300 F.

MEMBER REMPE: Okay, so the document should support somehow or other that you think the temperatures are appropriate for neglecting any temperature-indued --

MR. ESMAILI: We can add that information.

MEMBER REMPE: Okay. And then you have here that you assumed when the containment failed it had a failure area of 3 square feet. What was the basis for that size?

MR. ROSS: So that work was done NRC in house. Is there someone here who's familiar with the history behind that? Jose?

MR. PIRES: Good morning. My name is Jose Pires.

The failure area for steel containments is thought to be large. And the at-fault area of mechanism are of a steel containment that was obtained in experiments, in some cases was actually a very abrupt failure with a very large -- actually sometimes the containment even broke apart into pieces.

But those were small containments. The pressurization of the containment was probably rapid because the volume was small.

But the thought is that the failure area of containment will start as a crack that will seep open to a large extent. So an approximate -- a large area would be reasonable to use. This spreadsheet might end up be smaller than the actual area.

MR. SHACK: That's my question is did you do -- because 6920 has it as a catastrophic rupture. I mean, it pops through under their calculations. It runs unstable. So it's going to be a big hole.

And the question, you know, 3 square feet is a big hole, but would a 6 square feet hole make any difference? Probably not.

MR. ESMAILI: I don't think so, but can't say.

MR. ESMAILI: I think Kyle will show some of the results. The point is that we are expecting a very rapid depressurization. So, 3 feet square did the job, you know. Yes, you're right, maybe do 6 feet square. But we don't expect the rate of pressurization to be different as you increase the --

MR. SHACK: Big is big, yes.

MR. ESMAILI: Big is big.

MEMBER BALLINGER: But in the stuff that I was reading the failure pressures were identified in a range of failure pressures.

And then there was something about 2 to 3 percent strain at some pressure. But when you slowly pressurize one of these things I couldn't find what the failure strain would be.

It's probably in another report somewhere but it's not in here.

MR. SHACK: 6906 and 6920.

MEMBER BALLINGER: Okay. And then what happens if you get hydrogen, a pressure spike where now you have a very dynamic pressure spike where now you might not have the kind of -- the failure in a very particular spike is more dictated by the toughness of the material than it is by the yield strength, or the strength of the material.

So, I guess I've got to go back and look at those other documents. But it seemed to me like 3 percent is a pretty small number.

MR. SHACK: Well, that's the global strain. The argument always is that in their particular case they have thin plates and thick plates together. You localize strains.

MEMBER BALLINGER: Well, that's a

circumferential transition, right?

MR. SHACK: Right.

MEMBER BALLINGER: Okay. So anyway, I've got to look more carefully. But 3 feet seemed like a small number.

MEMBER REMPE: My comment about this, provide more documentation that this is a good assumption, or it's an assumption that doesn't matter but it just is out there. We assume this and it leaves me wondering.

Could you kind of help me understand a little bit how -- because I would think when you have ice condensers you're going to have a lot more humidity in the containment and then I would obviously think that I would be less -- I would think steam inertia would occur a lot more.

And that is carefully included in the MELCOR model. Can you just qualitatively kind of let me understand that?

MR. ROSS: Yes, and then I think we'll come to some slides that are particular to that too real soon.

MEMBER REMPE: Good. Okay.

MR. ROSS: But yes, we constantly looked at having too much steam to support a burn.

Yes.

MEMBER REMPE: Okay. And I know you did because I looked at the results, but I just was curious because the ice condensers. And it's because I've not looked at the MELCOR modeling to fully understand it, but it carefully, when the humidity from the ice is carefully included in the containment atmosphere is what you're telling me today.

MR. ROSS: Humidity. I don't know that we initialized the containment atmosphere differently because the ice was there.

But as the ice becomes involved in the accident and melts and water relocates and revaporizes all that is accounted for.

MEMBER REMPE: Carefully included. Okay, that helps. I just, again, I hadn't gone back to the MELCOR manuals to try and follow through.

You talk about this random ignition of and I assume frequency of a half hour. Was there any technical basis for assuming this half hour? I have a little pop recurring.

MR. ROSS: No, I don't believe we could say that there is. We just wanted to investigate

if there were sparks that were happening from whatever sources intermittently in time and space would it affect the MELCOR calculation set is all.

MEMBER REMPE: Okay.

MEMBER BALLINGER: So that calculation was pretty much, without knowing at all where hydrogen might pool or not pool you just said, you just have random sparks from some energy input from some area.

MEMBER CORRADINI: We're ahead two slides, but if we're going to ask about this, this one escapes me. I apologize, but that's the only --

MR. SHACK: Let me just ask a quick detail question. In one table you have a range of rupture pressures of 54 to 82 and you reference 6920.

Then say you're going to use those results. But the results you actually use are 55/74. And why did you change the numbers? Since you reference the result.

MR. ROSS: Yes, I appreciate it because they were up for commenting.

MR. PIRES: We had some other calculations done that shows somewhat lower

pressures outside the range of the 6920. That's where the 5 percentile and the 95 percentile of the distribution from there.

And this also shows somewhat higher areas. So we thought that because we are going to choose a triangular distribution so that -- sadly means not like a normal distribution. That there's a tail dictates to expand the range.

MR. SHACK: Okay.

CHAIRMAN STETKAR: Don't anybody say anything. Joy, did you get all of your items covered, or do you have other ones?

MEMBER REMPE: There's a couple more, but let's talk about random ignitions a little bit.

(Simultaneous speaking)

CHAIRMAN STETKAR: There are folks around here who would like to take a break and I want to make sure you get all of your stuff done before you have to leave. So that's doable.

MEMBER REMPE: Some of them are not related to containment so I was going to wait till he finished his modeling. But it's up to you if you want to take a break.

CHAIRMAN STETKAR: No, I --

MEMBER REMPE: -- now so it's fine.

CHAIRMAN STETKAR: Will you take the lead on the rest of them, sir?

MEMBER CORRADINI: If you want me to I'll do whatever you tell me.

CHAIRMAN STETKAR: Please do that since you converse in those terms. Let's regroup. Let's take a break. I'm going to be not so generous. Let's come back at 20 till 11, so 12 minutes from now on that clock. We're recessed.

(Whereupon, the above-entitled matter went off the record at 10:28 a.m. and resumed at 10:42 a.m.)

CHAIRMAN STETKAR: We're back in session and we're not having any conversations.

MEMBER BALLINGER: Harsh.

CHAIRMAN STETKAR: I don't know where we are when we left off, but Joy, you've got 18 minutes to get in whatever your parting shots are.

MEMBER REMPE: Well, there was one parting shot that's not really related. If you don't mind I would like to discuss it.

CHAIRMAN STETKAR: Is your mike on?

MEMBER REMPE: I would like to discuss in your model you have gone with this lower eutectic temperature. And I know that's been

touted for a while now, but the discussion in the text was the first time I'd seen some discussion of why that was selected.

And basically it was the VERCORS data that was inferred when you saw degradation of the experiment.

And because this is an important sensitivity and it used to be 2,800 K, using inferred data from one test, and maybe there's something from PHEBUS where you don't have direct instrumentation to me seems a big leap of faith, especially if it's important to the result.

Did anyone spend time to say well, why is this lower? What is different about VERCORS and PHEBUS if there are data from PHEBUS as opposed to a lot of other data that was obtained years ago that said that this eutectic diagram for UZr02 becomes liquid at 2,800. And I just would -- for my own education could you let me know why?

MR. ROSS: So there's clearly an uncertainty here in our opinion. The default best practice value has long been 2,800 K as indicative of a temperature that the zirc oxide cladding could no longer support the original structure of a fuel pan.

We made the assumption in looking at the VERCORS data that fuel pans collapsed in the experimental work as a result of eutectic formation, and that eutectic being susceptible to structural failure at a lower temperature than 2,800 K, at whatever temperature was registered at collapse.

So we have adopted that in the distribution that we applied here in the range of temperatures that were believable for fuel rod collapse. And it does not go up to 2,800.

MEMBER REMPE: I'm just curious. I mean, why not -- could someone tell me why they believe this inferred data is better than what was done in the past? You may be right, but I just would like to understand why.

Are you sure there wasn't something just fluky with the test? They had a bad batch of cladding or something?

MR. ROSS: I'm looking for Andy but he's not back there.

MEMBER CORRADINI: He's not going to save you?

MR. ROSS: It doesn't look like.

MEMBER CORRADINI: So, can I ask Joy's

question a little bit differently? Which is in the Surry uncertainty analysis you had -- now I can't remember. I didn't look it up. You had three parameters that you varied. This was not one of them?

MR. ROSS: This was one of them.

MEMBER CORRADINI: Okay. And so how big of an effect did this one have relative to the radial propagation. All three of them were not that important I seem to remember in terms of how they affected the results.

MEMBER REMPE: But didn't you also stop at -- you had used the same distribution in Surry. What I'm saying is maybe it should have been at 2,800 instead of centered around --

MEMBER CORRADINI: Well, but first I want to ask the question, let's just go back to Surry. I don't remember all three of the parameters for in-core modeling variations having a big effect on anything.

So, if I were to magically rerun this calculation and just move the S curve to the right I wouldn't expect to see a big difference. Because you're still accumulating material and it's still going down and hanging up because of just the

physics of it being an open core lattice. It just kind of holds up and then the curve goes kerplunk at some weight.

MR. ROSS: Right. But the primary effect of that is if the fuel pan stands longer the hydrogen production is extended. So you have more in-core hydrogen production than with a higher temperature for failure than a lower temperature.

MEMBER REMPE: And page 455 of their study indicates that this value is important.

MR. ROSS: We wanted to carry something into this core analysis that could affect core damage progression, but we didn't want to redo all that we had done in Surry.

MEMBER CORRADINI: So why -- that's why I asked the earlier question is the model of Surry for the vessel and primary system the same model as you use here.

And that should be unlinked to the containment, and the ice baskets, and all that stuff.

MR. ROSS: Right.

MEMBER CORRADINI: Right? So then that's why I don't remember in the Surry uncertainty that hydrogen generation was seriously

affected by this. It was affected by just essentially the overall assumption of how MELCOR treats the ability to gases to flow through the core. Regardless of whatever the disassembly.

MR. ROSS: I don't recall the Surry dependence that we found on eutectic temperature.

MEMBER CORRADINI: Whereas here you did. That's what's --

MS. GHOSH: I think we found some dependence for the total hydrogen production in Surry as well, but because hydrogen wasn't really an issue for that containment we didn't focus on it that much in our analysis of the results.

But yes. So, for hydrogen production it does show up, but not for the other metrics that we looked at.

MR. GAUNTT: I'm sorry. This is Randy Gauntt. I just walked in and I understood there were some questions about our eutectic modeling update. Did that get answered?

MR. ROSS: I didn't do a very good job. Could you ask your question again, Joy?

MEMBER REMPE: Sure. Okay, so the report, in fact Surry also assumed this lower eutectic temperature where the material becomes

molten.

And this report for some reason got my eye more because they talked about that the basis for that data was inferred data from VERCORS.

And it mentions something about PHEBUS, but when you're changing something that becomes fairly important that way from the old data from before. What's the difference that you decided to jump ship and go with the new inferred data versus maybe a lot of other tests that were done previously?

MR. GAUNTT: Right, right. So, it's been a long journey. If you reach back to source term code package days we melted fuel at 3,200 Kelvin.

And then as we understood a little bit more about this we started to try and account for the binary interaction between zirconium dioxide and uranium dioxide because you've got them in fuel debris.

And if you look at the phase diagrams for the U02ZR02 there's a large liquidous range that's around 2,800. And that's where we were in the past, at 2,800.

But then if you look at the ternary

phase diagram of U, ZR and O there are other liquidous ranges. One of the quasi binaries is alpha ZROUO2. And there you see some quite lower liquidous ranges. Okay. Clue one from the phase diagram data.

Clue two comes from modeling the response of the PHEBUS experiments. And it just looks like we get better predictions on fuel slumping if we drop that temperature to account for the fact that it's not a pure binary. There's metallic zirconium that's commingling and dropping those temperatures some more.

And then we looked to the VERCORS test where there were actually fission product release tests, but in many cases they had imaging, you know, X-ray imaging or something that would show as they ramped this fuel up in temperature they would show these fuel pellets kind of slumping at lower temperatures.

So we just became kind of convinced collectively looking at all the information that it was a better representation of -- or abstraction if you will of the more complex interaction between zirc metal, zirc oxide and uranium, and perhaps irradiated fuel effects as well to drop that

temperature more down and around the 2,500 K range.

And this is kind of consistent with the ASTEC modelers and other people modeling severe accident progression. So it's kind of a complex story, but that's how we got to this point.

MEMBER REMPE: I appreciate it. Did anybody think well, maybe there might be something different about these tests? Did they take some time to say these tests are truly representative?

MR. GAUNTT: You mean the VERCORS?

MEMBER REMPE: Yes, as well as PHEBUS. I mean, you're saying both of those tests.

MR. GAUNTT: It's what we've got to go on, you know.

MEMBER REMPE: That's it. Okay. Because there were a lot of other tests done previously.

MR. LEE: This is Richard Lee from Research.

All those tests, all those pilots come from the commercial reactors. PHEBUS.

MEMBER REMPE: Okay. Thank you.

CHAIRMAN STETKAR: Let's go back to where you were. I just wanted to make sure because Joy has to leave that she had a chance that she had

a chance to fully vet her concerns.

MR. ROSS: So, just some more details about containment modeling. Leakage past the freestanding steel containment vessel to the annular region between the vessel and the concrete shield building is included in the model. But this is a slight leakage.

Overpressure rupture of the steel vessel is accomplished with this 3 square foot hole as we mentioned before.

The shield building failure was imposed coincident with steel containment vessel rupture.

There were two paths understood to the environment. One is a ventilation opening high on the shield building wall. The other is a door leading from the auxiliary building.

CHAIRMAN STETKAR: I'm assuming that because of that it doesn't make any difference where you located the containment failure, is that correct? Because the analysis says it's most likely to come apart around the equipment hatch which is down pretty low.

MR. ROSS: Right.

CHAIRMAN STETKAR: And you assumed it was high up.

MR. ROSS: We just have one volume representing the annular space.

CHAIRMAN STETKAR: Yes, but so you just pressurize that annulus and the location of the release was determined by these two things.

MR. ROSS: Right. By these two penetrations.

MEMBER CORRADINI: Can you go back. Where are the penetrations just so I understand?

MR. ROSS: So there's a ventilation unit high on the shield wall.

MEMBER CORRADINI: So it's up by CV23 or CV22? Or lower than that? That's what I didn't understand.

CHAIRMAN STETKAR: Those two arrows out to the right is where they're modeled.

MR. ROSS: So there's -- yes.

MEMBER CORRADINI: Okay.

MR. ROSS: Right, that lower one would be the auxiliary building doorway.

MEMBER CORRADINI: Okay, thank you.

MEMBER REMPE: In your last point you talked about the vessel failure. And I note on page 3-7 you said that you had a vessel breach of variable size opens. What did you assume?

MR. ROSS: The containment vessel breach.

MEMBER REMPE: No, this is actually the reactor vessel, a variable size opens, I believe. Maybe I misread the document there, but I thought at the time you were talking about the reactor pressure vessel.

MR. ROSS: Let's see. The vessel breach would be either a hot leg rupture or a lower head --

MEMBER REMPE: What did you assume for the vessel failure size when you assumed the vessel failed?

MR. ROSS: It's double-ended break of a hot leg.

MEMBER REMPE: So you never had lower head vessel failure.

MR. ROSS: Yes. Yes. So the lower head always fails typically after there's a hot leg rupture, but not always. There are a significant number of MELCOR runs where there was no hot leg failure and vessel breach came at lower head melt-through.

MEMBER REMPE: What size did you assume?

MR. ROSS: It's a gross failure. I don't recall more than that except that it is a gross failure.

MEMBER REMPE: Okay, thanks.

MR. ROSS: The feature that we realized back in 2007, one earlier development worked on on the Sequoyah model under SOARCA was that the division between upper and lower containment in overall ice condenser containment structure is completed by a piecewise rubberized barrier that's designed to limit steam that can bypass the ice chests in a design basis LOCA.

In total this seal is 464 feet long and it covers an area of 135 square feet.

There are 12 vertical and 11 horizontal sections of this seal and they are on average 3 and a half inches wide.

The seal is intended to maintain its integrity during a design basis LOCA for a minimum of 12 hours exposing no more than half a square foot of leakage area during that time.

If a section of this seal were to fail, a single section of the seal were to fail the deck bypass area could increase from, for example, 5 square feet to 15 square feet.

The seal and its failure represented in the MELCOR model in conjunction with the nominal deck leakage.

MEMBER CORRADINI: So this is like -- I want to call it for want of a better word like a rubber bladder that tries to minimize bypass. So you took this as a variable in terms of potential leakage bypass.

MR. ROSS: Right. It's a rubberized fabric.

MEMBER CORRADINI: I just wanted to make sure I understood.

CHAIRMAN STETKAR: We'll talk this afternoon about the parametric distribution used for the failure rate for that?

MR. ROSS: Yes.

CHAIRMAN STETKAR: Okay.

MR. ROSS: Ignition sources. We ran two 600 calculation sets of MELCOR calculations.

In the first set there were two understood sources of ignition. One of those was a hot plume issuing from an RCS breach at a hot leg nozzle or in the pressurizer surge line.

The hot plume had to have a velocity that was greater than 0.1 meters per second, and it

needed a temperature that was greater than 847 K which was taken to be the autoignition temperature of hydrogen.

The second source of ignition was taken to be core debris on the containment floor.

In the second set of 600 calculations there was an additional source of ignition considered and we've been calling that random ignition.

So, the two above sources of ignition were still considered as viable ignition sources. But additionally we interjected a spark if you will of one second duration. We did it every half hour. And we put it somewhere in containment.

MEMBER CORRADINI: So, this is the one thing I don't get. So help me. Where did this come from?

MR. ROSS: So, this was to acknowledge that there could be sources of ignition, sparks of unknown origin.

One possibility it seems is if there were aftershocks following the big seismic initiator that piping would jostle, that debris would fall and that the generation of a spark was believable. And so we wanted to reflect that to

some degree in these calculations.

MEMBER CORRADINI: So, is there any experimental evidence of this in any controlled environment that you can point to? Because I understand what you're saying. I just don't understand -- well, I'm trying to get a mechanistic feeling for where has this been observed that justifies doing this.

MR. ROSS: Well, Fukushima I suppose. It's a reactor building as opposed to a containment. But that was a situation where --

MEMBER CORRADINI: They were trying to reinitiate power and doing all sorts of things, running about.

So, this one you're just kind of standing back and watching the thing occur. So I'm still struggling.

MR. ROSS: Yes. So, the -- well, they certainly were running about trying to reestablish anything they could. But it did seem like there was a full station blackout situation without electrical sources. Seems like that could have been the situation, but yet the containment did go. The reactor buildings did go.

MEMBER CORRADINI: So, to my original

question.

MR. ROSS: Yes.

MEMBER CORRADINI: There's nothing that's been in the hydrogen testing program back at Sandia in the seventies and eighties there's nothing that they tried to look at that. There's nothing that's been done in other research. When EPRI had their program they actually looked at random ignition sources and looked at this? I'm looking for an experimental basis.

MR. GAUNTT: Randy Gauntt. I'm just going to add maybe to -- from a Bayesian statistical standpoint we saw three reactor buildings blow up at Fukushima.

And I think we were trying to pay respect to the fact that, hey, you've got hydrogen. It finds a way to ignite.

Now, your point is we don't know what the ignition source was. But you could imagine people scrambling in the same way that they did at Fukushima trying to bring things about, or whether it's an aftershock that leads to a -- or just simply static, static buildup of some sort.

So, there's no more to it than that. We're not drawing upon any experimental study on

random ignition. Simply saying wow, we blew up three buildings at Fukushima. We can't ignore that in this case.

MR. LEE: Richard Lee from Research. I'm going to use the familiar things that Dana Powers asked us about aerosols.

When we do aerosol experiment we always make sure they are neutral. But the aerosol that released through the accident, the charge doesn't have to be damaged. So you will have an imbalance in charges and that could be a source of the hydrogen ignition.

That's true also could be for Fukushima when we see the reactor building blow up because we've been asking where is the ignition source.

MEMBER BLEY: So, remember back in the sixties the BWRs with the standby gas treatment systems had a whole series of pops that blew those things apart.

Most people think that was due to static buildup.

MR. LEE: But the aerosol is for when radioactive decays are not balanced.

But when we do experiment on aerosol we try to study aerosol phenomena, not the charges.

In the PHEBUS experiment we asked a friend can you look at these charges in balance. They said no because they have too many instruments, there are too many things to study.

MEMBER CORRADINI: I guess my feeling, maybe the discussion is -- unless I missed it there was not a lot of discussion as to how this came about.

So I thought maybe you were trying to model some sort of door closures where I smack something and I create some sort of impact. Then the impact essentially would be a trigger. But that's not the case.

MR. ESMAILI: Hossein Esmaili. I just want to clarify one thing that's not written here.

This random ignition, and correct me if I'm wrong, this happens at high hydrogen concentrations.

So, we always have -- so we do get this random ignition when the hydrogen concentration gets above 10 percent or so, correct?

MEMBER REMPE: My understanding was that you had a spark source --

MR. ESMAILI: It does.

MEMBER REMPE: -- and whether it

actually led to ignition or not depended on the concentration. But there was a 30-minute pulse every -- throughout this analysis.

MEMBER BLEY: A pulse every 30 minutes.

MEMBER REMPE: Yes, a pulse every 30 minutes throughout the analysis.

MEMBER BLEY: But the concentrations were three different ones.

MR. ESMAILI: No, no, not for the random ignition, correct?

MR. ROSS: We did require that hydrogen needed to be 10 percent in order to be lit by random sparks.

MR. ESMAILI: So this is the point. So, in the MELCOR all the studies that we have done we have always recognized, and this has going back to NUREG-1150 that they say it is some random, some unknown hydrogen ignition.

And we actually have models that says even without igniters, or even without some unknown, if the hydrogen concentration gets to about 10 percent you're assuming that the hydrogen combustion can occur. So it's always been there.

Here they're trying to treat this a little bit more formally, that we are not going to

have this hydrogen combustion everywhere as soon as the hydrogen concentration reaches 10 percent.

But we are looking at they are doing something, some things are happening, I don't know, maybe energizing some equipment. And there are things that are happening that can cause a random ignition.

But at the end of the day it is a random ignition. There's a history to it and this one we're just doing it a little bit more formally.

So it's not something that we are doing --

MEMBER REMPE: Some additional discussion in the text.

MEMBER BLEY: I'm a little confused though because when I read this section of the text it came up with a rationale for the distribution. I thought that was talking about random ignition.

It talked about lower flammability limits for hydrogen at 4, 6 and 9 percent in different directions and assumed one-third in each direction.

So, that's not -- this is something different?

MEMBER CORRADINI: Yes, it's different.

(Simultaneous speaking)

MEMBER CORRADINI: They're just modeling burning from volume to volume to volume. And it depends on where it propagates depending upon concentration.

CHAIRMAN STETKAR: But that says there's an equal probability that it goes up, sideways, or down.

MEMBER CORRADINI: If I satisfy the concentration.

CHAIRMAN STETKAR: Yes. To go down requires the highest concentration, 9 percent. With a random ignition you cannot get a burn unless the concentration is greater than 10 percent. Therefore the concentration is great enough for random ignition for it to go in any direction -- in all directions.

Does it go in any and all directions?

MEMBER BLEY: Or is it sort of deformed spherical which is what would make physical sense to me. If you meet the criteria in all directions you'd think it would go in all directions.

MR. ESMAILI: But this is the point. So, this is -- we have like, I don't know, 25, 27 control volumes.

What Kyle is doing is that he's allowing an ignition source in one of those control volumes.

So, as Dr. Corradini was saying, so once you get this at that point, you look at are the conditions such that I have sampled a half hour, do I have 10 percent hydrogen that controls.

If that happens then I'm going to initiate a combustion, blow a combustion in that volume regardless of my flammability.

But what happens after that is that all these control volumes are connected. So once it's trying to propagate from that volume to the other volume it is still subject to these flammability limits of 4 percent, 6 percent, or 9 percent depending on how these control volumes are connected together.

MEMBER BLEY: But when you propagate to the next control volume if its concentration is high enough to support all three directions, what's come out of that control volume only in one of those directions? Or does it come out in all of them? If it can support all directions.

MR. ROSS: So a burn propagates along from volume to volume along flow paths that are

defined by the user.

And the onus is on the user to define whether that flow path is horizontal or vertical. And so the code looks at that distinction in the input and then considers what hydrogen concentration is necessary to move in that direction and decides whether the propagation can happen or not.

MEMBER CORRADINI: But I think, Kyle, his question is a little bit different. I think what I was going to try to explain is ignore for the moment, leave out how I ignite.

Let's say I ignite, whether it be by random, an igniter, that happens to be where the hot leg is, that happens to be where the pressurizer is. Now it ignites.

Now your question is if I'm above 10 percent, I think your question was if I'm above 10 percent in that volume how does it propagate to surrounding volumes. That's your question.

MEMBER BLEY: My question is in the model and the real event. It seems to me in a real event if you can support it in all directions it's -- that's what happens.

MEMBER CORRADINI: Right, but -- I

don't want to speak for them, but these volumes are compartmental volumes. So it's not -- you can't think spherical. You've got to think connectivity between one compartment to the next compartment to the next compartment. That's how he answered the question.

But I think what he's asking you is if I -- let's just get away from Sequoyah. If I had it in one volume what is your logic about the uniform distribution? Would it actually then start burning in all three volumes if they satisfied the concentration criteria?

MEMBER BLEY: Or only one.

MEMBER CORRADINI: Or only one.

MEMBER BLEY: You flip a three-sided coin to decide which one.

MEMBER CORRADINI: Do you understand the question?

MR. ROSS: I apologize, I don't.

MR. ESMAILI: Okay, can I try to see if I can expand a little bit?

So, let's just give an example. So I have two control volumes sitting right next to each other.

The code that's as Kyle was saying

knows that this shows a propagation horizontally from this control volume to the next control volume. So in other words once I --

MEMBER CORRADINI: In other words, keep on going. You're doing great.

MR. ROSS: So you get that. So in other words, so I have 10 percent here. I have the hydrogen combustion initiated in this control volume. And the code will look at what's happening in the other control volume.

It is propagating this flame from this control volume into the next volume. Once it tries to propagate into this next control volume it still has to satisfy some certain condition. You know, like for example not being steam inerted.

And then it knows --

(Simultaneous speaking)

CHAIRMAN STETKAR: Just describe hydrogen. Don't complicate it more.

MR. ESMAILI: Okay. So then if it's going horizontally, and as Kyle said the code knows that this propagation is happening horizontally.

So in his control volume what I need is a hydrogen concentration greater than 4 percent.

If it was propagating to a control

volume up then I need -- in that control volume I need a hydrogen concentration of 9 percent.

So, it knows, so the code knows by defining how the control volumes are connected what type of propagation you get.

And we are only talking about the propagation from this control volume to the other control volume.

MEMBER CORRADINI: Okay, but that isn't what -- I think we all get that.

MEMBER BLEY: Suppose we have this control volume where you have fire now. And we have one above it. We have another control volume below it and one horizontally. It could go in all three directions.

MR. ESMAILI: That is correct.

MEMBER BLEY: But if it meets the criteria in all three directions does it go in all three directions, or do you say one-third of a chance it only goes up? Or flip another one and it only goes this way.

MR. ROSS: It goes in all directions.

MEMBER BLEY: It does go. Well, how do you use this 30 percent --

(Simultaneous speaking)

MEMBER CORRADINI: Given the figure that's what I think is bothering me. This thing.

MR. ROSS: So, this is what the volume that the burn is originating in. The spark. Maybe it's on the floor. Maybe the spark is at the ceiling, or maybe it's somewhere in between.

If it's at the ceiling for that spark to light a burn that can propagate.

MEMBER BLEY: So this was within a control volume is where you used it.

MR. ROSS: That's right.

MEMBER BLEY: Once the control volume actually lights up then it can go in any direction as long as it can be supported. Okay. That makes a lot more sense to me. I didn't get that reading.

MEMBER CORRADINI: The logic is reasonable.

MEMBER BLEY: I didn't get that reading the report, that you applied this within the control volume to see if it actually can --

MR. ROSS: That's the uncertainty that we addressed.

MEMBER CORRADINI: MELCOR knows in a volume where you are in the volume. However monstrously gross the volume is it knows whether

you're at the top, or the bottom, or the side.

MEMBER BLEY: Well, it's randomly picked, right?

MEMBER CORRADINI: This is.

CHAIRMAN STETKAR: But for random ignition, even with random ignition because we know that the concentration in that volume by definition must be greater than 10 percent. Therefore it's great enough to support all three directions.

There is still a parsing of the flame front within that volume.

MR. ROSS: So, in the long discussions we had about whether including this random ignition was viable or not there was a feeling expressed that maybe if there was a random source of ignition it would be very slight. It would be a very slight spark.

And so that it would require something like 10 percent to ignite. And so we made that a requisite. If we had a burn that was going to start with a random spark you had to have at least 10 percent fraction of hydrogen.

CHAIRMAN STETKAR: But I guess what I'm asking though is and what I thought I was hearing -- I don't know how hydrogen burns.

So, I know to get a random ignition I need to have greater than 10 percent. And from what I've read here it says if I have greater than 10 percent that's enough to support a three dimensional spherical burn.

MR. ROSS: Yes.

CHAIRMAN STETKAR: Is it true that whenever I have random ignition in any compartment I always get that type of burn such that it always --

MR. ROSS: No.

CHAIRMAN STETKAR: Okay. Why?

MR. ROSS: Because if you have a volume where the burn is initiated and you have surrounding volumes the -- maybe the volume above has 4 percent concentrated hydrogen. So yes, you could propagate it up.

But maybe the volume below also only has 4 and that would be insufficient.

(Simultaneous speaking)

CHAIRMAN STETKAR: I thought I heard you saying, well, in the control volume depending on the location of --

MR. ROSS: Yes, the spark.

CHAIRMAN STETKAR: -- the spark this

thing would somehow apply.

MR. ROSS: It would. But the exception is if we get a -- in the case of a random spark we're mandating that you have to have 10 percent in that special condition.

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: I guess what I have to make clear is this random ignition is in addition to that ignition source that we have as a result of hot leg failure.

CHAIRMAN STETKAR: We understand that. Do not complicate things where we understand.

I'm thinking about a random ignition inside a compartment.

MEMBER BLEY: I don't recall the justification for going to the 10 percent. Is it because you expect in a random location that the energy of the spark might be so low that it might not fire it off?

MR. ROSS: That feeling was expressed back and forth we had for weeks about whether to include this random.

MEMBER BLEY: But if it's 10 percent then this other stuff doesn't matter. Within that control volume it goes.

MR. ROSS: If it's 10 percent and you have --

CHAIRMAN STETKAR: And in the adjacent control volumes, again, adjacent, I have one above, one to the side and one below.

MR. ROSS: Right.

CHAIRMAN STETKAR: It will then ignite in those control volumes depending on if it's greater than 4 percent.

MR. ROSS: Yes, if it's greater than 4.

CHAIRMAN STETKAR: And less than 6 percent I will get an ignition in that control volume which can only propagate upwards. Right?

MR. ROSS: Well, say the volume below the volume where the burn initiates only has 4 percent hydrogen. Then the burn won't proceed that way.

CHAIRMAN STETKAR: Right. It will stop in that next volume.

MR. ROSS: It won't go into that volume below.

CHAIRMAN STETKAR: It won't go into the volume below?

MR. ROSS: Right.

MEMBER CORRADINI: Yes, he's saying it

correctly. I think the simple way to say it is that once you compute -- forget about this probability. I don't want to go back to that.

Once you compute that you now are burning the hydrogen in one volume it looks to every place it can flow and asks the question do I satisfy it in concentration, in steam mole fraction, yes or no. And those numbers depend upon their vertical or horizontal orientation.

MR. ROSS: Yes.

CHAIRMAN STETKAR: Right.

MEMBER CORRADINI: And once I've passed that filter it burns. But it's a volumetric burn.

MR. ROSS: Yes.

CHAIRMAN STETKAR: Okay.

MEMBER CORRADINI: You just start chemically combining.

So, I do think though, I'm back with the random ignition. You don't have to go back to it. Oh, I'm sorry. You're somewhere. I don't know where.

I think you've just got to explain your logic and why you chose this. Because to me it was so sparse I didn't understand where it was coming from. I thought it was for equipment. So you're

saying it's covering a whole raft of things, even what Dr. Lee was saying relative to potential static charges from aerosol transport.

That's got to be there. It just was missing. I didn't understand where this was coming from.

MR. ROSS: Very good, thank you.

MEMBER CORRADINI: So can I ask about where this burn is happening now? Now that I understand the modeling of that.

Given that it's either random burns, you're picking these randomly in any of your 26 volumes. They could be up high. They could be down low in the lower compartment or the other compartment. Is that correct?

MR. ROSS: That is, yes.

MEMBER CORRADINI: And you randomly pick one every half an hour somewhere, or you randomly pick one all 26 locations every 30 minutes?

MR. ROSS: We pick from the 26 possibilities each half hour. Just one.

MEMBER CORRADINI: And then you look and see if it's above 10 percent.

MR. ROSS: Yes.

MEMBER CORRADINI: So you do a sampling. And then once you sample you say is that one above 10 percent. But you don't sample anybody else. Okay.

MR. ROSS: Right.

MEMBER CORRADINI: And then I want to call it the raceway, but the circumferential connection above the ice baskets which long ago used to be the focus of all hydrogen worries, how is that modeled? One volume, or a series of volumes? Right above the ice baskets.

MR. ROSS: Let's see. There's more than -- I'm not sure.

MEMBER CORRADINI: Because historically that's where all the concern was with ice condensers about transition from deflagration to detonation because you had essentially a raceway that essentially you could build up a relatively large pressure. Is that treated as one volume or a group of volumes?

It's the -- I'm not sure how you model it, but you call it the upper plenum, CV23, CV22. Is that -- are those two volumes each 180 degrees? Is it burned volumetrically?

MR. ESMAILI: Upper plenum of the ice

chest is two volumes.

MEMBER CORRADINI: Okay. Thank you.

MR. ESMAILI: And this was also informed by the previous --

MEMBER CORRADINI: Yes, I understand. I just wanted to make sure I understood what you were doing. Thank you.

MR. ROSS: Some combustibility considerations just to mention a few things.

The combustibility is dependent upon relative concentrations of fuel oxidizer and diluent gases measured as mole fractions.

Too little fuel, or too little oxidizer, or too much diluent prohibits burning.

The strength of a burn depends on the amount of fuel available to burn that actually burns, i.e., the combustion completeness is quite important.

Lesser values of LFL relate to lesser accumulations of fuel at ignition and hence lesser strength burns.

Combustion completeness is a function of the fuel concentration at ignition. Well, just for example of the importance of combustion completeness, if you went from a fraction, a mole

fraction of 0.04 in volume to 0.08 you might infer there's twice as much fuel so you could have twice as energetic a burn.

But because of the complications of burn completeness dramatically really you can involve 33 times more hydrogen in the case of a 0.08 concentration burn than a 0.04 concentration burn.

MEMBER CORRADINI: And you treat this deterministically as I understand it, or is that also sampled later on in the uncertainty?

MR. ROSS: No, combustion completeness determination is inherent in MELCOR.

MEMBER CORRADINI: So it's a deterministic value in the calculation.

MR. ROSS: Right.

MEMBER CORRADINI: Okay, thank you.

MR. ROSS: For the origination of a burn the SOARCA Sequoyah model expands upon MELCOR's default fixed criteria like we've been talking to include LFL variability per the work of Kumar for the propagation of -- we talked about this maybe well enough.

There is some combustible mixture criteria modifications that we made to MELCOR that

I should mention briefly.

We additionally defined sufficient fuel being that hydrogen or carbon monoxide or a combination to be a function of diluent, meaning given how much steam you might have present the concentration of hydrogen that's required to support a burn varied depending on how much steam you have.

And we tried to, or we did incorporate that dependence in the Sequoyah combustion modeling.

We further included a temperature dependence that's modest but we felt an advantage to include such that if a temperature is hotter it's somewhat easier to ignite a burn, meaning you need a little less hydrogen than if you have a colder situation.

We maintain MELCOR's default requirement of needing a fractional content of 0.5 oxygen for a burn to be supported.

That was what we thought was important to point out about containment modeling features.

CHAIRMAN STETKAR: A couple of questions, and mostly because of my ignorance of this stuff.

If I don't have containment failure early. So it overpressurizes and fails late can I get a hydrogen burn when that occurs because I suddenly have a release of pressure? And would that affect my overall results in terms of releases?

MR. ROSS: It could, and it did happen in some of the cases.

CHAIRMAN STETKAR: You allow that to happen.

MR. ROSS: Yes, it's inherent.

CHAIRMAN STETKAR: Okay, good. Thank you. Good.

There was a lot of discussion -- this is something I know nothing about in terms of what's held up in the pressurizer relief tank, the PRT, and when whatever's held up there is released, and the conditions that it's released under.

There was some discussions that said well look, it's not going to get released until temperature gets really, really high. And oh by the way, that's above the melting temperature of the steel of the tank.

How is it actually modeled? Because the rupture disk on the relief PRT always opens

early. So, and I don't know at Sequoyah how big the rupture disk is.

It struck me as if I have a big hole in the pressurizer relief tank that it might not ever actually get very high, and all the volatiles would come out really early which I didn't get a sense that it was doing that.

MR. ROSS: So, the involvement of the pressurizer relief tank was very interesting. And we do have shortcomings in the modeling that we realized in the end.

But yes, the tank saturates and the pressure disk does burst before core damage in many of the cases, I guess in all the cases.

CHAIRMAN STETKAR: I think it would be all the cases realistically. Even if the pressurizer relief safety valves are cycling you're going to -- it's going to fill it and overpressurize that pretty early on.

MR. ROSS: Right. So, any core damage that's going on and pressure being relieved through the safety valves, those fission products are interjected to the pool that's in the pressurizer relief tank and scrubbed there fairly efficiently.

Then when the continual venting to the

tank and decay heat of the fission products uses up the pool eventually, and those fission products wind up being in the tank without any water and without any cooling. And there can be large amounts of the fission products and large amounts of the associated decay heat that's with them.

And we find that that drives a revaporization of the deposits. And when containment ruptures and depressurizes that aggravates the repressurization. And that is seen in the releases to containment.

CHAIRMAN STETKAR: The PRT is modeled as an open vessel with a fairly large hole in it?

MR. ROSS: Yes.

CHAIRMAN STETKAR: It is.

MR. ROSS: It's fairly physical. The three pressurizer valves as well as the pores all vent to a control volume that represents the tank. And there's a flow path atop the tank that represents the burst disk.

And the tank is initialized with a pool.

CHAIRMAN STETKAR: Okay, but after the disk ruptures it now communicates with the rest of the containment.

MR. ROSS: Yes.

CHAIRMAN STETKAR: And I understand that after some point in time all of the water evaporates in there, but it's still communicating with the rest of the containment.

MR. ROSS: Yes.

CHAIRMAN STETKAR: And it's communicating in a way such that the size of the hole in the PRT is not large enough to remove the heat being generated?

Because you're saying -- I think you're saying it eventually heats up and fails.

MR. ROSS: Well, it heats up above the melting temperature of steel. So.

CHAIRMAN STETKAR: I think my question is why does -- I don't know enough about the thermal hydraulics and the physics to know that if I have a tank with a big hole in it can I get enough convective heat removal from said tank such that it essentially just keeps evolving stuff and doesn't heat up to the point where it then suddenly fails.

MR. ROSS: Yes, so that's a good question. We could address that better in nodalization and flow path definition.

MEMBER CORRADINI: Can I ask John's question differently? Since I -- something I wasn't worried about.

The issue is that not only are you releasing gases, but you're actually heating up the walls of the volume?

MR. ROSS: Right, the decay heat.

MEMBER CORRADINI: But the walls of the volume are adiabatic. It doesn't know that there's something outside of it.

MR. ROSS: So I don't know if the tank is physically insulated or not. But not seeing this --

MEMBER CORRADINI: The model is adiabatic.

MR. ROSS: Yes, we didn't have it losing any heat.

MEMBER CORRADINI: So under those conditions, yes.

CHAIRMAN STETKAR: But in the real world wouldn't it lose heat?

MEMBER CORRADINI: Yes.

CHAIRMAN STETKAR: Okay. Not just convective heat.

MEMBER CORRADINI: What am I worried

about here?

CHAIRMAN STETKAR: I don't -- see, that's what I don't know about. This is not my -- but they seem to --

MEMBER CORRADINI: It's in the lower compartment so it's a no, never mind.

CHAIRMAN STETKAR: But there seems to be quite a bit of discussion about the timing of the releases from the PRT as a function of that model. And that's the only reason that I asked the question as if it makes a big deal to the difference of the timing of the releases.

And if it doesn't then I don't care. And if it does.

MR. ESMAILI: This is Hossein Esmaili and you're absolutely correct. It does make a big difference.

And we are going to show that. And this is one area that we can improve our modeling.

But the picture is the same as what you would expect of happening in the suppression pool. They have spargers inside this pressurizer relief tank and it captures -- the pool captures some of these fission products.

Over time heating up the water removes

all the water. And so what happens to the fission products are now they're sitting on top of this dry steel structures. And that heats up the structures. And at some point, once the vapor pressure is just right it leads to a release.

CHAIRMAN STETKAR: I'm sorry, the vapor pressure can't get above the containment pressure because this big tank, which is not all that big, has a big hole in it. It looks not quite as bad as this cup, but a lot like this cup compared to a suppression pool that has no holes in it.

MR. ESMAILI: But the heating of the fission products as they're sitting on top of those steel structures at the bottom once the pool is gone increases the temperature of the heat structure. And I think that's what Kyle was saying.

MEMBER CORRADINI: Where does the PRT reside?

CHAIRMAN STETKAR: Down in the basement.

MEMBER CORRADINI: In the basement? Wouldn't it be covered by all the ice that's flowing down after melting?

MR. ROSS: It's on a pedestal.

MEMBER CORRADINI: And it won't be covered. Ever.

MR. ROSS: Well, let's see. Water level does come up a lot more in an ice condenser containment than a large dry. But I don't know. That would be something to look at.

MEMBER CORRADINI: I had a comment later on about the water depth which is enormous in the ice condenser.

So my thought is this looks a lot like the AP1000. The whole floor is filled with meters of water.

MR. ROSS: We should check that. That would be something very good to check.

MEMBER CORRADINI: Maybe I'm off base there, but it strikes me that there's not dead volume. I thought the PRT was not in a dead volume, that if I start melting the ice it's going to flow all the water from that so it's essentially going to cover everything. Am I wrong?

MR. ROSS: That's a really good point.

CHAIRMAN STETKAR: I don't know how it's configured at Sequoyah, but they're typically down in a place down on the bottom of the basement that's a big enough space to put a fairly decent

sized tank. They're hard to miss.

And they're on a pedestal, but they're not on a 12 foot tall pedestal. They're on a thing that you can get enough anchor bolts.

MEMBER CORRADINI: You caught something that I missed. I'm just combining what you're worried about with my question later on which is where does the water go when I melt all the ice.

And if I melt all the ice my impression is it doesn't sit in the reactor cavity below the vessel. It's so much water that it essentially fills and essentially fills all these various volumes.

Does MELCOR consider that possibility with the appropriate flow paths?

MR. ROSS: It does. It does at that. And if we had connected the heat structure that represents the tank to the control volume, if there were a pool there it would have taken advantage of it. But we did not. And have not looked to see how the pool compares to where the tank is.

MEMBER CORRADINI: But considering you treat it as adiabatic it wouldn't matter. But it just strikes me though that the big effect in an ice condenser is once I melt this, good God,

there's water everywhere.

MR. ESMAILI: But it requires a lot of water before it can start actually flowing down to the lower regions and into the cavity.

MEMBER CORRADINI: How much? Is it tracked? In the calculation is it tracked?

MR. ESMAILI: It is tracked. In our calculation it is tracked. It requires about the entire RWSD plus 25 percent melting of the ice according to FSAR for water to start flowing into the cavity.

As a matter of fact, in a lot of the calculations that we see the cavity is dry at the time of lowering.

MEMBER CORRADINI: Okay.

MS. GHOSH: Is it okay if we skip over the process stuff which is exactly the same -- is it okay if we skip over the first few slides which just talked about our process which was the same as the Surry UA and then we just dive into the parameters themselves?

CHAIRMAN STETKAR: I was reading notes, I'm sorry. Where are you?

MS. GHOSH: We just skipped onto 37.

(Simultaneous speaking)

CHAIRMAN STETKAR: We heard about the storyboard process.

MS. GHOSH: Right.

CHAIRMAN STETKAR: Yes. Yes, yes, sure.

MS. GHOSH: And you already know we're focusing on containment.

CHAIRMAN STETKAR: That's all the same as -- I mean the process is the same as you described for Surry. We talked about that.

MS. GHOSH: Just to save a little time.

CHAIRMAN STETKAR: Well, just make sure you're -- and because nobody knows you make sure.

MR. KIRCHNER: My question before you go into these parametric cases is have you explored the noding of the containment since this is a containment focused study on things like hydrogen accumulation, stratification, burn?

Because these are -- yes, in particular by compartment or in the containment dome.

MR. ROSS: So nodalization studies are not something we've accomplished in the current work.

Hossein gave some good history of earlier work.

MR. ESMAILI: This is Hossein Esmaili. This was going back to the older work that was done in terms of DCH issue resolution and all the calculations for the GSI-189.

They did have finer nodalization of the containment, upper compartment, et cetera. And so they did some differences. But in our analysis we are looking at the big picture.

So, from what I remember they did not find a big sensitivity, even though the nodalization was still porous. So in the upper compartment it's not a CFD code, it's a systems code. They maybe modeled it for two control volumes. But they did not find any big sensitivity.

MR. KIRCHNER: So if I may ask another question, jumping ahead perhaps to the results, did these various hydrogen scenario, burn scenarios have any major impact or change in the results?

MR. ESMAILI: Talking about the old calculations, the previous nodalization calculations?

MR. KIRCHNER: In your study here.

MR. ESMAILI: Okay, so the short answer is that no, we were informed by the previous

calculations. We have not done a nodalization study here. Because we just learned what was done before.

We do change the flammability criteria. Don't forget that this is a huge volume, 4 percent, 6 percent, 9 percent. I think if I'm not mistaken Kyle takes care of some of that in terms of where a burn occurs, or how it propagates. But to tell you honestly, no, we did not do a nodalization here again.

Had we done a nodalization would it change? Probably. But will it change the overall picture? I don't think so.

MR. KIRCHNER: -- major impact on the consequences.

MR. ESMAILI: No, because even in these cases we are seeing a large fraction of the calculation resulting in early containment failure.

So for example, I know Kyle's going to get into this later, but for example an early containment failure can be 20 percent of the calculation we have early containment failure.

If it's done a little bit differently could it be 30 percent. Yes, it's possible. But you know, it's still a very high number.

MEMBER BALLINGER: I was going to ask about the stratification issue until I think I heard that MELCOR actually keeps track of that, of the hydrogen migration through the different volumes. Did I hear that correctly?

MR. ROSS: You did, yes.

MEMBER BALLINGER: Okay. So that sort of takes care of any stratification issues because it somehow has to deal with migration of hydrogen.

MR. ROSS: As the nodalization -- it would be probably a good venture to nodalization. That's the most difficult sensitivity to perform is changing the nodalization.

MEMBER BALLINGER: Because hydrogen moves pretty fast. And it does stratify.

(Simultaneous speaking)

MEMBER CORRADINI: The only experiments I'm aware of where you've seen stratification is where you have to have a very high concentration.

(Simultaneous speaking)

MEMBER BALLINGER: Cooling is what I'm saying.

MEMBER CORRADINI: It stratifies by compartment. The old HDR experiments showed stratification, but that was because by

compartment.

But if I'm in a large open volume it's pretty well-mixed. That's where I thought Walt was going with this but I didn't --

MEMBER BALLINGER: Well, after my adventure with hydrogen detonation we had to do a calculation in my lab for the distribution of hydrogen for a point source of hydrogen. And it definitely pooled in the ceiling quickly.

(Simultaneous speaking)

MEMBER CORRADINI: Once I mix it, it doesn't unmix. And then I'm getting continual buoyant plumes coming out of the ice basket that's doing this.

MEMBER BLEY: -- probably didn't mix. It streamed.

MR. ESMAILI: So, you're absolutely correct. And then we have to understand that we are talking about MELCOR. It's a long parameter model. The type of stratification, the type of things that you are seeing probably it's best handled with more sophisticated code or CFD codes.

But as Dr. Corradini is saying, if you expect a well-mixed condition we are capturing some of these phenomena.

MEMBER BALLINGER: You're assuming it's well-mixed.

MEMBER CORRADINI: Right, but I guess my -- I never was worried about this, only from past experiments that I've seen where you don't need much of a buoyant force to cause a mixing within a large volume.

Compartmentally I think they capture it because of the nodalization.

CHAIRMAN STETKAR: Anything more? Good. I'm going to make an executive decision here. Mike has to leave at 2 so we're going to take a lunch break here and I'm not going to give you an hour.

So let's come back at 12:30 and pick it up there. Because I don't want to break somehow after 15 minutes of this discussion. Let's reconvene at 12:30. Sorry about the short lunch break but there you go. We're recessed.

(Whereupon, the above-entitled matter went off the record at 11:47 a.m. and resumed at 12:33 p.m.)

CHAIRMAN STETKAR: We are back in session. And again, Mike, if you have any particular things that you -- no, seriously. If

you have anything particular that you want to explore, we should do that. Interject and -- no, apparently he's happy. Go forth.

MR. ROSS: All right. Let's see. We wound with nine parameters that we sampled as uncertain. There were parameters related to sequence, related to in-vessel accident progression, ex-vessel accident progression and containment behavior, hydrogen combustion and aerosol transported in deposition.

PARTICIPANT: We already talked about those --

(Simultaneous speaking.)

MR. ROSS: Yes, we described this slide. So the next few slides are real specific points about what we modeled failure-wise for the pressurizer safety valves, and they're taken straightaway from the Sequoyah report which we tried to write better descriptions with respect to the valves than we had in the Surry report.

But would it be good for me to go through each of these bullets about --

CHAIRMAN STETKAR: It's up to you if you want to go through -- I have some real problems

with this, and I still do --

MR. ROSS: Yes, I --

CHAIRMAN STETKAR: -- in a couple of areas. But if you want to go through the bullets for the benefit of the other members --

MR. ROSS: Okay.

CHAIRMAN STETKAR: -- it might be useful.

MR. ROSS: So the failure probabilities for the safety valves are obtained from table 20, Failure Probabilities for PWR Code Safety Valves Behavior after scrams, in NUREG/CR-7037.

This table reports on safety valve operation subsequent to actual scram events. Information is included for both main steam system valves and reactor coolant system valves.

The assumptions made in the UA that main steam valves and RCS safety valves are alike enough in construct and servicing that their failure data can be jointly considered.

Subsequent demands were assumed to have the same failure probabilities as initial demands.

Recovered valve function; e.g., a previously stuck-open valve closing when pressure

reduces, was not considered to be successful valve operation.

The Main Steam Safety Code Safety Valves section of table 20 reports no failures to open and 15 failures to close in 769 demands considering all failures, recovered and non-recovered.

The Reactor Coolant System Code Safety Valves section of the table reports zero failures to open, but two failures to close in only four demands considering all failures.

Combining the main steam system valve and the RCS valve failures identified no failures to open and 17 failures to close in 773 demands.

So these failure rates served as the bases for the uncertainty characterization of stochastic SV failure that we included in both the Surry and Sequoyah UAs.

And noteworthy with respect to these rates is that they are derived from actual events at U.S. plants and not from testing.

MEMBER SKILLMAN: Kyle, let me ask you, back on page 39 --

MR. ROSS: Yes.

MEMBER SKILLMAN: -- the third bullet, the assumption is that these valves are sufficiently similar --

MR. ROSS: Right.

MEMBER SKILLMAN: -- that their history can be considered jointly. In reality, while they are kind of similar, they can be very different. So I'm wondering why that is a valid assumption, because on the next page, again third bullet, I think what you're saying is 17 out of 773 is the failure rate, or the projected failure rate.

MR. ROSS: Right.

MEMBER SKILLMAN: The probably failure rate.

MR. ROSS: Right.

MEMBER SKILLMAN: The pressurizer valves could be a different design and the main steam valves can be either safety valves or relief valves, which are fundamentally different in their design. So unless there's been an attempt to identify the uniqueness of the hardware, then I would suggest that the assumption you make on page 40 may not be accurate.

MR. ROSS: Yes, so that may be a real

good point. I believe in the NUREG there was mention of that being a fair assumption, but I haven't got -- I'm afraid I can't quote where that is today. I should have thought of defining that.

MEMBER SKILLMAN: Well, if there is a justification that would address the appropriateness of that combining, I would say, okay, let it go, but unless there's such a justification, I would simply observe that the main steam valves and the primary relief valves can appear to be similar, but they can be very, very different in how they're constructed and what they're -- in all candor, what their guts look like.

MR. ROSS: Right. Yes, there was considerably more information for the main steam valves. There wasn't much for the primary valves. So that was a benefit to us to be able to use that, the more occurrences that there were available if lumped the two together. There was actually a stark difference in what the few events involving the primary valves resulted in. They failed. Well, there were four events and two failures. But we're thinking that maybe that was because of

flowing water that they failed as opposed to flowing steam.

The construction of the plants is such that there are water traps in front of the safety valves, and if those traps were full of water and if the information that EPRI has published about the vulnerability of a valve failing to close if it has to pass liquid water, we wonder if those two failures out of four were because the traps were full of waters. And the valves did pass water when they were demanded to lift.

MEMBER SKILLMAN: So that gets back to what John said a couple hours ago. The devil really is in the details here. The model is accurately -- you really need to be able to demonstrate --

MR. ROSS: Yes.

CHAIRMAN STETKAR: -- what that plumbing looks like.

MR. ROSS: Yes. And this was another case where we didn't foresee the dramatic influence that valve failure would have on the result metrics.

MEMBER SKILLMAN: Okay. Fair enough.

Thank you, Kyle.

MR. ROSS: So in great interest after the Surry meeting we did -- it was pointed out to us that there was some additional information, some EPRI information on valve failure that would be good for us to look at. And we have. And there are some implications that maybe there are considerations that would be better for us to make.

The major points are that -- one that there's a very big difference between what the likelihood of a valve to fail on first demand than the likelihood of it to fail on subsequent demands such that if it operates per design the first time it lifts, it will probably operate per design in all times that it lifts.

The second is that passing water is a true issue for these valves and that if they have to pass water, there's a real good chance that they won't re-close.

So it seems that maybe a better consideration of valve failure would be to have one distribution for the likelihood that a valve would fail to lift the first time it was asked to and then a different distribution for all subsequent

lifts of a valve in the modeling. That would lead to cases that either fail on the first lift of a valve or cases that valves don't fail at all for the most part in the sampling.

CHAIRMAN STETKAR: Let's stop here. I've struggled with this, and not for the reasons that Dick brought up. By the way, in the reference, NUREG/CR-7037, they do differentiate clearly between relief valves and spring-loaded safety valves, so it's not that type of issue.

Honestly, I do not understand where the numbers in table 20 of NUREG/CR-7037 come from. Honestly, I've been the through the appendix, Appendix B. I've been through the discussions in whatever section it is of the NUREG where those tables appear. I can't for the life of me understand certainly where the denominator comes from, the 769 and the 4, because I can't find that documented anywhere. I find things that are around 150 or so real demands. I find much smaller numbers of failures.

I'm assuming that the failures came from real data, but we know from experience that that might be -- the vast majority of them might

not be off-lay open. It might be a weeping valve. But because there's a small number of failures documented, and ostensibly that comes from either NUPICs or -- I'm sorry, EPIX or LERs, there ought to be actual descriptions of what happened in the failure, the numerator. I still can't figure out where the numerator came from, but I can come closer to that number. For the denominator I can't figure out where the denominator -- I honestly cannot figure out how that number was generated. I've tried and I can't.

So as a take-away I would really like somebody to go through -- contact the authors or something in that NUREG and explain where those numbers in that table 20 came from, because I can't figure it out. It's really frustrating because I can usually look at a NUREG and say, oh, okay, if I add up one from column A and one from column B and three from column C, I get five. This case I can't.

The thing that bothers me is there's a big discussion in that chapter about a risk-informed modeling of demands. And there are tables where I can count up an inferred number of demands

from things like high-energy line breaks and fires and things that they admit aren't really modeled, but I can't make up -- I can't get the inferred number of breaks from that to add up to the 769.

I can't get any of what is I think in the appendix supposedly based on actual demands, documented demands, but there even the demands documentation tends to say things like, well, yes, under certain types of events we would expect to see a demand and really in the LERs and the EPIX database the demands aren't documented, but we could look at the event and the trip and use our SPAR models to understand. Well, the SPAR models don't account for steam dumps. SPAR models often don't account for steam relief valves. So the SPAR models might challenge the safeties, secondary site safeties where we get most of the demands, much more than the real world really does.

So I don't know. Are there bases for the numerator or the denominator? Why is that important? Well, it's obviously important because that's the best estimate for your failure rate, 17 over 773.

MR. ROSS: Right.

CHAIRMAN STETKAR: And then you expand the uncertainty distribution beyond that.

You note on this slide that you don't account for the testing failures because, lo and behold, the testing failures show much, much lower failure rates.

MR. ROSS: Right.

CHAIRMAN STETKAR: There's only a couple of failures in, I don't know, 10,000 demands, or 8,000, or something like that.

MR. ROSS: Right.

CHAIRMAN STETKAR: If I look at though the fail-to-open from the testing, it's comparable to other estimates based on ostensibly real data, zero failures.

Something that bothers me more, and I want to put it on the record, is that the level 3 PRA project, which is a real PRA, will be using these tools and models for the Level 2 and 3 parts of that study, I've been told. In that report -- I only have a version of June of 2014 of the data part of that report, but in that report for the fail-to-close failure rate for safety valves, steam or pressurizer, because they've lumped them

together, they use mean values on the order of 7 times 10 to the minus 4, not 2 times to the minus 2.

MR. ROSS: Yes.

CHAIRMAN STETKAR: In fact, the 95th percentile of their distribution for failure to close is 1.9 times 10 to the minus 3, or a factor of 10 lower than your middle estimate.

So now we have two studies being done by the NRC that have wildly different failure rates for these valves that have been identified in both this study and the Surry study as, however they slice and dice the results, the most important or the second most important parameter in the overall assessment. And that really bothers me, because if suddenly the Level 3 PRA using their data come up with much, much different conclusions about releases and timing and everything, I don't know how who is right. And they're using data that's published in a different NUREG, NUREG, whatever it is, CR-6928 I think it is. Yes.

So we need to get to the bottom of this. And as I said, the thing that really troubles me about the 7037 is that I couldn't

figure out where the numbers came from. If I could figure out where those numbers came from, I could start thinking about how real they might be, but I couldn't. So that's -- we're not going to resolve that here, but it's a real concern. I brought it up in Surry, but you've pointed me -- I thought the numbers were coming out of the wrong tables in Surry. You pointed me to the right table in Surry, so now then I started to do homework on that table and I couldn't figure it out.

So it's -- but we certainly between the SOARCA projects and the Level 3 PRA, if we're two orders of magnitude different on the failure rate of something that now two of the SOARCA studies are saying is the most important single parameter for the conditional risk, that's not good.

MS. SANTIAGO: Yes, and we -- several of the staff are working on the Level 3 PRA. And so we'll look at that specific issue and see if we can't address that, or at least identify which is the best source.

CHAIRMAN STETKAR: By the way, there is a table 31 in NUREG/CR-7037 where they do combine the test data and the operational data. I can't

get those numbers to quite add up either, but they come around
-- the denominator is somewhere around 8,000 or so. They use the numbers from that table in their example in Appendix C as if those are the numbers they rely on for actual risk analysis, because they have a little example in Appendix C how the numbers from all of these tables would be used in a PRA. And they seem to snatch them the numbers out of that table 31, not table 20.

There's no reference made to table 20 for failure rates. That's the other reason why I'm not -- I have no idea where those numbers in table 20 fit into the whole picture there. And those numbers are down in the -- that table 31 number. They do have successive failure rates and things like that, but the initial fail-to-close is down in the middle 10 to the minus 4 range. It's much more comparable to the value that's being used in the Level 3 PRA.

And why is it important? Well, it's obviously really important. I mean, not only important for the purpose of looking at early failure of the containment in Sequoyah, but it's an

important parameter for the consequential tube rupture. There's no free lunch. Pressure stays high, the tubes rupture. Pressure gets low -- in this case you get a big release of hydrogen, but the tubes don't fail, if you were going to model them.

So, please, somehow sort that out.

MS. GHOSH: I guess, I think we can skip over this. These are more --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Turn your mic on, Tina.

MS. GHOSH: Sorry. I was just saying we can skip over the actual distribution. We've talked about this a lot, I think.

CHAIRMAN STETKAR: Yes.

MS. GHOSH: And we've also talked --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Yes. No.

MS. GHOSH: Sorry. Yes?

CHAIRMAN STETKAR: The one you skipped over --

MS. GHOSH: Yes?

CHAIRMAN STETKAR: -- is the one on 43.

And we want to reiterate that and then --

MS. GHOSH: Oh, okay. Yes, sorry. That's right.

CHAIRMAN STETKAR: Okay. Yes, we skipped that one.

MS. GHOSH: We didn't talk about this --

(Simultaneous speaking.)

CHAIRMAN STETKAR: -- So let's -- say it in three points, if you want to say them.

MR. ROSS: Yes, yes. So considering the fractional position that a safety valve would arrive at, given that it suffered a failure to close, we didn't -- we were not aware of any database to pull from in that regard, so we settled in on a uniform distribution.

MEMBER BLEY: Did you try talking to somebody who works with valves and tests valves? We talked about this the last time. I have fixed a lot of valves. I have. I've seen safety valves stick open. I've seen them go closed. I've seen them not quite all the way open and not quite all the way closed leaking a little bit.

MR. ROSS: Yes.

MEMBER BLEY: I've never seen them fail part way in between. I don't know if anybody on this committee has, but I -- talk to somebody who's -- people work on these things. People have seen them fail. Rather than in a database, I just don't think there's anything to support this -- and we use this assumption in a bunch of places of -- I don't quite know the answer, so I'll just assume everything's equally likely.

Well, there's knowledge just beyond what you find in a database, and you can use that to come up with more sensible distributions if you don't know exactly.

CHAIRMAN STETKAR: Even in this case, as I mentioned earlier, the thing -- let's presume the 17 events in the numerator in that table 20 are legitimate actual events that you can go pick up a piece of paper and read about them. There ought to be information there. I mean, typically doing data analysis I'd be thrilled to have 17 events that I could actually go read about. And if all 17 of those are small leaks, that would seem to inform my uncertainty distribution about the size of the leak. Or if 3 of them are stuck fully open and 14

were small leaks, that would seem to give me a different shape of my uncertainty distribution, even from those 17 events.

MEMBER BLEY: By the way, your colleagues doing the Level 3 analysis hired some real valve experts who could probably help you inform your uncertainty distributions on this.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: Yes, they weren't looking at that. They were looking at spurious opening of check valves and --

MEMBER BLEY: But they know valves.

CHAIRMAN STETKAR: But they know valves.

(Laughter.)

CHAIRMAN STETKAR: They're valve people.

MS. GHOSH: Appreciate the feedback. We appreciated the feedback from the Surry UA discussion. And as I mentioned before, this analysis was already complete by the time we got the feedback at the Surry UA meeting, but since then in our spare time, which has been not so great because we were still doing everything for

Sequoyah, we did try to go back, look at the EPRI reports with the -- all the -- that explained the testing to see qualitatively what can we get from the description that they would explain?

We found there was on description of an actual event, something from a scram, that it said that the valve failed partially open and it had no description of what that partial open area was. But we know that there's at least one qualitative data point that it wasn't full open or full closed.

We would like to talk to valve experts. We didn't have a chance to do that, but we are pursuing what we can gain for this area. Unfortunately, the folks who developed the NUREG-7037 and who maintain that database, it's basically the operating experience database that INL helps us with. We did talk to them and they've pulled the data since 2007 to see what additional operating experience-based data we could have since 2007 using that same category for the numerator and denominator. And we have the updated numbers for that. And since they just did that, we'll certainly ask them what exactly -- how exactly they consider the denominator in terms of the demands,

which is --

CHAIRMAN STETKAR: Yes, that's --

MS. GHOSH: -- a much harder number to come up with --

(Simultaneous speaking.)

CHAIRMAN STETKAR: It's just surprising to me -- from knowing pressurized water reactors, it's surprising to me that between 1987 and 2007, or whatever the database period is that they used, that there would have been 773 -- 769 actual demands for a main steam safety valve, because it's hard to get that to happen. You can get it to happen, but it's hard to get it to happen on a PWR. I mean, you have to have the condenser steam dumps not there. You have to have the atmospheric reliefs not there. And that's -- or you have to have one heck of a pressure spike in the secondary side. And it's hard to get those unless all of the MSIVs slam shut.

MEMBER BLEY: By the way, one other source of expertise. At least in OP E, and it used to be NRO, there were a fair number of former operators with years of experience who have been involved who might have some useful information for

you here within NRC.

CHAIRMAN STETKAR: One last comment on the valves, because we need to go on I guess, is that in this report, when I read about the area fraction, it's characterized in words consistently as the thermal fail-to-close open area fraction, which to me means the likelihood that they do not close given a high temperature condition, over normal temperature.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: And I know doggone well it's also applied for the stochastic. And it's not clear to me that the same uncertainty distribution would apply for both of those cases either. That's another valve.

MS. GHOSH: Thank you for pointing that. That was an artifact of previous modeling. And we tried to take out that language in the report, but I'm sure we missed a bunch of instances. We will update that --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Well, in the Surry report it was clear that the same distribution was used --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- for both. There was more discussion.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: But this one was pared down and only the thermal part was left in. And I thought when I first started reading, I said, oh, they're only modeling this for the thermal part. But it became clear really, really quickly that that wasn't the case. Okay.

MS. GHOSH: Okay.

MEMBER SKILLMAN: John, to the frequency, from '82, '84 through about the same -- 10 years later, '92, '94, the trip rates were up around seven to eight trips per plant per year.

CHAIRMAN STETKAR: Sure. Yes.

MEMBER SKILLMAN: And very often reactor trip would isolate the turbine.

CHAIRMAN STETKAR: Yes.

MEMBER SKILLMAN: And that would commonly lift six or eight --

CHAIRMAN STETKAR: Maybe on your plant; not on mine.

MEMBER SKILLMAN: Oh, yes.

CHAIRMAN STETKAR: I went through in five years more trips than you probably ever saw in your whole life. I was at Zion. We used to trip each unit at least once a month and we never lifted a steam generator safety valve.

I don't know what your experience has been, Matt. Have you ever lifted any?

MR. SUNSERI: Yes, this is Matt Sunseri, invited expert. My experience is on the PWRs every refueling outage we took the safety valves off the primary side, set them off to why they lapse. That is a surveillance test and if they fail that stroke, that counts as a demand failure.

CHAIRMAN STETKAR: Yes.

MR. SUNSERI: But then you'd be counting that data, too. That would get the number way up high, the number of demands.

CHAIRMAN STETKAR: That's true, but they've culled that out of here. That's most of the 8,000 testing demands that they're not accounting for. They're saying that there are 769 actual --

MR. SUNSERI: Actual actuations.

CHAIRMAN STETKAR: -- lift demands on a main steam safety valve for pressurized water reactors in an amorphous glob. But pressurized water reactors between 1987 and 2007, I think was the database period -- so it's 20 years, but it still seems like a lot.

MR. SUNSERI: Yes, so my experience on that would be --

CHAIRMAN STETKAR: Despite the fact that the plants were tripping, because we never lifted one at Zion.

MR. SUNSERI: Yes.

CHAIRMAN STETKAR: We had bizarre trips.

MR. SUNSERI: The B&Ws, John, would lift the whole set of them, so we'd get 11 or 16.

MEMBER BLEY: A lot of the older plants didn't have as much turbine bypass capacity.

MR. SUNSERI: Yes, so the BW plants really --

(Simultaneous speaking.)

CHAIRMAN STETKAR: But even then you have reliefs that were lower. You still had atmospheric steam generators that were supposed to

go before the safeties. You'd have to get a pretty doggone big pressure spike out there to pop a safety. Anyway, that's --

MR. ROSS: Let's see. I'll point out that in the calculations if the open fraction was 0.3 or greater, it was all the same. If a valve failed 0.3 of its area or bigger, it was --

CHAIRMAN STETKAR: That's probably reasonable. That was big enough so you never got a demand for the next valve.

MR. ROSS: Right.

CHAIRMAN STETKAR: Yes.

MR. ROSS: And then just to quickly point out, too, the regression input parameter that we used to represent the fractionally-open position of the safety valves on the pressurizer was the sum of the open fraction of the three valves, and as was the number of valve lifts. The input to the regression analyses was the combined number of lifts experienced by the three-valve system, not just any valve on its own.

CHAIRMAN STETKAR: When you think about presenting the results; and we'll get to -- I think you're going to talk about individual realizations

later. I don't remember.

MR. ROSS: Much later.

CHAIRMAN STETKAR: Much later? The description of those tables you can follow. The tables are very, very misleading.

MS. GHOSH: Sorry. Which tables?

CHAIRMAN STETKAR: When you talk about the individual regression analyses back in chapter 4 --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- they're comparative when you compare realization -- I'm sorry, not regression analysis. Individual realization. There are tables that -- the one that prompted my rant this morning about treating each valve independently.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: You easily delude yourself into the fact that despite the first valve failed 100 percent open, the second valve didn't fail until the 97th demand, when indeed it was really never demanded to open.

The discussion sort of walks you through that, but I'll tell you just trying to

figure out how the model really works looking at that could -- is misleading.

MS. GHOSH: Okay. Yes, that's a fair point. Yes, we just did the -- we just transcribed the data of the original inputs, but it could be misleading.

MR. ROSS: So next slide. So we've --

MS. GHOSH: We've covered this, yes.

MR. ROSS: -- covered this, it seems. Yes.

MS. GHOSH: And this, too.

MEMBER CORRADINI: So go back.

MS. GHOSH: To --

(Simultaneous speaking.)

MEMBER CORRADINI: So I want to make sure that I get this right. So in the Surry case you have the same functional shape, but just it was centered at a higher temperature?

MR. ROSS: No, it was identically the same.

MEMBER CORRADINI: Okay. But you --

MS. GHOSH: This is the SOARCA value. That's from the original best --

PARTICIPANT: Microphone.

MEMBER CORRADINI: That's what I thought.

But let's just -- so let me say it again and then -- I said it incorrectly. For the uncertainty this is what you used. For the best estimate you had it centered at a higher value. And then you've chosen only to use this to see if there's an important effect, which as I could tell by the realizations, when John gets to them, did not have a big importance compared to everything else.

MR. ROSS: Well, it did influence in-vessel hydrogen production, yes.

MEMBER CORRADINI: Essentially then and only then?

MR. ROSS: Right.

MEMBER CORRADINI: Okay.

MS. GHOSH: I think we talked about the --

MR. ROSS: I believe we did. We jumped ahead, right?

MS. GHOSH: Yes, we did. I think we talked about this, too. We talked a lot about this.

MR. ROSS: Yes, we did.

CHAIRMAN STETKAR: Now --

MS. GHOSH: You want to go back to -- okay.

CHAIRMAN STETKAR: No, no.

MS. GHOSH: Barriers?

CHAIRMAN STETKAR: We need to talk about the barriers here.

MR. ROSS: Okay. Good.

MS. GHOSH: Sure.

MR. ROSS: So I described this earlier. Is there something -- is there something in particular I could re-describe or --

CHAIRMAN STETKAR: Yes, and I'm not a -- if we could go to the next slide. Thank you.

PARTICIPANT: My God, write it down.

CHAIRMAN STETKAR: Holy cow. Let it be known I failed.

This model says that every square millimeters of the barrier seal has an equal probability of failure given an applied DP. And the size of the hole is only on the top scale. The distribution of the various sizes of the 26.

MR. ROSS: Right.

CHAIRMAN STETKAR: Thanks.

MR. ROSS: Right.

CHAIRMAN STETKAR: Is that really right? I mean, if I have small piece like this compared to a very large piece --

MR. ROSS: Right.

CHAIRMAN STETKAR: -- is it equally likely that every square millimeter of the small piece would fail compared to the large piece? And mostly I'm thinking about tearing along the edges where the circumference of the second piece is much, much larger than the circumference of the first piece.

MR. ROSS: Yes, so essentially we assumed that any small segment was as vulnerable as any other small segment. But then given the actual segments that there are and their lengths we assumed that the longer lengths had more chance of failing because they --

CHAIRMAN STETKAR: It's just apportioned by area of each segment?

MR. ROSS: Yes.

CHAIRMAN STETKAR: So that a segment with 10 times the area has a 10 times larger --

MR. ROSS: Correct.

CHAIRMAN STETKAR: -- likelihood of failure? That's the bar chart on the top there?

MR. ROSS: That's right. That's what we --

CHAIRMAN STETKAR: So what you do is your sample a DP from that part. At that DP there's some likelihood that every square millimeter fails and then you distribute the sizes according to the top part, right?

MR. ROSS: Yes, that's right.

CHAIRMAN STETKAR: Yes. And what I'm asking -- and I don't know. I don't know how people evaluate failures of membranes. I'm looking at people who might to know whether that makes sense. Does it? I mean, I just don't know.

It seems to me a reasonable approach to it. If I have a flexible membrane that's constrained at the edges --

MR. SHACK: Well, I mean, you're looking for a defect in it and that defect seems as likely in one piece as another. So the bigger one's likely -- you're likely to really find that defect in which put the pressure on it. Yes,

that's more likely to fail.

CHAIRMAN STETKAR: Assuming it's rigidly attached at the edges and those edges cannot tear. My concern is that the much larger circumstance of the bigger pieces has a higher conditional --

MR. SHACK: We've got damage --

CHAIRMAN STETKAR: -- likelihood of tearing around the circumference.

(Simultaneous speaking.)

MR. SHACK: -- area?

CHAIRMAN STETKAR: Not --

MR. SHACK: Yes, not the overall area.

CHAIRMAN STETKAR: Not the overall area, but there's -- and I don't -- I just don't know. I mean, I don't do that kind of work.

MR. SHACK: I'm sure you'd come up with a much different answer. I mean, the length of the clamped area is proportional to the size of the thing, so whether you take one or the other, you'd end up basically -- the big one's more likely to fail.

CHAIRMAN STETKAR: Well, but is it more likely to fail according to the ratio of the areas

or is there different --

MR. SHACK: Well, since the width of this is uniform --

CHAIRMAN STETKAR: R and R squared.

MR. SHACK: -- the perimeter is going to be the -- they're going to be proportional. So you'll never know whether it's really -- if we had different areas as well as different --

(Simultaneous speaking.)

CHAIRMAN STETKAR: It's if people understand how these things fail I'm getting pushback from, that's good enough. I was just raising the question because if it -- I have a swimming pool covered with tears on the edges. It doesn't tear in the middle.

MEMBER CORRADINI: Yes, but I thought Bill's point was that it's linear, so it's equally --

(Simultaneous speaking.)

MR. SHACK: I mean, if you use model or the other, you'd come up with the same answer.

CHAIRMAN STETKAR: Okay.

MEMBER BALLINGER: They typically do fail at the clamping region. They tear because

there's a concentrator there. Unless they've done something special to eliminate it. That's the -- stuff like that.

MR. SHACK: But it's still -- it depends on having the defect in the fabric presumably, which is accentuated by the concentration, but it's really the fabric then. It's the flaw density that you're worried about, unless you induce the flaw with a clamp when you install it.

MEMBER BALLINGER: Yes, typically --

(Simultaneous speaking.)

MR. SHACK: In this particular case you'd get the same answer.

CHAIRMAN STETKAR: Okay. That's all I'm looking for. It was a question.

I do have another question, though, that I don't understand. If I look at table 4-14 in the report, there's an entry in that table that has fabric seal open area fraction given tearing. That's one of the sample parameters. And this again, is the realizations. Several entries for that parameter list values that are greater than 1.0. How do you get a larger than total -- I mean,

this process shouldn't give you more than all of the seal area failure because the top part is designed such if you sum it all up, it comes out to 1.0. Right?

MR. ROSS: What table is that?

CHAIRMAN STETKAR: It's 4-14.

MS. GHOSH: On page 495.

CHAIRMAN STETKAR: You see the entries. I mean, if you're looking at the report, you see there are several entries that have greater than 1.0 for that parameter.

MR. ROSS: There sure are.

CHAIRMAN STETKAR: And that -- the red bar chart above sums to 1.0.

MR. ROSS: Yes.

CHAIRMAN STETKAR: I added them up. That's a curiosity. I don't --

MR. ROSS: Yes, it is a curiosity and --

CHAIRMAN STETKAR: I mean, you can't even say, well, it's -- you can't get multiple failures. They can't reseal themselves. Even if you get multiple pressure pulses it -- I don't know how it adds up to greater than one.

MR. ROSS: I don't either.

CHAIRMAN STETKAR: Okay.

MR. ROSS: I don't either, and I put those numbers in there. So I --

(Laughter.)

CHAIRMAN STETKAR: Okay. Well, take a look at that. I don't know. It's a curiosity.

MR. ROSS: I sure will.

MEMBER CORRADINI: It's not an actual square footage, is what I'm curious about, versus --

CHAIRMAN STETKAR: No, it's listed as open area fraction, and I'm assuming that's what it is, which you would get from this kind of thing. Okay.

MR. ROSS: Yes, I don't know.

To mention some of the points about the sampling that we set up, there was a discrete distribution applied in specifying the open area left by a failed segment. Prototypic testing of these seals was to 30 psid back when. The upper bound at the differential pressure that would load a seal to design capacity was 100 pounds per inch, which relates to 57.14 psid.

We used the beta distribution applied so that the majority of failures occurred between the qualified test pressure of 30 psi and the design strength of the material at 54 psi. The effective lower bound at 15 psid resulted consequential to the applied beta distribution. Ten percent of the samples are below design to somewhat acknowledge accident conditions, especially that elevated temperatures might be -- the seals might be exposed to elevated temperatures in a station blackout environment.

MEMBER BALLINGER: Yes, I had a question about -- not this -- I couldn't quite figure out why the two peaks --

MR. ROSS: Where?

MEMBER BALLINGER: -- at 6.7 and 18.6. Does that correspond with some unique dimension?

MR. ROSS: I think maybe there's a lot of segments that were that were --

MEMBER BALLINGER: Because it just all of a sudden --

(Simultaneous speaking.)

MEMBER CORRADINI: -- based on population, right?

CHAIRMAN STETKAR: That's right. For example, the 0.58, there are several segments --

MEMBER BALLINGER: Okay. So segments?

CHAIRMAN STETKAR: -- that have that square footage.

MEMBER BALLINGER: Okay. Got it.

CHAIRMAN STETKAR: And that's why it peaks in there.

MEMBER BALLINGER: Okay. That's what -- okay.

MR. ROSS: So we need to mention that in the course of preparing this slide we believe we found an error in the strength understood for this material, an error factor of two. So we think we nominally defined this fabric to be twice as strong as we should have in the sample. However, we had enough samples that there were 51 calculations where the strength was defined reasonably. And interestingly in those 51 cases the seal tore in 29 of them and all but one of the 29 seal failures were associated with a late container failure. Very interestingly the pressure difference that failed the seal was backwards from what might have been expected; i.e., the pressure was higher in the

dome in the lower containment.

Seal failure looks to be a consequence of -- so this is hard to tell. It's hard to tell whether seal failure is a consequence of a burn in the dome that is strong, but not strong enough to rupture containment or if it provides more expanse for a burn in the dome to fill when there is a big burn such that you'd be less likely to reach the failure pressure of containment. It's not clear to us which is the story here.

So I think we've talked about --

CHAIRMAN STETKAR: We talked about this one. The top -- now, let me just ask you about -- we talked about the bottom doors with the hinges and the design of those.

MR. ROSS: Right.

CHAIRMAN STETKAR: The top doors sounded like there was something that if I opened it past the vertical, it would flop over and stay open.

MR. ROSS: They do that.

CHAIRMAN STETKAR: Okay.

MR. ROSS: Yes.

CHAIRMAN STETKAR: So they're designed

differently from the bottom doors?

MR. ROSS: They are.

CHAIRMAN STETKAR: How were those modeled?

MR. ROSS: They were modeled -- they don't --

CHAIRMAN STETKAR: Does it make any difference?

MR. ROSS: I don't believe we ever lift those very well.

CHAIRMAN STETKAR: Oh, okay. Because there was some discussion about the fact that they're mostly treated as reversible, unless they're open fully, and there's some chance that they then basically stick open, if you will, because of this kind of falling backwards on it.

MR. ROSS: Because I remember standing there lifting one. Don't they -- they do pass vertical so that you -- I didn't -- someone didn't have to hold it open as we looked in at the baskets. Is that your understanding?

MEMBER SKILLMAN: My experience is that they gravity close.

CHAIRMAN STETKAR: Well, but I don't

know how these things are designed. The description, at least in the text, led me to believe that they literally were something like this. They opened up on a DP. They got to here. And if they got just a little bit, they would do this. And after that point they aren't going to go back closed this way, that they literally had a vertical --

MR. ROSS: These are the upper ones?

CHAIRMAN STETKAR: The upper doors.

MR. ROSS: Yes.

CHAIRMAN STETKAR: The upper plenum doors as opposed to the lower plenum doors, which I think sounded more like a louver-type situation.

MEMBER SKILLMAN: Yes. I don't have experience with that, so I can't --

(Simultaneous speaking.)

CHAIRMAN STETKAR: The text anyway, the way I read it, not knowing anything about the design, said, well, as long as they don't get full opened, then they'll behave -- they won't bind, that they'll behave as a reversible -- they'll re-close on gravity, but there is some chance that if they open fully, they literally flop over like

that.

Hossein?

MR. ESMAILI: Yes, so just to clarify, just going back to the NUREG-5586, the lower plenum door can open very easily, as 46 pascal kind of -- the intermediate and upper plenum doors, they require much higher pressures to be reversible. That means that once it opens. So you need much more pressure, like of the order or 28 kilopascals or so for them to open and just stay open. So most of the time the calculations that we have done, they are opening reversibly.

In addition to that, there are leakages past these intermediate and upper plenum doors that does not always lead to these doors being fully opened and crushed open.

CHAIRMAN STETKAR: But wait a minute, though. Getting back to something that if I have a DP opens like this and if it gets to this point, if it goes like that, it falls open like this. Is that the way that the intermediate and upper plenum doors work?

MR. ESMAILI: I think the Intermediate doors open like that. I think the upper plenum

doors open horizontally, correct?

MR. ROSS: The upper doors are horizontal. They're hinged on one side. And as I recall, we opened them, let go of them and then looked inside at the baskets. But I could be mistaken.

MR. ESMAILI: So those intermediate doors require a lot of pressure to --

(Simultaneous speaking.)

CHAIRMAN STETKAR: I don't care about the pressure, Hossein. I care about if I would go to open this like a door to my house, what does it look like? Because I was reading the description, which is all I know. I've never looked at an ice condenser in my entire life. Reading the description I was led to believe that the lower plenum doors on the bottom, the entrance doors, are designed and configured differently and that, again, reading what I could read, the design is such that once you reach a certain DP, they will open fully and some reason they're designed to deform, like their hinges bind. I don't know. But that the upper, the intermediate and the upper plenum doors aren't designed such that the hinges

bind. On the other hand, they can basically open to the vertical --

MR. ESMAILI: That is correct.

CHAIRMAN STETKAR: -- and gravity go that way.

MR. ESMAILI: That is correct.

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: So they're sitting like this. The two doors are like sitting like this and they open like this.

CHAIRMAN STETKAR: Yes.

MR. ESMAILI: And then they can --

(Simultaneous speaking.)

CHAIRMAN STETKAR: They can flop open. And how did you model the likelihood that they flop open? According to this thing?

MR. ESMAILI: No, we did not.

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: This was not part of the uncertainty distribution because the previous studies showed that it's more sensitive to the lower plenum doors. As I said, the upper plenum doors, in terms of opening, we did that. We did consider both that they open reversibly and

irreversibly.

CHAIRMAN STETKAR: Yes.

MR. ESMAILI: But we did not assign any uncertainty distribution to them informed by the --

CHAIRMAN STETKAR: Okay. What likelihood did you use that they opened irreversibly?

MR. ESMAILI: We were not assigning any likelihood.

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: It was just the DP. For example, for that intermediate door, that they're opening vertically up --

CHAIRMAN STETKAR: Yes.

MR. ESMAILI: -- it required a pressure of 28. Once it passed 28 kilopascals --

CHAIRMAN STETKAR: It was --

(Simultaneous speaking.)

MR. ESMAILI: -- we said all of them are okay.

CHAIRMAN STETKAR: It was then irreversible? Okay.

MR. ESMAILI: It was irreversible position.

CHAIRMAN STETKAR: Okay. Got it. Thank you.

MR. ROSS: Let's see. Aerosol shape factor just real quick. This was something we treated as uncertain taking into account that formed chains or whatnot rather than tight spherical shapes of particle accumulations. And so, this is the distribution we applied. It was bounded at five and it did show to be meaningful. The variance did show to have meaningful influence, but not strong.

We talked some about containment rupture pressure. Let's see if there's a point here we didn't -- so I think maybe we've touched on all these points here.

MEMBER CORRADINI: So if we have, I want to change the subject, but I want to wait until you're done.

MR. ROSS: I'm done.

MEMBER CORRADINI: Okay. All right. I actually am going to have to go, but let me ask about the realizations. I want to ask a general question, because if we get into the details, I'll let John do that. He has all the notes.

My question is if I would remove random -- I'm still back with random trigger, random ignition.

MR. ROSS: Yes.

MEMBER CORRADINI: I'm still bothered by that. So if I remove that from the case study --

MR. ROSS: Yes.

MEMBER CORRADINI: -- would I change any of the observations relative to early hydrogen combustion versus intermittent hydrogen combustion due to hot leg rupture, or lower plenum wall failure, or no combustion and just build up to pressurization?

My interpretation was if I could extract all those dots --

MR. ROSS: Right.

MEMBER CORRADINI: -- I wouldn't change the results.

MR. ROSS: So there would still be early failures. There would still be late failures. With random ignition there's -- all things considered, there's a considerably less early containment failures than late --

MEMBER CORRADINI: There would?

MR. ROSS: -- than with --

(Simultaneous speaking.)

MEMBER CORRADINI: How can I tell that from the results when you have the scatter plot and you have the cumulative plots? You don't have to go there if you -- ah, thank you. In other words, are the red and the --

MS. GHOSH: So, yes, the red curve here -- so this is a cumulative --

MEMBER CORRADINI: The red curve is without?

MS. GHOSH: Yes, red is without. Blue is with. One thing I want to clarify, the single realizations that Kyle is going to talk about later, those all come from the set without random ignition credited.

MEMBER CORRADINI: This is the 338 versus the 171?

MS. GHOSH: Yes, none of those have random ignition credit, because we evaluated the two sets --

(Simultaneous speaking.)

MEMBER CORRADINI: The 338 is the red

dot to the lowest left, and 171 is the --

MS. GHOSH: Yes.

MEMBER CORRADINI: -- biggest red dot to the right?

MS. GHOSH: Exactly. Yes.

MEMBER CORRADINI: Okay.

MS. GHOSH: Right.

MEMBER CORRADINI: So is it because of consequence modeling that I get -- I want to be able to take credit for or observe the difference between the two? Is that timing crucial? That's what I don't know. But I sense that's the reason, because I'm still --

MR. ROSS: Yes, the timing is the metric that's so different between the two.

MEMBER CORRADINI: Okay. And then, so just let me keep on going so I understand the overall behavior. So with the random I get the blue; without the random I get the red. Most of the red is due to the fact that I essentially have early failure of the SRVs, which pump out hydrogen. And I get an early burn regardless of the --

PARTICIPANT: When the hot leg ruptures.

MEMBER CORRADINI: What? I'm sorry. Thank you very much. When the hot leg ruptures regardless of when I -- not arbitrarily, but sample the vessel or the containment strength?

MR. ROSS: Yes.

MEMBER CORRADINI: In other words, for the strength you choose to be; I can't remember what it is, 60 to 100 because you have this range.

MR. ROSS: Right.

MEMBER CORRADINI: I will get this red distribution regardless of that strength? I got the impression there was an intersection of essentially when I get the SRV freezing open and early hydrogen combined with the range of the strength of the containment vessel.

MR. ROSS: Well, the strength of the containment vessel was always involved in both sets of counts, but it wasn't a -- overall it wasn't a make or break.

MEMBER CORRADINI: So I'm being driven by the SRV failures?

MR. ROSS: Yes.

MEMBER CORRADINI: Okay.

MS. GHOSH: Okay. I think this figure

is helpful. This shows the amount of hydrogen that came --

(Simultaneous speaking.)

MR. ROSS: Right, I think this figure is real helpful. It shows -- let's see, it shows --

MS. GHOSH: Yes, on the X axis is the cumulative flow of hydrogen that has come out through the PRT rupture disc. And this is for the set without random ignition. And then on your Y axis you have the containment rupture time. So you can see what range of cumulative hydrogen flow out of the PRT rupture disc can lead to early containment failure. So if you don't have enough hydrogen, you're getting to late containment failure. As you go further out, once you get beyond 400 -- our 400 kilograms, you're getting very likely early containment versus containment failure.

MEMBER CORRADINI: Can you just say that again, please?

MS. GHOSH: The red box is just what we call the early containment failure set --

MEMBER CORRADINI: Okay.

MS. GHOSH: -- which is it's coincident with your hot leg or RPV breach. At the same time you got containment failure you get a hydrogen --

(Simultaneous speaking.)

MEMBER CORRADINI: But those are all driven -- what I'm trying to get a handle on is those were all driven primarily by SRV failure times and opening area?

MS. GHOSH: Yes, because you need enough hydrogen to have gotten out into containment before that first ignition source, which is --

(Simultaneous speaking.)

CHAIRMAN STETKAR: What he's asking is that over the range of 52 to 78, or whatever your range of containment failure pressure is, it doesn't make too much difference whether you sample it at 78 or whether you sample at 52 if you get that early --

MS. GHOSH: It does sometimes, yes.

CHAIRMAN STETKAR: You're going to see it obviously --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- but not --

MS. GHOSH: Yes. Yes, it does

sometimes. And you see that in the single --

(Simultaneous speaking.)

MEMBER CORRADINI: But 338 happens to be the one with a low strength as well as early --

MS. GHOSH: Yes.

MEMBER CORRADINI: -- and large opening?

MS. GHOSH: Right, you need those confluence of things together. In some cases it's big enough to fail a stronger containment within that range. In some cases you need the lower containment rupture pressure to get you there. But it's the -- as far as aligning just right. And those are the key variable.

MEMBER CORRADINI: Okay. And so, I have another one that's out of phase. So I asked it partly because I want to make sure I got it on the record. So the way this containment is designed with the ice baskets melted, there is no water that flows into the reactor cavity where it's dry. All these calculations are essentially estimating that the cavity below the vessel is dry?

MR. ROSS: That's right. Water in the cavity is desirable because you want it available

for recirc. But if it's in there, it's not available.

MEMBER CORRADINI: Okay.

MR. ESMAILI: I mean, sorry, this is Hossein Esmaili. Let me just clarify this. So the cavity is supposed to be dry and there are conditions where the water overflows into the cavity. So we have those cases, too.

MEMBER CORRADINI: Say again, Hossein? I'm sorry.

MR. ESMAILI: There are cases when the containment fails. The ice has melted. A lot of ice has melted and come down to the lower compartment. And there is some swelling of the water when the containment depressurizes. It's just enough to get some water from the lower compartment into the cavity. But cavity is not flooded. It's just about a meter. And then once it becomes in, it just goes away.

MEMBER CORRADINI: Okay.

MR. ESMAILI: So there are conditions where the cavity is not dry.

MEMBER CORRADINI: But for all intents and purposes it's dry? Under a range of

conditions. The reason I'm asking is --

MR. ESMAILI: That's right. You require a lot of --

(Simultaneous speaking.)

MEMBER CORRADINI: -- in terms of what I'm modeling it's driven primarily by early SRV failure times and opening areas and hydrogen distribution. Everything else is of no consequence?

MR. ESMAILI: Correct.

MEMBER CORRADINI: Okay.

MS. GHOSH: So should we keep going?

MEMBER CORRADINI: Yes, I'm done.

MS. GHOSH: So the next thing we were going to talk about is at a very high level I want to go over the results from the Monte Carlo sets that we did. And again, we analyzed two unmitigated short-term station blackout cases for the uncertainty analysis, the case with random ignition and the case without random ignition. And the figures of merit that we were looking at were the same as for Surry. The cesium and iodine release magnitude, the in-vessel hydrogen generation, containment failure time and the time

of initial release. And we used the same methods as we've used before to -- that complement the Monte Carlo simulation.

We used four regression analysis techniques to identify what are the most influential parameters from a statistical perspective. And of course, we always supplement these looks with looking at individual realizations, which Kyle is going to talk about next, to be able to explain from a phenomenological perspective why you get the differences that you see.

And then we used 1D and 2D scatter plots because sometimes it's nice to get a visual picture of how a particular variable or two variables together might be influencing something you care about.

CHAIRMAN STETKAR: Tina, before you get into that, and I didn't get a chance to look far enough ahead, so I don't -- if you're going to address it later -- actually, maybe you are. You didn't talk about why not all 600 realizations went to completion anywhere?

MS. GHOSH: Well, we weren't planning

to talk about it today, but we certainly have that documented in the report.

CHAIRMAN STETKAR: You do --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- but if you're not going to talk about it, I had a question, because on both the short-term and the long-term -- in the short-term it said that out of however many didn't run 24 were attributed to an unexpected failure of the Sandia high-performance cluster. And I don't know what that is.

MS. GHOSH: Power outage, I think. That was a power outage, right?

MR. ROSS: Yes, hard to say. It looks like Doug might have some information on it.

CHAIRMAN STETKAR: It stopped running.

MR. ROSS: It stopped running.

CHAIRMAN STETKAR: Oh, okay.

MR. OSBORN: Yes, the run stopped --

MEMBER CORRADINI: Okay.

MR. OSBORN: -- and sometimes it was a power outage and sometimes it was just the cluster got filled and just died.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: It's a mechanical problem that doesn't have to --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Okay. The other one, the one that I really had a question about was you said the uncertainty algorithm that generated the pressurizer safety valve cycle failure unexpectedly included 31 realizations that were initialized within an immediate safety valve failure, which means that it apparently stuck open before it was demanded. How did that happen?

MR. ROSS: Yes, I --

CHAIRMAN STETKAR: And have you fixed that?

MR. ROSS: Yes, we have and we learned about it and so discounted those.

CHAIRMAN STETKAR: Well, the reason I ask that is, oh, it's really curious behavior, but if it was behaving that way in the 31 that you threw out, why do I have confidence that it was behaving fine in the 400-and-however-many that you kept?

MS. GHOSH: Yes, in the other ones it wasn't initialized in a failed situation. And I

think it had something to do -- I mentioned we had to do some pre-processing in order to create the description of how the valves would behave --

CHAIRMAN STETKAR: Yes.

MS. GHOSH: -- deterministically once we fed it to MELCOR. So all of the distribution description happens up front. We create the input decks. We feed that to the MELCOR uncertainty engine. So it was an issue that we discovered after we did the runs, that there were these 31 cases where it wasn't a zero. I guess somehow there's a zero that was input, so --

CHAIRMAN STETKAR: And you're sure that that phenomenon, however it got in there, would only appear as part of an initialization and that any other initialization values were okay?

MS. GHOSH: So, once we discovered --

(Simultaneous speaking.)

CHAIRMAN STETKAR: That's all I'm asking, because I don't know the mechanics of how these things are done.

MS. GHOSH: Yes, once we discovered that problem, we then went back and looked at what was sampled for those -- or input for those key

values in the other good realizations and they matched our intentions of the distributions we meant to put in. So there was just something odd about those 31.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: And we felt we had enough realizations that we could just --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Now, that's -- I don't care about the body counts. I care mostly about -- that the good body count --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- were you confident that the good body count was good --

MS. GHOSH: Yes, we --

CHAIRMAN STETKAR: -- given the fact that the bad body count was bad?

MS. GHOSH: We checked it over. Multiple people over the course of weeks.

CHAIRMAN STETKAR: Good.

MS. GHOSH: So many so were good with the good --

(Simultaneous speaking.)

CHAIRMAN STETKAR: That was my bigger

concern. Thank you.

MS. GHOSH: Okay. So we just talked about this. Because the timing of containment failure is something that is interesting in this study, we just plot here the cumulative distribution function for what a containment -- the containment failure time appears in both of the sets for with random ignition and without random ignition just to see.

And I think a couple of things to note. Even in the cases where you have random ignition, you can see some blue circles that are very early. It's just that there's fewer of them. But you can still get very early failure even with random ignition credited the way that we modeled it in our runs.

And you still get some early failure discounting -- even if you ignore those initial very early failures, you can see that the blue -- the cumulative distribution function still starts to rise before you get to the over-pressurization failures later. So it's still possible to get early containment failures even if you credit random ignition sources.

And eventually the curves kind of catch up by 72 hours. And in both cases you have a handful, a very small handful of realizations that didn't approach overpressure failure yet by 72 hours.

So you're going to see this figure again later when Kyle shows you what individual realizations he looked at from the whole set, but this is just a depiction of the entire set without random ignition. In terms of where the results lie with respect to the time of containment rupture versus the time of RPV breach, which would be the first known ignition source.

And you can see that many of the early failure cases lie along that line, that dashed line that you see, where basically your first known ignition source led to the hydrogen deflagration. There is a small group that's off that line, which means that that first opportunity didn't fail it, but a later hydrogen deflagration still failed it.

And then all of the blue marks are where you didn't have -- the early challenge didn't you and you fail due to eventual -- more gradual over-pressurization.

CHAIRMAN STETKAR: Tina, when I looked at these plots; I have to be very honest with you, I couldn't figure out what the heck they were trying to tell me.

MS. GHOSH: Okay.

CHAIRMAN STETKAR: Without the text --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- and the text kind of -- if I read the text carefully and I looked back and forth and back and forth and back -- that's, oh, maybe that's what they're trying to tell me. I know you understand these plots like the back of your hand, but I suspect that most -- even engineer types are going to really struggle with them. The text certainly helps --

MS. GHOSH: Okay.

CHAIRMAN STETKAR: -- but they're not at all apparent, or they weren't to me the kind of stories that I think you're trying to tell in them. I don't know how else to present them, because I didn't think about it, but it's --

MS. GHOSH: Do you like this better?

CHAIRMAN STETKAR: Yes, that one is --

(Laughter.)

CHAIRMAN STETKAR: There's an example.

(Laughter.)

CHAIRMAN STETKAR: As I said, the text --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- if I ignored my annoyance with the figures and simply read the text --

MS. GHOSH: Right.

CHAIRMAN STETKAR: -- my notes were it's a good engineering discussion of what's going on --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- which I think it was. This not so much.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: This is not as nearly a helpful visual aid as many of the other visual aids in the report, let me just say that.

MS. GHOSH: Okay. Yes, I think we struggle with how to communicate what we --

CHAIRMAN STETKAR: Yes.

MS. GHOSH: -- think our insights coming out of the work we've done, and one of the

things that's always interesting to me is to not just look at the influence of one variable, but multiple variables together, because it tells more of the story. And our regression techniques handle that synergistic thing, but then it's hard to communicate that other than just saying that we know that these parameters work together. In this one, we artificially drew this green line and --

CHAIRMAN STETKAR: Really?

(Laughter.)

MS. GHOSH: But it's just to kind of -- I guess this was our attempt at an additional visual aid to try to communicate the information. And one of the things we want to point out is there are 93 cases that fit right on the Y axis, which means that you didn't depressurize from the safety valve failure. Your valves kept cycling up to 70-some cycles and eventually you get hot leg failure before they stop cycling. And those always led to late containment failure.

So if you de-pressurized, if your safety valve didn't fail, then you're always going to get weight containment failure.

CHAIRMAN STETKAR: Right.

MS. GHOSH: If your safety valves failed, but failed with a small enough fraction, you also get to weight containment failure. I guess there's not enough hydrogen getting out. And then you get into this range on the right where you can go either way. And there are other variables that come into play to get there. So maybe that's kind of the method --

(Simultaneous speaking.)

CHAIRMAN STETKAR: The text discussion, when I read the story --

MS. GHOSH: Yes. Yes.

CHAIRMAN STETKAR: -- in the text --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- and I thought about things --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- it all hung together. And then after I figured out what the text was telling me, I could go back and look at this thing and say, oh, okay, I guess I understand what this thing is trying to tell me.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: But don't ever cut

back on the text, because without the text one would never understand what this is trying to tell you.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: But that's just --

MS. SANTIAGO: So is this even valuable, because I'm hearing that it's better --

CHAIRMAN STETKAR: It is.

MS. SANTIAGO: -- described than --

CHAIRMAN STETKAR: I think it's valuable only in the sense -- if I stand way back from it and kind of de-focus my eyes, I can see sort of where reddy and bluey things are. Okay? That's kind of useful after I've read the text and thought about what's going on physically.

Because if you go to the previous one that you had up there where you show much stronger clusters, for the life of me, it took a long -- and I'm still sure if I understand the little dashed line across the bottom there. But after I read the text, I understand what it's trying to tell me. And I understand what the blue density of stuff in the vertical kind of box is trying to tell me, but only after I read the text and thought carefully

about that. It certainly wasn't apparent looking at that and then going to the text for further elaboration.

MS. SANTIAGO: Yes, we struggled with this.

CHAIRMAN STETKAR: Yes, got it. So I would say don't get -- Mike, I can say anything about him because he's gone -- so that he didn't appreciate these types of scatter plots either. They were useful for me, but again only after I read the text that told me about, hey, go look and you can kind of -- wait until we get to this later. I want to ask a question about these.

MS. GHOSH: Okay.

CHAIRMAN STETKAR: But only after I read the text and said, hey, look, if you look at the scatter plot, you can sort of see this kind of behavior. Then I said, okay, maybe I can --

(Simultaneous speaking.)

MR. NOURBAKHSH: I think it's useful.

CHAIRMAN STETKAR: Do you? Okay.

MR. NOURBAKHSH: But you got to read the report.

CHAIRMAN STETKAR: You have to read the

report.

(Simultaneous speaking.)

MR. NOURBAKHSH: Some reports you can go and just look at the figures and --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Don't ever -- as I said, if you get -- don't ever cut back on the descriptive text on this and say that it's not like a thermal hydraulic analysis where you can see temperatures and pressures and level and say, oh, okay, I can see how these things are interacting, because --

MR. NOURBAKHSH: This is a picture requires a thousand words.

CHAIRMAN STETKAR: Yes, it is.

(Laughter.)

MS. GHOSH: I like that. Yes.

CHAIRMAN STETKAR: And you got to read the thousand words about six times before you figure out what the picture is telling you.

MS. GHOSH: We'll just make sure all the descriptions are up front and then you turn the page and see the picture.

CHAIRMAN STETKAR: No, honestly --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- that -- I think they are useful, but only after you -- and I wouldn't -- and I thought the descriptions were good. I'm not saying -- I think they were -- some of the better stuff in the whole report, quite honestly is the kind of engineering analysis in that section where you start talking about this stuff. I thought that was really done quite well.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: And others might say, well, they want more; I don't know, but certainly don't make it less.

MS. GHOSH: And Kyle is going to get to that more descriptive stuff in a minute.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: We just wanted to give you again the high-level snapshot of the whole population of results before Kyle digs into the more interesting explanations of why things happen.

So here again no surprises. We know there's a set of early containment failures, so that you see that some of the gray curves -- each gray curve is one realization. They start arising

fairly early. And then there's that gap. And then you have the late over-pressurization failures.

And just a quick reminder that the summary curves on this, they don't necessarily follow the behavior of any actual potential outcome, because they're just the arithmetic, statistical measures at each point in time. And we connect the dots and we --

(Simultaneous speaking.)

CHAIRMAN STETKAR: For the benefit of everybody, those summary curves are the blue, green, black and red that you're talking about.

MS. GHOSH: Right.

CHAIRMAN STETKAR: Each of the whisker things are an individual realization.

MS. GHOSH: Yes, exactly right. One potential outcome.

And this one is for iodine without random ignition. And then this is the same set with random ignition. Things move around a little bit. And I showed earlier the curves. It's hard to juxtapose now the families of curves on top of each other, so we didn't even attempt to do that. But things move around a little bit, but the range

of outcomes you see at 72 hours is roughly the same.

And then for the regression analysis, this time I did put both of them on the same graph. And the variables move around a little bit, but for the most part we can see that the safety valve behavior is important regardless of whether or not you credit the random ignition. The rupture comes up a little bit in the case with random ignition, but it's still there in the case without. And I think when Kyle gets into the discussion more from the phenomenological side, he explains more why it makes sense that these things are important.

And one thing we do want to point out, the safety valve open area fraction and the number of cycles, those are very interrelated. I mean, they're basically describing the same thing that you end up caring about. So while they appear as separate variables, in our minds we always think of those together.

So then here are the cesium release curves. You kind of see the same bifurcation. Since it's a linear scale, the stuff is kind of more squished to the left.

CHAIRMAN STETKAR: Well, it's kind of more squished -- the question that I had, and you can't put them all up here -- and when I looked at the executive summary, if I look at the -- go back a couple -- and I look at the uncertainty in the -- one -- a couple more so you get the -- there. That's probably good enough.

If I look at the uncertainty in the iodine release fractions, either for an early or a late release, the uncertainty is about the same, if I just look at the ranges of the gray. Right? You see outliers here and there, but for realizations.

If I now go to the cesium, but -- well, primarily it's the red line we're looking at here. I'm not looking at the red line. I'm looking at the range of the gray.

PARTICIPANT: Yes, I know. Well, for me it helped to look at the red. I can see --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Good. That helps you, but I'm looking at the range of the gray. And that -- if I look at this, the late cesium releases to me seem to say I have much, much larger uncertainty. Now, you might say, well, the red

line strongly increases at that late point. Why is that?

MS. GHOSH: I think Kyle's going to get into that --

CHAIRMAN STETKAR: Okay.

MS. GHOSH: -- in his discussion of individual realizations. It has to do with the late phase we made for realization in some part.

MR. ROSS: Yes, that's for sure a factor.

CHAIRMAN STETKAR: So it's the late? And it -- okay.

MR. ROSS: And it --

CHAIRMAN STETKAR: Because I don't understand any of the chemistry problem.

MR. ROSS: Yes, I think the chemical form of cesium is also important to keep in mind.

CHAIRMAN STETKAR: Okay. Because this is one thing that struck me when I was going through the executive summary trying to just understand at a very high level what it was trying to tell me. I said, good God almighty, something's happening in the modeling of cesium where my -- not the average amount that's released, but the

uncertainty in that. Why does the behavior in the uncertainty in this plot change dramatically for that late release --

MR. ROSS: Yes.

CHAIRMAN STETKAR: -- when the uncertainty in the iodine doesn't -- essentially doesn't change? I mean, you see a little bit. This is really dramatic.

MEMBER BLEY: It is, but I still -- it would be nice if we had them both up side-by-side. But when I look at them side-by-side --

MS. GHOSH: Yes.

MEMBER BLEY: -- the low end and the middle measures; and we have two middle measures, are a little higher for the iodine than they are for the cesium. The 95th -- did I switch pictures? Yes. No, I did switch pictures. It's the individual trial. Some of them are really high.

CHAIRMAN STETKAR: Yes. Well, but on the other hand, you're picking up a lot more individual trials that are a lot higher for the cesium.

MEMBER BLEY: But when you look at the 95th, that's why I was saying the red line --

MS. GHOSH: You mean to compare with --

(Simultaneous speaking.)

MEMBER BLEY: It's higher until you get right out the end. Then it's about the same. So it's -- 5 percent is going up really high and 95 percent is much more similar. So it's that handful of things that are going really high. But we're going to talk about that, what kind of weird chemistry is going on.

MR. ROSS: Those are real good observations. We'll certainly talk about some of the phenomenon that would be contributing to that.

MEMBER BLEY: We should.

MS. GHOSH: Yes, in fact, I think we're about to get to that, because this is the last slide in terms of the overall results. Again, you kind of see the usual suspects. In this case the dynamic shape factor comes up as really important for the cesium more than for the iodine. But again, it's the safety valve --

(Simultaneous speaking.)

MS. GHOSH: And, yes, so the next part of our talk was going to be Kyle's discussion of the individual realizations.

CHAIRMAN STETKAR: Sorry. We're just muttering among ourselves over here.

MS. GHOSH: Well, I think more will be explained in this discussion, but if at the end there are still questions, we can revisit something.

MR. ROSS: So to best understand the phenomenon important to the metrics --

CHAIRMAN STETKAR: But --

MR. ROSS: Yes.

CHAIRMAN STETKAR: Since we started at 12:40, before we get into the individual realizations, maybe it's okay to take a break rather -- because some of these talk to one another and once we walk into this, it's probably better.

So let's take a break; and I'll be generous this time, until 2:15. We're recessed.

(Whereupon, the above-entitled matter went off the record at 1:57 p.m. and resumed at 2:16 p.m.)

MR. ROSS: To best understand the phenomenon associated with the metrics in the UA, we chose a set of a limited number of MELCOR calculations to look at in depth. The choice of

which calculations to look at involved including outliers with respect to the recognized key phenomenon and metrics.

This is a list of the realizations that we chose. They were all from the set calculations that did not have random ignition sources included. And just to illustrate a couple of the cases and where they were chose, there's a reference case or base case that we looked at in depth. There's a case with earliest containment failure, the case with latest containment failure. We looked at the case with the smallest cesium release, the largest cesium release, et cetera.

This figure is similar to what we were showing before the break. It shows all the realizations, the containment rupture time versus the time of RPV breach, the time of hot leg failure in most cases, as gray triangles. And then the blue triangles are the individual cases of the previous slide. So they seem to fall such that they well represent the spread in the results, we feel.

And just to mention, this dash line, this coincident line would be at 45 degrees if the

left scale and the right scale had the same upper limits. The line just, if a triangle falls in that line, if a calculation falls in that line, it simply means the containment failed at the same time that the RCS ruptured.

CHAIRMAN STETKAR: Turn your mike on.

MR. SHACK: If I sorted out between lower head failures and hot leg failures, would I see a clustering along that line, or are they kind of scattered around?

MR. ROSS: You would see that all these floaters up right and all the cases far right on the coincident line were lower head failure cases, right, where there wasn't a hot leg rupture. Yes.

And just to mention a few real key considerations, I think, in considering the single realization results that follow, it's just that H2 ignition is requisite in these cases on hot leg failure or quarter beyond the floor. One or the other has to be present for an ignition of combustible gasses to occur.

Hydrogen concentration necessary to originate a burn is treated as uncertain but not to propagate a burn. So this is as we were talking

before. We're sampling and treating uncertainty in what is needed to start a burn in a volume, but we're letting MELCOR do what it would always do or normally do on some, as far as propagating a burn from where it starts to elsewhere in containment.

With respect to pressurizer SV function, again, we mention it a lot, but what happens to the three parallel valves as a system is paramount to what happens to any one of the valves individually.

Select realization results and indications. Principally important to containment failure timing is the amount of hydrogen that's vented by the RCS to the containment prior to the RPV hot leg or lower head failing. So this is the amount of hydrogen that's produced in core and then that is actually vented to the containment such as it's ready to burn when you first get a spark. Clearly, the more hydrogen you have, the more damage potential the burn has, as well.

Deterministic to the amount of hydrogen that is vented to containment is whether a pressurizer safety valve failure to close occurs and then, given that it occurs, the position that

the valve takes on from that point on. Safety valve failure to close was found to be necessary for containment failure to happen early. If the three valve system of safety relief on the pressurizer accomplished approximately 72 cycles, then that was always associated with, well, that was indicative of no safety valves having failed.

And then we find that the substantial failure to close open area was necessary for containment failure to come early. A valve had to fail significantly open for an early containment failure to occur.

If a pressurizer safety valve did not suffer a failure to close, if the system of valves functioned to relieve pressure, per design, then hot leg nozzle failure occurred at 3.7 hours. It always did that to those two significant figures. That was always when the hot leg failed.

Early containment failures, well, by definition, came immediate to H2 burns, meaning that in our definition of early, containment failure and breach of the RCS were at the, the failure was right after the breach, so they were very, very coincidental or occurring at very nearly

the same time.

High "dry" hydrogen buildup in the containment dome seemed to be a persistent precursor to containment failure, occurring immediate to an H2 burn, meaning when you look at a case where a hydrogen burn did fail containment, you would see that much of the energy in that overall burn was contributed from what happened, from what burnt in the containment dome. And the environment of the dome is always dry because the steam that's working its way through the, working its way to the dome has to move through the ice chest, and so it doesn't come out the other side.

So this next point, coincidentally, the uncertainty in containment pressure failure and the variability in the pressure resulting from H2 burns overlap. If you look at the horsetails of containment pressure and you see that you have spikes due to burns, some of them climb up above the threshold that fails containment. Some of them get close, which we just thought was interesting. I mean, I might have thought that enough hydrogen would exist to break containment two or three times over, but, in actuality, there were many cases

where, depending on what you were from the best-estimate containment pressure, plus or minus a little bit, you did or did not get a failure, kind of implying or pointing out that containment, well, they clearly weren't designed to withstand hydrogen burns. They almost are strong enough.

So when looking at the late containment failures in the subset of individual cases that we looked at real closely, we noticed that they were associated more often with early hot leg or lower head failure, they continually experienced insufficient hydrogen, excessive steam, a lack of sufficient oxygen, or a missing ignition source, such that deflagrations large enough to fail containment never occurred. Eventually, late containment failures resulted from monotonic pressurization driven by fission product decay primarily through steam generation but also non-condensable gas generation from core-concrete interaction.

Greater deposition of fission products in the pressurizer relief tank resulted as a consequence of early safety valve failure to close. And this was not a sustainable situation in the

calculations because of the decay power associated with those deposits.

So this figure and the next are meant to show the strong influence of how much of hydrogen produced in vessel is vented from the vessel to the containment before the first ignition source presents itself. In the blue cases, this realization released very much of what was produced to containment before the hot leg rupture. In the red case, there was less total production overall than the blue case but not a whole lot less, but dramatically different is the solid red line compared to the solid blue line. The red case just did not release the kind of hydrogen to containment. So when the hot leg failed and a spark was first available to light a burn, the burn potential in the red case was much less than the burn potential in the blue case.

And so the next two figures show the pressure response of containment for that blue and red case. You'll see the energy of the burn is represented on the left-hand access, and the containment pressure scale is on the right. You can see a hot leg rupture labeled on the time axis

on the X axis. When the containment failed, there was a very large burn, the energy contribution mostly from the burning in the containment dome. The result was a dramatic, very quick pressurization to above the containment failure pressure for this case and the steel shell gave way.

In the next slide, so this is the red case. And, again, there was a burn at hot leg rupture, but the magnitude of the burn was greatly less. The contribution from the dome was none, actually; so the burn must have originated in lower containment and didn't successfully promulgate to the dome. And the result was that containment felt the burn but nowhere near enough to suffer a failure from it.

So this is a complicated figure. We struggled with --

CHAIRMAN STETKAR: This was another set that's --

MR. ROSS: Yes, I understand.

CHAIRMAN STETKAR: -- kind of hard to follow. Not quite as the scatter blocks but . . .

MR. ROSS: Let me try to describe a

little about what's in here. What we're trying to do is, on one page, show what was going on with the different influences that were determined whether you could have a burn or not. So there was inerting gases, oxygen available, the fuel available. So there's traces for each of those.

If a trace on here is solid, that means, at that point in time, it was supportive of a burn. If it's broken, if it's dashed, then at that time that particular component of what's needed to have a burn wasn't supported. So you see a blue line for inerting gasses, you see a green line for sufficient oxygen, red line for sufficient fuel. So at any time on this plot, if all three colors are solid then the state in this volume is supportive of a burn, and if you get a spark it will have a burn. If any of the lines at that time is dashed, then that component is prohibitive of a burn at that time.

We've also shown in --

MR. SHACK: Is steam an inhibiting gas?

MR. ROSS: Yes. Also shown here in the gold is whether there was an ignition source present at whatever time in this volume. And you

can see that up until hot leg rupture, there was no existing source. At hot leg rupture, there was momentarily. You can see that it jumped up to one just momentarily, so that was enough. But immediately after that, some change in that plume coming from the hot leg, either it cooled off to a temperature that was below the ignition temperature of hydrogen or the velocity in that plume dropped off below that threshold that we watched to see if there was a significant flow or not, and that source of ignition didn't redevelop until sometime later.

So this was our attempt to try to capture all that information on one page, and I could see how it could be confusing.

CHAIRMAN STETKAR: The stories about it are really good if you read the text.

MR. ROSS: So the involvement of the pressurizer relief tank wound up to be very interesting and unexpected. We did see the same type of phenomenon occur in Surry, but it seemed more obvious in the Sequoyah analyses. So just to quickly -- we've probably been through most, if not all, of this, but the steam is relieved from the

SRS by the pressurizer safety valves which vent the steam to the pressurizer relief tank. Initially, steam condenses in the water pool, as per design. That's always held by the tank.

Given core damage, fission products are carried by the safety valve relief flows to the tank where they rather efficiently deposit in the water pool if there is one, if it hasn't evaporated.

There are spargers in the tank that introduce the flows into the tank and disperse them well, designed with the intent of the collapsing the steam that flows into the tank. And that is also an effective structural setup for good scrubbing of the fission products from the bubbles in the pool. So they do efficiently become captured in the pool.

In the station blackout, the water in the PRT eventually saturates and the tank pressurizers to rupture the burst disk, at which point the water evaporates and dry fission product deposits result in the tank. The dry deposits, with their incessant decay power, are susceptible to re-vaporization and further transport. The

degree of involvement of the PRT strongly dependent on the safety valve failure to close timing.

So the next couple of figures illustrate two calculations, one where the PRT was the deposition and the re-vaporization of fission products was a large player and one where it was not a large player with respect to release to the environment. These two plots show cesium. So the fractional amount of cesium inventory that was originally in the core is tracked by the gold line, the gold dash line on this plot. You can see the early deposition in the tank is quite significant, 40 percent of the original inventory.

So as we move out in time 12 hours or so, you can see that gold line is trailing off. That's indicative of re-vaporization of material from the tank at 50 some odd hours where there's the follow-off of that curve. That is a containment failure, and the containment failure drops very rapidly and that aggravates the re-vaporization of fission products from the tank.

You can see the purple, pinkish-purple line there. It's very closely a mirror image of the gold line, meaning that as that material is

coming out of the tank it's being carried efficiently to the hole in the containment shell and out the penetrations or the failure points in the shield wall into the environment.

So in the realization represented in this figure, the PRT doesn't become involved. It doesn't have the kind of initial deposition that the other calculation had. And what deposition there is doesn't seem to be susceptible to re-vaporizing. The gold line initiates and ends at the same magnitude.

So it there any phenomena we touched on earlier that we were hoping to talk about more in this presentation but haven't? Is there . . .

CHAIRMAN STETKAR: I don't know because this is well beyond my area of expertise. I guess I still don't -- I'm trying not to speak very loudly because we're getting the interference. Why in 142, that case you have there, that's a case where the pressurizer safety valves open, so everything, essentially, everything that's generated in the core goes out into the PRT.

MR. ROSS: Right.

CHAIRMAN STETKAR: What I don't

understand is why, apparently, so much is held up in the PRT, given the fact that the rupture disk on the PRT opens at about two hours, so now I have a pot with a big hole in it.

MR. ROSS: We do indeed, so we have some shortcomings in the modeling here. So the actual situation could possibly develop, the flow that's coming in and out of the hole, and we didn't support that possibility in the --

CHAIRMAN STETKAR: Yes, okay, okay. Well, and that's why, that's why I don't know what you can do about it, but, you know, drawing these conclusions about look how important the PRT is, I don't know if that's an artifact of the model compared to the real world.

MR. ROSS: Right. So this was kind of a situation, too, where we didn't perceive the large importance of this phenomenon. But we could address that counter-current flow possibility through the burst disk opening. And we also modeled this tank as perfectly insulated, and that, too, is not -- and then the point that Dr. Corradini made about possibly the flood level coming up to the tank is something that we should

certainly look at, too, and we hadn't.

MR. ESMAILI: But the fact is that, because of the sparger, a lot of this gets scrubbed and it's residing in the pool.

CHAIRMAN STETKAR: You know, you keep talking about this as if it's a suppression pool. It's a tank --

MR. ESMAILI: It's a tank --

CHAIRMAN STETKAR: -- that's not much bigger than out there. So, yes, for a while, until all of that water boils away, that's true. But that's not very long. It's not a beat-up suppression pool, so, yes, for a short period of time, you do get scrubbing, but not over the time frames that we're talking about here.

Is this the place -- because, again, I know nothing about the chemistry -- that we hear a little bit more about why the changes in the cesium-release fraction are different late versus early? The uncertainty in the cesium-release fraction, is that part of this?

MR. ROSS: That's a really good observation, but I don't think we've delved into that.

CHAIRMAN STETKAR: Okay, okay, okay. Thanks. Okay.

MR. ESMAILI: So I'm just going to talk briefly about the long-term station blackout. We did not do an uncertainty analysis on the long-term station blackout. This was a decision we made at the beginning. We were just focusing more on the short-term station blackout. And as you can see, we gained a lot of insights, you know, in terms of long-term station blackout that we have already received from the short-term station blackout.

So I have no intention of going through this table. It's just to show you that, for the most part, the sequence is very, very similar to what was assumed in the Surry short-term station, long-term station blackout, that, you know, once power is lost, main feedwater is lost, the turbine-driven aux feedwater immediately, you know, starts at one hour. There's cool down of the RCS at a rate of about 100 Fahrenheit per hour. This cool down, at some point, leads to enough lowering of the pressure in the primary system to actually start injection.

A little bit later, we have to stop this steam

generator cool down because we want to maintain some steam to run the turbine.

We also need eight hours of battery, and we are doing a sensitivity to the duration of the battery later on, so I will show you. But once the battery is exhausted, the atmospheric relief valves are going to re-close and the system is going to re-pressurize back again. At that time, once battery is gone, you know -- the aux feedwater system is very, very similar to the system in a BWR. It was just like what Fukushima happened.

So once the power is gone, the governor valve that's sitting upstream of the injection point, it opens fully. And once that happens, the turbine gets flooded.

CHAIRMAN STETKAR: Do we know that to be the case for Sequoyah, or do we just assume that to be the case?

MR. ESMAILI: We know that to be the case. We looked at it. It's a turbine, it's actually --

CHAIRMAN STETKAR: I've seen them designed both ways, where they fail close and they fail open. That's why I was asking do we know that

to be the case at Sequoyah.

MR. ESMAILI: Yes, at Sequoyah, they close open, they fail open. And then that leads to filling up the steam generator, and then the turbine gets, the turbine gets flooded, and we lose the aux feedwater about ten hours.

After that, we are losing -- the steam generators are going to cycle, you know, and we are assuming that they're using the best estimate values from the short-term station blackout, you know. So we have 45 cycles before the steam generators get stuck, and I will show it a little bit later in the next picture.

However, I want to point out is that, you know, even though we have eight hours battery or ten hours, it takes a long time for the core degradation to occur. We have, you know, until about 20 hours before the water level reaches below the top of active fuel. And then it pressurizes, and we have two hours before the first fission product gets released from the fuel. About two hours later, because of the hot gasses going through this, we receive a rupture of the hot leg nozzle, and that leads to a deflagration and the

failure of the containment.

Lower head occurs about four hours later, and the ice, it's estimated to be melted in about, at about 34 hours. So, you know, so we have melting of the ice water about 12 or 14 hours.

Next slide. So this is the response of the primary and the secondary systems. So you see the blue is the RCS pressure, so, immediately after scram, see the heat is being removed by the secondary side, the secondary side relief of cycling that leads to somewhat cooling of the RCS. At one hour, the red line shows that you're opening the atmospheric relief valve on one of the steam generator. That cools down, actually that cools down not only the RCS but the other combined three steam generators.

And at about three and a half hours, we stop the steam because we want to keep steam to run the aux feedwater, so we stop the cool-down. And at about eight hours, the DC fails, and then aux feedwater goes to full flow and the valves close. At that point, you see the whole system is re-pressurizing back to the SRV set points when there's steam generators. And at that point, as

you can see, the insert, what they show is that the water level in one of the steam generators goes full up because --

CHAIRMAN STETKAR: That's what I was going to ask you. Why does that happen?

MR. ESMAILI: What happens?

CHAIRMAN STETKAR: What you just said. The water level in the thing I'm calling steam generator one, not the lumped other steam generators --

MR. ESMAILI: Right.

CHAIRMAN STETKAR: Why does that water level go up and the other ones --

MR. ESMAILI: We are just assuming that we are feeding one steam generator.

CHAIRMAN STETKAR: I'm sorry. Why are you -- huh?

MR. ESMAILI: We are feeding only the one steam generator here.

CHAIRMAN STETKAR: I'm sorry. Up until that point, you were feeding all four, weren't you? The levels were all the same. Why are you only feeding that one?

MR. ESMAILI: This was the assumption

we made because of, you know, we have --

CHAIRMAN STETKAR: Well, wait a minute. That assumption is important because I'm later going to stick open that safety relief valve because I got water relief. So I want to know, in physics, why that one is behaving differently from the other three. Forget about modeling. In physics.

MR. ESMAILI: If you only feed one steam generator, this is what happens.

CHAIRMAN STETKAR: Yes, if I only feed one. But I want to know, in the real plant, why am I only feeding that one steam generator?

MR. ESMAILI: In a real plant, you need at least to feed two of the steam generators.

MEMBER BLEY: But the underlying question is we've kind of picked the worst case for open a safety valve. Is that why we're doing this?

CHAIRMAN STETKAR: I don't know why we're doing it because if you look at the plots up until eight hours when the turbine-driven aux feedwater pump goes, whew, big flow, all four steam generators have the same level because the operators are controlling the level. The operators

take manual control, and they control the level, which is good. That's what I would do. And now we say the turbine-driven aux feedwater pump takes off and steam generator number one, which is the one that I'm modeling separately, suddenly sees a whole hell of a lot of water going into it, the other ones apparently don't.

Now, the atmospheric relief valve is closed, so it's not like, you know, it's an open pot. Why does a little behave that way?

MR. ESMAILI: That's a good question. We can go back and look at it. This is the assumption that we made that we are just injecting, at that point we are injecting into one of the steam generators because we wanted to see what happens because we are not modeling all these tree loops separately. So we wanted to see what happens into one group versus the other combined loops. And so, you know, so that's an assumption in our calculations.

CHAIRMAN STETKAR: I was trying to, honestly trying to think out physically what would happen when turbine-driven aux feedwater pump took off with the atmospheric relief valves went closed.

Now, up until that point, maybe that was the only one that was open because it was trying to cool down. But they go closed.

MR. ESMAILI: Right.

CHAIRMAN STETKAR: So I'm going to start to pressurize what? Why would I feed that one preferentially? Maybe initially for a little bit but . . .

MR. ESMAILI: If you put water in all of the steam generators, all of the steam generators' water levels would go, and then it would change the behavior --

CHAIRMAN STETKAR: Yes.

MR. ESMAILI: -- of the SRVs. And as you can see here, that's what we were trying to show is that, with one of the steam generators full of water, we are not getting to that 45 cycles.

CHAIRMAN STETKAR: Yes.

MR. ESMAILI: The other three steam generators, you get to the 45 cycles because you are, you are relieving steam. In the one steam generator we are filling up with water, we are actually failing it based on water passage.

CHAIRMAN STETKAR: I know.

MR. ESMAILI: Okay. So that is the, that is, you know, that is the main difference that we wanted to show.

CHAIRMAN STETKAR: Okay.

MEMBER BLEY: Our analysis has turned from a scenario into a sensitivity case it sounds like.

MR. ESMAILI: These are, all the long-term station blackouts are, yes, sensitivity cases.

Okay. So I have not finished. So at about, after the SRVs start to open, you lose water in all the three steam generators. And then the second steam generator, SRVs get stuck open. And then by about after 18 hours, the steam generator gets dried out, and then the behavior looks like what you see in the short-term station blackout and then goes to the pressurizer SRV, gets stuck open, and then we have hot leg failure after that.

So here you see the effect of RCS cool-down. It lifts the shrinkage of the water inside the primary system. The top of the -- the core is still full of water, and you see some fluctuations in terms of water level that is responding to what happens on the primary pressure. And you have some

accumulator injection, as I said before. But most of the accumulator injection occurs after 24 hours when the hot leg fails.

So on the right-hand side, I'm just showing you what happens in terms of core degradation. The first one at 22.4 hours, even by the time the water level reaches the bottom of active fuel, the core is intact. It's highly oxidized, very hot, but it's still intact. It doesn't take much longer until at least the inner rings collapse and form a debridement on top of the core plate. And just a little bit later, the entire core has collapsed and on top of the core support plate.

At that time, we have a little bit later, about half an hour later, we have the hot leg failure that injects water. And as you can see, water is injecting at about 24.7 hour. And at that point, it leads to some cooling of the debris and the lower head failure happened, which is the last picture, in about four hours later.

Next slide. So this is what happens inside the containment. You know, the heat is being removed by the secondary side, so you see

that the containment is not pressurized. I mean, it's about 2 - 3 psig up until the time of the pressurizer relief tank failing.

During this time, there is some natural circulation going to the ice pit. By the time that the pressurizer relief tank fails, we actually have about four percent of the ice that has melted inside the ice pit. At that point, you know, we get some pressurization inside the containment, and a little bit later, when hydrogen starts coming off, you see another one.

But the important thing is that you see that, up until the time that the hot leg fails and the first hydrogen deflagration fails the containment, the pressure remains below design. At that point, the containment fails, as it's above the best estimate fragility.

By the time that the hot leg fails and the first hydrogen deflagration occurs, during that time about 20 percent of the ice in the ice pit has melted.

Next slide. So this is another way of what Kyle was talking about. I'm showing you the distribution of cesium and iodine. And what you

see in the green curve is that most of the cesium and iodine have been released from the fuel. Some of it is still inside the vessel. That includes, the green curve includes also the PRT that we're talking about.

So this is at about 24 hours, when the hot leg fails and the pressurizer SRV gets stuck open, some of these fission products get transported to the containment. But you can see there's a substantial amount that's still residing inside the vessel, about, you know, 70 percent of the iodine. It's not until about 40 hours, 42 hours, or so that you see a sharp drop in iodine distribution. That is when the pressurizer relief tank starts releasing all of its iodine back into the containment. And as a matter of fact, you see the blue line, which is the total containment deposited, goes up and the same for the total airborne containment, which is the red line, goes up. And this is what is being carried out to the environment because, by that time, the containment has failed.

What you see on the right-hand side is the cesium. The cesium is a little bit less

because it's driven by the cesium molybdate, which is less volatile than cesium iodide.

Next slide. So these are some of the sensitivities that we did. We were looking at, you know, the question of SRV. We were talking about SRV, so we did a whole bunch of calculations looking at cycling of the SRVs and the station battery's duration.

For the most part, the behavior is the same. The black curve is interesting because this is what Kyle was talking about that we assume in this case. The pressurizer SRV is assumed to be open at the first cycle, so, as soon as it opens, it depressurizes the vessel. But in all the cases that we see here, we are seeing early failure of the containment. There's enough hydrogen that is being produced in vessel that gets released into the containment, resulting in early failure of the containment. The pink curve is because you have four hours of battery, so everything is accelerated.

Next slide. So this is the superimposition of what we see from the long-term station blackout with the short-term station

blackout. So you've seen these figures of the short-term station blackout before. The color curves are the release profiles for the long-term station blackout. And as I said before, for the most part, they are very, very similar. And, qualitatively, they're also behaving similarly to the short-term station blackout. You see there there is a release at the time of containment failure, but, because containment failure has occurred very, very early, the release is limited. And then you have a period where the release is driven by re-vaporization, so it goes up and just kind of going up a little bit, and then, suddenly, you see a bigger increase in the release. And that you see both in short-term station blackout and the long-term station blackout, and that is what is driven by the pressurizer relief tank when it's re-vaporizing.

So in terms of -- so what we did, we did sensitivity to the case where we assumed the igniters are available. We didn't take into account, you know, what time we are assuming that the igniters are available just to show, if you have igniters available early, what happens.

So the blue curve is the short-term station blackout that Kyle was talking about. This is realization 338 that had the earliest containment failure, so that containment fails at about before four hours.

Now, if I activate the igniters, what you see is that, and assuming that the igniters are available, everything is available, what you see is you start burning the hydrogen at lower concentration just before three hours. So you get these small pressure spikes that go on, and the main impact of the igniters in the short-term station blackout is to avert this early containment failure. So igniters are very, very effective in terms of averting containment failure.

The next slide is -- okay. So this is the effect of igniters for the long-term station blackout. So without the igniters, we have containment failure and around the time that the hot leg fails. So in this case, if igniters become available, we can see in the red and the green lines that igniters are controlling the hydrogen, they're burning the hydrogen. So you get these small spikes. And after about 24 hours, the

pressure goes down because there is some, there's no hydrogen combustion until the lower head fails.

After the lower head fails, you see consistently that the pressure inside the containment is going up, and this is because of addition of non-condensables and the steam because of core-concrete interaction. And the pressure spikes that you see after lower head failure are not only due to hydrogen but a combination of hydrogen and CO. So you get a little bit bigger spikes after that time.

After about 36 hours, and that's what I'm trying to show you in the inserts, is that you have basically reduced the amount of oxygen inside the containment. So even though igniters are available, you are not actually burning anymore hydrogen. Hydrogen is going up, CO is going up, but you cannot burn them because you do not have enough oxygen.

And at that time, this is what we were talking about that the pressure inside the containment is monotonically increasing. In this particular case, by 72 hours, we have not reached

the lower bound of the containment pressure, so the containment does not fail in 72 hours.

You know, if you do enough sensitivity calculations and the combinations of what we saw from the short-term station blackout, it is possible to have a combination of parameters that leads to late containment failure. But that late containment failure is much, is closer to 72 hours and in most cases past 72 hours.

Okay. The next slide.

MEMBER BALLINGER: I guess it might be a naive question, but if you fail the containment when it's full of hydrogen and CO, there's oxygen outside the containment. What happens then?

MR. ESMAILI: You see a little bit of hydrogen burning inside the containment.

MEMBER BALLINGER: No, I mean out at 72 hours or whatever happens. If you fail the containment, you're going into the space between the containment and the shield building. That's got to have oxygen in it.

MR. ESMAILI: Right. That's what I'm saying. The blue curve is the one that the containment has failed.

MEMBER BALLINGER: Oh, I see. Okay.

MR. ESMAILI: The blue is where containment has failed. You see some pressure spikes, but, over time, because everything is being pushed out, it's just being dominated by all the non-condensable gasses. And still, as you can see here, the amount of oxygen inside the containment, so everything is being just pushed out of the containment.

Yes, it's possible, you know, you can have, if you have a source outside in the shield building. But you're assuming that as soon as the containment shell fails the shield building fails. It does not have enough capacity, so the failure of the containment also leads to the failure of the shield building almost instantaneously.

Okay. So this is a summary of all the insights that we obtained from MELCOR analysis that we discussed this already, that the containment over-pressure failure could be early and late, depending on whether you are getting the hydrogen burn, you know, at the time of vessel RCS breach, or, if that pressure is not enough to frame the containment because of a number of reasons, it gets

to an increase, in a monotonic increase, which is driven by non-condensable gas generation as a result of core-concrete interaction and steam production and heating up the containment atmosphere.

So the way this happens is that this timing, whether it's early or late, this verification depends on multiple phenomena. Kyle talked about some of the phenomena regarding how much hydrogen is being vented from the RCS prior to an ignition source becomes available. But there are other phenomena that are also playing.

The early containment failure depends on mechanisms that distribute hydrogen. So, you know, the only pathway for the hydrogen to get into the containment and actually lead to a hydrogen combustion is through the SRV. So that's why the SRVs are playing an important role.

The major impact of modeling of the random sources of the ignition is what you saw earlier that Tina was showing you, the accumulative probabilities between the red curve and the blue curve, is that if you credit random ignition, the way we have done it here, it leads to a delay in

terms of, overall delay in terms of containment failure. But you still get the early containment failure even with the cases that we are crediting random ignition.

Second point is that the magnitude of the source term was impacted by the parameters that affect the timing of the containment failure and the fission product re-vaporization. And, again, this goes back to the importance of the safety valves and how they distribute and how they affect the containment failure.

Next slide. The results of the UA showed that some of the other parameters that we considered, like the aerosol shape factor, the flammability limits, and the response of the lower ice chest door and the bypass did not affect the source term. The long-term station blackouts in the sensitivity calculations, almost all of them had early containment failure at hot leg failure due to hydrogen burn. These are the sensitivity cases that we considered. Again, as I said, if we vary enough parameters, we're going to come up with a combination that do not lead to early containment failure. And, qualitatively, the release signature

for the long-term station blackout, the one that was superimposed, shows very, it's very similar to the short-term station blackout failure cases.

We did sensitivity when we are crediting the igniters, and the igniters can avert early containment failure. The important thing is that, you know, in some of the reference cases that we saw, it leads to the failure of the containment. But for the cases that the containment does not fail, this ice is a passive system. It does melt over a period of time, you know, about 12 hours or so, and it does play into slowing the containment pressurization. And, overall, it can delay potential containment failure for the cases that do not lead into early failure.

I guess that's all I have.

CHAIRMAN STETKAR: Before we switch gears into MACCS, any other questions on MELCOR containment modeling, that sort of stuff? Go forth.

By the way, I'm trying to be cognizant a little bit of the time. Does anybody have any real time constraints at five? Okay. I don't want to go on too much longer, but if there were real

time constraints I'd make sure that we get things done.

MR. THOMPSON: Hello. My name is Dr. Shannon Thompson. Next slide, please. Today, I'm going to talk about the offsite consequence analysis which was done using MACCS.

MELCOR generates environmental releases, as we've seen in several of the previous slides. MACCS models the atmospheric transport dispersion and deposition of the releases, protective actions taken in the emergency and long-term phases, exposure pathways, doses, and health effects. The individual early fatality and latent cancer fatality risk results are the mean values over more than 1,000 weather trials.

The MACCS model was developed using site-specific information for Sequoyah, including weather data, land cover information, population, and local economic data. For this study, we assumed the road network has been affected negatively by the earthquake, and we analyzed selective deterministic scenarios and uncertainty analysis of the unmitigated STSBO.

Next slide. Sequoyah has a very

consistent river valley wind direction pattern in the near field. The north-northeast to the south-southwest directions shown on that figure at the right also mimic that 91 meters. We reviewed the site meteorological tower data from 2008 through 2012 and selected 2012 as the best representative year for weather data. MACCS has a requirement for one full year of weather data.

2012 was selected because it had a very high data recovery rate and also because the precipitation was the closest to the mean for the five-year span. This quick weather data compare very well with longer-term data from Chattanooga at a national weather station site, with one exception. Sequoyah is a little bit dryer than the Chattanooga --

MEMBER BLEY: I've got a question. This wind blow is kind of up and down the Tennessee weather. It meanders a little bit. The EPZ is out to 10 miles. At about 15 miles, we're getting into the suburbs of Chattanooga, 20 to 25 miles we've picked up most of Chattanooga. Does the model leave the EPZ meander along the river, or does it assume the winds are going straight, regardless of

how the river is bending?

MR. THOMPSON: I think the model is a straight line gassing plume segment, so the weather sampling encompasses releases going at a particular direction, for example north-northeast, at the first hour of release. As that plume would keep going in that direction, but, as the wind shifted, the model allows for the wind moving different directions at different times.

MEMBER BLEY: But that's all based on this wind right at the site. It doesn't worry that ten miles away --

MR. THOMPSON: Well --

MEMBER BLEY: I'm just wondering how you pick up the population at Chattanooga properly.

MR. THOMPSON: Right. This is the site-centric wind rows, and, while I would say, you know, it governs it if we were to sample many, many hours, the weather sampling regime includes, it's a semi-representative because binning includes wind speeds and directions and stability classes that would encompass a number of different directions. So --

MEMBER BLEY: But all based on what's

going on right at the site? It doesn't have a river valley model in it or a channel --

MR. THOMPSON: Right. It does not.

MEMBER BLEY: -- which is, at least it would seem to me, most likely what happens if it's head off down the river channel.

MR. THOMPSON: Right. And it's also going to, you know, have some buoyancy and go up and then be affected by the vertical wind fields --

MEMBER BLEY: That's true.

MR. THOMPSON: -- you know, above. So did that answer your question?

MEMBER BLEY: Kind of. We'll come back to this a little later. If it comes up like three slides from now, I'll have a lot more questions about this. CHAIRMAN STETKAR: Shannon, before you leave meteorological data, I know you selected 2012. That's an average year, and you got decent return on your hourly data. The thing that struck me is that in 2012 the precipitation was a little over 39 inches. In 2010, it was like 25.5, and in 2011 it was like 53 inches. And if you look at the -- you have nice plots in there about monthly precipitation.

There's a huge variability in precipitation, not so much in the 2012 which is kind of a nice vanilla year.

Now, I understand that the model likes to have hourly data, but you do sample from bins. Wouldn't it be better for an uncertainty analysis to capture more than 8,760 individual hours, throw those into bins from 1.0 years, wouldn't it better to capture five years of data, 8,760 hours times 0.97 or whatever, and throw those into the same bins that you sample with, so you show a much larger variability?

MR. THOMPSON: The quick answer to that is, yes, it would be better if it had more data to sample.

CHAIRMAN STETKAR: Well, you do.

MR. THOMPSON: But that's a model limitation at this point. I think --

CHAIRMAN STETKAR: No, I'm sorry. You still sample from bins, though, right?

MR. THOMPSON: That's correct.

CHAIRMAN STETKAR: You can still bin them. MR. OSBORN: Hi, this is Doug Osborn. There have been analysis done in the past,

you know, obviously not in a SOARCA, that have looked at sampling from five years' worth of data. And, again, since we're reporting our results as mean weather, if you were to take, for example, all of these and apply a mean average, you're still going to wind up essentially having very similar types of results. Now, they may change, but, overall, the trends and everything that we're going to see for latent and early fatality risks are essentially going to be the same.

CHAIRMAN STETKAR: Okay, thank you.

MR. THOMPSON: Next slide, please. This slide is just to really give an overview of some of the population information and to define the cohorts. The table at the left shows the cumulative population and population density at various distances from the plant. And for example, at 15 and 20 miles, you can see that the population density goes up, which would incorporate the population of Chattanooga. And then at about 50 miles, the population density goes back down, reflecting the regional population in the area.

The table at the right lists the cohorts model. There are eight cohorts in this

Sequoyah SOARCA study, and these include schools; special facilities; transit-dependent evacuees; and the general public; which includes the early, middle, and tail fractions; shadow evacuees from 10 to 15 miles; and the non-evacuating public. For zero to ten miles, non-evacuating public is just a 0.005 fraction, and the 10 to 15-mile non-evacuating public is 80 percent. And beyond 15 miles, everybody is considered non-evacuating.

CHAIRMAN STETKAR: Why in the SOARCA study, because you didn't do this in Surry or Peach Bottom, why do we now have the cohort 4, which is the early evacuees, when we've never had these before?

MR. THOMPSON: That's a good question. I noted that you mentioned that in the earlier conversation.

CHAIRMAN STETKAR: Could hear more about it but . . .

MR. THOMPSON: There are a couple of different cohorts here than were presented in the previous SOARCA studies, and one of the things is, for the general public, that is cohorts 4, 5, and 6, we had evacuation time loading curves, traffic

loading curves, that the 2013 Sequoyah ETE report broke down into 50-percent loaded, 90-percent loaded, and 100-percent loaded. So it was handy information. They gave, basically, a band that would say, for example, the early cohort would depart between 15 to 65 minutes, so we chose the midpoint of that time, 40 minutes and rounded up to the nearest quarter-hour interval. A couple of slides from now, we'll have the time line, and those are broken into 15-minute increments.

MEMBER BLEY: Yes, I don't know where to ask. I've got a whole bunch of questions on this chapter. I'm going to ask a few of them now and then save a few for later. Since cohort 4 was brought up, I have a couple of things. I find, as I read it, I think I see inconsistency. One inconsistency with cohort 4, it appears to me, is that you set these guys off as soon as they hear the first siren. They're getting ready to go.

The first thing that seems inconsistent is when you define the cohort you say, well, part of the reason we turn it loose is because they've been all shaken up by the earthquake and that probably means they won't get many of the messages

that are sent out, so they just go on the siren. They know there's been a big earthquake, so they go.

A little bit later, you talk about keyhole evacuation, which is what you do. But you've already got 40 percent, almost half of the people, heading out, so they aren't going to hear anything about keyhole evacuation. They're all going to be on the road.

Another thing that bothers me, the model assumes all the bridges inside the ten miles are gone. If you live around there, and I've driven around there a whole lot, the main ways you know in and out, even if you live there, aren't working. But they aren't going to know that, so I'm going to go driving down the road until I find a bridge that's out. I'm going to have to turn around by now all the other half of the people inside this area that are taking off or piling up behind me. By the time you get emergency responders out there, we've already got one doozy of a traffic jam, and here we're assuming this early cohort is moving fastest of all because the roads are clear and they're getting out, but

they're running into all these dead ends and having to find their way out without any help.

So by the time you get to your main, well, what one would think is the main cohort, number 5, but it's only 30 percent of the people, you've probably got a general mess, and I don't see any recognition of that in the discussion.

CHAIRMAN STETKAR: Well, there's a good chance that the cohort 5 also leaves before the general evacuation sirens go off, if you look at the time lines.

MR. THOMPSON: I guess I would say, when you say inconsistencies, it's inconsistencies in the thinking and modeling or inconsistencies with what was --

MEMBER BLEY: There's two kinds. The thing I just talked about without the bridges and that kind of stuff is I think an inconsistency in modeling. But you haven't accounted for what these people are going to run into with the bridges being out because I think the model assumes they'll just get on the road and drive the best way out, which they won't have a clue about the best way out.

MR. THOMPSON: In the upcoming slides,

we'll talk about that inconsistency in traffic re-routing.

MEMBER BLEY: Okay. But at this point, probably the emergency responders aren't out to help on the roads by the time these guys are out. Some inconsistency in what I see written is, a little later in the section on response time lines, we talk about cohort 4 and we say, well, you know, they already know about the school evacuations and other media broadcasts, so they're going to be responding that way. But in the definition, we said they don't know about that, they're just going on the sirens. So there's several things like that.

And I don't know about you, but I think most people, if they're parents and they don't know what's happening with their kids, the assumption that half of them are going to beat feet for the boonies without worrying about their kids seems wildly optimistic to me, especially if they're not hearing anything because of the problems with communications.

MR. THOMPSON: Okay. So let me respond first just about the cohort definition, and then

I'll address some of those other ones in the next couple of slides. Is that okay?

MEMBER BLEY: That's a good place to start.

MR. THOMPSON: Okay. So the cohort definition, cohort 4 is modeled to basically feel the large seismic event. The ETE said that there would be a relatively large fraction that was ready to depart early, so we originally built it --

MEMBER BLEY: The ETE is the --

MR. THOMPSON: The evacuation time estimate.

MEMBER BLEY: That's from the state?

MR. THOMPSON: That is from, Arcada is the contractor to TVA, yes.

CHAIRMAN STETKAR: Did the ETE study specifically evaluate large seismic events, or is this a sunny day ETE?

MR. THOMPSON: No, it's a sunny day ETE.

CHAIRMAN STETKAR: So they thought the bridges were all there?

MR. THOMPSON: Right. And we'll talk about that in the next slide.

CHAIRMAN STETKAR: And they thought the sirens were working, and they thought everything was good.

MR. THOMPSON: Right. And so we started from the ETE report, added the layer of complexity of large seismic event and subsequent damage.

CHAIRMAN STETKAR: By the way, do we know that the sirens work after a large seismic event? I mean, the model obviously knows they work perfectly, but do we --

MR. THOMPSON: I'm not sure about after the large seismic event. Maybe someone else can speak to that. But the test of the sirens that are done for regular exercises has a very high --

CHAIRMAN STETKAR: They apply a .5 g earthquake to the sirens during the --

MR. THOMPSON: I would have to speak to my colleagues and --

MS. SANTIAGO: You know, we did ask that question, I think, when we were at the site, and we also were working with Joe Jones who was formerly at Sandia National Labs, and that question did come up. I think we believe that the poles

would stay in a seismic event that the sirens were placed on, and Keith Compton is coming up to the microphone. Maybe he has additional information. We can look into the other information, as well.

MR. COMPTON: This is Keith Compton from the Office of Research. I work with Shannon on MACCS. One of the other things to think about -- first off, I will acknowledge this is an area of modeling uncertainty, so we know that there's --

MEMBER BLEY: Yes, I'll go along with that.

MR. COMPTON: Yes, so that's understood. With regard to sirens, one of the things, it's my understanding is that one of the roles of sirens is simply to alert people that something is happening so that they will turn on the televisions, they'll turn on the radios, and try to seek out information. I think part of the logic here is that if you had this large earthquake, it would be natural to start finding information and people would start finding out information --

MEMBER BLEY: That's generally true, but we define cohort 4 as probably we have troubles

with those communications, so they're just going to go on the siren.

MR. COMPTON: Right. And the other, I guess the other thing that I would add into this and Shannon, I think, is going to talk more about this later, he kind of alluded to it on the issue of loading curve. Part of the consideration of the cohorts is they're not kind of considered in isolation. The general public is going to evacuate in a dispersed manner, some people leaving early, some people leaving late.

So part of the modification of the cohorts from the evacuation time estimate is adjusting all of the cohorts. So as he said, he started out with the kind of early people, the middle, and the late. So some of it is moving some people early, but it also involves moving the middle and tail later for reasons of traffic congestion and stuff like that.

So at least from a model point of view, when I look at the results, I look at the effect on kind of all of the cohorts. So the effect of the earthquake seems to be to disperse the loading curve. Instead of having, you know, the later

leaving shortly after the first, not because of the earthquake, some are leaving sooner and some are leaving much later, so it's kind of spreading out.

MEMBER BLEY: That's probably a bit reasonable. I think also on this cohort 4, because it kind of bothers me that, well, now, we've got this big earthquake and we're seeing more people are moving earlier than usual, and it's a lot of anecdotal information from earthquakes about how people react. And probably the first thing you think about isn't the nuclear plant down the street. It's probably your family and your house and what the heck just happened. This is an earthquake like none of us have been in, and I lived in Southern California and got shaken quite a few times, but this is a whole lot worse than that. This earthquake that can do the damage we're seeing at the plant and all these bridges, you're not standing up. You're thrown on the ground and you're really confused. And to make the leap and say half the people are going to be on the road much more quickly than under another situation seems unjustified.

CHAIRMAN STETKAR: I don't think we saw

big exoduses from the city of San Francisco, the city of Oakland, the city of Los Angeles during the earthquake up north or the North Ridge earthquake down south. People weren't leaving. My cousin lived very close to the North Ridge epicenter, and they have large storage facilities that have been in the news up in that area and people didn't seem to be concerned that they were going to blow up. They kind of stayed around and protected their property.

MS. SANTIAGO: Shannon, didn't you do a sensitivity where there was less people going and so --

MR. THOMPSON: I did, and I will get to that.

MS. SANTIAGO: Yes, he got grilled before we came here on this particular . . .

MR. THOMPSON: This is well taken in the earthquakes and --

CHAIRMAN STETKAR: I think what we're saying is there might be a case where, because of the earthquake, yes, maybe a few people might leave early. Not 40 to 50 percent of the entire population but a few people might leave earlier

than they would have otherwise. Maybe a lot more people might hang around until people come out and say, hey, get the heck out of here, and nobody is hanging around that long. Most of it is not hanging around that long because --

MR. THOMPSON: That is the sensitivity that we did was to shift a large fraction of the cohort into the middle, to cohort 5.

CHAIRMAN STETKAR: But even cohort 5, once you pull up slide number 99, even cohort 5 is leaving before, a good probability before the general evacuation emergency sirens. Not on that.

MEMBER BLEY: On what?

CHAIRMAN STETKAR: On the uncertainty distributions about when they actually, the low-end tails of when they get going.

MEMBER BALLINGER: What kind of exceptions are you making with regard to ability to leave? I mean, if it's a big earthquake, your automobile might not exactly be --

MR. THOMPSON: Okay. So there are a number of assumptions, and, actually, Tina, could you go back to the previous slide. Let me walk through this, and I think we'll touch on some of

these things. So this is a map, at left is a map of the EPZ at Sequoyah. Sequoyah is the red star at center on the Tennessee River. Lake Chickamauga sort of divides the area into eastern and western halves, and for traffic road capacity analysis we further sub-divided it into quadrants: northwest, northeast, and so on.

I believe it was in CPRR, I'm not really sure, but one of the other studies, one of the feedbacks was that we should include a large seismic event as a starting point and the road impacts to that, rather than the sunny day scenario which is potentially very overly optimistic.

So we assumed the bridges within the EPZ are unusable, and here's a pictograph showing most of the bridges within the EPZ.

The road capacity analysis, part that was performed at Sandia National Lab and also I worked with Todd Smith to use, I believe it's called RtePM, to verify. As part of what they do for the EPZ, they show the effect of a major blockage of one of the major exit routes. I believe it was Route 58 headed south. And you can see, you know, there are alternate routes.

In this case, with this many bridges down, there were many, many alternate routes. And as you suggested, people, you know, had to go someplace and then turn around and figure out another way.

MEMBER BLEY: I mean, if you're out in front and had detour signs, that would be different. But you're not.

MR. THOMPSON: If you're the first, then that's not the way it's going to be. But we kind of feel like we addressed it in two ways, and one is that the evacuation speeds in general are much slower than -- so even for the evacuating cohort, we're talking about an average speed of, the first cohorts, we're talking about an average speed of five miles per hour.

Now, to this --

MEMBER BALLINGER: What if they're walking?

MR. THOMPSON: We didn't account for that. And to your question about, you know, a tree being down or a house being down on your car, we don't really have the modeling capacity to deal with that. We can only assume that --

MEMBER BLEY: On the other hand, in real emergencies, we find a lot of people helping each other.

MR. THOMPSON: Exactly. And somebody else mentioned a tree going down and somebody might have a chainsaw or something like that.

MEMBER BLEY: There's one thing that jumped at me, though. You've got two major freeways down in this part of the country, 27 is also a freeway and I-75. A lot of traffic on that is piling up because they're all coming up to get bridges. I don't know how many people that amounts to, but did you think about them?

MR. THOMPSON: Actually, no, we did not. This is focused of getting people out of the EPZ --

MEMBER BLEY: Okay. But a lot of the locals would have jumped up on it until they found they couldn't get on it or couldn't go anywhere.

MR. THOMPSON: Right. The road capacity analysis that was done, for example, the northeast quadrant is not very populated, so those folks would be getting out pretty easily. In the south, towards the town of Harrison, and getting

out to the east side of the river, but those areas would be, you know, very crowded and there would be lots of re-routing.

And so as to the last two bullets, the original sunny day estimate was on the range of, winter weekday to summer weekend, was on the range of five to six hours after being announced and all that, so we sum it up as being about eight hours from. And we added a lag to certain cohorts and reduced the speeds pretty greatly, and it increased the overall departure by 50 percent.

MEMBER BLEY: Okay. I'm just thinking out, those two big roads are the ones I was thinking about. You have people that are stranded between bridges, so somebody will have to come and get them somehow. I don't know if that's a lot of people or not. Depends on the day it happens. It can be really crowded roads and really not.

CHAIRMAN STETKAR: I have no idea how MACCS works, but, you know, you have the 20 percent shadow evacuation outside of the EPZ, right? And, again, I'm thinking Chattanooga, Tennessee. Both of those roads are now clogged up by people because there are big rigs that are backed up, there's all

kinds of stuff that's backed up. Can people in Chattanooga, your 20 percent, get out as easily? Because they're certainly not going to head toward the plant if they know what's going on. But even if they're trying to head away from the plant, if they're the major arteries and they're backed up, are they going to be able to get out?

MR. THOMPSON: We did not consider the secondary effect of traffic within the EPZ to those people outside of the EPZ.

MEMBER BLEY: Probably the model thinks all of the bridges outside of the EPZ are there and all the ones inside are gone. I don't know if you model anything on the outside of the EPZ. You talked about hot spots, and if you project an area over 5 rem that people get moved out. Are there any hot spots in Chattanooga? We couldn't tell. I couldn't see. To the people who didn't see it, it's just southwest of where we were looking, about half, again, as far.

MR. THOMPSON: The risk results are radially averaged, and so --

MEMBER BLEY: Okay. So you can't see that.

MR. THOMPSON: I didn't, you know, plot concentration --

MEMBER BLEY: We don't have an isopleth that shows where the dose ends up?

MR. THOMPSON: No, we do not.

MS. GHOSH: We do know that there's a location, so I can't answer the hot spot. But we did very recently look and there's two relocation criteria outside of the EPZ. It's the hot spot, which is the 5 rem, as you mentioned, and there's also --

MEMBER BLEY: And the 10 to 15-mile one, right?

MS. GHOSH: Yes. In fact, it could be anywhere within 50 miles, but you do see people who are getting relocated but not during this evacuation period but at the time that they would because they've hit that normal relocation criteria. The hot spot I don't know outside of --

CHAIRMAN STETKAR: When do those kick in in the model? Now we're talking about models. If I think, you know, the earthquake happens at t0. When does the model, there's an uncertainty distribution for how long it's going to take to

vaporize the population. They're removed immediately, the population, at that time. But what triggers that?

MR. COMPTON: All right. We're just going to jump into a little bit, and I'll say a little bit about --

CHAIRMAN STETKAR: Identify yourself.

MR. COMPTON: I'm sorry. This is Keith Compton, Office of Research. I'll say a little bit about modeling capabilities, and then I think Doug can say a bit more, as well, about this particular analysis.

The relocation models in MACCS are going to apply to kind of anyone that's not evacuated. And what they'll do is they'll relocate people. So for hot spot relocation, it will do a dose projection and, if you exceed 5 rem, it will relocate you at the time that you tell it. And we put in I think for hot spot relocation a fairly, I think it was something like 24 hours. Okay. So those people basically receive 36 hours of exposure before they're relocated. So the folks that are not in shadow evacuation, the 80 percent that don't shadow evacuate, or any one in the EPZ who, you

know, the 0.5 percent that don't evacuate, they would basically be getting, you know, that kind of exposure before they were taken away from the calculation.

What the outputs that we've added to MACCS recently is kind of a diagnostic output is that we then look at the number of people that were relocated by that criteria. We can look at how many people were relocated by hot spot by normal, and we can look at how many people within the 10 miles, how many people within 15. And so you can start getting a sense of how, you know, how far out did these doses.

I have not looked at -- there are many outputs in MACCS. I have not looked at all of them, so I don't know how many people were projected to be relocated in these simulations. But that's the capability that we have to look at.

CHAIRMAN STETKAR: Some of the concerns that we were thinking about is, you know, you have these, it's a, you know, these concentric rings, as if they're brick walls. My suspicion is that if somebody decides in the real world that some fraction of the city of Chattanooga is at risk,

Chattanooga is going to become a ghost town. They're going to evacuate the entire city. They're not going to evacuate within those little concentric rings. That's why I was curious about how the model actually moves those people and when it moves it.

MR. COMPTON: The model basically, and, again, just going to what the model does, the model basically moves people when you tell the model to move people. Now, what it will do, for evacuation, for example, it won't model the traffic flow. You go in and tell it what the speeds are, what the routes are. So you have to, in advance, due these analyses to kind of figure out what's reasonable to account for different kinds of things.

You know, again, for the relocation, the model essentially just says, okay, it's a pretty old approach. Okay. We're going to assume that after a sufficiently long period, we'll prevent people from getting extremely long, you know, a very large exposure. And the model will move them if needs to. It will move as many people as it needs to move, and that's why it's important to go and look at the outputs to make sure that

it's --

MR. OSBORN: This is Doug Osborn from Sandia National Labs. The actual evacuation model I implemented. So beyond 10 miles, we assumed that no bridges failed. And the model is set up such that you're going to wind up packing everybody on to, ultimately, the interstates or major U.S. routes is basically how we set it. And, again, it's those concentric rings. And as you get further out, it gets to be a much larger land mass. And, again, I did my due diligence in trying to guide everybody, and, ultimately, you will have these major congestion areas during the evacuation. But, again, as you move further out, those hopefully will become less and less of a problem.

MEMBER BLEY: I don't know if it's an odd question or not. We had some interaction with, I think it was here, with FEMA people at one point who claim that, before anybody does any kind of evacuation, they have to certify that it's safe to follow the road. Does that somehow get abated in this kind of situation, or has anybody thought about that --

MR. THOMPSON: I think that all

emergency evacuations are sort of local outward, and FEMA, sort of local offsite authorities would be involved, including sheriff, Hamilton County, Bradley County. And then, you know, when I think FEMA, from a limited exposure sense, I'm thinking then kind of it's the calvary from Washington or wherever --

MEMBER BLEY: Well, that's what I thought, too, until I heard this presentation that says I have the ability to declare areas -- yes, I don't think I would use the roads because of the damage from the earthquake.

MR. THOMPSON: We have done that in a lump fashion by saying that a certain fraction of them are going to be unusable that, if people aren't out because of the quick access to the roads, some people are going to have to find that out for themselves. So, we included rerouting.

MEMBER BLEY: Okay. I'm going to put one more question on the table, but I will let you go until you get to it.

MR. THOMPSON: Okay.

MS. SANTIAGO: Can Todd answer what you were just speaking to?

MEMBER BLEY: Sure.

MR. SMITH: Yes, Todd Smith, Emergency Preparedness Specialist, Office of Nuclear Security and Incident Response.

We helped Dr. Shannon here. What would be the impact of the evacuation from the earthquake scenario? First of all, we validated that two ways.

One, from the evacuation time estimate, there was a separate roadway impact study. That was done using a microscopic simulation model that actually tracks each vehicle on the highest-capacity road, which in this case was State Road 58 heading toward Chattanooga. The assumption there is that the road is completely blocked and the people have to bring themselves around that area. Okay? That takes about another three, three-and-a-half hours.

And then, the second way we validated it is a macroscopic model where we modeled the whole area and, again, took out the same bridges that they assumed in their hand calculation. When we ran that calculation, we got similar, exact same results as far as the time it would take for people

to work around the road closures.

The other question, though, about health giver actually paid for the locals to respond, we have reviewed some event reports from earthquakes in California and in other areas, and find that usually it only takes a couple of hours for the crews to be out there cleaning the roads. Even the people get out there themselves and move the obstructions out of the way, so they can get moving.

MEMBER BLEY: California hasn't had an earthquake in the last 100 years.

MR. SMITH: Yes, and the interesting thing with California is that, with this assumption that the bridges fail, that's what was happening in California. The bridges were failing. The roads were passable.

Now California has done so much with strengthening the bridges, you find the bridges are okay. But in a region like Tennessee we would expect to see the bridges would fail, but the roads would be passable.

And there was a recent earthquake up in Alaska, 7.1, just in January, and that is exactly

what we saw. The roads were slightly damaged, but still passable.

MEMBER BLEY: Okay. Thanks.

The question I would like you to get to somewhere in your talk -- can you see that picture?

MR. THOMPSON: Uh-hum.

MEMBER BLEY: It is the one that shows the keyhole thing.

MR. THOMPSON: Right.

MEMBER BLEY: When you talk, I would like to understand how you, because they seem separate, how you overlay these cohorts on this keyhole idea, especially Cohort 4.

MR. THOMPSON: Okay. So, let me address this slide and --

MEMBER BLEY: And the schools, I guess, 1 and 2 as well.

MR. THOMPSON: Okay. So, looking at this graph, we are tracking the evacuation timelines and travel speeds, as they were modeled.

MEMBER BLEY: And these are like median times or something?

MR. THOMPSON: Right. These are

average speeds, depending upon the phase of the progression. The timeline on the bottom, zero is the accident initiation, is the initiating event, the earthquake. The SAE and SAE sirens and GE and GE sirens are provided for reference. And then, the yellow is considered normal activity. Blue is sheltering. Green and orange are the evacuation timelines with the speeds inset in miles per hour.

The schools are notified at SAE. And so, we are assuming that those students are going to be sheltering. The period that they are sheltering is probably overly-conservative. The reason for that is --

MEMBER BLEY: You might say "overly-long". Is that what you mean?

MR. THOMPSON: Yes.

MEMBER BLEY: Overly-long?

MR. THOMPSON: Overly-long.

MEMBER BLEY: I don't know if it is conservative or not because they have to go outside if they are not sheltered.

MR. THOMPSON: Okay. Overly-long blue period.

Let me come back to that. Let me walk

through these, and then, I will talk about the schools specifically.

MEMBER BLEY: Okay.

MR. THOMPSON: So, the Cohorts 2 and 3 are the medical facilities and transit-dependent. Transit-dependent just means people don't drive, but we are mostly assuming elderly. Those cohorts are considered to be sheltering at the time and don't come out.

Cohort 4, which we will talk about just in a moment, does go on SAE sirens and feeling the quake. We can even talk about that off the table after.

Cohort 5 is the general public.

Cohort 6 is the general public tail, the final 10 percent.

CHAIRMAN STETKAR: And again, for the record, Cohort 4 is 40 to 50 percent, depending on where you read in the report, but it is in that order.

MR. THOMPSON: It's approximately 40 percent of the total zero-to-10-mile population.

CHAIRMAN STETKAR: Okay. And Cohort 5 is the rest of them. So, when you talk about the

general public of 5, it is almost the other half of the people.

MR. THOMPSON: Uh-hum.

CHAIRMAN STETKAR: So, 4 and 5 are, basically, about the same number of folks each.

MR. THOMPSON: Thirty-eight thousand people each.

CHAIRMAN STETKAR: Yes, but, I mean, it is not correct to think of Cohort 4 as a small fraction of the population. It is almost half.

MR. THOMPSON: Okay.

CHAIRMAN STETKAR: And 5 is almost the other half, except for the people who decide not to move.

MR. THOMPSON: Right.

MEMBER BLEY: Well, the 10 percent who are --

MR. THOMPSON: Cohort 7 is the shadow evacuation in the 10-to-15-mile zone.

As I mentioned, there are color codes for the activities. Yellow is considered normal activity. Blue is sheltering. Green and orange are the evacuation speeds by phase.

One thing to be reminded about is these

times, these speeds and evacuation delays are also considered in the uncertainty analysis. And so, they are varied for all these cohorts.

Under this set of assumptions, 100 percent of the evacuatees exit in approximately 12 hours, and about 90 percent exit the EPZ in about nine hours.

As I mentioned, we have a sensitivity study that we have done that didn't make it into the report. But, there, we shift the fraction of Cohort 4 from 50 percent of the general population down to 20 percent of the general population and move everybody else into Cohort 5, the middle.

MEMBER BLEY: Including the school kids because they aren't in school?

MR. THOMPSON: No, the school kids were still modeled as --

MEMBER BLEY: Okay. So, it is always the daytime? Okay. Go ahead.

CHAIRMAN STETKAR: Suppose you've moved, instead of getting 20 percent of the people out of there, 1 in 5, suppose you made everybody Cohort 5. How would the results change? Do you know?

MR. THOMPSON: Yes. I can just speak off-the-cuff for that --

CHAIRMAN STETKAR: Yes.

MR. THOMPSON: -- because Cohort 5, if you look at the evacuation timeline and you see they are coming in at about eight-fifteen, eight-and-a-half hours, is kind of close to the median of all the evacuations. If we lump everybody in 5, the risk results probably wouldn't change that much. Obviously, it would be cohort, but the emergency phase may go down some in some instances because the non-evacuatees get a larger fraction of the dose during the emergency phase. So, if we lumped everybody into 5, it would probably be similar risk results, but slightly lower in the emergency phase results.

CHAIRMAN STETKAR: Okay.

MR. THOMPSON: Okay. Next slide, please.

Now earlier you have seen several horsetail plots showing cesium and iodine releases. This is three selected cases shown on one environmental release profile. It shows the cumulative environmental release fractions of

cesium in the dashed line and iodine in the solid lines. Unfortunately, here the dashed lines aren't showing up.

CHAIRMAN STETKAR: Turn your microphone on, because whenever you speak, you've got to be on the microphone.

MS. GHOSH: Yes, I apologize. For some reason, the dashed lines don't show up in the presentation view. But I will just show you this, so you will have the mental image which ones are the dashed lines. And I can go back to the slideshow. I think the printouts are okay, right? The printouts of the dashed lines came out. But sorry about that.

MR. THOMPSON: Can you make that one bigger, Tina?

MS. GHOSH: Yes.

MR. THOMPSON: Okay. So, the green set of lines is an example on the early release for the STSBO without random ignition. That is one of those 400-and-some runs that was selected particularly because it was an early release.

The blue lines show the results for the LTSBO that Hossein talked about just a moment ago.

And then, the red line is for the STO without random ignition, an example of late release. Just for reference, the cesium results happen to be lower than all of the iodine results.

The reason why I wanted to show this was to say that the release durations are also important. As you can see, going out to 72 hours, the early release begins at about four hours. And so, we have 68 hours of cumulative release time. The increase is happening at about eight hours and at about 15 to 18 hours in that case. Similarly, there are step functions with the LTSBO that increase at 28 hours and another increase from 42 to about 48 hours. And then, on the STSBO late release, the red one, there is a small, almost leak-rate release beginning at four hours and, then, a pulse release at 52 hours.

Just for reference, the cesium range is from about -- the biocumulative release of cesium ranges from about 2 to 6 percent and iodine ranges from about 8 to 9 percent.

Next slide, please, Tina.

And these show the individual LCF risk results. For these three cases, the early fatality

results were zero. The Y-axis is conditional risk. On the X-axis is the distance from the plant. Again, the red symbolizes the STSBO late release. The green symbolizes the STSBO early release, and the blue is the LTSBO.

These stacked column charts are further fractionated between the risk results as a result of exposure in the long-term phase, which are the solid components, and the hatched are as a result of exposure in the early phase.

Just to go ahead and address the question about the results from the sensitivity, where we moved Cohort 4 to be 20 percent and moved everybody else into 5, the only real difference is, it really doesn't matter for the STSBO late release and the LTSBO cases because the interaction with the plume isn't in play, but for the zero-to-10-mile, the green bar there, the emergency phase release basically doubles, taking that hatched part to go from 8.5 E to the minus 4 to 9 E to the minus 4.

CHAIRMAN STETKAR: Did you start to pick up early -- when you said early fatalities are zero, they are statistically, as I look at the

results, there are some small likelihood. Did you pick up more in that case at all?

MR. THOMPSON: For SOARCA, we don't typically report early injury, but the early --

CHAIRMAN STETKAR: No, no, early fatalities.

MR. THOMPSON: Right. Early fatality results remain zero even --

CHAIRMAN STETKAR: Zero is the --

MS. GHOSH: That is the deterministic case.

MR. THOMPSON: Right.

CHAIRMAN STETKAR: Oh, the deterministic case. Okay.

MR. THOMPSON: Tina will be talking about the --

CHAIRMAN STETKAR: Sorry.

MR. THOMPSON: So, as you look at the red bars going further away from the plant, there is a general slight decrease, zero to 10, 10 to 20, zero to 50, but you see a proportionate increase in the emergency phase for 10 to 40, which is the people who didn't leave and there was some interaction with the plume.

For the early release, or the green bars, you see, again, the fraction of about 8 percent of the risk comes from the emergency phase, and zero to 10; that goes up to about 25 percent and 10 to 20. And it is about 15 percent for the whole zero-to-50-mile interval. The results for LTSBO are somewhere in between the two STSBO cases.

And I think that is all I need to say about that one.

The risk results have been shown, at least in these three cases, to be a function of release duration as well as magnitude and timing. For these deterministic cases, the longer release durations resulted in increased long-term LCF risks near the plant, particularly near the plant. A lot of releases are more widely dispersed because there is more weather play. So, the wind shifts, the stability can change. So, there is just greater dispersion in general. More dispersion and more dispersion in more directions leads to larger areas of contamination, again, particularly close the plant as the wind is shifting. This can result in exposure to more people, which increases the long-term LCF risk, particularly if the contamination is

just below the habitability criterion.

The next slide, please.

For the offsite consequence summary, for these three deterministic analyses, the risk results ranges from 5 to 9 E to the minus 4 for a zero-to-10-mile region and, generally, decreased with increasing distance from Sequoyah. The long-term risk results dominate the emergency phase risks. Obviously, it was higher for the early-release cases. For these cases, the early fatality risks are zero, and even in the case of very early release, the risk of similar magnitude as the other cases.

Did you have some other questions?

MS. SANTIAGO: No, we'll move on to Tina.

MS. GHOSH: Okay. We're going to keep going, hearing no questions.

If it's okay with you, I'm going to go fairly quickly through the first couple of slides.

So, the parameters we looked at in the uncertainty analysis, this time they are mostly identical to what we looked at at Peach Bottom and Surry. We added a couple of new parameters that I

don't think it is worth discussing unless you want to. We added one dispersion coefficient -- that is asterisked -- and another parameter we forgot to put in the tally, which was the amount of weather hours projected that are used in the keyhole evacuation. But neither of these new parameters show up as important in any of our regressions. So, if it is okay with you, I would skip ahead one slide to the results, unless there is something you want to talk about. Is that okay?

CHAIRMAN STETKAR: Well, I didn't have a grasp on the Surry and I know less, much less about this stuff. So, I figured all of this stuff was coming.

In a couple of cases I got confused. I don't know anything about the parameter itself, but there are tables of the uncertainty distribution parameters and there are figures that plot the uncertainty distributions. In several cases the figures did not seem consistent with the tables. And I will just give you a reference because we don't have these as handouts.

But, for example, if I look at the ground shine factor, the composite ground shine

factor, if you look at Table 5-12 and Figure 5-12, if I look at the values in Table 5-12, it says that the -- I have now lost my place, so you'll have to bear with me.

If I take the sheltering phase, it says the 50 percent quantile -- I'm going to read the right one here -- for the generation population Cohorts 3 through 8 in Table 5-12, the .5 quantile is a protection factor, if you will, of .2, right, from Table 5-12?

MS. GHOSH: Uh-hum.

CHAIRMAN STETKAR: If I look at Figure 5-12 for the smooth curve, the uncertainty distribution, the 50th percentile of the cumulative distribution function is, for that same group, is .1. That's a big difference. Some other places, okay, I can handle small differences in smooth-curve plotting. And there were a number of these cases where, when I looked at the smooth-curve plot versus the tabulated, if you will, best-estimate upper and lower bounds, the median value was quite a bit different than the tabulated median value. The upper and lower bounds were always real close.

So, I wasn't sure what was going on

when you were plotting the distributions and, then, what distribution was actually used in the study. Because a factor of two on shielding, I have no idea what this thing does in the model. I'm only looking at tabulated sets of numbers versus plotted distributions and identifying things that don't seem consistent.

MS. GHOSH: Yes. Now thank you for that. We will double-check that one.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: I'm running out of steam, too.

CHAIRMAN STETKAR: Yes. No, but the first time in Surry I didn't look at any of these things --

MS. GHOSH: Right.

CHAIRMAN STETKAR: -- because I just ran of time.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: So, this is the first time and I suspect it is probably exactly the same in Surry. I just didn't look at it. So, that's why you're just hearing it this time.

MS. GHOSH: Yes, yes. No, thank you

for that. We will look into that, yes.

CHAIRMAN STETKAR: But check all of that because there are a number -- I just happened to note this one because it was the first one on my list.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: And I am not going to list the other one. But there are several instances where the zero percentile and 100 percentile matched the curve. When I look at the middle part, it has shifted one way or the other.

MS. GHOSH: Uh-hum. Okay.

CHAIRMAN STETKAR: Okay?

MS. GHOSH: Yes, we will double-check that.

CHAIRMAN STETKAR: Smooth curves, what's -- well, you don't know.

(Laughter.)

MS. GHOSH: Well, we had an artist draw the curves.

(Laughter.)

We like pretty "S's".

No, we'll definitely check that.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: Thank you for that.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: Okay, yes, I will skip to the results. So, first, I am reporting the individual latent cancer fatality risk conditional on the accident occurring. This table is for the set that is without random ignition credited, and the table on the next slide is random ignition credited.

Just a reminder, these are actually the distributions of the means that come out of the weather distributions. So, they are means over the weather valuation. This is a description of the epistemic distribution at these intervals.

So, in these two tables, first, I am showing concentric circular areas, so starting centric around the plant. And then, after that, I have a graph that shows the annular rings, also centered around the plant.

Yes?

CHAIRMAN STETKAR: Also, just to get it on the record to remind you, I looked quite a bit also -- and again, because of time and we don't have the tables and figures in front of us here --

on both the delay times and the evacuation speed on certain distributions, several of them didn't quite seem consistent. In other words, the same evacuation speed might be used for a couple of cohorts where it wasn't at all clear why they would be the same based on this figure in terms of when those cohorts would be moving relative to the general congestion, if I accept the Cohort 4 leaving early, and so forth.

MS. GHOSH: Uh-hum.

CHAIRMAN STETKAR: And that's not this curve-fitting thing. It doesn't make sense, given when they are actually entering the flowstream, if you will. So, check those also.

MS. GHOSH: We will check those, too, yes. Thank you.

CHAIRMAN STETKAR: Thanks.

MS. GHOSH: In terms of the distribution that you see considering any of these radial intervals and the spread in the results, they basically all fall within, I would say, a relatively-narrow range to something just under 10 to the minus 4, which is the 5th percentile at 50 miles, up to just under 10 to the minus 3, which is

the 95th percentile at zero to 20 miles.

In the case where we credited random ignition, actually, the distribution is very similar, again, looking across all these different radial areas. In this case, the lowest of the lower bounds is a little bit lower, but, essentially, we are talking about the same order of magnitude in terms of LCF risk.

CHAIRMAN STETKAR: Tina?

MS. GHOSH: Yes?

CHAIRMAN STETKAR: If you go back to -- I've got too many notes here that I'm trying to keep up with.

If you go back -- there.

MS. GHOSH: The with random or without? Or does it matter?

CHAIRMAN STETKAR: With.

MS. GHOSH: With?

CHAIRMAN STETKAR: I have to put these on, so I can see.

Without random ignition.

MS. GHOSH: Without, okay, yes.

CHAIRMAN STETKAR: Yes. Now, in the zero-to-10-mile range here, the mean individual

conditional latent cancer fatality value is 3.5 E to the minus 4.

MS. GHOSH: Uh-hum.

CHAIRMAN STETKAR: And that's the result from the uncertainty analysis.

In the report it says, well, okay, that is somewhat lower than the corresponding result of 4.8 E to the minus 4 from the deterministic analysis.

MS. GHOSH: Uh-hum.

CHAIRMAN STETKAR: You say, "somewhat lower". Okay, but that's quite a bit lower to me.

MS. GHOSH: Uh-hum.

CHAIRMAN STETKAR: That's about 75 percent. Now the deterministic analysis I thought was supposed to be using the best-estimate values from each of the uncertainty distributions for the uncertainty analysis. So, if that is the case, why is the mean from the uncertainty analysis coming out to be substantially different from the so-called best-estimate analysis?

MS. GHOSH: Yes.

CHAIRMAN STETKAR: Why this one? I mean, we know --

MS. GHOSH: Right, right.

CHAIRMAN STETKAR: -- on the earlier studies why that is, because the best estimate certainly wasn't, but in this case I thought that it was supposed to be.

MS. GHOSH: Yes. So, the way the define the -- I don't even know what to call it -- the base case for the short-term, I'm going to get a short-term station blackout, you're right that the way that is defined is by the inputs going in.

CHAIRMAN STETKAR: Uh-hum.

MS. GHOSH: We know because there are synergistic and threshold effects in our modeling in both the accident progression and, I'll say, the consequences side, that your best estimate, if you were to do one calculation with your point estimates, your best evaluation for each uncertain input that you put in, is not necessarily going to match very well the mean once you put all the distributions on it and, then, promulgate that uncertainty --

CHAIRMAN STETKAR: No.

MS. GHOSH: -- because it is not Gaussian distribution of results as the other is.

I think in the theoretical example, if everything was normally distributed, you know, putting it in, and your distribution is normally distributed -- but it's not quite that way.

CHAIRMAN STETKAR: Okay. Let me stop for a second.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: I mean, I have a couple of questions here.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: My first question was, is this behavior -- and I have seen this behavior in a lot of risk assessment models -- is it simply because we did not perform a sufficient number of samples? In other words, is this the fact that we ran 400-and-some-odd realizations from distributions that may or may not be fairly broad, and we just didn't run enough -- it didn't converge, do we know that? Did you do any convergence checks?

MS. GHOSH: I don't believe convergence is an issue.

CHAIRMAN STETKAR: You don't?

MS. GHOSH: No.

CHAIRMAN STETKAR: Okay. Then, you can ask your question.

MEMBER BLEY: How did you pick the best estimates? The last time around, you kind of said, "Well, it didn't have anything to do with the distribution. It was just what our engineer thought was the right answer for the best estimate." This time we thought that would have been factored into your uncertainty distributions, so that it would have been the median or the mean or something in between them.

MS. GHOSH: Yes, I know, this is always the struggle of having parallel kind of analyses --

MEMBER BLEY: But where does it come from? How did you pick them?

MS. GHOSH: It was the same group of experts doing both sides of the analysis. So, in some cases, the base-case or best-estimate value of the input matched something, like the mean or the median. It wasn't always, you know -- but, for each input, it is evaluated as what is the best, if you had to pick one point --

MEMBER BLEY: No, but if you have the same people doing the distributions --

MS. GHOSH: Yes.

MEMBER BLEY: -- just tell you their best estimates --

MS. GHOSH: Right.

MEMBER BLEY: -- how can they reconcile their distributions with the best estimates if the best estimates aren't somewhere in the densest part of the distribution? I don't understand that. What are they thinking?

MS. GHOSH: Yes, I guess, you know, maybe --

CHAIRMAN STETKAR: By the way, part of our recommendation a long time ago, only the most recent letter that the ACRS wrote, is to do it all together, not in parallel as if they are different. And what I'm hearing is it is still being done in parallel as if they are different.

MEMBER BLEY: Because they are different.

CHAIRMAN STETKAR: Because they are different.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: Because, as Dennis said, I can't, if this is my best estimate and this

is my uncertainty distribution about it, I don't understand why, for this other parallel calculation, my best estimate --

MEMBER BLEY: I would pick a different number.

CHAIRMAN STETKAR: I would pick a different number.

MS. GHOSH: Yes, we have been trying to de-emphasize reading too much into the individual cases that we do analyze as individual cases in order to explain from a phenomenological standpoint what is happening in those cases, so that we can explain the differences in the outcomes. But we think that, once you have done the uncertainty analysis, it is valuable to look at the whole population of results and not just seize on one example or case as the representative. We have talked about this a lot.

MR. ESMAILI: Yes, we have talked about this. We have thought about this --

MS. GHOSH: Yes.

MR. ESMAILI: -- how we are going to say this best.

You know, in the short-term station

blackout, looking at what we call best estimate for the base case, the short-term station blackout base case results in a late containment failure. Okay? When we look at the long-term station blackout, assuming exactly the same parameters, it results in an early containment failure.

So, what Tina is trying to say is that, you know, if you go back to, for example, slide 61, everything is dependent on the amount of hydrogen. So, you can see that there is a range here in the amount of hydrogen that is produced that overlaps.

Like, for example, if the hydrogen is between 380 kilograms or 420 kilograms, you can see it is possible to have both early failure and late failure. So, just because we are using the mean or the median values when you are defining a base for a reference case, it could be going either way.

But look at the long-term station blackout, that long-term station blackout consistently produces much more hydrogen, over 500 kilograms. So, you always get an early containment failure.

So, it is very, very sensitive. We know that. You see that in 61, that you change

this hydrogen, you know, amount of hydrogen that is released through the containment; you can go either way. You can either have early failure or late failure.

We did not feel that there is any reason that if we assume, for example, the same median values or the same mean values or best-estimate values, that we are going to end up in --

MEMBER BLEY: Well, it might end up differently.

MR. ESMAILI: It might end up differently, too.

MEMBER BLEY: My only question was, if you are picking a distribution and you are picking a best estimate for a parameter, either why wouldn't you pick estimate that is pretty much in the center of that distribution or why wouldn't you pick a different distribution, if you didn't think that was the case? I just don't get it.

CHAIRMAN STETKAR: The whole point here is --

MEMBER BLEY: And they won't match perfectly.

CHAIRMAN STETKAR: I agree with that

because it is very --

MR. ESMAILI: Again, we want to de-emphasize. This is very important. The whole reason we are doing this uncertainty is because we want to de-emphasize any single calculation.

CHAIRMAN STETKAR: Okay. Great. Then, why in the summary of the results do you emphasize -- emphasize -- the fact that the mean value of the uncertainty results is lower than your base case? Why is it necessary to say that --

MS. GHOSH: Actually, that is a good point. You probably don't need to say that.

CHAIRMAN STETKAR: -- that the mean value, which is the only thing I believe, it is the only thing I believe from my uncertainty analysis, is truth as best within the limitations of the distributions.

MEMBER BLEY: If you are doing the base case just to kind of see things, then it is meaningful. But that's okay.

CHAIRMAN STETKAR: You could convey it to any arbitrary case and it might still --

MEMBER BLEY: But don't worry about it.

CHAIRMAN STETKAR: Yes, don't worry

about it. If you want to de-emphasize any particular --

MS. GHOSH: Yes, that's good.

CHAIRMAN STETKAR: -- realization or anything --

MR. ESMAILI: So, had we done this analysis the same way that we had done the original SOARCA, for example, just coming up with some best-estimate numbers, we would have seen that, yes, in the base case I'm going to have a late containment failure. But I already know how sensitive this is. So, I would have had to go back and find parameters because it is equally probable to have an early failure. So, I would have had to do the sensitivity.

And that is why we are trying to say that any single one of these realizations is not important because choosing one, even though it is best estimate, it is not telling you how many to end up --

CHAIRMAN STETKAR: See, in that sense, you use the term "best estimate"; I will use the accurate term "arbitrary". The arbitrary analysis that you did that you gave the main best estimate

--

MEMBER BLEY: Either that's true or the distribution is arbitrary.

CHAIRMAN STETKAR: Is arbitrary.

(Laughter.)

I'm willing to say that they thought about the distributions and, indeed, had some rationale, because they actually explained the distributions, regardless of whether you agree with them or not.

MR. ESMAILI: I don't think our distributions were arbitrary.

CHAIRMAN STETKAR: No, no, no. I'm saying I agree with the distributions because you at least -- I disagree with many of the distributions themselves, but I do not characterize them as arbitrary because each of them has an explanatory text associated with it. This is why we have this range. This is why it has its central tendency. This is where we got the information from. I can argue with the numerical values, but there is some credence to each of those distributions.

The other thing that I am calling

arbitrary is that thing that gives you 4.8 E to the minus 4, which you prefer to give the name "best estimate" and I prefer to give the name "arbitrary" because I can't tell the basis for that thing.

MR. ESMAILI: It's not an best estimate in terms of what we expect because of the complexity of the model.

CHAIRMAN STETKAR: Then, why do you call it a "best estimate" because, if I am reading this --

MS. GHOSH: We will change the language.

MR. ESMAILI: We can change that if you don't like the word "best estimate".

CHAIRMAN STETKAR: Thank you.

MS. GHOSH: Yes. We will do that, yes. Sorry.

Okay. I did want to show this slide, and I realize we are running short on time, but I think it is interesting to look at the annular ring. So, this now, again, is a complementary cumulative distribution function of the epistemic uncertainty. So, it is the distribution of the means over all of the weather trials.

You can see two things. First, that the two cases -- so, the colored lines are grouped together. Solid is without random ignition and dashed is without. And the same color for the different annular distances. You can see that those distributions are very close to each other, whether or not you credit the random ignition.

The other thing that is interesting is that the zero-to-10, 10-to-20, and 20-to-30, in fact, are very close together. They are kind of clumped. You don't see the risk significantly falling off until you get beyond 30 miles. You know, this is different for each scenario and each site that you would look at.

But, anyway, those are the main things I wanted to mention on that slide.

The Y-axis is linear; the X-axis is long.

MEMBER BLEY: So, the dropoff isn't like I'm used to when I see a log plot then?

MS. GHOSH: Right.

MEMBER BLEY: The dropoff is a factor of less than 2.

MS. GHOSH: Okay?

MEMBER BLEY: I think so.

MS. GHOSH: I was just going to quickly present a couple of the regression tables. Again, we're running short on time. But this is the one for the zero-to-10 miles. The things that are showing up as important -- this is a population that evacuates mostly. It is the safety valve fraction, which is no surprise. We know that that is important for source-term and that also translates to the LCF risks.

The CF risk factor, that is the cancer fatality risk factor, 8 is for the residual organ. And you probably heard me talk about this for Peach Bottom and Surry 2. The residual is a representative soft tissue. And that one also makes a lot of sense because in the long-term the main contributor is cesium ground shine. And so, it makes sense that that shows up as important.

In this case we have a couple of other parameters that show up. The CF risk is a cancer fatality risk factor for the colon. The ground shine factor, too, is the ground shine shielding factor for normal activity during the emergency phase, but it is perfectly correlated with the

ground shine shielding factor during the long-term phase. So, that one makes sense, too. And then, rupture, we already talked about that is the containment rupture pressure.

When you, then, look at the 10-to-20 mile ring, the changes you see, the CF risk 4 is for lung. That makes sense because a 10-to-20 mile, as we discussed before, they don't get evacuated unless they hit that normal relocation registry. So, they may be sitting in the plume longer until we get them out. So, it makes sense that the lung factor becomes important there. Here, also, we see the shape factor having some importance for this next ring.

For the zero-to-50 mile, you basically see the same actors, again, the lung. I think you may have protective actions out to 30 miles. So, once you look at the whole circle out to 50 miles, that lung factor, again, is showing up, and then, some of the same other ones that we have seen on the previous table.

And I think that was it for LCF risk.

You asked earlier about the early fatality risk. We do compute a non-zero number in

about 10 percent of the cases, but, again, like we saw in the past like for Peach Bottom, it is a small percentage of the weather trials in those cases, actually, stars aligning right to be able to get to an early fatality risk.

CHAIRMAN STETKAR: If it rains early and people don't move --

MS. GHOSH: Yes, yes.

CHAIRMAN STETKAR: -- you are going to pick up more, which is why I was asking about why, taking the average, you know, 2012 versus --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- if I took -- I don't remember which one it was. It was 2011 or 2010 where it rained a lot more -- a lot more --

MS. GHOSH: Right.

CHAIRMAN STETKAR: -- and getting cohort 4 out early, you could conceivably pick up more there.

MS. GHOSH: Right. Yes. Yes, that is true.

So, here we have the zero-to-1 mile out to the zero-to-4 mile is the smallest curve on the bottom. You can see on the Y-axis the CCDF, you

know, it starts below .1 because we have about 10 percent only that we can compute this number for.

And you can see the shapes. The numbers, the conditional risk numbers are still fairly low, which is why we continue to make the statement that the overall insight is that we have essentially zero risk, as your multiplying a bunch of low-probability things together.

I think Doug Osborn is going to --

MR. OSBORN: Doug Osborn from Sandia.

I will double-check the extreme cases, but for the majority of these cases that you see that have a non-zero early fatality risk, it has nothing to do with evacuation cohorts, but it has to do with the cohort that does not evacuate.

CHAIRMAN STETKAR: Okay.

MR. OSBORN: Yes.

CHAIRMAN STETKAR: Yes, I mean, that makes sense that that statement is made. I just don't know, if you delayed more of the population, whether you would pick up some of them or not.

MR. OSBORN: That is a possibility.

CHAIRMAN STETKAR: I just don't know. I just don't know.

MS. GHOSH: Yes. So, I think that was it. This is just an overall conclusion slide about the ranges of LCF risk we see within 10 miles. We continue to see that the long-term phase risk dominates the overall risk. As I said before, we say early fatality risk are essentially zero. We can calculate non-zero numbers with our powerful computers. And the parameters that show up as important are not surprising.

Keith, did you want to add something? I didn't mean to cut you off.

Keith Compton was just coming to the microphone when I started talking.

MR. COMPTON: Sorry. This is one of my more favorite topics. I was just going to add in, just one other thing to add on this.

I mean, we continue to look. This issue of very low, essentially zero, early fatalities is one of the kind of more interesting phenomena that I think we have been seeing recently.

There's one thing that I wanted to add. Rain is certainly a factor and was seen early, but one of the things that I have been seeing -- and

actually, I now understand, when I go back and read WASH-1400, which I didn't understand before, it is not just rain; it is also low wind speed --

CHAIRMAN STETKAR: Yes, I was going to say --

MR. COMPTON: -- stable conditions, kind of just nighttime conditions.

CHAIRMAN STETKAR: Sure, sure. Yes.

MR. COMPTON: And actually, it is often the case that kind of the mean of the results is driven by those relatively-common bins, those relatively-common weather conditions. Rain can certainly drive kind of total numbers up.

But I just wanted to add in the fact that we are starting to get a little bit more nuanced about it is stability; it is high concentrated. And the fact that we have the non-evacuating cohort is kind of useful. As mentioned, it is kind of an indicator that you have to delay evacuating cohorts to kind of be comparable to what those cohorts are doing to get those up.

Does that make sense?

CHAIRMAN STETKAR: I don't know because this model is --

MR. COMPTON: In the model, essentially, they would have to get an exposure comparable to the non-evacuating cohort before they started -- if you are not seeing it in the non-evacuating, you're probably not going to see it in the evacuating unless you did something to their exposure characteristics, so that they acted more.

CHAIRMAN STETKAR: You did do a sensitivity study, though, where you sheltered in place.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: You're not going to talk about that, but --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- that is kind of an interesting little story, that you sheltered in place, and then, you said, well, a good fraction of the windows might be broken. So, you increase their inhalation dose. You didn't pick up -- I don't remember whether you picked up more fatalities. You certainly picked up more latent cancers that way.

MS. GHOSH: Yes. I think Shannon is going to say something more about that. But, yes,

we didn't include that here.

CHAIRMAN STETKAR: You didn't include that here, but it was a nice story. It was a story that the conclusion is, regardless of -- you know, sheltering in place is not a good thing to do in these events if there is going to be damage to your buildings.

MS. GHOSH: Yes, uh-hum.

MR. THOMPSON: That's right. Sheltering in place is not a good thing to do. The added broken windows and the structures standing inhabitable was kind of a nuance that it wouldn't just be ground shine through the building, but also some inhalation. You would get some anyway. The sort of modeling simplicity was whether they would be there the whole time and that sort of thing.

CHAIRMAN STETKAR: Right. Yes.

MR. THOMPSON: But, in that case, we did see some early fatalities --

CHAIRMAN STETKAR: Did you? I couldn't remember. You did? Okay.

MR. THOMPSON: They were non-zero. I don't remember the range, but 10 to the minus 10, or something like that.

MEMBER BLEY: Well, even if you don't break windows, you assume some air turnover in buildings anyway, right?

MR. THOMPSON: Right.

MEMBER BLEY: At least in homes?

MR. THOMPSON: Right. The protection factor I don't recall right offhand, but it is something on the order of protection factor of -- I believe 1 is complete protection; zero is no protection, and something like .3 --

MEMBER BLEY: Yes That would allow -- yes.

MR. THOMPSON: Yes. And so, we varied it to basically no protection.

MS. GHOSH: If there are no more questions on that, Sal is going to talk next about the public comments we got.

MR. HAQ: My name is Salman Haq.

I have collected the public comments. What we did was we took the Draft Technical Report and placed in ADAMS for the public to review. A request for comments was published in The Federal Register. We gave them 30 days to provide comments. We also posted a blog for information

about the FRN, the Federal Notice, as well as about the public meeting.

We held a public meeting near the site, actually, at the training center in Soddy-Daisy, Tennessee. It was attended by about 30 people.

MEMBER BLEY: All from the site or did you get some local population in there?

MR. HAQ: Mostly local population attended.

MEMBER BLEY: I'm sorry, locals who are not site workers?

MR. HAQ: Well, okay. So, out of 30, there were two intervener-type people and, then, there were a few people -- the people who lived there have worked in the plant or knew somebody at the plant, they showed up. So, it was an interesting mix of people. There was a person who was in the hospitality business, and she showed up.

And we had a lot of discussion, informal discussion, before the public meeting, explained to them what we were doing and, then, a formal presentation. Then, we gave them a chance to ask questions.

MEMBER BLEY: Yes.

MR. HAQ: The questions were asked by two people, by two individuals, one from UCS, and he identified himself.

These two slides summarize the questions that they asked. Even though we summarized the questions, but we kept their words to kind of convey the meaning in there.

So, the first set of questions were: analysis seemed unbalanced in the treatment of damage due to beyond-design-basis earthquake. And then, there was some explanation that you have assumed extensive damage to the safety system, and yet, you have considered limited damage to the infrastructure within the 10 miles of EPZ.

And then, there were further comments that elaborated that a little bit more and kind of suggested that the analysis should be more rigorous -- analysis should more rigorously consider the impact of a large earthquake on evacuation time. For example, the analysis did not consider the impact of downed towers that may impede the roads, and it also didn't consider all the brides in the EPZ.

And we tried to answer them. Actually,

those answers are posted now on the public site as the summary notes for the meeting.

The next set of comments comes from another attendee, and he questioned the consequence -- he basically said that the consequence analysis was flawed for the following reasons. And one of the reasons he gave was that our modeling did not consider age and sex. He was concerned about the risk for the children, and so on and so forth.

The lessons from Fukushima had not been learned as to health consequences. And then, a reference report was provided, which was done recently by Physicians for Social Responsibility, and it is available on their website.

MEMBER BLEY: Was that leaning toward other effects than radiological effects?

MR. HAQ: They were mostly radiological effects.

MEMBER BLEY: Oh, they were still radiological effects?

MR. HAQ: Yes. They had some disagreement on what the other organizations had published.

They also questioned how risk can be

lowered after an accidental release as compared to the risk to the general population. In our conclusion, we compared that. So, they had a comment on that.

They also questioned the consequence from potential severe accidents in an area really are smaller than previously calculated.

And so, those were the comments we got in writing.

MS. GHOSH: Any questions?

(No response.)

MR. BARR: Okay. I'm Jon Barr, and I have a few slides that kind of bring together some of the conclusions from Sequoyah.

In order to provide some context for how these results compared to other things, back in the initial best-estimate analyses of Peach Bottom and Surry we had charts in which we compared the source-terms to the siting study from 1982, which is one of the historical reports that was commonly used beyond its original intentions. And so, one of our objectives was to compare to this.

Another point of comparison that we had previously made was to the NRC Safety Goal QHO. We

have removed that. We recognize the limitations of that and have not done that.

And so, we have some comparisons here. We recognize there are a lot of caveats. There are different assumptions, different frequencies associated with them. But, nevertheless, we wanted to answer some of the questions we got about, oh, what does this mean compared to some of the other things?

So, on slide 120, this is iodine release for individual cases from the different Surry, Peach Bottom, and Sequoyah scenarios. And then, of course, at the top in red is the siting study source-term one. That one had a one-and-a-half hour starting time of release and 45 percent stop time two hours later.

MEMBER BLEY: We talked early today about problems with comparisons.

MR. BARR: Right.

MEMBER BLEY: But what are showing up here? Are these means or are these your base cases?

MR. BARR: These are base cases.

MEMBER BLEY: Why, when that's not what

you most believe?

MR. BARR: We would have preferred to be able to show the uncertain distributions for the ones that were available, but this chart would have gotten way too messy.

MEMBER BLEY: No, if you had just shown the means. I mean, if you are going to pick one curve from a study that has got uncertainty treatment, why would you pick the one you can't quite justify as being --

MR. BARR: Right. Well, we wouldn't necessarily had a mean for -- we only would have had a few cases to do that, instead of all of them, since we didn't do an uncertainty analysis for each of the different scenarios.

MS. GHOSH: So, we did one scenario at each plant.

MEMBER BLEY: Well, that's true. You can't do it.

MS. GHOSH: Right.

MEMBER BLEY: You can't do the counts, yes.

MS. GHOSH: We have three examples, but the other ones are --

CHAIRMAN STETKAR: I don't even remember on Surry; it has just been so long ago. It was like three months.

(Laughter.)

What did you do the uncertainty analysis on Surry?

MS. GHOSH: We did unmitigated short-term station blackout, but we specifically put logic and modeling in to look for the in-use steam generator to rupture variation of the short-term station blackout. So, we ended up with, also, a bifurcation where we had about 10 percent of the cases going to tube rupture versus --

CHAIRMAN STETKAR: But the tube rupture cases, I don't remember. Did it have uncertainty propagated through those or not?

MS. GHOSH: Yes, yes, yes.

CHAIRMAN STETKAR: They did? Okay. That's okay.

But you didn't do it, for example --

MS. GHOSH: We're still working on addressing those causes.

CHAIRMAN STETKAR: But, in particular, like the biggest purply thing here, the interfacing

system LOCA didn't have any of the uncertainty, so that becomes troublesome. Okay.

MR. BARR: The point I wanted to make here is that the Sequoyah scenario is, along with the other SOARCA scenarios are well below the SST-1 release.

And now, on the next slide, the difference is even more obvious when we look at the cesium releases. The SST-1 had a 67-percent release starting at one-and-a-half hours. Again, these are the base-case source-terms for SOARCA.

We have, for the Sequoyah short-term station blackout, because it turned out that what we initially had as a base case was an overpressure late failure, we also have on here an example of an early release to show that, too.

Slide 122. Whereas, previously we had frequency weighted the conditional LCF risks and also made a comparison to the safety goal, we have gotten rid of that. There is no frequency consideration here, and we have just plotted the conditional risk of a fatal cancer for someone in the EPZ, assuming that the severe accident happens.

For the scenarios in which we have done

an uncertainty analysis, we have a distribution from the 5th to the 95th percentiles here. For the other ones, we just have a blue square representing the base case.

And this is meant to show just kind of how the Sequoyah results compared to those from the other SOARCA scenarios. One thing we see is that, even with some of the releases being a little bit higher and faster at Sequoyah for the short-term station blackout, they don't really translate into comparable increases in terms of the latent cancer fatality risk. Of course, part of that is driven by the habitability criterion, which effectively prevents people from exceeding annual doses over a certain threshold.

CHAIRMAN STETKAR: On the other hand, Jon -- and Joy brought it up earlier this morning -- if I look at Surry and I look at the short-term station blackout, and I look at the green triangle on that, and I look at the short-term station blackout with SGTR, and I look at the green triangle on that, I see about probably a factor of maybe 20 difference. I don't see any green triangle for short-term station blackout with SGTR

on Sequoyah. So, if it is 20 higher also on that one, it is up in the title of your slide somewhere, right?

MR. BARR: Yes. So, the --

CHAIRMAN STETKAR: So, my whole thing is, why am I comparing these things and saying, "Oh, look, they're all within kind of 10 to the minus 4ish."? Well, they might not be if you compared them all.

MR. BARR: We have looked at source-terms that are larger, and we have seen that those have been translated in terms of the LCF risk.

Another point of comparison was, from the containment protection and release reduction rulemaking work, we look at some of the highest conditional in cancer fatality risks there. The highest one we had was 2 E to the minus 3. So, we are seeing something that is fairly consistent, that even with some higher source-terms, they are really ending up primarily in this 1 E to the minus 4 to about 1 E to the minus 3 range.

Do you want to add something, Hossein?

MR. ESMAILI: Yes. I just want to say regarding the source-term that we saw, when you

look at the short-term station blackout with and without steam generator tube rupture, you see this big difference. But when you go look at the short-term station blackout for Sequoyah, I don't know what it is going to be with the steam generator tube rupture. I just personally do not expect it to be as different as we see in Surry.

The reason is that in the Sequoyah some of it is driven by the early containment failure. So, basically, a lot of cases we have seen that the containment has failed, even during a core degradation. Basically, you end up with containment not being at play here in terms of the source-term.

So, it would be somewhat like you are releasing -- you know, taking account for the steam generator, how much decontamination you get to the steam generator. It is substantially different. In the Surry station blackout without steam generator tube rupture you are limited by how this containment pressurizes, how it leaks, and over time. In short-term station blackout for Sequoyah it is completely different. You are losing the containment. You could possibly lose the

containment very, very early. By that time, all the releases are --

CHAIRMAN STETKAR: You could possibly do that, but you said most of the time you don't.

MR. SHACK: The steam generator tube rupture was about like the release we got from the containment failure. So, it may double the release.

MR. ESMAILI: Yes, exactly. So, 10 percent of the time you have steam generator tube rupture, right? Here we have 20 percent of the time we have early containment failure. So, I am trying -- it is not exactly. I'm just saying that, if you add steam generator tube rupture to it, it might shift this. It is not going to be as pronounced as the, say, station blackout with and without steam generator tube rupture.

MS. GHOSH: Well, right, because we didn't have that early containment --

MR. ESMAILI: Right. That early containment failure is a bad test.

MS. GHOSH: Yes. Yes.

MR. SHACK: I mean, once you have bypassed, you've bypassed.

MR. ESMAILI: That's right.

MS. GHOSH: Yes, yes.

MR. ESMAILI: That's right. So, that is the difference.

MR. OSBORN: This is Doug Osborn.

Also, keep in mind this is the EPZ. So, this also incorporates evacuations, evacuation cohorts. As we typically see, the evacuations work. So, the vast majority of this latent cancer fatality risk is the result of long-term exposure. And again, going back to habitability, Peach Bottom has a different habitability criterion than Surry and Sequoyah do as well. So, those are nuances that also need to be taken into consideration.

CHAIRMAN STETKAR: And they probably had different population densities and all kinds of things, which is why I'm really upset about trying to make these grandiose comparisons about look what we've learned.

MR. BARR: Absolutely.

MR. ESMAILI: But we know there is a big difference between how the containments respond Surry and Sequoyah.

CHAIRMAN STETKAR: That's good we

learned that.

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: But publishing comparisons like this, the public will pick it up, especially if you make all of these, "Look at what we've learned about our insights." The public doesn't understand all of the nuances that we have spent the last "N" hours of our day discussing here. They just don't.

MR. BARR: Slide 123. This is a list of some of the overarching conclusions.

For the unmitigated short-term station blackout in which we have no backup power for igniters, we see potential containment outcomes, either early failure or late failure. If we have successful use of igniters, then that can prevent the early failure. However, the late overpressure failure is still the likely potential.

MR. SHACK: I was a little surprised you didn't do more with the igniter system. You did that worst-case kind of realization thing, but, then, these things interact in such a way that I am not convinced or I can't quite convince myself that that really demonstrates to me that, if I had

igniters available, I would always avoid early containment failure.

MR. ESMAILI: Within the modeling capabilities that we have, this is what we are going to see. One thing we could do -- and you look at the short-term station blackout, you see that timing there is very, very short. So, that igniter thing, we assume it is available. As soon as the hydrogen comes out, it is available. We could have done some sensitivity saying that, okay, maybe they cannot get the igniters on by 2.7 hours, but if it comes up at three hours, what happens? I mean, we know that this time is very, very short. If you cannot get it to operate within 2.7 hours, it is possible to lose the containment in 2.7.

So, as far as I'm concerned, I think we have drafted the insights that we have patterned. This is the only sensitivity, and I don't know --

MR. SHACK: Okay. Okay. That's a good argument, that if you assume it's always available, you get one answer. If you delay that, you're back to your --

MR. ESMAILI: That's right. So, you just make sure that, you know, this is as fast as

the hydrogen can come out and this is how it can burn. And I could not tell you whether you are going to get hydrogen combustion within this -- you know, when I am going to get it.

MR. SHACK: Yes.

MR. BARR: When we compare the two sets of uncertainty analysis for the unmitigated short-term station blackout with and without random ignition sources, we see that the effect of the random ignition sources is that it delays early containment failure. It is still potential, but in many cases it is delayed. The ice is effective in mitigating the soil containment pressurization and can delay potential containment failure.

From the offsite consequence analysis, we continue to see essentially zero individual early fatality risk. And even for cases in which we have an early release to the environment coincident with the evacuation process, the conditional latent cancer fatality risks are small. From the last slide, we saw that the conditional individual latent cancer fatality risks are comparable to those from the other SOARCA analyses.

MS. SANTIAGO: So, I just want to make

a few statements, but I did want to thank the Committee for the detailed review that they gave to this document. It is very complex, and the staff has worked very, very hard, both NRC and the contractor. So, I especially also want to thank them again.

I would also list to just thank you for your biannual review that you provided a week or so ago. In that review, you recommended that we continue to improve the codes. And I think we mentioned today over the course of the last couple of years, since the original SOARCA pilot studies in 2012, we have done some improvements to those codes. And, of course, we continue to do that as we get additional information, either from Fukushima or other studies that are ongoing internationally or even within our own agency.

This analysis is important, and the methods that we use and the codes that we use in these analyses support key agency safety and security activities.

On the next slide -- and we have talked about many of these during the course of this discussion -- more recently was the containment

protection and release reduction technical analysis that we did. MACCS is currently being used to support the review of the economic cost/benefit analysis that is performed for regulatory analysis, and we will be updating guidance with the staff there.

The other thing that ACRS recommended in their program review and stated is that the severe accident research program is an essential element of the decisionmaking process for severe accident issues related to existing and future nuclear reactors, including certification of a new standard plan design.

So, I think Dr. Bley mentioned at the beginning, are we going to talk a little bit about the future analyses? And we had a little, short conversation with Dr. Stetkar a month ago in preparation for this meeting. We had said we were looking at including a review of new, small modular reactor designs due to the single and multiple module issues that affect accident progression mitigation options, emergency planning, and offsite consequences, especially in light of questions that we are getting from some recently-submitted topical

reports.

Recently, we reviewed a topical report for NuScale. As an example, we had a question as to what do we need as far as protective actions. So, we were considering doing a consequence analysis to better understand that and be able to be positioned to answer those types of questions when the applications come in.

The other one that we were looking at possibly doing a consequence analysis on was the AP-1000. Much of that was because of the passive-type systems that those designs have. Recently, Chairman Burns mentioned that there were some quality issues in some of the subcontractor modules that need to be examined. And it is critical who's passive systems. So, with that, so is our understanding, so that we can know what the different parameters are that might be most influential in any of these severe accidents.

On the next slide, I just wanted to talk a little bit about knowledge management.

CHAIRMAN STETKAR: Let me interrupt you for a second.

MS. SANTIAGO: Uh-hum.

CHAIRMAN STETKAR: On both SMRs and whether it is NuScale -- you know, you mentioned it -- or similar design or AP-1000, I think that SOARCA in the sense of an integrated uncertainty analysis might be very useful. Some sort of nominal case probably would not be all that useful because on those passive designs a good evaluation of the uncertainties in the behavior of the heat removal mechanisms would be very useful, some sort of nominal snapshot of "I assumed this value" is probably counterproductive on those.

Certainly, if you think about moving forward into either of those areas, don't try to do a quick-and-dirty with just some sort of point estimate value that is given some name to it. You know, you are going to have to do the uncertainty analysis because I think the uncertainties -- we've commented previously that the uncertainties in some of these passive cooling mechanisms might be a heck of a lot larger than people like to think they are.

MS. SANTIAGO: Thank you. And nothing we do is quick and dirty when we talk --

(Laughter.)

CHAIRMAN STETKAR: No, no. But as long

as you've got to spend a lot of time and effort, don't do it on just single value.

MS. SANTIAGO: Yes.

On the next slide for the knowledge management, we clearly had been providing information to the different classes over at RPBC on SOARCA, so that they can actually talk to many of our staff to better understand the accident scenarios in our consequence analyses.

Your program review also talked about continuing knowledge management and the core capabilities and competencies. I have to say, five years ago or 2010 when I came, we had a limited capability, at least in the MACCS area. MELCOR may be a little bit more. But in the last five years we actually have been using these analyses to train new staff and actually bring in more staff to better understand the codes and some of these consequences and phenomena.

And you can't just develop these core capabilities in the midst of an emergent need. You really have to maintain these core capabilities. So, I appreciate that.

We also had a series of seminars that

we were providing the staff on accident progression. And now that that is concluding, we are now developing the consequence and the uncertainty aspects to complete the entire series in the severe accident analysis arena.

Now, on the last slide, I will talk quickly about our schedule that we had originally come into today with. We were still addressing many office comments, but primarily those were clarifications that we were going to add to the report. So, I will discuss the public comments and, then, again, those are clarifications we felt we could add to the report.

We thought we would be able to take comments from today's meeting and modify the report and come back in June to the full Committee to make a final presentation. However, I think there has been enough discussion today that, clearly, we need to definitely make several clarifications based on your comments. There probably is additional explanation that we want to add in. Sometimes we were referring to Surry. Maybe we just need to pull more information into this report that fully explains what we did and why. I think there may be

another parameter with an uncertainty analysis that we may want to consider doing in that regard, too.

You gave us a couple of questions to go back and check with TBA to make sure we had some full information.

We also want to perhaps look at whether or not that Cohort 4 should be 40 percent, 20 percent. I mean, if you have a suggestion on this --

CHAIRMAN STETKAR: Pat, honestly, in summary -- well, we will wait and we will go around the table --

MS. SANTIAGO: Okay.

CHAIRMAN STETKAR: -- so you hear our kind of summary comments.

MS. SANTIAGO: One of the questions next would be, we have an original date to the EDO to provide the Sequoyah pilot analysis in September. If we want to do this additional work and make sure that we have captured everything, we may want to just come to you in June and say to you what we will be doing.

We would like a letter and we ask for a letter from the full Committee for this report to

accompany our package to the Commission. So, any suggestions as far as our schedule would be much appreciated.

CHAIRMAN STETKAR: Okay. Let's talk about that just a little bit. Hearing what you just said right now and kind of watching body language, it is not clear to me what benefit would accrue from coming to the full Committee in June, because it may be premature. It sounds like it would be premature to potentially generate a letter from the full Committee, and simply briefing the full Committee about, "Gee, we heard from the Subcommittee and we're planning to make a lot of changes" -- I don't know what "a lot" might mean -- but "We are planning to make some changes," may not necessarily be a productive use of our joint time and the resources that you put into preparing for a full Committee.

I don't know if any of the other members have any comments in that regard and we certainly should air them.

MEMBER BLEY: I'm not sure what you can do about the schedule you have. If you really want a letter from us in June --

MS. SANTIAGO: We wouldn't expect a letter in June.

MEMBER BLEY: Okay. I was going to say --

MS. SANTIAGO: We would actually revise the report and submit it to you probably in the September timeframe.

MEMBER BLEY: Well, then, I don't see a real reason for the full Committee meeting in June.

MS. SANTIAGO: Right.

CHAIRMAN STETKAR: We can do planning offline. I have absolutely no idea -- and you may not have any idea -- of what you can accomplish in what sort of timeframe. We may want to think about having a short Subcommittee meeting because we don't have the time in the full Committee to certainly think about and discuss fully what I would call substantive changes that you are going to make. We just don't have the time to do that in full Committee meetings.

MS. SANTIAGO: Right.

CHAIRMAN STETKAR: So, we can work out those details around our schedule. You are aware of our schedule, that we don't meet in the

summertime and we're already pretty full. But we can work that out with Hossein.

So, I think unless any of the Subcommittee members, or the remnants, strongly feel that you ought to come to the full Committee in June, I think we just postpone it.

It is important to us because I don't know if our Federal Register notice is out yet on our June full Committee meeting agenda.

MR. NOURBAKHSH: We can check that. We can revise it.

CHAIRMAN STETKAR: We can always remove things, but it is better form to not have it on it initially.

MR. NOURBAKHSH: I checked with Mark during the break. It is possible to change the schedule tomorrow.

CHAIRMAN STETKAR: Okay. Tomorrow?

MR. NOURBAKHSH: Yes.

CHAIRMAN STETKAR: Yes, because we're really close already. So, let's just decide to do that.

MEMBER BALLINGER: Are we going around the table yet?

CHAIRMAN STETKAR: No, not yet.

MEMBER BALLINGER: Okay.

CHAIRMAN STETKAR: Not yet. I was just looking right now because we are sort of making a decision about not coming --

MEMBER SKILLMAN: I concur with not.

CHAIRMAN STETKAR: Good. Seventy-five percent vote is good enough.

(Laughter.)

MEMBER BALLINGER: Being 25 percent of four, I'll agree as well.

CHAIRMAN STETKAR: Wow, it's unanimous.

Did you have anything else you wanted to say, Pat? Because, as I said, we can work out internally --

MS. SANTIAGO: Yes. No, I appreciate that. I was going to ask if there was anything else in the report that we didn't cover in the slides that you wanted to ask us about.

CHAIRMAN STETKAR: You covered everything. I mean, I wish we had had a couple of more hours because I wanted to whine about some of the things about moving the cohorts. I sort of telegraphed it to get it on the record. But some

of the times and rationale -- Cohort 1, the school children, was described and hung together the best of all of them. As I went down through the cohorts, there seemed to some internal inconsistencies about assumed evacuation speeds and delays, given the rest of the story. So, just think about that. I don't know if it makes much difference, but that's about the only area that I didn't really have a chance to kind of whine about specifics.

Before we go around the table and give the members a chance to kind of collect your thoughts, I do need to put the bridge, whatever is not open, to open on the bridge line.

While we are doing that, I will ask if there is anybody in the room that has any final comments to make. Come on up and identify yourself and do so.

And we will open up the bridge line for any comments from the public who are out there.

Is it Open?

Someone from the public who is out there, just do me a favor and say "hello," so that we can confirm that the bridge line is open.

MR. SCHULTZ: Hello.

CHAIRMAN STETKAR: Thank you.

Now, if there is anyone out there who would like to make a comment, please identify yourself and do so.

MR. SCHULTZ: John, this is Steve Schultz. I'm waiting for others, but --

CHAIRMAN STETKAR: Hi, Steve.

MR. SCHULTZ: -- I'll provide some comments for you if there are no other members of the public who would like to --

CHAIRMAN STETKAR: Keep speaking.

MR. SCHULTZ: Okay. Yes, my comment, I really enjoyed the presentations today as well as the report. My comments are really focused on Pat's recent comments related to what is going to happen going forward with regard to the report and the documentation.

The report really documents extremely detailed technical analyses and results and captures the analysis methods, models, analysis, results in great detail. But the detail does tent to mask some of the important results and conclusions that are drawn from the study. I think

this may have affected some of the public comments that have been received.

For example, in the offsite consequence analysis, there are several conclusions within the text of Section 6, they are really embedded within the description of analysis assumptions and results. There is a summary section provided, but it is relatively cryptic and doesn't really describe all the results and conclusions, but, rather, presents a qualitative summary of approach and results.

So, I think the document could well benefit from going through and trying to pull out information that is really the key results of the overall sections of evaluation and getting those in some form that is better presented for overall readability, not only capture it for the public, but also technically.

I also noted that the Executive Summary has got information within it that it may come from the document, but I think there is some amplification of what is in the document technically that is in the Executive Summary. I know that there are figures derived from tables,

and so forth. I would recommend that information in the Executive Summary be provided by that which is based upon documentation of conclusions in the document itself.

With regard to the technical discussions that we had just briefly on the steam generator tube rupture for Surry as translated to Sequoyah, I think it would be worthwhile, if there is time available, to at least develop a technical discussion or argument that is documented within the report to address that in some way.

And that's what I have for today.

CHAIRMAN STETKAR: Thanks, Steve.

Anybody else out there on the bridge line who would like to make a comment?

(No response.)

Hearing none, we will re-close the bridge line, so that we get about half the pops and crackles taken care of.

(Pause.)

(Laughter.)

CHAIRMAN STETKAR: Okay. I fear we are going to be accused of being anything other than a jerk. That's on the record now.

Now let's, as we always do, let's go around the table, and see if any of the members have any final comments. But I will give our consultant the first shot at doing this.

So, Bill, do you have any comments in closing?

MR. SHACK: I just want to say I thought it was very good presentation today. I do think this is one case where the uncertainty analysis really was necessary to even get a real picture of where things are -- you know, there's enough interacting things going on here, that picking any one set of values really gives you a distorted picture of what is going on.

My one technical criticism is the one that I made earlier, that it seemed to me that the hotleg or I guess the overall depressurization of the reactor vessel system, which is mostly hotleg failure, is an important enough variable and is really an uncertain enough variable that that uncertainty ought to be addressed in some way. Exactly how, I don't know.

CHAIRMAN STETKAR: Uniform distribution between zero and one somehow.

(Laughter.)

MR. SHACK: And you know, on the whole evacuation scheme, I think that is technically the hardest to justify. The uncertainties are large. I looked at the speeds that they assumed. They seemed very low, but, then, you come to the somewhat arbitrary 40 percent figure. It is a difficult problem is sort what I meant.

Well, I'll get my shot in on the steam generator tube rupture, too. I mean, it makes a difference of a factor of about 40 whether you have a steam generator tube rupture or not in terms of the release. I don't think you are going to get any more release from a steam generator tube rupture at Sequoyah than you do at Surry. So, it is going to maybe double the release. And so, I think it is the factor of the 40 in the source-term versus the factor of two. I was most interested in the containment behavior, and that is really what was addressed here.

But, again, I enjoyed reading it just simply because this really was one where you really had to have the distributions in order to make sense of it all.

CHAIRMAN STETKAR: Turn your microphone on.

MEMBER BALLINGER: Better you didn't hear what I just said.

(Laughter.)

I have gone through three of these now. Remember, I'm the metallurgist. Each one is on an average of 450 pages, the Surry and the Peach Bottom ones. And each one of them has a very large -- there are differences between them. They cover different areas, but they are all very important.

And I wonder whether or not it wouldn't be useful to produce a document which summarizes those three. I know that might be heresy, but, anyway, that is from a metallurgist.

MS. SANTIAGO: We actually were trying to do that, and that is on our schedule to extend --

MEMBER BALLINGER: So, we have two heretics in here.

(Laughter.)

Going through this, and apart from what Bill was saying, I was interested in Figure 6-6 through 6-10, which were the comparison, depending

on the dose model that you use. Pretty striking. Pretty striking. That wasn't discussed.

Other than that, I appreciate the presentation. It is a long one, but very thorough. I didn't pick up on the nuances that our other colleagues have, but I'll remember them, too.

Thank you.

CHAIRMAN STETKAR: Dick?

MEMBER SKILLMAN: Thank you, Mr. Chairman. Just one or two comments.

First of all, I want to commend staff for spending so much time to get into the very fine details of the uncertainty in the technical side of this and, also, on the evacuation side of this.

I will make the same comment -- I carry with me the logbook page of the TMI 2 accident, the people who were on watch at four o'clock in the morning and who relieved me, and it happened later in the day. It just intrigues me that at about 1400 there was a reactor building isolation valve. About 10 hours later, I had to look at your slide 80, and it takes about 10 hours for that hydrogen. This was a valve that failed to close and the core relieved, and the manhole cover blew out like it

was supposed to, and the hydrogen was breathed into the building. The core was failed at 140 minutes. But it is very well-duplicated in your information.

I would like to make a comment about the cohorts. And here's what I'm thinking about, and I want to give an example that will rivet your attention, but it will make my point.

Remember the first images of Sandy Hook. You saw law enforcement and you saw the parents. The same thing at TMI; the parents didn't evacuate, but they went right to school or to the bank, those two.

(Laughter.)

I'm serious. I'm serious.

That is, mothers and fathers that go to get their children; they have mothers and fathers out of healthcare or the hospital, and they go to get mommy because they know they have got to leave.

I just wonder how accurately that type of thinking is in the cohorts. And there is a good example for those of you who might remember. There was a terrible fire at Wilkes-Barre. It was a metal fire. And the Wilkes-Barre/Scranton Emergency Planner said, "What do we do now?"

because downwind of that fire was so dangerous. And they enacted the Susquehanna steam station. They activated the emergency plan and they implemented the plan for the first responders for emptying the nursing homes, evacuating the schools. And it worked quite well. That was not an earthquake, as John and Dennis have pointed, a massive earthquake like we have never lived through before.

But I am just wondering if the cohort identification is as accurate as it needs to be or if there are some transportations and combinations that would give perhaps a clearer vision of what might happen if we really did have to go to battle stations on one of these plants.

Thanks very much.

CHAIRMAN STETKAR: Dennis?

MEMBER BLEY: No issues to add. I appreciated all the discussion and got a lot of explanations that helped me understand what I couldn't understand from just reading text. So, I hope the text gets fixed up, so people can see that.

The things we talked about earlier

still stand.

CHAIRMAN STETKAR: I will reiterate, in terms of, if -- and I'm not -- if I were sitting upfront thinking about what I look at between now and some other incarnation, I would look real, real strongly at the safety valve data, real, real strongly, both the failure rate, lamda, and the uncertainty distribution for the open fraction. That is so doggoned important that you need to make sure you have an ironclad story about it.

And as I said earlier, it is important for the credibility of this standalone stuff and, in principle, Surry because it was also identified important there. And we've got to get it right on the Level 3 PRA because we can't afford to have a couple of orders of magnitude different of something that might be fundamentally very important to estimating offsite consequences and accident progression. So, look at that.

And we have already heard that the cohorts, that is the other thing. Think carefully about those.

Staff did a great job presenting. I didn't think we had a prayer of getting out of

here. I thought I would be cutting us off well before we got through the end and still leaving now. So, you did a great job presenting a ton of information, and coherently, and answering the questions. So, I really, really appreciate the discussion we had.

With that, we can go home. We're adjourned.

(Whereupon, the above-entitled matter went off the record at 5:22 p.m.)

State-of-the-Art Reactor Consequence Analyses (SOARCA) Project: Sequoyah Integrated Deterministic and Uncertainty Analyses

ACRS Subcommittee Briefing
May 19, 2016

Patricia Santiago, Chief
Accident Analysis Branch
NRC Office of Nuclear Regulatory Research

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Outline

Overview and Accident Scenario Development

Sequoyah MELCOR Model

MELCOR STSBO Uncertainty Analysis

MELCOR Select Individual Realizations Analysis

MELCOR LTSBO Analysis

MACCS Offsite Consequence Analysis

MACCS Uncertainty Analysis

Summary of Public Comments

Overall Results and Conclusions

Uses of SOARCA and Next Steps

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Protecting People and the Environment

Sandia National Laboratories

Overview

Jonathan Barr
Accident Analysis Branch
NRC Office of Nuclear Regulatory Research

Background

SECY-12-0092: SOARCA - Recommendation for Limited Additional Analysis

- Surry UA for a severe accident scenario
- Station blackout (SBO) consequence analysis of an ice condenser containment

Staff Requirements Memorandum: Analyses should complement and support:

- Level 3 PRA project
- Fukushima Near Term Task Force (NTTF) activities
    - 5.2 (reliable hardened vents for containment designs other than Mark I and II)
    - 6 (hydrogen control and mitigation inside containment or in other buildings)

Objectives

Develop body of knowledge on the realistic outcomes of severe reactor accidents

Incorporate plant improvements not reflected in earlier assessments

Incorporate state of the art modeling (MELCOR/MACCS)

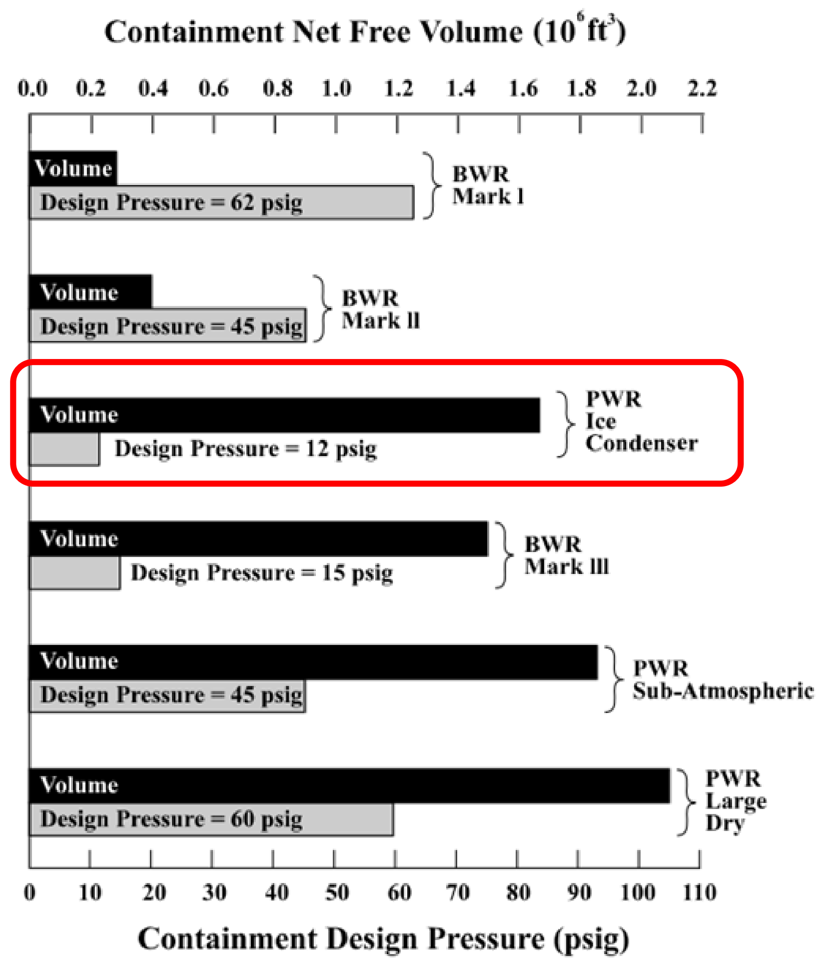
Enable the NRC to communicate severe accident aspects of nuclear safety to diverse stakeholders

Update the quantification of offsite consequences found in prior publications

Ice Condenser Containment

Relatively low design pressure and smaller volume leads to potential susceptibility to early failure from hydrogen combustion in a station blackout

Analyzed in Generic Safety Issue program (GSI-189)



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Sequoyah SOARCA Approach

Focus on issues unique to ice condenser containment

Use latest version of codes
- MELCOR version 2.1
- MACCS version 3.10

Consider latest plant- and site-specific information available including:
- Core inventory
- Population
- Emergency response

Integrate consideration of uncertainty into accident progression and consequence analysis

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Sandia National Laboratories

Accident Scenario Development

Tina Ghosh, PhD
Accident Analysis Branch
NRC Office of Nuclear Regulatory Research

Accident Scenario Development

Scope limited to station blackouts (SRM-SECY-12-0092)

NRC's Standardized Plant Analysis Risk (SPAR) model for Sequoyah and consideration of external hazards informed the identification of candidate scenarios

Approximate core damage frequency contribution estimates provided for context only

Sequoyah Accident Scenarios

Initiating event is a beyond design basis seismic event with greater than 0.5 g peak ground acceleration

Two variations of SBO selected:

- Short-term SBO (STSBO): loss of all AC power and turbine-driven auxiliary feedwater pump (TDAFW) not available
  - Estimate contribution to core damage frequency approximately one in 500,000 years of reactor operation
- Long-term SBO (LTSBO): loss of all AC power and TDAFW initially available but fails after batteries deplete
  - Estimate contribution to core damage frequency approximately one in 100,000 years of reactor operation

Key Assumptions

Assume that the steel containment, containment isolation systems, and ice condensers survive in both STSBO and LTSBO

In the STSBO, TDAFW is not available due to:

- Component failure (e.g., batteries/DC power),
- Failure to manually open the steam valve to TDAFW, or
- Failure to establish a water supply
- Do not credit human actions

In the LTSBO, TDAFW is initially available

Key Events - Boundary Conditions for MELCOR Unmitigated STSBO

LOOP initiating event: a beyond design basis earthquake, occurs.

The reactor is tripped.

The EDG, emergency AC, will receive start signal but will fail.

The reactor coolant pumps (RCPs) will coast down and stop within a few minutes and all other pumps normally running (e.g., chemical and volume control system (CVCS) charging pumps) stop due to loss of AC power.

Valves designed to fail-close, close to isolate systems and the containment (e.g., MSIVs).

Emergency AC power not available due to SBO, therefore Motor-Driven AFW (MD-AFW) pumps fail to start.

TDAFW is inoperable.

Safety valves (SVs) on primary and secondary system open to control pressure, which decreases the RCS and SG water inventories.

RCP seals start to leak initially at a nominal rate of 21 gpm.

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Key Events - Boundary Conditions for MELCOR Unmitigated LTSBO

Beginning same as STSBO – reactor trips, no AC power available, valves designed to fail-close, close to isolate systems and containment.

In this case, because DC power is available, AFW system automatically receives a start signal; water supply available.

TDAFW pump automatically maintains water level in steam generators, removing decay heat from core and preventing core damage as long as water inventory is available.

Manual DC load shedding and secondary depressurization is successful. Secondary depressurization stops to maintain adequate steam pressure for the TDAFW.

Battery runs out after 8 hours (i.e., nominally with successful load shedding) and all attempts to restore AC power prior to core damage are unsuccessful (both offsite and onsite).

AFW steam turbine control valve fully opens following loss of DC power and increases AFW injection. AFW steam turbine fails when SG overfills.

The MSL atmospheric relief valve closes following the loss of battery power and the SVs open to control pressure.

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"Mitigated" Accident Scenarios

The only potential mitigation that is modeled is crediting successfully supplying power to the hydrogen igniters

- STSBO sensitivity case, for a realization from the unmitigated set with an early containment failure
- LTSBO sensitivity case, with base case inputs

Sequoyah Scenario Variations

<table>
<tr><th>SBO</th><th>Mitigation</th><th>Variation</th><th>Approach</th></tr>
<tr><td rowspan="3">STSBO</td><td rowspan="2">Unmitigated (igniters not available)</td><td>No random ignition sources modeled</td><td rowspan="2">Integrated UA</td></tr>
<tr><td>Random ignition sources modeled</td></tr>
<tr><td colspan="2">Mitigated (only igniters available)</td><td>Sensitivity analysis</td></tr>
<tr><td rowspan="4">LTSBO</td><td rowspan="3">Unmitigated (igniters not available)</td><td>Hydrogen ignition criteria</td><td rowspan="4">Sensitivity analysis</td></tr>
<tr><td>Battery duration</td></tr>
<tr><td>Safety valve behavior</td></tr>
<tr><td colspan="2">Mitigated (only igniters available)</td></tr>
</table>

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Sequoyah MELCOR Model Overview

Kyle Ross
Sandia National Laboratories

Sandia National Laboratories

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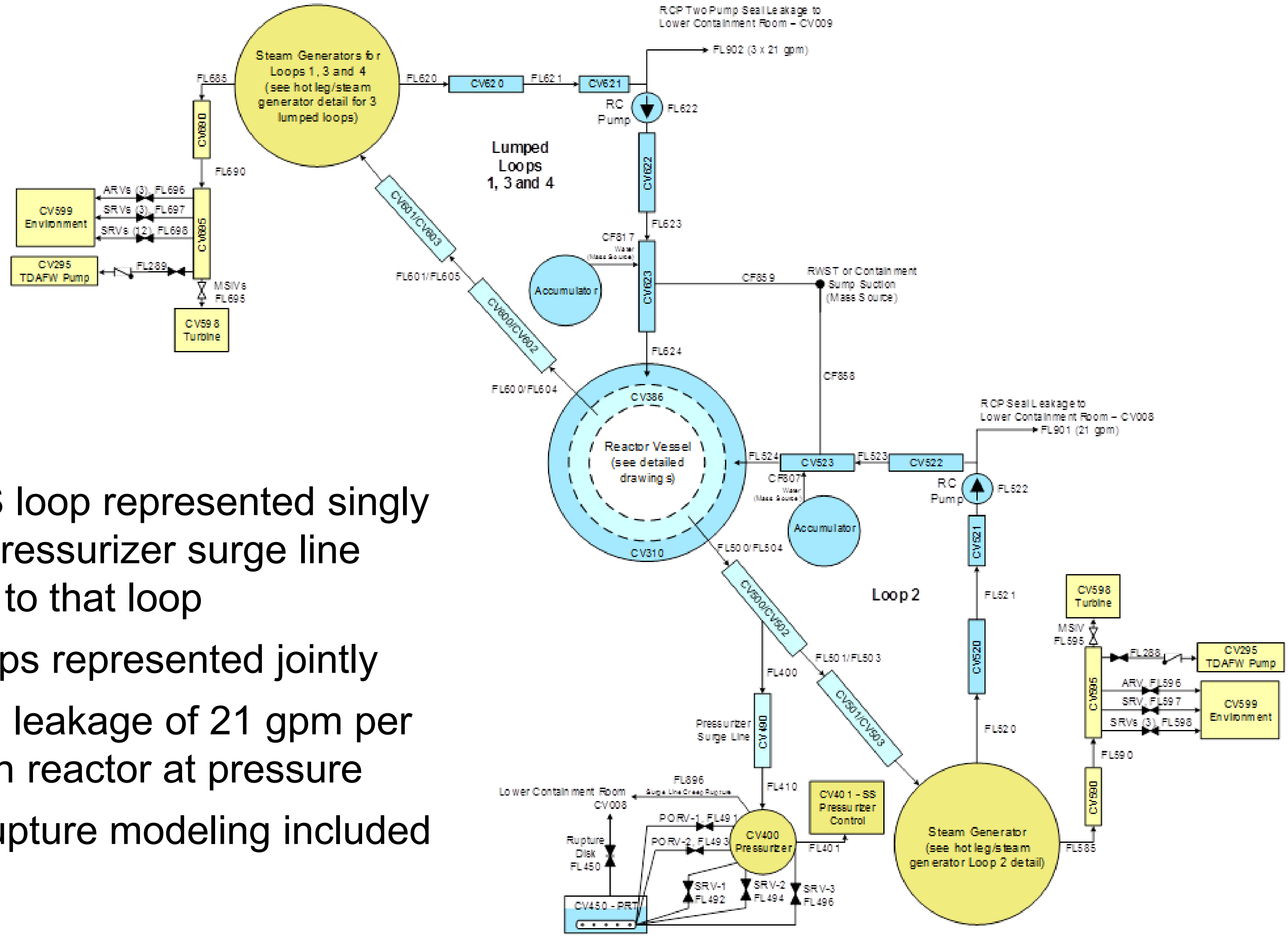
Sequoyah MELCOR Model

- Equally representative of either of the Sequoyah units which are 3,455 MW_{th} Westinghouse 4-loop PWRs with ice-condenser containments
- MELCOR 2.1 general purpose model for transient or severe accident modeling
- Exercised to date for only SBO scenarios
- Fission product inventory consistent with middle of cycle
- Current with respect to steam generator replacement

RCS Modeling



One RCS loop represented singly and the pressurizer surge line connects to that loop

Other loops represented jointly

RCP seal leakage of 21 gpm per pump with reactor at pressure

Hot leg rupture modeling included

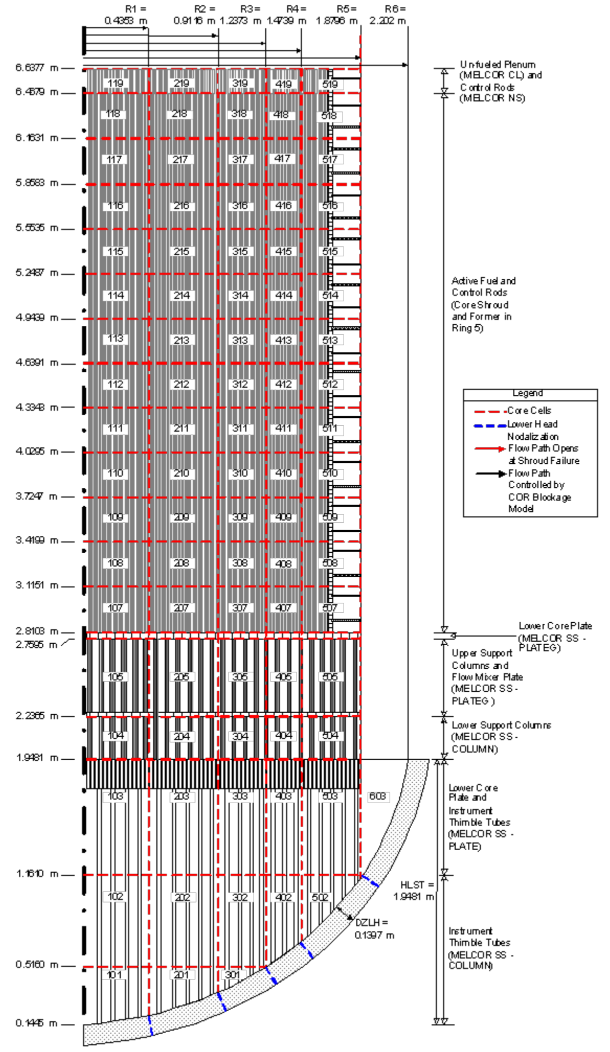
Reactor Core Nodalization

Active core represented with 12 axial COR levels and 5 radial COR rings

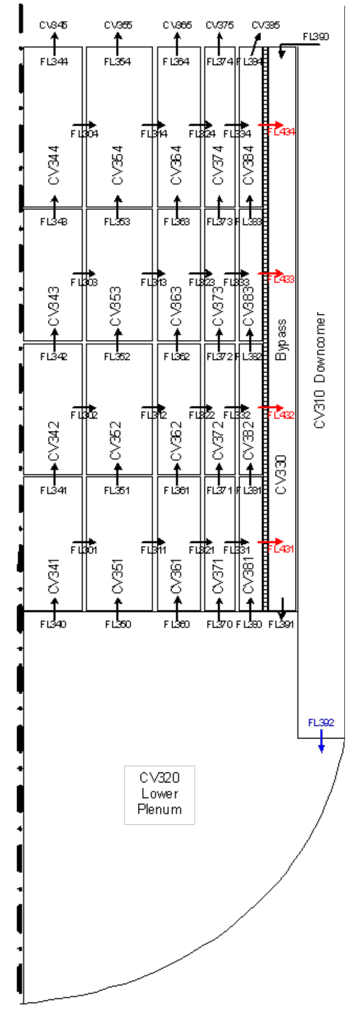
Active core fluid volume represented with 5 stacks of 4 control volumes

Supportive of in-vessel natural circulation

Lower head discretized into 7 segments with 10 radial nodes



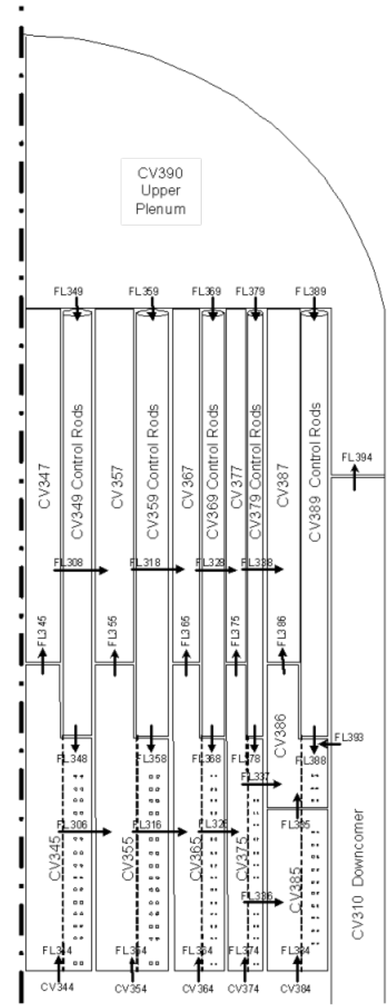
Westinghouse 4-Loop PWR with Ice Condenser Containment Core Nodalization



Westinghouse 4-Loop PWR with Ice Condenser Containment CVH/FL Nodalization

Upper RPV Nodalization

Supportive of in-vessel natural circulation



Westinghouse 4-Loop PWR
with Ice Condenser Containment
Upper RPV CVH/FL Nodalization

Hot Leg and Steam Generator Nodalization

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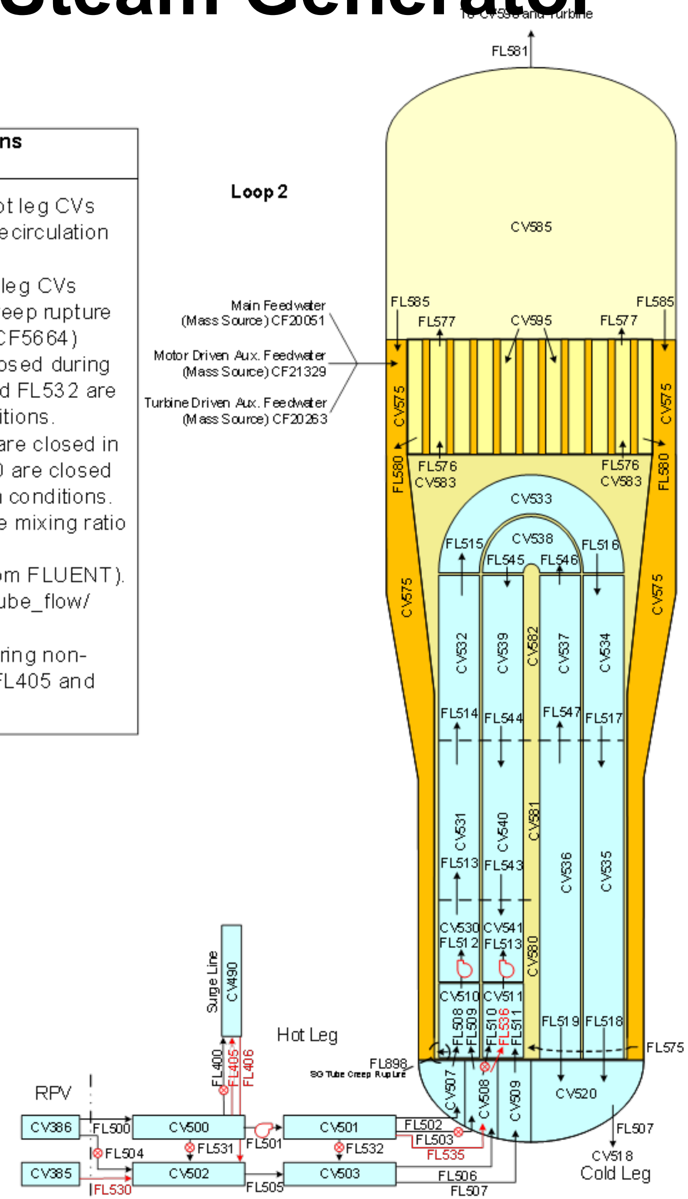
Hot leg and steam generator nodalization supportive of hot-leg countercurrent natural circulation

Natural Circulation Flow Path Definitions
(identified in red on diagram)

*Natural circulation entry conditions defined as (a) hot leg CVs <5% water, (b) >10 K super heat in hot leg, and (c) recirculation pumps tripped.
*Maintain natural circulation flow paths when (a) hot leg CVs <25% full of water, (b) pumps are off, (c) no major creep rupture failures, and (d) loop seal flow is <20% of HL flow. (CF5664)
*FL504, FL531 and FL532 are open and FL530 is closed during non-natural circulation conditions. FL504, FL531 and FL532 are closed and FL530 is open in natural circulation conditions.
*FL503 and FL510 are open and FL535 and FL536 are closed in non-natural circulation conditions. FL503 and FL510 are closed and FL535 and FL536 are open in natural circulation conditions.
*FL535/FL502 and FL536/FL511 are adjusted to give mixing ratio of 20%/80%.
*FL501 pressure drop adjusted to give C_D = 0.12 (from FLUENT).
*FL512 and FL513 pressure drop adjusted to give Tube_flow/HL_flow (M_{ratio}) = 2.
*FL400 is open and FL405 and FL406 are closed during non-natural circulation conditions. FL400 is closed and FL405 and FL406 are open in natural circulation conditions.



Natural circulation, consistent with CFD analyses of NUREG-1922, accomplished with proactive flow path openings/closings and momentum additions

Hot tube modeling, as included in the Surry UA model to best investigate SGTR potential, is not included in the Sequoyah model

Pressurizer SV Modeling

Important what the 3 parallel SVs do as a system more so than what any valve does individually

FTO and FTC possibilities

If a FTO occurs or a FTC but in a mostly closed position, pressure relief transitions from the affected valve to the next set-point valve (State 1 to State 2 for example)

If a FTC occurs, the RCS vents unregulated to containment (State 2 to State 4 for example)

Should all 3 valves FTO, State 5 (no relief) develops

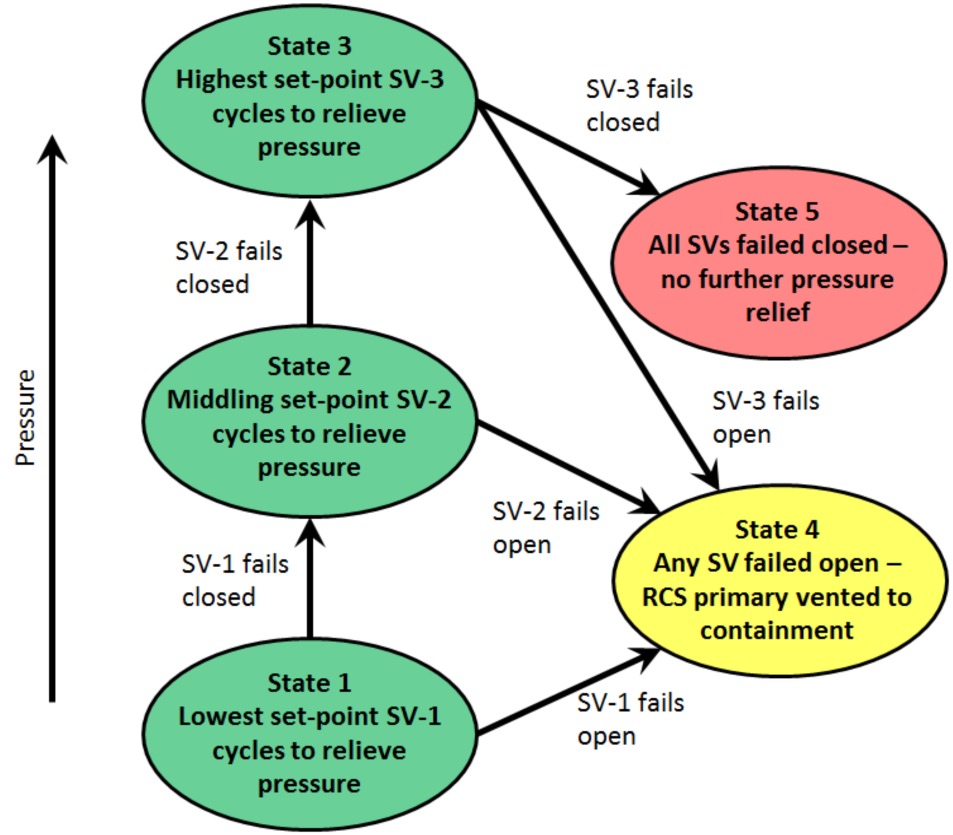
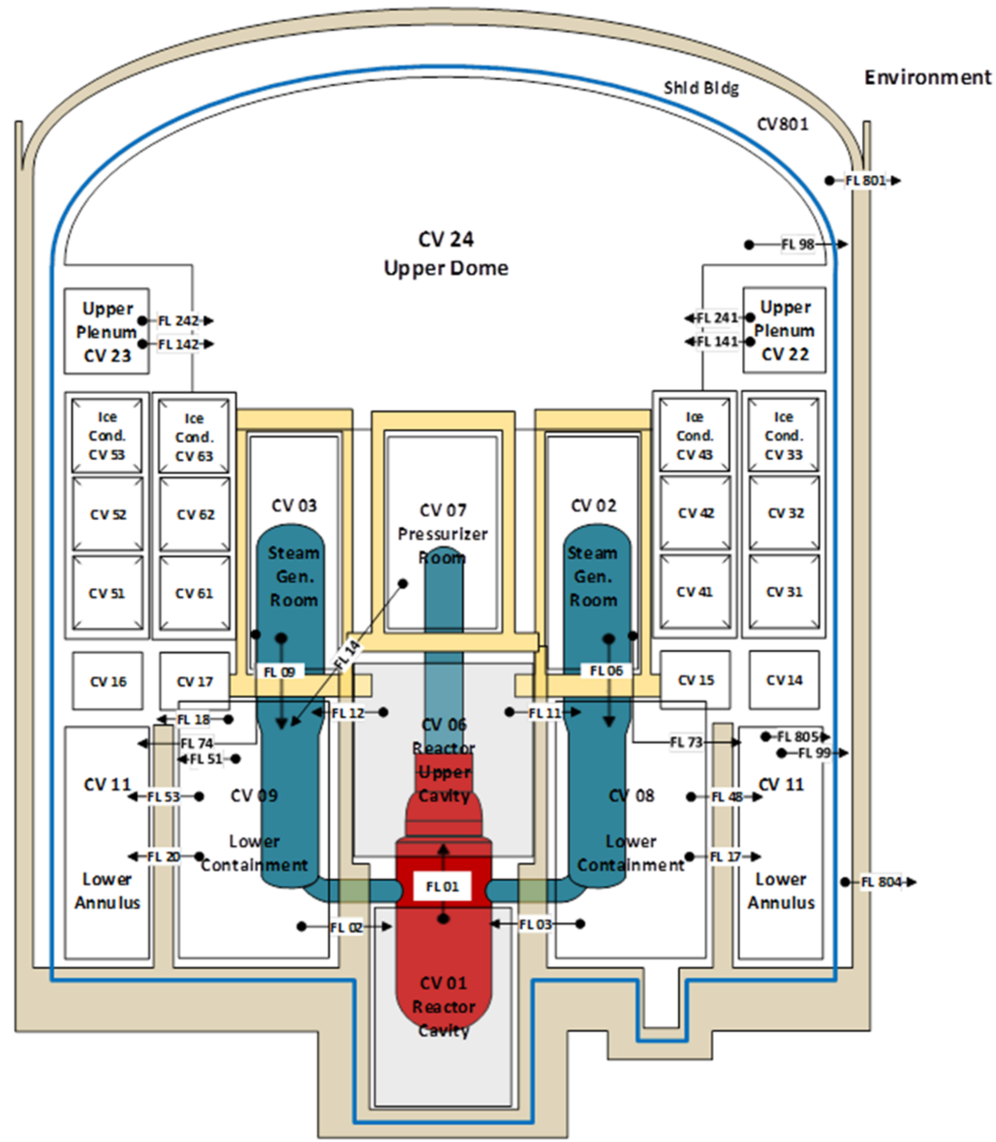


Figure 3-14: Possible transitions in the 3-SV pressurizer pressure relief system considering both FTO and FTC valve conditions

Containment Modeling

- Compartmentalized nature of an ice condenser containment reflected in model nodalization
- Ice chests represented with many control volumes as opposed to a few to allow steam admission to be more localized
- Incomplete sealing by ice condenser entrance doors assumed to be a normal condition
- Failure of ice condenser entrance doors to reclose if opened fully assumed, i.e., a forceful door opening is assumed to damage a door such that it cannot reclose



Containment Modeling (cont.)

Leakage past the free standing steel containment vessel to the annular region between the vessel and the concrete shield building is included (this is a slight leakage)

Overpressure rupture of the steel containment vessel accomplished with a 3-ft^2 hole from upper containment

Shield building failure imposed coincident with steel containment vessel rupture (at the ventilation opening high on the building wall and at the door leading from the auxiliary building)

Containment Modeling – Upper/Lower Containment Barrier Seal

Division between upper and lower containment in overall ice-condenser containment structure is completed by a piecewise rubberized barrier seal designed to limit the steam that can bypass the ice chests in a design basis LOCA

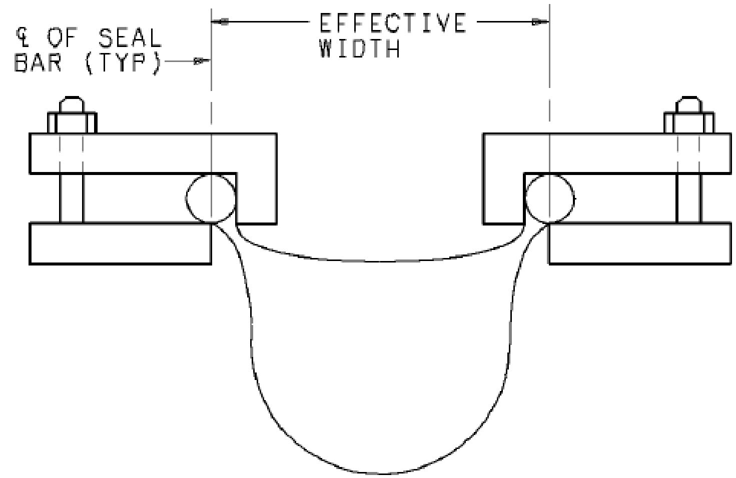
In total, the seal is 464 feet long covering an area of 135 ft^2

12 vertical and 11 horizontal ~3 ½”-wide segments

The seal is intended to maintain its integrity during a DBA for a minimum of 12 hours exposing a leakage area of no more than 0.5 ft^2

Failure of a seal segment could largely increase deck bypass area (5 ft^2 might go to 15 ft^2)

The seal and its failure are represented in the MELCOR model in conjunction with representing nominal deck leakage



DETAIL A-1
TYPICAL CONFIGURATION

Deflagration Modeling – Ignition Sources

In one of the two sets of 600 UA calculations performed, available sources of flammable gas ignition were:

- A hot plume issuing from an RCS breach at a hot leg nozzle or in the pressurizer surge line
  - > 0.1 m/s required
  - > 847 K (= H_2 auto-ignition temperature) required
- Core debris on the containment floor

In the other set of calculations ("with random ignition"), available sources of ignition were the above as well as an occasional momentary spark somewhere in containment characterized by:

- A 1-second duration
- A half-hour frequency
- Appearance in one randomly-chosen control volume on each occasion

Deflagration Modeling – Combustibility Considerations

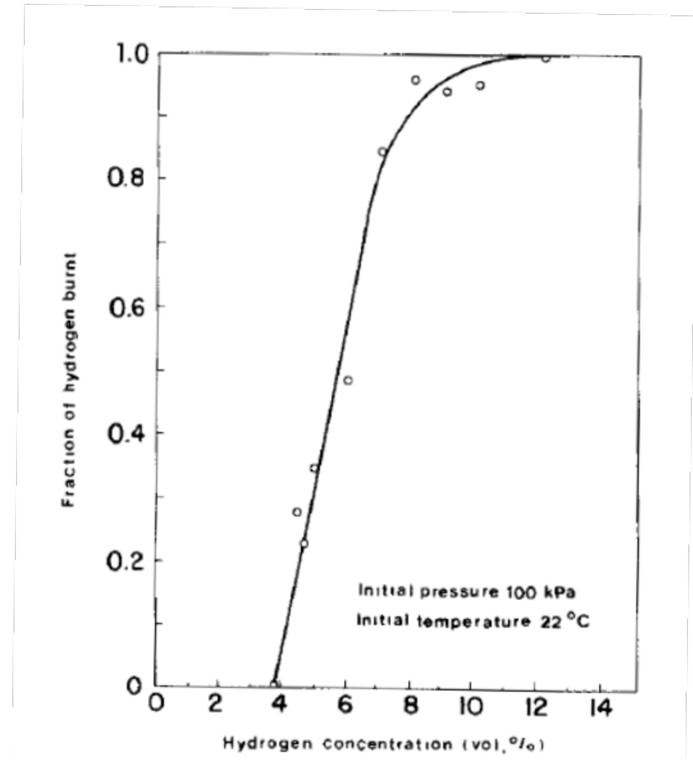
Combustibility is dependent upon relative concentrations of fuel, oxidizer and diluent gasses measured as mole fractions (= ratios of partial pressure to total pressure in an ideal gas mixture)

Too little fuel or oxidizer or too much diluent prohibits burns

The strength of a burn depends on the amount of fuel available to burn that actually burns, i.e., combustion completeness is important

Lesser values of LFL relate to lesser accumulations of fuel at ignition and hence lesser strength burns

Combustion completeness is a function of fuel concentration at ignition



Deflagration Modeling – Combustible Mixture Criteria

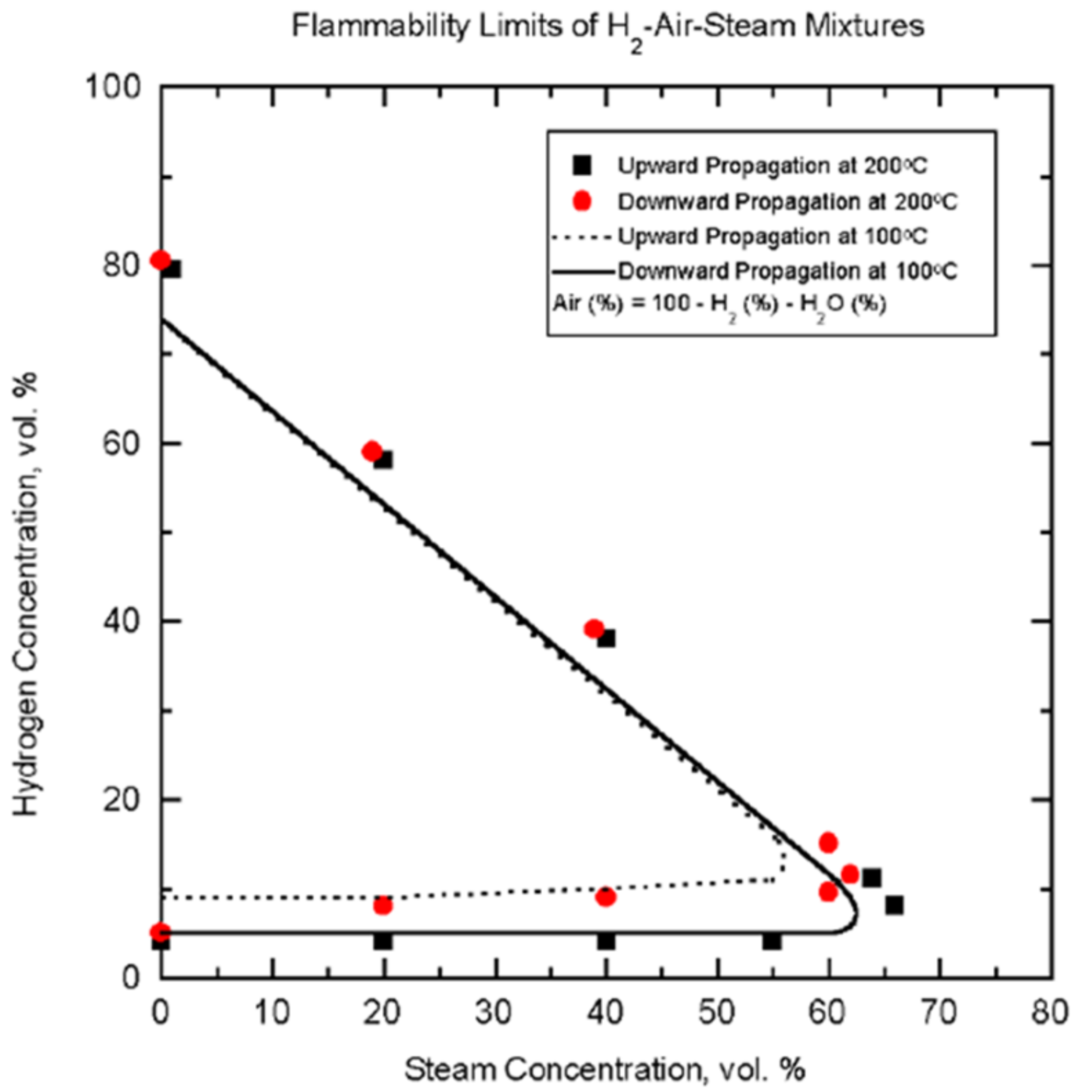
For the origination of a burn, the SOARCA Sequoyah model expands upon MELCOR's default fixed criteria of > 0.10 H_2, > 0.05 O_2 and < 0.55 H_2O to include LFL variability per work of Kumar

For the propagation of a burn from one control volume to another, MELCOR's default fixed criteria of > 0.04 H_2, > 0.06 H_2, and > 0.09 H_2, for upward, lateral and downward propagation, respectively, > 0.05 O_2 and < 0.55 H_2O, were maintained (propagation directionality being user-defined in flow path descriptions)

Provision added for variably defining H_2 LFL in consideration of the direction a burn would need to propagate from its origin: 0.04, 0.06 or 0.09 dependent upon propagation being upward (e.g., from a floor), lateral (e.g., in a horizontal duct) or downward (e.g., from a ceiling), respectively

Deflagration Modeling – Combustible Mixture Criteria (cont.)

- Additionally defined sufficient fuel (H_2 and or CO) to be a function of diluent (e.g., H_2O or CO_2) concentration
- Further included a temperature dependence of 0.005 and 0.01 less H_2 per 100° C for upward and downward propagation, respectively
- Maintained MELCOR default of 0.05 O_2 necessary for a burn



MELCOR STSBO Uncertain Parameters

Kyle Ross
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Uncertain Parameter Selection and Valuation

- Involved SNL and NRC staff having expertise in applying MELCOR and continuous involvement in SOARCA
- Included subject matter experts (SMEs) in review of data and consideration of phenomena
- Systematic considerations of the phenomenological areas of sequence, in-vessel and ex-vessel accident progression, containment behavior, and aerosol transport and deposition were made
- Influence on containment failure timing paramount

Uncertain Parameter Selection and Valuation (cont.)

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- Parameters treated as uncertain in the SOARCA Peach Bottom and Surry UAs were considered for inclusion as uncertain in the Sequoyah analyses
- Parameters excluded if irrelevant or shown to be unimportant in the accomplished UAs unless the determination was made that an ice condenser type of containment could raise their importance
- Additionally, uncertain parameters judged to have had their influences well characterized in the completed UAs (Cs dominant as CsOH or Cs_2MoO_4 for example) were also excluded

Uncertain Parameter Selection and Valuation (cont.)

- A ‘storyboard’ process was implemented
  – Required analysts to document and defend rationale for including a parameter as uncertain and for the shape and bounds of the probability distribution applied
  – Iterative and inclusive of SNL/NRC subject matter experts
  – Intent upon characterizing uncertain parameters with the best defensible basis available
- Uncertain parameters were considered relative to each other for correlative effects - none were identified

Uncertain Parameter Selection – Storyboard Illustration

Parameter Name:	Type of Distribution:
Owner:	
Technical justification for the uncertainties:	
Rationale for type of distribution:	
Were similar or related parameters considered and rejected.	
Graphic: (plot of the distribution)	

Figure 3-12: Parameter storyboard used to capture key information for each parameter investigated

Uncertain Parameter Selection and Valuation (cont.)

Departures from known technical bases were scrutinized (momentary ignition sources random in location for example)

9 parameters wound up being treated as uncertain

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Uncertain Parameter Selection – Chosen Parameters

Sequence Issues	• **Number of cycles to failure to open/close of pressurizer SVs** • **Pressurizer SV flow area given failure to close**
In-Vessel Accident Progression	• **Melting temperature of the eutectic formed between ZrO_2 and UO_2**
Ex-vessel Accident Progression and Containment Behavior	• **Containment failure pressure** • **Ice condenser inlet door stuck position following a forceful full opening** • **Containment barrier seal failure differential pressure and area**
Hydrogen Combustion	• **Flammability (direction of flame propagation from an ignition source)** • **Ignition source availability**
Aerosol Transport and Deposition	• **Dynamic shape factor**

Pressurizer SV FTC

Important what the 3 parallel SVs do as a system more so than what any valve does individually FTO and FTC possibilities

If a FTO occurs or a FTC but in a mostly closed position, pressure relief transitions from the affected valve to the next set-point valve (State 1 to State 2 for example)

If a FTC occurs, the RCS vents unregulated to containment (State 2 to State 4 for example)

Should all 3 valves FTO, State 5 (no relief) develops

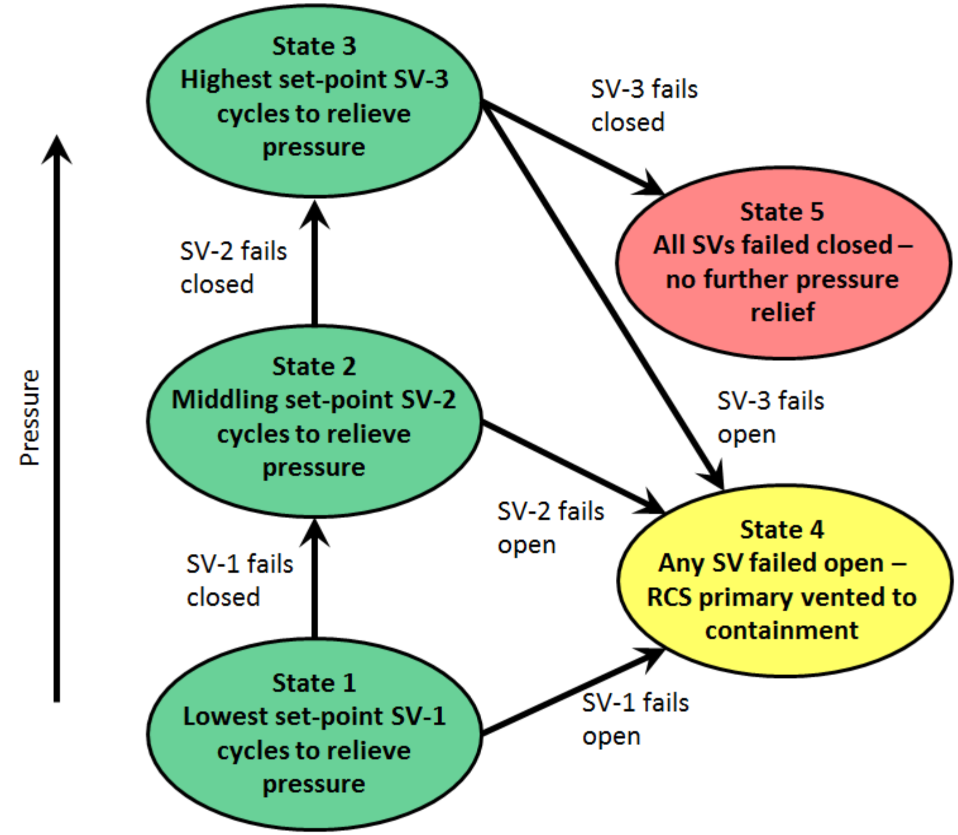

Figure 3-14: Possible transitions in the 3-SV pressurizer pressure relief system considering both FTO and FTC valve conditions

Pressurizer SV FTC (cont.)

Failure probabilities obtained from Table 20, “Failure probabilities for PWR code safety valves (behavior after scrams)”, in NUREG/CR-7037 informed the uncertainty characterizations of stochastic SV failure in the UA

Table 20 reports on SV operation subsequent to actual scram events. Information is included for both main steam system (MSS) and reactor coolant system (RCS) valves

The assumption was made in the UA that MSS and RCS SVs are alike enough in construct and servicing that their failure data can be jointly considered

Subsequent demands were assumed to have the same failure probabilities as initial demands

Recovered valve function, e.g., a previously stuck-open valve closing when pressure reduces, was not taken to be successful valve operation

Pressurizer SV FTC (cont.)

- The Main Steam System Code Safety Valves section of Table 20 reports 0 failures to open and 15 failures to close in 769 demands considering all failures (recovered and non-recovered)
- The Reactor Coolant System Code Safety Valves section of the table reports 0 failures to open and 2 failures to close in 4 demands considering all failures
- Combining the MSS and RCS valve failures identifies 0 failures to open and 17 failures to close in 773 demands
- These failure rates served as the bases for uncertainty characterization of stochastic SV failure in the UA
- Noteworthy with respect to these failure rates is that they are derived from actual events at US plants

Pressurizer SV FTC (cont.)

NUREG/CR-7037 (Table 22) reports on failure rates in SV testing but the rates differ markedly from the rates evidenced by actual plant events suggesting (to the UA analysts) that aspects of the testing were inconsistent with actual conditions experienced by an installed valve, and as such, the testing data was not considered applicable

Pressurizer SV FTC (cont.)

Beta distribution with α = 17.5, = 756.5 used to sample FTC probability

Distribution truncated at 1,000 cycles based on the judgment that an SV could not reasonably be expected to cycle hundred of times without failure

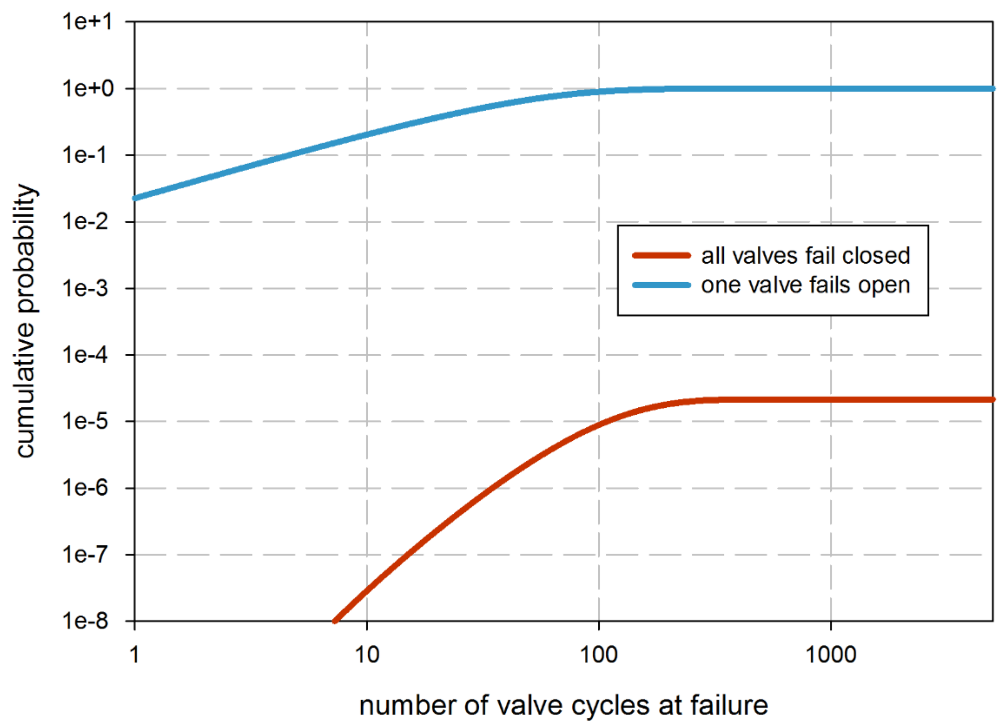

Figure 4-2: CDF for the number of cycles after which any single pressurizer SV will fail open compared to the CDF for the number of cycles after which all 3 valves will fail closed

The non-zero possibility of all 3 valves failing to open was supported in the sampling but didn't materialize in 1,200 trials

Pressurizer SV Position Given FTC (*SV_frac*)

Uncertainty in SV position given a stochastic FTC simply taken to be equally likely anywhere between slightly and fully open

Fractionally-open flow areas were sampled uniquely for FTC conditions of pressurizer SVs 1,2 and 3, respectively

Regression input parameter SV_frac formed as the sum of the individual open fractions of the 3 pressurizer SVs

Eutectic Melting Temperature

The uncertainty in the melting temperature of the eutectic formed between fuel (UO_2) and zirconium oxides is addressed thru the parameter *EU_melt_T*

Localized fuel rod liquefaction occurs when the temperature of the oxidized cladding shell reaches *EU_melt_T*

UO_2 and ZrO_2 melting temperatures are defined to be *EU_melt_T*

EU_melt_T serves a surrogate role in investigating the number of uncertainties associated with loss of fuel rod integrity

Historical default MELCOR value of 2,800 K

SNL best-practice value of 2,500 K

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Eutectic Melting Temperature (cont.)

Phebus and VERCORS experiments suggest that irradiated fuel and oxidized cladding exhibit eutectic liquefaction at lower temperatures

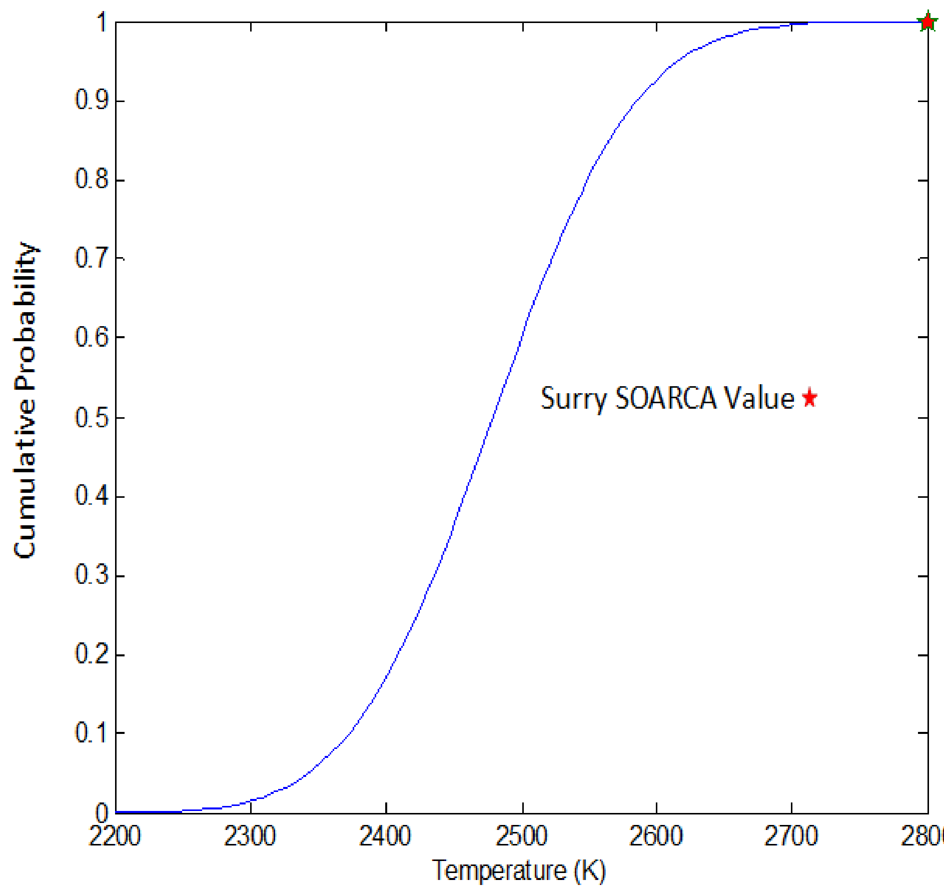
VERCORS Collapse Temperatures

Test	**Collapse Temperature (K)**
T1	2525
HT1	2550
HT2	2400
HT3	2525
V_6	2525
RT6	2350
Mean	2479
Standard Deviation	83

Eutectic Melting Temperature (cont.)

A normal distribution was fit to the VERCORS data to describe the general range of potential collapse temperatures given varying environmental conditions

Mean 2,479 K and standard deviation 83



Hydrogen Flammability

Within the compartmentalized construct of an ice-condenser containment, sources of ignition could be variably located such that the direction a burn would propagate from the source of ignition could be upward, downward or somewhere in between

The minimum H_2 concentration (LFL) supportive of a burn is dependent on propagation direction and is the basis for LFL sampling in the UA

A discrete uniform distribution is applicable since no one direction of propagation seems more likely than another (sampled parameter *burn_dir*)

Experimentally determined (Kumar) values of LFL = 0.04, 0.065 and 0.09 nominally adopted for upward, lateral and downward propagation, respectively, but with modest dependence on temperature and steam concentration

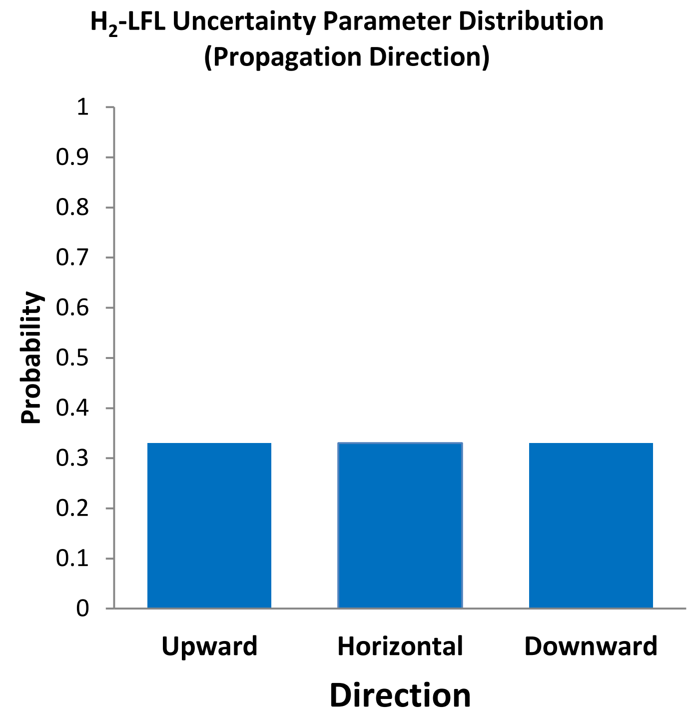


Figure 3-18 Uncertainty distribution for direction of burn propagation from source of ignition

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Hydrogen Flammability (cont.)

- H2 LFL sampling limited to the origination of a burn as opposed to the propagation of a burn from one control volume to the next
- A hot plume issuing from a hot leg nozzle breach or core debris on the containment floor necessary for ignition in one of the two sets of 600 MELCOR calculations made
- An uncharacterized or “random” source of ignition was introduced periodically somewhere in containment as an additional means of ignition in the other of the two sets of MELCOR calculations

"Random" Ignition of Combustible Gasses

In one of the two sets of 600 MELCOR calculations made, an uncharacterized or "random" source of ignition was introduced periodically somewhere in containment

Representative, for example, of a spark occurring as piping jostles during an aftershock following an earthquake

Additional to the sources of hot plume issuing from a hot leg breach or core debris on the containment floor

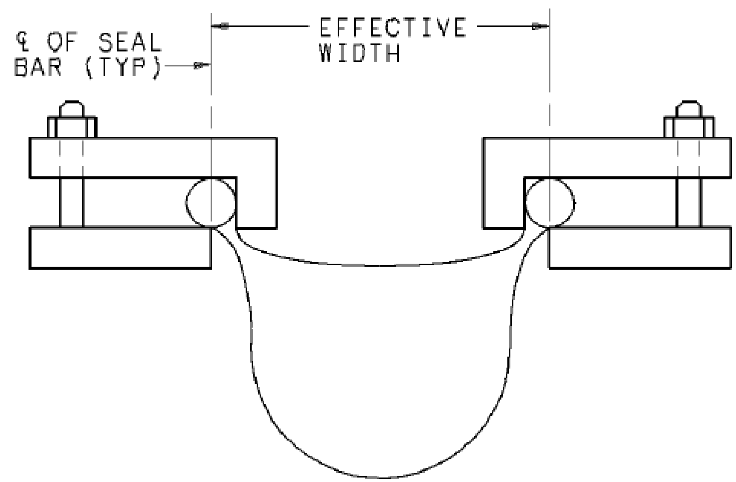
Every ½ hour from the time of the initiating event

Momentary (1-sec duration)

Randomly somewhere in containment

Containment Modeling – Upper/Lower Containment Barrier Seal

- Uncertainty in the differential pressure that will tear or otherwise fail the rubberized seal between upper and lower containment was addressed
- Uncertainty in the breached area resulting from a seal tear is also addressed
- Longer seal segments were taken to have a greater chance of local weakness and, accordingly, the probability of a segment tearing is defined proportional to its area



DETAIL A-1
TYPICAL CONFIGURATION

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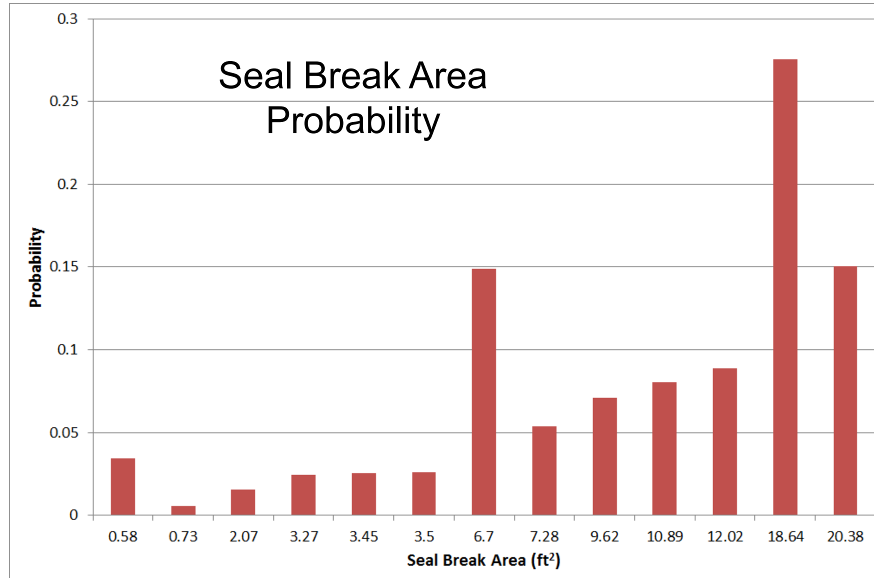
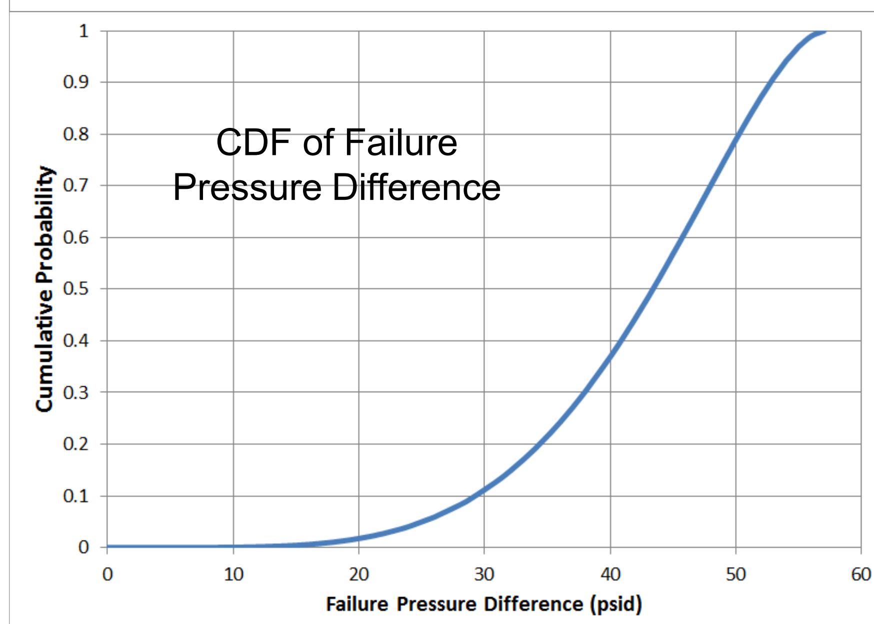
Upper/Lower Containment Barrier Seal (cont.)

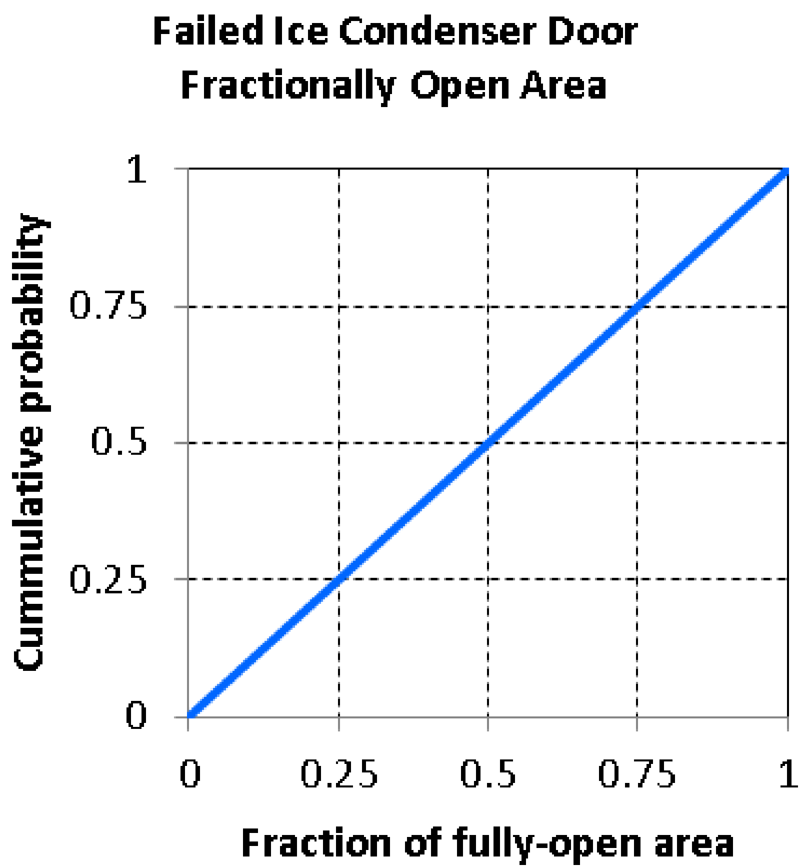
- Discrete distribution applied in specifying the open area left by a failed seal segment (sampled parameter *Seal_Open_A*)
- Prototypic seal tested to 30 psid
- Upper bound at the differential pressure that would load a seal to its design capacity (100 lb/in ➔ 57.14 psid)
- Beta distribution applied so that majority of failures occur between qualified (30 psid) and design (57.14 psid) differential pressure (sampled parameter *Seal_Fail_Dp*)
- Effective lower bound at 15 psid consequential to the applied distribution
- Ten percent of samples are below design to somewhat acknowledge accident conditions, e.g., elevated temperatures, which could lead to failures at lower pressure differentials
- One seal segment allowed to fail





Failed Ice Condenser Door Open Area Fraction

- The uncertain fractionally open area of a failed ice condenser inlet door is managed thru parameter *AJAR*
- Doors assumed to fail to reclose if at some point they open fully
- Same open fraction defined for each of the 5 flow paths representing ice condenser inlet doors via a uniform distribution between 0 and 1



Aerosol Shape Factor

Uncertainty in the shape of aerosol agglomerates is addressed thru the parameter *shape_factor*

MELCOR default value of 1.0 (spherical)

Sampling via a scaled beta
1 5
and bounded at 5.0 from consideration of experimental conclusions (Kasper, Brockmann and Hinds)

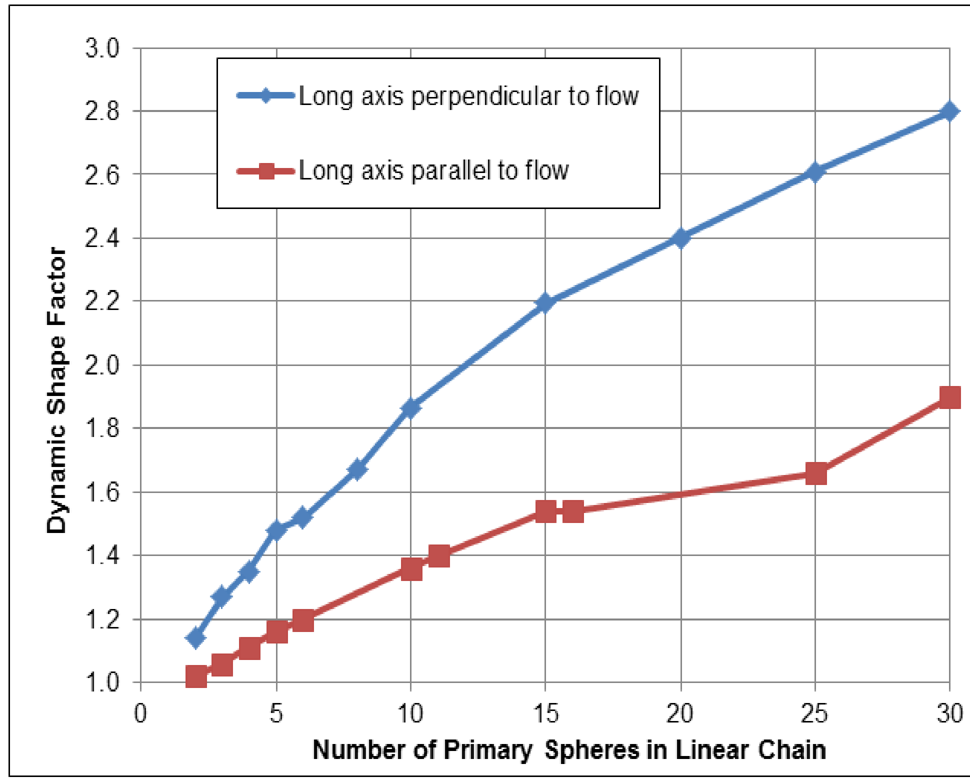


Figure 3-24 Shape factor versus the number of spheres within a chain

Aerosol Shape Factor (cont.)

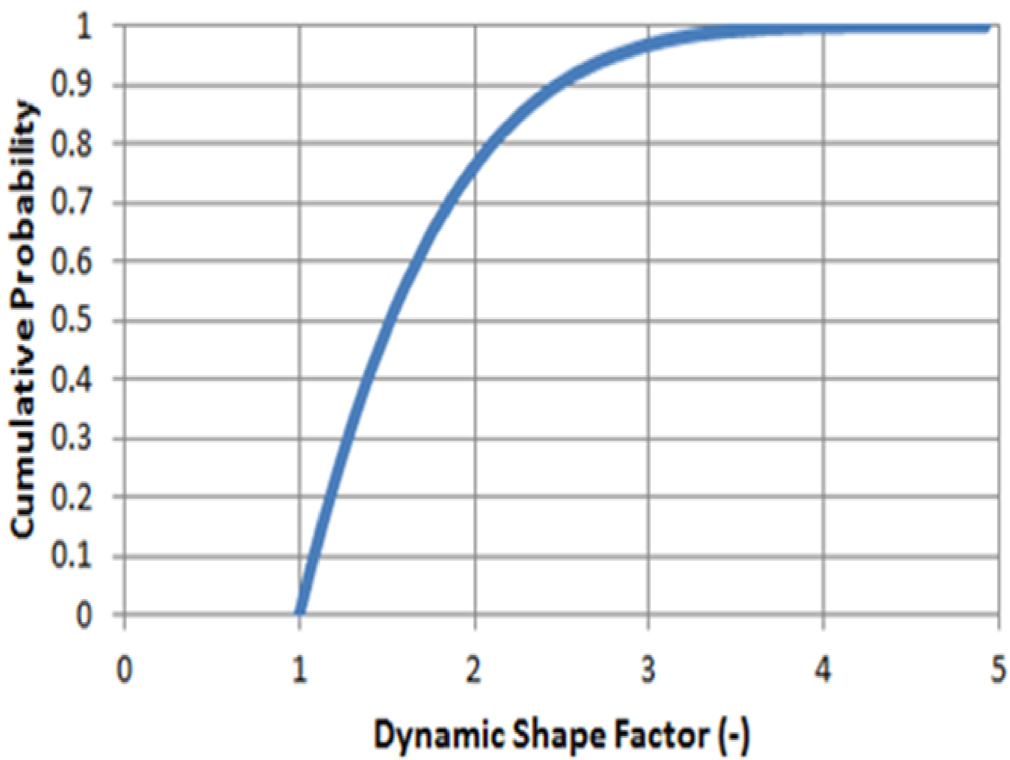


Figure 3-24 CDF of dynamic shape factor

value yields a function that has a peak close to the lower bound of 1.0, value yields about 75 percent of samples between 1.0 and 2.0 while allowing some samples at values up to 5.0.

Containment Rupture Pressure

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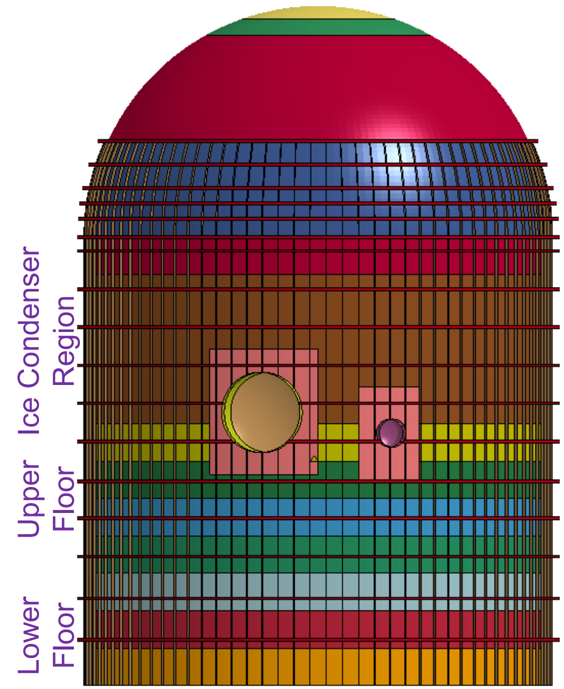
Uncertainty in the pressure that a Sequoyah free-standing steel containment vessel can withstand before rupturing is addressed thru parameter *rupture*

Historical NRC-sponsored and current NRC in-house finite element analyses consistently suggest a best-estimate very near 67 psig

Historical expert solicitation reported on in NUREG/CR-4551 and adopted in NUREG-1150, suggests 65 psig as best-estimate



Sequoyah free-standing steel containment vessel with circumferential stiffeners

Current interpretation of historical finite element results suggests a range between 52 psig and 78 psig

Accordingly, sampling of *rupture* was accomplished with a triangular distribution having a mode of 67 psig and bounds of 52 psig and 78 psig

MELCOR Uncertainty Analysis Results

Tina Ghosh, PhD
Accident Analysis Branch
NRC Office of Nuclear Regulatory Research

Scope of MELCOR UA for Unmitigated STSBO

- Figures of merit studied: cesium release magnitude, iodine release magnitude, in-vessel hydrogen generation, containment failure time, time of initial release
- Complementary methods used to analyze Monte Carlo simulation results:
  - Four regression analysis techniques to identify influential parameters based on a statistical perspective of the set of realizations
  - 1-D and 2-D scatter plots to identify individual and joint influences of parameters on figures of merit
  - Investigation of select individual realizations for phenomenological explanations of different results

CDFs of Containment Rupture Time with and without Random Ignition Sources

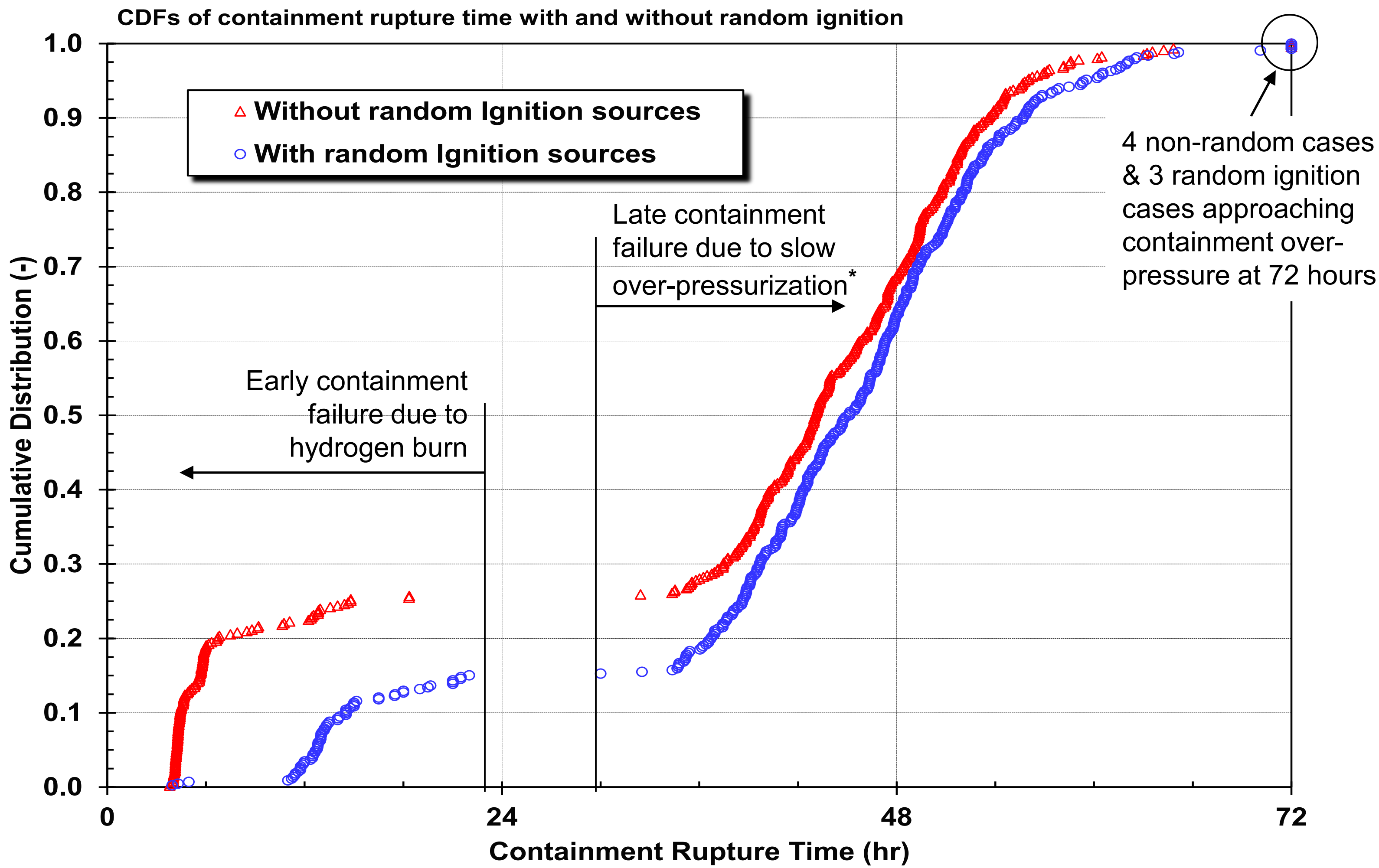


Figure 4-14 CDFs of containment rupture time with and without random ignition

** One random ignition case with a late containment failure due to hydrogen burn*

Containment Failure Time versus RPV Breach Time – without Random Ignition

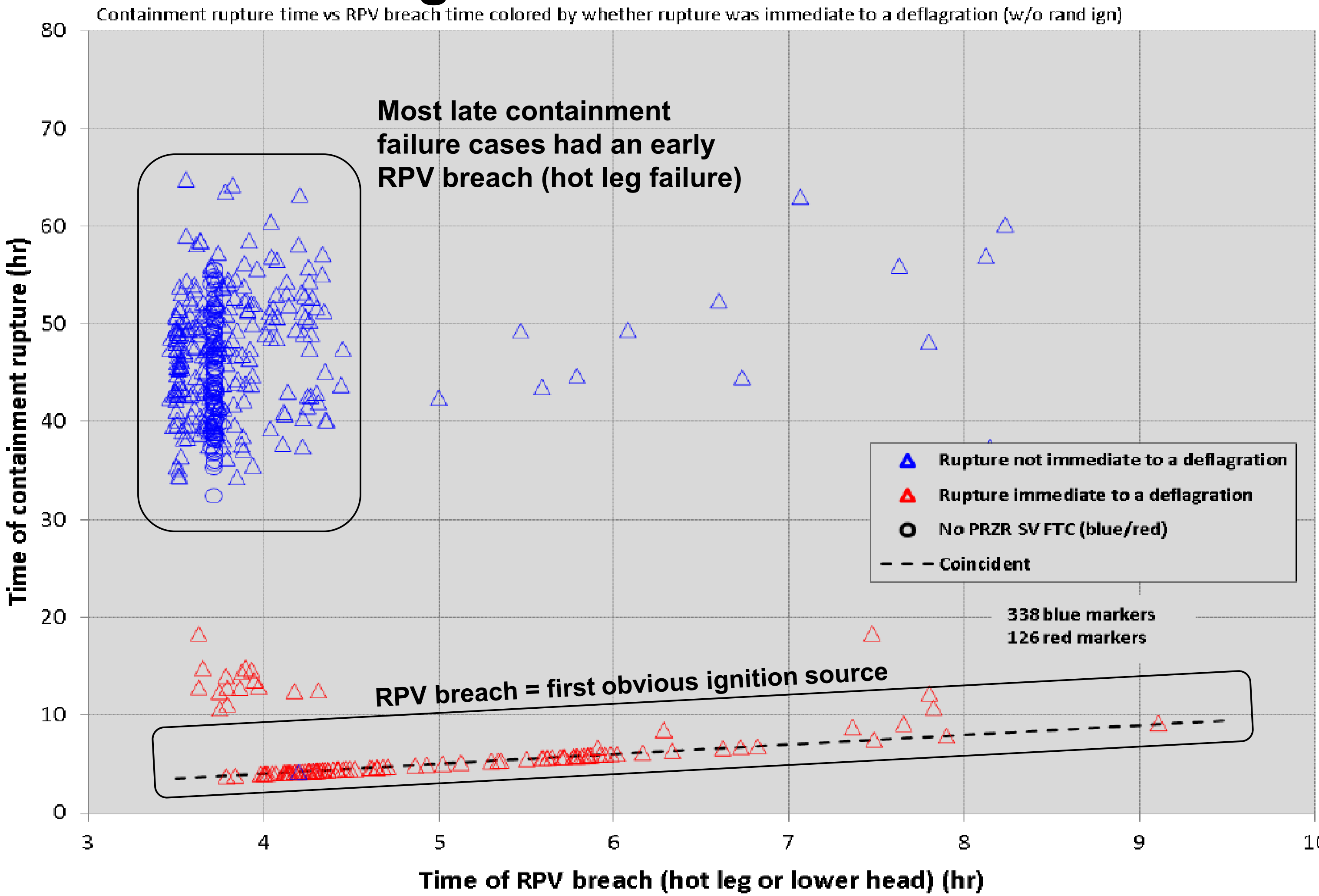


Figure 4-15 Containment rupture time versus RPV breach time colored by whether rupture was immediate to a deflagration – without random ignition

Primary Cycles versus SV Open Fraction Early (Red Δ)/Late (Blue O) Containment Failures – without Random Ignition

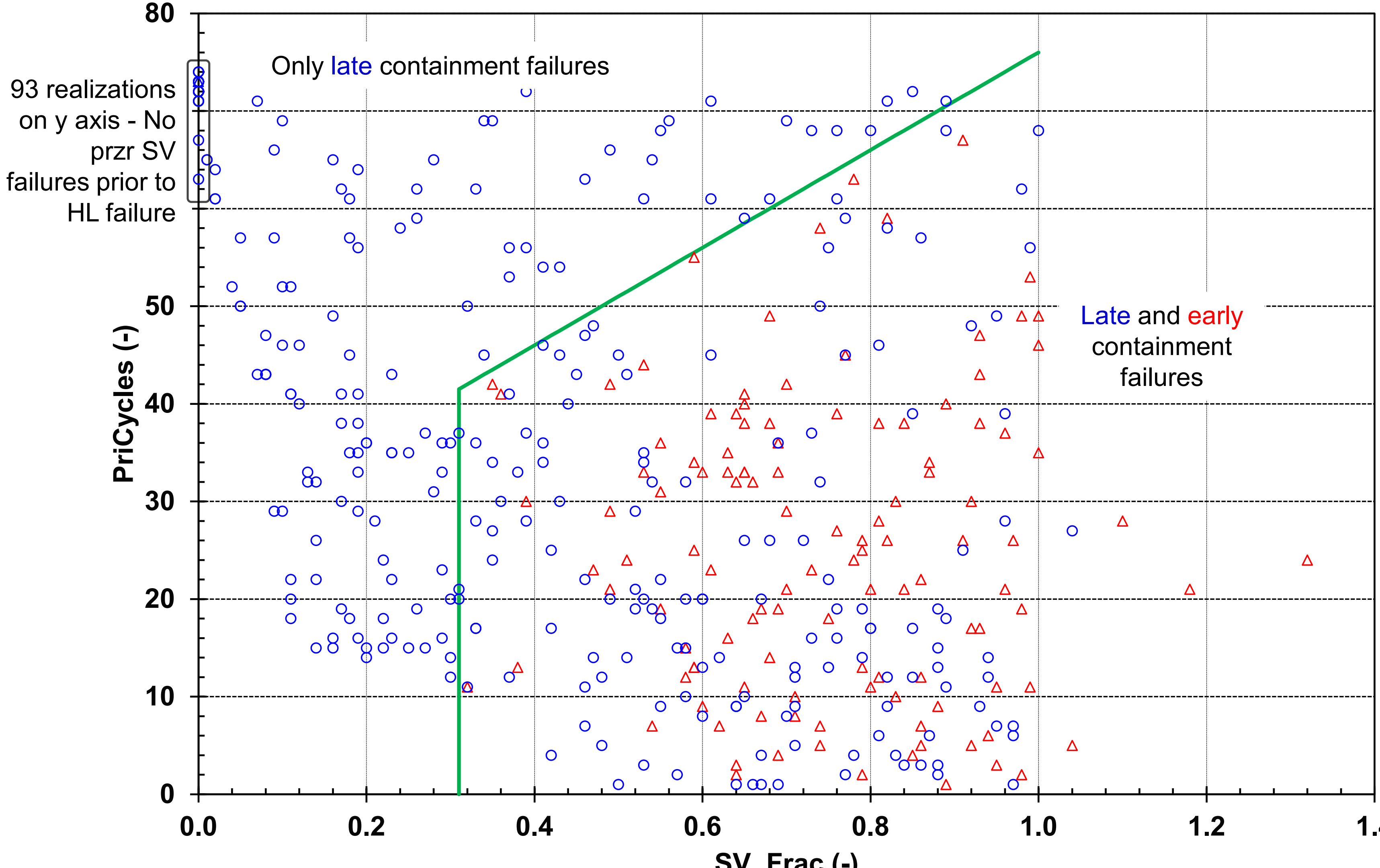
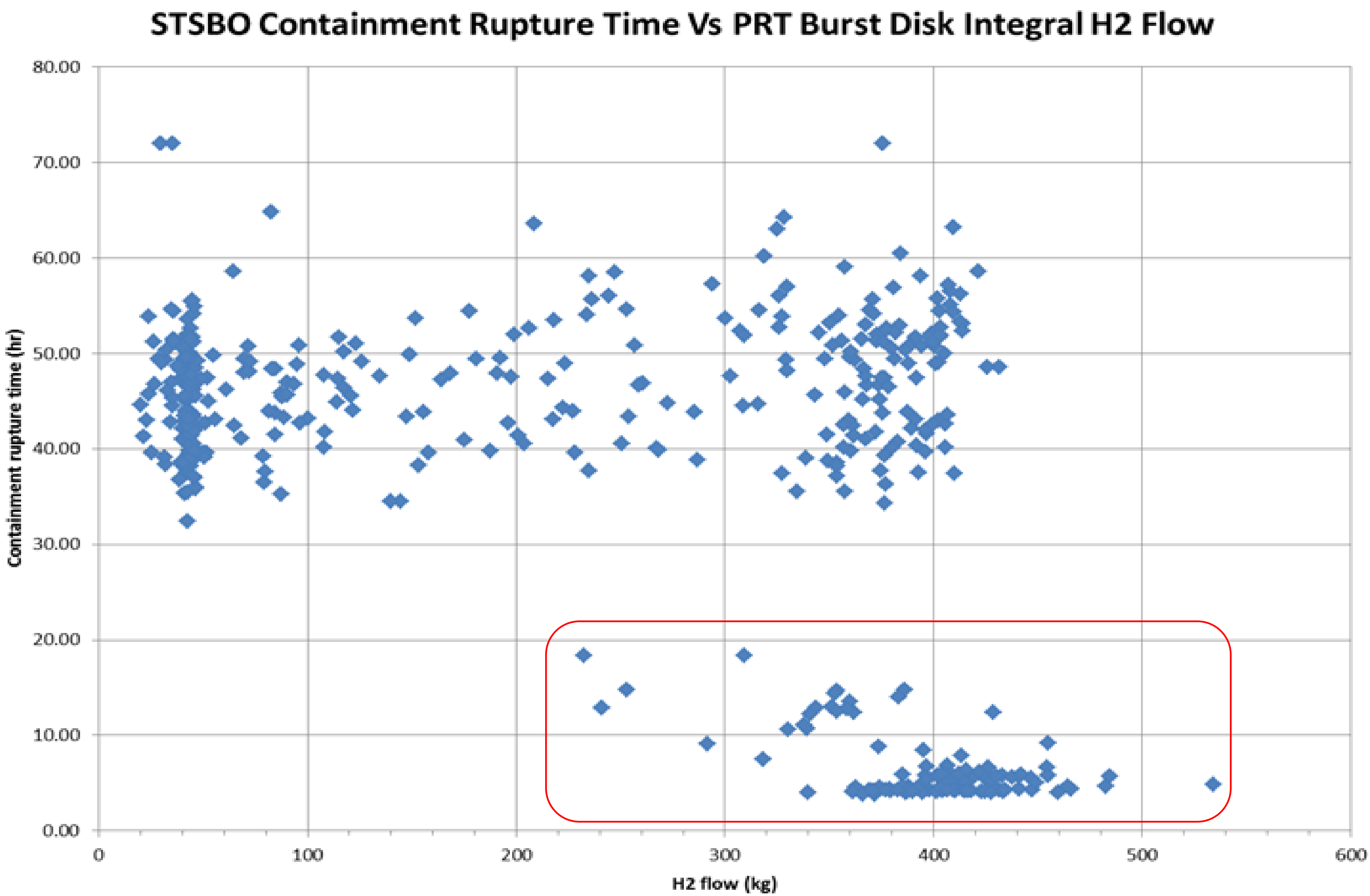


Figure 4-17 Relationship of priSVcycles to SV_Frac colored by containment failure timing – without random ignition

Containment Failure Time versus Hydrogen Flow through PRT Rupture Disk – without Random Ignition



Iodine Release Fractions for Realizations without Random Ignition

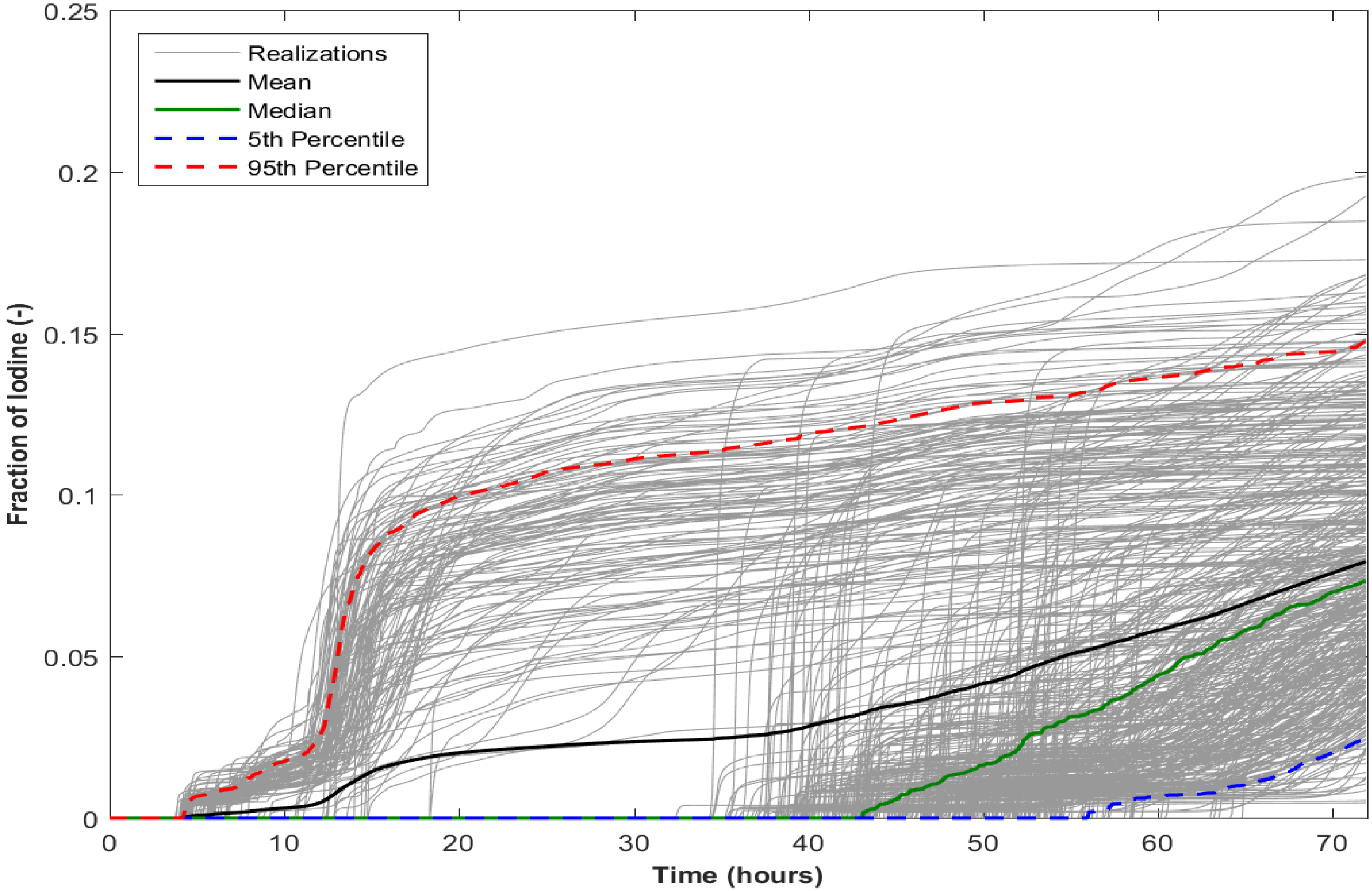


Figure 4-20 Iodine release fractions over 72 hours with mean, median, 5th and 95th percentiles

Iodine Release Fractions for Realizations with Random Ignition

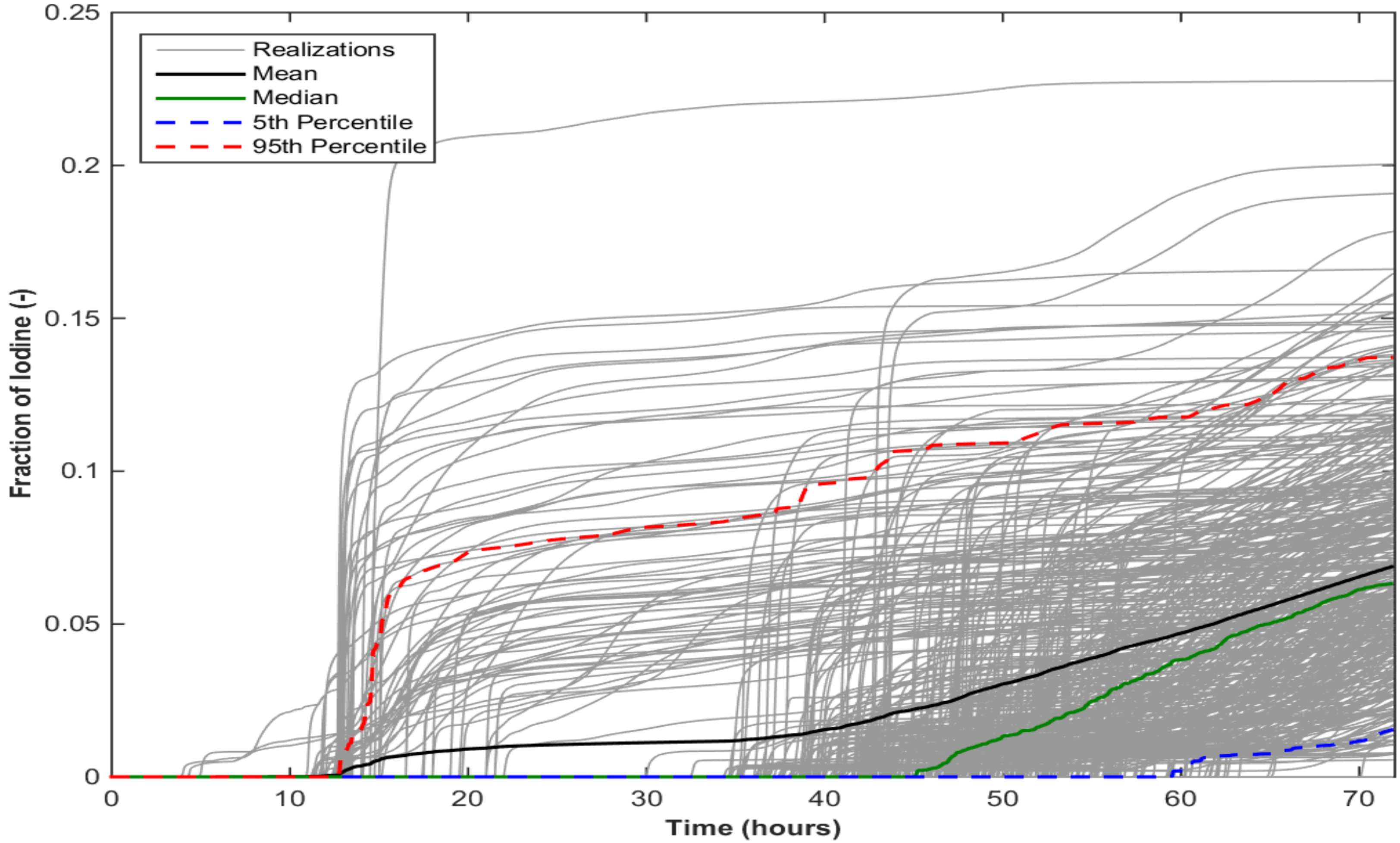


Figure 4-51 Iodine release fractions over 72 hours with mean, median, 5th and 95th percentiles

Regression Analysis of Iodine Release Fractions

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contribution	Conjoint Contribution
Final R^2	0.31		0.44		0.82		0.47			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
	Without Random Ignition Source									
SV_frac	0.20	0.29	0.34	0.58	0.27	0.57	0.33	0.32	0.18	0.12
priSVcycles	0.04	-0.27	0.14	0.34	0.13	0.42	0.49	0.49	0.11	0.11
rupture	0.03	-0.17	0.06	0.08	0.06	0.25	0.05	0.05	0.03	0.06
EU_melt_T	0.02	0.13	0.08	0.09	0.02	0.10	0.10	0.10	0.03	0.03
	With Random Ignition Source									
Final R^2	0.19		0.28		0.78		0.32			
rupture	0.07	-0.25	0.21	0.22	0.13	0.45	0.22	0.22	0.08	0.08
priSVcycles	0.07	-0.29	0.13	0.25	0.08	0.43	0.38	0.38	0.07	0.10
SV_frac	---	---	0.25	0.46	0.14	0.57	0.00	0.00	0.06	0.13
ajar	0.03	-0.18	0.05	0.11	0.02	0.31	0.22	0.21	0.03	0.08
EU_melt_T	0.01	0.07	0.06	0.08	0.01	0.09	0.18	0.19	0.02	0.03

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Cesium Release Fractions for Realizations without Random Ignition

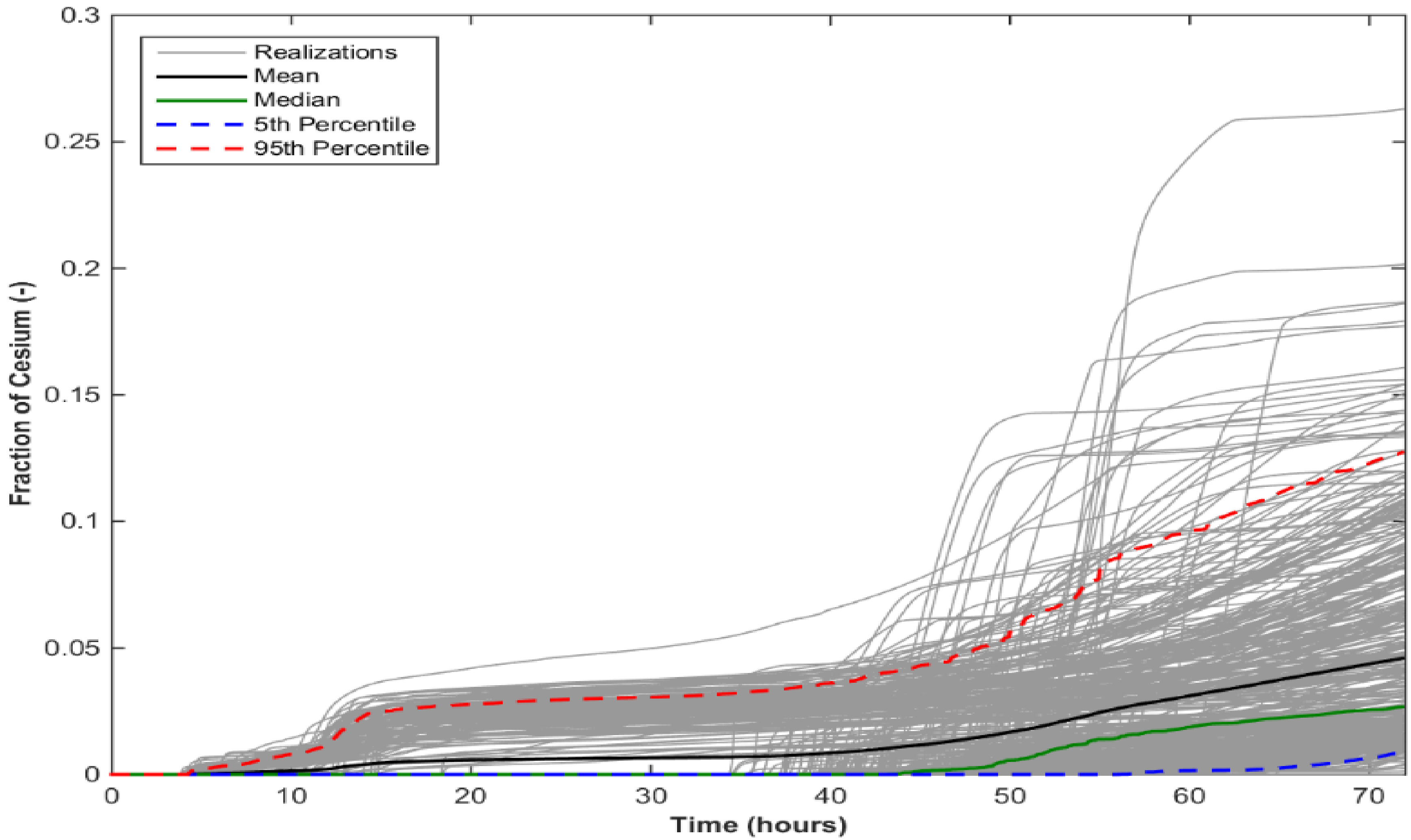


Figure 4-27 Cesium release fractions over 72 hours with mean, median, 5th and 95th percentiles

Cesium Release Fractions for Realizations with Random Ignition

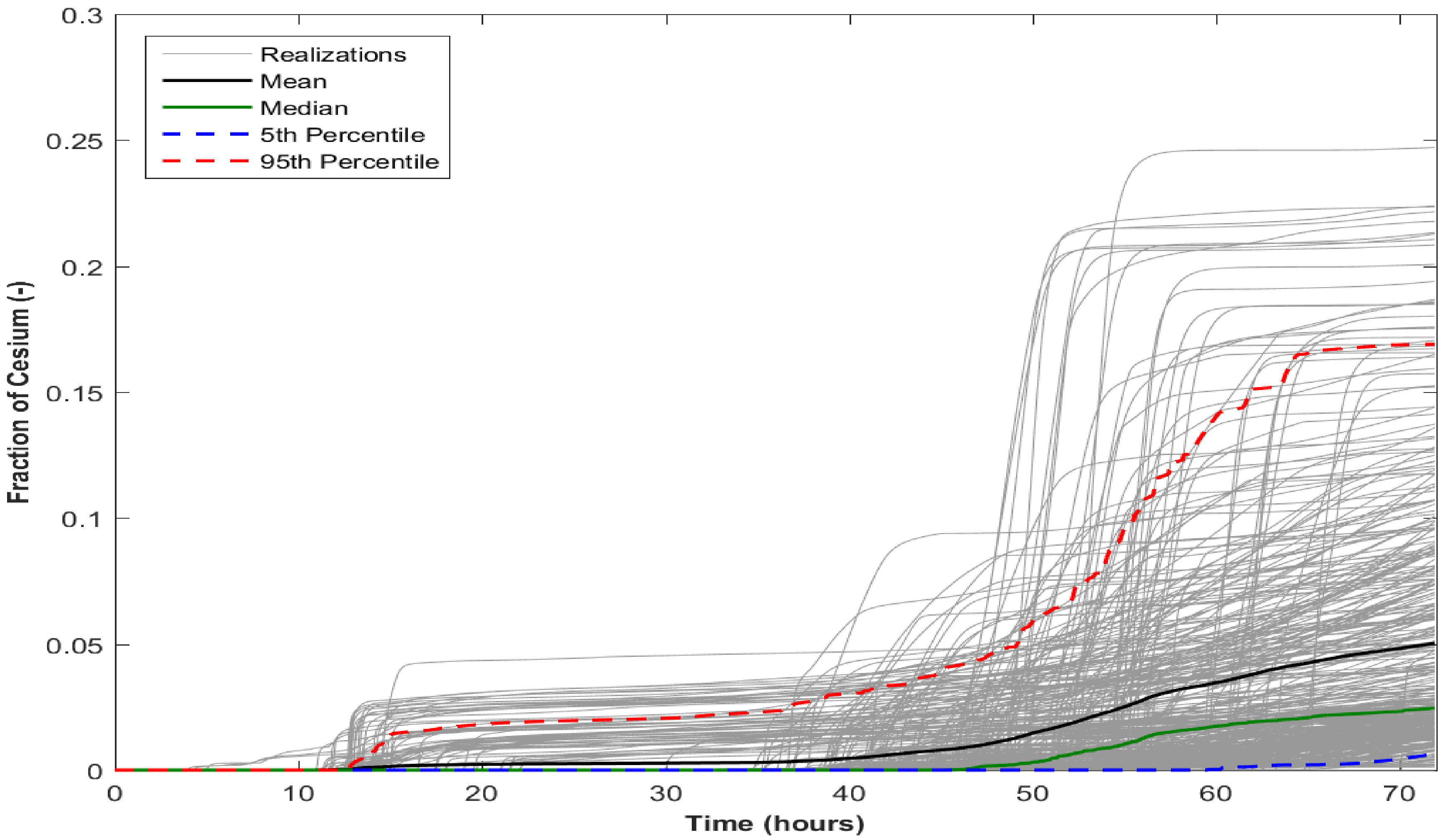


Figure 4-59 Cesium release fractions over 72 hours with mean, median, 5th and 95th percentiles

Regression Analysis of Cesium Release Fractions

Input	Rank Regression R² contr.	Rank Regression SRRC	Quadratic S_i	Quadratic T_i	Recursive Partitioning S_i	Recursive Partitioning T_i	MARS S_i	MARS T_i	Main Contribution	Conjoint Contribution
	Without Random Ignition Source									
Final R^2	0.71		0.68		0.88		0.75			
SV_frac	0.66	0.72	0.82	0.93	0.57	0.84	0.62	0.74	0.55	0.14
priSVcycles	0.02	-0.16	0.02	0.09	0.12	0.32	0.21	0.33	0.07	0.10
shape_fact	0.01	0.10	0.03	0.05	0.02	0.11	0.03	0.04	0.02	0.03
	With Random Ignition Source									
Final R2	0.63		0.61		0.89		0.63			
SV_frac	0.56	0.64	0.70	0.88	0.53	0.87	0.85	0.90	0.50	0.15
priSVcycles	0.02	-0.20	0.02	0.11	0.08	0.25	0.07	0.06	0.03	0.07
shape_fact	0.02	0.15	0.03	0.07	0.02	0.23	0.03	0.08	0.02	0.08

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

MELCOR Select Individual Realizations Analysis

Kyle Ross
Sandia National Laboratories

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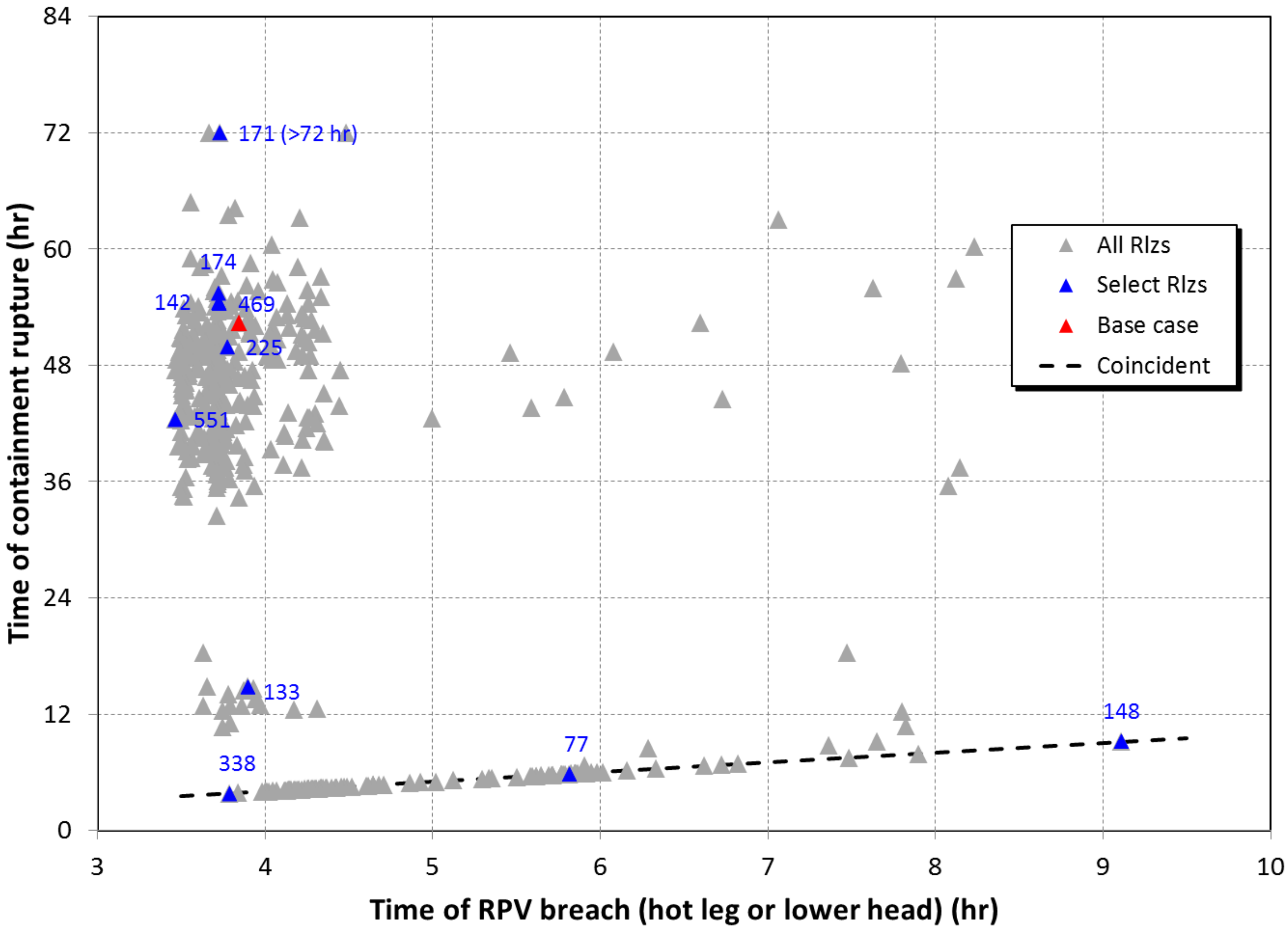
Select Realization Criteria

I.D.	Selection criterion	Demonstrative Rlz[1]
1	The base STSBO UA case	Base calculation
2	The case with earliest containment failure	338
3	A case with containment remaining intact at 72 hr	171
4	The case with the earliest FTC of a pressurizer SV	142
5	A case without a FTC of a pressurizer SV	469
6	A case with coincident RPV breach and containment failure	338
7	The case with the least in-vessel hydrogen production	469
8	The case with the most in-vessel hydrogen production	225
9	The case with the smallest Cs release to the environment	174
10	The case with the largest Cs release to the environment	142
11	The case with the earliest RPV breach	551
12	The case with the latest RPV breach	148
13	A case without hot leg failure	77
14	A case with early RPV breach and early containment failure not coincident	133

1. **All demonstrative Rlzs are without random sources of ignition included**

Select Realizations



Key Considerations

H_2 ignition requisite on hot leg failure or core debris on the floor

H_2 concentration necessary to originate a burn treated as uncertain but not to propagate a burn

With respect to pressurizer SV function, what happens to the 3 parallel valves as a system is paramount as opposed to what happens to any valve individually

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Select Realization Results and Indications

Principally important to containment failure timing is the amount of H_2 vented by the RCS to containment prior to RPV hot leg or lower head failure

Deterministic to the amount of H_2 vented is

- Whether a pressurizer SV FTC occurs
- SV position given a FTC

SV FTC necessary for containment failure before 24 hr

~72 SV cycles indicative of no SV failure

Substantial FTC open area necessary for containment failure before 24 hr

Select Realization Results and Indications (cont.)

If a pressurizer SV did not suffer a FTC, hot leg nozzle failure occurred at 3.7 hr

"Early" containment failures came immediate to H_2 burns

High "dry" hydrogen buildup in the containment dome was a precursor to containment failure occurring immediate to an H_2 burn

Coincidentally, the uncertainty in containment failure pressure and the variability in the pressure resulting from H_2 burns overlap

Select Realization Results and Indications (cont.)

"Late" containment failures:

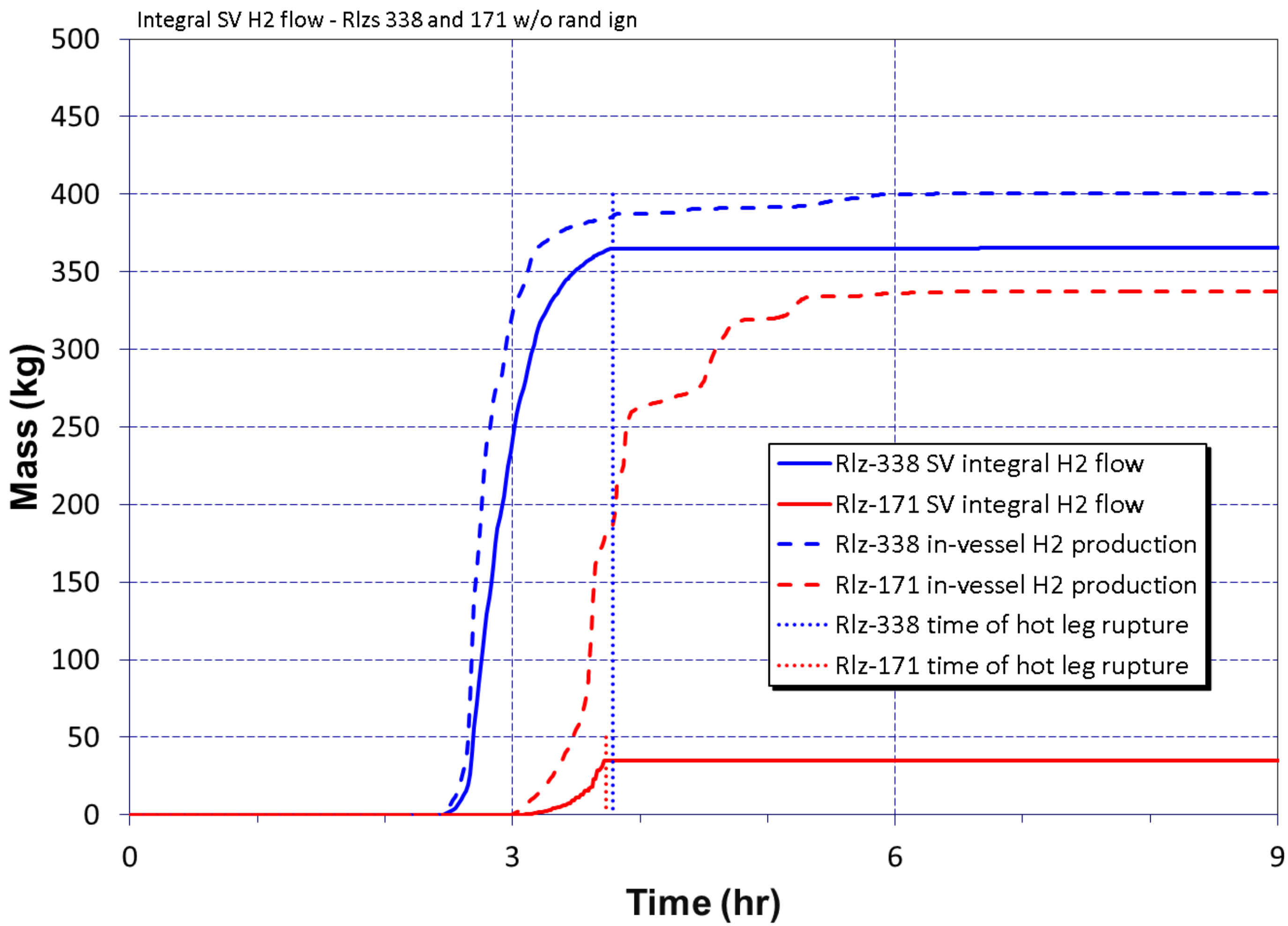
- Were associated more often with early hot leg or lower head failure
- Continually experienced insufficient hydrogen, excessive steam, a lack of sufficient oxygen or a missing ignition source such that deflagrations large enough to fail containment never occurred
- Eventually resulted from monotonic pressurization driven by fission product decay primarily thru steam generation but also non-condensable gas generation from core-concrete interaction

Greater deposition of fission products in the PRT resulted as a consequence of early SV FTC (an unsustainable situation because of associated decay power)

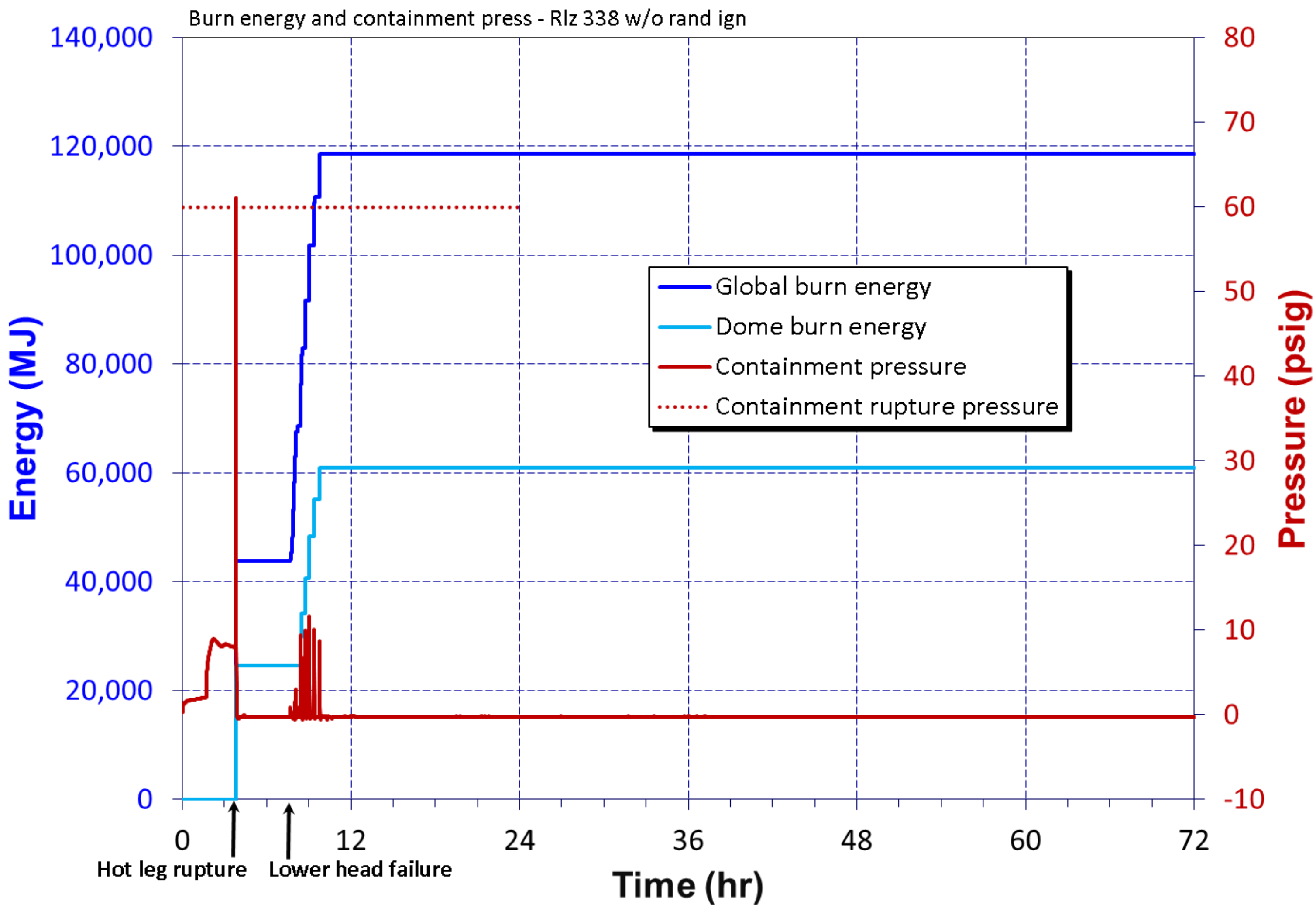
Influence of H_2 Amount Vented from RCS Prior to Hot Leg Failure

Rlz 338 fails containment immediate to hot leg rupture and an H_2 deflagration

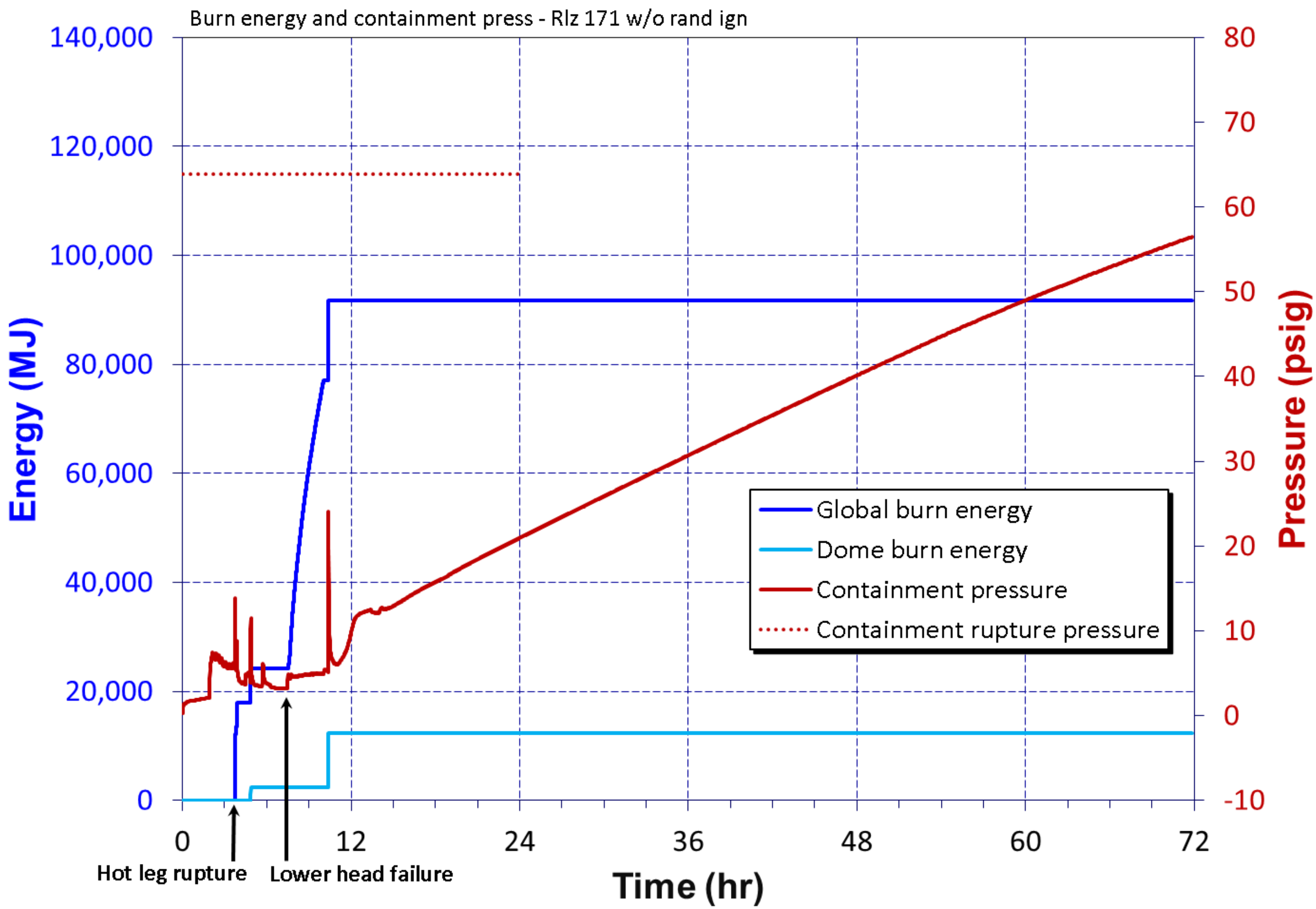
Rlz 171 fails containment after >72 hr following a monotonic pressurization



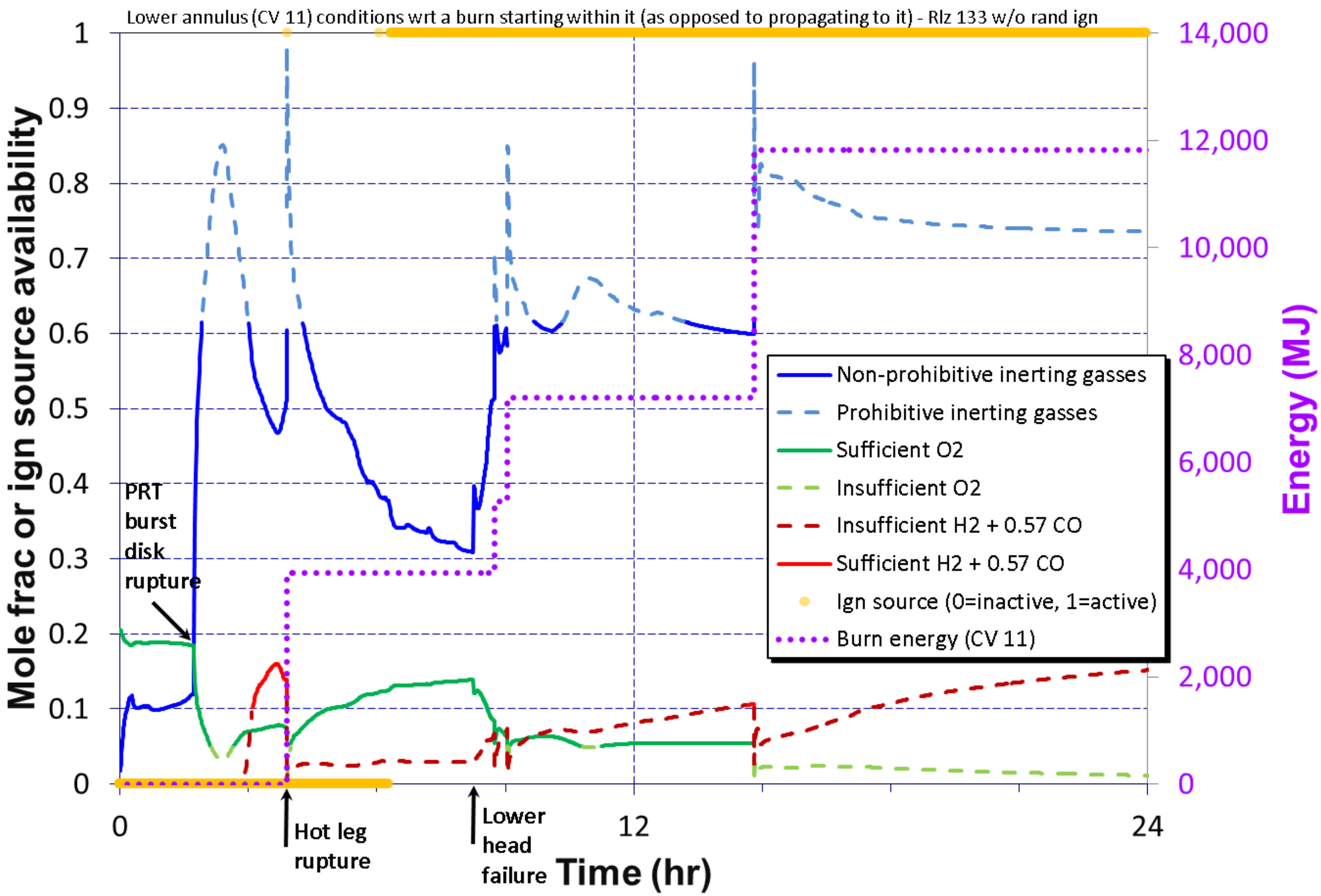
Influence of H_2 Amount Vented from RCS Prior to Hot Leg Failure (cont.)



Influence of H_2 Amount Vented from RCS Prior to Hot Leg Failure (cont.)



Influence of Deflagration Criteria



PRT Involvement in Fission Product Release

Steam relieved from the RCS by the pressurizer SVs vents to the Pressurizer relief tank (PRT)

Initially steam condenses in the water pool always held by the tank

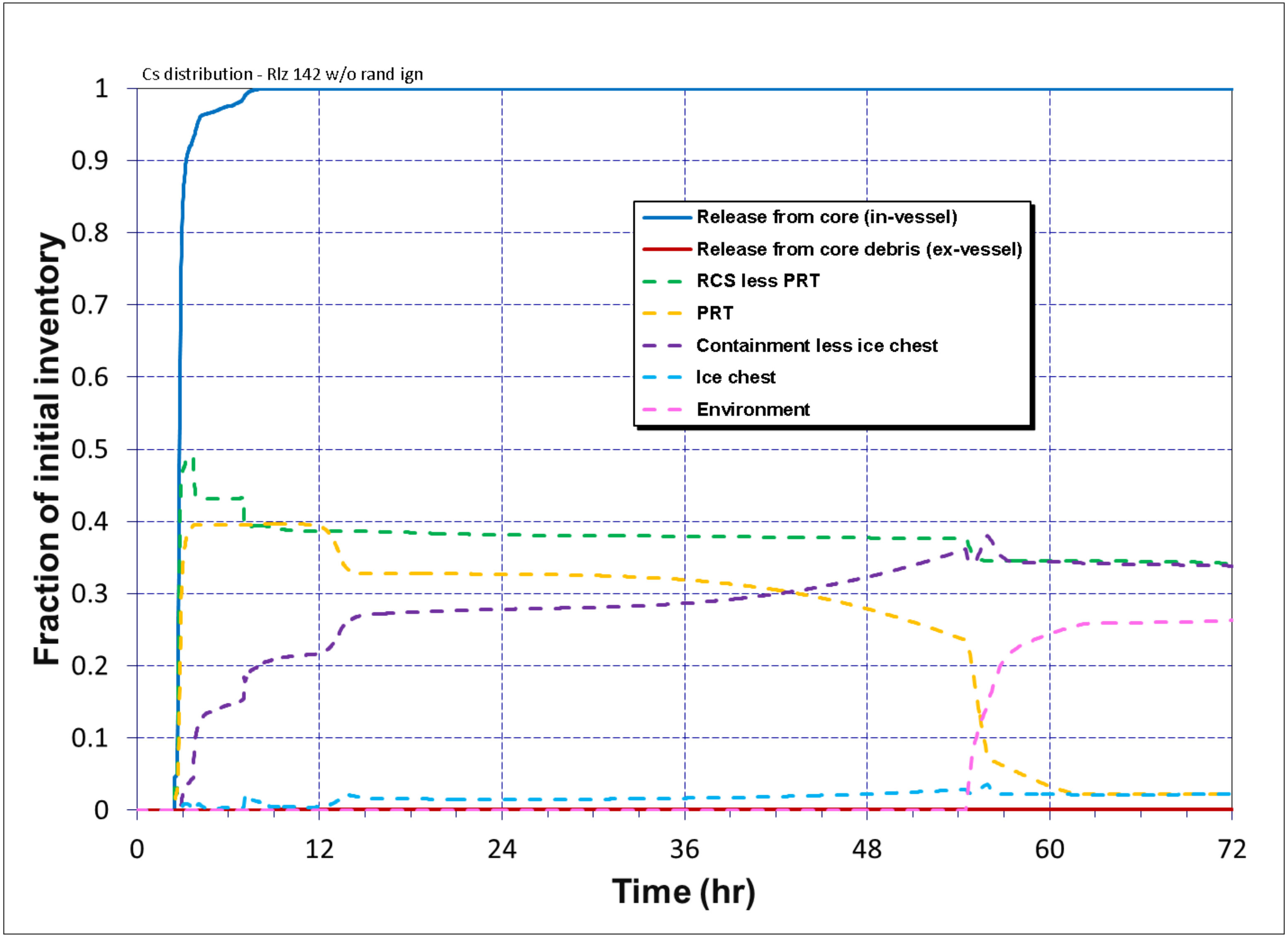
Given core damage, fission products are carried by SV relief flows to the PRT where they substantially deposit in the water pool if there is one

In an SBO, the water in the PRT eventually saturates and the tank pressurizes to rupture its burst disk at which point the water evaporates and dry fission products deposits result in the tank

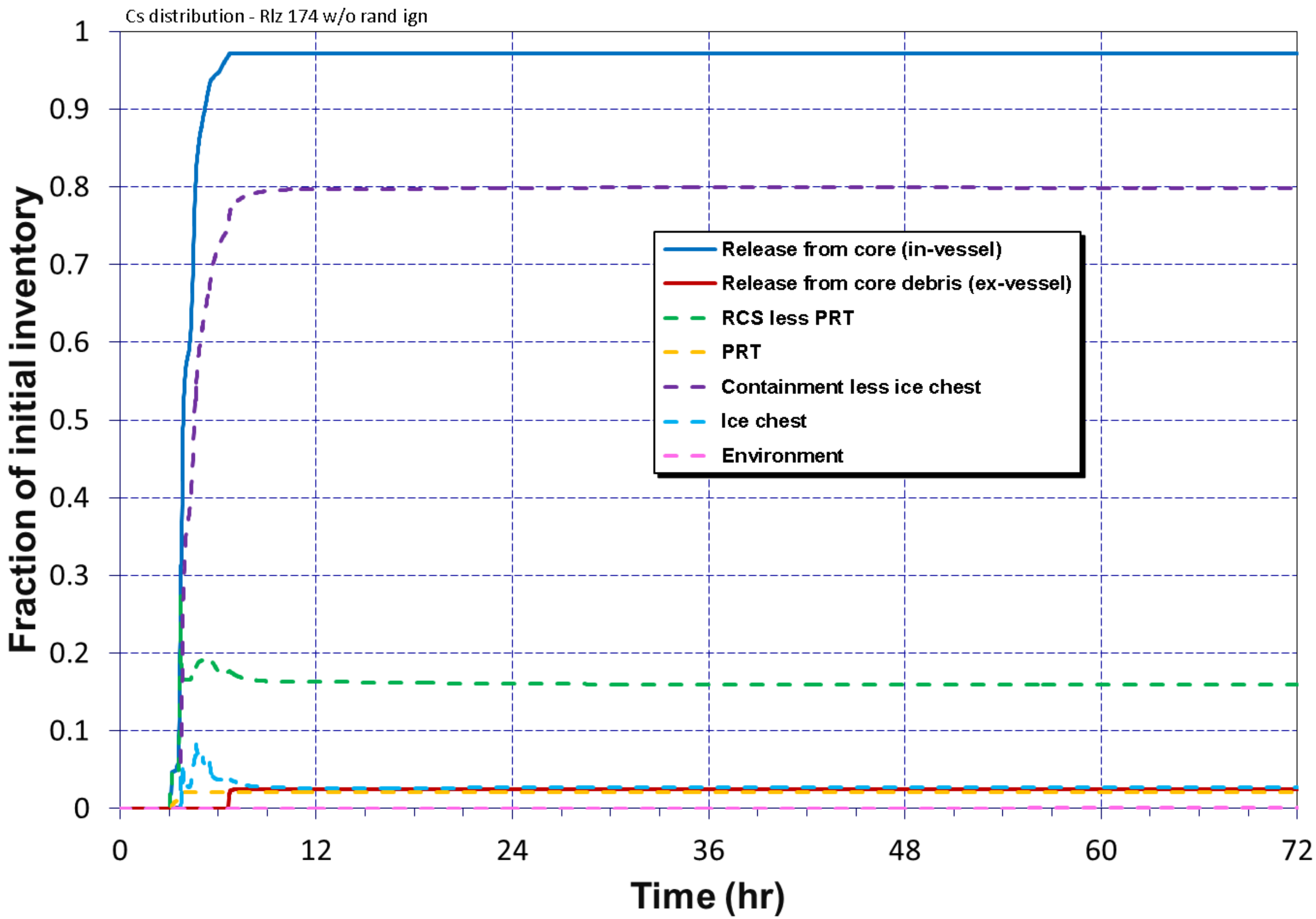
The dry deposits with their incessant decay power are susceptible to re-vaporization and further transport

Degree of involvement of the PRT strongly dependent on SV FTC timing

PRT Involvement in Fission Product Release – Very Early SV FTC



PRT Involvement in Fission Product Release – No SV FTC



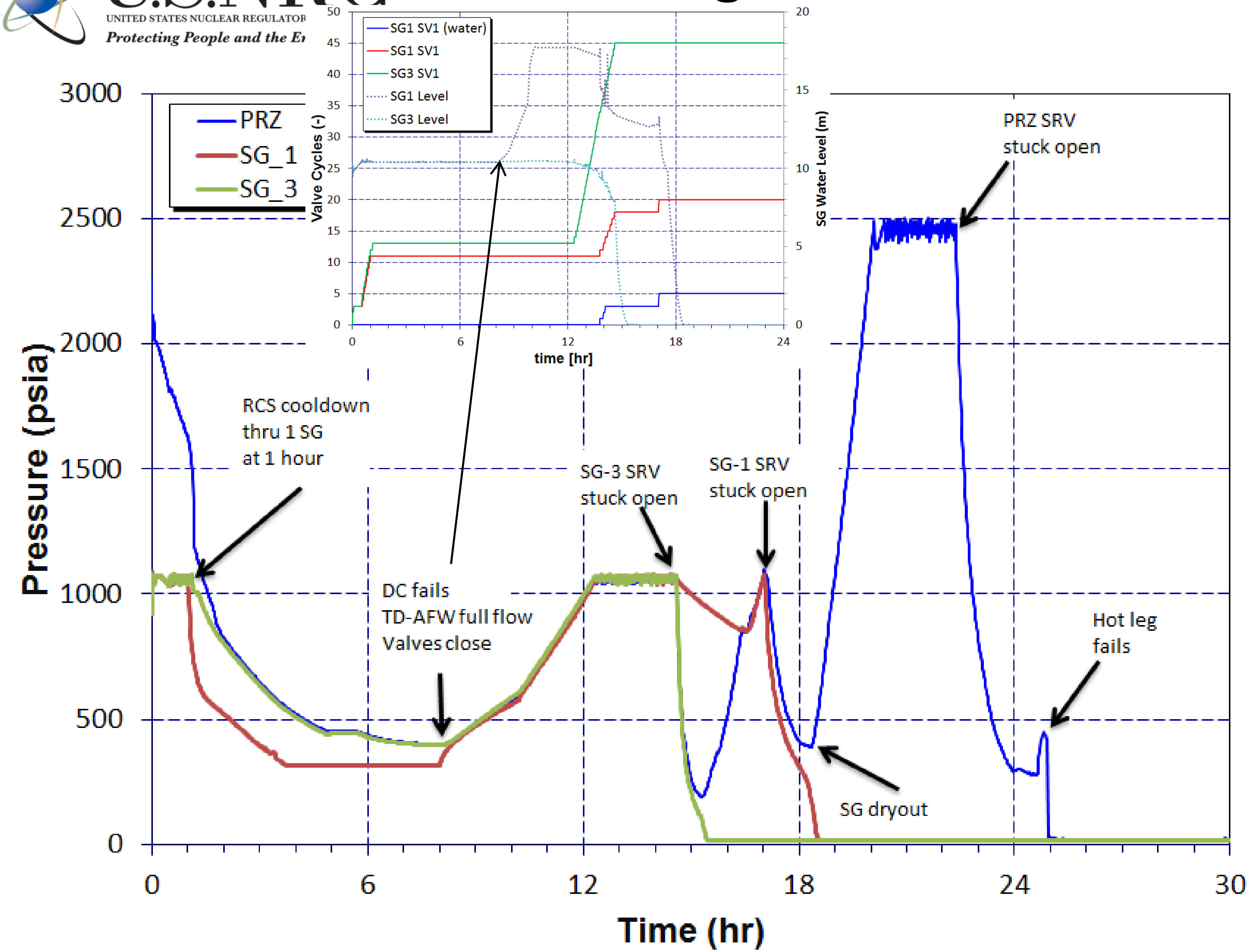
MELCOR Long-Term Station Blackout (LTSBO) Analysis

Hossein Esmaili
Fuel and Source Term Code Development Branch
NRC Office of Nuclear Regulatory Research

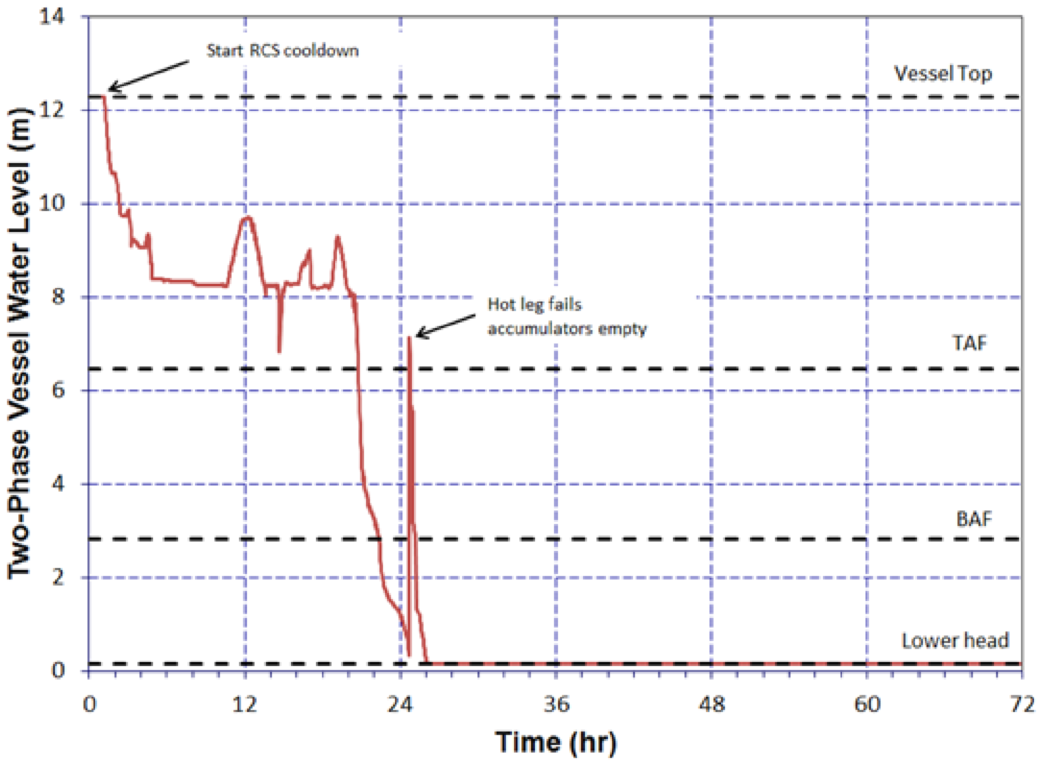
LTSBO Base Case Sequence

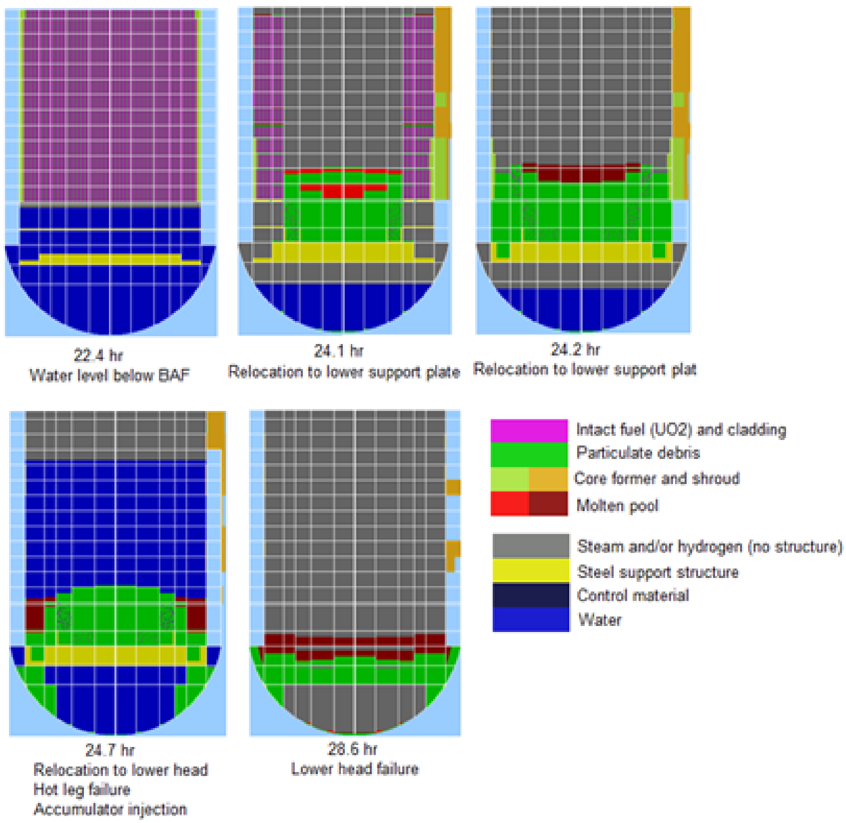
Event Description	Time (hh:mm)
Initiating event; Station blackout – loss of all onsite and offsite AC power	00:00
MSIVs close; Reactor trip; RCP seals initially leak at 21 gpm/pump	00:00
TDAFW auto initiates at full flow	00:00+
Operators control TDAFW to maintain level	00:15
Operators initiate controlled RCS cooldown of secondary at ~100°F/hr	01:00
Accumulators begin injecting	03:08
SG cooldown stopped at 300 psig to maintain TDAFW flow	03:42
DC Batteries Exhausted / SG ARVs reclose	08:00
SG1 fills up and floods the AFW steam turbine (AFW shuts down)	10:14
SG3 SV stuck open (after 45 cycles)	14:36
SG1 SV stuck open (after 5 cycles with water flow)	17:06
Pressurizer SV opens	20:06
PRT failure (4% ice melted)	20:30
Water level below top of active fuel	20:48
Pressurizer SV stuck open (45 cycles)	22:20
First fission product gap releases	22:48
Creep rupture failure of the hot leg nozzle in combined loop	24:42
1st hydrogen deflagration/containment failure	24:42
Vessel lower head failure by creep rupture	28:36
100% of the ice melted	34:38

Unmitigated LTSBO Results

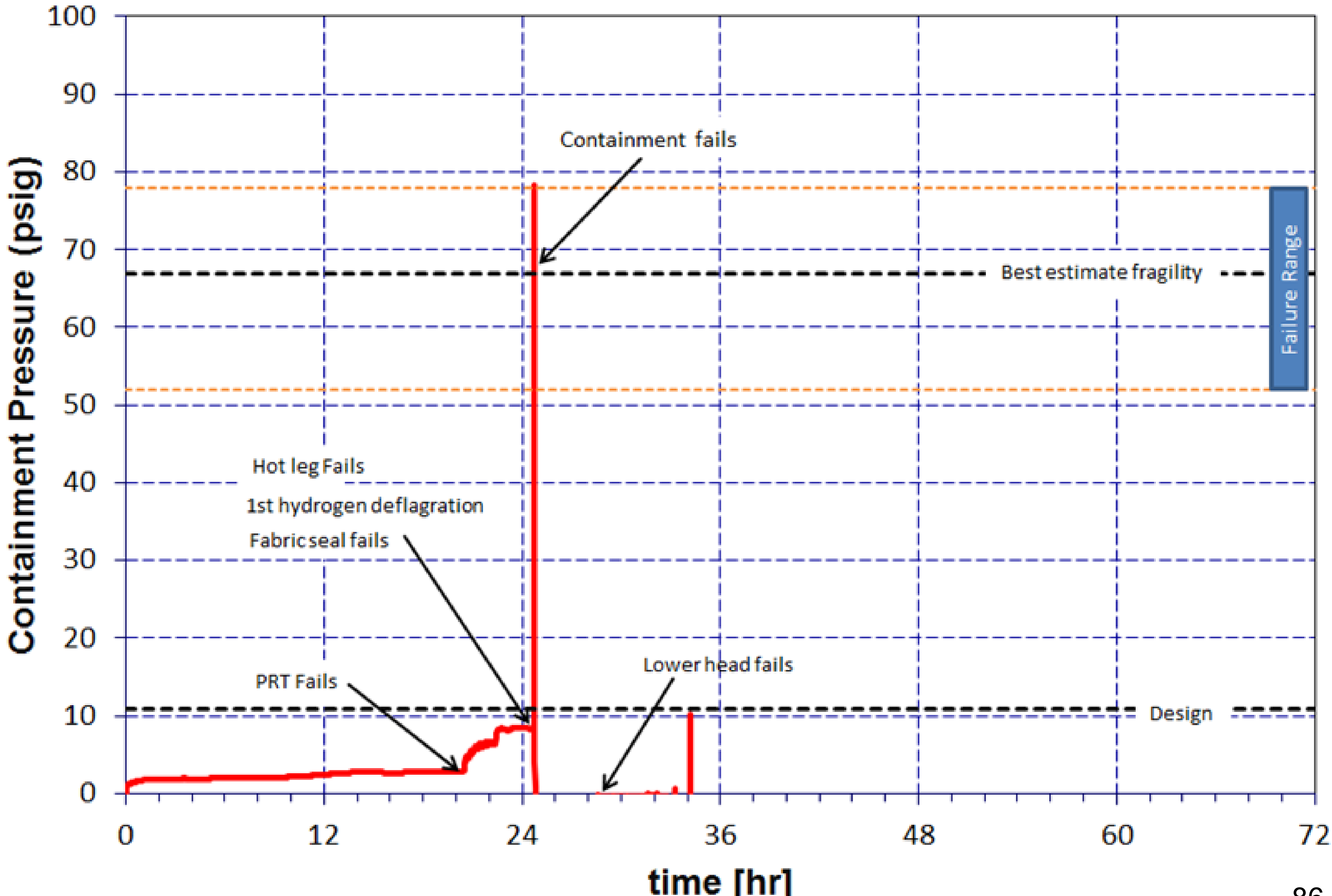


Unmitigated LTSBO Results



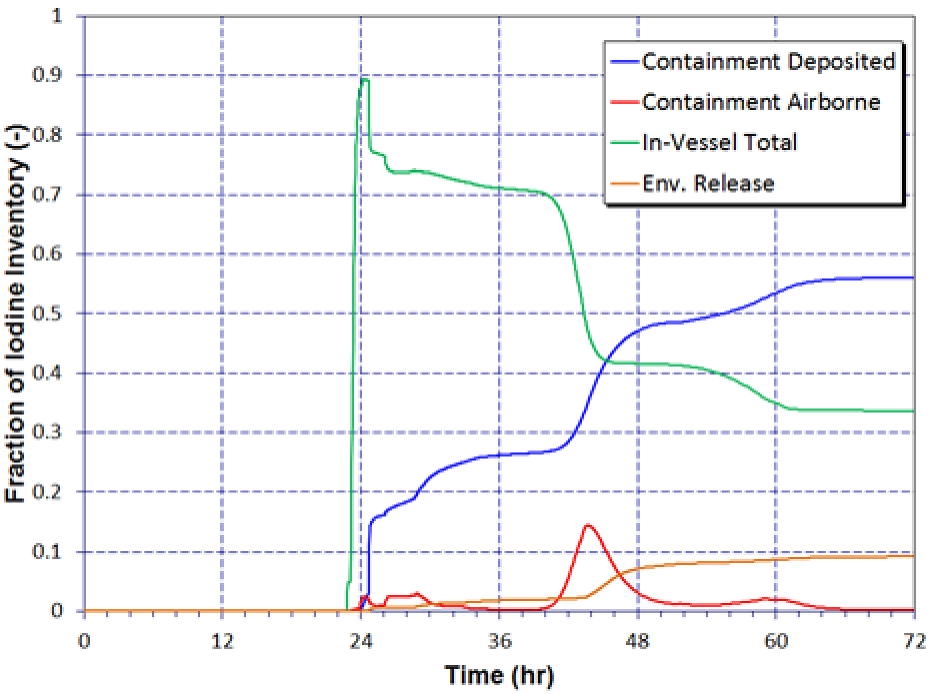


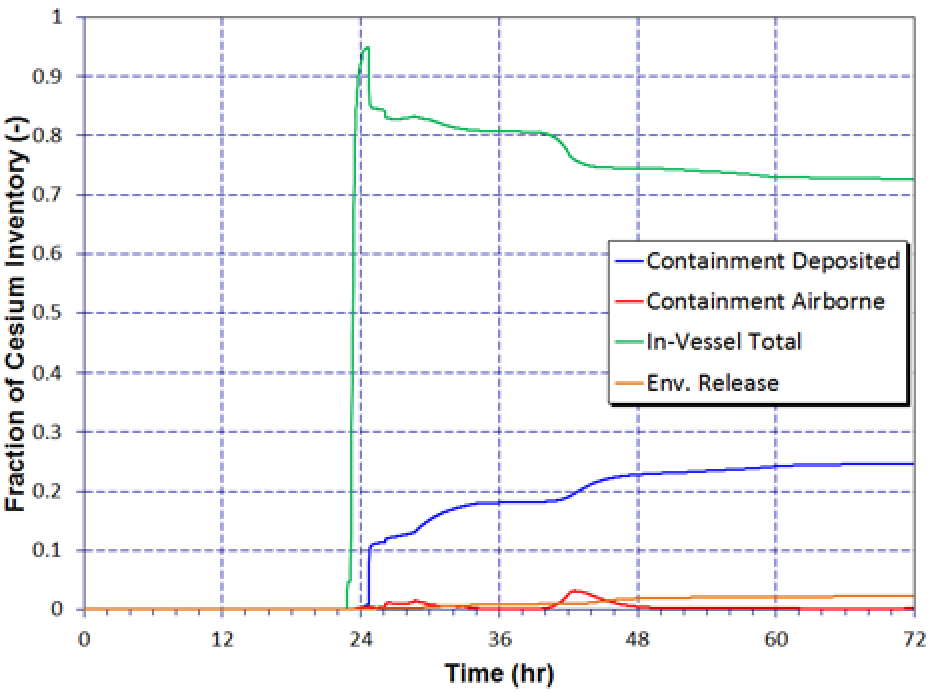
Unmitigated LTSBO Results



Unmitigated LTSBO Results

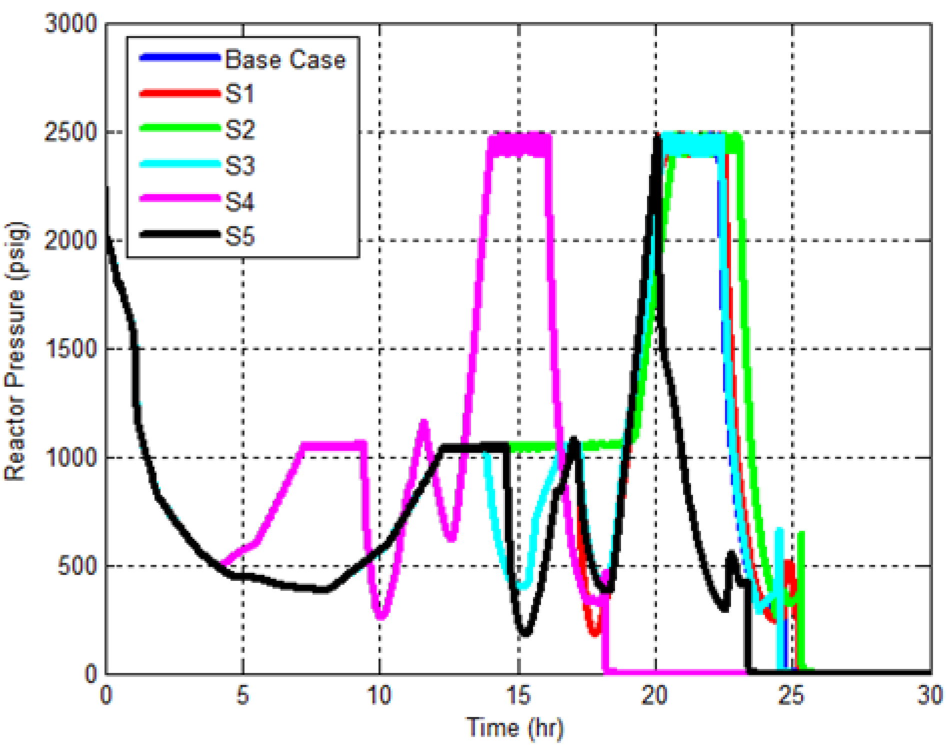
Fission product (Cs, I) distribution

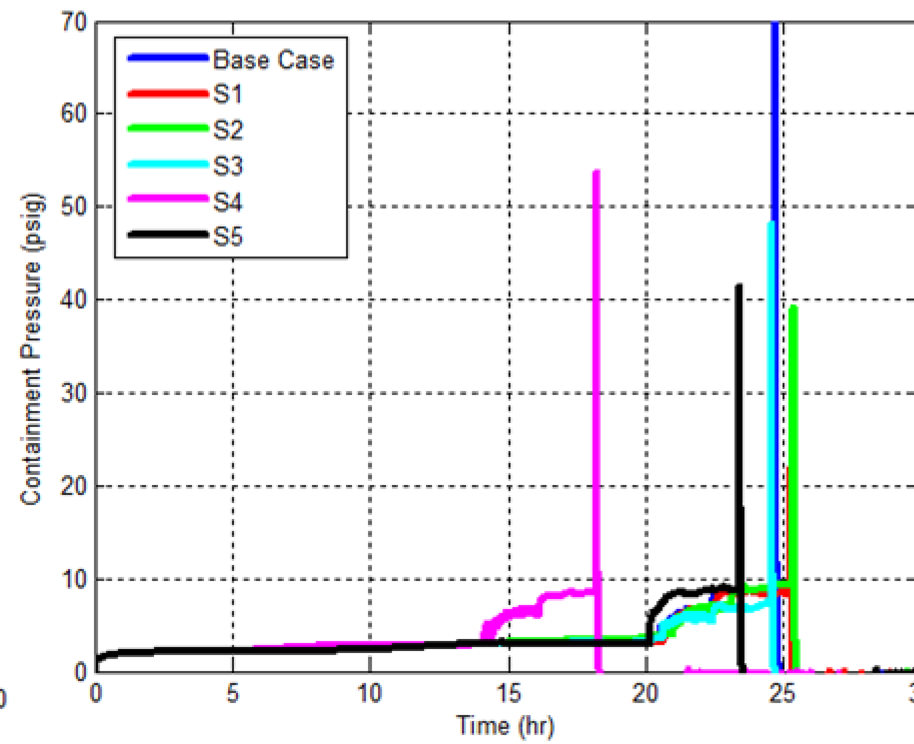




Unmitigated LTSBO Sensitivity

Case	Description
S1	SG1 SRV is assumed to fail at 1st cycle with water flow
S2	Secondary side SRVs do not fail
S3	Same as S1 but the flow area for SG3 SRV is reduced to correspond to a single SG
S4	Station batteries run out at 4 hours
S5	Pressurizer SRV is assume to stick open on 1st cycle





Unmitigated LTSBO Sensitivity

Case	Description
S1	SG1 SRV is assumed to fail at 1st cycle with water flow
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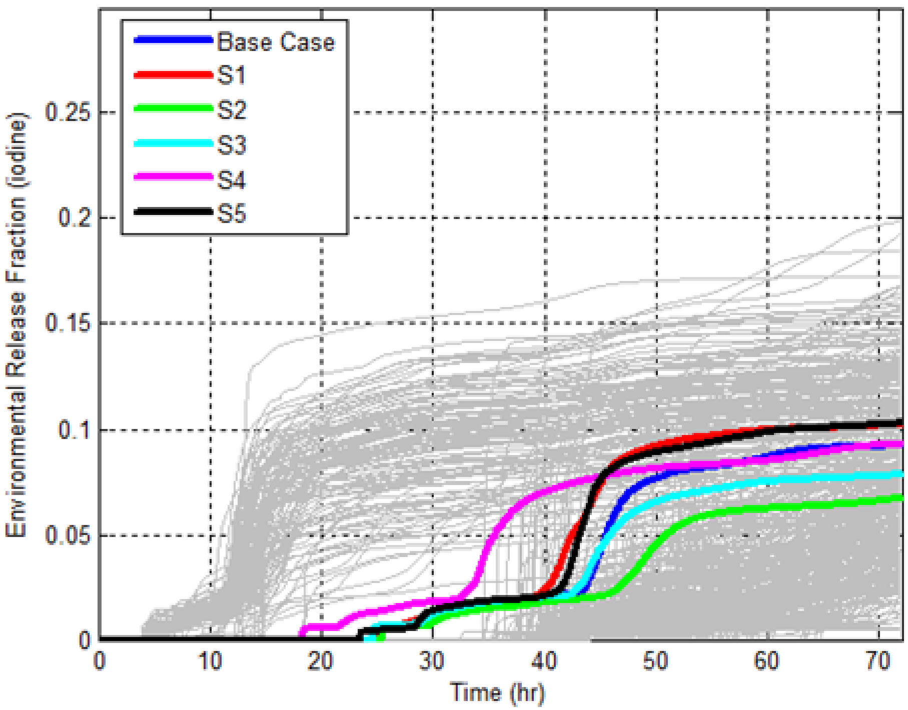
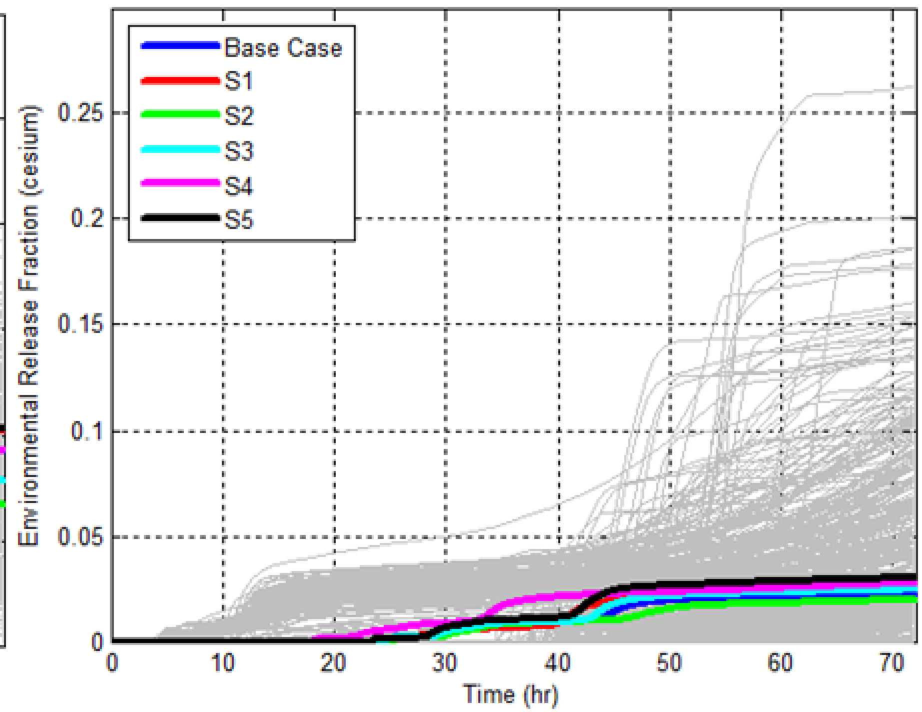
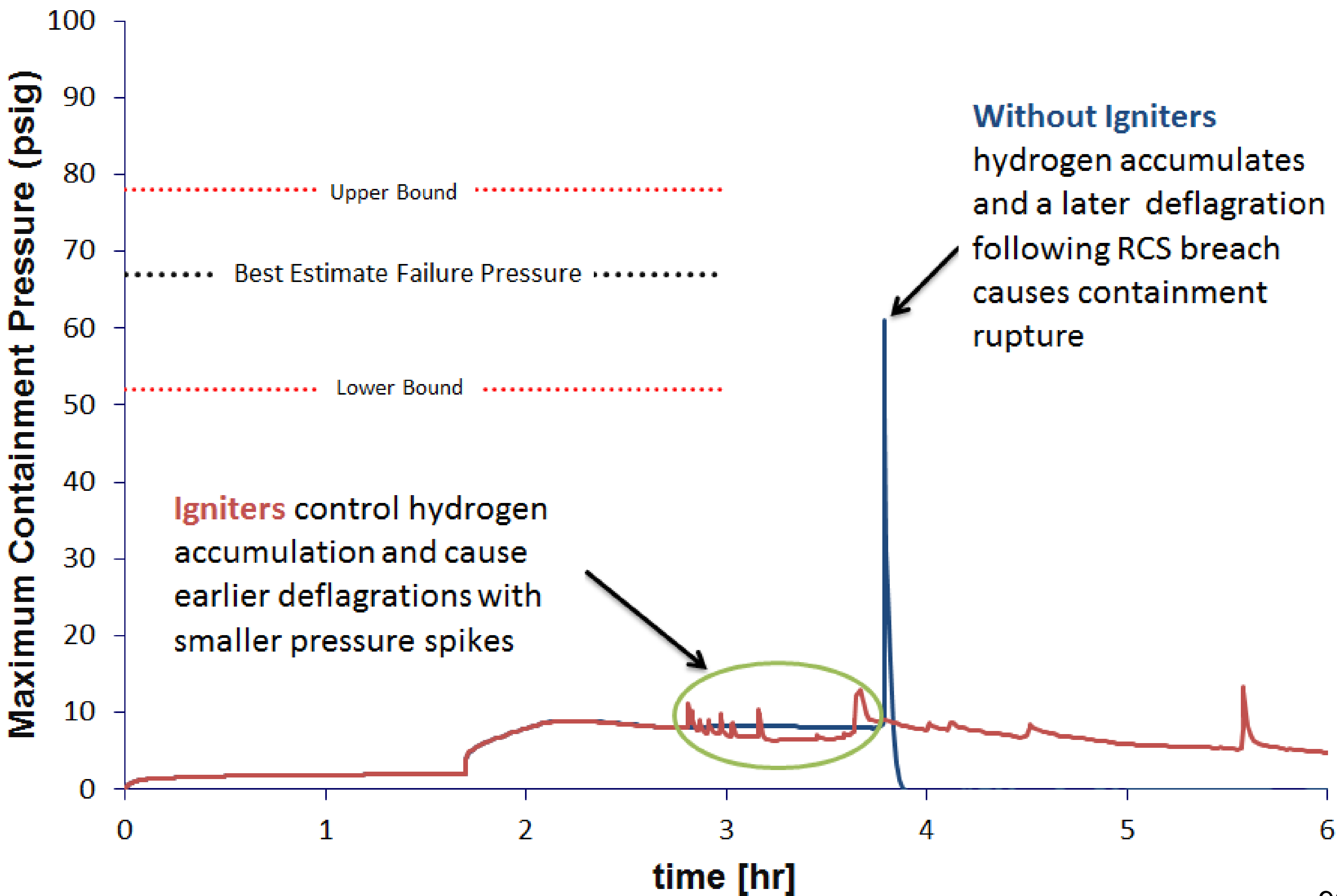


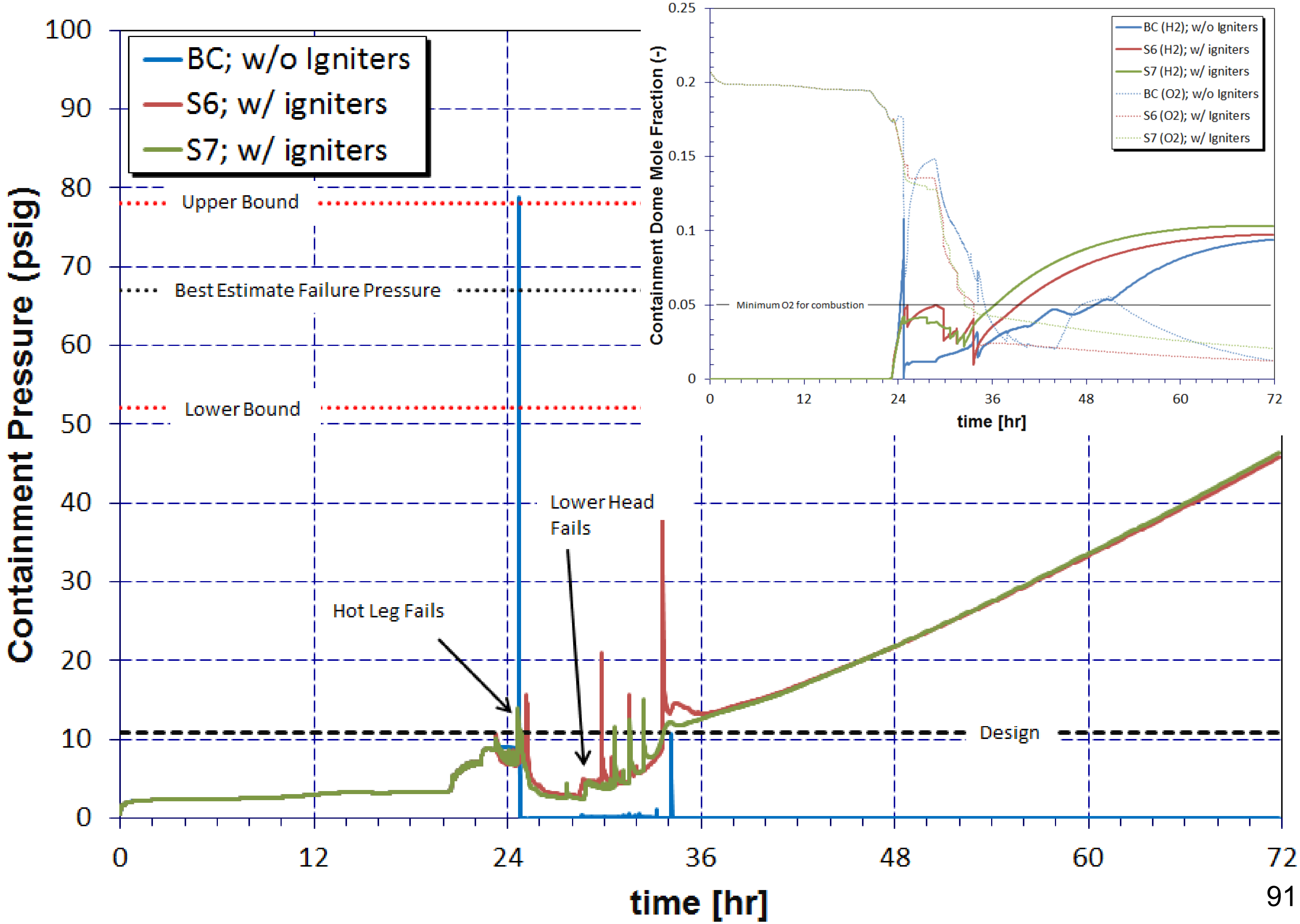


Note: Light curves are the unmitigated STSBO realizations without random ignition

Effect of Igniters (STSBO)



Effect of Igniters (LTSBO)



Summary of MELCOR Analysis

Containment overpressure failure could be early or late dependent upon whether the overpressure resulted suddenly from an H2 burn or eventually from a monotonic increase driven by steam production, non-condensable gas generation and heating of the containment atmosphere

- This early/late bifurcation in containment failure timing depends on multiple phenomena, for example, the amount of H2 vented from the RCS prior to an ignition source (hot leg failure) being present in containment
- Early containment failure depends on mechanisms to distribute hydrogen into the containment prior to ignition which was dependent on pressurizer SV function, and the containment failure pressure
- The major impact of modeling random sources of ignition in the unmitigated STSBO is to delay early containment failure by reducing large H2 buildups

The magnitude of the source term was most impacted by parameters that effect the timing of containment failure and those that contribute to fission product revaporization

- These parameters include pressurizer SV function and containment failure pressure

Summary of MELCOR Analysis (cont.)

The UA showed aerosol shape factor, LFL, the response of the lower ice chest door and ice chest bypass had a low impact on the source term

LTSBO sensitivity calculations had "early" containment failure at hot leg failure due to a hydrogen burn

– Qualitatively the release signature resembles most of the early STSBO failure cases

Successful use of igniters averts early containment failure

The ice is effective at slowing containment pressurization and can delay potential containment failure

MACCS Offsite Consequence Analysis

Shannon Thompson, PhD
Accident Analysis Branch
NRC Office of Nuclear Regulatory Research

MACCS Offsite Consequence Analysis

MACCS models the atmospheric transport and dispersion of releases, protective actions, exposure, doses, and health effects and reports early fatality and latent cancer fatality risks

Developed a site-specific MACCS model for Sequoyah

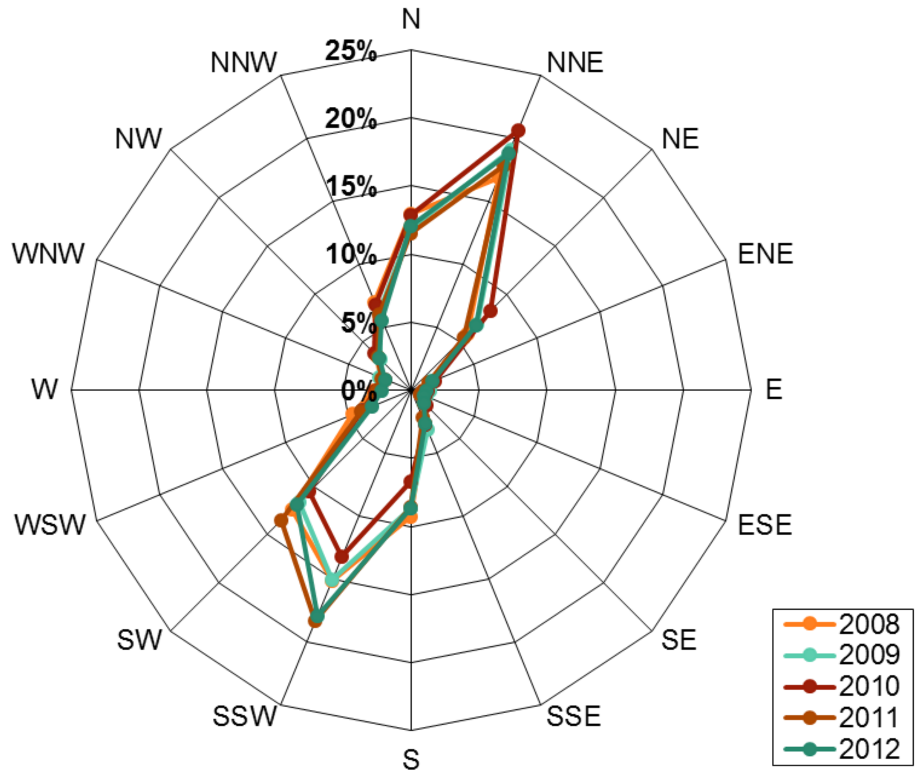
- Site meteorology, population, and land cover
- Seismically impacted road network and evacuation

Completed selected deterministic analyses of:

- STSBO
- STSBO Early Release
- LTSBO

Completed uncertainty analyses of unmitigated STSBO cases in parallel with the deterministic analyses

Sequoyah Weather Summary



2012 Meteorological Statistics

Category	Metric	Result
Stability Class	Unstable (%)	6.0%
	Neutral (%)	42.0%
	Stable (%)	52.0%
Precipitation	Total (in)	39.3
	Frequency (hr)	528
	Frequency (%)	6.0%
Wind Speed	Average (m/s)	1.66
Data Recovery	Percentage	99.4%

Sequoyah Population Data and Cohort Definitions

Population Density

Distance (mi)	Cumulative Population (2015)	Population Density (persons/mi^2)
1	236	75
2	2,654	211
5	27,081	345
10	97,731	311
15	280,278	397
20	483,009	384
30	692,837	245
40	906,106	180
50	1,101,452	140

Cohorts have unique emergency response characteristics

Cohort	Distance	Description
1	0 to 10 miles	Schools
2	0 to 10 miles	Special facilities (e.g., hospitals)
3	0 to 10 miles	Transit dependent evacuees
4	0 to 10 miles	General public early evacuees
5	0 to 10 miles	General public middle evacuees
6	0 to 10 miles	General public tail evacuees
7	10 to 15 miles	Shadow evacuees
8	0 to 10 miles, 10 to 15 miles, >15 miles	Non-evacuating public

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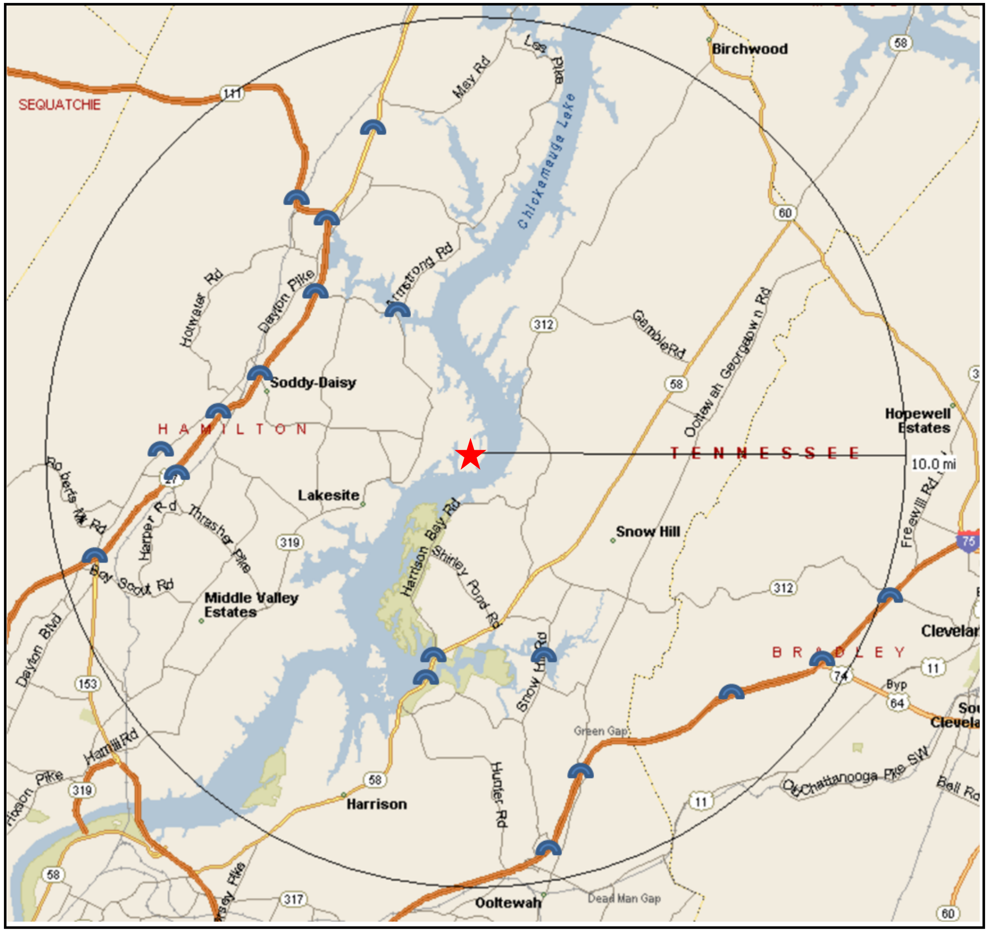
Seismically Impacted Evacuation of EPZ



10 mile EPZ

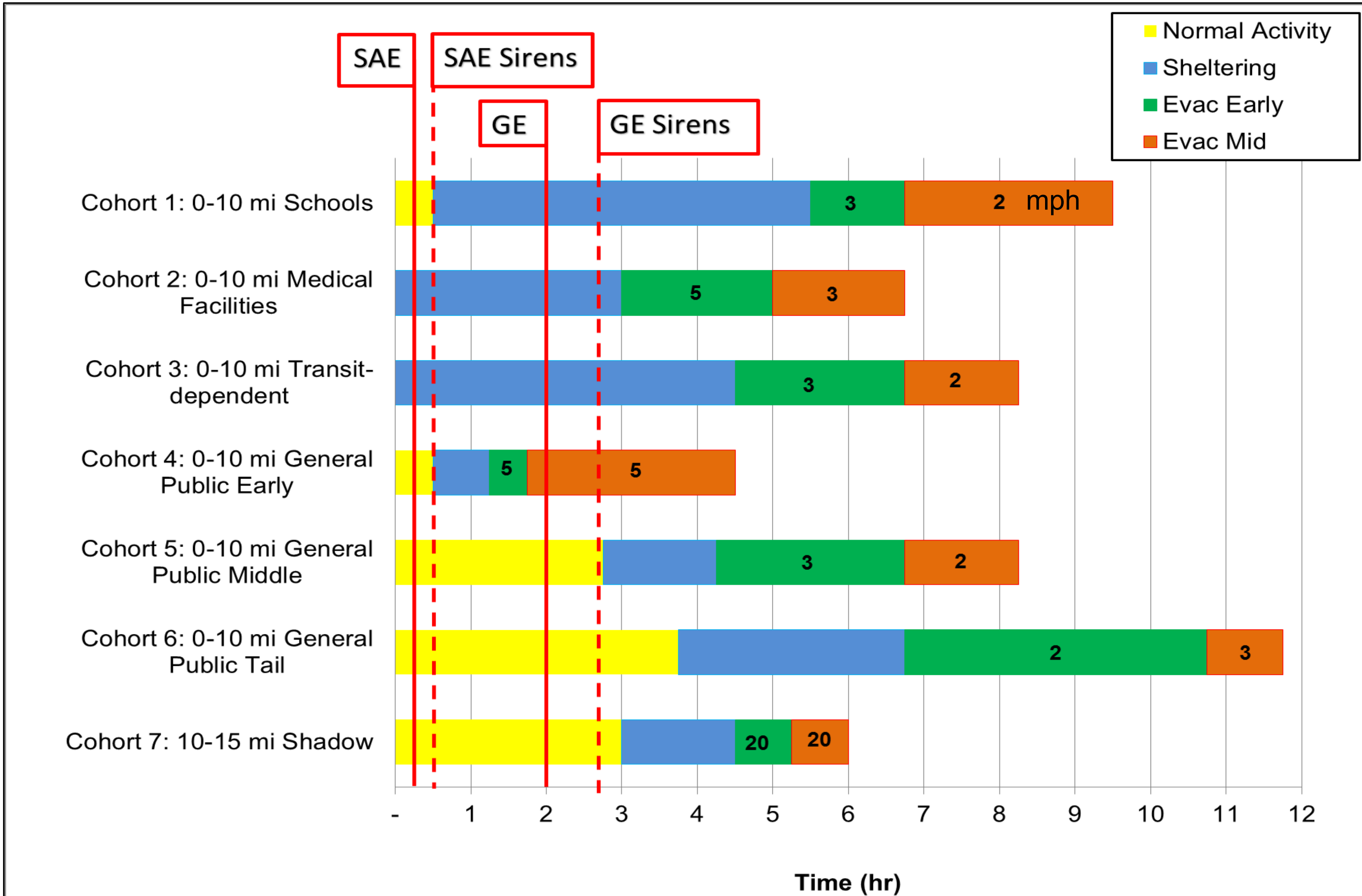
Initiating event is a beyond-design-basis earthquake

We assume bridges in the EPZ are unusable ( )

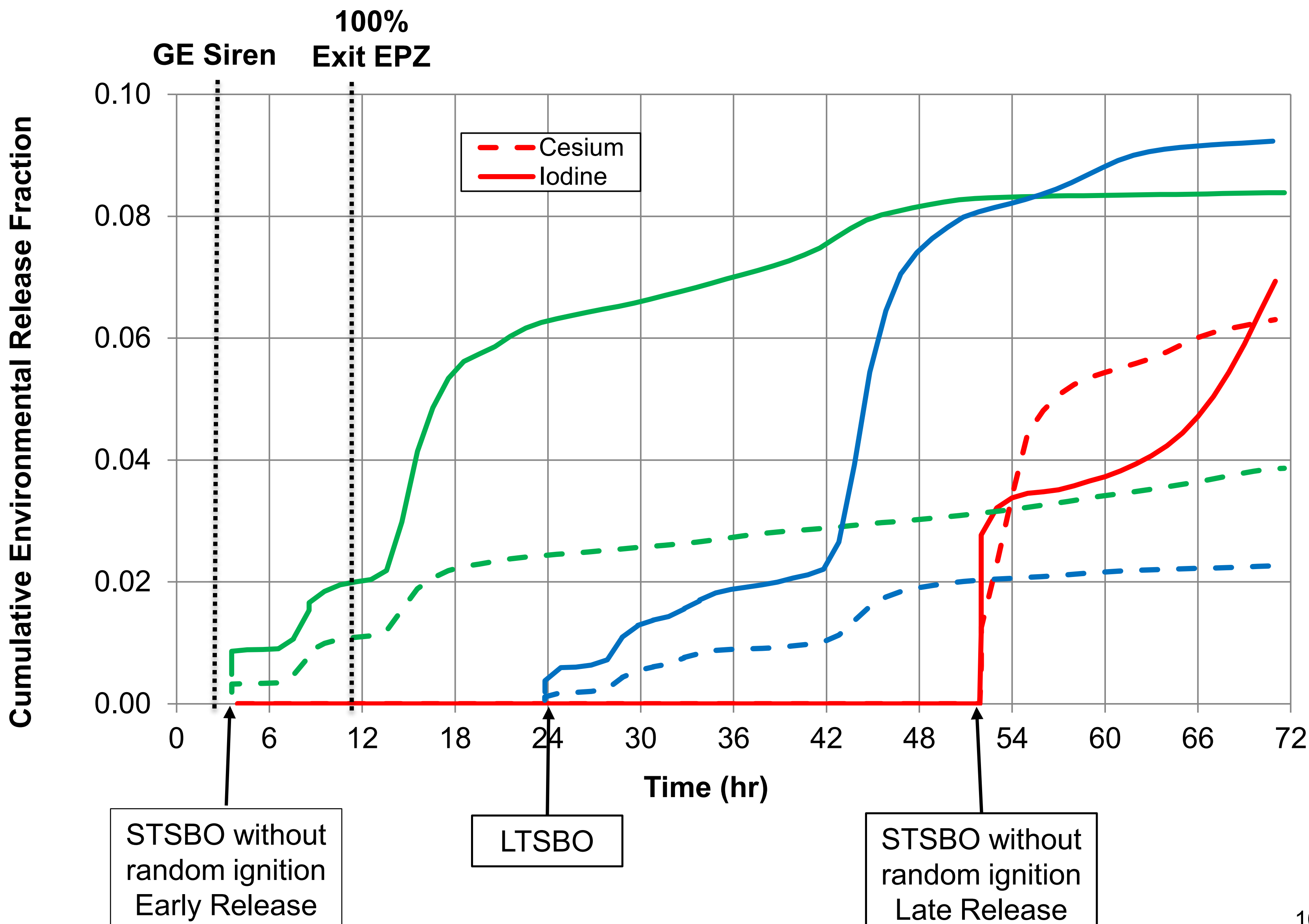
Road capacity analysis shows evacuation times increase from ~8 hours by 50% due to the reduced capacity and re-routing

EPZ evacuation time is completed ~12 hours from accident initiation

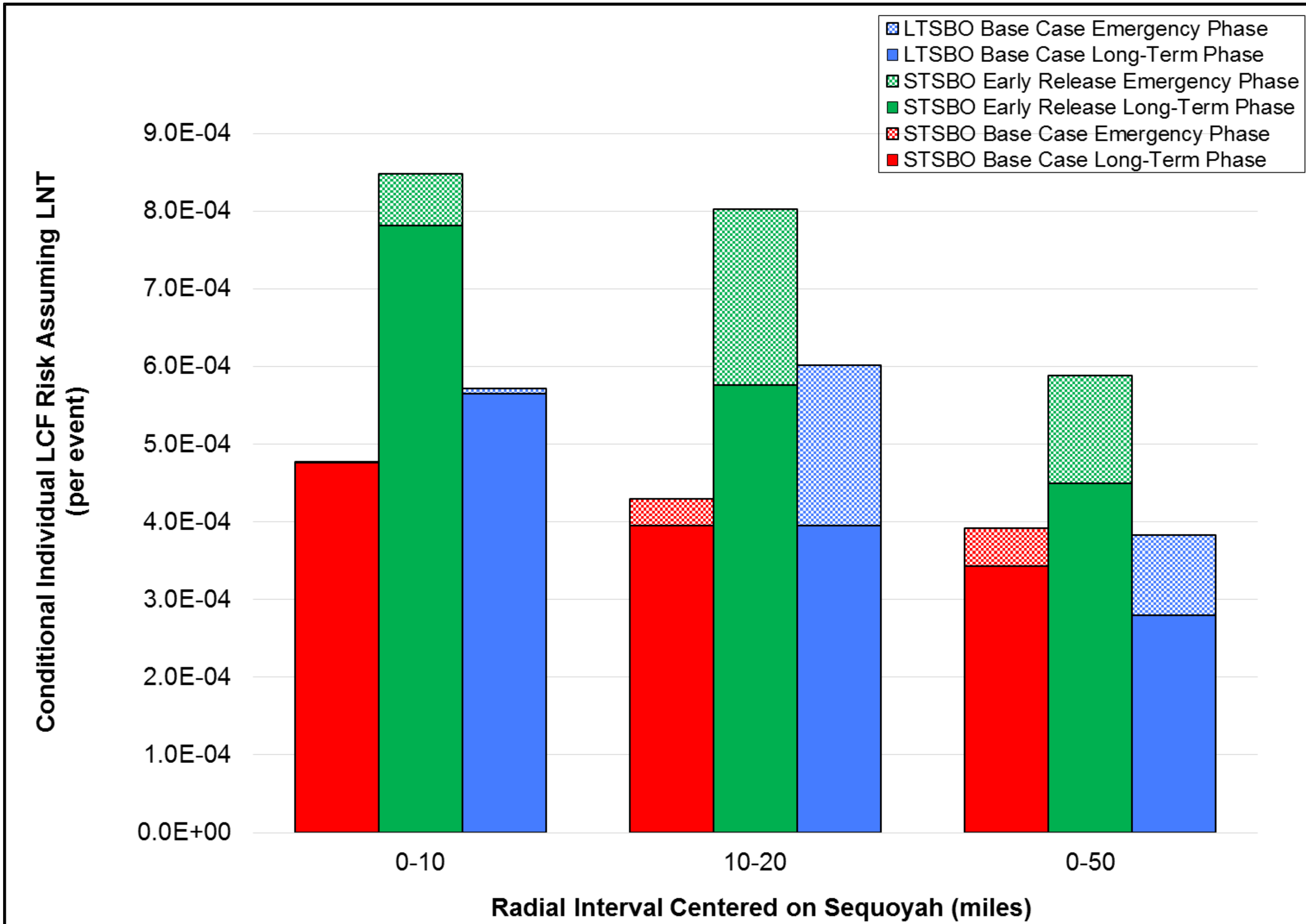
Evacuation Timelines and Travel Speeds



Release Profiles of Selected Cases



Individual LCF Risk Results



LCF Risk Result Trends

LCF risks are a function of release duration as well as magnitude and timing

For the deterministic cases, longer release durations result in increased long term LCF risks near the plant

Longer releases are more widely dispersed due to wind shifts

Dispersion in more directions leads to larger areas of contaminated land

This can result in exposure to more people which increases the long term LCF risk (particularly so if the contamination is just below the habitability criterion)

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Offsite Consequence Analysis Summary

The deterministic analyses LCF risk results range from 5E-04 to 9E-04 for the 0 – 10 mile region and generally decrease with increasing distance from Sequoyah.

The long term phase risks dominates the emergency phase risk results.

For these cases, the early fatality risks are zero.

Even in the case of a very early release, LCF risks are of similar magnitude as the other cases.

MACCS Uncertainty Analysis

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Accident Analysis Branch
NRC Office of Nuclear Regulatory Research

Sequoyah STSBO: MACCS Uncertain Parameter Groups

Deposition
- Wet Deposition
- Dry Deposition Velocities

Dispersion
- Crosswind Dispersion Linear Coefficient
- Vertical Dispersion Linear Coefficient
- Time-Based Crosswind Dispersion Coefficient*

Latent Health Effects
- Dose and Dose Rate Effectiveness Factor
- Lifetime Cancer Fatality Risk Factors
- Long Term Inhalation Dose Coefficients

Early Health Effects
- Threshold Dose
- Lethal Dose to 50% of population
- Hazard Function Shape Factor

Shielding Factors
- Groundshine Shielding Factors
- Inhalation Protection Factors

Emergency Response
- Evacuation Delay
- Evacuation Speed
- Hotspot Relocation Time and Dose Criteria
- Normal Relocation Time and Dose Criteria

Aleatory Uncertainty
- Weather Trials

**Other than this parameter, all the parameters were also uncertain in Peach Bottom and Surry UAs*

Time-Based Crosswind Dispersion Coefficient (CYCOEF)

- Linear coefficient for the time-based, crosswind dispersion model
- Switching to a time-based dispersion model at longer downrange distances (>30 km or 19 miles) is a commonly-used practice in atmospheric transport modeling

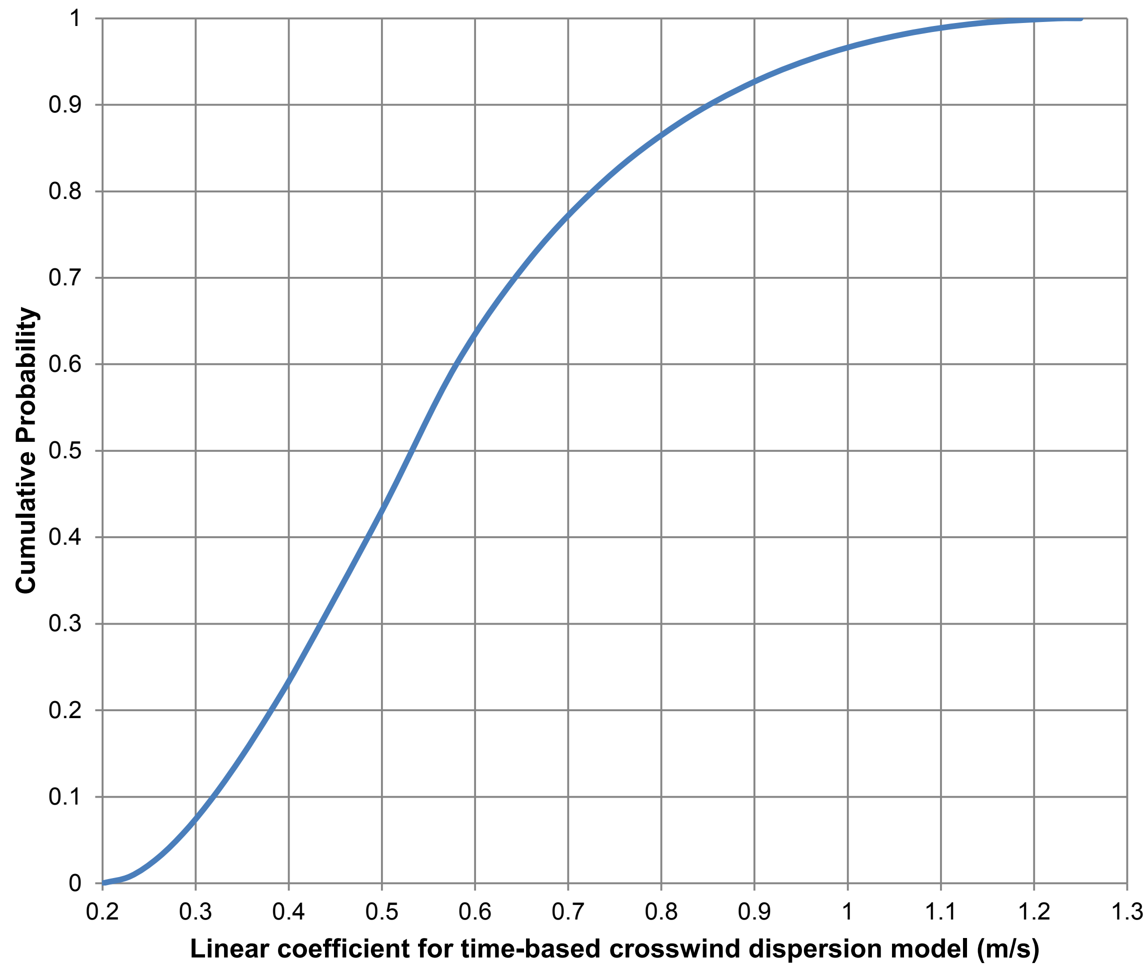

Figure 5-27 CDF of CYCOEF

Mean (over weather variation) individual LCF risk conditional on the STSBO w/o random ignition accident occurring (per event)

	0-10 Miles	0-20 Miles	0-30 Miles	0-40 Miles	0-50 Miles
Mean	3.5E-04	3.9E-04	3.6E-04	3.3E-04	2.9E-04
Median	2.9E-04	2.9E-04	2.8E-04	2.5E-04	2.3E-04
5th Percentile	8.4E-05	7.8E-05	7.6E-05	7.1E-05	6.7E-05
95th Percentile	8.3E-04	9.9E-04	9.0E-04	7.8E-04	7.1E-04

Table 6-7 Mean individual LCF risk conditional on the STSBO w/o random ignition accident occurring (per event) for five intervals centered on Sequoyah.

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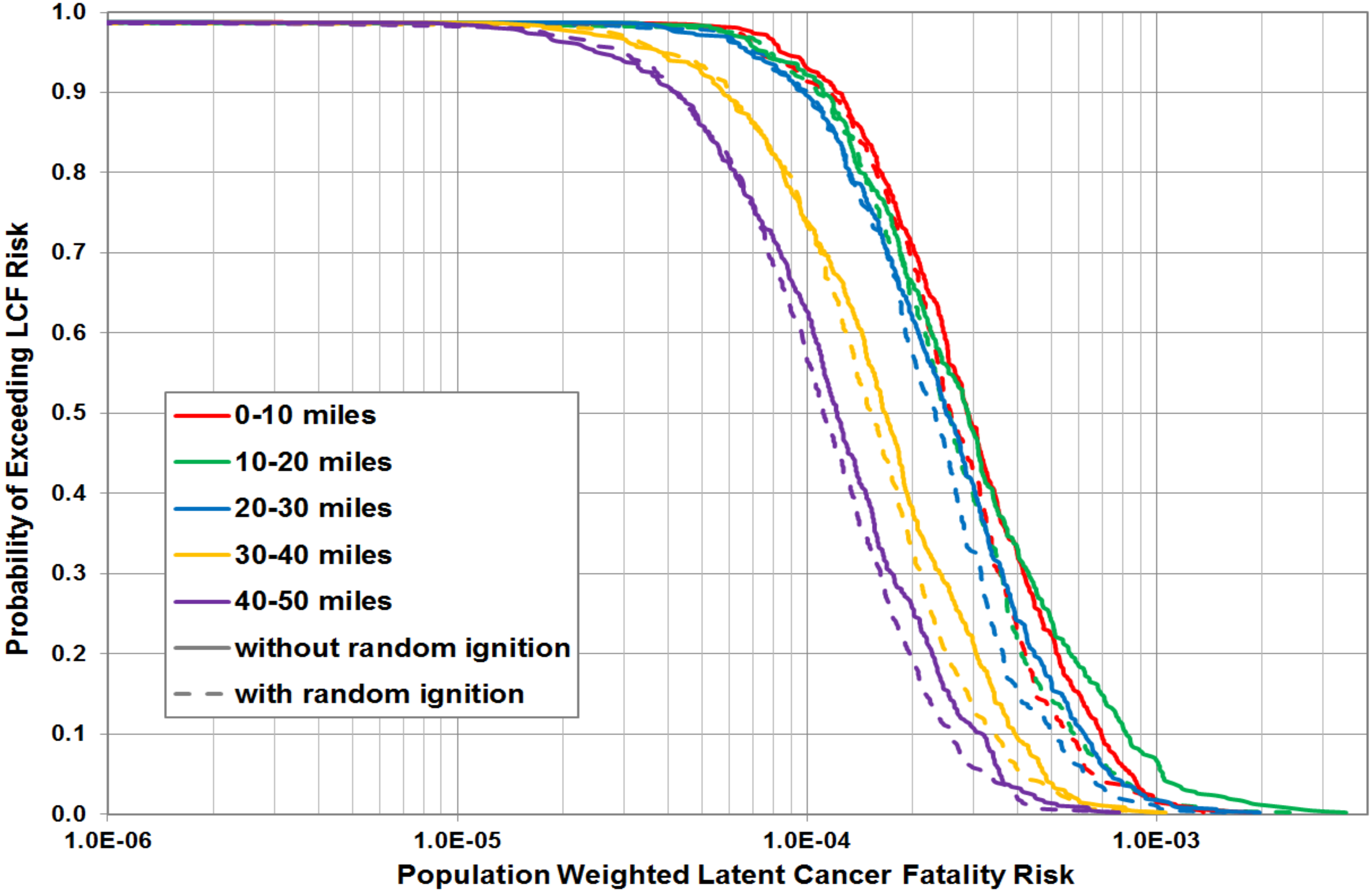
Mean (over weather variation) individual LCF risk conditional on the STSBO with random ignition accident occurring (per event)

	0-10 Miles	0-20 Miles	0-30 Miles	0-40 Miles	0-50 Miles
Mean	3.1E-04	3.1E-04	3.0E-04	2.7E-04	2.5E-04
Median	2.6E-04	2.5E-04	2.5E-04	2.3E-04	2.1E-04
5th Percentile	7.4E-05	7.7E-05	8.0E-05	7.5E-05	6.9E-05
95th Percentile	6.5E-04	7.2E-04	6.8E-04	6.1E-04	5.5E-04

Table 6-15 Mean, individual STSBO with random ignition LCF risk, conditional on accident (per event) for the MACCS uncertainty analysis for five intervals centered on Sequoyah.

Complementary Cumulative Distribution Function (CCDF) of Mean (over weather variation), Individual LCF Risk, Per STSBO Event, w/out Random Ignition



Mean, Individual, LCF Risk Regression Results within 0 – 10 mile for STSBO Based on LNT

Input	Rank Regression R^2 contr.	Rank Regression SRRC	Quadratic S_i	Quadratic T_i	Recursive Partitioning S_i	Recursive Partitioning T_i	MARS S_i	MARS T_i	Main Contribution	Conjoint Contribution
Final R^2	0.67		0.85		0.86		0.73			
	Without Random Ignition Sources									
SV_frac	0.28	0.50	0.28	0.36	0.26	0.55	0.35	0.39	0.25	0.12
CFRISK(8)	0.14	0.32	0.17	0.23	0.18	0.50	0.27	0.31	0.16	0.12
CFRISK(7)	0.06	0.26	0.08	0.11	0.01	0.08	0.10	0.11	0.05	0.03
DDREFA(8)	0.04	-0.21	0.05	0.07	0.03	0.17	0.07	0.09	0.04	0.05
GSHFAC(2)	0.03	0.19	0.03	0.08	0.02	0.11	0.05	0.05	0.03	0.04
rupture	0.04	-0.21	0.02	0.03	0.01	0.12	0.06	0.06	0.03	0.03
	With Random Ignition Sources									
Final R^2	0.62		0.82		0.89		0.76			
CFRISK(8)	0.18	0.42	0.28	0.38	0.25	0.54	0.38	0.50	0.23	0.14
SV_frac	0.17	0.37	0.17	0.24	0.21	0.52	0.10	0.15	0.14	0.12
DDREFA(8)	0.07	-0.26	0.06	0.09	0.06	0.26	0.08	0.14	0.06	0.08
CFRISK(7)	0.03	0.22	0.06	0.07	0.01	0.03	0.07	0.09	0.04	0.01
CFRISK(3)	0.03	0.17	0.03	0.11	0.01	0.07	0.05	0.10	0.02	0.05
GSHFAC(2)	0.03	0.20	0.03	0.03	---	---	0.04	0.08	0.02	0.01

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Mean, Individual, LCF Risk Regression Results in 10 – 20 mile Interval for STSBO Based on LNT

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contribution	Conjoint Contribution
Final R^2	0.64		0.80		0.80		0.72			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
	Without Random Ignition Source									
SV_frac	0.29	0.51	0.26	0.37	0.29	0.62	0.36	0.41	0.25	0.13
CFRISK(8)	0.09	0.25	0.12	0.19	0.06	0.15	0.10	0.16	0.08	0.06
CFRISK(7)	0.05	0.23	0.07	0.13	0.02	0.07	0.09	0.12	0.04	0.03
DDREFA(8)	0.03	-0.18	0.03	0.05	0.04	0.26	0.06	0.11	0.03	0.07
CFRISK(4)	0.04	0.17	0.04	0.06	0.01	0.07	0.05	0.05	0.03	0.02
rupture	0.04	-0.20	0.04	0.06	0.00	0.06	0.05	0.12	0.03	0.04
shape_fact	0.00	0.06	0.02	0.04	0.07	0.15	0.01	0.09	0.02	0.04
	With Random Ignition Source									
Final R^2	0.59		0.79		0.82		0.71			
SV_frac	0.22	0.39	0.16	0.26	0.20	0.55	0.15	0.47	0.15	0.20
CFRISK(8)	0.12	0.36	0.15	0.21	0.14	0.36	0.21	0.26	0.13	0.09
DDREFA(8)	0.05	-0.24	0.06	0.10	0.09	0.36	0.05	0.13	0.05	0.10
CFRISK(4)	0.02	0.14	0.00	0.09	0.01	0.13	0.05	0.35	0.02	0.13

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Mean, Individual, LCF Risk Regression Results within a 0 – 50 mile for STSBO Based on LNT

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contribution	Conjoint Contribution
Final R^2	0.64		0.80		0.80		0.72			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
	Without Random Ignition Source									
SV_frac	0.29	0.51	0.26	0.37	0.29	0.62	0.36	0.41	0.25	0.13
CFRISK(8)	0.09	0.25	0.12	0.19	0.06	0.15	0.10	0.16	0.08	0.06
CFRISK(7)	0.05	0.23	0.07	0.13	0.02	0.07	0.09	0.12	0.04	0.03
DDREFA(8)	0.03	-0.18	0.03	0.05	0.04	0.26	0.06	0.11	0.03	0.07
CFRISK(4)	0.04	0.17	0.04	0.06	0.01	0.07	0.05	0.05	0.03	0.02
rupture	0.04	-0.20	0.04	0.06	0.00	0.06	0.05	0.12	0.03	0.04
shape_fact	0.00	0.06	0.02	0.04	0.07	0.15	0.01	0.09	0.02	0.04
	With Random Ignition Source									
Final R^2	0.59		0.78		0.81		0.67			
CFRISK(8)	0.14	0.38	0.22	0.28	0.14	0.39	0.30	0.37	0.16	0.10
SV_frac	0.19	0.40	0.15	0.22	0.23	0.55	0.17	0.21	0.15	0.11
DDREFA(8)	0.06	-0.24	0.06	0.10	0.12	0.37	0.10	0.13	0.07	0.08
GSHFAC(2)	0.04	0.21	0.05	0.10	0.00	0.05	0.07	0.09	0.03	0.03
CFRISK(7)	0.03	0.16	0.04	0.07	0.02	0.03	0.05	0.05	0.03	0.01

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

CCDF of mean, population-weighted early fatality risk (STSBO w/o random ignition) per event

Of the 467 MACCS realizations, 47 realizations (about 10%) had doses high enough to calculate a nonzero early fatality risk within 5 miles.

- Even fewer, 12 realizations (3%) in the set with random ignition

There is no early fatality risk beyond 5 miles for any of the realizations.

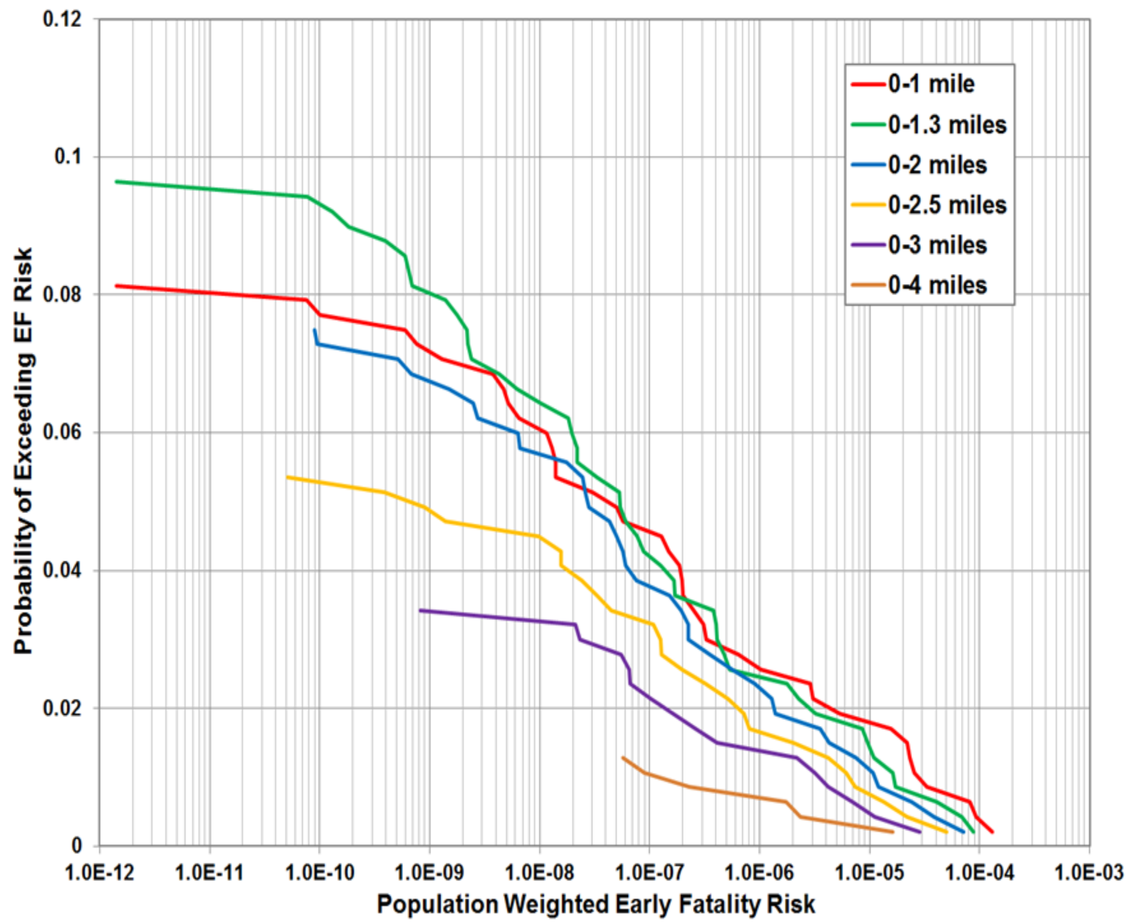


Figure 6-19 Complimentary cumulative distribution function of mean, population-weighted early fatality risk (STSBO w/o random ignition) within six distance intervals centered on Sequoyah

Conclusions for Offsite Consequences

Using LNT dose response, the conditional LCF risks of both deterministic and uncertainty analyses range from about 3E-04 to 9E-04 for the 0 – 10 mile region.

Contributions from the long term phase risks dominate the emergency phase risks for the large majority of the LCF risk results.

The early fatality risks are essentially zero.

Parameters that show up as important to uncertainty in LCF risk are: safety valve flow area, the cancer fatality risk and dose-and-dose-rate-effectiveness factors for the "residual" organ, the cancer fatality risk factors for colon and lung, containment rupture pressure, and groundshine shielding factors.

Summary of Public Comments

Salman Haq, PhD, PE
Accident Analysis Branch
NRC Office of Nuclear Regulatory Research

Public Comments

Draft Technical Report was placed in public ADAMS and request for comment was published in Federal Register on April 12, 2016

A blog was posted on NRC public website informing the public about the study

Public Meeting was held on April 20, 2016 at the Sequoyah Nuclear Plant Training Center, in Soddy-Daisy, Tennessee

Summary of Submitted Public Comments

The analysis seems unbalanced in its treatment of damage due to the beyond design basis earthquake

- Extensive damage to safety systems
- Limited damage to infrastructure within 10 mile EPZ

The analysis should more rigorously consider the impact of the large earthquake on evacuation times

- The analysis didn't consider the impact of downed power lines and poles that may impede evacuation pathways
- The analysis didn't consider all bridges within the EPZ

Summary of Submitted Public Comments (cont.)

Health consequences analysis was flawed:

- Health effects modeling did not consider age and sex
- Lessons of Fukushima have not been learned as to health consequences (referenced a Physicians for Social Responsibility Report dated March 2015)

Question how risk can be lower after an accidental release, as compared to general U.S. cancer fatality risk

Question if consequences from potential severe nuclear accident scenarios really are smaller than previously calculated

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Sandia National Laboratories

Overall Results and Conclusions

Jonathan Barr
Accident Analysis Branch
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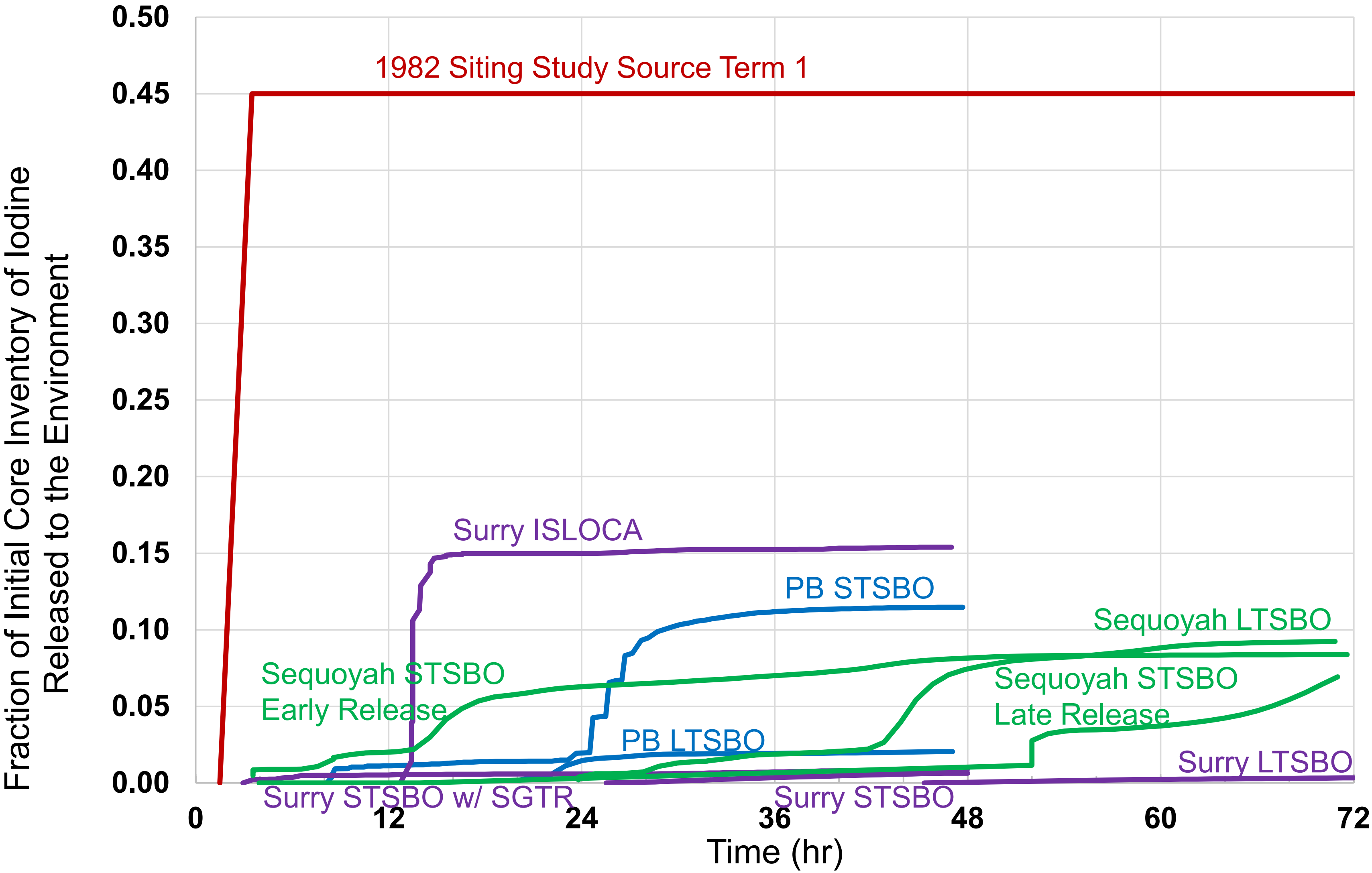
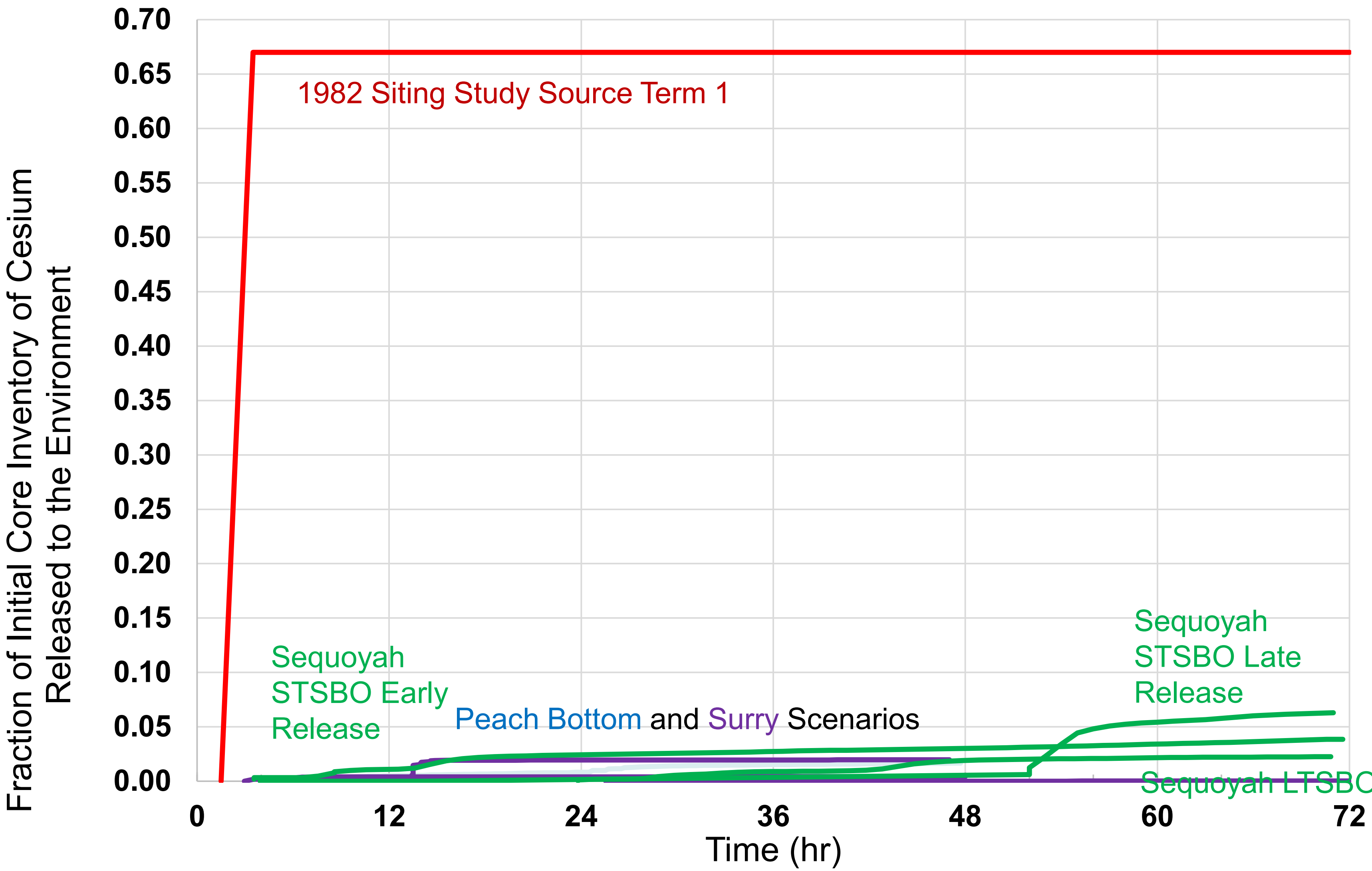
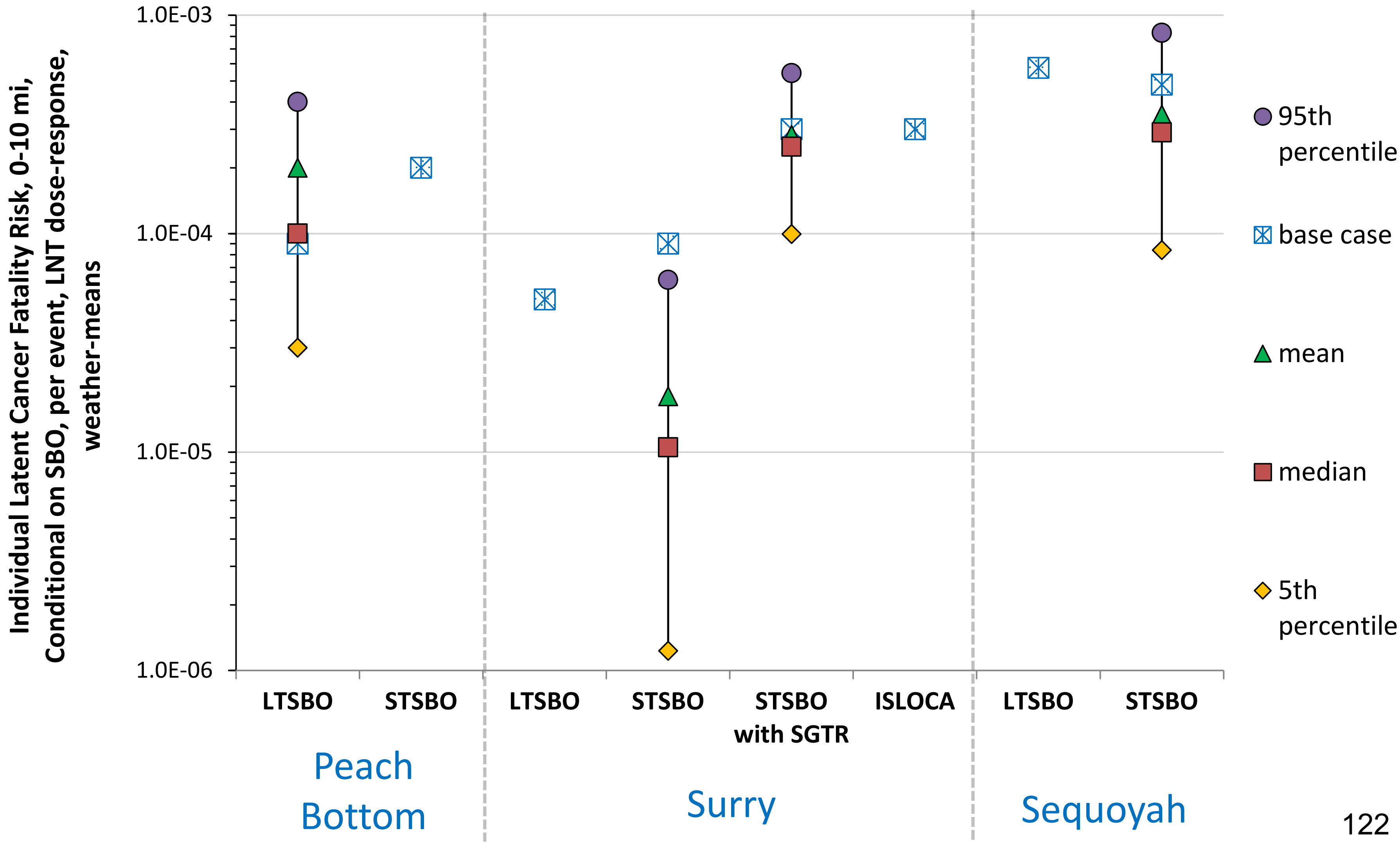
Comparison of SOARCA Results for Iodine Release to Environment for Unmitigated Base Case Scenarios



Comparison of SOARCA Results for Cesium Release to Environment for Unmitigated Base Case Scenarios



Comparison of SOARCA Results for Conditional Individual LCF Risk (0-10 mi) for Unmitigated Scenarios



Sequoyah SOARCA Conclusions

- For unmitigated STSBO (without igniters), the two potential containment outcomes are either early or late failure
- Successful use of igniters averts early containment failure
- The major impact of modeling random sources of ignition in the unmitigated STSBO is generally to delay early containment failure
- The ice is effective at mitigating the slow containment pressurization and it can delay potential containment failure
- Essentially zero individual early fatality risk was calculated for Sequoyah STSBO and LTSBO
- Even for cases resulting in early release to environment, the conditional individual LCF risk is small
- Conditional individual latent cancer fatality risk results for Sequoyah are similar to those from other SOARCA analyses

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Uses of SOARCA and Next Steps

Patricia Santiago, Chief
Accident Analysis Branch
NRC Office of Nuclear Regulatory Research

Uses of SOARCA Modeling to Support Agency Activities

Technical Bases for Regulatory Framework

MELCOR and MACCS analyses BWR Mark I filtered vent analysis and CPRR (Tier 3 – 5.1)

Other containments and hydrogen (Tier 3 – 5.2 and 6)

Expedited fuel transfer (MACCS)

Emergency preparedness – decommissioning exemption requests

Uncertainty analyses determine most influential parameters

MACCS parameter guidance supports new and advanced reactor designs, knowledge management for severe accident analysis

Licensing and Environmental Review Uses of MACCS

Environmental assessment and impact statement analyses

Waste Confidence technical bases for spent fuel fires and D/FGEIS

Hearing support for technical analyses (Indian Point; Seabrook)

Insights for Emergent Issues with MELCOR and MACCS

Supported NRC incident response to Fukushima event

Fukushima Forensic Analysis to better understand Fukushima accident progression

Uses of SOARCA Modeling to Support Agency Knowledge Management

Knowledge management for Severe Accident Analyses

SOARCA model and results

- Used in NRC training classes
- Used for staff knowledge about plant models
- Informs L3 PRA in modeling and analysis of severe accidents and consequences
- Updates the input decks for future needs and timely response in-house
- Inform international research planning and benchmarking

Next Steps

Prepare Response to NRC Office/Public Comments – May 30

ACRS full committee – June 8

Provide Information Paper on Sequoyah to OEDO – Sept 30

Submit NUREG for publication – Dec 31

Draft NUREG summarizing insights and lessons learned from SOARCA Uncertainty Analyses – Dec 31

Update Best Practices NUREGs – March 2017

References

SECY-12-0092, “State-of-the-Art Reactor Consequence Analyses – Recommendation for Limited Additional Analysis” (July 2012)

NUREG-1935, State-of-the-Art Reactor Consequence Analyses (SOARCA) Report (November 2012)

NUREG/BR-0359, Modeling Potential Reactor Accident Consequences, Rev. 1 (December 2012, update expected summer 2016)

NUREG/CR-7110, Vol. 1, SOARCA Project Peach Bottom Integrated Analysis, Rev. 1, (May 2013)

NUREG/CR-7110, Vol. 2, SOARCA Project Surry Integrated Analysis, Rev. 1 (August 2013)

NUREG/CR-7008, MELCOR Best Practices as Applied in the SOARCA Project (August 2014)

NUREG/CR-7009, MACCS Best Practices as Applied in the SOARCA Project (August 2014)

NUREG/CR-7155, SOARCA Project Uncertainty Analysis of the Unmitigated Long-Term Station Blackout of the Peach Bottom Atomic Power Station (May 2016)

Acronyms

BEIR	Biological Effects of Ionizing Radiation
CDF	Cumulative distribution function
CCDF	Complementary CDF
DBA	Design basis accident
EPZ	Emergency planning zone
ETE	Evacuation time estimate
FTC	Failure to open
FTO	Failure to close
gpm	Gallons per minute
LCF	Latent cancer fatality
LFL	Lower flammability limit
LHS	Latin Hypercube Sampling
LNT	Linear no threshold
LTSBO	Long term station blackout
MACCS	MELCOR Accident Consequence Code System
MCCI	Molten concrete core interaction
Przr	Pressurizer
PWR	Pressurized water reactor
RCPSL	Reactor coolant pump seal leakage
RCS	Reactor coolant system
Rlz	Realization
RN	Radionuclide
RPV	Reactor pressure vessel
SGTR	Steam generator tube rupture
SME	Subject matter expert
SNL	Sandia National Laboratories
SOARCA	State-of-the-Art Reactor Consequence Analyses
STSBO	Short term station blackout
SV	Safety valve
TDAFW	Turbine-driven auxiliary feedwater
UA	Uncertainty Analysis

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MELCOR and MACCS Parameter Names

SV_frac *– safety valve open area fraction or flow area*

priSVcycles *– number of primary safety valve cycles experienced*

rupture *– containment rupture pressure*

EU_melt_T *– effective temperature of the eutectic reaction for zircaloy oxide and uranium oxide*

ajar *– ice condenser doors open fraction*

shape_fact *– aerosol dynamic shape factor*

CFRISK(8) *– cancer fatality risk factor for "residual" organ*

CFRISK(7) *– cancer fatality risk factor for colon*

CFRISK(3) *– cancer fatality risk factor for breast*

CFRISK(4) *– cancer fatality risk factor for lung*

DDREFA(8) *– dose-and-dose-rate-effectiveness factor for "residual" organ*

GSHFAC(2) *– groundshine shielding factor for normal activity during emergency phase (fully correlated with same factor during long-term phase)*