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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

+ + + + +

WEDNESDAY

OCTOBER 18, 2017

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., John W. Stetkar, Chairman, presiding.

COMMITTEE MEMBERS:

JOHN W. STETKAR, Chairman

RONALD G. BALLINGER, Member

DENNIS C. BLEY, Member

CHARLES H. BROWN, JR. Member

MICHAEL L. CORRADINI, Member

JOSE MARCH-LEUBA, Member

DANA A. POWERS, Member

JOY L. REMPE, Member

GORDON R. SKILLMAN, Member

MATTHEW SUNSERI, Member

DESIGNATED FEDERAL OFFICIAL:

HOSSEIN NOURBAKHSH

ALSO PRESENT:

ANDREA D. VEIL, ACRS Executive Director

NATE BIXLER, Sandia National Laboratories

KEITH COMPTON, RES

HOSSEIN ESMAILI, RES

ED FULLER, RES

TINA GHOSH, RES

TREY HATHAWAY, RES

SALMAN HAQ, RES

DONALD HELTON, RES

WILLIAM ORDERS, NRR

EDWARD ROACH, NSIR

PATRICIA SANTIAGO, RES

AMY SHARP, RES

TODD SMITH, NSIR

CASEY WAGNER, Dycoda LLC

KIMBERLY WEBBER, RES

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

P R O C E E D I N G S

8:31 a.m.

CHAIR STETKAR: The meeting will now come to order. This is a meeting of the Advisory Committee on Reactor Safeguards, Regulatory Policies and Practices Subcommittee.

I'm John Stetkar, Chairman of the Subcommittee Meeting. And you're not.

Members in attendance today are Ron Ballinger, Matt Sunseri, Dick Stillman, Dana Powers, Mike Corradini, Jose March-Leuba, Dennis Bley, I forgot you, Charlie Brown, and Joy Rempe.

Hossein Nourbakhsh is the Designated Federal Official for this meeting.

The purpose of today's meeting is to discuss the State-of-the-Art Reactor Consequence Analyses Project for the Sequoyah Integrated Deterministic and Uncertainty Analyses.

Today we have Members of the NRC Staff and Sandia National Laboratories to brief the Subcommittee.

The ACRS was established by statute and is governed by the Federal Advisory Committee Act. That means that the Committee can only speak through its published letter reports.

We hold meetings to gather information to support our deliberations. Interested parties who wish to provide comments can contact our office requesting time after the meeting announcement is published in the Federal Register.

That said, we set aside ten minutes for spur-of-the moment comments from members of the public attending or listening to our meetings. Written comments are also welcome.

The ACRS section of the NRC public website provides our charter bylaws, letter reports and full transcripts of all full and Subcommittee meetings, including slides presented there.

The rules for participation in today's meeting were announced in the Federal Register on October 10, 2017. The meeting was announced as an open meeting.

No written statement or request for making an oral statement to the Subcommittee has been received from the public concerning this meeting.

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

Therefore, we will request that all participants in this meeting use the microphones located throughout the meeting room when addressing the Subcommittee.

The participants should first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

We have a bridge line established for the public to listen into the meeting.

To minimize disturbance, the public line will be kept in a listen-in-only mode, and I'll open it at the end of the meeting for public comments.

To avoid disturbance, I request that all attendees and everyone else in the meeting room put your electronic devices like cell phones, beepy things and whatever, in the off or noise-free mode.

Now, I'll proceed with the meeting, and I'll call upon Pat Santiago of the NRC Office of Nuclear Regulatory Research to begin today's presentations. Pat?

MS. SANTIAGO: Thank you, good morning.

We appreciate all the feedback that we got from the Subcommittee Members in the May of 2016 Subcommittee Meeting, as well as the June meeting of this year.

And we worked the last few months to address all the comments that we received.

On this second slide, I want to talk a little bit about our schedule.

Our Sequoyah Analyses is going to be published as a NUREG/CFR report, and it's due to the Commission November 30th. And we'll be sending it to the NRC Publications Branch at the same time.

We're relying on the ACRS Members to service our peer reviewers and we're requesting a letter documenting your review. To facilitate that process, we're briefing the full Committee in November of this year.

And this works well with our schedule since the NUREG is due to the Commission November 30th. So, we appreciate your support for that.

We hope to be able to address any final comments from today's Subcommittee meeting in the next few weeks, but as you can see from our schedule, any additional analysis would not be documented in this report.

Rather, we could possibility add additional clarifications on what was done within the scope of this study. And time to include it within the NUREG will be submitted for the Commission, and for publication.

Also, separately, much of the same team is in the process of updating the Surry Uncertainty Analysis. And currently, that NUREG report is due to the Commission June of 2018.

Previously, the Subcommittee had indicated that you may not need to review the updated Uncertainty Analysis.

However, we can discuss whether you'd like the team to come back with the updated Uncertainty Analysis next year.

MEMBER BLEY: Pat?

MS. SANTIAGO: In addition to that --

MEMBER BLEY: Pat? Excuse me. Can you say something about the extent of that update?

And is it to make things at least methodologically consistent with what's been done here?

MS. SANTIAGO: Okay, so the Sequoyah Update or the Surry --

MEMBER BLEY: The Surry Uncertainty.

MS. SANTIAGO: We'll talk a little bit about that --

MEMBER BLEY: Is that in the presentation?

MS. SANTIAGO: Okay, yes.

MS. GHOSH: So, there are a couple of main things.

CHAIR STETKAR: Move a little closer, please.

MS. GHOSH: Is this better?

You're aware of some of the major updates we made to the Sequoyah Analysis between last year and this year. Some of them are very relevant to Surry as well.

All of the safety valve parameters are important for Surry too, because of the induced steam generator tube rupture in that case.

And so for a different reason but still very important. So, we definitely wanted to make that update.

So, basically, all of the relevant updates in the Sequoyah Analysis in the last year, we are implementing in Surry, and that will change the results somewhat.

The other thing is at the Subcommittee on Surry, we got some comments from Bill Shack and others on our steam-generator tube rupture modeling. So, right now, we are currently in the throes of updating that modeling.

So, we've gone and gathered more information, talk to more experts, and we're in the process of updating that modeling. So, those are the two main areas.

MEMBER BLEY: Well, since you brought up that last one, are you going to talk about the reason why you don't have any steam-generator tube rupture in the Sequoyah Analysis?

Is that part of your presentation?

MS. GHOSH: No, we had tried -- okay, so we did not address induced steam-generator tube rupture.

MEMBER BLEY: So, you don't have to update it?

MS. GHOSH: For Sequoyah. No, that was not within the scope of the study, and we tried to make that clear in the introduction that we did not do that.

MEMBER BLEY: You made it very clear. What wasn't clear was why the scopes are different, why you didn't do it here and you've done it there.

And now you're going back and redoing it over on the other plant.

MS. GHOSH: That's what we say in the Sequoyah study, is look at the Surry study for insights on tube rupture because --

MEMBER BLEY: Yes, you do.

MS. GHOSH: -- because those should be

consistent. The scope of the Sequoyah study was really focused on containment issues.

MEMBER BLEY: You say that kind of at the end, that's where the focus was, but you don't say anything about why.

Is there a simple why, or it was just the way the scope was set up?

MS. GHOSH: You know, at the time --

MEMBER BLEY: This was added in to look at an ice condenser, I guess.

MS. GHOSH: Yes, when we went to the Commission and said we should finish this third pilot study for these reasons.

Some of the big motivators at the time were still doing the post-Fukushima regulatory actions, and we wanted to make sure we weren't missing anything in terms of hydrogen challenges for the ice condenser containment.

So, that was a major focus of this third plant. It was really focused on the unique aspects of the containment.

MEMBER BLEY: This kind of makes sense but, at least to this reader, it didn't jump off the page at me.

In fact, we're focused on the containment as kind of an afterthought at the end. It's like we didn't do these things but that's because we're focused on the containment.

So, you didn't really introduce it with --

MS. GHOSH: Ah, okay.

MEMBER BLEY: -- why you're doing this.

MS. SANTIAGO: I would re-look at the Executive Summary. I think that's an important point, to make sure that it's clear why we did what we did.

MEMBER BLEY: Maybe it is, but it wasn't clear to me.

MEMBER REMPE: Let's go a bit further.

If you had looked at it -- and since you are doing it at Surry, maybe you have some insights on what would have happened if you had looked at Sequoyah, especially with the Level 3.

It's a Westinghouse plant, right? Is there something you think you might be able to say?

For example, you did -- most of the time, as I recall, and Don's in the audience and he can correct me, but when they were doing the Level 3, they decided, well, because it's a Westinghouse plant, it wouldn't have been a big deal, except for if you had a loop seal that was about 480 gallons per minute.

And do you think you might have the same kind of conclusions that they had in the Level 3? And then, there's only that one sensitivity study you did, where you did have that high of a loop seal leak.

And I'm just wondering if you can kind of make some comments in your Executive Summary, if you're updating it, that could be based on engineering judgment from all the ongoing evaluations you've done.

And the only place I think you might want to think about acknowledging it makes a difference is how what happens with this 480 gallon-per-minute leak.

Because instead of talking about the containment failure, you might have had a bypass. And it looks like that someone is agreeing with me by -- up and down.

But anyway, it seems like there's some things you might want to go a bit further and say that it's probably -- again, you guys should say it, not me. But maybe it won't be that important except for that one case.

MS. GHOSH: That's a good comment. We can modify it. We can work on modifying it.

CHAIR STETKAR: I have to be cognizant of time. We have a 12:00 p.m. hard stop today.

We shouldn't try to speculate about differences between Surry and Sequoyah because they are difference plants. Period. They're different plants.

So, trying to speculate about lessons learned at Sequoyah based on what we know for Surry is against what we should be doing.

These are plant-specific studies and a conditional probability of a high-dry-low situation. And Sequoyah could be substantially different than high-dry-low.

At Surry, we don't know that, and that's the important insight from SOARCA, is that you must do plant-specific and site-specific, integrated, uncertainty analysis.

MEMBER REMPE: But then I would argue with you, sir, that the Executive Summary is really coming up with conclusions that may not be valid for a large lead rate.

CHAIR STETKAR: That's correct.

MEMBER REMPE: And again, the Vogtle analysis did something based on generic knowledge about --

CHAIR STETKAR: We're not talking about Vogtle here.

MEMBER REMPE: That's true, but again, I think you may want to -- without trying to extrapolate things from other plants, you may have some conclusions that are not valid for this particular plant --

CHAIR STETKAR: That's right.

MEMBER REMPE: Because you did not consider the consequential steam-generator tube rupture.

So, something needs to be acknowledged in that Executive Summary on the downsides of not considering it.

CHAIR STETKAR: It's fair game to talk about the Executive Summary for this study.

And the Executive Summary should, in my opinion, clearly delineate what was done, why it was done, what was not done, and why it was not done.

MEMBER REMPE: And what are the downfalls of not doing it? I would go a bit further on potential downfalls of not doing it, because some of the conclusions may not be valid because they had a limited scope.

And they should have some acknowledgment. And without extrapolating, they could say, hey, we've done a bunch of analyses and this may be important, you may have to do it plant-specific to determine it.

But I think you need to have some caveats in your conclusions. Okay?

CHAIR STETKAR: Okay, good. Pat, we kind of interrupted you midstream. Pick up the ball.

MS. SANTIAGO: One other thing that may help some of this discussion, actually, is we plan on doing -- basically, after the Surry RAY's done, we want to work on a compendium of important insights that we've gained from the three Uncertainty Analyses. And we're going to publish that as a summary NUREG as well.

But I do what to point out that in each of the SOARCA studies, each bottom study during Sequoyah, we do say that this is a site-specific study, and you may have to look at the specific site and other designs of other plants.

CHAIR STETKAR: And I think that whenever you come to that sort of general insight, NUREG or whatever, we would be very interested in seeing that.

Because I'm very concerned about trying to make the entire pressurized-water nuclear power industry look like Surry, and the entire boiling-water industry look like Peach Bottom. And the entire ice-condenser plants look like Sequoyah.

So, trying to develop broad generic insights from these very site-specific, plant-specific studies is dangerous and misguided. And that's on the record.

So, that's why I'd be really interested in strength seeing that Overall Insights study.

MS. SANTIAGO: Okay, and maybe when we do the Surry, we can give you an outline of what our plans are for that compendium.

CHAIR STETKAR: Yes, that would be good.

MS. GHOSH: Can I just make one quick clarification?

I don't think the point is to try to draw some broad-brush -- you know what, the main issue is just to make something that's more practical than having -- I mean, at this point, we have like 1900 pages of Uncertainty Analyses.

And people go in and they'll pluck out what they need, but we get a lot of complaints about just how much material you have to go through to pick out that nugget.

So, we're trying to make something that's a little bit easier to digest in one place, rather than having to go to the library and dig through all that material.

CHAIR STETKAR: Pat? Since you still have the ball to carry.

(Laughter.)

MS. SANTIAGO: Next slide, I did want to compliment the team members that worked on this because they worked very hard and diligently on this particular project, at the same time that they're working on the Surry Uncertainty Analyses and other projects, as you heard me talk about on October 3rd, when the Division of Systems Analysis briefed.

So, I did want to thank them all. Dr. Tina Ghosh and Doug Osborn were the co-leads. Doug Osborn's from Sandia National Labs.

We have a handful of folks in the audience to help us answer any questions as we brief, as well as those on the phone line.

On the last slide, I just wanted to talk a little bit about the meeting today, and the focus is going to be on the Sequoyah draft report.

Since our last June 6th Subcommittee Meeting, there's two significant additions that we've added to this draft report.

The first change is the new introductory material in Section 4 on accident progression, which was presented in June by Hossein Esmaili. And at that time, it wasn't included in the report.

We don't plan to brief on that material

since we did brief in June, but I just wanted to mention that.

The second addition is a new Appendix I in the draft report, which documents the supplemental Uncertainty Analyses that was focused on a range of safety valve parameter values where an early containment failure is possible.

Dr. Trey Hathaway will present this work today.

After's Trey's presentation, the team will walk through Member comments and discuss how they were investigated and addressed in this new draft report that we provided you last month.

And lastly, we'll seek the Subcommittee Members' feedback on how we should focus our presentation to the full Committee in November, since we'll only have 90 minutes at that particular meeting.

And I'll now turn it over to Trey and my Staff to present.

CHAIR STETKAR: Pat, thanks. A couple things.

First of all, in your introductory remarks, you characterized the ACRS's role in this project as providing a peer review. I personally take issue with that.

The ACRS has not performed a peer-review service. We do not delve into the depth of technical details that is implied by a full-scope technical peer review.

We focus on integration, we focus on consistency, we focus on high-level issues related to technical scope content, things like that.

But by no means do we perform a peer review service in the way that those words are typically understood throughout the industry, and throughout the Staff.

So, I would appreciate it if you don't characterize what we're doing as a peer review.

MS. SANTIAGO: Okay, so for Surry and Peach Bottom, we did have a full, complex, external peer review group.

And so for Sequoyah, based on knowing what the comments were for that and addressing all of that, we have guidance that says we can use or ask the ACRS to support review of the next document, which was Sequoyah.

So, I apologize for suggesting it's a peer review.

It's an internal review, I guess, that we would like to have a letter from the Committee, so that we can accompany or send it to the Commission about the same time as the report is going to the Commission.

So, thank you.

MEMBER BLEY: I have to add a little bit to what John said. We're giving the some kind of oversight in this study as we did to the others.

To imply that we're providing any sort of detailed technical review of the sort you'd had from your peer reviews is just wrong, and we don't do that. And you ought to not tell people that that's what ACRS has done for you.

CHAIR STETKAR: Also, please don't characterize this as an internal body. We are not internal to the NRC, we are an independent advisory committee.

MS. SANTIAGO: I forgot the right words.

CHAIR STETKAR: Are you going to discuss the Executive Summary?

I didn't see your slides until this morning, so I haven't had a chance to look through them.

Are you going to discuss the Executive Summary at all?

MS. SANTIAGO: We weren't planning on it.

CHAIR STETKAR: Okay, let me ask one general question and just get it out on the table, and kind of think about it.

As I read the Executive Summary and a few of the Chapters that have the new material in the current version of the report, I found kind of a different tenor from the previous version of the study.

And the difference seemed to be more emphasis on comparisons between the current version of the study, and what I'll characterize as the April 2016 version of the study that we saw a year and a half ago.

There's a lot of comparative stuff saying, well, we had this in the April 2016 version, and look, the same trends are here, but the results are different.

That obviously had to be a conscious decision because it's pervasive throughout the new material.

Why do you feel it's necessary to compare the results, insights, conclusions, models, and so forth from this current version of the study, to an outdated, non-technically-supported interim version of the study?

I mean, when I've done projects in the past and have made mistakes in the middle, I don't go back and say, hey, I made a mistake but, look, you know, this other stuff is all the same.

I just don't see the benefit of it. You changed models, you changed uncertainty distributions; those changes made a difference.

Why go back and say, well -- I mean, there are statements that say, well, the uncertainty distribution moved the results around but, look, the general trends would have been the same.

That's obvious. You can present that same level of insights and conclusions, just given the results of the current study.

It's obvious to a reader that if a pressurizer safety valve sticks open, big, early, it's not a good day.

You don't need to compare that to the fact that it used to stick open more, bigger, in a previous version of the study that was found to have some technical questions.

So, I'm not sure, because it was a conscious decision. The only reason I bring it up is why was that decision made to compare and contrast to the previous version of the study?

Because to me, it was not only from a philosophical perspective that I tried to mention here, it was actually distracting.

MS. GHOSH: Okay, so I think we understand your comment.

I mean, as to answering the why, I think in part, we've put out this big study, it's publicly available. It's going to be forever in Adams and --

CHAIR STETKAR: When you say this big study, you mean the 2016 version of the study?

MS. GHOSH: Yes, because it was out there. It was the first time we did this with the ice condensers. A lot of people had looked at it.

So, maybe it was in recognition that people might have been familiar with last year's study. But I understand your comment and I think we can take a look at --

MEMBER CORRADINI: I have a slightly different opinion than my colleagues. It's your study, you can do whatever you want. Then you'll get criticized for it. So, my view of it is that John's saying if you were reading it today, this would be his reaction. And another way to do it is to write the Executive Summary in such a manner that nobody had seen anything before, and say, oh, by the way, we had a draft version.

Go look at Appendix blah-blah and you'll be able to compare and contrast in blah-blah, but don't do it in the main body.

One, it detracts from what I assume you think is better. And two, it would confuse the reader that never took the time or the energy to read the one before.

But that's your style, it's your report.

MEMBER BLEY: And it sounds very defensive.

We would have certainly asked and we would have expected in this presentation you would do those sorts of things and show us what changed.

But I agree with John and, I think, with Mike.

CHAIR STETKAR: Certainly, as I read it, quite honestly, because I read the Executive Summary first, it's the first thing you come to, and quite honestly, it's what most folks will pay most attention to.

So, I wanted to kind of see what flavor of information was in there.

I found it very, very distracting in the Executive Summary. Once I got past that, it is infused in parts of Chapter 4 and in Chapter 7 in the results.

There, because those are -- well, Chapter 7 is kind of summary of results, so that's a companion to the Executive Summary. But Chapter 4 is more of a technical detail thing and I was less confused by it there.

But I think Mike's idea of pointing to an Appendix that compares and contrasts it -- I understand the desire to capture the differences and draw insights from the differences.

But, anyway, you've heard our comments.

MS. SANTIAGO: That's a good comment, thank you.

CHAIR STETKAR: And with that, I don't know who's -- Tina?

MS. GHOSH: Trey.

CHAIR STETKAR: Trey, you're up.

DR. HATHAWAY: Okay, what I'm going to present today is the third results of this sort of Focus Study that we performed kind of after the full UA --

CHAIR STETKAR: Is your mic on?

DR. HATHAWAY: I've got it here.

CHAIR STETKAR: You're just a very softspoken person.

DR. HATHAWAY: Sorry about that.

CHAIR STETKAR: Scream at the mic.

DR. HATHAWAY: So, the point of this was to really try to reinforce the conclusions and the study.

We knew we only had a very few number of early containment failures so we wanted to kind of focus in on this region of ventures to see if it reinforced conclusions of the full UA.

So, that's what I'm going to present to you today. Also, kind of see if it provided any additional insights, focusing in on this region.

So, the Figure on the right is the results of the 2016 UA, and we just kind of -- we were drawing conclusions -- not drawing conclusions, we were trying draw insights from these previous analyses, all the analyses that were performed to really hone in on what region of interest led to early containment failure.

And the reason we chose this is because we did have -- well, I'm presenting this because we had fewer early containment failures than the current study.

So, what you see here, the Figure on the right, plots the number, the total number of pressurizer safety valve cycles plus the open area fraction.

The points in blue represent the -- excuse me, all the points represent the various individual realizations, the Monte Carlo realizations. And the points in red represent cases that had an early containment failure.

And what you see is those sort of cases that begets early containment failure kind of housed in this region of cycles greater than 0.3 and less than 65, is what was chosen.

Really, my particular interest was less than hot leg failure, try to have a dent before hot leg failure.

So, yes, again, we were trying to use the previous studies to inform what we were going to look at for this sort of focused look.

So, we took the current model and just sort of used the identical distributions as before, except for two.

We bounded the distribution for the open area fraction to 30 percent, and then we also constructed a distribution based off the sample data from the previous study to try to focus in on this range of 165 cycles.

For this focused look, originally, it was for whether or not we had early containment failures. So, we limited the calculation to 15 hours, and that was just really to -- again, this was informed by previous studies.

Previous studies indicated that the early containment failures were before 15 hours. The model was identical to the previous study.

The only difference is there was this minor error in the study having to do with the fabric seal pressure.

That was corrected; it was a formatting error on a text file that was written so that was corrected for this study.

MEMBER CORRADINI: So, I'm going to ask a general question and you can address it whichever way you want. But I've been looking through the new version, and again, I could be missing it.

I'm just looking for data, test data, that helps decide if this is realistic, optimistic, or conservative.

I'm assuming you were trying to buy us at conservative, but I'm looking for safety-valve test data.

DR. HATHAWAY: While this particular study wasn't looking at --

MEMBER CORRADINI: No, I know you weren't. I'm just looking for a reference that tells me to go look over here.

So, your distribution of fraction of valve open after how many cycles bounds it, or represents it, or whatever.

MS. GHOSH: So, from the perspective of having the potential for early containment failure, we're looking at a more conservative range for this two-dimensional sample space.

Because that black box is basically what we're sampling, and that only represents about four percent, four or five percent, of the entire sample distribution.

But it's in that four or five percent where the early containment failure is possible.

So, we're artificially constraining it to explore this area where you can get early containment failure. But it's only a small percentage of the total sample distribution, which is --

DR. HATHAWAY: Okay, yes.

Again, this was to really try to focus in on the full study and just look at that region where early containment was most probable to really see if there were any additional insights.

CHAIR STETKAR: Trey, your slides, I read through this, I see what you're trying to do and what you did. But I got confused a bit.

Your slides don't have the Figures that I'm going to talk about, and the Figures I'm going to talk about right now are Figures I-2 and I-3 of the report, that develop the values and the range of values that you sampled from for this study.

I got confused by those because they do not, to me, anyway, seem consistent with the numerical values that are used in the main body of the study. So, I'd like somebody to explain to me where I'm wrong.

In particular, I look at your fifth-order polynomial fit to the valve failure rate for the valve data in Figure I-2.

And I noticed the first term is 3.26 times 10 to the -2, which, to me, ought to be the mean value for the conditional probability that a valve sticks open on the first demand.

If I'm wrong about that, please tell me because that's kind of fundamental.

Because in the main body of the report, we don't use that value. We use a value of 2.65e to the

-2, not 3.26. So, I'm curious why the first-order term is 3.26 versus 2.65.

The next-order term for failure is per demand. If I use just a linear model, it's 1.83e to the -3, and in the main body of the report, we use a value of 2.23e to the -3 for the demand.

So, the shape is -- the intercept at zero is slightly different and the shape is somewhat different.

So, somebody fit a fifth-order polynomial to stuff that ostensibly uses data from the main body of the report, and I can't draw that link.

Now, why is that important? Well, overall, I'm not sure, but it's certainly going to affect the number of samples in different regions.

The second -- let me get through the second part of it.

The second part of it is when you do the differentiation of that curve to get the density function in Figure I-3, that density function does indeed start out at 1.83e to the -3, at a value of 1.

Now, that's actually at a value of one-plus because if you differentiate the delta function on the first demand, you get infinite. So, you can't do that.

So, I want to make sure that the actual sampling used the correct probability for failure on the first demand, that it didn't use 1.83e to the -3, that it used something between 2 and 3e to the -2 in the actual runs that you made.

Why is that important? Because that's going to force more earlier failures.

Certainly, a lot more earlier failures than might be implied by the density function here in I-3.

So, if somebody could confirm to me where the numbers came from, and in particular, when you did your sampling?

Give me assurance that you used something on the order of two or three times ten to the -2 for the probability that it fails on the first demand?

The first demand, number one, not 1.83e to the -3.

I'd be a lot happier.

DR. HATHAWAY: I'll try to address your comment, I'll take a look at the comments, I can look at them.

But the distribution extended well past what I fit, because I was interested in just this range. I think that's what I said.

I was interested in the range of 0 to 250. So, I was really trying to get a fit to the data in that range.

The problem is, as you extend past that range, the shape of the distribution is different. It starts to curve.

CHAIR STETKAR: You're starting to talk about somebody who really likes to do curve-fitting. I'm asking you a more fundamental question. I'm asking a question that your first term says 3.26e to the -2.

In the main body of the report, there's a table that says the mean value for failure on the first demand is, and I've lost it again, --

MEMBER BLEY: It's about 3 --

CHAIR STETKAR: It's about 2.65. Now, it's about 3, but why is the value different? And the first term is also different.

So, why didn't you use the values that are tabulated, and then you can do all of your fifth-order polynomial curve-fitting, whatever you want to do, and extrapolate out?

It's the same mathematical problem, it's just a different curve, and I don't know why it has to be different. It's the same lambda.

MEMBER BLEY: You make a big, and I think reasonable, case that it's a different failure rate for the first actuation than for the rest?

CHAIR STETKAR: Yes.

MEMBER BLEY: So, you have --

CHAIR STETKAR: And that's what's used in the study. That's what's used in the real study.

MEMBER BLEY: Yes.

CHAIR STETKAR: This is supposed to be something that focuses on the first, nominally, 65 or so demands, and tries to force samples within that regime, that are consistent with the first study, given the fact that the first study extends -- uses all three valves.

I mean, the first study, the baseline study if you want to call it, that is very elegant in the way that it characterizes the valves and multiple failures, and things like that.

But the intent of this shouldn't be to develop different failure rates.

MS. GHOSH: So, I think we're going to take your comment and re-check the numbers after this meeting, because we're not going to be able to resolve it right here.

CHAIR STETKAR: Yes.

MS. GHOSH: So, we understand the comment.

The one thing I will say, I don't think we should attribute too much precision to all of this, because our main focus was to make sure we were within this range of sampling where we could get early containment failure.

So, in terms of the high-level insights that we can draw from this study, we feel that we accurately explored the two-dimensional sample space.

There may be some discrepancies between the exact frequency and in parts of the sample space. We'll have to go back and check that.

CHAIR STETKAR: Tina?

MS. GHOSH: Yes?

CHAIR STETKAR: I get it.

But you spend a lot of time in the Appendix, like a couple of paragraphs, and this plot, saying you fit a fifth-order polynomial because, well, if you tried to fit a fourth-order polynomial, you had a little bit of different glitches when you did the differentiation.

So, you had to fit a fifth order. If you wanted to use a simple model, why don't you use the linear model? Failure on demand at time zero plus lambda X. It would have been a straight line.

The differentiation is pretty good; it's got the same value at every demand. That's a real simple model.

It's not as elegant, it doesn't have this nice curvy shape to it.

MS. GHOSH: I understand that.

CHAIR STETKAR: But it's pretty simple in the sense of we're just trying to get an approximation and focus on this range of what might be important in that range.

MR. FULLER: Ed Fuller, from the Office of Research.

I want to make a comment on what John was saying because it was an issue that we encountered years ago, when I worked at Pole Star, doing a steam-generator tube integrity risk assessment for a variety of plants.

And we looked at operational data and test data at the time, to come up with some notion of whether or not there was a distinct difference between what would happen on the first demand versus all the rest.

And we concluded that at the time, anyway, most of the time when the data was pertaining to one demand right away failure, it was due to some maintenance errors.

And so I think, my own view is, take what John said and refit, starting with Demand 2; exclude the first demand from your Focus Study.

CHAIR STETKAR: The main report does that, and it does it in -- I tend to be not the most pleasant person in the world -- but in a very elegant way, the main body of the report.

The way it treats three individual demands -- three individual valves, sequential demands, sampling of the number of cycles to failure, is a very elegant model.

The only questions that I had on this Focus Study was the values that are used to plot that curve.

And then as a separate, non-related error, a question to make sure that you did use that initial demand failure for sampling for Demand 1 in the Focus Study.

DR. HATHAWAY: I don't know if this will clear this up or not --

CHAIR STETKAR: It's a factor of ten higher.

DR. HATHAWAY: In the UA, it was two distributions. It has one on the first sample and then it has one on the second.

What I'm trying to do by using the data is fit to that so it convolves those two distributions together.

CHAIR STETKAR: But your curve I-2 shows the concept of what's in the baseline study.

It shows a delta function at one, which corresponds to the conditional probability that it fails on the first time.

And then it shows some sort of shape in a accumulative form as you accumulate demands.

Now, the shape of that curve kind of depends on the model that you use, and the slope of that curve depends on the failure rate that you use per successive demand.

The same failure rate is used for each successive after the first, in the baseline model.

And depending on -- it's not a linear model if you account for the accumulative effects from failures.

But in a simple term, you could use a linear model, especially over the first 50, 60, 70 demands or so. This didn't.

And then when you differentiate this curve, you'd better account for that delta function at one.

And if you're not, you've got a real problem because you're ignoring a factor of ten or so, or more than a factor of ten, conditional probability that it fails on the first demand, which is, indeed, much more closer to the regime that you're trying to test in this Focus Study.

You're trying to see how sensitive the results are to earlier failures with a large open area.

MEMBER BLEY: Just maybe Figure I-3, which is the density function, was drawn without showing the delta function either.

CHAIR STETKAR: That's the reason why I ask it. It could be.

MEMBER BLEY: Maybe it's just the way you drew the Figure, and you didn't show that three minus two, right, at one.

That's kind of what John was hoping you would say.

CHAIR STETKAR: Yes, I was really hoping you would say, well, of course we used the three minus two for the first demand. Or two and a half minus two, or whatever it should have been.

But I didn't hear that.

MS. GHOSH: If you go to Figure I-4.

CHAIR STETKAR: I-7?

MEMBER BLEY: I-4?

MS. GHOSH: Yes, if you have that.

It has the actual sample then successful -- we were showing this for a different reason, to show that there wasn't a huge dependence in terms of the completed runs.

But at least to the first order of magnitude, we did sample that first demand higher.

CHAIR STETKAR: You did? I'm trying to pull it up right now.

MS. GHOSH: But I think in terms of the larger question of the exact numbers, we'll have to get back to you on that and we'll double-check.

MEMBER BLEY: Somehow, I'm not seeing it.

CHAIR STETKAR: Yes, I mean, if I had seen it in that Figure, I would have probably mentioned it. Yes, I'm not seeing it here.

MS. GHOSH: So, maybe we can talk during a break. We should probably move on and we can get back to you on that.

MEMBER BLEY: Are you sure you meant I-4? Okay.

BB: I know there's a bunch of --

CHAIR STETKAR: Turn your mic back on, Tina. Don't --

MS. GHOSH: Sorry, the bottom right of that six-figure -- there's a bunch of charts in that Figure.

Yes, I apologize, I meant to release all of them. Maybe we'll catch up later, and we can move on. Yes? Yes, sorry.

MEMBER BLEY: Thanks.

DR. HATHAWAY: So, we ran the models, and what we really looked at is when we constrained our distributions to this small portion of the sample space, where early containment failure was the most probable, we got about that 17 percent of our realizations resulted in early containment failure.

And looking at it, about 15 percent of the realizations, the BOC core conditions had an early containment failure of 16 percent of the MOC and 19 percent of the EOC.

So, the table on the right shows just the statistics for the various realizations. The mean of the BOC was about 6 hours, the mean of the MOC was about 6.6, and the EOC is about 6.6.

And what you also see is the BOC's kind of constrained closer to the mean, where the MOC and the

EOC are spread out further.

And we'll see this a little later on when we look at the cesium iodine release rates as a function of time.

CHAIR STETKAR: Trey, I don't know where to ask this so I'll ask it now, and tell me if I should ask it later. Let me get my notes here.

See, you see this number 17 percent? 17 percent of the realizations in the Focus Study resulted in early containment failure.

What you really mean is 17 percent of the 361 successful realizations had that behavior.

Out of 600 realizations, there were 239 that did not run to completion, which is 40 percent. 40, 4-0, percent did not run to completion when you tried to force this.

It's curious, and I went back and I did the numbers, that in the baseline study, 40 percent of the runs in the area that had a large early open fraction did not run to completion. Same percentage.

So, something about MELCOR in this region is not very good. 40 percent of the runs are not running to completion in this particular region.

A much higher percentage run to completion everywhere else.

MEMBER CORRADINI: But I thought we had asked that.

CHAIR STETKAR: Yes, and we never got an answer.

MEMBER BLEY: We talked about it a lot.

CHAIR STETKAR: We never got an answer.

MEMBER BLEY: The way it is is kind of --

CHAIR STETKAR: Yes, that's the way it is. Yes, we looked at it and it doesn't finish. Now --

MEMBER CORRADINI: But I thought -- if I just want to interrupt you, John -- I thought you guys promised, I thought, that you were going to go back and try to unravel the reasons for failure in that area?

CHAIR STETKAR: That's why I brought it up now, because I don't think they're planning to discuss it.

The reason I bring it up now is that in the results from this Focused Safety Study, they were reflected back through the overall insights and conclusions.

There is an inference that the same 17 percent early containment failure and the same conditional consequences apply for that 40 percent of the runs that did not finish.

And I do not know why that must be true. I have no evidence --

MEMBER BLEY: Or at least one wonders --

CHAIR STETKAR: I don't know if it is true.

MEMBER BLEY: -- if there's something going on where there could be a lot more of them.

CHAIR STETKAR: I would have been really happy if like 95 percent of these runs ran to completion and you saw that same fraction, but they didn't.

MEMBER BALLINGER: To turn it around, if you have 40 percent, I think I made this comment earlier.

If you have 40 percent that don't run to completion, what's to say that the numbers that did run to completion are actually good numbers?

I mean, if there's something fundamentally going on here, the fact that you've got runs that went to completion and you get numbers kind of makes you feel good, but what if there's something fundamentally wrong?

So, that's why I think we ask that you folks go back and find out exactly why the runs that didn't go to completion, what caused them to do that?

Was it some convergence issue? Or whether it's some flat-out mistake in a model or something. Because it does reflect back on these numbers.

MS. GHOSH: So, I'm going to just note something that I --

MEMBER BALLINGER: Remember, I'm metallurgist so...

MS. GHOSH: Yes, I'm not going to give a satisfactory answer, but one of the primary motivations to do this Focus Study, to do a bunch more realizations in this area was that we knew from the overall UA that we had 20-some, I think, realizations that we had tried to sample in this area.

And we had a 30-some percentage failure rate, which is much higher than the overall UA, which is pretty small.

So, we knew that there was something going on in this sample space, that we were getting higher incompletion rates.

So, that was one of the major motivations to do this study, is just to explore the response surface and the smaller area much more.

So, we have 361 additional points in this area, so we have more information but it doesn't answer the second part of your question.

MEMBER BLEY: But you didn't put any diagnostics in the code or anything to see where it was failing, what was going on?

MS. GHOSH: So, I'll let Casey, and if Hossein wants to speak, speak next. We did have our Code Guides.

We look at the reason for the failure in a bunch of these cases, and in many of them, it was frustrating.

I think we had documented some, or maybe we didn't end up putting it in the Appendix --

CHAIR STETKAR: It is not documented anywhere in the report, I can guarantee that.

MS. GHOSH: Okay, so that's probably something we could add because at least we know what the error meant. We know what the error message was, and in some cases it's easily explainable.

I think some cases, things are just happening too fast, the time steps are too small and you get hung up.

But in a bunch of cases, it was this executive abort signal detected, which is really frustrating, because it's very hard to untangle.

But let me turn it over --

MEMBER BALLINGER: I'll say the same thing as I did last time, and that is sometimes, exploring why these things didn't go to completion provides you with what I would call surprise, which actually means something.

And so I may be a metallurgist, but if a student comes to me with results from a model he's written -- he or she, excuse me, has written -- and says that 40 percent of the time, the thing didn't converge, then I would reject the results out of hand.

I would say there's no reason for me to believe that the ones that did run to completion were not fortuitous.

MEMBER CORRADINI: So, you guys don't have to try to explain more. My interpretation of what you're saying is you don't know, even though you tried to look.

So, if there is a documentation of however many it is and the failure message, it would be, to me -- and this is not a criticism because these are highly non-linear calculations that go off the reservation, potentially more easily, and the fact that you had the small percentage of the bigger population is a good thing.

But I do think you've got to do some sort of postmortem as to why.

MEMBER BLEY: I'd like to hear what Casey has got to say.

MEMBER BALLINGER: He's a thermo-hydrologist guy so he gets away with more.

MR. WAGNER: Casey Wagner, Dycoda. There's a couple of things that I have to point out.

This was already done and it used the same code as the last calculations.

So, whatever problems we were running into on the last set of calculations, we were going to run into it on this set of calculations.

MEMBER BALLINGER: See, I rest my case then.

MR. WAGNER: Well, more calculations were done there.

It went to a small time step and eventually, if you have enough time steps that are very small, it would give up and say that it was not able to converge. And so that was the stoppage.

The convergence criteria is there, I don't know if this will satisfy you, to assure that you do get convergence, and if you don't have convergence, it keeps on cutting the time step, trying to get there.

If it doesn't get there, those calculations are stopped, and we are considered failed.

The other ones, presumably, did satisfy that convergence criteria, and so we do accept them as successful and reasonable calculations.

MEMBER BLEY: It just seems there's no -- since you don't know quite what's going on, I don't see any basis for assuming that the same percentage of containment failures are in this group that died as in the other calcs.

There might be something that's happening on the way to containment failure that's blowing up the code.

MEMBER MARCH-LEUBA: Casey, can I ask you a couple of questions?

You mentioned you're using the same code. Do you mean the same version of the code, or the same number?

MR. WAGNER: It is the exact same version.

MEMBER MARCH-LEUBA: And all these variables didn't happen on initialization? It happened in the middle.

It did not happen during initialization, it happened in the middle of the transfer?

That typically happens when one of the nodes goes into other reservations that we mentioned before. Another's supposed to have water, it doesn't.

A little one goes through somewhere, and that happens all the time in other codes. But if it's water, somebody dies.

CHAIR STETKAR: When you say all the time, it's, to me, knowing nothing about the codes, it's just curious that in the baseline study, at least according to what I understand from the report, they only had 40 chances to get into this regime.

And out of those 40 chances, 17 didn't run. Here, they forced it to get 600 chances to get into this regime, and the same fraction of times didn't run.

MEMBER MARCH-LEUBA: You give these codes too much credit.

CHAIR STETKAR: But, okay, you folks are familiar with it, but we are in fact relying on this code to draw conclusions and insights about kind of a fundamental behavior of this machine.

MEMBER MARCH-LEUBA: The experience of people that run this code will tell you that they run 10 runs when it crashes for every run when it survives. And you converge on the model and you fix everything.

CHAIR STETKAR: But that wasn't the case.

In more of the well-behaved regime of this whole study, they got something, I don't know, Casey, 90 percent success rate or better?

MEMBER MARCH-LEUBA: Eventually.

CHAIR STETKAR: No, not eventually.

MEMBER MARCH-LEUBA: No, when you're trying to develop the input there, you're trying to describe your plant, you try once.

You run it, it crashes. Then you fix it. You run it, it crashes. And eventually, you fix it --

MEMBER BLEY: We kind of get that, but what John's talking about is there's this whole space they did calculations, 90 percent of the time, the code runs great.

There's this little vulnerable area for early containment failure. When they run in that regime, 40 percent of the time it crashes.

So, there's something in there that's causing trouble.

MEMBER MARCH-LEUBA: My point that I obviously didn't do correctly is that's worrisome because it happens in the middle of the run. Something is happening.

MR. WAGNER: Yes, I can explain what's happening to get it to make sense to you.

For those cases where the valve sticks open, we're moving, we're generating more hydrogen, we're going into early containment failures, we're getting more core damage.

And then the accumulator dumps, and so we're dumping an accumulator into a much more degraded core than the cases that are high-pressure, that we get the hotleg failure, and the core's relatively intact, and it quenches it pretty quickly.

And then we move onto a boil-off severe accident. This is much more challenging because the core has formed a debris bed and then we did a hotleg failure, or it seeps into the accumulator dump.

MEMBER CORRADINI: So, let's just backup. So, I don't do this anymore because I probably couldn't do it right. I couldn't even get the model to run at times.

So, if I were to do a detective job on this, the first question I ask is of the 40 percent that failed, did they all fail at approximately the same time in terms of event sequence?

You are hinting that the event of an accumulator, essentially, hotleg creep rupture, and then an accumulator dump, is a common time-to-receive failure.

Is that the case?

CHAIR STETKAR: This is a step up in valve but I just think --

MR. WAGNER: Yes, we also get the hotleg failure, even if the primary system pressure has come down.

The one thing that I don't think Trey shared with you is that we did forensics because we had so many more samples here on our uncertain parameters.

And was there in space in them where we didn't have representative successful samples?

And Trey did a very thorough job of going through all the different parameters, and they're interspersed. They're like this, failures and successes. And so there wasn't a common theme there.

On our sample parameters, we were missing a space.

We unfortunately got the 40 percent, which is very high, but the interspersed end of representation of what we were trying to look at for uncertain parameters was very good.

And we judged that as success in a lot more cases.

MEMBER CORRADINI: Okay, I get your point.

But what I'm asking, from the standpoint of just rudimentary failure times, is there an event that MELCOR records that at, or right after, that event, everything goes to hell in a handbasket regardless of the uncertain parameters or the variable parameters.

MR. ESMAILI: This is Hossein Esmaili, can I just say something? I think we don't know right now what caused this error.

We can going back and look at it, and when Larry was here on April 18, we went through the whole thing. We said that this is our quench model, this is how we do this, this is our comparisons with the experiments, et cetera.

When you apply those models to sophisticated whole-plan models, you can run into whether it's on the core side or whether it's the input model side.

We cannot answer this until we go to the actual source code to try to find out what caused this problem. But in most cases, we can resolve it.

It was not important, I guess as the panel here discussed. It was not important because we had an overall high success rate, and most of calculations that we did was over 99 percent success rate.

It's just that we got into this issue here. And we are going to find out exactly what happened in these calculations, and maybe we can let you know later.

Right now, I don't think we can tell you with certainty what happened in these calculations. It's often one thing, one thing in the code, that once you get over it, the calculations would go forward.

But I don't know until I talk to my colleagues about it.

MEMBER REMPE: And so your diagnostics don't tell you it's in --

MR. ESMAILI: No.

MEMBER REMPE: It doesn't tell you which nodes? It doesn't give you any clues? It just --

MR. ESMAILI: It gives you some idea but it doesn't tell you everything about it.

So, you really have to go run the code in a de-bug mode, find out what happened there, and find out exactly what was going on.

And in some cases, it could be a small error in how things are done, and --

MEMBER REMPE: And in the diagnostics that you've had, you've not gone through and the 17 --

MR. ESMAILI: It doesn't --

(Simultaneous Speaking.)

MEMBER REMPE: And the 17 or however many hundred cases, you've not ever tried to say, oh, it's always in that same place?

You've not had the opportunity to --

MR. ESMAILI: I don't know if we have done that. I don't know if we have put in the tracing model back on to the reference, but we can go back and look at it more.

I'm just saying that we cannot resolve it here because we don't have the right people, we don't have them here.

MEMBER BALLINGER: So, you're on record now as saying you're going to run this to ground?

CHAIR STETKAR: They said they'd get back to us. From an academic sense, not that I understand any of this stuff, I'm really interested to understand why the code is blowing up.

From a bigger picture regarding this study and its report, the study and its report does not highlight the conversation we just had.

It basically says look, look, look, we did this Focus Study, which is a really good idea.

The results from the -- we focused the sample space on a regime that we really wanted to test, that was a) blowing up more frequently in the baseline study, and b) generally more important for offsite health consequences, early containment failure anyway.

It has a good chance, but then we observe the same behavior, that 40 percent of the runs didn't run to completion. That's mentioned, it's documented, but it's just sort of mentioned in passing.

And the thing that concerns me most from the perspective of the study is the inference that 17 percent of that 40 percent of the runs that did not complete would have behaved the same as the 60 percent that did complete.

Ergo, yes, we might pick up two or three more early containment failures from the runs that didn't complete.

And therefore, our overall study results and conclusions and insights aren't affected by those run failures.

That latter point is the thing that bothers me, and drawing that inference, I believe, is misleading.

I think that you should just own up to the fact that the runs didn't run to completion and you don't really know what fraction of those incomplete runs may have gone into early containment failure.

You know from the baseline study that it can't be -- because I've got confidence in the baseline study sampling -- you know that it can't be more than I think 17 additional runs in the baseline study.

So, it can't be 200 more samples in the baseline study that would have gone into an early containment failure.

But you don't really know what fraction of those additional runs can go to early containment failure, and I think the overall study should highlight that.

It's just something we don't know right now.

MS. GHOSH: Okay, so I think we understand the comment. The one other thing I'll point out again is this Figure 1-4, which is in the Appendix.

This was our attempt to, at least from a statistical standpoint, to look at is there a region of the inputs that we're putting in?

So, that's only just the initial conditions. It doesn't get to what's happening midstream at all.

But at least, was there a region of even this focus input space where we were getting excessive failures.

So, the I-4, the one I think that Trey summarizes, is, once again, the one area where you see a little bit of a correlation in terms of the failures.

And we already knew this, because we knew this from the leger studies, that if you have very few cycles dealing with big, open areas, you continue to be more likely to have an incompletion rate.

But if you look at the successful realizations from those Figures which are shown in red, the blues were the samples that were attempted, and then the red ones are -- so the blue is the CDF of what we attempted to do.

The red is the cumulative distribution function of what actually succeeded in completing.

You can see that what Casey was saying, there's very good coverage across the range of the inputs, and for most of the input and variables, we get a very good match.

But one exception, again, is there's a little bit -- you can see a little bit of a difference in the CDFs for the number of cycles, and that's kind of pushed to success for a higher number of cycles.

So, you do see that same correlation.

But we, at least from a statistical standpoint, not having had been able to do the full forensics on these realizations, from a statistic standpoint, we felt comfortable to say that we thought it was likely that in the runs that didn't complete, you would see the same percentage of runs going into early containment failure.

So, again, from a purely statistical standpoint with the input space --

MEMBER MARCH-LEUBA: And that's why I wanted to get a little time, because I do know a little bit about this topic.

Based on the comments we receive here, I see likely unconcerned that the Staff is going to go into a five-year effort into de-bagging all those 40 percent run.

I'm thinking, using engineering judgment of the guys that know what's going on, can you look at the run and it fail, but I see the passage going, this is going to be okay.

And then I have to look at that one.

CHAIR STETKAR: That would be really good, wouldn't it?

MEMBER MARCH-LEUBA: Our guys who have

looked at the other 362 can look at one and say, yes, it crashed, but this isn't going anywhere.

Good. Don't go into a five-year effort.

CHAIR STETKAR: And, Tina, I'm sorry, but don't muddy it up with statistics on sampled parameters. Make it an engineering problem.

MEMBER MARCH-LEUBA: In fact, it would be perfectly good for this exercise. And you know it, you look at the run and say I'm wasting CPU.

CHAIR STETKAR: At 239 runs, 57 of them had this flavor, 52 of them had this flavor.

MEMBER BLEY: That would be much more convincing than just saying it's got to be the same percentage.

CHAIR STETKAR: Okay, I think we beat that horse. That's not a dead horse, it's beaten.

DR. HATHAWAY: So, I'm going to be doing the next part. The next Figures present the CDF, so the hydrogen-generated, because that was sort of that interest.

Really, when I first started out, I was just kind of interested in what was the early containment failure potential in this region.

So, just looking at the data a little bit more, we went in and pulled out the mass of hydrogen generated up to the time of first deflagration, and the mass of hydrogen reaching the dome up to the time of the deflagration reaching the dome.

Because those tend to be the important aspects of the -- that tends to be one of the important insights from the study.

So, what this Figure shows, the dots in blue represent the BOC realizations, the dots in purple represent the MOC, and the dots in green represent the EOC.

In these Figures, the dots that are highlighted in red represent the early containment rupture cases, and what you can see looking at hydrogen generation, there was slightly more hydrogen generated in the BOC realizations.

But the EOC and MOC realizations overlap pretty well.

But then when you look at the amount reaching the dome, that was consistent across all the realizations, and also, what you see is when you have a larger fraction of hydrogen reaching the dome, those tended to have a higher fraction of the cases leading to early containment failure.

MEMBER BROWN: Excuse me, for the uninitiated, what do you mean by early? I looked here, and you see the hydrogen buildup, is that over a 6-hour, 10-hour, 20, 1 day, period? After?

DR. HATHAWAY: No, it's within the first 15 hours of what the simulation.

What I meant by early is if you have a failure due to the hydrogen deflagration, not overpressurization, which is lead failure.

MEMBER BROWN: Okay, so it's about 15, in the ballpark of 15 hours for the hydrogen, for the deflagration --

DR. HATHAWAY: So, if you looked at the previous study, the times are presented on the right. So, six up to ten hours is what was the result.

MEMBER BROWN: I couldn't connect the dots after I listened for 30 minutes or 45 minutes on that last interchange of why the runs didn't work.

So, I lost the ball on this one. So, thank you, I appreciate it.

DR. HATHAWAY: -- the first 15 hours for this part of the analysis.

So, the next thing we kind of looked at is what is the pressure response in the dome?

So, what this Figure shows, the blue dots represent realizations and the redundant Focus Study. So, you can see that those have the early valve failures on that range of 1 to 65.

The purple represent the results from the full UA, and what this demonstrates is in the full UA, the axis on the right represents the difference between the peak dome pressure and the sample fragility.

So, what that really shows you is how close that early burn gets to actually rupturing the containment due to the hydrogen burn.

So, you can see that in the full UA, a lot of the results don't get within 25 PSI of the sample fragility, so they had smaller burns initially.

But in the Focus UA, you kind of fill out that region between the -- well, going back, a lot of the full UA studies have a hotleg failures, what the initiating event is first. Or what the depressurization then is.

So, when we were de-pressurizing before, you would kind of fill out that Region between that hotleg failure and the admission event.

And what you also see in the Uncertain Figure is a lot of the burns were still -- when they failed containment, it was when the sample fragility was less than the mode of the distribution.

But also, just looking back, if you look at the distribution, 42 percent of the fragility distribution is above the mode, but only 13 percent of the cases gave you a burn large enough to get into that region of the distribution.

So, after we did this, and really, this was after the last ACRS Meeting, we looked at it and I tried to look at the consequences analysis.

So, what we did is we went back and grabbed all the cases.

Since we truncated these cases to 15 hours, we went back and ran them all from the beginning and extended the problem 72 hours, to really be able to look at what the consequences of these cases are.

So, we did this for two sets. We looked at all the early-containment rupture cases, but we also looked at a subset of the late-containment rupture cases.

And we randomly selected late-containment rupture cases but tried to keep the same breakdown as the time cycle.

So, if there were 11 percent in the sample, try to pick 11 percent of the BOC cases, for example.

So, what this Figure is, you can kind of see where the BOC realizations clustered around six hours, but the MOC and the EOC kind of spread apart.

Also, these late-containment failures in the UA, a lot of them were smaller releases.

But we kind of looked at this, this came about very late, that some of these, when you had valve failures, produced larger late releases.

So, that's why we selected a subset of these late releases.

And this is just really showing the results, the average cesium release for the early-containment ruptures was 2.2, and the iodine was 6.3. The late-containment ruptures were 0.004 and 0.021.

MEMBER REMPE: So, can you go back to that slide?

I was going to bring this up later, but if you had done a sensitivity study where you assumed at the beginning of the cycle you had a higher eutectic melt relocation temperature, how do you think it would have affected all these results?

I mean, everything always says, okay, the cesium release fraction's kind of similar if it's all at the beginning of cycle.

And, again, if you do kind of a thought exercise on how all these things are interrelated in

MELCOR, what would have happened?

Because you would have had a longer time for the fuel to heat up, it would have gone to higher temperatures, you would have had a lot of more release.

And it would have really kind of thrown things off kilter, and I was really disappointed that you didn't do a sensitivity study.

I mean, you've got this 2500 plus or minus 83, instead of 200, which is what the Europeans recommend.

And I kind of think that's an important sensitivity technology, some of the limitations and uncertainties associated with this plan.

And all plans, frankly. But any comments on that?

MR. WAGNER: So, it's a timely comment because we're getting ready to do Surry and we struggled with your comment on how do we address it?

It makes sense that BOC is going to have a higher melting temperature, and maybe that's something we could look at in Surry, certainly at the sensitivity at a minimum.

But I agree with your conclusions but the releases would probably be a little bit higher at BOC potentially because --

MEMBER REMPE: Hydrogen generation could be higher. It could really kind of be an outlier there, that it's not being explored.

And so, again, it's kind of like you get up in the Executive Summary when you come up with these general insights from these results.

I'd really like to see some limitations to those insights, because of things like that, so that you don't get yourself over a bind, in this case, because there's some uncertainties.

In the case of the consequential tube rupture, your scope of your study is bringing you to a certain conclusion.

Whereas, if you consider other ongoing studies even, there's some knowledge that you guys have that ought to be acknowledged as a limitation.

And so that's why I kind of wanted to bring it up here because --

MR. WAGNER: To your comments there, maybe our standard deviation about that is too narrow. The inverse is maybe 200 and we really ought to go back and rethink that BOC again.

MEMBER REMPE: Yes, at this point, I'm not trying to say do more calculations, although, I would like to see what that result looks like because of the way the models have been constructed.

But just to acknowledge the limitations and uncertainties.

MEMBER POWERS: I've tried diligently not to make comments during this, but I can't anymore.

Do you really, really honestly think that melt relocation depends at all on how long the fuel's been burned up?

MR. WAGNER: I'd be looking for Randy at this moment. I thought so.

MEMBER POWERS: Do you have any solid evidence that the accumulation-efficient products as the fuel burns up changes the melting point of the fuel significantly?

MR. WAGNER: That's why we weren't able to address Joy's comment. We said we agreed with the hypothesis but we didn't know, we didn't have data to --

MEMBER POWERS: Well, there are data in the literature.

There are several people who have tried to measure the melting point of irradiated fuel, and they usually come back with results that are not wildly different from the melting point of unburnt fuel.

The fact of the matter is that in that particular range for LWR fuel, there's sufficient uncertainty and variability just by small changes in straight geometry and whatnot, that it's very hard to say.

But the overall phenomenon of melt relocation really doesn't depend very much on the melting point of the fuel. It really has to do with liquefaction with the clad interacting with the fuel.

And it seems to me that if you were to hypothesize burn-up has an effect, it would be because of something happening on the inside of the cladding to give you a larger region of oxide formation there, that inhibited liquefaction.

It would have had nothing really to do with burn-up, but it had to do with internal oxidation of the cladding.

MEMBER BALLINGER: But that is a function of the lambda, right?

MEMBER POWERS: It can be depending on what's happening on the inside --

MEMBER BALLINGER: So, the film on the inside of the cladding is going to be a function of the potential?

MEMBER POWERS: It depends on how the fuel chooses to behave.

And in particular, what it seems to depend most upon is how much of metallic modules you'd get in.

Because the molybdenum acts as a buffer for the accumulation of oxygen, and the availability of the oxygen to go oxidize that cladding.

So, it depends on a lot of things, but not which are sensitive enough, since the code works in nodes that are like this, as opposed to individual fuel rods, you're averaging over a lot of things here to get liquefaction.

And liquefaction is treated in the code in a relatively non-mechanistic fashion.

So, you do what they've done.

It's they take some breadth of that relocation number based on matching a variety of experiments, and they say, okay, you match these experiments and we take some temperature range as a criterion for relocation.

Honing that number down based on some hypothesis about what's going on strikes me as kind of futile.

MEMBER BALLINGER: They say about the great tragedy of science, the slaying of a beautiful hypothesis by an ugly fact.

MEMBER REMPE: So, there's also thermal cracking, there is cracking during the radiation. But the thing is that --

MEMBER POWERS: Joy, what happens --

MEMBER REMPE: Let me finish just for a minute, sir.

There's uncertainty with the phenomena but there's also what's embedded in the code, and because, again, there's uncertainty in the phenomena, I think it's a useful exercise to bump that thing up a bit.

Because, again, they only had plus or minus 83 degrees, and I've been reading up a lot of stuff from France, where they're saying there needs to be a larger uncertainty.

But just to see how it's all connected, because I would challenge or query that I think that if you did bump it up, you might see that because of the way the models are interspersed in that code, you're going to see some significant differences.

MEMBER POWERS: And presumably, if I take the ranges large enough, I will get some pretty radical differences. I'm sure of that.

MEMBER REMPE: Yes.

MEMBER POWERS: That one I'm confident of.

MEMBER REMPE: And I would like to see the sensitivity to explore that because they're getting some very similar things.

And I think it would be worthwhile to understand if it's realistic if you go a bit higher, even if your range is up --

MEMBER POWERS: I guess I don't understand how increasing your understanding improves your -- increasing your uncertainty improves your understanding.

A little bit of a mystery to me but I'm --

MEMBER REMPE: But, again, they've limited it to VERCORS for their plus and minus 83, and if you consider other things like PHEBUS, the French are saying go to plus or minus 200.

MS. GHOSH: Can I just clarify --

MEMBER POWERS: I think they're centered around PHEBUS.

MEMBER REMPE: The report indicates that they've centered a lot on VERCORS.

MS. GHOSH: That's true, but I do want to clarify --

MEMBER POWERS: VERCORS didn't involve any relocation at all.

MEMBER REMPE: It's their eutectic melt

temperature, and that's what they based it on.

MS. GHOSH: I just want to clarify that 83 is the standard deviation.

MEMBER REMPE: Right.

MS. GHOSH: So, our bound's actually go up to 200-plus, up and above. You just have less samples in that area.

MEMBER REMPE: Right. But I think an uncertainty would be worthwhile to explore.

MEMBER POWERS: I don't understand at all now. If the sigma is 83, then 3 sigma is huge.

MEMBER REMPE: I don't think we ever got up to that Figure. What was the little red dot?

Was it 2800 or something, and you never had any sampling up there at all?

MEMBER POWERS: The 3 sigma would...

CHAIR STETKAR: But again --

MEMBER POWERS: ...violate the --

CHAIR STETKAR: Please.

MEMBER POWERS: -- annihilate the molybdenum issue, annihilate everything.

CHAIR STETKAR: This is an Uncertainty Analysis, and if believe the uncertainty distribution, if you get a small number of samples out in that tail, you get a small number of samples out in that tail.

As you should, if you believe the distribution.

MEMBER REMPE: But then the conclusions on the Regression Analysis are that the temperature is very important, that it affects hydrogen, and eventually, it becomes pretty important in the follow-on documentation.

And that's why I am emphasizing it.

MEMBER POWERS: I guess I'm still at a loss that you concluded it's important.

MEMBER REMPE: But the models are --

CHAIR STETKAR: Well, it is important. Tina, I don't remember your Regression Analysis. I mean, it showed up in the mix.

Was it very important?

MS. GHOSH: The eutectic melt temperature is clearly very important for hydrogen generation. We've see that in all the studies.

It shows up in combination with other variables for things like cesium release.

So, it's got high interaction effects, again, for obvious reasons, because it has a big role to play in the hydrogen generation, which then affects early containment failure, et cetera, et cetera.

MEMBER POWERS: Also, I would think that

the analyses of transport wouldn't mediate the importance of fuel relocation on cesium release to the containment.

Just because cesium escapes from the fuel does not ipso facto mean that it goes into the containment.

There are a lot of slips towards the cup in the lip on that, and the uncertainties inherent in that transport process might work to mitigate the importance of fuel relocation.

Fuel relocation is very important for hydrogen because you cook things for a long time, and that does give you high cesium releases from the fuel, but doesn't necessarily affect the transport process.

So, it doesn't lead -- there's not a one-to-one correspondence between every location, and cesium release, just because of the transport process.

And quite frankly, you're putting a lot of heat in the fuel and you haven't got heat to put into the piping system. So, you end up with a cool piping system which leads to high deposition rates.

MR. WAGNER: Our volatiles are pretty close to 100 percent release.

MEMBER POWERS: You can't get --

(Simultaneous Speaking.)

MEMBER POWERS: That's released from the fuel, but it's not released into the containment.

MR. WAGNER: Right.

So, over most of these cases, we do get the volatiles out, but maybe I misspoke that our releases might change a lot.

Our hydrogen could change and that has dramatic effects, but maybe not so much what gets off the fuel.

The release coefficients that we have from the fuel, but most of the volatiles come off.

MEMBER REMPE: I'll have to look where I saw it, but I think there's a discussion somewhere on the report that does kind of have a different perspective.

But the higher relocation temperatures would increase the releases from the fuels somewhere. And I'd have to find where it is, but that may be something to look for.

DR. HATHAWAY: Okay, so I'm just sort of moving to the Consequence Analysis.

We took all of the source terms that were run to 72 hours, and we put them in the uncertain MACCS model of the site to see what the latent cancer fatality risk for the zero to ten-mile interval.

And again, as in the full study, these are conditional on the occurrence of the assumed --

CHAIR STETKAR: Trey, I want to make sure I understand both of these tables, but 11 is the one I want to understand.

These results, to me, mirror the results in the baseline study, which is encouraging in the sense that you're showing a bimodal effect on the conditional risk, the way the dots on the lower left are clustered.

And the numerical ranges are similar.

The difference is that the first column in 11 that you called really Containment Rupture is a subset of the scenarios in Figure -- I'll give you the Figure number -- 6.1 and 6.3 -- so that the right-hand side of those Figures, the 87 percent that goes to early containment failure.

The first column in this table is a subset of them, right?

The second column, the thing that's called late containment rupture, is a combination of so-called late containment failures that are also in the right-hand side of those figures in Chapter 6.

And the small set that never go to containment failure which is the far left-hand side, where the risks are really, really small, the other mode of that bimodal behavior.

And therefore, comparing the numbers in this table to numbers that appear in other sections of the report is a little bit misleading. I mean, you really have to kind of look through that stuff.

I was surprised that you didn't cast these results in the same way as the results in Chapter 6, so you could see that, yes, when we turned up the microscope in this area, we basically got the same shape, we got the same fractions in the bimodal, and roughly the same ranges in the conditional latent cancer fatality risk, coming through the zero to ten-mile snapshot here.

You can kind of see it from this, but you really have to, I think you really have to, study it.

Have I mischaracterized anything, Tina? I mean, you're really familiar with all of those curves.

MS. GHOSH: I think it's a good comment and we can supplement the --

CHAIR STETKAR: I mean, basically, on the Figure on the lower left, if I take a horizontal slice in that Figure at about 1e to the -6, everything above that horizontal slice, is what's on I'll call it the right-hand side of the Figures in Chapter 6.

Everything below the horizontal slice is on the left-hand side of those Figures approximately, right? I just want to make sure that I got it.

MS. GHOSH: Yes, I think that's right. These results are spread out a little bit more.

CHAIR STETKAR: Oh, yes, they're going to be spread out a little bit just because what you did here. But in the big-picture concept --

MS. GHOSH: Yes, I think that's a good idea. We can add a paragraph to compare back to the --

CHAIR STETKAR: Because I like what you did in Chapter 6, by the way.

We talked about that bimodal behavior, and then you split it out to more explicitly show -- and I really liked that, that kind of -- emphasize some of the points you were trying to make in the text.

And you can draw the same conclusions from this but it's a lot more difficult. I mean, you have to really study this. Okay.

DR. HATHAWAY: Okay, I probably won't dwell much on this.

The top table just is the sort of statistics on the total risk where the bottom is sort of statistics but looking at the emergency-phase contribution, and the long-term phase and intermediate-phase contribution individually.

So, finally, because we had more of these early containment ruptures, we were interested in the early fatality risk.

Previously, we only had four realizations that had early fatality. So, we just looked at these results. Past four miles, there was zero calculated early fatality risk.

And here are just the statistics, and again, this is a conditional long-term event occurring.

And what we tried to do, and this sort of all is dependent on the discussions from earlier, but we tried to scale these results to consider the full distribution because everything outside of this parameter space would be to zero.

So, we tried to just scale it out to consider the entire distribution, and that's what that second column is.

It would be what the risk would potentially be if you were to essentially add a lot of cases that had late containment.

CHAIR STETKAR: A comment on Chapter I think it's 6, where these similar tabulations are shown.

It would help a lot of people to provide a little bit more of a tutorial in that section about how one can have a mean value that's something on the order of ten to the minus, I don't know, small number, eight, or whatever those numbers are, nine.

And yet, have precisely zero values for the median 5th and 95th percentile.

The only reason I dwell on that is that people who use very broad and very skewed distributions routinely understand that.

Not necessarily everyone who reads this study will have that level of understanding, and a little bit of a tutorial will help to those folks to not get the impression that this is simply some sort of statistical mumbo-jumbo that has no basis in reality, kind of anchoring them back to say that, yes, I can get 95 percent of my samples to have precisely zero, and yet, the mean value can still be non-zero. And show them why that is.

Otherwise, the naysayers will say, well, obviously, this is just statistics and numbers and numbers games.

So, I think that would help in Chapter 6. Here also, but here, at least, most of them, the 95th percentile is above the mean, and that's reassuring to a lot of people.

DR. HATHAWAY: So, that was all I was going to cover in this.

CHAIR STETKAR: We're at a break time here. No, any more questions, first of all, on Appendix I? Because we're going to shift gears here entirely.

If not, let's take a break, and I'm going to try to hold us to ten minutes. I'll probably fail but try to get back here by 10:20; we'll recess until then.

(Whereupon, the above-entitled matter went off the record at 10:08 a.m. and resumed at 10:21 a.m.)

CHAIR STETKAR: We're back in session. We are back in session. Trey, Tina, I don't know, it says Casey. Anybody?

MEMBER BLEY: I don't know when to ask the -- thank you. I don't know when to ask this, but I'm going to ask it now, as we move into more general things. Sure.

(Laughter.)

MEMBER BLEY: But it's kind of the same cast of characters here and who we meet in the Level 3

PRA work. And over in the Level 3 PRA work, I think we've learned that some of the things you're doing in SOARCA aren't making its way over there, because it's not state of the practice.

But something comes up over there, I'm curious about it, and over there, in our recent review of the Level 2 PRA, I find a whole section of their report on model uncertainties that's really pretty thoroughly laid out, at least the thinking of things that might not work the way we expect them to work and ways that they might consider them in the report.

I don't find anything quite as thorough, I mean, you do a tremendous job on the parametric uncertainties, but a few sensitivity studies that are kind of picked here and there, but nothing as systematically thorough, of saying, where could there be things about the model that aren't right, that I see in this new part of the Level 3 PRA.

Have you thought about that? And probably the work you've done here led to how that evolved, but I would sure like to see it here. It seems that part of the uncertainty analysis that we've always kind of talked about and never gone very far on over here.

MR. FULLER: Yes. So, Dennis, unfortunately, Don and Hossein, who were, along with myself --

MEMBER BLEY: They were here all morning.

MR. FULLER: They just walked in and they missed your whole question.

(Laughter.)

MEMBER BLEY: You want them to handle it, you don't want to handle it.

MR. FULLER: So, what I will say is this, the team that worked on that document and that work, obviously Don Helton is the lead for all of that.

MEMBER BLEY: Right.

MR. FULLER: It's the same team, so you could say that --

MEMBER BLEY: It kind of seemed that way to me.

MR. FULLER: Yes. So, the portion of the SOARCA team that's been thinking about, we kind of focused our efforts in supporting Don in the Level 3 PRA study for that more systematic thinking. And, of course, the scope is much broader for the Level 3 PRA.

MEMBER BLEY: It is, but --

MR. FULLER: Yes.

MEMBER BLEY: -- SOARCA makes a claim that, one, it finally acknowledges that it isn't a risk assessment and you pick out particular scenarios. But then you do a thorough consequence analysis and now, you've added to that a thorough uncertainty analysis.

And, in fact, the parametric uncertainties affect the results substantially, and we see that. But the potential of the modeling uncertainty is to overwhelm what we see in the parametric uncertainties has always been an issue and we've talked about it occasionally.

But this new part of the Level 3 PRA on model uncertainties and the Level 2 analysis is the first attempt at being thorough in a qualitative examination of the things that might affect it.

It seems to me that at least some acknowledgment of that approach and that it hasn't been done in the SOARCA analysis would add to the comfort level of people who have been uncomfortable with SOARCA to this point.

MR. FULLER: I think -- thank you for that comment. I think it's a good comment and we can certainly add pointers to that work in our executive summary and the introduction and when we talk about the scope of our work being limited to the parameter uncertainty.

We can certainly refer to that work as an example of more thorough, at least qualitative thinking through of all the modeling uncertainties that we're not explicitly addressing.

MEMBER BLEY: And that's the first step. Eventually, one would like to be quantitative about it, but in SOARCA, we haven't even been qualitative about it, in a thorough way.

MR. FULLER: Okay. Yes, thanks for the comment. And I see Don is at the mic.

MR. HELTON: I just want to -- Don Helton, Office of Nuclear Regulatory Research. I do just want to point out, though, that that work is non-public and will remain non-public for, at least the current plan, well after this document would be finalized. So, there is --

MEMBER BLEY: The engineering thinking --

MR. HELTON: -- a challenge there.

MEMBER BLEY: -- beyond it has been around a long time. The application hasn't. And --

MR. HELTON: Yes, I mean, it can certainly --

MEMBER BLEY: -- maybe I shouldn't have mentioned it, because that is not --

MR. HELTON: No, I'm not worried about you mentioning it and it can be described from a conceptual standpoint as what was done.

MEMBER BLEY: That's what I'm after.

MR. HELTON: I'm just reacting to the idea that it's --

MEMBER BLEY: To have a pointer to it.

MR. HELTON: -- it can be cross-referenced --

MEMBER BLEY: Yes.

MR. HELTON: -- and that just presents some challenges, that I'm sure we can work through. But -- I don't know.

MR. WAGNER: Casey Wagner, Dycoda. So, I'm going to go through, not all, but we thoroughly went through the comments from the last meeting and I'm going to address some of the highlights of the more important ones and how we addressed them.

So, we're on the first slide here. First one had to do with the fabric seal failure criteria. And we had a range of different types of comments that were relatively extensive discussion on that.

And they concerned the error itself, a confusing description of the failure parameter, and then, there was also some questions about how we were handling the seal failure error. We tried to address these by adding new documentation to the next version of the report.

And in particular, we did a little bit more work on the impact of the fabric seal on early containment failure. And added some new text in there. In particular, the flow through the ice chest and the other leakage pathways allowed hydrogen to move around, whether that seal had failed or not.

So, there were other mechanisms for, over the time frames that we were looking at, that hydrogen could be moving throughout the containment. So, certainly, if the seal had failed, which with the error introduced, it failing more early, it flowed a little bit more thoroughly.

But nevertheless, there was other leakage paths that allowed that to occur. And so, there's some discussion about that. In particular, the focused value study had many cases and in all those calculations, the fabric seal had been corrected.

And so, we looked at the response of those and tried to compare that to the UA calculations and drew conclusions that the responses were similar on key metrics.

And then, finally, Hossein had presented last meeting a comparison to the 2016 UA results, which did have the correct -- well, it had a different distribution on the fabric seal failure, but it was not prone to failing early. And that discussion is also in there.

CHAIR STETKAR: Casey, when I read that, and I -- the discussion of this issue, by the way, is much, much improved. But I know nothing about the physics, but I thought that in the current study, when you looked at the limited cases that you ran for long-term, that you saw some -- I think a couple of those kind of transitioned to an early containment failure.

So, the question that I had is, how significant is that in the context of the full study? I mean, what fraction -- if you had the corrected model in all 600 realizations in the real study, do you expect a similar number to transition to early containment failure?

Because we're not finding many early containment failures, so, like two or three can double the number that we've seen. And I think I understand why that's happening, but I'm trying to get a sense of looking at the full spectrum of the baseline study.

You only ran a small number of these long-term or late containment failure scenarios, right? In the --

MR. WAGNER: Yes.

CHAIR STETKAR: -- focus study, there's --

MR. WAGNER: Yes.

CHAIR STETKAR: -- about 18 or 20 or something like that, that you ran, to examine it.

MR. WAGNER: Right. If you combine that with the 2016 calculations that had a more robust seal --

CHAIR STETKAR: But a different --

MR. WAGNER: Yes, it's --

CHAIR STETKAR: The reason why I dislike this notion of the comparisons is that the uncertainty difference -- the differences on the uncertainty distributions make a difference.

MR. WAGNER: They do, yes.

CHAIR STETKAR: So, you have to kind of focus in the regime of the 2016 study, where --

MR. WAGNER: This was about a -- in the couple cases I looked at, it was about a two to three PSI difference if the seal was there during a burn that was -- the ones that flipped over --

CHAIR STETKAR: Before the --

MR. WAGNER: -- that were just below the failure pressure and if the seal was intact, it was just above.

CHAIR STETKAR: Okay.

MR. WAGNER: And so, you had to be very

close. So, we judged --

CHAIR STETKAR: Yes, but I was going to say, but there are two. If I ran an infinite number of samples in the baseline study, such that you had the full distribution for everything, I'm trying to pulse what fraction, a guesstimate. Is it one percent, ten percent --

MR. WAGNER: Okay.

CHAIR STETKAR: -- half of a percent might transition?

MR. WAGNER: I certainly can't answer --

CHAIR STETKAR: Yes, okay.

MR. WAGNER: -- the percentage, but this plot might be kind of useful. So, we're looking at -- and unfortunately, the blue ones have the corrected seal behavior --

CHAIR STETKAR: Yes.

MR. WAGNER: -- versus the purple. But the purple ones that are within a couple PSI could have flipped over. So, I see maybe two there.

CHAIR STETKAR: And --

MR. WAGNER: I think that --

CHAIR STETKAR: But, again, they're -- when you say within a couple PSI, you're still limited by the number of samples from both the failure pressure distribution.

And, if you're not hitting many samples, that could either be fortuitous or -- I mean, you would expect those samples to be somewhere in the middle of the distribution, if there's only a small number of samples, right?

MR. FULLER: Yes, so, I guess just --

CHAIR STETKAR: The containment failure probability --

MR. FULLER: Right.

CHAIR STETKAR: -- distribution is what I'm talking about.

MR. FULLER: Right. So, if we get back to -- so, the focused study was looking at the range where you could have an early containment failure. So, you saw in 80-some percentage, it still went to the late containment failure versus the early. And what gives us some confidence -- and all of those have the fabric seal failure criteria fixed.

So, what gives us some confidence is that, in those realizations, the 360-something, with the fixed seal pressure, we still saw the same, roughly the same percentage of those cases where it's possible, actually going to early containment failure. So, that's what -- that's kind of where we're --

CHAIR STETKAR: That might, yes, that might be reassuring. I have to think about it that way, thanks. Thanks.

MR. WAGNER: I verbally gave you some information on late containment failure, because in the last set of documentation in June, you didn't have that and so, I documented that.

The Committee expressed some confusion on this parameter in general, because there was a thermal part and then, there was sampling. And so, that was rewritten and hopefully, that's much clearer now. And --

CHAIR STETKAR: On the record, it is.

MR. WAGNER: Great, great. It was a good comment and I'm glad we could improve the documentation. And then, there was also some questions about the seal failure area related to that comment, because we have some where it's sort of a cold mechanical failure and then, some where there's a thermal presence that's maybe more widespread and maybe that should have a different failure area.

And we hadn't thought about that and that was -- it was good that it was pointed out. It wasn't a controlling factor on flow, if you look at the K over A-squared and you look at the thermal hydraulic resistance, even the small holes that we were sampling allowed a pretty robust natural circulation to occur.

So, we concurred that that probably was likely, that there's maybe a wider, larger area that we should use, but it wouldn't impact the results, was my engineering judgment.

And then, if you look at the seal, it winds around the nooks and some of it's well-shielded from thermal radiation effects. And so, depending on where the fireball was, it -- there certainly is some uncertainty on what parts would be affected.

Next slide. The ice condenser doors, and we're going to call this AJAR parameter. The AJAR parameter in context of the next couple of bullets, is whether we fully opened up the doors to form the hinges somehow and they stuck open. We satisfied that criteria.

CHAIR STETKAR: Casey, just for the benefit of everybody else, make sure, these are the lower condense inlet doors?

MR. WAGNER: That's right.

CHAIR STETKAR: Okay.

MR. WAGNER: Exactly. And that's the only place we had done the sampling, because we thought those were by far the most important. And just a little background, those are modeled with five MELCOR flow paths, representing, I think it's about 20 doors.

They take about 46 pascals to fully open, so it's not a lot of pressure, but surprisingly, that took a bit of a pressure pulse in order to open them, all of them, because they open up such a large flow area, about ten square meters each. And so, you get these opening up and they start relieving a lot of pressure.

And the general comment was, we didn't have much discussion about how they behaved. We had the sample parameter, so we added discussion about how the valves or how the doors behaved and FSEM plots and tried to look at a range of different responses.

Where we might have had a hydrogen burn early, prior, due to the PRT, one that might have been delayed until hot leg failure. And then, one where there wasn't even enough hydrogen there, we had hot leg failure and looked at the response.

And the interesting thing that we also got, Trey was able to dig out a few more diagnostics out of his study, was that if we had a hot leg failure, we almost got 100 percent, all five doors sticking open.

So, that pressurization from the hot leg failure was enough. The pressurization from the PRT wasn't. And that is just a smaller tank, that's blowing down.

And even though I can imagine that's pretty violent down there, it was triggering some of the doors sticking all the way open, but not all of them. And in general, I call that a partial AJAR, because it's not all five MELCOR flow paths fully jammed open.

And so, we were able to get some really interesting insights, digging into the door behavior a little bit more. It's all documented in the report. We talked a bit about ACRS member comment on or it came up that -- we talk about burn direction, inside the report.

And we have known ignition sources and you said, we know the geometry, we know where they're located, does it make sense that we are randomly sampling the direction? That made maybe more sense when we had the random sampling, a couple studies ago.

And I'm going to try and make a distinction to you that, although we know the location of those ignition sources, they're coming from the hot leg or the PRT or from the ex-vessel debris, the distinction I want to make is where the ignition actually takes place may be a little ways away from there and elsewhere inside the compartment, because of the hot plume has to come out and it's got to mix with some oxygen.

It's coming out as just pure hydrogen and steam and that's not combustible until it mixes and maybe ends up in different locations. And so, there are -- or it's aerosols and hot things coming out of the ex-vessel debris. And so, we were -- we recognized the point that you were trying to make, but we felt like there is still uncertainty where ignition takes place.

And then, we kind of go one step further, recognizing the comment that you had is that, let me just read that last line, identifies the flame directions as it propagates within the compartments as surrogates for uncertainties in the ignition location. So --

MEMBER CORRADINI: So, I interpret that to mean you didn't change anything?

MR. WAGNER: We didn't change the words that we used. The upward, horizontal, and downward is pervasive through the document. And we tried to give some motivation that there's some uncertainty on where it was located.

CHAIR STETKAR: They -- as I read it, you changed nothing in the model --

MEMBER CORRADINI: Right.

CHAIR STETKAR: -- you added some words in the text to --

MEMBER CORRADINI: Justify what you're doing.

CHAIR STETKAR: -- impart this notion of perhaps a more randomization of the ignition location, despite the fact that you had three nominal point sources.

MR. WAGNER: We --

CHAIR STETKAR: Is that -- that's what I kind of hear you saying this morning, that --

MR. WAGNER: Yes, the --

MEMBER CORRADINI: I --

MR. WAGNER: -- that --

MEMBER CORRADINI: I'm sorry.

MR. WAGNER: Go ahead.

MEMBER CORRADINI: No, you finish, I'm sorry.

MR. WAGNER: We understood, we could -- that was, I think, Mike's comment at the time. We know where these things are, shouldn't we -- but that's where the hot gas leaves from. And then, it isn't ignited yet, it's -- you have to have adequate oxygen, adequate hydrogen, and you have to have that mixing and the source, the high temperature, in order for it to ignite. And so --

MEMBER CORRADINI: But -- okay. So, I'm not going to -- I mean, I don't think this is -- well, I don't know if this is a big effect, it may not be a big effect.

But in my simple mind, if I know where it's coming out and I know the geometry, and we're talking about a specific reactor, then I'm going to essentially entrain the constituents so that I could estimate when I'm going to get the right concentration and, therefore, decide whether it's up, down, or sideways, versus letting it be just a casting.

So, that's kind of the source of my -- right? If I'm dumping it out from hot gas to the hot leg, then I know how it's going to, not know, but I can, from an engineering judgment standpoint, decide how it's going to mix.

I'm -- I seem to remember, my memory is not so good, I seem to remember, I was just very confused about the directionality issue. And it just seems to add confusion, particularly when you know where it's coming.

So, I'll ask it a different way. Did you do a side calculation to confirm that it didn't make a big effect or was nothing done calculational, you just re-explained it? Re-rationalized it, excuse my English.

MR. WAGNER: It was the later.

MEMBER CORRADINI: Okay.

MR. WAGNER: It's coming out at sonic speeds, it's not combustible, what's leading --

MEMBER CORRADINI: Well, I mean, there's an old study -- so, I only remember things that are old. There was a study about 35 years ago from Spalding, which has a very set of simple experiments of a gas jet, whether it's a light gas or a heavy gas, he does everything.

And he basically does a very beautiful way of showing how far down the pathway, how the concentrations mixed with something, and you can use Spalding's study as a way to estimate, if I have the hot debris coming up PRT, it's so many L over Ds and, by God, that's where it's going to go and it's going to propagate in that direction and then, we're done.

MR. WAGNER: Yes.

MEMBER CORRADINI: Versus doing it stochastically. That's kind of what was going through my mind.

MR. WAGNER: Yes. Our dominate source is the hot leg failure, which we have a lot of uncertainty on --

MEMBER CORRADINI: Where it's going to happen.

MR. WAGNER: -- the working -- and the PRT, I think that probably makes more sense on. But most of our ignitions came from the hot leg. And then, the cavity is a little bit different animal, because it's hot debris --

MEMBER CORRADINI: I think --

MR. WAGNER: -- it's kind of closed up --

MEMBER CORRADINI: -- the reason I focused on this is, I have to admit, I still don't understand the logic of the directionality. Maybe other people do.

CHAIR STETKAR: I thought it got it when we had a uniformly distributed set of random ignition sources.

MEMBER CORRADINI: Because then they --

CHAIR STETKAR: Because they could --

MEMBER CORRADINI: -- can go anywhere in the volume.

CHAIR STETKAR: -- go anywhere in the

volume.

MEMBER CORRADINI: But when I know where it is in the volume, it seems to me, I've narrowed my possibilities. That's, in my --

CHAIR STETKAR: I think the truth is, there's still uncertainty, but it, in my opinion, it's much more constrained. It ought to be much more constrained in the current model. But what you said earlier, does the difference -- would it make a difference? I can't, I certainly can't answer that question, I have no idea. I have absolutely no idea.

MEMBER CORRADINI: I mean, I know you have -- sorry, my green light's on. I know you have limited resources and the uncertainty study is fairly complex, but I guess a side calculation, just to verify that directionality doesn't make a big difference once I have the known location of the source, would be an interesting way to kill this guy off. And I see we have a helper.

MR. ESMAILI: Let me -- well, I don't know.

CHAIR STETKAR: Hossein? Identify yourself first.

MR. ESMAILI: Let me --

CHAIR STETKAR: Hossein? We don't know you.

MR. ESMAILI: Oh, okay. Hossein Esmaili --

CHAIR STETKAR: Thank you.

MR. ESMAILI: -- Office of Nuclear Regulatory Research. So, I hope I'm not going to confuse you further. That's why we wrote down here, the location of ignition within the compartment. So, in other words, this is tied to the flammability limit.

So, we have this control volume, and you can shake your head if I'm going in the right direction or not, so, we have this control volume, right? Hot gases are coming from one source, right? I don't know whether my flame -- the way we are modeling it, we are modeling it as a flame, right?

Propagating either, within this control volume, not going to other control volume, whether it's going to go down, going sideways, or going upwards, right? So, this is one way of capturing where that flame is going to propagate from and whether I have satisfied my flammability criteria.

Typically, what we do is that, by default, in medical, for example, we say, if in a control volume, if you have more than ten percent hydrogen and other flammabilities are satisfied, then you have ignition. Or if the ignitor is available, it's seven percent.

Here, we don't know, because we don't know how this gas, as they are coming out of this hot leg or as they are going upwards, they are mixing within that control volume, so I don't know exactly where that frame front is propagating from.

So, this is not only the location of the ignition sources, but it's tied to how I am doing the flammability limit within that control volume. This is not propagating from this control volume to that control volume. That one, we know already.

MEMBER CORRADINI: Okay. So, within the volume, we're speaking?

MR. ESMAILI: Within the volume, we're speaking. So, as Casey said, this gases are coming up right at the location where the hot leg is or where the PRT is, that does not -- that may not satisfy the flammability limit, because he says that it is mostly hydrogen and steam. It has to mix with this volume.

And we are working within a control volume approach, right? So, what we really do is that, we are, in the control volume, we are trying to model a flame that propagates in that control volume and for that, we are just changing the ten percent to four percent, six percent, nine percent, because we don't know where it occurs. Did I --

MR. WAGNER: Yes. It originally went back to the Kumar data and if you ignite at ten percent, it can go downward. If you ignite at seven percent, it can go sideways and upward. And if you ignite at four percent, it only -- you're the expert, you can --

(Laughter.)

MEMBER POWERS: I simply relate an example I have actually used in a class I taught, where having explained how well we know these ignition directions and whatnot. That at 12 percent, you get kind of a uniform expansion of a flame. At nine percent, it has a tendency to go up. At seven percent, it has a profound tendency to go up.

And then, I show an example of a seven percent ignition and it does exactly as I said, the flame propagates upwards. Everybody watches and amazed that I'm so accurate in my prognostications. And promptly, the flame comes down. And it's because the gas gets heated up and pressurized.

Directionality of flame once it's ignited is an extremely complicated thing to model and doing it in a lump node code, where you've got all kinds of structures actually there, that are vacuous in the code, it seems to me to be braver than I am at these things, especially when you're talking about relatively lean flames.

It -- directionality is easy and reasonably concentrated, where reasonably concentrated is like ten to 11 percent. Once you get down to lean, around seven, it's going to depend on things other than just the concentration of the hydrogen in there, everything else is going to make a difference, too.

MR. ESMAILI: This is Hossein Esmaili, again, from Office of Research. One thing we want to make clear is that, when we say within the compartment, we don't mean every compartment that we model.

This one, we only imposed this in the lower compartment region, correct, Casey? Because that is where the location of the hot leg is. So, this only means that we do this uncertainty in the lower compartment, this control. So, the other compartments are still allowed to propagate. So, yes, this was another --

MR. WAGNER: Yes. That's a good point to be made. After we ignite it, in some ways, you could look at this as surveying a range of ignition strengths and that first place that it ignites. And then, it will propagate according to the default criteria, which uses Kumar also. I understand your point, Mike.

MR. FULLER: Okay. So, we'll move on to the next one. I don't know about you all, but after the last few years of struggling with the safety valve modeling, every time I see those words together, safety valve, I have this sense of dissatisfaction deep inside my core.

But we took your comments from the last Subcommittee meeting and we tried to address them as best we could. Oh, you missed my -- dissatisfaction deep in my core, every time I see those words together, safety valves.

(Laughter.)

MR. FULLER: But we're trying, we keep trying.

CHAIR STETKAR: You're very trying.

MEMBER POWERS: You're an obsessive-compulsive, that's all there is to it.

MR. FULLER: You're right, it takes a certain amount of OCD to keep going and soldiering on. We keep trying.

(Laughter.)

MEMBER POWERS: This is a psychosis that can be treated.

CHAIR STETKAR: Tina?

MR. FULLER: Yes?

CHAIR STETKAR: In all seriousness --

MR. FULLER: Yes.

CHAIR STETKAR: -- I'm really disappointed. I mean, I'm really, really disappointed. I asked, in June, a simple question. In June, we said, well, we had this data -- I'm sorry, I'm not even going to call it data anymore. We had these numbers from NUREG/CR-7037.

MR. FULLER: Yes.

CHAIR STETKAR: Those numbers are 15 failures in a total of 769 demands. That's all it's in. Then, you said, well, okay, NUREG/CR-7037 only goes up through September of 2007, so, gee, we went back to see what more recent experience is.

So, we looked at experience from October of 2007 through March 2016 and, lo and behold, we come up now with 16 failures, one more failure, in a total of -- I have to look up my numbers here, so that they're on the record. I can't find them and I'm not going to waste my time. A total of 75 more demands, which you've subdivided into initial demands and subsequent demands.

So, my question in June was, tell me how you estimated the additional 75 demands and where the additional failure came from, because that, at least, was done by people who are still living today and perhaps have a memory, and probably documented what they did, so that we could understand, at least from that snapshot, the thought process.

That does not at all come through in the current report. In the current report, all it says is, we needed to use engineering judgment and, yes, there's still a lot of uncertainty on what the failure rate should be. That's all it says. That's all it says.

So, the report does not answer any of my questions. So, I'm hoping that some physical body sitting in this room can answer my questions.

MR. FULLER: Yes.

CHAIR STETKAR: Because if you can't, I'm willing to say that the entire study is flawed. This is a big issue, you make assertions in the report that, oh, yes, well, things could slide to the left, things could slide to the right, but you see the same thermohydraulic behavior whether a valve fails to open.

That's great if you're doing a conceptual study. It's not good if you're drawing insights and conclusions about the behavior of the Sequoyah plant.

MR. FULLER: Okay. So, what we did add to the report was a summary paragraph. I realize, again, it's not satisfying, because a lot of engineering judgment does have to go in from the people who are reading these reports and capturing both the demands and the failures in the database.

So, we tried our best. We did talk to the people who actually do this, the coding of the operating experience, to figure out what they -- how they decide. So, everything is reliant on licensee reporting.

CHAIR STETKAR: I --

MR. FULLER: So, they have a requirement to report failures of the codes they see. They certainly don't have to report all the demands.

But typically, the operating events, these are post-scram events, you can try to guess how many demands were placed on the safety valves, because you do have at least information that the scram event happened and then, you can read the description of what happened.

Sometimes, there's information about system pressures, sometimes there isn't. The level of information just varies a lot from LER report to LER report.

So, in terms of the numerator, at least we have more confidence that, because of the reporting requirement, that we know how many failure events happened for the safety valves. For the denominator, the number of demands, we don't have a definitive answer.

And I think what we said in the report is that we may be undercounting the demands, because we have to use some judgment on --

CHAIR STETKAR: But --

MR. FULLER: -- when those valves were demanded and how many.

CHAIR STETKAR: Let me read verbatim what's in the report, so it's on the record. Many LERs describing the plant response to scram report the operation of SVs, but a significant portion of the LERs just use phrases like, all systems operated as expected. Hence, for the number of demands, expert judgment must be used to interpret information provided in the LERs and may be undercounted.

My assertion is that you may have substantially over counted the number of demands, because it's very, very surprising to me that between October of 2007 and March of 2016, we had a number of scrams that resulted in 75 demands to open main steam safety valves and/or pressurizer safety valves, but I would be really surprised if we opened any of them.

So, my assertion is that you may have substantially undercounted the denominator, making the whole failure rate substantially optimistic. And if the same process was used in NUREG/CR-7037, the numbers from that report may be substantially optimistic, the failure rate.

So, what I was trying to -- what I was hoping to achieve, and apparently will fail, is to get someone who actually did the work from 2007 to 2016 to tell us what criteria were used when you examine the operating experience to say, this event I judge today to result in a demand to open the main steam safety valves at Plant X and I assumed that n number of safety valves were demanded to open.

I'm not hearing that answer. And without that answer, I have absolutely no confidence in the failure rates that are used. And I firmly believe that they're optimistic. I can be convinced otherwise, but I need to be convinced otherwise by somebody who tells me what they did.

MR. FULLER: Yes, okay. So, I apologize. As I said, I summarized the input we got from the team in that one paragraph we inserted, but maybe we can arrange a side conversation with the folks --

CHAIR STETKAR: No, no, we don't do things --

MR. FULLER: -- who actually --

CHAIR STETKAR: -- in side conversations, this is a -- the Subcommittee has raised this --

MEMBER BLEY: At least, not technical information.

MR. FULLER: Right, okay.

CHAIR STETKAR: Not technical information.

MR. FULLER: So, maybe what we can do is try to set aside some time at the full Committee meeting to --

CHAIR STETKAR: Well, we have to decide on --

MR. FULLER: Okay.

CHAIR STETKAR: -- the problem with the full Committee meeting is that we have pretty good representation of Committee Members here and have had --

MR. FULLER: Yes.

CHAIR STETKAR: -- in previous meetings. But I don't think that we've had the full complement of the full Committee. And the Members who haven't attended the Subcommittee meetings need to get briefed on the whole scope of this study.

MR. FULLER: Yes.

CHAIR STETKAR: And even those of us who have attended the Subcommittee meetings have to have a better feel for how you're presenting the full study to the full Committee.

MEMBER BLEY: Other than an informal communication, which we can't do, if you wrote something up and sent it to us, that we could look at ahead of time.

MR. FULLER: Oh, okay, I see.

CHAIR STETKAR: Yes, if you want to write a little report --

MR. FULLER: I see --

CHAIR STETKAR: -- we can do that.

MR. FULLER: -- and then put it --

CHAIR STETKAR: But you --

MEMBER BLEY: A memo.

CHAIR STETKAR: A memo.

MR. FULLER: -- put it in the public record, yes. Okay. Actually, that's a good --

CHAIR STETKAR: And that might be a way, if we can get it -- the full Committee meeting is in less than two weeks.

MR. FULLER: Yes. Okay, yes. So, yes.

MEMBER SUNSERI: While you --

CHAIR STETKAR: But we can't do it informally.

MEMBER SUNSERI: While you --

MR. FULLER: Okay.

MEMBER SUNSERI: While you're thinking about that question, the question I had in association with this is, does the failure analysis include testing? I mean, you test these safety valves every refueling outage --

MR. FULLER: Yes. So, we had -- yes. We had previously talked about that quite a bit. We discovered through talking to the folks who actually conduct the testing that the testing setup doesn't quite test the reclosing probability, which is what we care about.

The testing requirements are focused on relieving pressure under design-basis overpressure accidents. So, they test very well the ability of that valve to open when you demand it to open, but they don't really test it under the conditions that we're looking for, repeatedly passing hot steam and so on. So, there is a --

MEMBER SUNSERI: Okay, so you don't

consider a valve that may be in an as-found test condition low on its setting to be a valve that would be open during an accident?

MR. FULLER: So, yes. So, the setpoint drift is a separate issue and you're right that we don't worry so much about the setpoint drifts that are typically found during testing.

It's true that a valve may open at a slightly lower pressure than it's supposed to, but in the grand scheme of things, we had looked a bit about the effect of that in the -- through sensitivity studies in the Peach Bottom study, which of course is a different plant and we were looking for different things.

But the setpoint drift issue is not as important as all the other things that are going on in the integrated analysis.

MEMBER SUNSERI: Okay, thank you.

MR. FULLER: And so, yes. So, we are not using the testing data, because if you look at the distributions are very different from the operating experience data. And we did kind of talk to the testers, we figured out that it's not applicable. That's why there's so much focus on just using the operating experience data, which is from actual scram events.

CHAIR STETKAR: Tina, if you -- the other thing that we brought up back in June is that, and I don't want to get into details of the uncertainty distribution, but you -- we've revised the uncertainty distribution for the open fraction, I forgot the parameter value, but given the valve sticks open, how big is the area that it sticks open, that was revised from 2016 to current time.

That distribution says that there is a 30 percent probability, for example, that a valve remains open more than 90 percent. And that -- the shape of the distribution seems intuitive, the distribution and the discussion of the distribution in the report is quite good.

It says that most likely it's just going to weep, it's going to stick open just a little bit. There's a non-zero, in this case 30 percent, chance that it could stick open big. And then, there's kind of a small probability that it could stick open somewhere in-between, and that equal probability was distributed over those open area fractions.

That all sounds really good, if indeed we have information about the numerator, because people report these things in LERs --

MR. FULLER: Right.

CHAIR STETKAR: -- and we have something on the order of 15 or 16 failure events, 30 percent of those would be something on the order of four, five, six events where we would expect to see a valve stuck open pretty big.

So, we would also ask the question, is the operating experience, even in the numerator, the things that you have more confidence in, because you can go read those things, does that operating experience support the uncertainty distribution for the open fraction?

MR. FULLER: Yes.

CHAIR STETKAR: And you said, yes, we're going to go back and look at that.

MR. FULLER: Yes. So, that's what my second bullet is about. We did go back and reread all of the LERs for those events and we actually had a valve subject matter expert also review both the LERs, as well as we had additional information from a proprietary database, which we can't get into the details of that in the study, but we explained in the report that we have access to this and we looked at it, but we can't explicitly refer to the information from that.

Unfortunately, in relooking at all of that, there was nothing new that we could tease out to kind of add additional details in the report that would be worthwhile. We continued to rationalize, I guess, the distributions that we had come up with. And for the open area fraction, it's even more frustrating because some of the reports had no information on it.

But, however, I will say for that large open area fraction, there were a handful of events, I can't remember if it was four or five, I want to say on the order of four, that were due to latent conditions, assembling errors, maintenance errors where the valve was left in the position that, when it was open, the internals got completely misaligned, something fell out of place, and it was basically stuck open with a pretty large area.

So, the mechanisms for the failing with the large open area versus the weeping area are kind of different. And what we saw in some of these events was, when there was a maintenance or assembly error, sometimes it got stuck open with a very large area and couldn't be recovered, even by lowering the pressure.

So, we had data, again, hard to quantify very specifically what percentage should fall in these different bins, but we did have data to support these two ends of the distribution.

MEMBER BLEY: You give, in a couple places, I think, a description of qualitatively the kind of people you talked to and how you tried to go at this and the data you looked at. I didn't see anything real clear on how you picked the 90 percent and the 30 percent.

I have a question about the design of the valve. I think the design of the valve stem is such that if you're 30 percent open on this kind of valve, but I'm not sure of this, that you get something like 30 percent mass flow.

I don't know if that's true. How did you translate valve open fraction into mass flow rate for your calculations?

CHAIR STETKAR: That's a MELCOR question, right?

MEMBER BLEY: Well, I --

MR. FULLER: It's a --

MEMBER BLEY: -- don't know if it's an input to MELCOR or if it's something done inside the code. I don't know if that aligns with how these particular valves are designed for flow rate versus percent open.

MR. FULLER: Yes.

CHAIR STETKAR: And in particular, take that very, very unlikely thing in the center, where it's stuck open 37 percent.

MEMBER BLEY: That could be a whole lot --

CHAIR STETKAR: That's --

MEMBER BLEY: That could be almost full flow or it could be fairly low flow, depending on exactly how that valve --

MEMBER MARCH-LEUBA: If it's high pressure --

MEMBER BLEY: -- seeping surface is designed.

MEMBER MARCH-LEUBA: If it's high pressure, it's likely to choke flow and be linear with area. And at this pressures, I would say it's choked flow, right?

MR. WAGNER: That's a true statement. Sometimes, you get what you're getting to, you get stem position versus flow area. And so, it will change and it can be nonlinear.

MEMBER BLEY: Well, no, actually, this isn't one, but if you have a gate valve and it opens, it only has to get a little bit open before you get nearly full flow through it.

If you have a carefully designed throttle valve, then the flow rate, ignoring the point you just brought up, can be proportional or some other relationship to the position of the valve stem. So, there's a lot of detail here and I don't know if the detail has been worked into the model, that's kind of what I'm asking.

MR. WAGNER: No, we have a simple linear representation of that.

MEMBER BLEY: Okay. Which would be sort of appropriate, if it's a good throttle valve, if that's the design.

MR. WAGNER: Yes, it --

MEMBER BLEY: I don't know that relief valves are designed to do that. They're designed to pop open and then, pop shut.

MR. WAGNER: Yes. Well, we're --

MEMBER BLEY: So, 30 percent open might be damned near full flow, I don't know.

MR. WAGNER: We don't have many samples in the middle there, we're kind of bimodal distribution of weeping or full --

MEMBER BLEY: Well, you got a --

CHAIR STETKAR: But, again, to answer Dennis's question, that's why --

MEMBER BLEY: Something near ten percent chance of 30 percent open.

CHAIR STETKAR: Yes. Well, but --

MEMBER BALLINGER: So, you're satisfied that 30 percent is the right number? I mean, that's an awful lot of really bad screw-ups in calibrating those valves, because those valves have to be -- they're tested quite frequently. It just seems like 30 percent of them --

CHAIR STETKAR: No, this is 30 -- given the fact that it's stuck open --

MEMBER BALLINGER: Oh, okay.

CHAIR STETKAR: -- what is the fraction of the area that it's stuck open at?

MEMBER BALLINGER: Okay.

MR. FULLER: So, I think --

CHAIR STETKAR: It -- what I'm -- my initial question was, how likely is it that it sticks open? And that number, right now, is on the first demand, 2.65 times ten to the minus two, I think, based on 16 failures in -- I've forgotten the -- I don't have the table in front of me here, but --

MEMBER BALLINGER: Okay.

CHAIR STETKAR: -- 16 failures in some couple hundred demands, 600 demands or something like that.

MR. FULLER: So, I think the one -- the reason we gave that ten to 30 percent a little bit more weight than the 30 to 90 percent is, again, we're trying to squeeze as much as we can out of a very small data set.

And in one of the events, we were able to calculate, they had enough information that we could calculate that there was 20 percent flow area that had opened up. So, that kind of said, well, so it does happen, maybe -- so, that was our -- we had that one data point that --

MEMBER BLEY: Just an overview.

MR. FULLER: Yes.

MEMBER BLEY: What you've done is a lot more physically pleasing than 2016 was, which wasn't at all physically pleasing. So, in general, the idea makes sense. I don't know how far to push this. I didn't find much to give me, in what's written, to give me confidence that where you ended up made a lot of sense either. But it's at least in the right direction, I would think.

CHAIR STETKAR: I think that, I mean, we've talked about this a few times, you're clear on our sort of concerns. My personal thought is that this issue, for the purposes of this particular study, ought to be met head-on.

Acknowledge the fact that perhaps you do not have good justification for that failure rate or perhaps even the specific probabilities that's shown for the uncertainty distribution, although the shape of the uncertainty distribution is -- makes a lot of engineering sense.

And that indeed will have implications on the pedigree of, again, I'll call them numbers, I will not call them data, I refuse to do that if nobody can tell me where it came from, on the numbers in NUREG/CR-7037.

And I personally would have absolutely no problem calling into question the fact that they are suspect and that they have not been corroborated as part of this effort. That doesn't say that the overall study is fatally flawed, it just says, use my data, get my results, and you used this data and you got your results.

Going forward -- and we raised this question back, I looked back through my notes, in the Surry study about the valve failure rate. And at that point, we said, well, it wasn't all that very much important, so get over it, it's from 7037.

Anything you do from here on out in the future, ought to, in my personal opinion, recreate a defensible failure rate for those valves. Even if you have to go back and start with a piece of paper that looks like a blank piece of paper and create your new failure rate, because it has to be justified somehow.

MR. FULLER: So, I think we will beef up the discussion in the report. What I did try to say in the executive summary and the conclusions was that, we recognize that what you assume for the safety valve modeling is very critical to the outcomes and that there's still considerable uncertainty in our state of knowledge.

And I think that's an accurate statement. I mean, even if you --

CHAIR STETKAR: It is, Tina, but it -- if I'm a reader of the executive summary, I get these words uncertainty thrown in and I get confused, because what you're talking about is, we have no confidence, I'll be blunt, we have no confidence in the numbers that come out of that NUREG.

We have uncertainty distributions about those numbers that we use and that we talk a lot about, because we do this parametric uncertainty analysis and we went back and revisited the open area fraction and we have an uncertainty distribution.

It gets muddied to a reader of the executive summary, because all you say is, yes, there's uncertainty about what the failure rate should be, in the executive summary. And I say, yes, you have an uncertainty distribution about the failure rate as a parameter in your model.

So, all you're doing, to me, as kind of a dispassionate reader of the executive summary, all you're doing is reinforcing the fact that you looked at uncertainty and that parameter. That's the way I came across, reading the executive summary --

MR. FULLER: Okay.

CHAIR STETKAR: -- rather than saying, look, we used the failure rate from this source, which is the best failure rate that we had at the time. We updated it with additional operating experience, but we still have questions about what the number is.

And that a takeaway from the study is somebody really needs to go back and reexamine the technical bases and operational history for not only the failure rate, but given a failure, what the relative fraction of the stuck-open area is.

MR. FULLER: Yes, okay. Yes. I --

CHAIR STETKAR: That's kind of what -- in the sense of a lessons learned. It isn't -- I'm coming across as kind of condemning the study, and I would condemn the study if it says, these are the data that we are going to hang our hat on, as far as drawing insights and conclusions about risks.

MR. FULLER: Okay. So, I think we are taking your comments again, we'll try to do a better job of framing the discussion. The only other thing I can offer is that, we've had pretty wide review of this at this point, because of all the questions.

We've presented the work at an ASME Pump and Valve Symposium, we've talked to more valve subject matter experts. Nobody has looked at the approach and said, you're doing this totally wrong, there's something a lot better out there. So, we haven't -- I understand your point.

Maybe we have to reframe how we present the work a little bit better, given that there is less confidence in maybe one of the key parameters here. So, again, as I said, every time I see the words safety valve on the paper, I have a deep sense of dissatisfaction.

CHAIR STETKAR: No, it's -- but honestly --

MR. FULLER: It's just kind of where we are.

CHAIR STETKAR: Having collected a ton of data in a previous life, it is absolutely true that there, in many cases, must -- in all cases, engineering judgment is involved, because in some cases, you can't even divine whether something was a failure or not.

So, there's even engineering judgment in how you throw the numerator into a box for the numerator. And very often, very often, there's more engineering judgment in calculating the denominator.

That being said, a responsible data analyst will document the judgment that was used to calculate the denominator. Today, I used the following criteria, based on my knowledge, for these types of events, will result in a safety valve demand. So, that's the type of event.

And then, if the event occurs at Plant X, this is the number of safety valves at Plant X that I included. Now, why is the second part important? Well, if you know something about the plants, for example, Plant X might have five safety valves, Plant Y might have three, typically not all of the safety valves will be demanded.

So, out of a population of five, you might get one or two. So, if I, today, assumed that all five were demanded at Plant X every time I achieved these criteria, I'm probably being very, very optimistic in the denominator.

But it's incumbent on me as a data analyst to document that engineering basis for that denominator. And if I don't do that, nobody should have any confidence in the numbers that I put forth, I have failed as a data analyst. I'm not a valve expert, I'm not somebody who attends a conference, I'm somebody who analyzes experience.

And I may have uncertainty about that denominator, there might be plus or minus a factor of two or three on that, which is fine, I can handle that. Okay?

MR. FULLER: Okay.

CHAIR STETKAR: We've beat that one up enough --

MR. FULLER: Yes.

CHAIR STETKAR: -- we do have to be cognizant of the time here.

MR. FULLER: The final bullet on that slide was just to note that we did add the discussion for why we didn't include the failure-to-open failure mode and hopefully that was convincing. We showed that there is a couple orders of magnitude less probability.

CHAIR STETKAR: I'll only make a statement that I have about a page and a half written up on that. I agree with you that the failure-to-open curve that you show in the report will lie below the failure-to-close curve.

I believe that the margin is considerably less than is shown there, because even the day -- sorry, I almost used that word again, let the record show I said day, not data -- even the numbers in NUREG/CR-7037 for the failure-to-open failure mode give you a higher curve than what you show.

And it's not clear at all to me how you accounted for common-cause failures. Because I -- the curve, I backed out a, if you want to call it an alpha factor, or a beta gamma factor combined, for all three valves of something on the order of couple times ten to the minus five.

And if I look at NUREG, the latest version of whatever the NUREG for common-cause failure parameters for safety valves, this is a -- for that combination of three spring-loaded safety valves failing to open, I get a couple of orders of magnitude higher than that.

I get kind of two or three or four, I've got the numbers here, but it's not worth whining about them right now, higher than that, so that if I apply the individual failure rate from NUREG/CR-7037 and the composite common-cause failure fraction from this other NUREG, I get a couple of orders of magnitude on the failure-to-open on the first demand, if you will.

MR. FULLER: Okay. No, that's a fair point.

CHAIR STETKAR: And that's -- again, the mark, it's still below --

MR. FULLER: Yes.

CHAIR STETKAR: -- and I think, qualitatively, you can still draw that conclusion.

MR. FULLER: Yes.

CHAIR STETKAR: But the curves that are plotted, they're --

MR. FULLER: No, that's --

CHAIR STETKAR: -- a little snarky.

MR. FULLER: That's a fair point, I think we have to discuss the common-cause aspect. So, we'll do that.

CHAIR STETKAR: The only reason I hang up on this is, I have absolute -- what happens, Casey would know this, what happens if none of them open at time -- on the first demand? Things get a lot more interesting, don't they?

MR. WAGNER: Yes.

CHAIR STETKAR: Okay.

MR. FULLER: Yes, so we haven't modeled that lower probability scenario. We did --

CHAIR STETKAR: Right.

MR. FULLER: -- do a sensitivity in Surry, but --

CHAIR STETKAR: Yes.

MR. FULLER: -- we haven't done that sensitivity here.

CHAIR STETKAR: Yes.

MR. FULLER: And you're right, it's a completely different --

CHAIR STETKAR: It's things -- so, for the purpose of this study, I think it's clear that you haven't modeled it. I think the rationale for why you've not modeled it is -- can be punched up a little bit.

MR. FULLER: Okay.

CHAIR STETKAR: Especially because somebody like me is going to go pull those numbers out and do that calculation. Okay.

MR. WAGNER: Do you have any guidance on time? We have about two slides left.

CHAIR STETKAR: We're going to try to get through as quickly as we can here. I'm going to make an executive decision just because --

SPEAKER: Control yourself.

CHAIR STETKAR: I cannot do that. I've tried in the past.

If we have to run a little bit long, we'll run a little bit long, but not much. So, let's try to get through these and I'll try to be good.

MR. WAGNER: The next area that we addressed is time in cycle sampling. And we had a lively discussion on that one, too.

We have a two-part approach to addressing the concerns that were brought up. The first part is we improved the discussion of the distribution between BOC, MOC and EOC in the current documentation. No changes were made, but we improved the documentation, the justification why we went into the three areas.

The second approach is we took that to heart and for the Surry UA we're going to do a much more continuous approach of using many more ORIGEN results across the full cycle.

CHAIR STETKAR: I saw that. I still, you know, it is what it is. The discussion is much improved. I understand the rationale here.

I still don't know why you picked 200 days and then said there must be 50 percent of the probabilities, you know, equally distributed on either side of that 200.

Why you didn't pick 200 and whatever the midpoint is, why you didn't pick 275 days, say, you know, 250 percent, but it is now explained.

Somebody can read through there and say, okay, I might take issue about your selection, but I understand the rationale that you used.

MR. WAGNER: To briefly go over the rationale, more ORIGEN work was done to develop the rationale. We looked at -- we had three points when we presented last time.

We did about 10 or 15 more ORIGEN calculations to kind of put those points in context and give us some insights on the buildup of the short-lived radionuclides.

And so, the -- try to get justification for the BOC space is for that buildup to the secular equilibrium of iodine-131, that was our surrogate for short-lived, and how the decay heat changes over that 60 days.

And then the EOC, the justification on that you can -- we had more data points on the Cesium-

137. And so, that was a definition of why we put an EOC boundary there.

And then the sampling looked -- the particular value that was used, looked at load decay heat. So --

CHAIR STETKAR: No, the story hangs together much, much better.

MR. WAGNER: Next slide.

And a question was on the stability -- statistical stability analysis conducted for the figures of merit. That was performed and that was added to Appendix A.

We recognized the possible impact of eutectic melt temperature on burnup. We didn't change anything, but we have some --

MEMBER POWERS: Since there is no eutectic in the system, I really wonder what that is. There's a monotectic.

MR. WAGNER: We use eutectic as our conglomerate melt and sort of dissociation temperature for the field.

MEMBER BALLINGER: In that case, eutectic should be put in quotes?

MR. WAGNER: Oh, sure. Yeah. Yeah.

MEMBER BALLINGER: Okay.

MEMBER REMPE: Just to follow up on the earlier discussion very briefly, I'm looking at a paper from IRSN and Karlsruhe, Comparison of Core Degradation Phenomena for Various Experiments.

There is a quote in there that clearly it's referring to some PHEBUS data and it has "irradiated fuel is dissolved more quickly than unirradiated fuel." It cites the PHEBUS-FP tests.

And in that whole paragraph they're comparing differences between irradiated fuel and temperatures versus unirradiated fuel. And so, again, it might be a good place to start looking for data.

MR. WAGNER: Yeah, took some notes on that. Thank you.

There was some added discussion on our approach for failed computer runs. It is already noted that we --

CHAIR STETKAR: Yes, there was. We've discussed that. Next bullet.

MR. WAGNER: I wanted to go on. There was some discussion of ice bed response during seismic event. Improved vessel failure discussion to address the cited issues.

MEMBER REMPE: So, again, a nitpick, but

it -- the text there needs -- first of all, what is "gross failure"? It's better than variable failure, but you have the word "gross failure."

To me, that implies it unzipped and the whole head fell off. I don't think that's what you assumed. You assumed a limited area, specific area, right? And so, find some nicer words that are more representative of what was done.

Also, there's another place where it used to talk about the drain line and John harangued you to get rid of that. And now it has something about that -- about penetration -- instrument failures or other failures. And you have in quotes, "instrument failures."

And so, look at the words there and just fix it, because it's -- you're having "instrument" both times and it's just --

MR. WAGNER: Oh, I see. Okay.

MEMBER REMPE: It's an editing thing. But I look for those things, because we have had some comments about it.

MR. WAGNER: Yeah. Yeah. The Committee pointed out that there was some confusing discussion on the long-term station blackout on specific generators versus symmetric and asymmetric responses.

And I tried to rewrite that to address that.

CHAIR STETKAR: That reads -- there's still obviously a numbering difference, but it's okay now.

MR. WAGNER: Great.

CHAIR STETKAR: As far as I'm concerned, it states what's done.

MR. WAGNER: Okay. Great.

MEMBER REMPE: Could I go back to the very beginning since it has the phrase "steam generator," or the acronym there, about our discussion earlier about the limited scope and not considering consequential steam generator tube rupture.

And you did consider countercurrent flows, so I bet your MELCOR runs actually have temperatures of the hot leg and temperatures of the tubes in it.

And so, again, my earlier point where I was slapped down because I was saying, can you use some insights based on other evaluations you've done so that up front you -- you're basically looking at different pump seal leakage rates and you're making comments about releases into the containment and things like that.

I bet you could even get those insights that are plant-specific based on the runs you have on

-- and infer that the temperatures of this tube versus the temperature of the hot legs, you might be able to come up with a conclusion about what would have been more likely or not, even.

And it would be plant-specific; right?

MR. WAGNER: Uh-huh. Yeah. I've noticed that when I've implemented the natural circulation modeling, three loop versus four loop. There are some differences.

MEMBER REMPE: Okay. Thank you.

MR. WAGNER: Oh, we looked at RCP leakage as an ignition source, the gases that might be coming out of the RCP and added a discussion.

CHAIR STETKAR: That's an interesting discussion. And that's all I'm going to say. That's an interesting discussion.

I have some questions about that, but we don't have enough time for it right now. There were a couple of curiosities that I --

MR. WAGNER: Okay.

CHAIR STETKAR: But it's an interesting discussion.

MR. WAGNER: The very last slide is just to point out that we added two things that we talked to you about. Hossein's discussion from last meeting got into the report, and then Trey's focused pressurizer safety valve study is now fully documented in the report.

On to MACCS.

MR. BIXLER: Okay. On the -- there were several comments also in the MACCS, the consequence analysis portion of the work from last time.

And I think I picked the more major things here and tried to respond to those and explain what we have done to address them.

The first thing here is a question on emergency response and triggering by -- in our case, we had some emergency response triggered by SAE siren. And there was a question, what is that? Does that really happen? Is that what TEMA would do?

And so, we went back and looked at that. And I think we offered at the time of the last meeting, that we would take a more careful look at that.

And what we found, we found a couple of things. One is that there's a documentation by FEMA in 2004, of an exercise for the Sequoyah Nuclear Power Plant that shows pretty clearly a time line of events.

And one of the earlier things on the time line is the SAE declaration. And then following that is a first siren.

And it notes there that that would trigger school relocation, that buses would be -- bus drivers would be notified to deploy, et cetera. A little bit more is included on the first SAE -- or the SAE message -- the EAS messaging.

And then following that, at a later time is the GE declaration. And then following that is -- it clearly specifies a second siren and a second EAS message.

So, that definitely corroborates what we were modeling the way we had modeled schools and some of the other cohorts that were also related to that.

MEMBER SKILLMAN: Nate.

MR. BIXLER: Yeah.

MEMBER SKILLMAN: This is Dick Skillman.

MR. BIXLER: Yeah.

MEMBER SKILLMAN: I asked that question.

MR. BIXLER: Yeah.

MEMBER SKILLMAN: And I asked that question, because the gentleman from -- when challenged, the gentleman from TVA said, "Yes, we really do move the children at a site versus a general," which is where, at least in my experience, the bulk of the evacuations occur.

MR. BIXLER: Uh-huh.

MEMBER SKILLMAN: And this has specifically to do with Cohort 2.

MR. BIXLER: Yes.

MEMBER SKILLMAN: And this is actually 20 percent of the population --

MR. BIXLER: Yes.

MEMBER SKILLMAN: -- of the area.

MR. BIXLER: Yeah.

MEMBER SKILLMAN: But what's interesting, and I'm going to go back to what either John or Dennis said, the study is a specific study for this site. And the study for this site is tied very closely to TEMA, to how Tennessee Emergency Management and TVA have agreed in their emergency plan.

Because what they do is at an alert, they dispatch the buses. So, the buses are staged before the site occurs, if the timing is --

MR. BIXLER: Right.

MEMBER SKILLMAN: So, from that perspective, one must be aware that this is not, for instance, applicable to McGuire, Catawba, TMI, Oyster Creek --

MR. BIXLER: Okay.

MEMBER SKILLMAN: -- pick your plant.

MR. BIXLER: Yeah. Okay.

MEMBER SKILLMAN: Because this is, at least in my experience, unusual that an alert -- actually, the control room calls the school.

MR. BIXLER: Uh-huh.

MEMBER SKILLMAN: That's what the gentleman from TVA said.

MR. BIXLER: Okay.

MEMBER SKILLMAN: So, I mean, the analysis is what it is if that agreement is as stated. I have no reason to believe it's not --

MR. BIXLER: Right.

MEMBER SKILLMAN: -- but one must be clear in one's own mind to recognize this is different.

MR. BIXLER: Okay. Yeah, and we -- I don't think we tried to draw any conclusions for other plants, just for this one.

MEMBER SKILLMAN: But this is 20 percent of the population.

MR. BIXLER: That's right.

MEMBER SKILLMAN: So, this is not an inconsequential --

MR. BIXLER: Yeah.

MEMBER SKILLMAN: -- uniqueness here.

MR. BIXLER: Yeah.

MEMBER SKILLMAN: This is important.

MR. BIXLER: Yeah. Okay.

MEMBER SKILLMAN: Thank you.

MR. BIXLER; And I think I can probably conclude with that. One additional thing I'd like to say, though, is that even for this case where we -- and we believe faithfully triggered the beginning of the emergency response by SAE, it's still fairly late.

It doesn't happen early on the overall time line, because we assume that there'd be quite a bit of delay for the buses to be able to get to the schools.

And so, it didn't end up being a particularly early evacuation of the school kids, it was just triggered earlier. It started the chain of events earlier, but it still took a while to accomplish by our time line.

MEMBER SKILLMAN: And one probably should recognize that there could be hours or minutes between the site and the general.

MR. BIXLER: Yeah. Yeah.

MEMBER SKILLMAN: And so, that's really the important piece here. When you tell the children "Go" at a site, they may be in the next county by the time their parents are told to leave.

So, there are some practical implications associated with this assumption.

MR. BIXLER: Yeah.

MEMBER SKILLMAN: Thank you.

MEMBER BLEY: Nate.

MR. BIXLER: Yeah.

MEMBER BLEY: There was one thing I think we talked about the last time, and I wonder if you folks thought more about it.

You do have an assumption that bridges within the ten miles are unusable --

MR. BIXLER: Right.

MEMBER BLEY: -- and you shoulder people until you find a route out for them, but I think we asked if -- what about the people who don't wait for that and who jump out on the roads and then find themselves cut off?

I know that area kind of well and there's a lot of bridges in a lot of place nearby there.

MR. BIXLER: Yeah.

MEMBER BLEY: Do you think about people who get stranded for a fair amount of time because they can't find their way back out again when they keep running into dead bridges?

MR. BIXLER: We did --

MEMBER BLEY: Did you leave a fraction of those who get caught outside rather than sitting inside?

MR. BIXLER: I think we ended up doubling the amount of evacuation time to account for the fact that people might reach dead ends and then have to retrace their steps and try something else.

MEMBER BLEY: Okay.

MR. BIXLER: So, we did try to account for that in our evacuation time line, hopefully, in a reasonable way.

MEMBER BLEY: Okay. Yeah, I guess that makes sense.

MR. BIXLER: Yeah.

MEMBER BLEY: Thank you.

MR. BIXLER: Okay. I think -- next slide.

Okay. There's also a question about how MACCS treats deposition under humid conditions, which I imagine you're likely to get in the Tennessee Valley area there. And that was a good question.

And at the time, we responded that that would have an impact -- in principal, it could have an impact, at least, if you have hygroscopic aerosols being released that are not fully saturated with water. They could grow to a greater size, deposit faster, et cetera.

That's something we don't specifically treat in MACCS at this point. There's no accounting for relative humidity in the area when you're doing a release.

But what we ended up doing in response to that, was to do a bounding estimate of how much difference that could make on the deposition velocity of the aerosols.

And I think that's reasonably well-documented now in the new version of the document, but what we ended up finding is that it wouldn't even be a factor of 2 in deposition velocity.

And our uncertainty range covered a range of ten plus or minus the square root of three -- or square root of ten, roughly a factor of 3 up and down.

So, I think we covered that uncertainty already in our uncertainty range, but it is -- it's something that we thought was worth adding some additional description/discussion in the document. So, we went ahead and did that.

Okay. Next slide. And last comment -- or set of comments was on the presentation of some of the risks that were in the -- with the earlier version of the document, both EF, early fatality, and LCF risks.

So, I basically agree that at the time that we met last time in June, that we would go back and reconsider that and add to the discussion. And so, we've done that now.

There's a specific discussion on the bimodal nature of the LCF risk and what that stems from and we reduced the amount of presentation on EF risk.

Basically, there used to be a table and a figure there that we eliminated and now there's just a minimal amount of discussion saying that it only occurs in a few of the realizations that we had.

And even there it's trivial overall that there's almost no chance of an early fatality. So, those are the things we did to address the comments from last time.

CHAIR STETKAR: I commented earlier on the early fatality. There's still a tabulation in there that shows the mean value and zeroes for all the other percentiles.

I think -- again, I think a tutorial will help.

MR. BIXLER: That might be in the appendix, are you thinking, or --

CHAIR STETKAR: I don't -- no, there's one in the appendix, but there's -- the one in the appendix has mean values and 95th percentiles, as Trey showed --

MR. BIXLER: Right.

CHAIR STETKAR: -- for four of the five distances that indeed are higher than the mean value, which is reassuring to a lot of people.

There is a table in the main body of the report and I, you know, if you want me to --

MR. BIXLER: In Chapter 6?

CHAIR STETKAR: I don't remember whether it's Chapter 6.

MR. BIXLER: Okay.

CHAIR STETKAR: It probably is. I think that does show non-zero mean values and zero values for the median, fifth and 95th percentiles.

MR. BIXLER: I think the intention was to take that table out.

CHAIR STETKAR: Well, it's still in there and it -- you can argue one way or the other about whether it's useful. It might be useful.

If it's in there, as I said earlier, I think that it would be very useful to have a bit of a tutorial for folks --

MR. BIXLER: Okay.

CHAIR STETKAR: -- about how that type of behavior makes sense --

MR. BIXLER: Right.

CHAIR STETKAR: -- because most people don't get it. And most people -- what I've found is many people do not support the notion of quantitative uncertainty analysis and are too easy to dismiss it as simply an abstract mathematical, statistical, nonphysical exercise that you push a button at the end of a study and something comes out.

So, I think that table is actually useful. It shows that the numbers are small --

MR. BIXLER: Okay.

CHAIR STETKAR: -- but it needs a bit of discussion.

Now, regarding the bimodal nature of the distribution, in the executive summary, the notion of a bimodal distribution is introduced. You show the cumulative curves. You still have this notion that risk decreases as a function of distance from the site, which is not entirely true.

The discussion in Chapter 6 is much, much better. You essentially - now, remember an executive summary has to be written not for people who did the work. It has to be written for people who want to understand what you did and what you're trying to convey with the results so that when you say a bimodal distribution, most people won't understand what the heck that is even from the shape of the cumulative. They just won't.

If you say, well, if we turn up the microscope on this funny-looking thing and show what's shown in Chapter 6, and then say, well, look, for the 80 percent -- 80, whatever the heck it is, seven percent of the time when we get an early containment failure; A, it's 87 percent of the time and here's what the risk -- conditional risk profile looks at for that 87 percent. And, look, this one's from the site, doesn't make any difference.

But the other 13 percent when you basically don't get a containment failure and have a very small, protracted release, the risk from that is really, really, really small. And in that case, distance from the site makes a big difference.

MR. BIXLER: Right.

CHAIR STETKAR: And I think telling that story that way in the executive summary, to most people who read it, is a lot more beneficial than just the words that are in there and the figures, because people won't get those figures.

MR. BIXLER: Okay.

CHAIR STETKAR: And the same way as publishing the results as a range from something on the order of ten to the minus ninth to something on the order of ten to the minus three is misleading without that corroborating story.

MR. BIXLER: Okay.

CHAIR STETKAR: Especially in the executive summary.

MR. BIXLER: Okay. All right. We'll take that comment to heart and go back and reword that a little better.

CHAIR STETKAR: Because it's really -- I think to a lot of people it's unexpected. That shape is unexpected and the fact that that for in this particular site, in this particular study, distance from the site for the vast majority of the things that you're looking at doesn't make any difference is also counterintuitive to a lot of people.

MR. BIXLER: Okay.

MS. SANTIAGO: So, that brings us back to talking perhaps about the topics that we should focus on for the November 2nd full committee presentation.

MS. GHOSH: Yeah. We didn't have any slides on this, because basically we're soliciting your input on what we should prepare and focus on.

CHAIR STETKAR: Well, in my opinion, I think recognize that there are some members of the Committee who I believe have not heard any of this story.

I've tried to go back and see who attended what meetings, but let's go on the presumption that there are at least one or more members of the Committee who have heard nothing of this.

That being said, I think that you need to provide an overview of the objectives of the study. The fact that it was tailored, in a sense, to examine hydrogen issues focusing primarily on early containment failure, I would not dwell too much on the long-term station blackout despite the fact that it's part of the study.

I think you don't have enough time to draw that distinction. I would keep focused on the short-term station blackout exclusively and said that's the -- that's where we did all of our analysis, that's where the integrated uncertainty analysis was done and we just don't have enough time to kind of talk about the others -- the other part of it.

I would show the -- certainly show the results and explain the results, the bimodal effect of the results, you know, where they're coming from so that other members can understand, you know, essentially the sources of risk and the kind of phenomena that are contributing to them.

And then certainly, you know, in my opinion, and I'll ask for other help here, I would grapple -- I would grasp the demon by the horns and say, you know, what are some of the lessons that we've learned from doing this study?

I think one of the lessons that we learned is that the -- paying careful attention to the uncertainties as you do the study rather than as an afterthought is; A, essential, and; B, can have a measurable effect on the results.

You may want to show a couple of examples of that in things like the open area fraction and the fabric seal type of things, but only as an illustration.

You don't want to get bogged down in 90 minutes in a full committee discussion with people who don't understand how the machine works and don't understand the study in terms of detail.

So, I' m not sure, you know, think about that, but I think part of the message is that the uncertainty analysis is not an afterthought and it's important that it's not an afterthought.

I would -- this is -- address the issues that we have discussed, the non-completion of MELCOR, the concerns about the pedigree of the numbers for the valve failure rate and, you know, you've already established the fact that that's an important parameter in the study. And perhaps one or two other issues, you know, that we've discussed.

I think that -- here's one area, and think about this, again, it's -- you're going to have to put together a lot of material in a 90-minute presentation.

A lot of the really elegant stuff in this study, both in terms of the modeling and how the sampling algorithms are done, is really, really neat stuff.

And all of the stuff that you did on regression analyses to kind of ferret out important contributors and how they may vary, are really, really interesting.

I don't know how you jam that into 90 minutes or even whether you try. And that's where I'm looking for -- oh, it's a lot of really neat stuff, but I'm not sure whether it's worth trying to get at.

MEMBER BLEY: Well, what do you want from the ACRS? I think that's what you ought to focus on. And anything that you really like, but isn't related to what you want from the Committee, you ought to put aside.

For example, you've done a lot of work recently on this focus safety valve study. I would put most of that aside.

On something that John talked about, I might go a little further, because when -- I like most of the things you talked about, John, but I think you ought to really narrow it down to the things that will -- what did you learn, what's good?

Now, the one thing I hoped you learned was when we started going with you through the uncertainty analysis and said, gee, everything is pointing to this open fraction of a safety valve, all the important stuff was sitting there that led you to go back and say, "Did we really do that right," and reevaluate it, I think that's an important story to tell.

Great details about what you actually did, I don't think, is that important, but that you learned from the uncertainty analysis that some part of your modeling was a lot more important than you probably figured it would be when you started.

I might be wrong about that, but that

would be my guess. So, that would be the one thing I would focus on a little.

MEMBER SKILLMAN: I'd like to offer a comment, if I could. The major take-away for me is the uniqueness of the applicability of this study to this plant.

John said it very well a couple hours ago. One should not paint all ice containments with this result.

McGuire and Catawba or Watts Bar, because of their site and because of their emergency plan, might have a very different outcome.

And so, in the abstract and in your -- what will become your executive summary, I think it's very important to communicate the unique applicability of this specific study for this specific plant.

That doesn't take away from the potential use of some of the results, but the final conclusions are unique and the users should be advised don't be so hasty just to apply this anywhere.

And if that caveat isn't in the Peach and the Surry analyses, it probably ought to be because these are very highly-specific, complicated analyses. It's a unique piece of -- it's a huge piece of work, excellent piece of work, but it's unique.

CHAIR STETKAR: Any other suggestions as far as the full committee presentation from our members?

MEMBER MARCH-LEUBA: Yes. For my benefit, I would like to hear something about why we're doing this and -- let me rephrase the question in a more final way.

Have you guys worked ourselves out of a job? the moment you write the report, we can fire you because we're never going to use this again. Or what are we going to use -- what benefit does the Agency gain from this exercise? How are we going to apply it?

Is it going to apply to licensing? Tell me why we did this.

CHAIR STETKAR: Okay. Be careful about that, because you haven't been here for the last decade. The --

MEMBER MARCH-LEUBA: More reason to tell me why we did this.

CHAIR STETKAR: Okay.

MEMBER CORRADINI: But I think Jose's point is fair. Ten years ago, or thereabouts, this was done for a particular reason. But now that you've done it, how do you use it going forward? I think that's what Jose is asking.

MEMBER BROWN: I would -- can I -- since I'm uninitiated and I've been here for nine years and I've sat in on several of these meetings, but not all --

MEMBER BLEY: Started the year before you joined us.

MEMBER BROWN: I know.

MEMBER BLEY: You're no longer uninitiated.

MEMBER BROWN: Well, after the first couple of meetings, I decided I would be more selective in the meetings that I attended.

(Laughter.)

MEMBER BROWN: I phrase that very succinctly. And I agree with Jose in that I think it's important since this is kind of the end product, to say why we did this and what are the conclusions we should gather from this in terms of how we use the results.

The ones I've sat in on hydrogen deflagration and stuff like that, how does it apply to the safety posture of the plants? How can it be generally applied, if at all, to plants other than the ice, you know, this specific plant?

Because, otherwise, if you go through a bunch of stuff where you show graphs and lots of ten to the minus seventh, eighth and ninth, it's just -- it's a sleeper. You're going to fall asleep.

You'd really like -- at least I would, you really want to know what conclusions do we draw out of this in terms of is this a big, big problem, safety posture, because it's just going to spread all over the place, or did we get -- were we able to come look at the results and say, look, yeah, this happens, but here's the general configuration or the impact at the site or the general areas, because that's -- it's the safety posture that counts, not the fun and games of playing with titivating models.

That's my personal opinion in terms of what ought to come out of the final presentation at the end. That's all I have, John.

CHAIR STETKAR: And something to keep in mind kind of along these lines of why did you do it, what are you going to do with it, etc.

The executive summary does touch on the fact that some of this, a good fraction of it, is developing methods, models and tools that can be used throughout the Agency in many other applications. So, it isn't just it, the study, and developing expertise within house on those.

And I think that the full committee --

MEMBER MARCH-LEUBA: We need to answer the question now.

CHAIR STETKAR: No, but, I mean, I wanted to kind of alert them that it isn't just it, this thing.

MS. SANTIAGO: Right. We've talked about that.

CHAIR STETKAR: Yeah.

MS. SANTIAGO: And it's a good point.

CHAIR STETKAR: Anything else on stuff for the full committee presentation? You have a challenge, clearly.

If not, I will ask if there are any members of the public in the room who would like to make comments. Please come up to the microphone and do so.

And not seeing a stampede, I'll ask if there are any members of the public on the bridge line who would like to make a comment. Please do so.

The bridge line should be open. Please identify yourself and make a comment.

(Pause.)

CHAIR STETKAR: That's a good indication

the bridge line is open. So, not hearing anyone, if there's anyone out there, again, please make a comment. Okay. We'll get that reclosed again.

As we always do in a subcommittee meeting, I'll go around the table and ask for final comments by each of the members.

Ron.

MEMBER BALLINGER: Well, I think we've pretty much beat the dead horse in understanding what went wrong with the computer runs and things like that. So, that would be my only comment.

CHAIR STETKAR: Matt.

MEMBER SUNSERI: I have no other comments. Thank you.

CHAIR STETKAR: Dick.

MEMBER SKILLMAN: No further comments. Thank you.

CHAIR STETKAR: Dana.

MEMBER POWERS: I have an organizational conflict of interest here, so I don't make comments.

CHAIR STETKAR: I got to remember that.

MEMBER POWERS: I don't believe in eutectics and the uranium-zirconium oxygen system based on a huge number of experiments.

MEMBER CORRADINI: I just thank the staff.

I do think, though, I guess my only emphasis would be I would treat the other four or five members that aren't here as your first shot at the intelligent public.

So, cast it in that regard for them, because you'll get "why" questions and what-do-you-use-it-for questions from some of the other members. And I think that's kind of how I would at least try to -- at least customize the beginning of your couple of hours in front of us in full committee.

Other than that, I appreciate all that you're trying to do to educate us for those that forget what you've done in the past since it has been a few years.

CHAIR STETKAR: Dennis.

MEMBER BLEY: Nothing more to add. Thanks.

CHAIR STETKAR: Jose.

MEMBER MARCH-LEUBA: Nothing.

CHAIR STETKAR: Charlie.

MEMBER BROWN: Nothing more.

CHAIR STETKAR: Joy.

MEMBER REMPE: I also want to thank you for your presentations and your efforts. I guess the only point I'd like to emphasize is as you finalize the executive summary, please consider caveats where needed when you have conclusions and statements.

And I think I've tried to emphasize a couple of examples today. Thank you.

CHAIR STETKAR: Now, as part of a -- let me also say thanks. Thanks for all of the work that you did between May or June or whenever and now. A tremendous amount of stuff has been done.

I think the report benefits from it. I think the understanding benefits from it. Although we probably won't discuss it, I like Appendix I. I think it's a confidence builder. And thanks for all of the work.

I was going to say -- oh, the ACRS will write a letter on this. The ACRS letter will be written against the material we have and today. So, there will be no update to the executive summary to effect an ACRS letter or anything.

If you want to send us a little side report to bolster confidence in the numbers for the valves, that's fine, but we're not going to entertain -- we can't because of our scheduling. So, we're going to write a report on what we have and today.

I would ask, and I don't know how we're going to do this given our other constraints, I really want some input from the individual members on what might go into a draft of our letter. And let's discuss that offline.

With that, the meeting is adjourned.

(Whereupon, the above-entitled matter went off the record at 12:07 p.m.)

State-of-the-Art Reactor Consequence Analyses (SOARCA) Project: Sequoyah Integrated Deterministic and Uncertainty Analyses

ACRS Subcommittee Briefing
October 18, 2017

Patricia A Santiago, Chief
Accident Analysis Branch
Division of Systems Analysis
NRC Office of Nuclear Regulatory Research

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Schedule

- ACRS Full Committee on SOARCA Sequoyah Analyses – November 2, 2017
- Transmit SOARCA Sequoyah NUREG/CR report to the Commission and NRC publications – November 30, 2017
- Updating SOARCA Surry Uncertainty Analysis – ongoing – June 29, 2018

Core Team Members

- MELCOR and severe accident progression: Kyle Ross, Jeff Cardoni, Chris Faucett, Troy Haskin, Randy Gauntt (SNL); Casey Wagner (dycoda); Hossein Esmaili, Trey Hathaway, Allen Notafrancesco, Salman Haq, Ed Fuller (NRC)
- MelMACCS: Nathan Bixler, Doug Osborn** (SNL); Trey Hathaway (NRC)
- MACCS, consequence analysis and emergency response: Nathan Bixler, Matthew Dennis, Joe Jones, Doug Osborn**, Fotini Walton (SNL); Trey Hathaway, Jonathan Barr, Keith Compton, Todd Smith, Edward Roach (NRC);
- UA methodology: Dusty Brooks, Matthew Denman (SNL); Tina Ghosh**, Trey Hathaway (NRC)
- Accident scenario development: Selim Sancaktar, Jose Pires (NRC)

***Co-leads*

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Outline

- Focused pressurizer safety valve study
  - New Appendix I in draft report
- Updates in response to ACRS members' comments from June 6, 2017 subcommittee meeting
- Discussion of topics for presentation to full committee

Focused Pressurizer Safety Valve Study

Trey Hathaway
Accident Analysis Branch
NRC Office of Nuclear Regulatory Research

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Focused Safety Valve Study

- Updated Sequoyah model run with sampled safety valve cycles less than 65 (13% of distribution) and open area fraction greater than 0.3 (40% of distribution)
- Most parameter distributions identical to UA
  - Safety valve distribution constructed from sampled values of UA and sampled on range of 1 65
  - Open area fraction distribution sampled with lower bound of 0.3
- Calculation initially limited to 15 hours
  - All early failures were less than 1 hr
- Identical model to UA
  - Sampled fabric seal failure pressure input corrected

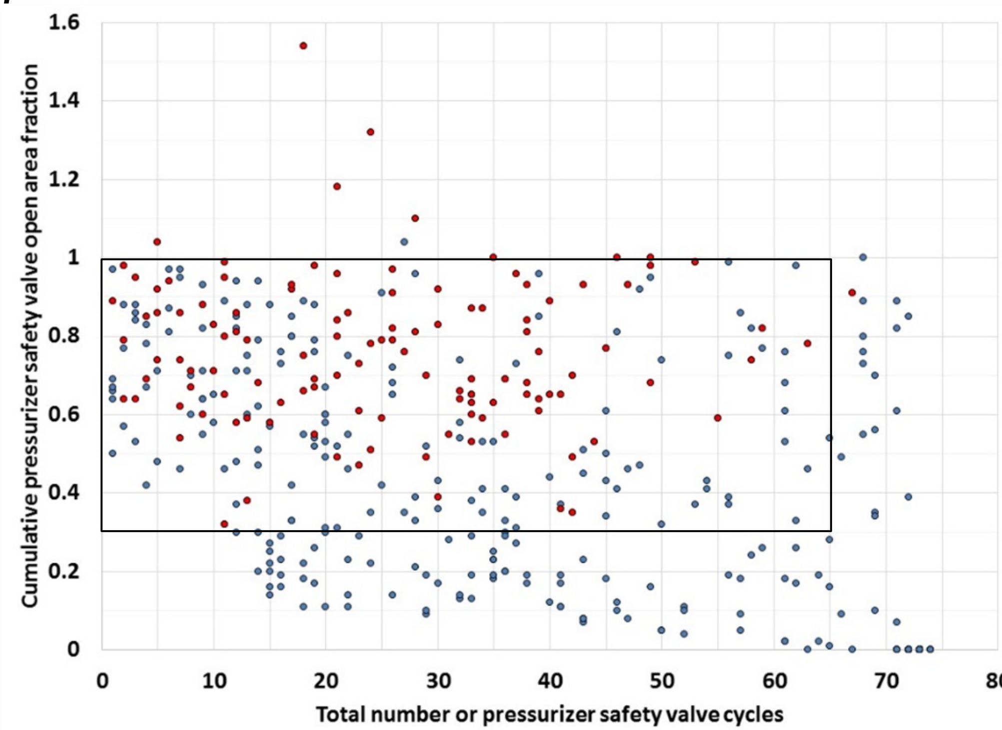


Figure I-1 Cumulative pressurizer safety valve open area fraction versus total number of safety valve cycles, where early containment failures are shown in red**

** NOTE: This data was taken from the draft 2016 Sequoyah SOARCA analysis (ADAMS Accession No ML16096A374)

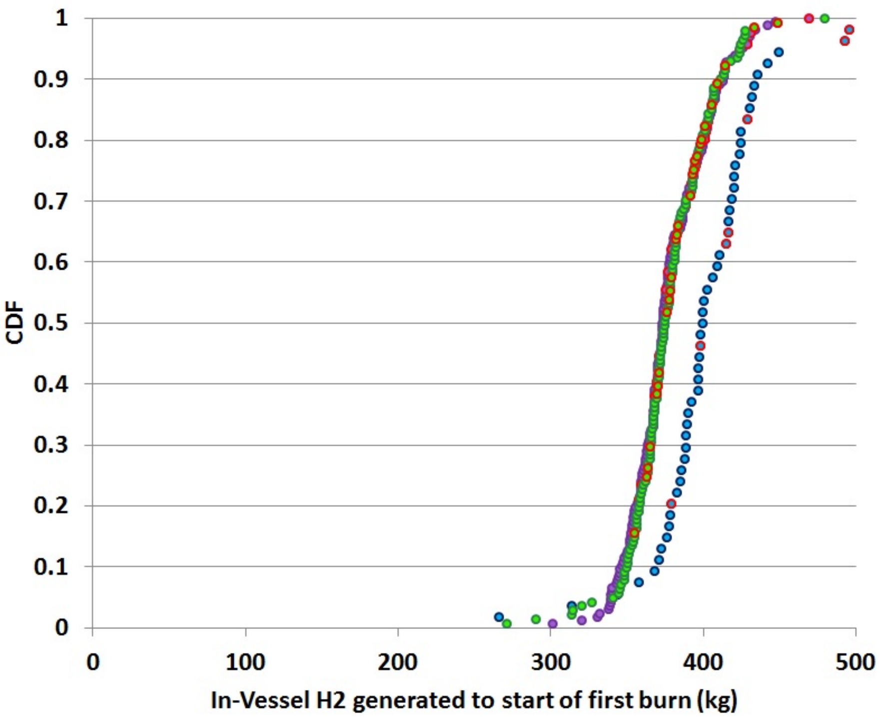
Early Containment Failure

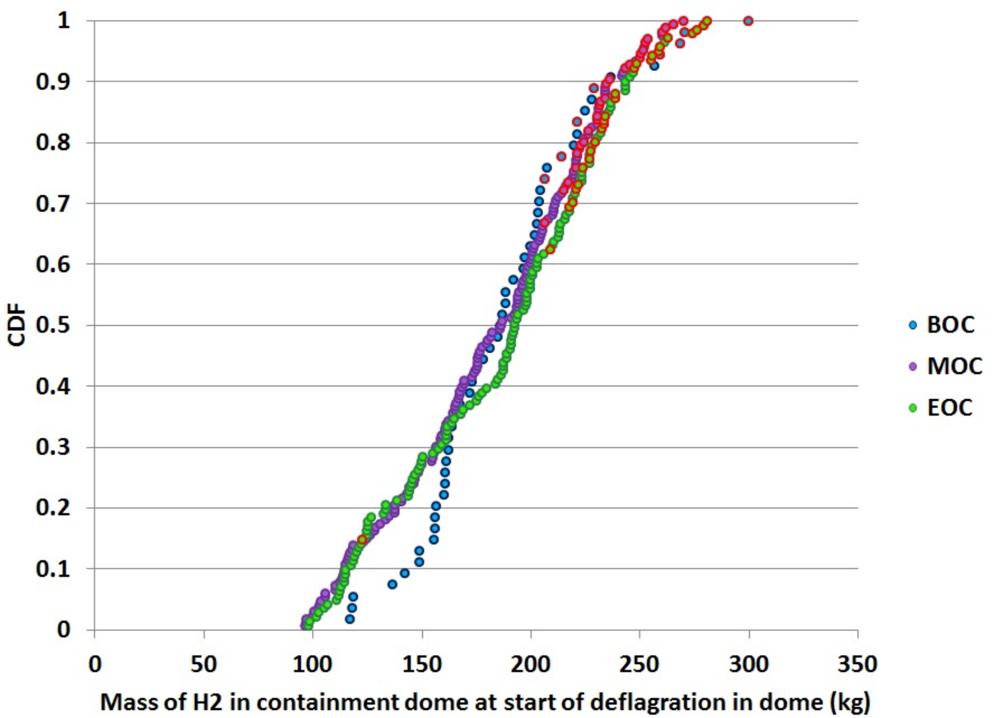
- Approximately 17% of the realizations in focused study resulted in early containment failure
  - BOC: ~15%
  - MOC: ~16%
  - EOC: ~19%

Table I-1 Statistics on time (hours) of early containment rupture for BOC, MOC, and EOC realizations.

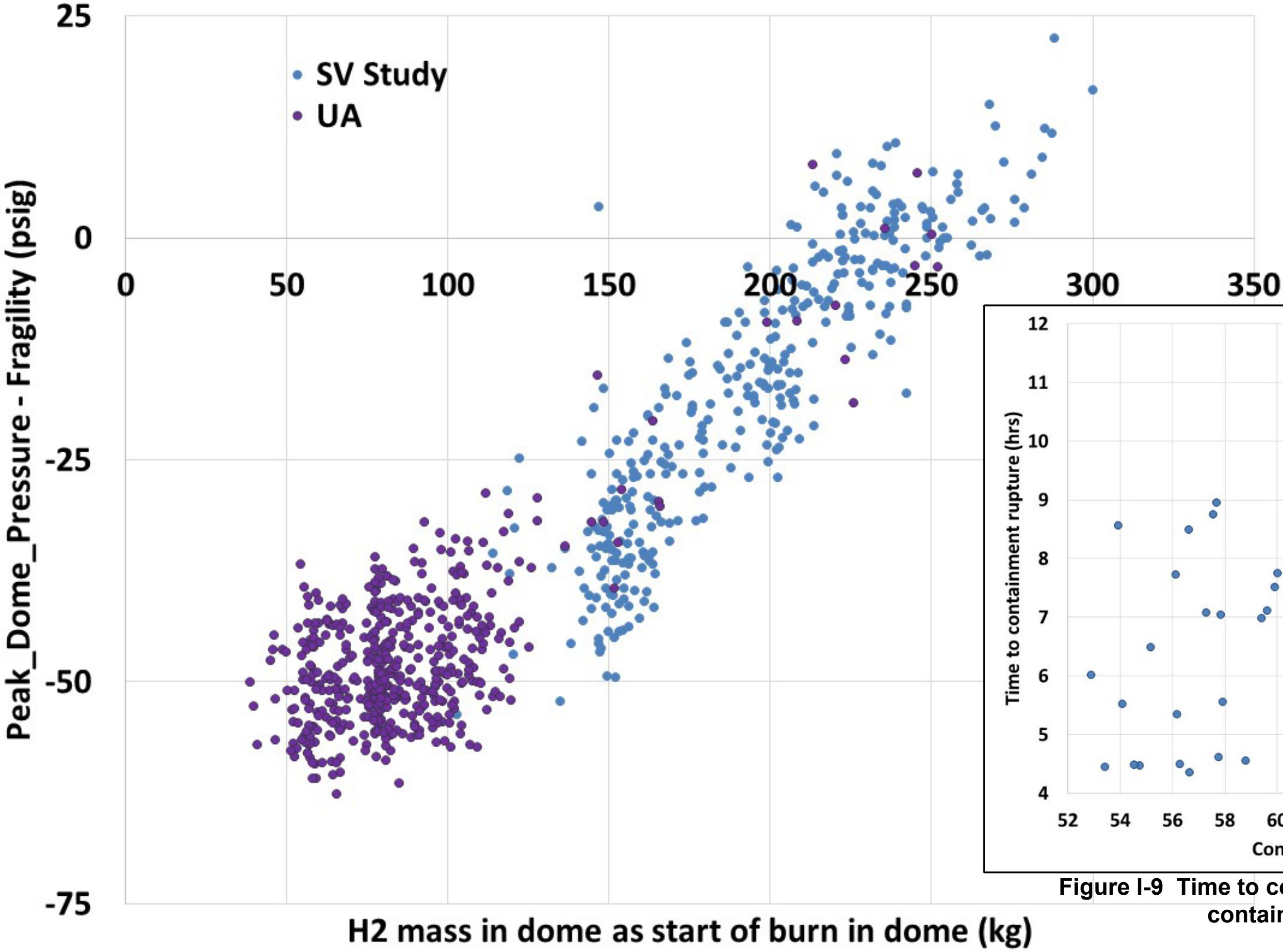
	BOC	MOC	EOC
Mean	6.0	6.6	6.6
Median	6.0	6.9	6.8
5th-Percentile	5.5	4.3	4.5
95th-Percentile	6.3	10.8	8.8

In-vessel Hydrogen Generation and Transport to Dome Up to First Deflagration





Hydrogen Deflagration Peak Pressure



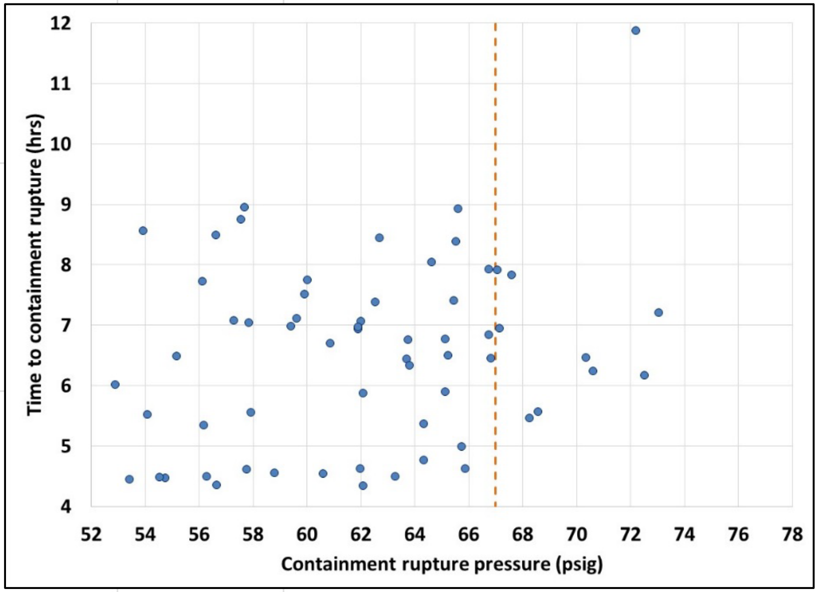


Figure I-9 Time to containment failure plotted against the containment rupture pressure.

Figure I-13 Difference between peak containment pressure and sampled containment fragility plotted against maximum hydrogen reaching the dome around the time of burn initiation in the dome.

Cesium and Iodine Release Fraction

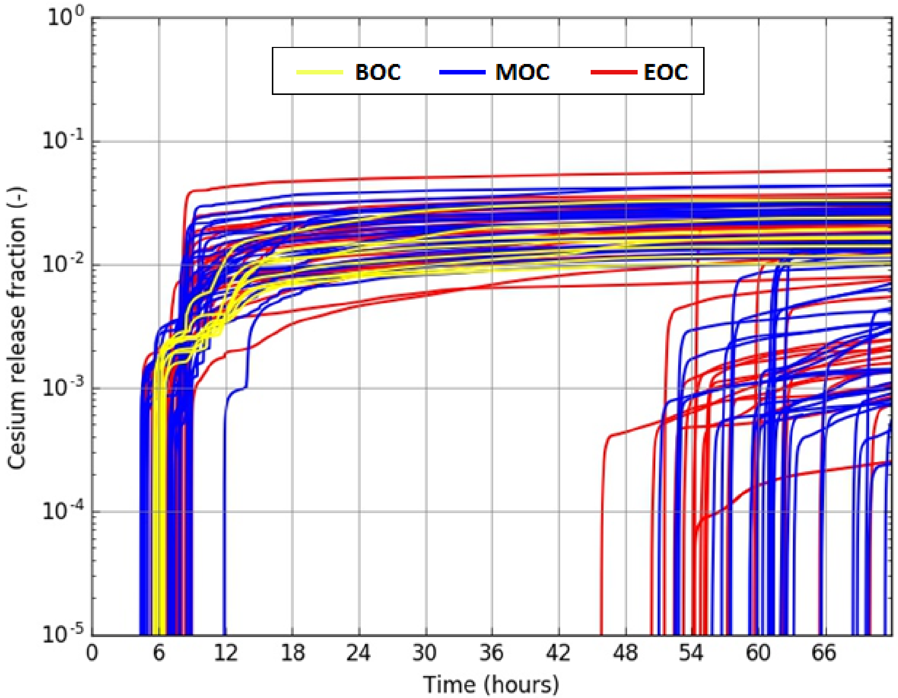


Figure I-14 Cesium release fraction versus time.

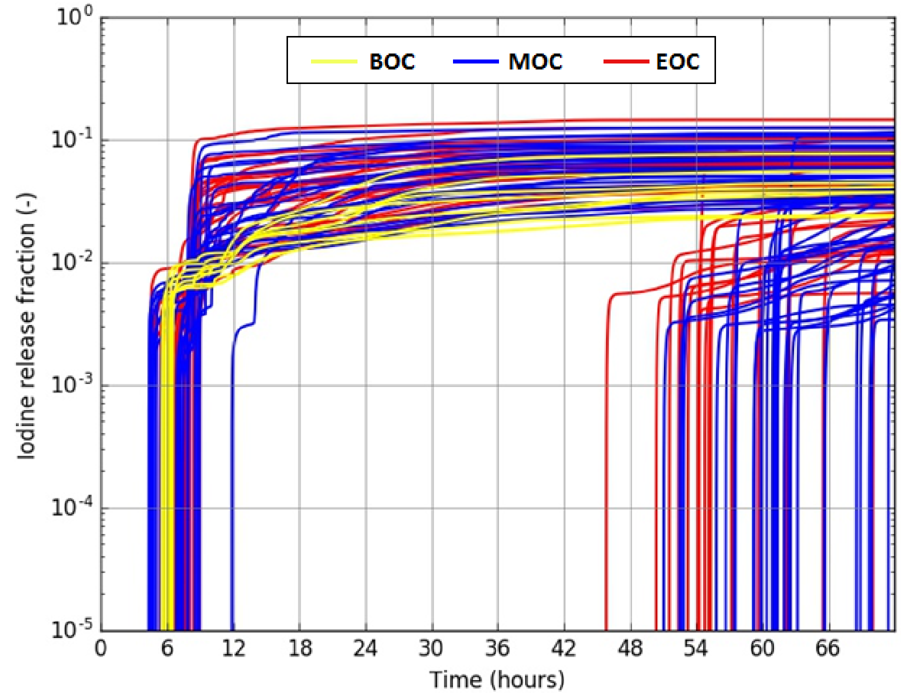


Figure I-15 Iodine release fraction versus time.

Table I-10 Statistics on Cesium and Iodine release fraction at 72 hours for the early and late containment rupture realizations.

	Early Containment Rupture		Late Containment Rupture	
	Cs release	I release	Cs release	I release
Mean	0.022	0.063	0.004	0.021
Median	0.022	0.06	0.002	0.014
5th Percentile	0.01	0.028	0.0003	0.004
95th Percentile	0.042	0.122	0.015	0.084

Latent Cancer Fatality Risk

Statistics on Total Risk

Table I-11 Statistics for the 0-10 mile individual LCF risk assuming LNT and conditional on the occurrence of a STSBO.

	Early Containment Rupture	Late Containment Rupture
Mean	7.0E-04	8.3E-05
Median	5.8E-04	6.7E-05
5th Percentile	2.1E-04	3.2E-09
95th Percentile	1.5E-03	2.2E-04

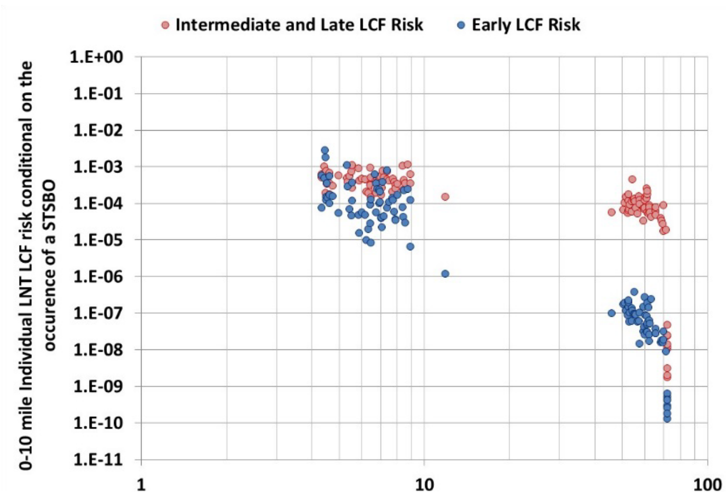


Figure I-18 0-10 mile Individual LNT LCF risk conditional on the occurrence of a STSBO as a function of containment rupture time.

Statistics on Emergency and Intermediate/Long-Term Risk

Table I-12 Statistics for the emergency phase, and intermediate and long-term phase contributions to the 0-10 mile individual LCF risk assuming LNT and conditional on the occurrence of a STSBO.

	Early Containment Rupture		Late Containment Rupture	
	Emergency	Intermediate and Long-Term	Emergency	Intermediate and Long-Term
Mean	2.4E-04	4.6E-04	7.6E-08	8.3E-05
Median	1.1E-04	4.2E-04	5.3E-08	6.7E-05
5th Percentile	8.5E-06	1.7E-04	2.5E-10	3.0E-09
95th Percentile	1.0E-03	1.0E-03	2.4E-07	2.2E-04

** NOTE: Only 20% of the late containment failure realizations were extended to 72 hours.

Early Fatality Risk

Table I-14. Statistics for mean, individual, early fatality risk conditional on the occurrence of a STSBO and on an early containment failure (within 15 hr of accident initiation). Tabulated statistics are means over weather variability and express epistemic uncertainties in the MELCOR and MACCS input parameters.

	0-1.0 mi	0-1.3 mi	0-2.0 mi	0-3.0 mi	0-4.0 mi
Mean	2.6E-06	1.7E-06	1.1E-06	3.8E-07	2.2E-07
Median	1.9E-12	-	-	-	-
5th-Percentile	-	-	-	-	-
95th-Percentile	2.0E-05	1.3E-05	6.8E-06	2.0E-06	-

Table I-15. Statistics for mean, individual, early fatality risk conditional on the occurrence of a STSBO. Tabulated statistics are means over weather variability and express epistemic uncertainties in the MELCOR and MACCS input parameters.

	0-1.0 mi	0-1.3 mi	0-2.0 mi	0-3.0 mi	0-4.0 mi
Mean	2.3E-08	1.5E-08	9.7E-09	3.4E-09	2.0E-09
Median	-	-	-	-	-
5th-Percentile	-	-	-	-	-
95th-Percentile	-	-	-	-	-

Updates in Response to ACRS Members' MELCOR Comments

Casey Wagner, dycoda, LLC

Tina Ghosh, Accident Analysis Branch, NRC Office of Nuclear Regulatory Research

Fabric Seal Failure Criteria

- ACRS member comments concerned identified error, confusing description of the failure parameter, and seal failure area
- New documentation added on the impact on early containment
  - Flow through the ice chest and other leakage pathways allow hydrogen transport to the dome prior to the first burn
  - Focused valve study includes fabric seal correction
  - Comparisons to draft 2016 UA results show continuous behavior
- New documentation added on impact on late containment failure
  - Intact seal failed following the first hydrogen burn in the dome

Fabric Seal Failure Criteria

- Fabric seal parameter description was re-written to clarify UA parameter sampling versus the deterministic thermal-mechanical failure criteria
  - Sampled UA parameter is for the cold failure differential pressure
  - Deterministic non-sampled failure criteria for elevated temperature conditions
- ACRS member questioned whether seal failure area is different for mechanical over-pressure versus due to elevated temperature
  - Range of failure areas allowed robust natural circulation
  - Failure area was not important parameter in regression results

Ice Condenser Doors

- Sampled AJAR parameter controls lower ice chest door open area after fully opening
  - Five separate flow paths (Only 46 Pa required to fully open the doors)
  - AJAR flow paths are large and not the controlling flow resistance when <5 flow paths are open
- New UA discussion illustrating range of door behaviors
- Analysis from the focused SV study and UA shows all 5 MELCOR flow paths usually satisfy AJAR criteria when there is a hot leg failure
  - Partial (<5 flow paths) or no AJAR is the usual response when there is no hot leg failure

Hydrogen Ignition and Burn Direction

- Known ignition sources
  - Hot gases from the hot leg and the PRT
  - Ex-vessel debris
- Location of the ignition within the compartment has uncertainty
  - May not be near the hot leg, PRT, or ex-vessel debris
  - Dependent on local gas concentrations, mixing, and strength of the ignition source, etc.
- New discussion identifies flame directions as it propagates within the compartment as surrogates for uncertainties in the ignition location

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Safety Valves

- Added discussion of how the number of demands (denominator) is calculated in NUREG/CR-7037 operating experience data

- Revisited publicly available information from Licensee Event Reports on the main steam safety valve failure-to-close events, and SME's summary of these events, but did not add anything further to the report

- Added discussion on rationale for not including failure-to-open SV failure mode

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Time in Cycle Sampling

- Two-part approach
  1. Improved the discussion of the distribution between BOC, MOC, and EOC in the Sequoyah documentation
  2. New Surry UA uses more continuous sampling of inventory & decay heat throughout the fuel cycle
- New Sequoyah discussion includes
  - New ORIGEN work to characterize selected Sequoyah values within the continuum of the fuel cycle
  - BOC sample space defined with consideration of decay heat build-up and development of secular equilibrium inventory of I-131
    - BOC investigates low decay heat response
  - EOC sample space is developed with consideration of long-lived Cs-137 inventory
    - EOC investigates high decay heat response and near maximum Cs-137 inventory

Other Additions

- Statistical stability analysis conducted for figures of merit and documentation added in Appendix A
- Recognized possible impact of eutectic melt temperature on burnup but also inadequate data to quantify
- Added discussion on approach for failed computer calculations
- Added discussion of ice bed response during seismic event
- Improved vessel failure discussion to address cited issues
- LTSBO discussion improved to address limitations on asymmetric SG response
- Added discussion/plots on RCP leakage as an ignition source

Other Additions

- Comparison of new UA results with draft 2016 results with different valve failure distributions
  - Presented last ACRS meeting
- Focused pressurizer SV study results

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Updates in Response to ACRS Members' MACCS Comments

Nathan Bixler, Sandia National Laboratories

Sandia National Laboratories

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Emergency Response Timing

- Question – Is emergency response normally triggered by GE?
- Response – Verified that emergency response modeling is faithful to TEMA planning
- Resolution
  - Final Exercise Report Sequoyah Nuclear Power Plant (FEMA, 2004) timeline lists sequentially
    - SAE declaration
    - First siren, first EAS message, “School relocation, notification for buses, stay tuned”
    - GE declaration
    - Second siren, second EAS message
  - Notes from conversation with TEMA state that “upon SAE, schools are evacuated to paired schools outside EPZ”
  - Discussion added to final document

Humidity

- Question – How does MACCS treat deposition under humid conditions?
- Response – authors indicated that humid conditions could cause hygroscopic aerosols to grow and deposit faster, but MACCS does not treat this mechanism
- Resolution
  - Performed bounding estimate to show that deposition velocity of hygroscopic aerosols could increase by less than a factor of 2 and this is within the uncertainty range
  - Expanded discussion in section on uncertainty of deposition velocity

Presentation of Risks

- Comment – Improve presentation of EF and LCF risks
- Response – Added discussion of bimodal nature of LCF risk, minimal dependence of risk on distance, and reconsider discussion of EF risk
- Resolution
  - Discussion of LCF risk was expanded to address comments
  - Figure and table showing EF risk was eliminated

DISCUSSION OF TOPICS FOR FULL COMMITTEE PRESENTATION

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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/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)
- NUREG/BR-0359, Modeling Potential Reactor Accident Consequences, Rev. 1 (December 2012, update in progress)

Acronyms & Abbreviations

AC	Alternating Current
BOC	Beginning of Cycle
CCDF	Complementary Cumulative Distribution Function
CCI	Core Concrete Interactions
CDF	Core Damage Frequency
CST	Condensate Storage Tank
DC	Direct Current
EOC	End of Cycle
EPZ	Emergency Planning Zone
EF	Early Fatality
GE	General Emergency
HL	Hot Leg
FLEX	Diverse and Flexible Coping Strategies
FTC	Failure to Close
FTO	Failure to Open
LCF	Latent Cancer Fatality
LNT	Linear No Threshold
LTSBO	Long-Term Station Blackout
MACCS	MELCOR Accident Consequence Code System
MCR	Main Control Room
MELCOR	Not an acronym - accident progression code
MelMACCS	MELCOR to MACCS Source Term Converter
MOC	Middle of Cycle

MSIV	Main Steam Isolation Valve
NTTF	Fukushima Near-Term Task Force
PDF	Probability Density Function
PGA	Peak Ground Acceleration
PRA	Probabilistic Risk Assessment
PRT	Pressurizer Relief Tank
PZR	Pressurizer
RCP	Reactor Coolant Pump
RCS	Reactor Coolant System
RLZ	Realization
RPV	Reactor Pressure Vessel
RtePM	Real Time Evacuation Planning Model
SBO	Station Blackout
SG	Steam Generator
SAE	Site Area Emergency
SIP	Shelter in Place
SME	Subject Matter Expert
SNL	Sandia National Laboratories
SOARCA	State-of-the-Art Reactor Consequence Analysis
STSBO	Short-Term Station Blackout
SV	Safety Valve
TDAFW	Turbine Driven Auxiliary Feedwater System
TVA	Tennessee Valley Authority
UA	Uncertainty Analysis