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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

(ACRS)

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REGULATORY POLICIES AND PRACTICES SUBCOMMITTEE

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TUESDAY

JUNE 6, 2017

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ROCKVILLE, MARYLAND

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

COMMITTEE MEMBERS:

JOHN W. STETKAR, Chairman
DENNIS C. BLEY, Member
MICHAEL L. CORRADINI, Member
JOSE A. MARCH-LEUBA, Member
DANA A. POWERS, Member
HAROLD B. RAY, Member
JOY REMPE, Member

GORDON R. SKILLMAN, Member

DESIGNATED FEDERAL OFFICIAL:

HOSSEIN P. NOURBAKHSH

ALSO PRESENT:

NATE BIXLER, Sandia National Laboratories

MATTHEW DENNIS, Sandia National Laboratories*

HOSSEIN ESMAILI, RES

RANDY GAUNTT, Sandia National Laboratories

TINA GOSH, RES

SALMAN HAQ, RES

TREY HATHAWAY, RES

DON HELTON, RES

DOUG OSBORN, Sandia National Laboratories

KYLE ROSS, Sandia National Laboratories

PATRICIA SANTIAGO, RES

TODD SMITH, NSIR

ANDREA D. VEIL, ACRS Executive Director

CASEY WAGNER, dycoda LLC

* = present via telephone

TABLE OF CONTENTS

P R O C E E D I N G S

8:31 a.m.

CHAIRMAN 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. Members in attendance today are Harold Ray, Dick Skillman, Dana Powers, Mike Corradini, Dennis Bley, Jose March-Leuba, and Joy Rempe, and Charles Brown. Hossein Nourbakhsh is the designated -- he=s here. I thought I heard him. Charlie is here, right? Correct the record. Charlie Brown apparently isn=t here. He might be here, and we don=t know. It=s a subcommittee meeting. I can say these things. I don=t have to be 100-percent correct. Ninety percent is not bad. We=ll hear more about that later today.

I=ve now lost my place. I think I said this, but I=ll repeat it. Anyway, 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 Analysis project for the Sequoyah Integrated Deterministic and Uncertainty Analyses. Today we have members of the NRC staff and Sandia National

Laboratories briefing 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.

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

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

The rules for participation in today=s meeting were announced in the Federal Register on May 24th, 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 request that participants in this meeting use the microphones located throughout the meeting room when they are addressing the Subcommittee. Participants should first identify themselves and speak with sufficient clarity and volume so that they can be readily heard. I=ll remind everybody up at the front desk push the button, turn the green light on when you speak, keep the green light off when you don=t speak so that we don=t have extraneous noise.

We have a bridgeline established for the public to listen in to the meeting. To minimize disturbance, the public line will be kept on in a listen-in only mode. To avoid disturbance, I requested attendees put their electronic devices, like cell phones and any other noisemakers you might have with you, in the off or noise-free mode.

Before we proceed with the meeting, I would like to congratulate Ms. Tina Ghosh of the NRC Office of Nuclear Regulatory Research who has been honored with an Arthur S. Fleming Award in Applied Science and Engineering from the George Washington University=s Trachtenberg School of Public Policy and Public Administration. The Arthur S. Fleming awards recognize outstanding men and women in the federal government. Dr. Ghosh was nominated for her longstanding work at the NRC, including her work on the SOARCA Uncertainty Analyses. Tina, congratulations. That=s really neat.

We=ll now proceed with the meeting, and I=ll call --

MEMBER POWERS: Chairman?

CHAIRMAN STETKAR: Yes, sir.

MEMBER POWERS: I would like to make people aware I have an organizational conflict of interest on this work, and I will be here just episodically. When I am here, people should pay no attention to any comment that I make or otherwise acknowledge my existence.

CHAIRMAN STETKAR: And with no further editorial comments, thank you, sir. It is on the record. And with that, we=ll now proceed with the meeting, and I call upon Pat Santiago, who=s hiding over there, of the NRC Office of Nuclear Regulatory Research to begin today=s presentations. Pat?

MS. SANTIAGO: Thank you, Dr. Stetkar. My name is Pat Santiago, Chief of the Accident Analysis Branch --

CHAIRMAN STETKAR: Move your mike a little better, close to you.

MS. SANTIAGO: Okay. Is this better?

CHAIRMAN STETKAR: That=s much better.

MS. SANTIAGO: Okay. The severe accident codes and models have been further improved since the 2012 SOARCA pilot study and provided valuable knowledge, development, and experience for our staff that can=t be gained through other training activities. The overall SOARCA project has been an important reference point for emergent needs and intermediate long-term research, both domestically and internationally. Just this last week, we received requests for MACCS support and economic cost benefit analysis, financial assurance and emergency preparedness, as well as MELCOR and MACCS support to respond to questions on spent fuel pool analyses.

Slide two lists the team members. I wanted to announce that Dr. Ghosh and Dr. Osborn are co-leads. Dr. Osborn is with the Sandia National Labs, and we have many other Sandia National Lab partners with us, Dr. Randy Gauntt at the table. We have several on the phone and that will respond to questions as we need. But this was a huge effort with the detailed analyses, so you can see many of the team members here on the list.

On slide three, it gives you a short outline of what we are going to cover today. And to be quick, I=m going to do the overview, and then we=ll proceed to Dr. Gauntt and Dr. Ghosh, who will do the MELCOR discussion on the model updates and the uncertainty analysis and so on in the afternoon.

Overall, these analyses are quite complex, and we have incorporated uncertainty analysis, as the Subcommittee recommended, with any MELCOR and MACCS analyses throughout this Sequoyah analysis. Overall, the ACRS has commended the SOARCA work as a major step forward. It provides a new, more integrated approach for analyzing important accident sequences and Level 2 and Level 3 PRAs, and the insights from these analyses are very useful in the regulatory decision-making process.

On the next slide, we have the background from the 2012 time period when we performed the first two pilot studies, and then we recommended and the Subcommittee endorsed that we perform a severe accident scenario for the ice condenser containment, and we also were going to continue to perform an uncertainty analysis for the Surry plant. We have been using these analyses as we=ve gone along to support the Level 3 PRA that the Agency is performing, as well as we=ve been able to use it to support the near-term task force activities to close some of the recommendations after

Fukushima.

We briefed the ACRS Subcommittee in May 2016 on this project, and we=ve taken this last year to update the analyses and respond to several good recommendations that we=ve received. In the last month, you=ve received a briefing on the update on the MELCOR code, and that should support this briefing, as well.

On slide five, we have, again, the outline on the approach that we used using the latest code version. We also considered the last plant site-specific information listed here. Core inventory, population, emergency response is some of them. But more importantly, we did integrate the consideration of uncertainty into accident progression and consequence analysis.

Slide six lists the scenarios. I won=t go through them. I just will note that we did not do any new work on CDF quantification.

And now I will turn it over to Dr. Randy Gauntt from Sandia who will discuss the accident progression modeling using MELCOR. Thank you.

DR. GAUNTT: Okay. So this slide presents a kind of outline of the things we=re going to talk about in the MELCOR partition of this, and there will be a separate MACCS partition that we=ll get to in the afternoon.

And what we wanted to do, there was so much material, and I think you probably found this report very technically dense, as Charlie Tinkler used to say, and we=re going to be going over these in some amount of detail on a few sequences. There=s a lot of interesting things there. So what we wanted to do is give you up-front, and that=s first bullet here, some general high-level observations in looking at the whole thing. These are some things that stand out to us as kind of remarkable.

So we=ll go down this. We have a short list in this bullet. Later on, when we conclude the MELCOR work presentation, we have a little more extensive list of the major takeaways. A lot of interesting things come out of this.

But we=ll, after we go through some of those high-level general findings, we=ll just survey the changes to the MELCOR model, the updates, and point out what the biggest changes were, namely the treatment of the safety relief valves. That=s probably the largest impact on the whole study. And I think maybe the second one might be how we=re modeling the pressure relief tank. And, of course, that came up at the last

ACRS meeting where we discovered an error in accounting for the flood up in the containment. It has a big impact.

Then we=ll move on to some of the short-term station blackout observations and conclusions. I think Hossein is going to run down that list. At that point, we=re going to, you know, there were about, oh, 12 or 14 individual realizations that we walked through in the report. We=re going to pick about four of those to just kind of, for example, walk through them and show some of the interesting features and insights from those. We find that digging in to each of those individual realizations always reveals the most fascinating system behavior of this.

We=ll show at the end of the MELCOR work a brief snap animation which kind of brings out some of the interesting dynamics of where does the cesium go and how does it move around. It=s very fascinating.

We=ll kind of change gears at that point and take a look at the regression work that was done on the sample variables. And Tina is going to, for the most part, carry that load.

We did discover one error, well, two errors, one that we know about, that has to do with the partitions. I forget what we exactly call them. And then a brief study on the impact of igniters and the benefit of igniters. Of course, the study itself, we don=t include the igniters. It=s simply the ignition sources from hot leg failure or lower head failure, or the PRT tank now has a source of potential hydrogen ignitions.

CHAIRMAN STETKAR: Randy?

DR. GAUNTT: Yes.

CHAIRMAN STETKAR: Since you mentioned ignition sources, in the previous version of the study that we saw, it had a so-called random ignition model in it. I think it was every 30 minutes you pulsed each compartment to see whether it would ignite. Why did you remove that?

DR. GAUNTT: Well, listen, I think the main reason is there=s no shortage of ignition sources from the accident itself. You=ll see what I mean by that. But perhaps, Hossein, you want to . . .

MR. ESMAILI: So I think what we wanted to learn from that random ignition is we learned from the previous study from last year in that, you know, if you allow for random ignition to occur, you=re going to have earlier burns that is going to preclude the later burns that could potentially fail the containment.

In this new round of calculations that we are doing and as you=re writing the report, we only have very, very few early containment failures. Most of the cases, we have only four realizations. So if you choose to do random ignition, it=s going to, again, reduce the number of early containment failures. That was the direction it was going last year, and we believe that=s the direction it=s going this year. So there=s nothing new that we were going to learn from a random ignition study this year.

CHAIRMAN STETKAR: I think I=m asking the question primarily from the perspective of realism. For example, one of the sensitivity studies that you may or may not discuss today looked at reactor coolant pump seal flow rates, leakage rates. In that particular sensitivity study, the containment failure for the kind of interesting realization still occurred earlier, but it occurred four hours later, and I think it=s simply because of the stylized single ignition sources that you had.

So I=m concerned about whether or not the results from this study are realistic in the sense of hydrogen ignition, the timing of hydrogen ignition and so forth.

MR. ESMAILI: What I will show you, I have some additional slides that was not covered in the main body of the report. What you=re talking about is the delay in ignition. We know that --

CHAIRMAN STETKAR: Exactly. And that=s my concern about removing the random sources also.

MR. ESMAILI: That=s right. So once I get to that, it starts from slide 21 or so --

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: -- I=m just going to, since we have a better understanding of what happens in the old one and the new --

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: -- but the truth is that we have not done a random ignition.

CHAIRMAN STETKAR: I know. I was just trying to find out, I think I=ve got the motivation for it anyway. So thank you.

MEMBER BLEY: When you get to that point, maybe you can comment on John=s question about realism because I don=t know which way is more likely to be the way the real world works.

CHAIRMAN STETKAR: I don=t either.

MEMBER REMPE: So while we=re in the beginning here with questions that are a little off the beaten path, there were a couple of things brought up at the last meeting that you addressed in the report and I=d like to discuss them various times during the meeting, and they=re MELCOR related, but one of them is real puzzling to me is the -- I brought up earlier about the eutectic temperature, and the report highlights this is an important parameter and they have the uncertainties. And during the discussion at the last meeting, it was said, well, this was inferred based on VERCORS and Phebus, and the write-up in the report says, well, this is one of the things maybe because it was irradiated data in the VERCORS test, right?

And so I went and looked at some of the references on that, and that=s like a very high burn-up fuel, and, yet, it=s not correlated with burn-up in the sensitivity analyses or the way you=ve done this evaluation, right? And should it be? I don=t know. I mean, this is something that differs in the way industry models things versus the way MELCOR models things, and that=s why I was kind of, I keep on the path. And can you tell me what I=m not understanding here?

DR. GAUNTT: That=s an interesting topic there. It seems to me, and I=m just speaking anecdotally, that as we compare our results to industry or, you know, not just industry but, for example, the Aztec Code group and whatnot, I think we=re getting, we=re kind of converging on what we think those type of eutectic fuel collapse temperatures are. I don=t think we know enough to tie that to specific burn-up, I guess I would say.

MEMBER REMPE: But the older tests were with new fuel, and they had something up to 2800, and it was older fuel or high burn-up fuel in VERCORS that was around 2500. And, again, the 2800 would give you more hydrogen, it would give you higher releases according to what your report has, and so that=s why I was kind of wondering maybe we=re missing something.

DR. GAUNTT: Well, you know, we do see a lot of still MAP calculations that are run that you see fuel melting way up at 3200. We=re far away from that. We=re liquefying fuel and collapsing fuel more in the 2500 --

MEMBER REMPE: But maybe you should have a broader uncertainty or maybe you should tie it to burn-up. I mean, you may not know enough to say I can tie it to burn-up, but to stop it so close around 2500 maybe is not a good idea is what I=m kind of wondering still.

DR. GAUNTT: Yes. Well, we=ll take note of that. Does it go up to 20?

MEMBER REMPE: What I read, it sure didn=t look like it. You know, there=s some tables in the report. I can pull them up as we go through today, but it sure didn=t look like you went that far. I mean, there=s one curve, and 2800 is like at the very, very far end. And I=m not sure you went that high. Anyway --

DR. GAUNTT: 2800 was a bold step when we were coming down from 3200.

MEMBER REMPE: But there are data to support that bold --

DR. GAUNTT: Yes, it=s kind of based on really sure material interaction between zirconium oxide and U02, and there=s a plateau in the liquidus there. We=ve come to believe that that is too high just based on collectively looking at Phebus experiments, and we feel that would be the upper bound, and I forget exactly what our upper bound was on this one.

MEMBER REMPE: I think there was some, again, I=d have to pull the exact sentences out, but I got the impression you didn=t sample beyond the range, and I got the impression you didn=t go that high to 2800, that you were at a much closer range to around 2500.

DR. GAUNTT: Twenty-seven.

MEMBER REMPE: Okay. Because it might be more important --

DR. GAUNTT: It=s a target that=s been

coming down and down as we just evolve --

MEMBER REMPE: It=s in the irradiated fuel, so if you=re going to do sensitivities on beginning of cycle and middle of cycle and end of cycle, it seems like --

DR. GAUNTT: Yes, we did not correlate it to burn-up. That=s true.

MEMBER REMPE: Anyway, that=s my first distracting question of the day.

DR. GAUNTT: Okay, good.

MEMBER SKILLMAN: Randy, will you speak about the sensitivity of the PRT. I=m intrigued that you=ve raised it two times in your opening comments, and I=m just curious.

DR. GAUNTT: So I=ll give you maybe a few spoiler alerts on it. It was a little embarrassing last year when we realized we had failed to inform the PRT that the containment was flooding up, and so what that meant was there=s a significant fission product accumulation in the PRT tank when it=s acting as a scrubber, you know, there=s water in it and it=s accumulating all this fission product. And without any heat loss from the PRT, basically all that material would come out later because it would just re-vaporize and even melt the PRT. I mean, it was just kind of not realistic, and we went back and fixed that.

So now what we see is, generally, that the ice melt flooding will come out and reach that PRT and start to, it goes about a third of the way up the wall. I=m kind of getting out of sequence on the slides here. And for the most part, that will stabilize the volatile fission products. The PRT will go dry because it will boil away the water that=s inside it. But with the external cooling, we find we stabilize the volatiles, in particular the cesium iodide.

I did see one case, there may be one or two cases where, because of early and maybe it=s larger heat accumulating earlier on in the PRT, I saw one case where the PRT boiled dry before the containment water level just got to it. And in that one, there was some re-vaporization of cesium iodide that came out.

MEMBER SKILLMAN: The reason I asked the question is I was thinking about how many different permutations and combinations there would be for buildings, but you answered the magic question in your explanation. That is, for an ice containment, you have the melted ice water inventory.

DR. GAUNTT: Yes, right.

MEMBER SKILLMAN: But it would seem to me, at some point, if you=ve dried out the PRT or the quench tank and now you have this huge volume of water and it=s enough to put some water level in the basement of your reactor building, you now have a floating tank that=s probably been sheared from its structure. It=s buoyant. That=s a big tank.

DR. GAUNTT: Yes.

MEMBER SKILLMAN: This is a matter of thinking about it. I was thinking containment is 5,000 - 6,000 gallons per inch, a number like that. You get two, three feet, four feet of water in there, you=ve got a pretty buoyant force on that --

DR. GAUNTT: Of course.

MEMBER SKILLMAN: -- containment. Thank you. And you answered it with the ice melt.

DR. GAUNTT: Yes, that=s an interesting thing. And another interesting thing we=ll come to later on is cesium iodide coming out of the steam generators as a volatile. I=m getting ahead. We=re going to show you an animation that kind of gives a very visual nice look at some of those dynamics.

All right. So at the end of the MELCOR presentation, we=re going to take a look at some sensitivities done on the long-term station blackout, and that=s with different assumptions about the operation and of turbine-driven aux feed, and those are also very interesting permutations. Some of them start to look a little like our beginning of cycle because putting you out on the long-term station blackout gets you down in decay heat, and it=s starting to look a little similar to some of the short-term beginning of cycle studies.

So with that, let=s see. Okay. High-level general observations. I initially had a list of about 14, and Hossein said, no, we=ve got to get it down to five. So my 14 comes later, but here are just some of the kind of high-level findings on this, and that is a big deal in this study, of course, is the potential to fail the containment early on a hydrogen burn. That=s been the classic concern about the ice condenser. And not unsurprisingly, maybe intuitively, the consequences are strongly affected by whether you have that early containment failure or if it is delayed, you know, out to 30, 40, 50 hours, or even out to 72 hours. So that all kind of makes sense if the early containment failure mode is by hydrogen burn. The late containment failure mode will be just long-term static pressurization.

This was interesting to survey all of 500 or so calculations we did. These early containment failures, they=re now a little bit rare, and this comes down to the treatment of the safety valves, and that=s the second biggest change in this whole thing. But if you=re going to have an early containment failure from a hydrogen burn, it only happens on the first burn. No subsequent burn will have enough energy or enough hydrogen involved to create a containment-failing pressure pulse, so that=s kind of interesting. It=s the lead-up to that first burn and how much hydrogen gets into the dome by the time you promulgate that burn.

Coming back to the safety relief valves, I=ll let Tina talk in-depth about it, but our new view upon reviewing all the information about safety valves is they may fail on a first cycle, but they=re more likely on subsequent cycles. If they don=t fail on the first cycle, they will cycle and cycle for quite a long time. What that means is, if the safety valve is cycling and cycling, as opposed to sticking in a wide-open position, you=re choking down the steam flow that=s leaving the vessel, and that, in turn, is having a feedback on the hydrogen generation. And the net result of that is a normally-cycling safety valve, you will produce less hydrogen by the time you get the first burn, first hot leg-initiated burn. Then if this valve cycles for a bit, seizes open, and then you=ve got this rush of steam coming through, that produces kind of more hydrogen.

So that was a general observation. We=re seeing less hydrogen with this more continuously-cycling safety valve.

MEMBER CORRADINI: So in the absence of a fail to close early, you don=t get any early failure?

MR. ESMAILI: I think we=re going to get into that --

MEMBER CORRADINI: That=s fine.

MR. ESMAILI: -- starting at slide 20, but, yes, you are right. You have to have a failure to close and with a large enough open area. If there is no failure to close, you are not going to have enough hydrogen produced and released to the containment.

MEMBER CORRADINI: Okay.

MR. ESMAILI: And this is for both for this year, you know, the current UA and last year=s UA. I will show you how we can collapse these data.

MEMBER CORRADINI: That=s fine. And then the follow-up question is the failure to close on first demand, is that outside of normal operating temperatures or normal? That is, what I=m trying to get to is is there actual reliability there that you actually see fail to close under the conditions that you assume they would fail to close here?

DR. GHOSH: So just hold that thought for maybe ten minutes because we=re about to talk a lot about that. But the short answer is there=s no data for what we=re trying to model exactly. We have some approximate data, and we=ll talk about that.

MEMBER CORRADINI: Okay. And then you can hold this next one, too, because as I looked at 266 versus 307 versus, I can=t remember the other numbers, there=s an interesting behavior that the plots I can=t see because you plot everything between 24 and 72 hours, and I=m focused on the first six hours. So, first, where can I find the first six hours in data? Because it seems to me the interplay of timing of when I get steam inerted and then get un-steam inerted just in time to burn could change the -- so if you=re going to get to it later --

DR. GAUNTT: Well, in the four realizations that we are going to look at closely, we kind of focus in on just those conditions. And if you=re really interested, I=ve got my big drive and all the data is on it.

MR. ESMAILI: Can I say something? If your question on the safety valve was that, if you=re assuming the first, it fails on the first cycle, we are not into core damage yet. So we have gotten, you know, we have steam-generated dry-out. It=s going up, and that=s when we get the -- so it=s still water in there, and we have not necessarily even uncovered the core.

MEMBER CORRADINI: Well, that=s the reason I asked about data. Okay, fine.

DR. GAUNTT: I think another kind of related thing is if the valve does stick open, it plays a role in moving hydrogen that has been generated up into the dome, and that=s another factor on whether that first burn is going to fail.

I guess the converse of that is the pressurizer safety valve failure to close with a large open area that=s above about 30 percent results in greater hydrogen production and increases the potential for early containment failure. It=s another sort of required but not sufficient condition in what we observed in all the calculations.

So we only saw four of those early containment failures. We=ll look at them a little bit later.

MEMBER SKILLMAN: Randy, let me ask this. That fourth bullet is interesting. Failure to close with a large open area. One of those valves, if it=s open one-quarter of its diameter, it=s 100-percent open. So it only has to be open a skosh to be I think what you=re labeling a large area. I mean, I=m talking just 60 mils, 70 mils, a millimeter. Is that what you=re communicating here?

DR. GAUNTT: Oh, I don=t know. What are we communicating about that?

CHAIRMAN STETKAR: Let=s wait for Tina to talk about valves.

MEMBER SKILLMAN: Okay. Fair enough. Thank you.

DR. GAUNTT: All right. Then, of course, the late containment failures, you benefit with a late containment failure that anything that was airborne early on is going to be all settled out. So that=s a good thing. And that=s generally the case that the late containment failures are going to have a reduced source term that benefits from that settling.

But we did see some instances where the decompression of the containment at that late containment failure led to some re-vaporization of cesium iodide that was in a hot spot. And that=s a kind of interesting phenomenon, as well. Often, it=s coming out of the steam generators.

So always exceptions to the rules, but, generally, the early first burn is going to give the biggest source term. Late in the sequence, you=re going to have the settling take place, but there=s some other dynamics of re-vaporization that can give you a little kick at the end.

All right. Those were the high-level things that I wanted you to be sure and have in case when we went through all the individual ones we didn=t scoop them all up and, you know, bring them out. So that=s the overview.

All right. The next few slides go into some of the changes in the model and in the uncertain input variables. And aside from the cycling valves, that=s actually the number one thing that changed the whole character of the outcomes.

The pressurizer relief tank was a really big deal, and I probably said enough about this. But accounting for that water pool flooding up on the tank serves to cool any fission products that had been deposited in there, and there=s quite a lot that will deposit in there. We=ll look at a figure to show what we did to account for that.

And then our modeling of this hydrogen ignition in the lower compartment as a result of hot gasses coming out of the PRT, this could be initiated by a valve seizing open and sending a lot of gas to the PRT. That is now a source of ignition is hot gas coming out of the PRT, in addition to the other two, which was hot leg failure would be producing a valid ignition source and, of course, the ex-vessel head failure which, once the core goes ex-vessel and begins this core concrete interaction, this is an ongoing and persistent source of both hydrogen and ignition. We=ll look at some plots on that later.

CHAIRMAN STETKAR: And, Randy, there are absolutely no ignition sources in the upper containment volume above the operator --

DR. GAUNTT: There are no ignition sources in the -- I guess in the little study on the igniter -- is that right, Casey? We did --

CHAIRMAN STETKAR: No, I=m talking about --

DR. GAUNTT: But in the base study, no other ignition sources. The only way it burns in the dome is it propagates up from the bottom.

MR. ESMAILI: No known sources of ignition --

CHAIRMAN STETKAR: No modeled sources of ignition in the upper --

DR. GAUNTT: That=s right, correct. Now, we=ve also included, in the cases when turbine-driven aux feed is available, kind of building on our post-Fukushima, we did a lot of work looking at RCIC turbine behavior in the Fukushima accidents and decided a table look-up on pressure versus performance wasn=t really the best we could do. And so we=ve developed new models, dynamic models, for turbine performance that mechanistically connects turbine RPM with a pump performance and makes use of a new homologous pump curve. We=ve just added that feature into it to scoop up what we learned in our Fukushima analyses. And then the last change that we put in was to model leakage of the MSIVs.

So now, because of the, you know, since it=s kind of a standard leakage from the MSIVs, these pressurizer, the steam generator secondary side will now bleed down over a period of several hours from where it sat. There=s some rationale in what we chose for the leakage. Being no performance requirements, we kind of roughly based this on what=s been observed in leak rates for BWR main steam isolation valves, and I think we took 20 times that. So that=s the number. The secondary now bleeds down over about two hours and seems more realistic.

Okay. Slide 11. Here is the improved PRT analysis that we did. What we did was informed the PRT that water was out there. It was always out there, but you have to tell the code that in order for it to communicate. The graphic there shows what we did. We broke the tank into kind of three segments: a ceiling part that=s probably always dry and above the water level and then two kind of general floor/wall areas that could see water depending on the level there. And you see how we broke it up with a 45-degree sector.

Each of those segments communicates now from the whatever is inside the PRT to the wall, conducts heat through the wall, and then communicates to the outside, whether that be the atmosphere or the water.

MEMBER SKILLMAN: What does the HS mean?

DR. GAUNTT: I=m sorry.

MEMBER SKILLMAN: HS. Wall HS --

DR. GAUNTT: Oh, okay. That=s MELCOR parlance for heat structures.

MEMBER SKILLMAN: Oh, okay. Thank you.

DR. GAUNTT: All right. Okay. For heat structure. Yes, that=s a heat structure. So we broke that into those segments so that now we can transfer heat through the wall to the outside water. The graphic shows the water coming up about halfway. As we actually look at the water elevation, it=s about a third of the way up. But it=s very effective to capture, stabilize the volatiles that come in there.

CHAIRMAN STETKAR: And you actually keep --

DR. GAUNTT: Yes, we do.

CHAIRMAN STETKAR: -- water in the tank for a reasonable amount of time --

DR. GAUNTT: There is.

CHAIRMAN STETKAR: -- in this model.

DR. GAUNTT: There is, yes. It will go dry.

CHAIRMAN STETKAR: Eventually, sure.

DR. GAUNTT: Yes. Okay. So that=s the improvement on the PRT, and it did change the nature of the source term.

Now, here we=re kind of serving over some of the sampled parameters for a short-term station blackout. We color-coded these to say some we added that were new, we didn=t have them before, and some that we have updated since our last PRA. And the big one is the primary safety valve failure to close. I=ll let Tina talk about that, but that had such a profound effect on the number of early failures.

CHAIRMAN STETKAR: Randy, I didn=t get a chance to look through your slides because I was trying to read too much stuff. I know you=re going to talk about the safety valves. That=s good. I=ve got a lot of questions on those. Are you going to talk more about the barrier seal, the barrier seals and the ice condenser doors? Do you have slides on those? I didn=t -- you do?

MR. ESMAILI: We have one. I think 20 is the ice ones.

CHAIRMAN STETKAR: Okay. How about the barrier seals?

DR. GAUNTT: And we do talk about the barrier seals, and this is one of the known errors that we=ll come to.

CHAIRMAN STETKAR: Okay, okay, good. Thank you.

DR. GAUNTT: So having updated the primary safety valve behavior, we went ahead and updated the secondary to just be consistent treatment there. So that=s an added thing that we sampled that was not there last time.

CHAIRMAN STETKAR: Randy, are you going to talk about sampling the secondary valves? Did you allow the secondary valves on both your, I=ll call it the isolated steam generator and on the lumped steam generators to cycle and stick open, or did you only model stuck open on the isolated steam generator?

DR. GHOSH: Yes, so we imposed the, we calculated the probability based on the four lubes that we imposed to that stuck-open valve on the single --

CHAIRMAN STETKAR: Thank you. Thanks for reminding me. I remember that. Thanks, Tina.

DR. GAUNTT: In the category of the in-vessel parameters, I guess we kept the melting temperature, the eutectic, and zirconium oxide the same as before. But because hydrogen is such a big deal in the ice condenser, we felt it prudent to expand the effective sampling of uncertainty on oxidation kinetics so included two other competing oxidation kinetics to sample amongst them. And we=ll look at -- it=s very subtle stuff. Some of them are more oxidizing and at a higher rate at lower temperatures and some more at a higher temperature. So we=re sampling across a broader range of uncertainty amongst the popular correlations out there. We have a slide prepared for that.

Then on the ex-vessel accident progression, the thing that=s been modified here is the barrier seal failure pressure. We had a little mistake on that that we=ll talk about. And then Hossein has got quite a lot of detail on these doors and how they behave.

All right. Oh, time at cycle, yes. So we have a slide on that, so I won=t say a lot about it. But we included beginning of cycle, middle of cycle, and end of cycle sampling, as well.

Next slide. This is where Tina tells us everything she=s learned about valves.

DR. GHOSH: Everything we learned. So, actually, the last couple of times we came to the Subcommittee, you know, for both --

CHAIRMAN STETKAR: Tina, just pull, either scream or pull the mike closer to you.

DR. GHOSH: I don=t like to scream. Is this better?

CHAIRMAN STETKAR: As long as we=re okay over --

DR. GHOSH: Yes, okay. All right, great. So this was a topic of conversation at both our Surry UA discussions when we came to Subcommittee, as well as again at the Sequoyah subcommittee last May. We=ve always known that the safety valve behavior has a large influence in how your accident progresses. We=ve seen this, in fact, even for the BWRs when we did Peach Bottom. So it=s kind of a consistent theme.

And after the subcommittee meeting in May, we did some more digging to see if we could do a better job at kind of capturing the state of knowledge on how these valves might fail and what kind of open area.

So this first slide is just a reminder of the system that we=re looking at. On the primary side, we have three parallel safety valves that are basically there to prevent over-pressure events. They have, the three valves have progressively higher pressure set points. They=re meant to be a redundant system to make sure that you don=t get to an over-pressure situation.

So if everything is going as planned, you start out in state one. When the system starts to over-pressurize, and just a reminder that for our scenarios we=re not crediting the PORVs, so we=re relying on the safety relief valves to relieve pressure. So, initially to relieve pressure, that first valve will start cycling. If --

MEMBER CORRADINI: Just a clarification. So the PORVs are just ignored?

DR. GHOSH: The PORVs are ignored, yes.

MEMBER CORRADINI: Because there=s no data or because it=s just --

MEMBER SKILLMAN: They=re assumed closed.

MEMBER CORRADINI: I understand. So they=re not there?

DR. GHOSH: Yes, we assume that there=s no way to control them; and, therefore, we=re relying on the passive safeties. We don=t credit much in our scenarios.

So the first valve is cycling. If it experiences a failure to open with a sufficiently large open area, you would get to state four, which is indicated in yellow, and you start to, you basically start to depressurize.

If the safety valve, the lowest set point safety valve fails to close but with a very, very small open area, you could start cycling the second safety valve to relieve pressure. And same thing there. If that one also failed to close with a very small area, you would start cycling the third valve.

Then you have those valves fail with a sufficiently closed area, you get to state four. Then --

MEMBER SKILLMAN: Tina, is that behavior based on operating experience or just based on your assumptions?

DR. GHOSH: Well, I guess, so far, we haven=t described any probabilities. This is just the possible states that the system might experience. But based on all testing and observed data, it=s very hard to reach state five. That=s the case where all three safety valves fail to open or fail to close with such a tiny area that you no longer have any way to relieve system pressure.

From everything we=ve seen and all the experts, you know, the knowledgeable people we talk to, this seems like an extremely unlikely state to reach. It can happen, but the probability is so small compared to one of the three failing to close with a sufficient area. I=ll get to this on the next slide. We had originally included this modeling, the failure to open of all three valves, in last year=s version, but we took it out because last year, in 1200 samples, we never sampled a case where all three failed to open. And just in terms of the probabilities, you know, the thousands of tests that have been done and also the little bit of operating experience we have on the secondary side, it just seems that it=s really kind of a very extremely, extremely low probability situation. State five is not very likely, but it=s possible. So these are kind of the possible --

CHAIRMAN STETKAR: Full-scope PRAs include that. There are data for safety valves failing to open, and there are common-cause failure rates. So if this were a PRA, it would include it. This is not a PRA.

DR. GHOSH: Okay. So what did we change from last year? So we tried to dig in more into the little bit of data that we do have on the safety valves and talked to additional people, the folks who actually test the valves, the people who code the operating experience and collect it for PRA purposes, the people who sit on the ASME committees for the testing requirements. We tried to gather as much information as we could. And we=re using the same approach in the current UA as we did in the draft UA. We=ve just tried to better inform how to use the available information.

So in terms of the information data sets that are available, NUREG-CR-7037, which we talked about last year, that=s the industry performance of relief valves at U.S. commercial nuclear power plants, that kind of captures all of the valve-related data up through 2007. So the authors of that report had collected everything from 1987 through 2007 both in terms of tests, as well as actual scram-based data. And in that report, there are two sets of data, one that=s based on testing of the valves, and there have been thousands of tests of these particular kinds of valves; and ones based on actual scram events.

So the first table I=m showing here, this is what we had used for the draft UA, and it probably wasn=t the best terminology. We call the number of demands the number of tests in that third column, but it=s actually demands. We chose to use the data that were based on actual scram events because we saw that the failure rates were very different based on actual scram events versus the testing.

The other thing we had done last year is that we combined the primary side safety valve data with the secondary side safety valve data, and that=s how we got to the 773 tests. There were actually 573 demands on the secondary side, initial demands, and then 196 subsequent demands. We added those together. And then there were four demands on the primary side, so you add all those together and you get 773.

And then for the number of failures, there were no failures to open based on actual scram-based data, but there were 15 failures to open on initial demand on the secondary side, sorry, failure to close on the secondary side and two failures to close on the primary side. So we added the 15 and 2 together to get the 17 failures.

So when we went back and revisited the data -- so I guess I should mention first a couple of things. So we wanted to dig into more why does the testing data look so different from the scram-based data, and from what we could gather in talking to the testers and looking at the testing requirements, the testing is really focused on making sure that these valves will relieve pressure if you have a design basis over-pressure event. That is the sole purpose, and, for that purpose, the testing requirements work, the testing works, it=s all great. But the tests don=t actually fully stroke the valves and pass steam, so there=s actually no test or testing what we care about, which is that you=re demanding the valve to open past, you know, steam and repeatedly do this for a long period of time. The testing doesn=t actually capture the situation that we=re interested in. Do you want to --

MEMBER MARCH-LEUBA: Yes, sorry to interrupt you. Of those 17 failures to close, I=m thinking of my experience with valves, and I cannot remember a single time in my life where a valve failed to close. There were a lot of times where the valve failed to close completely. Those actually closed, but there was a little leakage and they became a failure, or they had to stick open because a spring broke?

DR. GHOSH: Yes. So thank you for bringing that up. I=m going to talk more about that when we get to the open area discussion in a couple of slides. But, yes, in the -- we basically read the licensee event reports, which, for those of you who=ve read LERs, you know the level of detail varies very widely. It depends on how enthusiastic the report writer is. So some of them had a lot of detail. Other ones didn=t have much detail, and you=re trying to kind of guess. But in many of those descriptions, they described kind of a leaking situation. So I=ll get to that in a couple of slides.

So that was one of the major changes, as well, from last year. Last year, we assigned a uniform distribution on the open area, but this year we have a new distribution that much more weights that leaking situation, and I=ll get to that. But we also account for the other -- let me get to that when we get to that slide. There=s always a lot to discuss on the valves.

CHAIRMAN STETKAR: Tina, while we=re on this slide, I get to ask you questions now. I asked this a year ago, and I still don=t have an answer, so I=m going to ask it again because you=ve collected -- I forgot. Have you told the story that you went out and collected more data yet? You haven=t said -- okay. Tell us the story that you went out and collected more data now.

DR. GHOSH: The only new data that we

collected is that we asked the very kind authors of NUREG-CR-7037 to compile the data from 2007 to 2016 using the same methods and so on. So the only new data is basically, it=s as if NUREG-CR-7037, if that database were updated using the same data collection methods to March of 2016, that=s the only data that we have.

CHAIRMAN STETKAR: And to cut to the quick, they found one additional failure and 75 additional demands?

DR. GHOSH: Yes.

CHAIRMAN STETKAR: If you do the math, they found one additional failure and 75 demands. How do those folks calculate the denominator? It=s surprising to me, very surprising that for pressurized water reactors, from 2008, actually October of 2007 when the end of 7037 database applies through March of 2016 that we=ve had 75 demands to open main steam safety valves on pressurized water reactors. How were those 75 demands calculated? I couldn=t get an answer a year ago on how the 773 were calculated up through 2017 or 2007. I still don=t know that.

So since we have new data and people who have really looked at stuff really, really carefully, how were the 75 demands to open main steam safety valves on pressurized water reactors determined?

DR. GHOSH: So first let me tell you that denominator, I think we do the best we can with the information that we get. There are some -- in a lot of the LERs, they will say if safety systems have been actuated, but if they didn=t fail, you know, they=ll say that, you know, that such-and-such valve was opened and that it reseated, so it=s a successful demand.

CHAIRMAN STETKAR: I=m not hearing an answer to my question. If you made up the number or if they --

DR. GHOSH: Nobody made up the number.

CHAIRMAN STETKAR: Okay. But if it=s off by a factor of five, that can substantially affect the overall results. If it=s off by a factor of ten it can. And it is not credible to me, based on my knowledge, that we=ve had that many demands. Now, I might be wrong because I haven=t looked at the operating experience. I just don=t know. So I would really like to know how those denominators were calculated explicitly. I would like to know not we=ve done as good as we can. I want to know I, today, accounted for 12 demands on main steam safety valves for the following reasons because that=s really important.

DR. GHOSH: I understand.

CHAIRMAN STETKAR: No, wait. Let me finish. The reason it=s really important is that I=m trying to understand the nexus between this study and reality because these studies, the SOARCA studies, are receiving a lot of visibility and they=re being presented as the NRC=s state of the art, which will be interpreted as the NRC tells me to use these data, this is the best data that the NRC can derive. People will use these data and cite these reports for decades. So that=s one thing.

Number two, we have in parallel research a Level 3 PRA study in practice. The last I checked, the Level 3 PRA study is using a failure to close failure rate that=s about two orders of magnitude smaller than the failure rate that=s estimated by these data. So we have now potentially two state-of-the-art studies being done by the same organization, research, in parallel, both of which will have a lot of visibility. And unless the Level 3 PRA has updated their data, and I have not seen the updated database in a couple of years, we could have widely diverging estimates of the NRC=s state-of-the-art knowledge of this particular failure mode, and that=s why I=m so concerned about this. And it all hinges, I know everybody has scrutinized those 16 events as well as you can. I=m talking about the 844 other events in the denominator.

DR. GHOSH: Yes. We had a bunch of comments in there. Maybe I=ll address the last one. Well, let me make a broader statement. I think the reality is we have no real world, perfectly relevant data for what we are trying to model. We have some data, and we=re using it the best way we can. But there is a huge uncertainty in how these valves, you know, what the true failure rates might be and how they might behave if we were to have, you know, the accident that we=re modeling.

I think we tried to highlight that, that, you know, at the end of the day, we don=t have a whole lot of data to go on, so, hopefully, nobody is taking these numbers and running with them as very solid. So let me just start with that.

CHAIRMAN STETKAR: Okay. Tina, let me, before you go on. You say we have a huge uncertainty on these data, so I ran out the uncertainty distribution for the fail to re-close failure rate on initial demand, that 16 in 621 with that beta distribution. The fifth percentile is 1.34 times 10 to the minus 2. The 95th percentile is 3.91 E to the minus 2. That=s roughly a factor of three. That is not a huge uncertainty. That is really good data in the sense of collecting data.

So don=t say we have huge uncertainty on that failure mode. The uncertainty distribution that you derive is quite narrow, not so much for the second one where you have zero evidence in 223. That uncertainty distribution isn=t that much broader because of the sparsity of failures. So I just want to get that on the record as you come back at me.

DR. GHOSH: Okay. Let me address your comment about the Level 3 PRA team, and I see that Don Helton is in the audience and I may call on him to help me out here. But we=ve been in communication with our colleagues in research who are conducting the Level 3 PRA project. They=re aware of the work we=ve done. I think there is discussions of potentially how to update these failure rates in the SPAR models. And in the last SPAR update, we couldn=t kind of get everything in in time to have it updated, but we=re in discussion with those other folks, too. So eventually --

CHAIRMAN STETKAR: But I=m not talking about the SPAR models now. I=m talking specifically about the Level 3 PRA for the Vogtle Generating Station.

DR. GHOSH: Yes.

CHAIRMAN STETKAR: And they, indeed, do have failures to re-close of both primary valves and secondary valves in that model.

DR. GHOSH: Yes. Don, do you want to take this one?

MR. HELTON: This is Don Helton from the Office of Nuclear Regulatory Research. Let me manage expectations right off the bat by telling you you=re not going to be satisfied with this answer.

CHAIRMAN STETKAR: As long as it=s on the record, I=m satisfied.

MR. HELTON: Good. Then we=re good. So, I mean, what you=re going through, let=s focus on SOARCA for a moment, you=re raising the valid point that the state of knowledge is changing. It=s evolving right now. We on the Level 3 PRA side are trying to balance that and to not be left behind but also not to get on a moving train while it=s still moving.

So as Tina said, we are closely coordinating with the SOARCA team and trying to bring the insights from this dialogue into our study once they are at the proper level of maturity to do so. What that means at the moment is that in the Level 1 PRA for the Level 3 PRA, we are using the older information, but it also needs to be understood that the Level 1 PRA doesn=t use this type of formulation. They don=t use a failure probability per demand and then calculate the number of demands that they see for any one given sequence. Rather, the data is sliced in a way to say, if I have this event, then I have a cumulative probability of seeing a failure for this event of some value, and those can be translated back and forth to failure probabilities per demand, but that=s not a direct thing to do. So what I=m trying to say is we=re using the old data set, but we=re also using it in a slightly different way.

For the Level 2 PRA where we do use a formulation that=s more proximate to this, we are now using the very failure probability per demand results or formulations that Tina is presenting to you. We have updated that within the last year and are now in lockstep with what you=re hearing.

So, again, in summary, we are coordinating. We are trying to use this information when it=s at a proper level of maturity and when it makes sense to use it, but it is an evolving process. And so, you know, please don=t expect that the two are going to be uniform in alignment across the board.

MEMBER BLEY: Uniform is not -- that kind of sounds like they=re pretty close but they=re not the same. A couple orders of magnitude isn=t even in the same ballpark. And if you=re using one set of data for the Level 1 and another for the Level 2, that=s a step worse than we heard before.

But I=m going to go back to where John started. If, in fact, you just gathered eight or ten years= worth of new data, then whoever did that ought to be able to very clearly state how they decided there was a demand. Did they count reactor trips? Did they count something else? Did they count the times it was mentioned in a maintenance record? Exactly how it was calculated. And that would at least help us understand if something is missing.

DR. GHOSH: Yes, I understand. So let me take that as an action item and get back to you because I only have partial information. I can=t give you a definitive answer, so let me get back to you.

CHAIRMAN STETKAR: And that=s why I focused on, you said they use the same methodology, the same people did it. So if we could understand the source of the 75 demands from the more recent data set, that would give us some insights about how the larger population were derived from the older data.

DR. GHOSH: Yes, understood. So I will get back to you on that. And on exactly that point, you know, the demands, I remember last time you asked how could there have even been demands on the primary side, you know. And so we looked at those in even more detail, and each of those cases it was a huge, a very sudden loss of load event and the initial pressure spike was so big that it not only, you know, blasted open the PORVs, but it blasted open one or two of the lowest set point safety valves. So that was, you know, that was what happened there.

But, you know, when we looked at that and then we looked at the two -- there was only four of those that were actually recorded. And then we looked at the two failure events, and one of them had a failure mode that no longer exists in the fleet of PWRs. They used to have these --

CHAIRMAN STETKAR: Failure cause.

DR. GHOSH: Right. Failure cause. Thank you. They used to have these loop seals that were filled with water. And after, you know, a review of the operating experience in the mid 90s, you know, there was an EPRI recommendation to drain these loop seals, which subsequently happened. So that cause of failure is kind of defunct now in the current fleet.

So given that and then the difficulty, especially on the primary side of accounting for the two number of demands, there could have been many more demands that wouldn=t have been recorded on the RCS side.

MEMBER BLEY: That=s the point I was going to jump in on. You don=t have a clue how many demands there were. You really don=t. You know there were, whatever, two or three failures, whatever it was, but you don=t have a clue how many demands. So even trying to incorporate that data seems very suspect.

DR. GHOSH: Right. So we decided that -- okay. And then when we talked to the valve experts, you know, there are some differences in the safety valves on the primary and secondary side. But we thought that in weighing the difficulty of having essentially no data on the primary side versus having some data on the secondary side, even if the valves aren=t exactly identical, that it was better to just use the secondary side data and apply it to the valves on both sides.

So that=s kind of where we took out the two failures to close from last year that came from the primary side, we added one from 2007 to March of 2016, and we ended up with 16 failures to close. Those were all on the initial demand and then the updated demand counts.

The other thing that both based on the operating experience and talking to the valve testers, you know, it seems to be a consensus expert opinion that you=re much more likely to fail on the first demand. And if you survive the first demand, you=re more likely to keep on cycling, and that shows up in the data, too. So we decided it was worth modeling a different failure rate for the initial demand versus subsequent demand, and that=s how we get to the second table that we=re using in the current UA.

Okay. So then when we go to how do we actually implement this, we=re doing the same thing we did last year in that we=re trying to model both the epistemic and aleatory uncertainty in the valve behavior. So, first, we use a beta distribution that=s based on these tables to create the uncertainty, the epistemic uncertainty and the true failure rate on demand. So the one on the top left is for the initial demand, and the one on the bottom left is for subsequent demands. And we used a Bayesian approach starting from a non-informative which is shown in the black dashed line there.

And then given a sampled epistemic failure rate from the blue curves on the left, for the initial demand, you end up with this composite probability of failing to close on the first demand. And then for the bottom right, for the subsequent demands, you=re basically saying that in this three-valve system, given an epistemic failure rate, we=re using a binomial distribution to actually count up what=s the probability of a different number of cycles that you can achieve before you get to failure.

So for each of the points on the blue curve, you generate one of these curves that=s shown as kind of the rainbow spectrum on the right. And so for each Monte Carlo realization, you=re sampling from a single curve on the rainbow side for the three valves.

Now, in our updated UA, we rarely get into the situation of cycling beyond the first valve just because we=re not seeing nearly as many failures to close. And, actually, I=ll get to why in two slides.

Let me quickly talk about the open area for action. We already talked a little bit about this. So the orange curves on these two graphs show what we had done last year in the draft UA. We essentially assumed a uniform distribution between just if it fails to close being one-percent open all the way to fully open. So we thought we don=t have much information, and we just assigned an equal probability to the whole range.

But, again, after reading the descriptions in the LERs and talking to the valve testers and so on, it seems that it=s actually most likely that if a valve fails to close it=s not, it=s in a kind of weeping or leaking situation. Now, again, these are qualitative descriptions, so we don=t know what weeping or leaking exactly translates to in terms of open area fraction, but clearly it=s not a big open area fraction. So we kind of translated that into about a 50-percent probability that if it fails to close it=s between one percent and ten percent. So that=s the high density that you see on the left.

On the other side, and I think this goes to your point, if you have a maintenance or assembly type of error and then you=ve had a very large pressure spike that kind of blasts open the valve and then it can=t reseat properly, you don=t have to traverse much of the diameter before you get into a full-open area situation. And so for that type of situation, we assign about a 90 percent to fully open and we weighted that 30 percent. So that=s what you see as the larger blue area on the right side. We thought these were the most likely situations, that you=re just leaking a little bit or kind of blasted it and it=s out of, you know, it can=t reseat and it=s just misaligned.

But because, again, there=s not very much, you know, operating experience data, we didn=t want to completely rule out the possibility that it might be something in between. So we have this kind of small probability still that it might be something between the 10 percent and the 90 percent. So that=s how we came up with that.

CHAIRMAN STETKAR: Stay on this for a second. You do have in the report an anecdotal event where you had I think one valve, somebody said it was stuck about 20-percent open, which is why the little bump there in the distribution.

DR. GHOSH: Right.

CHAIRMAN STETKAR: Now, this shape of the distribution seems qualitatively appealing. The distribution says there=s a 30-percent probability that a valve sticks open 90 percent or more. That means if I have 16 events where valves fail to re-close, I would expect four or five of them to have stuck 90-percent open or more. Is that consistent with our operating experience in your other table?

DR. GHOSH: So, again, you know, we struggle a bit with the level of detail that=s in the LER reports, which is primarily where you actually get a description of the events. So in some cases, they very clearly say that it was, you know, leaking. But in other cases, they just say that the valve failed to reseat and, you know, that the operators, you know, took recovery actions to do whatever. Oh, that=s the other thing I wanted to mention. It doesn=t really explain, you know, what the open area was. But in some of the descriptions, it seemed clear that the valve just got completely misaligned, you know, and it wasn=t going to reseat no matter what. So we were trying to capture this possibility but --

CHAIRMAN STETKAR: Tina, and I don=t think you read them so you kind of protect the folks who did. When you read the LERs, the secondary safety valve sticks pretty much open. You=re going to, after a reactor trip, you=re going to get a heck of a cool-down and you=re going to get a safety injection, and that=s going to be written up in the LER. So there might be other evidence that you have to give you an indication of how much stuck open it was, not necessarily simply a description of the valve itself.

DR. GHOSH: Right, right.

CHAIRMAN STETKAR: And that=s why I=m kind of probing how much did people do forensic work on those 16 events. There are only 16 events. I mean, we=re not talking about thousands of pieces of evidence here. To sort of justify the numbers in this open-fraction distribution because, again, the shape of the distribution is qualitatively appealing. And all of the engineering discussion that=s in the report, I thought it=s much, much better justified now than it was a year ago. It makes a lot more engineering sense. Now I=m kind of trying to focus in on how well does it connect to reality because, again, that open, as we heard earlier from Randy, that open fraction, these two parameters that we=re talking about here is pretty much the whole story on Sequoyah SOARCA. So these are the things we really want to pay attention to. We want to pay attention to a lot of other stuff, too, but these are driving the whole results.

MEMBER SKILLMAN: Tina, I=m sitting here questioning whether I should say this, but let me put it on the record because this might be an information source that you are not aware of. In the mid 80s, B&W designs were called into questions and OE told the B&W owners group fix your plants or we will shut down all B&W plants. At the time, there were 11. They ranged from Oconee 1 to Davis-Besse and included Rancho Seco and everybody in the middle.

The B&W owners group had a trip and transient team, and that was an on-call team. If a transient occurred at Rancho Seco, people mobilized from all over the B&W locations, including Lynchburg. And all 240, 260 trips and transients were examined in detail.

Back in the day, the B&W plants, the combustion plants, and the Westinghouse plants were tripping approximately eight times per year, about once every six weeks. And for every major reactor trip, there were 12 or 16 MSSVs that went up. So there were 16 incidents for every trip. There are over 2,000 pieces of data in that report, and it=s in this building, it=s somewhere here near us. It=s called the Safety and Performance Improvement Program. I sat in front of ACRS four times to explain how we were going to demonstrate the B&W plants were safe.

Why am I telling you this? Times have changed, people have changed. The hardware is identical. The same relief valves that were on the pressurizer then are on pressurizers today, and the very same MSSVs, a dozen or so per plant, with blow-down rings and setting, are identical today. That data is as good now as it was in 1988, 1989, 1990. That would give you over 2,000 data points.

Now, in many of the cases --

CHAIRMAN STETKAR: Dick, are you saying 2,000 data points where main steam safety valves were actually challenged to open?

MEMBER SKILLMAN: Yes. Eight plants, eight trips per plant per year for almost the entire

PWR fleet in the late 80s.

CHAIRMAN STETKAR: I=m sorry. I operated a PWR for five years, and we had a trip pretty much once a month. So I sat through many trips. We never lifted a main steam safety valve. Never.

MEMBER SKILLMAN: Every time we --

CHAIRMAN STETKAR: Maybe that=s a B&W thing.

MEMBER SKILLMAN: Okay. But that would still be that number of --

CHAIRMAN STETKAR: It=s a small fraction of the total population.

MEMBER SKILLMAN: But it=s a heck of a lot more than 600. All I=m saying is that there may be some data that could help this. I=m not saying it=s a fix. All I=m saying is, in struggling to get more data, there is likely a data source that would be useful to this discussion and it could bring some clarity to this.

DR. GHOSH: Yes, thank you. I=m sorry. Go ahead.

MEMBER SKILLMAN: The situation was so grievous that the B&W fleet was threatened to be shut down. So I=m talking about many, many, many instances where we had a secondary plant that was being governed by the main steam safety valves, which is the kind of situation that you=re pointing to here and it had to do with performance of the MSSVs and the relationship of the thermal hydraulics of the primary system on reactor trip.

DR. GHOSH: Thank you.

MEMBER RAY: Are you separating main steam safety valves from atmospheric dump valves, or are you lumping them all together, both of them together?

MEMBER SKILLMAN: No, no, I was talking MSSVs. Atmospheric dump valves also would have participated.

CHAIRMAN STETKAR: But if the atmospheric dump valves work, the safety valves are never challenged, at least not on the vast majority of plants. But this is not --

MEMBER RAY: We never lifted the safety valve.

CHAIRMAN STETKAR: Right. Never.

MEMBER SKILLMAN: B&W plants, they go up like a calliope.

CHAIRMAN STETKAR: You had atmospheric reliefs on your steam generators?

MEMBER SKILLMAN: No, no.

CHAIRMAN STETKAR: You didn=t?

MEMBER SKILLMAN: Atmospheric dumps were

not on the steam generators. They were on secondary systems, and they worked in consequence with dumping to the condenser and then to the environment.

CHAIRMAN STETKAR: You mean --

MEMBER SKILLMAN: I=m just saying there=s a data source there.

DR. GHOSH: Okay. So this next slide, this is kind of a reminder of, you know, why we care so much, what does it all mean. You know, one of the major focuses of the Sequoyah analysis was on understanding the conditions that might lead to early containment failure. This graph is from last year=s UA, from the draft UA, and what we have here is on the Y axis is the number of primary safety valve cycles that were actually experienced by the system. And on the X axis is the total open area fraction that was experienced by the system. So each point is one of the successful Monte Carlo realizations.

The points that are shown as blue are the ones that resulted in late containment failure, and the ones that are shown as red triangles are the ones that resulted in early containment failure.

So a couple of things I want to point out. We have boxed up here on the zero safe area fraction axis, there=s about 93 points where the system depressurized before the safety valves failed to close. So after you got up about 70-something safety valve cycles in last year=s UA, the system depressurized in some way, and it doesn=t really matter what the safety valves are doing after that.

So in last year=s case, there was about 93 of those cases. Now --

MEMBER BLEY: I=m sorry.

DR. GHOSH: Yes.

MEMBER BLEY: The results that happened out beyond a fraction of 1.0, were those systems rupturing or something? What=s going on out there?

DR. GHOSH: Yes, sorry. Thank you for reminding me. I meant to point that out. I know it=s been a long time since May last year. Because we have three safety valves, so if the first safety valve failed with a very small open area, we did have quite a few cases --

MEMBER BLEY: Oh, okay.

DR. GHOSH: -- where you start cycling this. So that=s how it adds up to more than one. We don=t have a lot of cases, but, yes, so, in these cases, barely more than one safety valve failed to close because the first one --

MEMBER BLEY: And you can fail at those

different points, like you just showed us.

DR. GHOSH: Yes. So in last year=s UA, and this is similar to what we found this year, you basically have to have failed a safety valve open and --

CHAIRMAN STETKAR: Push the microphone --

DR. GHOSH: Oh, yes. Okay, yes, sorry. So, basically, you have to have a failed a safety valve open, which means, you know, that, once you get into this region, you know, your hot leg has failed, it doesn=t matter. You have to fail the safety valve open and it has to be with a, you see over here it=s all blue dots, you have to fail with a sufficiently large open area to have the potential for early containment failure. You still have to have other stars aligned just right in order to get early containment failure, but, basically, if you get up to high number of cycles, and this high is still pretty low, you know, 70-something, you can=t get to early containment failure, or if you=re depressurizing with a very small open area, you=re not going to get to early containment failure.

The reason I show this, and Hossein is going to go into much more detail on this later, is that with our updated safety valve distributions now for both the failure rates, as well as the open area fraction, the number of points that we=re sampling in this region are much, much, much smaller. We=re down to, this used to be 60-something percent of our total realizations and we=re down to about five percent. So this is kind of the primary reason that we=re seeing fewer early containment failures in this year=s UA.

CHAIRMAN STETKAR: Tina, and this is probably premature, but I do want to ask about it. Are you guys going to talk about the realizations that did not complete successfully any time today? Were you planning to do that?

DR. GHOSH: We have one sub-bullet on that on one slide.

CHAIRMAN STETKAR: Okay. Just alert me when we get to that slide if I don=t remember it. I=ll search for it because I want to talk about it.

DR. GHOSH: Okay.

CHAIRMAN STETKAR: I don=t want to interrupt us right now if there=s a sub-bullet later.

DR. GHOSH: Sure. I=ll just give you a really quick preview. We had a much higher success rate in general in this year=s UA, but we had a higher percentage of failures in this region that we care about. And that=s what we=ll talk about.

CHAIRMAN STETKAR: And that=s what I want to probe of it, and I know that you were looking into it I think a little more, and that=s why I want to -- but let=s not --

DR. GHOSH: We were.

CHAIRMAN STETKAR: -- talk about it now. Let=s keep going on.

DR. GHOSH: Okay. So I think that was it. That was kind of my main point. But, again, Hossein is going to cover this in much more detail.

MEMBER MARCH-LEUBA: Being that the results are so sensitive to how much a valve fails to leak or open completely, I question your decision of not to consider the testing and concentrate only on the real event.

DR. GHOSH: Okay.

MEMBER MARCH-LEUBA: Because the testing is very defined.

DR. GHOSH: So, yes, I think I tried to explain that. If you use the testing data, we may have gotten zero early containment failures because the success rates are very high. But, again, there=s about 8,000 tests, right? So there=s a lot of data points. But, again, the issue we struggled with is that they=re just testing to make sure when you ask that valve to open it=s going to open and relieve pressure. But they=re not actually passing, you know --

MEMBER MARCH-LEUBA: Oh, you=re not thinking the valve spring had broken and it stayed open 100 percent that will have reported? My claim is, my life experience, you fly on airplanes and there=s a problem with the door not closing and it turns out to be the sensor. It=s never the door. And most of these events, the 16 events where the valve didn=t close, I bet 14 of them was the sensor failed and the other two was leaking.

DR. GHOSH: I don=t know about that because, you know, in many of the cases -- and thank you for reminding me. I meant to mention this. I keep forgetting. In many of the cases, they eventually recovered the, they were able to reseat the valve when they significantly lowered the system pressure. So they had indication that it was, it truly leaking or opened. But then we didn=t model the recovery because none of those recovered valves were demanded to open again, so we had no information on whether, what the behavior would be.

MEMBER MARCH-LEUBA: But this, based on what John said before, those 16, really look at them in detail --

CHAIRMAN STETKAR: And -- and just -- these are just spring-loaded safety valves we're talking about here, so they don't have any control sensor-related stuff. They are just regular old mechanical safety valves.

MEMBER RAY: But they can still be either pilot-actuated or direct-actuated, but you're combining those two.

DR. GHOSH: So at this point, I am going to turn it back over to Randy for --

CHAIRMAN STETKAR: Yes. When are we going to talk about doors and seals?

DR. GHOSH: In the next --

CHAIRMAN STETKAR: Next? Okay.

DR. GAUNTT: The -- I am going to hit the next two slides. I will need some help with one of them on the cycle. And then -- and then Hossein has got a whole discussion on the doors on the ice condenser.

So like I said earlier, we -- we felt like it was prudent to include some more variability in hydrogen since hydrogen is such a -- such an operative phenomena in -- in the burns. And so we included two other popular competing kinetics correlations. I guess we will focus on the chart up here.

The Urbanic-Heidrick correlation has been the default used in -- in MELCOR for a long time, and we found it to be just fine in -- in all of the, you know, analyses of small experiments in Phebus and so forth. There are other competing correlations out there, and these are kind of amalgams of two. Usually you will see two -- two names here, and there's Cathcart and Pawel and Urbanic-Heidrick. Sometimes they are focusing on -- one guy is focusing on the lower temperature range of oxidation and -- and a little bit different expression of the upper range.

As you probably know -- maybe you know -- here -- here, around 1800 K, there is a phase change in the -- in the zirconium oxide. It is the -- the tetragonal versus cubic, which -- which supposedly facilitates the diffusion of oxygen into the cladding, so you see this jump in the -- in the oxidation range.

Those are kind of the features of it. Now you see some of these are slightly higher at the low end and slightly lower at the upper end of the low end. Others, the -- this is -- having trouble -- the Leistikov-Schanz has kind of a high end on the -- on the top.

Now how these play into the heat-up transient in the production of hydrogen, there is a bit of a horse race going on between oxidation rate and how quickly you -- you go through zircaloy melt and release the cladding, and then everything kind of shuts down at that point. So the way these kind of play into the sensitivities, for example, if you have a beginning of cycle core, it has got a lower decay heat, and that lower decay heat, it is going to take more time in this lower temperature range, and so it may -- it -- a lower decay heat core may be building more of their hydrogen at lower temperatures before they reach this sudden change in the kinetics.

And generally, all of them, when you reach this sudden change in kinetics, the heat-up rate goes from a few K per second to 10 or 12 K per second, and the duration between this phase change, kinetic, and actual melting of the cladding gets small, so they kind of interplay in fairly complicated ways, and we wanted to include some of those samples.

Here is the distribution on how we sampled them. It looks like we are favoring Leistikov-Schanz/Prater-Courtright, giving it 50 percent, but what I want to point out is that both of these, the Urbanic-Heidrick and the Cathcart-Pawel/Urbanic-Heidrick, are using the Urbanic-Heidrick correlation at the top end, and so they are kind of the same, and they differ very subtly in the -- in the lower end. So we -- we sort of have a 50/50 coin toss here between the Urbanic-Heidrick or Cathcart-Pawel -- they both use the Heidrick on the top end -- versus this Leistikov-Schanz.

This is how we sampled it. We wanted to introduce the additional fairly complex and subtle effects of these on the various transients. And just to show a -- a comparison point, I think CORA-13 was handy. There are lots of other experiments. CORA-13 is a fairly old experiment, but it shows you how the various models here compared up with -- with data. This is kind of interesting that the Leistikov-Schanz, you had the higher top end oxidation produced less hydrogen in the -- in the comparison to the experiment, and the Cathcart-Pawel and Urbanic-Heidrick here kind of look the same, so you have to speculate about why this -- why this produced less. It could be that because you have higher kinetics on the top end, you're spending less time before you go through melt, so it is -- it is all kind of a horse race thing.

MEMBER BLEY: As far as just looking at your curves, it looks like your two models kind of flatten out --

DR. GAUNTT: Yes.

MEMBER BLEY: -- at 500 -- 5000 seconds --

DR. GAUNTT: Yes.

MEMBER BLEY: -- forever, where the real data keeps going up and up.

DR. GAUNTT: The real data keeps going up, yes.

MEMBER BLEY: So if we get out to times like that, you don't believe --

DR. GAUNTT: I think we --

(Simultaneous speaking.)

DR. GAUNTT: -- I think we have some better comparisons with Phebus. I think CORA-13 was handy.

MEMBER BLEY: This is the Phebus data?

DR. GAUNTT: Yes, this isn't Phebus. This is one of these fresh-fueled, electrically-heated --

MEMBER BLEY: Oh, okay.

DR. GAUNTT: -- bundles that were done in --

(Simultaneous speaking.)

MEMBER CORRADINI: But I think the reason it jacks up -- the red line jacks up at 4750 is that is when they put water in.

DR. GAUNTT: Yes.

PARTICIPANT: This was also --

(Simultaneous speaking.)

MR. ESMAILI: -- and we don't have the physics for it, so there is some sudden burst of hydrogen that we cannot capture with our current model. But the importance of this CORA-13 is that it's also an international standard problem, so that's why we chose it, and this one was covered when Larry was here on April 18th discussing the model.

DR. GAUNTT: So that's all I am going to say about the kinetics at this point, and --

CHAIRMAN STETKAR: Randy, can I -- I ask you a favor? We have two members that need to depart for a short period of time. One of said members is interested in the ice condenser doors --

DR. GAUNTT: Oh.

CHAIRMAN STETKAR: -- so can we -- can we skip the next slide --

DR. GAUNTT: Yes.

CHAIRMAN STETKAR: -- and I do want to come back to the next slide. I've got some questions on it. But let's talk about the doors first so we --

DR. GAUNTT: All right.

CHAIRMAN STETKAR: -- make sure.

DR. GAUNTT: Go for it.

MR. ESMAILI: I thought there would be less discussion on the doors. But okay. So in terms of the containment model, what we have done is that we took into account the compartmentalizations of the containment at Sequoyah, as you can see on the -- on the graph, and we chose the necessary number of nodes to capture the important phenomena. And --

MEMBER CORRADINI: So this is where -- this -- he is talking about me. I am curious. I want to make sure I understand.

So you've got -- you've got -- you take the -- the ice chest and you break them into two 180-degree segments, one inner ring and outer ring, the way I see the cartoon. You have -- you have control volume, 31, 32, 33, 41, 42, 43, as if there are two half of the -- half of the dome connected laterally and horizontally, right?

MR. ESMAILI: Okay. So this is the -- this is the single loop.

MEMBER CORRADINI: Yes.

MR. ESMAILI: This is the single steam generator. This is the lower compartment. This is the triple loop, so we combined all the -- you know, so this is three of the quadrants, right? And so if ice chest -- these are the lower plenums, so they go -- they are symmetric. There are four of them in each quadrant, right?

MEMBER CORRADINI: Okay. So you combine three of the ice chests?

MR. ESMAILI: No no no no no. We combine the -- this part, the lower compartment here. We only have two control volumes for this in the lower compartment, and the way they are going into the ice chest, then we divide that into the number of doors. But the ice bed itself is symmetric in the sense that all these volumes are the same. There are like four that are going across the --

MEMBER CORRADINI: But the way -- the way you have it in the cartoon -- this is where I got confused. The way you have it on the cartoon at the right is -- is if I have got four of them in containment. The way you have them on the left is it's if they are split at 180 degrees because --

(Simultaneous speaking.)

MR. ESMAILI: This is not showing everything.

MEMBER CORRADINI: Oh.

MR. ESMAILI: So this is -- this is only showing 42 and -- okay, so yes. Actually, this is showing everything. This is showing these quadrants, right? So 31, so these are these, right?

MEMBER CORRADINI: Yes.

MR. ESMAILI: 41 are these, and this one. These around here are 51 and 61. But the important thing is that we are breaking up into four regions, right?

MEMBER CORRADINI: Okay.

MR. ESMAILI: So the ice -- so the -- so this annular region of the ice bed itself, okay, it is -- it is made into four segments.

MEMBER CORRADINI: Four 90-degree segments?

MR. ESMAILI: Four 90-degree segments.

MEMBER CORRADINI: Okay.

MR. ESMAILI: Okay? The same is true --

MEMBER CORRADINI: Two of the 90 degrees feed into one 180-degree upper plenum.

MR. ESMAILI: Two of them going to the upper plenum, and -- and the other two going to the -- to the upper plenum right here. Right.

MEMBER CORRADINI: And you took the lower ones and combined three of them?

MR. ESMAILI: Combined three of them, just because we have -- we have, you know, three steam generator -- they have done other studies. They have done other nodalization, as I was going to say in support of GSI-189 and direct containment heating issue. It doesn't show it is -- it is very, very sensitive as long as you have this minimum number of nodes --

MEMBER CORRADINI: Okay.

MR. ESMAILI: -- and you are -- you are capturing the behavior of these lower plenum doors, the intermediate doors, and these upper plenum --

MEMBER CORRADINI: Okay.

MR. ESMAILI: -- doors. But the important thing was that we had to break up the ice chest itself into actually at least three number of actual nodes to be able to capture, you know, the -- the conditions inside the -- the ice. So does that answer your question?

MEMBER CORRADINI: Yes. You can get back to the doors.

MR. ESMAILI: Okay. So the low -- so the lower plenum doors, there are 24 double doors, total of 48. Okay. There are 24 double doors, correct? Or -- yes, 24 double doors. Okay, I got -- and so these doors are normally closed during operations. They have a negative pressure on the ice itself, so they remain closed, but during the accidents, they can open, and they open very, very easily.

So what you see in this picture is that it requires about 47 pascal can actually just push them and just open -- yes, can blow on them and open, and they open fully, so they are very, very easy to open. So when they looked at it in the past studies was that when -- when they -- when they fully open, when they go past this 47, they have crushable hinges, so these hinges can break. So once they go fully open, they can just remain open. But there is so much uncertainty in how these doors behave.

CHAIRMAN STETKAR: Let me stop you there because I went back and I reread the NUREG that you cite, NUREG/CR-5586, and everything -- you say there's a lot of uncertainty. In that NUREG, they did three nominal sensitivity studies. What would the world be if I assumed this? That's a sensitivity study. It doesn't say anything about reality.

MR. ESMAILI: Right.

CHAIRMAN STETKAR: Their sensitivity study was suppose the doors remained fully open. Suppose that 50 percent of the doors remained open, or equivalently, each door remained half-open, whatever you had to have; and suppose none of the doors remained open. Those are --

MR. ESMAILI: Reversible.

CHAIRMAN STETKAR: Yes, that they were

reversible, that is right. Those are "what if" studies.

The -- the subject matter experts said they believed that the doors remaining -- all of the doors remaining fully open was the most likely condition, and that is what they used for their base case analysis in that particular study. And in fact, in the appendix to that study, and we brought this up last year, there is evidence that those -- there is -- there is a statement that says reference TVA 88 states that once the doors are open during a design basis accident now -- this is just a normal design basis accident -- the door hinge assemblies are designed to deform, preventing reclosure. So that tells me they are designed to be fully open.

MR. ESMAILI: They are designed to be fully open.

CHAIRMAN STETKAR: Yes. So now we questioned last year your use of a uniform distribution anywhere from 0 to 100 percent, so you have changed that such that now, it is a uniform distribution between 50 and 100 percent.

MR. ESMAILI: 50 and --

CHAIRMAN STETKAR: Why?

MR. ESMAILI: -- 100 percent.

CHAIRMAN STETKAR: If there's a very, very high likelihood that the things behave as designed, why isn't there a very, very high conditional probability that the open fraction is 1, with a small probability that it is less than 1? In other words, something that looks more like the shape of the distribution that we were talking about for the failure to close area fractions rather than -- than what is now what you show here?

MR. ESMAILI: Again, this is -- I go back to -- I know what you read, and I read --

CHAIRMAN STETKAR: And it's --

MR. ESMAILI: -- the same --

CHAIRMAN STETKAR: -- not --

MR. ESMAILI: -- thing.

CHAIRMAN STETKAR: -- uncertainty. Now don't confuse uncertainty with the way equipment behaves with "what if" things that people did in a sensitivity study. What if -- what if a meteorite crashed through this roof right now? That's a sensitivity study.

MR. ESMAILI: But my takeaway from that discussion was that they went to TVA, they asked the same questions, and they said that, yes, it is going to -- it is going to fully open, but they -- they were not sure exactly how these doors are going to behave under accident conditions.

CHAIRMAN STETKAR: But -- but they did a -- they did a "what if" sensitivity study. That doesn't mean that the people who wrote that report said they believed the doors were going to open. The people cited the design of the doors. They did a sensitivity study the same way that you do sensitivity studies for a lot of things with parameter values that you know are absurd. You just want to test a sensitivity. It doesn't mean that the authors of that report gave any credence whatsoever to either a fully reversible mode or a -- any probability that they -- that half of the doors would stick open.

MR. ESMAILI: Yes. I --

CHAIRMAN STETKAR: So it is --

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: And that is why now, in terms of our trying to capture uncertainty in this study is why I am questioning this new distribution. Is it consistent with what we understand about the design operation of those doors and the information from that previous study? It doesn't seem to be.

MR. ESMAILI: Okay.

MR. HAQ: My name is Salman Haq. I am part of the -- the Sequoyah team.

We went to the -- we went to the Watts Bar, and we looked at these doors, and we asked this question. So what happened is, for design basis accident, you have a certain pressure spike that comes in, and so the chances are these doors going to open and stay open.

In our case, the containment is pressurizing slowly, and so these doors would not fully open. It might just hinge open and then may come back open again. Whenever there is a hydrogen spike, it is certain that it's going to open and deform the hinge, but the rest of the time, we -- we played with those doors because the Watts Bar Unit 2 was under construction, so that is why we concluded that there is a possibility that these -- not all of the doors are going to open, and the hinges are going to behave as if it's a design basis accident.

CHAIRMAN STETKAR: Hossein, you want to -- you want to try to correct that?

MR. ESMAILI: So -- so the way -- the way these doors are going to operate right now, just let -- let me state what happens during the --

CHAIRMAN STETKAR: Let me -- let me make sure --

MR. ESMAILI: I agree with you that --

CHAIRMAN STETKAR: -- I -- let me -- let me --

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: -- make sure if I understand the model. The model says that they will remain reversible --

MR. ESMAILI: Right.

CHAIRMAN STETKAR: -- until you get a large enough pressure that --

MR. ESMAILI: That you pass that --

(Simultaneous speaking.)

CHAIRMAN STETKAR: -- that opens them fully --

MR. ESMAILI: Right.

CHAIRMAN STETKAR: -- and then this probability --

MR. ESMAILI: Right.

CHAIRMAN STETKAR: -- distribution --

MR. ESMAILI: Right.

CHAIRMAN STETKAR: -- kicks in. Okay.

MR. ESMAILI: Correct. And so -- so, and as you can see, to tell you honestly, you know, you're right. Maybe if we go back, we are going to put more weight on more of it is open. As a matter of fact, the reason we went to this 50 percent is because of a discussion that we had especially with you saying that why are you putting zero? So we just said, okay, let's capture some of these sensitivities through the uncertainty analysis.

But what happens -- the way these doors behave is that -- for this station blackout -- is that they are not going to get fully open until you have this hydrogen. So even when the rupture disk opens on the pressurizer relief tank, these -- these valves are going to open a little bit. As a matter of fact, some of the cases that you will see is that when the -- when the pressurizer is cycling, these doors are opening and shutting like this also, so they never open --

CHAIRMAN STETKAR: Yes. I didn't see any plots where you looked at these --

MR. ESMAILI: You don't --

CHAIRMAN STETKAR: -- doors --

MR. ESMAILI: -- see it --

CHAIRMAN STETKAR: -- so yes.

MR. ESMAILI: -- but we have more information.

CHAIRMAN STETKAR: Yes, I am sure you do.

MR. ESMAILI: So -- so this is how it behaves. They don't -- they don't fully open until you get to that hydrogen combustion that opens these doors fully. And -- and so at that point, basically you have a big heat source, so even if they don't crush -- get crushed open, there is enough pressure, there is enough hot material in the -- in the lower compartment that could push these doors open, and that is why, when you see the results, they are not very very sensitive, in our analysis, at least, to the operation of these lower plenum doors, because they are either going to get crushed --

CHAIRMAN STETKAR: Well, that is --

MR. ESMAILI: -- open or --

CHAIRMAN STETKAR: -- that is -- yes --

MR. ESMAILI: -- they're going to go open.

CHAIRMAN STETKAR: Okay. I didn't see any sensitivity, but -- but seeing no sensitivity because it might have been suppressed is one thing.

MR. ESMAILI: Right. We didn't do the sensitivity because we thought that, you know, we just -- basically, what you just said. They -- they said that this is possible. All the doors can be behaving even under direct containment heating when you're putting, you know, debris into the thing. It might be fully reversible, and 50 percent might get crushed depending on which --

CHAIRMAN STETKAR: Well --

MR. ESMAILI: -- doors --

CHAIRMAN STETKAR: -- but --

MR. ESMAILI: -- are going --

CHAIRMAN STETKAR: -- but --

MR. ESMAILI: -- to --

CHAIRMAN STETKAR: -- again, who said that was possible? I didn't read anything that anybody said it was possible. I read a --

MR. ESMAILI: The --

CHAIRMAN STETKAR: -- sensitivity --

MR. ESMAILI: -- the sensitivity, correct.

CHAIRMAN STETKAR: But that is not --

MR. ESMAILI: And I don't have the reference for that discussion when you're looking at -- you know, when you're looking at that reference, TVA 88. I don't have --

CHAIRMAN STETKAR: I -- I don't -- I couldn't find that --

MR. ESMAILI: I couldn't --

CHAIRMAN STETKAR: -- either --

MR. ESMAILI: -- find it --

CHAIRMAN STETKAR: -- obviously --

MR. ESMAILI: -- either --

CHAIRMAN STETKAR: -- but it is --

MR. ESMAILI: -- I asked --

CHAIRMAN STETKAR: -- cited in the NUREG that --

MR. ESMAILI: It is --

CHAIRMAN STETKAR: -- you use --

MR. ESMAILI: It is cited there. I asked --

CHAIRMAN STETKAR: -- in the appendix.

MR. ESMAILI: -- I asked the people who were there at the time. They didn't have that --

CHAIRMAN STETKAR: Yes.

MR. ESMAILI: -- information, and so that information is lost, and -- but I just went with what they said in terms of they -- not knowing exactly how these doors might behave once they fully open. And we kind of tried to capture that uncertainty here, and that sensitivity here, as part of the uncertainty --

CHAIRMAN STETKAR: I wonder --

MR. ESMAILI: -- knowing that --

CHAIRMAN STETKAR: I wonder how -- and this is speculation -- I wonder how the -- TVA must have a PRA for the Sequoyah plant. I wonder how they treat them in their PRA, unless -- unless they don't worry about Level 2. It might just be a Level 1 --

(Simultaneous speaking.)

MR. ESMAILI: Yes, or it might not be that important. Again, as I said, that, you know, these doors are just so easy to open with 40 pascal, you know. I mean, it is just very difficult even to model 40 pascal, 46, 47 pascal, the doors open. So when they get crushed open, or are they just going to open because now you have a source of hot material in the lower compartment?

So yes. I mean, if you go back and -- and change it, then we would be criticized I guess for why you didn't consider the other, you know? What was --

CHAIRMAN STETKAR: Well, but I --

MR. ESMAILI: -- why are you using uncertainty analysis? You know, why didn't you consider this other probability? You are right. Maybe it shouldn't be a uniform. Maybe it should be a -- you know, a --

CHAIRMAN STETKAR: Well, again --

(Simultaneous speaking.)

CHAIRMAN STETKAR: -- you know, from an engineering perspective --

MR. ESMAILI: I agree.

CHAIRMAN STETKAR: -- the -- the shape of the probability distribution, for example for the --

the safety valve stuck open area fraction, seems qualitatively, from an engineering perspective, reasonable.

MR. ESMAILI: Sure.

CHAIRMAN STETKAR: You know, I would challenge what the actual probabilities are. Here --

MR. ESMAILI: I agree.

CHAIRMAN STETKAR: -- it is not clear that this shape -- this shape has improved from -- from the straight uniform probability of last year.

MR. ESMAILI: We are rating it, the 50 percentile, we are rating it the same as we are rating the fully open --

CHAIRMAN STETKAR: Right.

MR. ESMAILI: -- and -- and you are right. If -- if, you know -- again, because we -- we cannot find all those references -- we went back and looked -- we don't know exactly how these doors behave, so we just -- we just left that sensitivity as part of the UA.

CHAIRMAN STETKAR: Well, Mike is here because he has been trying to get a word in edgewise.

MEMBER CORRADINI: John -- John is comprehensive. I just wanted to make sure: 50 to 100 percent really doesn't matter, is what -- I was waiting for you to say that.

MR. ESMAILI: Yes.

MEMBER CORRADINI: Okay.

MR. ESMAILI: So this is the -- the area of these doors is about 80 meters squared, so when you open 50 percent of them, it's about 40 meters squared. So you are -- you are relieving whatever the --

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: -- pressure is.

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: Right.

CHAIRMAN STETKAR: That's the important information that I wasn't necessarily getting out of anything that I could see --

MR. ESMAILI: Yes --

CHAIRMAN STETKAR: -- so --

MR. ESMAILI: -- yes. So -- so there is a large open area with 50 percent, 40 meters, compared to the normal, you know, leakage that's around 22 meters squared that is going to the ice. So -- so we were not, you know --

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: -- we didn't spend too much time trying to --

MEMBER BLEY: Before you leave ice, the

doors are one thing. The ice is another. And I didn't see any discussion about the ice in the parameters under which you consider uncertainty. I know years ago, there was great surprise when we had sublimation and some channeling possibilities and some resettling and changing the configuration. I know they change the ice at routine intervals now to minimize that effect. I don't know how big an effect it remains, and it would seem to me it could have substantial impact on at least the rate at which you get cooling --

MR. ESMAILI: Yes.

MEMBER BLEY: -- passing through the ice. You don't address that uncertainty at all, and I wonder why.

MR. ESMAILI: Well, we didn't -- we didn't consider -- if you're talking about the mass of ice that is --

MEMBER BLEY: Well --

MR. ESMAILI: -- that is sitting --

MEMBER BLEY: -- and how it is actually configured inside: how much surface area is exposed, has it resettled, has it -- you know, the ice changes form over years. They thought when they built these that they would fill it full of ice and keep it cold, and they didn't -- I remember talking to Westinghouse.

People said well, it is ice, so we didn't go out and get any real experts in ice. And sure enough, over time, with sublimation and -- and it reconfigured itself, and they were getting channels and blockages such that the damn stuff wouldn't work the way it was expected to work.

And now I know they changed the ice. I don't know what intervals. But between when they put fresh ice in and when it is time to put fresh ice in again, there must be some substantial changes, and I don't know if we thought about that and if it makes a difference.

MR. ESMAILI: Ice, we did talk -- we did think about that a little bit, but -- but what happens during the accidents -- let me see if I -- if this answers your question -- is that this ice, by the time you have the rupture disk open, you know, you have the flow of the gases going to the ice bed, you don't -- you don't really melt much of the ice. But -- but during the core degradation, you're passing a lot of hot gases through the ice, and this -- the ice that we have right now, it takes about 12 hours to melt all this ice.

So -- and during this time, when you look at the containment pressure, if you see in the report that containment pressure goes up slightly, and then it kind of hangs around until the ice melts and then starts taking off after --

(Simultaneous speaking.)

MEMBER BLEY: Let me do a reductio ad absurdum. Suppose this ice had settled and compacted such that we had a solid ring somewhere in there of ice such that the steam couldn't pass up through it until it gradually melted from the bottom through. That must -- that would probably make an enormous difference in the -- at least the rate of pressure change in the containment. And we are not worried about that, I guess. I am not sure --

MR. ESMAILI: Again --

MEMBER BLEY: -- why.

MR. ESMAILI: -- because the ice is melting over this time period of --

MEMBER BLEY: Under the assumption that you have --

MR. ESMAILI: Yes, under the --

MEMBER BLEY: -- flow through the ice.

MR. ESMAILI: -- assumption that we have -- yes. And this is --

MEMBER BLEY: Probably uniform flow through the ice. I don't know exactly what your model

--

MR. ESMAILI: Well, we are modeling -- we are actually modeling here, we are modeling the lower plenum doors, and this is the lower plenum doors, so this is where the lower plenum --

MEMBER BLEY: I know.

MR. ESMAILI: -- ice --

(Simultaneous speaking.)

MEMBER BLEY: The doors open, right?

MR. ESMAILI: But the doors --

MEMBER BLEY: I want to --

(Simultaneous speaking.)

MEMBER BLEY: -- forget --

MR. ESMAILI: -- but this is --

MEMBER BLEY: -- about the doors.

MR. ESMAILI: -- open, so -- so we are correctly calculating what the flow area is, and -- and the flow area --

MEMBER BLEY: Well, you don't -- what I am saying is --

MR. ESMAILI: Yes, I know, I know.

MEMBER BLEY: -- you don't know the flow area through the ice.

MR. ESMAILI: You are saying that there are uncertainties in these flow areas because --

MEMBER BLEY: Yes.

MR. ESMAILI: -- of --

(Simultaneous speaking.)

MEMBER BLEY: And they were significant enough that people are changing out the ice because of worries that it wouldn't have worked. That was 30 years ago, maybe more. But it is -- we're -- we're doing a study. We're trying to be state of the art, and we're trying to look at all the uncertainties, and one of our members isn't here who would probably ask things about chemistry and other things that I can't ask, and I don't know that we are considering all of those uncertainties, but this is one that just occurred to me, and it kind of sounds like you have not thought about it.

DR. GHOSH: Well, can I just -- I think we did have discussions --

MR. ESMAILI: Right.

DR. GHOSH: -- in fact after the May meeting, and maybe it was you who asked the -- the question --

MEMBER BLEY: I don't think so, but --

DR. GHOSH: Somebody --

MEMBER BLEY: -- maybe.

DR. GHOSH: -- asked about the ice mass,

and maybe the geometry was also discussed, and so we thought about it a little bit. I think Hossein kind of walked through some of the -- the thinking we did in terms of our accident progression. But I remember in grad school, back in grad school, now a couple decades ago, you know, when we were learning --

MEMBER BLEY: It can't have been that long.

DR. GHOSH: When we were learning about all the different containment types, you know, it was brought up that when this containment was first designed, you know, it's this grand design, and then there are all of these issues that came up, and from what I understand, they had to come up with a -- a new maintenance regime to make sure that the -- and now they --

MEMBER BLEY: That is true.

DR. GHOSH: -- switch out the ice to make sure that it will perform its designed function. So --

MEMBER BLEY: But it is aging over that period of time --

DR. GHOSH: It is -- I --

MEMBER BLEY: -- so --

DR. GHOSH: -- think --

MEMBER BLEY: -- it must be going from what we expected in the original design, which is probably what we're modeling, to something in some degraded form.

DR. GHOSH: Right. So --

MEMBER BLEY: And --

DR. GHOSH: -- I think the --

MEMBER BLEY: -- I don't know how much that degradation is, but I don't know if you do either.

DR. GHOSH: Right. But I think that extreme case of -- of the reconfiguration completely blocking the flow paths or something, that must be part of the --

PARTICIPANT: Well --

DR. GHOSH: -- maintenance program to make sure that doesn't happen --

MEMBER BLEY: Of course you are right.

DR. GHOSH: Yes.

MEMBER BLEY: But we are worrying about -- you just showed us uncertainties in hydrogen generation that are pretty minuscule. This might be bigger. I don't know.

MR. ESMAILI: Yes. So -- so obviously, they want material to go through the ice beds, but they don't want to really obstruct --

MEMBER BLEY: Yes.

MR. ESMAILI: -- the flow of gases even during design basis to go through this. The other thing is that, you know, this ice is melting. One way or another, as you're putting, you know, hot debris into there, it is going to melt. So it is going to change -- the melting of the ice itself is going to change the dynamics of what is happening inside. You know, this flow area --

MEMBER BLEY: And that is true too.

MR. ESMAILI: -- it is -- so what we -- so we don't, you know -- we didn't consider that. What we are doing is that as the ice is melting, we are not considering that the flow area, et cetera, is changing, correct?

MR. HAQ: That is true. So let me add --

CHAIRMAN STETKAR: Oh --

MR. HAQ: -- let's not forget that --

CHAIRMAN STETKAR: Oh, identify yourself --

MR. HAQ: Oh --

CHAIRMAN STETKAR: -- again.

MR. HAQ: -- Salman Haq again.

CHAIRMAN STETKAR: Thank you.

MR. HAQ: Let's not forget that these are ice baskets, and they are about nine inches or so, hanging 12 feet in the air, and the space around them is also about nine inches. So 50 percent is open area anyway.

MEMBER BLEY: So you modeled it as blocks of ice in the baskets?

MR. HAQ: Yes, so --

MEMBER BLEY: With only --

MR. HAQ: -- these are --

MEMBER BLEY: -- the --

MR. HAQ: -- shaved --

MEMBER BLEY: -- outside --

MR. HAQ: -- ice --

MEMBER BLEY: -- area exposed.

MR. HAQ: They are shaved ice, yes, and so there is little chance that it would block the entire flow of steam around them. It is quite possible that the porosity of the ice changes, and it might impact the -- the timing slightly. We didn't consider it to be huge. We talked about it, and I also asked, because I heard somewhere these are ice pellets, but then said no, they are shaved ice, which does get some pelleted, some frozen. There were bigger problems about it dropping or actually re-freezing up on the floor, which was the -- the open door problem, which we resolved by the way we modeled it.

But we never -- nobody thought that it would prevent the steam. Now, we do know that there is a tech spec change where they have found some reasons to go revise the total quantity of ice, and Sequoyah and some plants are doing that, increasing the amount of ice as it was required originally in the -- in the tech spec. Well, they are changing that amount to be a little bit higher, and we evaluated that also. And I am not sure if we changed the number or whether we in the end decided it's not going to make a significant difference, because all the ice is gone in 12 hours. And so it will probably add a few minutes if that is really what is needed.

So we thought about that, and I don't think that the ice blockage would completely block the -- the flow of steam or would have significant impact.

MEMBER BLEY: Of course, I don't either, and that is why they change out the ice. But the rate at which things happen could change, and I just wonder --

MR. HAQ: Right.

MEMBER BLEY: -- how that uncertainty compares with some of the ones you spent a lot of work analyzing.

MR. ESMAILI: Well, Casey, do you have something?

MR. WAGNER: Casey Wagner, dycoda. I wanted to add that we had some plant data, so after every outage, they made an estimate, or they weighed how much the ice was there. And so we have I think about ten years' worth of that, and so the procedures that stem from the problems that they had 30 years ago address a lot of those things, and there wasn't much change in the mass from year to year, or they made corrections. But I remember those numbers being fairly uniform as they sealed it up and -- and started the outage.

MR. ESMAILI: We did -- we did think about doing --

MEMBER BLEY: Okay.

MR. ESMAILI: -- sensitivity because --

(Simultaneous speaking.)

MEMBER BLEY: That helps -- that helps a little more. But yes, it is just I am wondering where you put a lot of effort versus --

MR. ESMAILI: Yes.

MEMBER BLEY: -- where it appears you put no --

MR. ESMAILI: Right.

MEMBER BLEY: -- no effort, but you put a little more than I thought.

MR. ESMAILI: Right.

MEMBER BLEY: I didn't even see a hint of it when I --

MR. ESMAILI: I think it is discussed in the report that we --

MEMBER BLEY: Is it?

MR. ESMAILI: -- looked at the data for that.

MEMBER BLEY: Didn't -- didn't make it into the -- the tables of what things you --

MR. ESMAILI: Oh --

MEMBER BLEY: -- looked at.

MR. ESMAILI: -- okay.

CHAIRMAN STETKAR: No, there's not --

MR. ESMAILI: All right. We will go back and check.

Okay. So anything else on this, or --

CHAIRMAN STETKAR: I don't think so, unless anybody has -- I am not going to let you off the hook before we take a break. There you go.

DR. GAUNTT: Okay. The last uncertainty component that we added to this -- this UA is to account for time-in-cycle. And I am -- since I shipped several of my guys out here to support, I will probably call on a lifeline to get a little more explanation here.

But here you see the decay heat curves for three values of life burn-up on a core: beginning of cycle, middle of cycle, end of cycle. And you can see -- I am going to let others talk about how we selected the distribution on -- on sampling those, but you can see based on the origin analysis of each of those cases of 18 gigawatt-days, 25, and 38 gigawatt-days per ton how the cesium inventory builds and how the iodine inventory builds.

And the cesium, as you expect, kind of just builds linearly as a function of -- of burn-up. And the iodine also builds a little with burn-up. It is a combination of secular equilibrium of production and decay as well as some other isotopes that are coming in from -- from other decays.

It -- it looks at first blush like the middle of cycle and end of cycle ought to be giving about the same result, but we actually see some sensitivities between those two cases, and they are kind of what you expect in terms of more decay heat associated with the end of cycle, and that will show up in some of the other dot charts and things that we're going to barrage you with.

The beginning of cycle shows some real

outliers. When we look at those, they are lower decay heat, and many of the -- actually, all of the beginning of cycle cases failed to produce a containment rupture within 72 hours. They were on a trajectory, but it could have been well beyond 72 hours because of that reduced decay heat, so that will -- that will be apparent in some of the cases that we looked at.

So in our sampling, we included -- this is the breakout -- we included roughly half of the realizations. This is middle of cycle, 300 samples of middle of cycle. And then we included 230 end of cycle cases and 69 in the category of the beginning of cycle.

Much above the -- the break point there between beginning of cycle and middle of cycle, and the blue curve starts to approach the green curve more. So those are -- those are the -- the decay heats we sampled, and I would like to ask my crew to explain a little bit more the rationale for why did we select 300 realizations of --

CHAIRMAN STETKAR: Well --

DR. GAUNTT: -- middle of cycle.

CHAIRMAN STETKAR: Remember, realizations are simply samples. What you did and what I want to understand is you said that out of the nominal 550-day period, 275 of those days are middle of cycle; 221.5

of those days are end of cycle; and 62.5 of those days are beginning of cycle. And when you do random sampling of that distribution, lo and behold, of 600 you get 300, 231, and 69.

DR. GAUNTT: Yes.

CHAIRMAN STETKAR: So I confirmed that Monte Carlo is working like Monte Carlo should work.

What I want to understand is why did you use this bizarrely skewed distribution of lengths and time? Because that's important because you do look at the results, all of these dots and lines and all the stuff that you're going to tell us. There are statements in there that says, well, you know, some of the beginning of cycle things came close but, you know, we didn't have very many samples in there.

Well, the reason you didn't have very many samples in there, you skewed the whole problem so that you wouldn't get very many samples at beginning of cycle.

DR. GAUNTT: All good questions.

CHAIRMAN STETKAR: Okay. So why did you parse up the world the way that you did?

DR. GAUNTT: We talked about that yesterday. And Doug's going to help out.

DR. OSBORN: This is Doug Osborn, Sandia

National Laboratories.

The purpose for unskewing it, in part for the source codes as well. You know, so this wasn't just the decay heat period. We're also taking into consideration the variances that we would also seek for the max analysis on the source code side. Because one of the things that this feeds into is for the consequences.

And we were purposeful in skewing it the way it was in that we could see a noticeable difference in the beginning of cycle. But we didn't want to have it essentially equally weighted to middle and end of cycle, burn-ups and source terms as well. So we were just trying to at least start to investigate, which is why when we skewed it like this we picked the bottom, basically the bottom 10 percent of that cycle and the top 10 percent on the cycle.

CHAIRMAN STETKAR: Let me. There's two things we're talking about here. Number one is I don't care what day you pick. I don't care that you pick 6.25 days to represent beginning of cycle and 528.75 days. That's what you pick.

DR. OSBORN: Uh-huh.

CHAIRMAN STETKAR: I care about the likelihood in a random sampling process that you pick that 6.25 days, and the likelihood that you pick the 528.75 days, and the likelihood that you pick that 200 days.

So why did you divide the whole 550 days into those three very different chunks of time? Why didn't you divide it equally, for example? Why did you divide it so the middle of cycle was 80 percent of the time or some other arbitrary thing?

MR. ESMAILI: So the way it worked was that we added this timing cycle a little bit later. So we started running the calculation for the middle of the cycle so we'd have the data at 200 days.

CHAIRMAN STETKAR: So that's why the middle of the cycle is 200 rather than 275, which would -- that's what we missed.

MR. ESMAILI: That's right. So we had the data at 200. Because we chose 200 as the day for the middle of the cycle. We had the data. We had done the origin calculation. So once we fixed that 200, 200 days, then half of the 275 days, which is half of the realization, so 300 of the realization --

CHAIRMAN STETKAR: Don't talk about realizations.

MR. ESMAILI: I know.

CHAIRMAN STETKAR: I don't want to talk

about real -- realizations are samples.

MR. ESMAILI: All right.

CHAIRMAN STETKAR: So why did you pick 275 days out of 550 and call that middle of cycle? Why didn't you pick 400 days and call it middle of cycle?

MR. ESMAILI: I think because we started running the calculations and --

DR. OSBORN: The Sequoyah burn-up was originally calculated well over a decade ago for another cycle.

MR. ESMAILI: No, no, no. He's ask -- Dr. Stetkar's asking why is the right side range, so --

CHAIRMAN STETKAR: Okay. I, first of all, I don't care, I don't care that you call the middle of cycle data point at 200 days because you have all of these really precise calculations. And I don't care that you call the end of cycle data point 528.75 days, which is really precise. And I don't care that you call the beginning of cycle data point 6.25 days.

You could have called those data points those three names regardless of the amount of time, calendar time now, that you assigned to each of those three discrete intervals. The amount of calendar time that you assign affects the number of samples. It affects the fact that you get 69 samples of the thing that you called beginning of cycle.

If you had assigned instead of 62.5 days, calendar days, if you'd assigned 10 days, you would have had very, very, very few samples. If you had assigned 200 days you would have had more than 69 samples.

So I'm asking you what is the rationale for dividing up 555 calendar days the way that you did? And don't confuse it with the things, those three blue dots on this slide. I don't care about the three blue dots.

MEMBER CORRADINI: John does it comprehensively. My simple question is why isn't it just a third, a third, a third?

CHAIRMAN STETKAR: That would be one notion.

MEMBER CORRADINI: Justify why it isn't that.

MS. SANTIAGO: Tina.

DR. GHOSH: Okay. If we take a step back. And philosophically, you know, if we had the computational path we could, we could have generated 600 inventories to span radium required inventories and decay for a span, times zero. You know, you just started and you had the accident, all the way to your end of cycle and you have it.

I think philosophically what we were

trying to do, so for practical reasons we can't have 600 inventories and decay points. We picked three points that were different enough that we can see the effect of having something close to beginning of cycle versus end of cycle. We always have the middle of cycle.

And then, you know, given that we can't do all 600, we were trying to pick the point in that 550-day timeline which, if we had these three blue dots, all right -- we have to still talk about the blue dots -- if we have to pick those points, at what point in the timeline do you start to look in terms of the things we care about, which is the decay heat curve and the inventory, more like the middle of cycle inventory that we're going with versus the beginning of cycle. And similarly for the other side.

So, basically, what we, unfortunately, had to discretize this continuous, you know, distribution into these re-bins. And we tried to figure out qualitatively at what point in the time cycle you look more like the other bin, you know, to start --

MEMBER BLEY: Can I ask a question that will help me? I don't know if it will help them.

Looking at the decay heat I can clearly see the difference between beginning of cycle and the other two. The other two are pretty darn close.

But what I don't have a good feel for is, is the inventory substantially different between middle of cycle and end of cycle?

DR. GHOSH: We have that at the bottom.

MEMBER BLEY: Oh, it is. There it is, yeah. And I looked at that. I did look at that.

MR. ESMAILI: I think that --

MEMBER BLEY: You know, and those are points where you have the data. I don't have the same issue you do. But if you wanted to skew the sampling, people do that, too. But I don't --

MR. ESMAILI: This is a very good point that you're raising. And this is what, exactly what we did when we looked at the spent fuel. You know, we broke it up into number of --

CHAIRMAN STETKAR: Hossein, don't. Why -- this is a question. I want an answer from the people who did the work. Why did you not divide the cycle into three equal portions?

MEMBER CORRADINI: I think she answered that. I think she answered it in a way that --

DR. GHOSH: Sixty days, yeah.

MEMBER CORRADINI: Can I say it back though, Tina.

DR. GHOSH: Yeah, yeah.

MEMBER CORRADINI: What you're really saying is, I thought you said it -- and let's just pick 300, or 600. I can't do 575 correct in my head.

But if there were 600, you need to do 200, 200, 200. You're saying since you had data at 6.25, assigning 200 of these with that inventory would be over-skewing it because it was too early in the 600 days. And that's what I think you said to me.

DR. GHOSH: Yes.

MEMBER CORRADINI: So you basically tried to -- hang on -- you basically tried to bias it, I'll use the word bias, you're trying to bias it so that whatever was getting in the cycle it was close enough to 6.25 that you didn't feel like it was physically stretching it too long to what it would have been in a middle of cycle representation.

That's what I thought you said to me.

DR. GHOSH: Yeah. Because otherwise you can't pick up on the differences between -- the meaningful differences that you get in the beginning of cycle. And it is skewed. It's not a third. A third of the way in you look more like the rest of the cycle than the beginning.

MEMBER CORRADINI: Right.

DR. GHOSH: So, in theory we actually had done a third, a third, a third. But just --

CHAIRMAN STETKAR: Don't confuse it. I want to talk about this.

DR. GHOSH: So we thought about it more. We really thought about it this time. And we tried to make it more representative for the physical reasons that we care about.

CHAIRMAN STETKAR: Tina.

DR. GHOSH: Yes.

CHAIRMAN STETKAR: You had data I know, old historical data for 200 days. There's some sort of history. Did you in the last year create the characteristics for 6.25 days and 528.75 days?

DR. GHOSH: Yeah, we did.

CHAIRMAN STETKAR: Okay.

DR. GHOSH: I think that we're starting to --

CHAIRMAN STETKAR: That's fine. That's fine. I just want to establish that.

DR. GHOSH: Yes.

CHAIRMAN STETKAR: You did that because you said, well, I'm going to take 62.5 days to represent beginning of cycle. And I'll nominally take 10 percent of that calendar time chunk and then fix that at 6.25

days and establish the conditions there. So the 6.25-day blue point was backed out from something that you established in the calendar time.

Similarly, the 528.75 days was backed out from something that says I'm going to take 212.5 days, call that end of cycle, and take a 10 percent backwards look from that, or 90 percent forwards look. And that came out 528.75 days.

So the two ends of the blue points, again, were derived from the way that you've divided the calendar time. They weren't things that you had as let me call them casting stone anchors, the same way as the 200-day point was.

DR. GHOSH: Yes. I think that's --

CHAIRMAN STETKAR: So, again, that's a little different from the way you characterized it. They didn't have the data.

MEMBER CORRADINI: But I think -- Yeah, I understand that. But I guess, I guess they had a thought process.

What I'm trying to get at is -- and I thought this was what you were going to say -- there's a non-linear effect here that days is not the proper measure. So what is the proper measure? It is the accumulation of all the fission products? Of a certain key fission product? What is the measure that made you anchor it the way you did?

That's what I thought you were going to get to.

DR. GHOSH: Yeah.

MR. ESMAILI: It depends. For MELCOR it's that decay heat that we are interested in. The difference is the decay heat is what's driving and making differences.

MEMBER CORRADINI: But you also said that you wanted to be consistent. So once you picked it here, you picked it also with the fission products. So you --

MR. ESMAILI: Yeah, yeah.

MEMBER CORRADINI: Okay.

MR. ESMAILI: So I'm just saying that this, this kilograms, that this does not directly affect, you know, my MELCOR calculation. It's more driven by how much decay we have. But once we pass it on to the next to do the consequence, then this becomes important at, you know, how much inventory you have at different, at different points in time.

DR. OSBORN: So you're right, the first anchor was 200. And then we went down to the VOC one. And once we set that one, because it was sufficiently different in decay heat, then in order to represent the EOC we did the exact opposite on the upper.

MR. ESMAILI: So we pinned that 200 days, this is 200 days, started running those 300 realizations, 300 calculations. The moment you do that, it's not going to be one-third/one-third, because you have to go from 62.5 to 337.5 is 275. That is half of it, just like the 300 is half of 600.

So then it leaves out the beginning of the cycle and the end of the cycle. And because we've already established 62.5 and 337.5, then you know exactly how many, how many samples you need to take from those ends.

MEMBER CORRADINI: So not to beat the dead horse, but again I'm just trying to understand.

So, 200 you have.

MR. ESMAILI: Two hundred we have. We have the data for 200.

MEMBER CORRADINI: You kicked 6.25 and 28 --

MR. ESMAILI: No, no, no. We had 200. We started running the calculations and --

CHAIRMAN STETKAR: No. Hossein, stop right there. Let me explain what I understand what you did, at least what I read in the report.

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: You had 200.

MR. ESMAILI: We had 200.

CHAIRMAN STETKAR: You said, let's assume that 275 days, half of 550, is centered on that point.

MR. ESMAILI: Right.

CHAIRMAN STETKAR: That's all you did.

MR. ESMAILI: Yeah.

CHAIRMAN STETKAR: Then you said, that gives me on the left end 62.5 days.

MR. ESMAILI: Correct.

CHAIRMAN STETKAR: And on the right hand end, 212.5 days.

You then took --

MR. ESMAILI: Correct.

CHAIRMAN STETKAR: -- the end 10 percent of those inter -- those two intervals --

MR. ESMAILI: Correct.

CHAIRMAN STETKAR: -- and created the two blue points.

MR. ESMAILI: Correct.

CHAIRMAN STETKAR: Is that what was done?

MR. ESMAILI: Correct.

CHAIRMAN STETKAR: Okay. Now --

MR. ESMAILI: But why did we choose 300? We could have chosen 200 --

CHAIRMAN STETKAR: No, it's not 300. Forget about 300. Three hundred comes from randomly sampling the three time intervals.

MR. ESMAILI: Right.

CHAIRMAN STETKAR: I did the calculation. It comes from randomly sampling. So the 300 is not. So what we're trying to search for here is what physical notions support 62.5 days at the front end and 212 days at the back end? None of that information comes out of this --

MR. ESMAILI: The report.

CHAIRMAN STETKAR: -- in the report. The report simply says exactly what I just put on the record.

MR. ESMAILI: Exactly. It doesn't say how we started doing the calculation. You anchored 200, you ran the 300. So once you do that you have, you have --

DR. OSBORN: Well, and then so after the 200 was anchored the next one was we wanted a sufficiently different enough decay heat curve with VOC.

So we picked that. And between 60 to 90 days is when the VOC curve starts looking very much like a middle of the cycle decay heat curve.

MEMBER CORRADINI: Say that again, Doug.

DR. OSBORN: Around 60 to 90 days into the fuel cycle the beginning of cycle decay heat curve starts looking more like a middle of cycle curve.

CHAIRMAN STETKAR: That's -- I wish the report would have said something like that.

DR. OSBORN: Yeah. And that will be added to the final report.

CHAIRMAN STETKAR: And at what point does the middle of cycle curve start looking like the end of cycle curve because they don't seem to look much different here?

So what's the basis for -- I have now heard some sort of technical basis for why the 62.5 is sort of in the 60 to 90 range.

DR. OSBORN: Right.

CHAIRMAN STETKAR: Why is --

MR. ESMAILI: And that's the decay heat.

CHAIRMAN STETKAR: And that's the decay heat. Doesn't have anything else, it's decay heat.

What is a similar technical basis for the break point on the right end at 337.5 days, or roughly 350 days.

MEMBER BLEY: And that ought to be inventory.

CHAIRMAN STETKAR: And it ought to be something.

MEMBER BLEY: Since there's no difference in decay heat.

CHAIRMAN STETKAR: Right.

DR. OSBORN: We're being symmetrical.

CHAIRMAN STETKAR: No, no, no, no, no. That's not -- that's, you're not hearing, no, you're not hearing what we're asking for. We're asking for technical justification for those intervals. Technical justification. I've heard justification for the left-hand one. I haven't heard yet one for the right-hand one. And that affects the overall results of the study because of the number of samples that you take from each of those intervals are then characterized.

MEMBER BLEY: By right-hand one we're aiming at 337.5.

CHAIRMAN STETKAR: Three thirty-seven.

MEMBER BLEY: Yes. Why is it there instead of either less than that or longer than that?

CHAIRMAN STETKAR: Yes. Like --

MEMBER BLEY: Symmetry is kind of a poor --

CHAIRMAN STETKAR: But 450 days, for example, it's driven by inventory and at 450 days. And

I'm just making up numbers here. There's something in the inventory that you can see changes.

MR. ESMAILI: So let me ask Doug this. Because of the way we did it, the way we did it we, we started running the 300 calculation -- so I'm just, I'm just going to ask this -- we started running the 300 calculations; right? We did middle of the cycle because we had the data at 200. We didn't want to throw those away. We didn't come back and re-do those calculations; right?

CHAIRMAN STETKAR: Uh-huh.

MR. ESMAILI: So once, so that 300, that 300 we got half of, half -- so the moment I anchored that 200 and I'm starting 300 with that middle of the cycle, then I have, then I have that 62.5 and 337.5. I have, I have anchored those two points also.

CHAIRMAN STETKAR: Hossein, you're talking about somebody who runs a code. You might have had 450 realizations at the 200 point if the right-hand 337 was pushed out to something on the order of 500 days. You might have had 400. And it doesn't make any difference except for the overall results of the study.

DR. GHOSH: Yeah. Can --

CHAIRMAN STETKAR: Because the overall results are affected by the number of realizations.

DR. GHOSH: Yeah. Let me take an action item.

CHAIRMAN STETKAR: Take it. Take it.

DR. GHOSH: That we need to add some more discussion in the --

CHAIRMAN STETKAR: Yes.

DR. GHOSH: -- report. I think we gave you some explanation for the 60, around the 60 days. But we clearly need to add some more explanation for why the difference between the MOC and EOC curves which are --

CHAIRMAN STETKAR: Yes.

DR. GHOSH: -- much closer together, but they do still make a difference in the outcome.

CHAIRMAN STETKAR: Well, on decay heat they're much closer together. But you might use a different criterion for, for example, inventory for differentiating where that breakpoint is.

DR. GHOSH: Right. So I'm taking the action item.

CHAIRMAN STETKAR: And that's what, that's what we're looking for because right now the story in the report I think it's pretty obvious that it doesn't hang together very well. You could still characterize the beginning of cycle at 6.25 days. I mean, you've got that data. You could still characterize the end of cycle at 528.75 for whatever reasons. But we're looking for some technical justification for the width of both of those sample sizes.

MEMBER BLEY: Given those points that you've quantified, what should be on a physical basis the breakpoints between them would make the most sense.

CHAIRMAN STETKAR: And then when you do your sampling algorithm you might come up with, you know, you would come up, perhaps, with different numbers of realizations within each of those three intervals.

MR. ESMAILI: And as Tina said, you know, as you mentioned, there could be differences. If I'm going from 200 going to 337, right, my decay is also changing.

CHAIRMAN STETKAR: Sure.

MR. ESMAILI: I'm not sampling on that. I'm sampling all of those, assuming that they are representative at 200.

CHAIRMAN STETKAR: Right.

MR. ESMAILI: Meaning that I have an average in that.

CHAIRMAN STETKAR: Right.

MR. ESMAILI: So this is, so we are not exact --

CHAIRMAN STETKAR: No.

MR. ESMAILI: -- in terms of that, so.

CHAIRMAN STETKAR: We're not asking you to be exact.

MR. ESMAILI: Okay.

CHAIRMAN STETKAR: We're asking for physical --

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: -- technical justification for each of those, the width of each of those --

MR. ESMAILI: Right.

CHAIRMAN STETKAR: -- three intervals in calendar time.

MR. ESMAILI: Right. Right.

MEMBER REMPE: So I missed a lot of this. And I apologize; that was not my own doing. But anyhow, this is where I guess I should have brought up the point I brought up earlier today about it's not just the source term that is affected apparently by where you are in the cycle, but perhaps it might also be the decay heat temperature.

I mean there was a lot of data done on fresh -- or obtained on fresh fuel, right, the prior? And so I really think it's worth exploring that. And I did find a place, and I think you mentioned, that the sigma was like 80-something degrees, the middle was less than 2,500, and that plot does stop at 2,700. And it just I think maybe we are using too low of a temperature. And it can affect the amount of hydrogen generated. It affects a lot of things.

And I just am kind of wondering if -- you know, I don't have the right answer, but to totally ignore that I think is a mistake.

DR. GAUNTT: We'll take that back and think about it. Every time we look at this my estimate goes down. And to explain what we see in the -- what we think we are seeing in the Fukushima progressions, we still feel like we need to bring it down lower. And so, we're going to --

MEMBER REMPE: Yeah, I don't have the absolute answer. But just I'm looking at this going, well, jeepers, you can't just always assume irradiated fuel.

DR. GAUNTT: Yeah. Yeah.

I think it's the cracks is a big deal. But we don't want to go down that hall.

MEMBER REMPE: Okay.

CHAIRMAN STETKAR: Anything more on cycle? If not --

MEMBER REMPE: But before you take a break on this section, I have another question that's related to what I brought up in the prior meeting last May and how it was addressed in the report. And it's about vessel failure discussion.

And I don't know, is there a better time to do that or is this the place to do it?

CHAIRMAN STETKAR: Let's do that --

DR. GAUNTT: After the break?

MEMBER REMPE: Okay.

CHAIRMAN STETKAR: We're going to recess until 11:15.

(Whereupon, at 11:01 a.m., the meeting recessed, and reconvened at 11:15 a.m.)

CHAIRMAN STETKAR: We are back in session. Please get past this slide.

MEMBER CORRADINI: It's your fault. Why are you making -- telling them to get past it.

MEMBER REMPE: Can I ask, is this a good time to ask this or is there a better time?

CHAIRMAN STETKAR: I don't know if they have more discussions of vessel failures.

DR. GAUNTT: No better or worse time now.

MEMBER REMPE: Yes, there's nothing I saw on the slides answering this morning.

CHAIRMAN STETKAR: Go for it.

MEMBER REMPE: Okay. So, last meeting Kyle said we only assumed one size of vessel failure, even though the report had stated that a variable size was assumed. And we still see that sentence about vessel breach of variable size open. So that statement's still in the report.

And there was a much nicer discussion in the report about vessel failure. But it still doesn't quite seem as accurate as it should be. First of all, you still referred to questions some members had about considering penetration, such as BWR drain lines. And I think John at the last meeting or one of the meetings said, What the hell is -- well, he didn't say it that way, but he said, Why are you talking about a BWR drain line for PWR?

And, yes, I think I was the one who mentioned it was the Peach Bottom. So and it was appropriate at the time but it's not appropriate in this report. So please get that sentence out.

But now you talk about the global vessel failure. And you said the subsequent gross failure of the lower heads, consistent with observations from the Sandia lower head failure experimental studies.

And if you'll recall, those experimental studies had a bunch of different sizes and shapes of vessel failures, depending on what pressure assumptions, which transfer assumptions you made, et cetera, et cetera. And so I don't know that that sentence quite sounds accurate to me, too, so I think that ought to be cleaned up.

And it's better than it was, but please clean it up a little more. Okay?

Also, I had a question still about temperature effects on containment failure. And I think at the last meeting in May, a year ago, Jose, you and I had an exchange about it. And you said, oh, the containment structure never goes above 300 degrees F. And I thought I saw a plot in this report, and I can find the page number, where it sure looks like it's going up above 300 F.

Is it -- I mean --

MR. ESMAILI: Which plot are you talking about?

MEMBER REMPE: Oh, I'll see if I can find it.

MR. ESMAILI: Maybe we have it in one of our slides, but.

MEMBER REMPE: The dome temperature is about 500 K, which is 227.

MR. ESMAILI: What figure number?

MEMBER REMPE: Unfortunately, I did not -- Figure 418. Here we go. I think.

MR. ESMAILI: Four eighteen?

MEMBER REMPE: Yeah. And there's a couple of places for short periods of time it's higher, but you're pretty close. It seems like toward the end there's some places where it goes above 500 K. And I just am wondering if that statement about you just don't need to consider any sort of degradation in containment failure assumptions should be considered, and the strength of the containment should be considered because of temperature.

MR. ESMAILI: But the temperature, this is the temperature of the atmosphere.

MEMBER REMPE: Oh, so it's not the structure and all that?

MR. ESMAILI: No. This is not the temperature.

MEMBER REMPE: So the structure is clearly much lower?

MR. ESMALI: And this is what we probably saw when we were presenting for NTTF 5.2 when we were showing, you know, Mark III's. And so, like, you see, you see, like, in the lower compartment, you know, temperatures in excess of 1,000. But the structures do not react immediately to that 1,000.

MEMBER REMPE: Well, how long does it take them to react? It's over a time period I thought that was fairly long there.

MR. ESMAILI: No, these are explos -- these are combustion events. They go very, very quickly.

MEMBER REMPE: But --

MR. ESMAILI: I have to go back and look at my NTTF 5.2 presentation. And there we said that, you know, we didn't see structures in excess of, I don't remember, 300 as you're saying.

But what is important is that even the atmosphere temperature, most of the time it's below 500 K. It's only in the cavity, it's only in the cavity that we get because you have the core that you get this huge, high temperature, because that's where the core is going to be. The rest of the containment, you know, especially the dome, you know, the equipment hatch, they do not see these, these high temperatures.

MEMBER REMPE: Okay. So it's not long enough. These are not the structures, and --

MR. ESMAILI: These are not the structures.

MEMBER REMPE: Okay.

MR. ESMAILI: These are containment.

MEMBER REMPE: And it is the cavity. But it, after 60 hours it's, what, it's up to 1,000 degree K for quite a long time period. But that will not affect --

MR. ESMAILI: Cavity's going to remain hot no matter.

MEMBER REMPE: Okay. And that won't affect the structure. Okay.

MR. ESMAILI: Right. But whatever is going to the rest of the containment is not, is not that hot.

MEMBER REMPE: Okay. Because, again, it's staying at 500 for the --

MR. ESMAILI: Not 500.

MEMBER REMPE: Yeah, but real close to it in the lower containment for quite a long period of time.

MR. ESMAILI: You want to say something?

MR. WAGNER: Casey Wagner, Dakota.

The red line is probably more typical, which is the annulus, which is closer to the lower containment outer wall --

MEMBER REMPE: Okay.

MR. WAGNER: -- rather than the lower containment that's inside of the Crane wale or the annulus.

MEMBER REMPE: Okay. So the lower, the annulus that's orange you think is more typical of the temperatures, which might be around 400 K, than the purple which is the lower containment?

MR. ESMAILI: Right.

MR. WAGNER: Right. That's gas temperature.

MEMBER REMPE: Okay. Works for me.

CHAIRMAN STETKAR: Remind me again when we're going to talk about the ice condenser barrier seals.

MR. ESMAILI: It's going to come up under errors.

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: When we're discussing the errors.

CHAIRMAN STETKAR: Never mind. Just wanted to make sure.

MR. ESMAILI: 4:00 in the afternoon.

CHAIRMAN STETKAR: I'd really like to get done by, you know, 6:00 at the latest. I have to check with people's times.

We do have to break, by the way, precisely at noon because two of our members have meetings that they need to attend. So we're going to do that.

MR. ESMAILI: So 22. So let's put this slide here because between the draft and the current view, the one that we presented last year and the one that we'll be presenting today there have been code changes. You know, we went from MELCOR 2.1 last year to MELCOR 2.2 this year.

And I've listed a number of code corrections and improvements here. And of these supplements that you see, the first two, the correction to the reflood quench model and the dryout model were thought to be most important in affecting what our result is.

So, we covered this. We had a presentation to ACRS Thermal Hydraulics Subcommittee. At that time the main core developer, Larry Humphries of Sandia, came here. We spent a whole afternoon going into details of how these modeling changes would affect the results that we are getting right now. And I think even at that, at the end of that discussion what we are finding right now is that we are finding that we have less, much less early containment failure compared to what we had last year.

And this is mainly to, mainly due to the modification that we made in the safety valve. It's not because of the code because the correction to the reflood quench model come in when the accumulators start ejecting. And that's, most of the time that's when the hot leg fails. And by that time you still have your hydrogen generation and you still have your first burst.

So eventually this conclusion that the changes in the early containment failure were mainly due to safety valve failure to close. And the reduction in hydrogen generation in vessel that was due to the -- we still, we still feel that there is some, maybe some code changes but it's not as important as the modeling input changes, and by that we mean the safety valve.

And I'm trying to get that, the next slide showing to you how the old UA and the new UA behave.

So, this is not in the report because we were looking at this thing once we were starting to recapture for. And as you can -- as Randy was mentioning, it's very technically dense. By the time you go to half of this select realization you forgot what realization 395 is or 307. So we tried, so here we were trying to put together to see what are the differences -- and we went through the MELCOR 2.1, MELCOR 2.2 differences -- what are the differences that can affect what we are trying to get here.

So this shows the simil -- Sorry.

CHAIRMAN STETKAR: Hossein, before we get into it. One of the reasons, by the way, that I was concerned about the partitioning of the timing cycle, other than we ought to have physical reasons for it, is that if you look at the in-vessel hydrogen generation there is much larger uncertainty for beginning of cycle conditions, and there is the maximum amount of hydrogen generated if you look at the uncertainty distribution --

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: -- can get up to about twice the amount for what you've modeled for middle and end of cycle. So that, for example, --

MR. ESMAILI: Like littering time you mean.

CHAIRMAN STETKAR: Littering time.

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: So that, for example, if we're under-sampling the things that we call the beginning of cycle conditions, we might be missing -- I don't want to say a number -- but we might be missing an early containment failure at beginning of cycle which would change a lot of these insights a little bit because everything is focused on middle and end of cycle, essentially dismissing beginning of cycle as functionally irrelevant.

And, you know, one out of four is, or one out of five is like 20 percent. So that's another reason that I think --

MR. ESMAILI: Well, let me go through this slide.

CHAIRMAN STETKAR: Yes. No, go on.

MR. ESMAILI: Maybe these slides are going to answer because it answered a lot of my questions. Because some of this information gather ups what happens --

CHAIRMAN STETKAR: Sure. No, go on.

MR. ESMAILI: And we could not bring it together. And here I am only looking at the middle of the cycle cases because I'm comparing the current UA with the draft UA. But I do have another slide here that's in the background -- if you want, we can bring it up -- and that shows, that shows comparisons with the beginning of the cycle and the end of cycle. We can get into that.

But, so, the reason we put this thing together was to see what is the mech -- what is controlling things, you know, what are the dominant phenomena here. And so the first thing you notice is that you see on the upper-left corner, you see that insert, the bar chart here, we are comparing the draft UA with the current UA in terms of what these safety valves, how do these safety valves behave.

So, all the things that you see in green are no failure to close cases. So that means that the green means that these valves are going to cycle until you have hot leg failure. They never fail to close. So this area fraction is out of discussion.

The yellow shows that the cases where it fails to close but the area fraction is less than 30 percent.

And the red is the failure to close and the area fraction is greater than 30 percent.

So you can see immediate -- and this is, this red one is identified where you have the potential for early containment failure. And we are going to explain to you why that's the case.

So the immediate thing is that the draft UA, the one we saw last year, you had 62 percent of the cases that was -- that had this characteristic, that the failure to close the safety valve area greater number. This year, in the current UA, that's down 62 percent to 4. So that tells me that, yes, I would expect a lot less number of early containment failure because I am not in that region that I can fail the containment.

Okay, so just keep in mind that the red is what is important. So at -- No, no, sorry.

For the early failure containment to occur, as Randy was mentioning, for the failure occurs on the first round. So if you get the containment failure after the first round, your containment fails, if you don't get it you're not going to -- the containment retains its integrity. You're not going to get additional burn to fail the containment.

So to try to see how we can present this data that is understandable to us. So the moment you say, okay, the first round is important, so on the Y axis I have the time from the time that hydrogen is generated -- this is the time that core damage starts -- up to the time of the first deflagration.

And then on the X axis is the amount of hydrogen that is generated by that time. Okay. So this is the X axis and the Y axis. And here I am showing to you the current UA results of MELCOR 2.2 and the draft UA, and they behave relatively nicely.

So I can see that I am, you know, the timings of the order of one hour I'm producing less hydrogen. And as I move towards the right-hand side, which is the red region where I have safety valves fail to close with an area .2, you know, my time goes up. So I need more time to, you know, fail the hot leg and start the deflagration.

So and the nice thing about this figure is that, you know, the blue and the red are on the same map. So you can see, you can see the reason we have less containment failure is because I have a lot less number of these blue lines because I have samples, less samples that fall in that region.

MEMBER CORRADINI: So can I? I want to understand this.

So you've got the current and the draft here. The orange is the draft, the blue is the current. So for a preponderance of the middle of cycle calculations you only have about an hour or less than two hours, and approximately an hour between the time you start generating hydrogen and you have a deflagration?

MR. ESMAILI: Yes.

MEMBER CORRADINI: So that means --

MR. ESMAILI: These are the, these are the everything that's on the left of the figure. The left of the figure that I would --

MEMBER CORRADINI: I understand.

MR. ESMAILI: Yeah.

MEMBER CORRADINI: So that means I'm releasing, based on your other models which we're going to eventually talk about, I somehow get steam uninerted in the lower containment. The steam concentration falls enough that it's not inerted and I essentially, essentially ignite this based on hot leg rupture?

MR. ESMAILI: That's right.

MEMBER CORRADINI: Because I can't anymore ignite it based on pressurized relief tank because the pressurized relief tank is too cold. All those essentially have been excluded.

MR. ESMAILI: Yes. But this is, this is how much hydrogen I'm generating in vessel. I'm have not, I have not -- we are not discussing what happens on the containment side.

MEMBER CORRADINI: Yes, I understand that.

MR. ESMAILI: We are coming from I'm generating that I can push out into the containment.

MEMBER CORRADINI: Okay, I understand. But I want to make sure I'm understanding what it means. It says basically that in the current, the blue dots --

MR. ESMAILI: Yeah.

MEMBER CORRADINI: -- throw away the 4 percent because they essentially don't exist except over to the right.

MR. ESMAILI: Yes.

MEMBER CORRADINI: So most of the blue dots on the left-hand side of the graph are in the green 89 percent.

MR. ESMAILI: 89 percent; correct.

MEMBER CORRADINI: Which means that this thing is cycling.

MR. ESMAILI: This thing is cycling.

MEMBER CORRADINI: Right. And so the moment I release any hydrogen I have to have an intersection of three things: I've got to have an ignition source; I've got to have steam uninerted; I've got to have a lower steam concentration; --

MR. ESMAILI: Right.

MEMBER CORRADINI: -- and then I've got to get the hydrogen popping out of that relief valve.

MR. ESMAILI: Have enough hydrogen in the containment.

MEMBER CORRADINI: Right.

MR. ESMAILI: Right.

MEMBER CORRADINI: But it's the relief valve that's not going to ignite it, it's going to be the cold leg -- the hot leg rupture that's going to ignite it.

MR. ESMAILI: Yeah, we have, we have some cases, as Randy was mentioned, that the PRT, in extreme cases like you see on the right-hand --

CHAIRMAN STETKAR: Extreme cases. But the vast majority it's hot leg rupture.

MR. ESMAILI: The point is that when you are cycling at high pressure you are producing less hydrogen. And we are producing less hydrogen so we have less hydrogen available to go.

And this is, this, there is no difference from last year. It's just that we are going up and down this curve, right, and if I change my characteristics of the safety valve then I'm going to have more points here and there. But the overall system response remains the same.

CHAIRMAN STETKAR: Okay.

MEMBER CORRADINI: But my three combinations of things having to happen together is what gives me the blue. Okay.

I'm going to come back to that because I, the one about the --

MR. ESMAILI: What gives you the blue, again, what gives you the blue, this is the hydrogen that's generated in vessel, what gives you the blue is that in these cases, again, it's cycling. When you are cycling you are more efficiently transferring heat. You know, your core is, your core is a little bit cooler, right, because you're more transferring to the boundary. And you are at system pressure. You are at 16 megapascals. So you only need a little bit of temperature to actually fail this hot leg.

So that's why the time to hot leg failure is much smaller compared to the right of the figure when you have the pressure. So now, now your pressure is lower, right, so you need a little bit more time for the quick rupture.

So this map shows all of those things.

MEMBER CORRADINI: Okay. You've explained it very nicely. But just to be clear, the hot leg rupture would be in, would be in CV 08 or 09; right?

MR. ESMAILI: Once the hot leg fails, yes.

MEMBER CORRADINI: And then when it burns it can either go up sideways, it doesn't have a chance to go down.

MR. ESMAILI: Well, once a burn occurs, like in the lower compartment or anywhere, it can just propagate everywhere. As long as those conditions that you're saying, as long as it's not inerted, as long as you have enough hydrogen there.

There is some cases we actually do not have hydrogen ignition inside the dome because we don't have, we have no transferred -- I'm going to get into that -- we have not transferred. Since I'm only producing 100 kilogram --

MEMBER CORRADINI: That's fine. You've answered my question. You've answered my question.

MR. ESMAILI: All right.

So the other thing is that when we are on the left-hand side of the table, I was telling you, it's even more difficult to transfer this hydrogen that's generated. So not only am I producing less, it's more difficult to transfer it into the containment because my safety valves are cycling and bottling up all this hydrogen. This lower plenum, as I was telling you, you know, they just open and close. Most of the hydrogen is retained inside there, inside the vessel up until the time of hot leg failure.

MR. ESMAILI: Okay.

MEMBER CORRADINI: So right now this has everything. This has failure to close and no failure to close cases.

But now on the next slide I'm just going to just focus on the no failure cases to see what the effect of the safety valve cycling is.

So what you see here is that most of these cases you are limited by how much hydrogen you are producing, about 250 kilograms. Starts around 240 to 250 kilograms. The time is less again because now in all these cases you are at system pressure. That is, all the -- whether it's current or draft you are.

There are some outliers here that do not exactly fall on this curve. And the reason is that because they are mainly driven by the delay in the burn. So you have produced this hydrogen. You have released it. But the conditions are not right at the time of hot leg failure to have the burn, so there is going to be a delay.

And on the right-hand side of the figure you see there are two points from the old -- from the draft UA. Those, those are, again, there is a delay in hydrogen burn but it's a little bit more complicated. Because if you remember, we had this issue with the quenching. So it's from the time that I insert the -- I inject the water through the accumulators up until the time there, I am actually generating more hydrogen, which I am not anymore since we corrected that error.

So in the insert that you see on the right corner, on the right-hand side corner, is that now if I just go and change the Y axis to the hot leg failure, right, up until the time that I have the hot leg, I can collapse all those, all those points onto the same figure.

DR. GAUNTT: Except for the circles.

MR. ESMAILI: Except for the old UA, those guys.

MR. ESMAILI: No. Those three guys that are in the 400 region, now they have shifted also to this, so there is no more outliers anymore. The Y axis is up to the time of hot leg failure.

MEMBER CORRADINI: So it's the hot leg failure timing that is disparate between the circles in the --

MR. ESMAILI: This is just telling you that the hot leg failure, this is just telling you that the amount of hydrogen that's produced when the SRV, when the safety valves are cycling, has a nice behavior.

MEMBER CORRADINI: Okay.

MR. ESMAILI: Once you go out, you know, those eight blue ones that are up there under three yellow ones is because there is a delay. You have your hot leg failure. You don't -- I think this is what Stetkar was talking about -- you don't have your burn at the time of hot leg failure because the conditions are not right. I think Randy is going to show some of the examples of what that happened.

But I was just saying this seems like outliers, but if I just properly normalize them I'm going to come to the same curve. Is that I am going to produce less amount of hydrogen when I'm cycling at norm --

MEMBER CORRADINI: So I'm still struggling to understand what the insert graph is in relation. You've got, you've got essentially six blue points. Have they been removed from --

MR. ESMAILI: No, they have not been removed. I have changed the Y axis.

MEMBER CORRADINI: Yes, I understand.

MR. ESMAILI: So if the Y axis is from the time -- the big one is from the time of hydrogen generation to first deflagration, right, the insert changes from the time of hydrogen to the time of hot leg failure. So, in other words I am getting rid of that delay, that delay from the time of hot leg failure to the time of hydrogen combustion. And then everything --

MEMBER CORRADINI: So what causes the six points to burn then?

MR. ESMAILI: They're going to burn a little bit later. They're not going to burn at the time of hot leg failure. There is going to be a delay when they're going to burn.

These are the conditions that are the conditions are not right. Either you don't have enough hydrogen -- you know, remember, these are no hydrogen. Sometimes there is steam emerging. And Randy is going to talk about three of them that shows that, you know, there is a window that you can burn these things. And sometimes you cannot burn them right at the time of hot leg failure.

But the important thing is that we are producing the same amount of hydrogen whether it's the old -- whether it's the current or draft UA, whether it's the MELCOR 2.1 and MELCOR 2.2, it has that behavior. But as long as you are at cycle and at safety valve pressure, you know, you are limited by how much hydrogen you can produce.

MEMBER CORRADINI: So can I say it back to you a different way? I said there were three things that have to be in concert to get it to burn. One of those other two things aren't there with the six blue dots. Either the steam inerted or it's not --

MR. ESMAILI: Not enough hydrogen.

MEMBER CORRADINI: Okay.

MR. ESMAILI: Not enough hydrogen. Right.

MEMBER REMPE: I'm sorry. I've been

looking at this a long time.

The three dots where you have it at 400 kilograms of hydrogen, did they become less hydrogen generated?

MR. ESMAILI: Yes. Because if you remember, when Larry was here on April 18th we were talking -- and I said, we said that we were talking about the differences in MELCOR 2.1 versus 2.2. So there were situations in MELCOR 2.1 that even when we started accumulator injection, because of the problems we had with the core with the reflood model, we were not actually quenching. So in other words, from the time, from the time I failed the hot leg and accumulator came in, I was able to produce more hydrogen, about maybe another additional 100, 150 kilograms.

MEMBER REMPE: But that first plot is actually a 2.1 result, the big one, and the insert is 2.2?

MR. ESMAILI: No, no, no, no. They're the same point.

MEMBER REMPE: This is all 2.2 results?

MR. ESMAILI: They're the same point. These are all current -- yes, these are the same points. The only difference is that I changed the Y axis.

MEMBER MARCH-LEUBA: For those three points on the circle, when you broke the hot let it didn't ignite --

MR. ESMAILI: It didn't ignite.

MEMBER MARCH-LEUBA: -- and continued to generate.

MEMBER REMPE: It continued to generate. And that's why it went to 4.0. Okay.

MEMBER MARCH-LEUBA: Yes.

MR. ESMAILI: But if I properly change the Y axis I'm going to collapse all of them. My point is that at high pressure this is how much hydrogen you can produce. And it's consistent with the current and the draft UA.

So this did not make it to your final report because we were still working on it. We wanted to make a story of all the things and trying to understand. It took us some time to understand what's going on here.

So this case is at 89 percent of no failure cases. These is an element of less hydrogen and there is an element of not having enough time. You know, the time is about one hour before you can actually have this.

The next slide.

So now I'm again blowing up that region and I'm looking at the effect of the oxidation model. In other words, help with the oxidation model.

Now, everything you see in the insert the draft, draft UA used by default used the Urbanic-Heidrick model. Here the green, the blue, and the yellow are showing these two different observation models that we are going to be using. And you can see a very nice clustering of these. You know, all the green ones this is in light of Schanz Pawel-Cathcart model, they have, they have lower kinetics at lower temperature; right? They are clustered towards having less hydrogen production. Just like you saw in CORA-13. That correlation produced less hydrogen compared to the other correlation.

The boundaries between the blue and the yellow is a little bit less, you know, defined. And what you see is that when you go to the draft UA, things are shifting a little bit to the right. And one of the reasons that's happening is that in the draft UA -- the credit is important -- in the draft UA on the secondary side we are cycling until we get to that 45 cycle. And then we fail the secondary side, we get area fraction of 50 percent.

That means that I still have steam generator water in there but I actually blow -- I mean I actually flashed over. So I could have gone, I could have, you know, get to the core damage a little bit later, which I do in the current UA because my cycles -- my safety valves are cycling more. So I actually can dry out the steam generator.

And so the timing of the core damage is different from the draft UA compared to the current UA. That's why you see there is some, there is some shifting of this draft UA to the higher hydrogen production. So that's mainly because of the, I really think about of the decay heat, the start of that.

And the other thing that we notice is that you see a nice downward trend; right? As I am producing more hydrogen, I have less time now to -- but most of the cases are driven by the time of hot leg failure. So most cases first deflagration starts at the time of hot leg failure.

So all this curve is telling you is that as you are producing more hydrogen it's indicative of the fact that you have a hotter core. You are transferring more heat to the boundaries, you know, to the hot leg, et cetera. Right? And the oldest cases my system pressure is at, you know, 16 megapascal; right? So I have the pressure. That is it.

So if I'm doing the quick rupture of the hot leg right now, if I have hotter temperatures the timing of that is supposed to be less. And indeed it is. So as I go move to the right it takes less time for me to fail that hot leg and start the deflagration.

So I was very surprise by how well, you know, these behaviors are characterized and we can explain, you know, some of these mainly similarities, to tell you honestly, between the draft and the current UA.

Okay, so now we talked about, we talked about the cases where we had no failure to close. Next slide, so this one I removed all the high pressure cases. Now we have failure to close. But the areas can be either greater or less than 30 percent.

The first thing you notice is that, you know, all those clustering of the points are removed. I have the few points to the left of the figure. And you can see that when the area fraction is less than 30 percent, you know, these are the yellow in the draft UA and the blue, they tend to look more like the no failure to close cases. Okay? Because in some of them, in some of them the area fraction is small. It's not so -- even though the, even though the valves may be failed open, but it takes time to --

MEMBER MARCH-LEUBA: Is this equivalent to the cycling?

MR. ESMAILI: It's equivalent to cyc -- Well, it not equivalent to cycling but it's going to be, it's going to be bottling up like those things.

The other thing about the bottling of the hydrogen inside the vessel is that the kinetics is important, but what I didn't mention was that as you are producing hydrogen you have problems accessing the steam. All right? Because at some point it's not driven by the kinetics alone, it's driven by how much access you have to steam because it's mass transfer limited. And this is what we model in MELCOR. So that if you don't have enough steam, obviously you cannot produce a lot of hydrogen.

And so these are the results of us showing that, a combination of these effects.

CHAIRMAN STETKAR: Hossein, it's interesting. I read the story about there's not a forensics like this in the current report.

MR. ESMAILI: No.

CHAIRMAN STETKAR: The forensics in the current report make the argument that the effect of the area fraction is subsumed in that accumulated number of relief valve cycles which is used, you know, as another regression variables.

In the beginning of the report there's a vague reference made to, well, yeah, if the valve sticks open -- the valve has to stick open I think it says 25 percent in there. But kind of like 25, 30 percent in order for you to get much of a difference.

This might also be useful when you think about recasting the results.

MR. ESMAILI: Yeah, this is, this is extremely helpful. Yeah.

CHAIRMAN STETKAR: I sort of bought the notion, you know, of the accumulated number of valve cycles to capture both the failure to reclose and the open area fraction. But this from a physics standpoint might, might have a little bit more --

MR. ESMAILI: Yes. We have a better story to tell, yes. Yes.

CHAIRMAN STETKAR: I just wanted to make that for you to think, --

MR. ESMAILI: Right.

CHAIRMAN STETKAR: -- you know, when you talk about the forensics on the results if you wanted to.

MR. ESMAILI: Rather than jumping from SE failure loops to early and late -- Yes, that's how these things comes about.

And so you can actually clear, you know, in the --

MEMBER CORRADINI: How did you get -- why -- I'm still struggling at the 30 percent. What are the physics that essentially branched at 30 percent? That's what I'm --

MR. ESMAILI: Well, you know, the 30 percent was, you know, especially in the current UA because, remember, in the current UA you have a lot of the cases less than, you know, like about less than, less than --

CHAIRMAN STETKAR:

I think the 30 is probably a little bit of an artifice with the shape of that distribution, you know.

MR. ESMAILI: Right. Right.

CHAIRMAN STETKAR: So I wouldn't hang my hat on 30 versus 27 versus 42.

MR. ESMAILI: Because it 30 or something like that.

CHAIRMAN STETKAR: It's sort of in that --

MR. ESMAILI: Right.

CHAIRMAN STETKAR: -- not zero and not --

MEMBER CORRADINI: Because the shape of the distribution is like this.

CHAIRMAN STETKAR: Yes.

MR. ESMAILI: And sometimes it becomes, what we are showing is that sometimes it becomes independent. You know, you see these valuations that are going up especially, but the important thing is that you are capturing this time that you have this delay to the first deflagration, that gives you time to transport this hydrogen into the dome; right? You're producing more hydrogen. You have no more time to -- and, and the fact is that there is a clear distinction right now.

So you see especially for the current UA, you see if I break it up into 30 percent all the blues -- I think there was a mention of about 350, 375 kilograms in the report, so all the blue that you see is to the left of that 350. All the green is to the right of that 350. And that's how it goes up.

CHAIRMAN STETKAR: And the interesting thing is that in the draft UA the difference between what looks to me to be golden and sort of reddish is that there were equal likelihoods of anything.

MR. ESMAILI: Exactly.

CHAIRMAN STETKAR: So there seems to be some physical notion that around 25 to 30 percent open, the physics changes.

MR. ESMAILI: That's right.

CHAIRMAN STETKAR: Because in the gold and the red there was equal likelihood that it could be any open.

MR. ESMAILI: And that's what's captured here.

CHAIRMAN STETKAR: That's right.

MR. ESMAILI: These are, these meters are being captured here. You are exactly right, that I have so many points going all the way to 200 because I am sampling from zero to 100 percent, whereas here I am just sampling on the two other extremes. And this is a clear distinction.

So, so this is what happens. So this is the effect of area fraction.

What's the next slide?

Okay. So now, so now here I said this, so this is where the area containment failure map is. So I started out with less number of nodes in the region where I could potentially fail the containment. I have about 9 or 10 of them. And I have about a few of them out of that 9 or 10 that are failing the containment.

In slide 26 I started off with a bigger number of, you know, things, and that's why I have the old calculations I have bigger number of early failures. But there is no discrepancy between the current UA and the old UA. It's just that safety valves are determining where I'm going to fall on these curves.

And again this is, so this is just how hydrogen is being transported. So, again, this is the middle of the cycle. And this is the region that we are producing, when we are producing this hydrogen we are producing, you know, we have to have more than about 150 kilograms going into the dome. So this just tells you this is where you are. You know, you have the time to get these things to the dome. Then you have your first deflagration. And by that time you have your, you have your failure.

Not all of these cases fail. Remember, some of these cases come pretty close to 250 but that does not fail because it becomes very, very sensitive, how close you are going to get to that failure pressure and not fail.

But I think it shows the consistency with the draft. You're right.

Okay, next slide.

So, so in this --

CHAIRMAN STETKAR: Hossein.

MR. ESMAILI: Yes, sir?

CHAIRMAN STETKAR: I think I'm going to stop you here, unless we can get through this thing in like a minute. Probably not. This one's a little more complicated.

So let's stop here and we will recess until 1:00 o'clock. Sorry. We're going to lose two important people.

(Whereupon, at 11:53 a.m., the meeting recessed, to reconvene at 1:01 p.m.)

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

(1:01 p.m.)

CHAIRMAN STETKAR: We're back in session, wherever we were. Continue.

MR. ESMAILI: Okay. So we went through that characteristic map of where we think, you know, failure and no failure would occur.

Here we are just going into a little bit of detail of how this hydrogen gets generated and gets transported. These are the time history plots.

The figure on the left-hand side is for realization 554. This is the case when the pressurizer safety valve failed to close on the first cycle. And as you can see for this case, the dashed green lines are the heat total in the in-vessel hydrogen generation.

And the blue lines are what is coming out of the pressurizer relief tank after it burst.

And the red line is how much of this, how much hydrogen actually makes it to the upper part of the containment.

So what you can see is that on the left-hand side when this thing happens more of the hydrogen, as it's being produced, is passing through the pressurizer relief tank and going into the containment. And it ends up with at least 150 kilograms. And then there's a little push once the hot leg fails, pushes additional hydrogen into the dome.

So you end up by the time that you have hot leg first deflagration you have about 200 kilograms of hydrogen in there.

CHAIRMAN STETKAR: Hossein, if the barrier, suppose the barrier seals were not there, would the red line track the blue line more closely or not?

MR. ESMAILI: When you say the barrier seals that it was open or?

CHAIRMAN STETKAR: Yes. The barrier seals. So it's you bypass the barrier seal. The cracks were open.

I'm trying to understand how the barrier seals would affect hydrogen move to the upper part of the containment.

MR. ESMAILI: Yeah, I'm asking Casey.

Did you do a sensitivity to that? Do we know what's going to happen? I could tell you honestly I don't, I don't want to say anything --

CHAIRMAN STETKAR: Okay. I just --

MR. ESMAILI: -- because we didn't run the calculation. I thought that maybe if we had done.

MEMBER CORRADINI: Well, can I ask an opposite question? What is the free volume in the dome, what is the free volume in the dome relative to the free volume of all the lower parts? Because it looks to me like you've got about 50 percent of the hydrogen before the push of hot leg rupture, you've got 50 percent of the hydrogen above and 50 percent below. I take 100 and I double, I get 300. That's about what coming out of the burst disk.

MR. ESMAILI: So this is the hydrogen that's in transit, the blue line. This is the hydrogen that's coming out of the vessel; right?

MEMBER CORRADINI: I understand.

MR. ESMAILI: Getting into the -- Yeah, yeah, right.

MEMBER CORRADINI: The blue line is the integration of what's left of primary system.

MR. ESMAILI: Right. Correct. So you have 50 --

MEMBER CORRADINI: I'm asking, I'm asking from a mass balance standpoint, I lost at 4 -- before the push, at 4 point epsilon hours.

MR. ESMAILI: Right.

MEMBER CORRADINI: I've got 350 kilograms outside of the primary system. Does it essentially, does it essentially proportion itself based on heat loss?

MR. ESMAILI: You're setting up different, you're setting up different -- you know, some of it is going to go to the ice chest, some of it is going to remain inside the lower compartment. Some of it is going to be recirculating back. But what's --

MEMBER CORRADINI: What do you mean circulating back?

MR. ESMAILI: Because we have, we have deck leakage from the upper containment to the lower containment. So as things are moving they can go all over the place is what I'm trying to say.

MEMBER CORRADINI: What I'm trying to do is rationalize the red and the blue based on simply free zone.

DR. GAUNTT: Sure. You now, Mike, my impression is you don't see the hydrogen getting into the dome region until 3.25 hours. But you've already put by then 200 kilograms of hydrogen has gone into the lower compartments.

So it looks to me like -- I think this is true -- that it's just simply displacing, gradually displacing pushing the hydrogen out.

MR. ESMAILI: Yes. That's what I was going to say is that we were talking about the lower plenum doors. And that's what I said, that he lower plenum doors are not going to get fully open until we get the hot leg rupture. So these doors are just going to open enough to remove this pressure.

Then you can see how the hydrogen in the green and the red line, how hydrogen is building up inside the dome. So there is never area --

CHAIRMAN STETKAR: But the upper plenum

doors take a lot more DP to open; right? So, so is a good --

MR. ESMAILI: It takes a lot more DP there.

CHAIRMAN STETKAR: -- is a good chunk of this hydrogen now trapped in the ice condenser?

MR. ESMAILI: yeah. Because --

CHAIRMAN STETKAR: That's the reason for my question about what happens if the --

MR. ESMAILI: Because now you're limited --

CHAIRMAN STETKAR: -- if the other stuff is open.

MR. ESMAILI: And then you get like 2 meters square. It's a torturous path now going to the ice.

MEMBER CORRADINI: I mean we're looking at one dead body versus others. So, since we're at 554, what's the area open of the fail to close; is it less than 30 percent or greater than 30 percent?

MR. ESMAILI: This one is, this one is greater than 30 percent. This is one of the earliest cases that failed to. It's one of the four cases that failed the containment.

MEMBER MARCH-LEUBA: In what state is the hydrogen? Is it dissolved or mixed with air or is it stratified?

MR. ESMAILI: It's not stratified because it --

MEMBER MARCH-LEUBA: It moves with the steam.

MR. ESMAILI: It moves with the steam and air as it goes through the different openings.

MEMBER MARCH-LEUBA: So does this move through the gate and what else?

MR. ESMAILI: Move through the ice chest and goes and ends up in the dome. That's because --

MEMBER MARCH-LEUBA: Are accumulators up there or?

MR. ESMAILI: This red line shows that it's getting accumulated --

MEMBER MARCH-LEUBA: That's because the pressure is increasing?

DR. GAUNTT: The containment pressure is following this out, right.

MEMBER MARCH-LEUBA: There is more mass upstairs.

DR. GAUNTT: Yes, right.

MEMBER MARCH-LEUBA: So as gases move all together up there they stay up there.

DR. GAUNTT: Yes.

MEMBER MARCH-LEUBA:. So it's not the ratio of the volumes?

MR. ESMAILI: Yeah, I don't, I don't want to say, you know, we can scale it to the volume because --

MEMBER MARCH-LEUBA: Yes, there is no -- the other end it isn't separate.

DR. GAUNTT: It's pressure difference driven flow and the whole containment pressure's just coming up.

MR. ESMAILI: You have deck leakage, you have flow through the ice chest. And this casing becomes even worse after the hot leg rupture because some of these doors are going to get crushed open and then you have all sorts of local and global circulation.

But the point is that, the point here is that what we have seen before is that as it becomes available it's being pushed into the containment. Whereas, on the right-hand side this is a case of 266 which was the pressurizer -- was no pressurizer safety valve made to close. So you can see that hydrogen is being introduced but not much of it is coming out. Most of it is getting bottled up inside the pressure vessel RPV.

And you can see the effect of the accumulator injection very clearly on this one because here by this time you have produced all you could do in terms of hydrogen generation.

And this is another point I wanted to make is that on the right-hand side you are actually failing the containment, you're failing the hot leg relatively early; right? I mean, look at the time here. You know, it's from less than three hours to almost 4.2 hours. And here it just takes maybe less than an hour.

And this is what we were saying before is that you're producing hydrogen, right, and then you are heating up everything. At this point your hot leg fails; right? And so you have not produced all the hydrogen that you could have produced. And that's why hydrogen production stops because now accumulator is coming in, is quenching things for a while until the water level goes down.

And then the important thing is that right after that we get the second heat up and then the hydrogen production keeps going up. But the important thing is that by that time, by that time you already had the hot leg failure and first deflagration. And this is the case that is not resolved in containment failure.

MEMBER MARCH-LEUBA: And you've got to explain negative masses.

MR. ESMAILI: The negative masses is, okay, so this is more difficult to explain, is because, because once the, in this case once the hot leg ruptures some of these intermediate doors and upper plenum doors, as we discussed, some of them get crushed open. So the situation of natural circulation becomes way more complicated.

So things are going in this direction and then they can reverse. They can reverse. Some of these low parts, you know, the gases are going down. Some of them are going up. But it's a very complicated flow path.

MEMBER MARCH-LEUBA: Negative masses?

MR. ESMAILI: Well, negative it's a --

MEMBER BLEY: It's a directional.

MR. ESMAILI: Negative mass means that, if you look at this book, this is an integral. So I'm integrating something that's going up positive. And then once it goes negative --

MEMBER MARCH-LEUBA: It's a mass flux?

MR. ESMAILI: It's a mass flux, yes.

MEMBER MARCH-LEUBA: Keep going.

MR. ESMAILI: I'm just integrating the negative number so it becomes negative.

MEMBER CORRADINI: So this is a flow junction measurement integrated with time.

MR. ESMAILI: Integrated with time. So when it reverses flow and when I'm reversing the flow and the flow is going back in the other direction, instead of plus 100 kilogram per second it goes minus 100 kilogram for a while. Then since it's an integral value it becomes negative.

MEMBER CORRADINI: Okay.

MR. ESMAILI: This is just an integrated value, so there is nothing. But the point is that at the time of hot leg failure this is where all those doors are going to behave in, you know, some of these doors there is enough pressure to crush them open, even though the pressure is pretty hot.

Okay, so --

CHAIRMAN STETKAR: You guys keep going.

MR. ESMAILI: Okay.

CHAIRMAN STETKAR: We're just arguing among us.

MR. ESMAILI: All right. So in terms of overall containment failure outcomes you basically have two basic outcomes. You either have early containment failure, like some of these cases here, at the time of the core damage. And just like I showed you in 554, is that the containment fails at the time of core damage.

If it doesn't fail at the time of core damage, then it starts to, you know, it starts to go off on this trajectory because of it is driven by ex-vessel phenomena. Now I have four concrete interaction, now I have ex-vessel gas generation, now I'm putting the decay heat inside the containment, et cetera.

And some of the cases actually within the 72 hours it does not result in containment failure. So these are the three outcomes that you are looking at.

So the end of the cycle, if you can look at the red and the blue, on the average if you look at them, end of the cycle has higher decay heat so you have more cases failure earlier than the blue cases. Right? So they are on a slightly higher trajectory.

You have a question?

MEMBER CORRADINI: I did. So actually I said earlier there were three things. One was you've got to have, you've got to have a failure to close, which causes. So it starts leaking earlier and you start dumping material into the lower containment which then goes up.

I've got to be not inerted.

And then I've got the hot leg's got to essentially fail.

So that's the only ignition source because PRT's not an ignition source anymore since it's too cool.

MR. ESMAILI: In some of the cases PRT is actually the ignition source. In two of the cases, actually the PRT causes the, causes the early failure.

MEMBER CORRADINI: Even though it's sitting, it's sitting in the water?

MR. ESMAILI: It's pretty -- at that time, you're talking about very, very early in the accident. So it's still --

MEMBER CORRADINI: So it's still above the water line?

MR. ESMAILI: Right. So you have a failure to close.

MEMBER CORRADINI: Hot gases.

MR. ESMAILI: Really hot gases are coming off, too. And there's enough temperature inside that that can cause the --

MEMBER CORRADINI: But my fourth thing I should have said was I'm curious about the three failures. Are they the lower rupture pressures around 52 PSIA versus the higher rupture pressures?

MR. ESMAILI: Not necessarily. I think, I think they -- I think when I looked at them they

Government around like maybe 67 PSIA, 66, 67 PSIA.

MEMBER CORRADINI: Or around the median, around the median.

MR. ESMAILI: Yeah. So it's, yeah, around the median.

CHAIRMAN STETKAR: A or G?

MR. ESMAILI: A. A.

CHAIRMAN STETKAR: A is the lower end. It's 52 PSIG to 70-something.

MR. ESMAILI: 50 PSI is the lower bound but --

CHAIRMAN STETKAR: G.

MR. ESMAILI: G is the lower bound. But the pressure, these are PSIA. These are the pressures that you're seeing here, right, the pressures that you're seeing here are absolute pressure.

CHAIRMAN STETKAR: Okay.

MR. ESMAILI: So these, so the only thing I want to, also want to mention is that look at the yellow lines. These are all the beginning of the cycle. They are definitely on a different trajectory because of much lower decay heat right now. But even those, you can see some of those cases are showing that you have spikes. You have produced hydrogen enough that you have a hydrogen combustion.

And, in fact, when I got looking at some of those slides I was showing before, we have some of those points that are in that region where the containment could have failed. So we cannot rule out early containment failure for the beginning of the cycle case, even though if that does not happen they are going to be on the slower trajectory. They are going to have different, they are going to have multiple burns later on, right, until you are depleting the oxygen. After you are depleting the oxygen you don't have more oxygen in the containment and you don't have any burst.

But the point is that it's still possible to get early failure from the beginning of the cycle.

Okay. So some of the basic statistics --

MEMBER MARCH-LEUBA: Sorry. Are all those cases when the valve fails to close?

MR. ESMAILI: Yes. Those are -- remember this slide that I showed you, this. This case. Some of those, if I showed you the green on that cycle, the valves, yes. But they have the potential to fail the containment early. But I have very few of them.

MEMBER MARCH-LEUBA: Would you say that a failure to close the valve is more relevant than where it is beginning of cycle or end of cycle?

MR. ESMAILI: It looks like it. It looks like if you are in this region, doesn't matter how I'm going to get into this region --

MEMBER MARCH-LEUBA: Sampling the time of life, time of cycle doesn't have much relevance --

MR. ESMAILI: Right. Right.

MEMBER MARCH-LEUBA: -- because it's whether --

MR. ESMAILI: Right.

So this is by far the most important parameter compared to other things that we consider.

CHAIRMAN STETKAR: Well, we're talking about something else. Sampling the time and life is important when you look at the differences in the amount of hydrogen generated. So, early in the cycle you can generate a hell of a lot more hydrogen. So, if you have more samples out in there --

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: -- you're going to get more hits for the early failure.

MR. ESMAILI: Yeah. And that's why I say I cannot give --

CHAIRMAN STETKAR: There's a figure coming up in several slides that will show the hydrogen, in-vessel hydrogen production. You have in the report there's three separate ones, and it's a lot more clear. The one that we'll later has a more --

MR. ESMAILI: I mean you can clearly see some of the red lines are spiking right here. So if you had more of them you have --

CHAIRMAN STETKAR: Well, not the red lines. You mean the orange lines.

MR. ESMAILI: The orange line, yeah. The yellow line, yeah.

CHAIRMAN STETKAR: Yellow line.

MR. ESMAILI: So they are showing this. And if you have the right parameters in there, it could fail the, you know, the containment.

Okay, so in terms of all the --

CHAIRMAN STETKAR: By the way, just in terms -- forget about the slide -- we're running over time. This is a really good discussion that we're having. I checked. We can go as late as 6:00. I don't want to go any later than 6:00.

So just everybody keep that sort of time scale in mind, then we can buy ourselves another hour this afternoon.

MR. ESMAILI: Okay.

CHAIRMAN STETKAR: But no more.

MR. ESMAILI: Sure. This is my last slide on this. And this is going to go pretty quickly actually.

You have 600, you have 600 UA. We have a good success rate here. 95 percent of the calculations actually went to completion.

All of these calculations that went to completion, as I said before, four failed the containment earlier. The majority of them did not fail, nearly 500 of them did not fail the containment early because what we saw in the previous slide. And 71 of them did not fail the containment by 72 hours. Most of them were beginning of the cycle.

As I said, there was a few of the cases from the middle of the cycle that still did not fail the containment by that 72 hours. But the major -- all the beginning of the cycle had that characteristic.

And, so, this is again the repeating of what I said about, you know, how many cases had. So we had the total of 85 cases that had pressurized or safety valve failure to close. And 40 of them had the opportunity because the failure rate was greater than 30 percent, had the opportunity to fail the containment earlier.

CHAIRMAN STETKAR: We have to have you on the -- you have to admit guilt on the record.

DR. GHOSH: Yes, I am admitting guilt. We updated this slide after we gave you the printouts, and we didn't replace it. So I think there's one sub-bullet missing in case you are trying to match it up. Sorry about that.

MEMBER CORRADINI: We were about to ask about that.

DR. GHOSH: Yes, sorry about that. We thought it was confusing the story. So, we'll let Hossein finish. If you have a question about that, ask me.

MR. ESMAILI: Okay. So everything that you saw in my previous slides was that 40 of these cases had failure to close with an area greater than .3. But these were all candidates for having early containment failure; right?

Out of this, 17 failed to complete. Twenty-three went to completion. And out of these 23, obviously four of them failed the containment.

MEMBER CORRADINI: So did you do forensics on the calculation that you couldn't restart the 17 to finish them? That's what I'm curious about is why you you didn't --

MR. ESMAILI: It didn't go back.

MEMBER CORRADINI: -- why couldn't you

restart and finish it?

MR. ESMAILI: Did we do the -- Kyle, did we run the calculations or what was, what was the story on the re-running of the calcs?

MR. ROSS: Kyle Ross with Sandia Labs.

No, we didn't. We didn't attempt to restart those calculations. When you do that you have to, you have to finesse the calculation to move, to move forward. Then in doing that you can introduce changes that can cause the calculation to take a slightly different path. So we didn't, we didn't attempt to restart those.

CHAIRMAN STETKAR: It strikes me -- I know nothing about MELCOR -- it strikes me that something isn't working right. Because the preponderance, as you're aware, of the failed runs involve precisely the complex conditions that lead -- that could lead, I don't know whether they would lead to early containment failure.

MR. ESMAILI: Right.

CHAIRMAN STETKAR: They involve stuck open relief valves and whatever else is going on. And it's by far the preponderance.

MR. ESMAILI: Right. So out of these 17 that failed to complete, it is very possible that a number of them could lead to early containment failure.

CHAIRMAN STETKAR: And we have no idea what number that is? It's someplace between zero and 17.

MR. ESMAILI: We don't have that, we don't have an idea. Yeah, we don't have an idea.

But what we did was that -- and this is something that we are not mentioning in the report. I think there is a footnote in the report.

CHAIRMAN STETKAR: There's a footnote in two or three places that mentions this so-called smaller-scale UA where it's not yet --

MR. ESMAILI: Where you are focusing in this, you know, we have just not changed, we are blowing up this failure to close with an area greater than .3, and that shows that as long as you have this condition then you are going to have more containment failure cases.

In our case, so, so in other words we have 4 out of 23; right? So 4 out of 23 we had fail. And I guess the point I am trying to make is that even if all these 17 cases led to failure of the containment, it's still going to be a very small fraction of the total number of the realizations. Because you are still going to be bounded by the fact that you have a very small number of the realizations that are falling in that region that has potential for failure.

CHAIRMAN STETKAR: But if they all went to early containment failure it could be five times as high as what is currently predicted.

MR. ESMAILI: Right, right.

CHAIRMAN STETKAR: And that's the concern. We don't know where it is within that, within that range.

MR. ESMAILI: No.

DR. GHOSH: Let me just insert here. So, we don't, we don't have the results to share at this meeting. We didn't have time to, you know, sufficiently review it and document it before today. But eventually we will include the documentation in the report, which right now is just footnoted as a place holder.

But we ran, so within the 5 percent of the sample space that we really care about, we recognize that there were a number of -- there are extra failures in exactly this region we care about. So we ran hundreds of more cases just in the zoomed-in region. So we constrained the sampling to be, to only have open area fractions greater than 30 percent, and have cycles to failure of less than 65.

CHAIRMAN STETKAR: That's not written

anywhere in the current -- back in Appendix E there is something that I thought you ran, like, 18 more cases. But you're saying hundreds of them.

MR. ESMAILI: Yeah.

DR. GHOSH: Yes. We, we approached this in a step-wise fashion. We originally ran tens of cases. And then we wanted to really be sure that we weren't missing anything, so we've run hundreds.

The ones that we can say in terms of the percentage of cases that go to early containment failure in this set of hundreds is similar to the percentage that we saw in 23, the 4 out of 23, whatever that is. It turned out to be about 17 percent of the cases in the zoomed-in region that we care about.

So that lends some confidence to, you know, the small sample size --

CHAIRMAN STETKAR: Take about two or three more tries.

DR. GHOSH: Exactly. Exactly. That's what, that's what we think we missed. There's probably an additional two failure cases that we missed in that 17, but not that, you know, that we missed something too.

MEMBER CORRADINI: Not 17. Not 17.

MEMBER BLEY: But let me follow up where

John started.

DR. GHOSH: Yes.

MEMBER BLEY: When you did these extra runs to test that area did -- because, as John said, it's kind of suspect that something's funny in that region and what the codes were -- did you have about the same number of uncompleted runs?

MR. ESMAILI: Yes.

MEMBER BLEY: Did you report that?

DR. GHOSH: Yes, yes.

CHAIRMAN STETKAR: Even in the 18 back in Appendix E, 4 of those --

DR. GHOSH: Yes.

CHAIRMAN STETKAR: -- didn't work.

DR. GHOSH: So we had a similar --

MEMBER BLEY: That's more than --

CHAIRMAN STETKAR: Yeah, that's more than this.

MEMBER BLEY: That's a higher percentage. That's what --

CHAIRMAN STETKAR: That is, that's, yeah.

DR. GHOSH: Right. So that continued to be a challenge. But it was just getting more samples successfully completed in this region to make sure we weren't missing something.

MEMBER BLEY: Because some spot in that region is --

MEMBER REMPE: So I'm puzzled and I'd like to understand better why you can't do restart runs. RELAP regularly over the years would have things happen and they would stop the run.

It's on.

And they'd save enough parameters you can do restart. That's how you look at a lot of sensitivity studies. Could you explain again why you can't start and go through it? I'm a little puzzled. Maybe I could understand it better.

MR. ROSS: Sure. So you have to, you just have to change something, submit enough on restart to get to the calculations to move along a little bit of a different path to get around whatever difficultly it was having. And in doing that you may impart enough of a difference to the calculation that it goes down a slightly different path than it would have if it had not failed originally.

MEMBER REMPE: So aren't there some parameters you can check like losses of mass, or things like that, so you know that you don't adversely affect the follow-on stuff.

MR. ROSS: There are, there are.

MEMBER REMPE: You could do it; right?

MR. ROSS: There sure are. Could add some believability to what you've done. But, still, just an experience, there's a question as to whether you might have moved the calculation to a different place than it was done originally.

MR. ESMAILI: So, what Kyle is talking about is exactly what Larry Hunt was just talking about on April 18th, that we have this, we have this inherent numerical variance; right? So the moment you start changing things like time step, et cetera, so you're going to be in a different trajectory. And this is what he did not want to do. He did not want to baby these calculations because it would change the result, it would not be --

MEMBER REMPE: I guess I'm still puzzled. If you watch those parameters, like mass loss and different like that, carefully, yeah, it takes time, but if you watch it carefully you shouldn't go to a different trajectory.

MR. ESMAILI: Yeah, you can. Because for him to run the calculations he would have to change things like the time steps, et cetera, come back. And we know that the moment you do these things, you are introducing some perturbations in the results. And this is what, this is what we don't want to --

MEMBER MARCH-LEUBA: One thing you can do in that case is throw away the calculation, that one bad. And set up a new one, and set up something again the property gives you a failure. Start a new one with a fail. Hopefully it -- otherwise you diluting the bad ones.

MR. ESMAILI: Right. I believe all the 600 calculations, I think the time step and all these conditions were exactly the same. And the moment we started being on a different --

MEMBER MARCH-LEUBA: You had an unlucky run. You drive out the node that gave you a positive something or other, and it failed. So, but instead of throwing it away, and it's one of your few sample calculations that's bad, I think you should replace it with a different one in that same set.

MR. ROSS: Yes. And I think these additional calculations that we've run have bolstered the conclusion that, yeah, maybe, maybe we missed one or two occurrences of really containment failure, but not a bunch.

MEMBER CORRADINI: So can I just go back and summarize because I'm still a little unclear.

So you've got the 17 of the 23 -- I'm sorry.

I'm green.

MEMBER MARCH-LEUBA: Seventeen of 40.

MEMBER CORRADINI: Well, no.

MEMBER MARCH-LEUBA: Seventeen of 40.

MEMBER CORRADINI: Seventeen failed, meaning on 23 -- 17 of the 40 failed to complete. And then in that region you then made sure the valve area was greater, open area was greater than 30 percent.

And what was the second thing you made sure of when you resumed again?

DR. GHOSH: That the cycles less than 65.

MEMBER CORRADINI: Oh, if it cycled, if it cycled it would, it would --

DR. GHOSH: It would be forcing a failure there to close.

MEMBER CORRADINI: Within 65.

DR. GHOSH: Before the hot leg goes or whatever. Yeah.

MR. ESMAILI: So basically, you know, we can't do anything with these 17. We actually ran the whole thing by restricting the failure --

MEMBER CORRADINI: No, I understand now. I understand.

So in terms of just this MELCOR issue, is there just something in the mechanics about backing the calculation up that's just not available in the current version of the code? Is it more mechanics in that regard? And then just rerunning a whole new batch is easier? Is it a matter of ease of computation versus having to noodle with it?

DR. GAUNTT: Well, I think there's a lot of ways you can run at this problem. And we've talked about some of them.

MEMBER CORRADINI: Okay. But this is the most --

DR. GAUNTT: But you can back up and take another shot at it with a different time step. The restarts are all there. The concern is just now you've got a new -- you know, it's not the original sample anymore.

MEMBER CORRADINI: All the slopes are not exactly the same.

DR. GAUNTT: Yeah.

MR. ESMAILI: Yeah, we cannot be sure of that.

MEMBER CORRADINI: Okay. Thank you.

CHAIRMAN STETKAR: But just out of curiosity, and you used 600 total really samples, runs. Have you done any forensics to gain confidence that that's adequate to achieve convergence of the results, in other words that you shouldn't run 6,000 runs? I mean this is part -- If you'd run 6,000 and that was converging, it doesn't make any difference where I set my random number generator, it's going to converge. It does if I have a limited number of samples.

DR. GHOSH: Yes, so in our previous iterations I think we have documented the bootstrapping methods we used to kind of show convergence and the metrics that we care about the most. And, again, we definitely felt 600 was enough ere, with the exception of this one sample --

CHAIRMAN STETKAR: I was going to say, --

DR. GHOSH: -- where we were in this one.

CHAIRMAN STETKAR: -- I wouldn't worry about resetting things and running another batch of 600 samples if I was confident that I would reach convergence, you know, within that 600.

DR. GHOSH: Yeah, it was just this one sample area that we were worried about. Everything else seemed to be stable.

MR. ESMAILI: Well, there is no reason to believe that there is something special about this 17 compared to the other 23. So the fact that out of this 23, 4 of them failed, and then we don't expect all the 17 of these things to fail. And this is what Trey has been doing by running these additional calculations, confirming that, indeed, we are not going to be in a space where all, you know, where all of the cases that have pressurized safety valve failure to close with an area greater than .3 are going to fail the container. They're still limited by that, that number that you had in the draft UA, which in this case is about 17 percent. Last year it was about 20, 25 percent.

It's still not going to change our overall picture of, you know, having a few more early containment failures.

CHAIRMAN STETKAR: As long as you need in the final study to really tell that story because the current report emphasizes very strongly four, only four, only four and only from two from the middle, two from the end of cycle. Four out of 600. Look how rare it is.

You know, it might be 8 out of 600, which is still a small number.

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: So don't, you know, you've got points that have nice red dots on them to reemphasize the fact that there are only four of those things. If there is still substantial uncertainty, yeah, it's only a factor of two, but you really need to tell that story.

Right now it's all hidden in -- well, I mean it's not told because it's in the footnotes.

MR. ESMAILI: I guess we would be more worried about it if, like last year, we were 25 percent and that 25 percent goes to 50 percent. Yeah, I understand.

CHAIRMAN STETKAR: That's still -- I'm just as worried about telling the story.

MR. ESMAILI: Right.

CHAIRMAN STETKAR: Even if it was the difference between 1 and 2.

MR. ESMAILI: Right. And I think we are 20 percent clear.

CHAIRMAN STETKAR: Rather than, you know, 25 to 50 percent.

MR. ESMAILI: Absolutely, yes.

All the forensics that we are doing we're trying to really, you know, zoom on what is important, what's not important. So the story telling is going to continue in time.

CHAIRMAN STETKAR: It's important to get the message across because it's, regardless of the rhetoric here, it's a really solid piece of work. And I'd hate to see it minimalized because of too much focus on precision.

MR. ESMAILI: Sure. I agree.

CHAIRMAN STETKAR: Because people are going to take those precise numbers out of context.

MR. ESMAILI: Yes. And that I think we have to make clear, you know what I mean. And you asked a lot of questions about the safety valve. And you know how important we are, how important the safety valve, how I can change this 408 to maybe 20 or 30. You know, that's another thing. That's even more than, you know, -- So, anyway, so the last, the last we said that in some of these, I think this one, Mike was asking that some of the cases we have ignition from the PRT, 23 of the cases.

Another, as a matter of fact, out of these 23, 2 of them had early containment failure. So meaning that, meaning that 2 of --

MEMBER CORRADINI: So -- did not get to the PRT so it stayed warm enough to ignite.

MR. ESMAILI: Or we didn't have enough ice melting by the time that could cool it.

And besides, you know, these things are coming off of the safety valve. They have a very, very small residence time. They're not going to cool immediately. So we are talking about cases where the core is really, really hot. These are the cases when the safety valve failure to close, core is very, very hot, so we would expect whatever is coming out it would be very, very hot. Pass right through the water and just make it very hot inside the PRT.

MEMBER CORRADINI: Okay.

DR. GAUNTT: Okay. Now we're going to look individually at several cases, only four out of the 16 or so we highlighted in the report. And, again, before we go through them, just presenting up this list of overall findings that came out of it.

I want to bring these bullets up before we step through any of the cases.

So, high level observations. Of the early containment failures, the four cases that failed, we note that the consequences with those early failures are generally higher than the cases of late failures where you benefit from the gravitational settling.

Those, and I mentioned this before, we've said it several times, the early containment failures only happen on the first hydrogen burn from -- and it only occurs with the in-vessel generated hydrogen. There's much more hydrogen created subsequently ex-vessel, but it's only that first burn from the in-vessel generated hydrogen that produces a containment failure burn.

MEMBER MARCH-LEUBA: Is that because of the availability of oxygen?

DR. GAUNTT: Actually, late in time generally there is a core-concrete interaction going on. And there is also a circulation pattern because those doors are now open. And what you're finding is the -- I'll show it in some plots later -- that the hydrogen that's produced from core-concrete interaction is almost continuously combusted because of the, you know, high temperature from the MCCI.

And it ends up pulling oxygen out of the dome and down into the cavity. And so it's just sort of prophylactically consuming the hydrogen before it can get to the dome. And ultimately what shuts it down, what shuts down the burns is there's no more air. You can see that in the, in the curve.

So these first burns that potentially can fail the containment they happen in the lower compartment -- that's where the ignition source is -- and propagate to the dome. And of all the calculations we looked at, you had to have accumulated 150 kilograms or more in the dome in order for it to fail.

Now, because we sampled on the containment rupture pressure, when you look at the first burn pressures that happen, some of them are pretty high. And if you had sampled the lower containment rupture pressure, some others may have, you know, accidentally gone into the category. So, wanted to point that out.

Then these early containment failure source terms are generally higher because there is at that time often considerable airborne cesium iodide and fission products early on.

In contrast, the late containment failures, as we've said a couple of times, the protracted safety valve cycling generally results in less hydrogen accumulating in the dome at the time of an ignition. And that's how we avoid the pressure failing combustion.

The ex-vessel, as I say, the ex-vessel core-concrete generated hydrogen greatly exceeds the in-vessel hydrogen. But it produces almost continuous ongoing combustion. So even though it's way more hydrogen, it never can accumulate to create a large burn in the dome.

Finally, this late hydrogen burns are ultimately shut down due to consumption of the remaining oxygen.

The late containment failures are coming on from this ramp-up, this steady sort of static over-pressurization, strongly influenced by the decay heat in terms of that ramp rate. It's interesting that, while the components of the pressure include the non-condensable gases that are produced from core-concrete interaction, they're not insignificant, but actually the largest partial pressure in that total static pressurization is from steam.

And that's just simply from the whole containment is heating up. And moisture in the containment is contributing to the overall partial pressure. That's a pretty interesting finding.

As we mentioned, all of beginning of cycle cases and a few of the middle cycle cases did not actually produce a failure until after 72 hours when we quit looking. They were on a trajectory but some of them may have been considerably longer before.

And I said generally speaking the late failures have a reduced source term because there's no longer that initial airborne from the early in-vessel. But we have seen in some cases a release at the time of containment failure that is caused by a revaporization of deposited cesium. And late in time it's most often coming off of the steam generators.

Now, at the conclusion of the walk-throughs here that we're going to do we have a short animation to just kind of graphically show that deposited cesium that's been sitting in the steam generators and how it can come off on decompression of the containment. So we'll see that at the end.

So let's proceed.

The next few slides we don't have to spend a lot of time on. But they, this is kind of just the generic station blackout signature that we see again and again. This is the I guess what we're calling the reference case.

It was selected as a reference case because it produced median-like values of hydrogen and fission product release and so forth. So we selected it as a reasonable median case. And we'll just look at some of the signatures.

This one in particular had no failure to close of the cycling valve, so these valves just continued to cycle and cycle until the hot leg failed. So that's the characteristic pressure signature that we see.

Of course, during the time that the pressurizer safety valves are cycling, as we see there, hydrogen is being produced, fission products are being released, and the whole RCS is in this sort of countercurrent steam flow through the steam generator.

This is what's taking the heat to the hot leg, but it's also moving through the steam generators. And it's during that period of time that you have the circulation pattern in the steam generators that hydrogen is being produced and fission products are being transported to the steam generators.

And the steam generators are just sitting there accumulating cesium iodide. That's how they get there. Because we'll come back to that later.

MEMBER CORRADINI: So, Randy, this is -- I have a question. I don't know where to ask it, so I'll tell you and then you tell me if it's now or later.

I'm confused about the uncertainty parameter on flame propagation upwards, sidewards, and downwards, because they strike me as physical parameters but you're treating those uncertainty parameters equally weighted. And I'm confused. Can you help me?

DR. GAUNTT: I don't think there's anything I'm going to address that covers that. And I'll ask for some help from Kyle on that particular issue. I know we've looked at that a lot.

MR. ROSS: Right. So depending on where a burn may originate, it's questionable whether that burn would need to propagate upwards, sideways, or downwards to move elsewhere. So the amounts of hydrogen you need to have, depending on which way a burn is going to move, are very low.

If a burn is going to move in the upward direction it only needs something like 4 percent concentration to do so. If it's going to move downward, it needs more like 9. And if it's doing some type of lateral movement, it's an intermediate value.

MEMBER CORRADINI: Right.

MR. ROSS: So we don't really know exactly where a spark is going to light a burn. We don't know if it's maybe towards a ceiling or towards lower or somewhere in between. So the uncertainty addresses that.

MEMBER CORRADINI: Okay. So you're using -- so let me -- Okay, then that, I missed that in the explanation, so let me say it back to you to make sure I got it right.

You're using the uncertainty of where the spark is within a room and using this upward, downward, sideways to address that uncertainty.

MR. ROSS: Right.

MEMBER CORRADINI: But it's the physics of it is if I knew where it was going to spark, 4 percent up, 6 percent sideways, 9 percent down, end of story.

MR. ROSS: Right.

MEMBER CORRADINI: So you're using that as the uncertainty in the room location?

MR. ROSS: Yes, that's right.

CHAIRMAN STETKAR: See, I thought I, I thought I understood that when you had the amorphous distributed random ignition sources a year ago. But you don't have those anymore. You have three ignition sources. So I should know, A) the compartment. And there shouldn't be too much uncertainty about these randomly distributed sources.

MR. ROSS: Well, that could be. I mean it's hard to say if you've got hydrogen, little bit higher concentration here or there. And when the hot leg, I mean the hot leg lets go it's probably sending stuff everywhere. So you don't --

MEMBER CORRADINI: But I guess what John's -- what threw me was the same thing that's throwing him, is you guys basically said there's three places that can light off: --

MR. ROSS: Right.

MEMBER CORRADINI: -- where the hot leg breaks; where the PRT exhausts; and when the lower head gives way. So it seems to me spatially I know exactly where it is. So those aren't uncertainty anymore. At

4 percent they go up, side, and down. That's what confused me about using -- it was on page, or figure 3-29 that confused me.

Am I asking the same question you're asking?

CHAIRMAN STETKAR: Yes.

MEMBER CORRADINI: Okay.

CHAIRMAN STETKAR: I thought I understood it. It was confusing a year ago. And I thought I had my head wrapped around it when we're talking about an amorphous spatially distributed random ignition sources that could occur. We didn't know where they were.

Now I'm not sure because it's, the way it's presented in the report it says that within the compartment, within this compartment where I now know I have a release from the PRT, there is somehow uncertainty applied to the flammability limit for hydrogen in that compartment. And I don't know what that means.

I mean, I understand that if it's greater than 9 percent it can go down, and there's not much place it can go from there. You know, and it should be able to go sideways and up --

MEMBER CORRADINI: Yes.

CHAIRMAN STETKAR: -- if it's greater than 9 percent. If it's 6 percent it can go sideways and up. If it's 4 percent it can go up.

MEMBER CORRADINI: So my reason -- thanks for explaining -- but my reason for asking the question was is I'm wondering if there's variability in the calculation that really isn't there, given that you nailed down --

CHAIRMAN STETKAR: That's my concern about that --

MEMBER CORRADINI: Given that you nailed those three places where it can, when it really ignites.

CHAIRMAN STETKAR: But some of the propagation might be artificially suppressed because there's, you know, 67 percent probability that the calculation, you know, suppresses it, or something like that.

DR. GAUNTT: I think we understand the question. And it does make more sense when you're thinking in terms of a --

CHAIRMAN STETKAR: Oh, yes.

DR. GAUNTT: -- random ignition where you don't know where it's at.

CHAIRMAN STETKAR: My note says this random ignition model in previous version assumed the 6 -- it makes sense in that construct, not so much now.

MR. ESMAILI: Yeah. You know, even when the conditions becomes right for an ignition, you know, in the old -- by default, for example, when igniters were available. And I think this is some of the cases that we ran that said that on average it has to be 7 percent. You know, this is taking an average that --

CHAIRMAN STETKAR: Yes, yes, yes. But and still it says when you ran the igniter case on average it's got to be 7 percent.

MR. ESMAILI: 7 percent. So I think, I think this is some of the uncertainty about, you know, the point of ignition. This is within that control value that how much hydrogen should you have before the ignition takes place.

And but I think that's a good point you're raising is that, is that --

CHAIRMAN STETKAR: I would look at it because --

MR. ESMAILI: Yeah. No, no, no, this is right here.

CHAIRMAN STETKAR: -- when you do the forensics on this stuff there are a few cases where that uncertainty on the hydrogen flammability -- I can't remember. Tina knows these backwards and forwards. --

where it does show up as, I don't remember as a single, but certainly within cohorts I think.

DR. GAUNTT: Yes, I think we should look at that. But I guess I'm suspecting of the cases that count we're propagating upwards from a burn that starts low. And so it's probably doing the right thing.

CHAIRMAN STETKAR: I'm just not sure whether it's only doing it 33 percent of the time or not.

MR. ESMAILI: We know when we are propagating from one control volume to the other control volume. We are not touching that because we are just going with this 4 percent, 6 percent, 9 percent.

CHAIRMAN STETKAR: Yes.

MR. ESMAILI: So it's just that when we are in one control volume, the issue that they're raising is that this is what the hot leg is in this control volume, so I know exactly where it is. That's what they're saying is that.

CHAIRMAN STETKAR: And they should know --

MR. ESMAILI: So why doesn't it propagate sideways as opposed to up or down. Yeah.

DR. OSBORN: This is Doug Osborn from Sandia.

It may not necessarily be the pipe that starts it. You know, it could be the aerosols or something else that was hot in that compartment that could have started that burn. So it's not necessarily the hot pipe or the hot PRT or even the hot vessel when it goes ex-vessel.

DR. GAUNTT: We'll look at that. We'll look at it. Thanks for pointing that out.

So where I left at, I guess, it was all during that high pressure phase where we've got the circulation going on, fission products are leaking out of the cycling port for sure, but they're also circulating through the steam generators.

On the second plot you see there the failure of the hot leg allows the accumulators to dump. And generally during that accumulator dump time you will shut down hydrogen production and the hydrogen will level off until you get well into that second boil-down and you will produce more hydrogen at that point.

Here's a couple of other pictures that show that case. And this one did not produce an early burn that failed the containment. You can see in the upper-left the cumulating burn, burn energy. That's global burn energy.

The red curve is showing the growing pressure. You don't see a burn in this case until just before 12 hours in the dome. And it produces a little pulse there and then proceeds along more or less statically in that case.

I'm having trouble seeing the -- what is that? Oh, this is the ice melt. I'm sorry.

So the upper right-hand corner is showing the ice melt out of the ice condensers. And as Hossein mentioned a little earlier, it takes about 12 hours to melt that ice down. And, of course, it begins to flood the compartments in the containment, and you see that flood level come up.

You can see in this case we reached the bottom of the PRT about 6, 7 hours or so, and flooded on up about 25 percent of the height of the -- And then the inside of the PRT you see the water level. And in this case we didn't boil the water out of the PRT.

On the bottom right are the temperature responses of the PRT surfaces and atmosphere. You can see when the water comes up and begins to touch the PRT, some of those temperatures cool down. You see the effect of the PRT.

I suppose the containment failed out there just, yeah, before 60 hours. And I think what we're seeing there is probably some flashing, water level spalling.

MR. ESMAILI: Yes. Yes.

DR. GAUNTT: All right.

All right, next slide.

So that's just a generic signature that you see very similarly again and again. In the full report we walked through a number of select realizations. And we picked them out for various characteristics that they had. We have the reference case 266.

Various other attributes. The case with the earliest containment rupture. The case with the most hydrogen vented through the containment, through the PRT. We walked through all of those in the report. And in the next few slides we're just going to hit these four because they kind of give you an idea of some of the spectrum of results.

So here is coming back to Mike's point about getting the magic combination of conditions that lead to that first burn.

And, wow, the print's so small.

So, to decode this, a lot of information is packed into this figure on the left. You probably already figured it out. But Kyle went to great lengths to identify when conditions were flammable. And they might be not flammable if there's too much steam. It could be steam inerted. That's the blue curve.

And the code there is if it's a solid blue then it's not steam-inerted. If it's a dashed line then there's too much steam.

Similarly, there needs to be enough hydrogen present. And the same kind of pattern there. Solid line means there is sufficient hydrogen; dashed line means you don't quite have enough hydrogen yet to burn.

And same story with the oxygen.

And so this is a case where the first burn -- I guess this came from the PRT, this one from the PRT. This is case 370. And what happened there was right at the time of the first burn you see, for a brief period it doesn't quite show up, but for a brief period all the conditions were right and the PRT initiating that first burn.

After that time --

MEMBER CORRADINI: Can I just stop you there?

DR. GAUNTT: Yes.

MEMBER CORRADINI: I'm trying to, this is the one that I was kind of joking about that everything was a longer time period so I couldn't see this.

What's causing the steam concentration to plummet in the lower -- I assume this is the lower containment.

DR. GAUNTT: Yes, it should be. This is where the burn initiates.

MEMBER CORRADINI: I'm sorry, yeah, it says lower container, CV8.

But just physically, what's making the -- the steam concentration is going up first because the PRT rupture dispersed. So now you get a lot of steam from flashing of the PRT. It comes down and it goes back up and it comes down. So it's just the, it's just the flow oscillation for the flow?

DR. GAUNTT: You know, I'm going to speculate now because each of these things we'd kind of have to dig into.

MEMBER CORRADINI: I know. I know.

DR. GAUNTT: Kyle might have more explanation. But there is going to be steam condensation in the ice beds. And as you produce hydrogen, it's concentration comes up, and that's bringing the partial pressure of steam down a bit. I'm just, you know, guessing now what's all the contributors. But they all add to one.

Kyle, do you have any other explanation

about what's happening there? I see the -- you see some, you know, oxygen concentrations coming up, the --

MR. ROSS: Well, when you get that first burn it looks like that was fairly energetic and that pushed a lot of that steam just in the lower compartment up into the ice chest, it looks like. And that's why it came crashing down. The burn didn't happen until it had --

DR. GAUNTT: Right.

MR. ROSS: -- had this squish down --

DR. GAUNTT: Right.

MR. ROSS: -- below the threshold.

DR. GAUNTT: Right. So, it just illustrates that the conditions have to be right in order to get the burn.

MEMBER CORRADINI: So let me ask a different question then and then I'll stop.

Just because I'm always looking for some piece of the primary system where the PRT is kind of like an extension of the primary system, getting a gush of steam, once I have that pressure of steam and it makes it through the beds do I then always see this kind of rise and fall?

You see what I'm asking? I'm looking for a qualitative signature that is common, or is there no common signature?

Because in this case it was the PRT flashing. But in other cases it could be the hot leg.

DR. GAUNTT: True.

MR. ESMAILI: And it's going to come up; right?

DR. GAUNTT: Well, let's look at a few others and see if, see if it starts to add up.

Casey, do you want to contribute something?

MR. WAGNER: I can adjust the hot leg. This is Casey Wagner, Dakota.

The hot leg does have a distinctive signature because after the hot leg fails you have a burst of hot hydrogen and steam coming out. And then the accumulators dump, and that boiling and flashing that goes on dumps out tons of steam out that hot leg failure, and it will inert the whole lower containment. So you kind of see some spikes of steam at the point of hot leg failure. That's later. That's going to be the second one on this one.

This one --

MEMBER CORRADINI: Okay, but you're kind of getting to my point which is there's a window. And unless, unless conditions are right in the window when the steam concentration is low, because it's high, then it's low below the inerting thing, and then it comes high again because of the fact the accumulators dump.

MR. WAGNER: The hot leg window is tiny. So when we have an auto-ignition source coming out of that hot leg failure, it's the time from the blow-down of a large break LOCA to accumulator dump and that steam coming out which is, you know, seconds, 30 seconds. But that's, you know, that's the burn we see at hot leg failure.

MEMBER CORRADINI: Okay.

DR. GAUNTT: Okay, then the plot on the right, we produced these for all the illustrations. Shows a little bit more global representation of where burns are happening, how much in vessel in-vessel hydrogen's been produced, how much ex-vessel. And in this case you can see towards the end of the in-vessel hydrogen generation we had a burn in the lower compartment at the hot leg. No, the first burn, that's from the PRT.

And that was the larger, that was the larger burn, it's the dashed blue line. And it propagated to the dome. There's a smaller, smaller burn that you see right here. So this is showing where the hydrogen's being burned. And in this particular sequence more hydrogen is being burned in the outside of the dome in the lower compartments than inside of the dome.

And here you see when the ex-vessel hydrogen begins to be burned. It kind of comes up with a slop here instead of those characteristic vertical lines because it's being burned more continuously in the ex-vessel phase.

It looks like after about 12 hours there's no more burns going on. And this is due to oxygen depletion. You can see that on the left-hand curve again, there's no enough oxygen although there's plenty of hydrogen.

Okay, next slide.

Here, this is Case 554. This is a interesting one. And we'll come back to this in the animation. I believe this is the one.

This one, 554, it's the earliest containment failure. In this one the safety valve opened up on the first cycle, first challenge. Opened up at a fairly large open fraction, .77. And so it kind of now is a candidate for one of those early containment failure cases.

In this case the conditions for a first

burn at the time, at the time of hot leg failure, first burn at 4.7, 4.37 hours. Hot leg failure in first burn. We met the burning criteria. By that time there was 152 kilograms had made its way into the dome. And so that's another criteria met. And you can see then here, this also, this also was kind of delayed relative to the in-vessel hydrogen. So this gave full, this gave a lot of time for hydrogen to migrate up into the dome when we met those conditions.

So you can see here. In this case the solid blue, which is the combustion in the dome, is quite a bit larger than the initiating combustion in the hot leg region. And that produced a containment failing pressure, as you can see here.

Another characteristic of this -- we don't show it right now -- but in this first containment failure event there is a small, smallish release, not a terribly -- a release of cesium iodide that happens here at the failure of the containment. But we're going to look at this animation I'll show you later. Sometime out here around 24 hours there is a renewed release, actually the larger release of cesium iodide happens out here at 24 hours from this evolution out of the steam generator. But we'll come back to that.

MEMBER CORRADINI: Not to -- So I want to make sure that I understand, I understand this. I can go back to your report. But in 370 this is the most hydrogen but containment did not fail?

DR. GAUNTT: 370, yeah.

MEMBER CORRADINI: But containment would not fail?

MR. ESMAILI: 370 the containment did not fail; correct.

DR. GAUNTT: It failed late.

MEMBER CORRADINI: I'm sorry, I'm sorry. But it didn't fail early. I should have said that it didn't fail early.

DR. GAUNTT: Yes.

MEMBER CORRADINI: And you had a release about 4,000 megajoules of energy from the hydrogen burn.

In 554, if I'm reading this right, it did fail early but I only released 250 megajoules of energy from the hydrogen burn. What am I missing?

MR. ESMAILI: This is only, this is only in the cavity. So this is only in one region. So this starts in the cavity. So in this --

MEMBER CORRADINI: So the burn energy is only in the cavity.

MR. ESMAILI: Yes.

MEMBER CORRADINI: Excuse me. I'm sorry.

MR. ESMAILI: But it's clear that, it's clear that, you know, because of this delay, by the time, by the time we get to the burning the hydrogen, you know, you have produced all your hydrogen, it has already moved. So you get this --

MEMBER CORRADINI: I'm just --

MR. ESMAILI: Yeah.

MEMBER CORRADINI: I was comparing apples to oranges. That's my mistake. I apologize.

DR. GAUNTT: And the next one -- what I'm going to say on this one -- Oh, here we go. Okay.

So this one is 316. Three sixteen. This was a beginning of cycle case. And it actually had a failure to close of a cycling safety relief valve. But it failed in such a close position that the next safety valve just picked up and continued to cycle. And so it looks very much like a failure to -- that it looks very much like a valve that continues to cycle.

So in this case, in this case we had conditions where the hot leg ruptured fairly early on before a lot of hydrogen was produced. And so no burn happened at the time of hot leg rupture. So what happened was the accumulators came in, flooded the core, and things are quiet for a bit.

Here you see the in-vessel hydrogen on this side. You see it goes flat which during that period the core is quenched and it's beginning to uncover again. And sometime after six hours you see the hydrogen pick up again following that second burn. And that's when, following the second boil-down, and that's when the first burn occurred in this phase. So this is an interesting twist on the vessel dynamics.

That first burn produced a small pressurization. But, let's see, that's the hot leg.

MR. ESMAILI: So I think what's important here is also that once the hot leg fails, now you have that ignition source. But you can -- so it is as if we have turned the igniters off. You can produce, hydrogen; right? But it's going to come off, you know, it's not going to come off immediately up here. So you have time to burn it.

DR. GAUNTT: Yes.

MR. ESMAILI: So this is the difference when we say that, you know, the first burn is very, very important is because now you didn't have any ignition. So if you're suddenly dumping a whole bunch of hydrogen in there, right, and that can lead to failure.

But if the ignition source is there by the time the hydrogen is coming in, it's going to burn it more slowly, which is shown here.

DR. GAUNTT: So the carry-on to that, when the hot leg ruptured there's potential ignition source but there's not enough hydrogen. So it's not until the second burn in the core produces enough hydrogen and then you see we meet at that point the conditions for a burn. And that's happening basically all in the lower compartment. That's this curve that's climbing up here. And then that proceeds when it goes ex-vessel and we continue to burn that hydrogen as well.

Okay. So the last little vignette here to look at is realization 395. And the main point of this is to show that there's a difference in the total -- in the pressure in the dome versus the lower compartments that if you blow up the time scale you can see.

In this case we have another large burn case where it's igniting in the lower cavity but more hydrogen's being burned up in the dome, almost 250 kilograms.

And then blowing up this pressure spot you can see the peak pressure developed in the dome in contrast to the somewhat lower subdued pressure in the lower compartments as they kind of come to equilibrium.

You know, I guess one of the points we made here is this belief that the weak point in the containment is in the what we call the dome region out towards one of the patches. Is that right, Kyle?

MR. ROSS: Equipment hatch.

DR. GAUNTT: Equipment hatch. And that that is where actually the peak dome pressure is reached. So it's --

MEMBER CORRADINI: So to understand the red versus the blue, I want to make sure I get this right. The reason I'm over -- I use the word overpressurizing, if the pressure in the dome is higher it's because I just have more inventory that's burning after it's propagated.

DR. GAUNTT: Yes.

MEMBER CORRADINI: And then it flows back down through the ice chest back into lower cavity to equalize after --

DR. GAUNTT: You can see them come back together.

MEMBER CORRADINI: -- fractions of an hour.

DR. GAUNTT: Right.

MR. ESMAILI: Eventually, yes, they have to accumulate.

MEMBER CORRADINI: That's the flow path.

DR. GAUNTT: I come out and I start a burn low.

MR. ESMAILI: You start the burn, so you have that ignition. But then --

MEMBER CORRADINI: Then it propagates off it.

MR. ESMAILI: You have a bigger burn in the upper compartment and then the gases will flow and then try to accommodate the pressure.

CHAIRMAN STETKAR: We're going to get to the barrier seal eventually. If you read the story, part of it is the barrier seals blow through and you get circulation that way also.

MR. ESMAILI: You can also get circulation through the, --

CHAIRMAN STETKAR: Yeah.

MR. ESMAILI: -- through the ice chest if those doors are --

CHAIRMAN STETKAR: If they're stuck open you can --

MR. ESMAILI: Yes.

CHAIRMAN STETKAR: -- the flow areas have got to be totally driven by the ice chest, not by the barrier seals.

CHAIRMAN STETKAR: Depends on how they model the barrier seals, which is one of my questions on barrier seals.

DR. GAUNTT: We're going to get to those barrier seals.

CHAIRMAN STETKAR: You're eventually going to get to the barrier seals, trust me.

DR. GAUNTT: So I'm sure you had a chance to look at all those different realizations. There's so much interesting cause and effect in those.

I want to highlight that one case, the, what is it, 566? 554, right.

This one, let me set it up. If you recall, this was an early containment failure. And in that early containment failure of course we had an initial release of fission product that was airborne at that time. Now we're going to move ahead.

The containment's been failed for a long time and we're moving out to must be about 20 hours.

MR. ESMAILI: This is 40,000, so it may be about 12 hours.

DR. GAUNTT: Is it 12? Okay.

MR. ESMAILI: We're starting the simulation from 40,000.

DR. GAUNTT: And we looked at so many of these. Okay.

This is, this is considerably out in time to 40,000 seconds. And what you see in this, the colors, let me explain them. So what we're seeing here, this little bar chart on the right, this is airborne cesium. And it only refers to airborne in the containment volumes, all right? And it also kind of corresponds to what's outside the containment here. That's airborne.

CHAIRMAN STETKAR: Whatever goes out of the containment goes out; right?

DR. GAUNTT: It does.

CHAIRMAN STETKAR: Orange doesn't do much here.

DR. GAUNTT: It does. It's kind of graphically shown here. But it's actually the great outdoors.

CHAIRMAN STETKAR: Okay, yes.

DR. GAUNTT: Right? And what is in the containment, that is airborne.

This other chart here reflects total deposited. It actually includes airborne but it's very dominated by deposited cesium. And it applies only to the RCS. We have to have kind of two color schemes to represent what's going on.

Let me explain that this is the triple represented generator. This is the single. There's actually the same amount in each steam generator but it's a little artifact we have to complain to the SNAP developers to normalize these.

So we kind of set up the color scheme to show. This is cesium that is deposited in the steam generator tubes. And it's something like 12 kilograms of cesium that's hung up in the tubes.

You also see cesium that's held up here in the PRT. You know, this PRT's sort of underwater, so it's cool. The steam generator tubes are gradually heating up in time. We have some plots; you can see it. But that deposit of cesium has put them on a ramp and they'll get up to almost 1,000 K. And what we're going to look at here is the animation as those tubes heat up to about 1,000, they're approaching 1,000 K.

There's also some up in the vessel head. And we're going to watch it kind of move around.

So do you want to start it?

MR. ESMAILI: Okay, so we have 45. We are speeding up this. I just want to get --

MS. SANTIAGO: Yes, because we're about two hours behind in the presentation.

DR. GAUNTT: This will only take a minute.

You'll see the airborne come up in the containment. And shortly after that you'll see the depletion of deposited in the steam generator.

MR. ESMAILI: Like around 75,000 seconds, then you'll start seeing this thing starting to come off.

MEMBER MARCH-LEUBA: So when is the break going to happen?

DR. GAUNTT: You're going to see -- Okay, already. Now the airborne's growing in the containment. We're driving it off of the steam generators.

You'll notice that the deposited in the PRT stays there.

MR. ESMAILI: This is getting -- this is going down. So this is getting lighter and lighter. So this is coming off and adding to this --

MEMBER MARCH-LEUBA: So the hot leg broke?

DR. GAUNTT: It's been broken for almost an hour.

MR. ESMAILI: It's been broken for a long time. It's just that it takes about 15 hours for the revaporization to start happening. And so you can see it.

So it's coming off here; right? It's going down on this route and things are being added to the containment and eventually going -- this is basically the environment.

MEMBER MARCH-LEUBA: So let me see if I understand. The steam generator is heating up and boiling off the cesium and iodine; is that what's happening?

DR. GAUNTT: The steam generators have been dry for a long time.

MEMBER MARCH-LEUBA: That was the thing before and it's blue now.

DR. GAUNTT: Yeah, yeah.

So the larger release of cesium came in this 24-hour time period, not the original containment failure. So I just think it's an interesting finding. And --

MEMBER MARCH-LEUBA: What draws it from inside the tubes to the containment?

MR. ESMAILI: Just heating it by itself. So it's depositing here, it's deposits, deposit on the steam generator but it gets vaporized. It just heats itself up. The structure heats up and these things is going to come off.

MEMBER MARCH-LEUBA: By heating the steam generators it's vaporizing it.

DR. GAUNTT: And it's a bigger source of cesium to the environment than the original early containment failure.

MEMBER MARCH-LEUBA: But this is temporary storage.

MR. ESMAILI: Yeah.

DR. GAUNTT: And you see now the gravitational settling is depleting out what's in the dome. But it's now been --

MEMBER MARCH-LEUBA: And there's no more moveable cesium in the core; right?

DR. GAUNTT: There is still a fair amount of cesium in cooler tubes. The cesium is coming off the hotter tubes in the steam generator.

MR. ESMAILI: Not in the core. I think everything, basically everything has come off.

MEMBER MARCH-LEUBA: It's already out.

MR. ESMAILI: For cesium iodide, yeah, they came off.

DR. GAUNTT: Okay. So we just wanted to show you that. I, it's just a very interesting dynamic I think, and something we hadn't quite appreciated about source term behavior.

So we're going to leave the MELCOR business now and go into the regression analyses.

CHAIRMAN STETKAR: Let's, I'm getting grumblings among the masses here. And I'm older also.

We're going to take a break now until 2:35. All right.

(Whereupon, at 2:20 p.m., the meeting recessed, and reconvened at 2:35 p.m.)

CHAIRMAN STETKAR: Again I will remind you we're going to stop at 6:00. So let's see what we can get through before 6:00.

DR. GHOSH: I'm going to try to do my part to speed things up a little bit. So the next part is basically showing you horse tails of the things that we care about, and then the regression tables.

And so just, for example, on the very first one, this is the horse tails for the in-vessel hydrogen generation. But to be honest, Hossein already told the most interesting part of this story because what we care about the most is the initial part of the generation, up to the time where before you get that first burn because that's really the region where you can have early containment failure possible.

Once you get later out in time the core concrete interactions kind of take over the flow pressurization, so it's not as interesting to look at this metric.

CHAIRMAN STETKAR: But then again, I'll just -- the report has a much more dramatic beginning cycle, middle end of cycle. That's, as I mentioned before, really why I want to make sure we have good foundation about the duration of that beginning of cycle. Because most of the realizations that are up more in the early time come from beginning of cycle in this, in this plot. You can't see it here.

DR. GHOSH: Right. Yeah, there's a bigger spread, and you can get to higher.

CHAIRMAN STETKAR: Right.

DR. GHOSH: That's true.

So just really quick, this was the regression results for the total in-vessel hydrogen, not just up to that first burn. But it's not surprising, the things that dominate the variations in this, the observation model kinetics that we added the sampling to, the time of that cycle. And in the report you have the three horse tails that show very dramatically the difference.

And then, again, we've talked at length about the effect of the primary safety valve cycle. So I'm not going to spend a lot more time on this.

This is an example of some of the many graphs and plots that we have in the report. This one is showing the influence of the observation model. And here you can kind of see that this observation model, too, you have a, in general, a lower in-vessel total production compared to the other ones. The scale is kind of the density of the dots.

Okay. So now we're going to move onto the cesium and iodine release fractions. So this is for all of the realizations, so it includes all three cycle times. And you can very clearly see that there's two sets of results. There's the four early containment failure cases where you have releases that started really early.

And Randy talked about the signature of a couple of those. And then if you survive this period where early containment is possible, and Hossein showed the horse tails where you then end up on this slow and constant pressurization curves until you reach the sample containment rupture pressure point for the various realizations which and you get this spread of results. Really the earliest late containment failure occurs around 40 hours. And then we had a handful of middle of cycle cases that went out beyond 72 hours. And then all of the BOC cases were still on that slow pressurization curve and hadn't failed containment by 72 hours.

So I'm going to talk next about the regression results for the cesium and iodine releases. And the results were run on the total fraction, release fraction, you know, given the core that you started with. So it's the core release fraction that is being used as the output metric.

So this time around we used the same four regression methods that we used the last time around versus the linear rank regression which doesn't capture the interaction effect. And then the three other regression methods which do capture interaction effects which is shown, which is captured under the TI index.

So because it's a lot of information to process, we also have the summary metrics, which are the last two columns. And the main contribution is kind of a representation, if you consider all four methods, how important it was by itself to explain the variation in the results that we're looking at, which in the top table is the cesium release fraction, the bottom table is the iodine release fraction.

MEMBER CORRADINI: So can I just ask something so I understand this?

DR. GHOSH: Yes.

MEMBER CORRADINI: So, if I look at the green, the blue, the red, and the purpose -- I love the colors -- and I accumulate them, cycle and safety valve cycling and the cycle, BOC, EOC, MOC --

DR. GHOSH: In the burn-up cycle, yes.

MEMBER CORRADINI: -- matter the most.

DR. GHOSH: Exactly. Right.

MEMBER CORRADINI: Okay.

DR. GHOSH: Those are shown to be the two most important parameters --

MEMBER CORRADINI: And rupture. Excuse me.

DR. GHOSH: Yes, the rupture pressure, right, is very important as well.

And then that last part on the right shows the conjoint contribution. And you can see for some of these parameters the effect conjoint contribution is quite high. And that also makes sense.

For example, for the number of primary valve cycles here, and also for both iodine and cesium, and then, for example, the eutectic melt temperature you can see how the large conjoint influence, even if there isn't an effect on its own on the total release fraction.

So because of this there are a number of scatter plots that we show in the report that try to attempt to show that the conjoint, the potential conjoint influence of two variables acting together on the result that you care about.

Did you want to say something or ask something?

CHAIRMAN STETKAR: He was just explaining to me what a conjoint is.

DR. GHOSH: Oh, okay. The other thing I do want to point out, we didn't include -- actually, this has already been said -- we decided this time around to not include the sample safety valve area fraction upon failure as an input to the regression. But we recognize that it's important.

So wherever you see the number of cycles pop up it's really a proxy for the safety valve system for both the number of cycles as well as the open area fraction. And the open area fraction is important. All of the higher releases you see for cesium and iodine, not just in terms of whether you can get to early containment failure, but even the late containment cases the area fraction is important.

CHAIRMAN STETKAR: I think, you know, kind of at this level with the explanation that you already have in the report, I think this is pretty useful. But as I mentioned earlier, to understand some of the physics, that other plot that -- I don't remember whether it was Hossein or Randy showed it -- is pretty interesting.

DR. GHOSH: Yeah. Yeah, I agree.

CHAIRMAN STETKAR: And I think that that part of the story, given the fact that you've amalgamated everything into this primary valve cycle thing, which makes sense, you know. You have to think about it a little bit but it makes sense. That other part of the story might be useful.

DR. GHOSH: Yes, I agree. And we do plan to -- actually Hossein's whole section is copied -- we plan to include those plots in the report. We just, we happened to do that after we finished the draft report that didn't make it into this draft. But we do want to add those.

You know, there's not a whole lot to talk about here. This is a scatter plot of the release fractions against the number of cycles. And, again, no surprises.

In order to get high releases you need a few -- you have to fail the safety valve open and you need a fewer number of cycles to be able to get to higher release fractions.

This is an example, again, of some of the plots we have that are trying to show multiple effects. This is looking at the fraction of cesium release on the Y axis versus when the containment ruptured.

This is you can see that all of the long-term containment rupture pressures, as well as the early containment, the four early containment failures up early kind of follow along this line. And I think Randy was talking about this a little bit earlier. Basically, the earlier you fail containment you have more time to release by the end of the 72-hour period. So the later you end up failing, in general you have less releases.

This is a 2D plot for iodine, kind of showing the combined effect of the number of cycles with the rupture pressure. We kind of alluded to this earlier. Even if you're in that sample area where early containment failure is possible, there's still other things that come into play. And obvious one is the containment rupture pressure. I think Hossein mentioned there's some places where you get early spikes that are just under the sample point. And if you have sampled, you know, something lower. So this is kind of it makes sense that they would act together.

This is just another example for the eutectic melt temperature. This one is -- the other one, you know, the lower-left quadrant you're more likely to get higher releases. In this case it's the upper-left quadrant because the higher eutectic melt temperatures can leave you generating more hydrogen and so on.

So now this one is the iodine release versus the eutectic melt temperature on its own, but color coded by BOC, MOC, and EOC. And you can kind of see some of the influence. The BOC cases releases are all just leakage releases because we didn't fail containment. So they're kind of lying at our cutoff for the Y axis.

And you can see that the EOC cases release a little bit more iodine compared to the MOC cases. And you also see a bit of this positive trend in terms of as you get higher eutectic melt temperatures, those are the -- that's the area, the 2-dimensional area where you can get higher releases.

This one is by time at cycle. And it's showing something similar in a different way. You have slightly higher releases in the EOC cases.

And I think I'm going, we're going to probably move on unless anybody has questions about that, just because we're so short on time.

CHAIRMAN STETKAR: The only thing I'll mentioned is that I really appreciated the effort that you made when you went through this exercise to pin things back to engineering and physics rather than just tables of numbers and plots of dots. That I think really helps people. So, I know it took some work, but that's good.

DR. GHOSH: I'm going to turn it over to Casey.

MR. WAGNER: Okay. Casey Wagner, Dakota.

I'm going to start off with input model error assessment.

So we can go to the next slide.

And there were two input errors that were discovered after we finished the 600 calculations. Of course; right? And so we, after they were identified and they came when we were trying to understand the results, so we were digging in and we were making additional single calculations, specific calculations to understand what was going on when we came across them.

And one of them had to do with the barrier seal, the sampled failure pressure. And then the other one had to do with the hot leg rupture temperature model. And so those were the two.

They're discussed in the report briefly in Section 4.3. And then there's a little bit more discussion in Appendix E and some of the effort to understand their input.

CHAIRMAN STETKAR: These -- Yeah, go on.

MR. WAGNER: The, you know, to be defensive I guess, the reason we have the barrier seal program was that we changed from a year ago the way we were sampling the uncertainty in the barrier seal to this year. And that led to a different way that it got input into the model. And it, we ended up with an error. It was reading in a value. There was an exponential notation. It got truncated. And so every single one was a factor of 10 too small.

CHAIRMAN STETKAR: Or factor of whatever the exponential might have been.

MR. WAGNER: And since we were sampling between 15 and 60, I believe, --

CHAIRMAN STETKAR: Yes.

MR. WAGNER: -- it was exactly --

CHAIRMAN STETKAR: Ten.

MR. WAGNER: -- factor of ten.

CHAIRMAN STETKAR: Well, some of them were right. No, --

MR. WAGNER: No, it's always --

CHAIRMAN STETKAR: That's right, yeah.

Now, help me out. I have a few questions about the barrier seal. And this is my only chance to ask them. You only have this slide.

In Section 3 of the report there's the whole story about the barrier seal before the error. And in that section of the report there is the curve that shows failure pressure and temperature. It's Figure 3-32. And that same plot is repeated back in the error.

However, I'm led to believe, reading Chapter 3, that the curves used in Figure 3-34 and Figure 3-35 are used in the study. So I got really confused reading Chapter 3.

MR. WAGNER: Okay.

CHAIRMAN STETKAR: This, this is the curve that's got -- Now, tell me how the curve on the right relates to the curve on the left?

MR. WAGNER: Okay.

CHAIRMAN STETKAR: Because I couldn't figure that out at all.

MR. WAGNER: Yeah. So, we had two sampled criteria. And they're different. And they're ORed. And so we look --

CHAIRMAN STETKAR: Ah.

MR. WAGNER: -- we look at them both every single time step to see whether we have gone into the failure zone.

CHAIRMAN STETKAR: Okay. Let me just --

this is a comment. You need to keep moving.

Chapter 3 does not tell that story.

MR. WAGNER: Okay.

CHAIRMAN STETKAR: Chapter 3 shows me the curve on the left. It shows me an example of sampling from the curve on the left. It then shows a probability density function that corresponds -- and a cumulative -- that shows the curve on the right. And I'm led to believe in Chapter 3 anyway, or at least when I read it, that only the cume curve on the right as a function of DP is used. And, therefore, I lost the notion of what you were trying to tell. I was trying to figure out whether you were somehow correlating DP to some assumptions about what the gas temperature had to be or whatever.

So you actually do in Appendix E, where you talk about the error, I'm then led to believe that you do look at the combination of temperature and pressure.

MR. WAGNER: Yeah.

CHAIRMAN STETKAR: And if that's what you do, I think you need to elaborate on that in Chapter 3 where you're developing these basic uncertainty distributions. Because that, to me anyway, it didn't come through.

MR. WAGNER: Yeah, I appreciate that it

didn't come through. On page 359 we go, two criteria were used to characterize the seal failure. First, the differential pressure distribution is specified, giving a figure, blah-blah-blah. And so we tried to talk through it but it did come out very --

CHAIRMAN STETKAR: The end sentence on that page is, "The cumulative failure probability is shown in Figure 3-35."

MR. WAGNER: Yes, I understand.

CHAIRMAN STETKAR: Which is the one on the left. That's the end of the whole story.

MR. WAGNER: Yes. So the fact that you were confused meant that we didn't convey it.

CHAIRMAN STETKAR: Right. Right. Okay.

So, I got the fact that the intent has always been to sample both temperature and pressure. Good.

How do you treat the error that's in I'll call it the calculation of record right now overestimates the effect of DP because of the factor of 10. And it underestimates the factor -- the effect of temperature is in Appendix E, you have the plot that shows that. Right?

MR. WAGNER: In Appendix E, okay, and so this probably goes to your confusion, it's showing the

ORed evaluation. We, the temperature criteria is actually a separate piece of coding from the DP sampling. So we looked for did we fail on the DP? Or did we fail based on this DP temperature curve?

And so because the DP by itself is off, it kind of shoves us to the left.

CHAIRMAN STETKAR: Yeah.

MR. WAGNER: And but the temperature criteria is still --

CHAIRMAN STETKAR: It's still in there.

MR. WAGNER: Yeah.

CHAIRMAN STETKAR: Okay. Boy-oh-boy, I couldn't -- I missed that completely.

Let me ask you then, so we fix the error. How does the model treat temperature failures. There's a discussion somewhere -- and for the lack of time I won't try to dredge it out -- where it says, well, if we have, for example, a burn in the upper volume of the containment we can have a really hot condition up there. Doesn't last very long necessarily.

That hot condition, or my bigger concern is actually protracted burns in the lower compartment, that there's a temperature plot somewhere that shows that the kind of the average temperature in the lower compartment -- I'll use degrees Fahrenheit -- is kind of around 350 degrees Fahrenheit for a good fraction of the time. And it gets up to 430, 440 or whatever. And there are spikes, you know.

How does the model treat those temperature effects? Does it fail all of the seals or does it only fail the seal segments according to the distribution of seal areas, that figure that shows the area fractions? Which I agree should certainly apply if I have a cold DP.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: How does it treat hot conditions; does it fail all of them or does it fail one by one?

MR. WAGNER: Okay. And maybe it should. But the hot uses the same sampled area.

CHAIRMAN STETKAR: It does. Ah. Ah.

MR. WAGNER: And it probably gets back to the point that we had before. I don't think it matters. I was running calculations to yesterday or Saturday looking for this.

CHAIRMAN STETKAR: Okay. This is where I wanted to get to now that I've kind of framed it.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: Because I don't know whether it matters or not. If you -- my sense is I'm concerned about if, let's take the limit, all of the seals go away, that opens up a pretty big bypass flow path. The bypass flow path can do two things: number one, it can propagate hydrogen both ways; number two, it can bypass the ice condenser. And I have no sense of how that effects subsequent event progression even if I have an early containment failure. Follow me?

MR. WAGNER: Yep.

CHAIRMAN STETKAR: So, allay my fears here.

MR. WAGNER: Yes. So when in Appendix E when we looked it, and then I did more calculations after Appendix E even, even a small sampled area we'd get a robust, natural circulation flow. So it doesn't have to be terribly big. And we would establish that.

We established it too easily in the 600.

CHAIRMAN STETKAR: Yes.

MR. WAGNER: But when we look at the cases that are in the corrected ones that are reported here in E and in other calculations that I've done, it doesn't fail until we get a burn in the dome. So what was fortuitous about Appendix E cases were we were most concerned about the cases that had the conditions that might lead to early containment failure.

CHAIRMAN STETKAR: I know. But, see, that's my concern is that you focused your microscope on that specific set of cases. And --

MR. WAGNER: That dawned on me.

CHAIRMAN STETKAR: -- and sort of I think maybe partially allayed my concerns, but I'm learning more in realtime.

I was more concerned about kind of the middle section of things. Can we get kind of middle timing failures because of the dynamics rather than this bifurcation that we see?

MR. WAGNER: Yeah, I don't think we appreciated that when we wrote Appendix E.

CHAIRMAN STETKAR: Right.

MR. WAGNER: But last week I did.

CHAIRMAN STETKAR: Okay. Well, you're at least about six days ahead of me because mine kicked in on a plane Sunday.

MR. WAGNER: Yeah. So, I ran more calculations.

CHAIRMAN STETKAR: Yes.

MR. WAGNER: And when we had, in particular this is one of the cases that Randy had and that's what kind of got me interested, was we were making this presentation, is that case that had the least hydrogen going through the PRT.

CHAIRMAN STETKAR: Yes, okay.

MR. WAGNER: And so it was one that had some burns in the lower containment. I believe that case never had a burn in the dome. It just kept on burning in the lower containment --

CHAIRMAN STETKAR: Yes.

MR. WAGNER: -- until it's all done. And I thought, oh, my goodness, what could happen if that barrier seal had, you know, did fail?

CHAIRMAN STETKAR: That's right. And that's exactly why I was concerned about how you're treating the temperature effects because you're burning a lot down below.

MR. WAGNER: Well, it's buffered from that. It doesn't see that because it's off the annulus control volume which, you know, I think I was pointing out to Joy, that temperature kind of stays cooler over there. So the burning that's going on here that you get the sharp peaks, by the time it gets over here to the annulus up high it's cool, and it's not enough to satisfy this failure criteria.

So this case we need because we, can we get this natural circulation flow if we modeled the seals as we intended? And, sure enough, we don't fail the seals in the beginning when we have those first burns. In the other case, we did.

And what happened was we're burning down below. But some hydrogen keeps on going up to the dome and we start collecting more and more up at the dome. But because of this continuous burning that's going on in the bottom, once we finally hit four percent up there, then the propagation, you know, with each of those burns I was looking, can I propagate? No. No. No.

Then finally we get up to four percent. And meanwhile we're shoving more and more up there, and we have a weak natural circulation flow. We burn it. That fails the barrier seal. And then you see a robust. And so we get to the same place a little bit later in that case.

And I'll add that to Appendix E.

CHAIRMAN STETKAR: One of the questions that I had is you found the error. I mean orally here we've had a discussion about it. You going to rerun the whole thing with the area fix?

DR. GHOSH: That is not in our plan right now. I think we have done additional --

CHAIRMAN STETKAR: Okay. We'll talk more about plans later then.

DR. GHOSH: Yeah. We've done some additional sensitivities that we will document.

CHAIRMAN STETKAR: Well, but at some point you get to the point where you have kind of the calculation of record. And over here you have a growing list of we've done all of these sensitivities, we've looked at this, and we've looked at this. And we've looked and, yeah, this could be a little bit different. This could be a little bit different.

At some point it might be useful to --

DR. GHOSH: Yeah. I --

CHAIRMAN STETKAR: -- just push the button and let the lights dim somewhere.

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: In Albuquerque he's been keeping it bright for a while.

DR. GHOSH: I just, I guess, in the runoffs that we've done so far there's been nothing that caused us to say -- and we've been there before. Oh, my God.

CHAIRMAN STETKAR: Yeah, I know.

DR. GHOSH: Oh, my God, we have to rerun everything. So we haven't gotten there yet.

So right now it's not in the plans. That doesn't mean we might not get there someday. But right now --

CHAIRMAN STETKAR: Okay.

DR. GHOSH: -- there is nothing that alarmed us to the point that we need to rerun the baseline.

CHAIRMAN STETKAR: Well, I hear you. And, again, I think it then becomes even more incumbent on you to very carefully package this report. Up front people need to know where the uncertainties are. And by uncertainties I don't mean scatter plots and the things that you actually quantified. I mean the kind of weak things that if you were going to push the button again could change stuff. Maybe not a lot, but change stuff.

MR. HATHAWAY: This is Trey Hathaway in the Office of Research.

I believe those source cases we did that focused on the area of increased failure did have this corrected. I'll have to go back and check.

CHAIRMAN STETKAR: A couple of the, a couple of the sensitivity cases, at least if I read the report, did have it corrected, in Chapter 4 did have the error corrected. But they were sensitivity cases, they weren't kind of --

MR. HATHAWAY: Well, this is the way we looked at where we were just focused in from the 0 to 65 samples, I don't know, above 30 percent as an area fraction of the safety valves. I believe it was corrected for those.

CHAIRMAN STETKAR: That's okay. But remember, any words that I can read today has no information whatsoever about that exercise. That's another exercise.

And part of these things, the cumulative effects, for example the area seal is one, this mini-UA or whatever you want to call it about instabilities in the model, if I can call it that, the treatment of the beginning of cycle, middle of cycle, end of cycle, you know, samples, whatever you work out with the fail rates for the valves, each of those individually, if you examine them individually within, you know, constraints, you may convince yourself that within these constraints they don't make much difference.

On the other hand, at some point the cumulative effects of all of them raise questions. So, anyway, that's enough description.

Thanks for explaining it. If nothing else, even if you don't make it push the button again, please re-read section whatever the heck it is, 3 point something or other, as a somebody who doesn't know anything.

MR. WAGNER: Yeah.

CHAIRMAN STETKAR: And make sure it tells the story that you want to tell about both temperature and pressure.

MR. WAGNER: Yeah, okay.

CHAIRMAN STETKAR: Thanks.

MR. WAGNER: So Appendix E is where we'll have that stuff.

And we were trying to hang our hat on the last bullet, that there's little impact. But we've heard what you say.

For both the short-term and the long-term station blackout we did a couple of calculations.

CHAIRMAN STETKAR: I have one more thing, then I'm going to be quiet for most of the rest of the afternoon, maybe.

MEMBER REMPE: It's on the record.

(Laughter.)

CHAIRMAN STETKAR: I said maybe.

You're not going to talk about the sensitivity case you ran on reactor clearing pump seal leakage today. Here's a question:

You folks know how the model works. You ran a sensitivity case. You ran the extremes. You said, show me the earliest containment failure and show me one where everything pretty much works like it's supposed to. And it didn't make -- it made a measurable difference in the early one because it shifted the ignition from the hot leg, the hot leg to the PRT discharge. So you went from four hours to eight hours or something like that on containment failure.

The other one it didn't make too much difference, which we wouldn't expect.

The question is are there cases where the PRT -- I'm sorry, where the pressurizer safety valve sticks open let's say partially but kind of enough -- pick a number, 25 percent open -- where the additional open area if you have the large seal leakage can get you enough hydrogen release such that you can get an early failure? Because you're now blowing down more hydrogen than you were before.

And when I was thinking about doing a sensitivity calculation, I was thinking, well, if I took that nominal 182 GPM, you know, that flow orifice, and then looked at how, how much or how little must the pressurizer safety valve stick open to then start generating early failures, that would be more of a confidence builder than the two extremes that you'll look at.

MR. WAGNER: Yes. I --

CHAIRMAN STETKAR: I don't know what your sense is on --

MR. WAGNER: I guess from the one extreme that I did look at, which was a case, you know, a case that we had -- we had 20 cases that led to conditions that might lead to early containment failure. This is one of them.

CHAIRMAN STETKAR: Yes.

MR. WAGNER: The amount of flow through the relief valve was much larger than what came through for simultaneous loop seal failures.

CHAIRMAN STETKAR: Through the relief valve at what open area fraction, though?

MR. WAGNER: Well, that --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Because I am necking down the open area fraction to something like 25 percent, not 75 percent.

MR. WAGNER: Yes. So did we look at that case? No. But for the -- for a larger case, it is so much --

CHAIRMAN STETKAR: Oh yes. I have that.

MR. WAGNER: But maybe -- maybe there is some gray area there. It just seemed like there was so much you have to uncover, you have to drain them first and get --

CHAIRMAN STETKAR: Yes.

MR. WAGNER: -- to the point where you are spitting out some hydrogen, and so a lot of that hydrogen had already -- well --

CHAIRMAN STETKAR: If -- if -- a lot of it is gone is you have a big hole, that is right.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: I am looking for is there some sort of sweet spot, if you will, that the combination of the two --

MR. WAGNER: Those are -- that's where the UA always surprises us.

CHAIRMAN STETKAR: Yes.

MR. WAGNER: You find some combinations that --

CHAIRMAN STETKAR: The other question I had with regard to that, as long as we're talking, if you were going to do that, can hot gas flows through the seals be an ignition source? Because you are still limited about where your ignition sources are.

MR. WAGNER: Yes. I --

CHAIRMAN STETKAR: You know, that is the --

MR. WAGNER: I suppose so. By the time it gets down there, it might not be quite as hot --

CHAIRMAN STETKAR: Yes.

MR. WAGNER: -- but it certainly could be one.

CHAIRMAN STETKAR: Because, you know, you're still limited about certain conditions coming out of the PRT or out of the -- out of the hot leg or whatever that you're not going to show a burn until those two control elements exhibit the right kind of flows and temperatures --

MR. WAGNER: Yes.

CHAIRMAN STETKAR: -- despite the fact that you -- I am not sure. I don't know enough of the --

MR. WAGNER: I didn't look --

(Simultaneous speaking.)

CHAIRMAN STETKAR: -- that's where you can get a burn --

MR. WAGNER: -- I didn't look --

CHAIRMAN STETKAR: -- coming out of the other --

MR. WAGNER: -- at the temperature of the gas. I just looked at the amount coming out --

CHAIRMAN STETKAR: Yes.

MR. WAGNER: -- through the seals --

CHAIRMAN STETKAR: Yes, yes.

MR. WAGNER: -- and it -- for this extreme case, it was --

CHAIRMAN STETKAR: For the --

MR. WAGNER: -- small.

CHAIRMAN STETKAR: -- extreme case, it would be small --

MR. WAGNER: Yes.

CHAIRMAN STETKAR: -- if you have -- okay, anyway, that is enough on the -- on the seal.

MR. WAGNER: This is one of the few plants that isn't going to use the shutdown seals, and so they are susceptible to this about a 20 percent probability.

CHAIRMAN STETKAR: Yes, and the fact there is a small probability with the -- the Westinghouse seal model, that you can get a lot more flow through those --

MR. WAGNER: Yes.

CHAIRMAN STETKAR: -- seals.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: Okay.

MR. WAGNER: We ran a couple cases to look at the hydrogen mitigation system, which is the igniters, and here is an example from the short-term station blackout, and then a couple were also run for the long-term station blackout. And it's the same story that we showed you a year ago. Basically, we are looking at a case that did have early containment failure, but if we have igniters available, and we just assumed they are available always, but we didn't need them until three hours, or didn't meet the conditions that they were assumed to ignite at until 3.02 hours, so roughly three hours. So if there is recovery of igniters in that time frame, that seems to burn off the hydrogen as it is coming out through the PRT at a rate that can prevent early containment failure.

This plot is truncated at 12 hours. If we were to carry it out, it doesn't prevent containment failure. It just prevents early containment failure. We still got a late containment failure, about 50 hours in this case. So that's all I am going to say about that.

Now we're going to move on to the long-term station blackout, and in some ways, the results here are similar, but we -- we did take -- ended up redoing it because there was some valuable recommendations from -- from this committee that had us take a look at the modeling.

And so on this first slide, I am going to summarize the key changes from last time we visited to now. We contacted Sequoyah. We talked to them about how they would run their turbine-driven aux feedwater in a station blackout condition, and so they kind of walked us through the system, what it looked like, what valves, positions, how they would fail with a loss of AC power, and would they fail open or fail closed? Confirmed what actions they would do in order to operate in this long-term station blackout scenario where they only have DC power and a working turbine-driven aux feedwater. And they confirmed that they would want to try and maintain to the extent possible symmetric conditions so that the -- the plant is -- is controlled and cooled down in a symmetric fashion.

CHAIRMAN STETKAR: Casey, I have to ask this, and I don't know that it makes much difference, but there seems to be a disconnect between what is the unique loop and what is the lumped loop. In the primary model, it is clear that Loop 2 is the unique loop, and 1, 3, and 4 are lumped together. In a lot of the long-term blackout scenarios, you highlight Steam Generator 1 --

MR. WAGNER: Yes.

CHAIRMAN STETKAR: -- and 2, 3, 4.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: Why?

MR. WAGNER: So it was probably me being careless. I think that technically, it is -- Sal actually knows. It is Loop 2 has the pressurizer --

CHAIRMAN STETKAR: Loop 2 has the pressure --

MR. WAGNER: -- and --

CHAIRMAN STETKAR: -- the reason it makes a difference, I will tell you, in terms of hanging things together from just a simple country boy who does engineering, is that there is this whole story about which of the atmospheric relief valves can they get to easily, and how those are treated in the lumped or individual models, and one of those happens to be on Loop 2, but they are -- they are both treated in the lumped model as -- as the 2, 3, and 4.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: So I am -- it would -- it would be good if the primary side story hung together with the secondary side story for the long-term station blackout.

MR. WAGNER: Yes. So we had some struggles on what to do about that, and we ultimately decided that they can operate all four ARVs to the loops. It is far easier. The other two, they can by connecting plant air. And because they -- they desired to do a symmetric cooldown, we simulated that all four generators were being controlled.

CHAIRMAN STETKAR: I am -- you get to the same point in the model, but the stories don't hang together. That is my whole -- my whole point. No matter how you're going to do it, you will get to the point where your simulation is going to show that they are going to cool down using two or more steam generators. It is just that a reader who picks it up, this report now --

MR. WAGNER: Okay.

CHAIRMAN STETKAR: -- will, as I did, kind of notice a disconnect and say this doesn't seem right.

MR. WAGNER: That --

CHAIRMAN STETKAR: It doesn't seem right that on the secondary side, you're grouping -- the way -- even as Tina mentioned, the way you're treating the stuck-open safety relief valves, which I agree -- safety valves on the secondary side were -- they're all hung on the unique steam generator. That unique steam generator is always called Steam Generator 1. It doesn't make any different what you call it. The model would be the same. You could call it Steam Generator 2 just as easily, as long as calling it Steam Generator 2 and modeling it as Steam Generator 2 doesn't somehow actually change the way MELCOR does things. And I know nothing about the way MELCOR does things.

MR. WAGNER: Yes. So we took a symmetric

approach, and that probably needs to be clarified in our write-up that we did it to all four generators. We opened up ARVs in all four. We had a choice to do it in one properly or four, and we picked four.

CHAIRMAN STETKAR: Well, the -- in -- in the write-up, I am led to -- some places it looks like you open them on all four. In other places, I got the impression that you opened up one on Steam Generator 1 and the equivalent of one on 2 and 3 and 4, so that you had a cooldown for the equivalent of two ARVs open.

MR. WAGNER: Yes --

CHAIRMAN STETKAR: And in other places, I get the sense that you opened them up on all four. And --

MR. WAGNER: Yes. So maybe -- okay.

CHAIRMAN STETKAR: Okay?

MR. WAGNER: I understand your --

CHAIRMAN STETKAR: It's on the record.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: Go back and look through --

MR. WAGNER: Yes.

CHAIRMAN STETKAR: -- it. It's just as I read through the thing --

MR. WAGNER: We certainly stuck open

safety valves --

CHAIRMAN STETKAR: When you stuck open -- what I don't know is that -- would people who understand these things -- if you stuck open -- if you treated Steam Generator 2 as the -- the different steam generator, or the unique steam generator, on the secondary side, would the integrated model behave any differently if you stuck open the valves on that particular loop rather than Loop 1? Now I am talking primary and secondary, all integrated.

MR. WAGNER: So --

CHAIRMAN STETKAR: Given what you have assumed over on the primary --

MR. WAGNER: Yes.

CHAIRMAN STETKAR: -- side.

MR. WAGNER: We can't do anything, correct, on the part of the RCS that represents the lumped three --

CHAIRMAN STETKAR: Right.

MR. WAGNER: -- on an individual generator --

(Simultaneous speaking.)

CHAIRMAN STETKAR: No no, that is right. You are limited to --

MR. WAGNER: A lumped --

(Simultaneous speaking.)

CHAIRMAN STETKAR: -- to 1, 3, 4 is the way it is on that side.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: The question is --

MR. WAGNER: Oh, does the -- does doing something to the pressurizer, the loop with the pressurizer, matter compared to doing a loop that doesn't have a pressurizer?

CHAIRMAN STETKAR: Yes.

MR. WAGNER: Yes. No, and again, our nodalization limits on what we can explore there, but we were continuously frustrated by the -- the 1/3 nodalization and our ability to kind of explore some of these things. But --

CHAIRMAN STETKAR: All right. Well.

MR. WAGNER: Okay. I think Randy covered earlier that as an action from the last meeting, we put in a centrifugal pump model into -- to model the aux feed, and so the -- the pump model was part of this new component that is available in MELCOR to model it.

The turbine side of that pump was modeled more from side calculations of the amount of steam flow it would take to drive the pump to that particular power and RPM. The uncertain variables in the long-term station blackout, unless noted, are at the median values that we observed in the short-term station blackout, so the default would be to -- and we will go to the next slide -- would be that we set them to their median values.

And we thought about median, and we -- versus mean. And in most cases, it didn't matter to the response. The median was more physically appropriate from a simulation perspective, and if we had gone to the mean, say on whether we used the mean or the median on the failed to close number of cycles, it was well beyond where we would have hit a failure in either case, so it didn't matter which one we picked.

This shows all the -- the sensitivity calculations that were performed for the long-term station blackout, and we varied things like battery life and when the primary system safety valves failed to close on the secondary side, whether we did some ignition sensitivity studies, and I just wanted to highlight two of the cases right here that are in the red with the boxes.

The -- if the -- the pressurizer safety valves continue to work as they are designed, that would be like Case 0 up in the -- in the first row. We only generated 38 kilograms at the time of the first ignition. If it failed on the first one, and this is commensurate with what we had showed you on the short-term station blackout, we get an order of magnitude larger amount of hydrogen that is discharged to the containment at the time -- or generated, that is at the time of the first ignition, so it echoes the same responses that we had in the short-term.

MEMBER CORRADINI: But because you have the igniters on, you get a series of small --

MR. WAGNER: There's actually three cases being shown there, Mike, so not every case had igniters.

MEMBER CORRADINI: Oh, I am sorry. I thought long-term --

MR. WAGNER: Yes, so they are --

MEMBER CORRADINI: Oh, I see, even --

MR. WAGNER: -- they are --

MEMBER CORRADINI: -- though --

MR. WAGNER: -- sub-cases --

MEMBER CORRADINI: -- you were doing long-term station blackout, you still did an uncertainty on igniters working and not working?

MR. WAGNER: Yes.

MEMBER CORRADINI: Oh, I misunderstood.

MR. WAGNER: Yes, and so it had all those attributes the same, but there was three different calculations done there with different ignition assumptions.

MEMBER CORRADINI: Okay.

MR. WAGNER: Same thing for the next one. And so -- but they all sort of echo the -- the same type of response on the amount of hydrogen that was generated.

So here is a typical long-term station blackout response. This is the reference case, Case 0. Everything is at the median values. In the main graph there, I am showing the primary and secondary pressures, and it looks a little bit different than what we saw in the short-term station blackout just because we have aux feed available. We are able to inject water, cool down the reactor. We have time for operator actions. We cool down, and then at eight hours, the batteries fail, and at that point, we assume that there is a -- a loss of availability of the injection, and ARVs are closed.

In the upper graph, it is showing the secondary response, and I am showing the level of response, and so we are -- unlike the short-term station blackout where we dried out the generators in about an hour, here we can keep them full while the aux feed is available, so at the time that we lose injection, we have full generators, four generators that are full of water, so that's a lot of reserve that will allow us to weather heating that up, boiling it off, and it really extends out that accident.

So if you look at some of those timings there, we get to steam generator dry-out at 18 hours, whereas we lost injection at eight hours, so we get quite a bit of bang there. Pressurizer safety valve sticks open at 19 hours in this case, and then we had hot leg failure at 24 hours. Well, it opens. It doesn't stick open.

So that is a typical case, and now what I am going to move on to is the -- I went one graph too far. Nope, that is good. Next one. This is sort of zooming in on sort of the thermal response of this case, this long-term station blackout, and so the peak temperature response is shown in the graph there, and there's a couple of vents that are identified: the start-of-fission product release, and then hot leg failure, and then we see a cooldown in the peak cladding temperature after the dump of the accumulators, and then we get the second heat-up, and that is actually the much bigger heat-up that generates most of the hydrogen and releases most of the fission products.

If we were to look at what the

configuration of the core is at that time, right before is this picture, and those red lines mean intact fuel assemblies. We are -- we are uncovered, and that is the moment before the hot leg fails. We dump the accumulators, and it looks much like a Large-Break LOCA. We flood the core, and that is what the -- the quench that occurs.

And so if we wanted to zoom in on what do those conditions look like that led to creep rupture, and -- and why do we have such little amount of hydrogen, we haven't gotten into sort of the -- the accelerated oxidation phase, and we are -- this lower graph is showing how we are heating up, and so the top line would be the peak core cladding temperature, and then you can look at the gases that are circulating inside the vessel and going out into the hot leg. They are a little bit cooler, but more or less tracking what the peak cladding temperature is.

And then if we start looking at structure temperatures of the hot leg -- they are the lower curves -- we get up to a point where we are at 1000 degrees, 60 megapascal pressure drop across that -- that hot leg, and we have generated 38, and that is the point where we get to the creep rupture criteria and we fail.

The next two slides are going to show the

-- the sensitivity results. And first of all, we showed the -- the generator response, and whether it was the single or the triple, they are pretty similar responses. And we had a range of calculations and sensitivities that we looked at.

The fastest to core damage would be the -- the case was at 19.7 hours, and that was Case 5, and that was our shorter battery life one, so that kind of accelerated everything. The case that I talked about in the previous two slides got to the start of core damage at 24 hours, and the case that was the longest is when the operators were successful on some of their FLEX procedures to control aux feed. After the loss of batteries, they have some steps to recover instrumentation, kind of on a piecemeal basis, and they have curves on how to control the aux feed, so if they were successful and did that and ran it until the CST emptied, you could have -- the start of core damage could be delayed all the way out to 45 hours.

Next slide.

This shows the containment response associated with those sensitivity curves, and so it kind of mimics what we observed from the start of core damage. We have a range of results there. They were all trending towards containment failure, but it is going to be well past 72 hours and well below the range of failure pressures that we might have sampled. And again, the fastest and the slowest cases were the same as what I described previously.

On the righthand side, that is a little bit different curve than the one on the left, and this is one of the ignition sensitivity cases. And so I wanted to point out that --

CHAIRMAN STETKAR: Use -- you can use the mouse, Casey.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: You have to speak toward the microphone so we pick you up.

MR. WAGNER: Thank you. So we are -- we are showing three cases on here, and we have a case where igniters are available, and that is this -- the least interesting, the screen case. And that is very similar to the short-term. We burn it so we don't have big pressure rises.

The red case is our -- our base case response, and so that is where we actually had PRT ignition on this one. This is on the first -- this is a case where on the first cycle, the pressurizer safety valve stuck open with 100 percent area, so I went to the extremes, blew down, a lot of fission products in the PRT. They boil away the water, and we get our ignition source actually during the -- the blowdown there.

If we turned off that PRT ignition source and ignored it, even though it was present, we were above the auto-ignition temperature of the gases that were coming out of the PRT, if we ignored those and said it had to get to the next strong active ignition source, that would be hot leg failure. That occurs just a little bit later. That delay allows more hydrogen to get out, and this kind of goes to his -- Hossein's story that time is important, so more hydrogen being generated, more gets out, we have a stronger burn then. That would fail the containment, as you can see there.

CHAIRMAN STETKAR: Casey, I have to apologize. I have to go back to the barrier seals, because I -- when I -- there is a Table E-1 in Appendix E where you ran a number of realizations when you corrected the error.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: And I noticed two -- I will give you a chance to find it if you --

MR. WAGNER: Yes, I've got it.

CHAIRMAN STETKAR: If you look at the last two in that table, 562 and 589 --

MR. WAGNER: Yes.

CHAIRMAN STETKAR: -- what I noticed was that in 562, the differential timing on the barrier seal failure is 10.8 seconds, and that 10.8-second difference made a difference from an event where the containment remained intact for 72 hours to an event where it failed at 62 hours. In other words, it moved up the time of containment failure by more than 10 hours for an 11-second difference in the timing of the barrier seal failures, so that was curious.

And the last event, number 589, is also, it's a 12-second difference, and it changed the timing in the other direction --

MR. WAGNER: Yes.

CHAIRMAN STETKAR: -- by like an hour and a third.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: To me, that seems like an awful lot of sensitivity to very small differences in the timing of those seal failures. Do you have any idea what is going on there?

MR. WAGNER: No. We didn't -- I didn't dig into --

CHAIRMAN STETKAR: Because it is mentioned in Appendix E, but it is only mentioned as an observation. To me, that seems fairly strange.

MR. WAGNER: It was -- it was bothersome. No, we -- I didn't do any more analysis on those cases, and --

CHAIRMAN STETKAR: Again, there -- there -- you know, because you were focused on early failures and only early failures, I -- I get why you focused where you focused, but to me, that degree of sensitivity is very curious.

MR. WAGNER: Yes. No, we -- we didn't dig into it. I think it --

CHAIRMAN STETKAR: And it then raises questions -- I mean, Tina said that you didn't look at uncertainty in the barrier seal as -- as an important attribute, right? You just didn't identify it? It certainly isn't, at least through this table, an important attribute that would change this bifurcation between early and late, but it seems to say that something in this model is extremely sensitive to the timing and the conditions of those seal failures, such that that's another thing that raised sort of my level of interest in how are they actually being modeled in terms of both temperature and pressure and sampling of -- I have no idea whether it's the sample of the seal failure area, or I have no idea why.

MR. WAGNER: No, they --

CHAIRMAN STETKAR: So I will just raise that.

MR. WAGNER: They would have the -- they would have the identical seal failure characteristics.

CHAIRMAN STETKAR: Yes, I mean --

(Simultaneous speaking.)

CHAIRMAN STETKAR: -- between, yes.

MR. WAGNER: Everything was the same between the two cases. It was just --

CHAIRMAN STETKAR: And yet --

MR. WAGNER: -- that the --

(Simultaneous speaking.)

CHAIRMAN STETKAR: -- 11 seconds different results and more than 10 hours difference in the timings of containment failure. That is really curious.

MR. WAGNER: Yes. It is -- it is -- you have to -- that's an energetic time. It is during the -- the dome burn.

CHAIRMAN STETKAR: Yes.

MR. WAGNER: That -- that is all I can offer --

CHAIRMAN STETKAR: Okay.

MR. WAGNER: -- right now. It's a -- it

-- it's the most energetic portion of the calculation. It is the first burn, and it is the dome burn.

CHAIRMAN STETKAR: And the second, the 589 is around the hot leg rupture time, I am assuming? It is the four, fourish hours time?

MR. WAGNER: Yes.

CHAIRMAN STETKAR: Okay.

MR. WAGNER: Yes.

CHAIRMAN STETKAR: It's on the record. I just wanted to --

MR. WAGNER: Sure.

CHAIRMAN STETKAR: Sorry.

MR. WAGNER: I think you saw with Larry's presentation a month ago, you know, some of the uncertainty that we can have in -- in identical calculations, and -- but here, we -- we do have, you know -- okay, a bit of difference.

DR. GHOSH: So I think we are finally going to get to some offsite consequences. Should we give it -- Nate, why don't you come on up?

(Pause.)

MR. BIXLER: Okay. We are going to move on now to the MACCS part of the modeling for Sequoyah, and I have a little kind of flow diagram here that illustrates some of the parts of MACCS and how things fit together overall.

I am guessing that most of you are fairly comfortable or -- or have a good understanding of the MACCS code at this point, so I wasn't going to go through all the details on this slide. But I wanted to highlight a few points.

We are going to be looking at -- as the -- as the set of presentations go forward, we are going to look at meteorological data. We're going to look at a number of source terms, including the whole set of them for the uncertainty analysis. We are going to look at emergency response variations, and we will talk about each of those things and how they -- how they affect the results that we get.

The -- as far as quantifying the results, MACCS is capable of producing quite a few different types of consequence results, but for the purpose of SOARCA, we focused on two different ones. Those are individual early fatalities and individual latent cancer fatalities, so the two that are circled there. One other thing I wanted to mention is you just heard about a long-term station blackout calculation with MELCOR. For the purpose of the consequence analysis, we are going to focus just on a short-term station blackout, so you will -- you will be hearing about the short-term station blackout.

Okay. One thing that is new with Sequoyah, and I wanted to make sure this was understood, is compared with the previous SOARCA studies, Peach Bottom and Surry, we included an intermediate phase. So I thought it might be worthwhile to explain a little bit about the different phases that we model in MACCS. These come out of the EPA PAGs, and they describe the three phases that are listed here. But in terms of what they are and what their purpose is, I thought it would be worth just giving a short description.

So the three phases are the emergency phase, the intermediate phase, and the long-term phase. The main purpose of the emergency actions during the emergency phase are to reduce public exposures, and there are three things done to -- actions taken to achieve that goal. One is sheltering the public. The second and main one that we usually consider is evacuation. The third one is called relocation.

Relocation is treated a bit differently than evacuation in MACCS because evacuation is an automatic response to a -- a level at the -- declared at the plant, like a general emergency or a site area emergency. Usually those two things potentially can trigger actions to take place, and those would generally include sheltering and evacuation.

Relocation is a bit different in that it is triggered not by a declaration by the plant itself. It is in response to people who would otherwise receive too large of a dose. So there is a user -- and the way MACCS works is there is a user criterion that is established, and if a person would exceed whatever the specifications of that criterion are, then the person would be relocated.

Okay. And the next phase is the intermediate phase, and that is kind of a continuation where people can -- would potentially continue to be relocated if necessary, but it also has the purpose of planning. It's a planning phase in preparation for the long-term phase, which focuses on cleanup and recovery.

In the long-term phase, then, there are three actions that are taken, three potential actions depending on the circumstances: decontamination, interdiction, and condemnation. Interdiction simply means that land or property is not able to be used, so that implies that people are not there. So those two things go hand-in-hand.

If you look at the durations for the three phases -- and these are -- these are variable in the input, but these are the values that we used for

Sequoyah. You can see that the -- the durations increased by about a factor of 50 between the emergency phase, the intermediate phase, and the long-term phase. For the exposure pathways, the ones that we have considered in Sequoyah are listed there at the bottom of the -- of the slide, and I put them in order of importance.

For the emergency phase, the most important exposure pathway is inhalation. For intermediate and long-term phases, the most important exposure pathway is groundshine. So those are ones that we specifically considered as being uncertainty in the uncertainty analysis, and we will talk about those a little bit as we go forward.

Okay. So this slide focuses on what is different between the analysis that I am -- that we are presenting today and the graphed analysis that was presented about a year ago. The first two bullets kind of go together somewhat hand-in-hand. We redefined the cohorts and the parameters that go with those cohorts in terms of emergency response, and that was largely motivated by comments that you all provided about a year ago. And we went back and -- and rethought some of the things that went into the model. We talked with TEMA. We talked with folks in NSIR and came up with a -- a fairly substantially modified approach for how the cohorts are modeled and how they evacuate, et cetera. So that -- and that first two set of bullets does impact, as I will show later, fairly substantially some of the results that we get, specifically for the cases where we have early release.

Okay. Next, our updated shielding factors: we went back and rethought those. I think that was probably more self-motivated, that we -- we rethought the way that we were treating the shielding factors, and I will talk about that in a slide or two down the -- down the road.

And then the last two are kind of minor updates. The fourth bullet there, the economic values, we updated because the new values became available, and we wanted to include them. I don't think they ended up having much of an impact on the result metrics that we are talking about today and are discussed in the report.

The last one is meander factor. We thought about the -- the derivation of the meander factor. It comes out of -- of Reg Guide 1.145. And that meander factor is derived explicitly using a certain type or category of dispersion data. We were not using that data. We were using something different. So we decided maybe it wasn't so good of an idea to include the meander factor from Reg Guide 1.145. However, that has a pretty minimal impact on the results. It mainly would have affected doses right near the site boundary, and if you were having early fatalities, that could have been an important thing, but we're not seeing -- we hardly see any early fatality risk, so it turns out not to be important.

Okay. So this is kind of a summary of our new representation of the emergency response cohorts. The first one listed there is -- is kind of unique. It is -- it is not within the EPZ. It is beyond the EPZ, the 10-mile EPZ. It is the 10 to 15 mile shadow, and the timeline there is -- for that cohort is shown to the right. That one is a little bit different than the others because the people evacuating start out outside the EPZ, so they are not affected by some of the things that we assume to be greatly slowing down the evacuation within the EPZ, so that evacuation goes a bit faster than any of the other ones.

Within the EPZ, we have actually Cohorts 2 through 9. 9 is not shown here. It is the non-evacuating cohort that we assume to be 0.5 percent of the population. I will just mention that, but I will talk more specifically about the other cohorts.

Cohort 2 is the schools cohort. 3 is special facilities. 4, transit-dependent. Those are all kind of special cohorts. And then we -- in 5 through 8, we get into the more general population cohorts, and we have an early, middle, late, and tail.

MEMBER SKILLMAN: Nate -- Nate, let me ask this, please.

MR. BIXLER: Yes.

MEMBER SKILLMAN: I am confused.

MR. BIXLER: Okay.

MEMBER SKILLMAN: All of the work that I did with emergency planning as an emergency director and support director is we did not -- we did not begin a recommendation for evacuation until the general. We did not take action at the site.

MR. BIXLER: Yes.

MEMBER SKILLMAN: Site was solely to mobilize the site, bring security and bring extra operators on.

MR. BIXLER: Yes.

MEMBER SKILLMAN: And -- and so if you are suggesting that the trigger is the site versus the general, I -- I believe that is contrary to current practice.

MR. BIXLER: Well, it -- my understanding is at Sequoyah, the schools would be triggered. The evacuation of the schools would be triggered by a site area emergency, the sounding of the sirens or transmission of the information from the local authorities --

MEMBER SKILLMAN: Well --

MR. BIXLER: -- and that would --

MEMBER SKILLMAN: -- well that could be the way TVA does it. Okay. That is different than many years that I had where we didn't do that until the PAR, and the PAR didn't come until the general.

MR. BIXLER: Okay.

MEMBER SKILLMAN: And so the protective area recommendation came with the PAR, and that is what triggered either shelter-in-place, evacuation, or keyhole.

MR. BIXLER: Right. In this case we are using a keyhole model because that --

MEMBER SKILLMAN: But --

MR. BIXLER: -- that is --

MEMBER SKILLMAN: -- but you're beginning -- you're beginning with a site area emergency --

MR. BIXLER: Just -- just for two cohorts, though. Only two cohorts are triggered by site area emergency. That is schools, and we also take the early general population cohort to be triggered -- or sorry, the -- yes, the early general population cohort is triggered by site area emergency siren because, well, that -- that group is basically evacuating very early: by our assumptions, ahead of time, before they are actually told to evacuate.

MEMBER SKILLMAN: Is that --

MR. BIXLER: So --

MEMBER SKILLMAN: -- is that the TVA emergency plan? Is that how the plan --

MR. BIXLER: No, no. That doesn't come from TVA. That is just our -- our model for how we think the population might actually respond.

MS. SANTIAGO: Well, we work closely with Tennessee Emergency Management Agency. We ask very specific questions, and I don't know if Matt Dennis is on the line, but we went back after the last meeting with the ACRS Subcommittee and we really worked very closely with TEMA to make sure some of this information that is written here is -- is very much in line with what they would do.

MR. BIXLER: Yes. I think this is a reasonably good look at what we think would actually happen. The -- the Cohort 5 would not be instructed to evacuate at site area emergency. It is just our belief that they might respond early and actually begin to evacuate based on the knowledge that there has been an earthquake and that there has been a site area emergency declaration at the plant. They may be aware that the schools are starting to evacuate, since they are officially triggered at site area emergency, and they may decide to evacuate nonetheless.

But our Cohort 5 -- I should tell you what population fractions each of these are. Our Cohort 1 is 20 percent of the population from 10 to 15 miles. 2 is -- the schools is a little less than 20 percent of the EPZ population. Special facilities I believe is 0.8 percent, so it is a small -- it is a small cohort. Transit-dependent, which are folks who are more homebound and don't have their own vehicle, are unable to evacuate on their own, is 1.5 percent.

And then the rest of the population that is not in any of the other cohorts that we call the general population, we divide it up into 10/40/40/10 split. So 10 percent of the general population in Cohort 5 we assume responds earlier than they are told to do, somewhat on their own. As a precautionary thing, they evacuate early. But by and large, the general population follows TEMA instructions, does what they are supposed to do, and evacuates on a timeline that is -- we tried to make it somewhat consistent with the ETE from Sequoyah.

Okay. So that kind of explains the set of cohorts here. You can see that the general population has --

MEMBER BLEY: Is --

MR. BIXLER: -- the very early -- sorry, go ahead.

MEMBER BLEY: Yes. Is -- is the identify of Cohort 4 known to anybody?

MR. BIXLER: Sorry? The --

MEMBER BLEY: The identities of the people in Cohort 4: are those known, or this is just how you think they --

MR. BIXLER: This is -- this is largely our judgment of the --

MEMBER BLEY: Because that is a --

MR. BIXLER: -- the way the cohorts --

MEMBER BLEY: -- that is a --

MR. BIXLER: -- would respond --

MEMBER BLEY: That is -- you know, the other ones I am sure are pretty easily identifiable. That one --

MR. BIXLER: Okay.

MEMBER BLEY: -- I am not so sure about.

MR. SMITH: Yes. Hi, this is Todd Smith, Emergency Preparedness Specialist at Office of Nuclear Security and Incident Response.

First, in relation to the Cohort 4, yes, those are known, identified. So in every EPZ, the transit-dependent residents will register with the --

MEMBER BLEY: Oh --

(Simultaneous speaking.)

MR. SMITH: -- or with the --

MEMBER BLEY: Ah, okay.

MR. SMITH: -- local community. So they are identified, and --

MEMBER BLEY: Through the --

MR. SMITH: -- and we know the --

MEMBER BLEY: -- through the police or somebody --

(Simultaneous speaking.)

MR. SMITH: -- resources --

MEMBER BLEY: -- like that, okay.

MR. SMITH: -- to pick them up. They know where to go to be picked up.

MEMBER BLEY: Oh, thank you.

MR. SMITH: And then back again with the early evacuees, the -- the assumption there is not so much that they are responding to the siren or the --

the potential casualty at the nuclear plant, as Nate was saying. The assumption here is that an evacuation has impacted the entire EPZ, and there's people who have maybe found that their house was just destroyed, they are without essential services. There's reason for them to maybe mobilize and leave the evacuation -- the EPZ in advance of what is happening at the nuclear plant. And it's a very small portion of the overall population, and it is driven by really the data from the ETE that told us what percent of the population could readily mobilize and -- and leave the area.

MEMBER SKILLMAN: Let me push back: I would like to believe what you have proposed is accurate, but your site area emergency is set by your emergency action levels, your EALs, and so is your general emergency. Is that accurate?

MR. SMITH: Yes.

MEMBER SKILLMAN: Okay. So the SAE comes not because there is some global plan in the community. It comes because there are people like us in the control room who look at the emergency action levels and -- and conclude we have -- we have gone beyond a UE, unusual event. We have gone beyond alert, and we are now at a site emergency. And at that point, we begin to take site action mobilizations and generally do not go into the community unless the TVA emergency plan requires that. If not, the public doesn't get notified until there is a declaration of a general emergency, a 15-minute timeout, a notification to the authorities, and then the recommendation for the PAR.

MR. BIXLER: Okay. Do you want to respond to that, Todd?

MR. SMITH: Yes. Again, for the public, what has happened to them because of the earthquake is what would be driving mostly their early evacuation.

MEMBER SKILLMAN: Well, that could be, but that is probably not because of the plant's site area emergency, but that is probably because of some -- some community activity that has been prescribed by the community, not because of the nuclear plant emergency plan.

MR. SMITH: Right. And -- and from our discussions with TVA and TEMA, the -- the plans don't get that detailed as far as we want the community to do this when this happens or when this happens. It is going to be very situation-dependent, and so the assumption here is that one of those situation-dependent events has happened. And it could also be that when they hear the sirens, they just decide, hey, it is -- it is time for them to leave.

MEMBER SKILLMAN: Yes, but you don't do the area sirens until the general. You don't do the site area emergency sirens in the community. You do not go to -- to the plant sirens until you get to a general, because general normally means core damage.

MR. SMITH: Correct.

MEMBER SKILLMAN: The point I am trying to make is, if this timeline is accurate, then salut, let it be, if that is how the TVA emergency plan is written. But I will tell you from many years of experience, the site area emergency does not sound the community sirens. You do not go to the community sirens until you get to the general, until you declare the general.

MR. BIXLER: My understanding was that the site area emergency sirens would precede the general emergency sirens. Can you -- can you say anything about that, Todd?

MR. SMITH: I would have to go double-check --

MR. BIXLER: Go back?

MR. SMITH: -- in the --

MR. BIXLER: Okay.

MR. SMITH: -- in the plan.

MEMBER SKILLMAN: Let's just stop there. I am trying to raise a flag because that might have an impact on what you're communicating here by the difference between your SAE flag and your GE flag, which appears to be maybe two hours, two-and-a-half hours.

MR. BIXLER: Right, yes, something like that.

MEMBER SKILLMAN: Thank you.

MR. BIXLER: Okay. All right. I will -- I will just say that this -- after -- and I wasn't involved in the communications with TEMA, but some of the team members were, and this was our picture, our understanding of the way things would be done there at that site and the ways that school evacuation would be triggered, et cetera. So that is --

CHAIRMAN STETKAR: But I mean, in other meetings, I seem to recall hearing a staff position that when a site area emergency is declared, people will start to evacuate. I believe I have heard that.

MR. BIXLER: Yes, yes. I believe --

CHAIRMAN STETKAR: And so this is -- this is an issue that is bigger than this particular slide here or --

MR. BIXLER: Yes.

CHAIRMAN STETKAR: -- this particular study. So if the NRC staff believes that, and that is not the case, there are several studies that have been done, some of the post-Fukushima stuff, that have -- that have accounted for people starting to leave when a site area emergency is declared, right?

MR. BIXLER: I believe that is true, that depending on which site you're looking at --

CHAIRMAN STETKAR: Yes.

MR. BIXLER: -- some of them trigger all emergency response on general emergency and then GE siren, while other sites specifically trigger some actions based on SAE. So that is my understanding. I am reasonably confident that that is true.

Okay. Any other questions here? I think I covered everything I wanted to say about this slide.

(Pause.)

MR. BIXLER: Okay.

MEMBER REMPE: Actually -- okay --

MR. BIXLER: Okay, now that I've moved forward.

(Laughter.)

MEMBER REMPE: I was involved in a discussion in another place where they -- with an industry organization where they had said they recently ran a drill, and they did realtime today situations with all the social media and everybody with their iPhone and the videos, and they said that again, they were trying to investigate what would occur in real time, and all of the normal procedures and processes were quickly overshadowed by everybody on their iPhone, and they were trying to simulate that to understand it. And so that might be something else to think about as you're looking at this nowadays, that it doesn't always go as it was planned in the old days.

MR. BIXLER: Yes.

MEMBER REMPE: But anyway, I just thought I would offer that too.

MR. BIXLER: Okay.

MEMBER SKILLMAN: I don't know how many plants have gone to a site. I was the ESD for the site area emergency at TMI, and it was a sunny morning, and we held at a site because we did not meet the requirements in the EAL to go to a general, and we did not ring sirens, and we were very circumspect before alarming the public because we know what will happen. Because when you make that jump from SAE to general and you ring those alarms, you actually introduce some level of panic.

And so there is a need to be extremely circumspect before crossing that line. So I am -- I have lived through an SAE. I know exactly what happened then, and I don't think anything has really changed since then. I think unless the utility has cut the deal with the NRC on their emergency plan to mobilize at a site -- and it might be that in Soddy-Daisy, Tennessee, they do that -- I would think that's more the exception than the rule.

MR. BIXLER: That could be true. Okay. We will take it as an action item to go back and just verify that the way that we're modeling this is consistent with what they would do at Sequoyah.

MEMBER SKILLMAN: Thank you.

MR. BIXLER: Okay.

Okay. Next, this is the list of the uncertain input parameters that we considered. These are -- the categories here are consistent with what we used in the earlier SOARCA work. In particular, I wanted to -- to highlight the -- the ones that are shaded blue there. Those are ones that we have changed since the -- the previous draft of the Sequoyah SOARCA work that you all saw presented about a year ago. So these are ones I am going to say a little bit more about on the subsequent slides -- oh, yes.

MEMBER REMPE: Again, I have a question. This is just my -- me needing to be educated, but I was looking through the report about the impact of dry deposition versus wet deposition, and my understanding from the report is you only invoke the wet deposition if it's raining.

MR. BIXLER: That is right, yes.

MEMBER REMPE: So is dry deposition in MACCS model the same if you're based in a very humid climate like Tennessee versus a climate like Idaho or Albuquerque where it is dry, and how do you consider the fact that it is not -- should that velocity be buried?

MR. BIXLER: So I think you are thinking of hygroscopic aerosols --

(Simultaneous speaking.)

MEMBER REMPE: And is that --

MR. BIXLER: -- and that kind of thing.

MEMBER REMPE: -- considered in the dry deposition, that you have hygroscopic effects?

MR. BIXLER: It is not specifically considered. However, the user could consider it, and you could for example modify the deposition velocities. The deposition velocities are user input, so the user -- it is up to the user how they want to treat that, and in -- some of the aerosols probably would be hygroscopic. Perhaps others wouldn't be. So it is a little bit difficult to -- to make the decision because all of the -- all of the chemical groups, the nine chemical groups that are calculated by MELCOR are all mapped into our set of aerosol size bins, and there are usually ten of them. And so we don't necessarily distinguish from -- from one point in time to another whether the mixture of chemical groups is changing or not.

These are -- the dry deposition velocities are specified once for the whole calculation, so we use a set for the whole transient. So it would be -- it's a little bit difficult to treat that very explicitly in the MACCS models that exist right now.

MEMBER REMPE: And again, because that emphasized that dry deposition is what is driving things in this particular analysis, then I am kind of wondering if maybe that is something -- would it have much of an effect, in your expert opinion, if you --

MR. BIXLER: Well --

MEMBER REMPE: -- did try and consider the fact that it is more humid there than --

MR. BIXLER: Well, it is --

MEMBER REMPE: -- other places?

MR. BIXLER: -- one of the things that we varied over a fairly broad range, and so -- and we didn't specifically think about hygroscopic aerosols absorbing moisture from the air when we thought about the distribution to use, but I think it would probably be -- would fit nonetheless into our distribution, and -- and we certainly would learn something about it by doing the uncertainty analysis that we did. So -- so in a sense, it is there. Not very explicitly, but in a sense, it is treated as an uncertainty in the aerosol deposition velocity.

MEMBER REMPE: Thank you.

MS. SANTIAGO: They just unmuted the lines from Sandia because Matt Dennis I think was trying to respond to the --

MR. BIXLER: Oh --

MS. SANTIAGO: -- prior slide.

MR. BIXLER: Okay. Matt, did you have anything additional to add?

MR. DENNIS: Yes. Can you guys hear us now?

CHAIRMAN STETKAR: Yes.

PARTICIPANT: We can hear you.

MR. DENNIS: All right. Well, thank you.

Yes, this is Matt Dennis from Sandia, and just to respond to the previous question about the timelines, we did incorporate the emergency action levels and some real response data from the Sequoyah after action report in formulating those response timelines in reference to the SAE siren and the GE siren, so we did attempt to be very accurate to the site-specific response that would occur. And Nate was correct in saying that the schools do respond off the SAE sirens, and that is a unique feature for Sequoyah. So we were adding 15 minutes in here and there for the different station blackout scenario that would be experienced, and then how that would get promulgated to the offsite response organization.

MR. BIXLER: Okay. Thank you, Matt.

CHAIRMAN STETKAR: Thank you.

MR. BIXLER: Okay. One thing I should have mentioned while I was on this slide that I just realized I forgot to mention is how this compares with other timelines, for example, the ETE. The ETE, if you look at their normal assumption, no earthquake damage, sunny day kinds of evacuation, you would get a -- from a GE siren to completion of evacuation, you would get a time of about six hours. It is not a high density site, so it doesn't take too long to evacuate.

If you modify the roadmap to account for all the bridges that we would presume might be broken, be out of -- out of order, out of service, that extends the timeline to about eight hours. Here, we modeled the evacuation as taking about 14 hours. We are trying to account for additional delays in things like perhaps uncertainty in how to get out of the EPZ without running into a broken bridge, those kinds of things, so we added quite a bit of additional time for the overall evacuation, especially the tail. They hang out there on the righthand side of the picture. So there is quite a bit of additional time that we accounted for, just because we thought there would be some confusion, things would take longer, people might be stuck somewhere or whatever, and so the overall evacuation would take an extended period of time.

For a comparison in the draft uncertainty analysis that you saw about a year ago, the -- this is about a 50 percent longer timeline than you would have seen back then. So those are some -- some of the things that we changed based on your recommendations.

Okay. Whoops, I am going the wrong way. Okay. So next, I will -- I will talk a little bit more about the shielding factors and the delays in evacuation speeds. These are the shielding factor uncertainty distributions that we ended up using. The top one is for groundshine, the bottom one for inhalation.

One of the things that we did was we -- we tried to -- to make consistent the point values that we selected for the deterministic analyses with the -- the curves that you see here. All of the deterministic analyses used the midpoint of these curves, these distributions that you see on the chart -- the two charts here.

We didn't end up changing the inhalation protection factor distribution. This is the same set of distributions that you would have seen a year ago. However, we did change -- actually, I think we changed all of the -- the groundshine shielding factors. We re-derived those, and I think we did a pretty careful job of -- of re-deriving them. We actually created a couple new distributions that wouldn't have been on there before.

One is for Cohort 3, this -- the special needs group, that would -- you would expect to be in hospitals, nursing homes, places like that. We used high-shielding buildings as the paradigm for how to -- to choose those. And by the way, all of the data to support this set of curves comes out of the expert elicitation that was done, oh, back in the late '90s, NRC/CEC collaboration.

The other new curve that's on the -- on the chart here, the top one, is the one for the schools. We ended up doing a blend of -- of the fact that the school kids would be in school, and we ended up, based on some expert elicitation data, coming up with 15 percent of the hours of the year, kids would be in school. The other 85 percent of the time, they would -- we assumed that they would be behaving like the normal -- the general public. So it's a 15/85 percent blend of those two other curves, and that is how we got the yellow curve. So I think this all holds together pretty well.

Okay. Next is the -- the timing for evacuation. These are the uncertainty curves that we came up with for each of the eight cohorts that do evacuate. Cohort 9 doesn't need a speed since -- a timing or a speed since they don't evacuate. And the -- the general thing that we did was we started out with our kind of mode value, the value that we thought was the most likely to occur. Well, we thought about that and thought about the lower bound, and to get an upper bound, we multiplied our mode value by a factor of two. We just kind of did that consistently throughout this set, and that is how we got those curves. They are all triangular distributions as PDFs.

In the bottom there, you see the evacuation speeds, and all of the cohorts that reside within the EPZ we assigned a mode of 2 miles per hour, a minimum of our triangular distribution of 1 mile per hour, an upper bound of 5 miles per hour. Depending on the state of the infrastructure, the bridges and so forth within the EPZ, we thought that was a reasonable range to represent the timing of their evacuation. For the cohort -- the 1 cohort, Cohort 1, outside the EPZ, the 10 to 15 mile shadow, we gave them a higher speed distribution to reflect less traffic congestion in that area.

Okay. And I think this is my last slide. This is a comparison to give you an idea of how things have changed since the draft SOARCA work that you saw a year ago. This is a specific MELCOR realization. Number 554 happens to be the earliest containment failure, earliest release case. It is an end-of-cycle source term, and you can see the cesium and iodine release fractions are listed there. We coupled that with our point values. For all the other inputs that were uncertain, we -- we just simply coupled the point value with that one source term and then compared the model that you would have used a year ago with the model that we're using now, and -- and those results are shown there.

The draft model is the purple one. That is the year-ago model. And the current model is the one we're using now. And you can see that there is -- for example, for the 10 -- 0 to 10 mile population, those that reside within the EPZ, there is a significant increase in the emergency phase risk. It -- it is much larger than it used to be.

There is also a small decrease in the long-term phase risk. That is because of the change in the groundshine shielding factor for the long-term phase. We made -- as I mentioned before, we made that consistent with the median of our distribution. That happened to be a drop from what was used before that was based on NUREG-1150, and so that accounts for that change.

One thing that is important to understand here, though, this is one of our four -- and again, four is not a hard-and-fast number -- but of the -- in the uncertainty analysis, we ended up with four early release cases. This is one of those, and so the large contribution from the emergency phase there that you see, especially for 0 to 10 miles, would not be there. It would be pretty much gone if we were looking at a late release case source term because those -- by and large, those don't start until 40 hours or later, and we have lots of time for evacuation to occur before that.

Okay. Any questions? I think that is my last slide.

MEMBER BLEY: Hang on a minute.

MR. BIXLER: The emergency phase contribution for 0 to 50 miles would decrease a little bit if -- relatively speaking, if we were looking at a late release, but most of the 0 to 50 mile population don't evacuate anyway, so that part -- that part of the curve wouldn't drop so much -- just a little bit.

MEMBER POWERS: Nate, you have focused on public dose in this activity. One of the areas where I continue to have persistent difficulties --

MR. BIXLER: Sorry, persistent -- ?

MEMBER POWERS: Consistent difficulties, especially in the -- our work on DCDs, design control documents, for new licensees, and even in power uprates, is dose to the main control room and the technical support center --

MR. BIXLER: Yes.

MEMBER POWERS: -- where Gaussian plume kinds of concepts are completely un-useful there.

MR. BIXLER: Yes, I would not use a Gaussian plume model for that.

MEMBER POWERS: And the question comes up: what would you use for that --

MR. BIXLER: Yes.

MEMBER POWERS: -- kind of activity, and is there any attention being given in this SOARCA program to that particular activity, since it is proving --

MR. BIXLER: Yes.

MEMBER POWERS: -- challenging. At least in my mind, it is challenging to do that.

MR. BIXLER: The gold standard for how you would approach that problem would be to use CFD to do the calculations.

MEMBER POWERS: Yes, that is right. But the problem we get into is it is somewhat challenging to get somebody to attack the problem. Now, we have -- we are familiar with French experiments that they have done in wind tunnels with little models of their plants, and quite frankly, I look at the results, and they look completely chaotic to me. You know, they set up experiments that in the long range do set up essentially Gaussian plume kinds -- at least locally around the plant, it is very, very confused.

MR. BIXLER: Yes.

MEMBER POWERS: Is any particular attention being devoted to that problem?

MR. BIXLER: The compromise model that I

am aware of that perhaps could be useful is -- I think it is called Quick. It is developed I believe at Los Alamos, and it's a simplified building weight model, not so complicated as CFD, but tries to get the building weight modeling more or less right. It may be more of a correlation to other data. I am not exactly sure how that model is built. But that might be a good compromise to use for that kind of situation.

MEMBER POWERS: So you see that for us, oftentimes, the limiting dose is actually the main control room dose or the technical support center rather than the public dose.

MR. BIXLER: Yes --

MEMBER POWERS: And so it --

MR. BIXLER: -- yes.

MEMBER POWERS: -- looms larger in my mind --

MR. BIXLER: Yes.

MEMBER POWERS: -- and now, as we move to facilities producing some small amounts of radioactive material, it is clearly the -- the operations staff that is most at risk --

MR. BIXLER: Yes.

MEMBER POWERS: -- and yet that seems to be the area that we don't have a -- a well-understood

--

MR. BIXLER: Yes.

MEMBER POWERS: -- tool, whether it is exactly right or not. I am wondering, you know, is there a -- are there any plans to upgrade our capabilities there?

MR. BIXLER: I am not aware of it. I used to work on a code called RADTRAD that was developed for the NRC. I think it is still used by the NRC to get control room dose estimates. It's a decent code, but it requires the user to put in a dilution factor for how much the plume gets diluted by the time you get to the control room, and that's all okay, but you need to have good input then to --

MEMBER POWERS: Yes.

MR. BIXLER: -- get a reliable result out of it.

MEMBER POWERS: I can get any number I want is --

MR. BIXLER: Yes.

MEMBER POWERS: -- the problem.

MR. BIXLER: So I think -- I don't know. In my opinion, some attention to that problem would probably be well worthwhile.

MEMBER POWERS: Thank you.

MEMBER BLEY: I -- I seem to recall a discussion the last time we talked about this about -- wondering if -- you know, Sequoyah's just 15 miles upstream on the Tennessee River from downtown Chattanooga --

MR. BIXLER: Yes.

MEMBER BLEY: -- if -- if you guys looked at the possibility of, you know, the -- this being a preferred path down along the river between the hills, and if that's in the model.

MR. BIXLER: Well, it is -- it is in the sense that the wind rose reflects that as a preferred wind direction. I don't know if you looked at the wind rose in the documentation, but it is very nearly bidirectional: either it goes north -- north-northeast along the river, or south-southwest.

MEMBER BLEY: Yes.

MR. BIXLER: And a little bit other directions, but barely. The -- the large fraction of the time --

MEMBER BLEY: That really does account for the fact that it was --

(Simultaneous speaking.)

MR. BIXLER: Yes --

MEMBER BLEY: -- okay.

MR. BIXLER: -- and if it happens to be in the mode where it is going south-southwest, it is heading right to Chattanooga.

MEMBER BLEY: Right, okay.

MR. BIXLER: So that is modeled pretty well I think in this case.

MEMBER BLEY: Okay. Yes, I think that covers it. Thanks.

MR. BIXLER: Yes.

DR. GHOSH: So in the next part of the presentation -- and again, I will try to go through this quickly -- this is the results of the uncertainty analysis from the mass portion, and --

(Laughter.)

CHAIRMAN STETKAR: Just ignore that.

DR. GHOSH: People are abandoning ship.

(Laughter.)

DR. GHOSH: This -- this first slide shows the -- the spread of the results in a tabular form, and the next slide shows the complementary cumulative distribution function. And I will just note a couple of things.

So first, this is the conditional risk, so you assume that the accident has happened. And these are the conditional individual latent cancer fatality risks for these different distance intervals. So the first -- one, two, three, four -- the first five intervals are basically annular rings centered around the plant -- well, the first one happens to be a circle too, the 0 to 10 -- out to the 40 to 50 mile annular ring, just so you can see the difference in the risk as you go out, and then the very last column is the 0 to 50 mile circle, because at the NRC, you know, the two distances that we look at the most are the 0 to 10 mile or the 0 to 50 mile.

So if you look at the means going across these annular rings, there -- it is relatively stable. You know, your -- from about 6e-5 up to about 1e-4, and same with the 95 percentiles. They are kind of all in the 2e-4 range, and then in the fifth percentile -- and I will show you on the next slide -- they go down several orders of magnitude.

So what we see from this graph here is that clearly, there is kind of a bimodal distribution, and then distributions within these two modes, and you can probably guess -- yes?

CHAIRMAN STETKAR: Yes, go on. Explain why it works this way.

DR. GHOSH: Yes, so you can probably guess that the -- these initial very small doses that we're getting, these are all cases where the containment has not failed, so these are largely your BOC cases, and probably, you know, the handful of MOC cases where containment didn't fail. So you're getting very small doses --

CHAIRMAN STETKAR: Okay.

DR. GHOSH: -- basically from containment leakage, but you have not had gross failure of the containment.

CHAIRMAN STETKAR: Okay.

DR. GHOSH: If you fail containment, then you jump onto the second distribution of possibilities.

MEMBER CORRADINI: So -- so these are -- these guys love this -- you guys love these curves. This is independent of when it happens? This is --

DR. GHOSH: Yes.

MEMBER CORRADINI: -- strictly the integrated amount through the 72 hours?

DR. GHOSH: Right. Well so the -- yes, the release fractions we show have been the -- if you -- if you take the 72-hour mark, the cumulative --

MEMBER CORRADINI: So this is the 72-hour value?

DR. GHOSH: But -- but these are --

(Simultaneous speaking.)

DR. GHOSH: -- the -- yes, these are -- yes, we did -- okay. So we truncated the simulation at 72 hours, so it's anything that came out within the 72-hour period, and then this is the calculated individual latent cancer fatality risk. So I think, you know, as Nate showed in the modeling, it's the totality of the cancer effects from that release that happened over 72 hours. So some of it --

MEMBER CORRADINI: So it's in those bin areas?

DR. GHOSH: Yes, all of this. But this, again, these very -- these very low-risk outcomes are because you haven't failed containment, so you're only getting a little bit of leakage out until you fail containment. And these realizations, 85 percent of them, you have failed containment, so you have jumped onto a new CCDF, essentially. So --

CHAIRMAN STETKAR: Tina -- Tina --

DR. GHOSH: Yes.

CHAIRMAN STETKAR: -- did you want to explain more about this, or can I make a comment?

DR. GHOSH: The only observation I was going to make is that these CCDFs are pretty tight as you go --

CHAIRMAN STETKAR: Yes.

DR. GHOSH: -- out in the -- the annular rings.

CHAIRMAN STETKAR: In -- in the -- I saw this. This appears in the executive summary, and a lot of folks are going to read the executive summary. And I found the executive summary a bit confusing because the text says "Using the linear no-threshold dose response model, the conditional individual latent cancer fatality risks for the uncertainty analysis ranged from about 2e-4 to 1e-8 for the 0 to 10 mile region, and the individual latent cancer fatality risks generally decreased with increasing distance from Sequoyah. See Figure ES-4." This is Figure ES-4.

If I just read those words, it says there's a really broad uncertainty in the latent cancer fatality risks, and they really decrease the further away from the site that I am. Well, first of all, that is not what this says. It says that there's a bimodal distribution that is around 10^(-4), and it doesn't make too much difference --

DR. GHOSH: Yes.

CHAIRMAN STETKAR: -- how far away I am from the site. It's around 10^(-4).

DR. GHOSH: Right.

CHAIRMAN STETKAR: And then there is

another distribution that is around 10^(-8)ish, and there, you do -- you do see the differentiation.

DR. GHOSH: Yes.

CHAIRMAN STETKAR: So I think that when you describe the results from this study in the area that people are going to read --

DR. GHOSH: Yes. Yes, I think we agree with you, and we're going to update that language. Even this last week, we have been talking amongst ourselves on how best to --

CHAIRMAN STETKAR: Because --

DR. GHOSH: -- summarize --

CHAIRMAN STETKAR: -- quite honestly --

DR. GHOSH: -- the results.

CHAIRMAN STETKAR: -- you know, as I read through the study, I couldn't figure out why the heck this was shaped this way, and it -- and there was nothing in that executive summary to -- to even lead me to -- to understand this.

DR. GHOSH: Yes. Okay, yes, good comment, but yes, we will work on --

CHAIRMAN STETKAR: Okay.

DR. GHOSH: -- rewriting that.

MR. BIXLER: I think we're in a little bit the same boat as some of the MELCOR folks, where we documented -- we put together the documentation, but then in preparation for this meeting, we have given things a lot more thought, and I think we are starting to appreciate some aspects of the results that we may not have thought about when we originally did the documentation.

DR. GHOSH: Okay. So then on the next slide, we have a lot of regression results in the report, but here, I am just showing the ones for the 0 to 10 mile circle centered on the plant and the 0 to 50 mile centered on the plant. Again, this is for the short-term station blackout. And you can kind of see some parameters that look familiar because they turned out to be very important for source terms. So this is -- surprise, surprise, that translates to the latent cancer fatality risk. So we've already talked a lot about the time-in-cycle, the primary safety valve cycle's number to failure.

I will just -- and I know we mentioned this earlier -- I will just note that the time-in-cycle now you can see has risen to the top, and that is because it has a double effect. It has an effect on the release fraction, but it also has an effect on the fact that that release fraction is tied to a different -- different initial core inventory for the EOC, MOC, and

BOC, so it kind of has a -- that effect is even more exaggerated when you come to the latent cancer fatality risk, so that makes sense to us that that has risen to the top.

Now the new -- the MACCS-specific parameters that show up, the most important that we see here is the cancer fatality risk. It is designated eight, and I know we have talked about this before at previous meetings, so you might remember that MACCS is limited to eight organs. So the eighth organ is the residual organ, where actually a whole bunch of organs are mapped onto this residual organ, and in fact, it has quite a large uncertainty as well because all of the uncertainties in those organ-specific cancer factors are mapped onto this residual organ. And because -- so anyway, it makes sense that that one shows up as the most important amongst the MACCS factors.

And I -- I will probably move on, unless there's questions about these results.

(No audible response.)

DR. GHOSH: Okay. So then for the early fatality risk, we continue to compute it. In this case, we have -- similar to the draft UA, we -- we couldn't find a lot of non-zero numbers, but there were three realizations out of the 567 that we computed a non-zero early fatality risk, so we put together this table. But it is really just based on three individual realizations. And even in those realizations, we didn't compute any risk beyond two miles from the plant. And this is consistent with our prior studies.

CHAIRMAN STETKAR: And -- and that is three out of the four early containment failures --

DR. GHOSH: Yes, that is right.

CHAIRMAN STETKAR: -- so three out of the --

DR. GHOSH: That is right.

CHAIRMAN STETKAR: -- 75 percent of them --

DR. GHOSH: Yes.

CHAIRMAN STETKAR: -- gave you a teeny-tiny --

DR. GHOSH: Right, yes, exactly --

CHAIRMAN STETKAR: Okay.

DR. GHOSH: -- because again, this is conditional on the --

CHAIRMAN STETKAR: Yes, yes.

DR. GHOSH: -- accident actually --

CHAIRMAN STETKAR: Yes, yes.

DR. GHOSH: -- happening.

CHAIRMAN STETKAR: Yes.

MEMBER BLEY: I am just a little confused by this chart.

DR. GHOSH: Yes.

MEMBER BLEY: I probably was last time. If -- if you can calculate a mean so that the mean must be aligned -- this must be such a broad distribution the mean is aligned to the right of the 95th.

DR. GHOSH: Yes. The mean is based on --

MEMBER BLEY: It's way up there somewhere.

DR. GHOSH: Right. The mean is based on three out of 567, so it is like the -- the --

MEMBER BLEY: So it is 99, yes, okay.

DR. GHOSH: -- 99.9th percentile.

MEMBER BLEY: Got it.

DR. GHOSH: I don't even know if we should show the table --

CHAIRMAN STETKAR: You --

(Laughter.)

DR. GHOSH: -- we should use the report --

CHAIRMAN STETKAR: -- you --

DR. GHOSH: -- you know, but --

CHAIRMAN STETKAR: -- you probably shouldn't.

(Laughter.)

DR. GHOSH: Yes. It's another thing we

struggle with, how to report the results, because we did have three non-zeros. But it is based on three data points out of 567. Okay.

MEMBER BLEY: Or at least say it's really broad and the mean is way the heck out there.

CHAIRMAN STETKAR: You can't plot it because you couldn't see it.

(Laughter.)

DR. GHOSH: We did try to -- we -- you probably saw our plot in the report, but in retrospect, we decided maybe that was a little bit ridiculous to plot the CCDF based on the three points.

So okay. I am going to turn it over to Trey Hathaway now, and he is going to talk about the reference and sensitivity cases.

MR. HATHAWAY: So yes, I am going to talk about some reference and sensitivity cases we performed using the MACCS model developed for this project. All of the sensitivities are deterministic calculations. We started off with the base model generated for the uncertainty calculation.

CHAIRMAN STETKAR: Trey, is your mic on? Just --

MR. HATHAWAY: I am sorry. Is that better?

CHAIRMAN STETKAR: It is. It is mostly for our transcript.

MR. HATHAWAY: Sorry. So like I -- I will just lean in.

The sensitivities are deterministic calculations. The base MACCS model for the uncertainty analysis is used for all these deterministic calculations, and each sensitivity assumes the same seismic impact on the evacuation.

The sensitivities can be broken up into a few categories. We looked at sort of reference case sensitivities where all we did was vary the -- the source term used in the calculation. We then explored some calculations to look at the effect of a shelter-in-place where we assumed a shelter-in-place of 12 hours or 48 hours, and this is kind of to explore the possibility that the offsite response organization wanted to take time to examine the evacuation network prior to ordering the evacuation, so we just set in a -- a 12- or 48-hour shelter-in-place.

Sort of hand in hand with that, we looked at modifications to the sheltering shielding parameter to sort of look at the effect of the size of an impact on the -- the buildings where people are sheltering. Additionally, we looked at the weather year. That was also brought up last year, so we ran calculations just to look at the weather year and see how the answers scattered. And I am not going to really go into it here, but the report does talk about non-LNT dose response models. But just in general, increasing the threshold decreases the latent cancer fatality risk.

So here is just a summary of the source terms we looked at for sensitivities. The first four source terms are the ones we examined just to look at the effect of the -- the source term. And as you can see, we are looking at three of the four early releases, and one release, which we have discussed before, is the median behavior source term.

The median source term is a middle-of-cycle source term, and it has a 10th percent cesium release at 72 hours and a 0.4 percent iodine release at 72 hours. As you can see, this is a late release. It starts at approximately -- the large increase in the release is at 57.6 hours, and that is approximately when the containment ruptures.

The next realization we looked at was Realization 554, which we have again discussed as the earliest containment rupture. This is end-of-cycle. It has 1.8 cesium release at 72 hours and 5.1 percent iodine release at 72 hours, and you can again see, its release begins at 3.6, and MACCS divides the plume up into hour increments, so the containment ruptures in that hour increment, 3.6 to 4.6.

The next two sensitivities we looked at were to explore the influence of the amount of cesium released. So originally, we were looking at Realization 36 because it had the highest release fraction of all the realizations, but when we went back and were thinking about it, just because it has the highest release fraction does not mean it has the highest release because it has a middle-of-cycle inventory as opposed to an end-of-cycle inventory, so we went back and looked at Realization 395, which is an end-of-cycle inventory. And you can see that 395 has a slightly higher cesium-137 activity release where Realization 36 has a slightly higher iodine activity release. They both have their release at approximately seven hours.

And the last two source terms we looked at are really under the assumption of a 48-hour shelter-in-place. Most of the source terms begin at 40 hours, so with the sort of nominal evacuation plan that is modeled in the -- the Sequoyah uncertainty analysis, most of the EPZ is evacuated prior to the plume release. But when you have this assumed 48-hour shelter-in-place, your evacuation is starting to overlap these late releases, so we went back and looked at some cases with this assumed 48-hour release.

We looked at one of the large late releases. This happened essentially at 56 hours, so it is essentially eight hours after that assumed evacuation would begin. But then we also went back and looked at a release that is at 40 hours. This has a release fraction that is more comparable to the median release case, and it was about eight hours prior to when this evacuation would have begun.

So this is -- this slide presents the individual latent cancer fatality risk, assuming a linear no-threshold dose response model, and it is conditional on the occurrence of the short-term station blackout. Just to say this once, I am going to present the same type of results on each slide. Did someone --

MEMBER CORRADINI: I will wait until you are done because it looks like you're going to present things in a number of different ways, so I will wait until you --

MR. HATHAWAY: Right. So what this is is just source term. We started with the same base model and put in four different source terms to see how the results change.

The purple bar represents the -- oh, and also, in this slide and the subsequent slides, the solid region of the bar represents the intermediate and long-term phase risk -- contribution to the total risk, and the hatched region of the bar represents the emergency phase contribution to the total risk.

So for the median source term case, the case in purple, the conditional risk is 7.6 x 10^(-5). There is a small contribution to the emergency phase that is due mostly to the non-evacuating cohort. The blue bar represents this earliest release case, so that increases the total risk to 1.1 x 10^(-3). And approximately -- approximately 50 percent of the contribution is now due to the emergency phase, and the rest is due to the long-term phase.

The red and green bars present the -- essentially, the sensitivities that look at the amount of cesium released, and because they really released approximately the same amount, they kind of produce the same results. And about a quarter of the total risk is due to the emergency phase. And in these three cases, now a majority of the risk is attributed to these, the tails, the long portion of the evacuation. That is because these releases are so early, the release is actually overlapping the evacuation.

In general, the risk decreases with the increasing radial distance. And also, I just want to say that -- again, that these results look kind of biased to these early releases just because, if we look at a lot of the late releases, the -- the EPZ is essentially evacuated, so we're seeing lots of numbers where a lot of the results are actually probably more in line with this purple bar for these calculations.

So going back, you know, to the previous slide, we have the question of the earliest release didn't quite have the highest magnitude of the release, but it had the highest risks. So we went, and what this plot plots is the evacuation and the EPZ as a function of time. So this is the percentage of the population remaining in the EPZ as a function of time.

As you can see, by the time the release begins for this earliest release case -- oh, and also, overlaid on this plot is the iodine release fraction, and we're plotting iodine because it is important to the health effects, but also -- and the cesium would follow a similar trend. But what you can see in this plot is by the time the release begins for this earliest release, approximately 95 percent of the EPZ is still in the -- 95 percent of the population is still in the

EPZ when the -- excuse me, when the release occurs. But for these later releases that had a higher magnitude, that -- that three-hour delay essentially allows you to get approximately 30 percent of the population out of the EPZ, and during that hour and a half of this largest release, a further 15 percent of the population is evacuated, and that can really attribute to why that emergency phase contribution reduced the time for those two cases.

So this -- this slide has a lot in it. These are the -- well, we --

MEMBER CORRADINI: Can you remind me again: emergency phase, intermediate, and long-term phase? I am sorry that I don't remember now.

MR. HATHAWAY: So the emergency phase is modeled as the first seven days of the --

MEMBER CORRADINI: Ah --

MR. HATHAWAY: -- calculation.

MEMBER CORRADINI: Okay, fine.

MR. HATHAWAY: So it is --

MEMBER CORRADINI: I got it now.

MR. HATHAWAY: Right. It is essentially when the plume is overhead, or the accident is actively occurring.

MEMBER CORRADINI: I should have

remembered that. Thank you.

MR. HATHAWAY: So what this plot is showing is we were looking at shelter-in-places. So we started with the same -- we started with the earliest release case just because that would maximize the effect. It also -- again, it presents the latent cancer fatality risk, assuming a linear no-threshold model, depending on occurrence of the STSBO.

So as I said, what we have done is we have introduced a 12-hour delay and a 48-hour delay in the -- the evacuation before the evacuation occurs. And I was trying to think of what is the best way to kind of present these results rather than just giving you small release categories. But what you see here is the long-range -- excuse me -- the long-term and the intermediate phase risk is pretty constant for all of them, and that makes sense because, you know, these are what's the -- the population is evacuating in these where everyone is sort of gone that is going to leave for these long-term and intermediate phase contributions.

What you see is the introduction of the 12-hour and 48-hour delay alone sort of gives you a sub-linear increase in the risk. So this is an approximately 2 percent -- a factor of two increase in the emergency phase risk for the 12-hour delay and an approximately 3 -- a factor of three increase in the 48-hour delay.

So the next thing we looked at is we wanted to see the effect of degraded shielding factors, so we assume a minimal shielding that is the equivalent of evacuation shielding, and we apply that to the cohorts during that shelter-in-place period. And that increased the risk by a factor of eight for the 12-hour shelter-in-place and a factor of 13 for the 48-hour shelter-in-place.

MEMBER CORRADINI: Are you integrating over these various distance rings, or is this all the way out to --

MR. HATHAWAY: This is the 0 to 10 mile EPZ. I should have --

(Simultaneous speaking.)

MR. HATHAWAY: -- mentioned that. I am sorry.

MEMBER CORRADINI: Okay.

MR. HATHAWAY: Yes. It is written in the --

MEMBER CORRADINI: Yes, it is down there. I just missed it. Sorry.

MR. HATHAWAY: Yes, so this is the risk in the 0 to 10 mile --

MEMBER CORRADINI: Okay.

MR. HATHAWAY: -- evacuation EPZ. And the purple -- again, the -- the green represents we were applying evacuation shielding parameters. This is minimal shielding, and that would really represent very catastrophic damage to the housing within the EPZ. The purple represents -- it is essentially the average of the normal and the minimal, just to give like some cut in between. And it reflects that there is just some fraction of the population in degraded shielding, or just there is some degraded shielding that affects everyone in the EPZ. And that actually ended up being approximately the -- the risk result actually ended up being approximate average of the late and the early. And again, just for context, you see that for the late releases alone, it is more in the 10^(-4), where these early ones are in the 10^(-3) range.

So as I mentioned earlier, most of the evacuation is complete by the time these late releases occur, but when we have this 48-hour shelter-in-place, we are really starting to allow the evacuation of the EPZ and the release to begin to overlap. So we went back and did some sensitivities assuming the same thing. We just increased the shelter-in-place duration and the shielding factors. And let me make sure I am not getting lost.

So this release occurred at 56 hours. It was a very large release, but you're essentially allowed the EPZ eight hours to evacuate. So -- and again, as before, the long-term and intermediate phase contribution to the total risk is pretty constant for the individual source terms.

In this case, while for this case the emergency phase contribution is very small and mostly attributed to the non-evacuating cohort, with the introduction of the 48-hour shelter and release, it increases to -- to about 9^(-4). And again, for the degraded shielding, slightly increases that, but it only increases it by a factor of 1.25 with the minimum evacuation.

This -- again, this is a smaller release. It is more in line with the median results, but it allows you about eight hours of plume release prior to the evacuation, and that introduces -- it is very small, it is like 1^(-6) in this case -- emergency phase contribution, which increases training 6.1 x 10^(-4) and 2.3 x 10^(-3) with the minimal shielding.

Here is the last set of results I am going to present. I know it was brought up last year, what is the -- you know, are you selecting the proper weather year in your calculations? So what we did is we had five years' worth of data available to us, and again, we used that earliest plume release to maximize the influence of the weather. And what this presents is the risk for each year plus the mean risk, just taking the arithmetic average of the years. And you can see that the -- the 2012 year, which was used for the study, is only about 3 percent less in the 0 to -- for the total risk, it is about 3.4 percent less for the 0 to 10 mile ring relative to the mean, and only 0.5 percent in the 0 to 50 mile range relative to the mean. And this makes sense, considering as you go further, everything is kind of dispersed more.

And also, the year we selected is within the bounds of all the weather years. It is not really an outlier, so we felt comfortable that the results are -- you know, it's reasonable to use this year. And also, in the report, I didn't cover it, we also looked at sort of correlations of the weather year to just certain parameters to see how you could sort of a priori pick a case that might bias your results, but since the changes are so small, we didn't really report it today.

CHAIRMAN STETKAR: It is -- it is interesting there because one of the comments that we made a year ago, or observations, anyway, that 2010 was a -- was a dry year, and 2011 was a wet year. And this shows that there's not a big difference, but that the risk is lower in a wet year than it is in a dry year -- the purple is below the green -- which to me, is I guess a little surprising, but you would think you would sample a lot more rainfall during the --

MR. BIXLER: Rainfall is pretty good at giving a concentrated dose to a smaller group of the --

CHAIRMAN STETKAR: Yes.

MR. BIXLER: -- public. Then --

CHAIRMAN STETKAR: And you're --

MR. BIXLER: -- typically --

CHAIRMAN STETKAR: -- getting most of them out.

MR. BIXLER: And then you typically get most --

CHAIRMAN STETKAR: Yes.

MR. BIXLER: -- of them out, so it ends up --

CHAIRMAN STETKAR: Yes.

MR. BIXLER: -- it could potentially give you more early fatalities, but it doesn't typically give you more latent cancer fatalities.

CHAIRMAN STETKAR: Latent cancer fatalities. That is interesting. Thanks.

MR. BIXLER: Yes.

DR. GHOSH: Okay. I think I was going to summarize. I will make this quick.

The next two slides are -- this one is just kind of a summary of the -- the offsite consequence portion of the analysis, and the next slide is kind of just very broad overall conclusions from the study. And since we just talked about all of these, I don't know that I need to -- I think we -- we will update the executive summary to explain a little bit better. Sometimes, when you're trying to summarize, it doesn't tell the whole story, so we will work on --

CHAIRMAN STETKAR: It is --

DR. GHOSH: -- you know, how to --

CHAIRMAN STETKAR: -- it is good to have a separate set of eyes read it because you're all --

DR. GHOSH: Yes.

CHAIRMAN STETKAR: -- you all know the story that you want to tell --

DR. GHOSH: Yes.

CHAIRMAN STETKAR: -- and the separate set of eyes are going to be the folks who pick it up stone cold and try to learn things from it.

DR. GHOSH: Yes, and so we appreciate all the reviews. We really do. And we will take those comments back.

In terms of the overall study conclusions, we see that for the unmitigated short-term station blackout, when we don't credit the igniters, we have two potential containment outcomes. We can either fail early or late. And in the sensitivities where we credit the igniters, we show that you can avert the early containment failure.

We continued to see essentially zero individual early fatality risk, and even for cases where we had early containment failure and early releases to the environment, the conditional individual LCF risks were -- were still small. And in terms of general magnitude, the conditional individual latent cancer fatality risk results are similar to what we have observed in other SOARCA analyses.

So I think with that, I am going to turn it back over to Pat.

MS. SANTIAGO: Thank you so much. And I just will take a look at this slide quickly to note that the SOARCA project has really benefitted the agency in numerous ways beyond the original objectives of SOARCA when we began many years ago, and the models have been updated each time we have done one of these detailed analyses. Even for the Sequoyah analysis, we improved the MELCOR model so that we could look closer at the hydrogen. That was one of the reasons that we were doing the Sequoyah ice condenser containment analysis.

MELCOR studied the accident progression sequences in significant detail, and so the information we gained there actually supported, as I stated earlier, the closure of several of the near-term task force recommendations, which was very important. We also look at math which is used by industry to see if we can learn anything more there and what they are developing to continue development of our codes, as well as international experience.

MACCS is unique, though, because MACCS is used not only by our agency, but by many other federal agencies for much of their work, and it is used domestically with other groups such as universities, and it is used internationally. And MACCS, we have been able to look at environmental reviews. We have looked at economic consequences. We have looked at cost-benefit regulatory analyses, new reactor design analyses, emergency planning risk analyses.

And so the Sequoyah analysis presented today is a very detailed analysis and in general just addresses the Commission's request that we look at the ice condenser plant due to its smaller containment and unique issues. And if I recall correctly, at 1:47 p.m. this afternoon, Dr. Stetkar did say that this was a solid piece of work. So I want to thank you.

(Laughter.)

PARTICIPANT: He has no memory of it. Should we read back --

MS. SANTIAGO: That is why I wanted --

PARTICIPANT: -- the transcript?

MS. SANTIAGO: -- to repeat it.

(Laughter.)

CHAIRMAN STETKAR: Don't push me, Pat --

MS. SANTIAGO: No, no, I know --

CHAIRMAN STETKAR: -- there are many -- many -- there --

MS. SANTIAGO: We have to add some clarifications here.

CHAIRMAN STETKAR: Many connotations of the word "solid."

(Laughter.)

MS. SANTIAGO: So with this analysis, we looked at a lot of different things, and we are still looking at what we need to do in the future for the severe accident models. Sandia is collaborating with

Korea Hydro & Nuclear Power to perform a SOARCA-type analysis on the APR1400, and our staff will follow that research to see if NRC could benefit by updating anything in our model.

We also are starting to look at new and advanced reactor designs, and so the question will come up: is there any code development that we need to do there to answer many questions? Because this list will tell you that we didn't realize how much we could use our SOARCA analysis to answer many different issues that came up over the last five years. I couldn't have predicted this list to you when I started back in 2010, but as we have gone along, it is really -- these analyses are important for a lot of different technical issues that come up.

The other thing I would just like to say is that the in-house capability of the NRC staff has been developed over the last five years, and that is important. There's a handful of folks that are sitting in this room from Sandia National Labs, and there's a handful of folks from the NRC staff, that really have evolved and continue to develop their knowledge and broaden their knowledge in the severe accident analysis. And I think unfortunately Hossein left me, but -- Esmaili -- but these detailed analyses are very important, and the same staff that are working on these analyses are also working on many other projects, including the Level 3 PRA.

So although we are learning things from the Sequoyah analysis, it is not yet fully integrated into the Level 3, but I can assure you that we will make sure that there is a smooth transition because these are bodies of knowledge that are really used as references by many other countries and by many other people. I think we tried to do a search, and we came up with 270 other references that talk about SOARCA. So the improvements in our tools and our methodologies of our staff technical expertise has been significant by doing these analyses, and it really goes to NRC's ability to more efficiently and effectively carry out its mission to protect the public health and safety and the environment.

Now the next slide I am going to talk to a little bit is our next steps. Our plan is to send an information paper to the Commission that will enclose the final draft Sequoyah SOARCA analysis, and we will note the ongoing work that we have with the Surry UA as well as insights from these analyses that support the Level 3 PRA. It also discussed the need to maintain the severe accident models, continue to complete V&V

efforts, identify some current work requests that we have and relate how the models and analyses are supporting this, and that we need to maintain the staff expertise.

In the past, the Commission has identified the agency's severe accident research program as an essential element of the decision-making process for severe accident issues related to existing and future nuclear reactors, including certification of new standard plant designs, so we do need to consider what analysis may be needed for the new and advanced reactor designs in light of questions we are getting even now.

There continues to be uncertainties in our current understanding of severe accident phenomena. The agency relies on these computer codes developed from the severe accident research program to consider these uncertainties and estimate the margins. Results obtained from these analyses provide the agency essential input for regulatory decisions, and more research can reduce these uncertainties and assess the importance of new phenomena, not only in accident progression, but in cost-benefit analysis, environmental reviews, emergency planning, and health risk effects.

NRC needs to also continue to be aware of analyses and approaches taken by other countries and other organizations and identify areas where further code enhancement may be warranted. We have done some of this. However, it needs to be developed further to maintain a minimum state of practice. Stakeholders more and more are raising good questions on the capabilities of our codes.

We dearly appreciate the subcommittee's review of this report. It was a 600-page report that -- the technical details of which probably many others outside of this room haven't read yet, and hopefully they will in the future. But we just thank you for all your questions. We think we have done a good job. We question ourselves, but when we come here, we certainly do get additional questions that help us make these reports better, and so I thank you for that. And I want to commend the staff for the amount of time and effort and work that they have put into these analyses.

So with that, I think we have closed for today. The expectation is we would like to make a final presentation at the ACRS Full Committee in September, and then as I said, we will update the Surry UA as we go on further.

MEMBER BLEY: Pat?

MS. SANTIAGO: Yes.

MEMBER BLEY: The updating of Surry --

MS. SANTIAGO: Yes.

MEMBER BLEY: -- will that be bringing it into technical agreement with the things you accomplished here, or is there something additional?

MS. SANTIAGO: Primarily -- oh, go ahead.

DR. GHOSH: Yes. You may remember that the safety valve behavior had a profound impact on whether you get to induce steam generator tube rupture, which we don't cover in the Sequoyah analysis. So we -- we wanted to make that update, the PRT model update that you heard about. So we're basically wrapping in all the things we have updated as part of the Sequoyah project also into Surry.

CHAIRMAN STETKAR: Tina, when you do that, because I am a valve person, as you might have recognized --

MS. SANTIAGO: I am sorry. I can't hear you.

CHAIRMAN STETKAR: I am a valve person. I -- I sort of recall, but I am not sure, that in the Surry model -- because here in the Sequoyah model, your incentive is to try to get stuff out into the containment. In the Surry model, because it looks at consequential tube rupture, valves not sticking open or valves failing to open can be important because they keep pressure high.

I don't remember in the Surry model whether you have -- I think you have the fail to open failure mode in there --

DR. GHOSH: We do, yes.

CHAIRMAN STETKAR: -- and the same concerns apply to that in terms of events and frequency, and -- and in particular, in that case, treating common cause failures of the valves. So just keep that in mind as you go forward because there is that other piece of the puzzle in terms of valve behavior that you have not investigated at all --

DR. GHOSH: Yes.

CHAIRMAN STETKAR: -- for the Sequoyah work --

DR. GHOSH: Right.

CHAIRMAN STETKAR: -- which is, not only might they not stick open, they might not open at all. And -- and there are -- there's indeed some data to support that failure mode.

DR. GHOSH: Okay. Yes, thanks. We will go take a look.

CHAIRMAN STETKAR: I wanted to ask kind of a process question here. We have had a daylong discussion about what's in the current draft of the report, and according to what we have on the -- the screen there, you plan to send the I will call it a final draft of the report to the Commission at the end of August try?

MS. SANTIAGO: That is our current --

CHAIRMAN STETKAR: Okay.

MS. SANTIAGO: -- plan.

CHAIRMAN STETKAR: And --

MS. SANTIAGO: And --

CHAIRMAN STETKAR: -- that final draft of the report will be different from the draft of the report that we discussed today. You are expecting a Full Committee meeting with I am assuming the expectation of an ACRS letter on this in our September meeting.

This is a big report. This is 584 pages. Thank you. It's a lot of information for the Full Committee to digest. It's especially a lot of information if there are going to be substantive changes to any of the analyses, and that is my concern here. I am a bit concerned about a 584-page report being sent to the ACRS in the second week of August, which we would need to support our September Full Committee meeting, 30 days, that folks who have spent a -- probably too much of their lives sitting around this table reading this incarnation of the report are then surprised and trying to learn a lot more technical information such that we can be appropriately educated for the Full Committee meeting.

I don't know how to -- how to tackle that problem. I -- I don't know whether we ought to have another subcommittee meeting. That brings into it a lot of logistics that we don't need to discuss here on the record. I -- I just don't know. I'd feel -- I personally would feel a lot more comfortable having a subcommittee meeting where the subcommittee was at least familiarized with any changes in that final draft report compared to the current draft report before we just come to the Full Committee, where the --

MS. SANTIAGO: I guess I didn't expect substantive changes. Rather, I expected that we would be adding some additional clarification and things like that. And I think Trey -- and there is a placeholder in this report -- but I think that Trey presented that information today, right?

CHAIRMAN STETKAR: Well, there's the whole thing about the mini uncertainty analysis study, and --

MS. SANTIAGO: Yes.

CHAIRMAN STETKAR: -- I have no idea how you're going to deal with some of the questions that came up today. It's something -- you can't answer it --

MS. SANTIAGO: Right.

CHAIRMAN STETKAR: -- today.

MS. SANTIAGO: Yes.

CHAIRMAN STETKAR: I am just --

MS. SANTIAGO: Yes.

CHAIRMAN STETKAR: -- sort of raising that bit of a concern, that if there's nothing -- if there are no -- I don't know how to quantify substantive -- if there are no substantive changes, then okay. If there are, then I think we would -- there would be mutual benefit of having some discussion about them, not necessarily a full-day meeting, because it's just too much material to present to the Full Committee for the Full Committee to understand everything that's in there and in that context also discuss substantive differences in something that the -- the subcommittee has seen. You don't want a letter necessarily saying hey, we --

MS. SANTIAGO: Yes, no --

CHAIRMAN STETKAR: -- we were --

MS. SANTIAGO: -- I don't.

CHAIRMAN STETKAR: -- surprised --

(Laughter.)

CHAIRMAN STETKAR: -- by this change --

MS. SANTIAGO: I want the letter --

CHAIRMAN STETKAR: -- still don't -- still don't --

MS. SANTIAGO: -- solid analysis --

CHAIRMAN STETKAR: -- agree with the way that you described how, you know, something was functioning. So we can discuss that offline. It -- we do have problems in terms of scheduling subcommittee meetings also, but we will deal with that. I think there are some --

MEMBER BLEY: Some spots have opened up, so if we --

CHAIRMAN STETKAR: Oh, are they?

MEMBER BLEY: -- needed to --

CHAIRMAN STETKAR: Okay. Thank you, sir, may I have another?

(Laughter.)

CHAIRMAN STETKAR: Anyway, let's talk about that offline and -- and, you know, after you have -- have had a chance to caucus and discuss sort of what you want to do internally.

With that, let me do the things that I need to do. If there's anyone in the room here who would like to make a comment, please come up to the microphone and do so. And because this is a public meeting, if there are any members of the public on the bridge line, please speak up, identify yourself if you would like to make a comment, and do so.

(No audible response.)

CHAIRMAN STETKAR: All right. Now Theron, is the bridge line open? That's another indicator.

MR. BROWN: It's open.

CHAIRMAN STETKAR: Okay. Thanks. So hearing no comments, again, if there's anyone from the public who would like to make a comment, just identify yourself and please do so.

(No audible response.)

CHAIRMAN STETKAR: Hearing nothing, we will close the public comment period. And as we always do at subcommittee meetings, I will go around the table and see if any of the members have final comments that you would like to make. And I will start with Harold. Harold?

MEMBER RAY: I don't have any comments to make. This is educational for me, and I appreciate the work that has been done and the opportunity to get more informed.

CHAIRMAN STETKAR: Okay. Thank you. Dick?

MEMBER SKILLMAN: No further comment. Thank you.

CHAIRMAN STETKAR: Dr. Corradini, sir? Professor? Esteemed?

MEMBER CORRADINI: Thanks to the staff. I appreciate it.

I guess if this is going to come in September as planned, and you're only going to have a couple hours, I think the staff ought to focus on -- on early hydrogen behavior and those uncertainty parameters that affect that. And that would be enough for I think the Full Committee to try to appreciate, because all the rest of it after that kind of leads from that, as I see it. And to the extent that -- to the extent that you can do that, I think that would be beneficial. Particularly, that would bring up the questions that Chairman Stetkar has so ably done in terms of valves and their frequencies and their areas, et cetera, et cetera. But I think if you just focus on the hydrogen-related topics, which makes Sequoyah unique anyway, that would -- that is probably doable.

CHAIRMAN STETKAR: Thank you. Dennis?

MEMBER BLEY: No additional comments. I appreciate some of the improvements that we saw since last time around in the logic of how things are laid out. So nothing additional.

CHAIRMAN STETKAR: Jose?

MEMBER MARCH-LEUBA: No comment.

CHAIRMAN STETKAR: Joy?

MEMBER REMPE: Okay. I also second what Dennis said, but I thought there were a lot of comments a year ago about why certain sequences were focused upon, and I did think you did clean that up in the report a bit, and I appreciated that.

I still am very curious about using the 2500 K eutectic temperature and not having it correlated with radiation effects, because it does seem to be important, and if you did consider that, even though the data are sketchy, you might have a different result for beginning of cycle. And so I just would be interested in how that plays out, if there is something that could be done to satisfy my curiosity.

On the -- I -- I second what Pat said about the overall benefit of this analysis and this -- the insights gained and how important that was, and I saw that last paragraph in the summary -- executive summary, and I thought that was a nice paragraph about the benefits of the analyses. And I guess I should have asked, but maybe I will try to put it as a comment: after the initial SOARCA stuff, there was a high-level brochure that was issued about the benefits of it. And maybe you do it after updating the Surry uncertainty analysis, but another -- you commented today this is a very technically dense report, and so another brochure might be something the agency might consider doing?

MS. SANTIAGO: We actually did start to update the brochure. In fact, we added a separate chapter on --

MEMBER REMPE: Oh, good.

MS. SANTIAGO: -- uncertainty analysis. Part of our work that we have to finish is trying to describe the differences between the Surry and the Peach Bottom analyses and how the Sequoyah one differed, but we are updating that brochure.

MEMBER REMPE: That is good to know. So I should have asked you earlier, but thank you for responding. And so again, I appreciated all the presentations, and I would really think a half-day meeting to discuss the results before it goes to Full Committee would be very beneficial. And that is it. Thanks.

CHAIRMAN STETKAR: Thank you. Again, thanks a lot. You have covered a heroic amount of material today.

Personal comment, I think that the -- the report is much, much better than it was. I think that there is a lot more thought in some of the explanations, especially Chapter 3. Chapter 4 stuff is organized quite well, I thought.

As you -- as you think about drafting the final version of the report, I will again emphasize something I mentioned earlier, that in cases where you have made known simplifications or where the results might be affected by assumptions -- and again, because of my focus on the valve data -- be careful to -- to aggressively note that, because this is -- this is cast as an uncertainty analysis, and that is a different source of uncertainty. So in terms of having confidence and saying well, we are -- we are 90 percent confident that the risk is between x and y because we have done all of these 600 realizations with 573 successes, or whatever they are, make sure that you have another section that addresses stuff that is still out there, if you will.

And that is part of that -- you know, sources of that data. It's part of the mini study on the -- whatever you want to call it, the uncertainty analysis. Several of these issues: if you're not going to rerun the things because of the barrier seal stuff, you know, all of that sort of thing.

With that, again, thanks for just a really good presentation. I thought discussions were really good. And, you know, miraculously, we didn't make it until 6 o'clock. I am trying desperately here to do that, but we are adjourned.

(Whereupon, the meeting went off the record at 5:18 p.m.)

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State-of-the-Art Reactor Consequence Analyses (SOARCA) Project: Sequoyah Integrated Deterministic and Uncertainty Analyses

ACRS Subcommittee Briefing
June 6, 2017

Patricia A Santiago, Chief
Accident Analysis Branch
Division of Systems Analysis
NRC Office of Nuclear Regulatory Research

Core Team Members

Today's speakers in bold

- MELCOR and severe accident progression: Kyle Ross, Jeff Cardoni, Chris Faucett, **Randy Gauntt** (SNL); **Casey Wagner** (dycoda); **Hossein Esmaili**, 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*

Outline

- Overview - Background and Approach
- Accident Progression Analysis (MELCOR)
  - Model Updates
  - Updated Analysis
- Offsite Consequence Analysis (MACCS)
  - Model Updates
  - Updated Analysis
- Conclusions
- Uses of SOARCA Modeling
- Next Steps and Schedule

Background

- SECY-12-0092: SOARCA - Recommendation for Limited Additional Analysis
  - Surry UA for a severe accident scenario
  - Station blackout (SBO) consequence analysis of an ice condenser containment
- SRM: Analyses should complement and support:
  - Level 3 PRA project to develop a risk profile for multi-unit sites and spent fuel storage facilities as well as external events
  - Fukushima Near Term Task Force (NTTF) activities 5.2 and 6
    - → Sequoyah SOARCA analyses supported closure of these items in SECY-15-0137 and SECY-16-0041
- Briefed ACRS Subcommittee in May 2016

Sequoyah SOARCA Approach

- Focus on issues unique to ice condenser containment
- Use latest version of codes
  - MELCOR version 2.2
  - MACCS version 3.10
- Consider latest plant- and site-specific information available including:
  - Core inventory
  - Population
  - Emergency response
- Integrate consideration of uncertainty into accident progression and consequence analysis

Sequoyah Accident Scenarios

- Seismically initiated SBO
  - Earthquakes greater than 0.5 g PGA
- Two primary variations of unmitigated SBO
  - Short-term SBO is the focus of uncertainty analysis: loss of all AC power and turbine-driven auxiliary feedwater pump (TDAFW) not available
  - Long-term SBO: loss of all AC power and TDAFW initially available but fails after batteries deplete
- STSBO contribution to CDF ~ 2E-6/yr
- LTSBO contribution to CDF ~ 1E-5/yr

Severe Accident Progression

Presentation Topics

- STSBO High Level General Observations
- MELCOR Model and UA Input Updates
- STSBO Observations and Conclusions
- STSBO Insights from Individual UA Realizations
- STSBO Uncertainty Regression Analysis
- STSBO Model Errors and Impact of Igniters
- LTSBO Analysis with Sensitivities on TDAFW

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Severe Accident Progression STSBO High Level General Observations

- Consequences strongly (and intuitively) affected by *early vs. late* containment failure. Early containment failure dominated by hydrogen combustion, and late containment failure results mainly from ex-vessel phenomena (e.g., CCI)
- Early containment failures occur *only on the first hydrogen burn* (subsequent burns do not challenge containment integrity)
- Protracted SV cycling produces *lower in-vessel hydrogen* by the time of first burn
- PZR SV failure to close (with large open area) results in greater hydrogen production and transport to the containment prior to the first burn, which increases the potential for early containment failure
- Late containment failures generally have reduced source term release benefiting from gravitational settling

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MELCOR Model and Uncertainty Analysis (UA) Input Updates

Major Model Updates

DRAFT UA vs Current UA

- Pressurizer relief tank (PRT)
  - Heat transfer to the water pool on the outside of PRT
  - Modeling of fission product distribution in the PRT atmosphere and pool, and deposition on the walls
- Modeling of hydrogen ignition in the lower containment as a result of flow of hot gases from PRT (in addition to hot leg and lower head failure)
  - No random sources of ignition
- Modeling of TD-AFW performance using the new homologous pump model
- MSIVs now include leakage

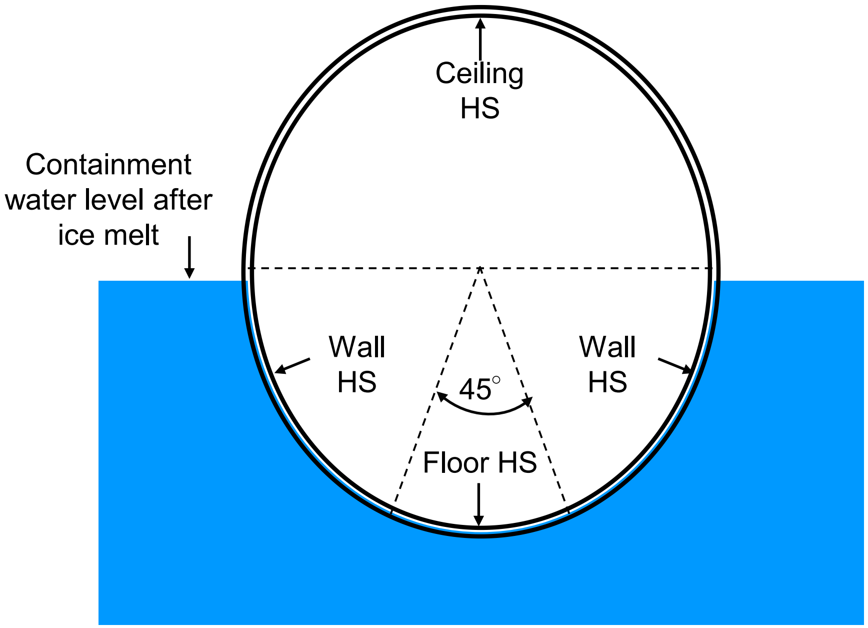
Improved PRT Modeling

Summary

- Draft UA showed high importance of PRT for late revaporization of captured fission products, especially after containment failure. Also showed non-physically high structure and gas temperatures.
- PRT in the Draft UA did not model heat loss.

Changes

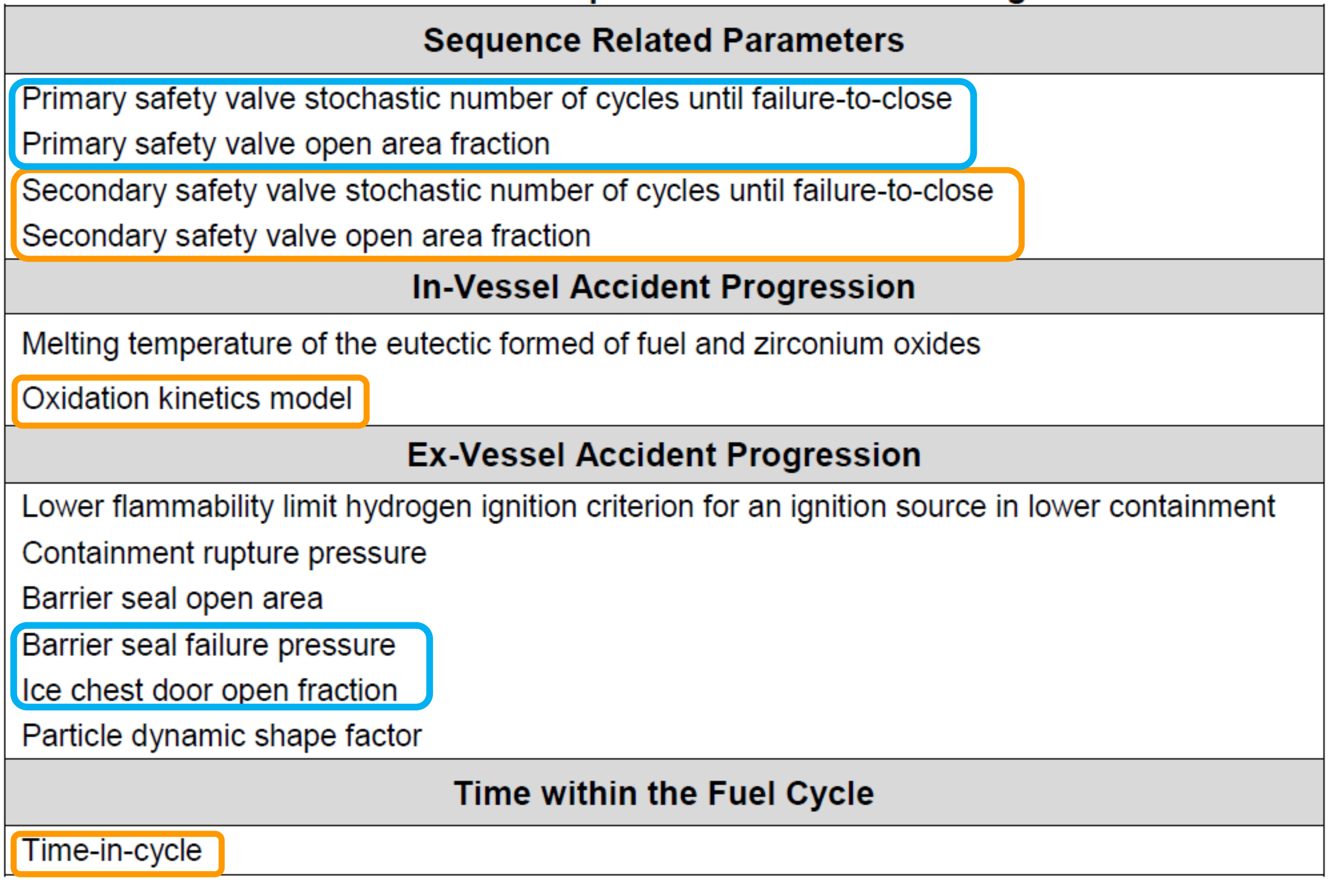
- Added heat loss to containment & ice melt water
- Three heat structures to represent floor, side wall, and ceiling
- Gravitational settling is concentrated on 45° segment
- MELCOR continuously evaluates heat transfer and pool levels inside and outside of the PRT
- All radionuclide decay heat energy retained in the PRT
  - Sensitivity studies evaluated gamma deposition



MELCOR Model Parameters (STSBO)

Figures of merit studied include cesium/iodine release magnitude, in-vessel hydrogen generation, containment failure time, and time of initial release

Table ES-1 Uncertain MELCOR parameters used in unmitigated STSBO UA

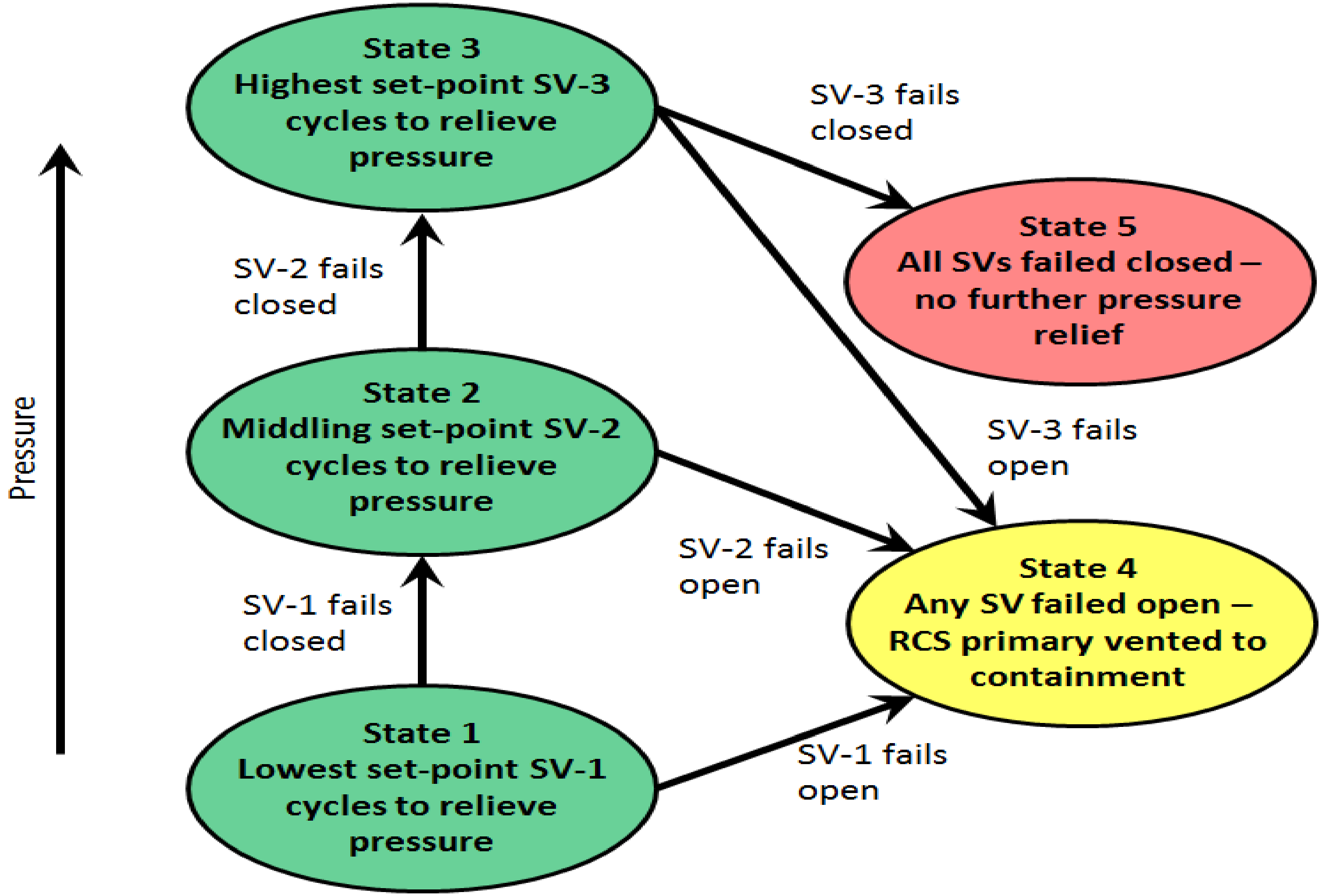


Sequence Related Parameters
Primary safety valve stochastic number of cycles until failure-to-close
Primary safety valve open area fraction
Secondary safety valve stochastic number of cycles until failure-to-close
Secondary safety valve open area fraction
In-Vessel Accident Progression
Melting temperature of the eutectic formed of fuel and zirconium oxides
Oxidation kinetics model
Ex-Vessel Accident Progression
Lower flammability limit hydrogen ignition criterion for an ignition source in lower containment
Containment rupture pressure
Barrier seal open area
Barrier seal failure pressure
Ice chest door open fraction
Particle dynamic shape factor
Time within the Fuel Cycle
Time-in-cycle

Orange indicates additional parameters considered in current UA

Blue indicated updated parameters considered in the current UA

Primary Safety Valve System



Safety Valve Failure-to-Close (FTC) Uncertainty Modeling Updates

Draft UA **- SV failure data and associated epistemic uncertainty distributions for FTO (failure-to-open) and FTC on demand**

Mode	**# Failures**	**# Tests**	**Distribution**
FTO	0	773	β(α = 0.5, β = 773.5)
FTC	17	773	β(α = 17.5, β = 756.5)

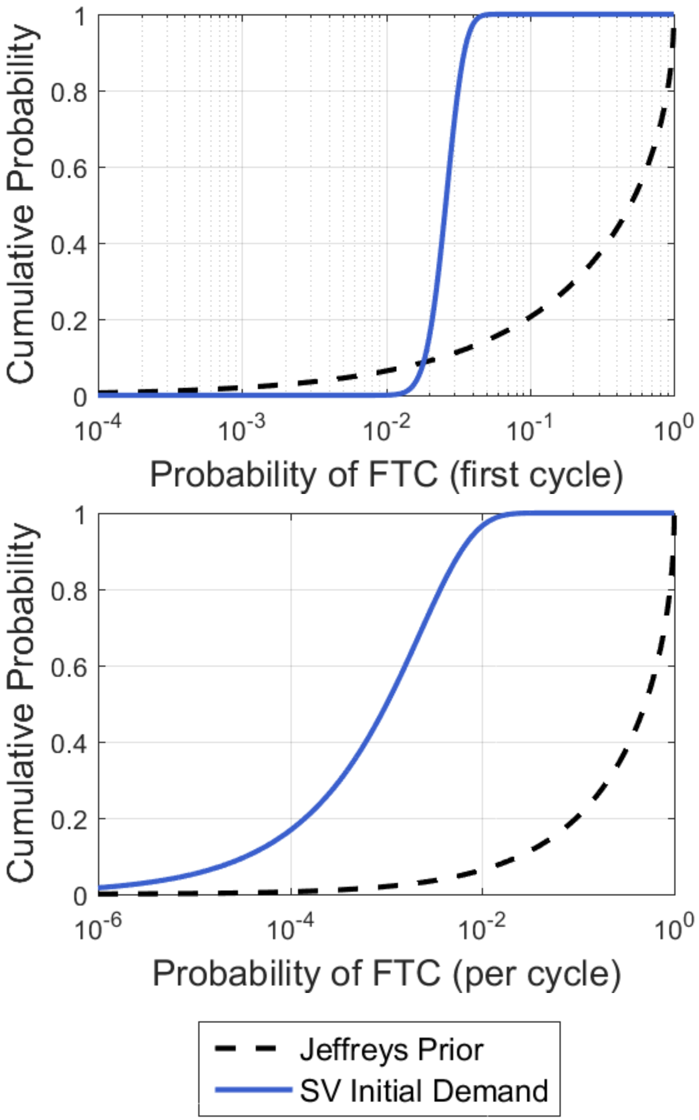
Current UA **– Updated FTC distribution**

Demand	**# Failures**	**# Demands**	**Distribution**
Initial	16	621	β(α = 16.5, β = 605.5)
Subsequent	0	223	β(α = 0.5, β = 223.5)

Pressurizer and SG SV FTC

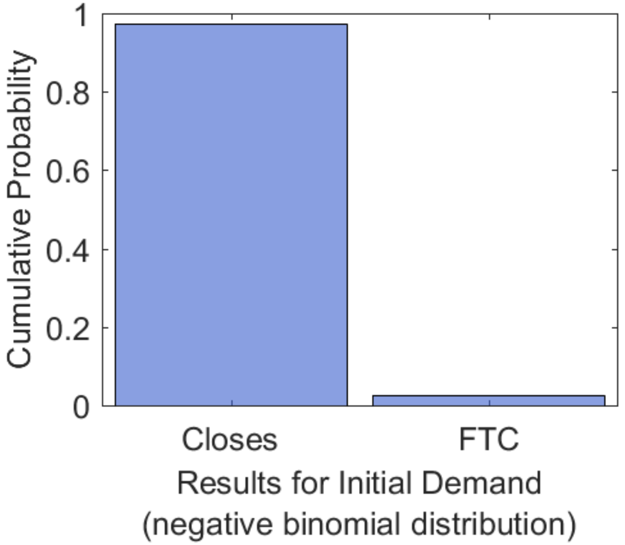
(1) Initial Demand

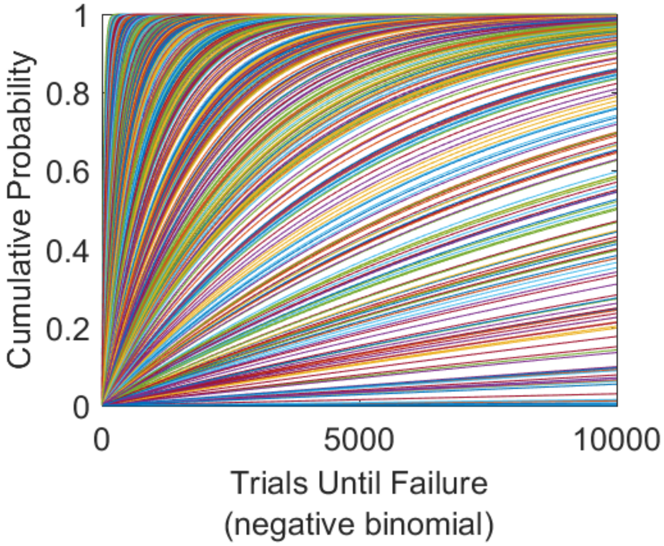
(2) Subsequent Demand



(1)

(2)

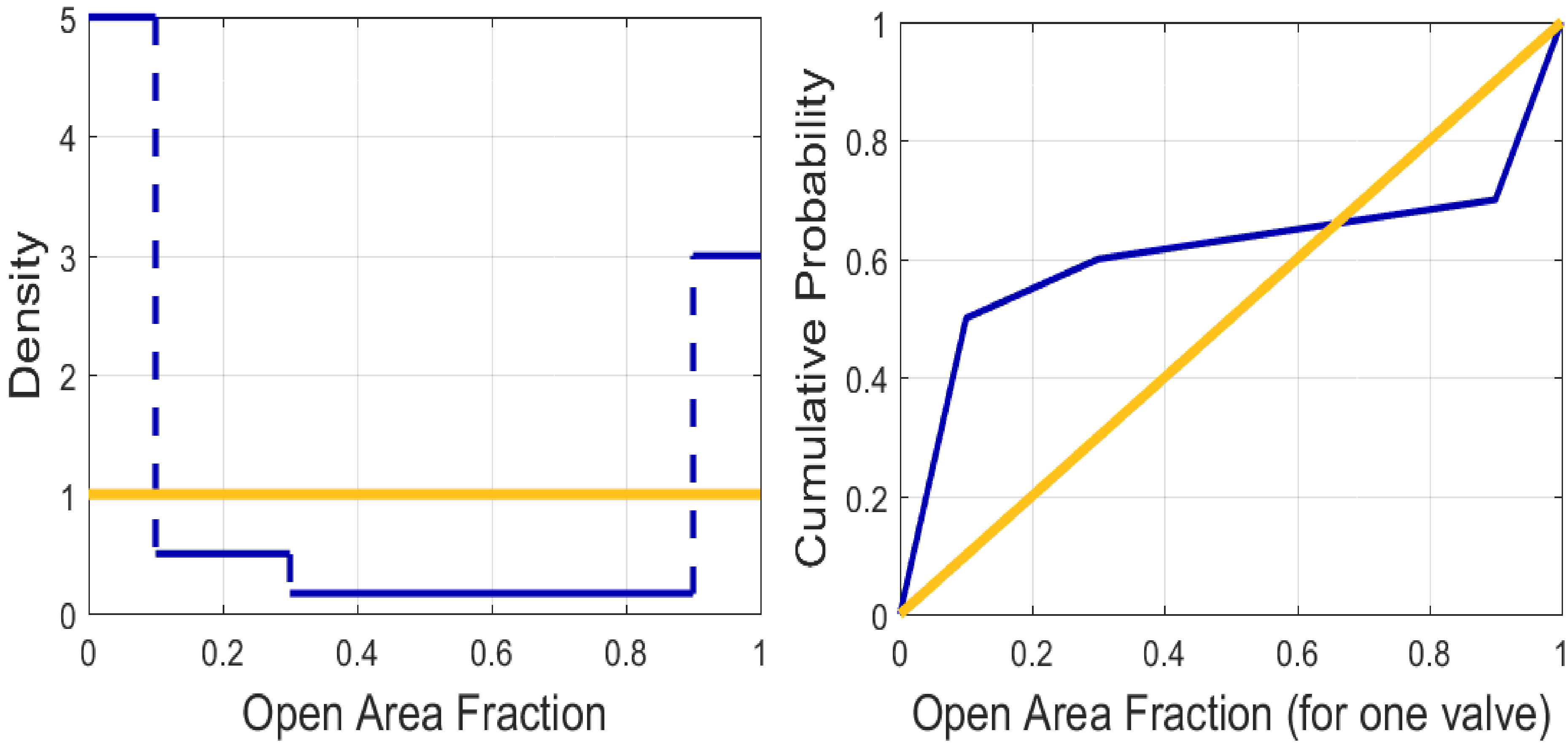




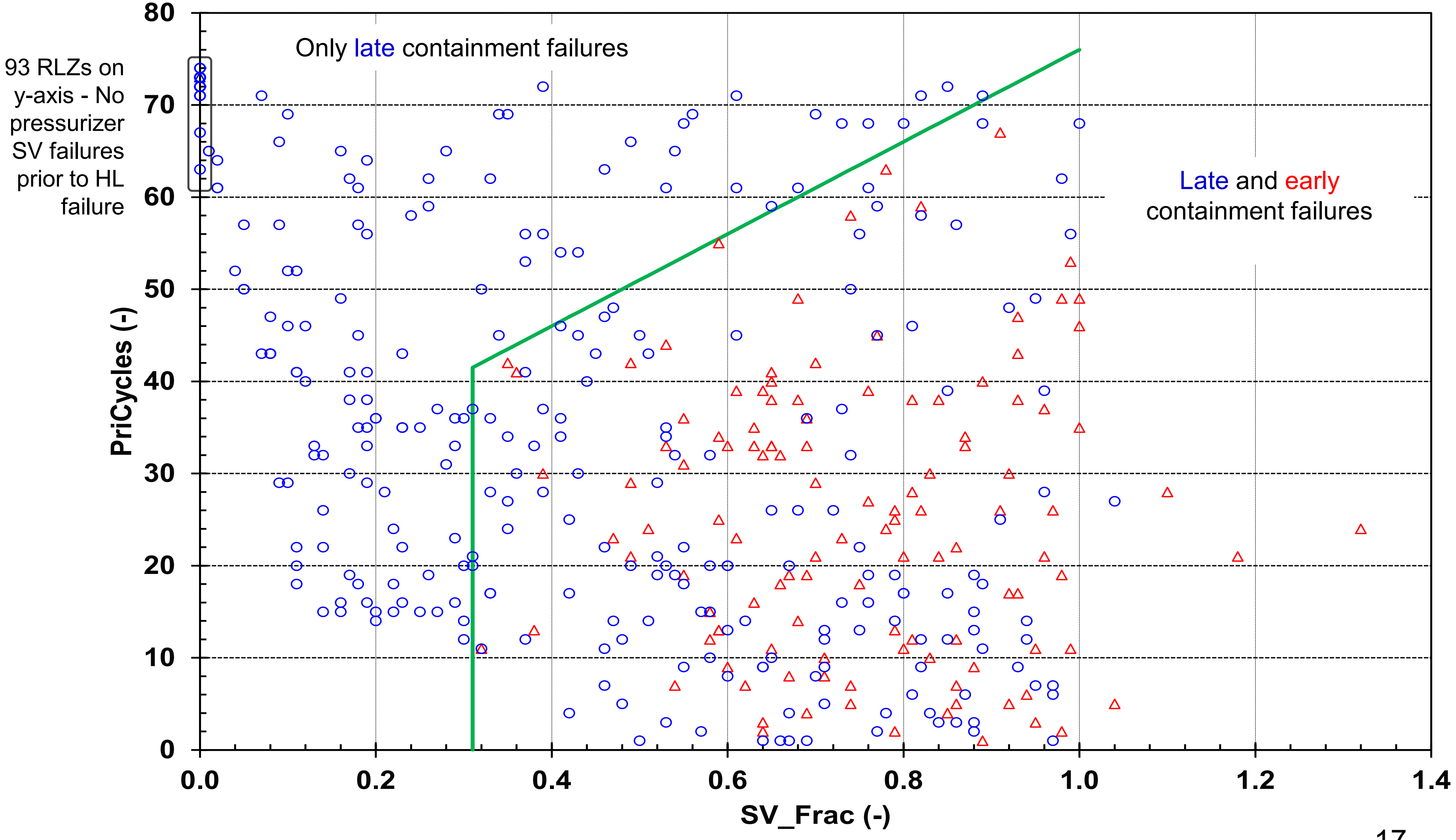
Pressurizer and SG SV FTC Open Area Fraction

Draft UA —

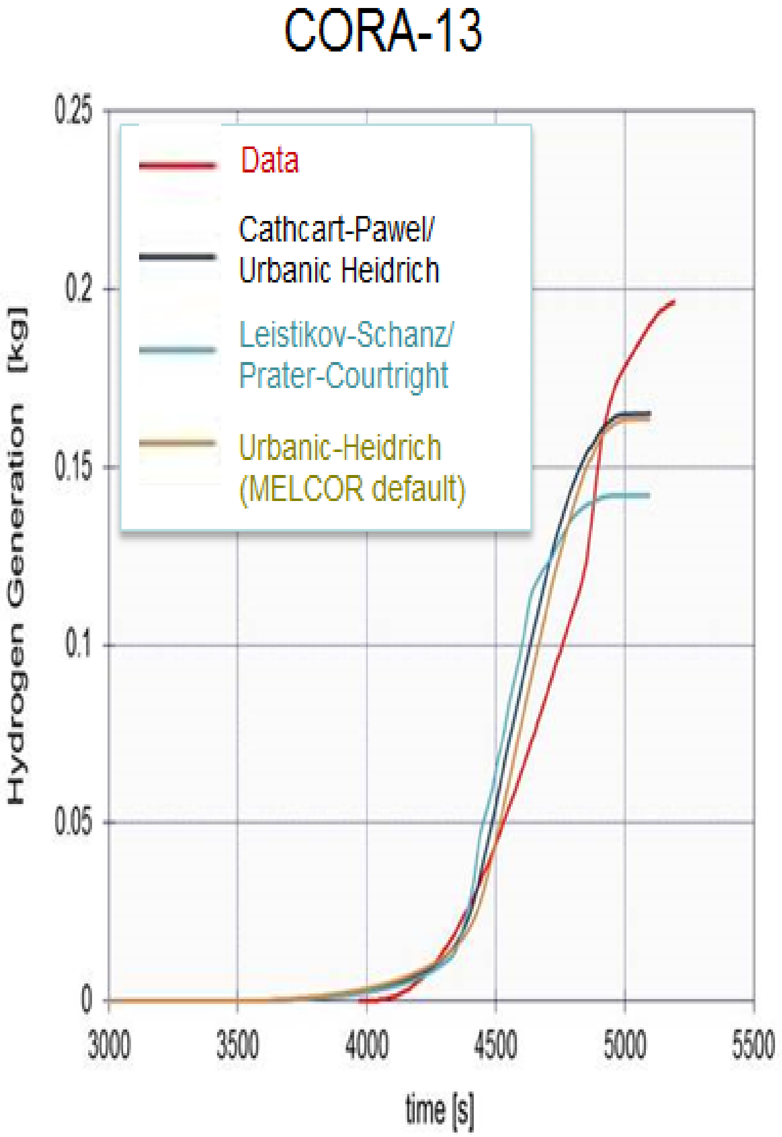
Current UA —

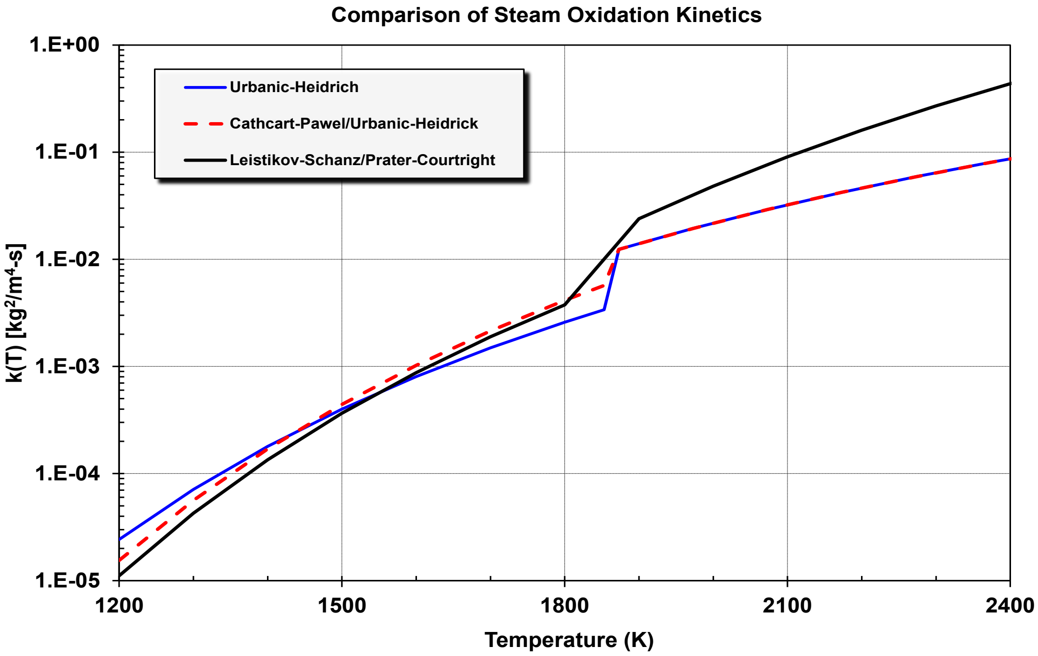


Primary Cycles versus SV Open Fraction Early (Red Δ)/Late (Blue O) Containment Failures in Draft UA



New oxidation kinetics
Uncertainty parameter

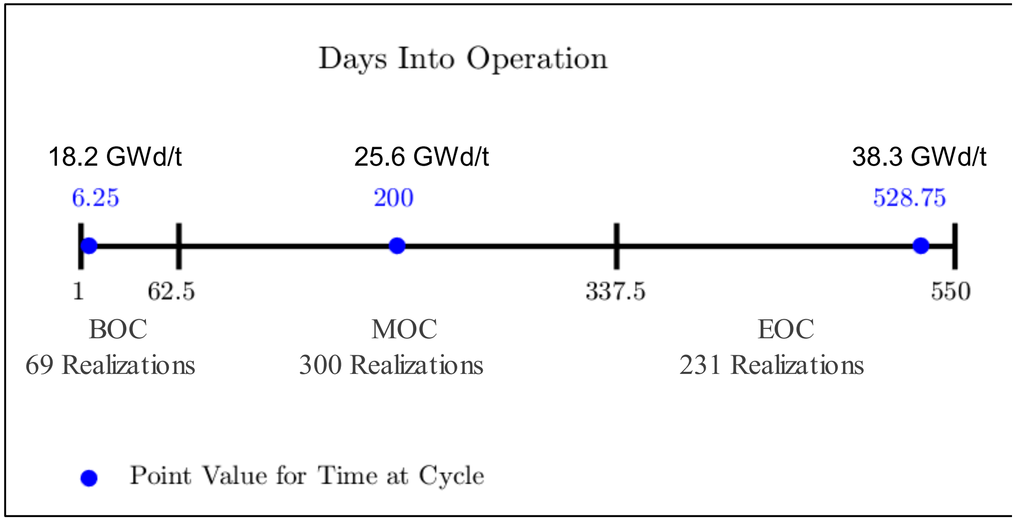




- Urbanic-Heidrick (25%)
  - Used in *DRAFT* UA
- Catchart-Pawel/ Urbanic-Heidrick (25%)
- Leistikov-Schanz/ Prater-Courtright (50%)

Time-In-Cycle

- Included as uncertain to investigate the influence of the degree of fuel burnup at accident onset



Days Into Operation

18.2 GWd/t
25.6 GWd/t
38.3 GWd/t
6.25
200
528.75
1
62.5
337.5
550

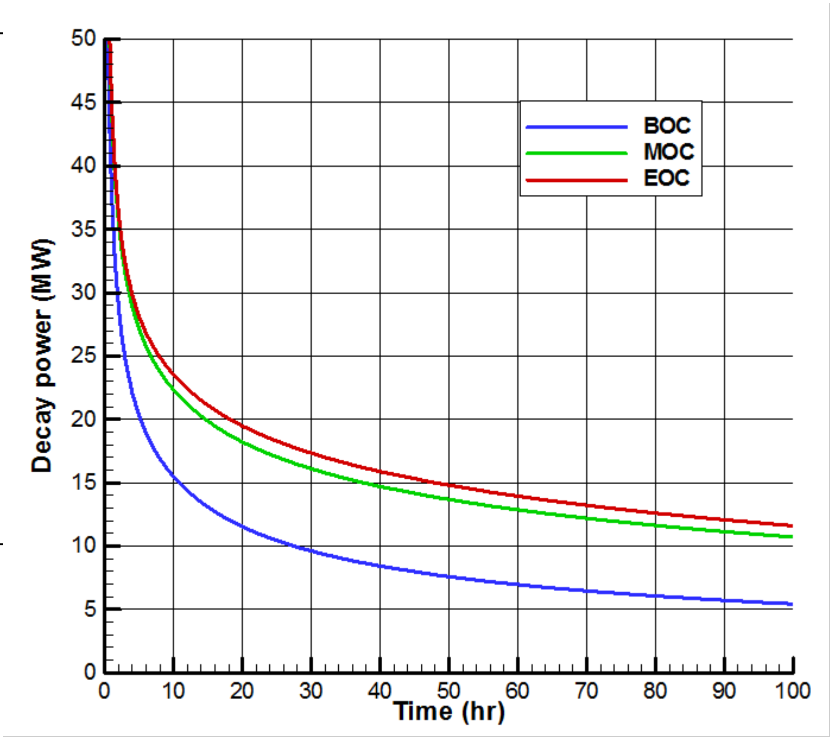
BOC
69 Realizations

MOC
300 Realizations

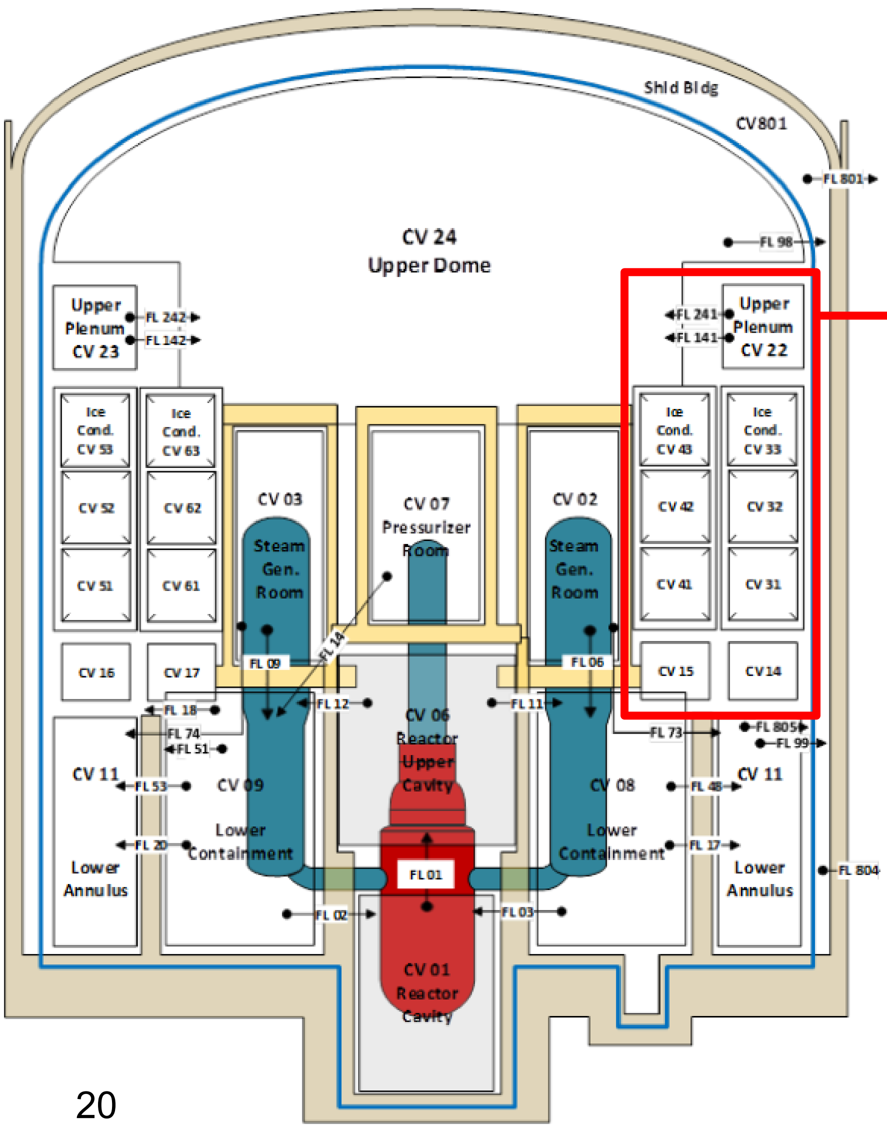
EOC
231 Realizations

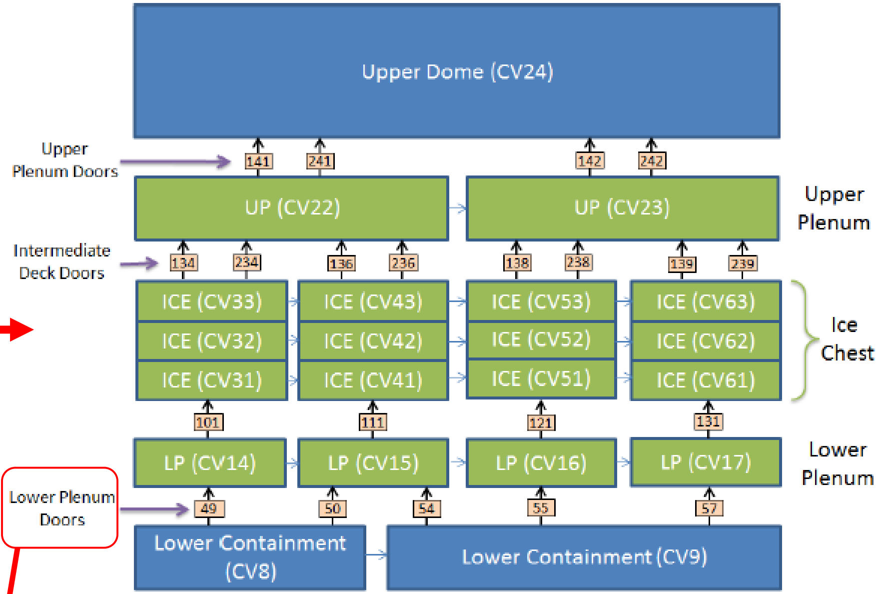
● Point Value for Time at Cycle

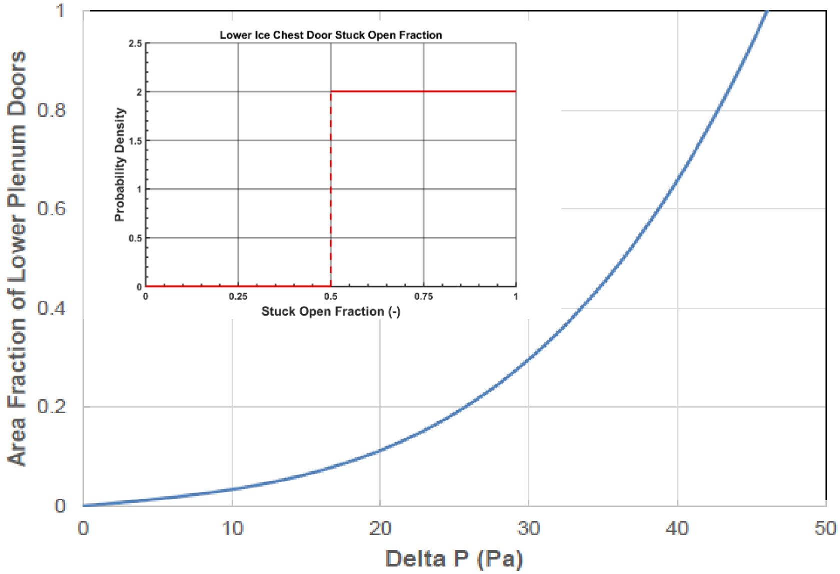
	BOC	MOC	EOC
Total cesium class mass (kg)	160.65	225.29	328.94
Total iodine class mass (kg)	10.17	14.48	21.67



Ice chest inlet plenum doors (ajar)







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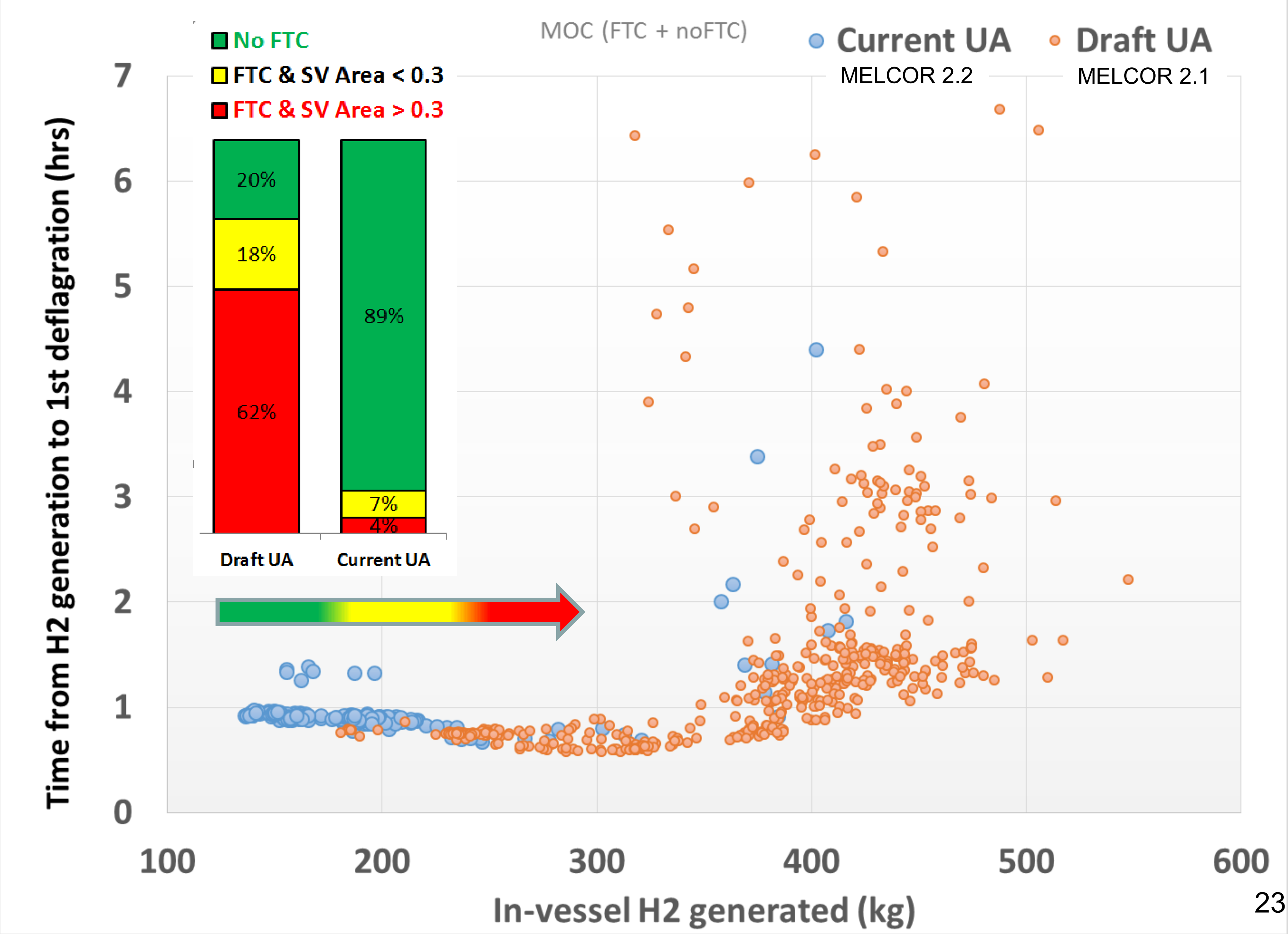
Accident Progression Analysis
STSBO Observations and Conclusions

Code Updates

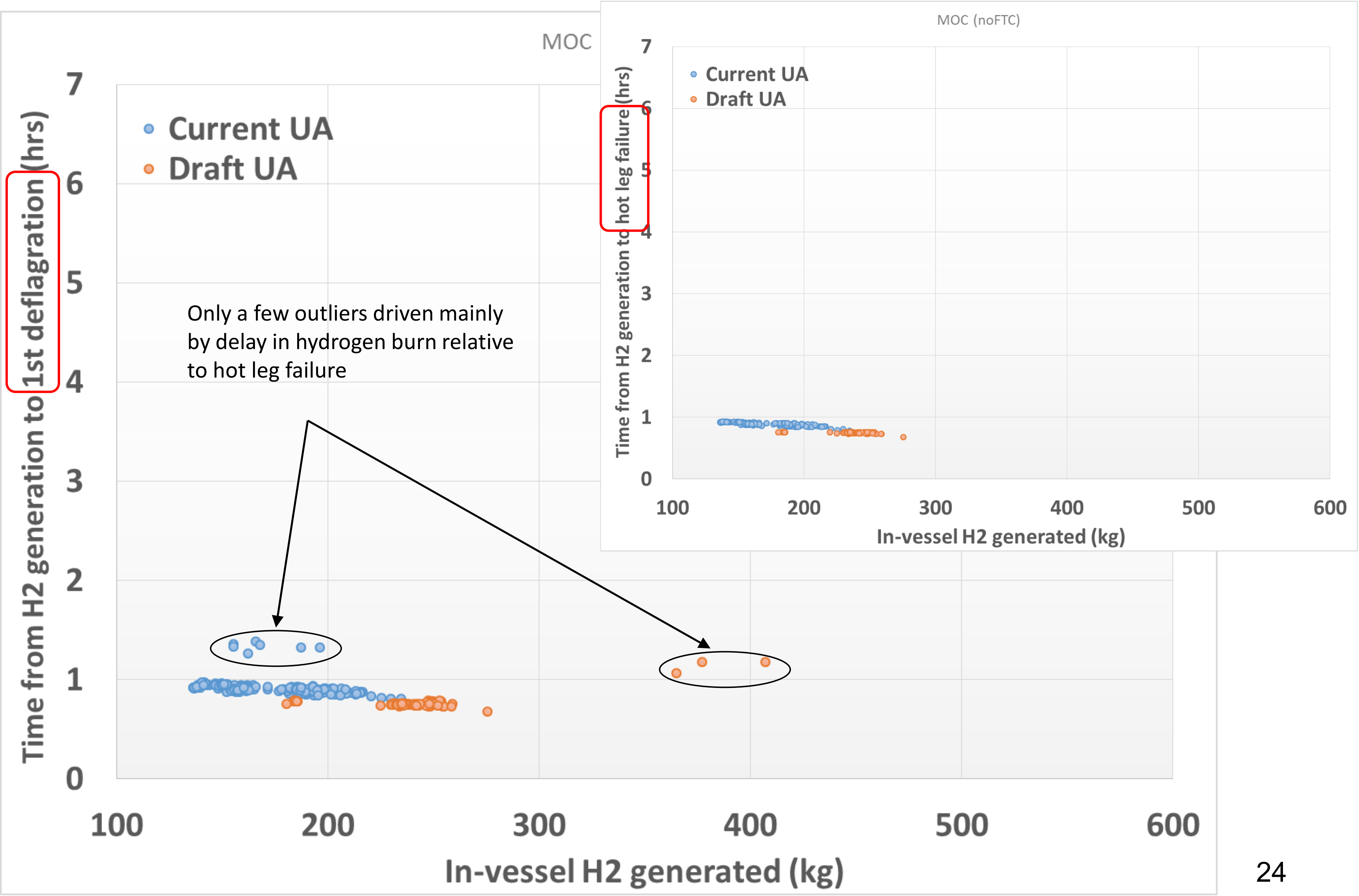
Draft vs. current UA

- Various MELCOR 2.2 code updates including
  - Corrections to the reflood quench model
  - Lipinski dryout model not used above the core support plate
  - Decay heat transfer to small fluid volumes
  - Correction to fuel rod collapse modeling (temperature failure criteria)
  - Ex-vessel debris cooling and spreading models
- Presentation to ACRS T-H Subcommittee on April 18, 2017
  - Changes in early containment failures in current UA (MELCOR 2.2) calculations are mainly due to modifications in the safety valve failing to close
  - Reduction in hydrogen generated in-vessel due to code changes not as important as model input changes

STSBO Overall System Response

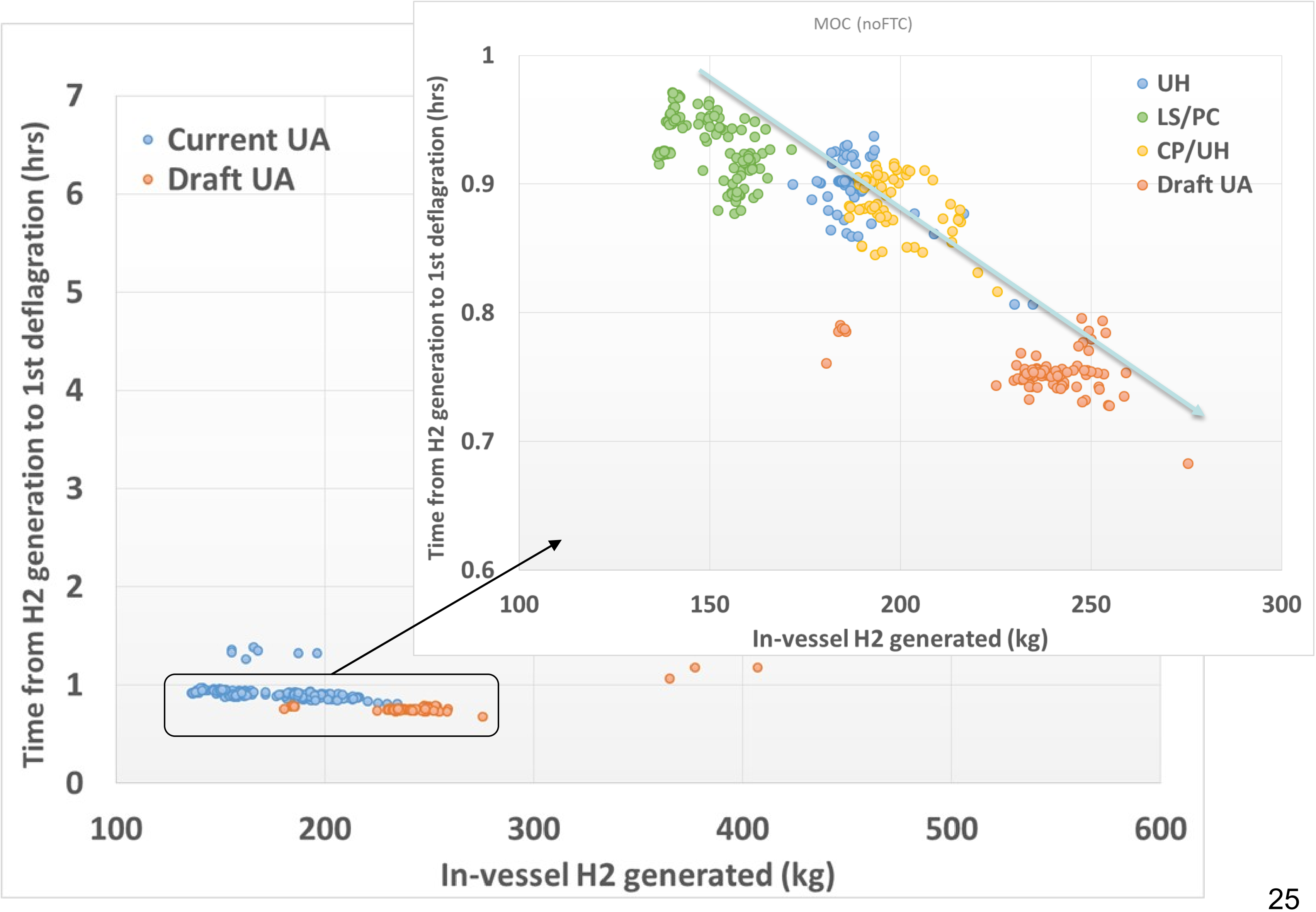


STSBO - Effect of PZR SV cycling

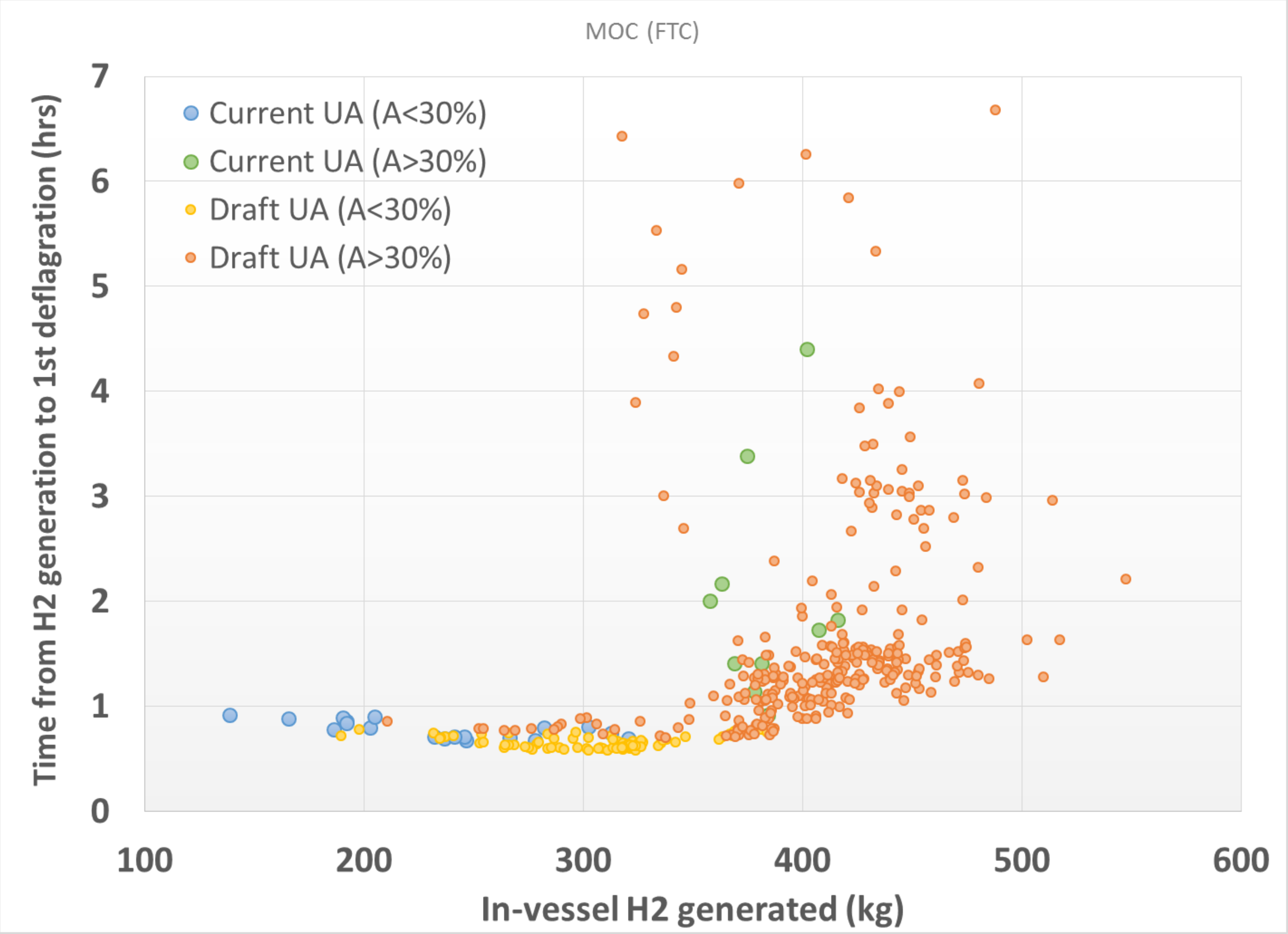


Only a few outliers driven mainly by delay in hydrogen burn relative to hot leg failure

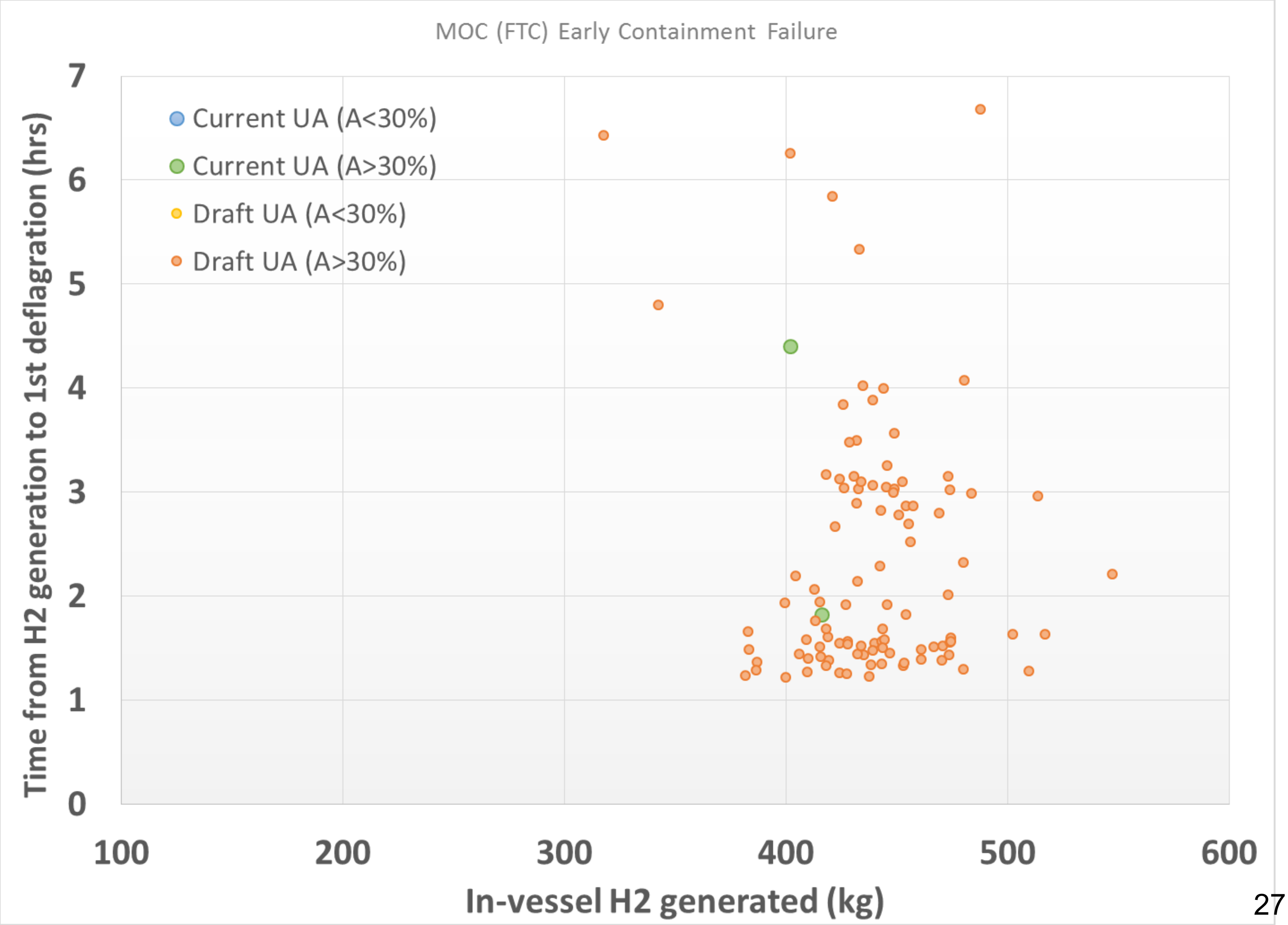
STSBO - Effect of oxidation model



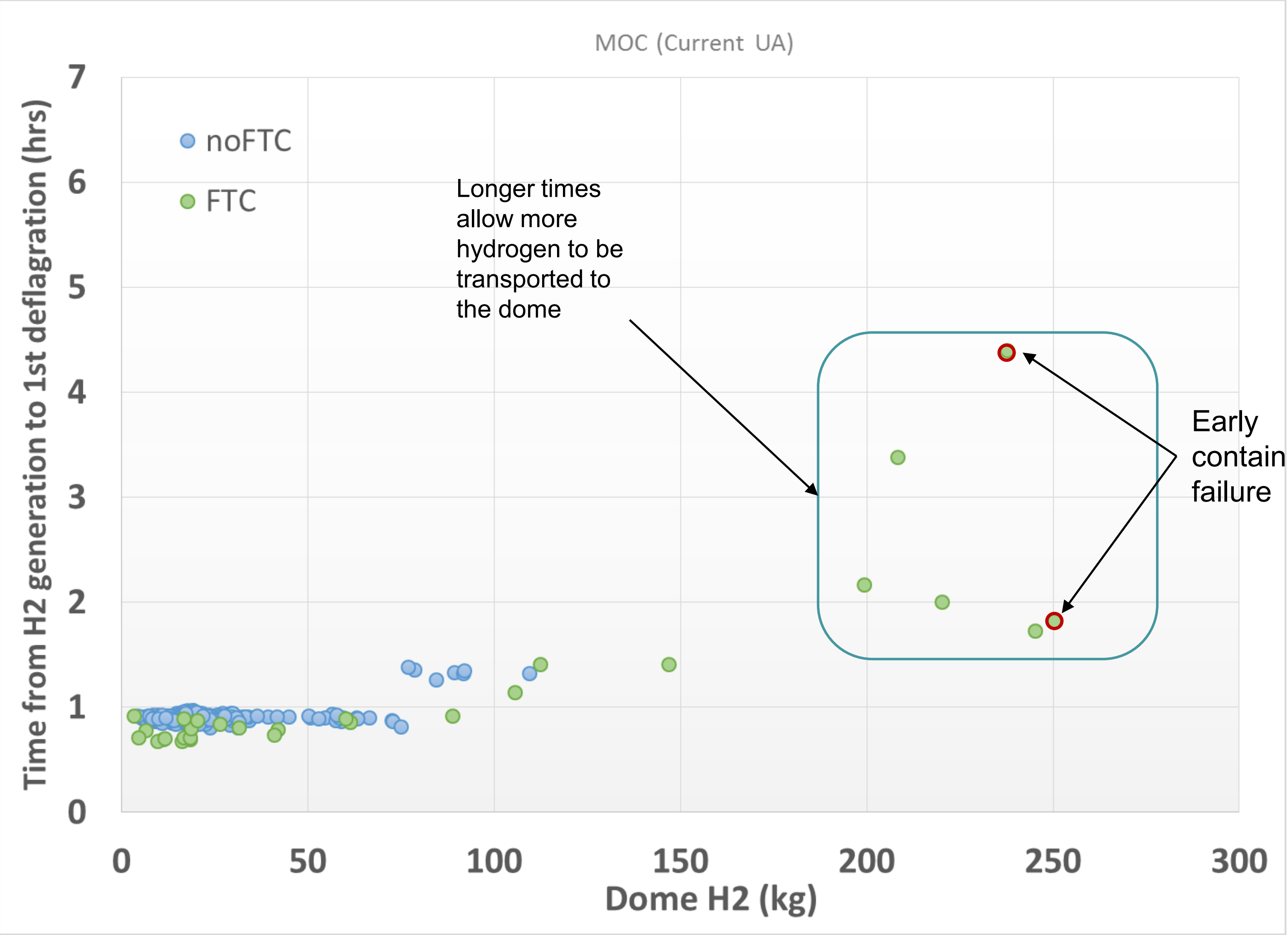
STSBO - Effect of SV area fraction



STSBO – Early Containment Failure Map



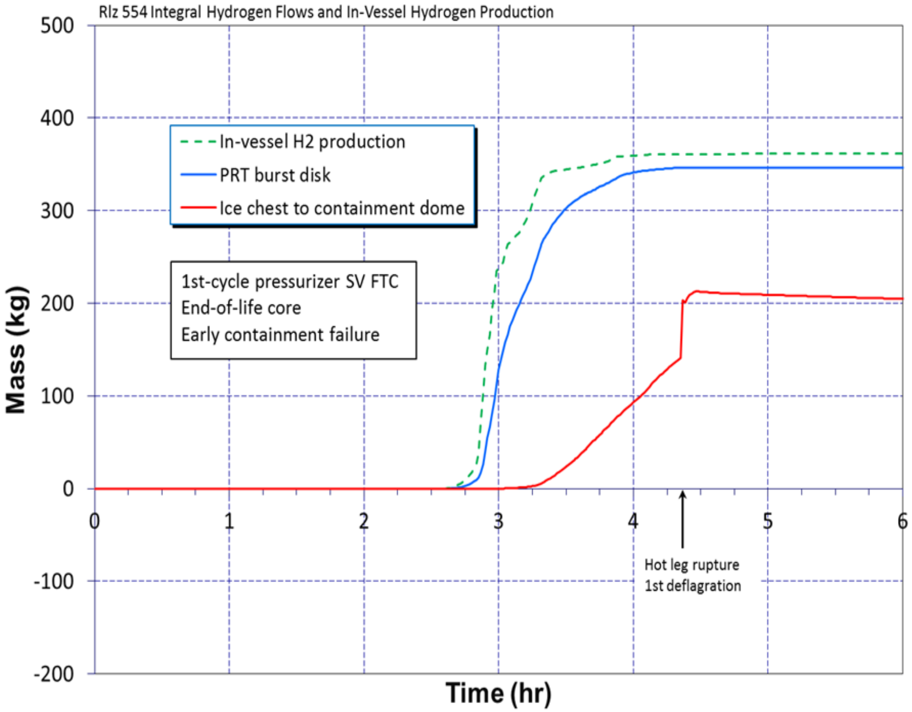
STSBO – Hydrogen Transport

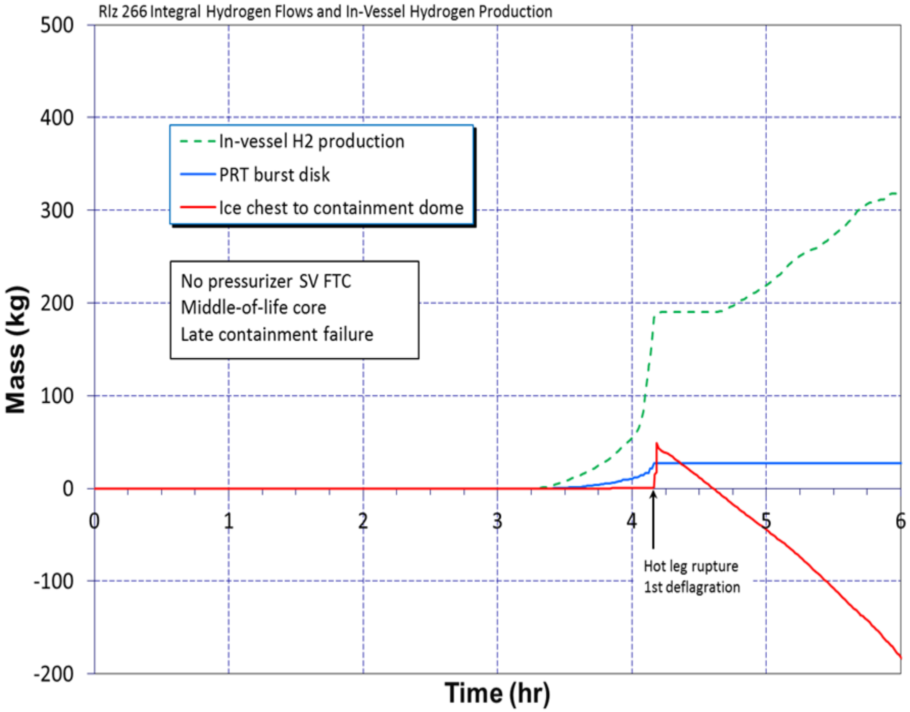


Longer times allow more hydrogen to be transported to the dome

Early containment failure

Fundamental difference between containment failure (early vs late)

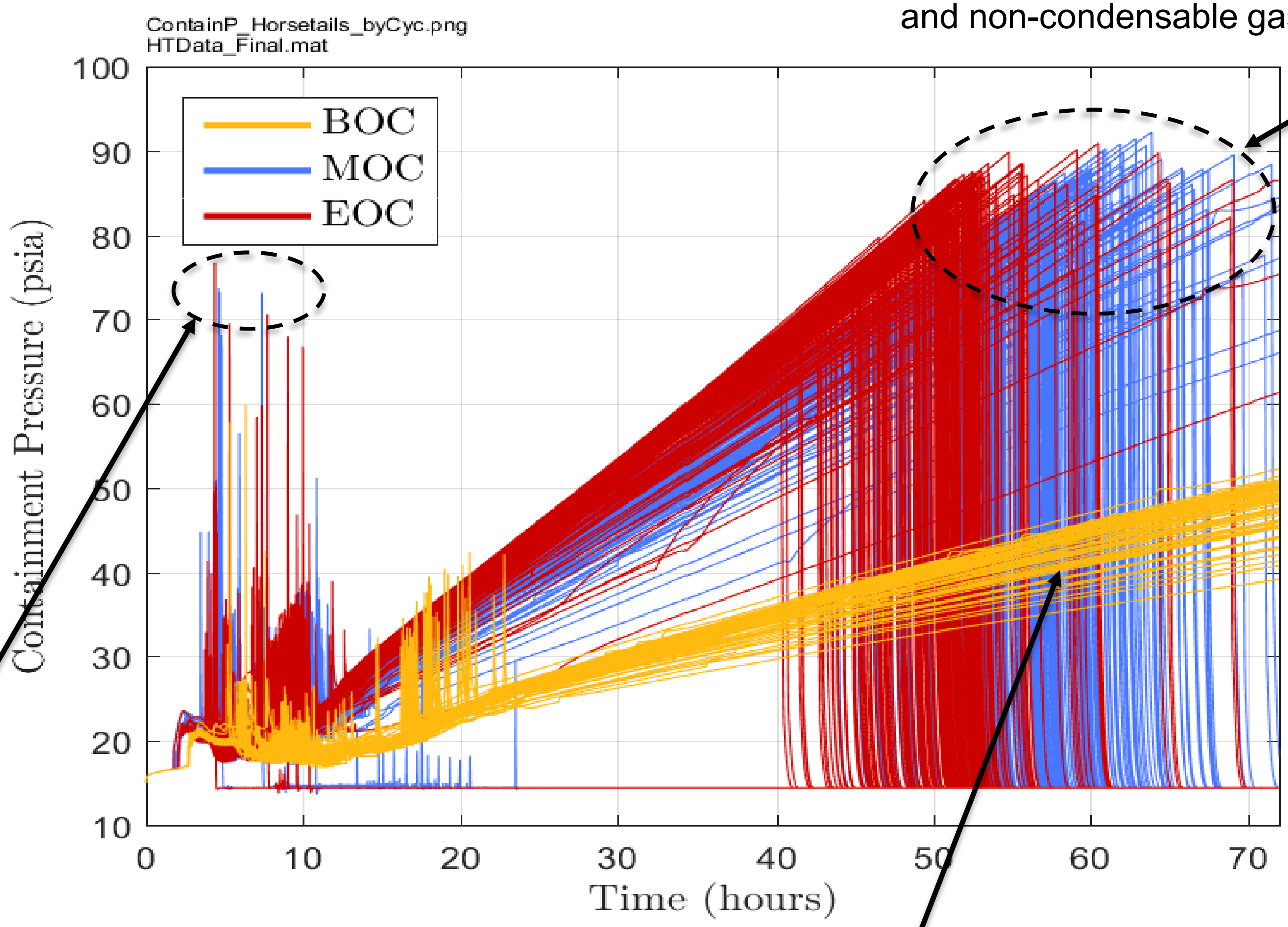




- Pressurizer SV FTC on first cycle
- Relatively much hydrogen vented to containment (through the PRT) and migrated to the dome prior to the first burn
- Containment ruptured early

- No pressurizer SV FTC
- Relatively little hydrogen vented to containment and migrated to the dome prior to the first burn
- Containment not ruptured early

Overall Containment Failure Outcomes



Long-tem containment over-pressurization failure due to prolonged steam production and non-condensable gas generation

Early containment overpressure failures due to sufficiently large burns in containment

No BOC cases exhibit long-term overpressure failure before 72 hours

Basic UA Statistics

- In the 600 total UA calculations, 567 completed to 72 hours
- Of the completed calculations
  - 4 failed containment early on a sudden increase in pressure immediate to the first hydrogen deflagration
  - 492 failed containment between 36 and 72 hours after a gradual monotonic progression in pressure to rupture
  - 71 did not fail containment by 72 hours
    - 65 had a BOC reactor core represented
- In the 600 total UA calculations, 85 had a pressurizer SV FTC; of these 85:
  - 40 had a fractionally open position of the failed valve greater than 0.3
    - 17 failed to complete, meaning that only 23 of the total 600 UA calculations actually had potential to fail containment early
- First burns were ignited by hot gas issuing from the PRT in 23 of the successful 567 UA calculations and among these 23 there were 2 early containment failures

General Observations and Findings From STSBO Accident Progression

Early Containment Failures

(4 out of 567)

- Consequences strongly (and intuitively) affected by *early vs. late* containment failure
- Early containment failures occur *only on the first hydrogen burn* from in-vessel generated hydrogen
- In-vessel generated hydrogen is maximized when pressurizer SV sticks open early at greater than 30% open and with higher temperature fuel collapse criteria
- First burns that fail containment initiated in lower compartment from HL rupture or PRT venting and propagate to dome where more than 150kg hydrogen was present
- Some early burns were just under the sampled containment failure pressure
- Early containment failure source terms generally higher due to unsettled airborne fission products

Late Containment Failures

(492 out of 567)

- Protracted SV cycling produces *lower in-vessel hydrogen*
- Ex-vessel CCI-generated hydrogen greatly exceeds in-vessel hydrogen but produces ongoing small burns
- Ex-vessel burns in cavity prevent large dome hydrogen concentrations from accumulating
- Late hydrogen burns are terminated by insufficient oxygen for combustion
- Late containment failures from static overpressure: increasing temperature, rising steam pressure, accumulating gases
- BOC and some MOC did not fail containment before 72 hours due to lower decay heat and slower pressurization
- Late failures generally have reduced source term release benefiting from gravitational settling

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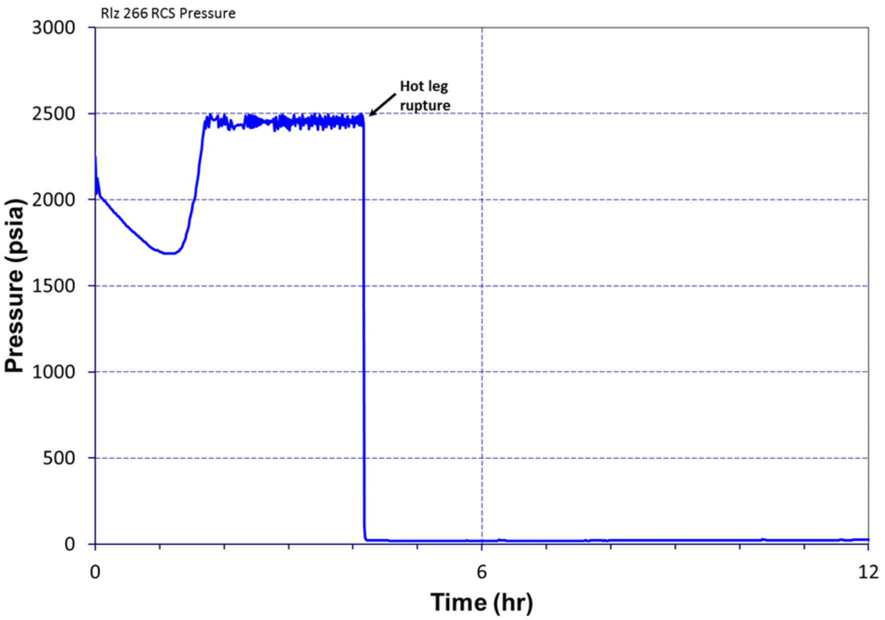
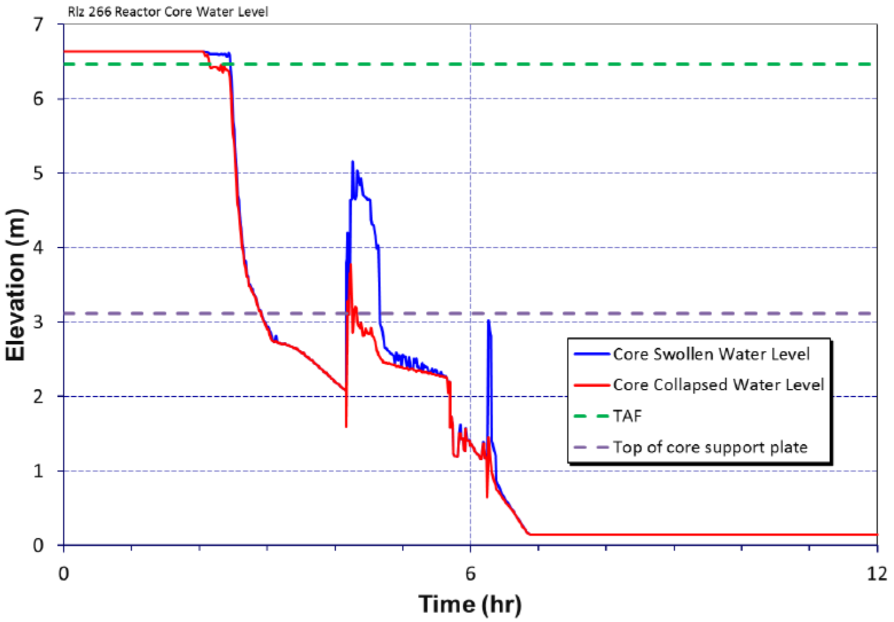
Accident Progression Analysis STSBO Insights from Individual UA Realizations

Reference Realization

In-Vessel Accident Progression

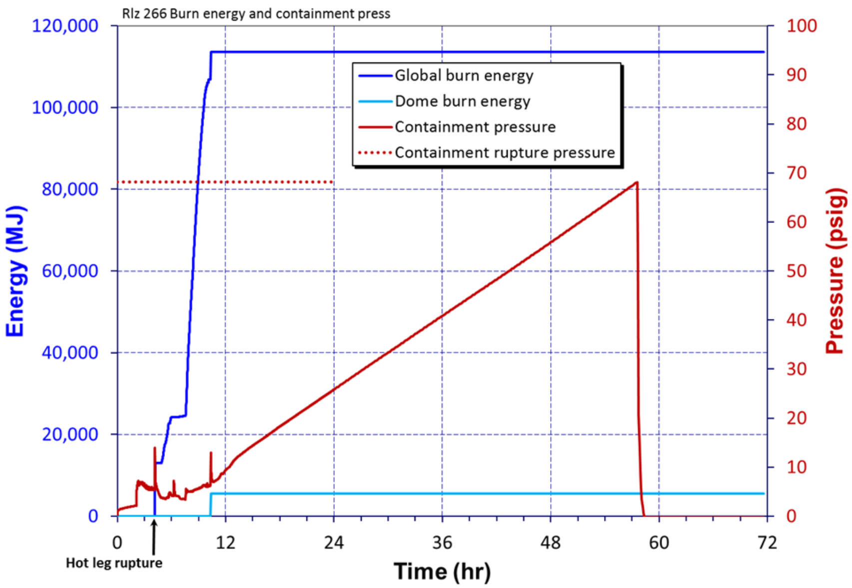
- Core heat up, degradation, and relocation
- Melting (eutectic interaction) and debris formation
- Hydrogen generation
- RCS pressure boundary rupture (e.g., hot leg and ignition sources)
- Effect of system depressurization and accumulator injection
- Fission product deposition in the primary system

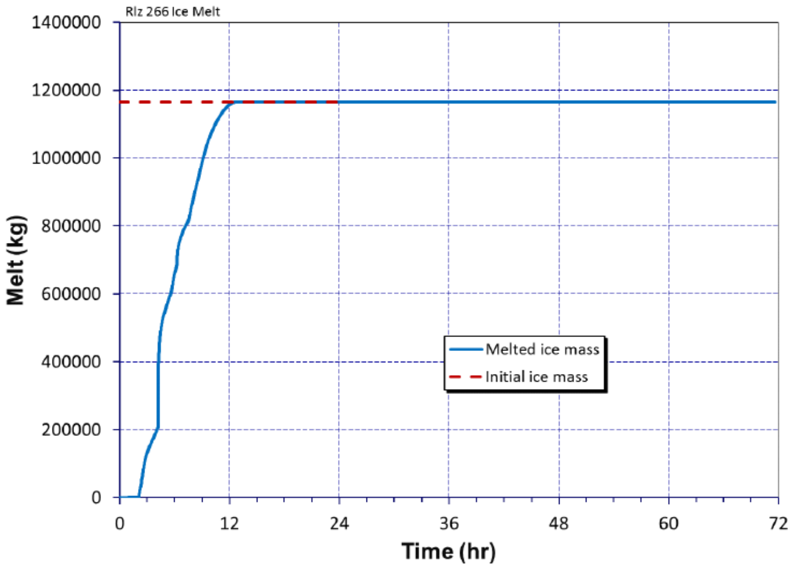


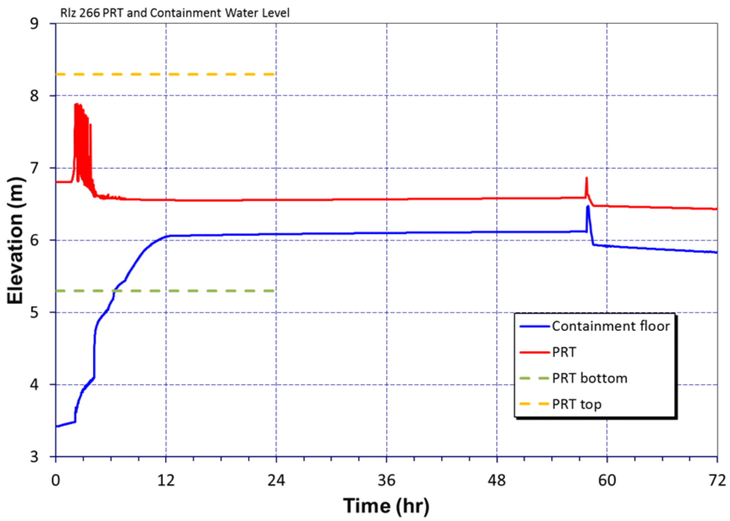


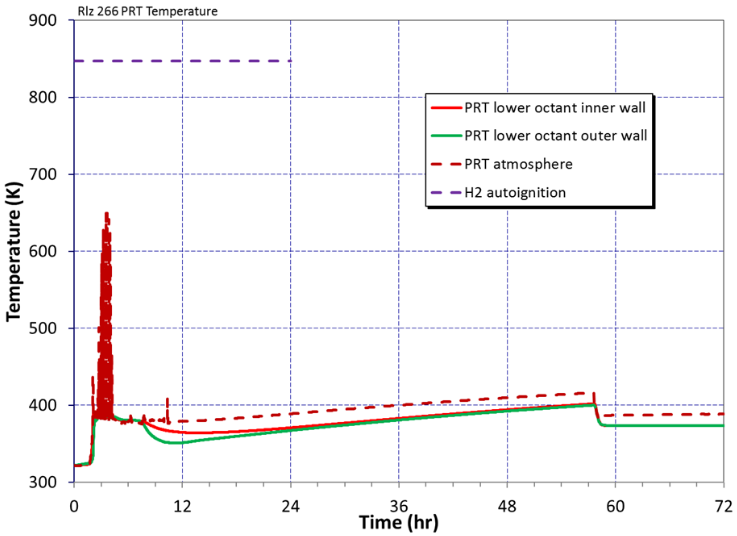
Reference Realization

Ex-Vessel Accident Progression & PRT





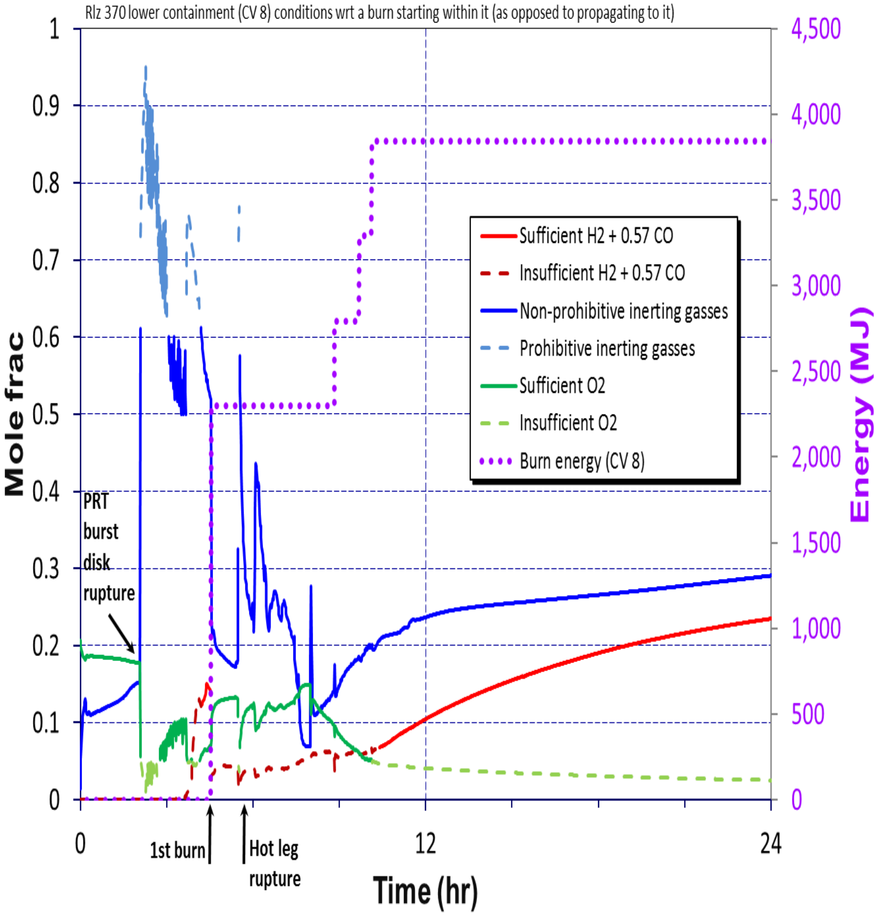


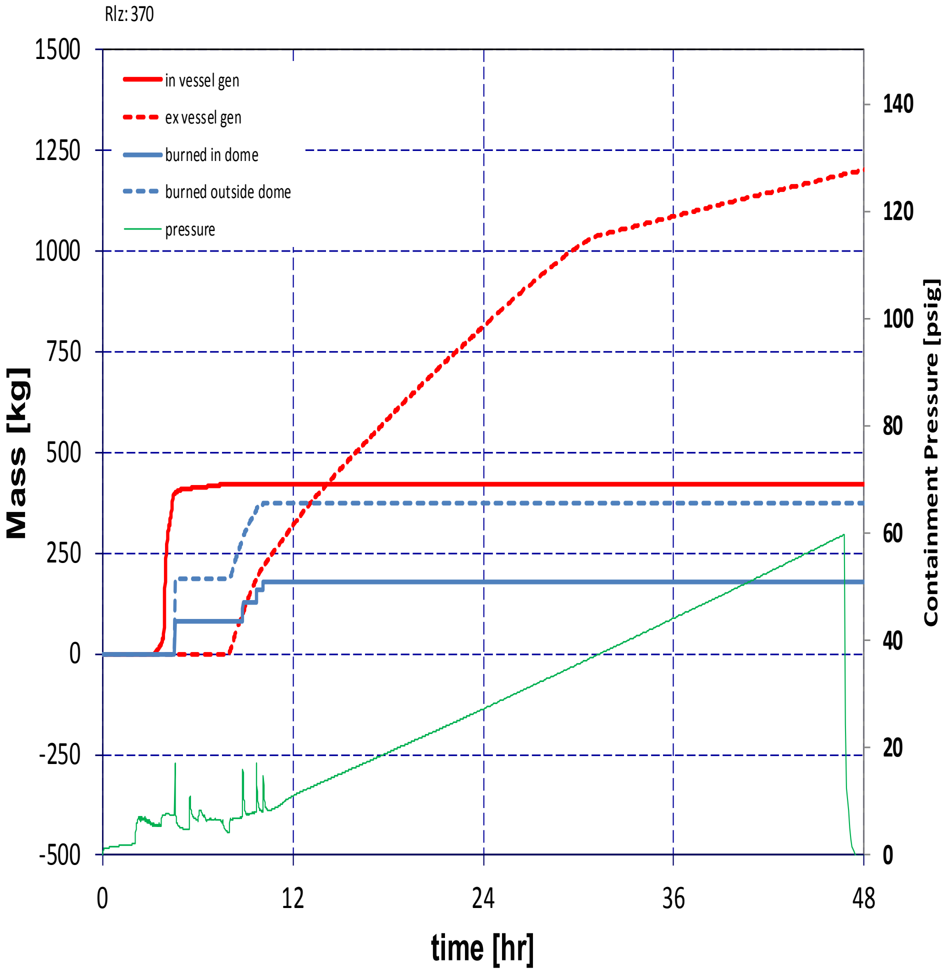


Select Realization Criteria and Identification

Selection criterion	Rlz
The reference STSBO UA case	266
The case with earliest containment rupture	**554**
A case with containment remaining intact at 72 hours	307
A case with a FTC of a pressurizer SV on the first cycle	554
A case without a FTC of a pressurizer SV	307
The case with the least hydrogen vented to containment through the PRT	316
The case with the most hydrogen vented to containment through the PRT	**370**
The case with lowest in-vessel hydrogen production	307
The case with highest in-vessel hydrogen production	318
The case with the largest Cs release to the environment	36
The case with the earliest RPV breach	432
The case with the latest RPV breach	328
A case without hot leg rupture	562
A case where hot gases issuing from the PRT ignite the first hydrogen burn	**316**
One of the few cases that experienced early containment failure	**395**

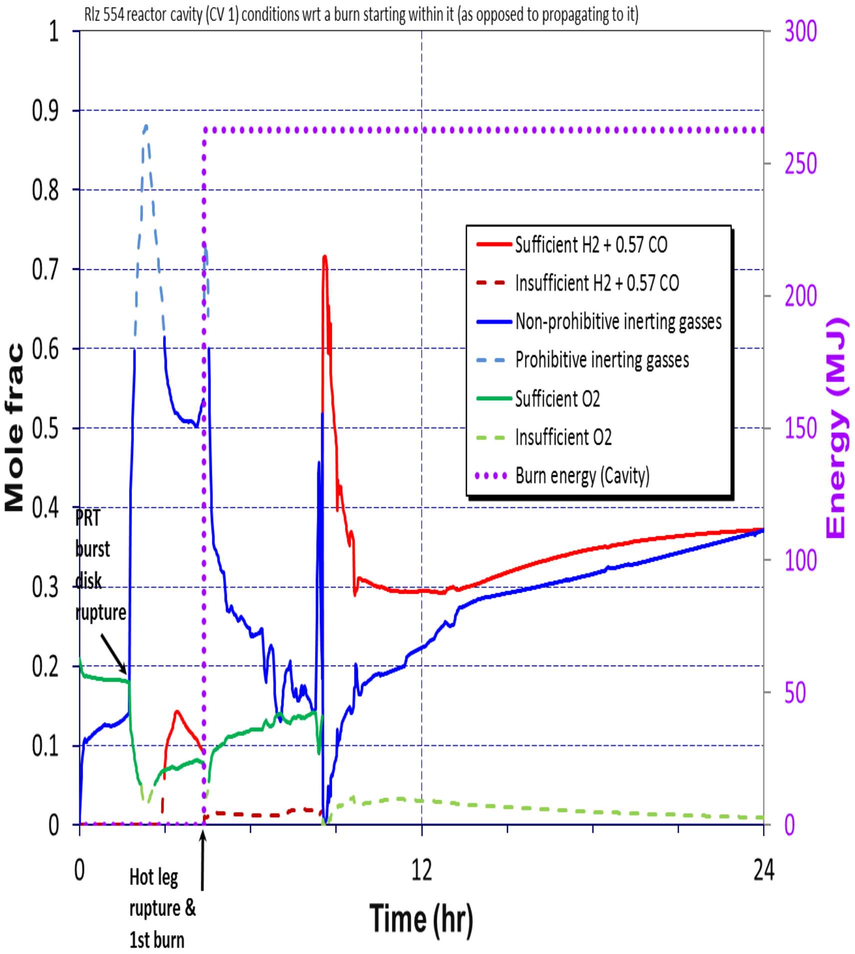
Conditions Leading to First Burn Before RCS Breach

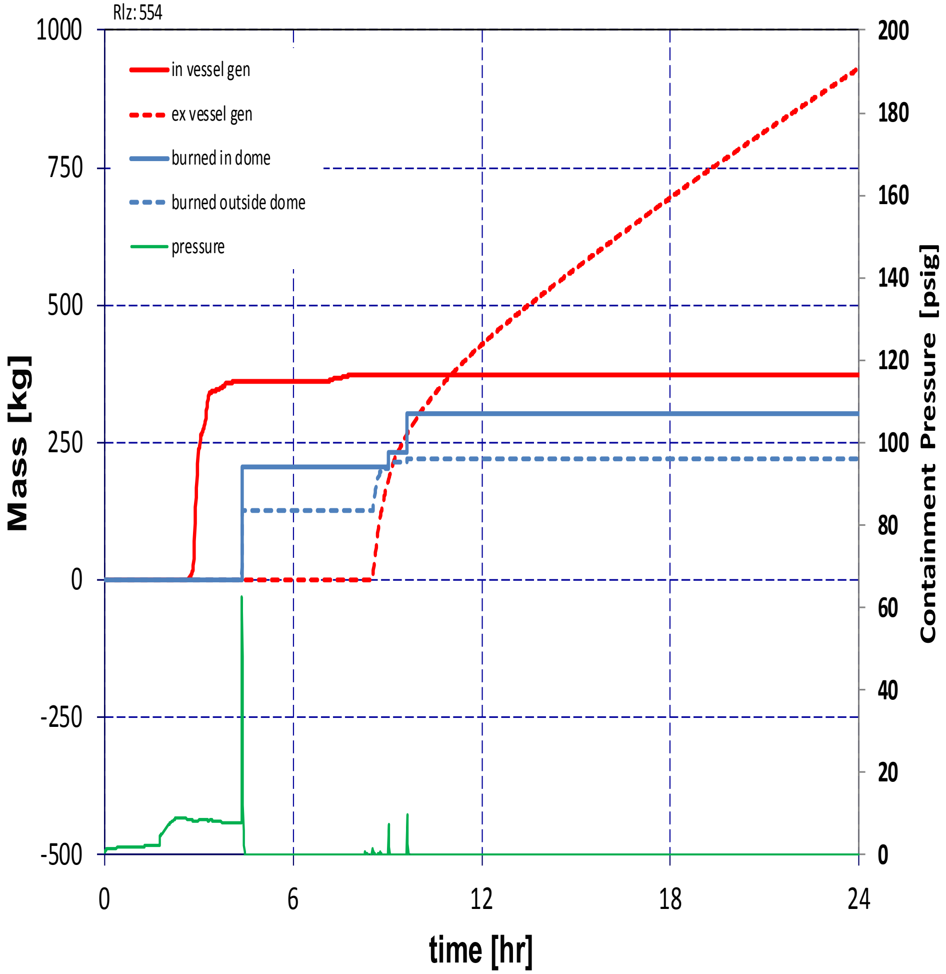




The case with the most hydrogen vented to containment through the PRT – RLZ 370

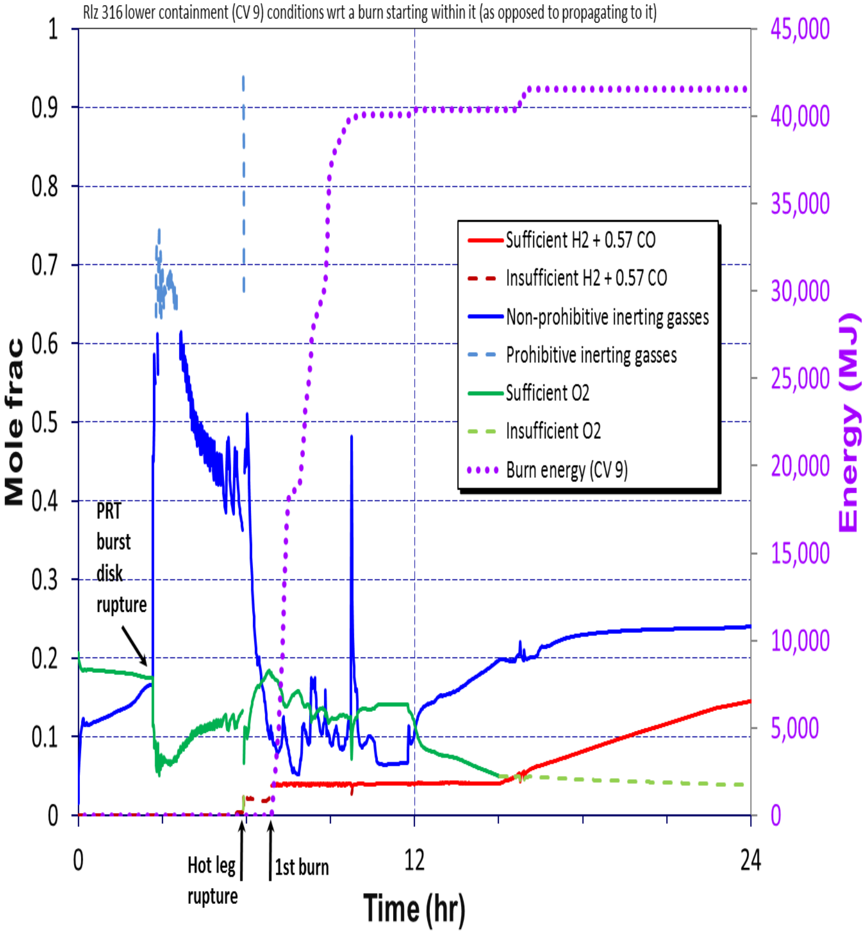
Conditions Leading to First Burn Immediate to RCS Breach

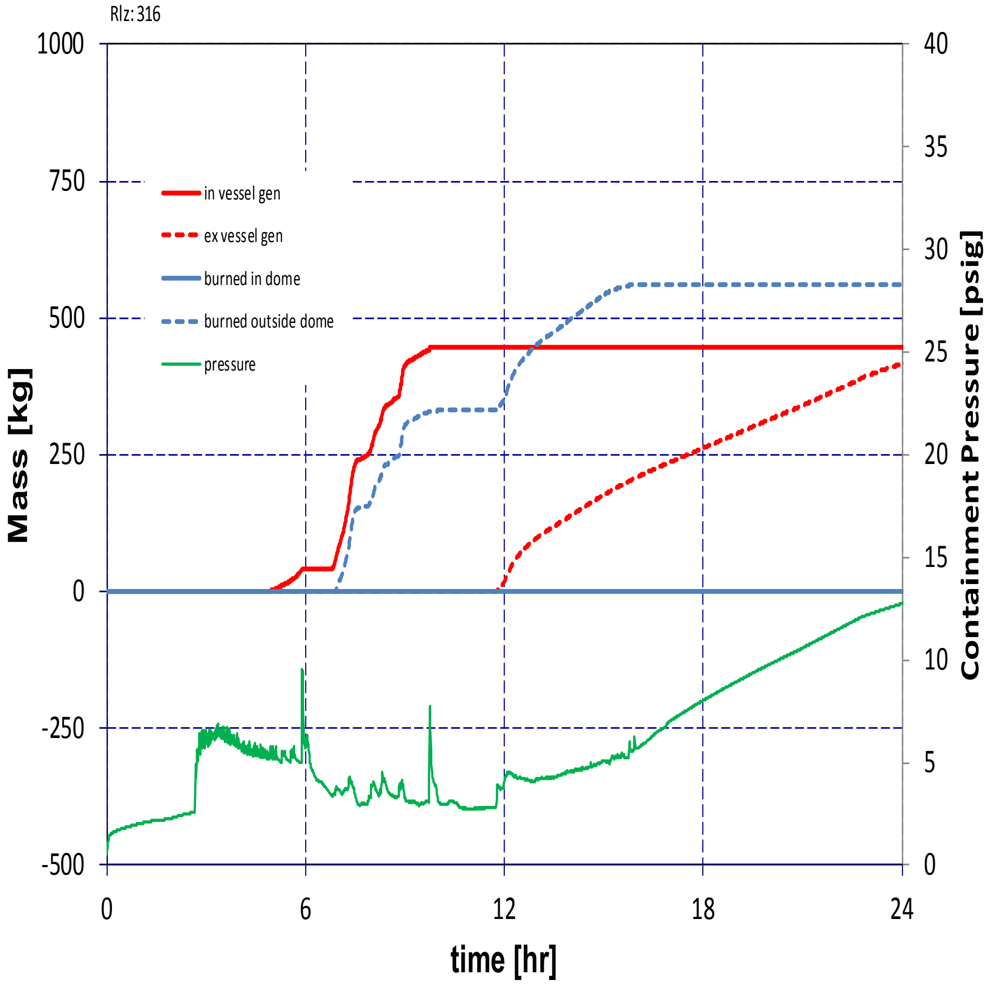




The case with earliest containment rupture – RLZ 554

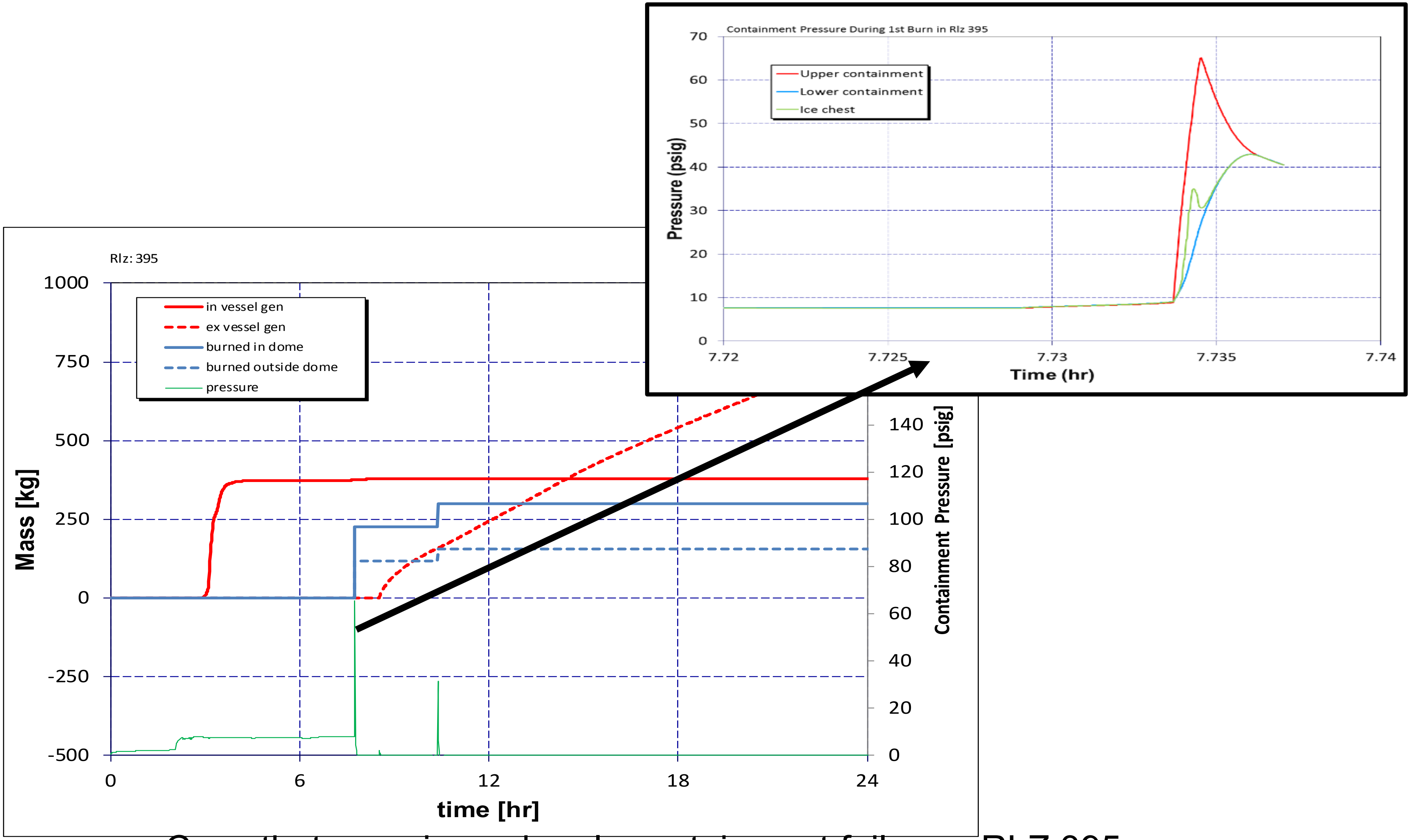
Conditions Leading to First Burn Significantly After RCS Breach





The case with the least hydrogen vented to containment through the PRT – RLZ 316

Containment Pressure Differences During a Burn



Case that experienced early containment failure – RLZ 395

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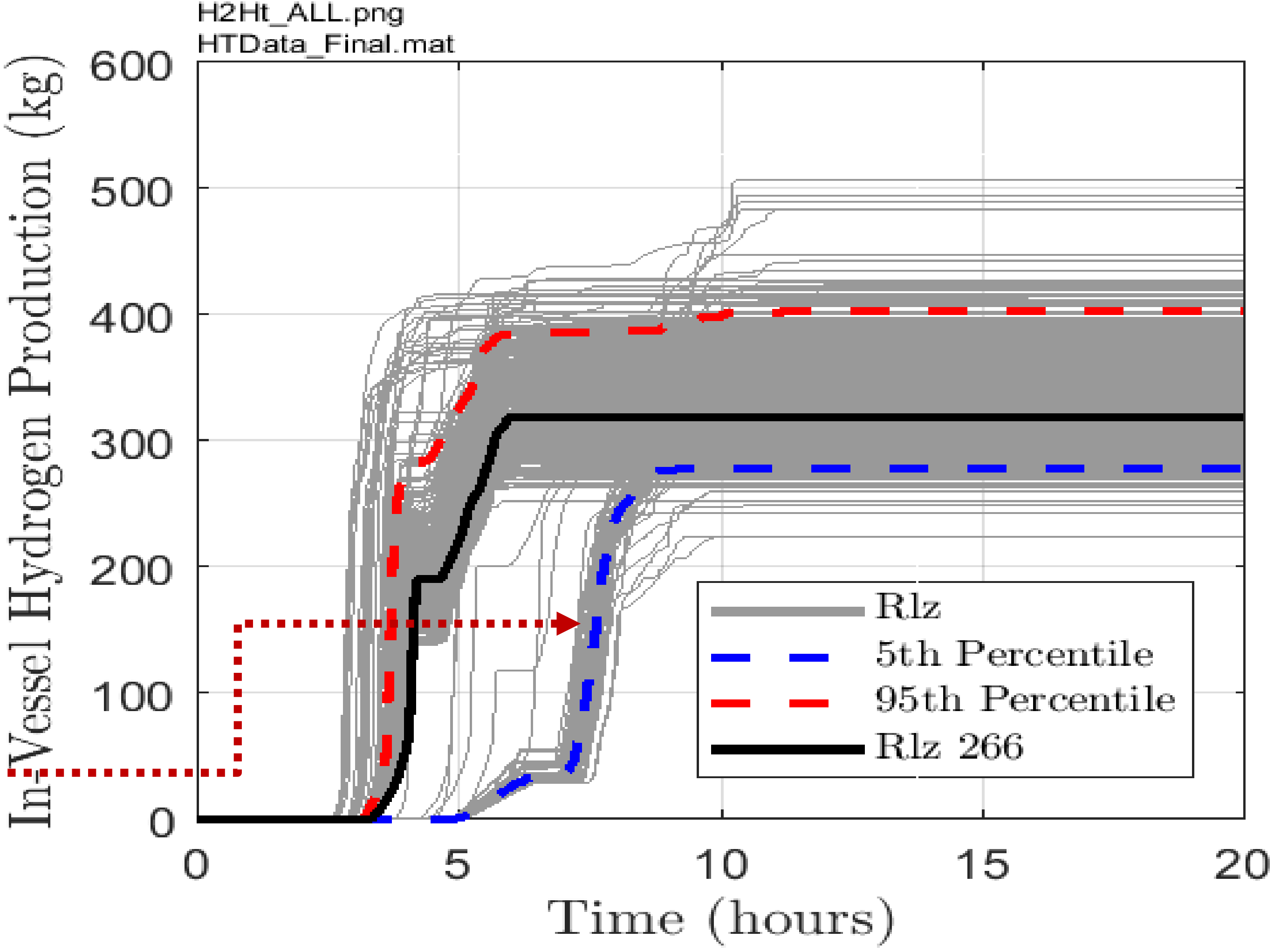
Accident Progression Analysis
STSBO Uncertainty Regression Analysis

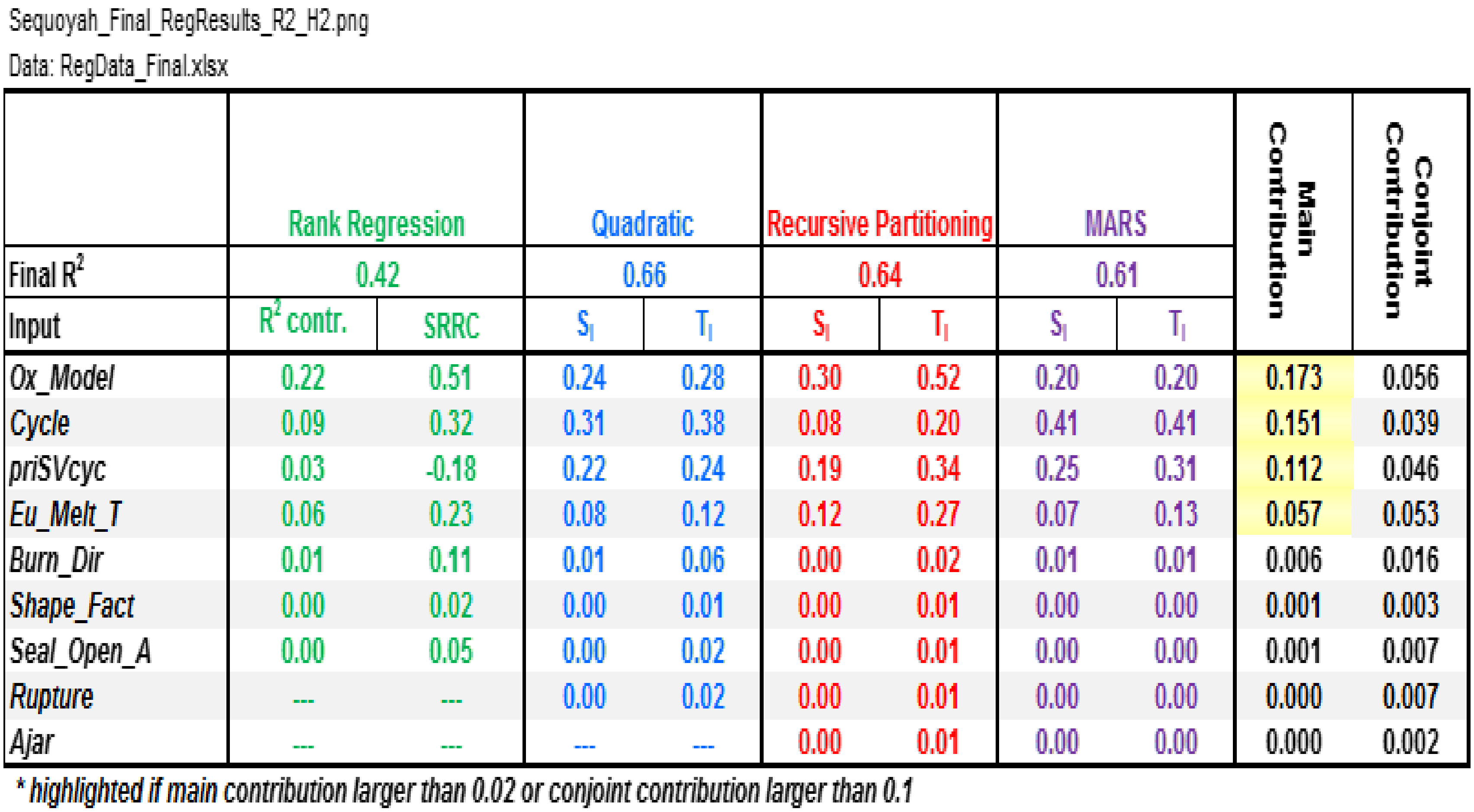
In-vessel H_2 generation results

All realizations



BOC cases lag MOC and EOC cases, but exhibit larger spread after 10 hours

In-vessel Hydrogen regression



Sequoyah_Final_RegResults_R2_H2.png

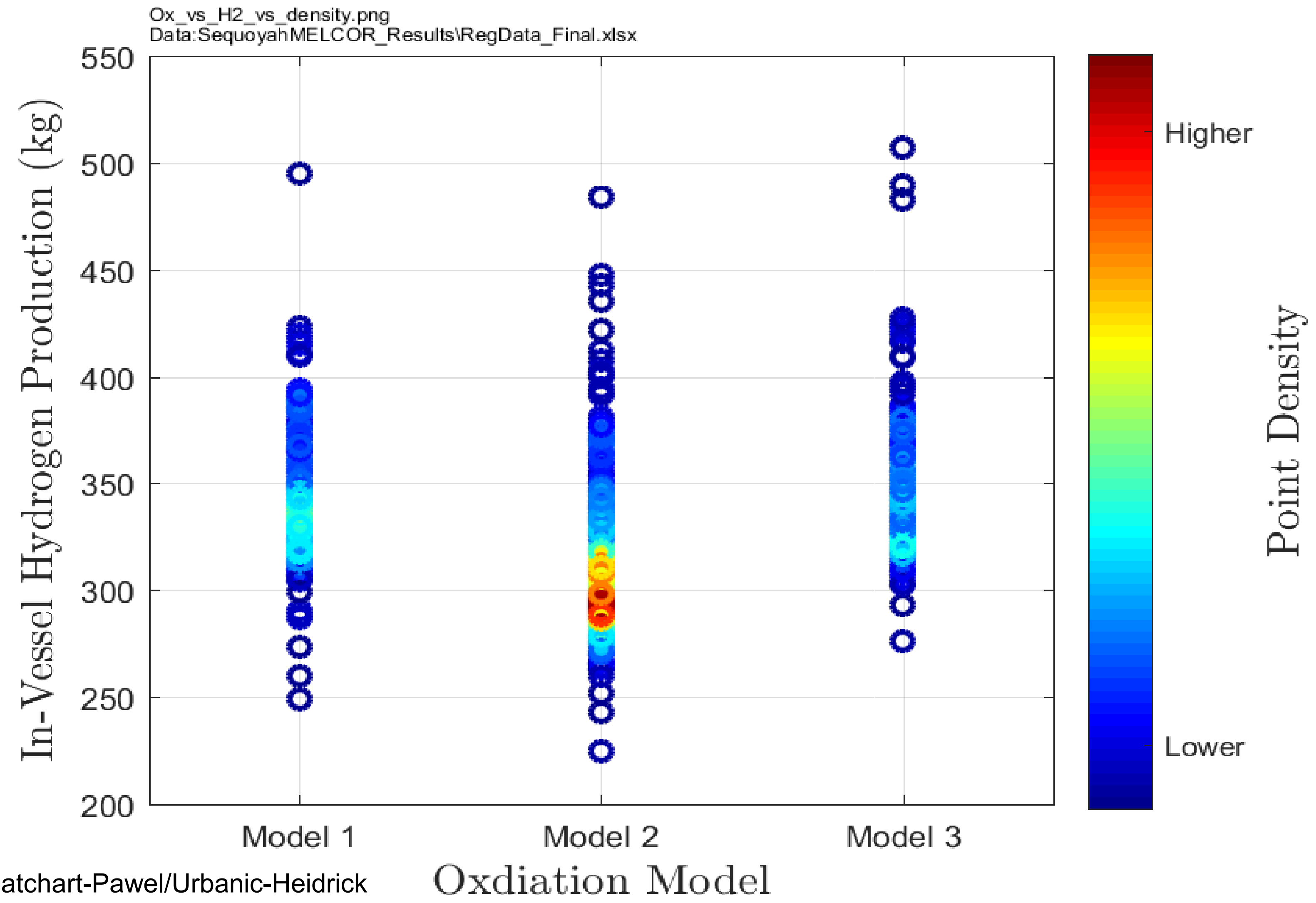
Data: RegData_Final.xlsx

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contribution	Conjoint Contribution
Final R^2	0.42		0.66		0.64		0.61			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
Ox_Model	0.22	0.51	0.24	0.28	0.30	0.52	0.20	0.20	0.173	0.056
Cycle	0.09	0.32	0.31	0.38	0.08	0.20	0.41	0.41	0.151	0.039
priSVcyc	0.03	-0.18	0.22	0.24	0.19	0.34	0.25	0.31	0.112	0.046
Eu_Melt_T	0.06	0.23	0.08	0.12	0.12	0.27	0.07	0.13	0.057	0.053
Burn_Dir	0.01	0.11	0.01	0.06	0.00	0.02	0.01	0.01	0.006	0.016
Shape_Fact	0.00	0.02	0.00	0.01	0.00	0.01	0.00	0.00	0.001	0.003
Seal_Open_A	0.00	0.05	0.00	0.02	0.00	0.01	0.00	0.00	0.001	0.007
Rupture	---	---	0.00	0.02	0.00	0.01	0.00	0.00	0.000	0.007
Ajar	---	---	---	---	0.00	0.01	0.00	0.00	0.000	0.002

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

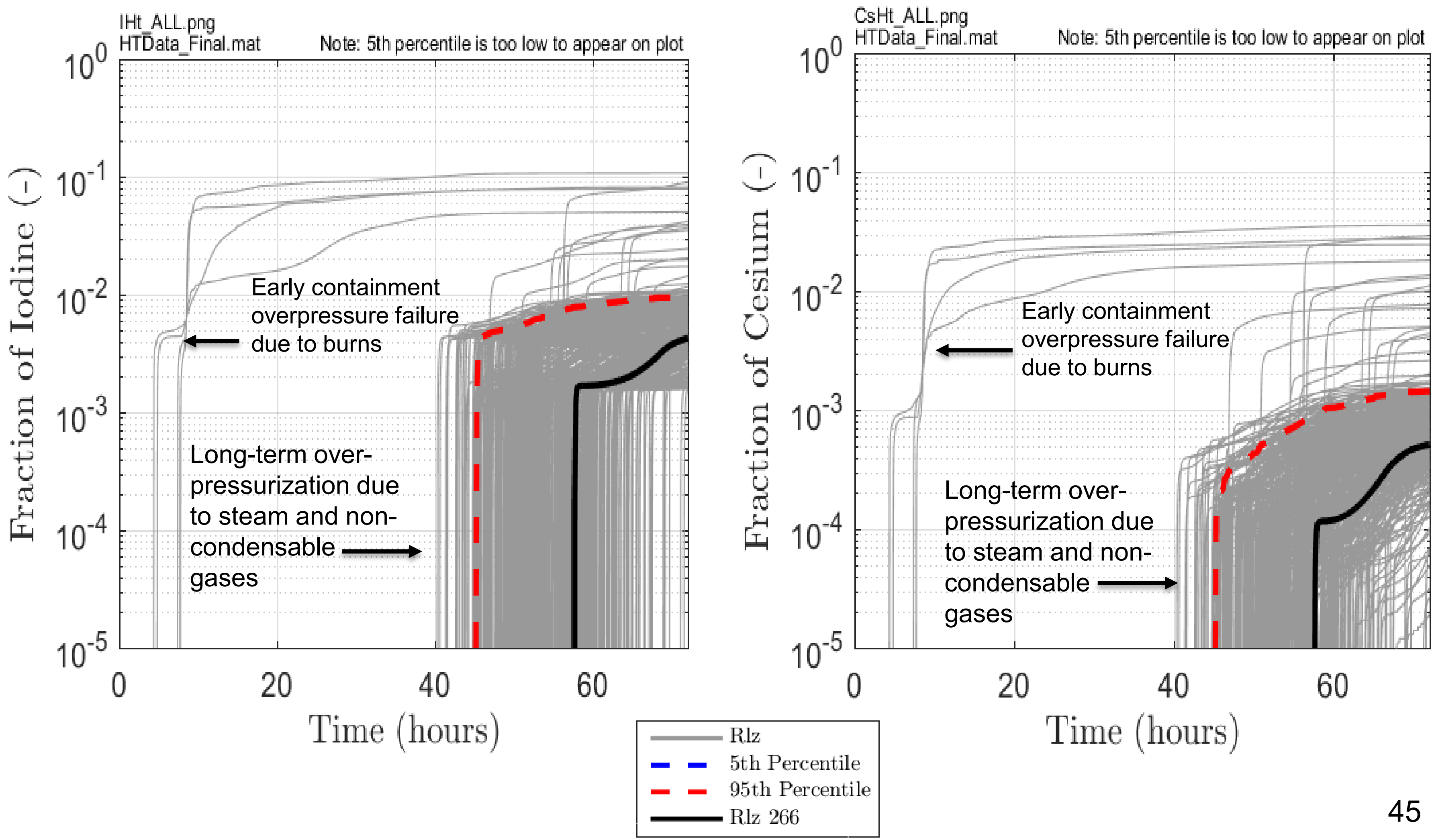
In-vessel Hydrogen

Oxidation model



Model 1 = Catchart-Pawel/Urbanic-Heidrick
Model 2 = Leistikov-Schanz/Prater-Courtright
Model 3 = Urbanic Heidrick

Cesium & Iodine release fractions



All realizations - Iodine

All realizations - Cesium

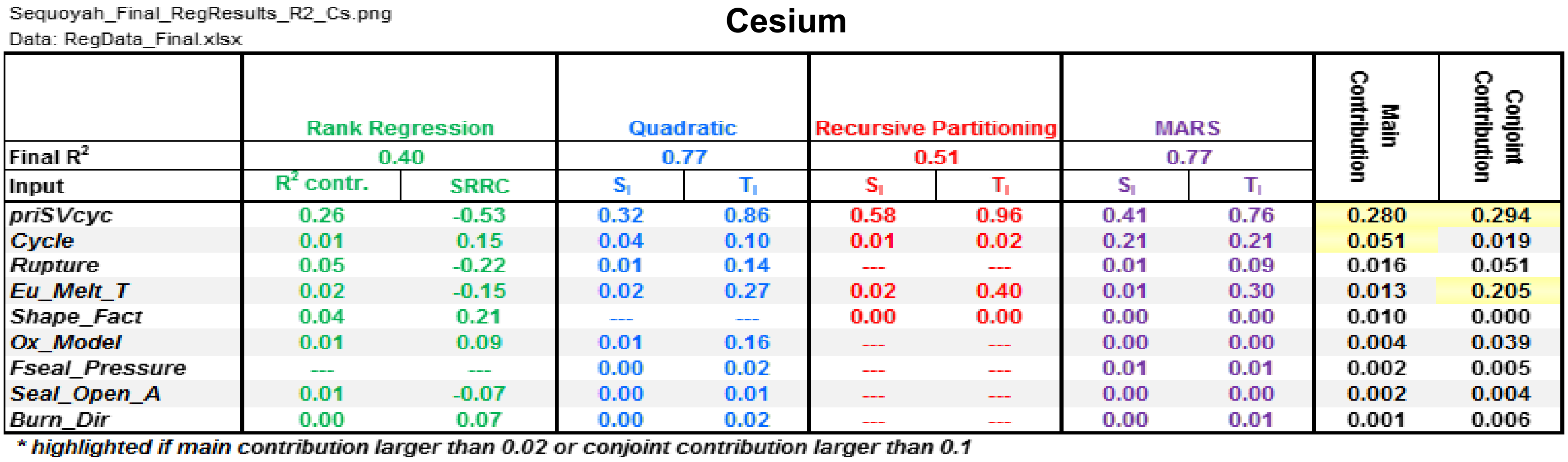
Early containment overpressure failure due to burns

Long-term over-pressurization due to steam and non-condensable gases

Early containment overpressure failure due to burns

Long-term over-pressurization due to steam and non-condensable gases

Cesium & Iodine regression

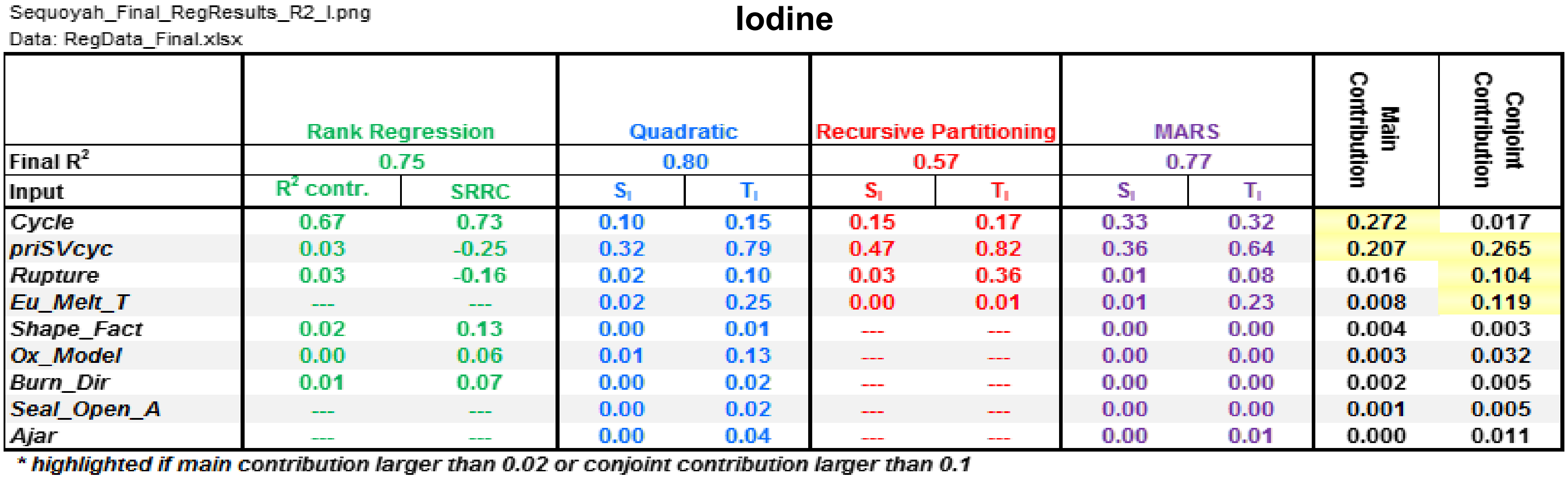


Sequoyah_Final_RegResults_R2_Cs.png
Data: RegData_Final.xlsx

Cesium

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contribution	Conjoint Contribution
Final R^2	0.40		0.77		0.51		0.77			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
priSVcyc	0.26	-0.53	0.32	0.86	0.58	0.96	0.41	0.76	0.280	0.294
Cycle	0.01	0.15	0.04	0.10	0.01	0.02	0.21	0.21	0.051	0.019
Rupture	0.05	-0.22	0.01	0.14	---	---	0.01	0.09	0.016	0.051
Eu_Melt_T	0.02	-0.15	0.02	0.27	0.02	0.40	0.01	0.30	0.013	0.205
Shape_Fact	0.04	0.21	---	---	0.00	0.00	0.00	0.00	0.010	0.000
Ox_Model	0.01	0.09	0.01	0.16	---	---	0.00	0.00	0.004	0.039
Fseal_Pressure	---	---	0.00	0.02	---	---	0.01	0.01	0.002	0.005
Seal_Open_A	0.01	-0.07	0.00	0.01	---	---	0.00	0.00	0.002	0.004
Burn_Dir	0.00	0.07	0.00	0.02	---	---	0.00	0.01	0.001	0.006

**** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1***



Sequoyah_Final_RegResults_R2_I.png
Data: RegData_Final.xlsx

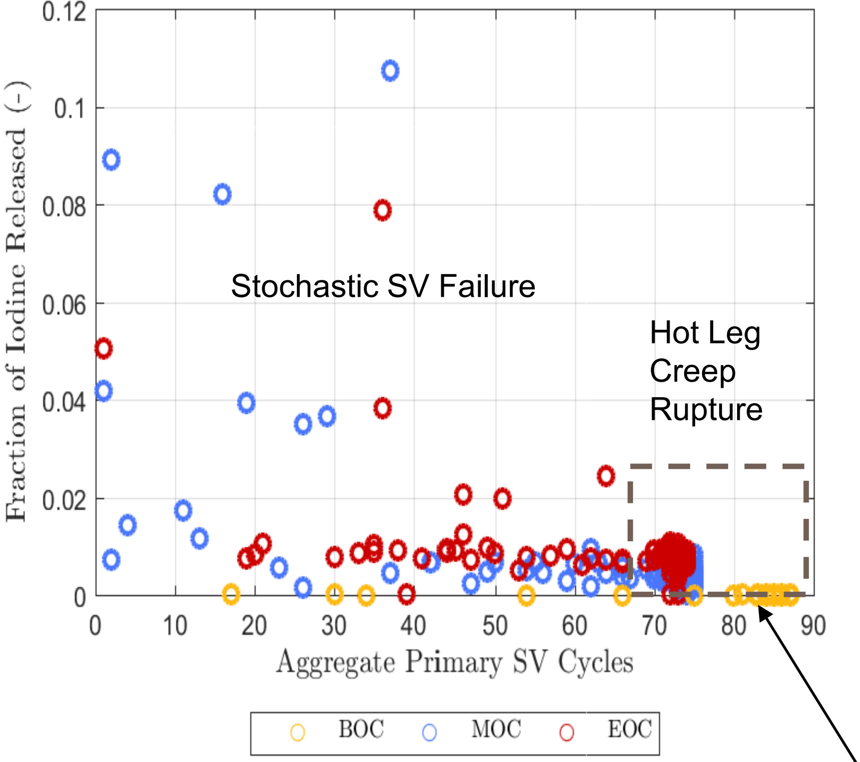
Iodine

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contribution	Conjoint Contribution
Final R^2	0.75		0.80		0.57		0.77			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
Cycle	0.67	0.73	0.10	0.15	0.15	0.17	0.33	0.32	0.272	0.017
priSVcyc	0.03	-0.25	0.32	0.79	0.47	0.82	0.36	0.64	0.207	0.265
Rupture	0.03	-0.16	0.02	0.10	0.03	0.36	0.01	0.08	0.016	0.104
Eu_Melt_T	---	---	0.02	0.25	0.00	0.01	0.01	0.23	0.008	0.119
Shape_Fact	0.02	0.13	0.00	0.01	---	---	0.00	0.00	0.004	0.003
Ox_Model	0.00	0.06	0.01	0.13	---	---	0.00	0.00	0.003	0.032
Burn_Dir	0.01	0.07	0.00	0.02	---	---	0.00	0.00	0.002	0.005
Seal_Open_A	---	---	0.00	0.02	---	---	0.00	0.00	0.001	0.005
Ajar	---	---	0.00	0.04	---	---	0.00	0.01	0.000	0.011

**** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1***

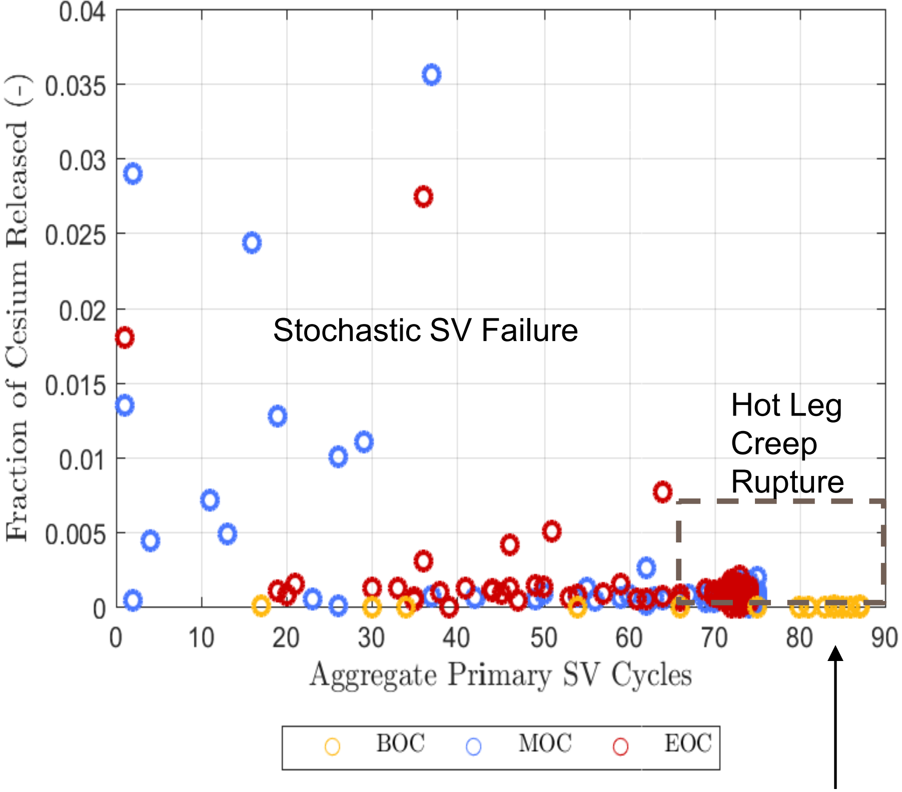
Cesium & Iodine release

Primary SV cycles



I_vs_priSVcyc_vs_cyc.png
Data:SequoyahMELCOR_Results\RegData_Final.xlsx

Iodine



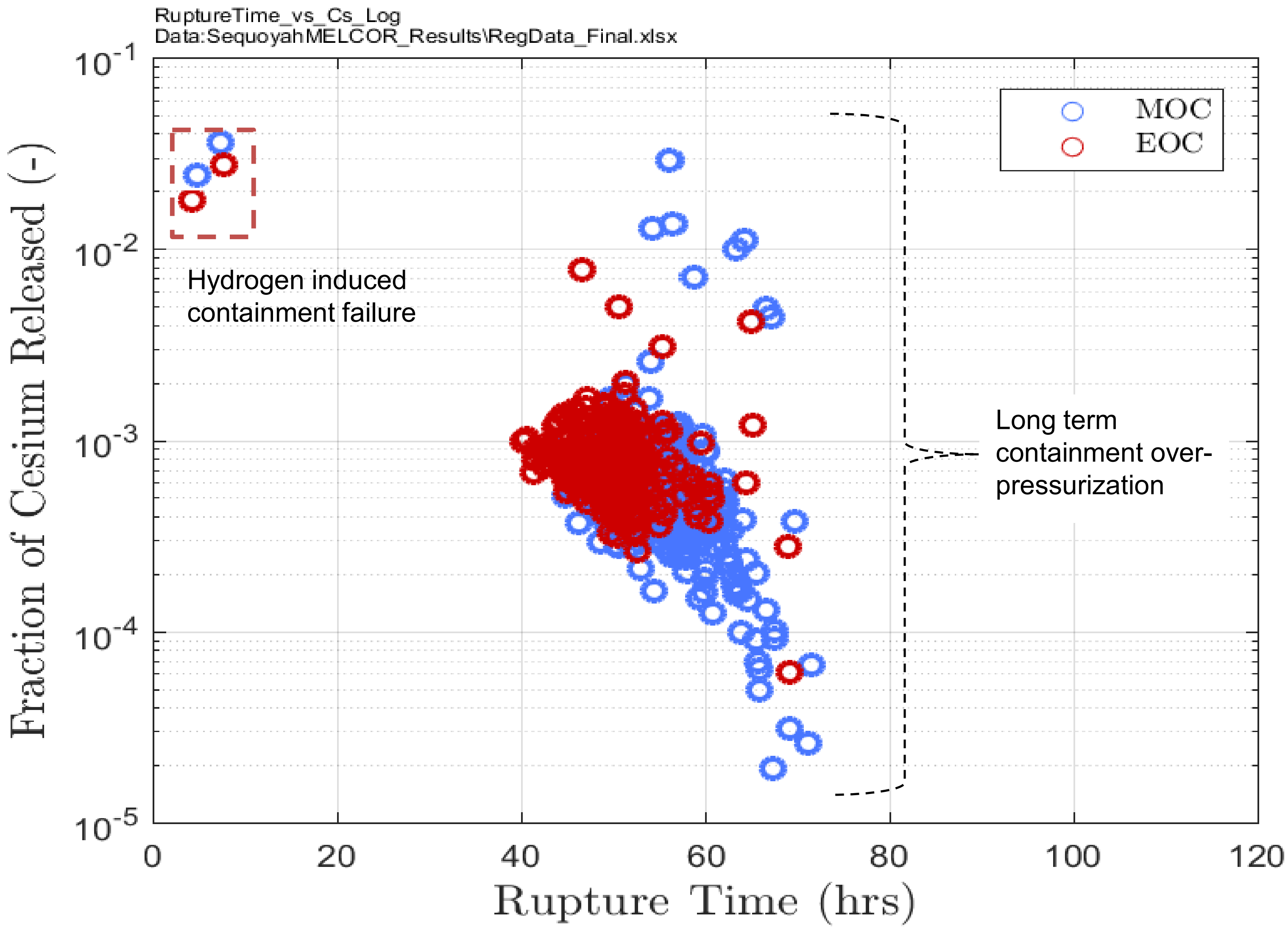
Cs_vs_priSVcyc_vs_cyc.png
Data:SequoyahMELCOR_Results\RegData_Final.xlsx

Cesium

Eventual hot leg creep rupture precludes total primary SV cycles beyond ~75 cycles for MOC/EOC cases, and ~85 for BOC cases

Cesium release

Timing of containment rupture

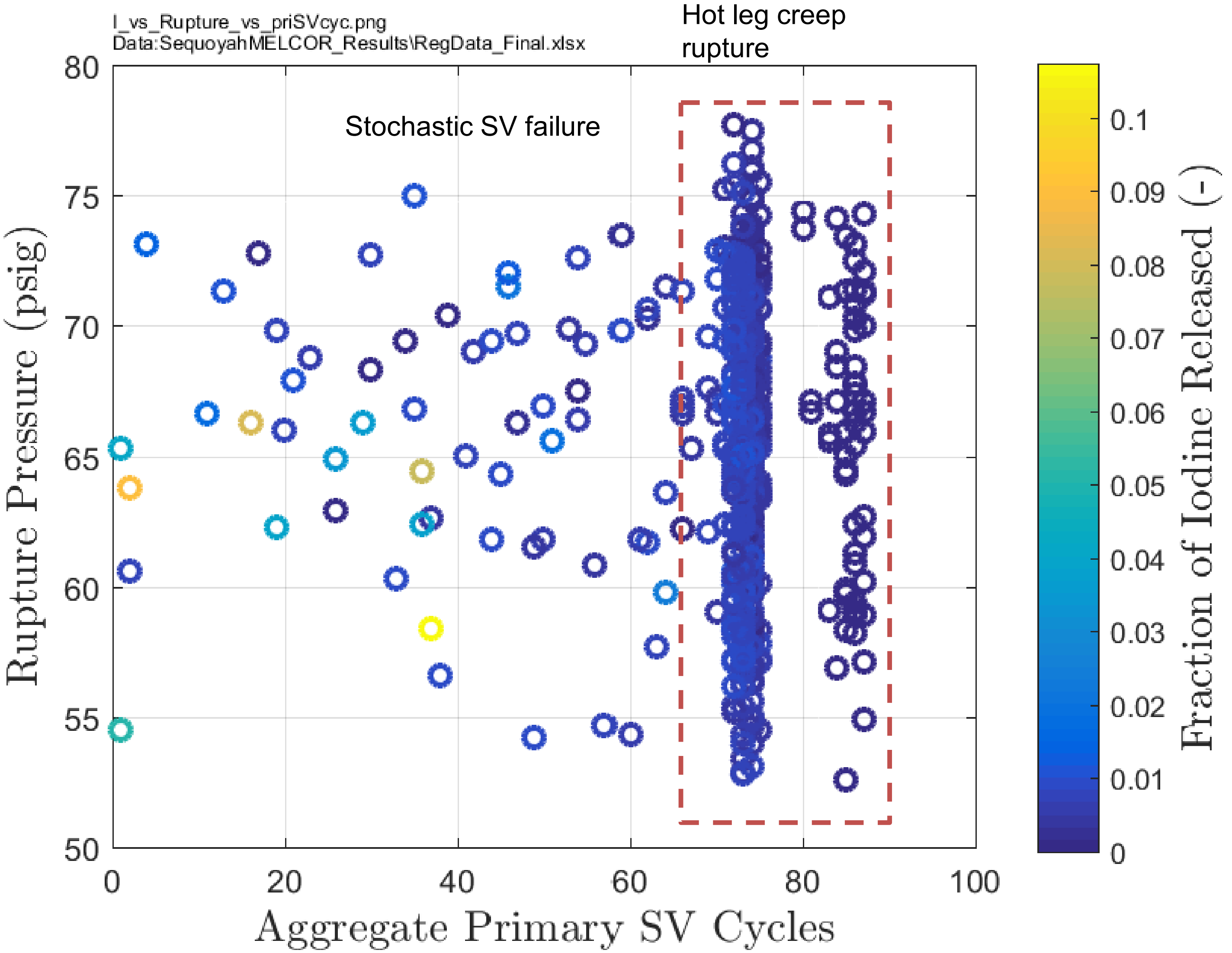


Hydrogen induced containment failure

Long term containment over-pressurization

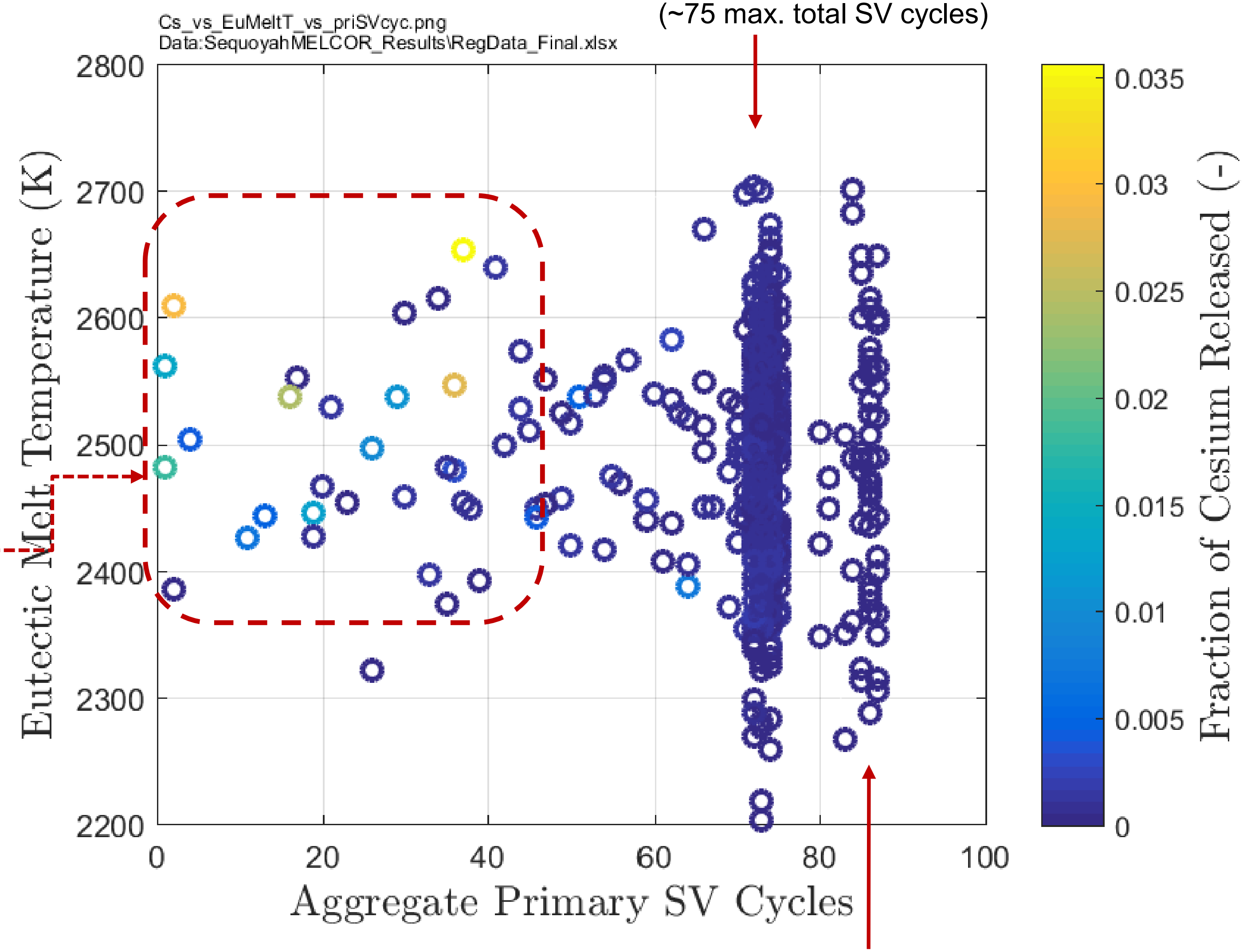
Iodine release

Containment rupture pressure



Cesium release

Eutectic melt temperature



Cs_vs_EuMeltT_vs_priSVcyc.png
Data:SequoyahMELCOR_Results\RegData_Final.xlsx

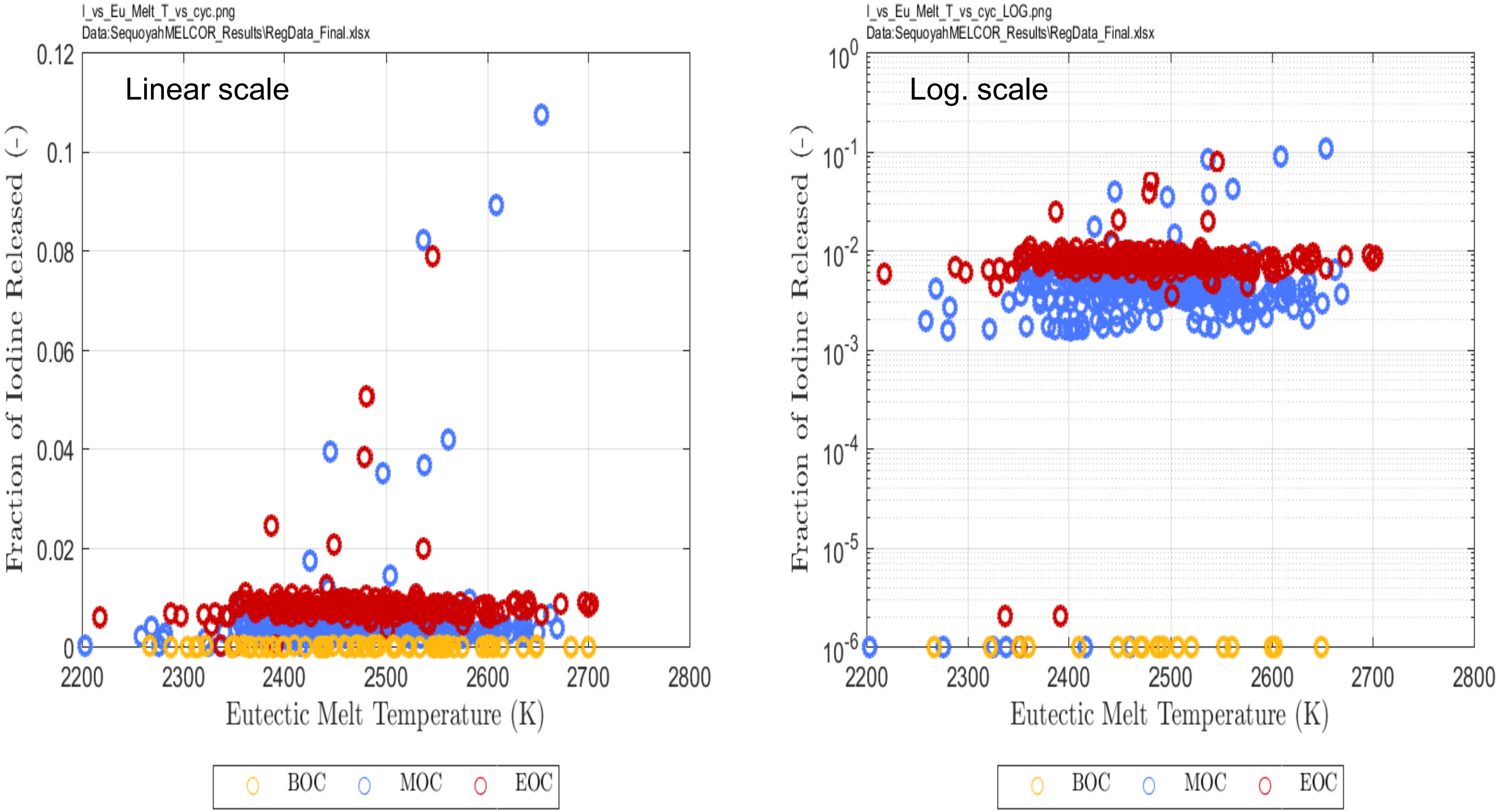
MOC/EOC cases with creep rupture (~75 max. total SV cycles)

Cases with SV failure exhibit trend of higher liquefaction temperature and higher releases

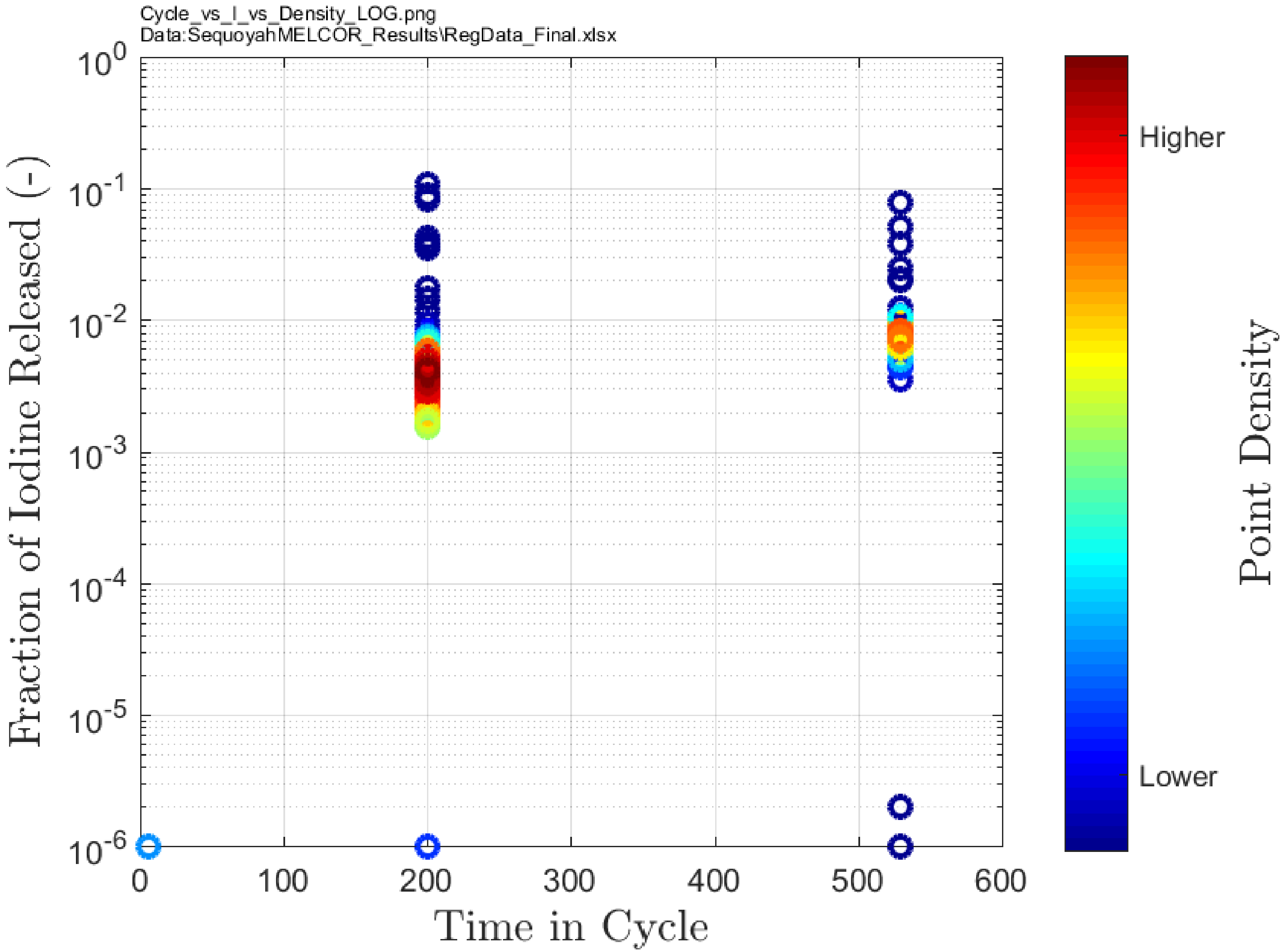
BOC cases with creep rupture (~85 max. total SV cycles)

Iodine release

Eutectic melt temperature



Iodine release by time in cycle



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Accident Progression Analysis
STSBO Model Errors and Impact of Igniters

Input Model Error Assessment

- Two input errors discovered after the completion of the UA calculations
  - Discussed in Section 4.3 and Appendix E

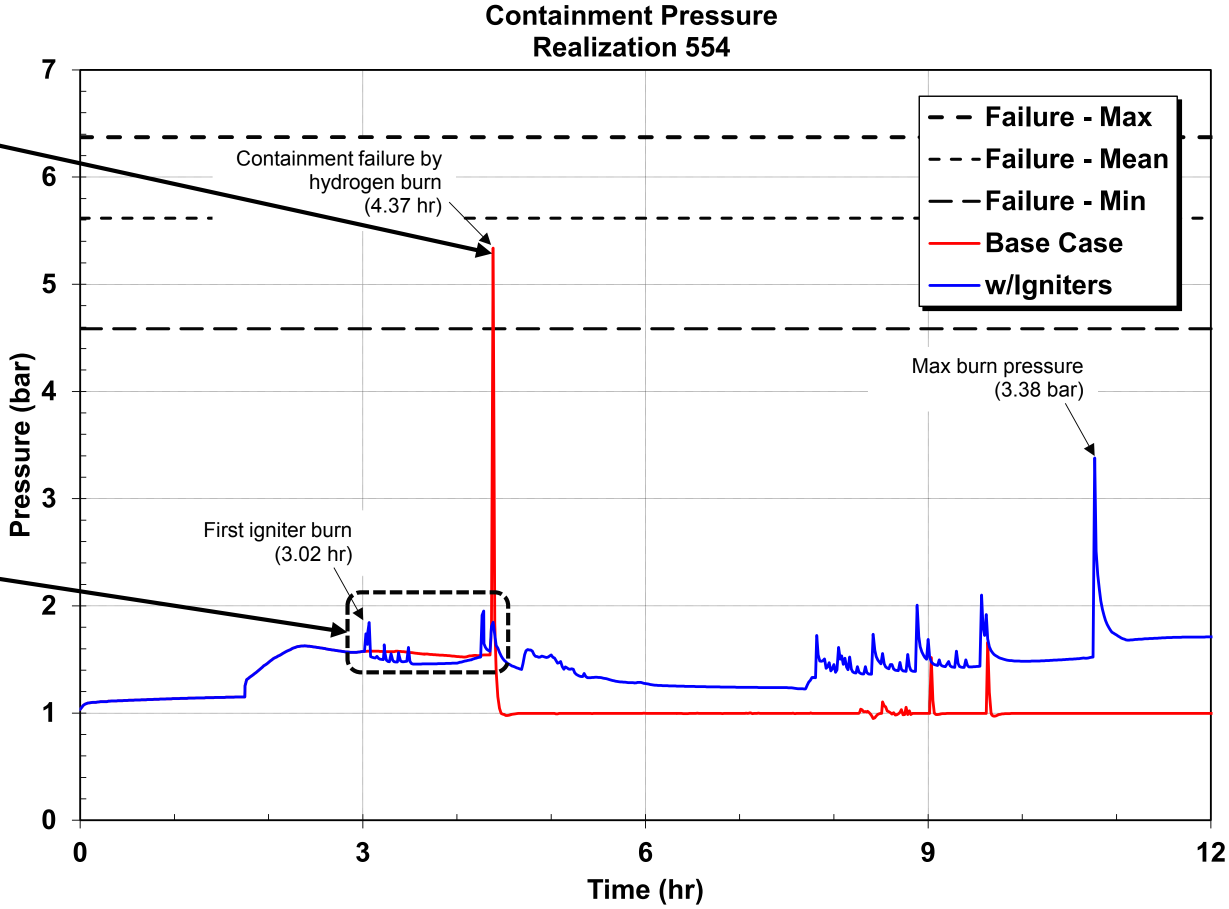
    #1 = Barrier seal sampled failure pressure

    #2 = Hot leg creep rupture temperature model
- Shift to uncertainty sampling for the barrier seal failure was via an external file from an uncertainty program
  - Data from uncertainty file was truncated when read in MELCOR
  - Failure pressure was 10X too small (e.g., 40 psid became 4 psid)
- Sensitivity calculations were performed and documented to confirm low impact of each error

Effect of Igniters (STSBO)

STSBO Realization 554 (Early Containment Failure) Sensitivity Study

- Without igniters hydrogen accumulates and a burn at the RCS breach ruptures the containment
- Igniters control hydrogen accumulation and cause earlier deflagrations with lower peak pressures



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Accident Progression Analysis
LTSBO Analysis

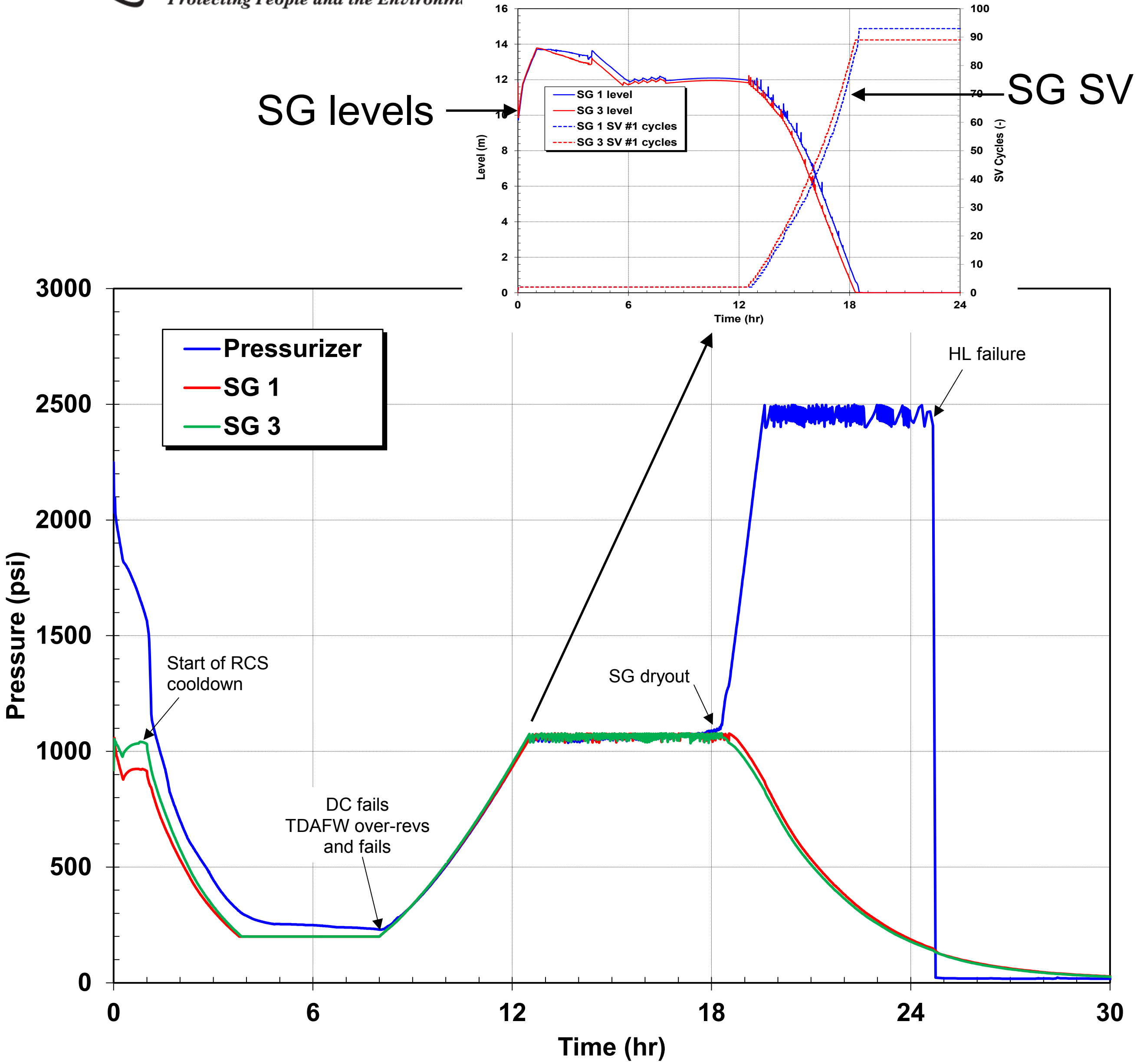
Key changes from Draft UA calculations

- Contacted Sequoyah on TDAFW operation in SBO conditions
  - Confirmed valve failure status at t=0 s
  - Confirmed operator actions
  - Symmetric cooling desired
- Updated secondary AFW modeling
  - Modeled AFW pump using new MELCOR pump model and manufacturer pump performance curves
  - Specified AFW turbine steam flow based on pump power and efficiency
  - Added control logic to replicate manual AFW control with sensitivities on operator and equipment failures
- Uncertain parameters set to median values

LTSBO Sensitivities

Name	Battery Life	PZR SV FTC	PZR SV FTC Area	SG1 SV FTC	SG1 SV FTC Area	TDAFW Failure	Igniters	In-vessel H_2 at first Ignition
LTSBO-0	8 hr	720 (median)	0.1 (median)	146 (median)	0.1 (median)	Over-speed at DC failure	No	38 kg
LTSBO-0a							No + no PRT ignition	38 kg
LTSBO-0b							Yes + no PRT ignition	26 kg
LTSBO-1	8 hr	1	1.0	146 (median)	0.1 (median)	Over-speed at DC failure	No	390 kg
LTSBO-1a							No + no PRT ignition	412 kg
LTSBO-1b							Yes + no PRT ignition	162 kg
LTSBO-2	8 hr	720 (median)	0.1 (median)	146 (median)	0.1 (median)	Run at rated until flooded after DC failure	No	88 kg
LTSBO-3	8 hr	720 (median)	0.1 (median)	146 (median)	0.1 (median)	Run on decay heat curve until CST is empty	No	88 kg
LTSBO-4	8 hr	720 (median)	0.1 (median)	1	1.0	Over-speed at DC failure	No	74 kg
LTSBO-4a (no ARVs)								
LTSBO-5	4 hr	720 (median)	0.1 (median)	146 (median)	0.1 (median)	Over-speed at DC failure	No	74 kg

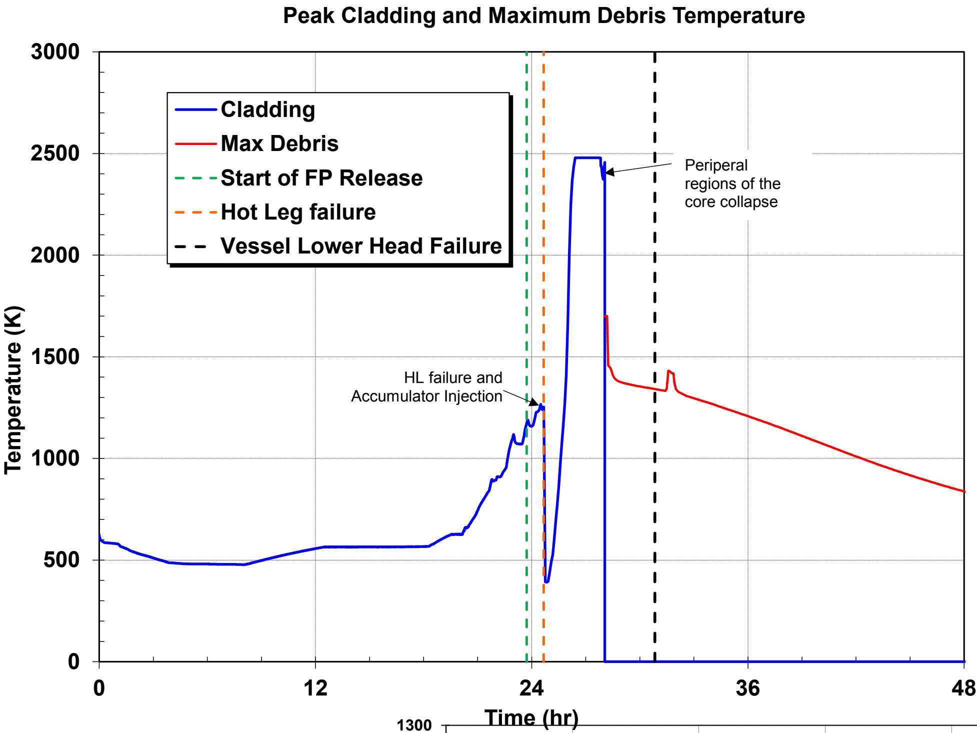
Reference LTSBO – median UA values

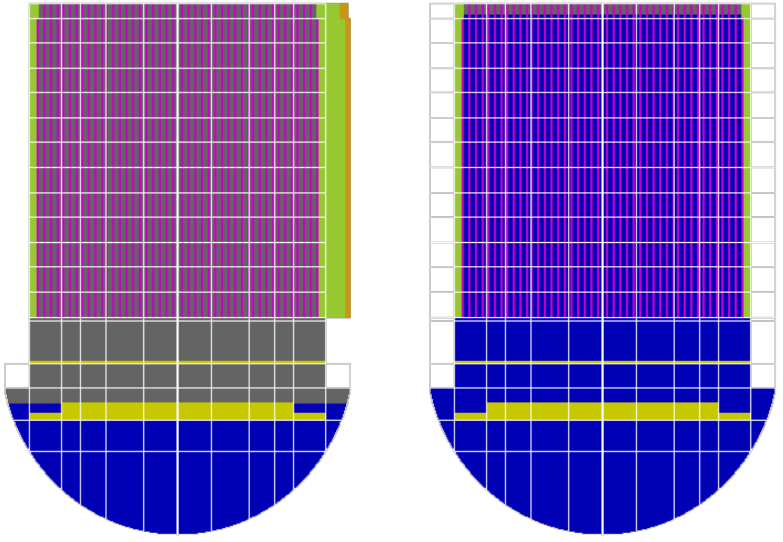


- Start SG cooldown at 1hr to 160 psia
- Fill all 4 SGs using TDAFW
- DC batteries fail at 8 hours and MCR control of TDAFW
- SG dryout at 18hr 45min
- Pressurizer SV opens at 19hr 37 min
- HL failure at 24hr 40min

Reference LTSBO

median UA values



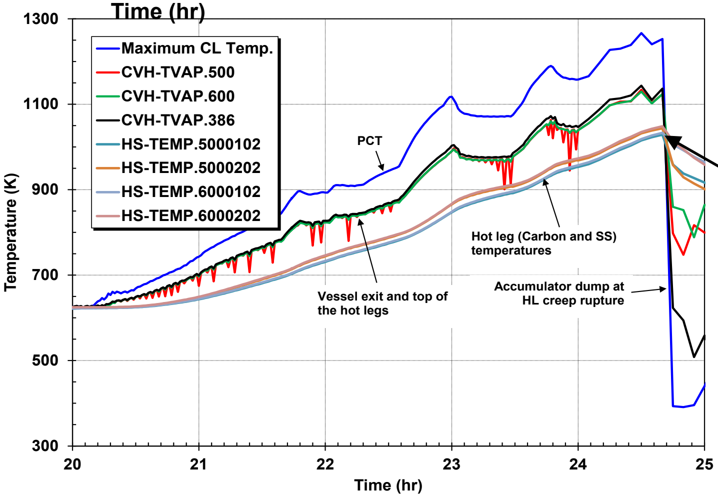


24 hr 40 min
Complete core uncover prior to HL creep rupture failure

24 hr 42 min
Core reflood following HL creep rupture failure

🟪 Intact fuel (UO2) and cladding
🟩 Particulate debris
🟩🟧 Core former and shroud
🟥🟥 Molten pool
▨ Steam and/or hydrogen (no structure)
🟨 Steel support structure
⬛ Control material
🟦 Water

- HL failure at 24hr 40 min
- Core intact
- Quench by remaining accumulator water



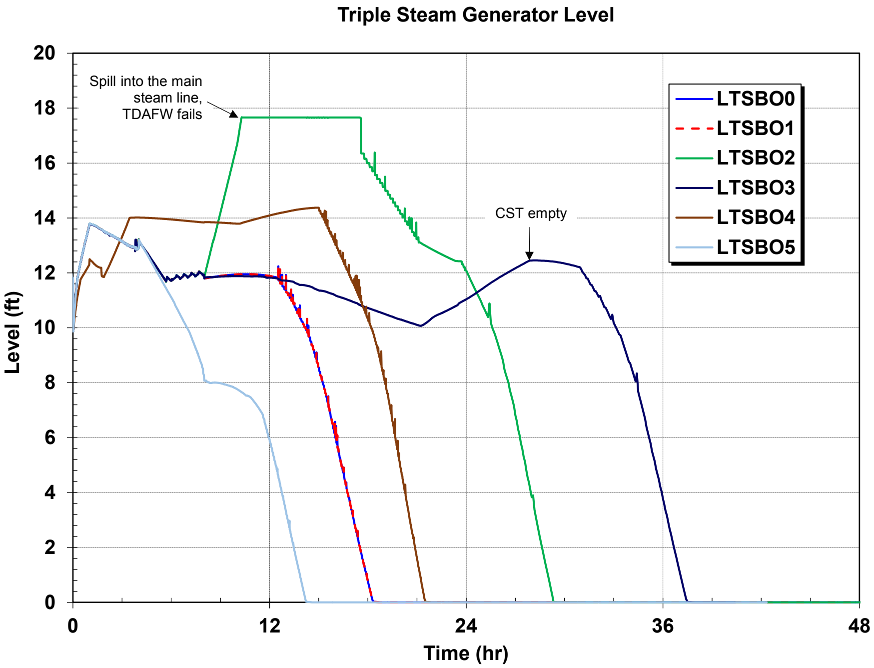
T_{HL} = 1050 K

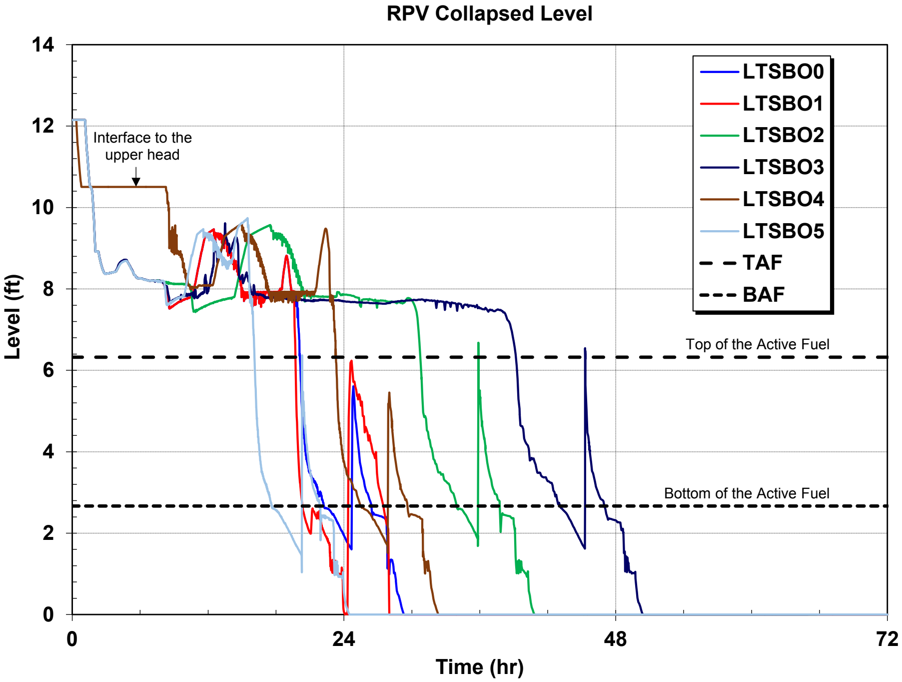
ΔP_{HL}=16 MPa

m_{h2} = 38 kg

LTSBO Sensitivity Cases

Case	Description
LTSBO-0	All uncertain variables at the median value, TDAFW over-speeds upon the loss of DC power.
LTSBO-1	Same as LTSBO-0 but the pressurizer SV sticks fully open on first cycle
LTSBO-2	Same as LTSBO-0 but TDAFW runs at rated after the loss of DC power until SG is flooded.
LTSBO-3	Same as LTSBO-0 but TDAFW runs until the CST is empty.
LTSBO-4	Same as LTSBO-0 but SG1 SV sticks fully open on first cycle
LTSBO-5	Same as LTSBO-0 but station batteries run out at 4 hours

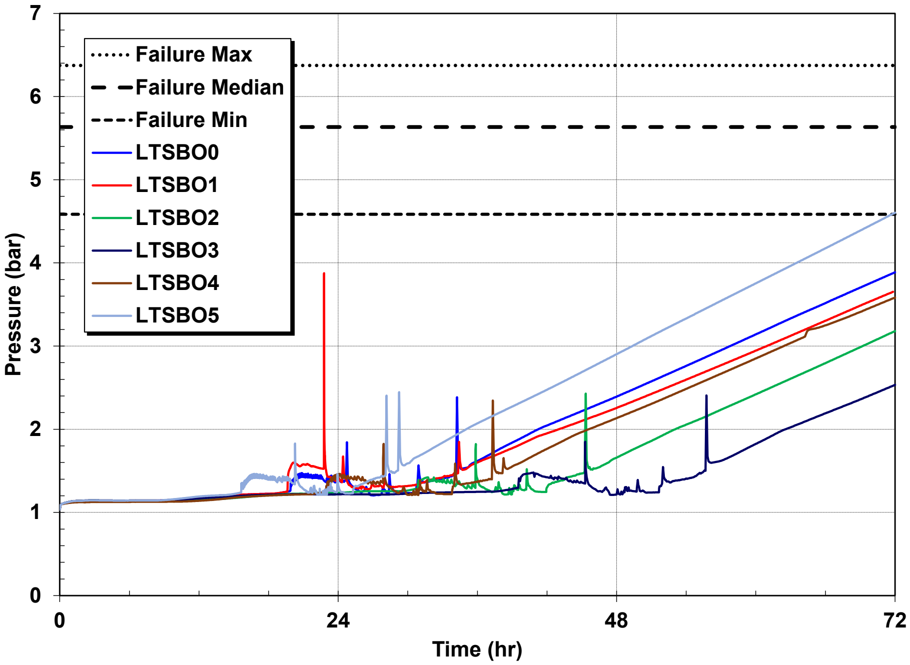




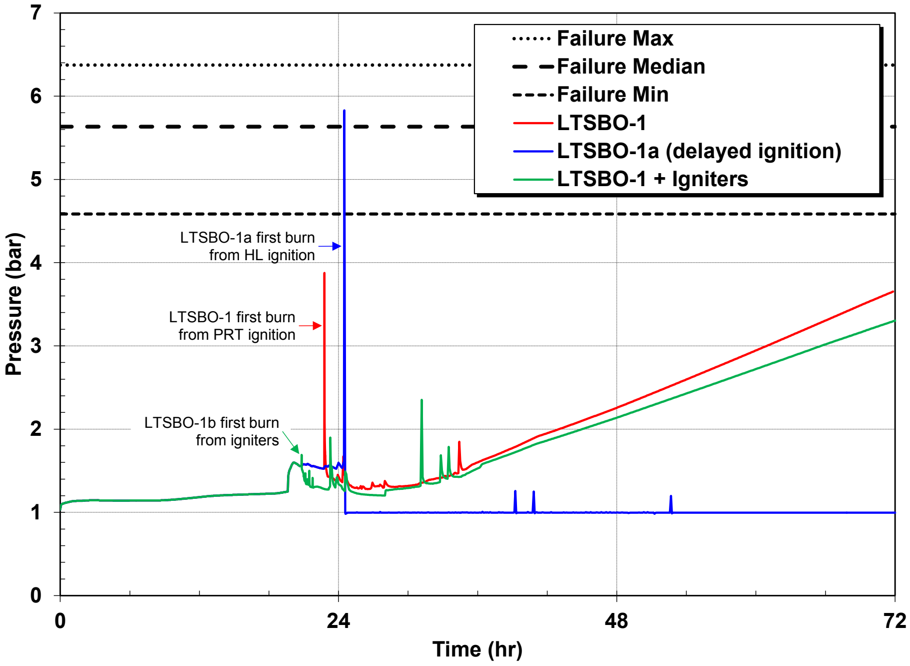
- LTSBO-5 fastest to core damage (19.7hr)
- LTSBO-0 core damage at 23.7hr (median values)
- LTSBO-3 slowest to core damage (44.7hr)

LTSBO Sensitivity Cases

Case	Description
LTSBO-0	All uncertain variables at the median value, TDAFW over-speeds upon the loss of DC power.
LTSBO-1	Same as LTSBO-0 but the pressurizer SV sticks fully open on first cycle
LTSBO-1a	Same as LTSBO-1 but no PRT ignition
LTSBO-2	Same as LTSBO-0 but TDAFW runs at rated after the loss of DC power until SG is flooded.
LTSBO-3	Same as LTSBO-0 but TDAFW runs until the CST is empty.
LTSBO-4	Same as LTSBO-0 but SG1 SV sticks fully open on first cycle
LTSBO-5	Same as LTSBO-0 but station batteries run out at 4 hours



- Only LTSBO-5 (DC=4 hr) approaches containment failure <72 hr



- Igniters mitigate the most severe burn cases

Offsite Consequence Analysis - Presentation Topics

- Sequoyah MACCS Model and Updates
- STSBO Uncertainty Analysis
- STSBO Reference and Sensitivity Cases
- Summary

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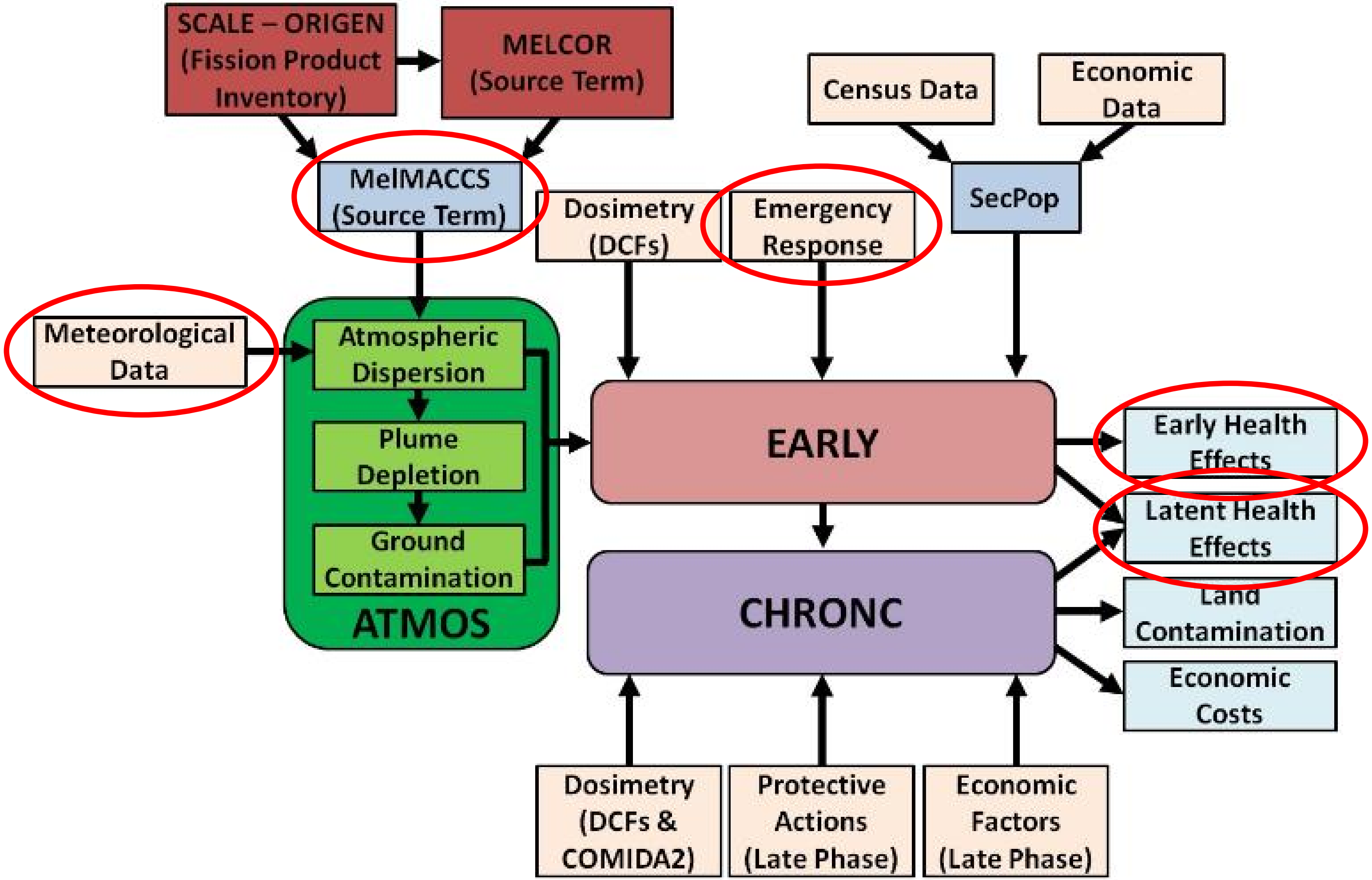
MACCS Model for Sequoyah

MACCS Overview

MACCS estimates offsite consequences of a hypothetical release of radioactive material into the environment



Phases Used in Sequoyah SOARCA

	Emergency Phase	Intermediate Phase	Long-Term Phase
Objectives and Actions	• Reduce public exposures to releases −Sheltering −Evacuation −Relocation	• Protect public by continued relocation • Plan for long-term cleanup and recovery	• Conduct long-term cleanup and recovery −Decontamination −Interdiction (implies relocation) −Condemnation
Duration	1 week	1 year	50 years
Exposure Pathways	• Inhalation • Groundshine • Cloudshine • Skin Deposition	• Groundshine • Inhalation	• Groundshine • Inhalation

Model Updates

- Redefined cohorts
- Modified evacuation parameters (delays and speeds)
- Updated shielding factors
- 2015 site economic values projected from 2012 data
- Eliminated meander factor in crosswind dispersion

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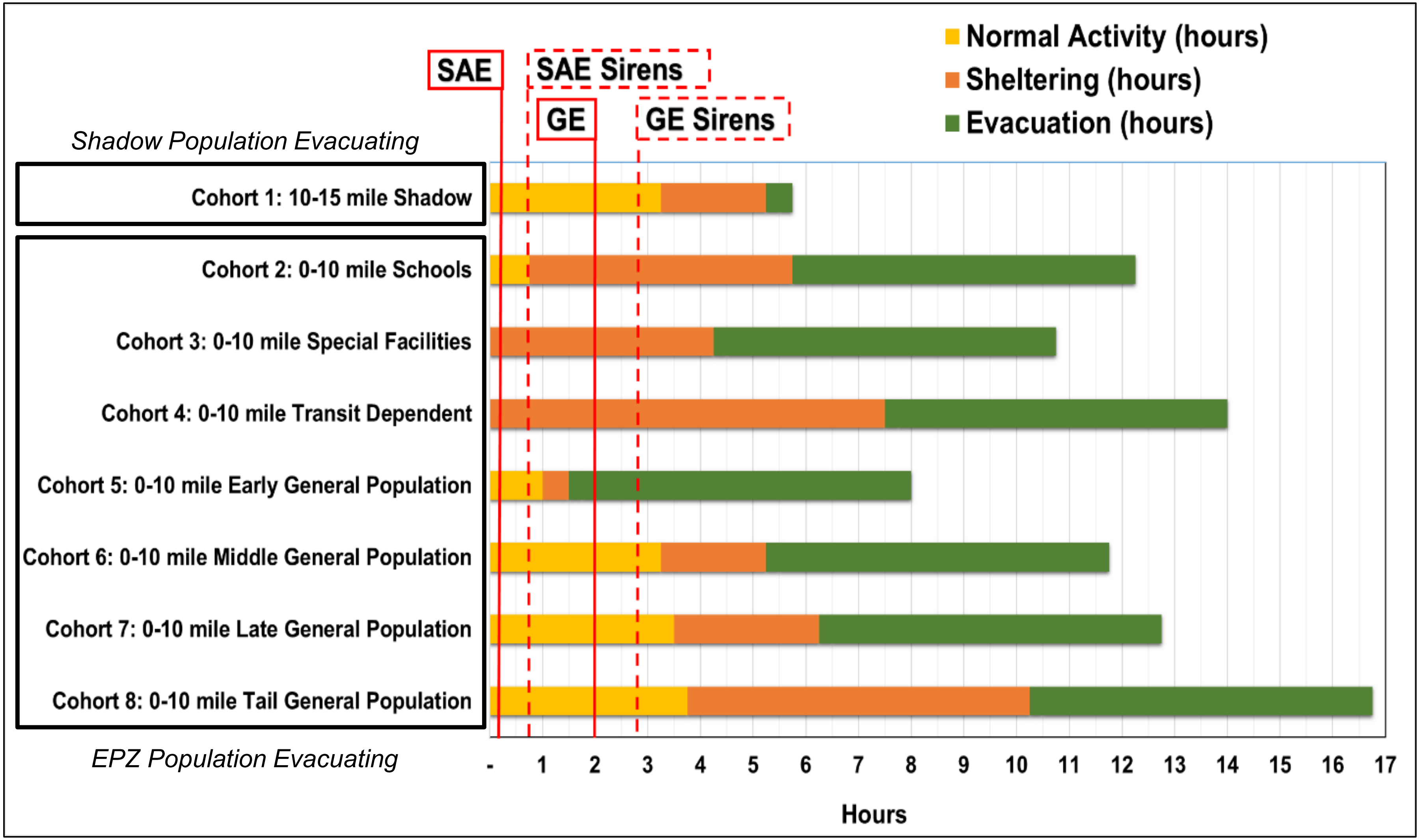
Emergency Response Timeline



Sequoyah STSBO: MACCS Uncertain Parameter Groups

Deposition

- Wet Deposition
- Dry Deposition Velocities

Dispersion

- Crosswind Dispersion Linear Coefficient
- Vertical Dispersion Linear Coefficient
- Time-Based Crosswind Dispersion Coefficient

Latent Health Effects

- Dose and Dose Rate Effectiveness Factor
- Lifetime Cancer Fatality Risk Factors
- Long Term Inhalation Dose Coefficients

Early Health Effects

- Threshold Dose
- Lethal Dose to 50% of population
- Hazard Function Shape Factor

Shielding Factors

- ***Groundshine Shielding Factors****
- ***Inhalation Protection Factors****

Emergency Response

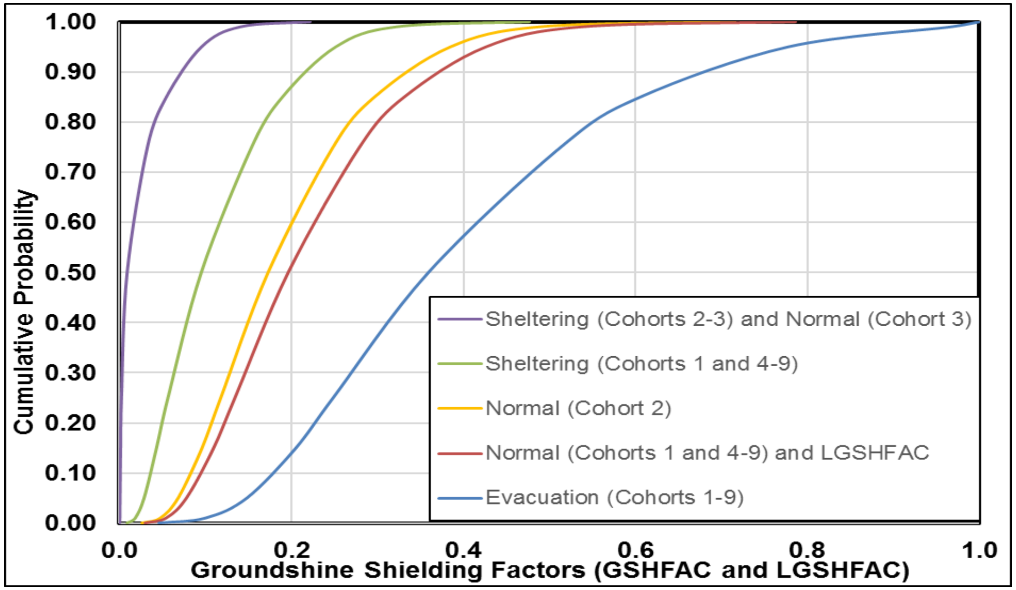
- ***Evacuation Delay****
- ***Evacuation Speed****
- Hotspot Relocation Time and Dose Criteria
- Normal Relocation Time and Dose Criteria
- Keyhole Forecast Time

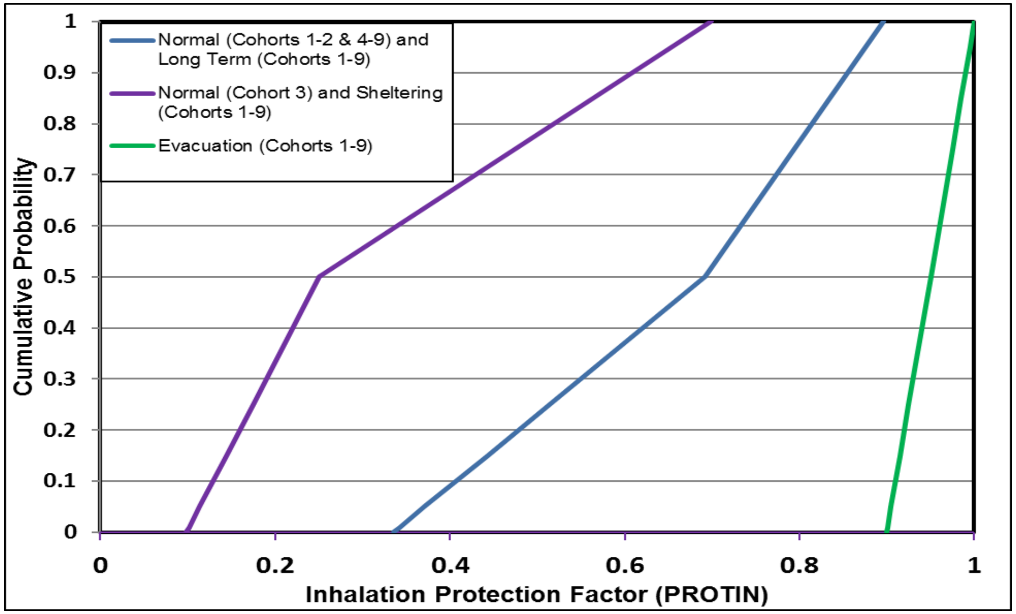
Aleatory Uncertainty

- Weather Trials

*Blue text indicates parameters updated from earlier draft Sequoyah SOARCA report (ML16096A374)

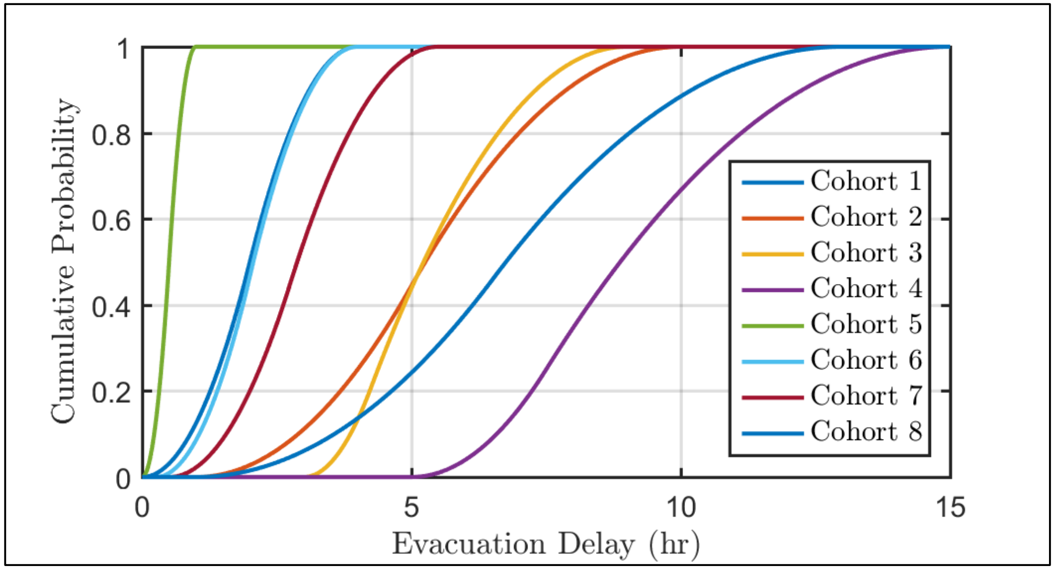
Model Updates: Shielding Factors Distributions

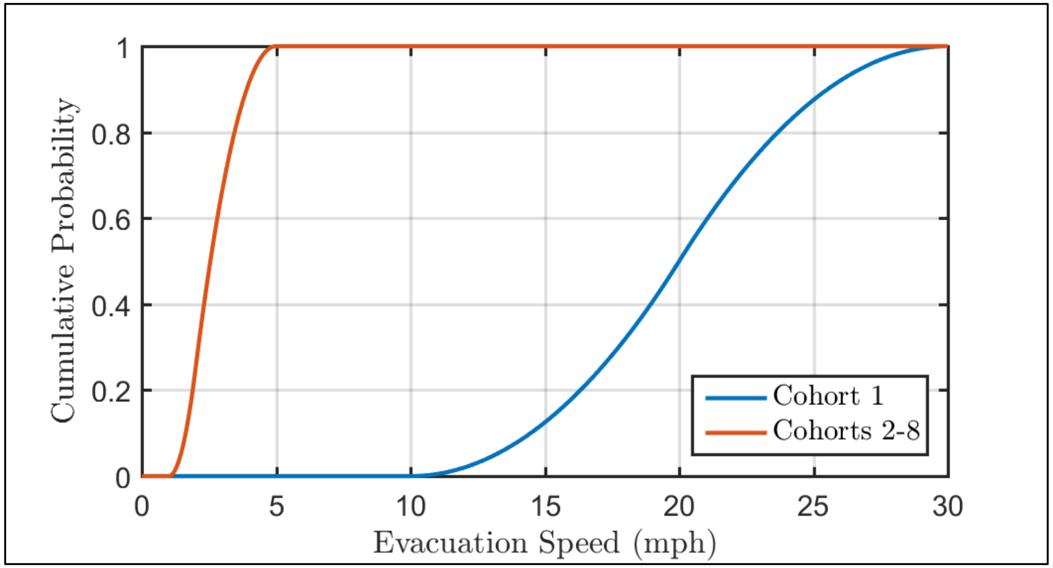




- Shielding factor point estimates
  - Originally from NUREG-1150
  - Now medians from distributions
- New distributions were derived for groundshine shielding factors
  - Originally combination of NUREG-1150 and Gregory et al.
  - Now taken directly from Gregory, *et al*., for most cohorts
  - Distributions for special facilities and schools based on high-shielding building data
- Inhalation protection factor distributions same as original

Model Updates: Evacuation Delay and Speed Distributions

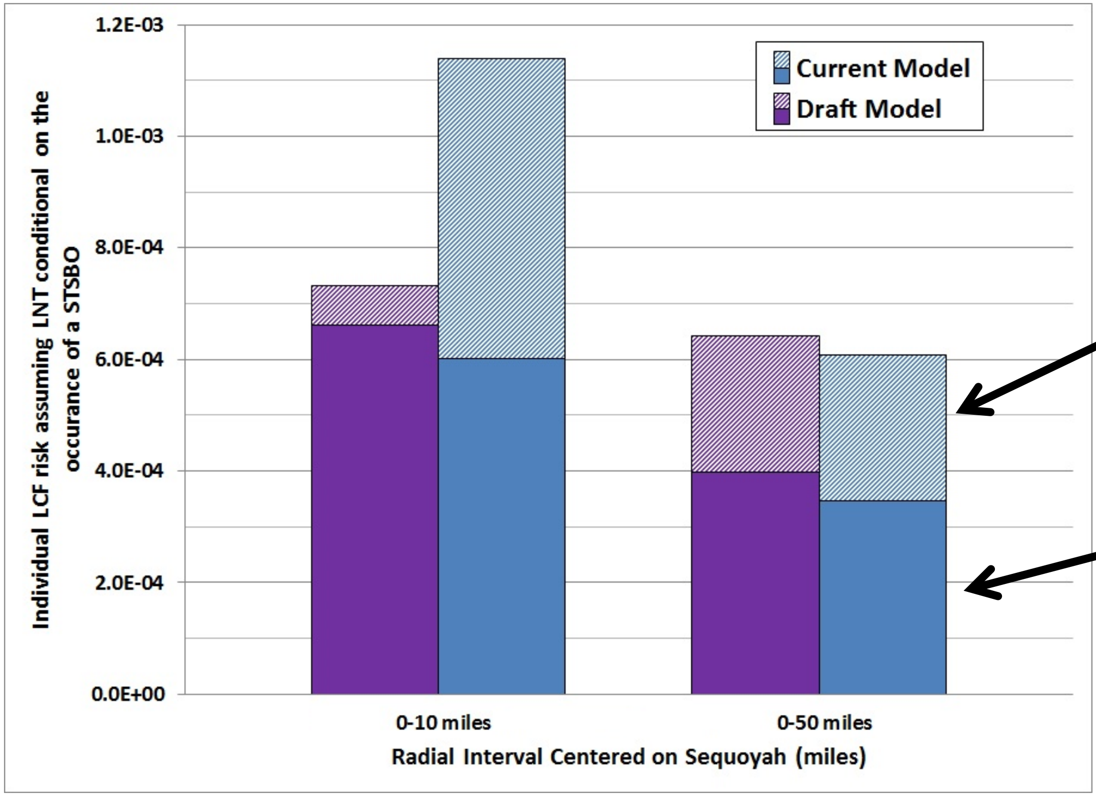




- New cohort definitions necessitated reevaluation of delay and speed distributions
- Evacuation delay and speed modeled as triangular distribution with new upper and lower bounds
- Cohort 1 (shadow evacuees) chosen to have a higher evacuation speed
  - Little traffic congestion outside EPZ

Example Comparison of Current Model and Draft Model Results

MELCOR Realization	Scenario	Time in Cycle	Release Fraction		Released Activity (Bq)		Time (hr)	
			Cs	I	Cs-137	I-131	Start*	Increase**
554	STSBO Earliest Release	EOC	0.018	0.051	7.3E+15	1.6E+17	2.7	3.6



Emergency Phase

Intermediate and Long-Term Phase

*The “start” time indicates the timing of the first environmental release, no matter how small.

**The “increase” time indicates the timing of the first significant increase in the rate of release.

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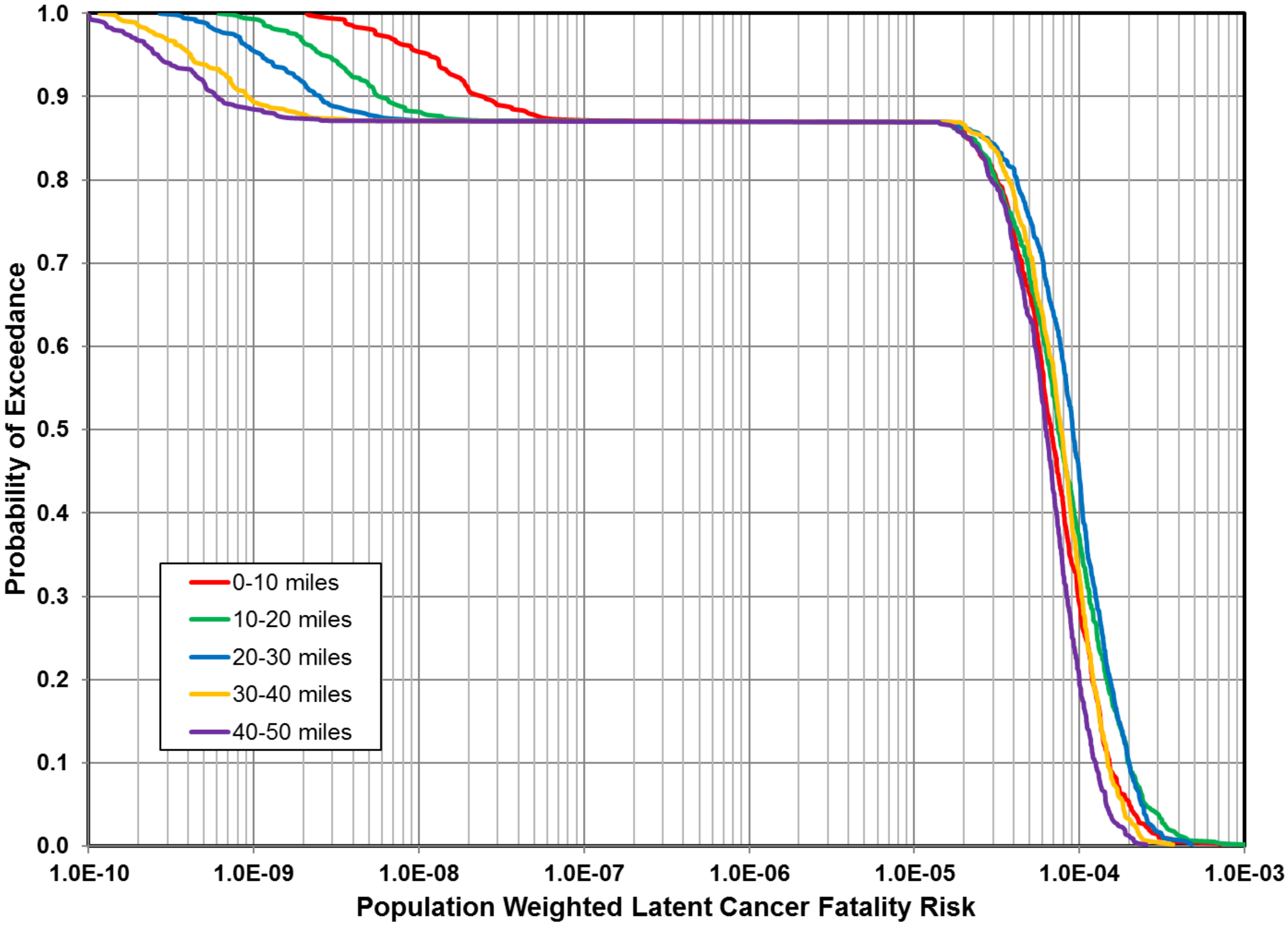
MACCS STSBO Uncertainty Analysis

Mean (over weather variation) individual LCF risk conditional on the STSBO accident occurring (per event)

	0-10 Miles	10-20 Miles	20-30 Miles	30-40 Miles	40-50 Miles	0-50 Miles
Mean	8.0E-05	9.7E-05	1.0E-04	8.2E-05	6.6E-05	8.8E-05
Median	6.7E-05	7.5E-05	9.1E-05	7.8E-05	6.2E-05	8.1E-05
5th Percentile	1.2E-08	2.7E-09	1.1E-09	4.2E-10	2.6E-10	2.3E-09
95th Percentile	2.0E-04	2.5E-04	2.4E-04	1.8E-04	1.4E-04	2.1E-04

Complementary Cumulative Distribution Function (CCDF) of Mean (over weather variation) individual LCF risk conditional on the STSBO accident occurring (per event)



Mean, Individual, LCF Risk Regression Results within 0 – 10 mile and 0 – 50 mile for STSBO Based on LNT

0 – 10 Mile	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contribution	Conjoint Contribution
Final R^2	0.67		0.86		0.58		0.78			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
Cycle	0.36	0.58	0.23	0.29	0.40	0.60	0.20	0.20	0.237	0.056
priSVcyc	---	---	0.04	0.15	0.12	0.15	0.14	0.31	0.070	0.083
CFRISK(8)	0.09	0.29	0.07	0.12	0.08	0.23	0.10	0.09	0.068	0.042
Rupture	0.06	-0.24	0.06	0.08	0.07	0.18	0.09	0.15	0.054	0.046
CFRISK(7)	0.03	0.19	0.06	0.10	0.05	0.11	0.08	0.10	0.040	0.031
GSHFAC_6(2)	0.05	0.22	0.02	0.06	0.01	0.05	0.04	0.03	0.026	0.021
CFRISK(6)	0.01	0.09	0.04	0.11	---	---	0.04	0.07	0.018	0.029
CFRISK(3)	0.02	0.11	---	---	0.00	0.01	0.03	0.10	0.011	0.018
DDREFA(8)	0.01	-0.11	0.03	0.04	---	---	---	---	0.010	0.002

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

0 – 50 Mile	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contribution	Conjoint Contribution
Final R^2	0.59		0.86		0.65		0.75			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
Cycle	0.23	0.52	0.24	0.31	0.36	0.44	0.21	0.21	0.208	0.038
CFRISK(8)	0.06	0.24	0.09	0.13	0.05	0.14	0.09	0.08	0.059	0.029
Rupture	0.05	-0.21	0.06	0.10	0.05	0.22	0.10	0.25	0.052	0.086
CFRISK(4)	0.05	0.23	0.07	0.10	0.04	0.15	0.08	0.09	0.048	0.037
CFRISK(7)	0.04	0.22	0.05	0.07	0.02	0.10	0.08	0.11	0.040	0.028
TIMNRM	0.04	0.22	0.04	0.07	0.06	0.30	0.05	0.06	0.038	0.061
CYSIGA(1)	0.03	0.19	0.03	0.04	0.01	0.05	---	---	0.015	0.013
DDREFA(4)	0.02	-0.13	0.02	0.02	0.00	0.04	0.02	0.02	0.013	0.011
CFRISK(6)	0.01	0.08	0.03	0.12	---	---	0.02	0.08	0.012	0.042

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Mean (over weather variation) individual early fatality risk , conditional on accident occuring (per event)

	0 - 1 miles	0 - 1.3 miles	0 - 2 miles
Mean	3.0E-09	1.8E-09	8.6E-10
Median	0.0E+00	0.0E+00	0.0E+00
5th percentile	0.0E+00	0.0E+00	0.0E+00
95th percentile	0.0E+00	0.0E+00	0.0E+00

- Nonzero early fatality risk within 1 mile for three realizations
- No early fatality risk beyond 2 miles for any realization
- Only 3 realizations out of 567 resulted in non-zero early fatalities

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Reference and Sensitivity Cases

Reference and Sensitivity Cases

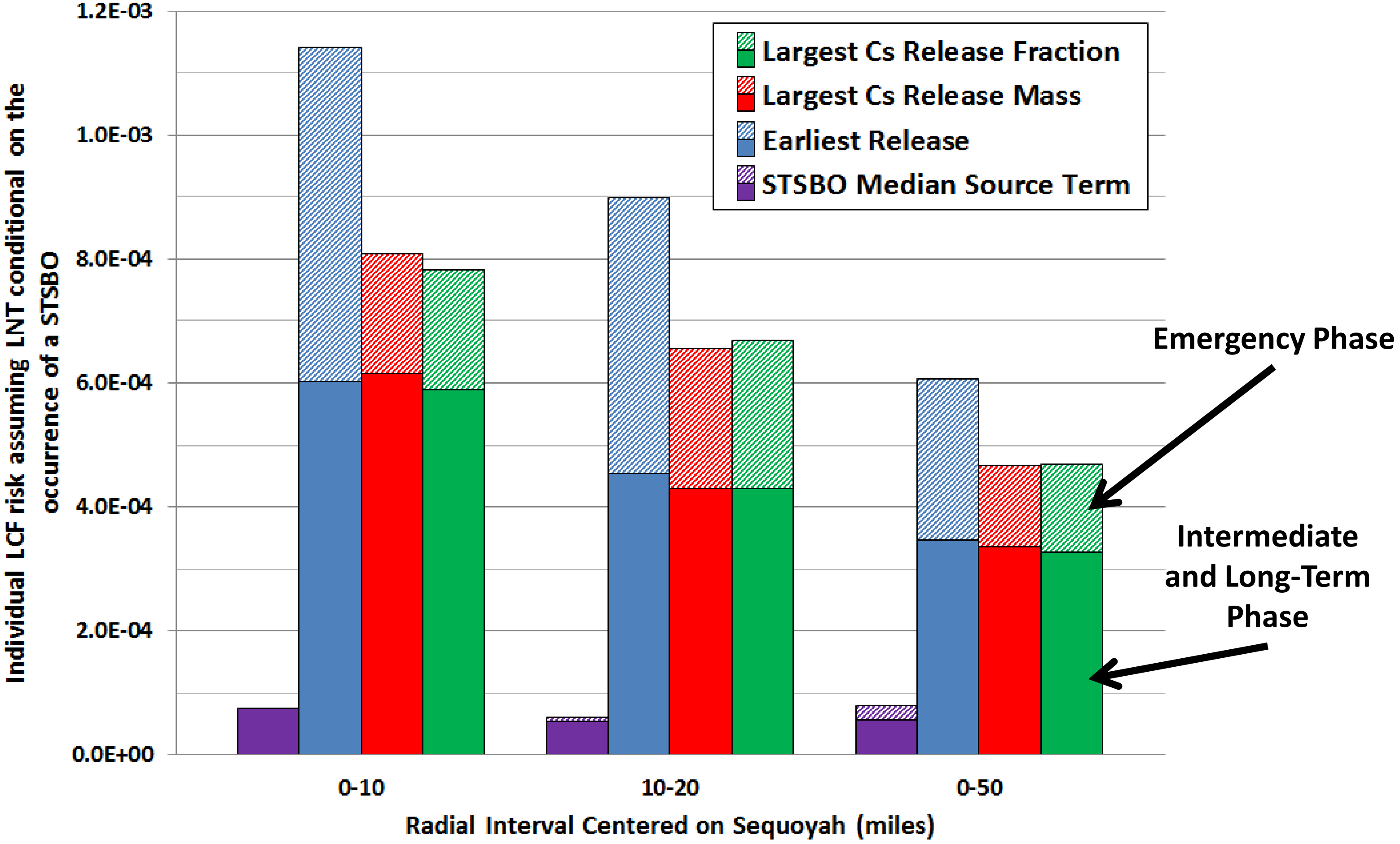
- All cases assume seismic impact on evacuation network
- Reference case
  - Nominal shielding parameters
  - Evacuation order
  - Basis for uncertainty analysis
- Sensitivity cases
  - Shelter-in-place (SIP) while offsite response organization evaluates infrastructure
  - Shielding changes due to seismically degraded buildings
  - Weather year
  - Non-LNT dose response

Source Terms for Reference and Sensitivity Cases

MELCOR Realization	Scenario	Time in Cycle	Release Fraction		Released Activity (Bq)		Time (hr)	
			Cs	I	Cs-137	I-131	Start*	Increase**
266	STSBO Reference	MOC	0.001	0.004	1.4E+14	1.1E+16	3.4	57.6
554	STSBO Earliest Release	EOC	0.018	0.051	7.3E+15	1.6E+17	2.7	3.6
395	STSBO Highest Cs Release Mass	EOC	0.027	0.079	1.1E+16	2.6E+17	2.9	6.9
36	STSBO Highest Cs Release Fraction	MOC	0.036	0.107	9.7E+15	3.4E+17	3	7
146	Large Release at 56 hours	MOC	0.029	0.089	7.9E+15	2.4E+17	2.6	55.6
382	Release at 40 hours	EOC	0.001	0.008	4.0E+14	2.3E+16	3.3	40.3

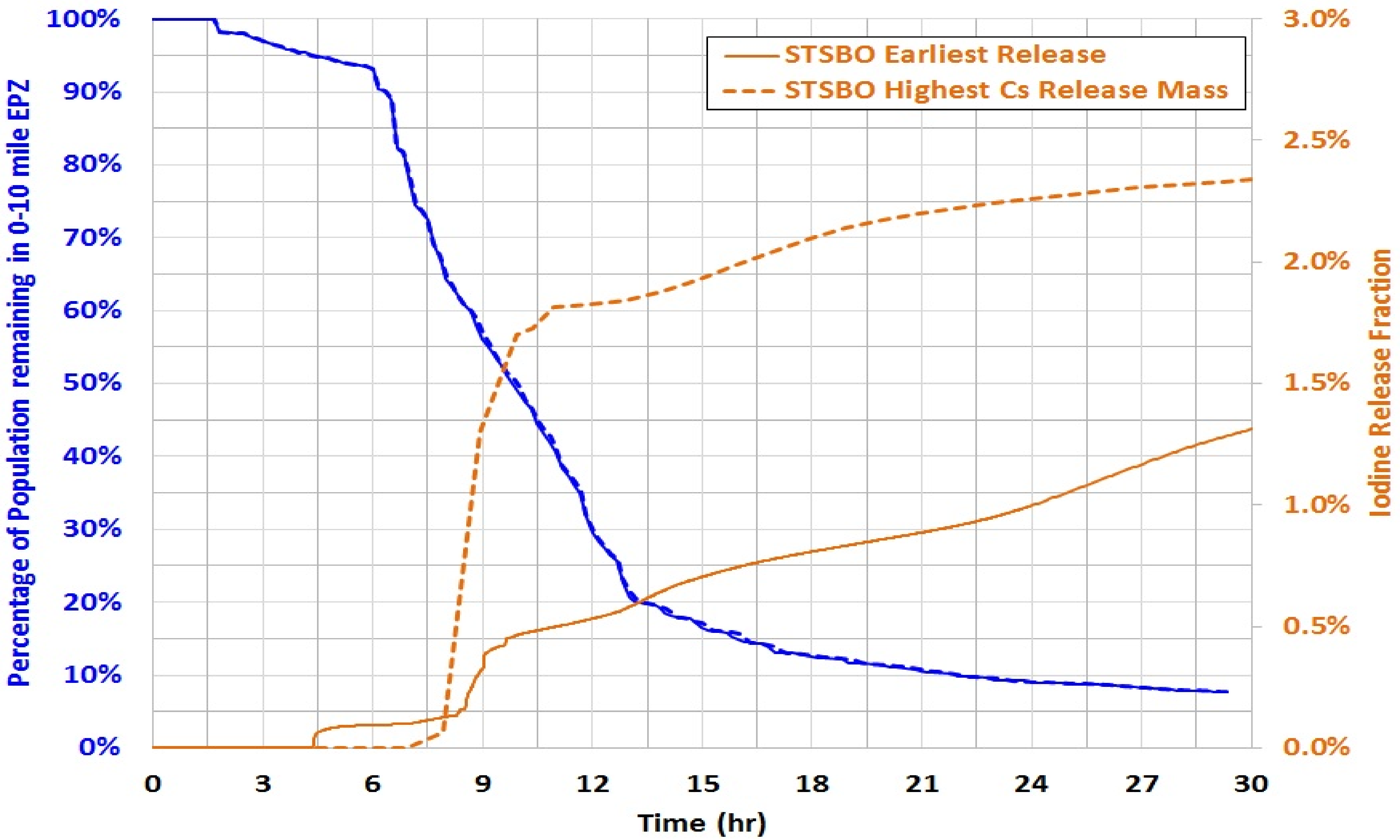
*The “start” time indicates the timing of the first environmental release, no matter how small.
**The “increase” time indicates the timing of the first significant increase in the rate of release.

Individual LCF Risk Results

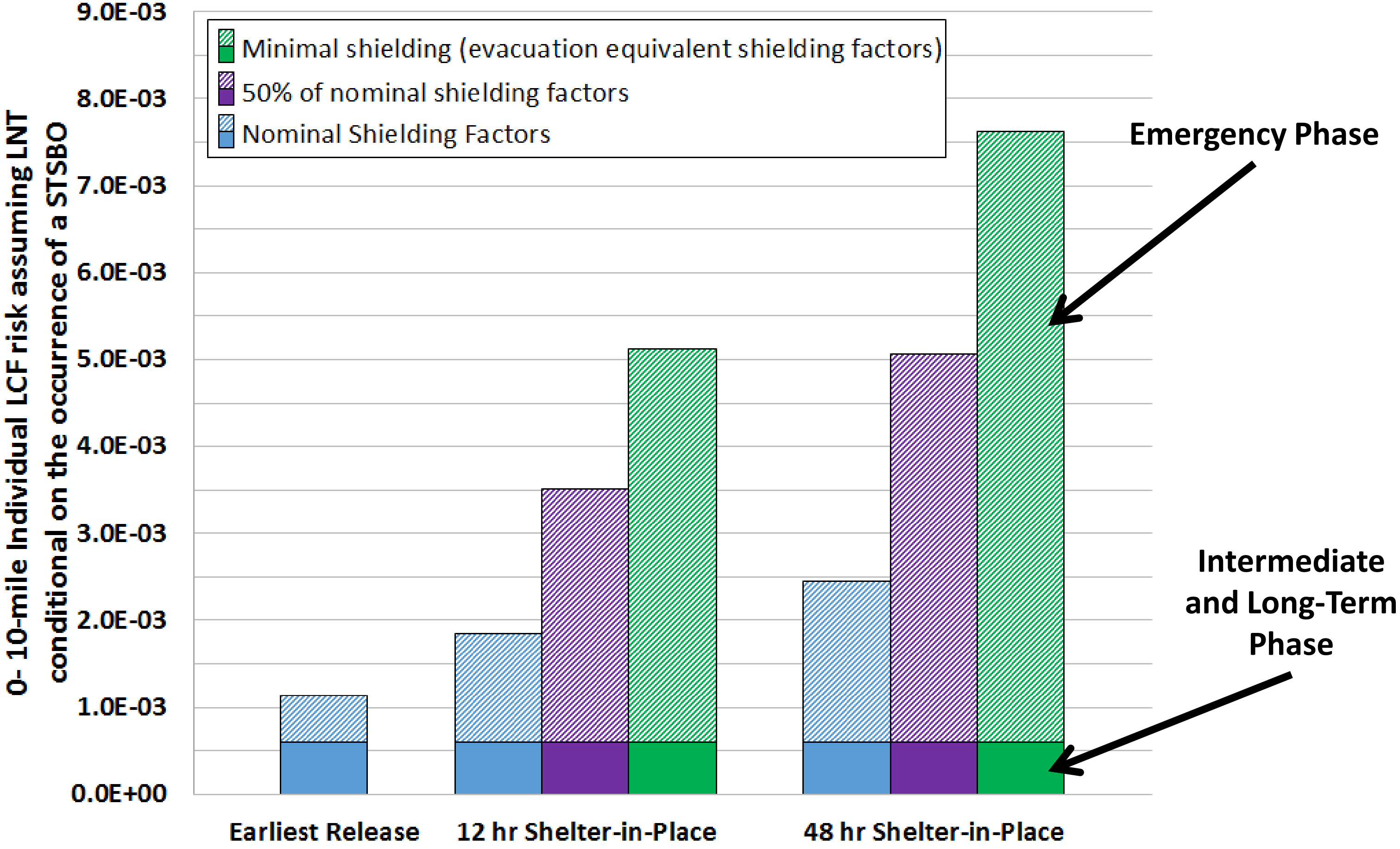


- Conditional individual LCF risks generally decrease at longer distances

Population Movement Compared to Early Releases

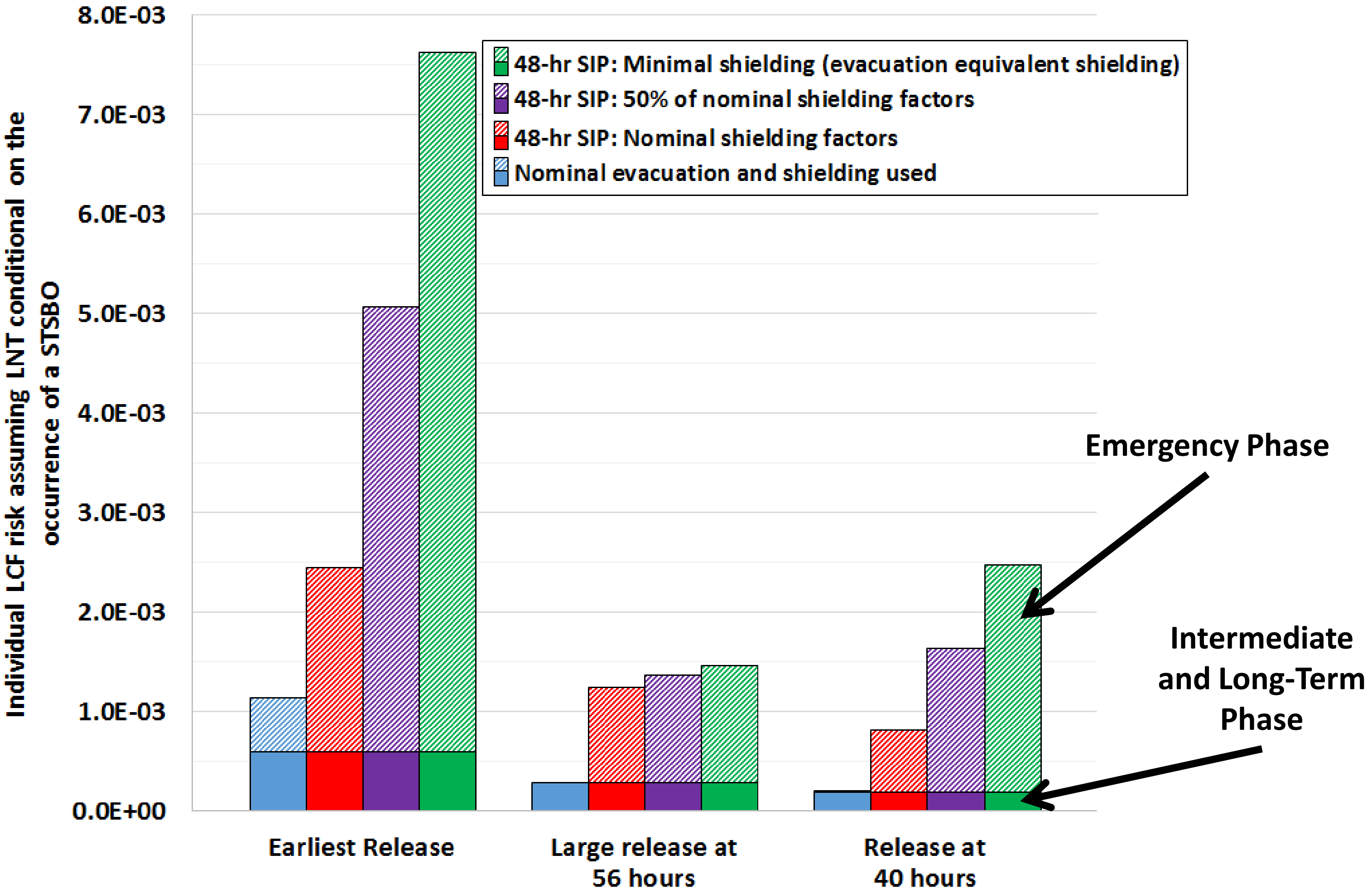


Sensitivity 1 and 2: SIP and shielding factor due to seismically degraded buildings (0-10 miles)



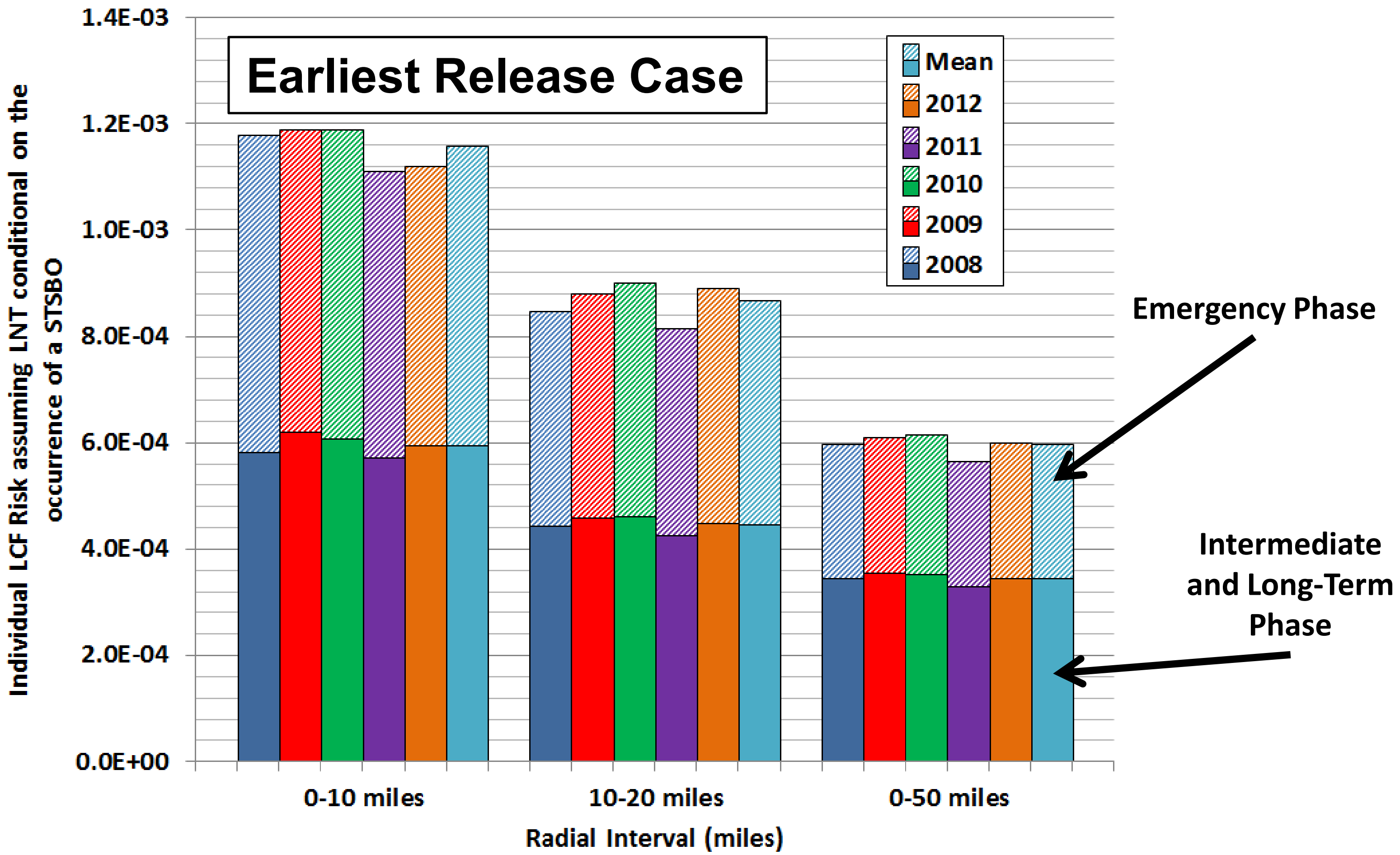
- Conditional individual LCF risks
  - Are roughly 1E-04 in 0- to 10-mile region for late releases
  - Are roughly 1E-03 in 0- to 10-mile region for early releases

Sensitivities 1 and 2: Mean Conditional Individual LNT LCF Risks for SIP and Shielding Factor Variations



- 48-hour shelter-in-place increases 10-mile EPZ risks
  - By factor-of-2+ with nominal shielding factors
  - By factor-of-7+ with fully degraded shielding factors

Sensitivity 3: Impact of Weather Year Risk



Conditional individual LCF risks are nearly independent of weather

Offsite Consequence Analysis Summary

- Individual, conditional LCF risks:
  - Range from about 1E-09 to 1E-03
  - Generally decrease at longer distances
  - Generally dominated by intermediate and long-term phase exposures compared to emergency phase exposures
  - Increase but by less than an order of magnitude for 48-hour shelter-in-place and degraded shielding factors
  - Nearly independent of weather year
  - Decrease with increasing dose-truncation level

- Parameters most important to uncertainty in individual LCF risk:
  - Time of accident during fuel cycle (most important at all distances)
  - Cancer fatality risk factors for “residual” organ, lungs, and colon
  - Containment rupture pressure
  - Number of safety valve cycles prior to failing open (more important at shorter distances)
  - Normal relocation time (more important beyond 10-mile EPZ)
  - Groundshine shielding factors (more important within 10-mile EPZ)

Sequoyah SOARCA Conclusions

- For unmitigated STSBO (without igniters), the two potential containment outcomes are either early or late failure
- Successful use of igniters averts early containment failure
- Essentially zero individual early fatality risk was calculated for Sequoyah STSBO
- Even for cases resulting in early release to environment, the conditional individual LCF risk is small
- Conditional individual latent cancer fatality risk results for Sequoyah are similar in magnitude to those from other SOARCA analyses

Uses of SOARCA* Modeling to Support Agency Activities

Technical Bases for Regulatory Framework

- BWR Mark I filtered vent analysis and CPRR (Tier 3 – 5.1)
- Other containments and hydrogen (Tier 3 – 5.2 and 6)
- Expedited fuel transfer
- Emergency preparedness – decommissioning exemption requests
- Uncertainty analyses determine most influential parameters
- MACCS parameter guidance supports new and advanced reactor designs
- Knowledge management for severe accident analysis

Licensing and Environmental Review Uses of MACCS

- Environmental assessment and impact statement analyses (SAMA/SAMDA)
- Waste Confidence technical bases for spent fuel fires and final statement (FGEIS)
- Hearing support for technical analyses (Indian Point; Seabrook)

Insights for Emergent Issues

- Supported NRC incident response to Fukushima event
- Fukushima Forensic Analysis to better understand Fukushima accident progression

**Cited in 270 publications domestically and internationally*

Next Steps and Schedule

- Submit SECY to Commission – August 31, 2017
- ACRS Full Committee – September 2017
- Updating Surry UA – ongoing, target Q2 FY18

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 expected summer 2017)

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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
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
SIP	Shelter in Place
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

MELCOR and MACCS Parameter Names

priSVcyc – *number of primary safety valve cycles experienced*

rupture – *containment rupture pressure*

EU_melt_T – *effective temperature of the eutectic reaction for zircaloy oxide and uranium oxide*

Cycle – *time within the fuel cycle*

Ox_Model – *Oxidation kinetics model*

ajar – *ice condenser doors open fraction*

shape_fact – *aerosol dynamic shape factor*

CFRISK(8) – *cancer fatality risk factor for "residual" organ*

CFRISK(7) – *cancer fatality risk factor for colon*

CFRISK(4) – *cancer fatality risk factor for lung*

TIMNRM – *normal relocation time*

GSHFAC_6(2) – *groundshine shielding factor for normal activity during emergency phase (fully correlated with same factor during long-term phase)*

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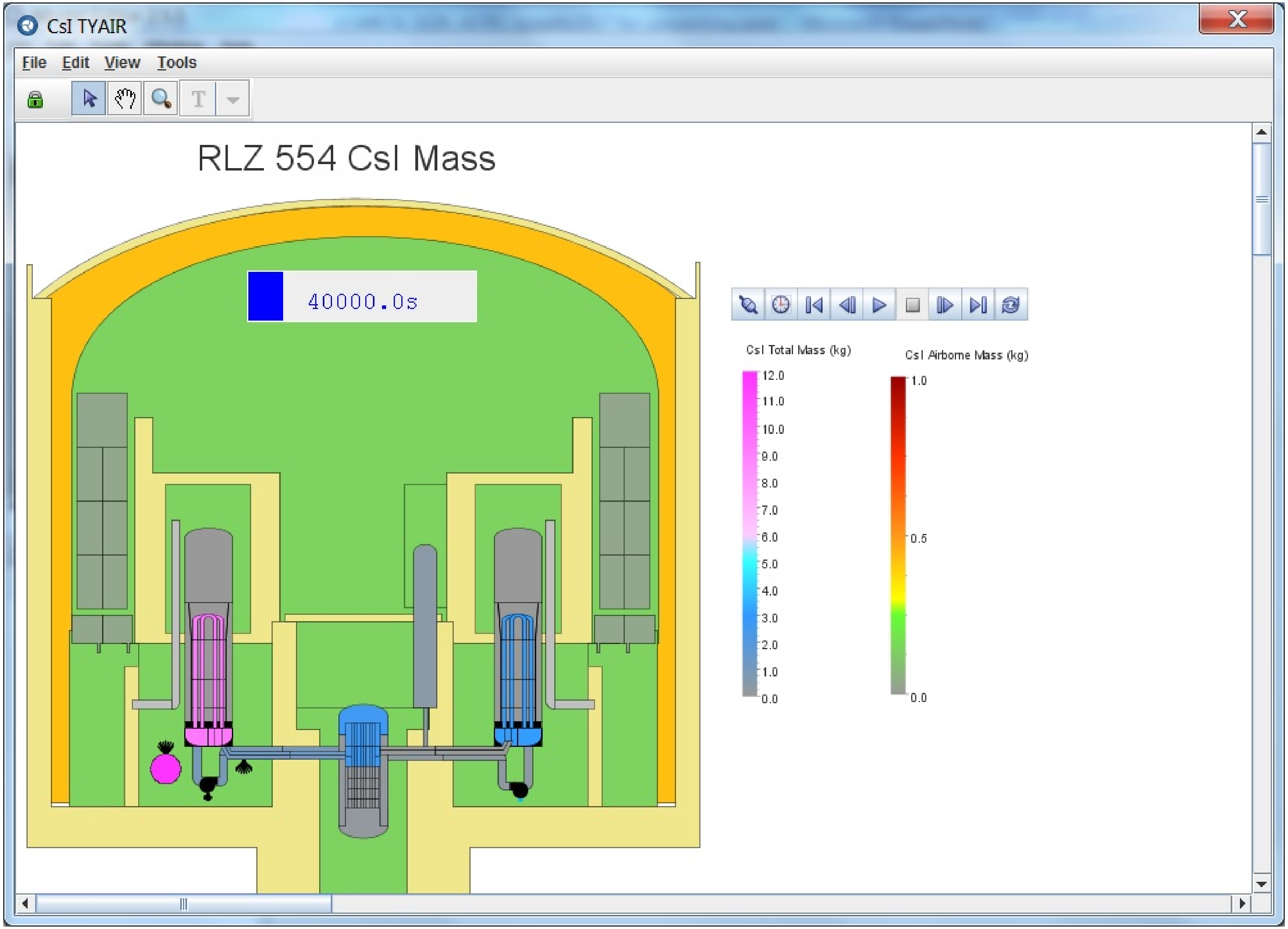
Animation of MELCOR Rlz 554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CsI Total Mass (kg)
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CsI Airborne Mass (kg)
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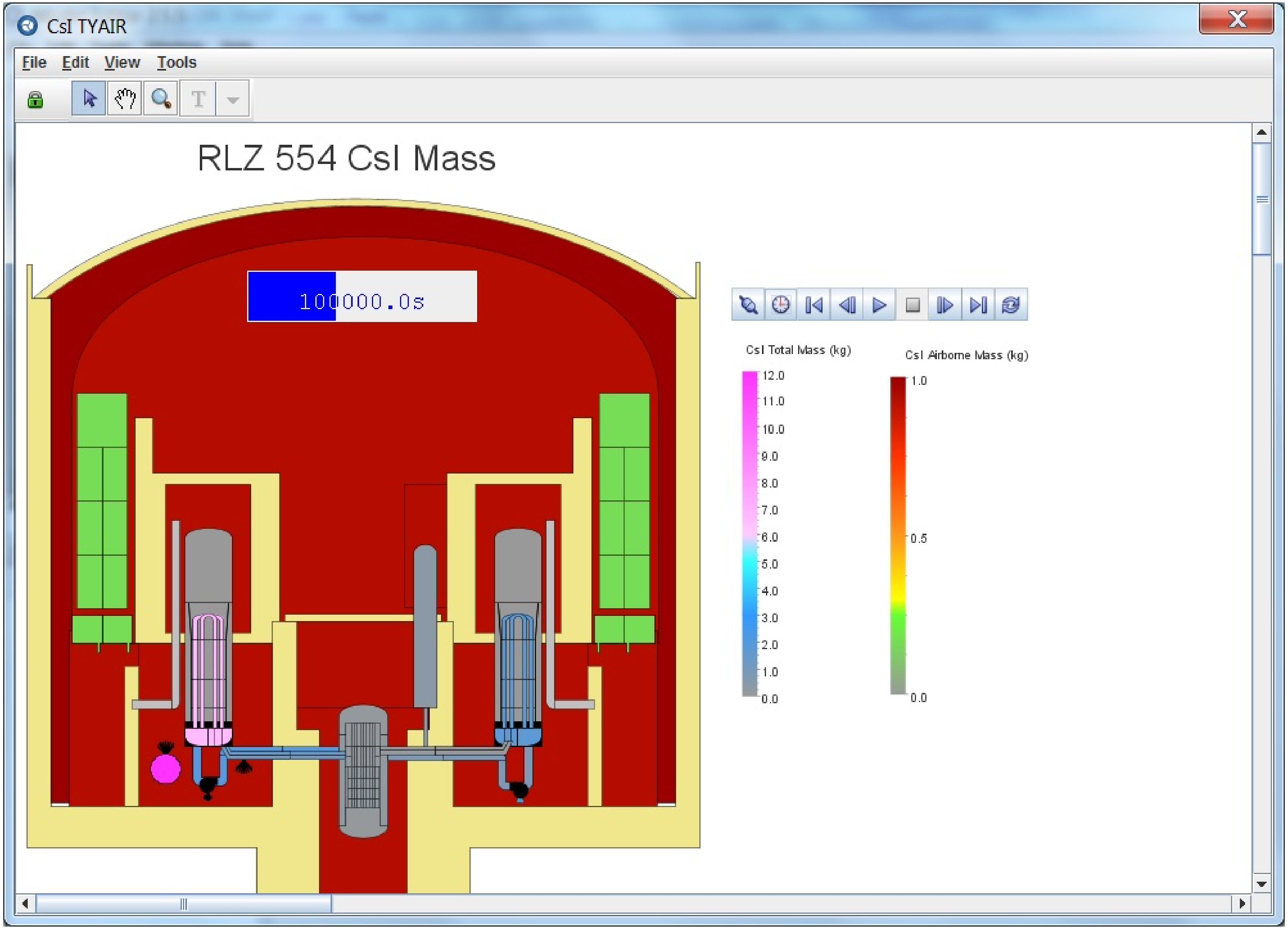
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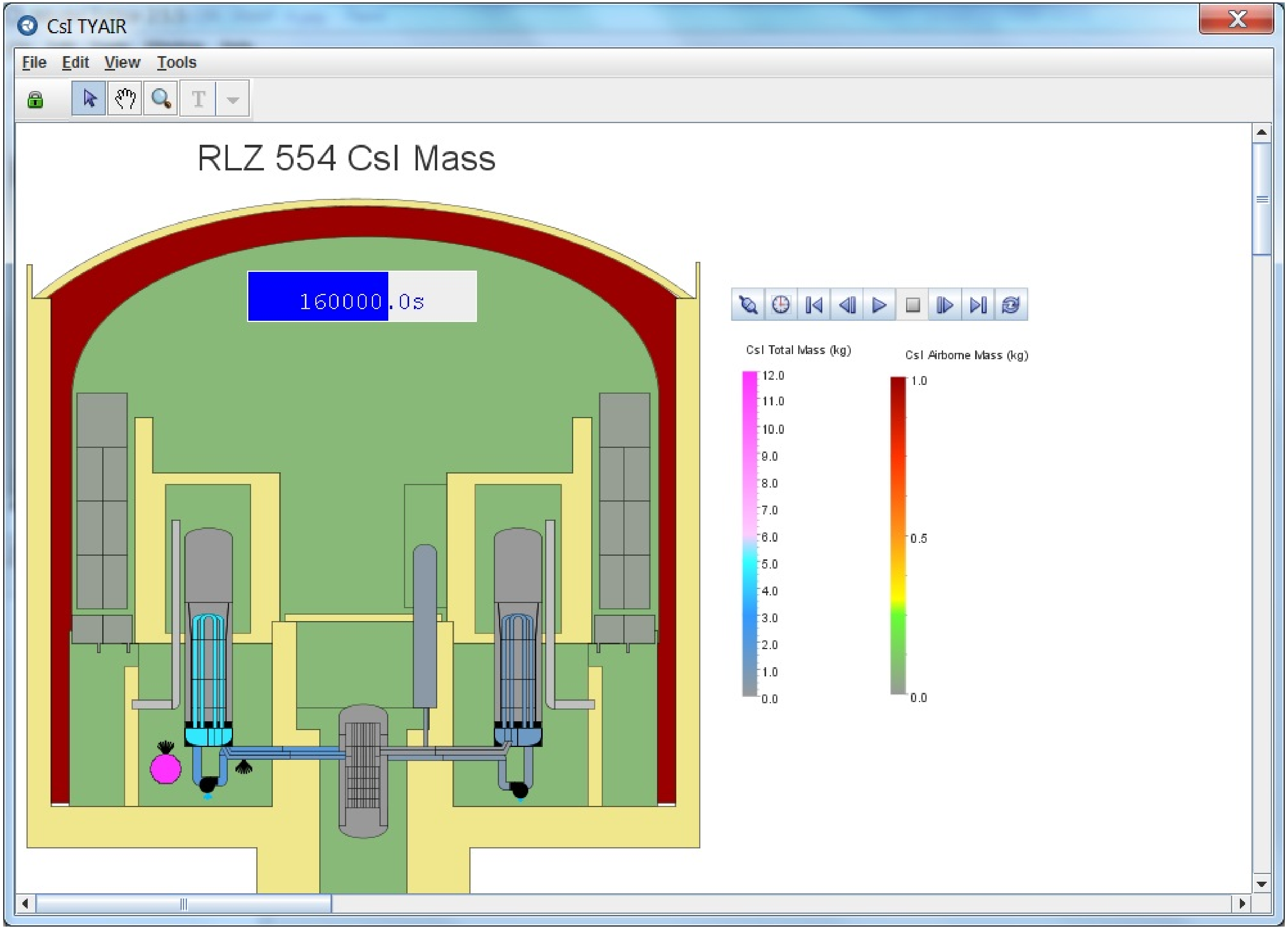

RLZ 554 CsI Mass

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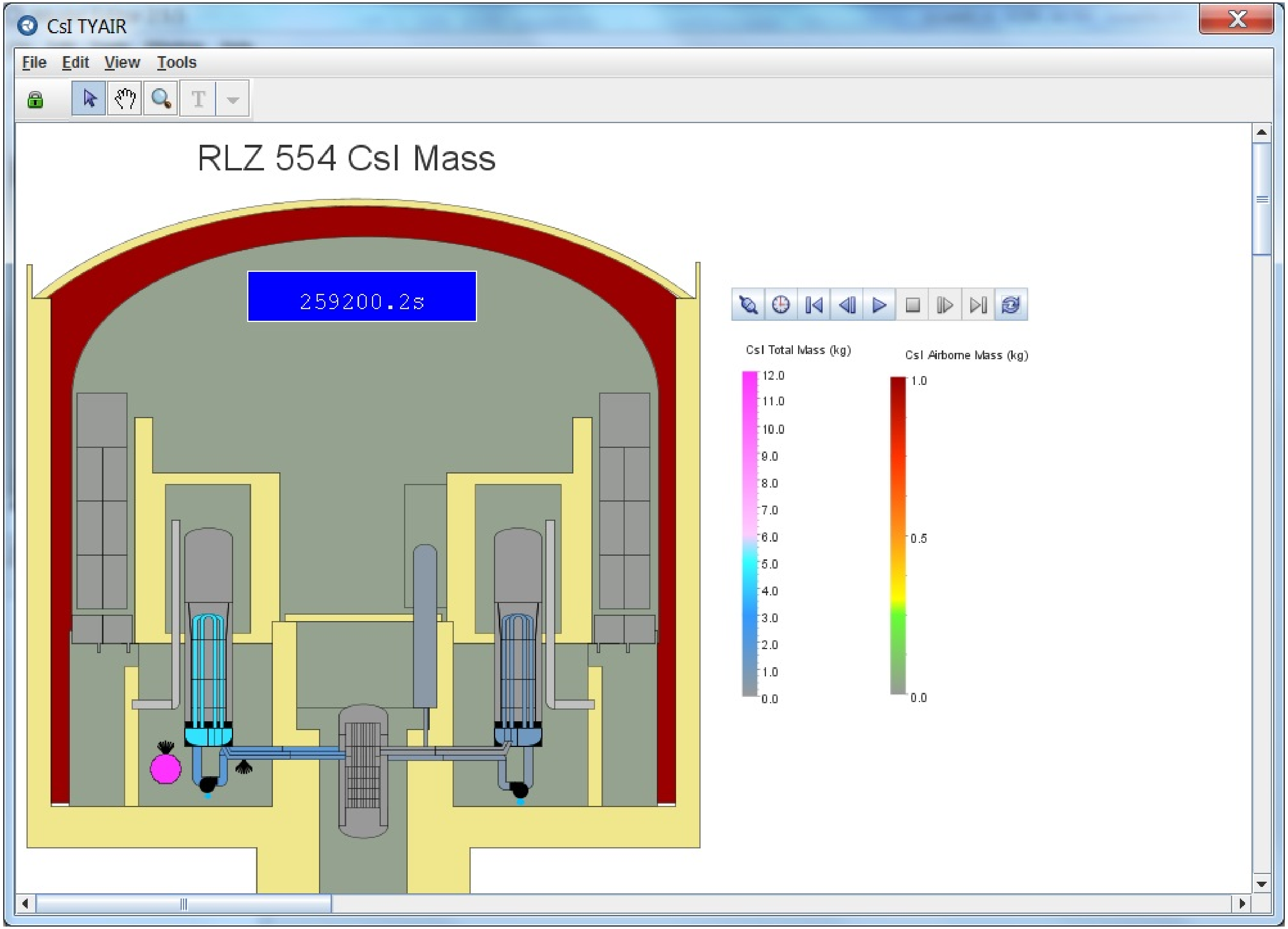

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CsI Airborne Mass (kg)
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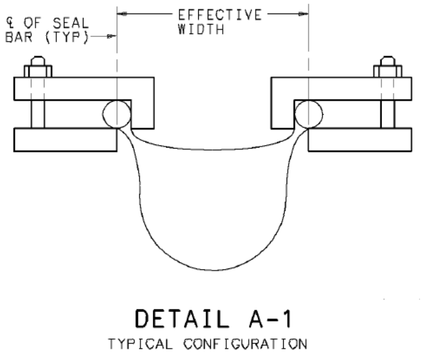
Backup Slide

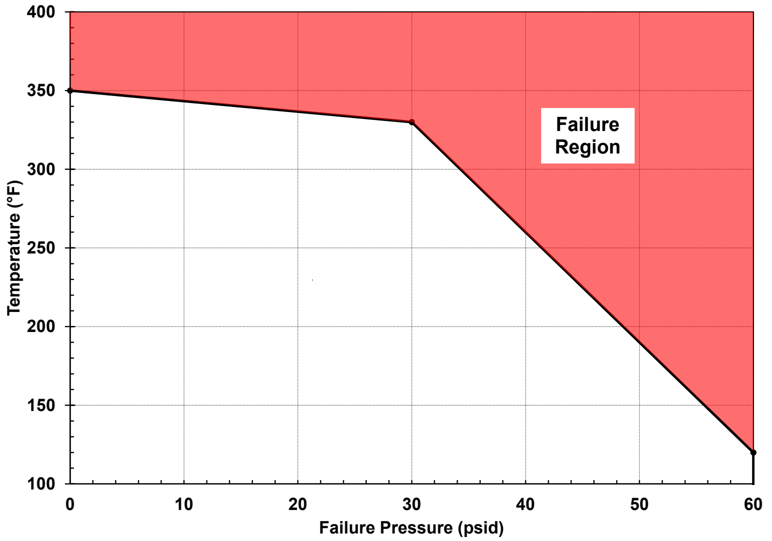
Containment Modeling

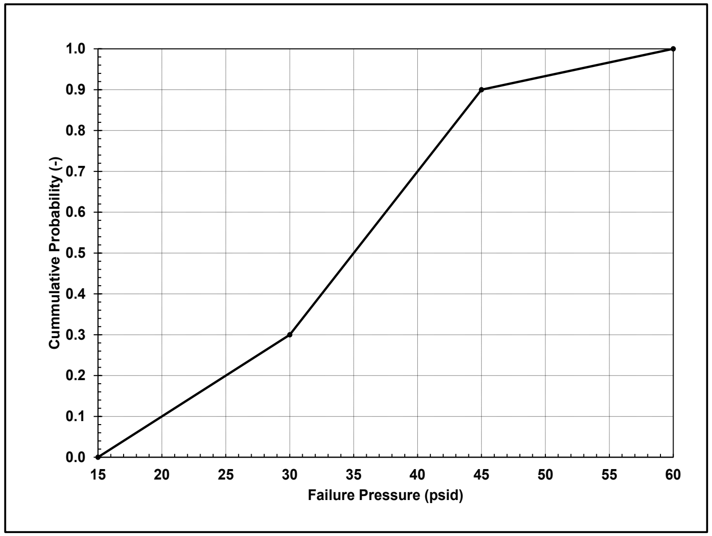
Upper/Lower Containment Barrier Seal

Changes – Failure Criteria

- Two criteria used for failure evaluation
- #1 - Sampled ΔP
- #2 – Specified pressure versus temperature
- Barrier seal fails on sampled ΔP (#1) or pressure-temperature relationship (#2)







- Failure sampling spanned plausible differential pressure range from design and surveillance reports and recognized weakening at high temperature