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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

FEBRUARY 2, 2016

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B1, 11545 Rockville Pike, at 8:35 a.m., John W. Stetkar, Chairman, presiding.

COMMITTEE MEMBERS:

JOHN W. STETKAR, Chairman

DENNIS C. BLEY, Member

RONALD G. BALLINGER, Member

MICHAEL L. CORRADINI, Member

JOY L. REMPE, Member

GORDON R. SKILLMAN, Member

ACRS CONSULTANT:

WILLIAM SHACK

DESIGNATED FEDERAL OFFICIAL:

HOSSEIN P. NOURBAKHSH

ALSO PRESENT:

NATHAN BIXLER, Sandia National Laboratories

MICHAEL CASE, RES

MATTHEW DENMAN, Sandia National Laboratories*

HOSSEIN ESMAILI, RES

EDWARD FULLER, RES

TINA GHOSH, RES

ERICA GRAY, Public Participant

EDWARD HACKETT, Director, ACRS

DONALD HELTON, RES

JOE JONES, Sandia National Laboratories

CHRISTIANA LUI, ACRS

KYLE ROSS, Sandia National Laboratories

PATRICIA SANTIAGO, RES

KC WAGNER, Dycoda

*Present via telephone

A-G-E-N-D-A

P R O C E E D I N G S

8:35 a.m.

CHAIRMAN STETKAR: The meeting will now come to order. First of all, I'd like to wish you all a happy groundhog's day. I haven't seen the reports so I'm not sure what we're in for yet. Spring is coming. That's good news.

This is a meeting of the ACRS Subcommittee on Regulatory Policy and Practices. I'm John Stetkar, chairman of the subcommittee meeting.

Members in attendance are Ron Ballinger, Dick Skillman, Mike Corradini, Joy Rempe. We will be joined, I'm told, soon by Dennis Bley and also in attendance is our consultant, Bill Shack. In fact, Dennis Bley just walked in.

The purpose of this meeting is to discuss the draft report State of the Art Reactor Consequence Analysis Project on certainty analysis of the unmitigated short-term station blackout of the Surry Power Station.

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

Dr. Hossein Nourbakhsh is the designated federal official for this meeting. The entire meeting is open to the public. Rules for the conduct of and participation in the meeting have been published in the Federal Register as part of the notice for this meeting.

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

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard and I'll remind everyone to please check your little communications devices and turn them off.

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

However, I understand that there may be folks on the bridge line who are listening in on the proceedings and we'll open the bridge line at the end of the meeting to check to see if anyone has comments.

It's been a while since we've had a meeting on SOARCA. I think November 2013, if my recollection is correct, was the last one when we heard about Peach Bottom.

Changes have been made. Improvements have been made and we're interested in hearing about them. So we'll now proceed with the meeting and I'll call upon Pat Santiago, the Office of Nuclear Regulatory Research, to open the presentations.

MS. SANTIAGO: Thank you. My name is Pat Santiago and I'm the branch chief for accident analysis branch in the division of systems analysis, Office of Nuclear Regulatory Research.

And I know Dr. Stetkar said he's been excited for at least 17 minutes with regard to this new presentation on SOARCA.

He's correct. The last time we briefed was in 2013 on the Peach Bottom uncertainty analysis and over the last two years we've been working on the Surry uncertainty analysis.

And we did take back several recommendations from our past briefs to the subcommittee as well as the full committee with regard to our analysis.

So today we are talking about the Surry uncertainty analysis and we better document the parameter development process, as you had recommended in the past.

We also look at the effects of key input parameter uncertainties and that unmitigated short-term station blackout accident scenario.

Another recommendation that you had made to us is to conduct the uncertainty analysis in parallel from the beginning of the project, which we are doing for the SOARCA Sequoyah analysis and we hope to bring that presentation to you sometime this spring.

The SOARCA project and uncertainty analysis touch on many different disciplines and are relevant to the severe accident consequence model.

This multi-disciplinary project was conducted with numerous colleagues at NRC, Sandia National Laboratory and other contact organizations.

Today, three team members will make presentations - Dr. Tina Ghosh of my staff, KC Wagner, who is Dycoda but formerly of Sandia National Laboratories and worked with us on the prior SOARCA consequence analyses.

He's replacing Dr. Randy Gauntt, who has been our MELCOR expert and has not been able to attend today. Hopefully, he may be on the bridge line to help answer questions later on. And Dr. Nathan Bixler, also of Sandia National Laboratories.

Other team members are in the audience - Trey Hathaway of my branch and other members of Sandia National Laboratories are Joe Jones, Kyle Ross and Dusty Thompson.

And with that, I'll turn the presentation over to Dr. Ghosh.

MS. GHOSH: Okay. Thank you.

The first slide I have here, just to continue on what Pat was just talking about, this was very much a team effort and we have a subset of the team here and we just wanted to recognize every who's helped out with the project on this slide.

Most of the people directly contributed to the report. We also had several internal reviews of parameters in the draft report itself.

CHAIRMAN STETKAR: Pull that - pull that closer. I'm not sure he can hear you.

MS. GHOSH: Oh, are you having trouble hearing? Yes? Okay.

Is that better? Yes. Okay. Great. We

also had some fellow NRC staff review our parameters during the development process as well as reviewing the draft report and we really appreciate everyone's support.

Unfortunately, a couple of the primary authors of this work have left Sandia since the work was completed. But they're still listed as Sandia because that's where they were at the time that the work was done.

And anyway, so this is a more full listing. So just thanks to everyone who's contributed to this project.

So what we'll talk about this morning we expect to go over what were the objectives at this Surry uncertainty analysis and give an overview of what we did and what the overall conclusions were.

This time around, unlike with the Peach Bottom uncertainty analysis, we did implement some MELCOR model enhancements and updates and we'll just give an overview of that.

Then we'll talk about our parameter development process. As Pat mentioned, we had to take these - the lessons of feedback on the Peach Bottom uncertainty analysis and document better how we chose which parameters to vary, which parameters we considered but we didn't and including the certainty analysis and what the basis was for the distributions that we chose.

Then we'll go into what were the set of MELCOR parameters that we looked at. We'll give you the whole list and talk about a subset of them.

Then we'll go into the MACCS parameters that were chosen. Same thing - give you the whole list and talk about a subset of them.

Then we'll go through the MELCOR analysis results, the MACCS consequence analysis results and wrap up with a quick summary.

So this time around the objectives of this uncertainty analysis were similar to the objectives of the Peach Bottom uncertainty analysis with a little bit of an addition.

We know from past studies that you can have a lot of uncertainty in these complex system models that we have but not all of the uncertainties evenly contribute to the uncertainty and the results.

So we wanted to develop some insight into what are the model inputs that the results that we care about are actually most sensitive to it.

We wanted to identify the most

influential input parameters for all the phases of the modeling project.

So, basically, what contributes to the most interesting variations in the accident progressions and so differences in source term timing and magnitude and then in the offsite consequence results that we looked at and in this case we kept the offsite consequence results that we were looking at still to the individual latent cancer fatality risk and individual early fatality risk.

And this time around we - one of - an additional goal was to also complement and support the NRC's ongoing site level three barrier project as well as some of the post-Fukushima regulatory activities that are continuing.

And I quoted those words straight out of the commission's SRM, or staff requirements memorandum, that came back to us to make sure that we conducted the study in a way that would complement and support these other projects.

MEMBER BLEY: Tina, you highlighted that you - this time around you've changed the documentation and expanded it some and also made some changes in - was it MELCOR?

MS. GHOSH: Yes, we - right. We changed -

MEMBER BLEY: Were there any other changes besides those two and how you did this study compared to the first one?

MS. GHOSH: I think - yes, there were some additional studies. I think as we go through this morning as I describe what we did and when we talk about some of the parameters you'll notice that there were some additional changes.

So I guess just to give you an example, you know, with Peach Bottom we showed our regression analysis results. Sometimes it was very hard to process all the information because we had four methods for the consequence - you know, offsite consequences alone we were looking at five circular -

MEMBER BLEY: Right.

MS. GHOSH: It was just a tremendous amount of information to process. So we tried to come up with a better way to both summarize the insights that we're getting from the four methods taken together.

So we changed the presentation of the material slightly and also added a couple of intermediate steps to try to help that information processing step. That's one example.

With a couple of the MACCS parameters, the MACCS parameter's set was the same but we updated our distributions and I think Nate will talk about a couple of those when he gives his presentation.

MEMBER BLEY: Okay. Good.

MS. GHOSH: So that's just a couple of examples. You know, it had been a few years and we continue to learn and progress our thinking so there are some additional changes.

MEMBER BLEY: One change I thought was you didn't use Latin hypercube sampling this time.

MS. GHOSH: Thank you. Yes, you're right. We didn't.

MEMBER BLEY: Did you have - it didn't seem to me that you made the sample sizes any larger and I've been wondering how you covered the parameter or you -

MS. GHOSH: Yes. So - right, so -

MEMBER BLEY: You did seem to cover the parameters well when you looked at your clarification but how did you know that ahead of time?

MS. GHOSH: You know, I think actually we relied heavily on the extensive work we did for Peach Bottom with the different sampling schemes to convince ourselves that with 1,003 successful realizations that having the simple random sampling is just - essentially just as good as Latin hypercube sample.

Maybe at the very tails we could have achieved a little bit more, you know, with the Latin hypercube sampling but it's almost in the noise at that point when we get to 1,003.

But, you know, it is something to consider for future projects. You know, if we don't want to expend the computational resources every time to do a thousand or so realizations, you know. You know, Latin hypercube sampling is still a good thing but for this - for the purposes of this project we were very comfortable that the simple random sampling was certainly good enough with the number of samples that we had.

MEMBER CORRADINI: But that was just judgment. It wasn't anything provable. That's what I -

MS. GHOSH: In this - yes.

MEMBER CORRADINI: - I don't understand

about the difference. I know the Latin hypercube you slice it into bins to make sure you cover the bins.

MS. GHOSH: Right. Right.

MEMBER CORRADINI: But just because it's a thousand it just -and just by kind of just inspection it looks about the same?

MS. GHOSH: Well, with Peach Bottom we had the quantitative proof of how well the results were converged to each other. In this case again we were relying on the Peach Bottom results to be a similar enough system that we were comfortable. But in this case if we had theoretically done it both ways it would have been similar.

DR. SHACK: But you did the bootstrapping, which sort of gave you some confidence on how robust the sampling was.

MS. GHOSH: Right. Right. And the bootstrap -yes, and the bootstrapping is, you know, better done with a simple random sample because there you don't have the, you know, dependence issues that you do with the Latin hypercube.

CHAIRMAN STETKAR: The fact of the matter is most of the distributions are not very broad. They did a lot of work - they had initial broad distributions and they did a lot of work to narrow them down.

So if you have very narrow distributions it works. If you have quite broad and skewed distributions, which they actively tried to stay away from, doesn't look so good.

MS. GHOSH: That's a fair look at the - for the parameters that matter though the most, yes.

So the scenario - so this time again we did the uncertainty analysis on one of the SOARCA scenarios and the one we chose was the unmitigated short-term station blackout.

For Peach Bottom we had done the long-term solution blackout but here again it's the unmitigated versions. So you're not crediting the SAMGs and the - any of the new flux strategies and so on.

And the focus was on the epistemic or the state of knowledge uncertainty and the input - the model input parameter's value. So we weren't explicitly looking at other kinds of epistemic uncertainty. And we handled - we looked at some aspects of aleatory uncertainty.

We used the same approach for the

aleatory uncertainty that comes from the fact that you just don't know when an accident might happen in the future.

So you don't know what the weather is going to be at the time that the accident happens. We used the same approach. We looked at Peach Bottom and the standard approach pretty much for all our MACCS analyses these days.

MEMBER CORRADINI: Just one clarification. So the only difference that I remember from a station short-term to a long-term is that batteries aren't available and therefore the aux feed water system doesn't function.

MS. GHOSH: That's exactly right, yes.

MEMBER CORRADINI: The operator action to be pressurized still, you know, as many times. I'm just trying to remember the difference.

MS. GHOSH: There's no operator action. There's actually no operator actions because there's no -

MEMBER CORRADINI: So there's not even the depressurization?

MS. GHOSH: There's nothing, yes. We have do DC, no AC power so -

CHAIRMAN STETKAR: Tina, is that - I want to make sure that I understand that. This - because the description of the scenario said aux feed water fails because seismic failure of the emergency condensate storage tank. It did not say that DC power was not available. So is DC power available or is it not available?

MS. GHOSH: Yes. Actually that's a good point. In this case, because we don't credit anything it doesn't matter. I think the -

MEMBER CORRADINI: You do credit things because you credit level two use of instrumentation and notification of offsite resources and instrumentation in the control room and communications. You don't - I mean, it's not explicit but -

MS. GHOSH: Yes.

MEMBER CORRADINI: And if DC power is available relief valves will work and -

MS. GHOSH: Yes, thank you. I apologize. You're right.

MEMBER CORRADINI: So -

MS. GHOSH: I think - yes.

MEMBER CORRADINI: - I'm really confused about what's available in the scenario and that's important for me to understand because I don't know whether the analysis is conservative or optimistic. My sense is it may be optimistic.

MS. GHOSH: Okay. You're right. I should be more careful about how it describe things. Thank you. But we assume there are - there is no battery power and aux feed is not working.

But that's the - those are the main things. The safety relief valves are working. We don't - we're not crediting any power operator.

MEMBER CORRADINI: But the - well, you have to be careful. The spring-loaded safety valves are working. The atmospheric relief valves are assumed to not open on the steam generators and the pressurizer power-operated relief valves -

MS. GHOSH: Exactly.

MEMBER CORRADINI: - are assumed to not open. So therefore they cannot open and stick open, which is an optimism for both of them, which they could possibly with DC power but I don't know how Surry is designed.

MS. GHOSH: Okay. You're saying that instead of the SRV sticking open that some other valves could stick open.

MEMBER CORRADINI: Yes.

MS. GHOSH: That is not considered.

MEMBER CORRADINI: So the likelihood of getting stuck open valve scenarios could be a lot higher.

CHAIRMAN STETKAR: Other valves that would open sooner.

MEMBER CORRADINI: Other valves that would open sooner and cycle more quickly, for example, and might not be designed for water relief, for example, as well as the safety valves.

So it's really important to understand whether DC power is available and nowhere could I find in the - the only thing I could find in the description of the scenario was the statement that it is a short-term station blackout because seismic failure of the emergency condensate storage tank disables auxiliary feed water. That was explicitly stated. There was no statement about DC power.

MEMBER CORRADINI: He's much more precise than I am. The only reason I asked my question was is that I'm trying to understand if I went to long-term DC is available.

Potentially, feed water works and you'd then tell the operators that they can depressurize and I assume when you said short-term all three of those things were off the table.

MS. GHOSH: Right. Right.

CHAIRMAN STETKAR: That's right. The difference between long-term and short-term is whether aux feed water works for some period of time until the batteries -

MEMBER CORRADINI: But with the DC power then the operators can depressurize based on some operator action and here that's not the case. You just rely on the safety valves to pop when they should pop.

MEMBER BLEY: But it's not just a matter of precision. John's pointing out that supposedly a conservative assumption - no DC power - actually precludes failure modes that might happen in the real world.

MEMBER CORRADINI: Sure.

MEMBER BLEY: So it might not be as - so it might not be conservative.

CHAIRMAN STETKAR: It is a scenario that is a scenario but it's important when you describe that scenario to tell people what it - what is available and what is not available and why it's not available because this might not be -

Okay. Because in the main body of the

report it doesn't say that.

DR. SHACK: No, it doesn't say it anywhere.

CHAIRMAN STETKAR: In fact, it explicitly describes why it's a short-term station blackout because of the seismic failure of the condensate storage container.

DR. SHACK: I think in the later report they - you're never quite sure which part you're reading but I think the later report also has the DC power. But in the NUREG it's only in the appendix.

MS. GHOSH: Yes, and we should probably put that in the body of the report. You know, one of the things is we keep relying on the NUREG 7110 volume two as well.

But that's probably worth repeating in the body of the UA report. Thank you for the comment.

CHAIRMAN STETKAR: But anyway, the - from my understanding the basic assumption is that DC power is not available.

MS. GHOSH: Yes. KC, did you want to add something?

MR. WAGNER: Yes. So I don't believe we

credit any instrumentation unless you found evidence of that in the report. We didn't have any DC power, AC power so -

CHAIRMAN STETKAR: But you don't - when you say credit you think explicit branch points being event trees and fault trees.

I think of event response and how people in the control room know what's going on and how they interact with the outside world and how they communicate with emergency responders and how the technical support center doesn't exist because it doesn't have any information, you know, that sort of stuff.

That is not explicit branch points in any event tree or fault tree that you've developed. However, it is implicit that all of those things work fine because all of the emergency response stuff works fine.

MS. GHOSH: Yes. So Joe, I don't know if you can - Joe Jones is one of the EP experts.

I believe if you lose all AC and DC power you have a very precise triggering point for declaring the emergency. But Joe, you can speak to that.

MR. JONES: This is Joe Jones. Exactly.

You know you're in a short-term station blackout fairly quickly.

The on-site resident inspectors have satellite phones to make off-site contact with the NRC headquarters operation center so they can make their contacts and then the on-site ERO has redundant systems to run to contact their folks - ERO, the emergency response organization.

So that's why we don't have any delays in the off-site is because we know at the very least we have on-site resident inspectors with satellite phones for direct communication off site.

DR. SHACK: It is interesting in the later report from Sandia there's a statement no DC power considered. Sandia will ensure this is stated in the document as a boundary condition that affects the sequence.

MS. GHOSH: Yes. Yes. Thank you.

DR. SHACK: We know where to stick it now.

MS. GHOSH: We very much appreciate your thorough review. You know, we always make our documentation better after we come to the ACRS. So thanks for - thank you for that.

CHAIRMAN STETKAR: Well, the only reason

I wanted to make sure that I understood I thought that was the case.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: But I wanted to make sure because as Dennis mentioned there - if DC power were available it would not change any of your MACCS type stuff - the emergency response - because of the assumptions that have been made that it's all perfect.

Everybody knows exactly what they have to do and all communications work despite the fact that this was a bigger earthquake than anybody could ever imagine, which we'll get into later.

If DC power were available then the scenario progression could be substantially different than what is modeled because you could get pressurizer PORDs cycling open and closed.

I don't know the Surry design. I don't know whether they're DC operating pilots. I don't know whether they're solenoid valves. I don't know how the block valves work.

I don't know whether they're pneumatic or hydraulic. I don't know anything about them. The same is true for the atmospheric relief valves on the steam generators in terms of cycling and possibly sticking open.

So the scenario progression and the timing of events could be different from a MELCOR respect if DC power were available and the assumptions are that you only rely on cycling of the spring-loaded safeties.

MS. GHOSH: Right. Right. Yes, absolutely. This is a very scenario-specific analysis where a lot of it is prescribed up front.

CHAIRMAN STETKAR: Well, we'll get into the scenario-specific earthquake part of it later when we talk about evacuation and assumptions about emergency planning.

MS. GHOSH: Right. Okay. So the other thing I wanted to point out is that actually one of the other differences from the Peach Bottom UA was that we considered some additional aleatory aspects of the modeling this time around and specifically we looked at the effect of when in the burn up cycle you might be at the time of the accident.

So we actually sampled the time at cycle instead of just using a single point in the time at cycle to see how the importance of that compared with the other uncertainties we were looking at.

And in terms of the safety relief valve

behavior, we also investigated some aleatory aspects of that - of the safety valve behavior.

So in addition to the epistemic uncertainty in the failure rate of the valves we also looked at - we imposed in addition some aleatory modeling in terms of given the failure rate how many times it may cycle before a failure. So we added those aleatory aspects to otherwise epistemic uncertain parameters.

MEMBER REMPE: Tina, with respect to looking at the time at cycle, it's my understanding that maybe things have changed that MELCOR does not consider thermal conductivity degradation as a function of burn up.

So you are considering within a cycle whether it's beginning of cycle or end of cycle. But you don't ever consider other effects that occur with the fuel with respect to time that might be important. Is that a true statement still?

MS. GHOSH: I don't know if KC can understand - can explain. I'm not sure I understood your question.

MEMBER REMPE: Well, thermal connectivity with other fuel decreases significantly with respect to burn-up. Well, it would be something that would be time dependently changing and I don't think the code does consider that effect. So what I'm trying to get at - because there's a lot of uncertainty still that's not captured in today's model. Is that true?

MR. WAGNER: Yes, that's true. So we don't vary the gap connectivity as a function of time. There is - you know, half of the fuel is from a batch before so it's a little bit older.

I mean, there's - it would be fresh fuel. We don't - we didn't adjust the gap connectivity.

MEMBER BALLINGER: But this - you're only considering the source term effects, right?

MEMBER REMPE: Yes, but the heat transfer and just the way the thing would regress.

MEMBER BALLINGER: Isn't that already built in?

MEMBER REMPE: In some codes for design or for, like, a LOCA analysis, yes, they do consider it. But a sever accident code like MELCOR does not consider that at this time is what I'm trying to get at.

MS. GHOSH: Then as I mentioned before we updated our MELCOR model and we'll talk a little bit about that. And the main reasons were it had been several years since the original model was created. We've moved to a new MELCOR version and we don't support the version that was originally - the original model was built with anymore and -

MEMBER CORRADINI: So let me ask this question and I'm looking so I admit that I may - haven't found it in the 500 pages yet.

MEMBER BLEY: 495.

MEMBER CORRADINI: Thank you. Is there a comparison calculation that shows that when you redo it you actually get the same result?

MS. GHOSH: Well, it shows you the new result that you get. So it -

MEMBER CORRADINI: Oh, okay. That's not what I'm asking. What I'm asking is - I mean, I'm back to a hand calculation. If I - if I change my mode and I do a hand calculation and I get a different result I ought to know why my result is different. Where would I look for that?

MS. GHOSH: So in Appendix A we've actually documented fairly extensively all the step changes from the old model to the new model and we explained the reasons.

So MELCOR 2.1 has the new modeling in it that is based on our updated understanding that didn't exist in 1.86. There are enough changes even in the code version that it makes sense that some things change a little and then we also updated our - the MELCOR model itself and KC's coming up to elaborate.

MEMBER BLEY: But to Mike's point, I think it also said somewhere I saw that you do get differences but you don't know why because there are so many changes.

MEMBER CORRADINI: That was in the executive summary. That part I read.

MEMBER BLEY: Okay.

MS. GHOSH: Yes. I think it's difficult to pinpoint, you know, every specific - to map all of the changes onto the very specific effect.

But we tried to give you the cumulative differences for various intermediate steps in the process. But KC, yes, whatever -

MR. WAGNER: So what was done in Appendix A and that's where I direct you to is we had the 1.86 calculation, the vintage 2007.

We did a straight conversion of that input deck using SNAP to 2.1 and with the code version that was being used for the current calculations we just compared 1.86 to 2.1 and I would judge that comparison was very close - the timing and it's spelled out.

Then to go from there to the UA model there was a set of corrections to the model and we'll talk about those today, and then there was enhancements to support the UA and we'll talk about those today also.

And then so the 2.1 calculation that was the straight conversion of the 2007 calculation was compared to the new UA and that response was substantially different and we talk about the reasons for those differences in there and why they make sense.

MEMBER BLEY: Thank you.

MEMBER REMPE: But just out of curiosity is there something that you can point to that will say man, that, we think, makes the most difference? Because I couldn't follow that and I will admit that by the time I got to Appendix A it was pretty late.

You know, but was it, like, the steam generator utilization or is there something that really made more of a difference in the changes?

MS. GHOSH: Yes, I'll let you - I'll let you take the first one.

MR. WAGNER: For as long as I've been involved in Surry and that started in the '80s at NEL we thought that there was limestone concrete in the Surry containment.

In reality, it's basaltic and that makes a huge difference. And so that the kind of got you.

MEMBER REMPE: Okay. But I can remember for some other work I did that it seemed to be that the nodalization was pointed out as - the timing even before you got X vessel that things started extending out and, like, I think you quote some values in Appendix A. But, you know, 50 hours longer before something occurs is before -

MR. WAGNER: Little bit of that. When we did the original SOARCA we didn't have - we didn't do new arching calculations and at the time there was a Hyberna program going on at NRC Research.

And so we used the decayed heat from that model because it's the best thing that we had available at the time in lieu of doing, for lack of a better word, best guesstimate origin type calculations for decay heat. So decay heat was maybe skewed a little bit high in the original SOARCA but we -

MEMBER REMPE: Okay.

MR. WAGNER: - felt like that was, you know, it was better than what was built in MELCOR and it was the best that we had at the time.

MEMBER REMPE: Okay. Thanks.

MEMBER SKILLMAN: Before you exit, and building on Dr. Rempe's question, it seems that there's an ah-ha in the failure of the lower support plate timing. There's almost an hour shift between the original calc and the revised calc and there is a - there is a sequence there.

The first failure of the support plate - lower support plate - lower plant dry out, then lower head failure. Those times are shifted by approximately an hour in the new calc. Why is that, please?

I understand the limestone versus basaltic concrete issue. But I'm wondering why there is such a time difference laid in the sequence. This is seven hours, seven and a half hours.

MR. WAGNER: Can you point me to which two calculations you're talking about? Are these the 2.1 calculations?

MEMBER SKILLMAN: I'm looking at 2.1 -

1.8 versus 2.1.

MR. WAGNER: Okay. So -

MEMBER SKILLMAN: And the area of interest that I'm pointing to is the approximate hour shift laid in the scenario.

The first failure of the lower support plate goes from 6:36 - six hours and 36 minutes - and the newer calc is 7:33 - seven hours and 33 minutes - almost an hour later. Lower plant dry out goes from hour 6:39 to hour 7:35 in the new calc. Approximately an hour.

MR. WAGNER: So the dry out is usually a function of when the debris gets there. We have to get some down there in that. So the changing of the core plates will change the timing of the dry out.

Why there was the changing of the dry out of the core plate we have about eight years of co-development and so I think the models did change a little bit and improve. I can't point to anything specifically.

MEMBER SKILLMAN: Well, it's the same hardware - same machine, same reactor vessels, same internals.

MR. WAGNER: That's not - their model is

nowhere close to it.

MEMBER SKILLMAN: Let me finish. And so obviously what's occurred here is we had the conservatism in their earlier version and that might have been so sobering that as the conservatism is released we see the greater time.

MR. WAGNER: I mean, one hour in the timing of the core plate failure is - the timing to the start of release and the failure of the containment I think was pretty similar.

There was some differences in the vessel accident progression. We judge those relatively close.

MEMBER SKILLMAN: Thank you.

MS. GHOSH: Okay. So getting back to the overview of what we did, we looked at the uncertainty in key model input.

So the first step was to come to a set of what are the key uncertain input parameters that we - when varied and then we propagated those uncertainties in a two-step Monte Carlo simulation we first generated a source of - a set of source terms using the MELCOR model and then we combined the source terms with a set of MACCS realizations that sampled all of the MACCS' uncertain parameters to get a distribution of consequence results.

And we ended up with 1,003 successful MELCOR realizations that completed to 48 hours and each of those were coupled with a successful MACCS realization and because the question sometimes comes up, just so you know of the ones - the MELCOR realizations that didn't complete we didn't analyze the reason that those didn't complete and we also looked for -we did a regression analysis on the incomplete realizations to make sure that there weren't areas in our sample space that were consistently failing the runs and we did determine that we had sufficient - that the failures happened randomly in the parameter sample space.

So we had a sufficient - the set of runs that we had sufficiently covered the entire sample space and all dimensions. So we were comfortable with -

MEMBER CORRADINI: So since I kind of do these calculations once in a blue moon -

MS. GHOSH: Yes.

MEMBER CORRADINI: - these are - these are - the reasons they - that the calculation dies should tell you something. So was there some attribute about all these failures that told you something about the calculation?

In other words, were they all failing in a certain subroutine? Were they all failing because of time step - we couldn't restart? There is just some model that froze up every time you access it? I mean, these sorts of things.

MS. GHOSH: Right. So that's exactly the kind of analysis that was done so we categorized all the failures by which subroutine - you know, what was the problems.

MEMBER CORRADINI: So where is that?

MS. GHOSH: Do we have that in the - we have that. Oh, it's not in the report. I think we just summarized -

MEMBER CORRADINI: Failure at - you learn more from failure than success.

MEMBER BLEY: But kind of - what kind of things were there? I mean, divide by zero - whatever you get, somewhere something's going wrong and that - one would think would lead you to decide if it's a problem in the way the code was written, if it's a modeling problem and -

MS. GHOSH: Right. Right. Yes.

MEMBER BLEY: - and since, you know, it ought to be -

MS. GHOSH: So let me - let me -

MEMBER BLEY: You ought to understand it and fix it because it might be something important.

MS. GHOSH: Yes. KC is coming up. I think he - KC has a summary. We have a table of summarizing what the reasons were. But if we could back up a step.

With all of these large projects we've done we have found things in the code that we were - that we saw where we were able to improve maybe a small bug or just some improvement that could be made which got our success rate up to a thousand out of, you know, 1,200.

And the reason that I say that we're comfortable with the stuff that we ended up with is that when we analyzed the reasons for the incomplete realizations we were able to convince ourselves that they don't affect the validity of the results that we get.

There's no correlation in terms of if these three parameters come together in this way it always fails the code. There is nothing like that that came up.

They are - the failures are just randomly distributed in this end dimensional sample space.

So we're comfortable with the results that we ended up with. That's from my side in terms of the overall uncertainty analysis.

From the MELCOR side, they also did look at the specific reasons and it's not the same reason every time. There's a set of reasons and I don't know if, KC, if you want to elaborate on that.

But there are various, you know, subroutines and reasons but they're not - it's not pointing to any failure that's always going to happen when things converge in a certain way with the model.

But yes, go ahead, KC.

MR. WAGNER: Yes. I guess as a long time user there are problem areas that crop up often where I would say probably we have to co-develop or look at them and see whether they could be fixed.

They usually ended in the code saying you couldn't converge anymore and so it stops because it can't get a satisfactory convergence and the routines are in the cavity package and one of the debris temperature calculations in the lower head.

MEMBER CORRADINI: Exclusively or primarily?

MR. WAGNER: Primarily.

MEMBER CORRADINI: So the cavity package is three, right?

MR. WAGNER: Right.

MEMBER CORRADINI: So it's dying there most of the time of the 200?

MR. WAGNER: In Sequoyah, which we aren't talking about today, yes. In Surry it was more often in the core package.

MEMBER CORRADINI: Oh.

MR. WAGNER: So it was evaluating a debris temperature in the lower head. We asked the developer to take a look at it.

He looked at quite a few of them and -

MEMBER CORRADINI: Okay. So -

MR. WAGNER: Did have - did have a patch in the time frame for this project.

MEMBER CORRADINI: That's fine. I mean, I appreciate how hard this is so I'm not - it's not meant for as a criticism, just trying to learn.

So you're saying in the Surry case it was - I'll just use the word primarily in the core package and primarily in the lower - the lower head calculation?

MR. WAGNER: Yes.

MEMBER CORRADINI: Okay. So in the phase one of the crosswalk since that I'm familiar with, between DoE and NRC, MAP and MELCOR has substantially different models in the lower plenum.

So and I know this is the case because the Sandia team with the, I'll just say, EPRI team have been talking about this.

So it - so as you're - what I hear you saying is they think they understand where it is but in the time frame of the study it got - didn't get fixed. Is that -

MR. WAGNER: Yes.

MEMBER CORRADINI: - is that a fair characterization?

MR. WAGNER: That is correct.

MEMBER BALLINGER: So but there's a flip side to this, which is kind of insidious, and that is your runs that didn't finish and so you've got an analysis and it says so in the document.

But have you verified that some of the runs that did finish are not fortuitously finishing because you've got - because of the same problem?

Maybe I'm not saying it right. Last time I wrote code they were using Wang computers and stuff. But they - what we used - we used to stick in things where if something failed we knew exactly what statement it failed on and the system stopped, right.

So presumably you can do that, right? But are you sure that you're not getting yourself comfortable because ah-ha, we have 1,003 successful runs?

They were successful because they converged or they're successful because - is convergence tantamount to saying everything went fine and the answers are fine or are you - is there a potential for deceiving yourself into thinking that they're fine when they're not fine because of some fortuitous other errors that suddenly make a thing work okay or converge.

I don't even mean work okay. I just mean converge. I mean, so in other words, if you track - if you track down every single case where it didn't converge or whatever the - whatever constituted failure to find out that A, it was a model that was out of its range or something happening other than just divide by zero and other kinds of things those are really insidious errors because you can divide by zero someplace and have the thing keep running. So am I stating this -

MS. GHOSH: Okay. So let me - it is an interesting - I think it's a very deep question.

I can give you maybe a higher level answer. I'm not a micro coder or analyst or, you know, any of those things. I think at the end of the day - so the purposes of this study what we're - what we're trying to do is to get some distribution on the results, understand what the variations can be and how the system behavior may progress and be able to identify what - in terms of everything we've put in the pod what are the most influential parameters for the results that we care about.

We feel that with the set of things that we did and the way that we analyzed the results that those insights are reliable. I think when you go down a couple of levels deeper and ask questions for every single of the 1,003, you know, realizations, you know, how confident are we.

You know, it's very hard to answer that question but I think that we have reasons to be confident and then kind of the sum total of what we've done because we've sliced and diced the numbers.

We took a statistical approach with a

stability analysis to see, you know, how stable the results are.

With the regression analyses we don't rely just on the regression analysis results in terms of the regression numbers that come up but we look at individual realizations of interest that exhibit a variety of behavior - a different behavior to see what we can explain what happened in those particular realizations. So taking all of those together, you know, we have a comfort level.

MEMBER BALLINGER: Sometimes getting the right answer is - you fool yourself because it's an answer you're expecting, right, and that's even sometimes worse than getting an answer which you know to be wrong.

MEMBER REMPE: To kind of - even though what was it, like, you said 90 percent of the non-steam generator to rupture realizations or 90 percent of the time you didn't get a steam generator to rupture but then 10 percent of the time dominated the released.

Well, were all these failures - is there the potential that they might have change that conclusion is what - to try and put it physically is what I would do and maybe you or KC has an idea and say oh no, those were cases that aren't going to give you a big release even though they failed.

MEMBER BALLINGER: Because the creep parameters are very sensitive.

CHAIRMAN STETKAR: Since it's 16 percent of the wrongs or something like that. I did the math right. Yes, 200 over 1,200 didn't go.

MS. GHOSH: But if I could - if I could - I'll repeat what I already said just to emphasize. We did a regression analysis so, you know, we varied.

In total there's 24 - 23 MELCOR parameter groups that we varied that covered the span of all the aspects that we were modeling and we did a regression analysis on what part of the sample space was covered by the failed runs versus the successful runs and if there were some combination of things coming together that would every time fail the MELCOR run and therefore leave us with a set of successful realizations that weren't successful we would have expected some of that to show up in the regressions to say that you have some part of the sample space that's failing your runs every time.

Our regressions show that those failures were completely - were randomly distributed. There was no correlation between what was sampled in the - all the parameters that we sampled and which runs were successful or incomplete.

So that gives us a lot of confidence that there isn't some - you know, some, you know, the stars aligning in a certain way every time is going to fail your MELCOR, you know, run and therefore we missed some part of the -

MEMBER REMPE: Well, I heard you say that but then I heard KC say oh, no, that it's always in the core package. And so that's what's puzzling me is there's a disconnect that -

MEMBER CORRADINI: I guess - can I ask Joy's question a little differently and then we can stop torturing her for a while. The way I interpret this is - the way I interpret this is there's got to be something to be learned from the 200 failures about the model.

Whether or not it affects your uncertainty I don't think it does because you've already proven that it's the boundary conditions and initial conditions that drive your uncertainty.

It's not how the stuff models once you hit the go button and things start going to hell in a handbasket.

So it seems to me this model set of - if it really is occurring primarily in the core package and primarily in lower plenum that's interesting to learn what you can do to improve it. But I'm not sure it affects the uncertainty analysis. That's what I heard you saying.

MS. GHOSH: And that - and that's a very good point and I think we will continue to work on making sure that some of these are code issues and improved in the future.

And in the process of the project we did fix a number of issues that, you know, running MELCOR this many times in the same model you do it - it's a model validation exercise or a core validation. You do uncover things and I think that's fair and we would continue to kind of track those down.

As KC mentioned, on the time frame that we had we got to a point where we felt it was good enough and you kind of have to stop there.

MEMBER BALLINGER: Okay. Let me ask one more dumb question. For these runs that fail, did you pick one run that failed and then run the same exact run a number of times to see if it failed at the same place every time?

I mean, that's another one of these techniques that people use.

MS. GHOSH: Yes, you know, Ron, I think when we - in the past after we fixed a bug we've done that to make sure that it's fixed but this time I don't think we did that particular step for - but yes. Go ahead. Sorry. Go ahead.

MR. WAGNER: I guess I'll just add I understood your question perfectly and that was a good one.

And we didn't individually look at each run other than to the extent that we test on - well, we test them on to the developer and he looked for common themes in subroutines where we did have convergence problems.

But I would add that the code does have energy checks and when it does converge we do trust it because of the energy balance checks and the things that are done to assure convergence, you know, conservation in energy, mass and volume.

The code has been validated in volume three of the - the user's guide came out with the validation cases. But we didn't get into each one of those to see or we - I wouldn't characterize -

we didn't feel like we were lucky.

We felt like if it converged in the code it satisfied the criteria to move on to the next time step. We were comfortable with that run.

MS. GHOSH: Thanks, KC. Okay. So moving on to the next slide. The figures of merit that we looked at were the cesium and iodine released to the environment by 48 hours.

The in-vessel hydrogen production, the timing of the initial fission product released to the environment, which we defined as 1 percent of noble gases, and in terms of the offsite consequences as I mentioned before we kept the same metrics as the original SOARCA study of the individual early fatality risk and individual latent cancer fatality risks.

And we used the same four regression methods to analyze the results that we used in the Peach Bottom UA.

We also used scatter plots and also, again, we did phenomenological investigation into selected individual realizations to make sure that we understood the regression results and specifically differences in the behavior of the system.

So this is just an overview. It's a little bit - just a reminder. In the original SOARCA calculation we did what's depicted on the left. Basically, the weather uncertainty was reported as a mean, which is shown as the age on the X axis - the red age on the X axis in the figure.

So when we now do the uncertainty analysis we are varying a set of inputs into the MELCOR model as well as a set of inputs into the MACCS model but continuing to keep the - what we call the inner loop for the weather.

So I think eventually in the report we're going to add some figures that look like the family of figures on the right, which will show both the uncertainty due to weather in addition to some example of the spread of individual curves from the epistemic uncertainty.

But right now whenever you see the curves in the report or the tables what we're reporting is the distribution on the mean consequence from the set of weather trials, given one set of epistemic inputs from both the MACCS and the MELCOR sides of the equation.

So it's the distribution of the ages -

the ages and the double bars.

So the regression techniques that we used were the linear rank regression. It's the simplest method and historically the one that's been used most often in past studies like NUREG 1150, and then we used the same three we added for Peach Bottom, which was the quadratic regression, recursive partitioning and MARS.

And these methods are more advanced in that they create regression models that can also capture interaction effects amongst the variables as well as capturing nonmonotonic effects and those two things are not possible with just the linear rank regression modeled by itself.

And we used multiple approaches to pulse process the set of Monte Carlo results that we got and we think it provided better explanatory power with regard to identifying which input parameters were the most influential with respect to results and this was demonstrated previously in the Peach Bottom UA.

The overall conclusions from this Surry uncertainty is that it continues to corroborate the SOARCA study conclusions, that in an absolute sense the public health consequences in terms of the matrix we looked at, which are individuals, early fatality risk and individual latent cancer fatality risk are smaller than previously calculated and in the original SOARCA study we were - we did a comparison with the siting study - the Sandia siting study from 1982, and the delayed releases we find continue to provide time for emergency response actions and the long-term ends up dominating the offsite health effect risks and we continued to compute essentially early fatality risk.

We did have a handful of nonzero numbers but they were extremely small, which is why we use this essentially zero terminology. And a major determinant this time around of the source term magnitude is whether or not the accident progresses to a steam generator two rupture and then you get order of magnitude roughly more release in that case.

And the mean individual - the mean - this is mean over the weather variation - individual latent cancer fatality risks assuming at linear known threshold, the dose response model conditional on the accident actually happening is still less than needed to be very small.

For reasons that we explain in the report they're lower than the risk that was evaluated in the original SOARCA study, both within ten miles, and the ten-mile risk in this case is the highest population at risk and that risk decreases at longer distances.

CHAIRMAN STETKAR: Tina, just for the record - I don't want to dwell on this - everything you state here is very carefully stated within the context of the particular scenario that was evaluated.

A footnote here notes that the frequency of that is on the order of about 10 to minus six per year for that particular seismic acceleration.

Full scope seismic PRAs have typically shown that seismic-related station blackout occurs more frequently because of seismic events that are perhaps not as strong as this seismic event but include hardware failures of the emergency diesel generators, which was one of the reasons why I asked about the ability to receive power.

This assumes that the event is so strong that it destroys all of the safety-related AC and DC stuff inside the plant, which is a really big earthquake.

We'll get back to that later. I just want to make that for the record.

So making conclusions about absolute frequencies here in terms of how they relate to offsite health consequences is very dangerous.

It is true within the context of a specific scenario with the assumptions -

MS. GHOSH: Yes

CHAIRMAN STETKAR: - that were modeled here and those assumptions aren't necessarily always conservative.

MS. GHOSH: Yes, I know. That's fair and we did no work on the frequency side of the equation. So yes, that's fair. Thank you.

CHAIRMAN STETKAR: Well, you did no work on the frequency but you do allude to it in these types of presentations -

MS. GHOSH: We do alluded to it, yes. It's -

CHAIRMAN STETKAR: - about why things are really, really small.

MS. GHOSH: Right. Yes, it's always a struggle because everything we calculated was conditional on the set of assumptions leading up to it and we're trying to provide context. It's always a struggle how to provide the context.

CHAIRMAN STETKAR: Yes, we've had this - you know -

MS. GHOSH: Yes, I know. Okay.

CHAIRMAN STETKAR: - we don't need to - we've had the discussion before.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: Don't try to oversell the results in terms of -

MEMBER BLEY: It's being generalized.

CHAIRMAN STETKAR: Yes, in terms of generalized. This report has done a much better job of not doing that than the previous report.

MS. GHOSH: We took your advice into consideration.

MEMBER REMPE: So as you do that to this report, even though it does say you've used updated EOPs and things like that, it doesn't mention the fact that flux isn't out there and so there might be a need to put a caveat like that in the document. I mean -

MS. GHOSH: Yes. Okay. And that's a - that's a good feedback - you're right - because at the time of the original SOARCA there was no flags. Yes.

MEMBER TEMPE: Targets. Yes.

MS. GHOSH: And we just - we're going back to the original scenario specifications. But you're right, this is published in 2016. There may be an expectation to explain how it relates or doesn't. Thanks.

Okay. So the next section we'll just quickly go through what were the model enhancements and actually we started some of this discussion already.

We updated the model to MELCOR 2.1 and then between, you know, about 2007 and when we started this study in earnest we realized that there were some errors and unintended things in the model that we figured if we're enhancing the model anyway we should just go ahead and update all of those.

And I'll just go through a quick listing of what those were and, again, this is documented in detail in Appendix A and summarized in Chapter 5, I believe.

So the model enhancements - the main ones included enabling the molten core of concrete interactions to take advantage of recent code enhancements and corrections that are thought to add realism. We increased the steam generator nodalization that we have a picture on a subsequent slide.

MEMBER CORRADINI: We're going to get back to these by later, right? This is just a summary.

MS. GHOSH: This is a - yes, this is a summary. We included the hot tube modeling in the SGTR logic and we redefined the admission criteria and extended the hot leg nozzle modeling to consider the stainless steel cladding.

MEMBER REMPE: Tina, before you lift that slide even though this is just a summary, if I - what exactly is in the hot leg nozzle at this time?

Because I was looking at Page 57 out of - which is like three three in your report. Is it carbon steel with stainless steel cladding or is it later at the bottom of the page where it says the hot leg nozzle was not on the original Surry analysis of stainless steel but was found to be Inconel. What exactly was modeled or -

MS. GHOSH: Yes, I think we need to correct that in the report. If I understand correctly, what we added, and KC or Kyle, correct me if I'm wrong - I believe we originally had carbon steel and we added the stainless steel cladding next time.

MEMBER REMPE: Okay. But what was that last - that's what the top of that page says but then the bottom of the page has Inconel.

MS. GHOSH: Yes.

MEMBER REMPE: Is that just a typo that needs to be fixed?

MS. GHOSH: I think we had a- right. We had an error in the report.

MEMBER REMPE: Okay. That's fine. Okay. I just was -

MEMBER BALLINGER: Typically, what happens is it is a carbon steel nozzle line with stainless steel but the weld between the nozzle and the pipe has got an Inconel or some filler metal which is equivalent to Inconel in between to match thermal expansion. So it's a lot more complicated than just saying it's Inconel.

MEMBER REMPE: I'm surprised they have that level of detail. They're doing a weld in MELCOR. Can someone - so that's just totally a wrong sentence at the bottom of that page?

MS. GHOSH: Yes, we need to fix it.

MEMBER BALLINGER: Because it definitely

was not an Inconel nozzle.

MEMBER REMPE: Okay. I was curious.

MEMBER BALLINGER: Okay.

MEMBER REMPE: That's the way it's reading, okay?

MEMBER BALLINGER: That's a first.

MR. WAGNER: That is a typo and the tubes or models is Inconel. In the original SOARCA it's just modeled as carbon steel and here we tried to represent a stainless steel cladding underneath, you know, before the carbon steel nozzle. And I guess we were influenced by how tough it was to hold up with just the stainless steel cladding.

MS. GHOSH: And there were some corrections that we also identified that we implemented. There were some errant vapor pressure coefficients for control rod materials that were corrected and KC already mentioned that the original analysis assumed a limestone aggregate for the containment concrete.

But during research for the UA we found out that the aggregate was actually basaltic and the main steam line was found not to be isolating. So those were fixed.

And then we developed a current Surry

core inventory to facilitate the time at cycle sampling. And just for reference, the Surry SOARCA have implemented a high burnout core inventory.

So this is a - just a picture of the difference in the nodalization. On the - on the left is the original nodalization. On the right is the increased nodalization for the UA implemented in part because we wanted to study more of the steam generator to ruptured variation of the short-term station blackout.

MEMBER CORRADINI: So you had - so you had more control volumes in the tube sheet? I'm just trying to look at the difference. I only see more control lines in the tube sheet. Is that correct?

MS. GHOSH: Yes, the tube -

MEMBER CORRADINI: Tube bundles though?

MR. WAGNER: Yes, the descending side of the tube bundle.

MEMBER CORRADINI: The descending side. Exactly. Okay. Thank you.

MEMBER BALLINGER: Was there any attempt to nodalize it with respect to where the support plates are? That's a constraint.

MR. WAGNER: She mentioned that one of

the new additions for this - these sets of calculations was the hot tube modeling and so we followed what had been done at INL before with SCDAP/RELAP and that there's a site calculation where we have a scope of the tool is representative that I think was just six inches or a foot long.

And so and it's fed with boundary conditions based on what the main calculation is doing, and then the temperature is BIOS based on the CFD work that Chris Boyd had done. We're going to cover that a little bit later how that calculation is done.

But there was a local or a hot tube analysis that really focused close to the tube sheet to make sure that we were able to capture the hottest part of the flume entering the speed generator.

MEMBER BALLINGER: So it's a thermal model. Okay.

MS. GHOSH: And that's just a quick summary of the MELCOR model enhancements. I'm going to wrap up the overview section with the parameter development process that we used.

And again, this was in an effort to better document how we chose the parameters and why and then the basis for the distribution that we had.

So we, of course, involved staff both through Sandia and NRC with expertise in the MELCOR MACCS modeling for SOARCA and we got involved a wider group of subject matter experts to provide reviews of the data and parameters.

And we started with a review of the parameters that were used in Peach Bottom and then we also performed a systematic review of the phenomenological areas as relevant for Surry.

So, for example, in terms of the MELCOR side, the sequence issues, the in-vessel and ex-vessel accident progression, containment behavior and the chemical form in aerosol disposition.

We - the group reviewed the phenomenological topics covered in the MELCOR reference manual just to make sure we didn't miss anything and we also reviewed a comprehensive MACCS parameter list.

On the MACCS side things were a little bit simpler in that the change from a BWR to the PWR doesn't matter as much in terms of offsite parameters.

We did a thorough review but ended up

with the same list of parameters that we had used for Peach Bottom. We just reconfirmed that that was the - a good set to vary.

So we developed an initial list of candidate parameters and then we implemented what we called a story board process where basically an analyst kind of took the lead in documenting the justification and the rationale for each parameter and then we had a series of iterative discussions and review meetings which involved others at Sandia and NRC.

And the focus was on confirming that the parameter representations appropriately captured the key sources of uncertainty with respect to that parameter and that the probability distributions we ended up assigning to them were reasonable and had a defensible technical basis.

During the - during the course of this very iterative process we had repeated meetings on, you know, logical groups of parameters. We decided to omit some parameters from further considerations and we added others along the way.

And some parameters, much like in Peach Bottom, ended up being exploratory in the sense that we didn't have a lot of basis for an uncertainty distribution. But we have the sense that they are uncertain and we wanted to gain insights into what variations in that parameter how it might affect the results. So we -

CHAIRMAN STETKAR: Tina, help me - two questions, one on parameters that were omitted and the topic that you just mentioned -

MS. GHOSH: Yes.

CHAIRMAN STETKAR: - if you want to call them exploratory uncertainty distributions. When is it better to discuss those? When we get into the actual MELCOR parameters presentation or now?

MS. GHOSH: Maybe either time. Do you have a theoretical - an overview of discussion to have or -

CHAIRMAN STETKAR: Well, I have one question about a particular parameter. If you want to - doesn't make any difference, I guess, when we discuss it. But one parameter - bear with me while I find my notes.

MS. GHOSH: Yes, sure.

CHAIRMAN STETKAR: I'm not very well organized this morning. There's a statement that said that you didn't consider a reactor pressure vessel drain line because you couldn't find one, which, you know, is pretty much the case for pressurized water reactors.

Pressurized - many pressurized water reactors have a large number of in-core instrumentation tubes around the bottom head that could conceivably fail.

Did the models include those? Does Surry have them, first of all, and if so did the models include failures of those tubes?

MS. GHOSH: Yes. So I will let KC answer the second part of the question. With respect to the first part of the question, you know, it's kind of of funny with these projects.

There's a lot of cost dependence and that statement came out of a review of our record on the Peach Bottom UA to make sure that everything - you had covered everything on the - on the with respect to the drain line.

We haven't had a lot of discussion with Peach Bottom so that - so that was, you know, the reason that -

CHAIRMAN STETKAR: Yes, I'm aware of that and also where the boiling water reactors have - reactor water cleanup system drain lines off the bottom of the vessel. I'm talking about a pressurized water reactor now.

MS. GHOSH: I understand. We should - we should revise - we should revise that in the report. But yes, that was just a packet dependence thing because we were - we started with a boiling water set and we were weeding out what we don't, you know, need to consider. So that was a funny result.

CHAIRMAN STETKAR: My more fundamental question is does Surry have lower head penetrations for in-core instrumentation with the guide tools and if so were they included in the model.

That's the fundamental question. I don't care about that part that it doesn't have a drain line.

MS. GHOSH: We - so maybe KC can help me with this but we don't model the instrument. We should actually just let KC answer but just not yet.

CHAIRMAN STETKAR: Well, but if you knew pressurized water reactors you'd look for the in-core instrumentation tubes so -

MR. WAGNER: Yes. Surry does have those.

CHAIRMAN STETKAR: They do? Good.

MR. WAGNER: We didn't model them.

CHAIRMAN STETKAR: You did not?

MR. WAGNER: We did consider -

CHAIRMAN STETKAR: You did?

MR. WAGNER: We considered but we didn't do it. Mark had done some work on the BWR for Peach Bottom and then I guess in the original SOARCA I remember listening to Bob Henry talk about his modeling of TMI and, you know, release of maybe some gases through the instrument lines and high readings in certain parts of the containment.

So we didn't view it as a bypass mechanism and we didn't really have the models other than if we kind of did like what was done in the Peach Bottom analysis to try and mock up something.

But we didn't go to that level of detail here because the conclusions from Peach Bottom was that it wasn't terribly important. Our impression from -

CHAIRMAN STETKAR: But again, Peach Bottom is a boiling water reactor and I don't want to talk about a boiling water. I want to talk about Surry.

The statement is made in the report that there are no high-pressure melt scenarios and one of the questions that I had is can you get a high pressure ejection through failed instrument tubes into the reactor cavity.

MR. WAGNER: I suspect that would be possible under pressure.

CHAIRMAN STETKAR: Okay. And the question is why then didn't you evaluate that?

MR. WAGNER: We have low pressure accidents.

CHAIRMAN STETKAR: You do? And I guess we'll understand later why they're all low pressure. I would suspect that some - I don't know anything about thermal hydraulics and I know less about materials. But an early failure of a instrument tube or several instrument tubes before you get some sort of depressurization going on through, like, a hot leg failure or a stuck open cycling valve could in fact, I would think, lead to a high pressure scenario.

How likely that is I have no idea because I don't know the tubes and I don't know how to modify the event scenario progresses.

MEMBER BLEY: It would seem the logic by which you rolled it out ought to be here somewhere. My memory is vague. Mike probably remembers better.

The TMI - a fair amount of corium went down into those tubes, surprised people that it didn't come out - that there was really good heat transfer, I guess, on those tubes.

MEMBER REMPE: Actually, since I was involved in this -

MEMBER BLEY: Yes.

MEMBER REMPE: - a long time ago, yes, some of the corium went in but the post-accident evaluations saw that the melt could never travel enough below the lower head to cause that type of failure.

Frankly, we never saw anything that degraded those tubes enough to result in a high pressure ejection and I believe that the folks from Sandia should say but that's why the MELCOR does not model that.

CHAIRMAN STETKAR: Even with the tube thinning that a lot of plants have seen?

MEMBER REMPE: The tube thinning instrumentation -

MEMBER BLEY: This is the guide tube -

CHAIRMAN STETKAR: The guide - the guide tube thinning.

MEMBER REMPE: I've never heard of people seeing guide tube thinning. You've seen steam generators tube thinning but -

CHAIRMAN STETKAR: No, no, no, no. Well -

MEMBER REMPE: - guide tube thinning on the left head?

CHAIRMAN STETKAR: Yes. Yes, look it up. In-core instrumentation guide tube thinning. A lot of people have replaced a lot of - plugged and replaced a lot of guide tubes.

MEMBER REMPE: I have not heard of that happening. I've heard of plugging of some generators but not guide tubes. But -

CHAIRMAN STETKAR: I'll have to look up the - I don't know, Dick, if you remember -

MEMBER SKILLMAN: I'd offer just two comments. I was there with TMI tubes. They're one in Schedule 160.

They're basically a gun barrel and they're welded on tight and while there was corium in the lower portion TMI tube had there was also water there and so there was excellent heat transfer.

There was some penetration. But

remember, in the in-core too is an instrument that's about three-eighths of an inch in diameter and so the opening is really the annulus between the ID of the tube and the instrument itself.

CHAIRMAN STETKAR: It depends on where they park them. A lot of them park them back up at the seal table.

MEMBER SKILL MAN: All 52 fully engaged because those gave the burn-up from the iridium detectors. But like Joyce said, it was excellent heat transfer off the bottom.

MEMBER REMPE: Right. And in fact there - you could see gaps between the relocated debris and the nozzles because the debris shrinks when it solidifies.

And so they just never saw - there was damage where it cut across above where the stubs of the tubes were left but they never saw a melt go down below.

But I - they're not off the hook though totally because one of the things that I think is bizarre is that the way the MELCOR model - my understanding of it is is when they have vessel failure they assume some particular area and that is based on engineering judgment, in my opinion.

They can - but that's what they always assume and to me it seems like the - why wasn't that varied as a parameter that's uncertain because there's no basis for assuming that other than expert opinion and I didn't see it documented in the story boards that that was considered something worth jiggling around a bit and seeing if it's important or not.

MEMBER BLEY: I'm going to go back to the other one though. You told us about TMI.

MEMBER REMPE: Right.

MEMBER BLEY: But Dick just said that they always kept the instruments in the core and at least some years ago many of the plants that I had looked at do just what John said and they put them in to take measurements and they pull them back out so that they're - so that there's an empty tube down there, which is a different story than the one you described.

MEMBER REMPE: Yes, that's some information that they examine and replace some flood vents.

MEMBER BLEY: I didn't know about the thinning but I did know that they're empty much of the time.

MEMBER REMPE: But yes.

MEMBER BLEY: I mean, empty of instruments.

MEMBER CORRADINI: So I just think - I think at least the report ought to explain the logic of why you ignored it because - or decided by judgment that it wasn't - because I think Joy's explanation is what I remember, which was you had water always down there no matter how long you tried to degrade the core.

And so you pretty much - and I guess I'd - even if I had a thin tube I'm not going to blow those as long as I've got water available.

But once the melt comes down now it's a question of timing. I'm kind of curious and I don't remember in all the various of your many, many runs if you had the steam generator tube ruptured that late in the game.

I thought it was occurring way before I'd have slumping. So if I had it way before slumping I'm not worried about it there. I'm worried about it going somewhere else.

MS. GHOSH: Right. So and let me ask the author. This was a - this was actually a long point of discussion, the instrument tubes. In the

- from the original SOARCA peer review we had discussions with Bob Henry and I believe we had peer review comment and comment responses that are documented.

Unfortunately, I can't remember all of the details of the - what we have documented. But as part of the original SOARCA study -

CHAIRMAN STETKAR: Original Surry SOARCA or -

MS. GHOSH: The Surry - original Surry SOARCA.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: We have Bob Henry's comments and our responses to the comments in the peer review report that's publically available.

But I think it's a good comment that we should repeat the logic here because of - it's a natural question.

CHAIRMAN STETKAR: At least - yes, at least clean up the documentation to provide the rationale that indeed you thought about the instrument tubes.

MS. GHOSH: Yes

CHAIRMAN STETKAR: Yes, in my opinion - okay, you look for a drain line. It doesn't have a drain line but we know that it does have other penetrations in the bottom head and there - the report anyway is silent about them.

MR. FULLER: This is Ed Fuller. I just want to provide a little perspective here. The MAP code does model this instrument tube failure early on and , of course, the model has Bob Henry's official seal of approval on it.

But what happens in the MAP analysis is that it happens very early, shortly after core damage, and then as soon as material starts moving consistent with what Dr. Rempe was saying before, it indeed plugs up -the molten material plugs up the tubes and freezes and essentially it stays frozen through the whole melt progression.

What is important with these instrument tubes is what happens when the core debris gets to the lower plenum and if you model those tubes properly you end up usually predicting with the MAP calculation that they fail first or the - and it would be not at the bottom of the vessel head but on the side somewhere about where the hot spots are.

So it's important but it's not important early when all the zirc oxidation is going on in the core region.

CHAIRMAN STETKAR: Thanks, Ed.

MS. GHOSH: So I guess the last bullet on the slide just denote that the parameters that we considered but didn't include and we integrated uncertainty analysis we listed in the report.

CHAIRMAN STETKAR: Tina, I got sidetracked a little bit there. I was going to ask about the - I think you characterized them as exploratory -

MS. GHOSH: Yes.

CHAIRMAN STETKAR: - parameters.

MS. GHOSH: Right.

CHAIRMAN STETKAR: Is it appropriate to ask about them now or are you -

MS. GHOSH: Yes. We may continue the discussion. But you can ask your question now.

CHAIRMAN STETKAR: Let's bring up - I'm trying to switch gears and look forward. There were two that I had particular questions about and I don't know if you're going to address them later. You probably do. They had to do with the debris, radial and -

MS. GHOSH: Yes. The -

CHAIRMAN STETKAR: - radial actual debris location. Are you going to talk about them later?

MS. GHOSH: We don't go - we don't talk about them because we didn't learn anything new since Peach Bottom. It's - it continues to be something we don't know a lot about, which I believe is why we characterize it as an exploratory parameter because we want to know what the effect of those are but we don't have a whole lot to go on.

CHAIRMAN STETKAR: Well, but now I'll be - I'll wax philosophical. I read the discussions of those and they're much like what you just briefly summarized orally.

They say, well, we don't know very much about this - we're going to take a uniform distribution over an order of magnitude range and I think there's a statement that I hung up on that said something like thus the inclusion of this parameter is really to see what happens when it's varied.

That's not an uncertainty analysis. That is not an analysis. Every other parameter that I read about in the whole study with the exception of those two had technical justification for what is the range of the parameter and what is the shape of the distribution.

One might disagree with that technical justification but this just says well, we put a uniform distribution in there and we kind of let it vary.

Well, if you put a different distribution in there might - maybe it would have been important. A uniform distribution over an order of magnitude is not going to get very many realizations - many samples at extreme values.

If you believe those - I don't even know if you believe the extreme values. So we just put an order of magnitude in and look, it wasn't important. Well, maybe it wasn't important because your range or the distribution that you put in there was absurd.

MS. GHOSH: Yes, and I guess it - you know, it's always a struggle. You know -

CHAIRMAN STETKAR: Well, you have experts.

MS. GHOSH: Right. But if the experts agree that there is uncertainty around the plants and for the most part everybody out there is using point estimates.

But it's an uncertain quantity. You

know, I guess there was a best attempt at assigning some uncertainty to that but -

CHAIRMAN STETKAR: But it isn't in - and my point is philosophically for those two parameters - I read the stories about all of the other parameters. I don't understand the stories because I'm not an expert in every area.

But all of the others with the exception of those two seem to have some reasoned arguments about why we set the lower bound here, why we set the upper bound here and why we ferret in some sort of shape distribution for our uncertainty between those bounds based on technical issues.

Those two parameters have nothing like that. Said everybody uses point estimates, everybody reasoned there's a lot of uncertainty. We stuck in a flat distribution over a nominal order of magnitude and to see what might happen and that's not - I don't get it.

That's completely - philosophically it's not consistent with the rest of what you're calling an uncertainty analysis.

So if you have experts who understand their uncertainty they ought to provide - be able to provide some reasonable input on what the upper and lower bounds could be and there might be some justification for what that is - technical justification - and if they can't agree on a particular shape of a distribution there could be some technical justification of why it is a uniform distribution between those bounds.

MS. GHOSH: Okay. I -

CHAIRMAN STETKAR: Those are the only two exceptions that I could find.

MS. GHOSH: Yes. Well, I'm glad for that.

CHAIRMAN STETKAR: Well, they're the only one that I could find. Other people who are smarter than I am might have -

MS. GHOSH: Okay. You know, I think that's good feedback. We can work on what more we could say about that to kind of bolster the choices. Maybe not all of the expert thinking is documented sufficiently at this point. I don't know. I'm not an expert in that area.

CHAIRMAN STETKAR: I'm not either, obviously. But it - but those two in particular and you said - I think, you know, you used the term exploratory parameters.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: I've forgotten what I wrote in my notes. But from a philosophical standpoint I'm more troubled about the philosophy of saying well, we're going to do an uncertainty analysis by throwing something in there that we know we don't have any confidence in but just to see what might happen.

That's not an uncertainty analysis that you are later than relying on to draw conclusions about offsite risk from - in the context of this particular scenario.

MS. GHOSH: KC, did you want to add something?

MR. WAGNER: I don't know if I'll be able to shed much light on this. I guess I have two comments. Do you see any benefit from having exploratory parameters for guidance MELCOR severe acts than analysis?

CHAIRMAN STETKAR: No, not in the context of this analysis. This is supposed to be a - I'll use the term scientifically-based uncertainty analysis.

If you want to have - if you want to explore MELCOR response to variations in parameter values, do point estimate sensitivity calculations.

Don't call them an uncertainty distribution.

If you want to see how the code responds to extreme values of parameters without assigning a probability to the occurrence of that extreme value, put an extreme value in and see what happens.

That's different from an uncertainty analysis because every other part of this uncertainty analysis has some technical justification whether it's - whether it's data related, experiment related, expert judgment related on the range and the shape of the distribution.

It's not in there to explore how the code is going to respond. It's there to actually inform quantitative results.

DR. SHACK: But isn't an order of magnitude on a point estimate as sort of an expert judgment that the expert - if he thinks the point estimate is a reasonable value an order of magnitude 3.3 either way?

MR. WAGNER: That's the origin of these - the defaults in the code. They're - our scaled experiments don't have the width to do this and so we needed some sort of models so we didn't have stacks of things - stacks of fluid over here in one area and none over here and so we needed some sort of mechanism for relocation.

And expert judgment at the time the models were developed gave us our defaults and we felt it was -

DR. SHACK: I'll disagree with John. I mean, it seems to me if you have no basis for a better distribution I would at least like to see some uncertainty range considered and they were honest in the story board saying we don't have much of a basis for this but, you know, this - there is no real agreement on a thing, you know, and maybe you can find more experts that we - you know, but if the experts if that hard to come by I'm not sure I believe their expert judgment is going to get done.

MR. WAGNER: There's other parameters where we did some of those sensitivity analysis and these probably should have been more appropriately put in there and in -

CHAIRMAN STETKAR: Well, again, I would sort of like to see, you know, maybe both for those kinds of parameters but it seems to me that an uncertainty analysis that pretended there was no uncertainty in this parameter isn't an uncertainty analysis either. Now, that's not - it's certainly not what I'm advocating. I'm advocating an uncertainty distribution that has a technical basis.

We've told the experts, not just we looked at things and experts said well, it's kind of an order of magnitude.

The point estimates -

DR. SHACK: Well, but I'm assuming if they're really going to do that they really did say that the experts had no real basis for a thing.

I mean, yes, I agree. You know, you get one guy in a room that says I don't know the answer - pick an order of magnitude. No. But I'm assuming that they did a - they argued for a while.

MEMBER CORRADINI: But I - but I think that if these are the two that I remember there are no experiments that are large enough that you have a -

MS. GHOSH: That's what KC was just saying, yes.

MEMBER CORRADINI: - that you have a way of determining that.

The CORA experiments, the PHEBUS

experiments are all very small bundles so MAP and MELCOR did marvelously and got really good agreement and then you expand the size of the core and things tend to change. That's my memory. Am I remembering correctly?

MR. WAGNER: Yes, I believe that's written in there that we didn't have any large scale and we measured some sort of model for it and that's why it's in the code.

So we didn't have a strong experimental defensible position that we could put in there. It would have been polling of expert judgment, maybe hand calculations or something of that sort.

MEMBER BLEY: There's a - you know, I saw the story boards and I like that. Getting all the things that could be uncertain down there to think about is important.

There's a lot of experience with eliciting expert judgment that shows if you start at a median or a best estimate as your first estimate you tend to just kind of mush the answers around or you get locked to that if you're looking at what was done before.

The way that generally works best is once you identify the things that could be important you talk people through building the little model of what could make it its worse highest value and what could make it its lowest - estimating those and then putting a best estimate in last rather than first because that really anchors you and creates one of the biggest biases and elicitation there is.

So I don't know how you guys did that stuff. But even if you don't have experiments if you got the parameters - the factors that could make it worse and you try to say how bad could it be and how could it be you tend to do much better than the other way around.

MS. GHOSH: Right, and I think we need to beef up our documentation because even if we - I think there was more thinking that went in than what is coming away - that the reader is coming away with.

So I think we should better that in the document.

MEMBER REMPE: In fairness to you guys, at the end of Section 4.1.2.3 after talking about PHEBUS and CORA they did have this one sentence in there about well, we thought this order of magnitude would take care of partially molten, fully molten.

So I don't know. I wasn't there for your story board but that's what I took away was why they picked that order of magnitude was that they don't have a good basis for picking the value to start off with but they thought that that would take care of partially molten.

But I don't know. That's what I'm reading.

CHAIRMAN STETKAR: Go on. There's nothing more to talk about on that.

MS. GHOSH: This next slide is just the information flow diagram. I'm not going to talk about all of these boxes.

Basically, you have the MELCOR uncertainty engine which takes all of the uncertain inputs and creates the MELCOR input file.

We run MELCOR - that gets fed into MELMACCS to create source terms for MACCS. MACCS takes all of the source terms and matches up one source term with one vector of uncertain MACCS parameters to create the outputs.

This is more about the - how the MELCOR uncertainty application works. I'm not sure that we need to talk about this. It's an elaboration of what was on the previous slide. Nobody has questions.

I think - yes, I think that was -

DR. SHACK: Well, I'll make one comment on the report. To make sense out of that you really have to go back to the Peach Bottom report.

CHAIRMAN STETKAR: Bill, turn it back on again because you turned it off.

MS. GHOSH: Yes. But that's a fair comment too. You know, we got a lot of complaints about the size of the peach bottom document.

So we were trying to gain some efficiency by - we were both trying to better document our thinking on the - you know, the parameter development and at the same time, you know, same some pages where we could. If there are specific things that you think we should re-import back because I know we just refer - either refer back to that report or have a very short summary.

We can add some pages and then we'll have a record of why we're adding it back in. But yes, I apologize. We have to make a judgment call on how much to import into this document, too.

So for the complete story I think you need both the 7110 volume of the original Surry analysis and the Peach Bottom uncertainty analysis with this one. We tried to make it as standalone as possible but you kind of need the back story in there.

DR. SHACK: Well, I mean, you devoted more space to the story board stuff which is, I think, the important part.

I mean, you know, to understand the mechanics you can go back to the Peach Bottom thing. But, I mean, the real story here is how you picked these parameters and you've done a much better job this time, for all the complaints we're going to have.

MS. GHOSH: Okay. So I think that was the end of the overview section.

MEMBER BALLINGER: Can I have one more -

CHAIRMAN STETKAR: Absolutely, sir.

MEMBER BALLINGER: I mean, I've been trying to follow this - because the steam generator two rupture is so important I've been trying to follow the logic through this on the steam generator two rupture part.

And there's some places where there's a pretty good explanation of, you know, why you chose the hot tube and was there a difference between where the hot tube was and where the failure was likely to occur and you say well, you did a sensitivity analysis and there was no problem.

But then you say there's also adoption, include a set of correlations to calculate the direct stress multiplier in two cracking.

There's a strong basis for this method and has been employed during NRC research in the past. Okay, so that says okay, this is important.

However, the use of these correlations will introduce a new set of multiples - uncertain parameters - that would be much more difficult to incorporate into the MELCOR model and was therefore not included.

Okay. On the one hand it's important and on the other hand well, it's too damn complicated to put in there so to heck with it. We won't do it.

But there's no - nothing in between that says this is why - you know, there's a good reason why we decided not to do something that we thought was important before for the following reasons.

So there's - I couldn't find the progression from one sentence to the next because just because it isn't easy doesn't mean that it's not worth doing.

So was there something done in the sensitivity analysis or something to say well, we tried cracking here and it didn't make any difference, sort of like the previous statement with regard to the hot tube.

MS. GHOSH: Yes, I'll take the comment that, again, we should beef up our documentation on that but I don't know if anybody else wants to offer anything at this point.

Or we can also talk about it more when we talk about the MELCOR parameters in the next session.

MEMBER BALLINGER: Yes, that was the next place where it's brought up. Yes.

MS. GHOSH: Yes. Yes. That might be a better time to discuss it more. But at a minimum I think -

DR. SHACK: Yes, I have some comments on that too so I think we need to get to that and can discuss it -

MS. GHOSH: Okay. Yes.

CHAIRMAN STETKAR: Yes, let's table that once we get to that.

MS. GHOSH: Okay.

CHAIRMAN STETKAR: I'm sure there'll be a lot of discussion.

MEMBER SKILLMAN: Tina, before we move on I'd like to ask a question that's been bothering me since we started this session.

Among the event description and the initiated event is the SVO and its loss of all AC and DC. I'm on Page 87, I think.

In the next line, which is almost instantaneous, the MSIVs close. How do they get closed? An MSIV is about as big as a Volkswagen. They're normally powered by 120-volt vital AC. They got a motor about so big. But these are enormous valves.

CHAIRMAN STETKAR: Just for the record, many many main steam isolation valves are pneumatically or hydraulically powered, spring opened - I'm sorry, spring closed hydraulics or pneumatics to keep them open with DC solenoids. My guess is they close on DC here.

MEMBER SKILLMAN: Could be. But it seems to me that this is a very critical assumption.

CHAIRMAN STETKAR: That's my whole point about -

MEMBER SKILLMAN: I got that.

CHAIRMAN STETKAR: - is DC available or

not and what things depend on DC power.

MEMBER SKILLMAN: So it's really the configuration of the plant.

CHAIRMAN STETKAR: Of the plant, and the fundamental assumption of that loss of - if DC is not lost it's not at all clear what the scenario looks like in this plant, whether the MSIVs stay open, whether the atmospheric reliefs on the steam generator cycle, if the MSIVs are closed, whether the condenser steam dumps are available. Probably not, because they're typically nonsafety-related AC.

MEMBER SKILLMAN: What I'm really getting to here is that is a critical assumption in this whole scenario. I spent a lot of time in compartments that are 130 degrees Fahrenheit where these valves are located. Some are electrical, some are hydraulic, some are pneumatic.

But the assumption that these will close is a critical assumption because if they don't close then you've got your entire steam system out there breathing with whatever is occurring from this scenario.

MS. GHOSH: Yes, the - you know, that scenario description dates back to the original

SOARCA study and the plants reviewed all of that as part of the plant fact check.

So that has been confirmed that that would occur and loss of AC/DC power by the intent. So that dates back to the original Surry SOARCA analysis.

CHAIRMAN STETKAR: Thank you. Any other questions for Tina? All I can say is you got off pretty easy today so far. Let's -

MS. GHOSH: That was only the beginning.

CHAIRMAN STETKAR: Yes, that's right. And I'll make the point I always make. None of us have lives so we could be here at midnight.

DR. SHACK: Speak for yourself.

CHAIRMAN STETKAR: Let's take a break and recess until 10:35.

(Whereupon, the above-entitled matter went off the record at 10:18 a.m. and resumed at 10:39 a.m.)

CHAIRMAN STETKAR: We are back in session. KC, just be careful of those mics, they're really sensitive so they pick up rustling paper and all that sort of stuff.

MR. WAGNER: My name is KC Wagner and I'm going to talk about the MELCOR parameters.

So here's the list of the MELCOR uncertain parameters, and based on what we learned at the Peach Bottom uncertainty analysis there was a strong emphasis on looking at valve failures. So on primary safety valves we had not only failed to close but failed to open was considered. In addition to that, similar to Peach Bottom, we looked at high temperature failures of the safety valves, and also due to passing water and so there was really a complete look at both the valves that were working for this scenario which are the safety valves on the primary side and on the secondary. Reactor coolant pump seal leakage, that was varied to look at potential for normal leakage to multiple seal failures. Then we touched on this a little bit earlier; we had a hottest steam generator tube model and that was one of the parameters that was varied based on guidance from the CFD work that Chris White had done. The tube thickness is where we get our stress multiplier that we talked about and you'll probably have some more comments on that.

MEMBER SKILLMAN: KC, before you proceed to the next comment let me ask you a question, please.

MR. WAGNER: Sure.

MEMBER SKILLMAN: I'm reading the text on what is page 402, I think it's A3 or A4, is A4, and here's the question. In the second paragraph on page 402, the words that are presented are these words: "The safety valves on the pressurizer begin opening and closing to remove excess energy." Operative word is safety valve, meaning spring, and probably blow down rate, safety valve. Next sentence: "The pressurizer relief valve flow causes a steady decrease in the primary system's coolant inventory." I believe the second sentence is really clarifying the first sentence but instead of safety valve what is written there is pressurizer relief valve, and I would ask if that's what the author really intended because those are different pieces of hardware.

MR. WAGNER: Now we're talking about --

MEMBER SKILLMAN: Safety valves only.

MR. WAGNER: Same piece of hardware, and Surry has some funny names for these things. We've -- the first sentence would be correct, "spring operated."

MEMBER SKILLMAN: Then I would suggest you might want to take a look at this document and correct that.

MR. WAGNER: Yes.

MEMBER SKILLMAN: That was on page 402 in the second paragraph from the top.

MS. GHOSH: Sorry, is that 402 in the PDF file?

MEMBER SKILLMAN: Yes.

CHAIRMAN STETKAR: Make sure your thing is on.

MS. GHOSH: Oh.

MEMBER SKILLMAN: It is on page Alpha 4, it is the second --

MS. GHOSH: Okay, thank you.

MEMBER SKILLMAN: It is the second paragraph from the top. And I make that comment because the safety valves really are the large spring valves, and a relief valve is commonly actuated by some other medium. Right?

MR. WAGNER: It was sloppy.

MEMBER SKILLMAN: It's only the spring valve that you're talking about.

MR. WAGNER: Yes.

MEMBER SKILLMAN: Okay, thank you.

MEMBER REMPE: So while you're being interrupted, on the reactor coolant pump seal leakage in the write-up on page 4-23, I got confused because some of this work was done a long time ago that you cite before people went to like the improved elastomers on the RCP, and did Surry, did they go to the improved elastomers? Did you use the appropriate data for quantifying reactor coolant pump seal leakage is what I'm curious about?

MR. WAGNER: We used the historical data. They have gone to the new elastomers. They probably are tougher than what is reflected but there was -- we did have good uncertainty guidance for the new elastomers, so --

MEMBER REMPE: So basically you've used data assuming that it leaks more than probably it would leak, and your results are probably conservative is what I should take away from this question.

MR. WAGNER: Right.

MEMBER REMPE: Okay, thanks.

MR. WAGNER: I would note that it's different modeling and the original SOARCA where we had seal failures every -- when the system went saturated near the pump seal we said that it failed, and so that went to the 181 GPM, which was

-- kind of changed the progression of transit. This is improved because there's much lower likelihood that you'd get the seal failure.

These are some of the in-vessel accident progression models that are the parameters that were being adjusted and the Zircaloy melt breakout temperature, that's similar to what you'd seen with Peach Bottom. The molten clad drainage rate is another parameter so after the Zircaloy becomes molten it could break through the Zircaloy oxide crust, and how fast that flows out was an uncertain variable.

Our two exploratory parameters which were the radial solid and molten debris relocation. The time at cycle was a huge undertaking and a big change for -- and Surry I believe is the only UA that will have done it. Peach Bottom didn't do it, and Sequoyah isn't, but we looked at time of cycle and so beginning of cycle, middle cycle, and end of cycle.

MEMBER CORRADINI: So can I take you back to the third -- on the right-hand side the third and fourth bullet? You picked those because as we had discussed previously is that there is no experimental experience, and heuristically things ought to level out whether it's solid or liquid between the various radial rings, so that's why you picked it.

The crosswalk between MAAP and MELCOR also identified the size of the debris and the porosity factor in terms of how many gases come through in hydrogen production. At least that's my memory for the crosswalk, and yet those don't show up here. It seems to me the debris size and the porosity, the allowable porosities -- again, I'm remembering. If I remember correctly, in MELCOR you can never get down to no porosity, you can never block a radial ring. You always can have some flow through. Was those two thought about and just thought not to be enough uncertainty to do it and the values you guys default use because to me those seem more uncertain and the crosswalk identified them as two things that made MAAP and MELCOR evolve differently. And maybe it's just timing of when you chose this and did the analysis versus what the crosswalk found out. Do you understand what I'm asking?

MS. GHOSH: If I could just insert. I'll let you answer, KC, but a lot of the same people --

MEMBER CORRADINI: Oh, yes.

MS. GHOSH: -- working on this -- you know, were also involved in the crosswalk so they were very much aware of the findings from that effort, as well. And I believe that, you know, the outcome of which parameters we ended up with was a result of the totality of their thinking which would have included what they had been discovering in, you know, the MAAP/MELCOR crosswalk discussions. KC, if you want to elaborate on that.

MR. WAGNER: I don't have anything else to --

MEMBER CORRADINI: So that was my polite question. My impolite question is, I'm still struggling since MAAP and MELCOR show dramatically different results because of those two parameters, I'd expect they'd be here. If they're not here, the reason they're not here is?

MS. GHOSH: One of the discussions we had ongoing as a team is that with MELCOR often you can use different parameters to get at the end effect of a particular set of processes. And I don't know the specific details of this one, but I know that we went through the thinking of, you know, there may be 10 parameters that you could actually vary that would get at the same variation in a particular set of processes, and we end up varying a subset of those just because it's simpler and we think that we can get at the end effect of, you know, that set of processes. So I think some of that thinking went in this. I can't recollect the specifics of the two parameters that you mention and how it fits into this set, but --

MR. WAGNER: Yes, so maybe it has to do with how the models work, also. We have -- we don't have as much control over the porosity. We have a size dimension that's kind of been selected to be characteristic of EO2 pellets for the oxide when it drops down there, but when we have molten metals they fill in the interstitial spaces and it will fill up and reduce the porosity. I would assume MAAP has a similar model.

MEMBER CORRADINI: MAAP can go to zero. As far as I understand, MELCOR cannot go to zero.

MR. WAGNER: The only reason we don't go to zero --

MEMBER CORRADINI: A block. I guess what I'm trying to get at is -- so let me tell you the observable.

MR. WAGNER: Okay, sure.

MEMBER CORRADINI: And then I'll --

MR. WAGNER: Sure, that would help.

MEMBER CORRADINI: So the observer was a great difference in hydrogen -- the amount of hydrogen generated and a very large difference in the melt temperature upon this stuff coming into the lower plenum.

MR. WAGNER: After it's in the lower plenum?

MEMBER CORRADINI: No, the initial temperature that it enters the lower plenum is quite different because of these -- potentially these two parameters.

MR. WAGNER: Oh, so to be clear, are you talking about the porosity in the lower plenum or the porosity --

MEMBER CORRADINI: No.

MR. WAGNER: -- above the core plates?

MEMBER CORRADINI: Yes, the thinking at least -- again, I had to go back to the crosswalk report, but my understanding is in the crosswalk report the estimate from the teams were that with the presence of porosity I keep on producing a lot of hydrogen because steam can still flow through in the MELCOR simulation, and it keeps it cool enough so then when things start slumping it slumps with lower temperatures and a lot of hydrogen. And MAAP is -- will just say 180 degrees opposite, which is not very much hydrogen but very hot because it sits there bottled up and then when it slumps it comes down quickly and much hotter. So my question is, that seems to be a big uncertainty and it wasn't here.

MEMBER REMPE: But that was for a BWR in a dry event. Right?

MEMBER CORRADINI: Well, this is a drill down. This is a station blackout in both cases. So I've asked my question.

MR. WAGNER: Yes. We looked at -- the things that we looked at were prior to that configuration as you can see. We looked at the Zirc melt breakout temperature and the drainage rate. We didn't -- we don't have that in there.

MEMBER CORRADINI: But my only guess was, is that because, as Joy said, the BWR -- the crosswalk was on a BWR and so the differences here might have been smaller. So I guess maybe you did some side calculations that said this is not a big deal here because it's a PWR geometry.

MR. WAGNER: Yes.

MEMBER CORRADINI: That's the only

justification I could guess that these don't show up in your list, that's all. Am I making sense?

MR. WAGNER: Yes, you are.

MEMBER CORRADINI: Okay.

MEMBER REMPE: But it would be good to document it, why some of these parameters are not considered.

MR. WAGNER: Yes, there was a whole host of ones that --

MEMBER CORRADINI: I'm sure, yes. I know there's a lot of them.

MR. WAGNER: Yes, and that's certainly an area that I know was thought about, but --

MS. GHOSH: Yes. Unfortunately, the person who could probably best answer everything with respect to that is in bed quite sick and I don't know if he's listening on the phone. We may be able to get you more details later today but whether --

MEMBER CORRADINI: That's good. I wanted just to state it because you have in your thinking process --

MS. GHOSH: Your mic.

MEMBER CORRADINI: I'm sorry, I'm off. That you boil it down to like four or five classes of uncertainties, and the in-vessel class is the one that has a lot of parameters, and it's hard to tell which ones you pick and why you pick them as to uncertainty. So I was just -- those two were missing, and I just wanted to at least get a story as to why they've been set aside.

MR. WAGNER: Maybe -- the one place it did come in, so I guess I would add, is on the eutectic temperature which is this --

MS. GHOSH: The last bullet there.

MR. WAGNER: Yes, the last bullet. At the temperature that the Zirc and the EO2 form eutectic and molten, and that changed the characteristic of the core which would lead to much more blockage to the extent that MELCOR is able to calculate that. We have troubles when there's no volume, CVH can't converge or it runs -- it causes numerical problems as we approach, you know, completely filled cells. But certainly that eutectic temperature changed the characteristic of the melt that was going down to the lower plenum.

You see that on a hydrogen generation. You'll see some pretty low values and those were with low eutectic temperatures where a big chunk of the core is much more molten than say if that eutectic temperature was higher. So probably that's

-- recalling that that's probably the way we -- since that was such a big nod there, that was probably how we were looking at, you know, a variance in that.

MEMBER REMPE: A couple of questions before you leave it. First of all, you said today well, we're not doing the time in the fuel cycle for Sequoyah. But as I recall, this report said that was important so why is that not being done for Sequoyah?

MR. WAGNER: I think I'll come back to that, if you don't mind. But the MOC and the EOC were not a whole lot different. Where we picked the BOC was substantially different, but you -- and I think we calculated in about 30 days your -- that BOC is starting to look like an MOC, as far as --

MEMBER REMPE: With a transient this is important.

MR. WAGNER: Yes.

MEMBER REMPE: Is the short-term answer. The second thing is natural circulation. Why was that decided that isn't -- to me there's limited testing that people have for that, and I'm surprised that wasn't something people decided to use as an uncertainty parameter.

MR. WAGNER: We addressed that in two fashions. We -- from original SOARCA to the UA, Chris Boyd's 1922, NUREG-1922 report came out, and so he had much better guidance for us, so we updated it based on that, so we are -- what his recommended values were. And we -- rather than -- so we -- based on that we could have -- we looked at his comments in the report and he varied a bunch of boundary conditions in his CFD work, and the conclusions were the recirculation ratio which is the ratio between the flow in the hot leg and the tubes didn't change very much. The Drake coefficient that he calculated didn't change very much. The size of the hot spot didn't change very much, and so -- but there was a little bit of a jumping around of the hottest tube, and that was my impression, I was the one that wrote this one up. There was the most uncertain variable, so it was a strong effort because we're worried about correlation to kind of pick maybe the key parameter that was -- could capture the most things, and that was our -- how hot is that hot plume that's entering into there, so that was what we picked. And then it was kind of geared towards looking for SGTRs, and then the thinning of the tubes would have been the other parameter. So, we did give it quite a bit of thought and we settled on we're just going to vary that normalized temperature of the hottest tube.

MEMBER REMPE: Okay, thank you.

MR. WAGNER: Ex-vessel accident progression. We wanted to explore the possibility of hydrogen burns after say hot leg failure or the PRT fails on the pressurizer, the rupture disk fails on the PRT, because in the original SOARCA we got into steam inerted, and then we became oxygen limited as the non-condensable gases from MCCI pressurized the reactor, or the containment. So we were looking for the lower flammability limit and so that was selected as one of the parameters to explore under the 10 percent which was used in the original SOARCA to look at flammability at lower levels, so that was an uncertain factor.

SGTR, we did the uncertainty of the location because that affected the decontamination of the aerosols in the secondary side, and so that was another parameter.

On the containment behavior there was two related to leakage and failure. One was the design leakage, and I don't know if we talk about that one later but tech specs allow above the .1 percent per day for certain periods of time under certain situations, so we wanted to explore higher leakage that could be allowed and see what its impact was. And then there's an expert judgment process based on scaled experiments to go to predict the fragility code for the containment, and we wanted to explore one of the parameters on when the containment liner would rupture, and so that was an uncertainty parameter.

The condensation which kind of fed into whether there was a potential for a hydrogen burn, the assessment primarily was contained in the DBA work. They found out that the condensation coefficient for inside the containment was low with the correlation and MELCOR is using the same correlation, so in contain based on their DBA work they had increased that. So based on those insights and some uncertainty and condensation heat transfer that became an uncertain parameter which might create conditions where hydrogen burns might be possible, so we wanted to make sure that we explored some of the parameters that might lead to a containment failure.

The radionuclide part, the RN, I guess we didn't define that in the report. I need to fix that, but that has to do with our radionuclide transport. There was two parameters related to iodine and cesium, and this was another kind of ambitious undertaking because although it sounds rather straightforward, it was a lot of code input to implement these. But we looked at the amount of iodine gas that was present and so that was an uncertain variable, something that wasn't considered in the original SOARCA. And then the chemical form of cesium, and that's changed a lot over the years based on insights from PHEBUS going from I think some of the original Reg Guides, it was primarily a cesium hydroxide, and now we agree that it's more cesium molybdate based on evidence from PHEBUS, but cesium hydroxide is another possibility. Their vapor pressures are radically different and their mobility, and so we wanted to make sure that we explored some variability of the compound makeup of the cesium.

And then finally, this was also in Peach Bottom, and I think is going to be in Sequoyah, is the dynamic shape factor, and that's the aerodynamic shape factor for aerosols. So when the aerosols form they make chains and they settle differently than the spheres which is the default in MELCOR, and so that was explored as an uncertainty parameter.

CHAIRMAN STETKAR: I'm trying to look ahead. You don't have another slide on the chemical forms, do you?

MS. GHOSH: I don't think --

CHAIRMAN STETKAR: You're not planning on discussing that.

MS. GHOSH: Yes, sorry. We didn't include that this time.

CHAIRMAN STETKAR: I'm going to -- I'll telegraph. I'm going to beat you up an awful lot on the valves which you do have slides on.

MS. GHOSH: Okay. But first you'll beat us up on --

CHAIRMAN STETKAR: Unless you tell me to wait until a later slide, and I haven't heard that.

MR. WAGNER: No, this would be the time to talk about --

CHAIRMAN STETKAR: Oh, all right. Let me -- I had a question. The -- I'm not a chemist. Dana isn't here. You refer to the PHEBUS experiments as evidence for the distribution that you used. That distribution is -- in the study is capped at 3 percent of the iodine inventory. I looked at the figure, it's Figure 4-38 from the PHEBUS experiments for low enriched fuel and it seems to show quite a number of data points at higher than 3 percent for low enriched fuel at burnups around higher than about 50,000 megawatt days per ton. What's the current burnup at Surry?

MR. WAGNER: I think it's around 42 or so.

CHAIRMAN STETKAR: 42, so it's below that 50, okay. Because you made reference to Surry's specific burnup information but you didn't cite what it was, or at least not in the part that I read. And that's the primary justification for essentially disregarding those higher release fractions?

MR. WAGNER: Yes.

CHAIRMAN STETKAR: Okay.

MR. WAGNER: So we did a fit to it and --

CHAIRMAN STETKAR: But it accounted for a burnup lower than -- it's about 50,000 where you start to see additional releases. Okay, thank you. Okay, I'm done.

MEMBER CORRADINI: So you're done going through the parameters. So now you took these parameters, ran through Monte Carlo and came up with 1,200 samples. Did you -- I was going to use the word "unscramble," but the character of the parameters are different in the sense that they're sequence parameters, they're in-vessel parameters, they're -- so that it doesn't surprise me that initial conditions and boundary conditions dominate, so if I have the different initial condition or boundary condition and I've got a tube that breaks, that's really a big deal.

Is there a way to unwrap the 1,200 or 1,000 successful ones so that I ought to look at what's important in in-vessel progression given a sequence, or ex-vessel progression given a sequence in in-vessel. You know what I'm asking? In some sense, I was looking at your summary table and the things that drive it don't surprise me, but yet all the rest are kind of in the noise, so do I interpret that everything is in the noise?

MS. GHOSH: We did try to do subsets of results in the regressions.

MEMBER CORRADINI: Yes.

MS. GHOSH: Yes, the way -- actually, there's a slide at the end of this little --

MEMBER CORRADINI: Okay, that's fine.

MS. GHOSH: -- assessment that I think

explains it.

MEMBER CORRADINI: All right. So if you bring it up there, then I'll ask my question.

MS. GHOSH: Yes.

MEMBER CORRADINI: That's fine.

MS. GHOSH: Just for example, we looked at steam generator tube ruptures just by itself and then the non-SGTR just by itself, but we'll get to it.

MEMBER CORRADINI: Okay, that's fine. I'll wait. Thank you.

MR. WAGNER: Those are many UAs to focus in on those. I'm a little nervous now.

(Laughter.)

CHAIRMAN STETKAR: Be very afraid but launch into it.

MR. WAGNER: Because this is not my area of expertise but Tina is going to help me and others.

So the way the safety valves can operate and fail have all sorts of possibilities, and because we're doing the stochastic sampling on them we get a lot of different possibilities that needed to be considered. In most cases the failure to close occurs on the lowest safety valve because they each have their own set points. And, in general, we only operate in a one valve at a time whether it's the primary or the secondary. And so that valve would do its thing and then we would move on to the next valve, if needed, depending on what happened with that valve. So the things that could happen with that valve, we didn't have any thermal failures in the valves.

MS. GHOSH: Right. We modeled it but we didn't see any --

MR. WAGNER: Okay, so --

CHAIRMAN STETKAR: KC, you mentioned -- I was going to bring it up later, but I might as well do it now. You mentioned that you didn't even consider thermal failures of the main steam safety valves because -- I'll paraphrase because I lost my note. Because they always operate at design conditions. That's not true once you get hot gases ejected through the ruptured tubes, is it?

MR. WAGNER: At that point we weren't on the valves any more.

CHAIRMAN STETKAR: Well, how does the model -- this is something I don't know about the model. When you model releases now through the failed steam generator does the release fraction depend on the stuck open area of the safety valve? In other words, this parameter, SVOAFRAC or something like that, does the amount of release depend on the value of that parameter?

MR. WAGNER: Not very much. That didn't --

CHAIRMAN STETKAR: Not very much. Well, why?

MS. GHOSH: Yes, there is some --

MR. WAGNER: Some dependence.

MS. GHOSH: -- dependence. There's some dependence.

CHAIRMAN STETKAR: Well -- but if it does depend on that and if the valves are likely to fail open due to high temperature conditions, couldn't that change the nature of the releases substantially, if that SVOFRAC value is simply based on assumed normal operation of the valves?

MS. GHOSH: Okay. If I -- I don't know if I -- I'm not sure I understand your question completely.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: But I think --

CHAIRMAN STETKAR: Let me walk you through the scenario.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: A core damage occurs, hot gases go into the steam generator tubes, tubes fail, hot gases are now released into the secondary side of said steam generators which are pressurized to the steam generator safety valve set points which are cycling and are failing with some likelihood. If they fail they have some assigned open fraction to them which is an uncertain distribution. My question is will those safety valves be exposed to temperatures above their design ratings during these particular scenarios with the release, and would that affect -- would that high temperature condition affect either -- affect both, the likelihood that they stick open and the open area if they stick open, and then subsequently the amount and timing of the release?

MS. GHOSH: We did model the thermal seizure and we varied the thermal seizure criteria --

CHAIRMAN STETKAR: You did not for the secondary safety valves.

MS. GHOSH: Okay, okay.

CHAIRMAN STETKAR: That's what I'm talking about.

MS. GHOSH: Yes. I mean, we have the safety scale from overcycling at a lower number of cycles and we would have --

CHAIRMAN STETKAR: That was before the release though. Right?

MS. GHOSH: Yes.

MR. WAGNER: Yes. So I think --

CHAIRMAN STETKAR: When I say "release," before the release into the steam generator.

MR. WAGNER: It's a fair question, and for completeness maybe it should have been done. But in practicality the situation didn't arise. We had already dried out the steam generator and that's what leads us into --

CHAIRMAN STETKAR: I know how you got pressure low in the steam generator, how you got the high dry low condition.

MR. WAGNER: Well, so we had already cycled and dried out the steam generator --

CHAIRMAN STETKAR: Right.

MR. WAGNER: -- which in most cases led to valve failure of some area under weighted conditions.

CHAIRMAN STETKAR: Okay.

MEMBER CORRADINI: So before degradation

in most cases --

MR. WAGNER: Yes.

MEMBER CORRADINI: -- the valve cycling popped open and stuck at some fraction.

MR. WAGNER: Yes.

MEMBER BLEY: Essentially guaranteed stuck open after somewhere near 100 cycles.

MR. WAGNER: Yes.

MEMBER BLEY: Yes.

MR. WAGNER: So the generator dries out at an hour and 10 minutes or so, and that's the strong cycling. There's some continued cycling just to the heating of the gas that's in there. That was a lot of cycles getting to that point, and most of our calculations had -- on top of that we also had leakage around the MSIVs, and so that by itself would depressurize the system.

CHAIRMAN STETKAR: I guess what I'm trying to probe is, though, how the safety valves were modeled after the tube failure, and how those models might or might not affect the amount of the subsequent release. And I just don't know, because I --

MR. WAGNER: So what happened in the evolution of the accident there, most of the time the valve was in whatever position it was because it had failed. We have some leakage that's going on around the MSIVs. The particles that we're worried about, this is not addressing your gas problem just yet, but the particles that are coming through are all the small ones.

CHAIRMAN STETKAR: Yes.

MR. WAGNER: Because now an improvement through this version was size dependent decontamination factor.

CHAIRMAN STETKAR: Yes.

MR. WAGNER: And based on the hardest test. And so all the small particles we calculated at BF, but they're all floating through and they get out whether it's through that MSIV leakage, or whether it's through the stuck open valve. So you look at the range of results that we have and they're an order of magnitude higher than without the tube ruptures, but there isn't a lot of -- I mean, there's a decent variance there but those small particles get out. And once they leave the MSIV we conservatively put them in the environment.

CHAIRMAN STETKAR: Okay.

MR. WAGNER: They're gone at that point and available for --

CHAIRMAN STETKAR: If I can just make sure. Essentially, you're saying that for practical purposes everything that comes into the steam generator for most of the realizations goes out to the environment. Is that --

MR. WAGNER: Yes.

CHAIRMAN STETKAR: Am I correctly interpreting what you said?

MR. WAGNER: Except for the big aerosols that have some impaction, you know, on the tubes just coming through the break.

CHAIRMAN STETKAR: Right. Okay. Okay, thanks. That isn't what I was going to beat you up on about.

MR. WAGNER: No, I'm sure there's more coming.

CHAIRMAN STETKAR: That was just education. Thank you.

MR. WAGNER: So I'll give a rudimentary description of this graph which I think makes sense, so that as valves fail you have the ability to move on to other valves. If that's sufficient to remove the energy that needs to be removed from the system then no more valves open. There is a possibility that all the valves could fail to close, or fail to open and that possibility is included in there. And that would be State 5.

Well, what is that possibility of the fail to open? It turns out that it's pretty low. We did 100,000 samples looking at this and we didn't find any. It doesn't mean that it couldn't happen, but its likelihood is pretty low, and it's most likely that we're going to have one or more valves stick open. Now these are distributions.

CHAIRMAN STETKAR: Are you going to say any more about that? Yes you are. I had questions -- we discussed safety valves whenever it was on Peach Bottom, and some of my same questions apply today. But I thought about them a little bit more in the context of a pressurized water reactor which I'm more familiar with.

You have a table 4-2 in the report that essentially shows the data that you used plus the parameters of these particular distributions. And there's some discussion of why you selected what you selected, and why you discounted stuff that you didn't use. In effect you said well, we looked at -- I think you got the reference wrong, go check your references because you refer to one NUREG that -- you refer to an Appendix A.2.42 of NUREG/CR

7037, and I think you're actually looking at Appendix B to that report, but that's okay. I mean, that's a bookkeeping thing.

You said, essentially, that you looked only at safety valve failures after scrams, that you discounted test data because you felt that the test data was not prototypic. Okay, my first question is why is test data not prototypic for behavior of the valves? Because there were failures?

MR. ROSS: Could I make a comment?

CHAIRMAN STETKAR: Yes, come on up and comment on it.

MEMBER CORRADINI: You have to identify yourself and speak with sufficient clarity and volume.

CHAIRMAN STETKAR: To be readily heard.

MR. ROSS: I'm Kyle Ross with Sandia Labs. Yes, the body of data in the two components. One was testing, one was actual response to a scram.

CHAIRMAN STETKAR: Yes.

MR. ROSS: And the cycles, or the failure probability was quite a lot larger for the actual events, the testing.

CHAIRMAN STETKAR: I'm sorry, it was the

other way around.

MR. ROSS: Well, the testing of --

CHAIRMAN STETKAR: For failure to open.

MR. ROSS: The testing valves cycle longer before failing.

CHAIRMAN STETKAR: Yes, they have fewer fail -- it depends on how you characterize what you're calling a failure. The time to failure was longer or the number of failures within a given time period was lower.

MR. ROSS: Yes, yes.

CHAIRMAN STETKAR: Now my question is the data are -- and you just said the data that you used are from demands on pressurizer safety valves after a reactor scram. It's really, really, really, really, I'm not going to belabor it any more, difficult to get a demand for a pressurizer safety valve to open after a reactor scram. It like never occurs, and yet somehow you counted up 773 of these things. I would be really curious where those 773 demands of pressurizer safety valves came from, because the experiments might be like zero failures and zero demands, not zero and 773.

MR. ROSS: Yes, so those words are errant. The database for the valves included those secondary and primary valve --

CHAIRMAN STETKAR: Oh, both secondary and primary, but you only included primary in the numerator, didn't you? Because you didn't take secondary safety valves because they're not published for pressurizer water reactors in that report. They're published for BWRs and they've had failures, but you didn't use BWR valves. They had failures and more demands. So why are you cooking the data?

MR. ROSS: Well, there's certainly no intentional cooking of data.

CHAIRMAN STETKAR: Okay, but as best as I can tell it was cooked pretty strongly. You took a large number of demands of safety valves that cannot be justified from operating experience. You took zero failures, you discounted test data because you said well, the failure rate is higher during test data which is the only data that I have for pressurizer safety valves, and I can't reproduce -- I can't even find the failure to close data that you used because I can't find it published in that report anywhere. All of these parameters are really important to these results.

MR. ROSS: Well, I mean, it sounds like

what -- you're very knowledgeable, but the difference for my interpretation of those NUREGs.

CHAIRMAN STETKAR: Okay. Find me the evidence in the NUREGs of the 17 failures to close and 773 demands on pressurizer safety valves.

MR. ROSS: Okay. I --

CHAIRMAN STETKAR: Please, and I'd like if you can before the end of the day, I'd appreciate that. And find me where -- I know where you got the 773 demands. I read that in a table. I don't believe that. it's published as demands of pressurizer safety valves after a scram. I fundamentally --

MR. ROSS: And it's not. They have errant words there. It was for safety valves period being whether they're on a steam generator steam line --

CHAIRMAN STETKAR: Well, the cited NUREG does not contain data for steam generator safety valves. I couldn't find it.

MR. ROSS: No, but I believe it has a statement that says the valves are not distinguishable.

CHAIRMAN STETKAR: But it doesn't include the numerator or the denominator for the data for those valves, I don't believe.

MR. ROSS: So it's been a year or so since I looked at these NUREGS, but I have the ability to look at them again --

CHAIRMAN STETKAR: Okay. I think just take -- we don't need it but take that away because the way that the data are presented in this report -- and, in fact, I raised this when we were talking about Peach Bottom. The data in the cited NUREG are in many cases contrived, and as best as I can tell you simply took the data as if they're fact and said well, we're going to create a beta distribution about that evidence.

MEMBER BLEY: I think what John has suggested is a good idea, but lest you go overboard, I would caution that if you go to test data you really understand it because most test data I have seen on safety valves report a failure if you don't lift by some percentage above the set point, where here you will drive it well above that point such that usually that's a small adjustment and it would have lifted a little bit later, so you can go way too far the other way.

CHAIRMAN STETKAR: Yes, and I'm not advocating necessarily including all of the test data --

MEMBER BLEY: But reacting to your comments that get us there.

CHAIRMAN STETKAR: In particular for the failure to open, on the other hand the stuck open, the test data might absolutely be valid.

(Simultaneous speaking.)

CHAIRMAN STETKAR: Because once it opens --

MEMBER BLEY: Then it's absolutely valid.

CHAIRMAN STETKAR: But I have real questions about the data and how well you probed that data. The other question that I had, as long as you're taking notes, is -- and I agree with you completely that a spring-loaded safety valve is a spring-loaded safety valve, code spring-loaded safety valve, shouldn't make any difference whether it's on a steam generator, whether it's on a pressurizer, or whether it's on a boiling water reactor. To increase the population of both numerator and denominator in terms of your experiential evidence to inform the uncertainty distributions did you think about using valve data also, spring-loaded safety valve operation data from boiling water reactors, because the same NUREG does publish data about BWR safety relief valves operating in the safety mode, not the pilot relief mode. They distinguish between that, both testing and post-scram data. And, in fact, on boilers there are actually more legitimate demands on those valves. So the question is, you know, in terms of the distributions are based -- for failure to open are based on zero and 773 with a data distribution fit around that. And for failure to close, stuck open is 17 failures and 773. I couldn't find the 17 stuck open failures. That may be too high.

MR. ROSS: I'm confident I can find --

CHAIRMAN STETKAR: The answer smells about right, but I couldn't trace it back.

MR. ROSS: Yes, I'd be glad to find that number.

CHAIRMAN STETKAR: That would be great.

MR. ROSS: Yes.

CHAIRMAN STETKAR: That would be great.

MS. GHOSH: Yes, so we'll follow-up on that. Just to repeat what Kyle said, there was no effort to cook the data. You know, and I know we had extensive discussions about this as a team. Unfortunately, it was two and a half years ago, so it's a little difficult to remember all the details of our discussion, but we did put a lot of thought into this because we knew it was going to be important. It turns out to be important.

CHAIRMAN STETKAR: Yes.

MS. GHOSH: We just need to do a better job of documenting where the numbers came from and say more of the why.

MEMBER BLEY: If it's a citation problem, let us know. I think the first part of John's comment you won't find it that way. If you end up having trouble here, I remember there was a big EPRI program doing a lot of testing. I know it was on PORVs in the '80s. I don't remember if they did safety valves, too.

CHAIRMAN STETKAR: They mention that but it's -- but only in passing. And ostensibly this NUREG/CR-7037 is much more recent than that, and it compiles much more experience data from actual demand. And it actually is a pretty thorough reference for safety and relief valves and all that kind of stuff.

MEMBER BLEY: Yes, and I --

MR. FULLER: This is Ed Fuller. The -- I'm familiar with this, with that program. There were a lot of safety valves tested back in the '80s, and we originally when I was at Pulstar, we did the steam generator tube integrity risk assessment work. We actually have a separate report on what we did investigating those valve failures. So I somewhere still have some of that stuff.

MEMBER BLEY: You might want to share it with your colleagues.

MR. FULLER: But I don't know where some of it is right now. By the way, I did share it with them two and a half years ago.

MS. GHOSH: Yes.

MR. WAGNER: Okay. Ready to move on?

CHAIRMAN STETKAR: Is it appropriate to discuss now how the sampling algorithms for these various failure modes were implemented in MELCOR, or do you want to wait?

MS. GHOSH: I think it's fair to discuss it now because we don't have more detailed slides on that.

CHAIRMAN STETKAR: Yes.

MS. GHOSH: Maybe -- I don't know which side is better, this data diagram or the distributions? But as I mentioned in the first overview talk we implemented both an epistemic and aleatory aspect of the safety valves behavior, so I think the more complex --

CHAIRMAN STETKAR: Let me -- when -- I know you make a lot of points about the difference between aleatory and epistemic uncertainty. Let's just say you implemented uncertainty for the valves because valve failure data include both epistemic and aleatory, and it's really, really difficult to separate those two. So you had an uncertainty distribution.

MS. GHOSH: We had an uncertainty --

CHAIRMAN STETKAR: It is what it is.

MS. GHOSH: But the -- okay, I guess the reason I bring it up, the way that it was implemented was to take a two-step process to both first assume a sampled failure rate, and then given that failure rate how many cycles the three valves would experience were they called upon to cycle a certain number of times. And then for a given sampled failure rate the cycles to failure for those three valves were different which represented the aleatory nature given a failure rate that you know or is true, how many cycles you could actually experience.

CHAIRMAN STETKAR: Let me probe that, and it was easier for me to frame my question if I think about the steam generator safety valves than the pressurizer safety valves, because the -- my question about the sampling algorithm applies equally to both of them.

On the steam generators first principles, all three safety valves should be challenged to open at the same time. Right?

MS. GHOSH: Yes, and --

CHAIRMAN STETKAR: I mean, they're not --

MS. GHOSH: We model one valve per steam generator.

CHAIRMAN STETKAR: Yes, you're modeling the lowest set point valve.

MS. GHOSH: Right, that's right.

CHAIRMAN STETKAR: And that's why -- you know, you don't run into the physical complications on the pressurizer where you have staged --

MS. GHOSH: Right.

CHAIRMAN STETKAR: -- pressures. So if I now think of three nominally identical valves cycling open and closed, and I have a single state of knowledge uncertainty distribution for the failure rate of those valves. So if the world works according to Sample 1, I should expect each of those valves to have failure rate 1X. If the world works according to Sample 2, I should expect each of those valves to have failure rate 2Y. It's a different sample, a different failure rate, but why would I believe that each valve would have a different failure rate? It may fail independently in a logic model sense --

MS. GHOSH: Right.

CHAIRMAN STETKAR: -- so it's X cubed or Y cubed to have all three fail.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: But not different failure rates.

MS. GHOSH: They shouldn't -- in a given Monte Carlo realization where we're fixing a state of knowledge, they should have an identical failure rate.

CHAIRMAN STETKAR: Okay, but that's not what I understood by the discussion about the sampling algorithm because it says that the failure to close distribution for the valves is independently sampled three times to obtain the numbers NSGAI, NSGBI, and NSGCI of demands for which the lowest safety valve on steam line A, B, and C. That leads me to believe that three separate values were used.

MS. GHOSH: Right. And, actually, maybe

Dusty can help me out --

CHAIRMAN STETKAR: They're not correlated.

PARTICIPANT: We have Matt on the phone.

MS. GHOSH: Oh, Matt is on the phone. Okay. Yes, and I may have misspoken. Maybe that was an attempt -- because I think we did a little bit different modeling on the pressurizer side, which I'm more familiar --

CHAIRMAN STETKAR: Yes, let's talk --

MS. GHOSH: -- with than the secondary side. So on the secondary side, we think -- so we have some additional members of the team who are on the bridge line. I don't know how difficult it would be --

CHAIRMAN STETKAR: We can get the bridge line open if they can add --

MS. GHOSH: Yes, because we have Matt Denman from Sandia who actually implemented.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: Made all of this come to pass in terms of the sampling, so he may be the best person to have --

CHAIRMAN STETKAR: The second part of the question while we're trying to get that open is that the discussion about the sampling -- and this is for the -- I think the line is open, for the fraction. It's open. There's some discussion about well, we use the fraction open for valves in number one and three, and we took the complement of the fraction open for number two because we didn't -- we wanted to avoid something or other.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: And that completely confused the heck out of me.

MS. GHOSH: Right.

CHAIRMAN STETKAR: But let's get down to the first question about whether or not --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- the three valves were correlated. And in my mind it would also apply to the pressurizer safety valves despite the fact that they open in a staggered -- you know, the second one isn't challenged to open unless the first one is -- fails to open. There are two, though. If the world works according to our state of knowledge sample 1, then all three of those valves ought to have failure rate 1X.

MR. DENMAN: So this is Matt Denman at Sandia.

CHAIRMAN STETKAR: Yes.

MR. DENMAN: Can you hear me?

CHAIRMAN STETKAR: Yes, we can.

MR. DENMAN: Okay. So in any given MELCOR simulation there was a draw of what we were calling the epistemic failure rates for the valves, and then that epistemic failure rate fed an aleatory negative binomial distribution which was sampled three times to give you your three number of cycles until failure within that MELCOR simulation. And then the next MELCOR simulation, a new epistemic draw was taken and fed a single negative binomial distribution which had three aleatory draws from that negative binomial distribution. So for any given MELCOR simulation all of the steam generator safety valves, all of the safety valves on the primary system --

MS. GHOSH: So I think, John, this goes to the differences in how we were discussing the world -- I'm sorry. I'm sorry. I think because in your mind you are mapping everything onto one uncertainty as you called it. We were attempting to model both the epistemic and random aspects of that parameter which, of course, at the end of the day if you think in composite you can come up some composite, you know, distribution of what it would look like. But the way we implemented it, the reason you have the different cycles to failure of the three valves is because you start with one epistemic failure rate, but then you model the fact that random -- you know, the random aspect that given that failure rate you could have different cycles to failure.

MEMBER BLEY: Failure and time is random given the failure rate. That's the way it --

CHAIRMAN STETKAR: How did you divine the failure in time though?

MS. GHOSH: Well, in this case it's demand -- on demand --

CHAIRMAN STETKAR: This is number of cycles because --

MS. GHOSH: Number of cycles --

CHAIRMAN STETKAR: -- inverse of the failure rate, isn't it?

MR. DENMAN: So the inverse of the failure rate is the average, or is the mean of the negative binomial distribution.

CHAIRMAN STETKAR: Okay.

MR. DENMAN: But the negative binomial distribution is a distribution. It's not just this mean.

CHAIRMAN STETKAR: Where is that negative binomial distribution documented in the report? I didn't -- I missed it.

MR. DENMAN: I mean, we can try to find that for you.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: If you could, I'd appreciate it. I mean, you know, we're going to break for lunch sometime, so you don't have to try to do it in real -- I'd really appreciate that because I get what you're doing if I had seen it described that way with that binomial distribution for the aleatory variability in the number of cycles given the -- and you're saying you selected the same -- the same failure rate applies for all of the valves. Right?

MR. DENMAN: Within a single MELCOR --

CHAIRMAN STETKAR: Within a single MELCOR simulation.

MS. GHOSH: Right, right.

CHAIRMAN STETKAR: Huh. Okay.

MS. GHOSH: Yes, we should -- if we haven't sufficiently documented it, we should --

CHAIRMAN STETKAR: Well, I missed it, but I might have been hanging up too much, Tina, as you said on trying to --

MS. GHOSH: Composite --

CHAIRMAN STETKAR: Yes, compile everything in my head, so maybe I missed something there.

MS. GHOSH: We'll take a look. Either it's there and --

CHAIRMAN STETKAR: Just do it over lunch, don't try to do it in real time.

MS. GHOSH: Yes. Okay, I see that it is here, but yes, let's talk after --

CHAIRMAN STETKAR: Okay. The second question I had then may relate to this, is why did you take -- and you did this for both the primary and the secondary valves. You took a -- at least on the secondary side you explicitly implemented it. It sounded like you tried to do it on the primary side but it didn't work so good for some reason.

You said that you -- on the secondary side, if fails stochastically -- this is for the stuck open area fraction. Sample value of variable SVOAFRAC was applied identically in the case of safety valve 1, and in the case of safety valve 3, but in the case of safety valve 2, the complement of SVOAFRAC was applied. This was done to prevent the unlikely physical situation where all three valves fail stochastically.

MS. GHOSH: Right. So this -- yes, this was an unintended sampling that we first implemented in our MELCOR. We had some discussion. Initially, that -- the safety valve open area fraction was modeled as an epistemic uncertainty and that every time the valve failed you would always fail with a certain percentage open area regard -- but in reality we don't think that mimics reality, that it's more -- it has an aleatory aspect to it.

CHAIRMAN STETKAR: Sure.

MS. GHOSH: So we unintentionally were applying the same safety valve open area fraction to every single failed valve in the system, and we didn't mean to do that.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: We found this very late in the process, so that's why we created this workaround. It's not really what we wanted to do.

CHAIRMAN STETKAR: Yes, but logically I don't understand why your -- but you're treating -- I could understand treating SVOA -- I get hung up on the alphabet soup. The open fraction as we're just discussing an aleatory uncertainty for each valve independently for that particular epistemic realization of the failure rate.

MS. GHOSH: Right.

CHAIRMAN STETKAR: But I don't understand why you need to take the complement of that --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- aleatory failure rate for one particular valve to avoid some undesired condition.

MS. GHOSH: Right.

CHAIRMAN STETKAR: I don't understand what undesired condition you get.

MS. GHOSH: The -- if we did it again, if we do it again we're not going to do it this way. This was a fix to a problem that was discovered very late in the process.

CHAIRMAN STETKAR: But what kind of -- I mean, physically, if I step way back from all of the math.

MS. GHOSH: Yes, sure.

CHAIRMAN STETKAR: Physically what kind of problem were you running into?

MS. GHOSH: So we sampled a 10 percent open area for the safety valve fraction.

CHAIRMAN STETKAR: Okay.

MS. GHOSH: If you have the first valve let's say on the pressurizer fail at a 10 percent open area, it's very likely you're going to start cycling the second valve. If you've modeled it as an epistemic uncertainty now the second valve cycles and gets stuck, is going to fail at some point, get stuck at 10 percent open area. You can create -- then the third valve starts cycling, also gets stuck at a 10 percent open area. You can get into a situation where the team judged was very improbable that you stuck open all your safety valves in an almost closed position.

CHAIRMAN STETKAR: And the problem with that is --

MS. GHOSH: We don't think it's --

CHAIRMAN STETKAR: -- because it's not very likely?

MS. GHOSH: Right. It's a combination of we don't think it's very likely that all of the safety --

CHAIRMAN STETKAR: But wouldn't the uncertainty distributions -- are you now talking about the version of the model which assigned a particular value, or are you talking about a version of the model where that open fraction is actually applied as an aleatory uncertainty?

MS. GHOSH: Right. So the error we discovered very late in the game was that we were sampling and applying the open area fraction value as an
epistemic --

CHAIRMAN STETKAR: Right, and I can see how you can get --

MS. GHOSH: -- across the globe.

CHAIRMAN STETKAR: Sure.

MS. GHOSH: We didn't mean to do that.

CHAIRMAN STETKAR: Right, so --

MS. GHOSH: So we devised a workaround that we would not use again because we're not advocating that it should be modeled this way.

CHAIRMAN STETKAR: Yes.

MS. GHOSH: But in order to avoid a nonphysical situation we devised this workaround kind of as a interim solution because --

CHAIRMAN STETKAR: When I -- let me stop you to make sure I understand what the workaround is doing.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: The workaround is still using though only a single value for that open fraction for --

MS. GHOSH: And --

CHAIRMAN STETKAR: It is not using the aleatory distribution.

MS. GHOSH: No, and it creates an artificial dependence of the second and third safety valve open areas on the sample value for the first pressurizer safety valve. So we artificially created this dependence that you wouldn't do again in order to avoid the nonphysical situation of having all of the valves fail with a very --

CHAIRMAN STETKAR: Well, you said it's nonphysical. Let's just say it is physically possible but you don't believe the probabilities.

MS. GHOSH: Right. And that's --

CHAIRMAN STETKAR: I let the probabilities say what they are.

MS. GHOSH: Yes. We think it's a very -- right. We think it's a very low probability situation. We did do a sensitivity that we documented in the report where we failed all the valves closed just to see what would happen. But, you know -- so the one thing we do say is in terms of the open area fraction still shows up as very important --

CHAIRMAN STETKAR: Well, that's -- yes.

MS. GHOSH: I think if we had implemented the sampling the way we intended but didn't achieve it probably, if anything, would have increased in importance.

CHAIRMAN STETKAR: Well, that's my concern now, is -- I can -- I'll tell you, reading through the report, I couldn't begin to understand what you just described in five minutes.

MS. GHOSH: Okay.

CHAIRMAN STETKAR: The concern I have is whether this kind of -- this workaround as you call it which logically didn't make any sense when I read it.

MS. GHOSH: Okay.

CHAIRMAN STETKAR: How that might affect the overall results and conclusions, not only from an overall risk perspective but in terms of the uncertainty characteristics.

MS. GHOSH: Yes. If I could offer, in the majority of the cases, and I forget the exact percentage. I think it's close to 70 percent of the time you only cycle that first valve so that's what -- the behavior of the first valve is the most important because you don't get up to the second and third.

CHAIRMAN STETKAR: Right. I get that. It's still easier for me to think out on the secondary side before we get into the primary side, because the same workaround was applied, as I understand it, on the secondary side with the safety valves because the open fraction -- in fact, that's where I ran into it first. It's discussed first for the secondary safety valves.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: I believe that's the case. Am I -- tell me if I'm wrong.

MS. GHOSH: I think we only modeled one valve, one safety valve for steam generator on the --

CHAIRMAN STETKAR: That's true, but it says -- I'll read you the quote that I found.

MS. GHOSH: Give a page number so --

CHAIRMAN STETKAR: Well, it's Section 41131. I don't have the page number here immediately but it's only a couple of pages. And this is talking specifically about the secondary side. It says, "In applying SVOAFRAC given a stochastic failure of a safety valve on the main steam lines to close, the intention was to set the open area fraction of the failed lowest set point valve on main steam lines A or C to the complement of the sampled value of that fraction." Then says, "To not affect all three steam generators with identical safety valve failure position, open area fraction for the lowest set point on main steam line B given a fail to close was set to SVOFRAC." So it's logic errors in the MELCOR model; however, allowed valves on main steam lines A and C to fail only in the fully closed position. Open area fraction in the case of the lowest set point safety valve on main steam line B was accomplished per intention. So that tells me that some sort of finagling of complement distributions was also applied on the secondary side.

MS. GHOSH: Yes. I think that was a second issue. Also, a separate issue on the secondary side, but there were two loops that the valves failed to close fully closed. And it wasn't intended to be modeled that way. So we have one steam generator --

CHAIRMAN STETKAR: But what -- you said it wasn't intended to be modeled that way.

MS. GHOSH: Yes. But we still had the one steam generator where we have the sampled open area fraction that was applied. And we evaluated what the effect of that is; for example, actually one of your earlier questions in terms of do you see a difference, you know, a difference in how much, you know, radionuclides get out for the open area fraction? And we didn't see -- there wasn't a huge difference as far as we can tell in terms of the total release --

CHAIRMAN STETKAR: Well, the question is would there have been if you had done the sampling correctly? But the same motion of an aleatory distribution --

MS. GHOSH: Right.

CHAIRMAN STETKAR: -- given an epistemic realization, I'll call it that way.

MS. GHOSH: I think it would be worthwhile for us to do a follow-up kind of mini UA kind of sensitivity to test our -- what we believe to be true at this point because we don't have enough quantitative information to back up the specifics. But we don't think it affects our results greatly, and I think largely for the reason KC mentioned earlier we have so many radionuclides getting out through the leakage area that at the end of the day that open area fraction on the secondary side doesn't contribute as much, if we hadn't had those large, in some cases very large sampled areas for the leakage on the secondary side.

MEMBER CORRADINI: Through the MSIV.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: It's leakage through the MSIV, yes.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: I mean, that might be a fortuitous -- well, fortuitous conclusion for this particular model, but it --

MS. GHOSH: But I think it would be worth following up with some kind of joint sensitivity analysis to confirm that.

CHAIRMAN STETKAR: Are you going to correct the math for Sequoyah? Has it been corrected?

MS. GHOSH: We did correct it for Sequoyah.

CHAIRMAN STETKAR: You did correct it. So you are sampling --

MS. GHOSH: Yes.

CHAIRMAN STETKAR: -- the way we talked about.

MS. GHOSH: Well, the way we intended.

Right. We did correct it for Sequoyah.

CHAIRMAN STETKAR: Okay.

MR. WAGNER: I understand Sequoyah better than Surry because it's more straightforward.

CHAIRMAN STETKAR: That wasn't too bad, it was typical John ranting. But those were the basic two issues, one was fidelity of the underlying data that are used for all of the failure to open and stuck open failure modes for the safety valves, and then given the distribution, you know, how those sampling algorithms were established in the models. You still may want to better document that Section 41131 where it discusses these algorithms about taking complements of the area fractions because I certainly didn't get from that what you just described.

MS. GHOSH: Oh, yes, we'll definitely look at that again and see --

CHAIRMAN STETKAR: Okay.

MS. GHOSH: -- how we can better write it up.

CHAIRMAN STETKAR: Sorry.

MR. WAGNER: Push on?

CHAIRMAN STETKAR: Push on.

MR. WAGNER: The decay heat cycle, the

factor part cycle represents the time at cycle and is varied from the beginning, the middle, and to the end of the cycle. Cycle had an impact on a number of things, including the radionuclide inventory, so as there is more burnup the inventory increases. And baseline decay heat curves were developed for each cycle representative of BOC, MOC, and EOC. For each realization, though, there was a variation from the base decay heat curve based on uncertainty in the decay heat calculations.

The cycle directly affects the MELCOR source term calculation through the decay heat, and also through the amount of mass that is passed on to MACCS. And cycle is the only parameter that has this kind of dual status where it affects the accident progression and it has a direct effect on the inventories inside the MACCS calculation.

From a MELCOR perspective the decay heat is the most -- one of the most important things. And this shows what is the baseline BOC, MOC, and EOC decay heat. The times were based on cycle 20, we have good fuel data from Surry to do our origin calculations. And BOC was selected as set days, MOC was at 200 days, and EOC is at 505. And what you can see from this graph is that MOC starts approaching EOC and there isn't a whole lot of difference, and so the question came up how much difference -- how long does it take for BOC to approach MOC? We think that's about 30 days that it's going to get into the vicinity qualitatively of what an MOC looks like.

MEMBER CORRADINI: So what did SOARCA originally do since this is new?

MR. WAGNER: They did an EOC --

MEMBER REMPE: That was a long time ago. Earlier today you mentioned it, that was one of the factors that did change the results a bit because you'd done it a long time ago.

MR. WAGNER: Yes, and there was -- it was based on origin calculation, but it's in support of the high burnup program so it was kind of not the -- it was biased a little high.

MEMBER CORRADINI: Higher than the red.

MR. WAGNER: Higher than the red. So that would have been the original SOARCA, just a little bit, not a whole lot.

MEMBER CORRADINI: Okay.

MR. WAGNER: The containment fragility, some modifications were made on the -- how we interpreted and how we handled the containment fragility going from SOARCA to the UA. And on the right-hand side you can see how it was modeled, the top graph is how it's modeled in the original SOARCA, the lower one is how it's going to be -- how it was modeled for the Surry UA. They both are based on interpretation of data from the One Six Scale experiments that were done at Sandia, and a method was developed to reconcile the scale, an idealized nature and then above and beyond that there was a 15 percent conservatism put on in that their failure would shift all those values about 15 percent for things that were considered kind of a stretch on how we interpreted the data.

Both original SOARCA and the UA use the top three points for the -- once you get to gross rebar failure what the leakage area might look like. But what was changed was the onset of leakage, and that became an uncertainty variable in the UA where the first thing which is called the liner yield, that was varied from -- across a span. It's not actually on this plot but perhaps the biggest difference between how SOARCA handled it in the UA was the variability of that liner yield. So I've got to flip back and forth once or twice here, so if you look at that lower graph, the lowest point which is the start of large leakage, about factor 10 larger than it is design leakage is the liner yield. And that is the point that's going to be varied. The best estimate was, or the interpretation of the data with the conservatism was around 1.55 for pressure ratio. And if we go to the next slide we can see that that was varied all the way down to design pressure up to the point where the rebar yield would occur. So it was kind of constrained between those two values. We didn't think it should be less than design leakage, but it could be delayed, liner yield could be delayed almost as high as where we get the rebar fail, and it all happened at the same time.

This effect combined with other sample parameters such as the time of the cycle and the nominal leakage is -- the UA realizations for containment pressure remains lower for a longer period of time because we get some leakage coming out and we don't drive to the higher leakage areas.

MEMBER CORRADINI: So there's a leakage rate that goes with the blue curve that is also nonlinear? In other words, you're sampling -- I'm trying to remember.

MR. WAGNER: Yes.

MEMBER CORRADINI: So you would sample here and say okay, I got a pressure. Am I leaking? You go to the probability curve, it says at this pressure, no, or yes. Then there's some leakage rate at that pressure that's deterministic.

MR. WAGNER: Yes.

MEMBER CORRADINI: Okay.

MEMBER REMPE: When I read this in the report, these 15 percent reductions, it talks about there's always uncertainty in the approach when you're going from the ones scaled down, but basically you're making the assumption that whatever you've done has resulted in something that's conservative. And how do you know that it's appropriate to assume that what you've done is conservative versus non-conservative in the approach? And again I'm not an expert in this topic, so maybe it's --

MR. WAGNER: Me neither.

MEMBER REMPE: But if you can explain why you know it's appropriate to assume it's conservative?

MR. WAGNER: So the interpretation from SOARCA moving onto the UA, that was the judgment of the team that looked at it. Conservative is the wrong word, they said that that was appropriate to kind of get the midline of the uncertainty that they felt in the valuations from the One Six and how we interpreted and changed the scale.

MEMBER REMPE: So there was some data that led them to believe that it was conservative.

MR. WAGNER: Yes.

MEMBER REMPE: And that's why they had to reduce it. Okay, that helps a little bit. Thanks.

MR. WAGNER: Ignition criteria. So this was varied also, and in the base SOARCA calculations we had -- used the default for spontaneous burn which was 10 percent hydrogen concentration. And we used the work of Kumar and some of his experimental work to look at the lower flammability limit. And based on the work of Kumar it kind of depends on where the ignition occurs or where the spark occurs on how much hydrogen you need and oxygen in order to propagate the burn. And there's some fantastic experiments that were run where they have a sphere or a test chamber that you look into and they put the spark source in different locations and had different concentrations of hydrogen and if you're burning upwards you only need about 4 percent hydrogen in order for it to -- the spark to propagate and move upward because it kind of follows the thermal loft and the direction of the flame. You could have 4 percent concentration and move that to the middle or the top and there's no propagation of the burn. It just kind of fizzles out. You move that up to 7 percent and it kind of projects sort of horizontally and upward, and so somewhere around horizontal propagation is judged to be about at 7 percent. If you want to try and propagate downward, say the ignition source which we treated as random and unknown where its location was, it takes about a 9 percent hydrogen concentration for the burn to propagate downward.

All three of these were less but than what was done in the original SOARCA, but they represent the real flammability limit in a place where ignition could go and propagate, and so we sampled on those so we had cases where the default ranged from the upward, the horizontal, to the downward, and that would be in all cells inside the model.

The hottest steam generator tube, this is -- came up a little bit earlier. We had a separate effects model that was kind of inside the calculation, and so based on the conditions that we were seeing observed in the hot leg as we developed the natural circulation flow we had a hot stream go in and mix in the generator, go through the tubes and come back, and we had a cold stream returning.

Chris Boyd's work on CFD characterized what was the hottest spot for the plume going into the steam generator, and he characterized that in two different ways. One, if he said I'm going to look at a given tube and figure out what is its temperature as the CFD model was sort of jumping around to different tubes as the hot plume moved around, and he quantified at a given tube location what would be the peak temperature that we might expect for characteristic conditions. And then he also looked at and non-dimensionalized it, and then he looked at if we followed that flame around what would that be? And so that was our source of data, was based on the CFD model which goes back to benchmarking. I believe you've had a number of presentations on that maybe, goes back to the One Seven Scale test and comparisons.

And finally, this is how we did the regression evaluation for -- no, actually, yes, it's a regression evaluation for MELCOR.

MS. GHOSH: Yes, I can speak to this slide, but unfortunately just last night he was asking about how we looked at I guess some groups of the results.

MR. WAGNER: Okay.

MS. GHOSH: Anyway, we can -- I guess just the table kind of summarizes it. We did multiple sets of regressions and different ones for the different metrics, the different figures of merit that we were looking at in terms of the MELCOR results. We knew that for -- when we looked at the whole set of realizations, because the steam generator tube ruptures led to the order of magnitude higher release magnitudes that often ended up dominating the entire -- the results for the entire set, and we thought may mask some of the things that would show up important if we looked at, you know, the non-steam generator tube rupture group. So we wanted to look at both all realizations but also the group of just steam generator tube ruptures alone, and then the group of non-steam generator tube ruptures by themselves to see what pops up as important when for if you don't get to a steam generator tube rupture, they're still variations in behavior and what pops up as important.

And in addition to that since for the first time in this study we looked at the time at cycle, we knew that the things that might be important for beginning of cycle may be different than what's important at middle or end of cycle, so we also did regression the subset of results for the beginning of cycle, middle of cycle, and end of cycle to see what might be different variations in that. And, you know, the beginning -- what ends up being important to beginning of cycle, you know, is different from what ends up being important to end of cycle. And I guess that's expected but we wanted to see what types of things would pop out if we looked at subsets of the data. I think that was my last -- yes?

MEMBER REMPE: I'm not quite sure how to ask this, but where I think Mike was going about the crosswalk, and where I was kind of pointing out about the natural circulation. With respect to what's been predicted for Fukushima in MAAP versus MELCOR, a lot of it's embedded in assumptions with respect to this filling or the way the melt relocates in the core and the hydrogen production.

And so although a lot of the sensitivity -- I believe you that your regression analysis is showing these things, but there's still some assumptions based on lack of knowledge embedded in the code that are difficult to quantify. And I -- it's just a comment, and I mean, you all are struggling with that problem.

MS. GHOSH: Right. I think that's fair, and any results that we're teasing out are based on what we've thrown into the pot.

MEMBER REMPE: Right, I know.

MS. GHOSH: There are things we didn't throw into the pot, so everything is always predicated on the set of -- the scope of our world which does not include --

MEMBER REMPE: We don't know.

MS. GHOSH: Yes.

MEMBER REMPE: Yes, it's just a situation.

MS. GHOSH: I think that was our last slide for this section.

CHAIRMAN STETKAR: Are you going to do the tube thickness?

MS. GHOSH: Do you want --

CHAIRMAN STETKAR: Ron is not here. Let's bring that up after lunch, issues with the tube modeling, thickness, cracking.

MS. GHOSH: Okay, yes.

CHAIRMAN STETKAR: Because Ron and you need to engage.

MEMBER BLEY: Yes. John was focused on the modeling as you got to the end of that story on the safety valves, and I didn't bring it up then but I wanted to now.

I just don't buy the distribution you came up with. You give some arguments about how the valves work, how it might hang, what might happen, and those are pretty good. And then you say but I don't have any basis for a quantifiable likelihood so I'll just draw a straight line, I'll assume a uniform distribution. You gave arguments and I've seen it. When the safety valves pop open, they bang open, and sometimes they cock when they do that, and sometimes they stick wide open. I've seen it happen. Sometimes after they shut down they'll weep. I doubt you care about weeping, but maybe you do. From your story and from what I've seen, and your discussion of the thermal side, too, I think -- and if you've gotten a valve person, maybe call Crosby and talk to him, they might have had some ideas here, too. I think a distribution that has a real high chance of wide open and a fairly reasonable chance of weeping if you care about that. I would be tempted to ignore that and extremely low chance of hanging somewhere in between. I just don't think those happen. My experience isn't enough to say they never happen, but as you're shutting down it's a more smooth process. And sometimes when they hang open they then go shut after banging around and shaking for a while, or if somebody hits them with a hammer. I'd hate for him to do that. But I just think it's a really funny distribution, and I think you give enough arguments about how they work that you could do better. I know we drove you to do uncertainties but I think that one you didn't think through even with the arguments you gave, so I don't quite get it.

CHAIRMAN STETKAR: Even with -- and I know -- since you gave me the open, I'll follow-up on it. I completely agree with Dennis on the uniform distribution for what you call the stochastic stuck open failure mode. It doesn't make much sense, and it doesn't seem that you really discussed it with a valve person.

There's also a more -- there's more

discussion in the report on the stuck open fraction for thermal-related failures which you say well, we used the uniform distribution. But if I read -- when I read that I said well, gee, it sounds like the people you talked to would advocate a distribution that sounds more like what Dennis is talking about, a higher probability of it being open or higher probability of it being only slightly open with a low probability that it's stuck somewhere midway. You then say well but, you know, we didn't have any thermal demands on the valves anyway, so it doesn't make any difference.

Well, the part of this that I'm sensitive to is that you are now enshrining forever the NRC accepted not only methods but data that people will use. And people will point to these distributions and say the NRC spent bazillions of taxpayer dollars, licensee dollars if any licensees are listening out there, and they judged that this is, indeed, the distribution that shall be used, regardless of whether or not you actually ever have a thermal demand, you know, a demand that exceeds the thermal rating on those safety valves. And, in fact, regardless of whether or not in a practical sense the stuck open area fraction, at least on the secondary side, I don't have a good sense of what's going on on the primary side, but regardless of whether that fraction really contributes much to your overall conclusions in this particular study of the releases. So paying attention to some of those things is worthwhile.

MS. GHOSH: Yes, thank you for the comment. I think we can work on improving the discussion about parameter. It certainly one we talked about a lot and struggled with, and went back and forth. You know, I don't know if you remember the early Peach Bottom days we had talked about sampling the open area only for thermal seizure and for stochastic failures just assuming it blows open and it's --

CHAIRMAN STETKAR: Yes.

MS. GHOSH: You know, we changed our thinking this time around, and we modeled more valves. You know, we --

CHAIRMAN STETKAR: You did, but see the problem is you made your MELCOR-type models a lot more sophisticated, but then just sort of spray painted something in there that doesn't necessarily sound like it's well justified based on experience.

MS. GHOSH: And having the open area

fraction, yes.

CHAIRMAN STETKAR: Yes.

MS. GHOSH: Okay.

CHAIRMAN STETKAR: I don't know whether different distribution -- how or whether a different distribution would affect the overall results very much. I don't know, but at a different level one ought not to make conclusions about having something that is not justified simply because you don't think it makes any difference.

MEMBER SKILLMAN: I would like to add that about 25 years ago this issue of safety relief valve or safety valve performance became a very significant topic in the industry. In fact, it was one of the triggers for reporting from the plants to the NRC and the INPO of OE, so I would think that there is a very solid database of safety valve performance. Years ago these things would lift. What we learned is that they weren't adjusted properly and the blowdowns were much greater than anybody anticipated. What happened was that the owners began to get very serious about adjustment of the blowdown rings so that the blowdown percentages were what they should be for the analyses for the plants. I would think that there's data here at the NRC for the performance of these valves, but I know that INPO has the data because industry was reporting all of these events so that there's probably a goldmine if you were to tap into it.

MR. WAGNER: From the perspective of the thermal-hydraulic response, and to the timing of MELCOR, there's not a whole lot of difference between 50 percent and 100 percent. It depressurizes --

CHAIRMAN STETKAR: That's okay, but the problem is if you put a uniform distribution in there and you run 1,200 samples of which you get 1,000, there's a measurable chunk of probability of it being less than half open, of being, you know, where a different probability distribution, one that is high likelihoods of being very open or not open very much would give you a much different set of samples. Now if the difference doesn't make any difference, let the analyses show that, don't try to justify something that might be fundamentally not physical ---

MR. WAGNER: That's probably why we did the zero to one, was we were afraid to miss something that we maybe didn't know, but I hear your arguments that maybe valve people would have steered us in another direction.

CHAIRMAN STETKAR: Certainly for a different -- well, I don't want -- I'm not a valve person, but a different shape of the distribution, rather than saying there's an equal likelihood that it's 37.265 percent stuck open, as it is 99.38 percent stuck open.

Anything more for Tina and KC? If not, let's recess for lunch, and I'm going to be a hard taskmaster. Let's come back at 1:15, please.

(Whereupon, the above-entitled matter went off the record at 12:18 p.m. and resumed at 1:17 p.m.)

CHAIRMAN STETKAR: We are back in session. We're going to hear about MACCS this afternoon first.

MR. BIXLER: All right. First of all, I'm Nate Bixler from the Sandia National Labs. Can you all hear me okay? This mic is working?

(Off microphone comment.)

MR. BIXLER: Okay, now it's on. Okay. So my conclusion from this morning's session is that I'm sure glad there are no valves in MACCS.

MEMBER CORRADINI: But there's weather.

MR. BIXLER: There's weather. Yes, I don't know if that's a winning situation. Maybe I come out behind.

(Off microphone comment.)

MEMBER BALLINGER: --the last couple of weeks.

MR. BIXLER: Okay. This is kind of an overview picture of some of the functions in MACCS, broken into core or three main modules, ATMOS, EARLY and CHRONC. ATMOS does the Atmospheric Transport and Dispersion; EARLY does the emergency phase. So it's handling the emergency response of individuals who are usually described as being in cohorts, and then CHRONC does the long-term phase and looks at the longer term remedial actions in doses, and all together we calculate that doses and health effects, risk developed effects and things like that.

MACCS right now is based on a dispersion model. We've referred to it as a plume segment model because we have a number of plume segments. Each has a front and each has a back, and usually we make those our alarm plume segments. We can model up to 200 of them.

MACCS treats radioactive decay and

ingrowth, and it -- as we just said or someone just said a minute ago, it treats the aleatory uncertainty from random weather. So that's part of the calculation, and we'll talk a little bit about that as we go through the slides.

Okay. This is a picture to help illustrate the idea of emergency response. During the emergency phase, we model evacuation, sheltering. Sheltering would normally precede evacuation, so it's really the opposite order.

Potentially KI ingestion to reduce the amount of exposure from radioiodine from inhalation, and we also have another type of action that we can model, which is called relocation. I think there's a bullet on relocation here. That's the bottom one, so I'll wait until I come to that.

Evacuation speeds and evacuation directions can be developed, and they're usually based on ETEs that each plant has to publish. We can have up to 20 cohorts but in both Surry and Peach Bottom for the purposes of doing the SOARCA analyses we define six cohorts, and those are the same basically as the ones in the original SOARCA work that we used for the uncertainty analysis as well.

Each cohort can have its own unique response. So typically we'll have the large body or the large part of the public as one cohort, maybe a tail as another cohort. Special facilities might be their own cohort etcetera, and each of those can have their own unique response characteristics.

As far as relocation of the public, that's treated differently in MACCS than evacuation. Evacuation is triggered generally by a declaration of an emergency at a plant, and it's done regardless of -- with no knowledge necessarily of what the release is going to look like.

Relocation is done on the basis of dose projections. So that typically happens on a slower time frame, takes a little longer to occur. In MACCS we have two types of relocations. One is called hot spot relocation; the other is called normal relocation. The idea is, and you can collapse those into just one set of relocation parameters if you want.

But you can try to -- as a user, you can try to prioritize that folks with a dose projection of a higher dose would receive priority treatment over those who are projected to get a lower dose. So that's the idea for that.

Okay. These are the MACCS parameters that we made uncertain in the Surry work, and basically it's almost the same set as we had as uncertain parameters in the earlier Peach Bottom study. There are a couple of deposition parameters for wet deposition and dry deposition.

Dry deposition, we usually have ten aerosol bins and each of those has its own deposition velocity that characterize the influence on aerosol size on the way it deposits. We have a couple of dispersion parameters for cross-wind and vertical dimensions, a couple of shielding factors, one for ground shine and one for inhalation that characterize how much shielding or protection a person would get from the direct exposure to the plume.

The baseline is direct exposure to the plume. The shielding factor is scale that exposure down to be what might be more realistic. One thing that we included in the earlier Peach Bottom study that we didn't include here is uncertainty in the shielding factor for cloud shine.

Cloud shine turns out to only contribute typically something like one percent of the total dose through the ground shine or the cloud shine pathway. So we didn't consider it to be -- we did consider it earlier in the Peach Bottom work, but decided to drop it as a parameter here.

There are several parameters that affect latent health effects. I won't go into each of those. We'll talk about them later. Early health effects also. Early health effects turn out not to be very important in this uncertainty analysis because there's essentially no early health effects. So these parameters tend to turn out not to have much impact on the answers that we get.

Also we looked at a set of emergency response parameters that are listed here, evacuation delay and speed, hot spot and normal relocation times, hot spot and normal relocation dose as uncertain, and we'll talk about some of those. Finally, weather is the single parameter that we treat as being an aleatory uncertainty, and we'll talk in a little bit of detail about how the weather sampling works.

Okay. So for ground shine, that's our first -- the first set of parameters that we're going to talk about, these are the curves that we use to describe them. There are separate curves for normal activity, for sheltering and for evacuation. The trend is that sheltering is always the lowest normal activity, somewhere in between, and evacuation is the highest.

Generally those are bounded by zero on the bottom side and one on the top side. Ground shine is particularly importantly because while it turns out to have -- to represent more than 50 percent of the total doses that are received from an accident, 75 percent is probably kind of a common number that you would expect to get through the ground shine pathway.

So it's definitely very important, and the ground shine shielding factor directly -- it's a direct multiplier that affects how much dose a person would potentially get from ground shine. There are several things that we're trying to account for when we make the ground shine shielding factor uncertain.

Those include the amount of time spent indoors versus outdoors; the amount of shielding or protection that a person gets from being in a house, depending on the construction of the house and so forth, and also deviations from the -- from an infinite, the flat plane.

The dose conversion factors for ground shine are calculated as though a person is standing on an infinite flat plane with a uniform concentration, ground concentration surrounding him or her. So obviously that's not the real situation.

We have at least small variations in the ground around us, and we have buildings and other structures, a whole variety of things that might offer some level of protection from the simple assumption that is used in creating the dose conversion factor.

Oh, one thing that this slide doesn't mention is that the dose conversion factors themselves are also uncertain, and that's not folded into the curves that are shown here on this slide. But ultimately we did include uncertainty in dose conversion factors for ground shine doses, and we folded that together with the other uncertainties and made a modified set of curves that account for that as well.

Okay. The next slide here shows the uncertainties in the cancer fatality risk factors. The way we calculate cancer fatalities in MACCS is we start out by calculating individual organ doses for eight types of health effects, and then each of those organ doses is multiplied by a risk factor to estimate what the potential for -- the potential risk for cancer is.

The results that I'm going to present today are all based on a linear no threshold dose response assumption. So we're going to assume that there are effects of receiving a dose all the way down to infinitesimal doses. Okay, let's see. The risk factors that we used --

MEMBER BLEY: Not that I want to pursue it any further, but since we're treating uncertainties in all other places, did you think about treating uncertainties in dose response models, and what led you to just use the --

MR. BIXLER: Well in the report we discuss three different dose response models, so those are included in the report. I don't think I have any of those results to show in the slides for today. But if you read the report or look through it, you'll see that there are other dose response models that we considered.

That's certainly a large form of uncertainty. Up to -- at this point we haven't included it as an uncertain parameter. I think it could be. Some of the health physics folks don't like that idea and have discouraged us from doing it. But it seems to me that it's a legitimate uncertainty that maybe we should be modeling.

MEMBER BLEY: Well can you -- are they here?

MR. BIXLER: Who, the health physics folks?

MEMBER BLEY: Yeah. I wonder why they don't like considering it. Is it just that so you won't get the argument with people who say that's drastic?

MR. BIXLER: I don't know, to tell you the truth. I'm not sure what the thinking behind that is. They think it's completely fine to consider it as a parameter uncertainty separated from the overall uncertainty, where you just make different assumptions on the dose response model and present the results, and that's what we've done.

MEMBER CORRADINI: You just use a different supposition versus linear?

MR. BIXLER: Yeah, yeah. It's a different model for how health effects are induced by receiving doses.

MS. GHOSH: Yeah, we have two alternate models. It's the same ones we had in the base. I think it's -- we used the same two alternate models that we used in the base, the original SOARCA study as well as the Peach Bottom UA.

MR. BIXLER: Yep, that's true.

MS. GHOSH: So they're, yeah.

MEMBER BLEY: I know we're not planning a follow-on, well except the next study maybe. I'm a little curious about their arguments as to why they don't think it should be built into the overall uncertainty calculation.

MS. GHOSH: Yeah, we don't have the right people here to engage on that topic.

MR. BIXLER: Yeah. I'm sure I'm not the right person to represent that argument. So I don't think I would want to even give it a shot.

MEMBER SKILLMAN: Nate, what is residual? I understand all the organs, but I don't understand residual.

MR. BIXLER: That's a good question. Okay. So the way we model the cancers, as I mentioned, is by looking at specific doses to different organs and modeling specific cancer types. For example leukemia is associated with a dose to the bone marrow, etcetera. You can think of an organ that goes with each of these types of cancers.

But there's a bunch of cancers left over. We only have seven specific cancer types that we've modeled. So whatever is left over we've modeled as residual cancers, and we've associated those with a dose to the pancreas as a kind of a surrogate tissue to represent soft body tissues in general that would be responsible for the other types of cancer not specifically included in the list. So that's the idea.

MEMBER SKILLMAN: So that would be a DDE? That's a deep dose equivalent. It's an organ dose.

MR. BIXLER: Yeah.

MEMBER SKILLMAN: All right, thank you. Got it.

MR. BIXLER: Okay. All of this is based on BEIR V. BEIR VII is out now. We haven't at this point updated the BEIR VII because not all the pieces of the model are really -- are really put together to go with a BEIR VII type model. So we've stuck with a slightly older document, BEIR V, and we got Keith Eckerman, who's considered an expert in this area, to put together the curves for us for how they -- the uncertainties for each of the organs that are displayed here on the plot.

The triangles, and you'll see this on subsequent slides too, represent our SOARCA, original SOARCA estimate for each of those uncertain parameters. So those are the values that we used in the original SOARCA study. They're not necessarily right at the 50th percentile, but those are the ones that you would get straight out of BEIR V if you go and interpret the information there.

Okay. Along with the cancer risk factors, what goes along with that is thing called DDREF. DDREFA is the term that we use in MACCS. It stands for dose and dose rate effectiveness factor. The idea is that, and this comes out of BEIR V, is that for larger doses, you use the doses themselves for calculating cancer risk.

For the smaller ones below the one recommended by BEIR V is 0.2 Sieverts, 20 rem. Below that dose threshold you divide by DDREFA. So basically the linear on threshold model that you think of is not really completely linear. It has two slopes, one for low dose rate and a higher slope for higher dose rate.

So we employ that part of the BEIR V model as well in what we do in MACCS. There are two different curves here. One for all of the organs other than breast, and then a separate one for breast. They each have their own distributions. The nominal value is one and two for those two types of tissues, and that's what's recommended in BEIR V. But here we accounted for uncertainty on those parameters as well.

Okay, the next parameter is dry deposition velocity. As I mentioned earlier, there are ten of them representing the ten different aerosol bins that we get straight out of a MELCOR analysis. That's a user choice, but I think that's the default in MELCOR is to have ten aerosol sized bins.

So for each of those bins, we define a deposition velocity. The median value on those curves is taken from expert elicitation data that was performed by NRC and CEC back in the 90's. But for this study, we decided that the distributions that they had prescribed were too wide, too broad.

What we realized is that the original expert elicitation was soliciting day or hour by hour variations that you might get in deposition velocity, whereas what we wanted to do is apply a value or a set of values for a whole year's worth of weather data.

It wasn't really fair to include all of the uncertainty in the expert elicitation data, if you're applying the same exact value for a whole year's worth of weather trials. For one thing, you would bias -- the means for a year would be too broad. They would not be representative of what you would really expect.

So we modified the distributions. We made some arguments and judgments in the report that describe what we thought was reasonable to do and used that instead. Okay. So that's one departure from the earlier Peach Bottom work, by the way, is that we used a narrower distribution.

CHAIRMAN STETKAR: Nathan?

MR. BIXLER: Yeah.

CHAIRMAN STETKAR: The argument that you made regarding well, the uncertainties were too broad from the expert elicitation because you felt they were too broad for a representative value to be applied over an entire year's worth of data. That similar type of argument is made in several areas for reducing the uncertainty.

I quite honestly don't understand that. So can you explain it in sort of layman's terms about why that is?

MS. GHOSH: I'll take a crack at it, and then you can elaborate.

CHAIRMAN STETKAR: Sure, go ahead.

MS. GHOSH: I think that when we went back to the documentation of what the experts were providing in the elicitation data, Nate's already said this. I'll just say it in a different way. It seems that they were also considering the weather variations and their impact that we explicitly considered in our weather variation portion of the modeling, also in their described description for this parameter.

So we wanted to -- we felt that in the original implementation we were in fact double-counting some of that uncertainty by applying their distributions, where part of that variation was meant to account for the weather. Wherein the MACCS approach, we're also explicitly accounting for the weather variations.

CHAIRMAN STETKAR: Okay. Let me probe that a little. I get that.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: For some of the parameters. How would it apply to this, because they were somehow accounting -- this is a dry deposition velocity.

MR. BIXLER: One of the things they were accounting for was variation in aerosol size, because at that time that the expert elicitation was done, most people were modeling all aerosols as being a single size, as though they could all be collapsed into a single definition for --

CHAIRMAN STETKAR: So they only had a single distribution for velocity, a single distribution for the uncertainty in the deposition velocity?

MR. BIXLER: To account for all --

CHAIRMAN STETKAR: To account for any size aerosol?

MR. BIXLER: No. They had aerosol size as a parameter, but they realized that people would -- the way people would use that distribution is they would use the size of the aerosol that they were providing to you as a mean for what was really an aerosol distribution.

So they were only treating the mean of

the aerosol distribution -- mean size of an aerosol distribution as a parameter, and they gave you the information that would allow you to vary that size, but not to account directly for deposition velocity.

CHAIRMAN STETKAR: And what does that have to do with average over an annual weather?

MR. BIXLER: Yeah. I think -- actually, I think I misspoke. I mean the annual weather really applies more to what I'll get to in just a minute, which is dispersion.

CHAIRMAN STETKAR: Right. That's --

MR. BIXLER: Okay. So yeah, so that's a different thing. Here, I think the reasoning is that the intention of these distributions is that you would use a mean aerosol size to characterize all of the aerosols that you were modeling --

CHAIRMAN STETKAR: Which you have done here?

MR. BIXLER: Pardon?

CHAIRMAN STETKAR: Which you have done here?

MR. BIXLER: We're using -- no. We're using ten bins. So we're modeling different aerosol sizes as having their own deposition velocity.

CHAIRMAN STETKAR: But I thought you told me -- I asked you originally, maybe not well enough, did they have only a single uncertainty distribution for dry deposition velocity that applied to all aerosol sizes, any aerosol size?

MR. BIXLER: In a sense, yes. They characterized variations in the mean but not -- the intention was not to characterize the actual distribution itself.

CHAIRMAN STETKAR: But the distribution -- that's like the ground mean?

MEMBER BLEY: Yeah.

CHAIRMAN STETKAR: Which 30 years ago caused us problems in failure rates.

MR. BIXLER: But the intention was that you would pick a --

MEMBER BLEY: Going back to way Tina described it, after you did this, it sounds kind of arbitrary. We just shrunk them a bit to cover that. If you go back and play it against the weather and particle sizes and see if you've kind of covered their intent in those elicitations, or is it just since we were doing those separately, we now just felt knocking it down by a factor ought to be kind of okay?

MR. BIXLER: We didn't go back and do any comparisons with the original data, to see how that would have played out. But so to do that, I think we would have had to have varied the -- to do -- to faithfully do what expert elicitation had intended, we would have needed to use just a single size -- single aerosol size, one of these curves from the set of curves that we used, and then use that --

MEMBER BLEY: So faithfully, you know, you could do like a back of the envelope and say well we covered these weathers and covered these sizes and then with this distribution, we're kind of mapping roughly what they were intending because, you know, they were the experts --

MS. GHOSH: I guess we haven't done that yet, but it's a good -- it's a good suggestion.

MR. BIXLER: I'm not -- I'm just trying to think of myself how I would even go about doing that. I'm not sure how I would do it. It's not clear to me how I would go back with the model that we used being different than the older models, where you just have --

MEMBER BLEY: If I ask it another way, what gives you confidence this reduction that you did is a reasonable thing to do, given what you were trying to accomplish?

MR. BIXLER: It adequately captures the experts' state of knowledge on certainty about the phenomenon.

MEMBER BLEY: But did you go back to the experts and show them what you've done and said gee, given what we're doing here, does this kind of makes sense, to even a few of the experts just to --

MR. BIXLER: We have -- we did get some feedback from one expert, and I think he was one of the ones who originally contributed to this, Steve Hanna, and he told us that our distribution was too broad, the one that we had used for Peach Bottom. That was part of the motivation for reducing --

MEMBER BLEY: After he saw how you were modeling it?

MR. BIXLER: Yeah. After he looked at what we were doing and what the results looked like, he thought that we -- our distribution was way too broad, and that we should rethink it, so we did.

MEMBER BLEY: You saw that, but did you rethink this is what I'm asking?

MR. BIXLER: We put thought into it. We looked at the different types of things that would lead to uncertainty, the different mechanisms. Things like particle density, shape factors, a variety of things like that that would potentially give you a different deposition velocity, and we specifically included things that we thought should be included, and excluded things that we thought we were already accounting for separately and should not be included. So I think if you read that section of the report --

MEMBER BLEY: That sorts of makes me feel a little better.

MR. BIXLER: Okay.

MEMBER BLEY: And I didn't study that section in detail.

MR. BIXLER: Yeah, that is described. I think it's documented pretty well in the report. So if you look that over, hopefully that will answer your questions.

CHAIRMAN STETKAR: Nathan, let me ask you what I hope is just a really simple, silly question. But I have to. On this plot, and in fact in the supporting table of the distributions, I note that the dry deposition velocity for a .29

micron aerosol is less, lower, than the dry deposition velocity for a .15 micron aerosol.

There's a statement in the study that says "VDEPOS is assumed to be perfectly rank order correlated across aerosol sizes. This prevents small aerosols from depositing faster than large aerosols, which would contradict our understanding of aerosol physics. The red and the dark green lines on this curve seem to contradict our understanding of aerosol physics."

MR. BIXLER: Okay. Well generally it's true that larger aerosols deposit faster than small ones. But you're right. It's a well-known feature of deposition velocities that there's a minimum in the curve. The reason is that there are competing deposition mechanisms.

Brownian motion tends to dominate it for very small aerosols, gravitational deposition tends to dominate for large ones. In the middle of the range that we're interested in, there is a real minimum in the curve for deposition velocity. You'll see that in a number of publications where they plot -- they have a figure showing deposition velocity as a function of size.

So that was actually intended. It should be there. It's a real feature of aerosol mechanics. But our simple generalization did not capture that feature.

CHAIRMAN STETKAR: Okay. I know nothing about what you just said, but you at least have a good reason, so thanks.

MR. BIXLER: Okay, okay. Now we'll move on to the dispersion parameters, and I'm just going to discuss the cross-wind ones, but we do basically the same thing for the vertical dispersion parameters. Here again, in this case we were thinking specifically in terms of weather.

Since we're looking at a realization and capturing a mean result as our primary result that we're trying to capture for a whole year, it's not really fair to use a value that is too strongly biased to represent a whole year's worth of weather samples.

If we were sampling individual weather trials and choosing a large value or a small value of dispersion, and then averaging that over the whole year, then I think we would have done what the experts had intended us to do.

But since we're using one value for a dispersion parameter to capture a whole year's worth of data, then representing that as an average, too much bias one way or the other in the dispersion parameters doesn't give the right answer. It would bias the mean.

And so what we chose to do was again define a narrower distribution, this time for a little different reason but the same general concept.

CHAIRMAN STETKAR: But in this case, I had some real questions about this one, not knowing anything about the physics again, but just reading what was documented. In this case, it's noted that the expert elicitations spanned about an order of magnitude uncertainty with a 90 percent confidence interval and about two orders of magnitude to capture the full range. So they're pretty broad.

MR. BIXLER: Yep.

CHAIRMAN STETKAR: You've reduced those uncertainties to a factor of 6.25 over the full range, which is --

MR. BIXLER: 2.5 squared.

CHAIRMAN STETKAR: 2.5 squared, that's exactly correct, and it covers now the 100 percent confidence interval as shown on these curves.

There's a lot of discussion that I couldn't quite understand. Part of the discussion notes that there are a number of other sources for CYSIGA, three of which are given as examples in the MACCS users guide.

These values were compared to the best estimate values from expert elicitation. It was found that two-thirds of the values were within a factor of three of the best estimate value, meaning two.

You don't discuss what the third one. I'm assuming that the third one was more than a factor of three different, which would sort of corroborate larger uncertainties that you've assigned.

I felt really uneasy about this thing. I don't know how important it is, but again in the sense of I don't particularly care about how important it is. I care about the technical justification for reducing something that experts assess as quite uncertain to something that seems quite certain.

The factor of six range over the full, you know, 100 percent confidence interval is quite certain.

MR. BIXLER: No, go ahead.

MS. GHOSH: The original distributions that we had in Peach Bottom, which we initially had just taken from the expert elicitation, again were criticized quite a bit for being way too broad, both by a couple of the peer reviewers who had seen at least the initial uncertainty distributions before we completed the project, as well as again Steve Hanna.

So we had external feedback that our use of the expert, original expert elicitation data was not appropriate, that it was too broad for our purpose.

CHAIRMAN STETKAR: Okay. Let me try to pin you down though, because on the record I want to make sure that we have a good justification here. People often say the uncertainties are too large. I can't deal with uncertainties that are that large. In fact, for some things that uncertainties are very large.

So just a criticism saying the uncertainties are too large doesn't tell me why were the uncertainties too large for the purposes of the way you're treating these parameters in this particular study?

MS. GHOSH: I think again it has to go back -- it goes back to the way we combine, that we're explicitly modeling the year of weather data, along with the sampled parameters. I don't know if you want to give more.

MR. BIXLER: Yeah. So if what we had done was used a larger or dispersion parameter distribution, say even two or three orders of magnitude wide, and sampled for each of our thousand or so weather trials we had chosen a different dispersion value for that sample, and then done that for a year.

I think what we would have done would have been what the authors of the expert elicitation had really had in mind, and if we average those values over a year, we would end up with something much closer to the mean of their distributions than we would have -- for each of our weather clouds we would have ended up with something much closer to the mean of their distribution.

But all we're trying to do is capture that lower uncertainty that you have when you're using the same value for a year's worth of data.

CHAIRMAN STETKAR: You said a couple of

important ifs there. If you had something and if you had done something, then maybe we would have captured the uncertainty. That implies to me that you didn't actually do that, because I know you didn't have that number of weather trials initially for different wind conditions.

MR. BIXLER: We did not.

CHAIRMAN STETKAR: You had a very limited set.

MR. BIXLER: Yeah.

CHAIRMAN STETKAR: And just simply trying to do something that reproduces somebody else's thing that they have called a mean value is not the purpose of this study. The purpose of this study is to examine the actual uncertainties, and how those uncertainties may affect things.

So trying to get something that's narrow enough to comes back to a mean value that somebody else had is not what I think this study should have been doing.

MS. GHOSH: That wasn't the purpose. I think we took the criticism that we heard as a criticism of the technical basis for the original distributions we had, and I completely agree with you. There are some legitimate uncertainties out there that may be four or five orders of magnitude.

The criticism didn't come simply from the how wide the distributions, but for the technical justification for those original distributions as we were using them.

CHAIRMAN STETKAR: But Tina for example, if the criticism in this particular case was that well, the experts were trying to combine both uncertainty and the weather parameter and the dispersion parameter, and you ought not to do that because you're separately considering uncertainty in weather.

But if you have not adequately considered the uncertainty in the weather, especially in the extremes, are you now then artificially reducing the composite uncertainty by separating the variables and having narrower distributions for both them? In other words this comes back to something that Dennis asked earlier.

Did you go back to the experts and say well, the way we've broken apart the problem, the two separate now treatments of uncertainty, does the composite uncertainty of our results replicate what you were trying to do in your expert -- in your elicitation?

MEMBER BLEY: Or even if you can't do it that way, are you comfortable with the way we've reformatted it and the uncertainties we're using now?

CHAIRMAN STETKAR: Considering the variations in the weather sampling that you used, the data, the supporting data and the sampling algorithm that you used.

MR. BIXLER: Yeah. We have not done that stuff. We haven't gone back and talked to any of the experts to see if they agree that these are reasonable distributions. But --

MS. GHOSH: We'll take it as a comment.

MR. BIXLER: Yeah.

MS. GHOSH: We probably --

CHAIRMAN STETKAR: There's a couple of reasons. Number one, this kind of seems to make sense. But number two, but you rely very, very heavily, you rely entirely on the results from those expert elicitations, that NUREG that's cited, for the nominal mean value. You put 100 percent confidence in those expert elicitations, saying yes indeed, that's the thing that we're going to rely on and try to hold fast to. And yet you haven't gone back and talked to them about the ranges of the uncertainties?

MR. BIXLER: All right. We'll take that as an action item to --

MS. GHOSH: Comment noted.

MR. BIXLER: Yeah. Okay. Next, we're going to talk about the way weather is actually modeled in MACCS, and the way -- the method that we use for doing the weather uncertainty modeling typically is to divide all the weather into a set of bins. There are some that are called rain bins and then there's a set that are based on stability class and wind speed.

This is a standard approach that's been used since NUREG-150, maybe even earlier than that. The idea is that for situations -- first of all, you take all 8,760 hours of the year and put each hour, each starting hour into one of the bins. So you end up with 8,760 samples to represent a year of data.

Each bin, for the rain bins represents a rain intensity and a distance that the plume would travel before rain starts. So we're looking at specific situations where the plume would travel a distance, then you would get some precipitation, and it would fall out perhaps over a population center or something like that.

The other set of stability or weather bins is based on stability class, which affects the amount of dispersion that you get and wind speed, which obviously affects the duration and length of a plume as it's traveling through the grid.

Okay. So that's the basic idea, and I know it's a little difficult to conceptualize what, how all this modeling works when we're looking at a set of plume segments. So I constructed an animation to give you an idea. Each plume segment can travel in its own direction, which is the direction that the wind happens to be blowing at the start of the weather trial, and each plume segment will have a different length, depending on the speed of the wind as it's exiting the source.

The width will be varying, depending on how much dispersion occurs along the length of the plume segment as it's traveling through the grid. So each plume segment is different in those regards, but it's also different in terms of its activity, the activity content of it. Some plume segments will have a lot of activity, some only a little because we're coupling a time-dependent release, a source term with time-dependent weather.

So I think this hopefully gives you an illustration of how the modeling goes together between following the weather and following the pattern of the release. This is for one of the weather trials that we happen to use for this analysis.

CHAIRMAN STETKAR: Okay. The weather data were based on two years of experience?

MR. BIXLER: One year of weather data.

CHAIRMAN STETKAR: One year of weather data, but you looked at, if I recall, two years of met tower, right?

(Simultaneous speaking.)

MR. BIXLER: That's right, two years.

CHAIRMAN STETKAR: Two years of met tower and selected one as being --

MR. BIXLER: The two were not terribly different, for one thing. So we chose one of them. I don't recall, but one of the criteria for choosing is that you would like to have a good recovery, data recovery rate where there aren't a lot of hours of the year with missing data. That's one thing, and secondly you would like to find one that's typical for that area, as far as rainfall and other general characteristics.

CHAIRMAN STETKAR: Depending on what typical means.

MR. BIXLER: Well yeah. Not different than the -- too different than the averages for that area.

Okay. Next thing is speed of evacuation. I mentioned that there are six cohorts. The cohorts are listed here. Each cohort has its own distinct characteristics and one of those characteristics is how fast does it evacuate. The timing of the cohort of when does it begin to evacuate is also distinct, that the speed of evacuation is distinct.

One of the six cohorts is non-evacuating. By assumption we have a half a percent of the population that we model as not being -- not evacuating. So the other five are represented here. The SOARCA values of the triangles in the curves are the distributions that we used.

The basis for this comes out of the ETE, the Evacuation Time Estimate report that each of the plants produces. Those ETE reports have variations depending on time of day that the accident occurs, time of year, whether there's adverse weather, a whole variety of things. So you get some sense of uncertainty in the timing based on that.

CHAIRMAN STETKAR: This particular -- back to this one.

MR. BIXLER: Yeah.

CHAIRMAN STETKAR: This particular scenario, according to the documentation, is initiated by an earthquake with a peak ground acceleration of somewhere between .5 and 1G, which is strong enough to disabled safety-related equipment inside a nuclear power plant. How did the evacuation time estimates account for those types of scenarios?

MR. BIXLER: Joe, correct me if I'm wrong, but I don't think the published ETEs specifically look at earthquake situations, do they?

(Off microphone comment.)

MR. BIXLER: But we did make, put an effort into going to fairly low evacuation speeds here to cover what we thought the damage to the road structure might do as far as evacuation speeds. So we did try to model a very low end evacuation speed to specifically account for degradation of the road network.

But we don't find published values to support that. That was our own judgment of how we should do that.

CHAIRMAN STETKAR: Something you haven't talked about, and I don't know how the model does it. How much -- how does the model account for initial sheltering, and does that make a difference in terms of the accumulated dose?

MR. BIXLER: The model allows for treatment of sheltering. That's a user input, and we did model sheltering in this case. Our main uncertain parameters though were delay to evacuation and evacuation speed.

CHAIRMAN STETKAR: I don't think any of us have been in an earthquake that is anywhere near this strong. I've been in a few earthquakes, and the initial response of most people is to run outside and stay outside and not go back inside those darn buildings that are going to fall down and hit me on the head. That's true for schools, it's true for members of the public, it's true for businesses.

So you wind up getting an earthquake. I know people in the North Ridge earthquake who were pitching tents out in their backyard, because they didn't want to go back inside the buildings for days, because of the aftershocks. Also people are generally reluctant to leave their homes when they're damaged and the bad guys can come in and start looting.

So how did you consider that effect in terms of the efficiency and effectiveness of sheltering and possible delays in evacuation because A, the public doesn't want to go inside, despite the fact that maybe there's something going on down the street in the nuclear plant, and B, maybe they want to get out of town really fast and clog up everything because everything's damaged and there's something going on down the street.

Or C, maybe they don't want to get out of town because they don't trust those nuclear plant people and they want to stay home and protect all their belongings, because all their windows are broken.

It's very, very unique scenario these seismic events. It's not a random weather pattern.

MR. BIXLER: Yeah, right. Our thinking here was that our primary focus on emergency response was not on sheltering, that it was on evacuation, which I think is pretty typical.

That's the more likely response that would be triggered.

And so we were focusing on things leading to evacuation, people getting out of the area, how long would they stay put before they leave, and then how long would it take them to evacuate. You want to add any more detail to that Joe?

MR. JONES: This is Joe Jones with Sandia, and I did the emergency planning work on this working with Nate and with Randy Sullivan, beginning with the early analyses in Surry, in the Surry SOARCA document. We need to remember a few things unique about Surry.

One is the vast majority of people are on the easterly side of the James River, which is about five miles wide. So given the travel time of a plume and the delays and the releases, from an evacuation time estimate perspective they are all departing within a few hours.

So the sheltering, whether or not there's a sheltering need here to assess, you know, the broken windows or people being outdoors, we consciously did not think we needed to address that.

CHAIRMAN STETKAR: Is that true also even for the early releases from the faulted steam generator cases?

MR. JONES: I believe those are in the three hours' time frame, and an hour or so to release and get across the river, if the wind happens to be going in that direction.

CHAIRMAN STETKAR: So you're still kind of on the margins?

MR. JONES: We're on the margins, when people would be getting on the road and evacuating.

MS. GHOSH: Which is why we don't -- we continue to see no -- it's very hard for us to calculate an early fatality risk, because the plume doesn't really catch up with even a greatly delayed population leaving.

MR. JONES: Now with regard to the looting and people wanting to stay, we've done a lot of research for the NRC in evacuations, and looting is a minor issue in an actual emergency where there's a hazardous material of some sort. It's fairly infrequent.

CHAIRMAN STETKAR: Is it? Okay.

MR. BIXLER: Any other questions?

MEMBER BLEY: Not to throw a monkey wrench, but I think you've done the best you can with this. But I want to share something that we found about 30 years ago. We hired some human performance people to try to address this question, of what people would do after a very strong motion earthquake.

They weren't able to do it thoroughly. They made some judgmental estimates, but they brought back a lot of anecdotes. The anecdotes were really interesting because -- and these are the kind of earthquakes, the ones that knock you off your feet and do a lot of damage. It's something that's outside of the experience of almost all of us.

The one thing we anchor to is the earth and it's acting up. What they found was many, many cases people remembered the earthquake and the next thing in their memory was they were home or ten miles away, somewhere else, and they had no connection. They did a bunch of things automatically. They got in their cars and drove from their office to somewhere else, and they had absolutely no memory of it.

We're talking about people doing all these rational things during this period of time, and I don't think anybody's really thought hard enough about that.

MR. BIXLER: Okay. The next parameter for discussion is the -- and there are a couple of slides that go with this, is the hot spot relocation criteria. As I mentioned, there's a dose that goes with that and that's the next slide. But this slide shows the amount of time that it would take to relocate.

For the base case analysis, the original SOARCA work we used 24 hours. Here, we assumed a relocation time in the range of 12 to 30 hours. We just chose here a uniform distribution, because we didn't think we had any basis for biasing the distribution towards either end of the range.

So this is the distribution that we used for time hot, and there's an equivalent value or a curve rather that goes with time norm, the normal relocation parameter. Then along with that goes this distribution for the dose that would trigger normal or hot spot relocation.

Nominally we used a value of five rem or .05 Sieverts, and we have a distribution that goes from one up to seven and a half. One is what we nominally use for normal relocation, so we didn't want to go below that.

So here that's kind of -- so we made that our lower bound in seven and a half rem or upper bound. I think this is based on a triangular distribution that we sat with. The mode of the distribution is the base case value. Okay. That's all the MACCS parameters.

MS. GHOSH: Are there any questions on the MACCS parameters? Okay. KC, would you mind joining? Thanks.

Okay. So we'll move on to the MELCOR analysis results.

CHAIRMAN STETKAR: Before we go to the results, the good Doctors Ballinger and Shack, when is it an appropriate time for you to grill folks?

(Off microphone comments.)

CHAIRMAN STETKAR: Okay. I just didn't want to get a steam roller going and then suddenly -- okay, fine.

(Simultaneous speaking.)

CHAIRMAN STETKAR: Thank you. People always berate me for reminding people who have given up.

MS. GHOSH: Okay. So we just wanted -- this is just a quick review of where we ended up the beginning of our earlier session on the parameters. Once we got all of the Monte Carlo results we -- oh Mike, you might have been out of the room for this, because you were asking earlier whether we looked at logical groupings of results to get additional insights.

We did think that it would be useful to look at the steam generator tube rupture cases on its own and then the non-steam generator tube rupture case on its own in addition to the whole set, because we thought we could get some good insights, especially for the non-SGTR cases, that we could discover what would be important for that set that gets masked when you look at all of them together, because that's really driven by the fact that the steam generator tube ruptures have an order of magnitude higher release.

So the parameters that are important for that end up showing as masking what might be important in other population. Then we also looked at, since this was the first time we were looking at time at cycle, we also wanted to look at the time at cycle independent results. So we looked at the beginning of cycle, middle of cycle and end of cycle cases to see. So that's just the preface. We already talked about that and then --

MEMBER CORRADINI: So you took the 1,003 and broke them into subgroups?

MS. GHOSH: That's correct, right. So we had about 104, about ten percent that went to steam generator tube rupture. So we then -- we looked at the whole set and we segregated the 104 that went to tube rupture, looked at that. Then we looked at the complement and the whatever 900 that didn't go to tube rupture.

Then we had roughly a third of the realizations that fell in the beginning of cycle versus middle of cycle, end of cycle. So we looked at all of those groups about the need to see what we could discover about what becomes important.

For example, when you look at just the non-SGTR cases, when you look at everything that's dominated by what drives you to a steam generator tube rupture or not. So should I start this out and then you ask your question, or do you want to ask first? It's up to you.

CHAIRMAN STETKAR: Well, let's get the material stuff out of the way first.

MS. GHOSH: Okay.

MEMBER BALLINGER: Bill and I are pretty much on the same page about -- my comment earlier was going through a logical analysis of why you chose to treat rupture the way you did, in terms of just using wall thinning as a parameter. Bill is going to say that you could do it with wall thinning no matter what, no matter what the actual cause is, just changing the distribution.

So but I'm still interested in understanding why you didn't deal with cracking in an explicit way. I can't understand. There's no comment in there about how you went from it's an important thing to we didn't do it, because I don't see it in the -- you know, see what effect it might have been on the distribution, it you had chosen to treat cracking.

Same thing, right Bill?

DR. SHACK: Similar, you know. I don't have a problem with the parameterization of using thickness or --

MEMBER BALLINGER: But thickness is euphemism for -- yeah.

DR. SHACK: The distribution you chose, and again is this is one problem where you actually do know the ranges. That's the good news. The distribution that you chose just doesn't make sense to me, and unlike most cases, I think you actually have data for all this.

I mean you could have gone to Surry. They have multiple inspections. You could have seen what the flaw distribution looked like, you know, and so there was no need to make a distribution --

Again, your argument that you're looking at the most degraded tube, I get my gut feeling, without obviously having looked at all those inspections, is that you've got far too broad a distribution if you're really looking for a distribution of the most degraded tube in a steam generator.

Now so you're -- a non-conservative kind of distribution, you then team that with a conservative assumption that it's sitting in the middle of the hot spot. So I multiply a non-conservatism with a conservatism and I get ten percent, and I have no idea, you know.

Your results are just dandy if you want to see what happens and the consequences of a steam generator. Whether the ten percent number means anything at all, you know, is to me totally fictitious. But again, the main reason is why didn't you go back and actually look at the data?

MR. FULLER: Bill, I shouldn't need to remind you that since the industry has by and large replaced all the original steam generators, that essentially cracking is not an issue anymore. But volumetric wear and other volumetric mechanisms are.

DR. SHACK: I've got no problems with that.

MR. FULLER: Okay.

DR. SHACK: But you know what the wear and, you know. You can go to Ken Kowalski and look at the operational assessments for Surry. You'll find results from inspections. You'll find defect populations, and you know, whether those defect populations give you anything that look like the distribution that's chosen. I'd be surprised. I mean I could be wrong. That's certainly happened before.

MR. FULLER: I've been out of the steam generator business for a few years now. But I would be surprised if there was significant degradation in Surry.

DR. SHACK: Oh no. You probably plug a tube every time you look at it, just because you know --

MR. FULLER: From where?

DR. SHACK: From where. But that's just fine and dandy. They don't care why the tube dies, as long as -- you know, if the tube dies, it dies. So you've got a high degradation mechanism that may only affect one or two tubes. But they're looking for the most degraded tube.

I'll agree if I looked at the distribution of degraded tubes it looks a whole lot better. But if I'm looking for the most degraded tube in the steam generator, I would guess I'd get a pretty narrow distribution down near that bottom edge.

MR. FULLER: Indeed, for a volumetric mechanism, the probability of detection is very high.

DR. SHACK: Right.

MEMBER BALLINGER: Yeah, but then you won't get anything more than 40 percent through wall, because they'll be plugged.

MR. FULLER: That's correct. So what it comes down to is the wear rate.

MEMBER BALLINGER: Rate.

DR. SHACK: And your -- essentially your air, your probability of detection, your probability of sizing. I mean I can take the reported results. I can, you know, manipulate them for growth. I can manipulate them for errors. But I still end up, I would argue, with something that's going to be a whole lot narrower than the distribution they're using, which is basically spread over the whole range.

MEMBER BALLINGER: The probability of detection for cracks is way different than the probability -- if you get 40 percent through wall, the probability of detection is pretty darn high.

MR. FULLER: Yeah. But I think that as a practical matter, the likelihood of getting significant attack from cracking on the new steam generator tubes is much lower than on the original steam generators.

MEMBER BALLINGER: If it's 690 for sure, thermally treated most likely, and these are thermally treated.

MR. FULLER: Yeah. These are Alloy 600 thermally treated, yeah.

MEMBER BALLINGER: Yeah.

DR. SHACK: Well the last inspection I could find any information on, they did have some cracks.

MEMBER BALLINGER: For thermally treated?

DR. SHACK: Thermally treated.

MEMBER BALLINGER: But again, I think you know, I'm fully confident that most of the damage is where? That's just -- I mean my only question is why not use the data rather than, you know, an impressionistic kind of hand waving argument that got you to the triangular distribution with your mode at .69.

DR. SHACK: For 690 it will all be wear.

MEMBER BALLINGER: For 690 it will probably all be wear.

MR. WAGNER: So we cite NUREG-1740 for that 60 percent --

MEMBER BALLINGER: Yeah. That's the plugging limit. But that doesn't tell you, you know, what the actual flaws are. As I said, the flaws that you find would be determined by the flaws that you find plus your errors, your probability of detection, your probability of sizing error.

MR. WAGNER: Okay. So your recommendation is the site-specific?

DR. SHACK: Yeah, I do. Go talk to Ken Kowalski and see what he thinks of that distribution. Look at least at an expert.

MEMBER BALLINGER: Because it's going to be way narrower.

MR. WAGNER: Smaller wear?

DR. SHACK: No, no. Narrow towards the -- biased towards the bad end, because you're looking for the deepest crack. Now again I can, you know, that will give you a conservative result I think when you're all said and done. If you did that, if you got that distribution and then you timed it with your, you know, my most degraded tube sits in the middle of the hot tubes.

You might talk to Ken about whether that degradation is kind of randomly spread around. You know, could you take -- you know, could you look at the whole degradation profile and take samples of that and find out what the likelihood of getting a tube in the hot spot is. I can see that can lead to unconservative, but probably more realistic results.

MR. WAGNER: We're probably -- I mean to address that, we probably ought to beef up our modeling of the location of it and, you know, our hot tube modeling.

DR. SHACK: Yeah. Well the trouble is

that spot kind of moves around, and but --

MR. WAGNER: But I'm then thinking more distance away from the tube, you know. The round is -- I think we kind of accept that, that it could be --

(Simultaneous speaking.)

MR. WAGNER: But what you see is the temperature jumps pretty fast as you move into the tube any distance, and we based it all, you know, on that hot spot model near the tube sheets.

DR. SHACK: Near the tube sheet.

MR. WAGNER: So it's very conservative that way.

MEMBER BALLINGER: I think that's where the wear would be minimal.

MR. WAGNER: Yeah. So if we --

MEMBER BALLINGER: Up in the top is where it's going to be --

MR. WAGNER: If we did a better job modeling, you know, near the braces where there's maybe a little bit of vibration, we're going to get kind of two benefits. We'll get, you know, the full resistance getting to that spot and then we'll also get the cool down and what the stream would be by the time we got there.

We considered that, and there was already -- the model was getting very -- that requires if we're sampling on that to create that input for the model. There's some mechanics problems or complications, and we had already kind of had a bunch of that with --

DR. SHACK: Radionuclides.

MR. WAGNER: Yeah. So we thought by going over that full distance, we would survey the --

DR. SHACK: Well and again, as long as you only look at the consequences and the difference in consequences that's fine.

(Simultaneous speaking.)

DR. SHACK: Any credit to the ten percent becomes, you know, the real question here.

MR. WAGNER: Yeah.

MEMBER BALLINGER: And how far up before the temperature drops to say below 600 C, below 500 C?

MR. WAGNER: 500 C probably not. I mean the core return is -- so we're coming in there maybe 1,000 or 1,100.

MEMBER BALLINGER: Fahrenheit or Centigrade?

MR. WAGNER: Kelvin, Kelvin. So let me go to Celsius. I want to talk Celsius, so coming back at 500 Celsius. So we would be maybe a meter or so in.

MEMBER BALLINGER: A meter. So that's two or three support plates?

MR. WAGNER: Yeah.

MEMBER BALLINGER: Okay.

CHAIRMAN STETKAR: I just wanted to make a comment, Bill, in regard to that ten percent. The ten percent is important.

MEMBER BALLINGER: That's right.

CHAIRMAN STETKAR: Because I mean you said it well. If you don't care about the ten percent. In my mind, one of the notable conclusions of this work is indeed the fact that these consequential tube ruptures can be an important contribution to risk and early releases. Now regardless of what's been done about the consequences of those early releases, that's important.

Now if that was, you know, one-tenth of one percent, that's a much different conclusion. And if it's 30 percent, that's an even different conclusion, because you might start to pick up some other things.

MEMBER BALLINGER: And it was big to start with.

CHAIRMAN STETKAR: Bill, turn your microphone on.

DR. SHACK: Yeah, I agree. I mean it's an important number. It is. So you know, it's worth -- it's worth some attention.

MEMBER BALLINGER: Because I think if you find, if you look at the real data, you will discover that the wear pattern, if it's wear, is not down in the lower part of the bundle. It's in the upper part of the bundle. So we're saying we're choosing a hot tube to be the place where we have the maximum wear.

Well, I don't know that that's actually -- it's a conservative assumption, but I'm not sure it's an actual realistic assumption.

MR. WAGNER: Yeah. So we thought about trying to model, you know, the penetration to deeper levels to get the thermal conditions, thermal mechanical conditions.

(Simultaneous speaking.)

MEMBER BALLINGER: Depends on how far up you got hot.

MR. WAGNER: And that created a host of complications. So it was ruled out. But it was a step forward to do the hot spot modeling and to model the hottest tube and bring in the CFD there results.

DR. SHACK: Again, it will be fine to be consistently conservative, you know. What bothers me is I've now got a problem where I'm conservative in one thing and non-conservative in another, and I don't know where that leaves me. But if I was consistently conservative, then ten percent would be --

CHAIRMAN STETKAR: No. But Bill the point of this is not to be consistently conservative; it's to realistically assess the uncertainties based on our current understanding of the physics and the materials.

This isn't the licensing basis calculation where you have to have assurance that you're consistently conservative. It's supposed to be a realistic analysis with an appropriate assessment of uncertainty.

DR. SHACK: But sometimes you can assess uncertainties in terms of a conservative assessment, and if that's the best you can do without making --

MEMBER BLEY: And bound the problem, at least you know something. Or you don't know.

DR. SHACK: You know something. My problem is now I don't know whether this is a conservative or non-conservative, and so that's quite a --

(Simultaneous speaking.)

MR. FULLER: Can I shed some light on this? About more than ten years now I guess ago, there was foreign object wear above the tube sheet, an ANO tube that caused a leak and it was -- it was either the first or the second cycle after the steam generator was replaced.

Now that's kind of down where they're putting their failure here, right above the tube sheet. So as far as I'm concerned, the conservative approach is to assume you might get foreign object wear that's going to pretty bad damage.

MEMBER BALLINGER: But I don't see foreign object wear in any of this.

MR. FULLER: It's what causes --

(Simultaneous speaking.)

MEMBER BALLINGER: I understand what

causes it. But you're talking about creep rupture due to a hot --

DR. SHACK: Well no. You can get foreign object wear to give you degradation, because you can -- essentially it gives you the stress multiplier for thinness. I mean it doesn't have to completely go through. All it has to do is do some --

As I say, you can lump all kinds of degradation into the parameterization, you know. That's --

MR. WAGNER: Well currently we surveyed the bounds and our mode was picked, you know, based on when you would need to. If they caught it through their testing during outage, it would be caught at that 40 percent. So that was -- that was our rational, short of going to -- doing more elaborate modeling and going to point-specific data.

MEMBER BALLINGER: I mean I guess our overall point is you have this -- in this case, you have data that you can go look at.

DR. SHACK: Yeah. Nobody's going to melt down reactors for you to do a better job of some of that relocation. But in this case --

MR. WAGNER: Now what we found, I mean I think you're going to get to it, is the tube fitting was important but if you get a, you know, a hot -- if the high dry low conditions, we have a good chance of getting there before -- well not a good chance. But we have to have all those conditions, and then -- and then I guess we do have to have a week or two.

(Simultaneous speaking.)

CHAIRMAN STETKAR: KC, it's part of what Tina said is really introduction this morning is that some of these analyses in this work is being used and will be used to support the ongoing Level 3 PRA project for Vogtle. They are, to my knowledge anyway, explicitly trying to account for these consequential tube rupture scenarios.

A seismically induced station blackout is one way you might be able to get to those high dry low conditions. I personally, it's my own personal belief is that there are many other scenarios that may occur at higher frequencies that you can get to those conditions also.

Therefore, kind of understanding this and having a reasonable model could be quite important, not only for the narrow purpose of this particular

Surry SOARCA analysis or a comparison with Sequoyah, but in the grander scheme of kind of a more comprehensive evaluation of the sources of risk. So that's my bigger concern.

MR. WAGNER: Don Helton, yeah. If Don Helton's here, and he could kind of -- they have a little bit different approach in Vogtle.

MR. HELTON: Don Helton, Office of Research. I guess just to respond to Dr. Stetkar's point, we are following, sort of modeling a consequential steam generator tube rupture in this project, but we are also following the modeling in what we refer to as the C-SGTR project, which you've also been briefed on.

Probabilistically, we are following that project's approach more closely, so we can use it for the C-SGTR calculator that takes into account flaw distributions and other things.

So we are following it here and we are sort of periodically meeting between the three projects, to understand where we're seeing likenesses, where we're seeing differences, and obviously the ten percent that appears here versus different numbers that appear when we apply the different tools for those other plants is, you know, one of those focuses of those discussions. But I guess I would just encourage don't get too bogged down --

CHAIRMAN STETKAR: Don't get too -- okay, thanks, thanks Don.

(Simultaneous speaking.)

MR. HELTON: --with the fact that we're, you know.

CHAIRMAN STETKAR: And that's really good to have on the record. Thank you.

MS. GHOSH: Should I start the slides? Are there any more questions? I won't repeat the first bullet. You've talked about that a lot. I'll just note that when we did get a SGE on TR, we also got a hot leg nozzle rupture in every one of those cases.

CHAIRMAN STETKAR: Let me stop you at the second bullet.

MS. GHOSH: Okay.

CHAIRMAN STETKAR: And this I hope is easy. Somewhere I read, and it's in my notes here but it's kind of burned in, that the -- on average, the hot leg nozzle rupture occurred 28 minutes after the tube failure. In fact, if you looked at over, over all of the 104 or however many.

That to me -- Rick, I didn't look up the previous results. But my recollection is that in most cases, the hot leg was rupturing, failing before the tube rupture occurred in previous analyses. What changes -- is it only the changes in the characterization of the stainless steel sheeting of the nozzle and the nozzle materials? Is that what led to the delay?

DR. SHACK: In the hot leg failure?

CHAIRMAN STETKAR: In hot leg failure relative to the timing of the tube failure.

DR. SHACK: But they had lots of hot leg failures without steam tube ruptures.

CHAIRMAN STETKAR: It's just the timing of them. What I'm talking about is the timing. Previously, you got a hot leg failure so you never got the tube rupture, because you blew down.

MEMBER BLEY: And I think that started way back in the consequential tube rupture study as well.

MR. WAGNER: So I guess what -- I hope I'm answering your question. I guess if you look at NUREG-CR-6995, which was all the SCDAP/RELAP work that supported the tube rupture valuations. So they were working close with Chris Boyd at the time and they were running SCDAP/RELAP.

Consistent with what we're seeing here, we had to have a stress multiplier in order to get tube failure prior to hot leg failure. The wealth of analysis in probably a decade of work done using SCDAP/RELAP prior to us, we benefitted from their approach.

They looked and surveyed the stress multipliers all over the map, with much more sophisticated modeling than in some aspects than what we're doing. They needed a stress multiplier, about two or so in order to get a failure of the tube.

So if we had -- if we didn't have something in the hot spot or on the sending side or with a stress multiplier, our best tools would say that hot leg comes first.

Now in the original SOARCA, I tried to answer that question, because it came up from the peer review. So I prevented a hot leg failure and let it run more minutes, and what you see is on our measure for failure, creep rupture, it goes up orders of magnitude in the next to 10 to 20 or 30 minutes.

So it was a very compelling reason that

that hot leg's going to fail, given that there wasn't a pressurizer stuck open valve, that we get to damage levels that would give us complete confidence in hot leg failure.

So we had to have these stress multipliers in order to get the failure in MELCOR, and quite frankly we were doing the results based on the CFD to match the conditions that they had surveyed and the right boundary conditions for when we're in natural circulation, that would lead to, you know, a tube rupture.

MEMBER BLEY: I kind of understand everything you said, but I didn't -- don't think I heard an answer to what John asked, which is why are we now seeing ten percent of the time we're getting a tube rupture? Is it assumptions about the tube? Is it something different about the tube? Is it a change in the nozzle?

MR. WAGNER: Okay. Nope, we didn't include -- the nozzle had a minor influence.

MEMBER BLEY: And that the difference in the nozzle was just accounting for the stainless steel?

MR. WAGNER: Yeah, the cladding.

MEMBER BLEY: Then before we leave that

one, I have a little question for you.

MR. WAGNER: Okay.

MEMBER BLEY: In the real nozzle, you have a stainless steel clad nozzle. But you also have a weld --

MR. WAGNER: A safe cylinder?

MEMBER BLEY: Yeah, and the weld is of --

MEMBER BALLINGER: I sent you a picture.

MEMBER BLEY: Yeah, I saw the picture. It would be nice to put it up here. But the weld is of another material, and did you consider that weld, could it be a weaker spot? Was it in the modeling? I don't have any idea, or did you think about it and say it wouldn't matter?

MR. WAGNER: No. We thought about it, and you know, I think there has been NRC 3D conduction research on that, you know, more sophisticated 3D models trying to look at the temperature distribution. We have got a simple representation, so we're representing something that looks --

MEMBER BLEY: Kind of what I'm asking if you modeled that more realistically, might we see more or less tube rupture percentage?

MR. WAGNER: I think we would see more.

Oh, tube rupture, more hot leg.

MEMBER BLEY: You'd see more hot leg failure, because of weakness there.

MR. WAGNER: Right, yeah.

MEMBER BLEY: Then you didn't -- I cut you off before you got to the thing John was asking about. You said the nozzle was a small contributor to why we're now seeing more tube ruptures. What's the big contributor to that?

MR. WAGNER: Adding the stress multiplier.

MEMBER BLEY: Okay, and that wasn't done before at all?

MR. WAGNER: No, no.

MEMBER BLEY: Okay. It was just done in sensitivity studies before?

MR. WAGNER: Yeah, yeah. So in the original SOARCA, we didn't have the sophistication of a stress multiplier at the time, but we had to force it because our model wouldn't naturally develop it without a stress multiplier.

MEMBER BLEY: And now when you use it you get it, and there's our ten percent. Okay.

MS. GHOSH: Okay. The third bullet. Prior to core damage, the secondary side depressurized through main steam line leakage and safety valve failure to close. Actually, we talked about that this morning. And then we note that a safety valve on the primary side, on the pressurizer line failed to close in 68 percent of the realizations.

So actually in 32 percent of the realizations, we depressurized it some way before we hit that failure point. But it stopped cycling before it would have failed. But in 68 percent of the time, we did have a failure to close at one of the safety valves.

The steam containment liner yielded or tore in 74 percent of the realizations, and the containment area yielded in seven percent of the realizations, which led to a larger open area in the containment. So that the next slide just shows --

MEMBER BLEY: Just I think it's related. What fraction of the time, and it shouldn't have been much if at all, did all the safety valves fail closed?

MS. GHOSH: Zero.

MEMBER BLEY: Zero. They concluded that? Well, they kind of did. I mean there's a ten to the minus 5th chance of it happening or something like that.

MS. GHOSH: But because there was so much --

MEMBER BLEY: That's what I thought, but I just was wondering if it was any way related to that last thing you had there.

MS. GHOSH: Yeah, no. We did sample for that possibility, but it didn't occur in the thousand realizations.

MEMBER BLEY: That's close to reality, I think. Okay.

DR. SHACK: The 10,000.

MS. GHOSH: Yeah. We would have needed 10,000. But we did use --

DR. SHACK: But that still wouldn't have happened.

MS. GHOSH: No, we did -- it could have happened, ten percent chance in the 10,000 realizations. But we did do a sensitivity study, to see what would have happened if we had failed all three closed. But we continue to believe that's an extremely low probability outcome.

Okay. So this slide shows one of the primary metrics we care about, the cesium release to the environment, because really the cesium dominates the long-term health risks or the offsite consequences. So we're always interested in what is happening with cesium.

So this plot is showing the release for the 48 hour simulation time. We're only showing the first 300 out of the 1,003 successful realizations, and the reason for that is because it's interesting to try to take out some of the variations in the individual gray curve, each gray curve.

(Off microphone comment.)

MS. GHOSH: Right. If you put too many on the plot, then you can't -- they all just look like one big gray bar and you can't really pick out individual variations any more. Maybe three --

MEMBER BLEY: You still have to make space between the two points.

MS. GHOSH: Right, right, right, which is one of the key kind of outcomes of this. So each gray curve is one realization. So it's the results of one set of samples or parameters for each MELCOR input parameter that was sampled.

The summary curves are not -- are calculated statistics for each point in time. So they are not necessarily representative of any, you know, subset of realizations. The median kind of falls in the non-SGTR cases. But these are just purely arithmetic averages for the mean and then for the 50th and 95th percentile calculated at each point in time.

So we can clearly see the bifurcation in the results. We have one set of results, 104, that go to steam generator tube rupture. Those start on the earlier side and they result in an order of magnitude roughly higher, and the cumulative magnitude of release is by 48 hours and --

MEMBER CORRADINI: So can I ask. I know you're going to have conclusions. So the upper band is all steam generator tube ruptures.

MS. GHOSH: Yeah, are all steam generator ruptures.

MEMBER CORRADINI: Whether they one, two, three, five, because there was somewhere in your notes, in your explanations that five looks like three; three might be worse than one but so --

MS. GHOSH: So this is all for one tube. In the integrated uncertainty analysis where we varied all the parameters, if we had a steam generator tube rupture we failed one tube. We did a separate sort of mini-uncertainty analysis or joint sensitivity analysis, whatever you want to call it, where we varied the number --

MEMBER CORRADINI: Okay. That's what I was remembering. Excuse me.

MS. GHOSH: And that's documented in a separate part of the report, and there we sampled up to five tubes failing, not just one. We also varied -- the reason we call it a mini-uncertainty analysis is because we didn't vary all of the uncertain parameters.

But we varied those parameters that were most important to steam generator tube ruptures, so that we could try to get a sense of what the real variability might be and the results of having multiple tubes fail.

CHAIRMAN STETKAR: Just for reference, I stumbled over that also. So I went back and I looked at the consequential tube rupture NUREG that was published a year ago. I guess it isn't published yet, but -- and in that report they say that the conditional probability of a single tube failure, one and only one, is about 1E to the minus 2 for the conditions that they assigned.

The conditional probability of two tubes is about 5E to the minus 5. It's much, much, much lower, and more than two is negligible or whatever. They used the word "negligible." So it gives you a little -- because that's one of the things I was worried about is you did that sensitivity study and said well, it doesn't make too much difference if you get more than three.

MS. GHOSH: Yeah.

CHAIRMAN STETKAR: But the likelihood of apparently having more than one is, at least from that, whatever stage that research is in is pretty small.

MS. GHOSH: Yeah, and I think we've arrived on the same data and we had access to that draft report. That gave us confidence to use the one tube for the --

CHAIRMAN STETKAR: It's just when you reported the results of the sensitivity, you didn't discuss at all the likelihood of any of those conditions --

(Simultaneous speaking.)

CHAIRMAN STETKAR: You know, it just says well, we looked at one, we looked at two, we looked at three, we looked at more than three or something like that.

MS. GHOSH: You're right, and that's a very good point. I think that's why we were trying to characterize that as a joint sensitivity analysis rather than a true uncertainty analysis, because we have inflated -- I think we have inflated weight of sampling in a higher number of tube areas than one would expect as reality, because we wanted to --

It was really more to see how sensitive the consequences were. So we did have a higher weight of sampling in those higher number of tubes.

MEMBER CORRADINI: So I had a second question. So that explains one thing. So now I've got the gray lines above and I've got the gray lines below. Is the important variables that caused the upper set of gray lines to be two percent or, looking at all this and I'm not sure if that's two.

But let's say a few percent of cesium versus a few tenths of percent of cesium. Is that rank ordering of the parameters that caused that spread the same rank order of the parameters that caused the spread below?

MS. GHOSH: No, and that's why we were -- that's what I was trying to explain before. We did regression analyses on different subsets of the results. Because of this big spread in the results, the things that are important to steam generator tube rupture ended up being important to the entire population as a result.

MEMBER CORRADINI: Okay. So the answer to my question is yes.

MS. GHOSH: Yeah.

MEMBER CORRADINI: So for example, end of cycle is more important than the beginning of the cycle, for example?

MS. GHOSH: Well but okay. But let me just -- so I don't forget, if I could complete this. I can talk about that.

MEMBER CORRADINI: Sure, sure.

MS. GHOSH: So we removed the steam generator tube rupture and then looked at what are the things that are just important to the STSBO scenario as we have described it, if it hadn't progressed to a steam generator tube rupture.

You do see a different ranking of the variables that are most important to that, because now you're no longer just worried about whether or not you're going to drive to a steam generator tube rupture. So the ranking changes depending on what subset you're looking at.

For some things, and I think we talk about that later, like hydrogen, you know, production that doesn't matter --

MEMBER CORRADINI: Well if you're going to talk about it later, I'll wait.

MS. GHOSH: Because if it, you know, it doesn't matter to hydrogen production. But for something like cesium release magnitude, you know, it does make a difference and you'll see that in the regression.

MEMBER BALLINGER: Once again, not to beat dead horse on the steam generator tube rupture thing, but I look at this and I say my gosh, we really need to do the best job we can related to steam generator tube rupture and what the issues are.

MS. GHOSH: Okay, and that should be maybe independent of what percentage of the gray curves end up in that population.

MEMBER BALLINGER: Well, what I mean the net result is nobody dies.

MS. GHOSH: Exactly, right. Yeah. So the one other thing I'll point out, you know, this is one of those things. So because the statistical summary measures are basically mathematically computed at each point in time, you'll see that the mean curve is in the middle.

There's no actual outcomes that look anything like the mean curve. That's just a mathematical average of everything added together. But clearly we have these two distinct populations. The other thing, we talked about a reduced set. I know that the lower set of gray curves still kind of blend together.

But you can kind of see that there's some inflection points basically where you get liner yield or in the few percentage of cases where you also get rebar yield. The curve starts going up at a higher slope. We talk about later, you know, we did do some sensitivity calculations, because the question always comes up to 72 hours, to see what difference it would make in the source term.

The ones that are still going up, you get a higher -- you get a higher source and some are leveling out. But then you might hit a new yield point related to containment and you get that new inflection. So it kind of depends -- it's just dependent on kind of where you are in the sequence of things.

So we also wanted to show where the original SOARCA results lie with respect to the set of UA cases, and so we have here -- I think we already talked about this in the original SOARCA. We have the station, the short-term station blackout scenario on its own, and then we did the sensitivity for the steam generator tube rupture.

So that's what those two yellow curves are. The one on the bottom is the original SOARCA, STSBO unmitigated curve, and the SGTR sensitivity is the one that's closer to the gray population, just for a comparison of how things have progressed.

We just make a number of observations. The environmental release fractions are equal or lower from the UA for the Surry than the original calculation, except when an SGTR occurs, and in general you have an earlier start time to release. But the total magnitude at 48 hours is lower compared to the original SOARCA, and there are a number of reasons for that.

The earlier start time is because we are sampling a nominal leakage that goes up to one percent in this case. We did switch out the concrete type, which we talked about before. In the original SOARCA, we had the rebar yield at 25-1/2 hours, which we don't have any more with the new containment modeling.

This time, the uncertainty analysis SGTR results, the population for the most part are higher than the sensitivity we had captured, and due to one of the changes we talked about earlier, that KC also talked about, we have size-dependent aerosol capture.

So now we have a lot more of the smaller particles getting out in essence. So when we do have a steam generator tube rupture, we're seeing higher magnitude of releases on the original SOARCA sensitivity calculation.

MEMBER BLEY: If you'd leave that one back for a second. You don't draw -- on the new calculations, you don't draw the mean value without a tube rupture and the mean value with a tube rupture. But if I draw it by eye, it looks like on the right side probably the new mean without a tube rupture is close to the old SOARCA, where on the left side the new is about a factor of ten higher.

It looks like with a tube rupture, the mean would be about a factor of ten higher all the way across. So some of the things you said didn't quite jibe with my eyeballs. If we break it into the two pieces, with the tube rupture and without, they never drew the mean value for the new calculation under each of those cases.

They draw the mean of everything, which is halfway between the two cases. But the mean is going to be toward the high end of the gray areas for each one.

CHAIRMAN STETKAR: In effect the red dashed line --

MEMBER BLEY: Is a mean.

CHAIRMAN STETKAR: --looks like the median.

MEMBER BLEY: It's exactly the median, because ten percent -- half of ten percent is five percent.

CHAIRMAN STETKAR: An the green line on the bottom is closer to --

MEMBER BLEY: Not the mean. It's not high enough. It's not quite high enough. It's above the median.

MS. GHOSH: So I think that's a good comment. We can plot the means of those two populations, because I think it's a valuable comparison point. I believe the mean is lower than the yellow SOARCA curve, because we did look at the distribution --

MEMBER BLEY: But it's sure not the 430 hours.

MR. WAGNER: Oh no.

MS. GHOSH: No, no, no. You're right.

MEMBER BLEY: But on the right side, with 48 hours, I'll bet they're close. But it would be interesting to see when you actually do it, and with the tube rupture it's way above what you got.

MS. GHOSH: It's hard to tell and I apologize. It is hard to tell from this graph. But the gray lines at the 48 hour mark are getting pretty sparse by the time you -- because we're looking at --

MEMBER BLEY: Yeah but the high ones, you know, it's still --

MS. GHOSH: Yeah. Anyway, it would be somewhere between the median and the SOARCA line. But you're absolutely right, you know. Because of the new containment modeling with this higher leakage, the earlier releases are all higher than what was originally modeled.

MEMBER BLEY: Now there are some changes in modeling, but there's also uncertainty. I don't know that you've done this. If the modeling were all the same but now all we were seeing is the difference of doing uncertainty, we'd see that the uncertainty was clearly very important to understand what's going on.

I don't have a clue what the SOARCA yellows would look like if you did your old point estimates with the new modeling.

MR. WAGNER: Appendix A tries to do that for -- not for the steam generator tube rupture, but for the non-steam generator.

MEMBER BLEY: Yeah.

MR. WAGNER: So there, you sort of see a base case for the UA, compared to what would be representative of SOARCA.

MS. GHOSH: Yeah, but there's multiple -- sorry. But there's multiple steps in that appendix. The first comparison is just the straight conversion to 2.1. By the time you get to the end of that, near the end of that appendix, we have the comparison of the new base case, I believe. So I know it's a long appendix, but it is in there.

MR. WAGNER: Okay. They're the same code, so we remove that variability? So it's best approximation of the historical SOARCA with MELCOR 2.1 and what we are saying is the base case for the UA.

MEMBER BLEY: Did you actually do a curve or just some update or table on that?

MR. WAGNER: No, there's a lot of pots in there. It goes out.

MEMBER BLEY: I'm looking at it now. I haven't found it yet, but okay.

MS. GHOSH: Right. Yeah, there are a lot of pots.

MEMBER CORRADINI: It starts on page A-8 are all the curves.

MS. GHOSH: Did you have a comment? I don't know if I interrupted you.

MEMBER CORRADINI: No, I think KC answered my question. I'm fine.

MS. GHOSH: Okay. So next slide. This is the first set of regression results that we're showing. So I'm just going to quickly run through what's actually in these tables. You have a set of these tables for cesium, iodine, hydrogen and then the consequence results later.

The elements are going to be the same in all of the tables. So I mentioned in the introduction we used the same four regression methods that we use for Peach Bottom. The first is the linear rank regression. That's the traditional method, was used in 1150 and a variety of other studies.

That one doesn't capture non-monotonic effects and interaction effects between variables. The other three methods do do that. So the quadratic recursive partitioning in MARS are all more advanced methods that are able to capture non-monotonic effects or some interaction effects.

So what you have for the measures that you have in the rank regression, this SRRC is basically a measure of how much more variance that you can explain by adding the variable that's listed, you know, to the regression equation. The R square is the same. The SRRC kind of shows you the direction of the dependence, whether they're inversely related or directly proportional.

In the more advanced methods, the SI index tells you, gives you a measure of how important that variable is on its own, and the TI index gives you an indicator of the total importance of that variable.

MEMBER CORRADINI: So just so -- so for

no steam generator tube rupture, the path length for the leakage is the most important?

MS. GHOSH: Yeah. That's the design leakage. So the way that the leakage was translated is it's implemented as a leakage path, right? That's the translation of the leakage area.

MEMBER CORRADINI: So you invent the pipe length, it makes the leak less than a different pipe length?

MR. WAGNER: Yes, yes. So it gave us the .01 percent to one percent volume per day.

MEMBER CORRADINI: Okay. But maybe then I misunderstood an explanation you said earlier, because I thought -- I thought that was a deterministic coupling to the pressure and the failure. So that if I had low pressure, the probability of failure was low, but there was a deterministic leak rate at that pressure.

So because later on you've got down here, I forget what all these things stand for, but somewhere further down on the list is the CFC, and I think that's the failure. So I felt the two were linked. So if I knew the pressure, if I computed the pressure and I looked at that pressure and I looked at that, that would tell me the probability of containment failure.

With that probability of containment failure, there's some associated deterministic leak rate. I didn't know that the leak rate was also variable given a pressure and a probability. It seems like they should be deterministically linked. I've got a pressure or am I missing something?

MS. GHOSH: I think the CFC is giving you the yield pressures, and this is giving you the translation of leakage area.

(Simultaneous speaking.)

MEMBER CORRADINI: So can I give it back to you a different way? So you're saying if I have a -- if I compute a pressure with MELCOR and I look up on the CFC chart and it says your chance of failing at that pressure is five percent. Still, there's an uncertainty at what the leak rate would be at that five percent, and that's the D leak?

MR. WAGNER: Nope. The D leak -- why don't I describe it. There's two different leakage paths. One leakage is design leakage, and that's D leak and --

CHAIRMAN STETKAR: Oh. That's just what it's sitting there doing today.

MEMBER CORRADINI: Excuse me, I'm sorry.

Got it.

(Simultaneous speaking.)

MR. WAGNER: And then the other one is exactly kind of what you were describing.

MEMBER CORRADINI: Okay. So D leak is just the operational leakage whatever it might be in between the containment leak rate test. Then I have to screw it down to keep it within the limit?

CHAIRMAN STETKAR: Right, and they have uncertainty about that, anywhere from whatever it is, .01 percent.

MEMBER CORRADINI: Okay. So they're totally separate. I misunderstood.

CHAIRMAN STETKAR: Totally separate.

MEMBER CORRADINI: Okay, thank you. Sorry.

MS. GHOSH: Right. So then -- so I guess getting back to the metrics of reporting in the regression tables. So if you -- essentially if you subtract to the SI from the TI for the more advanced methods, it gives you an indicator of how strong that variables effects are in interaction with other variables, because the SI is what it's doing by itself. The TI is the total effects.

If you subtract the two, it tells you how

-- whether there may be strong synergistic effects with that variable combined with others. Now with Peach Bottom, we kind of left the regression tables as that. But it does get to be difficult to process all of that information.

So this time around, we added what we hope was a process improvement in trying to come up with a summary measure, which is the last two columns in this table. The first one is the main contribution, which we're calling the contribution of that variable acting on its own. The last column is the conjoint contribution, which is a summary measure of how influential that parameter is in interaction with other variables.

We came up with this summary by basically -- oh, the other thing I should mention. This final R square row right underneath the names of the regression methods tells you kind of how much of the variance in the output the regression model would be able to explain.

So the larger that number is, it tells you that the regression model came up with a way to explain more of the variance and the output results. So the way that we came up with our summary measures was to basically take the individual contributions from all four of the methods and weight them by how much of the variance that method was able to explain, given the total, I guess, some of the variance that all the methods we're able to.

So the equations are in the report. I don't want to say much more about it. It's in Chapter 3 if anybody's curious. But that's kind of how we tried to come up with a weighted summary of the importance of these variables, according to the regression models.

Okay. So that said, this is the first set of results. As I mentioned, we split the results into the SGTR and non-SGTR populations, as well as looking at all of them, because we do see different things pop up as important. So if you remove the SGTR cases and we're looking at all the cases that didn't go to SGTR, the design leakage turns out to be the most important parameter, in terms of the cesium release magnitude at 48 hours.

Then the next two that pop up as important are the time at cycle and the particle shape factor. Most of the uncertainty seems to be explained by those three parameters. But we highlighted in yellow those things that we thought were significant enough to say that they clearly have some important contribution to the variation in the results.

In this case, it also included the containment failure curve sampling, the deviations from the decay heat curves given the time at cycle, as well as the chemical form of cesium.

MEMBER BLEY: You rank these by the main contribution. But when you get on the conjoint contribution, you see a few down below that have quite a bit higher conjoint contributions, and you don't highlight those.

Even though -- even though it's a small individual effect like the next one that isn't yellowed, the conjoint's quite a bit higher and I'm just -- I haven't thought this all through very much.

MS. GHOSH: Right, right. I mean, you know, what we decided --

MEMBER BLEY: So it's making others -- it's combining with other things to make things more important.

MS. GHOSH: Exactly, right. What we ended up deciding, it's this very tiny footnote at the bottom of the table. We thought it was worth highlighting any individual contributions that were at least a .02 or larger, or if the conjoint contribution was .01 or higher.

MEMBER BLEY: And nothing is .01?

MS. GHOSH: Not in this table. But in other results we did have ones that showed up.

MEMBER BLEY: In some others they are, yeah. Okay.

MEMBER CORRADINI: So just help me. Conjoint means that so if I go look at D leak, if it's married with something else, it's a .077? But what is it married with that makes it .077? Who knows.

MS. GHOSH: Well right, and that's what we can't decipher from just looking at the regression results itself. This is telling us the statistical answer, and the way we try to get at that is through the single realization analyses, where we try to phenomenologically explain the differences and some of the realizations that we had.

So in the report, we have another section that documents our explanation of what happened in some of the more interesting realizations. We also look at scatter plots, where we try to see, you know, how samples, variables contributed to outcome. We try to take all of that together to come up with our conclusions and insights.

Because the regression methods are great, but it's one step, you know. It doesn't tell us the whole story. It gives us a statistical answer.

MEMBER CORRADINI: But just to push my point so you can say I'm wrong, if I look at SC-1132, which I looked that up; I couldn't remember what the hell that was, which is not the Zircaloy melt breakout temperature but the detected temperature, that's approximately 100 times less of a main contribution than D leak.

MS. GHOSH: Uh-huh, yeah.

MEMBER CORRADINI: And that's the first -- that's the first physical one. All the stuff before that doesn't surprise me. It's beginning of cycle, end of cycle, uncertainty and decay heat which we've always known decay heat's kind of important. It's the -- it's physical parameters in the machine, whether it be the leakage rate or the structural capability.

That's the first one that shows up. Well, there's a valve. I have that, but it's a valve. Sorry. It's just interesting that I go down a significant amount of contribution on the physical models. So are you telling me as long as I balance mass and energy, it doesn't matter what I do? Everything is the initial boundary conditions?

MS. GHOSH: If I could jump ahead a little.

MEMBER CORRADINI: Would you go back to hot drop and melt clog?

(Simultaneous speaking.)

MS. GHOSH: If you'd allow me. Well if we -- there are physical parameters. It depends on what you're looking at and by the way, I apologize. This slide should say "In Vessel Hydrogen Production." I don't know if it's still possible to correct this slide for the record, just because --

Anyway, this should say In Vessel Hydrogen Production. But I pull this up because there are instances where depending on what outcome you're looking at, that particular parameter that you pointed out does show up as important. For the in vessel hydrogen production, it's the second-most important parameter, which also makes sense, you know, because --

CHAIRMAN STETKAR: Tina, don't worry

about the slides, because we always publish the slides as an addendum to the transcript and you have now corrected this slide orally.

MS. GHOSH: Oh okay, okay, yeah, all right.

CHAIRMAN STETKAR: Thank you.

MS. GHOSH: Thanks. But with regard to the cesium non-SGTR, those are the results that we got. We also looked at the SGTR realizations separately. As some people have pointed out, you know, that is the higher magnitude release group. So we also want to understand what would lead to important differences in the release magnitude for that group of outcomes.

We have a different set of parameters that show up as important, and there too it's not surprising. The first two are the safety valve open area fraction and the tube thickness. I think tube thickness we've already talked about. It's intuitive. Of course that's important.

The safety valve open area fraction acts in two ways, I think, to be significant for SGTRs. One is it, along with other parameters such as the number of safety valve cycles that you experience, basically explains the deep pressurization rate for the most part that you have, which is important to whether or not you're driving to an SGTR.

So that makes sense on primary side. On the secondary side, it also acts as part of your area for release to the environment. We talked earlier about the fact that this may end up being swamped by the leakage area that we also modeled. But that's a potential contribution.

That's another one that we can't from the statistical analyses alone be able to separate the exact, you know, contribution in these different ways. But at least from a phenomenological standpoint, we can discuss those qualitatively as making sense, ways that it would contribute to the leak.

CHAIRMAN STETKAR: Well, that's why I was trying to probe this morning on that secondary side how important that is, because all I could read from everything is just that variable name and it's important. The fact that it's assigned to two different -- two different functions if you will.

MS. GHOSH: Yeah, yeah.

CHAIRMAN STETKAR: You know, what I heard orally is that it doesn't affect much on the secondary side. But what I'm hearing now is you're not really sure about that, unless you know something more about the model.

MR. WAGNER: No. I think it's extremely important on the primary side.

CHAIRMAN STETKAR: Yeah, obviously. Sure, sure.

MR. WAGNER: To get a --.

CHAIRMAN STETKAR: Got it, yeah.

MR. WAGNER: The second side is more confusing.

CHAIRMAN STETKAR: Okay.

MEMBER CORRADINI: So can I ask? So tube thick for this important. That I understand. So why is tube temp at the bottom of the barrel?

(Off microphone comment.)

CHAIRMAN STETKAR: Yeah. Well that one it makes sense, because there's no tube failure.

(Simultaneous speaking.)

MEMBER CORRADINI: It's the highest -- mine's on. It's the highest tube normalized temperature.

MS. GHOSH: Yeah. So okay. So it's an interesting thing, you know. Now we're not looking at all of the realizations together but just the subset that led to SGTR. So you know, one possibility is that there may be things that drive you to an SGTR, may be very important in deciding whether you get an SGTR.

But once you're within that population may not contribute a lot to release magnitude differences within that population. But I do want to -- I want to mention one other thing though.

CHAIRMAN STETKAR: But be careful then. What's your argument for SVOA frac on the primary side if it's driving you to the SGTR?

MS. GHOSH: Yeah. I think it continues to -- yeah, but I think it continues to contribute to the accident progression. Yeah, I don't know.

MEMBER BALLINGER: I don't know. It may be that the tube temp for these cases is already so high that it doesn't --

CHAIRMAN STETKAR: Ah, that might be. Maybe that's it.

MEMBER BALLINGER: That it doesn't matter. That's the only thing that makes sense.

CHAIRMAN STETKAR: The uncertainty doesn't make any difference --

MEMBER BALLINGER: Yeah, doesn't make any difference.

CHAIRMAN STETKAR: Because the absolute

value is always high enough.

MR. WAGNER: In a matter of minutes, with the temperature escalation, the lower sample gets there anyhow.

CHAIRMAN STETKAR: Yeah, yeah. That could be.

MR. WAGNER: Now the SV frac was -- we just had to have the primary system at high pressure. You can see that in all these cases. If we had a larger failure, we just didn't get any tube ruptures.

MEMBER REMPE: Well what's interesting, again I'm kind of thinking about what you said. Well Sequoyah, because of this plot you showed us earlier today, what was it, how many days that the beginning of cycle becomes mid-cycle and end of cycle.

Earlier today you also said this was a major effort to include this, but we decided not to with Sequoyah because we decided it wasn't so important. It seems like you didn't need to do all of the analysis to decide that beginning of cycle would become mid-cycle within so many days and all that effort.

I mean I'm not throwing stones at you,

but I'm just wondering some of the history behind that.

MS. GHOSH: Yeah. I don't think that tells the whole story. The story is more complicated. It's always more complicated. But there are multiple aspects to the time at cycle. I think there's the inventory as well as the decay heats. I think the answer we gave is not the whole story. I don't know if you want to elaborate.

MEMBER REMPE: John said we could stay until midnight tonight. Go ahead.

CHAIRMAN STETKAR: Some of us are going to need to take a break here and I was hoping we'd get through the cesium regression analysis before we do that, but keep going.

MEMBER REMPE: Because I mean it's showing up as important here and that's why I had that perception.

MR. WAGNER: The BOC stands out and sometimes the statistics will pick up that low BOC. You can -- Cal just made last week or something, he plotted all the containment pressurizations and I believe it's in the report too, and the BOCs really kind of stand out. So you -- for the non-SGTR, that's where it pops up as, you know.

MEMBER REMPE: But it isn't doing too bad here for the CG SGTR. It's showing up in one of the highlighted areas.

MR. WAGNER: Yeah. So it's a little bit hotter and temperatures are a little bit hotter and we get to hotter times before we could fail a valve. So all those facts. I probably misspoke a little bit on it.

MEMBER REMPE: But the bottom line is that even though it's showing up as important, it was decided, because it was so much work, not to include it in Sequoyah then later on even -- because everyone stepped back and said well, eventually it will go to mid-cycle.

MS. GHOSH: Well, I don't want to get too much into Sequoyah, because I think we're going to come back to -- we're going to come back to you all and talk more about it. The Sequoyah analysis was meant to be a reduced scope in some ways, because we've done all these work on Surry and there's ongoing work on Vogtle and other things in terms of PWRs in general.

But it was supposed to be expanded in scope specifically for ice condenser, you know, parts of the analysis. So you know, the team kind of tried to make a judgment of which were the most important parameters to include, to be able to explain ice condenser-specific, you know, variations and what might happen.

In terms of the time at cycle, I think we felt we had done enough with Surry that we could -- we have some idea of what the magnitude and nature of the effect is of having the beginning of cycle versus the middle or end of cycle.

We're not sure what more we would have gained by continuing to include that for Sequoyah. We could probably have a separate qualitative discussion that explains what the impact is if you're closer to beginning of cycle versus middle and end of cycle.

We just -- for the purposes of that analysis, we didn't think it was worth continuing to include that, given that we did do all this work for Surry and we do have very good insights into the differences.

MEMBER REMPE: Okay.

MS. GHOSH: But we'll be coming to talk to you about that certainly at some point.

CHAIRMAN STETKAR: I'm now going to intercede. A couple of constraints. We need to finish no later than, no later than 5:30. So despite of all of my ranting, we will finish by 5:30. Let's take a recess. I'm going to try to hold us to ten minutes if we can do that. Let's come back at 3:30.

(Whereupon, the above-entitled matter went off the record at 3:21 p.m. and resumed at 3:30 p.m.)

CHAIRMAN STETKAR: I'm going to try to get us done by 5:30. So we are back in session.

MS. GHOSH: Okay. I think we finished talking about the cesium regression results. This is a set of results for iodine. The general characteristics of the iodine releases pretty much follow what's going on with cesium. You see the two groups, SGTR versus no SGTR. You see the inflection points for the non-SGTR where you have various containment yield points. And we have in the backup slides the comparison to the original SOARCA curve, but again there wasn't anything terribly new and interesting compared to the cesium comparison.

MEMBER BLEY: Is that in the package you gave us?

MS. GHOSH: Yes, it's in the back,

towards the back. You have some backup slides there.

So for the non-SGTR realizations for iodine we see a similar set of results in terms of what's important, what's most important to the cumulative magnitude of iodine fraction release by 48 hours. The time at CYCLE here is -- it becomes most important, but the containment failure curve sampling and the design leakage rate sampling continues to be important. And we also see here the chemical form of iodine becoming a little bit more important. And it makes sense for the reasons we discussed before. We're sampling on how much of the iodine is gaseous which is much more mobile. So it makes sense that that pops up for iodine.

This is the distribution of the in-vessel hydrogen production.

MEMBER CORRADINI: Can we get a clarification?

MS. GHOSH: Yes?

MEMBER CORRADINI: So we're just debating with each other here. I think I understand why CYCLE beats D leak and CFC, okay, for iodine. I think I understand that. But does the sum of the main contribution have to add up to one? So if one wins, something else has to lose in terms of contribution?

MS. GHOSH: Let me think about --

MEMBER CORRADINI: In other words, if CYCLE is 0.38, the larger the main contribution is with the top dog, do all the other ones suffer?

MS. GHOSH: Yes, and I think that's generally true. The way we have done it, because we're showing you a summary measure across the four methods, it may not add up perfectly; and Dusty can correct me if I'm wrong, but that's roughly the idea.

MEMBER CORRADINI: So it does have to add up to -- it all has to sum up to the same value?

MS. GHOSH: Yes.

MEMBER CORRADINI: So if one gains in importance, the others have to lose in relative importance.

MS. GHOSH: In relative importance. Exactly.

MEMBER CORRADINI: Okay. Then I understand.

MS. GHOSH: It's all relative, right.

MEMBER CORRADINI: Okay. All right. That explains it. Thank you.

MS. GHOSH: So this is the in-vessel hydrogen production. And I would say it's fairly well-behaved. Everything is generated by 10 plus a little bit hours. And the spread is roughly between and 200 and 600 kilograms at the end of 48 hours. And the regression results are -- in this case it doesn't matter whether or not you have an SGTR. And the most important parameters that show up are really a proxy for depressurization, which in this case is the safety valve open area fraction as well as the effective melt temperature.

MEMBER CORRADINI: So two things here.

MS. GHOSH: Yes.

MEMBER CORRADINI: So the one thing is that this all occurs before this bifurcation of the -- most of it occurs before the bifurcation of steam generator tube rupture. Things start cooking inside the vessel before we decide that we go hot leg or go steam generator tube. I think I'm correct there.

The second thing is you said that you accidentally had the wrong concrete. Where do I see the accident had the wrong concrete effect? Because I would assume I produce a whole lot more hydrogen with limestone common sand.

MEMBER BALLINGER: This is in-vessel.

CHAIRMAN STETKAR: This is in-vessel.

MEMBER CORRADINI: Oh.

CHAIRMAN STETKAR: This is in-vessel. She corrected this slide.

MEMBER CORRADINI: Sorry.

MS. GHOSH: Yes.

MEMBER CORRADINI: I'm sorry. Sorry. Thank you. Never mind.

MS. GHOSH: Sorry about that.

(Simultaneous speaking.)

MS. GHOSH: And I think this is our last slide for this section. As I noted earlier, we did extend a handful of the realizations, nine of them, out to 72 hours to see what the effect would be on the release fractions of any containment failures that might occur beyond 48 hours. And I think we already talked about this. In essence, for cases in which the rebar yield was reached, the pressure temps to level off and then gradually decrease as leakage more than compensates for the steam generation and heating of the atmosphere, but in some cases there are marked increases in cesium and iodine release at the pointer of liner yield or rebar yield. And if that happens after 48 hours, you can get up to an order of magnitude increase between that 48 hours and 72-hour simulation times.

Any questions before we move to the MACCS analysis results?

MEMBER REMPE: Just a question on this slide.

MS. GHOSH: Oh, yes, thank you. Joe's reminding me. Oh, go ahead. On this slide, yes.

MEMBER REMPE: Yes, it just says that the results are in the process of being updated. And I didn't hear that today. I didn't remember seeing that in the report.

MS. GHOSH: No, this is fresh, hot off the press --

(Laughter.)

MS. GHOSH: -- that information.

MEMBER REMPE: What was the reason. I wanted to ask earlier, but I didn't.

MS. GHOSH: Before we get to that, can I just ask -- Kyle Ross has looked up the information on the numerator that you were asking about from NUREG/CR 7037, and he's prepared to discuss it --

CHAIRMAN STETKAR: Oh, okay.

MS. GHOSH: -- when it's a good time. Is this a good time?

CHAIRMAN STETKAR: This would be a great time, because otherwise we'll lose it.

MR. ROSS: Yes, so someone was kind enough to print out the NUREG, the question, and I left one page on my chair, but there's a table 20 on page 42 that I worked from. And there's information here for main steam system valves and reactor coolant system valves. And there is failure to close and failure to open.

MEMBER CORRADINI: Where are you? Can you say again where you are?

MR. ROSS: It's in a different NUREG.

MEMBER CORRADINI: Or a different NUREG?

CHAIRMAN STETKAR: It's a different NUREG and it's -- I printed out the tables in the appendix.

MR. ROSS: Yes, I think it's NUREG/CR 7037.

MEMBER CORRADINI: What was that page number again?

MR. ROSS: Forty-two.

CHAIRMAN STETKAR: Okay. We've got the table, table 20.

MR. ROSS: Yes, it will be -- you see the differentiation between main steam system valves and reactor pump system valves.

CHAIRMAN STETKAR: Yes.

MR. ROSS: And the distinction between open and closed. So it's failure to open and failure to close distinction.

CHAIRMAN STETKAR: Yes.

MR. ROSS: And all that I looked at was the non-recovery probability numbers.

CHAIRMAN STETKAR: Yes.

MR. ROSS: So for failure to close there were 769 demands and 5 failures.

CHAIRMAN STETKAR: Yes, but --

MR. ROSS: On the bottom half of the table for the reactor coolant system valves there were -- again under non-recovery probability there were --

CHAIRMAN STETKAR: Yes.

MR. ROSS: -- four demands and two failures. So summing those, assuming that the valves on the secondary side and valves on the primary side are quite similar --

CHAIRMAN STETKAR: Yes.

MR. ROSS: -- then you have seven failures to close out of 773.

CHAIRMAN STETKAR: Right. In table 4-2

in your report the distribution is based on 17 failures to close in 773, not 7.

MR. ROSS: Yes, it does. Yes.

CHAIRMAN STETKAR: Yes, it does. And the distribution actually comes out -- I ran out the distribution.

MR. ROSS: Comes out with --

CHAIRMAN STETKAR: 17/773 is the expected value of the data.

DR. SHACK: It looks like it's all failures recovered and non-recovered plus recovered and non-recovered.

CHAIRMAN STETKAR: Yes, and most of these are actually from -- the main steam stuff is all from boilers, because I don't think they -- if you go back to the appendices, the appendices don't have pressurized water reactor steam safety valves, which is fine. I'm not arguing about compiling the two. But anyway the table 20 is what you used?

MR. ROSS: It is.

CHAIRMAN STETKAR: Okay.

MR. ROSS: I guess I went back to my chair thinking it was seven. I'm looking for 7 and 17 was --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Well, I found some 15s and 2s and stuff like that, but I couldn't find any 17s with 773. So I was --

MR. ROSS: Yes.

CHAIRMAN STETKAR: -- I honestly tried to kind of gin up those numbers, and I couldn't get them. Anyway, that's -- I did not look at this table. I was back in the appendix. There are several tables in the appendix for both pressurized water reactors --

MR. ROSS: Okay.

CHAIRMAN STETKAR: -- and boiling water reactors. And I was trying to compare those two and add up demands and failures and I still couldn't get them to add up.

MR. ROSS: No, I can understand.

CHAIRMAN STETKAR: And this is -- I didn't look at this table, so I don't know how they compiled the values in this table from the appendix.

MR. ROSS: The other issue, on the next page is table 22, and it is failure probabilities based on testing. And the numbers are so dramatically different than the ones that we just talked about.

CHAIRMAN STETKAR: Yes, and as we were talking earlier, I -- from my own experience for failure to open testing in many cases they'll report a failure if it opened at a half a pound higher than the range on its set point. Failure to close on the other hand might be valid from testing, because once it's open if it sticks or binds mechanically, that might be valid. I don't know how that stuff is reported.

MR. ROSS: Well, what spooked me out from using the failure probabilities based on testing is that they are so dramatically different than the ones from behavior after scram.

CHAIRMAN STETKAR: Yes.

MR. ROSS: That's why I used the 17 out of 773.

CHAIRMAN STETKAR: But part of what I hung up on, if you go back in the appendix to this report, if you look at table B-6 in the appendices -- I think it's B-6. Let me get to it. You may not have it there in front of you, but it's failure probabilities for pressurized water reactor code safety valves. SVV failed to open not recovered given a scram is 0 in 773. And the 773 demands, scram related demands for pressurizer safety valve is what I was taking issue with. And that 773 happens to be the same denominator as your 769 plus 4.

MR. ROSS: Right.

CHAIRMAN STETKAR: So anyway, at least in the interest of time I think we probably both need to do a little bit more homework on this. And thanks for pointing me to that table 20, because I immediately went to these tables in the appendix.

MS. GHOSH: Okay. So getting back to your question, our cryptic note that you may have noticed here. So as often happens with our codes, sometimes we discover issues, and we discovered an issue very late last week in the middle of preparing for this meeting. And we're not prepared to talk a whole lot about it today, but we discovered that it does affect our MACCS results for the Surry UA. It was an issue with MELMACCS where it was case-sensitive in a way that we didn't anticipate or didn't know, so that the MELMACCS translation of the MELCOR output to MACCS ended up inputting to MACCS a source term that was roughly a factor of two lower than what it should have input to MACCS.

So actually over the weekend we re-ran

most of the MACCS calculations. We still need to rerun the sensitivity calculations from what I understand. We reran the regressions. And we know that we need to update all the results in the report, but we literally just found out about it late last week, so we couldn't up[date the report or all of the slides in time for this meeting. But this is just to let you know that the report is going to be updated. So all the quantities are going to change in terms of the tables and the graphs that are there for the MACCS results, but our conclusions and insights don't change.

We reran the regressions, essentially the things that were previously shown to be important. For the most part the most important things continue to show to be important. And we have in the slides an example just to show you -- give you some indicator of the magnitude of the changes that you might eventually expect.

So the only comparison we have here is just for the 0 to 10-mile latent cancer fatality risk. And on the left we're showing you the complementary cumulative distribution functions for LCF risk for the five radial distances, the circular distances. And I see our label is gone.

This is for all five distances. On the right is the old LCF risk results. And what we've put in the table, just to give you some indication of the magnitude of the fact, is how much higher the new results are to the old results.

So the source term roughly went up by a factor of two. As we expected, in terms of the lower percentiles up through the median, and we don't know where this levels off, there's a roughly linear effect between source term and the LCF risk. When you get to the higher percentiles, it kind of starts curving over like this, so you end up with a sublinear effect. So by the time you get to the mean, the new mean is about 1.6 times as high as what we previously calculated. The 95th is about 1.5 times as high. But that's just to give you a sense again of the magnitude of the fact.

So we're going to update the report with all of the new results, and so all the graphs and tables will be replaced. But again, it doesn't change our conclusions. And what we see is still showing up as important in terms of the regressions are the same.

So later in this portion of our talk we do show some other consequence results. Those are going to be updated. But in terms of the insights we don't expect them to change. So we thought it would still be worthwhile to talk about it today.

MEMBER SKILLMAN: Tina, how confident are you that you've been thorough in identifying all of the areas that are affected by that change in source term?

MS. GHOSH: You mean for this project? Right now we're doing kind of an extensive -- we're in the middle of an extensive condition evaluation to understand how it impacts other projects. We know that it doesn't impact the Peach Bottom uncertainty analysis that we did. It did impact also the Sequoyah uncertainty analysis we did, but we haven't come to you with those results yet. So we'll update them before we come to you.

MEMBER SKILLMAN: So you're doing some form of an extended condition review?

MS. GHOSH: Yes. Yes.

MEMBER SKILLMAN: And is that part of your internal processes here at the NRC, or is that something you're doing just as a matter of being good soldiers?

MS. GHOSH: Well, I think -- well, Sandia is conducting that, but I think that is -- whenever we find an issue, we need to know what it impacts with regard to our ongoing and past projects. I think it's part of our standard processes. I don't know if it's formal somewhere, but we're doing that evaluation now to see what else might be affected.

MEMBER SKILLMAN: Thank you.

MS. GHOSH: Okay. So I'm going to turn it over to Nate.

MR. BIXLER: Okay. So this is sensitivity regression analysis for -- in this case we're looking at within 10 miles and we're looking at all 1,004 realizations and looking at the most important parameters that affect the results. And the two that go at the top are tube thickness and SV open area fraction, which are the two parameters that Tina talked about earlier as being highly influential on determining whether you get an SGTR or not. So roughly you get something like an order of magnitude jump in the source term depending on whether you get the SGTR. And these two parameters influence that, so it's not hard to imagine why they should be right at the top.

The next parameter in order there is the time at CYCLE. And as was described before, the primary difference is between beginning of CYCLE and middle -- end of CYCLE. But that does end up having kind of a twofold impact on the max results because it influences the MELCOR source term that's calculated, but it also influences the core inventory that goes directly into the MACCS input that determines if we have a release fraction from MELCOR. That release fraction multiplies a core activity and then -- so it directly impacts the consequences through that.

MEMBER SKILLMAN: Nate, throughout these slides and also in the report the words "circular area" are used. From my background it was always radius near the center --

MR. BIXLER: Yes.

MEMBER SKILLMAN: -- for emergency planning and for EPZs.

MR. BIXLER: Right.

MEMBER SKILLMAN: Is the 10 mile and the 50 mile and 20 mile the same as the radius as in an EPZ?

MS. GHOSH: Yes.

MR. BIXLER: Yes, that the radius. Those distances are the radius defining a circular area.

MEMBER SKILLMAN: I thank you for the clarification. I did not find that anywhere in the report. I was looking for it as a definition.

MR. BIXLER: Okay.

MEMBER SKILLMAN: Okay. Thank you.

MR. BIXLER: Okay.

MS. GHOSH: Yes, thanks for that. Yes.

MR. BIXLER: Thanks for that comment. That's a good comment.

Okay. And then the fourth parameter on the list that -- I think five are highlighted here, I believe. The fourth one of the list is groundshine shielding factor, which I had described earlier as being one of our uncertain parameters. It's accounting for things like the time spent indoors versus outdoors and how protective a building is in terms of reducing groundshine dose, etcetera. So that turns out to be a very important parameter. Not surprising also, since groundshine is the dominant dose pathway for the overall analysis.

And then I think the last one that's highlighted is the D leak parameter. That's related to the design leakage rate that's used in the MELCOR. So those are the most important parameters. And I think we see those parameters showing up quite consistently through most of our results.

Let's see, this is for -- by the way, these are all LNT. The last slide and this one are LNT results. And those are the ones that I'm going to talk about today. This is also for a 10-mile area, or 10-mile radius, 10-mile circular area, radius of 10 miles, but just selecting the cases where there is no SGTR. And in this case time at -- since we don't have an SGTR, the parameters that influence whether you get an SGTR are not on the list here, or at least not important. They're not at the top of the list. But the time at CYCLE is important. The groundshine shielding factor, the design leak rate.

And one additional parameter comes up here as being important. That's the risk factor for the -- No. 8 there stands for the residual cancer risk factor, which ends up being the most important contributor overall to risk of all the risk factors that we have on the list. So that one ends up being an important one.

Okay. Here we're looking at a 50-mile radius, circular area with 50 miles, all realizations. And so again the tube thickness and the SV open area fraction show up as being the most important ones, groundshine shielding factor as also important, but down the list.

And similarly to the results that I presented before, this is 50-mile radius, but just the subset without SGTR. And here CYCLE is at the top. So the time at CYCLE, the groundshine factor, the design leak rate, and again the cancer risk factor for residual cancers.

Okay. These are some single realization examples that we looked at. There are two curves for each color. One represents 0 to 10 miles. The second one represents 10 to 20 miles. The 10 to 20-mile one are all lower than the 0 to 10 miles. And the yellow curve here, or the two yellow curves represent the basic Surry uncertainty analysis results that we got. And what's different about the colored, the blue, green and red-colored curves is that those are for single MELCOR source term results, but we're sampling on the other MACCS parameters other than source term. So we're just looking at an individual source term description definition and looking at all the other uncertain parameters in MACCS in those, in the blue, green and red curves.

PARTICIPANT: Can you say that again --

MR. BIXLER: Okay.

PARTICIPANT: -- because I don't understand.

MR. BIXLER: Okay. Sure. The yellow curves are SOARCA uncertainty analysis results, so those consider all of the uncertain parameters in MELCOR plus MACCS.

And the other curves, the blue, green and red are just looking at MACCS uncertain parameters for a fixed MELCOR source term. And the blue is -- it's pretty obvious from looking at the curves here, but the blue curves are for relatively small end of the source term range, green somewhere in the middle and red towards the upper end of the source terms.

And one thing you can see is that the curves by and large overlap each other, or overlap with the yellow curve, except for the red ones at the upper end of the range extend beyond the yellow curve, which indicates that there are some combinations of large source term with other MACCS parameters that would give you an ever larger risk that are not captured in our basic uncertainty analysis.

CHAIRMAN STETKAR: If I look at this,

though, one thing it tells me, if I compare the general shape and -- let's say the range of the yellow to the range of any of the other colors, is that most of the uncertainty in the overall Surry results comes from the MELCOR part of the analysis and not from the MACCS part of the analysis.

MR. BIXLER: Yes.

CHAIRMAN STETKAR: I guess that to me is a bit surprising.

MEMBER CORRADINI: I was going to say didn't you make that same comment when we were in Peach Bottom? I seem to remember.

CHAIRMAN STETKAR: I probably did --

MEMBER CORRADINI: You kind of pondered the table a little bit.

CHAIRMAN STETKAR: -- but I don't remember this morning.

MR. BIXLER: Yes, we found the same relationship between the two sources of uncertainty in Peach Bottom that we see here, and that's that source term contributes more to the overall uncertainty than all the other uncertain parameters that go into the consequence analysis.

CHAIRMAN STETKAR: Do we actually believe that?

MS. GHOSH: Yes.

MR. BIXLER: I think we do, yes.

MS. GHOSH: I think we do, but let me just add one thing: Just a reminder that the distribution we're plotting here is the distribution of the means from all the weather trials.

CHAIRMAN STETKAR: Right.

MS. GHOSH: So in the Peach Bottom work, some of the add-on work that we did, in fact in response to questions that were raised by the ACRS, we also separated out the contribution of the weather variability and the source term variability and the MACCS variability.

MR. BIXLER: Yes.

MS. GHOSH: The weather variability actually is a significant contribution as well. Here the reason you don't see it pop up is because we're looking at the means of the weather variability. So here we're just looking at the contribution of the epistemically uncertain MACCS parameters.

MEMBER CORRADINI: So can I say it differently? You have all sorts of relatively fancier terms from me. The weather is highly variable. We don't see it here because you've taken the mean value over a year?

CHAIRMAN STETKAR: But you do see it in the yellow?

MR. BIXLER: No, you don't.

CHAIRMAN STETKAR: You don't?

MR. BIXLER: Yellow is also averaged.

MEMBER CORRADINI: Oh, okay.

MR. BIXLER: These are all points that are averaged --

(Simultaneous speaking.)

MEMBER CORRADINI: So I would expect to see the weather would broader all this out?

MS. GHOSH: Exactly.

MR. BIXLER: Well, it would.

MS. GHOSH: Exactly.

MR. BIXLER: It would if we plotted that along with the other uncertainty. But let me make one more point: There's one additional uncertainty that we're not accounting for here because these are only LNT results, and that's the uncertainty in dose response. If we were to include that and add that as an uncertainty, then it would broaden things quite a lot.

CHAIRMAN STETKAR: That's clear, but I'm

trying to hold us to within the constraints of what was done.

MS. GHOSH: Right.

CHAIRMAN STETKAR: And given the fact that the yellow and the other colors are all still based on the mean of the weather, then my comment still holds that most of the uncertainty comes from the MELCOR part of the equation and not from the MACCS part of the equation. If we were to add the uncertainty in the weather to both, we would see a broadening, a flattening, if you will, of these curves, but they would flatten in a relative sense the same way.

MR. BIXLER: That's right.

MS. GHOSH: Yes, I think that's right.

CHAIRMAN STETKAR: And that's why I ask if you're just looking at the non-weather related uncertainties in MACCS, evacuation times and dose response correlations and things like that, it's surprising that those uncertainties are relatively modest if I look at an order of magnitude spread on those curves.

MS. GHOSH: And I think we believe the results, you know the -- a lot of the MACCS modeling is tied to the habitability criterion, which kind of fixes the long-term dose rate that people are able to incur. And that's set at 2 rem the first year and then 500 millirem all subsequent years. So once you get past the initial emergency phase -- and we find again here that the long-term dominates that the health risk is from people either never having to evacuate because their dose rates meet that criterion or coming back after that dose rate is met. That kind of provides a backstop in how much dose you can end up incurring over your lifetime.

CHAIRMAN STETKAR: I get that. What I'm challenging is have we adequately captured the uncertainty in the emergency phase, the early part of this stuff?

MR. BIXLER: For this particular calculation the doses and health effects that would potentially stem from the emergency phase are a smaller contributor. They don't contribute 50 percent. I think on the average more like 25 percent to the whole. So variations in that really don't have a gigantic impact on the final results that we're plotting here.

CHAIRMAN STETKAR: Okay. Yes, I was going to say I guess I can understand that in latent cancer.

MR. BIXLER: Yes, our releases are not that early. There's time at least to start evaluation.

CHAIRMAN STETKAR: Latent cancer, LNT, I guess I can grudgingly accept that.

MEMBER BLEY: Kind of what you're looking for I think is if in fact evacuation were greatly delayed, would that have a big impact on early fatalities?

CHAIRMAN STETKAR: Yes, exactly. Yes.

MEMBER BLEY: Or if you got caught.

CHAIRMAN STETKAR: Because you just get such a -- under the LNT assumption you just get so many people dosed from --

MEMBER BLEY: A little bit.

CHAIRMAN STETKAR: Well, re-habitability dose --

MR. BIXLER: Yes.

CHAIRMAN STETKAR: -- in the long term is that I guess it just swamps anything that you could possibly get by dosing at the initial -- people during the initial --

MS. GHOSH: Yes, so we don't talk in this -- I think we don't have slides on the early fatality risk, but we did compute them. And we do get a handful of non-zero numbers, which is -- it's the model validation exercise so that we can compute these small numbers. But I think there is a non-evacuating cohort that's sitting there and doesn't evacuate. And I think the only time they do is -- we assume that if they're told that they're sitting in a hot spot, eventually they get out, but that happens at a later time when they've already been sitting in the plume and so on.

CHAIRMAN STETKAR: Yes.

MS. GHOSH: We did vary the delays to evacuation and how slow it would be, even taking all of these things into consideration. And we did have some releases that started on the earlier side, given the ETEs for Surry, but I think for the reasons Joe mentioned earlier; maybe you're about to repeat them, we don't see that overlap where we're getting -- we're able to get large doses in the early phase to people.

MR. JONES: This is Joe Jones. What I wanted to add was that as I mentioned earlier we have depart times, but we have a cohort that is the evacuation tail that Nate mentioned earlier. And if you look at the depart time, which includes a delay to shelter and a delay to evacuation, and if you look at the upper bound of that, they wouldn't leave for almost 13 hours.

And if you look at the lower bound of the speed, which is a half a mile an hour, you're looking at almost 20 hours before some of these people would be out of the area. And so we did look at the results and found that there was no relationship between these longer leaving people and the consequences, which leads us to believe it's the non-evacuees, as Tina just mentioned.

CHAIRMAN STETKAR: Okay. But the non-evacuees are definitely sheltered?

MR. BIXLER: The non-evacuees are just going about normal activities.

CHAIRMAN STETKAR: Yes, well, but 70 percent of the time, or whatever you did there, they're inside?

MR. BIXLER: Yes.

CHAIRMAN STETKAR: In some sort of --

MR. BIXLER: Right around 80 percent of the time we assume they're inside and the other fraction outside. So, yes, that's true.

CHAIRMAN STETKAR: Okay.

MR. BIXLER: Okay. All right. This

shows some sensitivity results for phrased durations and for dose projection periods. Let's see if I can read the small print here. There are cases here with seven-day emergency phase. That's kind of our standard calculation that we did for this analysis. We looked at 15 and 30-day emergency phases, so extending those to longer durations. We look at a variation where we had a six-month intermediate phase.

We haven't really talked too much about the intermediate phase in MACCS, but intermediate phase is between the emergency phase and the long-term phase. And it's a period of time where the only activity, the only action is relocation, continued relocation of the public. The larger actions, activities begin in the long-term phase, and those include decontamination. So basically it postpones for a period of time when you begin to decontaminate.

MEMBER CORRADINI: That's a time window? So I thought there was a definition for early, intermediate and late. Is it a time window definition or a dose, an expected dose definition? That's what I don't remember.

MR. BIXLER: It's a time definition.

MEMBER CORRADINI: So a few days is early, intermediate is a few weeks, and then late is --

MR. BIXLER: In this case six months.

MEMBER CORRADINI: Okay.

MR. BIXLER: We looked at a variation with an intermediate phase of six months.

MEMBER CORRADINI: Okay.

MS. GHOSH: But our base case has no intermediate phase. It had a zero duration for the intermediate phase. So we're looking at the relative importance of a six-month delay in when you begin to decontaminate.

MEMBER CORRADINI: So --

MS. GHOSH: And --

CHAIRMAN STETKAR: Oh, go ahead. I had a different question, but I'm not sure where to ask it, so maybe I'll just try it and then Tina will me to wait or whatever.

So this was done with what dispersion model in MACCS?

MR. BIXLER: This is done with the Gaussian plume segment model that I talked about earlier and Pat showed the --

MEMBER CORRADINI: So as somewhere in the document said, you weren't looking at model uncertainties. But now you guys have instituted -- you told me this, and I forgot what it's called -- from the military side, the three-dimensional --

MR. BIXLER: Oh, Lagrangian particle tracking?

MEMBER CORRADINI: Yes, high-split, right?

MR. BIXLER: High-split, yes.

MEMBER CORRADINI: Okay. But that has not been looked at here as to how --

MR. BIXLER: No, that model was not available and ready to use --

MEMBER CORRADINI: That's fine.

MR. BIXLER: -- for this work.

MEMBER CORRADINI: Would you expect anything significantly different if I took a model that took in the third dimension and how things move around?

MR. BIXLER: I would expect some differences, yes.

MEMBER CORRADINI: Both positive and negative? Both higher and lower, I would assume?

MR. BIXLER: Yes, I think so. And I don't feel confident that I know the answer to your question, but one thing that does happen when you account for all three dimensions is you have aerosol particles that will rise up into the atmosphere, and usually the wind speeds are higher as you get above the ground farther. So those will travel faster.

And then some of them will come back down near ground level again, but they'll arrive more quickly than what you get with the Gaussian plume-type model. So it does affect the timing of when contaminants reach a location, but it also at the same time affects the amount of dispersion you get along the pathway, depending on the atmospheric conditions and so forth. So the trade-off between all those things I'm not real sure about how that would come out, but for sure one thing that does happen is the arrival time is typically earlier with Lagrangian particle tracking than it is with the Gaussian model.

MEMBER CORRADINI: Okay.

MEMBER BLEY: I have a very naïve question.

MEMBER CORRADINI: I've still --

MEMBER BLEY: Oh, go on.

MEMBER CORRADINI: -- got just one

quickie. So you said in your base case there was no intermediate phase?

MR. BIXLER: Right.

MEMBER CORRADINI: Which mean decontamination processes occurred earlier?

MR. BIXLER: Yes, that's right.

MEMBER CORRADINI: The reason I asked the question about high-split is because I was guessing that things moved faster. So you would probably want -- not want -- it would be good if you could start decontamination processes earlier rather than letting it all sit there and drag people out.

MR. BIXLER: Yes, I think that's a general -- that's a true statement regardless of which atmospheric transport model you're using.

MEMBER CORRADINI: Okay.

MR. BIXLER: But just in general --

MEMBER CORRADINI: But the quantification of that is done parametrically here?

MR. BIXLER: Yes.

MEMBER CORRADINI: That is, no intermediate/intermediate?

MR. BIXLER: Right.

MEMBER CORRADINI: Okay.

MR. BIXLER: That's right.

MEMBER CORRADINI: Thank you.

MS. GHOSH: Just a quick note. Just for the record, we are implementing this high-split as an alternative ATD module to use. And we do expect some differences, but the reason we're comfortable to comfortable to continue using the Gaussian plume segment model for the typical applications we have is that we do have a benchmarking study that had been published to compare the results of using the existing MACCS ATD module against these other modules.

MEMBER CORRADINI: Yes, I wasn't inferring that there was something way out of whack, because we had done -- I think you're aware of the student that came here that compared high-split to RASCAL to MACCS, and they were all fairly similar within assumed source terms.

MR. BIXLER: Yes, we did a benchmark comparison some years back that is documented in a NUREG/CR report. Don't recall the number of it right off the bat, but something you can look up. And in that we compared MACCS with RASCAL and another variant at the time, which I think is more like RASCAL is today called RATCHET that was also being developed at PMML. And then the LODI code from Nareack, which the Lagrangian particular tracking. So we were looking at all three types of the most standard models that would be used for APD: Gaussian plume, Gaussian puff and Lagrangian particle tracking.

MEMBER CORRADINI: Okay.

MR. BIXLER: And from that study we found that averaged over a year's worth of weather data that the three tended to agree within about a factor of two, and in some extreme cases about a factor of three. And by the way, I probably should have brought that up earlier when we were talking about the distributions that we used for dispersion. That was part of the thinking that went into coming up with the factor that we did use for that.

CHAIRMAN STETKAR: That is mentioned in the report.

MR. BIXLER: Yes.

CHAIRMAN STETKAR: There's a paragraph that -- it doesn't cite the codes, but it does -- I tend to kind of glaze over on code comparisons because in many cases they're tends to be a lot of inbreeding in terms of the assumptions and boundary conditions that are set in the codes.

(Off microphone comment.)

CHAIRMAN STETKAR: Well, that I didn't know. MR. BIXLER: Okay.

MS. GHOSH: Did Dr. Bley have a question? I think I interrupted you.

MEMBER BLEY: No.

MS. GHOSH: No? Okay.

MR. BIXLER: Okay. So this shows two families of curves, one set for 0 to 10 miles, a 10-mile radius circle, and the other one a 10 to 20-mile annular area. And for those two families of curves all the results essentially fall on top of each other with one exception, and that's the 0 to 10-mile case for the six-month intermediate phase. When we introduced that, we actually ended up with larger risks than our base case with no intermediate phase.

I've thought about that trend, that it's larger, and I can't really convince myself right now that it being larger is a general result. I think the fact that it ends up to be larger here just is circumstantial, and if we considered a different source term magnitude, it might have ended up being smaller, or larger, or maybe almost the same as before. So I don't think that's necessarily a general trend that you can conclude that if you have -- in all cases if you have a six-month intermediate phase that the risks go up. I don't think that's the case.

MEMBER BLEY: Before you go I will ask my question. I got a brain freeze here.

MR. BIXLER: Okay.

MEMBER BLEY: When we talk about risk, we often mean different things. Here the latent cancer fatality risk -- for instance, if it's 1E minus 3, it means that within whatever area we're talking about the average change of dying from cancer is one chance in a thousand. Is that right?

MS. GHOSH: Given the accident itself.

MR. BIXLER: Yes.

MEMBER BLEY: The average.

MR. BIXLER: The increased chance from the accident occurring.

MEMBER BLEY: Yes, from the accident.

MR. BIXLER: Obviously there's a background risk of cancer, but the increase over the background risk we're calculating to be one in a thousand or one in ten thousand or something like that. And the way that's calculated is it's the number of fatalities within say 10 miles divided by the population within 10 miles. So it does account for population distributions within that area. It's not just averaged around the compass uniformly.

MEMBER BLEY: Okay.

MR. BIXLER: Did you want to speak to this one, Tina?

MS. GHOSH: Yes, sure. This is just a super quick summary. When we looked at -- we mentioned earlier we did a mini-UA or a joint sensitivity analysis, whatever we want to call it, to look at the effect of having multiple steam generator tubes rupture, if you have an SGTR versus just one. And we varied some of the most important parameters contributing to SGTR to look at in addition to sampling the number of tubes that would rupture once you have a rupture in order to see what the potential effects might be. And the range there, we're spanning about 10 to the minus 5 to 10 to the minus 3 for the mean population weighted, which is the same thing as individual. That's another way to say conditional LCF risk.

And the second bullet I guess is kind of obvious. The CCDFs overlap the part of the overall CCDF in the UA where you see the SGTRs, but certainly it extends beyond that because you do get higher consequences when you rupture more than one tube. So I think that's all we were going to say there.

MR. BIXLER: So this sort of summarizes the figure that we saw earlier in the presentation.

MS. GHOSH: Okay. And so this is the summary. I think these are my concluding remarks, which none of this will be new. It's basically a summary of what we've discussed today.

As I mentioned earlier, our uncertainty analysis results here corroborate our original SOARCA study conclusions. We find that a major determinant of the source term magnitude and health consequences in terms of what we looked at, the individual latent cancer fatality risk, is whether or not a steam generator tube rupture occurs. The most influential input parameters that contribute to accident progression, cesium release magnitude -- and again, we care most about cesium with respect to LCF risk, which is the main off-site health risk we're seeing, since we don't really see early fatality risk -- is the following set of parameters:

The safety valve open area fraction, and we think in combination with a number of cycles that are experienced by the safety valves, the tube thickness for the reasons we talked about earlier, the time at CYCLE, the containment leakage rate; that's the D link parameter, the particle shape factor and the groundshine shielding factor. And we talked about all of these factors in terms of why we understand why they're important to the results.

In our study tube ruptures occurred in about 10 percent of the realizations, and that resulted in a one to two order of magnitude larger release when you do get a tube rupture. In this case there is always a thermal and pressure element to the tube ruptures.

And just I guess FYI, when we did the number of tubes joint sensitivity analysis, we did have one realization with five tubes failing that had not hot leg creep, which led to the highest release fractions that we saw in any calculation, either sensitivity or base UA that we --

(Simultaneous speaking.)

MEMBER BLEY: So I'm kind of back to where we started, and that is one's impression when you look at a slide like this is a generalization.

And we can say this for the unmitigated short-term station blackout scenario under the conditions that were assumed.

MS. GHOSH: Right. Yes, everything is predicated on the going in --

MEMBER BLEY: It's almost got to say that at the --

MS. GHOSH: -- assumptions.

MEMBER BLEY: -- top for me. Once these get loose and wander the world --

MS. GHOSH: Yes. No, I think that's a good idea, right, a reminder at the top that this is all for what we modeled.

CHAIRMAN STETKAR: It's also dangerous; and I mentioned this earlier, but since you brought it up there, the last bullet, when you're doing sensitivity analysis just to say -- or do the sensitivity analysis, and we had one realization with five tubes failing, no hot leg creep rupture and highest release fractions. Oh, my God. That was just a sensitivity analysis. What's the actual risk?

MS. GHOSH: Yes, the thing is --

CHAIRMAN STETKAR: And when you do sensitivity analyses in isolation and just simply report results without providing any other context, like what is the likelihood of that particular scenario that's not modeled, it certainly leaves you vulnerable --

MS. GHOSH: Yes, and I guess --

CHAIRMAN STETKAR: -- and open to a lot of challenges.

MS. GHOSH: Right, and I don't remember how much we summarize in that part of the report, but we talked about earlier the fact that we thought that only one tube rupturing was the most likely scenario, which is why we included it in the pot of the integrated UA. We don't think that's very likely, but we wanted to understand what the possibilities were, hence the sensitivity risk is uncertainty analysis. But we can revisit how we've written that up to see whether we should add some context to that discussion.

We talked about this, too. In most of the realizations iodine and cesium release fractions were higher earlier in the transient due to the design leakage sampling, but they were significantly lower at 48 hours. And I think -- yes, and this is for the non-SGTR.

The median release of 48 hours is lower

than the original SOARCA calculation in part due to the higher pressurization in the original SOARCA and that we had the containment rebar yield at 252 hours, which we didn't see in the new UA calculations. And the early fatality risks we compute as essentially zero. And the latent cancer fatality risks were observed to be lower than the original SOARCA calculation. We have lower source terms in the UA, so that makes sense.

And actually we were just talking about this. When we did the single realization analyses and looked at just the MACCS epistemic uncertainty, it looks like the distributions are more narrower when you only consider the MACCS uncertain parameters. In terms of an epistemic uncertainty the MELCOR uncertainty seemed to have a larger contribution to the overall consequence metric. And then Nate mentioned this already, but this is true when we're looking at a single dose response model, but this would likely change if we were then also to include uncertainties in the dose response model itself.

And the last set of bullets. Yes, this is just saying again the UA-calculated LCF risks are low, a little bit lower than they were for the original SOARCA study. And the highest risk within this case is the 0 to 10 miles, and they get lower at longer distances. And most of the risk is from the long-term exposure beyond the emergency phase. Ninety-nine percent within the 10 miles, because almost everybody's evacuating there. Eighty-four percent beyond 10 miles.

I think that was it for the summary and insights. And we were just going to wrap up with next steps.

What are we doing next? We're planning to finalize the report. We still have some updates to do. And following this meeting we have some new action items that we will put on the list, but we're hoping by the end of September to have that report finalized.

We're also trying to develop a product that might be more useful going forward for regulatory applications than the current 1,100 pages of documentation we have in the two uncertainty analysis volumes. We want to create a summary NUREG that would pull out what are the most important things that we learned from the Peach Bottom, Surry and Sequoyah uncertainty analyses?

And then we plan to contribute to

identifying key sources of model uncertainty. That's kind of language from the standards, guidance for PRA in terms of the Level 2 and Level 3 portions of the Level 3 PRA.

CHAIRMAN STETKAR: On that, we've touched on it a couple times, we've mentioned, you've steadfastly resisted quantifying uncertainty in the dose response models, among the three models. To me that's a really important source of uncertainty, maybe more important in terms of the overall published results than anything else you might be addressing in terms of -- I don't know, whether it's a MACCS parameter or MAP parameter modeling versus some MELCOR parameter modeling or some of that other internal stuff. Do you plan to --

MS. GHOSH: Oh, hold on.

CHAIRMAN STETKAR: Yes, sorry. I'm treading on hallowed ground there, I know. Is part of the next steps to address that?

MS. GHOSH: I guess the only thing I can say at this point is we've at least generated results for these alternate dose models. This is a research project. We have the liberty to do that. We feel like we've done as much as we could. At the end of the day right now NRC policy is still to use LNT. So any time we're talking about regulatory applications, those sensitivity results kind of get ignored. So I think we can have those as qualitative discussion points, but --

CHAIRMAN STETKAR: To me some of the dangers though of developing a summary NUREG report with all of these insights and stuff that we've learned -- within the context of what you've done that might make sense, but people take that out of that context.

MS. GHOSH: Yes, I would hope that we would have a chapter in that NUREG that explains the differences you get from the alternate dose response models, because we have --

CHAIRMAN STETKAR: I would certainly hope so, too.

MS. GHOSH: Yes, because we have extensive information at this point from the calculations that we have done. It's just that we kept those separate from the rest of the uncertainty analysis. We've still done a lot. It's just it's not thrown into the pot along with the other uncertainty so that we could demonstrate the relative importance of that. But we've still done a lot. I think we can certainly say a lot based on what we have done. So think it would be a separate chapter for posterity to use as they can or wish.

CHAIRMAN STETKAR: My only point is that it's my personal opinion that it's very important to address that in the same context or document or whatever it is as all of these other insights that you're talking about.

MS. GHOSH: Yes.

CHAIRMAN STETKAR: Because without that, despite the fact that you're reluctant to try to quantify it -- but without that information people may start to focus on things that are much, much less important to public health and safety than perhaps grappling with what sort of models we should be using for health effects on the public.

MS. GHOSH: Yes. Okay.

CHAIRMAN STETKAR: In deference to the person who's sitting to my left who loves to noodle with all of those other little things.

MEMBER REMPE: With respect to other little things --

(Laughter.)

MEMBER REMPE: -- okay, so the Peach Bottom was entirely analysis for SOARCA and the uncertainty was done entirely with 1.8.6. Are there any other changes that -- we saw a lot of changes with -- this is kind of an in-between case. I assume Sequoyah we just will have the version 2.1 stuff. And are there any things we're missing from the Peach Bottom because you had a new code and didn't use it --

MS. GHOSH: Yes.

MEMBER REMPE: -- that you missing some things?

MS. GHOSH: I think Nate and I are the wrong people to ask. I don't know if Hossein is here or if KC wants to comment on that.

MR. ESMAILI: Well, I don't think this is going to answer your question because we haven't done it, but 1.8.6 and 2.1 were not at some point fundamentally different. This is the same code. 1.8.6 is the same -- 2.1 is the same code as 1.8.6. Just the source for that was different.

And what we saw in the SOARCA calculation is what's in Appendix A. It's that then we converted the input deck, the 1.8.6 input deck from five years ago. And we got essentially the same type of results that we got in 1.8.6. Had we continued with 1.8.6, we would have been probably seeing the differences because we are continuously debugging the code, we are finding how to improve the code, etcetera.

So if you're asking to go and convert the Peach Bottom and run uncertainties, maybe we can get some answers, but -- get some differences in the answers, but we don't consider dose to be greatly affected by which code versions, because some of these cases that we have run the different versions of the code essentially produce the same type of results. Does that answer your question?

MEMBER REMPE: Sort of, yes.

MR. ESMAILI: Okay. But I just want -- and the thing is that the 2.1 we have added additional capabilities. You brought up this CCI modeling. We've improved the code. We have debugged the code. So in that regard things might change a little bit. Okay? But I don't think that results are going to be drastically different.

MEMBER REMPE: Okay. The other question, when you talk about the higher level insights from looking at these uncertainty analyses, I'd like to go back about my point about the CYCLE. In some cases you're going to say this is important, but in other cases you're going to say, well, we decided we learned enough, so it's not important for Sequoyah, but it was -- it showed up a lot of times in your regression analyses. And so I think you might be opening yourself up to some criticism on how to do that. But maybe you can wordsmith around it. It's up to you all.

MS. GHOSH: Yes. That's a good point. The time at CYCLE is also not something we can do much about. That's a true aleatory uncertainty, we don't know when the accident would occur. It's more just if we have an understanding of how different the results might be at a different point of the CYCLE. And I think as we've discussed, as long as we're not choosing the beginning of CYCLE to do our base analysis, we're probably okay in terms of that we haven't missed something big, because the middle of CYCLE, end of CYCLE, close enough. We shouldn't pick a point in the CYCLE that is going to look very different from what you can get from end of CYCLE or middle of CYCLE. But it's certainly worth us spending more time to think about how we explain the choices we made for what we've included and not included.

MEMBER REMPE: And the results.

MS. GHOSH: Yes.

DR. SHACK: You've mentioned several times you get some realizations that get you to the rebar yield.

MS. GHOSH: Yes.

DR. SHACK: Did you ever get anywhere until you got to the two percent global strain?

MS. GHOSH: Yes, we didn't. I think Kyle just looked at this. Yes, we did not. Yes.

I think I was on the fourth bullet. Just two other quick notes. We also have some MACCS input parameter guidance that's under development, and we anticipate that this work would contribute to the guidance that we're developing as well in terms of what uncertainties to look at, what's important.

And we also have an appendix that's under development for -- it's an appendix on severe accident consequence analysis that supports cost-benefit analyses. This is in relation to the Agency's cost-benefit analysis guidance update. And we expect that essentially the summary NUREG that we're developing and the insights that we've gotten would contribute to that work as well.

CHAIRMAN STETKAR: How does that contribute to regulatory analyses for issues that are not focused solely on a complete station blackout, a loss of all AC and DC power that is not mitigated, which is the only thing that you've ever thought about? So if I'm looking at regulatory analyses that might extend out into health effects from LOCAs, for example, this is completely irrelevant as best as I can tell because you've not looked at that.

MS. GHOSH: This is one source of information that we would draw from. I mean, our task is to survey kind of everything that's out there and be able to say something. Depending on the problem you're modeling, how can you draw from what has already been done?

CHAIRMAN STETKAR: No, this is quite obviously my concern of extrapolating too far and generalizing too far from what has been an awful lot of work done on a one specific completely square, completely black, fully defined scenario.

MS. GHOSH: Yes. And, no, I think it's a very fair point, and we have to put some hard thinking into what is applicable, when is it applicable and being able to characterize that for sure.

CHAIRMAN STETKAR: The danger being is

very few other people besides you appreciate what has been done, but more importantly appreciate what hasn't been done. And they want to pick things up and say, whoa, the Agency's SOARCA analyses show that these issues are completely unimportant, so we don't need to make any regulatory decisions about those issues --

MS. GHOSH: Right.

CHAIRMAN STETKAR: -- because they're completely unimportant to risk.

MS. GHOSH: Yes, so --

CHAIRMAN STETKAR: Health risk.

MS. GHOSH: Right. So maybe that's part of the guidance that has to be developed as to understand the applicability of -- depending on what you're trying to do. We're in the very early stages of that. At this point we're trying to capture the current state of practice, which is not represented at all in our guidance documents, which are rather old. I know that the rulemaking groups in the technical bases have come and briefed you.

CHAIRMAN STETKAR: Yes.

MS. GHOSH: A lot of that that -- what was done, it's not documented anywhere. I mean, what is documented is not even stated practice, forget trying to push the --

(Simultaneous speaking.)

CHAIRMAN STETKAR: Yes, it seems to be kind of ad hoc on a case-by-case basis in many cases.

MS. GHOSH: Yes. That's how a lot of these things progress, right? I mean, the need arises and then you create a new state of practice and kind of advance the thinking forward. We're trying to capture that now in our documents that are outdated. But it's a good question. Thank you.

MEMBER CORRADINI: So, are you waiting for our questions now are you going to go --

(Simultaneous speaking.)

CHAIRMAN STETKAR: No. No, I'm trying to get to a point there's a diminishing returns here on --

MEMBER CORRADINI: Sure. So I have questions.

MS. GHOSH: I think the slides are done. The last slide was just the references of our growing SOARCA library. And the Peach Bottom uncertainty analysis has been with publication since the end of September. And we have our fingers crossed that sometime this month it will be out. But that's just a list of references.

MEMBER CORRADINI: So I had a question. Or do you want to wait to go to the outside world first?

CHAIRMAN STETKAR: What I'm going to do is if there are no other questions directly related to the presentation material, I will go ask for public comments and then we'll go around the table and wrap up. If you have a question that's related to the presentation material, please speak up.

MEMBER CORRADINI: So if this were TRACE and you were doing an uncertainty, would you do anything differently? You said one of the biggest contributions I thought you said at the very end was it's not the results since this is one sequence for one reactor. Now you're going to do three reactors with one sequence, all unmitigated, but yet you developed a methodology. Right? That's what I thought I heard you say. So is this methodology different how -- what TRACE is doing for their uncertainty analysis, or have you even looked?

(No audible response.)

MEMBER CORRADINI: I mean, it's all one

agency. It's all one happy family. So I assume the uncertainty analysis here ought to be somehow similar to --

MS. GHOSH: I think at the very -- it depends on what level you want that question answered.

MEMBER CORRADINI: I'm not trying to be flip. I'm actually being serious in the sense that I do think you've developed a -- I mean, to put it another way, you ask for 1,200 calculations, you hit the go button, you got 904. So that's pretty good considering MELCOR is hundreds of thousands of God-knows-what, 4-TREN, maybe even 4-TREN-90, I'm not sure, and it worked. So that's pretty phenomenal.

So my question is is the process you're using, the methodology translatable to what the others are doing in uncertainty world, or are they so different that you can't learn from each other, or --

MS. GHOSH: No, no. I think it's very similar. In fact, if you -- I don't know if anybody from industry is here, but if you look at what EPRI is doing or other people, the general Monte Carlo approach to getting a distribution of results based on what you want to describe for your uncertainties, it's a pretty standard method. So I would say at a high level, yes, they're very similar approaches. I think the basic steps of what you would ideally want to do any time you want to consider uncertainty are the same.

We're working on getting more coordination amongst all the groups who are doing different things on uncertainty across the Agency. I think some of that will come out as we're developing some of the more practical products. And we have had some coordination, but we're improving in that area. But I think that a high level the answer is, yes, the basic process is the same that you'd want to follow.

MEMBER CORRADINI: I'll stop for now.

CHAIRMAN STETKAR: Any other questions related to the presentation?

(No audible response.)

CHAIRMAN STETKAR: If not, I have one more observation on the infamous valve data. If you look at table 20; this is just taking notes, and do the recovered plus non-recovered, you can get to the 17 out of 773.

MR. BIXLER: Is that for Kyle or for all

of us?

CHAIRMAN STETKAR: That's for Kyle.

MEMBER CORRADINI: I observed that before.

CHAIRMAN STETKAR: Huh?

MEMBER CORRADINI: I observed that before. Weren't you paying attention to Dr. Shack?

CHAIRMAN STETKAR: I was not.

(Laughter.)

With that, if there are no other questions, what I'd like to do is get the bridge line open. I don't know if there are any members of the public on the bridge line.

PARTICIPANT: We have one request.

CHAIRMAN STETKAR: We have? Okay. Good. So let's get the bridge line open and see if there's anyone out there.

While we're doing that, if there's anyone in the room who would like to make a comment, please come up to the microphone and do so.

I'm starting to hear pops and crackles, so there's some indication the bridge line is open.

Anyone out there, could you please do me a favor and just say hello so that we confirm that it's open?

MS. GRAY: Hello.

CHAIRMAN STETKAR: Thank you. Good. I always have to apologize for this. It's the only way we can confirm it's open.

Now, if there's anyone on the bridge line who'd like to make a comment, please identify yourself and speak.

MS. GRAY: Yes, hello. Can you hear me?

CHAIRMAN STETKAR: Yes, we can.

MS. GRAY: My name is Erica Gray and I'm calling from Richmond, Virginia. And I have, well, a couple of comments or questions.

I'd like to start with I attended an aging reactor meeting about a week or two back and it was stated that a lot of reactors no longer have capsules available. In dealing with Surry are the capsules available for the surveillance program?

CHAIRMAN STETKAR: We'll note that question. It's not relevant to the topic of today's meeting, but we'll note that question for the record.

MS. GRAY: The meeting was long. I'm not sure if you covered surveillance of dealing with the reactor core and the embrittlement issue.

CHAIRMAN STETKAR: No, that's not part of this particular research project because of the nature of the accident scenario that's being modeled here. As I said, certainly we'll note for the record, but I can at least respond that it's germane to other issues that we're certainly following as the ACRS, but not particularly today's discussions.

MS. GRAY: Okay. Well, I did want to mention, I know there was talk about the tubing, the steam generator tubing.

CHAIRMAN STETKAR: Yes.

MS. GRAY: And I agree that obviously in particular with Surry they should go on the data that's available. I was able to pull up data for Unit 1 and 2. Of course the most latest data was from Unit 1 showing that there was, I don't know, 111 tubes that have already been plugged. And Unit 2 I think I couldn't pull anything earlier -- or later than 2010, which showed 94. And it seemed like there was a lot of issues with Unit 2 having foreign material inside of them. So just to say that I think obviously going on data that's present at the reactors themselves is probably important to do.

But I also wanted to state and talk not

about modeling and dispersal of gaseous cesium or other radionuclides. Well, as a citizen, I mean, I'm very concerned because I think that there's very well-known knowledge out there that we were able to detect cesium all the way to Vermont with the Fukushima disaster. So I'm hoping that these dispersion models will really be complete and a lot more detailed.

I can't think of anything else at the moment that I'd like to mention, but I do believe there is definitely going to be issues in running these facilities for 80 years. Thank you.

CHAIRMAN STETKAR: Great. Thank you very, very much.

Are there any other members of the public on the bridge line who'd like to make a comment?

(No audible response.)

CHAIRMAN STETKAR: If not, we'll re-close the bridge line from your end so that we don't hear the pops and crackles that tend to come across. You'll still be able to hear our closing remarks.

And as we usually do at the end of a Subcommittee meeting, I'd like to go around the table and ask each person at the table for any final comments or observations and at least -- and

I'm sorry, Bill, I'm going to have to exclude from this, but the members, whether or not you think we should bring this subject to the Full Committee.

So I will start, staring at Dr. Rempe, I'll start with Dr. Ballinger.

(Laughter.)

MEMBER REMPE: I was ready to push my button.

(Laughter.)

CHAIRMAN STETKAR: Turn your mic on.

MEMBER BALLINGER: I mean, I don't have anything more to add other than the conversation that we've had all day.

As far as bringing this before the Full Committee, I think in a significantly abbreviated length, yes.

CHAIRMAN STETKAR: Dick?

MEMBER SKILLMAN: Tina, to you and your team, thank you. This has been a huge amount of work, it's obvious. Very informative. Thank you.

Whether to bring it to the whole Committee, I agree with Dr. Ballinger, in an abbreviated form. I think it's important enough to get it in front of the Full Committee, yes, sir. Thank you.

CHAIRMAN STETKAR: Okay. Dr. Corradini, sir?

MEMBER CORRADINI: Thanks to the staff and their contractors. I think they did an excellent job. I guess at this point it's kind of still a work in progress. I'm not sure we want to bring it to the Full Committee and write a letter because I'm not sure what the purpose of writing a letter would be for. So I guess I would hold off a bit until we see a little bit more results in Subcommittee format before we would do that.

I do think though -- at least my observation at this point is that there's the methodology part. That's the reason I asked the last question, which you've settled on now a methodology that if asked again you would know exactly how you want to attack the problem, or at least better attack the question. But since this is just one sequence with now the second reactor, I'm not exactly sure what I'd do with it. So I kind of would challenge the staff to say, okay, now what do you guys want to do with it from a results standpoint before it comes in front of the Full Committee and we write a letter about it, because

I'm not sure how we attack it.

CHAIRMAN STETKAR: Be careful, because it's not one sequence with a second reactor. It is a different sequence --

(Simultaneous speaking.)

MEMBER CORRADINI: I'm sorry. I'm sorry. Excuse me. Excuse me. It's a different sequence --

CHAIRMAN STETKAR: Peach Bottom was a long-term station, but ---

(Simultaneous speaking.)

MEMBER CORRADINI: I apologize.

CHAIRMAN STETKAR: -- because the timing is different.

MEMBER CORRADINI: But a single sequence with a different reactor. So I guess I'd ask the staff the question of how one would use the results at this point to go further; and I can think of some things, but my only thought is until that's discussed maybe in a Subcommittee it would be inappropriate to come to the Full Committee and write a letter on something.

CHAIRMAN STETKAR: Okay. Dennis?

MEMBER BLEY: Yes, I appreciate what a formidable task you took on to do this, and I'm very pleased that you've done this formidable task. We were critical of SOARCA early on for not doing this kind of work and I think it's important. I think we learned quite a bit from this effort and I hope that it will support the Level 3 work. I don't have any additional things to add beyond the comments I made during the session, but congratulations on a lot of very good work here.

I would at least I think weigh in at least for the summary report on the insights from the three analyses. That would be a better place for us to go to the Full Committee.

CHAIRMAN STETKAR: Okay. Bill, I'll skip you for a moment.

And, Joy?

MEMBER REMPE: I agree with my colleagues that we should thank you for your efforts. Yes, we picked here and there on the report things that needed to be changed, but in general I thought the quality of the report was very good for a draft report. And so, I think you did a good job in that way. And I also thought the way you responded to the questions today indicates some depth of knowledge that's nice to see. And so I appreciate that.

With respect to bringing it to the Full Committee, I wholeheartedly agree with Mike and Dennis that we shouldn't write a letter yet, but I'm also thinking of how long it's been since we had anything on SOARCA to the Full Committee. Even for the Subcommittee it's been a long time. And so, it might be worthwhile, just an update, because the Committee is changing faces. And so by the time you bring it to the Full Committee to write a letter, there are going to be people who don't know what SOARCA is. And so, I think it might be good to -- and those of us who were here for writing the letter may have forgotten what it is. So I don't think it would hurt to have an information meeting. That's again something that we can discuss more.

MEMBER BALLINGER: Again, I'm not advocating writing the letter, just exactly what we did.

MEMBER SKILLMAN: Yes, that's what I said, too, I think.

MEMBER BALLINGER: I'm not advocating writing a letter. I'm just voting --

(Simultaneous speaking.)

CHAIRMAN STETKAR: First of all, the Subcommittee, nobody can decide whether the

Committee's going to write a letter. We can only recommend whether or not it should come to the Full Committee. The Committee then decides whether or not a letter is warranted. So discussing whether or not the ACRS issues a letter on it in this forum is irrelevant.

MEMBER BALLINGER: The two people on your left and right did suggest not writing the letter.

CHAIRMAN STETKAR: It doesn't make any difference. They're only two members of the Committee.

MEMBER BALLINGER: He ignores us all the time.

(Laughter.)

CHAIRMAN STETKAR: Okay. Joy, anything else?

MEMBER REMPE: No.

CHAIRMAN STETKAR: Bill, last but not least?

DR. SHACK: One of the things you can carry over to other uncertainty analyses I think is the story board concept, which I think was a great improvement over what we did with Peach Bottom. If nothing else it presents everything up in a way that I can suddenly look at it and criticize it, disagree with it.

(Laughter.)

DR. SHACK: So I think you've made a substantial contribution there in a way to document an uncertainty analysis, as well as the other things you've done with sort of the mechanics of how to do an uncertainty analysis. So that's very good. As I say, my technical point I've made before. Congratulations on an enormous piece of work.

CHAIRMAN STETKAR: And I'll close. Yes, congratulations. The documentation here, the technical justification, as I said, I'll echo Bill, I might not necessarily agree with all of the distributions, but I understand for the most part now the background and the rationale behind them. I understand the basic process. And I think this is, from that perspective, a tremendous improvement over the Peach Bottom report. And I think you deserve congratulations for that. It's a tremendous improvement.

Regarding going to the Full Committee, I'm personally kind of torn. I think that an information briefing would be very useful. I think that perhaps such an information briefing would be even more useful. We have new members coming on the ACRS, well, in an indeterminate time. It will probably be three or four or five months from now.

MEMBER BLEY: Maybe in time for that summary review.

(Laughter.)

CHAIRMAN STETKAR: Well, depends on what you're timing is for -- you mentioned this spring for Sequoyah.

MS. SANTIAGO: Right.

CHAIRMAN STETKAR: The Subcommittee.

MS. SANTIAGO: We have two things coming up, which would be the Sequoyah --

CHAIRMAN STETKAR: Well, you --

MS. SANTIAGO: Oh, I'm sorry.

CHAIRMAN STETKAR: Yes, actually that's --

(Simultaneous speaking.)

MS. SANTIAGO: Is it?

CHAIRMAN STETKAR: Yes.

MS. SANTIAGO: Okay. So we do want to come and brief you on the Sequoyah analysis.

CHAIRMAN STETKAR: And the timing of that would be?

MS. SANTIAGO: It's probably April or

May.

CHAIRMAN STETKAR: April or May? Okay.

MS. SANTIAGO: And that depends on --

(Simultaneous speaking.)

CHAIRMAN STETKAR: I'm assuming -- yes. But I won't hold you, but I'm trying to get a sense of --

MS. SANTIAGO: Well, and we want to go to the public, because I think in the past the ACRS Subcommittee recommended to us to go to the public meeting and then come and inform you about any comments that we got from --

CHAIRMAN STETKAR: Right. Right.

MS. SANTIAGO: -- members of the public.

CHAIRMAN STETKAR: Right.

MS. SANTIAGO: So that's why I'm saying April or May.

CHAIRMAN STETKAR: And I'm assuming that the compilation of insights from the three studies would be delayed. We're probably looking at the end of the calendar year for that.

MS. SANTIAGO: Right. We're looking to try and get the draft reports to the Commission in September.

CHAIRMAN STETKAR: The three draft --

MS. SANTIAGO: Well, the summary report was not --

MS. GHOSH: Would be later, yes. Right.

MS. SANTIAGO: -- would be later than that.

CHAIRMAN STETKAR: Yes.

MS. SANTIAGO: Just so that we have some time to really think and --

MS. GHOSH: We're still finishing Sequoyah.

CHAIRMAN STETKAR: Well --

MS. GHOSH: There's only so much we can do at the same time.

CHAIRMAN STETKAR: Okay. We'll have to discuss it.

MS. SANTIAGO: So we were expecting eventually to come brief the Subcommittee on Sequoyah.

CHAIRMAN STETKAR: Yes.

MS. SANTIAGO: And then we thought perhaps we'd go to the Full Committee with both analyses for the Sequoyah and this Surry uncertainty analysis.

CHAIRMAN STETKAR: Well, they probably have to be all three of them. I think that the

Committee -- this is my own personal opinion, that the Committee ought to have in front of it the three uncertainty analyses, recognizing that it's an evolutionary process --

MS. SANTIAGO: Right.

CHAIRMAN STETKAR: -- but not focusing only on the two pressurized water reactors.

MEMBER REMPE: You mentioned you were going to have something, public comment on Sequoyah. I hope you come let us see it before it goes for public comment like you have done on this document. Is that a true statement? I mean, you mentioned you hoping to have it out for public comment.

MS. SANTIAGO: Yes, in the past; and I'll go back and re-look at our process, we actually went and had a public meeting and had the licensee do a fact check on the inputs and --

MEMBER REMPE: Has this --

CHAIRMAN STETKAR: That's a good idea.

(Simultaneous speaking.)

MEMBER REMPE: -- for public comment already before we saw it today?

MS. SANTIAGO: For the Surry uncertainty analysis, no.

CHAIRMAN STETKAR: But the Surry SOARCA has.

MS. SANTIAGO: Correct.

MEMBER REMPE: Right. Oh, okay. I thought you meant the --

MS. SANTIAGO: For Sequoyah --

MEMBER REMPE: The Sequoyah --

MS. SANTIAGO: -- we're doing it in --

MEMBER REMPE: -- uncertainty analysis or the --

MS. SANTIAGO: -- concert, in parallel.

MEMBER REMPE: Okay.

CHAIRMAN STETKAR: It's only one for --

MS. SANTIAGO: Right.

CHAIRMAN STETKAR: Effectively one for Sequoyah.

MS. SANTIAGO: Because the Committee recommends --

MS. GHOSH: That was your advice, do it all at the same time.

CHAIRMAN STETKAR: Is it going to be one report?

MS. GHOSH: Yes.

MS. SANTIAGO: Yes.

CHAIRMAN STETKAR: Okay. Even better.

So you don't have the --

MS. SANTIAGO: Yes. Okay.

CHAIRMAN STETKAR: -- SOARCA thing and the UA thing.

MS. SANTIAGO: So if you want us to come --

CHAIRMAN STETKAR: No.

MS. SANTIAGO: Okay.

MS. GHOSH: Okay.

CHAIRMAN STETKAR: I think that we follow what we've been doing. I think that we certainly want to have any public input and certainly the licensee's input for fact checking some reasonable mature document at the time before it comes to us at the Subcommittee. Even at the Subcommittee level, because otherwise you get into this kind of endless -- not endless loop, but nested loop where we ask questions. You say, well, we haven't checked that. We have to go back to TVA and ask the people at Sequoyah. And they come back. And it isn't productive.

MEMBER BLEY: But you're looking to send it to the Commission in September?

CHAIRMAN STETKAR: In September. I think the ACRS would like to be able to weigh in in that time frame. And that would be useful in our guesstimates in terms of when our new members will be coming on board, because they probably can't spell SOARCA yet. So it would be good getting them up to speed also. So we're probably looking at a -- we don't have Full Committee meetings in August, so we might be targeting the July time frame for maybe a Full Committee briefing, with or without a letter. I mean, we can do that with the Sequoyah Subcommittee meeting earlier than that, in April or something.

MS. SANTIAGO: But I do want to say we deeply appreciate all the comments that we get from the Committee members, because on this particular analysis it's fairly complex, intricate and technical and we spent the last 18 months debating amongst a lot of experts in our field as well. And so we do appreciate the comments that you've made. And also that we don't want people to take what we've done out of context. And so, we still have to think about some of these things.

So I do want to also thank the team. And as you say this team has been together pretty much for five years since we started the original SOARCA analyses, and it's made it good in a lot of ways.

And we're trying to grow other staff so that they have the background and knowledge that we've developed over the course of five years.

Unfortunately, we may have some new folks and lose some folks that have been with us for that five years, so we -- in fact, I asked Tina yesterday do we have to give another little summary brief on SOARCA, because we hadn't been here for two years. And I forget things after a while, so I think it's a great time to really introduce new members if you're going to have new members --

CHAIRMAN STETKAR: Yes.

MS. SANTIAGO: -- and give them that background and that continuity that you have. So thank you again for all your support.

CHAIRMAN STETKAR: And with that, unless anyone has anything else, we are adjourned.

(Whereupon, the above-entitled matter went off the record at 5:00 p.m.)

U.S.NRC
UNITED STATES NUCLEAR REGULATORY COMMISSION
Protecting People and the Environment

State-of-the-Art Reactor Consequence Analyses (SOARCA) Surry Uncertainty Analysis (UA)

ACRS Subcommittee Briefing
February 2, 2016

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NRC Office of Nuclear Regulatory Research, Accident Analysis Branch

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Sandia National Laboratories

Core Team Members and Advisors

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- UA methodology: Cedric Sallaberry, Dusty Brooks, Aubrey Eckert-Gallup, Jon Helton, Matthew Denman (SNL); Tina Ghosh, Trey Hathaway (NRC)

Outline

- Objectives
- Overview and overall conclusions
- MELCOR model enhancements
- Parameter development
- MELCOR parameters
- MACCS parameters
- MELCOR analysis results
- MACCS consequence analysis results
- Summary and insights

Objectives of the Uncertainty Analysis

- Develop insight into overall sensitivity of results and conclusions to uncertainty in model inputs.
- Identify the most influential input parameters contributing to variations in accident progression, source term, and offsite consequence results.
- “Complement and support” the NRC’s Site Level 3 PRA project and post-Fukushima activities including Tier 3 items. (Staff Requirements Memorandum SECY-12-0092)

Overview

- Analysis of uncertainty in the Surry SOARCA unmitigated short term station blackout (STSBO)
- Focus on epistemic (state-of-knowledge) uncertainty in input parameter values, and limited aleatory uncertainty
  - Aleatory (random) uncertainty due to weather handled in the same way as the SOARCA study
  - Time-at-cycle (burn-up) and stochastic nature of safety valve failure investigated (aleatory aspects of some input parameters)
- The Surry MELCOR model was updated
  - It had been 5 years since SOARCA base case was developed
  - MELCOR 2.1 had been released (1.86 was used for the SOARCA original analysis)
  - Updated “base case” documented in report

Overview (continued)

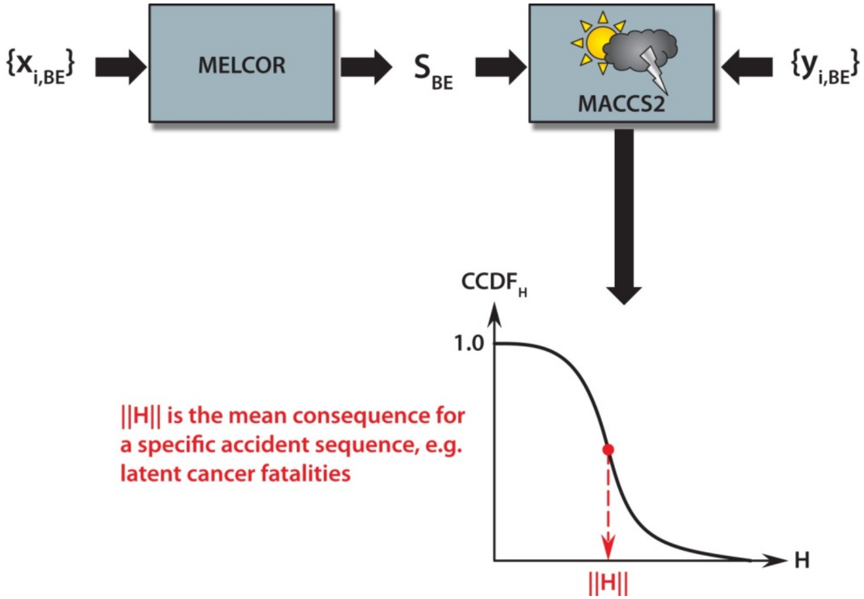
- Investigated uncertainty in MELCOR and MACCS inputs
- Key uncertain input parameters were identified
- Uncertainty in these parameters was propagated in a two-step Monte Carlo simulation:
  - A set of source terms generated using MELCOR model
  - A distribution of consequence results generated using MACCS model
- 1003 successful MELCOR Monte Carlo “realizations” completed to 48 hours were each coupled with a successful MACCS realization
  - Of 1200 originally run, incomplete MELCOR realizations were attributable to numerical simulation issues

Overview (continued)

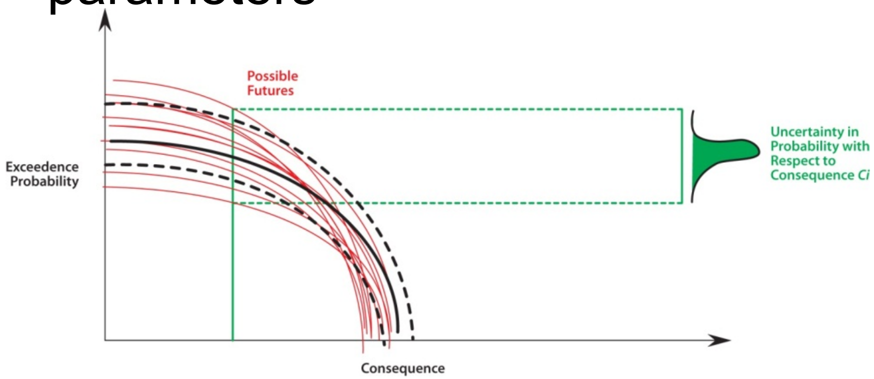
- Results reported with regard to figures of merit investigated:
  - MELCOR: Cesium and Iodine release to the environment by 48 hours, in-vessel hydrogen production, and timing of initial fission product release to the environment
  - MACCS: Individual early and latent cancer fatality (LCF) risk
- Results analyzed with statistical regression based methods, scatter plots, and phenomenological investigation of selected individual realizations
  - An individual realization is a single run (or "realization") selected from the set generated in the Monte Carlo simulation

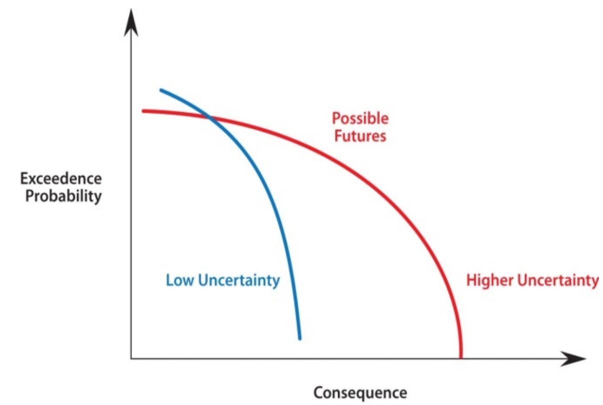
Uncertainty Analysis

Base calculations include weather variability in the consequences



Probabilistic UA includes both the weather variability and the uncertainty in the epistemic input parameters





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Multiple regression techniques were implemented

- Regression techniques included:
  - Rank regression,
  - Quadratic regression,
  - Recursive partitioning, and
  - Multivariate adaptive regression splines (MARS)
- Use of multiple approaches to post-process and analyze Monte Carlo results provided better explanatory power with regard to which input parameters are most important to uncertainty in results
  - Demonstrated in the Peach Bottom UA

Overall Conclusions

- Surry UA corroborates SOARCA study conclusions
  - Public health consequences from severe nuclear accident scenarios that were modeled are smaller than previously calculated, and very small in absolute terms
  - Delayed releases calculated provide time for emergency response actions such as evacuating or sheltering
  - Long-term phase dominates health effect risks because emergency response is faster than progression to release
  - “Essentially zero” early fatality risk projected

Overall Conclusions (2)

- A major determinant of source term magnitude and health consequences is whether or not a steam generator tube rupture (SGTR) occurs
- Mean, individual, LCF risks assuming a linear-no-threshold (LNT) dose response, conditional on the occurrence of an accident, estimated in this UA of the Surry unmitigated STSBO[1] are very low, lower than the risk evaluated in the original SOARCA study, which was 9×10^{-5} within 10 miles, and lower at longer distances.

[1] Frequency STSBO – 1×10^{-6} to 2×10^{-6} pry; STSBO with SGTR – 1×10^{-7} to 8×10^{-7} pry (NUREG/CR-7110 Vol 2, pg. 2-3).

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MELCOR Model Enhancements

MELCOR Model Enhancements

- MELCOR 1.86 was used for the original Surry SOARCA study.
- MELCOR 2.1 became available in 2010
  - MELCOR code enhancements continued.
  - Version 1.86 is no longer maintained.
- Given the evolution of the code, it was determined reasonable to correct known errors and implement additional model enhancements to improve the analysis.

Surry MELCOR Model Enhancements

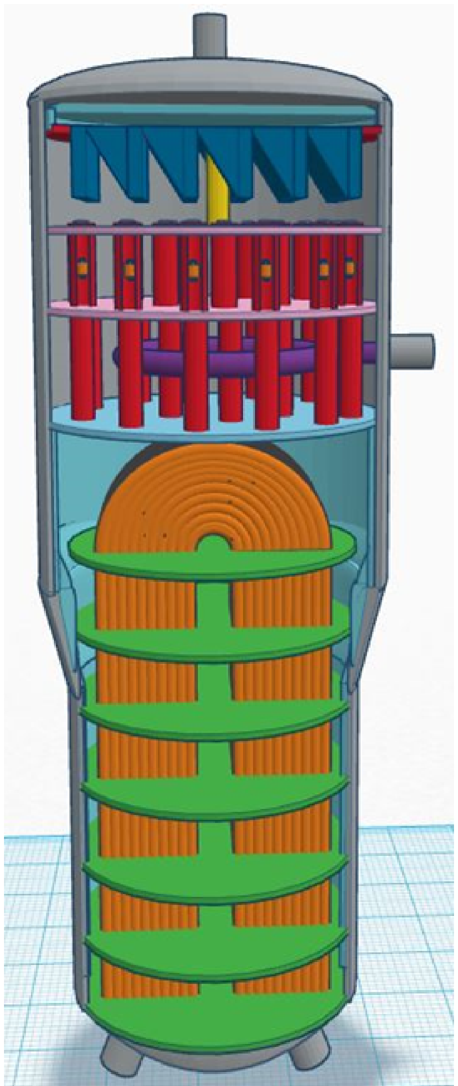
- A few Surry model enhancements are described below:
  - Enabled optional molten core concrete interaction (MCCI) modeling input to take advantage of recent code enhancements/corrections that add realism;
  - Increased the steam generator nodalization (see next slide);
  - Included hot tube modeling in the SGTR logic;
  - Redefined the ignition criteria for H2/CO deflagrations;
  - Extended hot leg nozzle creep rupture modeling to consider the stainless steel cladding.

Surry MELCOR Model Corrections

- A few Surry model corrections are described below.
- Corrected errant vapor pressure coefficients for control rod materials silver, indium, and cadmium.
- Corrected the containment concrete.
  - The Surry SOARCA analysis used a limestone aggregate. Research in the UA identified the aggregate to be basaltic.
- Main steamline drains were found not isolating in Surry SOARCA and were fixed.

- Developed a current Surry core inventory to facilitate time-at-cycle sampling: the Surry SOARCA analysis had implemented a high burnup core inventory.

Example: Increased steam generator tube nodalization



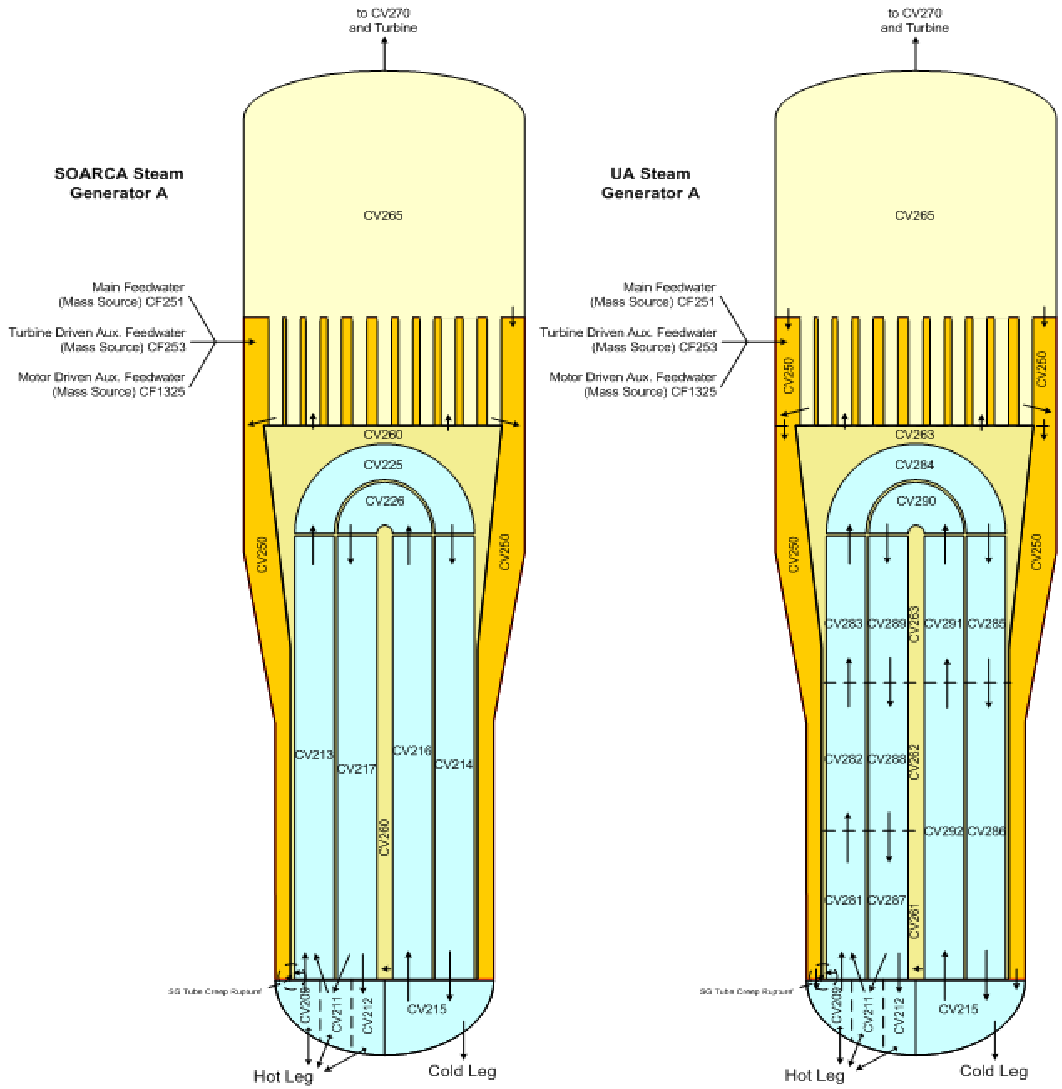


Figure 3-3 Comparison of NUREG/CR 7110 Vol 2 and UA steam generator nodalizations

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Parameter Development

Implemented a Process for Choosing Parameters and Establishing Distributions

- Involved staff from SNL and NRC with expertise in MELCOR and MACCS modeling for SOARCA
- Subject matter experts (SMEs) provided support in reviews of data and parameters
- Reviewed parameters used in Peach Bottom UA
- Performed a systematic review of phenomenological areas (sequence, in-vessel and ex-vessel accident progression, containment behavior, chemical form and aerosol deposition)
- Reviewed the phenomenological topics covered in the MELCOR Reference Manual
- Reviewed a comprehensive MACCS parameter list

Process (continued)

- An initial list of candidate parameters was then developed.
- Implemented a ‘storyboard’ process
  – Required analysts to document justification and rationale for each parameter
  – Iterative and involved joint NRC reviews
- Focused on:
  – confirming the parameter representations appropriately reflect key sources of uncertainty, and
  – ensuring model parameter representations (i.e., probability distributions) are reasonable and have a defensible technical basis.

Process (continued)

- During the course of the project (typically storyboard reviews), some parameters were omitted from further consideration and others were added for the analysis.
- Some parameters were exploratory
  – Little basis for the uncertainty distribution, but analysts had an interest in gaining some insights
- MELCOR and MACCS parameters that were considered but not included are listed in the report.

Diagram of the code information flow

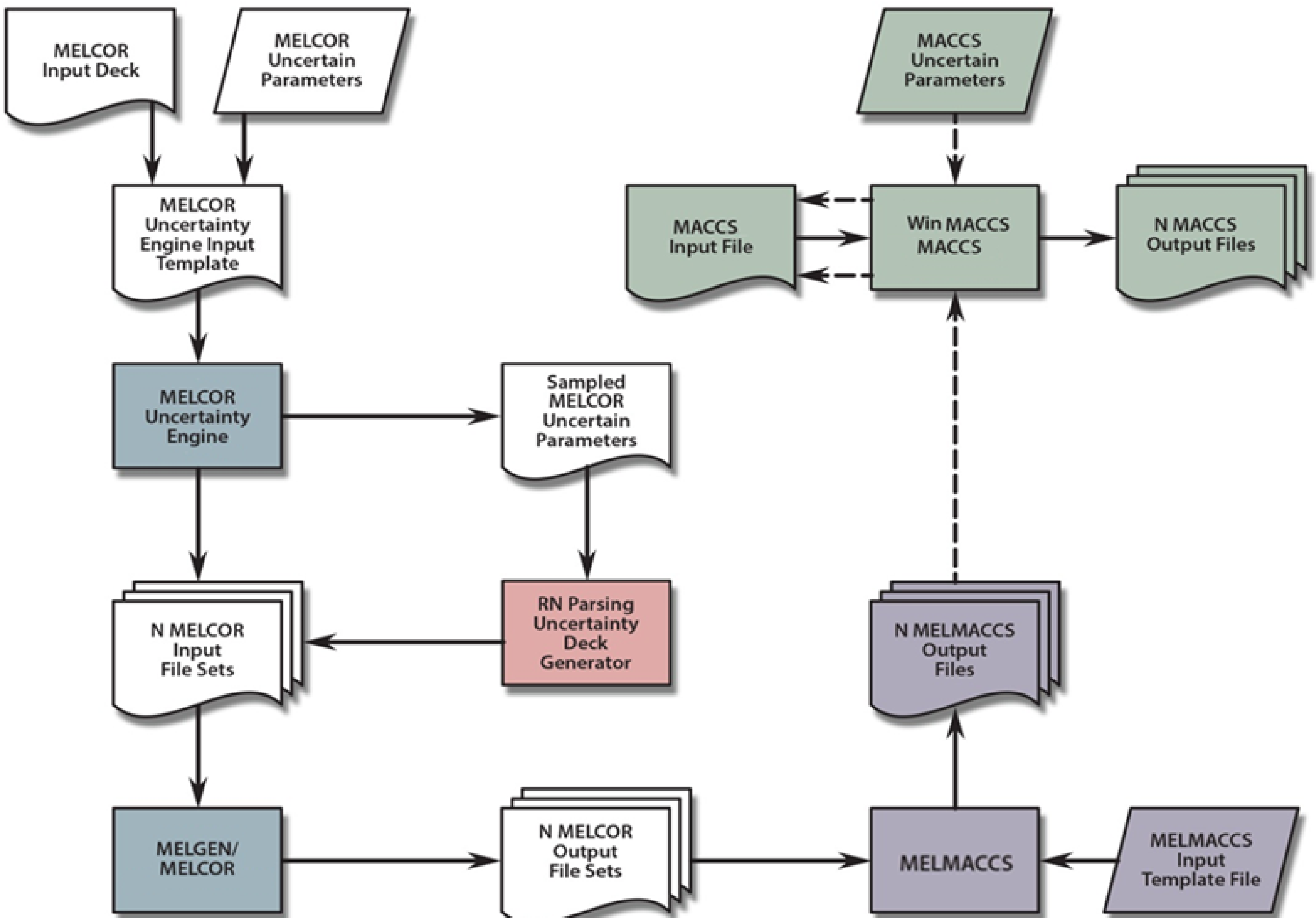
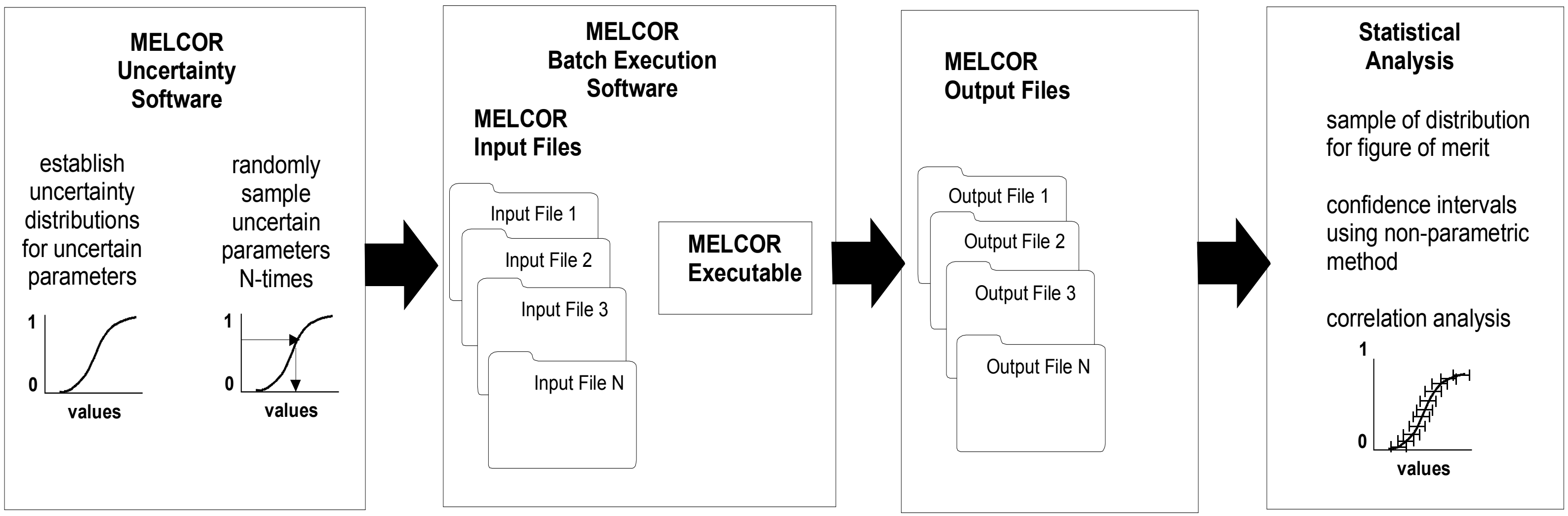


Figure 3-1 Diagram of code information flow

MELCOR uncertainty application



- Define uncertain parameter distributions
- MELCOR Uncertainty software samples distributions
- MELCOR Uncertainty software incorporates "n" sets of sampled parameters into "n" MELCOR input files

- Batch Execution software runs "n" MELCOR input files on server system

- MELCOR output files are manually retrieved from server system

- MELCOR output files are post processed

Figure 3-2 Melcor uncertainty application

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MELCOR Parameters

MELCOR Uncertain Parameters

Sequence

- Primary SV stochastic FTC
- Primary SV stochastic FTO
- Primary SV FTC due to passing water
- Secondary SV stochastic FTC
- SV open area fraction
- Primary SV FTC due to overheating
- Reactor coolant pump seal leakage (RCPSL)
- Normalized temperature of hottest SG tube
- SG tube thickness (mm)

In-Vessel Accident Progression

- Zircaloy melt breakout temperature**
- Molten clad drainage rate**
- Radial molten debris relocation time constant (RDMTC)
- Radial solid debris relocation time constant (RDSTC)
- Time in the fuel cycle of the accident (BOC, MOC, or EOC)
- Decay Heat (DEV_DECAY_HEAT)
- Melting temperature of the eutectic formed between UO_2 and ZrO_2

** indicates parameter was uncertain in the Peach Bottom UA

MELCOR Uncertain Parameters (continued)

Ex-vessel Accident Progression

- Hydrogen ignition criteria (H_2 LFL)
- SGTR location (for decontamination factor per ARTIST)

Containment Behavior

- Containment design leakage rate (DLEAK)
- Containment fragility curve (CFC)
- Containment convection heat transfer coefficient

Chemical Forms of Iodine and Cesium

- CHEMFORM iodine**
- CHEMFORM cesium**

Aerosol Transport and Deposition

- Dynamic Shape Factor (PARTSHAPE)

** indicates parameter was uncertain in the Peach Bottom UA

Safety Valves

- In most cases, a failure to close (FTC) occurs on the lowest set-point safety valve (SV) and the system transitions from state 1 to state 4.
- If failure to open (FTO) occurs, or a FTC with a sufficiently small open area, the system transitions from cycling on the lowest set point (state 1) to cycling on the middle, then highest set point SVs (states 2 and 3).
- Should all 3 valves FTO, State 5 (no relief) develops.

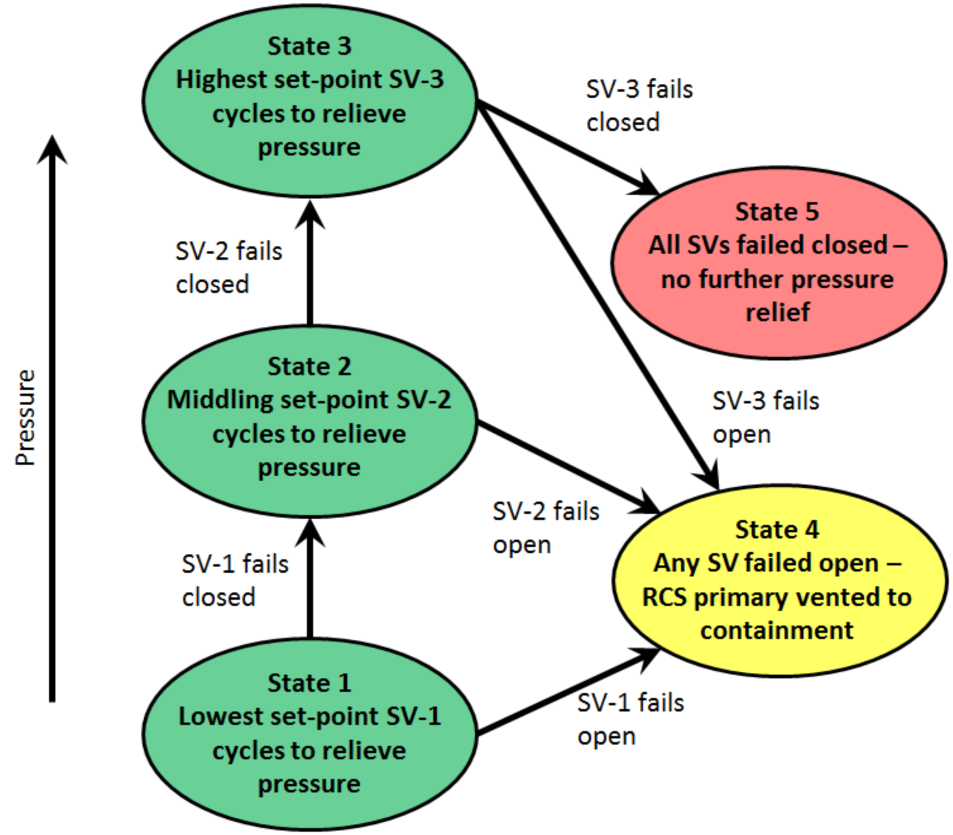

Figure 4-1 Possible transitions in the 3-SV pressurizer pressure relief system considering both FTO and FTC valve conditions

Safety Valves

- Truncated at 1,000 cycles based on professional judgment that an SV would likely not cycle more than a few hundred times prior to failure
- Note the very low possibility that all 3 valves would fail to open was identified, but was never sampled in the 1003 realizations

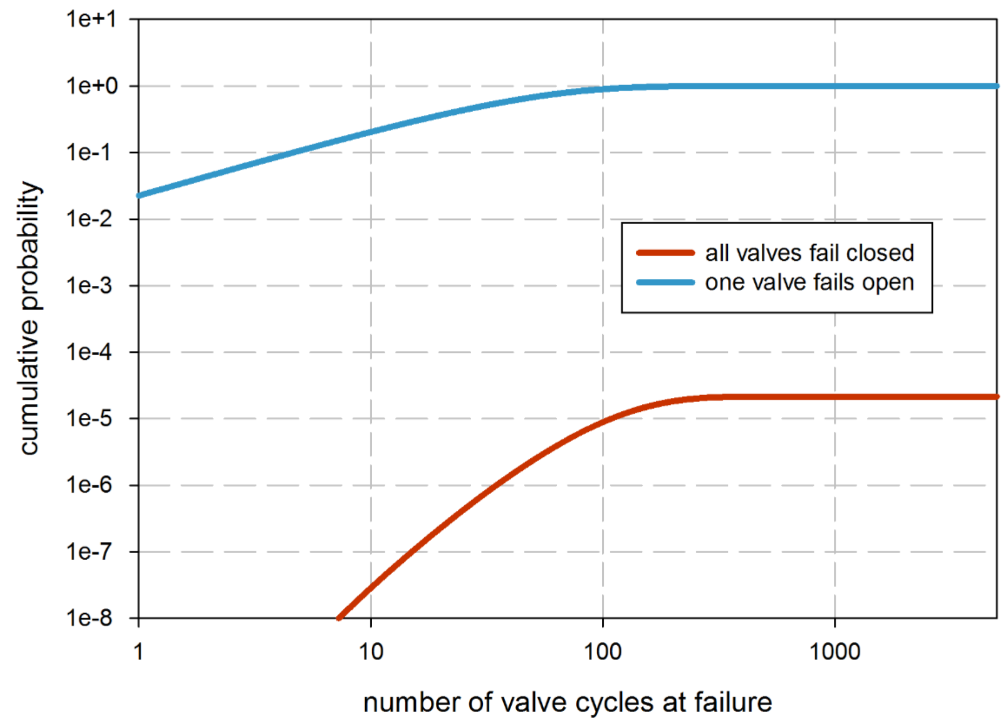

Figure 4-2 Cumulative distribution function (CDF) for the number of cycles ~~at~~ after which any single SV will have failed open compared to the CDF for the number of cycles after which all 3 valves failed closed

Safety Valves

Uncertain Parameter	Distribution type	Distribution Parameters	Lower Bound	Upper Bound
Primary SV stochastic FTC (-)	Beta[1]	α : 17.5 β : 756.5	0	1
Primary SV stochastic FTO (-)	Beta[1]	α : 0.5 β : 773.5	0	1
Primary SV FTC due to passing water (-)	Beta[1]	α : 0.5 β : 4.5	0	1
Secondary SV stochastic FTC (-)	Beta[1]	α : 17.5 β : 756.5	0	1
SV open area fraction (-)	Uniform	-	0.01	1
Primary SV thermal failure to close (K)	Beta	α : 5 β : 10.7615	811	1422

- From Table 4-1 MELCOR sampled parameters.

Decay Heat (Cycle)

- CYCLE represents the time at cycle and was varied between beginning, middle, and end of cycle (BOC, MOC, and EOC).
- Cycle determines the radionuclide inventory which is the source of decay heat
  - Baseline decay heat curves were developed for each time at cycle.
- For each realization, variation from the base decay heat curve was sampled.
- Cycle directly affects the MELCOR source term calculation through decay heat and directly affects the MACCS consequence analysis through fission product inventory
  - CYCLE is the only parameter that has this dual status.

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Decay Heat

- The baseline BOC, MOC, and EOC decay power curves are shown with respect to the time considered for the STSBO scenario.
- The times of shutdown in cycle 20 were chosen to be 7 days for BOC, 200 days for MOC, and 505 days for EOC.

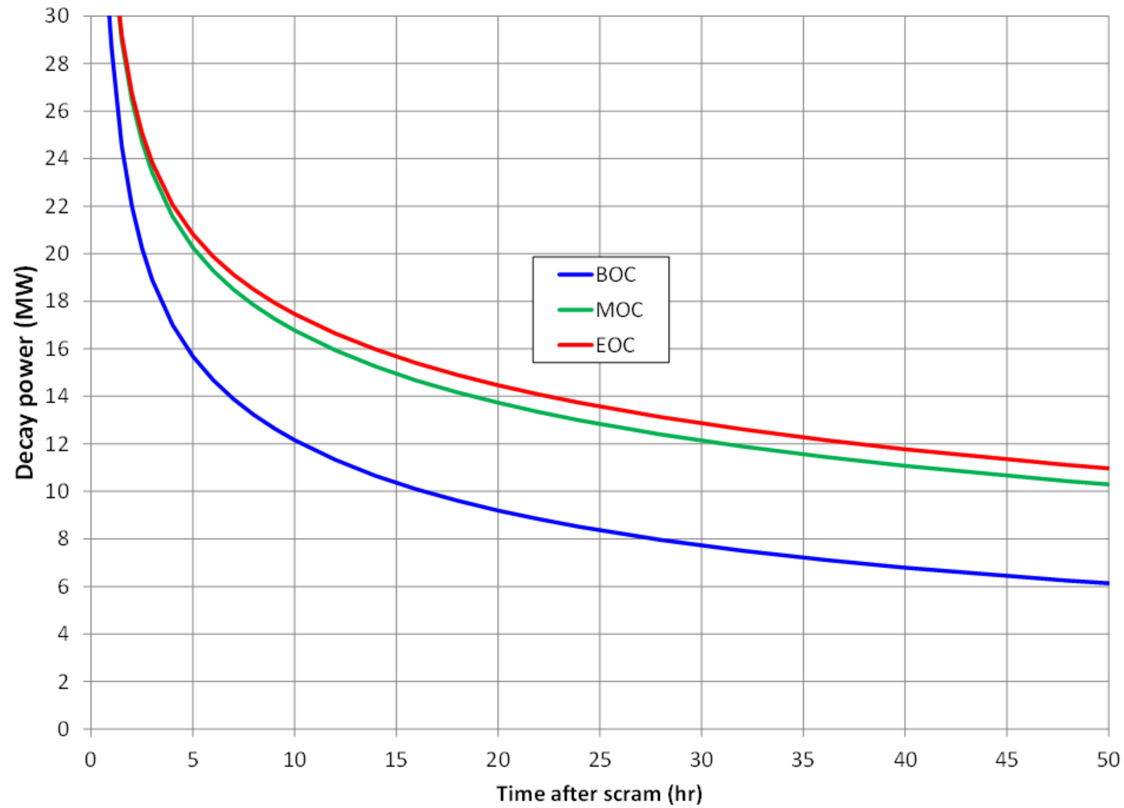
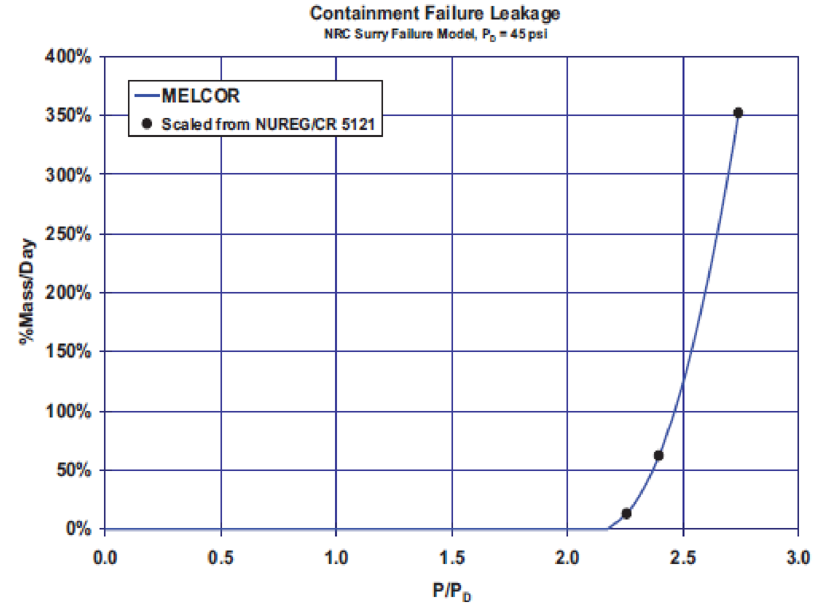


Figure 4-21 BOC, MOC, and EOC decay power curves

Containment Fragility Curve

- The containment failure curve was modified -- original Surry SOARCA (top) and the UA curve (bottom).
- Data points shown reflect leak rate data collected from 1:6 scale experiments
  - Developed with a simple NRC method to reconcile scale and idealized nature of experiment.
- The 3 calculated pressures were all reduced 15 percent in SOARCA for conservatism because of the above approach.
- Both curves use 3 of the same points, based on scaled containment testing at Sandia. However, the UA includes the liner yield point, which results in the enhanced failure leakage not initiating until much higher containment pressures.
- The 4th data point, liner yield, shown at 1% volume / day was adopted directly (pressure and leak rate) from experimental data, also with the pressure reduced 15 percent.



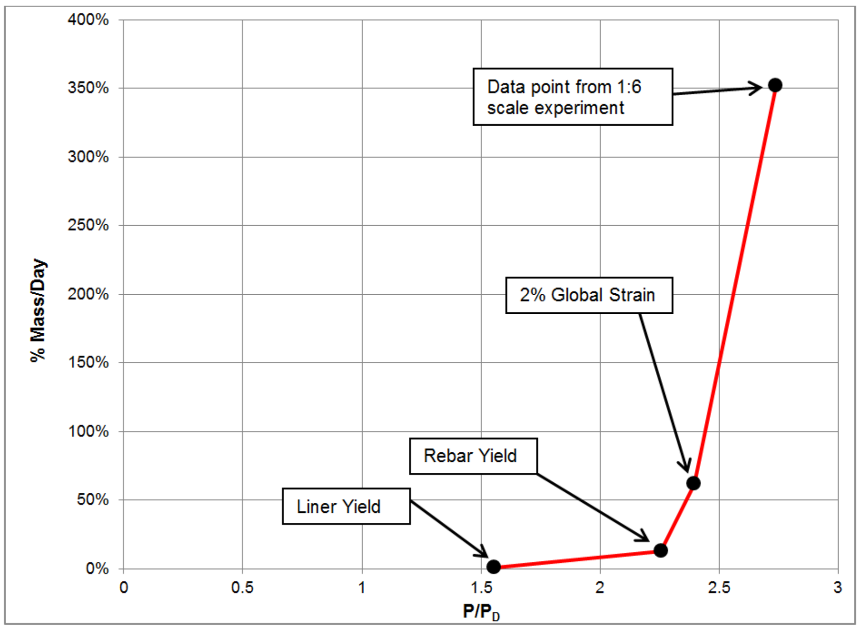


Figure 4-34 Containment functional failure leakage

Containment Fragility Curve

- The Surry SOARCA study neglected the liner yield point; thus overpressure leakage before rebar yield was assumed to be subsumed by design leakage, and early enhanced leakage was assumed insignificant for calculation of integral releases.
- The effect, combined with other sampled parameters such as time at cycle and nominal leakage, is that in the UA realizations containment pressure remains lower for a longer time period.

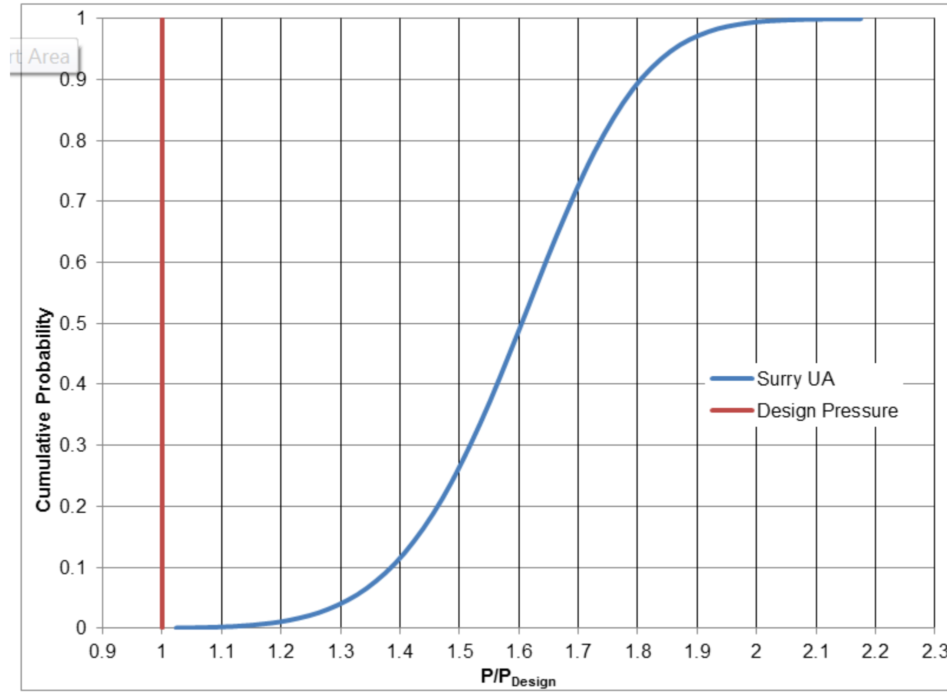


Figure 4-35 CDF for containment overpressure ratio for liner yield

Hydrogen Ignition Criteria

- Hydrogen ignition criteria accounts for the ignition location for a hydrogen deflagration and the corresponding flammability limit (volume percent of hydrogen) to represent uncertainty in the direction of propagation from the ignition source for upward, horizontal, and downward propagation.

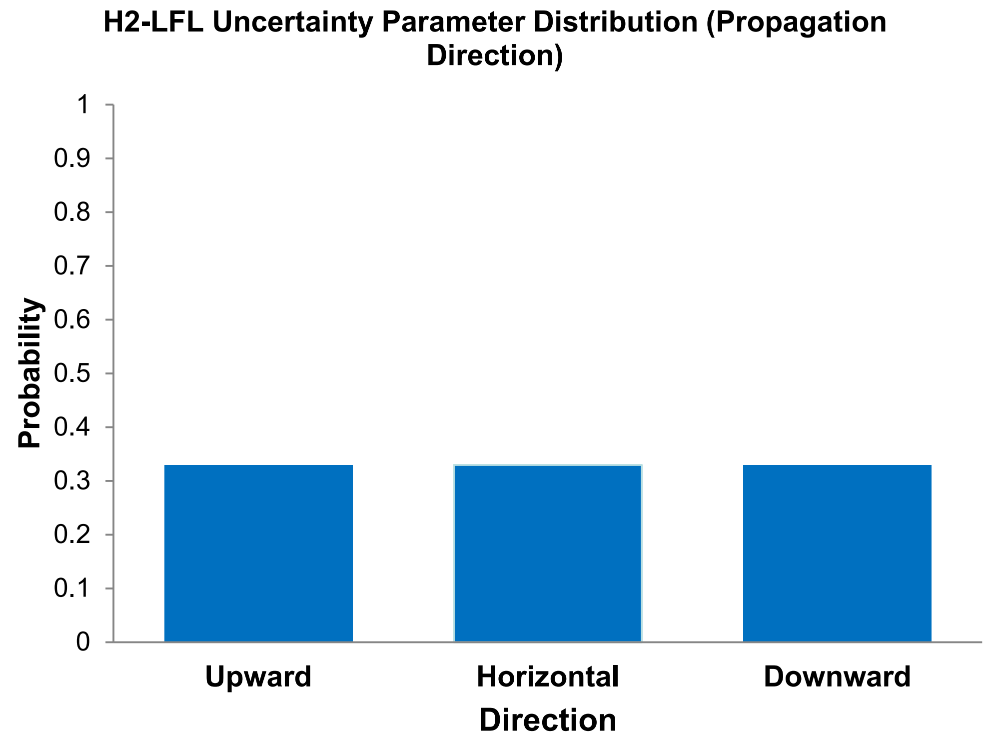


Figure 4-28 Uncertainty distribution for ignition propagation direction

Hottest Steam Generator Tube

- Normalized SG tube temperature is represented by this parameter, which along with the MELCOR-calculated hot leg and cold tube temperatures is used to determine the time-dependent hottest tube temperature applied to the single tube model.

$$T_n = \frac{T_{ht} - T_{ct}}{T_h - T_{ct}}$$

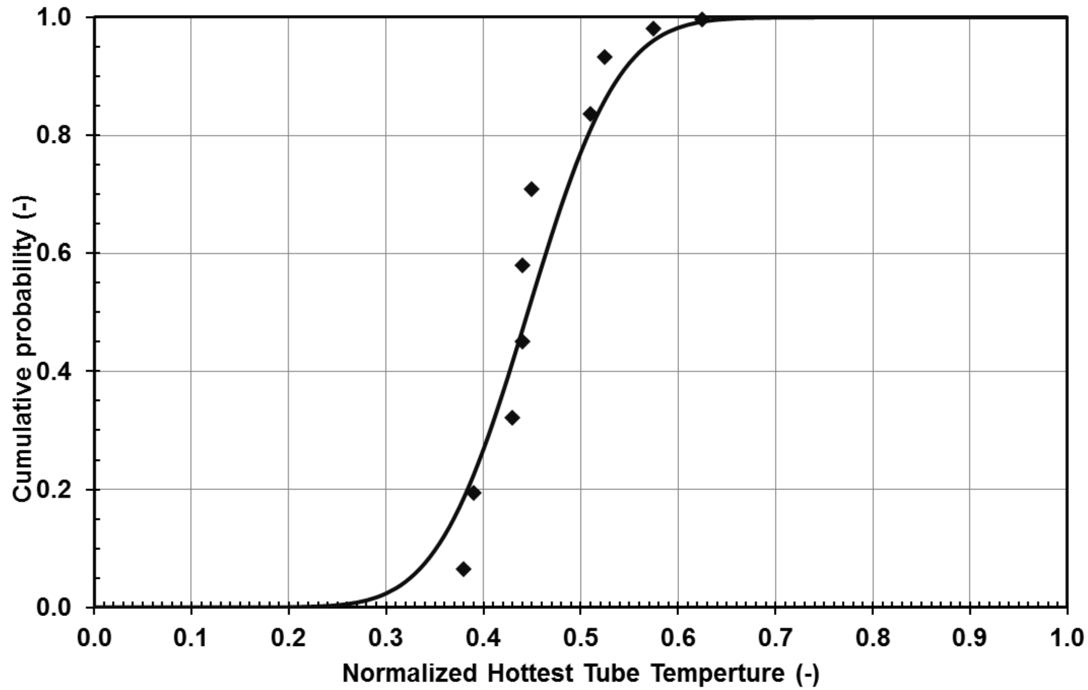


Figure 4-12 CDF of normalized hottest tube temperature

MELCOR Modeling

- Updated MELCOR base case.
- 1003 successful realizations in Monte Carlo simulation.
- Regression analyses performed for figures of merit identified in table below.

Output Parameter Data set used based on occurrence of SGTR												
Cycle	Cs_frac			I_frac			H2_Prod			Release Timing		
	SGTR	Non-SGTR	Both	SGTR	Non-SGTR	Both	SGTR	Non-SGTR	Both	SGTR	Non-SGTR	Both
All	✓	✓	✓	✓	✓	✓	✓	✓	✓		✓	✓
BOC		✓	✓		✓	✓		✓	✓			
MOC		✓	✓		✓	✓		✓	✓			
EOC		✓	✓		✓	✓		✓	✓			

Table 6-1 MELCOR regression analyses completed

MACCS Consequence Analysis

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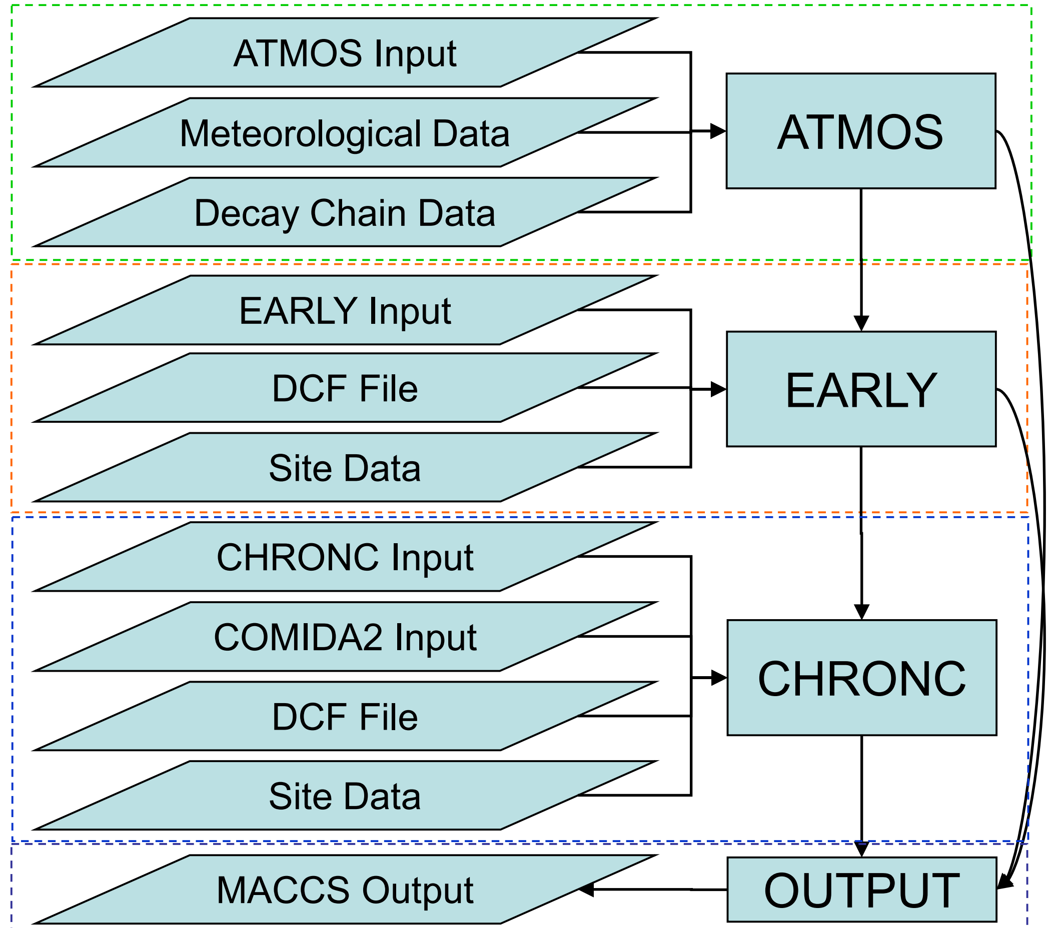
MELCOR Accident Consequence Code System (MACCS)

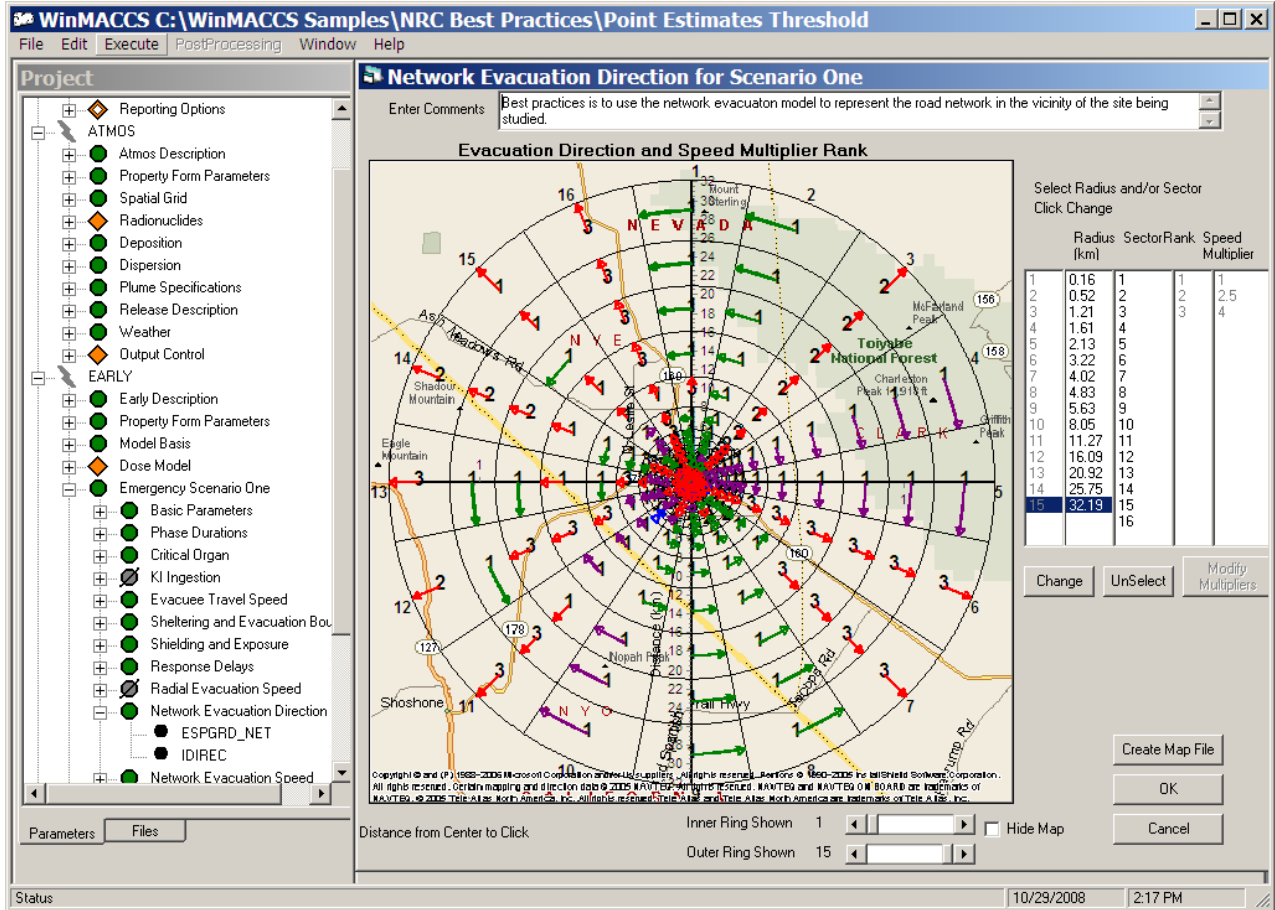
- Tool used to assess the risk and consequence associated with a hypothetical release of radioactive material to the atmosphere.
- Dispersion based on Gaussian plume segment model (with provisions for meander and surface roughness effects)
- Multiple Plumes (i.e. up to 200)
- Radioactive decay and ingrowths
- Models aleatory (random) uncertainty due to weather



MACCS model structure data and data flow

MACCS models realistic response

- Models evacuation, shelter, and KI protective actions
- Evacuation speeds and direction developed from site specific evacuation time estimate (ETE)
- 6 cohorts modeled for Surry (also Peach Bottom)
- Unique response characteristics for each cohort
- MACCS also models relocation of the public.



MACCS input screen showing radial directions and speed reduction multipliers (sample site, not Surry)

- Relocation implemented, typically beyond the EPZ, for hotspot and normal dose criteria and for habitability criteria

MACCS Parameters

MACCS Uncertain Parameter Groups

Deposition

- Wet Deposition (CWASH1)
- Dry Deposition Velocities (VDEPOS, m/s)

Dispersion

- Crosswind Dispersion Linear Coefficient (CYSIGA)
- Vertical Dispersion Linear Coefficient (CZSIGA)

Shielding factors

- Groundshine Shielding Factors (GSHFAC)
- Inhalation Protection Factors (PROTIN)

Latent Health Effects

- Dose and dose rate effectiveness factor (DDREFA)
- Lifetime Cancer Fatality Risk Factors (CFRISK)
- Long Term Inhalation Dose Coefficients

Early Health Effects

- Early Health Effects LD_{50} Parameter (EFFACA)
- Early Health Effects Exponential Parameter (EFFACB)
- Early Health Effects Threshold Dose (EFFTHR)

All of these parameters were uncertain in the SOARCA Peach Bottom UA too

MACCS Uncertain Parameter Groups (continued)

Emergency Response

- Evacuation Delay (DLTEVA)
- Evacuation Speed (ESPEED)
- Hotspot Relocation Time (TIMHOT)
- Normal Relocation Time (TIMNRM)
- Hotspot Relocation Dose (DOSHOT)
- Normal Relocation Dose (DOSNRM)

Aleatory Uncertainty

- Weather trials

All of these parameters were uncertain in the SOARCA Peach Bottom UA too

Groundshine Shielding Factors (GSHFAC)

- Values of GSHFAC are important because doses received from groundshine are directly proportional to these factors and groundshine is usually the most important of the long-term dose pathways.
- Uncertainty exists in factors that affect GSHFAC, such as indoor residence time, housing shielding value, and degree of departure from the infinite flat plane assumptions.

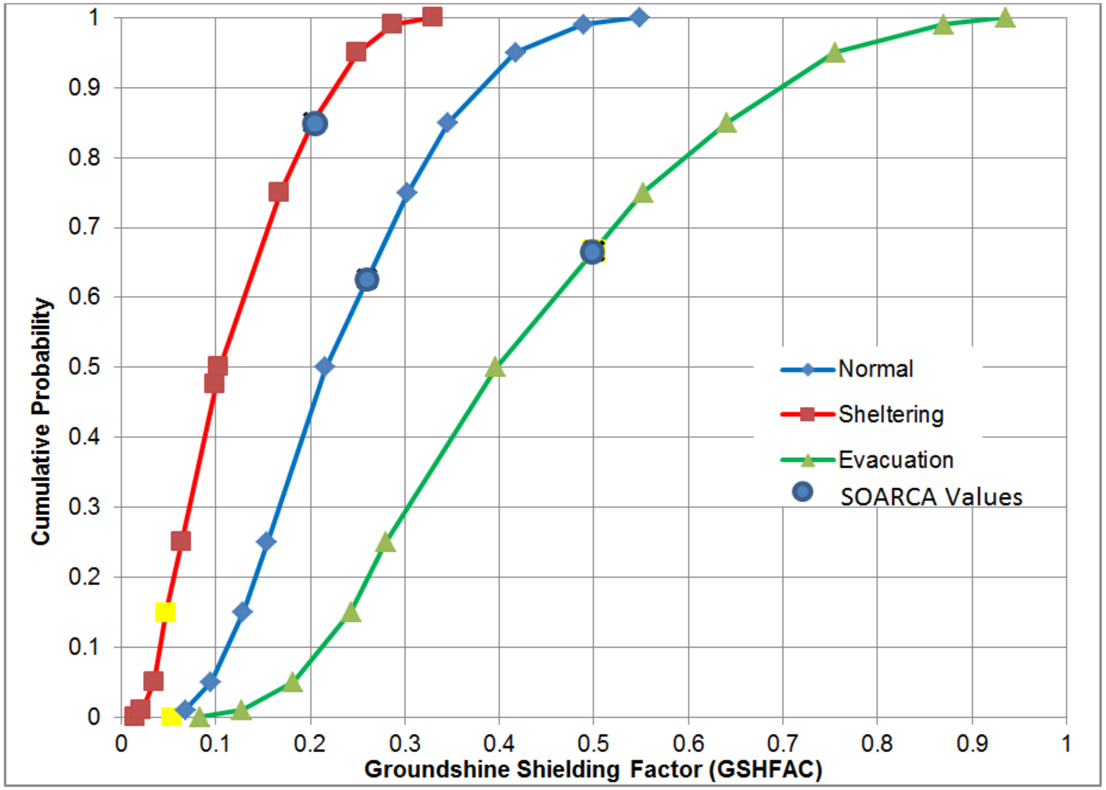


Figure 4-45 Cumulative distribution functions of GSHFAC for normal activity, sheltering, and evacuation based on expert elicitation data

Lifetime Cancer Fatality Risk Factors (CFRISK)

- Risk factors (CFRISK) are based on a 50-year lifetime dose commitment to specified target organs (risk/Sv).
- Probability of a lifetime cancer fatality is calculated separately for each cancer syndrome related to each target organ.
- Based on the technical approach described in BEIR V.

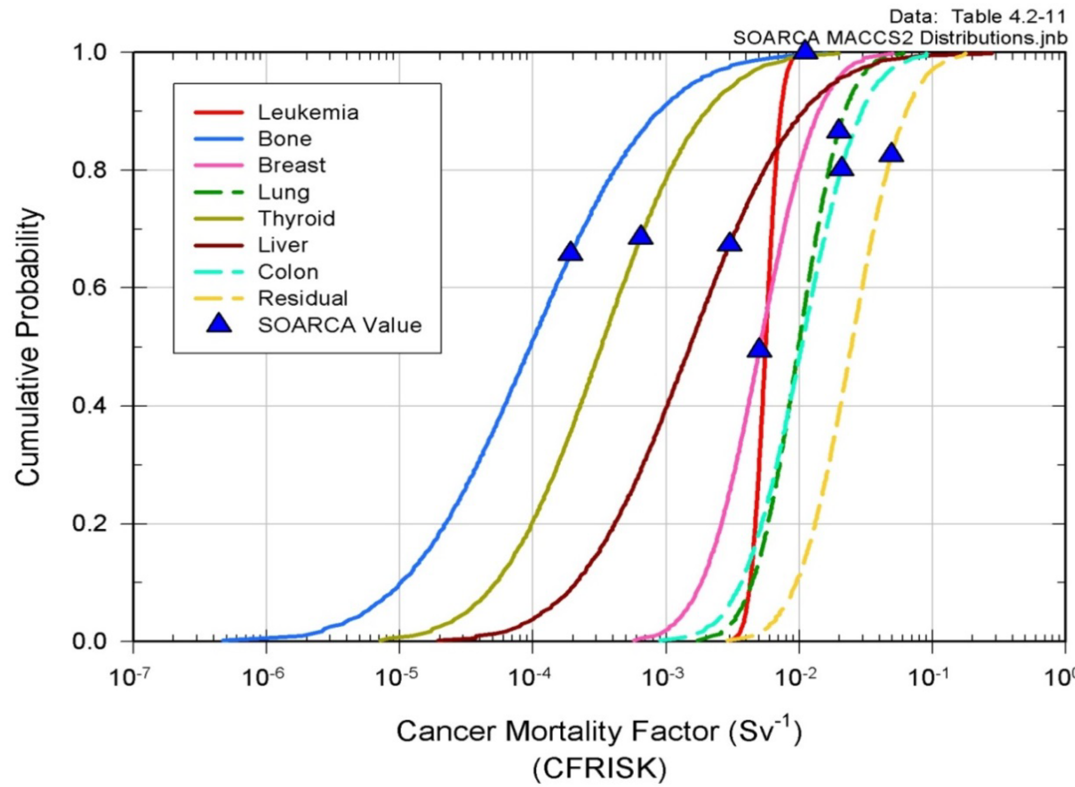


Figure 4-54 Cumulative distribution functions for CFRISK for each of the organs included in the analysis

Dose and Dose Rate Effectiveness FActor (DDREFA)

- In MACCS, doses received during the emergency phase are divided by DDREFA when the committed dose is less than 0.2 Sv.
- Doses received during the long-term phase are assumed to be controlled by the habitability criterion to be well below 0.2 Sv, so these doses are always divided by DDREFA in the calculation of latent health effects.

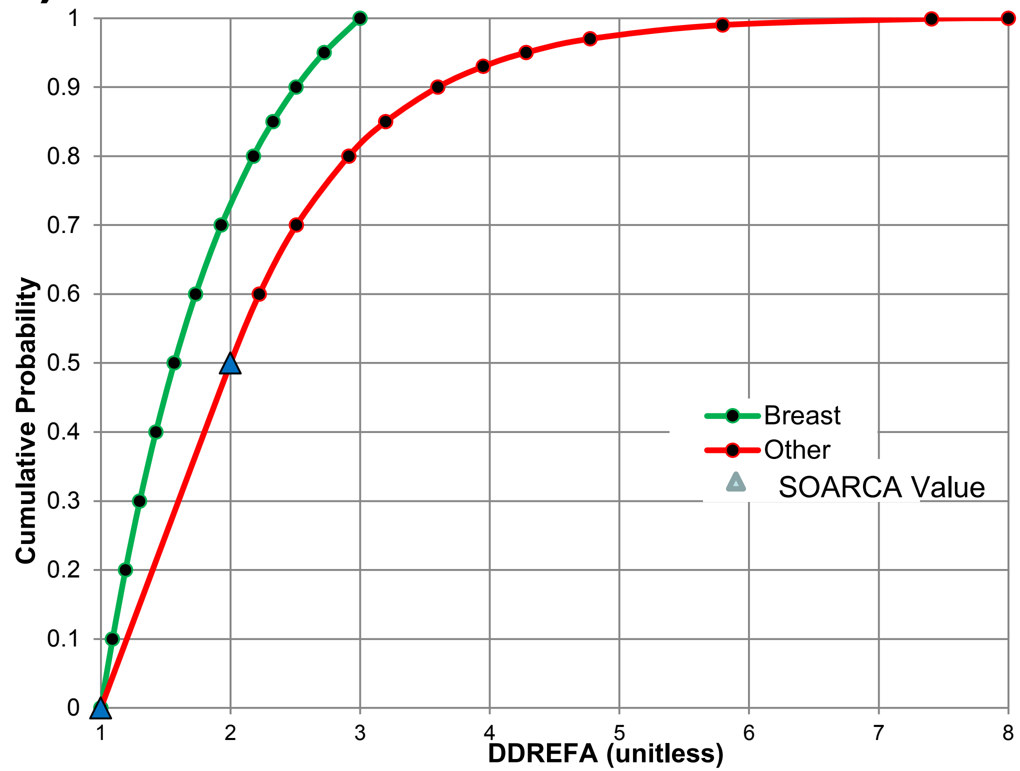


Figure 4-53 Cumulative distribution functions of DDREFA for breast and other cancer types

Dry Deposition Velocity (VDEPOS)

- Dry deposition velocities are established by aerosol bins to represent the dependence of deposition velocity on particle size.
- Dry deposition is the only mechanism for deposition onto the ground for more than 90 percent of the hours of the year at Surry.
- Long-term exposures usually contribute more than 50 percent of the overall exposure, thus deposition is important because deposited material is the only source of exposure during the long term.

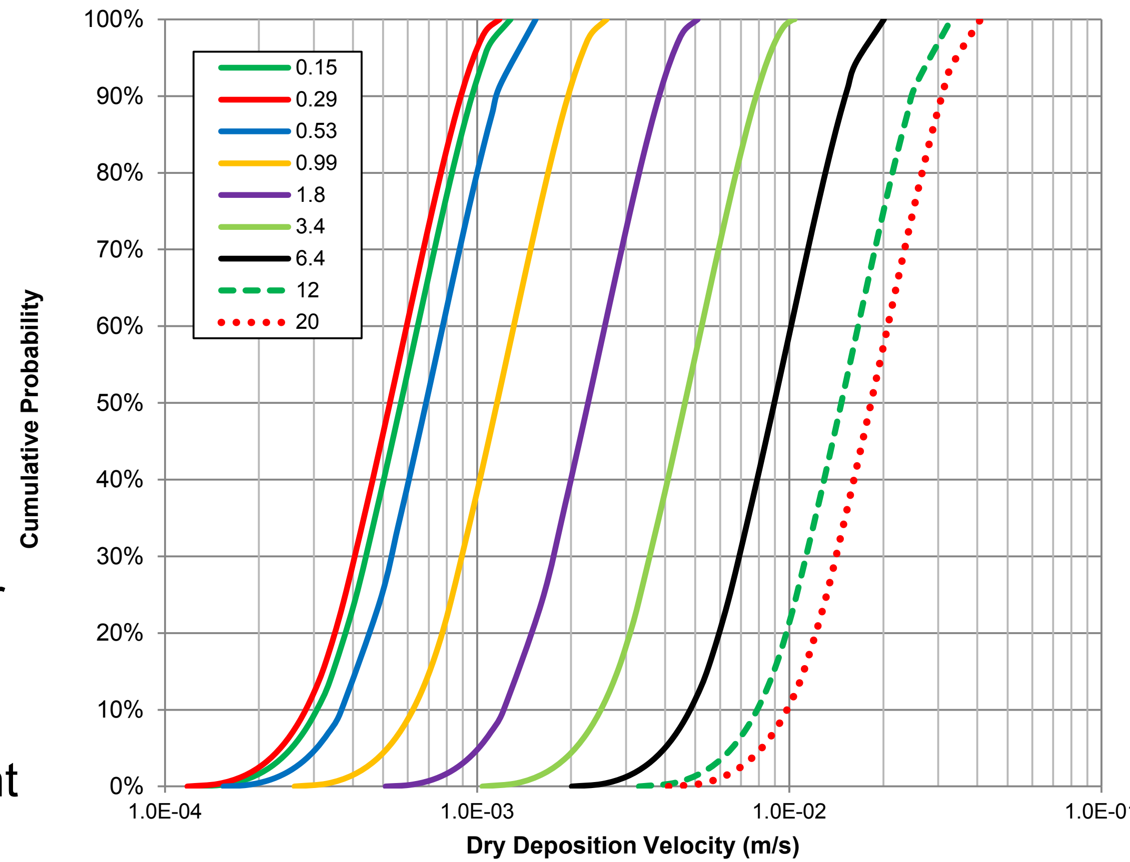


Figure 4-44 CDF of dry deposition velocities for mass median diameters representing MACCS aerosol bins

Note: The Peach Bottom UA indicated that dry deposition velocity is the most important parameter of all those considered for individual latent cancer risk

Crosswind Dispersion Linear Coefficient (CYSIGA)

- Medians of expert data were chosen as medians of SOARCA UA distributions, but they were made narrower to reflect that sampled values would be used to represent a year of weather data
- CZSIGA is the vertical dispersion linear coefficient and is treated similarly.

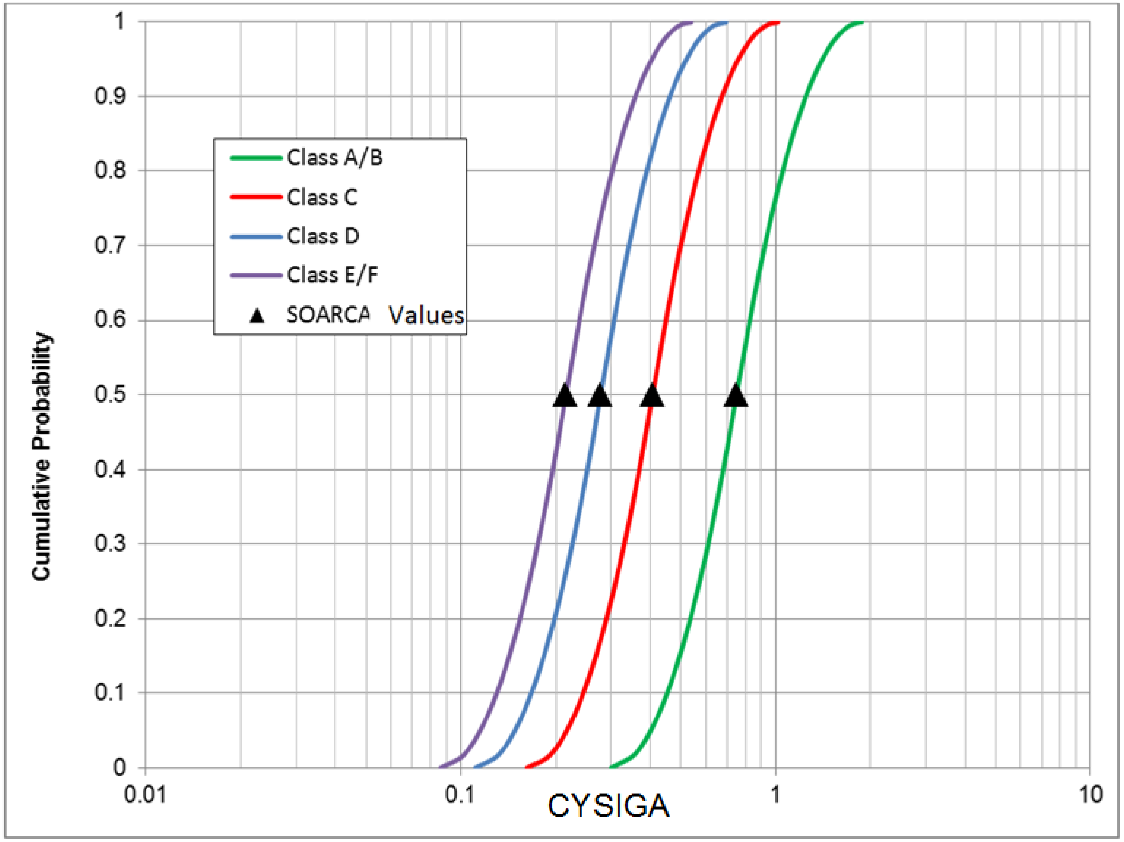


Figure 4-57 CDFs of CYSIGA for individual stability classes

Weather

- Weather-binning approach in MACCS was implemented.
- Consists of 16 predefined bins for combinations of stability class and wind speed and 20 user-defined bins for rain occurring before the plume travels 32 km (20 miles).
- Probability of weather in each weather bin is proportional to the number of hours of data that go into that bin, and thus the weather bins are not equally probable.

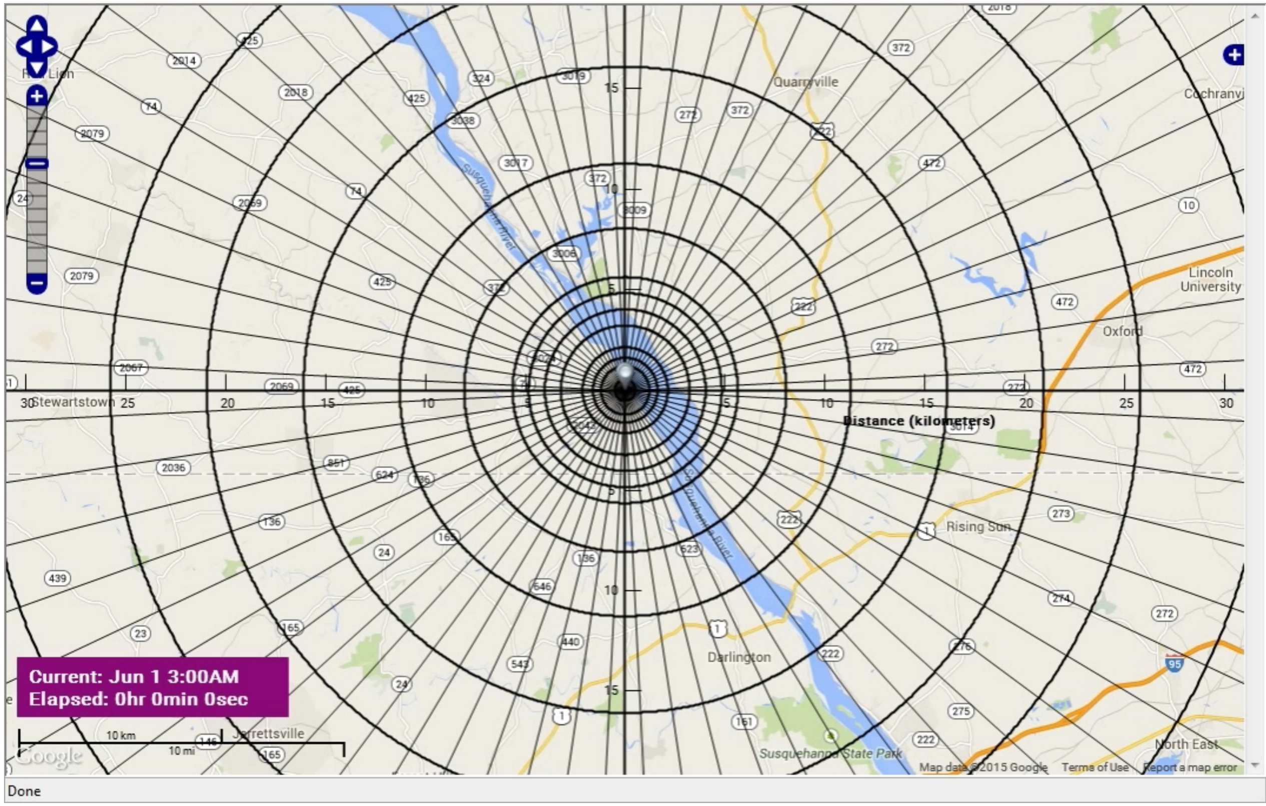
	Rain Bins			
Rain Distance (miles)	Rain Intensity (mm/hr)			
	0 - 2	2 - 4	4 - 6	> 6
< 2 miles	Bin 17	Bin 18	Bin 19	Bin 20
2 - 3.5 miles	Bin 21	Bin 22	Bin 23	Bin 24
3.5 - 7 miles	Bin 25	Bin 26	Bin 27	Bin 28
7 - 13 miles	Bin 29	Bin 30	Bin 31	Bin 32
13 - 20 miles	Bin 33	Bin 34	Bin 35	Bin 36
> 20 miles	Not a rain bin – use wind speed and stability class binning			

	Wind Speed and Stability Class Bins					
Stability Class	Wind Speed u (m/s)					
	0 - 1	1 - 2	2 - 3	3 - 5	5 - 7	> 7
A/B	Bin 1			Bin 2		
C/D	Bin 3	Bin 4	Bin 5	Bin 6	Bin 7	Bin 8
E	Bin 9	Bin 10	Bin 11	Bin 12		
F/G	Bin 13	Bin 14	Bin 15	Bin 16		

Table 4-16 Rain bins and wind speed and stability class bins.

Weather Illustrations

- Illustration showing how plume segments move with wind shifting from northwest to northeast
- Each segment has its own width depending on the amount of dispersion that has occurred as it experiences varying weather conditions
- Each segment has a unique length depending on wind speed



Video display of plume segments

Evacuation Speed (ESPEED)

- Represents the speed for each of the evacuating cohorts for the duration of the middle phase (most congested period of travel within the EPZ).
  - Cohort 1 (0-10 public)
  - Cohort 2 (10-20 shadow)
  - Cohort 3 (schools)
  - Cohort 4 (special facilities)
  - Cohort 5 (evacuation tail)
  - Cohort 6 (non-evacuating)
- Triangular distribution was used to represent uncertainty because there is confidence in the mode derived from the ETE report.

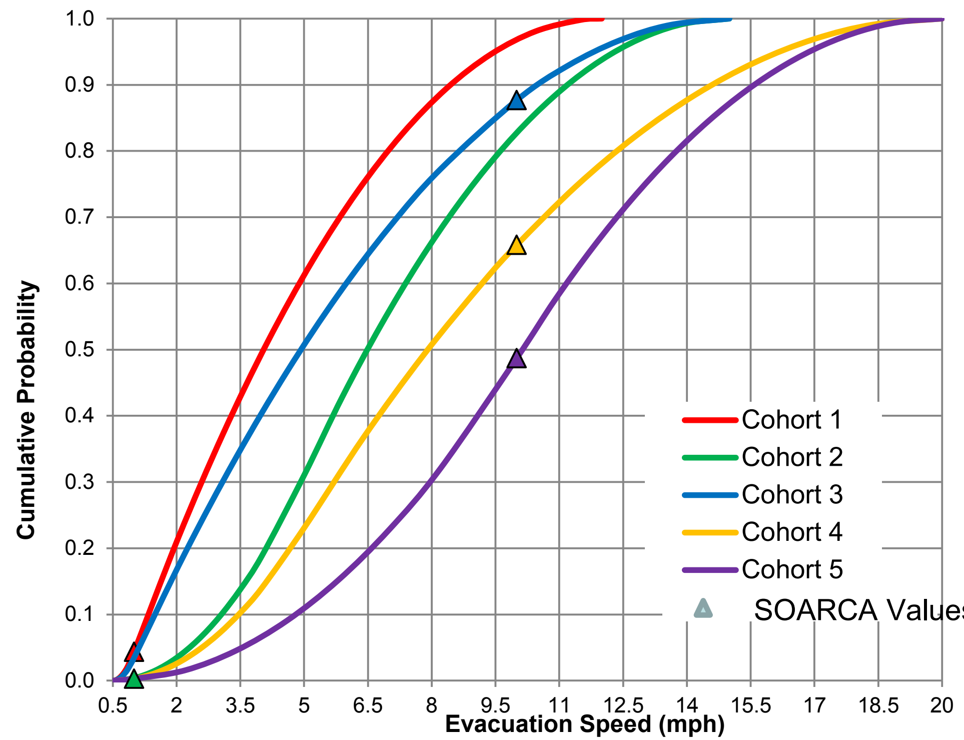


Figure 4-61 CDFs of ESPEED for each cohort

Hotspot Relocation Time (TIMHOT)

- TIMHOT is the estimated time needed to relocate residents from areas that exceed the hotspot dose threshold (DOSHOT).
- MACCS implements by removing the entire affected population from the dose equation at the time specified by TIMHOT after plume arrival.
- Actual relocation would occur over a period of time, thus an average value was developed for the affected population.
- Normal relocation time (TIMNRM) is applied similarly with a longer time period to account for a larger affected area.

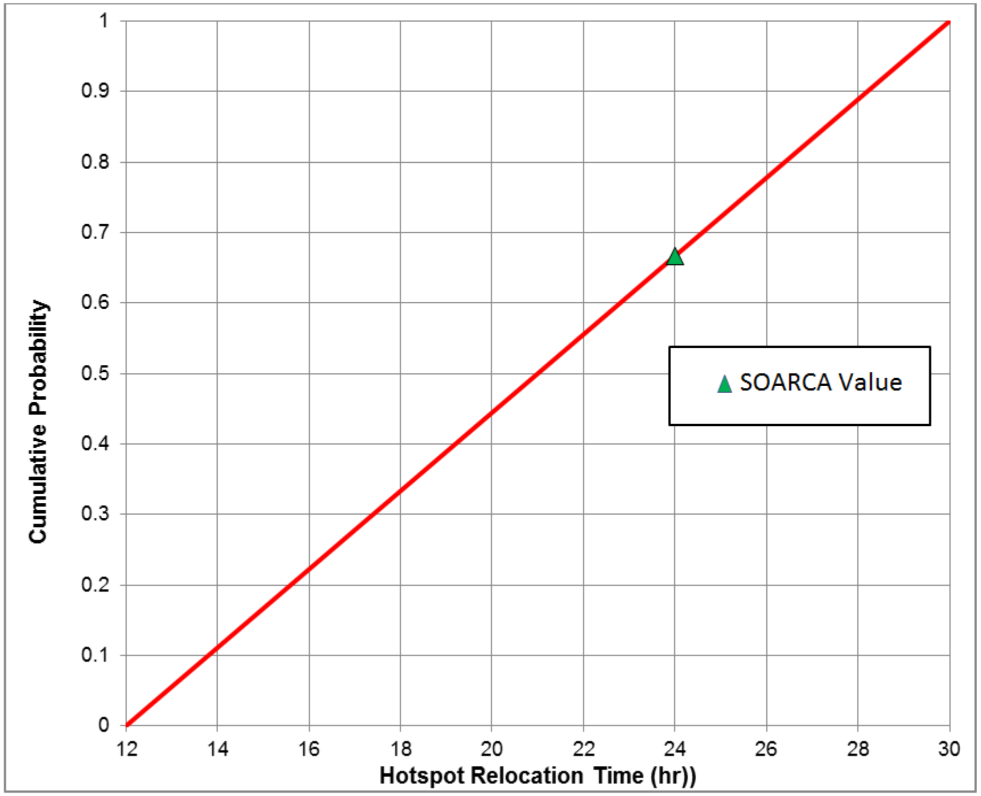


Figure 4-63 CDF of TIMHOT

Hotspot Relocation Dose (DOSHOT)

- DOSHOT is projected dose used to initiate hotspot relocation. Surry SOARCA modeled 5 rem over 7 days.
- If the total dose to individuals exceeds DOSHOT, affected people are relocated (i.e., removed from the analysis) at a user specified hotspot relocation time (TIMHOT).
- Normal Relocation Dose (DOSNRM) is applied similarly with a lower dose threshold. Surry SOARCA modeled 1 rem over 7 days.

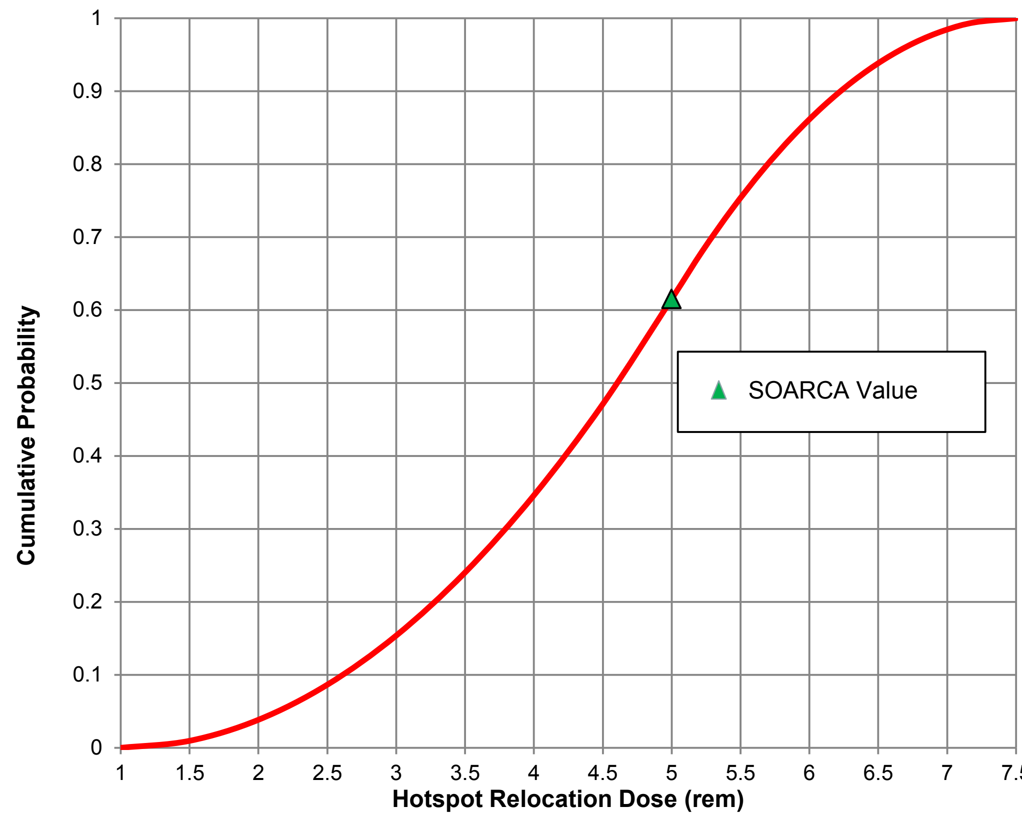


Figure 4-64 CDF of DOSHOT

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MELCOR Analysis Results

MELCOR Modeling

- For review: 1003 successful Monte Carlo realizations
- Regression analyses: one for all realizations, one for just SGTR realizations, and one for non-SGTR realizations. Additional regressions were done for the three times at cycle independently (BOC, MOC, and EOC) identified in table below.

Output Parameter Data set used based on occurrence of SGTR												
Cycle	Cs_frac			I_frac			H2_Prod			Release Timing		
	SGTR	Non-SGTR	Both	SGTR	Non-SGTR	Both	SGTR	Non-SGTR	Both	SGTR	Non-SGTR	Both
All	✓	✓	✓	✓	✓	✓	✓	✓	✓		✓	✓
BOC		✓	✓		✓	✓		✓	✓			
MOC		✓	✓		✓	✓		✓	✓			
EOC		✓	✓		✓	✓		✓	✓			

Table 6-1 MELCOR regression analyses completed

Overview of MELCOR Results

- An SGTR occurred in 10% of realizations, and a hot leg nozzle rupture occurred in 93% of realizations
- In every realization that an SGTR occurred, a hot leg nozzle rupture also occurred
- Prior to core damage, the secondary side depressurized through main steamline leakage and safety valve FTC
- An SV on the RCS primary side (on the pressurizer) failed to close in 68% of realizations
- The steel containment liner yielded/tore in 74% of realizations
- Containment rebar yielded in 7% of realizations.

Cesium Release Fraction to Environment

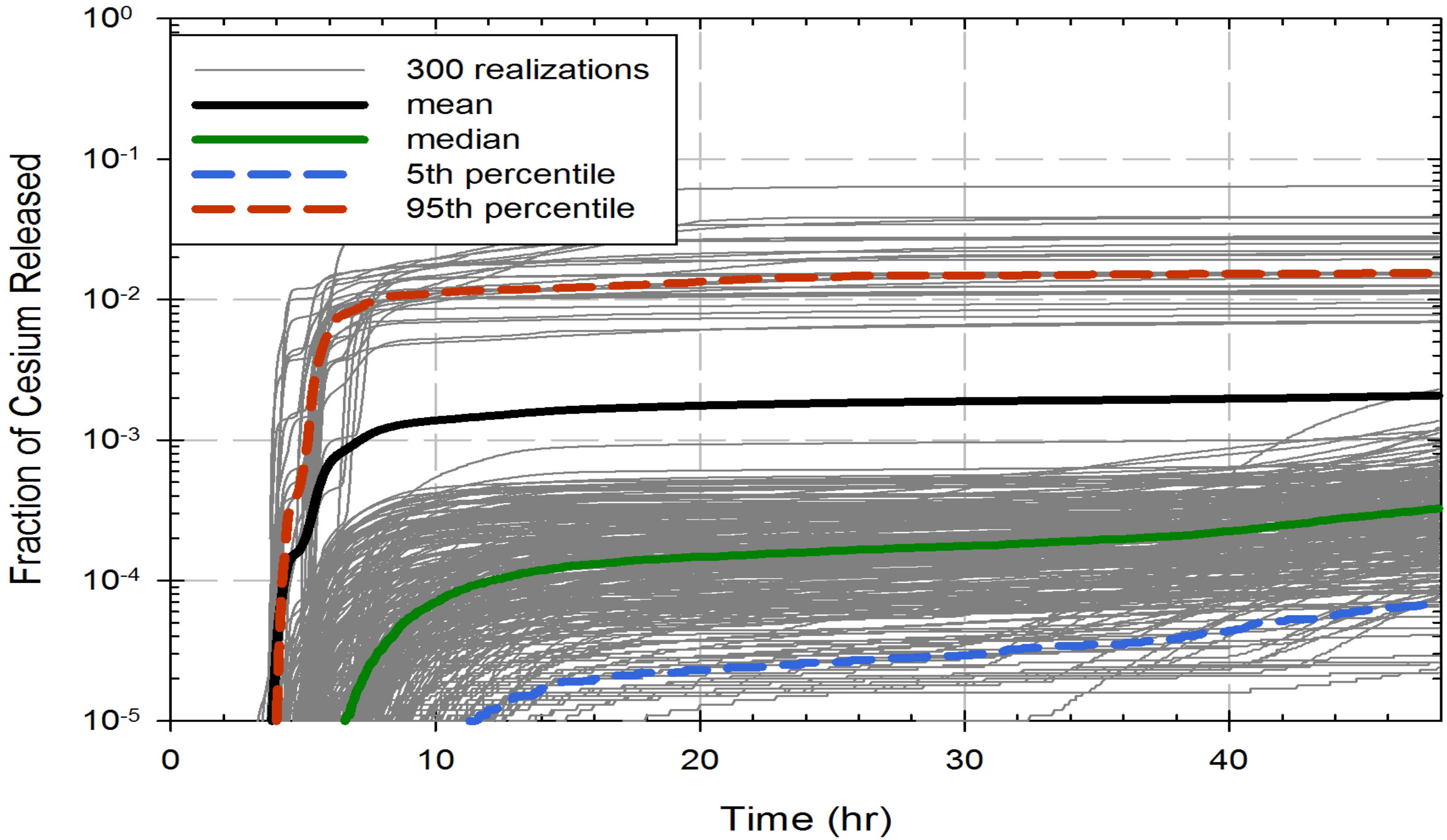
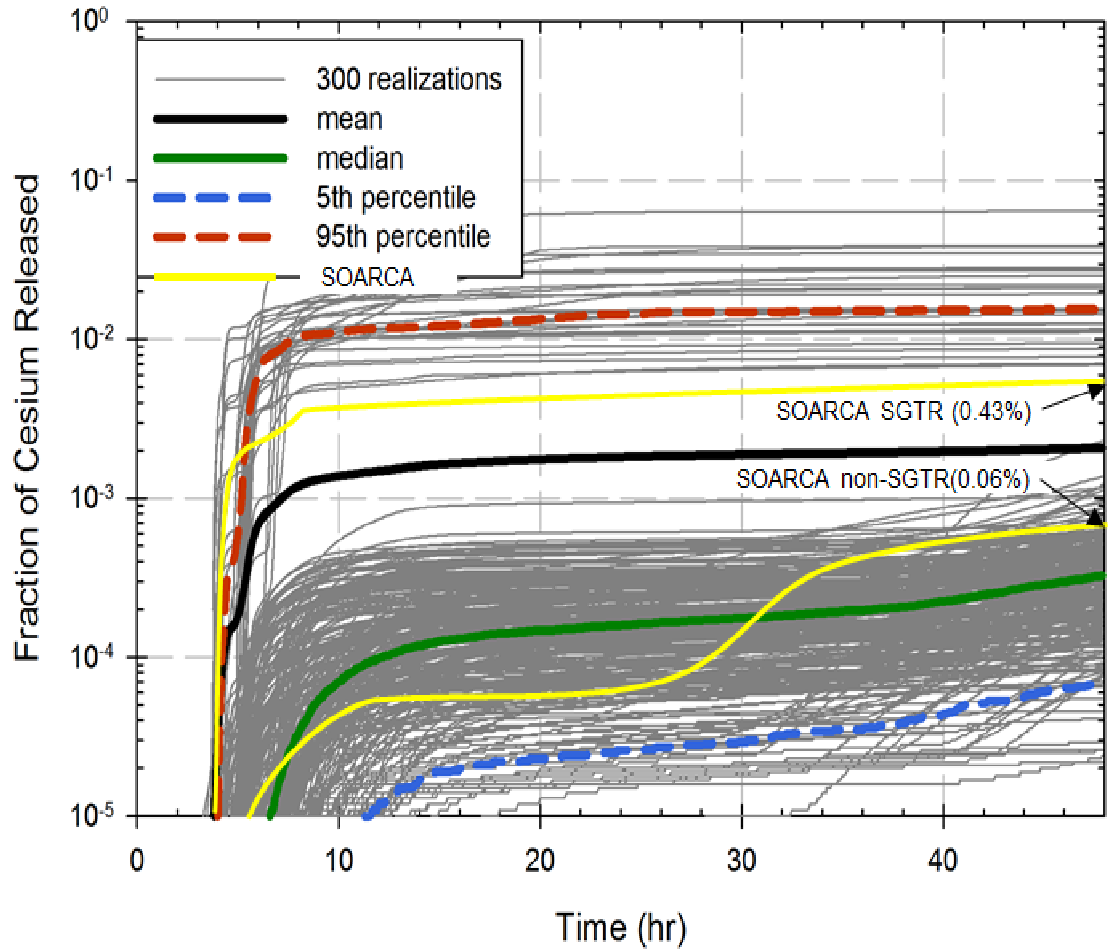


Figure 6-13 Cesium release fractions over 48 hours with mean, median, 5th and 95th percentiles (which are calculated at each point in time)

Cesium Release Fractions

- UA cesium environmental release fractions are equal to or lower than the Surry SOARCA calculation except when an SGTR occurs.
- The early UA non-SGTR median release is higher than SOARCA due to nominal leakage sampling (0.1%-1%) in the UA.
- SOARCA release was higher than the UA non-SGTR median release at 48 hour due to higher pressurization (limestone concrete) and resultant containment rebar yield at 25.5 hour.
- The UA SGTR results are higher than SOARCA due to addition of size-dependent aerosol capture.



Cesium release fractions over 48 hours with mean, median, 5th and 95th percentiles

Cesium Regression Analysis for non-SGTR Realizations

- Design leakage was identified by each regression technique as the largest contributor to uncertainty.
- The next two parameters, time at cycle and shape factor, explain the majority of the remaining uncertainty that is explained by the regression models. There is some uncertainty that is not explained by the regression models.

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Conjoint Contr. *
Final R^2	0.67		0.79		0.83		0.68			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
DLEAK	0.35	0.58	0.29	0.34	0.25	0.48	0.35	0.35	0.258	0.077
CYCLE	0.15	0.42	0.19	0.29	0.12	0.30	0.20	0.20	0.135	0.073
PARTSHAPE	0.05	0.23	0.12	0.16	0.09	0.33	0.18	0.21	0.085	0.081
CFC	0.04	-0.20	0.07	0.12	0.03	0.20	0.11	0.12	0.050	0.060
DEV_DEC_HEAT	0.02	-0.15	0.04	0.07	0.05	0.23	0.05	0.06	0.031	0.061
CHEMFORMCS	0.03	-0.16	0.03	0.06	0.00	0.05	0.04	0.04	0.020	0.022
SVOAFRAC	0.01	-0.12	0.00	0.05	0.01	0.10	0.01	0.04	0.006	0.044
SC1132	0.01	0.07	0.00	0.02	0.00	0.02	0.01	0.03	0.004	0.015
TUBTHICK	0.00	0.03	0.01	0.00	0.00	0.02	---	---	0.003	0.006
CONDENS	0.00	-0.06	0.00	0.02	0.00	0.02	0.01	0.01	0.003	0.011
SGTRLOC	0.01	0.07	0.00	0.01	---	---	0.00	0.01	0.002	0.003
RDSTC	0.00	0.06	---	---	0.00	0.02	0.00	0.01	0.001	0.005
SC1131	0.00	-0.04	0.00	0.00	0.00	0.02	---	---	0.001	0.004
RCPSL	0.00	0.08	---	---	0.00	0.01	0.00	0.01	0.001	0.003
SRVFAILT	---	---	---	---	---	---	0.00	0.01	0.000	0.001
SV_STATUS	---	---	0.00	0.06	---	---	0.00	0.00	0.000	0.016
SG_B_SV_cycl	---	---	0.00	0.01	0.00	0.01	---	---	0.000	0.008
TUBETEMP	---	---	0.00	0.01	0.00	0.01	---	---	0.000	0.006
CHEMFORMI2	---	---	---	---	---	---	0.00	0.00	0.000	0.000

* highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1

Table 6-4 Regression analysis of cesium release fraction in non-SGTR realizations.

Cesium Regression Analysis for SGTR Realizations

- Safety valve open area fraction (SVOAFRAC) is a partial indicator for the open fraction of both the primary and secondary SV system at 48 hours.
  - The number of safety valve cycles is also thought to be important, though not shown in regression results
- Sampling thickness of hottest SG tube (TUBTHICK) effectively also samples stress multiplier on the creep equation.

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Con-joint Contr.*
Final R^2	0.54		1.00		0.81		0.54			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
SVOAFRAC	0.13	0.58	0.30	0.75	0.39	0.71	---	---	0.188	0.234
TUBTHICK	0.21	-0.27	0.00	0.02	0.07	0.14	0.36	0.36	0.115	0.023
SV_STATUS	---	---	0.04	0.12	---	---	0.33	0.33	0.072	0.026
CYCLE	0.08	0.34	0.00	0.02	0.06	0.12	0.22	0.22	0.062	0.024
PARTSHAPE	0.12	0.36	0.00	0.00	0.05	0.09	0.09	0.09	0.052	0.011
CHEMFORMCS	---	---	---	---	0.06	0.27	---	---	0.015	0.058
DLEAK	---	---	0.04	0.59	---	---	---	---	0.014	0.184
SC1141	---	---	0.00	0.00	0.01	0.06	---	---	0.004	0.013
SV_WTR_CYC	---	---	0.01	0.09	---	---	0.00	0.00	0.003	0.028
SGTRLOC	---	---	0.01	0.03	---	---	---	---	0.003	0.009
H2LFL	---	---	0.00	0.00	0.00	0.00	0.00	0.00	0.002	0.001
SC1131	---	---	0.00	0.00	0.01	0.01	0.00	0.00	0.002	0.002
RDSTC	---	---	0.00	0.00	---	---	---	---	0.001	0.000
SRVFAILT	---	---	0.00	0.01	---	---	0.00	0.00	0.000	0.004
CONDENS	---	---	0.00	0.00	---	---	0.00	0.00	0.000	0.002
RDMTC	---	---	---	---	0.00	0.02	0.00	0.00	0.000	0.006
RCPSL	---	---	---	---	0.00	0.00	0.00	0.01	0.000	0.001
SC1132	---	---	---	---	---	---	0.00	0.01	0.000	0.001
SV_NBCYC	---	---	---	---	---	---	0.00	0.00	0.000	0.001
DEV_DEC_HEAT	---	---	---	---	---	---	0.00	0.00	0.000	0.000
TUBETEMP	---	---	---	---	---	---	0.00	0.00	0.000	0.000

Table 6-9 Regression analysis of cesium release fraction for SGTRs.

Iodine Release Fraction to Environment

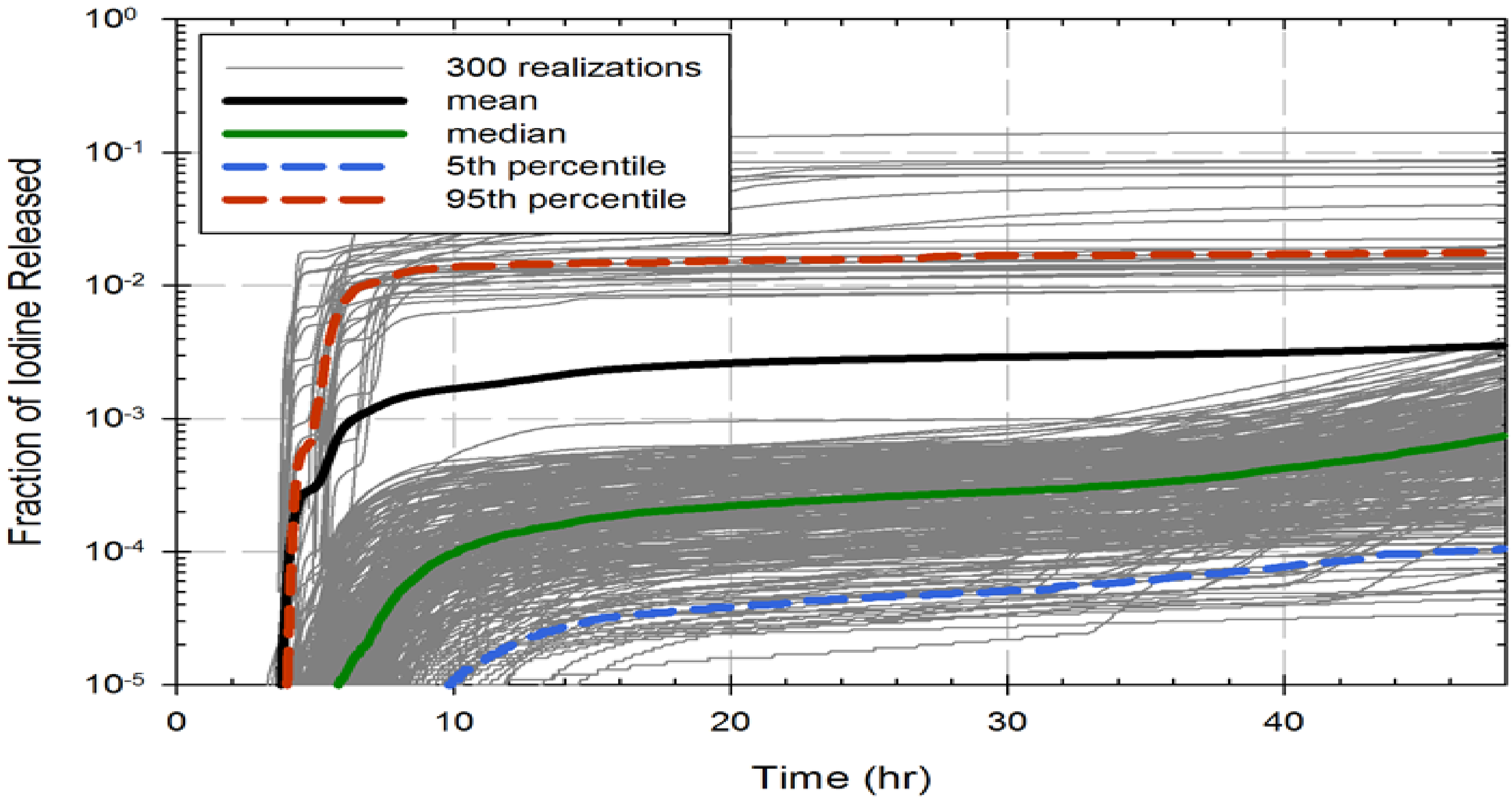


Figure 6-1 Iodine release fractions over 48 hours with mean, median, 5th and 95th percentiles (which are calculated at each point in time)

Iodine Regression Analysis – non-SGTR realizations

- CYCLE determines timing of fuel heat-up, which in turn causes initial radionuclide releases, and later in the accident progression, the rate of containment pressurization.
- The set of most important variables to iodine and cesium release are consistent.

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Conjoint Contr. *
Final R^2	0.77		0.92		0.88		0.82			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
CYCLE	0.51	0.69	0.39	0.50	0.37	0.60	0.40	0.40	0.380	0.102
CFC	0.04	-0.21	0.09	0.15	0.08	0.25	0.12	0.16	0.074	0.080
DLEAK	0.11	0.32	0.08	0.11	0.04	0.10	0.09	0.09	0.073	0.029
CHEMFORMI2	0.03	0.18	0.06	0.10	0.09	0.29	0.11	0.17	0.063	0.089
DEV_DEC_HEAT	0.03	-0.18	0.06	0.11	0.03	0.16	0.08	0.11	0.043	0.063
PARTSHAPE	0.02	0.15	0.04	0.06	0.02	0.11	0.07	0.11	0.034	0.041
SC1132	0.00	0.07	0.01	0.02	0.00	0.01	0.01	0.03	0.005	0.011
SV_STATUS	0.00	0.09	0.02	0.09	---	---	---	---	0.004	0.023
CONDENS	0.01	-0.07	0.01	0.01	---	---	0.00	0.01	0.004	0.003
SV_NBCYC	---	---	---	---	0.00	0.00	0.01	0.01	0.002	0.000
SRVFAILT	---	---	0.00	0.01	0.00	0.01	0.00	0.00	0.002	0.005
SGTRLOC	0.00	0.05	0.00	0.02	0.00	0.02	---	---	0.002	0.009
RDSTC	0.00	0.04	---	---	0.00	0.01	---	---	0.001	0.001
CHEMFORMCS	---	---	0.00	0.01	---	---	0.00	0.01	0.001	0.005
SG_B_SV_cycl	---	---	---	---	---	---	0.00	0.01	0.001	0.001
TUBETEMP	---	---	0.00	0.01	0.00	0.01	---	---	0.001	0.005
RCPSL	0.00	0.06	---	---	---	---	---	---	0.001	0.000
RDMTC	---	---	---	---	---	---	0.00	0.00	0.000	0.000
TUBTHICK	0.00	0.03	---	---	0.00	0.00	---	---	0.000	0.000
SVOAFRAC	0.00	-0.08	0.00	0.01	0.00	0.02	---	---	0.000	0.009
SC1131	0.00	-0.03	0.00	0.02	---	---	0.00	0.01	0.000	0.009
SV_WTR_CYC	---	---	---	---	0.00	0.01	0.00	0.00	0.000	0.003

* highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1

Table 6-3 Regression analysis of iodine release fraction for non-SGTR realizations

Hydrogen Production

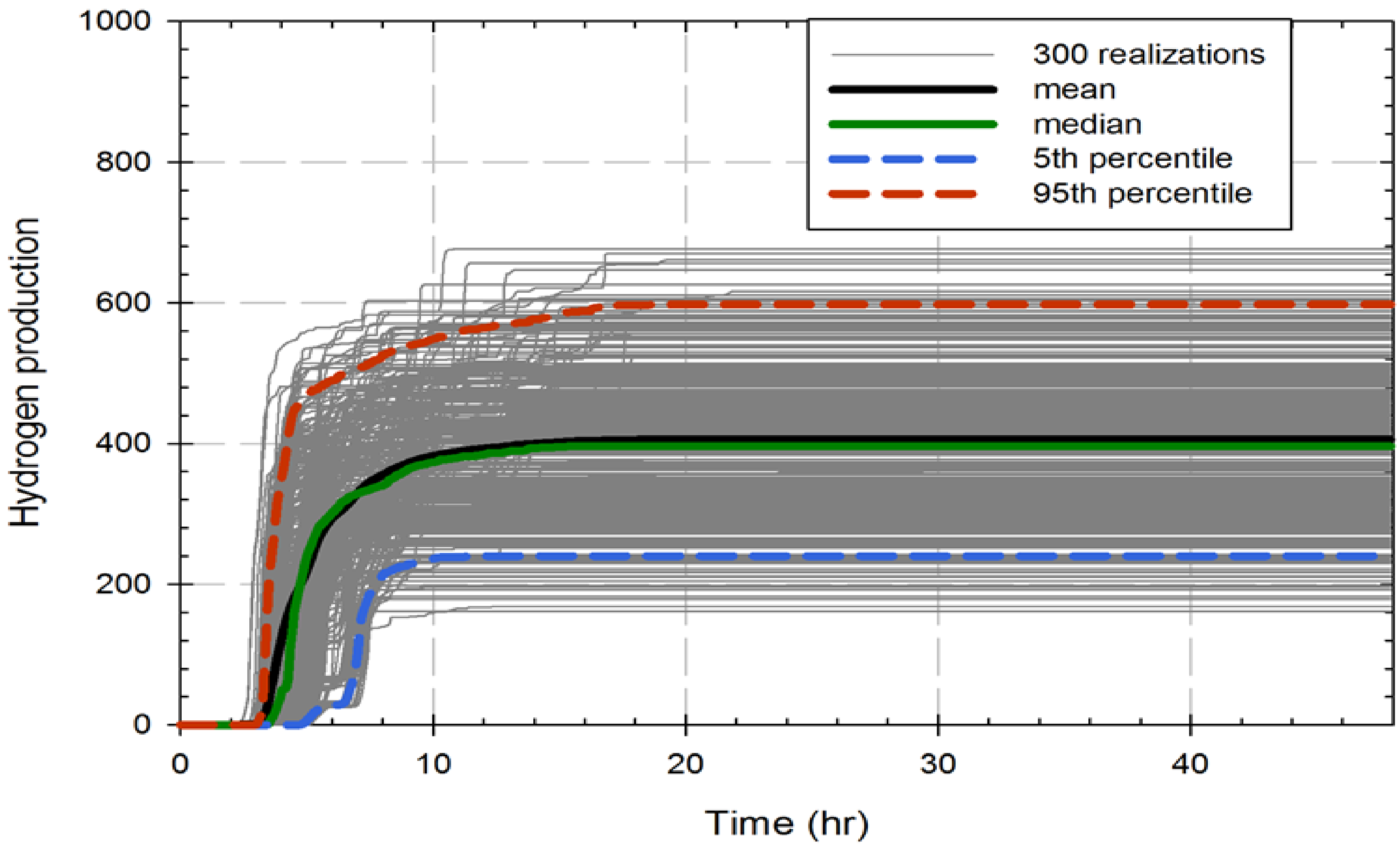


Figure 6-24 Total Hydrogen production over 48 hours with mean, median, 5th and 95th percentiles.

Hydrogen Production Regression Analysis

- Regression results for hydrogen uncertainty are essentially the same, regardless as to whether or not there is an SGTR.
- Depressurization (SVOAFRAC) and effective melt temperature remain as the most important parameters.

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Conjoint Contr. *
Final R^2	0.54		0.73		0.88		0.71			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
SVOAFRAC	0.03	0.23	0.52	0.54	0.31	0.45	0.54	0.54	0.266	0.046
SC1132	0.16	0.41	0.19	0.22	0.19	0.42	0.20	0.24	0.151	0.087
CYCLE	0.08	0.32	0.14	0.16	0.17	0.31	0.14	0.13	0.106	0.047
SV_STATUS	0.25	0.30	0.04	0.04	---	---	0.03	0.03	0.076	0.000
SC1131	---	---	0.01	0.04	0.02	0.18	0.01	0.06	0.010	0.066
SV_NBCYC	0.01	-0.13	0.01	0.00	0.00	0.05	0.02	0.02	0.007	0.015
RCPSL	0.00	0.06	0.02	0.05	---	---	0.02	0.01	0.007	0.007
DLEAK	---	---	0.01	0.01	---	---	---	---	0.001	0.000
RDSTC	---	---	0.00	0.01	0.00	0.01	0.00	0.00	0.001	0.004
SGTRLOC	0.00	0.07	---	---	---	---	0.00	0.00	0.001	0.000
SC1141	---	---	0.00	0.01	0.00	0.01	0.00	0.00	0.001	0.006
CHEMFORMI2	---	---	---	---	0.00	0.03	0.00	0.01	0.001	0.010
DEV_DEC_HEAT	---	---	---	---	0.00	0.01	---	---	0.001	0.004
SG_B_SV_cycl	---	---	---	---	0.00	0.01	0.00	0.00	0.001	0.003
TUBETEMP	---	---	---	---	0.00	0.02	0.00	0.00	0.000	0.006
TUBTHICK	---	---	---	---	0.00	0.01	0.00	0.00	0.000	0.003
CFC	---	---	---	---	---	---	0.00	0.01	0.000	0.001
PARTSHAPE	---	---	0.00	0.01	---	---	---	---	0.000	0.001
CONDENS	---	---	0.00	0.00	---	---	---	---	0.000	0.000
RDMTC	---	---	---	---	0.00	0.01	---	---	0.000	0.004
H2LFL	---	---	---	---	0.00	0.01	---	---	0.000	0.003
SV_WTR_CYC	---	---	---	---	0.00	0.01	---	---	0.000	0.003

* highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1

Table 6-5 Regression analysis of hydrogen production in non-SGTR realizations

MELCOR Analysis Extended to 72 Hours

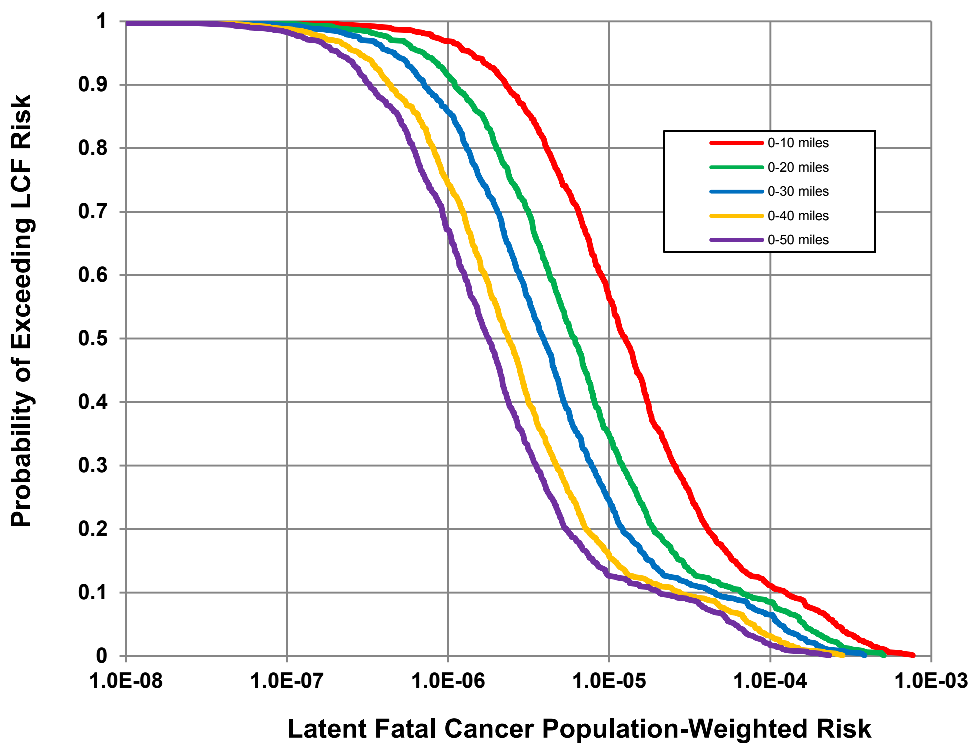
- An analysis of selected realizations was also performed to evaluate physical effects that impact environmental release fractions, particularly containment failure, that might occur beyond 48 hours.
- Extended the MELCOR calculations for 9 individual MELCOR realizations to 72 hours.
- For cases in which rebar yield was reached, the pressure tends to level off (to a plateau) and then gradually decrease as leakage more than compensates for steam generation and heating of the atmosphere.
- There are marked increases in cesium and iodine environmental release at the point of liner yield (or rebar yield if reached), with some increases of an order of magnitude from 48 to 72 hours.

MACCS Analysis Results

Note that results and draft report are in the process of being updated

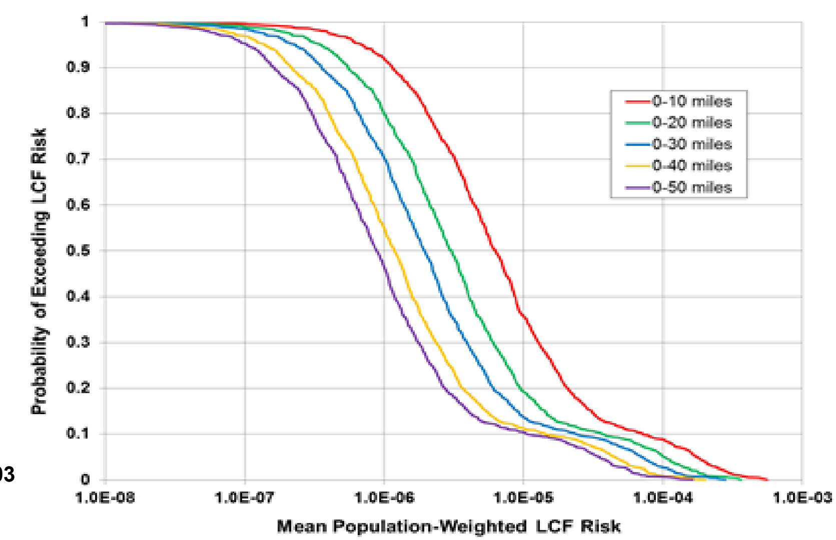
Preliminary Individual LCF Risk Consequence Results

Mean (over weather), individual, LCF risk (based on LNT) per event, 0-10 miles



New LCF Risk results

0-10 miles LCF Risk	Difference - higher
Mean	1.6x
Median	2x
5th percentile	2x
95th percentile	1.5x



Old LCF Risk results

Consequence Regression Analyses, LNT (10 mile, All Rlzs)

- The most important parameter is tube thickness.
- Second most important is the SV open area fraction.
- The third most important input parameter is the time at cycle.
- Fourth is groundshine shielding factor for normal activity during the emergency phase, GSHFAC.2, which is fully correlated with the groundshine shielding factor for the long-term phase.

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Conjoint Contr. *
Final R^2	0.54		0.60		0.86		0.74			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
TUBTHICK	0.04	-0.20	0.33	0.53	0.30	0.86	0.35	0.80	0.189	0.309
SVOAFRAC	0.03	-0.18	0.23	0.40	0.09	0.55	0.11	0.45	0.082	0.250
CYCLE	0.18	0.44	0.01	0.02	0.01	0.01	0.02	0.02	0.050	0.005
GSHFAC.2	0.13	0.35	0.02	0.05	0.00	0.00	0.01	0.03	0.038	0.011
DLEAK	0.08	0.26	0.01	0.04	0.01	0.01	0.00	0.01	0.022	0.010
CFRISK.8	0.02	0.15	0.02	0.05	0.00	0.06	0.02	0.08	0.011	0.037
SV_STATUS	---	---	0.04	0.04	---	---	---	---	0.006	0.000
DDREFA.8	0.01	-0.12	0.00	0.02	0.00	0.05	0.00	0.03	0.004	0.025
CYSIGA.1	0.02	-0.13	---	---	---	---	---	---	0.004	0.000
TUBETEMP	---	---	0.02	0.02	0.00	0.00	0.01	0.03	0.004	0.006
DEV_DEC_HEAT	0.01	-0.09	0.00	0.03	0.00	0.03	0.01	0.02	0.004	0.015
VDEPOS.1	0.01	0.09	0.01	0.01	0.00	0.04	---	---	0.003	0.011
CFRISK.7	0.01	0.09	---	---	---	---	---	---	0.002	0.000
CFC	0.01	-0.09	---	---	---	---	0.00	0.01	0.002	0.001
CFRISK.6	0.01	0.07	---	---	---	---	0.00	0.02	0.002	0.003
PROTIN.2	---	---	---	---	0.01	0.09	---	---	0.001	0.023
CHEMFORMCS	0.01	-0.06	---	---	---	---	---	---	0.001	0.000
SGTRLOC	0.00	0.06	0.00	0.01	---	---	---	---	0.001	0.002
CFRISK.2	---	---	0.00	0.03	---	---	---	---	0.001	0.005
LA.140_ICH.9	---	---	0.00	0.04	0.00	0.03	0.00	0.01	0.000	0.018
PARTSHAPE	---	---	0.00	0.01	0.00	0.01	---	---	0.000	0.004
CHEMFORMI2	---	---	---	---	0.00	0.02	0.00	0.01	0.000	0.009

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Table 6-24 Mean, individual, LCF risk (based on LNT) regression results within a 10-mile circular area for all realizations.

- The top two parameters largely control whether an SGTR occurs, which has a dominant effect on consequences. Both parameters have large conjoint contributions which imply that there is some synergistic influence on LCF risk from TUBTHICK and SVOAFRAC in conjunction with each other or other parameters.

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Consequence Regression Analyses, LNT (10 mile w/o SGTR)

- The most important parameter is the time during the fuel cycle at which the accident occurs, CYCLE.
- Second is the groundshine shielding factor for normal activity during the emergency phase, GSHFAC.2, which is fully correlated with the groundshine shielding factor for the long-term phase.
- The third most important input parameter is the containment leakage path length, DLEAK, which is inversely proportional to containment leakage rate.

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Conjoint Contr. *
Final R^2	0.73		0.83		0.84		0.59			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
CYCLE	0.27	0.52	0.14	0.23	0.18	0.55	0.30	0.30	0.179	0.128
GSHFAC.2	0.19	0.44	0.22	0.38	0.18	0.54	0.25	0.27	0.166	0.152
DLEAK	0.12	-0.33	0.06	0.20	0.03	0.16	0.09	0.09	0.062	0.077
CFRISK.8	0.03	0.16	0.04	0.07	0.02	0.14	0.07	0.07	0.030	0.040
CYSIGA.1	0.03	-0.18	0.02	0.02	0.01	0.04	0.04	0.05	0.019	0.012
VDEPOS.1	0.02	0.12	0.02	0.06	0.01	0.24	0.03	0.07	0.017	0.081
PARTSHAPE	0.01	0.09	0.01	0.03	0.00	0.04	0.05	0.06	0.012	0.018
CFRISK.7	0.01	0.12	0.02	0.03	0.00	0.02	0.03	0.03	0.012	0.007
CFC	0.02	-0.13	0.02	0.03	0.00	0.02	0.02	0.02	0.010	0.009
DEV_DEC_HEAT	0.01	-0.08	0.02	0.04	0.00	0.04	0.02	0.04	0.010	0.021
CFRISK.6	0.01	0.09	0.02	0.07	0.00	0.04	0.01	0.01	0.009	0.023
DDREFA.8	0.01	-0.11	0.01	0.03	0.01	0.03	---	---	0.006	0.014
CHEMFORMCS	0.01	-0.09	---	---	---	---	0.02	0.03	0.005	0.001
GSHFAC.3	---	---	0.02	0.07	---	---	---	---	0.005	0.014
CFRISK.4	0.01	0.09	---	---	---	---	---	---	0.002	0.000
CFRISK.3	---	---	---	---	---	---	0.01	0.02	0.001	0.003
SV_NBCYC	---	---	0.00	0.02	---	---	0.00	0.00	0.001	0.005
SGTRLOC	0.00	0.04	0.00	0.03	---	---	0.00	0.00	0.000	0.009
CFRISK.1	---	---	---	---	0.00	0.12	---	---	0.000	0.033
DLTEVA_5.12	---	---	---	---	0.00	0.04	---	---	0.000	0.011
ZR.95_ICH.9	---	---	---	---	0.00	0.03	---	---	0.000	0.008

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Table 6-30 Mean, individual, LCF risk (based on LNT) regression results within a 10-mile circular area for realizations that do not involve SGTR.

Consequence Regression Analyses, LNT (50 mile, All Rlzs)

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Conjoint Contr. *
Final R^2	0.37		0.60		0.86		0.59			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
TUBTHICK	0.04	-0.21	0.26	0.43	0.35	0.89	0.47	0.88	0.194	0.269
SVOAFRAC	0.02	-0.14	0.21	0.37	0.08	0.55	0.12	0.53	0.071	0.247
GSHFAC.2	0.12	0.34	0.02	0.06	0.00	0.05	---	---	0.033	0.022
DLEAK	0.07	0.24	0.01	0.08	---	---	---	---	0.018	0.014
CYCLE	0.05	0.23	0.00	0.01	0.00	0.02	0.01	0.01	0.014	0.006
SV_STATUS	---	---	0.04	0.11	---	---	---	---	0.008	0.013
CFRISK.8	0.01	0.13	0.01	0.04	0.00	0.04	---	---	0.005	0.016
DDREFA.8	0.02	-0.12	0.00	0.02	0.00	0.01	0.00	0.01	0.004	0.009
DEV_DEC_HEAT	0.01	-0.09	0.01	0.06	---	---	---	---	0.004	0.009
CFRISK.6	0.01	0.06	0.01	0.11	0.00	0.01	0.00	0.00	0.003	0.022
CFRISK.7	0.01	0.11	---	---	---	---	---	---	0.003	0.000
SV_NBCYC	---	---	0.01	0.04	---	---	0.00	0.01	0.002	0.007
PARTSHAPE	0.00	0.05	0.00	0.02	0.01	0.02	0.00	0.01	0.002	0.008
CYSIGA.1	0.01	-0.09	---	---	---	---	---	---	0.002	0.000
PROTIN.2	---	---	0.01	0.01	0.00	0.03	---	---	0.002	0.007
CWASH1.	0.01	0.08	---	---	---	---	---	---	0.002	0.000
CHEMFORMI2	---	---	---	---	0.01	0.01	0.00	0.02	0.002	0.003
CFC	0.01	-0.07	---	---	---	---	---	---	0.001	0.000
CHEMFORMCS	0.01	-0.06	---	---	---	---	---	---	0.001	0.000
VDEPOS.1	---	---	0.00	0.03	---	---	---	---	0.001	0.004
CFRISK.2	---	---	0.00	0.05	0.00	0.03	0.00	0.00	0.001	0.019
CE.143_ICH.9	---	---	---	---	0.00	0.00	0.00	0.00	0.001	0.000

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Table 6-26 Mean, individual, LCF risk (based on LNT) regression results within a 50-mile circular area for all realizations.

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Consequence Regression Analyses, LNT (50 mile w/o SGTR)

- The most important parameter is the time during the fuel cycle at which the accident occurs, CYCLE.
- The second most important input parameter is the groundshine shielding factor for normal activity during the emergency phase, GSHFAC.2.
- The third most important input parameter is containment design leakage.

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Conjoint Contr. *
Final R^2	0.72		0.84		0.82		0.67			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
CYCLE	0.29	0.54	0.14	0.23	0.18	0.55	0.28	0.28	0.185	0.128
GSHFAC.2	0.16	0.41	0.19	0.30	0.18	0.56	0.22	0.26	0.155	0.144
DLEAK	0.12	-0.34	0.06	0.20	0.03	0.16	0.10	0.11	0.065	0.077
CFRISK.8	0.03	0.16	0.04	0.06	0.01	0.11	0.06	0.07	0.027	0.037
PARTSHAPE	0.01	0.09	0.02	0.02	0.00	0.03	0.05	0.07	0.015	0.013
CYSIGA.1	0.03	-0.15	0.01	0.02	0.01	0.03	0.03	0.03	0.014	0.009
CFC	0.02	-0.13	0.02	0.09	0.01	0.06	0.02	0.02	0.013	0.035
DEV_DEC_HEAT	0.01	-0.08	0.01	0.09	0.01	0.03	0.02	0.04	0.011	0.031
CFRISK.6	0.01	0.09	0.03	0.08	---	---	0.02	0.06	0.011	0.023
VDEPOS.1	0.01	0.09	0.02	0.03	0.01	0.22	0.01	0.05	0.010	0.070
CFRISK.7	0.01	0.12	---	---	0.00	0.06	0.02	0.03	0.008	0.017
CHEMFORMCS	0.01	-0.08	0.01	0.02	0.00	0.04	0.02	0.03	0.007	0.016
DDREFA.8	0.01	-0.11	---	---	0.01	0.08	0.01	0.02	0.006	0.021
GSHFAC.3	---	---	0.02	0.07	---	---	---	---	0.006	0.014
CFRISK.4	0.01	0.10	---	---	---	---	0.01	0.01	0.003	0.001
RCPSL	---	---	0.01	0.13	---	---	---	---	0.003	0.034
CFRISK.3	---	---	---	---	---	---	0.01	0.04	0.002	0.006
CWASH1.	0.01	0.07	---	---	---	---	---	---	0.001	0.000
SGTRLOC	---	---	0.00	0.02	---	---	---	---	0.001	0.004
CM.242_ICH.9	---	---	---	---	0.00	0.05	---	---	0.001	0.013
CFRISK.1	---	---	---	---	0.00	0.08	---	---	0.001	0.020
PROTIN.2	---	---	0.00	0.02	---	---	---	---	0.000	0.005

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Table 6-32 Mean, individual, LCF risk (based on LNT) regression results within a 50-mile circular area for realizations without an SGTR.

MACCS Uncertainty Analysis on MELCOR Single Realizations

- MACCS parameters-only Monte Carlo simulation run with three MELCOR source terms.
- The results show that the Surry UA CCDFs span the results for the single MELCOR realizations, with the exception of the upper end of the curves for the large source term (Large ST).
- This shows that there are low probability combinations of input parameters that can produce larger consequences than any of those in the set of 1003 realizations.

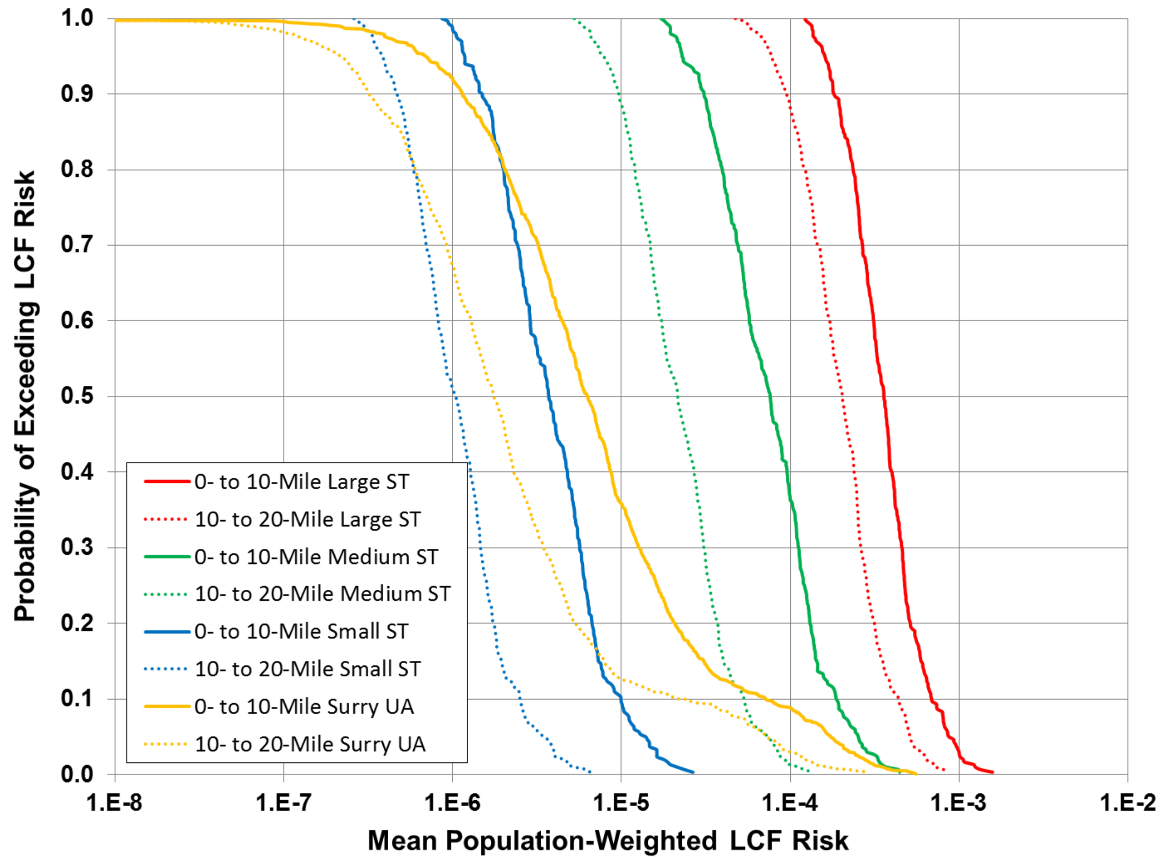

Figure 6-123 CCDF of mean, population-weighted LCF risk (based on LNT dose response) within two annular areas centered on the Surry site for three single realizations and for the base Surry uncertainty analysis results

Sensitivity Results for Phase Durations and Dose Projection Period

- The Figure shows results are all essentially the same with one exception, the risks for the 0- to 10-mile distance interval are noticeably larger for the case when the intermediate phase is 6 months than when its duration is 0 (no intermediate phase).
- The increase in risk for the 0- to 10-mile interval indicates that less decontamination occurs when the intermediate phase is included and that more individuals receive a larger dose when they return home than receive a smaller dose.

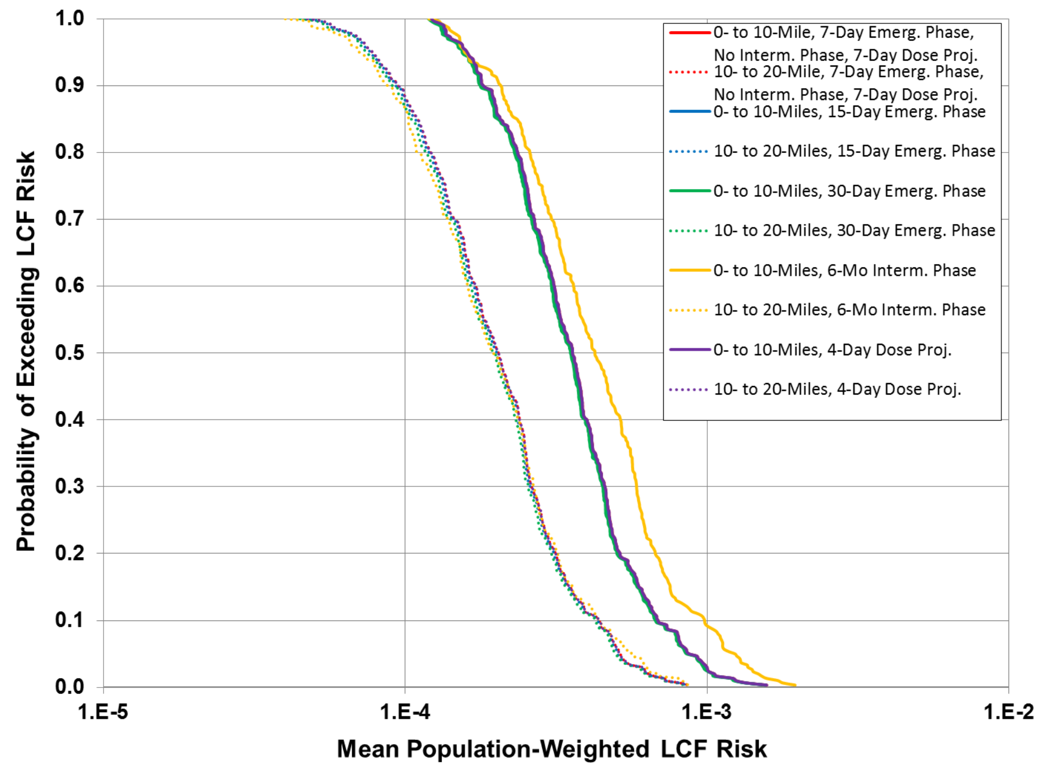

Figure 6-124 CCDF of mean, population-weighted LCF risk (based on LNT dose response) within two annular areas centered on the Surry site for four sensitivity cases and for the Large Source Term case

Evaluation of Multiple Tube Ruptures

- The CCDFs for the SG tube UA span the range of 10^{-5} to more than 10^{-3} mean, population-weighted, conditional LCF risk (per event.)
- The CCDFs for the SG tube UA overlap the portion of the Surry UA CCDFs representing a single SGTR (probability of exceedance below 0.1), but also extend beyond them at the upper end of the range. This is because the source terms are larger when multiple SGTRs occur in a realization.

Summary and Insights

- Surry UA corroborates SOARCA study conclusions.
- A major determinant of source term magnitude and health consequences is whether or not a steam generator tube rupture (SGTR) occurs.
- The most influential input parameters contributing to accident progression, cesium release magnitude, and individual LCF risk were found to be:
  - SV open area fraction and the number of cycles experienced by SVs
  - SG tube thickness
  - Time at cycle (BOC, MOC, or EOC)
  - Containment leakage rate
  - Dynamic particle shape
  - Groundshine shielding factor
- SGTRs occurred in about 10% of the Monte Carlo realizations and had release fractions 1 to 2 orders of magnitude larger than realizations without an SGTR.
- SGTRs always included a thermal *and* pressure element.
- In the number of SG tubes joint sensitivity analysis, one realization with 5 tubes failing had no hot leg creep leading to the highest release fractions.

Summary and Insights

- In most of the Monte Carlo realizations, iodine and cesium environmental release fractions were higher early in the transient than the Surry SOARCA calculation; but all were significantly lower at 48 hours (except that cesium was equal in a few realizations).
- The UA non-SGTR median release at 48 hr is lower than SOARCA due to higher pressurization (limestone concrete) and resultant containment rebar yield at 25.5 hr in SOARCA.
- Early fatality risks for this scenario are essentially zero.
- The LCF risk was observed lower than the Surry SOARCA calculation and is attributable to the lower source terms from the UA.

Summary and Insights

- The consequence analysis showed that the mean population-weighted LCF risk distribution is much narrower when only uncertain consequence parameters are considered than when both source-term and consequence parameters are considered in the analysis. It appears the results are more heavily influenced by uncertainties in source term than by uncertain consequence parameters, just as they were for the Peach Bottom UA.
  - This is true when a single dose-response model (LNT) is used, but uncertainties in risks created by uncertainties in the dose-response model are large and most likely would have altered this conclusion if dose response had been included as part of the integrated UA.
- Mean (over epistemic uncertainty and weather variability) individual LCF risks assuming LNT dose response, conditional on an the occurrence of an accident, estimated in this UA are very low, lower than the risk evaluated in the original SOARCA study, which was 9×10^{-5} within 10 miles, and lower at longer distances.
  - The primary reason for this reduction in estimated risk is attributed to refinements in the MELCOR model
- Most of the risks (99% within 10 miles and about 84% beyond 10 miles from the plant) are from long-term exposure following the emergency phase.

Next Steps

- Finalize NUREG/CR report documenting SOARCA Surry Uncertainty Analysis by September 2016.
- Develop summary NUREG report on insights from the SOARCA Peach Bottom, Surry, and Sequoyah Uncertainty Analyses.
- Contribute to identifying key sources of model uncertainty in the level-2 and level-3 portions of Level 3 PRA.
- Contribute to MACCS input parameter guidance under development.
- Contribute to appendix under development on severe accident consequence analysis supporting cost-benefit analyses.

References

- NUREG-1935, State-of-the-Art Reactor Consequence Analyses (SOARCA) Report (November 2012)
- NUREG/BR-0359, Modeling Potential Reactor Accident Consequences, Rev. 1 (December 2012)
- 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 (expected February 2016)
- SECY-12-0092, “State-of-the-Art Reactor Consequence Analyses – Recommendation for Limited Additional Analysis” (July 2012)
  – Staff recommended “UA for a severe accident scenario at Surry”

Questions?

Acronyms

BEIR	Biological Effects of Ionizing Radiation
BOC	Beginning of cycle
CDF	Cumulative distribution function
CCDF	Complementary CDF
EOC	End of cycle
EPZ	Emergency planning zone
ETE	Evacuation time estimate
FTC	Failure to open
FTO	Failure to close
KI	Potassium iodide
LCF	Latent cancer fatality
LHS	Latin Hypercube Sampling
LNT	Linear no threshold
MACCS	MELCOR Accident Consequence Code System
MCCI	Molten concrete core interaction
MOC	Middle of cycle
PWR	Pressurized water reactor
RCPSL	Reactor coolant pump seal leakage
RCS	Reactor coolant system
RN	Radionuclide
RPV	Reactor pressure vessel
SGTR	Steam generator tube rupture
SME	Subject matter expert
SNL	Sandia National Laboratories
SOARCA	State-of-the-Art Reactor Consequence Analyses
STSBO	Short term station blackout
SV	Safety valve
UA	Uncertainty Analysis

Backup Slides

Iodine Release Fractions to Environment

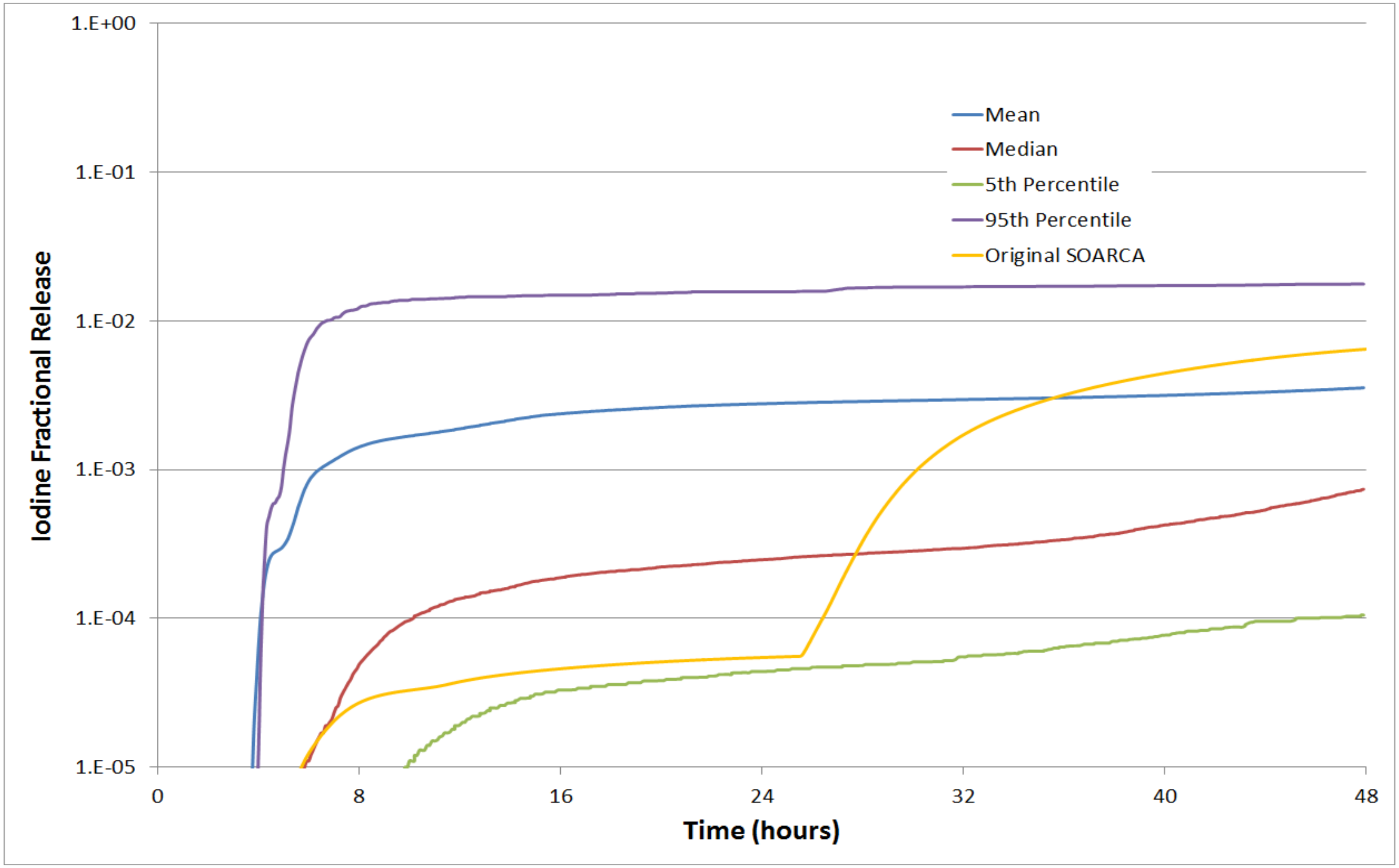

Figure 6-3 Comparison of iodine release fraction in the original Surry SOARCA STSBO to the calculated values of all successful realizations

Consequence Regression Analyses, LNT (10 mile with SGTR)

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Conjoint Contr. *
Final R^2	0.66		1.00		0.71		0.51			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
CFRISK.8	0.12	0.33	---	---	---	---	0.28	0.28	0.067	0.000
VDEPOS.1	0.12	0.25	---	---	0.01	0.07	0.26	0.26	0.065	0.013
TUBTHICK	---	---	0.02	0.22	0.15	0.43	0.12	0.12	0.065	0.130
GSHFAC.3	0.09	0.21	---	---	0.04	0.29	0.25	0.25	0.063	0.059
I.134_ICH.9	0.02	0.19	0.01	0.01	0.18	0.24	0.00	0.01	0.040	0.016
SV_STATUS	---	---	0.11	0.31	---	---	0.00	0.00	0.037	0.067
DLTEVA_5.7	0.06	0.14	---	---	0.08	0.22	---	---	0.029	0.033
DLEAK	0.01	0.09	0.08	0.59	---	---	0.00	0.00	0.023	0.171
CO.60_ICH.9	---	---	0.07	0.08	---	---	---	---	0.023	0.002
CFRISK.6	0.03	0.21	0.01	0.01	---	---	0.09	0.10	0.021	0.001
PROTIN.2	0.05	0.16	0.02	0.07	0.01	0.02	---	---	0.017	0.023
SVOAFRAC	---	---	---	---	0.07	0.12	0.00	0.00	0.016	0.013
CYCLE	0.03	0.14	0.02	0.03	---	---	---	---	0.012	0.006
SR.91_ICH.9	0.04	-0.17	---	---	---	---	---	---	0.011	0.000
GSHFAC.2	0.02	0.23	0.01	0.01	0.01	0.03	0.00	0.00	0.007	0.008
RH.105_ICH.9	0.03	0.22	---	---	---	---	0.00	0.01	0.007	0.001
DLTEVA_2.8	0.02	-0.08	---	---	---	---	---	---	0.004	0.000
TE.132_ICH.9	0.01	-0.15	---	---	---	---	0.00	0.00	0.004	0.000
DLTEVA_5.8	0.01	0.09	0.00	0.08	---	---	---	---	0.004	0.025
DLTEVA_3.5	---	---	0.01	0.16	---	---	---	---	0.003	0.049
DLTEVA.2	---	---	0.01	0.01	---	---	---	---	0.003	0.002
DLTEVA.7	---	---	---	---	0.01	0.07	0.00	0.00	0.001	0.014

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Table 6-27 Mean, individual, LCF risk (based on LNT) regression results within a 10-mile circular area for realizations with an SGTR.

Consequence Regression Analyses, LNT (50 mile with SGTR)

	Rank Regression		Quadratic		Recursive Partitioning		MARS		Main Contr.*	Conjoint Contr. *
Final R^2	0.69		1.00		0.79		0.63			
Input	R^2 contr.	SRRC	S_i	T_i	S_i	T_i	S_i	T_i		
TUBTHICK	---	---	0.04	0.15	0.15	0.55	0.16	0.16	0.087	0.142
VDEPOS.1	0.11	0.31	---	---	0.02	0.06	0.25	0.25	0.071	0.010
GSHFAC.3	---	---	---	---	0.00	0.03	0.27	0.27	0.057	0.008
GSHFAC.2	0.16	0.29	0.02	0.10	0.06	0.20	0.00	0.01	0.056	0.064
CFRISK.8	0.05	0.29	0.02	0.05	0.01	0.01	0.14	0.15	0.041	0.011
SVOAFRAC	---	---	0.01	0.27	0.09	0.15	0.00	0.00	0.025	0.104
DLEAK	0.04	0.09	0.03	0.61	0.03	0.10	0.00	0.01	0.023	0.214
CYSIGA.1	---	---	0.00	0.01	0.01	0.00	0.09	0.08	0.019	0.004
DLTEVA_5.6	0.02	-0.16	---	---	---	---	0.09	0.09	0.019	0.000
RCPSL	0.07	0.23	---	---	---	---	---	---	0.018	0.000
DLTEVA_4.8	---	---	0.00	0.04	0.06	0.36	---	---	0.017	0.093
SR.91_ICH.9	0.01	-0.10	0.02	0.05	0.03	0.09	---	---	0.013	0.027
PROTIN.2	0.05	0.17	---	---	---	---	---	---	0.013	0.000
CFRISK.7	0.03	0.16	0.01	0.20	---	---	0.00	0.00	0.010	0.064
DLTEVA_5.8	0.04	0.09	---	---	---	---	---	---	0.009	0.000
TE.132_ICH.9	0.03	-0.10	---	---	---	---	---	---	0.008	0.000
DLTEVA_2.11	---	---	0.02	0.16	0.00	0.02	---	---	0.007	0.052
DLTEVA.2	---	---	0.02	0.02	---	---	---	---	0.007	0.000
TIMHOT.	0.02	-0.18	0.01	0.11	---	---	---	---	0.007	0.034
DLTEVA_2.4	0.02	0.17	---	---	---	---	0.00	0.00	0.006	0.001
DLTEVA_2.8	0.02	-0.14	---	---	---	---	0.00	0.00	0.005	0.001
I.134_ICH.9	0.02	0.14	---	---	0.01	0.07	0.00	0.00	0.005	0.017

** highlighted if main contribution larger than 0.02 or conjoint contribution larger than 0.1*

Table 6-29 Mean, individual, LCF risk (based on LNT) regression results within a 50-mile circular area for realizations with an SGTR.

Updated MELCOR Base Case

Timing of Key Events: MELCOR Updated Base Case compared to Surry SOARCA

Event Timing (hh:mm)	Updated Base Case	Original SOARCA Value
STSBO – loss of all AC and DC electrical power, AFW unavailable	00:00	00:00
Reactor trips Main steam isolation valves (MISVs) close RCP seal leakage initiates at 21 gpm/pump	00:00	00:00
RCP seal failure (enhanced leakage)	-*	02:22
SG SV fails to close	01:11	-*
SG dryout	01:22	01:16
PRT rupture disk breaks	02:09	01:46
Pressurizer SV fails to close	03:00	01:27
First fission product gap release	03:22	02:27
Loop A hot leg nozzle rupture	04:28	-*
Loop C hot leg nozzle rupture	-*	03:45
Accumulators begin discharging	04:28	03:45
Accumulators empty	04:29	03:45
RPV lower head breach	07:41	07:16
Reactor cavity dry	07:46	07:27
Containment pressure reaches design (45 psig)	25:01	11:00
Containment liner yields	41:05	-*
Containment rebar yields	-*	25:32
End of calculation	48:00	48:00

*Indicates event did not occur