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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

+ + + + +

MONDAY

SEPTEMBER 16, 2013

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ROCKVILLE, MARYLAND

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

COMMITTEE MEMBERS:

JOHN W. STETKAR, Chairman

DENNIS C. BLEY, Member

MICHAEL L. CORRADINI, Member

JOY REMPE, Member

MICHAEL T. RYAN, Member

STEPHEN P. SCHULTZ, Member

WILLIAM J. SHACK, Consultant

NRC STAFF PRESENT:

HOSSEIN NOURBAKHSH, Designated Federal Official

ED FULLER, RES

KATHY GIBSON, RES

TINA GHOSH, RES

PATRICIA SANTIAGO, RES

ALSO PRESENT:

NATHAN BIXLER, SNL

RANDY GAUNTT, SNL

JOE JONES, SNL*

DOUGLAS OSBORNE, SNL*

KYLE ROSS, SNL*

CEDRIC SALISBURY, SNL*

*Present via telephone

A-G-E-N-D-A

P R O C E E D I N G S

8:31 a.m.

CHAIRMAN STETKAR: Meeting will now come to order. This is a meeting of the ACRS Subcommittee on Reactor Policies and Practice - Regulatory Policies and Practices.

I am John Stetkar, chairman of this meeting. Members in attendance are Dennis Bley, Steve Schultz, Mike Ryan, Joy Rempe and Mike Corradini.

Also in attendance is our consultant, Bill Shack. The purpose of this meeting is to discuss draft NUREG/CR-7155, state of the art reactor consequence analyses project uncertainty analysis of the unmitigated long-term station blackout of the Peach Bottom Atomic 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 - it's going to be a long day - 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.

We received no written comments or requests for time to make oral statements from members of the public regarding today's meeting. However, I understand that there may be folks on the bridge line who are listening in on today's proceedings.

I do believe we do have the bridge line open with folks from Sandia. Is that right? I don't know if it's open both ways. If someone's out there can you say something?

MR. JONES: This is Joe with Sandia.

CHAIRMAN STETKAR: Okay. Great. So we know that it's open both ways. Some - we can't tell. We'll now proceed with the meeting and I call upon Patricia Santiago, the Office of Nuclear Regulatory Research, to open the presentations.

MS. SANTIAGO: Thank you. Good morning. My name is Pat Santiago. I'm branch chief of the Accident Analysis Branch. I work for Kathy Gibson, the division director of the Division of Systems Analysis.

I led - our branch led the state of the art reactor consequence analysis project and the associated Peach Bottom uncertainty analysis that we'll discuss today.

In the last year and a half we've briefed the ACRS subcommittee and full committee several times and we've gotten a lot of important feedback from them and the ACRS has been very positive and the feedback has been very helpful.

Most recently in July we briefed the full committee on the Peach Bottom uncertainty analysis results and conclusions and we've provided the draft NUREG as you noted earlier.

Members at that time requested that we return in September to a subcommittee meeting to discuss some specific parameters in more detail as well as we were asked to consider conducting some additional MACCS runs.

We've completed that and the report is still being edited. Dr. Ghosh will present the results and conclusions from these additional MACCS runs today and we will discuss some additional specific details that you've asked of us.

We have asked Dr. Nathan Bixler who is a MACCS expert from our contractors at Sandia National Laboratories as well as Dr. Randy Gauntt to support these discussions.

DR. GAUNTT, I understand, is in the building so we should see him shortly. The other individuals on the phone are from Sandia National Labs and they're Joe Jones, as he announced. We also have Kyle Ross.

DR. GHOSH: We have Douglas Osborne and Cedric Salisbury.

MS. SANTIAGO: Is there anybody else from Sandia on the phone that would like to identify themselves? Thank you. And I'll turn over the presentation to Dr. Ghosh.

DR. GHOSH: Okay. I'm Tina Ghosh. As Pat mentioned we're basically back here - we were here two months ago. We're back to answer some of the questions that we didn't have time to address during the full committee meeting in July.

And also we kind of did a marathon quick, you know, analysis session to squeeze in all the additional MACCS runs that we - at least in our interpretation we had asked for and we did some additional runs also to convince ourselves of certain things.

So we're going to talk today, actually, the first thing this morning about these additional MACCS runs that we did and what the results are, and actually my slide set is for the entire day so the agenda is actually for the whole day.

My understanding is that the plan is to take the morning to discuss the new MACCS results and then in the afternoon we'll go into the MELCOR and the MACCS parameters that were identified as being of further interest to the committee in terms of better understanding what the basis was for the uncertainties that we assigned to them.

Okay. So first - for this first section I first listed what the comment was as we understood it from the committee and then I'll go through what our approach was to adjust the comment and show you the results that support the response.

So the first comment had to do with the fact that in our draft report we followed the convention of the SOARCA project in that we reported the means from all of the aleatory weather trials for our uncertainty results.

So all the distributions that you see in the draft report as it stands right now is basically the distribution that represents the epistemic uncertainty in both the MELCOR and the MACCS parameters.

But we did not separately break out the - showing the distribution of the weather results for particular runs. So the first comment was that for the combined - for the existing combined MELCOR MACCS results we should also include and display the full weather aleatory uncertainty.

And in fact there were other comments we had gotten in the past that other stakeholders had expressed interest in seeing what the combined distribution would be so not just looking at the epistemic uncertainty with the - what we call the aleatory means which is the, you know, the mean result from all the weather trials but actually looking at the full possible range.

So we went ahead and did that because it's fairly straightforward to generate those curves. We basically convoluted all of the complementary cumulative distribution functions, the CCFs that were generated for each of the 865 epistemic runs.

We had one weather CCDF and we convoluted those results with the epistemic to produce a combined result.

So if we go to the next slide, this is just a reminder, and I want to apologize up front. I know the terminology in this slide presentation and the written material we provided you ahead of time can get confusing because we are talking about so many different kinds of uncertainty and we've done different things in different ones.

So I'm going to try to keep repeating what each result that we're showing you represents and -

CHAIRMAN STETKAR: One comment or question because I admit I'm more confused now than I was three months ago.

The title of this slide says "Conditional Mean Individual Latent Cancer Fatality Risk Per Event for Combined Results." This is not the conditional mean. The conditional mean is a value.

DR. GHOSH: Yes, and the fact -

CHAIRMAN STETKAR: This is - and you're consistently wrong through all of the descriptions in the text -

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: - by characterizing it that way.

DR. GHOSH: Right.

CHAIRMAN STETKAR: This is the conditional individual latent cancer fatality risk per event.

DR. GHOSH: Right. So let me clarify. Actually I was about to -

CHAIRMAN STETKAR: Isn't it?

DR. GHOSH: - explain. It is the mean of the aleatory CCDFs. That's why we call it the mean. It's conditional because it's conditioned on -

CHAIRMAN STETKAR: No, no, no. The mean is a mean.

DR. GHOSH: The mean is a mean. Yeah. Right. So in this case -

CHAIRMAN STETKAR: That though is the question.

DR. GHOSH: - the mean of what? This is the -

CHAIRMAN STETKAR: It's not the mean of the conditional latent cancer fatality risk.

DR. GHOSH: Okay. Let me - let me tell you what it is and then we can -

CHAIRMAN STETKAR: Okay.

DR. GHOSH: - say what it should be called.

CHAIRMAN STETKAR: We'll call it A for the moment. What is A?

DR. GHOSH: Let's call it A. What is A? So first we say conditional because it's conditional on the Peach Bottom station's long-term station blackout scenario occurring as we've defined it in the SOARCA project. So that's conditional.

The actual result is the individual latent cancer fatality risk that is calculated by the MACCS code for these given radii. So we looked at zero to 10 through zero to 50. So that's the actual result, the individual latent cancer fatality.

Now, the - before I get to the mean part the - I think the next part is confusing and, again, I apologize for that. We don't need to maybe specify combined results.

This is for the 865 total realizations that we did. So we're showing now the distribution across the 865 epistemic ones that we did.

So the percentiles - the 5th percentile median mean 95th and also the SOARCA base case which is just there for reference but the statistics are for the - across the 865 runs.

CHAIRMAN STETKAR: Just to make sure that I - that I try to get less confused, the SOARCA base case is what was published with the so-called - and I'll call them point estimates -

DR. GHOSH: Yes. Right.

CHAIRMAN STETKAR: - so that doesn't account for uncertainty at all.

DR. GHOSH: That is correct, in the SOARCA project.

CHAIRMAN STETKAR: Okay. So it's not really an uncertainty analysis base case. It was some point values that were run through models, right?

DR. GHOSH: Yeah, and I realize we're

adding additional confusion here with the -

CHAIRMAN STETKAR: I just want to make sure that - no, that's - the answer to that question is yes. Continue.

DR. GHOSH: But the reason we call it the UA base case is that we had to update our models between the Peach Bottom SOARCA point estimate study and our study and that's why it doesn't match exactly what you would see in the SOARCA project's Peach Bottom analysis.

CHAIRMAN STETKAR: It's a set of numbers. But the important thing for the purpose of this meeting is that set of numbers does not account for uncertainties at all.

DR. GHOSH: That is correct.

CHAIRMAN STETKAR: It is simply a set of numbers.

DR. GHOSH: That is correct. Right.

CHAIRMAN STETKAR: So that bottom line there is just a set of numbers.

DR. GHOSH: Right.

CHAIRMAN STETKAR: Could be any set of numbers.

DR. GHOSH: Yeah. Okay. Right.

CHAIRMAN STETKAR: No, honestly, I - you know, we're interested in the - how you treated uncertainties -

DR. GHOSH: Yeah, I - yeah.

CONSULTANT SHACK: But we're also interested in how the uncertainty analysis compares with the point estimate because that's what we deal with most of the time.

CHAIRMAN STETKAR: In some sense we are. But we already know that the uncertainty means were quite different from the so-called best estimate point estimate. So therefore the - that point estimate is in my mind kind of irrelevant.

MEMBER BLEY: Well, except at the last meeting there was a long discussion about the staff having more confidence in that than in the results of their uncertainty analysis.

CHAIRMAN STETKAR: There was that discussion.

MEMBER BLEY: There was that.

CHAIRMAN STETKAR: I just wanted to put that bottom line in some pretty critical perspective because -

DR. GHOSH: So then the final piece of the puzzle is that the 865 results that we have originally, you know, as we got from the code were actually distributions of 984 weather trials.

We had a histogram of results for each of those 865 realizations.

MEMBER CORRADINI: Say that again please. I'm sorry.

DR. GHOSH: For each of the 865 realizations that we did with, you know, one set of epistemic parameter values the MACCS code actually generated a weather trial base CCDF.

So we have 865 distributions of results for each of those epistemic trials.

MEMBER CORRADINI: And - okay, and then if I might ask a question at this point. Can I? Then the 900 and something were sampled from HEINCAS for that site over some time period. In other words, how did you get the 900 and something?

DR. GHOSH: Oh, right. The weather trials. Yeah, the weather trials - right. You - that's typically done which is what we followed is we took an entire year of weather data -

MEMBER CORRADINI: Just picked 900 of them?

DR. GHOSH: No, not quite. We created statistically significant bins that grouped similar weather and -

MEMBER CORRADINI: Independent of chronological time?

DR. GHOSH: Independent of -

MEMBER CORRADINI: In other words, you could have a similar weather pattern on January 13th as a similar pattern on May 20th?

DR. GHOSH: Right. Right. That is correct. And last time we talked about in Chapter 6 of our draft report we also analyzed how the results might have changed if we used the entire year's worth of data and you see very little difference, which gives us confidence that our 984 statistically significant trials are a very good representation, you know, of what you would have done if you used the entire year's worth of weather data which is 8,000 and some points.

But it's very - computationally it becomes very cumbersome to use the entire year of data.

MEMBER CORRADINI: So again a clarification. So if you use 8,766 hours in the day - so you're saying the starting time is what you define as the point.

So it may evolve and with MACCS or all of these things you evolve it out for tens of hours anyway. But the starting time is what you identify as the different point of the accident.

DR. BIXLER: That's right, and part of that identification or part of the process is that the release is changing in time. The weather is changing in time.

Those are matched up hour per hour. So the release starts at hour one and then the release evolves while the weather is evolving.

MEMBER CORRADINI: Okay. So I knew all this. So this approach you just spoke about to get the 900 and something out of the 8,000 and something is a known approach, an accepted approach, an approach every uses?

DR. GHOSH: Yeah. Right.

MEMBER CORRADINI: Okay.

DR. GHOSH: But in order to convince ourselves that it is a good representation of the entire year of data -

MEMBER CORRADINI: I'm looking at this study -

DR. GHOSH: Right. Right. So maybe I should talk - in order to convince ourselves that it was a proper representation we also did a sensitivity study on the entire year of weather data and as I said the results matched very well. So we were confident -

MEMBER CORRADINI: And the year you chose was?

DR. GHOSH: 2005. I have to double check. It's in the report which year -

MEMBER CORRADINI: Okay. That's fine. That's fine. Okay.

DR. GHOSH: - we used. But I believe it was around 2005.

MEMBER CORRADINI: So for every one of the 865 there's really a hidden distribution function that represents the year 2005 -

DR. GHOSH: Yes.

MEMBER CORRADINI: - as best as you know it based on -

DR. GHOSH: That is correct. Yes.

MEMBER CORRADINI: - a procedural - a protocol that everybody likes and agrees with?

DR. GHOSH: Right. Right. That is correct. And so then that - so then from that distribution that's where this word "mean" comes in because we're aren't - in the original set of results as in SOARCA we're not showing you the entire distribution from the weather trials.

We're taking the mean value that's generated from these 984 weather trials. So that's why the word "mean" comes in because we have the CCDF but we're only looking at the mean - the metric of the mean of those and then we are plotting a distribution of those means across the epistemic uncertainty. Okay.

CHAIRMAN STETKAR: That's the wrong way to characterize it but that's okay. I understand what you did.

DR. GHOSH: Okay. Good. Yeah, yeah, yeah.

CHAIRMAN STETKAR: It's -

MEMBER BLEY: Perhaps if you said weather mean or mean over weather -

CHAIRMAN STETKAR: Conditional individual latent cancer - this is the conditional individual latent cancer fatality risk per event for combined results - 865 MELCOR samples using the mean weather. That's what this is.

DR. GHOSH: But it's not the mean weather. We actually ran the model, you know, the 984 times and, you know, statistically weighted it properly and calculated the mean of the results. So it's not the mean weather but it's the mean of -

MEMBER BLEY: Of the weather data.

CHAIRMAN STETKAR: Of the weather data.

DR. GHOSH: Convoluted with the model.

CHAIRMAN STETKAR: The mean of the weather results.

MEMBER BLEY: Mean of the weather results.

CHAIRMAN STETKAR: Not on weather data.

MEMBER BLEY: That's true.

DR. GHOSH: Yeah, yeah. Exactly right.

MEMBER BLEY: Results on calculations.

CHAIRMAN STETKAR: Right. Right. Yeah.

DR. GHOSH: So the comment I think you just made is what I think motivated what you originally asked us to do and we have gone ahead and done that. So I think - I think what you're looking for is on the next slide.

So I believe, you know, the comment was show us the entire curve with the weather uncertainty convoluted with the epistemic uncertainty and so we went ahead and generated that because as I said, you know, the data is there in the MACCS output.

It's just a matter of putting it together. So we have here originally just a couple of the radii. These graphs get very busy when we continue to look at all five radii and, frankly, beyond 10 miles the results are very well correlated with each other.

So for example the 30-mile result, the 40-mile result, the 50-mile result the position of the curves will change but in terms of the spread of the uncertainty they're correlated like 99 percent or something.

So looking at the 50 and 10 kind of give you a good idea of what's happening in the intermediate radii.

MEMBER CORRADINI: So I should look at the green and the red.

DR. GHOSH: Yes. I would say those are - those are the ones that kind of bound the range of, you know, what you're looking at. So and the reason the 10-mile is different is because that's the EPV and we have special protective measures.

You know, you evacuate the EPV so the folks who are getting dosed in the early phase are just that .5 percent of the population who is assumed not to evacuate and that's the only contribution from the early phase.

Whereas beyond 10 miles you'd start to get contributions not just from the long-term phase but also the early phase from the plume passage. And the long-term phase you have a hard backstop that's provided by the habitability criterion because people are not allowed to return home until the habitability criterion - the return criterion is met.

So that's why qualitatively you'll see a difference in the shape of the 10-mile curve versus anything that's beyond 10 miles.

MEMBER CORRADINI: And the habitability criteria is what?

DR. GHOSH: For - we used the Pennsylvania state guideline, which is 500 millirems per year starting the first year.

The EPA guidelines which may be changing but at least right now is two rem in the first year followed by 500 millirem per year.

MEMBER CORRADINI: But I thought there was another criteria which I guess you didn't use but at least in my memory is there that you can't get more than five rem over 50 years.

Isn't there - isn't there - that's not a habitability criteria. That's another measure. Am I remembering well?

DR. GHOSH: That may be another measure. I'm not familiar with that additional metric.

DR. BIXLER: It certainly wasn't used in this analysis.

MEMBER CORRADINI: I didn't think it was. I just wanted to make sure there is - I'm sorry?

MS. SANTIAGO: I think that's in part 20. I will look -

MEMBER CORRADINI: That's what - I know it was somewhere in regulations.

DR. GHOSH: You know, the -

MEMBER CORRADINI: I'm learning.

MS. SANTIAGO: But it's not emergency preparedness or -

MEMBER CORRADINI: Don't take these questions as criticisms. I was just trying to -

MS. SANTIAGO: - habitability, yeah.

MEMBER CORRADINI: That's fine. Okay. Thank you.

DR. GHOSH: And I think we go through this in the report and also the main SOARCA study. I think if a accident were actually to occur it would be up to the states to set the final criteria.

But for the purposes of this study we just went with what the guidelines are, you know, the expectation and then what would actually happen is, you know, hard to know - to predict.

So the green line is the zero to 10-mile curves and the red lines are the zero to 50-mile curves. The solid lines are what we have in the report.

So those are looking at just the - showing you the distribution of epistemic uncertainty taking the mean values from all of the aleatory weather trials.

And then the new curves are the dashed line and as expected you can see that there is a greater spread once you convolute the aleatory and the epistemic uncertainty together.

And one thing we've known for a long time and which is one of the reasons we're very comfortable using the mean as the metric for - if you have to use, you know, a single value the mean is pretty high up on the distribution typically.

I mean, I think in past analyses when we've looked at where does the mean fall with respect to the weather distribution it's, you know, anywhere from the 60th to the 80th percentile. So it's well above the median.

So in terms of using a metric, you know, it's a higher consequence value typically than even the median and, you know, we think it's not a bad representation for the curve.

But again, for those who are interested I mean it's understandable. Some people want to see what the full spread of results would be and you can see that the - once you convolute the weather uncertainty it can go - on the lower end it kind of spreads out more so than on the higher end.

So we generated these results and I think we're going to add them to the report somewhere because the question has come up and, you know, it's not a bad thing to have. So if there are no questions about that I'll move to the next one.

CHAIRMAN STETKAR: Tina, do you have any idea how those might - you know, you sampled 1.0 years worth of weather data which gives you, I think, a pretty decent picture of the uncertainty for that particular one-year snapshot of weather.

Do you have any sense of how that might change if you sampled 10 years or 50 years or 100 years worth of weather data, recognizing that you're not going to take 100 years times 8,766 starting points but if you broadened those samples and said based on, pick a number - 30 years is usually pretty easy to gather -

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: - out in the extremes.

DR. GHOSH: Right. We have looked at this issue before and especially and in fact some of our applicants have looked at this in the area of SAMA analyses for license renewal and what we've typically found in the past and across a variety of sites is that as long as you take an entire year of weather data it doesn't matter all that much which year you choose or even if you looked at multiple years. I mean, the -

CHAIRMAN STETKAR: Folks in Boulder, Colorado would probably differ with you right now.

DR. GHOSH: Are there any plans - but yeah. I mean, there may be some - you know, it's true there may be some idiosyncratic things about particular sites but in general when we've seen an evaluation of multiple years of weather data you don't get a huge variation. I'd say -

MEMBER BLEY: I've heard that before. The thing I haven't known is when people have tried that have they taken an effort to pick an unusually dry year or an unusually wet year, that sort of thing, or did they just pick, you know, three years instead of two, instead of one?

DR. GHOSH: Yeah. Not so - not so much. You know, I think -

MEMBER BLEY: But I think that's where the questions are coming from. Aren't there - sometimes when the weather is much more extreme than usual it could have a big impact. I mean, it could go either way -

DR. GHOSH: Right.

MEMBER BLEY: - on the impact but -

DR. GHOSH: My gut feeling on that one because I think typically the NRC guidances look at five years worth of data and what we typically see is that whenever you're doing the analysis you look at the last five years.

It's not necessarily a search for extreme years. But there is so much smeared out of results because the modeling project is so complex that you have so many different interactions that it's hard to see how, you know, let's say a 100-year, you know, strange weather pattern might truly influence the results significantly.

I mean, I think you can move - you can possibly move the needle a little bit but when you then try to think about what - how it would change your entire distribution if you have to then weight the fact that that type of weather only occurs every hundred years and so you multiply - I mean, this is a very crude kind of thought process but if you multiply that by 1 percent chance of happening and you add that in with all the other uncertainties I mean, things start to get smeared out pretty quickly and I think -

MEMBER BLEY: I can sort of buy that. Have you looked or has anybody looked - just in extreme case of looking at what happens if you use the weather on the East coast on the same input function as if you did it for a plant out in the desert?

Is there much difference in the consequence results when you do that?

DR. GHOSH: Okay. So I can tell you that informally we've started looking at things like this because we're starting to think about these things more as we're trying to support, you know, rule making and other regulatory actions with level three type information.

We don't have anything published yet but we have started looking at that kind of thing and Nate, you can correct me if I'm wrong.

I'm not sure how much we've talked to you about all this but preliminarily it seems that the site characteristics in terms of population distribution and property values and things like that have a bigger impact than the weather at a particular site, and we've just started doing more targeted calculations to really understand the site to site variability across the country.

I can't give you a definitive answer but it seems that the site specific population distribution and economic, you know, property values and so on have a much bigger impact than the weather in terms of -

DR. BIXLER: Let me - let me add just a little bit to that answer. I think you would find a bigger variability if you considered a site on the East coast, say, like Peach Bottom and a site somewhere in Arizona where the weather is drier, sunnier. You get more unstable weather.

That difference would be much bigger, I believe. I've never done a real careful comparison of this but I believe that difference would be much bigger than the year to year variation at a specific site.

The year to year variation tends to keep you in kind of a plus or minus 10 percent range where people have looked at sequential five years like Tina mentioned a minute ago.

That variation is not large. But if you go from one very different site, maybe a rainy site to a dry one, one where it's - there are lots of clouds to one with lots of sunshine like in the example that I just gave between Peach Bottom and Arizona that can make a substantial difference in the answers that you get.

So I would be - I think that the difference in site is much greater than annual variations in weather even considering that some years you have maybe twice as much rainfall as other years and that kind of thing.

The thing with rainfall is typically at very few places at least in the continental U.S. you have rainfall more than 10 percent of the hours of the year. So it's not having a huge impact on the overall distribution.

DR. GHOSH: Okay. So on slide six is the next comment or maybe a suggestion I should call it, which was to select the MELCOR realization that produced the largest conditional prompt fatality consequence in our existing SOARCA uncertainty results and for that realization I do a sort of a separate, you know, MACCS uncertainty only study.

So look at varying all of the epistemic parameters in MACCS for that one source term that produced the largest conditional prompt fatality consequence.

And then demonstrate convergence of the results in terms of combining the weather uncertainty with the epistemic uncertainty for that source term in terms of prompt fatality.

So we went ahead and did that and we can go to the next slide but I want to point out one thing.

At the very last page of your slide package is a crosswalk table of all of the new MACCS runs we did because it gets very confusing, and that one shows you the run numbers and what each run number is.

And because we wanted to confuse everybody even further, if you look in the left most column you'll see that the file names end in CAP something, C-A-P something.

So for example CAP17 is our original uncertainty analysis run which we have now renumbered Run 1 in attempt to be a little bit more clear but we've added a lot of terminology.

So as we go through the presentation, I mean, of course interrupt me if you're not sure what we're presenting at a given moment but also that table in the back is the crosswalk of what each run number represents and also what the associated CAP number is because I think in some of the graphs even in this presentation and the write up you might also see the CAP numbers. So those are all in that last table.

MEMBER CORRADINI: So before we go to this one I really am challenged about - before we go to this one, your conclusion from the previous one was that because the - let me try to resay what you said - that the difference in the percentile for the center of all this in terms of the CCDF are not that different.

Adding in the aleatory uncertainty does not change things other than expected that the spread gets bigger - other than that, the changes are not significant in your view. Do I have that right?

DR. GHOSH: Yeah. I mean, I would say yes. The influence of the epistemic uncertainty is greater than the influence of the aleatory uncertainty.

In chapter six we have a - we did a little sensitivity study to look at, you know, the spread of particular results in terms for weather.

If you look at those spreads, I mean, in general the epistemic spread is larger than, you know, the weather spread that you get from a particular -

MEMBER CORRADINI: So if I could just ask the question. So how would the dashed line have to look before you start getting worried?

DR. GHOSH: Before you get - oh.

MEMBER CORRADINI: In other words, I understand your judgment. Since I have no clue personally how I would determine a judgment how would you - when would you start getting nervous?

DR. GHOSH: So as I mentioned, I think - okay, this is theoretical but because - I mean, one could question, you know, what the decision maker should really rely on in terms of a metric and, you know, why we're looking at this information.

But I think if we - if we produce these curves and the tail of the - the tail on the right went out to consequence levels that were orders of magnitude greater than what we had seen with the mean we would - we would be worried.

MEMBER CORRADINI: Okay.

DR. GHOSH: But we don't see that and as I mentioned for particular weather CCDFs the means are higher than the median. They're something like 70th percentile or greater.

So we're already capturing kind of a higher consequence in terms of the weather distribution. So if you have to pick a number, you know, we think it's not a bad thing to be looking at the means to characterize -

MEMBER CORRADINI: Okay.

MEMBER BLEY: I mean, the mean is always bigger than the median.

DR. GHOSH: For nuclear - yeah, for nuclear -

MEMBER BLEY: Any distribution is -

DR. GHOSH: If you had a perfect reading, yeah.

MEMBER BLEY: Yeah. That's referred to or greater -

DR. GHOSH: Yeah. Right. Right. And certainly we have - we tend to see log normal distribution of results. So the means are quite a bit greater than the median.

MEMBER BLEY: I think from Mike's question -

DR. GHOSH: Yeah.

MEMBER BLEY: - I often get more information looking at the density function. But if the right hand -

MEMBER CORRADINI: Thank you because I was looking for that and I kept on looking at these things.

MEMBER BLEY: If the right hand side - the differences you see on the left hand side with the dashed lines being spread well to the left if we were seeing the dashed lines spread to the right substantially at the high end - at the high end then you'd really start to see it.

MEMBER CORRADINI: Well, I was guessing that's what - but I'm just looking for -

MEMBER BLEY: From about the mean up they're pretty close.

MEMBER CORRADINI: Okay. Thank you.

DR. GHOSH: Okay. So then on slide seven we identified the source term that produced the largest conditional prompt fatality risk consequence and we just tell you what source term that happens to be, and then for that source term we did three Monte Carlo runs where we took a sample size of 1,000.

So basically three replicates of an identical analysis using a different Latin hypercube seeds. So we used Latin hypercube sampling, generated 1,000 realizations and did that three times in order to be able to compare, you know, what the difference is when doing - use a different seed.

CHAIRMAN STETKAR: Tina, I'm not - I've run a lot of Monte Carlo stuff. Did you - 1,000 samples from 350 input values of moderately broad uncertainties tend not to generally give you enough samples to reach convergence or reasonable convergence in the mean. Did you examine the convergence of your sampling?

DR. GHOSH: We did and I think we're going to get in - more into that. So remember the bootstrapping we did for the MELCOR results?

I think you thought that was a good approach and we had done that for the MELCOR results because we used simple random sampling when we could.

When I get towards the end of this portion of the presentation we'll show you. We went ahead and did the same thing for the MACCS results to convince hopefully you and ourselves that we had -

CHAIRMAN STETKAR: I mean, I didn't understand that at all so you're going to have to walk me through that. I was just trying to ask you a simple question about did you look at convergence.

DR. GHOSH: Yes. Yes, we did.

CHAIRMAN STETKAR: Okay, and -

DR. GHOSH: So the first way we looked at convergence was to do these three replicates using Latin hypercube sampling and see how well do they match each other and then the additional way was to use the bootstrap.

So we did a second set of sampling with simple random sampling and then we used the bootstrapping similar to what we did for the MELCOR results but this time for the MACCS results so that you could see how the MACCS results by themselves converge and also how the combined results converge. So that's going to be towards the end of this portion of the presentation.

CHAIRMAN STETKAR: Okay.

DR. GHOSH: And just a note that we - for all of the new ones we used the same 984 weather trials because, again, those are the statistically significant bins and we had already convinced ourselves that they're a very good representation of the entire year's worth of weather data. So we kept using those same weather trials.

So the next slide now shows the - this is in tabular form before we show you some graphs of what the results were and -

CHAIRMAN STETKAR: This is - just to again orient me -

DR. GHOSH: Oh, sorry.

CHAIRMAN STETKAR: - this is in the report. This is in the NUREG?

DR. GHOSH: That's right.

CHAIRMAN STETKAR: Okay.

DR. GHOSH: So slide eight first just for reference is what was the original results or what we're calling Run 1.

That's the uncertainty analysis. And, you know, this is just a - you know, as we recall the - there are very few trials. The terminology gets confusing.

There were very few realizations that - the seven realizations where we calculated a non-zero number. So all of the statistics are really driven by the tails and you can see that once you get out past two and a half miles there's less than 5 percent of the results that are driving any kind of calculation.

So there's only 13 percent of the realization's total that calculated a non-zero number and once you get out in further radial distances that number shrinks drastically.

So slide eight was the -

MEMBER CORRADINI: Run one is unmitigated.

DR. GHOSH: Yeah. Actually everything we did is unmitigated.

MEMBER CORRADINI: That's what I remembered. I just wanted to -

DR. GHOSH: Yeah, that's right. It's the

unmitigated long-term station blackout. That's right. So slide nine now is the new results so what we called Runs 3 to 5.

That's the highest prompt fatality source term from our existing set in the uncertainty analysis. And then it's the - what we're showing you is how the statistics of the mean, median 75th to 95th compare to each other with respect to these five radial distances and the three replicates of the Latin hypercube sampling runs we did.

And I have to apologize. I believe there is an error on this slide that we need to - we need to fix.

I think after the break we can confirm what the numbers should be. For the median, the Runs 4 to 5 I think are a copy and paste error because the - if you look at the curves it shouldn't be the 3.3 and 3.2. I think it's something closer to four and I can look up what those original numbers were.

But if we look at some of the other metrics you can see, for example, the means and the zero to 1.2 mile they match each other fairly well. Also, the zero to two and a half mile, the zero to three and a half mile.

Given that there is so little data even for the largest source term in terms of the prompt fatality risk they match reasonably well but, you know, we're struggling here.

For the prompt fatality risk, you know, they're small numbers and they're driven by the tails of the distribution. So even for this very large source term you don't have a lot of data to produce these statistics.

I think given that setup it's not a bad match in terms of how the three replicates compare to each other.

The one other thing I'll point out so you can see that once you get out to about zero to seven miles, you know, even for this largest source term from the original set you have less than half non-zero results and even for those the non-zero results that are calculated I think the majority of the weather trials give you a zero result.

So you're kind of averaging, you know, in some cases the tail in some cases, you know, for the largest source for maybe 25, 30 percent of weather trials for, you know, the realizations that give you non-zeros.

So what I'm trying to say is you have a whole bunch of zeroes that you have to basically account for then when you start to make statistics and for this reason our MACCS post processor kind of breaks down when you try to look at the combination of the aleatory and the epistemic uncertainty because there are just a tremendous number of zeroes that the post processors have to account for.

So for the - all of the latent cancer fatality risk results we'll talk about later. We did do the convolution of the aleatory weather uncertainty and the epistemic uncertainty. But we had difficulty producing those results for the prompt fatality risk on the order of two months and we were trying to do a lot of additional analyses.

So just for the prompt fatality risk we continue to report just the means from the aleatory weather trials because our post processor couldn't quite, you know, make sense of the tremendous number of zeroes in the actual results that it had to convolute.

So what you see here is, again, the aleatory means and if we go to the next slide, slide ten, these are the - again, the mean results if the CCDF that's generated from the - from all of the epistemic parameter uncertainty.

So this first one is for the zero to 1.3 miles. So all of the dashed lines - the dash of, you know, different sizes there's a dotted line you could see in the different dash lines.

Those are all from the new runs for the largest source term and then Run 1 just for reference is the solid red line. So you can kind of see where the results lie with respect to Run 1.

So, of course, as expected we're looking at, you know, we're looking at a very high source term in terms of prompt fatality risk.

So as expected, you know, the curve shifts a bit to the right but it's not, you know, outrageously off the map with respect to the original distribution of results we had and in terms of how the three curves match each other they match reasonably well, you know, given, again, the sparse data.

And if we go to slide -

CHAIRMAN STETKAR: See, now don't go yet. On all of these curves - and you're going to have to educate me here - why do these curves asymptotically approach values that are not 1.0 since these are CCDFs?

This is a conditional probability and I don't understand why they are not one and some of them are quite a bit less than one. So I'm curious about that.

DR. BIXLER: It's because of the large number of zeroes.

CHAIRMAN STETKAR: It's okay. I can - I can have 99 percent probability of being zero which you could - which you could arbitrarily assign to be 10 to the minus 11 or 10 to the minus 200 or whatever.

DR. BIXLER: Okay.

CHAIRMAN STETKAR: And I would eventually get to one.

DR. BIXLER: Okay. We didn't try to do that. Since it's a log scale we didn't show any zeroes on the plots.

CHAIRMAN STETKAR: So then I don't understand what these probability distributions mean in terms of real world engineering.

MEMBER CORRADINI: Can I ask you a question differently since I'm - so you're bothered by it doesn't go to one but that would mean the slope can't be zero on the left.

It's got to be imperceptibly negative in this crazy definition so that if I make it small enough I eventually get to one. Did anybody check that or is there something about assumptions of what you cut off?

CHAIRMAN STETKAR: Usually what people - well, I don't want to say usually what people do - see what they did. It ought to go to one someplace.

DR. GHOSH: Are you asking why the - on the CCDF doesn't start at the number one?

CHAIRMAN STETKAR: The cumulative

probability must eventually sum to one.

DR. GHOSH: Right.

CHAIRMAN STETKAR: Someplace.

DR. GHOSH: Yes. We - so for the zero to 1.3 mile - you know, the - I guess the long - the biggest or the smallest number that we calculated is something on the order of 10 to the minus eleven.

And by the way, these are the conditional risk numbers so we have to multiply that times three times 10 to the minus six. We're looking at something on the order of 10 to the minus 17.

CHAIRMAN STETKAR: Don't confuse - don't confuse the record and the members with throwing numbers around. The simple question is why do these cumulative probability distribution functions not go to one. That's a simple question.

DR. GHOSH: Right, and I'm sorry. I am trying to explain. So the - let's see - so it's .1. So if you go up to where we do end so .2, .3, .4, .5, .6, something on the order of 62 percent of the calculated results even for the worst source term in terms of original results we calculated a non-zero number.

In 38 percent of the cases it was zero and we could - we could have artificially put a zero on the X axis but we plotted this on a log log scale so it's hard to do.

I mean, if we had a - if we could somehow represent a zero on the log scale -

CHAIRMAN STETKAR: Dennis mentioned that it's easier for people to understand uncertainties as you look at the probability density function rather than these sort of plots.

DR. GHOSH: Yeah. Right.

CHAIRMAN STETKAR: Because you can represent zeroes on a probability density function quite easily.

DR. GHOSH: Right. Right. So if you looked at the histogram, and that's a good point. If you looked at the histogram you'd have 38 percent of the PDF being zero and then the other 62 percent would be spread out.

So basically what we're missing from this graph is that 38 or so percent of zeroes. That is a struggle to, you know, put on the - on the graph.

So the next curve is the three and a half mile result and, again, the solid line is the result from our original run and then the dashed lines are the new runs and I would say they match reasonably well, again, with each other.

There's even less data when you go out to three and a half miles about - it looks like two, three, four - there's 40 some percent of calculated non-zero results. And in this case, you know, at the very tail end one of the results are maybe about a factor of two higher than the others.

But, again, there's very little data to kind of populate these distributions in the first place. So it's not terrible.

CHAIRMAN STETKAR: How do you distinguish between zero and 10 to the minus 12?

DR. GHOSH: How do you - well, because you have such a powerful computer. You know, that's what I'm saying. When I say non-zero it's what the computer has decided to calculate -

CHAIRMAN STETKAR: No, no, no, no. How do you as an engineer distinguish between zero and 10 to the minus 12?

DR. GHOSH: I think we can pick - you know, we can pick whatever number that we want that is meaningful to somebody. I don't know. What's a meaningful number to you?

CHAIRMAN STETKAR: Well, no. My question is I'm trying to probe this notion of zero and what these curves mean.

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: Because it's not clear to me whether the resolution in your tool by the calculator that runs out only shows me six significant figures. I can't tell the difference after that six significant figure. It's lost in the noise.

I'm trying to understand whether your really, really powerful computers with your really, really sophisticated routines can distinguish between 10 to the minus 12 and zero. And if you can't, then I don't understand what these curves mean.

DR. BIXLER: In principle, they can - and I'm not sure precisely how to answer your question because there certainly is numerical roundoff that's occurring here.

But on the other hand, there is a threshold dose for these prompt fatalities that is part of the model itself and it could be that we're getting below that threshold in all cases at all locations. And so it really is - the number that you get out of it is precisely zero. I suspect that's the case. I'm just not sure.

CHAIRMAN STETKAR: Okay.

DR. GHOSH: And, you know, the struggle with all of the prompt fatality risks is that we are - again, we're talking about minuscule absolute risks because if you take the frequency of the scenario and then you multiply by these numbers even at the highest end, I mean, this is - well, I know we're not supposed to use the term but well below regulatory interest -

CHAIRMAN STETKAR: That's - Tina, and I'm going to always catch you on this on the record - this is not a risk assessment.

We are not interested in the purposes of this meeting with absolute risk. And it's not responsible to compare the absolute risk from one and only one sequence through an event model with the safety goals that apply to the entire risk from operation of the facility under all operating modes, under all sources of initiating events and under all sources - you know, all sources accident sequences. So you cannot make those comparisons.

DR. GHOSH: Yes, and -

CHAIRMAN STETKAR: And they are not legitimate and that's on the record. Now, so go back to this discussion.

DR. GHOSH: Yes. Fair enough.

CHAIRMAN STETKAR: But we're interested in -

MEMBER BLEY: Before you do, let me - let me offer something because I think we've gotten a bit tied around an axle here.

This is the individual risk and I think Nathan hit it right on the nose. There is some probability that there would be a -

CHAIRMAN STETKAR: A delta function would be - a delta function at zero because of physics.

MEMBER BLEY: When you immediately pass zero it drops to something smaller.

CHAIRMAN STETKAR: Something smaller. Right.

MEMBER BLEY: Not small. I mean, it's .5 here chance that you die from it - from the leaks. So it's reasonably high but it's not zero. So that thing is never going to go except at the delta function -

CHAIRMAN STETKAR: Right. At the delta -

MEMBER BLEY: - which you can't see on the low point.

MEMBER CORRADINI: You guys are - I'm still worried about the uncertainty of the input source term. So let's move on.

CHAIRMAN STETKAR: That's okay. That's why the PDF will be somewhat more useful to see that.

MEMBER BLEY: Well, you'd see a spike and then a density curve. But okay.

DR. GHOSH: Okay. So I will move on to the next question which had to do with the latent cancer fatality risk and here certainly we do calculate - there's a lot more data to plot with the distribution and the suggestion was to select the MELCOR realization that produced the largest conditional latent cancer fatality consequence and for that realization once again sample from just the MACCS on uncertain inputs.

So keeping the source term fixed and looking at the MACCS uncertainty by itself and with the 984 weather trials and then demonstrate convergence of those combined results - weather plus epistemic uncertainties. We can go to the next slide.

So we went ahead and we did that. So for slide 13 we identified, you know, what was the source term and wrote the existing study that produced the largest conditional latent cancer fatality risk, and for that source term we once again did three replicates of sample size 1,000 using Latin hybercube sampling. So we used three different seeds for the 350 uncertain MACCS inputs.

So if we go so slide 14 now you'll see the results there and, again, we have a lot more data to populate the distributions in this case and the three replicates pretty much lie right on top of each other.

We - it's very hard to distinguish, you know, among the curves but the steeper curve is the 10-mile curve and the one that shows up as purple that's the - is that right? Yeah. The zero to 50-mile curve and the three replicates match each other very well.

And I think in part this answers the question of whether, you know, 865 samples or 1,000 samples was good enough given that we have 350 uncertain parameters.

This - one's confidence, you know, to some degree when you have the replicates so close together that it does seem that a 1,000 - a sample size of 1,000 was adequate for our purposes.

So if we go now to the next slide, this is just the epistemic uncertainty with the aleatory means just for comparison purposes with regard to the full set of results that we had in the original study.

So Run 1 in the solid red line for the zero to 10-mile radius and the solid black line for the zero to 50 was our original set of results and then the three replicates for the worst source term for latent cancer fatality risk are now plotted.

The distribution of those are plotted in dashed lines. And here I can see that the presentation as projected is you can't - they all look like solid lines unfortunately but in the printouts that you have in front of you it's much more clear.

MEMBER BLEY: Well, in the printout I have and what you sent me - I was looking at the ones you brought today - I can't quite tell. You've got several red ones grouped together and then you have one all by itself. Is that the solid line?

DR. GHOSH: Right. So the one that's by itself -

MEMBER BLEY: That's Run 1?

DR. GHOSH: Yeah. The one that's by itself is Run 1 and then the three that are grouped together, again, they should be dashed.

MEMBER BLEY: And then the same for the black ones?

DR. GHOSH: Yes. Right.

MEMBER BLEY: The one on the left is -

DR. GHOSH: Yeah.

CONSULTANT SHACK: After you did the last graph if you just plotted Run 6 on here and it would have been a lot clearer.

DR. GHOSH: Right. Right. You're right. The curves to the right are the ones for the worst source term, the new runs, and the curves to the left are the original run.

CHAIRMAN STETKAR: I found it useful to look at - if any of the subcommittee members have Figure 10 in the material that you sent us which does plot Run

6 and Run 1.

The dashed and the solids are reversed from this presentation but it's a lot cleaner. And I wanted to ask you about that. It's a little more difficult to see from this so I'm going to stare at Figure 10.

DR. GHOSH: Do you want me to pull it up?

CHAIRMAN STETKAR: Yeah, if you can actually. That would be I think a little more useful than this because it's - it at least cuts down the number of different curves.

CONSULTANT SHACK: Then it combines epistemic and aleatory for Run 6 and epistemic only for Run 1 but -

CHAIRMAN STETKAR: But Run 1 is the - Run 1 is the results in the SOARCA NUREG.

DR. GHOSH: Yeah, that is right. Actually, that is a difference. The Figure 10 we have the combined result for run six and the epistemic mean result for - sorry. The epistemic uncertainty with the mean aleatory result for Run 1.

CHAIRMAN STETKAR: Run 1 is what's in NUREG 7155.

DR. GHOSH: Yes. Yeah, that's right.

CHAIRMAN STETKAR: The dashed curves on this - in this -

DR. GHOSH: That's right.

CHAIRMAN STETKAR: And the solid curves is what you - what you ran in Run 6?

DR. GHOSH: Right. That's right.

CHAIRMAN STETKAR: Now, if I look at this, if I stand way back, to me the uncertainty is less - lower in Run 1 compared to Run 6, right? Run 6 curves are much -

CONSULTANT SHACK: But you've added aleatory uncertainty to the Run 6. Run one -

CHAIRMAN STETKAR: Run 6 though is the conditional latent cancer fatality from one and only one replicate. It does not account for any of the MELCOR uncertainty, right?

Run one accounts for all of the MELCOR uncertainty and in principle, however they did it, the epistemic and aleatory uncertainty sort of kind of -

CONSULTANT SHACK: No, not aleatory, epistemic only.

CHAIRMAN STETKAR: The aleatory as it's represented through their mean - through its mean. The point that I'm trying to get is that I don't understand - as I started to stare at this stuff I don't quite understand how all of this is working because it's generally been my experience that you increase uncertainty as you go through the process and this at least - and that's what we're trying to explore a little bit by these little exercises to see what is the incremental uncertainty from each step of the process and I have not been able to understand that quite well, and especially because this seems - and I might be wrong so, you know, help me if I'm understanding this wrong - this seems to tell me that the uncertainty in the combined aleatory and epistemic - it's called weather in MACCS - for a single replicate is broader than the overall uncertainty that you're publishing in the NUREG.

DR. GHOSH: Yeah. Okay. So let me - a couple of -

CHAIRMAN STETKAR: Is that - first of all, is that a correct interpretation? If it's not -

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: - just help me out.

DR. GHOSH: Okay. So but first let me clarify one thing. I think when you're - if you compare the Run 6 zero to 10-mile it is a smaller spread than the Run 1 result.

It's more, you know, kind of straight up and down and, you know, I think we talked before because you evacuate the EPZ there's less - there are less contributors to the uncertainty in the zero to 10-mile risk.

And as expected, if you have a higher source term you're just shifting that zero to 10-mile curve and also in this case as -

CHAIRMAN STETKAR: If you look at the - let me try this - if you look at the full distribution, the 95 over five through zero to 10 is 22.4 and for Run 6 it's 9.6.

DR. GHOSH: Right. So there are two things with this graph, you know, the -

CONSULTANT SHACK: For the zero to ten?

CHAIRMAN STETKAR: For the zero to ten. It smooths out more as you go to the zero to 50.

DR. GHOSH: Yeah. That's right.

CONSULTANT SHACK: Okay.

DR. GHOSH: Okay. So then for the zero to 50 now what Dr. Shack was mentioning the little - the difficulty in this comparison is that the - we've put in the aleatory uncertainty in for the high source term curve which is Run 6 and we're comparing it back to the curve that was generated for just the aleatory means.

And then if you think back when we had the curve of the combined aleatory and epistemic, which is on slide five, for the original Run 1 results you would follow that - the curve, especially on the left side of where it crosses spreads out quite a bit, you know, to the - to the left.

MEMBER SCHULTZ: So, Tina, it seems as if perhaps we've - in our comment and direction it was select the MELCOR realization that produced the largest condition of fatality consequence. In doing that and then coming back today and comparing this new information with the old information, which was not selecting the largest conditional failure case, have we confused the issue, you know, in trying to perform this comparison?

By choosing the largest case it would appear that that has its own set of results and when we try to compare it to what we were looking at previously if we try to make that comparison and line them up side by side and draw conclusions from them we've got one additional parameter which is we were working with a particular set. Now we're saying pick the maximum and go from there.

DR. GHOSH: But I think we complicated matters further by the set of results we put in the Word writeup and I have to apologize. As I said, we had - we had two months to do all of the work, interpret all the results and provide you some writing.

I mean, if we had to do it again we probably wouldn't have provided the comparisons because it is a little bit the apples to oranges comparison.

I think the slide 15 is a better comparison because at least you're comparing apples to apples. So in slide 15 we're looking at - and just pretend those three curves on the right are one curve. I realize that it doesn't - it probably wasn't that helpful to plot all three since they're right on top of each other.

But here now if we compare the original set of results which is the broader curve to the left with the high source term results which are - the spread is smaller.

Those are the curves to the right - if we compare the red 10-mile results as well as the black 50-mile results the original results are spread out more when you do the apples to apples comparison.

So in this - so in this case we're showing the aleatory means and the full epistemic spread across the -

MEMBER BLEY: Actually the black ones look pretty darn similar. They cover two orders of magnitude. That's essentially -

DR. GHOSH: Yeah, and I think -

MEMBER BLEY: And the mean of the right hand one, the Run 6 - well, the uncertainty runs is quite a bit higher. It looks - eyeballing it it looks like a factor of four or five.

CHAIRMAN STETKAR: But it should be because that's specifically for the replicate. It would force - the replicate forced the curves on the right to the right. So I don't care about the mean - you know, I didn't pay any attention to the mean. I cared about 5th to 95th and spread.

MEMBER BLEY: And they're pretty similar.

CHAIRMAN STETKAR: And those are - those are very similar.

DR. GHOSH: Yeah. Now, for the - for the zero to 50-mile results which are the black curves, I mean, we have to think about okay, so what are we looking at in terms of our other difference in effects.

We know this is a very high source term so we're somewhere out on the tail of, you know, the original - the black curve and it's a complex system. I mean, there are a lot of counteracting effects. But so what are we looking at?

For the second curve, the one on the right, we're looking at what is the effect of the MACCS uncertainty when you consider a source term that's way out on the tail of the original MELCOR distribution.

And so in terms of the, you know, comparing the full range of original uncertainty to this uncertainty the full range - the black curve on the left is showing you, you know, you're taking a variety of source terms, everything from low, medium, high and then combining them with the MACCS, you know, parameter values that are spread over the whole thing versus you have a very high source term and then what does the spread in the MACCS results give you.

And yeah, I think it's not a bad - just eyeballing it it seems to be about a similar spread and I think what it tells us is that yeah, you have a similar spread.

If you take a - if you fix the source term you can get a similar spread from just the consequence, you know, only uncertainty as, you know, when you're looking at the whole thing and you're kind of throwing it in -

CHAIRMAN STETKAR: That's what - that's what started to confuse me, Tina, as I kind of stood back from all of this and I did some of the similar things that Bill did. I compared 95ths to 5ths from various places, and there weren't, you know, to two significant figures there were differences.

But they weren't large differences. The 95th to the 5th ratios were - I used error factors so I'll take the square root of them - but they were on the order of anywhere from three to about five, okay, which are modest uncertainties in the world of uncertainty analysis - quite modest uncertainties, and they were about the same everywhere.

In other words, the uncertainty in - I'll call it the left hand curves on this slide that's reported in the NUREG tended to be on the order of about three to five.

DR. GHOSH: Yeah. Yeah.

CHAIRMAN STETKAR: And the uncertainty now if we pick a particular replicate and ostensibly look at the uncertainty in the MACCS results for that particular replicate is also on the order of about three to five.

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: The right hand curves here.

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: I don't know what to make of that. I did - have you thought about it much? Have you - I mean, to me it -

DR. GHOSH: We have.

CHAIRMAN STETKAR: - it's just curious.

DR. GHOSH: We have. We've thought about it a lot over the last two years and I think, you know, one of the things is that you do see a dampening effect when you come through the source terms to the consequence result because of this hard backstop of the habitability criterion.

So we know that you're never going to be able to dose people beyond the habitability criterion in the long-term phase.

And then I think we're - for these larger source terms where the uncertainty received - where it comes down to is how much early phase the people are - early phase dose the people are getting beyond the 10-mile range.

So we're playing with a fraction - so for the larger source terms we're playing with only a fraction of the total dose that can be incurred and that long-term dose is pretty much fixed at, you know, the maximum you can get is 500 millirem per year from, you know, for once you get past the early phase.

So I think because of this feature of the protective actions it produces the nature of the results that we see.

DR. BIXLER: Maybe another way to put that is that if you think about, say, the cesium release that you get from MELCOR and the spread that you get in that, if you run that set of source terms through MACCS you'll get a compression - a sub linear type of effect on the outcome for MACCS because the biggest source terms get proportionately more action - remedial action to compensate for the size of the source term as opposed to the small source terms.

So what starts out as a big distribution for MELCOR gets compressed quite a lot as you filter it through MACCS and then now we're adding in some other uncertainties that somewhat spread that back out again, the aleatory weather uncertainty or the other epistemic uncertainties that we considered.

But overall it doesn't - it doesn't end up being as broad as you might think because of the compression due to the types of remedial actions that are considered.

DR. GHOSH: Right. So that's the - you know, it's - yeah, so it's a complex system and you have dampening and spreading out effects and, you know, but we think we understand and it makes sense. We don't think we've gotten erroneous results.

MEMBER CORRADINI: And that discussion you just went over is somewhere in your report?

DR. BIXLER: No.

DR. GHOSH: You know, I -

MEMBER CORRADINI: Because I'm hearing what you're saying and my interpretation is you think X and Y but you haven't investigated unfolding X and Y to prove what you think.

CHAIRMAN STETKAR: I sort of - when I read the NUREG I think there might be a paragraph that sort of alludes to that somewhere but it certainly isn't -

CONSULTANT SHACK: They did the calculation. They couldn't do much more than a kind of hand waving statement.

MEMBER CORRADINI: I understand. I understand.

CONSULTANT SHACK: And I guess they could go into MACCS and take away that CAP.

DR. GHOSH: Right. Right.

MEMBER CORRADINI: Then you'd see that but -

DR. GHOSH: Yeah, that's right.

CONSULTANT SHACK: Your way out, yeah.

MEMBER CORRADINI: The way out.

DR. GHOSH: I mean, that's exactly right. Yeah.

MEMBER SCHULTZ: That might be the next phase but I do think it's important to capture the thoughts about - the practical thoughts about why you believe or why you understand that for zero to 50 you get this type of result, and I appreciated your explanation in terms of the practical matter of the consequence evaluation which is what this project is all about - how it affects the result and you explained a piece of it as to what we're thinking about zero to 50.

I'd be interested to see if you could create the same discussion about zero to 10 in terms of explaining the differences.

There's a similarity zero to 50 that you've explained with regard to the protective features associated with the event.

If you could come then back and say and qualitatively the explanation associated with the differences and the zero to 10 results that we're seeing here would be explained - could be explained in the following manner to be investigated further in another project but -

DR. GHOSH: And I think -

MEMBER SCHULTZ: - you haven't done it yet but as you said over the last couple years you've certainly thought about this as you've seen the results and I'm sure you've tried to explain each one as they've come forward. If you could bring some of that -

DR. GHOSH: Yeah.

MEMBER SCHULTZ: - to light in the report it would be very helpful.

MEMBER BLEY: I think it would be helpful on a few counts. One, it'll help people reading it understand that five, 10 years from now this issue might come up again and it'll be lost if you don't document it pretty well.

DR. GHOSH: Right. I've made a note for myself. I'm going to go - I'll revisit what we did say in the report. I think we tried to put in some of that reasoning but to be honest I don't know how much we've captured. So we can revisit what we did say.

You know, the zero to 10 miles is easier to explain because the early phase contribution is so minimal it's just to that .5 percent of the population that's assumed to refuse to evacuate.

And even that .5 percent of the population get relocated if they meet - if they hit the hot spot of relocation criterion or the normal relocation criterion, and the thinking behind that is that if you - maybe some people refuse to evacuate but then if you actually knock on their door and tell them they're about to incur X dose in the next 10 hours maybe they would be more motivated to actually leave.

So the 10-mile dose has many fewer contributions than the zero to 50 where, you know, where you only have the hot spot and normal relocation and that doesn't occur for a certain amount of time that, you know, is assumed it would take, you know, to implement those protective actions.

Okay. So I guess I'll - we'll move to the next slide, slide 16, and so the next suggestion was to select MELCOR realization that produced a small but non-zero contribution to the latent cancer fatality list and to do - essentially do the same for that.

So we've done that. We've basically used the same approach. If you go to slide 17 we - but the difference here was that in order to pick, you know, what would be a representative low source term we kind of - we kind of did a little mini study to characterize, you know, what would be representative source terms across the entire spectrum in results that we had.

So in the next few slides I'll just go quickly through the methodology that we used in order to come up with what would be a representative, you know, source term from the distribution.

CHAIRMAN STETKAR: By the way, this was the first part that I read after getting really, really, really confused by what you wrote up.

I finally figured out what you were trying to do which we were trying to probe - what we're trying to probe in this is, as I mentioned before, what were the uncertainties coming through from the weather and MACCS.

And we asked you to pick the largest replicate for prompt fatalities, which you did - the largest replicate for latent cancer fatalities, which you did, because those are easy to identify and that would be pretty easy to do.

The last one was - well, pick something that's low but non-zero. Just pick one. Just pick one. So that we could investigate some of the things that you were - we're talking about related to the early slide is how much is the uncertainty in MACCS2, let's say what you can measure, is being driven by the fact that you pick that artificially high replicate that was actually, you know, driving the latent cancer fatalities versus a different replicate that wasn't driving but had results.

And I finally figured out what you did. I don't know why you went to all of that trouble to just pick some.

DR. GHOSH: Actually, this was useful for us to have for ourselves also for the future because we wanted to see how well we could characterize a very large set of results with just a few, you know, points and actually we did very well.

I think it turned out much better than we had hoped. So maybe this is a lesson learned for us for the future that, you know, how we might simplify things going forward.

But I realize the process was confusing. We didn't have the - write it all up in the middle of the writeup we provided you. But, again, we had two months to do all of this and digest it and try to explain so -

CHAIRMAN STETKAR: Well, if you spend a month and a half doing the first part of the thing don't do it again.

DR. GHOSH: So the - actually the next slide, which is slide 18, I think also answers a different question, which is so we've done now a number of runs where we fixed a source term and we're looking at the MACCS2 only portion of the epistemic uncertainty. So here we did the flip side.

CHAIRMAN STETKAR: Tina?

DR. GHOSH: Yes.

CHAIRMAN STETKAR: I'm just looking at time here. There are quite a few slides here. Would it be better - I don't know where we are on the agenda for a break but would it be better if we -

MEMBER CORRADINI: She has one more. She's going to finish this, then she's going to go off to her -

CHAIRMAN STETKAR: No, she has several slides that -

DR. GHOSH: We have - it's slide 31 for this question of - for the morning.

MS. SANTIAGO: Yeah, MELCOR -

DR. GHOSH: Because the slides are for the entire meeting actually.

MS. SANTIAGO: Right. So we're doing pretty well.

CHAIRMAN STETKAR: In terms of taking a break, a morning break -

DR. GHOSH: Yes.

CHAIRMAN STETKAR: - when is the appropriate time to do that? Is it -

DR. GHOSH: I wouldn't mind taking a break now, to be honest. I need to use the restroom.

CHAIRMAN STETKAR: Yeah, I mean, it's - end of discussion. Let's recess until, since you're really confident, 10 minutes after 10:00.

(Whereupon, the above-entitled meeting went of the record at 9:49:46 a.m. and resumed at 10:07:55 a.m.)

CHAIRMAN STETKAR: We're back.

MEMBER CORRADINI: We're having a conversation of a technical nature over here.

MS. SANTIAGO: You're on the record.

CHAIRMAN STETKAR: You're on the record

now.

MEMBER CORRADINI: That's okay. It's technical.

DR. GHOSH: So getting back to where we were. So on slide 18, so this is - this slide is actually answering the flip side of the question that if we have a fixed source term what does the MACCS epistemic uncertainty on its own do in terms of the consequence result.

This - we did Run 2 to pick the representative source terms but it also helps us answer the flip side of the question which is if we kept the MACCS parameters fixed at their nominal or, you know, point estimate values for the uncertain ones what does the source term uncertainty do to the final consequence results.

So what Run 2 is that we use the 865 source terms that came out of exercising the MELCOR epistemic uncertainty and then used point values for all of the MACCS parameters, and the spread and the results you see from the zero to 10 to the zero to 50 radii is just the spread from the source term uncertainty contribution to the consequence results.

And as expected, if you look at the spread in the results it shrinks. You know, when you - when you add in the MACCS uncertainty on top of the MELCOR uncertainty all of these curves spread out.

So this was an expected result and we did this for a couple of reasons because one, we wanted to see what the flip side of the equation would tell us but also we did this to help us choose kind of a representative low, medium and high source terms which helps us answer that last question in terms of pick a - pick a low source term.

But we also did it for our own reasons because we wanted to see, you know, whether we could come up with a good methodology to characterize a source term sufficiently, you know, for - kind of for future projects. Yes?

CHAIRMAN STETKAR: This is interesting because I didn't quite appreciate - I think something you just said that this essentially shows the uncertainty from only the MELCOR, right?

DR. GHOSH: Right. Right.

CHAIRMAN STETKAR: You said -

DR. GHOSH: And passed through to the consequence of health - you know, the latent cancer fatality.

CHAIRMAN STETKAR: Yeah, but with a thick set of -

DR. GHOSH: MACCS parameter.

CHAIRMAN STETKAR: - MACCS parameters. So this is essentially your MELCOR uncertainty.

DR. GHOSH: Yes. Yes.

CONSULTANT SHACK: And it's a ratio of about nine versus a ratio of about 15, wouldn't you say?

CHAIRMAN STETKAR: Yeah, but that's exactly where I was going is that you said that it - that the curves became, you know, in this configuration more vertical. In other words, the uncertainty is reduced. It's not reduced a lot.

It's reduced, you know, nine is an error factor of about three. Fifteen is an error factor of less than four. You know, so let's say it was 16. It would be four. It's not a lot of additional uncertainty from the other stuff - the weather and the MACCS2.

DR. GHOSH: Yeah, and -

CHAIRMAN STETKAR: That I think was the surprising stuff. That's the conclusion that I came away from the full committee presentation is that most of the uncertainty in the NUREG report as it's published, those curves that are showing a spread of pick a number anywhere from about 50 to maybe 25, 95th to 5th or 5th to 95th, however you want to characterize it, most of that uncertainty seems to be coming from the MELCOR and that

- this seems to - this seems to corroborate that.

DR. GHOSH: Yeah, and I think it goes back to, you know, what we were talking about before with respect to you get a bit of a dampening effect. You know, if you look at the source term spread, you know, I believe it's a little bit greater than the LCF risk spread and that's because you have this backstop once you put your source term so that you take protective actions and you - there's only so much you can dose people.

CHAIRMAN STETKAR: One other reason I'd like to explore this and I don't know when it's better to do it now. It might - better to do it now since we just started talking about this.

As you talk to people about gee, why don't you do level three PRAs and the answer is oh, it's going to cost a billion dollars and, you know, it's just too labor intensive and besides the uncertainties are so large that you don't learn from - anything from it. This says the uncertainties aren't very big at all.

MEMBER CORRADINI: You skipped over the cost argument. You're getting to the -

CHAIRMAN STETKAR: Well, because I don't believe the cost argument at all.

DR. GHOSH: It's interesting. I think I might have mentioned this last time. Before we did this rule uncertainty from, you know, two plus years ago a lot of people thought that the dominant contribution from the uncertainty and that it would be huge would be from the off-site consequence portion of the modeling.

CHAIRMAN STETKAR: Yes.

DR. GHOSH: And our results show that that is not the case. In fact, the accident progression part of the modeling has a huge contribution to the uncertainty and you do see some dampening effect even when you include the MACCS parameters.

Some of the parameters we have orders of magnitude of distribution on the parameter value because nobody really knows, you know, what the - you know, the - it could be. And you still - yeah, you don't get this blow up effect from the off-site consequence portion of the uncertainty analysis.

CHAIRMAN STETKAR: Honestly, you know, we probed this a little bit before the break and Pat, Tina, I - you know, we don't have any control over your budget or writing or any of this stuff obviously.

But if there's any way that the NUREG could discuss this - if you're really confident - now, you have to be really confident that you understand the reasons why the uncertainty in the off-site consequence part of the problem are as bounded as they seem to be.

If you really understand from a technical basis why that's true and have a lot of confidence in that understanding I think that's a really important message to get out in this NUREG.

Not six significant figures and a lot of math but why that is so from a fundamental principles perspective because it certainly addresses one issue that people do raise regarding how much more would we learn from doing a level three PRA.

You know, part of the argument obviously is the resource requirement but the other part of the argument is well, we wouldn't learn anything because the uncertainties are so broad that what can you tell.

MS. GIBSON: May I answer the why we're not doing a level three PRA question?

CHAIRMAN STETKAR: No, this is the industry why they're not doing the level three PRA. The staff is doing the level three PRA.

MS. GIBSON: Well, the industry has done level three PRAs and they update them periodically. I was going to answer that the NRC is doing a level three PRA of the Vogtle plant -

CHAIRMAN STETKAR: Yes.

MS. GIBSON: - and part of the reason for doing that is to see what the resource commitment is both in dollars and staff time. Another reason for doing it is for staff development, I guess. We're doing as much of it in-house as we can to develop that knowledge and skills.

CHAIRMAN STETKAR: And we're aware of that. I mean, we're following that closely.

MS. GIBSON: I'm sure you were.

CHAIRMAN STETKAR: I speak in more of the feedback that we tend to hear from the industry about the reluctance to do level three PRAs by the industry.

They tend to be both resource arguments and we won't learn anything because the uncertainties are so broad that you can't make any meaningful conclusions and this activity seems to contradict that latter argument that indeed you might be able to - the uncertainties might not be very large.

As I said, you have to be really, really confident in the fact that you've - you'll understand why the uncertainties at least from these presentations seem to be fairly modest from let's say the MELCOR out through, you know, whether it's latent cancer fatality or prompt fatality consequences.

DR. BIXLER: Yeah. One thing that we would expect is if - this is a linear result. LCF we're using linear note threshold here. So this is a linear result.

This is going to have less width to the distribution than some of the other results that are non-linear. But here in particular you don't see a whole lot of range in the uncertainty.

CHAIRMAN STETKAR: That's something else that I was going to ask at a high level but since you brought it up you're a good entree.

The NUREG shows results for the three different - those consequence models. But it doesn't combine them at all, right. All of the results are strictly linear low threshold - LNT model.

Why didn't you try to address what the uncertainties would be rather than just showing three separate snapshots which tend to address them but kind of in isolation?

Because you're right. That would tend to broaden the uncertainties quite a bit if you - if you assigned weights, for example, to each of the three models that you use.

DR. GHOSH: You know, the - yeah, we talked about that a lot and there was a proposal among the team to weight the three dose models to kind of - to come up with a combined this is everything we've done, you know, single kind of number.

But I don't - I think there's a couple of reasons we wanted to do the three snapshots. The big one NRC policy still has to do LNTs so people are always interested in LNT, you know, results by themselves.

But I think the second thing is it's very hard to know what would be consensus weights to put on the different models because to be honest even within, you know, the NRC or Sandia everybody you ask has a different opinion about what the real models should look like and even whether those three alternatives that we - well, LNT, okay, I'll put that aside - but whether the two dose special models we chose were the best thresholds to choose.

There's so much debate about - and actually, I don't know, Nate, if you remember. In our very first round of the uncertainty analysis - this is more than two years ago at this point - we had put in the dose threshold as a variable. Do you remember that?

DR. BIXLER: Mm-hmm.

DR. GHOSH: And we varied that variable from zero to two so instead of doing the three alternate models we have that as an additional.

But we just - we had a lot of strong reactions against that because people feel very strongly. Everyone's got a very strong opinion about what is the right model and what should be the right threshold if they believe that LNT is not the right model.

So we just struggled with what are we going to come up with as consensus weights and in the end we decided to do the same thing that the SOARCA project did which was to show the three separate sets of results.

And if I could just add one more thing. In terms of the spread of results, you know, Nate brings up a very good point.

But the LNT is always the highest consequence so this large spread you would see is always going to be off to the left in terms of the consequence results.

MEMBER REMPE: I know we're going to get into it this afternoon more but while you have this up here, when I looked at some of the responses that were given to us for the MELCOR uncertainties I thought in at least several cases I saw, well, we didn't have any data for characterizing this.

And what I'm wondering is do you think you really bounded all the uncertainties in the MELCOR input? And so maybe this distribution would actually even be more uncertain if there's some things that haven't been captured.

CHAIRMAN STETKAR: Let's talk about that this afternoon.

MEMBER REMPE: I know but I just would like a high level without going into this particular parameter that one - think you got it all?

DR. GHOSH: I mean, I think that's a fair question and as we were very, I think, straightforward up front to say that we are only varying the parameters that we have available in the MELCOR code and we came up with a subset that we kind of think are the key parameters for this model.

But certainly there are issues of model uncertainty -

MEMBER REMPE: Absolutely.

DR. GHOSH: - that are not wrapped into this. We tried to look at the potential effect of some of those separately, and as you know from the report some of those sensitivities can be large.

So then the question would be if you had - if you could theoretically integrate all of those model uncertainties with the parameter uncertainties, you know, what would the real distribution look like. And those are very hard questions to answer and so we did some of those, the sensitivities, on the side. But this is really just a representation of our best guess of what the spread of the key parameters' values might be.

MEMBER REMPE: Given the current models

today.

DR. GHOSH: Given the current models. But, you know, I think it's interesting - the world of trying to model what happened at Fukushima as being some very interesting like MAP to MELCOR comparisons and there you're really getting more, I think, into questions of how, you know, one group models certain processes versus another because the underlying models in MAP and MELCOR are different.

I think we're going to learn, you know, more about, you know, the underlying bases for some of this in a few years and, you know -

MEMBER REMPE: If we can get data. That was the other point I hope to be emphasizing this afternoon -

DR. GHOSH: Yes. You bet.

MEMBER REMPE: - on some of these questions.

DR. GHOSH: Absolutely. Right.

MEMBER REMPE: But yeah, I - again, I just had to take a little detour and mention it now.

CHAIRMAN STETKAR: Pat and Tina, just kind of thinking out loud here. I mentioned earlier that it would be useful, I think anyway, to highlight this kind of lessons learned about what are the uncertainties in the consequence part of this process.

As part of that, it's also I think useful to emphasize up front that you - although the study is accounting for systemic uncertainty it's accounting for systemic uncertainty within the constraints of the models that you have and the LNT model.

And if indeed you want to address this issue of what are sources of uncertainty in the consequence part at the same time you ought to address how big are the uncertainties from LNT versus, you know, if you compared LNT versus the two other models, you know, kind of up front.

Because if that's the biggest contribution to the uncertainty that's the biggest contribution to the uncertainty, which it might be, that says in research space where do we spend our money - where do we try to learn more.

Do we try to learn more about minutiae of do people walk or do they take bicycles or, you know, how do they evacuate?

Or is it more important for understanding public health risk to really start to understand what these models are? Because that's also, you know, very - it could be a very important part of that epistemic uncertainty.

MEMBER SCHULTZ: And the reverse would be true as well. That is, if there is very little difference, which I don't think anyone believes it would be the case, then you would have demonstrated something.

But yeah, I just think it's important to reemphasize that an important contribution here will be to capture in this document these lessons learned or the general conclusions that have been taken from all of the number crunching that's been performed.

Because you're really in a position to do that and if they're not presented here it will be very difficult for a different team to just look at all of the numbers that you've generated and draw conclusions from them that you would come back and say they are the right conclusions.

It's much more likely that you would come back and say how did they draw those conclusions to all of that work that we did. So now is the time to get those down in writing.

DR. GHOSH: Thanks. So on the next slide, we'll just quickly go through the methodology that we used in order to pick three representative source terms. And as I mentioned before, I mean, we had additional motivations on just picking a low source term.

We wanted to see whether we could

characterize the results well enough to be able to sort of summarize, you know, the entire 865 set in a - with a smaller number.

And the metrics we used were the latent cancer fatality risk at the five radial locations that we looked at, and as I mentioned before, you know, beyond 10 miles the dose results are correlated very highly. So we took that into account in the reading.

And then we looked at the fraction of release from the source term for five key radionuclides. So typically you see cesium as coming up as the most important for the long-term phase, you know, from groundshine and iodine as the most important from the short term, you know, for a prompt fatality risk if there is one.

But in our calculations we found that the barium, cerium and tellurium groups are also important contributors to many of the realizations. So we made sure to take that set of five rather than just looking at cesium and iodine alone.

And then we looked at also release time, which is particularly important for potential prompt fatality. And then the goal - the theoretical goal was to choose the source term whose metrics' range would come closest to the one-sixth, half and five-sixths, you know, positions among the entire population kind of as a rough choice for the low, high and medium source terms.

And if we go to the next slide, slide 20, so this - so the important thing here - I know there's a lot of curves and we don't - we don't need to read each of the gray curves. It's there more for effect just to give you kind of a glance for how individual -

MEMBER RYAN: I won't live that long.

DR. GHOSH: But the general trend of the gray curves give us an idea of how these metrics relate to each other with respect to a particular realization.

So, for example, you can see that the 30- to 40-mile metrics are very well correlated with each other because the ranks for those metrics are pretty much straight lines across which means that, you know, if it was - if the 30-mile metric was fifth highest in the population the 40-mile was also about fifth highest.

But you can see that in terms of the other metrics they go up and down quite a bit. So if you looked at any one metric alone it's not going to give you the whole picture of, you know, where it lies with respect to the final consequence results. So you definitely need to look at multiple metrics. You can't really boil it down to one or two.

And the final source terms that we chose as the representative ones are shown in red, green and blue. The blue is the high - you know, the theoretical high source term or representative.

The green one is the representative medium source term and the red one is the representative low source term. And the black dashed lines show you where the theoretical point was where we were trying to get closest.

So you can see the blue line is pretty close to the high. The green is sort of spanning the middle. The red is close to the low.

We actually weighted the release timing the lowest which is why you might see that the - they don't match up necessarily quite as well.

But, again, you know, with prompt fatality risk there's so little risk of we didn't feel that source term needed to be weighted as highly - sorry, that metric.

So if we go to the next slide, which is slide 21, then for each of those three representative source terms which we just named here - I'll have to complete this in case anybody's curious.

So we ended up with a representative low source term, representative medium, representative high and for each of those three source terms we did three Monte Carlo runs again with a sample size of 1,000.

So we're doing three replicates using Latin hypercube sampling once again to kind of check for convergency, how well we match each other, and then using the full 350 uncertain MACCS input parameters and the same weather trials as before.

So if you go to slide 22 now, this is the results for the representative low source term which I guess was the - at least in terms of the ACRS's question was the motivation for this little side study that we did.

So this was the results now for the representative low source terms, and if you look at for each of the statistics - mean, median 5th 95th - once again we have very good agreement among the three replicates.

So we think we've demonstrated convergence not only for the full set of results and a high source term but also even for the low source term we seem to be converging pretty well when you take three different random seeds using Latin hypercube sampling.

Then if we go to slide 23, this is the results for the low source term and Run 1, again, for reference is the - and I apologize.

If you look on your slide printout it's easier. Run 1 is the reference. That's the original run. Again, this is the aleatory means as showing just the epistemic uncertainty.

So the red solid line which is to the right is a zero to 10-mile result and the black line - solid line is a zero to 50-mile and you can see two things.

One, that as expected for the low source term the results shift over to the left and then also once again even for these low source terms the three dashed lines are very well in agreement with each other with respect to the CCDF and once again this is now just for the aleatory means. So this is due to the epistemic uncertainty.

If we go now to slide 24, we can see essentially the same thing. This is - slide 24 is the medium results and the medium results which was, you know, closest to the theoretical 50th quantile or the medium, is actually - matches pretty well with our original run. So, again the Run 1 is the solid line and you can see that the original run is spread out a little bit more.

The dashed lines are pretty much right on top of each other so they converged well with each other and are steeper than the original runs as expected because you're taking off a single source term. So that's the median results.

Then if we go to slide 25 that is for the high source term, and I realize we already went through this exercise with the high source term from the original set of results but this is now our representative high source -

CONSULTANT SHACK: This is the high rather than the highest?

DR. GHOSH: Exactly. This is a representative high rather than the highest. That's exactly right. And here again, same thing - they converge very well with each other in terms of the new high runs and they shift - they're shifted over to the right of the original runs and they're more - the spread is smaller compared to the original spread.

So if there are no more questions about that then I'm going to go so slide 26, and this is just to give you a snapshot of what is the - what was the average difference between the three separate Latin hypercube sample runs that we did over all of the aleatory weather distributions so 1st to 99th percentile.

So comparing, you know, how the replicates did and were within 1 percent of each other. So we think we got pretty - we think we got very good convergence for the LCF, our risk numbers.

So I think if everybody's okay with that portion we will now move into the discussion of the new bootstrapping we did to look at comparing Latin hypercube sampling with a simple random sampling.

So I'm going to - I'll motor ahead unless somebody stops me.

CHAIRMAN STETKAR: Motor. Motor.

DR. GHOSH: So slide 27 -

CONSULTANT SHACK: There was one thing that was interesting. When I looked at the spreads, the 95 to 5th for those three cases, they were about the same for all three cases. It was sort of independent of whether it was high, medium or low.

DR. GHOSH: Yeah. Yeah.

CHAIRMAN STETKAR: That's - my whole - everything seems to have about the same spread no matter how you slice the pie. That's the -

CONSULTANT SHACK: Well, I mean, I am only looking at one, you know, MACCS2 uncertainties but again, that seemed to be driven too much by the source thing that I start with.

DR. GHOSH: Right. Right. Yeah.

CONSULTANT SHACK: Again, it's just an observation but I'm not quite sure, you know, I understand exactly why but, again, it's just interesting.

DR. GHOSH: Yeah. I guess - well, we can

think a little bit more. So just so you know what the plan is, you know, we did a lot of this additional work but it's not only going to be included as part of the main report for our study.

I think the one thing we will include is showing the spread of the convoluted aleatory and epistemic results, which makes sense.

I think a lot of this is going to go to an appendix to our NUREG/CR report so and I think that maybe - so it's something for us to consider, you know, what we - what discussion we might include in that appendix to help people make sense of the results and what it means and why.

MEMBER CORRADINI: I guess - I go back to what Steve said which is if you have some observations that are - whether it be relative to level three uncertainty or to whatever and it's stated there ought to be a - there ought to be a trail even for you.

DR. GHOSH: Yeah.

MEMBER CORRADINI: When you get older you forget things. You're young. You don't forget. So that once you have the statement you know this statement links to this appendix which links to this analysis which essentially - otherwise it'll get lost and you'll end up redoing it.

DR. GHOSH: Yeah. Yeah, I think I mentioned - as I mentioned before, we've tried to be very thorough in our documentation but there's always more to do as more people read it.

I think it's a very good point. So we're in the process now of rereading the entire 520-page report plus the extra 90 some pages we've produced now to answer the ACRS questions to kind of see where to put what discussion and, you know, to make sure that thread is there because -

MEMBER CORRADINI: Okay. I think you've done -

MS. SANTIAGO: To summarize some of the things like you've said that we've concluded from this additional analysis. Right now it's 500 pages and so I think the appendix was what we decided the real detail in looking at some of these things and then carrying them forward.

I think Tina mentioned we do have some summaries in that report itself but it's possibly not as thorough.

CHAIRMAN STETKAR: They tend to be scattered, quite honestly, Pat. The report itself is big and, you know, the 90 pages or whatever it is that we got is what it is.

But, honestly, for a reader who picks up the thing there's a lot of what we've seen this morning detail - tables and tables and tables of numbers and discussions about different ways of thinking about how to sample all of this stuff.

You know, let me say it - things that a mathematician would really get into. There might be some nuggets hidden in places.

MEMBER CORRADINI: There are nuggets.

MEMBER BLEY: Well, there are. There are nuggets out there. I don't have to - what you've written down is what you did and what you found.

CHAIRMAN STETKAR: Right.

MEMBER BLEY: But its logic of - you know, the inferences you're making that kind of stuff isn't - there needs to be something else.

CHAIRMAN STETKAR: It's written from the perspective of somebody who's constructing a building and gets really interested in the pitch of a thread of a particular bolt, and you're not quite sure whether you have a house or a skyscraper or what, you know, or what you've learned.

Maybe it was better to use rivets. Okay. Just keep that in perspective. You said you're going back and rereading the report. The problem is you're reading it from the perspective of people who did all of this work.

DR. GHOSH: Right. Right. And that's why - that's why your feedback is so helpful because I think we've tried to put the nuggets in. I think they're spread out all over the place because they're spread out wherever we've found them -

CHAIRMAN STETKAR: That's right. It's kind of like gee, look - we did all of this stuff and bing, here it is. Okay. Now we did some other stuff and look.

DR. GHOSH: But there's like 90 pages in between the nuggets.

CHAIRMAN STETKAR: Right. Right.

DR. GHOSH: So it's a very - I appreciate the point. I think we need to look at where is a good place to kind of summarize to tell the whole story in a row rather than having to read 400 pages.

CHAIRMAN STETKAR: Well, you have an executive summary but even the executive summary tends to be - you know, it's a snapshot of perhaps too much detail.

MEMBER SCHULTZ: Well, it's very descriptive of what you have done and how it was completed and the results but not the conclusions from the results.

MS. SANTIAGO: And we'll take another look at that. We kind of run to get all this done and we just need to sit and digest it in the next couple of months.

MEMBER SCHULTZ: It's great.

MS. SANTIAGO: But we do appreciate that.

MEMBER SCHULTZ: It's very worthwhile to take the time to do that.

MS. SANTIAGO: Right.

MEMBER SCHULTZ: Because you have, as I've seen, really appreciate the work that has been done in the last two or three months. From the mathematical and calculational point of view you've done just what we asked and it's well documented in terms of, again, what was done.

These conclusions that we're discussing now about what was learned about the entire methodology there's some nuggets here that are really important.

MS. SANTIAGO: And it's important to the future so we totally agree.

DR. GHOSH: So the question came up a lot in the past discussions - some of it online, some of it offline - about why do we keep using Latin hybercube sampling and, you know, I guess one school of thought is not very fond of Latin hypercube sampling.

So we - you know, I went to the statistical community. There's an understanding that when -

especially when you want to estimate the, you know, big distributions and you want to get good estimates of the tails Latin hypercube sampling is more efficient at providing, you know, estimates, especially in the tails compared to simple random sampling.

And I think the MACCS code as we had it up until maybe two months ago we only allowed Latin hypercube sampling because that was generally thought of as the preferred method. But we had planned already to do a code update which is fairly simple to allow simple random sampling.

So we went ahead and did that so that - and so now we can compare the results using simple random sampling versus Latin hypercubes, see how they match up and, you know, which does better in terms of estimating different metrics and so on.

And then since we were able now to do a simple random sampling we also did the bootstrapping very similar to what we did for the MELCOR results so that we produced a gazillion theoretical CCDFs of the results to see how they matched up, you know, with each other and generated confidence bounds for the CCDFs.

And I'll give you the result - our overall conclusion before I show you the results. We are even more confident now that our results are very well converged and that Latin hypercube sampling is valid to use.

Okay. So if we go into the results now hopefully everybody will be convinced as well. So if you - again, as I mentioned if we start to - if anybody is confused about what the different runs mean we have that crosswalk table which is the very last page of the presentation.

CHAIRMAN STETKAR: So helpful. No, it is. Without that table I wouldn't have understood anything.

DR. GHOSH: So Run 1 among the team we also called CAP17 so that's why the legend says CAP17. That - this is our original results from the uncertainty analysis.

It's showing the conditional mean individual latent cancer fatality risk. But what we added here is we did a second run with - using Monte Carlo sampling.

So we used the same, you know, MELCOR results that we had and this time instead of using Latin hypercube sampling for the MACCS portion we used Monte Carlo sampling and you can see that for the sample size of, I guess, 865 the results are pretty much right on top of each other.

So, again, I know that - I think on the

slides you can tell a little bit better which one is the dashed line and which one is the solid line.

But for all practical purposes they pretty much lie on top of each other. So it wouldn't have mattered for our model whether we use Latin hybercube sampling or simple random sampling which here we've maybe a little bit confusingly termed Monte Carlo sampling. So the MC is - stands for the simple random sampling.

But anyway they're pretty much right on top of each other. That was for LCF risk. Slide 29 shows the prompt fatality risk results out to three and a half miles and once again as expected you do start to see differences between the Latin hybercube sampling and then the simple random sampling or Monte Carlo sampling once you get out to results that are pretty much driven by the tails.

So as we talked about before, for the - once you get out past two miles and, frankly, even the two mile results it's a small percentage of the total realizations that you get a calculated non-zero number.

MEMBER BLEY: This is for the same number of samples in both cases?

DR. GHOSH: Yes. Yeah, exactly.

CHAIRMAN STETKAR: Well, but this is - well, same number of samples. This is - make sure I understand how this is done - CAP17 is 865 Monte Carlo realizations and for each realization you pick one value for each of the 350 MACCS input parameters. That value is selected based on a sampling routine, right?

DR. GHOSH: Right. Right. Exactly.

CHAIRMAN STETKAR: So it's -

MEMBER BLEY: But the number of samples that they take for Monte Carlo or for Latin hypercube they're doing the same number and Latin hypercube would claim that you'd have to do a lot more Monte Carlo samples to get -

DR. GHOSH: Exactly.

MEMBER BLEY: - something similar, and I guess I would have been interested in seeing -

DR. GHOSH: How many?

MEMBER BLEY: Well, what happened to -

DR. GHOSH: That'll be our third round.

MEMBER BLEY: Do they really converge or is something funny going on? I think the people who don't like Latin hypercube have something. I don't fully understand what bothers them about it.

DR. GHOSH: Yeah. Yeah.

MEMBER BLEY: If in fact Monte Carlo eventually converges to it that would be interesting to know.

DR. GHOSH: Right.

MEMBER BLEY: I've never seen anybody do that numerical experiment anywhere.

DR. GHOSH: You know, I feel like -

MEMBER BLEY: Somebody must have originally -

DR. GHOSH: I heard that a group at Sandia in fact who we work with - it's probably John Helton's group - I feel like at some point I have seen some work that demonstrates, you know, what it takes to start to - basically if you're doing simple random sampling you keep adding - you can just keep adding runs one at a time until you start to see that - your variance every time you add a result is not changing.

And I think somebody has done that to kind of of show for a different model, not for ours. But there have been, you know, some experiments done with regard to showing that.

But for our model I think we already knew that the three and a half mile prompt fatality risk was altering that - was driven by the tail and it seems that 865 is not enough to have an adequate representation of the tail which is what's driving the mean - the aleatory means results here.

So that's where you start to see a

divergence between the Latin hypercube sampled results and the simple random sampling.

So the black line is the Latin hypercube sampling results and you see, you know, it's a factor of, I don't know, maybe even on the tail about an order of magnitude higher than what you would have gotten with a similar number of just simple random sampling results. But the other two are pretty well in agreement - the two mile and the 1.3.

MEMBER SCHULTZ: So why would it work in close and not further out? Why would it work in close - 1.3 and two look fine and then suddenly you have a different result?

DR. GHOSH: Yeah. So if we - there are very few non-zero results once you get out past more and more - as you get further and further away they're very -

MEMBER CORRADINI: You mean it's just with the data?

DR. GHOSH: If you go to - yeah, if you go to slide eight -

MEMBER CORRADINI: Yeah, let me see.

DR. GHOSH: If you go to slide eight. So this was the original set of results that were based on Latin hypercube but it gives you an idea of the percentiles we're talking about.

The mean - you know, the mean result when you get past two and a half miles is driven by results that are beyond the 95th percentile. So there's a very small percentage of the 865 realizations where you calculated a non-zero number.

At the 1.3 to, you know, two-mile range something on the order of 11 to 13 percent of the results, which is still a very small number, but it's really small once you get out past two and a half miles.

I mean, your little bitty itty tail and I think I mentioned this before. In even those handful of realizations on the tail it's a handful of the weather trials in that - in each of those individual realization that's driving the results.

I mean, it's a ridiculously small percentage that's driving your results and for those types of case there's Latin hypercube sampling because you use a stratified sampling routine that forces you to pick numbers from the tails. It generally does a better job of estimating the result than a simple random sampling routine would.

So that's why there's just so few non-zero results that 865 is not enough to get a good idea of what the results are.

Does that make sense? So that's why as you go further out it's getting more and more unreliable in terms of the results that you implied.

So we were on slide 29 so that's why you see that difference and we've done -

CHAIRMAN STETKAR: It's just surprising - it's just a bit surprising that the - yeah, I understand the math.

DR. GHOSH: Yeah. Yeah.

CHAIRMAN STETKAR: It's just a bit surprising that relatively small increments -

DR. GHOSH: Oh, in distance? Yeah.

CHAIRMAN STETKAR: Right. Zero to 1.3, zero to 2 you get really good agreement here. Then if I go another mile and a half -

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: - things diverge tremendously, you know, in terms of -

DR. GHOSH: You mean physically?

CHAIRMAN STETKAR: - high level physically what's going on in that extra mile and a half.

DR. GHOSH: Right. Right.

CHAIRMAN STETKAR: Do you know?

MEMBER CORRADINI: Increasing the area is not a factor of four but -

CHAIRMAN STETKAR: Increasing the area -

MEMBER SCHULTZ: You're going down by a factor of three and then a factor of three. As you go from curve red, green, black factor of three, factor of three.

DR. BIXLER: You're probably increasing the population even more than proportionateley with the area too because not too many people tend to live real close in.

DR. GHOSH: Yeah, so that's - right. That's one explanation. So the, you know - so we have a discussion in our report when we did the single realization analyses about how we got that very strange result, that one realization out of 865 where we calculated a non-zero prompt fatality risk, you know, beyond 10 miles.

And I think, you know, we talk about some of the mechanisms of how one gets a - how one can get a prompt fatality calculation at all and I'm trying to think whether there's something helpful in that discussion with regard to - I mean, maybe it's a big point is the dilution of the population is increasing so much.

The population is increasing so much once you get beyond that when you weight the risk by how many people there are because there's only a finite amount of material that's - maybe that is the - that's the dominant

-

CHAIRMAN STETKAR: I think that's an important part of it.

DR. BIXLER: Yeah.

CHAIRMAN STETKAR: Maybe a -

DR. GHOSH: Okay. So then slide 30 returning now back to the latent cancer fatality risk results. So here's where we're showing the results of the bootstrapping.

So we have the three replicates which we're calling CAP37, CAP38 and CAP39, which is Runs 15, 16 and 17. So sorry, but that's why we provided the crosswalk table so you have multiple numbering schemes here.

And what we're showing is - so the black curve is the CDF from one of those replicates and then the red and green curves are the theoretical confidence bounds from doing the bootstrapping. So you can kind of see, you know, how close they come to each other.

So in our writeup we did this for looking at each individual replicate against the confidence bounds of the other two. But this one just gives you an example.

And I believe on the next slide, slide 31, we have the same comparison for the 50-mile radius. So the first one was for the 10-mile radius and the slide

31 is for the 50-mile radius.

And we believe - once again, we believe there's reasonably good agreement in order to be confident that our results are convergent for our purposes.

And I think actually that was our last prepared slide for this portion of the discussion so our MELCOR folks are happy. But yes, if there are -

MEMBER CORRADINI: I'm weathered out.

MEMBER REMPE: You were too optimistic.

CHAIRMAN STETKAR: Do any of the members have any more questions about this part of the process? I know you don't over in the corner. If not, we're ahead of schedule so we might as well get into the things that that corner of the table will be more active on.

MEMBER REMPE: So we're going to -

CHAIRMAN STETKAR: We'll - yeah, we'll just press on through and we'll figure out an appropriate place to break for lunch, you know, between parameters or something like that.

MEMBER SCHULTZ: Before we start, maybe it's best to put my comment in here rather than to save it to the end of the meeting and that's just I wanted to go back to the discussion related to the - those consequence models, the linear no threshold and you -

there are the models that had been explored here, and you mentioned that there was a hesitation to do a weighting that based upon reactions that one gets when you talk to even a group of expert specialists associated with this type of modeling and because of that reaction it was determined along with the NRC's approach of using LNT as a way to present results.

So that combination of things led you to conclude not to go forward with a weighting of the different models.

Having said that, and we did mention it previously, developing from what we have learned here a full understanding of how the selection of those models affects the results is very important.

When we say, well, NRC would always use at this point in time LNT - that's the approach that has been selected to present the evaluation - that's certainly of interest.

But having that said, to do work and to show the comparisons and the differences with other models, other approaches, and draw conclusions from them associated with the - and add on to the basic results or conclusions of the study with LNT would be very important to do.

So I think there's more that can be done than to just present results and say, well, here they are but to see if we can draw conclusions associated with these different evaluations that have been done, the uncertainty evaluations and so forth - what would be the impact if one were to have selected a threshold model and done the same type of work and looked at the same type of evaluations with regard to model uncertainty, regulatory uncertainty and so forth.

DR. GHOSH: Right. We - yeah, and we can do a better job of summarizing that and when we looked up - we did a sensitivity study in the report looking at aleatory uncertainty this is before we did all these new runs we did also look at the effect of the weather uncertainty if you use the dose - the dose threshold models.

So we present those results as well and I think as Nate mentioned before with the dose threshold models you have more uncertainty but all the results are pushed lower.

So you have a bigger spread but much lower results compared to the LNT and that's discussed in the report but we can certainly think more about if there's some, you know, more perspective that we can put on that, you know, up front in the report because that's kind of buried in the sensitivity -

MEMBER SCHULTZ: Why do we conclude that there - you have more uncertainty if you select a different model?

DR. GHOSH: The results are a lot smaller plus you have this threshold effect where you basically don't count any risk.

You're not counting any risk. You're not counting any risk so you get beyond a particular threshold and then you start calculating the risk and the numbers are smaller.

MEMBER SCHULTZ: Right.

DR. GHOSH: But having that threshold effect then because with the smaller results you have a larger uncertainty contribution from the weather you get a bigger spread in the dose threshold model results but that - but it's still -

MEMBER CORRADINI: A bigger - a bigger spread and a bigger uncertainty.

DR. GHOSH: Yes.

MEMBER CORRADINI: The distribution gets wider -

DR. GHOSH: Exactly.

MEMBER CORRADINI: - although the absolute - the total number of effects gets smaller.

DR. GHOSH: Exactly. Exactly right.

Right. So you have a larger spread but it's all shifted over to a smaller - around a smaller -

MEMBER RYAN: I mean, there are other things I guess too that would also shift them around like means distribution and populations and all those kind of things. Have you thought all that as well or no?

DR. GHOSH: Well, you know, that one and actually it's an interesting one. So we had Oak Ridge actually help us implement our - the health modeling for SOARCA and they did kind of a stylized implementation based on FGR13s.

It was Keith Eckerman at Oak Ridge, and the models that they use are supposed to be an average of the U.S. population as a snapshot in time. So they take the kind of the entire - the characteristics of the whole population.

So it's supposed to be across all ages, you know, across all relevant characteristics and I'm - actually I'm not a health physics model. I don't know what the relevant characteristics are. I've only heard -

MEMBER RYAN: Just to pick on one, I mean, I think most folks would tend to focus on thyroid, iodine and young people.

That's a big subgroup of interest and it's probably maybe half a dozen other ones like that that might be more important than probably everything else. I just wondered if you had focused on those kinds of subsets.

DR. GHOSH: Right. So we have a supporting report from Oak Ridge that explains what we did for both of the SOARCA study as well as for this uncertainty study and, again, the numbers like the - I don't know if I'm using the right terminology but those convergent factors and so on are a population averaged number when you take the point estimate and then there's a very detailed methodology in FGR13 on how to construct the uncertainty distributions around that point estimate and that's what we implemented in this study.

MEMBER RYAN: What I'm trying to understand is, you know, if a child study is one-tenth the size of an adult study the same intake is 10 times the dose right off the bat so and I guarantee that Keith understands that and probably took care of it some way. I'd just like to know a little bit more about it.

DR. GHOSH: I think - right. And I think the way - this is my understanding again. The way he weighted that effect was to take whatever percentage of a population that are children who would experience that effect and weight the higher, you know, convergent factor by the percentage of the population experiencing -

MEMBER RYAN: Right. Okay. So that's how you did it was you weighted the convergent factors based on age.

DR. GHOSH: Yeah.

MEMBER RYAN: And the population in that age bracket I guess, right? Something like that?

DR. GHOSH: Right. Right. Right.

MEMBER RYAN: Okay.

DR. GHOSH: So it's - it was very - it's very complicated and I didn't do that implementation but yes, that's why he characterizes it as a snapshot of the entire U.S. population for some given year.

I don't remember what year that is but they went through grading all the - yeah, the differences among population times the blood percentage of the population -

MEMBER RYAN: That sounds right. I'd sure appreciate a copy of the report just so I could kind of go through the details.

DR. GHOSH: Yeah, no problem.

MEMBER RYAN: That would be great. Thank you.

DR. GHOSH: So according to the agenda we were going to discuss the MELCOR parameters first and so the original questions were - well, I guess the questions as we understood and as we got was really a list of parameters that the committee was interested in talking about further and kind of understanding how did we characterize the uncertainties for these parameters and why did we choose the shape of the distributions that we chose for these particular parameters. So -

MEMBER REMPE: May I interrupt before you get into specific parameters? One of the comments I think we were pretty good about saying during the meeting was is there going to be some sort of discussion not only about what parameters you selected but the other parameters that could have been selected but were ruled out because they weren't as important and is that discussion - I don't think I saw that discussion in the written response back to us last night.

Is that a true - of what I did see and didn't see? And I think the peer reviewer said that too.

DR. GHOSH: Right. We have a section in the report. I believe it was 4.3, which is called other phenomena or something where it's certainly not a comprehensive list of everything that could have been included.

But we did identify additional aspects of our modeling problem that we recognize are uncertain that we did not include in this study.

So it's not a comprehensive list but we did give that some thought and we captured some of that in the Section 4.3.

MEMBER REMPE: So I think that section existed by the time you came to see us last time and I still have the comment about specific parameters that weren't identified that - you did a sensitivity study and said well, that's good enough and again, I guess I'd like to have seen more depth in it.

DR. GHOSH: Okay. Yeah, I apologize. The writeup we got did not repeat that point but, you know, we can -

CHAIRMAN STETKAR: Let's - before we get into - I think it's- - you know, we're ahead of schedule here and I know you were really interested about this issue, and you're right that this stuff that we received last week didn't really address it very well.

So why don't we before we get into, you know, valves and all of these other specific issues why don't we see if we can address that? Because I don't think - you didn't plan to discuss that particular topic today if I look through -

DR. GHOSH: Yeah, because I was -

CHAIRMAN STETKAR: Okay. So why don't -

DR. GHOSH: - actually it's in my reading - I apologize.

CHAIRMAN STETKAR: Okay. No, that's fine.

DR. GHOSH: In reading my material I did not pick up on that.

CHAIRMAN STETKAR: That's fine. We're ahead of schedule so why don't - why don't we flesh out some of this while we have the time here?

MEMBER REMPE: I don't know if that's fair to make them do that when they've not seen -

CHAIRMAN STETKAR: Well, one thing I'd ask you, Joy, do you have specific examples of other parameters that you think they ought to have looked at?

MEMBER REMPE: I do and but -

CHAIRMAN STETKAR: Okay. Good. That's where I wanted to get to.

MEMBER REMPE: - the thing is is that it was in something that I'd prepared and sent to our missing chairman from the time when this was occurring and so, again, I'm looking for that.

CHAIRMAN STETKAR: Okay.

MEMBER REMPE: No, and so I again -

MEMBER CORRADINI: I just think they should proceed and we'll - if there's something that pops in our head then they can take note of it because they haven't prepared for it. So in some sense this is ad hoc.

MS. SANTIAGO: And we didn't look at additional parameters.

MEMBER CORRADINI: No.

MS. SANTIAGO: Which is basically what I hear you saying. And we can talk about the specific ones.

CHAIRMAN STETKAR: I think that - and I try to speak so Joy can search through her -

MEMBER REMPE: I found what I'm looking for.

CHAIRMAN STETKAR: Okay.

MS. SANTIAGO: We're trying to give you time.

MEMBER REMPE: Yeah, but yeah, you can go ahead and speak though if you want to first.

CHAIRMAN STETKAR: No, I thought this - so the comment I think we did add it and it was kind of down on the bottom of our laundry list of things to look at for this meeting was yeah, was there a - an active - what types of active decisions were made to select the particular set of - and I've forgotten the body count, 21 I think it was.

MEMBER CORRADINI: Twenty-two is what I remember - 21 or 22.

CHAIRMAN STETKAR: Something like that - parameters to characterize the uncertainties. How do those either bound the uncertainties? You know, for example you selected 350 MACCS2 parameters. Why 21, why not 27, why not 227 for MELCOR.

DR. GHOSH: For the MACCS parameters just to clarify we - those 350 individual parameters can be boiled down to 20 parameter groups because there's so many correlated parameters in terms of the health effects modeling, you know, the deposition velocity, just to give you some examples.

So it sounds like a really big number but it's really - we think of it more as 20 parameters rather than 350. But, Randy, do you want to go first or do you want me to go first?

DR. GAUNTT: Well, I can just recollect what our, you know, thought process was and to some extent we were building on a collection of uncertain parameters that we put together, I don't know, about 10 years ago I think when we were looking at uncertainty in hydrogen and I believe it was in Sequoyah pointed at a particular regulatory issue about igniters, and about 10 years ago we did a study to try and scope out what would be the uncertainty in hydrogen generation for a given station blackout I think in Sequoyah.

And so that was actually our first MELCOR foray into uncertainty quantification and I can - I can remember that we spent by far the largest fraction of the total time in doing the project on surveying for parameters that we thought would be important in the timing and extent of melt progression and hydrogen generation and then on - trying to characterize what we thought were best estimate values and uncertainty ranges.

So we were, I think, in SOARCA beginning with that list and then maybe expanding a bit, culling the list a bit and expanding others and so that as I recall the overall strategy was to have some representation across the board of in-vessel phenomena without necessarily, you know, doing everyone you could think of.

Partly that's a cost consideration because each one of those parameters that you sit down and have to study you really put a lot of time in rationalizing what values that you - that you use.

So I think in - I'm trying to dredge up the old brain cells here. But I think we wanted to have representative uncertainty parameters that would affect things such as the amount of hydrogen generation, the overall rate of core melt progression and, you know, degradation in geometry, things such as that and that's what led to our final list.

Then there's other parameters that weren't considered I think in our Sequoyah study like this behavior of the SRV and relative importance of steam line rupture.

Those turn out to be sort of like cliff edge phenomena and so we put some effort into doing our best to characterize the behavior of the SRV and whether or not you would lead to steam line rupture versus SRV seizing open so bifurcation in the - in the melt progression - in the accident progression.

So I don't know if that helps but that is sort of the thought process as pretty much a - pretty much an engineering judgment exercise as well.

MEMBER REMPE: So to give you a load of information or questions I had, I was wondering why I never saw any direct comparison data for many of the uncertain parameters in the distributions that were shown.

Back in the old days when we did AP 600, for example, a lot of times we tried to characterize, you know, this is what the data are and this is the way we came up with this distribution.

It's hard to do when you just have experts but still the discussions to the - quantify the higher level surrogate parameters rely heavily on qualitative expert opinion and, of course, that's difficult to reproduce.

I know you've said you have a balance that encompasses selective parameters from the various phenomena but when I was looking at it and, again, this was written several months ago but what I thought I saw a lot of uncertainty parameters related to aerosol transport agglomeration deposition low level parameters which could be compared to data but not as much for in-vessel progression.

I didn't see things that - like debris composition effects which I thought might be all this radial spreading and radial relocation and things like that which I think composition would affect things.

And so I'm wondering if we missed some correlated variables - emissivity and oxidizing environments, things like that that could have been sampled.

And, again, we'll never be totally complete in the documentation. I understand that. But I guess I kind of had - I would have liked to have seen more discussion on why certain things weren't there just because the next time when somebody picks it up 10 years from now they're going to say well, that's odd - I wonder why they didn't do anything in that area.

DR. GAUNTT: Yeah. It is what it is, I guess, at this point and - but I could add - I mean, I'm going to add somewhere. I don't know where the right point is to interject it.

Based on the schedule limitations and level of effort and all, this is kind of how we scoped it for this study and I want to add that in a separate effort we're doing right now for Department of Energy really related to Fukushima we are currently embarking on a much more - a broader sampling of code uncertainties. We're specifically going to look at one of the Fukushima sequences just to see how - some of these other possible parameters.

So it's going to be a much larger list. This committee might be interested in tracking, you know, that as we make progress. But I mean, I acknowledge we have just a limited set of parameters that we studied.

MEMBER CORRADINI: If I could just follow on, Joy, because I think, Randy, you kind of started in that historically it came from some source. So you've got 20 some of them here. The SRVs are clearly important. You're going to talk about those.

Then in-vessel you've got some that are correlated in terms of the - what do you call it, the fuel failure criteria, radial spread that Joy was talking about.

Ex-vessel you've got debris relocation and drywall - dry well liner failure and hydrogen and then you've got again more equipment stuff - the dry well equipment structural effects, the some sort of opening of doors in the I guess you call it the railroad but I'll call it the separate building door as well as dry well and then some other things. So all of these were developed historically based on past calculations that you saw at the biggest - I'm trying to get the logic.

So you picked these because of past calculations of accumulated - this is where you saw the variability and other places you didn't see the variability so you proceeded just to leave those go? I'm still trying to understand the overall logic.

DR. GAUNTT: Yeah. I don't - I don't know if it goes that deep, Mike. I'm not recalling why we chose the, you know, railway door. I wasn't really participating in that.

Some of my colleagues who are listening on at Sandia can fill in the gaps where I'm not able to tell you.

DR. GHOSH: I think, yeah, the question with that was the magnitude of the effects from the chimney effect, you know, and, you know, what we found there was it's not how much the opening is but it's whether or not that you blow open the doors in the first place ended up being more important than the parameter that we actually sampled which was how big was the opening -

MEMBER CORRADINI: Right.

DR. GHOSH: - taken to the uncertain.

MEMBER CORRADINI: But I guess I would characterize things - I mean, just to - again, I'm just trying to bundle things so I can remember. The first bundle is equipment actuation or lack of actuation during the severe accident degradation which is the SRVs primarily and batteries.

Then there's the in-vessel phenomena where you pick a couple of things and other things were correlated to those coupled. Then there's some ex-vessel phenomena where you pick a couple of things and other things are correlated.

The one - I remember when you guys were here in July I asked a lot about water and I think Randy's - maybe you were on the phone. I don't think you were here in July.

DR. GAUNTT: I wasn't here.

MEMBER CORRADINI: Okay. Somebody said

over the phone well, in SOARCA there is no water in the dry well per the accident scenario which led me to ask the question geez, that's odd - aren't there leaking pipes all over the place - there's got to be some water.

And I think at that time we were - we were told that there's this separate issue relative to - a couple ex-vessel and then a couple - again, I call them equipment but more kind of - I was going to say criteria or state of the plan in terms of doors open or doors closed, dry well performance.

But they kind of break down into four of those and then you get into the aerosol, and my only point is I'll accept for the moment that was a judgment.

I'm just trying to understand the logic to that judgment so that other things - if I get Joy's point is other things were put aside because just in the sum total of all these years of calculation these were the things that popped up as important. That's what I'm hearing.

MEMBER REMPE: I mean, again, I guess what I see it's easier to vary things. You can easily vary sometimes as was that also part of the - I mean, aerosol stuff was pretty easy to vary whereas some of these other things when you came up with a surrogate parameter it would have been harder to do and it is a really unrealistic approach to life and I'm kind of wondering if that was what dominated the process too a bit.

DR. GAUNTT: Well, I mentioned the - you know, the melt progression thing. So that was leveraging heavily off of our hydrogen uncertainty study.

The things we varied in the aerosol dynamics kind of stuff we had also developed a list of uncertain parameters for work we've done for NRR. Some - you know, we typically would do an uncertainty quantification on transport behavior. And so we have those parameters easy at hand and justification for what, you know, we thought were the uncertainty ranges on them. So we included those as well.

MEMBER REMPE: But maybe not because they were so important but because you already had -

DR. GAUNTT: They were handy.

MEMBER REMPE: Yeah.

DR. GHOSH: I don't think - I don't think that's a completely fair characterization because, you know, the SOARCA project had progressed several years, you know, before we started the uncertainty work and we had an external peer review committee who kind of, I don't know, maybe a couple years into the project started tracking the work and asking those very hard questions along the way as part of the original SOARCA project.

So for the - for the point estimates that we created during the development of that and the discussions with the external peer review we had already identified a number of issues where we were getting asked a lot of questions do you really know that this is the way, you know, it should be modeled - are these the correct values.

So even before we started the uncertainty work there had already been quite a bit of discussion about particular areas of uncertainty that it was clear, you know, kind of the severe accident modeling community were aware and were making us aware that there would be questions about these.

I think really there was a combination of places where the original kind of list of things started from, certainly the hydrogen uncertainty study, some of this other uncertainty work we did. But a lot of it also came out of the discussions with the external peer reviewers from the original SOARCA study.

So then in the SOARCA study we did a bunch of sensitivity studies just to look at individuals one at a time. But we knew that, you know, once we did the uncertainty analysis it made sense to revisit that and put it all together to kind of see how important the whole group of things are.

So, I mean, there is quite a bit of history, you know, going back to -

MS. SANTIAGO: And you're right. I mean, the main list started out of all the questions we were getting from the peer review committee. And then during the SOARCA pre-briefs and briefings with ACRS we said well, this is the list of parameters we're going to look at in the uncertainty analysis because we even had a preliminary discussion with you to ask if there are other parameters we should be looking at.

We also took a look at some of the questions we were getting from the commission and what we might look at and dive into a little deeper. It's not an entire list.

MEMBER CORRADINI: No, I didn't expect it - I didn't expect there would be. I guess what I'm - again, I'll speak for myself. I'll let Joy say it differently.

It strikes me that you're using kind of accumulated judgment to decide what should go and what shouldn't, which is fine, with the peer - and I guess I didn't realize the peer committee was asking.

But I'm just remembering back to 1150 when we had to do this for the plants. We had a bunch of people in a room. We fought for days on end as to what should be in, what should be out - once it's in what's the range - now what's the range, now you got to settle what's the shape.

And I think what - if you skipped all that because of just an accumulated history somewhere in the documentation this ought to be at least discussed so it's clear. And, again, I forgot about the peer committee being, of course, an input into what they're worried about.

MS. SANTIAGO: I think the appendix in the SOARCA report talked a little bit about this.

MR. FULLER: This is - this is Ed Fuller, the senior technical advisor for severe accidents. You're talking a lot about expanding a little bit on a project that was all encompassing but in a very narrow focus, namely the scenario as defined in the SOARCA project.

There are several major areas of uncertainty that really dwarf what you're talking about here and that need to be addressed in the coming years. I'll give a couple of examples.

First, Mike brought up water. Yeah, there's water that might be getting out of the vessel as you're boiling it all the way.

It might come out and land for a little while on a containment floor but water you really ought to be talking about is that which operators are going to put in to try to mitigate the accident - dry well flooding, for example.

You didn't bring up other operator actions which are very important such as venting and the strategies for venting.

Then there are very important phenomena that probably aren't even modeled properly that could be, and I'll give you an example. We know that the lower portion of a BWR underneath a vessel has all of these control rod drive columns going up made largely of steel.

If you want to talk about hydrogen oxidation, especially when you're thinking about water in the pedestal region in the dry well, start thinking about oxidation of that steel after vessel fails and what that might do to your hydrogen production.

Just an example. I mean, it could be a game changer. We don't currently model something like that.

So if you want to really get serious about what to add to an uncertainty analysis think about the broader concept of what a comprehensive accident management activity ought to entail. And I hope that the Office of Research will get into that sort of thing in the coming years.

MEMBER REMPE: But for this report, again, I realize documentation is about all we could ask for to try and beef it up. I think it's where we are today. Maybe we can't ask for too much of that.

But I think, again, more detail should be provided for the surrogate parameters and it may be - again, I'm guessing but you can vary that and that's why some of those parameters were selected.

They're in the code and they may have acknowledgment that there may be underlying effects such as composition that are not considered that may affect things and correlate things. It just would be good for documentation.

I'm guessing, like this radial debris and we'll talk about it later, is just an expert opinion and three expert opinions - what the experts used like they did back in the 1150 studies, how they came up with that distribution. Something on that level would really, I think, be helpful.

But again, it's up to you how you spend your money and how much money you get to do it. I don't know.

MR. JONES: This is Joe Jones at Sandia. Could I clarify one point, please? It was mentioned that for NUREG 1150 there were multiple meetings to discuss parameters that should be in or out and the like, and I just want you to be sure we had those meetings here with the MELCOR experts - Mark Leonard, Casey Wagner, Randy was in those.

These date back as far as 2009 and we did vet a number of parameters and identified which ones we thought and at the time why should be included, and then we discussed the ranges and the types of curves. So we didn't miss that step in this process. I just wanted that to be clear.

MEMBER REMPE: I agree that - from discussions with Tina that some discussions like that did occur. But what I don't see is documentation of it and back in the 1150 supporting documents they documented what kinds of calculations they looked - that the experts thought were important, what calculations they looked at and just to give people a flavor of what was considered is what I'm asking for.

DR. GHOSH: Yeah. And I guess, you know, after we met back - last April we did try to add some additional description of what was done.

But I think what we are missing and what we can't really recreate at this point is the type of documentation that you would get from we met on this parameter on this date and, you know, this was the reason that was discussed and this was one of our resource challenges from the beginning.

And I know that's not a satisfying answer but to do something that's more akin to the documentation you see for like a formal PIRT process, for instance, would have probably doubled or tripled the budget that we had and so that was our challenge from the beginning.

We did a lot of that and we tried to describe what we did qualitatively in the discussion generally but we don't have that level of documentation like you had for NUREG 1150 just because it was way beyond the scope of what we were allowed to - you know, what we could pursue.

MEMBER REMPE: Well, maybe it won't be like that but any ideas on especially the surrogates of what they thought were important I think would be helpful.

But - and maybe you've added it already and I just haven't seen the updated version. But this is something to keep in mind.

MS. SANTIAGO: We can go back and talk to Sandia and some other folks and see.

DR. GHOSH: Yeah. I guess the question with the surrogate parameters, you know, I don't know if - yeah, I guess I'm not sure what to do about that.

I think in the report where we talk about surrogate parameters we're talking about I think as you noted parameters in MELCOR that are kind of lumped parameters that represent different things that are going on.

But so I think that's why we're calling them - that's why we're calling them surrogate parameters. But those are the actual parameters in MELCOR and there's no separate occasion of the other mechanistic parameters that is getting mathematically combined into that overall lumped parameter. So maybe it's the terminology that's confusing.

You know, if that parameter is the MELCOR parameter and there aren't lower level parameters and then the code that are getting combined in order to -

MEMBER REMPE: So the radial spreading, for example, you get the same value for that radial parameter whether you have ceramic melt, metallic melt, whatever.

DR. GAUNTT: That's right. I mean, that level of mechanistic distinction in MELCOR is not there, you know, like to query what is the composition. I mean, MELCOR knows what the composition is but we haven't connected that -

MEMBER REMPE: There's no viscosity or anything like that?

DR. GAUNTT: - to viscosity and, you know, I think a lot of times when we talk about surrogate parameters they are - in all these codes there are modeling abstractions on the melt progression process and, you know, so the - one thing we may talk about here - I think it's in the slides - is the core degradation sort of a lifetime rule - when does the core, under what conditions does the core begin to lose raw geometry and start to collapse.

And so there's three different lifetime models implemented there to sort of capture what is uncertain about all the mechanistic details of, you know, very localized collapse and so forth.

So its intent - that type of model is just intended to be the abstraction of what's really a very, you know, complicated physics problem to -

MEMBER REMPE: It must be real hard for the next person to come up with a distribution as we go through some of these and -

DR. GAUNTT: Yeah, and it's a lot of engineering judgment that takes place and if I can jump ahead and tell you for the - for the fuel rod lifetime model prior to putting that in we found that, well, what we had before that was a temperature threshold. We reach a certain temperature and we now judge the fuel can't be rods anymore - it's got to be rubble, and run some uncertainty studies on that and find out, well, sometimes you just go right under that temperature and sometimes you go right over that temperature and then it takes you to a branch point.

And so, you know, I think MAP does some of the same kind of things but to avoid having those kind of unrealistic - I feel like they're unrealistic bifurcations, we went to this lifetime rule such that if you were just under that threshold, well, maybe you could go a little bit longer but finally collapse a fuel rod and if you go over that threshold it all happens a little bit faster.

MEMBER CORRADINI: But it always happens.

DR. GAUNTT: Yeah, it'll eventually happen and, you know, unless you're far from - far from that threshold. I don't know if that helped but -

MEMBER REMPE: It helped.

CHAIRMAN STETKAR: I missed something. I was scribbling notes here. I thought I heard you say that Sandia is doing or has some work in progress to do a more - I don't know, comprehensive is probably not the right word - more, right, evaluation of uncertainties in MELCOR. But that's a DOE project in particular?

DR. GAUNTT: It's a DOE project. It's very Fukushima-centric and it's really aimed at - it's really aimed at characterizing a broader spectrum of possible realities of, you know, given reactor core degradation such that when TEPCO finally opens up the reactors we have a, you know, a broader map of what they might find.

CHAIRMAN STETKAR: Is - what I'm - there's obviously a lot of interest in these issues and I'll point to the back corner of the - the northeast corner of the table from my perspective.

Is NRC research tracking that work or is that - is that strictly - I understand type of contract may be set up but obviously it has trickle down implications.

DR. GAUNTT: We have promised to stay in communication with Richard Lee's branch and get, you know, get their take on it. We're beginning with the stuff we've generated here in SOARCA and now kind of adding more things to that list.

CHAIRMAN STETKAR: Because I think there would be some interest, you know, among this subcommittee or members of this subcommittee or some subcommittee about what you're learning from that process and, you know, because we're NRC-centric here. We can have people come in and give us presentations.

MEMBER CORRADINI: But I think Randy did - I thought - I mean, I think I was on the phone. It was in some year recently that you came here and gave a presentation - I was on the phone - about the Fukushima calculations that Sandia and Oak Ridge were doing in a reconstruction project from, I think, it was in the spring of '12.

CHAIRMAN STETKAR: Yeah. That was pretty early on. Right.

MEMBER CORRADINI: Right. But I think - I guess just to jump to the - where I'm - where I was - that's why I'm getting back just to engineering judgment which is MELCOR has - and I think Dan is the one who's been alerting us this for years, I think, is that MELCOR is sort of evolving to come up with the general engineering judgment that MELCOR tends to hold up the core, create essentially a lot of in-vessel degradation before things move along, and that's - at least in the presentation I seem to remember that Randy gave us that's where MELCOR does - where its predicting reality is and MAP tends to predict a reality that things meltdown very quickly with little hydrogen production.

CHAIRMAN STETKAR: And that in-vessel sets the table for what might occur ex-vessel. Am I off base?

MR. FULLER: This is Ed Fuller. I can answer your question very quickly. You have it a little backwards. Yeah, MAP calculates that you get earlier time to vessel failure but it calculates that in the melt progression process in the core region you basically form a blockage which prevents hydrogen from going through the debris instead the steam has - steam from going through the debris but instead has to go around whereas MELCOR keeps the passageways open so that things progress a little more slowly and in lower temperature in the MELCOR approach and more quickly at a higher temperature than the MAP approach.

MEMBER CORRADINI: Thank you.

DR. GAUNTT: If I could just mention there's another DOE-sponsored effort because, you know, we had done the Fukushima forensics with MELCOR.

Likewise, EPRI has done Fukushima forensics with MAP and we had opportunity recently to kind of look at those two analyses side by side and we can see, you know, for a while they're the same and then they start going a little bit down different pathways and one of the - one of the other tasks we picked up from Department of Energy is to do a MELCOR- MAP crosswalk is what we're calling it and to look very much in detail at what's happening in the core degradation and where do we start to, you know, to depart in our modeling abstraction I think is really what it comes down to.

MEMBER REMPE: So my understanding is one of the important goals of that crosswalk is to inform some of the hopefully - hopeful to occur inspections of this data would help us to resolve some of these big differences in the modeling and I think that's an important -

DR. GAUNTT: Yeah, it's to identify, you know, how these modeling - I want to call them an abstraction because we kid of abstract the whole process a bit and MAP tends to have a very TMI-centric view of core degradation.

And if you remember Steve Hodge - any of you guys in the old days - proselytize on how BWR melt progression goes he had a slightly different view. MELCOR tends to lean a little bit more to what he was describing, which is a more gradual relocation of core materials as opposed to this in-core molten pool.

CHAIRMAN STETKAR: I think, you know, what we should do is try to keep in touch with the staff. It sounds like - I sort of recall a meeting that Mike recalls much better than I do because -

MEMBER CORRADINI: It was April 12th. I looked it up.

CHAIRMAN STETKAR: Okay. Thank you. I don't understand any of this stuff so I tend not to recall anything.

But there is quite a bit of interest and especially this notion of MELCOR versus MAP - what are we learning in terms of MELCOR - how does that affect not necessarily SOARCA so much because quite honestly the gates have come down, the horses have already died somewhere on SOARCA.

But for future uses, you know, do we learn anything that's useful for the level three PRA work or NRC research going forward from these exercises, and I think members of ACRS - I don't know whether it's this particular subcommittee or the thermal hydraulics subcommittee or the Fukushima subcommittee, some one of our incarnations, would probably be interested sometime in the next few months depending on where that work is and learning about, you know, what's been done.

DR. GAUNTT: We're scheduled to do a two-year update. We're scheduled to do this crosswalk mid-October.

CHAIRMAN STETKAR: Mid-October. So -

DR. GAUNTT: And I think we're going to do it here in D.C. somewhere so -

CHAIRMAN STETKAR: Well, mid-October is too early for us because we're already up to here. But sometime in the first quarter of next year we may want to think about that. So I don't know, Hossein, if you want to keep in touch with I don't know who.

MEMBER REMPE: To digest the results too then it might even be worth a letter to talk about some of the -

CHAIRMAN STETKAR: We'll worry about letters later on but at least get -

CONSULTANT SHACK: When changes are made to MELCOR - I mean, there's not a DOE MELCOR and an NRC MELCOR, is there -

DR. GAUNTT: No. There's just one.

CONSULTANT SHACK: Do you discuss differences with - you know, results that you get from DOE presumably inform what you're going to deal with MELCOR for NRC and -

DR. GAUNTT: Up to now we really haven't changed anything in MELCOR. We started - with the Fukushima work we started with our Peach Bottom model here in SOARCA and it, you know, replicates the observable measurables from Fukushima awfully well, and you'll often hear Richard Lee say we're not changing anything in MELCOR until we open up the vessels.

Well, we're pretty - you know, we don't just flit around changing the code models. It's pretty much a long road of history that got us to this point.

CONSULTANT SHACK: I mean, I guess I would have assumed it was - there had been a lot of discussion before you make any -

DR. GAUNTT: And there's - yeah, and there's not a DOE MELCOR or an NRC MELCOR. I'd say pretty much there's an NRC MELCOR. DOE is offering us opportunities for other, you know, validation and other kind of application work.

MEMBER REMPE: The NRC's involved in the DOE program through an MOU. The funding thing is a little less clear to me how it involved because there isn't a date definitely.

Richard's review you might - the forensic report you did and things like that and provided comments just like we did.

CHAIRMAN STETKAR: I'm scribbling notes here. Just a second.

MS. SANTIAGO: So should we start -

MEMBER REMPE: Do you want to take your lunch break, John, or what do you want -

CHAIRMAN STETKAR: I'm just scribbling notes here if you'd give me a second. Thanks.

MS. GIBSON: While you're scribbling let me just say as far as MELCOR is concerned it's pretty seamless between the NRC staff and the staff at Sandia.

We communicate, I would say, almost on a daily basis and decisions about what to do with a code are made jointly and, you know, obviously it comes down to funding but we prioritize what needs to be done and in some cases DOE has money to provide Randy's expertise for meetings and things and in other cases we provide the funding.

But when it comes to MELCOR we're kind of, you know, all - one for all and all for one, I suppose.

CHAIRMAN STETKAR: Okay. I think that - thanks for accommodating me. I'm a slow writer and I can't read my writing after 10 minutes anyway.

So and I think we will in some way, you know, try to follow up on this effort because I think there is quite a bit of interest among, you know, ACRS members in particular. So we'll try to target that at an appropriate time.

Sounds like early next year might be the appropriate time once you've gotten through some of - we've gotten a little more maturity in terms of what you know and what you don't know.

And with that, I do think it's probably an appropriate time to break for lunch because it's just - you know, otherwise we'll talk about relief analysis and -

MEMBER CORRADINI: And we promise not to

ask any of these questions again.

MEMBER REMPE: Get down to the nitty gritty on -

CHAIRMAN STETKAR: And if - I'm trying to also make sure that we can cover most of the material before Mike has to disappear.

So anybody have any problems coming back at 12:45 rather than 1:00? If not, we will recess until 12:45.

(Whereupon, the above-entitled meeting went off the record at 11:43:48 a.m. and resumed at 12:48:34 p.m.)

DR. GHOSH: So we'll go into the MELCOR parameters of interest and the first one - so we've in this presentation we've ordered them in the list that you indicated was roughly the - your priority of, you know, level of interest and the first one on that list was the SRV stochastic failure rate.

So just before I delve into the individual parameters we talked a little bit right before lunch about kind of the general process of how we came up with distributions and so on. I just want to add a little bit to that.

You'll see that for some of the parameters there was some data out there or some, you know, experiments or some kind of real-life type experience that we could draw from to help build the distribution.

You know, for some of them they're past studies. You know, for some of them we had more information to draw on than others. For other parameters you'll see there was little to no data or experiments - you know, very little to go on.

And so we tried to explain, you know, what we did base our judgements on. You know, everything comes down to - it was kind of a collective expert judgement of the team which was fairly large who did the study - I think we had about 20 people - the collective judgment of the team and also informed by, as I said, multiple years of discussions with the external peer review committee as well as the ACRS last year.

We actually revised a couple things after the April 2012 discussion that we had and the first one is an example of that.

So we tried to explain, you know, what we based on and I think we were pretty up front for some of the parameters, that there isn't much to go on and for those the - that experiment was to think of what would be a reasonable variation in the parameter values.

So there was some confidence in what a nominal value might be, you know, just from the - as Randy talked about the years of experience, you know, using the MELCOR code and whatever experimental data and other studies have been done.

There's some, you know, some - maybe some confidence in what a nominal value would be. But then with that experiment was what would be a reasonable range around that value that we could represent that we think yeah, we know it's uncertain.

Maybe this is within the range and we're not going to violate any of the physical modeling principles of our - of our model problem. So that was kind of the thinking behind some of the ones where we didn't have much data to go on.

It was more to explore what would - what could be a reasonable range and to see what the effect of those parameters would be.

Now, as expected, you know, many times when you have a very complex system where you have a lot of interaction effects and so on, you can start with a very large set of uncertain parameters.

But at the end of the day it really is just a handful of parameters that drive the results in your - the uncertainty in your results and we pretty much found that in this case too.

So, you know, out of the 20, you know, MELCOR parameters and 21 MACCS parameters when we really looked at what was driving the variation in the results it's a much smaller set of parameters that's actually driving the uncertainty in the results.

So anyway, so that's just a very clear kind of - just a little synopsis of the general thought process and so I'll go into the individual parameters and I'm sure we'll talk more as we get into individual ones.

Okay. So the first one is the SRV stochastic failure rate. We've said this before and I think you'll recognize this. This is one of the most important parameters both in terms of the magnitude of the source term as well as the latent cancer fatality risk results.

And this parameter in conjunction with the SRV open area fraction which we'll get into later are really the determinants of which subscenario you end up seeing.

So for the SOARCA study we basically had a relatively early seizure of the SRV due to a stochastic failure and that is one sub scenario that we observed in our 865 realizations as well but then in addition we have a second set - a second and a third set really of the long-term station blackout scenario where if you don't have the early failure of the - you don't have an early stochastic failure of the SRV and instead it cycles for a longer period of time and you end up eventually seeing it fail because of a thermal seizure.

And when we model that it should fail by thermal seizure it may fail in a position that's not fully open. So for the stochastic failure we said if it's a failure to reclose then it's open - it doesn't reclose. It fails fully open.

If it fails because of thermal seizure we then sampled well, it may not fail fully open. It may fail partially open and we sampled what that effective open area is for the SRV.

And in that subset of scenarios which was about half in our study some portion of those scenarios also led to main steam line creep rupture in which case, you know, you no longer have the benefit of the scrubbing.

You're venting into the dry well and you have unscrubbed releases, which can be quite a bit larger. And so for this reason because the set of thermal seizure scenarios is more consequential than the early stochastic failure and the main steam line rupture scenarios is still more consequential, when you compare the spread of the uncertainty analysis results to the original SOARCA study, you know, as a number of you have noted the SOARCA results are kind of in the lower path to - on the lower end of the full spread of the uncertainty results and that's largely because of the fact that we observed these two other sets of scenarios that were not modeled as sort of the best - you know, the - our best guess of how a scenario might evolve.

Our best guess of the early stochastic failure in the uncertainty cases is about half the time and then the other half the time you have the SRV thermal seizure either with or without main steam line rupture.

So with that we realized this fairly early on. I think we talked about this last time. We actually did many iterations of the uncertainty study. So in the first iteration of the study, we had a distribution assigned for the SRV stochastic failure rates which nobody was completely thrilled by because this is one where there is basically no relevant data out there to support the distribution.

And by relevant data I mean there is testing data and there is this NUREG/CR-7037 which actually defines a failure rate - stochastic failure rate for, you know, BWR SRVs of this type.

But that data there's only, like, one or two failures. It's basically based on testing where you just - you trigger the SRV to close and if it doesn't close, you know, you count that as a failure.

So it's testing in a controlled environment and it's - and very different from the - what you would actually see in a severe accident where you're repeatedly triggering the SRV to open and - to open, I guess, because you're relieving pressure repeatedly and, you know, under - with the - in the pressure sensing mode over and over and over.

It's a very different failure sort of case actually than just testing it once and seeing whether or not it opens and close as you expect.

So everyone, you know, sort of recognized this but we really struggled with what exactly to do about it because we knew - so we have this data base in 7037 which has, you know, one or two data points of having a failure out of many, many, many trials where you try to open it.

But it's not a - the test is not representative of what you would actually experience during a severe accident. So we really struggled okay, so how do we - do we just take that distribution and use it, which is what we did in the first iteration, or do we try to use something different knowing that that isn't representative?

So after we talked with the ACRS last April, we mentioned that we were revisiting, you know, kind of what is our consensus expert opinion about what we should use as the distribution and we evaluated what are all the sources of data we have.

So we went beyond 7037. There's another NUREG, NUREG/CR-6928. We also evaluated the data there.

We looked at the database that our original SOARCA value came from and that's the Peach Bottom-specific IPE. And then when we revisited that we saw that they had a second distribution also for the extreme environment.

So it's extreme environment versus the normal data set, and what we ended up doing is - because most people think that the epistemic parameter distribution for a failure on demand should be approximately a beta distribution we used the same methodology that they used in NUREG/CR-7037 so to construct the beta distribution we used that same methodology to construct distributions based on these other references. And we basically came up with this set of roughly - it was five curves. So we have five curves to kind of choose from.

This is the universe of possibility that we could conceive of in terms of what we have available to us and the one that we settled on was the Peach Bottom IPE curve which is the red one and there are a number of reasons for that which I think we go through in the report.

It kind of falls in the middle of the possible curve so we didn't do a formal weighting of, you know, the possible curve so it falls roughly in the middle.

We think it's more representative of conditions than the original curve. So, you know, it's going better in the right direction. And in the end, what really matters is the density of the curve between approximately nine times - 10 to the minus three failure rate and about five times 10 to the minus two because below nine times 10 to the minus three you pretty much are always going to get a thermal seizure.

So how - you know, it doesn't really matter what the shape of a curve is below that. And above five times 10 to the minus five, I mean, you fail so early that, you know, it doesn't really matter beyond that too.

So we're really looking at what should be the - you know, the probability that the true value is between about, you know, around the 10 to the minus three point and about five times 10 to the minus two and we thought the red curve was, you know, kind of in the middle of what the possibilities might be.

CHAIRMAN STETKAR: Thanks. First of all,

what I'd like to understand is these curves. I looked at NUREG/CR-7037 and I also looked at NUREG/CR-6928. You've characterized NUREG/CR-6928 by the purple curve.

That is not the uncertainty distribution in NUREG/CR-6928. It is very, very far different - very far different. In fact, NUREG/CR-6928 and NUREG/CR-7037 have distributions that are very similar because they were both initially based on a noninformative constrained Jeffreys prior.

In the case of NUREG/CR-6928 they used data of two failures in 3,142 - 3,142 demands. In 7037 they had a little bit more data - two failures in 3,536.6 demands. So if you look at the curves 70 -

DR. GHOSH: How do you measure .6 demands?

CHAIRMAN STETKAR: - 7037. Because of the way they counted the demands.

DR. GHOSH: Oh, okay.

CHAIRMAN STETKAR: 7037 is slightly different from 6928. It is not this difference. The purple curve - I don't know where you came up with the purple curve. 6928 doesn't have anything that looks anything like that for this failure mode.

So I have no idea where it is. Now, what I - what I don't know - let me finish this.

MEMBER CORRADINI: Well, just a

clarification. You're saying - just so I understand - you're saying -

CHAIRMAN STETKAR: If you move the purple curve to essentially where the blue curve is - because 7037 and 6928 are not independent sources. They both started with the same thing. 7037 collected a few more tests - you know, about 400 more tests. That's all.

MEMBER CORRADINI: That's all I wanted to ask. Thank you.

CHAIRMAN STETKAR: So they're not independent sources.

DR. GHOSH: Right. Right. Right.

CHAIRMAN STETKAR: And they're - and if you look at the data - the distributions they are not that different.

Now, what I don't know is what the green curve is because I'm not quite sure. I didn't study 7037 to understand what that green curve is.

DR. GHOSH: So 7037 had data for multiple SRV triggering modes and so the green curve represents the subset of pressure triggered.

So I think they - I don't know. If Kyle or Doug is on the line they can remind me what the - if they remember what the other modes were. But there were multiple triggerings for - multiple modes of triggering that SRV to open so and some subset of that was pressure triggers.

And we thought that was more representative of our situation and they actually broke that out in the NUREG because in our case that's what we're modeling, that the SRV is relieving is the triggering on high pressure. So we thought that was more - that was relevant to also show the -

CHAIRMAN STETKAR: I didn't look at that because you didn't present that as a comparison in NUREG/CR-7155, did you?

DR. GHOSH: No, because we -

CHAIRMAN STETKAR: You compared 7037 to 6928 in the Peach Bottom IPE and the Peach Bottom disavowed IPE. Now, what I - what I'd like to explore - this is important because when you explained how you came up with the red curve you actually took the number that they used in the IPE as a mean and fit the beta distribution with the parameters of the beta distribution from 7037.

So you basically fixed it at the mean of 3.7 times 10 to the minus three and spread it from that as if that is the truth.

Peach Bottom themselves have disavowed that number, right? They don't use that number anymore.

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: So now my question is if we're -

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: Let me - let me finish. If Peach Bottom doesn't believe it and if two NUREGs don't believe it, if indeed everybody believes the curve have shifted much to the left which says from the stochastic failure standpoint the dials will cycle for a long, long time - in other words, this stochastic sticking open would probably be a very, very small contributor to the overall results - if that is actually our state of knowledge why are we living with the red curve?

DR. GHOSH: So here's -

CHAIRMAN STETKAR: And if we're living with the red curve only because experts sitting at this table and on the phone line believe in their heart that's the curve they want to force the results into a certain area, you ought to present it in that light.

We, the assembled multitudes, decided we wanted to use this curve because it would then give us results from that early sticking open failure mode for no other reason.

DR. GHOSH: Okay. So let me - this has been one of the biggest challenges.

CHAIRMAN STETKAR: I understand that and it's important. But we're talking about uncertainty and the basis for that uncertainty.

DR. GHOSH: We had - well, we had extensive discussion, not just amongst this team but also with our external peer review panel and everybody - everybody agreed that the testing data was not representative -

CHAIRMAN STETKAR: Good. Then don't use it. Simply say it's irrelevant. End of story. If that's what everybody decided, that the testing data in 7020 - the 7037 to 6928 was irrelevant don't confuse people by saying look, look, what we used is in the middle of all this stuff.

A, it's not in the middle - it's skewed. And B, if everybody agreed it's not relevant what are you comparing apples to orange for?

You're trying to justify something that you made up and you wanted to use as if it's - as if it's - you know, as if it has relevance to something else. So if you all decided it's not -

DR. GHOSH: Because I think the - right, because I think the question comes up all the time. You know, you're basically modeling a failure mode that's named in this database, you know, this NUREG database - you know, why don't you say something about it. I think that -

CHAIRMAN STETKAR: You can say something about it -

DR. GHOSH: - there's more challenges, yeah.

CHAIRMAN STETKAR: You just did. You said we looked at it. We decided it wasn't relevant for the following reasons and that's why it's not relevant, period. And given that, we used this.

MEMBER CORRADINI: So can I again get a clarification? John asked the question in the middle of his long question - what is the - can somebody - since now you've said that you guys relatively uniformly didn't like the data what's the difference between all data and pressure data? That's what I didn't understand.

DR. GHOSH: Oh. So there were multiple triggering modes for the SRVs that they tested. One of them was the pressure trigger, so, you know, the end which we think is the best match - if we had the right temperature pressure kind of conditions is the best match for what we're modeling here because in our model we're pressure triggering those SRVs to open. But they had other ways of triggering the SRV and then, you know, they'd record why they're not able.

MEMBER BLEY: This was in the tests?

DR. GHOSH: In NUREG - yeah, in the 7037, right.

MEMBER BLEY: What were some of the other triggers? I don't know.

DR. GHOSH: Yes. I was in fact just calling on - if Kyle or Doug are on the line if you remember because I don't recall -

MS. SANTIAGO: Is anybody from Sandia on?

CHAIRMAN STETKAR: I wonder if they called back in.

MR. JONES: Joe Jones is on and I believe Kyle and Doug are on. I'll go double check with them.

DR. GHOSH: Okay.

MEMBER BLEY: The other thing, John, when they come back if somebody can explain where that purple line -

(Simultaneous speaking)

MR. JONES: -- SRV stuff to try to find it.

DR. GHOSH: Thanks. We'll get back to you on that because I don't remember off the top of my head.

MEMBER BLEY: Okay.

DR. GHOSH: Sorry. You were saying?

MEMBER BLEY: I was saying in addition to the last thing John asked about the first thing he mentioned was your purple curve doesn't really come from

6928. Where the heck did it come from?

DR. GHOSH: Yeah. I need to double check that. To be honest, it's been over a year since we generated these. I need to double check what we did to get that.

CHAIRMAN STETKAR: I ran out -

DR. GHOSH: I'll get back to you on that.

CHAIRMAN STETKAR: Yeah. I ran out the 6928 distribution and it has a 5th percentile on the order of about three times 10 to the minus six, a median on the order of about four times 10 to the minus four, a mean on the order of about eight times 10 to the minus four and a mean on the order of about three times 10 to the minus three.

So it's - as I said, it's close to the - it's really close to the blue curve because the way they - these noninformed prior are constrained by the data and the data really are not all that much different for the -

MEMBER BLEY: Could you perhaps take - like you did on the Peach Bottom take their point estimate and apply some other distribution to it?

DR. GHOSH: I think that that's possible but I don't want to give you the wrong answer. Let us double check what we did.

CHAIRMAN STETKAR: The 6928 point estimate is eight times 10 to the minus four. It's - it didn't do that.

MEMBER BLEY: Yeah, I was - well, there was another case we looked at where people took the low end and anchored it and used the spread.

CHAIRMAN STETKAR: Oh, okay.

MEMBER BLEY: She could have done that.

CHAIRMAN STETKAR: Yes. They could have.

DR. GHOSH: So we'll get back to you on that because I don't - it's been a while.

CHAIRMAN STETKAR: Back to the more - the fundamental question, see, I have no problem with engineering judgment - I, John Stetkar, today decided that I'm going to use this distribution for the following reasons. I have no problem with that.

I mean, if that's - if that's all that's available - if the - if you disagree with the test data or you have, you know, essentially no evidence to support your knowledge there's nothing wrong with that at all.

But it ought to be presented that way, not something, well, we kind of liked the number that was in the Peach Bottom study despite the fact that Peach Bottom doesn't use it anymore and we used that as our best estimate and we used somebody else's parameters of a data distribution because they're in a NUREG to express our uncertainty.

That is just relying on other crutches. It's not saying I, John Stetkar, today decided to use this distribution because. I used this - I used this number because it appeared in this report and I used this spread because it appeared in somebody else's report and I liked those things because I didn't have to - I didn't have to make that decision myself.

DR. GHOSH: Yeah. I mean, you know, again, we'll be rereading the whole report to kind of see how we say things. So we'll revisit what we say in that section. I thought we tried to at least explain why we thought, you know, the data were not relevant.

CHAIRMAN STETKAR: My question though is I read some of that and I have no idea whether 3.7 times 10 to the minus three as a best estimate is good data or indifferent for the type of behavior that you're trying to model - successive multiple cycles of a relief valve under, you know, pressure - high temperature pressure conditions.

I have no idea. I'm not a valve person. I don't understand how they might fail. I just don't. So I - all I'm saying is that I don't know whether that 3.7 times 10 to the minus three is a best estimate from a valve person or whether it ought to be .1 or whether a valve person says no, if - you know, we believe it would behave as if the people who looked at the uncertainty distribution for the testing program is essentially the blue line. I just don't know. Did you talk to valve people?

DR. GHOSH: We tried. We had heard that there was some additional testing data available in industry. We tried really hard to get access to some of that data.

So the short answer is as far as NRC could, you know, gather we couldn't track down any other data or thoughts on this matter beyond what we had amongst our team and the external peer reviewers and when we talked about it here last April.

MS. SANTIAGO: Well, we had the Division of Engineering take a look at the SRV failures and they did a separate analysis with Abacus and -

DR. GHOSH: For the thermal seizure.

MS. SANTIAGO: Yeah. Yeah.

CHAIRMAN STETKAR: That's for the thermal.

DR. GHOSH: For the thermal seizure.

CHAIRMAN STETKAR: That's for the thermal seizure. That's a different failure mechanism.

MS. SANTIAGO: We can go back and ask them if they have any additional -

DR. GHOSH: We have. We have. This is the best we could do. We asked everybody we knew under the sun and the only thing is it seems there may be other information out there that's proprietary and unavailable to us. That's just -

CHAIRMAN STETKAR: Let me ask this from kind of a pragmatic - I mean, you have identified - I actually didn't use the important stuff until the end. I looked at the parameters and had questions about the first and then I discovered this thing is important for some reason.

How different would the overall SOARCA results be if the stochastic failure rate looked like the blue curve rather than the red curve? Do you have a sense of that?

DR. GHOSH: You know, we could -

DR. GAUNTT: I could guess because -

MR. FULLER: This is Ed Fuller. To cut to the chase, John, if you're much less likely to fail stochastically before you get to core damage then the likelihood becomes quite a bit higher that you will have valve seizure at high temperature.

You'd probably easily get to 500 or 600 lifts until you get to that point and from that - what that would mean probably in terms of the conclusions here is that you'd be more likely - much more likely to have a seizure and hence more likely to have a main steam line creep rupture.

CHAIRMAN STETKAR: And that, because I have no idea how this stuff works in the real world -

MR. FULLER: It's because the stochastic -

CHAIRMAN STETKAR: No, I understand that. But in terms of the overall SOARCA consequences then what are the implications of that?

MR. FULLER: Well, you talked about the SOARCA - main SOARCA?

CHAIRMAN STETKAR: Yeah. Well, no.

DR. GHOSH: So I think the uncertainty report gives some indication of that. So we - because we saw right away that we have these three subscenarios that behave quite differently, we present the source term results from the three subscenarios separately and I think there is uncertainty about this distribution.

If the blue curve were more likely or the true curve you could weight the two other subscenarios higher than the stochastic.

So right now with this - with the distribution we did implement about half of the time you end up with the early stochastic failure and the other half of the time you end up with thermal seizure and some subset of that thermal seizure is main steam line rupture.

If you look at the three distributions of results that are generated from early stochastic seizure versus thermal seizure versus main steam line rupture, basically you could weight higher the results from the thermal seizure cases and the main steam line cases to see what the effect would be of using the blue curve versus the red curve which is -

CHAIRMAN STETKAR: Yeah. That's what I'm asking right now. Do you have a sense for what - because I don't. I'm not a level two, level three person.

DR. GHOSH: Do you -

MR. OSBORNE: This is Doug Osborne from Sandia National Laboratories. The split occurs right around where SRVLAM is between one and three times 10 to the minus three.

CHAIRMAN STETKAR: Yeah. So about - anywhere from 300 to 1,000 cycles. And that's okay. I'm not asking precise numerical questions. I'm saying suppose it was - don't think about weighting. Don't think about anything else.

Suppose it was all from thermal seizures. How would that affect the overall results of SOARCA? How would it affect the latent cancer fatalities or the early fatalities?

DR. GHOSH: The consequences go up.

CHAIRMAN STETKAR: They would go up?

DR. GHOSH: Right.

CHAIRMAN STETKAR: Okay. Thank you. That's all I wanted to know.

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: Now, if it's shoved way to the right - if the valve experts were to tell you oh, yeah, it's really likely to stick open after, you know, a hundred demands so that everything was shifted further to the right that would drive the consequences down?

DR. GHOSH: Yes.

CHAIRMAN STETKAR: Okay. Thanks.

DR. GHOSH: That is correct.

CHAIRMAN STETKAR: Because I didn't - I probably knew that when I read the report however many months ago but I didn't quite make that connection.

DR. GHOSH: Right.

CHAIRMAN STETKAR: Okay. Well, I think, you know, from my perspective I'm bothered by this notion of you, you know, comparing things to data that you admit that you don't think is relevant, especially if the - because I don't understand what the purple curve is, you know, justifying that you're in the middle of the available data when A, there's two failures in both of those reports and B, that the reports are essentially correlated at least for the - what do you call them, the stochastic failure mode and I did not look for the green curve in 7037. I thought I - you know, it's a 177-page report and I didn't read every page of that one.

So I'm curious where that came from because I don't know. I suspect they didn't have any real failures.

DR. GHOSH: Yeah, it's possible.

CHAIRMAN STETKAR: At least the other - at least the blue and where the purple ought to be was constrained by actual evidence.

Now, you might - you might discount - you've discounted that evidence because you said the testing isn't necessarily relevant. And I had no idea where the 3.7 times 10 to the minus three came from other than Peach Bottom used it in their IPE for some reason.

DR. GHOSH: Now, you're - this is before my time.

CHAIRMAN STETKAR: But I mean -

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: My concern is that the - you've identified this as a very key parameter for the overall study results and you're using this type of comparison to say well, it looks like we've captured the range of uncertainty - we extend from high numbers - let's say a high failure rate which says there's maybe a 50/50 chance that it fails early to a very low failure rate - that there's a 50/50 chance that the thermal seizures get you first. I understand that. But trying to then justify that the red curve - I'm at a bit of a loss.

DR. GHOSH: Well, we've been at a loss too about what to do. You know, I think we've taken our -

CHAIRMAN STETKAR: But in other cases you've used engineering judgment. See, my whole point is in other cases you've used - Joy has mentioned a few places where you've relied very, very heavily on engineering judgment -

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: - for phenomenological issues, for example, in MELCOR.

DR. GHOSH: You know, I think it's fair to say we are using engineering judgment for this one too and we probably haven't written it up correctly in terms of how we explain why we are using what we use.

So I think we can do a better job of that. I mean, I think it's clear from this discussion that we can improve on what we've said, yeah.

CHAIRMAN STETKAR: And at one level I understand, you know, the desire - kind of the academic desire, if you will, to have a distribution that spans sort of those - that range of failure rates so that you can examine gee, if it were really small how would the world behave - gee, if it was somewhat larger how would the world really behave.

That's kind of an academic gee, let's test a few things and see how - however, if then that is characterized as the results of a study that purports to report the actual best estimate results from a current state of knowledge uncertainty analysis that's a different connotation than just looking at an exercise in examining suppose it was this small, suppose it was this big - how would that affect the results.

DR. GAUNTT: So John, I think - I don't remember how these distributions came about myself so I think we should see if we can reconstruct -

CHAIRMAN STETKAR: I'm pretty sure I know how the red one was generated. I'm pretty sure it was anchored at a mean value of 3.7 times 10 to the minus three using the alpha and beta parameters, essentially the spread of the distribution from NUREG 7037 because if you looked at shapes of the distribution they're essentially the same shape.

DR. GHOSH: Right. Right.

CHAIRMAN STETKAR: So I'm pretty sure that's what was done to actually create the red curve which is saying that your best estimate is anchored at that 3.7 times 10 to the minus three number from the original Peach Bottom IPE.

DR. GAUNTT: Seems likely. I mean, I'd say I'm pretty sure that's probably what was done.

DR. GHOSH: Yeah. No, I think that's -

CHAIRMAN STETKAR: Or it actually says that's what was done.

DR. GAUNTT: Yeah, that's why I was pretty sure that - I was trying to -

CHAIRMAN STETKAR: Randy said he wasn't - couldn't quite remember how it was developed but okay. I don't know. I mean, that's - I've ranted as much as I can on this particular one.

I'd recommend that you pretty carefully look at the justification for that distribution, granted that, you know, I know because of budget constraints you're not going to change it I don't think now because you have to rerun the whole - you have to rerun the whole study if you change that red curve.

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: But at least to give the

- see, the way it's presented right now the reader is given the impression - an uninformed reader is given the impression that in your introduction look, we have five sets of pieces of data.

There's a lot of uncertainty about what is relevant but look, the thing we picked is in the middle. So we're probably okay in the middle, and I don't think that the evidence quite supports that story.

MEMBER SCHULTZ: Well, it also implies that you had this information and in fact used it to pick the red curve, or at least it influenced the decision.

DR. GHOSH: Yeah, which I think is a correct characterization. I think there are multiple reasons we were comfortable with what we ended up using, recognizing again that there is a lot of uncertainty about what the two distribution should be and I think we try to explain that we know that this one is very uncertain and it influences the results.

With regard to - I think in the future if we had more information that would help inform what the true distribution would be. In our range of results we have captured a broad enough set of possibilities that we could do some kind of reweighting of our set of results to better reflect what this distribution might be.

So for example if it turns out, you know, that the stochastic, you know, failures are much more likely then you could weight higher the subset of the early stochastic failures we have.

It if turns out that it's a lot less likely we would weight higher the thermal seizure and main steam line rupture results that we have.

I think if we were to get more information on this particular distribution in the future we could do some reweighting of the range of results we have because we've captured a broad range of possibilities. But we've struggled with the level of information we have for this particular parameter, which is not much.

I mean, there is expert judgment about the fact that the data we do have is not representative and why. But, you know, what the actual distribution should be, I mean, it has been a struggle for the last few years.

MS. SANTIAGO: I'm just going to move on to the next parameter if that's okay.

CHAIRMAN STETKAR: Any other members have any other questions about this particular one?

MEMBER REMPE: Move on to number two.

DR. GHOSH: Okay. So this was the chemical form of iodine and cesium fraction. We provided some additional information from the Phebus test, you know, with regard to where the fractions came from but I don't know if members want to elaborate on what the questions were with regard to this parameter.

CHAIRMAN STETKAR: Yeah. This was another of mine. Let me reorganize my notes here. The biggest - first of all, I know nothing about cesium and iodine other than the fact that they're some sort of chemicals that are not good for you to eat.

On the other hand, I do - it's my understanding that the - what you did is you had - at a high level you had four tests from the Phebus experiments and - you had four tests from the Phebus experiments and those tests gave differences in the iodine fractions and quite large differences in the iodine fractions.

And what I think you did and let me make sure is that you, first of all, took a linear average of all of those tests and created a what you're calling here a combination number five so that's a made up combination. And then you went back and you weighted combination number five.

You gave that a 50 percent probability of being the way the real - the world works and gave each of the four test results equal 12.5 percent probabilities.

Now, in terms of uncertainty analysis, I'll use this ludicrous exercise - in my pocket right now I have a penny and a nickel, two small things. I have a fifty-cent piece and I have a dollar. There's kind of a range of, if I look at those four experiments, the average value of those four coins is 39 cents. What is the probability if I reach into my pocket that I pull out a 39-cent piece?

DR. GAUNTT: Zero.

CHAIRMAN STETKAR: Right. So if I'm doing an uncertainty analysis why am I assigning a 50 percent probability of pulling out a 39-cent piece? Isn't the - and because of that I am artificially reducing the uncertainty.

I'm putting the highest confidence in something that I have no actual evidence except for the fact I gave equal credence to those four tests when I created that artificial combination number five.

I said I'm going to treat them equally because I weighted them as a linear average. Just took the results, added them together and divided by four.

DR. GAUNTT: So let me see if I can help.

CHAIRMAN STETKAR: And - hold on a second. My concern is constraining the uncertainty. If you took the four test results and gave them each 25 percent weight, when you got done with the whole process I'm not sure if the mean would come out close to the linearly weighted average because I don't understand all of the complexities.

But it would seem that weighting those four equally 25 percent would preserve the inherent uncertainty that you've essentially accepted.

DR. GAUNTT: I think - I think we believed there's less uncertainty than randomly picking them up, so four. And if I could just explain what I think combination number five I think is our current best belief about cesium and iodine behavior based on the Phebus tests.

Now, one of the Phebus tests, FPT3, is an outlier because it makes use of a boron carbide pellet type control assembly which we don't use in BWRs. They're apparently used in European reactors.

So the impact of that FPT3 test was they got a lot of oxidation with this boron carbide and as a result they got a lot of chemistry between boron compounds and cesium compounds. Somebody help me get this right.

But it basically tied up all the cesium between molybdate and borades that allowed the gaseous iodine - the elemental iodine - to come up. So they saw quite high gaseous iodine in FPT3.

We think for American reactors that is kind of an aberration because we know - we know in the BWRs based on our experience that the boron carbide and steel in our bladed control assemblies becomes liquified very early, runs down and doesn't produce all of this boron oxidation that they saw in FPT3.

So sort of the collective knowledge across all of the Phebus experiments - maybe collective suspicion is a better word - is the old belief was cesium and that's probably a combination number one - cesium would be a hydroxide or iodide and basically tie up all the iodine essentially with cesium.

That's why you have 97 percent. Three percent gaseous iodine kind of follows the source term, the regulatory source term construct, and the balance being cesium hydroxide.

Phebus suggests that the principal form of that cesium is not hydroxide but a less volatile molybdate form and so that's why we weight molybdate -

MEMBER CORRADINI: Say that last part, Randy, again. I didn't understand the last part. Can you repeat that part?

DR. GAUNTT: Yeah, I know. This is -

MEMBER CORRADINI: I got the - I got yeah, you believe combination five makes the most sense.

DR. GAUNTT: Yeah.

MEMBER CORRADINI: But then you started

talking about a molybdate behavior.

DR. GAUNTT: Because aside from that outlier in FPT3, the general theme coming out of Phebus is that cesium is not the hydroxide form that we thought.

It's a less volatile molybdate form and so that's why we weight the molybdate higher in combination five. We actually believe combination five is closer to reality.

But in order to preserve some possibility of the, you know, original thinking combination one has cesium hydroxide, 3 percent gaseous iodine and the balance tied up as cesium iodide. That's kind of the old - the old school picture.

MR. FULLER: This is Ed Fuller. I want to take things a little further than what Randy just did. From my understanding, talking to people like Mike Salay who's been communicating with Dana Powers, it's actually more complicated than that.

Yeah, the cesium was released as cesium molybdate and that the cesium and molybdate seem to be deposited at the same locations in the RCS.

They are deposited at temperatures that are too high for cesium hydroxide to stay deposited. However, in the Phebus fission product revaporization tests much of the cesium is revaporized at temperatures consistent with cesium hydroxide chemical form.

So it's presumed that cesium hydroxide is formed later on from chemical reactions between the cesium molybdate and steam, and people's best estimates are that maybe somewhere between the quarter and the third of this cesium ends up in this form as it transports through the - through the rest of the reactor.

Now, I'm no expert either on this. However, what I just said makes sense to me. So I believe that the right way to do this analysis is to come up with a model that models the chemical reactions and the subsequent revaporization.

MEMBER REMPE: I have a couple of questions and one comment. I actually was looking at the original report and I was going to try and find the references and the references cited in the April version are incorrect so you need to fix that.

But remind me about the Phebus tests. Were they done for PWR-specific conditions or BWR-specific conditions or would you - if you were doing this put the same distribution for both Ps and Bs?

DR. GAUNTT: The Phebus tests were quite largely PWR-centric tests, even the one that had the boron carbide because it uses a very different form of boron carbide than we use in the BWRs. So we really can't compare FPT3 to the U.S. for - on that account.

MEMBER REMPE: But then you're telling me that probably you've generated a distribution that - because of lack of data you would apply to Ps and Bs both but you think it's less relevant to Bs? Is that what I think I heard you just say?

DR. GAUNTT: I think as far as - yeah, the speciation of cesium and iodine I think they are largely the same in Ps and Bs.

MEMBER REMPE: So you wouldn't see any difference then?

DR. GAUNTT: Yeah.

MEMBER REMPE: Okay.

DR. GAUNTT: Based on the - based on the Phebus stuff. We preserve the old school thinking of cesium hydroxide and cesium iodide and that's combination number one, and I guess we ascribed the 25 percent to that. I forget now.

DR. GHOSH: Twelve and a half.

DR. GAUNTT: Twelve and a half. But that's principally why combination five is higher weighted is -

CHAIRMAN STETKAR: But here it's in my mind, because I'm not a chemist so I can look at it this way, it's somewhat similar to the discussion we had about the safety relief valves. You took four tests. The values for combination number five are made up.

I understand - I understand what you're saying about some of the chemistry, why you believe it might work that way. But the reason that combination number five has an iodine - gaseous iodine fraction of 2.77 percent is it is affected by that combination number four, which you say you don't believe in. It's FPT L3 - FPT3.

DR. GAUNTT: No, I don't think the 2.7 - the FPT3 I think gaseous iodine as high as 20 percent or higher was observed.

CHAIRMAN STETKAR: I don't know.

DR. GAUNTT: So the numbers that are in the range of like 3 percent, 2.7 percent I - and again, somebody help me but I think -

CHAIRMAN STETKAR: But how - Randy, the way that that 2.77 was calculated is if you add up .03 plus .002 plus .00298 plus .0757 and divide that sum by four, you get precisely .0277.

DR. GHOSH: Right. So okay.

CHAIRMAN STETKAR: That's how that - how you would -

DR. GAUNTT: You could be right.

CHAIRMAN STETKAR: I am right.

DR. GAUNTT: But it's - but it's like 3 percent. It's the - it's sort of the standard assumption of gaseous iodine in -

CHAIRMAN STETKAR: But my point is if the experts wanted an uncertainty distribution to characterize the way the experts felt about iodine and cesium percentages and form, then why didn't you just create the uncertainty distribution? Why did you add numbers together and then do all of this silly weighting?

MEMBER BLEY: You've said this several times and I guess I want to rephrase the question because what I hear from you, John, is pick the - pick the one you think is most right and put all your - put that as 100 percent.

Well, that's not the right way to go if you think there's some possibility the others might exist under some conditions that could occur. I mean, there's pretty complex stuff going on inside there that we haven't actually watched and -

CHAIRMAN STETKAR: But I'm not saying pick the one that you think is right and put all your weight there. I'm saying develop a distribution that spans your state of knowledge, given everything you know.

MEMBER BLEY: That's good.

CHAIRMAN STETKAR: And everything you know is characterized by those four tests plus everything else you know.

And it's not - if you don't assign any credence or you assign very little weight to the fact that that one test is relevant to this particular issue for U.S. boiling water reactor then it's okay. You can assign a non-zero weight to it so it might be that way.

You don't want to discount it completely. But you've assigned it equal weight to everything else and then created something else that you're assigning -

MEMBER BLEY: This doesn't seem to be what you say is your stated knowledge.

CHAIRMAN STETKAR: Right. That's right.

DR. GHOSH: But can I - can I insert here?

CHAIRMAN STETKAR: You're still assigning 12.5 percent probability.

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: That's the way the world will work.

DR. GHOSH: Okay. So -

DR. GAUNTT: Tina, do you want to -

DR. GHOSH: You can go first if you -

DR. GAUNTT: No, I just - I've got my own view on it but I don't - I don't want to - I don't really remember the numerics that went into generating this table. But at least until the revaporization stage that Ed was talking about we believe the baseline behavior of the cesium is molybdate and cesium iodide and we give a little - a little bit of a fraction to gaseous iodine because there's always some gaseous iodine hanging around.

And so we would normally lead with combination number five and I think some of the - some of the other combinations here, in particular, one, is to preserve the possibility that the old school thinking, you know, is see what would - see what would be the implications of, you know, the old view on behavior of a cesium.

CHAIRMAN STETKAR: But, see, in a different format I would then quiz you well, okay, you wanted to preserve that but how confident are you that that's a reflection of reality?

MEMBER REMPE: I think at this point -

CHAIRMAN STETKAR: Right now you're assigning 12.5 percent confidence that that's the reflection of reality to something that you want to preserve and equal confidence to FPT3 which you're saying you don't really have a lot of confidence in.

DR. GAUNTT: I don't think FPT3 is represented in this amalgam. I think it's combination number four.

MEMBER REMPE: But I think, again, they're not going to redo it. So all you want really right now isn't it, John, just to have them document differently? I mean -

CHAIRMAN STETKAR: Right. Right. Right. This is - I'm not going to understand - this is another issue of essentially trying to put too much reliance on something that somebody else did and saying well, we won't take any ownership for this. We -

MEMBER REMPE: And if they document it differently.

CHAIRMAN STETKAR: We'll acknowledge - we'll give equal weight to all of these things. We don't have to own this. We'll create this other thing that's just a linear average of them and you did take the ownership of putting 50 percent probability to that.

But if you really think it's 95 percent probability, in terms of a realistic assessment of the uncertainties that's all I'm trying to look for.

I'm trying to understand the thought process that went into the development of these uncertainty distributions.

DR. GHOSH: Right. You know, I see that there is a bit of a communication barrier between what, you know, we did and what we think we explained and how it's coming across.

I think for us it's true that we explain kind of mathematically how we came up with both the bins in terms of the fractions of the iodine and the cesium speciation and then the weights that we assigned to the bins.

But implicitly, I mean, my understanding is everything we've decided to go with we have used our collective engineering judgment to say that this is a pretty good representation of the uncertainty that we think is out there.

So yes, for the most part we think that cesium molybdate would be the dominant species but there is some uncertainty about whether cesium hydroxide shows up.

In the newer experiments, which I think were not available at the time that we were finalizing this distribution, it seems that you have this late phase - some late phase of cesium hydroxide observation and we've captured the potential effect of that and the distribution that we did come up with what the exact weights should be for the particular fractionation.

I mean, we don't have a whole lot of confidence about that but we are pretty confident that we have captured a good range of the behavior and in terms of - so that's one point.

In terms of this last bin where we averaged the other four and your example of the coins, you know, I think that the question there is have we done something incorrect because perhaps there's only three discrete possibilities that you can have a quarter or a nickel or a penny and we've somehow created this artificial construct in having a 39-cent possibility. But - and anybody correct me if I'm wrong.

I think when it comes to the iodine fractionation, for example, it's a continuous - it's kind of a continuous possibility so I don't think we've done something incorrect by averaging that possibility.

So it's not that it would be the 8 percent or 3 percent or a 0 or 1 percent. Rather, it's something between zero and whatever percent.

So I don't it's physically incorrect to have just - to have a measure of what we think is most likely to have averaged over the evidence that we do have. And if, you know - again, and maybe we're not clear about this in the report.

Three percent comes out as kind of the rough number that people take to be a good guess for what the actual fraction of iodine would be if you had to pick one number.

CHAIRMAN STETKAR: Okay. But then I would say, you know, from - I would cut you off there and say if that's the case why don't you use combination one but just instead of assigning a 1.0 to cesium hydroxide move one point over to cesium molybdate.

You know, if that's what - if that's what you're using for justification that the 2.77 percent is close to the 3 percent, you know, gaseous iodine.

DR. GAUNTT: Looking back on this in retrospect, I almost - what I think we were trying to do here is to represent alternative degrees of belief. If you ask another expert they might have said well, I think it's going to be this and I think that was the train of thought we were on.

Were I to do this over today I think I would - I would prescribe maybe one source term and populate, you know, populate all of these to some extent to try and represent.

CHAIRMAN STETKAR: Or you could have used four and assigned different weights to each of the four if you wanted to retain some small likelihood that FPT3 is - might be the way the world works.

DR. GAUNTT: Yeah, the -

CHAIRMAN STETKAR: But creating that -

creating that fifth and assigning most weight - remember, if I sample things 100 times that fifth one is going to get sampled 50 out of those 100 times.

MEMBER REMPE: Again, I guess I -

CHAIRMAN STETKAR: And it might be an okay representation for the way that you understand the world to work. But the story about how it's developed doesn't quite hang together very well.

It's too much I took a number from here and I didn't - I sort of felt okay with the overall results and - but it relies basically on addition and division rather than engineering justification.

MEMBER REMPE: I guess I - also, I'm still questioning about the BWR versus PWR because basically isn't it - I'm reading your report.

They're talking about how the behavior of iodine chemistry with respect to paint, sweated surfaces, buffered and unbuffered pools, undergoing radiolysis - these things I think would be - is that what's driving the chemical form of iodine and cesium is the things like paints in the building and -

DR. GAUNTT: Well, not so much - not so much in the accident phase. The iodine chemistry in paints and all of that stuff is longer term behavior of iodine in the containment. It's also on the research forefront. We don't have models yet in hand.

MEMBER REMPE: Because, I mean, combination one is due to steam condensation on the painted condenser is what they're saying xo if I read what's in this NUREG.

And so I'm just kind of wondering, again, based on what I'm reading I'm wondering well, maybe you should have looked at Peach Bottom and decided which geometry was most representative too and was that done at all by the experts or they said nah, we're just going to go with something we believe feels right because of the way the experiments were performed and what was in the experiment?

It wasn't the geometry of the experiment and painted surfaces or pools in the experiment. All they can - you know, I just am wondering why they didn't.

DR. GAUNTT: I'm going to have to go back, Joy, and look at what you're reading because I'm not recalling that.

MEMBER REMPE: It's in the NUREG - the 70 analysis NUREG. Yeah, again, documentation is really important because if we can't remember a few months - about a year from now.

MR. OSBORNE: Can I make a comment here?

MEMBER REMPE: You bet.

DR. GHOSH: Yeah, please.

MR. OSBORNE: Yeah. The quote on steam condensation on painted condensers and absorbed processes and other containment surfaces or both that's that 0.3 percent plus or minus 0.16 percent. That does not pertain to the 3 percent that we used for combination one.

MEMBER REMPE: Okay. But then even in combination two, again, they're talking about the gaseous - okay. So basically then it's not the geometry for the cesium. It's cesium iodide and the cesium molybdate. It's not the geometry of the test. It's just whether the fuel -

MR. OSBORNE: That was actually - whether it was the first or second oxidation phase in which we believed - which of those oxidation phases we believe best represented the iodine concentration.

MEMBER REMPE: And when you say the oxidation phases it's based on -

MR. OSBORNE: From the Phebus experiment. If you look at combination -

MEMBER REMPE: Is it the fuel or is it - what is it that's driving it? What is the oxidation phase? Is it the zircaloy used in the cladding or what is it?

MR. OSBORNE: My understanding, yes. It's the - it's the zircaloy oxidation phase.

MEMBER REMPE: Okay. So then did Phebus use the same type of zircaloy for all tests?

DR. GAUNTT: Pretty sure.

MEMBER REMPE: So why would the results be so different then or somewhat different?

DR. GHOSH: This is the first versus the second oxidation, Doug, you were saying?

MR. OSBORNE: I'm not exactly sure. I just worked with someone that was - that was intimately familiar with the Phebus test and he provided me the peak iodine concentrations for the first or second oxidation phase because I guess some of these experiments you didn't have a single oxidation occurrence.

And then trying to wrap our heads around, as Randy was pointing out for FPT3, they had a much higher oxidation or release of iodine observed for the main oxidation phase, which we didn't really think was appropriate.

MEMBER REMPE: Okay. So like combination four had like 7.5 - over 7.5 percent of iodine and actually iodine that is in the gaseous phase will drive the dose.

And so even though the painted surfaces and stuff like that may not be that important it actually could be even though we're just talking about the gaseous iodine phase, right?

MR. OSBORNE: Right, but you're talking a very low percentage from my understanding as far as what you're seeing from those surfaces coming on and off paint, and also from my understanding those particles are extremely small and they just kind of reach an equilibrium within containment going in and out of the - out of the paint surfaces.

MEMBER REMPE: And the expert you talked to thought it was relevant for BWR geometries to knowing a USBWR geometry. Is the expert from France?

MR. OSBORNE: No. Actually, he used to work here at Sandia but he doesn't work here anymore.

DR. GAUNTT: I think maybe he's talking about Casey. I don't know.

MR. OSBORNE: No. Greg June.

DR. GAUNTT: Oh, right. Okay.

MEMBER REMPE: Okay. Again, I think documentation would help big time on what was picked to be the most relevant and why.

MR. OSBORNE: The only thing else I'd take into consideration it's not just iodine we're looking at here.

We're also trying to look at the interplay between cesium hydroxide and cesium molybdate, which is why combination two and four you have a - you know, you have a low and high iodine concentration and then a 50/50 split between cesium hydroxide and cesium molybdate mainly because we're just trying to see if something were to show up during the uncertainty analysis that, you know, may be important there.

MEMBER REMPE: Okay.

DR. GHOSH: I think we talked about we did find that for the high temperature scenarios like the main steam line rupture scenarios when we sampled the cesium hydroxide form we got actually lower releases, which was counter intuitive, you know, from what we were originally thinking because of the chem absorption on the upper internals, which was kind of interesting. And we - and we ran that down to make sure that that was a legitimate result and apparently it is.

MELCOR has this chem absorption model and it occurs for the cesium hydroxide and not for the molybdate because we were thinking with the molybdate you'd always have lower releases.

But in fact with the chem absorption at high temperatures you have - for the high, high temperature scenarios you have less release with the cesium hydroxide which is very interesting. It's an interesting finding.

Were there any more questions on the CHEMFORM or should we move to the next parameter?

MEMBER SCHULTZ: I guess just one more comment. Perhaps it reinforces what John said but in what you want - what you want to present associated with the selection of these combinations and especially the determination for combination number five is that - the feature of engineering judgment that was used.

You have some data that is there but in terms of developing then the uncertainty features of each of the combinations or the characteristics of each of the combinations that's - was just stated engineering judgment in order to derive - in order to drive the understanding of what the situation could be different sets were selected.

Combination of requirements completely I presume - not completely - it is - it represents the engineering judgment of some consensus that what we ought to do is take averages for the iodine and we ought to also create the molybdenum cesium aspect as one versus hydroxide as zero.

That's fully engineering judgment and one hopefully that would represent some determination by the team that one would assign that - the combination number five will be a .5 weight.

So that ought to be explained somewhat differently than at least the presentation would imply.

DR. GHOSH: Okay. Okay.

CHAIRMAN STETKAR: Yeah, I'd echo that. The summary that we heard here today orally made a lot more sense than what you can read in the report.

If you read in the report it just says we took these four tests, we weighted them equally, we created a fifth, we gave that four times the weight of anything else and that's it, and that's what we used.

DR. GAUNTT: So we maybe should revisit that.

DR. GHOSH: Yeah. All right. So we'll read the transcript of this meeting -

CHAIRMAN STETKAR: It's important. Again, it's important to document what you did and why you did it and why you feel confident, and after we're all gone somebody's going to pick up this report or even if we're still here somebody's going to pick up the report and say oh, in NUREG/CR-7155 they used this distribution and gave it this weight, therefore that is the way the world works. That's the way the NRC has done things. Twenty years from now -

MEMBER SCHULTZ: Which is here in the discussion we heard well, we left in combination one in its form as shown here because it allowed us to explore that aspect of the uncertainty distribution. We may or may not have believed that at all.

But in order to explore it we left it there. But it's - he inputted or determined no, that's not real - everything else is representative but that's not real - we'll keep it out.

CHAIRMAN STETKAR: Well, but again in terms of characterizing the uncertainty which is the whole - we're not chemists but in terms of characterizing the uncertainty the study says that the collective wisdom believes that 12.5 percent confidence they would - they would make that bet with those odds that that's the way the world works and that's what it means here.

MS. SANTIAGO: We'll explain it. We'll work on that.

DR. GHOSH: Okay. The next parameter was the dry well liner failure area. I think one of the questions had to do with water.

MEMBER CORRADINI: Well, that's already been answered.

DR. GHOSH: Yes, that's been answered.

MEMBER CORRADINI: There is none.

DR. GHOSH: Right. That's right. Because this is -

MEMBER CORRADINI: And the second part is I want - I know we're not going to get through all the pieces so I'm willing to stipulate I understand why you have the blue triangle where it is.

DR. GHOSH: Okay.

MEMBER CORRADINI: It's the pressurization rate and the fact there's no water.

DR. GHOSH: Yes. Right.

MEMBER CORRADINI: And let's move on.

DR. GHOSH: Okay. Okay.

MEMBER CORRADINI: Otherwise, we're not going to get to the fun core melt ones.

DR. GHOSH: Okay. All right. So the next one is the battery duration. I think this is actually a combined question, both a question of why did we pick the distribution that we did pick and another one had to do with the timing of the operator action.

So we did provide you a table that shows the timing of the operator actions that are assumed and why that's based on the EOPs and, you know, our discussion with the plant personnel as part of the original SOARCA project.

For the distribution of the battery durations, this was largely - the upper end was largely based on discussions with plant personnel and the lower end is the tech spec section.

So according to tech specs you always have to have a minimum of two hours of battery and so in essence if you - if you have the worst case where you just have the battery, it's getting old, you don't do any load shedding, et cetera, et cetera, you're required to have at least two hours of battery life.

And on the upper end - we got a lot of questions about this after the Fukushima accident - the upper end is capped at eight and that's largely because of discussions that we had with plant personnel in terms what they thought would be the - a distribution for the potential battery life.

DR. GAUNTT: What - okay. Go on.

DR. GHOSH: And what it should represent is that the operators have done an effective load shedding and it's - maybe it's a newer battery. So, you know, it lasts longer.

The four hours is kind of a nominal - you know, what the expectation is. But, you know, the collective wisdom is that it can certainly last a lot longer. So anyway so that's kind of -

CHAIRMAN STETKAR: Collective wisdom based on what?

DR. GHOSH: Based on - oh, so if you had like the two-hour battery life that's required and you - and you have effective load shedding. So if you have an -

CHAIRMAN STETKAR: What - well, no. With effective load shedding I can get to four. So this is super effective load shedding or is it shedding more loads or -

DR. GHOSH: There's also - so there's a couple of points. It's kind of when you do the load shedding. So I think for - to get to the four hours you had to do the load shedding by a particular time.

CHAIRMAN STETKAR: And what is that time?

DR. GHOSH: I think if you start - we put that in the writeup.

CHAIRMAN STETKAR: I think it was one hour.

DR. GHOSH: I can look it up for you. We put it in the writeup. But yeah, there's an assumed time. So there's some chance that when you recognize you're in a station blackout, you know, scenario you may start to do earlier load shedding.

But also from what I understand there's just natural variability depending on how old the battery is. So if the batteries are newer they should last quite a bit longer but, you know, there's a - there's kind of a battery lifetime in terms of, you know, as it ages if it's not going to last as long.

So there's some natural variability with regard to at the time of a potential accident what the - you know, what the kind of nominal battery life would be with the assumed load shedding. So that's where the upper end of the distribution comes from.

And as you know - so, again, this is the unmitigated case. You know, in our scenario basically RCIC duration is tied, you know, one on one. It's deterministic based on the battery life.

We got a lot of questions about the fact that at Fukushima you had RCIC running a whole lot longer, you know, than eight hours. Eight hours is pretty short.

But, you know, we stuck with our original, you know, scenario as we had defined it and this is the unmitigated long-term station blackout case.

CHAIRMAN STETKAR: I just observed that the best estimate or the expected value of the duration of the batteries is 4.2 hours from this distribution.

It's about 10 minutes longer than the four-hour value that they say they can get with effective load shedding, whatever effective load shedding is.

So that that additional four hours in this distribution makes a big difference.

DR. GHOSH: But that -

CHAIRMAN STETKAR: It's a big difference.

DR. GHOSH: Okay. For the expected four hours, again, I think that's based on - that's almost like meeting a safety criteria.

So taking a conservative guess at - to take a conservative estimate oh, the battery might be old and, you know, how long can you -

CHAIRMAN STETKAR: Okay. Let me - let me try this. I've talked to a lot of plant operators and met these people and they're always optimistic about how good stuff is.

They're always optimistic about how good stuff is. Stuff always works better and lasts longer and costs less than it does in the real world.

In this particular case, you are using from what I'm hearing without real engineering analyses or data word of talking to people at the plant to double the life of the battery.

And there might be justification for that if indeed they've actually tested battery - their batteries.

DR. GHOSH: Yeah, but -

CHAIRMAN STETKAR: And if they haven't I'm curious about what that is. Now, the question is, again, I don't care what the distribution is if the differences don't make any difference.

But in this case if you said that the upper bound of the battery life was 4.5 or 5 hours rather than 8 hours so you had a mean duration on the order of, I don't know what it would be, 3.5 something like that.

How much difference would that make to the overall results? You'd essentially have RCIC failing much earlier.

DR. GHOSH: Yeah. The - you know, it's interesting because going into this study we thought that the battery duration was going to matter a lot. It did not show up as one of the top variables in the regression analysis. So I -

CHAIRMAN STETKAR: I noticed that. That was curious.

DR. GHOSH: Yeah. So I don't think the exact distribution matters all that much. But I will tell you this is one of the difficulties but it informed our engineering judgment.

Our understanding was that there are calculations that exist that show the battery life could be as high as 12 hours or more but we don't have any public report to rely on and in the end, you know, when we discuss with plant personnel, you know, what should we use, I mean, it was decided that they didn't want to go above eight hours even though they had calculations that show it could be 12 hours or more internally.

You know, from - I guess from some team members who weren't core team members we had people pushing us to go as high as 16 hours because they felt that the four-hour number is horribly conservative and that there are calculations that exist out there that show batteries can last a lot longer.

But, again, in the end, you know, based on all the information that we had we settled on this distribution and then after we saw that it didn't make much difference to our results. We didn't probe that much further to see, you know, what - to try to come up with a more precise distribution.

CHAIRMAN STETKAR: Were the - one last thing. Provoke someone else over here. Were those calculations that you saw out to 12 hours based on different assumptions about the timing and amount of load that was shed or is that just variability?

Given the fact that you shed loads X by time Y you saw variability of, let's say, four to 16 hours in the expected life of the battery.

DR. GHOSH: Yeah. To be honest, unfortunately I wasn't at those plant visits. So I wasn't part of those conversations to know what went into those engineering calculations.

I don't know if anybody here or anybody on the phone from Sandia knows. I don't think actually any of us who are here today or on the phone went to those visits where we gathered that informal information.

CHAIRMAN STETKAR: Because as I understand it the timing of this is based on a presumption that the operators will successfully shed the required loads at a particular time.

There isn't any uncertainty in the human reliability analysis for the timing of that load shedding or the amount of load that's shed. It's just simply a chunk of load that comes off guaranteed at a particular time.

DR. GHOSH: I think for the numbers that were sampling below four hours in essence we are assuming that they've not done a good job of load shedding and that's why the battery is not lasting as long.

So the two hours representing -

CHAIRMAN STETKAR: If they don't shed any loads it ought to be -

DR. GHOSH: It's two hours, yeah.

CHAIRMAN STETKAR: Yeah.

DR. GHOSH: Right. So two hours is kind of, you know, if they didn't do any kind of load shedding and the battery is getting old you're going to have about two hours before you run out.

So that's kind of the limiting case. Well, oops, we didn't shed when we were supposed to and the battery is old.

It's, you know, two hours is going to be the - what we have and so between two and four hours kind of represents that it's an older battery and maybe the load shedding isn't as effective as it should be.

So that's kind of a, you know, the below the median distribution is. At least in our minds that's the thinking of what that represents.

MEMBER SCHULTZ: But the question is going beyond two hours to four hours and then certainly beyond four hours and it sounds like, as John said, that it is relying upon the capability of the operators and it also sounds like it could be relying upon the excess of other plant equipment to perform flawlessly.

DR. GHOSH: I think it - yeah, you know, the beyond four hours again it's a largely - a lot of it is battery life. So if you have a newer battery it's going to last longer. But the tech spec limit is two hours. So they, you know, they'll run - they'll keep a certain battery for a certain age and it's random when the accident happened.

So you don't know how old that battery is.

So the four hours is perhaps somewhat conservative in that that's the tech spec limit plus the assumed and EOP-directed actions for load shedding.

So the beyond the four hours is, I think, at least as we represented I don't think it's necessarily you're doing anything on top of what the EOP requires you to do.

It's more that you have a newer battery, which, you know, you have some chance that if the accident happens when you have the newer battery so you expect that the battery's going to last longer.

MEMBER BLEY: I think - you know, the only thing I'd be curious about I've seen people associated with plants do some calculations that go out for long times. But they weren't for the accidents you're looking at and they - they just had those calcs. So they were more under kind of normal - not accident.

CONSULTANT SHACK: What the report says is that you got - the NUREG 1150 assume 10 to 12 hours -

DR. GHOSH: Yeah.

CONSULTANT SHACK: - and the licensee said no, we can't see anything beyond eight.

DR. GHOSH: Yeah.

MS. SANTIAGO: They didn't want us to go beyond eight. They could see - they just didn't want us to -

DR. GHOSH: Right. And I think that they did have calculations that supported the possibility. But unfortunately I don't have the details of those calculations and in the end we deferred to their judgment of limiting it to eight, yeah.

DR. GAUNTT: And as far as the analyses goes, what the battery life determines is when the operators lose control of the SRV for pressure control and when they assume - when they assume that the - they lose control of RCIC and the RCIC dies.

CHAIRMAN STETKAR: RCIC doesn't die then. It fills up the steam line and then dies sometime later.

DR. GAUNTT: Right. Right. But that's on the death path. Since you mentioned it though -

CHAIRMAN STETKAR: Yeah.

DR. GAUNTT: - it's another one of those issues like SRV behavior under real-world conditions and it's a thread we're pulling with DOE and EPRI and the BWR owner's group is that apparently this happened in Fukushima unit two, and without battery power the RCIC ran for 72 hours apparently in this flowing up to the steam line and it didn't kill the - didn't kill the Terry Turbine.

So we're kind of very interested in this post-Fukushima post-SOARCA what's the real world performance of critical equipment that we typically assume fails. In the SAMGs we assume you lose battery power and you lose RCIC.

CHAIRMAN STETKAR: Do you happen to know, Randy, did they have the old style Terry Turbine with a mechanical governor?

DR. GAUNTT: I think so.

CHAIRMAN STETKAR: Does Peach Bottom have that kind or, you know, a lot of the new turbines have been retrofitted with electronic governors.

DR. GAUNTT: Yeah. But I think in Fukushima I'm told - and if Kyle's listening I know he knows all about it - I think they were mechanical.

CHAIRMAN STETKAR: Okay.

DR. GAUNTT: And they should have oversped or something like that but the thinking is liquid water carryover got into the turbine and slowed the turbine down and sort of got into the self regulating mode.

We're trying to interest whoever's interested in investigating real-world performance and safety relief balance and RCIC pumps and aux feed steam driven pumps and so forth.

CHAIRMAN STETKAR: Anything else on the batteries?

DR. GHOSH: Okay. So the next one is the SRV open area fraction, and as I mentioned earlier when the SRV fails stochastically we assume that it fails in a fully open position because the failure is the failure to reseal, and when we have a thermal seizure of the SRV in that case we sample an open area because the thinking is that it may - it may seize in some position that's not fully open.

The distribution that we assign to that is skewed more towards the fully open area and this - you know, this came about through extensive discussion both among the team as well as the peer reviewers - our external peer reviewers.

So that's why you see that, you know, the curve is steeper over to the right instead of just the uniformed distribution which is what we originally had. But when we thought about it more it made sense that it would be skewed more towards being fully open.

And in this case, you see that in the SOARCA study they assume that if you had a thermal seizure that it would open - fully open but it was never relevant because this was never exercised in the SOARCA, you know, study because we had the early SRV stochastic failure.

So it didn't get to a thermal seizure. But that's just there for reference.

CHAIRMAN STETKAR: How is this treated - we heard a little bit earlier because, again, I don't understand any of this part of the phenomena. We may or may not have lost or gained a person on the bridge line.

DR. GHOSH: Yeah. Are there still folks on the bridge line at Sandia?

MR. JONES: This is Joe Jones. Yeah, I'm here.

CHAIRMAN STETKAR: Okay. Things happen and we have no knowledge of what's going on. It's an open -

MR. OSBORNE: Doug Osborne here.

DR. GHOSH: Okay, great. And Doug, that last one - okay.

CHAIRMAN STETKAR: In the model if this failure mode is invoked does - how far open does the valve have to be to actually depressurize the reactor? Obviously, if the valve is stuck fully - open stochastic failure mode presumes that it's stuck fully open and it depressurizes. How far open does the valve have to be to do that?

DR. GAUNTT: What I'm recalling - this is the gist of it is what I'm recalling and then I'll ask Kyle to correct me. But if that SRV sticks open at any open position greater than about half it will depressurize quickly enough that you can - you can avoid any main steam line rupture.

CHAIRMAN STETKAR: Okay. So according to this distribution there's an 80 percent probability that that's what it looks like.

DR. GAUNTT: Occurs.

CHAIRMAN STETKAR: Roughly. I think it's 78. Not that I didn't run out the number.

So you're essentially saying that given it does stick it still sticks - it's a three to one likelihood that it sticks open far enough to depressurize.

DR. GAUNTT: You know, I don't - I don't know if I - that's four to one.

MR. ROSS: Randy?

DR. GAUNTT: Yeah?

MR. ROSS: Actually it's 70 percent.

DR. GAUNTT: Seventy percent if it sticks open -

CHAIRMAN STETKAR: Like 72 percent if you want to be precise. I ran out the distribution. That's the - 50 percentile is 72 percent open.

But that's - I'm sorry. Fifty percent open is about 70 - I didn't run out the 50 percent open. Forty-eight percent open is the 20th percentile.

So it's about 78 I guess that's what he said. The - and that's okay. What evidence did you use for this? You actually talked to valve people about this failure mode?

DR. GHOSH: Yeah, and so we had the Division of Engineering at NRC do some work on what a thermal seizure for the SRV looks like and kind of what the temperature of, you know, of when this could possibly be.

Actually, that's also a separate variable. That's the temperature at which it could seize, and then we also talked about, you know, what the open area is likely to be, you know, if it were to seize in that thermal mode.

Maybe I'll defer to Kyle because I think he's our resident expert in this area with respect to the open area fraction.

Kyle, I don't know - if you want to add something go ahead. I'm also going to look up what we - what we've said most recently.

MR. ROSS: Yes. I'd like to - I can't hear the ACRS questions real good.

CHAIRMAN STETKAR: Oh.

MR. ROSS: Voices come in and out. But basically the valve needs to fail late with respect to number of cycles in order to get a failure by overheating and then those are - that's necessary. And then you also need to have a limited open area of the stuck valve for that to lead to a main steam line rupture. Is that - is that -

CHAIRMAN STETKAR: What I was - and I speak up because unfortunately the mikes that pick me up are about as far away from me as they can get.

The curve that we're looking at, Kyle, on - I'm assuming you have the handouts - is on sheet 37 of the presentation. That curve shows a probability distribution for the open fraction.

MR. ROSS: Okay.

CHAIRMAN STETKAR: And that probability distribution essentially says that we're 78 percent confident that the valve, if it fails by this thermal failure mode, will fail open far enough to depressurize the reactor. In other words, that you will not have a thermal pressure reduced steam line failure.

MR. ROSS: Okay. Yes.

CHAIRMAN STETKAR: And what I was asking about is what's the basis for that curve. Did you consult with experts on valve operation, these types of valves under -

MR. ROSS: No. This was MELCOR results.

CHAIRMAN STETKAR: No. This curve is not

MELCOR results. This is an input.

DR. GHOSH: The question is how did we come up with the distribution for the SRV open area fraction when you have a thermal seizure.

MR. ROSS: Okay.

DR. GHOSH: So that's like - it's at Page 411 of our report - I just pulled it up - where we talk about that, and I don't think we added a whole lot in our writeup that we provided.

CHAIRMAN STETKAR: You did not add a whole lot in the writeup that we got last week.

DR. GHOSH: Right.

CHAIRMAN STETKAR: So that's why I was asking.

MR. ROSS: Aren't we just from .1 to 1 on the fraction uniformly or -

CHAIRMAN STETKAR: It's not uniformly.

DR. GHOSH: We have a triangular distribution. I'm realizing now - so okay. This is a to-do for me.

We have more than what we put in the report and also what we gave you last week because I remember the discussion. But I see we did not capture it very well in our writeup.

CHAIRMAN STETKAR: Yeah. I didn't learn a lot by this.

DR. GHOSH: Yeah, I apologize for that. There is a reason that we skewed it towards the more fully open areas because we originally had a uniform distribution and we were given the feedback by multiple knowledgeable people that that wasn't appropriate, that it was much more likely to be a more open area.

You know, in other words, that values that are skewed towards fully open is much more likely than values, you know, towards the lower end and - but I see we did not capture that in our writing. So we need to do that.

CHAIRMAN STETKAR: Good. I mean, that's exactly what I was looking for, Tina -

DR. GHOSH: Yeah, I apologize.

CHAIRMAN STETKAR: - is that whatever basis you had for the shape of that distribution. You know, I don't care whether it's log uniform or -

DR. GHOSH: Sure.

CHAIRMAN STETKAR: - why it's skewed the way it's skewed. But it's certainly skewed toward higher probabilities of a big enough open area to depressurize here and there must -

DR. GHOSH: Right.

CHAIRMAN STETKAR: - must be some better

documentation for that.

DR. GHOSH: Yes. I apologize for that.

CHAIRMAN STETKAR: No, that's fine.

DR. GHOSH: I have it in my notes and I have to - I have to put it in there for -

CHAIRMAN STETKAR: That's - I trust that you do. I hope you put it in the report.

MEMBER REMPE: The analysis was done assuming a skewed distribution.

DR. GHOSH: Right. Right.

MEMBER REMPE: Okay.

CHAIRMAN STETKAR: Yeah. The analysis was based on the curve that we have on sheet 37, right?

DR. GHOSH: But the thing that Doug pointed out and I'll just repeat that is the - that even open areas as high as 70 percent could result in main steam line ruptures.

When we - when we did the sensitivity studies in the SOARCA, you know, project in 7110 we had a section on sensitivities that just had a limited number of sensitivities to look up the effect of main steam line rupture area, and there we had - we had used a fraction of 10 percent of open area to see, you know, if you have 10 percent that you could get main steam line creep rupture and at the time when we first started the uncertainty study we were thinking that only very small open fractions could lead to main steam line ruptures.

But we found through the more comprehensive uncertainty study that in fact even with much larger open areas you can still get main steam line rupture.

CHAIRMAN STETKAR: You said as large as 70 percent open?

DR. GHOSH: Yeah. Isn't that what you said, Doug?

MR. OSBORNE: That's correct.

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: Okay.

DR. GHOSH: So we will - we will definitely add something on that. Should we move to the next one or is there anything else -

CHAIRMAN STETKAR: Unless anyone has any other other questions about the valve.

MS. SANTIAGO: So we're on slide 38 now?

DR. GHOSH: Right. So this now is the - this is the area fraction of the main steam line when you have a main steam line rupture.

And here I think we did explain much better why we skewed the area so much more fully open and there's some experimental dates I think from Europe for saying why we think it's more fully open.

We had a pretty detailed discussion on this with the external peer reviewers and I think we provided you some of that discussion as well.

CHAIRMAN STETKAR: Well, the only comment I had on this is that there's very high confidence that the - that the size of the opening is quite large here and therefore, you know, in terms of is - in general, you know, our discussion of why did you pick certain parameters for examination in the uncertainty analysis I can't imagine that the uncertainty in this contributes anything to the overall uncertainty because it's presented as a cumulative here. But if you looked at the PDF it would be a very, very narrow distribution with a little tiny little tail.

DR. GHOSH: Right. Yeah, I think - right. It was - if we found something interesting in this small percentage of cases where we have a small opening, right.

CHAIRMAN STETKAR: Okay. Okay.

DR. GHOSH: Okay. Slide - so the next one is on slide 39 is the radial debris relocation time constants and the next two slides kind of go together. So the first one is for the solid and the second one is for the liquid on slide 40. So slides 39 and 40.

And, you know, these were - I'll let Randy tag after me but these were an example where we tried to pick a reasonable range of variation around what is sort of the best practice, you know, value that's typically used in MELCOR models and we thought it was reasonable to go up by - go up and down by a factor of two and we thought yeah, it's plausible that it's - that this constant is only half as big. It's plausible that's it's twice as big. Maybe hard to imagine a lot more variation beyond that and we thought it was equally likely that it would be less or more.

So we picked a triangular distribution with the peak at the mode, which is sort of the best practices MELCOR value, and then with the values on either side being equally likely and kind of falling away in terms of how likely it is when you get further away from this nominal value.

So that was the thinking behind the next two distributions for the solid and the liquid radial debris relocation time constants.

MEMBER REMPE: So what do you do? You run the code and you use one value and then you double it and you run the code again and say well, maybe that's possible the relocation could occur that way in that amount of time?

I mean, there's no data so that must be - so you just kind of run the code for a particular scenario and then you've applied this distribution for all the scenarios?

DR. GAUNTT: We've used these nominal values for a long time and they're just engineering gut is what I would say, and I'll invite Kyle if he knows any more rationale behind why these numbers are the way they are.

MR. ROSS: Well, I mean, not the absolute values of the numbers but I might add that we did correlate them. So we didn't - so in doing the random sampling we didn't wind up with solid material that relocated radially faster than liquid material.

CHAIRMAN STETKAR: Good, Kyle. That's - I didn't find that in the report, by the way, because it's one of the questions that I had was I have no fundamental physics knowledge of what's going on here but it seemed like they ought to be correlated. And you said that you did correlate them?

MR. ROSS: We did, yeah.

CHAIRMAN STETKAR: That ought to -

MEMBER REMPE: What about ex-vessel? Is there any correlation between how you assume spreading and relocation in-vessel versus spreading ex-vessel or is this -

MR. ROSS: There were two similar variables for ex-vessel that we - that we also correlated.

MEMBER REMPE: Okay. I don't recall that being in the report either. Maybe I missed it but -

DR. GAUNTT: But I don't know, Joy, if your question was did we correlate in-vessel spreading time constants with ex-vessel spreading time constants.

MEMBER REMPE: Yeah. That was part of my question too. Is there any - I mean, I think it depends on composition and my engineering judgment a little bit too and it seems like that there ought to be some relationship.

But, again, I think you are basically running the code and saying yeah, that's a reasonable value still too is what -

DR. GAUNTT: Yeah. It's actually an area where we're looking at some code improvements with Argonne code called MELTSPREAD.

MEMBER REMPE: Ex-vessel, not in-vessel.

DR. GAUNTT: That's ex-vessel. So Kyle, do you have anything else to say about the ex-vessel?

MR. ROSS: Well, the - so those parameters aren't especially to the variables for in-vessel. They're - they are parametric variables in the - in the logic that spreads material on - across the concrete floor that isn't especially mechanistic actually.

DR. GAUNTT: Kyle, help us here. It does consider temperature and debris height, right?

MR. ROSS: Yes, that's right. Those are - those are the two variables that it worries about.

DR. GAUNTT: So if it's a big pile it's going to tend to want to spread out and the hotter it is it's going to tend to want to spread out?

MR. ROSS: That's right.

MEMBER REMPE: I guess - again, it's been ages since I've read this and you're right, it does have the - it does relate to temperature in the writeup. But it wasn't clear to me - there aren't any equations and I think that's still true.

There aren't any equations - it's just somewhere is - if I went to the manual would it say okay -

DR. GAUNTT: Well, there is an equation - I'm sure Kyle can help us out here - but it's not looking at the composition, for example.

MEMBER REMPE: But it does look at the temperature and the height and -

DR. GAUNTT: Does look at the temperature and the head height that would drive spreading.

MR. ROSS: Yeah, that's right. And I don't - I don't recall that we've ever tried to put the relationships that are in the model to the document.

MEMBER REMPE: Oh, so this isn't a user input entirely. It's something that - it's some sort of correlation in the code that looks at the temperature and the height and then it picks a value somehow or other?

MR. ROSS: No. It actually is a user specified from end to end.

MEMBER REMPE: But the user knows to look at the temperature?

(Simultaneous speaking)

MR. ROSS: -- by MELCOR.

DR. GHOSH: But in terms of when it happens it also depends on the height and temperature, right?

MR. ROSS: That's right. MELCOR is coming up with the height of the debris. That's right.

MEMBER REMPE: I'm sorry. I'm getting a little confused. So you put in a user input value for the radial debris relocation time.

MELCOR has some sort of correlations then and modifies it based on temperature and debris height. Is that what I'm hearing? Or -

DR. GAUNTT: I think the in-vessel spreading behavior is distinct -

MEMBER REMPE: Okay.

DR. GAUNTT: - from the ex-vessel.

MEMBER REMPE: Okay. But for this particular one does the code modify it based on the user input based on -

DR. GAUNTT: In-vessel.

CHAIRMAN STETKAR: This is in-vessel.

MEMBER REMPE: Okay. Does the code modify what - the user specifies this value normal - a point estimate, right?

DR. GAUNTT: Right.

MEMBER REMPE: And then the code modifies what's used based on the temperature of the debris?

DR. GAUNTT: Ex-vessel is different from this.

MEMBER REMPE: Okay. So in-vessel the code does nothing?

DR. GAUNTT: In-vessel the code uses that time constant.

MEMBER REMPE: And that's all it uses?

DR. GAUNTT: And that's all it uses.

MEMBER REMPE: There's no modification? I thought somehow or other - I guess I brought up the ex-vessel so I got what I deserved. But I got confused that there was -

DR. GAUNTT: Now you're challenging my memory.

MEMBER REMPE: Okay. But there's nothing - the user has no values on temperature on this?

DR. GAUNTT: It won't move material, I know, unless there's a difference in height in-vessel.

MEMBER REMPE: In-vessel.

DR. GAUNTT: So there's got to be a difference in height, and then how quickly it levelizes everything is determined by this time constant.

MEMBER REMPE: Okay.

DR. GAUNTT: And more than that I'd have to dig in and, you know, try and tell you better what it does.

MEMBER REMPE: Okay.

MEMBER CORRADINI: So I'm late to the party but so what test - if somebody said to you I'll buy off on this, excuse my English, integral fudge factor what test do you point to that says we fudged it so at least the test is close to this?

DR. GAUNTT: I'm not aware of any test that -

MEMBER CORRADINI: There's not even the old - I thought long ago memory served me - you know, I'm forgetting that I thought early in the development there were two or three tests in ACRR that are about the only tests about core melt progression. Am I remembering wrong?

DR. GAUNTT: Well, I probably did those tests but I don't remember connecting them to this parameter.

MEMBER CORRADINI: So this parameter was born long after and wasn't used to model anything historical to make sure that things were reasonable?

DR. GAUNTT: I think -

MEMBER CORRADINI: Do you see what I'm getting at? This one I can imagine. I figure you guys were talking about the in-vessel one. To me this is really witchcraft, excuse my English, but engineering witchcraft.

But in some sense it's a judgement and then I'm just trying to figure out how you felt the judgement was reasonable. Was it - I thought it was - eventually you tried to at least do some sort of experimental or some calculation relative to an experiment, I thought.

DR. GAUNTT: Memory is failing me.

MEMBER CORRADINI: Okay.

DR. GAUNTT: If there was.

MEMBER REMPE: Out of curiosity, did you ever try and run the code with some value that was way beyond and see what the code does?

DR. GAUNTT: Guys, help me here. I think that - I don't know if recall this clearly or not. But

I think there's some numerical stability issues that might be associated with these numbers.

But I don't - the - like that 360 we've used that for a very long time and I'd have to try and dig to find out where - what the - what the reason for that was.

We have the liquid melt equilibrating faster than, you know, a big pile of debris. So at least that's kind of an intuitively correct order of things. But after that I can't tell you.

MEMBER CORRADINI: I just couldn't remember. I'm sorry.

CHAIRMAN STETKAR: Anything else on these?

MEMBER CORRADINI: Thank you.

CONSULTANT SHACK: There's a reference to a Siemens report but I can't actually find a reference to it. It just says Siemens uncovered sensitivities here and explored ranges.

CHAIRMAN STETKAR: That's in what we received last week.

DR. GAUNTT: Is that the - is that in reference to the hot leg?

CONSULTANT SHACK: No, it's these time constants.

DR. GAUNTT: Okay.

DR. GHOSH: I believe that's the -

CONSULTANT SHACK: It blends LLNL for the change in values.

DR. GAUNTT: Yeah, I think you're right about that. I do recall that.

CONSULTANT SHACK: And then it - you know, it says something about Siemens but then I tried to look for some report that would connect these things and I couldn't actually find a reference. But okay, nothing comes off the top of your head?

DR. GAUNTT: Maybe we can find that. I'm recalling the Siemens report. It predated our hydrogen study 10 years ago or so.

CONSULTANT SHACK: Oh, so this is, again, a ten-year-old kind of - I thought - it made it sound as though it was something new.

CHAIRMAN STETKAR: Yeah, that's the way it was presented in here and oh by the way there's some -

DR. GHOSH: Yeah. I think that's a reference in the older hydrogen study. We can check that data. I was trying to track that data too. We will - we can track it down. Yeah.

CHAIRMAN STETKAR: Anything else? If not, I'm going to recess for a break because it's probably about time to do that and let's reconvene at 3:00 o'clock.

(Whereupon, the above-entitled meeting

went off the record at 2:39:53 p.m. and resumed at 3:00:03 p.m.)

CHAIRMAN STETKAR: Okay. We're back in session. Next.

DR. GHOSH: Yeah. So the next - to be honest these are - these next two parameters are among the least interesting parameters in the study for the following reason.

We wanted - so the reason we were looking at this in the first place is because when the railroad doors are open you get this chimney effect that can reduce the residence time that you have for the - your source terms.

So we wanted to see that, you know, if you have a different fraction than what was assumed in the SOARCA study would that make a difference.

So we sampled a fraction, an open fraction, for both the inner doors and the outer doors when they blow open, which is the majority of the time, to see if it has an effect and what we found is that if the doors are open it really doesn't matter how much they are open. So these parameters showed up as not important at all.

However, what is important is whether or not they blow open. So what we ended up doing is we created a - when we did our regression analyses we created a paramater that was just an indicator for each realization of whether or not the doors blew open.

We assigned it a new name and, you know, that's based in MELCOR on the pressure calculation. Once you reach a particular pressure your doors blow open.

After that it really didn't matter what the open fraction was because as long as they blow open you get some chimney effect and that's what really matters is whether or not you blow open the doors.

CHAIRMAN STETKAR: What you're saying is this distribution doesn't make any difference at all?

DR. GHOSH: Yeah.

CHAIRMAN STETKAR: Okay.

DR. GHOSH: So both of these. So slide 41 is the inner doors. Oh, actually I didn't even bother producing the two because they had an identical distribution and they really don't show up as important.

So what was a little bit important was this indicator variable for whether or not the doors actually blow open. But then the fraction that they're open really doesn't matter.

CHAIRMAN STETKAR: But the indicator variable wasn't input. That's just - that came out of the calculation?

DR. GHOSH: That's right. Yeah. And that's based on the pressure that -

CHAIRMAN STETKAR: Yeah.

DR. GHOSH: And most of the time they do blow open. Okay. So the next one is the hydrogen ignition criteria. So I don't know if you had a question about this one but this one we struggled a little bit about what more to say.

This is - you know, we revisited what we had written in chapter four and we weren't sure what more to say about this. So I guess I'll ask, you know, what the question might be with regard to this parameter.

MEMBER REMPE: Well, I'll speak up. I'm not sure if anyone else has questions but, again, I'm looking in chapter four from an April version of the report. Is that an old version of it?

DR. GHOSH: It is an old version.

MEMBER REMPE: I'm not - I'll have to dig around and find a more recent one. But in this when you say where flammable sometimes, like in that legend, does that mean that if you have a case where you have too high a concentration of steam that you don't consider using this parameter?

Is that true that you look and see if you have that flammability criteria met and then you apply this?

DR. GAUNTT: The criterion is there but if it's - if it's outside of the flammable, you know, the curve I'm talking about, that triangle -

MEMBER REMPE: The triangle is what I'm thinking about too, yeah.

DR. GAUNTT: Right. So if it's steam inerted or inadequate oxygen or something like that then you don't - a burn is not valid. But otherwise if it - otherwise if it is flammable and you exceed this concentration then it will assume a burn happens.

MEMBER REMPE: Okay. That answers a lot of my questions and I looked at this paragraph again last night in the section four and, again, I think it's just maybe the way I'm reading it or whoever wrote it but it didn't clearly say that and that would have answered my questions.

DR. GHOSH: I think on that one we did try to add some explanation. It may not still get there. We'll revisit it. But it's on Page 425 it starts with the new - the newer version before the July meeting of this year.

MEMBER REMPE: Let me pull up what I have from the July's subcommittee meeting too. Sometimes I get confused on the versions too. But anyway as long as you're taking care of it that's all that's important.

DR. GHOSH: Yeah. We'll revisit that to see where -

MEMBER REMPE: I'm not sure which section in there.

MEMBER CORRADINI: 518 Page 1.

DR. GHOSH: Okay. That's the - that should be the latest. I don't think our - what we gave you a -

MEMBER CORRADINI: It doesn't have a date on the one but -

DR. GHOSH: Yeah. The 518 is the latest version that we did.

MEMBER CORRADINI: There's no date listed. It just says date - transcript completed date is -

DR. GHOSH: But that's the draft, yeah, that we provided you.

MEMBER CORRADINI: Okay.

DR. GHOSH: Okay. I guess if there are no other questions on that one we will move to the particle density, which is the RHONOM on slide 43.

So here I think we had some discussion on chapter four that perhaps elaborated on a little bit in the writeup that we sent you last week and I guess I'll defer again to the - unless, Randy, you want to start with anything I'll defer to the committee members in terms of what the specific questions might be on that one.

DR. GAUNTT: Opening up the question.

CHAIRMAN STETKAR: The only - and my original observation on this is kind of displayed. I understand I think the bases for the upper and lower bounds on this curve - you know, why it's set at whatever it is, 870 and whatever the upper bound is - 4,037.

The original SOARCA value of that triangle there's about a 97 percent probability that the density is greater than the best estimate value and that's what it was called in the original SOARCA study. Is that - is this now the current state of knowledge?

DR. GAUNTT: I would say my thinking on this has shifted and we should be using more like a most probable value here and that - on the 1,000, you know, kind of reflects a very wet aerosol and I guess my thinking is kind of changing on this. But -

CONSULTANT SHACK: Okay. So you would - represents the -

DR. GAUNTT: I think the red line represents the spectrum of real aerosol.

CHAIRMAN STETKAR: Okay. This is something I know nothing about. I can look at curves. I can read words. But I don't know anything about aerosols or aerosol behavior. How much - what's the overall effect of the particle density in terms of -

DR. GAUNTT: Well, the particle density factors into a lot of the deposition rates such as, you know, of course, gravitational settling, that along with the apparent aerodynamic size of the particle.

So it will affect the - it will affect deposition rates. There's a lot of different ones. Diffusiophoresis, thermophoresis - all of these are impacted by these a little.

MEMBER BLEY: This was the distribution used in this.

DR. GAUNTT: The red distribution was used in the uncertainty.

MEMBER BLEY: Did this one have a big impact on the results, SOARCA uncertainty distribution?

DR. GHOSH: This did not show up as, you know, one of the top, you know, three or four. But -

MEMBER BLEY: That seems surprising.

CHAIRMAN STETKAR: I mean, this effectively says you ought to be getting a lot more deposition than the original study showed.

DR. GHOSH: But, Doug or Kyle, I have a vague recollection that this showed up as more important than maybe, for example, the subset of the main steam line rupture scenarios.

Sorry. I guess, Doug or Kyle, if you recall whether this showed up as important in one of the subscenarios when we did the regression analyses for that. Do you -

MR. OSBORNE: Yeah, I kind of remember the same thing but I don't remember exactly where.

CHAIRMAN STETKAR: I mean, it certainly was not highlighted in the NUREG in the report as something that was - that was an important parameter.

DR. GHOSH: Right. That's right. Yeah. So we had done essentially four sets of regression analyses for the source term results.

We did one on the composite distribution of source terms which included all four - sorry, all three subscenarios. And then we also looked at each subscenario to see whether there are particular parameters that contributed more to, you know, that subscenario.

And we can get back to you on this but the - so the RHONOM did not show up as important for the full set but I have some vague recollection that for perhaps the - for example, the main steam line rupture subset the regression analyses showed it as becoming important for that subset of scenarios.

MR. OSBORNE: Yeah, it was - it was meaningful as I remember in one of them. But it wasn't - it wasn't top three.

DR. GHOSH: Right. Right.

MEMBER BLEY: This gives me a chance to go back to the thing that still bothers me from the first meeting and that general statement that the staff, despite the fact that from what you're telling us, in your opinion all of these distributions represented your best judgment of - I'll use the Shack approach words best I can - your best judgment of the state of uncertainty of the technical community about these parameters.

And despite that you had said you had more confidence in the point estimates of the original report, which still leaves me a bit confused, and given - I don't know if Randy's change of opinion on this is reflected by others, this is just one parameter out of a big group.

But it would strike me that things like this if in fact these do represent your best judgments of the judgment of the technical community you still hang on to that thing that you like your point estimates better and you believe them more, and if so try to teach me why you think that.

DR. GAUNTT: Do you want a shot at that, Tina?

DR. GHOSH: Well, okay. So this one - this one is a funny one. We tried to reflect the judgment that at the time that we kind of fixed this distribution and I think Randy's thinking may have evolved since then that this was probably more than a year ago at this point - that at the time we were still thinking a thousand was the most likely.

However, the values lower than that were not very likely and they could go as high as, you know, the 4,037 or whatever that upper end is. And so we used a similar thinking in constructing the distribution and we made a triangular distribution putting the peak at 1,000.

But because the distribution is completely skewed in one direction, it doesn't show up very well in our composite distribution - that that triangle is at a pretty low percentile because even though that's the most likely value because there are so many values -

MEMBER BLEY: I'm going to stop you right there. Most likely has a distinct meaning, right?

DR. GHOSH: Yeah. If you look at the PDF it's where the PDF peaks. Because the PDF -

MEMBER BLEY: A PDF for this one doesn't peak way down there.

DR. GHOSH: We put the - well -

MEMBER BLEY: In fact, that's the 10th

percentile.

DR. GHOSH: Right, because we plotted - we plotted the CDF here so as I'm saying it's - the distribution is so wide to the right that it's very hard to see the effect.

But the PDF has a very tiny peak at 1,000. You can see that the curve is actually steepest through that 1,000 mark. But, again, because it's spread so far to the right that this translated to a CDF percentile of a very small number. And, you know, perhaps we could have put a little bit more thought into making the shape, you know, fit a little bit better.

But again, you know, we had the thinking that we think that's the - still think that's the most likely value but it could go up as high as 4,000.

CHAIRMAN STETKAR: When you say most likely value what is the most likely value? There's a 50 percent probability that - according to this there's a 50 percent probability that the density is greater than about - and I'm not going to try to get real precise here - maybe about 2,200 to 2,400.

DR. GHOSH: I guess when I say most likely I'm strictly speaking of just the mode of the distribution. So it's not connected to the percentile. So that's - I know, that's a little bit -

CHAIRMAN STETKAR: I think most people would think most likely would be the expected value.

MEMBER BLEY: Well, most - he's right but that can't be the mode.

CHAIRMAN STETKAR: That can't be the mode.

MEMBER BLEY: Or you didn't translate it right I think coming off as for -

DR. GHOSH: I think the translate - right. I think in a way something was lost in translation but perhaps at the time was already reflecting our judgment that we were comfortable with the fact that we may in the future want to use values that are larger than a 1,000.

I think this was in the middle of some transitional thinking. But, you know, in the end it does have some effect but it didn't show up as one of the most important parameters with respect to driving the results.

CHAIRMAN STETKAR: But going back to what Dennis asked originally is that I hear you saying that the triangle is what you feel to be the most likely value.

DR. GHOSH: Actually, I would say -

CHAIRMAN STETKAR: And what is the - what from this distribution if I were a betting person what is the expected value?

What value would I bet my life savings on from this distribution? And it kind of looks like it's, I don't know, 2,500, 2,600 - quite - like more than twice.

DR. GAUNTT: I would say so. This is a learning process for us and it might raise the philosophical question should we even do deterministic point analyses.

Should we - should we do the uncertainty study, you know, right up front and it's just - you know, it's kind of where we came from.

All of these best estimate values that we've - that we used in the, you know, point estimates of the analyses are the result of cumulative study over the past 20 years, looking at various experiments, and we don't wildly change our numbers from, you know, from assessment case to assessment case.

And so when we launched out to do the baseline SOARCA case that was sort of the sum cumulative knowledge of all the, you know, tests we've looked at - aerosol tests, the fuel meltdown tests, things like that.

Then we come along and start thinking about putting uncertainty distributions and some of our thinking kind of shifts as we put more thought into what's reasonable here. And this is one of the cases that kind of stands out.

It probably has little effect on the outcome because many of these aerosol mechanics processes are happening on real short time frames.

So we can be off on deposition rates and not really change the - change the net results. I'm suspecting that's why it's not - doesn't come out as a strong parameter.

MEMBER CORRADINI: It didn't come out anywhere near. It was one of the weakest of all the ones we investigated, if I remember correctly from the paper.

DR. GAUNTT: So that's probably indicative - you know, these processes like agglomeration and all they happen on very, very short time frames and the deposition process is similarly.

So it could be that having a difference in the terminal velocity inside the containment is not going to be a big deal.

MEMBER BLEY: I guess - I'll say this now so I don't say it at the end - if in fact you still have more confidence in your point estimate calculation than in these results then the learning process hasn't made its way far enough because you haven't come up with the distributions that you really believe.

It's just got to be true. It can't be any other way, because this is - you're really trying to express what you think the true state is and yet you're saying this isn't it when you're all done.

So something has not been learned yet or, you know, if you went back through it again maybe you'd have different distributions and maybe - I don't know how you did the - well, you just said you picked three points and drew a triangular but I don't think you quite translated your triangular into the density function and to this cumulative thing right. So I think something went funny somewhere in the process.

DR. GHOSH: Yeah. I mean, just glancing at the curve I think probably maybe a log triangular would have been more appropriate than a triangular because it is skewed so far to the right.

MEMBER BLEY: That density function - the area under the curve has got to be one so maybe you didn't do that or something wasn't right if you're still saying you believe that more than you believe the results of this regression.

DR. GHOSH: Well, I think that - yeah, I mean, in this case it's so hard to even talk about the most likely value because the mode is like this big because the area under the curve has to be one and we've drawn it out so far to the right.

I mean, then - I mean, you think one is right to ask, you know, how meaningful is it that it's the most likely if it's got a probability of 3 percent or something once you draw the entire curve.

MEMBER BLEY: Well, that's what I'm saying. Something wasn't done right.

DR. GHOSH: Yeah.

MEMBER BLEY: Maybe you needed something approaching a delta function at the beginning to put a large hunk of the probability in. I don't know if that's happened on other ones but -

DR. GHOSH: Right.

MEMBER BLEY: - if in fact at the end we're still saying we believe our point estimates better somewhere in this process we didn't do it right or we - equivalently we don't really believe these distributions or if we showed them as densities we don't really believe these densities.

That isn't what we were trying to say. So somehow we never went back and looked at those density functions and said is this really what I believe about this characteristic given I have that original point estimate that I believe in to some extent as being the most likely.

CHAIRMAN STETKAR: The reason I brought this up from - kind of repeating the same sort of knowledge statement is -

MR. OSBORNE: You know, there was another influence here I remember us worrying about. It was the shape factor of the particle as well as the density. I don't recall if it influenced the shape of the distribution that we - that we chose or not.

MEMBER BLEY: Yeah, we're talking about this one particular one. But the comment is really much more general. There's something in the whole set of them that, you know, must not be showing us your real state of knowledge - your real belief.

CHAIRMAN STETKAR: Thank you. That's the whole reason. I don't care about particle density nor do I know anything about aerosols.

The whole reason I highlighted this in my comments was exactly what Dennis was saying is that this picture illustrates what seems to be a radical change in the engineering community's understanding of this particular issue compared to the original triangle.

Now, if that's really true that's good. I mean, if the engineering community has thought about this process and said yes, the shape of this distribution is sort of what we wanted, the bounds on the - there's real reasons for that, that's really good.

Dennis' comment is you have to have done that first because that's the real essence of what determines ultimately, you know, from a numerical perspective your so-called best estimate. It kind of falls out of that process.

On the other hand, if this red curve isn't the engineering community's best estimate it's not at all clear what we're doing in this uncertainty analysis.

DR. GAUNTT: I think the red curve represents a realistic estimate of the distribution and I think - I think also it reflects a shift in our view on how to do this.

CHAIRMAN STETKAR: So I guess in principle asking if you ever - if you ever get the funding in time and everything to go ahead with the Surry have you thought about doing the uncertainty analysis first for that?

DR. GAUNTT: Well, we've done the Surry Point analyses, of course - the overview of ice containment.

DR. GHOSH: That's right. The ice condenser. Yeah, and you know -

CHAIRMAN STETKAR: Or at least not get so married into that point estimate that -

DR. GHOSH: Right. Right. Right.

CHAIRMAN STETKAR: - you try to rationalize why it's still your highest confidence result or whatever you want - however you want to characterize it.

DR. GHOSH: I guess having heard this conversation now we may put a note in the report for this parameter in terms of our progressed thinking on this. You know, well -

CHAIRMAN STETKAR: It's not just this one though. I mean, it's -

DR. GHOSH: Yeah. Yeah. I understand. Yeah.

CHAIRMAN STETKAR: - it's in effect all of them.

DR. GHOSH: Right.

DR. GAUNTT: This one does kind of stand out.

CHAIRMAN STETKAR: Oh, it does. That's what I - that's the only reason I picked this one is it's so different.

MEMBER SCHULTZ: But there are others that you could certainly comment on in the same vein which is were we to do this again we would have done the point estimate evaluation differently because of what we learned in the uncertainty analysis.

I think you're comfortable with this even as it is because it didn't show up to be very important.

DR. GHOSH: Right.

MEMBER SCHULTZ: To really be important and therefore okay, we didn't change it.

DR. GHOSH: Right. Right.

MEMBER SCHULTZ: But we don't feel we have a strong need to change it.

CHAIRMAN STETKAR: I mean, you know, if I can play devil's advocate if I go back to that relief valve thing, forget - suppose that Peach Bottom had never put that 3.7 times 10 to the minus three in their IPE study.

What would you have had? Well, you would have had those two NUREGs that had pretty low numbers and apparently input from experts who said well, those don't feel very good to us - that we think that ought to be shifted to the right to some extent.

In that case, that change of thinking, you know, would have shown up as being something important and, you know, it might have represented a legitimate, you know, rethinking of the process, you know, through the process of doing the uncertainty analysis.

In some cases, you're just very fortunate that this one didn't show up as important. If it did then a lot more scrutiny of this distribution.

MEMBER SCHULTZ: In the case you mentioned, it was demonstrated that we don't - we don't understand the rationale that was - that seemed apparently behind the decisions that were made.

Here we understand the decision or understand the rationale which would support a change based on what Randy has put forward. But we also understand that to move forward with that change for our current work is not that critical based on the results of your sensitivity study.

But certainly worth capturing - as you said, worth capturing is a list of things, you know, what would you do differently in the next evaluation.

Here's a parameter we would treat differently and we've learned that based upon the work that we have done. It's definitely worth capturing.

DR. GHOSH: Okay. So the next one is the fuel failure criterion and we have on slide 44 the weights to the three alternate models.

And slide 45 is the three models, and Randy was talking a little bit about this before. These are the time at temperature of models in terms of when the fuel breaks down.

MEMBER CORRADINI: But if I - if I might add, in some sense all three are connected. They're not independent.

DR. GHOSH: Right. Right. It's the same model. It's a matter of changing the temperature and the time - that you need to be at that temperature so that that's what the curves are. The time is on the Y axis and the temperature on the X axis.

DR. GAUNTT: What it says is we're 80 percent confident in the blue - the SOARCA value, right?

DR. GHOSH: The blue one in the middle, yeah.

DR. GAUNTT: The blue one in the middle there. So we're 80 percent confident that that's -

CHAIRMAN STETKAR: Oh, I see.

DR. GAUNTT: - that's the described well, the loss of geometry in the fuel - in the fuel bundle when the fuel changes from raw to - you know, to debris like geometry.

And how to read that curve what it says is that if you get up to, say, 2,100 K you have to - you have to get up to 2,100 K which is getting really close to the melting point of the zircaloy cladding before the fuel rods are going to be subjected to loss of geometry.

When you hit 2,100 K you're not quite melting yet and so we're saying that you can sit there for 10 hours but the fuel is heating up, still heating up.

So it's going to get hotter and you see the lifetime to failure is decreasing. So it's sort of a

Larson-Miller kind of lifetime rule. I mean, you are going to use this in a linear damage way that -

CONSULTANT SHACK: You're going to integrate up as -

DR. GAUNTT: Integrate up. Exactly. Right. That's kind of how this works. And so then the other two curves we put lesser confidence in but just to encompass the possible range of uncertainty in how quickly can the core lose geometry.

MEMBER CORRADINI: And when you say - let's just stick with one curve, if I understand it correctly. If we take the blue curve - to get back to Bill's question when you integrate up I don't - this is not - this is a ring value.

There is some volume that's being watched. All the volumes are being watched. When the volume in that ring - in that some axial and radial ring location gets at 2,100 it says aha, I'm at a point now where I'm hot enough that it goes from infinity to something finite.

And as it continues to heat up it essentially then watches and has a counter and as time marches along at some point the counter compared to this parameter cross and bing, what happens?

DR. GAUNTT: And then it will - it will

convert from intact rods into debris. In the meantime, generally at this point in time fuel heat up rates are like 10 to 15 K per second.

So you're moving through this pretty quickly and I - there are some other simultaneous processes going on. The fuel break out that's another - the melt breakout reg that's another kind of connected parameter.

MEMBER REMPE: When I was reading the writeup for this - by the way, the hydrogen writeup didn't change so please do correct that -

DR. GHOSH: Okay. Yeah. Thank you.

MEMBER REMPE: They said that this was based upon these results from Phebus at 1 and 2 - FPT 1 and 2?

DR. GAUNTT: I would just say the collective observation what do we see from the Phebus tests.

MEMBER REMPE: Okay. So I guess when I was reading it I was going well, I wonder how different it is from the prior tests and if it was somehow or other collectively related to the test is there some way you can plot some data or something to show that yeah, this is kind of bounding what we saw or something?

I mean, it was just - I don't know. When

I read it I wasn't - I didn't feel confident that the experts really - what they'd done. I couldn't figure it out.

DR. GAUNTT: You know, as I alluded to earlier, the reason for going to this lifetime model was to address a kind of a cliff edge effect we were seeing in the code calculation and prior to this we had all our code models calibrated against Phebus.

The general trend in Phebus is compared to fresh fuel test the radiated fuel tends to - it looks like it comes apart at a little bit lower temperature. It's hard to tell from these in-pile tests because all you have are temperature measurements. You don't really have a view, you know, of what's happening.

But, you know, overall we kind of capture the signature - temperature signatures of the test and end of test final configuration, and like I say prior to this lifetime model we would use just simply a temperature threshold and, for example, we would - we would allow molten zirconium to escape the oxide cladding at some number, 2,400 K or something like that.

We would allow the fuel to collapse from raw geometry at, say, 2,600 K but then what we would find is we would occasionally run two very similar calculations, one that would just come right under 2,600 and one that would just go right over 2,600, and they would go to, you know, a case where we would have 24 - you know, 2,550-degree fuel and it's sitting there for hours and hours and hours and hours, and we looked at that said no, that just - that's not right. It can't be.

MEMBER REMPE: But if it does have data that helps you come up with this lifetime rule and so that implies that you've got data that shows that -

DR. GAUNTT: Joy, I'd say it's kind of indirect data because like I say you cannot look into these fuel experiments and see exactly when is -

MEMBER REMPE: But you could see temperature behavior and peak temperature at various locations and you would see in-state and say okay, it failed and you have time at temperature as a function of time.

It depends on how well instrumented your test was, obviously. But I just wondered do you really have data that you can compare with this or this is just something to overcome the differences in the code?

DR. GAUNTT: The blue curve we're using here produces results that are not inconsistent with everything we're seeing in these in-pile tests. There's a few other data points and some of the irradiated small pin tests that are done in Vercor and so forth they do

- they do have I believe some radiography that shows the fuel collapsing at a certain temperature. So we know that the irradiated fuel loses its geometry sooner than fresh fuel.

These are all, you know, collective integral information that we've gathered over the years and the blue curve is kind of our best understanding of what we're seeing in these experiments.

MEMBER CORRADINI: So it's a bit off topic, Mr. Chairman, but so what does MAP do?

DR. GAUNTT: I'm sorry?

MEMBER CORRADINI: What does MAP do? They don't have this time and temperature. They have the previous - they have this previous - they have this other methodology in terms of the temperature. When you hit a temperature some event occurs.

DR. GAUNTT: I think - you know, they don't let you look inside MAP very freely but I think they do have a time at temperature.

MEMBER CORRADINI: Okay.

DR. GAUNTT: I believe they do.

MEMBER CORRADINI: Potentially different but a similar logic.

DR. GAUNTT: Right. Because I'm sure they've run into the same numerical oddities that we've seen.

CONSULTANT SHACK: So that's the trouble with any magic temperature criterion and you said when you -

DR. GAUNTT: Right.

CONSULTANT SHACK: - get yourself in a case where -

DR. GAUNTT: Right. I mean, that was the - that was the - you know, we ran a BWR case where the channel boxes should have failed at 2,600. I don't know what the number was, and the calculation hovered up at 2,580 or something for hours, and this kind of changed our thinking.

We need a more realistic behaving treatment that still kind of reflects the general temperatures when these things begin to come apart. Some of the other values on here, you know, on the lower bound here, 2,100 K, that's kind of dictated by when zircaloy melts.

The 2,700 K is an upper bound because of the eutactic - it's not a eutactic, it's a paratactic or Dana will tell you between zirconium oxide and uranium oxide.

So if you melted all the metal and you were just left with oxide shards up there then you would have this material interaction between U02 and Zr02 and you could not have anything solid above, say, 2,700 K. So there's kind of physics rationale for the shape of this and some timing implications as well.

CHAIRMAN STETKAR: Anything more on this one?

MEMBER REMPE: The discussion helps a lot. Again, it's probably less tied to the data as perhaps one could infer from the text is what I - I guess I might take away from it.

DR. GAUNTT: We feel pretty happy about that advantage in the modeling. It's - the code behaves a lot better now.

DR. GHOSH: Okay. The next one is the molten clad drainage rate. Once again, I'll turn it over for questions on this one, specific questions on this.

CHAIRMAN STETKAR: The only reason I - I mean, I added this to the list and this is just another example where the uncertainty distribution is measurably different from where that triangle sits - again, I don't know anything about the physics.

So I was just looking at this one and the particle density were two areas I highlighted where the results of the uncertainty analysis, you know, thinking about the phenomenon seemed to provide substantially different - at least best estimate mean, you know, expected value, whatever you want to call that -

DR. GAUNTT: I think there's probably some of that in the value that we used before we sat down and ironed out what do we think these really are. I believe that the value that we used here - so what this parameter is is when you - when you're heating up in that oxidation transient and this is like between 1,800 K, 1,700 K and beyond, you're going along at a 10 or 15 K per second clip. You're soaring through the melting temperature of zircaloy but you've got this nice little oxide scale on the outside that retains the melt.

So you've got melt sitting behind this oxide. And then we reach a - we didn't talk about it but we reach a break out, you know, criteria and what this - what this does this spells out how quickly does that molten material drain from behind this cladding, and my recall on this blue point here is that this is the value that sort of replicates relocation front movement that we have measured from experiments like CORA where they had video and you could see this melt front moving down.

And I think this is where - this is where that particular point came from. I don't remember about the shape of this distribution.

DR. GHOSH: Yeah. The - I guess what we added to what was in the draft report last week is that from the same CORA experiments that some of the free falling droplets moved really rapidly.

So I think this was an attempt to kind of capture the range of possibility with respect to maybe the average movement. But it could fall a lot quicker. So that -

CHAIRMAN STETKAR: It's a lot clearer that - the triangle looks like it's a lot closer to the mode of this distribution than, for example, that other.

DR. GHOSH: Right. So I think here we assigned something like a log triangular to put the peak at the -

CHAIRMAN STETKAR: Right. That would do it.

DR. GHOSH: And it's not - you know, it's not the median or but, you know, we were comfortable that, again, once you - when you put all these distributions into the Monte Carlo simulation the exact shape of the distribution in the end doesn't influence -

CHAIRMAN STETKAR: Not too much.

DR. GHOSH: - you know, that much, yeah. So you didn't worry too much beyond that.

CHAIRMAN STETKAR: Anything more on this one? I want to see if we can fit most of the stuff in before we lose Dr. Corradini.

DR. GHOSH: So then I think on the agenda we had a separate item for other issues but I guess our understanding from the identified issues was that most of these were MELCOR issues. So I lumped it at the end of this discussion.

I think we've talked a little bit about what we mean by surrogate parameters. I didn't know if there was additional questions on that - if we're good at that.

We added some additional writeup on the lower head penetration failure modeling that we did. Were there any questions on that?

MEMBER REMPE: I have a question. Okay. You talked about - okay. So and what I read this time I believe, and I'm paraphrasing, you said well, the diameter of the instrumentation tube isn't much different than the diameter of the drain line. So we kind of think we're close. But we had - and I don't think it was in this uncertainty report.

It was in - you did the sensitivities at the last meeting and I had some issues with some of the characterization then because there's a big difference because there's no in-vessel structures with the drain line.

In addition, they're quite different because they don't have instruments within them. So saying yeah, the diameter is about the same well, that's not quite true.

The instrumentation too has a bunch of stuff in it and so the drain line can open past the flare out of the lower head. But probably what got me most excited for the last meeting was that somebody referred to well, at TMI they saw that the melt had stopped in the instrumentation tube so we don't think it's that important.

And, again, the - if you look at those pictures of the melt we found at the TMI nozzles it was around the in-tube structures - the instrumentation. So there was annulus where the melt could go through.

It was a very small diameter - it dribbles down in those tubes. So that saying that it stopped at TMI is irrelevant for this drain line plus the drain line has - is 105, 106 steel which can very much lower temperature. So I still have some issues with that. And so I -

DR. GHOSH: The only reason we brought up that TMI freezing phenomenon is because we don't model that in MELCOR.

So MELCOR allows the material to keep going, and I think that's the only reason we put that note in the report and probably the presentation slides as well is that - the melt - the model for the lower head penetrations right now that we have available to us in MELCOR doesn't model any kind of freezing. So it's just to make - to make that clear.

MEMBER REMPE: And does it - when you model the instrumentation tube did you assume it was totally an open annulus?

DR. GAUNTT: I would say we're not using the - we're not using the penetration failure modeling. And the penetration fail modeling that's in MELCOR is really critical and I'll let - I'll let Kyle explain some more.

But I have a different interpretation that I don't know if it made it into the writeup or the explanation. But in these accidents in the BWR what we're seeing in MELCOR, and this is the world according the MELCOR, is the first materials to drop down into the lower head are the lower melting point control blade materials that are melting, falling down into the lower plenum.

Everything is falling down there and becoming quenched by all this water. Okay. So and then by the time - by the time the water dries out and you start to heat up the lower head again sufficient core damage has happened that you failed a steam line or depressurized the vessel.

So you have a kind of a situation where

you're reheating all of these materials and you're at atmospheric or you're at, you know, you're depressurized.

So under those conditions what we're finding is that - is that as these materials in the bottom of the head, and it's subsuming all the drive tubes and steel structures in the lower plenum, as these things are melting and running down you're basically heating the whole head up including the penetrations.

And so the failure mode that we're seeing in these depressurized accidents that are heating up from a quenched situation is that we first start melting the inner surface of the - of the head and just node by node melting through the head until there's no strength left. And so that's the - that's the modeling abstraction that we're doing right now.

MEMBER REMPE: So there's the Corbis data, remember, from Switzerland where they ran metal thermite through a drain line and that sucker just ablated through real quick, you know. So that's one thing to kind of keep in mind. And secondly, I mean, you do failure area sensitivities, and it's too late. I know that the report's done and all that but for other things like the dry well liner you did failure area.

You considered things like heat transfers from the liner and I don't think you did any sensitivities right on the area size of the lower head failure. Am I right in that?

DR. GAUNTT: I think we just fail a whole ring at a time.

MEMBER REMPE: And so I guess I'm just wondering how important is that area. You looked at other areas. Would that not have mattered?

DR. GAUNTT: I think you're seeking a granularity that's beyond MELCOR right now. But -

MEMBER REMPE: And that's okay. And again, I don't expect this - the report to change. But I guess that was the point I was trying to make this morning with the fact it was good that you found that the phenomenological uncertainties are larger than the weather uncertainties on the whole process.

But I think, again, that point I was trying to raise well, do you think we've captured everything. No, because of the model, and it's just something else to bring up is all I'm - is the only point I'm trying to make.

DR. GAUNTT: And I don't mean to brush you off either.

MEMBER REMPE: I know.

CHAIRMAN STETKAR: Sure you do.

MEMBER REMPE: You can try. No, but I mean, I just think that we need to think about that because, again, maybe there's some things we could learn from Fukushima and stuff we learned after TMI.

DR. GAUNTT: What - you know, I'll just mention this on the side as we - as we study Fukushima we've got the - I think the MAP folks may have a penetration failure model.

But overall, timing wise we're not seeing such a big difference in - between the codes on when the lower head fails. It seems, you know -

MEMBER REMPE: But, of course, we have no hard data to even say that we know that it failed at any of the vessels at TMI.

DR. GAUNTT: We have some indirect data.

MEMBER REMPE: No hard data though.

DR. GAUNTT: Yeah.

MEMBER REMPE: Okay. And so that's why I'm just thinking again that it'd be good to have a few caveats in there.

DR. GAUNTT: I know you love that drain line.

MEMBER REMPE: Sorry. I'm done.

CHAIRMAN STETKAR: We hit the second bullet. Any more discussion on the third bullet -

DR. GHOSH: I think we answered -

CHAIRMAN STETKAR: - where we talked about the size and -

MEMBER CORRADINI: There's no water.

DR. GHOSH: Right. There's no water.

MEMBER CORRADINI: I keep on bringing up water and I've been told go away, there's no water in this calculation. So okay.

CHAIRMAN STETKAR: So if Mike's happy that there's no water -

MEMBER CORRADINI: No.

CHAIRMAN STETKAR: - he doesn't -

MEMBER CORRADINI: I've been told - I've been told that I'm off in a different land. That's fine.

CHAIRMAN STETKAR: And finally, the fourth bullet, we touched on it to some extent with the operator actions for shedding loads.

The only other operator action that's modeled is the operators are guaranteed to open the SRV to depressurize at - I don't remember what time it was, two hours I think - something like that. And that's always guaranteed to be successful at that time, right?

DR. GHOSH: Yeah. We didn't - for the integrated uncertainty analysis we did not vary that time because we didn't have a basis to come up with a variation.

We kind of said from the beginning that the HRA was outside of the scope of our study and in the unmitigated scenario there's only a couple of actions.

So we did a separate sensitivity study to look at if that time were different what would the effect be. So that was separate kind of one sensitivity we did rather than integrating that uncertainty into the whole mix.

So we - so we did look at the case where the operators don't manually depressurize as an extreme case.

What if they never do it? And then there were a couple more cases - what if they do it - they really jump the gun - they know they're going to have to do it and they do it super early and then similar modest variations around the nominal time.

CHAIRMAN STETKAR: One of the questions that I had and this is - I'm kind of reading notes in real time here and I'm not doing all that well - the sensitivity study that you did looked at them opening the SRV at a half hour, an hour, two hours, three hours and not opening it at all.

One of the questions that I had - let me just read my notes here for a second. Bear with me here. Do the melt - when the operators open the SRV do the MELCOR models assume that the operators open it fully? In other words, it's all the way open?

DR. GAUNTT: My recall is they dropped the pressure down to - was it 150 PSI?

CHAIRMAN STETKAR: I think it's 120. Kyle?

DR. GHOSH: Kyle, did you hear that question?

MR. ROSS: Yeah. Yeah, I did. So yes, they can only open or close the valve. They can't position -

CHAIRMAN STETKAR: So they open it fully?

MR. ROSS: But it turns out at least from MELCOR calculation that opening one valve fully gives you about 100 degree per hour combined with, you know, making up with the RCIC on the feed.

So it's probably about what the operators see is that opening one valve and feeding RCIC as required gives them something like 100 degrees per hour.

CHAIRMAN STETKAR: Well, it's - if I - if I look at the case that you ran for them opening it at half an hour -

MR. ROSS: Yes.

CHAIRMAN STETKAR: - it shows that RCIC

trips due to low steam line pressure at 2.1 hours into that run, I think.

MR. ROSS: Right. So that was a - so that was a bad result.

CHAIRMAN STETKAR: Yeah. Well, but that says that you depressurize enough such that you get below 75 pounds in about 1.6 hours.

MR. ROSS: Yes, that's right.

CHAIRMAN STETKAR: Okay. Now, if I open the SRV at one hour RCIC never trips due to low steam line pressure. RCIC keeps running. It's assumed in this case that the batteries deplete at four hours.

RCIC keeps running until the steam lines flood at 5.2 hours. How come if I open up the SRV at one hour RCIC never trips from low steam line pressure?

MR. ROSS: Right. Right. That's what we saw.

DR. GAUNTT: Do you have some insight into that, Kyle?

CHAIRMAN STETKAR: Do you have some - I'm asking why is that. If I depressurize in an hour and a half for 1.6 hours by opening it at a half an hour why don't I depressurize within pick a number, 1.6 hours, a couple hours if I open it at one hour, you know? Why can RCIC hang in there for another 4.5, 4.2 hours?

MR. ROSS: so one thing - I don't think we modeled this especially completely. The operators wouldn't have been hands off. If they saw pressure dropping too far for RCIC to operate they would have interrupted the SRV.

CHAIRMAN STETKAR: Yeah, but the darn guys didn't do that when they opened it at a half an hour, did they?

MR. ROSS: Yeah. So we didn't like - so we didn't - we probably didn't capture actual operator action especially well here.

CHAIRMAN STETKAR: Okay. The only reason I'm asking this is this - the sensitivity study is presented in the context of examining different guesses about operator performance as a function of time rather than the nominal whatever it is, two hours I think was used.

MR. ROSS: Yes.

CHAIRMAN STETKAR: And the conclusion is well, if they open it up too early it's really - it's a bad day and if they never open it up at all it's a bad day.

In between there it doesn't make much difference. But in between there some of the things that are causing I think the bad day don't seem to be modeled, like pressure getting too low for RCIC operation.

I think one of the reasons it's a bad day if they open it up too early is that RCIC now goes away at two hours.

MR. ROSS: Yeah. So opening too early is not an issue because the operators would respond and they would not let pressure get too low to drive the turbine.

CHAIRMAN STETKAR: But my point is your sensitivity studies don't seem to be examining - in the half hour case you said well, that's really bad.

They're opening it up too early and look, lo and behold, if they don't intervene RCIC will fail at 2.1 hours. Now, if I look at the one-hour case, the two-hour case and the three-hour case, RCIC never seems to get a chance to fail from low pressure.

So I don't get to examine that behavior and I don't understand why in those cases suddenly when I ran those cases the operators now get smart and prevent it from failing whereas in the first case they got stupid and didn't prevent it from failing.

DR. GAUNTT: I'm just guessing but I -

CHAIRMAN STETKAR: So I'm not sure if I understand whether these sensitivity studies are done on the same level playing field in terms of this particular phenomena.

DR. GAUNTT: Yeah. Modeling the realistic operator action I guess, you know, that opening it early, doesn't reflect the fact that maybe they wouldn't let it depressurize so much.

I'm guessing what happened - the thread I would pull and I'm just guessing is that they're high on the decay heat curve and they're boiling off the water in the - in the core. And then when the water level falls too low in the core then you lose pressure.

And I'm guessing that's why that case goes to that point. You wait a few more hours you're lower on the decay heat curve.

CHAIRMAN STETKAR: Yeah, but Randy, you're not waiting a few more hours. In one case you're waiting - 30 more minutes gains you three and a quarter hours, okay.

DR. GAUNTT: I don't - I don't know. I'm guessing at that.

CHAIRMAN STETKAR: Okay.

DR. GAUNTT: I'm guessing it's related to where you are on the decay heat curve and where the water level is in the core.

DR. GHOSH: Yeah. But I think we talk in the report about the fact that we don't think we modeled the half hour scenario completely correctly because we think that there's some unrealistic assumptions in there about the operator just depressurizing by opening up and then hands off and just letting it go.

CHAIRMAN STETKAR: That's why I asked do they open it up fully. The answer is yes, they do. Okay. If they do I understand how we're getting to the low pressure -

DR. GHOSH: Right.

CHAIRMAN STETKAR: - you know, with whatever time so people can boil water. What I don't understand is the one-, two- and threehour cases. I understand the never - you know, hands off, never open it at all.

DR. GHOSH: But I think the one-hour case we have the - there's a - there's a couple of operator actions. At one hour you start the depressurization and at two hours you take manual control of RCIC.

And I think what may be happening also in the half-hour case is I don't know if because things are happening too fast if we don't get to the - that point of taking manual control of that -

CHAIRMAN STETKAR: Yeah. Maybe that's it. Maybe that's it because the half-hour case RCIC is going away at about that two-hour point.

DR. GHOSH: Right. Right. And I think in

their - the one-hour case, you know, the operators have planned to - you know, were doing the planned depressurization and were going to take control of RCIC at the two-hour point.

And with the half-hour I think - again, I don't know if this is a realistic modeling but they may not have a chance to - they haven't taken manual control and they need to.

I think that might be one of the big differences between the half-hour and the one-, two-, three-hour case. And we did write up a little bit about we think this probably isn't the best representation of one - of the variation one might expect with regard to that lower end. I mean, I don't - I didn't know.

So maybe they would have a chance to take proper manual control. But I think we haven't - the way we've modeled it it doesn't. And Kyle, you can correct me if I'm wrong about that.

MR. ROSS: Yeah. So I'm trying to wade through this in my - in my mind. But RCIC comes on full and stays on full until they throttle it at two hours in all cases.

DR. GHOSH: Okay. So that explains it. So that - I think that is the big difference.

CHAIRMAN STETKAR: That's it. That's

what's - that's what's doing it is that two hours just happens to be kicking in, you know, with one-, two- and three-hour cases before pressure gets low enough.

DR. GHOSH: Yeah. Yeah.

CHAIRMAN STETKAR: Okay. Thank you. That actually answers my question about that.

MS. SANTIAGO: I'm going to go on to the next section.

DR. GHOSH: So I think with that - that was all the MELCOR items we had and then we're - the next part was the MACCS parameters of interest, unless anybody had -

CHAIRMAN STETKAR: These are all very well formed.

DR. GHOSH: Okay. So the first one was the hot spot normal and - sorry. The normal and hot spot relocation doses and times.

So what we have on slide 49 is the dose curves for both normal and hot spot and on the next slide, slide 50, are the distributions for the times for both hot spot and normal.

So we did provide some additional writeup on that and I'll just - I'll ask the committee if they have specific questions on those. No? Okay. Great. So we can move on to the next one.

CHAIRMAN STETKAR: Hold on - hold on a second.

DR. GHOSH: Oh, okay.

CHAIRMAN STETKAR: I was writing. I actually had -

MEMBER CORRADINI: I figured he is going to ask something about evacuation speed. We might as well - might as well just simply go to that.

CHAIRMAN STETKAR: You know, quite honestly, I don't - in real time I think to keep us moving here -

DR. GHOSH: Yeah, I guess we can - we can come back to it if we need to.

CHAIRMAN STETKAR: Yeah. Why don't we do - to keep us moving why don't we go on to the next ones?

DR. GHOSH: So the next one was the evacuation speed and those were specified for the six different cohorts. So we have the distributions here. In the uncertainty analysis, we assumed the same distribution for some of the cohorts. That's why you see three curves instead of five.

Actually, I guess we also did the same for the SOARCA study so that's why some of the triangles are on top of each other too. And we did provide some additional writeup on that last week and I'll ask what the questions are on that one.

CHAIRMAN STETKAR: I'm still writing notes. I do have a question just to -

MEMBER CORRADINI: We figured you did.

CHAIRMAN STETKAR: The only - the only questions I had about all of these, the relocation times and the evacuation speeds and so forth, at the moment these distributions are developed for the purposes of this study. You keep correctly reminding us that this study is done specifically for the unmitigated long-term station blackout scenario at the Peach Bottom Nuclear Power Plant.

How would these distributions change if I told you that this scenario was initiated by the worst earthquake that you've never felt before?

DR. GHOSH: So which I guess -

CHAIRMAN STETKAR: Have you thought much about that? Are these - are these the distributions that would apply regardless of the initiating event for that scenario?

DR. GHOSH: Yeah. We have thought about that and, Joe Jones, if you're - I think you're still on the line.

MR. JONES: Yeah, I'm still on the line.

DR. GHOSH: Joe is actually our evacuation expert and I'll let him respond to that. You heard the question?

MR. JONES: I did hear the question and you're right, this is for a specific accident. It's also for a specific site, which is Peach Bottom, and at Peach Bottom there were very few bridges or crossings of waterways of any kind within the EPZ.

So unless it's an earthquake that would cause roads to shift in many areas and actually separate, this distribution would be representative enough.

You know, if it's - your description of your earthquake is a little bit qualitative. If it's the worst one I can envision then I can envision all of the roads failing and this would not satisfy that.

CHAIRMAN STETKAR: Well, I mean, my point is that I'm not sure - the report in particular for a lot of these off-site evacuation issues - timing and speed in particular - if you keep thinking about the fact that this is a particular scenario and you don't know what the initiating event is, if it's a plain vanilla loss of off-site power like birds flew into transmission lines and other transmission lines fell down and diesel generators failed to start and it's a nice sunny day, the question is do these uncertainty distributions - were they tailored to fit some presumption about what's happening in the external environment or not?

MR. JONES: Only to the extent that people will evacuate or have a means to evacuate. So to the extent that the roadways would be accessible, not necessarily all of them.

But at Peach Bottom, for instance, if one roadway is out, in almost any quadrant there's an easy drive around and it's not a heavily populated site so it doesn't affect it dramatically at Peach Bottom. For a very high populated site - population density site it would be different.

DR. GHOSH: So those are very site specific.

CONSULTANT SHACK: And you did do it differently at Surry, right? Didn't you - you did consider there was an earthquake and take a -

MR. JONES: Yes. We had a seismic scenario at Surry and it was kind of a split case there.

West of the river a similar situation as Peach Bottom - not a lot of crossings or anything. So the roads pretty much stayed intact. But as you get east of the river into the Williamsburg area the interstate collapses and it dramatically affected the evacuation time - more than doubled it.

CHAIRMAN STETKAR: Okay.

DR. GHOSH: Does that answer the questions? Should we move to the next one?

CHAIRMAN STETKAR: And that was my - those were my biggest concern about both of these is how site specific - how site specific are they or scenario specific for that particular site.

I guess what I'm hearing is although they are developed for this specific scenario because of the characteristics of this site they might apply for other initiators. Okay.

DR. GHOSH: Right. And I think - yeah, I think the potential effects of the seismic event was the sensitivity that was considered and it made a difference for Surry and not so much for Peach Bottom.

So it's very site specific, the impact of -

CHAIRMAN STETKAR: Okay.

DR. GHOSH: Okay. So the final, I guess, parameter that we had identified for discussion was the groundshine shielding factor, and on slide 52 we plotted here the curves for normal, sheltering and evacuation because we're assuming people are doing different things during those activities so they have different exposure from the groundshine and that's why the curves are different.

And Nate, unless you wanted to elaborate we can again turn it over to the committee in terms of what the specific question might be for that one.

DR. BIXLER: Thank you. I'll say just a few words before doing that. One thing is that this particular parameter was - is really - has a dual role in our analysis.

There's the question of what kind of shielding people get between the source of the groundshine and where they're at and that's a real - simply a shielding parameter kind of issue.

There's - we also considered and folded into the groundshine shielding factor the idea that the dose that someone gets from a - from some kind of radiation may - to an organ may differ and it would differ depending on age and size of the person, whether it's male or female, you know, whole variety of things.

So we tried to - and Keith Eckerman came up with a way of handling that part of the distribution.

We ultimately folded the two together into a single distribution that went into groundshine shielding factor because that was the efficient way of doing this sampling from our point of view. So ultimately this factor encompasses a couple different concepts for what we were trying to do.

MEMBER RYAN: I'm assuming you took from, you know, infants and small kids all the way up to older folks in all that. Did you account for the fact that they have different radiation sensitivities as well?

DR. BIXLER: I don't know if - I don't think that was part of Keith's writeup so I don't believe he did that. That would have been - yeah, actually that would be reflected separately in when we do the analysis we have a set of dose convergent factors.

Then we apply a set of risk factors to estimate the number of cancers, and both of those were uncertain here. So those two were treated independently.

MEMBER RYAN: You'll catch it in the uncertainty factors?

DR. BIXLER: Sorry?

MEMBER RYAN: You'll catch it in the uncertainty factors. Is that where you'll catch this point of variability?

DR. BIXLER: Yeah. That's right, as far as -

MEMBER RYAN: A child's thyroid is about one-tenth of an adult so for the same intake it's 10 times an adult's. So all that kind of stuff, yeah, that you'd have to capture.

DR. BIXLER: That should be - that should be - yeah, that would be in the dose convergent factors. Then the risk for health effects would be encompassed in the risk factors.

MEMBER RYAN: Good. Thank you.

DR. BIXLER: Yeah. Let me ask one and your later writeup may have explained something. I got confused when I was reading the NUREG.

DR. GHOSH: I think we decided to replace that writeup with our newer one because we thought -

CHAIRMAN STETKAR: Yeah. This is one thing that I fell asleep last night before I got to read this, quite honestly, because it was right at the end.

It's - I mean, I'm sorry. I fell asleep this morning before I got a chance to finish reading this.

When I read Section 4.2.5 of the NUREG, and I'm just paraphrasing from my notes here, my notes say that section explains the distribution for the composite uncertainty in groundshine dose and shielding factors - GSHFAC, this variable up here - is the independent product of the groundshine shielding distribution, GSFAC, and a distribution for the groundshine dose coefficients.

DR. BIXLER: Right. That's what I was just explaining a minute ago. Was that clear or do you still have a question about that?

CHAIRMAN STETKAR: The question that I have is is there now - in what I've seen GSFAC is a common distribution that's now used in two places as if -

DR. BIXLER: It's one distribution and there - you might be thinking about a typo that we found in the original documentation. I think we had put in groundshine shielding factor with a slightly different acronym. It might have been GSFAC or something like that.

CHAIRMAN STETKAR: Yes.

DR. BIXLER: That was a typo.

CHAIRMAN STETKAR: That's just a - oh, okay. That's - all right.

DR. BIXLER: One - that's the only one parameter.

CHAIRMAN STETKAR: Okay.

DR. BIXLER: There's really only one -

CHAIRMAN STETKAR: Okay. Thanks. Because I was getting - if you're using one parameter to create two other things you're treating independently you're not capturing the uncertain. But if it's the same parameter it's a typo. Thank you.

DR. GHOSH: Yeah, so we're fixing the typo and I think we rearranged the material in that subsection

-

CHAIRMAN STETKAR: Yeah.

DR. GHOSH: - to be more clear.

CHAIRMAN STETKAR: Thanks. That would probably explain - again, I just think about uncertainties. I don't know what these things are really in practice.

I just know the people in the past in many cases have taken a single distribution and multiplied it by three other things and then treated those as independent parameters and multiplied them together and it pulls the uncertainties down from accounting for that single parent distribution.

But a typo would explain this. Thank you. That was easy. That was - I'm glad I didn't spend a lot of time this morning reading this.

DR. GHOSH: So that completes our list of items that we understood, you know, that you all wanted us to discuss further and we'll certainly turn it over for more questions.

But just to - I just - just next steps FYI we have a CSARP presentation on Wednesday to present the results of the uncertainty analysis and for - we have several papers at next week's ANS PSA conference. On Thursday we have a two-hour SOARCA session and it's really five papers on the uncertainty analysis. And we're still working on the report.

You know, we've been - we're in the process of addressing some of the prior comments and we're going to take what we got today to further hopefully improve the documentation in the report and, you know, nominally we're expecting to send it to publication very late this fall.

But, you know, we - I think we'll - the schedule is a little - you know, is somewhat questionable and I think we are waiting to see whether the committee is going to write a letter to see whether we should, you know, wait for kind of a final word or we can go ahead and finalize the feedback we got today and so on.

We have a public draft that's available so there's isn't a tremendous amount of pressure to publish the final by a certain deadline. But, of course, you know, the longer - the longer we wait, as you see the state of knowledge kind of advances and then more questions come up with respect to what - how what we did -

CHAIRMAN STETKAR: And the state of memory as it decays.

DR. GHOSH: Right. It works in both directions, yeah.

MEMBER CORRADINI: And the half life for

you is?

MS. SANTIAGO: Well, we do want to try and finish it because we have a lot of work that we're looking at for filtered vents, some other things. Mitigating strategies is coming up that we'll be involved in and economic consequences so -

DR. GHOSH: Right. We're juggling a lot of projects. But we - yeah, we're trying to bring this one forward. So yeah, I'll turn it over for questions and comments.

CHAIRMAN STETKAR: Any other - first of all, any other questions about the specific topics that we've covered? If not, I have a few administrative duties that I need to handle here.

I'll ask if there are any questions or comments from anyone in the room.

CONSULTANT SHACK: Drove most of them away.

CHAIRMAN STETKAR: I don't know what the situation is with the bridge line. If we just have a single bridge line open only to Sandia or let's open up the bridge line to see if there are any members of the public out there who can have an opportunity to make any comments. Can we do that?

We'll wait a couple of seconds because like everything here we have absolutely no indication of whether or not the bridge line is open without asking for oral feedback. Sometimes you hear pops and crackles that give you an indication.

CONSULTANT SHACK: It's pretty quiet today.

CHAIRMAN STETKAR: It is. It's really quiet today. There we go. If there are - if there's anyone other than Sandia folks who are out there listening in on the bridge line could you do us a favor and just acknowledge your presence so that we know that the bridge line is open and we can hear you?

CONSULTANT SHACK: Supposed to be open, John.

CHAIRMAN STETKAR: Anyone - yeah, I -

COURT REPORTER: I suspect they may have dropped off.

CHAIRMAN STETKAR: Okay. I'll ask if there's anyone out there. If there's anyone - that's - it's open. If there's anybody out there from Sandia who wants to say anything.

CONSULTANT SHACK: Well trained.

CHAIRMAN STETKAR: Thank you. So we've done that. Now, what I'd like to do is two things. We normally go around the table and see if any of the members have any closing comments that they'd like to make, and

I'll ask as we're going around the table before the other thing that I want to get some feedback from the subcommittee is do we feel that we'd like to have another full committee presentation, given what we've learned today.

We had the original presentation that prompted this subcommittee meeting. Do we want to have a full committee meeting which may or may not prompt a letter? It's something that you were asking about in terms of logistics.

So if you can weigh in, since we need to be a little bit considerate of time here. I'll ask Dr. Corradini first. Do you have any closing comments or -

MEMBER CORRADINI: No. I do want to thank the staff. I think you did an awful lot of work in two months. So thanks for working like Stetkar does and matching him blow for blow.

I do think though there's one general thing I guess I wanted to - and I don't know if it's appropriate on the uncertainty report but consider it. It kind of goes back to a corporate memory issue, which is we've been kind of getting after you about can you explain this, can you explain this better, can you, you know, provide the - how the distribution connects to an experiment or to a judgement or to whatever.

But also in the final report I think it would be useful if, again, since this is going to look beyond this one project if you - if you didn't do something that in retrospect you should have done this is kind of - excuse my English - this is kind of like a Ph.D. thesis.

You have conclusions, you have observations and you have recommended future work. I think this deserves a relatively long list of recommended additional things that might need to be done upon further reflection whether it be a redoing of a distribution, a connection to something, additional calculations, waiting for Fukushima unwrapping of the site to learn more.

Those sorts of things I think actually point to - would be useful - not a long list but a key list that would help future staff and point them to what they would do next.

To me, particularly because this is going to be like a - one thing you're teaching others what you would do if you had all the time in the world but you really don't want to do. I think those sorts of listings would be very helpful.

To me, I think that would be very helpful as part of this, particularly the uncertainty part of this because we seem to be highly interested in uncertainty these days.

So that was my only thing, other than thanking you all for doing it so quickly. And to your point, I don't think we need a full committee meeting. I'm sure there's enough of us in the room that have enough memory that we can reflect upon this if you're going to write a letter, Mr. Chairman, or offer up a letter.

CHAIRMAN STETKAR: The committee writes letters.

MEMBER CORRADINI: I understand that but the chairman starts with an initial rough draft.

CHAIRMAN STETKAR: Well, but for the committee to write a letter we'd need a briefing of the full committee to learn, you know, salient things that we learned today.

MEMBER CORRADINI: But we had written a letter from the briefing we had in -

CHAIRMAN STETKAR: We did not. We decided to have this subcommittee meeting because there were enough details that we wanted to probe.

MS. SANTIAGO: Came back in July. It's when we briefed you in SOARCA I think six to eight months prior to that. We did talk about the uncertainty analysis there and I think the letter from that full committee meeting had a couple sentences with regard to

- I'm glad Bill is sick has said yes.

CHAIRMAN STETKAR: That letter - that letter if I can paraphrase it - and Bill's here so he can recall things a lot better than I can - that letter said it looked like you were headed in the right direction on the uncertainty analysis and we'd be really interested in hearing back from you when you had it done. So -

CONSULTANT SHACK: We didn't wrap it up and say goodbye.

CHAIRMAN STETKAR: Yeah, that's right. We didn't say yes, you're - well, then you're blessed.

MEMBER CORRADINI: Then I - can I retract my previous statement then? If we're going to - if they're going to go through the effort of documenting what they've done I think a brief presentation for the full committee would be in order. But they're not going to go through anything like this, I would hope.

MEMBER REMPE: No cobweb charts.

DR. GHOSH: That's the best part. I'll even offer a picture. It's a work of art.

MEMBER REMPE: Beauty is in the eye of the beholder.

MEMBER CORRADINI: I guess - I guess I don't want to - I don't want to get ahead of you, Mr. Chairman, but I guess if you were to do a full committee meeting my thought would be then that they'd kind of start with the conclusions and be ready to delve into things as topics come up versus what we had today, which is every slide - every curve known to man.

CHAIRMAN STETKAR: No, that's right. That's -

MEMBER CORRADINI: And the 11 ways in which they came up with a curve.

MEMBER REMPE: In response to our queries. They were just doing as asked.

MEMBER CORRADINI: I know. That's why we thanked them.

CHAIRMAN STETKAR: Okay. Thanks. Anything else, Mike?

MEMBER CORRADINI: No.

CHAIRMAN STETKAR: Joy?

MEMBER REMPE: I also want to thank everybody for their efforts on this because they were given a long list of things to do and not much time and I appreciated and learned a lot from it.

I hope - again, I think I harped enough on it about documentation, not only what we would do differently but also there - sometimes some individuals - I can remember a former chairman of the subcommittee say oh, we've done MELCOR so much.

Now we know - if there is - you know, there's not this much uncertainty, and I think there is a lot of uncertainty in the severe accident modeling capabilities because we just don't have data. And so I'm hoping that that flavor not only gets taken into account with the individual distributions but also at the upper part of the document, in the executive summary or whatever.

And so I would like to have a meeting and also maybe we can emphasize it because I think, again, I noted in several different places about the need to get into Fukushima and trying results and uncertainties. I'm done.

CHAIRMAN STETKAR: Mike?

MEMBER RYAN: Thanks again, and my thanks to the presentations. They were very well done. One thought that struck me is that as the plant becomes more and more well known it's important that we don't forget the releases that have occurred from the kind of plant area to the northwest across what is a very fertile part of Japan.

There's a lot of farming and vegetable growth and bamboo and all sorts of other stuff and it's also an area for wild boar that are eaten routinely by the population.

And IAEA has got a couple of missions going on in that topic. I would suggest that you at least touch on that because that's local food. You know, hit some of the pathway analysis that you're going to do.

I'm not saying it should be a big giant huge effort but it probably should be something you at least stay current on in terms of reports and other things. I happen to be on one of the IAEA committees.

I'd be happy to help you get your hands on the information because I think it would nicely augment some of the - maybe the assumptions that you're making about pathways and population groups and, you know, things of that sort to see what the range could be. So it's a comment plus an offer to help. Thank you, John.

CHAIRMAN STETKAR: Thank you. Steve?

MEMBER SCHULTZ: I really want to thank the staff and Sandia as well associated with the work that has been done since we met last.

That has been a very high quality and the presentations that you've brought forward today have also been very helpful and I would recommend that we do bring this to the full committee because I know that other members are - would be interested to hear a summary - a brief summary of these conclusions including the spider web pictorials.

There are more than you actually showed today but certainly to demonstrate what you have done which has - which has been very thorough and I think very well completed.

I would echo what Mike and Joy said about things to include in the report and I would really encourage the staff, the management to be sure that the report reflects the findings, the recommendations and the conclusions of the work that has been done here because there is a lot that has been - that has been learned and it's really important to capture that.

So in each of the sections where we have done all this technical work, which is very well described in terms of process and results, what I want to see in the report is from this we conclude and then the highlights associated with that.

And I do encourage also that the report be reviewed to pull those findings into some sort of either executive summary or technical summary for the report.

Just that it's been a long effort and very thorough and very helpful moving forward and I am concerned that when it's done at the end of this year with this NUREG that it may not be opened up for a while.

So I'd encourage that we complete it nicely in that regard. Thank you.

CHAIRMAN STETKAR: Dennis?

MEMBER BLEY: There's a lot here that I like a lot in IT. I want to thank you and compliment you on the amazing amount of work. I still have some nagging things about it that trouble me a bit.

If we do have a meeting, and we probably should, we'll need to decide whether we write a letter on this uncertainty methodology or we write a letter on SOARCA and the uncertainty analysis or we write a letter on the more general topic of SOARCA or a more narrow topic - it depends on how you look at it - would probably talk about the things that you didn't look at with respect to uncertainty in that SOARCA analysis and then there's a number of things that weren't part of this metric analysis and I think we'll have to do that.

Yeah, I think showing it to the full committee is a good idea. I like Mike's idea of - it's kind of lessons learned from this, some kind of paper to the - the rest of the staff can draw on in the future to guide how you do this kind of work and I think that would be very useful.

That's all. The other things I said during the meeting I still hang onto.

CHAIRMAN STETKAR: Dr. Shack?

CONSULTANT SHACK: Well, let me add my

thanks to - again, you did a tremendous amount of work. I found a number of the additional calculations very helpful, you know, and you say you want to put all this in the appendix.

You know, some of these - I'd like the Run 1, Run 2 comparison to be up in the main document. I think the full aleatory uncertainty should be in the main document to compare with.

I would also recommend just in the main document to help simpleminded folks, I like the 95 over five error factor kind of numbers. They kind of - you know, I can look at the regression analysis maybe and figure out when parameters are important and then figure out that those are MACCS or MELCOR parameters.

But the other ones give me a much quicker sort of overall view of where my uncertainties are coming from and so I think that would add to the clarity. Again, I assume this is going to end up in an appendix.

You know, I think the work that you did on simplifying the source terms is a useful sort of thing that, you know, as we were discussing at lunch time, you know, you're not going to do this very often and you need more simplified approaches and it's sort of good to have that document, that you went through that.

And, again, the documentation of the

uncertainty - and then I think part of it came back today. I mean, there's nothing wrong with making engineering judgments.

Just make clear that it's an engineering judgment, highlight where your engineering judgment leaves you with very large uncertainties and, you know, that's life. But, you know, try to make that as clear as possible and, again, everybody always says write clearly.

Well, if we knew how to write clearly I wouldn't have to give you the advice. But that's where I'm at. But I think it's a very good job. I hope you get a chance to go ahead with the Surry analysis.

I think it would be useful now that you've got all this under your belt to see how well we go. I also am glad to see that MACCS2 now has the capability to do the simple random sampling.

I still like bootstraps when you're telling me how things have converged and it's a kind of a, you know, every once in a while I can at least fall back on that to find out and make myself convinced that you're really there and now you have that capability sort of as a routine feature.

CHAIRMAN STETKAR: Thank you. I'll echo - thanks a lot. You folks did a heck of a lot of work in the last two months and I certainly really appreciate everything that was done. I know it was - a lot of effort went into it and you're really responsive, I think, to our requests.

Quite honestly, I learned a lot more during our discussion today than I did from your 100 or so pages of written material.

I'll tell you that in many cases - and this kind of echoes a little bit of what Bill said and I think what you've heard in other places - is that the folks who do the analyses sometimes get so close to the details that it's obvious to you the point that you're trying to make.

But large numbers of tables of very small numbers oftentimes kind of miss the point. So in many cases, you know, the words that we heard today are much more effective than a lot of really busy figures and a lot of really busy tables.

Those might be nice to, you know, in the old days of computer output - stacks of things that ran off a spool printer for days and days on end. That might be nice to document what you did.

MEMBER BLEY: Can I toss something on that -

CHAIRMAN STETKAR: Yeah.

MEMBER BLEY: - because of - and then what

Bill said, the tables - if you have something in an appendix the tables could be in an appendix. If you had your figures and if you put the error factors down on the figures, man, that would communicate really well and the tables are just awful trying to look at this and look at this and figure out -

CHAIRMAN STETKAR: Including the error factor, right? Someone wrote that I mentioned Lord knows how many hours ago the fact that gee, your error factor from a MELCOR stand alone uncertainty where you took the mean, if you want to - the mean weather and fixed the MACCS parameters that gives you kind of an error factor of around three or four, somewhere in that ball park. And now gee, look, if I add - if I do a full blown, if I want to call it that, MACCS uncertainty analysis that gets larger to about four or five.

Doesn't go up to, like, 15 to 20, and that gives you intuitively a much better sense of where are the uncertainties coming from. And I said well, gee, the biggest - if that's actually true - I'll still withhold some sort of skepticism on that notion but I don't know anything about consequence analysis - if that's honestly true it says well, from an uncertainty perspective then we certainly do want to focus more on the MELCOR type stuff given the fact that we've not done anything on the uncertainties on the initiating events or anything else in the accident, you know, development.

And then the question is well, do we drill down and see where the biggest sources of uncertainty are there. And you finally come down to this thing that we've been toying with.

Is it - are the sources of uncertainty because we have data and there's a lot of, you know, a lot of variability in the data - we have a lot of data and we're just limited by variability in the data?

And I don't like to use the words aleatory and epistemic in this sense because I think they're - it's too easy to throw things into, you know, a black box.

So I'll just say uncertainty. Is it due to the fact that there are - there's no evidence and we just - we just needed to rely on our engineering judgment, which is okay but it says well, if that's really driving our understanding of risk maybe we ought to be doing a little bit more research to refine that information.

So I think some of that higher level presentation of here are the uncertainties and here they're coming - here's where they're coming from, here's how big they are, here's how small they are, why are they this big, why are they that small, you know, is really important to cast this into a context that, yeah, somebody who's not been deeply involved in this project for three years or five years or however long you people have been doing this can pick up the report and sort of gain that level of understanding.

And I think, you know, what I've heard regarding a full committee briefing I think we probably should have one to bring this to closure.

I think the folks sitting at the table today learned an awful lot. I think that the rest of the committee would benefit from that discussion.

The committee would need to decide whether we write a letter or not but I think we should probably plan on a full committee meeting and then take it from there regarding whether we write a letter.

So you need to work with Hossein and figure out where we could fit - mutually agree to schedule for that.

MEMBER SCHULTZ: John, there's a - I wanted to follow up just a moment on what your comments were on engineering judgment because the third case that we discussed today, which is there is data - there is data that's available, but what has been done in the study is to use that data combined with engineering judgment to determine the uncertainty contribution.

So that, you know, that's required either to represent consensus of the engineering community or to just move forward with uncertainty distributions. But we need - we need to capture that as engineering judgment and not a depiction that data has been used to determine uncertainties alone.

CHAIRMAN STETKAR: Yeah. Thanks. That's something - some of the comments that I made about - it's too easy to point to something and say well, look, this number is in this report and this number is in this report and look, the number that we used is between those numbers, which doesn't necessarily tell the story that we heard today. It's a different story.

Anything else? With that, thank you again very, very much and we are adjourned.

(Whereupon, the above-entitled meeting concluded at 4:42:37 p.m.)

SOARCA
Peach Bottom Uncertainty Analysis (UA)

ACRS Briefing

Tina Ghosh, PhD
RES/DSA/AAB

September 16, 2013

Agenda

- ACRS comments on MACCS2 weather uncertainty integration and convergence of results, and staff responses
- MELCOR parameters of interest
- MACCS2 parameters of interest

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MELCOR – MACCS2 – Weather Uncertainty Integration

ACRS Comment:

- For the combined MELCOR-MACCS2 results, the report currently presents only results averaged over the weather trials.
- The report should also present results that include and display the full weather aleatory uncertainty

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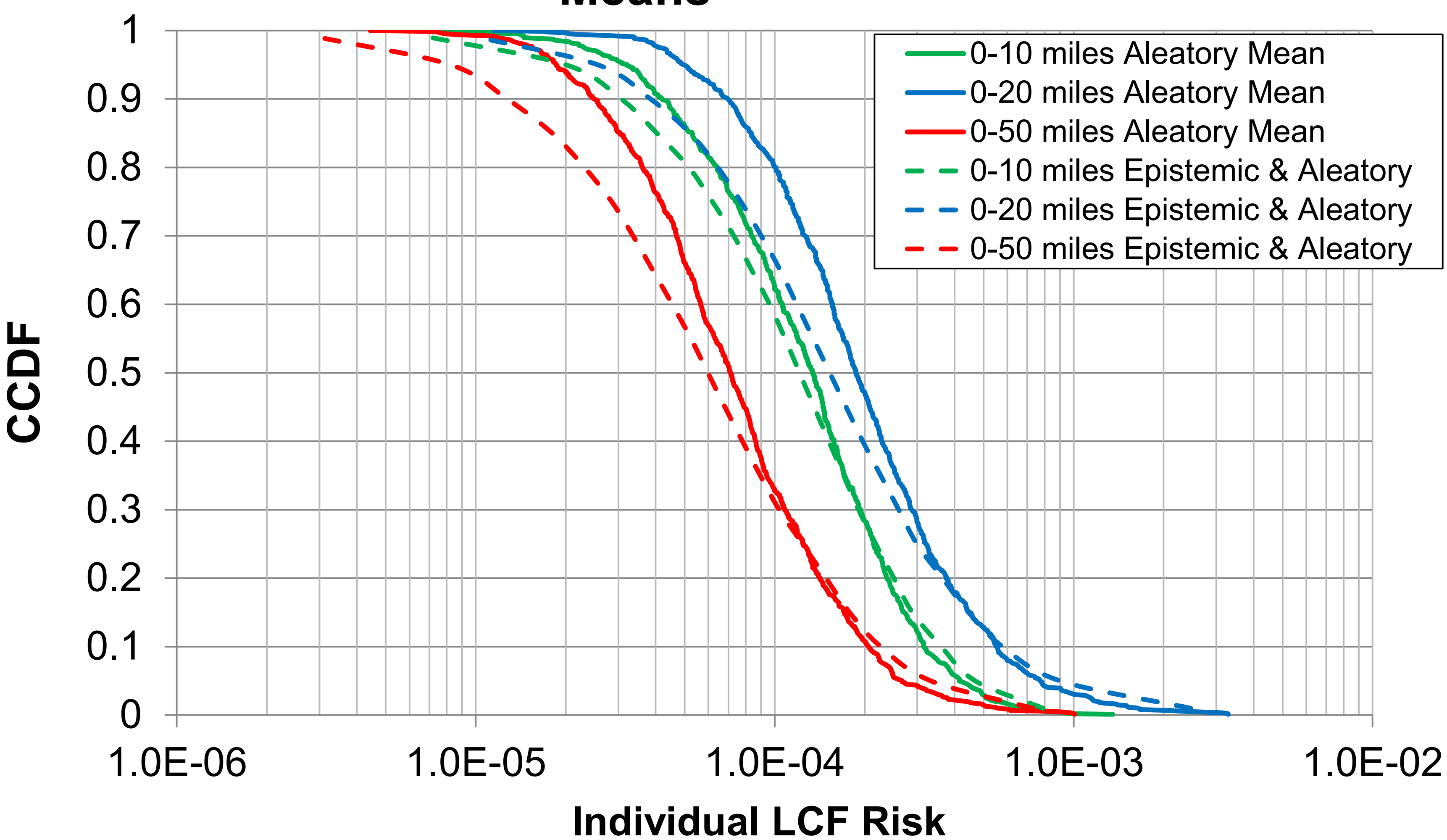
Conditional mean, individual latent cancer fatality (LCF) risk (per event) for combined results (865) with LNT model

	0-10 miles	0-20 miles	0-30 miles	0-40 miles	0-50 miles
5th percentile	3.1×10^{-5}	4.9×10^{-5}	3.4×10^{-5}	2.2×10^{-5}	1.9×10^{-5}
Median	1.3×10^{-4}	1.9×10^{-4}	1.3×10^{-4}	8.7×10^{-5}	7.1×10^{-5}
Mean	1.7×10^{-4}	2.8×10^{-4}	2.0×10^{-4}	1.3×10^{-4}	1.0×10^{-4}
95th percentile	4.2×10^{-4}	7.7×10^{-4}	5.3×10^{-4}	3.4×10^{-4}	2.7×10^{-4}
SOARCA UA Base Case	9.0×10^{-5}	8.3×10^{-5}	5.8×10^{-5}	3.7×10^{-5}	3.0×10^{-5}

Conditional Individual LCF Risk (per Event) CCDFs for Combined Aleatory and Epistemic Uncertainty and Epistemic Uncertainty with Aleatory Means



MACCS2 and Weather Uncertainties for Prompt Fatality Risk

ACRS Comment:

- Select the MELCOR realization that produced the largest conditional prompt fatality consequences in the current SOARCA uncertainty results.
- For that realization, sample from the 350 MACCS2 input parameters, and for each epistemic sample generate 984 weather cases to derive an uncertainty distribution for the conditional prompt fatality consequences at each distance.
- Demonstrate convergence of the combined MACCS2-weather uncertainty analysis results.

MACCS2 and Weather Uncertainties for Prompt Fatality Risk (cont.)

Approach:

- MELCOR Replicate 2, Realization 291 identified as the source term that produced the largest conditional prompt fatality risk consequence
- For that source term, three Monte Carlo runs of sample size 1000 were completed (Runs 3, 4, 5) using three different LHS random seeds for the 350 MACCS2 input parameters
- The same 984 weather trials were used

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Conditional, mean, individual prompt-fatality risk (per event) statistics for the MACCS2 Uncertainty Analysis for specified circular areas (Run 1)

	0-1.3 miles	0-2.5 miles	0-3.5 miles	0-7 miles	0-10 miles
Mean	4.5×10^{-7}	8.9×10^{-8}	3.5×10^{-8}	8.3×10^{-9}	4.8×10^{-9}
Median	0.0	0.0	0.0	0.0	0.0
75th percent-ile	0.0	0.0	0.0	0.0	0.0
95th percent-ile	1.9×10^{-6}	3.5×10^{-8}	0.0	0.0	0.0

Run 3-5 conditional, mean, individual prompt-fatality risk (per event) statistics for specified circular areas

		0-1.3 miles	0-2.5 miles	0-3.5 miles	0-7 miles	0-10 miles
Mean	Run 3	3.3E-06	1.0E-06	3.4E-07	4.7E-08	9.5E-09
	Run 4	3.3E-06	9.4E-07	3.0E-07	4.2E-08	8.9E-09
	Run 5	3.2E-06	9.8E-07	3.0E-07	4.7E-08	1.3E-08
Median	Run 3	4.9E-07	1.2E-07	0.0	0.0	0.0
	Run 4	3.3E-06	9.4E-07	0.0	0.0	0.0
	Run 5	3.2E-06	9.8E-07	0.0	0.0	0.0
75th percent-ile	Run 3	4.0E-06	1.0E-06	2.0E-07	3.8E-09	0.0
	Run 4	3.7E-06	8.8E-07	2.2E-07	1.1E-08	0.0
	Run 5	3.9E-06	9.6E-07	1.9E-07	8.2E-09	0.0
95th percent-ile	Run 3	1.4E-05	4.1E-06	1.5E-06	2.1E-07	1.2E-08
	Run 4	1.6E-05	4.7E-06	1.8E-06	2.3E-07	0.0
	Run 5	1.4E-05	4.4E-06	1.6E-06	2.0E-07	0.0

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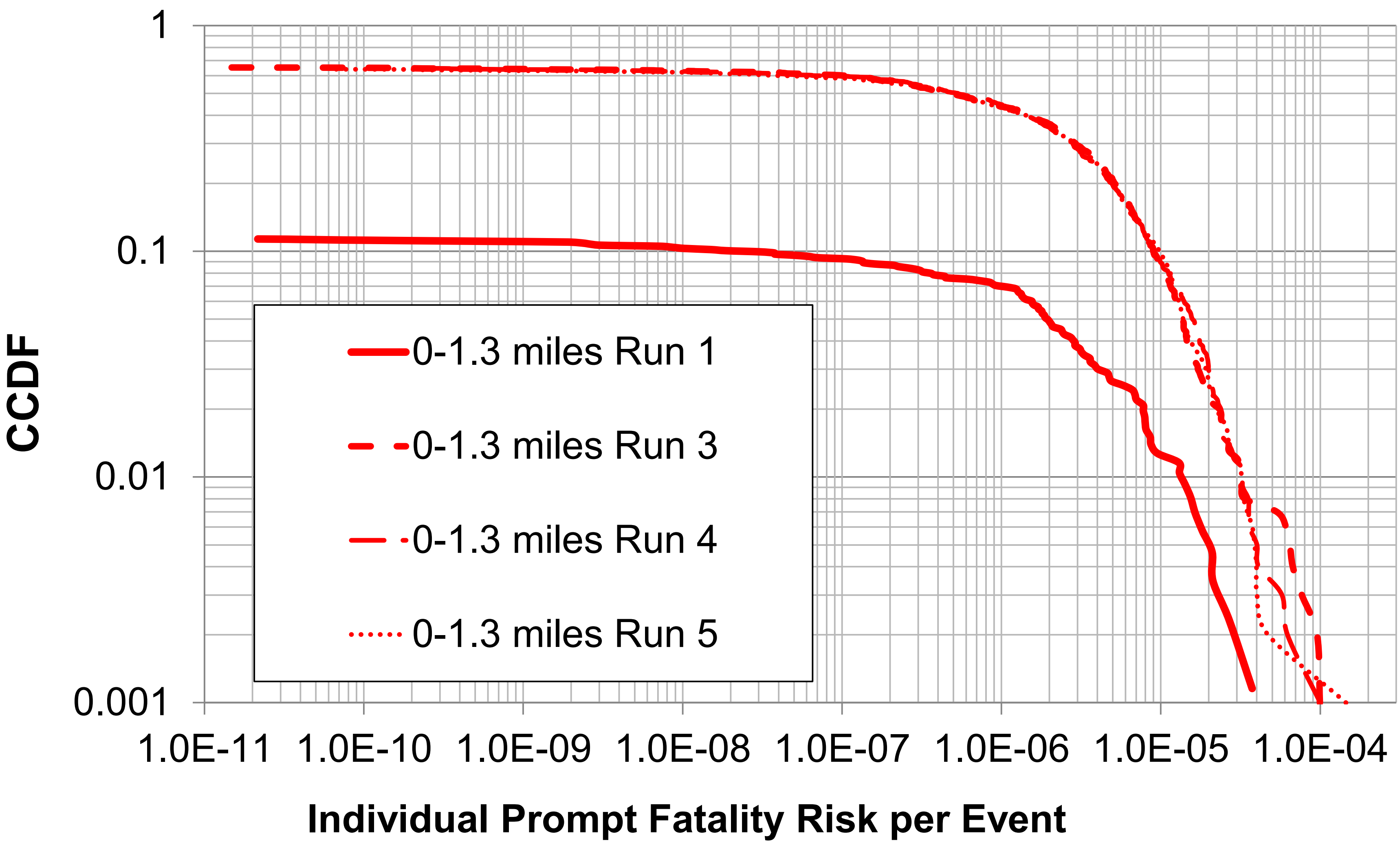
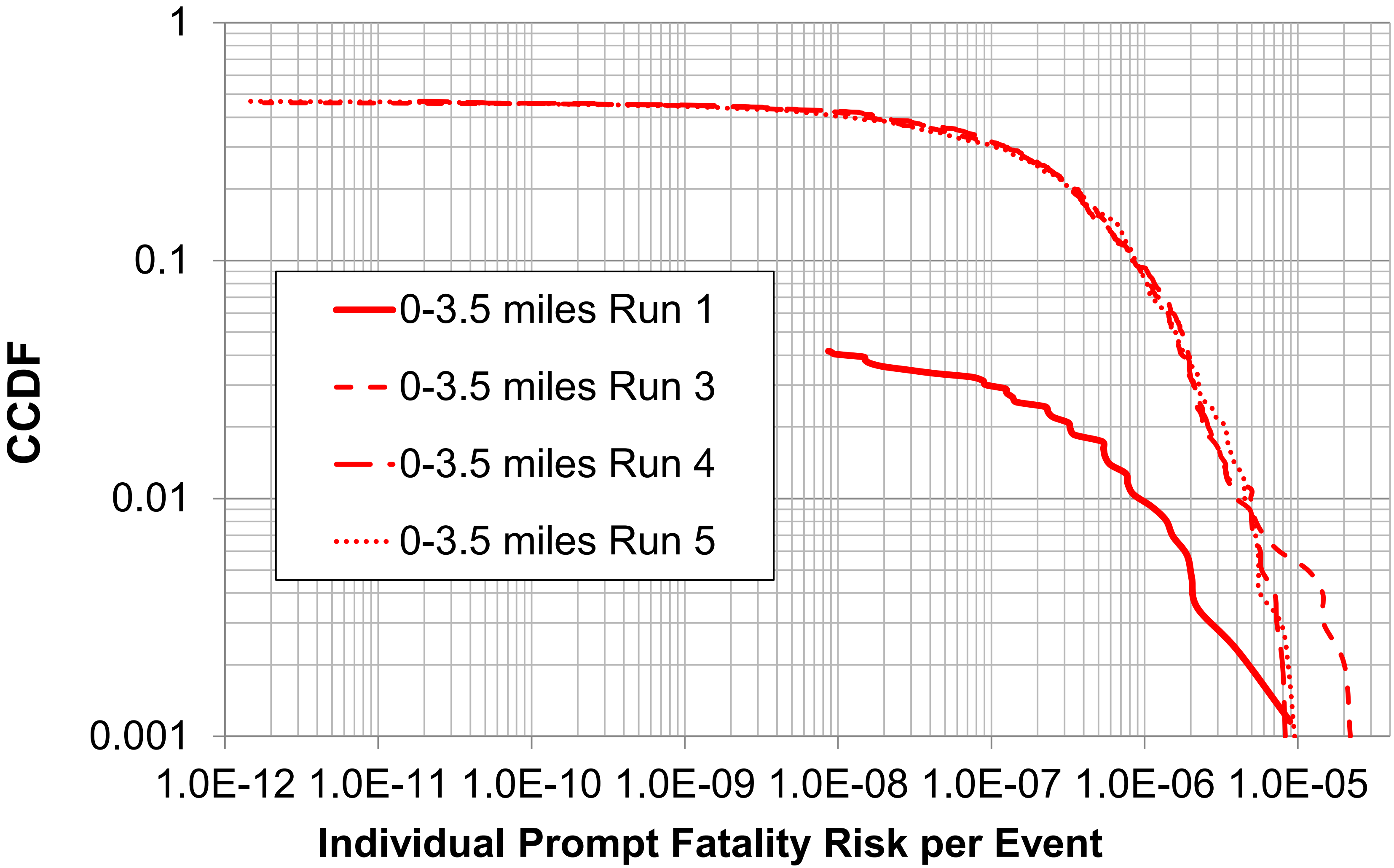
Runs 3-5 and Run 1 Conditional, Mean Individual Prompt Fatality Risk (per Event) Epistemic Uncertainty CCDF, at 1.3 Miles



Runs 3-5 and Run 1 Conditional, Mean Individual Prompt Fatality Risk (per Event) Epistemic Uncertainty CCDF, at 3.5 Miles



MACCS2 and Weather Uncertainties for LCF Risk 1

ACRS Comment:

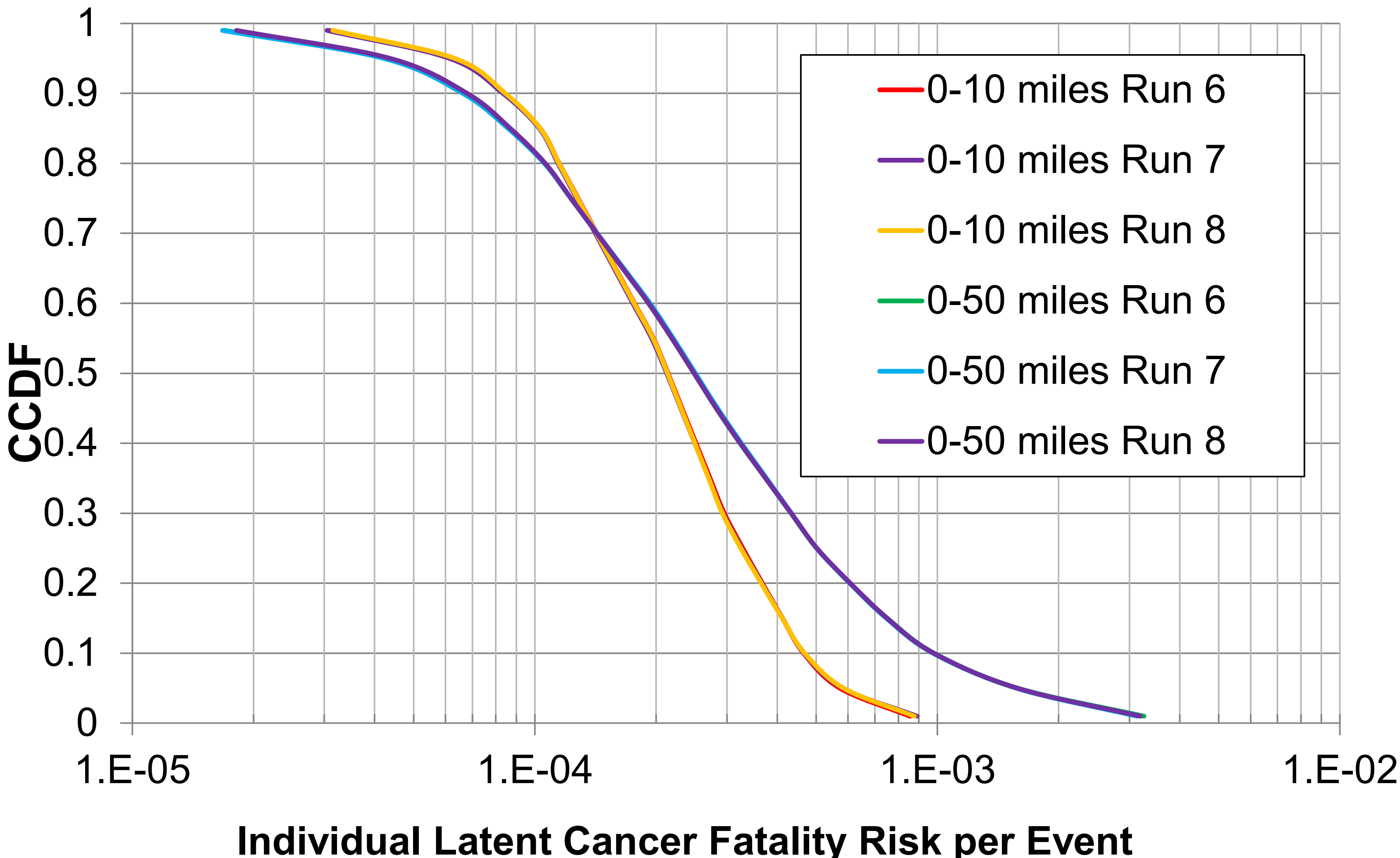
- Select the MELCOR realization that produced the largest conditional LCF fatality consequences in the current SOARCA uncertainty results.
- For that realization, sample from the 350 MACCS2 input parameters, and for each epistemic sample generate 984 weather cases to derive an uncertainty distribution for the conditional LCF fatality consequences at each distance.
- Demonstrate convergence of the combined MACCS2-weather uncertainty analysis results.

MACCS2 and Weather Uncertainties for LCF Risk 1 (cont.)

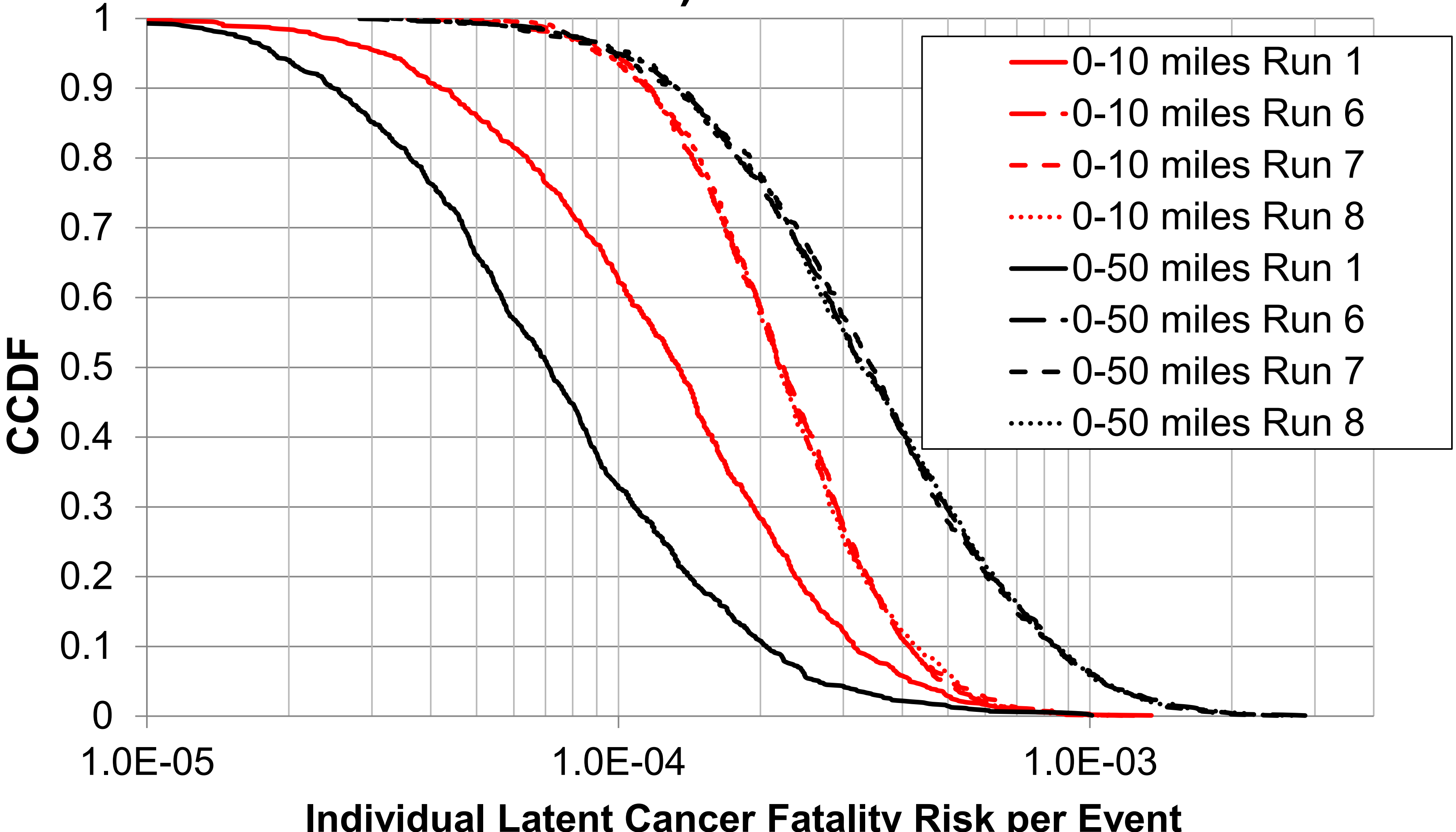
Approach:

- MELCOR Replicate 3, Realization 46 identified as the source term that produced the largest conditional LCF risk consequence
- For that source term, three Monte Carlo runs of sample size 1000 were completed (Runs 6, 7, 8) using three different LHS random seeds for the 350 MACCS2 input parameters
- The same 984 weather trials were used

Run 6-8 Combined Aleatory and Epistemic Uncertainty Conditional Individual LCF Risk (per Event) CCDF



Runs 6-8 and Run 1 Epistemic Uncertainty with Aleatory Mean, Conditional Individual LCF Risk (per Event) CCDFs



MACCS2 and Weather Uncertainties for LCF Risk 2

ACRS Comment:

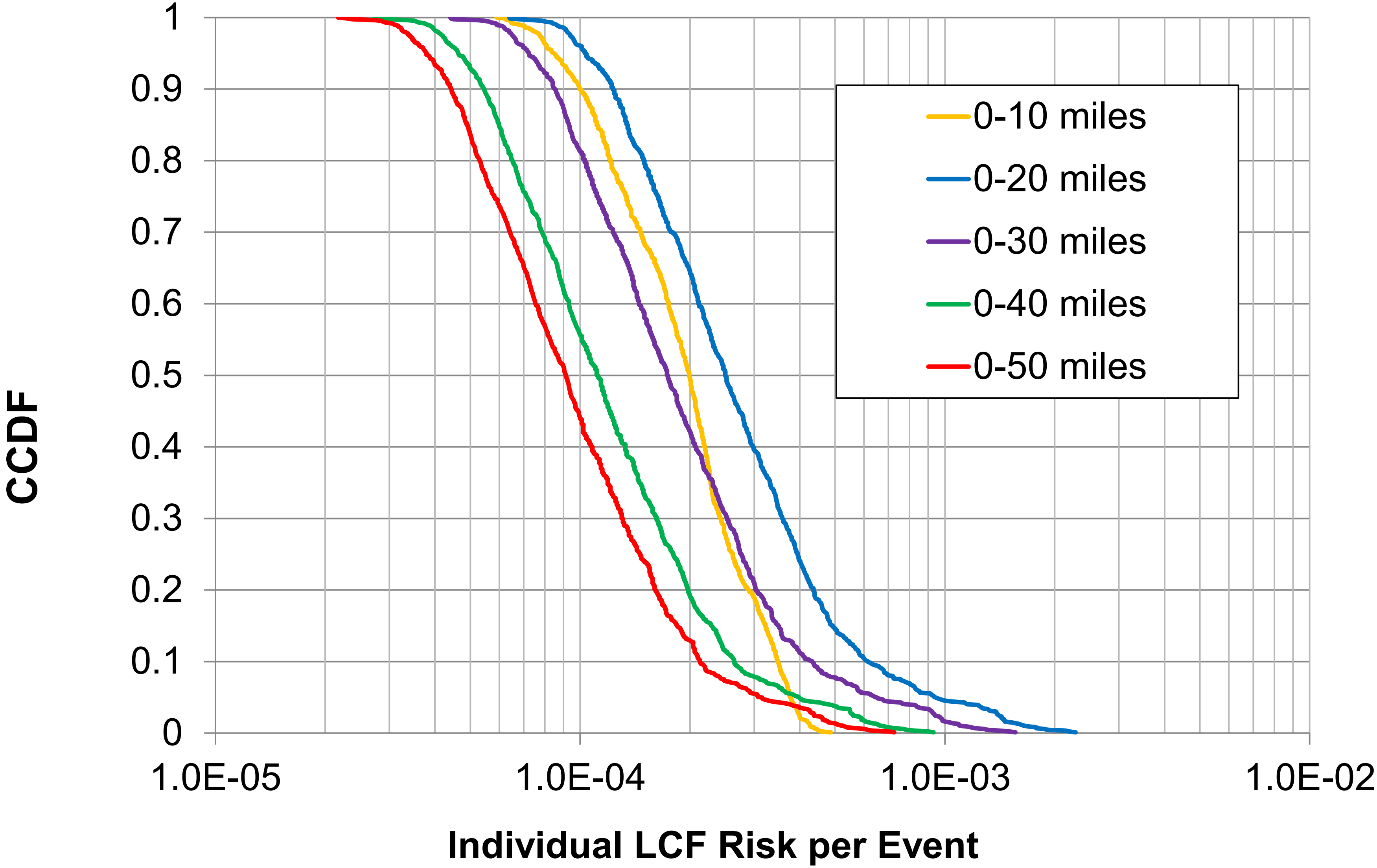
- Select a MELCOR realization that produced a small, but non-zero, contribution to the conditional LCF fatality consequences in the current SOARCA uncertainty results.
- For that realization, sample from the 350 MACCS2 input parameters, and for each epistemic sample generate 984 weather cases to derive an uncertainty distribution for the conditional LCF fatality consequences at each distance.
- Demonstrate convergence of the combined MACCS2-weather uncertainty analysis results.

MACCS2 and Weather Uncertainties for LCF Risk 2 (cont.)

Approach:

- Three representative source terms were chosen
- First an initial MACCS2 run (Run 2) used all 865 source terms while all MACCS2 parameters were set to their SOARCA point estimate values.
  - To assess the influence of the source term when MACCS2 parameters are fixed

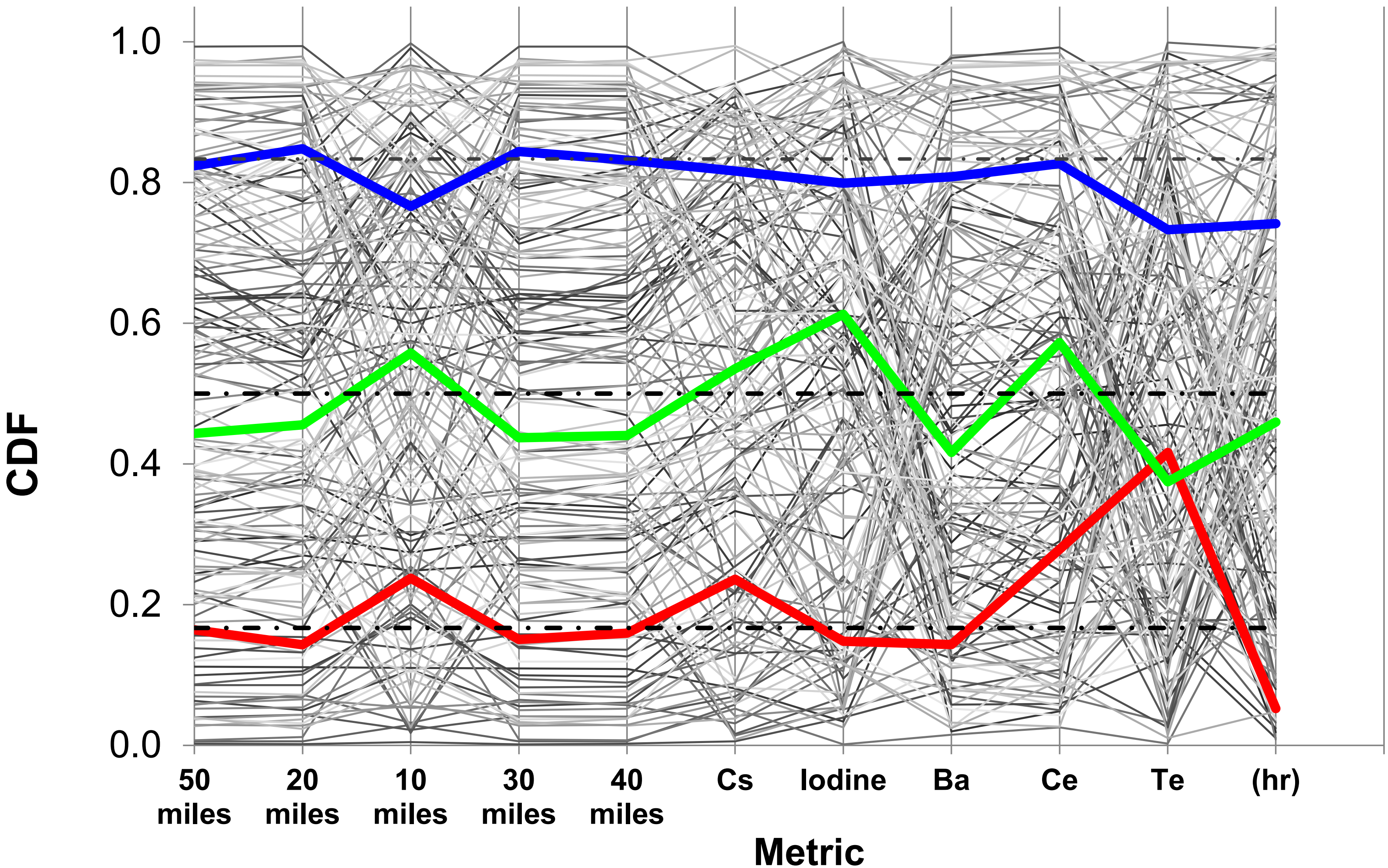
Run 2 Conditional Mean, Individual LCF Risk (per Event) for 865 Source Terms and Fixed CCDF



MACCS2 and Weather Uncertainties for LCF Risk 2 (cont.)

- A set of 11 results have then been used as metrics to select three representative source terms:
  - Latent Cancer Fatality (LCF) risk at 5 different locations (10, 20, 30, 40 and 50 miles)
  - Fraction of inventory released for 5 radionuclides (Cs, I, Ba, Ce, Te)
  - Release time
- Goal is to choose three source terms whose metrics' ranks come closest to 1/6, 1/2, and 5/6 among the population

Results: Cobweb Graph for Selected Source Terms



MACCS2 and Weather Uncertainties for LCF Risk 2 (cont.)

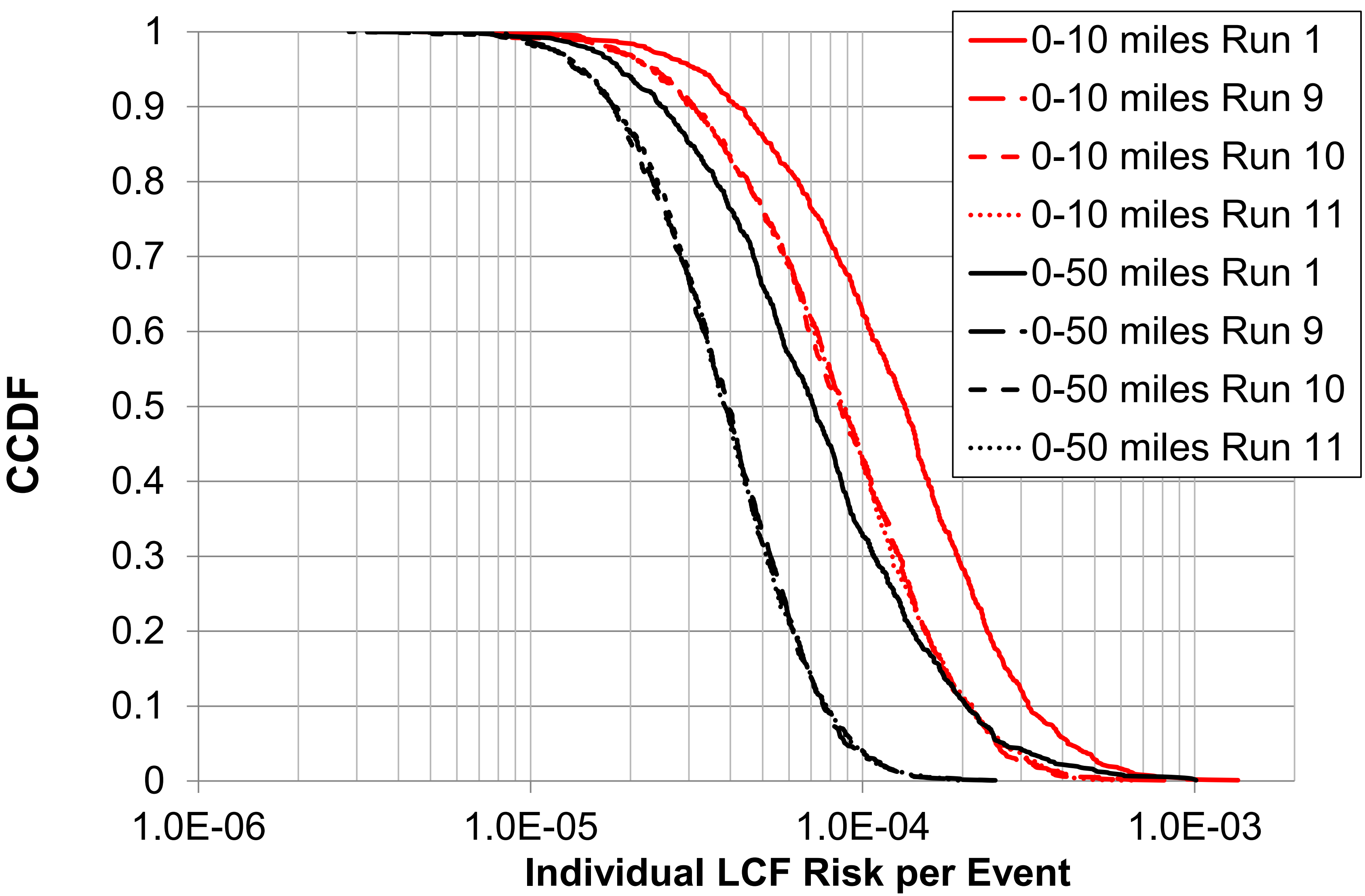
Approach (cont.):

- With respect to conditional LCF risk:
  - MELCOR Replicate 3, Realization 187 identified as the representative low source term
  - MELCOR Replicate 1, Realization 75 identified as the representative medium source term
  - MELCOR Replicate 1, Realization 290 identified as the representative high source term
- For each of these source terms, three Monte Carlo runs of sample size 1000 were completed (Runs 9-11,12-14, 15-17 respectively) using three different LHS random seeds for the 350 MACCS2 input parameters
- The same 984 weather trials were used.

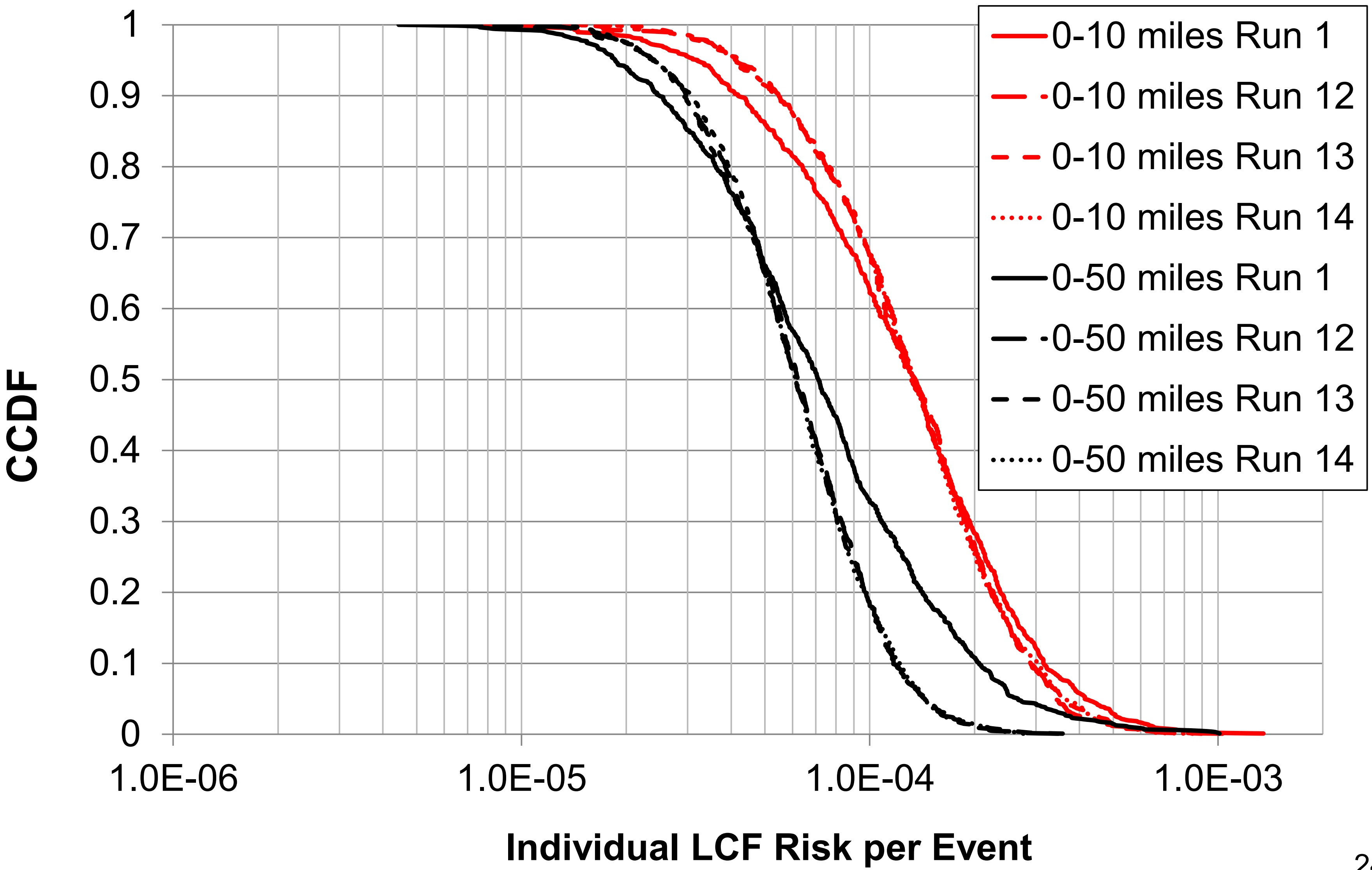
Runs 9-11 (Low Source Term) Conditional, Mean, Individual LCF Risk (per event) Statistics

Statistic	Run #	0-10 miles	0-20 miles	0-30 miles	0-40 miles	0-50 miles
Mean	Run 9	1.1E-04	1.2E-04	8.3E-05	5.4E-05	4.4E-05
	Run 10	1.1E-04	1.2E-04	8.3E-05	5.4E-05	4.4E-05
	Run 11	1.1E-04	1.2E-04	8.3E-05	5.4E-05	4.4E-05
Median	Run 9	8.8E-05	1.0E-04	7.2E-05	4.7E-05	3.9E-05
	Run 10	8.6E-05	1.0E-04	7.4E-05	4.8E-05	3.9E-05
	Run 11	8.8E-05	1.0E-04	7.2E-05	4.7E-05	3.9E-05
5th percentile	Run 9	2.3E-05	3.8E-05	2.7E-05	1.7E-05	1.4E-05
	Run 10	2.2E-05	3.8E-05	2.6E-05	1.7E-05	1.4E-05
	Run 11	2.3E-05	4.0E-05	2.7E-05	1.8E-05	1.4E-05
95th percentile	Run 9	2.5E-04	2.4E-04	1.7E-04	1.1E-04	8.9E-05
	Run 10	2.6E-04	2.4E-04	1.7E-04	1.2E-04	9.5E-05
	Run 11	2.7E-04	2.4E-04	1.7E-04	1.1E-04	9.4E-05

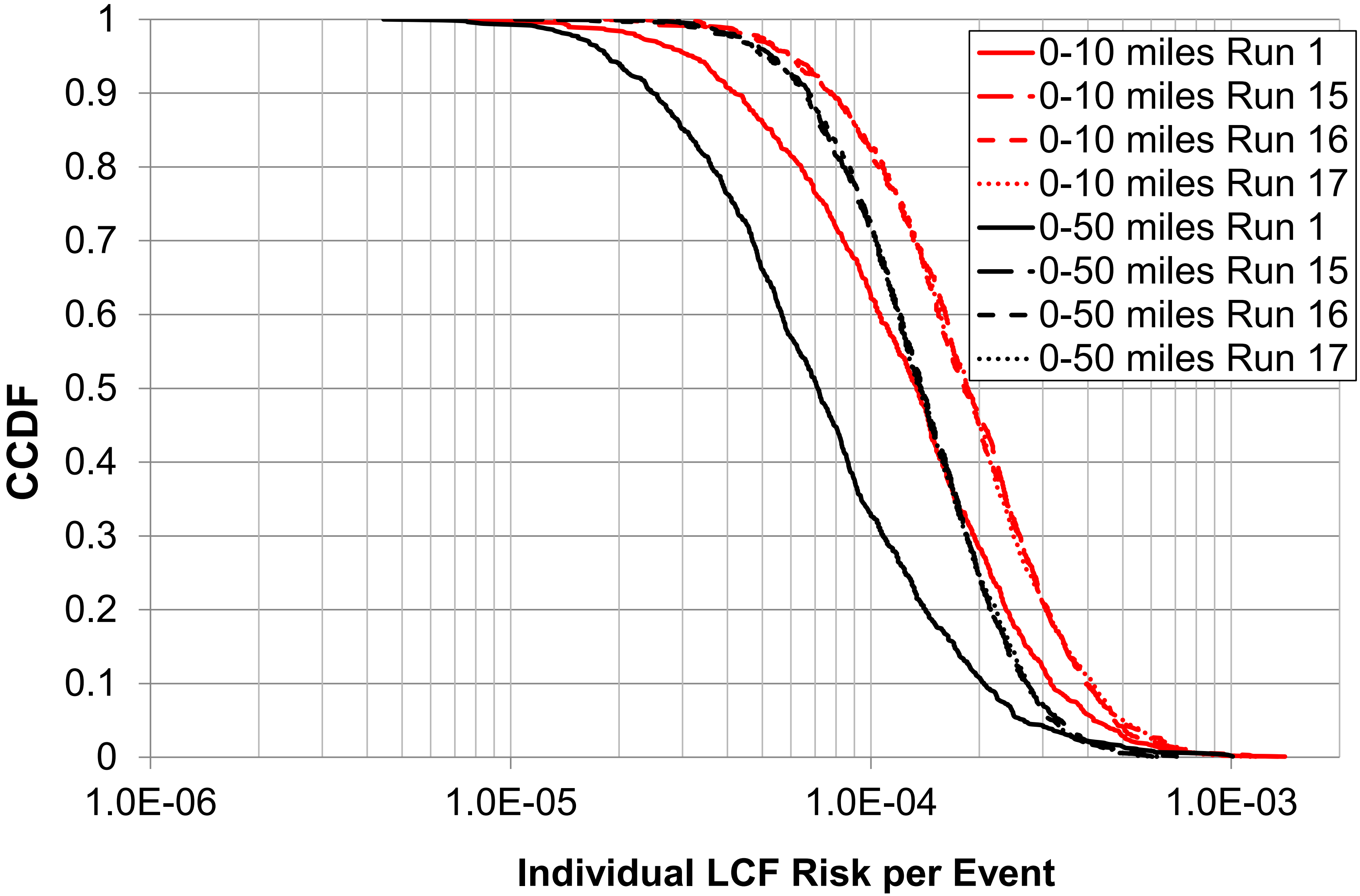
Runs 9-11 and Run 1 Epistemic Uncertainty Conditional, Mean, Individual LCF Risk (per Event) CCDFs



Runs 12-14 (medium) and Run 1 Epistemic Uncertainty Conditional, Mean, Individual LCF Risk (per Event) CCDFs



Runs 15-17 (high) and Run 1 Epistemic Uncertainty Conditional, Mean, Individual LCF Risk (per Event) CCDFs



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Average difference between the three separate LHS runs over all Aleatory Weather Distributions (1st to 99th percentile)

Source Term	Conditional LCF Risk 0-10 miles	Conditional LCF Risk 0-50 miles
Highest Prompt Fatality Risk – Runs 3-5	0.8%	0.8%
Highest LCF Risk – Runs 6-8	0.8%	0.9%
Low – Runs 9-11	0.9%	0.8%
Medium – Runs 12-14	0.8%	0.9%
High – Runs 15-17	1.0%	0.6%
Overall Average	0.9%	0.8%

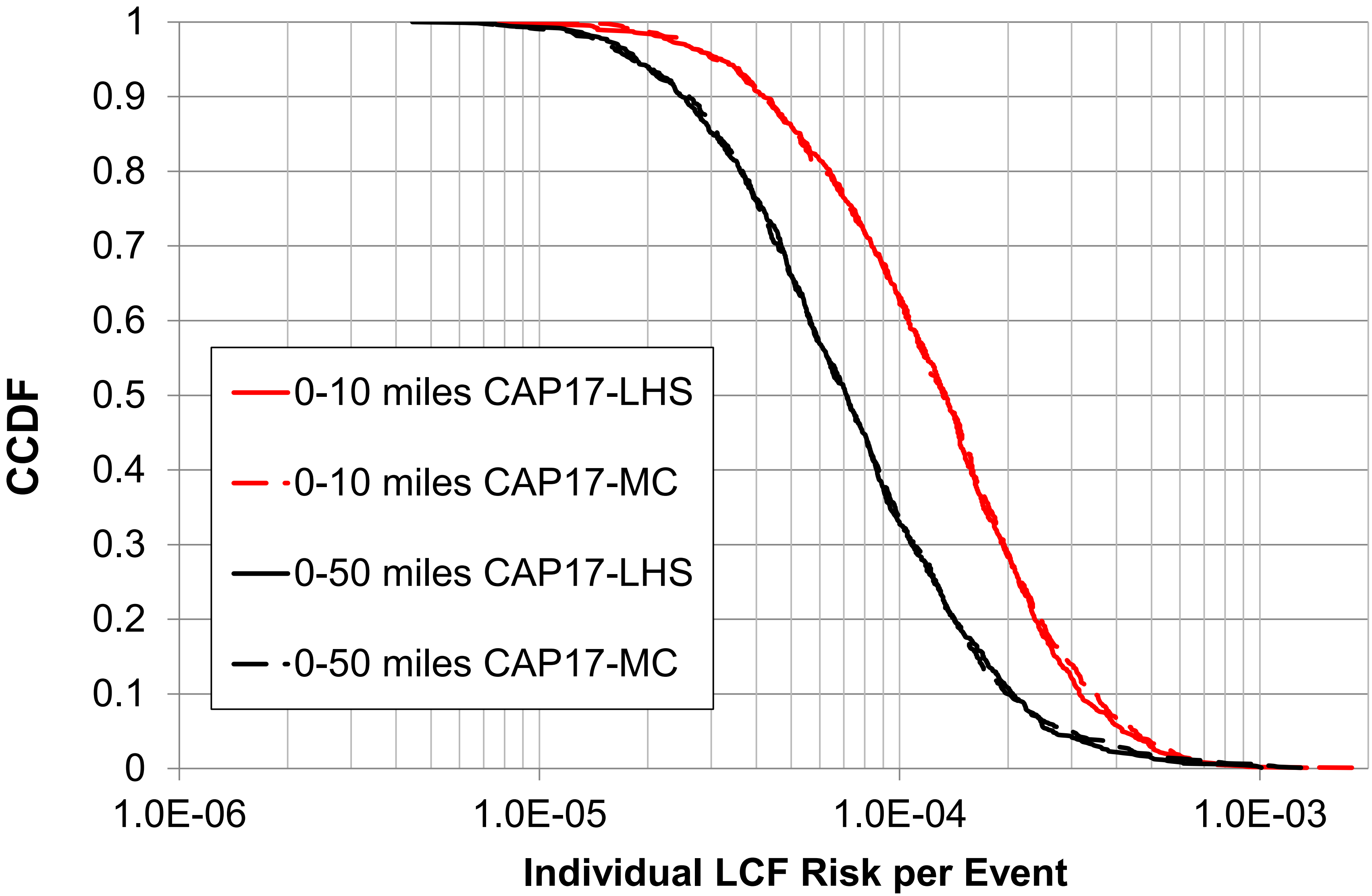
MACCS2 Stability Analysis Using Bootstrap Approach

Approach:

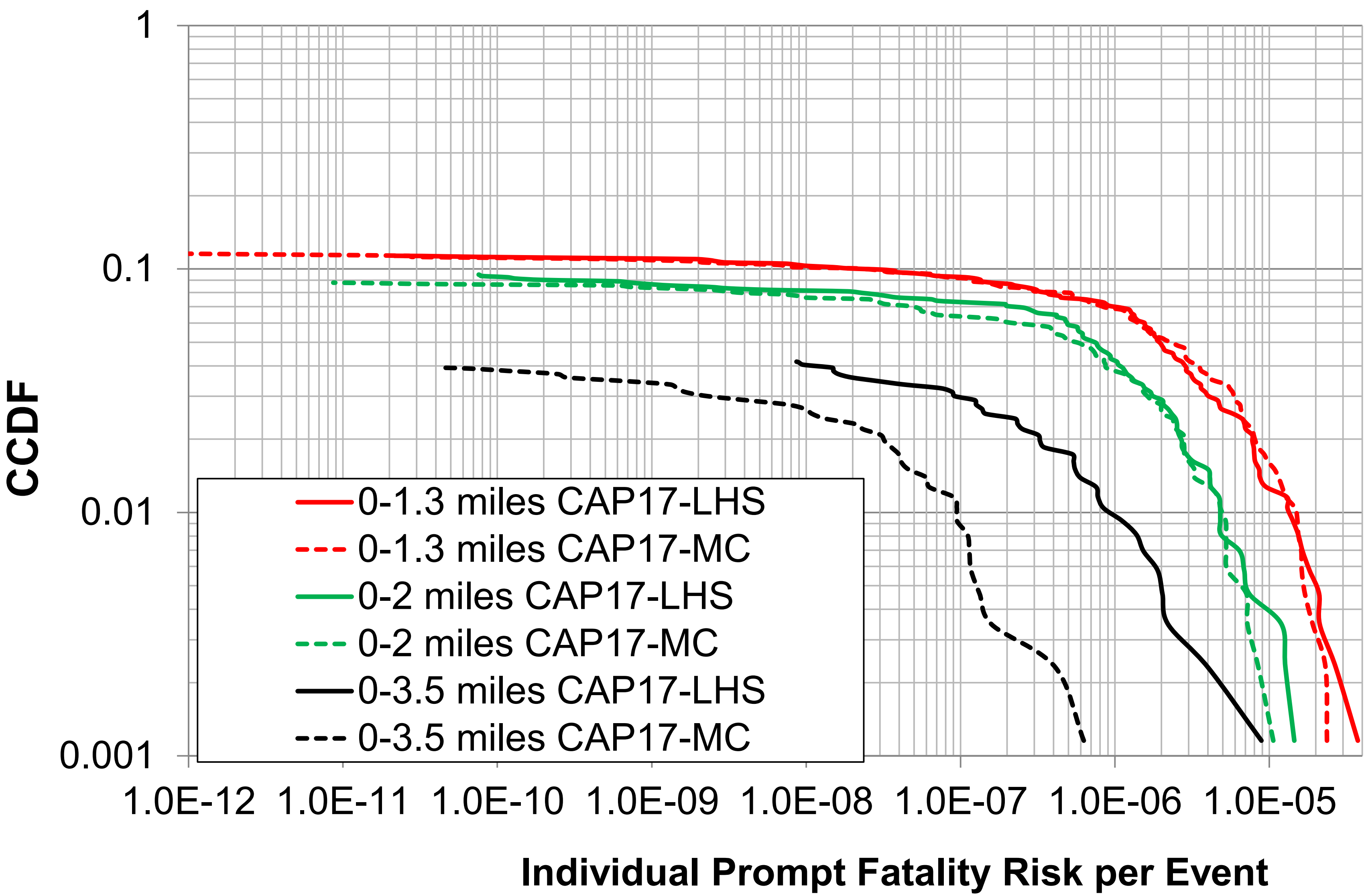
- MACCS2 code modified to allow simple random sampling
- The 'high' source term (i.e., Replicate 1 Realization 290) and the SOARCA UA MACCS2 Analysis (Run 1) were selected to compare between Simple Random Sampling (SRS or MC) and Latin Hypercube Sampling (LHS) in order to validate the use of LHS
- Bootstrapping performed (similar to approach with MELCOR results) to estimate confidence bounds

- Conclusion: Results of the uncertainty analysis are well converged and LHS use is valid

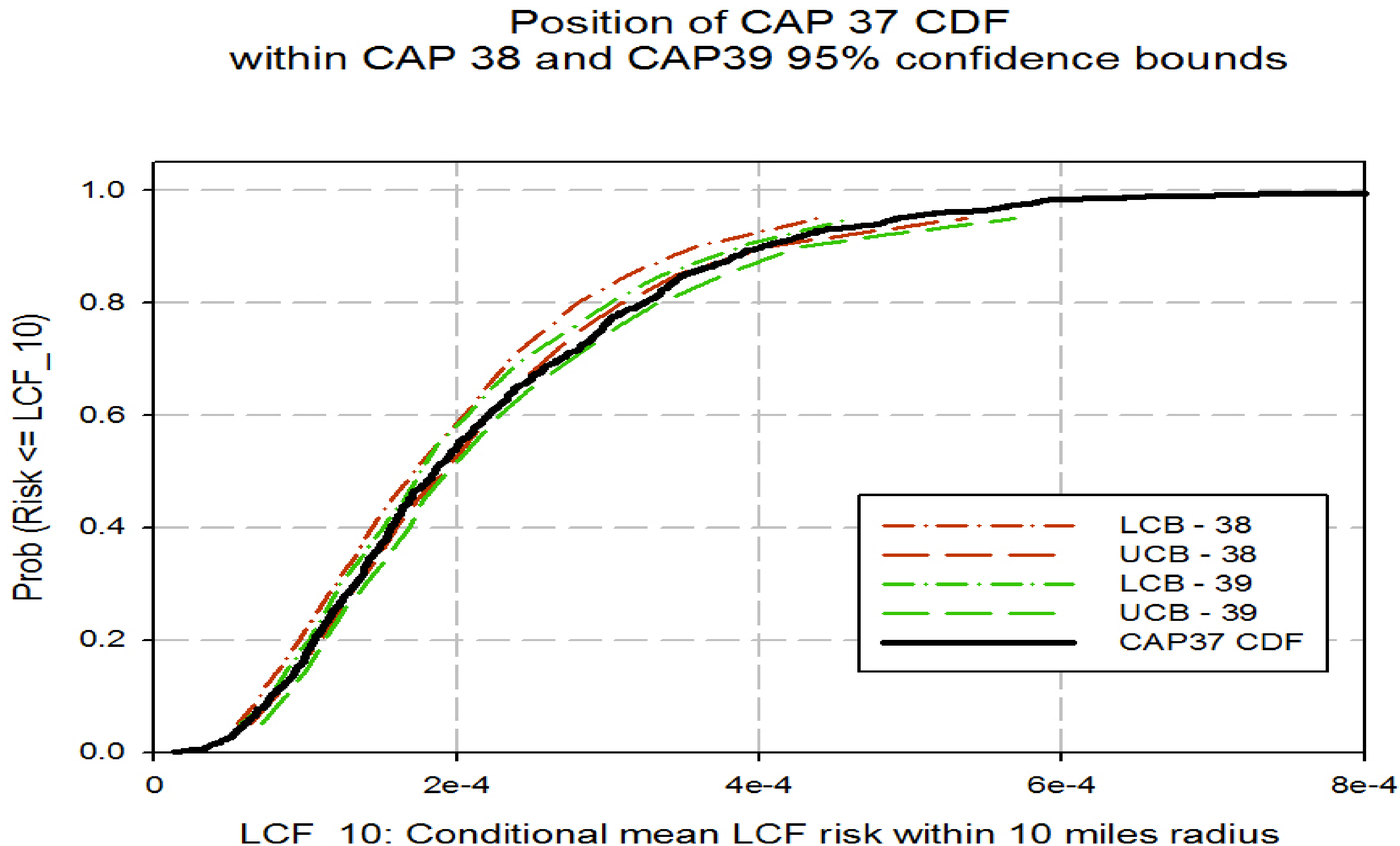
Run 1 (CAP17) Conditional, Mean, Individual LCF Risk (per Event) CCDF with LHS and MC Sampling



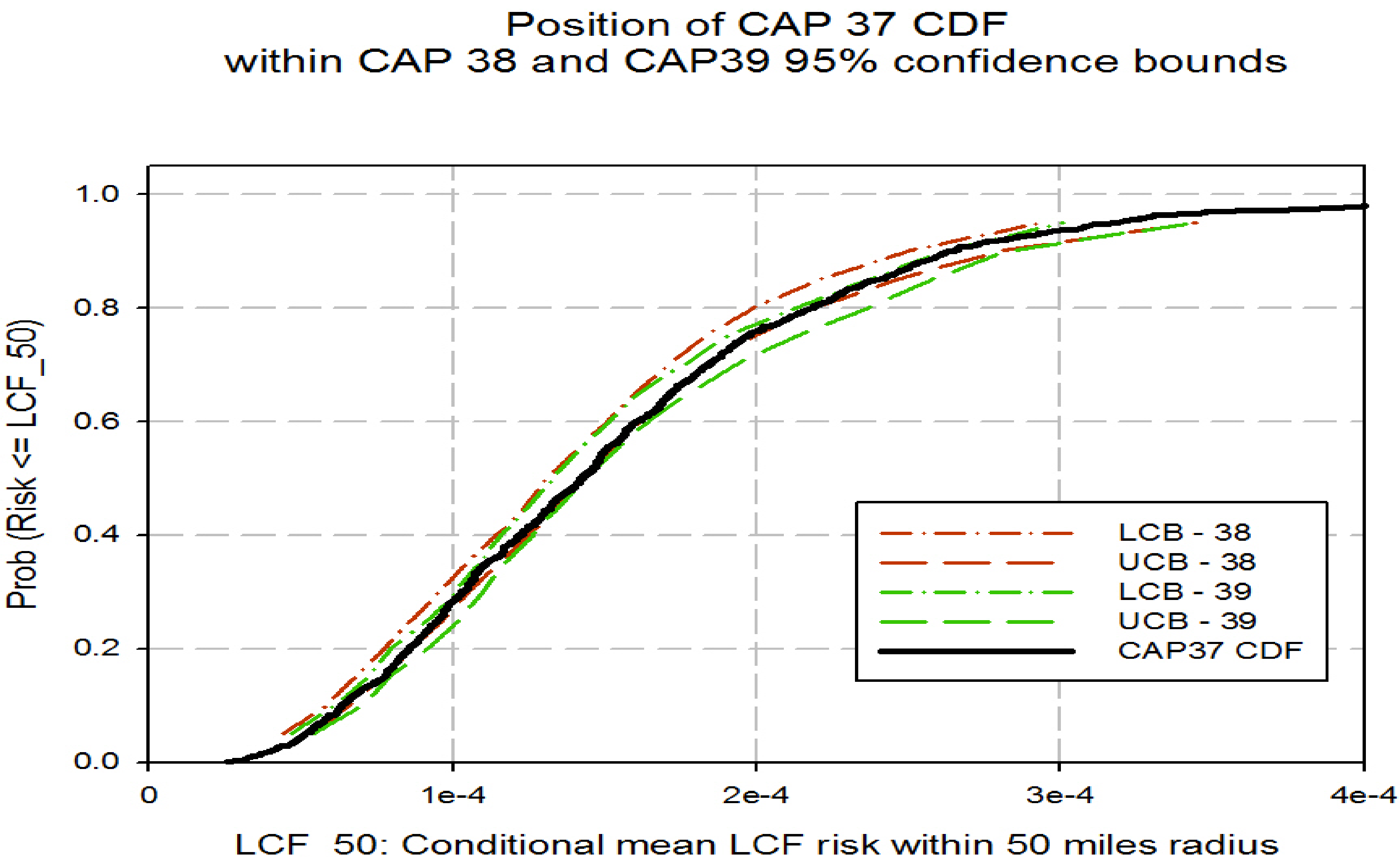
Run 1 (CAP17) Conditional, Mean, Individual Prompt Fatality Risk (per Event) CCDF with LHS and MC Sampling



10-mile Conditional, Mean, Individual LCF Risk (per Event) CDF for Run 15 (CAP37) and 95% Confidence Interval Upper and Lower Bounds for Runs 16 & 17 (CAP38 & 39) with SRS



50-mile Conditional, Mean, Individual LCF Risk (per Event) CDF for Run 15 (CAP37) and 95% Confidence Interval Upper and Lower Bounds for Runs 16 & 17 (CAP38 & 39) with SRS



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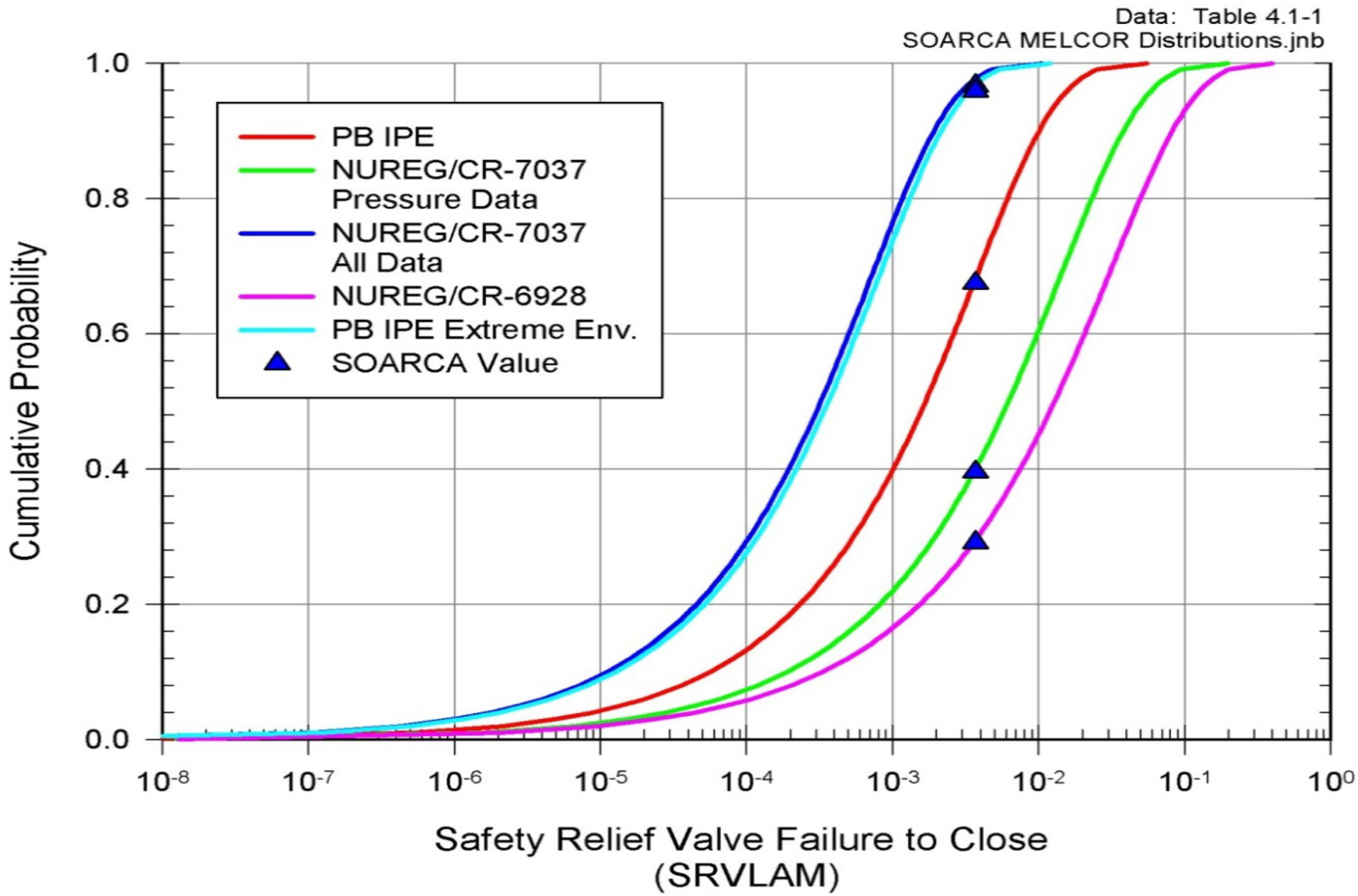
MELCOR Parameters of Interest

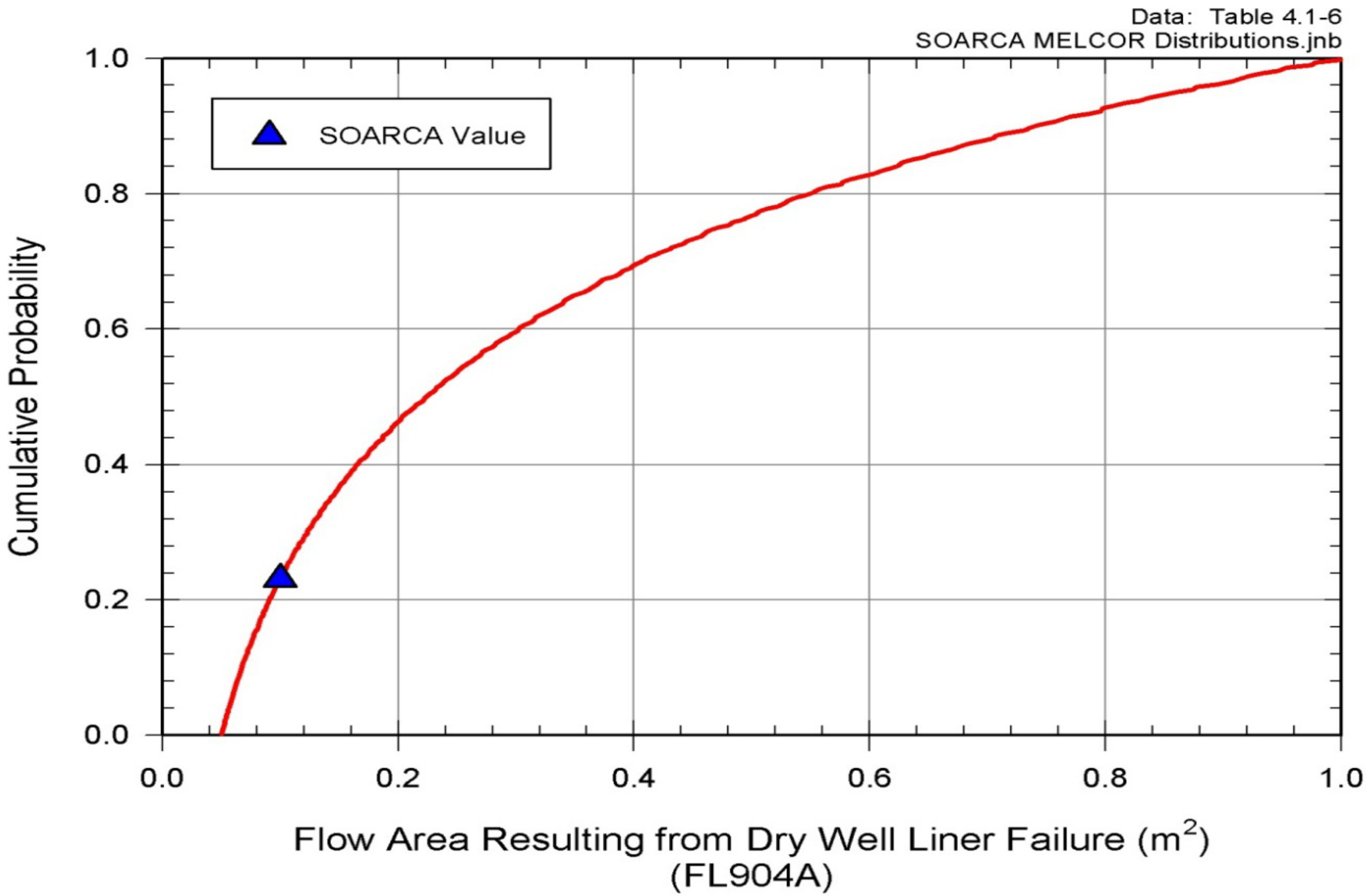
SRVLAM – SRV stochastic failure to reclose



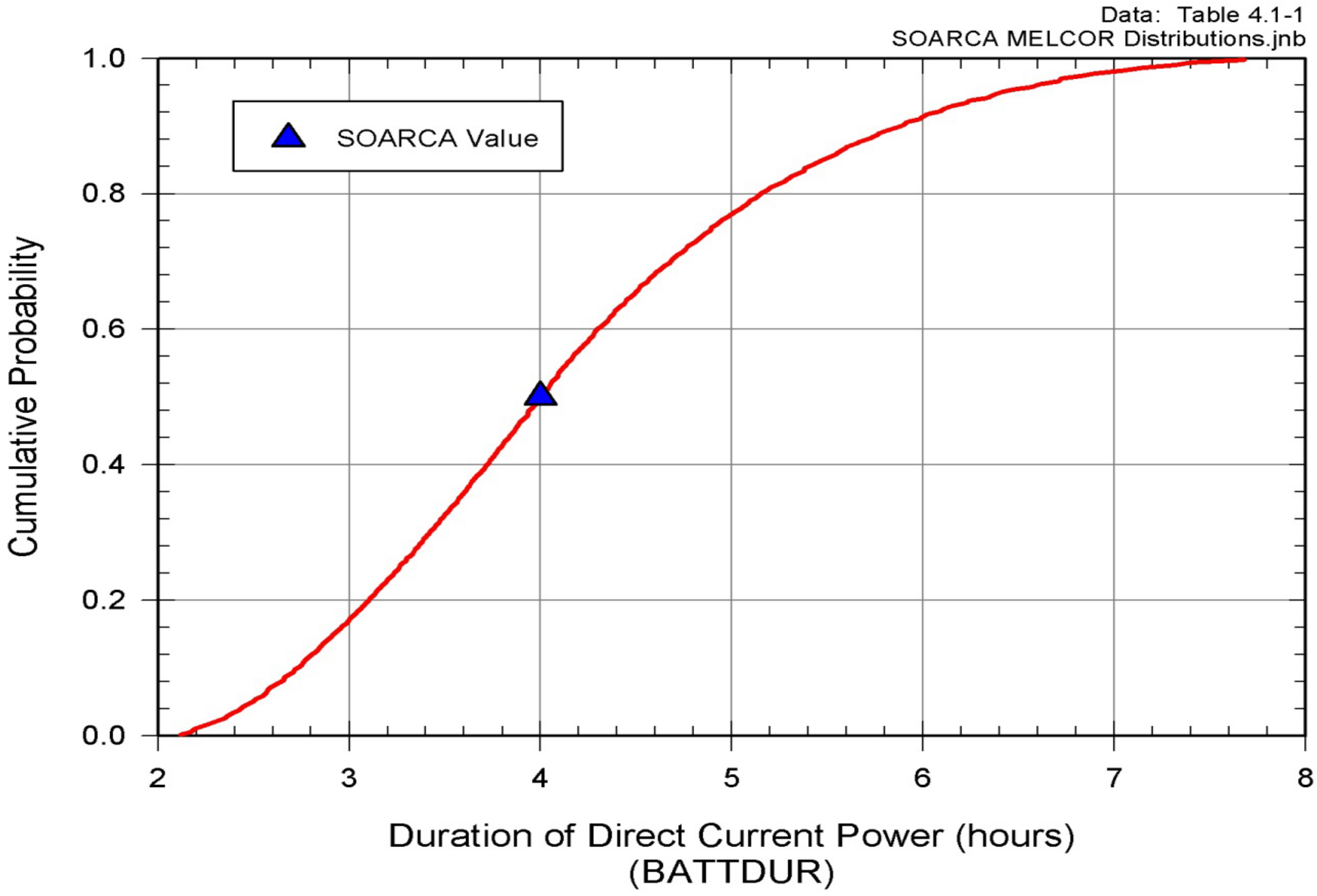
CHEMFORM – Iodine and cesium fraction

Parameter		Distribution			
CHEMFORM: Five alternative combinations of RN classes 2, 4, 16, and 17 (CsOH, I_2, CsI, and Cs_2MoO_4) Note the fraction cesium below represents the distribution of 'residual' cesium which is the mass of cesium remaining after first reacting with the amount of iodine assumed to form CsI.		Discrete distribution Combination #1 = 0.125 Combination #2 = 0.125 Combination #3 = 0.125 Combination #4 = 0.125 Combination #5 = 0.500			
Five Alternatives		**Species (MELCOR RN Class)**			
		CsOH (2)	I_2 (4)	CsI (16)	Cs_2MO_4 (17)
Combination #1	fraction iodine	--	0.03	0.97	--
	fraction cesium	1	--	--	0
Combination #2	fraction iodine	--	0.002	0.998	
	fraction cesium	0.5	--	--	0.5
Combination #3	fraction iodine	--	0.00298	0.99702	--
	fraction cesium	0	--	--	1
Combination #4	fraction iodine	--	0.0757	0.9243	--
	fraction cesium	0.5	--	--	0.5
Combination #5	fraction iodine	--	0.0277	0.9723	--
	fraction cesium	0	--	--	1
SOARCA estimate	Fraction iodine	--	0.0	1.0	--
	Fraction cesium	0.0	--	--	1.0

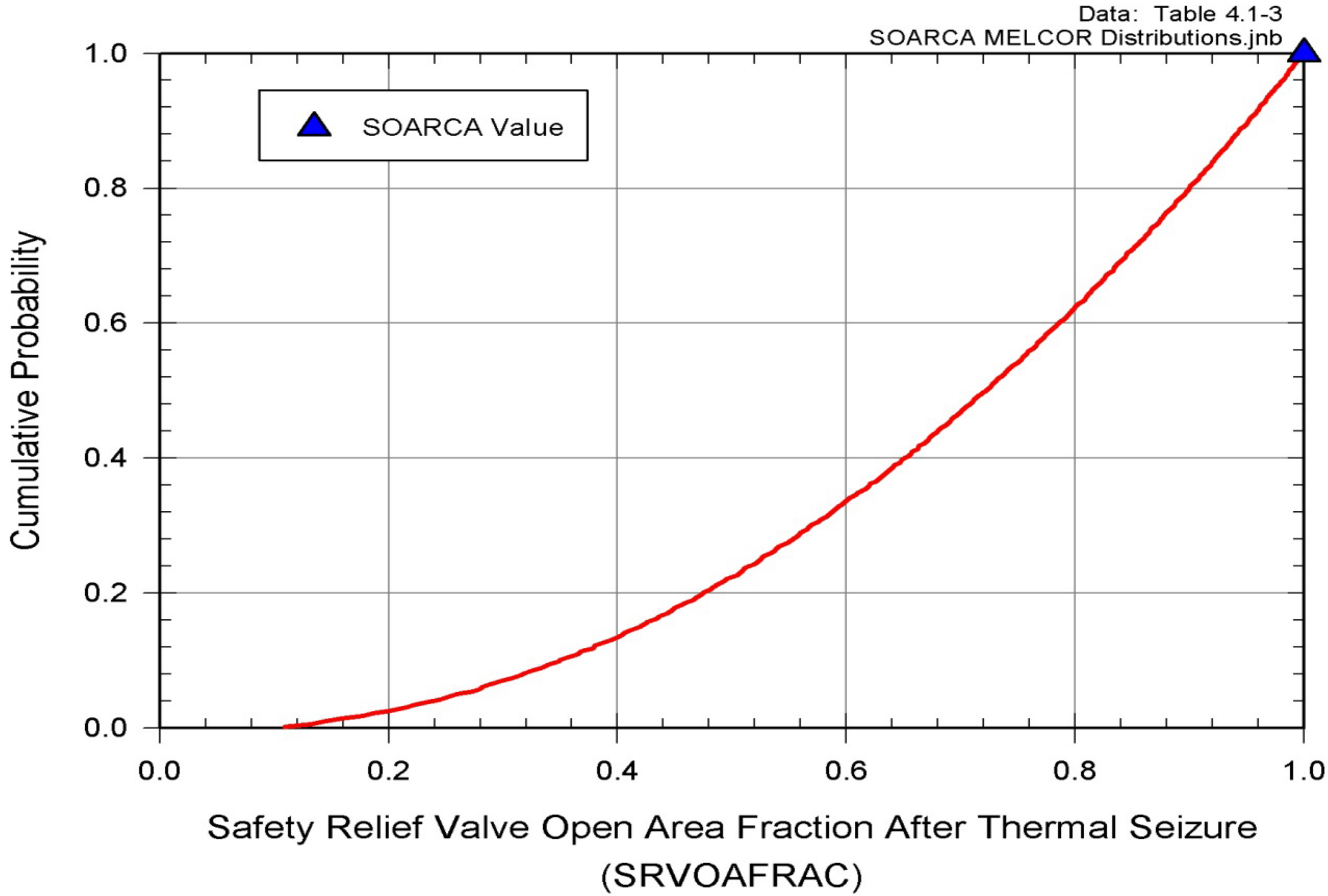
FL904A – Drywell liner failure flow area



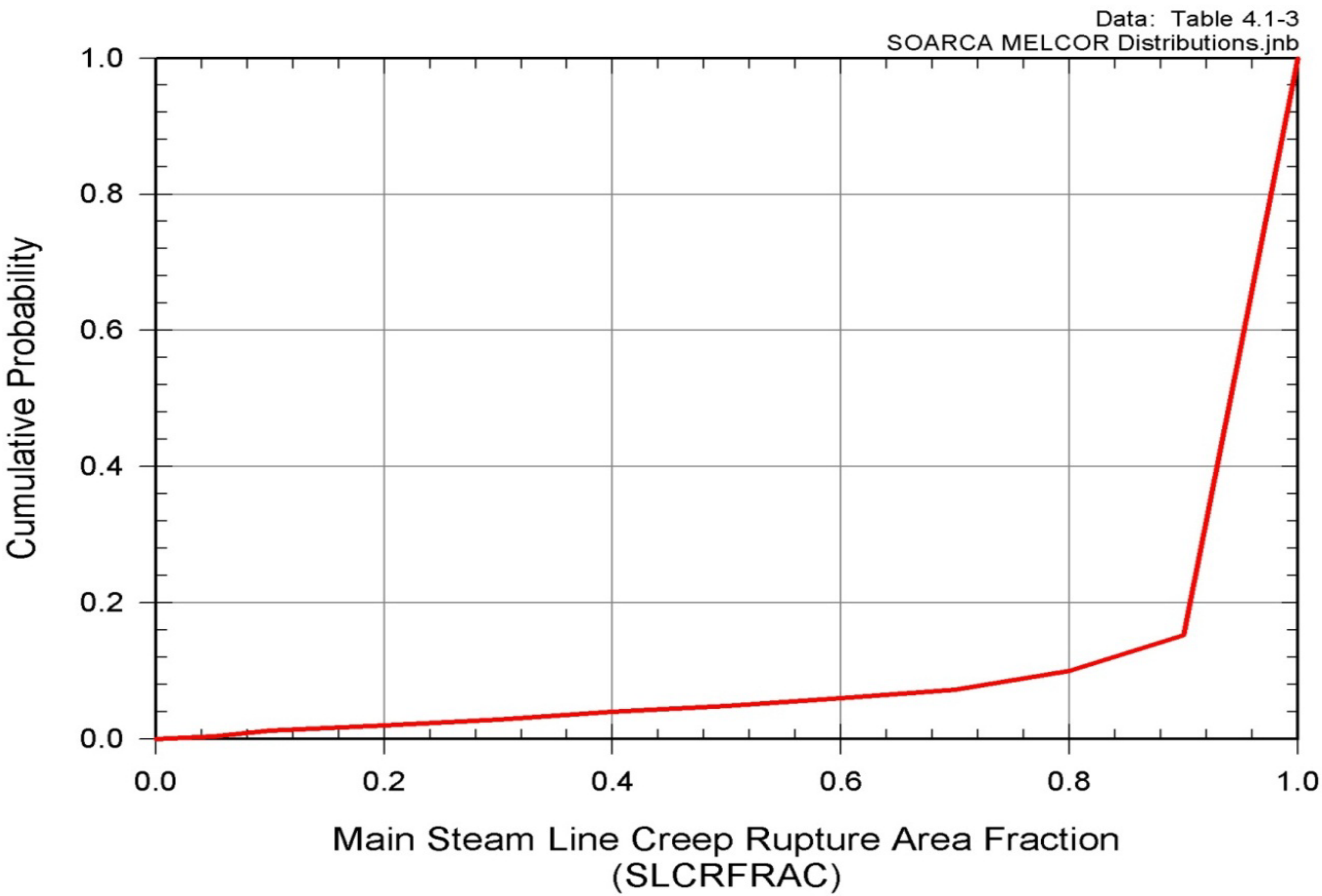
BATTDUR – Battery Duration



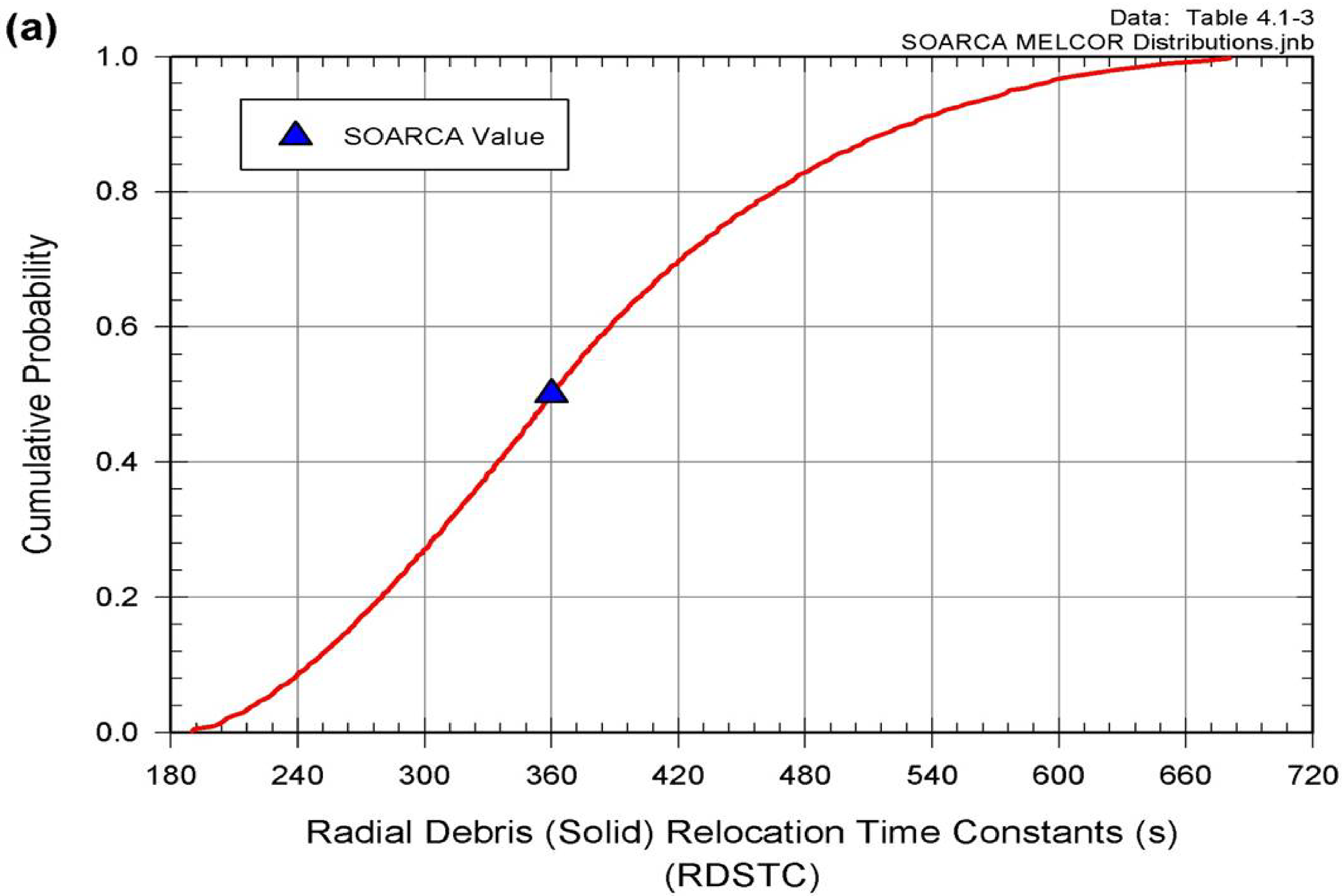
SRVOAFRAC – SRV open area fraction



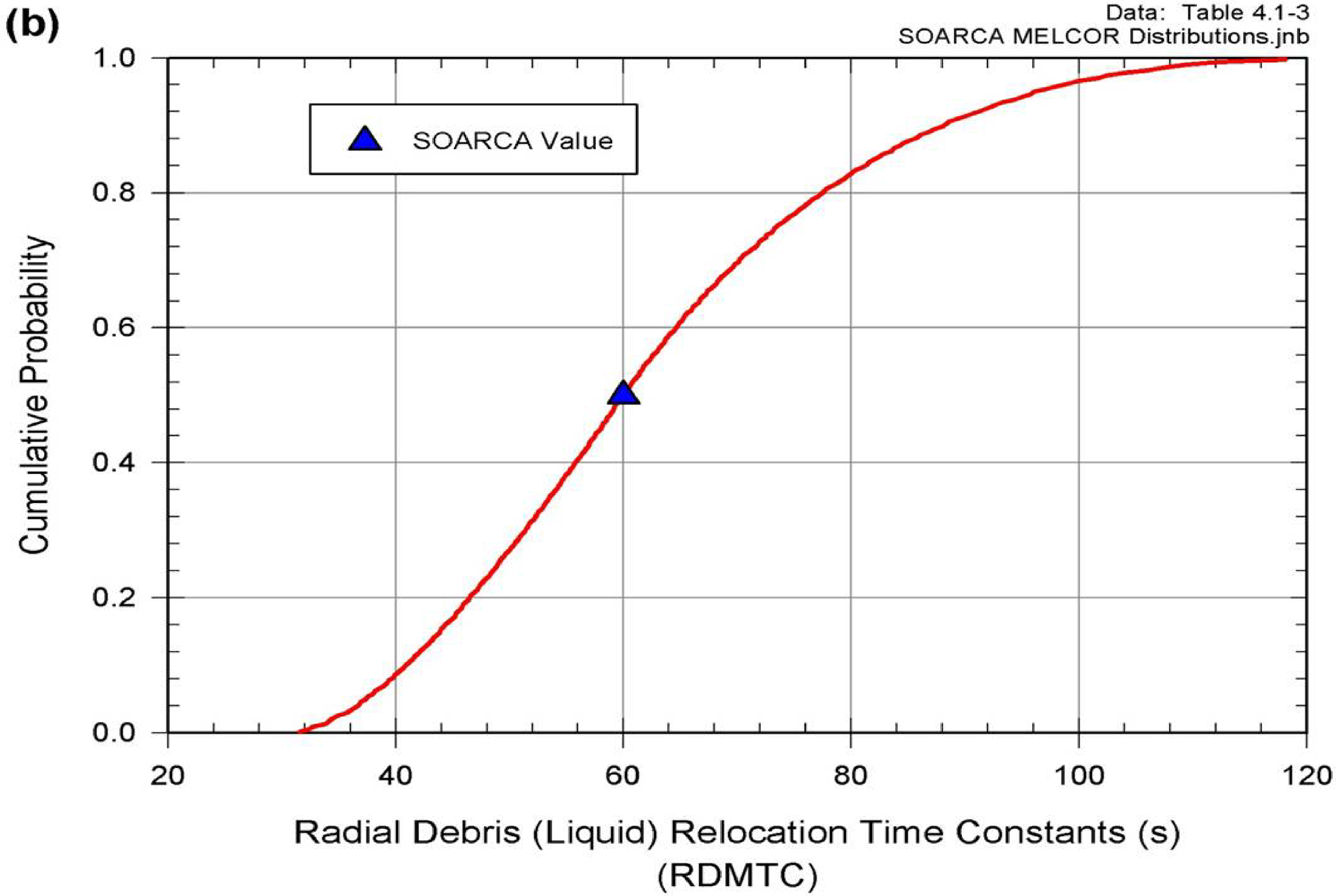
SLCRFRAC – Main steam line creep rupture area fraction



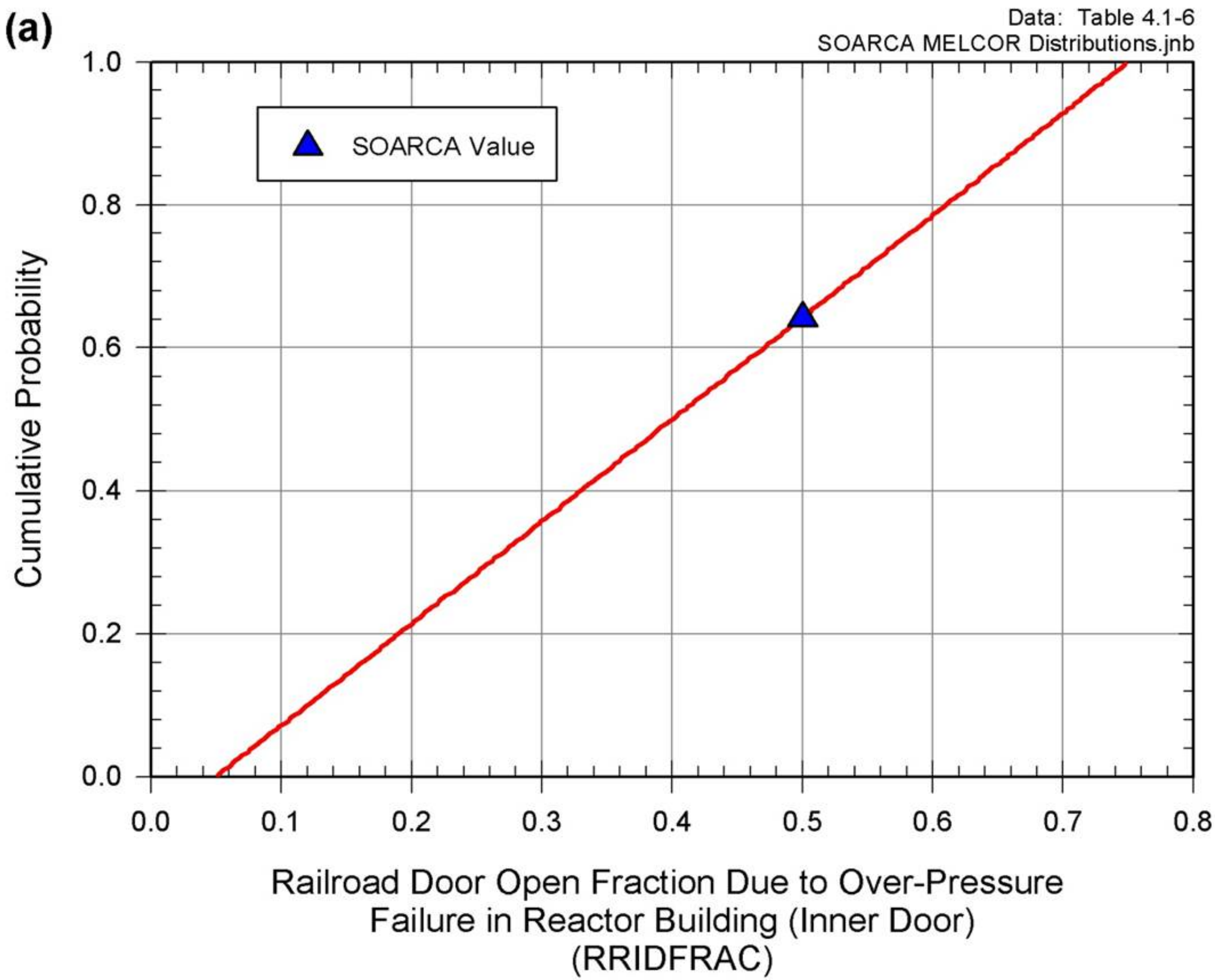
Radial debris relocation time constants – RDSTC (solid)



Radial debris relocation time constants – RDMTC (liquid)



RRIDRFRAC, RODRFRAC – Railroad door open fraction



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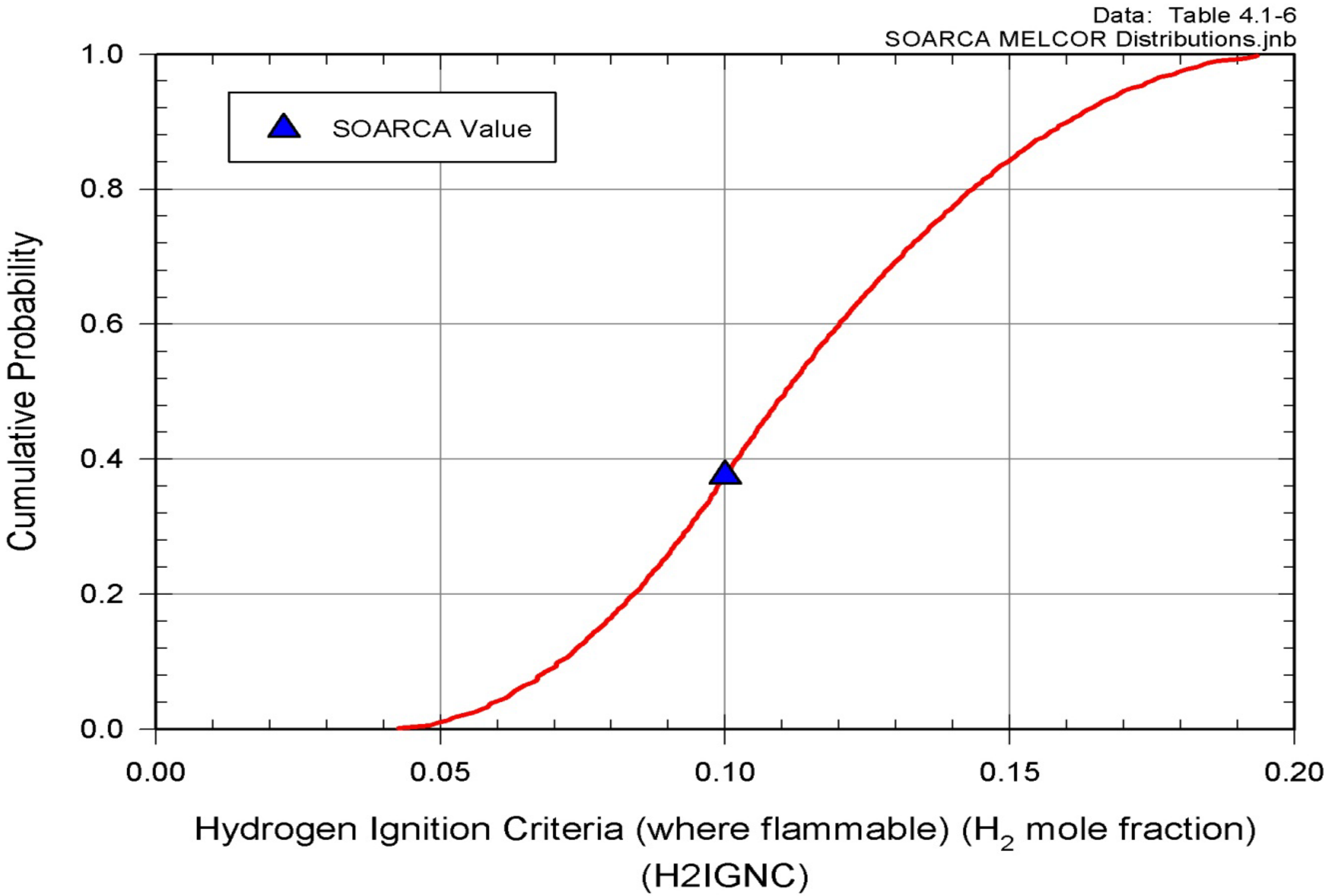
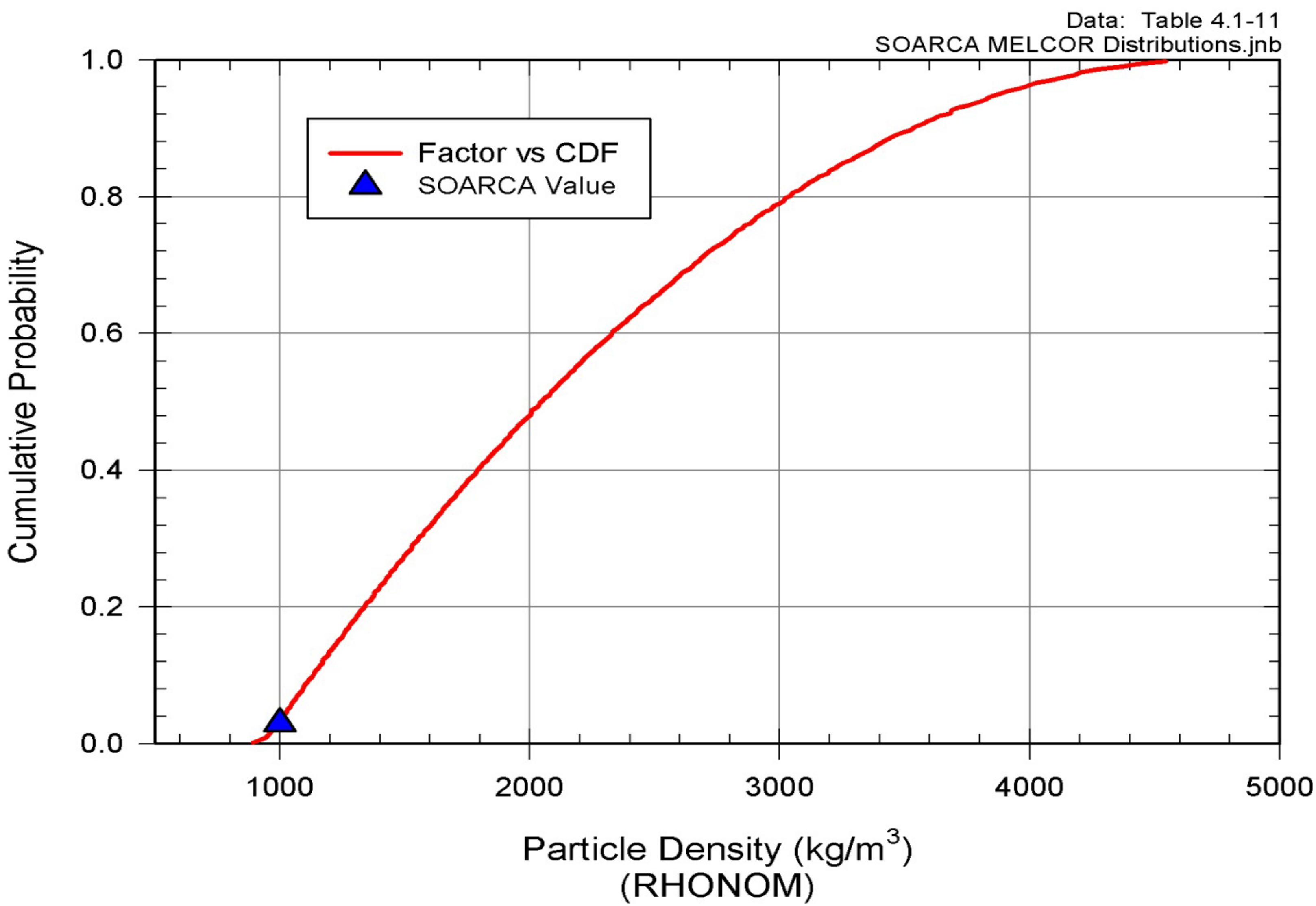
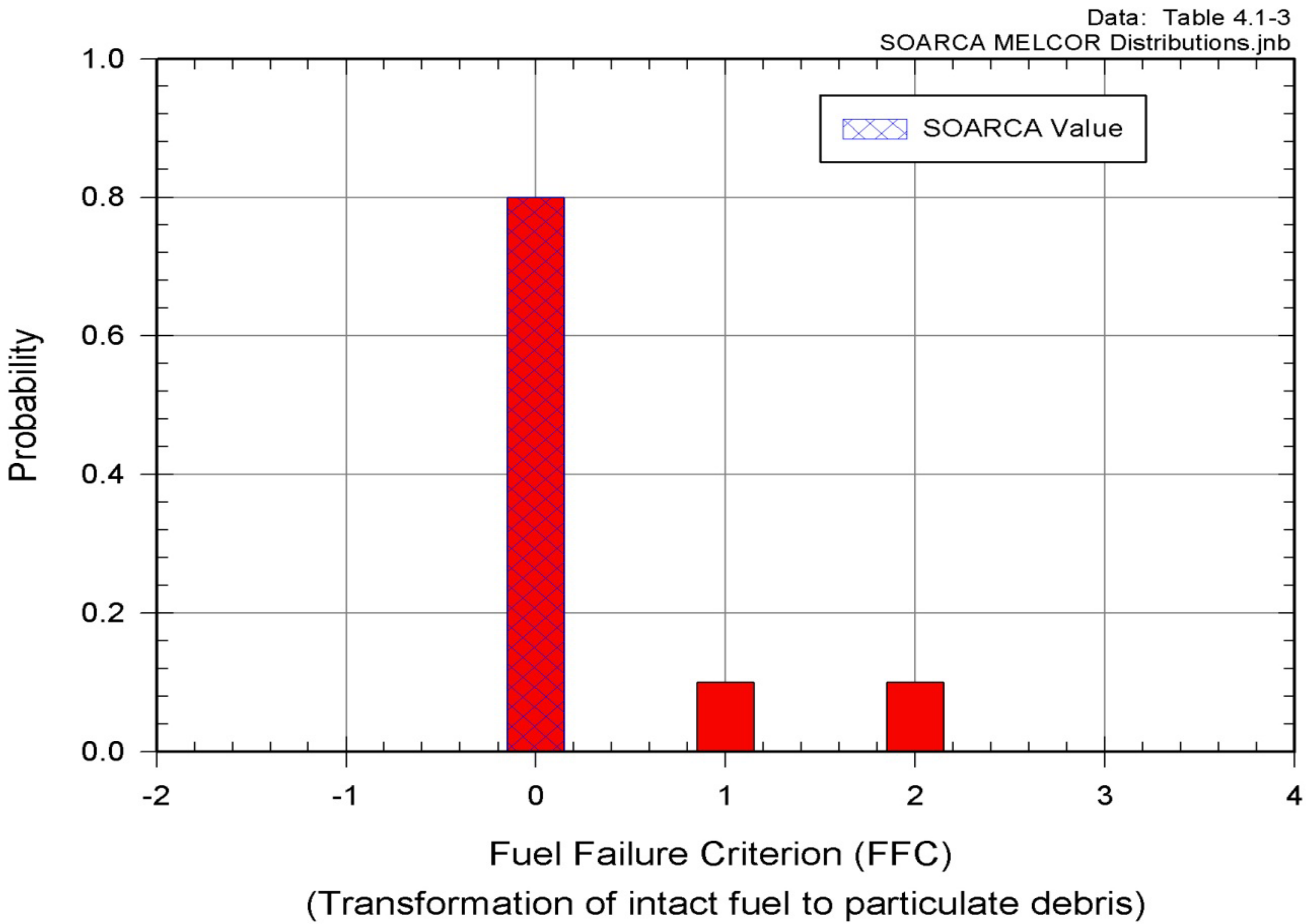
H2IGNC – Hydrogen ignition criteria



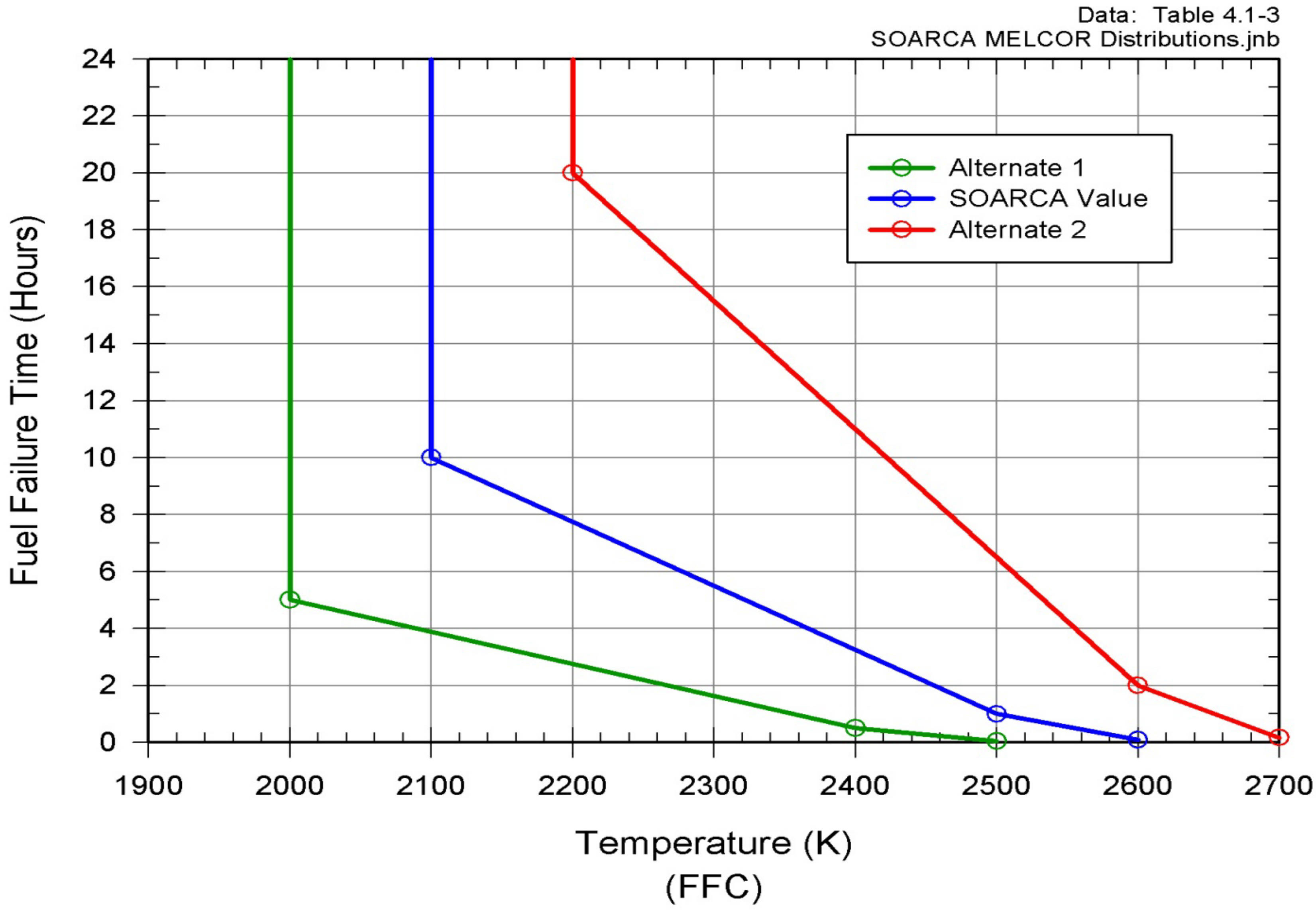
RHONOM – Particle density



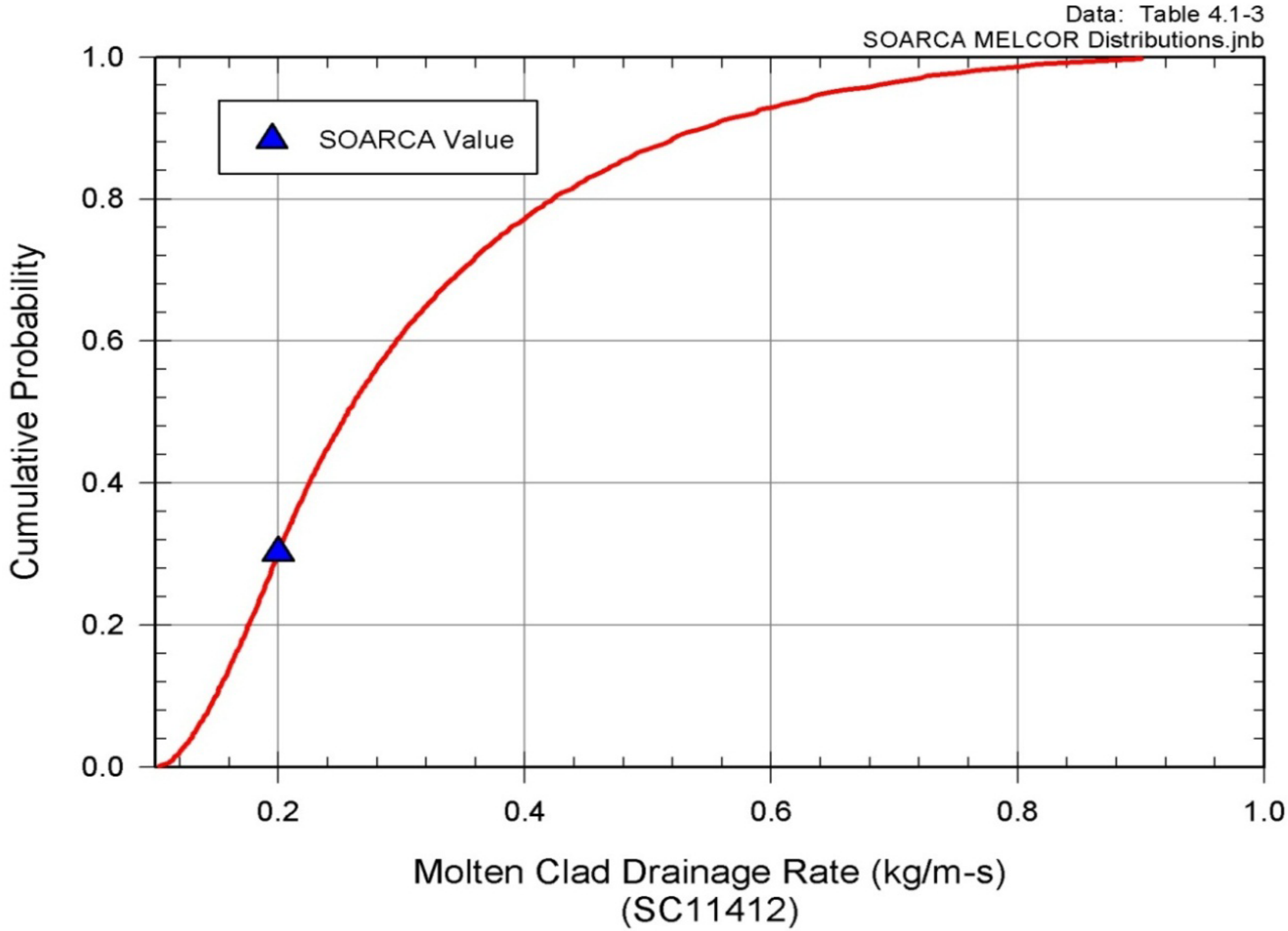
FFC – Fuel failure criterion



FFC – Fuel failure criterion (continued)



SC1141(2) – Molten clad drainage rate



Other MELCOR Items of Interest

- Surrogate parameters
- Lower head penetration failures
- Drywell liner failure model
- Operator actions

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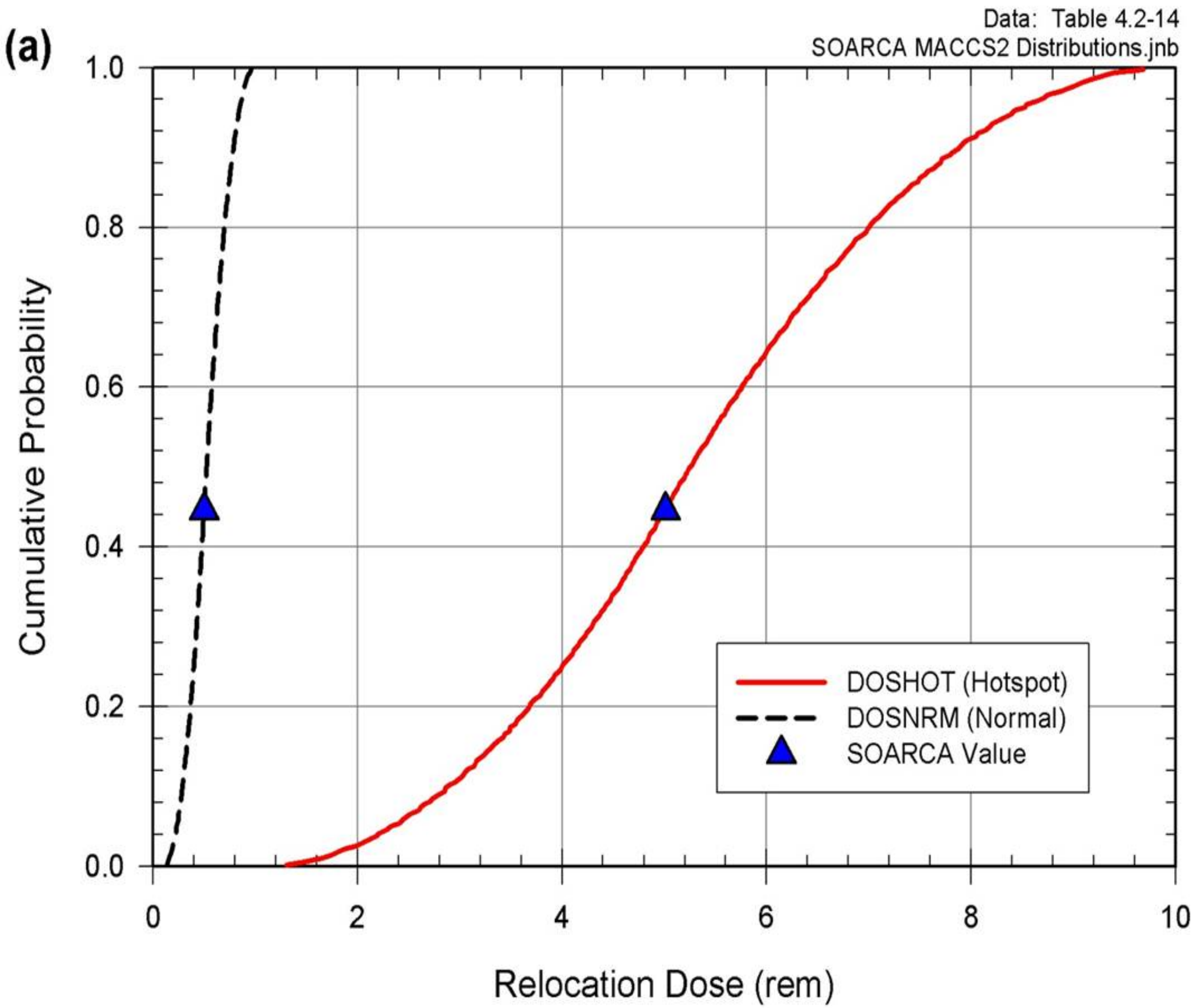
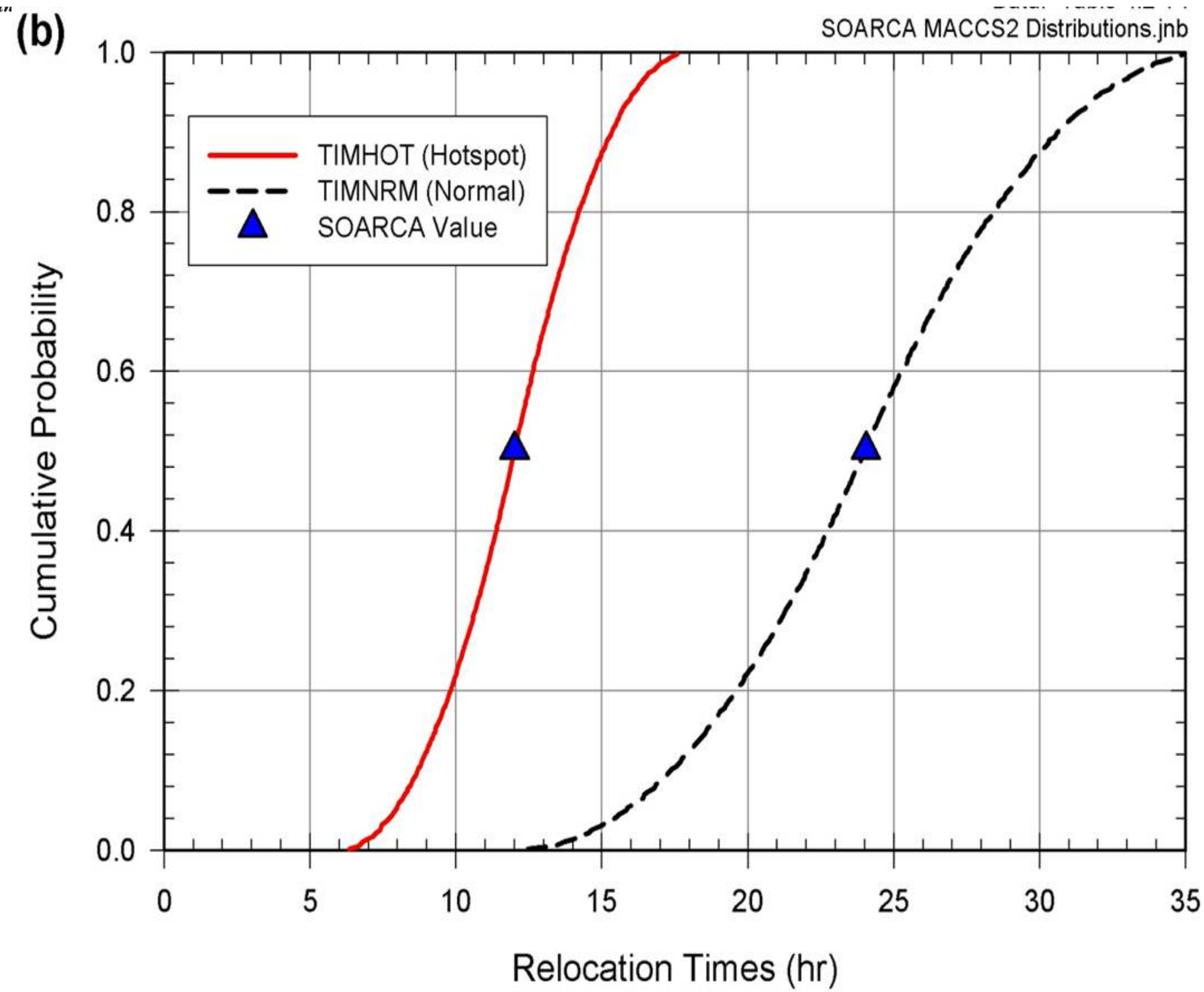
MACCS2 Parameters of Interest

DOSNRM, DOSHOT – Normal and Hotspot Relocation Doses

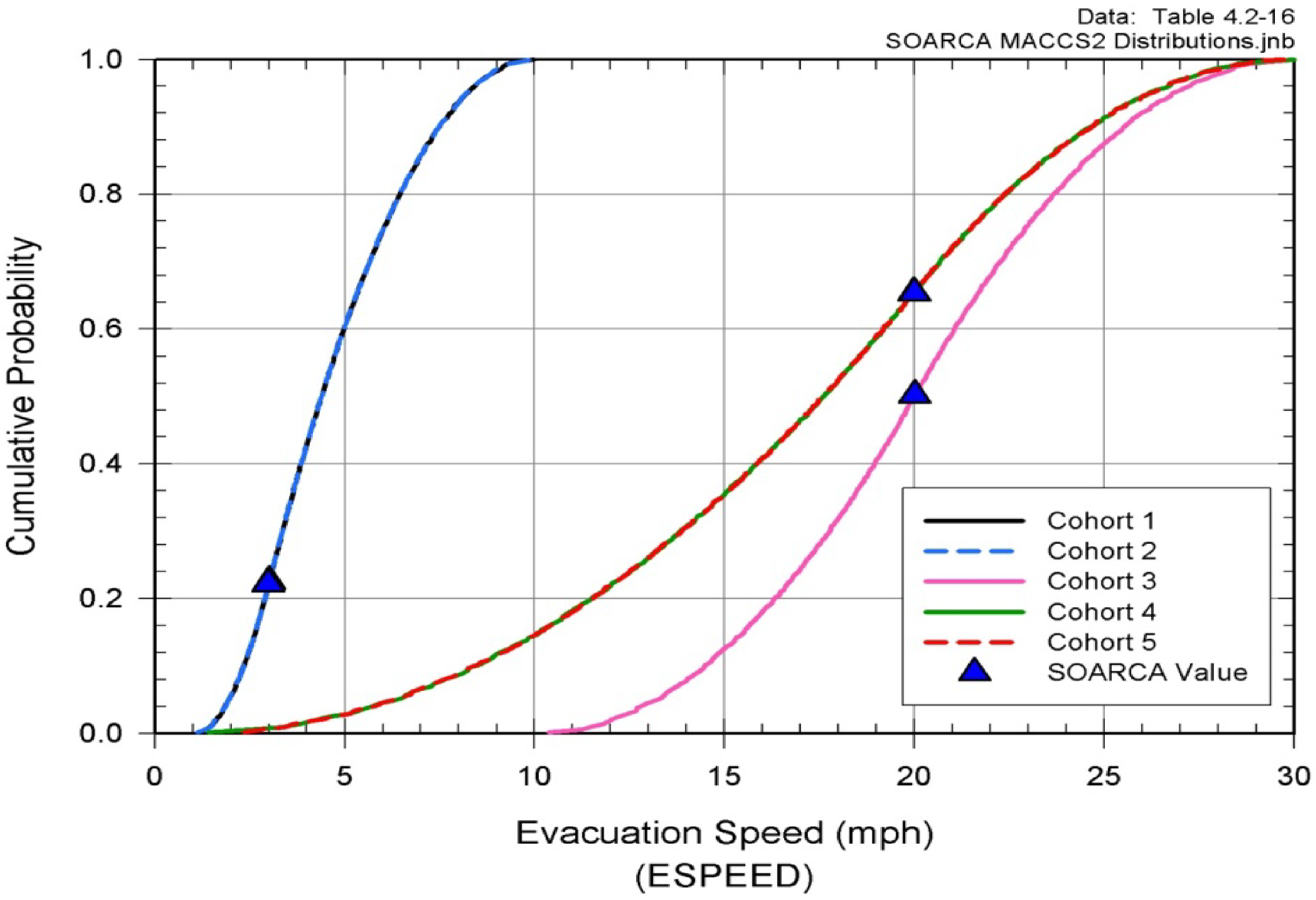


Data: Table 4.2-14

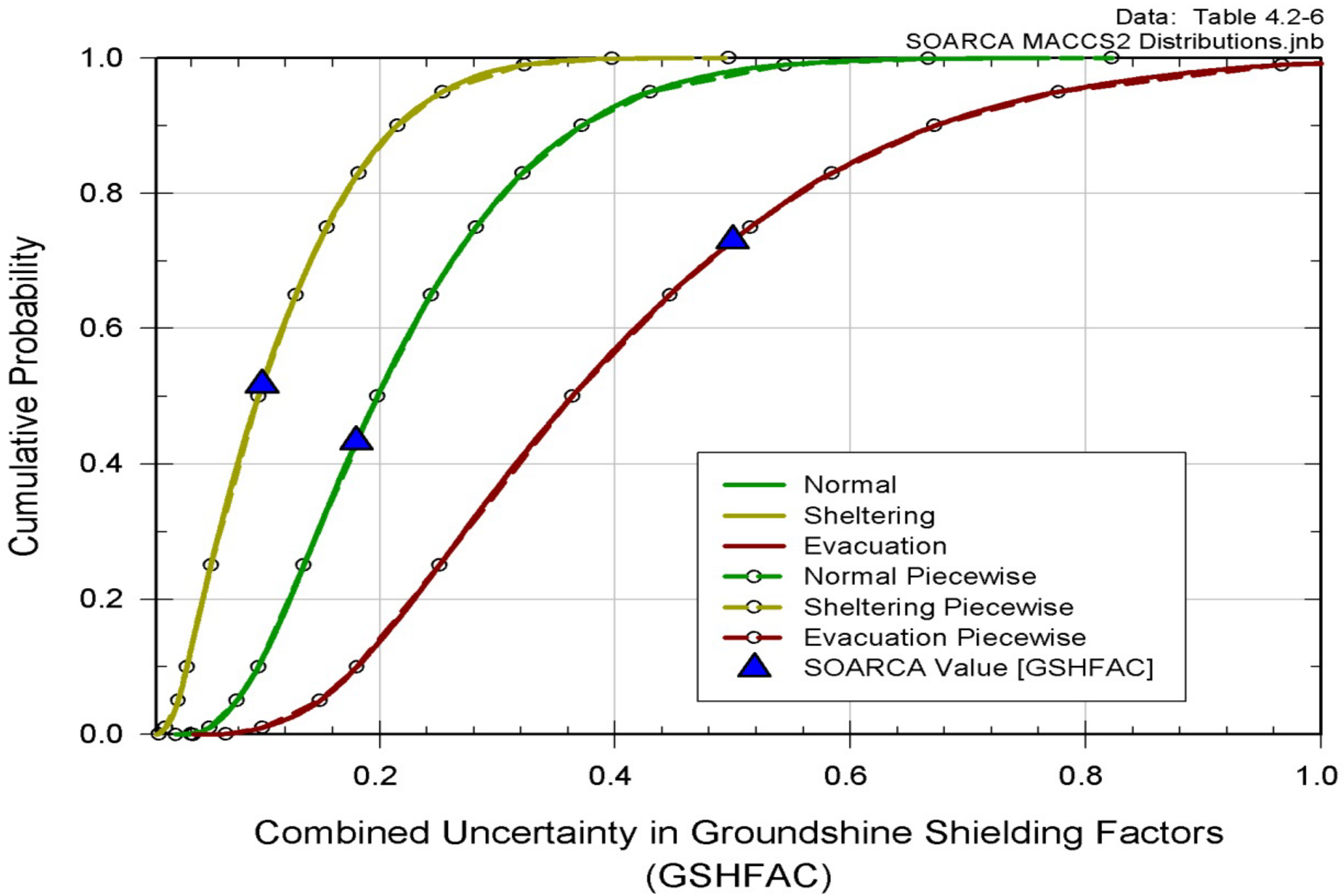
TIMNRM, TIMHOT – Normal and Hotspot Relocation Times



ESPEED – Evacuation speed



GSHFAC – Groundshine Shielding Factor



Next Steps

- ANS PSA Conference presentation and papers and CSARP presentation
  - September 2013

- Send final NUREG/CR-7155 report for publication – Fall 2013

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Questions and Comments

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Note that all results in these presentation slides are conditional (per event) on the potential occurrence of a long-term station blackout (LTSBO) scenario, and modeling the SOARCA unmitigated LTSBO.
The LTSBO scenario frequency is estimated in SOARCA to be ~3x10^{-6} per reactor year.