

Official Transcript of Proceedings

NUCLEAR REGULATORY COMMISSION

Title: Advisory Committee on Reactor Safeguards
 Regulatory Policies and Practices

Docket Number: (n/a)

Location: Rockville, Maryland

Date: Monday, September 16, 2013

Work Order No.: NRC-261

Pages 1-361

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1 UNITED STATES OF AMERICA

2 NUCLEAR REGULATORY COMMISSION

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4 ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

5 (ACRS)

6 + + + + +

7 REGULATORY POLICIES AND PRACTICES SUBCOMMITTEE

8 + + + + +

9 MONDAY

10 SEPTEMBER 16, 2013

11 + + + + +

12 ROCKVILLE, MARYLAND

13 + + + + +

14 The Subcommittee met at the Nuclear
15 Regulatory Commission, Two White Flint North, Room T2B1,
16 11545 Rockville Pike, at 8:30 a.m., John W. Stetkar,
17 Subcommittee Chairman, presiding.

18 COMMITTEE MEMBERS:

19 JOHN W. STETKAR, Chairman

20 DENNIS C. BLEY, Member

21 MICHAEL L. CORRADINI, Member

22 JOY REMPE, Member

23 MICHAEL T. RYAN, Member

24 STEPHEN P. SCHULTZ, Member

25 WILLIAM J. SHACK, Consultant

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1
2 NRC STAFF PRESENT:

3 HOSSEIN NOURBAKHS, Designated Federal

4 Official

5 ED FULLER, RES

6 KATHY GIBSON, RES

7 TINA GHOSH, RES

8 PATRICIA SANTIAGO, RES

9
10 ALSO PRESENT:

11 NATHAN BIXLER, SNL

12 RANDY GAUNTT, SNL

13 JOE JONES, SNL*

14 DOUGLAS OSBORNE, SNL*

15 KYLE ROSS, SNL*

16 CEDRIC SALISBURY, SNL*

17 *Present via telephone
18
19
20
21
22
23
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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.

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1 A transcript of the meeting is being kept
2 and will be made available as stated in the Federal
3 Register notice. It is requested that speakers first
4 identify themselves and speak with sufficient clarity
5 and volume so that they can be readily heard.

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

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

15 MR. JONES: This is Joe with Sandia.

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

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

25 I led - our branch led the state of the art

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1 reactor consequence analysis project and the associated
2 Peach Bottom uncertainty analysis that we'll discuss
3 today.

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

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

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

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

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

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

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

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

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

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

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

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1 actually for the whole day.

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

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

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

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

23 But we did not separately break out the -
24 showing the distribution of the weather results for
25 particular runs. So the first comment was that for the

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1 combined - for the existing combined MELCOR MACCS results
2 we should also include and display the full weather
3 aleatory uncertainty.

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

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

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

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

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

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

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

10 DR. GHOSH: Yes, and the fact -

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

14 DR. GHOSH: Yeah.

15 CHAIRMAN STETKAR: - by characterizing it
16 that way.

17 DR. GHOSH: Right.

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

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

22 CHAIRMAN STETKAR: Isn't it?

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

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

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

5 CHAIRMAN STETKAR: That though is the
6 question.

7 DR. GHOSH: - the mean of what? This is the
8 -

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

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

13 CHAIRMAN STETKAR: Okay.

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

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

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

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

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1 latent cancer fatality.

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

6 This is for the 865 total realizations that
7 we did. So we're showing now the distribution across the
8 865 epistemic ones that we did.

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

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

17 DR. GHOSH: Yes. Right.

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

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

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

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

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1 adding additional confusion here with the -

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

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

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

14 DR. GHOSH: That is correct.

15 CHAIRMAN STETKAR: It is simply a set of
16 numbers.

17 DR. GHOSH: That is correct. Right.

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

20 DR. GHOSH: Right.

21 CHAIRMAN STETKAR: Could be any set of
22 numbers.

23 DR. GHOSH: Yeah. Okay. Right.

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

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1 uncertainties -

2 DR. GHOSH: Yeah, I - yeah.

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

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

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

16 CHAIRMAN STETKAR: There was that
17 discussion.

18 MEMBER BLEY: There was that.

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

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

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

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

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

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

11 MEMBER CORRADINI: And - okay, and then if
12 I might ask a question at this point. Can I? Then the
13 900 and something were sampled from HEINCAS for that site
14 over some time period. In other words, how did you get
15 the 900 and something?

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

20 MEMBER CORRADINI: Just picked 900 of them?

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

24 MEMBER CORRADINI: Independent of
25 chronological time?

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1 DR. GHOSH: Independent of -

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

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

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

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

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

24 DR. BIXLER: That's right, and part of that
25 identification or part of the process is that the release

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1 is changing in time. The weather is changing in time.

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

5 MEMBER CORRADINI: Okay. So I knew all
6 this. So this approach you just spoke about to get the
7 900 and something out of the 8,000 and something is a
8 known approach, an accepted approach, an approach every
9 uses?

10 DR. GHOSH: Yeah. Right.

11 MEMBER CORRADINI: Okay.

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

15 MEMBER CORRADINI: I'm looking at this
16 study -

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

22 MEMBER CORRADINI: And the year you chose
23 was?

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

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

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

5 MEMBER CORRADINI: So for every one of the
6 865 there's really a hidden distribution function that
7 represents the year 2005 -

8 DR. GHOSH: Yes.

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

11 DR. GHOSH: That is correct. Yes.

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

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

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

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

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

6 CHAIRMAN STETKAR: It's -

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

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

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

19 MEMBER BLEY: Of the weather data.

20 CHAIRMAN STETKAR: Of the weather data.

21 DR. GHOSH: Convolved with the model.

22 CHAIRMAN STETKAR: The mean of the weather
23 results.

24 MEMBER BLEY: Mean of the weather results.

25 CHAIRMAN STETKAR: Not on weather data.

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1 MEMBER BLEY: That's true.

2 DR. GHOSH: Yeah, yeah. Exactly right.

3 MEMBER BLEY: Results on calculations.

4 CHAIRMAN STETKAR: Right. Right. Yeah.

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

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

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

19 So for example the 30-mile result, the
20 40-mile result, the 50-mile result the position of the
21 curves will change but in terms of the spread of the
22 uncertainty they're correlated like 99 percent or
23 something.

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

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1 radii.

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

4 DR. GHOSH: Yes. I would say those are -
5 those are the ones that kind of bound the range of, you
6 know, what you're looking at. So and the reason the
7 10-mile is different is because that's the EPV and we have
8 special protective measures.

9 You know, you evacuate the EPV so the folks
10 who are getting dosed in the early phase are just that
11 .5 percent of the population who is assumed not to
12 evacuate and that's the only contribution from the early
13 phase.

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

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

24 MEMBER CORRADINI: And the habitability
25 criteria is what?

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

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

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

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

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

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

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

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

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

24 DR. GHOSH: You know, the -

25 MEMBER CORRADINI: I'm learning.

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

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

5 MS. SANTIAGO: - habitability, yeah.

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

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

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

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

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

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

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

6 I mean, I think in past analyses when we've
7 looked at where does the mean fall with respect to the
8 weather distribution it's, you know, anywhere from the
9 60th to the 80th percentile. So it's well above the
10 median.

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

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

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

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

6 Do you have any sense of how that might
7 change if you sampled 10 years or 50 years or 100 years
8 worth of weather data, recognizing that you're not going
9 to take 100 years times 8,766 starting points but if you
10 broadened those samples and said based on, pick a number
11 - 30 years is usually pretty easy to gather -

12 DR. GHOSH: Yeah.

13 CHAIRMAN STETKAR: - out in the extremes.

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

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

24 DR. GHOSH: Are there any plans - but yeah.
25 I mean, there may be some - you know, it's true there may

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1 be some idiosyncratic things about particular sites but
2 in general when we've seen an evaluation of multiple
3 years of weather data you don't get a huge variation.
4 I'd say -

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

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

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

17 DR. GHOSH: Right.

18 MEMBER BLEY: - on the impact but -

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

24 It's not necessarily a search for extreme
25 years. But there is so much smeared out of results

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1 because the modeling project is so complex that you have
2 so many different interactions that it's hard to see how,
3 you know, let's say a 100-year, you know, strange weather
4 pattern might truly influence the results significantly.

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

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

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

22 DR. GHOSH: Okay. So I can tell you that
23 informally we've started looking at things like this
24 because we're starting to think about these things more
25 as we're trying to support, you know, rule making and

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1 other regulatory actions with level three type
2 information.

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

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

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

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

24 That difference would be much bigger, I
25 believe. I've never done a real careful comparison of

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1 this but I believe that difference would be much bigger
2 than the year to year variation at a specific site.

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

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

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

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

22 DR. GHOSH: Okay. So on slide six is the
23 next comment or maybe a suggestion I should call it, which
24 was to select the MELCOR realization that produced the
25 largest conditional prompt fatality consequence in our

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1 existing SOARCA uncertainty results and for that
2 realization I do a sort of a separate, you know, MACCS
3 uncertainty only study.

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

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

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

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

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

22 So for example CAP17 is our original
23 uncertainty analysis run which we have now renumbered Run
24 1 in attempt to be a little bit more clear but we've added
25 a lot of terminology.

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

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

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

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

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

25 If you look at those spreads, I mean, in

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1 general the epistemic spread is larger than, you know,
2 the weather spread that you get from a particular -

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

6 DR. GHOSH: Before you get - oh.

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

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

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

21 MEMBER CORRADINI: Okay.

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

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1 So we're already capturing kind of a higher
2 consequence in terms of the weather distribution. So if
3 you have to pick a number, you know, we think it's not
4 a bad thing to be looking at the means to characterize
5 -

6 MEMBER CORRADINI: Okay.

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

9 DR. GHOSH: For nuclear - yeah, for nuclear
10 -

11 MEMBER BLEY: Any distribution is -

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

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

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

20 MEMBER BLEY: I think from Mike's question
21 -

22 DR. GHOSH: Yeah.

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

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

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

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

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

13 MEMBER CORRADINI: Okay. Thank you.

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

20 So basically three replicates of an
21 identical analysis using a different Latin hypercube
22 seeds. So we used Latin hypercube sampling, generated
23 1,000 realizations and did that three times in order to
24 be able to compare, you know, what the difference is when
25 doing - use a different seed.

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1 CHAIRMAN STETKAR: Tina, I'm not - I've run
2 a lot of Monte Carlo stuff. Did you - 1,000 samples from
3 350 input values of moderately broad uncertainties tend
4 not to generally give you enough samples to reach
5 convergence or reasonable convergence in the mean. Did
6 you examine the convergence of your sampling?

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

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

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

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

21 DR. GHOSH: Yes. Yes, we did.

22 CHAIRMAN STETKAR: Okay, and -

23 DR. GHOSH: So the first way we looked at
24 convergence was to do these three replicates using Latin
25 hypercube sampling and see how well do they match each

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1 other and then the additional way was to use the
2 bootstrap.

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

10 CHAIRMAN STETKAR: Okay.

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

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

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

23 DR. GHOSH: Oh, sorry.

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

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1 DR. GHOSH: That's right.

2 CHAIRMAN STETKAR: Okay.

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

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

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

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

19 So slide eight was the -

20 MEMBER CORRADINI: Run one is unmitigated.

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

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

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

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

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

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

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

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

24 Given that there is so little data even for
25 the largest source term in terms of the prompt fatality

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1 risk they match reasonably well but, you know, we're
2 struggling here.

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

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

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

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

22 So what I'm trying to say is you have a whole
23 bunch of zeroes that you have to basically account for
24 then when you start to make statistics and for this reason
25 our MACCS post processor kind of breaks down when you try

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1 to look at the combination of the aleatory and the
2 epistemic uncertainty because there are just a
3 tremendous number of zeroes that the post processors have
4 to account for.

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

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

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

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

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

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

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

14 And if we go to slide -

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

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

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

24 CHAIRMAN STETKAR: It's okay. I can - I
25 can have 99 percent probability of being zero which you

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1 could - which you could arbitrarily assign to be 10 to
2 the minus 11 or 10 to the minus 200 or whatever.

3 DR. BIXLER: Okay.

4 CHAIRMAN STETKAR: And I would eventually
5 get to one.

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

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

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

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

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

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

25 CHAIRMAN STETKAR: The cumulative

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1 probability must eventually sum to one.

2 DR. GHOSH: Right.

3 CHAIRMAN STETKAR: Someplace.

4 DR. GHOSH: Yes. We - so for the zero to
5 1.3 mile - you know, the - I guess the long - the biggest
6 or the smallest number that we calculated is something
7 on the order of 10 to the minus eleven.

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

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

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

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

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1 to do.

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

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

8 DR. GHOSH: Yeah. Right.

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

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

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

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

25 There's even less data when you go out to

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1 three and a half miles about - it looks like two, three,
2 four - there's 40 some percent of calculated non-zero
3 results. And in this case, you know, at the very tail
4 end one of the results are maybe about a factor of two
5 higher than the others.

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

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

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

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

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

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

25 DR. GHOSH: Yeah.

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

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

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

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

21 CHAIRMAN STETKAR: Okay.

22 DR. GHOSH: And, you know, the struggle
23 with all of the prompt fatality risks is that we are -
24 again, we're talking about minuscule absolute risks
25 because if you take the frequency of the scenario and then

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1 you multiply by these numbers even at the highest end,
2 I mean, this is - well, I know we're not supposed to use
3 the term but well below regulatory interest -

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

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

16 DR. GHOSH: Yes, and -

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

20 DR. GHOSH: Yes. Fair enough.

21 CHAIRMAN STETKAR: But we're interested in
22 -

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

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

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

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

8 CHAIRMAN STETKAR: Something smaller.
9 Right.

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

14 CHAIRMAN STETKAR: Right. At the delta -

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

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

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

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

24 DR. GHOSH: Okay. So I will move on to the
25 next question which had to do with the latent cancer

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1 fatality risk and here certainly we do calculate -
2 there's a lot more data to plot with the distribution and
3 the suggestion was to select the MELCOR realization that
4 produced the largest conditional latent cancer fatality
5 consequence and for that realization once again sample
6 from just the MACCS on uncertain inputs.

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

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

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

23 We - it's very hard to distinguish, you
24 know, among the curves but the steeper curve is the
25 10-mile curve and the one that shows up as purple that's

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1 the - is that right? Yeah. The zero to 50-mile curve
2 and the three replicates match each other very well.

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

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

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

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

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

25 MEMBER BLEY: Well, in the printout I have

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1 and what you sent me - I was looking at the ones you
2 brought today - I can't quite tell. You've got several
3 red ones grouped together and then you have one all by
4 itself. Is that the solid line?

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

7 MEMBER BLEY: That's Run 1?

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

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

13 DR. GHOSH: Yes. Right.

14 MEMBER BLEY: The one on the left is -

15 DR. GHOSH: Yeah.

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

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

23 CHAIRMAN STETKAR: I found it useful to
24 look at - if any of the subcommittee members have Figure
25 10 in the material that you sent us which does plot Run

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1 6 and Run 1.

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

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

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

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

14 CHAIRMAN STETKAR: But Run 1 is the - Run
15 1 is the results in the SOARCA NUREG.

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

21 CHAIRMAN STETKAR: Run 1 is what's in NUREG
22 7155.

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

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

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1 DR. GHOSH: That's right.

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

4 DR. GHOSH: Right. That's right.

5 CHAIRMAN STETKAR: Now, if I look at this,
6 if I stand way back, to me the uncertainty is less - lower
7 in Run 1 compared to Run 6, right? Run 6 curves are much
8 -

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

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

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

18 CONSULTANT SHACK: No, not aleatory,
19 epistemic only.

20 CHAIRMAN STETKAR: The aleatory as it's
21 represented through their mean - through its mean. The
22 point that I'm trying to get is that I don't understand
23 - as I started to stare at this stuff I don't quite
24 understand how all of this is working because it's
25 generally been my experience that you increase

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1 uncertainty as you go through the process and this at
2 least - and that's what we're trying to explore a little
3 bit by these little exercises to see what is the
4 incremental uncertainty from each step of the process and
5 I have not been able to understand that quite well, and
6 especially because this seems - and I might be wrong so,
7 you know, help me if I'm understanding this wrong - this
8 seems to tell me that the uncertainty in the combined
9 aleatory and epistemic - it's called weather in MACCS -
10 for a single replicate is broader than the overall
11 uncertainty that you're publishing in the NUREG.

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

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

16 DR. GHOSH: Yeah.

17 CHAIRMAN STETKAR: - just help me out.

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

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

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1 risk.

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

5 CHAIRMAN STETKAR: If you look at the - let
6 me try this - if you look at the full distribution, the
7 95 over five through zero to 10 is 22.4 and for Run 6 it's
8 9.6.

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

11 CONSULTANT SHACK: For the zero to ten?

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

14 DR. GHOSH: Yeah. That's right.

15 CONSULTANT SHACK: Okay.

16 DR. GHOSH: Okay. So then for the zero to
17 50 now what Dr. Shack was mentioning the little - the
18 difficulty in this comparison is that the - we've put in
19 the aleatory uncertainty in for the high source term
20 curve which is Run 6 and we're comparing it back to the
21 curve that was generated for just the aleatory means.

22 And then if you think back when we had the
23 curve of the combined aleatory and epistemic, which is
24 on slide five, for the original Run 1 results you would
25 follow that - the curve, especially on the left side of

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1 where it crosses spreads out quite a bit, you know, to
2 the - to the left.

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

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

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

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

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

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

11 Those are the curves to the right - if we
12 compare the red 10-mile results as well as the black
13 50-mile results the original results are spread out more
14 when you do the apples to apples comparison.

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

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

21 DR. GHOSH: Yeah, and I think -

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

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

7 MEMBER BLEY: And they're pretty similar.

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

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

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

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

23 And so in terms of the, you know, comparing
24 the full range of original uncertainty to this
25 uncertainty the full range - the black curve on the left

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1 is showing you, you know, you're taking a variety of
2 source terms, everything from low, medium, high and then
3 combining them with the MACCS, you know, parameter values
4 that are spread over the whole thing versus you have a
5 very high source term and then what does the spread in
6 the MACCS results give you.

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

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

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

22 But they weren't large differences. The
23 95th to the 5th ratios were - I used error factors so I'll
24 take the square root of them - but they were on the order
25 of anywhere from three to about five, okay, which are

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1 modest uncertainties in the world of uncertainty
2 analysis - quite modest uncertainties, and they were
3 about the same everywhere.

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

8 DR. GHOSH: Yeah. Yeah.

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

13 DR. GHOSH: Yeah.

14 CHAIRMAN STETKAR: The right hand curves
15 here.

16 DR. GHOSH: Yeah.

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

20 DR. GHOSH: We have.

21 CHAIRMAN STETKAR: - it's just curious.

22 DR. GHOSH: We have. We've thought about
23 it a lot over the last two years and I think, you know,
24 one of the things is that you do see a dampening effect
25 when you come through the source terms to the consequence

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1 result because of this hard backstop of the habitability
2 criterion.

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

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

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

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

20 DR. BIXLER: Maybe another way to put that
21 is that if you think about, say, the cesium release that
22 you get from MELCOR and the spread that you get in that,
23 if you run that set of source terms through MACCS you'll
24 get a compression - a sub linear type of effect on the
25 outcome for MACCS because the biggest source terms get

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1 proportionately more action - remedial action to
2 compensate for the size of the source term as opposed to
3 the small source terms.

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

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

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

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

21 DR. BIXLER: No.

22 DR. GHOSH: You know, I -

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

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1 prove what you think.

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

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

8 MEMBER CORRADINI: I understand. I
9 understand.

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

12 DR. GHOSH: Right. Right.

13 MEMBER CORRADINI: Then you'd see that but
14 -

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

16 CONSULTANT SHACK: Your way out, yeah.

17 MEMBER CORRADINI: The way out.

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

20 MEMBER SCHULTZ: That might be the next
21 phase but I do think it's important to capture the
22 thoughts about - the practical thoughts about why you
23 believe or why you understand that for zero to 50 you get
24 this type of result, and I appreciated your explanation
25 in terms of the practical matter of the consequence

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1 evaluation which is what this project is all about - how
2 it affects the result and you explained a piece of it as
3 to what we're thinking about zero to 50.

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

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

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

16 DR. GHOSH: And I think -

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

22 DR. GHOSH: Yeah.

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

25 MEMBER BLEY: I think it would be helpful

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1 on a few counts. One, it'll help people reading it
2 understand that five, 10 years from now this issue might
3 come up again and it'll be lost if you don't document it
4 pretty well.

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

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

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

22 So the 10-mile dose has many fewer
23 contributions than the zero to 50 where, you know, where
24 you only have the hot spot and normal relocation and that
25 doesn't occur for a certain amount of time that, you know,

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1 is assumed it would take, you know, to implement those
2 protective actions.

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

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

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

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

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

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

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

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

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

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

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1 going forward.

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

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

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

15 CHAIRMAN STETKAR: Tina?

16 DR. GHOSH: Yes.

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

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

24 CHAIRMAN STETKAR: No, she has several
25 slides that -

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

3 MS. SANTIAGO: Yeah, MELCOR -

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

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

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

10 DR. GHOSH: Yes.

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

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

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

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

21 CHAIRMAN STETKAR: We're back.

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

24 MS. SANTIAGO: You're on the record.

25 CHAIRMAN STETKAR: You're on the record

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1 now.

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

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

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

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

24 And as expected, if you look at the spread
25 in the results it shrinks. You know, when you - when you

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1 add in the MACCS uncertainty on top of the MELCOR
2 uncertainty all of these curves spread out.

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

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

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

19 DR. GHOSH: Right. Right.

20 CHAIRMAN STETKAR: You said -

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

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

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1 DR. GHOSH: MACCS parameter.

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

4 DR. GHOSH: Yes. Yes.

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

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

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

17 DR. GHOSH: Yeah, and -

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

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1 - this seems to - this seems to corroborate that.

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

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

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

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

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

24 DR. GHOSH: It's interesting. I think I
25 might have mentioned this last time. Before we did this

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1 rule uncertainty from, you know, two plus years ago a lot
2 of people thought that the dominant contribution from the
3 uncertainty and that it would be huge would be from the
4 off-site consequence portion of the modeling.

5 CHAIRMAN STETKAR: Yes.

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

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

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

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

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

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

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

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

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

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

23 CHAIRMAN STETKAR: Yes.

24 MS. GIBSON: - and part of the reason for
25 doing that is to see what the resource commitment is both

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1 in dollars and staff time. Another reason for doing it
2 is for staff development, I guess. We're doing as much
3 of it in-house as we can to develop that knowledge and
4 skills.

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

7 MS. GIBSON: I'm sure you were.

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

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

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

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

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

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

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

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

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

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

25 But I don't - I think there's a couple of

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1 reasons we wanted to do the three snapshots. The big one
2 NRC policy still has to do LNTs so people are always
3 interested in LNT, you know, results by themselves.

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

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

18 DR. BIXLER: Mm-hmm.

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

22 But we just - we had a lot of strong
23 reactions against that because people feel very
24 strongly. Everyone's got a very strong opinion about
25 what is the right model and what should be the right

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1 threshold if they believe that LNT is not the right model.

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

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

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

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

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

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

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

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

10 But certainly there are issues of model
11 uncertainty -

12 MEMBER REMPE: Absolutely.

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

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

25 MEMBER REMPE: Given the current models

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1 today.

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

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

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

16 DR. GHOSH: Yes. You bet.

17 MEMBER REMPE: - on some of these
18 questions.

19 DR. GHOSH: Absolutely. Right.

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

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

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1 the consequence part of this process.

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

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

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

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

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

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

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

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

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

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

25 We wanted to see whether we could

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

4 And the metrics we used were the latent
5 cancer fatality risk at the five radial locations that
6 we looked at, and as I mentioned before, you know, beyond
7 10 miles the dose results are correlated very highly. So
8 we took that into account in the reading.

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

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

21 And then we looked at also release time,
22 which is particularly important for potential prompt
23 fatality. And then the goal - the theoretical goal was
24 to choose the source term whose metrics' range would come
25 closest to the one-sixth, half and five-sixths, you know,

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1 positions among the entire population kind of as a rough
2 choice for the low, high and medium source terms.

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

8 MEMBER RYAN: I won't live that long.

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

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

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

25 And the final source terms that we chose as

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1 the representative ones are shown in red, green and blue.
2 The blue is the high - you know, the theoretical high
3 source term or representative.

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

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

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

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

18 So if we go to the next slide, which is slide
19 21, then for each of those three representative source
20 terms which we just named here - I'll have to complete
21 this in case anybody's curious.

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

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

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

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

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

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

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

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1 the epistemic uncertainty.

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

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

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

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

25 Then if we go to slide 25 that is for the

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1 high source term, and I realize we already went through
2 this exercise with the high source term from the original
3 set of results but this is now our representative high
4 source -

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

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

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

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

24 So I think if everybody's okay with that
25 portion we will now move into the discussion of the new

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1 bootstrapping we did to look at comparing Latin hypercube
2 sampling with a simple random sampling.

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

5 CHAIRMAN STETKAR: Motor. Motor.

6 DR. GHOSH: So slide 27 -

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

12 DR. GHOSH: Yeah. Yeah.

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

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

20 DR. GHOSH: Right. Right. Yeah.

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

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

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

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

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

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

19 DR. GHOSH: Yeah.

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

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

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

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

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

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

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

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

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

9 MEMBER CORRADINI: There are nuggets.

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

13 CHAIRMAN STETKAR: Right.

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

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

23 Maybe it was better to use rivets. Okay.
24 Just keep that in perspective. You said you're going
25 back and rereading the report. The problem is you're

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1 reading it from the perspective of people who did all of
2 this work.

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

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

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

13 CHAIRMAN STETKAR: Right. Right.

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

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

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

25 MS. SANTIAGO: And we'll take another look

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1 at that. We kind of run to get all this done and we just
2 need to sit and digest it in the next couple of months.

3 MEMBER SCHULTZ: It's great.

4 MS. SANTIAGO: But we do appreciate that.

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

7 MS. SANTIAGO: Right.

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

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

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

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

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

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

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

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

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

23 And I'll give you the result - our overall
24 conclusion before I show you the results. We are even
25 more confident now that our results are very well

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1 converged and that Latin hypercube sampling is valid to
2 use.

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

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

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

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

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

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

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

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

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

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

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

23 DR. GHOSH: Yes. Yeah, exactly.

24 CHAIRMAN STETKAR: Well, but this is -
25 well, same number of samples. This is - make sure I

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1 understand how this is done - CAP17 is 865 Monte Carlo
2 realizations and for each realization you pick one value
3 for each of the 350 MACCS input parameters. That value
4 is selected based on a sampling routine, right?

5 DR. GHOSH: Right. Right. Exactly.

6 CHAIRMAN STETKAR: So it's -

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

12 DR. GHOSH: Exactly.

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

15 DR. GHOSH: How many?

16 MEMBER BLEY: Well, what happened to -

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

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

22 DR. GHOSH: Yeah. Yeah.

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

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1 DR. GHOSH: Right.

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

4 DR. GHOSH: You know, I feel like -

5 MEMBER BLEY: Somebody must have
6 originally -

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

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

19 But for our model I think we already knew
20 that the three and a half mile prompt fatality risk was
21 altering that - was driven by the tail and it seems that
22 865 is not enough to have an adequate representation of
23 the tail which is what's driving the mean - the aleatory
24 means results here.

25 So that's where you start to see a

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

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

10 MEMBER SCHULTZ: So why would it work in
11 close and not further out? Why would it work in close
12 - 1.3 and two look fine and then suddenly you have a
13 different result?

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

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

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

21 MEMBER CORRADINI: Yeah, let me see.

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

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

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

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

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

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

25 Does that make sense? So that's why as you

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1 go further out it's getting more and more unreliable in
2 terms of the results that you implied.

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

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

8 DR. GHOSH: Yeah. Yeah.

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

11 DR. GHOSH: Oh, in distance? Yeah.

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

15 DR. GHOSH: Yeah.

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

18 DR. GHOSH: You mean physically?

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

21 DR. GHOSH: Right. Right.

22 CHAIRMAN STETKAR: Do you know?

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

25 CHAIRMAN STETKAR: Increasing the area -

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

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

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

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

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

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1 -

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

4 DR. BIXLER: Yeah.

5 CHAIRMAN STETKAR: Maybe a -

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

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

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

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

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

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1 31 is for the 50-mile radius.

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

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

9 MEMBER CORRADINI: I'm weathered out.

10 MEMBER REMPE: You were too optimistic.

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

16 MEMBER REMPE: So we're going to -

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

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

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

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

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

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

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

25 So I think there's more that can be done than

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1 to just present results and say, well, here they are but
2 to see if we can draw conclusions associated with these
3 different evaluations that have been done, the
4 uncertainty evaluations and so forth - what would be the
5 impact if one were to have selected a threshold model and
6 done the same type of work and looked at the same type
7 of evaluations with regard to model uncertainty,
8 regulatory uncertainty and so forth.

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

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

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

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

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

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

11 MEMBER SCHULTZ: Right.

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

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

19 DR. GHOSH: Yes.

20 MEMBER CORRADINI: The distribution gets
21 wider -

22 DR. GHOSH: Exactly.

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

25 DR. GHOSH: Exactly. Exactly right.

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

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

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

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

17 So it's supposed to be across all ages, you
18 know, across all relevant characteristics and I'm -
19 actually I'm not a health physics model. I don't know
20 what the relevant characteristics are. I've only heard
21 -

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

25 That's a big subgroup of interest and it's

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1 probably maybe half a dozen other ones like that that
2 might be more important than probably everything else.
3 I just wondered if you had focused on those kinds of
4 subsets.

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

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

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

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1 by the percentage of the population experiencing -

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

5 DR. GHOSH: Yeah.

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

8 DR. GHOSH: Right. Right. Right.

9 MEMBER RYAN: Okay.

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

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

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

21 DR. GHOSH: Yeah, no problem.

22 MEMBER RYAN: That would be great. Thank
23 you.

24 DR. GHOSH: So according to the agenda we
25 were going to discuss the MELCOR parameters first and so

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1 the original questions were - well, I guess the questions
2 as we understood and as we got was really a list of
3 parameters that the committee was interested in talking
4 about further and kind of understanding how did we
5 characterize the uncertainties for these parameters and
6 why did we choose the shape of the distributions that we
7 chose for these particular parameters. So -

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

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

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

24 But we did identify additional aspects of
25 our modeling problem that we recognize are uncertain that

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1 we did not include in this study.

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

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

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

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

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

24 DR. GHOSH: Yeah, because I was -

25 CHAIRMAN STETKAR: Okay. So why don't -

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

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

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

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

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

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

14 MEMBER REMPE: I do and but -

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

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

21 CHAIRMAN STETKAR: Okay.

22 MEMBER REMPE: No, and so I again -

23 MEMBER CORRADINI: I just think they should
24 proceed and we'll - if there's something that pops in our
25 head then they can take note of it because they haven't

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1 prepared for it. So in some sense this is ad hoc.

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

4 MEMBER CORRADINI: No.

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

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

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

12 CHAIRMAN STETKAR: Okay.

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

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

17 CHAIRMAN STETKAR: No, I thought this - so
18 the comment I think we did add it and it was kind of down
19 on the bottom of our laundry list of things to look at
20 for this meeting was yeah, was there a - an active - what
21 types of active decisions were made to select the
22 particular set of - and I've forgotten the body count,
23 21 I think it was.

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

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

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

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

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

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

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

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

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

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1 what led to our final list.

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

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

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

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

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

24 It's hard to do when you just have experts
25 but still the discussions to the - quantify the higher

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1 level surrogate parameters rely heavily on qualitative
2 expert opinion and, of course, that's difficult to
3 reproduce.

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

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

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

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

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1 why they didn't do anything in that area.

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

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

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

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

24 Then in-vessel you've got some that are
25 correlated in terms of the - what do you call it, the fuel

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1 failure criteria, radial spread that Joy was talking
2 about.

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

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

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

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

24 DR. GHOSH: I think, yeah, the question
25 with that was the magnitude of the effects from the

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1 chimney effect, you know, and, you know, what we found
2 there was it's not how much the opening is but it's
3 whether or not that you blow open the doors in the first
4 place ended up being more important than the parameter
5 that we actually sampled which was how big was the opening
6 -

7 MEMBER CORRADINI: Right.

8 DR. GHOSH: - taken to the uncertain.

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

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

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

24 DR. GAUNTT: I wasn't here.

25 MEMBER CORRADINI: Okay. Somebody said

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

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

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

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

20 MEMBER REMPE: I mean, again, I guess what
21 I see it's easier to vary things. You can easily vary
22 sometimes as was that also part of the - I mean, aerosol
23 stuff was pretty easy to vary whereas some of these other
24 things when you came up with a surrogate parameter it
25 would have been harder to do and it is a really

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1 unrealistic approach to life and I'm kind of wondering
2 if that was what dominated the process too a bit.

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

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

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

17 DR. GAUNTT: They were handy.

18 MEMBER REMPE: Yeah.

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

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1 along the way as part of the original SOARCA project.

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

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

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

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

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1 group of things are.

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

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

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

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

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

24 But I'm just remembering back to 1150 when
25 we had to do this for the plants. We had a bunch of people

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1 in a room. We fought for days on end as to what should
2 be in, what should be out - once it's in what's the range
3 - now what's the range, now you got to settle what's the
4 shape.

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

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

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

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

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

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

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

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

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

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

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

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1 the coming years.

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

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

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

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

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

23 MR. JONES: This is Joe Jones at Sandia.
24 Could I clarify one point, please? It was mentioned that
25 for NUREG 1150 there were multiple meetings to discuss

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1 parameters that should be in or out and the like, and I
2 just want you to be sure we had those meetings here with
3 the MELCOR experts - Mark Leonard, Casey Wagner, Randy
4 was in those.

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

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

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

22 But I think what we are missing and what we
23 can't really recreate at this point is the type of
24 documentation that you would get from we met on this
25 parameter on this date and, you know, this was the reason

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1 that was discussed and this was one of our resource
2 challenges from the beginning.

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

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

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

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

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

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

25 I think in the report where we talk about surrogate

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1 parameters we're talking about I think as you noted
2 parameters in MELCOR that are kind of lumped parameters
3 that represent different things that are going on.

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

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

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

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

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

25 DR. GAUNTT: - to viscosity and, you know,

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

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

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

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

19 DR. GAUNTT: Yeah, and it's a lot of
20 engineering judgment that takes place and if I can jump
21 ahead and tell you for the - for the fuel rod lifetime
22 model prior to putting that in we found that, well, what
23 we had before that was a temperature threshold. We reach
24 a certain temperature and we now judge the fuel can't be
25 rods anymore - it's got to be rubble, and run some

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1 uncertainty studies on that and find out, well, sometimes
2 you just go right under that temperature and sometimes
3 you go right over that temperature and then it takes you
4 to a branch point.

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

13 MEMBER CORRADINI: But it always happens.

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

17 MEMBER REMPE: It helped.

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

24 DR. GAUNTT: It's a DOE project. It's very
25 Fukushima-centric and it's really aimed at - it's really

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1 aimed at characterizing a broader spectrum of possible
2 realities of, you know, given reactor core degradation
3 such that when TEPCO finally opens up the reactors we have
4 a, you know, a broader map of what they might find.

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

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

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

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

24 MEMBER CORRADINI: But I think Randy did -
25 I thought - I mean, I think I was on the phone. It was

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1 in some year recently that you came here and gave a
2 presentation - I was on the phone - about the Fukushima
3 calculations that Sandia and Oak Ridge were doing in a
4 reconstruction project from, I think, it was in the
5 spring of '12.

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

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

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

23 MR. FULLER: This is Ed Fuller. I can
24 answer your question very quickly. You have it a little
25 backwards. Yeah, MAP calculates that you get earlier

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1 time to vessel failure but it calculates that in the melt
2 progression process in the core region you basically form
3 a blockage which prevents hydrogen from going through the
4 debris instead the steam has - steam from going through
5 the debris but instead has to go around whereas MELCOR
6 keeps the passageways open so that things progress a
7 little more slowly and in lower temperature in the MELCOR
8 approach and more quickly at a higher temperature than
9 the MAP approach.

10 MEMBER CORRADINI: Thank you.

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

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

25 MEMBER REMPE: So my understanding is one

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1 of the important goals of that crosswalk is to inform some
2 of the hopefully - hopeful to occur inspections of this
3 data would help us to resolve some of these big
4 differences in the modeling and I think that's an
5 important -

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

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

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

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

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

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

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

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

19 CHAIRMAN STETKAR: Mid-October. So -

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

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

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1 want to keep in touch with I don't know who.

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

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

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

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

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

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

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

25 CONSULTANT SHACK: I mean, I guess I would

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1 have assumed it was - there had been a lot of discussion
2 before you make any -

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

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

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

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

17 MS. SANTIAGO: So should we start -

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

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

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

25 We communicate, I would say, almost on a

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1 daily basis and decisions about what to do with a code
2 are made jointly and, you know, obviously it comes down
3 to funding but we prioritize what needs to be done and
4 in some cases DOE has money to provide Randy's expertise
5 for meetings and things and in other cases we provide the
6 funding.

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

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

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

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

21 And with that, I do think it's probably an
22 appropriate time to break for lunch because it's just -
23 you know, otherwise we'll talk about relief analysis and
24 -

25 MEMBER CORRADINI: And we promise not to

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1 ask any of these questions again.

2 MEMBER REMPE: Get down to the nitty gritty
3 on -

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

7 So anybody have any problems coming back at
8 12:45 rather than 1:00? If not, we will recess until
9 12:45.

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

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

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

24 You'll see that for some of the parameters
25 there was some data out there or some, you know,

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1 experiments or some kind of real-life type experience
2 that we could draw from to help build the distribution.

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

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

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

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

24 So there was some confidence in what a
25 nominal value might be, you know, just from the - as Randy

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1 talked about the years of experience, you know, using the
2 MELCOR code and whatever experimental data and other
3 studies have been done.

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

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

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

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

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

25 So, you know, out of the 20, you know, MELCOR

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1 parameters and 21 MACCS parameters when we really looked
2 at what was driving the variation in the results it's a
3 much smaller set of parameters that's actually driving
4 the uncertainty in the results.

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

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

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

19 So for the SOARCA study we basically had a
20 relatively early seizure of the SRV due to a stochastic
21 failure and that is one sub scenario that we observed in
22 our 865 realizations as well but then in addition we have
23 a second set - a second and a third set really of the
24 long-term station blackout scenario where if you don't
25 have the early failure of the - you don't have an early

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1 stochastic failure of the SRV and instead it cycles for
2 a longer period of time and you end up eventually seeing
3 it fail because of a thermal seizure.

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

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

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

17 You're venting into the dry well and you
18 have unscrubbed releases, which can be quite a bit
19 larger. And so for this reason because the set of
20 thermal seizure scenarios is more consequential than the
21 early stochastic failure and the main steam line rupture
22 scenarios is still more consequential, when you compare
23 the spread of the uncertainty analysis results to the
24 original SOARCA study, you know, as a number of you have
25 noted the SOARCA results are kind of in the lower path

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1 to - on the lower end of the full spread of the uncertainty
2 results and that's largely because of the fact that we
3 observed these two other sets of scenarios that were not
4 modeled as sort of the best - you know, the - our best
5 guess of how a scenario might evolve.

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

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

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

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

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

8 It's a very different failure sort of case
9 actually than just testing it once and seeing whether or
10 not it opens and close as you expect.

11 So everyone, you know, sort of recognized
12 this but we really struggled with what exactly to do about
13 it because we knew - so we have this data base in 7037
14 which has, you know, one or two data points of having a
15 failure out of many, many, many trials where you try to
16 open it.

17 But it's not a - the test is not
18 representative of what you would actually experience
19 during a severe accident. So we really struggled okay,
20 so how do we - do we just take that distribution and use
21 it, which is what we did in the first iteration, or do
22 we try to use something different knowing that that isn't
23 representative?

24 So after we talked with the ACRS last April,
25 we mentioned that we were revisiting, you know, kind of

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1 what is our consensus expert opinion about what we should
2 use as the distribution and we evaluated what are all the
3 sources of data we have.

4 So we went beyond 7037. There's another
5 NUREG, NUREG/CR-6928. We also evaluated the data there.

6 We looked at the database that our original
7 SOARCA value came from and that's the Peach
8 Bottom-specific IPE. And then when we revisited that we
9 saw that they had a second distribution also for the
10 extreme environment.

11 So it's extreme environment versus the
12 normal data set, and what we ended up doing is - because
13 most people think that the epistemic parameter
14 distribution for a failure on demand should be
15 approximately a beta distribution we used the same
16 methodology that they used in NUREG/CR-7037 so to
17 construct the beta distribution we used that same
18 methodology to construct distributions based on these
19 other references. And we basically came up with this set
20 of roughly - it was five curves. So we have five curves
21 to kind of choose from.

22 This is the universe of possibility that we
23 could conceive of in terms of what we have available to
24 us and the one that we settled on was the Peach Bottom
25 IPE curve which is the red one and there are a number of

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1 reasons for that which I think we go through in the
2 report.

3 It kind of falls in the middle of the
4 possible curve so we didn't do a formal weighting of, you
5 know, the possible curve so it falls roughly in the
6 middle.

7 We think it's more representative of
8 conditions than the original curve. So, you know, it's
9 going better in the right direction. And in the end,
10 what really matters is the density of the curve between
11 approximately nine times 10^{-3} failure
12 rate and about five times 10^{-2} because
13 below nine times 10^{-3} you pretty much
14 are always going to get a thermal seizure.

15 So how - you know, it doesn't really matter
16 what the shape of a curve is below that. And above five
17 times 10^{-5} , I mean, you fail so early that,
18 you know, it doesn't really matter beyond that too.

19 So we're really looking at what should be
20 the - you know, the probability that the true value is
21 between about, you know, around the 10^{-3} point
22 and about five times 10^{-2} and we
23 thought the red curve was, you know, kind of in the middle
24 of what the possibilities might be.

25 CHAIRMAN STETKAR: Thanks. First of all,

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1 what I'd like to understand is these curves. I looked
2 at NUREG/CR-7037 and I also looked at NUREG/CR-6928.
3 You've characterized NUREG/CR-6928 by the purple curve.

4 That is not the uncertainty distribution in
5 NUREG/CR-6928. It is very, very far different - very far
6 different. In fact, NUREG/CR-6928 and NUREG/CR-7037
7 have distributions that are very similar because they
8 were both initially based on a noninformative
9 constrained Jeffreys prior.

10 In the case of NUREG/CR-6928 they used data
11 of two failures in 3,142 - 3,142 demands. In 7037 they
12 had a little bit more data - two failures in 3,536.6
13 demands. So if you look at the curves 70 -

14 DR. GHOSH: How do you measure .6 demands?

15 CHAIRMAN STETKAR: - 7037. Because of the
16 way they counted the demands.

17 DR. GHOSH: Oh, okay.

18 CHAIRMAN STETKAR: 7037 is slightly
19 different from 6928. It is not this difference. The
20 purple curve - I don't know where you came up with the
21 purple curve. 6928 doesn't have anything that looks
22 anything like that for this failure mode.

23 So I have no idea where it is. Now, what
24 I - what I don't know - let me finish this.

25 MEMBER CORRADINI: Well, just a

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1 clarification. You're saying - just so I understand -
2 you're saying -

3 CHAIRMAN STETKAR: If you move the purple
4 curve to essentially where the blue curve is - because
5 7037 and 6928 are not independent sources. They both
6 started with the same thing. 7037 collected a few more
7 tests - you know, about 400 more tests. That's all.

8 MEMBER CORRADINI: That's all I wanted to
9 ask. Thank you.

10 CHAIRMAN STETKAR: So they're not
11 independent sources.

12 DR. GHOSH: Right. Right. Right.

13 CHAIRMAN STETKAR: And they're - and if you
14 look at the data - the distributions they are not that
15 different.

16 Now, what I don't know is what the green
17 curve is because I'm not quite sure. I didn't study 7037
18 to understand what that green curve is.

19 DR. GHOSH: So 7037 had data for multiple
20 SRV triggering modes and so the green curve represents
21 the subset of pressure triggered.

22 So I think they - I don't know. If Kyle or
23 Doug is on the line they can remind me what the - if they
24 remember what the other modes were. But there were
25 multiple triggerings for - multiple modes of triggering

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1 that SRV to open so and some subset of that was pressure
2 triggers.

3 And we thought that was more representative
4 of our situation and they actually broke that out in the
5 NUREG because in our case that's what we're modeling,
6 that the SRV is relieving is the triggering on high
7 pressure. So we thought that was more - that was
8 relevant to also show the -

9 CHAIRMAN STETKAR: I didn't look at that
10 because you didn't present that as a comparison in
11 NUREG/CR-7155, did you?

12 DR. GHOSH: No, because we -

13 CHAIRMAN STETKAR: You compared 7037 to
14 6928 in the Peach Bottom IPE and the Peach Bottom
15 disavowed IPE. Now, what I - what I'd like to explore
16 - this is important because when you explained how you
17 came up with the red curve you actually took the number
18 that they used in the IPE as a mean and fit the beta
19 distribution with the parameters of the beta
20 distribution from 7037.

21 So you basically fixed it at the mean of 3.7
22 times 10 to the minus three and spread it from that as
23 if that is the truth.

24 Peach Bottom themselves have disavowed that
25 number, right? They don't use that number anymore.

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1 DR. GHOSH: Yeah.

2 CHAIRMAN STETKAR: So now my question is if
3 we're -

4 DR. GHOSH: Yeah.

5 CHAIRMAN STETKAR: Let me - let me finish.
6 If Peach Bottom doesn't believe it and if two NUREGs don't
7 believe it, if indeed everybody believes the curve have
8 shifted much to the left which says from the stochastic
9 failure standpoint the dials will cycle for a long, long
10 time - in other words, this stochastic sticking open
11 would probably be a very, very small contributor to the
12 overall results - if that is actually our state of
13 knowledge why are we living with the red curve?

14 DR. GHOSH: So here's -

15 CHAIRMAN STETKAR: And if we're living with
16 the red curve only because experts sitting at this table
17 and on the phone line believe in their heart that's the
18 curve they want to force the results into a certain area,
19 you ought to present it in that light.

20 We, the assembled multitudes, decided we
21 wanted to use this curve because it would then give us
22 results from that early sticking open failure mode for
23 no other reason.

24 DR. GHOSH: Okay. So let me - this has been
25 one of the biggest challenges.

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1 CHAIRMAN STETKAR: I understand that and
2 it's important. But we're talking about uncertainty and
3 the basis for that uncertainty.

4 DR. GHOSH: We had - well, we had extensive
5 discussion, not just amongst this team but also with our
6 external peer review panel and everybody - everybody
7 agreed that the testing data was not representative -

8 CHAIRMAN STETKAR: Good. Then don't use
9 it. Simply say it's irrelevant. End of story. If
10 that's what everybody decided, that the testing data in
11 7020 - the 7037 to 6928 was irrelevant don't confuse
12 people by saying look, look, what we used is in the middle
13 of all this stuff.

14 A, it's not in the middle - it's skewed.
15 And B, if everybody agreed it's not relevant what are you
16 comparing apples to orange for?

17 You're trying to justify something that you
18 made up and you wanted to use as if it's - as if it's -
19 you know, as if it has relevance to something else. So
20 if you all decided it's not -

21 DR. GHOSH: Because I think the - right,
22 because I think the question comes up all the time. You
23 know, you're basically modeling a failure mode that's
24 named in this database, you know, this NUREG database -
25 you know, why don't you say something about it. I think

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1 that -

2 CHAIRMAN STETKAR: You can say something
3 about it -

4 DR. GHOSH: - there's more challenges,
5 yeah.

6 CHAIRMAN STETKAR: You just did. You said
7 we looked at it. We decided it wasn't relevant for the
8 following reasons and that's why it's not relevant,
9 period. And given that, we used this.

10 MEMBER CORRADINI: So can I again get a
11 clarification? John asked the question in the middle of
12 his long question - what is the - can somebody - since
13 now you've said that you guys relatively uniformly didn't
14 like the data what's the difference between all data and
15 pressure data? That's what I didn't understand.

16 DR. GHOSH: Oh. So there were multiple
17 triggering modes for the SRVs that they tested. One of
18 them was the pressure trigger, so, you know, the end which
19 we think is the best match - if we had the right
20 temperature pressure kind of conditions is the best match
21 for what we're modeling here because in our model we're
22 pressure triggering those SRVs to open. But they had
23 other ways of triggering the SRV and then, you know,
24 they'd record why they're not able.

25 MEMBER BLEY: This was in the tests?

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1 DR. GHOSH: In NUREG - yeah, in the 7037,
2 right.

3 MEMBER BLEY: What were some of the other
4 triggers? I don't know.

5 DR. GHOSH: Yes. I was in fact just
6 calling on - if Kyle or Doug are on the line if you
7 remember because I don't recall -

8 MS. SANTIAGO: Is anybody from Sandia on?

9 CHAIRMAN STETKAR: I wonder if they called
10 back in.

11 MR. JONES: Joe Jones is on and I believe
12 Kyle and Doug are on. I'll go double check with them.

13 DR. GHOSH: Okay.

14 MEMBER BLEY: The other thing, John, when
15 they come back if somebody can explain where that purple
16 line -

17 (Simultaneous speaking)

18 MR. JONES: -- SRV stuff to try to find it.

19 DR. GHOSH: Thanks. We'll get back to you
20 on that because I don't remember off the top of my head.

21 MEMBER BLEY: Okay.

22 DR. GHOSH: Sorry. You were saying?

23 MEMBER BLEY: I was saying in addition to
24 the last thing John asked about the first thing he
25 mentioned was your purple curve doesn't really come from

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1 6928. Where the heck did it come from?

2 DR. GHOSH: Yeah. I need to double check
3 that. To be honest, it's been over a year since we
4 generated these. I need to double check what we did to
5 get that.

6 CHAIRMAN STETKAR: I ran out -

7 DR. GHOSH: I'll get back to you on that.

8 CHAIRMAN STETKAR: Yeah. I ran out the
9 6928 distribution and it has a 5th percentile on the order
10 of about three times 10 to the minus six, a median on the
11 order of about four times 10 to the minus four, a mean
12 on the order of about eight times 10 to the minus four
13 and a mean on the order of about three times 10 to the
14 minus three.

15 So it's - as I said, it's close to the - it's
16 really close to the blue curve because the way they -
17 these noninformed prior are constrained by the data and
18 the data really are not all that much different for the
19 -

20 MEMBER BLEY: Could you perhaps take - like
21 you did on the Peach Bottom take their point estimate and
22 apply some other distribution to it?

23 DR. GHOSH: I think that that's possible
24 but I don't want to give you the wrong answer. Let us
25 double check what we did.

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1 CHAIRMAN STETKAR: The 6928 point estimate
2 is eight times 10 to the minus four. It's - it didn't
3 do that.

4 MEMBER BLEY: Yeah, I was - well, there was
5 another case we looked at where people took the low end
6 and anchored it and used the spread.

7 CHAIRMAN STETKAR: Oh, okay.

8 MEMBER BLEY: She could have done that.

9 CHAIRMAN STETKAR: Yes. They could have.

10 DR. GHOSH: So we'll get back to you on that
11 because I don't - it's been a while.

12 CHAIRMAN STETKAR: Back to the more - the
13 fundamental question, see, I have no problem with
14 engineering judgment - I, John Stetkar, today decided
15 that I'm going to use this distribution for the following
16 reasons. I have no problem with that.

17 I mean, if that's - if that's all that's
18 available - if the - if you disagree with the test data
19 or you have, you know, essentially no evidence to support
20 your knowledge there's nothing wrong with that at all.

21 But it ought to be presented that way, not
22 something, well, we kind of liked the number that was in
23 the Peach Bottom study despite the fact that Peach Bottom
24 doesn't use it anymore and we used that as our best
25 estimate and we used somebody else's parameters of a data

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1 distribution because they're in a NUREG to express our
2 uncertainty.

3 That is just relying on other crutches.
4 It's not saying I, John Stetkar, today decided to use this
5 distribution because. I used this - I used this number
6 because it appeared in this report and I used this spread
7 because it appeared in somebody else's report and I liked
8 those things because I didn't have to - I didn't have to
9 make that decision myself.

10 DR. GHOSH: Yeah. I mean, you know, again,
11 we'll be rereading the whole report to kind of see how
12 we say things. So we'll revisit what we say in that
13 section. I thought we tried to at least explain why we
14 thought, you know, the data were not relevant.

15 CHAIRMAN STETKAR: My question though is I
16 read some of that and I have no idea whether 3.7 times
17 10 to the minus three as a best estimate is good data or
18 indifferent for the type of behavior that you're trying
19 to model - successive multiple cycles of a relief valve
20 under, you know, pressure - high temperature pressure
21 conditions.

22 I have no idea. I'm not a valve person. I
23 don't understand how they might fail. I just don't. So
24 I - all I'm saying is that I don't know whether that 3.7
25 times 10 to the minus three is a best estimate from a valve

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1 person or whether it ought to be .1 or whether a valve
2 person says no, if - you know, we believe it would behave
3 as if the people who looked at the uncertainty
4 distribution for the testing program is essentially the
5 blue line. I just don't know. Did you talk to valve
6 people?

7 DR. GHOSH: We tried. We had heard that
8 there was some additional testing data available in
9 industry. We tried really hard to get access to some of
10 that data.

11 So the short answer is as far as NRC could,
12 you know, gather we couldn't track down any other data
13 or thoughts on this matter beyond what we had amongst our
14 team and the external peer reviewers and when we talked
15 about it here last April.

16 MS. SANTIAGO: Well, we had the Division of
17 Engineering take a look at the SRV failures and they did
18 a separate analysis with Abacus and -

19 DR. GHOSH: For the thermal seizure.

20 MS. SANTIAGO: Yeah. Yeah.

21 CHAIRMAN STETKAR: That's for the thermal.

22 DR. GHOSH: For the thermal seizure.

23 CHAIRMAN STETKAR: That's for the thermal
24 seizure. That's a different failure mechanism.

25 MS. SANTIAGO: We can go back and ask them

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1 if they have any additional -

2 DR. GHOSH: We have. We have. This is the
3 best we could do. We asked everybody we knew under the
4 sun and the only thing is it seems there may be other
5 information out there that's proprietary and unavailable
6 to us. That's just -

7 CHAIRMAN STETKAR: Let me ask this from
8 kind of a pragmatic - I mean, you have identified - I
9 actually didn't use the important stuff until the end.
10 I looked at the parameters and had questions about the
11 first and then I discovered this thing is important for
12 some reason.

13 How different would the overall SOARCA
14 results be if the stochastic failure rate looked like the
15 blue curve rather than the red curve? Do you have a sense
16 of that?

17 DR. GHOSH: You know, we could -

18 DR. GAUNTT: I could guess because -

19 MR. FULLER: This is Ed Fuller. To cut to
20 the chase, John, if you're much less likely to fail
21 stochastically before you get to core damage then the
22 likelihood becomes quite a bit higher that you will have
23 valve seizure at high temperature.

24 You'd probably easily get to 500 or 600
25 lifts until you get to that point and from that - what

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1 that would mean probably in terms of the conclusions here
2 is that you'd be more likely - much more likely to have
3 a seizure and hence more likely to have a main steam line
4 creep rupture.

5 CHAIRMAN STETKAR: And that, because I have
6 no idea how this stuff works in the real world -

7 MR. FULLER: It's because the stochastic -

8 CHAIRMAN STETKAR: No, I understand that.
9 But in terms of the overall SOARCA consequences then what
10 are the implications of that?

11 MR. FULLER: Well, you talked about the
12 SOARCA - main SOARCA?

13 CHAIRMAN STETKAR: Yeah. Well, no.

14 DR. GHOSH: So I think the uncertainty
15 report gives some indication of that. So we - because
16 we saw right away that we have these three subscenarios
17 that behave quite differently, we present the source term
18 results from the three subscenarios separately and I
19 think there is uncertainty about this distribution.

20 If the blue curve were more likely or the
21 true curve you could weight the two other subscenarios
22 higher than the stochastic.

23 So right now with this - with the
24 distribution we did implement about half of the time you
25 end up with the early stochastic failure and the other

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1 half of the time you end up with thermal seizure and some
2 subset of that thermal seizure is main steam line
3 rupture.

4 If you look at the three distributions of
5 results that are generated from early stochastic seizure
6 versus thermal seizure versus main steam line rupture,
7 basically you could weight higher the results from the
8 thermal seizure cases and the main steam line cases to
9 see what the effect would be of using the blue curve
10 versus the red curve which is -

11 CHAIRMAN STETKAR: Yeah. That's what I'm
12 asking right now. Do you have a sense for what - because
13 I don't. I'm not a level two, level three person.

14 DR. GHOSH: Do you -

15 MR. OSBORNE: This is Doug Osborne from
16 Sandia National Laboratories. The split occurs right
17 around where SRVLAM is between one and three times 10 to
18 the minus three.

19 CHAIRMAN STETKAR: Yeah. So about -
20 anywhere from 300 to 1,000 cycles. And that's okay.
21 I'm not asking precise numerical questions. I'm saying
22 suppose it was - don't think about weighting. Don't
23 think about anything else.

24 Suppose it was all from thermal seizures.
25 How would that affect the overall results of SOARCA? How

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1 would it affect the latent cancer fatalities or the early
2 fatalities?

3 DR. GHOSH: The consequences go up.

4 CHAIRMAN STETKAR: They would go up?

5 DR. GHOSH: Right.

6 CHAIRMAN STETKAR: Okay. Thank you.
7 That's all I wanted to know.

8 DR. GHOSH: Yeah.

9 CHAIRMAN STETKAR: Now, if it's shoved way
10 to the right - if the valve experts were to tell you oh,
11 yeah, it's really likely to stick open after, you know,
12 a hundred demands so that everything was shifted further
13 to the right that would drive the consequences down?

14 DR. GHOSH: Yes.

15 CHAIRMAN STETKAR: Okay. Thanks.

16 DR. GHOSH: That is correct.

17 CHAIRMAN STETKAR: Because I didn't - I
18 probably knew that when I read the report however many
19 months ago but I didn't quite make that connection.

20 DR. GHOSH: Right.

21 CHAIRMAN STETKAR: Okay. Well, I think,
22 you know, from my perspective I'm bothered by this notion
23 of you, you know, comparing things to data that you admit
24 that you don't think is relevant, especially if the -
25 because I don't understand what the purple curve is, you

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1 know, justifying that you're in the middle of the
2 available data when A, there's two failures in both of
3 those reports and B, that the reports are essentially
4 correlated at least for the - what do you call them, the
5 stochastic failure mode and I did not look for the green
6 curve in 7037. I thought I - you know, it's a 177-page
7 report and I didn't read every page of that one.

8 So I'm curious where that came from because
9 I don't know. I suspect they didn't have any real
10 failures.

11 DR. GHOSH: Yeah, it's possible.

12 CHAIRMAN STETKAR: At least the other - at
13 least the blue and where the purple ought to be was
14 constrained by actual evidence.

15 Now, you might - you might discount - you've
16 discounted that evidence because you said the testing
17 isn't necessarily relevant. And I had no idea where the
18 3.7 times 10 to the minus three came from other than Peach
19 Bottom used it in their IPE for some reason.

20 DR. GHOSH: Now, you're - this is before my
21 time.

22 CHAIRMAN STETKAR: But I mean -

23 DR. GHOSH: Yeah.

24 CHAIRMAN STETKAR: My concern is that the
25 - you've identified this as a very key parameter for the

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1 overall study results and you're using this type of
2 comparison to say well, it looks like we've captured the
3 range of uncertainty - we extend from high numbers - let's
4 say a high failure rate which says there's maybe a 50/50
5 chance that it fails early to a very low failure rate -
6 that there's a 50/50 chance that the thermal seizures get
7 you first. I understand that. But trying to then
8 justify that the red curve - I'm at a bit of a loss.

9 DR. GHOSH: Well, we've been at a loss too
10 about what to do. You know, I think we've taken our -

11 CHAIRMAN STETKAR: But in other cases
12 you've used engineering judgment. See, my whole point
13 is in other cases you've used - Joy has mentioned a few
14 places where you've relied very, very heavily on
15 engineering judgment -

16 DR. GHOSH: Yeah.

17 CHAIRMAN STETKAR: - for phenomenological
18 issues, for example, in MELCOR.

19 DR. GHOSH: You know, I think it's fair to
20 say we are using engineering judgment for this one too
21 and we probably haven't written it up correctly in terms
22 of how we explain why we are using what we use.

23 So I think we can do a better job of that.
24 I mean, I think it's clear from this discussion that we
25 can improve on what we've said, yeah.

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1 CHAIRMAN STETKAR: And at one level I
2 understand, you know, the desire - kind of the academic
3 desire, if you will, to have a distribution that spans
4 sort of those - that range of failure rates so that you
5 can examine gee, if it were really small how would the
6 world behave - gee, if it was somewhat larger how would
7 the world really behave.

8 That's kind of an academic gee, let's test
9 a few things and see how - however, if then that is
10 characterized as the results of a study that purports to
11 report the actual best estimate results from a current
12 state of knowledge uncertainty analysis that's a
13 different connotation than just looking at an exercise
14 in examining suppose it was this small, suppose it was
15 this big - how would that affect the results.

16 DR. GAUNTT: So John, I think - I don't
17 remember how these distributions came about myself so I
18 think we should see if we can reconstruct -

19 CHAIRMAN STETKAR: I'm pretty sure I know
20 how the red one was generated. I'm pretty sure it was
21 anchored at a mean value of 3.7 times 10 to the minus three
22 using the alpha and beta parameters, essentially the
23 spread of the distribution from NUREG 7037 because if you
24 looked at shapes of the distribution they're essentially
25 the same shape.

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1 DR. GHOSH: Right. Right.

2 CHAIRMAN STETKAR: So I'm pretty sure
3 that's what was done to actually create the red curve
4 which is saying that your best estimate is anchored at
5 that 3.7 times 10 to the minus three number from the
6 original Peach Bottom IPE.

7 DR. GAUNTT: Seems likely. I mean, I'd say
8 I'm pretty sure that's probably what was done.

9 DR. GHOSH: Yeah. No, I think that's -

10 CHAIRMAN STETKAR: Or it actually says
11 that's what was done.

12 DR. GAUNTT: Yeah, that's why I was pretty
13 sure that - I was trying to -

14 CHAIRMAN STETKAR: Randy said he wasn't -
15 couldn't quite remember how it was developed but okay.
16 I don't know. I mean, that's - I've ranted as much as
17 I can on this particular one.

18 I'd recommend that you pretty carefully
19 look at the justification for that distribution, granted
20 that, you know, I know because of budget constraints
21 you're not going to change it I don't think now because
22 you have to rerun the whole - you have to rerun the whole
23 study if you change that red curve.

24 DR. GHOSH: Yeah.

25 CHAIRMAN STETKAR: But at least to give the

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1 - see, the way it's presented right now the reader is
2 given the impression - an uninformed reader is given the
3 impression that in your introduction look, we have five
4 sets of pieces of data.

5 There's a lot of uncertainty about what is
6 relevant but look, the thing we picked is in the middle.
7 So we're probably okay in the middle, and I don't think
8 that the evidence quite supports that story.

9 MEMBER SCHULTZ: Well, it also implies that
10 you had this information and in fact used it to pick the
11 red curve, or at least it influenced the decision.

12 DR. GHOSH: Yeah, which I think is a correct
13 characterization. I think there are multiple reasons we
14 were comfortable with what we ended up using, recognizing
15 again that there is a lot of uncertainty about what the
16 two distribution should be and I think we try to explain
17 that we know that this one is very uncertain and it
18 influences the results.

19 With regard to - I think in the future if
20 we had more information that would help inform what the
21 true distribution would be. In our range of results we
22 have captured a broad enough set of possibilities that
23 we could do some kind of reweighting of our set of results
24 to better reflect what this distribution might be.

25 So for example if it turns out, you know,

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1 that the stochastic, you know, failures are much more
2 likely then you could weight higher the subset of the
3 early stochastic failures we have.

4 It if turns out that it's a lot less likely
5 we would weight higher the thermal seizure and main steam
6 line rupture results that we have.

7 I think if we were to get more information
8 on this particular distribution in the future we could
9 do some reweighting of the range of results we have
10 because we've captured a broad range of possibilities.
11 But we've struggled with the level of information we have
12 for this particular parameter, which is not much.

13 I mean, there is expert judgment about the fact
14 that the data we do have is not representative and why.
15 But, you know, what the actual distribution should be,
16 I mean, it has been a struggle for the last few years.

17 MS. SANTIAGO: I'm just going to move on to
18 the next parameter if that's okay.

19 CHAIRMAN STETKAR: Any other members have
20 any other questions about this particular one?

21 MEMBER REMPE: Move on to number two.

22 DR. GHOSH: Okay. So this was the chemical
23 form of iodine and cesium fraction. We provided some
24 additional information from the Phebus test, you know,
25 with regard to where the fractions came from but I don't

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1 know if members want to elaborate on what the questions
2 were with regard to this parameter.

3 CHAIRMAN STETKAR: Yeah. This was another
4 of mine. Let me reorganize my notes here. The biggest
5 - first of all, I know nothing about cesium and iodine
6 other than the fact that they're some sort of chemicals
7 that are not good for you to eat.

8 On the other hand, I do - it's my
9 understanding that the - what you did is you had - at a
10 high level you had four tests from the Phebus experiments
11 and - you had four tests from the Phebus experiments and
12 those tests gave differences in the iodine fractions and
13 quite large differences in the iodine fractions.

14 And what I think you did and let me make sure
15 is that you, first of all, took a linear average of all
16 of those tests and created a what you're calling here a
17 combination number five so that's a made up combination.
18 And then you went back and you weighted combination
19 number five.

20 You gave that a 50 percent probability of
21 being the way the real - the world works and gave each
22 of the four test results equal 12.5 percent
23 probabilities.

24 Now, in terms of uncertainty analysis, I'll
25 use this ludicrous exercise - in my pocket right now I

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1 have a penny and a nickel, two small things. I have a
2 fifty-cent piece and I have a dollar. There's kind of
3 a range of, if I look at those four experiments, the
4 average value of those four coins is 39 cents. What is
5 the probability if I reach into my pocket that I pull out
6 a 39-cent piece?

7 DR. GAUNTT: Zero.

8 CHAIRMAN STETKAR: Right. So if I'm doing
9 an uncertainty analysis why am I assigning a 50 percent
10 probability of pulling out a 39-cent piece? Isn't the
11 - and because of that I am artificially reducing the
12 uncertainty.

13 I'm putting the highest confidence in
14 something that I have no actual evidence except for the
15 fact I gave equal credence to those four tests when I
16 created that artificial combination number five.

17 I said I'm going to treat them equally
18 because I weighted them as a linear average. Just took
19 the results, added them together and divided by four.

20 DR. GAUNTT: So let me see if I can help.

21 CHAIRMAN STETKAR: And - hold on a second.
22 My concern is constraining the uncertainty. If you took
23 the four test results and gave them each 25 percent
24 weight, when you got done with the whole process I'm not
25 sure if the mean would come out close to the linearly

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1 weighted average because I don't understand all of the
2 complexities.

3 But it would seem that weighting those four
4 equally 25 percent would preserve the inherent
5 uncertainty that you've essentially accepted.

6 DR. GAUNTT: I think - I think we believed
7 there's less uncertainty than randomly picking them up,
8 so four. And if I could just explain what I think
9 combination number five I think is our current best
10 belief about cesium and iodine behavior based on the
11 Phebus tests.

12 Now, one of the Phebus tests, FPT3, is an
13 outlier because it makes use of a boron carbide pellet
14 type control assembly which we don't use in BWRs.
15 They're apparently used in European reactors.

16 So the impact of that FPT3 test was they got
17 a lot of oxidation with this boron carbide and as a result
18 they got a lot of chemistry between boron compounds and
19 cesium compounds. Somebody help me get this right.

20 But it basically tied up all the cesium
21 between molybdate and borates that allowed the gaseous
22 iodine - the elemental iodine - to come up. So they saw
23 quite high gaseous iodine in FPT3.

24 We think for American reactors that is kind
25 of an aberration because we know - we know in the BWRs

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1 based on our experience that the boron carbide and steel
2 in our bladed control assemblies becomes liquified very
3 early, runs down and doesn't produce all of this boron
4 oxidation that they saw in FPT3.

5 So sort of the collective knowledge across
6 all of the Phebus experiments - maybe collective
7 suspicion is a better word - is the old belief was cesium
8 and that's probably a combination number one - cesium
9 would be a hydroxide or iodide and basically tie up all
10 the iodine essentially with cesium.

11 That's why you have 97 percent. Three
12 percent gaseous iodine kind of follows the source term,
13 the regulatory source term construct, and the balance
14 being cesium hydroxide.

15 Phebus suggests that the principal form of
16 that cesium is not hydroxide but a less volatile
17 molybdate form and so that's why we weight molybdate -

18 MEMBER CORRADINI: Say that last part,
19 Randy, again. I didn't understand the last part. Can
20 you repeat that part?

21 DR. GAUNTT: Yeah, I know. This is -

22 MEMBER CORRADINI: I got the - I got yeah,
23 you believe combination five makes the most sense.

24 DR. GAUNTT: Yeah.

25 MEMBER CORRADINI: But then you started

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1 talking about a molybdate behavior.

2 DR. GAUNTT: Because aside from that
3 outlier in FPT3, the general theme coming out of Phebus
4 is that cesium is not the hydroxide form that we thought.

5 It's a less volatile molybdate form and so
6 that's why we weight the molybdate higher in combination
7 five. We actually believe combination five is closer to
8 reality.

9 But in order to preserve some possibility
10 of the, you know, original thinking combination one has
11 cesium hydroxide, 3 percent gaseous iodine and the
12 balance tied up as cesium iodide. That's kind of the old
13 - the old school picture.

14 MR. FULLER: This is Ed Fuller. I want to
15 take things a little further than what Randy just did.
16 From my understanding, talking to people like Mike Salay
17 who's been communicating with Dana Powers, it's actually
18 more complicated than that.

19 Yeah, the cesium was released as cesium
20 molybdate and that the cesium and molybdate seem to be
21 deposited at the same locations in the RCS.

22 They are deposited at temperatures that are
23 too high for cesium hydroxide to stay deposited.
24 However, in the Phebus fission product revaporization
25 tests much of the cesium is revaporized at temperatures

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1 consistent with cesium hydroxide chemical form.

2 So it's presumed that cesium hydroxide is
3 formed later on from chemical reactions between the
4 cesium molybdate and steam, and people's best estimates
5 are that maybe somewhere between the quarter and the
6 third of this cesium ends up in this form as it transports
7 through the - through the rest of the reactor.

8 Now, I'm no expert either on this.
9 However, what I just said makes sense to me. So I believe
10 that the right way to do this analysis is to come up with
11 a model that models the chemical reactions and the
12 subsequent revaporization.

13 MEMBER REMPE: I have a couple of questions
14 and one comment. I actually was looking at the original
15 report and I was going to try and find the references and
16 the references cited in the April version are incorrect
17 so you need to fix that.

18 But remind me about the Phebus tests. Were
19 they done for PWR-specific conditions or BWR-specific
20 conditions or would you - if you were doing this put the
21 same distribution for both Ps and Bs?

22 DR. GAUNTT: The Phebus tests were quite
23 largely PWR-centric tests, even the one that had the
24 boron carbide because it uses a very different form of
25 boron carbide than we use in the BWRs. So we really can't

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1 compare FPT3 to the U.S. for - on that account.

2 MEMBER REMPE: But then you're telling me
3 that probably you've generated a distribution that -
4 because of lack of data you would apply to Ps and Bs both
5 but you think it's less relevant to Bs? Is that what I
6 think I heard you just say?

7 DR. GAUNTT: I think as far as - yeah, the
8 speciation of cesium and iodine I think they are largely
9 the same in Ps and Bs.

10 MEMBER REMPE: So you wouldn't see any
11 difference then?

12 DR. GAUNTT: Yeah.

13 MEMBER REMPE: Okay.

14 DR. GAUNTT: Based on the - based on the
15 Phebus stuff. We preserve the old school thinking of
16 cesium hydroxide and cesium iodide and that's
17 combination number one, and I guess we ascribed the 25
18 percent to that. I forget now.

19 DR. GHOSH: Twelve and a half.

20 DR. GAUNTT: Twelve and a half. But that's
21 principally why combination five is higher weighted is
22 -

23 CHAIRMAN STETKAR: But here it's in my
24 mind, because I'm not a chemist so I can look at it this
25 way, it's somewhat similar to the discussion we had about

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1 the safety relief valves. You took four tests. The
2 values for combination number five are made up.

3 I understand - I understand what you're
4 saying about some of the chemistry, why you believe it
5 might work that way. But the reason that combination
6 number five has an iodine - gaseous iodine fraction of
7 2.77 percent is it is affected by that combination number
8 four, which you say you don't believe in. It's FPT L3
9 - FPT3.

10 DR. GAUNTT: No, I don't think the 2.7 - the
11 FPT3 I think gaseous iodine as high as 20 percent or
12 higher was observed.

13 CHAIRMAN STETKAR: I don't know.

14 DR. GAUNTT: So the numbers that are in the
15 range of like 3 percent, 2.7 percent I - and again,
16 somebody help me but I think -

17 CHAIRMAN STETKAR: But how - Randy, the way
18 that that 2.77 was calculated is if you add up .03 plus
19 .002 plus .00298 plus .0757 and divide that sum by four,
20 you get precisely .0277.

21 DR. GHOSH: Right. So okay.

22 CHAIRMAN STETKAR: That's how that - how
23 you would -

24 DR. GAUNTT: You could be right.

25 CHAIRMAN STETKAR: I am right.

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1 DR. GAUNTT: But it's - but it's like 3
2 percent. It's the - it's sort of the standard assumption
3 of gaseous iodine in -

4 CHAIRMAN STETKAR: But my point is if the
5 experts wanted an uncertainty distribution to
6 characterize the way the experts felt about iodine and
7 cesium percentages and form, then why didn't you just
8 create the uncertainty distribution? Why did you add
9 numbers together and then do all of this silly weighting?

10 MEMBER BLEY: You've said this several
11 times and I guess I want to rephrase the question because
12 what I hear from you, John, is pick the - pick the one
13 you think is most right and put all your - put that as
14 100 percent.

15 Well, that's not the right way to go if you
16 think there's some possibility the others might exist
17 under some conditions that could occur. I mean, there's
18 pretty complex stuff going on inside there that we
19 haven't actually watched and -

20 CHAIRMAN STETKAR: But I'm not saying pick
21 the one that you think is right and put all your weight
22 there. I'm saying develop a distribution that spans
23 your state of knowledge, given everything you know.

24 MEMBER BLEY: That's good.

25 CHAIRMAN STETKAR: And everything you know

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1 is characterized by those four tests plus everything else
2 you know.

3 And it's not - if you don't assign any
4 credence or you assign very little weight to the fact that
5 that one test is relevant to this particular issue for
6 U.S. boiling water reactor then it's okay. You can
7 assign a non-zero weight to it so it might be that way.

8 You don't want to discount it completely.
9 But you've assigned it equal weight to everything else
10 and then created something else that you're assigning -

11 MEMBER BLEY: This doesn't seem to be what
12 you say is your stated knowledge.

13 CHAIRMAN STETKAR: Right. That's right.

14 DR. GHOSH: But can I - can I insert here?

15 CHAIRMAN STETKAR: You're still assigning
16 12.5 percent probability.

17 DR. GHOSH: Yeah.

18 CHAIRMAN STETKAR: That's the way the world
19 will work.

20 DR. GHOSH: Okay. So -

21 DR. GAUNTT: Tina, do you want to -

22 DR. GHOSH: You can go first if you -

23 DR. GAUNTT: No, I just - I've got my own
24 view on it but I don't - I don't want to - I don't really
25 remember the numerics that went into generating this

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1 table. But at least until the revaporization stage that
2 Ed was talking about we believe the baseline behavior of
3 the cesium is molybdate and cesium iodide and we give a
4 little - a little bit of a fraction to gaseous iodine
5 because there's always some gaseous iodine hanging
6 around.

7 And so we would normally lead with
8 combination number five and I think some of the - some
9 of the other combinations here, in particular, one, is
10 to preserve the possibility that the old school thinking,
11 you know, is see what would - see what would be the
12 implications of, you know, the old view on behavior of
13 a cesium.

14 CHAIRMAN STETKAR: But, see, in a different
15 format I would then quiz you well, okay, you wanted to
16 preserve that but how confident are you that that's a
17 reflection of reality?

18 MEMBER REMPE: I think at this point -

19 CHAIRMAN STETKAR: Right now you're
20 assigning 12.5 percent confidence that that's the
21 reflection of reality to something that you want to
22 preserve and equal confidence to FPT3 which you're saying
23 you don't really have a lot of confidence in.

24 DR. GAUNTT: I don't think FPT3 is
25 represented in this amalgam. I think it's combination

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1 number four.

2 MEMBER REMPE: But I think, again, they're
3 not going to redo it. So all you want really right now
4 isn't it, John, just to have them document differently?
5 I mean -

6 CHAIRMAN STETKAR: Right. Right. Right.
7 This is - I'm not going to understand - this is another
8 issue of essentially trying to put too much reliance on
9 something that somebody else did and saying well, we
10 won't take any ownership for this. We -

11 MEMBER REMPE: And if they document it
12 differently.

13 CHAIRMAN STETKAR: We'll acknowledge -
14 we'll give equal weight to all of these things. We don't
15 have to own this. We'll create this other thing that's
16 just a linear average of them and you did take the
17 ownership of putting 50 percent probability to that.

18 But if you really think it's 95 percent
19 probability, in terms of a realistic assessment of the
20 uncertainties that's all I'm trying to look for.

21 I'm trying to understand the thought
22 process that went into the development of these
23 uncertainty distributions.

24 DR. GHOSH: Right. You know, I see that
25 there is a bit of a communication barrier between what,

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1 you know, we did and what we think we explained and how
2 it's coming across.

3 I think for us it's true that we explain kind
4 of mathematically how we came up with both the bins in
5 terms of the fractions of the iodine and the cesium
6 speciation and then the weights that we assigned to the
7 bins.

8 But implicitly, I mean, my understanding is
9 everything we've decided to go with we have used our
10 collective engineering judgment to say that this is a
11 pretty good representation of the uncertainty that we
12 think is out there.

13 So yes, for the most part we think that
14 cesium molybdate would be the dominant species but there
15 is some uncertainty about whether cesium hydroxide shows
16 up.

17 In the newer experiments, which I think were
18 not available at the time that we were finalizing this
19 distribution, it seems that you have this late phase -
20 some late phase of cesium hydroxide observation and we've
21 captured the potential effect of that and the
22 distribution that we did come up with what the exact
23 weights should be for the particular fractionation.

24 I mean, we don't have a whole lot of
25 confidence about that but we are pretty confident that

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1 we have captured a good range of the behavior and in terms
2 of - so that's one point.

3 In terms of this last bin where we averaged
4 the other four and your example of the coins, you know,
5 I think that the question there is have we done something
6 incorrect because perhaps there's only three discrete
7 possibilities that you can have a quarter or a nickel or
8 a penny and we've somehow created this artificial
9 construct in having a 39-cent possibility. But - and
10 anybody correct me if I'm wrong.

11 I think when it comes to the iodine
12 fractionation, for example, it's a continuous - it's kind
13 of a continuous possibility so I don't think we've done
14 something incorrect by averaging that possibility.

15 So it's not that it would be the 8 percent
16 or 3 percent or a 0 or 1 percent. Rather, it's something
17 between zero and whatever percent.

18 So I don't it's physically incorrect to have
19 just - to have a measure of what we think is most likely
20 to have averaged over the evidence that we do have. And
21 if, you know - again, and maybe we're not clear about this
22 in the report.

23 Three percent comes out as kind of the rough
24 number that people take to be a good guess for what the
25 actual fraction of iodine would be if you had to pick one

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1 number.

2 CHAIRMAN STETKAR: Okay. But then I would
3 say, you know, from - I would cut you off there and say
4 if that's the case why don't you use combination one but
5 just instead of assigning a 1.0 to cesium hydroxide move
6 one point over to cesium molybdate.

7 You know, if that's what - if that's what
8 you're using for justification that the 2.77 percent is
9 close to the 3 percent, you know, gaseous iodine.

10 DR. GAUNTT: Looking back on this in
11 retrospect, I almost - what I think we were trying to do
12 here is to represent alternative degrees of belief. If
13 you ask another expert they might have said well, I think
14 it's going to be this and I think that was the train of
15 thought we were on.

16 Were I to do this over today I think I would
17 - I would prescribe maybe one source term and populate,
18 you know, populate all of these to some extent to try and
19 represent.

20 CHAIRMAN STETKAR: Or you could have used
21 four and assigned different weights to each of the four
22 if you wanted to retain some small likelihood that FPT3
23 is - might be the way the world works.

24 DR. GAUNTT: Yeah, the -

25 CHAIRMAN STETKAR: But creating that -

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1 creating that fifth and assigning most weight - remember,
2 if I sample things 100 times that fifth one is going to
3 get sampled 50 out of those 100 times.

4 MEMBER REMPE: Again, I guess I -

5 CHAIRMAN STETKAR: And it might be an okay
6 representation for the way that you understand the world
7 to work. But the story about how it's developed doesn't
8 quite hang together very well.

9 It's too much I took a number from here and
10 I didn't - I sort of felt okay with the overall results
11 and - but it relies basically on addition and division
12 rather than engineering justification.

13 MEMBER REMPE: I guess I - also, I'm still
14 questioning about the BWR versus PWR because basically
15 isn't it - I'm reading your report.

16 They're talking about how the behavior of
17 iodine chemistry with respect to paint, sweated
18 surfaces, buffered and unbuffered pools, undergoing
19 radiolysis - these things I think would be - is that
20 what's driving the chemical form of iodine and cesium is
21 the things like paints in the building and -

22 DR. GAUNTT: Well, not so much - not so much
23 in the accident phase. The iodine chemistry in paints
24 and all of that stuff is longer term behavior of iodine
25 in the containment. It's also on the research

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1 forefront. We don't have models yet in hand.

2 MEMBER REMPE: Because, I mean,
3 combination one is due to steam condensation on the
4 painted condenser is what they're saying so if I read
5 what's in this NUREG.

6 And so I'm just kind of wondering, again,
7 based on what I'm reading I'm wondering well, maybe you
8 should have looked at Peach Bottom and decided which
9 geometry was most representative too and was that done
10 at all by the experts or they said nah, we're just going
11 to go with something we believe feels right because of
12 the way the experiments were performed and what was in
13 the experiment?

14 It wasn't the geometry of the experiment and
15 painted surfaces or pools in the experiment. All they
16 can - you know, I just am wondering why they didn't.

17 DR. GAUNTT: I'm going to have to go back,
18 Joy, and look at what you're reading because I'm not
19 recalling that.

20 MEMBER REMPE: It's in the NUREG - the 70
21 analysis NUREG. Yeah, again, documentation is really
22 important because if we can't remember a few months -
23 about a year from now.

24 MR. OSBORNE: Can I make a comment here?

25 MEMBER REMPE: You bet.

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1 DR. GHOSH: Yeah, please.

2 MR. OSBORNE: Yeah. The quote on steam
3 condensation on painted condensers and absorbed
4 processes and other containment surfaces or both that's
5 that 0.3 percent plus or minus 0.16 percent. That does
6 not pertain to the 3 percent that we used for combination
7 one.

8 MEMBER REMPE: Okay. But then even in
9 combination two, again, they're talking about the
10 gaseous - okay. So basically then it's not the geometry
11 for the cesium. It's cesium iodide and the cesium
12 molybdate. It's not the geometry of the test. It's
13 just whether the fuel -

14 MR. OSBORNE: That was actually - whether
15 it was the first or second oxidation phase in which we
16 believed - which of those oxidation phases we believe
17 best represented the iodine concentration.

18 MEMBER REMPE: And when you say the
19 oxidation phases it's based on -

20 MR. OSBORNE: From the Phebus experiment.
21 If you look at combination -

22 MEMBER REMPE: Is it the fuel or is it - what
23 is it that's driving it? What is the oxidation phase?
24 Is it the zircaloy used in the cladding or what is it?

25 MR. OSBORNE: My understanding, yes. It's

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1 the - it's the zircaloy oxidation phase.

2 MEMBER REMPE: Okay. So then did Phebus
3 use the same type of zircaloy for all tests?

4 DR. GAUNTT: Pretty sure.

5 MEMBER REMPE: So why would the results be
6 so different then or somewhat different?

7 DR. GHOSH: This is the first versus the
8 second oxidation, Doug, you were saying?

9 MR. OSBORNE: I'm not exactly sure. I just
10 worked with someone that was - that was intimately
11 familiar with the Phebus test and he provided me the peak
12 iodine concentrations for the first or second oxidation
13 phase because I guess some of these experiments you
14 didn't have a single oxidation occurrence.

15 And then trying to wrap our heads around,
16 as Randy was pointing out for FPT3, they had a much higher
17 oxidation or release of iodine observed for the main
18 oxidation phase, which we didn't really think was
19 appropriate.

20 MEMBER REMPE: Okay. So like
21 combination four had like 7.5 - over 7.5 percent of iodine
22 and actually iodine that is in the gaseous phase will
23 drive the dose.

24 And so even though the painted surfaces and
25 stuff like that may not be that important it actually

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1 could be even though we're just talking about the gaseous
2 iodine phase, right?

3 MR. OSBORNE: Right, but you're talking a
4 very low percentage from my understanding as far as what
5 you're seeing from those surfaces coming on and off
6 paint, and also from my understanding those particles are
7 extremely small and they just kind of reach an
8 equilibrium within containment going in and out of the
9 - out of the paint surfaces.

10 MEMBER REMPE: And the expert you talked to
11 thought it was relevant for BWR geometries to knowing a
12 USBWR geometry. Is the expert from France?

13 MR. OSBORNE: No. Actually, he used to
14 work here at Sandia but he doesn't work here anymore.

15 DR. GAUNTT: I think maybe he's talking
16 about Casey. I don't know.

17 MR. OSBORNE: No. Greg June.

18 DR. GAUNTT: Oh, right. Okay.

19 MEMBER REMPE: Okay. Again, I think
20 documentation would help big time on what was picked to
21 be the most relevant and why.

22 MR. OSBORNE: The only thing else I'd take
23 into consideration it's not just iodine we're looking at
24 here.

25 We're also trying to look at the interplay

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1 between cesium hydroxide and cesium molybdate, which is
2 why combination two and four you have a - you know, you
3 have a low and high iodine concentration and then a 50/50
4 split between cesium hydroxide and cesium molybdate
5 mainly because we're just trying to see if something were
6 to show up during the uncertainty analysis that, you
7 know, may be important there.

8 MEMBER REMPE: Okay.

9 DR. GHOSH: I think we talked about we did
10 find that for the high temperature scenarios like the
11 main steam line rupture scenarios when we sampled the
12 cesium hydroxide form we got actually lower releases,
13 which was counter intuitive, you know, from what we were
14 originally thinking because of the chem absorption on the
15 upper internals, which was kind of interesting. And we
16 - and we ran that down to make sure that that was a
17 legitimate result and apparently it is.

18 MELCOR has this chem absorption model and
19 it occurs for the cesium hydroxide and not for the
20 molybdate because we were thinking with the molybdate
21 you'd always have lower releases.

22 But in fact with the chem absorption at high
23 temperatures you have - for the high, high temperature
24 scenarios you have less release with the cesium hydroxide
25 which is very interesting. It's an interesting finding.

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1 Were there any more questions on the
2 CHEMFORM or should we move to the next parameter?

3 MEMBER SCHULTZ: I guess just one more
4 comment. Perhaps it reinforces what John said but in
5 what you want - what you want to present associated with
6 the selection of these combinations and especially the
7 determination for combination number five is that - the
8 feature of engineering judgment that was used.

9 You have some data that is there but in terms
10 of developing then the uncertainty features of each of
11 the combinations or the characteristics of each of the
12 combinations that's - was just stated engineering
13 judgment in order to derive - in order to drive the
14 understanding of what the situation could be different
15 sets were selected.

16 Combination of requirements completely I
17 presume - not completely - it is - it represents the
18 engineering judgment of some consensus that what we ought
19 to do is take averages for the iodine and we ought to also
20 create the molybdenum cesium aspect as one versus
21 hydroxide as zero.

22 That's fully engineering judgment and one
23 hopefully that would represent some determination by the
24 team that one would assign that - the combination number
25 five will be a .5 weight.

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1 So that ought to be explained somewhat
2 differently than at least the presentation would imply.

3 DR. GHOSH: Okay. Okay.

4 CHAIRMAN STETKAR: Yeah, I'd echo that.
5 The summary that we heard here today orally made a lot
6 more sense than what you can read in the report.

7 If you read in the report it just says we
8 took these four tests, we weighted them equally, we
9 created a fifth, we gave that four times the weight of
10 anything else and that's it, and that's what we used.

11 DR. GAUNTT: So we maybe should revisit
12 that.

13 DR. GHOSH: Yeah. All right. So we'll
14 read the transcript of this meeting -

15 CHAIRMAN STETKAR: It's important.
16 Again, it's important to document what you did and why
17 you did it and why you feel confident, and after we're
18 all gone somebody's going to pick up this report or even
19 if we're still here somebody's going to pick up the report
20 and say oh, in NUREG/CR-7155 they used this distribution
21 and gave it this weight, therefore that is the way the
22 world works. That's the way the NRC has done things.
23 Twenty years from now -

24 MEMBER SCHULTZ: Which is here in the
25 discussion we heard well, we left in combination one in

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1 its form as shown here because it allowed us to explore
2 that aspect of the uncertainty distribution. We may or
3 may not have believed that at all.

4 But in order to explore it we left it there.
5 But it's - he inputted or determined no, that's not real
6 - everything else is representative but that's not real
7 - we'll keep it out.

8 CHAIRMAN STETKAR: Well, but again in terms
9 of characterizing the uncertainty which is the whole -
10 we're not chemists but in terms of characterizing the
11 uncertainty the study says that the collective wisdom
12 believes that 12.5 percent confidence they would - they
13 would make that bet with those odds that that's the way
14 the world works and that's what it means here.

15 MS. SANTIAGO: We'll explain it. We'll
16 work on that.

17 DR. GHOSH: Okay. The next parameter was
18 the dry well liner failure area. I think one of the
19 questions had to do with water.

20 MEMBER CORRADINI: Well, that's already
21 been answered.

22 DR. GHOSH: Yes, that's been answered.

23 MEMBER CORRADINI: There is none.

24 DR. GHOSH: Right. That's right.
25 Because this is -

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1 MEMBER CORRADINI: And the second part is
2 I want - I know we're not going to get through all the
3 pieces so I'm willing to stipulate I understand why you
4 have the blue triangle where it is.

5 DR. GHOSH: Okay.

6 MEMBER CORRADINI: It's the pressurization
7 rate and the fact there's no water.

8 DR. GHOSH: Yes. Right.

9 MEMBER CORRADINI: And let's move on.

10 DR. GHOSH: Okay. Okay.

11 MEMBER CORRADINI: Otherwise, we're not
12 going to get to the fun core melt ones.

13 DR. GHOSH: Okay. All right. So the next
14 one is the battery duration. I think this is actually
15 a combined question, both a question of why did we pick
16 the distribution that we did pick and another one had to
17 do with the timing of the operator action.

18 So we did provide you a table that shows the
19 timing of the operator actions that are assumed and why
20 that's based on the EOPs and, you know, our discussion
21 with the plant personnel as part of the original SOARCA
22 project.

23 For the distribution of the battery
24 durations, this was largely - the upper end was largely
25 based on discussions with plant personnel and the lower

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1 end is the tech spec section.

2 So according to tech specs you always have
3 to have a minimum of two hours of battery and so in essence
4 if you - if you have the worst case where you just have
5 the battery, it's getting old, you don't do any load
6 shedding, et cetera, et cetera, you're required to have
7 at least two hours of battery life.

8 And on the upper end - we got a lot of
9 questions about this after the Fukushima accident - the
10 upper end is capped at eight and that's largely because
11 of discussions that we had with plant personnel in terms
12 what they thought would be the - a distribution for the
13 potential battery life.

14 DR. GAUNTT: What - okay. Go on.

15 DR. GHOSH: And what it should represent is
16 that the operators have done an effective load shedding
17 and it's - maybe it's a newer battery. So, you know, it
18 lasts longer.

19 The four hours is kind of a nominal - you
20 know, what the expectation is. But, you know, the
21 collective wisdom is that it can certainly last a lot
22 longer. So anyway so that's kind of -

23 CHAIRMAN STETKAR: Collective wisdom based
24 on what?

25 DR. GHOSH: Based on - oh, so if you had like

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1 the two-hour battery life that's required and you - and
2 you have effective load shedding. So if you have an -

3 CHAIRMAN STETKAR: What - well, no. With
4 effective load shedding I can get to four. So this is
5 super effective load shedding or is it shedding more
6 loads or -

7 DR. GHOSH: There's also - so there's a
8 couple of points. It's kind of when you do the load
9 shedding. So I think for - to get to the four hours you
10 had to do the load shedding by a particular time.

11 CHAIRMAN STETKAR: And what is that time?

12 DR. GHOSH: I think if you start - we put
13 that in the writeup.

14 CHAIRMAN STETKAR: I think it was one hour.

15 DR. GHOSH: I can look it up for you. We
16 put it in the writeup. But yeah, there's an assumed
17 time. So there's some chance that when you recognize
18 you're in a station blackout, you know, scenario you may
19 start to do earlier load shedding.

20 But also from what I understand there's just
21 natural variability depending on how old the battery is.
22 So if the batteries are newer they should last quite a
23 bit longer but, you know, there's a - there's kind of a
24 battery lifetime in terms of, you know, as it ages if it's
25 not going to last as long.

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1 So there's some natural variability with
2 regard to at the time of a potential accident what the
3 - you know, what the kind of nominal battery life would
4 be with the assumed load shedding. So that's where the
5 upper end of the distribution comes from.

6 And as you know - so, again, this is the
7 unmitigated case. You know, in our scenario basically
8 RCIC duration is tied, you know, one on one. It's
9 deterministic based on the battery life.

10 We got a lot of questions about the fact that
11 at Fukushima you had RCIC running a whole lot longer, you
12 know, than eight hours. Eight hours is pretty short.

13 But, you know, we stuck with our original,
14 you know, scenario as we had defined it and this is the
15 unmitigated long-term station blackout case.

16 CHAIRMAN STETKAR: I just observed that the
17 best estimate or the expected value of the duration of
18 the batteries is 4.2 hours from this distribution.

19 It's about 10 minutes longer than the
20 four-hour value that they say they can get with effective
21 load shedding, whatever effective load shedding is.

22 So that that additional four hours in this
23 distribution makes a big difference.

24 DR. GHOSH: But that -

25 CHAIRMAN STETKAR: It's a big difference.

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1 DR. GHOSH: Okay. For the expected four
2 hours, again, I think that's based on - that's almost like
3 meeting a safety criteria.

4 So taking a conservative guess at - to take
5 a conservative estimate oh, the battery might be old and,
6 you know, how long can you -

7 CHAIRMAN STETKAR: Okay. Let me - let me
8 try this. I've talked to a lot of plant operators and
9 met these people and they're always optimistic about how
10 good stuff is.

11 They're always optimistic about how good
12 stuff is. Stuff always works better and lasts longer and
13 costs less than it does in the real world.

14 In this particular case, you are using from
15 what I'm hearing without real engineering analyses or
16 data word of talking to people at the plant to double the
17 life of the battery.

18 And there might be justification for that
19 if indeed they've actually tested battery - their
20 batteries.

21 DR. GHOSH: Yeah, but -

22 CHAIRMAN STETKAR: And if they haven't I'm
23 curious about what that is. Now, the question is, again,
24 I don't care what the distribution is if the differences
25 don't make any difference.

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1 But in this case if you said that the upper
2 bound of the battery life was 4.5 or 5 hours rather than
3 8 hours so you had a mean duration on the order of, I don't
4 know what it would be, 3.5 something like that.

5 How much difference would that make to the
6 overall results? You'd essentially have RCIC failing
7 much earlier.

8 DR. GHOSH: Yeah. The - you know, it's
9 interesting because going into this study we thought that
10 the battery duration was going to matter a lot. It did
11 not show up as one of the top variables in the regression
12 analysis. So I -

13 CHAIRMAN STETKAR: I noticed that. That
14 was curious.

15 DR. GHOSH: Yeah. So I don't think the
16 exact distribution matters all that much. But I will
17 tell you this is one of the difficulties but it informed
18 our engineering judgment.

19 Our understanding was that there are
20 calculations that exist that show the battery life could
21 be as high as 12 hours or more but we don't have any public
22 report to rely on and in the end, you know, when we discuss
23 with plant personnel, you know, what should we use, I
24 mean, it was decided that they didn't want to go above
25 eight hours even though they had calculations that show

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1 it could be 12 hours or more internally.

2 You know, from - I guess from some team
3 members who weren't core team members we had people
4 pushing us to go as high as 16 hours because they felt
5 that the four-hour number is horribly conservative and
6 that there are calculations that exist out there that
7 show batteries can last a lot longer.

8 But, again, in the end, you know, based on
9 all the information that we had we settled on this
10 distribution and then after we saw that it didn't make
11 much difference to our results. We didn't probe that
12 much further to see, you know, what - to try to come up
13 with a more precise distribution.

14 CHAIRMAN STETKAR: Were the - one last
15 thing. Provoke someone else over here. Were those
16 calculations that you saw out to 12 hours based on
17 different assumptions about the timing and amount of load
18 that was shed or is that just variability?

19 Given the fact that you shed loads X by time
20 Y you saw variability of, let's say, four to 16 hours in
21 the expected life of the battery.

22 DR. GHOSH: Yeah. To be honest,
23 unfortunately I wasn't at those plant visits. So I
24 wasn't part of those conversations to know what went into
25 those engineering calculations.

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1 I don't know if anybody here or anybody on
2 the phone from Sandia knows. I don't think actually any
3 of us who are here today or on the phone went to those
4 visits where we gathered that informal information.

5 CHAIRMAN STETKAR: Because as I understand
6 it the timing of this is based on a presumption that the
7 operators will successfully shed the required loads at
8 a particular time.

9 There isn't any uncertainty in the human
10 reliability analysis for the timing of that load shedding
11 or the amount of load that's shed. It's just simply a
12 chunk of load that comes off guaranteed at a particular
13 time.

14 DR. GHOSH: I think for the numbers that
15 were sampling below four hours in essence we are assuming
16 that they've not done a good job of load shedding and
17 that's why the battery is not lasting as long.

18 So the two hours representing -

19 CHAIRMAN STETKAR: If they don't shed any
20 loads it ought to be -

21 DR. GHOSH: It's two hours, yeah.

22 CHAIRMAN STETKAR: Yeah.

23 DR. GHOSH: Right. So two hours is kind
24 of, you know, if they didn't do any kind of load shedding
25 and the battery is getting old you're going to have about

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1 two hours before you run out.

2 So that's kind of the limiting case. Well,
3 oops, we didn't shed when we were supposed to and the
4 battery is old.

5 It's, you know, two hours is going to be the
6 - what we have and so between two and four hours kind of
7 represents that it's an older battery and maybe the load
8 shedding isn't as effective as it should be.

9 So that's kind of a, you know, the below the
10 median distribution is. At least in our minds that's the
11 thinking of what that represents.

12 MEMBER SCHULTZ: But the question is going
13 beyond two hours to four hours and then certainly beyond
14 four hours and it sounds like, as John said, that it is
15 relying upon the capability of the operators and it also
16 sounds like it could be relying upon the excess of other
17 plant equipment to perform flawlessly.

18 DR. GHOSH: I think it - yeah, you know, the
19 beyond four hours again it's a largely - a lot of it is
20 battery life. So if you have a newer battery it's going
21 to last longer. But the tech spec limit is two hours.
22 So they, you know, they'll run - they'll keep a certain
23 battery for a certain age and it's random when the
24 accident happened.

25 So you don't know how old that battery is.

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1 So the four hours is perhaps somewhat conservative in
2 that that's the tech spec limit plus the assumed and
3 EOP-directed actions for load shedding.

4 So the beyond the four hours is, I think,
5 at least as we represented I don't think it's necessarily
6 you're doing anything on top of what the EOP requires you
7 to do.

8 It's more that you have a newer battery,
9 which, you know, you have some chance that if the accident
10 happens when you have the newer battery so you expect that
11 the battery's going to last longer.

12 MEMBER BLEY: I think - you know, the only
13 thing I'd be curious about I've seen people associated
14 with plants do some calculations that go out for long
15 times. But they weren't for the accidents you're
16 looking at and they - they just had those calcs. So they
17 were more under kind of normal - not accident.

18 CONSULTANT SHACK: What the report says is
19 that you got - the NUREG 1150 assume 10 to 12 hours -

20 DR. GHOSH: Yeah.

21 CONSULTANT SHACK: - and the licensee said
22 no, we can't see anything beyond eight.

23 DR. GHOSH: Yeah.

24 MS. SANTIAGO: They didn't want us to go
25 beyond eight. They could see - they just didn't want us

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1 to -

2 DR. GHOSH: Right. And I think that they
3 did have calculations that supported the possibility.
4 But unfortunately I don't have the details of those
5 calculations and in the end we deferred to their judgment
6 of limiting it to eight, yeah.

7 DR. GAUNTT: And as far as the analyses
8 goes, what the battery life determines is when the
9 operators lose control of the SRV for pressure control
10 and when they assume - when they assume that the - they
11 lose control of RCIC and the RCIC dies.

12 CHAIRMAN STETKAR: RCIC doesn't die then.
13 It fills up the steam line and then dies sometime later.

14 DR. GAUNTT: Right. Right. But that's on
15 the death path. Since you mentioned it though -

16 CHAIRMAN STETKAR: Yeah.

17 DR. GAUNTT: - it's another one of those
18 issues like SRV behavior under real-world conditions and
19 it's a thread we're pulling with DOE and EPRI and the BWR
20 owner's group is that apparently this happened in
21 Fukushima unit two, and without battery power the RCIC
22 ran for 72 hours apparently in this flowing up to the
23 steam line and it didn't kill the - didn't kill the Terry
24 Turbine.

25 So we're kind of very interested in this

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1 post-Fukushima post-SOARCA what's the real world
2 performance of critical equipment that we typically
3 assume fails. In the SAMGs we assume you lose battery
4 power and you lose RCIC.

5 CHAIRMAN STETKAR: Do you happen to know,
6 Randy, did they have the old style Terry Turbine with a
7 mechanical governor?

8 DR. GAUNTT: I think so.

9 CHAIRMAN STETKAR: Does Peach Bottom have
10 that kind or, you know, a lot of the new turbines have
11 been retrofitted with electronic governors.

12 DR. GAUNTT: Yeah. But I think in
13 Fukushima I'm told - and if Kyle's listening I know he
14 knows all about it - I think they were mechanical.

15 CHAIRMAN STETKAR: Okay.

16 DR. GAUNTT: And they should have oversped
17 or something like that but the thinking is liquid water
18 carryover got into the turbine and slowed the turbine
19 down and sort of got into the self regulating mode.

20 We're trying to interest whoever's
21 interested in investigating real-world performance and
22 safety relief balance and RCIC pumps and aux feed steam
23 driven pumps and so forth.

24 CHAIRMAN STETKAR: Anything else on the
25 batteries?

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1 DR. GHOSH: Okay. So the next one is the
2 SRV open area fraction, and as I mentioned earlier when
3 the SRV fails stochastically we assume that it fails in
4 a fully open position because the failure is the failure
5 to reseal, and when we have a thermal seizure of the SRV
6 in that case we sample an open area because the thinking
7 is that it may - it may seize in some position that's not
8 fully open.

9 The distribution that we assign to that is
10 skewed more towards the fully open area and this - you
11 know, this came about through extensive discussion both
12 among the team as well as the peer reviewers - our
13 external peer reviewers.

14 So that's why you see that, you know, the
15 curve is steeper over to the right instead of just the
16 uniformed distribution which is what we originally had.
17 But when we thought about it more it made sense that it
18 would be skewed more towards being fully open.

19 And in this case, you see that in the SOARCA study
20 they assume that if you had a thermal seizure that it
21 would open - fully open but it was never relevant because
22 this was never exercised in the SOARCA, you know, study
23 because we had the early SRV stochastic failure.

24 So it didn't get to a thermal seizure. But
25 that's just there for reference.

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1 CHAIRMAN STETKAR: How is this treated - we
2 heard a little bit earlier because, again, I don't
3 understand any of this part of the phenomena. We may or
4 may not have lost or gained a person on the bridge line.

5 DR. GHOSH: Yeah. Are there still folks on
6 the bridge line at Sandia?

7 MR. JONES: This is Joe Jones. Yeah, I'm
8 here.

9 CHAIRMAN STETKAR: Okay. Things happen
10 and we have no knowledge of what's going on. It's an open
11 -

12 MR. OSBORNE: Doug Osborne here.

13 DR. GHOSH: Okay, great. And Doug, that
14 last one - okay.

15 CHAIRMAN STETKAR: In the model if this
16 failure mode is invoked does - how far open does the valve
17 have to be to actually depressurize the reactor?
18 Obviously, if the valve is stuck fully - open stochastic
19 failure mode presumes that it's stuck fully open and it
20 depressurizes. How far open does the valve have to be
21 to do that?

22 DR. GAUNTT: What I'm recalling - this is
23 the gist of it is what I'm recalling and then I'll ask
24 Kyle to correct me. But if that SRV sticks open at any
25 open position greater than about half it will

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1 depressurize quickly enough that you can - you can avoid
2 any main steam line rupture.

3 CHAIRMAN STETKAR: Okay. So according to
4 this distribution there's an 80 percent probability that
5 that's what it looks like.

6 DR. GAUNTT: Occurs.

7 CHAIRMAN STETKAR: Roughly. I think it's
8 78. Not that I didn't run out the number.

9 So you're essentially saying that given it
10 does stick it still sticks - it's a three to one
11 likelihood that it sticks open far enough to
12 depressurize.

13 DR. GAUNTT: You know, I don't - I don't
14 know if I - that's four to one.

15 MR. ROSS: Randy?

16 DR. GAUNTT: Yeah?

17 MR. ROSS: Actually it's 70 percent.

18 DR. GAUNTT: Seventy percent if it sticks
19 open -

20 CHAIRMAN STETKAR: Like 72 percent if you
21 want to be precise. I ran out the distribution. That's
22 the - 50 percentile is 72 percent open.

23 But that's - I'm sorry. Fifty percent open
24 is about 70 - I didn't run out the 50 percent open.
25 Forty-eight percent open is the 20th percentile.

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1 So it's about 78 I guess that's what he said.
2 The - and that's okay. What evidence did you use for
3 this? You actually talked to valve people about this
4 failure mode?

5 DR. GHOSH: Yeah, and so we had the Division
6 of Engineering at NRC do some work on what a thermal
7 seizure for the SRV looks like and kind of what the
8 temperature of, you know, of when this could possibly be.

9 Actually, that's also a separate variable.
10 That's the temperature at which it could seize, and then
11 we also talked about, you know, what the open area is
12 likely to be, you know, if it were to seize in that thermal
13 mode.

14 Maybe I'll defer to Kyle because I think
15 he's our resident expert in this area with respect to the
16 open area fraction.

17 Kyle, I don't know - if you want to add
18 something go ahead. I'm also going to look up what we
19 - what we've said most recently.

20 MR. ROSS: Yes. I'd like to - I can't hear
21 the ACRS questions real good.

22 CHAIRMAN STETKAR: Oh.

23 MR. ROSS: Voices come in and out. But
24 basically the valve needs to fail late with respect to
25 number of cycles in order to get a failure by overheating

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1 and then those are - that's necessary. And then you also
2 need to have a limited open area of the stuck valve for
3 that to lead to a main steam line rupture. Is that - is
4 that -

5 CHAIRMAN STETKAR: What I was - and I speak
6 up because unfortunately the mikes that pick me up are
7 about as far away from me as they can get.

8 The curve that we're looking at, Kyle, on
9 - I'm assuming you have the handouts - is on sheet 37 of
10 the presentation. That curve shows a probability
11 distribution for the open fraction.

12 MR. ROSS: Okay.

13 CHAIRMAN STETKAR: And that probability
14 distribution essentially says that we're 78 percent
15 confident that the valve, if it fails by this thermal
16 failure mode, will fail open far enough to depressurize
17 the reactor. In other words, that you will not have a
18 thermal pressure reduced steam line failure.

19 MR. ROSS: Okay. Yes.

20 CHAIRMAN STETKAR: And what I was asking
21 about is what's the basis for that curve. Did you
22 consult with experts on valve operation, these types of
23 valves under -

24 MR. ROSS: No. This was MELCOR results.

25 CHAIRMAN STETKAR: No. This curve is not

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1 MELCOR results. This is an input.

2 DR. GHOSH: The question is how did we come
3 up with the distribution for the SRV open area fraction
4 when you have a thermal seizure.

5 MR. ROSS: Okay.

6 DR. GHOSH: So that's like - it's at Page
7 411 of our report - I just pulled it up - where we talk
8 about that, and I don't think we added a whole lot in our
9 writeup that we provided.

10 CHAIRMAN STETKAR: You did not add a whole
11 lot in the writeup that we got last week.

12 DR. GHOSH: Right.

13 CHAIRMAN STETKAR: So that's why I was
14 asking.

15 MR. ROSS: Aren't we just from .1 to 1 on
16 the fraction uniformly or -

17 CHAIRMAN STETKAR: It's not uniformly.

18 DR. GHOSH: We have a triangular
19 distribution. I'm realizing now - so okay. This is a
20 to-do for me.

21 We have more than what we put in the report
22 and also what we gave you last week because I remember
23 the discussion. But I see we did not capture it very well
24 in our writeup.

25 CHAIRMAN STETKAR: Yeah. I didn't learn a

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1 lot by this.

2 DR. GHOSH: Yeah, I apologize for that.
3 There is a reason that we skewed it towards the more fully
4 open areas because we originally had a uniform
5 distribution and we were given the feedback by multiple
6 knowledgeable people that that wasn't appropriate, that
7 it was much more likely to be a more open area.

8 You know, in other words, that values that
9 are skewed towards fully open is much more likely than
10 values, you know, towards the lower end and - but I see
11 we did not capture that in our writing. So we need to
12 do that.

13 CHAIRMAN STETKAR: Good. I mean, that's
14 exactly what I was looking for, Tina -

15 DR. GHOSH: Yeah, I apologize.

16 CHAIRMAN STETKAR: - is that whatever basis
17 you had for the shape of that distribution. You know,
18 I don't care whether it's log uniform or -

19 DR. GHOSH: Sure.

20 CHAIRMAN STETKAR: - why it's skewed the
21 way it's skewed. But it's certainly skewed toward
22 higher probabilities of a big enough open area to
23 depressurize here and there must -

24 DR. GHOSH: Right.

25 CHAIRMAN STETKAR: - must be some better

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1 documentation for that.

2 DR. GHOSH: Yes. I apologize for that.

3 CHAIRMAN STETKAR: No, that's fine.

4 DR. GHOSH: I have it in my notes and I have
5 to - I have to put it in there for -

6 CHAIRMAN STETKAR: That's - I trust that
7 you do. I hope you put it in the report.

8 MEMBER REMPE: The analysis was done
9 assuming a skewed distribution.

10 DR. GHOSH: Right. Right.

11 MEMBER REMPE: Okay.

12 CHAIRMAN STETKAR: Yeah. The analysis was
13 based on the curve that we have on sheet 37, right?

14 DR. GHOSH: But the thing that Doug pointed
15 out and I'll just repeat that is the - that even areas
16 as high as 70 percent could result in main steam line
17 ruptures.

18 When we - when we did the sensitivity
19 studies in the SOARCA, you know, project in 7110 we had
20 a section on sensitivities that just had a limited number
21 of sensitivities to look up the effect of main steam line
22 rupture area, and there we had - we had used a fraction
23 of 10 percent of open area to see, you know, if you have
24 10 percent that you could get main steam line creep
25 rupture and at the time when we first started the

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1 uncertainty study we were thinking that only very small
2 open fractions could lead to main steam line ruptures.

3 But we found through the more comprehensive
4 uncertainty study that in fact even with much larger open
5 areas you can still get main steam line rupture.

6 CHAIRMAN STETKAR: You said as large as 70
7 percent open?

8 DR. GHOSH: Yeah. Isn't that what you
9 said, Doug?

10 MR. OSBORNE: That's correct.

11 DR. GHOSH: Yeah.

12 CHAIRMAN STETKAR: Okay.

13 DR. GHOSH: So we will - we will definitely
14 add something on that. Should we move to the next one
15 or is there anything else -

16 CHAIRMAN STETKAR: Unless anyone has any
17 other other questions about the valve.

18 MS. SANTIAGO: So we're on slide 38 now?

19 DR. GHOSH: Right. So this now is the -
20 this is the area fraction of the main steam line when you
21 have a main steam line rupture.

22 And here I think we did explain much better
23 why we skewed the area so much more fully open and there's
24 some experimental dates I think from Europe for saying
25 why we think it's more fully open.

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1 We had a pretty detailed discussion on this
2 with the external peer reviewers and I think we provided
3 you some of that discussion as well.

4 CHAIRMAN STETKAR: Well, the only comment
5 I had on this is that there's very high confidence that
6 the - that the size of the opening is quite large here
7 and therefore, you know, in terms of is - in general, you
8 know, our discussion of why did you pick certain
9 parameters for examination in the uncertainty analysis
10 I can't imagine that the uncertainty in this contributes
11 anything to the overall uncertainty because it's
12 presented as a cumulative here. But if you looked at the
13 PDF it would be a very, very narrow distribution with a
14 little tiny little tail.

15 DR. GHOSH: Right. Yeah, I think - right.
16 It was - if we found something interesting in this small
17 percentage of cases where we have a small opening, right.

18 CHAIRMAN STETKAR: Okay. Okay.

19 DR. GHOSH: Okay. Slide - so the next one
20 is on slide 39 is the radial debris relocation time
21 constants and the next two slides kind of go together.
22 So the first one is for the solid and the second one is
23 for the liquid on slide 40. So slides 39 and 40.

24 And, you know, these were - I'll let Randy
25 tag after me but these were an example where we tried to

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1 pick a reasonable range of variation around what is sort
2 of the best practice, you know, value that's typically
3 used in MELCOR models and we thought it was reasonable
4 to go up by - go up and down by a factor of two and we
5 thought yeah, it's plausible that it's - that this
6 constant is only half as big. It's plausible that's it's
7 twice as big. Maybe hard to imagine a lot more variation
8 beyond that and we thought it was equally likely that it
9 would be less or more.

10 So we picked a triangular distribution with
11 the peak at the mode, which is sort of the best practices
12 MELCOR value, and then with the values on either side
13 being equally likely and kind of falling away in terms
14 of how likely it is when you get further away from this
15 nominal value.

16 So that was the thinking behind the next two
17 distributions for the solid and the liquid radial debris
18 relocation time constants.

19 MEMBER REMPE: So what do you do? You
20 run the code and you use one value and then you double
21 it and you run the code again and say well, maybe that's
22 possible the relocation could occur that way in that
23 amount of time?

24 I mean, there's no data so that must be -
25 so you just kind of run the code for a particular scenario

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1 and then you've applied this distribution for all the
2 scenarios?

3 DR. GAUNTT: We've used these nominal
4 values for a long time and they're just engineering gut
5 is what I would say, and I'll invite Kyle if he knows any
6 more rationale behind why these numbers are the way they
7 are.

8 MR. ROSS: Well, I mean, not the absolute
9 values of the numbers but I might add that we did
10 correlate them. So we didn't - so in doing the random
11 sampling we didn't wind up with solid material that
12 relocated radially faster than liquid material.

13 CHAIRMAN STETKAR: Good, Kyle. That's - I
14 didn't find that in the report, by the way, because it's
15 one of the questions that I had was I have no fundamental
16 physics knowledge of what's going on here but it seemed
17 like they ought to be correlated. And you said that you
18 did correlate them?

19 MR. ROSS: We did, yeah.

20 CHAIRMAN STETKAR: That ought to -

21 MEMBER REMPE: What about ex-vessel? Is
22 there any correlation between how you assume spreading
23 and relocation in-vessel versus spreading ex-vessel or
24 is this -

25 MR. ROSS: There were two similar variables

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1 for ex-vessel that we - that we also correlated.

2 MEMBER REMPE: Okay. I don't recall that
3 being in the report either. Maybe I missed it but -

4 DR. GAUNTT: But I don't know, Joy, if your
5 question was did we correlate in-vessel spreading time
6 constants with ex-vessel spreading time constants.

7 MEMBER REMPE: Yeah. That was part of my
8 question too. Is there any - I mean, I think it depends
9 on composition and my engineering judgment a little bit
10 too and it seems like that there ought to be some
11 relationship.

12 But, again, I think you are basically
13 running the code and saying yeah, that's a reasonable
14 value still too is what -

15 DR. GAUNTT: Yeah. It's actually an area
16 where we're looking at some code improvements with
17 Argonne code called MELTSPREAD.

18 MEMBER REMPE: Ex-vessel, not in-vessel.

19 DR. GAUNTT: That's ex-vessel. So Kyle,
20 do you have anything else to say about the ex-vessel?

21 MR. ROSS: Well, the - so those parameters
22 aren't especially to the variables for in-vessel.
23 They're - they are parametric variables in the - in the
24 logic that spreads material on - across the concrete
25 floor that isn't especially mechanistic actually.

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1 DR. GAUNTT: Kyle, help us here. It does
2 consider temperature and debris height, right?

3 MR. ROSS: Yes, that's right. Those are -
4 those are the two variables that it worries about.

5 DR. GAUNTT: So if it's a big pile it's
6 going to tend to want to spread out and the hotter it is
7 it's going to tend to want to spread out?

8 MR. ROSS: That's right.

9 MEMBER REMPE: I guess - again, it's been
10 ages since I've read this and you're right, it does have
11 the - it does relate to temperature in the writeup. But
12 it wasn't clear to me - there aren't any equations and
13 I think that's still true.

14 There aren't any equations - it's just
15 somewhere is - if I went to the manual would it say okay
16 -

17 DR. GAUNTT: Well, there is an equation -
18 I'm sure Kyle can help us out here - but it's not looking
19 at the composition, for example.

20 MEMBER REMPE: But it does look at the
21 temperature and the height and -

22 DR. GAUNTT: Does look at the temperature
23 and the head height that would drive spreading.

24 MR. ROSS: Yeah, that's right. And I don't
25 - I don't recall that we've ever tried to put the

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1 relationships that are in the model to the document.

2 MEMBER REMPE: Oh, so this isn't a user
3 input entirely. It's something that - it's some sort of
4 correlation in the code that looks at the temperature and
5 the height and then it picks a value somehow or other?

6 MR. ROSS: No. It actually is a user
7 specified from end to end.

8 MEMBER REMPE: But the user knows to look
9 at the temperature?

10 (Simultaneous speaking)

11 MR. ROSS: -- by MELCOR.

12 DR. GHOSH: But in terms of when it happens
13 it also depends on the height and temperature, right?

14 MR. ROSS: That's right. MELCOR is coming
15 up with the height of the debris. That's right.

16 MEMBER REMPE: I'm sorry. I'm getting a
17 little confused. So you put in a user input value for
18 the radial debris relocation time.

19 MELCOR has some sort of correlations then
20 and modifies it based on temperature and debris height.
21 Is that what I'm hearing? Or -

22 DR. GAUNTT: I think the in-vessel
23 spreading behavior is distinct -

24 MEMBER REMPE: Okay.

25 DR. GAUNTT: - from the ex-vessel.

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1 MEMBER REMPE: Okay. But for this
2 particular one does the code modify it based on the user
3 input based on -

4 DR. GAUNTT: In-vessel.

5 CHAIRMAN STETKAR: This is in-vessel.

6 MEMBER REMPE: Okay. Does the code modify
7 what - the user specifies this value normal - a point
8 estimate, right?

9 DR. GAUNTT: Right.

10 MEMBER REMPE: And then the code modifies
11 what's used based on the temperature of the debris?

12 DR. GAUNTT: Ex-vessel is different from
13 this.

14 MEMBER REMPE: Okay. So in-vessel the
15 code does nothing?

16 DR. GAUNTT: In-vessel the code uses that
17 time constant.

18 MEMBER REMPE: And that's all it uses?

19 DR. GAUNTT: And that's all it uses.

20 MEMBER REMPE: There's no modification? I
21 thought somehow or other - I guess I brought up the
22 ex-vessel so I got what I deserved. But I got confused
23 that there was -

24 DR. GAUNTT: Now you're challenging my
25 memory.

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1 MEMBER REMPE: Okay. But there's nothing
2 - the user has no values on temperature on this?

3 DR. GAUNTT: It won't move material, I
4 know, unless there's a difference in height in-vessel.

5 MEMBER REMPE: In-vessel.

6 DR. GAUNTT: So there's got to be a
7 difference in height, and then how quickly it levelizes
8 everything is determined by this time constant.

9 MEMBER REMPE: Okay.

10 DR. GAUNTT: And more than that I'd have to
11 dig in and, you know, try and tell you better what it does.

12 MEMBER REMPE: Okay.

13 MEMBER CORRADINI: So I'm late to the party
14 but so what test - if somebody said to you I'll buy off
15 on this, excuse my English, integral fudge factor what
16 test do you point to that says we fudged it so at least
17 the test is close to this?

18 DR. GAUNTT: I'm not aware of any test that
19 -

20 MEMBER CORRADINI: There's not even the old
21 - I thought long ago memory served me - you know, I'm
22 forgetting that I thought early in the development there
23 were two or three tests in ACRR that are about the only
24 tests about core melt progression. Am I remembering
25 wrong?

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1 DR. GAUNTT: Well, I probably did those
2 tests but I don't remember connecting them to this
3 parameter.

4 MEMBER CORRADINI: So this parameter was
5 born long after and wasn't used to model anything
6 historical to make sure that things were reasonable?

7 DR. GAUNTT: I think -

8 MEMBER CORRADINI: Do you see what I'm
9 getting at? This one I can imagine. I figure you guys
10 were talking about the in-vessel one. To me this is
11 really witchcraft, excuse my English, but engineering
12 witchcraft.

13 But in some sense it's a judgement and then
14 I'm just trying to figure out how you felt the judgement
15 was reasonable. Was it - I thought it was - eventually
16 you tried to at least do some sort of experimental or some
17 calculation relative to an experiment, I thought.

18 DR. GAUNTT: Memory is failing me.

19 MEMBER CORRADINI: Okay.

20 DR. GAUNTT: If there was.

21 MEMBER REMPE: Out of curiosity, did you
22 ever try and run the code with some value that was way
23 beyond and see what the code does?

24 DR. GAUNTT: Guys, help me here. I think
25 that - I don't know if recall this clearly or not. But

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1 I think there's some numerical stability issues that
2 might be associated with these numbers.

3 But I don't - the - like that 360 we've used
4 that for a very long time and I'd have to try and dig to
5 find out where - what the - what the reason for that was.

6 We have the liquid melt equilibrating
7 faster than, you know, a big pile of debris. So at least
8 that's kind of an intuitively correct order of things.
9 But after that I can't tell you.

10 MEMBER CORRADINI: I just couldn't
11 remember. I'm sorry.

12 CHAIRMAN STETKAR: Anything else on these?

13 MEMBER CORRADINI: Thank you.

14 CONSULTANT SHACK: There's a reference to
15 a Siemens report but I can't actually find a reference
16 to it. It just says Siemens uncovered sensitivities
17 here and explored ranges.

18 CHAIRMAN STETKAR: That's in what we
19 received last week.

20 DR. GAUNTT: Is that the - is that in
21 reference to the hot leg?

22 CONSULTANT SHACK: No, it's these time
23 constants.

24 DR. GAUNTT: Okay.

25 DR. GHOSH: I believe that's the -

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1 CONSULTANT SHACK: It blends LLNL for the
2 change in values.

3 DR. GAUNTT: Yeah, I think you're right
4 about that. I do recall that.

5 CONSULTANT SHACK: And then it - you know,
6 it says something about Siemens but then I tried to look
7 for some report that would connect these things and I
8 couldn't actually find a reference. But okay, nothing
9 comes off the top of your head?

10 DR. GAUNTT: Maybe we can find that. I'm
11 recalling the Siemens report. It predated our hydrogen
12 study 10 years ago or so.

13 CONSULTANT SHACK: Oh, so this is, again,
14 a ten-year-old kind of - I thought - it made it sound as
15 though it was something new.

16 CHAIRMAN STETKAR: Yeah, that's the way it
17 was presented in here and oh by the way there's some -

18 DR. GHOSH: Yeah. I think that's a
19 reference in the older hydrogen study. We can check that
20 data. I was trying to track that data too. We will -
21 we can track it down. Yeah.

22 CHAIRMAN STETKAR: Anything else? If not,
23 I'm going to recess for a break because it's probably
24 about time to do that and let's reconvene at 3:00 o'clock.

25 (Whereupon, the above-entitled meeting

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1 went off the record at 2:39:53 p.m. and resumed at 3:00:03
2 p.m.)

3 CHAIRMAN STETKAR: Okay. We're back in
4 session. Next.

5 DR. GHOSH: Yeah. So the next - to be
6 honest these are - these next two parameters are among
7 the least interesting parameters in the study for the
8 following reason.

9 We wanted - so the reason we were looking
10 at this in the first place is because when the railroad
11 doors are open you get this chimney effect that can reduce
12 the residence time that you have for the - your source
13 terms.

14 So we wanted to see that, you know, if you
15 have a different fraction than what was assumed in the
16 SOARCA study would that make a difference.

17 So we sampled a fraction, an open fraction,
18 for both the inner doors and the outer doors when they
19 blow open, which is the majority of the time, to see if
20 it has an effect and what we found is that if the doors
21 are open it really doesn't matter how much they are open.
22 So these parameters showed up as not important at all.

23 However, what is important is whether or not
24 they blow open. So what we ended up doing is we created
25 a - when we did our regression analyses we created a

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1 paramater that was just an indicator for each realization
2 of whether or not the doors blew open.

3 We assigned it a new name and, you know,
4 that's based in MELCOR on the pressure calculation.
5 Once you reach a particular pressure your doors blow
6 open.

7 After that it really didn't matter what the
8 open fraction was because as long as they blow open you
9 get some chimney effect and that's what really matters
10 is whether or not you blow open the doors.

11 CHAIRMAN STETKAR: What you're saying is
12 this distribution doesn't make any difference at all?

13 DR. GHOSH: Yeah.

14 CHAIRMAN STETKAR: Okay.

15 DR. GHOSH: So both of these. So slide 41
16 is the inner doors. Oh, actually I didn't even bother
17 producing the two because they had an identical
18 distribution and they really don't show up as important.

19 So what was a little bit important was this
20 indicator variable for whether or not the doors actually
21 blow open. But then the fraction that they're open
22 really doesn't matter.

23 CHAIRMAN STETKAR: But the indicator
24 variable wasn't input. That's just - that came out of
25 the calculation?

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1 DR. GHOSH: That's right. Yeah. And
2 that's based on the pressure that -

3 CHAIRMAN STETKAR: Yeah.

4 DR. GHOSH: And most of the time they do
5 blow open. Okay. So the next one is the hydrogen
6 ignition criteria. So I don't know if you had a question
7 about this one but this one we struggled a little bit
8 about what more to say.

9 This is - you know, we revisited what we had
10 written in chapter four and we weren't sure what more to
11 say about this. So I guess I'll ask, you know, what the
12 question might be with regard to this parameter.

13 MEMBER REMPE: Well, I'll speak up. I'm
14 not sure if anyone else has questions but, again, I'm
15 looking in chapter four from an April version of the
16 report. Is that an old version of it?

17 DR. GHOSH: It is an old version.

18 MEMBER REMPE: I'm not - I'll have to dig
19 around and find a more recent one. But in this when you
20 say where flammable sometimes, like in that legend, does
21 that mean that if you have a case where you have too high
22 a concentration of steam that you don't consider using
23 this parameter?

24 Is that true that you look and see if you
25 have that flammability criteria met and then you apply

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1 this?

2 DR. GAUNTT: The criterion is there but if
3 it's - if it's outside of the flammable, you know, the
4 curve I'm talking about, that triangle -

5 MEMBER REMPE: The triangle is what I'm
6 thinking about too, yeah.

7 DR. GAUNTT: Right. So if it's steam
8 inerted or inadequate oxygen or something like that then
9 you don't - a burn is not valid. But otherwise if it -
10 otherwise if it is flammable and you exceed this
11 concentration then it will assume a burn happens.

12 MEMBER REMPE: Okay. That answers a lot of
13 my questions and I looked at this paragraph again last
14 night in the section four and, again, I think it's just
15 maybe the way I'm reading it or whoever wrote it but it
16 didn't clearly say that and that would have answered my
17 questions.

18 DR. GHOSH: I think on that one we did try
19 to add some explanation. It may not still get there.
20 We'll revisit it. But it's on Page 425 it starts with
21 the new - the newer version before the July meeting of
22 this year.

23 MEMBER REMPE: Let me pull up what I have
24 from the July's subcommittee meeting too. Sometimes I
25 get confused on the versions too. But anyway as long as

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1 you're taking care of it that's all that's important.

2 DR. GHOSH: Yeah. We'll revisit that to
3 see where -

4 MEMBER REMPE: I'm not sure which section
5 in there.

6 MEMBER CORRADINI: 518 Page 1.

7 DR. GHOSH: Okay. That's the - that should
8 be the latest. I don't think our - what we gave you a
9 -

10 MEMBER CORRADINI: It doesn't have a date
11 on the one but -

12 DR. GHOSH: Yeah. The 518 is the latest
13 version that we did.

14 MEMBER CORRADINI: There's no date listed.
15 It just says date - transcript completed date is -

16 DR. GHOSH: But that's the draft, yeah,
17 that we provided you.

18 MEMBER CORRADINI: Okay.

19 DR. GHOSH: Okay. I guess if there are no
20 other questions on that one we will move to the particle
21 density, which is the RHONOM on slide 43.

22 So here I think we had some discussion on
23 chapter four that perhaps elaborated on a little bit in
24 the writeup that we sent you last week and I guess I'll
25 defer again to the - unless, Randy, you want to start with

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1 anything I'll defer to the committee members in terms of
2 what the specific questions might be on that one.

3 DR. GAUNTT: Opening up the question.

4 CHAIRMAN STETKAR: The only - and my
5 original observation on this is kind of displayed. I
6 understand I think the bases for the upper and lower
7 bounds on this curve - you know, why it's set at whatever
8 it is, 870 and whatever the upper bound is - 4,037.

9 The original SOARCA value of that triangle
10 there's about a 97 percent probability that the density
11 is greater than the best estimate value and that's what
12 it was called in the original SOARCA study. Is that -
13 is this now the current state of knowledge?

14 DR. GAUNTT: I would say my thinking on this
15 has shifted and we should be using more like a most
16 probable value here and that - on the 1,000, you know,
17 kind of reflects a very wet aerosol and I guess my
18 thinking is kind of changing on this. But -

19 CONSULTANT SHACK: Okay. So you would -
20 represents the -

21 DR. GAUNTT: I think the red line
22 represents the spectrum of real aerosol.

23 CHAIRMAN STETKAR: Okay. This is
24 something I know nothing about. I can look at curves.
25 I can read words. But I don't know anything about

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1 aerosols or aerosol behavior. How much - what's the
2 overall effect of the particle density in terms of -

3 DR. GAUNTT: Well, the particle density
4 factors into a lot of the deposition rates such as, you
5 know, of course, gravitational settling, that along with
6 the apparent aerodynamic size of the particle.

7 So it will affect the - it will affect
8 deposition rates. There's a lot of different ones.
9 Diffusiophoresis, thermophoresis - all of these are
10 impacted by these a little.

11 MEMBER BLEY: This was the distribution
12 used in this.

13 DR. GAUNTT: The red distribution was used
14 in the uncertainty.

15 MEMBER BLEY: Did this one have a big impact
16 on the results, SOARCA uncertainty distribution?

17 DR. GHOSH: This did not show up as, you
18 know, one of the top, you know, three or four. But -

19 MEMBER BLEY: That seems surprising.

20 CHAIRMAN STETKAR: I mean, this
21 effectively says you ought to be getting a lot more
22 deposition than the original study showed.

23 DR. GHOSH: But, Doug or Kyle, I have a
24 vague recollection that this showed up as more important
25 than maybe, for example, the subset of the main steam line

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1 rupture scenarios.

2 Sorry. I guess, Doug or Kyle, if you recall
3 whether this showed up as important in one of the
4 subscenarios when we did the regression analyses for
5 that. Do you -

6 MR. OSBORNE: Yeah, I kind of remember the
7 same thing but I don't remember exactly where.

8 CHAIRMAN STETKAR: I mean, it certainly was
9 not highlighted in the NUREG in the report as something
10 that was - that was an important parameter.

11 DR. GHOSH: Right. That's right. Yeah.
12 So we had done essentially four sets of regression
13 analyses for the source term results.

14 We did one on the composite distribution of
15 source terms which included all four - sorry, all three
16 subscenarios. And then we also looked at each
17 subscenario to see whether there are particular
18 parameters that contributed more to, you know, that
19 subscenario.

20 And we can get back to you on this but the
21 - so the RHONOM did not show up as important for the full
22 set but I have some vague recollection that for perhaps
23 the - for example, the main steam line rupture subset the
24 regression analyses showed it as becoming important for
25 that subset of scenarios.

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1 MR. OSBORNE: Yeah, it was - it was
2 meaningful as I remember in one of them. But it wasn't
3 - it wasn't top three.

4 DR. GHOSH: Right. Right.

5 MEMBER BLEY: This gives me a chance to go
6 back to the thing that still bothers me from the first
7 meeting and that general statement that the staff,
8 despite the fact that from what you're telling us, in your
9 opinion all of these distributions represented your best
10 judgment of - I'll use the Shack approach words best I
11 can - your best judgment of the state of uncertainty of
12 the technical community about these parameters.

13 And despite that you had said you had more
14 confidence in the point estimates of the original report,
15 which still leaves me a bit confused, and given - I don't
16 know if Randy's change of opinion on this is reflected
17 by others, this is just one parameter out of a big group.

18 But it would strike me that things like this
19 if in fact these do represent your best judgments of the
20 judgment of the technical community you still hang on to
21 that thing that you like your point estimates better and
22 you believe them more, and if so try to teach me why you
23 think that.

24 DR. GAUNTT: Do you want a shot at that,
25 Tina?

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1 DR. GHOSH: Well, okay. So this one - this
2 one is a funny one. We tried to reflect the judgment that
3 at the time that we kind of fixed this distribution and
4 I think Randy's thinking may have evolved since then that
5 this was probably more than a year ago at this point -
6 that at the time we were still thinking a thousand was
7 the most likely.

8 However, the values lower than that were not
9 very likely and they could go as high as, you know, the
10 4,037 or whatever that upper end is. And so we used a
11 similar thinking in constructing the distribution and we
12 made a triangular distribution putting the peak at 1,000.

13 But because the distribution is completely
14 skewed in one direction, it doesn't show up very well in
15 our composite distribution - that that triangle is at a
16 pretty low percentile because even though that's the most
17 likely value because there are so many values -

18 MEMBER BLEY: I'm going to stop you right
19 there. Most likely has a distinct meaning, right?

20 DR. GHOSH: Yeah. If you look at the PDF
21 it's where the PDF peaks. Because the PDF -

22 MEMBER BLEY: A PDF for this one doesn't
23 peak way down there.

24 DR. GHOSH: We put the - well -

25 MEMBER BLEY: In fact, that's the 10th

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1 percentile.

2 DR. GHOSH: Right, because we plotted - we
3 plotted the CDF here so as I'm saying it's - the
4 distribution is so wide to the right that it's very hard
5 to see the effect.

6 But the PDF has a very tiny peak at 1,000.
7 You can see that the curve is actually steepest through
8 that 1,000 mark. But, again, because it's spread so far
9 to the right that this translated to a CDF percentile of
10 a very small number. And, you know, perhaps we could
11 have put a little bit more thought into making the shape,
12 you know, fit a little bit better.

13 But again, you know, we had the thinking
14 that we think that's the - still think that's the most
15 likely value but it could go up as high as 4,000.

16 CHAIRMAN STETKAR: When you say most likely
17 value what is the most likely value? There's a 50
18 percent probability that - according to this there's a
19 50 percent probability that the density is greater than
20 about - and I'm not going to try to get real precise here
21 - maybe about 2,200 to 2,400.

22 DR. GHOSH: I guess when I say most likely
23 I'm strictly speaking of just the mode of the
24 distribution. So it's not connected to the percentile.
25 So that's - I know, that's a little bit -

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1 CHAIRMAN STETKAR: I think most people would
2 think most likely would be the expected value.

3 MEMBER BLEY: Well, most - he's right but
4 that can't be the mode.

5 CHAIRMAN STETKAR: That can't be the mode.

6 MEMBER BLEY: Or you didn't translate it
7 right I think coming off as for -

8 DR. GHOSH: I think the translate - right.
9 I think in a way something was lost in translation but
10 perhaps at the time was already reflecting our judgment
11 that we were comfortable with the fact that we may in the
12 future want to use values that are larger than a 1,000.

13 I think this was in the middle of some
14 transitional thinking. But, you know, in the end it does
15 have some effect but it didn't show up as one of the most
16 important parameters with respect to driving the
17 results.

18 CHAIRMAN STETKAR: But going back to what
19 Dennis asked originally is that I hear you saying that
20 the triangle is what you feel to be the most likely value.

21 DR. GHOSH: Actually, I would say -

22 CHAIRMAN STETKAR: And what is the - what
23 from this distribution if I were a betting person what
24 is the expected value?

25 What value would I bet my life savings on

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1 from this distribution? And it kind of looks like it's,
2 I don't know, 2,500, 2,600 - quite - like more than twice.

3 DR. GAUNTT: I would say so. This is a
4 learning process for us and it might raise the
5 philosophical question should we even do deterministic
6 point analyses.

7 Should we - should we do the uncertainty
8 study, you know, right up front and it's just - you know,
9 it's kind of where we came from.

10 All of these best estimate values that we've
11 - that we used in the, you know, point estimates of the
12 analyses are the result of cumulative study over the past
13 20 years, looking at various experiments, and we don't
14 wildly change our numbers from, you know, from assessment
15 case to assessment case.

16 And so when we launched out to do the
17 baseline SOARCA case that was sort of the sum cumulative
18 knowledge of all the, you know, tests we've looked at -
19 aerosol tests, the fuel meltdown tests, things like that.

20 Then we come along and start thinking about
21 putting uncertainty distributions and some of our
22 thinking kind of shifts as we put more thought into what's
23 reasonable here. And this is one of the cases that kind
24 of stands out.

25 It probably has little effect on the outcome

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1 because many of these aerosol mechanics processes are
2 happening on real short time frames.

3 So we can be off on deposition rates and not
4 really change the - change the net results. I'm
5 suspecting that's why it's not - doesn't come out as a
6 strong parameter.

7 MEMBER CORRADINI: It didn't come out
8 anywhere near. It was one of the weakest of all the ones
9 we investigated, if I remember correctly from the paper.

10 DR. GAUNTT: So that's probably indicative
11 - you know, these processes like agglomeration and all
12 they happen on very, very short time frames and the
13 deposition process is similarly.

14 So it could be that having a difference in
15 the terminal velocity inside the containment is not going
16 to be a big deal.

17 MEMBER BLEY: I guess - I'll say this now
18 so I don't say it at the end - if in fact you still have
19 more confidence in your point estimate calculation than
20 in these results then the learning process hasn't made
21 its way far enough because you haven't come up with the
22 distributions that you really believe.

23 It's just got to be true. It can't be any
24 other way, because this is - you're really trying to
25 express what you think the true state is and yet you're

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1 saying this isn't it when you're all done.

2 So something has not been learned yet or,
3 you know, if you went back through it again maybe you'd
4 have different distributions and maybe - I don't know how
5 you did the - well, you just said you picked three points
6 and drew a triangular but I don't think you quite
7 translated your triangular into the density function and
8 to this cumulative thing right. So I think something
9 went funny somewhere in the process.

10 DR. GHOSH: Yeah. I mean, just glancing at
11 the curve I think probably maybe a log triangular would
12 have been more appropriate than a triangular because it
13 is skewed so far to the right.

14 MEMBER BLEY: That density function - the
15 area under the curve has got to be one so maybe you didn't
16 do that or something wasn't right if you're still saying
17 you believe that more than you believe the results of this
18 regression.

19 DR. GHOSH: Well, I think that - yeah, I
20 mean, in this case it's so hard to even talk about the
21 most likely value because the mode is like this big
22 because the area under the curve has to be one and we've
23 drawn it out so far to the right.

24 I mean, then - I mean, you think one is right
25 to ask, you know, how meaningful is it that it's the most

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1 likely if it's got a probability of 3 percent or something
2 once you draw the entire curve.

3 MEMBER BLEY: Well, that's what I'm saying.
4 Something wasn't done right.

5 DR. GHOSH: Yeah.

6 MEMBER BLEY: Maybe you needed something
7 approaching a delta function at the beginning to put a
8 large hunk of the probability in. I don't know if that's
9 happened on other ones but -

10 DR. GHOSH: Right.

11 MEMBER BLEY: - if in fact at the end we're
12 still saying we believe our point estimates better
13 somewhere in this process we didn't do it right or we -
14 equivalently we don't really believe these distributions
15 or if we showed them as densities we don't really believe
16 these densities.

17 That isn't what we were trying to say. So
18 somehow we never went back and looked at those density
19 functions and said is this really what I believe about
20 this characteristic given I have that original point
21 estimate that I believe in to some extent as being the
22 most likely.

23 CHAIRMAN STETKAR: The reason I brought
24 this up from - kind of repeating the same sort of
25 knowledge statement is -

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1 MR. OSBORNE: You know, there was another
2 influence here I remember us worrying about. It was the
3 shape factor of the particle as well as the density. I
4 don't recall if it influenced the shape of the
5 distribution that we - that we chose or not.

6 MEMBER BLEY: Yeah, we're talking about
7 this one particular one. But the comment is really much
8 more general. There's something in the whole set of them
9 that, you know, must not be showing us your real state
10 of knowledge - your real belief.

11 CHAIRMAN STETKAR: Thank you. That's the
12 whole reason. I don't care about particle density nor
13 do I know anything about aerosols.

14 The whole reason I highlighted this in my
15 comments was exactly what Dennis was saying is that this
16 picture illustrates what seems to be a radical change in
17 the engineering community's understanding of this
18 particular issue compared to the original triangle.

19 Now, if that's really true that's good. I
20 mean, if the engineering community has thought about this
21 process and said yes, the shape of this distribution is
22 sort of what we wanted, the bounds on the - there's real
23 reasons for that, that's really good.

24 Dennis' comment is you have to have done
25 that first because that's the real essence of what

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1 determines ultimately, you know, from a numerical
2 perspective your so-called best estimate. It kind of
3 falls out of that process.

4 On the other hand, if this red curve isn't
5 the engineering community's best estimate it's not at all
6 clear what we're doing in this uncertainty analysis.

7 DR. GAUNTT: I think the red curve
8 represents a realistic estimate of the distribution and
9 I think - I think also it reflects a shift in our view
10 on how to do this.

11 CHAIRMAN STETKAR: So I guess in principle
12 asking if you ever - if you ever get the funding in time
13 and everything to go ahead with the Surry have you thought
14 about doing the uncertainty analysis first for that?

15 DR. GAUNTT: Well, we've done the Surry
16 Point analyses, of course - the overview of ice
17 containment.

18 DR. GHOSH: That's right. The ice
19 condenser. Yeah, and you know -

20 CHAIRMAN STETKAR: Or at least not get so
21 married into that point estimate that -

22 DR. GHOSH: Right. Right. Right.

23 CHAIRMAN STETKAR: - you try to rationalize
24 why it's still your highest confidence result or whatever
25 you want - however you want to characterize it.

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1 DR. GHOSH: I guess having heard this
2 conversation now we may put a note in the report for this
3 parameter in terms of our progressed thinking on this.
4 You know, well -

5 CHAIRMAN STETKAR: It's not just this one
6 though. I mean, it's -

7 DR. GHOSH: Yeah. Yeah. I understand.
8 Yeah.

9 CHAIRMAN STETKAR: - it's in effect all of
10 them.

11 DR. GHOSH: Right.

12 DR. GAUNTT: This one does kind of stand
13 out.

14 CHAIRMAN STETKAR: Oh, it does. That's
15 what I - that's the only reason I picked this one is it's
16 so different.

17 MEMBER SCHULTZ: But there are others that
18 you could certainly comment on in the same vein which is
19 were we to do this again we would have done the point
20 estimate evaluation differently because of what we
21 learned in the uncertainty analysis.

22 I think you're comfortable with this even
23 as it is because it didn't show up to be very important.

24 DR. GHOSH: Right.

25 MEMBER SCHULTZ: To really be important and

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1 therefore okay, we didn't change it.

2 DR. GHOSH: Right. Right.

3 MEMBER SCHULTZ: But we don't feel we have
4 a strong need to change it.

5 CHAIRMAN STETKAR: I mean, you know, if I
6 can play devil's advocate if I go back to that relief
7 valve thing, forget - suppose that Peach Bottom had never
8 put that 3.7 times 10 to the minus three in their IPE
9 study.

10 What would you have had? Well, you would
11 have had those two NUREGs that had pretty low numbers and
12 apparently input from experts who said well, those don't
13 feel very good to us - that we think that ought to be
14 shifted to the right to some extent.

15 In that case, that change of thinking, you
16 know, would have shown up as being something important
17 and, you know, it might have represented a legitimate,
18 you know, rethinking of the process, you know, through
19 the process of doing the uncertainty analysis.

20 In some cases, you're just very fortunate
21 that this one didn't show up as important. If it did then
22 a lot more scrutiny of this distribution.

23 MEMBER SCHULTZ: In the case you mentioned,
24 it was demonstrated that we don't - we don't understand
25 the rationale that was - that seemed apparently behind

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1 the decisions that were made.

2 Here we understand the decision or
3 understand the rationale which would support a change
4 based on what Randy has put forward. But we also
5 understand that to move forward with that change for our
6 current work is not that critical based on the results
7 of your sensitivity study.

8 But certainly worth capturing - as you said,
9 worth capturing is a list of things, you know, what would
10 you do differently in the next evaluation.

11 Here's a parameter we would treat
12 differently and we've learned that based upon the work
13 that we have done. It's definitely worth capturing.

14 DR. GHOSH: Okay. So the next one is the
15 fuel failure criterion and we have on slide 44 the weights
16 to the three alternate models.

17 And slide 45 is the three models, and Randy
18 was talking a little bit about this before. These are
19 the time at temperature of models in terms of when the
20 fuel breaks down.

21 MEMBER CORRADINI: But if I - if I might
22 add, in some sense all three are connected. They're not
23 independent.

24 DR. GHOSH: Right. Right. It's the same
25 model. It's a matter of changing the temperature and the

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1 time - that you need to be at that temperature so that
2 that's what the curves are. The time is on the Y axis
3 and the temperature on the X axis.

4 DR. GAUNTT: What it says is we're 80
5 percent confident in the blue - the SOARCA value, right?

6 DR. GHOSH: The blue one in the middle,
7 yeah.

8 DR. GAUNTT: The blue one in the middle
9 there. So we're 80 percent confident that that's -

10 CHAIRMAN STETKAR: Oh, I see.

11 DR. GAUNTT: - that's the described well,
12 the loss of geometry in the fuel - in the fuel bundle when
13 the fuel changes from raw to - you know, to debris like
14 geometry.

15 And how to read that curve what it says is
16 that if you get up to, say, 2,100 K you have to - you have
17 to get up to 2,100 K which is getting really close to the
18 melting point of the zircaloy cladding before the fuel
19 rods are going to be subjected to loss of geometry.

20 When you hit 2,100 K you're not quite
21 melting yet and so we're saying that you can sit there
22 for 10 hours but the fuel is heating up, still heating
23 up.

24 So it's going to get hotter and you see the
25 lifetime to failure is decreasing. So it's sort of a

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1 Larson-Miller kind of lifetime rule. I mean, you are
2 going to use this in a linear damage way that -

3 CONSULTANT SHACK: You're going to
4 integrate up as -

5 DR. GAUNTT: Integrate up. Exactly.
6 Right. That's kind of how this works. And so then the
7 other two curves we put lesser confidence in but just to
8 encompass the possible range of uncertainty in how
9 quickly can the core lose geometry.

10 MEMBER CORRADINI: And when you say - let's
11 just stick with one curve, if I understand it correctly.
12 If we take the blue curve - to get back to Bill's question
13 when you integrate up I don't - this is not - this is a
14 ring value.

15 There is some volume that's being watched.
16 All the volumes are being watched. When the volume in
17 that ring - in that some axial and radial ring location
18 gets at 2,100 it says aha, I'm at a point now where I'm
19 hot enough that it goes from infinity to something
20 finite.

21 And as it continues to heat up it
22 essentially then watches and has a counter and as time
23 marches along at some point the counter compared to this
24 parameter cross and bing, what happens?

25 DR. GAUNTT: And then it will - it will

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1 convert from intact rods into debris. In the meantime,
2 generally at this point in time fuel heat up rates are
3 like 10 to 15 K per second.

4 So you're moving through this pretty
5 quickly and I - there are some other simultaneous
6 processes going on. The fuel break out that's another
7 - the melt breakout reg that's another kind of connected
8 parameter.

9 MEMBER REMPE: When I was reading the
10 writeup for this - by the way, the hydrogen writeup didn't
11 change so please do correct that -

12 DR. GHOSH: Okay. Yeah. Thank you.

13 MEMBER REMPE: They said that this was
14 based upon these results from Phebus at 1 and 2 - FPT 1
15 and 2?

16 DR. GAUNTT: I would just say the
17 collective observation what do we see from the Phebus
18 tests.

19 MEMBER REMPE: Okay. So I guess when I was
20 reading it I was going well, I wonder how different it
21 is from the prior tests and if it was somehow or other
22 collectively related to the test is there some way you
23 can plot some data or something to show that yeah, this
24 is kind of bounding what we saw or something?

25 I mean, it was just - I don't know. When

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1 I read it I wasn't - I didn't feel confident that the
2 experts really - what they'd done. I couldn't figure it
3 out.

4 DR. GAUNTT: You know, as I alluded to
5 earlier, the reason for going to this lifetime model was
6 to address a kind of a cliff edge effect we were seeing
7 in the code calculation and prior to this we had all our
8 code models calibrated against Phebus.

9 The general trend in Phebus is compared to
10 fresh fuel test the radiated fuel tends to - it looks like
11 it comes apart at a little bit lower temperature. It's
12 hard to tell from these in-pile tests because all you have
13 are temperature measurements. You don't really have a
14 view, you know, of what's happening.

15 But, you know, overall we kind of capture
16 the signature - temperature signatures of the test and
17 end of test final configuration, and like I say prior to
18 this lifetime model we would use just simply a
19 temperature threshold and, for example, we would - we
20 would allow molten zirconium to escape the oxide cladding
21 at some number, 2,400 K or something like that.

22 We would allow the fuel to collapse from raw
23 geometry at, say, 2,600 K but then what we would find is
24 we would occasionally run two very similar calculations,
25 one that would just come right under 2,600 and one that

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1 would just go right over 2,600, and they would go to, you
2 know, a case where we would have 24 - you know,
3 2,550-degree fuel and it's sitting there for hours and
4 hours and hours and hours, and we looked at that said no,
5 that just - that's not right. It can't be.

6 MEMBER REMPE: But if it does have data that
7 helps you come up with this lifetime rule and so that
8 implies that you've got data that shows that -

9 DR. GAUNTT: Joy, I'd say it's kind of
10 indirect data because like I say you cannot look into
11 these fuel experiments and see exactly when is -

12 MEMBER REMPE: But you could see
13 temperature behavior and peak temperature at various
14 locations and you would see in-state and say okay, it
15 failed and you have time at temperature as a function of
16 time.

17 It depends on how well instrumented your
18 test was, obviously. But I just wondered do you really
19 have data that you can compare with this or this is just
20 something to overcome the differences in the code?

21 DR. GAUNTT: The blue curve we're using
22 here produces results that are not inconsistent with
23 everything we're seeing in these in-pile tests. There's
24 a few other data points and some of the irradiated small
25 pin tests that are done in Vercor and so forth they do

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1 - they do have I believe some radiography that shows the
2 fuel collapsing at a certain temperature. So we know
3 that the irradiated fuel loses its geometry sooner than
4 fresh fuel.

5 These are all, you know, collective
6 integral information that we've gathered over the years
7 and the blue curve is kind of our best understanding of
8 what we're seeing in these experiments.

9 MEMBER CORRADINI: So it's a bit off topic,
10 Mr. Chairman, but so what does MAP do?

11 DR. GAUNTT: I'm sorry?

12 MEMBER CORRADINI: What does MAP do? They
13 don't have this time and temperature. They have the
14 previous - they have this previous - they have this other
15 methodology in terms of the temperature. When you hit
16 a temperature some event occurs.

17 DR. GAUNTT: I think - you know, they don't
18 let you look inside MAP very freely but I think they do
19 have a time at temperature.

20 MEMBER CORRADINI: Okay.

21 DR. GAUNTT: I believe they do.

22 MEMBER CORRADINI: Potentially different
23 but a similar logic.

24 DR. GAUNTT: Right. Because I'm sure
25 they've run into the same numerical oddities that we've

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1 seen.

2 CONSULTANT SHACK: So that's the trouble
3 with any magic temperature criterion and you said when
4 you -

5 DR. GAUNTT: Right.

6 CONSULTANT SHACK: - get yourself in a case
7 where -

8 DR. GAUNTT: Right. I mean, that was the
9 - that was the - you know, we ran a BWR case where the
10 channel boxes should have failed at 2,600. I don't know
11 what the number was, and the calculation hovered up at
12 2,580 or something for hours, and this kind of changed
13 our thinking.

14 We need a more realistic behaving treatment
15 that still kind of reflects the general temperatures when
16 these things begin to come apart. Some of the other
17 values on here, you know, on the lower bound here, 2,100
18 K, that's kind of dictated by when zircaloy melts.

19 The 2,700 K is an upper bound because of the
20 eutactic - it's not a eutactic, it's a paratactic or Dana
21 will tell you between zirconium oxide and uranium oxide.

22 So if you melted all the metal and you were
23 just left with oxide shards up there then you would have
24 this material interaction between UO2 and ZrO2 and you
25 could not have anything solid above, say, 2,700 K. So

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1 there's kind of physics rationale for the shape of this
2 and some timing implications as well.

3 CHAIRMAN STETKAR: Anything more on this
4 one?

5 MEMBER REMPE: The discussion helps a lot.
6 Again, it's probably less tied to the data as perhaps one
7 could infer from the text is what I - I guess I might take
8 away from it.

9 DR. GAUNTT: We feel pretty happy about
10 that advantage in the modeling. It's - the code behaves
11 a lot better now.

12 DR. GHOSH: Okay. The next one is the
13 molten clad drainage rate. Once again, I'll turn it over
14 for questions on this one, specific questions on this.

15 CHAIRMAN STETKAR: The only reason I - I
16 mean, I added this to the list and this is just another
17 example where the uncertainty distribution is measurably
18 different from where that triangle sits - again, I don't
19 know anything about the physics.

20 So I was just looking at this one and the
21 particle density were two areas I highlighted where the
22 results of the uncertainty analysis, you know, thinking
23 about the phenomenon seemed to provide substantially
24 different - at least best estimate mean, you know,
25 expected value, whatever you want to call that -

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1 DR. GAUNTT: I think there's probably some
2 of that in the value that we used before we sat down and
3 ironed out what do we think these really are. I believe
4 that the value that we used here - so what this parameter
5 is is when you - when you're heating up in that oxidation
6 transient and this is like between 1,800 K, 1,700 K and
7 beyond, you're going along at a 10 or 15 K per second clip.
8 You're soaring through the melting temperature of
9 zircaloy but you've got this nice little oxide scale on
10 the outside that retains the melt.

11 So you've got melt sitting behind this
12 oxide. And then we reach a - we didn't talk about it but
13 we reach a break out, you know, criteria and what this
14 - what this does this spells out how quickly does that
15 molten material drain from behind this cladding, and my
16 recall on this blue point here is that this is the value
17 that sort of replicates relocation front movement that
18 we have measured from experiments like CORA where they
19 had video and you could see this melt front moving down.

20 And I think this is where - this is where
21 that particular point came from. I don't remember about
22 the shape of this distribution.

23 DR. GHOSH: Yeah. The - I guess what we
24 added to what was in the draft report last week is that
25 from the same CORA experiments that some of the free

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1 falling droplets moved really rapidly.

2 So I think this was an attempt to kind of
3 capture the range of possibility with respect to maybe
4 the average movement. But it could fall a lot quicker.
5 So that -

6 CHAIRMAN STETKAR: It's a lot clearer that
7 - the triangle looks like it's a lot closer to the mode
8 of this distribution than, for example, that other.

9 DR. GHOSH: Right. So I think here we
10 assigned something like a log triangular to put the peak
11 at the -

12 CHAIRMAN STETKAR: Right. That would do
13 it.

14 DR. GHOSH: And it's not - you know, it's
15 not the median or but, you know, we were comfortable that,
16 again, once you - when you put all these distributions
17 into the Monte Carlo simulation the exact shape of the
18 distribution in the end doesn't influence -

19 CHAIRMAN STETKAR: Not too much.

20 DR. GHOSH: - you know, that much, yeah.
21 So you didn't worry too much beyond that.

22 CHAIRMAN STETKAR: Anything more on this
23 one? I want to see if we can fit most of the stuff in
24 before we lose Dr. Corradini.

25 DR. GHOSH: So then I think on the agenda

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1 we had a separate item for other issues but I guess our
2 understanding from the identified issues was that most
3 of these were MELCOR issues. So I lumped it at the end
4 of this discussion.

5 I think we've talked a little bit about what
6 we mean by surrogate parameters. I didn't know if there
7 was additional questions on that - if we're good at that.

8 We added some additional writeup on the
9 lower head penetration failure modeling that we did.
10 Were there any questions on that?

11 MEMBER REMPE: I have a question. Okay.
12 You talked about - okay. So and what I read this time
13 I believe, and I'm paraphrasing, you said well, the
14 diameter of the instrumentation tube isn't much
15 different than the diameter of the drain line. So we
16 kind of think we're close. But we had - and I don't think
17 it was in this uncertainty report.

18 It was in - you did the sensitivities at the
19 last meeting and I had some issues with some of the
20 characterization then because there's a big difference
21 because there's no in-vessel structures with the drain
22 line.

23 In addition, they're quite different
24 because they don't have instruments within them. So
25 saying yeah, the diameter is about the same well, that's

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1 not quite true.

2 The instrumentation too has a bunch of stuff
3 in it and so the drain line can open past the flare out
4 of the lower head. But probably what got me most excited
5 for the last meeting was that somebody referred to well,
6 at TMI they saw that the melt had stopped in the
7 instrumentation tube so we don't think it's that
8 important.

9 And, again, the - if you look at those
10 pictures of the melt we found at the TMI nozzles it was
11 around the in-tube structures - the instrumentation. So
12 there was annulus where the melt could go through.

13 It was a very small diameter - it dribbles
14 down in those tubes. So that saying that it stopped at
15 TMI is irrelevant for this drain line plus the drain line
16 has - is 105, 106 steel which can very much lower
17 temperature. So I still have some issues with that.
18 And so I -

19 DR. GHOSH: The only reason we brought up
20 that TMI freezing phenomenon is because we don't model
21 that in MELCOR.

22 So MELCOR allows the material to keep going,
23 and I think that's the only reason we put that note in
24 the report and probably the presentation slides as well
25 is that - the melt - the model for the lower head

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1 penetrations right now that we have available to us in
2 MELCOR doesn't model any kind of freezing. So it's just
3 to make - to make that clear.

4 MEMBER REMPE: And does it - when you model
5 the instrumentation tube did you assume it was totally
6 an open annulus?

7 DR. GAUNTT: I would say we're not using the
8 - we're not using the penetration failure modeling. And
9 the penetration fail modeling that's in MELCOR is really
10 critical and I'll let - I'll let Kyle explain some more.

11 But I have a different interpretation that
12 I don't know if it made it into the writeup or the
13 explanation. But in these accidents in the BWR what
14 we're seeing in MELCOR, and this is the world according
15 the MELCOR, is the first materials to drop down into the
16 lower head are the lower melting point control blade
17 materials that are melting, falling down into the lower
18 plenum.

19 Everything is falling down there and
20 becoming quenched by all this water. Okay. So and then
21 by the time - by the time the water dries out and you start
22 to heat up the lower head again sufficient core damage
23 has happened that you failed a steam line or
24 depressurized the vessel.

25 So you have a kind of a situation where

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1 you're reheating all of these materials and you're at
2 atmospheric or you're at, you know, you're
3 depressurized.

4 So under those conditions what we're
5 finding is that - is that as these materials in the bottom
6 of the head, and it's subsuming all the drive tubes and
7 steel structures in the lower plenum, as these things are
8 melting and running down you're basically heating the
9 whole head up including the penetrations.

10 And so the failure mode that we're seeing
11 in these depressurized accidents that are heating up from
12 a quenched situation is that we first start melting the
13 inner surface of the - of the head and just node by node
14 melting through the head until there's no strength left.
15 And so that's the - that's the modeling abstraction that
16 we're doing right now.

17 MEMBER REMPE: So there's the Corbis data,
18 remember, from Switzerland where they ran metal thermite
19 through a drain line and that sucker just ablated through
20 real quick, you know. So that's one thing to kind of keep
21 in mind. And secondly, I mean, you do failure area
22 sensitivities, and it's too late. I know that the
23 report's done and all that but for other things like the
24 dry well liner you did failure area.

25 You considered things like heat transfers

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1 from the liner and I don't think you did any sensitivities
2 right on the area size of the lower head failure. Am I
3 right in that?

4 DR. GAUNTT: I think we just fail a whole
5 ring at a time.

6 MEMBER REMPE: And so I guess I'm just
7 wondering how important is that area. You looked at
8 other areas. Would that not have mattered?

9 DR. GAUNTT: I think you're seeking a
10 granularity that's beyond MELCOR right now. But -

11 MEMBER REMPE: And that's okay. And
12 again, I don't expect this - the report to change. But
13 I guess that was the point I was trying to make this
14 morning with the fact it was good that you found that the
15 phenomenological uncertainties are larger than the
16 weather uncertainties on the whole process.

17 But I think, again, that point I was trying
18 to raise well, do you think we've captured everything.
19 No, because of the model, and it's just something else
20 to bring up is all I'm - is the only point I'm trying to
21 make.

22 DR. GAUNTT: And I don't mean to brush you
23 off either.

24 MEMBER REMPE: I know.

25 CHAIRMAN STETKAR: Sure you do.

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1 MEMBER REMPE: You can try. No, but I
2 mean, I just think that we need to think about that
3 because, again, maybe there's some things we could learn
4 from Fukushima and stuff we learned after TMI.

5 DR. GAUNTT: What - you know, I'll just
6 mention this on the side as we - as we study Fukushima
7 we've got the - I think the MAP folks may have a
8 penetration failure model.

9 But overall, timing wise we're not seeing
10 such a big difference in - between the codes on when the
11 lower head fails. It seems, you know -

12 MEMBER REMPE: But, of course, we have no
13 hard data to even say that we know that it failed at any
14 of the vessels at TMI.

15 DR. GAUNTT: We have some indirect data.

16 MEMBER REMPE: No hard data though.

17 DR. GAUNTT: Yeah.

18 MEMBER REMPE: Okay. And so that's why I'm
19 just thinking again that it'd be good to have a few
20 caveats in there.

21 DR. GAUNTT: I know you love that drain
22 line.

23 MEMBER REMPE: Sorry. I'm done.

24 CHAIRMAN STETKAR: We hit the second
25 bullet. Any more discussion on the third bullet -

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1 DR. GHOSH: I think we answered -

2 CHAIRMAN STETKAR: - where we talked about
3 the size and -

4 MEMBER CORRADINI: There's no water.

5 DR. GHOSH: Right. There's no water.

6 MEMBER CORRADINI: I keep on bringing up
7 water and I've been told go away, there's no water in this
8 calculation. So okay.

9 CHAIRMAN STETKAR: So if Mike's happy that
10 there's no water -

11 MEMBER CORRADINI: No.

12 CHAIRMAN STETKAR: - he doesn't -

13 MEMBER CORRADINI: I've been told - I've
14 been told that I'm off in a different land. That's fine.

15 CHAIRMAN STETKAR: And finally, the fourth
16 bullet, we touched on it to some extent with the operator
17 actions for shedding loads.

18 The only other operator action that's
19 modeled is the operators are guaranteed to open the SRV
20 to depressurize at - I don't remember what time it was,
21 two hours I think - something like that. And that's
22 always guaranteed to be successful at that time, right?

23 DR. GHOSH: Yeah. We didn't - for the
24 integrated uncertainty analysis we did not vary that time
25 because we didn't have a basis to come up with a

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1 variation.

2 We kind of said from the beginning that the
3 HRA was outside of the scope of our study and in the
4 unmitigated scenario there's only a couple of actions.

5 So we did a separate sensitivity study to
6 look at if that time were different what would the effect
7 be. So that was separate kind of one sensitivity we did
8 rather than integrating that uncertainty into the whole
9 mix.

10 So we - so we did look at the case where the
11 operators don't manually depressurize as an extreme
12 case.

13 What if they never do it? And then there
14 were a couple more cases - what if they do it - they really
15 jump the gun - they know they're going to have to do it
16 and they do it super early and then similar modest
17 variations around the nominal time.

18 CHAIRMAN STETKAR: One of the questions
19 that I had and this is - I'm kind of reading notes in real
20 time here and I'm not doing all that well - the
21 sensitivity study that you did looked at them opening the
22 SRV at a half hour, an hour, two hours, three hours and
23 not opening it at all.

24 One of the questions that I had - let me just
25 read my notes here for a second. Bear with me here. Do

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1 the melt - when the operators open the SRV do the MELCOR
2 models assume that the operators open it fully? In other
3 words, it's all the way open?

4 DR. GAUNTT: My recall is they dropped the
5 pressure down to - was it 150 PSI?

6 CHAIRMAN STETKAR: I think it's 120.
7 Kyle?

8 DR. GHOSH: Kyle, did you hear that
9 question?

10 MR. ROSS: Yeah. Yeah, I did. So yes,
11 they can only open or close the valve. They can't
12 position -

13 CHAIRMAN STETKAR: So they open it fully?

14 MR. ROSS: But it turns out at least from
15 MELCOR calculation that opening one valve fully gives you
16 about 100 degree per hour combined with, you know, making
17 up with the RCIC on the feed.

18 So it's probably about what the operators
19 see is that opening one valve and feeding RCIC as required
20 gives them something like 100 degrees per hour.

21 CHAIRMAN STETKAR: Well, it's - if I - if
22 I look at the case that you ran for them opening it at
23 half an hour -

24 MR. ROSS: Yes.

25 CHAIRMAN STETKAR: - it shows that RCIC

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1 trips due to low steam line pressure at 2.1 hours into
2 that run, I think.

3 MR. ROSS: Right. So that was a - so that
4 was a bad result.

5 CHAIRMAN STETKAR: Yeah. Well, but that
6 says that you depressurize enough such that you get below
7 75 pounds in about 1.6 hours.

8 MR. ROSS: Yes, that's right.

9 CHAIRMAN STETKAR: Okay. Now, if I open
10 the SRV at one hour RCIC never trips due to low steam line
11 pressure. RCIC keeps running. It's assumed in this
12 case that the batteries deplete at four hours.

13 RCIC keeps running until the steam lines
14 flood at 5.2 hours. How come if I open up the SRV at one
15 hour RCIC never trips from low steam line pressure?

16 MR. ROSS: Right. Right. That's what we
17 saw.

18 DR. GAUNTT: Do you have some insight into
19 that, Kyle?

20 CHAIRMAN STETKAR: Do you have some - I'm
21 asking why is that. If I depressurize in an hour and a
22 half for 1.6 hours by opening it at a half an hour why
23 don't I depressurize within pick a number, 1.6 hours, a
24 couple hours if I open it at one hour, you know? Why can
25 RCIC hang in there for another 4.5, 4.2 hours?

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1 MR. ROSS: so one thing - I don't think we
2 modeled this especially completely. The operators
3 wouldn't have been hands off. If they saw pressure
4 dropping too far for RCIC to operate they would have
5 interrupted the SRV.

6 CHAIRMAN STETKAR: Yeah, but the darn guys
7 didn't do that when they opened it at a half an hour, did
8 they?

9 MR. ROSS: Yeah. So we didn't like - so we
10 didn't - we probably didn't capture actual operator
11 action especially well here.

12 CHAIRMAN STETKAR: Okay. The only reason
13 I'm asking this is this - the sensitivity study is
14 presented in the context of examining different guesses
15 about operator performance as a function of time rather
16 than the nominal whatever it is, two hours I think was
17 used.

18 MR. ROSS: Yes.

19 CHAIRMAN STETKAR: And the conclusion is
20 well, if they open it up too early it's really - it's a
21 bad day and if they never open it up at all it's a bad
22 day.

23 In between there it doesn't make much
24 difference. But in between there some of the things that
25 are causing I think the bad day don't seem to be modeled,

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1 like pressure getting too low for RCIC operation.

2 I think one of the reasons it's a bad day
3 if they open it up too early is that RCIC now goes away
4 at two hours.

5 MR. ROSS: Yeah. So opening too early is
6 not an issue because the operators would respond and they
7 would not let pressure get too low to drive the turbine.

8 CHAIRMAN STETKAR: But my point is your
9 sensitivity studies don't seem to be examining - in the
10 half hour case you said well, that's really bad.

11 They're opening it up too early and look,
12 lo and behold, if they don't intervene RCIC will fail at
13 2.1 hours. Now, if I look at the one-hour case, the
14 two-hour case and the three-hour case, RCIC never seems
15 to get a chance to fail from low pressure.

16 So I don't get to examine that behavior and
17 I don't understand why in those cases suddenly when I ran
18 those cases the operators now get smart and prevent it
19 from failing whereas in the first case they got stupid
20 and didn't prevent it from failing.

21 DR. GAUNTT: I'm just guessing but I -

22 CHAIRMAN STETKAR: So I'm not sure if I
23 understand whether these sensitivity studies are done on
24 the same level playing field in terms of this particular
25 phenomena.

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1 DR. GAUNTT: Yeah. Modeling the realistic
2 operator action I guess, you know, that opening it early,
3 doesn't reflect the fact that maybe they wouldn't let it
4 depressurize so much.

5 I'm guessing what happened - the thread I
6 would pull and I'm just guessing is that they're high on
7 the decay heat curve and they're boiling off the water
8 in the - in the core. And then when the water level falls
9 too low in the core then you lose pressure.

10 And I'm guessing that's why that case goes
11 to that point. You wait a few more hours you're lower
12 on the decay heat curve.

13 CHAIRMAN STETKAR: Yeah, but Randy, you're
14 not waiting a few more hours. In one case you're waiting
15 - 30 more minutes gains you three and a quarter hours,
16 okay.

17 DR. GAUNTT: I don't - I don't know. I'm
18 guessing at that.

19 CHAIRMAN STETKAR: Okay.

20 DR. GAUNTT: I'm guessing it's related to
21 where you are on the decay heat curve and where the water
22 level is in the core.

23 DR. GHOSH: Yeah. But I think we talk in
24 the report about the fact that we don't think we modeled
25 the half hour scenario completely correctly because we

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1 think that there's some unrealistic assumptions in there
2 about the operator just depressurizing by opening up and
3 then hands off and just letting it go.

4 CHAIRMAN STETKAR: That's why I asked do
5 they open it up fully. The answer is yes, they do.
6 Okay. If they do I understand how we're getting to the
7 low pressure -

8 DR. GHOSH: Right.

9 CHAIRMAN STETKAR: - you know, with
10 whatever time so people can boil water. What I don't
11 understand is the one-, two- and threehour cases. I
12 understand the never - you know, hands off, never open
13 it at all.

14 DR. GHOSH: But I think the one-hour case
15 we have the - there's a - there's a couple of operator
16 actions. At one hour you start the depressurization and
17 at two hours you take manual control of RCIC.

18 And I think what may be happening also in
19 the half-hour case is I don't know if because things are
20 happening too fast if we don't get to the - that point
21 of taking manual control of that -

22 CHAIRMAN STETKAR: Yeah. Maybe that's it.
23 Maybe that's it because the half-hour case RCIC is going
24 away at about that two-hour point.

25 DR. GHOSH: Right. Right. And I think in

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1 their - the one-hour case, you know, the operators have
2 planned to - you know, were doing the planned
3 depressurization and were going to take control of RCIC
4 at the two-hour point.

5 And with the half-hour I think - again, I
6 don't know if this is a realistic modeling but they may
7 not have a chance to - they haven't taken manual control
8 and they need to.

9 I think that might be one of the big
10 differences between the half-hour and the one-, two-,
11 three-hour case. And we did write up a little bit about
12 we think this probably isn't the best representation of
13 one - of the variation one might expect with regard to
14 that lower end. I mean, I don't - I didn't know.

15 So maybe they would have a chance to take
16 proper manual control. But I think we haven't - the way
17 we've modeled it it doesn't. And Kyle, you can correct
18 me if I'm wrong about that.

19 MR. ROSS: Yeah. So I'm trying to wade
20 through this in my - in my mind. But RCIC comes on full
21 and stays on full until they throttle it at two hours in
22 all cases.

23 DR. GHOSH: Okay. So that explains it.
24 So that - I think that is the big difference.

25 CHAIRMAN STETKAR: That's it. That's

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1 what's - that's what's doing it is that two hours just
2 happens to be kicking in, you know, with one-, two- and
3 three-hour cases before pressure gets low enough.

4 DR. GHOSH: Yeah. Yeah.

5 CHAIRMAN STETKAR: Okay. Thank you.
6 That actually answers my question about that.

7 MS. SANTIAGO: I'm going to go on to the
8 next section.

9 DR. GHOSH: So I think with that - that was
10 all the MELCOR items we had and then we're - the next part
11 was the MACCS parameters of interest, unless anybody had
12 -

13 CHAIRMAN STETKAR: These are all very well
14 formed.

15 DR. GHOSH: Okay. So the first one was the
16 hot spot normal and - sorry. The normal and hot spot
17 relocation doses and times.

18 So what we have on slide 49 is the dose
19 curves for both normal and hot spot and on the next slide,
20 slide 50, are the distributions for the times for both
21 hot spot and normal.

22 So we did provide some additional writeup
23 on that and I'll just - I'll ask the committee if they
24 have specific questions on those. No? Okay. Great.
25 So we can move on to the next one.

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1 CHAIRMAN STETKAR: Hold on - hold on a
2 second.

3 DR. GHOSH: Oh, okay.

4 CHAIRMAN STETKAR: I was writing. I
5 actually had -

6 MEMBER CORRADINI: I figured he is going to
7 ask something about evacuation speed. We might as well
8 - might as well just simply go to that.

9 CHAIRMAN STETKAR: You know, quite
10 honestly, I don't - in real time I think to keep us moving
11 here -

12 DR. GHOSH: Yeah, I guess we can - we can
13 come back to it if we need to.

14 CHAIRMAN STETKAR: Yeah. Why don't we do
15 - to keep us moving why don't we go on to the next ones?

16 DR. GHOSH: So the next one was the
17 evacuation speed and those were specified for the six
18 different cohorts. So we have the distributions here.
19 In the uncertainty analysis, we assumed the same
20 distribution for some of the cohorts. That's why you see
21 three curves instead of five.

22 Actually, I guess we also did the same for
23 the SOARCA study so that's why some of the triangles are
24 on top of each other too. And we did provide some
25 additional writeup on that last week and I'll ask what

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1 the questions are on that one.

2 CHAIRMAN STETKAR: I'm still writing
3 notes. I do have a question just to -

4 MEMBER CORRADINI: We figured you did.

5 CHAIRMAN STETKAR: The only - the only
6 questions I had about all of these, the relocation times
7 and the evacuation speeds and so forth, at the moment
8 these distributions are developed for the purposes of
9 this study. You keep correctly reminding us that this
10 study is done specifically for the unmitigated long-term
11 station blackout scenario at the Peach Bottom Nuclear
12 Power Plant.

13 How would these distributions change if I
14 told you that this scenario was initiated by the worst
15 earthquake that you've never felt before?

16 DR. GHOSH: So which I guess -

17 CHAIRMAN STETKAR: Have you thought much
18 about that? Are these - are these the distributions that
19 would apply regardless of the initiating event for that
20 scenario?

21 DR. GHOSH: Yeah. We have thought about
22 that and, Joe Jones, if you're - I think you're still on
23 the line.

24 MR. JONES: Yeah, I'm still on the line.

25 DR. GHOSH: Joe is actually our evacuation

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1 expert and I'll let him respond to that. You heard the
2 question?

3 MR. JONES: I did hear the question and
4 you're right, this is for a specific accident. It's also
5 for a specific site, which is Peach Bottom, and at Peach
6 Bottom there were very few bridges or crossings of
7 waterways of any kind within the EPZ.

8 So unless it's an earthquake that would
9 cause roads to shift in many areas and actually separate,
10 this distribution would be representative enough.

11 You know, if it's - your description of your
12 earthquake is a little bit qualitative. If it's the
13 worst one I can envision then I can envision all of the
14 roads failing and this would not satisfy that.

15 CHAIRMAN STETKAR: Well, I mean, my point
16 is that I'm not sure - the report in particular for a lot
17 of these off-site evacuation issues - timing and speed
18 in particular - if you keep thinking about the fact that
19 this is a particular scenario and you don't know what the
20 initiating event is, if it's a plain vanilla loss of
21 off-site power like birds flew into transmission lines
22 and other transmission lines fell down and diesel
23 generators failed to start and it's a nice sunny day, the
24 question is do these uncertainty distributions - were
25 they tailored to fit some presumption about what's

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1 happening in the external environment or not?

2 MR. JONES: Only to the extent that people
3 will evacuate or have a means to evacuate. So to the
4 extent that the roadways would be accessible, not
5 necessarily all of them.

6 But at Peach Bottom, for instance, if one
7 roadway is out, in almost any quadrant there's an easy
8 drive around and it's not a heavily populated site so it
9 doesn't affect it dramatically at Peach Bottom. For a
10 very high populated site - population density site it
11 would be different.

12 DR. GHOSH: So those are very site
13 specific.

14 CONSULTANT SHACK: And you did do it
15 differently at Surry, right? Didn't you - you did
16 consider there was an earthquake and take a -

17 MR. JONES: Yes. We had a seismic scenario
18 at Surry and it was kind of a split case there.

19 West of the river a similar situation as
20 Peach Bottom - not a lot of crossings or anything. So
21 the roads pretty much stayed intact. But as you get east
22 of the river into the Williamsburg area the interstate
23 collapses and it dramatically affected the evacuation
24 time - more than doubled it.

25 CHAIRMAN STETKAR: Okay.

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1 DR. GHOSH: Does that answer the questions?
2 Should we move to the next one?

3 CHAIRMAN STETKAR: And that was my - those
4 were my biggest concern about both of these is how site
5 specific - how site specific are they or scenario
6 specific for that particular site.

7 I guess what I'm hearing is although they
8 are developed for this specific scenario because of the
9 characteristics of this site they might apply for other
10 initiators. Okay.

11 DR. GHOSH: Right. And I think - yeah, I
12 think the potential effects of the seismic event was the
13 sensitivity that was considered and it made a difference
14 for Surry and not so much for Peach Bottom.

15 So it's very site specific, the impact of
16 -

17 CHAIRMAN STETKAR: Okay.

18 DR. GHOSH: Okay. So the final, I guess,
19 parameter that we had identified for discussion was the
20 groundshine shielding factor, and on slide 52 we plotted
21 here the curves for normal, sheltering and evacuation
22 because we're assuming people are doing different things
23 during those activities so they have different exposure
24 from the groundshine and that's why the curves are
25 different.

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1 And Nate, unless you wanted to elaborate we
2 can again turn it over to the committee in terms of what
3 the specific question might be for that one.

4 DR. BIXLER: Thank you. I'll say just a
5 few words before doing that. One thing is that this
6 particular parameter was - is really - has a dual role
7 in our analysis.

8 There's the question of what kind of
9 shielding people get between the source of the
10 groundshine and where they're at and that's a real -
11 simply a shielding parameter kind of issue.

12 There's - we also considered and folded into
13 the groundshine shielding factor the idea that the dose
14 that someone gets from a - from some kind of radiation
15 may - to an organ may differ and it would differ depending
16 on age and size of the person, whether it's male or
17 female, you know, whole variety of things.

18 So we tried to - and Keith Eckerman came up
19 with a way of handling that part of the distribution.

20 We ultimately folded the two together into
21 a single distribution that went into groundshine
22 shielding factor because that was the efficient way of
23 doing this sampling from our point of view. So
24 ultimately this factor encompasses a couple different
25 concepts for what we were trying to do.

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1 MEMBER RYAN: I'm assuming you took from,
2 you know, infants and small kids all the way up to older
3 folks in all that. Did you account for the fact that they
4 have different radiation sensitivities as well?

5 DR. BIXLER: I don't know if - I don't think
6 that was part of Keith's writeup so I don't believe he
7 did that. That would have been - yeah, actually that
8 would be reflected separately in when we do the analysis
9 we have a set of dose convergent factors.

10 Then we apply a set of risk factors to
11 estimate the number of cancers, and both of those were
12 uncertain here. So those two were treated
13 independently.

14 MEMBER RYAN: You'll catch it in the
15 uncertainty factors?

16 DR. BIXLER: Sorry?

17 MEMBER RYAN: You'll catch it in the
18 uncertainty factors. Is that where you'll catch this
19 point of variability?

20 DR. BIXLER: Yeah. That's right, as far as
21 -

22 MEMBER RYAN: A child's thyroid is about
23 one-tenth of an adult so for the same intake it's 10 times
24 an adult's. So all that kind of stuff, yeah, that you'd
25 have to capture.

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1 DR. BIXLER: That should be - that should
2 be - yeah, that would be in the dose convergent factors.
3 Then the risk for health effects would be encompassed in
4 the risk factors.

5 MEMBER RYAN: Good. Thank you.

6 DR. BIXLER: Yeah. Let me ask one and your
7 later writeup may have explained something. I got
8 confused when I was reading the NUREG.

9 DR. GHOSH: I think we decided to replace
10 that writeup with our newer one because we thought -

11 CHAIRMAN STETKAR: Yeah. This is one
12 thing that I fell asleep last night before I got to read
13 this, quite honestly, because it was right at the end.

14 It's - I mean, I'm sorry. I fell asleep
15 this morning before I got a chance to finish reading this.

16 When I read Section 4.2.5 of the NUREG, and
17 I'm just paraphrasing from my notes here, my notes say
18 that section explains the distribution for the composite
19 uncertainty in groundshine dose and shielding factors -
20 GSHFAC, this variable up here - is the independent
21 product of the groundshine shielding distribution,
22 GSFAC, and a distribution for the groundshine dose
23 coefficients.

24 DR. BIXLER: Right. That's what I was just
25 explaining a minute ago. Was that clear or do you still

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1 have a question about that?

2 CHAIRMAN STETKAR: The question that I have
3 is is there now - in what I've seen GSFAC is a common
4 distribution that's now used in two places as if -

5 DR. BIXLER: It's one distribution and
6 there - you might be thinking about a typo that we found
7 in the original documentation. I think we had put in
8 groundshine shielding factor with a slightly different
9 acronym. It might have been GSFAC or something like
10 that.

11 CHAIRMAN STETKAR: Yes.

12 DR. BIXLER: That was a typo.

13 CHAIRMAN STETKAR: That's just a - oh,
14 okay. That's - all right.

15 DR. BIXLER: One - that's the only one
16 parameter.

17 CHAIRMAN STETKAR: Okay.

18 DR. BIXLER: There's really only one -

19 CHAIRMAN STETKAR: Okay. Thanks.
20 Because I was getting - if you're using one parameter to
21 create two other things you're treating independently
22 you're not capturing the uncertain. But if it's the same
23 parameter it's a typo. Thank you.

24 DR. GHOSH: Yeah, so we're fixing the typo
25 and I think we rearranged the material in that subsection

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1 -

2 CHAIRMAN STETKAR: Yeah.

3 DR. GHOSH: - to be more clear.

4 CHAIRMAN STETKAR: Thanks. That would
5 probably explain - again, I just think about
6 uncertainties. I don't know what these things are
7 really in practice.

8 I just know the people in the past in many
9 cases have taken a single distribution and multiplied it
10 by three other things and then treated those as
11 independent parameters and multiplied them together and
12 it pulls the uncertainties down from accounting for that
13 single parent distribution.

14 But a typo would explain this. Thank you.
15 That was easy. That was - I'm glad I didn't spend a lot
16 of time this morning reading this.

17 DR. GHOSH: So that completes our list of
18 items that we understood, you know, that you all wanted
19 us to discuss further and we'll certainly turn it over
20 for more questions.

21 But just to - I just - just next steps FYI
22 we have a CSARP presentation on Wednesday to present the
23 results of the uncertainty analysis and for - we have
24 several papers at next week's ANS PSA conference. On
25 Thursday we have a two-hour SOARCA session and it's

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1 really five papers on the uncertainty analysis. And
2 we're still working on the report.

3 You know, we've been - we're in the process
4 of addressing some of the prior comments and we're going
5 to take what we got today to further hopefully improve
6 the documentation in the report and, you know, nominally
7 we're expecting to send it to publication very late this
8 fall.

9 But, you know, we - I think we'll - the
10 schedule is a little - you know, is somewhat questionable
11 and I think we are waiting to see whether the committee
12 is going to write a letter to see whether we should, you
13 know, wait for kind of a final word or we can go ahead
14 and finalize the feedback we got today and so on.

15 We have a public draft that's available so
16 there's isn't a tremendous amount of pressure to publish
17 the final by a certain deadline. But, of course, you
18 know, the longer - the longer we wait, as you see the state
19 of knowledge kind of advances and then more questions
20 come up with respect to what - how what we did -

21 CHAIRMAN STETKAR: And the state of memory
22 as it decays.

23 DR. GHOSH: Right. It works in both
24 directions, yeah.

25 MEMBER CORRADINI: And the half life for

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1 you is?

2 MS. SANTIAGO: Well, we do want to try and
3 finish it because we have a lot of work that we're looking
4 at for filtered vents, some other things. Mitigating
5 strategies is coming up that we'll be involved in and
6 economic consequences so -

7 DR. GHOSH: Right. We're juggling a lot of
8 projects. But we - yeah, we're trying to bring this one
9 forward. So yeah, I'll turn it over for questions and
10 comments.

11 CHAIRMAN STETKAR: Any other - first of
12 all, any other questions about the specific topics that
13 we've covered? If not, I have a few administrative
14 duties that I need to handle here.

15 I'll ask if there are any questions or
16 comments from anyone in the room.

17 CONSULTANT SHACK: Drove most of them away.

18 CHAIRMAN STETKAR: I don't know what the
19 situation is with the bridge line. If we just have a
20 single bridge line open only to Sandia or let's open up
21 the bridge line to see if there are any members of the
22 public out there who can have an opportunity to make any
23 comments. Can we do that?

24 We'll wait a couple of seconds because like
25 everything here we have absolutely no indication of

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1 whether or not the bridge line is open without asking for
2 oral feedback. Sometimes you hear pops and crackles
3 that give you an indication.

4 CONSULTANT SHACK: It's pretty quiet
5 today.

6 CHAIRMAN STETKAR: It is. It's really
7 quiet today. There we go. If there are - if there's
8 anyone other than Sandia folks who are out there
9 listening in on the bridge line could you do us a favor
10 and just acknowledge your presence so that we know that
11 the bridge line is open and we can hear you?

12 CONSULTANT SHACK: Supposed to be open,
13 John.

14 CHAIRMAN STETKAR: Anyone - yeah, I -

15 COURT REPORTER: I suspect they may have
16 dropped off.

17 CHAIRMAN STETKAR: Okay. I'll ask if
18 there's anyone out there. If there's anyone - that's -
19 it's open. If there's anybody out there from Sandia who
20 wants to say anything.

21 CONSULTANT SHACK: Well trained.

22 CHAIRMAN STETKAR: Thank you. So we've
23 done that. Now, what I'd like to do is two things. We
24 normally go around the table and see if any of the members
25 have any closing comments that they'd like to make, and

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1 I'll ask as we're going around the table before the other
2 thing that I want to get some feedback from the
3 subcommittee is do we feel that we'd like to have another
4 full committee presentation, given what we've learned
5 today.

6 We had the original presentation that
7 prompted this subcommittee meeting. Do we want to have
8 a full committee meeting which may or may not prompt a
9 letter? It's something that you were asking about in
10 terms of logistics.

11 So if you can weigh in, since we need to be
12 a little bit considerate of time here. I'll ask Dr.
13 Corradini first. Do you have any closing comments or -

14 MEMBER CORRADINI: No. I do want to thank
15 the staff. I think you did an awful lot of work in two
16 months. So thanks for working like Stetkar does and
17 matching him blow for blow.

18 I do think though there's one general thing
19 I guess I wanted to - and I don't know if it's appropriate
20 on the uncertainty report but consider it. It kind of
21 goes back to a corporate memory issue, which is we've been
22 kind of getting after you about can you explain this, can
23 you explain this better, can you, you know, provide the
24 - how the distribution connects to an experiment or to
25 a judgement or to whatever.

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1 But also in the final report I think it would
2 be useful if, again, since this is going to look beyond
3 this one project if you - if you didn't do something that
4 in retrospect you should have done this is kind of -
5 excuse my English - this is kind of like a Ph.D. thesis.

6 You have conclusions, you have observations
7 and you have recommended future work. I think this
8 deserves a relatively long list of recommended
9 additional things that might need to be done upon further
10 reflection whether it be a redoing of a distribution, a
11 connection to something, additional calculations,
12 waiting for Fukushima unwrapping of the site to learn
13 more.

14 Those sorts of things I think actually point
15 to - would be useful - not a long list but a key list that
16 would help future staff and point them to what they would
17 do next.

18 To me, particularly because this is going
19 to be like a - one thing you're teaching others what you
20 would do if you had all the time in the world but you
21 really don't want to do. I think those sorts of listings
22 would be very helpful.

23 To me, I think that would be very helpful
24 as part of this, particularly the uncertainty part of
25 this because we seem to be highly interested in

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1 uncertainty these days.

2 So that was my only thing, other than
3 thanking you all for doing it so quickly. And to your
4 point, I don't think we need a full committee meeting.
5 I'm sure there's enough of us in the room that have enough
6 memory that we can reflect upon this if you're going to
7 write a letter, Mr. Chairman, or offer up a letter.

8 CHAIRMAN STETKAR: The committee writes
9 letters.

10 MEMBER CORRADINI: I understand that but
11 the chairman starts with an initial rough draft.

12 CHAIRMAN STETKAR: Well, but for the
13 committee to write a letter we'd need a briefing of the
14 full committee to learn, you know, salient things that
15 we learned today.

16 MEMBER CORRADINI: But we had written a
17 letter from the briefing we had in -

18 CHAIRMAN STETKAR: We did not. We decided
19 to have this subcommittee meeting because there were
20 enough details that we wanted to probe.

21 MS. SANTIAGO: Came back in July. It's
22 when we briefed you in SOARCA I think six to eight months
23 prior to that. We did talk about the uncertainty
24 analysis there and I think the letter from that full
25 committee meeting had a couple sentences with regard to

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1 - I'm glad Bill is sick has said yes.

2 CHAIRMAN STETKAR: That letter - that
3 letter if I can paraphrase it - and Bill's here so he can
4 recall things a lot better than I can - that letter said
5 it looked like you were headed in the right direction on
6 the uncertainty analysis and we'd be really interested
7 in hearing back from you when you had it done. So -

8 CONSULTANT SHACK: We didn't wrap it up and
9 say goodbye.

10 CHAIRMAN STETKAR: Yeah, that's right. We
11 didn't say yes, you're - well, then you're blessed.

12 MEMBER CORRADINI: Then I - can I retract
13 my previous statement then? If we're going to - if
14 they're going to go through the effort of documenting
15 what they've done I think a brief presentation for the
16 full committee would be in order. But they're not going
17 to go through anything like this, I would hope.

18 MEMBER REMPE: No cobweb charts.

19 DR. GHOSH: That's the best part. I'll
20 even offer a picture. It's a work of art.

21 MEMBER REMPE: Beauty is in the eye of the
22 beholder.

23 MEMBER CORRADINI: I guess - I guess I don't
24 want to - I don't want to get ahead of you, Mr. Chairman,
25 but I guess if you were to do a full committee meeting

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1 my thought would be then that they'd kind of start with
2 the conclusions and be ready to delve into things as
3 topics come up versus what we had today, which is every
4 slide - every curve known to man.

5 CHAIRMAN STETKAR: No, that's right.
6 That's -

7 MEMBER CORRADINI: And the 11 ways in which
8 they came up with a curve.

9 MEMBER REMPE: In response to our queries.
10 They were just doing as asked.

11 MEMBER CORRADINI: I know. That's why we
12 thanked them.

13 CHAIRMAN STETKAR: Okay. Thanks.
14 Anything else, Mike?

15 MEMBER CORRADINI: No.

16 CHAIRMAN STETKAR: Joy?

17 MEMBER REMPE: I also want to thank
18 everybody for their efforts on this because they were
19 given a long list of things to do and not much time and
20 I appreciated and learned a lot from it.

21 I hope - again, I think I harped enough on
22 it about documentation, not only what we would do
23 differently but also there - sometimes some individuals
24 - I can remember a former chairman of the subcommittee
25 say oh, we've done MELCOR so much.

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1 Now we know - if there is - you know, there's
2 not this much uncertainty, and I think there is a lot of
3 uncertainty in the severe accident modeling capabilities
4 because we just don't have data. And so I'm
5 hoping that that flavor not only gets taken into account
6 with the individual distributions but also at the upper
7 part of the document, in the executive summary or
8 whatever.

9 And so I would like to have a meeting and
10 also maybe we can emphasize it because I think, again,
11 I noted in several different places about the need to get
12 into Fukushima and trying results and uncertainties.
13 I'm done.

14 CHAIRMAN STETKAR: Mike?

15 MEMBER RYAN: Thanks again, and my thanks
16 to the presentations. They were very well done. One
17 thought that struck me is that as the plant becomes more
18 and more well known it's important that we don't forget
19 the releases that have occurred from the kind of plant
20 area to the northwest across what is a very fertile part
21 of Japan.

22 There's a lot of farming and vegetable
23 growth and bamboo and all sorts of other stuff and it's
24 also an area for wild boar that are eaten routinely by
25 the population.

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1 And IAEA has got a couple of missions going
2 on in that topic. I would suggest that you at least touch
3 on that because that's local food. You know, hit some
4 of the pathway analysis that you're going to do.

5 I'm not saying it should be a big giant huge
6 effort but it probably should be something you at least
7 stay current on in terms of reports and other things. I
8 happen to be on one of the IAEA committees.

9 I'd be happy to help you get your hands on the
10 information because I think it would nicely augment some
11 of the - maybe the assumptions that you're making about
12 pathways and population groups and, you know, things of
13 that sort to see what the range could be. So it's a
14 comment plus an offer to help. Thank you, John.

15 CHAIRMAN STETKAR: Thank you. Steve?

16 MEMBER SCHULTZ: I really want to thank the
17 staff and Sandia as well associated with the work that
18 has been done since we met last.

19 That has been a very high quality and the
20 presentations that you've brought forward today have
21 also been very helpful and I would recommend that we do
22 bring this to the full committee because I know that other
23 members are - would be interested to hear a summary - a
24 brief summary of these conclusions including the spider
25 web pictorials.

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1 There are more than you actually showed
2 today but certainly to demonstrate what you have done
3 which has - which has been very thorough and I think very
4 well completed.

5 I would echo what Mike and Joy said about
6 things to include in the report and I would really
7 encourage the staff, the management to be sure that the
8 report reflects the findings, the recommendations and
9 the conclusions of the work that has been done here
10 because there is a lot that has been - that has been
11 learned and it's really important to capture that.

12 So in each of the sections where we have done
13 all this technical work, which is very well described in
14 terms of process and results, what I want to see in the
15 report is from this we conclude and then the highlights
16 associated with that.

17 And I do encourage also that the report be
18 reviewed to pull those findings into some sort of either
19 executive summary or technical summary for the report.

20 Just that it's been a long effort and very
21 thorough and very helpful moving forward and I am
22 concerned that when it's done at the end of this year with
23 this NUREG that it may not be opened up for a while.

24 So I'd encourage that we complete it nicely
25 in that regard. Thank you.

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1 CHAIRMAN STETKAR: Dennis?

2 MEMBER BLEY: There's a lot here that I like
3 a lot in IT. I want to thank you and compliment you on
4 the amazing amount of work. I still have some nagging
5 things about it that trouble me a bit.

6 If we do have a meeting, and we probably
7 should, we'll need to decide whether we write a letter
8 on this uncertainty methodology or we write a letter on
9 SOARCA and the uncertainty analysis or we write a letter
10 on the more general topic of SOARCA or a more narrow topic
11 - it depends on how you look at it - would probably talk
12 about the things that you didn't look at with respect to
13 uncertainty in that SOARCA analysis and then there's a
14 number of things that weren't part of this metric
15 analysis and I think we'll have to do that.

16 Yeah, I think showing it to the full
17 committee is a good idea. I like Mike's idea of - it's
18 kind of lessons learned from this, some kind of paper to
19 the - the rest of the staff can draw on in the future to
20 guide how you do this kind of work and I think that would
21 be very useful.

22 That's all. The other things I said during
23 the meeting I still hang onto.

24 CHAIRMAN STETKAR: Dr. Shack?

25 CONSULTANT SHACK: Well, let me add my

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1 thanks to - again, you did a tremendous amount of work.
2 I found a number of the additional calculations very
3 helpful, you know, and you say you want to put all this
4 in the appendix.

5 You know, some of these - I'd like the Run
6 1, Run 2 comparison to be up in the main document. I
7 think the full aleatory uncertainty should be in the main
8 document to compare with.

9 I would also recommend just in the main
10 document to help simpleminded folks, I like the 95 over
11 five error factor kind of numbers. They kind of - you
12 know, I can look at the regression analysis maybe and
13 figure out when parameters are important and then figure
14 out that those are MACCS or MELCOR parameters.

15 But the other ones give me a much quicker
16 sort of overall view of where my uncertainties are coming
17 from and so I think that would add to the clarity. Again,
18 I assume this is going to end up in an appendix.

19 You know, I think the work that you did on
20 simplifying the source terms is a useful sort of thing
21 that, you know, as we were discussing at lunch time, you
22 know, you're not going to do this very often and you need
23 more simplified approaches and it's sort of good to have
24 that document, that you went through that.

25 And, again, the documentation of the

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1 uncertainty - and then I think part of it came back today.
2 I mean, there's nothing wrong with making engineering
3 judgments.

4 Just make clear that it's an engineering
5 judgment, highlight where your engineering judgment
6 leaves you with very large uncertainties and, you know,
7 that's life. But, you know, try to make that as clear
8 as possible and, again, everybody always says write
9 clearly.

10 Well, if we knew how to write clearly I
11 wouldn't have to give you the advice. But that's where
12 I'm at. But I think it's a very good job. I hope you
13 get a chance to go ahead with the Surry analysis.

14 I think it would be useful now that you've
15 got all this under your belt to see how well we go. I
16 also am glad to see that MACCS2 now has the capability
17 to do the simple random sampling.

18 I still like bootstraps when you're telling
19 me how things have converged and it's a kind of a, you
20 know, every once in a while I can at least fall back on
21 that to find out and make myself convinced that you're
22 really there and now you have that capability sort of as
23 a routine feature.

24 CHAIRMAN STETKAR: Thank you. I'll echo -
25 thanks a lot. You folks did a heck of a lot of work in

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1 the last two months and I certainly really appreciate
2 everything that was done. I know it was - a lot of effort
3 went into it and you're really responsive, I think, to
4 our requests.

5 Quite honestly, I learned a lot more during
6 our discussion today than I did from your 100 or so pages
7 of written material.

8 I'll tell you that in many cases - and this
9 kind of echoes a little bit of what Bill said and I think
10 what you've heard in other places - is that the folks who
11 do the analyses sometimes get so close to the details that
12 it's obvious to you the point that you're trying to make.

13 But large numbers of tables of very small
14 numbers oftentimes kind of miss the point. So in many
15 cases, you know, the words that we heard today are much
16 more effective than a lot of really busy figures and a
17 lot of really busy tables.

18 Those might be nice to, you know, in the old
19 days of computer output - stacks of things that ran off
20 a spool printer for days and days on end. That might be
21 nice to document what you did.

22 MEMBER BLEY: Can I toss something on that

23 -

24 CHAIRMAN STETKAR: Yeah.

25 MEMBER BLEY: - because of - and then what

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1 Bill said, the tables - if you have something in an
2 appendix the tables could be in an appendix. If you had
3 your figures and if you put the error factors down on the
4 figures, man, that would communicate really well and the
5 tables are just awful trying to look at this and look at
6 this and figure out -

7 CHAIRMAN STETKAR: Including the error
8 factor, right? Someone wrote that I mentioned Lord
9 knows how many hours ago the fact that gee, your error
10 factor from a MELCOR stand alone uncertainty where you
11 took the mean, if you want to - the mean weather and fixed
12 the MACCS parameters that gives you kind of an error
13 factor of around three or four, somewhere in that ball
14 park. And now gee, look, if I add - if I do a full blown,
15 if I want to call it that, MACCS uncertainty analysis that
16 gets larger to about four or five.

17 Doesn't go up to, like, 15 to 20, and that
18 gives you intuitively a much better sense of where are
19 the uncertainties coming from. And I said well, gee, the
20 biggest - if that's actually true - I'll still withhold
21 some sort of skepticism on that notion but I don't know
22 anything about consequence analysis - if that's honestly
23 true it says well, from an uncertainty perspective then
24 we certainly do want to focus more on the MELCOR type
25 stuff given the fact that we've not done anything on the

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1 uncertainties on the initiating events or anything else
2 in the accident, you know, development.

3 And then the question is well, do we drill
4 down and see where the biggest sources of uncertainty are
5 there. And you finally come down to this thing that
6 we've been toying with.

7 Is it - are the sources of uncertainty
8 because we have data and there's a lot of, you know, a
9 lot of variability in the data - we have a lot of data
10 and we're just limited by variability in the data?

11 And I don't like to use the words aleatory
12 and epistemic in this sense because I think they're - it's
13 too easy to throw things into, you know, a black box.

14 So I'll just say uncertainty. Is it due to
15 the fact that there are - there's no evidence and we just
16 - we just needed to rely on our engineering judgment,
17 which is okay but it says well, if that's really driving
18 our understanding of risk maybe we ought to be doing a
19 little bit more research to refine that information.

20 So I think some of that higher level
21 presentation of here are the uncertainties and here
22 they're coming - here's where they're coming from, here's
23 how big they are, here's how small they are, why are they
24 this big, why are they that small, you know, is really
25 important to cast this into a context that, yeah,

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1 somebody who's not been deeply involved in this project
2 for three years or five years or however long you people
3 have been doing this can pick up the report and sort of
4 gain that level of understanding.

5 And I think, you know, what I've heard
6 regarding a full committee briefing I think we probably
7 should have one to bring this to closure.

8 I think the folks sitting at the table today
9 learned an awful lot. I think that the rest of the
10 committee would benefit from that discussion.

11 The committee would need to decide whether
12 we write a letter or not but I think we should probably
13 plan on a full committee meeting and then take it from
14 there regarding whether we write a letter.

15 So you need to work with Hossein and figure
16 out where we could fit - mutually agree to schedule for
17 that.

18 MEMBER SCHULTZ: John, there's a - I wanted
19 to follow up just a moment on what your comments were on
20 engineering judgment because the third case that we
21 discussed today, which is there is data - there is data
22 that's available, but what has been done in the study is
23 to use that data combined with engineering judgment to
24 determine the uncertainty contribution.

25 So that, you know, that's required either

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1 to represent consensus of the engineering community or
2 to just move forward with uncertainty distributions.
3 But we need - we need to capture that as engineering
4 judgment and not a depiction that data has been used to
5 determine uncertainties alone.

6 CHAIRMAN STETKAR: Yeah. Thanks. That's
7 something - some of the comments that I made about - it's
8 too easy to point to something and say well, look, this
9 number is in this report and this number is in this report
10 and look, the number that we used is between those
11 numbers, which doesn't necessarily tell the story that
12 we heard today. It's a different story.

13 Anything else? With that, thank you again
14 very, very much and we are adjourned.

15 (Whereupon, the above-entitled meeting
16 concluded at 4:42:37 p.m.)
17
18

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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

MELCOR – MACCS2 – Weather Uncertainty Integration

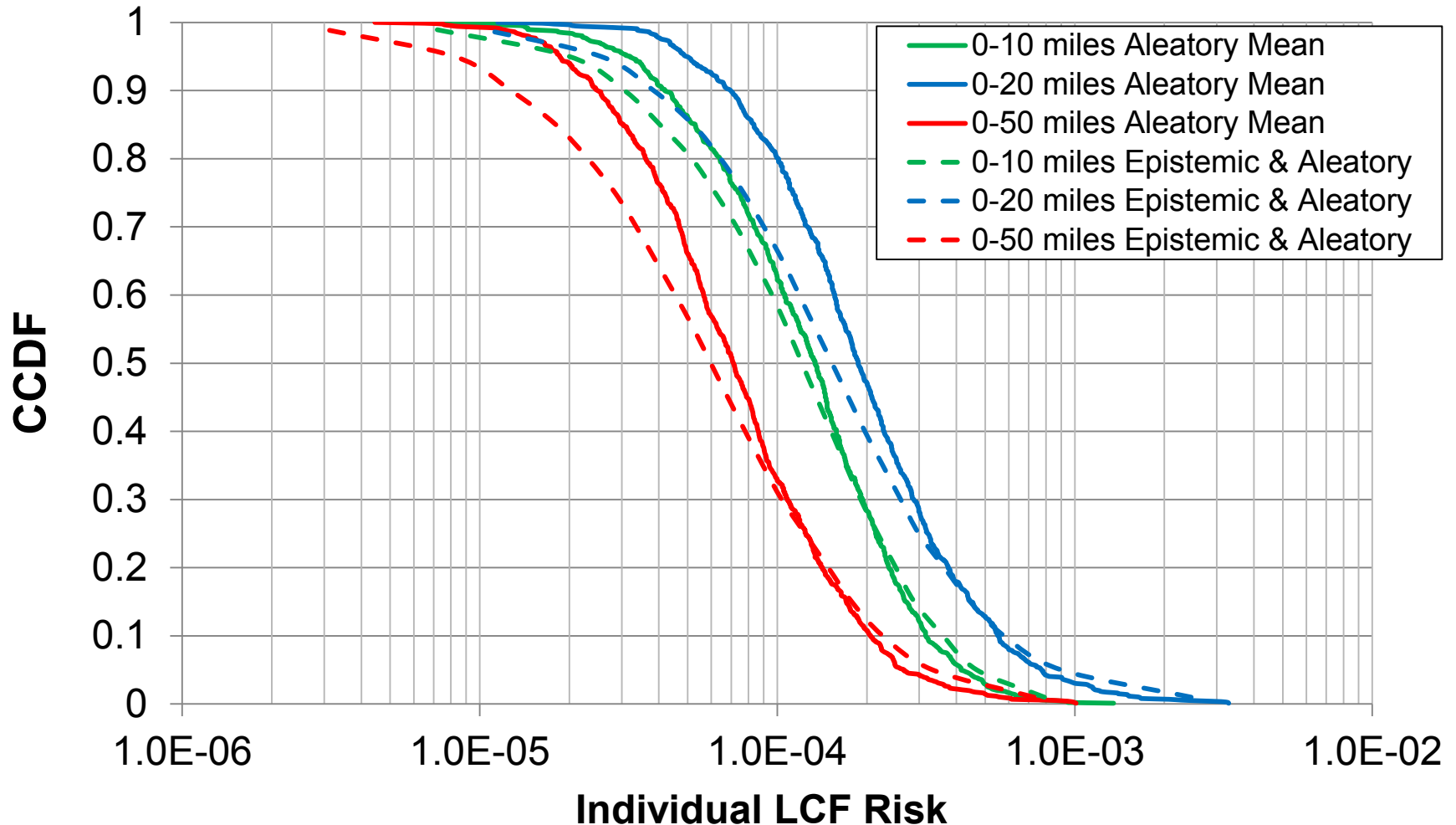
ACRS Comment:

- 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

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

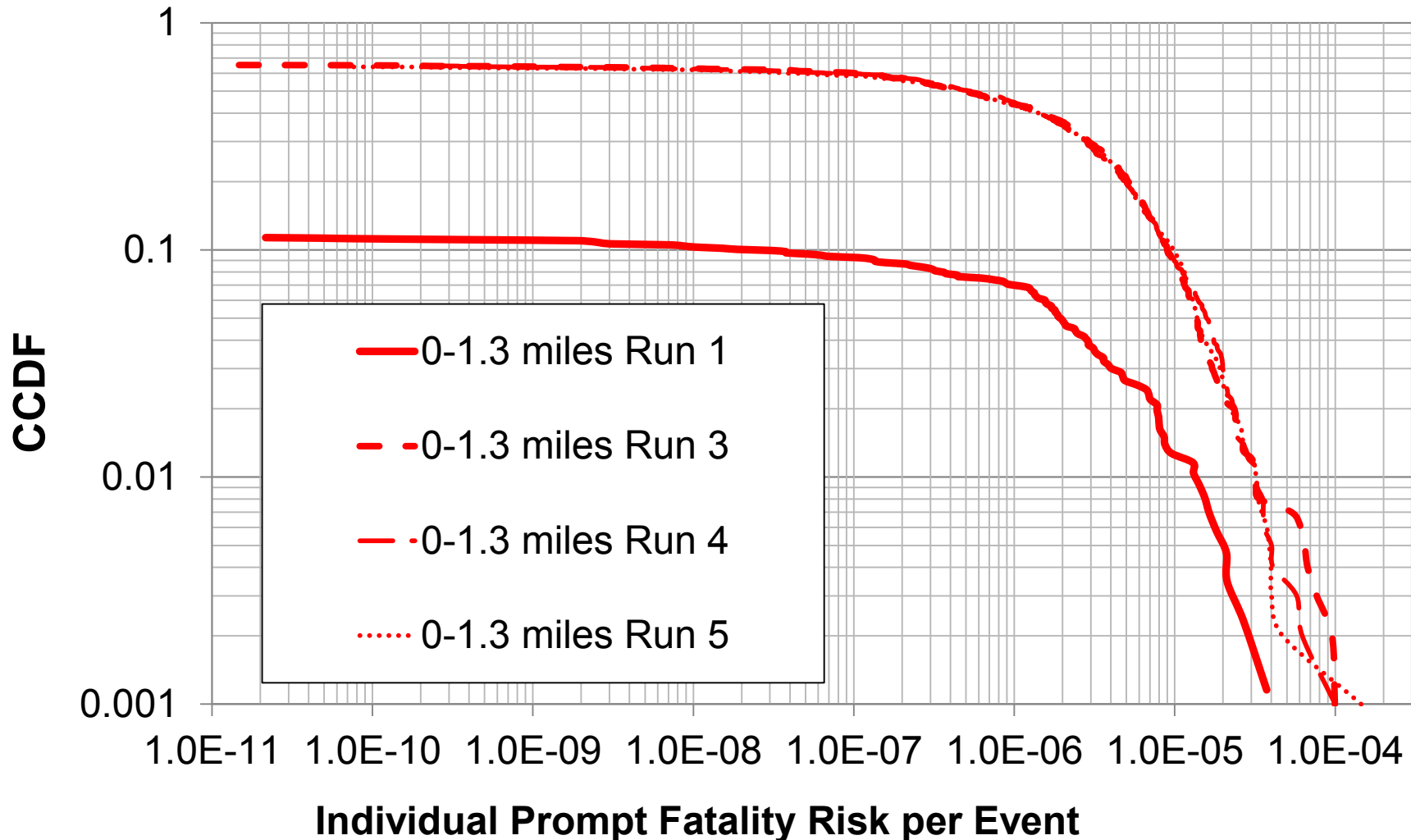
**Conditional, mean, individual
prompt-fatality risk (per event)
statistics for the MACCS2
Uncertainty Analysis for specified
circular areas (Run 1)**

| | 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 |

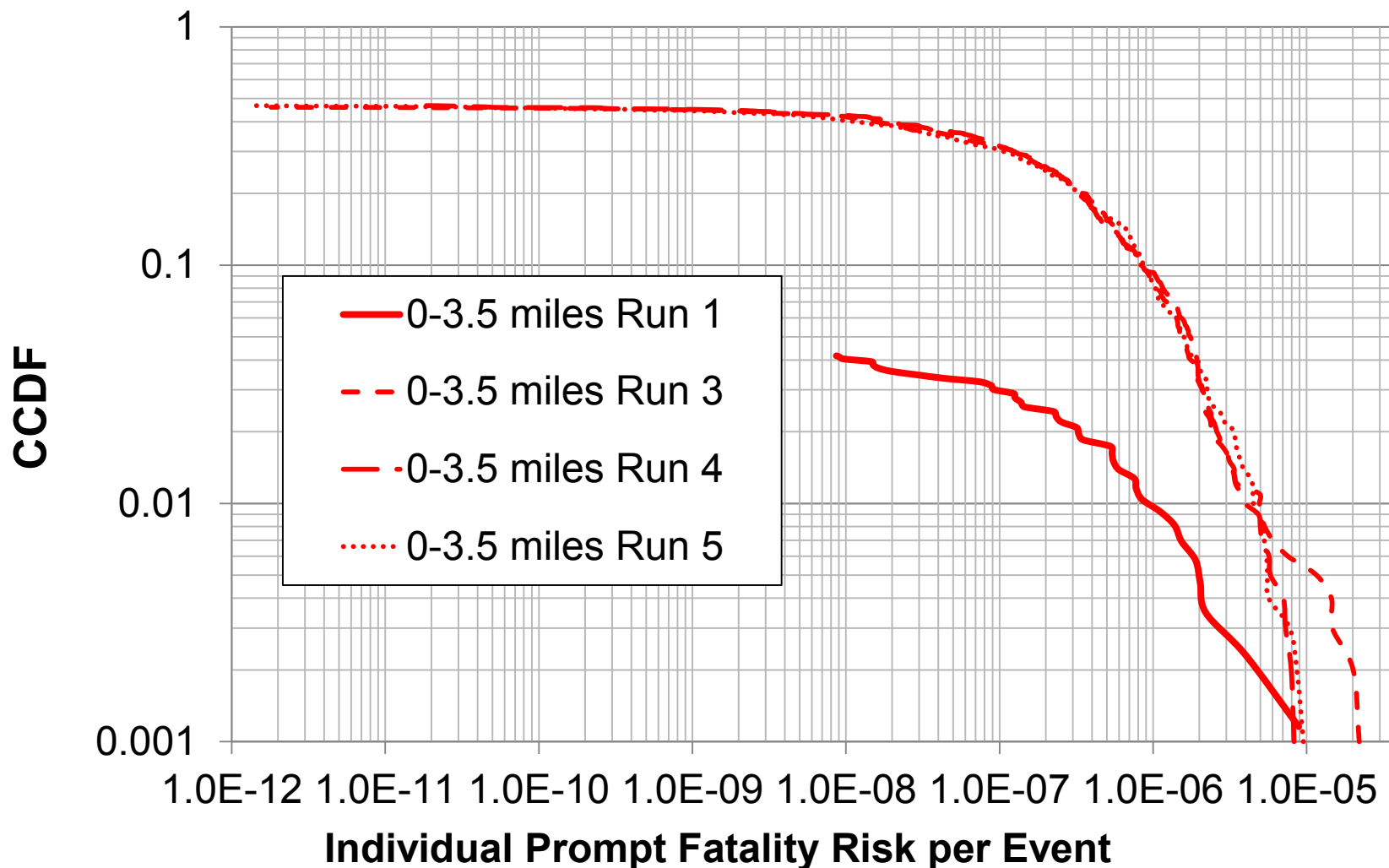
Run 3-5 conditional, mean, individual prompt-fatality risk (per event) statistics for specified circular areas

| | | 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 |

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:

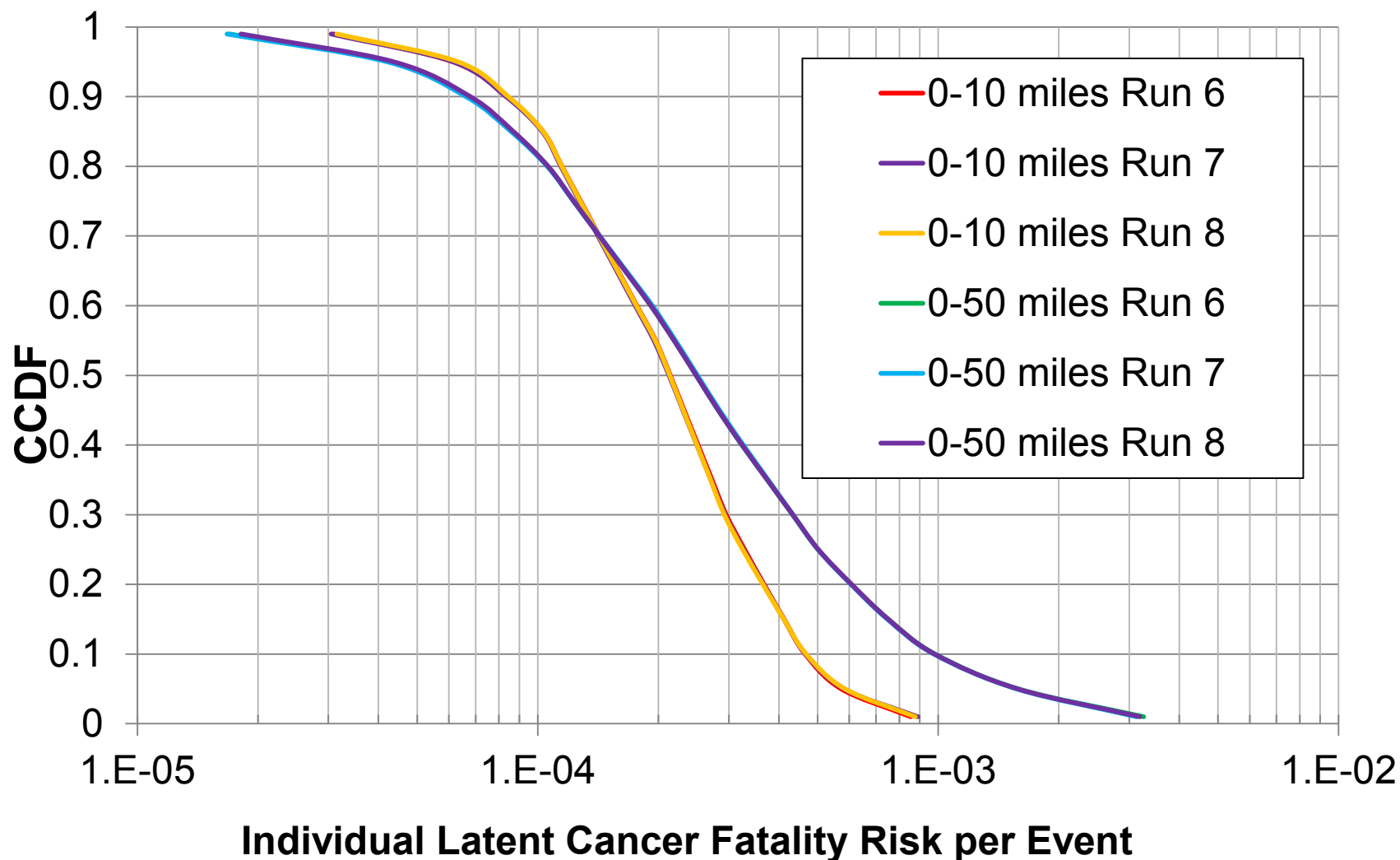
- Select the MELCOR realization that produced the largest conditional LCF fatality consequences in the current SOARCA uncertainty results.
- For that realization, sample from the 350 MACCS2 input parameters, and for each epistemic sample generate 984 weather cases to derive an uncertainty distribution 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.)

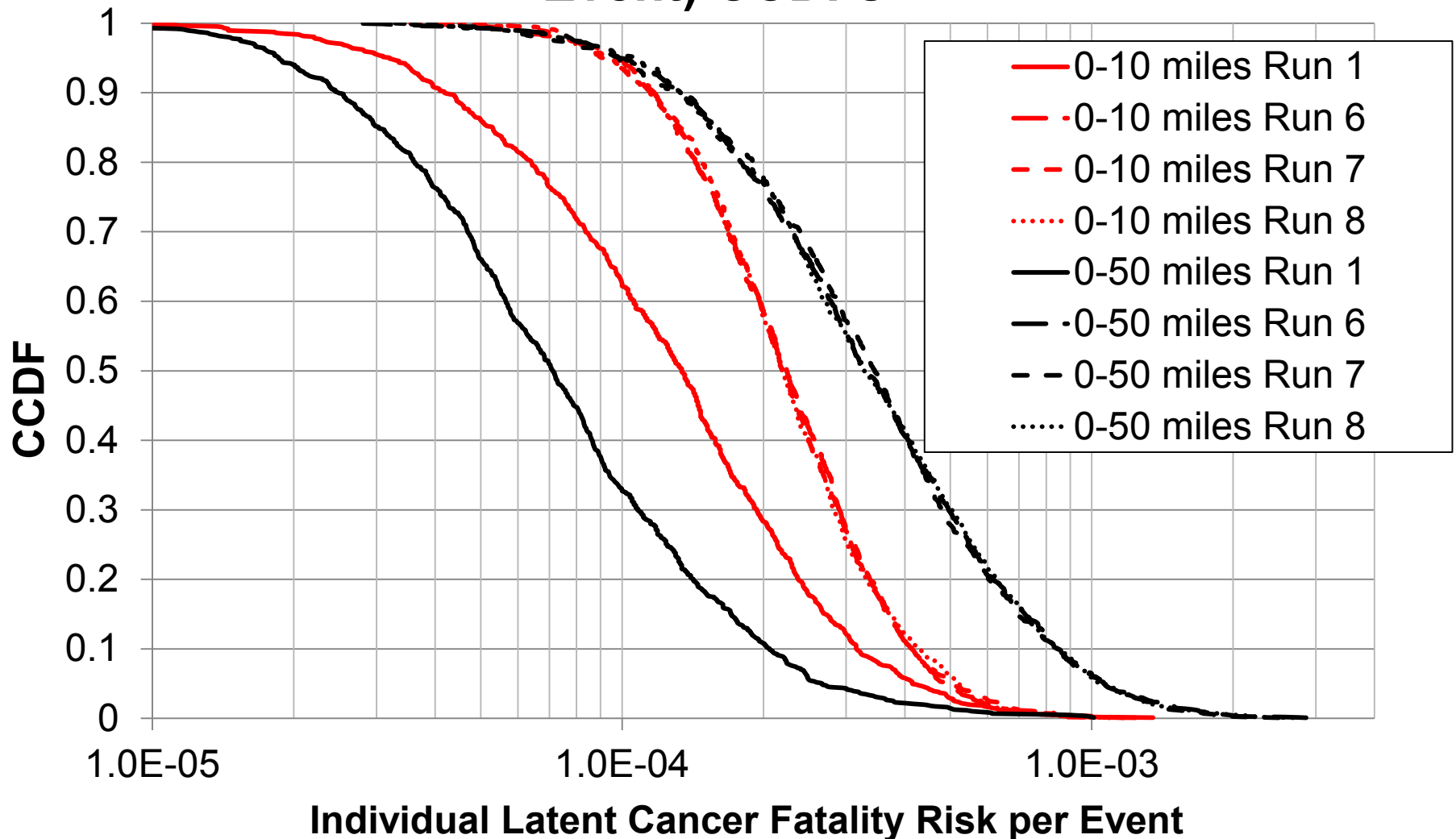
Approach:

- 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:

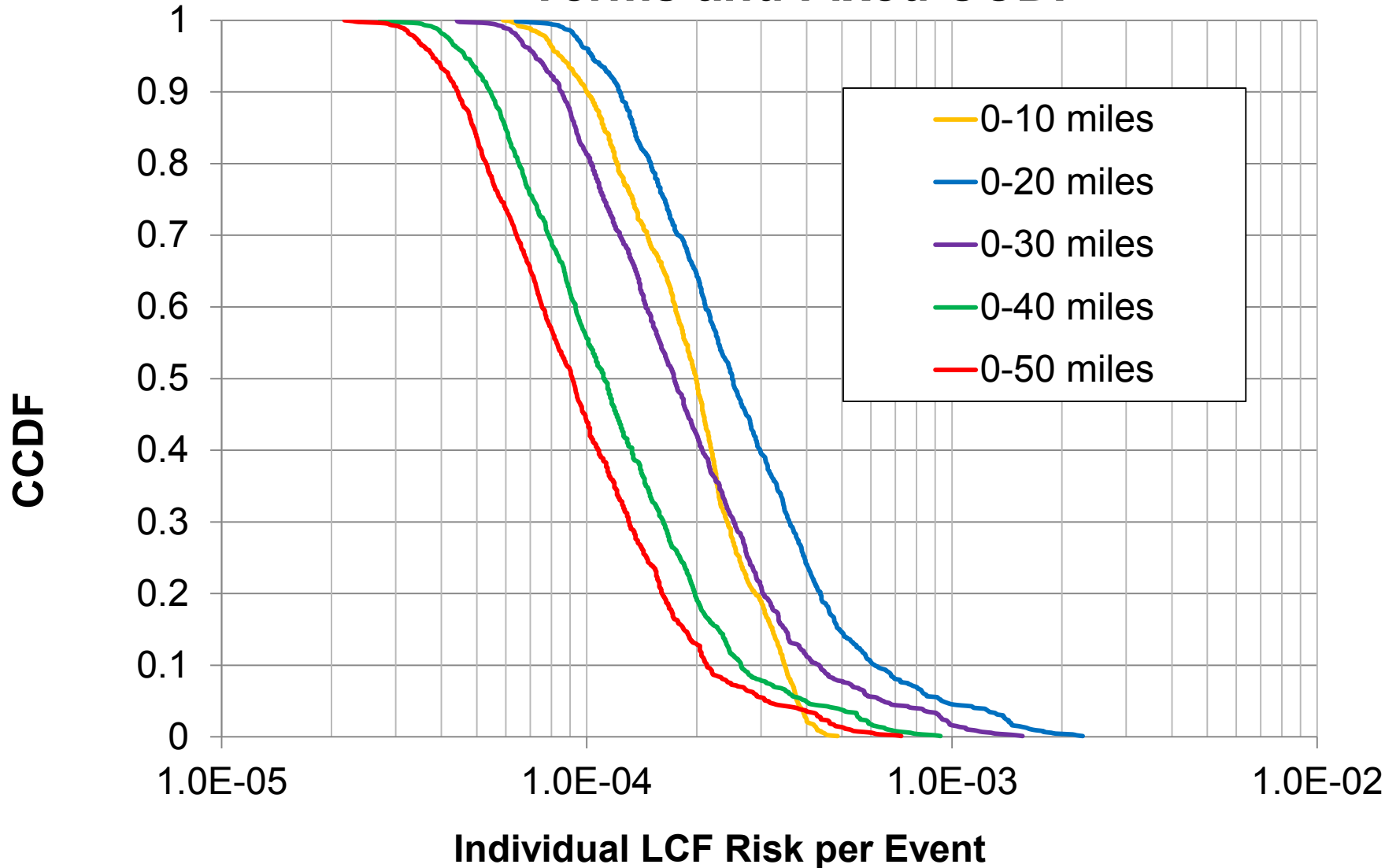
- Select a MELCOR realization that produced a small, but non-zero, contribution to the conditional LCF fatality consequences in the current SOARCA uncertainty results.
- For that realization, sample from the 350 MACCS2 input parameters, and for each epistemic sample generate 984 weather cases to derive 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

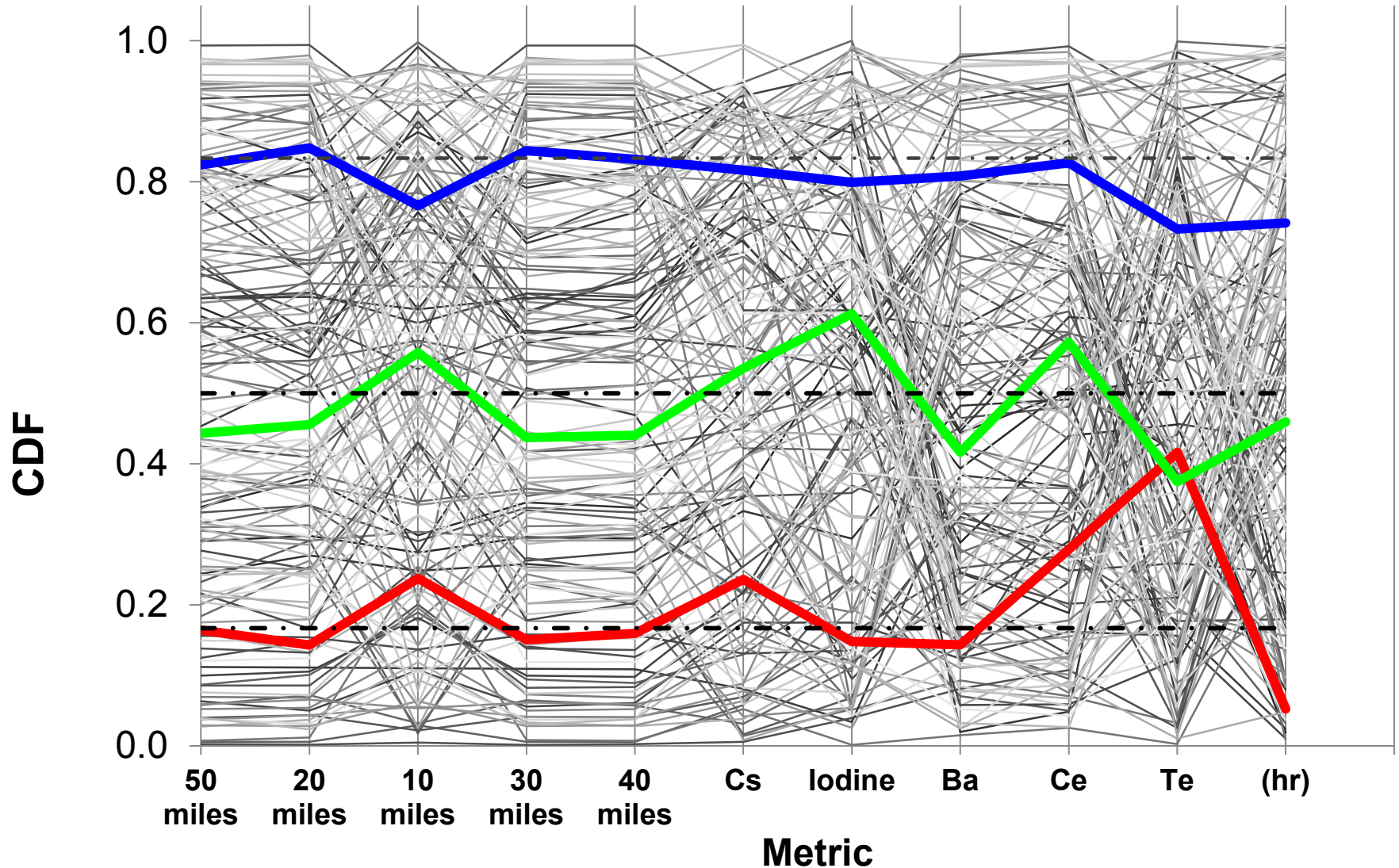
Run 2 Conditional Mean, Individual LCF Risk (per Event) for 865 Source Terms and Fixed CCDF



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.)

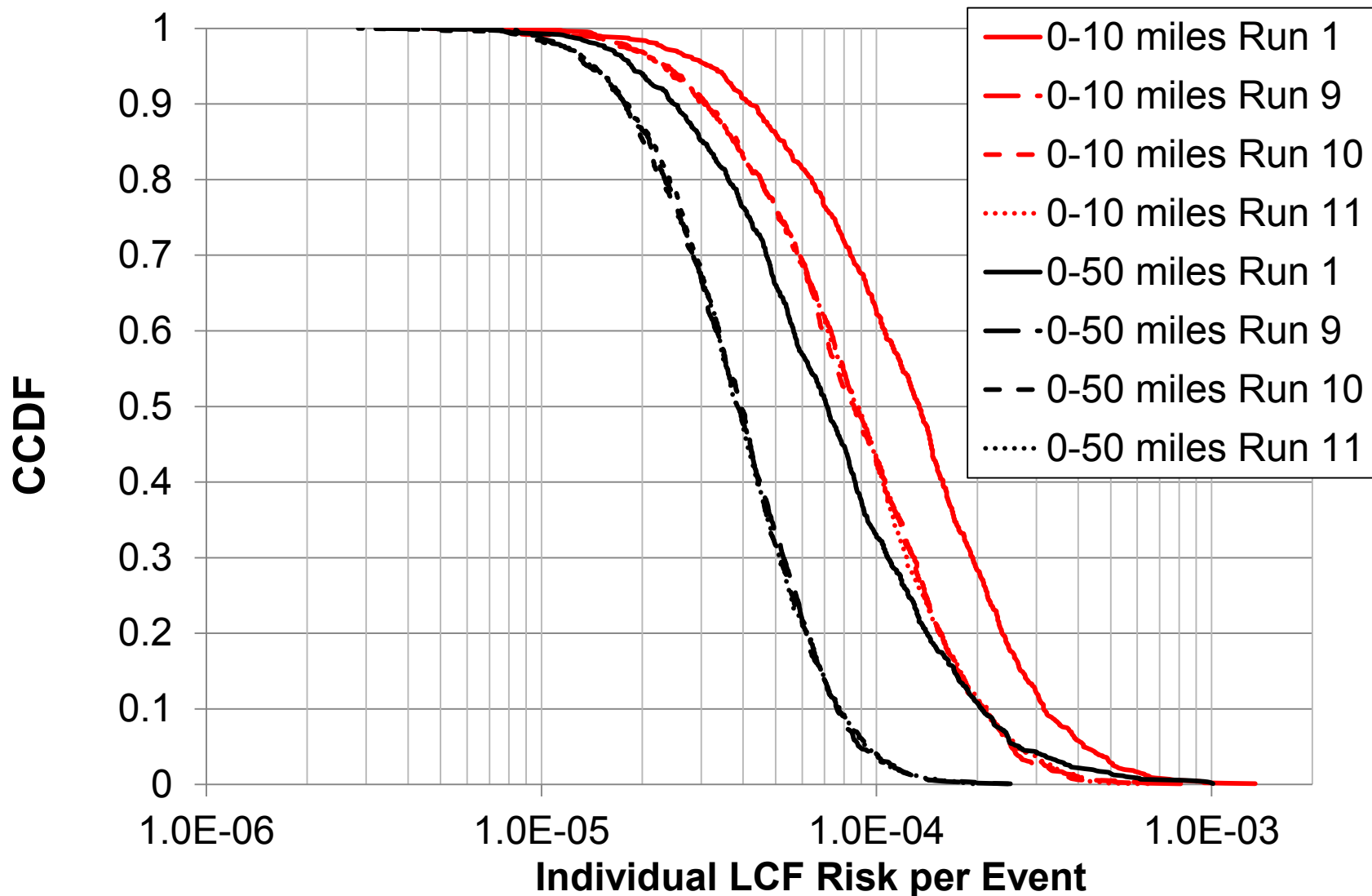
Approach (cont.):

- 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.

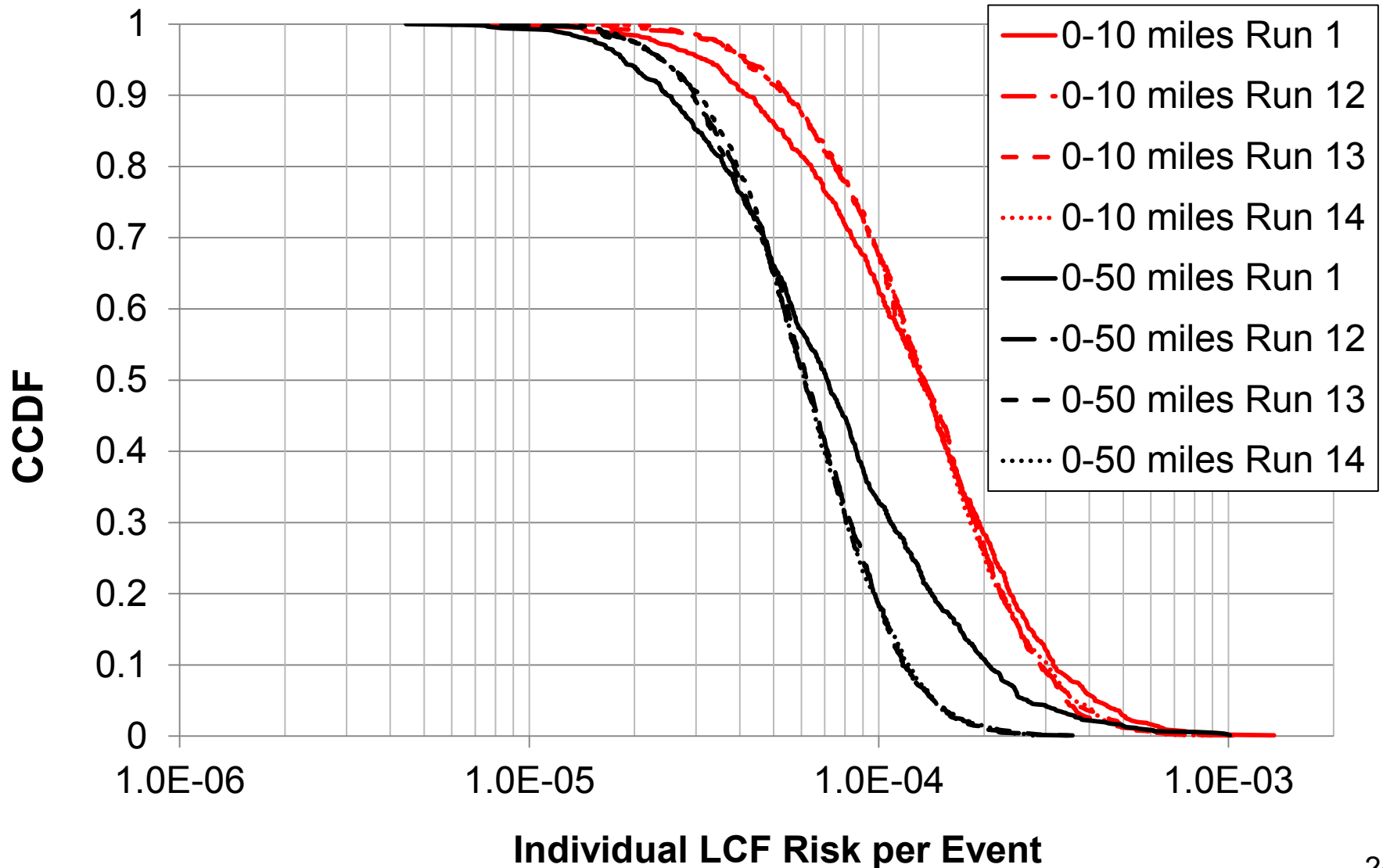
Runs 9-11 (Low Source Term) Conditional, Mean, Individual LCF Risk (per event) Statistics

| 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 |

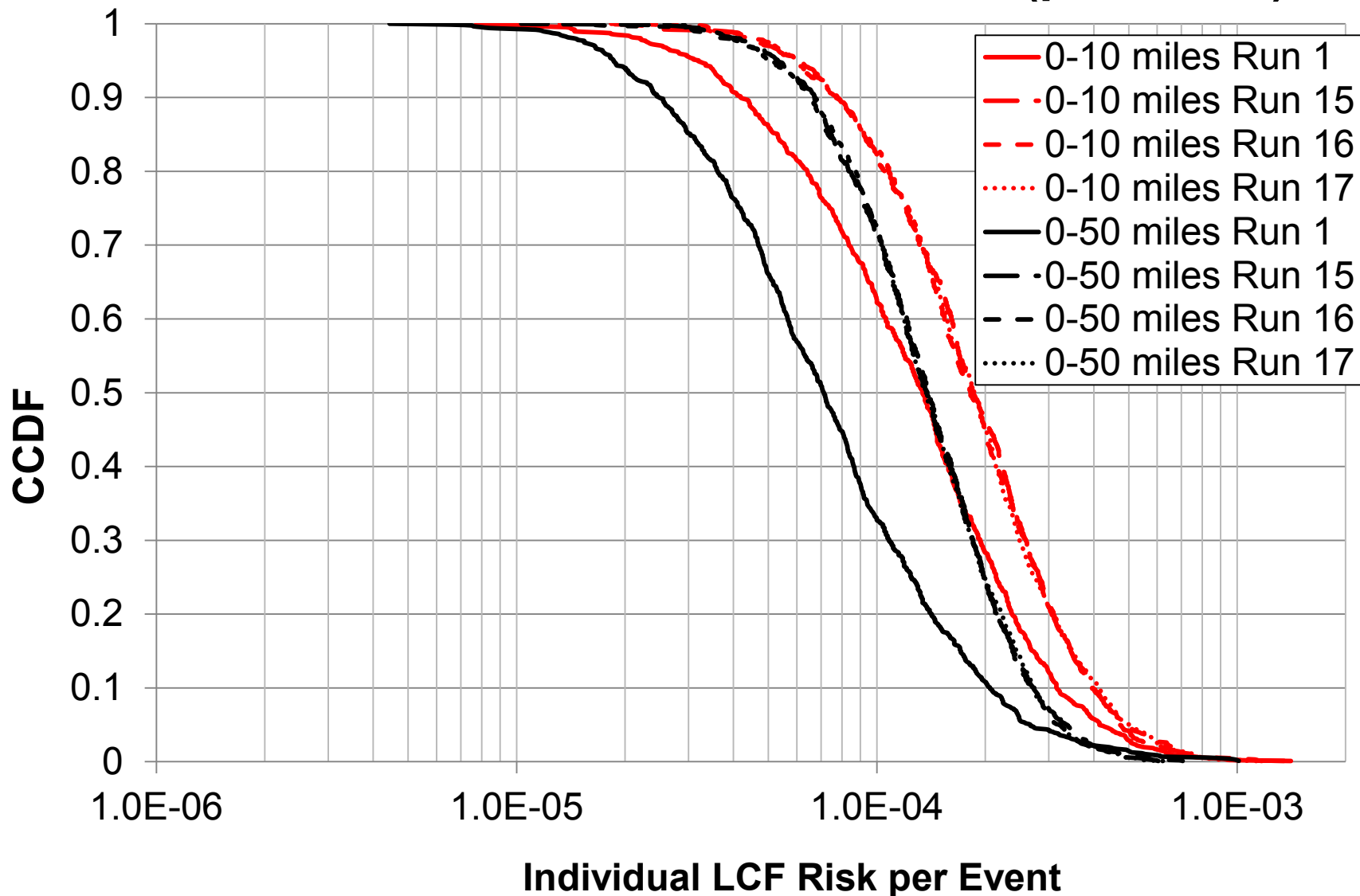
Runs 9-11 and Run 1 Epistemic Uncertainty Conditional, Mean, Individual LCF Risk (per Event) CCDFs



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



Average difference between the three separate LHS runs over all Aleatory Weather Distributions (1st to 99th percentile)

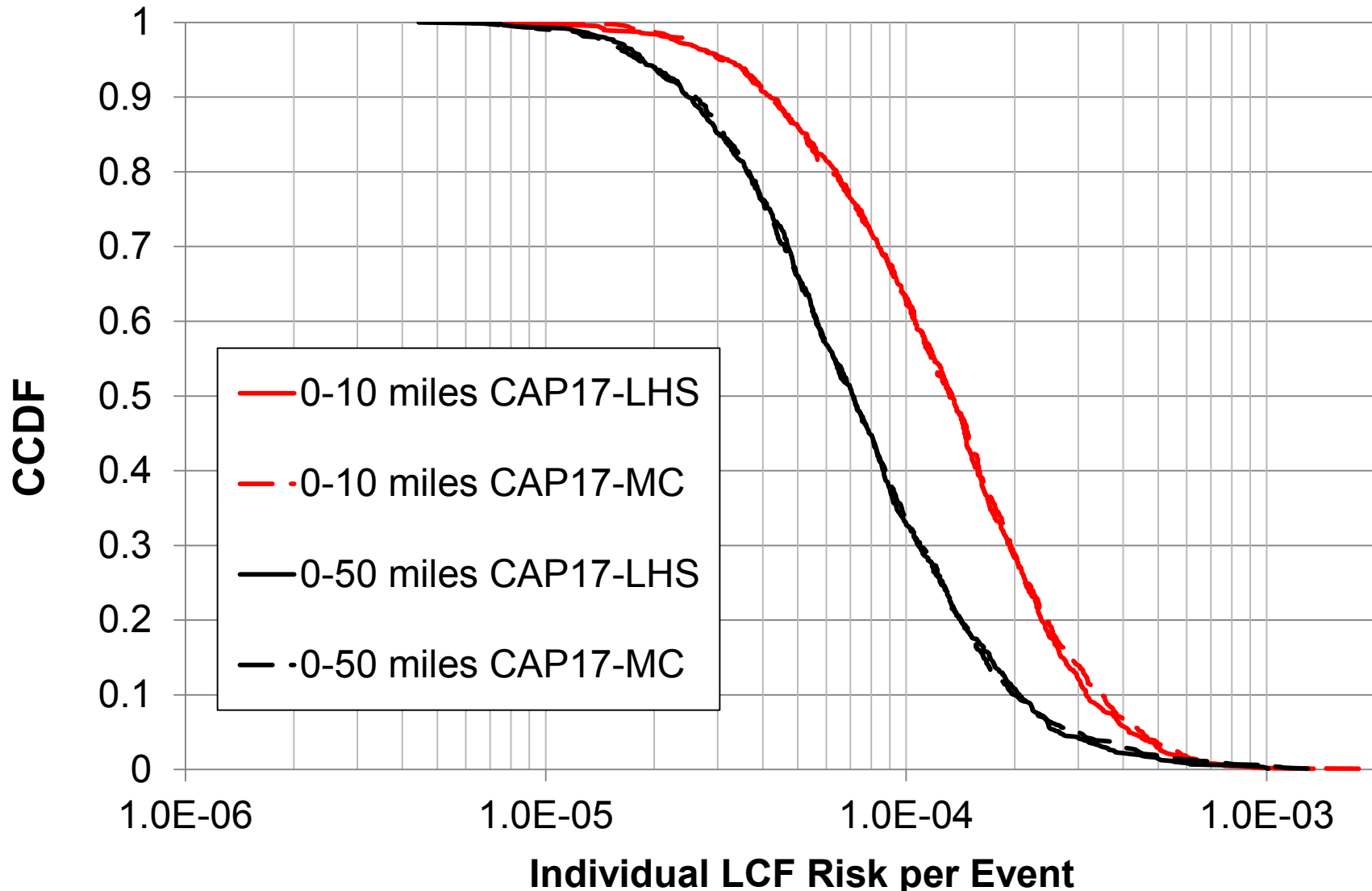
| Source Term | Conditional LCF Risk 0-10 miles | Conditional LCF Risk 0-50 miles |
|---|------------------------------------|------------------------------------|
| Highest Prompt Fatality Risk – Runs 3-5 | 0.8% | 0.8% |
| Highest LCF Risk – Runs 6-8 | 0.8% | 0.9% |
| Low – Runs 9-11 | 0.9% | 0.8% |
| 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

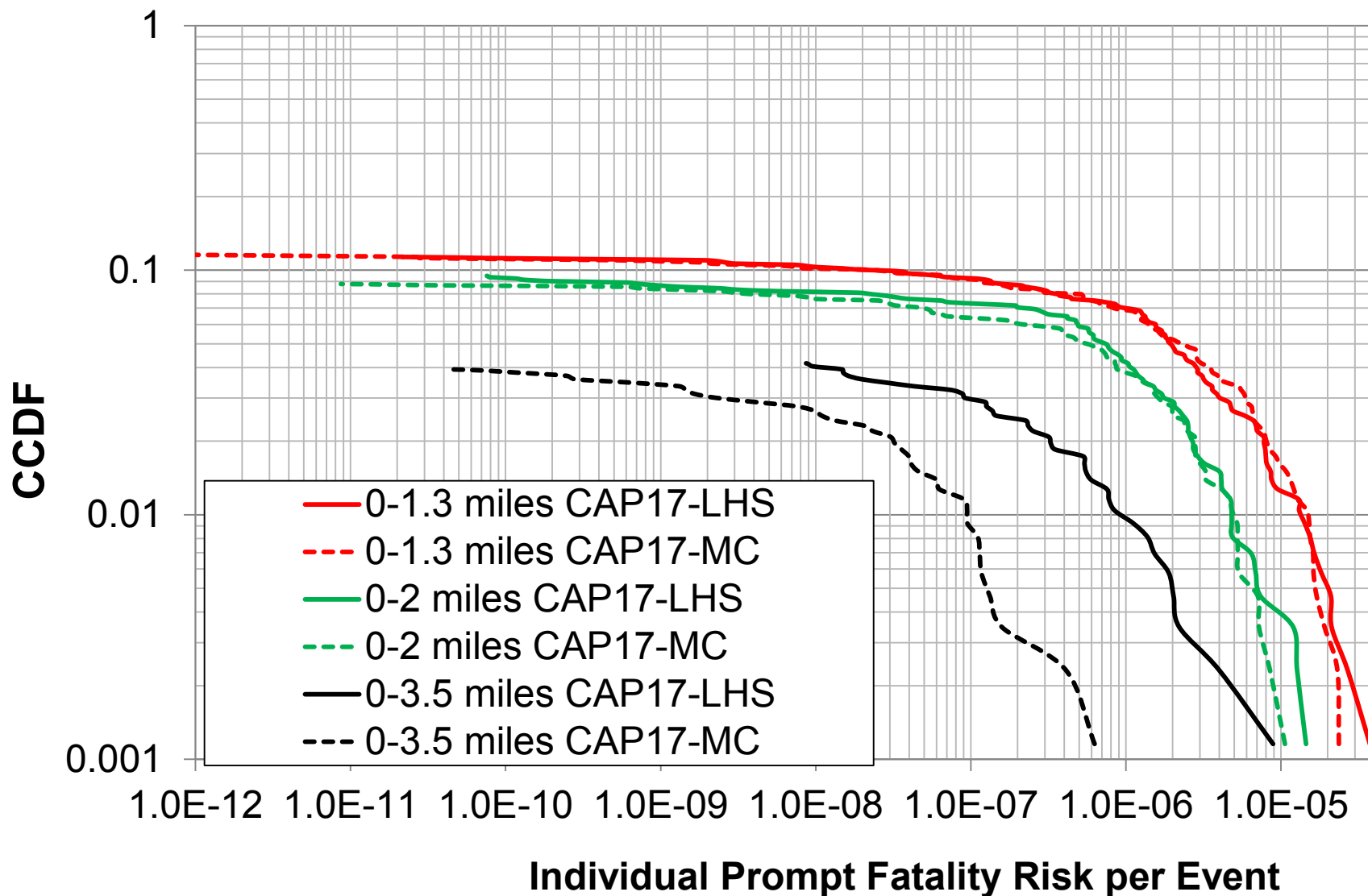
Approach:

- 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

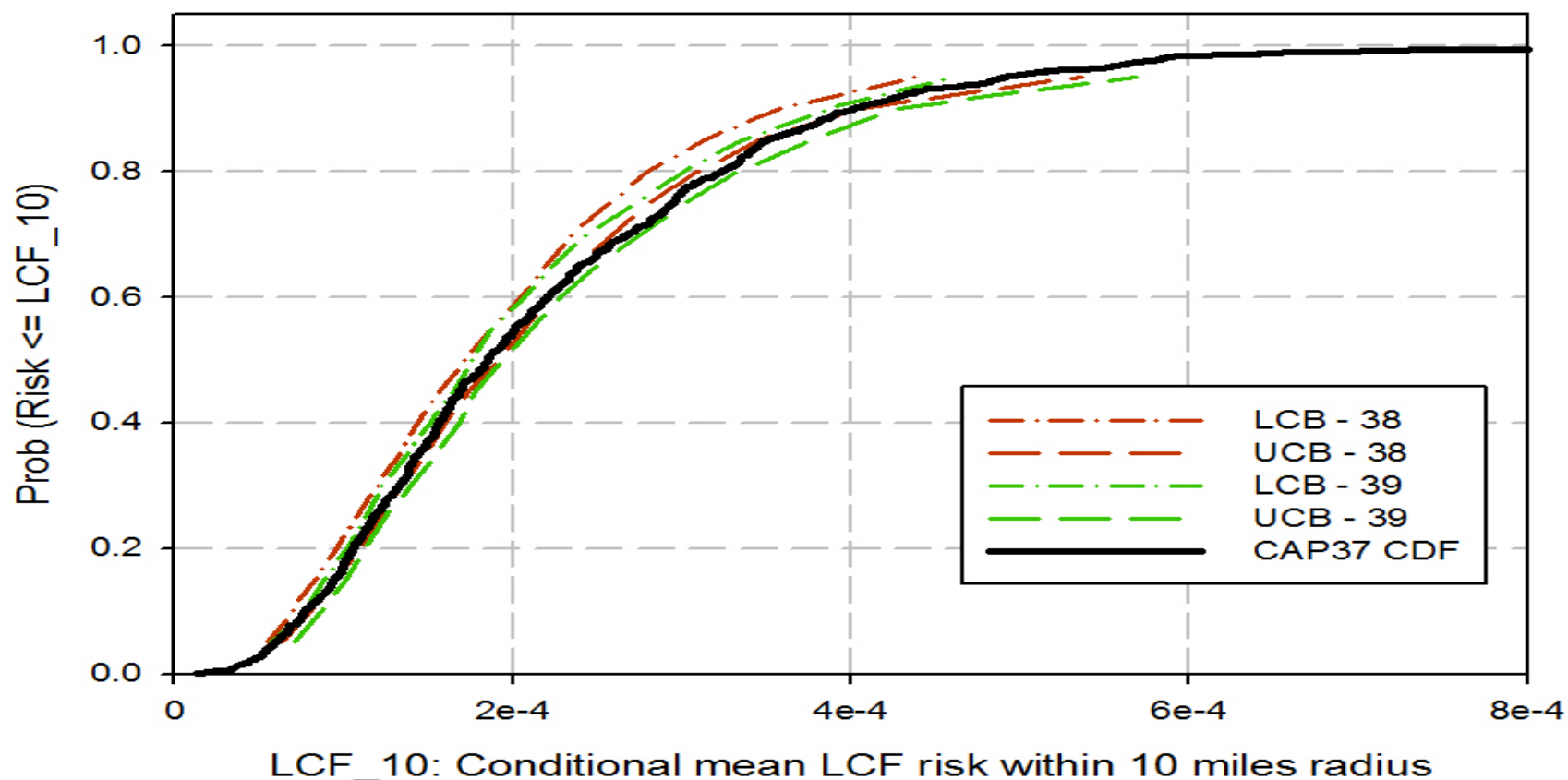


Run 1 (CAP17) Conditional, Mean, Individual Prompt Fatality Risk (per Event) CCDF with LHS and MC Sampling



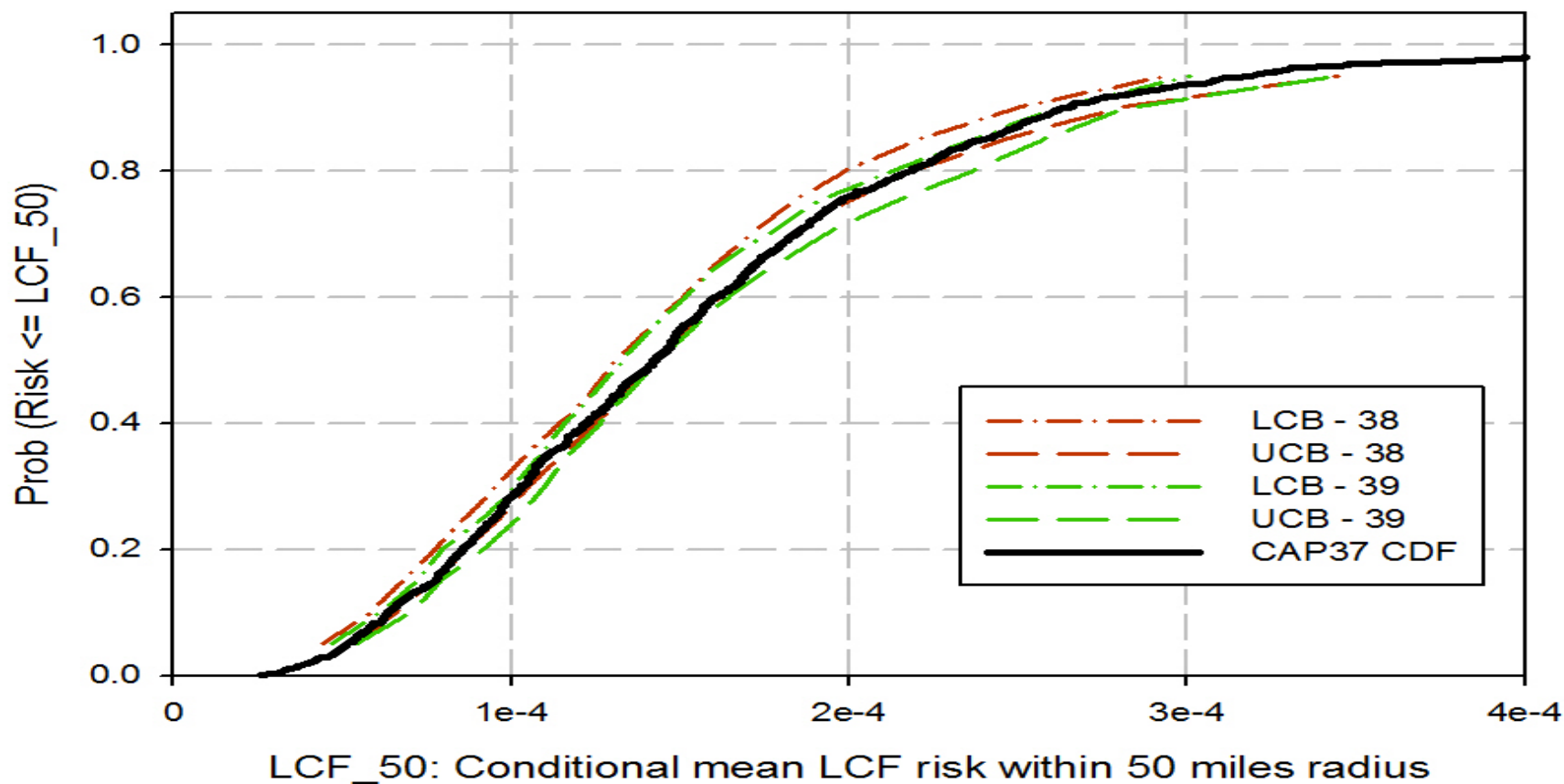
10-mile Conditional, Mean, Individual LCF Risk (per Event) CDF for Run 15 (CAP37) and 95% Confidence Interval Upper and Lower Bounds for Runs 16 & 17 (CAP38 & 39) with SRS

Position of CAP 37 CDF
within CAP 38 and CAP39 95% confidence bounds



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

Position of CAP 37 CDF
within CAP 38 and CAP39 95% confidence bounds





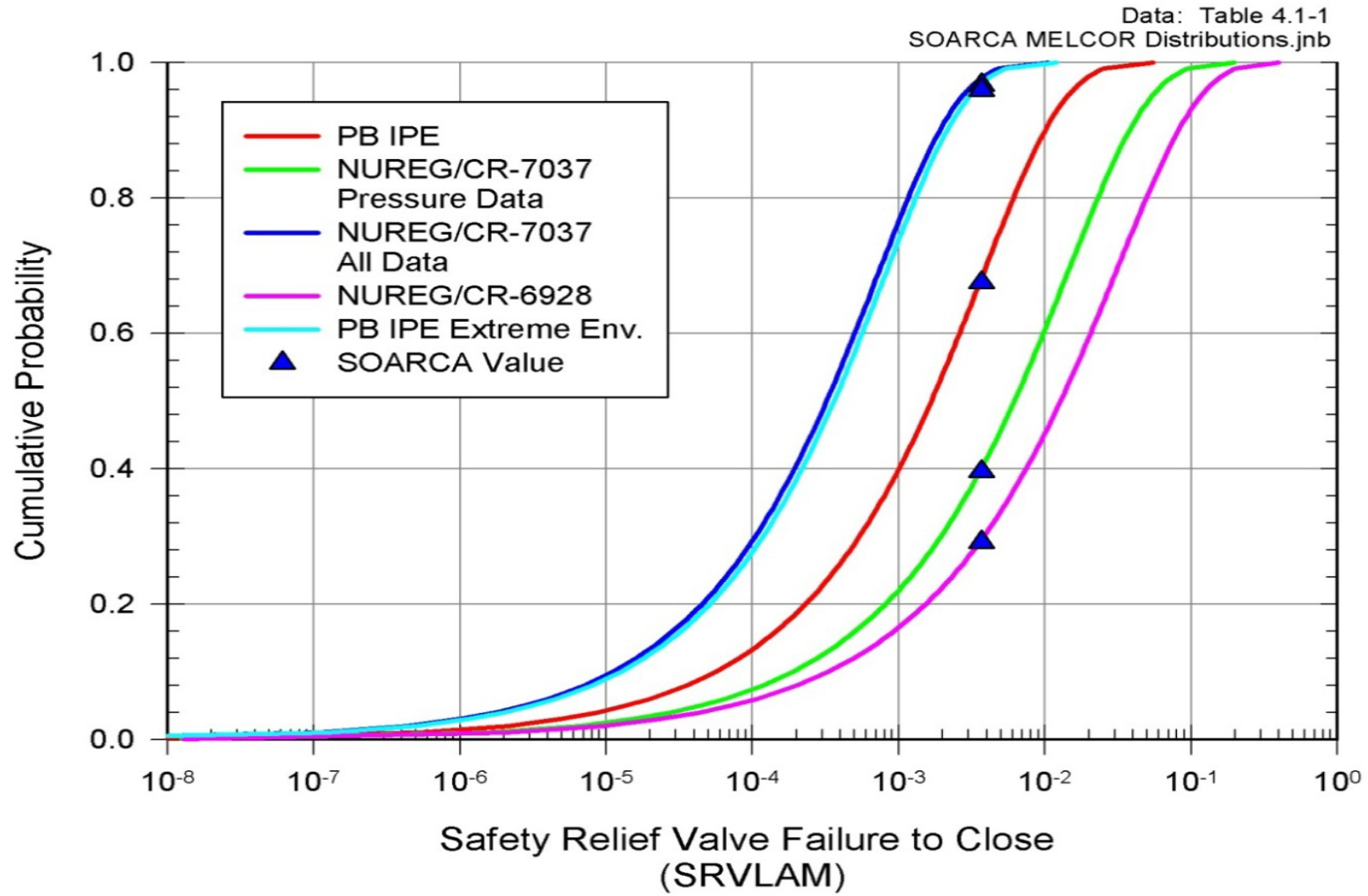
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MELCOR Parameters of Interest

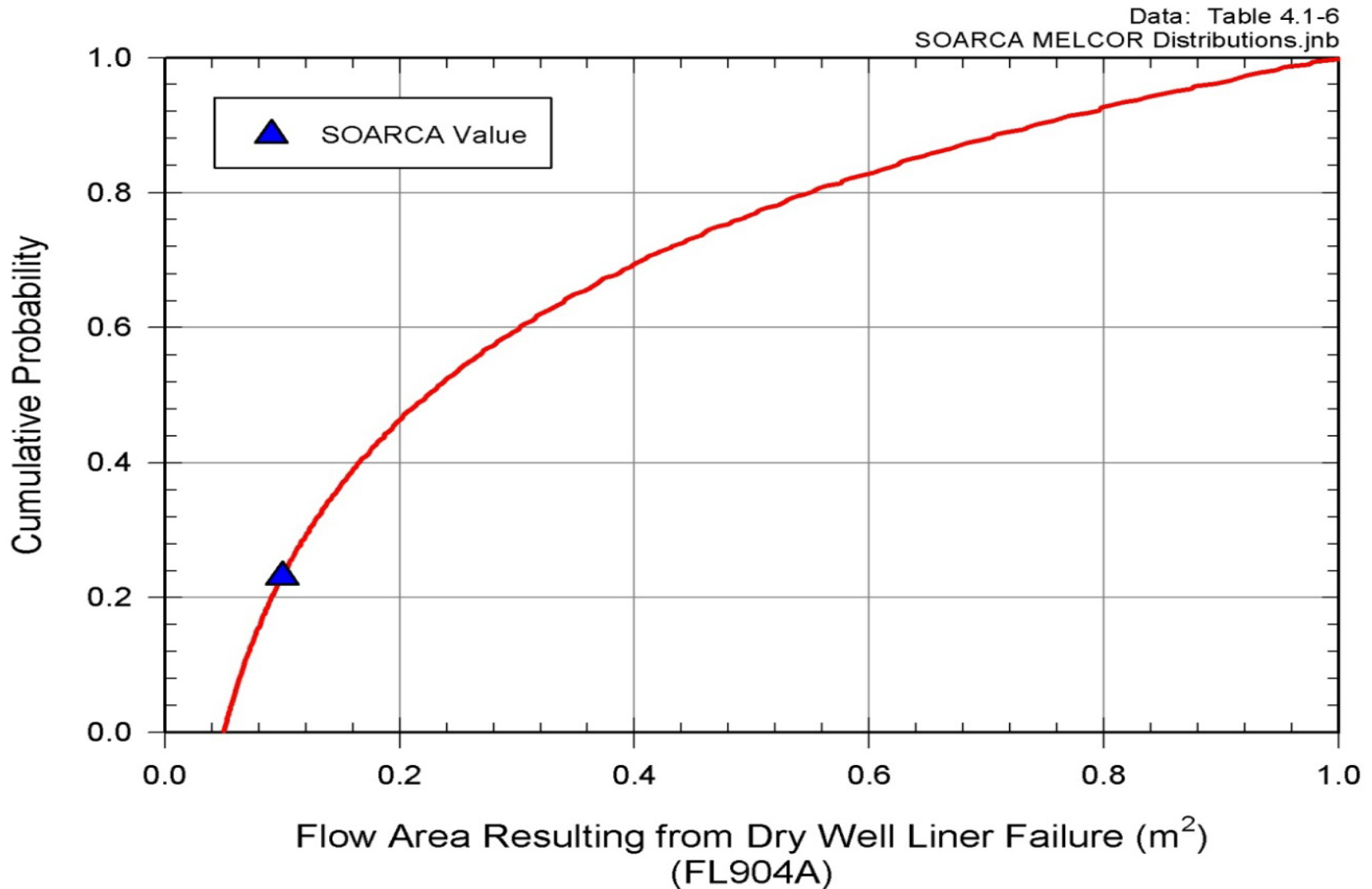
SRVLAM – SRV stochastic failure to reclose



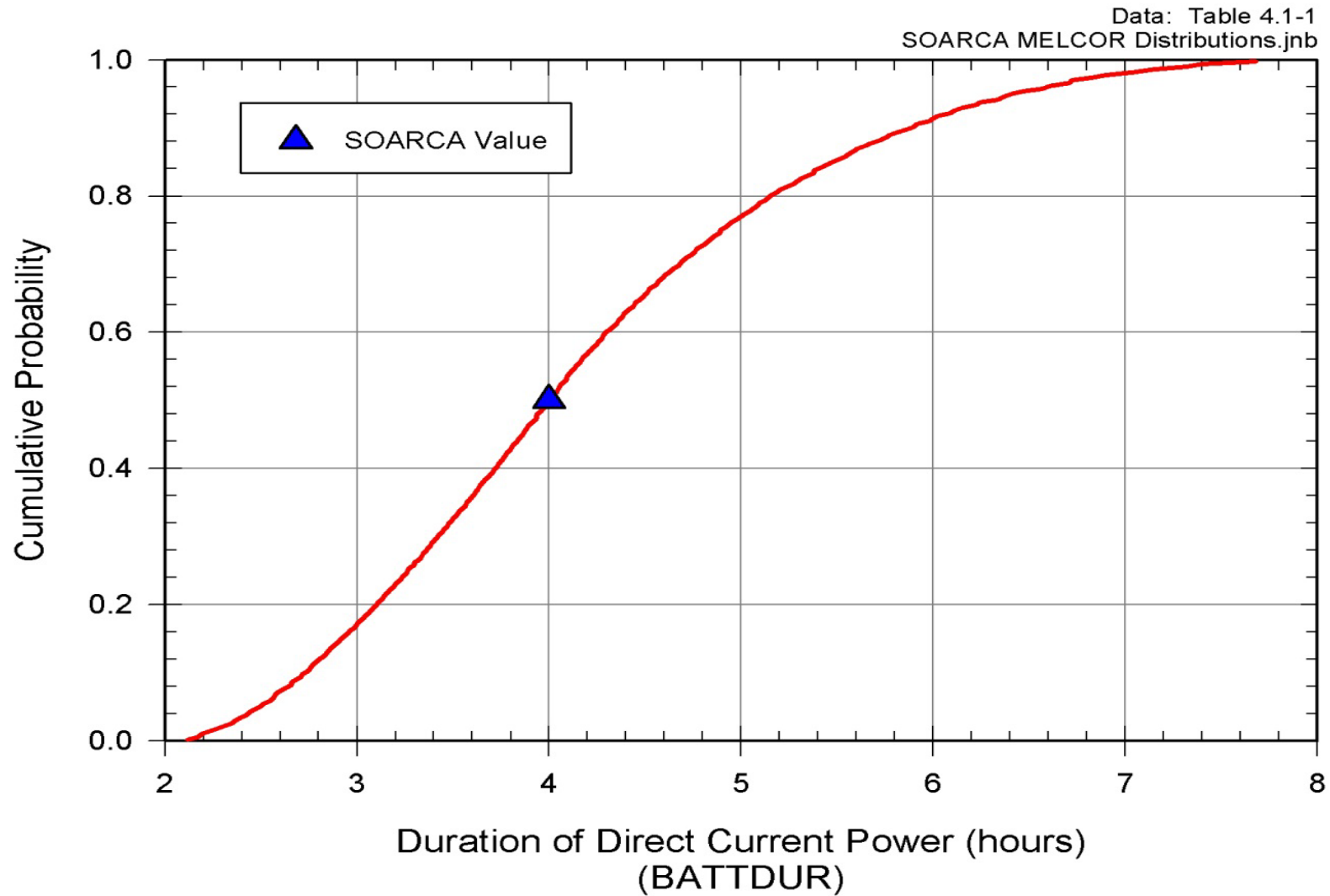
CHEMFORM – Iodine and cesium fraction

| Parameter | | | Distribution | | |
|---|-----------------|----------|---|----------|--------------------------------------|
| CHEMFORM: Five alternative combinations of RN classes 2, 4, 16, and 17 (CsOH, I ₂ , CsI, and Cs ₂ MoO ₄) | | | Discrete distribution Combination #1 = 0.125 Combination #2 = 0.125 Combination #3 = 0.125 Combination #4 = 0.125 Combination #5 = 0.500 | | |
| 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. | | | | | |
| Five Alternatives | | | Species (MELCOR RN Class) | | |
| | | CsOH (2) | I ₂ (4) | CsI (16) | Cs ₂ MO 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

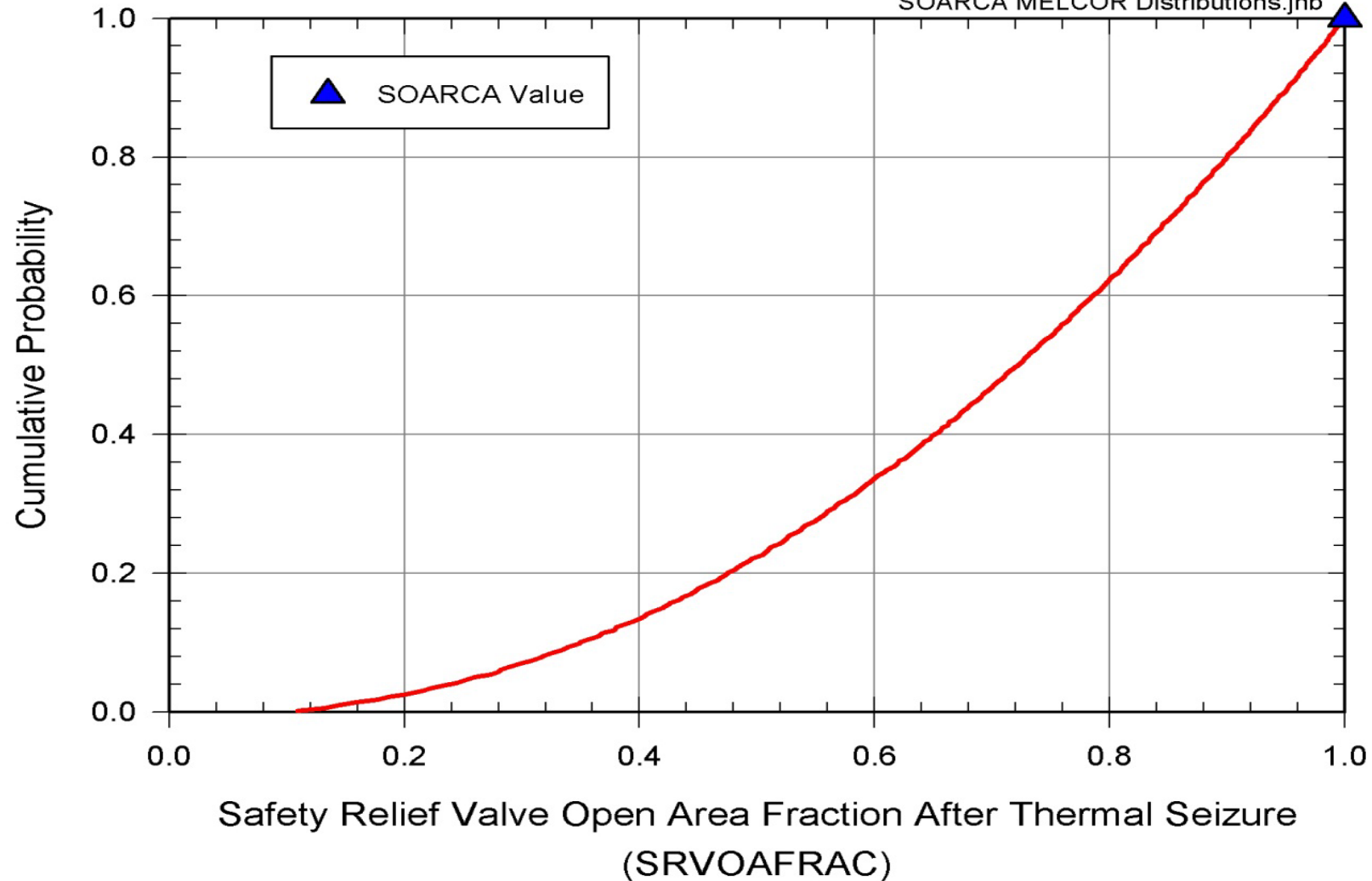


BATTDUR – Battery Duration

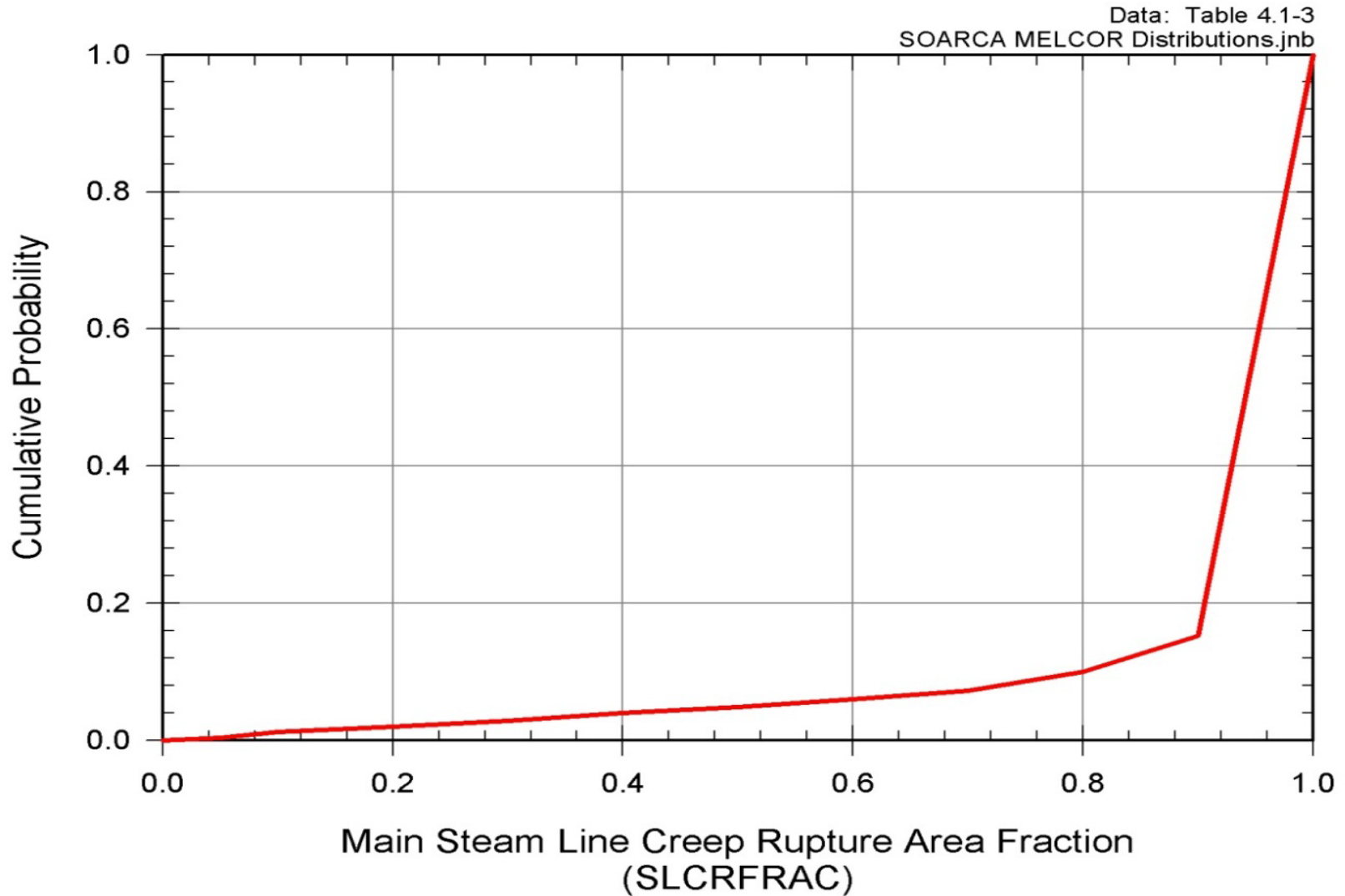


SRVOAFRAC – SRV open area fraction

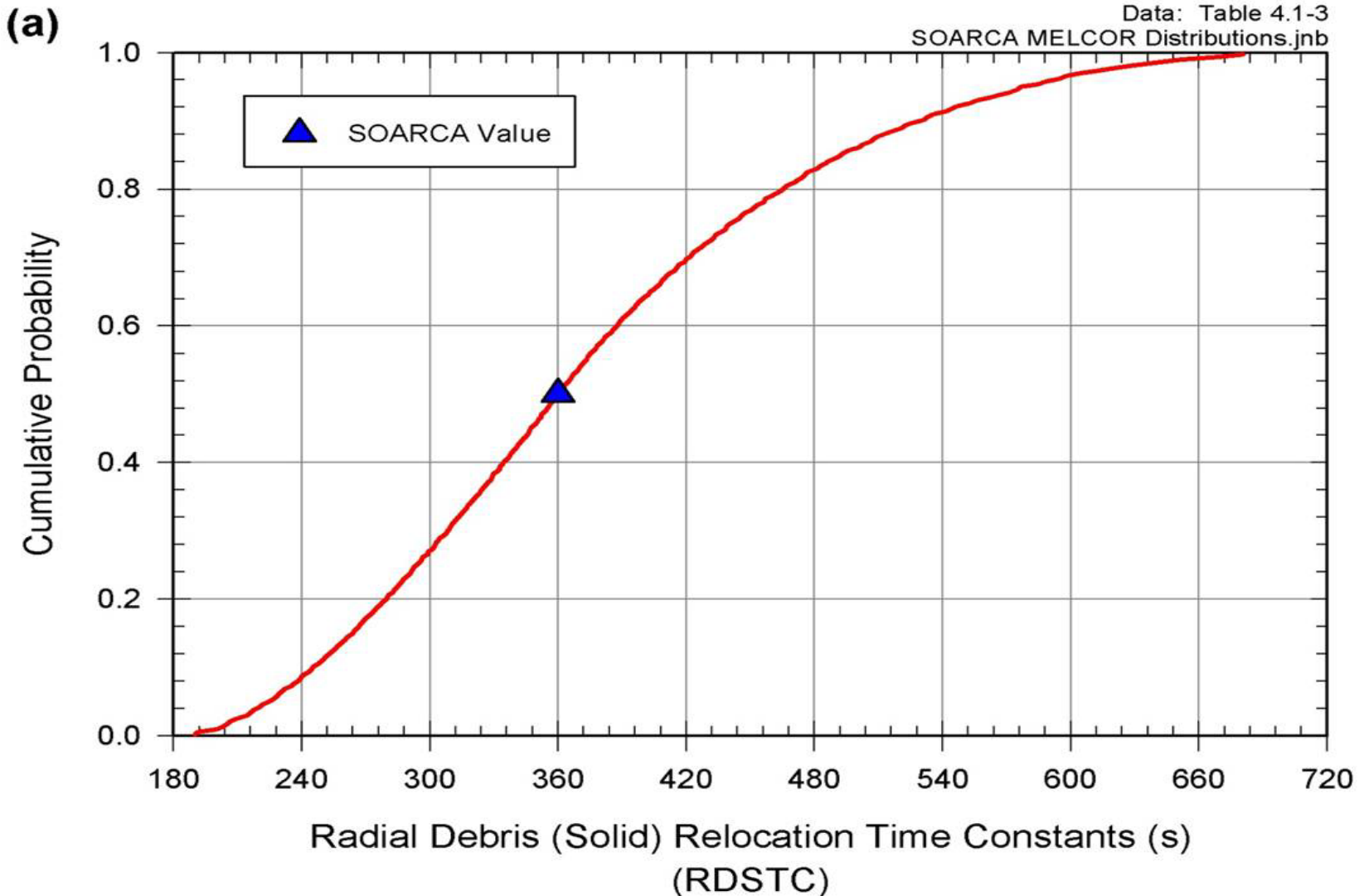
Data: Table 4.1-3
SOARCA MELCOR Distributions.jnb



SLCRFRAC – Main steam line creep rupture area fraction

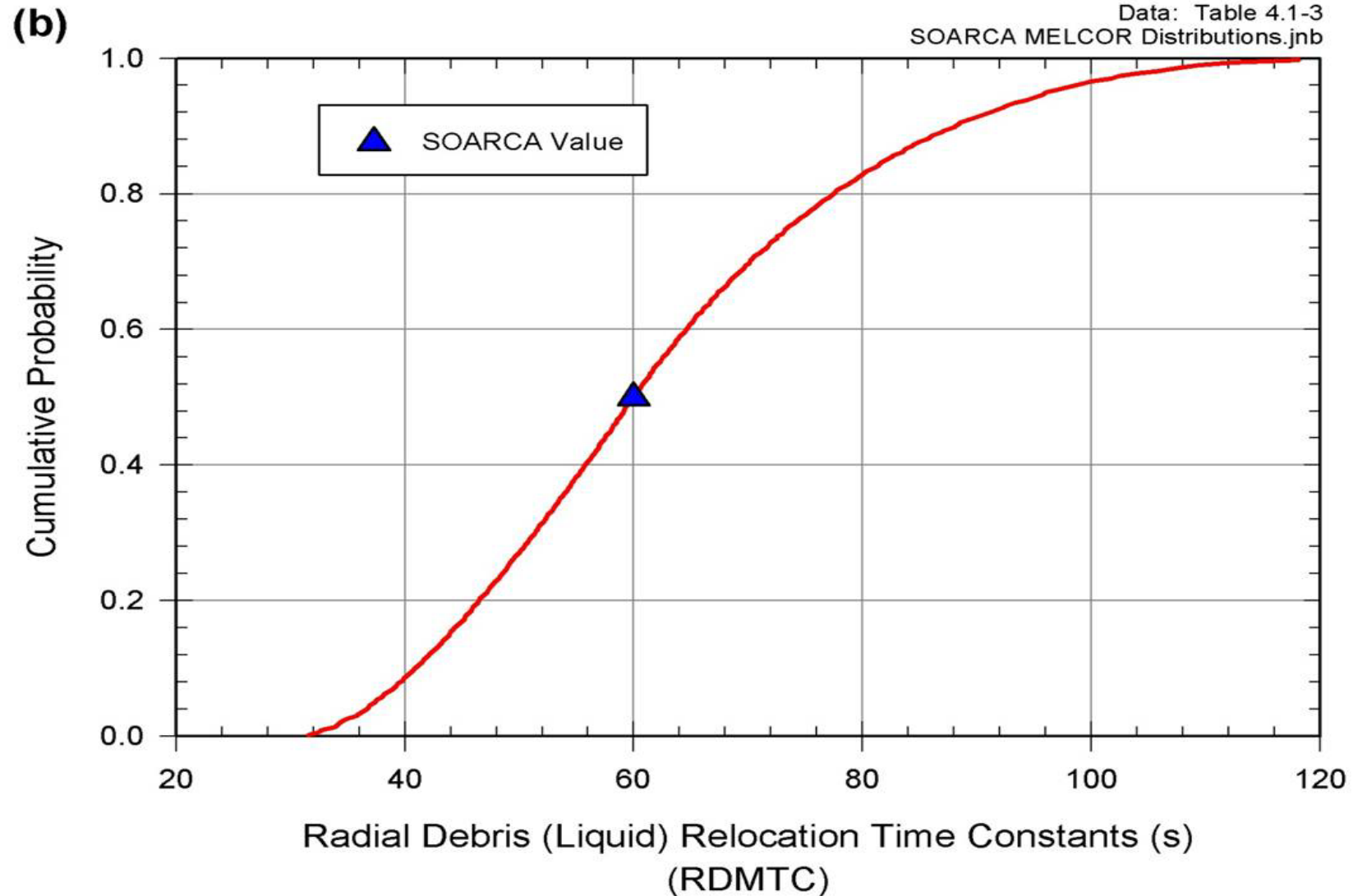


Radial debris relocation time constants – RDSTC (solid)

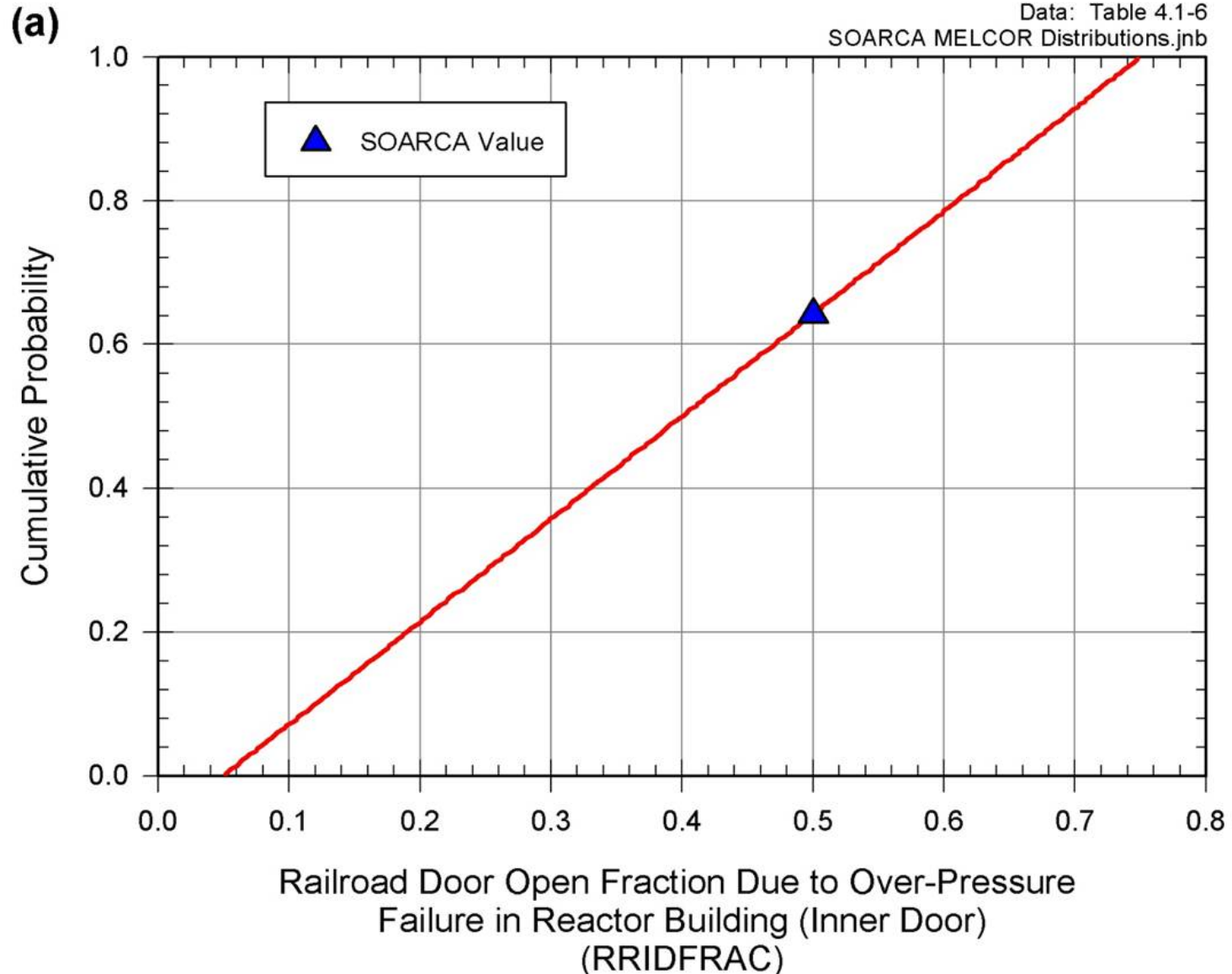




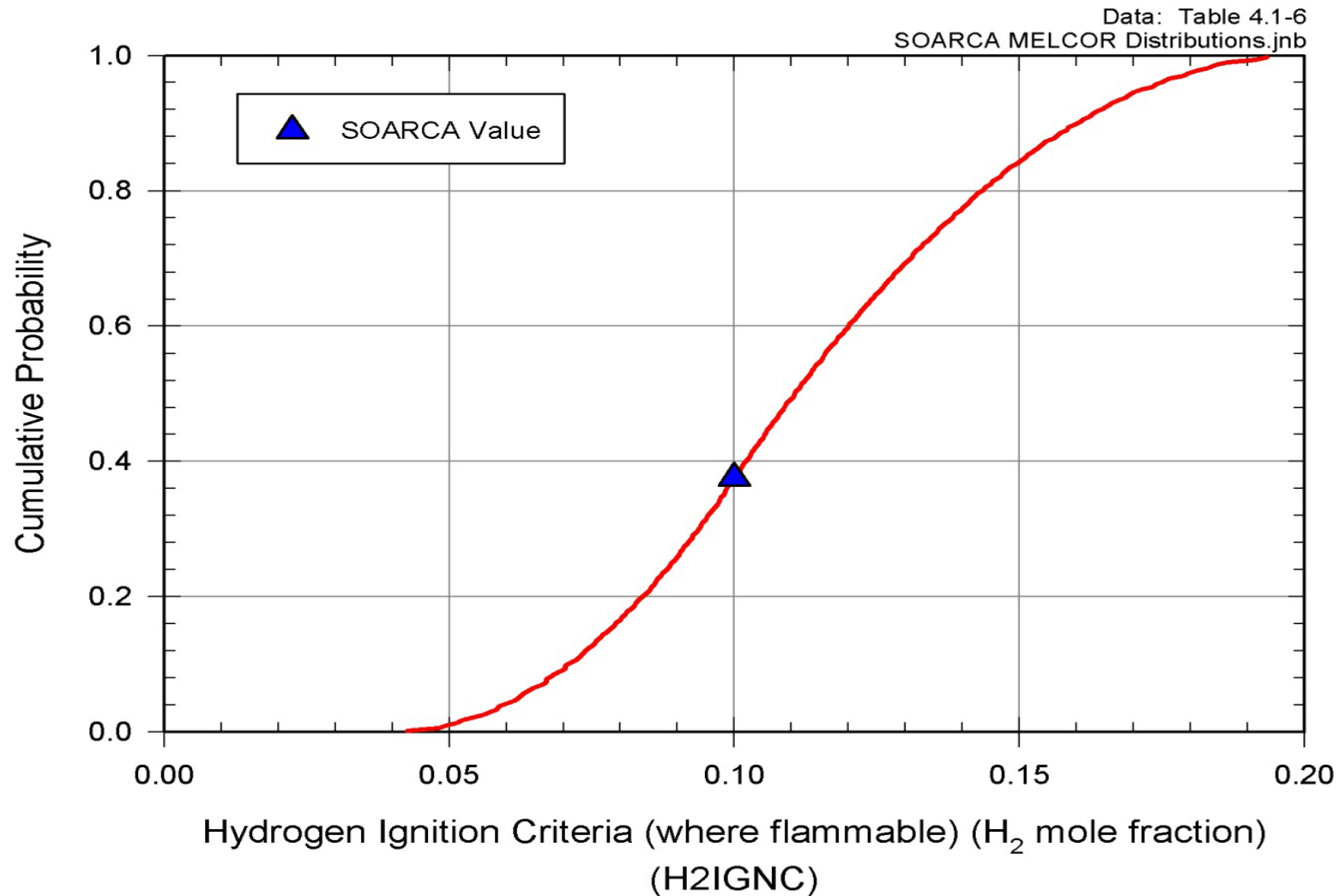
Radial debris relocation time constants – RDMTC (liquid)



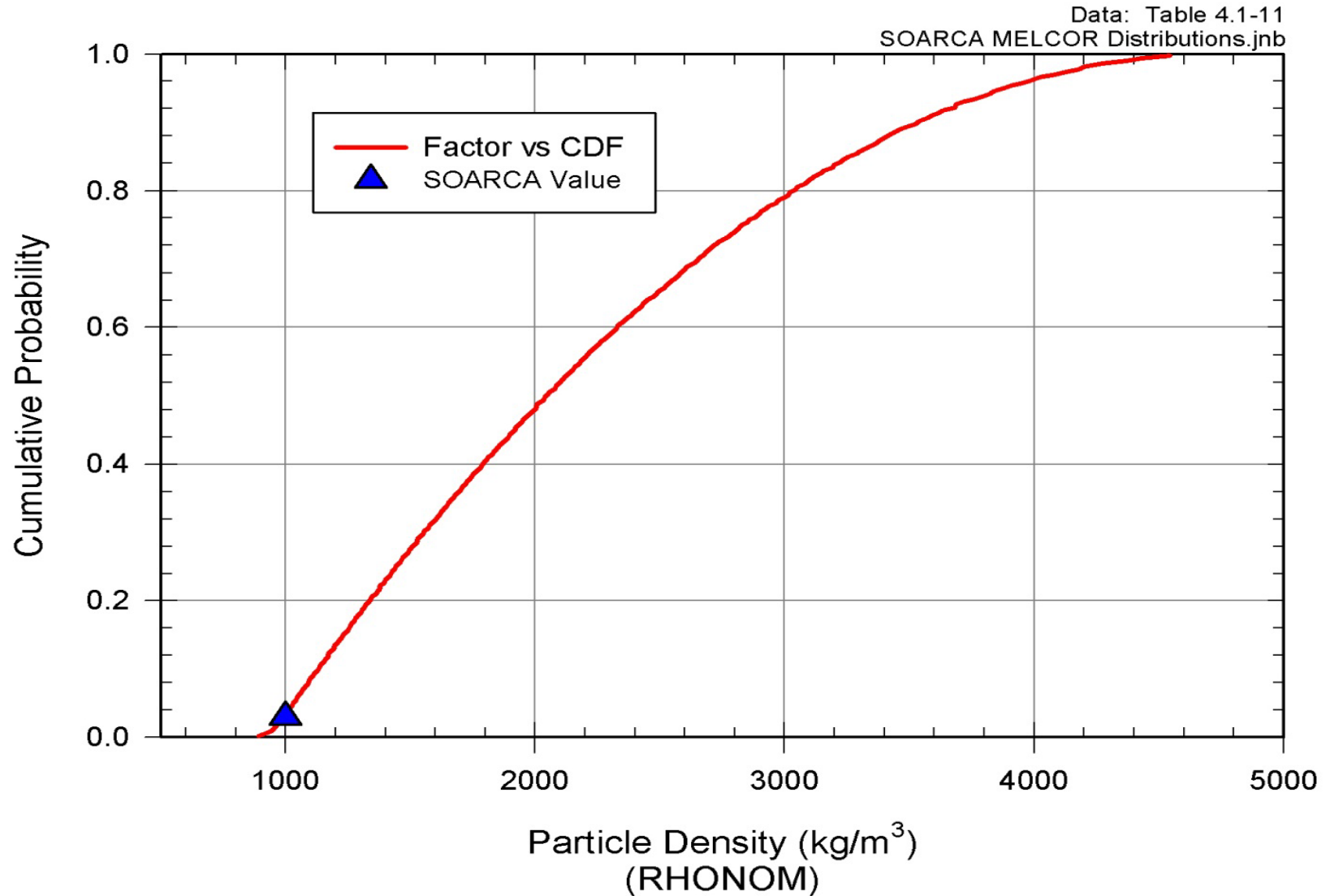
RRIDRFRAC, RODRFRAC – Railroad door open fraction



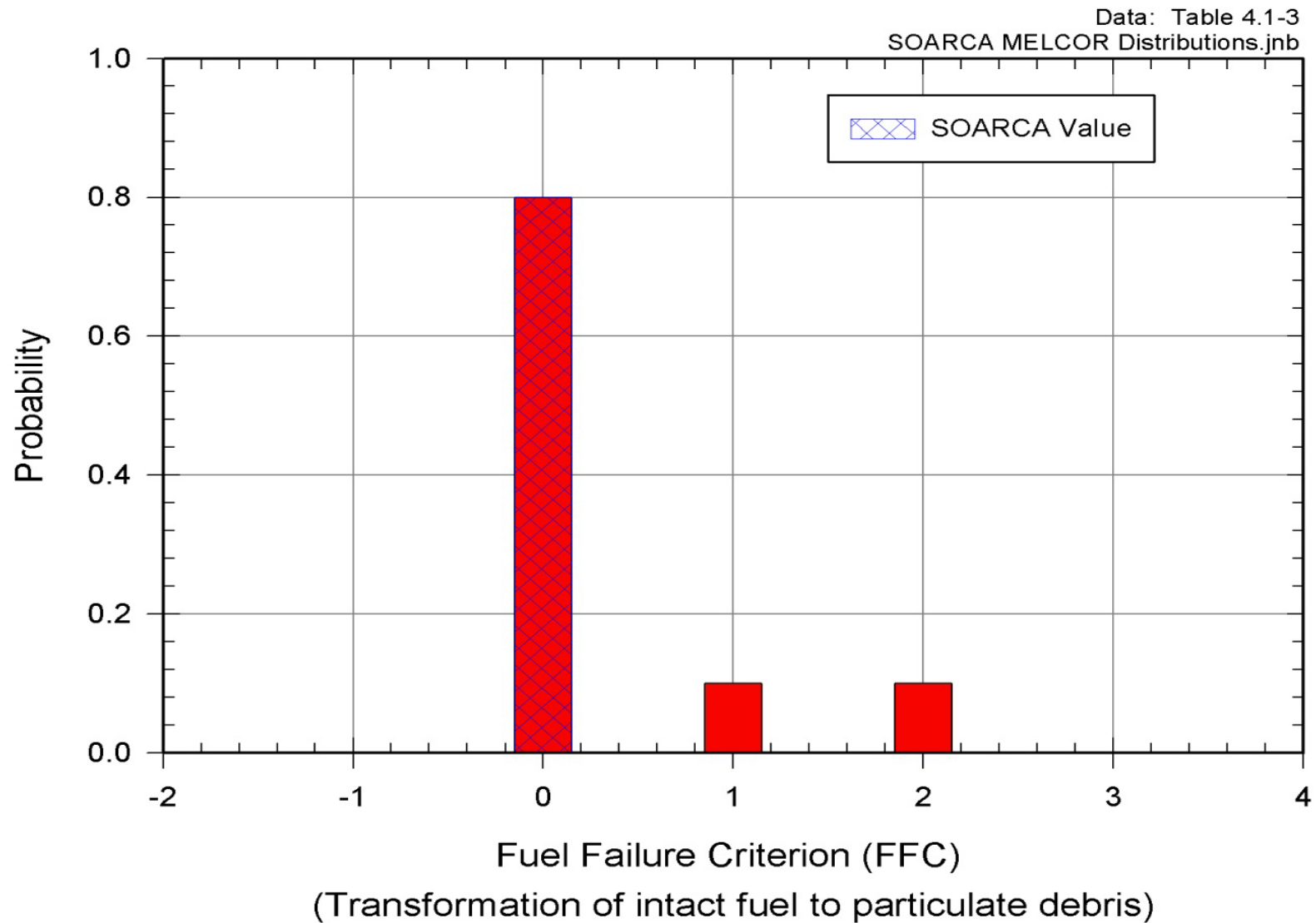
H2IGNC – Hydrogen ignition criteria



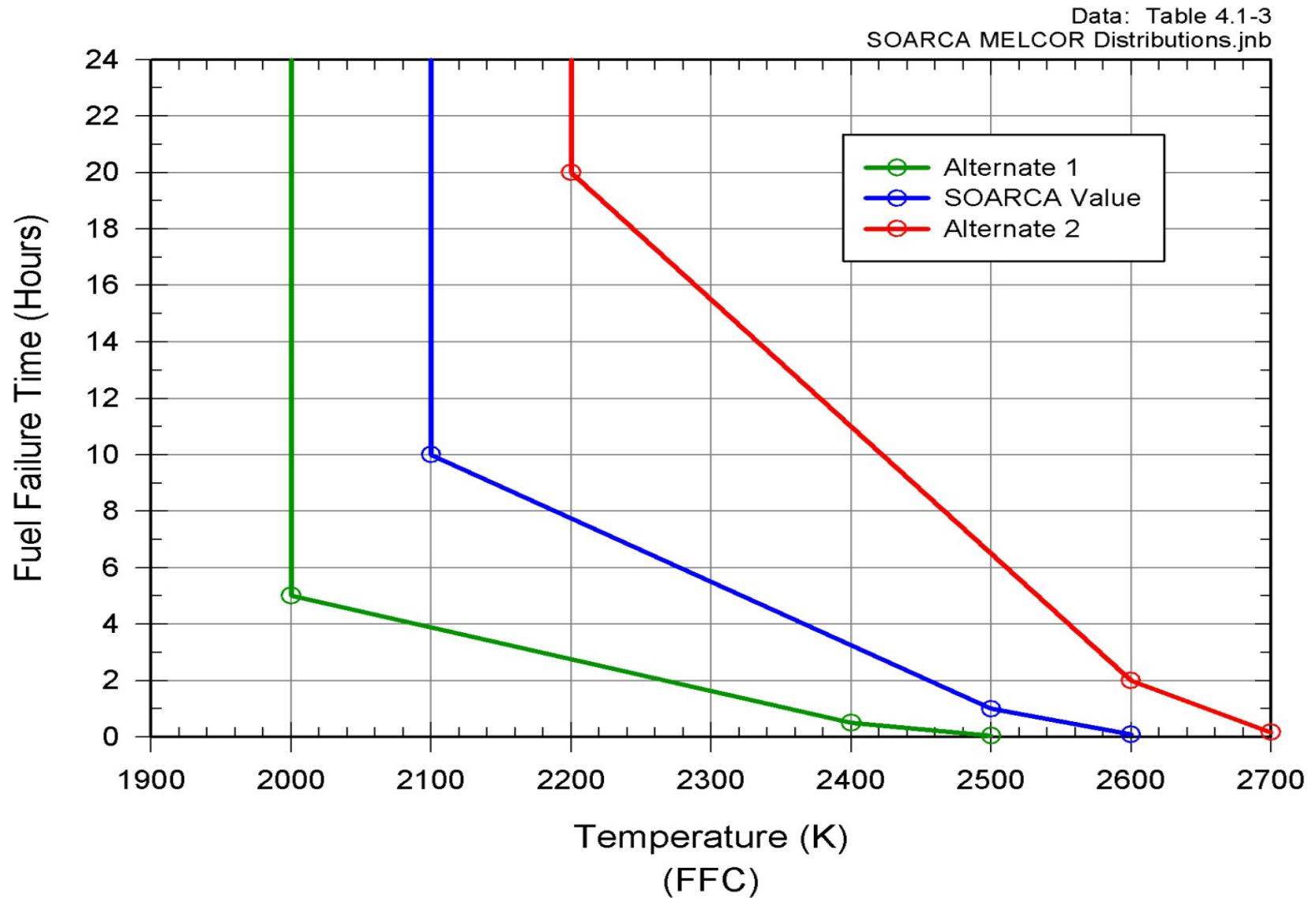
RHONOM – Particle density



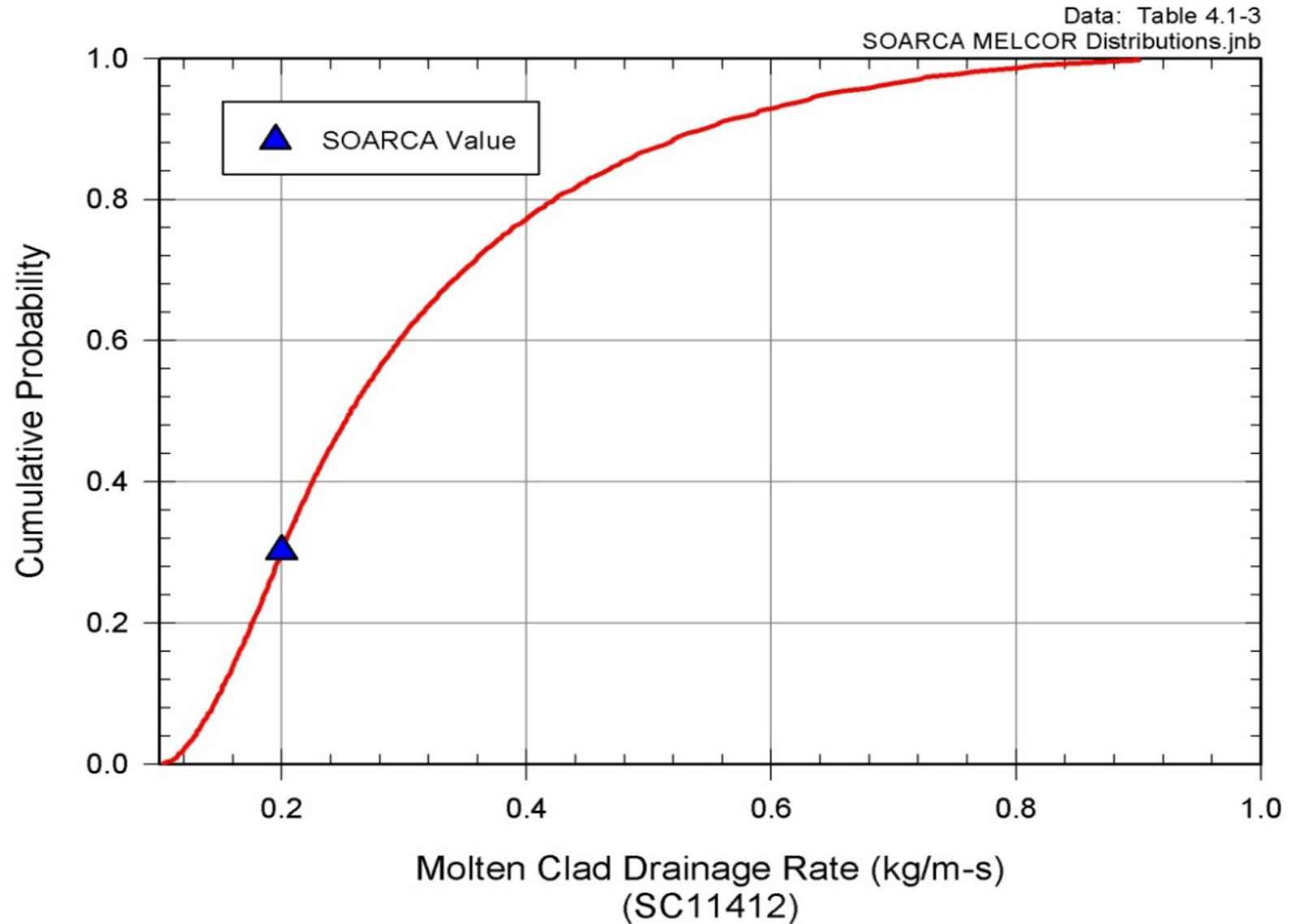
FFC – Fuel failure criterion



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



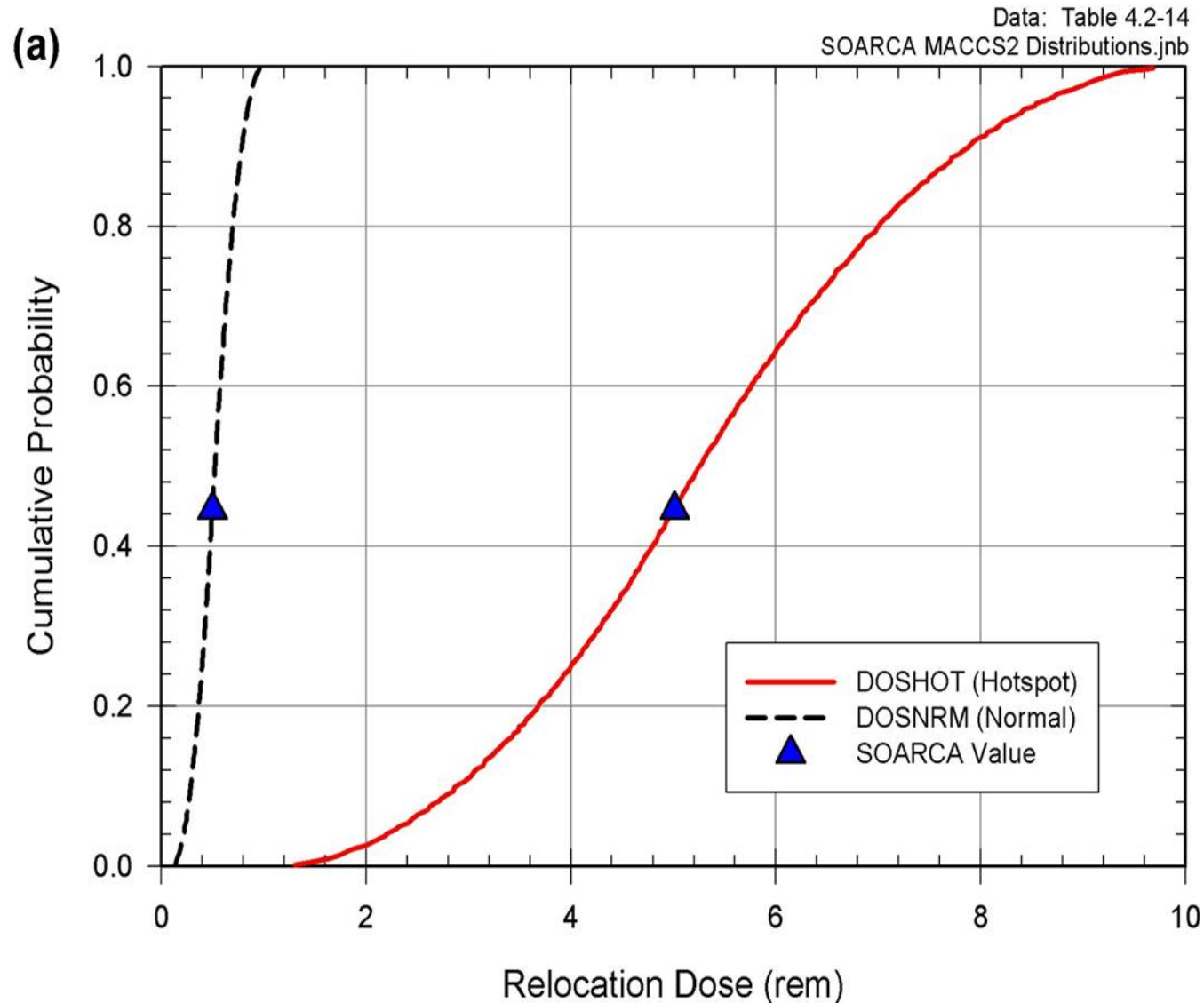
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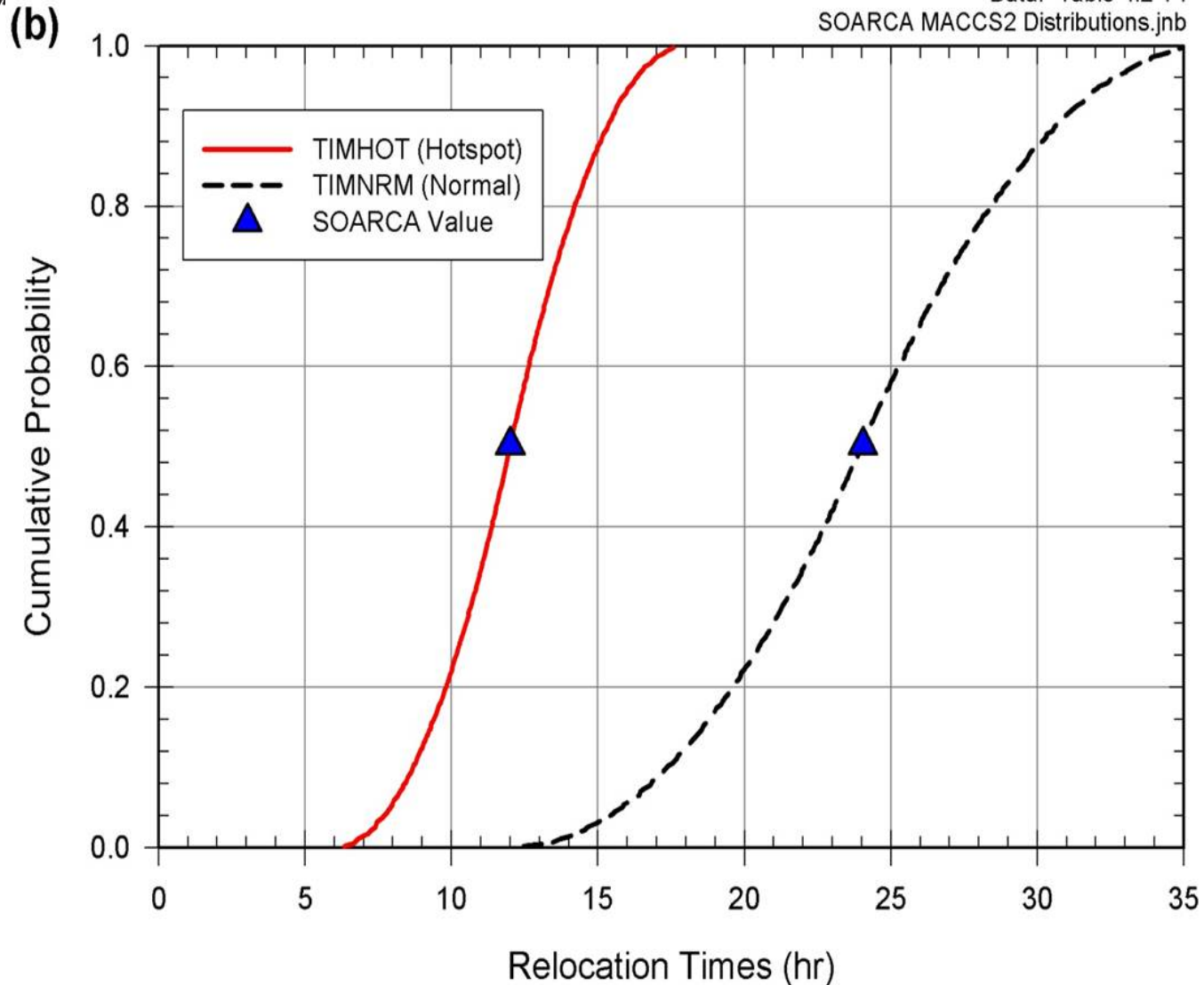
MACCS2 Parameters of Interest

DOSNRM, DOSHOT – Normal and Hotspot Relocation Doses

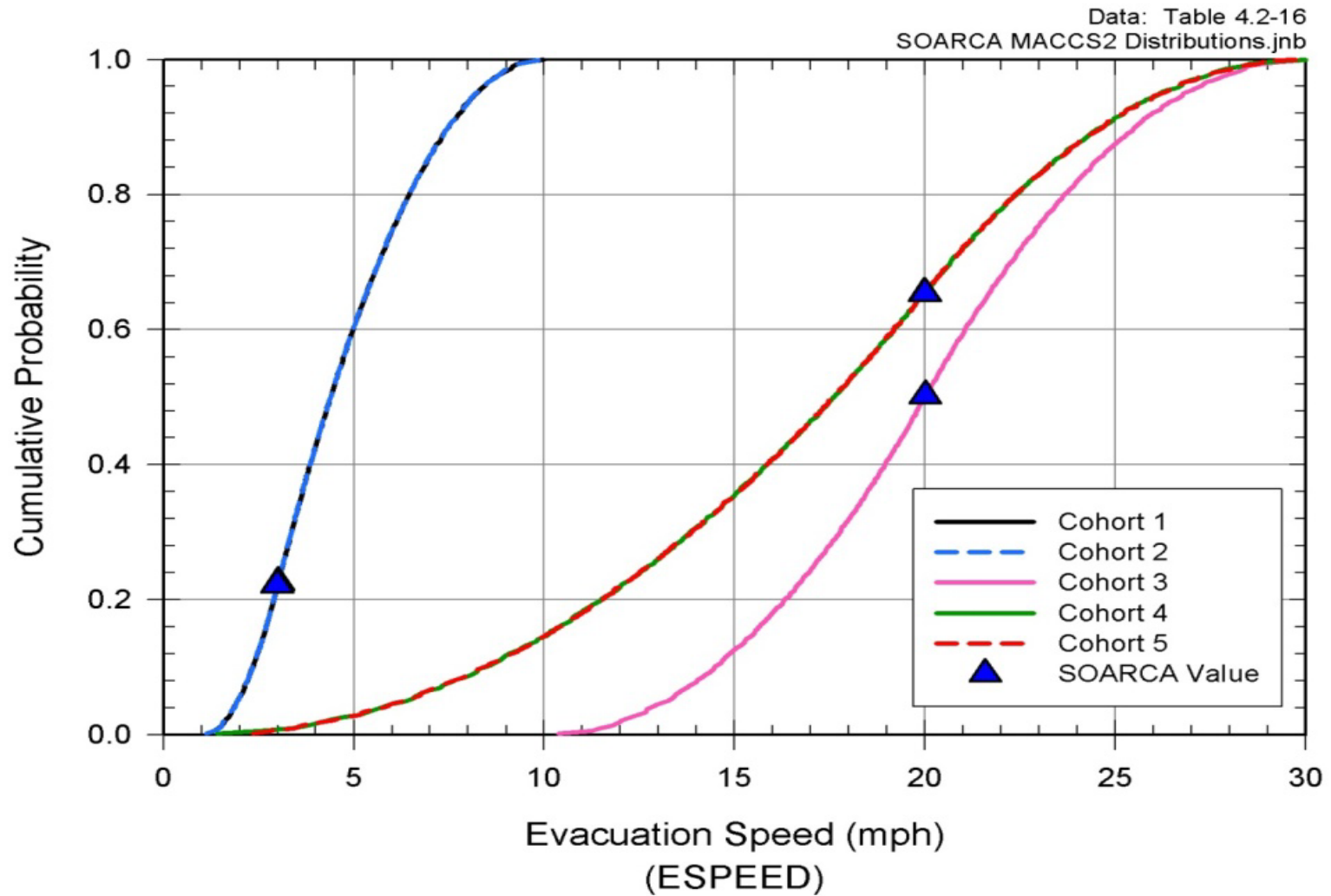




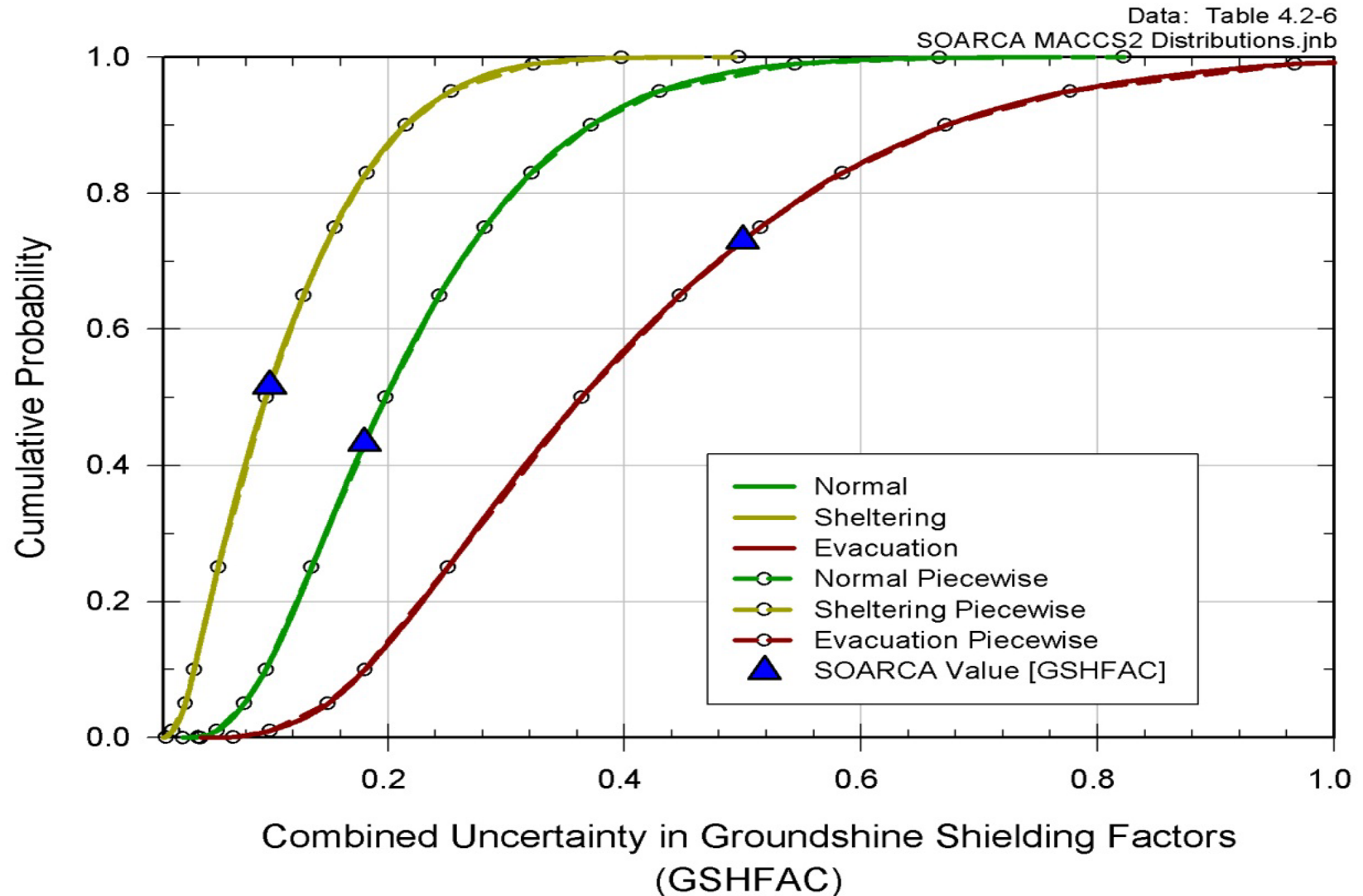
TIMNRM, TIMHOT – Normal and Hotspot Relocation Times



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 $\sim 3 \times 10^{-6}$ per reactor year.