

HLWYM HEmails

From: Michael Waters
Sent: Friday, August 11, 2006 10:06 AM
To: Christopher Ryder; Rosemary Reeves; Albert Wong
Subject: Help Needed: Final draft PCSA ISG

Importance: High

Albert/Chris/Rosemary,

Can you help me resolve three of Brit's comments. Below is my long-winded discussion/concerns.

A 30-min discussion this afternoon may be beneficial.

Thanks,

Mike

1. Brit refers to the SOC for Part 63 (66 FR 55742) when discussing the use of mean reliability estimates in the ISG body. I'm not clear what our position is? Is the position that the mean release probability can be used to categorize event sequences, taken into account uncertainties and limitations. And/or that mean reliability estimates can be used to categorize event sequences? No one has told me if you can multiply the mean by the mean by the mean for each SSC to determine the mean event sequence???

I could find no discussion of mean estimates in the SOC. In addition, 66 FR 55742 was written in the context of using point estimates for dose calculations.

Based on direction from the HLWRB, I am putting into the main ISG body that "The use of mean release frequencies to categorize events in the PCSA is acceptable with adequate consideration of uncertainties and the limitations associated with reliability methods and data."

2. Brit's comment to replace every use of "probability" with "frequency" in Appendix A is perplexing. I thought we had a common understanding. I am still concerned that there is a belief that the Category 1 and 2 definitions were purposely written for two different statistical concepts of expectation versus chance!! I highly doubt this was the intent of the Commission!!

In the simplest terms of the risk-triplet definition, we should be distinguishing between events that are more likely ("expected") vs. less likely ("unexpected but has a chance"). This is what the facility must be designed to protect against in terms of dose objectives.

Why can't we unify the concepts together, but recognize that frequency and probabilities estimates may be subsidiary measures for estimating likelihood in terms of Event Categorization (1 & 2). Part 63 is meant to reasonably protect the public and workers, not become a statistical debate about correct terminology.... Will it change the regulatory conclusion?

At this point, I am not inclined to make the cut and replace substitutions for frequency vs. probability in Appendix A.

3. Finally, note Brit's comment of why we shouldn't discuss Event Categorization. Second, please look at the seismic ISG that categorizes event sequences for seismic hazards!! Does Brit's philosophy apply to the seismic ISG? I would also appreciate a final sanity check of footnote 2 which was supposed to cover this issue.

At this point, I am inclined to keep out the discussion of Event Categorization.

Thanks,

Mike

>>> Brittain Hill 08/10/2006 8:31 AM >>>

My perspective on the Center comments and remaining issues, preceded by ***BEH below.

Thanks-
Britt

>>> Amitava Ghosh < aghosh@cnwra.swri.edu > 08/07/2006 6:40 PM >>>

Mike:

Here is our combined response to item #2, #3, #5, and #6. We are working on other items. If we have any substantial response, I will get back to you.

>2. Need to clarify that the empirical analyses is of the crane data,
>not the probability of the actual crane at the GROA. Can we state
>upfront that we are using the empirical estimate as a reasonable
>surrogate value for the crane in the PCSA? (Need Adams/Ryder view)

#2: Add text on line #9 page A-4:

"It should be noted that the crane systems to be employed in the GROA may have characteristics quite different from the cranes considered in the database NUREG-1774 [2]. Additionally, as discussed before, operator training, maintenance, operational environment, and quality assurance may affect assumptions of equivalence between these cranes to those in NUREG-1774 [2]. As suggested in this Appendix, the reviewer should establish whether the performance information available in NUREG-1774[2] would be applicable for the crane systems proposed to be used in the GROA. In such cases, the estimated reliability presented here using the information from NUREG-1774 [2] would not be applicable for the cranes employed in the GROA."

***BEH Concept is valid but wordy, and we don't really need to draw the link to the GROA facility to provide an example. Suggest revising p.3 L7-10 as:

"NUREG-1774 [2] provides empirical data that might be used to develop a reliability estimate and address uncertainty, if justification is provided to support the analogy between these data and the event sequence being considered. In this simplified example, NUREG-1774 data are assumed to be appropriate analogs for the crane design and operational conditions being analyzed."

>3. The confidence interval example gives the impression that the
>chi-square tables in Reference 4 should be used for 3 drops. But the
>chi-square table is in terms of degrees of freedom. To use this table,
>the degrees of freedom would have to be calculated using the following
>equations. For the upper bound, $DF(\text{upper}) = 2(x+1)$ and $DF(\text{lower}) = 2x$,
>where x is the number of events. Does it make more sense to refer
>directly to Table 10 in the Applying Statistics Book? (Need Adams view)

#3: Add text on line 29 page A-4 after $\lambda_{0.95}$ "with $2/x$ and $2/(x + 1)$ degrees of freedom, where x is the number of drops,"

***BEH Added text looks appropriate.

>5. Is it okay not to relate the results of the example to event
>categorization, in terms of what the confidence interval shows us? Does
>the last-minute footnote 2 cover it, or should this be dropped?? (Need
>Adams view)
>

#5 It is not ok to drop the relation between the results of the example to event categorization. Categorization of the event sequences is the basic requirement to do the reliability estimate. We clearly told that to DOE in the TE. We also emphasized that it will involve more scrutiny when the event frequency falls close to one of the regulatory limits; hence, the need to assess the uncertainty and sensitivity of parameters in those cases.

***BEH Lets think about the converse situation, though. Here, we are using a simple confidence interval as the sole method to evaluate uncertainty, with the result being an estimate that the event sequence would be expected to occur. Although this appears a reasonable basis for including the sequence, what about the situation where uncertainty is only measured by the same simple confidence interval, which shows the event sequence is not expected to occur (e.g., P of 0.8)? I would not accept that basis to screen out a Cat1 event, because the analysis is occurring in a near vacuum and there is little relevant discussion on the fundamental sources of epistemic and aleatory uncertainty in these data. We do not want to be sending the signal that a simple statistical test is acceptable basis for evaluating uncertainty, which I readily interpret from using this basis to categorize the example event sequence.

If the authors feel strongly using this basis to categorize event sequences, I will defer my concern to the OGC review and have them determine if the categorization is appropriate to provide in an example method.

>6. Why does Appendix A address sensitivity analyses but the other ones
>do not? Doesn't sensitivity apply to the other Appendices? Does this
>need to be clarified? (Need Teams view)

#6 Appendix A gives sensitivity analysis as it uses an existing database of performance information cranes working at similar facilities, not in the proposed facilities. Although they may be built using the same ASME NOG-1 standard, operator training, maintenance, environment, etc. may make each crane system somewhat unique. In addition, the failure (in this case, the drop) of the crane is assessed based on descriptive information available for the event. Assessing whether the event should be counted as a failure or not requires analyst's judgment. Hence, there is a need to assess the sensitivity of the input parameter (here number of drop events) on the estimated reliability. One of the issue (and key message to DOE) is to assess the database and determine its applicability for the specific task to estimate reliability of a given SSC. Additionally, different appendices are aimed at highlighting different issues in reliability estimation. There is no implication that sensitivity analysis is not an issue in the other appendices. In fact, the HVAC (industrial data used to estimate failure rates), Canister (empirical data used to estimate ultimate strength) and Concrete (empirical data used to estimate properties of concrete and rebar) are all connected to this issued. In order to avoid repetitious calculations and conclusions, Appendix A was chosen to be the main case where sensitivity is explicitly addressed. Where it is in fact highly justified since the reliability is being based on a single number obtained from data that may or may not reflect the cranes at the GROA.

***BEH I am comfortable with wording of the version 08-02-b text in this section.

++++++Appendix A++++++

Lines 6 and 7 page A-3:

Recommend removing this sentence: "In practice, this means that the event probability is adequately represented by the frequency (i.e., recurrence rate) of the event."

Line 6 on page A-4:

(i.e., probability < 1) should say (i.e., less than 1 drop).

We actually calculated an expected number of events and got a value of 0.84. The expected number of events during the time period is less than 1. This is not the same as a probability value. Equations for probability values have been removed.

***BEH [[ATTN CHRIS RYDER]] As now written, a probability is NOT being calculated in this example because frequency is not equivalent to probability in this case. Following what we discussed with Lee, Cat1 is based on "expectation" and Cat2 on "chance", which are different concepts. In the example, we are calculating the expectation of drops, i.e., an estimate of the frequency, not the likelihood that a drop occurs. Need to make the following changes for consistency:

A2, L42: replace "number" with "the frequency"

A3, L1: replace "probability" with "frequency"

A3: Reconcile footnote 1 with revised text.

A3: delete lines 2-7, as the text then goes on to ignore the Poisson distribution and simply estimates frequency.

A3, L18: replace "probability" with "frequency"

A3, L24: replace "probability" with "frequency"

A4, L6: replace "probability" with "frequency"

A4, L18-21 replace text with "range of uncertainty in the drop frequency based on the number of empirical data."

A4, L25: replace "probability" with "frequency"

A4, L28: replace "probability" with "frequency"

A4, L36: replace "has the potential" with "is expected"

A5, L17: replace "probability" with "frequency"

A6, L24: replace "probability" with "reliability"

Line 33 on page A-4:

Should be 2.1 drops (Note: $1.4E-4 * 15000 = 2.1$) ***BEH OK

Lines 23 and 24 on page A-4:

The statement concerns me a bit. This statement came from Brit's version, but the only reason I used the 90th percentile confidence interval was because a similar example calculation is provided on page 6-6 of NUREG 6823.: "...a 90th percentile confidence interval represents a generally accepted engineering practice for a reasonably characterized operation such as a crane lift."

Mike's version of this text used section B.4.2 of NUREG6823 to develop a statement indicating that .99, .95 and .90 were considered typical. I think the statement the way Mike worded it originally would be better than the sentence above.

***BEH regardless of the interval used, a basis must be provided to justify the selection. If authors want alternative wording for the basis used to select a 90% CI, that's fine (e.g., a 90% CI was selected in this example for consistency with NUREG-6823). However, I disagree that we can implicitly state that 0.99, 0.95 or 0.90 can be arbitrarily selected, else we're opening this up to capricious selection of a CI in licensing. DOE must select and justify the CI used based on confidence in the data, not based on ease in selecting a screening criterion.

++++++Appendix C++++++

>9. Is there a vulnerability using the 168-hr normal mission time in the
>HVAC example? Is it too conservative as an illustration? Does it need

>to be explained better? Changes in mission time could affect failure
>probability by several orders of magnitude (Need SIR team view)

>
#9: Mission time is very important to the calculation of reliability.
It is not possible to understand a reliability value without also understanding the mission time. The appendix shows that mission time is a key piece of information for the reliability calculation.

***BEH I think the current explanation in the text C-2, L20+ is adequate.

>10. In Appendix C, there is a fault tree of the exhaust side of the
>HVAC system. The presents of the fault tree raises the question of why
>the fault tree is not solved with the software that is being used for
>the uncertainty. Why go though the manual calculations as illustrated
>in the example? (Need Adams/Wong view)

>
#10: The manual calculations illustrate to someone looking at this system that one train is in normal operation and a standby train is being relied upon should the normal train fail. When considering the standby train, both the probability that a component fails to start and a probability that it fails to continue running need to be considered.

For the normal train, the probability that a component fails to run is being considered. If just a fault tree and a solution from SAPHIRE were developed instead, then it may not be clear how the reliability for such a normal and standby train was developed.

***BEH Indicating SAPHIRE could be used to characterize uncertainty seems appropriate for the uncertainty discussion. I agree with the Center comment that it is more straightforward to understand the method by doing it manually as in the example

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>Remaining Issues (Big & Small)

>
>ISG Body

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>1. Mean: Need to clarify statement regarding mean reliability estimates
>under treatment of uncertainty. Need to check what Statements of
>Consideration has said regarding use of mean. Are we talking about mean
>reliability estimate for each SSC, or really the mean estimate of the
>overall release frequency (e.g. for Event Categorization)? Are we on
>the same page regarding what "mean" means, and differences between
>"mean", "averages" and "point estimates"? Can we multiply the
>mean/average point estimates of the crane, canister, and HVAC failure
>probabilities to simply get the mean release frequency and categorize
>the event sequence? What if a large interval of the frequency
>distribution goes across a Category limit? (too bad?)

>
>How should the ISG be revised (simplistically) to resolve this
>confusion. (Need Team view)

***BEH SOC does not address use of the mean in preclosure. Wording in text appears appropriate with concepts in SOC that DOE has broad flexibility in the PCSA, DOE is not required to do an ISA or probabilistic analysis for the PCSA, and that DOE must address the uncertainty in any approach used in the analysis (e.g., 66 FR 55742). As for the remaining concern of propagation of a mean, this would not be acceptable as there is no discussion of uncertainty. If you have a mean, there is an associated distribution of data to support calculation of the mean, thus, variability in the data can be calculated and uncertainty in the data estimated

without any significant additional work. DOE would be expected to address uncertainty in the case of using means, as uncertainties are readily derived from the underlying data distribution.

>Appendix A

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>

>4. Do we need to clarify that the confidence interval only
>characterizes one type of uncertainty, but there are others that would
>need to be considered in a real calculation that leads to event
>categorization? What are they? Is this true for other Appendices (Need
>Team View)

***BEH This would be consistent with concepts in #5.

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>7. Appendix A and others could be soften with using "could" instead of "may"
***BEH agreed. "May" implies permission (e.g., may be used) whereas "could" is truly conditional (i.e., support is needed)

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