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William B. Cottrell
Nuclear Safety Journal
Oak Ridge National Lab.
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Oak Ridge, Tennessee 37830

Dear Mr. Cottrell:

Thank you for affording me an opportunity to review the manuscript, "An Assessment of the Frequency of Failure to Scram," submitted for publication in Nuclear Safety.

After a careful review of the proposed article, I have concluded that:

1. The article has not considered some of the recent publications which have a bearing on the subject of this paper.

   Examples: NUREG-0460, Vol. 3, December 1978.

   "A First Approach of the Rare Event Problem by the Study of the Reliability of the Protective System of the Fessenheim 1 PWR Reactor," A. Carnino, et al., January 5, 1979.

   In my detailed comments (see enclosure) I have identified sections of Vol. 3 to NUREG-0460 which could influence the characterization of the NRC analyses.

2. The presentation of the issues is adequate but incomplete and possibly biased.

   Again, the enclosure provides a number of recommendations which, if followed, would enhance the quality of the presentation.

3. Finally, I consider the manuscript to be acceptable if

   a. the authors present the sensitivity of the posterior to a range of priors (see General Comment and comments 9, 13 thru 19, and 21);

   b. the authors reasonably represent NRC studies (see comments 4 thru 8);

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c. the authors represent the scram system more completely in the development of the prior (see comments 11 and 12).

I hope these comments are helpful in improving the quality of the proposed publication.

Sincerely yours,

Original signed by:

Ashok C. Thadani

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Enclosure

General Comments

The main virtue of this paper is its description of the steps involved in the calculation of the posterior distribution of the frequency of RPS failure. However, because of the arbitrary and unjustified nature of so many of the numbers which went into the calculated posterior, the reader is left with serious questions as to the validity and meaning of the calculated posterior. Except in the rather unlikely circumstance of his agreeing completely with all of the author's subjective probability estimates and judgments, the reader cannot judge the extent to which his posterior would differ from the author's. In order to place the author's posterior into proper perspective, it is essential that the sensitivity of the posterior to the subjective probabilities and judgments be investigated. Otherwise, the reader may attribute far more validity* to the author's posterior than is warranted.

Another reason for carrying out a sensitivity study stems from the possibility that the choice of a "prior" has been influenced by the author's knowledge of the safety record of nuclear reactors, thus giving the "experiential data" more weight than the Bayesian analysis calls for. The range of priors considered in the sensitivity analysis should be broad enough to encompass all reasonable priors which might have been chosen if this analysis had been carried out before any nuclear reactors were put into operation.

Detailed Comments

1. Page 5:

   Equations 1 and 2 are incorrectly characterized.

   a. Equation 1 is based on time-dependent model.

   b. Equation 2 is based on a combination demand-dependent model and a time-dependent model. It leads to the nonsensical result that P depends on N.

   c. NUREG-0460 uses the time-dependent model.

   Recommendation: The authors should review p. II-32 of Vol. 2 of NUREG-0460 and correctly characterize the NUREG-0460 approach.

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*For any reader, validity refers to the closeness of agreement of the author's posterior with his posterior.

2. Page 6 above Eq. 31 to the bottom of page 8:

The NRC analysis was based on the theory of confidence bounds for a Poisson failure rate. The author's derivation is equivalent to this.

Recommendation: Replace these pages with a simple statement that the NRC results are classical confidence bounds for a Poisson failure rate.

3. Page 9:

This explanation of the EPRI analysis is tantamount to saying that, while EPRI claimed that they were using a demand model, in reality they were using a time-dependent model.

4. Pages 10 and 11, last paragraph on page 10:

It is important to note that the use of the rectification concept in itself does not result in significantly different estimates by NRC and EPRI. In fact, NRC has accounted for rectification, testing, and improved designs to modify the scram unreliability to 3×10^{-5} (see pages 7, and D-5 thru D-8, Vol. 3, NUREG-0460). Thus NRC did not ignore the benefits of rectification. This value should then be compared with the EPRI estimates for one failure and for no failures (for example, see page 24 of Part 1, EPRI NP-251, August 1976). Major differences between NRC and EPRI are

a. Frequency of useful testing

b. Use of Naval data

c. Synthesis models.

Recommendation: Review pages II-45, Vol. 2; pages 7 and D5-D8, Vol. 3, of NUREG-0460.

Correctly represent the NRC study which was not "too pessimistic" and which did not "ignore the learning curve effect." NRC did ignore the learning curve effect as suggested by EPRI.

5. Page 11, paragraph above Section 4.2:

If Kahl failure is excluded, then experience prior to this failure should have been excluded.

Authors should make this point for consistency.

6. Page 11, Section 4.2:

   Authors should note that NRC also included foreign (except USSR) LWR experience in their data base.

7. Page 12, top paragraph:

   This important difference is inadequately treated.

   I recommend that the authors review pages 4 to 8 of Appendix D to Vol. 3 of NUREG-0460 to provide perspective on this important point.

8. Page 12, Section 4.3, first paragraph:

   NRC describes the EPRI "Synthesis Model" on pages II-51 on.

   NRC (NRR) has emphatically rejected the "Square Root Method". See pages II-55 to II-58.

   Recommendation: Authors must rectify the erroneous conclusion on "fault trees." "Fault trees" are useful, but the model is questionable, at best. (See lines 11, 12, 13, and 14 on page 13 of this paper.)

9. Page 15:

   The authors make the point that the "experiential data" will not be included in formulating the "prior" distribution. While it is not included in a formal sense, the possibility exists that the authors' extensive background in the operation of nuclear reactors influenced their choice of the prior, so that the "experiential data" had a double influence on the posterior. This possibility might have been dispelled by a careful discussion of the source of the authors' subjective estimates of the reliability of the various subsystems, but no such discussion was forthcoming.

   Recommendation: It would be extremely difficult, if not impossible, for the authors to demonstrate that their prior has not been tainted by the "experiential data." The possibility of this contamination is another argument for the importance of a sensitivity study to determine the effect of changes in the prior on the authors' conclusions.

10. Pages 18 and top of 19:

    Perhaps the "prior" has to be extremely subjective; however, one should not disregard the design features, e.g., indications for most sensors are displayed in the control room. In a BWR for most transients separate, redundant and diverse (in principle of operation) sensors are available.

Recommendation: Authors should acknowledge difficulty with and the arbitrariness of the assumptions in this section.

11. The authors have omitted the signal conditioning and bistables portions of scram. This is a most serious error since these portions may well be quite susceptible to common cause failures.

12. Page 20:

Inconsistency between the statement on Scram Discharge Volume (Section 5) and the Sensors (Section 1) is hard to understand. Operator inadvertently leaving the discharge volume tank drain valve closed in conjunction with level sensor failure could result in failure to insert any rods.

Recommendation: The authors should carefully study the BWR scram system and give better engineering design considerations (as claimed at the top of page 15).

13. Page 20, last paragraph:

If 10^{-3} is too high, why not take 10^{-4} instead of 10^{-5}? Also, this seems to be a clear case where the chosen value of 10^{-5} was not a prior value but was chosen to reflect the "experiential data."

Recommendation: Examine the sensitivity of the posterior to all values of the frequency of unidentified failure modes which are consistent with experience.

14. Page 21, first paragraph:

The conclusion does not follow from the premise. The authors' assertion that failure frequencies smaller than 3×10^{-4} are very difficult to justify by analysis would seem to imply that 3×10^{-4} is a lower bound. (It certainly does not imply that it is an upper bound.) Accordingly, the derived value of 10^{-5} is a lower bound and is therefore nonconservative.

15. Page 21, last line:

The fact that the lognormal is used extensively does not justify its choice in this case.

Recommendation: Study sensitivity to the choice of distributions other than the lognormal. (While some of the reliability data in WASH-1400 is consistent with a lognormal distribution, the same data are also consistent with an exponential or normal distribution.)

16. Page 22, second paragraph:

The choice of 1.5×10^{-4} for the 95th percentile appears to be an arbitrary choice. Furthermore, the phrase "in the absence of evidence to the contrary" suggests that this choice is not a "prior" one but was strongly influenced by the authors' knowledge of how scram systems have operated.

17. Page 23, last sentence in Section 5:

The large spread of the prior does not "result from our preceding analysis." It is due solely to the choices of the lognormal and the values for the mean and 95th percentile given by Eqs. (9) and (10).

18. Page 25, line 4:

What does it mean to say that the number of failure lies between zero and one? Since these are disputed values, the uncertainty should not be glossed over by some arbitrary weighting. Instead, all the posteriors should be exhibited as part of the sensitivity study .

19. Page 25, line 6:

The final distribution introduces still more arbitrariness into this procedure, and the weights assigned to the posteriors in Fig. 3 are not even stated.

20. Page 26, line 5:

The definition of scram failure as the failure of five adjacent rods to insert has not been used in the derivation of the prior, except to show (pp. 16-17) that the probability of a random independent failure is dominated by the probability of dependent failures.

21. Page 28, line 8:

The authors rule out the possibility of a tighter distribution, but what about a wider distribution or one skewed more towards the right?