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November 23, 1999

MEMORANDUM TO: Gary Holahan, Director
Division of Systems Safety and Analyses
Office of Nuclear Reactor Regulation

FROM: Thomas L. King, Director
Division of Risk Analysis and Applications
Office of Nuclear Regulatory Research

SUBJECT: REVIEW OF DRAFT TECHNICAL STUDY OF SPENT FUEL POOL ACCIDENTS FOR DECOMMISSIONING PLANTS (TAC# MA5099)

We received a copy of the subject draft technical study for review and comment. We were specifically asked to: a) evaluate the technical accuracy, scope, and quality of the study; and b) provide recommendations on measures that could be taken to refine the technical working group's (TWG) efforts.

The report provides a good first cut at many important areas. (A key area, the radiological consequences of a spent fuel pool release, is being addressed separately.) However, there are three major points that need to be addressed in further activities of the TWG. They are as follows.

1. Adiabatic fuel heatup calculations are extremely conservative and may be sufficiently unrealistic to preclude their use in risk-informed decision making. The improvements to the SFUEL code needed to address the issues raised in the study do not seem to be very difficult to make. We recommend that key improvements be made and the modified results used to better identify conditions under which runaway oxidation of the spent fuel zirconium cladding is possible. SMSAB

2. The technical bases for many of the preliminary risk assessment results and findings need to be provided in the report. As written, insufficient detail is provided to evaluate the calculations. For example, more information on analysis details concerning the human reliability analysis, the treatment of dependencies (especially with respect to recovery actions), the analysis of spent fuel pool heatup, and the draindown rate for large loss of inventory events is needed. In addition, a stronger connection is needed between the analysis and the conclusions raised in Section 3.3 of the report. SMSAB

3. Uncertainties in the analysis need to be addressed more consistently. In some cases (e.g., in calculations of pool heatup upon loss of cooling), conservative analyses are used. In others (e.g., many of the conventional PRA analyses), apparently best estimate (or nearly so) calculations are used. In still others (e.g., heavy load drops), ranges of values are used. A consistent approach will support the comparison of analysis results across initiators.

3/30

Detailed technical comments and a number of editorial comments are attached. If you have any questions, please contact Nathan Siu of my staff.

Attachments: As stated

CF	Y	N
PDR	Y	N

Distribution: DRAA Chron PRAB r/f N. Siu, M. Cunningham, T. King, A. Thadani, M. Federline

* Previously concurred.

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Comments: NRR Draft Technical Study of Spent Fuel Pool Accidents for Decommissioning Plants, August 3, 1999

Detailed Comments on Assumptions and Findings

1. Section 2.2.3, 2nd and 3rd paras. Some discussion is needed to show why building failure is a potential issue prior to onset of rapid oxidation. The discussion should address the spaces and construction involved, typical temperatures and event durations needed to pose a challenge. Note that the discussion towards the bottom of p. 32 (regarding ruggedness of spent fuel structures) might be relevant.

2. p. 16, 2nd para. Section 2.2.3 identifies a number of technical issues with existing tools and calculations. Does this mean that the 6 kW/MTU critical decay heat level may be nonconservative? If so, the 3 year time window would be nonconservative as well. SMSAB

3. Section 2.2.4. The 2nd para provides a qualitative argument for why the 6 kW/MTU estimate is considered to be reasonable, despite the issues raised in Section 2.2.3. Have any quantitative studies been done to show that increased radiation heat transfer will counterbalance the flow issues? Have calculations been done to show that imperfect ventilation will lead to an additional 1-2 year delay? If so, they need to be described. (Section 2.2.3 states that intermediate ventilation cases "could be worse" than perfect ventilation cases.) SMSAB

4. Section 2.2.4. It is not clear why a plant-specific analysis would necessarily lead to a shorter critical decay time, unless the "generic analysis" is actually conservative. Given the uncertainties discussed in Section 2.2.3, it isn't clear if this is the case. SMSAB

5. Section 2.2.5. As noted by the working group, the adiabatic analysis is very conservative. (Appendix 3 states that it is "non-realistic".) It isn't clear that the SFUEL calculations, despite all of the issues raised in Section 2.2.3, aren't better indicators of heatup time. If studies have been performed to investigate the effects of potentially non-conservative factors (e.g., imperfect ventilation, higher flow resistances, flow mixing) raised, then the working group should have the tools needed to compensate for the SFUEL weaknesses. In fact, it appears that compensating for imperfect ventilation and for higher flow resistances is feasible without excessive effort; the former would require some relatively simple modifications to SFUEL to address building heatup, and the latter might be addressable through either code input or changes to hardwired code constants. Modifications to address flow mixing might be possible by using the FLUENT results to develop appropriate correction factors. SMSAB

   If at all possible, improved calculations should be used instead of the adiabatic model. At the least, the SFUEL results should be shown alongside the adiabatic results to indicate the quantitative uncertainties associated with fuel pool heatup time.

6. Section 3 and Appendix 2. Insufficient details are provided to evaluate the PRA calculations. Won

a. There is insufficient information on how a number of the human error probabilities were assessed. At the minimum, some discussion of the tasks to be performed and the relevant performance shaping factors is needed. Note that the nomenclature for the recovery actions listed in Table 3.1-2 doesn't match that used in Appendix 2, so it is difficult to quickly track down a number of events (e.g., the REC-OSS's in the event trees turn out to correspond to the RC-INV-OFFSITE's in Table 3.1-2). Note also that the mapping between the fault trees and event trees in Appendix 2 needs to be made explicit.

b. Some of the sequences involve potentially dependent events (e.g., failure to recover offsite power and failure to recover cooling water from offsite sources during severe weather events). It isn't clear how these dependencies have been identified or treated. (Example: The same value of 0.05 is used for REC-OSS for LOSP due to grid-centered and severe weather causes.)

c. The degree of conservatism in the calculated time windows for power recovery is unclear, so the degree of conservatism in the calculated frequency of fuel uncover (FFU) cannot be assessed. The details of the model used to assess pool heatup need to be provided.

d. More information is needed concerning the evaluation of fire frequencies. In particular, p. 28 indicates that the fire frequency is based on generic data for pump-initiated fires and electrical cabinet-initiated fires, but does not say how many pumps or cabinets are assumed. It also does not specify how cable fires are addressed. From an engineering perspective, it is also important to discuss if and how separation plays a role for the spent fuel cooling systems.

e. The seismic analysis results (and the study's recommendations regarding seismic follow up) rely heavily upon the recovery factor (RESS-OSS) used when the pool is intact. Some discussion of this factor (including actions required, the resources, timing, other performance shaping factors) is needed, given that the earthquake magnitude is 3 times the SSE.

7. There are some errors in the loss of inventory (LOI) event trees in Appendix 2 and these errors are carried forward into the main report. In particular, multiplication of the split fractions shown leads to different sequence frequencies than those shown for at least some sequences (e.g., S04, S13).

8. Considerable credit is given to operator actions for large loss of inventory (LOI) events (given that the operators notice that the event is occurring). For example, multiplication of the split fractions shown for Case 1 (see Sequence S04) shows that the overall recovery factor is 2.2E-4 (given a large loss of inventory). The basis for this credit needs to be better described (see earlier comment #6a). Note also:

   a. The correspondence between the human error probabilities (HEPs) given in Table 3.1-2 and the split fractions shown in the LOI event trees isn't obvious. See for example HEP-INV-MKUP-E. This has a value of 0.01 for Case 1, but

there aren't any split fractions with this value in the upper branches of the Case 1 LOI event tree.

b. The LOI HEPs in Table 3.1-2 appear to refer to the HEPs developed in INEL-96/0334 incorrectly in at least in one case. HEP-INV-MKUP-E is associated with operator actions in isolating a large inventory loss and initiating normal coolant makeup. Table 3.1-2 refers to INEL event SFP-XHE-MANIOS-E. However, in INEL-96/0334, this HEP refers to isolation of a small leak; significantly larger HEPs are reported for large leaks. The differences between the two analyses need to be discussed.

9. There is little consideration of uncertainties in most of the preliminary risk analysis. The heavy load drop analysis is an exception. However, only the upper end of the heavy load drop analysis results appears to be used in drawing a significant conclusion (see Section 3.3, Conclusion #6). Given that uncertainties have not been addressed for other initiators, it is not clear that Conclusion #6 is justified.

10. The basis of most of the conclusions provided in Section 3.3 is unclear. Note that the preliminary risk assessment addresses fuel uncovery but not radioactive release or public health consequences.

   a. Conclusion #1 is not clearly supported. Seismic and load drop events provide significant contributions to FFU (according to Table 3.1-3), and these analyses appear to be insensitive to the use of sled mounted systems. Also, the report provides no sensitivity studies comparing the sled mounted systems against the systems they replaced.

   b. See Editorial Comment #25 below regarding Conclusion #3.

   c. Section 3 does not appear to address the issues underlying Conclusion #4. (It isn't clear if the issues are discussed anywhere else in the report either.)

   d. See Comment #6e above regarding Conclusion #5.

   e. See Comment #9 above regarding Conclusion #6. Also, the basis for assuming 200 lifts per year needs to be provided.

11. Section 4.1.2. As noted in earlier comments, considerable credit is being given in the PRA to the use of alternate sources of water for numerous scenarios. Are these alternate sources included in the 3rd bullet on p. 49 ("SFP emergency or normal makeup water supply")? If not, they should probably be added.

12. Section 4.1.3. Given that the risk drops off significantly with time, it isn't clear if the 24-month time window for periodic evaluation makes sense. Wouldn't it be better to check more frequently early on, and then reduce the frequency after the risk of significant pool heatup is negligible?

Editorial Comments

1. Section 2.1.1, 1st para, 1st sentence. The reference to external fires seems to be extraneous. The issue is if coolant is lost, no matter the cause.

2. p. 8, 1st para, last sentence. Is it possible to say what the SNL tests reported (rather than indicate what the working group's beliefs are)?

3. Section 2.1.2. .Given the intended audience (see the plain English explanations provided in Section 1), it would be helpful to strengthen the tie between this discussion and NPP scenarios.

4. Section 2.2.1, 1st para. The reference to 800°C isn't completely consistent with the discussion in Section 2.1.1.

5. Section 2.2.1, 2nd para, 4th sentence. This sentence needs to be reworded to support the working group's conclusion stated in the 1st sentence of the 1st para. Similar comment for last paragraph in section.

6. Section 2.2.1, 3rd para, 4th sentence. Need to explain why eutectic formation is important.

7. Page 12, last para. The definition of "critical decay time" provided in Section 2.2.4 needs to be moved (or at least copied) here.

8. Page 13, 1st para. Need to provide the basis for the working group's estimate of 3 years (discussion is on p. 16).

9. p. 16, 2nd para. The 4th sentence states that there are no reliable decay heat calculations available to the staff. Some discussion is needed as to why NUREG/CR-5625 is acceptable. There may be some confusion between calculations for decay heat level (for which I thought reliable methods are available) and those for fuel heatup following a loss of cooling (which are subject to all the limitations discussed in Section 2.2.3).

10. Sections 2.2.3 and 2.2.4. Section 2.2.3 tells a first-time reader that there are a host of potentially serious problems with SFUEL. Section 2.2.4 then says that things aren't as bad as they seem. Some revision is needed to make sure the reader gets the right message first.

11. Section 2.2.5, first 2 paragraphs. These seem somewhat out of place. The focus of Section 2.2 seems to be on technical analysis issues, whereas these two paragraphs seem to focus on how the results of heatup analyses are to be used. The first paragraph is also difficult to read.

12. Section 2.3.1. Given the conclusions of bullets 1-3, the opening sentence should be modified to indicate that these are potential scenarios that were reviewed. The second sentence of Bullet 3 should be more definitive, if possible. Were any calculations done?

13. Section 3.0, p. 24, 3rd para. It would help some readers to indicate what kinds of actions can be taken in 5 days and, therefore, if there is any reason to worry about events that take this long.

14. Section 3.0, p. 25, 4th para. Also footnote 2. It should be clarified that internal floods and internal fires are included with internal events in this analysis.

15. Section 3.1, p. 26. The details of the model used to analyze pool heatup need to be presented. (Page 24 refers to "simple calculations with conservative assumptions.")

16. Section 3.1. Recommend that scenarios be specified more carefully (and consistently) in some of the equations to avoid confusion. Examples.

   a. Scenario 1ii on p. 27.

      i The description of this scenario indicates that offsite power is recovered, but includes a reference to the diesel-driven fire pump in the equation's first term. Scenario 1i on p. 26 also refers to failure of the diesel driven pump, but shows it as a second, separate term. This is how the pump is actually treated in the event tree (see Appendix 2). Note also that while the event tree in Appendix 2 does treat Scenarios 1ii and 1iii as different (there are different failure probabilities associated with operator use of a make-up system), differentiating these two scenarios in the main report can be confusing - the natural question regarding Scenario 1ii is why the diesel-driven pump is needed if offsite power is recovered.

      ii Both Scenarios 1ii and 1iii on p. 27 refer to the failure of the cooling system (to restart and run), and actually use the same fault tree. However, the scenario descriptions are different. (The description of 1iii refers to hardware failures as well as operator errors.) The descriptions should be identical, given their treatment in the model.

   b. Scenario 3i on p. 28. "Offsite sources" in the second parenthetical term refers to sources of water, rather than power; this is stated more clearly for some of the earlier scenarios (e.g., 1i on p. 26).

17. Section 3.1, p. 30, Case 3. Description should be revised; Appendix 2 states that, for Case 3, the pumps have been removed.

18. The presentation of the loss of coolant inventory (LOI) trees shown in Appendix 2 is slightly confusing for readers expecting increasingly worse events to generally appear towards the bottom of the tree. It might be helpful to provide additional comments or labels in the trees.

19. The presentations of the heavy load drop and aircraft analyses don't match those of the other initiating events. Revision of the report to provide a consistent presentation of scenarios should be considered.

a. The analyses employ ranges of numbers rather than point estimates. (This is not a bad thing - uncertainties should be addressed to some extent for the other initiating events.)

b. The analyses do not use event trees as a presentation tool.

c. The analyses do not distinguish between Cases 1, 2, and 3, and no explanation is provided.

20. Table 3.1-1 shows an initiating event frequency of 2.5E-6/yr for load drops. This value actually appears to correspond with the upper end of the frequency of pool failures due to load drops for single-failure proof systems. (It is possible that a best estimate initiating event frequency is 2.5E-6, but the analysis does not appear to use best estimates.)

21. Section 3.2, p. 33, 2nd para. The "mathematical shortcut" needs to be described explicitly.

22. Section 3.2, p. 35, 2nd full sentence. Delete "point" in front of "estimate".

23. Minor editing is needed to reduce jargon, e.g., "values are significantly lower than other failure mores". (Failure modes are qualitative; sentence is referring to the frequencies of the other failure modes.)

24. Table 3.1-1. Similar comment to #20 above. Frequency for aircraft impact is the upper end of the frequency of direct strikes. The analysis also considers loss of support systems.

25. Section 3.3, Conclusion #3. This is the first mention of the time available given spent fuel pool failure due to a heavy load drop or large seismic event. This issue should be discussed as part of the respective analyses.

26. The connection between the heatup calculations of Section 2 and the PRA of Section 3 needs to be explicitly made to help most readers. (One way to do this is to discuss the probability, rather than the frequency, of events.)

27. Appendix 5, p. A5-5, 3rd para, 3rd sentence. The parenthetical clause "(using an estimate... total path)" is somewhat cryptic and needs to be expanded. See also the first sentence in the 5th para.

28. Appendix 5, p. A5-5, 5th para, last sentence. The exponent in the "2.0×10^{-7}" must be wrong; the value is higher than the frequency of load drops.

29. Section 4.1.3. Drop apostrophe when referring to multiple licensees.

30. Section 4.3.1, p. 52, penultimate sentence. Refers to Section 3.3 regarding releases of radioactivity, but Section 3 only deals with FFU.

31. Section 4.3.2, p. 53, 3rd line from bottom. Statement that "sources of water should be available" is vague, given tone of most of section (which covers features that are provided to perform specified functions). More importantly, discussion needs to be better connected with PRA, which implies an alternate representation of defense-in-depth.

32. Section 4.3.2, p. 55, 3rd and last paras. Indicates requirements for release mitigation which seem to contradict Overall Design Consideration D (which is raised in the context of ISFSI design - see p. 53). Additional discussion is needed.

33. Appendix X. Change "spend fuel" to "spent fuel".