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June 16, 2000

Dr. Rateb Abu-Eid
Division of Waste Management
NMSS
U.S. Nuclear Regulatory Commission
Washington, DC 20555-0001

Dear Dr. Eid:

In accordance with your request made at the public workshop on "The Technical Basis for Dose Modeling Evaluation", June 7-8, 2000, I am providing comments on relevant parts of the staff's Decommissioning Standard Review Plan (SRP). In particular I am commenting on the SRP Section 5 - Dose Modeling Evaluations, the SRP Appendix C - Technical Basis for Dose Modeling Evaluation, and presentations made by the staff at the workshop. Although I would have liked to provide a very thorough and in-depth review of these sections and the entire SRP, other demands on my time have prevented this.

I have three broad technical policy comments and numerous technical comments; the technical comments are provided in Attachment 1 to this letter.

Before I state my broad technical policy comments, let me make it very clear that I think the staff has, overall, performed an admirable job with this effort. The staff has responded within a tight schedule to provide guidance which is extremely well thought out and largely technically correct. Many of the issues addressed in this guidance are at the cutting edge technically and enter the realm of risk-informed regulation. The staff has done an excellent job on a tough problem.

I see no "fatal flaws" in the SRP; I believe the staff should move ahead to publish it. However, I believe my technical comments could be accommodated with minor changes and should be considered before publication. My broad technical policy comments raise issues that I believe are very important, but they may not be able to be addressed on the current schedule. However, these issues should be addressed, both in the continuing development of decommissioning policy and procedures and in the ongoing consideration of how to risk-inform activities in the Office of Nuclear Materials Safety and Safeguards (NMSS). At the workshop, Division of Waste Management staff and management indicated that the SRP was a "living document", which would be revised as appropriate. I hope the technical policy comments that follow spur early revisions to the SRP.

Technical Policy Comments.

1. The SRP, especially in the treatment of screening analysis, appears to be overly conservative, given the risk-informed regulatory context of this review plan. A more

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realistic screening analysis would provide appropriate regulatory relief, while adequately protecting public health and safety. The SRP, the presentations by the staff at the workshop, and the supporting documents all indicate a considerable degree of conservatism in the screening models, their parameter distributions, and the default exposure scenarios analyzed for screening. Superposed on these considerable conservatisms are the choice of the 90th percentile of the peak dose distribution as the performance measure to compare to the 25 mrem regulatory limit. This is in stark contrast to the site-specific case where the mean of the dose distribution is used to compare with the dose limit. Furthermore, in the site-specific case not only may the licensees use the mean of the peak doses, they have the additional flexibility to use the peak of the mean dose (almost always smaller than the mean of the peak doses) for comparison with the regulatory limit. The use of the 90th percentile for screening appears to be an anomaly. The answer usually provided is that less is known in the case of screening, so more conservatism is needed. However, this argument applies for deterministic analyses, where the uncertainties have not been quantified, but are generally thought to be lower in site-specific cases. In probabilistic analyses, such as are used both in screening and in site-specific analyses, the uncertainty is quantified by the distribution of doses. The only difference in the uncertainty quantification between screening and site-specific cases is that the distribution in screening is expected to be much broader than for the site-specific case. I can think of no good reason to use a different decision criterion in the two cases. This is an excess conservatism that should be removed. The relationship of decision criteria for regulatory decision-making for screening vs. site-specific analyses may be a generic enough issue that it should be considered in the effort to risk-inform the NMSS regulations.

2. The SRP indicates that a "high confidence" is required to assure a low probability that the 25 mrem dose limit is not exceeded; there does not appear any cognizance given to the fact that the consequences of slightly exceeding the 25mrem dose limit is small, and therefore the confidence in meeting this requirement need not be as high as the assurance required to prevent serious health and safety hazards, e.g., the prevention of a core-melt event at a reactor. In a risk-informed context, where some or most uncertainties have been quantified, the degree of assurance required for a decision may need to have a defined relationship to the consequences, if the decision is incorrect. This also appears to be a generic issue for NMSS.

3. The requirements for supporting a site-specific analysis are stringent. This is in stark contrast to the screening analysis, where a minimum of information is required. However, the additional cost of preparing a site-specific analysis to this very stringent standard of support will encourage most licensees to consider the screening values to be de facto cleanup levels. This was not the stated intent of the regulation. Either the screening levels should be made less conservative, the movement into a site specific analysis should be eased, or both.

Once again let me congratulate the staff and management of the Division of Waste management for producing an excellent Standard Review Plan. This was a difficult job and required a lot of diligence and expertise to accomplish. I would be happy to discuss my comments with you. You can reach me by telephone at (301) 384-6507 or by e-mail at nae@bellatlantic.net.

Sincerely,

Norman A. Eisenberg

cc: Mark Thaggard, DWM
Richard Codell, DWM
James Danna, DWM
Sandra Wastler, DWM
Joseph Holonich, DWM
John Greeves, DWM
Martin Virgilio, NMSS
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Attachment 1

Detailed Technical Comments

1. Notwithstanding my technical policy comments, there appears to be a non-conservative aspect of the SRP. In particular, on page 5.6 of the SRP four conditions are listed that, if present at a site, would preclude the use of the screening analysis. However, page C.8 of the Technical Basis Document has a much more broadly stated admonition, which I think is more correct: "Reviewers should examine specific physical conditions at the concerned site that would invalidate the model and code assumptions associated with the screening code/model." Why not put this statement in the SRP before the list of four conditions? Furthermore, Tables C5.1 and C5.3, for the DandD and RESRAD codes respectively, provide even more extensive lists of site conditions that may be incompatible with screening codes (and in the case of DandD with the tables of screening values, Tables C2.2 and C2.3). The more extensive lists in Tables C5.1 and C5.3 should be referenced in Chapter 5 as additional cautions.

2. On page C.11 there is a discussion of sites with large areas contaminated. It is acknowledged that some sites, with contaminated areas larger than the default area of cultivation, 2400 m^2, could lead to doses higher by a factor of 2-3 than those calculated by the DandD code, for cases where ingestion of drinking water and cultivated foodstuffs is an important pathway. However, neither the cultivated area, nor the area of contamination appear to be random variables in DandD version 1 code. Therefore, the argument that such exceedances would be considered in the 10% of screening cases allowed to be over 25 mrem seems specious. Would it not be consistent to cover sites with large areas of contamination by eliminating them from the screening analysis, along with other restrictions (see comment 1)?

3. On page C.42 the document states, "This uncertainty should be addressed by developing multiple alternative conceptual models and proceeding forward with the conceptual model(s) that provide the most conservative estimate of the dose and yet is consistent with the available data." This is very restrictive and essentially requires a "worst-case" analysis. Some appropriate relief could be provided by allowing licensees to enumerate alternative conceptual models and attach probabilities to them. The probability-weighted average dose could then be used for comparison with the standard.

4. There appears to be a typographical error in equation (C8.2). Should the denominator of the first factor on the right-hand side of the equation be:

$$\bar{d}$$

i.e. average dose, rather than:

$$d(\bar{x}_i)$$

as stated?

5. All the Sensitivity Analysis methods discussed in Section 8.5 appear to be applied to the set of peak doses that result from the set of realizations. Essentially we are looking at how the peak dose changes for a fractional change in a parameter. However, the performance measure indicated in equation (C8.1) clearly indicates that the variable of interest is the peak of the mean dose, based on all realizations (at least this is the case for site-specific analyses). Should not the sensitivity analyses be directed toward the actual performance measure (peak of the mean dose or mean of the peak doses), rather than the components of that performance measure (the peak dose or dose vs. time for each realization)? Since the peak of the mean dose or the mean of the peak doses are essentially a statistic based on an ensemble of values, should the sensitivity of these results be examined based on the statistics of the underlying parameter distributions, e.g. the mean or standard deviation of a particular input parameter distribution? The staff indicates that "the primary aim of a sensitivity analysis is to identify the input parameters that are the major contributors to the variation or uncertainty in the calculated dose." The staff should clarify whether it means the performance measure, such as peak of the mean dose, in this regard, or whether the dependence of dose on a parameter is intended. In general the aggregate performance measures, peak of the mean dose or mean of the peak doses, will be much less dependent on any particular parameter and will have much less variability than the underlying dose distribution.

6. The ending sentences in paragraph "8.5.3.1 Normalization" are confusing. The text states, "Although this is a useful measure, it treats all sensitivity results equally in spite of the value of peak TEDE." However, the average (or some other statistic of the ensemble of values) is used to normalize the dependent variable (peak TEDE). If a fractional change in an input variable produces a large absolute change in the TEDE, the relative change compared to the average TEDE, will be large also. Is not this the behavior that we seek to detect? It is unclear what is the relevance of the next to the last sentence of the paragraph.

7. The last sentence in paragraph "8.5.3.1 Normalization" states, "Furthermore, sensitivities calculated from normalized variables do not take into account the uncertainty in the independent variables." Each of the sensitivity analysis techniques cited by the staff has a particular purpose in environmental analysis. The document should more clearly state what is the end goal of the sensitivity analysis. In particular, if the staff wants to assure that a highly sensitive parameter is determined within a particular range of variability, so the fractional change in dose is maintained within limits, then the normalized sensitivity coefficients may prove to be most useful. If the staff wants to know what fraction of the overall variability in dose is attributed to the variability in a particular parameter, then the standardization procedure (paragraph 8.5.3.4) might be most useful; this might also help licensees determine on what parameters to focus further investigations, so variability in dose might be reduced . Other methods, such as the stepwise multiple linear regression, without dimensionless variables tend to be difficult to interpret. Rank transformations may mask or unduly emphasize some sensitivities. This section could be improved for

reviewers and licensees by more clearly stating the goal or multiple goals of the sensitivity and uncertainty analyses.

8. Paragraph "8.5.3.2 Rank Transformation." Analyses with ranks tend to show a greater sensitivity than results with untransformed variables, ..." Doesn't the validity of this statement depend upon the range of the dependent variable compared to the number of samples. For example, if one has 10 samples, but the untransformed variable ranges over several orders of magnitude, the sensitivity of the untransformed variable may be much greater than the rank transformed variable.