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Ms. Paula vanHaagen
Office of Environmental Guidance
and Compliance
Department of Energy
Washington, D.C. 20585

Dear Ms. vanHaagen:

This letter is in response to the request from Raymond Berube to Robert Browning of November 24, 1986, that I participate in a review of the Remedial Action Priority System (RAPS), which will be used by the Department of Energy for determining the priority of further investigations and cleanup at DOE's CERCLA sites. Mr. Browning and my other supervisors approved of my participation in this review as long as it was understood that my comments would be taken as personal opinion, and not the official position of the Nuclear Regulatory Commission.

I have read the RAPS methodology and validation reports by Gene Whelan and others. I think the authors have done a commendable and thorough job in presenting and justifying their rationale for the RAPS methodology. The models are presented in a clear and thorough manner, and the limitations discussed. The validation efforts are performed on several DOE sites where there are data for the transport properties of the pollutants involved, and there has been a well-documented history of releases into the environment. The results for the most part indicate that the forms of the models chosen are valid for screening purposes.

My main concern is not so much with the technical details of their chosen methods, but how the method will be practiced. My concerns are expressed in the general and detailed comments on the source documents given below.

General Comments

1. I am concerned that the values of the coefficients used in the models would not be available in the large majority of cases where the RAPS methodology would be applied. A convincing argument can be made for simple analytical models which employ conservative coefficients to predict pessimistic, conservative results. Frequently, these types of models can be used to demonstrate that concentrations and doses are within legislative or safety limits, without the need for costly site-specific data and complicated models. It is not adequate, however, to simply choose the most conservative values of the coefficients from a range, since the purpose of the RAPS methodology is to rank the hazard of different sites. This would be especially true of sites which are very dissimilar, as the effect of the conservatisms would be difficult to assess. It should be a tenant of the methodology that the most realistic values of the parameters be chosen. I noticed that the case of the Hanford site, for example, conservative values of the retardation coefficients were used for the plutonium and americium releases, rather than values more typical of middle of the range. Where there is great

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uncertainty about the value of the parameter, ranges of possible outcomes should be presented based on techniques such as Monte Carlo sampling from a range of possible input values.

Detailed Comments - "Development of the Remedial Action Priority System (RAPS): Preliminary Mathematical Formulations"

1. Page 3.1 - Does infiltration analysis properly account for infiltration through a damaged cover? Subsidence of earthen covers on landfills can lead to localized infiltration much greater than indicated by the methods of this section.
 2. Page 4.42 - How does the contaminant source get into the surface water flow? Is the rate of release proportional to flowrate? On what meteorological condition of precipitation is runoff calculated for overland flow contamination? Is a long period record used or is there recognition that particulate transport is usually carried during periods of flooding? An average value of runoff will not be indicative of the sediment load in this case.
 3. Page 5.3 - I can think of important exceptions to the case where groundwater recharge is caused only by infiltration of local precipitation; e.g., seepage ponds.
 4. Page 5.33 - There is no discussion up to this point on where one is supposed to get the coefficients for the models, or the validity of the models; e.g., the choice of a constant dispersion coefficient, even though it is well known that dispersivity is a function of scale.
 5. Page 8.18 - Eq. 8.25 frequently overestimates shoreline exposure for long-lived radionuclides such as Cs-137, because no mechanism is included for the removal by natural causes other than radioactive decay. The error can be substantial, leading to the unrealistic conclusion that shoreline radiation can be the dominate mode of exposure for water bodies.
- B. Detailed Comments on "A Demonstration of the Applicability of Implementing the Remedial Action Priority System (RAPS) at Hazardous Waste Sites"
1. Page 3.6 - A statement is made that development of an accurate release scenario is extremely important. You are not always consistent in this philosophy in the demonstration presented later. There is frequent reference made to the conservatism of the assumptions. See p. 4.4, for example.
 2. Page 4.14 - Releases from the Mound site were for the most part deliberate and planned. They do not represent conditions of inadvertent releases

that one would expect in an evaluation of a CERCLA site. It isn't likely that you would ever have the conditions as well defined in the real evaluation of a site.

3. Page 4.39 - Annual or long-term averages were used for the flowrate for the dilution calculations. A more meaningful flowrate would have been the harmonic mean, since $C = W/Q$ where W is the rate of release and Q is the flowrate. Averaging both sides gives $\langle C \rangle = \langle W \rangle \langle 1/Q \rangle$.
4. Page 5.56 - The choices of equilibrium coefficients looks suspicious, especially those values used for americium. The smaller values give better results, but under what conditions would one have chosen them? If you did not actually have the answer to start with, wouldn't there have been just as strong a reason to have picked the higher values?
5. Page 5.56 - Where would one have chosen the values for the dispersion coefficient, had field measurements not actually have been taken? Is the 10% rule for dispersion length being advocated by the developers of RAPS?
6. P.5-59 - Hanford is an exceptionally well studied site because of the long history of waste disposal and the existence of BWIP, and a low level site. I wouldn't expect data to be as comprehensive at most other places in the U.S.
7. The data-model comparisons, while quantitatively correct, are in some cases several orders of magnitude apart. This leads one to believe that comparison of one site to another could be orders of magnitude higher or lower than the actual difference of their hazards.
8. Page 5.76 - Once again the conservative choice for an equilibrium coefficient was made, instead of the more realistic one.
9. page 5.87 - Where would one have gotten the longitudinal and lateral dispersivities for this case if they had not actually been measured in the field and also backed out of other modeling studies? Some guidance on the choice of the dispersivity and other parameters is needed.

Conclusions

The biggest unanswered question on the validity of the RAPS methodology is how the investigator can get information on the site which is accurate enough to allow him to rank one site against the next. While simplified models are often appropriate for screening analyses, and can be used to demonstrate that the releases from a site are within legislated limits, care must be used when these same models are applied to ranking methodologies, so that the relative hazards of the sites are not lost in the conservatism. More emphasis should be placed on the use of the models to give a measure of the central tendency of the hazard. The conservative end of the range should not be used alone. The

distribution of possible outcomes from the hazard analyses might be a more useful output of the RAPS methodology.

Finally, I must remind you that the comments in this review are mine alone, and do not necessarily represent the official views of the Nuclear Regulatory Commission. It is my belief that the RAPS methodology as it will be applied by the Department of Energy, does not affect any licensing activities of DOE facilities by the Nuclear Regulatory Commission.

Thank you very much for the opportunity to comment on the RAPS methodology.

Sincerely,



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OFFICIAL CONCURRENCE AND DISTRIBUTION RECORD

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SUBJECT: RAPS REVIEW

DATE: FEB 3 1987

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CONCURRENCES

ORGANIZATION/CONCUREE	INITIALS	DATE CONCURRED
WMGT/RCode11	<u>RC</u>	1/29/87
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