RAC RESPONSES TO PEER REVIEWER COMMENTS

Task 2: Computer Models

Radionuclide Soil Action Level Oversight Panel

May 1999

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> runoff following rainstorm events. Unpreventable phenomena that could cause major disturbances are fires, tornadoes, and floods. These should perhaps be considered by the *RAC* as stochastic events with a certain probability of occurrence. If any of these phenomena were to occur, then short to medium-term increases in resuspension or runoff, perhaps of dramatic proportions, could result.

This perceptive comment sets a potentially difficult task for this project. We expect to be able to check model predictions of resuspension against (at least) Langer's measurements in the 1980s, which provide two years of data, but which consider only the ground cover that existed at that time. A fire that denuded the landscape would increase resuspension by an unknown amount. A tornado that touched down near the site of the 903 pad would immediately send substantial quantities of contaminated soil and litter airborne, and the resulting disturbance of ground cover and surface soil would permit an enhanced resuspension of radioactivity until the previous state was restored. Credibly quantifying the aftermath of these events is very difficult. They can be discussed in the reports, but systematically incorporating them into scenarios would require a great deal more effort and debate than the stringent schedule of this project permits.

Reviewer Three

This reviewer appears to have missed some things in his or her reading of the report. Hopefully the responses below help to clarify these.

> . . . The review of the models, in general, seems sufficient with a few exceptions. The report lacks a clear, concise statement of the criteria used to identify the models that would be selected for review. This should appear in the Introduction.

Such a list of five criteria appears at the beginning of Section 4.1 (page 29). It could be replicated in the introduction, but the existing placement seems more appropriate to us.

> . . . In addition, *RAC* did not explicitly address the models' capabilities to address offsite exposures. This was explicitly mentioned in the RFP and *RAC*'s proposal of work and should be explicitly addressed in the review.

In the overview of GENII, Section 4.4.1, third paragraph, we find the following: "The proposed soil action levels developed for the RFETS are essentially based on a near-field scenario. The RESRAD code is not capable of addressing directly what GENII defines as a far-field scenario, and therefore, GENII appears to have an advantage as a model that may provide dose estimates to off-site individuals." Perhaps the point also deserves mention in the introduction to Section 4. With regard to offsite exposures, it will be pointed out that the approach we are taking to derive RSALs requires that people living farther away (i.e. offsite) will receive less exposure than those individuals who live on the area where the RSAL is being calculated. Therefore, "offsite" exposures are being taken into account implicitly.

1) Include a list of definitions of acronyms and variable names used in the equations.

3

Responses to Reviewers' Comments on RSAL Task 2 Report

Since the reviewers are not openly identified by name, there is no satisfactory way to indicate which reviewer's comments we are responding to at any particular time. This situation thwarts a topical organization of these responses. Instead, we present the responses in five sections (one per reviewer), and we identify each reviewer by the number of pages in his or her printed copy (fortunately, no two reviewers produced copies of equal page length). In each reviewer's section, we respond to selected comments in the order in which they appear in the copy. References are placed at the end of the section in which they were called out.

Reviewer Two

This is a useful and helpful review. The reviewer is familiar with the Rocky Flats site and the history of radionuclides in the soil there. We will give serious consideration to all of this reviewer's suggestions.

2. It is extremely important to use every opportunity to apply site-specific data for soil concentrations and parameter values and their uncertainty distributions to the models that are chosen for the analysis. It is equally important to understand the inherent structure and workings of the models and to be able to modify them as necessary to make them relevant to Rocky Flats. The models should be both verified and validated to the extent possible.

3. I do not feel that *RAC* should limit their analysis to one or two models such as RESRAD or GENII. Other models that may have been used to develop soil action levels at Rocky Flats or elsewhere should also be examined in an effort to understand why such different numerical action levels have arisen. One recent report ("Recommended screening limits for contaminated surface soil and review of factors relevant to site-specific studies", NCRP Report 129, issued January 29, 1999) should definitely be consulted, for example. As a general philosophical point, the skill, knowledge and effort of the model user is often more important that the model itself in arriving at credible predictions.

These comments support *RAC*'s contention that this project should place less emphasis on specific computer programs and more on appropriate models (remembering that we are careful to distinguish between models and computer programs), data, and the knowledge and skills of the analyst. NCRP Report No. 129 was not available before about April 1 (at which time the work for this task was in its late stages). We are familiar with the report and are examining it for its relevance to this work.

5. The amount of resuspension of contaminants from the soil surface is dependent on many processes, both natural and anthropogenic. It is my experience that management of the land is a first-order determinant of resuspension, and this should be recognized and built into the various land use scenarios. Any form of human disturbance, especially anything which disturbs the natural vegetation cover, is bound to increase resuspension during high winds, as well as surface

We will consider this recommendation. If the reviewer means variables used in the equations, this could be done, but variable names in the programs run into the hundreds and including them would be difficult.

2) The second paragraph of the introduction requires clarification. In order to "...make clear our *[RAC's] conception of the task* to which the programs would be applied..." , *RAC* provides a vague definition of SALs. The introduction should be where a succinct, readily understandable definition is provided. I suggest:

We will reexamine the definition and decide whether we believe it requires further work. As part of an effort to make the technical reports more understandable, we intend to include a layman language summary at the beginning of each report. Hopefully this will help the non-technical reader understand the project better.

3) In the detailed discussion of the use [of] SR (Section 2) , it should be emphasized that the use of the SR is predicated on the assumption that the model *estimated radiation dose is linear to the initial* radionuclide concentration in soil. It is important to ensure that this is true for the models reviewed.

This condition is set forth as Equation 2.1-2. Few assessment models are implemented with nonlinear dependence of committed dose (the end point of these predictions) on environmental concentrations. If the reviewer knows otherwise, we would appreciate knowing more about them.

4) In eq. 2.1-1, it seems to me that there is no reason to include scenario as an index. It confuses the discussion. In addition, EPA and et al. have traditionally kept exposure scenario- and dose limit -specific SALs separate (e.g., Table 5-1 in US DOE, 1996). When a particular SAL is selected for a site, it seems sufficient to indicate that the selected SAL is or is not protective of whatever other exposure scenario/dose limit combinations have been evaluated.

In our analysis, a scenario corresponds to a single individual. Thus the rancher, his wife, and his child would ideally be implemented as three correlated scenarios. However, we acknowledged that "as a practical matter, we may wish to treat different scenarios as if they were independent" (page 9, parenthetical remark in the next-to-last sentence).

5) I am not sure how the soil action levels "represented as a joint probability distribution" that *RAC* proposes developing should be interpreted in field applications. After all, the purpose of SALs is to be useful in the field, i.e., to provide either a means of determining the acceptability of measured radionuclide concentrations and/or a quantifiable remediation goal. How will measured concentrations be compared to SALs specified as joint distributions (i.e., compare means, variances, and correlation coefficients?—what if mean is the same, but variance or correlations are different?) I think SALs are more appropriately expressed deterministically for comparison to mean measured contaminant concentration levels, as described in Yu et al.(1993) for sites with homogeneous contamination (1993,see p.33-34, and especially see eq. 3.4. Note

that there is a separate discussion on how to handle inhomogeneous contamination on p. 35). (In addition, RESRAD (Yu et al., 1993) (and likely the other models ??) assume uniform initial contaminant concentrations in the contaminated soil layer. This is, to be sure, a simplification of reality. When contaminant concentrations are not uniform, the deterministic initial contaminant concentrations input to the model can most appropriately be interpreted as the spatially-weighted mean contaminant concentration. There is, to be sure, some uncertainty in this mean due to sampling statistics. However, this uncertainty can be minimized by an adequate sampling strategy. I would caution against thinking that applying an uncertainty distribution to the input initial contaminant concentration would account for variability of contaminant concentrations in the contaminated layer.)

We explained our recommendation for using the distribution of the sum of ratios as an action level criterion (Section 2.2 and Fig. 2.2-1). There is nothing in the formulation to preclude handling the concentrations as constants, if everyone is satisfied that this approach is justified by estimates of sampling error and consideration of possible uncertainties in the representations of the concentrations as spatial averages. We deliberately left this choice open. However, the SALs in the denominators of the ratios are still uncertain, and the sum of ratios needs to be treated as a distribution. It appears that the reviewer may uncomfortable with the idea of applying uncertainty to environmental assessments. Perhaps the reviewer is just expressing caution with regard to including uncertainties in the analysis, and this is a valid point. There is no question that the document of Yu et al. describes deterministic models, and RESRAD was designed to implement such models. Nevertheless, we do not believe this justifies that the methodologies should not expand to accommodate a more contemporary view, especially uncertainties. The reviewer may not be aware that there is a beta-test version of RESRAD that incorporates Monte Carlo facilities for parameter uncertainties, which indicates an awareness on the part of the developers of the changing methodology.

6) I suggest that it is more appropriate to develop SALs by answering the following question: What is the contaminant concentration in soil that results in an acceptable dose limit (for a specified exposure scenario) with a specified level of confidence (given uncertainty in environmental fate/transport and exposure parameters)? I propose use of the equations presented below as a straightforward means of addressing this question.

We believe we have posed this question, along with considerable discussion to guide the reader. What follows this comment is the reviewer's proposed formulation consisting of five equations with some explanation of the notations (which are similar to the ones we have used). We have the following problems with the presentation:

A) It is based, in part, on an erroneous assumption.
B) The introduction of the ratios b_i, it seems to us, clarifies nothing. In particular, if such ratios are to be explicitly introduced, it would be preferable to refer every nuclide to ^{239}Pu; those ratios are available from Krey et al. (1976) and are less awkward in the formulation.

The erroneous assumption consists of the following:

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c) The **maximum** [our emphasis] total dose due to any individual radionuclide can be calculated using:

$$D_i = T_i \cdot C_i \qquad (2)$$

At Rocky Flats, some of the radionuclides are decay products of others; in the most important case, ^{241}Am is a decay product of ^{241}Pu, which in turn decays to ^{237}Np, a long-lived alpha emitter. At present, the levels of ^{241}Am (and ^{237}Np) are rising as ^{241}Pu decays, and they will do so until the early 2030s (Krey et al. 1973; our calculations give the same result). Thus it would be incorrect to assume, for any initial time before 2030, that the proposed equation (2) represents the maximum dose from ^{241}Am and particularly ^{237}Np. Whether or not this would result in palpable error in the total dose remains to be seen from the Task 5 calculations (the early plutonium dose may dominate the much later neptunium dose and render the point moot). Also, different rates of removal of isotopes from the surface soil complicate the question. Our approach was to develop the formulation with sufficient generality that such questions are likely avoided in preference to having them arise later and require additional calculations and explanations.

Krey P., E. Hardy, H. Volchok, L. Toonkel, R. Knuth, and M. Coppes. 1973. Plutonium and Americium Contamination in Rocky Flats Soil. Report HASL-304. U.S. Energy Research and Development Administration, Health and Safety Laboratory.

Reviewer Five

> This reviewer has at least one suggestion for an additional source of information, similar to a computer model, that *RAC* should consider [this seems to refer to the item just below].
>
> P. 29 The candidate computer programs are introduced. The choice of codes for review is sensible but not necessarily complete. *RAC* should at least make a comparison to screening levels *already* calculated for various scenarios by the National Council of Radiation Protection and Measurements (Report 129, issued January 1999, see the reference list).
>
> In addition, a review of how each of these models treats soil ingestion is reviewed in *Health Physics* (Simon 1998) and should be referenced. It can be seen from Table 5 of that publication that soil ingestion values for the GENII code, in particular, are not credible.

The NCRP document (which was also recommended by Reviewer Two) has been examined (it was distributed about the beginning of April and was not available to us during most of the work on Task 2). It will be used to the extent that it is relevant. It is interesting that this reviewer, who elsewhere demands such stringent adherence to the letter of the contract, now advocates that something other than a computer program be examined. Matters related to bringing the GENII database up to date will be dealt with in Task 5.

> In addition, a level of commentary was included in the report which I found to be inappropriate. In particular, those comments directed to the Department of

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> Energy, which is neither a sponsor or direct recipient of this report, are out of place.
>
> Furthermore, I found it interesting that *RAC* discouraged the Rocky Flats Citizens Advisory Board (RFCAB) and Rocky Flats Soil Action Level Oversight Panel (RFSALOP) away from the concept of soil action levels. Though I might agree with that insight, I can not help but feel that such advice is inappropriate in this report for the following two reasons: 1) the report is allegedly concerned only with the suitability of a set of specific computer models, and 2) the contract with *RAC* was (apparently) for the purpose of evaluating those computer programs assuming the concept of soil action levels was already accepted. It seems to this reviewer that it is presumptuous on the part of *RAC* to try and steer the Advisory Board and Oversight Panel away from the concept in this document. That level of discussion should be held in public meetings or in contractor/contractee negotiations.

We are confident the reports will be read by the Department of Energy. We consider the recommendations we made to be constructive and entirely appropriate. As to the contractual obligation to comment on and develop soil action levels, we think our report makes it clear that we are fulfilling that obligation. But our proposal made plain our intention also to explore more contemporary approaches to this assessment.

> P.7, 1st paragraph. The text states: "Thus, the same set of soil action levels could be used for determining the need for remediation, planning the remediation and verifying that the remediation has been successful..." It is unclear whether *RAC* is saying that the <u>same</u> soil action level is <u>necessary</u> for all of these activities. There is actually no scientific reason that is apparent to me to force the same action level for all activities. It would be perfectly acceptable and reasonable to have different soil action levels for different activities, depending on their purpose.

We do not understand what the reviewer is objecting to. We had in mind a comprehensive set of soil action levels, based on all relevant scenarios and dose limits. These action levels, after all, do not depend on specific concentrations, and thus they should indeed be suitable for the applications we enumerated.

> P.8, 3rd paragraph. The text discusses the notion that soil action levels are not needed. As mentioned above, this discussion is outside the goal of reviewing computer programs suitable for the purpose intended. It seems to self-defeating as well as a means for the contractor to control the direction of the study, which also seems improper.

Perhaps this reviewer did not have an opportunity to read our proposal. We do not believe there is anything improper in our suggestions for decreasing reliance on soil action levels.

> P. 9. 2nd paragraph. The text states: "In general, we allow both the numerators and the denominators ... to be uncertain quantities." The approach discussed here is appropriate, however, the discussion does not illuminate the fact that <u>spatial</u>

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> variability is a more important concept to the numerator than is uncertainty (i.e., lack of knowledge).

The statement does not indicate which is more important because we do not yet have final formulations that settle the representation of spatial variability. The reviewer seems confident that this will be the more important component, and that may be the case. But the question is better dealt with in Task 5.

> P.10. Following eq. 2.1-2, it is stated that "...the dose limits are not the same for all scenarios." I don't have a dispute with this statement but it needs clarification. Admittedly, this location in the report is probably not the best place to discuss details of the various scenarios and their dose limits, but it would help to at least reference parenthetically where in the report such a discussion could be found.

Another reviewer suggested saying "the dose limits are not necessarily the same for all scenarios, and this addition may be sufficient to alert the reader. The scenarios sketched in the 1996 DOE/EPA/CDPHE document Action Levels for Radionuclides in Soils for the Rocky Flats Cleanup Agreement are not uniform in their limiting doses, and we are allowing for such disparities, but as noted, this is not the place in the text to go into detail

> P. 12. 1st paragraph in Section 2.2. The text states "...The 1000 doses define an empirical distribution..." I have a bit of a quarrel calling this distribution "empirical." Such a term gives the distribution more credibility than it deserves because it implies that the values are derived from experiment or observation. Monte Carlo calculations are only simulations and may not represent reality at all. In fact, this particular distribution characterizes "uncertainty" which is not even a directly measurable quantity. The authors need to better characterize the distribution as a calculation of possible alternatives which include a substantial degree of subjectivity; there is nothing empirical about it.

This usage, in exactly this context, is fairly common, even in authoritative published material (for example, IAEA 1989). In fact, one is doing a kind of "experiment" with a computer, by analogy with taking samples in real world measurements. Throughout the history of Monte Carlo methods (which go back to the 1940s at least), computer scientists regularly described these methods in terms of carrying out experiments with computers. The word "empirical," as the reviewer knows, is intended to distinguish the distribution from its theoretical counterpart. The nature of the process is described in the surrounding text.

> Throughout the report there are a number of locations, where as a reader, I could not determine why *RAC* was discussing a particular subject in detail. The first of these is located on p. 14, 2nd large paragraph. The discussion of the methods for determining weighted breathing rates seems out of place in a major section on Exposure Scenarios. How the weighed breathing rates are determined is best suited for a Methods section (which does not exist in this report) rather than a section which defines the scenarios.

We do not share the reviewer's organizational preferences for the report.

> P. 16, Scenario 9. The soil ingestion rate described here (88 grams per year) is an interesting, but not credible, value unless it is an upper bound. First, I cannot help but wonder how a figure of 2 significant digits was arrived at. Second, a continual daily ingestion of 240 mg per day (every day for a year) is not a credible estimate, particularly for adults. There are no studies anywhere, except perhaps those relavent [*sic*] to indigenous populations living primitive lifestyles, that have provided evidence of such high continuous, inadvertent intakes. This particular issue will likely be controversial throughout the entire RFETS evaluation process. Numerous publications in this field should be consulted, e.g., Calabrese et al. (1994), Sheppard (1995), Simon (1998), only to name a few. These references are noted at the end of this review. I note from Table 2.3-1 that similar values have been recommended by *RAC* for additional scenarios and their credibility is equally questionable.

The scenarios proposed and briefly described in the Task 2 report were provided as "examples of the scenarios that are under consideration." An important part of the process has been to involve the panel in the development of the scenarios by thoroughly reviewing studies with a range of possible input values for the parameters such as soil ingestion. We are selecting parameter values for the scenarios using the data from scientific literature for use in developing uncertainty distributions. When data from a number of studies on soil ingestion (Calabrese et al., 1991, Stanek and Calabrese 1995, Thompson and Burmaster, 1991; Simon 1998) are used to develop a distribution of soil ingestion values (with ingestion values for geophasic children removed from the distribution),and with each study weighted equally, then the median, or 50th percentile of the lognormal distribution is 200 mg per day (5th and 95th percentile values of 60 and 730 mg per day).

RAC agrees that most soil ingestion studies, even the more recent studies using a mass-balance approach, are conducted under fairly idealized conditions, or during more mild seasons of the year (Calbabrese et al. 1991; Binder et al. 1986). This timing factor provides conditions where children may have more ready access to open play areas and outdoor activities and adults may be more involved in gardening activities. While these values that are derived from studies conducted from a few days to a few weeks are quite valid in estimating daily soil ingestion rates, there is a need to carefully consider the implications of translating this daily soil ingestion rate to an annual soil ingestion rate when the year includes large periods of time where outdoor inadvertent soil ingestion activities may be somewhat limited by snow cover, frozen ground, and inclement weather. Because we are estimating an annual rate, *RAC* is using the 50th percentile of our distribution of daily soil ingestion rate, rather than the more conservative 95th percentile value. From the daily soil ingestion rate, we then calculate an annual soil ingestion value based on the number of days of exposure. In the scenario noted by the reviewer, we had chosen a central value from the distribution.

RAC is aware of the publications noted by the reviewer and will reference them in the Task 3 report, Inputs and Assumptions. Our approach to selecting input parameter values will be thoroughly described in the Task 3 report.

> P.19, 2nd paragraph. The text states: "Soil action levels are defined in terms of dynamic models..." This statement came as a complete surprise. Furthermore, I can not see that there is any basis for the statement. Soil action levels are actually a value derived from conditions which are assumed to represent a

> steady-state contamination condition, an accepted dose standard, and a lifestyle description (which is used to describes the pathways of potential exposure). The only use for a dynamic model would be if the contaminant has to be modeled from its release point until environmental conditions equilibrate or at least, become predictable. However, I would never want to base soil action levels on such calculations. I see no use for this sentence.

Dynamic models *are* the basis for these calculations, and we strong believe this is appropriate. A model of the surface soil compartment, as implemented in RESRAD and other codes, simulates removal of radionuclides from this compartment over time and the movement of the material into ground water (if that option is exercised). It is this dynamic process that gives calculated annual doses that vary with time during the 1000-year period that we are required to consider. The decay chain calculations that run throughout these assessment programs are based on a dynamic model of nuclear transformation. Even when steady-state conditions are applied to estimates, the conceptual (and often the practical) basis for the steady-state is generally a dynamic model represented by a system of ordinary or partial differential equations. To assert that dynamic models are the basis for a calculation does not necessarily imply that transients are being explicitly solved for and examined.

> P. 20. Section 3.1.1 The first mention is made that the temporal scope of the scenarios is 1000 years. If I were to give RFCAB or RFSALOP advice, I would state how ludicrous the idea is of predicting consequences more than 50 years into the future. Not only is there no environmental data or models on which to base those assumptions, human behavior, societal norms, and societal stability, etc. is impossible to predict. Soil action levels should be determined only for those conditions which are presently understood. Anything more than that is part of the "garbage in/garbage out" syndrome of modeling. Furthermore, it deludes the public that scientists are capable of more than is actually possible.

For the record, we stipulate that millennial predictions of the kind required by the contract are, in our opinion, almost meaningless. Even as we carry them out, as we are required to do, we intend to help readers achieve a proper perspective about what (if any) meaning can be derived from such predictions. We would add that in the forecasting business, even 50 years is a very long time.

> P.21. Section 3.1.2 This is a rather small point but the phrase "Figure 3.1.2-2 shows the variation of ^{239}Pu concentrations" should actually read "Figure 3.1.2-2 shows the trend in ^{239}Pu concentrations". It is not incorrect to state that it shows the variation but it is misleading for the following reason, Actinide contamination of soil is extremely variable, primarily because of the particulate nature of most plutonium contamination — a reflection of the circumstances which generated the contamination and its low solubility. Few studies carefully document this variation except on a gross, macroscopic scale. Here the data points are a km apart. Variation of plutonium contamination exists on a spatial scale measured in cm.
>
> Though only a word change is suggested above ('variation' to 'trend'), the idea has greater importance in the discussion which states "RESRAD proceeds on the

> assumption of a uniformly contaminated area..." and 'For some scenarios, it could be desirable to subdivide the site .... each having a uniform concentration." What does it mean: "**...could** be desirable? At what spatial scale do you make a determination of "uniform concentration" and what is the rationale for that scale? There is no discussion of the ramification of ignoring the heterogeneity of the contamination, yet, there should be. When spatial variation is properly considered, the extremely wide probability range of possible doses become apparent. It is my opinion that none of the programs reviewed can adequately handle the true spatial variation of actinide contamination in predicting environmental transport and dose to human. Thus, it is necessary to at least state this weakness and possibly discuss the consequences of this inability to model the environment correctly.

(First paragraph) Point taken, but in the text the concern has to do with differences over a two dimensional region, and this seems more appropriately described as "variation." The word "trend" suggests low frequency variation along a line (i.e., one dimension).

(Second paragraph) We think the reviewer knows that this is a question without an easy answer. We are working on it for Task 5, and we cannot answer it in this Task 2 report. The codes reviewed here could be applied to one subplot at a time and the results summed, but the process is complicated to set up and execute and difficult to explain to casual readers, and we are not convinced that such a scheme would be necessary or even useful.

> P.23, 2nd large paragraph. In this paragraph I note that concentration units of pCi per grain are used but elsewhere, units of Bq per gram are used. I advocate two things: 1) SI units exclusively, and 2) consistency throughout the document. Many reviewers give the caveat that they are reporting what previous authors used and thus, are hesitant to change. This negative inertia only serves to continue an outdated system.

This was an oversight. For the illustration cited, the unit can just as well be Bq.

> P.23, last paragraph. The text states: "47 $\mu g\ m^{-3}$ with a standard deviation of 9.0 μm. These units are not stated to be the same though they must be made consistent.

This was a misprint.

> P. 27. Section 3.2 1 found the reference of "introduction of radioactivity into blood through injection" as a contamination pathway to be offensive and inane. It contradicts P. 19 which defines "pathway" to be "the succession of environmental media through which radionuclides move."

Injection of radioactivity has been, for many years, one method of introducing radioisotopes into the body for therapeutic and imaging purposes. This specific intake mode is not likely to be applicable to the problem at hand, but when one is making a generic list of intake modes, this is one of them. Nothing sinister was intended, and we think that would be obvious to any reasonable reader.

And in the other matter raised in this remark, we have not confused our usage of the words "mode" and "pathway," as the reviewer seems to allege. A careful reading of the first sentence reveals that the word "pathway" refers back to discussions of pathways (e.g., soil to air) in which some exposure modes (e.g., inhalation) were mentioned. A mode can be talked about in connection with a pathway without being confused with it.

> P. 27, Section 3.2 The speculation that beta emitters in close proximity to the skin may "possibly [cause] skin cancer" should either have a legitimate literature citation that provides evidence of that effect or be removed.

The hedging here had to do with how much, how close, and how long. NCRP Report No. 106 (p. 11) can be cited. [National Council on Radiation Protection and Measurements (NCRP). 1989. *Limit for Exposure to "Hot Particles" on the Skin.* NCRP Report No. 106. NCRP, Bethesda, Maryland.]

> P. 28. The discussion of the various metrics of radiation dose (with its various combinations of weighting factors) seems out of place in a section on "Exposure modes." Furthermore, I doubt whether discussion on the concept of "effective dose" has a place at all in that only the ICRP has found a use for this concept. I have never been convinced that the concept, which simply dilutes the absorbed dose to a specific organ, by the use of weighting factor (less than 1.0), to be of any value. Risk coefficients (other than those derived by ICRP) are organ specific and not applicable to effective dose.

The dose limit is expressed as (annual) effective dose, and we are required to use that metric. We are also required to perform corresponding estimates of risk.

> P. 29 The candidate computer programs are introduced. The choice of codes for review is sensible but not necessarily complete. *RAC* should at least make a comparison to screening levels *already* calculated for various scenarios by the National Council of Radiation Protection and Measurements (Report 129, issued January 1999, see the reference list).
>
> In addition, a review of how each of these models treats soil ingestion is reviewed in *Health Physics* (Simon 1998) and should be referenced. It can be seen from Table 5 of that publication that soil ingestion values for the GENII code, in particular, are not credible.

As noted previously, we will consider NCRP Report No. 129 for its applicability. However, the reviewer needs to be reminded that we were required to consider computer programs, not tables or unprogrammed models. The matter of the GENII predictions may have to do with an obsolescent database, which we will be examining in Task 5.

> P. 43. Mention is made that GENII uses organ weighting factors from ICRP 26 (a 1977 publication). I have to question why such old data is used (newer factors were recommended in 1991 by ICRP) though again, the doubtful usefulness of the effective dose is still an issue. Though this may not be the forum to debate the wisdom of the effective dose concept, it is particularly important that public

> readers understand that actinides do not contaminate or expose the body uniformly, thus, the organ dose to the lung, liver, or skeleton will be greatly diminished through the use of the weighting factor. The unfortunate situation exists that the same metric (Sv) is used for both equivalent and effective dose, thus leaving the uniformed [*sic*] reader with little information as to what the calculated dose really applies to.

Indeed, this is not the forum for debating the usefulness of the effective dose, which we are required to compute. The GENII database will need to be made comparable to that of RESRAD to permit meaningful comparisons, and this is work for Task 5.

> P. 53. Paragraph 5. *RAC* again urges "everyone ... to pay less attention to soil action levels and instead concentrate . . ." Again, it seems inappropriate that the contractor attempts to circumvent the intention of their task in print. This level of discussion should be relegated to workshops and discussion sections.

We strongly disagree that we are attempting to "circumvent the intention of [our] task in print." We fully intend to satisfy the terms of our contract and calculate soil action levels; there has never been a question about that. But we believe that such hazard indices conceal information that ought to be explicitly reviewed, and we intend to remind all parties to the discussion of that fact and to direct their attention to other ways of viewing the relationship between radionuclides in the soil and possible consequences — as we have every right and obligation to do.

> P. 54. The recommendations to the Department of Energy regarding their choice of computer interface is embarrassingly out of place in this text. DOE is neither the sponsor or a recipient of this report. Such recommendations should be make by private communication from the contractor to DOE or at most, brought to light public meetings.

This remark is very much out of place and is contradicted by other reviewers. A careful reading of the recommendations would have indicated that we were not criticizing the choice of an interface or that the graphic user interface (GUI) did not serve a purpose for many users of the program ("We are not suggesting that the GUI be eliminated . . ."), but only that it gets in the way of using RESRAD in the way we want to use it. We pointed out how the program can be made more useful for applications like this one, without changing anything about how most people use it. It is appropriate that such recommendations be conveyed in a context in which the relevant subjects and motivations are under active discussion, and that the recommendations be precisely documented, as they are in this report.

References

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Sheppard, S. C. 1995. Parameter values to model the soil ingestion pathway. Environmental Monitoring and Assessment 34(l):27-44.

Simon, S. L. 1998. Soil ingestion by humans: a review of data, history, and etiology with application to risk assessment of radioactively contaminated soil. Health Physics 74(6):647-672.

Reviewer Six

These are useful comments from a very well-informed reviewer. We are particularly impressed by his (or her) examination of background documents. The reviewer's major comment, concerning our view of treating the parameterization of each scenario as a set of constants indicates that we have not yet communicated this part of our methodology clearly, because the comment does not accurately depict our view or intended approach. We do not intend to respond to this point in detail here, but rather we will amplify the discussion in the Task 2 report in an effort to clarify it for readers (or possibly defer some aspects of it to the Task 5 report). If it is not clear to this reviewer, we accept that we probably have not made it clear to anyone.

> P. 7. Points 3 and 4 would benefit by being generalized to encompass dose or risk coefficients, and annual dose <u>or</u> lifetime risk. This would be less parochial (i.e., radiation oriented) and more consistent with Superfund. Soil Action levels are most frequently used for chemicals, based on lifetime risk and the present action levels based on dose are themselves a special case that is derived from the Superfund risk criterion of 10^{-4} lifetime risk from carcinogens (40 CFR Part 300.450(e)(2)(I)(2)).

We do not disagree in principle, but we agreed to the dose criteria as part of the contract. A lifetime risk calculation is required for each of the dose criteria, and we will provide that.

> P. 10. Following eq. 2.1-2: ... are not *necessarily* the same for all scenarios.

The dose limits presented to us are not all the same, but we agree with the added word.

> P. 11, First full para. following eq. 2.1-9: The probability that the inequalities hold in the real world also depends on the accuracy of the scenario choice. The standard must be met for most real world people, and with a reasonably good probability.

This is part of our reason for viewing the scenario as a standard rather than a statement about real people. The standard must be carefully defined with the aim in mind that meeting it would protect most real world people finding themselves in the exposure situation hypothesized by the scenario. There is no difference of opinion on the goal, but only on the best formulation for attaining the goal.

> P. 13. Last para.: Scenarios do not usually represent single people, but significant subgroups of a population that, it is assumed, can be represented by a common set of characteristics. (E.g., it would be inconsistent with the concept of RME individuals to use average breathing rates, unless the RME individuals received above average exposures for reasons not related to inhalation.)

This depends on the use to which the calculations are to be put. And we are not proposing the use of average breathing rates for a scenario subject, as the next paragraph should indicate.

> P. 14. First full para.: Why must this process be any different from that described for environmental parameters in Section 2. 1 ?

In principle, it is not. But as a matter of interpretation, combining the uncertainties associated with the source term and environmental transport with parameter distributions for a conceptual population that may or may not ever contribute a member to the envisioned exposure conditions yields a composite distribution that requires careful probabilistic interpretation, and to us the interpretation seems strained and possibly misleading. We must think of the probability that an individual chosen at random from such a population, given that such an individual encounters the exposure conditions of interest at the specified place and time, receives an annual dose not exceeding the given limit. It seems preferable to us to formulate the scenario according to the principle that the parameters should be chosen to define a hypothetical individual who would experience a dose per unit exposure at least as great as, say, 95% of the population that the individual is assumed to represent. Then this fixed scenario functions as a standard, which can be specified by listing its parameter values (not a set of distributions). With this formulation, our interpretation of the probabilistic statement is simple: it is the probability that the dose limit will not be exceeded for this scenario, period, and we may focus attention on the environmental uncertainties. This formulation is more conservative than the one the reviewer prefers, but we think not unreasonably so. Of course it is possible to combine the two kinds of distributions, but the question is, should one?

> P. 21. Is there any way to provide for the possibility of colloidal transport in the uncertainty analysis?

We are considering this question. We do not yet know the answer.

> Section 3.1.4.2. Table 3.1.5-1 indicates that the dose from Am-241 could be increased by a factor of two if ground water is included in the analysis. Given the major contribution from this isotope, it would seem imperative to include this pathway in calculating soil action levels. This is particularly the case for the rural residential scenario Tier 11 case, when institutional controls are assumed to be absent. It should also apply to any residential case applied to Tier I analyses if institutional control is not assured for the full 1000 years. It seems reasonably obvious to this reviewer that it should be assumed that the RME individual will use ground water if it is not institutionally prevented.

We substantially agree.

> P. 40. The resuspension issue is clearly critical In view of the precedents found in draft Task I for action levels at other sites it would appear to be essential to

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> make a strong case for any lower value to be applied to the Rocky Flats site. Perhaps an uncertainty analysis of environmental parameters, coupled with a somewhat conservative view on the degree of assurance required for compliance with the standard by the RME individual would be the most supportable approach. In this regard (the degree of conservatism appropriate), to what extent can we predict the effects of climate over a 1,000-yeat period on enhancement of resuspension?

The question is a reasonable one and is similar to one raised by another reviewer. The programs can be manipulated to permit analysis with different assumptions about resuspension, but the only real calibration available to us is tied to measurements made under the environmental conditions of 1983–1984. It is possible, for example, to assume that a tornado (or fire) denudes the soil east of the 903 area and enhances the resuspension for nearby off-site scenarios who may have escaped the immediate fury of the natural events. We can explore such possibilities, but our time and budget will severely limit the extent to which they can be pursued.

> Section 4.3. Could not deterministic comparisons be made, once the relevant values of parameters (e.g., 50 and 90% confidence levels) had been evaluated using RESRAD?

Without making a commitment, we will consider this possibility.

> P. 43. Second full para.: I assume that the outdated external and internal exposure factors in GENII would be updated by *RAC* for the relevant isotopes for any use of this model.

> P. 45. The result showing differences for external exposure is particularly disturbing. This pathway should be the least subject to large differences between models. I would have thought that this code would by now have incorporated the newer calculations of Eckerman and Ryman reported in Federal Guidance Report 11, in place of the old 1981 calculations of Kocher, or the 1983 soil calculations of Kocher and Sjoreen.

To the extent possible, we will reconcile the databases of GENII and RESRAD. Even RESRAD does not have the most up-to-date dosimetric data.

> P. 53. Next to last para.: While I emphatically disagree with the comment that soil action levels will become cumbersome to deal with and will offer little if any advantage, I equally emphatically agree with the suggestion that primary attention should be paid to the dose levels achieved. Even more to the point would be to pay attention to the lifetime risk levels achieved. To this end, it is recommended that the Task 5 report include a calculation of the lifetime risk for each of the action levels. This can be carried out without any difficulty using the tables in Federal Guidance Report 13 - Part I "Health Risks from Low-Level Environmental Exposure to Radionuclides" by Eckerman et al.

We think the reviewer would find that soil action levels for individual radionuclides would become cumbersome if represented by correlated distributions (think of a computer file with

1000 lines, and ten or so numbers to a line). But if isotope ratios derived from measurements by Krey and others may be assumed, it would be possible to maintain a distribution of the SAL for ^{239}Pu, which would be derived with the assumption that the specified isotope ratios prevailed at the starting time for the scenario.

The calculation of the lifetime risks is part of the contract and will be done.

> Regarding the first point, introducing uncertainties should assist rather than deter the selection of action levels. The relative abundances of the various isotopes should not vary widely over the areas of significant contamination, and thus the conditions set forth inequation2.1-1 should be relatively stable across the relevant area at the limiting levels of concentration for each scenario. It should not be difficult to select a single value for each isotope, based on the probability distributions for the SALs (as shown in Fig. 2.2-1), once the desired probability of satisfying the dose criterion is specified. Such values would be clearly easier to implement onsite during cleanup than the implied alternative, which could require extensive inputs of expensive-to-obtain point-by-point analytical data, in addition to field use of computer modeling.

We do not disagree with the comment, if we are interpreting it correctly. We think it likely that the relative abundances estimated by Krey et al. (1976), corrected for radioactive decay and formation of progeny from the early 1970s to the baseline time for the SAL, can be assumed to vary little from point to point. We did not intend to recommend the excessive analysis that would result from ignoring these isotope ratios, but we wanted to leave the handling of the question open until we formulated the Task 5 calculations.

Reviewer Seven

This reviewer's extensive and thoughtful comments deserve a fuller response than we are able to give them.

First, regarding the concern about excluding MEPAS. The rigidly enforced schedule of this project made it unavoidable that computer programs for which access could not acquired in the first two or three months could not be given further consideration. The intent, of course, is not to express prejudice against MEPAS, but we would be unable to treat MEPAS on an equal basis with the other programs. We have said in response to a previous reviewer's comment that we will consider making some deterministic calculations with MEPAS, if there is time to carry them out and include them in the report, but we do not intend that this statement be taken as a commitment that we will do so.

> The draft is thorough, accurate, and credible. It is coherent, and even though there were several authors, it does not appear to be written by a committee. However, it will not be easily understood by those unfamiliar with the task requirements, history of this particular issue at Rocky Flats, etc. Consideration should be given as to whether the final report for each task should also have a separate brief document (not the abstract in the draft) that presents the results and conclusions in a manner more generally accessible to interested non-professionals. More important, if it is not already planned, *Risk Assessment Corporation* (*RAC*) and the Oversight Panel should be planning One or more

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> summary reports at the end of the project that present the overall conclusions in a manner easily understood by various segments of the public.. This might include audio-visual summaries as well as written ones. (It would probably be more efficient overall if the summary segment on each task was prepared at the same time that the final report on each task is completed).

In technical reports, one is obliged to deal with technical matters in some detail; otherwise, reviewers complain that the authors have not been forthcoming with supporting information. We believe that an executive summary of the final report can deal with the reviewer's concerns, and we take the point about preparing task summaries as the tasks are completed.

> Page 3-4. The distinction between deterministic and probabilistic approaches is presented about as clearly as it could be. However, it should probably be stated that the 1996 soil action levels (SALs) were developed deterministically, and *RAC* might want to provide its opinion as to whether that was standard *at that time,* or whether in *RAC*'s view a probabilistic approach would have been the "contemporary modeling practice" even then.

It would be awkward to try to designate a date marking a transition of contemporary practice in this regard. The development of uncertainty analysis as a part of environmental assessment methodology goes back at least to the 1970s. It still lacks uniform and explicit acceptance by government agencies, particularly where regulatory definitions are involved, but we believe it is fair to say that contemporary practice in assessment methodology supports uncertainty analysis (and has done so for a decade or more).

> Page 4. I suggest adding one or more summary tables that provide the key comparative features of the five models considered, either here or in Section 4 (e.g., developer, year first published, applicable directly to radionuclides, yes or no; etc.) Editorial: GENII is termed a "mature and stable" product. No other model is anointed with either such a fulsome (or denigrating) short summary. (RESRAD and MMSOIL probably deserve tile same description.) There should be a summary statement for all or for none.

We will consider the comparison table. "Mature and stable" meant nothing more than that GENII has been through numerous versions and is unlikely to be modified further. But RESRAD is likely to undergo further development; we do not know about MMSOILS.

> Page 5. Editorial. Is it worth considering telegraphing the conclusion regarding previous and current versions of RESRAD here?

Probably so.

> Page 7. Editorial. The statement in the first paragraph "The soil action levels as defined do not depend..." will probably be confusing to many readers. I suggest this paragraph be broken in two, with one paragraph defining soil action levels and a second one, which might come later, discussing the "sum of ratios" topic. Also, perhaps an example could be given to more specifically show the

> relationship of soil action levels to actual concentrations (need for remediation) and the other uses.

We will add another clause to the flagged sentence. We would prefer to defer comparisons of soil action levels with existing levels in RF soils to Task 5.

> Page 10. Editorial. it might be helpful for there to be a second figure, after Figure 2.1-2, to show the geometric interpretation for a slightly more complicated scenario, especially since *RAC* emphasizes the sum of ratios approach throughout the draft. (Also, shouldn't this figure be 2.1-1, to be consistent with later numbering? (See, e.g., Fig. 2-2.1 on page 13).

We do not know what kind of second figure would be effective. A three-dimensional interpretation would be less clear because of the difficulty of indicating the inside, outside, and boundary of the tetrahedron that would correspond to the triangle in Figure 2.1-1 (number corrected), and we do not think such a figure would add any information. Perhaps some words added to the caption, indicating that all combinations of C1, C2 for which the point (C1,C2) lies on the line would make SR = 1 (although the labels in the figure also indicate this).

> Page 12. Editorial. Most readers who get this far will know what Monte Carlo techniques are, but Latin hypercube sampling may be less familiar. Do you really need to mention it specifically, or could you just refer to "other sampling techniques"?

It is not necessary to mention Latin hypercube sampling specifically.

> Page 12 and elsewhere, general point. Intellectually, I understand and agree with *RAC*'s emphasis on the use of uncertainty analysis, though that feature will eventually prove very hard to present to many segments of the public in an educational sense. However, there is another implication. Assuming the original SALs were developed deterministically (and if *RAC* has the view that was wrong at that time -- see my earlier point), then consciously or unconsciously *RAC* is raising the specter that the original SALs should be re-done. This is, as far as I can tell, both beyond the scope of the contract and more important beyond the scope of the agreement between DOE and the Rocky Flats Citizens Advisory Board. *RAC* should not lightly set the stage for such a confrontation. The *technical* answer may lie in the realm of running the models *RAC* chooses (including the newer version of RESRAD) in a "deterministic" manner (using single values instead of distributions, perhaps with a choice of reasonable but high, reasonable but low, and some median level for key parameters), to compare them "head to head" with the original SALs, as well as in the *RAC*-preferred probabilistic manner. This is an important point in my mind, perhaps one of the two most important in my review of the draft.

We do not see the conflict. *RAC* will calculate SALs as required by the contract, but *RAC* made clear in its proposal that its approach was about more than specific computer programs. *RAC* will provide deterministic SALs, along with distributions, and the deterministic versions may or may not agree with the ones that DOE has computed. *RAC*'s methods do indeed imply a critique of the DOE SALs, and we see no way of avoiding this implication of a comparison (but if this

document review proves anything, it certainly demonstrates that *RAC*'s methods will also be subject to scrutiny). After reviewing our calculations, DOE may wish to revise its own or it may defend them. It is not up to *RAC* to make decisions about how our information will be used. We do not agree with the conclusion that the deterministic calculation of vintage 1996 must be "wrong" if the uncertainty approach could have been considered contemporary at that time. Assessment analysts have frequently found themselves involved with obsolescent (even obsolete) and new methodologies at the same time. What is new and considered "best" usually languishes for a long time until the nuts and bolts can be assembled to permit everyone to implement it, and sometimes regulatory criteria are not promptly revised to accommodate it. For example, the dose conversion factors in RESRAD belong to a methodology that is at least 25 years old, and the replacement factors from ICRP are now mostly available. But we suspect that the conversion will be some time coming.

> Page 15. The resident rancher scenario has the rancher spending a total of about 15 days per year (one hour per day) off the ranch. I am personally familiar with both ranching and farming families in the northern Rockies and other semi-rural areas, and believe that this underestimates the amount of time spent off the site (trips to town for supplies, coffee shop visits with other ranchers, picking up the mail, longer duration business or family travel, vacations, etc.). Unless the scenario has been accepted by the RSAL already, or *RAC* has studies to support the one hour per day estimate, I recommend increasing it to 2 hours per day, and based on the ranching families I know, even 2 hours is probably conservative (that is, a low estimate of the time spent off the ranch).
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> Page 16. The current industrial worker scenario is an excellent addition. If the overall list of scenarios is shortened for some reason, this one should definitely be retained. As a minor point, if the current union contract stipulates only 2 weeks of vacation for a new employee, then 50 weeks is an appropriate time period. However, if there is a pattern of overtime suggesting that 2100 hours per year (or 50 weeks total time per year) is routinely exceeded, even for new employees, then 52 weeks per year should be used. In contrast, if new employees are given more than two weeks vacation per year, and there is no pattern of overtime, then a smaller number of weeks should be used.

While the recommendations made by the reviewer are reasonable for exposure scenarios in a retrospective study, for this project we must develop exposure scenarios for the distant future when we are quite uncertain about the land use. As a result, we think it is appropriate to bias some of the scenario parameters in a way that would increase estimated annual radiation dose. One of these parameters is time spent on site. We are not certain what the future may hold and therefore assume, for some of the scenarios, on-site occupancy time of 52 weeks per year. We are still in the process of finalizing our scenarios and will consider the comments made by the reviewer very carefully.

> Page 20. Editorial. The phrase at the end of the second full paragraph, beginning "sometimes they cannot..." may shed not fight but rather cast a shadow on the first clause. I recommend it be dropped. Alternatively, in later reports on other topics, *RAC* could explicitly point out where it strays from the highly appropriate "general guidelines" that are presented here.

This is only a "full disclosure" impulse that is based on our experience. If we elaborated to explain those occasions when the guidance cannot or should not be followed, it would become tedious. Since we have used the word "try" in the sentence preceding the one in question, we think deleting the offending sentence would be the better choice.

> Page 21. It is appropriate to mention the colloidal transport mechanism. Even though there is no body of data available to calibrate the models for this phenomenon, is there a way for some of the model runs to incorporate "worst case" assumptions as the analysis proceeds? Or perhaps there is another way to deal with this issue in a later task? It is important for *RAC* to try to find a way to address this issue, if at all possible under the terms of the contract it has. At the least, *RAC* should consider providing a perspective on the potential importance of such transport, and/or recommendations how DOE or others should follow up on this issue, either right away or in the near future. Otherwise, at the end of the project, no matter what *RAC*'s overall conclusions are, there will be a lingering worry that this potential threat will dwarf any other potential risks in the future.

We continue to ponder this question. We do not know what would constitute a worst case for colloidal transport, and we are doubtful that much theory can be developed during this project.

> Page 21. Regarding dividing Rocky Flats into smaller plots of land for the purpose of this project, I firmly agree with *RAC*'s "reluctance to recommend this refinement". In the final version of the report, I suggest that *RAC* be even more conclusive. This could mean a firm opinion that this degree of refinement is simply not justified, given known site conditions (in particular, the small area of high contamination, which will no doubt dominate the results), or, less satisfactory in my view, listing the "factors" that, after "careful evaluation", would require such a step, and then concluding the evaluation means this step not be taken.

This issue affects calibration of the resuspension model as well as routine calculations, and the full solution will have to await Task 5. The problem will be better formulated in terms of how the soil concentrations should be spatially averaged.

> Page 26. Editorial primarily, with one substantive suggestion. Section 3.1.4.2 states that the *RAC* team agrees with the cited 1996 study, but then states that research should be continued and groundwater issues should not be dismissed. Colloidal transport could well be mentioned as a specific research/monitoring need that others should definitely pursue (see my earlier comment), and would give some precision to the statement, In addition, one of the scenarios postulates groundwater use, and could be mentioned here as one step *RAC* is taking to deal with groundwater. In that regard, as a suggestion, some consideration could be given to revising one of the Woman Creek scenarios to substitute ground water in whole or in part for surface water. However, I do not recommend that additional scenarios be added-there are enough already.

> Page 26. 1 am not certain the phrase "simple screening exercise" does justice to the choice made and analysis done by *RAC* and the way both are presented.

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Instead, I suggest that *RAC* not use that phrase and elaborate more on why it chose to do what it did and reached the final view that it did ("should perhaps be investigated further.")

Page 26-27. Primarily editorial. The last full paragraph on page 26 and the next paragraph on page 26-27 should be clarified and firmed up. One change is to move the sentence starting "For the radionuclide..." up to be the last sentence in the prior paragraph, and starting the next paragraph with "The results of this exercise... " The implications of Table 3.1.5-1 should probably be spelled out more explicitly. Even more important, there should be a better explanation of why *RAC* "will ignore the groundwater pathway" (in fact, one of the scenarios includes it), and what the implications are (minor, major or unknown) of ignoring it. In addition to its technical implications, the way these two paragraphs ate worded raise the same specter noted earlier regarding colloidal transport. I can imagine the reaction of some segments of the public: How can we put any trust in the *RAC* conclusions if, according to their own report, *RAC* chose to 'ignore the groundwater pathway"?

We have incorporated these suggestions for editorial changes and have added some additional text to provide further explanation of the Soil Action Levels that include the groundwater pathway. In doing so, we have uncovered several misinterpretations of the analysis and have made corrections.

On the basis of these comments and the fact that one of the scenarios included groundwater ingestion, we have decided to include the groundwater pathway in our calculations for at least one of the scenarios. The groundwater analysis will only consider dissolved phase transport because colloidal transport models have not been extensively developed and could not be implemented within the time and budget constraints of this project. We note that this will probably make little difference in the overall action levels because doses are driven by inhalation and external radiation sources for most nuclides. The nuclides where differences are expected include ^{241}Pu, ^{241}Am, and ^{234}U.

Page 29. I have two major comments on this page.

First, the draft states that RESRAD was included "in accordance with the contract," which is of course true and also fundamentally needed-since this project is the direct result of the earlier use (of an earlier version) of RESRAD that led to the levels currently embodied in the cleanup effort. However, the use of the quoted phrase implies that but for the contract, *RAC* would not have chosen RESRAD. In short, this is damning with faint praise. Is this what *RAC* believes? In other words, on the basis *of* the five criteria, would RESRAD have been *rejected?* If so, say so. If not, and RESRAD would on the merits meet the five selection criteria (I think it definitely would), say so. (Editorial: why is "nominal" used before "criteria"? Are there "nominal" criteria and separate "really important" criteria?)

Second, the fifth criterion sets the final stage for rejection of MEPAS, though the scenery for this final act was put in place earlier in the draft report. I take at face

value *RAC's* statement that the criteria were developed before final decisions were made, and I understand the practical reasons MEPAS was dropped (presented on page 4 1). However, this is not totally satisfying. MEPAS is very well-known in the modeling community, as indicated by the benchmarking exercise cited in the draft, and at least in my experience is for more widely known (and understood-and used) than *GENII. (GENII* was not included in the benchmarking exercise.) In my opinion, it is a very serious matter that MEPAS was rejected, even though I understand why (because the source code was not provided).

Separately, as part of this review, as a policy issue, I am recommending that the Oversight Panel consider formally asking DOE to direct Battelle to release the source code immediately for *RAC's* evaluation, even if on a confidential basis. In a more technical mode, for *RAC's* consideration, I strongly urge that *RAC* determine if there is a way that MEPAS can be evaluated, even though (and if) source code or the equivalent "special instructions" (page 41) is not available to you. One possibility would be to reduce the results *of* the probabilistic runs *RAC* makes to single or a small set of single values (such as mean, median, mean + one standard deviation, mean - one standard deviation) and use these as inputs to a few runs of MEPAS. There may be other approaches that skilled modelers can conceive that would overcome the problem that the "front end" of MEPAS as now available to *RAC* does not lend itself to the use of the Monte Carlo approach that *RAC* understandably prefers. (In fact, it seems likely to me that this particular problem has probably been faced conceptually in recent years as the probabilistic approach has become the preferred approach, while many earlier models, not just the ones *RAC* is considering, were developed based on a "deterministic" basis.)

It is virtually certain that RESRAD would have been included in the lineup in any case, and perhaps the language used here should clarify that. The word "nominal" refers to the fact that these criteria were stated in the RFP and proposal, but other sections of the draft report indicate why some of them (e.g., (2)) should not be interpreted too literally (pure validation results are unlikely to be available for the codes, for reasons indicated in Sections 4.1.1 and 4.1.2, but they may be suitable for some validation comparisons using local data). We can drop the word "nominal" if it causes confusion.

As to MEPAS, insufficient time and resources are available at this point in the project to prepare front-end code for doing uncertainty calculations with MEPAS. We hope the panel will not follow the reviewer's well-intended recommendation to make another attempt. We have indicated previously that we will consider performing some deterministic calculations with MEPAS for Task 5 if time and resources permit, although we cannot make a firm commitment to do this.

Page 31. Editorial. Many readers will not automatically understand that "claiming validation is akin to accepting a null hypothesis." Perhaps a better comparison can be found.

We do not know a better analogy. Perhaps more explanation could replace the reference to a null hypothesis.

> Page 33ff. The issues related to different versions of RESRAD, different manuals, etc. are as well presented as they possibly could be. However, I recommend *RAC* consider, either in this report or perhaps better in a later report, presenting in some way (perhaps using tables) major differences that would result if the newest version of RESRAD were run, compared to the version used to develop the original soil action levels. My own prediction is that except for the soil resuspension issue, there will probably not be dramatic differences. If *RAC* does not undertake this comparison as part of its original work, some entities, including very possibly the Oversight Panel itself, will ask that it be done later.

We will show the comparison in Task 5. The differences are all in the resuspension pathway, and if that is exempted from the comparison, there should be no difference.

> Page 36. Editorial. Why is "virtually" used before "exhaustive"?

Clients and reviewers will always find something else that they want to see in a printout.

> Page 37. *RAC*'s recommendation that DOE provide the RESRAD source code more readily is right on the money, and separately I am recommending that the Oversight Panel itself make that recommendation to DOE. If I understand the draft correctly, *RAC* itself is able to resolve the problem of the inconsistencies in the materials and can work with the source code available to it. Instead, the spirit of *RAC*'s observation is more to advance the quality of RESRAD in the long term, not to solve a current need that *RAC* has.

Contrast this with the inexplicably negative comments of another reviewer concerning this recommendation.

> Page 37. Editorial. I suggest adding the word "regarding" between "have" and "unauthorized".

This was a misprint and will be corrected.

> Page 38. Editorial. I suggest that for clarity, "(AF)" be added after *area factor.*

We will do this.

> Page 37-41. This was a particularly hard section to understand. Perhaps the easiest solution is to present part of the overall conclusion that begins on the bottom of page 40 ("In general ....) early in this paragraph, as a roadmap for the entire section. An additional idea might be to break this into smaller subsections. Because of the overall importance of the resuspension issue, this entire subsection should be made crystal clear. This is the only subsection that needs substantial editorial work to improve its clarity.

We doubt that we can make this material crystal clear for the casual reader, but we can add some prefatory material, as the reviewer suggests. The subject is technical, as is the RESRAD supplementary document that details and defends the changes. We do not think that several smaller technical subsections would be clearer than the one larger technical subsection. Without undertaking a rather long textbook type of exposition of the substantial body of theory on which this material depends, we really do not know how to make it clearer to a general reader. We certainly can flag the details as being of primary interest to specialists (as we did for the equations defining SALs in Section 2.1) and rely on the prefatory summary to give the general reader a qualitative idea of what the results are.