RAC RESPONSES TO PEER REVIEWER COMMENTS

Task 5: Independent Calculation

Radionuclide Soil Action Level Oversight Panel

January 2000

"Setting the standard in environmental health"

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TASK 5: INDEPENDENT CALCULATION
RESPONSES TO COMMENTS ON DRAFT REPORT

INTRODUCTION

In response to our draft report "Task 5: Independent Calculation," comments were received from anonymous peer reviewers as well as from panel members and the public. In this document, we present these comments and respond to them. Most of these comments were very helpful, and, as expected, helped *RAC* to identify some elements of the Task 5 report that needed improvement. As the result of these comments, *RAC* will adapt the Task 5 report and release it in its final version in February 2000.

Each set of comments is divided by a header that identifies from whom the comments came. Each individual comment is responded to by *RAC* in text following the comment. If the comment warrants a change to the final report, that change will be made as described.

PEER REVIEW COMMENTS

Reviewer A

General Comments

1. I feel that this is, overall, an excellent piece of work. The authors have been very responsive to previous review comments, and they have used innovative approaches and mostly sound logic to extend the state of the art for this type of calculation. This is not to say that the report cannot be improved even further. I think a few more considerations, discussed below, are warranted before this is finalized.

2. Some things that are particularly impressive about this work include the degree to which it has attempted to use and integrate existing, site-specific soil data from previous research, the use of air monitoring data to refine resuspension estimates, the use of the newest inhalation and ingestion dose factors, the front-end Monte Carlo driver to RESRAD to obtain probabilistic results, the more than pedestrian modifiers to RESRAD, and a generally clear specification of what considerations were or were not utilized in the conduct of the work.

RAC thanks this reviewer for the above comments. It was certainly our goal for this work to reflect site-specific information to the fullest extent possible.

3. Some things that I think require further consideration include the initial effects, timing, magnitude and duration of secondary impacts of a prairie fire on resuspension and water erosion; the surprising effects of irrigation on actinide mobility (leaching and resuspension); and the surprisingly high ingestion doses for some scenarios (as compared to inhalation). These concerns are further explained in the specific comments.

We will address these concerns as they arise in the specific comments.

4. A critical reading of this report requires, especially, the Task 3 report entitled "*Inputs and assumptions*". One suggestion would be to combine the Task 3 and Task 5 reports, or alternatively and perhaps more easily, to include a chapter or appendix in the Task 5 report which summarizes the input parameters and assumptions used in the computations so they can be easily referred to. This would be particularly useful in trying to better-understand and rationalize the "surprises" noted in General Comment 3.

We will not combine the Task 3 and Task 5 reports, nor will we make Task 3 an appendix to Task 5. The primary reason for this is that the reports will be combined in the final report for this project. This final report will summarize the project as a whole and will include, as attachments, each of the Task reports. It would double our effort to combine the reports at this time.

Specific Comments

1. It appears to me that the effect of a prairie fire as considered in the report was the removal of vegetation cover and a subsequent increase in the rate of resuspension. It does not appear that the release of actinides during the burn itself was evaluated. Previous research gives estimates of the amount of Pu in vegetation and organic litter. I would think that using such information, a "bounding" calculation could be performed to estimate the upper limit perhaps of the release *during* the burn. Research on this issue has not been performed as far as I am aware, so there is much uncertainty on this topic resulting from a lack of knowledge. Obviously, the area covered by the burn, as well as the specific location, would be major determinants of the magnitude of the release, so these factors would require a reasonable treatment, perhaps a stochastic one.

The question of the mobilization in smoke of plutonium attached to grass and litter is a completely legitimate and important one, and many of the ingredients for carrying it out are available. Our decision not to treat this question in this report was based on the time available for performing the calculations and developing explanations and justifications for the assumptions. It seems to us that this component of potential exposure calls for yet another scenario for one or more individuals living off site, say east of Indiana Avenue. It would be necessary to modify the programs that estimate air concentrations. It would also require substantial extra effort to incorporate results of this special calculation into the RSAL scheme, because we thought it unlikely that we would be permitted to stop with estimates of dose for such a scenario. The fact that it was not practical to include this calculation in the present report should not be interpreted as a disparagement of its importance or potential interest, and we will bring this out in the recommendations.

2. I would think that the timing of a prairie fire would be very important in determining the intensity of the burn and the duration of the bare-soil condition. An early season fire, for example, would have less and greener biomass, and offer the possibility of recovery and recolonization of vegetation prior to the end of the growing season. A late growing season fire would have more and drier biomass to feed upon, leading to a hotter fire, and there would be little opportunity for revegetation until late the following spring. Also, the size of the area burned would affect the potential for resuspension and water erosion, as well as the

natural recovery rate of the vegetation. Such factors might be considered in the analysis of the effect of fire on dose to people. I am certainly pleased that the authors brought this issue up in their analysis, but the analysis suffers from the over-simplicity of the treatment, and the lack of relevant research.

We agree with the reviewer's points. What they implicitly outline is a somewhat more elaborate fire model than was carried out for the draft. The fire model for the final report will be more realistic. We will try to add some explanation of the shift toward the ingestion pathways.

3. The argument for increasing the resuspension flux by a factor of about 200 based on data from other sites (p. 5-15) is not particularly convincing to this reviewer, since the effect of vegetation removal is likely to be modified by other factors such as topography, soil texture, other effects of the fire such as depletion due to releases during the fire, plant resin releases, increases in bulk density of the surface soil due to loss of organic matter, and intense heating. While lack of knowledge is compensated for to some unknown degree by choosing large uncertainty bands, this analysis may or may not reasonably represent the actual event of a fire. I think the only solution to this problem is to strongly recommend that some highly focused research on this issue be undertaken.

We substantially agree (although some of the data were from this site). We did include a paragraph in Section 10 pointing out the potential usefulness of meteorological and dust-concentration measurements that might be taken at burned fields in the general vicinity of Rocky Flats. We will be happy to elaborate the call for the kind of research that might shed light on the fire scenario.

4. The fire analysis appears to assume that the burn covers a very large area, perhaps all of the area east of the 903 Pad containing the bulk of the contamination. By assuming scenarios with and without fire, there will likely be a tendency for RSALs to be based on the fire scenario, since this is most conservative. I would prefer an approach where the probability of a fire, as well as the probability that it would cover various fractions of the most highly contaminated zones, is simply built into the probability distribution of the resuspension factor (which should be time-dependent to account for the fire itself, the duration of the bare soil condition, and the re-vegetation phase). This way, two separate scenarios (fire vs. no fire) collapse into one basic scenario. This approach would require statistics on the annual probability of prairie fires from the Front Range region or from similar areas. I'm not certain whether such data exist, but perhaps county planners or fire departments would have some data on this.

As noted above, our fire model will be elaborated for the final report, and fire events will be treated probabilistically, incorporating the possibility of fire into every scenario.

5. I like the analysis of the effect of time on the 0-3 cm inventory fraction using historical data sets (Fig. 4-1, p. 4-3). However, I am unclear as to whether the equation refers to the top 3 or top 5 cm of soil. Is there a typo here or what?

The curve refers to the top 3 cm, but some of the data not used in the curve-fitting operation are for other depths. The dashed line refers to a data set for a 5-cm sampling depth.

6. On p. 4-8, 2nd paragraph, next to last line, "exercise" is misspelled.

Thank you for pointing out this error. The spelling will be corrected in the final version of the report.

7. On p. 4-10, 4th paragraph, last line: I think "7 x 10^{-3}" should be "7 x 10^{3}", correct?

Correct. Thank you for noting this error.

8. On p. 5-1, 2nd paragraph, it is noted that changes in the dose coefficients for inhalation and ingestion lead to "substantial" changes in the relative importance of these pathways. I would like to see a small table in the report, showing the old and new dose coefficients, so that the reader would have a more quantitative idea of just how large these changes are. This is important, because many are likely to be surprised, as I was, about the relative importance of ingestion outlined in chapter 8.

This table was shown in the Task 3 report, but we take this reviewer's point that it would be valuable to see it again at this point in the Task 5 report. We will copy this table from the Task 3 report and insert it at this location in Task 5.

9. The approach used to estimate resuspension, calibrating to real data, was very impressive and the observed vs. predicted graph for air concentrations (Fig. 5-1, p. 5-8) gives the old skeptics like me a lot of comfort.

We appreciate this comment.

10. In Table 5-1, p. 5-2, a bulk density of 1.3 g cm^{-3} for the top 1 mm of soil is probably too high, because this layer contains a large amount of organic matter and un-decomposed litter which is very light (see Webb et al., 1997, p. 11-4).

In the context of the aftermath of a fire, which is where the value is used for converting resuspension factors to resuspension rates (Section 5.3), the value seems more reasonable than it might for unburned soil. There is some difficulty in deciding exactly what we mean by the top 1-mm layer of soil.

11. In chapter 6, the RESRAD approach to resuspension is explained. However this work used an entirely different, and I think better, approach. My question is then, what is the utility of this chapter, especially since some people skimming over the report may see this and then conclude that the RESRAD default was the approach used here? Am I missing something?

We will keep this comment in mind as we revise the report.

12. On p. 7-4, first paragraph, it is implied that depletion of activity from the soil surface compartment is a function of the water infiltration rate and the K_d value. Research at CSU has shown in the laboratory that hundreds of years of simulated rainfall through soil columns does not budge particle-bound actinides. Rather, physical processes such as soil cracking from drying, animal movements, plant roots, etc. appear much more important determinants of the rate of depletion of the soil surface activity. Even dust fall tends to dilute and thus "deplete" the concentration of actinides at the soil surface. Of course, water movement through channels in the soil will carry small particles (and their actinide burdens) downward, but this is not dissolved-phase transport.

We agree completely with the comment and with its unstated conclusion, namely that tacitly using an aqueous-ion transport model, parameterized by K_d, as a surrogate for the dominant processes enumerated by the reviewer is unsatisfying at best, and is possibly misleading. It is better generally, in the absence of appropriate process-level models, to work with explicitly empirical approaches when data can be found to support them (see G.G. Killough, S.K. Rope, B. Shleien, and P.G. Voillequé, "Nonlinear estimation of weathering rate parameters for uranium in surface soil near a nuclear facility," Journal of Environmental Radioactivity 45 (1999): 95–118). However, we have stated a number of times throughout this project that we did not intend to conduct a comprehensive groundwater assessment and were looking at the pathway only to evaluate its potential for dose. We have stressed to the panel the importance of continued research in this field.

13. On p. 7-4, the risks due to cleanup are mentioned. While this was not considered in the RAC study, I think it is an issue of great importance and relevance to RSAL values.

We agree and feel that the panel should carefully consider these issues.

14. On p. 8-9, it is stated that irrigation with 1 m y^{-1} will cause substantial leaching of actinides from the surface soil. This appears to lower resuspension and to increase the relative importance of ingestion pathways. I have two problems with this logic. First, with the possible exception of uranium, I don't think irrigation alone will move the material down into the soil that much. Secondly, most of the contamination immediately east of the 903 Pad is in very rocky soil (Webb et al., 1997, p. 11-4), which is not very suitable for tilling and growing irrigated crops. I think a grazing scenario is very plausible, however. As one approaches Indiana Street, the soil becomes less rocky and a tillage/irrigation scenario becomes more plausible. Of course, tillage would effectively cover much of the surface contamination and result in lower resuspension rates of the actinides (after the dust settled from the plowing).

The reviewer is referring to a discussion that tacitly accepts the RESRAD transport model for the soil column, which must be interpreted as a surrogate for whatever natural processes are redistributing the radionuclides. As our response to comment #12 indicates, we have misgivings about this approach. We can add some words of caution to the passage in question, but we have no time to modify the calculations, and it is not clear how they should be modified to reflect the reviewer's doubts.

15. The relative importance of ingestion pathways, as mentioned, is quite unexpected. I would like to see some rational explanation of this; otherwise the credibility of these results is likely to suffer. In addition to the higher ingestion and lower inhalation dose factors, what crops are envisioned, and what were the plant/soil CR values assumed? Were the vegetables washed? What gut absorption values were assumed in the choice of ingestion dose factors? What fraction of the food ingested by the rancher family was produced on the local site?

The importance of the ingestion pathway surprised us, too. But there are several reasons why it became more important than inhalation. First, the calibration of plutonium resuspension to measured air concentrations resulted in lower air concentrations relative to air concentrations estimated using the mass loading factor as was done in the original DOE/EPA/CDPHE analysis. Second, it is important to note that soil ingestion accounts for about half the ingestion doses, and plant ingestion doses are primarily from foliar deposition and not root uptake (at least for the plutonium isotopes). Third, the ICRP 70 ingestion dose conversion factors for the plutonium isotopes of interest are substantially higher than the ICRP 30 ingestion dose conversion factors; The opposite is true for the inhalation pathway (see Table 1 and 2 below). Therefore, the ingestion pathway becomes more important in the *RAC* analysis.

Table 1. Comparison of ICRP 30 and 70 Inhalation Dose Conversion Factors used in the Calculation of RSALs by DOE/EPA/CDPHE and *RAC*

Nuclide	ICRP-70 Sol Class[a]	ICRP-70 f1	ICRP 70 (mrem/pCi)	ICRP-30 Sol Class	ICRP-30 f1	ICRP-30 (mrem/pCi)	% Difference[b]
^{234}U	s	0.002	0.03478	Y	0.05	0.132	-280%
^{235}U	s	0.002	0.03145	Y	0.05	0.123	-291%
^{238}U	s	0.002	0.0296	Y	0.05	0.118	-299%
^{237}Np	s	0.0005	0.0444	W	0.01	0.54	-1116%
^{241}Am	m	0.0005	0.1554	W	0.001	0.444	-186%
^{238}Pu	s	0.00001	0.0592	Y	0.00001	0.288	-386%
^{239}Pu	s	0.00001	0.0592	Y	0.00001	0.308	-420%
^{240}Pu	s	0.00001	0.0592	Y	0.00001	0.308	-420%
^{241}Pu	s	0.00001	0.000629	Y	0.00001	0.00496	-689%
^{242}Pu	s	0.00001	0.0555	Y	0.00001	0.293	-428%

a. s = slow, m = medium, f = fast – Dose conversion factors for an adult
b. (ICRP-70 – ICRP-30)/ICRP-30

7

Table 2. Comparison of ICRP 30 and 70 Ingestion Dose Conversion Factors used in the Calculation of RSALs by DOE/EPA/CDPHE and *RAC*

Nuclide	ICRP 70[a] f1	ICRP 70 (mrem/pCi)	ICRP 30 f1	ICRP 30 (mrem/pCi)	% Difference[b]
^{234}U	0.002	0.0001813	0.05	0.000283	-56%
^{235}U	0.002	0.0001739	0.05	0.000267	-54%
^{238}U	0.002	0.0001665	0.05	0.000269	-62%
^{237}Np	0.0005	0.000407	0.01	0.00364	-794%
^{241}Am	0.0005	0.00074	0.001	0.00444	-500%
^{238}Pu	0.00001	0.000851	0.00001	0.0000496	94%
^{239}Pu	0.00001	0.000925	0.00001	0.0000518	94%
^{240}Pu	0.00001	0.000925	0.00001	0.0000518	94%
^{241}Pu	0.00001	0.00001776	0.00001	7.66×10^{-7}	96%
^{242}Pu	0.00001	0.000888	0.00001	0.0000492	94%

a. Dose conversion factors for an adult
b. (ICRP-70 – ICRP-30)/ICRP-30

The scenarios described in Task 3 show that RESRAD limits the definition of crops to two categories: leafy vegetables and non-leafy vegetables. The soil-to-plant transfer factors for these crops were given a stochastic definition in Task 3, based on information from NCRP Report No. 129. This information is given in Table 1 in the Task 3 report. The rancher's family consumed 100% of their food from food produced on the local site. Although this is another example of a reason to combine the Task 3 and Task 5 reports, we will refrain from doing this at this time, deferring to the later final report for the project, which will contain a combination of all of the Task reports.

16. I noticed that the soil ingestion rates used were not age-specific. I think there are good data available on typical age-specific soil ingestion rates.

Our report on *Inputs and Assumptions* (Task 3) explained our approach to selecting the parameter values for soil ingestion. Soil ingestion is difficult to verify and quantify, and both inadvertent and intentional soil consumption is seen worldwide, in all cultures and age groups. Many previous soil ingestion studies focused primarily on children, under fairly idealized conditions or during more mild seasons of the year. Nevertheless, more recent studies that have considered uncertainty in their evaluations have recommended the same median soil ingestion values for children and adults, with broader uncertainty ranges for soil ingestion by children than by adults.

17. On p. 8-13, it is noted that soil and plant ingestion were treated as fixed parameters. I would think these should, if possible, be treated stochastically.

We felt it was important to **not** treat the scenario parameters as stochastic. Environmental models and parameters represent something we do not control. Scenarios, on the other hand, are under our explicit control as hypotheses that we set, not real people. They provide a means of constructing criteria for interpreting the predicted radionuclide levels in environmental media.

When we perform calculations, the calculations are really about the uncertainty in the environmental media. It seems to us to be generally confusing, if not misleading, to mix environmental probabilities with scenario population statistics to make uncertainty statements about exceeding dose limits.

18. The limiting scenarios are based on fire, and this is likely to be a quite temporary effect. Should the dose limit be applied to a single year, even if doses in all other years were likely to be much lower? Would it be appropriate to consider some averaging, since dose limits basically relate to lifetime probabilities of getting cancer from long-term exposures?

The criterion presented to us was the maximum annual dose to a scenario subject. The question of whether this is the best criterion is not entirely scientific, and there may be other options that one might prefer. However, we accepted the maximum annual dose criterion as given, just as we have had to accept other initial conditions of the study if we are ever to finish. We do plan to present probability calculations, as suggested by this reviewer. In addition, we will provide a comparison of a lifetime risk criterion (in which an integration is performed over high and low exposures) and the maximum annual dose criterion. The panel will be encouraged to take such considerations into account.

19. The groundwater analysis based only on Litaor's work (Appendix B) may be ill advised. There are other data out there to show that reducing conditions are more likely to decrease, not increase, the solubility of Pu. If a more comprehensive analysis is desired, then a much larger effort will be needed.

We did not actually base the groundwater analysis in the Task 5 report itself on measurements made by Litaor. We present these measurements in Appendix B as requested by panel members. It is true that a comprehensive analysis of the groundwater situation at Rocky Flats would require a much more extensive research effort.

20. In Appendix D, a Pu K_d of 5350 ml g^{-1} is used, while on p. 3-1, a value of 2000 is assumed. Should these values be consistent?

The maximum total dose is relatively insensitive to the plutonium K_d value for K_d values greater than ~1000 mL g^{-1}. Therefore, it makes little difference whether we used a K_d value of 2000, 2300, or 5350 mL g^{-1}. The purpose of the Figure 3-1 and Appendix D was to simply illustrate decay and ingrowth in soil and the relationship between total dose and time respectively. The figure below (Figure 1) shows the relationship between the maximum total dose after the year 2000 and the plutonium K_d value. The graph was generated using 500 RESRAD trials and holding all other model parameters constant. Plutonium K_d values were sampled from a distribution having a geometric mean of 2300 mL g^{-1} and a geometric standard deviation of 5.6. Maximum doses were achieved before the year 2100 in all cases and doses were driven by soil and plant ingestion. Higher soil K_d values result in longer soil residence times. Longer soil residence times allow for greater ingrowth of radioactive progeny and lead to higher doses.

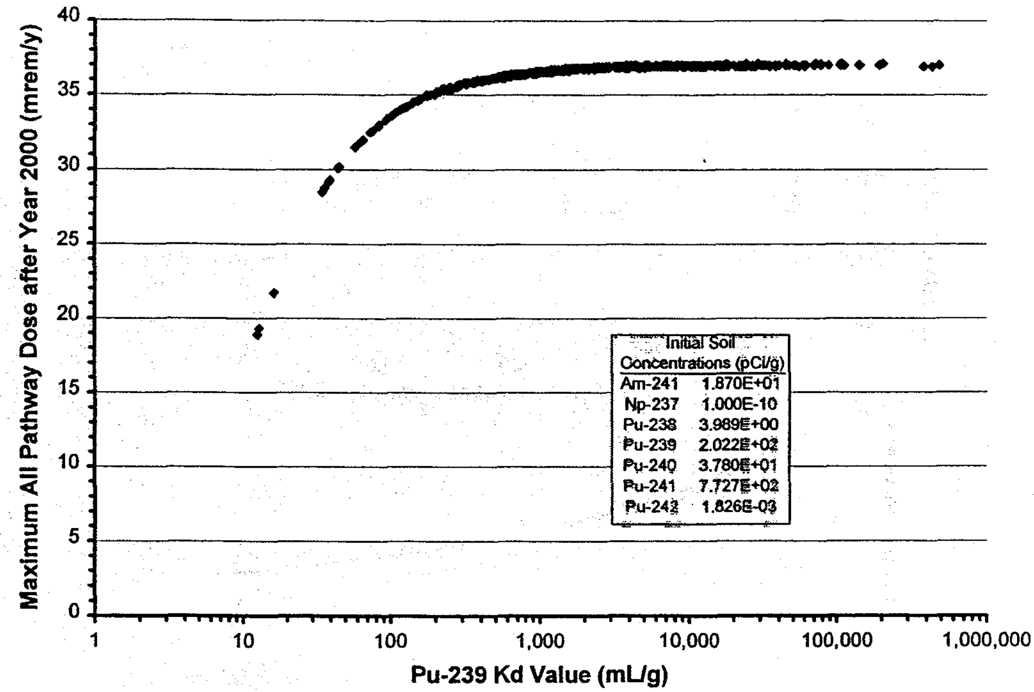

Figure 1. The maximum all pathway dose for plutonium isotopes as a function of the plutonium K_d value. Doses increase with the K_d value because higher K_d values result in lower leaching and greater activity retained in the surface soil.

We have also revised Figure D-1 in Appendix D. Figure D-1 shows the total dose as a function of time for plutonium (lower graph) and uranium (upper graph) isotopes. For both graphs, we added more points so as to better deliniate the dose in the first 100 years. We have also added 2 additional curves to each graph showing the effects of the K_d value on the time of maximum dose. For plutonium, the total dose increases slightly during the first 100 years, then drops off exponentially over time. The increase in dose during the first 100 years is caused by ingrowth of radioactive progeny, ^{241}Am and ^{237}Np. After that, dose decrease over time due to depletion of the surface soil by leaching. The leach rate is inversely related to the K_d value. Groundwater doses were negligable during the time frame of interst because transit times in the unsaturated zone exceeded 2000 years.

For the uranium isotopes, groundwater doses are appreciable because K_d values are substantially lower, but total doses are also complicated by contributions from ground exposure and ingrowth from radioactive progeny. For higher K_d values, groundwater doses are delayed because of longer transit times in the unsaturated zone.

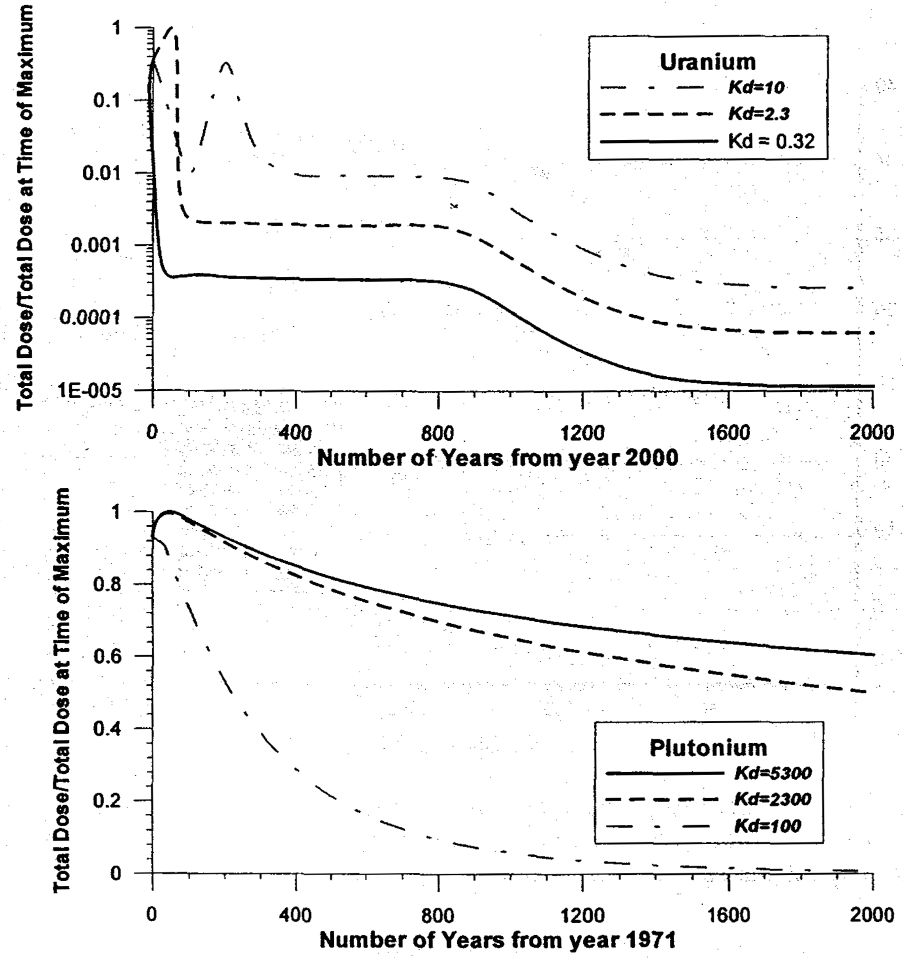

Figure 2. Ratio of the total dose to the maximum total dose as a function of time from the start of the simulation.

Reviewer B

Overall Comments

1. Overall the content and results of this task are well presented, though the several editorial glitches occasionally distracted me from the overall flow of the logic. Several sections present quite complicated information and analyses in a form that a non-specialist reader, with careful attention, will understand. I suggest, however, that consideration be given to putting some of the heavily mathematical sections (e.g., parts of Section 5, most if not all of Section 6) in appendices for review by specialists.

These Task reports for this project are intended to be technical reports, perhaps not for consumption by the general public. We have included the general summary to serve this purpose in each of the reports, and the final report for the project will be one that is made up of a general summary of the entire project, followed by the technical reports, included as attachments. Some of the sections allow themselves to more readily be adapted to the non-specialist readers, but the sections that do not lend themselves to this adaptation should not be removed from the text of the report. These mathematical sections are the work that the entire report is based upon, and it would be detrimental to the report for us to remove them.

2. More important, because the final recommendations are at such great variance with the RSALs recommended by the agencies, I strongly recommend the addition of very clear summaries, both in the General Summary and the body of the report and/or in an Appendix, perhaps with tables or lists, that present direct comparisons between the RAC and agency RSALS, and further highlight the key reasons for the differences. See, for example, page 8-3, Section 8.1.1.1 where one such a comparison is made between the agencies' RSALs and RAC's RSALs. This information should not be buried in the back and hard to tease out.

   The General Summary states in broad terms why RAC expected its RSALs (without the fire) to be "somewhat lower" than the agencies' RSALs (for the same agency-chosen scenarios, though this is not stated as explicitly as it might be). Table GS-1 presents *RAC's* numerical results for the three agency scenarios, yet there is no specific comparison provided with what the *agencies'* equivalent results were for the three scenarios in a similar table. My quick review of the background materials provided months ago did not uncover a single short table from the agencies' analysis that can be cited and immediately compared to RAC's recalculation of the three agency scenarios.[1] However, the raw material is found at various places in Section 8 of the RAC draft. I urge that such a direct tabular comparison be developed and incorporated in the final report for this task in the General Summary. It may be that some of the details behind such a comparison will need to be in an Appendix rather than the body of the report.

   In addition to providing clarity within the report itself as a technical matter, this analysis/comparison will be needed by many stakeholders if (perhaps "when" is more

[1] In addition, RAC's choice of the 5-10% probability level may be another significant reason for the difference between original agency RSALs and RAC's re-calculated values.

appropriate) the agencies challenge the RAC recommendations, as seems highly likely when the report is finalized and released to the public.

It is true, as this reviewer points out, that the reasons for the differences between the RSAL values recommended by *RAC* and the ones recommended by the agencies are numerous. It would be difficult to provide a direct tabular comparison without being misleading. For instance, as this reviewer points out, much of the difference in the RSAL values has to do with selected probability levels for the *RAC* calculations. There is not comparable value from the DOE/EPA/CDPHE calculations. We provide the textual comparisons as a means of clarification, but the tabular comparison would promote lifting that table for purposes that might be entirely out of line with our goal. The important point to note in reading the report is that there are a number of reasons why the DOE/EPA/CDPHE recommended RSAL values do not line up with the ones *RAC* has calculated. The graphical representations of the *RAC* calculated RSALs are important – to reduce those distributions of values to a single number in a table would be counter-productive.

Detailed Comments

1. Page iii. Editorial, 2nd paragraph, line 8 and elsewhere. The phrase "than a dose limit" is not quite right. I suggest "than a specified dose" or some similar re-wording. In the next line, "dose limit" should be similarly revised, as should the last sentence in the third paragraph on this page. A review of all other places this phrase is used is also a good idea.

We appreciate this editorial comment, and we, too, have struggled with this phrase. We have chosen the phrase "dose limit" to define the 15 and 85 mrem y^{-1} doses imposed as constraints to this project. We refrained from using a phrase such as "specified dose" to make it clear that *RAC* did not specify the doses to be used in this project. We think that "dose limit" accurately reflects our limitations.

2. Page iii. Editorial, 2nd paragraph, last complete sentence. The importance of the fire to the final results is so important that it should be highlighted in some way this early in the General Summary, even though stress is provided on page iv.

At this place in the summary, we are simply identifying pathways for exposure. On page iv, we discuss results.

3. Page iii. Editorial, 3rd paragraph, last sentence. There should be some adjective, such as "measured" or "specified" or "estimated" between "given" and "levels."

This change is appropriate, and we thank the reviewer for this comment.

4. Page iii. Fourth paragraph. I agree with *RAC*'s rejection of RESRAD's resuspension model and replacing it with an approach more closely linked to site data. This is an important underpinning for the final calculations and recommendations. Because of that, it is important to elaborate in the General Summary on the reasoning behind the replacement. (Remember

that many important components of the audience will only read the General Summary or press articles based on it, not the full report.)

We will add some text that reflects our reasoning behind not using the RESRAD resuspension model.

5. Page iv. Editorial, 3rd paragraph, last sentence. I suggest dropping this sentence. It doesn't really add anything substantial to the major points in the General Summary, and non-expert readers may be confused by the blunt elimination of the role of the water pathway. If this is done, then the reference to the water pathway being turned off in the first sentence of the next paragraph should also be eliminated.

The sentence in the general summary is misleading, and we will change it to more accurately reflect our intent. It is important to note, however, that the water pathway has no impact on the RSAL, so we plan to leave some mention of the concept in the paragraph.

6. Page iv. Editorial, 4th paragraph, 3rd line. The "sum of ratios" concept is introduced with no explanation, and I do not think it is really needed at all in the General Summary. I suggest dropping this entire sentence.

We will remove the reference to sum of ratios, but will introduce the concept in lay terms in this summary so as not to mislead the reader.

7: Page iv, 4th paragraph, 4th line. I suggest adding to the end of this sentence, so that it ends "...radioactive progeny, while the DOE/EPA/CDPHE RSALs were based only on some of these isotopes."

We will change the paragraph to be more accurate and explicit, but this suggested change will not be incorporated, as it is not entirely true. One of RESRAD's finer points is the inclusion of radioactive progeny. The DOE/EPA/CDPHE calculations are simply done differently, and we have not made that clear in this paragraph.

8. Page iv. Editorial, 5th paragraph, 7th line. The sentence "A significant difference..." should be the first sentence in a new paragraph.

We will make this change based on this good comment.

9. Page v, Table GS-1. To reinforce the issue raised in my overall comments, if possible there should be two tables in the General Summary. The first table should present a "head to head" comparison of the calculated soil concentrations for the three agency scenarios versus RAC's calculated levels for the same scenarios, specifically using the plutonium (no fire) and uranium (no fire) RAC figures now in Table GS-1. The second table should basically be the current GS-1. (One further option is to carve out the "with fire" options and put them into a third table. This would further highlight the important implications of this consideration.)

H

We will not include a tabular comparison, as we want to make accurate and appropriate comparisons and this is not possible within the context of our stochastic methodology. We will do our best to draw meaningful conclusions about the similarities and differences between the two methodologies.

10. Page 1-1, 3rd paragraph, 2nd line. In reviewing the draft Task 3 report, I do not find that an "annual limit" is specified for each of the seven scenarios. Instead, it appears that RAC simply adopted the 15 and 85 mrem/year dose limits as appropriate. (In fact, the only statement of the dose levels/annual limit in the draft report of Task 3 is in Table 1, page 3.) The second sentence beginning "Each scenario..." should be revised to accurately reflect what the annual limit/dose is for each scenario; perhaps a table would be helpful.

RAC did not adopt the 15 and 85 mrem y^{-1} limits as appropriate; these limits were imposed upon this project. In the final version of the Task 3 report, we incorporated these limits into the scenario table.

11. Page 1-1. Editorial, 3rd paragraph, various lines. "Consider" in line 6 should be "considered"; and "high" in the same line should be "higher."

We thank the reviewer for noticing these errors.

12. Page 1-1, 3rd paragraph, lines 6-7. Here (and later) the phrase dose limit is used. Again I suggest that there be a modifier, such as "suggested dose limit".

Although we do appreciate this suggestion, we still feel that "dose limit" accurately reflects the definition of these values.

13. Page 1-2, 3rd paragraph. I question whether the "4 orders of magnitude..." phrase needs to be in this section. (The first part of the sentence is probably OK to leave in place.) First, given the overall shape of RAC's recommendations, this observation sets the stage for some to say that the RAC recommendations, which are driven by the fire scenario, are far too conservative (at one extreme, some observers might suggest that the RAC numerical recommendations be *increased* by 4 orders of magnitude). Second, all discussions of how this large uncertainty is dealt with by RAC should be centralized in just one location, Section 5 (not Section 6), which I believe is the only other place where this range is presented.

We believe the phrase is accurate and appropriate at this point.

14. Page 1-3, 1st full paragraph. This is somewhat more than editorial. I suggest that "deliberately" be dropped and some other changes made, so that the phrasing instead be "...not surprising that in more conservative scenarios, such as the resident rancher's child,..."

We will make this change as suggested by the reviewer.

15

15. Page 1-3. Editorial, 3rd full paragraph. The updating didn't have (one) "simultaneous" effect; it had two effects acting in opposite directions. It would have had a simultaneous effect if only *one* coefficient had been updated.

We will change the sentence to read "Updating the dose coefficients had the effect of simultaneously reducing the ... and increasing the ..."

16. Page 4-1, 1st paragraph, last sentence. This sentence will be hard for the non-expert to understand until he/she reads the rest of this section. The clarity is improved if "accounts for" is replaced by "incorporates."

The phrase "accounts for" more accurately describes how the model handles the problem of nonuniformity. We thank the reviewer for this comment, but we choose to leave the phrase as it is currently written.

17. Page 4-2. Editorial, last paragraph. The sentences beginning "A 95% confidence..." and "Note that this..." provide useful insights to experts, but not to a more generally interested audience. I suggest these two sentences be put in a footnote. The sentences just before and just after these two, coupled with the generally intuitive clarity of Figure 4-1, makes the main point.

We have changed our analysis. Instead of using a confidence interval (two-sided), we will be using a confidence upper bound (one-sided), and this material will be rewritten. However, we cannot really agree with the reviewer that a careful statement of the result belongs in a footnote.

18. Page 4-3, Figure 4-1. I have several points. First, having some historical familiarity with the Poet and Martell work and the subsequent debate, I agree with the choices of the data chosen for use in the regression, and also the choice of data to be omitted. Second, the dashed line (the "separate analysis") clutters up the figure. Can the same confirmation be presented in another way, perhaps in the text? Third, why doesn't the shaded triangle extend out to capture the Webb 1996 data point? Finally, in the legend, does the last sentence imply that there is a 15% probability that the rate might be zero or even positive, that is, that plutonium might have been *accumulating* in the top 3 centimeters? I suggest you drop this last sentence altogether.

We do not agree that the dashed segment should be deleted from the figure, but the triangle will be taken out because of the different type of analysis that has been done (confidence bound rather than confidence interval for the rate coefficient). The last sentence of the caption will need to be rewritten (or removed) for the same reason. To answer the reviewer's question: the possibility of plutonium being added (positive rate coefficient) is a defect of the standard model for a log-linear regression. A more satisfactory model would incorporate the constraint of a negative (or at least nonpositive) rate, but such a model would be non-standard and more difficult to calibrate.

19. Page 4-5, 3rd paragraph, last sentence. This sentence is not clear, and also not helpful. What types of remediation decisions would require a "revisiting" of this question? In fact, what exactly is the "question"? Is it whether taking into account the age of the samples would make a difference? If you are right that insufficient data exist to justify creating a model, what good would revisiting be? I suggest dropping the last sentence altogether.

A more extensive analysis than we have had the opportunity to do could lead to a different decision about how the data should be adjusted. Our wording about the existence of profile data was not an assertion but an expression of uncertainty. However, we have no objection to dropping the last sentence.

20. Page 4-6, Figure 4-2 and page 4-9, Figure 4-4. Editorial. Unless you are very familiar with the site, it is hard to find the 903 pad. Perhaps a star or some other identifying mark should be used.

The 903 Area is identified with a diamond shape that is placed over all the other layers of details on the graphic. This open diamond seemed to give a clear contrast to the other symbols overlapping within the graphic. It still seems like the best suited for the purposes of locating the 903 Area. We will investigate the effect of making it larger.

21. Page 4-8. Editorial, 2nd paragraph, next to last line. The last word in this line should be "exercise."

Thanks to the reviewer for noticing this error.

22. Page 4-10. Editorial, 3rd paragraph, last acronym/word. I think you have generally used either RFETS or Rocky Flats, rather than RFP, in the rest of the report.

We will change this reference to Rocky Flats, and will search the document for other uses of RFP.

23. Page 4-10, 4th paragraph. I think there is at least one mistake in this paragraph. Most likely, the "with" and "without" the fire figures were reversed in the text. According to your overall reasoning (which I agree with), the plutonium air concentration would be *higher* with the fire (0.15 pCi per cubic meter) than without the fire (7.6×10^{-4} pCi per cubic meter). The current wording has either the numbers or the words reversed. In addition, while I agree with the scaling approach, I could not find the *specific* source of either the 0.15 or 7.6×10^{-4} pCi per cubic meter figures *as such* elsewhere in this report or in the Task 3 report. These two numbers are at the core of the reasoning leading to the RAC recommendations, so they need to be fully and clearly documented here and probably also in Section 5, presented in these exact units as well as Bq per cubic meter (see, for example, Table 5-4 on page 5-17). You need to make this part of your reasoning crystal clear to any reader.

The reviewer is correct to notice that the words "with" and "without" were transposed in the text. We will make this adjustment. The air concentrations for Pu were calculated with the methods of Section 5, using the program that integrates the point-source Gaussian plume over the areal soil concentration. The factor of ≈200 can also be deduced from Figure 5-6 as the ratio of the fire and grass cover resuspension fluxes. We will add an explanation at this location in the report to explain the source of these numbers.

24. Page 4-11. Editorial, end of first line. "Te" should be "The".

    Thanks to the reviewer for noticing this error.

25. Page 5-1. Editorial, 2nd paragraph, last sentence. As previously noted, the use of the new coefficients didn't have (one) "simultaneous" effect; it had two effects acting in opposite directions.

    This is not the same sentence as the one previously noted. This one is correct as written.

26. Page 5-1. Editorial, 3rd paragraph, 10th line. I suggest replacing "crude" with "simple", and dropping the "simple" before "box model".

We believe "crude" is more descriptive of the earlier RESRAD model than "simple." As to the box model, it consists of a single box. Accordingly, some adjective such as "simple" is appropriate.

27. Page 5-1. Editorial, 4th paragraph, 4th line. To clearly distinguish the RESRAD resuspension model from the RAC model, I recommend this line end as follows: "...radioactivity. The RAC model..."

    This is another good suggestion which we will take.

28. Page 5-5. Editorial, first paragraph under 5.2.2, last line. "Longer-temp temporal" should be "Longer-term temporal".

    Thanks to the reviewer for noticing this error.

29. Page 7-2, Figure 7-1. I want to identify a problem that may not have a good solution. I understand the reason for the figure. However, many people (including this reviewer) have difficulty thinking in probabilistic ways. First, at the least, the x-axis should be labeled *Radionuclide* Soil Action Level, to be in accord with the RSAL acronym in the legend. Second, the text does not adequately explain the conclusion in the legend. Third, none of the later distributions are exactly like this one, even though some have the same overall shape. The reason: Figure 7-1 has its y-axis starting at 0.01 and being *logarithmic*. **All** of the other later figures in Section 8 have the y-axis starting at 0.00 and being **linear.** This will be confusing to all but the cognoscenti. I suggest that you add another explanatory section before the specific scenarios, presenting in a generic sense the two different shapes that will be found in the later scenarios, and distinguishing these from Figure 7-1. An alternate is to

completely eliminate Figure 7.1 altogether, and simply make the same points in another way (with a table or in words).

We will make this graphic more consistent with its counterparts, linear in scale, and more comparable in a generic way to the graphics which appear later in the text. We do, however, intend to leave a graphic of this variety in the text. Although the reviewer feels that this representation is misleading to the public, we have found explanatory graphics like this one to be invaluable in describing our intent to broad audiences of people.

30. Page 7-5. Not just editorial. Section 7.2.4, last line, first word. "Decision" should be "Recommendation".

    Good comment. We will incorporate this change.

31. Page 8-3, Section 8.1.1.1. See Overall Comment earlier regarding the comparison of the agencies' RSALs to RAC's RSALs for identical scenarios.

    Again, we do not plan to include such a table.

32. Page 8-3, Table 8-1 and all subsequent tables on the agencies' scenarios in Section 8. All the tables should clearly note that these are *RAC's* recalculations of the fraction of dose, not the agencies' calculations.

Not only are they *RAC*'s recalculations of the fractions of the dose from each pathway, they are *RAC*'s recalculation of the dose and soil action level altogether. This is clarified by a sentence early in the text stating, "The RSALs presented here represent this same scenario calculated stoachastically using the methodology developed by *RAC*." We will add some text to the table title like "as calculated by *RAC*" to make this entirely clear.

33. Page 10-1. Editorial, 1st paragraph, 5th line. This should read "...probability **of** not exceeding..."

    Thanks to the reviewer for noticing this error.

34. Page 10-2. Editorial, 1st full paragraph, 4th line. This should read "...and other radionuclides **at** the Fernald site..."

    Thanks to the reviewer for noticing this error.

35. Page B-2. Editorial, 1st paragraph, 5th line. This should read "...data measured **by** Litaor..."

    Thanks to the reviewer for noticing this error.

Reviewer C

Review Summary

The content of the above named report is focused on presenting RAC's analysis of seven exposure scenarios, though, there is considerable other material also included.

This report was organized in a reasonable way. Sufficient detail was presented for some parameters, while little detail was presented for others. For example, there are no references provided for most of the parameters in Table 5-1. Possibly, that detail is in previous reports or it means that RAC has developed or chosen values from their own analyses. However, it is difficult for me to try and check the validity of such values without searching through previous documents—a task that is beyond the time I have available. I suggest RAC develop tables which are more suitable to "stand-alone".

We will see what the table is lacking and complete it for the final report.

As usual, RAC's analysis is lengthy and generally comprehensive in terms of the range of topics to be treated. Much of the material represents good work and certainly a significant expenditure of time and effort. At the same time, as a reviewer I am not satisfied with all that I read. At first read-through, this report seems comprehensive and adequate. Upon greater reflection, I have noted several problems, one which I believe is *very serious,* however, RAC may reply that they were only responding to the scope of work as it was written. A brief explanation follows with more detailed comments in order as they appear in the text.

Primary Concern

RAC has recommended a radionuclide soil action level (RSAL) based on what they believe to be the most restrictive scenario (i.e., the scenario that with the smallest soil concentration, predicts that the dose limit will not be exceeded with 95% confidence). The dose limit they use is 15 mrem/yr. However, there is a single major fault in the reasoning which led them to choose the RSAL that they did.

The limit that should guide the selection of an RSAL should be a lifetime risk of 10^{-6} – 10^{-4}, without concern for reasonably small variations of annual dose from 15 mrem/yr. I realize that this is primarily an issue that the state of Colorado should address, however, it is of paramount importance. Because RAC has adhered to the annual dose limit for any single year, they are compelled to recommend an RSAL which prevents the dose limit from being exceeded even during a relatively short period of time (one or two growing seasons). The cost of that decision is exorbitant, however. Because the land that could potentially be bared of vegetation by a prairie fire can recover its ground cover within a year or two, the cost of remediating down to a soil concentration so that those one or two years comply with the 15 mrem/yr standard is senseless, as well as contributes to squandering public resources.

> RAC recommends an RSAL that is smaller than any similar recommendation that I am familiar with anywhere in the world, and will (if accepted) obligate tax-payer's dollars to remediate the land to a significantly lower level than is required to meet the lifetime risk limits of EPA—which are some of the most protective limits in the world.
>
> Even if Colorado and DOE were to ignore the important concept of lifetime risk but still require an annual exposure limit of 15 mrem, RAC has failed to make clear that exceeding the 15 mrem year limit due to ground cover destruction by fire might only occur during part of a one or two years time (the rest of the year may have snowcover, and/or other moisture, and low-lying ground cover will revegitate after a single growing season, or two years at most). Furthermore, during the year when ground cover has been diminished (due to the fire), actions could be implemented such as spray irrigation (to minimize resuspension) during windy seasons.
>
> Moreover, RAC has not assigned a probability or likelihood of a prairie fire taking place, and to the amount of land that would likely be barren afterwards. Thus, they chose to minimize the RSAL (and to maximize the attendant costs of remediation) by setting the probability of a fire to be 100%. This is equally not acceptable and no credible analyst in the field of uncertainty would consider doing such a thing.

Rhetoric aside, these are mostly important points. In the final draft, our calculations will be extended to consider lifetime risk as a limiting criterion, enabling us to calculate RSALs that correspond to the EPA range 10^{-6} to 10^{-4}. The resulting RSALs will be compared with the ones developed from the 15 mrem annual limit. The fire calculation in the draft report (given the data) represents the worst case. As we have indicated in responses to another reviewer, we have subsequently formulated an approach that takes the annual probability of a fire into account and considers the possibility of a fire that occurs in some year (not necessarily the first year considered) and the possibility of no fire, using appropriate probability estimates for each case. When a fire occurs, random burn area (based on regional statistics) will be simulated, together with other mitigating factors (e.g., distance of burn area from occupants, regrowth time for ground cover). This reviewer has continued to question the 1000-year temporal scope of the assessment, but we feel obligated to retain this.

> For the two reasons noted above, I find the analysis invalid and believe that it contributes to a waste of tax-dollars without significantly contributing to public protection. I strongly recommend that RAC redo their analysis and their recommendations as well as notify the public and the press of the problems that I discussed above.

The final report will constitute due notification of the final results.

> Other comments follow, but none are important as that noted above.

21

Detailed Comments

1. p. 1-1. 3rd paragraph: Change as follows. "A concentration in soil higher than the level predicted as the soil action level for each radionuclide ~~would~~ could lead to a dose that would exceed the dose limit for the scenario...." Without the change, RAC fails to acknowledge uncertainty.

We might quibble that the definition is deterministic, and that for any Monte Carlo realization the RSAL is a single number defined by the scenario specification and the criterion (in this case, the maximum annual dose). However, we have no problem with either choice of words.

2. p. 1-2. RAC states: "The possibility of catastrophic natural events cannot realistically be ignored." However, RAC purposefully ignores the probability of such events. They set the likelihood of a prairie fire to be 100%.

The objection to the handling of the fire has already been answered in the general comments.

3. p. 2-2. I want to make a comment that will probably not be well received, yet I think it is worthwhile to mention. RAC frequently cites their own task reports by the primary author's name, e.g., "Weber 1999", etc. which appears in the text the same way as published, peer-reviewed literature. Three such references appear on p. 2-2. Upon looking up the reference in their Reference List, I find that these are simply previous RAC task reports and not published literature. Though there is nothing technically wrong that I can point to, I think it is misleading and I find it to be irritating.

Because this work has been building on itself since the beginning, we have found it necessary to cite our own reports throughout the duration of the project. The bibliography characterizes the reports accurately, and copies presumably could be obtained. One wonders why the reviewer considers "worthwhile" a comment that offers no alternative recommendation.

4. As in at least one previous RAC report, the authors of this report decide that some of the data published by Krey and his colleagues is inconsistent and they will not use it in their analysis. Do I need to remind the RSALOP that Krey and colleagues made long careers of environmental monitoring and RAC has little, if any, experience in field sampling, laboratory measurements and in interpretation of measurements (other than data they obtained from others publications). Was Krey consulted on this matter? If not, can RAC justify their decision, other than to say, "Data from some of the locations sampled were omitted from the regression because of the apparently inconsistent interpretations." Nothing in this paragraph convinces me of RAC's arguments. Their decision to omit data seems strictly for convenience.

To provide additional support for our choice of data used in the regression, we have identified the number of sampling sites that define each point on the graph (see Figure 3). Note that the number of sites sampled by Webb and Little far outnumber the number of sites represented by Krey and others. Had the data from each individual site been readily available, it would have been worthwhile to plot the entire data set and perform the regression using the data from individual sites. We believe we would find a similar relationship had we had done this.

Results from these studies are perplexing. There appears to be a clear evidence of a decrease in the 0–3-cm plutonium inventory between 1972 and 1989 based on the work of Little (1974), Webb (1992), and one sampling site in Krey et al. (1977). However, two of the other sites measured by Krey et al. (1977) show substantially less plutonium in the surface (0–5 cm) than was observed by Webb and Little. Little (1976) measured depth profiles at 10 sites and Webb (1992) resampled these same sites in 1989, while Krey's later measurements were from only three sites.

Numerous processes can influence plutonium migration in the subsurface, and these processes are both temporally and spatially variable. These processes include soil erosion (Webb 1993); colloidal movement (Bates et al. 1992); biotic perturbation (Litaor et al. 1994; Winsor and Whicker 1982); and soil cracking (Higley 1994). In summary, these processes are not well understood and are currently an area of research at the RFETS. Recent work by Litaor has suggested that under saturated soil conditions, plutonium can migrate very rapidly. This work is currently unpublished; however, it suggests that certain discrete events (such as heavy rainfall) may have moved plutonium into the subsurface in a relatively short time. Most of the time, plutonium has migrated very little.

We do not doubt the accuracy of the work Krey performed in the 70's, and we think it is likely that depth distributions will vary among locations. Krey's data certainly suggest large variability both spatially and temporally. The regression equation is simply an empirical means to summarize the gross behavior of plutonium in the soil.

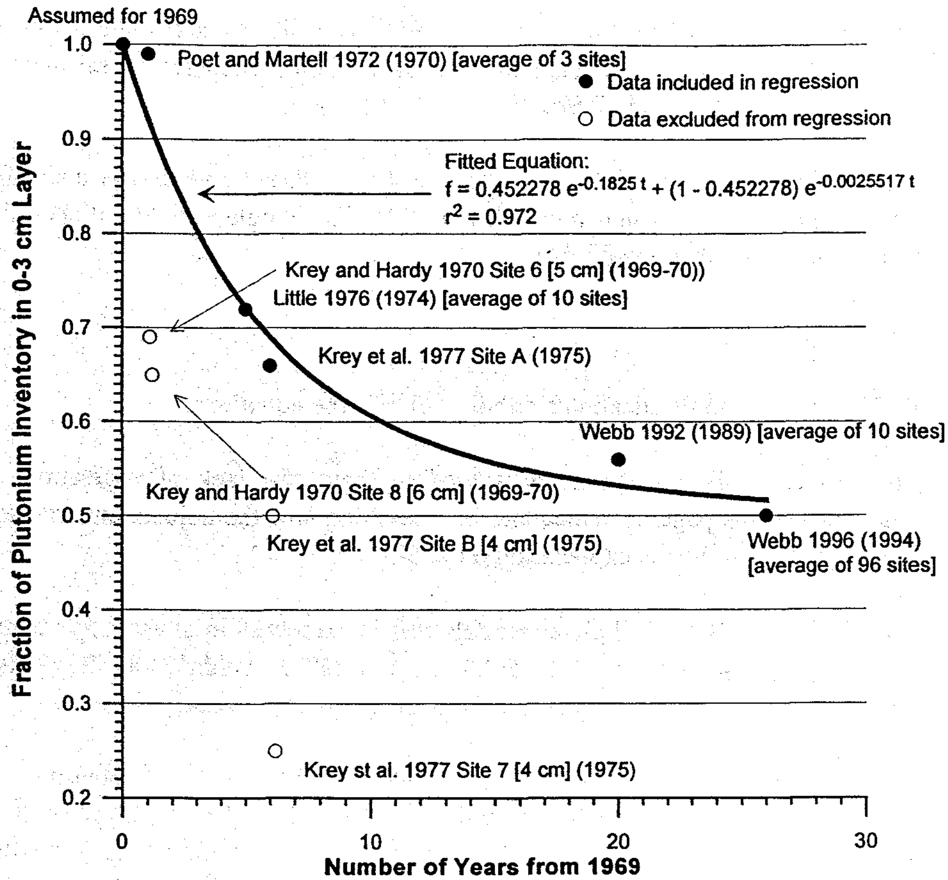

Figure 3. Regression of the percent of total plutonium inventory in the 0–3-cm soil layer as a function of time. The year in parenthesis was the year sampling took place. Data were obtained from the summary provided in Webb (1996).

5. I now raise the same question I raised in an earlier review regarding Fig. 4-4. Along a west-east line at coordinate Northing of 441.0, there is a line of measurements that are all gray circles (10-100 Bq kg^{-1}), yet they fall well outside the 2 Bq kg^{-1} contour. Where is the discussion explaining these measurements and the lack of agreement of the contours with the measurement data? What is the implication that the data is greater than the model predictions?

From the second paragraph on page 4-8: "The points shown outside the 2 Bq kg^{-1} contour indicate some observations that exceed background in the 2–10 and 10–100 Bq kg^{-1} ranges." Continued discussion regarding this figure is also found in the caption. We state quite clearly that there were sample locations above background where the model would predict background, and that our model does not accurately predict concentrations at individual locations, but is intended for integrating resuspension fluxes over large areas. The historical dose reconstruction predicted releases from the 1957 fire to have progressed in a southerly direction from the plant, and this may well have introduced perturbations which a model based on long-term wind-driven releases from the 903 Area would not predict. In any case, one cannot assume that these contours (or any set of contours based on plutonium concentrations in soil at Rocky Flats) provide exact partitions according to magnitude. Elevated off-site readings near the junction of Indiana Street and

Highway 72 have been noted by Litaor et al. (1995), who did not speculate about the source. (Ref. M.I. Litaor, D. Ellerbock, L. Allen, and E. Dovala, "Comprehensive appraisal of $^{239+240}Pu$ in soils around Rocky Flats, Colorado," *Health Phys.* 69(6), 1995.)

6. Eq. 4-5 should be reformatted. It would be fine if each variable were only a single letter, but since that is not the case, you cannot tell that "ML" is a single variable. It should be written as follows (or some other equal way):

   $C_a = ML\ x\ C_s$.

   We will introduce a multiplication symbol to clarify the equation.

7. p. 4-10. describes the change in mass loading factor for lack of vegetation. Upon first reading, I noted this page as inadequate in describing *how* the adjustment was done. I later found what I think to be the explanation on p. 5-3.

   In general, the level of detail about models that is presented in chapter 5 is confusing. For example, what of sections 5.2.1 to 5.2.3 is relevant? I couldn't tell. It seems that your explanation of how you derived your mass loading is on p. 5-3 and 5-15.

   The document at this point appears to be a combination of material submitted by different individuals because of the much different levels of technical material presented. I am not criticizing the presentation of technical material, but I was unable to determine that part that was germane. Some of what was presented surely was not necessary for the development of the parameter values.

Section 4.2 is about uranium, and Section 5 is about plutonium. Different approaches were used. The adjustment of the uranium mass loading factor for the fire scenario is based on the factor of 200 that was estimated for soil resuspension fluxes estimated from plutonium data. This should either have a forward reference to Section 5.3 or section 4.2 should be moved to the end of Section 5 and renumbered accordingly. All of the material in Sections 5.2.1 to 5.2.3 is quite technical, but it is important for providing a complete picture of the steps taken to calibrate the model using site-specific data. All of it feeds into both the regression for grass cover and the estimation of plutonium air concentration at any location given a soil flux.

8. Table 5-1. The origin of most of the numerical values is not given here.

   The soil density specified is 1.3 g/cm^3, generally a reasonable value. However, according to Webb's analysis (eq. 4-4) which is quoted in this report, that soil density would occur at a depth of 8 cm. Can RAC explain this?

This value of the bulk density was used generically and only for converting resuspension factors (m^{-1}) to resuspension fluxes (mg m^{-2} s^{-1}) relative to the top 1-mm layer of soil after a fire. It may be better to use a value consistent with Webb's profile, and we will consider making this change or at least pointing out the inconsistency.

9. The scale on Fig. 5-1 needs to be made more readable. Its written in a rather unconventional fashion.

The scale is an eccentricity of a statistical computation system. We will recreate this figure with more conventional numerals for the final report.

10. p. 5-15. For what reason did you associate the (logarithmic) mid-point of the range (3.3E-5 to 0.33 mg m^{-2} s^{-1}) with bare soil? Would not bare soil lead to the highest resuspension?

We associated most of this range generically with bare soil. The range was suggested by the tabulation of Sehmel (1984) and includes (but is not restricted to) data taken on and above bare soil. The range is interpreted as a 90% uncertainty interval, defining a lognormal distribution. Estimation of this parameter could benefit greatly by flux data specific to a recently-burned Front Range site.

11. Table 5-4. Are the soil concentrations for the site workers (17 Bq/kg) and rancher (8900 Bq/kg) correct? If so, it needs some explanation.

The site worker was located 0.6 km NNW from the center of the 903 pad. The rancher's primary location is 0.3 km E from the center of the 903 pad. The model that generated the contours in Figure 4-4 estimated these values for these locations.

12. I found Fig. 7-1 either to be labeled wrong, or you have failed to convey what you are talking about. The x-axis should read "soil concentration." If so, the RSAL is the value of the soil concentration (x-axis) chosen at whatever probability level (y-axis) that is deemed acceptable.

We will redo this graphic and correct the label.

13. p. 7-2. RAC acknowledges that a prairie wildfire is a low-frequency event (implying low probability), but they intentionally chose not to estimate the likelihood.

As noted in previous responses, the probabilistic events will be simulated.

14. p. 7-3. in paragraph 7.1.5, RAC reproduces the same confusion I found on Fig. 7-1. It should read: "...you must select a probability level that ~~conveys a degree of confidence~~ gives sufficient assurance that the selected ~~RSAL~~ soil concentration will not result in doses greater than the prescribed limit."

I cannot understand why RAC confuses RSAL with soil concentration. The RSAL is a soil concentration value that is chosen because it has the required level of confidence associated with it to ensure that the dose limit is not exceeded. The chosen value of soil concentration IS the RSAL, not the other way around.

We will change this language, which is indeed incorrect.

15. p. 7-3, paragraph, 7.1.6. The target dose limit (15 mrem/yr) is discussed. This is annual dose such that the lifetime risk will not be exceeded. Therefore, the scenarios should be lifetime representative. The prairie fire does not meet that criteria. It is a short-term perturbation only.

Once again, the prairie fire will be dealt with, and a lifetime risk criterion will be demonstrated that is lifetime representative.

16. p. 7-4. paragraph 7.2.3. The statement that "This dose limit [15 mrem/yr] is *somewhat* less than the dose constraint of 100 mem/yr..." understates the difference which is 6.7-fold.

Perhaps we understate the difference, although it is not intentional, since we obviously show the numbers to which we refer. We will take out the word "somewhat."

17. RAC further discusses the lifetime risk concept but fails to use this concept in determining their recommended RSAL.

As stated in a previous response, we will incorporate the concept of lifetime risk into the final report for this task.

18. In several of the scenarios, the "Conclusions" sections states: "...While the pathways of concern are different, the RSALs reported in DOE/EPA/CDPHE are *included* in the distributions presented here." What does this mean (that is, what does "*included* in..." mean)?

The point we intended to make was that the DOE/EPA/CDPHE values for the RSALs fall between the upper and lower limits of the RSAL distribution calculated by *RAC*. The language will be reconsidered.

19. Section 9. The main shortcoming of this section is the emphasis on any single year not exceeding 15 mrem/yr. A fire could result in exceeding the dose for only a couple of growing seasons at most. Thus, other years would not exceed the annual limit, and the lifetime dose and lifetime risk would not exceed their respective limits.

As noted in previous responses, the probabilistic events will be examined, and a more accurate lifetime exposure will be considered for comparison.

20. Section 10. The expository text is this section, as in others places in the report that I have noted, fails to accurately convey the important concept as a result of poorly written or confusing wording. The 2nd and 3rd sentences should be reworded as follows:

"These results are presented as distributions of ~~possible values~~ soil concentrations for each of seven exposure scenarios. Each soil concentration value has an associated probability of exceeding the annual dose limit. ~~Each scenario specifies an annual limit for radiation dose to~~

~~the receptor resulting from Rocky Flats radionuclides.~~ The scenarios are used to derive our recommendation of a soil concentration level to be established as the RSAL.

I found here (as in paragraph 7.1.5), that RAC confuses the RSALs with the range (or distribution) of soil concentrations from which a RSAL is chosen.

We will fix this language.

21. Appendix C. I previously commented that RAC should discuss global background concentrations of plutonium. I believe the subject could have been adequately treated in no more than half of the length of material provided in the appendix. In particular, the table of values from Holleman (Annex to the appendix) seem unnecessary and adds clutter to the report.

We included this information as an appendix in order not to add any unnecessary information to the body of the report. As the appendix is a reprint from earlier work, we intend to leave it intact.

22. Appendix D. The appendix which addresses the effect of "time" on the RSAL missed the most important time-dependent concept, that being that the ground area denuded of vegetation by a fire would revegetate quickly. Thus, the effect of time (after the fire) would be to reinstate the lower resuspension values that were applicable before the fire.

The statement the reviewer makes is true. However, the figures in Appendix D (which have been revised) apply to the no-fire situation; therefore revegetation is irrelevant. The scenario used to develop these graphs should have been stated in the text and figure caption. One other point needs to be made concerning revegetation after the fire. We are calculating annual doses, not time-integrated doses over the exposure period. Therefore, it is irrelevant whether the dose drops off after the fire because the standard we are comparing to is the annual dose during the time frame of compliance (1000 years) does not exceed the specified standard (15 mrem). If one assumes that it takes a year to revegetate the land, the annual dose will be bounded by the doses received during the year the fire occurred.

As we noted in a previous response, we will make a comparison between the maximum annual dose criterion and the maximum lifetime risk criterion. The latter calculation will take into account the effect of revegetation.

23. Appendix E. This appendix provides support to the arguments I have provided that the annual dose limit of 15 mrem/yr corresponds to a lifetime risk of 10^{-6}-10^{-4}. Why RAC has chosen to present this analysis, but then to ignore the concept of lifetime risk in the development of the RSAL, is incomprehensible to me.

We plan to include calculations that consider the concept of lifetime risk, and compare this calculation with the calculations of RSAL values for the 15 mrem dose criterion.

Reviewer D

General Comments

On the positive side, I think the way that the results generated by RAC are presented (i.e., the figures in Section 8 of this report) is concise and useful for establishing and debating the RSALs for RFETS. However, I find that the report, while long on the details of the methodology for estimating the concentrations of the radionuclides in air, lacks a comprehensive overview and provides an incomplete and unorganized description of the methodology used to generate the figures in Section 8 and the RSAL recommendations. The report (along with the companion Task 3 Inputs and Assumptions report) should live up to the standard of a scientific paper where it is required that the methodology be described in sufficient detail to allow a reader to reproduce the results (albeit with a great degree of effort). I do not believe that this draft report lives up to this standard. In addition, there are redundancies between the Task 3 and Task 5 reports that are unnecessary and contribute to the perception that the report is unorganized. I would like to see effort put into re-writing and re-organizing this report to eliminate these problems. I think it is very important for this report to be well organized and comprehensible to establish the credibility of the results and recommendations. I have some suggestions.

1. In the introduction (or a new methodology overview section), RAC should provide an overview flowchart figure of the method used for generating the results used in the figures in Section 8. I think this is absolutely essential. Their Figure 6-1 is too late, too little, and too difficult to follow to serve this purpose. Their report should be organized around such an overview flowchart. (i.e., the report should flesh out the details of the calculations/methodologies summarized in the flowchart boxes.) In other words, the flowchart should serve as the roadmap for the methodology as well as the written report. From the results generated and my understanding of similar analyses, below is a first cut on what I think the flowchart should look like for the Pu analysis. Obviously, if the Ur analysis was significantly different, it would need its own flowchart. If not, the flowchart should be generic enough to describe both analyses.

   I present the flowchart below in pseudocode style, but it should be easy to see how it could be converted to flowchart form. Obviously, it could be streamlined a great deal with judicious footnoting and supplementary text. I present this as an example of the level of detail that I think is necessary. (It may not accurately reflect RAC's analysis, but that is the point-- the report should, but does not, have sufficient details to give the reader an accurate picture of the methodology used.)

   1) i=1 where i the index for the initial Pu-239 +Pu-240 concentration in soil, [Pu]

   2) Specify $[Pu]_i$ (as a point value).

29

3) Get isotopic/nuclide ratios in soil relative to $[Pu]_i$ (See Section 1 and Task 3 Table 2) for all n where n is the isotope/nuclide index (n=Am-241, Np-237, Pu-238, Pu-239, Pu-240, Pu-242)

4) Calculate initial concentrations in soil for all n ($[Am\text{-}421]_i$, $[Np\text{-}237]_i$, ...)

5) j=1 where j is the monte carlo realization index

6) Generate random parameters for realization j (here or somewhere RAC should list all locations in report where distribution assignments for stochastic parameters are documented e.g., Task 3: Table 4 and Table 9)

7) Run RAC air concentration model to get $(MLxAF)_j$ (See Sections 4 and 5)

8) Write ResRad input file for j (See Task 3 Table 4, Table 11 and ?? . Note: here or somewhere RAC should include a comprehensive list of Tables that provide ResRad input parameter assignments used in the independent calculation.)

9) Run ResRad as modified by RAC (See Section 6)

10) For realization j, calculate/store:
   a) dose fractions by pathway (p) and nuclide (n) for all pathways and nuclides where p is the pathway index (p= e.g., ground, inhalation, radon, plant, meat, milk, and soil).
   b) SR_j, the sum of ratios where :
   $SR_j = [Pu]_i/(RSAL\text{-}Pu)_j + [Am\text{-}241]_i/(RSAL\text{-}AM\text{-}241)_j + \cdots$
   where $(RSAL\text{-}Pu)_j$ is the ResRad output RSAL for Pu for iteration j

11) Done with monte carlo iterations? If ($j<N_j$) then j=j+1 and go to (6) for next realization else go to (12)

12) Calculate/store for current $[Pu]_i$.
   a) $SRFrac_i$, the probability of exceeding the dose limit given $[Pu]_i$ determined from the fraction of SR_j greater than one, for all N_j iterations, and
   b) average dose fractions by pathway and nuclide, averaged over N_j iterations

13) Done with array of [Pu] values? If ($i<N_i$), i=i+1 and go to (2) for next [Pu], else go to (14)

14) Plot $[Pu]_i$ versus $SrFrac_i$ for all i (e.g., Figure 8-1 to Figure 8-10)

Some of us have used pseudocode algorithmic summaries in published papers, but we dislike flowcharts. We will consider introducing a pseudocode summary, but we have some misgivings. We are confident that many readers would find such a display distracting and skip over it. In full dress, it would be rather long (probably longer than the reviewer's cut at it) and uninviting. The PERL scripts are in an appendix and are extensively documented with comments.

We do not intend to do a great deal of reorganizing of the report. Other reviewers have found the general organization adequate.

2. The current Sections 3 and 4 have a lot of redundancies with the Task 3 report. These sections of this report and the Task 3 report should be carefully reviewed to eliminate these redundancies. If additional details on the isotopic ratios and the spatial distributions are required, there should be a clear statement on how these Sections relate to and/or build on the details in the Task 3 report.

We generally tried to eliminate redundancies with the Task 3 report. There were some details, however, that we thought it might be helpful for the reader to see in the context of this report. The isotopic ratios and the spatial distributions are good examples of these, because these two inputs to the code have a direct bearing on the material presented in the Task 5 report. We appreciate the reviewer's opinion, but we still feel this information is valuable at the point in the Task 5 report that it is presented.

3. I suggest that the report be reorganized as shown below. As is, the section that describes the use of ResRad is too late. Since the remainder of the report is driven by the input needs of ResRad and its deficiencies for this analysis, the ResRad section should come first after the intro and background material. In addition, the ResRad section should be substantially re-written to:

- include an overview of ResRad inputs and outputs (a summary figure would be nice)
- establish and detail the need for the development of isotopic ratios and an external mass loading model (i.e., establish the purpose of Sections 3 through 5).

In addition, currently the details of the Uranium analysis are scattered in the document. The suggested reorganization improves the consistency of the presentation of Pu and Ur analyses. Some heading edits are also suggested.

Suggested outline revisions:

1. Introduction
2. Background
3. The use of RESRAD (here the justification for the following sections should be established)
4. Isotopic Ratios
   4.1 For Pu and daughter products
   4.2 For Ur (move here from the current 4.2.2)
5. Recent spatial distributions in soil
   4.1 For Pu-239
   4.2 For Ur
6. Estimating concentrations in air
   6.1 Pu
      6.1.1 Model of resuspension and atmospheric transport...

6.1.2 Nonlinear...
6.1.3 Resuspension ...
6.1.4 Resuspension ...
6.2 Ur (move here from the current 4.2.1)

7. Scenario Results (move here from section 8, change 'distributions' to 'results'. More on this later. The results section should come before the section describing how results are used to establish RSALs.
8. Considerations in selecting...
9. Discussion and RSAL recommendations (note slight heading change)

This suggestion for reorganization is noted. Some reviewers suggested moving Section 4.2, but most seemed to believe the organization was adequate. Some minor rearrangements may be done, and some subsections will need to be added to accommodate new material, but anything major is impractical at this time.

Specific Page-by-Page Comments

1. p.iii. para.3, last sentence implies that the RSALs for Ur and Pu were determined in a joint analysis whereas p. 4-11 states that they were calculated independently. Be clear/consistent about this here.

Thank you. We will make sure that this statement is clear about the fact that plutonium and uranium RSALs were calculated separately and not in a joint analysis.

2. p. iv, para 2, first sentence 'For each scenario we present distributions...'. Use of the term 'distributions' is misleading and confusing. The standard and conventional use of the term 'distribution' is to refer to a probability distribution function (also called probability density function (pdf)) or a continuous distribution function(cdf), both of which illustrate graphically probability or cumulative probability as a function of the value of a continuous random variable. Here, as far as I can tell the concentration in soil (or RSAL) is not treated as a random variable and the figures presenting the results in section 8 are definitely not pdfs nor cdfs (thus, not distributions). This misleading term-- 'distributions'-- is used throughout the document and should be corrected throughout. The phrase 'we present distributions' could read 'we present plots'

A correction to the statement of the comment: the meaning of cdf is <u>cumulative</u> (not continuous) distribution function. In the case cited, one of the terms “plots” or “graphs” would work. There are other instances of this erroneous usage in Section 8, and they will be fixed. To say they occur throughout the document overstates the case.

3. p. iv third para. Were evaluations at > 1000 years performed? If not, is it possible that if the contamination reached the aquifer at say 1500 years then 1500 years would be the year of maximum dose? In other words, could the dose from the contaminated aquifer be large as well as late?

Our calculations limited the duration of the simulation to 1000 years. It is possible that the dose from the contaminated aquifer could come later. We recognize this shortcoming but feel obligated to continue to use the 1000-year convention.

4. p. iv, para 4, '...we would expect our RSALS to be somewhat lower than those reported previously by DOE/EPA/CDPHE'. At this point I want to know how they compare to the DOE/EPA/CDPHE numbers. I would like to see a Table of DOE/EPA/CDPHE values as well as background values here to put the recommended RSALs in perspective. An alternative to a separate table would be to augment Table GS-1 to include the DOE/EPA/CDPHE values.

We have had numerous requests for such a table. Because our RSALs were calculated stochastically and the DOE RSALs deterministically, comparisons are difficult and misleading. A table will likely be lifted and used in various other media, and we want to be very careful about what information we present in this form. We have decided not to present such a tabular relationship because of concern that it might misrepresent our own work.

5. p.v Table GS-1. for DOE-1, DOE-2, DOE-3 under column with heading 'Pu(with fire)' and 'Ur (with fire);key pathway', put a reference to a note which reads: 'fire considered in RAC scenarios only'. Why are the NA's there for RAC-4 under 'Pu(with fire)' heading? Add a note.

Thank you. We plan to complete this calculation for the final Task 5 report.

6. p. v first para, last line ' the RSAL value' should read 'the recommended RSAL value'. At end of para add something like, 'This is based in the limiting scenarios RAC-1 and RAC-2 with fire.'

This enhancement would clarify the text, but we plan to make the final report clearer in terms of our intent to provide only an <u>example</u> of how a recommendation might be formulated. The final decision about a recommendation lies with the panel.

7. p.vii. See above comments on the outline and suggested re-organization and heading changes.

Thank you.

8. p. 1-1:, para 3, line 6: 'receiving doses high' . should change 'high' to 'higher'.

We will make this change.

9. p.1-1, last para, first line. add 'at' between 'soil and 'concentration'. In last line delete 'say ^{238}U' . Add something like: 'In this study ^{238}U is used.'.

We will make such a change.

10. p.1-2, first para, first sentence. Following 'presented', delete and modify to 'as the probability of exceeding the dose limit as a function of $^{239+240}$Pu and ^{238}Ur concentrations in soil.'

We will make such a change.

11. p.1-2, last para. The first sentence seems to contradict the executive summary, p.iv, para 3.

We will change the wording in the executive summary to make our point clearer.

12. p. 1-3, 2nd para., sentence starting "in some cases the contaminated water...' also seems to contradict the executive summary p, iv, para 3.

We will change the wording in the executive summary to make our point clearer.

13. p. 1-3, para 4 ending with '(Section 8)'. Add 'as a function of radionuclide concentration in soil' to last sentence.

We will make this change.

14. p.1-3, para 5 starting with 'The probability curves...'. Replace with 'The figures...' See comment 2 above. This has to be corrected throughout the document.

No. These curves are appropriately referred to as probability curves, inasmuch as they show the probability of exceeding the dose limit as a function of radionuclide concentration in the soil.

15. p. 1-4, last line replace 'distribution' with 'figures'

No. We will say "probability curves." The original intent was probability distributions, but the sentence was written before the final mode of presentation was settled.

16. See General comments. Here is where an overview flowchart and a road map to the documentation of the analysis could be presented.

As noted in response to the general comments, we will consider pseudocode, but we will not use a flowchart.

17. p. 2-1, para 3. Reference Task 3 document at beginning of para. In last para, sentence 3 starting with 'Each scenario..', add '(i.e., exposure parameters were treated deterministically in this analysis)' to the end of the sentence.

We will make such a change.

18. p. 3-1. Why isn't this in Task 3? It was discussed in Task 3 section titled 'Initial Concentrations of radionuclides' page 7. Be clear about how it adds to the Task 3

discussion. Also, in Table 3-1, there is a superscript 'a' next to the column heading "pCi g^{-1}" that is unexplained. Is there a note missing?

We feel that the initial concentrations of radionuclides are such an important part of this calculation that it is worthwhile to reiterate this information at this point in the Task 5 report. This information is at the heart of the sum-of-ratios calculation, and it is very important to remember that this calculation is inherently included in our analysis. The footnote to which the superscript referred was misplaced. It will be restored.

19. p. 4-1.. A lot of the material in this section is redundant (verbatim) with sections in the Task 3 report. Eliminate redundancies and if this section remains, be clear about how it adds to/builds on the Task 3 material.

Again, the spatial model of contamination is the backbone of this analysis. The information included in this analysis portrays the site-specificity of our calculation, and we believe it is important to highlight it in the Task 5 report.

20. p. 4-10. Section 4.2.2. Move to Section 3.

The point here was to put the special case of uranium in one section. The entirety of Section 4.2 might be better placed somewhere else, but we have doubts about splitting it up.

21. p. 4-11, first two sentences. This is important in interpreting results. Make this point clear in the executive summary.

We will make this change to the executive summary.

22. p. 5-2, Table 5-1. It would help in reading this table to do something to differentiate heading levels either with larger indents or some formatting.

We agree with the reviewer about the format of the table and will try to make it clearer.

23. p. 6-1 first para, sentence starting "It is reasonable to apply...' After this sentence, refer back to Section 5 with something like: 'The purpose of Section 5 was to develop such a mass loading factor model for exposure locations within RFETS.' Ideally, document should be reorganized as suggested in my general comments and this section should be rewritten to establish and detail the need for the development of isotope ratios and a mass loading model.

We will consider this enhancement as a part of the reorganization.

24. p. 6-2.Figure 6-1 . See general comments. This figure should be revised to be compatible with the overview flowchart or ideally, it should be incorporated into the overview flowchart.

We still believe this figure — or something like it — to be valuable, even in the presence of a pseudocode summary.

25. p. 6-5, last 2 para.: If RAC develops an overview flowchart of their methodology as discussed in my general comments, it should help them explain this. As is, I cannot follow this description.

It is rather dense. As noted before, we will not use a flowchart. Pseudocode is possible, but we do not promise it. We will see what (if anything) can be done for this section.

26. p. 7-1. The use of the word 'criteria/criterion' in this section is wrong. Replace with 'factor'. Criterion is defined as a standard on which a decision or judgement may be based. Most of the items in the list are not decision criteria at all. In addition the first seven factors should be listed separately as the factors that are considered. The remaining should be listed as those that are not considered (are outside the scope of the work), but are discussed briefly. With this reorganization, the first sentence in the paragraph starting 'Other criteria....' needs to be revisited. This change would make this introduction more consistent with the remainder of this section.

We use the words criterion/criteria here as an alternative to other words already overused in this section. We will rearrange the introduction to this section as suggested by the reviewer.

27. Section 8, All Figures, it would be nice to mark the DOE/EPA/CDHPE RSALS on the figures.

For the scenarios DOE-1, DOE-2, and DOE-3, we recalculate the RSALs based on our stochastic methodology. It would be misleading to represent the deterministic DOE values in this context.

28. p. 8-3 Table 8-1. Are these averages over all realizations and initial Pu concentrations? This should be made clear in the text as well as in flowchart of the analysis that I suggest RAC develop.

The information in the table are not averages over all realizations but the output from a single realization. Obviously, the proportions would change somewhat between each realization, and in some cases, the order of importance would change. We will change the tables to represent the fraction of dose by pathway as determined using the nominal values (or a predefined set of values) for the various stochastic inputs.

29. Appendix C. I did not review in detail, but note that it would be useful to note the background concentrations in the executive summary for comparison to the recommended RSALs.

We will add this information to the executive summary.

30. Appendix E. This is an interesting discussion, but I did not see it referred to in the main report. Is it? If not, it should either be eliminated or referred to in an appropriate location in the main report. If it remains, it should be cleaned up.

We refer to this appendix on page 7-5. Other references will appear when we add a comparison between a lifetime risk criterion and a maximum annual dose criterion.

31. Appendix E: first para. Risk should be defined. Is it risk of a fatal radiation cancer, risk of radiation cancer, or risk of death. In first para, sentence 2, is the lifetime risk of $3x10^{-4}$ based on a 70 yr lifetime, then the sentence should read '...lifetime risk of about $3x10^{-4}$ (EPA 1997)based on a 70 year lifetime and a risk coefficient of $3x10^{-4}$ rem^{-1} where risk is defined as (fill in the blank)'.

It is the risk of fatal cancer resulting from radiation exposure. The text will be clarified accordingly.

32. Appendix E first para, sentence mid para starting 'This risk is now....'. Why are the citations in this sentence earlier than the EPA 1997 citation. Comment on this in the document by saying something like-'Earlier estimates of the risk coefficient (ICRP 1991, etc) have become more widely accepted since EPA 1997 and the risk coefficient is now estimated to be ....'

It is the risk coefficient (the risk per unit dose) for exposure to low-LET radiation that is recognized to be 5×10^{-2} Sv^{-1}. The EPA (1997) has stated that an individual effective dose limit of 15 mrem y^{-1} is equivalent to a lifetime risk of fatal cancer of about 3×10^{-4}. The text will be revised to present the two pieces of information more clearly.

33. Appendix E, End of the first para sentence starting 'We will assume...'. Why does RAC have to assume anything about the risk as at the target dose? Their whole analysis is dose based. Eliminate this sentence.

Although the analysis for the RSALs is based on dose, Appendix E provides a commentary on the risk that an annual dose limit of 15 mrem represents. The sentence in discussion provides a risk estimate for 70 years of exposure to 15 mrem every year based on the risk coefficient for exposure to low-LET radiation (5×10^{-2} Sv^{-1}) with uncertainty estimates.

34. Appendix E, last para. Add comment on Ur. Is it uniformly distributed and therefore not in need of a discussion such as the one presented for Pu here.?

No comment was provided on uranium at this point because, as the text makes clear, uranium is a lesser problem than plutonium. Also, we do not have readily available information on uranium risks that is needed for a precise evaluation as we do for plutonium in Grogan et al. (1999).

Reviewer E

As with all but one of the previous reports by these authors, this is a well prepared and mostly excellent draft. I have only a few major comments, these center around (1) the relevance of the scenarios that are valid for only a few years, (2) the validity of the dose limit for the industrial scenario, (3) possible wider application of the scenario incorporating a fire, and (4) the need for a justification for the choice of the probability level for compliance.

Major Comments

1. Relevance of infant and child scenarios to the CERCLA risk criteria

As is well known, the risk criterion set forth in the National Contingency Plan (NCP) for carcinogens provides the regulatory policy basis for the 15 mrem/y dose criterion used in this report for many of the scenarios. This risk criterion is specified as 10^{-4} to 10^{-6} *lifetime risk* (40 CFR 300.430(e)(2)(I)(A)). The relevant time frame for consideration of an annual 15 mrem dose is therefore a full lifetime. (Clearly, a year or two of exposure at 15 mrem/y would not constitute a violation of the NCP risk criterion.) In deriving the 15 mrem annual dose criterion from the lifetime risk criterion, EPA has already incorporated the risks accumulated throughout all of a normal lifetime, including those during infancy and childhood. For this reason, it is neither necessary nor appropriate to derive separate RSALs for infants and children. They are already protected to the level of the lifetime risk criterion through the RSAL for adults. Cleanup to lower RSALs derived for infants or children would result in lifetime protection at risk levels below those required by the NCP risk criterion. Fortunately, for the results presented here, since the RSALs derived for these special cases are the same or higher than those for adults in corresponding scenarios, this misuse would not occur. However, for accuracy and consistency, these inappropriate scenarios should either be dropped or just noted as consistent with the results for adults, but not necessary to protect children or infants.

In the final draft, we will present RSALs based on a maximum lifetime risk criterion in the range 10^{-6} to 10^{-4}. We will introduce factors that will represent integration of the annual dose over the lifetime of an individual, taking into account variation of source, and in particular, the year or so following a fire, considering that the exposure might be greater than in previous or subsequent years. This will (at least approximately) resolve the question of using as a criterion the dose for one year at the 15 mrem level.

As to the age-specific scenarios, we appreciate and understand the comments of this reviewer on the subject. Although we plan to present lifetime risk calculations in the final report, we did begin this project with the directive that our criterion for evaluating dose was an annual limit of 15 or 85 mrem. We then proceeded to select scenarios and parameters that were meaningful and important for us as researchers and for the panel. The child scenario is necessarily one of these, and we plan to leave it in the analysis.

What we will endeavor to do is to explain, within the context of the child scenario, the limitations of that scenario and the implications of the 15 mrem dose criterion and the lifetime risk criterion.

2. Applicability of the 85 mrem/y criterion to industrial workers

We have commented previously (in connection with the Task 2 report) on the misuse ofthis dose criterion in the original DOE report that set out the scenarios designated here as DOE-1, -2, and -3. As noted before, the 85 mrem/y dose criterion was proposed by EPA as a supplementary upper bound on the possible exposure of individuals in order to assure a minimum level of protection in the event of *unanticipated* failure of institutional controls, not as an alternative dose limit. Further, such failure was expected normally to be of short duration, because it was assumed to be corrected when identified. The criterion was not intended for application to planned long-term uses when institutional controls are assumed (i.e. planned) to no longer exist (as in the three DOE scenarios noted above) and *it was certainly never intended for use as a occupational standard,* as it is used in the *RAC*-4 scenario. The Superfund does not recognize different risk (or dose) criteria for individuals exposed as workers vs. other members of the public after a site has been cleaned up. The only way an increased dose to a worker over that permitted any member of the public would be permissible is for the situation in which the worker is exposed to be the result of licenses activities involving radiation as a part of the work product. Of course, at an industrial site, it is appropriate to take account of the decreased residency of a worker, as was done in scenario *RAC*-4. However, *the dose criterion that should be applied in RAC-4 is 15 mrem/y, not 85 mrem/y.* We note that, in the current directive under which EPA regulates radiation cleanups (OSWER Directive No. 9200.4-18; August 1997), the 85 mrem/y criterion has been dropped entirely, since it is assumed to be unneccessary.

Use of the appropriate dose criterion (15 mrem/y) would appear to reduce the RSAL for this industrial scenario (no fire) to 140 pCi/g. However, it also is not clear why this scenario was not evaluated for the case of a fire. Surely an industrial activity could continue following such an event. In this case the RSAL would appear to approach a few tens of pCi/g, close to the limiting values set by the other scenarios.

In any case, the viability of any industrial scenario depends on the guaranteed continued effectiveness of institutional control. It remains not obvious to this reviewer that either the commitments or assurance of effectiveness for the necessary institutional control exist. The DOE report depends on the "Rocky Flats Vision" for assurance of such control. This document was not available for review. However, a "vision" is not a legal commitment, and the discussion of near and immediate term land uses and, more significantly, the absence of any discussion of long-term land use (e.g. in the last paragraph on p. 6-15 of the DOE report) creates the impression that the state of commitments for and assurance of effectiveness of institutional controls in the future is very uncertain. If the lead agency (DOE), State, and local officials cannot provide reasonable assurance of maintaining effective institutional control for 1000 years, then consideration would have to be given to cleanup of the site to 15 mrem/y under scenarios that do not depend on the presence of such control. Obviously, if the RSAL for industrial use is found to be close to the for unrestricted use, the importance of such a consideration is greatly reduced.

Both the 15 and 85 mrem y^{-1} limit will be used to calculate the industrial worker scenario, giving the panel the option to choose between the two, and the fire will be incorporated probabilistically into all scenarios.

3. Applicability of the scenarios involving fire

Two considerations occur to this reviewer. The first, and most important, is that the analyses forming the basis of this report have not been able to take into account the possibility (probability, many would say) of significant future climate change during the 1000-year time horizon involved. Such changes could lead to much drier conditions that are effectively mimicked by the fire scenario. The fact that the analysis of resuspension, which has been artfully handled in this analysis, has been based on current site condition rather than on more general findings greatly heightens this concern. It would strengthen the analysis as well as the recommendations of this report if the so-called "fire" scenarios could be treated also as possible future climatic changes scenarios, or if such scenarios could be independently assessed, based on resuspension parameters of these soil types in much drier areas.

The second consideration is related to the concerns discussed under heading (1) above. The fire scenario, taken as a genuine fire (not as a surrogate for climate change as suggested above), would denude the landscape for only a limited part of a normal lifetime– perhaps a few decades. Undoubtedly there is some data on the recovery time for such events. In any case, this scenario potentially also would violate the considerations relative to *lifetime* risk noted above, albeit not so severely as the infant scenario.

The fire event, occurring (or not) sometime in the next 1000 years, will be incorporated probabilistically into all scenarios in the final report. The 1000-year temporal scope obviously introduces major uncertainties, and the prospect of climate change to an arid regime that would also enhance resuspension in ways unforeseen by our analysis of the year-2000 case must be acknowledged. We must be content to mention this in the recommendations rather than to treat it analytically in this report. Development of credible scenarios of climate change and probabilities associated with them would require resources that are not available to us.

4. Basis for the recommended probability level for compliance

The report recommends the use of between 5% and 10% for this level, but provides no basis for selection other that in "…represents a consensus among *RAC* scientists." We respectfully suggest that this is a matter on which science can offer only a definition of the range and probability of exposure, not the selection of a probability of compliance criterion. However, there is a relevant regulatory policy support for just those values (the scientists did choose the right value, its just that *their* opinion shouldn't carry any special weight!). Under CERCLA, the statute that applies in this case, the RSAL is intended to assure protection of the "*Reasonable Maximum Exposed*" (RME) individual. The following quotes are typical of EPA guidance on this subject:

"*…actions at Superfund sites should be based on an estimate of the* <u>*reasonable maximum exposure (RME)*</u> *expected to occur under both current and future land use* *conditions. The reasonable maximum exposure is defined here as the highest exposure that is reasonably expected to occur at the site...* " ("Risk Assessment Guidance for Superfund, Volume 1, Human Health Evaluation Manual (Part A) Interim Final," EPA-502/1-88-020.)

"The high-end of the risk distribution is, conceptually, above the 90^{th} percentile of the actual (either measured of estimated) distribution. The conceptual range is not meant to precisely define the limits of this descriptor, but should be used carefully by the assessor as a target range for characterizing "high-end" risk. " ("Guidance on Risk Characterization for Risk Managers and Risk Assessors," Memo from Henry Habicht II, Deputy Administrator, EPA, to Assistant Administrators and Regional Administrators, February 26, 1992).

These quote speak to the choice of the point in the distribution of risk that the RSAL should protect against, and that is what the choice of the probability level for compliance in effect does. It would add weight to the report to provide a discussion of the basis for the choice of the 5-10% probability level. It should show that it was chosen to provide protection to the RME individual. That conclusion is an important part of the recommendations.

In this connection, the report does not give sufficient attention to uncertainty in the overall projected risk distribution in future populations. That is, while the report does an excellent job in handling the distribution of dose due to uncertainty in parametric values for the models describing environmental pathways, there is not much discussion of the relative likelihoods of the different exposure scenarios. The bottom line should provide sufficient information to serve as the basis for justifying the selection of the RME individual. The report should recognize and address this issue, at least qualitatively, since it is basic to the relationship between action levels, dose limits, and the probability level selected for compliance.

The quotations provided by the reviewer assert regulatory support for the chosen 5%–10% probability levels, and they work very well with the maximum annual (effective) dose criterion. However, more than our collective opinion went into the recommendation of these probability levels. Ninety-percent confidence intervals are prevalent in practical parameter estimation, and the de facto default for tests of hypothesis is 5%, as almost any statistics text will indicate by its examples. In any case, we are pleased to cite the regulatory documents.

The probability of similar levels of protection for future populations is much harder to answer. We make a sharp distinction between environmental variables and scenario definitions, and it seems that the reviewer is concerned with predicting appropriate scenarios for future years. We have less confidence than the reviewer does about realistic prospects for this kind of prediction. Our fire-related exposures are applied to hypothetical individuals in future centuries when the fires occur, on the assumption that these individuals exist in the same exposure scenario definitions that govern their year-2000 counterparts. How else should one proceed? It is possible that 600 years in the future, the region may be sparsely populated, with no human habitation within miles (kilometers) of the site. But one can also imagine the land crowded with huts and children playing among the crumbling paving ruins of the former 903 pad. If one wishes to consider really extreme possibilities (and we do not intend to be facetious), one might postulate a terrorist with access to future technology, who manages to separate plutonium from the soil on the site and prepare it as an aerosol that he disperses among populations remote from the site. How does one assign probabilities to such cases?

It might help to remember that dose or risk estimates based on a scenario are not statements about real people. Rather, they are statements about the predicted or measured temporal evolution of the exposure environment in which the scenario subject is imagined to exist. The scenario is a mechanism for placing the state of the exposure environment on a one-dimensional scale that is crudely related to possible experiences of real people. It represents something similar to a potential function of the environmental state variables, whose value is a rough measure of what a real person might experience if a real person lived and behaved as the scenario indicates. Assessing the probability of such existence and behavior in the remote future is speculative at best. Assembling a space of scenarios over time that would credibly exhaust the universe of practical possibilities is a challenge that we doubt has been successfully addressed.

We are thus reduced to defining scenarios that are invariant in time and that seem to offer a level of potential exposure, and thus protection through inferred control levels, that seems subjectively acceptable and is incorporated into regulations. But scenarios that are invariant in time are doomed to fail as models of the future, and we believe we must be careful not to pretend otherwise.

This is a subject for futurists; we do not believe that it fits into a project with the scope and resources of this one. The questions are relevant, if only for the difficulties they raise, and we will try to find some remarks that introduce them into the report. Our recommendations might point to some research directions that could be taken. But we are not optimistic about giving them the kind of treatment the reviewer seems to contemplate.

Minor comments

p. iii Addition of the fire scenario is a major improvement. See also #3 above.

We plan to introduce the fire scenario into all of the scenarios for the final report.

p. iv Use of separate RSALs for $^{239+240}Pu$ and ^{238}U only is an excellent approach that appears to be well justified (p. 1-1). Use of the sum-of ratios calculation is also a major improvement over the DOE/EPA/CDPHE approach to calculating RSALs.

We are pleased that the reviewer concurs with our approach. We, too, believe it to be well-justified and appropriate for Rocky Flats.

p. v Why is the fire scenario omitted for the three DOE scenarios and the RAC-4 scenario (Table GS-1)? RSALs for these fire scenarios should be included or a convincing explanation added for their omission.

We plan to introduce the fire scenario into all the scenarios for the final report.

1-2 It is not clear to this reviewer that the extensive work that went into modeling the current level of exposure due to resuspension is really needed, in view of the uncertainty about the effect of future climate change. A more general approach based on experience for a wider range of climatic conditions than those presently obtaining at the site would be more convincing. However, this shortcoming can be overcome by broader consideration of the implications of the fire scenario.

As implemented, the current level of exposure applies only to the first year considered (2000), since (in the absence of a fire) that is the time of maximum exposure for Pu (though not necessarily for U). If we incorporated effects of future drought explicitly, a flux more like the one associated with the fire would be used for that period. We believe a credible analysis of the resuspension flux was required even for 2000 with contemporary ground cover because of the competition of the inhalation and soil ingestion pathways for dominance. Earlier versions of RESRAD, when applied to the DOE resident scenario, tended to favor inhalation because of older dose conversion factors and the RESRAD resuspension model. The newer RESRAD resuspension model, which is based on more realistic models, decreases the estimate of the air concentration. However, the developers themselves urge users to apply available air concentration data instead of the program's predictions. That is what we have done, bypassing the RESRAD resuspension model. The analysis of uncertainty, which we consider indispensable, required most of the work.

2-1 RAC is to be commended for the work it has done to expand the scenarios to more meaningful cases. The importance of this is demonstrated by the fact that none of the former scenarios is limiting for the choice of RSALs.

Thanks to the reviewer for this endorsement. The panel's involvement in the selection of scenarios was pivotal to the success of this project.

2-2 The use of a Monte Carlo interface for RESRAD to estimate dose distributions is commended.

This Monte Carlo interface was a required portion of the project, but it does significantly enhance the presentation of results.

4-9 We continue to be puzzled by the fact that the measured soil concentrations (Figure 4-4) below Hwy. 72 have average values that are clearly greater than the 2 Bg/kg contour shown in the figure. (We count only 18 points <2 Bq/kg, but 21 points 2-10 Bq/kg and 7 point 10-100 Bq/kg. What are we missing?) Why does this not invalidate the results obtained using the model described in Section 4.1?

As noted in the text, some of these readings could have their origin in the 1957 fire, which would not conform to the model of wind transport from the 903 pad. Elevated readings were noted near the junction of Highway 72 and Indiana Street by Litaor et al., Health Phys. 69(6), pp. 923–935, 1995, who offered no explanations. The model is based on radial power-function fits from the 903 Area, of which the graphs in Figure 4-3 are examples. We would be pleased to see all of the soil data reexamined (including some data that we had to exclude because they would have required too much time to document), adjusted for time and sampling depth, and this or a different smoothing technique (e.g., kriging) applied to produce a possibly better spatial distribution model, which would improve predictions of air concentrations. Such an undertaking was not possible within our time constraints and budget. No model will avoid the large variability indicated by the scatter in the graphs in Figure 4-3, and thus validity of the predictions must be judged by the model's mission. This is a smoothing model. It cannot be expected to give accurate point estimates at specific locations, but it provides a basis for integrating resuspension fluxes over large areas for calibration. Point predictions of concentration must be interpreted as generic.

4-10 There appear to be a couple of typos in the last two sentences of the para. preceding 4.2.2. (Inverted order, and incorrect negative exponent).

The reviewer is correct to notice this. These mistakes will be corrected.

4-11 The argument for separate RSALs for Pu and U only is sound.

We thank the reviewer for this endorsement of our technique.

5-2 Some discussion of the implication of possible large scale agricultural cultivation would be useful. (This might occur is future climate change produced more, rather than less, precipitation.)

Here again, it seems to us, is an activity for future refinement of the scenarios. It is something we can mention and treat in the recommendations.

5-14 The roughness height, z_o=0.05 m, seems low for prairie grass.

Perhaps it is. This is a number that has been recommended for uncut grass (McRae et al., "Development of a Second-Generation Mathematical Model for Urban Air Pollution I," Atmospheric Environment 16, pp. 679–696, 1982). The roughness height is approximately 1/10 of the height of the obstacle, which would correspond to grass 0.5 m tall, about knee-high for a man of average height.

5-17 Figure 5-6 is most helpful.

We are pleased that the reviewer found this figure useful.

7-2 As noted earlier, the discussion in 7.1.1 should be expanded to include the CERCLA definition of the Reasonably Maximum Exposed (RME) individual. After all, this is a CERLCA cleanup!
We fully concur with the recommendations in 7.1.2 concerning significant digits.
In section 7.1.3 the considerations noted above regarding climate change might be introduced.

The discussion in 7.1.1 will be expanded, and climate change will be mentioned in 7.1.3.

7-3 In section 7.1.5 some discussion of the regulatory policy basis (see above) for selection of this value would be more appropriate than noting scientific consensus.

We will proceed along the lines recommended by the reviewer in general comment #4.

7-4 Section 7.2.3 should be omitted since it only confuses the issue. Neither of the limits cited are comparable, since neither applies to the cleanup of this site. The ICRP recommendation applies to prospective operation of practices and is not legally applicable in this case. Although the Clean Air Act limit applies to this facility, it addresses only the air pathway, and is not a cleanup standard.

These points are well taken. We will consider deleting this section.

7-5 Section 7.2.4. Although we have not reviewed the work of Grogan, the calculation appears to be based on the use of effective dose. Such calculations are suspect, in part because of uncertainties in the weighting factors used. A more reliable estimate may be that using the uncertainties given in the just released Federal Guidance Report 13, *Cancer Risk Coefficients for Environmental Exposure to Radionuclides*, EPA 402-R-99-001, September 1999 (FG 13). Using that reference we calculate a lifetime risk of 0.6 x 10^{-4} for an inhalation dose of 15 mrem/y from Pu-239, using the dose coefficient of 0.059 mrem/pCi employed in this report (Task 3, p.10). This risk fall clearly within the EPA acceptable range of 10^{-4} to 10^{-6} lifetime risk.

We note in passing that some of the comments in Appendix E are not correct, in part because they also rely on use of effective dose. In particular, the discussion in the first paragraph regarding the risk associated with the 15 mrem/y should be based on radionuclide-specific risk calculations that do not depend on the use of weighting factors. Useful discussions of this problem are found in FG 13 at pp. 1, 2 and pp. C-22 to C-24. In the example given, thorium-232, inhalation risks derived using effective dose are 4.3 time higher than those calculated using the direct risk calculation employed in FG 13.

Finally, the last paragraph of Appendix E is not relevant to cleanup of man-made contamination, and should be deleted – the comments is gratuitous: a numerically almost identical comparison could be made of the ICRP recommendation for radon and their recommended limits for individual practices, and it would be equally irrelevant.

The calculations in the work of Grogan et al. (1999) are not based on effective dose. Organ-specific dose estimates and risk estimates were determined. No use of weighting factors was made in the analysis. Thus the concerns of the reviewer, although legitimate, are unfounded in this case.

We are obtaining a copy of the recently released Federal Guidance Report 13, *Cancer Risk Coefficients for Environmental Exposure to Radionuclides*, EPA 402-R-99-001, September 1999 for review.

We agree that the statement in the last paragraph of Appendix E is gratuitous, but because else where rigid risk limits seem to be expected it is necessary to point out that this is not always feasible and radon is a case in point – good radiation protection practices can be recommended nevertheless.

9-1 In Section 9.1, the sentence "No institutional controls have been taken into account, so the dose limit is 15 mrem/y," is puzzling and potentially troubling. The dose limit is always 15 mrem/y, whether of not institutional controls are present. The only change under the old proposed EPA standards for the case of institutional control was the addition of a further requirement that the remedy satisfy 85 mrem/y in the (assumed temporary) absence of that institutional control.

This language will be changed.

9-2 Last paragraph. This important result deserves more discussion and prominence in the recommendations. It appears to mean that onsite scenarios that depend on institutional control would not provide adequate protection to the offsite individual.

This paragraph is one that will have to be changed to reflect the incorporation of the fire into all scenarios. The conclusions will be given more prominence in the final report.

10-1 This reviewer is puzzled by the comments regarding the adequacy of RSALs for characterizing potential exposure – particularly those based on non-uniformity of contamination. We assume that the calculation of residual exposure assumed cleanup of all contamination that is above the RSAL to the level of the RSAL, and that contamination below the RSAL is assumed to be left in place. Modeling that took into account the exact distribution of contamination for all potential locations of receptors would be unreasonable, and probably not economically feasible. The comment that "the definition of a soil action level requires that the exposure environment be uniformly contaminated" is therefore misleading, at best. Of course cleanup results in exposure that is normally below the selected cleanup criterion – that is the hoped for results, and it is perfectly consistent with the underlying risk criterion, which extends two orders of magnitude below the upper bound represented by the 15 mrem/y dose limit.

The reviewer reads more into the soil action level than it actually contains if we are interpreting this comment correctly. The calculation does not evaluate residual exposure after remediation. RESRAD can only estimate the level of uniform contamination of a radionuclide that would (according to the models) give the 15 mrem annual dose limit. In our adaptation, RESRAD is tricked into using a local air/soil contamination ratio that is based on external calculations that consider the existing nonuniform spatial distribution of radioactivity. If the spatial distribution changes (e.g., by partial remediation), the air/soil contamination ratio changes, and this change will be reflected in the estimate of a soil action level. Thus, in this sense, the soil action level is not uniquely defined. The magnitude of the air/soil contamination ratio's change may be unimportant, but the lack of a clean objective definition forces us into the position that the RSAL is whatever this algorithm produces.

We appear to have a philosophical difference with the reviewer. We believe that hazard indices such as soil action levels should be applied only in very restricted and well-defined circumstances. Others seem comfortable in trying to extend their use ad hoc to far more complicated settings (this trend has been going on for more than 25 years and has been promoted by the agencies). We are doing our best to assist in making such an extension work, but we have a lingering concern that people may be reading properties into the soil action levels that they do not possess.

OTHER REVIEW COMMENTS

Brady Wilson

General

1. The purpose of this report is, clearly, to recommend soil action levels for the Rocky Flats Environmental Technology Site (RFETS) and to explain the basis for the recommendation. The report also outlines other considerations to be taken into account when stakeholders, regulators, and the DOE select the official soil action levels.

2. I applaud *Risk Assessments Corporation* (*RAC*) for outlining a list of considerations for the selection of an official soil action level. I found the list to be comprehensive and informative. I appreciate the listing of considerations that were not taken into account for *RAC*'s part in the selection process.

3. I also applaud *RAC* for their calculation of soil action levels that would be protective of an offsite rancher at Indiana Ave. This particular calculation is very relevant to the selection of a soil action level in that there are currently ranchers living and working in the immediate vicinity of RFETS.

   We thank the reviewer for all of the above comments.

4. I found that the values reported by *RAC*, for a 10% probability of exceeding the dose limit, are comparable to the values reported by DOE/CDPHE/EPA in 1996 for the same scenario and dose limit; except when the modifying factor for the effects of a fire on soil resuspension is applied.

Not just the modifying factor is different. The *RAC* calculations and the DOE/EPA/CDPHE calculations are fundamentally different because of the inclusion of the stochastic methodology in the *RAC* calculations, the change in dose conversion factors, and the resuspension model.

5. Although I agree that the effects of a fire on the resuspension of soils need to be considered, I disagree that the same rate of resuspension would exist for an entire year. Assuming that a fire occurs during a dry year and at an inopportune time in that year, it is not unreasonable to assume that the vegetation may not have an opportunity to re-establish itself to full quality within a single year - although, it is likely that within that year some re-establishment of vegetation will occur. Leaving that aside, soil moisture and snow cover within any given year would reduce the resuspension rate. The factor of 200 used to represent the occurrence of a fire should be reduced to some extent to reflect the periods of reduced resuspension during the wet months of the year.

There is, of course, room for considerable refinement of the fire scenario, but it involves more effort than we have had the time to undertake, and data that we know about are insufficient to provide a convincing range for the flux parameter. What we have presented in the draft is a rather crude formulation with very large uncertainty. We have reformulated the fire model, and it will be applied probabilistically to all scenarios. It will implicitly consider the possibility of different burn areas and other mitigating factors, such as regrowth time and distance of scenario subjects from the burned region. It does not consider augmentative effects such as delayed regrowth due to drought.

Specific Comments

1. Page 1 - 1, Fourth Paragraph
   Recently reported elevated values in surface water at a point of evaluation in the Industrial Area indicate Pu: Am ratios other than the typical ratio. The site believes that this is likely due to a source of Am in the Industrial Area, but this has not been verified (Water Working Group meeting). Therefore, it may not be appropriate to assume that the ratio of Pu to Am is always constant.

We are not assuming that the ratio of plutonium to americium is always constant. We are assuming that we know something about the ratio of plutonium to americium in soil today, based on the ratio measured in the early seventies and allowing plutonium to decay and form americium until a starting condition is defined (in our case, the year 2000). RESRAD continues this radioactive decay throughout the course of the calculation. However, our model cannot reproduce such anomalies as the one cited.

2. Page 4 – 8, Section 4.2.1
   This section should be moved into Section 5 of the report. Only information concerning the spatial distribution of uranium should exist in Section 4.

We are considering some rearrangement of the report as a result of the comments of reviewer D.

3. Page 4 – 8, Section 4.2.1
   This Section outlines a method for determining the U concentration in air that differs from that used for Pu. This Section should contain more discussion as to why the different methodology is being used for U, and why this methodology is inadequate for Pu.

Plutonium contamination is much more widespread than uranium contamination at Rocky Flats. We cannot, therefore, treat plutonium contamination as hotspots. This is identified in a sentence immediately preceding section 4.2.1

4. Page 4 – 10, Section 4.2.1, Last Sentence
   $200 * 35\ \mu g\ m^{-3} = 7.0 * 10^3\ \mu g\ m^{-3}$, correct?

   Many adept reviewers noticed this mistake, and we are grateful to them.

5. Page 8 – 3, Section 8.1.1.1, First Paragraph
   Computationally, the 115 pCi/g value reported by DOE/CDPHE/EPA in 1996 is comparable to your 170 pCi/g value because they are both sum-of-the-ratios values calculated for a 15

mrem dose limit for the same scenario. Likewise, the 1996 value of 651 pCi/g is computationally comparable to your 960 pCi/g value for an 85 mrem dose limit.

As noted above, the comparison is not as useful as one might first think. Several things were done differently, and some tend to cancel others. Recent inhalation factors and a different resuspension model reduced the inhalation component, and recent ingestion factors increased the ingestion component. The fire was not considered, but it will be incorporated into all scenarios for the final report.

6. Page 8 – 5, Section 8.1.2.1, Second Paragraph
This paragraph discusses the 15 mrem dose limit distributions for the open space scenario but the graph shown on the previous page shows the distribution for the 85 mrem dose limit. The distribution for the 15 mrem dose limit should be added to the graph, or replace the 85 mrem dose limit distribution.

   We will show both calculations in the final report.

7. Page 8 – 7, Section 8.1.3.1, First Paragraph
This paragraph discusses the 15 and 85 mrem dose limit distributions for the office worker scenario but the graph shown on the previous page shows the distribution for the 85 mrem dose limit only. The distribution for the 15 mrem dose limit should be added to the graph.

   As previously noted, we will also show the 15 mrem calculation.

8. Page 9 – 3, Figure 9 – 2
RAC should consider adding the distribution for the offsite rancher scenario without the effects of fire to this figure.

   Instead, we are incorporating the fire probabilistically into all scenarios.

Bruce Dahm

1. Page 4-10, 4th paragraph, last sentence:
It appears that a typographical error was made in the mass loading factor for the fire case ($200 \times 35\ \mu g\ m^{-3}$) is $7 \times 10^{3}\ \mu g\ m^{-3}$, not $7 \times 10^{-3}\ \mu g\ m^{-3}$. We suggest simply stating this value as $7000\ \mu g\ m^{-3}$ for ease of comparison to the TSP baseline value of $35\ \mu g\ m^{-3}$.

   This was a typographical error. We thank the reviewer for pointing this out.

LeRoy Moore

In general, the Draft Report for Task 5 is a thorough, well-done culmination of the work of a year, but I do have questions. Most of my comments are about details, a few raise more substantive issues. All comments are presented according to specific pages of the draft report.

1. p. iii, line 7 of second para: Shouldn't words "should be considered" be changed to "is required"? On line 8, shouldn't "predicted" be changed to "specified"? If not, please explain.

We used the phrase "should be considered" to reflect the many decision-making processes that go into selecting a soil action level. Just because we calculate some value does not necessarily mean that that value dictates the cleanup standard. As we point out in the Task 5 report, many aspects of decision-making are involved. We present our calculation as an example.

The word "predicted" is used to indicate that we predict a concentration in soil that would be protective by means of our calculational tools. By our definition, a concentration in soil higher than the soil action level we "predict" by means of our calculations would result in a dose above the designated limit.

2. p. v, first sentence after table should read 10 pCi g^{-1} "of soil."

We will adapt the sentence to more clearly state the appropriate meaning.

3. p. vii, give a section number and page number for References.

We will make this change.

4. p. 1-1, third para, line 6: Shouldn't "should be considered" be changed to "is required"? Line 7: Shouldn't "predicted" be changed to "specified"? (see note on p. iii above)

We used the phrase "should be considered" to reflect the many decision-making processes that go into selecting a soil action level. Just because we calculate some value does not necessarily mean that that value dictates the cleanup standard. As we point out in the Task 5 report, many aspects of decision-making are involved.

The word "predicted" is used to indicate that we predict a concentration in soil that would be protective by means of our calculational tools. By our definition, a concentration in soil higher than the soil action level we "predict" by means of our calculations would result in a dose above the designated limit.

5. p. 4-2, second and third para: Check the dates in the several references to Krey and Krey et al, esp. the 1974 date which does not appear in the references.

We could not locate the error to which the reviewer refers. The references in the reference list are Krey 1974, Krey and Hardy 1970, Krey et al. 1976, and Krey et al. 1977 in that order. This is the appropriate order for the reference list. They are called out accurately in the text.

6. p. 4-3, text immediately following the table: Nothing corresponding to the "computer archive" appears in references either under CDPHE or Litaor.

This archive consists of the data given by Litaor to the CDPHE in electronic form. We will insert some sort of reference in the reference list to define this data set, or possibly the data will be cited in a footnote.

7. p. 4-5, four lines from bottom: Define "power functions."

We will include the definition of power functions that we used in the Task 3 report.

8. p. 4-8, second para, second from last line: Spell "exercise."

Thanks to the reviewer for noting this error.

9. p. 4-10, second para, line 4: Explain "TSP" or at least list it in the references; it appears in references to an appendix on p. C-25 but not in references to this portion of the text.

TSP stands for total suspended particulate. We will note this in the report.

10. p. 4-11, first line: Spell "The."

Thanks to the reviewer for noting this error.

11. p. 5-13, lines 2-5: The assertions regarding air monitoring efficiency at Rocky Flats are questionable. W. Gale Biggs has repeatedly criticized the location of monitors as well as their efficiency in capturing particles of some sizes, including particles most susceptible to resuspension. Harvey Nichols, who has made similar criticisms, also indicts the type of monitors used at Rocky Flats; he advocates monitors that move into the wind and that can vary intake flow according to wind speed. It seems to me that *RAC* should either recalculate the monitoring efficiency or state explicitly that the calculation it makes ignores certain criticisms regarding the validity of air monitoring at Rocky Flats and is based on the sampling methods historically employed at the facility

As a part of the Historical Dose Reconstruction at Rocky Flats, an extensive study on the monitoring and particle collection capabilities of the Rocky Flats high volume particulate monitors was carried out. This study, referenced as Rope et al. 1999, carefully looked into all aspects of the historical Rocky Flats air monitors and accounted for their inadequacies. These inadequacies have been taken into account in this work as well.

12. p. 5-14, line 1 of text for Figure 5-5: Should the reference be Rope et al (1997)?

The reviewer is correct that the references do not match from the report call outs to the reference list. We will insure that the most recent version of the Rope et al. report is referenced and called out throughout the report (the one from 1999).

13. p. 7-2, item 7.1.1: Would it help to add the phrase, "the potential maximally exposed individual"?

The phrase recommended by the reviewer is one that is commonly used in regulatory guidance, and, for that reason, we choose to avoid using it here so as not to mislead the reader.

14. p. 7-5, item 7.2.4: The reader should be referred to Appendix E.

We already refer the reader to Appendix E in the section.

15. p. 7-5, item 7.2.7: The criterion, "At what soil action level would you be willing to move into the area and live on the property that has been remediated?," is certainly admirable. But it is not the same thing as "public acceptance," as is implied in the heading of this section. Members of the public may accept an RSAL of 10 pCi/g of soil for Pu without being willing to move onto the site. In June 1995 the broadly representative Rocky Flats Future Site Use Working Group produced a consensus recommendation that Rocky Flats should be cleaned to average background radiation levels when it becomes fiscally and technologically feasible to accomplish this in an environmentally sensitive manner (the Citizens Advisory Board subsequently adopted this same recommendation). An RSAL for Pu of 10 pCi/g is still 250 times the 0.04 pCi/g average background level often cited for Pu in soil along the Front Range in Colorado (the RSALs adopted in 1996 assumed an average background number for Pu of 0.038 pCi/g). From the perspective of the above-mentioned recommendations, any RSAL adopted for Rocky Flats must be seen as an interim standard that needs to be accompanied by a pledge of ongoing research in remediation to move the Rocky Flats site closer to the long-term goal of cleanup to average background level.

Many of these considerations are ones that we cannot deal with in our analysis. We will consider making the heading more consistent with the text, however.

16. p. 8-3, line 2: Shouldn't the number be 1429 rather than 1432?

We thank the reviewer for noting this error.

17. p. 8-3, third para: The text states that "our RSALs include the sum-of-ratios calculation whereas the DOE/EPA/CDPHE RSALs do not." Why not use their sum-of-ratios numbers -- that is, 651 rather than 1429, and 115 rather than 252? They do provide these numbers.

The statement in the draft is wrong. The RSALs calculated by DOE/EPA/CDPHE (1996) in their Table 5-2 were calculated by sum-of-ratios as an example. As explained above, for several reasons these numbers are not comparable to our calculation, and when the fire is included in all scenarios, the difference will likely be greater.

18. p. 8-5, item 8.1.1.2: Isn't it the case that the open space scenario was not used by DOE et al in adopting RSALs in 1996? That is, this was not for them a "limiting scenario." Shouldn't this be stated somewhere in the discussion of this scenario? Perhaps the best place is in the conclusion, but it might also be well to state it at the beginning. Also, is this paragraph misnumbered? Should it be 8.1.2.2?

The paragraph was misnumbered. We appreciate the reviewer noting this error. As to whether this was a limiting scenario for DOE/EPA/CDPHE, we simply reanalyzed it as required for this project. We have not made statements as to the interpretation of the scenarios in the previous DOE/EPA/CDPHE document in any of the other sections; we merely present the DOE/EPA/CDPHE results in each section as they appeared in the previous report and accompany them with our new calculations.

19. p. 8-7, item 8.1.3.1: Again, DOE et al did not use the 85 mrem dose for the office worker as a limiting scenario, so it seems as if *RAC* misrepresents their work in including the 6200 pCi number.

Again, we merely present the numbers calculated and reported in DOE/EPA/CDPHE 1996. We do not make statements as to their interpretation in that document.

20. p. 8-10, final line: Why not delete "about" and say "the RSAL for 239Pu, rounded to the nearest factor of five, is 10 pCi/g."

At this point, we are not rounding the values to the nearest 10, we are merely identifying the calculated 5-10% RSAL values. We will leave the text as it stands.

21. p. 8-13, final line: See preceding suggestion. Ditto for p. 8-17, final line; p. 8-25, final line; p. 8-28, final line

At this point in the text, we are not rounding the values to the nearest 10, we are merely identifying the calculated 5-10% RSAL values. We will leave the text as it stands.

22. p. B-2, para 1, line 5: Insert "by" after "measured."

We thank the reviewer for noting this error.

23. p. B-2, para 2, line 3: change "its" to "it's" or "it is."

We will make this change.

24. p. C-20, final line of first full para: Can the data provided be translated in pCi/g, perhaps in a footnote, since this appendix is a reprint from another text?

On page 4-4 in the text, we provide the conversion from bequerel per kilogram to picocurie per gram. When an unrecognizeable unit appears for the first time in the text, we chose to provide the conversion at that location and have that footnote carry throughout the text. This prevents an abundance of conversions appearing as footnotes throughout the text.

25. End of Appendix C: Either at the beginning or end of this appendix it would be helpful to include a very brief statement relating this information directly to the task of calculating the RSALs. See my second note re. p. 7-5 above.

We provide the information about plutonium background in the vicinity of Rocky Flats as a source of information only. We do not intend to make judgements about the state of cleanup at Rocky Flats based on this information.

26. p. D-2, fourth from last line: add "Pu" after "for."

We thank the reviewer for noting this error.

27. p. E-1, second line after first formula: Isn't it a mistake to say that CERCLA allows a lifetime risk of 3 X 10^{-4}? Doesn't CERCLA say a lifetime risk should be 1 X 10^{-4}? Furthermore, doesn't CERCLA state that the acceptable range for permissible exposure lies between 10-4 and 10-6, so that 1.6 or 3 X 10-4 falls outside the range of acceptable exposure according to CERCLA?

We agree there is confusion about exactly what risks are "allowed". Statements are made that it is undesirable for lifetime risks to exceed the range 10^{-6} to 10^{-4}, but 15 mrem/y is an allowed limit that corresponds to a higher lifetime risk, namely 3×10^{-4}, as stated by OSWER – Directive No. 9200-4-18. We think one problem is that rigid limits on risk, with risk methodology still so uncertain, are probably not as feasible as operational limits on dose like 15 mrem/y to provide good radiation protection.

28. p. E-1, second from last line of text: I previously questioned using the number 20 as the RBE for Pu. *RAC* should at least state that this number is recommended by certain cited ICRP publications.

Citations to the appropriate publications will be inserted in the text.

29. p. E-2, first full para: I do not understand this paragraph. Why should a 15 mrem/year dose from Pu delivered to specific internal organs be less harmful than a similar dose from another material delivered to the whole body? I realize the RBE has already been taken into account, but something more is needed to help me understand the logic here. Perhaps Helen Grogan can write a brief statement that will explain the text as it stands. I certainly am confused and in my confusion am inclined to question the principal assertion of this paragraph. It seems to say that a 15 mrem/year dose from Pu is only one-third as harmful as a like dose from, say, tritium. Can this be true?

The evidence does suggest that the risk from a given dose of plutonium is not the same as the risk from the same dose from a radionuclide that emits low-LET radiation and is uniformly distributed throughout the body. The difference arises from a combination of factors including, the non-uniform distribution of plutonium-239 within specific organs and tissues, and throughout the body, and the differing radiosensitivity of tissues and organs. The text will be revised to help explain the situation more carefully and in more detail.

Joel Selbin

1. p 1-1, 3rd para "...considered.....higher..."

We thank the reviewer for noting this error.

2. p 1-2, 3rd para "...has 4 orders of magnitude of uncertainty" How does this impact calculated RSALs?

The impact of the prairie fire on the RSALs is shown in Section 8 of the Task 5 report. Each of the rancher scenarios is calculated both including and excluding the impact of the fire. The uncertainty percentiles on the resuspension factor calculated for unvegetated soil conditions are included in the fire calculations.

3. p 1-2, last sentence on page reads poorly. maybe change to (top 1-3): "...to radionuclides which now reside in the soil..."

We do not agree that the sentence reads poorly. If the suggested alternative were used, it probably should read "to radionuclides <u>that</u> now reside in the soil . . ."

4. p 1-3, 3rd para, might be worth (if it were done) to put in how the 2 changes (above) affected the 1429 pCi/g value specifically.

It seems premature at this point in the report to begin talking about specific effects on a single number when much more methodology is yet to be laid out.

5. top p 1-4, "...but alternatives might also be possible" This allows imaginations to run wild and doubters to have a way out and criticize with impunity.

It is true, however, that alternative methodologies for calculating soil action levels are possible, and we would be remiss if we did not state that.

6. p 3-1, 3rd line, remove ed from presented

This was a transcription error and will be corrected.

7. top p 3-2, remove word "that"

This sentence was garbled in editing and will be corrected.

8. p 4-10, 4th para, The calculation appears wrong (e.g., 200 x 35 = 7 x 10exp3, not exp-3) and "with and without the fire" numbers seem reversed. or explain why not.

Many other adept reviewers noted this same error. We are grateful to all the reviewers for bringing this to our attention.

9. p 5-1, 3rd para, "If the contaminated airborne particles are assumed..." Is this a good assumption and does it have a citable basis?

The point of this and the next sentence is that it is generally not a good assumption. RESRAD first calculates the airborne concentration of radioactivity using this assumption and then corrects the result with the area factor.

10. p 7-2, 3rd line, remove "of these"

We could not identify the phrase to which the reviewer referred.

11. 7.1.2, last line means that all RSALs between 1 and 94 (or 95), go to ONE significant figure, not TWO. Thus if 20 is meant to express two significant figures then it should be written 20., i.e., with a decimal point. 10 is one sig fig, whereas 10. is two sig figs.

We will change the text to better convey our meaning, but we do intend that values be rounded to the nearest ten, and when larger than 100, only two significant figures be used.

12. 7.1.5, 1st sentence, "...one must select..." 4th line: "...and scientific interests as well as ones values"

We will change the text to reflect the spirit of these changes.

13. 9-2, 1st para: see my comment on 7.1.2 above.

We will make sure that our intent is reflected in the statement we make about the example of how to determine an RSAL.

Carol Lyons

1. The primary and most important finding of the draft report should be reported and emphasized: The independent calculation of Soil Action Levels (SALs) produced numbers fully consistent with previous calculations and the numbers currently being used by DOE-RFFO.

We do not believe this comment is an accurate statement. As we indicated in the draft report, the DOE scenarios were not analyzed with the fire scenario as will be done for the final report. When this is taken into account, the results will change significantly. Furthermore, and as indicated at the December 9 meeting, the two approaches are not consistent and should not be "compared." Our methodology does not use the RESRAD resuspension model, and the dose conversion factors have been changed to reflect data that are more current. Differences between the two methodologies will be much more apparent in the final report.

2. The draft report apparently fails to include analysis for soil resuspension after a fire for the DOE scenarios. These are the federally-mandated and most likely future use scenarios. This

gap (if true) could lead to serious questions about the technical credibility of the overall work. Those calculations need to be completed and reported for all scenarios.

We plan to incorporate fire calculations in all of the scenarios for the final report.

3. Sections 8.1 and 8.2 of the report need to be expanded significantly to include a full discussion of each of the DOE scenarios.

Scenario discussions were contained in the Task 3 report. We included in the Task 5 report a discussion of the results that are presented for each DOE scenario. The same discussion is included for each of the *RAC* scenarios. The addition of the fire calculation to each scenario will make the discussion sections following the presentation of results for each scenario similar. When the final report is completed, all of the task reports will be included as attachments, and then a complete discussion of all aspects of the project will be located in one place.

4. It appears that the analysis of soil resuspension after a fire is not based on local conditions. After any fire at Rocky Flats, the ground is revegetated within a few weeks. If, in fact, local conditions of rapid revegetation were not used in the analysis, the calculations need to be redone for local conditions.

Other reviewers have raised the question of the time required for revegetation after a fire. We have parameterized the fire model to simulate random recovery times that vary from a few weeks to a year. Other mitigating factors (such as burn size and distance of the scenario subjects from the burn location) are considered in a simple way. Simulations will treat the occurrence and time of the fire (within the 1000-year temporal scope) according to an annual probability estimated from regional fire statistics. Further research, which cannot be accomplished within this project, might improve this approach. One reviewer has suggested consideration of droughts that might be anticipated within the coming millennium.

5. The numerical results should be presented clearly and comprehensively (as in Table GS-1). Numerical results for all scenarios, particularly the DOE scenarios, should be presented and explained clearly and comprehensively.

We will review our summary sections on the scenarios to ensure that there is a complete and accurate discussion, but we have included a detailed description in the case of all results, including a table which outlines the breakdown of dose by pathway (we will add a table like this to scenario DOE-3 and scenario *RAC*-4).

All comments regarding selection of future land use or selection of one scenario over any other should be deleted. Specifically, delete:
The last 2 paragraphs of General Summary
Page 7-1, bullets 1, 4, 6, 8, 9, 10, 14 and all discussion related to these topics (e.g., paragraph 7.1.1, 7.1.4, etc.). They are not within the scope of this contract.
Page 7-1, first paragraph after the bullet list
Paragraph 8.1.3.2

Paragraph 8.1.4.2 needs to be deleted or rewritten. Same for 8.1.5.2, 8.1.6.1, 8.1.6.2, 8.1.7.1, 8.1.7.2, 8.2.2.2, etc. (for uranium)
Chapter 9 needs to be rewritten to present the numerical results for all scenarios.
Chapter 10 should be revised accordingly.

The example of how to develop an RSAL is provided in the text only as an example. We have carefully attached clauses in this report to describe issues we did not consider that certainly must be considered before selecting a final RSAL. We recognize that our example will not translate into the final RSAL value, nor is that our intent. There is a great deal of thought and consideration that needs to go into evaluating these results or additional results that might be the outcome of future research. The numerical results will be presented in full within the context of the report.

6. It should be made clear in the report that the actual area of soil contamination at Rocky Flats is small compared to the total size of the site. In the current report, one gets the impression that all 6,000+ acres are contaminated. A clear picture of the limited extent of contamination after cleanup should be presented.

The maps included in section 4 give the reader an accurate perspective on how broadly the contamination resulting from Rocky Flats is spread. Comments on the extent of the contamination after cleanup would be speculative. That depends entirely on what sorts of decisions are made. We can provide approximate areas of regions within some of the contours shown in Figure 4-4. These numbers might be helpful to readers in putting the relative levels of contamination into perspective.

7. All presentations and discussions by the consultant (and accompanying hand-out material) on this task should present only the full chart of calculated numbers (as in Table GS-1, after all the new calculation results are added). Any discussion of a given scenario should be completed and presented similarly for all scenarios. As in the report, all comments regarding selection of future land use or selection of one scenario over any other should be deleted.

Our scenario presentation is quite uniform throughout the report. We present a single example of how the panel might go about selecting a final RSAL, using only a small fraction of the considerations we believe to be important.

DOE Comments

1. In Section 4.2, please explain why RAC feels that it is appropriate to use the RESRAD mass loading routine for uranium but not for plutonium. It is not understood why the areal extent of contamination should change the air dispersion models being used.

The variation in concentration of the contamination is the reason to develop a different suspension model for plutonium. RESRAD is intended to handle concentration variations up to a factor of 3. The plutonium contamination at Rocky Flats varies over a factor of several orders of magnitude across the extent of the contamination. Uranium contamination at Rocky Flats, on the other hand, is mostly isolated in "hot spots" where the contamination is confined to a relatively small area. The latter is a source configuration that is well within the design scope of RESRAD.

2. In Section 4.2.1, Why is Total Suspended Particulate (TSP) being used instead of PM-10 (Particulate Matter < 10 microns) values?

The TSP value was used for conservatism. In the final calculations, the PM-10 value, which based on the analysis in Hodgin (1998), is 30 to 40% of the TSP concentration will be used. This value will be treated stochastically.

Reference:

Hodgin, C.R. 1998. *An Analysis of Colorado Department of Public Health and Environment Air Monitoring Data for Particulates and Plutonium at the Rocky Flats Environmental Technology Site.* AlphaTRAC, Inc. Westminster, Colorado. Report prepared for the Colorado Department of Public Health and Environment. February 3.

3. Section 5.1, There have been a number of wind tunnel studies performed at the Rocky Flats Environmental Technology Site (RFETS) by Langer in the 1980s and at Operable Unit #3 in the 1990s. These studies used site specific analyses to assess resuspension. Why was the resuspension factor of "Anspaugh et al. 1975" used over these site specific resuspension studies? Please explain.

These site specific studies of resuspension at Rocky Flats provide resuspension factors to be used for estimates of air concentration due to resuspension. Anspaugh et al. (1995) report the widely used suspension model of Gillette and Shinn that we cite on page 5-3 of the report. It is this <u>model</u> that we are using to represent resuspension fluxes. For existing ground cover, the model was calibrated by regression with data from air samplers on the site during 1992–1994. Anspaugh et al. (1975) also provided resuspension estimates at other locations with different ground cover. This was helpful in our assessment of bare soil resuspension, for which site specific studies have not been done at Rocky Flats.

4. Section 5.2.1, Please explain why a gaussian plume model is being used for a point source instead of an area source model. Surface soil concentrations of radionuclides are found over a large area. Why would a point source model be used for a large area source?

An area source model is derived by integrating a point source model over the desired source area. The contaminated region is partitioned into a large number of differential area elements. The integration (as is explained in considerable detail in Section 5.2.1) multiplies each differential element of area (treated as a point) by a factor that accounts for radionuclide concentration, soil flux due to resuspension, and transport from the source location to the receptor. The integration represents a summation over all differential area elements that contribute to the air concentration at the receptor. Moreover, this is done for each wind speed, wind direction, and atmospheric stability, weighted by the joint relative frequency of observations on these variables, and summed.

5. Section 5.3, This section states that there will be increased air resuspension after a fire. Please elaborate on the assumptions regarding the impact of a fire. Does RAC assume that the soil will be bare for an entire year after a fire? Does RAC assume that there will be any vegetation left after the fire, such as root structures?

RAC did assume for the draft that the ground will be bare for one year. Since the resuspension estimate is based on empirical data, assumptions about surviving root structures are not part of the model. The fire model is being extended for the final report and will simulate occurrence at different times during the 1000-year period (or no occurrence), different burn areas, recovery times from a few weeks to one year, and other mitigation such as distance of scenario subjects from the burn location. The fire occurrence will be treated probabilistically as part of every scenario.

6. Section 5.3, Did RAC consider how the assumptions about a brush fire could impact other aspects of the resident rancher scenario. For example, if RAC assumes the fire will completely remove vegetation for some period of time, this has potential implications for the viability of ranching during that time period, which in turn impacts consuming home grown food, number of hours per year the rancher is on site, etc. Did RAC examine these secondary effects of a fire on the resident rancher scenario?

We did not consider the effects that a fire might have had on the scenario. We assume that the rancher and family return to the site and act as before. By reviewing the tables of breakdown of dose by pathway, it is easy to see that inhalation after a fire contributes the majority of the dose, and even if the farmer is able to maintain home grown food consumption, that will impact the dose minimally (~7%). So even if the rancher did not consume home grown food during that period, inhalation would provide the majority of the dose. If the rancher left the site, his dose would decrease, but we did not consider that in the fire evaluation. The point is to ask what the effect would be if someone were there.

7. Section 5.3, Please reference how it was estimated that 99% of Pu-239 activity is associated with particles < 15 microns.

The lognormal distribution with respect to particle mass was assumed to have GM = 6 µm and GSD = 5 (this assumption came from generic assumptions suggested by NCRP Report No. 129, indicated elsewhere in the report). Plutonium activity was assumed to be uniformly distributed on the particle surfaces. For the distribution with respect to surface area, GM = 0.450 µm with the same GSD. The conversion is based on the equation $\overline{D}_\gamma = \overline{D}_1 \exp(\gamma \ln^2 \sigma_g)$, where the subscript γ values refer to particle count (γ = 1), distribution with respect to surface area (γ = 2), and distribution with respect to volume or mass (γ = 3). $\overline{D}_\gamma$ is the geometric mean, and σ_g is the geometric standard deviation. A reference for the equation and discussion of particle size distributions is J.H. Seinfeld, *Atmospheric Chemistry and Physics of Air Pollution*, Wiley, 1986. Using the equation, we can write $\overline{D}_1 = \overline{D}_3 \exp(-3\ln^2 5)$ and then

$$\overline{D}_2 = \overline{D}_3 \exp(-3\ln^2 5)\exp(2\ln^2 5) = 6\exp(-\ln^2 5) = 0.450 \ \mu\text{m}.$$

The 98.5 percentile of the standard normal distribution is 2.17. Thus the 98.5th percentile of the lognormal activity distribution is $0.450 \times 5^{2.17} = 14.8$ µm.

8. Section 6, Figure 6-1, Please explain in this section the software quality control procedures used to assure that the PERL scripts were written correctly and performed as they were intended. Please explain the procedures used to verify and validate the RAC developed software.

Since this methodology is still in research stages, a formal QA procedure has not been completed. We have done considerable amounts of debugging, and we did do some comparisons between RESRAD in its original version and RESRAD with the PERL script attached, and the results showed good agreement. Our goal was to provide the panel and the agencies with a demonstration of an approach that would provide an enhanced methodology. We have performed extensive calculations with the script and its variations and are convinced that it is giving reliable results, but more formal testing would be required to release this script for production purposes. We would prefer that others attempting similar calculations modify the scripts to their own requirements. The setup, as the comments warn, is fragile and requires some care to implement on different Windows and Unix systems.

9. Section 6, Page 6-5, It is stated that, "The Monte Carlo simulations shown in Figure 6-1 produced a file of soil action levels for the plutonium, americium and neptunium species of interest." Please include these radionuclide soil action level distributions in the report so a direct comparison can be made with the current radionuclide soil action levels.

The file runs to thousands of numbers for each scenario and variant thereof and does not belong in the report. We can think of no helpful way to exhibit this information.

10. Section 6, Page 6-5, It is not readily apparent how the distribution of RSALs was compared with soil concentrations to develop a probability of exceeding the dose limit curve. Please work through an example in the text. Also, please explain how this methodology compares with the "Sum-of-Ratios" methodology currently used to assess the radionuclide soil action levels at a site.

It is the same sum-of-ratios method, with uncertainty in the individual RSALs and (possibly) in the corresponding soil concentrations.

11. Section 8, Please include a copy of the computer software and documentation that RAC developed so that all the RSAL distributions can be evaluated.

At the conclusion of the project, we will turn over an electronic copy of the software created for this project to the panel.

12. Section 8, Please recommend a methodology for assessing a site when plutonium, americium and uranium contamination are present in ratios different than what RAC has assessed.

The method is essentially generic and could be applied to any spatial distribution of radionuclides with concentration ratios that do not vary much from one location to another. If such a pattern does not exist, one cannot key the results to a single radionuclide combination such as $^{239+240}$Pu, and the work and presentation of results become much more complicated.

13. Section 8, In a number of places, RAC has used the single radionuclide soil action level currently being used at RFETS as a basis for comparison with the RAC derived soil action level. Please compare the RAC derived soil action level with the RFETS radionuclide soil action level using the "Sum-of-Ratios" method.

The *RAC* calculations and the DOE/EPA/CDPHE calculations are fundamentally different because of the inclusion of the stochastic methodology in the *RAC* calculations, the change in dose conversion factors, and the resuspension model. We will not be including any comparisons of the values in the final version of the report.

14. Section 8.2.2, Please include a discussion of the resident rancher that includes 1) When the resident rancher would be expected at RFETS, 2) The extent of the resident rancher's property and what is the surrounding land use, and 3) What type of ranch is expected (i.e., what crops and animals would be expected at the property).

The discussion of the rancher scenario was included in Task 3, and we intend to combine these reports in the final project report. The rancher is expected to be at the site full-time. We allow the ranch to be 10^7 m^2, occupying most of the site including and eastward from the 903 Area. As constrained by the RESRAD definitions, the rancher grows two categories of crops: leafy vegetables and non-leafy vegetables. Cows (dairy and range) are assumed to be at the property for subsistence purposes.

15. Section 8.2.2, With the drinking water turned on, is the resident rancher drawing water from the shallow ground water (Water present at < 50 ft depth) or from deep ground water (Water present from > 300 ft depth).

Water is assumed to be drawn from the shallow aquifer underlying the site. This is discussed in the Task 3 report on page 46. Water pathway sources were dismissed in the in the DOE/EPA/CDPHE calculation. We agreed with their assessment of the surface water pathway, but disagreed with regard to the groundwater pathway. We argued that a well that produces 2 gal min^{-1} would be adequate to provide drinking water and perhaps water a few head of cattle under subsistence conditions. In the Task 2 report, we provide an overview of the groundwater and surface water transport. We believe it is unlikely that contamination will migrate to the deep aquifers underlying the site because of the hydrologic characteristics of the geologic media. Therefore, the only potential pathway of exposure is the shallow aquifer. Evaluating this pathway also provides a bounding estimate for any migration of contamination from groundwater to surface water.

We would also like to point out that the DOE/EPA/CDPHE assessment for the resident used an irrigation rate of 1.0 m/y. We do not know if that was their intention, but turning the irrigation on has the net effect of depleting the surface soil concentration substantially, thereby lowering the surface exposure pathway.

16. In the public meeting on 11/11/99, it was stated that the appropriate soil ingestion rate for a child is 75 grams/year in an open space scenario. Please explain this statement in greater detail.

We are using the soil ingestion rate of 75 grams per year to describe the soil ingestion for a child of a rancher. We apologize for any misinterpretation of this point.

17. There appears to be some inconsistency in [citing] the surface soils action levels as referenced in the "Action Levels for Radionuclides in soils" 1996 document you site. For Scenario DOE-1, residential you [cite a] Pu 239 value of 1432 pCi/g, which is for Pu 240. The correct value should be Pu 1429. For Scenario DOE-2, Open space, the correct value for Pu 239 is 9906 pCi/g, not 10580. Please check your values and if you do not agree, please explain the differences. It would also be of great benefit to list your values as either sum-of-ratios numbers or only Pu 239. Mixing the reported values causes confusion.

We apologize for the incorrect references. We do not plan to make any comparisons of the DOE/EPA/CDPHE values to the *RAC* values in the final report, as the calculations are fundamentally different and any comparisons thereof are generally uninformative. The differences between the methodologies are 1) different dose conversion factors, 2) different resuspension model, 3) *RAC* has employed a stochastic methodology, and 4) the *RAC* calculation reveals a stronger dose dependence on the ingestion pathway.

18. Please expand your discussion of the off-site resident rancher action level of 30 pCi/g. From the information presented in section 9.1 it is unclear how you came to that value.

We will be certain that the calculation is more readily understandable in the text.

Victor Holm

1. I continue to be impressed and satisfied with the work you have done. I believe in the end we will have a much improved tool with which to work.

Thank you for this comment.

2. I have used Monte Carlo simulation several times to assist in making large financial decisions. I have also been told that it is very important that the input data not be biased. If the distributions are skewed the entire process is defeated. The output distribution is not a simulation of reality rather it is a subjective representation of the biases of the researchers. To then make the statement that the 0.1 probability line represents a 10% chance of the 15 mrem dose being exceeded is neither mathematically nor actually correct. I objected strongly, early in the study, that the scenarios were being biased. I was assured by *RAC* that even though they did not believe distributions for the behavioral variables should be

introduced into the study; the environmental variables would be included and they would not be biased. I was therefore surprised to find in the Task 5 report that safety factors had been placed on nearly all these variables. A few of the instances of this biasing

- The breathing rate was set at the 95 percentile
- The rancher never leaves the contaminated zone (100 percentile)
- The inside of the ranch house has the same dust level as outside (100 percentile). We may discuss whether the correct value is 0.4 or 0.7; but, we know for certain it is not 1.0. This is exactly the type of problem for which Monte Carlo was developed, why not use it.
- The fire destroys all vegetation for one entire year (Low probability).
- No reduction in dust levels during the year due to removal of fines leaving a protective layer of larger particles on the surface (low probability). Most studies indicate that bare soil does not produce a steady state flux for a full year.
- As described on P. 5-11 you arbitrarily increase the variance of the estimated flux without support from the data (Safety factor of 2.5).
- Choosing the highest value from Anspaugh (1975). The Nevada Test Site is not equivalent to Rocky Flats. The alkaline lake beds at the NTS produce much higher fluxes than the well graded soils at Rocky Flats.
- Although the dose limit is defined as a yearly dose the risk is based on a lifetime exposure. If the soil action level is set at 10 pCi/g, in a year without a fire the rancher would only receive about 1.5 mrem, only in the fire year would he receive 15 mrem. I am not suggesting that a lifetime exposure dose be used, but simply point out that this results in additional conservatism.

The joint probability distribution for all these inputs is not a distribution of likely doses the rancher would receive; but rather, a skewed distribution that already has a very small probability exceeding the dose limit. To then suggest that the 90% or 95% probability be used represents worst case or a bounding estimate. While this certainly an interesting number to know it not a soil action level. A soil action is a compromise between public heath risks and health risks to the workers, the environmental costs, and monetary costs. A worst case or bounding estimate seldom meets these goals.

First, the reviewer's usage of the term "skewed" in reference to distributions does not quite conform to statisticians' parlance. Distributions such as the lognormal, are skewed, and this merely refers to their asymmetry, and not to something inherently bad about them. People often speak of "skewed data" or "skewed results" to indicate the introduction of bias, and we assume his meaning is analogous.

From some reading between the lines, we believe this objection generally refers to *RAC*'s separation of scenarios from the environmental models and parameters that we use to estimate levels of radioactivity in relevant exposure media. Our position is summarized in the following paragraphs.

The environmental models and parameters represent something that we do not control. Mostly, this something is the natural environment (or a very restricted part of it), but it can also include anthropogenic processes such as a source term (sometimes we might wish to consider the source term as part of a scenario; this is a gray area). The models represent this environment as a system of state variables, including those that stand for concentrations of radionuclides in soil, air, and so on. We attempt to estimate the past or predict the future of this system, and to quantify uncertainties about those estimates or predictions (generally we say "predict" in either case). The representations of uncertainty are themselves models, and their application includes subjectivity.

The scenarios for radiation protection, on the other hand, are under our explicit control. They are hypotheses that we set, not real people. They provide a means of constructing criteria for interpreting the predicted (or measured) radionuclide levels in environmental media. Prospective calculations that we perform are really about the environmental media. But they are expressed in terms of dose or risk to a scenario subject to place them on a more meaningful (and lower-dimensional) scale. To suggest that outcomes for these scenarios cannot be associated with probabilities is a misinterpretation. Probability statements can be made about rather arbitrary functions of the environmental state variables, but it is these state variables with which the probabilistic information is associated, not with the arbitrary function. Such probability statements are indeed mathematically correct, but as a matter of application, we need to bear in mind that all estimated probabilities are conditioned on the assumptions that went into their calculation.

Probability distributions associated with the environment, which we do not control but which we must somehow simulate, are of a different character from distributions associated with variability within populations from which a scenario subject is imagined to have been drawn. It seems to us generally confusing, if not misleading, to mix the two kinds of probabilities together in order to make uncertainty statements about exceeding dose limits. It seems to us much clearer to choose our scenario subjects with properties (breathing rates, behavioral characteristics) that would be protective of a reasonable fraction of the population from which we assume the subjects come. If multiple properties are involved, then we obviously cannot set them all at the 95th percentile and claim that we are conservative for only 95% of the population. But we do believe that they should be set to fixed values, in such a way as to define the subject as being credibly protective of an acceptable proportion of the population. Certainly, it is always reasonable to review an assessment as a whole and ask whether too much conservatism might have been introduced. But care has to be taken in doing so.

Note that when a millennium is the time domain of a prospective study, the scenario becomes a succession of hypothetical individuals, all having similar location and characteristics, but with their exposure environment evolving from generation to generation. If one were to treat the scenario subjects statistically, would successive generations be stochastically independent with respect to their physical and behavioral properties? Or would one consider autocorrelations, to account for family traits in different generations? Or would we sample one set of properties at each Monte Carlo realization and apply them equally to all generations? Many questions of this kind can be raised to illustrate the conceptual problems that arise when one starts down the path of "realism" expected from treating scenario subjects as samples from real populations.

Finally, let us contrast the situation described above, for prospective assessments, to retrospective studies such as dose reconstructions. If a risk analysis is carried out for such a study, the affected populations are real, and distributions of properties of those populations can (at least in principle) be estimated (e.g., by Census statistics and sampling surveys). We can then quite reasonably consider these distributions as part of the total uncertainty in the risk estimate and combine them with distributions of concentrations in exposure media. The outcome, for example, might be the number of health effects that would be predicted to result from the collective exposure under study. This number is uncertain, not only because of our uncertain predictions of environmental concentrations, but also because of variability within the affected population with respect to our determination of the relevant properties (e.g., breathing rates, diet, proximity to contaminated media). Here, we do not have the luxury of defining a hypothetical individual whose properties suffice to protect most people who might be exposed. The purpose of the dose reconstruction is not to protect anyone, but rather to study potential or realized effects of what has already happened.

With regard to the reviewer's concern about the value for the fire flux, we did not choose the NTS value from Anspaugh et al. (1975) as generic for the fire flux. On page 5-16, we indicated that the generic range (given initially in terms of resuspension factors) is from the tabulation of Sehmel (1984). The logarithmic midpoint of this range (converted to flux units), as we noted on the page 5-15 (last full paragraph), is within a factor of two of the NTS value (the NTS value is higher). We offered this observation for comparison, not calibration. We will have to do some rewriting in Section 5.3 anyway, and we will try to make this point clearer.

3. I do not have access to all the 903 pad characterization data; but it appears to me that 10 pCi/g is economically infeasible and possibly technologically impossible. It would also result in near total destruction of the very environment we are trying to save as open space. Another indication of the unreasonableness of your recommendation is that you do not consider that some of the scenarios are mutually exclusive. If the site is developed into single family housing, a far more likely scenario than the rancher, then the action level becomes 170 pCi/g. You are in effect recommending that the most cost effective cleanup would be to let the property be developed. This is patently absurd. The problem stems from the skewed dose distribution for the fire. If this distribution is corrected then the difference becomes less than a factor of 2 instead of 17.

It is important to remember that we presented the value of 10 pCi g^{-1} as an example of how the panel might go through the process of determining a soil action level in light of the many considerations raised in this report. We did not account for all of these considerations in our example, but it will be quite important for the panel to give weight to each of them. Also, it was important, not only to the panel but to *RAC*, to allow for a number of different types of scenarios from which the panel could choose the most likely land usage or, alternately, select the most conservative scenario in terms of dose. The fact that different scenarios produce different RSAL values in no way makes any statement about the most cost-effective clean-up; it makes a statement only about each scenario and the dose delivered in a year from each scenario. These results will be altered in the final report by the modification of the fire model and its probabilistic introduction into all scenarios.

4. As you state, cost was not considered in your recommendation of a soil action level. I understand your reason for this; but, it seems to be in conflict with the way health standards are set in this country. For many chemicals, especially those that are carcinogens, none is the best standard; but the EPA and other agencies have compromised this ideal by using the concept of acceptable health risk. The 15 mrem dose limit is such a standard. As you point out, this limit already has some conservatism built in since Pu is not uniformly distributed throughout the body. It was never the intent for a soil action level to be set at the point of no risk. Because of the biased input variables we now have no way to evaluate the actual risks to people living on the site. If time remains, I would hope you can provide us with a run for

the rancher based on the most probable distributions of the variables. We could then chose a safety factor.

We defer here to our response to concern #2, raised by this same reviewer.

Specific Comments

1. Chapter 4: I consider the use of on site sampling to determine the spatial distribution of Pu in the soil one of the most important contributions that RAC has made to this study. One editorial comment, on P.4-10 last line of 4.2.1, I believe 200 x 35 micrograms should be 7 X 10^3 micrograms or 7 x 10^{-3} gm^{-3}.

   Correct. Thank you for noticing this error.

2. Chapter 5: Again I was impressed with the method used in the regression. In geostatistics a similar method called conditional simulation is used. I continue to take exception with the arbitrary way in which variances are adjusted to add a safety factor. I know we differ on this point; but, I believe in a Monte Carlo simulation the input distributions should be unbiased. Your regression analysis showed that your estimated values consistently overestimated the actual values (P. 5-15). You nevertheless increased the GSD from 3.06 to 4.0. In a Monte Carlo simulation in which you intend to use a high confidence value increasing the variance is equivalent to increasing the mean. It is clear you understand this point since you write "we make this precautionary adjustment as a measure of conservatism in the calculation". Since this variance is deeply embedded in a series of complex calculations, I have no idea how it effects the final soil action level, if at all. I don't believe you know either without checking. How did you arrive at a GSD of 4.0? Why not 5.0 or 6.0, it would provide more conservatism. I hate to keep harping on the same point; but the place to be conservative in a Monte Carlo simulation is in the output distribution not the input distributions. All science is based on subjective judgements, but when you have just completed a very elegant nonlinear regression using Monte Carlo to simulate the joint distribution and then find good agreement with the original data set you have accomplished a real feat, why then add unnecessary subjectivity.

   As we discussed in one of the technical sessions, resuspension fluxes do not remain constant with time. In a well-graded soil such as at Rocky Flats the fines tend to be suspended very rapidly and are carried away. Stronger windstorms do then suspend some of the coarser particles. With more time the fluxes decease. I have personally observed this in mines. A year is too long a time for the assumption to be made that the soil acts passively. If you are going to adjust the variance I would adjust it down to account for this effect.

In the RAC proposal for this project, we stated a list of principles (A–E) that we follow in applications of uncertainty analysis. Principle B reads (in part) as follows: "RAC generally recommends that calculations not be deliberately biased high to compensate for lack of knowledge. Rather, analysts should do their best to keep their procedures free of bias. Conservatism, when warranted, should be expressed by increasing the variance of a quantity's uncertainty distribution while keeping its 'center' (e.g., 50th percentile) fixed." Note the specific mention of 50th percentile, which we prefer in place of the mean as a central statistic for skewed distributions. Then in this context, unbiased means that we should not deliberately distort the median of the distribution.

The distribution of predicted/observed ratios shown in Figure 5-4 was approximated by a lognormal distribution with geometric mean (GM) 1 and geometric standard deviation (GSD) 3.02, with parameters estimated from the empirical distribution. First, let us observe that increasing this GSD to 4 without changing the GM does not bias anything, because the GM is the median of the distribution, our central statistic of choice.

One most certainly can object to this subjective increase from assumed factor-of-five to order-of-magnitude precision in the estimate. Our decision to increase the GSD was motivated primarily by the relatively brief period represented by the data (3 years) and secondarily by concerns about the adequacy of the spatial coverage of the samplers. We were less concerned about the number of samplers than about the fact that they are spatially concentrated in the parts of the site that may be less typical of the soil resuspension flux we are trying to estimate. We also anticipated concerns about sampler efficiency. We strongly doubt that all uncertainty about the soil flux is accounted for by the variability expressed by these data. There is no denying that our data constitute a sample of convenience, which is a common problem in environmental studies.

If the reviewer does not agree that some adjustment of the uncertainty is appropriate here, his view is noted and will be considered. His implication that the procedure we followed would bias our central estimate of the soil flux is not correct. However, without checking, we would expect that propagation of the increased variance into the curves that show the probability of exceeding the dose limit would tend to decrease the SALs based on low probabilities (e.g., 10%). If resuspension is the only exposure mechanism, increasing the variance should rotate the curve clockwise about its (approximately) 50% point, distorting it so that its asymptotes are preserved. When other mechanisms, such as ingestion of soil and contaminated foodstuffs, are involved, the picture is more complicated. But we did not make the adjustment with the purpose of causing the SALs to decrease.

As to the regression's overestimation of the samples at S-07, S-08, and S-09, one must realize that it also underestimates the values at numerous other samplers (Figure 5-1). The regression seeks the best fit, in the sense of least squares, when all locations and dates are considered. No regression based on these data and a constant resuspension flux is likely to do much better. We should also point out that when we increase the GSD of a lognormal distribution while we keep its GM fixed, both tails are influenced, i.e., probability density is moved into the upper tail, but the same amount of probability is displaced toward zero.

3. Chapter 6: I wish to complement you for the work you did modifying RESRAD. I was able to follow the PERL script as written. I was a little disappointed that no user interface was included; but, this can be easily added at a later date.

We are glad that the PERL script was easy to follow. It will undergo considerable revision for the final report.

4. Chapter 7: I agree that only two significant figures be shown. I would however round to the nearest five below 50. The difference between 10 and 15 could have major economic

consequences. I have some question that a uranium soil action level of 20 pCi/g can be distinguished from the high background uranium found along the Front Range.

We will consider this enhancement.

5. Chapter 8: 8.1.7.1 I am confused why you report the 85 mrem level instead of the 15 mrem value, as you are aware the dose for radiation workers is much higher than for the public. More interesting would be to use this scenario for an open space park worker. 8.2.1.1 Editorial: I had trouble following this paragraph, I think there may be some number transpositions.

We will include the 15 mrem and the 85 mrem analyses in the final report.

6. Chapter 9: I am disappointed with your statement that 30 is only slightly different than 10. The numbers vary by a factor of three. The cost of cleanup to 10 instead of 30 is more than an order of magnitude and would effectively destroy the ecology of the site.

We did not intend to diminish the difference between the two numbers in any way, particularly in view of the cost of cleanup. We were trying to show how similar the results from the two scenarios were, and how cleaning up to protect the onsite individuals would also protect the offsite individuals.

7. Chapter 10: I agree with your suggestions for future work. I hope they are implemented.

8. Appendix C: Thanks for the conversion table H-17

We appreciate both of the above comments.

9. Appendix D: It's a small item but I am curious why you used a Kd of 5350 in this appendix while the median value used in the study was 2000. This again points up the problem with Kd. A low Kd will result in groundwater becoming an important pathway, while at the same time it reduces the inhalation and ingestion risk. We must be careful that natural attenuation does not become the preferred cleanup strategy.

The same question was raised by and answered for Reviewer A.

Steve Gunderson

1. Since the effects of a hypothetical grass fire make a considerable difference in the calculations and because there is a large uncertainty associated with the modifying factor applied, more information about the development of this factor would be useful. Specifically:

- The range of values used to derive the modifying factor of 200. Are all the values from various sources given equal weight in the derivation process and are all considered equally valid for use in these scenarios at this site?
- If the resuspension rate is constant throughout the year affected by the lack of vegetation, are collateral effects on parameters such as ingestion of homegrown fruits, vegetables, and meat accounted for?
- Were the following references considered during the development of resuspension parameters under the fire scenario:
  - Gerhard Langer's *Resuspension of Rocky Flats Soil Particles Containing Plutonium Particles – A Review (1989)* and
  - CDPHE's *Technical Report – Buffer Zone Brush Fires Investigation* (1999)
- What additional data could be collected or research conducted to reduce the large uncertainty surrounding the fire-scenario mass-loading modifying factor?

We plan to include enhancements to our fire calculations in the final report, as mentioned in response to many of the previous reviewers. These enhancements will include calculations of the probability of a fire in any given year, which will hopefully make the fire calculation more applicable for these purposes. The factor of 200 was developed as described in Section 5.3, with a large range of uncertainty. With limited sources of data, we considered any available data that fit the parameters of our analysis to be valid and useful. Including the probability of a fire will enhance this calculation as much as it can be without additional research. We will recommend such research in our final report, but it would likely include a specific study on the effects of a fire on resuspension at Rocky Flats. Also of interest would be an issue raised by Reviewer A, that of the impact of the actinides in soil burning and what sorts of exposures that might cause.

For the year that the impacts of the fire were felt, we did not account for any impact that might have on the farming of homegrown food. But, as we pointed out to a previous reviewer, the inhalation pathway dominates the year after a fire (~87%) and that pathway, combined with soil ingestion (which could still, theoretically, exist) make up 92% of the total dose during that year. So even without the ingestion of agricultural products, the total dose would not be impacted that significantly.

We did have access to Langer's work, but were not aware of the CDPHE technical report. At this point in the project, we would have to defer that report to later research.

2. Section 4.2.1 discusses the mass loading factor used in the uranium calculations. The text identifies the factor used in the original RSAL calculations which is based on measured PM_{10} values. Why are TSP values compared to this value and used as a basis for the mass loading factor?

The TSP value was used for conservatism. In future calculations, the PM-10 value, which based on the analysis in Hodgin (1998), is 30 to 40% of the TSP concentration will be used. This value will be treated stochastically.

Reference:

Hodgin, C.R. 1998. *An Analysis of Colorado Department of Public Health and Environment Air Monitoring Data for Particulates and Plutonium at the Rocky Flats Environmental*

Technology Site. AlphaTRAC, Inc. Westminster, Colorado. Report prepared for the Colorado Department of Public Health and Environment. February 3.

3. Not all of the parameters used in the calculations are defined. In order to evaluate the RSAL calculations, it would be useful to have each parameter explained and justified.

Each parameter used in the calculation was explained and justified in Task 3: Inputs and Assumptions. Some of those parameters were enhanced in the Task 5 report. For the final summary report for this project, we plan to include all of our task reports as attachments, so at that point, all of the necessary information will be in the same place.

Mary Harlow

I would like to take this opportunity to comment on the Task 5 report. The report is impressive and well done. However, I do have concerns about the defensibility of some of your conclusions. If we are to convince the regulators that the soil action level should be lower we need hard evidence.

1. A lightening caused prairie fire is certainly a possibility at Rocky Flats, but what is the probability of this happening in any one year? Where would the fire occur, how large would it be and how long would it take for the vegetation to regrow? I do not believe that the effects of this fire would be felt for an entire year unless there were multi-coincidence events occurring such as a drought. Should a drought be considered in this worst case scenario?

We will be considering grass fires from all causes in the near and remote future with probabilities estimated from fire statistics in the past century. Mitigating effects, such as variation in burn areas, regrowth time, and distance of subjects from fire, will also be considered. We will recommend that periods of drought might be considered, but we will not be able to include this factor explicitly in our analysis.

2. The question you posed as to "At what soil action level would you be willing to move into the area and live on the property that has been remediated "does not apply to setting a soil action level that is protective of the offsite community and future users of the site. As we have seen at Love Canal, historical memory fails within ten years. What we need to support the 10 pCi/g that you have suggested is good hard scientific data to back up your conclusions if this study is to be acceptable and replicable.

The 10 pCi/g should not be treated as a recommendation; it is based on a worst-case fire scenario, which, as the previous answer indicates, is being extended to a more realistic simulation. We included the development of this value as an example for the panel to follow in developing their RSAL recommendation. The critical parameter for the fire is the resuspension flux for unvegetated soil similar to that of the site, and as far as we are aware, "good hard data" for that parameter do not exist. Support for the kinds of research that might have led to better data for these estimates was mostly terminated in the early 1980s.

3. Please provide tables that compare the RAC scenarios with the DOE scenarios with and without fire so that a reader can easily look at the data and note the differences.

Such comparisons are not very informative. As noted above, we intend to include the fire probabilistically in every scenario. Even without it, similarities of magnitude between previous RAC calculations and numbers included in the DOE/EPA/CDPHE document are not particularly surprising, but they do not reveal much of anything either. Remember that the two sets of calculations were performed with (1) different dose conversion factors and (2) different resuspension models and data. (3) In the DOE calculation, the principal pathway was inhalation; in the corresponding RAC estimate, it was ingestion. (4) The DOE calculation was deterministic, whereas the RAC numbers represented the 90th percentile of a stochastic simulation. If we put a RAC simulation that involves the fire side by side with a DOE estimate that does not, we will run the risk of promoting an apples and oranges comparison. Our purpose is to show how we believe the assessment should be done and to present numeric results that demonstrate our methods.

4. The safety factors that have been place on all the variables are of a concern. It would seem more appropriate to have a higher soil action level with an ALARA calculation than to have data skewed by over conservatism. Was ALARA even considered in your methodology?

First, we have not placed any "safety factors" on estimates, and it is not clear what the phrase "all variables" means. Second, our methods do not skew data; "skew" implies the introduction of bias. If the concern is directed to our estimate, in Section 5, of the resuspension soil flux for existing ground cover, we must point out that the procedure rests on principles that we have followed consistently. The uncertainty estimated by the regression process is limited by the data, which, for example, cannot tell us on the basis of data for the period 1992–1994 what 2000 will look like. A longer cycle would be needed. There are also questions about the degree to which the spatial coverage of the data is representative. The next two paragraphs are extracted from our response to another panel member.

In the RAC proposal for this project, we stated a list of principles (A–E) that we follow in applications of uncertainty analysis. Principle B reads (in part) as follows: "RAC generally recommends that calculations not be deliberately biased high to compensate for lack of knowledge. Rather, analysts should do their best to keep their procedures free of bias. Conservatism, when warranted, should be expressed by increasing the variance of a quantity's uncertainty distribution while keeping its 'center' (e.g., 50th percentile) fixed." Note the specific mention of 50th percentile, which we prefer in place of the mean as a central statistic for skewed distributions (meaning asymmetric distributions, generally restricted to nonnegative numbers, such as the lognormal). Then in this context, unbiased means that we should not deliberately distort the median of the distribution.

The distribution of predicted/observed ratios shown in Figure 5-4 was approximated by a lognormal distribution with geometric mean (GM) 1 and geometric standard deviation (GSD) 3.02, with parameters estimated from the empirical distribution. Increasing this GSD to 4 without changing the GM does not bias anything, because the GM is the median of the distribution, our central statistic of choice. In particular, any frequency that is added to the upper tail of the distribution is balanced by frequency added near the lower end.

We are not certain what is meant by the question about ALARA (As Low as Reasonably Achievable). Our methods can provide a basis for ALARA considerations, but "the first task [of the ALARA process] is to ensure that the area being remediated is at or below the authorized limit or dose constraint [the 15 mrem annual limit, which is built into our calculations]; the second is to determine that the residual radioactive material is reduced to levels that are as low as reasonably achievable *below the dose constraint*." (our emphasis; the quotation is from the RESRAD manual).

5. **The document has numerous typographical and grammar errors I am sure that the Peer reviewers will point out.**

We have noted the errors and will correct them.

73/73