AlphaTRAC
Meeting the Challenge of the Atmosphere

March 15, 2002

Dear Stakeholder:

The Rocky Flats Cleanup Agreement (RFCA) Stakeholder Focus Group will meet at the Broomfield Municipal Center at One DesCombes Drive on March 20, 2002 from 3:30 to 6:30 p.m.

The agenda for the March 20 meeting is enclosed (Attachment A). We will discuss the following topics:

- Agency Responses to Wind Tunnel Studies Peer Reviews
- Agency Responses to RSALs Task 3 Report Peer Reviews
- Uranium Surface RSAL Calculation and Draft Modeling Results

The handouts from the February 20, 2002 RFCA Focus Group meeting are enclosed as Attachment B, and include:

- RSALs Working Group Responses to Wind Tunnel Peer Review Comments, and
- Shaking the Foundations? The DOE Low-Dose Study Program.

Bob Nininger presented the "Response to Peer Review Comments Wind Tunnel Analysis." His presentation is Attachment C.

Attachment D is the RSALs Working Group Meeting Notes for the February 28 and March 2, 2002 meetings.

Attachment E is the agency responses to the RSALs Task 3 Report Peer Reviews.

You may call either Christine or me if you have any questions, comments, or suggestions concerning the RFCA Stakeholder Focus Group or the upcoming meeting.

Sincerely,

C. Reed Hodgin

C. Reed Hodgin, CCM
Facilitator / Process Manager

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RFCA Stakeholder Focus Group
Meeting Agenda

When: **March 20, 2002 3:30 - 6:30 p.m.**

Where: **Broomfield Municipal Hall, Bal Swan and Zang's Spur Rooms**

3:30-3:40	Ground Rules, Agenda Review, Objectives for this Meeting
3:40-4:10	Wind Tunnel Studies Peer Reviews Group Discussion and Response to Agencies / DOE
4:10-5:00	Agency Response to RSALs Task 3 Peer Reviews - Presentation and Group Discussion
5:00-5:10	Break
5:00–5:40	Agency Response to RSALs Task 3 Peer Reviews - Presentation and Group Discussion (Cont.)
5:40–6:20	Uranium Surface RSAL Calculation and Draft Modeling Results - Presentation and Group Discussion
6:20-6:30	Set Next Agenda
6:30	Adjourn

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RFCA Stakeholder Focus Group
Attachment B

Title:	February 20, 2002 Meeting Handouts: • RSALs Response to Wind Tunnel Review Comments, and • Shaking the Foundations? The DOE Low-Dose Study Program.
Date:	March 15, 2002
Authors:	Reed Hodgin
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	Review Comments – Wind Tunnel Reviewer #1	Response
	General Comments:	
1	A key question is how much saltation-size soil and burn debris of similar size were mobile and would move downwind and generate additional PM-10 by breakage of the moving material and abrasion of the downwind surface at high wind speeds? The tunnel test results do not report threshold velocities for neither coarse particles nor measurements of the amount of these particles and burn debris removed during testing. ...The implicit assumption in the wind tunnel test protocol was that incoming saltating soil and debris particles would be absent, and only wind would affect the test surface during a windstorm.	The wind tunnel tests captured both coarse particles and burn debris eroded from each test plot as wind speeds increased over the course of each test. This material was segregated into ≤10 micrometer (μm) and >10 μm particle sizes, aerodynamic equivalent diameter. It is reasonable to assume that larger particles (>PM_{10}) captured in the cyclone may include saltating particles that entered the wind tunnel inlet. However, since the concentration of particulate matter entering the wind tunnel inlet was subtracted from the wind tunnel effluent concentration, only the net impact of such particles on the wind tunnel test plot are included in the measured erosion potential of each wind tunnel test. That is, only the particles eroded from the test plot through saltation by incoming particulate or wind shear are counted in the test plot erosion potential. Assigning threshold velocities to individual surface sites has limited applicability to natural soil surfaces given the complexity and heterogeneity of such surfaces. While the threshold velocity for a given particle size may be determined with some reliability for a storage pile or similar homogenous surface, surfaces as complex as the Rocky Flats buffer zone do not lend themselves to such characterization within reasonable bounds of confidence.
2	The test wind tunnels are probably too small in cross-section and too short in length to accurately simulate atmospheric boundary layer flow over a significant portion of the test section on the rough, test surfaces at Rocky Flats. ... Second, some of the roughness elements were large, relative to the tunnel size, thus creating blockage effects. ... There are also edge effects where the tunnel sides meet the uneven ground surface.	While the portable wind tunnel does not generate the larger scales of turbulent motion found in the atmosphere, the turbulent boundary layer formed within the tunnel simulates the smaller scales of atmospheric turbulence. It is the smaller scale turbulence that projects wind flow into direct contact with the erodible surface and contributes to particle entrainment (macro-scale turbulence must still penetrate ground cover and liberate erodible material on a micro-scale). As was observed by Peer Reviewer 2, the ratio of the test section length to the roughness length is greater than 100:1, providing a good indication of boundary layer development. The main reason for assuring boundary layer development and stability is to characterize and

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		control the shearing stress on the surface. The confounding effects of surface roughness elements and uneven test plots are mitigated in the test protocol. For example, standing vegetation was trimmed prior to testing to prevent the deformation of vegetation by the working section, which leaves the potentially-erodible particle reservoir at the base of the vegetation undisturbed but minimizes the damping effect of the standing vegetation on centerline wind speed. Edge effects were mitigated through selection of relatively level test plots and the use of weighted skirts along the sides of the working section, which protected against air and particle infiltration.
3	Another difference between the wind tunnel and atmospheric winds is that the latter vary in the wind direction about the mean direction. The directional fluctuations during a storm would likely increase total PM-10 discharge a few percent above that measured from the straight winds in the wind tunnel.	It is true that small amounts of erodible material may be sheltered by surface roughness elements from the entraining energy of the wind tunnel due to a predominant wind direction. However, the boundary layer generated at soil level is not uni-directional, having turbulent eddies and wakes created through wind interaction with surface elements. This turbulence reduces the sheltering effect of surface irregularities, as observed by the experimenters.
4	Because the soil [at Rocky Flats – ed.] is a 'limited source' some period of time may be needed between wind events to replenish the loose particles through weathering, deposition, or disturbance processes. The 'limited source' concept means that when considering potential emissions on successive days following a windstorm, the present tunnel results would tend to overestimate the PM-10 available for resuspension.	The wind tunnel test results clearly illustrate the 'limited reservoir' nature of erodible surface material following each step in wind speed. Real-time optical particle counter data show rapid decays in particulate concentration over time following each step-increase in wind speed. Over-estimation of PM_{10} erosion potential is acceptable to the Working Group given the end use of the data to develop final Radioactive Soil Action Levels (RSALs).
	Specific Comments:	
5	The selection process for the test plots was not described, but there is considerable scatter among plots in the potential erosion data	The prescribed burn wind tunnel test location was selected within a region of homogenous soil type, similar standing vegetation, and relatively flat topology within the test burn acreage. Prior to the prescribed fire, the test area was staked off and protected from anthropogenic impacts other than the fire itself. Individual test plots for each temporal iteration were adjacent, to maximize similarity of the test surfaces (i.e., the April burned-surface test

		plots were adjacent to one another; the May test plots were nearby the April plots and also adjacent to one another; etc.). Individual test plots were sampled in sequence, with no repeat testing of any surface and no anthropogenic disturbance of any plot prior to testing. No effort was made to limit natural disturbances prior to testing (rain splash, wildlife intrusion, etc.). Scatter of results in wind tunnel testing is typical, and is well documented in portable wind tunnel test literature including the background documentation for EPA-recommended industrial wind erosion emission factors presented in *Compilation of Air Pollutant Emission Factors (AP-42)*. The scatter typically results from the complexity and heterogeneity of surfaces tested; even relatively homogenous surfaces such as storage piles demonstrate detectable differences in the erodibility of individual areas. The forces that inhibit erosion (surface moisture, static attraction, crusting, surface roughness elements, etc.) are not uniform regardless of macro-scale homogeneity among test surfaces. Additionally, the air stream turbulence that causes particle entrainment has a significant degree of randomness. To ensure satisfactory statistics between replicate results, three wind tunnel trials were combined into each test run, and three test runs were bounded and averaged to describe each test condition. As noted by Peer Reviewer 2, "...in order to characterize differences in surface cover and surface roughness, the tunnel has to be moved several times...and the tests replicated. That gives satisfactory statistics between replicate results." This was accomplished.
6	It is also not clear how well the selected tunnel test plots might represent the contaminated areas that will be subjected to fires. Additional measurements to characterize the soil and vegetation conditions at the test sites would have been useful for interpreting the wide variability in the test results and estimating applicability of the test site data to comparable contaminated areas.	While the performance of pre- and post-fire erosion potential measurements on plutonium-contaminated regions of concern would provide the best site-specific data in support of RSAL development, pursuit of such experiments is unlikely to gain approval. Fortunately, the geologic units underlying both the prescribed fire plot and the tablelands east of 903 Pad are identical (Rocky Flats Alluvium), and support these data as being

		representative of contaminated areas. Soils underlying the prescribed fire were top-slope cobbly sandy loams, while the contaminated area soils consist primarily of top slope cobbly sandy loams and side slope clay loams. Vegetation varies between xeric tallgrass (burn area and contaminated tableland) to mesic mixed grasses (contaminated hillside) and reclaimed mixed grasses (previously remediated areas). Though these differences may contribute to minor variance in erosion potential, the bounding of wind tunnel study data and the conservative analysis of that data mitigates these subtle differences. [SOURCE: *Report on Soil Erosion and Surface Water Sediment Transport Modeling for the Actinide Migration Evaluation*, 00-RF-01823 (2000)]
7	Unfortunately, neither the measurement heights nor the measured values for the wind speed profiles were reported in the data. However, the practical result of the scaling problems cited above mean that the aerodynamic roughness and friction velocity values obtained from the wind speed profiles in the tunnel should be regarded only as rough estimates. … As a consequence, the atmospheric wind speeds at the 10 m height calculated from these values also should be considered only as rough estimates.	Wind tunnel centerline wind speed was measured at 11 points between 0.5 and 15.2 centimeters (cm) above soil surface. The specific heights were 0.5, 0.7, 1.0, 1.4, 2.0, 2.8, 3.8, 5.0, 7.0, 10.0, and 15.2 cm, respectively, selected to fit a logarithmic distribution. The average roughness length of all test runs for a given temporal scenario (i.e., all nine wind tunnel trials that comprised three test runs for each scenario) was used to estimate 10-m equivalent wind speed, as detailed in the example calculation in Appendix D of the controlled-fire test report. The small variations in roughness length observed between trials, while real, have negligible impact on the estimated 10-m equivalent wind speed given that wind speed varies as the natural log of the corresponding roughness length. More to the point, the importance of precision and accuracy when estimating the equivalent 10-m wind speed for each wind speed step is minimized by the use of normalized 95 mph wind speeds to describe erosion potential from soil surfaces. The conservatism that is built into the post-fire mass loading multipliers by normalizing wind speeds to 95 mph more than compensates for any uncertainty extending from the well-documented relationship between surface roughness length and

		equivalent wind speed at a given height above ground.
8	To increase accuracy of tunnel estimates it would have been useful to have a cyclone preseparator on the ambient PM-10 filter.	Because the wildfire report examined the very low concentration of actinide in airborne dust particles and compared it to the actinide concentration in the underlying soil, it was critical to the precision and accuracy of the ambient background correction that the PM_{10} to TSP ratio be known. Therefore, Colorado Department of Public Health and Environment data from ambient air particulate matter samplers located within several hundred yards of the wildfire area were queried and the average PM_{10}:TSP ratio for the area determined to be 0.3895. For the controlled burn data correction, where the results were used to develop post-fire erosion potential multipliers based on comparisons of erosion from adjacent burned and unburned plots, an estimate of the background correction was sufficient. As the following sensitivity analysis shows, the error introduced by assuming a PM_{10}:TSP ratio of 50% was small: Test Run CB-7 (from Appendix D) Wind-tunnel PM_{10} net mass: 9.15 mg Background net mass: 8.49 mg Estimated (50%) PM_{10} background mass: 4.24 mg Calculated (38.95%) PM_{10} background mass: 3.31 mg PM_{10} erosion potential (50% ratio): 0.12 g/m2 PM_{10} erosion potential (38.95% ratio): 0.14 g/m2 The calculated (38.95%) background PM_{10} correction would result in a net growth in erosion potential for both burned and unburned plots. Remember, however, that the end use of the data is to develop a post-fire mass-loading multiplier by calculating the ratio of burned to unburned plot results. That multiplier contains the same PM_{10} correction in both the numerator and the denominator. Since the denominator is a smaller erosion potential (unburned) than the numerator (burned), a decrease in

		the PM_{10} correction, as reflected here, would result in a smaller post-fire multiplier. By using the estimated background PM_{10} correction, the multiplier used in the RSAL calculations is larger than it should be, hence is conservative.
9	The post-fire erosion potential multiplier for the spring fire appears to be a reasonable application of the measured wind tunnel results. This is partly true, because precipitation events near the burn event are more frequent than at other seasons.	Seasonal differences in vegetative recovery, with the resultant effects on surface erosion potential, were considered during analysis of the wind tunnel data. The resulting post-fire erosion multipliers are qualified for seasonality. See comment 10 for additional discussion.
10	The post-fire erosion potential multiplier for the fall fire is estimated without a clear basis…	According to local ecologists, vegetative recovery will occur along a similar trajectory regardless of the time of year a fire occurs – the start of significant recovery is simply delayed in a late-season fire until the following spring growth cycle. Some "green up" would occur immediately after a fall fire, but plants would send up only a few inches of new growth out of plant crowns. It is likely that only the grass species would send up much growth; forbs would not be likely to respond substantially until spring. This contrasts with a spring fire where both grasses and forbs would begin growth immediately and continue to full plant height, thus reducing wind speeds at the ground surface and the potential for wind erosion more quickly. Since the vegetative recovery trajectories are similar, the shapes of the erosion multiplier curves (a function of vegetative recovery) would also be similar, though the initial fall fire multiplier (y-intercept) is greater because a fall fire has more and dryer fuel available than a spring fire and generally taller and denser standing vegetation. The fall y-intercept value was determined experimentally as the ratio of burned-area to unburned-area erosion potential measured in June (which was much higher than the same ratio measured in April due to greater unburned vegetation density). Fitting the spring fire multiplier curve to the fall y-intercept value produced the estimated fall fire multiplier curve, which is integrated to annualize the multiplier.
11	The estimated multipliers shows fall fire raises the erosion potential for 24 months. It is not clear that the second 12 months was counted in the	Both RESRAD and the risk assessment guidance consider a series of annual exposures in developing the probabilistic RSAL.

	frequency distribution matrix Table IV-5 page 45. The second year of exposure following a fall fire would likely result in less mass loading than the spring fire scenario, but more than the median non-fire scenario. Such events were included in the mass loading distribution as more probable than would normally be observed, because of the manner in which the empirical mass loading distribution was developed.	The probabilistic risk assessment used the "fall" fire events in this same context. While it is true that multiple-year events would be correlated for a fall fire, one must also recognize the overall uncertainty that is implicit in the mass loading distribution developed for a fall fire. The fall fire scenario is predicated on the false assumption that every six-month period has the same post-fire recovery characteristics. The development of the mass loading distribution also assumes fall fires have the same probability as spring fires, despite the fact that spring fires are known to occur over the six months of the year with the greatest recovery potential and the greatest likelihood for natural wildfires. Remember that the contaminated areas are well isolated from other fire influences such as cigarettes, sparks from vehicles, etc., yet a wildfire is postulated to occur once every ten years on the 300 contaminated acres of a 6400 acre site. The wildfire is thus assumed to occur with a frequency much greater than would be expected due to natural occurrence. Together, these factors cause the fall fire to have a much higher estimated frequency than would actually be expected. This suggests that its weighting in the distribution is greater than warranted, and is likely to offset any reduced effect resulting from neglect of multiple-year correlation. In addition, for the long-term risk exposure calculations, the working group did not exclude multiple consecutive-year fires on the contaminated area. While fires could occur two years in a row on the same area, the second fire would in reality be of significantly reduced intensity compared to the first, and compared to the one whose effects were studied using the wind tunnel. By not excluding such events, a more conservative risk assessment than is realistic results.
12	While the estimates for annual erosion multipliers appear reasonable for use in RESRAD and RAGS, the submitted material is difficult to evaluate because of the absence of information about topography, soil texture, surface roughness, rock cover, etc. High winds have a great capacity to	RESRAD and RAGS outputs are independent of intermittent changes to soil surface condition provided the mass loading inputs to these models adequately account for such changes on an annualized basis. Given the current, well-vegetated condition of

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	move erodible soil, so the statue of the surface when high winds occur is the major control factor. To illustrate the effect of high wind speeds after a fire on a sandy soil that is not a 'limited source', see the attached photo taken in southwest Kansas in 1996. … If there are contaminated areas that could act as unlimited source areas during high wind speeds, the rarity of these events would not greatly impact the annual values of PM-10 used in RESRAD. Nevertheless, such wind events could act to greatly expand the area of contaminated surfaces at Rocky Flats. … Hence, it would seem important to identify, stabilize, and restrict activity on those portions of the contaminated areas that might become highly erodible, if the vegetation were removed. Such measures would help to insure that the assumptions such a 'limited sources' made in developing the RSAL remain valid.	the Site's areas of contamination, the characteristic crusting that occurs in cobbly and clay loams that are characteristic of the contamination areas, and the land-use scenarios under evaluation, an infinite-reservoir model would not be "reasonable" unless major, repeated disturbance of the soil surface were assumed (e.g., intensive large-scale agriculture) which was rejected as a reasonable post-closure land use. If studied, any such disturbance that would increase potential short-term dose to downwind receptors would also dilute surface contamination through mixing with uncontaminated subsurface soil. Therefore, any hypothetical evaluation of long-term dose effects from a disturbed, unlimited reservoir source term must consider the reduced specific activity of the radioparticulate source compared to the existing limited-reservoir surface contamination.

	Review Comments – Wind Tunnel Reviewer #2	Response
	General Comments:	
1	The appropriateness of this wind tunnel application should be thought of in the proper context. … The wind tunnel is artificial in many ways. It is designed in a way that controls the mean wind speed but cannot reproduce the scale (size) of wind speed variations ("turbulence"). … The ground area exposed to controlled wind erosion is only about one square meter … but the variability should be significant between adjacent square meters due to differences in surface condition. So testing several one-square-meter plots becomes essential.… Using this method the equivalent 10-m wind speeds reported are very extreme.… Yet, the erosion potentials so obtained have use in establishing Radioactive Soil Action Levels, providing that we expect that the extreme erosion potentials observed are unlikely to ever exist in nature.	The reviewer's list of the limitations of an artificial evaluation of wind erosion from natural surfaces is well reasoned and comprehensive. These limitations were mitigated through equipment design, protocol development, and strict quality control. Specific concerns of the reviewer were addressed as follows: • While the portable wind tunnel does not generate the larger scales of turbulent motion found in the atmosphere, the turbulent boundary layer formed within the tunnel simulates the smaller scales of atmospheric turbulence. It is the smaller scale turbulence that projects wind flow into direct contact with the erodible surface and contributes to particle entrainment. As observed by Peer Reviewer 2, the ratio of the test section length to the roughness length is greater than 100:1, which is a good indicator of boundary layer development. • Sampling nine plots per test scenario (three plots per test run, three runs per scenario) provided sufficient replicates to describe differences in surface roughness. This provided

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		satisfactory statistics between replicate results. • It was desired that any bias present in the analytical method tend toward conservatism of dose estimation; therefore, the creation of sustained 10-meter equivalent wind speeds in the wind tunnel that were greater than could be reasonably expected based on historic meteorology is acceptable.
2	It is a matter of controversy that erosion only occurs after a certain wind speed threshold. … More recent observations show that there is an emission of small particles at speeds below the observed thresholds for saltation, and while this amounts to a relatively small emission loss, it affects the surface condition. …	Evidence of the sub-threshold emission was seen in these studies. By using mass loading rather than erosion potential to drive radionuclide transport and dose assessment, the role of wind speed threshold as a factor in radionuclide migration is minimized. By assuming that all eroded dust is contaminated in a 1:1 ratio comparing airborne specific activity to soil specific activity, the mass loading approach accounts for sub-threshold wind erosion. (Haines, et al., show the actual ratio for undisturbed burned soil to be less than 1:1 in *Correlating Plutonium Activity in Fugitive Dust to Plutonium Concentration in Surface Soils at Rocky Flats, Colorado,* (2001)),
3	In the protocol, each test involves step increases in wind speed and adds accumulated emissions from each step. In the wind tunnel saltation, the onset of avalanching may be a product of the peculiar small scale of turbulence, and more soil might be available than under natural winds.	The wind tunnel is unable to exactly replicate the atmospheric conditions that may occur at the Site. However, the methods applied appear to overestimate actual erosion potential. Any conservatism created though the use of the approach is acceptable, given the application of these data toward RSAL development.
	Specific Comments:	
4	In answer to Focus Group Question 1, regarding equipment suitability for this application: This reviewer feels that the equipment is in good standing with the scientific community.	The Working Group concurs with this reviewer. The fact that this equipment has been used extensively to develop emission factors for modeling industrial wind erosion in a regulatory setting (presented in US EPA's *Compilation of Air Pollutant Emission Factors (AP-42)*) was considered an endorsement of the technique for the given application.
5	In answer to Focus Group Question 1, regarding review quality and thoroughness, appropriateness and adequacy: This reviewer will make an attempt to show that the observations made by the wind tunnel method provide a set of data that are sufficient to proceed with the determination of Radioactive Soil Action Levels. … For example, I hope to show…that particular observations are sufficient to bound the worst-case possible	The use of 95 mph wind speed (10-meter equivalent) to normalize wind tunnel data is believed to be appropriately bounding, given that: • Peak wind speeds of 95 mph or more, while rare, are not unprecedented at Rocky Flats; • Lesser wind speeds would not have exhausted the available

	inhalation scenario, while I acknowledge that normalizing the emission potentials to 95 mph winds are a bit of an extreme. … In my view there is no need for further study if all we need is to determine Radioactive Soil Action Levels. No study may be more definitive in that respect.	limited reservoir of erodible material and would have required interpolating the upper region of the erosion potential multiplier curves developed through these experiments; and • Statistics between replicate results were satisfactory.
6	In answer to Focus Group Question 2, pitot tube adequacy for this application: The pitot tube is essential even though various electronic velocity probes … would be more elaborate. … I doubt that we would have any significant change to the results by finer profile measurements.	The pitot tube method has two primary qualities recommending it for this application: • It is an EPA reference test method for determining air velocity in ducts; and • It is sufficiently rugged for the field application (i.e., it will not be compromised by particle impacts or contact with the ground.
7	In answer to Focus Group Question 3, regarding working section dimensions for developing desired wind conditions: While details [of the wind tunnel design – ed.] are not discussed in the reports, this is not a new tunnel design, and I believe that the design is adequate. The ratio of the test section length to the roughness length is greater than 100:1, which is a good indicator of boundary layer development. The main reason for assuring boundary layer development and stability is to characterize and control the shearing stress on the surface…. The wind tunnel does that adequately.	The prescribed burn wind tunnel is one of two reference wind tunnels used by Midwest Research Institute (MRI) to develop the emission factors for industrial wind erosion presented in US EPA's *Compilation of Air Pollutant Emission Factors (AP-42)*. [NOTE: The reviewer's comment on the adequacy of the wind tunnel test section to develop stable boundary layer conditions speaks to a number of other comments.]
8	In answer to Focus Group Question 4, regarding small-scale effects of surface cover and roughness: One limitation of this wind tunnel design is the small working area of the tunnel on exposed soil. … In order to characterize differences in surface cover and surface roughness, the tunnel has to be moved several times … and the tests replicated. That gives satisfactory statistics between replicate results.	Adequate replicates were performed to ensure representativeness and satisfy quality criteria, as expressed in response to prior comments.
9	Continuing the answer to Question 4, regarding small scale turbulence: Turbulent variations on a small scale are abnormal in this wind tunnel, however, … inlet flow conditioning … serves to remove the natural large-scale turbulence and create small-scale turbulence. The result is that … flow variations are high-frequency causing particles on the surface to oscillate, something that would not be as important in nature. The concept of soil binding is that the release of any particle … does not occur until the aggregate containing the particle is stressed by force imbalance. Oscillations cause different forces than direct shearing stress. An abnormal surface particle behavior may explain why dust concentrations as measured	Regardless of the mechanism of individual soil particle liberation from the soil matrix, the small-scale turbulence created in the wind tunnel boundary layer (in lieu of large-scale shearing forces) appears to fully deplete the material available for erosion. Given the end use of the data, the potential excess in the resultant erosion potential is acceptable to the Working Group.

	by the tunnel effluent appear to this reviewer to be very large, and gives cause for concern that the tunnel method over estimates emission loss and erosion potential. … In my opinion, the larger values of PM-10, TSP, and erosion potential reported may be construed as upper bounds, and thus provide a factor of conservatism to protect against unusual inhalation exposure.	
10	In answer to Focus Group Question 5, regarding surface roughness acting to retard release of surface particles: At the high speed in the wind tunnel it is likely that once a particle is in motion it remains in motion until it exits the test section.	Scouring of the internal surfaces of the wind tunnel at peak wind speeds is well documented by MRI in these and prior experiments, consistent with the reviewer's comment. Experimenters have observed that particle entrainment continues at least to the sampling point once a particle is liberated from the test surface.
11	In answer to Focus Group Question 6, regarding appropriateness of sampling period: The sampling period is "appropriate" for this particular protocol. … The soil material measured at the tunnel exhaust is the integration of all the observed peaks and the data are summed over all previous wind speed step changes.	The sampling period was appropriate because it allowed essentially all available particulate matter to be eroded at every wind speed step before increasing the speed to the next level. Wind speed steps of approximately 2 m/s (5 mph), from zero to the maximum wind speed attainable for the given surface condition, continued until the full wind speed potential of the tunnel was reached for each test plot. (NOTE: differences in the roughness length of individual test plots resulted in different observed peak wind speeds between test runs.) Each step in wind speed proceeded only after optical particle counter data showed a return to baseline particle count rates. See Figure 3 of the controlled burn report.
12	In answer to Focus Group Question 7, regarding ability of wind tunnel to reproduce actual meteorological conditions expected during high winds at Rocky Flats, and the availability of validation data: The wind tunnel causes resuspension only by increased shearing stress on the surface (measured by friction velocity). Wind records at Rocky Flats show that 95% of the time the winds are less than 18 mph, and … the friction velocity would be less than 50 cm/s. But the wind tunnel results are expressed for 95-mph winds and friction velocities of about 250 cm/s. So at 95 mph the … shearing stress is 25 times the 95^{tth} percentile values observed at Rocky Flats. By extrapolation from the frequency distribution of winds observed at Rocky Flats I estimate that the likelihood of sustained 95-mph winds at Rocky Flats is just a few hours each year. We have indeed chosen an extreme	Any conservatism created though the use of the approach is accepted by the experimenters, given the application of the data toward RSAL development. Because limited-reservoir soil erosion is a function of wind speed peaks, rather than average wind speed (as evidenced by the rapid decay in wind tunnel particulate concentration following each step change in wind speed), and because of differences in roughness length among test plots which limited peak centerline wind speed, the normalization of wind tunnel erosion potential to 95 mph is appropriate despite its conservative bias.

	case.	
13	In answer to Focus Group Question 8, regarding wind tunnel's ability to realistically and adequately account for vertical wind velocity: The average vertical velocity at the ground surface is zero, both in the wind tunnel and outside the tunnel. Only the variations (turbulence) in the vertical wind velocity are important, and the "typical" (root-mean-square) vertical variations are about the same as the friction velocity. … it is my opinion that at high speeds the high frequency turbulence would cause abnormal particle behavior on the soil surface, in that the oscillations of the particles would cause an over estimation of erosion potential.	The reviewer's assertion that high-turbulence conditions created in the wind tunnel generate conservative estimates of erosion potential relative to "real world" conditions is consistent with the beliefs of the experimenters. It is important to note that the vertical vector of wind sheer is consistently orders of magnitude smaller than the horizontal vector at Rocky Flats, based on horizontal and vertical wind speed data, and therefore has far less impact on soil erosion. The rare occurrence of a meteorological event with a significant vertical component (e.g., a dust devil) would be short-lived and of limited horizontal extent, and would therefore have very little impact on annualized exposure estimates such as those produced using RESRAD.
14	In answer to Focus Group Question 9, regarding adequacy of wind tunnel to represent the effects of rapid fluctuations in wind speed, wind direction and turbulence: The rapid fluctuations in wind speed are taken into account through the friction velocity in the wind tunnel. The turbulence outside at Rocky Flats may be large, but we think of it as "gusts" that are large in scale (tens of meters) as compared to the wind tunnel where the turbulence is more like 0.01 meter in scale. … I can accept this turbulence scale difference because I believe that it leads to an over estimate of suspended dust….	It is the smaller scale turbulence that projects wind flow into direct contact with the erodible surface and contributes to particle entrainment, as described in response to prior comments. The well-developed boundary layer created within the wind tunnel generates significant small-scale shearing forces that may tend to liberate erodible material in a more effective manner than the natural erosive process.
15	In answer to Focus Group Question 10, regarding effectiveness of wind tunnel in interacting with differently sized particles: The particulates that are resuspended are rarely primary particles. That is, they are clusters of many kinds and sizes of particles called aggregates. The resistance to wind erosion thus depends on the strength of the aggregate bonding. … The wind tunnel provides sufficient shearing stress at the surface to suspend particle aggregates in the size ranges far greater than the respirable-size particles. … Redeposition [in the tunnel – ed.] is negligible.	Prior studies using the MRI reference wind tunnels, such as those that resulted in the EPA-recommended industrial wind erosion emission factors presented in *Compilation of Air Pollutant Emission Factors (AP-42)*, document the resuspension and capture of particle sizes on the order of 100 μm aerodynamic diameter in the wind tunnel effluent. Particles of such size play a role in liberating finer particles through physical interaction with the soil surface but have insignificant direct impact on human exposure via the inhalation pathway.
16	In answer to Focus Group Question 11, regarding the effectiveness of the wind tunnel at reproducing resuspension at different wind speeds for different particle sizes: The wind tunnel does control wind speed and can	The effects of wind speed steps on coarse and fine particle erosion is adequately quantified though the wind tunnel protocol, as noted by the reviewer. If the wind tunnel protocol had serious

	thus be used to estimate erosion potential as a function of wind speed. … The wind tunnel provides a means of measuring the full range of wind speed effects on erosion potential…. These results are not subject to any limitation with respect to threshold debates. So the data are very useful for determining Radioactive Soil Protection Levels regardless.	limitations in duplicating the effects of differing wind speeds on the erosion of differently-sized particles, though such effects are not in evidence, then the normalization of data to 95 mph 10-m equivalent wind speed would mitigate any limitations related to lower wind speed effects.
17	In answer to Focus Group Question 12, regarding appropriateness of particle sampling protocol: There remains one discrepancy that the authors have not satisfactorily explained. That is, the Dust TRACK unit which was calibrated with a standard dust (Arizona road dust) did not agree with the mass sampling train. … The main function of the DustTRACK was to provide real time particle concentration data and this function was not seriously compromised by the data adjustments.	The operating principle of the DustTRAK is based on 90° light scattering. Light scattering (deflection) by local variations in refractive index is caused by the presence of particles whose size is comparable to the wavelength of the incident light. The theoretical detection efficiency peaks at about 0.2-0.3 μm and decreases in a physically predictable manner for larger particle sizes. The DustTRAK PM_{10} monitor was calibrated against the actual PM_{10} mass collected on the backup filter of the wind tunnel effluent sampling train during a given test run. Calibration of the DustTRAK data against the PM_{10} filter mass eliminated the bias of the optical particle counter against larger particles (i.e., particles approaching 10 μm aerodynamic diameter). This calibration required an integration of the real-time DustTRAK PM_{10} concentration profile (versus time) and calculation of the average DustTRAK PM_{10} concentration. The average DustTRAK PM_{10} concentration was then compared to the average PM_{10} concentration calculated from the PM_{10} mass collected on the backup filter below the cyclone. Use of the DustTRAK monitor provided a more comprehensive analysis of surface erodibility than wind tunnel effluent sampling alone. This is particularly appropriate for surfaces that do not have a well-defined wind erosion threshold velocity.
18	In answer to Focus Group Question 13, regarding the treatment of deposition and resuspension in the wind tunnel: It is a safe bet that deposition (or, redeposition) is not occurring in the test section of the wind tunnel for reasons stated previously. So particles are entering the sampling train that normally might be redeposited and held at a higher bonding energy. The wind tunnel results would tend to over-predict erosion potential.	The subtraction of background concentration eliminates the over-prediction that might be associated with ambient dust concentrations entering the wind tunnel; however, the saltation impacts of ambient dust on the soil surface may contribute to greater effluent dust concentrations than would be measured if natural deposition mechanisms were not overshadowed by the high winds generated within the tunnel. Any lingering over-

		prediction is acceptable to the experimenters given the end use of the data.
19	In answer to Focus Group Question 14, regarding methods used to verify sampling efficiency of the wind tunnel: One of the best methods of verifying one type of sampling efficiency would be to used the wind tunnel on radioactively-labeled soil. But of course that was done here, quite independently, during the investigations following the wildfire. … There are other types of verifications that could be done, but there is no indication that the tunnel is underestimating suspended mass because of some inefficiency problem. In face, it is my opinion that the wind tunnel overestimates the erosion potential; see question 8.	The post-wildfire wind tunnel studies clearly demonstrated that activity-enrichment of resuspended dust from contaminated soils is not occurring. The post-wildfire study used Pu-239 as a radioactive tracer-of-opportunity and verified the effectiveness of the wind tunnel to collect erodible material from undisturbed and disturbed surfaces with specific activities that were consistent with the activities measured in the erodible layer of the underlying surface soils.
20	In answer to Focus Group Question 15, regarding activity related intake by humans: For all practical purposes the enhancement factor argument can be neglected at Rocky Flats as this data indicates. ["data" are wildfire study data – ed.]	Haines, et al., demonstrated in *Correlating Plutonium Activity in Fugitive Dust to Plutonium Concentration in Surface Soils at Rocky Flats, Colorado* (2001) that actinide contamination in surface soils will be resuspended by wind at a specific activity not exceeding the specific activity in the soil reservoir. That is, actinide concentration in dust eroded from the contamination area east of 903 Pad is 1:1 or less compared to the actinide concentration in the soil reservoir. No enrichment of actinide concentration through wind erosion was observed (in fact, dilution was observed in the PM_{10} particle size range, probably due to preceding deposition of diluting materials onto the contaminated soil surfaces).
21	In answer to Focus Group Question 16, regarding representativeness of increased air concentration determined by wind tunnel: It is the opinion of this reviewer that the results are likely to be an overestimate of suspended dust and erosion potential compared to the worst that would ever be observed in nature. … Additional analysis of the data may be helpful, however.	As stated throughout this response, study results that provide conservative inputs into RESRAD and the risk assessment to produce reasonably conservative RSALs are acceptable to the Working Group. In the field studies performed, it is not reasonably possible to eliminate this bias.
22	Response to "Evaluate if the wind tunnel results are being properly used in developing input values for application in the selected … models: Because of the extensive data available for screening level purposes, the resuspension factor used in risk assessments is recommended (NCRP 129, 1999) to decrease as t^{-1} and this is in agreement with the wind tunnel observations at Rocky Flats…. In the Appendix A of the RSAL Task 3 Report, …I saw that the air concentrations as well as the base erosion	The relative agreement of the Site-specific Rocky Flats resuspension factor to independently-developed resuspension studies performed at Chernobyl reinforces the experimenters' belief that the wind tunnel study results are representative of real processes. The further agreement with NCRP recommendations should quell any lingering concerns with the applicability of these results to the intended purpose.

	potential multiplier decrease as $t^{-0.69}$ which is a confirmation that recovery from fire is not unlike the decrease in resuspension factors observed following Chernobyl. We should all feel more confident that this is a unifying observation and in line with the NCRP recommendation for screening level risk assessments.	The fact that the post-fire erosion potential multiplier curve produced in this study is based on a very limited set of data suggests that its relative agreement with other studies would support implementation of the more theoretically based t^{-1} dependence. The analysts chose instead to use the more conservative empirical result.
23	I am in complete agreement with the choice taken by the Task 3 Working Group authors to use the observed mass loading distributions for Rocky Flats as the site-specific data and preferred over any mass loading data inferred directly from the wind tunnel study. … The approach is much more realistic than other risk assessment approaches known to this reviewer…for the case of fire effects.	The Working Group has confidence in the quantity and quality of the local ambient particulate matter concentration data and modeling inferences used to develop the probabilistic mass loading distribution.

	Review Comments – Wind Tunnel Reviewer # 3	Response
	General Comments:	
	No general comments require response	
	Specific Comments:	
1	Report A [Wildfire Report – ed.] uses 38.95% as the ratio of PM10 to total suspended particulate mass but Report B [Controlled Burn Report – ed.] uses 50%. Since 50% sounds like an approximation and 38.95 sounds like a measurement, I would suggest revising Report B with the 38.95%.	Because the wildfire report examined the very low concentration of actinide in airborne dust particles and compared these to the actinide concentration in the soil from which the dust was eroded, it was critical to the accuracy of the ambient background correction that the PM10 to TSP ratio be known. Therefore, Colorado Department of Public Health and Environment data from ambient air particulate matter samplers located within several hundred yards of the wildfire area were queried and the average PM_{10}:TSP ratio for the area determined to be 0.3895. For the controlled burn data correction, where the results were used to develop post-fire erosion potential multipliers based on comparisons of erosion from adjacent burned and unburned plots, an estimate of the background correction was sufficient. As the following sensitivity analysis shows, the error introduced by assuming a PM_{10}:TSP ratio of 50% was small: Test Run CB-7 (from Appendix D) Wind-tunnel PM_{10} net mass: 9.15 mg Background net mass: 8.49 mg Estimated (50%) PM_{10} background mass: 4.24 mg Calculated (38.95%) PM_{10} background mass: 3.31 mg PM_{10} erosion potential (50% ratio): 0.12 g/m2 PM_{10} erosion potential (38.95% ratio): 0.14 g/m2 The calculated (38.95%) correction would result in a net growth in PM_{10} erosion potential for both burned and unburned plots. However, because the end use of the data is to develop a post-fire mass-loading multiplier by calculating the ratio of burned to

		unburned plot results, the same PM_{10} correction is applied in the numerator and the denominator of the multiplier. Since the denominator is a smaller erosion potential (unburned) than the numerator (burned), a decrease in the PM_{10} correction, as reflected here, will result in a smaller post-fire multiplier. By using the estimated PM_{10} background correction, the multiplier used in the RSAL calculations is larger than it should be, hence is conservative.
2	I got confused with the discussion of the mass collected, until I came to the realization that mass collected by the cyclone doesn't have PM10. … I think that some rewriting of this section should be done to prevent people like me from getting confused. There is no problem with Report B where isokinetic sampling was done.	This comment will be noted to the authors of the original report.
3	Tests were run until the end of soil movement. I think it would be informative to compare the times needed for the end of soil movement for the different locations.	Such a comparison would be complicated by differences in roughness length between locations. It was the observation of the experimenters that roughness length (which limits peak centerline wind speed) increased as vegetation recovered over time. The increase in roughness length was more likely to have driven differences in time required to achieve complete collection of available erodible material than test plot geography, given that all plots were collocated atop a common soil type and geologic unit of relatively level elevation.
4	(Trivial) The last line of page D-6 should have 0.0022945 pCi/cubic meter.	The reviewer's comment is noted. The example calculations were copied into document format from spreadsheets, so background rounding of multiple-place decimal values may create the appearance of minor errors.
5	These assumed values may or may not be correct, but the curve is dominated by the assumptions, not by experimental data. The multipliers should be labeled as "assumed post-fire erosion potential multipliers."	The reviewer's use of the word "assumed" to describe the post-fire erosion potential multiplier curves is acknowledged. However, in the case of the spring fire curve, the return of erosion potential to its ground state (pre-fire conditions) has been observed in the prescribed burn plot and is not an assumption. Therefore, the zero values that dominate the spring fire multiplier curve beginning month 13 are not assumed. The fall multiplier curve is certainly less well characterized, and depends on the assumption that a fall post-fire multiplier curve (as a function of the vegetative recovery rate) has a shape similar to the spring

		curve, but this assumption is supported by local ecologists. See the response to Comment 10 from Peer Reviewer 1 for additional discussion.
6	Addressing FG Q1: The scientists and equipment have a long history of quality work in measuring fluxes of particles emitted by wind erosion.	The fact that this equipment was used to develop emission factors for industrial wind erosion (presented in US EPA's *Compilation of Air Pollutant Emission Factors (AP-42)*) was considered an endorsement of the technique for the given application.
7	FG Q2: The pitot tube methodology is adequate for characterizing the wind profile since fast-response anemometry is not needed.	The pitot tube method has two primary qualities recommending it for this application: • It is an EPA reference test method for determining air velocity in ducts; and • It is sufficiently rugged for the application (i.e., it will not be compromised by particle impacts or contact with the ground.
8	FG Q3: One must consider that the results are relative to the length of the wind tunnel and that the work done was self-consistent under the conditions that are described in the methodology. That is, I think that no portable wind tunnel would exactly duplicate all possible fetch effects, but that some wind tunnel had to be used and that this wind tunnel is probably as good as most would be relative to the fetch effect.	It is true that small amounts of erodible material may be sheltered by surface roughness elements from the entraining energy of the wind tunnel when a predominant wind direction exists. However, the boundary layer flow generated at soil level is not uni-directional, but is accompanied by turbulent eddies and wakes created through wind interaction with surface elements. This turbulence reduces the sheltering effect of surface irregularities, as observed by the experimenters.
9	FG Q4: This wind tunnel adequately accounts for small-scale variations in surface cover and surface roughness. It does not account for large-scale or middle-scale variations, however.	As presented by Peer Reviewer 2 and stated repeatedly in response to comments, the small-scale turbulence created in the wind tunnel boundary layer (in lieu of large-scale shearing forces) appears to have produced conservative post-fire mass loading enrichment factors for use in RESRAD and risk analyses. Therefore, given the end use of the data, the limitations of the wind tunnel to reproduce natural, large-scale wind effects are minimal and likely resulted in higher than actual erosion potentials for prevailing conditions at Rocky Flats.
10	FG Q5: Roughness can act to dam or retard rather than release particles. This happens in nature too. Consequently, I think that this phenomenon is adequately modeled in a wind tunnel.	The experimenters agree that the presence of roughness elements is essential to the development of representative measurements of erosion potential. Variability in roughness element size between test plots required replicate tests to provide satisfactory statistics, which was accomplished.
11	FG Q6: I assume that the DustTRACK instruments were used to measure	The reviewer's assumption is accurate, as evidenced by Figure 3

	when the dust concentration returned to the level from which it started before wind erosion started. Therefore, I assume that the sampling periods were adequate.	of the controlled burn report.
12	FG Q7: The wind tunnel was designed to reproduce conditions near the ground during high winds. From tests of the wind tunnel for other locations, this tunnel is well suited for this job.	The boundary layer developed in the wind tunnel generates wind shear stress that mimics or exceeds the erosive force of natural winds of the same magnitude.
13	FG Q8: Vertical wind variations are modeled well with the wind tunnel. See Question 9.	It is important to note also that the vertical vector of wind sheer is consistently orders of magnitude smaller than the horizontal vector at Rocky Flats, based on historic horizontal and vertical wind speed data, and therefore has far less potential impact on soil erosion. The rare occurrence of a meteorological event with a significant vertical component (e.g., a dust devil) would be short-lived and of limited horizontal extent and would therefore have very little impact on annualized exposure estimates such as those produced using RESRAD.
14	FG Q9: In wind tunnels, the flux of momentum is carried by smaller-scale fluctuation than in outdoor work. However, one gets the same results by comparing resuspension for the same friction velocity in a wind tunnel or outdoors experimentation. That is, for the same friction velocity (momentum flux) you get the same resuspension, even though the turbulent spectrum is different for outdoor and wind-tunnel winds.	The large-scale components of wind turbulence have little overall effect on wind erosion; only the small-scale turbulence and resultant sheer stress is effective at penetrating surface roughness elements and dislodging particles that ultimately contribute to the soil flux. These small-scale components are more influenced by surface roughness than would be large-scale components. As was stated by Peer Reviewers 2 and 3, the inability of the wind tunnel to mimic large-scale turbulence has little or no affect on its ability to produce small-scale turbulence within the surface boundary layer, causing wind erosion of the available particle reservoir at a representative or even conservative rate.
15	FG Q10: See answer 9 above. For the resuspension of PM10, the dominant mechanism is the sand-blasting of the surface by particles larger than 100 micrometers. …	The influx of ambient dust into the wind tunnel, combined with the resuspension of larger aggregate from the soil reservoir as wind speeds increased, provided sufficient quantity of larger particles to initiate saltation and liberate PM_{10}. The subtraction of background concentrations of TSP and PM_{10} from wind tunnel effluent concentrations accounted for the net numerical influence of incoming, saltating particles without allowing their presence to bias the erosion potential measurement of the test plot itself.
16	FG Q11: Yes, wind tunnels and outdoor experimentation give consistent threshold friction velocities for different particle sizes.	If the wind tunnel protocol had serious limitations in duplicating the effects of differing wind speeds on erosion of differently-

		sized particles, of which there is no evidence, then the normalization of data to 95 mph wind speed would mitigate any limitations for a given wind speed.
17	FG Q12: Non-isokinetic flow is corrected for in the report	Representative samples for all particle sizes of interest were obtained through isokinetic sampling and, when isokinetic conditions could not be maintained during the wildfire tests, through correction of results to account for potential non-isokinetic bias.
18	FG Q13: The wind tunnel results give a net flux for the area sampled by the wind tunnel. … For the scale involved, however, the wind tunnel test is adequate.	The experimenters agree that the net erosion potential is measured on plots that are small in scale relative to the area of the fire. However, the approach is adequate given the number of replicate test runs and the conservative nature of the resulting data analysis.
19	FG Q14: See answers to above questions.	No additional comment is offered.
20	FG Q15: Activity or dust concentration increases with wind speed and this is shown in the data.	Haines, et al., demonstrated in *Correlating Plutonium Activity in Fugitive Dust to Plutonium Concentration in Surface Soils at Rocky Flats, Colorado* (2001) that actinide contamination in surface soils will be resuspended by wind at a specific activity not exceeding the specific activity in the soil reservoir. That is, actinide concentration in dust eroded from the contamination area east of 903 Pad is 1:1 or less compared to the actinide concentration in the soil reservoir. No activity enrichment of actinide concentration through wind erosion was observed (in fact, dilution was observed in the PM_{10} particle size range, probably due to preceding deposition of diluting materials onto the contaminated soil surfaces).
21	FG Q16: Yes, increases in air concentrations associated with increasing wind speeds are reasonable.	No additional comment is offered.

Perspectives on Nuclear Weapons and Community Health
A Newsletter of the Community-Based Hazard Management Program

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Shaking the Foundations? The DOE Low-Dose Study Program

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Senator Pete Domenici of New Mexico, an ally of the nuclear weapons industry and an advocate for nuclear power, complains from time to time about "the immense costs that we may be incurring by using highly conservative [radiation protection] standards." In a 1997 speech at Harvard he pinpointed the problem: "We regulate exposure to low levels of radiation using a so-called 'linear no-threshold' model, the premise of which is that there is no 'safe' level of exposure." To enforce such standards, he complained, "We spend over $5 billion each year to clean contaminated DOE sites to levels below 5 percent of background." In response, he authored legislation that created the DOE Low Dose Radiation Research Program, a 10-year $200 million "program to understand how radiation affects genomes and cells so that we can really understand how radiation affects living organisms. For the first time, we will develop radiation protection standards that are based on actual risk." [1]

Domenici says this DOE low-dose study "offers our best hope for increased scientific understanding on which better standards eventually can be based" [2]. He hopes this program can lead to relaxed radiation protection standards, thereby reducing the costs for all things nuclear — cleanup, waste management, a revival of nuclear power. The following offers a critique of this little-known program.

In October 1997, pursuant to legislation authored by Senator Domenici, DOE initiated its Low Dose Radiation Research Program. The program will "help resolve the low-dose problem" [3] — namely, that heretofore it has not been possible directly to detect induction of cancer from low-dose radiation exposure. Low-dose effects thus have been extrapolated from results observed at higher doses, usually according to the linear no-threshold hypothesis with its assumption that "each unit of radiation, no matter how small, can cause cancer." [4]

The claims made for the low-dose study by DOE's Office of Biological and Environmental Research (BER), where the program is housed, are not modest. First, by building on advances in molecular biology and instrumentation not available to earlier researchers, scientists in this program are "able to evaluate biological changes at levels of radiation exposure and biological organization that previously were not measurable." They thus "will be able to determine if low doses of radiation pose a greater or lesser health risk than currently assumed." Indeed, their research "may result in major paradigm shifts in radiation protection and biology." [5]

While the program is sometimes described as pure science, according to the program's director the science is policy-driven: "The goal was, and still is, to provide new scientific information that can be used in the development of future radiation risk regulatory policy." [6]

The program sponsors research in the following areas:

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- Radiation-induced versus normal oxidative damage: How are they alike or dissimilar?
- Adaptive response: To what extent does the organism recognize and repair low-dose radiation damage?
- Thresholds: Are there levels below which adverse effects are either nonexistent or reparable by normal processes? If so, "then there should be no regulatory concern for exposures below these thresholds." [7]
- Genetic susceptibility: Are some individuals or groups more sensitive to damage from low-dose radiation exposure?

What should one say about a program like this? First, that the agenda is impressive. Second, that there are obvious lacks. For instance, while the program includes research into genetic factors that may affect susceptibility to low dose exposure it apparently excludes research on the genetic effects of low-dose exposure, a topic about which too little is known. Other areas evidently omitted from the research program are non-cancer effects from low-dose exposure, cumulative effects from multiple sources of radiation, and synergistic effects from radioactive and non-radioactive toxins.

The biggest problem with the DOE program, however, the one that dwarfs all others, is that it is a DOE program. DOE personnel administer the program, identify needed research, organize peer reviews, award contracts, monitor work in progress, and ensure "a balanced research portfolio focused on the DOE, BER and Program missions" [8]. Of the 44 grants made as of this writing, close to half fund research that is being done by DOE personnel at DOE facilities — which means that probably well over 50 percent of those funded would be shown to have ties to DOE were their backgrounds scrutinized closely.

Affected populations are conspicuous by their absence. Public participation is scant — mostly limited to hits on the program's web site. When I asked a prominent critic of the DOE why he wasn't paying more attention to this project, he observed: "Nothing that comes out of a DOE-run study on radiation effects will have credibility." His attitude is widely shared among public interest groups. Yet a ten-person advisory committee appointed by DOE and serving at its pleasure provides a facade of public involvement and an imprimatur of sorts.

A DOE-controlled study of radiation and risk is troubling not simply because DOE, with its well-documented record of damage and deceit [9], is one of the least credible agencies of the U.S. government. It is even more troubling because of the inherent conflict of interest. Jim Thomas, formerly with the Hanford Education Action League, subsequently for several years research director for the Hanford Health Information Network, states the issue forthrightly: "DOE should have nothing to do with this research. The funding should be transferred elsewhere. DOE, facing an astronomical clean-up mortgage, has everything to gain from lowering standards. It has a direct conflict of interest." [10]

Interestingly, the program briefly — during FY 1999 and 2000 — was funded in part from DOE's Environmental Management or cleanup budget. In effect, for roughly a year-and-a-half of the program's expected life of 10 years, DOE was using its own cleanup funds to pay for work intended to explore whether the radiation protection standards that drive up cleanup costs at its facilities should be relaxed.

What is said above should not be taken as a condemnation of all the science done under the auspices of this program. Based on my observations of a recent research report session [11] it appears that the linear no-threshold approach to calculating risk is strongly supported by a number of research reports. Some even suggest supralinear effects. In the three-day session I attended, the only explicit attempt to demonstrate hormesis proved a failure, but not in the eyes of the researchers, who speculated that hormetic effects must require a longer period for expression than they allowed in their project (this, I thought, set them up nicely for requesting renewed funding). The verdict on adaptive response to low-dose exposure certainly seems mixed — and much of the talk in this area raises issues of where adaptive response ends and bio-engineering begins, an issue fraught with ethical considerations not addressed. Several reports seemed also to demonstrate the reality of bystander effects — that is, that radioactive hits on a cell also adversely affect nearby "bystander" cells.

All these topics, and more, need to be well and thoroughly understood. Just because some of the research findings support a conservative rather than a permissive approach to low-dose exposure does not automatically mean the studies are credible. It is adherence to scientific method and public accountability that determines whether any finding is a credible one. Regarding the first, even the peer reviewers for work done under the auspices of this program are selected by DOE, and of course the overall design and direction of the program fits the management goals of DOE. It is a shame and a hindrance that some undoubtedly very good work is right now being done under the DOE cloud.

As for the second point, to ensure public accountability the Low-Dose Radiation Research Program should be moved completely out of DOE, preferably to an independent, non-governmental research entity, perhaps at a major university, where it would continue to be publicly funded, as befits our collective responsibility. A broadly representative body of scientists and non-scientists, including people from affected populations, should be given the task of designing, managing, and overseeing the research program in an open manner that would be a credit to our democracy and a fitting precedent for the future. A policy-driven scientific research program should reflect public

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concerns and values. This can only be achieved in an open process that closely ties together high quality, independent science with meaningful participation of all those who are affected. Such a program would free the scientists to do their work without constraint or fear of taint. More importantly, it would encourage and cultivate the public trust.

Sources

[1] Senator Pete V. Domenici, "A New Nuclear Paradigm," speech at Harvard University, October 31, 1997. See www.senate.gov/domenici/nuclear/harvard

[2] Senator Pete V. Domenici, speech to National Academy of Engineering Symposium on "Nuclear Power: The Option for the 21st Century," Irvine, California , February 9, 2001.

[3] Overview Prepared for the DOE Low Dose Radiation Research Program Advisory Committee, U.S. DOE, Office of Biological and Environmental Research, Life Sciences Division, August 2000, p. 1. http://lowdose.org

[4] http://www.er.doe.gov/production/ober/lowdose.html

[5] Overview Prepared for the Low Dose Radiation Research Program Advisory Committee, U.S. DOE, Office of Biological and Environmental Research, Life Sciences Division, August 2000, p. 1. http://lowdose.org

[6] DOE Low Dose Radiation Research Program Coordinator David Thomassen, personal communication, June 15, 2001.

[7] http://www.er.doe.gov/production/ober/lowdose.html

[8] Thomassen, personal communication, June 15, 2001.

[9] This point can be documented for every DOE weapons facility. For an overview, see H. Jack Geiger, D. Rush, David Michaels, *Dead Reckoning: A Critical Review of the Department of Energy's Epidemiologic Research* (Washington, DC: Physicians for Social Responsibility, 1992); Arjun Makhijani, Howard Hu, and Katherine Yih (eds.), *Nuclear Wastelands: A Global Guide to Nuclear Weapons Production and Its Health and Environmental Effects,* by a Special Commission of International Physicians for the Prevention of Nuclear War and The Institute for Energy and Environmental Research (Cambridge, MA: The MIT Press, 1995), chaps. 1-6.

[10] Personal communication from Jim Thomas, February 26, 2001.

[11] Joint DOE/NASA Radiation Investigators' Workshop, Arlington, VA, June 27-29, 2001.

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Response to Peer Review Comments

Wind Tunnel Analysis

Bob Nininger

presenting on behalf of

RSAL Working Group

Wind Tunnel Study History

- Two wind tunnel studies performed in CY2000 by MRI and URS Corporation
- Studies quantify site-specific conditions pertaining to soil wind-erosion potential following a fire on Buffer Zone soils.
- Information used to develop annual mass-loading multiplication factors for use in estimating RSALs.

Peer Review Comments

- Three peer-level scientists reviewed the two wind tunnel reports, and methodology used to develop the RSAL mass loading distribution
- 56 significant comments have been identified from this review, including answers to specific questions posed by Focus Group members
- Comments tended to provide a clear discussion of wind tunnel capabilities and limitations.

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Comment Categories

Peer review comments are grouped into four categories for the purposes of this presentation:

- equipment adequacy
- ability of wind tunnel to replicate representative meteorological conditions
- sampling/data representativeness
- data interpretation

Comments dealing with readability are not included in this discussion

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Equipment Adequacy

Focus Group asked reviewers to evaluate ability of equipment/expert to perform erosion potential measurements. Reviewers had several comments:

- recognized expertise and wind tunnel methodology --"…equipment is in good standing with scientific community…"; … pitot tube is essential …"; "… scientists and equipment have a long history of quality work …"
- noted limitations and their effect on the tunnel's ability to generate useable results --"… pitot tube is adequate since fast-response anemometry is not needed."
- "… I think that no portable wind tunnel would exactly duplicate all possible fetch effects, but that some wind tunnel had to be used and that this wind tunnel is probably as good as most would be relative to the fetch effect."

Latter comment addresses more specific meteorological and representativeness questions, as follow

Meteorological Representativeness

Comments relating to meteorologically induced effects have three general themes:

1. wind tunnels do not simulate large scale turbulence, and vertical wind components
2. small working section limits surface roughness variations and their effect
3. saltation effect is limited to the wind tunnel

Comments for each theme category follow; most issues become self-explanatory.

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Meteorological Representativeness (cont')

1 Small-scale wind tunnels do not simulate large scale turbulence (wind speed variations), and vertical wind components

Reviewers offered:

- "The test wind tunnels are probably too small in cross-section and too short in length to accurately simulate atmospheric boundary layer flow over a significant portion of the test section on the rough, test surfaces at Rocky Flats."
- "The ratio of test section to length is greater than 100:1, which is a good indicator of boundary layer development. The main reason for assuring boundary layer development and stability is to characterize and control the shearing stress on the surface… The wind tunnel does that adequately."
- "Yet the erosion potentials so obtained have use in establishing Radioactive Soil Action Levels, providing that we expect that the extreme erosion potentials observed are unlikely to ever exist in nature."

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Meteorological Representativeness (cont')

1 Small-scale wind tunnels do not simulate large scale turbulence (wind speed variations), and vertical wind components (cont')

- "Turbulent variations on a small scale are abnormal in this wind tunnel… The result is that … flow variations are high-frequency causing particles on the surface to oscillate, something that would not be as important in nature. … In my opinion, the larger values of PM-10, TSP, and erosion potential reported may be construed as upper bounds, and thus provide a factor of conservatism to protect against unusual inhalation exposure."
- "In wind tunnels, the flux of momentum is carried by smaller-scale fluctuations than in outdoor work. However, one gets the same results by comparing resuspension for the same friction velocity in a wind tunnel or outdoors experimentation. That is for the same friction velocity (momentum flux) you get the same resuspension, even though the turbulent spectrum is different for outdoor and wind-tunnel winds." [This comment also goes to the representativeness of the data, discussed next.]

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Meteorological Representativeness (cont')

1 Small-scale wind tunnels do not simulate large scale turbulence (wind speed variations), and vertical wind components (cont')

- "The rapid fluctuations in wind speed are taken into account through the friction velocity in the wind tunnel…. I can accept this scale difference because I believe that it leads to an over estimate of suspended dust…."

On vertical velocity:

- "The average vertical velocity at the ground surface is zero, both in the wind tunnel and outside the tunnel."
- "Vertical wind variations are modeled well with the wind tunnel"

Meteorological Representativeness (cont')

2 The layout and size of the working sections limit surface roughness variations and their effect

Comments included:

- This wind tunnel adequately accounts for small-scale variations in surface cover and surface roughness. It does not account for large-scale or middle-scale variations, however."
- "In order to characterize differences in surface cover and surface roughness, the tunnel has to be moved several times … and the tests replicated. That gives satisfactory statistics between replicate results."

These scale-effects noted were addressed in the study:

- The wind tunnel was moved repeatedly on each plot.
- Plots were chosen to be representative of the burned area.

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Meteorological Representativeness (cont')

3 Saltation effect is limited to the wind tunnel

Saltation was addressed:

- "The implicit assumption in the wind tunnel test protocol was that … only wind would affect the test surface during a windstorm."
- "For the resuspension of PM-10, the dominant mechanism is the sand-blasting of the surface by particles larger than 100 micrometers."
- "In the wind tunnel, the onset of avalanching may be a product of the peculiar small scale of turbulence, and more soil might be available than under natural winds."
- "More recent observations show that there is an emission of small particles at speeds below the observed thresholds for saltation, and while this fact amounts to a relatively small emission loss, it affects the surface condition."

Sampling/Data Representativeness

Comments relating to data representativeness focused on three potential issues:

- ability of wind tunnel to obtain representative data,
- representativeness of particle resuspension mechanism, and
- sampling artifacts.

1 Does the wind tunnel have the ability to obtain representative data?

- In general, the comments indicate the wind tunnel will provide data that are adequate for the purposes of developing a conservative RSAL.
  - "This reviewer will make an attempt to show that the observations made by the wind tunnel method provide a set of data that are sufficient to proceed with the determination of Radioactive Soil Action Levels."

Sampling/Data Representativeness

2 Is the particle resuspension mechanism inhibited by the wind tunnel?

- The reviewers commented regarding wind tunnel ability to erode particles, compared to nature:
  - "The wind tunnel provides sufficient shearing stress at the surface to suspend particle aggregates in the size ranges far greater than the respirable-size particles… Redeposition is negligible."
  - "It is the opinion of this reviewer that the results are likely to be an overestimate of suspended dust and erosion potential compared to the worst that would ever be observed in nature."
  - "Roughness can act to dam or retard rather than release particles. This happens in nature too. Consequently, I think that this phenomenon is adequately modeled in a wind tunnel."

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Sampling/Data Representativeness

3 Do sampling artifacts cause the data to not be representative?

- One reviewer questioned calibration of the DustTRAK instrument, but concluded "The main function of the DustTRAK was to provide real time particle concentration data and this function was not seriously compromised by the data adjustments."

We agree and comment further: The DustTRAK calibration depends on the size distribution of the test aerosol; the instrument employs optical scattering. It was used for purposes of establishing depletion, and did not need absolute calibration.

43.

Sampling/Data Representativeness

3 Do sampling artifacts cause the data to not be representative?

"To increase the accuracy of tunnel estimates it would have been useful to have a cyclone preseparator on the ambient PM-10 filter."

We agree with this comment. It is likely that the uncertainty in the multiplication factor introduced by the empirical correction is around 10 percent, based on PM_{10}:TSP ratio observed near the wildfire site. The multiplication factor is conservative with the empirical correction used.

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Data Interpretation

A number of significant comments relate to interpretation of the results for RSAL purposes:

- Soils at Rocky Flats are a "limited source" -- "The 'limited source' concept means that … the present wind tunnel results would tend to overestimate the PM-10 available for resuspension"
- "The post-fire erosion potential for the fall fire is estimated without a clear basis" -- Vegetative regrowth is predictable, and regrows with a similar pattern whether the regrowth starts in the spring or fall. As a first approximation, the shape of the "recovery curve" would be expected to be similar in form.
- Regarding the shape of the "recovery curve" -- "the resuspension factor used in risk assessments is recommended (NCRP 129, 1999) to decrease as t^{-1} and this is in agreement with the wind tunnel observations at Rocky Flats."

Data Interpretation

- One reviewer inferred correctly that the fall fire consequences are truncated after the first year. While this is true, it is also true that fires in consecutive years on the same area are not excluded in the manner the data are used probabilistically, even though their consequences would be negligible. The second year of the fall fire recovery would have a multiplier that is smaller than for the spring fire.
- The "appropriateness" of the sampling periods relates to data interpretation -- "The soil material measured at the tunnel exhaust is the integration of all the observed peaks and the data are summed"…to estimate the erosion potential.
- Regarding use of observed site-specific mass loading data -- "I am in complete agreement with the choice taken by the Task 3 Working Group authors to use the observed mass loading distributions…"

Summary Response

- The peer reviews provided very valuable insight into the viability of the wind tunnel for studies of this type.
- Comments reinforce the Working Group's use and interpretation of the data, without change.
- One highlight of the reviews is the sense that the resulting erosion potentials are likely to be overestimated. The effect this has on the mass-loading multiplication factor is likely to be less influential.
- One reviewer's comment caused us to determine that the PM_{10} correction to the erosion potential calculation resulted in a higher than anticipated bias in the mass-loading multiplication factor. However, this finding does not suggest a bias so significant that the multiplication factor should be reduced.

RFCA Stakeholder Focus Group
Attachment D

Title:	RSALs Working Group Meeting Notes for February 28 and March 7, 2002
Date:	March 15, 2002
Authors:	Sandra MacLeod
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NOTES FROM RSALs WORKING GROUP MEETING ON 2/28/02

ITEMS COVERED ON 2/28:

1. Discussed Task 3 response to comments.

ACTIONS

Action Item	Who	When	Notes
Send RESRAD input files to Jim Benetti.	Carl Spreng	3/4/02	
E-mail updated response to comments table to Steve Gunderson.	Each working group member	3/15/02	
Send electronic version of Task 3 report to working group members.	Mark Aguilar	3/4/02	
Make electronic changes to the Task 3 report and e-mail report to Mark Aguilar after ALL changes are made.	Each working group member	3/29/02	
Review responses that have been drafted and were distributed at today's meeting.	Each working group member	3/7/02	Bring comments to 3/7 working group meeting.

DECISIONS

1. Re-run wildlife refuge worker and office worker numbers using adult slope factors.
2. Use a distribution for adult soil ingestion rate.

NEXT MEETING: THURSDAY, 3/7/02, 8:30 a.m., at EPA Conference Center

Agenda Items:

1. Discuss status of Task 3 response to comments.
2. Discuss responses that have been developed.
3. Discuss any changes to calculations.

NOTES FROM RSALs WORKING GROUP MEETING ON 3/7/02

ITEMS COVERED ON 3/7:

1. Discussed 3/20 Focus Group meeting presentation on Task 3 response to comments.
2. Discussed schedule for finalizing response to comments and Task 3 report.
3. Discussed Task 3 response to comments.

ACTIONS

Action Item	Who	When	Notes
Each working group member should e-mail their completed portions of the response to comments table to Steve Gunderson.	Each working group member	COB 3/11/02	
Compile all responses to comments into one table and bring electronic version to 3/14 meeting.	Steve Gunderson	3/14/02	
Make electronic changes to the Task 3 report and e-mail report to Mark Aguilar after ALL changes are made.	Each working group member	3/29/02	
Complete uranium calculations (dose and risk) for distribution to Focus Group on 3/20.	Jim Benetti (dose); Susan Griffin (risk)	3/20/02	
Complete plutonium and americium dose and risk recalculations and submit to Mark Aguilar for the Task 3 report.	Jim Benetti (dose); Susan Griffin (risk)	3/29/02	

DECISIONS

1. Use new plant uptake distribution from Phil Goodrum.
2. Add rural resident to decisions from last week.
3. The working group recommends the following Task 3 comment main topics to be presented at the 3/20 Focus Group meeting:
   - Introduction (Tim Rehder/Steve Gunderson).
   - Parameters: back calculation; adult cancer slope factor; soil ingestion; spreadsheet analysis (Susan Griffin/Diane Niedzwiecki).
   - Uncertainty discussion (Susan Griffin/Diane Niedzwiecki).
   - Recalculation (Jim Benetti).
   - Subsistence farmer response (Tim Rehder).
   - Policy issues (Steve Gunderson).

NEXT MEETING: THURSDAY, 3/14/02, 8:30 a.m., at Rocky Flats B060, Room112

Agenda Items:

1. Final editing of Task 3 response to comments.
2. Discuss 3/20 Focus Group presentations.
3. Discuss any calculations/recalculations that have been completed.
4. Discuss changes to the Task 3 report.

	Review Comments – Task 3 Peer Reviewer 1	Response
	General Comments:	
1	"The decision structure and the nature of the information used have not been made sufficiently clear in the presentation." Reviewer thinks report needs more discussion of its context. How RSALs are used as one of a number of hazard management tools Reviewer thinks the concepts involved in setting an RSAL need to be specifically discussed in the report. Reviewer thinks report needs a clearly articulated approach to the treatment of uncertainties. Reviewer thinks report needs a clear approach to the treatment of differences between people (variability). Acknowledge historical difficulties such as history of public distrust in the text in an effort to develop a credible basis for planning. What is an RSAL? Why does Rocky Flats need them? What were the previous efforts at developing RSALs and why might they change? How will a RSAL be used? (two uses: to decide where the surface can be left alone, and as one input in deciding the degree of cleanup required). How do RSALs work with other hazard management tools? (Important that everyone understand that RSALs are not the only	Task One of the RSAL Report and Attachment 5 of the Rocky Flats Cleanup Agreement, called the Action Level Framework, describes the regulatory approach for the establishment of an RSAL.

	tool). What are the uses and limits of science in developing an RSAL? ▪ What is the risk? ▪ What is the dose? ▪ What are the circumstances for which risks or doses should be estimated? ▪ How are differences between people treated? ▪ How are uncertainties accounted for? ▪ What is a "reasonably maximum exposed (RME) person? ▪ Why do you need scenarios? ▪ How do you choose them?	
2	Reviewer wants more transparent explanation of what the science says and doesn't say, what is uncertain, what are alternative possibilities, and what choices the managers have for dealing with uncertainty. Uncertainties important to setting RSALs need to be presented in a clear, informative way to both managers and concerned parties.	As stated at the end of Section V, we agree that it is important to convey the uncertainties in the available information to risk managers. Section VI discusses the general approach to quantifying variability and uncertainty, and Table VI-1 summarizes the effect that sources of uncertainty may have on dose and risk estimates. The Appendices provide a more detailed description of the alternative approaches that were available to specify probability distributions to characterize variability. In order to improve the clarity of the presentation of potential impacts of uncertainty, Section VI will be expanded to include the following: 1) paragraph on how uncertainty was considered when defining probability distributions to characterize variability (PDFv); 2) an overview of the information gained from the sensitivity analysis; and 3) the collective impact of the uncertainties in setting RSALs for each exposure scenario. In addition, a semi-quantitative ranking of the level of confidence (i.e., low, medium, high) in the PDFv for each input variable will

		be added to the Appendix.
3	A clearer framework for addressing uncertainties will lead the authors to revisit their discussion of certain key parameters in their model which cause significant uncertainty in the dose and risk levels; the most notable of these are: ▪ "mass loading", ▪ "soil ingestion rates" ▪ the EPA dose and risk estimators. These issues should be addressed up-front, at the beginning. A discussion of the strategy and context of the RSALs should be included up-front, right at the beginning. This would increase the clarity of the presentation a) Obligation to acknowledge the uncertainty in a value that is supposed to represent a given percentile of behavior. b) Choice not to include pica child in the child soil ingestion distribution should have more justification c) Variability in dose and risk factors requires more discussion.	We agree that a more comprehensive summary of the uncertainties in the assessment can be added to Section VI and the Appendices. See response to previous question. a) The comment is unclear. The probability distributions used to characterize variability are selected with the intent of describing the full range of percentiles. Point estimates are generally selected to characterize the RME individual, which is consistent with EPA guidance. If the suggestion is to note how the point estimate corresponds with a percentile of the probability distribution for a given input variable, this information can be presented. Often a point estimate is used when there is insufficient information to justify selecting a probability distribution – in such cases, it would not be possible to identify the percentile represented by the point estimate. b) Appendix A, page 19, Section (ii), discusses the rationale for selecting an upper bound of 1000 mg/day for the soil ingestion rate distribution for children. The choice reflects an interpretation of the available data on soil pica behavior that suggests most children will exhibit day-to-day spikes in ingestion rate, but the long-term average is likely to be much lower. The literature suggests that soil pica behavior is an example of an acute exposure scenario which may be of concern for some acutely toxic chemicals. This acute exposure potential is already being addressed in the Industrial Area and Buffer Zone sampling plans by the hot spot methodology. In a chronic exposure scenario, which the RSALs are developed for, we are concerned with

		long-term average soil ingestion rates. The selection of 1000 mg/day is considered to be conservative (health protective) upper bound for the population. c) Appendix A provides detailed discussions of the variability in factors used to quantify dose and risk. **Also see response to comment #8 from this same reviewer.**
4	Reviewer believes that even in a qualitative uncertainty analysis, "one would like some sort of statement of confidence" about how likely the risk estimate is not likely to be exceeded using that choice of parameter. The Reviewer gave an example of categorizing uncertainty into 4 groups: a) a best estimate, b) an unspecified degree of confidence (some added conservatism), c) high confidence, and d) very high confidence that future information will be consistent with the estimate.	We agree that it would be useful to assign a semi-quantitative ranking of confidence in the probability distribution for variability for each factor discussed in Appendix A. This information can be used to expand the Section VI discussion of the confidence in the corresponding risk distribution, based on knowledge of the important sources of variability from the sensitivity analysis. A three-tier ranking system will be used to reflect level of confidence (i.e., low, medium, and high).
5	Reviewer urges agencies to use "high confidence" values for developing the RSAL, rather than the "best estimate" or "conservative estimate of unspecified degree" values that largely were used, in order to increase the robustness of the choice.	The comment appears to reflect a preference to use different words to describe the point estimates and probability distributions selected for the RSAL calculations. We agree that it is desirable to use "high confidence" values when they are available. The intent of the discussion of uncertainty in Section VI and Appendix A is to present the information on uncertainty.
6	Reviewer thinks it would be useful to include a direct quantitative comparison of the newly selected RSALs with previous values, and why there are differences, if any. Doing this will help understanding and indicate the robustness of the selection.	There are substantial differences with the approach used in these calculations as compared with approaches used in the establishment of RSALs in 1996, and with the recommended values as calculated by RAC in 2000. The differences between the current effort and that performed in 1996 are that the current effort: -uses probabilistic methodology -accounts for the elevated concentrations of contaminants in air that would result from periodic grass fires. -calculates risk in addition to dose.

		-considers two additional exposure scenarios: wildlife refuge worker and resident. The two most important ways in which the current effort differs from the work performed by RAC are in how itaddresses grass fires, and in the choice of exposure scenario: the RAC modeled a very conservative resident rancher scenario. The current effort also calculates risk directly whereas RAC calculated risk indirectly The agencies do not intend to retain the RSALs that are currently in the Action Level Framework of RFCA. The agencies do not feel that the effort to prepare a robust quantitative comparison of the parameters used in the calculations over the past six years is warranted. The authors of the Task 3 Report have presented tables and discussion that allow the interested reader to compare the inputs and results of the 1996 and present RSAL calculations, and to better understand the bases for the group's parameter selections in the present work. Detailed information about how the Agencies address the fire issue and how that differs from the RAC methodology is given in Appendix G.
7	The discussions of various uncertainties need to be synthesized (integrated?) "so as to provide a reasonably transparent description of how using any particular calculated value for a RSAL represents taking a position with respect to the underlying uncertainties". Key uncertain parameters that would have a substantial impact on the RSALs, if changed, should be identified.	This comment will be addressed by expanding the discussion of uncertainties in Section VI, as described in response to Comment #2 above.
8	Reviewer wants a) greater discussion of uncertainty and variability in ICRP 72 dose coefficients and FGR 13 risk coefficients, b) quantitation of confidence level in coefficients selected, c) consideration of selection of dose and risk coefficients appropriate	a) Chapter VI is being rewritten to include a greater discussion of, among other things, sources of uncertainty in dose and risk coefficients. The discussion will include the excellent list of sources of uncertainty and variability

	for an RME individual.	contained in Appendix D of Federal Guidance Document 13, relative to the estimate of risk coefficients. Since most of the same sources of uncertainty affect the estimates of dose coefficients, this discussion will suffice for the ICRP 72 dose coefficients used in the Task 3 computations as well. This discussion will remain qualitative only at this time. It is noteworthy that even the ICRP, whose work forms the basis of the dose and risk coefficients used in this Task, has not made a quantitative estimate of uncertainty relative to their recommendations. Sources of uncertainty which will be discussed in the rewrite include: Uncertainties in the structure of biokinetic models: Model of the respiratory tract Gastrointestinal tract model and f1 values Uncertainties in information used to construct biokinetic models for plutonium: Direct information on humans Information on humans from chemically similar elements Direct information on non-human species Information on animals from chemically similar elements Uncertainties in interspecies extrapolation Uncertainties in inter-element extrapolation Uncertainties in central estimates stemming from variability of human populations b) The Working Group feels that it is not possible at this time to quantify the confidence interval of the dose and risk coefficients selected (which are as listed in ICRP 72 and FGR 13), although quantification of uncertainty may be possible in the not distant future. EPA's Office of Radiation

		and Indoor Air (ORIA) is currently tasked with making estimates of uncertainty in the FGR 13 risk coefficients, which is a pioneering effort for a regulatory/guidance agency. The work by the Risk Assessment Corporation which this reviewer has cited as a starting point will be considered by ORIA in its task. The Working Group will incorporate the results of ORIA's work in an Addendum to this Task, if it is felt necessary to revise the dose or risk coefficients recommended by ICRP 72 and FGR 13, in the light of ORIA's work. This is not to dismiss the Reviewer's concern, which is legitimate. At this time the Working Group believes that it has made several prudent decisions in the selection of dose and risk coefficients which argue in the direction of reduced uncertainty: 1. The choice of ICRP 72 ingestion dose coefficients for plutonium over those of ICRP 30 results in a defacto selection of an absorption fraction (f1) some 50 times higher than the f1 value associated with plutonium oxides (used by the Working Group in 1996). Although the FGR 13 still estimates the uncertainty in the f1 value for plutonium to be on the order of a factor of 5, this is an improvement over ICRP 30 and significantly increases the importance of the soil ingestion pathway. 2. The choice of the M absorption type over the less conservative S absorption type for the plutonium inhalation dose and risk coefficients represents a prudent choice in the face of uncertainty in the chemical and physical form of the plutonium in the

		environment, and represents the majority of the Working Group's position that there is uncertainty in the degree of oxidation of the plutonium from the 903 Pad spill, and the size and nature of soil particles to which it is attached. (The DOE disagrees, and believes that this uncertainty is low, and that the S absorption type, appropriate for a pure plutonium dioxide should have been used). In response to the comments of other reviewers, a more complete discussion of the basis for selection of the M absorption type will be included in the revised Task 3 Report. 3. The choice of ICRP 72/FGR 13 coefficients represents a move toward the most complete, and accurate biokinetic models, with a corresponding reduction in uncertainty. c) As to the selection of a special dose or risk coefficient pertinent to the RME individual, the Working Group believes that this goes outside the boundaries of the RME concept and should not be done. "Exposure" as used in the Task 3 Report means the combination of external radiation exposure and internal intake of radionuclides (this concept originates from the more general Superfund context of exposure to hazardous materials, and may not appear to be consistent with exposure as it is used in the field of Health Physics). Reasonable Maximum Exposure means that the combination of scenario features and input parameters which affect exposure (exposure conditions) are considered collectively at their reasonably maximum values – for example the 95th percentile of the cumulative probability distribution. RME does not

		include conversion to dose or risk – to do so would be to introduce additional conservatism or consideration of human variability into the RME concept. The consideration of uncertainty in dose and risk coefficients is best kept separate.
9	Reviewer believes uncertainty and variability of ICRP and EPA dose and risk coefficients should be discussed.	See response to comment #8 above.
10	Reviewer believes the discussion of the sensitivity analysis is "not always helpful or balanced". He believes that the sensitivity analysis together with what is known about the uncertainty in various processes should be used to identify the key uncertainties that will impact the selection of a RSAL".	Section VI will be expanded to include a discussion of how the sensitivity analysis was used to identify the key exposure pathways and variables. This discussion will be tied to the information presented on uncertainty in each point estimate or probability distribution selected for the input variables.

	Review Comments – Task 3 Peer Reviewer 2	Response
1	Paragraph 2 of Overall Summary. Validity of backward calculation method because this method "ignores potential correlations between risk or dose and input variables".	Correlations among exposure variables used to estimate dose or risk are a source of uncertainty in Monte Carlo simulations. In this risk assessment, no information was identified to correlate input variables. The fact that input variables were treated as independent in the Monte Carlo simulation does introduce uncertainty into the resulting risk distribution for forward calculations, and RSAL distribution for back calculations. However, we disagree that it somehow invalidates the back calculation approach.
2	Paragraph 3 of Overall Summary. Inadequate statement of purpose of the probabilistic analysis, up-front. Definition must go beyond a simple determination of a range of outcomes because "the distributions have to be determined in a consistent manner with the overall purpose."	The difference between point estimate and probabilistic approaches is first described in Section II, page 4. Further discussion of the goals of probabilistic risk assessment (PRA) is given in Section VI. The report consistently emphasizes that the purpose of the PRA is to quantify variability in risk or RSAL based on variability in exposure, using probability distributions for inputs. There is no reference to providing a "range of outcomes".
3	Paragraph 4 of Overall Summary. "Interjection of bias by the working group by refusing to assign distributions for variables with sparse data, and using, instead, point estimates.	As explained in Section IV-4 of the report, it may not be appropriate to develop probability distributions for all parameters. For some variables, the existing studies may contain serious design flaws, may not be representative of the site population, or may have an inadequate number of study subjects. The result is lack of confidence in the ability of a distribution to represent the site population. In these situations, a point estimate may be selected to represent a particular variable. If the variable is known to be influential, (per the sensitivity analysis) the use of a point estimate can bias the outcome. For example, if the point estimate is a high-end value, the distribution of risk may be right-shifted (e.g., it is biased in the conservative direction). In situations such as this, it is important that the risk

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		assessor communicate to the decision makers the consequences of this choice. Section VI will be revised to qualitatively communicate the uncertainty and/or bias in the selection of each variable and its impact on the outcome.
4	Paragraph 5 of Overall Summary. Confusing presentation of uncertainty discussion in Section VI. ▪ Lack of separation between variability and uncertainty; ▪ unclear labeling of particular distributions as representing variability or uncertainty. (Column 2 in Tables VI-2 to VI-5).	Section VI will be expanded to provide a clearer distinction between variability and uncertainty, and to reiterate the concept that selections of probability distributions for variability are a source of uncertainty. Column 2 will be removed from Table VI.
5	Paragraph 6 of Overall Summary. Applicability of cancer risk factors taken from Federal Guidance 13, which are derived for mixed age group populations, to single age groups, such as populations that are only adults.	We agree with the reviewer. We will revise the report to use adult-specific cancer slope factors when appropriate.
6	Paragraph 7 of Overall Summary. Quality of presentation. ▪ Wrong fonts for symbols in equations. ▪ References in the text are inconsistent with Table headings. ▪ Tables presented in difficult to read format. ▪ Failure to present some important parameter values, eg., the cancer slope factors referred to on p.46. Reference list has mixture of citation styles.	The typographical errors will be corrected.
7	The reviewer had issues related to a) improper modeling, b) mixing of variability and uncertainty, and c) assigning biased point estimates in lieu of distributions all generally lead to overly conservative conclusions. Reviewer could not tell whether the computed RSALs are appropriate, legitimate, or useful, since the reviewer could not determine the degree of bias in the calculations.	a) As discussed in the response to Comment 1 from this reviewer, we disagree that the back calculation method used in the modeling done to calculate dose and risk-based RSALs was improper. The Working Group was aware of the limitations of the back calculation method in calculating the RSALs. However, the Monte Carlo simulations were run with the assumption of independence among input variables because no information was identified to specify correlations. Moreover, both the concentration term and the risk were characterized by point estimates rather than

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		distributions. Both of these conditions satisfy the criteria under which back calculation is a valid approach (Burmaster et al., 1995; Ferson, 1996; Bowers, 1998, as referenced in the text). b) As discussed in the response to Comment 4 from this reviewer, Section VI will be revised, as necessary, to differentiate more clearly between uncertainty and variability. c) Finally, conservative default values recommended by EPA for calculating RME exposures were used as point estimate values when the incompleteness of the available data precluded much confidence in any distributions. The Working Group decided on this conservatism deliberately, in order to be health-protective. Our rationale is discussed further in the response to Comments 3 & 8 from this reviewer. As mentioned above, the purpose of Section VI is to qualitatively communicate the uncertainty and/or bias in the selection of each variable and its impact on the outcome. It will be revised to more clearly describe, qualitatively, the uncertainty and/or bias inherent in the choices made.
8	Paragraph 9 of Overall Summary. The Working Group should "add some expertise to their group and compute new values of the RSALs in a way that is state-of-the-art and credible to the entire scientific community". This work would be rejected for publication.	The work of this group is based on sound scientific principles and has been performed by professionals well grounded in their disciplines. The staff tasked with working on the calculation of RSALS consider their audience to be the stakeholders involved with the Rocky Flats cleanup and the RFCA parties. The agencies recognize that although an attempt was made to be objective in the selection and calculation of the modeling input parameters, there was bias in the process. This bias was based on recognition of community preferences and input as well as a conscious choice to err on the side of conservatism when there was

		uncertainty. This reviewer, as well as others, have pointed out that some of the parameters were overly conservative. The Working Group believes that it generally employed the appropriate amount of conservatism in light of the uncertainties surrounding certain parameters.
9	Sensitivity analysis problems. Reviewer appears to have understood that the sensitivity analysis for dose and for risk were both performed using Crystal Ball. Text needs to be revised to make it clear exactly how RESRAD was used to perform the sensitivity analysis	RESRAD was used to perform the sensitivity calculations and generate the tables and figures shown in the Task 3 report. The text will be revised to clarify the use of RESRAD for this purpose.
10	Text refers to Fig. IV-4 (2nd paragraph, p.27), but the figure is labeled Fig. IV-5. Figure IV-4 is missing.	Figures are numbered incorrectly; text will be corrected..
11	Addition of 'mass loading for inhalation' parameter to the most sensitive list should not have been done because of "interest" in this parameter since the addition of ad hoc parameters is not objective or based on sound scientific principles	It is true that mass loading for inhalation is not as sensitive of a parameter as were some of the others. In addition to the interest of the community in this parameter, there was general agreement among the working group members that this parameter required special consideration because of the possible effects of a prairie fire and their potentially significant contribution to overall dose to the receptor. Furthermore, the independent RSAL review performed by RAC identified the inhalation pathway as the most important contributor to dose.
12	Sensitivity analysis problems. Impact of using crudely estimated probability distributions on the sensitivity analysis. Reviewer questions why final probability distributions were not used in the first place in the sensitivity analysis.	The working group feels that the reviewer has misinterpreted the text. The sensitivity analysis as described in Section IV-1 of the report used a ratio method based on *point values* to find the most influential variables. Once determined, the process of developing distributions for those variables began. The distributions were used in the RSAL calculations, they were not used in a sensitivity analysis.
13	Reviewer points out that by choosing a conservative quantile of the output distribution to define a "reasonably maximally exposed	As stated above in comment number 8, this reviewer, as well as others, have commented that some of the parameters

	individual", the cleanup costs, including those to the environment will be greater.	were overly conservative. The Working Group believes it has selected the appropriate level of conservatism given the uncertainty of certain parameters. The agencies do not believe it is appropriate to consider the cost of cleanup or the extent of environmental damage that could result from cleanup while performing a risk assessment. However, these factors will weigh heavily in the risk management decisions.
14	Bias is interjected when point estimates are used instead of all probability distributions. Reviewer thought the open space and office worker scenarios should have been done probabilistically too, and that a more complete explanation should have been given as to why this was not done.	We agree with the reviewer that a probabilistic assessment of the office worker and open space user would have made for a more complete report. The Working Group had four exposure scenarios, each having hundreds of parameters. It takes time and resources to develop distributions for each of those parameters in each scenario. The Working Group made a decision to focus not only on the parameters that were most influential but also on the exposure scenarios that would most influence the remedial decision. Given that the agencies don't believe either open space or office worker scenarios will play an important role in the decisions on action levels and cleanup levels, this additional work will not be undertaken.
15	Little or no attention was given to whether the contamination in soil is uniform enough (on a micro-scale) to be adequately described by a single concentration value. Reviewer supplied a graphic to support his point. Reviewer believes any impacts of non-uniform contamination in soil on sampling, on ingestion and on long-term risk calculations need to be addressed.	The premise of hot particles of plutonium metal is likely to be more of a concern in the case of weapons accidents or intentional dispersion of plutonium, e.g. safety shots. The contamination scenario at Rocky Flats is quite different. The Working Group did consider the distribution of the plutonium contamination in the soils at the Site. While the data are somewhat limited, and subject to interpretation, data collected near the 903 Pad, in air, indicate a plutonium activity distribution that is proportional to the mass of the airborne soil-derived particulate matter, for a number of different airborne particle sizes (several partitions were

		examined including particles from submicron to greater than 10 microns in size). These data suggest that the plutonium particles are attached to small soil particles, which in turn make up a soil matrix that becomes airborne as aggregated particles of different sizes. If, instead, the contamination were attached to solid soil particles of substantially different sizes, the airborne contaminant distribution would show a characteristic proportionality to the area of the airborne particle distribution, and the specific activity would decrease with increasing particle size. This was not observed. The Working Group appreciates the perspective brought to this issue by this reviewer. The Working Group did not consider the contaminant distribution for the purpose of understanding the dynamics of soil ingestion in the body. It was instead concerned about the relative distribution of contamination in fine soils subject to inhalation, compared to the contaminant distribution in a larger range of airborne soil-particle sizes subject to deposition on plants and the subsequent ingestion of this deposited contamination. The graphic provided by the reviewer would be somewhat modified in consideration of this new information, and would show reduced overall sensitivity to particle size, assuming the agglomerates would break up in the food preparation and digestive process in some predictable way. The Working Group recognizes, along with this reviewer, that the exposure calculated for ingestion is conservative. The relatively high amounts of contamination that are assumed to become airborne and subject to deposition, and the fractions assumed to remain with the plant material

		through the food preparation process suggest an overestimate of the ingestion dose and risk. Any reduction in exposure due to particle size/absorption interactions can only increase this conservatism.
16	Confusing presentation of uncertainty discussion in Section VI. Expand uncertainty discussion of proper absorption category (M or S) for dose conversion factors to show that the different agencies held different beliefs.	The Report will be revised to include additional information used in the decision to select type M, and its implications to uncertainty, as suggested. In essence, the differences between agencies centered around the degree of uncertainty in the chemical and physical form of the plutonium in the environment around Rocky Flats. DOE believes that there is high confidence that the plutonium in the environment is present as pure plutonium dioxide, for which the absorption type S is the appropriate choice. The other agencies did not hold such high confidence of complete oxidation of the plutonium released to the environment, and also admitted the possibility of additional confounding factors such as attachment to small soil particles, for which absorption from the lung to the blood may be influenced by the rate of dissolution of the soil matrix as well as the chemical form of the plutonium. ICRP Publication 71 provides the result of new studies, done since the publication of ICRP 30 which show greater variability in the absorption behavior of plutonium under environmental (as opposed to workplace) conditions, describes a number of chemical and physical complicating factors, and advocates the selection of type M, as a measure of prudence, in the absence of site specific information. Although there is limited site specific information at Rocky Flats which indicates that plutonium dioxide is present under the 903 Pad, the majority of members of the Working Group felt that there was uncertainty in the degree of oxidation across the entire site, and the presence or absence of other complicating factors, and that it was therefore prudent to select type M for use in

		dose and risk calculations in this Task.
17	Wrong number of significant digits expressed in the americium:plutonium activity ratio.	The activity ratio used in the draft report compares HPGe gamma measurements for Am (reported to one decimal place) to alpha spectroscopy results for Pu (reported to four decimal places). This ratio will be replaced with an activity ratio, which compares alpha spectroscopy results for both elements. The activity ratio used to re-calculate the sum-of-ratio values will be rounded to 0.17.
18	Decision to use 0.4 instead of 0.8 as a building shielding factor was a good one.	It is likely that the selection of 0.4 was overly conservative, given the low energy photons from americium and plutonium which are addressed in this calculation. The Technical Background Document for the Soil Screening Guidance for Radionuclides describes the decision to revisit the default GSF of 0.7 and change it to 0.4. Essentially this revision addresses the fact that earlier in-home measurement studies did not account for the fraction of exposure due to cosmic and building material sources. The revision appears to be based upon terrestrial and contaminant photons of intermediate energy, however, suggesting that it is conservative in the case of 60 keV photons. The Working Group's decision to use the new default was based on the TBD revision and also on the fact that external exposure contributes little to the overall dose/risk.
19	Decision that erosion potential quickly decreases after a fire is reasonable. The decision that drought could occur 20% of the time also is realistic	We appreciate this reviewer's comment. It was gratifying to the Working Group that the data from the wind tunnel experiments supported the intuitive observations of the individuals within the RSAL Working Group regarding the resuspension of contaminated soils from within vegetated cover of increasing density. The drought frequency was guided by site-specific data and the insight gained from literature provided by the National

		Drought Mitigation Center's website.
20	Discussion regarding the soil ingestion rate was too long, given the weaknesses in the data.	We agree that the discussion of the soil ingestion rate variable is long relative to that of other variables. However, the sensitivity analysis highlights this variable as being an important factor in the risk estimates. In addition, there is considerable discussion in the scientific community on the appropriate methodology for incorporating available study data into risk assessments for both children and adults.
21	NCRP Publication 129, "Recommended Screening Limits for Contaminated Surface Soil and Review of Factors Relevant to Site-specific Studies" should have been referenced and utilized in this Task.	The applicability of NCRP 129 to the computation of RSALs was considered early in the Working Group's process. Page 8 of NCRP 129 states that: "It is again emphasized that the guidance proposed in this Report is for use in screening and is not intended for use as cleanup criteria, since the conservative nature of the guidance given here could result in greater amounts of soil being removed than would be necessary with *realistic, site-specific calculations.*". Moreover, the comparison with EPA and NRC appears on page 8: "However, the limits proposed by NRC and EPA, *which are intended for cleanup* of contaminated sites are *based on the median dose to an individual in the most critically exposed population* rather than the *maximum dose to any individual* as used in this Report." (emphasis added). This statement leads one to expect that the NCRP screening levels, computed for generic sites will be much more conservative (and possibly much less realistic), than those computed using the EPA methodology. Owing to the fact that the computational basis of screening levels in NCRP 129 and in the Soil Screening Guidance used by EPA is different, and to the fact that NCRP 129 is not applicable to deriving cleanup levels, whereas the EPA SSG is routinely used in Superfund to derive preliminary

		remediation goals, the Working Group opted to exclude NCRP 129 from consideration.
22	Central tendency values for children were reasonable. However, the reviewer was "skeptical of how long the maximum consumption value (1000 mg/d) might actually be sustained by a child".	We agree with the reviewer that the selection of an upper truncation limit of 1000 mg/day is very high, and acknowledge that the intent is to be protective. As stated in Appendix A (p. 23), it corresponds with the 99.8th percentile of the lognormal distribution fit to the data presented as long-term average values. The choice of the truncation limit reflects professional judgment that weighs the confidence in the empirical data (i.e., medium), the skewness of the probability distribution (meaning the relationship between the standard deviation and mean, which in this case the CV = SD/mean = 2.4, which is high), and a rule of thumb to avoid overly truncating the distribution.
23	The soil ingestion rate for an adult does not seem reasonable.	The EPA default reasonable maximum exposure (RME) soil ingestion rate for adults was used because the workgroup was concerned with the adequacy of the existing database on adult soil ingestion. We agree with the reviewer that the use of this high-end value as an input to this influential parameter, will interject a conservative bias into the outcome. Now that an RME point estimate calculation is to be included in the report (per another reviewers comments), it would be beneficial to use a distribution for adult soil ingestion for comparative purposes.
24	Figure A-7 is off the page and useless, and the text on page 32 is continued to some unknown location.	This will be corrected in the final report.
25	Reviewer proposes a point-by-point comparison of RSALs computed in the Task 3 Report with screening levels computed for similar scenarios in NCRP 129, and suggests that there is good agreement between them.	Upon closer examination, it appears the reviewer has not selected the appropriate NCRP scenarios for comparison with the Task 3 scenarios. The scenario PV, as described in the key does not admit dwellings, but is instead a scenario for non-residential farm workers, and as such, does not

		compare with the rural resident scenario. It appears from the key that the scenario SU (suburban sites with gardens and children) more closely compares with the rural resident. Likewise, it appears that the NCRP scenario PS (for a sparsely vegetated, arid grazing land) more closely compares with the resident rancher scenario than the AG (which does not admit children and is a farm rather than a ranch). With the proper match for rural resident the agreement between NCRP and our effort is not as good: 32 pCi/g for the SU scenario and 209 for the rural resident. The agreement is also not as good in the case of the rancher either: 16 pCi/g for PS vs 45 pCi/g for the rancher. (It is worth noting that the RAC rancher scenario, presented in Appendix G, includes an entirely unrealistic value for mass loading, and if the same scenario were modeled using a mass loading distribution similar to what has been used in the Task 3 scenarios, that a value in excess of 100 pCi/g would be computed.) If the scenarios are properly matched, and the rancher scenario is adjusted for realistic mass loading, it becomes obvious that the NCRP 129 approach is much more conservative than that used by the Working Group, and that the caveat appearing on page 8 of NCRP 129 is well founded.
26	The recovery curves following a fire made sense to the reviewer..	While the recovery curves are based on a very limited data set, the results are consistent with other results in the literature with regard to the shape of the recovery curve, but seem to indicate a somewhat slower recovery than has been observed in other settings.
27	The discussion of the RESRAD Inhalation area factor was not clear	The Working Group will review the text, and attempt to

	to the reviewer.	clarify this relatively complex discussion. The area factor is a mathematical representation of the phenomena associated with the influence on dust loading from variable source areas. A smaller source area will contribute less airborne dust than a larger source. Coupled to this simple observation is the additional simple observation that a source area distant from a receptor (someone breathing the dust) has less influence than a nearby source area. An increasing area, while it contributes more, also carries the physical consequence that the additional emission contribution is further from the receptor. The two factors eventually reach a balance in which the increase in area is offset by the increased distance, to the extent that the amount of dust inhaled by the receptor does not measurably increase with the increase in area – in other words, the inhalation pathway becomes "saturated", not responding any more to changes in the source area.
28	In general, the reviewer thought that the values recommended in the child soil ingestion rate distribution are consistent with other analyses he has seen, and that "the ingestion rates have been adequately quantified for the intended purposes". The reviewer expressed some doubts as to whether 1000 mg/d could really be sustained by a child for any length of time	Same as Comment #22 above.
29	Confusing presentation of uncertainty discussion in Section VI. Reviewer was unclear as to why draft Task 3 identified possible sources of uncertainty if it wasn't going to be quantified. The reviewer indicates that a "2-dimensional analysis must be conducted whereby separation (between variability and uncertainty) is maintained.	Section VI will be expanded to include further discussion of how uncertainty relates to the choice of probability distributions for variability, how the sensitivity analysis plays a role in interpreting the importance of the sources of uncertainty, and collectively what the overall uncertainty is in risk and RSAL values based on a semi-quantitative ranking of the confidence in the values (or distributions) selected for input variables. Table VI-1 will be revised to reduce ambiguity in the descriptions of variability and

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		uncertainty. While we agree that a 2-D MCA can be informative for risk managers, it represents an additional complexity in the analysis that was beyond the scope of this assessment. Uncertainties were discussed qualitatively in this assessment.
30	The reviewer thinks that combining data from different studies which are weighted appropriately according to whether they used mass-balance or not would likely not result in a different distribution than that from the Anaconda study. Since this distribution is not inconsistent with that from the independent NCRP Report 129, 1999, the reviewer though the analysis done in draft Task 3 was appropriate.	We agree.
31	Page 6. Reviewer has never heard of the concept of pathways being considered complete.	The concept of complete pathways is described in EPA's 1989 Risk Assessment Guidance for Superfund, Part A on page 6-17. A pathway is complete if there is 1) a source or chemical release from the source, 2) an exposure point where contact can occur, and 3) an exposure route by which contact can occur.
32	Page 9. Reviewer has problem with the use of the term "conduit" to describe a pathway. He recommends using conventional jargon, not to change terms or invent new definitions.	The text will be revised to use conventional jargon.
33	Page 9. Reviewer has never heard of the term "active pathways". All pathways should be realistic. Use conventional radioecological definitions.	The concept of "active pathway" is not intended to represent some physical phenomenon. Instead, it is a way of denoting the state of the model in the way it will handle parameters associated with that pathway. An active pathway is that segment of the model's code that is used to estimate dose or risk from a certain physical pathway, an inactive pathway is one that is turned off in the model.
34	Page 9. Reviewer does not believe that the assumption that the "surrounding areas" of the residential site are uniformly contaminated is realistic, and cites several articles that indicate that Rocky Flats is not uniformly contaminated. Reviewer believes that	The Working Group has discussed the implications referred to in this comment. While it is true that the residential site could be located where the surrounding area is not uniformly contaminated, the scenario examined was a 5

	overly conservative bias is interjected into the analysis by oversimplifying the model in order to make calculations easier	acre plot sited in the most impacted point within a 300 acre area that would have been cleaned to the RSAL level and no more. This results, appropriately for the calculations, in a uniformly contaminated surround. For any resident other than this one, however, the Working Group agrees with the reviewer that the results would be overly conservative. It further agrees that the scenario may be conservative even for the most impacted resident since it is unlikely that the 300-acre area would be entirely contaminated to the RSAL level.
35	Page 18. As part of the water discussion, the Reviewer would like a discussion of how activity of particles can change with size.	The Working Group does not see the utility of the discussion suggested by this reviewer in the context of this document. Water is not considered a viable contributor to dose or risk in the scenarios examined by the Working Group. In addition, there is insufficient information available regarding the size distribution of the plutonium attached to the colloidal particles to provide more than academic interest to the discussion.
36	Page 19. The equation for the RSAL based on risk provides no units for the parameters. The multiplication signs in the risk equation show in the document as left-printing arrows. The paragraph above the risk equation uses the wrong terminology. First, there is no dose equation. Second, the Reviewer believes the word "activity" should have been used instead of "exposure". Similarly the word "exposure" was used instead of "intake" on p. 46, 2nd paragraph, last sentence.	Units will be added to the risk equation, font corrections will be made, and terminology will be corrected, as necessary. The dose equation will be inserted into the text. "Exposure" as we used it refers to external radiation and internal intakes and is consistent with the more general definition of exposure typically used in Superfund risk assessment.
37	Page 20. The uncertainty around EPA cancer slope factors and dose	We agree with the reviewer that the toxicity values may be

	conversion factors should be quantified, since these factors are the most uncertain parameters. The reviewer believes that using point estimates for these parameters "falsely expresses a belief in the values used as extremely high and that alternative values are unlikely".	a significant source of both variability and uncertainty in a risk assessment. At this time, however, EPA recommends that probabilistic distributions not be developed on a site-specific basis for human toxicity values. The qualitative uncertainty surrounding the use of dose conversion factors and cancer slope factors is discussed in Section VI of the report as well as the impact of their use on the results.
38	Page 29. The reviewer did not understand the material presented in Section IV-3. Specifically, the reviewer did not understand what was meant by a "saturated" pathway	See the response to this reviewer's comment #27 for an example of the concept of "saturation". Saturation (*of the modeled dose)* occurs in a calculation of pathway contribution when an additional increase (or decrease) in one of the variables driving the pathway contribution no longer results in a significant increase in the pathway contribution. The pathway becomes insensitive to additional increases (or decreases) in that variable.
39	Page 31. Reviewer wants qualification of the statement that inhalation rate is linearly related to dose and risk only when the particle size remains constant.	This is a very important conceptual comment. The statement carries with it the assumption that an increase in breathing rate will not change the particle size distribution of contaminated material being deposited in the lungs. It is possible that increased breathing rate will result in a change from nasal breathing to mouth breathing, with the consequent admittance into the oropharynx (region extending from the soft palate to the glottis, essentially the "throat") of larger contaminated particles than would be admitted through the nasal passages. These larger particles are not efficiently transmitted through the tracheobroncheal region (windpipe) into the pulmonary region (deep lung), and very few, if any, of the larger particles would be deposited in the lungs. Instead the particles would be deposited in the throat and ultimately ingested (swallowed), or expectorated (spit out), depending on the habit of the receptor and possibly the intensity of the dust exposure. It

		is likely that mouth breathing would result in a net increased dose, compared to nasal breathing, but through the digested fraction rather than the inhaled fraction. However, the models assume all particles are 1 μm size. This assumption consistently overestimates transport into the lungs and GI tract.
40	Page 34. Interjection of bias by the working group by refusing to assign distributions for variables with sparse data, and using, instead, point estimates. Reviewer believes uncertainty should always be quantified.	See response for comment #3 above.
41	Page 36 to 41. Quality of presentation. Tables IV-3 and IV-4 are needlessly confusing and sloppy. Pages that are continued do not have column headings. Way these tables are presented in the document is so that one has to read right to left. The parameters of each distribution are shown, but the definitions for them, i.e., min, max, etc. are not.	Text and tables will be edited to improve presentation. Definitions for parameter of probability distributions will be added.
42	Inadequate statement of purpose of the probabilistic analysis, up-front. Reviewer wants a quantitative uncertainty analysis. Confusing presentation of uncertainty discussion in Section VI.	Same as comment #2 above.
43	Page 53, first paragraph, last sentence. Interjection of bias by Working Group's refusal to assign distributions for variables with sparse data, and using point estimates instead.	We feel that the reviewer misinterpreted the text. On page 53 of the report we state "As general practice the RSAL working group tried to present data as accurately and factually as possible without interjecting bias. However, when data sets were sparse and highly uncertain, the working group defaulted to a conservative point estimate". We did not say that we were always accurate in our representations. We acknowledge that there are times when there is not enough information to accurately represent a variable. In those situations, we realize that we may be interjecting bias by defaulting to a conservative value, and

		we discuss this qualitatively in the uncertainty section (Section VI). The intent of Section VI is to inform the decision makers of the consequences of interjecting bias and uncertainty into the calculation.
44	Page 55 and 56. Confusing presentation of uncertainty discussion in Section VI. Reviewer wants a quantitative uncertainty analysis. Quality of presentation. Tables VI-1 through VI-5 read from right to left, with successive pages located to the right.	Section VI will be expanded to include further discussion of how uncertainty relates to the choice of probability distributions for variability, how the sensitivity analysis plays a role in interpreting the importance of the sources of uncertainty, and collectively what the overall uncertainty is in risk and RSAL values based on a semi-quantitative ranking of the confidence in the values (or distributions) selected for input variables. Table VI-1 will be revised to reduce ambiguity in the descriptions of variability and uncertainty. While we agree that a 2-D MCA can be informative for risk managers, it represents an additional complexity in the analysis that was beyond the scope of this assessment. Uncertainties were discussed qualitatively in this assessment.

	Review Comments – Melissa Anderson	Response
1	Page 8 It seems confusing to me to put volatilization from the soil in the Site Conceptual Model and then in a subsequent paragraph state that volatilization is not considered in this report because that is only an issue with uranium, and not plutonium or americium. Will that be addressed differently when uranium is added to the report? This issue arises with all of the Site Conceptual Models.	The Conceptual Site Models are designed to include both radionuclides and other contaminants. The models will be modified to distinguish between volatization and radon pathways. Radon ingrowth from site-contributed uranium isnot an issue due to the extremely long time required for ingrowth of significant amounts of the radium parent of radon. The radon inhalation pathway will be clarified and designated as insignificant on the Conceptual Site Models and the footnote removed.
2	Page 18, III-3, 1st paragraph The AME group now believes that americium in the environment at RFETS is due to its being released with plutonium, and not due to in-growth. Does this new information have any effect on the results?	The information provided in a public meeting with the AME advisors was not new information to the RSAL Working Group, nor does it have any influence on the results of the RSAL calculations. RSALs have been calculated individually for americium and plutonium, and those results combined through the sum-of-ratios method to provide the example calculation of an RSAL for weapons-grade plutonium. In areas where the ratio of plutonium-to-americium differs from the weapons-grade ratio, the sum-of-ratios method still applies to the calculation of an RSALs. (The sum-of-ratios method will use site-specific information on americium and plutonium concentrations to derive site-specific RSALs.)
3	Page 18, III-3, 3rd paragraph Just a comment that, as per Chris Dayton, the aseptic groundwater wells showed Pu contamination (albeit very low-level) and so the search continues for the source of contamination.	The comment is correct; work continues in the AME to more cleanly sample groundwater wells that have shown detectable amounts of plutonium in order to better understand the origin of that contamination. Notwithstanding the absence of results from those additional samples, there is no evidence of plutonium or americium-contaminated groundwater plumes at Rocky Flats. The site does have plumes contaminated with

		uranium.
4	Page 19 What are the units on the RAGS equation parameters?	Units will be added.
5	Page 23, Table IV-2 The value used for the Area of Contamination Zone is outside of the range of sensitivities tested. While the model is not very sensitive to this parameter, is it anticipated that the effect of this parameter on the final number will not differ at higher values from the effect at lower values?	The reviewer's anticipated answer is correct. The areas being modeled for Rocky Flats are large enough that all pathway contributions have reached their limiting values.
6	Table IV-2 Were parameter values labeled as "distributions" in the "Value Used" column within the sensitivity ranges tested? If not, I ask the same question I asked in 5.	The extremes of many of the distributions may be found to lie outside the sensitivity ranges tested, but the results of the sensitivity analysis are still valid when the majority of the distribution lies inside the range. It is important to remember the purpose of the sensitivity tests and the basis of the mathematical formulations that are being tested. The purpose of the sensitivity tests is to detect any nonlinear behavior that could portent a behavior in the model that would not be adequately represented in the choice of an input parameter, or interacting parameters. The mathematical formulations and their interactions, while understood, could yield overlooked consequences if not tested over a range of input variables that allows assessment of the model's response characteristics. The tests for sensitive parameters will reveal one or more of several behaviors - little or no sensitivity to changes in the input variable; a change in output that is more-or-less directly proportional to the change in input, and resulting in relatively large changes in output; or a change that exhibits strong non-linear response to changes in input. The latter will be identified as extremely sensitive responses, the more greatly influenced proportional case will typically be sensitive or moderately sensitive, and the small proportional response or non-response will be insensitive. The

		conclusion that the sensitivity analyses are valid for the distributions comes from the realization that these parameter responses are really well characterized in the model and have relatively simple interactions with other parameters. The Working Group will examine the discussion to ensure better clarity.
7	Table IV-2 The value used for the external gamma shielding factor was outside of the range of sensitivities tested. The model is moderately sensitive to this parameter. I ask the same question asked in 5.	This parameter is moderately sensitive and was hence important to examine in detail. Since the gamma shielding factor is a physical parameter, its characteristics can be readily predicted and adequately represented as a point value.
8	Page 45 How and why was the 96th percentile mass loading value used for calculations? Does this percentile take the fall fires into account, since they are above the 96th percentile?	The deterministic (point-estimate) RSAL calculation was performed using a mass loading value very close to the 95th percentile. The probabilistic RSAL calculations were performed using the entire distribution. The risk-based RSAL results exhibited in the Executive Summary and any future RSAL recommendation on a final RSAL selection will be based on the probabilistic calculations, and will by default be based on a distribution of mass loading that takes the fall fires into account.
9	Page 49 Another suggestion. Make it clearer within the text and title for the SOR table that the SOR table shows only an example of RSALs, based on a given location and that if the Pu:Am ratio changes, the RSALs will also change.	The titles of the SOR tables will be changed to include the word "example." The last sentence of Section V will be modified to read, "The approach for calculating sum-of-ratios is discussed in Section V-1 below, and example sum-of-ratio values are shown in Tables V–1 and V-2." The last paragraph of Section V-1 will be modified as follows: "Whenever a sum-of-ratios-adjusted action level is presented, it is important that the Am:Pu activity ratio used be specified. The Am:Pu activity ratio used to calculate the examples in Tables V-1 and V-2 is 0.17, which is the inverse of the 5.815 Pu:Am activity ratio reported in the 903 Pad characterization report (DOE, 2000)."
10	Page 50, V-2	The dose-based calculations are based on one year of

	What is the time frame for RSAL exposures? Are they to be protective over a 25-yr average, or for an annual average for 25 years?	maximum dose (which was year one). The time frame for the risk-based calculations were treated in a probabilistic manner (Rural Resident and Refuge Worker); each realization of the probabilistic analysis represents a hypothetical lifetime exposure. The parameter of exposure duration was input as follows: • Rural resident: 1 to 87 yrs with mean of 12.6 and standard deviation of 16.2 years; • Wildlife refuge worker: Distribution with mean value of 7.2 years with a standard deviation of 7 years; The analysis for Open Space User and Office Worker are point estimates of lifetime risk where the exposure durations are: • Open Space User: 30 years • Office Worker: 25 years
11	Page 51, last paragraph "Because RSAL calculations, for the most part, are the inverse of risk calculations, the reasonable maximum exposed range for RSALs corresponds to the 1st through 10th percentiles, with the 5th percentile as the recommended starting point." Are RSAL calculations the inverse of risk calculations? Is the intention to say that the 99th % RME risk corresponds to the 1st % RSAL?	The reviewer has the correct concept of the relationship between the risk distribution and the RSAL distribution. The footnote in Table V-3 indicates that the 10thto 1st percentile for RSAL range corresponds to the 90th to 99th percentile of the risk distribution, which is also referred to as the RME range.
12	12) Appendix B, page 4 What is the Area Correction Factor used in the RAGS equations for External Exposure? Is this the same parameter used in RESRAD? If so, I thought the RAGS equations didn't use that dilution factor.	The reviewer is correct; the RAGS equations do not use the Area Correction Factor (ACF). However, the newly revised equations for external exposure in EPA's Soil Screening Guidance for Radionuclides: User's Guide (2000) (SSG for Rads)(referenced in the text) were used instead. The ACF in the SSG for Rads corrects for a reduced gamma exposure in the case of small areas of contamination (hot spots). It would not have entered into the calculations for plutonium or americium in a significant way since the areas of

		contamination modeled (5 acres) are much larger than the hot spot areas where external exposure reaches its limiting value (a few hundred m^2). However, the ACF is significant in the case of the uranium calculations, since uranium contamination at Rocky Flats is present as small hot spots. The ACF in SSG for Rads is found by looking up the area and photon energy in a pre-calculated table. It appears that RESRAD, which uses a point kernel mathematical formula and calculates the ACF based on area and photon energy gives identical results to RAGS SSG for Rads for identical inputs, and the Working Group has assurance from the ORIA staff who authored the Soil Screening Guidance that the mathematical formulas in RAGS SSG for Rads and RESRAD are the same. We therefore propose to use the RESRAD formula to calculate ACFs for use with the Standard Risk equations for uranium isotopes and areas not appearing in the SSG for Rads table.

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	Review Comments – Robert Underwood	Response
1	Overall, the spreadsheet is crafted nicely and easy to follow. There are a few issues of style that I will discuss later.	No response needed.
2	The single largest concern is that of security. None of the cells in the spreadsheet are locked. It is very easy to modify cells unintentionally.	The working spreadsheets were used by only a few members of the workgroup, and calculations were cross-checked. Spreadsheets were distributed for others to use to understand and test various assumptions, but their results were not incorporated into the RSAL calculations. However, we agree that once the risk assessment is finalized, it would be necessary to develop a duplicate copy of all spreadsheets that has security features.
3	In my examination of the four spreadsheets, I uncovered only one spreadsheet whose equations were not consistent with Appendix B. The risk equation for inhalation for an Open Space User read in Appendix B as: $Risk_{inhalation} = PRG * IR_{a\ age} * ED * EF * ET * ML * CF_I * SF_{inh}$ where * indicates multiplication. In the actual spreadsheet, this computation is given as: $Risk_{inhalation} = PRG * IR_{a_age} * ED * EF * ET * ML * CF_I * [ET_0 + ET_i*DF_i]* SF_{inh}$ Where ET_0 = Exposure time fraction, outdoors, ET_i = Exposure time fraction, indoors, and DF_i = Dilution factor, indoor inhalation. This latter formula is analogous to the one used for the residential	The equation used in the spreadsheet is correct, since a variable for exposure time is needed in the Open Space User scenario. The equation in the text will be updated.

	scenario.	
4	There is also an error in labeling. The acronyms for "Inhalation rate, child" and "Inhalation rate, adult" in cells C14 and C15 are reversed. Both Am-241 and Pu-239 have their inhalation risk computed using this latter formula.	The labels on the spreadsheet will be modified as suggested by the reviewer.
5	One further comment is on style. It would preferable to have the adult data consistently placed before the child data in this spreadsheet. In light of this remark, interchanging rows 21 and 22 would be helpful. However, all of the formulas are correct and consistent with the current arrangement.	This apparent minor modification would require substantial effort to change all equations and re-perform a QA/QC check. We believe the minor change is not warranted.
6	In the process of examining the Open Space scenario, there appears to be a mistake in the variable definitions as follows: IR_{a_child} = inhalation rate for children, and IR_{a_adult} = inhalation rate for adults, should be ingestion rates. If not there is a further error in the spreadsheets regarding these variables.	The labels on the spreadsheet will be modified as suggested by the reviewer.
7	All of the slope factors for toxicity levels new and old inputs. The spreadsheets consistently reference only the new data. It is not clear to me why the other "old" data is entered at all but the references are consistent throughout all of the spreadsheets.	The database of slope factors has evolved during the course of the risk assessment. The spreadsheet simply documents the different values that have been considered.
8	The residential scenario spreadsheet: • Is ED the same as $ED_{_age}$ in cell C24? It appears that it is. • Why is cell E16 rounded from 8.71 to 8.7? • Why is 210/1445 in cell E20 rounded to .15? • Why is 1235/1445 in cell E21 rounded to .85? The equation for food risk in cells E60, E61, E69, and E70 are cumbersomely implemented. However, they are correct	a) Yes, ED and ED_age are the same. The subscript denotes variables for which an age-group weighted value is presented. b) Comment no longer relevant as point estimate has been changed to 8.3 based on data provided by Layton. c) and d) The values in cells E20 and E21 should sum to 1.0 to correspond with 1440 minutes per day. d) no change.
9	The wildlife refuge scenario spreadsheet:	This computation is used to scale the beta distribution to

	The origin of the computation (J14+(K14-J14)*F14) for the probabilistic risk in cells C41 and C42 are unclear. It appears as though the inhalation rate is computed using this formula.	reflect the inhalation rate data. The beta distribution is theoretically defined across the interval 0 to 1. A discussion of the scaling approach is given in Appendix A (pp. 47-49).
10	The office worker scenario spreadsheet: The point estimate and probabilistic data are identical. Why have the two separate schemes if only one is going to be used. In cells C6 through F6 and C7 through F7, the values are toggled between point estimates probabilistic using a value input in cell B52. If B52 = 1, the values recorded in theses cells will be based on point estimates. Otherwise they will be based on probabilistic estimates. In this spreadsheet there are no probabilistic estimates being used. However the value in B52 is set to 2, indicating that probabilistic estimates are requested. All in all this approach seems to be unnecessary.	Because of the time and resources required to develop distributions for multiple scenarios with hundreds of parameters, a decision was made by the workgroup to conduct probabilistic assessments only for the scenarios which would add the most value to the remedial decisions at Rocky Flats. These were the Rural Resident and Wildlife Refuge Worker. The open space user and office worker scenarios were done using a point estimate approach. Because we wanted to maintain consistency across the spreadsheets, the spreadsheets for Open Space User and Office Worker are set up to take distributions for entries, but only point estimates were entered and used. We agree that this may appear redundant, but we felt that the consistent approach would make review much easier for the layperson.

	Review Comments – Jerry Henderson	Response
1	Overall, the report is well-organized, surprisingly readable given the number of contributors to it, and contains key information necessary to understand the science supporting the risk assessment.	No response needed.
2	While not necessary for communication between professionals, page numbers within specific citations would help the layperson find information contained in some of the bigger documents. I had trouble, for instance, finding a statistic of interest in the Exposure Factors Handbook. As this reference is really a compendium of studies, it would be helpful to know exactly which study the statistic came from and on what page number it could be found.	The working group will add to the citations as appropriate.
3	Inconsistencies between the scenario description and the scenario parameters chosen can be extremely misleading. For instance, the refuge worker is not someone "assumed to work eight hours per day for five days per week and for 50 weeks per year." (p.7)	The working group will make appropriate revisions to the report.
4	Tables VI-1,2,3,4 should clearly distinguish between those areas where the Working Group has followed standard methods used by risk assessors to account for uncertainty (e.g. placement of the receptor on the contaminated area is a standard assumption in risk assessment) and where they have added an extra measure of conservatism (e.g. setting depth of contamination equal to depth of roots). This would better enable to risk managers to assess whether the risk estimates strike an appropriate balance between realism and conservatism	Section VI will be expanded to include further discussion of how uncertainty relates to the choice of probability distributions for variability, how the sensitivity analysis plays a role in interpreting the importance of the sources of uncertainty, and collectively what the overall uncertainty is in risk, dose, and RSAL values based on a semi-quantitative ranking of the confidence in the values (or distributions) selected for input variables. The tables in Section VI will be revised to more clearly distinguish between those parameters and assumptions where the Working Group followed standard methods used by risk assessors and where we have added an extra measure of conservatism.
5	Although the conclusion of Section VI makes a weak attempt to show that the risk assessment strikes a reasonable balance ["This conservatism is balanced ***somewhat*** by use of average ingestion	The conclusions in Section VI will be reviewed and rewritten, as necessary, in order to provide a clearer explanation of the sources of conservatism or realism in the

	rates… By doing this, it was ***hoped*** that a balance could be struck …" (p. 84, emphases mine)], the tables themselves (Tables VI-1,2,3,4) do not seem balanced, and run the hazard of giving the risk managers and DOE headquarters the impression that the risk assessment is unrealistically conservative. An example of this, I believe, is the exposure frequency for the rural resident (p.70). The distribution is based on data from the Exposure Factors handbook that show the average person spends 64% of their time at home. This choice, which the report calls "relatively conservative," is arguably quite realistic.	RSAL calculations.
6	As stated in the report, risk assessment guidance supports giving point estimates along with the probabilistic results. This could easily have been done, and perhaps should have been done, for the benefit of the risk managers, who need to know if the probabilistic calculations differ significantly from the point estimate approach, and if so, why.	We agree with the reviewer that point estimates should be provided along with the probabilistic estimates for perspective. These will be added to the report. When comparing the two, the point estimate value (which represents the RME individual) should be compared to the probabilistic RME range (e.g., the values between the 90th to 99th percentiles). If the point estimate is markedly different from the RME risk range, the reader should examine the inputs to the risk equations and seek to understand why they are different.
7	I believe the report should also do a better job of explaining the strengths and weaknesses of the risk assessment process used in the Task 3 Report. For instance, the risk managers should be aware that, while EPA guidance does not recommend modeling cancer slope factors as probability distributions, the point estimates used are central tendency estimates. The study "Assessing the Risks of Exposure to Plutonium from Inhalation and Ingestion" (Grogan, et al) speaks to the possibility that the cancer risk of exposures to plutonium may vary by orders of magnitude. Consequently, had this variability been reflected in the inputs for the cancer slope factors, there might have been a substantial effect on the RSAL.	See response to comment #37 above from Peer Reviewer #2.
8	P. 7, para 3: Refuge worker scenario description is misleading. "Refuge worker is assumed to spend 8 hr/day, 5 days/week, 50	We agree – the description is relevant only to the point estimate assessment. The text will be clarified.

	weeks/year" on site. This implies use of a point estimate, when in fact the exposure frequency parameter is being treated probabilistically, with an average of 225 days per year and a range of 200 to 250 days per year.	
9	P. 9, para 4: Rural resident scenario description is technically correct when it says resident spends "up to 350 days per year on site." More informative, however, would be to give the range (175 – 350) and the average value of 234 days per year.	We agree. The text will be clarified.
10	P. 43, last para: Change the word "RESRAD" to "RSAL."	This will be changed.
11	P. 53, 1st para: Report speaks to importance of assessing the strengths and weaknesses of information used in the modeling (e.g. parameter inputs), then says: "These strengths and weaknesses should be communicated to the risk decision makers for them to make health-protective remedial decisions." Now that the Working Group is no longer in a rush to finish the report, they should go through the report methodically to make sure they have achieved this goal in a balanced, accurate fashion.	Section VI will be expanded to include further discussion of how uncertainty relates to the choice of probability distributions for variability, how the sensitivity analysis plays a role in interpreting the importance of the sources of uncertainty, and collectively what the overall uncertainty is in risk and RSAL values based on a semi-quantitative ranking of the confidence in the values (or distributions) selected for input variables. Table VI-1 through Table VI-5 will be revised to reduce ambiguity in the descriptions of variability and uncertainty.
12	P. 55, para 5: Report states: "no attempt was made in this assessment to quantify uncertainty." Is this really true? Probability distributions were chosen for some scenario parameters, such as exposure frequency and duration. Page 56 states: "There is scenario uncertainty intrinsic in all of these choices."	The reviewer's comment reflects a common practice of loosely using the terms variability and uncertainty whenever a probability distribution is used. For probabilistic risk assessments, a more rigorous distinction is needed because the concepts are different (see descriptions in Section VI), and different approaches can be used to quantify each. One of the more confusing points is that our choice of distributions for variability are themselves a source of uncertainty. The text will be expanded to make this clear. The intent of the statement that no attempt was made to quantify uncertainty is to clearly tell the reader that the Monte Carlo analysis was restricted to exploring variability,

		not uncertainty.
13	P. 57, para 1: "In other cases, such as exposure duration for the rural resident, quite a lot of confidence can be placed in the distribution chosen." This distribution came from a recommendation made by EPA in the 1997 Exposure Factors Handbook. EPA assigned a confidence rating of high, medium or low to the various parameters recommended. Exposure duration received a medium confidence rating.	We agree, the text will be modified to read, "In other cases, such as exposure duration for the rural resident, greater confidence can be placed…"
14	Sect. VI, p57-83: A potentially important piece of information that appears to be missing from this section is whether the modeling choices made by the Working Group adhere to standard practice in risk assessment. This would enable the RFCA principals to ascertain where in the risk assessment the WG has added an extra measure of conservatism, and where they have simply followed accepted methods.	The general approach was to follow existing Agency guidance when possible. For many of the exposure variables, the Exposure Factors Handbook is a useful resource, but the workgroup did not restrict its evaluation of available information to the guidance. For example, a more recent analyses were considered in the development of inputs for inhalation rate (e.g., Allan and Richardson, 1998) and childhood soil ingestion rate (Stanek and Calabrese, 2000). A distinction should be made between the approaches used for developing point estimates and probability distributions. The following guidelines were followed: a) For variables described by point estimates in both the point estimate and probabilistic approaches, the same value was used b) Preference for point estimates was given to EPA's recommendations for reasonable maximum exposure (RME) If a probability distribution was used for the probabilistic approach, the corresponding point estimate was evaluated for consistency with the probability distribution (e.g., central tendency or 95th percentile).
15	P. 58, 59: Report fails to point out large uncertainties inherent in cancer slope factors. Slope factors themselves are central tendency estimates that may either over- or under- estimate risks.	See response to comment #37 above from Peer Reviewer #2.

16	P. 59, last entry Report fails to point out that for inhalation pathway, RESRAD also assumes dilution of contaminated dust from upwind fetch. The model assumption of wind constantly blowing means model is taking credit for constant dilution as well. Wind tunnel studies suggest that, while this assumption may be appropriate for point source emissions, it is an oversimplification in the case of fugitive dust emissions, such as occur with dispersed surface soil contamination.	This is an important comment to explore. The wind tunnel results are properly interpreted to indicate the contribution from wind events will be highly dependent on the direction of wind during the relatively short-lived events. The direction of the wind during the next high wind event will not necessarily be in the same direction and will not necessarily contribute to the same receptor direction. The consequence of this is that the estimated annual mass loading attributions in the post-fire years are overstated. Regarding the effect of dilution, fugitive emissions are the source contributing to these RSAL calculations, and depend on the wind for both emission strength and dilution. The use of the total emission rate to be overestimated under lesser wind conditions than the extreme used to calculate the erosion potential. The overestimated emissions would then be diluted in a typical dispersion model. RESRAD and the Standard Risk equations do not perform dispersion modeling, but rely instead on simpler area weighting to estimate the contribution of a limited source area to the estimated mass loading. Since the mass loading following a wildfire was estimated using a simple multiplication factor developed from the wind tunnel date, the implicit assumption is that the emission rate will be increased by the same amount at all wind speeds. Dilution effects are also assumed the same in both normal and burned areas.
17	P. 60, 4th entry INCORRECT. Fire is NOT assumed to occur every year on contaminated area, but only 10% of the time. Also, statement on burn frequency is confusing. Burn frequency of once every 10 years, or 10% is assumed. While this may be a conservative assumption, the probability of a wildfire on contaminated grassland at some point in the future is 100%. Conceiving fire as a prescribed burning regimen was done mainly for	The reviewer is correct; this entry needs to be corrected in the table. The reviewer is also correct as to the origin of the distribution; it is based on a predictable frequency. The consequence of a more conservative result is a secondary outcome.

	ease of computation, and the difficulty of estimating a burn frequency due to wildfire, not simply to add a margin of conservatism.	
18	P. 60, last entry: Not necessarily. I know a doctoral candidate at Colorado State University whose research has focused on Rocky Flats, and who asserts that the maximum Am dose occurs at Year 2038.	It would not be possible to know the exact year for the maximum effect of americium in-growth in the residual contamination at Rocky Flats. The americium present in the environment is mostly from contamination deposited around the 903 Pad, resulting from spills of weapons-grade plutonium cuttings generated in the '50's and early '60's. The actual age of the contamination is thus subject to an uncertainty of +/- ten years or so. It is important to recognize however that the in-growth of americium has proceeded for at least 40 years from that time, resulting in an in-growth that is more than 90 percent of its maximum value. Other areas on the site have americium in the soil that appears to be the result of direct contamination from americium source material. In these areas, the additional in-growth of americium from aged plutonium will be inconsequential. The RSAL triggers an action based on the level of contamination, both Pu and Am, found in the soil. A cleanup, once initiated, will result in levels of contamination less than the triggering concentrations. The consequence of in-growth on the residual risk, considering this process, is negligible.
19	P. 62, 1st entry: For the rural resident, whose 5 acre ranchette is much smaller than the contaminated area, the assumption that he/she spends the entire time on the contaminated area is realistic, not "very conservative" as characterized by the Working Group. The same assumption for the refuge worker, whose geographic range would likely extend over the entire 6500 acres, is very conservative.	The text will be revised. Also, see answer to Alexander Williams' comment 3.

20	P. 62, 3rd entry: For the adult soil ingestion parameter, on which almost no data exists, it is speculative to say the 100 mg/day point estimate is "relatively conservative." Better to call it a highly uncertain parameter.	The decision to use a point estimate for the adult soil ingestion rate variable has been reconsidered; a uniform distribution will be used. In addition, for each exposure variable discussed in Appendix A, an additional statement will be made about the level of confidence in the point estimate or distribution. This statement will follow a semi-quantitative ranking (i.e., low, medium, high) based on professional judgment.
21	P. 70, 2nd entry: When exposure time is viewed in conjunction with exposure frequency and outdoor time fraction, it is clear that the receptor being modeled is not homebound or an invalid. On days when the resident is home, he/she is indeed home 24 hours. However, since the distribution being used for exposure frequency has a mean of 234 days per year, the average receptor actually spends a great deal of time (a third of the year) away from home.	The reviewer's observations are correct. While 234 days/year is the central tendency of the distribution, the high end is 350 days per year (only 2 week away from home). These data are based on relatively large (n > 1000) surveys of time use patterns among U.S. adults.
22	P. 70: The 75th percentile values used for indoor/outdoor time fraction seem are neither average values, nor upper end values, but something in between. Is this what is meant by the term, "relatively conservative?"	Yes. This variable presented a challenge since the total of indoor and outdoor time fractions need to sum to 1.0. The use of the 75th percentiles was a professional judgment.
23	P. 70, last entry: The exposure frequency distribution is based on one statistic, the percentage of time the average American spends at home (64%). Multiplying by 365 days per year gives 234 days per year, which becomes the mean of the triangular distribution developed by the working group. The upper and lower truncation limits were chosen on the basis of professional judgment, with 350 days considered to be the maximum and the minimum arbitrarily chosen as half that. Use of a triangular distribution implies the parameter is poorly characterized. Is this the case for exposure frequency, or is better data available from which to develop a more accurate distribution? If there is better data, why didn't the working group use it?	The reviewer's assessment of the exposure frequency distribution is correct. The 234 days/year central tendency is the U.S. EPA default value, based on national survey data. The use of a triangular distribution reflects limitations in the available information – in this case, the original database was not obtained. The workgroup will pursue the availability of the database in order to develop a more refined distribution, if a sensitivity analysis suggests that use of alternate distribution types will have a substantial affect on the risk and RSAL estimates.
24	P. 72, 1st entry: Choice based on standard practice in risk assessment, not the possibility that contamination will be forgotten. While it does	The choice to locate the wildlife refuge worker on the 300 most contaminated acres on-site is not the professional

	likely result in over-estimate of risk, report should emphasize that to do otherwise the working group would have been deviating from the professional norm.	norm in CERCLA risk assessments. Rather, it is the limiting, most conservative possibility, necessary when calculating RSALs. Text will be revised for clarity. Also, see answer to Alexander Williams' comment #3.
25	P. 73, 1st entry: Report should emphasize that the point estimate of 100 mg/day is for agricultural workers, not just an average sedentary adult.	EPA's standard default RME soil and dust ingestion rate for adult residents is 100 mg/d. This value is thought to represent the upper-bound value for soil and dust ingestion, and is based on a limited study (n =6) by Calabrese, et al., 1989, 1990, as referenced in the report. This soil ingestion rate is recommended for use as an RME value for both residential and agricultural adults (EPA, 1991, referenced in the report).
26	Appendix A P. 31, bottom: "For this analysis, the ultimate goal is to use quantitative information on variability and uncertainty in exposure to help inform the risk management decision at Rocky Flats." Contradicts page 55, paragraph 5.	The sentence will be revised to delete the word "quantitative" to improve the accuracy of the sentence.
27	Appendix A P. 47, last para: Replace "simply" with "simplify".	Agreed.
28	Appendix A P. 54, 3rd para: "The following probability distribution is recommended for use in risk equations that are based on EPA RAGS guidance…" Misleading. The guidance recommends the equations, not the distribution. The working group chose the distribution based on information from a survey at Rocky Mountain Arsenal.	Agreed. The sentence will be revised for clarity.
29	Appendix A P. 56, 2nd entry: This receptor's residency period on site is divided between childhood and adulthood; hence, the exposure duration parameter involves an additional layer of complexity that is not	The reviewer is correct in his interpretation of the complexity in the approach. The Appendix will be revised to include this discussion.

	transparent in the report. If the exposure duration were a point value of 30 years, the parameter would be partitioned as 6 years of childhood followed by 24 years of adulthood. However, since this parameter is modeled as a distribution, it is not clear from the report alone how the breakdown between child and adult exposures is being handled. (Examination of the risk spreadsheet reveals that, for each monte carlo realization, the first six years of exposure is attributed to the child – which the working group claims is standard practice in risk assessment.)	
30	Appendix A P. 61, 2nd para: Once again, report implies this exposure frequency distribution for the rural resident is recommended by guidance, when in fact the working group chose it based on data published in the EPA Exposure Factors Handbook.	The particular section referenced by the reviewer pertains to the Wildlife Refuge Worker, not the Rural Resident. Nevertheless, the sentence will be revised for clarity.

	Review Comments – RFCAB	Response
1	Professional judgment is used as justification for many of the parameter choices in the report. However, the phrase "professional judgment" by itself is not particularly informative. To the degree possible, the working group should fully explain the rationale used to arrive at parameter selection.	Risk assessment is often based on professional judgment. Quantitative scientific data are used when available, but often there is a high level of uncertainty in these data. Interpretation and approach has to rely on professional judgment. "Professional judgment" should be taken to imply the use of scientifically gathered data, concurrent with the application of the risk assessor's experience in using such data within the professional guidelines established for performing risk assessments.
2	Incomplete citations make it difficult to independently verify some of the conclusions reached by the working group.	The working group will make appropriate changes.
3	The report doesn't do justice to the rigorous scientific debates that took place within the working group. In some cases, the rationale given in the report does not fully reflect the logical argument behind the parameter selection. A prime example of this is the indoor dust filtration factor, where the report fails to explain why a value at odds with EPA guidance was used.	It is difficult in a report of this length and complexity to provide detailed discussions of all the factors considered when selecting input parameters. Although the Working Group used guidance and precedent whenever available, it also exercised the option to improve on existing guidance to be consistent with site-specific or scenario-specific conditions. As the reviewer mentions, an example of this is the selection of values for the indoor dust filtration factor. The Group opted to use the default value of 0.4 (high protection), for the office worker, based upon the fact that windows would be closed year round, but modified to 0.7 for the resident based upon the assumption that the windows would be open for about half of the year. Citations are available from the literature to suggest the value could be as low as 0.3 in a closed home.
4	The report should explain parameter selection criteria and the process of how parameters were chosen.	Section IV-4 provides a general description of the process for development of probability distributions, including a flow chart depicting the conceptual approach. Appendix A provides detailed information for each variable including

		the data sets available, the strengths and weaknesses of each study, the data sets selected and how the data was fitted to develop the distribution used in this assessment.
5	Highly technical language in some sections of the report creates a barrier to understanding for members of the general public who may not have a scientific background.	The agencies recognize this problem. Significant effort has been made to provide a document understandable to the layman, but the sound development of exposure pathways requires the use of sophisticated scientific tools in many cases. In some of these cases, it was not possible to reduce the science to lay terms without loss of the necessary rigor in the analysis. The authors attempted to summarize such passages in simpler terms, when it appeared necessary. Nonetheless, prior to issuing the draft for public comment, the authors will attempt to identify unfamiliar terms, and replace them with language more accessible to the general public.
6	The report should make better use of diagramming and tables. Charts and tables should stand alone and make the point so that key information could be gleaned even without reading the entire text of the document.	The figures and charts are used for the purpose of illustrating discussions contained in the text. They will not be modified to stand alone.
7	Tables VI-1, VI-2, VI-3 and Vi-4, the main part of the section on uncertainty, could be improved through reorganization. A grouping according to source uncertainty would be helpful.	The tables in Section VI will be revised for clarity.
8	At the RFCAB modeling workshop, one of the presenters referred to a soil ingestion study just completed in the state of Washington by a researcher named Davis. Did the working group follow up to see whether any data from that study might be useful to the RSAL calculation in estimating this important parameter?	We followed up with Dr. Scott Davis on his soil ingestion study in children with pica. Because of lack of funding the study was never completed or published. We also followed up with Dr. Scott Bartell, whom a RFCAB member asked about at a meeting. As a graduate student at the University of Washington, he presented an abstract on back calculating soil ingestion rates from blood lead levels in children. He estimated a mean childhood soil ingestion rate of 10 mg/day and a 95th percentile of 93 mg/day when the negative mean estimates were included. If the negative estimates were

		excluded, the mean was 42 mg/day and the 95th percentile was 115 mg/day. This is consistent with the data from the Anaconda soil ingestion study that was used as the basis of the distribution for childhood soil ingestion.
9	The RSAL calculations for the rural resident and open space user scenarios do not take into account extreme soil ingestion behavior that has been observed in a small (but not negligible) percentage of children. If the goal of risk assessment is a realistic estimate of exposure, is it permissible to ignore this real phenomenon?	See response to Peer Reviewer #1, question #3(b.
10	The risk equations assume the office worker and open space user both ingest the majority of their daily soil intake while onsite. Is this assumption scientifically defensible?	The assumption that the office worker and open space user ingest most of their daily soil intake while on site is probably overly-conservative. Based on reviewers' comments and further review of the data, the Working Group will use an adult soil ingestion distribution in the final calculations. This distribution will reflect the range of soil ingestion an individual might experience. The related uncertainty will be discussed in Section VI.
11	Is it appropriate to use soil screening equations, which are simplistic and overly conservative and don't take into account ingrowth and decay of radionuclides, to derive an RSAL?	We feel that the use of the soil screening equations is defensible given the extensive peer review that they have undergone both within the EPA and within the scientific community. The equations do not take radioactive ingrowth or decay into consideration, which tends to make them somewhat conservative, since the initial conditions were selected so that maximum exposure occurs at time zero and decreases with time. This bias is discussed qualitatively in the uncertainty section of Section VI. Also see the response to Jerry Henderson comment # 18.
12	The exposure frequency distribution (number of days per year spent on site) for the rural resident is a triangular distribution based mainly on professional judgment. It has been said within the working group and elsewhere that use of a triangular distribution implies the parameter is not well characterized. Indeed, the only actual data point in the distribution developed by the working group is 234 days	The reviewer's assessment of the exposure frequency distribution is correct. The 234 days/year central tendency is the U.S. EPA default value, based on national survey data. The use of a triangular distribution reflects limitations in the available information – in this case, the original database was not obtained. The workgroup will pursue the

	per year, taken from a survey of the amount of time the average American spends at home each year. Is more information available on this parameter? If so, how does the 95th percentile of the working group's distribution (318 days per year) correspond with actual survey data?	availability of the database in order to develop a more refined distribution, if a sensitivity analysis suggests that use of alternate distribution types will have a substantial affect on the risk and RSAL estimates.

	Review Comments – W. Alexander Williams	Response
1	In general, this is a good report, clearly written with a thorough and thoughtful process. The authors have done a very good job. This analysis is one of the most comprehensive and complete ever sent to headquarters.	None needed.
2	There is much discussion throughout the document about the CERCLA risk range; specifically, how the risk range goes from 10^{-4} to 10^{-6}. However, EPA officials have repeatedly stated that the risk range extends to 3×10^{-4}. In addition, OSWER No. 9200.4-18 states, "Guidance that provides for cleanups outside the risk range (in general, cleanup levels exceeding 15 millirem per year which equates to approximately 3×10^{-4} increased lifetime risk) is similarly not protective under CERCLA and generally should not be used to establish cleanup levels." Consequently, for this set of risk calculations, it appears that the upper value for the risk range should be 3×10^{-4} rather than 1×10^{-4}. The calculations in this report, as summarized in the table in the Executive Summary on page 1, clearly demonstrate that an annual 25 millirem cleanup level can be within the CERCLA risk range when the risk range is extended (per EPA policy) to 3×10^{-4}. The risk range can be extended to 3×10^{-4} by multiplying the entries at the risk level of 10^{-4} by 3 and comparing the product to the 25-mrem annual dose column. For the cases in which there were probabilistic calculations, the 25 millirem per year entry is within the CERCLA risk range. For the deterministic calculations, the 25 millirem is not within the CERCLA risk range, however, the 25 millirem limit is subject to ALARA. There are two points to this comment. First, if the goal of the analysis is to show the range of cleanup alternatives that can be considered, the risk range calculations should be extended to 3×10^{-4}. This will provide a more comprehensive range under which CERCLA modifying factors can be considered or in the cases of AEA-based standards, define the limit for the ALARA process to consider. Second, the document	The agencies are aware that EPA policy considers values close to 1×10^{-4}, such as 3×10^{-4} as essentially equal to 1×10^{-4}. However, the policy document in question should not be interpreted to mean that 3×10^{-4} is the new, de facto cleanup level for radiologically contaminated sites. The National Contingency Plan, the implementing guidance for the Superfund Law, states that remedial action is generally warranted when risk levels exceed 10^{-4}, and when action is warranted, cleanup to 1×10^{-6} should be the point of departure in the planning of the cleanup. Also, the time spent on site is input as a distribution of values to account for individuals who work offsite as well as stay-at-home fathers/mothers and shut-ins.

	will better show that the CERCLA process using its risk-range constraints and modifying factors results in cleanup options essentially equivalent to the AEA-based 25 mrem/year plus ALARA process (*i.e.*, the process being implemented at Rocky Flats will satisfy all applicable or relevant requirements). The document clearly shows to the perceptive reader that the two processes are very compatible and it would be valuable to make that clearer for those that might not notice.	
3	The wildlife worker scenario is overly conservative. Only 300-400 acres of Rocky Flats has significant levels of residual radioactivity. Given the site area of thousands of acres, it is incorrect to assume that a wildlife refuge worker was employed full-time on a small portion of a much larger parcel. It is recommended that a more realistic assessment of outdoor occupancy be provided. Clearly, given the ratio of lands that contain residual radioactivity to those that do not, it is very conservative to assume all of the workers outdoor time is spent in the areas containing residual radioactivity. If it is not possible to get a better estimate of remote to office-based activities for the workers, the conservative assumption should be clearly stated in the Wildlife Refuge Worker section (III-1)a).	We believe the assumption that the wildlife refuge worker would spend all of their work time on the most contaminated 300-400 acres is conservative, but plausible, given CDPHE's estimate of an appropriate size exposure unit for this receptor. Using the data on specific tasks done by wildlife refuge workers from the survey performed as part of the risk assessments for the Rocky Mountain Arsenal, the time-weighted average size exposure unit calculated for either "all wildlife refuge workers combined" or for "only those who spent at least 50% of their time on-site outside" is 450-460 acres. In addition, a significant proportion of the wildlife refuge workers in that survey reported spending no time/year in tasks that would typically be done on large areas (500-6000 acres). If only this latter group of workers is evaluated, the time-weighted average exposure unit size is approximately 130 acres. Therefore, evaluating exposure to a wildlife refuge worker on an area the size of that which contains the highest concentrations of plutonium and americium on-site (down to the 10 pCi/g contour east of the 903 Pad) does not seem unreasonably conservative. Rather, for the purposes of calculating a range of plausible RSALs, it could be considered the limiting condition for an average wildlife refuge worker.
4	It would also be useful, for clarity, in the first paragraph of this	We agree. The text will be amended.

	section, last sentence, to insert after "...scenario represents.." something that says this worker is the critical group or maximumly exposed individual under this use (e.g., "...scenario represents the maximumly exposed individual under the most likely future use of Rocky Flats."). The reason is many will note that there are likely to be others on the site (even though the most effected of those others are the campers and hikers who are addressed in a separate analysis) and this statement clarifies that the worker has the highest risk or dose.	
5	The other scenarios discussed lifetime exposure assumptions (up to 40 y for rural resident and 25 y for office worker) but for some reason this section does not specifically state a time period. This is not critical as later in the table on page 16 it is listed.	This information will be added to the text of the report for the wildlife refuge worker.
6	There is an assumption that the fires burn off vegetation which, in turn, leads to higher airborne particulates and a higher radiation dose. Fires of a sufficient severity to denude the site of vegetation would likely damage or destroy structures. How is it that the assumed fires do not burn houses or crops? This consideration should be acknowledged in the report.	This is a very good comment for discussion. It is very likely that a large, heavily fueled, wind-sustained wildfire would consume everything in its path to some level of severity. On the other hand, the Working Group could not ignore the ready possibility that a less intense wildfire could just as easily burn to the edge of an irrigated garden area and not destroy a significant portion of the crop. It is important to recognize that the grassland fires experienced at Rocky Flats have not been high intensity fires, but they have on occasion consumed a reasonably large area before being brought under control. The report will be modified to acknowledge that fires could damage structures and gardens to the extent that they could become uninhabitable or unavailable for some period of time following the fire.
7	The relatively high level of irrigation (assumed to be 1 meter per year) is necessary to grow the hypothetical plant foods. But fire severity and frequency would likely be much lower in cultivated, irrigated land than in open prairie. This circumstance should be discussed. In addition, the high assumed rate of irrigation would greatly increase plant recovery after a fire. The report should	Again, the reviewer is correct on both points if the assumption is made that the entire landscape surrounding the garden is irrigated to the same extent. The scenario, however, does not assume grass cover on the areas occupied by the ranchettes, suggesting that native vegetation would be present instead. Whether this is a realistic assumption

	acknowledge this consideration.	could easily be argued but it is plausible. This point will be discussed in the report.
8	The assumptions involving hypothetical fires are contradictory since it is assumed that the fires consume vegetation, yet plant foods grown on-site are eaten as food. Consequently, it is recommended that the "prairie fire" scenario for the rural resident be revised by comparing the radiation doses from the plant food ingestion pathway and the inhalation pathway. If the dose from the inhalation pathway is larger under assumed fire conditions, then the plant food pathway should be ignored; alternatively, if the plant food pathway is larger under assumed fire conditions, then the incremental inhalation exposure from the hypothetical fire should be ignored. However, it is a gross overestimate to assume both the consumption of all vegetation by a fire and consumption of plant foods grown on-site.	This comment expresses the same tenets as comment #6 above. The response is the same; under a catastrophic wildfire scenario, the resident would have neither domicile nor garden, however the severity of grassland wildfires is not typically that extreme, at least not as observed at Rocky Flats. The Working Group will acknowledge the possibility of a devastating fire, but will not modify the pathway analysis that was performed for this scenario.
9	The "rural resident" land use has some other assumption that overestimates dose and risk. The very act of building a home and garden tends to dilute and disperse radioactivity through land use activities, such as excavation, construction of foundations, installation of water, sewer, and septic systems, plowing, clearing of land, establishment of roads and the like. Most of the residual plutonium is in the top 2-3 inches of soil, and these activities would tend to mix the soil in a more homogenous manner. The assumed mixing zone thickness (.15 meters) of soil for inhalation and soil ingestion purposes is appropriate for some of these activities, but not for all. In short, the very act of constructing a house and garden would lead to a further reduction of the concentration of any residual radioactivity and thereby reduce dose. This consideration should be discussed in the report.	The choice of a 5-acre owner occupied site, as opposed to a subdivision lot (which would be heavily developed) was based upon a presumption of little or no developmental dilution. This is, of course a conservative assumption, since some development is likely to occur, but it is intended to address the uncertainty in how much development might occur in a prudent way. No credit was taken for dilution of surface contamination through land development activities of the rural resident (building, digging, plowing, etc.), a conservative assumption. The tables in Section VI will be revised to include these kinds of considerations, as necessary. The reader is also referred to the response to LeRoy Moore's comment #14 for further discussion of construction-related dilution.
10	The installation of roads would decrease airborne radioactivity and also decrease the effects of a fire. The decreased effects from a fire would come from the road being a firebreak, from the pavement	The reviewer is correct in his assumption of reduced airborne radioactivity from areas that are paved or improved. With sufficient density of roadways, the

	preventing radioactivity from becoming airborne before or after a fire, and from the road facilitating fire fighting efforts.	probability of a significantly sized fire would also be diminished, and accessibility for conducting fire-fighting activities would be improved far greater than exists now. The extent to which this modifies the rural resident scenario is not easily quantifiable, but does illuminate the conservative nature of the calculations, as related to the 90th and higher percentile mass loading estimates.
11	The assumption that residents could remain on site for as much as 24 hours a day for 350 days a year for 40 years is a clear overestimate. It is much more likely that adult residents would have some form of outside employment, and this employment would lead to residents being off-site, perhaps 45 hours per week. The income from outside employment would be needed to pay for utilities (irrigation, water, sewer, telephone, power, gas, etc.), property taxes, off-site foodstuffs (meat, milk, grains, etc.), and other cash expenses. It is also likely that children would attend school, in keeping with public policy. The notion that site residents would remain on site for 40 years without leaving is not plausible. While site occupancy was handled as a probabilistic variable, even the possibility of near full time occupancy is very dubious.	For the point estimate risk assessment, the choice of 350 days/year reflects the Agency's policy for characterizing the reasonable maximum exposed individual. For the probabilistic risk assessment, the 350 days/year value is assumed to be representative of the maximum of a triangular distribution in which the most likely value is 234 days/ year, and the minimum value is 175 days/year (50% of time away from home). The exposure duration estimates are based on national surveys of population mobility. The choice of 30 years reflects the Agency's policy for characterizing the reasonable maximum exposed individual, and corresponds to approximately the 90th – 95th percentile of the distribution.
12	In short, the rural resident land use has a series of unlikely assumptions: ● All land use controls are lost; ● The Federal, State, and municipal governments do not intervene; ● Farms are constructed with a size of 5 acres; ● Construction for homes and roads do not affect the residual radioactivity despite the excavation and grading for roads, utility pipes, and buildings; ● These farms produce sufficient income to pay taxes and utility costs;	The agencies recognize that the rural residential scenario that was used for these calculations is conservative. The agencies do not consider this scenario to be a farm that would sell crops for income. Rather, this is a 5-acre residence with a large garden for home produce. There are several examples of residences of similar size in the vicinity of Rocky Flats and in other portions of the Denver Metro Area. Potential grassfires would not be envisioned to be of a magnitude to damage structures.

	• The farm residents do not necessarily have outside employment; • Children spend most of their time on-site and may not attend school off-site; • Irrigation is adequate for growing vegetables, which are part of the resident's diet; • Fires occasionally affect the farm, notwithstanding the irrigation levels; • The municipal fire departments do not exist or (alternatively) are unable to fight the fire; • Farm roads and streets do not act as firebreaks or otherwise facilitate firefighting; • After a fire, airborne dust is elevated; • Irrigation does not affect the regrowth of vegetation; and • Despite the fires consuming vegetation, structures and homes are not affected. Taken as a whole, these assumptions are quite unlikely.	
13	The office worker scenario assumes that a fire would burn all vegetation but not damage or destroy the building. While reasonable land management would be expected around an office building and this management would likely control an area a few acres around the buildings to landscape the building, construct parking lots, minimize fire hazards and ameliorate post-fire impacts. But these same land management steps would reduce airborne radioactivity from non-fire situations. In short, the assumption that a fire would burn the vegetation without destroying buildings is a dubious assumption. But the assumption that buildings are protected without a reduction in airborne dust from the office land use is equally dubious.	The Working Group did not spend as much time developing the scenarios for the office worker or open-space user as it did for the wildlife refuge worker and rural resident. Neither of the former scenarios was considered a reasonable land use scenario in light of then pending, now final, legislation That said, the reviewer is correct, the consequences of a fire near an office building will not be severe. If one assumes minimal land improvement around the office building as is frequently practiced in many industrial parks in this area, it is not difficult to envision prairie landscape very close to the building. In that scenario, the mass loading following a prairie fire could be reasonably well described by the same mass loading as is used in the wildlife refuge worker

		scenario. The location of the office worker would not necessarily be one where the worker is immersed in the maximally impacted region of the airborne plume, however. Adding to this, in an energy-conservative environment, the amount of air that is actually admitted to the building and its impaired quality are likely overstated in the calculation. It is clear that the calculated RSAL for the office worker is more conservative than even those calculated for the rural resident and wildlife refuge worker.
14	The office worker scenario does not examine consider the maintenance or landscaping of the office building. However, the scope and duties for a building maintenance job are similar to those of wildlife worker. Consequently, the likely impacts to an office maintenance employee have already been considered, albeit under a different scenario. This section should discuss those employees (under this scenario) that may spend time out of doors and specifically state they are considered under the other scenario or quantitatively or at least qualitatively discuss the difference from the "office worker."	The Working Group agrees that the office worker definition excludes the maintenance worker who might spend much of the time outdoors in a setting similar to that for the wildlife refuge worker. The text will be modified to ensure that this issue is captured.
15	It might be argued that a wildlife worker worked all over the site, while an office maintenance worker worked only in close proximity to the buildings for which he or she is responsible. However, the amount of excavation required to build an office building and parking lot would significantly reduce the soil concentration of any residual radioactivity through soil mixing. Thus, construction activity would tend to offset the possibility that an office building was located in an area with elevated plutonium concentrations.	See response to Alexander Williams comment # 9 concerning dilution of contamination due to excavation and soil mixing.
16	Comparability to Other Cleanups: These RSAL calculations show cleanup criteria with dose and risk that are much lower than the dose and risk from cleanups of sites involving radium. At these sites, a cleanup criterion of 5 pCi/g is typically used; the sites at which this criterion have been used include Montclair (NJ), Landsdowne (PA), Radium Chemical (NY), Denver Radium (CO) and numerous	The agencies focused on site-specific conditions and potential future uses that were specific to Rocky Flats. Assessments conducted at other sites had their own specific approaches. Task 5 of the RSAL process addresses cleanup levels at other sites.

	uranium mill tailings sites. Consequently, why should the dose and risk after cleanup at Rocky Flats be lower for any particular scenario than at sites that are planned for free release. After all, at Rocky Flats the most likely future land use is a wildlife refuge, and residential use is likely at many of these other sites. It is recommended that this comment be addressed by inserting 5 pCi/g of radium-226 into the parameter sets for the computer codes and examining the dose or risk of the output.	The cleanup level of 5 pCi/g Ra-226 is a contaminant-specific ARAR that was developed to guide the decontamination of properties under the UMTRA program. The agencies do not believe this level was intended to be used as a benchmark risk level that would guide the cleanup at all radiologically contaminated sites.
17	Authors: On the cover sheet, the names of the authors and their affiliations should be shown. Similarly, the names of reviewers (both technical reviewers and reviewers within the management of the various organizations) should be listed separately, perhaps in an acknowledgment section.	Since this report is a product of multiple agencies and many contributors, the working group and the agencies feel that it is most appropriate to list only the names of the agencies on the report. As for the reviewers, some of the reviewers' names are listed with their comments. Other reviewers, however, are anonymous and their names cannot be included.
18	RESRAD Version: On page 1, mention is made that RESRAD version 6.0 was used for calculations. Was this version used by mutual agreement of the different organizations? The current version of RESRAD available from Argonne National Laboratory is Version 6.1. It may be that an agreement was reached to freeze the RESRAD version because of the length of time required for the calculations and to avoid rework simply because a new RESRAD version became available. If there was such a "freeze" agreement, it should be mentioned.	When the agencies started the process of revisiting the RSALS, RESRAD 6.0 was the latest version available. RESRAD 6.1 has only minor changes relative to 6.0. The text will be modified to reflect that the version of RESRAD was "frozen" during the process.
19	Dose Factors: Dose conversion factors are discussed frequently within the document. This document uses "updated dose conversion factors" from ICRP report 60 and later dosimetry. The problem with this usage is that DOE, NRC, EPA, and the State of Colorado all <u>officially</u> use EPA Federal Guidance Reports 11 and 12 for dosimetry, and these documents are based on ICRP reports 26 and 30. For example, the NRC "Decommissioning Rule" specifies an annual dose limit of 25 millirem effective dose equivalent; the term	Comment 19 indicates that there is no regulatory precedent for use of the dose factors from ICRP 60-72. However, the agencies believe that there are several advantages to using ICRP 60-72 dose factors: ▪ ICRP 60-72 embodies improved science (more precise biokinetic models of the respiratory system and more accurate apportionment of dose to the gastrointestinal tract). This has the effect of reducing uncertainty.

	"effective dose equivalent" is a term defined in ICRP 26 and 30, but not in ICRP 60 and later reports. Dosimetry from ICRP 26 and 30 are heavily incorporated into a host of EPA, NRC, and DOE requirements, including (but not limited to) 40 CFR 191, 40 CFR 192, 40 CFR 61, 10 CFR 20, 10 CFR 835, and DOE 5400.5. All of these regulations specify or imply the use of organ weighting factors and other details, which are exclusively used in ICRP 26 and 30 dosimetry. The usage of dose factors other than those specified in these regulations raised a host of issues as to whether the requirements are, in fact, being complied with. Further, the "updated dose conversion factors" have not been officially approved by EPA, since EPA has not withdrawn Federal Guidance Reports 11 and 12. Consequently, the use of ICRP 60+ dosimetry without clear-cut official approval is problematical, and there is a serious policy question about the development and use of dose factors at individual sites (DOE, NRC, EPA) in an *ad hoc* manner	▪ The biokinetic models and human and animal database used in the development of ICRP 60-72 are the same as those used in the development of the risk coefficients in Federal Guidance Report 13/HEAST. Use of ICRP 72 dose factors assures consistency with use of the latest HEAST risk factors, whereas use of ICRP 30 dose factors does not. ▪ ICRP 72 dose factors were specifically developed to be applied to members of the public exposed to environmental contaminants, as opposed to workers exposed under more carefully controlled conditions (all previous ICFP dose coefficients were developed for application to workers). ▪ It is likely that quantitative estimates of uncertainty will be computed for the biokinetic models and human and animal data used in ICRP 72 computations (ORIA is tasked with developing quantitative estimates of uncertainty for the FGR 13 risk coefficients). With respect to the regulatory issues: ▪ The dose based RSAL is not a regulatory cleanup level, although it may be used to influence the development of a cleanup level. ▪ It is highly likely that the risk based RSALs (using FGR 13 risk coefficients) will be the selected RSALs. The DOE site annual compliance report and the derivation of RSALs will be kept separate. Of course, all site compliance calculations of dose will continue to be

		performed using ICRP 26/30 methodology, as required by DOE Orders.
20	Presentation of Results: The authors do an excellent job of factually presenting rationale, assumptions, parameters, calculations and sensitivity analyses in a scientific manner. In doing so, they have developed a very credible report. However, they should also take as much care in presenting the results. Clearly, these analyses and the results are probably only good to one significant digit at best. The results provide for example in Tables VI-1, V-2, V-3,V-4 and V-7, as well as in the Executive Summary, should have only one but certainly no more than 2 significant digits. More than 2 significant digits portrays a precision that greatly exceeds the knowledge base. If for some reason, it is felt necessary to maintain the digits for calculation accuracy, at least place a footnote on each table indicating that the "analyses only justify one significant digit but are presented as calculated because..." This should also be discussed in Section VI.	Calculated results will be rounded to 2 digits and the tables will include the following footnote: "Analyses only justify one significant digit, but values are presented with two digits for comparison purposes." Two significant figures will help compare and distinguish values for different radionuclides that were calculated using input parameters that have the same amount of inherent precision. The amount of significant figures can be considered when risk management decisions are made to select final action levels.
21	Sensitivity Analysis: In Section IV, it is surprising that the sensitivity analysis feature of RESRAD was not used for this work.	As stated in the first sentence of Section IV-1, Sensitivity Analysis Process, it was.
22	Page 3, third bullet: the EPA rule was never formally proposed or promulgated. In fact, EPA withdrew the draft rule from review at the Office of Management and Budget prior to its publication as a proposed rule in the Federal Register.	The Working Group concurs with the reviewer, the text will be modified.
23	Page 3, last complete sentence at the bottom of the page: This sentence should be reworded to read as follows: "Earlier versions of RESRAD were used by the agencies in 1996; later, the Risk Assessment Corporation modified RESRAD for its own use."	No change will be made to this sentence. It is a true statement that the Risk Assessment Corporation (RAC) "used" RESRAD. It is beyond the scope of this report to explain how RAC used RESRAD or whether it was modified by RAC.
24	Page 4, Second bullet: In the last sentence of the bullet, there is a discussion that EPA guidance requires consideration of the maximally exposed individual. Both NRC and DOE also require this consideration within their respective regulatory frameworks.	The text will be modified.

25	Page 7, last sentence in the first paragraph: change the last part of the sentence to read: "the wildlife refuge worker scenario represents the maximally exposed individual from the most likely future use of Rocky Flats."	The text will be modified.
26	Page 7, second paragraph: The assumption that residual radioactivity is present at the entire site at the RSAL level badly overestimates the radiation exposure of workers, since most of the site has little or no plutonium.	See response to Alexander Williams comment # 3.
27	Page 7, second and third paragraphs: It is likely that the number of wildlife workers at Rocky Flats would be small, and the small number of workers would prohibit an on-site childcare facility because of economic considerations. Specifically, there would not be enough workers to make a childcare facility economically viable	As stated in the text, a childcare facility at the refuge is not considered.
28	Page 9, first paragraph in section b: There is a discussion of periodic wildfires, which would "burn off accumulated vegetation." How do the fires burn off the vegetation without burning off the homes and crops?	Most grass fires that impact the Front Range of Colorado are not of a magnitude that would threaten structures. The working group considered a plausible outcome from a grass fire to be a burned contaminated area immediately adjacent to an irrigated garden plot whose growth would be little affected by the aftermath of the fire yet be subject to wind-blown dust from the burned area.
29	Page 11, second paragraph for the Open Space User Scenario: There is a brief discussion of increases in airborne particulates following fires. It should be noted that, after a fire, visits might increase from curiosity seekers but decrease over the longer term because of the adverse smell.	Based on experiences with grass fires that occur at Rocky Flats, the extent of the burned land is relatively small, the land recovers from the effects of a fire relatively rapidly. Any residual odor also diminishes rapidly.
30	Page 17: In the second to last sentence in the "Direct Dermal Absorption Contact Pathway," mention should be made of the current usage of municipal water systems in the area.. A similar comment should be inserted in the last sentence of the second paragraph in the section entitled, "Ingestion of Surface Water, Ground Water, and Food."	Most residences east and southeast of Rocky Flats rely on municipal water, but there are homes in the vicinity of Rocky Flats that are more widely spaced and get water from private wells.
31	Page 18: In the first paragraph of the section entitled "Solubility of Plutonium and Americium," the discussion of RESRAD in the fourth	The reviewer is correct in stating that the section needs to be rewritten to better reflect the intent of the statement. It

	sentence is in error. This sentence states: "The RESRAD groundwater transport calculations treat plutonium and americium separately, and do not adequately represent the behavior of weapons-grade material containing both." RESRAD uses distribution coefficients (Kd) to describe the partitioning of radionuclides in solution. The user specifies the distribution coefficients by inputting them or using default values. Alternatively, the user can specify solubility limits to describe the behavior of aqueous radionuclides, and RESRAD will calculate a Kd using the specified solubility limit. The problem mentioned here arises when the wrong Kd is input by a user. If the dissolution of Americium is similar to that of plutonium, they would have the same Kd. This paragraph needs to be rewritten to indicate that the behavior of americium is atypical because of its association with plutonium in many on-site areas. However, there were separations of americium from plutonium at Rocky Flats, and there is a potential for americium to be present without an association with plutonium. But since most of the americium in soil (including the 903-B pad) is associated with plutonium, it is correct to use similar Kds for both elements. A clarification of this topic should be made in the report, and references to Kd or other geochemical measurements should be inserted	was not intended to imply that there was a limitation in RESRAD that restricted the proper use of the distribution coefficients. Instead, it was intended to illustrate exactly what the reviewer states – that the americium is associated with the plutonium in much of the contamination, and needs to be treated as plutonium when considering its behavior in water.
32	Page 19: Just above section IV-1, there is a statement, "EPA policy recommends against developing site-specific probability distributions for human health toxicity values...." All Federal agencies have long used the linear, non-threshold approach to radiation effects on the assumption that the assumption prudently and conservatively addresses the possible effects of radiation at low doses. This usage has been made in the full knowledge that this theory probably overestimates health effects. Consequently, the slope and dose conversion factors used in this study probably overestimate effects, as well.	See response to comment #37 from peer reviewer #2.
33	Page 28: There is considerable discussion about dose conversion	The agencies agree with the comment. Even though there

	factors and their usage, as well as the selection of dosimetry from ICRP 60 and later publications. The more recent dosimetry has not been accepted by Federal or State agencies for general use, although their use has been approved on a case-by-case basis in a few instances. No Federal agency (EPA, DOE, NRC, OSHA) has given public notice of the revision of its radiation protection rules to change rules from the dosimetry in ICRP 26 and 30 to that of ICRP 60. EPA has not withdrawn Federal Guidance Report 11 and 12 (which are based on ICRP 26 and 30 dosimetry) in favor of the more recent models. All Federal agencies have agreed to use Federal Guidance Reports 11 and 12 for radiation protection purposes; although the Federal Agencies lead by EPA are reevaluating the possible use of the ICRP 60+ dosimetry but have not made any general recommendations at this time. So, because of the difference in organ weighting factors (discussed in the second full paragraph on page 28) there is a potential for regulatory disconnects between different dosimetry models. However, the authors of this draft report have identified the reason for the fact that ICRP 60+ dosimetry is not used widely within the Federal government. In the third full paragraph on page 28, they observe: "However, the working group has examined the relative changes in these parameters and has concluded that the parameters being examined in detail would not have changed." On a larger scale, this is a succinct description of why ICRP 60+ dosimetry has not been embraced by the Federal government – there are very significant costs and very little benefit in the way of health protection. And in the case of the RSALs, it appears that the difference in dose factors does not change the RSAL in a significant way	are regulatory "disconnects", the end-result for calculating RSALS is insignificant. See also the responses to this reviewer's comments #19 and #38.
34	Page 30: In the discussion about the "Outdoor Time Fraction" parameter, the correlation between the indoor and outdoor time fraction should have been a negative correlation, since, as the text	The point is well taken. The RESRAD calculations will be redone using a correlation factor of –0.999 for indoor and outdoor time distributions, among other changed

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	indicates, time spent outdoors cannot be spent indoors. In the discussion on the "Depth of Roots," the choice of setting the depth of roots equal to the contamination thickness is proper, because in the process of plowing and tilling the soil of a garden, the residual radioactivity would be homogenized throughout the thickness of the contaminated zone and the soil mixing layer.	parameters.
35	Page 31: In the discussion of the "Mass Loading for Inhalation" parameter, an assertion is made that recent air monitoring "does not adequately represent potential perturbations to the annual mass loading that might be experienced by a future user at Rocky Flats." Shouldn't the monitoring data reflect the ambient conditions? Have there not been wildfires, both on and off site? Are there not a large number of vehicles driving onto the site with workers? Do these fires and vehicles not "perturb" the airborne particulates at the site, and introduce more dust into the air than would otherwise be present? After closure, wouldn't the large number of vehicles traveling to and from the site decrease in a dramatic way? While the use of a distribution of values is prudent, the text in the report is in need of some revision	The issue to be discussed here is whether the recent air monitoring for PM-10 and TSP adequately reflects the activities that go on at the site. The air monitoring for particulate mass loading is performed at locations that would not necessarily capture the influence of Rocky Flats activities *per se*, but is instead intended to capture samples that represent the regional air quality in this area. If the modeling is to adequately represent the effects of actual land perturbations on dose and risk, the inputs need to reflect the direct influence of those activities. The Working Group attempts to capture those potential activities by estimating the baseline influence of such activities relative to the present regional observations. This results in an increased baseline mass loading because the "sampling" would be done at a receptor who is closer to the activity than is represented by a regional air monitor. The text will be clarified regarding this point.
36	Page 42: In Section IV-6, there is no discussion of the rate of irrigation affecting airborne particulates. If the site were to be irrigated at the assumed 1 meter per year rate, the airborne dust would be significantly reduced.	The scenario assumes irrigation is used only for the garden, not for lawn or landscaping use. Dust would be suppressed in the garden area by this irrigation, but not in the surrounding areas. See the discussion in response to Alexander Williams Comment # 7.
37	Page 43: In the paragraph at the bottom of the page, a better description of the administrative details of the wind-erosion studies should be presented. The text should read: "Under contract with [DOE, Kaiser-Hill, etc.] the xyz corp conducted a wind erosion study...."	The reviewer is correct in noting that this attribution is missing. The text will be modified appropriately.

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38	Pages 45-48: This discussion does not mention that EPA used ICRP 26 and 30 dosimetry to produce Federal guidance Reports 11 and 12, and that DOE, and NRC have agreed to use the EPA reports for radiation protection purposes. EPA has not issued any successor to those reports or announced their withdrawal from use.	Comment 38 indicates that DOE is obligated to compute doses for annual compliance reports using tissue weighting factors from ICRP 26 and dose conversion factors from Federal Guidance Report 11 (based upon ICRP 30). As stated in the response to Alexander Williams comment number 19, the DOE site annual compliance report and the derivation of RSALs will be kept separate. Of course, all site compliance calculations of dose will continue to be performed using ICRP 26/30 methodology, as required by DOE Orders.
39	Page 48: In the last sentence of the first paragraph, the text should read: "The current NRC, State of Colorado, EPA, and DOE radiation regulations relevant to determining total effective dose equivalents are based on ICRP 30."	The text will be modified to incorporate this suggestion.
40	In the third paragraph of page 48, there needs to be an expansion of the discussion involving the inhalation class of plutonium. The text might be something like: "...disagree on this point (on the basis of environmental data at Rocky Flats and elsewhere, DOE advocated use of the slowest absorption type, S type but because EPA felt that this data did not provide absolute certainty, M type should be employed for conservatism). All Parties..."	See response to Peer Reviewer 2, comment #16.
41	Page 50: Just above section V-2, an assertion is made that the americium to plutonium activity ratio is .1527. What is the correlation coefficient for the linear regression of the data from the 903-B Pad characterization?	The activity ratio used in the draft report compared HPGe gamma measurements for Am to alpha spectroscopy results for Pu. A linear correlation of Pu alpha spectroscopy to Am alpha spectroscopy data in the 903 Pad characterization report yields a Pu:Am ratio of 5.815 (Am:Pu ratio of 0.17). The correlation coefficient (R) for the linear regression is 0.89. The text will be modified.
42	Page 60: In the very last table entry on this page, the failure of the EPA risk methodology to consider radioactive decay will definitely overestimate risk but probably not at Rocky Flats. There are no significant short lived radionuclides, and future ingrowth of radionuclides in decay chains is not significant. Nonetheless, the text	Text will be modified.

	should read that this "will over-estimate risk" rather than "is likely to over-estimate risk."	
43	Page 61: Tables VI-1, VI-2, VI-3, VI-4, and VI-5 – the following concerns should be added to these tables, as appropriate: Assumption that there are foodstuffs available to a rural resident notwithstanding a simultaneous assumption that the assumed farm is denuded of vegetation. Assumption that a heavily irrigated (1 meter per year) agricultural area is susceptible to fire to the same extent as unirrigated areas and that post-fire dust levels in irrigated areas are also comparable to unirrigated areas.. Assumption that irrigation has no effect on vegetation regrowth after a fire. Assumption that buildings (rural resident home and wildlife worker office) are not destroyed by fire despite all vegetation being burned. Assumption that the establishment of buildings (and utilities-- sewer, water, gas, electricity, etc.) will not mix, bury, and otherwise dilute and disperse residual radioactivity during construction	The tables in Section VI will be revised, as appropriate.
44	Page 67: In the discussion of contaminated zone thickness, the text should explain that plowing or tilling of soil for agricultural use will mix the soil, and that 0.15 meters is a reasonable approximation for the depth of mixing.	The rationale will be modified to read, "Accounts for the possibility that all contaminated dust can eventually be inhaled. Surface soil profiles at Rocky Flats indicate that 90% of the contamination is in the upper 15 cm. No credit was taken for dilution since the Working Group considered only limited tilling in a garden and not large-scale farming activities. Tilling will mix soil and 0.15 m is a reasonable approximation for the depth of mixing."
45	Page 72: In the first table entry, the word "Work" should be inserted before the word "time." The text should read, "Time on-site: Worker is assumed to spend 100% of his/her **work** time on-site within the approximately 300 acres that is contaminated above 10 pCi/g." Based on the data presented here, the outdoor exposure for the wildlife workers should be evaluated for reduction.	See answer to Alexander Williams' comment # 3 above.
46	Page 72: In the discussion of the possibility of a day care facility for	We agree.

	children, it is unlikely that there would be enough wildlife workers employed at the site to make an on-site day care facility economically feasible. A provision for a day care facility for people not employed at a wildlife refuge is a commercial use not consistent with the proposed status of the site as a wildlife refuge.	
47	Page 74: There is an error in the stated RESRAD occupancy factors for Exposure Time and Indoor Time Fraction. For RESRAD, there is no occupancy factor. For RESRAD, the indoor time fraction for occupational exposure should be about 20 hours per week divided by 168 hours per week or 0.12, which represents the fraction of a year spent indoors on-site.	The appropriate adjustments were incorporated in the indoor and outdoor time fractions. The table will be corrected.
48	Page 75: In the discussion of Outdoor Time Fraction, the RESRAD parameter should be about 0.12, not 0.5. This input is the fraction of a year spent on-site, outdoors.	See response to comment # 47.
49	Page 77: The assumption that an open space user will spend 100% of his/her time in 300 acres of a 6400-acre tract is overly conservative. The exposures should be scaled be dividing by a factor of 10 to account for this circumstance.	The working group agrees that the assumption is conservative, but does not believe it is overly conservative. Given the passage of the Congressional Act making Rocky Flats a wildlife refuge, the open space user should be regarded as a wildlife refuge visitor. A typical wildlife refuge visitor differs from a park or open space user. Typically, visitors to wildlife refuges are not allowed free access to an entire site, and activities such as mountain biking are not allowed. Both of these uses are not consistent with the primary purpose of the refuge, which is protection of wildlife habitat and populations. Rather, visitors are usually constrained to existing walking trails. Using this as a basic assumption, CDPHE calculated an activity-weighted exposure unit for the wildlife refuge visitor of approximately 10 acres.
50	Page 79: The RESRAD Outdoor Time Fraction is not correct. With	The appropriate factors were used in the calculation. The

	the exposure defined as 100 visits per year and 2.5 hours per visit, the total time is 5 hours per week, or, for the RESRAD input, 0.03. (The RESRAD input is the fraction of a year spent on-site, outdoors.)	table will be corrected.
51	Page 82: The RESRAD Occupancy Factor and Indoor Time Fraction are not correct. There is no "occupancy factor" in RESRAD. The indoor time fraction will be 8 hours per day, 50 weeks per year, or .24, which is the fraction of a year spent on-site, indoors.	The appropriate factors were used in the calculation. The table will be corrected.
52	Appendix A: Page 2: The thickness of the contaminated zone is appropriate set for 0.15 meter. This is the likely depth for plowing, and, if one assumes agricultural use, plowing is certain. The same value for thickness of roots is appropriate, with the understanding that this may overestimate root uptake of some crops.	The Working Group agrees with this comment. No credit was taken for the dilution that would occur in the limited garden area.
53	Appendix A: Page 3: Setting the soil mixing layer to 0.15 meters is appropriate, if agricultural activities are assumed. It is likely that plowing the soil would mix the soil over this depth.	See response to comment # 52.
54	Appendix A: Page 5: In the discussion of mass loading of dust in the air, it is possible that the dust in air at the site would decrease after closure because of the decrease in human and vehicular traffic. There are presently hundreds of people and vehicles driving and walking through the site. After closure, this will greatly decrease. Consequently, there is a possible reduction in airborne dust from the current measured values after site closure. This possibility should be discussed in this section.	The reviewer is correct that site activities will decrease. However, it is not thought that the regional dust loading presently being measured is particularly influenced by Rocky Flats activities. As noted in response to comment #35 from this reviewer, the dust loading used in the modeling calculations needs to be representative of the actual activities that are being modeled. The text will be clarified to reveal this point in more detail.
55	Appendix A: There is a brief discussion about irrigation decreasing airborne dust for the rural resident. The assumed irrigation will decrease dust by increasing the growth of vegetation and increasing soil moisture. Further, in the event of any fires, irrigation would decrease the extent and severity of fires, and irrigation would grow back much faster because the irrigation would facilitate the regrowth of plants.	As stated in response to comment #36 by this same reviewer, the Working Group assumed irrigation would be applied only to the garden area.
56	Appendix A: There has also been an extensive and commendable	The Working Group appreciates this comment. It will

	effort to identify airborne dust levels both near Rocky Flats and at other sites within Colorado. This data is presented in summary form in Appendix F.	however attempt to better explain the relevance of this information to the mass loading calculations.
57	Appendix A: Page 22: Are the concentration units mg/day throughout this table? The units should be shown.	The units mg/day will be added to the table.
58	Appendix A: Page 51; As discussed above, the possibility that a person is present on-site for as much as 24 hours per day for 350 days per year is quite dubious. While the parameter is handled in a probabilistic manner, the distribution should be examined to verify that it is sound.	See response to comment #11 above. EPA recommends an RME of 24 hours per day, 350 days per year for a residential scenario. It is true that most residents will be away from home more often than this length of time, however there are citizens who are elderly or disabled and leave home very infrequently.
59	Appendix A: Page 54: There is an extensive discussion of the exposure frequency for a wildlife worker. However, there is residual radioactivity in only a small portion of the site, and it is incorrect to assume that all of the time "on-site" is in an area where there is residual radioactivity.	The RSAL calculation, appropriately, focused on the limiting condition, rather than a realistic condition in assuming that the wildlife refuge worker would spend all of his time on-site in the most contaminated area. As discussed in the response to Alexander Williams Comment 3, we do not believe that the size of the area used to calculate the RSAL was unreasonable.
60	Appendix B: These equations do not account for radioactive decay. This circumstance does not affect the calculations at Rocky Flats in a significant way.	The reviewer has correctly noted that omitting consideration of radioactive decay in the risk calculation will have little effect on the result. Ingrowth is also not considered in the Standard Risk Equations, and has perhaps the potential to affect the results even more than decay. The Working Group addressed this in its selection of an americium to plutonium ratio which is very near the equilibrium value for weapons grade plutonium, thus assuring near maximum ingrowth in its initial conditions. Also, the risk equations are used to calculate risks based upon radioactive inventories and environmental conditions which are typical of the early period of contamination, when weathering and radioactive decay have not significantly reduced the level of

		contamination, thereby computing a conservative RSAL. Finally, the long half lives of the plutonium isotopes and steady state conditions (equilibrium) of the americium inventory assure that there is little change in exposure conditions over the relative short exposure durations considered. These aspects of the problem effectively compensate for the limitations of the risk equations, as used at Rocky Flats.
61	Appendix C: Page 1: in the first bullet, the shape affects the direct gamma radiation exposure pathway, but not the other pathways. For shapes other than circular and for exposure positions other than in the center, the direct gamma radiation dose is lower. Since direct gamma radiation is not significant at Rocky Flats, this assumption does not have much of an effect.	The point is well taken. The feature of RESRAD which enables non-circular shapes and non centrally positioned receptors to be considered is very useful, particularly when modeling smaller areas of contamination. In addition to the point made by the reviewer that the external exposure component of total exposure is small in this calculation, there is also the fact that the area modeled for plutonium and americium contamination is quite large in this problem. It has been our experience when using RESRAD that the gamma exposure pathway reaches its limiting value (saturates) at relatively small areas under conditions of ideal geometry - on the order of a few hundreds of square meters. The area modeled at Rocky Flats is many times larger than this, suggesting that the shape of the contaminated area and positioning of the receptor are not important unless the receptor is positioned close to an edge of the contaminated area.
62	Appendix C: Page 2: In the second paragraph, the discussion of the area correction factor is wrong. There was a model change in the "area factor" between RESRAD 5.61 and RESRAD 6.1. But since the area factor calculation is different between the two versions, the conclusion of the paragraph is correct: ".... the results [of the previous work] are not directly comparable to the results of this	The reviewer is correct in his reference to Appendix D: page 2. The statement on page 2 should reflect that the calculation of the area correction factor was not the same in the RESRAD code used in 1996. This error will be corrected in the text.

	task."	
63	Page 5: The input data includes distribution coefficients for Pu, AM, and U. Were these measured? What is the reference?	The reviewer refers to Appendix D, page 5. The distribution coefficients stated in this table are those used in the 1996 RSAL calculations. Since the water pathways were not turned on for the calculations of the RSAL, these distribution coefficients were not used. Protection of surface water quality will be considered separately from the RSAL calculation.
64	Appendix G: The discussion on page 3 compares actual air monitoring data and the RAC modeling results. This presentation is very helpful.	Considerable confusion appears to exist as to the reasons for the differences in values of RSALs calculated by the RAC methodology and by the Working Group's approach for a similar scenario. There has been speculation that RAC's lower numbers are due to selection of more extreme scenario and exposure conditions (maximally exposed individual) versus those values used by the Working Group (reasonably maximum exposed individual). From our work with the RAC scenario, we are convinced that the single most important factor, by far, which is responsible for the majority of difference in computed RSAL values between RAC and the Working Group, is the use by RAC of a calculation algorithm for annual average mass loading in air, following a fire, which results in very high values of mass loading at the upper end of its distribution. It is obvious from comparing the RAC dose components (where the inhalation pathway completely dominates) with those of the Working Group (where the soil ingestion pathway contributes most) that the choice of mass loading value is the critical difference, in spite of all other scenario differences. Since the Working Group chose to use a mass loading distribution based upon empirical data, as opposed to a

		calculation algorithm, we wanted to see how the critical numbers of RAC's distribution of calculated values would compare with empirical data for annual averages of small particles in air. EPA's database offered a ready opportunity to compare PM10 data for annual averages as measured in the US and elsewhere, with the numbers generated by RAC's algorithm. We felt that it was important to present this comparison in an effort to clear up misunderstanding. Based on this comparison, we are convinced that there would be minor differences in RSAL values computed by RAC and the Working Group if RAC had used an empirically measured mass loading distribution (with empirically measured post fire data as well) similar to the one we developed.

	Review Comments – Le Roy Moore	Response
1	pp. 1,49: The table of dose and risk calculations for various scenarios needs to show numbers for the resident rancher under the CERCLA risk levels, in order to make the resident rancher scenario readily comparable to risk calculations for the other scenarios. Also, it would be valuable to have a column for the 15 mrem/y dose level used in 1996 by RAC in 2000.	The agencies had committed to model the Resident Rancher scenario as described in the RAC Independent Calculation using RESRAD 6.0 for the purpose of comparing the computational methods employed by RAC to those employed by the agency work group. The agencies did not agree to perform risk calculations for the Resident Rancher scenario using the EPA risk equations, which would be a significant amount of additional work. The agencies considered that the rural resident scenario with 5-acre lots was a more realistic land use in the event of institutional control failure, representing an RME individual. The proximity of Rocky Flats to a major metropolitan area that has encroached from the south, east and north also makes the development of Rocky Flats as a full-scale ranch unlikely.
2	p. 4, ¶ 2: Correct "principle" to "principal"	This change will be made.
3	p. 7: There is nothing specifying the number of years the refuge worker is expected to work at the site (this info is given on p. 16).	This information will be added to the text of the report for the Wildlife Refuge Worker.
4	p. 9: Re. the rural residential scenario, is it realistic to assume this person will be on the site 24 hours/day for up to 350 days/year but outdoors no more that 20% of the time?	It is reasonable. These data are taken from EPA's default central tendency recommendation for residential exposure. The percent time indoors includes a person's activities year-round in both warm and cold seasons, including eating, sleeping. It is certainly true that a person could be outside for longer periods during warm seasons, but those longer time periods will likely be offset staying indoors more frequently during colder periods.
5	pp. 17-17: More detail and documentation is needed to support the	Given its very limited capacity, the shallow alluvium at

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	assertion that onsite water would not be used under any scenario considered. Could damming of streams provide enough water? Could this be supplemented by wells? One thing clear here is that the scenario selection precludes adequate attention to the to the water-use question. What would it look like to calculate possible water use for the resident rancher or subsistence farmer scenario?	Rocky Flats is not considered to be a viable source for drinking or irrigation water. The Laramie-Fox Hills aquifer, located approximately 600 feet below the surface, is a regional aquifer. The Working Group believes that water would have to be imported or pumped from the deep aquifer to support any agricultural or residential use of the land.. With respect to the availability of surface water for use at a ranch, preliminary results from the Site-Wide Water Balance Study indicate that post-closure conditions in Walnut Creek are likely to be much drier than they are today. This is due to the fact that the site is purchasing water for potable use and that this water is discharging into entering Walnut Creek from both leaky pipes in the Industrial Area and from the wastewater treatment plant. This water use will end. In addition, the impermeable paved surfaces in the Industrial Area cause precipitation to discharge directly into Walnut Creek via storm sewers. These paved surfaces will be removed from the Industrial Area, which will make the creek's watershed to be much more similar to the more natural conditions found in the Woman Creek watershed. In the Woman Creek drainage, the vast majority of precipitation evaporates rather than leaves the site as surface water.
6	p. 18, III-3: David Janecky, at a recent AME meeting, said he had found unusually high concentrations of Am in certain areas of the site. I gathered from his presentation that the Am about which he spoke is above and beyond what would show up as daughter product of weapons-grade Pu. Does the sum-of-ratios method for calculating RSALs account for these unusually high levels of Am?	David Janecky presented no new information regarding the possible origin of americium on the site; this was known by the individuals working on the 1996 RSAL report, and before. The sole purpose of the sum-of-ratios calculation is to deal with varying relative concentrations of contaminants, such as documented by Dr. Janecky. The Working Group has calculated separate action levels for americium and plutonium; those action levels apply to any relative mix of the isotopic concentrations, through the sum-of-ratios calculation. There may be some confusion on this

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		issue because of the way the RSAL values are presented in the Executive Summary of the Task 3 Report. There, the RSALs are presented as sum-of-ratios for the Am-to-Pu ratio observed at the 903 Pad. The results are presented this way to be consistent with the presentation of results from the 1996 RSAL Report and from the work of RAC. Also see response to Melissa Anderson's comment #9.
7	p. 18, final sentence: This sentence suggests that no adverse effects can be expected from movement of Pu in shallow groundwater. Isn't movement of Pu in shallow groundwater a possible source of the 1997 exceedances to the state's Pu-in-surface-water standard?	There is no evidence to suggest that the values observed at monitoring location GS03 are from shallow groundwater, nor is there reason to believe that a shallow groundwater plume contaminated with plutonium would exist in that area. Data from wells installed in the vicinity of GS03 indicate plutonium concentrations consistent with values measured in clean "blank" water samples submitted as part of the AME investigations. Erosion modeling performed as part of the Actinide Migration Evaluations shows that erosional transport from even moderately contaminated surface soils can, under the right circumstances, cause concentrations in surface water to exceed the 0.15 pCi/l standard. However, that modeling was performed on an "event" basis, and does not allow one to conclude *per se* that the underlying standard would be exceeded.
8	p. 21, lines 3 and 4 and elsewhere: Please explain and demonstrate what is meant by selection of "a health protective point estimate."	A health protective point estimate is a single value used in lieu of a distribution when available data are deemed inadequate to create a distribution or where the parameter is considered to not be influential in significantly affecting the resulting calculation. An example of a health protective point estimate would be to assume that, for the rural resident scenario, all home-grown produce is considered to be contaminated.
9	p. 25: What is the "outdoor time fraction" so insignificant?	The outdoor time fraction variable contributes to the dose from each of the exposure pathways. Therefore, it is more likely to modify the total dose than many of the other

		exposure variables that appear in only one of the exposure pathways.
10	p. 27, ¶ 2: Where is Figure IV-4 ?	Figures were numbered and referenced incorrectly. The text will be modified.
11	p. 44: Assuming we get a green light on the way the wind tunnel data has been used in this report, I will here raise two points. First, in calculating mass loading from fire, the agencies should get data re. possible climate change in the Rocky Flats area over the next century and beyond, as far as projections have been or can be made by, say, NCAR. Is the area likely to be wetter or dryer, according to prevailing climatic trends? How might this data affect the possibility of fire and thus the understanding of mass loading in association with fire? Second, the information given suggests that a short-term calculation for a fire has been made, that is, one that assumes the continued utilization of controlled burns. Since there is strong public opposition to controlled burns, what other alternative short-term calculation can be offered – that is, one that does not assume ongoing controlled burns? Then, with respect to the long term, what will happen regarding mass loading when the practice of controlled burns has ceased? There are two different ways of asking for attention to the absence of controlled burns. The first assumes there might be an alternate near-term practice, the second that any attempt to offset the danger of fire will some day disappear and thus that the fire potential should be calculated assuming controlled burns are not happening.	Regarding the first question: The Working Group took the published guidance of the National Drought Mitigation Center as its basis for using the existing 35 years of validated site meteorological data in assessing the influence of precipitation on the land-use scenarios. Regarding future prediction of change, in its global warming website, "EPA reiterates the warning provided by all climate modelers to people considering the impacts of future climate change: *the projections of climate change in specific areas are not forecasts, but are reasonable examples of how the climate might change.*" (EPA, 2001; http://www.epa.gov/globalwarming/climate/future/usclimate.html). Even if the Working Group had the appropriate tools to deal with future climate change, projections into the future regarding weather influences would require more changes to the scenarios than a simple change in mass loading parameters. A shift in seasonally averaged temperatures in the Front Range area may result in significant changes in the types of vegetation, as well as significant changes in the number and intensity of storms, etc. For example, on EPA's website, there are discussions that suggest shrub-like vegetation could be favored over the prairie grasses that presently abound; also there are projections suggesting more rainfall, but potentially drier soil. One could even question the validity of the land uses themselves, depending

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		on the severity of the changes. Consequently, the parameters that would be modeled would change in ways that the Working Group could not predict. The Working Group chose to work within the confines of "reasonably foreseeable" land uses, as prescribed in CERCLA. Regarding the second question: We must first agree that the risk incurred by the aftermath of a fire is independent of the fire's cause, whether naturally occurring or man-made. Secondly, we must recognize that the purpose of conducting controlled burns is not primarily for fire risk reduction, but for prairie grassland management. The major contribution to fire management from controlled burns is to reduce the rate and intensity at which a possible fire might expand, not its frequency. Once that basis is established, the question to be considered, and the one actually considered by the Working Group, is "what is the most conservative, and reasonably predictable, fire frequency, and what is its influence on dose and risk. The "short-term calculation", i.e., one in ten probability, turns out to be a more conservative calculation of fire probability than the other cases presented in this question. Apparently, the report does not expand sufficiently on the range of possibilities explored by the Working Group. Consider several possible approaches: 1. A wildfire on the site was considered a likely event. Its probability in a given year on a contaminated area could be developed through a number of reasonable assumptions including: ▪ Assume one fire a year (this frequency is more often than has been typically observed at the site)

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		▪ Assume one 300 acre parcel of land is significantly contaminated The probability of a fire on the contaminated 300-acre parcel can be estimated as 300/6400 per year; that is approximately 5% probability/year, or one fire every 20 years on the contaminated parcel. There is no reason to assume preference of one area over another for naturally occurring fires. We might observe additional fires near the perimeter roads, but those have no influence on the probability of fires in the contaminated areas. Assuming the entire 300 acre parcel were to burn, it is not reasonable to speculate that the same parcel would burn two years in a row. The amount of fuel available the second year would not support a significant burn and, if prairie management were the driver, such an occurrence would not even make good sense. This consecutive-year exclusion would have the net effect of reducing the influence of fires in any given multi-year risk calculation. If fewer than 300 acres were burned, the remaining contaminated area could burn the next year, with the reduced consequences of the smaller exposed area. (This multi-year exclusion was not exercised in the Working Group calculations). While this area-based probabilistic calculation was favored by at least one member of the Working Group, the group settled on controlled burns to establish the more predictable, and higher, frequency, since that ten-year rotation yielded fires

		at twice the rate of the wildfires. To make the calculation more realistic under institutional control, one could assume that controlled burns would be conducted in areas removed from significant contamination. The Working Group discarded this assumption recognizing that a wildlife refuge could continue even though institutional history was lost. 2. One could also take an entirely different perspective and view the fire events as occurring randomly over the entire site with fires of the size and frequency of occurrence as are observed across the Front Range. Based on data from the Colorado Forest Service for 1999, of 390 grass/wildfires reported in the Front Range, almost all were less than 1 acre, with seven reported between 1 and 6 acres and only one reported larger, at 352 acres. Based on acerage, only 1 acre in a thousand would be expected to burn in any given year. For a fire of 5 acres or more, the probability is far less than 1% per year, all other factors being the same. The Working Group considered these data in its discussions and settled on the ten-year frequency (10% probability) as being a more reliable indicator. It turns out also to be more conservative.
12	Appendix A, p. 6. ¶1, final: The first sentence here seems to misrepresent the nature of RAC's work, which was not a peer review of the 1988 work but a independent analysis and calculation for RSALs for Rocky Flats.	The reviewer is correct. The text will be modified.
13	Appendix A, p. 9. ¶1, final sentence: Please explain why zero rainfall was not considered a feasible condition to assess.	An event in the Front Range characterized by zero annual rainfall would be disastrous without regard to the conditions at Rocky Flats, in part because it would have to be preceded by other very significant changes in climate. Such an

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		extreme assumption would be a distortion of any reasonable scenario representation, causing speculation far outside the ranges of available data or rational assumptions, and would misappropriate the process of estimating probabilistic risk to a "reasonably maximally exposed individual". Zero rainfall would be by definition, the 100th percentile rain-deficient condition, in other words an extreme event with essentially zero probability of occurrence. Were the Working Group to consider such an unrealistic condition, the Working Group would have to adjust a number of scenario assumptions including the rate of homegrown vegetable consumption, the time fraction spent indoors and outdoors, the time spent on-site, the dust shielding factor for the building, etc., as well as related input parameters such as soil ingestion rate. To understand the simple projections used to estimate the effects of deficient rainfall on mass loading, we need to keep in mind that we are already working with a semi-arid environment. Using the same algorithm as used to calculate the 95th percentile effect of deficient rainfall would suggest an increase of 30-plus percent in the mass loading, compared to the 14 percent increase at 95th percentile deficiency. Of course, the assumptions going into the algorithm would likely be invalid under conditions of zero annual rainfall, as would many of the conditions and assumptions in the land use scenario itself.
14	Road construction: Given that the legislation to make Rocky Flats a wildlife refuge includes provision for construction of a segment of the Northwest Parkway along the eastern border of the site, all	Large-scale construction projects such as road construction move vast quantities of soil and completely disrupt the soil in the area over which the projects are executed. There

	scenarios for which the RSAL calculation is being made should include information of the condition of this portion of the site and possible effects of such construction.	would be no long-term increase in risk from the soils in the area of the construction project. In fact, the result would be a net reduction in risk, because the relatively small quantities of contaminated soils would be mixed into clean soil and covered in a manner that limits the erosion potential to negligible levels. The remediation performed on OU-3 is an example of such dilution, whether deemed desirable or not by a particular reviewing party. A more pertinent question could be asked regarding the short-term dose during the period of construction, actually only for a period during which the surface soil is either being disturbed or removed. While this event is not considered in any of the land use scenarios, it is accommodated in the leading assumptions to the choice of the median soil concentration used to "seed" the probabilistic mass loading distribution. In the initial investigation of the effects of soil disturbance activities that might play in a rural resident or wildlife refuge worker scenario, small-scale construction and soil disturbance activities, of the type that would be supported by the specific scenarios, were considered. These activities essentially doubled the presently observed air mass loadings for the years in which the activities were performed. This resulted in the selection of a median mass loading of about 22 ug/m^3 for the starting mass loading distribution estimate. For the purpose of discussion, consider in more detail the process of earth-moving and filling. The repeated scraping, filling, and compacting of the soil serves to dilute the actinides in the soil that is being worked. The thin contaminated layer of several inches would be quickly covered and/or graded and mixed with larger volumes of

		uncontaminated soils, resulting in much lower actinide concentrations in the potentially exposed materials subject to wind and water erosion. While the initial disturbance would involve fully contaminated soil, the disturbance would necessarily be of short duration prior to the soil being mixed and mostly covered. Very little of the contaminated soil material would actually be available for suspension and erosion into the atmosphere. Risk would be significantly reduced in such a scenario.
15	On what basis do the authors of the Task 3 Report disagree with IEER's finding regarding the scientific validity for using the subsistence farmer scenario to calculate the RSALs for Rocky Flats? Why is it reasonable to reject this scenario, given the long-term danger posed by contaminants at Rocky Flats? Please note the detailed historical analysis of this scenario provided by IEER's full report.	The agencies are obligated to make their decisions based upon established regulations and policy, which put forth a strong preference for basing cleanup decisions on the anticipated future land use. An extensive discussion of these regulations and policies, and their application at Rocky Flats was presented in the Task 1 document, "Radionuclide Soil Action Level Regulatory Analysis", Revision 2, dated January 24, 2001. Further explanation was provided in the response to comments on that document. INEER's December 2001 report describes a subsistence farmer scenario that has many similarities to the work groups rural resident scenario. Page 19 of the INEER report describes a scenario where 25% of the diet comes from food grown onsite. Page 23 of the report describes a subsistence farmer who is technologically advanced to the point where he or she could grow much of their own food, yet be able to devote much of their time to other pursuits. The agencies' rural resident scenario assumes that all fruits and vegetables come from contaminated soil onsite and that the resident spends 350 days per year, 24 hours per day on site.
16	What is the basis for the evident determination by the authors of the Task 3 Report that it is appropriate to assume that site control,	The agencies do not assume that institutional controls are likely to last thousands of years. It is interesting that the

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	institutional memory, and legal land use restrictions will prevail for thousands of years?	INEER report reveals a dichotomy as to whether there are legal bounds on the living conditions or not, even though the authors state there should not be. The implication of a technologically advanced methodology for farming would imply an infrastructure in society through which the technology can be obtained and applied, yet the farmer has the option to do as he or she pleases without any worry of government sanctions. To farm this land in this technologically advanced manner would imply the application of soil amendment, deep tilling and use of a reliable, readily available water source. This level of sophistication plays against the concept of unchanging contaminant concentrations, and the use of limited shallow ground water for both irrigation and drinking water. Instead, this reinforces the idea that it would be reasonable to assume the use of imported water, and suggests once again that the agencies have been overly conservative in the risk assumption that the soil remains at a fixed contaminant level.
17	Why do DOE and the regulators assume that calculating RSALs to protect a wildlife refuge worker provides an adequate basis for long-term public health protection? What is the basis for this assumption?	The agencies intend to select RSAL and cleanup levels that fall within the CERCLA risk range for the anticipated land use, which is a wildlife refuge worker. It is likely that these selected numbers will also fall within the CERCLA risk range for the rural resident scenario. The agencies consider these ranges to be protective.
18	An RSAL for plutonium calculated to protect a wildlife refuge worker at a 10-6 risk level would fall within the 1 to 10 picocuries per gram level recommended by IEER. Do the agencies expect to set the RSAL at this level? If not, why not?	No. See response to comment 17 above.
19	Given the postulation of a genetic "uncertainty principle," can the agencies demonstrate conclusively that they can protect wildlife over the long-term with an RSAL set at a risk level less protective than 10-6?	There is no risk level associated with this postulate. The manner in which this postulation is formed would require a different basis for resolution than has been used to estimate the excess risk associated with setting RSAL levels.

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		The postulate supposes there is genetic change in a population caused by radiation damage that is not manifested in individual species of the population until essentially the entire population is affected. If true, this would imply that genetic changes are inevitable (and unpredictable – "genetic uncertainty", but hardly a "principle") due to radiation exposure. The increase or reduction of radiation exposure would only be a means to increase or reduce the rate of such uncharacterized damage in the species. By its very nature, this damage would be manifested through a continuous process even in the absence of any influence from residual contamination, because background radiation is contributing at least an order of magnitude greater intensity than is contributed by the residual contamination itself at any of the RSAL levels calculated. The necessary conclusion from this examination is that reduction in exposure would serve only to slow the process but would not reduce or in any other way change the risk that the process is ongoing. We know of no mechanism for dealing with such uncharacterized temporal risk in the literature, nor is the conjecture of such an approach a constructive use of resources at this time.