

COMMENT SHEET

Title: Estimated Exposure and Lifetime Cancer Incidence Risk from 903 Area Plutonium Releases at the Rocky Flats Plant
Date: January, 1999

Comment #	Page #	Line #	COMMENT	RESPONSE	ACCEPT
1	i 2	footnote 1 footnote 1	I suggest mentioning that the Pu composition is in weight percent.	accepted - change made	
2	General		It appears that the Am-241 that ingrows from the decay of Pu-241 has been omitted from the source term.	Yes it has, but the amount of ingrowth is small in the time frame we are interested in	
3	ii	paragraph 2	I suggest adding some text to this paragraph that states that larger particles are not a hazard because they are not respirable (e.g., see text on page iii).	accepted - changes made	
4	ii	paragraph 2	In the last two sentences of this paragraph, the report implies that > 50 µm particles would have settled out before reaching the nearest receptor at 3 km. If this is the case, then it should be explicitly stated. (see also page 6, paragraph 3)	Calculations were not performed using 50 µm particles, but is suspected based on the 30 µm particle sizes	
5	iii	paragraph 3	Since the operation of the Rocky Flats Plant meteorological tower began in 1984, why did the report use data only from 1989-1993? Shouldn't all available data be used? (see also page 18, paragraph 2).	Compilation of all the data is not a trivial task. We chose to use data that was made available to us from the RFP. In future work, added years could be obtained and annual average X/Q values calculated. I doubt this would make much difference in the results	
6	iv	bullet 2	Exposure Scenarios. Since this report does not discuss the 1957 fire, I suggest deleting the material in the parentheses in the second bullet.	accepted - change made	
7	iv v	last paragraph first paragraph	Inhalation of suspended Pu from the 903 Area is the only pathway analyzed in this study. As such, the text should be modified to make a clear distinction between the suspension of Pu from the 903 Area and the more regional resuspension of Pu, which was not analyzed in the report.	accepted - changes made	
8	General	Executive Summary	Although this is an editorial comment, I suggest not using acronyms in the Executive Summary (for readability).	ignore	
9	v 64	2nd bullet 2nd bullet	Since you really don't know what the minimum or maximum value in a Monte Carlo simulation would be (if you ran 2000 runs, you might get a different minimum or maximum), I suggest saying that there is a 2.5% probability that the cancer incidence exceeds 3.3E-5 and a 2.5% probability that the risk is below 4.4E-8. Also, it should be stated that the uncertainty does not include the uncertainty in the cancer incidence risk coefficients, except for particle size.	accepted - changes made	
10	9	paragraph 2	"Soul" should be "soil."	accepted - changes made	
11	1	figure 1	In my copy of the report, North Walnut Creek and Woman Creek do not show up.	I believe the quality of the photocopy is poor	
12	8	table 2	The 6 largest release days are based on the less than 30 µm release data, yet the risk assessment is based on the less 15 µm data. Therefore, the report should discuss whether the 6 largest less than 30 µm release days correspond to the 6 largest less than 15 µm release days.	This would be difficult to do - and for all practical purposes, the 6 highest <30 µm releases would be the same as the <15 µm releases because the fraction of activity attached to each size fraction did not vary by that much.	

Reviewer:	S.J. Maheras	Date: 11 June 1999
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13	18	paragraph 3	If a final climatological report been issued it should be discussed.	It is unknown whether a final version was ever released	
14	20	paragraph 1	While a 5-year period may be suitable for compliance purposes, in a science-oriented project such as this, more data should be used if it is available. Therefore, the report should provide technical justification for excluding the 1984-1988 data from the analyses; citing federal regulations is not adequate technical justification since regulations often are compromises between science and policy.	Compilation of all the data is not a trivial task. We chose to use data that was made available to us from the RFP. In future work, added years could be obtained and annual average X/Q values calculated. I doubt this would make much difference in the results.	
15	22	paragraph 1	The report should provide a basis for the statement that few receptors were present in the foothills while the Rocky Flats Plant was operating.	accepted - changes made	
16	8	table 2	It is curious that so many of the high release days are during the winter, when presumably the ground was frozen or snow-covered. The report should briefly comment on this subject.	Added text in the predicted concentration section that states that the passage of synoptic weather fronts were responsible for high winds.	
17	33		The report should discuss how it was determined that 1000 RATCHET runs were appropriate. Was an analysis done to justify this choice? (this is often done by examining the variance of the output as a function of the number of runs)	A good question which was addressed in the comprehensive risk report. We are primarily interested in the confidence we have in any given percentile value. A distribution-free approach has been used to address define the confidence interval around the output percentiles..	
18	39-42		There is no basis provided for the conclusion that the Denver data would look similar to the Cincinnati data after the 5-year procedure was applied. There is also no basis provided for using the Fernald data, a site over 1000 miles from Rocky Flats. The situation at Fernald may be an isolated case and may not be applicable anywhere else—the report presents insufficient analysis to show that the conclusions drawn at Fernald/Cincinnati apply at Rocky Flats/Denver.	I strongly disagree with these statements. If what your saying is true, then we also have no basis for using 5 years of meteorological data to represent past and future annual average dispersion conditions at any site. Remember, these are not absolute, but relative values. Evidence of the validity of this approach was provided by the Denver Stapleton data which showed a similar variation in the X/Q values for each year of the annual average composite data set.	
19	11	lines 4-5	I suggest quantifying what is meant by a large prediction uncertainty, i.e., "on the order of xxx."	accepted - changes made	
20	35	paragraph 2	The report should clarify what is meant by the model output was treated in straight-line Gaussian plume mode."	accepted - changes made	
21	12-16		Atmospheric Model Selection. I suggest adding an introductory paragraph for this section that describes the selection criteria for the model. As it is written now, this material is scattered throughout the section. Then I suggest objectively evaluating each model's performance against the criteria.	There was no quantitative selection criteria used to select a model. As stated in the introductory paragraph, the model comparison study determined what models, if any, performed best in the Rocky Flats environs for a given set of modeling objectives. If a quantitative criteria were established, what would it be? An what if no models passed the criteria?	

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22	7	lines 2-6	On line 2, the report says that there were 10 particle size distributions, but on line 6, 4 ranges are listed. Were the 10 distributions compressed into the 4 ranges? If so, the report should state this.	This section was re-written to clarify what was done concerning particle size distributions.	
23	7	paragraph 3	The report should state which data (10-, 25-, or 61-m) from the 61-m tower were used in the release calculations. This same comment applies to the data used in baseline atmospheric concentration calculations.	accepted - changes made	
24	18-20		Meteorology. I suggest adding a table to the report that summarizes what atmospheric data are available for what time periods at what measurement heights at which locations (e.g., Kent, Jefferson County Airport, Rocky Flats, Stapleton, etc.).	accepted - Table added	
25	General		The report is very confusing in regards to what atmospheric data are available for what time periods at what locations and also in regards to what atmospheric data were used for what calculations and why the specific data were chosen. I suggest that the sections of the report on Meteorology and Data Processing be substantially revised to describe what was done in regards to atmospheric data and why it was done. This same comment also applies to the section on Prediction Uncertainty, especially the section on Prediction Uncertainty for Baseline Releases and the subsection on Meteorology Uncertainty where the Fernald data were used.	There were some changes made to the text to clarify what we did. In addition, the Table as indicated above was added.	
26	46-47	tables 14-16	These tables should explicitly mention that they are for discrete releases.	accepted - changes made	
27	48, 50, 54	figures 10, 11, 12	I suggest presenting figures for the same particle size (currently figures 10 and 12 are for < 15 µm while figure 11 is for 10-15 µm). Also, I suggest replacing figure 11 with a figure that has atmospheric concentrations, not X/Q.	Figure 10 and 12 were changed to represent the same size fraction (<15 µm). Figure 11 is only shown to give the reader an idea of the annual average dispersion patterns.	
28	53		The report states that the discrete events were not important from an atmospheric concentration perspective. However, these events were detected only because of their atmospheric concentration at sampler S-8. This appears to be a logical inconsistency in the report.	I changed the text to state that discrete releases were responsible for most of the releases and offsite concentrations, but do not yield proportionally higher concentrations had the activity been released over a longer period of time. There is no logical inconsistency - high winds yield high releases and high concentrations. But concentrations would be higher if the <i>same</i> activity were released during lighter wind conditions.	
29	63	table 22	The cancer incidence risk in this table summed over all organs for 1 µm particles is about 0.03 per µCi of Pu-239 inhaled. This is extremely close to the Federal Guidance Report No. 13 value of 0.033 per µCi inhaled, for Type S Pu-239.	HELEN	
30	29	table 9	If the number of puffs per hour is 4 (15 minutes per puff), then why is the minimum time step set at 10 minutes? (shouldn't the two be consistent?)	There is a typo in the table. The minimum time-step is 1 minute	
31	32	table 10	Stability classes are usually denoted A-G, not 1-7.	This may be true, but in computational routines, we typically designate them in numerical format.	

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32	56	table 18	It appears as if the ages and genders of the receptors were chosen somewhat arbitrarily. Therefore, the procedure used for choosing the ages and genders of the receptors should be discussed in the text. Also, the description of which receptor types were allocated to each location should be expanded.	The receptors were chosen to simply demonstrate the methodology. One can argue for any particular scenario, and therefore, infinite possibilities are possible	
33	60	table 21	A reference should be provided for the dose conversion factors presented in this table. The absorption type and age should also be noted.	HELEN	
34	63	table 22	In ICRP-71, the Pu-239 Class S lung dose conversion factors for younger age groups are larger than for adults, which implies that the cancer risk factors would also be higher. However, in this table the cancer incidence coefficients show the opposite pattern.	HELEN	
35	60-63		Risk Coefficients. Inhalation dose conversion factors are calculated based on inhalation rates for reference man and, therefore, the inhalation rates and dose conversion factors are correlated. However, in this report, inhalation rates are treated as independent from the cancer risk coefficients. While there is not much the authors can do about this, this should be acknowledged in the text.	HELEN	
36	60-68	tables 21, 22, 23, figures 13, 14, appendix B	I suggest making the notation used in these tables and appendix consistent. For example, always use the term "Bone Surface," not just "Bone" and use the term "Bone Marrow," not "Leukemia."	accepted - changes made	
37	21-29		In the section on Atmospheric Transport Model Parameters, I suggest discussing the relationship between AED and AMAD.	ignore	
38	v	paragraph 2	I suggest consolidating the paragraph on inhalation rates with the paragraph after the bullets on page iv.	ignore	
39	ii 6	paragraph 2 paragraph 3	The term "saltation" should be briefly defined.	ignore	
40	66	last paragraph	This paragraph should mention whether the uncertainty estimates for the risk coefficients include the uncertainty in biokinetic data or metabolic models.	HELEN	