

COMMENT SHEET

Title: Estimated Exposure and Lifetime Cancer Incidence Risk from Plutonium Released from the 1957 Fire at the Rocky Flats Plant
Date: February, 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. | accept - 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 | | The report should specify whether the 1.8E-5 Sv was calculated using ICRP-30 or ICRP-60 dosimetry. | HELEN | |
| 4 | ii | | The report should state that the cancer risk estimates are actually cancer detriment estimates (7.3% per Sv is the ICRP-60 cancer detriment coefficient). | The report clearly states that these are cancer <u>incidence</u> risk | |
| 5 | ii | paragraph 1 | Switch "upper" and "lower" (i.e., the 5E-5 is the upper bound 95% confidence interval, not the lower). | accept - change made | |
| 6 | ii | paragraph 4 | Check the value of 3E-8 ($1.8E-5 \text{ Sv} \div 6.2^{1.96} \times 7.3E-2 \text{ per Sv} = 4E-8$). | ok | |
| 7 | ii | | The text should briefly state why the Phase II release estimates were so much higher (i.e., use the text on page 6). | It is stated that ChemRisk estimates were based on calibration to environmental measurements in vegetation. | |
| 8 | ii | | The text should specifically state that ChemRisk ran INPUFF, in order to avoid confusion with the text on page iii on Environmental Transport Modeling (same comment applies to page 4). | accept - change made | |
| 9 | General | Executive Summary | Although this is an editorial comment, I suggest not using acronyms in the Executive Summary (for readability). | ignore | |
| 10 | v 44 | 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 1.6E-5 and a 2.5% probability that the risk is below 5.8E-7. Also, it should be stated that the uncertainty does not include the uncertainty in the cancer incidence risk coefficients, except for particle size. | accept - change made | |
| 11 | v | | On page iv, you have already stated that the exposure scenarios are meant to provide a range of potential lifestyles, so the last paragraph on page v is superfluous and could be consolidated with the text on page iv. | ignore | |
| 12 | 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 | |
| 13 | 4 | line 7, 9 | I suggest defining the particle size associated with "coarse particles" and "fine particles." | The particle size used in the calculation was not reported - only the deposition velocity. For coarse particles, a deposition velocity of 18 cm/s was fit based on plutonium concentrations in vegetation | |
| 14 | 6 | paragraph 6 | If the particle sizes of the Pu is not known, what is the basis for assuming that the size ranged from 1 to 10 μm , other than overestimating risks? | | |
| 15 | 8 | line 2 | Convert 3.3 miles to km. | accepted - change accepted | |

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| Reviewer: | S.J. Maheras | Date: 11 June 1999 |
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| 16 | 7-8 | | Ambient Air Monitoring. It appears that the whole point of this section is to say that the ambient air monitoring data are of no use in validating model predictions. Therefore, I suggest deleting the bulk of this section. | I do not agree. It is important to understand what measurements were made the limitations of those measurements. | |
| 17 | 10 | lines 2-3 | I suggest quantifying what is meant by a large prediction uncertainty is, i.e., "on the order of xxx." | accept - change made | |
| 18 | 10 | paragraph 5 | The temperature of the WVTS release should be stated. | This detail is found in Rood 1999. | |
| 19 | 10 | last paragraph | I suggest combining the first two sentences in this paragraph. | ignore | |
| 20 | 11 | paragraph 2 | If the release duration of the WVTS was 11 hours (page 10), then why is 9 hours the appropriate time interval? (why not 11 hours?) | It is also stated in the same paragraph that releases were for 11 hours, but measurements were only made for the last 9 hours of the release. | |
| 21 | General | | Sometimes INPUFF is referred to as INPUFF and other times it is referred to as INPUFF2. This should be made consistent. | accept change made | |
| 22 | 10-13 | | 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? | |
| 23 | 14 | table 3 | Under plume rise, I suggest mentioning that Brigg's equation includes momentum and temperature. | This is clearly stated in the text | |
| 24 | 14 | | The difference between the environmental and concentration grids should be briefly stated. | This is stated in the 2nd paragraph of the model domain and receptor grid section | |
| 25 | General | | Sometimes "Cleere" is spelled "Cleere" and other times it is spelled "Cleare." This should be made consistent throughout the document. | accept - change made (it should be Cleare) | |
| 26 | 16 | paragraph 2 | If the operation of the tower began in 1984 and was collected according to strict QA standards, then why was data from only 1989-1993 used, i.e., why wasn't all the available data used? | 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 | |
| 27 | 15-17 | | Meteorology. The case made for using the March 20, 1970, data is not a compelling one. While I do not disagree with using this data, the justification should be expanded. | The justification is simple - there are no other data available for the RFP at the time of the fire. This is clearly stated in the text. | |
| 28 | 17 | | Data Processing. It is not clear what data were used and processed. At some points, it sounds like just the Cleere/Cleare data were used; at other points, it sounds like the Denver airport and 1989-1993 data were also used. This should be clarified. If all three data sets were used, then what each data set was used for should be specified. | The reviewer apparently does not understand how a puff dispersion model works. Both the Denver and RFP data sets were used. Wind speed and direction were interpolated at each node using a 1/r2 routine. | |

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| 29 | 21 | | On page 21, the report implies that the particles are known to be less than 10 µm, while on page 6 this is listed as an assumption. This should be clarified. | | |
| 30 | 19-22 | | Dry Deposition and Gravitational Settling. I suggest discussing the relationship between AED and AMAD in this section. | ignore | |
| 31 | 24-26 | | Plume Rise. If the material on plume rise is presented in the RACHET or INPUFF manuals, I suggest just summarizing the results (e.g., keep the last paragraph in the section and Table 5, delete the rest). | That would be fine - but other reviewers insisted on more detail | |
| 32 | 27 | table 6 | 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 | |
| 33 | 29 A-1 A-2 | table 7 table A-1 table A-2 | 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. | |
| 34 | 32 | figure 8 | The legend on this figure is truncated on the right hand side. | accept - change made | |
| 35 | 33 | figure 9 | In the text on page 31, the units of the concentration are pCi-h/m ³ , but in the figure the units are Ci/m ³ . | The text was clarified to differentiate between average concentration and time-integrated concentration | |
| 36 | 33 | figure 9 | This figure needs additional explanation in the text. For example, it appears that the plume was lofted and did not touch down until about Arvada. This phenomenon should be discussed in the text. | Additional text was added | |
| 37 | 34 | table 8 | Based on the text on page 35, it is implied that the data in this table include the maximum ground-level air concentration at the specified probability levels. If this is correct, then it should be discussed in the text. If not, then the maximum ground-level air concentrations at the specified probability levels should be presented. | The maximum concentration in the model domain at the 95% level is referenced back to Table 8 | |
| 38 | 36 | table 9 | 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 | |
| 39 | 41 | table 12 | A reference should be provided for the dose conversion factors presented in this table. The absorption type and age should also be noted. | HELEN | |
| 40 | 43 | table 13 | 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 | |
| 41 | 40-43 | | 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 | |

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| 42 | 44-45 | | Lifetime Cancer Incidence Risk Estimates. The comparison to the Phase I results should be a separate section so that it does not detract from the results presented in the report. | ignore | |
| 43 | 41-47 | tables 12, 13, 14, 15, 16, 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." Also, in Tables 15 and 16, leukemia is spelled incorrectly (i.e., leukimia). | accepted - changes made | |
| 44 | 46-47 | tables 15, 16, 17 | I suggest presenting the results in these tables as graphics—the tables do not do justice to the analysis. | How do present orders-of-magnitude differences when zero is included in the distribution? | |
| 45 | General | | If it is not too large, I suggest including the Cleere/Cleare letter as an appendix to this report as it is an important data source. | ignore | |