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Review of FY 99 Report: Air Transport and Deposition of Actinides at the Rocky Flats Environmental Technology Site

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1. Introduction

This report addresses emissions and subsequent atmospheric transport and dispersion of actinides that exist in the environment at Rocky Flats. Background information is provided on the history of the material and how it came to be in its present distribution in the soil and vegetation. Also the report describes the effects of weathering leading to the present status of plutonium being physically bound to soil grains, some of which are deposited on vegetative surfaces. This background material sets the stage for evaluating the likelihood and extent of resuspension and subsequent windborne spread of actinides.

A well written review of past resuspension research provides the reader with an adequate understanding of how the research community has developed methods to estimate wind erosion in agriculture and radionuclide resuspension. The work of Gerhard Langer at the Rocky Flats site itself has provided a good basis for estimating future windborne emissions.

Section 3 of the report describes the modeling work performed as the centerpiece of the year's work. This includes a rationale for the selection of computational frameworks for source strength (natural resuspension, wildfire emissions, anthropogenic effects), atmospheric transport and diffusion, and dose calculations. Also, the section discusses preparation of model input data files and the selection of important empirical parameters. Section 4 compares model estimates with monitoring observations. As a reviewer I was very pleased to see this component of the report.

2. Reviewer's Observations

The report has been well edited and reads very smoothly. In order to establish some critical standards that would be useful to the authors, I chose to:
1) attempt to reproduce some of the calculations and/or apply the model suggestions
2) review the package of reference material that was generously supplied.

This helped me to find possible gaps in the discussion and formulate questions for the authors. In this section I will present the observations and questions. The next section will seek to integrate and summarize.

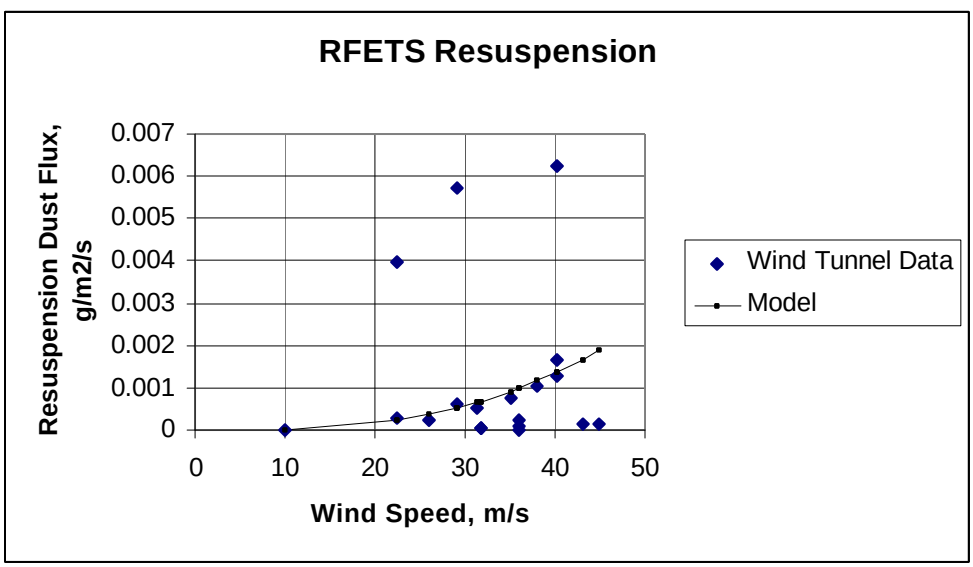
a. Sec. 2.3.3 The last paragraph on p. 2-16 is pretty important regarding the physics of the process. Langer found a strong dependence of resuspension rate on time during the first several minutes. This suggests that the soil somehow adjusts to the wind forces to reduce the resuspension potential by streamlining itself relative to fixed obstacles. This says, in turn, that the natural process is driven most strongly by gusts with duration of a few seconds to a minute or more. Notice that a surface that has adjusted to gust A will be vulnerable to enhanced resuspension from gust B that may have a slightly different direction. The wind

tunnel cannot capture this physics and we need to be careful in extrapolating to hourly rates from short-term wind tunnel observations because we may <u>underestimate</u> the resuspension rate.

I tried to give some thought to methods of in situ resuspension estimation. An elastic backscatter lidar will capture non-uniform particle fields with resolution of a meter or so over areas up to 10's of km^2. Prof. Bill Eichinger at Univ of Iowa. Uses Monin-Obhukov parameterization to estimate vertical flux from lidar-observed gradients and fluctuations. These methods would supply resuspension estimates. The cost may be a factor in planning but discussions with Dr. Eichinger would help frame the feasibility.

b. The wind field model parameter that expresses the surface wind stress is u* and it's worth some sidebar consideration of the properties of u* and its micrometeorological foundations. In the profile models it's proportional to the mean wind and the sidebar may end up being that simple. However, horizontal homogeneity probably doesn't prevail at the site and some good u'w' covariance measurements might be extremely valuable to understanding time space dependencies of resuspension.

c. The text surrounding Table 2.2, Figure 2.3, and the equation on p. 2-18 was somewhat confusing and I set out to sort it out. I plotted the values from Table 2.2 and the equation and got a plot that looks just like Fig 2.3 with the equation representing the upper envelope of points (except for he east field data that is probably abnormally high). My figure satisfied me that the equation is conservative and would be much easier to discuss. Incidentally, the third power dependence on wind speed (read u_*) seems somehow pretty fundamental and is consistent with Gillette's results. Some brief fiddling with equations didn't offer any breakthroughs. Sehmel (1984) presents a range of power dependencies on U from 1.0 to 4.8 depending on particle size but the ensemble fits a U^3 relationship quite well. The equation on 2-18 is presented suddenly without description. This presentation of the data makes the equation pretty obvious EXCEPT here I used the coefficient $2x10^-8$ rather than $2x10^-9$ as in the report. Check what you're using in your calculations.



d. Sec. 2.3.4 addresses dust resuspension in $gm^{-2}s^{-1}$ with a suggestion that Pu concentrations at S-107 look similar. No method is given for calculating Pu resuspension. Later in 3.2.4 it says that actinide emissions were estimated in section 2. This looks like a discussion that was dropped between 2.3.4 and 3.2.4.

e. Sec. 3.2.4 dwells on a procedure for calculating the amount of Pu in the soil but the concept is never expressed. This is important because the emission is estimated as a resuspension rate times the soil concentration. The section would be improved with a contour map of observed soil Pu. and a paragraph emphasizing the concept of emissions being the area integral of soil Pu times the resuspension rate. It can be expressed as an equation. Then the last paragraph on p. 3-7 takes its place as just a reliable method of evaluating he integral. Incidentally, this is a good place to take credit for using the theoretically favored resuspension *rate* rather than other parameters, e. g. the resuspension *factor* to estimate the emission. Horst (1981) as well as others points out the benefits of the approach used. Further, a good soil contamination map is at the heart of the method and that's a strength of the working data set. It's good to lead with this strength. There isn't enough information in the report to independently estimate the source strength, except to back-calculate from the concentration fields. When I do that with some pretty gross estimates of annual plume parameters I get an average source strength of about 0.1 to 10pCi/s. Is this close to the value used?

The larger estimate would be equivalent to about .4Ci/yr, or 3% of the total inventory, mobilized and transported to the fence line each year. The surface pattern of soil contamination would be expected to change measurably over a few years. At the lower end of the range we wouldn't see much change in the pattern.

f. Selection of Dispersion Model

The selection of a dispersion model makes very good sense. There are many models to choose from and selecting from the EPA suite of products can head off some controversies that can be distracting without adding any essential value. As pointed out in the report, ISCST3 has important routines for dry deposition and irregular area source configurations. The selection of the short term version that builds upon 1-hour basic time steps is also wise since the source is so dependent upon episodic events such as strong wind speeds. Also, in addition to annual averages, subsequent analyses can focus on case studies without changing the model framework. The EPA provides technical support for the software.

g. Meteorological Input Data

The preparation of meteorological inputs described in the report are conventional and for the assessment as carried out, I believe, adequate. There are a number of places in the report where a compromise was apparently made due, in part, to a lack of some essential micrometeorological data. ***Future monitoring programs will benefit greatly from a modest but well designed micrometeorological measurement program.*** I would see a baseline program to include one to

three 10-m masts instrumented at three to five levels with micrometeorological sensors (e. g. bivanes and thermometers or sonic anemometers at 0.5, 1, 2, 4, 10-m). A good way to start would be to have a guest team with good micromet credentials set up for a field campaign, perhaps in support of the upcoming controlled burn. Based on what is learned from a campaign design decisions for a fixed network could be made. Some folks whose micromet expertise I trust are Chris Doran, PNNL; Tom Horst, NCAR; Ray Hosker, NOAA ATDD. Consider hosting a 2-day workshop to get the benefits of perhaps 8 to 10 people's experience.

Section 3.2.2 describes data inputs to calculate dry deposition in ISCST3 that would be provided by the micromet data plus a basic radiometric suite (e. g. net radiometer, rotating shadowband short wave radiometer, downwelling longwave radiometer). On site real time observations of –L, u_*, z_0 would be far superior to parameters provided by a package such as MPRM based on remote observations.

h. Wildfire Emissions

I became curious about the emissions in a wildfire compared with the chronic resuspension source. Section 2.4 provides the formulae for making this estimation for the amount of soil material. Choosing mid-range values for the parameters in section 2.4 gives a release of plant-borne soil particles of $5x10^{-4}$ $gm^{-2}s^{-1}$. To transform that to Pu released requires an estimate of the Pu activity in soil. There are probably data sets of this quantity but I estimated it quite crudely from values given in the report and got a range from 5 to 100 pCi Pu/gram of soil. A modest fire that burned for 1 hour and consumed 1 hectare would release a range from .0001 to .002 Ci. In both cases this would be 2% of the chronic annual release. A 10 hectare fire would release 20% of the annual chronic emission.

i. Data Comparison

Section 4, which includes comparison of model results with monitoring data is a strong and very positive contribution to the report. In particular, the monitoring stations in the buffer zone and at the fence line show that the model estimates are high by a factor of 18 at the buffer zone and 19 to 75 at the fence line. The high model bias is conservative on the safe side and the authors have plans to study mechanisms that would reduce the bias and get closer agreements by a factor of 5 or so.

In reviewing the data I found an apparent discrepancy between samples at S-007 and S-107 that requires explanation. The two are essentially co-located yet S-007 was 17% lower than S-107 on the annual average. If we group the S-007 (monthly) data to estimate the same quarterly basis as S-107 we see that the disagreement was dominated by the period of Oct-Nov, 1996 when S-007 values were less than 50% of S-107 values. This large difference seems to push the explanation based on sampler design given in Section 4.3.1.

The two fence line samplers exhibited different model/observed ratios. That's not a problem but it gives one pause to think about why. Notice that the annual observed concentrations are within 10% of each other so the bias difference is in the model. Figure 3-4 shows that the two sites are on the edge of the modeled plume. With the corrugated terrain in that portion of the fence line

it's quite possible that the model underestimated the dilution effects of Woman Creek and Smart ditch ravines. The authors are better equipped than I am to explain this phenomenon. If the overestimate model bias increased from the buffer zone to the fence line it would support an argument for the importance of plume depletion.

j. Sensitivity Analyses

A useful part of Section 4 is the sensitivity analyses to test:

1) effects of large areas of low soil contamination… (small)
2) applicability of Langer's constant site resuspension rate…(worth more study)
3) plume depletion…(important, factor into future work)
4) statistical dependence of enhanced resuspension on strong westerly winds but not on rainfall. Some features of this result piqued my curiosity. Why the dependence on wind direction? Is that where the strongest winds come from or is the wind gustier in that quadrant? The dependence on monthly mean winds would probably be greater if account were taken of the probability density of winds (gusts) greater than some expected threshold values. This would require a different sampling protocol to work with at least hourly data but could be done on the basis of *an imbedded series of case studies.*

3. Integration of Comments and Conclusions

The most important fact about the report, Air Transport and Deposition of Actinides at the Rocky Flats Environmental Technology Site, is that it describes an assessment effort that was carefully designed and conducted. Appropriate physical processes were considered in model selection and application. When uncertainties were irreducible, safe-side conservative choices were made for model parameters. The work described in the report forms an excellent stepping off point for the next phase of study and assessment.

The crucial issue addressed is the rate of released material. In the 1999 report the focus was on natural chronic resuspension and spread of actinides already in the environment. The uncertainty of the resuspension flux appears to be a factor requiring more study. The authors were able to select a conservatively high estimate so that safety is not compromised. The 1999 report introduced the concept of releases due to other mechanisms in preparation for future studies. These include wildfires and the post fire environment, and anthropogenic surface disturbances.

Many of my comments in section 2 of this letter report are simply the result of following thought processes introduced by my reading of the report. I sought to reproduce calculations, look for consistencies (internal and with the literature), and tried to scope the magnitudes using the "big picture" perspective. I chose to share those comments so that the authors could see how the report looked to an independent reader.

I recommend that future studies be supported with additional data collection, in particular a modest micrometeorological monitoring system. A well-designed micrometeorological data set will help eliminate compromises in model implementation. All the support data will be available to run the most appropriate models in each case.