General

Los Alamos
NATIONAL LABORATORY

Environment, Safety, and Health Division
ESH-17, Air Quality Group
P.O. Box 1663, MS J978
Los Alamos, New Mexico 87545
(505) 665-8855/Fax: (505) 665-8858

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Date: September 20, 2001
Refer to: ESH-17:01-437

Barbara Hoditschek
NMED DOE Oversight Bureau
2905 Rodeo Park Drive East, Bldg. 1
Santa Fe, NM 87505

Dear Barbara:

Below are the comments on Technical Memorandum, "Analysis of Exposure and Risks to the Public from Radionuclides and Chemicals Released by the Cerro Grande Fire at Los Alamos", Task 1.3.

General Comments:

Commenter: Tom Buhl, ESH-DO LANL

General: Both the reports being reviewed here were informative and well-written. They successfully integrated and organized a large amount of data taken by a number of organizations, which is a significant accomplishment.

Comments on Task 1.3: Analysis of Potential Releases of Radionuclides and Chemicals to Air

Commenter: Jean Dewart and Craig Eberhart, ESH-17 LANL

1. Page 14. It would be of interest to list the magnitude of conservatisms used in the screening process. A back of the envelope estimate would be a factor of 1000. If so, the list of chemicals/radionuclides to be considered from the air pathway would drop from 68 to about 26.

***RAC* response**: We have reduced the level of conservatism in the revised screening. This served to reduce the number of contaminants that are considered to be a priority. It was never our intention to quantify the conservatisms in this calculation – screening is a conservative process and is only intended to remove contaminants from consideration that are clearly not of interest from a risk perspective.

2. Page 16. It is difficult to understand the assumption of an "exposure duration to the concentration of 30 years" when the exposures took place over approximately 2 weeks.

13566

***RAC* response**: Based on a number of comments, we revised the exposure duration to 14 days. We had originally used the 30 years exposure period because it was consistent with the exposure period for which the risk values were developed for chemicals. These results are shown in the Task 1.4.2 report.

3. Page 17 - Second paragraph under DATA LIMITATIONS: In my opinion the first sentence in this paragraph is incorrect. Samples were collected from most of the 50 sites for 100% of the time during the fire. The quarterly concentrations included all of the short-term samples and showed no unexpectedly high concentrations. The data were not compromised as stated.

***RAC* response**: We were restating what we had inferred from discussions with LANL personnel about the air monitoring data. We will, however, change the sentence to more accurately reflect our concerns about the lack of information available from the air monitoring data.

4. Page 18. The statement is made, "Concentrations of radionuclides and chemicals measured in air probably do not represent the maximum concentration within the model domain." Although this is a true statement, in the sense that this is true of any monitoring network, the monitoring data do bound the question for pollutants that were monitored. Although individual sample volumes were smaller than desirable, sample volumes over the quarter including the fire were comparable to routine sampling - thus if there were high levels of contaminants in the air during the fire (excluding naturally occurring radionuclides), high sample concentrations (e.g. pCi/sample) would have been measured - this was not the case.

***RAC* response**: We agree that the monitoring provides information about the contaminants for which measurements were made, and that the samples summed over the quarter provide some information abou what contaminants were not measured at the LANL boundary locations. It is possible, however, that maximum concentrations existed elsewhere within the model domain. We were merely stating this possibility.

5. The statement is made, "Because air concentration data exist for some chemicals and radionuclides, we will use that information to determine predicted concentrations throughout the model domain using our air dispersion model." As you know, there may be some problems with these comparisons - some of the air monitoring stations were probably impacted by wind re-suspension of particles outside the burn area. These data may be difficult to match up with pollutants carried aloft by the fire produced plumes.

***RAC* response**: As we move through this project, the complexities associated with using the data in more traditional ways become ever more apparent. In Task 1.4.2, we calculated air concentrations of contaminants that would have been released during the fire and discovered that the air monitoring data could not be used to validate this modeling. We speculated in that report that the increased concentrations measured in air were a reflection of resuspension from areas that did not burn, which is consistent with this comment.

6. Page D-2. The statement is made, "The modeled concentrations of PM compared well to the measurements taken during the fire." A phrase should be added, "with the exception of the measurements made close-in to the fire at TA-54." This may be a fairly important observation - because the very high levels of PM were measured near the fire - and these are difficult to model.

***RAC* response**: The concentrations of PM measured near TA-54 were actually quite helpful in developing the model of the fire. The results of this modeling are more completely discussed in Task 1.4.2

7. Page D-2 . There is a concern about modeling the fire against PM-10. Visibility during the fire was very close to zero near to the fire (from fire fighter reports), and so concentrations of TSP would have been highest close in. The first modeling studies show maximum PM-10 concentrations near San Ildefonso. How will the distribution of contaminants by particle size be addressed?

***RAC* response**: We discuss this modeling effort further in Task 1.4.2.

8. Page D-2, Table D-1. a more appropriate reference for these data is Jackie Hurtle, Environmental Surveillance at Los Alamos during 2000 (draft).

***RAC* response**: Although we agree that this is a more appropriate reference for these data, it is not the reference from which we obtained our information. This reference was not available to us when we drafted this document, and we had to obtain the information through other means.

Commenter: Laars Soholt, ESH-20 LANL

1. It appears that the screening for PRS entrainment in ambient air used release and exposure periods that differ among the toxicant categories. For radionuclides, by back calculation I deduced that the receptor is exposed for 2 weeks to the equivalent of a concentration derived from a 1 day release period. For non-radionuclide carcinogens, receptors are exposed for 7 years to the equivalent of a concentration from a 2 week release. For non-carcinogens, both exposure and release periods are 2 weeks. I am not sure where the exposure duration fit into the reference concentration calculations, since IRIS RfCs assume lifetime exposure for the most part. For measured concentrations the exposure periods are 30 years for radionuclides and 7 years for non-radionuclide carcinogens. I cannot help but wonder if the screening results might have been different if comparable release and exposure duration had been used for all three categories, particularly in the cases of those substances where RI or HQ falls near the criterion value.

***RAC* response**: Due to the many comments on this issue, we have now completed the screening using both 30-year exposure periods and 2-week exposure periods, and we used the 2-week exposure period for the screening calculations to determine the contaminants of highest priority.

Commenter: Tom Buhl, ESH-DO, LANL

1. This report appropriately developed screening levels for radionuclides and chemicals in order to eliminate those that present negligible risk and concentrate on the more important materials.

***RAC* response**: Thank you.

2. The screening levels were based on a pure inhalation exposure. This does not take into account other exposure pathways from airborne materials, such as deposition on and ingestion of foodstuffs. Inhalation may be the most important pathway, but the other pathways should be discussed in order to show that they are small relative to inhalation. This is especially important because of the public concern over materials deposited on gardens in northern New Mexico.

***RAC* response**: Because of the nature of the fire, it was difficult to predict dispersion and deposition patterns until later in the course of the work. In the report that discusses the risk from the fire, we will discuss the risk from deposition of airborne contaminants.

3. A large effort is being made in the report in evaluating LANL emissions through investigation of the PRS data base. Naturally occurring and fallout radionuclides may be just as important in defining the risk from the Cerro Grande fire. The report did not discuss how source terms for these radionuclides and chemicals, which would not be obtained from the PRS data, would be developed. Including non-LANL sources in the report is essential in order to provide perspective to the public on the risks involved.

***RAC* response**: We will primarily discuss risk from fire-derived components in a qualitative manner. When possible (such as when air concentration data for such contaminants are available) we will address risk more qualitatively.

4. Equation (6) on page 12 of the report does not contain the exposure time, which would be needed in order to calculate the total intake.

***RAC* response**: For the case of radionuclides, we calculated a time-integrated concentration, using the first part of Equation (6) (inventory · chi/Q). This time-integrated concentration eliminates the need for use of an exposure time.

5. Figure 5 on page 12 should indicate at what distance the X/Q's were calculated.

***RAC* response**: The X/Qs were calculated at 100 m. We will ensure that this is clear.

6. The report seems to prefer source terms calculated from the PRS data base and subsequent meteorological modeling to air sampling data. While this approach can be comprehensive in that it can consider many more radionuclides and chemicals than obtainable from the air sampling, the uncertainties associated with this approach may be considerable. The PRS data was not collected in order to estimate source terms, and obtaining a source term from this data – especially estimating the release fraction - may be difficult. Modeling the dispersion during highly turbulent fire conditions in the complex terrain around Los Alamos is also difficult. This implies that there will be a high degree of uncertainty in the modeled results.

   I would recommend that the report indicate that it will review the modeled air concentrations at each air sampling location, and either require that modeling results agree with the measurements (within the uncertainties of each method), or review the two methods and explain why a discrepancy between the modeled and measured results is acceptable.

***RAC* response**: This process is better explained in Task 1.4.2, and so we refer the reviewer to this report.

7. Assuming 30 years of exposure to the maximum radionuclide concentrations (p. 16), and 7 years for chemical concentrations (p. 14) seems excessive even for a screening calculation.

***RAC* response**: Due to a number of comments on this issue, we have revised the screening calculations. We will present results for both 30-year and 14-day exposure periods, and use the 14-day exposure period to determine contaminants of concern from a risk perspective.

Risk Assessment Corporation
417 Till Road, Neeses, South Carolina 29107
phone 803.536.4883 fax 803.534.1995
www.racteam.com

November 6, 2001

Mr. John Parker
Chief
New Mexico Environment Department
Dept. of Energy Oversight Bureau
2905 Rodeo Park Drive East, Building 1
Santa Fe, NM 87505

Re: Subject: Comments on Contract Number 01-667-5500-0001, Deliverable 2, Tasks 1.3 and 2.1

Dear Mr. Parker:

Please find below our responses to the comments of the New Mexico Environment Department DOE Oversight Bureau on contract deliverable 2, which were summarized in your letter dated October 22, 2001. For clarity, the original comment is printed and our response follows.

Please do not hesitate to contact me if additional discussions are required.

1. How will the assessment address the question of what are the risks to the public posed by the forest fire itself? This would presumably include development of an estimate the amount of contaminants released by the 50,000 acres of burned forest. Such an estimate was not included in Deliverable 2, Task 1.3. While we acknowledge the reason the assessment was commissioned is due to concerns over releases from LANL, we further discussed the need for an assessment of the whole fire to provide necessary perspective.

   An estimate of the amount of contaminants released by the burning forest should be developed. The procedure might incorporate information from the literature about releases from other fires, or might apply knowledge about the mechanisms of uptake and distribution of chemicals on the surface and in vegetation.

RESPONSE

This is a difficult problem because developing a comprehensive source term for combustion products of the fire is a separate project in and of itself. There are numerous chemicals and radionuclides (both naturally occurring and man-made) in vegetation and to address every one is beyond the scope of this project. We already have release estimates and atmospheric concentrations for some pollutants emitted as a result of the fire that were calculated in the Emissions Production Model (EPM). These pollutants are PM-10, PM-2.5, CO, CO_2, and CH_4 These products are important for air quality and visibility issues, but may not present long-term health impact issues.

We believe it is important to put potential releases from LANL facilities into perspective. There are some methods we could apply for a few radionuclides and chemicals where estimates of the mass or activity of contaminant or radionuclide per kg of biomass exists. Fuel loading rates that were used in the EPM calculations would then provide a means to estimate total inventories of contaminants available for release. Assuming the release is proportional to one of the pollutants modeled in EPM, and that contamination is uniformly distributed over the model domain, air concentrations could be estimated by a simple ratio between the contaminant release rate and the pollutant release rate calculated by EPM.

Currently, we have committed to addressing fire-related pollutants on a qualitative basis. Any kind of quantitative assessment such as described earlier will require additional investigation as to whether data exists to make such a calculation.

2. A related question involves public concerns about broadly dispersed contamination in the environment of the Pajarito Plateau and Jemez Mountains due to fifty years of operations at LANL. If there is broadly dispersed contamination, the Cerro Grande fire could have released contamination different from other forest fires. How will the assessment address this?

RESPONSE:
This is a complex and extensive question to answer and, based on our experience, would require considerable effort and time to answer comprehensively even if the necessary data were available. As discussed above, we will consider a qualitative analysis of the fire based on readily available data regarding radionuclides and chemicals present in background environments.

3. During the fire a number of LANL structures burned. While we acknowledged in our discussions that an estimate of risk resulting from the burning of townsite residences is not a requisite part of this study, we do ask how the assessment will address potential risks to the public resulting from releases from the burning of structures and their contents that are located on LANL property.

RESPONSE:
We will inquire what information is available regarding the contents of the buildings that burned on site. If the necessary information is available, we can include it in our assessment. If the information is not available or if the structures did not contain hazardous materials, there is no way to address them separately in our analysis.

4. Deliverable 1.3 does not clearly provide an estimate of the amounts of radionuclides and chemicals released by the fire.

   According to the Workplan, Task 1.3, "The goal of this task is to estimate the amounts with uncertainties of radioactivity and chemicals released to the air during the fire." The Air Pathway Risk Analysis flow chart (Figure 1) indicates that the purpose of this deliverable is to "develop release estimates (source term) using available air and biota monitoring data." The text discusses this further: "The objective of this technical memorandum, as laid out in our work plan, was intended to document estimates of the amounts, with uncertainties, of radionuclides and chemicals released into the atmosphere during the fire (i.e., the source term)." Based on this, we expected this task to produce an

estimate of the amount (mass in grams or activity in Becquerels) of materials that were released to the air.

Please explain how *RAC* plans to estimate the amount of contaminants that were released into the atmosphere.

RESPONSE:
This is a true statement and we fully intend to provide this information in the revised Task 1.3 report and in Task 1.4.2 on atmospheric concentrations of contaminants.

5. The deliverables do not clearly indicate how the monitoring data will be used. The case was presented that the data cannot be used as originally envisioned. However, the flow charts indicate that the monitoring data is of some use, and we feel that the monitoring data should as a minimum be used as a check against the model.

RESPONSE:
It is difficult to draw any definite conclusions from the monitoring data because

- concentrations were below analytical uncertainties
- measurements were made at random locations and times making it difficult to establish spatial and temporal tends that are essential for model calibration and validation.
- Information about background concentrations is limited. This is essential for any kind of model validation or calibration.
- Sampling dates, times, and locations are uncertain.

There is one exception and that is PM-10. Most PM-10 monitors provided a good time history of the PM-10 concentrations before, during, and after the fire. These data provided the basis for establishing background and to observe any increase as a result of the fire. The data at TA-54 provided the clearest signal and the most complete time record and were used, along with other stations in the model domain to calibrate the model.

We will use whatever other data that are available as a check of our model but it may be difficult to draw any conclusions from these data for the reasons stated above.

6. We understand that to make the risk assessment manageable, it is necessary to screen (eliminate from consideration) some contaminants, based on estimated negligible contribution to risk. Our understanding of the process of screening and the development of a release estimate might include the following elements:

   a. Consider all contaminants that could have been released or contribute to risk, produced either by LANL operations and deposited in or on the soil at known contaminated sites, the burning of LANL structures, the burning of nearby contaminated land, or the burning of natural forests.

   b. Evaluate the possible risk from these contaminants using conservative assumptions.

   c. Eliminate those materials that make negligible contribution to risk.

   d. Estimate the amount of the remaining materials that were released. This release estimate is the <u>source term</u>.

According to this process, two release estimates would be developed: (1) a high estimate for screening and (2) a realistic estimate for the source term.

RESPONSE:
We have or will provide these values as stated above in the revised Task 1.3 report, except for the following for the reasons stated above.

- release estimates resulting from deposition of airborne contaminants on surrounding lands from 50-years of LANL operations
- release estimates from the combustion of LANL structures, except in cases where known contaminant inventories existed in these structures
- release estimates from the fire itself except for release of CO, CO_2, PM-2.5, PM-10, and CH_4, and a few other contaminants if concentrations in and on biomass are available

Other Specific Comments:

Page 5. Figure 2. For the high estimate of how much was released (for screening purposes), we recommend not eliminating PRSs that have been proposed for no further action. Very few PRSs have actually been cleaned up.

RESPONSE:
No PRSs that have been proposed for no further action were eliminated in the screening process. Five PRSs (09-010(a), 22-014(a), 60-007(b), 69-001, C-12-004) were eliminated from our air source term development in the screening process because excavation or backfilling had occurred since collection of the sampling data, and because the characterization data were no longer considered representative of existing site conditions.
We will revise Figure 2 in the Task 1.3 report to reflect the procedure used more closely.

Page 8. There should be another Appendix listing contaminants that are released during any forest fire (not near a nuclear facility).

RESPONSE:
Appendix D of the revised Task 1.3 report will discuss and list components of smoke from forest fires.

Risk Based Criteria, pages 8 and 9. It is not clear if the risk values discussed are for the study as a whole or for each individual carcinogen. For individual contaminant screening purposes, we recommend that conservative risk values (10^{-6} RI and 0.1 HQ) be used.

RESPONSE:
Risk values are for each individual carcinogen. We agree that a risk index of 10^{-6} or a hazard quotient of 0.1 is appropriate for screening realistic risk estimates. However, given the many conservatisms built into our screening calculations we consider a risk index of 10^{-5} and hazard quotient of 1 to be more than adequate, even accounting for the additive risk from multiple contaminants.

Page 9. There should be another Appendix listing the calculated RfC_i values. Are other exposure routes (for example, dermal) also possibilities?

RESPONSE:
We are adding the RfC values to Appendix B in the revised Task 1.3 report. The dermal exposure pathway is not important for assessing risks from chemicals and radionuclides in air in this event.

Page 10. We recommend using the 95th UCL of the mean for a high estimate of the concentration of contaminants.

RESPONSE:
We have used average measured concentrations to determine the inventory available for release at each PRS, excluding all "nondetect" results. Doing this biases the average concentrations on the high side by eliminating the lower "nondetect" values from consideration. The process we are using to estimate source area concentrations and associated releases to the environment is conservative in many respects, and we believe the methodology we are using to characterize these source areas is adequately conservative. Furthermore, there is only one sample result for many analytes on which we can base our PRS characterization, so the 95th UCL would be impossible in these instances.

Page 10. The screen should be to a risk index of 10^{-6} or a hazard quotient of 0.1 to account for additive risk from multiple contaminants.

RESPONSE:
We agree that a risk index of 10^{-6} or a hazard quotient of 0.1 is appropriate for screening realistic risk estimates. However, given the many conservatisms built into our screening calculations we consider a risk index of 10^{-5} and hazard quotient of 1 to be more than adequate, even accounting for the additive risk from multiple contaminants.

Page 11. Even though less conservative, perhaps a more realistic release fraction such as 0.5 could be used.

RESPONSE:
There is much about the dynamics of the fire that remains unknown, making a screening level determination of release fraction is somewhat premature. Even within our calculation of source terms based on the revised screening results, we did not choose to select arbitrary release fractions but rather partition contaminants into volatiles and particulates, each exhibiting their own release characteristics. These results will be available in the Task 1.4.2 report.

Page 12, Eq. 6. What are the conversion factors if the contaminant is not a radionuclide, and the intake is not in Bq but in mg?

RESPONSE:
Equation 6 is never used for chemicals. Equations 8 and 9 show how risk is calculated for chemicals, using available inventory to calculate a potential concentration in air if the entire inventory were released over 14 days.

Page 12. For the screening and subsequent modeling, are you using one receptor or multiple receptors? In the final risk assessment, will there be one or multiple maximally exposed persons?

RESPONSE:

For the screening, we essentially defined one maximally exposed individual, but placed that individual at such a location for each separate PRS exposure calculation that it would be maximally exposed to each PRS, and the risks from that exposure were summed for each contaminant to produce a total risk to each contaminant. This is another example of the conservatism built into the PRS exposure calculations.

For the final risk assessment, we plan to define four scenarios that describe the range of exposures that occurred. These will be based on representative activity levels for various types of individuals because inhalation is the primary exposure pathway for atmospheric releases. The time-integrated air concentrations will be calculated throughout the model domain. For the risk assessment, the risk to the representative individuals throughout the model domain will be calculated.

Page 13, Eq. 7. How were non-radioactive carcinogenic chemicals handled?

RESPONSE:

In the paragraph below equation 7, we describe how the concentrations calculated using this equation are compared to the reference concentrations to compute a hazard quotient.

Pages 13 and 14. Exposure times should be standardized for the calculation of risk. The exposure times should also be consistent when using monitoring and modeling data or results. This is important to allow comparisons of the dangers from different types of contaminants (carcinogens, radionuclides, noncarcinogens).

RESPONSE:
For the revised screening report, we have standardized exposure times to reflect exposures during the fire, so we used an exposure period of 14 days for all PRS exposure calculations, except the radionuclide calculations, which do not require the definition of an exposure duration.

Page 17, Table 2. Why does tritium appear in Table 2, but shows a RI in Appendix C of 4.34×10^{-8}?

RESPONSE:
Our mistake. We were able to calculate risk from tritium 2 ways – using PRS data and using air concentration data. Tritium passed the screening using the air concentration data, and we neglected to include that screening value in our table. This will be corrected in the final version.

Page 19. Although Po-210, Bi-210, and Pb-210 may be naturally occurring decay products of radon, the risk from these and other naturally occurring contaminants should be considered. The risk from these contaminants may be the most significant risk to firefighters from chemicals and radionuclides. The other nuclides discussed to paragraph 3 on page 20, including K-40, should be retained, and only eliminated through the quantitative screening process.

RESPONSE:
We will consider the risk to naturally occurring radionuclides primarily in a qualitative fashion, but will be able to perform some quantitative calculations using available air concentrations measured during the fire. These will be the only quantitative risk calculations to these radionuclides that we will be able to perform within the scope of this work.

Page 25. "Uncertainties associated with the data in Table 5 were reported by the analytical laboratories as 2-sigma uncertainty." Were the uncertainties associated with the chemical data also reported as 2-sigma, or were they reported at all?

RESPONSE:
The EPA did not report variability or uncertainty values. The EPA reported results for single samples at each location with no standard deviation (n=1).
Appendix C. Several of the metals (manganese, aluminum, and barium) show up twice in the table. Is this correct?

RESPONSE:
Contaminants that appear twice in the table are contaminants for which risk was calculated using both PRS data and air monitoring data. We report all risk values, and their appearance twice is correct.

Appendix D, Page 5. Some metals and radionuclides were elevated in post-fire sediment. Isn't it possible that some of these materials were entrained in the smoke plume?

RESPONSE:
If sediment is the predominant soil at a PRS, or the soil of a PRS consists of sediment, then concentration in sediments were considered in our estimate of potential releases from the PRSs. Monitoring data suggest that the post-fire sediment samples with elevated levels of radionuclides may have come from the erosion of ash from burned areas. Consequently, these elevated levels of contaminants in the sediments would not have been in smoke produced by the fire days before.

Sincerely,

John E. Till

John E Till, Ph.D.

cc. Tim Michael, NMED DOE OB
Barbara Hoditschek, NMED DOE OB
Greg Lewis, Director NMED WWMD
Helen Grogan, RAC.

Review of RAC Deliverable Task 1.3
Evaluation of Surface Water Monitoring Data
8/10/01

The introduction states that this task analyzes only the risks associated with radionuclides and chemicals that may have come from LANL during the fire. The work plan states that this task will look at risks associated with these types of releases from the fire, which would seem to imply that the contribution from the combustion of the forest itself would be included.

***RAC* Response:** *From the start of this work, we have stated that we will consider the risk associated with releases from the combustion of the forest itself. It is not, however, reasonable to estimate a source term of combustion products in the forest. We will address this risk by using the measured air concentrations of combustion products, radionuclides and chemicals routinely measured in forest fires.*

The work plan specified development of a source term of contaminants that may have been released from PRSs using the monitoring data taken during the fire. RAC felt there was insufficient monitoring data to accomplish this task, so they developed a theoretical source term from which potential concentrations in the smoke were derived using the model. I believe there are some significant problems with their implementation of this approach.

The air concentrations were modeled assuming that all contaminants detected at all the PRSs in the burn area were released into the smoke plume with no regard to how heavily the PRS was burned or what depth the detection came from. Some materials from the PRSs were released as vegetation and duff burned and some amount of soil would be entrained in the rising hot air and join the smoke plume, but certainly not all of it. I think that this type of catastrophic inventory release model generates risk and hazard numbers far exceeding even the potential releases from these sites, and is really only useful for justifying why the sites were placed in the RCRA Corrective Action program.

***RAC* Response:** *The goal we hoped to accomplish in this report, given the late arrival of any data on which to base our calculations, was to complete an initial, conservative screening of contaminants to develop a prioritized list for further research. We fully intend to consider such things as intensity of the burn and depth of soil available for resuspension in our dispersion and risk calculations. Given that we could predict very little about what fraction of the inventory was released due to these considerations, it would have been premature to start applying qualitative factors in an attempt to rule out contaminants that may deserve additional consideration.*

For the analysis of monitoring data taken during the fire, the exposed persons were assumed to breathe the smoke for 30 years for radionuclides and carcinogens. The exposed person was assumed to breathe the smoke for 7 years for noncarcinogens. A chronic exposure such as this is at least a potential reality with exposures to sediment and water (where contaminants may actually settle and remain in the media), but is

nonsensical with respect to a smoke plume. Again, this assumption also results in the generation of extreme overestimates of risk.

***RAC* Response:** *We state several times in the report that we are not calculating actual risk or hazard to contaminants, but are using conservative assumptions to produce conservative risk indices and hazard quotients as a means of comparison to a predetermined risk or hazard level. This methodology ensures that nothing is eliminated prematurely in the study. Additionally, we are faced with the problem of how slope factors and reference concentrations for chemicals were developed. These risk and hazard metrics are based primarily on sub-chronic exposures of 7 years or chronic exposures of 30 years. Very little is known about acute exposures to chemicals, and the response cannot be considered to be linear.*

Bounding scenarios can be useful for eliminating potential contaminants, but this particular scenario has bounded right over the fence out of the realm of reality. While it may eliminate some potential contaminants, a more realistic set of exposure and release criteria would have eliminated more of the contaminants that don't represent a risk and provided risk screening numbers that were more valid for comparison with NMED target goals. This risk screen represents more of a weighted ranking of possible contaminants than a risk screening.

***RAC* Response:** *We chose not to use the word "ranking" because that implies that we would give more consideration to contaminants that "ranked" higher on the list. The singular goal of this initial conservative screening is to develop a list of contaminants for further study with regard to release fraction, burn intensity, depth profile, etc.*

I am concerned about the way in which these numbers were generated in part because each section of this study is being made available to the public. Without understanding the limitations of this initial screen, the public may take the numbers given in Appendix C as actual estimates of the risk to those exposed to the smoke from the Cerro Grande fire.

***RAC* Response:** *We firmly believe that the public will understand the principles that underlay the screening completed for this task. We explicitly state in the report on page 14, "Applying risk factors, slope factors, and reference concentrations to these very conservative estimates of air concentration from contaminant inventory provided a screening level estimate of risk index/hazard quotient. These values certainly do not represent the actual risk/hazard to this contaminant during the fire, but instead they provide a conservative evaluation of either potential risk index or hazard quotient by which to compare the impact of various contaminants." The public needs to be convinced that no contaminant is being eliminated without sound justification in this evaluation process. Our methodology systematically removes contaminants based on defined criteria and will continue to do so as we learn more about the nature of the contamination at the PRS locations.*

I've reread the section on screening of COCs with PRS data several times, and I can't follow how exposure time was incorporated into the calculation of total radionuclide intake. It seems that the model assumes a concentration based on suspension of the total inventory in the smoke cloud at a given point of time with a proportion of the smoke cloud being inhaled as the dose. I'm not sure why the modeled airborne radionuclide dose appears to be time-independent and the dose from monitored air concentrations occurs over 30 years. Since the model will be used again later to recalculate the potential dose using a fraction of the material from the PRSs, it's important to explain what length or type of exposure this represents.

***RAC* Response:** *For internal radionuclide exposure where acute effects are not considered, exposure time is irrelevant because it is the total intake of activity that is important. Risk coefficients developed in EPA's Federal Guidance Report Number 13 are in units of Bq^{-1}, which must be multiplied by the total intake of radionuclides over the exposure period. The total intake of radionuclides is calculated by multiplying the time-integrated concentration by the breathing rate.*

The location of the receptor in the air model is unclear; is this inhaled dose for a person standing at the PRS or in a particular location downwind?

***RAC* Response:** *The receptor is standing at the location of maximum exposure at a downwind distance of 100 m.*

Also, the Task 1.3 deliverable states that the air modeling of potential releases and air concentrations will be redone with better estimates of the fraction of contaminants released from the PRSs, but there is no indication of how this will be done. This step is crucial to gaining meaningful results from the air exposure modeling; I think RAC should provide the methodology for how they will do this to NMED before continuing with further work on this aspect of the risk assessment.

***RAC* Response:** *We agree that the step involving release fraction is a crucial step in this process. We are currently collecting data from LANL to support an identification of the form of the contaminants at the PRS locations, as well as compiling information about release potential, solubility, volatility, stability, and fate and transport for the contaminants. This information will all be applied, in combination with institutional knowledge, to develop quantitative release fractions.*

Recommendations:

At the progress meeting, RAC should explain that this is an inventory transport model and describe the assumptions included in that type of model.

We need additional information on length/duration of exposure represented in the model for radionuclides.

RAC should provide NMED with an additional technical letter or memo detailing how the source release fraction will be calculated and how this information will be incorporated into the air model. NMED should review this information prior to RAC continuing with the air pathway risk assessment.

COMMENTS ON TASK 1.3: ANALYSIS OF POTENTIAL RELEASES OF RADIONUCLIDES AND CHEMICALS TO AIR.

FROM: John Pinder

GENERAL COMMENTS

The report emphasizes the releases from the PRS (potential release site) locations and maps the location of these sites. It would be helpful to have documentation stating what fraction of the area of LANL is contained in PRS and what fraction of this area was burned. Furthermore, are there data on the concentrations of naturally occurring radionuclides in non-PRS LANL soils? This may be computable from the associated data files, but a simple mention in the text would be useful.

***RAC* Response:** Very little of the total area at LANL was burned during the Cerro Grande Fire. The total acreage at LANL that burned during the fire was about 3×10^7 m^2. Of that area, about 1.3×10^6 m^2 was made up of contaminated PRSs. So, approximately 5% of the burned areas on LANL property were contaminated with PCOCs. Additionally, there were 320 PRSs contained within the burned boundary, but only 189 (60%) of those PRSs actually burned. For our source term calculations for the air pathway, then, about 3% of the actual area that burned at LANL was considered as a potential source of contaminated release for the air source term.

SPECIFIC COMMENTS

Page 7

There are numerous discussions throughout this and other task reports emphasizing the conservative approach used in estimating risk, but I'm not sure that the elimination of analytes with measured concentrations below detection limits (< DL) is consistent with this conservative approach. If a PRS or air sample was submitted for analysis and a routine, standard suite of radionuclides and chemicals was automatically analyzed including some analytes for which there was no known or suspected source term for that PRS or air sample, then omitting those chemicals with < DL seems valid.

If, however, the selection of analyses performed was guided or controlled by information of known source terms or potential source terms of radionuclides and chemicals likely to be present in that PRS or air sample, then eliminating < DL data may not be conservative. If the analysis was performed based on presumed occurrence of the PCOC in the sample matrix, then it is conservative to presume that some amount of that PCOC is in the sample. It may be < DL, but it does not immediately follow that < DL means a risk < 10^{-5}. From a strictly conservative approach, where there is presumption of a PCOC occurring, then < DL does not mean no PCOC and does not mean no risk.

Where the analyses were guided by a presumption of a source term, there are several ways to consider the < DL number. One would be to simply assign the DL as the concentration and proceed with the risk assessment. A second might be to assign ½ the DL as the concentration, and proceed.

***RAC* Response:** We agree with the reviewer's statements that the selection of analyses to be performed on a PRS soil sample is important to understand to help determine whether or not eliminating less than detect values is indeed conservative. However, this type of information was not readily available, so there was no clear justification to assume some fraction of the reported detection limit as representative of a real concentration. Because the timing of delivery for the PRS characterization data did not enable a detailed investigation of the importance of nondetect values, we elected to eliminate those values. In addition, because of the extremely large number of potential contaminants and limited resources, it was important to adopt certain methodologies that would help focus and refine our assessment on those analytes that had been positively detected in contaminated area soil samples. We maintain that eliminating nondetect values biases our source term estimates on the high side and is appropriate.

For environmental monitoring data, the reviewer is absolutely correct in stating that a nondetect value does not necessarily equate to a risk less than 10^{-5}. For this reason, it is important to evaluate values reported as nondetected to determine what those values may mean in terms of risk. For example, there are several instances where analyses for certain chemicals in water samples resulted in nondetect values that correspond to high risk numbers. To the extent that resources allow, we are evaluating concentrations reported as nondetect values to understand the implications that current detection limits may have on our ability to make statements about risk. For the air pathway, we have elected not to take such an approach. The results of Task 1.4.2 seem to indicate that the majority of the airborne risk during the fire was not from burned PRS contaminants but probably from contaminants located in soils across the site that were resuspended during the extreme winds experienced during the fire. It is not prudent, then, to dedicate resources to reviewing the implications of the detection limits when it would certainly not impact the risk results.

Page 10

How where means computed for PCOC at a PRS when some of the analyses were < DL?

***RAC* Response:** Means were calculated using the remaining sample values above the detection limit.

Page 13

Why were the PCOC assumed to be released over a two-week period? The two-week extent of the fire is not a two week extent of fire on LANL is it? And, it is the extent of the fire on LANL that is the source term. The consistency with the two-week exposure period is clear, and perhaps use of a two-week average versus a time-varying concentration during the exposure period does not make any difference to the models employed for noncarcinogens. But, the same 14-day procedure is used for carcinogens where it is the maximum and not the average concentration that is used to compute the RI. The procedures here

could me more clearly stated. Is this approach really consistent with the conservative philosophy?

***RAC* Response:** The PCOCs were assumed to be released over a two week period because the fire burned for two weeks from start to finish. Since we are taking the entire contaminant inventory located at the PRSs and releasing it over the two week period, we consider that we are being extremely conservative. While this contaminant inventory is calculated using an average concentration, it is still conservative to assume that it is all released. As pointed out by this reviewer, calculations of risk index using air monitoring data did use maximum air concentrations collected during the sampling period instead of averages. It is not, however, equitable to compare the two risk calculations and require that they be the same – both sets of calculations were done to ensure that no contaminants of concern were prematurely eliminated, but the two calculations required very different methodologies because the nature of the data available to us was so different.

Page 15

What, if any, is the importance of the sequence of PCOC in Table 1?

***RAC* Response:** There is no significance to the order in this table – it is merely the order of the PCOCs in our working database.

The screening of PCOC in the air monitoring data does not adjust for background concentrations of these materials in the atmosphere. This does not seem consistent with that used for the assessment of risk for PCOC in surface waters where an incremental risk was based on changes in concentrations between background and fire effects.

***RAC* Response:** The screening assessment for surface water and sediment monitoring data, as reported in the Task 2.1 memo, did NOT adjust for background concentrations; it considered only maximum measured concentrations of each analyte. Nor did it report any calculations of incremental risk. In addition to screening and identifying PCOCs in surface water and sediment following the fire, the Task 2.1 memo considered the available monitoring data and attempted to assess whether or not impacts from the fire at potential exposure locations were apparent based on these data. Final calculations of incremental risk for both the atmospheric and surface water pathways will consider available background data as they relate to developing LANL-related source terms.

Does the screening of the atmospheric data also exclude those data below detection limits or levels of analytical uncertainty as described in Task 1.1: Analysis of Air and Biota Monitoring Data?

***RAC* Response:** Yes, data below individual analytical uncertainty were excluded from the screening. These data do not provide statistically reliable concentrations in the atmosphere and were not usable for the screening calculations.

Pages 16 and 17

For the screening of Air Monitoring data that results in Table 2, which of the air monitoring stations in Figure 3 were used? If all the stations were used, were any of the PCOC included due to concentrations measured at remote stations?

It would be useful to show those stations reporting the maximum concentrations in Table 2.

***RAC* Response:** The monitoring stations shown in Figure 3 are PM-10 monitoring stations. In most cases, the PCOCs were included in the analysis because of concentrations measured either onsite or at LANL boundary locations. In fact, Table 5 and Figure 7 on page 24 of this report shows exactly the information which the reviewer requests.

Page 19

The discussion of tritium seems confusing. The paragraph states "tritium ... was maintained for further study based on the screening criteria." Yet, the screening criteria in Appendix C for tritium was $< 10^{-7}$. Were there separate screening criteria results from air monitoring data that are not in Appendix C? But, if that is the case, how did tritium get in Appendix C when the paragraph also states that "no samples at any PRS location had any concentrations of tritium."

***RAC* Response:** No PRS locations had any concentrations of tritium, but tritium is released from a number of the effluent stacks at LANL. Tritium had a screening value of 4.9×10^{-5}, as shown on Page C-5, which would include it is our analysis. Because tritium is a common effluent at LANL and is not commonly seen in fire plumes, we excluded it from our final risk analysis.

RESPONSE TO COMMENTS BY KEN SILVER ON TASK 1.4.2

December 21, 2001

Arthur S. Rood

Risk Assessments Corporation Research Team

Comment

After the first week of the Cerro Grande fire's encroachment onto Lab property, it was clear that LANL buildings containing large sources of radioactive materials did not go up in flames. At that point, semi-quantitative reasoning could have been applied in the following straight-forward manner:

> *If risk assessments of contaminated lands performed over the years by the Environmental Restoration Program (and its many expert consultants) had estimated cancer risks to be no greater than 10^{-3} or*
> *10^{-4} from a lifetime of residence in a contaminated site, then "fall-out" from the Cerro Grande fire over a period of a couple of weeks would produce much lower risk levels.*

Persons with just a modicum of training in quantitative risk assessment might have engaged in slightly more refined thinking, taking into account the following step-wise processes: fractionation of radionuclides, dilution in non-radioactive smoke, atmospheric dispersion, and deposition in sparsely populated northern New Mexico. By using such semi-quantitative reasoning, I felt confident by late May 2000, that the Cerro Grande fire was not a radiological hazard of great public health importance.

As expected, RAC's state-of-the-art quantitative risk assessment confirms minimal risk levels to human health as a result of radionuclides in the Cerro Grande fire.

Response

Agreed. At this point in the assessment we do not think the Potentially Contaminated Sites (PRS) that were burned contributed significant quantities of radionuclides to the atmosphere such that inhalation health risk is a concern. However, for chemicals, there is less certainty.

Comment

Yet non-radioactive particulate matter has been largely ignored, despite:

> *a) explicit public concern over the health effects of "smoke";[1]*
> *b) reliable anecdotal information about fatalities among elderly and persons with pre-existing cardiopulmonary conditions on the Pueblos[2] and in La Mesilla; and*
> *c) reports of increases in emergency room admissions for respiratory conditions in Rio Arriba[3] and Santa Fe counties.*

These are precisely the kinds of health outcomes that are wholly expected as a result of widespread public exposure to PM_{10} and $PM_{2.5}$.[4, 5] This expectation is based on 20 years of rock-solid epidemiologic studies, by leading researchers on several continents.[6-11]

Within the DOE complex, New Mexico is sometimes described as the "laughing stock" of environmental sciences. A more generous appraisal holds that we exist in a "time warp." In my view, these aspersions are due to several factors. A great deal of the environmental science work in New Mexico is dominated by large bureaucracies with a bias toward inaction. The historical lack of progress on clean-ups by the LANL Environmental Restoration Program is just one example. Another factor is fear among qualified public health professionals over speaking out. Also, in my view, the combined disarmament and environmental agendas of public interest groups concerned with LANL leads directly to "premature ejection" of objectivity, when a "hot" issue like the Cerro Grande fire comes along.

But the health effects of non-radioactive particulates are no laughing matter. My comments are focused on this issue.

RESPONSE:
I do not agree that health effects of particulate matter have been largely ignored. The first step in assessing health effects is to estimating the concentrations persons may have been exposed to. This step has been taken within the confines of Task 1.4.2.

Comment
1. Is particulate matter (PM_{10} and $PM_{2.5}$) a "potential contaminant of concern" (PCOC) -- or is it not?

At an NMED meeting in May (Rodeo Road), the consultants' team stated their cognizance of the linear dose-response relationship for particulate matter and cardiopulmonary outcomes. At the NMED meeting in October (Cerrillos Road), I drew RAC's attention to the Health Effects Institute's pre-eminent scientific review of this issue.[12] The role of Dr. Jonathan Samet, formerly of the University of New Mexico, in leading national-level assessments of the risks of PM_{10} and $PM_{2.5}$ was emphasized.[13]

Hence, I am dismayed that the Task 1.4.2 Technical Memorandum (11/5/01, pp. 25-30) does not explicitly adress the health outcomes likely to have been caused by particulate exposures. Evidently, particulates are not being treated as PCOCs.

This incomplete approach to the particulates issue leaves the door open to irresponsible and misleading uses of RAC's work product. For example, someone seeking to downplay particulate health effects might simply compare the concentrations in Table 8 and Figure 4 to EPA's standards under the Clean Air Act and argue that RAC and NMED have concluded that no particulate-related health effects have occurred. To obviate such irresponsible uses of this work, the report should cite at least some of the overwhelming body of epidemiologic[14] and risk assessment literature[15] which supports PM_{10} and $PM_{2.5}$ health effects at concentrations well

below EPA standards. The report should also state explicitly that PM_{10} and $PM_{2.5}$ were not treated as PCOCs.

RESPONSE:
The purpose of Task 1.4.2 was to estimate contaminant concentrations in air within the model domain. It was not to estimate exposure and risk. We can provide additional detail concerning PM_{10} and $PM_{2.5}$ concentrations in the model domain at selected locations in the model domain. This information should be helpful for health risk assessment of particulate matter.

Comment
2. ***Data Point for TA-1 During Fire is Missing***

I attended official meetings of the Clean Air Act audit in White Rock in June 2000. A LANL air scientist presented a data point for PM_{10} at TA-1 measured during the Cerro Grande fire of over 1,000 $\mu g/M^3$. I do not have precise details of the sampling time frame, nor other details of his method. However, it is distressing to note the absence of this data point from Technical Memorandum 1.4.2 (11/5/01). Moreover, page 25 of the report states: "The maximum value measured during the fire was at TA-54 on May 13 (181 $\mu g\ m^{-3}$)."

Granted, very few people were present at TA-1 at the peak of the Cerro Grande fire when the missing data point was measured. But the same can also be said for TA-54.

I have done enough mathematical modeling in the environmental health sciences to know that in a "data-poor" setting, an exposure model can be very sensitive to the exclusion of a single large data point. RAC and NMED should demand further details about the missing TA-1 data point from LANL and investigate what effect its inclusion would have on the "predicted" average PM_{10} concentrations in Figure 4.

RESPONSE
We will investigate the TA-1 measurement you refer to in your comment. In order for this measurement to be useful, we will need to know the sampling time and dates. In general, I agree with your statement about a "data poor" environment and welcome any additional data that may shed some additional light on the behavior of the fire.

Comment
3. ***EPA's Entire Data Set Appears to Have Been Excluded***

Attached is PM_{10} monitoring data I downloaded from one of the web sites which was publicly-touted during the Cerro Grande fire. I think the data were collected by EPA. It reports PM_{10} concentrations measured at White Rock, Santa Clara, San Ildefonso and a fourth location abbreviated "SB." (Maybe the state office building in Santa Fe). These values range from 26.30 $\mu g/M^3$ to 117.11 $\mu g/M^3$ between May 13 and May 16, 2000. These data should at least be recognized in the Technical Memorandum. If possible, they should be included in the model and presented in Table 8 and Figure 4. Or the basis for their exclusion should be stated explicitly. If not included, the data should be presented in a separate table or appendix.

The Santa Clara Pueblo data are of special interest from the standpoint of public health. Two of the fatalities among individuals at high risk for particulate effects reportedly occurred at Santa Clara Pueblo.[2]

RESPONSE
These data were obtained and reviewed by RAC. However, location of the measurements were not explicitly defined. For this reason, they were omitted from the model calibration. However, with the information you have provided about their location, we can incorporate these measurements in the model calibration. These data, if valid, are significant in terms of our overall understanding of the behavior of the fire.

Comment
4. *WHO Visibility-PM_{10} Conversion Factors*

The World Health Organization offers an approximate relationship between visibility conditions and concentrations of PM_{10} in wildfire smoke.[16] The Technical Memorandum should indicate whether an attempt was made to obtain routinely-collected visibility data in northern New Mexico for the weeks of the Cerro Grande fire. If such data are, in fact, available then an effort should be made to further refine the model of particulate concentrations.

RESPONSE
We are not familiar with either the WHO visibility-PM_{10} conversion factors or the routine visibility measurements. They may provide additional data to work with however, I am somewhat skeptical of there use in the model because what we are estimating are concentrations at ground level where as visibility measurements may reflect particulate concentrations well above ground level. While the model is capable of calculating concentrations above ground level, calibrating to these estimates is less certain because the PM_{10} concentrations estimated with visibility may reflect a spatial average over a considerable distance.

Comment
5. *Overall Critique of Methods of "Outlier Analysis" Still Sorely Needed*

At an early stage of the Clean Air Act audit in October 1997 I recommended that RAC undertake a critical review of LANL's traditions of "outlier analysis;" that is, the criteria used to discard extreme values from environmental monitoring reports. Scientists in different disciplines -- from analytical chemistry to health physics to hydrogeology -- have different methods and criteria for analyzing and discarding outliers. As a public health scientist, I am acquainted with the formal rules of outlier analysis employed by epidemiologists and biostatisticians. As a "consumer" of LANL historical and environmental surveillance documents, I am often struck by how extreme values sometimes trigger additional sampling, while on other occasions extreme values are discarded ipso facto as erroneous. I have almost never seen a citation to a formal statistical test as the basis for these decisions.

Seemingly arbitrary criteria for discarding outliers is a source of mistrust among other "consumers" of official documents (activists, public interest staffers, journalists, et al) pertaining to LANL health concerns.

Excluding Newnet climatic data collected at a station located between buildings at San Ildefonso seems justified, in the context of a modeling exercise. However, the specific reasons for not including: a) LANL's own measurement of PM_{10} at TA-1; and b) EPA's PM_{10} data need to be made explicit.

I still hope that RAC will turn its attention -- and great prowess -- to the issue of outlier analysis in some future consulting project.

I am submitting these comments as an individual public health professional. Attached is my curriculum vitae. In the future, NMED should make available mini-grants to support the development and submission of public comments from parties that have proven to be interested and well-informed.

RESPONSE

As stated earlier, we will include the EPA data in the model calibration and look into the TA-1 measurements made by LANL.

RESPONSE TO COMMENTS BY LANL ON TASK 1.4.2

Comments by Tom Buhl, ESH-DO

The report discusses possible post-fire resuspension as having been responsible for several of the relatively higher radionuclide measurements. How will these measurements be addressed? Will these emissions be modeled in a later report?

RESPONSE

Before we will attempt to model any post fire resuspension, we want to examine the environmental monitoring data. Specifically looking at the time histories of Pu, Am, and U measurements near the site. We suspect that in the past, high reading have also been observed, which would suggest that the "high reading" observed for the fire were not out of the ordinary. If this is the case, we do not intend to pursue the matter any further. If upon review of these data there appears to be a clear increase in concentrations as a result of the fire, we will investigate using a wind-speed dependent resuspension model and coupled with the measurements and suspected source locations, back calculate a source term.

Equation (6) assumes that the radionuclide concentration in a soil sample is constant throughout the sample. However, the impact of areas with primarily surface contamination would be underestimated if the radioactivity is averaged throughout the entire sample. This is especially true for the surface samples that may gone fairly deep which would average the surface radioactivity over the clean soil at depth. This would lead to an underestimate of what the report calls the resuspendible inventory.

RESPONSE

If we had data supporting surficial contamination accompanying the PRS data, we would certainly take this into consideration. However, characterization of the PRS sites appear to be crude at best and do not include that kind of detail in the assessment.

Comments by Lars F. Soholt, EHS-20

In Table 7, the duff/litter depths and fuel loading per acre don't seem to compute. If the same conversion factor for load to depth (or vice versa) are being used for duff and litter, the duff/litter ratios should be the same for depth and fuel load, shouldn't they?

RESPONSE:

There is a mistake in the reported density of the duff and litter. The correct densities are 0.107 g cm^{-3} for duff and 0.0265 g cm^{-3} for litter. Using these density values and duff/litter loading yields the correct duff/litter depths. For example consider duff depth for a duff loading of 18.2 tons/acre. The duff depth D is

$$D = \frac{18.2\,\frac{\text{tons}}{\text{acre}} \times \frac{\text{acre}}{4.04687 \times 10^{7}\,\text{cm}^{2}} \times \frac{907184\,g}{\text{ton}}}{0.107\,\frac{\text{g}}{\text{cm}^{-3}}} \times \frac{\text{in}}{2.54\,\text{cm}} = 1.5\,\text{in}$$

p.32. I am a little confused by the discussion of concentrations at the bottom of the page. The tables present results in ug(aCi)-d m^-3 and compares them to measured concentrations. Dividing by 2 days cancels out the days and yields weight or activity per cubic meter. It doesn't change the bottom line of the discussion, but it is confusing as to what was actually done here.

RESPONSE:

Your point is well taken. However, the reason for calculating time-integrated concentrations is to allow concentrations of different averaging times to be added. We can divide by any averaging time (1 day, 4 days etc). The longer the averaging time the lower the average concentration. The point is, that concentrations are all underpredicted even if you assume a 1 day averaging time.

RESPONSE TO COMMENTS BY MARY UHL, NMED ON TASK 1.4.2

December 10, 2001

Arthur S. Rood
Jill M Aanenson
Shawn Mohler
Risk Assessments Corporation Research Team

General Comment

I have reviewed the two technical memorandums, Task 1.4.2 and Task 1.5. The analysis is well-conceived and well-performed. The documents are written well and easily understood, especially 1.4.2, which relays quite a bit of highly technical information about emissions and dispersion modeling. I commend RAC for making this discussion so clear and easy to follow. This will assist those not well-versed in modeling in reviewing this document and others that follow. The following are my comments and questions:

General Response

We are overall pleased with the review provided by Ms. Uhl and found the comments to be both constructive and enlightening. She has pointed out several weaknesses in our methodology that will be addressed.

Response to Specific Comments

1.) I agree with the switch to 30-year and 14-day exposure for all contaminants. The 14-day exposure better corresponds to the duration of the Cerro Grande Fire and is thus more appropriate for this analysis.

RESPONSE:
Agreed.

2.) Assuming the mean soil temperature to be the maximum temperature reported during other high intensity fires is indeed conservative, but I believe there could be more discussion on pages 7-8 to explain why this is so in relation to chemical volatilization and potential release to the atmosphere.

RESPONSE:
We will expand the discussion of the assumed mean soil temperature to include a more thorough explanation of its conservatism.

3.) Given that the assumed mean soil temperature of 600 degrees Celsius could be somewhat variable and soil temperatures may have been slightly higher (or lower), would it be reasonable

to conservatively assume that arsenic and selenium also volatilized? Their boiling points are 613 and 684 degrees Celsius. For that matter, since temperatures above 1000 degrees Celsius are uncommon (according to literature review), could one also assume that Cadmium and Zinc volatilize also, as a worst-case scenario?

RESPONSE
In light of the fact that we are performing conservative estimate of exposure and risk, I think your suggestion is well taken and will assume 100% volatilization of arsenic, selenium, cadmium, and zinc.

4.) On page 12 you discuss wet deposition. I was somewhat surprised, given that there was no precipitation during the fire. You document this later on in the memorandum, but perhaps it would be appropriate to mention this on page 12.

RESPONSE
Agreed. We will state that although wet deposition is an important process in removing contaminants from the air, it was not important for the Cerro Grande fire because no precipitation occurred during the period.

5.) Given the known problems with the Santa Fe airport meteorological data, I was surprised to see that this data was used in CALMET. I am curious as to whether the meteorological fields calculated by CALMET would be more representative without this data. There appeared to be some highly questionable wind data from the Santa Fe airport during the fire that did not correlate well with other data in the area. This could bias the model results.

RESPONSE
The meteorological data used from the Santa Fe Airport consisted of only cloud cover data. These data are required by CALPUFF and were unavailable at the other stations. Wind data, temperature, and humidity data were taken from the NMED station. This change was made in response to comments from NMED and Jean Dewart from LANL on the Task 1.4.1 report, and I failed to report the change in this document.

6.) I am somewhat troubled by the methodology for the modification to EPM. I like the idea of defining the fire by smaller elements and then summing the elements. This seems more appropriate to me, and I'm curious as to what difference this will make in the emission results.

RESPONSE
I myself share your concern but after reviewing some of the available literature, I believe the approach outlined in the report is justified. Let me reiterate several points made in the document that provide some justification to the modifications to EPM. First, the *rate* at which biomass is consumed in EPM (which is related to the emission rate of pollutants and heat) is totally dependent on the user's choice of ignition time. However, the <u>total</u> amount of pollutant emissions is related to the fuel loading and moisture content of the fuel, and the default emission factors. This value (total amount of pollutant emissions) was the same between the modified EPM output and the original. The code depends on the user's discretion to correctly estimate how long it took to ignite the fuel load to estimate correctly both the emission rate of pollutants and the heat

release rate. The equations in EPM were designed for slash pile and controlled burns–not large wildfires. And based on field observations and comparisons to EPM output, there appear to be large discrepancies between the observed and calculated smoldering stage. In a slash burn, most of the fuel is relatively dry, burning proceeds relatively rapidly, and much of the biomass is consumed during flaming phase. In contrast, a rapidly spreading wildfire may have a very short but active flaming stage followed by a long smoldering stage in which much of the emissions are produced. A wildfire may consume both dried "dead" fuels and standing timber. Standing timber is more likely to generate a long smoldering stage compared to dry dead wood. Tim Reinhardt of the URS corporation (the company contracted by the Forest Service to update EPM) agreed that the smoldering algorithm in EPM is inadequate, especially for large wild fires, and this algorithm was being changed, along with much of the code. Modifications to the code will not be ready until sometime next year.

Based on the observed pattern of elevated PM10 concentrations measured in the model domain, along with the timing of the fire, it appeared to me that the longer smoldering stage was not being adequately represented. The smoldering stage is also likely to have much greater ground-level representation because plume rise is minimal compared to the active burning stage. I want to reiterate that the modified EPM output retained the total amount of pollutant and heat released as estimated by EPM. These totals were then re-partitioned so that more of the emissions occurred during a simulated smoldering stage of the fire when heat release was minimal.

We performed a review literature regarding wildfire emissions. Many of the articles (Coffer et al., 1996; Levine 1999; Ward 1999; Ward 1991) focused on emission ratios of various pollutants released during wildfires that included CO_2, CO, CH_4 and NOx, in addition to VOCs (formaldehyde, ammonia, HCN, benzene, toluene, xylene, and hexane). Coffer et al. (1996) reports emission ratios (defined as the ratio of the net trace gas concentration to that of CO_2) for flaming and smoldering stages of wildfires occurring in three types environments; wetlands, savanna, and boreal. He concludes that grass fire emissions are less significant during the smoldering stages compared to wildfires in more diverse ecosystems such as boreal. Mean CO emissions ratios for a grass fire were 4.7% during the flaming phase and 5.3% for the smoldering phase. In contrast, the boreal ecosystem wildfire had CO emission ratios of 6.7% for the flaming phase and 12.3% for the smoldering phase. Total emissions partitioned into the flaming and smoldering stage appears to be about 50-50 for the grassland fire and about 30-70 for the boreal, respectively. The Los Alamos ecosystem is neither grassland or boreal, but believed to be somewhere in between.

Ward (1999) also provided some insight into the relative amount on emissions that occur during the flaming and smoldering portion of a fire. As shown by Coffer et al. (1996), the relative proportions are fuel-dependent. For a prescribed burn in the Pacific Northwest, its reasonable to assume that 10% of the emissions are released during the flaming portion, 70% during the first smoldering stage, and 20% for the final smoldering stage. Emissions released during flaming combustion are accompanied by the release of significant quantities of heat which lofts the smoke plume well above ground level. In contrast, heat generation during the smoldering stage is less, resulting in smoke plume staying relatively close to ground level. The problem I found with EPM was that the heat and emissions were coupled – that is, the heat released appeared to be directly related to the emission release rate. My modifications to EPM, in effect, decoupled the heat release rate and emissions, allowing for a smoldering stage of the fire to be represented by the model.

I have experimented with EPM, segmenting the fire into a number of smaller fires. For example, assume that the total area burned for a given day was 1000 acres and the total burn time was 10 hours. You can divide this into ten 100-acre fires of 1-hour per fire. When you compare the two results, they are identical. That is, you get the same pollutant and heat release rate for one 1000 acre fire that burns over 10 hours as you get for ten 100-acre fires of 1 hour duration. I also encountered many problems running EPM with multiple segmented fires. I was not able to obtain the source code to find where the problems were.

If another emissions model were available, I would have at least tried it. There is another emissions model available on the National Forest Services web page (CONSUME); however, it only provides total emissions and does not provide emission rates or heat emitted. I do not contend that the reapportioning and decoupling of heat and emissions is valid in every case. The methodology can be considered semi-empirical and based only on study of the limited number of PM-10 measurements taken during the Cerro Grande Fire. I certainly think this an area where additional research is needed, but do not believe this research can be performed within the confines of this project.

References

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Cofer W. R., J.S. Levine, E.L. Winstead, D.R. Cahoon, D.I. Sebacher, J.P. Pinto and B.J. Stocks. 1996."Source composition of trace gases released during African Savanna Fires". *J of Geophysical Research.* 101:D19. 23,597-23,602.

7.) On page 25, the reference to Table 7 should be to Table 8.

RESPONSE
Accepted and will make the change.

8.) Although I understand that this study cannot include a rigorous study of chemical reactivity, I believe that chemical transformation may be important in the formation of secondary chemicals and particulates not studied here. These secondary pollutants may have some associated health risks that should be studied at some later time.

RESPONSE
We agree with your assessment. Some of the degradation products of the PCOCs may be important to consider, and we will do so in a future report.

9.) In Table 8, the time periods of "before fire" and "after fire" should be defined, for example, was "before fire" the month before the fire, the two weeks before the fire, or the year before the

fire? This is important to properly interpret the data, as background levels vary seasonally throughout the year in New Mexico and the background concentrations are predominant.

RESPONSE
Agreed. Also, in response to other reviewers comments, we are expanding the discussion and analysis of PM-10 data. Preliminary results confirm what you have suspected. That is, the PM-10 concentrations vary seasonally and are also affected by precipitation, mean wind speed, and human activity.

10.) It should be noted that model predictions within a factor of 2 are considered reasonable for air dispersion modeling analyses; therefore, the CALPUFF model is performing reasonably well for this simulation.

RESPONSE
Agreed.

11.) There are quite a few instances of missing punctuation and misspellings, for example, "patters" instead of "patterns".

RESPONSE
We will perform additional technical editing of the document.

RESPONSE TO COMMENTS ON THE TASK 1.4.2 REPORT BY JOHN PINDER

December 12, 2001

Arthur S. Rood
Jill W. Aanenson
Shawn Mohler
Risk Assessments Corporation Research Team

An Introductory Comment
Before reviewing Task 1.4.2, I believed it to be necessary to review the draft copy of Task 1.3 that was provided with this task. Although I now believe that Task 1.3 has been completed with a final report filed, my comments on the draft copy are provided because 1) they are relevant to the review of Task 1.4.2 and 2) they may be of use in considering revisions on Task 1.4.2. The comments on Task 1.3 should be read before the following comments on Task 1.4.2.

RESPONSE:
Agreed.

General Comments
The evaluation of air concentrations in the model domain are restricted to the list of PCOC developed in Task 1.3 and listed in Table 1 of the current task. Any errors of omission in the selection of PCOC in Task 1.3 propagate through this task.

RESPONSE
Agreed.

The task report clearly reflects a comprehensive and often innovative effort to merge the science of several disciplines into a model that will address the distribution and deposition of PCOC introduced into the atmosphere from the PRS sites burned during the Cerro Grande fire. The effort depends largely on the merging of several "off the shelf" pre-existing models, but each of these have been modified or used in ways that could make it difficult for even the designers of the models or the writers of their code to assess the effect of the modifications and uses on the accuracy of the resulting predictions.

RESPONSE
In general, I agree with your statements concerning the "merging of several off-the-shelf pre-existing models" but take issue with the statement "...these have been modified or used in ways that could make it difficult for even the designers of the models or the writers of their code ..". The statement is probably true as it relates to the Emissions Production Model. The model is being used to model a large wildfire – not a slash pile burn or prescribed burned as it was designed to do. However, the reviewer needs to recognize that there are currently no models except EPM that estimate emission rates and heat released during biomass burning. While there are many literature values (and other codes) available for pollutant emission factors from forest

fires, these factors are only part of the story. The heat released during combustion is critical to the dispersion of these pollutants over the model domain. This is why EPM was used because it is the only model that I am aware of estimates heat generation during combustion.

In regards to CALPUFF, the model was applied to this problem in exactly that way it was designed. The model has been used in the past to assess air quality impacts from prescribed burns in the Pacific Northwest. To assist in these assessments, the CALPUFF author has written preprocessors that links EPM output into CALPUFF. This preprocessor was obtained from the CALPUFF author and used without modification.

General Comments on Model Accuracy and Formulation

Model Accuracy

To evaluate the accuracy of the model the authors have compared predicted to measure PM10 values (Figure 4), but the degree to which this is valid is questioned by their acknowledgement that the "process was an iterative and involved many adjustments ... to bring the measured and predicted values within a reasonable range of agreement." Thus, the model was tweaked to match the results, and the agreement with these results cannot be used as a confirmation of accuracy unless there was some prearranged plan to use some of the measured PM10 data to train the model and the remaining to test the model once the training had been done. If this type of plan was used, it should be clearly stated. However, it seems unlikely that such a plan could have been used given that measured PM10 data are only available from 4 main locations (by the way, where is Hernandez? It doesn't seem to be on the maps) and ≤ 13 daily measurements at each location. If no separation of data into training and testing sets was done, it should also be clearly stated. These statements are needed so that there can be a clear interpretation of the significance of the data in Figure 4 and Table 9.

RESPONSE

The reviewer recognizes the limited data set we had to work with and correctly concludes that we did not train the model with one data set then test it with another. This will be clearly stated in the revised text. In effect, I calibrated the model to the all available measurements and therefor should not conclude that the model is validated, but calibrated. Separating the data set into a "training" and a "testing" would have been preferable had more data been available, but it was not at the time (see response to next comment). Additionally, many of the measurements (with the exception of TA-54) were close to the mean background, making it difficult to discern a true "signal" from the fire (more on this later in the comment responses). In practice, I focused the calibration measurements at TA-54 because this monitor provided the most complete time history and had the most definite signal from the fire. Then turned my attention to the other monitors in the model domain. In most cases, little or no modification was done.

Since the time the report was submitted, additional calibration work was performed and additional PM-10 air data were obtained. The work focused on reviewing the fire progression history, combined with the PM-10 measurements, and estimated meteorological wind fields. Changes were made to the temporal and spatial positioning of individual fire units. In some cases, large fire units were separated into smaller ones (May 10th). In other cases (May 15th), fire units were eliminated because of errors in the original geographical information system (GIS) coverages provided by the laboratory. We will also put the location of Hernandez on the base maps.

Also, are there any CO data that may be used to evaluate the accuracy of this portion of the model.

RESPONSE
Not that I am aware of.

Given that the data in Figure 4 and Table 9 are the best estimates of the accuracy of the model predictions, there are two reasons to be concerned about accuracy. First, the model appears to be more accurate at the distant stations. The median predicted-to-observed ratios for Santa Fe, Espanola and Hernandez are 0.83, 0.95 and 1.07. The median predicted-to-observed ratio for TA-54 in White Rock is 0.63 and 6 of the 13 ratios are < 0.5. There also appear to be clear discrepancies in temporal trends between predicted and observed data in Figure 4 for TA-54. These patterns suggest that the errors in predictions may be greater for locations near LANL where exposures are greater. Thus, the predicted exposures for higher exposures are more likely to be in error than the lower predicted exposures for more distant populations.

RESPONSE
Before responding directly to the above comment, I want to explain the overall difficulty and complexity involved in calibrating the model. The PM-10 concentration at any receptor point in the model domain can be thought of as the resultant of a 4-dimensional vector. Three of the dimensions comprise the spatial orientation of the plume in a Cartesian coordinate system. The forth dimension is time.

Horizontal position of the plume is a function of the estimated wind vectors in the *X-Y* (horizontal) plane at the height of the plume, and the location of the fire. The vertical position of the plume is a function of the vertical wind vector and more importantly, the maximum height of the plume. The height of the plume is primarily dictated by the heat release rate. Ground-level concentrations are highly influenced by the vertical vector because the plume can be simply "lofted" over the receptor resulting in low ground-level concentrations near the fire. Additionally, the vertical component also influences the trajectory of the plume because emissions lofted into upper air levels advect according to the wind field at that level and not the surface wind fields. The temporal dimension is also important because it incorporates not only the changing meteorological conditions, but also a changing source term. For example, releases from a smoldering fire that occur during evening hours and under calm conditions tend to result in high ground-level concentrations. In contrast, fires that were actively burning during the daylight hours tended to produce low ground-level concentrations.

The basic procedure used in model calibrations was to 1) establish the meteorological wind field where by pollutants introduced into the model domain are advected, 2) partition the fire into 24-hour periods representing each day the fire actively burned, 3) establish the geographical extent of burned area(s) for each day of the fire, 4) estimate fuel loading and calculate emission rates for each fire (note: fuel loading is terrain elevation specific), and 5) determine the time the fire started and the length of time it burned. Pollutant sources were then inserted into the CALPUFF model as a buoyant area source of arbitrary source strength. Establishing the meteorological wind field was done using the CALMET code and described in our Task 1.4.1 report. Our "degrees of freedom" in the meteorological model consisted of 1) adjusting the wind

field interpolation variables, and 2) removing meteorological stations from consideration which were either known to be sited poorly, or appear to have erroneous data based on examination of the wind roses. Once a wind field was developed, PM-10 source term estimates were made using a modified version of EPM. The location and the timing of the flaming and smoldering stage of the fire, along with the total emissions from the fire were then adjusted to yield PM-10 concentrations that best represented the measured data. All PM-10 estimated were adjusted for background contributions.

The PM-10 concentration at a particular sampler is not a simple linear function of the source strength of a given fire. Rather, it is a convolution of multiple fires that are either currently burning or are remnants from the previous day fires that may be still smoldering. The location of each fire, the fuel loading associated with that fire, and existing pollutant mass already in the model domain also contribute to the observed PM-10 concentration. It is important to note that little quantitative data exists on the timing and burn rate of the fire and much of it had to be inferred from qualitative descriptions of fire progression. Great efforts were made to examine the temporal history of the limited PM-10 measurements combined with the qualitative history of fire progression and the meteorological wind fields to understand the overall behavior of the smoke plume. Recalibration efforts our currently underway using some newly acquired PM-10 data and new information about spot fires on the Laboratory after May 11th.By making new assumptions about the timing and extent of the fire, we can arrive at a calibration that is significantly different from the previous one. In essence, the calibration shown in Task 1.4.2 is a work in progress and more work is being done to improve the calibration and understand the overall behavior of the fire.

The last point I wish to make concerning this comments relates to the accuracy of atmospheric transport models in general. Your comments suggest the model is not performing well and the P/O ratios are unacceptable. On the contrary, the P/O ratios are quite good for short-term estimates of concentration given the uncertainties present. Under optimum conditions, short-term (<24 hours) estimates of pollutant concentrations are at best within a factor of 2 (Miller and Hively 1987). In complex-terrain, P/O ratios are noted to range from 0.01 to 100 using the Gaussian Plume Model. In a study comparing estimated concentrations from five atmospheric transport models to 9-hour average measured concentrations of a SF_6 tracer released from Rocky Flats Colorado, P/O ratios were distributed lognormally and had geometric standard deviations of 4 or higher. Only about 75% of model predictions were within a factor of 5 of the observations (Rood et al., 1999). Indeed, one reviewer (Mary Uhl from NMED) commented that ±2 is very good model performance for short-term estimates in complex terrain.

The second reason to be concerned about accuracy is the discrepancy between the predicted physical composition of the PM10 particles and their measured composition at Espanola (page 25). The measured PM10 compositions at Espanola suggest that there was greater resuspension of soil particles than considered in the model.

RESPONSE

The reviewer is commenting on the information presented by Popp et al. (2001) which states that PM10 measurements taken in Española showed a large contribution from crustal alkali and alkaline elements suggesting convective dispersion of soil particles and incompletely burned biomass was the dominant mechanism for particle generation during the Cerro Grande Fire. I have contacted the author of this paper (Carl Popp) in an attempt to gather more detailed

information about his measurements. As of this time, I have not received his data. I understand the potential implications of this work. However, it is difficult to quantify without knowing exactly was is meant by "large contribution". The mean PM-10 concentration for the measurement period between May 12–17 was 30 $\mu g\ m^{-3}$ which is about a factor of 2 higher than the mean background concentration of 14 $\mu g\ m^{-3}$. Assuming background contributions to PM-10 are mainly from crustal sources, then the fact that crustal alkali elements appeared as a large contributor is not surprising. However, without more information about the measurement specifics, it is difficult to make any quantitative statements about the origin of PM-10 at Española.

A second point I wish to make is that any particles generated by soil suspension in the vicinity of the fire will be entrained with the smoke plume and should follow the same trajectory as other pollutants emitted from the fire.

Several things could be added to the report to provide a greater indication of the behavior, function and accuracy of the model. If separate model runs could be made for some of the separate fires discussed on page 31 (e.g., the North and South fires on May 13) and the results displayed and compared to the predictions of the entire model, this would provide some feel for the model's complexity. Although the authors state that "Runtimes for CALPUFF did not allow for individual runs to be performed for each of the PRS contaminants," the use of CALPUFF to make predictions for at least one contaminant and the comparison of those predictions to the data in Table 10, 11 or 12 would be informative.

RESPONSE

I agree in general with the review comments and intend to provide more detail concerning the overall complexity of the model. The reviewer suggests separate model runs for some of fires and overlay them on the composite run. I would limit these runs to PM-10 because we have y monitoring data with which to compare with. Doing the same for some other contaminants does not appear to be as useful an exercise mainly because estimated contaminant concentrations are well below the range of measured values. Additionally, these are no (or very few) background concentrations in which to compare with.

Since the submittal of Task 1.4.2, I have taken a closer look at available measurement data. Of particular interest were the measurements of ^{239}Pu, ^{241}Am, and uranium isotopes made by LANL at several locations near and on the LANL facility. These measurements are particularly attractive because they have time histories both before and after the fire, although the time resolution was poor (quarterly averages). The data from TA54 Area G were plotted as a function of time (Figure 1) and the results indicated a noticeable increase in the ^{238}U and ^{234}U concentration for the quarter that included the Cerro Grande fire. The same cannot be said for ^{239}Pu and ^{241}Am, which showed a drop in their quarterly-average concentration from the previous month. Both ^{234}U and ^{238}U are naturally occurring and the fact that they show increases during the fire while the man-made ^{239}Pu and ^{241}Am do not suggests a natural source for this increase.

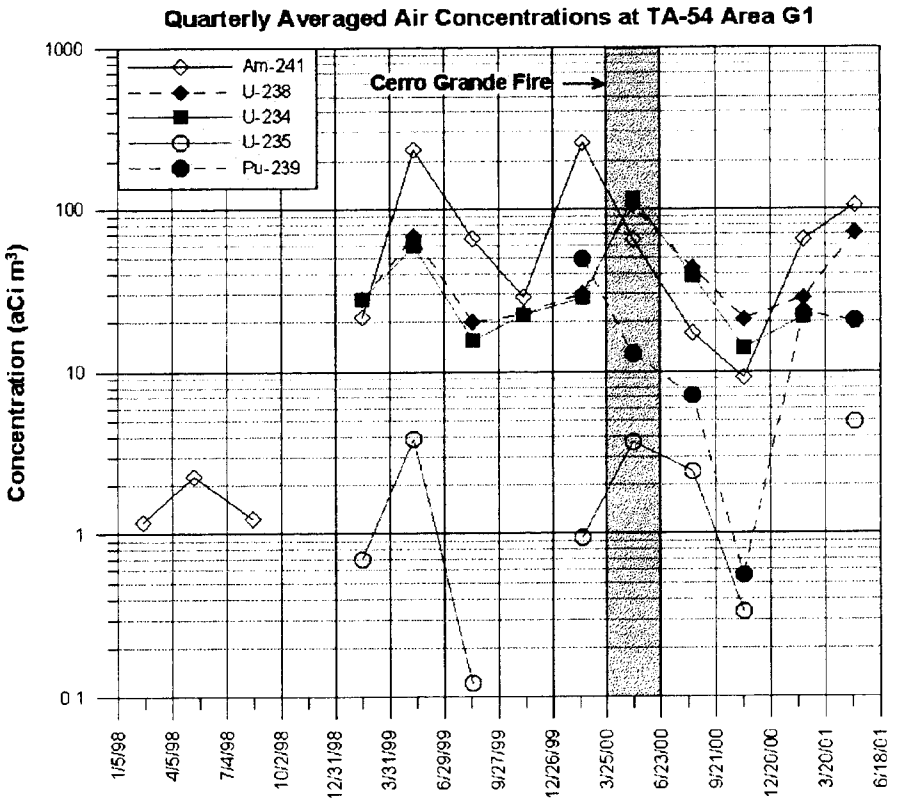

Figure 1 Quarterly average concentrations of ^{239}Pu, ^{241}Am, ^{238}U, ^{234}U, and ^{235}U taken at TA-54 AreaG1.

Model Formulation

As mentioned above, the model's parameters were tweaked to ensure that the predicted and observed PM10 concentrations during the fire were matched. This raises a disturbing question relative to the data displayed in Figure 3 on page 27.

This figure compares pre-fire PM10 data from a number of sites with that recorded during the fire at some of these sites. There is generally a 2x increase in the means (geometric means?) from pre-fire to during fire. The fire data is obviously from a limited time period, and the time period over which the pre-fire data is averaged is not given. It is assumed that this difference was due to the smoke of the fire, and the model was formulated to fit these data.

However, the fire is not the only potential cause of this difference. Wind speeds during part of the time of the fire were also elevated, and may also be a cause of elevated PM10 concentrations. This may also be indicated by the presence of crustal material in the PM10 concentrations. Some of the upper bounds for PM10 concentrations during the fire are not that much elevated from the upper bound for the pre-fire period (with the exception of TA-54). Thus, the during fire data are largely within the range of data observed before the fire. This may be expected if the difference is due to wind speed, but should it be expected with such a catastrophic fire?

IT IS ABSOLUTELY CRITICAL TO THE WHOLE PREMISE OF THE FIRE MODELING THAT THE DATA IN FIGURE 3 BE ANALYZED WITH SOME THOUGHT TO ADJUSTING FOR WIND SPEEDS. THIS COULD BE DONE BY LIMITING THE PRE-FIRE DATA TO PERIODS

OF SIMILAR WIND SPEEDS OR USING WIND SPEED AS A COVARIATE IN AN ANALYSIS OF VARIANCE OF PRE-FIRE AND DURING FIRE DATA.

IF THE DIFFERENCES IN PRE-FIRE AND FIRE PM10 CONCENTRATIONS ARE DUE TO WIND SPEEDS AND NOT DUE TO THE FIRE, THEN THE WHOLE PREMISE OF THE TWEAKING OF THE MODEL TO FIT THESE DATA IS INCORRECT AND THE ESTIMATED CONCENTRATIONS IN TABLES 10 THROUGH 12 MAY BE MEANINGLESS.

RESPONSE

The issues raised by the reviewer are valid and warrant investigation. The comment states that "*...Wind speeds during part of the time of the fire were also elevated, and may also be a cause of elevated PM10 concentrations. This may also be indicated by the presence of crustal material in the PM10 concentrations.*" I assume he is referring to the increase in suspension of soil particles as a result of higher wind speeds. Higher wind speeds do tend to result in higher soil suspension however, the process is complicated by other factors that include 1) moisture content of the soil, 2) snow cover, 3) site-specific soil conditions, and 4) human activities (such as construction). These factors can either enhance or retard wind-driven suspension and must be considered in any analysis.

To address the reviewers comment we needed a reasonably long data set of PM-10 concentrations combined with wind speed data. The PM-10 data at Capshaw and Runnels located in downtown Santa Fe coupled with meteorological data taken at the Santa Fe Airport and the NMED location meet these criteria. Data were obtained for the period beginning in January of 1999 and ending in September of 2000. The PM-10 monitor at TA-54 coupled with TA-54 wind speed data were also analyzed, however these data only go back to April of 2000. The stations at Española and Hernandez only operated during the fire. The PM-10 data represent 24-hour averages values while meteorological conditions represent hourly observations. To bring these two data sets into the same time domain, 24-hour average wind speeds were computed for each day a PM-10 measurement was available. What I was interested in was the relationship between 24-hour average wind speed and the corresponding 24-hour average PM-10 concentration.

Before performing the regression, it was useful to examine some of the raw data. Figure 2 shows a plot of the 24-hour average PM-10 concentrations and the hourly average wind speed for the time period April to June, 2000 in Santa Fe. Qualitatively, there appears to be a relationship between the wind speed and PM-10 concentration, with one notable exception being the PM-10 measurements made during the Cero Grande Fire on May 15th.

Linear regression was performed on the various subsets of the data. The linear correlation coefficient (r) was given by (Bevington and Robinson 1992)

$$r = \frac{N\sum x_i y_i - \sum x_i \sum y_i}{\left[N\sum x_i^2 - \left(\sum x_i\right)^2\right]^{1/2}\left[N\sum y_i^2 - \left(\sum y_i\right)^2\right]^{1/2}} \qquad (1)$$

where N = the number of x-y pairs. The probability of exceeding r in a random sample of observations taken from an uncorrelated parent population is given by

$$P_c = 2\int_{|r|}^{1} P_r \, dr \tag{2}$$

P_r is given by

$$P_r = \frac{1}{\sqrt{\pi}} \frac{\Gamma[(v+1)/2]}{\Gamma(v/2)} \left(1 - r^2\right)^{(v-2)/2} \tag{3}$$

where $v = N$-2 and Γ is the gamma function approximated by (Bevington and Robinson 1992)

$$\Gamma(n) = \sqrt{2}\, e^{-n}\, n^{(n-1/2)} \left(1 + 0.0833/n\right) \tag{3}$$

Values for P_c were tabulated in Table C3 in Bevington and Robinson 1992 and intermediate values were interpolated.

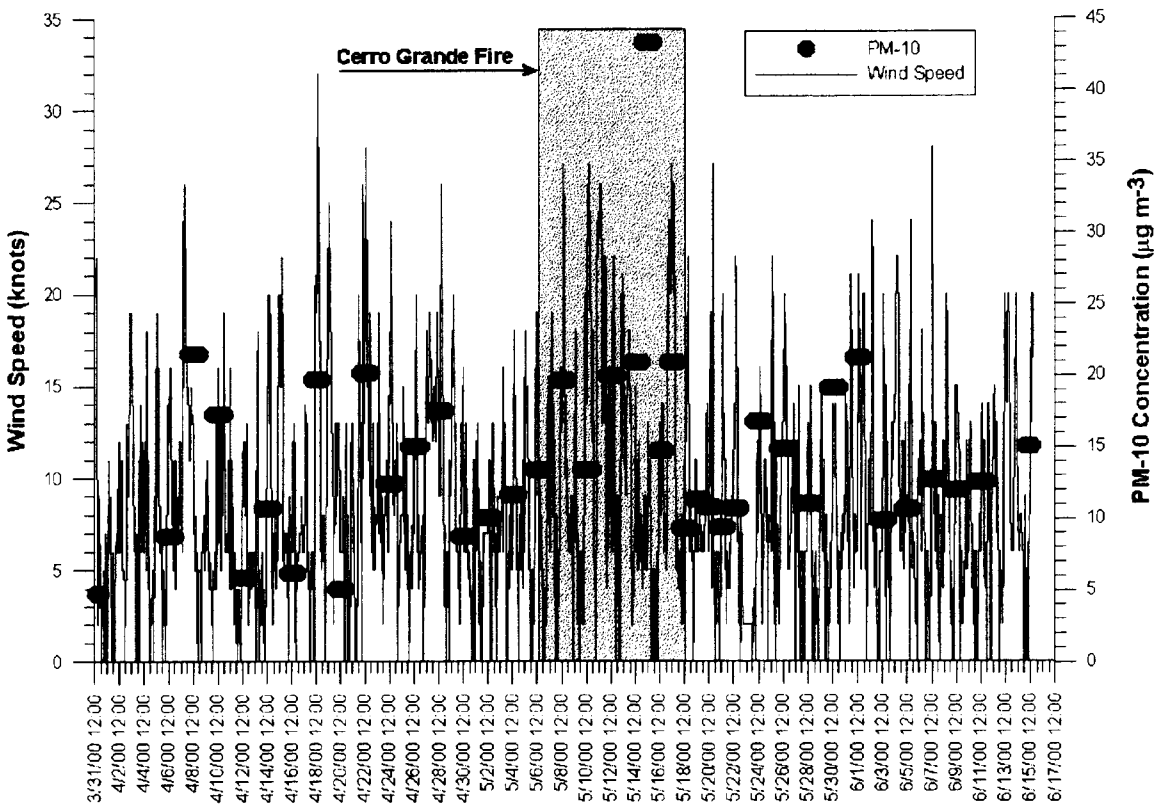

Figure 2. Hourly Average wind speed and 24-hour average PM-10 concentration at Runnels and Capshaw in Santa Fe. PM-10 concentrations represent an average of the Runnels and Capshaw measurements. Note that the highest reading (~35 µg m^{-3}) occurred during relatively calm conditions.

The results for Capshaw and Runnels (Table 1) show a poor correlation between wind speed and PM-10 when all data were considered. A somewhat better correlation was obtained by eliminating those days in which precipitation was measured. This regression yielded a r value of 0.222 and had a P value of 0.028. Limiting the regressions to snow-free months (March-October)

yielded a slightly better regression (r = 0.24, P_c = 0.016). Limiting the regression to the months before and after the fire yielded a r value of 0.552 and corresponding P_c value of 0.0025 (Figure 3). When the data from the fire was (May 6–18) regressed, a negative correlation was calculated, however the P value was >0.69 indicating probably no correlation at all. The results for TA-54 also showed similar results (Table 2 and Figure 4).

These results show that for certain periods of the year when precipitation was lacking and soil moisture contents were presumably low, 24-hour average PM-10 concentrations are correlated with the 24-hour average mean wind speeds. The fact that wind speed and PM-10 concentrations during the fire were not correlated indicates an additional source of PM-10 which is presumably the fire.

Using the regression coefficients, we now have a method to estimate the 24-hour PM-10 concentration from background sources based on the measured mean wind speed. The regression equation for Santa Fe and other locations within the Rio Grande valley is

$$C_{PM10} = 3.8 + 1.05u \tag{4}$$

where u = the 24-hour average wind speed (knots) and C_{PM10} = the 24-hour average PM10 concentration (μg m^{-3}). The equation for TA-54 is

$$C_{PM10} = 5.4 + 1.35u \tag{5}$$

This analysis provides evidence that the elevated PM-10 observed at the four monitoring stations were more than likely due to the presence of the smoke plume and not increased dust suspension. Although, increased dust suspension probably did occur during some of the windy days of the fire. Additional PM-10 sampling data were obtained for Santa Clara and San Ildelfonso Pueblos and the city of White Rock. Data from these samplers are restricted to the days of May 14-16 and PM-10 concentrations ranged from 48 to 117 μg m^{-3}. These data will be added to model calibrations in the future.

These results are not surprising considering the other factors that affect soil suspension mentioned earlier. Precipitation appears to have a large impact on PM-10 concentrations. Not only does it suppress soil suspension, it also removes particulates from the air through wet deposition. The regression analysis and examination of Figure 2 and 4 also show that some of the highest PM-10 concentrations during the fire occurred during relatively calm conditions. During calm conditions, the smoke plume from a smoldering fire will hold close to the ground and sink into valleys and other topographic lows during evening hours under nocturnal drainage flow that is common in mountainous terrain.

Table 1. Summary of Regression Statistics of PM-10 and average wind speed for Capshaw and Runnels.

Statistic	All data (Jan, 1999- June, 2000)[a]	All data (Jan, 1999- June, 2000)[a,b]	March-October, 1999–2000[a,b]	April-June 2000[a,b]	May 6-19, 2000[c]
n	213	163	111	28	10
r	0.109	0.222	0.24	0.552	–.017
P_c	0.294	0.028	0.016	0.0025	>0.5

a) Excluding data from May 6–18, 2000
b) Excluding precipitation days
c) Cerro Grande fire days

Table 2. Summary of Regression Statistics of PM-10 and average wind speed at TA-54.

Statistic	All data (April, 2000-June, 2000)[a]	All data (April, 2000-June, 2000) [a,b]	May 6-19, 2000[c]
n	186	56	13
r	0.247	0.385	0.036
P_c	0.014	0.0025	>0.5

a) Excluding data from May 6–18, 2000
b) Excluding precipitation days
c) Cerro Grande fire days

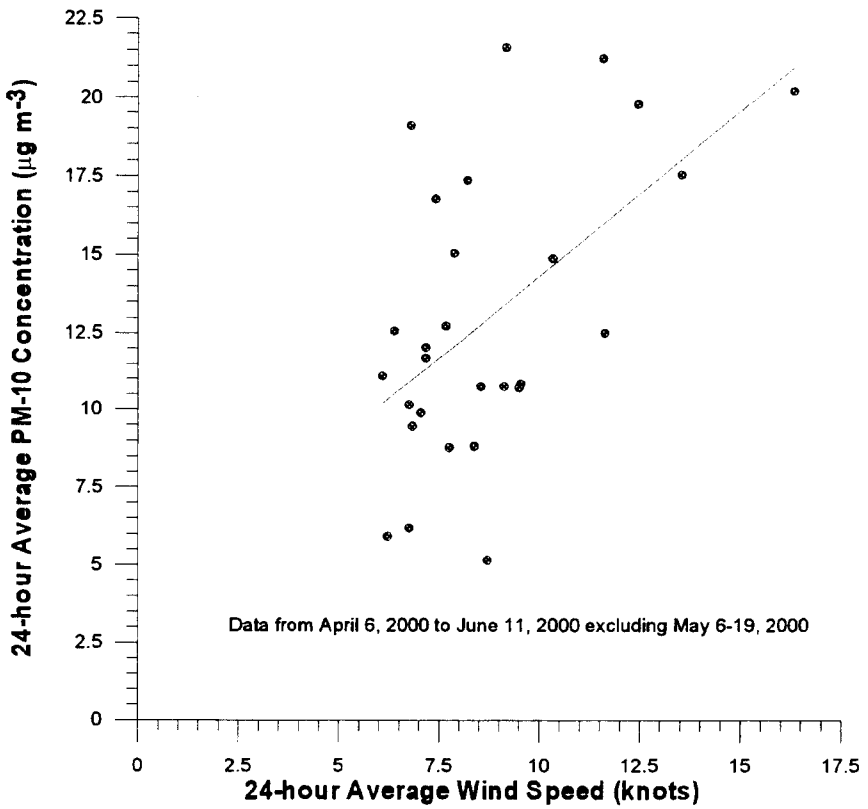

Figure 3. Scatter plot with regression line of 24-hour average PM-10 concentration as a function of 24-hour average wind speed as measured in Santa Fe. PM-10 data were taken from the Runnels and Capshaw stations located in downtown Santa Fe. Measurements taken during the Cerro Grande Fire (May 6-18) and days with measurable precipitation were eliminated.

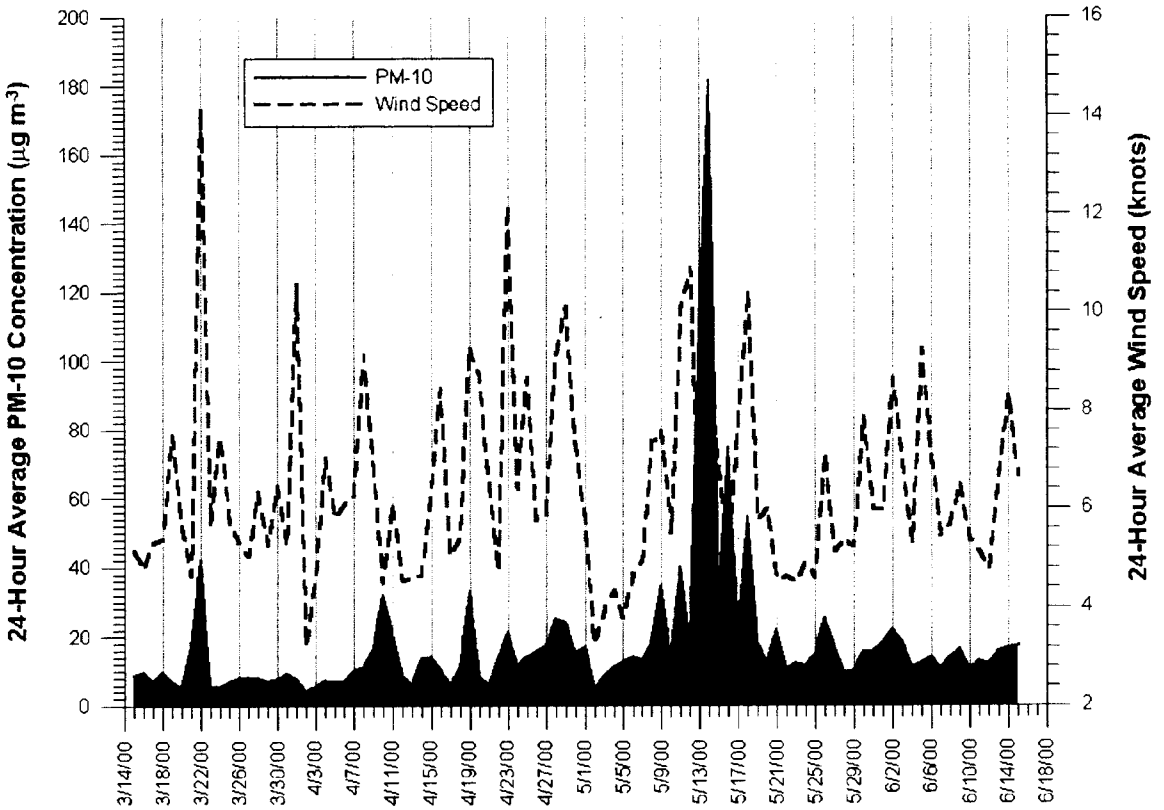

Figure 4. 24-hour average wind speed and 24-hour average PM-10 concentration at TA-54. The highest reading was observed on May 13.

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Miller, C.W. and L.M. Hively, 1987. "A Review of Validation Studies for the Gaussian Plume Atmospheric Dispersion Model." *Nuclear Safety* 28(4), 522–531.

Rood, A. S., G.G. Killough, and J.E. Till, 1999. "Evaluation of Atmospheric Transport Models for use in Phase II of the Historical Public Exposures Studies at the Rocky Flats Plant." *Risk Analysis*, 19(4), 559–576.

COMMENTS ON THE RISK ASSESSMENT CORPORATION REPORTS FOR TASK 1.5

Task 1.5: Exposure scenarios and risk factors for assessing the air pathway

Tom Buhl, ESH-DO
On page 4, the report states that it will compare the integrated exposure from inhalation to the integrated intake from consuming contaminated produce to determine the relative importance of these exposure pathways. The screening procedure, however, that was used to choose which radionuclides to evaluate only selected those giving a higher inhalation risk, and so may have been biased against those having a higher ingestion risk.

In the case of radioactivity, exposure to external radiation from deposited material (ground shine) and from the cloud (submersion) should also be considered for completeness in addition to the risks from inhalation.

***RAC* Response**: Because inhalation risks are so low, we do not intend to pursue these other pathways for our risk calculations. Environmental monitoring data collected after the fire indicate that soils and produce were unaffected by the fire.

The "weighted daily average breathing rate" for a firefighter in Table 1 is slightly different than the value given in Table 3.)

***RAC* Response**: You are correct. Thank you for pointing this out. We will make certain that the appropriate breathing rate is cited in the final report.

Lars F. Soholt, EHS-20
reference p. 5: the formulation for going from airborne concentrations to risk that is presented at the top of the page is really only applicable to radionuclides. I think that the risk/hazard evaluation here consists of 3 components: 1) risk based upon the intake amount (radionuclides; Bq taken in during the exposure period); 2) risk based upon the intake rate (chemical carcinogens; (mg d^-1 kg^-1)^-1 during the exposure period); and 3) comparison to a reference concentration representing no hazard (chemical non-carcinogens; m^3 d^-1 during the exposure period). So some other parameters that need to be calculated here include the daily inhalation intake per unit mass by the 4 different receptors (which requires some assumptions about weights of the receptor categories). We also need a time-weighted estimate of airborne concentrations at receptor locales to compare to reference concentrations. Another point is that the RfCs and SFs are based upon a lifetime or at least decades-long exposure duration; the actual periods of exposure to CGF smoke are some fraction of this duration and this needs to be taken into account through an 'occupancy factor' or 'contact rate' built into the calculations. For the radionuclides, this is built into the time-integrated concentrations. This brings up something that had not occurred to me before: we have not really discussed the temporal domain of interest, have we? It has been kind of implicit in some of the reports, but it is really important when discussing exposures leading to risk/hazard. Just a thought.

***RAC* Response**: We have revised the calculational methodology since the production of the draft report. Instead of using reference concentration as a comparison, we will be using reference dose to calculate hazard quotient for noncarcinogens. The new equations should clear up most of the questions raised by this reviewer. We use time-integrated concentration for all risk/hazard quotient calculations, eliminating the need to worry about temporal domain.

Jean Dewart, ESH-17
It appears that exposures are calculated for resident adult and resident child for 14 days including the time when the town was evacuated. Will there be assumptions made on the adult and child being in a different location for the evacuation period?

***RAC* Response**: No, the receptors are assumed to be at the same location for the entire exposure period.

Review of RAC Deliverable Task 1.5
Exposure Scenarios and Risk Factors for Assessing the Air Pathway
11/9/01 Kirby Olson

The deliverable proposes comparing total contaminant intake during the fire to EPA's inhalation reference concentrations (RfCs). RfCs are air concentrations (usually in mg/m^3) corresponding to an allowable lifetime daily dose for a receptor with certain default parameters. As a concentration, the RfC is not directly comparable to a total dose from the fire. Also, RfCs use 20 m^3/day as the inhalation rate for the receptor. Each receptor in the RAC scenarios has an individually calculated inhalation rate; none of them are equal to the default one used to generate RfCs. Because the RfCs were often originally derived from subchronic studies, it may be possible to adjust the chronic RfC by removing the subchronic to chronic uncertainty factor and correcting the calculation for the differences in inhalation rate. EPA has a guidance document on how RfCs were calculated: *Methods for Derivation of Inhalation Reference Concentrations and Application of Inhalation Dosimetry, EPA/600/8-90/066F, October 1994.* Unfortunately this paper is not available through EPA's website, so I don't have a copy to send on to you.

There are also other toxicity values that could be used in place of RfCs for assessment of non-cancer risk. EPA is developing reference values for acute inhalation exposure to chemicals that are designed to protect receptors for a continuous exposure of up to 24 hours. Development of these values is described in *Methods for Exposure-Response Analysis for Acute Inhalation Exposure to Chemicals, Development of the Acute Reference Exposure, Review Draft, April 1998 (EPA/600/R-98/051).* This paper is also not available on the EPA web site, so I can't tell you if the values will be in doses or air concentrations. I would anticipate that these values could be put in a form that could be compared to the dose from the Cerro Grande fire. Occupational exposure limits may also provide a good comparison for firefighters and emergency response personnel, but may not be appropriate for resident because they are not designed to be protective of sensitive subpopulations such as children. Also, the basis of occupational standards varies, and needs to be explicitly provided for those standards used for comparison to doses from the fire. The basis for dividing the TLVs by 10 should be provided as well.

***RAC* Response**: A conference call between NMED and *RAC* was held on January 16, 2002 to discuss this issue, and all misunderstandings were resolved. The risk information used to complete the calculations is described in detail in Appendix B to the Task 1.6 report.

For the radionuclides, both morbidity and mortality coefficients are provided in the deliverable, but the text doesn't state if comparisons to both types of values will be provided in the risk assessment or if one set will be chosen.

***RAC* Response**: For radionuclide calculations, morbidity risk was calculated.

The scenarios and parameter values provided in Task 1.5 appear to be both reasonable and well-researched, but the number of days each receptor is exposed is not explicitly provided in each scenario. Table 1 states that each receptor is exposed for the "entire release period" but I'm not clear whether this is the entire time of the fire or the three days that portions of the LANL facility and Los Alamos were on fire or some other number of days. The actual number of days exposed should be provided on the table for each receptor.

***RAC* Response**: Because time integrated concentrations were used to calculate risk, exposure duration (in terms of days exposed) is not a necessary parameter for these calculations.

I have contacted EPA's risk assessors to get copies of the guidance documents I cited here, and will give copies of them to Barbara Hoditschek immediately if I receive them. I will also try to get guidance from EPA regarding extrapolating subchronic values from RfCs.

***RAC* Response:** Again, these documents were discussed during our January 16, 2002 conference call and the reviewer was satisfied that we had obtained the necessary documentation.

LANL COMMENTS ON RAC TASKS 1.6

Task 1.6. Estimation of Risk from Releases to Air within the Model Domain

Tom Buhl, ESH-DO

1. Many valuable conclusions are reached throughout the report. It would be useful to collect these in an Executive Summary, either in the report itself or in a summary report at the end of the project. These conclusions would be placed in the very front of the report and should include –

   - A conclusion on the magnitude of the health risks found in the study – are there health concerns with the radionuclides, chemical carcinogens, or chemical non-carcinogens

   How the Cerro Grande fire affected levels of airborne contaminants, and whether the Laboratory contributed significantly to these levels

   RAC Response: We added an Executive Summary to the final report for the Air Pathway (Task 1.7).

Lars F. Soholt, ESH-20

2. P. .55, second bullet from the bottom. it looks like the RfDs that were used were for chronic exposure (see p. 60, first paragraph) not subchronic as stated here; also, it looks like the chemical risk calculations are base on a lifetime (70y) exposure rather than subchronic.

   RAC Response: We changed the wording with regard to the RfDs in the Task 1.7 report to reflect the fact that subchronic values were used **when available**. This is further explained in Appendix E. We also have labeled the RfD values that were used as "chronic" or "subchronic" in Table E-2 of the summary report.

   The 70 years is the averaging time for the risk calculations, not the exposure duration. This is a standard averaging time for chemical risk calculations with slope factors.

3. In Tables 22 & 25 the hazard values are identified as HQs; I am assuming that these values are the sums of the HQs for individual non-carcinogenic toxicants, so the correct term here would be the Hazard Index (HI).in equations 25 and 26 the units for BR change to m^3 per day rather than m^3 per second; also, the TIC changes to mg-d per m^3. or, SF, RfD, and AT need to be expressed in seconds.

   RAC Response: We have continued to use the term Hazard Quotient to describe even these sums of HQs for consistency and to eliminate reader confusion.

4. In Table B-2 the units for RfD are inverted; should be just mg/kg-day.

   RAC Response: The reviewer is correct in noting the inversion of the units for RfD. This has been corrected in Appendix E to Task 1.7.

Jean Dewart, ESH-20

Appendix A

1. It would be helpful to have a discussion of the spatial representativeness for the vegetation and soils contamination data used in these calculations. The reason this would be of interest, is these values are assumed to represent all 47,000 acres that burned - this

could lead to very conservative calculated air concentrations. On page A-16, the document indicates that the air concentrations from burned vegetation are significantly higher than the air concentrations from the burned PRSs. This might not be true - rather an artifact of assuming the contaminant data represent all 47,000 acres that burned. If one assumed that the PRS contaminant data represent of all 47,000 acres burned, then the two sources of air concentrations might be similar.

RAC Response: We did not have sufficient data to comment on the spatial representativeness of either the vegetation or PRS soil concentration data. However, to avoid overestimating the contribution from vegetation and thereby understating the relative contribution from PRSs, we made comparisons using the minimum of the range of vegetation concentrations we were able to locate. Further, we included a comparison that shows the contribution of the litter and bark portion of vegetation only, which is likely more representative of what actually burned. These assumptions are not unrealistic and would not be expected to overstate the relative importance of burning vegetation across the 47,000 acres that burned because our calculations are based on actual fuel loading data for the area.

In contrast to the assumptions made for estimating the contribution from burning vegetation, we made conservative (i.e., likely biased high) assumptions to develop both inventory and release estimates for the PRSs. It would not be realistic to assume the PRS data as representative of the entire 47,000 acres that burned.

Assuming the minimum of the range of vegetation concentrations for comparison to likely high estimates for PRS releases suggests that the % contribution from burning vegetation we calculated and reported may in fact be underestimated. Even so, the contribution from vegetation appears to be the dominant contributor to chemical and radionuclide air concentrations during the fire, and the risks associated with maximum vegetations concentrations are less than risks defined as acceptable by the US EPA.

2. On page C-1, "hospital emissions" should be "hospital admissions".

RAC Response: We corrected the spelling.

3. The public has been concerned that a plume of LANL pollutants was released during the fire, lofted to high altitudes, and settled down in Truchas, Taos, eastern NM, and Oklahoma - missing any of the ambient monitoring. This idea is reinforced by the satellite photos showing the fire plume travelling hundreds of miles east and north. The modelled air concentrations indicate that pollutant concentrations fall off with distance. Can the report address why the plume was observed to travel so far, but the concentrations do not.

RAC Response: A section has been added to the Task 1.7 report (Section 4.4.1) to address the transport of chemicals and radionuclides entrained in the smoke plume outside of the model domain. The concentrations in the plume decrease with distance as a result of dilution in the atmosphere and depletion of materials entrained in the plume. This does not mean that the plume would not be visible, as it would still contain very small particulate matter capable of traveling in the air hundreds of miles or more. Predictions can be made for concentrations

at more distant locations outside the model domain, but time and computing capability constraints dictated that we focus our evaluation on locations within the model domain. However, based on the results of the analysis presented in Section 4.4.1, it is unlikely that concentrations exceeding those predicted in Espanola would occur at the more distant locales noted by the reviewer.

Responses to Comments from Kirby Olson

Review of RAC Deliverables Task 1.6
Estimation of Risk from Releases to Air Within the Model Domain
02/15/02 Kirby Olson

This deliverable does a good job of addressing the comments and concerns regarding earlier deliverables. Appendix A models the potential releases of radionuclides and metals from burning vegetation using data collected by LANL, NMED, and data from literature papers. The release estimates are then assumed to be proportional to the PM10 releases (as were the potential PRS releases) and the same burn model is used to generate an estimated dose to the same set of receptors. The appendix also includes a comparison for different locations/receptors of the proportional contribution from burning vegetation and from PRSs. This information in this appendix is a good response to the concerns raised by reviewers regarding the issue of background.

Appendix B describes the toxicity data, and includes the responses to issues raised over previous deliverables. The reference air concentrations (RfCs) and threshold limit values (TLVs) were adjusted to compensate for the differences in body weight, breathing rate, etc. between the receptors in this model and the assumptions used to generate the toxicity benchmarks. For noncarcinogens, chronic toxicity data was still used for a number of compounds, but this appears to be unavoidable since subchronic values were unavailable.

*In reviewing the main text, I noticed one problem. I was unable to get the units to cancel out for equation 26 (HQs for noncarcinogens). Assuming that the TIC for this equation is the g-s/m^3 mentioned on page 55, I kept getting g/(mg*kg^2). Was there a conversion factor in that equation that wasn't reproduced in the text?*

RESPONSE:

Equation 26's units do cancel out, but the units for Reference Dose are not written in such a way that they are easy to decipher. The units for RfD are mg kg^{-1} d^{-1} and they are written in the equation definitions as mg kg-d^{-1}, which assumes that the reader puts the kg-d together and inverts them. Unfortunately, it appears that this has carried through into the draft Task 1.7 report. We will clarify these units for the final version of Task 1.7.

Responses to Comments from Mary Uhl

Subject: Comments on Task 1.6
Date: Wed, 13 Feb 2002 05:23:16
From: "Mary Uhl" <maryuhl@hotmail.com>
To: Barbara_Hoditschek@nmenv.state.nm.us

Barbara,

I have finished reviewing Task 1.6 and am generally in agreement with the air dispersion modeling and emission modeling methodologies. RAC has done a good job with the modeling, as evidenced by the reasonable agreement of modeled particulate matter with observed (within a factor of 2). I commend them on the successful completion of a difficult task.

A few minor comments:
On page 5, there is mention of no wet deposition being considered, but no mention that dry deposition is considered, although later in the report that is explained. It would be useful to mention the dry deposition sooner.

RESPONSE:
We have included this information in Section 3.3 of the Task 1.7 summary report, as suggested.

On page 19, Eric should be "Erik".

RESPONSE:
This is corrected in the Task 1.7 report.

Figure 2--it would be helpful to show pre- and during-fire samples for each site on the same graph, perhaps color-coding would be appropriate.

RESPONSE:
Figure 2 was found to have problems and was omitted from the Task 1.7 report. The problem was in the graphing software that erroneously plotted the minimum, maximum, and mean concentrations for some of the locations. Instead, we show a time-history plot of PM10 concentrations at various locations both in an out of the model domain. While this plot is difficult to use quantitatively because there are many data points, is serves as a good qualitative review of the data.

I still wish there was a better emissions processor than EPM--hopefully the revision that is currently being worked on will result in a better model. For the time being, looks as though we are stuck with EPM and the problems with the smoldering stage.

RESPONSE:
We agree.

Again, Art has done a good job with the modeling portion, and this is not an easy analysis.

I didn't review 2.5 and 2.6, as these appear to be entirely related to the water pathway.

Please let me know if you have any questions--I'll be back in the office on 2/18, or you can call me at 505-670-9022 if you need to speak with me
sooner.

Mary

Get your FREE download of MSN Explorer at http://explorer.msn.com/intl.asp.

RESPONSE: We appreciate Ms. Uhl's comments and will let her know when the new EPM model is released. We will ignore this advertisement by Microsoft for their new internet explorer!

Responses to Comments from John Pinder

COMMENTS ON TASK 1.6: ESTIMATION OF RISK FROM RELEASES TO AIR WITHIN THE MODEL DOMAIN

FROM: *John Pinder*

GENERAL COMMENTS

In my earlier review of Task 1.4.2 I commented that the task report reflected a comprehensive and often innovative effort to merge the science of several disciplines into a model that will address the distribution and deposition of PCOC released from burning PRS sites. I think it is important to reinforce that statement in regard to the Task 1.6 report. This is a significant piece of work.

My earlier review of Task 1.4.2 also raised questions concerning the parameter estimates in the model, the estimation of model accuracy using PM10 data, and the possible errors introduced by not considering wind effects on background PM10 concentrations. I should now state that I believe that the current task report has satisfactorily addressed these concerns. The revisions to parameter values, the analyses of wind effects on PM10 concentrations, and the testing of model predictions against PM10 data adjusted for wind velocities has resulted in a much more confidence inspiring model of plume formation and dispersal. The model appears to project the current, limited understanding of plume formation in fires into accurate estimates of plume dispersal into the surrounding area.

GENERAL RESPONSE:

We appreciate this reviewer's comments as well as the endorsement of our modeling effort.

SPECIFIC COMMENTS

PAGE 8

The increase in assumed soil temperature from 600 to 1000°C makes this section even more conservative than it was before. Perhaps it would be worthwhile to include some discussion of the degree of conservatism applied. My calculations, which are based on some simple assumptions and may be affected by some misinterpretations, suggest a > 70X conservatism based on: 1) assuming that all the contamination is in the upper 2.5 cm rather than uniform over 0.18 m which introduces a conservative factor of 7.2; 2) assuming a 1000°C temperature throughout the top 0.025 m rather than a linear gradient of 500°C to 100°C which introduces a factor of 3.33X; and 3) assuming that the same proportion of PRS sites are severely burned as LANL (i.e., 34 %) rather than 100% which introduces a factor of 2.94X. The total conservatism just based on these assumptions becomes 7.2 x 3.33 x 2.94 = 70.6. This doesn't include the assumption of 100 % release of PCOCs. The value of this computation – or a more defensible version of it based on the actual change in which PCOCs would be affected by the temperature changes – would be that it would provide some simple perspective on the occurrences of HQ > 2 on LANL.

RESPONSE:

In the Task 1.7 report, which includes the final version of the material included in Task 1.6, we discuss uncertainties and conservatisms in the calculations in a qualitative fashion. A detailed discussion of the conservatism and uncertainties involved in the source area concentrations was added to Chapter 3 of the Task 1.7 summary report, "Source Term Development" (Section 3.2.5, "Limitations and Uncertainties Associated with Estimated Source Area Concentrations"). Other conservative assumptions that may have led to an overestimation of risk estimates are described in the sections corresponding to the stage of the risk assessment process during which they were made. A summary of the conservatism in the exposure and risk estimates is provided in Chapter 6 of the Task 1.7 summary report for the air pathway. We agree that the occurrences of HQ >2 on LANL are most likely due to the conservatisms built into our calculations and not due to actual excursions above reference doses for these materials.

PAGE 8

A figure is given of 850 m2 for the area of PRS burned. Is this correct? Is the area of PRS sites burned actually less than that enclosed by a square plot of ground that is 100 feet on a side? If so, perhaps this should be stated.

RESPONSE:

This figure is not accurate, and it has been corrected in the Task 1.7 summary report (Chapter 3, Section 3.3.1.2). The total area of the burned PRS sites that comprise the source term is approximately 855,000 m^2.

PAGE 11

More than ½ the organic compounds in Table 2 degrade in the atmosphere with half-lives less than 24 hours. Although it is clear and defensible why these degradation mechanisms and products cannot be incorporated in the model with present databases and technologies, it would be useful to discuss those that may have degradation products that are significantly more toxic than the measured PCOCs. Discussing the possibility of more toxic products would be in keeping with the emphasis on maintaining a conservative approach. Further, if a more toxic product is formed, the risk computations could be alternatively computed assuming that all the parent PCOC is immediately converted to this product and dispersed in the atmosphere.

RESPONSE:

Your point is well taken and we appreciate your understanding of the difficulty of incorporating degradation processes into the fate and transport calculations. Your suggested remedy also seems reasonable and if resources are available, we will investigate degradation products in the final Task 1.7 report.

PAGE 20

The data in Table 7 are interesting but may raise more questions than they answer. In particular, the PM10 concentrations at these stations are 2 to 3 times greater than most of those at TA-54. There are no pre-fire or post-fire data from these locations to tell whether the greater values actually represent greater impacts at these sites or differences in methodology between these data and those in Figure 2. Perhaps these data should be omitted.

RESPONSE:

The data in question were taken by the EPA during the Cerro Grande Fire. Measurements were made at locations where no other measurements existed and therefore, filled in important gaps in the spatial coverage of measured concentrations. For this reason, they were included in our assessment. However, the data are less than desirable. We were unable to obtain the original sampling records from EPA and we are not certain what time period they were taken over. That is, the time when the sampler was turned on and the time when the sampler was turned off. It appears that the EPA was "plume chasing" when they performed these measurements. While the data set is less than desirable, they do fill in important gaps and aid in our understanding of the behavior of the fire. Namely, the behavior of the smoldering phase of the fire from the large areas burned on May 10th and 11th. Additionally, other reviewers and concerned citizens criticized us for not using these data in the Task 1.4 report and we had to be responsive to their concerns. For this reason, we will present the data and use it with the noted caveats.

PAGE 21 AND FOLLOWING

The use of the statistic "r" is inappropriate. It should only be used where the distributions of both x and y are approximately normal which – as illustrated by Figure 4 – they are not. Some additional data snooping in this area might prove useful. Especially in looking at log-log relationships between PM10 and wind velocities. The use of a linear model does not seem completely appropriate for the data in Figure 4, and a linear model might not be expected for the mass carrying capacities of wind as a function of velocity. This is offered as a suggestion and ***not*** *a criticism of the existing analysis.*

In Table 9, the absence of a correlation during the fire is taken as an indication that wind velocities were not the cause of greater PM10 concentrations. This does not follow when TA54 is in the midst of the fire.

Moreover, the critical question is whether the large PM10 concentrations at more remote locations are affected by wind velocity which is indicated in Table 8 and Figure 4. This argument could be dropped because the fundamental point is that the model can explain elevated PM10 concentrations at remote locations after adjusting for the effects of wind (Figure 9).

RESPONSE:

We assume the reviewer is referring to the use of "r" as a linear correlation coefficient and not the r derived from some transformation of the data. The comment did spark our interest on the correlation between wind speed and measured PM10 and other transformations were considered. Soil suspension is known to increase as a function of the wind speed raised to some power, typically 3 to 5. We performed this transformation on the mean 24-hour wind speed using an exponent of 3 and subsequently took the natural log of the quantity. Performing the regression of the 24-hour average PM-10 concentration against $\ln(u^3)$ where u = the 24-hour mean wind speed provided a much improved regression for both Santa Fe (r= 0.99) and TA-54 (r = 0.77). This model was used in our final calculations in the Task 1.7 report.

I am not sure I understand what the reviewer is driving at in regards to the comment in the second paragraph. Perhaps we can state more definitively that there in an inverse relationship between

PM-10 and the mean 24-hour wind speed in the valley locations compared to a station like TA-54, which is not situated in a valley.

In regards to the last paragraph of the comment, we agree that the fundamental point is that the model accounts for increased PM10 due to higher wind speeds.

PAGE 32

Because you are trying to predict the 97.5% of the distribution of M rather than a confidence interval for M in equation 21, z should be used instead of t. However, depending on the df, t $\geq$ z.

RESPONSE:

We have erroneously used t instead of Z. In this case, t values were ~2 while the Z value (for the 97.5 percentile) is 1.92. Therefore, the fuel loading data were slightly over estimated. We will make this change in the final Task 1.7 report.

PAGE 38

The greater deposition rates east of Espanola are hard to see in Figure 12. Because there may be local interest in this pattern, perhaps a separate figure with different color patterns could be inserted.

RESPONSE:

We have re-plotted all the figures in the Task 1.7 report omitting the color shading so the different contour lines can be easily distinguished.

PAGE 42

Most of the PRS sites were assigned to two fires on May 11. Which other fires involved PRS sites?

RESPONSE:

Other fires include the May 12-13 fire and the other two fires on May 11. We have provided a more detailed accounting of the fires in the Task 1.7 Report. We will also provide in an Appendix in the Final Task 1.7 report that lists all the PRS Sites that burned, the chemicals and radionuclides detected at each of these sites, and the fire ID attached to each PRS.

PAGE 44 AND FOLLOWING

The inclusion of Figures 13 through 17 is extremely useful in illustrating model predictions and the variability among dates in plume dispersal.

RESPONSE:

Thank you for this comment.

PAGE 57

It took some considerable time for me to digest the risk factors in Tables 20 through 22. Some supporting discussion showing that risks generally declined in the order Ta54 to San Ildefonso to Espanola to Los Alamos to White Rock to ... would be useful. In particular this would highlight the fact that risks were greater, although negligible, in Espanola – a more distant location – than

in Los Alamos or White Rock due to the pattern of dispersal of the plume on the days that most PRS sites were involved. This is an interesting result that is important to the communities involved and should be made more apparent.

RESPONSE:

Accepted. We also think another important result to draw attention to is the extremely low risk across the entire model domain. We do include a greater discussion of risk in the Task 1.7 report.

PAGE 58

Tables 23 and 24 are useful for showing locations of greatest risk for various PCOC. However, the use of UTM is difficult to extrapolate to the existing maps because of their scale. It would be useful to provide plots of these locations for the 4 most significant risks for radionuclides and the 6 most significant risks for chemicals. These would more clearly communicate the spatial patterns of these maximum risk locations to the reader.

RESPONSE:

We have revised this discussion in the Task 1.7 report, removing those tables because of the confusion they caused. We have also added isopleth maps of risk when appropriate.

COMMENTS ON APPENDIX A

This is very interesting analysis! It clearly shows that releases from PRS sites were small compared to the possible remobilization of materials from areas not defined as waste sites. This has general implications for the evaluation of radionuclide distributions at DOE sites. Is there any plan to expand this analysis?

The concentrations in air projected from this analysis appear to be greater than those actually measured. The report discusses the fact that this may be due to the conservative assumption that all biomass was completely consumed in the fire. Another possible explanation is that much of the material was released in the flaming stage of the burn and thus dispersed by the greater heat production to higher altitude plumes than the material released in the subsequent smoldering phase to lower altitude plumes.

Is there a possibility that a more detailed analysis of these data and matching them to the greater number of measured dispersal data could resolve some of the questions concerning the EPM and dispersal models? There may be some ability to use the relative abundances of different isotopes in different plant and litter materials to partition burning and dispersal processes.

RESPONSE:

We agree with the reviewers suggestion and if time and resources permit, we will take a closer look at the measured data and compare it to model estimated concentrations. One problem with these comparisons is that the measurements did not use consistent time-averaging periods or spatial coverage, making it tedious and time-consuming to compare with modeled data. We also agree with the reviewers suggestion that the overestimation might be due to the emissions of radionuclides and metals occurring mostly during the flaming portion of the fire. This factor can

be important and if time and resources permit, can be looked into in greater detail. We should note however that this analysis is ancillary to the primary goals of this project and any future analysis will be given a relatively low priority.

COMMENTS ON APPENDIX C

This appendix is also useful in evaluating competing risks from the effects of the fire. Although assessing the distribution and fates of the common components of smoke is beyond the scope of the project, it would be useful if the statement "PM concentrations at various times and various locations ... were sufficient to cause adverse health effects" was supported by a map showing those locations. This would also require the discussion that these effects were related only to the fires being analyzed and that additional effects may occur within the mapped area due to fires in the area surrounding LANL that were not explicitly considered in the current analysis.

RESPONSE:

We could make a plot showing the locations where the PM10 exceeded some threshold value, say a regulatory limit. For example, an isopleth map showing the extent of 24-hour PM10 concentrations that exceeded the EPA 24-hour limit of 150 $\mu g\ m^{-3}$ might be considered. However, we could not extend this statement to state that this is where adverse health effects may have been incurred because the level of PM10 where health effects are observed is apparently still under debate.

RESPONSE TO COMMENTS BY BERND FRANKE

Comments on Task 1.4, November 25, 2001.

Review of RAC Draft Reports "Analysis of Exposure and Risks to the Public from Radionuclides and Chemicals Released by the Cerro Grande Fire at Los Alamos"

These comments describe work performed by the Institute for Energy and Environmental Research based in Heidelberg, Germany (IFEU) under contract with the Department of Radiological Health Sciences, Colorado State University. Given the limited resources and the overall scope of the project undertaken by RAC, these comments are cursory in nature and focus on my particular areas of expertise.

General comments

The approach of the project appears to be suitable to achieve the stated objectives. The technical memoranda are well documented and allow an in-depth review of the models and data used. I note that a major objective is to determine the "magnitude of incremental exposure and associated risks to the public, emergency response personnel, and firefighters from transport of **radionuclides and chemicals** associated with the LANL facility released as a result of the fire through the air transport pathway (emphasis added). This excludes the risk associated with particulate matter (expressed as PM10 or PM2.5). In order to put the results of the study in a meaningful context, the calculated risks should be compared to the detrimental effects expected from PM10 exposure. Using the WHO[1] risk factor of 1% to 4% increased annual mortality per 10 $\mu g/m^3$ of PM10, a 10-day exposure to 100 $\mu g/m^3$ of PM10 is equivalent to a mortality risk of 4×10^{-5} to 1.6×10^{-4} if a linear dose response relationship is assumed. It is thus possible that the calculated risk from PM10 is greater than that from all chemicals and radionuclides combined. Since the overall risk of the consequences associated with air pollutants resulting from the fire is of concern, calculating the contribution of the risk associated with radionuclides and chemicals associated with the LANL facility to the total risk appears to be a useful and necessary piece of information.

RAC Response: Please see the response to the PM10 issues raised in the Take 1.7 comments later in this response.

NEWNET station weather data

The CALPUFF modeling relies extensively on meteorological data provided by the NEWNET stations. In the course of the LANL Air Quality Audit, I discovered in July of

[1] WHO (1999b): World Health Organization and United Nations Economic Commission for Europe (Task Force on Health Aspects of Long Range Transboundary Air Pollution) Health risk of particulate matter from long range transboundary air pollution – preliminary assessment. WHO European Center for Environment and Health, Bilthoven Division, 1999

2000, that wind directions in NEWNET output files were incorrectly reported because of an incorrect algorithm in converting vector data to wind speed and wind directions. While the algorithm should have read 270+arc sine, it was reading 270-arc sine in the algorithm. Dane Knowlton confirmed this in an August 3, 2000 email. The error was corrected shortly thereafter in the database and all downloads should to reflect the correct wind direction. I confirmed that this change did indeed take place for the East Gate NEWNET station. The RAC team did not use the Los Alamos High School NEWNET wind direction data "because of the significant difference in predominant wind direction". This is evident in the comparison of wind direction data from three stations in close proximity: East Gate, DP West and Los Alamos High School (LAHS) for May 10, 2000 (Figure 1). The wind direction data for LAHS on May 10 matches the data for the two other locations much better if a value of 180 degrees is added to the reported direction data for the second half of the day. In contrast, the data for May 29, 2000 for the three stations shows a good agreement in reported wind directions (Figure 2).

Figure 1. NEWNET wind direction data for three stations on May 10, 2000 [NOT SHOWN]

Figure 2. NEWNET wind direction data for three stations on May 29, 2000 [NOT SHOWN]

The inspection of the data may suggest that not all data was corrected in August 2000 and that the May 10, 2000 wind direction data for LAHS may be plagued with a systematic bias. I suggest exploring this hypothesis, i.e. determining whether or not all NEWNET wind direction data as it can be downloaded from the NEWNET server for May 2000 is indeed accurate for all stations. If the data is obviously incorrect for one station (LAHS) on some days but appears to be correct for other dates, how can one be sure that the data for other stations is accurate? I suspect that a likely source of error is in the conversion of NEWNET instrument data into the data stream submitted to the server and the subsequent conversion of the raw data dump received by the NEWNET server into the format provided to users of the site. That this conversion is not to be trivial and that erratic data is introduced by "line noise" became evident when inconsistencies in reported gamma dose rates and wind directions were discussed with LANL staff during last year's clean air audit.

In addition, I note that not all NEWNET data was used for CALPUFF modeling. I note that data for the San Ildelfonso Pueblo was not used "because of placement of the station between two buildings." Visual inspection of the station information on the NEWNET website (http://newnet.lanl.gov/info.asp?number=1703&loc=3) indicates that while the station is indeed placed between two buildings the wind direction sensor is placed significantly above the building roof. Having said this, I do believe that the inclusion of the San Idelfonso station will result in a major change on the overall results. A comment on this would be appreciated.

RAC Response: We share you concerns about the meteorological data; however, we really did not have the luxury during this project to do an in-depth quality control evaluation of all the NEWNET stations. We plotted a wind rose for the month of May, 2000 for each station we considered using in our CALMET model simulation. If the wind rose from a given station appeared to be reasonable based on knowledge of the prevailing winds, then we used that station in the CALMET simulation. We did not use the San Ildefonso station for the reasons stated in the report. Comparisons of wind vectors and air concentrations with and without this station were not substantially different. Please note that the initial trajectories of the plume were really dictated not by surface conditions but winds aloft. Measured wind speeds and directions aloft were extracted from the upper air data taken routinely (every 3 to 6 hours) at Albuquerque International Airport.

Tritium

The RAC Task 1.3 report states that tritium is excluded from further examination because (a) no samples at any PRS location had any concentrations of tritium and (b) tritium was likely not released from LANL as a result of the fire, but rather from routine emissions. Given the magnitude of measured concentrations around TA-54 and the fact that if the measured concentrations were associated with the fire, the associated screening risk index would exceed the 10^{-5} level, it appears appropriate to indicate the magnitude of tritium release from nonroutine sources that would be necessary to exceed the screening risk index of 10^{-5}. Is it possible that HTO from routine releases has deposited in a wider area around the stacks from which it was released and that the fire evaporated this reservoir of HTO?

RAC Response: We used all the available soil characterization data to estimate inventories for each burned PRS. The statement about there being no samples at any PRS location with detected concentrations of tritium was incorrect in the original Task 1.3 memo. It is not included in subsequent reports.

Partitioning of Chromium Releases

In the RAC screening process, a ratio of 1:6 Cr(VI):Cr(III) was assumed. Since chromium has the highest calculated screening risk index, this assumption deserves some further explanation. A report of SAI as part of the Cumulative Exposure Project of the USEPA states[2]

"Analysis of the airborne measurements of hexavalent chromium in Southwestern Ontario is presented in (Bell and Hipfer, 1997). In the natural environment, Cr(VI) is less stable than Cr(III) in the presence of organic and inorganic reducing agents. From the ambient air studies, Cr(VI) was found to comprise less than 1% of the

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[2] 2 Systems Applications International, Inc. Modeling Cumulative Outdoor Concentrations of Hazaredous Air Concentrations, Attachment 6: Molecular Form of Emissions of Se-lected Pollutant Classes. SYSAPP-99-96/33r2. February 1999, available from:
http://www.epa.gov/cumulativeexposure/CEPpdfs/att6.pdf

total airborne chromium in rural areas, but between 10 and 40% of the total chromium in urban/industrial areas having known chromium sources, with a three year average of approximately 20-25%. Similar results were obtained during field studies in New Jersey (Scott et al., 1997). For 19 site concentrations Cr(VI) represented 23% of total chromium on average."

Given the large variability of Cr(VI):Cr(III) it is certainly suitable to select 1:6 for screening purposes. For final risk calculations, a closer look at the realistic ratios appears necessary.

RAC Response: This is an important consideration and the information you provided on realistic values from field studies is very useful. The EPA's inhalation slope factor is derived for a Cr(VI):Cr(III) ratio of 1:6. We also estimated a more conservative risk value using a slope factor for Cr(VI), essentially assuming that all of the chromium is the more toxic and carcinogenic form. We agree that realistic risk values would be important to determine if the HQ for chromium is large. LANL researchers may have information on the ratio that has been found at waste sites from characterization studies done for CERCLA. Although it would certainly help refine the risk values to be more realistic, currently a determination of realistic values for the ratio is not within the scope of this project.

Differences between calculated and measured air concentrations

The results and discussion section of Task 1.4.2 draft report is certainly most interesting and a logical starting point for many questions regarding the model and the data, which was fed into it. As the authors themselves state, the results indicate that the major part of the source term is not adequately reflected with the model assumptions and input data. I have no access to the raw data, which was used to derive the PSR contaminant inventory that is a crucial factor in the calculations; therefore I have to rely on analogies and ballpark estimates. I suggest relying on measured data and evaluating whether the observed air concentrations can be reasonably explained by the available inventory at the LANL site. For example, let us assume that the maximum air concentrations at the LANL for Am-241 of 230 aCi-d/m ^{3} were associated with an average PM10 concentration of 100 µg-d/m ^{3} . If we further assume that 20% of the PM10 actually consisted of resuspended soil particles, the soil concentration in soil would have to be as large as 11 pCi/g (0.43 Bq/g) of Am-241 in order to explain the observed concentrations. The largest soil concentration reported in the 1998 Environmental Surveillance Report for LANL[3] was 0.014 pCi/g on the TA-21 (DP Site). The observed discrepancy is indeed puzzling. Likewise, the reported concentrations for chromium (Table 11 of RAC Task 1.4.2 reports 1.4 to 3.8 µg/m ^{3}) and for iron (14–1260 µg/m ^{3}) are difficult to interpret. I have no access to the reference source[4] and do not know about the locations and sampling

[3] Environmental Surveillance at Los Alamos during 1998, LA-13633-ENV, Table 6-1 on page 247

[4] ATSDR 2001b. *Health Consultation: Potential Public Health Impacts from the Cerro Grande Fire Airborne Emissions*. U.S. Department of Health and Human Services, Atlanta, Georgia. Public Comment Release June 28, 2001.

times. If the value represents a 24-hr average and chromium were to come from resuspended soil (assuming again that 20% of PM10 or 20μg/m ^{3}), the maximum concentration of chromium in soil at LANL (14 μg/g at the R-Site Rd) could not explain this value. The chromium content in wood (I assume an average value of ~10 mg/kg dry) yields chromium content in ash (0.5% ash content wood) of 0.2%. If one were to assume that the PM10 at the time of chromium sampling consisted entirely of wood ash, I calculate that the PM10 concentration during chromium sampling was between 700 and 1,900 μg/m ^{3}. Can that be correct?

Based on information I have, the concentration of iron in wood is similar to that of chromium; a value of 14–1260 μg/m ^{3} of iron in ambient air is difficult to believe. Could there be a typographical error in the table?

It is obvious that I have to rely on guesswork in order to make sense of the results. Based on what limited information I have, I believe the following approach to be sensible:

- Determine the quality of ambient air data taken during the fire, accounting for potential bias and uncertainties in sampling and analysis;
- Determine the inventory of radioactive and non-radioactive pollutants which can be expected in the fly ash of vegetation consumed in the fire;
- Calculate a term for soil resuspension in CALPUFF modeling; and Use observed concentration data to fractionate between contribution from vegetation fly ash, resuspended soil and LANL PRS contribution.

I may have to revise my suggestion once I had the chance to review the quality of the PRS inventory data and the raw data for measurements in air of selected analytes (in particular Am-241, Pu-239/240, chromium and iron).

RAC Response: In subsequent versions of this material (in the Task 1.6 and 1.7 report) we have removed the reference to the measured air concentrations. After thinking about it for some time, it was apparent to us that a meaningful comparison between the predicted, 3-day time integrated concentration and concentrations of various averaging times could not be made. The problem with most comparisons of measured and predicted radionuclide and chemical concentrations is that we do not really know the short-term fluctuations in background concentrations of these compounds. Most pre-fire measurements were longer-term averages (presumably quarterly for most samples). Without a clear understanding of the short-term fluctuation in background concentrations of these compounds, it is difficult to make any comparison or perform any back-calculation of source term. For PM10, we did not have this problem. We had relatively long time histories of 24-hour average PM10 concentrations at various locations in the model domain. After correcting for background contributions, we saw a clear signal from the Cerro Grande Fire in the measurements.

Comments on Task 1.7 , May 10, 2002

Review of RAC Draft Report to Task 1.7 "Estimated Risks to the Public from Releases to Air"

These comments describe work performed by the Institute for Energy and Environmental Research based in Heidelberg, Germany (IFEU) under contract with the Department of Radiological Health Sciences, Colorado State University. Given the limited resources and the overall scope of the project undertaken by RAC, these comments are cursory in nature and focus on my particular areas of expertise.

General comments

This document is difficult to understand without the appendices that are referred to. My review job was made difficult by the fact that I was not informed about the schedule of the report and the status and content of other task reports (mainly those dealing with water pathway). I learned from a recent phone conversation with Joni Arends (CCNS) that the presentation of RAC's final report is planned for June 5, 2002. Had I been informed about this, I would have planned (and expedited) my review job in a different way.

Comparison of estimates with observed concentrations

The report concludes that the contribution of radionuclides and heavy metals from burned vegetation is much larger than that from PRS. What is lacking is a discussion of the fact that the maximum predicted concentrations for ^{239,240}Pu and ^{238}U are smaller than the maximum observed concentrations summarized in chapter 2. The table below summarizes the data. [TABLE IS NOT REPRODUCED]

I note that the estimates for air releases associated with burned vegetation are based on measurements in (Gonzalez 2001) for conifer bark (for ^{239,240}Pu: maximum2.8 x 10^{-2} pCi/g, minimum 1.8 x 10^{-5} pCi g^{-1} fresh weight). I attempted to download the document (Gonzalez 2001) from the LANL website without success. I therefore have no information about the frequency distribution of the measurements. It appears to me that the maximum value of 2.8 x 10^{-2} pCi g^{-1} fresh weight dominates the calculated ^{239,240}Pu inventory and is mainly responsible for the calculated maximum air concentration. While this is a conservative approach, it still does not agree with the maximum observed air concentrations. This is also true for ^{238}U. I understand that the RAC team has concerns about the quality and lack of completeness in the documentation of the environmental data. I share these concerns. However, the discrepancy between predictions with a very complex model with many parameters does and observed values is puzzling and deserves a much closer look at the monitoring data. One should bear in mind that the sketchy nature of the radionuclide-specific monitoring data does not allow to determine a the upper limit of the air concentrations. Hence it is possible that the monitoring data may be biased low. Without a reasonable explanation of those and other differences, I consider the report incomplete and inconclusive.

RAC Response: In the final version of the Task 1.7 report, we discuss the measurements you are referring to; in particular, the ^{239}Pu measurements. If you look at the quarterly-averaged samples, the average of the pre and post fire concentrations are not significantly different from one another. While some stations had higher ^{239}Pu concentrations for the quarter that included the fire compared to the prefire quarter, others exhibited lower concentrations. During the fire, sampling times were reduced to 1 or 2 days. Although most of the measurements were below detection limits, several samples had concentrations in the hundreds of aCi per cubic meter. The EPA also measured some high concentrations although the sampling time for these samples could not be verified. The fact that the quarterly-averaged samples that include the fire do not show significantly higher concentration than the prefire quarter suggests that the concentrations of ^{239}Pu were not substantially different than background. The problem as we see it is that we really do not know what the short-term fluctuations in background ^{239}Pu concentrations are because no short-term (1-2 day sampling time) pre-fire samples were taken. The same could be said about ^{238}U. We have suggested to the laboratory in the Task 3 report that they characterize background concentrations of key radionuclides in the short-term, so as to have relevant data to compare to in the event of an episodic release. Dr Whicker has also responded to these comments in a separate letter.

PM10 Risk

I'd like to reiterate my concern that the RAC fails to quantify the health effects of PM10. The statement on page 6-3 that "we did not quantify the risk of adverse health effects from PM exposure because research in this field has not adequately determined what concentrations cause health effects over what time frame" is disappointing. If one compares the magnitude of PM10 health data with the extent of data supporting the risk factors for the radionuclides and chemicals for which RAC feels comfortable to calculate risks for, I'd like to argue that the evidence for demonstrated health effects at the levels observed or calculated during the fire is much greater for PM10 than for the radionuclides and chemicals for which risks were quantified by RAC. This is not a trivial matter. Given the fact that the calculated risks from carcinogens is well below 10^{-6}, and that available risk factors such as those from WHO for PM10 suggest PM10 risk to be orders of magnitude larger than those calculated by RAC for all carcinogens combined, I believe that a balanced presentation of the scientific facts is appropriate. I consider PM10 a mixture of chemicals and the report should quantify the PM10 risk is necessary to complete the picture. Without this I consider the report incomplete.

RAC Response: It is probable that the calculated risk from PM10 is greater than the risk from all chemicals and radionuclides combined. As you point out, any comparison needs to note that PM is emitted from any wildfire and its release is probably not related to current or past activities at LANL. Comparing risks estimated for chemicals and radionuclides to the detrimental effects expected from PM10 exposure might be one way to help put the risks into context. However, we hesitated to do this for a number of reasons. Characterizing the risk from PM was not specifically in the scope of work for this project. Because of public interest, we address it in Appendix F of the Task 1.7 report. Appendix F is lengthy and describes much of the epidemiological evidence available on PM exposure. Appendix F also includes a description of other toxic components of wood smoke. In light of your comments we strengthened the discussion to acknowledge that dose-response values exist. Appendix F now contains much of the following.

The EPA's PM10 standards are 150 μg m^{-3} for 24 h average and 50 μg m^{-3} (expected annual arithmetic mean) averaged over 3 years (EPA 2002). Although epidemiological studies of regional PM10 air monitoring data in many U.S. cities demonstrate increases in daily mortality and morbidity trends at levels less than the current NAAQS, the EPA has not developed toxicity values or risk factors that can be used for risk assessment.

The WHO risk factor (1% to 4% increased annual mortality per 10 μg m^{-3} of PM10) for increased mortality that you refer to was derived from studies on long-term exposure to PM in air pollution. The collective data for U.S. cities suggest that for day-to-day fluctuations in the mass concentration of 10 μg m^{-3} airborne PM, an increase of about 0.6 to 1% excess mortality occurs (Costa 2001). The U.S. EPA derived similar values after summarizing many studies on increased mortality and morbidity associated with PM exposure for their *Third External Review Draft of Air Quality Criteria for Particulate Matter - April 2002*. The EPA reported that estimates for the increase in mortality per 10 μg m^{-3} 24-h increment in PM10 or PM2.5 ranged from about 0.4 to 13%. Estimates for increases in asthma symptoms and in hospital admissions and respiratory-related doctors visits associated with increased PM10 concentrations have also been derived. Calculations for the 1969-1971 life tables suggested that a chronic exposure increase of 10 μg m^{-3} of PM was associated with a reduction of 1.3 years for the entire population life expectancy at age 25 (EPA 2002).

We recognize that values of this type exist and could be used to derive some estimate of risk. Indeed, we now provide a provisional estimate of risk in Appendix F, as you suggested. Assessing exposure to PM, determining the toxicity of different particles under different conditions for many different endpoints is very complex and a good understanding of both exposure and toxicity assessment is needed to estimate risk. Although many studies on the effects of PM exposure on humans and animals have been done in recent years, the basic understanding of the dose-response is not sufficient for quantatative risk assessment. We believe that the uncertainty associated with quantifying the detrimental effects of PM10 is too great, for a number of reasons, to allow meaningful estimates of risk to be calculated. Nevertheless, one may apply the risk estimates described earlier to the estimated PM10 concentrations if a quantitative risk estimate is desired. For example, using the increase in daily mortality from exposure to PM10 of 0.07% ± 0.012% per μg m^{-3} of PM10 (presumably average over 24-hours) estimated by the WHO and the maximum estimated 24-hour average PM10 concentration from the Cerro Grande Fire (see Table 4-14 in the Task 1.7 report), the estimated increase in daily mortality from the Cerro Grande Fire ranged from 3% (0.07% m^3 μg^{-1} × 39 μg m^{-3} ≈ 3%) at Española to 20% at TA-54 (0.07% m^3 μg^{-1} × 288 μg m^{-3} ≈ 20%) assuming a persons was exposed to this concentration. We show this calculation to illustrate the possible application of PM10 risk data; however, such calculations should be treated with extreme caution. Some of the sources of uncertainty associated with developing risk factors from morbidity and mortality data are summarized below.

The toxicity of PM is complex and depends on many characteristics of the PM, including: particle size, physical and chemical properties, solubility in the lung, reactivity and particle composition. The latter is particularly important because PM can consist of many different types of inorganic and organic compounds, including toxic and carcinogenic metals, irritants, and biogenic compounds like endotoxin. Particles with different compositions will exhibit different dose-response relationships. Different kinds of particles cause different kinds of health effects with different times between exposure and disease or exposure and deaths.

Pollution containing PM may also contain carbon monoxide and oxides of sulfur and nitrogen and other pollutants that also adversely affect the cardiovascular and respiratory systems.

Estimating community wide exposure concentrations for use in epidemiological studies from monitoring data is uncertain because annual PM concentrations in urban areas can differ over time more than 100 $\mu g\ m^{-3}$. PM10 concentration measurements taken at the Capshaw School in Santa Fe in 1999 and 2000 ranged from 5 to 50 $\mu g\ m^{-3}$.

Different sources produce different types of PM. Much of the studies have been on urban air pollution, road dust suspension from vehicular traffic, diesel exhaust or other engine emissions. The PM from the Cerro Grande Fire was from burning vegetation and suspended soil.

Costa, D. 2001 "Air Pollution". In: *Casarett and Doull's Toxicology.* 6th edition. Edited by C.D. Klaassen, McGraw-Hill, New York. 979-1012.

EPA (U.S. Environmental Protection Agency. 2002. *Third External Review Draft of Air Quality Criteria for Particulate Matter* EPA/600/P-99/002aC. April.

Ward Whickers Response to Bernd Franke's Comments on Task 1.7

BERND FRANKE'S REVIEW COMMENTS ON RAC TASK 1.7: A RESPONSE

Ward Whicker

May 15, 2002

INTRODUCTION

Bernd Franke reviewed Task 1.7 on "Estimated Risks from Release to Air" and emailed his comments on May 10, 2002. Supplemental comments were received May 15. The overall evaluation by Mr. Franke was that the report was "incomplete and inconclusive". I consider these comments to possibly be unfairly damaging to the credibility of the work. Because this general characterization was very different from those offered by Dr. Pinder and myself, and because of my role as coordinator of the review process, it seemed appropriate to offer an independent response to be shared with all concerned. My taking this course of action was encouraged by the overseers of the project at NMED.

It should be noted that Mr. Franke received the Task 1.7 report late, with only a very limited amount of time to read it and provide comments, and he was not provided the report's Appendices until two days after he sent comments. I accept full responsibility for this unfortunate situation. At the time of my initial receipt of the Task 1.7 report, I judged that only Dr. Pinder and myself would be able to review the report rapidly enough to allow RAC a fair response time. However, after subsequent communication with Mr. Franke, I send the Task 1.7 report to him, and he returned comments within a few days. His not receiving the Appendices with the main report was an inadvertent oversight on my part.

The following are my thoughts on Mr. Franke's review comments:

COMPARISON OF ESTIMATES WITH OBSERVED CONCENTRATIONS

1. Mr. Franke correctly noted that RAC concluded that the contribution of radionuclides and heavy metals from burning vegetation was much larger than that from potential release sites (PRS) at LANL. I feel RAC did show that burning vegetation generally yielded one to several order of magnitude greater risks to regional communities than did burning of PRSs. However, I do not believe that RAC intended the comment to apply necessarily to all areas, particularly on or near LANL, nor to all possible circumstances. For example, the observed maximum ^{241}Am air concentration (260 aCi/m^3) exceeded the maximum mean value at TA-54 (400 aCi-d/m^3 ÷13 d or 31 aCi/m^3) estimated from burning vegetation, but this was at Area G, a waste site at LANL. This is not surprising to me.
2. Mr. Franke's comparison of maximum time-integrated air concentrations at TA-54 estimated by models from burning vegetation, to maximum observed air concentrations at other specific locations (e.g. ^{241}Am at Area G, $^{239,240}Pu$ at Tsankawi National Monument and ^{238}U at TA-5 is not, in my view, an appropriate thing to do for model evaluation purposes, for several reasons. Some are listed below:
    a. Actual source terms applying to the specific observations at close-in locations are not known, nor could they expected to be, especially for any specific 24-hour collection period.
    b. The observations were apparently 24 hour measurements, while the estimates were based on a mean over 13 days. Individual day estimates could easily have varied greatly from the 13-day mean value estimates.
    c. The accuracy and precision of the point measurements referred to are not evident, and probably not known. Although I usually tend to trust measured data more than model estimates, not all data can be assumed correct, especially a single datum. A single value, sampled from a large population of potential samples, and a single analysis of that sample, gives no clue as to the overall confidence one can place in that value. It may or may not have any real meaning.
    d. The vegetation data were based on measurements from the general area on and around LANL, but they cannot be expected to represent the maximum possible values in the LANL environment. For example, there could be some much more highly contaminated spots near old outfalls in the Canyons or other local sites where vegetation might have contained much more radioactivity than that revealed in the data base used by RAC.
    e. It is conceivable that actual soil resuspension with relatively high levels of contamination were entrained in the plumes which affected the observed data, but we can probably never know this for certain.
    f. Scientific comparisons of model predictions to observed data normally use means or other measures of central tendency, because means are far more stable and robust as statistical estimators than are values near the tails of distributions. Comparing a single observed maximum value to an average maximum estimate should not necessarily be expected to yield good agreement, even if both observed and estimated values were determined for the same location. In this case, we are not even talking about the same location.

3. I was surprised by Mr. Franke's comment that "Without a reasonable explanation of those and other differences, I consider the report **incomplete** and **inconclusive**." My view is that virtually no environmental model is capable of

simulating nature, or accurately predicting all single measurements for all times and places, even if the source terms and other variables were known precisely, which they are not here. The source term alone is so complex, for example, that no reasonable amount of monitoring could ever provide "complete" knowledge of it. Nor can all the spatial and temporal variations in wind patterns and a myriad of other relevant factors be completely known.

Furthermore, I think no report on a topic of this nature could ever be accurately described as being "complete". Most any environmental science report can be characterized as "incomplete", however, this characterization, as used here, could be interpreted inappropriately, and therefore it is a potentially damaging comment. I would certainly not claim that the report is so "complete" that more effort in the future would be useless. Certainly, with more time, resources and information, a more complete picture of the health risks of *fire only*, and *fire + LANL contaminants* can indeed be obtained.

The use of the PM-10 data to test the model was, in my view, the most reasonable approach available to RAC in this study. This testing resulted in an overall geometric mean Predicted to Observed (P/O) ratio of 0.87 with a geometric standard deviation of 1.7. I believe that this is excellent agreement for this type of modeling. I fail to see, and both Dr. Pinder and Mr. Franke failed to describe, an alternative way of proceeding that might have been superior to this.

The comment that the report is "inconclusive" is particularly hard for me to understand. Fundamentally, the goal of the report was clearly stated to consist of estimating health risks to people in the region of the Cerro Grande Fire as a result of it burning over LANL property and releasing radionuclides and chemicals generated by LANL operations to the atmosphere. Such risks, calculated using available data, along with reasonable and understandable methods, were conservatively estimated to be extremely low. I was certainly convinced that even if the available data and modeling approaches had led to one or a few order-of-magnitude under-predictions, the basic conclusion that the risks were at least very low, would still remain. This does not necessarily mean that risks not studied by RAC were inconsequential.

PM-10 RISK

1. PM-10 risk was not in the scope of work. Nevertheless, it was considered perhaps more carefully than Mr. Franke realized, since he did not have Appendix F at the time he was preparing his first set of comments on Task 1.7.
2. Except for a few brief excursions at isolated locations, measured and predicted PM-10 concentrations were well-below EPA's 24-hour standard of 150 $\mu g/m^3$, and even well-below the annual standard of 50 $\mu g/m^3$. I realize that these standards have been challenged, and I would not say there was no PM-10 risk.
3. There are no specific toxicity values or slope factors in EPA's data bases (such as IRIS) for PM-10 because of the non-specific meaning of the term. Depending on the constituents in the particulates less than 10 μm, they could have vastly different

toxicities. The constituents in PM-10 aerosols during the Cerro Grande fire could have varied greatly in time and space, and it is my impression that relatively little is known about them, and their individual and especially their combined toxicities.

4. The large majority of the PM-10 materials likely came from burning vegetation and organic litter, and not from LANL-derived contaminants. Thus, the PM-10 related risks would be present from any forest fire at any location in the world.
5. I would agree with both RAC and Mr. Franke that the PM-10, and even more so, the PM-2.5 risks, may not have been trivial. This subject is certainly open to further investigation, which of course would require more time and resources.

WHERE DO WE GO FROM HERE?

1. I trust that the different overall characterizations of the Task 1.7 report resulted from honest and completely independent evaluations. I strongly believe that the review process, and RAC's response to it, should be completely and openly revealed to all involved in the development of the study and to all stakeholders.
2. I encourage RAC to find ways to communicate reactions and ideas from all the reviewers, even when the reviewers disagree among themselves. Along this line, I would recommend that RAC point out the possibility, evident in the monitoring data, that concentrations of contaminants at some locations, particularly those on or very near LANL, may occasionally exceed model predictions by a considerable amount. One can speculate about possible reasons for this, but usually it comes down to conjecture, simply because of the complexity of the problem and the lack of knowledge. This then can feed into lessons learned from the study, such as designing future site characterization and monitoring programs to better detect and quantify effects of extreme events, such as fire, floods, tornadoes, terrorism, etc.
3. I agree with Mr. Franke that more attention can be given to particulate matter (PM) risks. However, since this was not in the scope of work, because PM toxicity is under so much debate, and because of the lack of data on the Cerro Grande PM composition, I do not think it is fair to label this study as "incomplete" or "inconclusive". I do feel that RAC adequately described how they used PM-10, what they left out, and why.

Response to Bernd Franke's Supplemental Comments on May 18, 2002

Review of RAC Draft Reports "Analysis of Exposure and Risks to the Public from Radionuclides and Chemicals Released by the Cerro Grande Fire at Los Alamos"

Dear Ward:

I would like to comment to your May 15 response to my review. I am sorry about the confusion that my comments appear to have caused. It was not intention to cause undue delays and request impossible things from RAC. Let me clarify my position as to the two issues which are in debate.

Comparison of estimates with observed concentrations

I do not argue against the model used by RAC to determine the contribution of radionuclides from PRS sites. It shows a good agreement between estimated and observed PM10 concentrations. I do not advocate using any other model. I agree that RAC did indeed show that burning vegetation generally yielded one to several order of magnitude greater risks to regional communities than did burning of PRSs. I agree with you that a comparison between observed and modeled concentrations is complicated in this case for the reasons that you stated. However, I do not fully agree with your criticism of my comparison of modeled concentrations for TA-54 with measurements at other locations (^{241}Am at Area G, ^{239,240}Pu at Tsankawi National Monument and ^{238}U at TA-5). First of all, it was used to illustrate that it is possible to undertake such an exercise. It may not be easy but it could be done.

RAC Response: We reiterate our major concern to using any of the radionuclide and chemical air concentration data to back-out a source term or make other extrapolations. Short-term fluctuations in background concentrations of radionuclides and chemicals are not well characterized. Without a clear understanding of short-term fluctuations in background concentrations, it is difficult to use these data in a model in any meaningful way. The data are useful for calculating exposure and risk; an example is provided for ^{239}Pu in the Task 1.7 report.

RAC used exposure scenarios describing using representative, but hypothetical individuals impacted by the fire and determining the risks to them. If the estimates for TA-54 are not representative for the maximum location (firefighter sce-nario) justification of TA-54 for the onsite location should be provided.

RAC Response: We have provided estimates of risk across the model domain and report the maximum, regardless of location in the Task 1.7 report.

I agree with the description of the other limitations in your response but not with all of your conclusions:

(a) I agree that source terms at close-in locations are not known.

(b) I agree that most measurements were 24-hour averages and that elevated concentrations from PRSs and/or contaminated vegetation are unlikely to have persisted over the entire 13-day period. Expressing both as time-integrated air concentration (aCi-d m $^{-3}$) is appropriate and that is what I did.

(c) I guess RAC was unable to validate the quality of most measurements. Nevertheless the data exists and therefore a discussion section dealing with those difficulties in the model validation exercise should be added to the RAC report.

(d) Though I agree that the vegetation data cannot be expected to represent maximum possible results for LANL site, I previously noted that RAC used a conservative approach at least for ^{239,240}Pu by assuming Mortandad Canyon data (an area of 3.7 km ^{2}) for the entire170 km ^{2} of burn area.

RAC Response: This is not entirely true. Mortandad Canyon was but one location used to derive ^{239}Pu concentrations on vegetation.

(e) Soil resuspension may have been higher than assumed by RAC.

RAC Response: We do not doubt that soil resuspension could have been higher. However, it would have had to have been 5 to 7 orders of magnitude higher than our estimates so that the contribution from PRSs to the observed air concentrations was significant. We do not think this is reasonable based on current understanding of the resuspension process.

I did not mean to suggest that the RAC exercise is invalid. Rather, I'd like to see a section dealing with the above issues. I believe such a section is necessary because of possible misunderstandings regarding the word "maximum" in the conclusion of the Task 1.7 report.

To put things into perspective, I also suggest to present the calculated risk based on maximum observed concentrations that will show that calculated risks are still low if one were to rely on observed air concentrations at face value.

RAC Response: As stated earlier, we have provided estimates of risk across the model domain and report the maximum, regardless of location in the Task 1.7 report. Additionally, we have calculated the risk from ^{239}Pu using the maximum measured air concentration and an overly conservative exposure scenario. The results of this calculation showed that the risks were still minimal.

PM-10 risk

I am well aware that PM-10 is not LANL specific and that it originates from burning vegetation and other organic matter. I was unaware that the coverage of the PM-10 risk is beyond the scope of RACs contract which I have not seen. Despite the contract limitations, it is certainly a good idea to quantify the risk (e.g. by using WHO risk factors) and it is my understanding was that RAC attempted this whether or not the contract required it. Sure, such risk factors have limitations when applied to the Cerro Grande Fire -- but so do all other risk factors that RAC used. My back-of-the-envelope calculations suggest that the calculated risk from PM-10 is orders of magnitude larger than the calculated risk from chemicals and radionuclides.

Since coverage of the PM-10 risk appears to be a "voluntary" part of RAC report, I would not press any further details in this matter. In that case, I suggest to eliminate the sentence "we did not quantify the risk of adverse health effects from PM exposure because research in this field has not adequately determined what concentrations cause health effects over what time frame" because this general statement is not correct (or should be applied to all other risk factors as well). My earlier statement that I considered the report "incomplete and inconclusive" could have been more carefully explained. I hope that this letter clarifies my intentions and suggestions for resolution.

RAC Response: We have made the changes to the text as you suggest and have provided an illustrative example of application of the WHO PM10 risk factors in Appendix F. While dose response data do exist, conversion of the dose response to a risk factor that may be applied universally to an environmental concentration (averaged over a specific time interval) is still forthcoming. We are reluctant at this time to report quantifiable health effects on a comprehensive basis for this reason. However, we agree with your statement that exposure to PM10 resulted in health effects that were probably orders of magnitude larger than the risks posed by releases from PRS.

Other than the two issues above, I consider the Task 1.7 Report a careful evaluation of the fire impact. It is well written and documented. The selection of the model assumptions and parameters is suitable and transparent. I agree with the overall conclusion that the risk from radionuclides and other carcinogens from the fire was very small. I see no evidence to the contrary.

RAC Response: We appreciate your comments.

Please feel free to contact me if you have any questions regarding my contribution.

Best regards,
Bernd Franke

LANL COMMENTS ON RAC TASKS 1.6

Task 1.6. Estimation of Risk from Releases to Air within the Model Domain

Tom Buhl, ESH-DO

1. Many valuable conclusions are reached throughout the report. It would be useful to collect these in an Executive Summary, either in the report itself or in a summary report at the end of the project. These conclusions would be placed in the very front of the report and should include –

   - A conclusion on the magnitude of the health risks found in the study – are there health concerns with the radionuclides, chemical carcinogens, or chemical non-carcinogens

   How the Cerro Grande fire affected levels of airborne contaminants, and whether the Laboratory contributed significantly to these levels

   RAC Response: We added an Executive Summary to the final report for the Air Pathway (Task 1.7).

Lars F. Soholt, ESH-20

2. P. .55, second bullet from the bottom. it looks like the RfDs that were used were for chronic exposure (see p. 60, first paragraph) not subchronic as stated here; also, it looks like the chemical risk calculations are base on a lifetime (70y) exposure rather than subchronic.

   RAC Response: We changed the wording with regard to the RfDs in the Task 1.7 report to reflect the fact that subchronic values were used **when available**. This is further explained in Appendix E. We also have labeled the RfD values that were used as "chronic" or "subchronic" in Table E-2 of the summary report.

   The 70 years is the averaging time for the risk calculations, not the exposure duration. This is a standard averaging time for chemical risk calculations with slope factors.

3. In Tables 22 & 25 the hazard values are identified as HQs; I am assuming that these values are the sums of the HQs for individual non-carcinogenic toxicants, so the correct term here would be the Hazard Index (HI).in equations 25 and 26 the units for BR change to m^3 per day rather than m^3 per second; also, the TIC changes to mg-d per m^3. or, SF, RfD, and AT need to be expressed in seconds.

   RAC Response: We have continued to use the term Hazard Quotient to describe even these sums of HQs for consistency and to eliminate reader confusion.

4. In Table B-2 the units for RfD are inverted; should be just mg/kg-day.

   RAC Response: The reviewer is correct in noting the inversion of the units for RfD. This has been corrected in Appendix E to Task 1.7.

Jean Dewart, ESH-20

Appendix A

1. It would be helpful to have a discussion of the spatial representativeness for the vegetation and soils contamination data used in these calculations. The reason this would be of interest, is these values are assumed to represent all 47,000 acres that burned - this

could lead to very conservative calculated air concentrations. On page A-16, the document indicates that the air concentrations from burned vegetation are significantly higher than the air concentrations from the burned PRSs. This might not be true - rather an artifact of assuming the contaminant data represent all 47,000 acres that burned. If one assumed that the PRS contaminant data represent of all 47,000 acres burned, then the two sources of air concentrations might be similar.

RAC Response: We did not have sufficient data to comment on the spatial representativeness of either the vegetation or PRS soil concentration data. However, to avoid overestimating the contribution from vegetation and thereby understating the relative contribution from PRSs, we made comparisons using the minimum of the range of vegetation concentrations we were able to locate. Further, we included a comparison that shows the contribution of the litter and bark portion of vegetation only, which is likely more representative of what actually burned. These assumptions are not unrealistic and would not be expected to overstate the relative importance of burning vegetation across the 47,000 acres that burned because our calculations are based on actual fuel loading data for the area.

In contrast to the assumptions made for estimating the contribution from burning vegetation, we made conservative (i.e., likely biased high) assumptions to develop both inventory and release estimates for the PRSs. It would not be realistic to assume the PRS data as representative of the entire 47,000 acres that burned.

Assuming the minimum of the range of vegetation concentrations for comparison to likely high estimates for PRS releases suggests that the % contribution from burning vegetation we calculated and reported may in fact be underestimated. Even so, the contribution from vegetation appears to be the dominant contributor to chemical and radionuclide air concentrations during the fire, and the risks associated with maximum vegetations concentrations are less than risks defined as acceptable by the US EPA.

2. On page C-1, "hospital emissions" should be "hospital admissions".

RAC Response: We corrected the spelling.

3. The public has been concerned that a plume of LANL pollutants was released during the fire, lofted to high altitudes, and settled down in Truchas, Taos, eastern NM, and Oklahoma - missing any of the ambient monitoring. This idea is reinforced by the satellite photos showing the fire plume travelling hundreds of miles east and north. The modelled air concentrations indicate that pollutant concentrations fall off with distance. Can the report address why the plume was observed to travel so far, but the concentrations do not.

RAC Response: A section has been added to the Task 1.7 report (Section 4.4.1) to address the transport of chemicals and radionuclides entrained in the smoke plume outside of the model domain. The concentrations in the plume decrease with distance as a result of dilution in the atmosphere and depletion of materials entrained in the plume. This does not mean that the plume would not be visible, as it would still contain very small particulate matter capable of traveling in the air hundreds of miles or more. Predictions can be made for concentrations

at more distant locations outside the model domain, but time and computing capability constraints dictated that we focus our evaluation on locations within the model domain. However, based on the results of the analysis presented in Section 4.4.1, it is unlikely that concentrations exceeding those predicted in Espanola would occur at the more distant locales noted by the reviewer.

COMMENTS ON TASK 1.7: ESTIMATED RISKS FROM RELEASES TO AIR

FROM: John Pinder

GENERAL COMMENTS

Much of this document has been previously reviewed by me as parts of earlier drafts, and the authors have been very positive in their responses and very thorough in their attention to issues that may have affected the estimates of risk. Therefore, my comments here are largely restricted to: 1) the new text sections (e.g., the Executive Summary and Chapter 6 – Summary and Conclusions); 2) the areas addressed in my recent review of Task 1.6; and 3) sections that have received more extensive revision (e.g., Appendix D).

The current document is consistent with the well-prepared, well-organized and well-written drafts that have preceded it. This report addresses a massive effort combining data from a variety of sources and encompassing expertise from a number of disciplines and summarizes it in a straight-forward, concise and readable format.

Only three omissions on a general level warrant comment. First, the towns of Espanola, Santa Clara, San Ildefonso, Sante Fe, White Rock and Los Alamos are frequently mentioned, but there appears to be no one map that shows the location of all of these towns. Could one of the early maps in the document be modified to do this.

RAC Response: We added additional locations to the maps; in particular, the locations where PM10 was measured and the locations where concentrations and risks were calculated and reported in tables.

Second, there are risk estimates reported for these towns, but no isopleths of risks (except for HQ) that could be used by more rural residents to evaluate their risks. These perhaps should be presented for the resident adult and resident child exposures.

RAC Response: We added isopleth maps of carcinogenic risks from radionuclides and chemicals to Chapter 5.

Third, the maps are generally very effective in showing data and patterns displayed above a topographic surface for the domain. However, this topographic backdrop doesn't seem to be referenced or explained in any of the figures. The backdrop should remain, but it should be discussed to prevent possible reader confusion.

RAC Response: The topographic surface is discussed in Chapter 4 in Section 4.2.1.2. We have added a description of the surface relief map that was generated using the processed terrain data to the figure caption at its first occurrence.

SPECIFIC COMMENTS

Comments on the Executive Summary

Page v: The assessment of the accuracy of the predated PM10 concentrations is useful, but some limited expansion would be useful in reinforcing the accuracy. Three items should be mentioned. First, that about 50% of the predicted values were within 30% of the measured value. Second, that as many predicted values greater than observed occurred as predicted values less than observed. Third – and probably most important - there was no gradient in accuracy across the domain. Thus, there were no broad regions where the model consistently undepredicted the risk. The only place where this could be a problem was in Santa Clara, but the data for nearby Espanola do not support a large hole in the model at that location.

RAC Response: We added additional explanation to the executive summary as suggested.

The data in Table ES-1 would benefit from a division of risks into those levels experienced on LANL and those experienced in the off-site surrounding areas. The preceding paragraph discusses the high HQ for a resident adult of 2 being on-site where no resident adult exposure exists. The fact that this HQ of 2 doesn't really exist could be easily missed by a casual read. These comments are also relevant to Table 6-1.

RAC Response: We think that separating the risk into risks incurred on and off LANL boundaries will complicate Table ES-1. The key point we want to convey is that cancer risks were minimal despite the conservatisms in the calculation. We think it is important to state what the maximum HQ values were, and then state the caveats associated with those values. We added isopleth maps of cancer risk from radionuclides and chemicals to Chapter 5 as suggested so that the spatial extent of risk estimates can be clearly identified.

There isn't a clear, readable discussion in the ES on how HQ relates to risk.

RAC Response: We included a definition of HQ and how it relates to risk.

The ES would benefit from the inclusion of the 1 sentence paragraph on page 6-3 that begins "The risks calculated for ..." It is not clear from reading the ES that these risks apply to the burning of similar natural vegetation at any site and are not specific to LANL.

RAC Response: We revised the text to refer to chemicals and radionuclides released from PRS sites during the fire as "LANL-derived radionuclides and chemicals." We also added a statement that the risks incurred from exposure to radionuclides and chemicals on vegetation are not specific to LANL and would be experienced during any forest fire.

Table ES-2: The meanings of the terms Low, Moderate and High as modifiers of conservatism and uncertainty are not understandable without reference to the footnotes. Their interpretation would be easier if it was contained in a longer table heading. It might also be better to replace these words with their corresponding levels of ≥ 2, ≥ 5 and ≥ 10 for conservatism and 2X, 5X and 10X for uncertainty. Maybe the more quantitative expressions could be used in Table 6-2, and the word descriptions retained here. But also see comments below on Table 6-2.

RAC Response: We incorporated your suggestions into Table ES-2 and Table 6-2, using the quantitative descriptions of uncertainty and conservatism.

The ES would benefit from a simple map of the domain area.

RAC Response: We decided against reproducing Figure 1-1 in the ES.

Comments on Chapter 6 - SUMMARY AND CONCLUSIONS

The comments on Table ES-1 given above also apply to Table 6-1.

Page 6-3 – Paragraph 2. The comments concerning the risks from all forest fires might be augmented to discuss the fact that risks were also incurred for fires burning off the LANL at the same time as well as the fires that burned before and after LANL was involved. The risks of the forest burning places the risk of the PRS burning in context, but the context of LANL's forests burning should also be mentioned in respect to the larger context of all the forests that burned. LANL's forests are only a small proportion of the total burn, and those people exposed by LANL's forests are only some of the New Mexico population that was impacted by the fires.

RAC Response: We have added additional explanation as suggested to this paragraph.

Page 6-3 – Paragraph 6. For a final report, the discussion of the use of a population average risk factor for estimating the risks to children should also include some of the potential underestimation of risks that may result. For example, it is not unreasonable to assume and mention that the risks for children, which are mostly trivially small, may be as much as 3 x that presented in Table 6-3.

RAC Response: We think that separating the risk into risks incurred on and off LANL boundaries will complicate Table ES-1. We think the major point we want to make is that carcinogenic risks were minimal despite the conservatism in the calculation. We think it is important to state what the maximum HQ values were, and then state the caveats associated with those values. We have added isopleth maps of radiological and non radiological carcinogenic risk to Chapter 6 as you have suggested so that the spatial extent of risk estimates can be clearly identified.

Table 6-2. Because of a factor of 5 X conservatism for Overall carcinogenic risk and a factor of 10 X for uncertainty, could this mean that risks are potentially underestimated by a factor of 2? The table could be interpreted this way. Is this what you meant to imply? If this interpretation is reasonable, I reiterate my comments about separating on-site and off-site risks.

RAC Response: Your interpretation is correct. The cancer risks have the potential to be underestimated by a factor of 2, but given the estimated risk values, this is not of great concern. Subchronic health effects of noncarcinogens appear to be less certain, and we found large discrepancies between reference doses published by EPA and occupational standards. Because HQ values are substantially higher and in some cases, exceed 1.0, we rated the conservatism as high.

Comments on Other Sections of the Main Text

Chapter 3

Section 3.2.5 presents a reasoned and informative discussion of the uncertainties in source term identification that is useful to the reader in understanding potential sources of error, but note the numbering of the sections is misleading.

RAC Response: The section numbers have been corrected.

Section 3.3.2 presents a reasonable discussion of the limitations of addressing the toxicity of potential degradation products in the atmospheric plumes, but this is one of the few instances within this risk assessment process that a less than aggressively conservative approach has been used. The effects on the resulting risk analyses cannot be evaluated without further work, but the effect is likely to be small given the overly-conservative assumption that there is no loss of toxic compounds by degradation as the plume disperses.

RAC Response: In Chapter 4, we expanded the discussion of the limitations to the methodology and its implications regarding bias (under or over prediction) in the exposure and risk estimates.

Chapter 4

The isopleth maps in Figure 4-17 and following figures are much more informative and interpretable than the color graded exposure maps in earlier drafts.

RAC Response: Thank you. We worked to improve their presentation based on your earlier comments.

Comments on Appendix D

Appendix D figures importantly in the final report by providing the data for assessments of risks from metal, naturally-occurring isotopes and isotopes resulting from nuclear tests and weapons production activities that were present in, and released from, the forest vegetation as it burned.

Basing the predicted air concentrations on releases from bark and litter but not bole wood seems to be a reasonable assumption given the presence of standing trees in the burned areas. It may still be conservative in that only the surface of the bark was consumed in many areas.

Table D-3. The statement in the paragraph preceding this table to the effect that there were no on-site to background trends in radioisotope concentrations seems at odds with the Cs-137 data in Tables D-3 and D-4 and the Sr-90 data in Table D-4. These trends – even if true – make little contribution to the overall risk because of their small risks compared to those for the naturally-occurring isotopesPo-210 and Ra-226 in Table D-16. However, concerns about the contributions from past releases from LANL operations that are not confined to PRS locations are possible and should be carefully considered. If the apparent trends of increasing concentrations from off-site to on-site are not statistically significant or are artifacts of the vegetation sampled, then there should be some discussion of this. Also, the appendix seems vague concerning which of these concentrations was used to generate the source term in the model. If it was the on-site LANL data, which is appropriate for estimating the effects of the LANL forest burning, it might be well to say so.

RAC Response: We agree that there is an indication of higher ^{137}Cs concentrations for onsite samples, based on data in Table D-3, and we have revised the text preceding the table accordingly. However, the data presented in Table D-1 also suggest the possibility of higher ^{137}Cs concentrations in samples collected at locations that are not expected to be impacted by LANL operations as compared to the onsite sample concentrations noted in Table D-3. We also agree that the concentrations in samples from Mortandad Canyon (Table D-4) suggest higher concentrations of both ^{137}Cs and ^{90}Sr, as well as the other radionuclides listed, particularly for bark, and we make note of this in the text immediately preceding Table D-4. We did not have the time or resources to consider whether there were statistically significant trends, either spatially or by vegetation type, and it is not apparent that the available data would enable such analyses. Additional data could help determine the fraction of burned vegetation potentially impacted by LANL

operations and help better understand the variability in concentrations, both as a function of location and vegetation type.

Because of the generally wide range of average concentrations from different sources, as reported in Table D-6, we elected to estimate a range of vegetation and predicted air concentrations using the maximum and minimum average values. The subsequent risk calculations were based on the maximum average values (as noted in the title of Table D-16), many of which (but not all) are associated with the onsite samples collected from Mortandad Canyon (see Table D-4). As discussed in the report and noted by the reviewer, the risks based on these maximum concentrations are still small.

COMMENTS ON RAC'S DRAFT TASK REPORT 1.7: ESTIMATED RISKS FROM RELEASES TO AIR

By
F. Ward Whicker

May 3, 2002

General Comments:

1. This report represents the comprehensive effort by RAC to evaluate potential risk to people from radionuclides and chemicals that may have been released to the atmosphere when fire burned over sites at LANL having known contamination in soil. The health risks considered were lifetime cancer incidence and other risks that might have been incurred from the inhalation of airborne contaminants in the smoke plume of the Cerro Grande fire, during the time the fire was occurring.

2. It was found that although a considerable amount of air monitoring data were gathered within the general region prior to and during the fire, the data were insufficient to allow direct calculations of risk from inhalation. This led to the necessity to develop a modeling approach involving source term development and atmospheric dispersion calculations to estimate air concentrations over time, within the region of interest. Selected air monitoring data for PM10 were used to test the predictive accuracy of the model.

3. I think this report represents an outstanding effort. It is very clearly written, and with only one or two minor exceptions, explains well the approach and rationale for methods and assumptions. I think the assessment used credible and widely accepted models, and displayed clever and innovative ways of dealing with background contributions to the aerosol levels, with source term uncertainties, and other issues. The literature review was very impressive, and the use of it effectively brought current scientific understanding into the analysis. Limitations and uncertainties in the analysis are clearly revealed and expressed.

Specific Comments:

1. Page iv, first paragraph: In the explanation of the model domain, the southern boundary is not provided. Why not add it?

RAC Response: The southern boundary has been added to the explanation of the model domain.

2. Page vi, 3rd paragraph: This discusses a "discrepancy" to account for a high hazard quotient. I could not understand exactly what the discrepancy was. This needs to be clarified.

RAC Response: The text has been revised to indicate that the high hazard quotient is attributed to the use of the chronic reference dose that is based on air concentrations several orders of magnitude lower than recognized occupational standards.

3. Page vi, Table ES-1: The title indicates "Maximum estimated risks—". What is meant by maximum? Highest location, or what? The 2.0 value in the last column in the resident adult row should be aligned properly.

RAC Response: The maximum refers to the maximum calculated value in the model domain. We have corrected the alignment of the 2.0 value.

4. Page vii, Table ES-2: The first comment indicates maximum detected concentrations were used to estimate contaminant inventories. On page 3-2, step 2 indicated average concentrations were used. Are these statements in conflict? Also, this table would be easier to use if spaces were inserted between the rows. Finally, in footnote b, if you say a "factor of 10" for uncertainty, I think that means something like the best estimate, ÷/x 10, not ± 10.

RAC Response: The comment has been revised to state that average concentrations were used but that the removal of non-detect values likely biased the results high. The layout of the table has been revised as suggested. Footnote b has been corrected.

5. Page vii, Conclusions, line 3: This statement is a little confusing. Are you referring to vegetation growing on, or outside of PRS? Are we talking about uncontaminated vegetation, or vegetation that may or may not have been contaminated from historic LANL releases.

RAC Response: The reference is to all vegetation that burned during the fire. We utilized available data regarding measured concentrations in vegetation from both onsite (potentially contaminated) and offsite (background) locations. There were insufficient data to derive an estimate of the fraction of vegetation that may have been impacted by LANL operations. For this reason, we calculated a range of possible releases and consequent air concentrations, using the maximum and minimum values obtained from several different sources.

6. Page 2-23, Fig. 2-11: I think this graph is for PM10 concentrations, but the caption is not clear on this.

RAC Response: The graph is for PM10 concentrations. The caption has been revised to reflect this.

7. Page 2-24, Section 2.5, first paragraph, line 8: The type of risk should be stated here, even though it is spelled out clearly later in the report.

RAC Response: The text has been revised to specify cancer as the type of risk.

8. Page 3-2, 6th line from bottom: I suggest the word "global" before "weapons testing fallout", as there was local fallout from emissions & experiments at LANL.

RAC Response: The word "global" has been inserted here, and at the other places in the report where this phrase appears.

9. Page 3-8, paragraph following equation 3.2, 2nd line: The assumption that the entire inventory was released seems incredibly conservative. I would like to know the range of D_{mean} values used in this calculation for perspective, and see a sentence or two here discussing just how conservative this might be for different classes of contaminants, say volatiles vs. refractories, etc. Maybe this is OK, now that I realize this is for the screening calculation only. I guess on page 3-30, you are using 1cm for D_r, which is still conservative, but more reasonable. Am I interpreting this correctly?

RAC Response: The mean sampling depth ranged from 0 (surface soil sample) to 45 cm. This depth is reported in Section 3.3.1.2. We apologize it was not pointed out in the equation.

10. Page 3-21, Section 3.3.1.2, second paragraph, line 1. I would think a temperature of 1000 deg C would destroy most all the organics. Isn't it quite possible that this would greatly affect the HQs for the non-carcinogens (Table 5-7, for example), which could otherwise seem fairly alarming if taken out of context. In section 3.3.2 this possibility is mentioned, but I wonder if a call to the right expert on the subject could add some quantitative insight into this question.

RAC Response: We do think an expert in this area would shed some light on the question; however, we also think that the destruction/degradation processes due to high heat will also be chemical-specific. We would particularly be interested in the effects on the high explosive compounds, RDX and HMX, since these were the primary sources of estimated risk. The time constraints of the overall project however limited our ability to investigate this any further than what we did.

11. Page 4-1, 3rd paragraph, line 4: CALPUFF is mis-spelled.

RAC Response: The spelling has been corrected.

12. Page 4-8, 2nd paragraph, line 7: Should "Figure 4-4" be Table 4-3?

RAC Response: Yes, we corrected the reference.

13. Pages 4-8 to 4-10: The analysis of PM10 vs wind speed to permit corrections for background is very nice!

RAC Response: Thank-you.

14. Page 4-12, second paragraph, line 2: insert "we" before "were more interested-".

RAC Response: The word has been inserted.

15. Page 4-26, first line after equation 4.9: T_{ign}, not T_{ing}.

RAC Response: We corrected the symbol subscript.

16. Page 4-33, 3rd paragraph: I think more details are needed for the reader to understand and critique the development of dry deposition velocity estimates. Can the resistance model of Pleim et al. (1984) be summarized here? At least the concepts and basic equations, unless way too complex.

RAC Response: The deposition velocity was calculated internally by CALPUFF and not by ourselves. (We apologize that the text was not clear on this.) We added some additional equations to describe the basic procedure. A full description would require several pages and we refer the reader to the CALPUFF manual.

17. Page 4-40, equation 4.16: I think this is a clever way to scale to the many contaminants that one had to deal with, without monitoring data for any sort of verification. I guess this approach assumes that the behaviors of the particulate contaminants are all similar to PM10 particles and that volatile compounds are similar to CO in terms of atmospheric behavior. I'm not sure how valid this assumption is. I wonder if an atmospheric chemist might be asked about this.

RAC Response: The approach taken does have limitations; including not only the assumptions the reviewers states (behavior of the contaminants are assumed to be similar), but additional assumptions about the release history of contaminants. We added a section that discusses these limitations and their implications in terms of model bias.

18. Pages 4-52 & 4-53, Tables 4-16 & 4-17: The second column in each table is labeled "Maximum concentration (aCi-d m^{-3} or mg-d m^{-3}). If the units are correct, then shouldn't the heading be labeled "Maximum time-integrated concentration"?

RAC Response: These are now numbered Table 4-17 and Table 4-18 because a new table was added. The column headings have been corrected.

19. Page 4-54, last sentence of text: I didn't follow the logic leading to this statement about Taos being approximated from Espanola.

RAC Response: We reworded the paragraph to make the extrapolation of concentrations from Española to Taos more clear.

20. Page 5-1, 3rd line: I think it might be a little more clear to replace "dose response" with "risk per unit exposure".

RAC Response: We changed the wording as suggested.

21. Page 6-5, Table 6-2: The table would be easier to read if spaces were added between the rows. Also, in footnote b, if you say a "factor of 10" for uncertainty, I think that means something like the best estimate, ÷/x 10, not ± 10.

RAC Response: You are correct in your interpretation of the "Factor of ± 10 uncertainty." The footnote has been revised accordingly.

Compilation of Comments and Responses on *RAC* Deliverables Submitted to NMED on April 2, 2001

The comments relate to the following three deliverables.

Task 1.1: Analysis of Air and Biota Monitoring Data (*RAC* Report No.3-NMED-2001-Draft)

Task 1.4: Model Domain, Topography, Land Use, and Meteorology for the CALPUFF Model (*RAC* Report No.1-NMED-2001-Draft)

Task 2.3: Domain for Surface Water Model (*RAC* Report No.2-NMED-2001-Draft)

Comments on RAC Deliverable 1

Tim Michael 4/11/01

Looks good. Just a few comments. I hope they don't seem too nitpicky.

1.1, page 12, last paragraph. ".concentration is less than the 2-sigma analytical uncertainty of the measurement, then it is not considered highly reliable."

The 2-sigma cutoff is a common and no doubt reasonable choice (for assessing analytical reliability), but nevertheless a somewhat arbitrary choice.

Response

Although the choice of 2-sigma may seem arbitrary, any choice of statistical metric for evaluating these data is subject to judgment. The analytical laboratory utilized by LANL reports these 2-sigma values as a part of their data product for LANL. These values are produced using the counting statistics for each sample. This same approach was used by the EPA to report the uncertainty from their analytical laboratory. The 2-sigma value represents the variability in the measured analytical value, and is a reasonable expression of how well we can separate the reported value from indeterminacy.

It is important to note that rarely does a large reported value fall below the 2-sigma "cutoff" line. In general, it is the small measured values that produce large uncertainties, and this trend would be expected. Certainly, this "cutoff" is not fixed, and additional data can be evaluated as necessary throughout the duration of the project. For the purposes of separating values from indeterminacy and identifying data trends, however, the 2-sigma value is considered a good metric for evaluation.

1.1, page 16., next to last paragraph. ".are generally in a 1:1 ratio with each other."

Isn't this specifically a 1:1 activity ratio? Also, in the last sentence of the paragraph, is it really possible to discern depleted uranium from natural uranium using activity ratios? And on the last sentence of the page shouldn't it say isotopic activity concentrations?

Response

Yes, this is a 1:1 activity ratio. We will change the wording to reflect this more accurately. The comparison of the ^{238}U to ^{234}U reported value provides some indication of increased levels of either depleted or enriched uranium. For this initial evaluation of the data, we needed to explore these data trends. The final sentence should say isotopic activity concentrations.

1.1, page 17., third paragraph from bottom. "because one of the two values is not statistically valid."

Statistically validity is based on your choice of a cutoff level (2-sigma).

Response

Yes, but we reinforce our selection of the use of 2-sigma for the purposes of this data evaluation to identify trends.

1.1, page 20., last sentence in top paragraph ".appear to be no significant increases."

I know that it is too early now, but at some point it will be necessary to more rigorously evaluate statistical significance.

Response

We anticipate using the environmental monitoring data to validate our model predictions, which will be based on the available inventory for removal by fire events and the atmospheric transport modeling. Although we can continue the discussion of statistical significance ad infinitum and not reach a consensus opinion, even among experts, the distinctions will become less important at the level of model validation.

When validating a model's predictions, the difference between evaluating a data point that has an analytical value of 5 and 2-sigma of 3 (above the "cutoff") or an analytical value of 5 and 2-sigma of 6 (below the "cutoff") would be insignificant compared to the larger variance commonly seen between predicted and observed values, due primarily to unavoidable model uncertainty. We commit to not making any final determinations about how to use the data until a later date, but we want to stress that the use of these environmental data at this stage in the project is to identify trends, and the statistical evaluation has allowed us to do that. Later in the project, the data will likely be used for different purposes, where statistical reliability will be a less significant issue.

1.4, page 14. ".the predominate wind direction."

Isn't it the predominant wind direction?

Response.

Yes, you are correct. However, the paragraph containing this sentence has been revised in the final report as a result of the inclusion of the meteorological data provided by Mary Uhl.

Tim Michael, NMED DOE Oversight Bureau, Phone (505)827-1536

April 11, 2001
Phil Fresquez
Ann:
I just recieved a copy of Task 1.1 Analysis of Air and Biota Monitoring Data, and a fast review shows that (1) the LANL soil mean data was employed instead of actual (individual) measured

site data (like NMEDs), (2) only NMED U isotopic data is given but LANL U isotopic is not (3) only NMED soil trace element data is given but LANL soil trace element data is not.

All of these data mentioned (five letters addressed to the Santa Fe Farmers Market Task Force describing soil radionuclides, trace elements, and organic constituents in soils collected from farming areas downwind of the Cerro Grande Fire, nine tables representing soils, produce, and fish data collected after the Cerro Grande Fire as part of the Environmental Surveillance Program at LANL, and a disk was also enclosed that contains the table data) that were not reported in your preliminary Task write-up were given to Helen Grogen, via a letter addressed to John Till on February 28, 2001.

Response: Phil-

Thanks much for your comments on our report. You were correct to notice that we used only LANL mean data in our soils analysis. We do have the data that you transmitted to us as Microsoft Word tables.

The air monitoring data were of critical importance for this deliverable, therefore we focused much of our effort on analyzing these data and translating them into useable formats. Because of the many different sources of data, this required considerable effort. Although we had the detailed soils data available to us, we did not have the time before the production of this deliverable to convert the data into a format that allowed us to plot and compare the information. The soils data provided little information about the spread of contamination during the fire, so they were less critical to analyze immediately.

We have since converted these Word tables into Excel and will produce graphics that show the detailed comparison of all the available soil data.

We plan to provide an updated deliverable to NMED after all comments on the document are received.

Our chemical and trace metals analysis continues, as well.

We apologize for any misunderstanding.
Thanks much for your comments-
Jill Aanenson

April 10, 2001
Barbara,
I have finished reviewing the 1st deliverable from RAC, especially Task 1.4, which is essentially the modeling protocol for the use of the CALPUFF model to analyze air quality impacts from the fire. I believe that the methodology described for the analysis is appropriate. The only comment that I have to add is that the NMED also collects surface meteorological data in Santa Fe at the Public Safety Department off of Cerrillos Road. This is probably a couple of miles directly east of the Santa Fe airport, but the station is fully automated and includes solar radiation and a 2-meter and 10-meter temperature, which may be more helpful than the Santa Fe airport data. I am

attaching the met data from May 2000, because RAC may find it helpful to have more surface data for the meteorological input to CALPUFF.
Can you forward this data to RAC, also?
Let me know if you have any questions.
Mary Uhl

Response:
The meteorological data Ms Uhl refers to has been received and is now incorporated into the CALMET simulation. These meteorological data are an important addition to the simulation because there were several large data gaps in the Santa Fe Airport data that are now covered. RAC wishes to thank Ms Uhl for providing these data.

April 11, 2001
Ralph Ford Schmidt
In establishing the surface water domain it would be helpful for RAC to list the locations of receptors and actual surface water pathways it plans to include in their calculations.

Response:
The actual locations of receptors and the specific pathways have not been defined for the surface water modeling. Typically these are defined through the development of the site conceptual exposure model. However, the surface water domain was defined considering locations where potential receptors could come in contact with surface water or obtain water for use away from the surface water source including, places where surface waters cross the facility boundary, at the confluence between the canyon surface waters and the Rio Grande, and the discharge below the Cochiti Reservoir. The types of exposure pathways that will likely be included in the analyses are wading, swimming, ingestion of surface water, irrigation with surface water, fish consumption and crop consumption. These will be more particularly defined in the site conceptual exposure model.

Comment
LANL has done extensive modeling of projected storm water flows as a result of the Cero Grande fire. RAC should contact Everet Springer for more detailed information at:
E-mail Address
everetts@lanl.gov
Work Phone
+1 505 667 0569
Fax Number
+1 505 665 3866
Street Address
Los Alamos National Laboratory
MS J495 EES-10: ENVIRONMENTAL DYNAMIC & SPATIAL ANALYSI
Los Alamos, New Mexico 87545

RESPONSE: The RAC team has met with Everett Springer and Cathy Wilson from the LANL ESH-18 group to discuss their surface water and erosion modeling. ESH-18 has

agreed to share modeling input and results along with monitoring data with the RAC team. The surface water modeling and erosion estimation to be conducted by RAC will greatly benefit from their input and experience.

April 19, 2001.
Subject: Risk
Hi Barbara:
Good meeting you. I was able to do a little outreach in putting in a few calls to some folks re: additional questions for John Till. There appears to be a GREAT DEAL of interest in his study down here with among those I have spoken with. I hope you will get at least a few questions by end of today. At this time my concern is: what levels of contaminants might we expect to see coming down the Rio Grande River from LANL, water that is used for irrigating crops and for ceremonial purposes at the Pueblos?

Many thanks, I'll be on touch.
Sue Dayton

April 19, 2001.
Dario Rodriguez
Subject: Pollution in the Rio Grande
I would like you to forward my letter to John Till, who I understand is with a risk assessment company and has done some work for the New Mexico Department of the Environment

I live in Albuquerque and I am trying to get information on the real science research that is investigating the type , amount, and human effect of any water contamination of the Rio Grande. Before the fire we know that Acid Canyon was and still is seriously contaminated. With budget cuts there are fears that the needed clean-up will not occur. I will be attending a water forum tomorrow that is seriously considering using water from to San-Juan Chama to provide water to the largest city in New Mexico. Can you comment on the safety and any other things to beconcerned about? Thank you.

Response
Our work focuses on the potential for increased health risk from Los Alamos National Laboratory that was created by the Cerro Grande fire. Although I hope our work will also help to answer questions about the risk from other types of contaminants that might be in the Rio Grande, it would be misleading to say we are addressing this question directly. Certainly I believe our findings will help people to understand what the risks are and how they are being monitored. There are very good techniques for measuring radioactive materials in water, and we will be checking to ensure this is being done adequately--or we will make recommendations for improvements, if necessary. The same is true for chemicals. So in this sense, our work will be quite useful to people who are concerned about the long term aspects of making sure the water remains safe for whatever purpose it is used. This includes organic farming and other uses of the water where the quality is of very high importance. On the other hand, we can only make recommendations to NMED and others and it will be up to those organizations to implement them should they choose to do so.

We are not investigating which areas of Los Alamos National Laboratory should be cleaned up, or the priorities for that cleanup. However, as I mentioned above, I believe the work we do can be useful to decision makers about these issues too.
Thank you for your comments.

John Till

April 19, 2001.
John Till,
It is my understanding that you are accepting questions and comments through today regarding the environmental risks from the Cerro Grande Fire. One large concern is that communities south of Cochiti were not informed about or invited to the community meetings recently held in Santa Fe. The Cerro Grande fire has serious implications about LANL pollutants being washed down with the spring rains. Every river community has concerns and questions the quality of water in our rivers and should be a part of the dialogues.

RESPONSE: I am sorry that communities south of Cochiti were not informed about the meeting. It is always difficult to find out how to get in touch with everyone who has an interest in our work and hopefully we can figure out ways to contact people better. Perhaps you can help us by providing New Mexico Environment Department with the names and e-mail address for some of these individuals. We will be happy to make sure they are kept informed about future meetings. One reason we were interested in having news coverage of the meeting was to get the word out to people, and some people who saw the coverage contacted us and requested to be placed on our contact list. In any case, we certainly hope to inform as many people as possible and will continue to expand our list as we go through the project.

There will be more public meetings and there are also progress meetings with NMED where members of the public are actively participating in the work. We encourage people to do this.

As you may know, Albuquerque is discussing a plan that will divert river water for drinking and used in various ways within our water system. The question is repeatedly asked about nuclear waste in the river. No one seems to know how this is being addressed. Is it preventable? Is it treatable?
How will this affect our health and the river's health? What about our crops? We have organic farms and farmers markets throughout the Rio Grande Valley how will this impact the quality of food? Will family gardens be affected by polluted waters in our communities? How can we protect the river water that we will eventually be drinking? Amigos Bravos requests assurances from the city of Albuquerque, yet they have not yet secured this information.

RESPONSE: Our work focuses on the potential for increased risk that was created by the Cerro Grande fire. Although I hope our work will also help to answer questions about risk from routine operations at Los Alamos, it would be misleading to say we will be addressing this question directly. Certainly I believe our findings will help people to understand what the risks are and how they are being monitored. There are very good techniques for measuring radioactive materials in water, and we will be checking to ensure this is being done adequately--or make recommendations for improving this monitoring if necessary. The same is true for chemicals. So

in this sense, what we are doing will be quite useful to people who are concerned about the long term aspects of making sure the water remains safe for whatever purpose it is used. This includes organic farming and other uses of the water where the quality is of very high importance. On the other hand, we can only make recommendations to NMED and others and it will be up to those organizations to implement them should they choose to do so.

Are you working with other Albuquerque city officials? If so who?

RESPONSE: We are not working with any Albuquerque city officials at this time. Should they wish to contact us, we would be pleased to explain our work to them.

Will you be returning to New Mexico to meet with other river communities before your analysis is complete? This is a serious concern that Pueblos and farmers and environmentalists may not have been given the opportunity to participate and offer their statements in your process. How do you plan to address these improprieties?

Sincerely,
Cynthia Gomez

RESPONSE: Of course we will be returning as the work progresses and have made it clear that we will meet with any individuals or groups who have ideas or concerns. It is important to keep in mind that our work does not cover all aspects of contaminants in the river, and we must keep within our scope of addressing the fire as a primary purpose. However, we want to listen to all people we can to address as many issues as we can. Hopefully my comments to the question address how people can be informed about what we are doing and become involved.

Thank you for you comments.
John Till
--
Cynthia Gomez
Albuquerque Projects Director
Amigos Bravos: Friends of The Wild Rivers
925 Sixth Street NW
Albuquerque, New Mexico 87102
Ph: 505 924-2223 Fax: 505 924-2229
cgomez@amigosbravos.org

I am writing to express my concern about runoff from the Cerre Grande Fire reaching Albuquerque by way of the Rio Grande. The City of Albuquerque is proposing to remove water from the Rio Grande for consumption. If contaminated sediments reach Albuquerque, it could prove disasterous for the residents here. Every drop of water is precious in New Mexico. We cannot afford to lose this supply of water due to contamination from upstream pollution sources. Many believe this any contamination from contaminated runoff would settle out in Cochiti, but contamination from Los Alamos has been found downstream from Cochiti, south of Albuquerque.

Please take my concerns seriously. We believe that Albuquerque should be included in the Cerre Grande Fire Risk Assessment.

Brian Gifford
NMPIRG Water Associate
P.O. Box 40173
Albuquerque, NM 87196
(505) 254-1244

T.E. HAKONSON REVIEW OF TASK 2.1

Analysis of Exposure and Risks to the Public from Radionuclides and Chemicals Released by the Cerro Grande Fire at Los Alamos

TASK 2.1: EVALUATION OF SURFACE WATER MONITORING DATA ACQUIRED IN RELATION TO THE CERRO GRANDE FIRE

Contributing Authors
H. Justin Mohler, Independent Consultant
Kathleen R. Meyer, Keystone Scientific, Inc.
Patricia McGavran, Environmental Risk Assessment, Inc.
Lesley Hay Wilson, Sage Risk Solutions LLC

Principal Investigator
John E. Till, Risk Assessment Corporation

My Approach To This Review

In conducting this review, I focused primarily on the technical aspects of the work described in the report including my opinion on the validity of RAC's approach to analyzing and interpreting the LANL and NMED sampling data and the conclusions they drew from their analysis. I specifically tried to evaluate whether they achieved their stated objectives and goals for this subtask. My comments are presented in 3 sections as follows:

- **General Comments-** Listing of stated RAC goals and objectives for this subtask.
- **Section Comments-** Detail comments by section
- **Summary Comments-** My overall evaluation of the report.

I did not dwell on editorial details but suggest that the report be thoroughly edited as some sections are very difficult to read and interpret. There are numerous misspellings in the report such as Mortandad Canyon (not Mortendad) and Otowi (not Otawi).

> ***RAC Response:*** *We have edited the report and made the spelling changes as indicated.*

General Comments

The technical memorandum prepared by Risk Assessment Corporation (RAC) describes the results from one of several subtasks for evaluating the surface water pathway in contributing exposure and risks to the public from

radionuclides and chemicals released by the Cerro Grande fire at Los Alamos. RAC states that the overall objective of this subtask was to "Evaluate available surface water monitoring data and procedures, examining trends in the data, strengths and weaknesses of the data, and potential usefulness of the data".

Specifically, RAC intended to answer the following questions:

1. What surface water monitoring data are available? What organizations provided the data?
2. What are the limitations of the data?
3. Spatially, are there differences in concentrations of radionuclides or non-radioactive chemicals above and below LANL?
4. Temporally, are differences in concentration seen before and after the fire?
5. How can the surface water monitoring data be used to support an analysis of health risk?
6. Are there ways to improve the surface water monitoring program in place at the time of the fire?

My technical comments relate primarily to how well RAC was able to answer these 6 questions.

Section Comments

1. p. 5, line 9-

The statement, "spring snowmelt runoff that occurs over days to weeks at a low discharge rate and sediment load" implies that sediment loss during snowmelt at LANL is small. In fact, long duration snowmelt events at Los Alamos can completely scour out stream channel sediments down to poorly weather Tuff (very coarse stuff) or bedrock. Sediment movement via snowmelt is primarily as bedload movement along the channel bottom.

> ***RAC Response:*** *We will revise this statement to better reflect the potential impact of spring snowmelt runoff on erosion.*

2. p5, 3rd paragraph

The sentence, "Historically, samples were---" The word samples should be qualified to read runoff samples as tens of thousands of samples were collected over the years and at times when significant rainfall was not occurring. Also, surface water and storm water sampling locations differ because most of the surface water sampling sites were associated with fixed location surface water sites with effluent releases from sewage sites, treated liquid effluents, and other anthropogenic sources as the source of the water. In contrast, rainstorm runoff in the canyons is localized and of variable duration and intensity because of the highly localized nature of the convective rainstorms that occur in the Los Alamos

environs. Also, the fact that not all storm water sampling locations (Figure 3 in report) were not sampled after each storm event may simply indicate the highly localized nature of Los Alamos rainstorms (i.e. it can be raining heavy on one mesa and be completely dry on an adjacent mesa).

RAC Response: *We will revise this statement to include those terms.*

3. p. 6, 1st paragraph

Something RAC should keep in mind is that automated storm water samplers with fixed position intakes give highly biased data on sediment and contaminant fluxes because the samplers do not provided depth integrated results. Sediment loads (g/l/sec) in runoff for much of the hydrograph are very strongly a function of depth from which the sample was taken in the flow. The message is, don't assume data collected with automated samplers is "better" than manual collection methods.

4. p. 14, Section on "ASSESSMENT OF CONTAMINANTS---

RAC is likely aware that ESH-18, ER and NMED each may have had different sample processing methods for surface and storm runoff samples concerning whether or not they removed suspended sediments from the sample. These differences will make comparison of pre- and post fire data both by a particular organization and between organizations very difficult. For example, ESH-18 analytical results for surface and storm water samples are usually reported with an F (filtered) or UF (un-filtered). For storm water they <u>try</u> to run both F and UF. For surface water they typically filter the samples for metal analysis (except Hg and Se to make the measurements comparable to WQCC standards) and **<u>they run the rads unfiltered</u>**.

Because the suspended sediments in surface and storm water samples may contain anywhere from 90-100% of the radioactivity being transported (true for most rads) and their concentrations are strongly a function of sediment particle size, depth in the flow, and time during a runoff event), risk assessments and comparisons with time, space, and between agencies is problematic without some sort of evaluation of what the surface and storm water data really represent. Additionally, such sampling and analytical details varied a bunch over 50+ year history of the laboratory, including QA/QC. While all of this may or may not effect the outcome of the risk assessment, the quality of the assessment will depend on the nature of the input data.

RAC Response: *The objective of the screening procedure in this task was to be as inclusive as possible and not eliminate any analyte from further analysis without clear and just cause. For this reason, we began with fairly cautious or conservative assumptions for the screening process, including drinking water from onsite surface water streams, and using the highest*

concentrations measured in all water samples in these early screening stages. As this iterative process proceeds, we will continue to refine the screening procedure with less caution or conservatism given to various parameters. One of these refinements would be to use the measurement results from only filtered samples. The final risk assessment will be based on available water monitoring data, certainly with a consideration for the difference between filtered and unfiltered water. This is only one of the "problematic" aspects of the various comparisons necessary for this project.

5. p. 14, Section on ---Setting a Risk-based Decision Criteria-

While the discussion in this section may be useful background information, it seems to me that (after all that discussion) the selection of 10^{-5} was sort of arbitrary (i.e. the midpoint of the EPA recommendation). If the intent was to be conservative, why not pick 10^{-6}?

Assuming that suspended sediments were present in the water samples, then the assumption that the water AND sediments were ingested as "drinking water"will add anywhere from 10-1000 times more conservatism to the risk estimates because of the near quantitative association of the rads with the suspended sediments. Sediment loads in storm runoff at LANL can run over a 100g/l (10% by weight) which makes the water pretty much too thick to drink but too thin to plow.

RAC Response: *Without quantitative information about the sediment loads in individual samples, it is not appropriate to assume they were all "pretty much too thick to drink but too thin to plow". Therefore, we chose to conservatively use unfiltered samples in our initial screening assessment. More realistic risk calculations will account for the differences between filtered and unfiltered samples.*

6. p.18, general discussion-

The use of "maximum" or "average" concentrations for the risk assessment is moot until RAC has some idea of what the data represent as per my previous discussion.

RAC Response: *Average concentrations were used to further refine the screening process. We acknowledge this process uses data that may reflect concentrations higher than should be assumed for a "realistic" risk calculation, which this is not intended to be.*

7. p. 19, Table 3-

Value of risk index of Cesium-134 is below 10^{-5}.

RAC Response: *For some risk indices that were near 1 x 10^{-05}, like the index noted by the reviewer for Cs-134 (9.5 x 10^{-06}), we rounded up and included them in our Stage 1 screening assessment to ensure a conservative approach.*

8. p. 21, Section on CHEMICALS-

I presume my comments about rads re suspended sediments also apply to metals and potentially other chemicals.

RAC Response: *Please see response above about filtered and unfiltered samples.*

9. p. 27, ASSESSMENT OF CONTAMINANTS IN SEDIMENTS-

The assumption of an ingestion rate of 75 g/yr assumes average concentration of rad/chemical in whole sediment sample. However, most rads in sediments are very strongly associated with the finer sediment size fractions (those which contaminate crop surfaces). These finer fractions may contain as much as a factor of 10 higher rad concentration that the bulk sediment sample. The point is, 75 g/yr ingestion rate may not be conservative, by as much as a factor of 10, if the ingested sediments consists of finer size fractions.

RAC Response: *Since we do not have any information upon which to base a partitioning of particle sizes to collected samples, it is difficult to identify a method to modify the sampling results to account for this partitioning for this initial analysis. This is a good point, though, and we will consider this point in developing our exposure scenarios and accompanying assumptions regarding ingestion and other pathways.*

10. p. 31, last paragraph-

There is well over 100,00 kg of expended depleted uranium (U-238) munitions at LANL firing sites .

11. p. 32, last sentence-

The statement "This unusual outcome---" should be deleted. I think 0.5 pCi/l is about background for H^3 isn't it? Same comment for U-^{238}. New Mexico soils have highly variable U content. Because this individual value is "higher" than the long term average doesn't mean squat.

RAC Response: *It is not clear to which value in the report the reviewer is referring. The actual purpose of the sentence was to point out that it is not clear why the average concentration for results reported as "nondetects"*

would be higher than for results reported as "detects" for both tritium and U-238.

12. p. 35, ESH-18 Surface Water -

I am troubled by the statements such as "concentrations are not different from concentrations at background locations" and "--data show measurable increases". These statements imply that a statistical test provided the basis for the statement, although I suspect such was not the case. If a stat test was used to support the statement, then it should be described. This comment applies throughout the remainder of the document in that statements about the interpretation of results are made with no supporting statistical details (i.e. see Fig 8 and 9 captions).

RAC Response: *Examining detailed aspects, such as those related to seasonal variation, of tens and even hundreds of thousands of data records within the available budget and the timescale of the project is not feasible. Statistical analyses of the various data are not warranted until the large mass of available data is examined on a more qualitative basis. Our primary focus at this point was to examine spatial and temporal trends, primarily in a visual sense, to see if and where impacts may be suggested and where additional investigation may be warranted. These types of detailed analyses are not needed at this point*

13. p.41, 1st paragraph

The statement "Interestingly, the data for $^{239,240}Pu$, ^{241}Am, and ^{90}Sr at below LANL locations show a more distinct pattern, with increases in concentrations in the data collected after the Cerro Grande Fire" is not supported by Figure 11 nor Table 13.

RAC Response: *We have revised this discussion for clarity and accuracy.*

14. p. 42, 1st paragraph

The statement, "spatial trend analysis" implies some sort of analytical procedure was used to look at the spatial aspects of data. Was this so or do you simply mean that the data were plotted on x-y coordinates?

RAC Response: *As with the suggested statistical analyses, a detailed analytical procedure to evaluate the spatial aspects of the data is not warranted at this stage in the study. Data were evaluated spatially in that plots of relative concentration were examined on overlays of area maps to understand where the highest concentrations were occurring as well as how concentrations at various locations that may have been impacted by LANL operations have varied temporally.*

15. p. 57, 4th paragraph

The statement "These data suggest some amount of LANL impact on ^{238}Pu concentrations" are unsupported without some discussion as to what the data represent. Where and how the samples were taken is critical to their interpretation. RAC should look at W. Graf's report on Pu migration from LANL to Rio Grande.

> ***RAC Response:*** *The fact that a relatively high concentration of ^{238}Pu, exceeding typical concentrations measured at background or fire impacted locations, was measured at a location downriver from the LANL facility suggests the possibility of some amount of LANL impact. Obviously, this cannot be categorically stated without additional analyses of more samples. However, it does suggest the need for additional sampling at this and other locations, both upriver and downriver from the LANL facility, to better assess the potential for LANL-related contamination. Details regarding exactly where and how the samples were collected are not available.*

16. p. 61, Spatial Trends-

I don't agree that a Pu239 concentration of 1.15pCi/g is a notable exception a lack of impact of the fire for other rads. I don't think you can compare one value to a mean and interpret it as a difference.

> ***RAC Response:*** *When looking at Figure 30, that concentration is clearly notable. The reviewer is correct in stating the limitations of comparisons of individual values to average values; however, we clearly make the comparison of this value to the maximum measured value used to calculate the mean value, and it is higher than that value.*

17. p. 64, 2nd paragraph

I don't see from Table 15 and Fig. 31 where the following statement is true. *"As was seen with ^{137}Cs concentrations, $^{239,240}Pu$ concentrations higher than average concentrations measured in other fire-impacted areas (Table 15) are evident in locations along the Rio Grande River (Figure 31).*

> ***RAC Response:*** *This sentence mistakenly refers to Figure 31 (Cs-137) and should instead refer to Figure 32 (Pu-239,240) and this notation will be corrected. As stated in the figure caption, the two red-circled symbols along the Rio Grande River indicate concentrations higher than the upper bound of fire-impacted average concentrations. This was also the case with Cs-137.*

18. p.75, Conclusions, 2nd bullet-

I don't believe that RAC has made a defensible case for saying that pre-fire concentrations for all of these radionuclides suggest the possibility of LANL-related contamination existing before the Cerro Grande Fire at locations below LANL and downriver. This same comment applies to next bullet.

> ***RAC Response:*** *Figures 24 through 28 suggest that concentrations measured at below LANL and downriver locations before the fire may be higher than what would be expected, based on concentrations measured at background locations.*
>
> *The next bullet should refer to post-fire concentrations, and the data we have examined do suggest the possibility of LANL contribution. In no way do we state that the evidence is clear in this regard for any of our conclusions because of the paucity of available data on which to base a definitive conclusion. We merely point out where there appears to be an aberration from expected values, based on concentrations measured at background locations.*

<u>Summary Comments</u>

My summary comments by the questions posed by RAC are as follows:

1. "What surface water monitoring data are available? What organizations provided the data?"

RAC apparently has accessed most of the current and historical monitoring data as well as data collected by ER at some of the environmental restoration sites. There is also another source of water quality data at LANL resulting from directed research and special studies conducted by both research and monitoring Groups. I suggest looking at publications by Purtymun (dozens of publications), Stoker, and Gallagher, all from the original monitoring Group at LANL. An analysis of Pu migration in the Rio Grande (by W. Graf) and in the canyons at LANL (Lane) will be useful in any future modeling by RAC.

> ***RAC Response:*** *We will consider these additional documents for our future assessments.*

2. What are the limitations of the data?

This is a major weakness of the RAC report in my opinion in that it is not apparent that an attempt was made to determine what the data used in their analysis actually represents. "Water sample" data as use by RAC could represent a mixture of processing methods that will certainly complicate the analysis and interpretation of risk and either add to or reduce the conservatism of the risk

estimates. As mentioned, whether a water sample was filtered or not for suspended solids may or may not have bearing on the outcome of their screening analysis.

RAC Response: *We agree with the reviewer that there are limitations to the data that will be considered in our final risk assessment calculations. As mentioned in our report, there are concerns of greater importance that relate to the lack of consistently monitored locations and the existence of representative background locations. A lack of consistency between agencies responsible for sample collection, processing, analysis, and analytical result compilation are all issues that complicate the analyses.*

3. Spatially, are there differences in concentrations of radionuclides or non-radioactive chemicals above and below LANL?
4. Temporally, are differences in concentration seen before and after the fire?

I do not believe RAC has answered questions 3 and 4 quantitatively. Answering these questions will require methods that account for the large spatial variability in the processes controlling contaminant movement in the LANL watershed, the large sampling variability, the limited resources available for monitoring, and the lack of a regulatory imperative for research quality data will limit the ability to resolve radionuclide and chemical transport by surface water in space and time. I don't think such data presently exists nor will it in the future.

RAC Response: *The point of this task is to evaluate the available data, and in a timely fashion, determine if there are readily apparent impacts in post-fire concentrations measured at offsite locations that can be attributed to LANL operations. We acknowledge that this a complex process involving many factors, including those cited, and the reviewer is correct that answering these questions quantitatively may be impossible based on existing data.*

5. How can the surface water monitoring data be used to support an analysis of health risk?

There is no question that the surface water monitoring data can be used to support a risk assessment since this is the basis for establishing the monitoring program in the first place. The question is how well the monitoring data represents the assumptions used in doing the risk assessment.

RAC Response: *We do not agree with the statement that because a monitoring program has been established necessarily implies the data are adequate to support a defensible risk assessment. Such an assessment requires sufficient data to show whether or not LANL-generated contamination is migrating offsite to locations where members of the public*

could be exposed and to quantify that excess exposure related to LANL-generated contamination. Our analysis shows that monitoring data are available to make some statements about possible risks, or the lack thereof; however, limitations regarding available regional background data, pre-fire data, or data for particular locations may preclude definite quantitative statements about the magnitude of incremental health risk.

6. Are there ways to improve the surface water monitoring program in place at the time of the fire?"

- RAC recommendations to coordinate sampling efforts among various divisions onsite is a good one. In addition, their recommendation to coordinate sampling methods and analytical protocols are much needed. However, RAC's discussion about the benefit gained by using protocols for storm water sampling that include multiple grab samples is unconvincing. Likewise, RAC's suggestion to collect samples "over a period of time, depending upon the duration of the storm event but long enough to detect changes in concentrations of analytes, if present" is also unconvincing. I also do not subscribe to the idea that continuous proportional sampling is preferable to grab samples. If RAC believes this to be the case, then the appropriate documentation should also be presented.

  RAC Response: *The purpose of this discussion was to indicate the limitations some sampling methods. In many cases, LANL collects multiple grab samples during a single event that are then composited for analysis. This also complicates understanding the time history of contaminant movement. We will revise the statements to reflect this.*

Review of *RAC* Deliverable Task 2.1
Evaluation of Surface Water Monitoring Data
8/8/01

Introduction states that this task analyzes only the risks associated with radionuclides and chemicals that may have come from LANL during the fire. The work plan states that this task will look at risks associated with these types of releases from the fire, which would seem to imply that the contribution from the combustion of the forest itself would be included.

RAC Response: *The primary purpose of this work is to look at the risks associated with the fire specifically because it burned a portion of LANL property and may have impacted the potential for release from contaminated areas there. However, the process of distinguishing between LANL-associated increases and fire-associated increases and separating one from the other necessarily means we have to assess increases in concentrations from both perspectives; and therefore the risks associated with each. So, while our evaluation will determine the importance of LANL-contaminated areas on increased risk, it will also assess the general increases seen in concentrations in various media simply because of the fire. We will revise the wording in the introduction to reflect this goal more clearly.*

In evaluating storm and surface water against drinking water standards, the maximum concentration in *filtered* samples should be used. Raw storm/surface water in this system isn't drinkable because of the high suspended sediment content (it's practically mud) and risk of microbial illnesses. Filtered sample results could model the potential risk from use of surface water (including storm water mixed into the river or lakes) as a drinking water source for Santa Fe and Albuquerque, particularly since this is a planned future use for the river water. Unfiltered sample results are really appropriate just for modeling direct exposures such as swimming or for the spatial and temporal analysis of runoff to the Rio Grande.

RAC Response: *The objective of the screening procedure in this task was to be as inclusive as possible and not eliminate any analyte from further analysis without clear and just cause. For this reason, we began with fairly cautious or conservative assumptions for the screening process, including drinking water from onsite surface water streams, and using the highest concentrations measured in all water samples in these early screening stages. As this iterative process proceeds, we will continue to refine the screening procedure with less caution or conservatism given to various parameters. One of these refinements would be to use the measurement results from only filtered samples.*

As we develop our exposure scenarios for the surface water risk analysis, and evaluate water usage for drinking , irrigation, or swimming, etc., these issues of filtered vs. unfiltered water will come into play.

The RAC risk assessment considered sediment, sludge, muck and ash as all the same for their sediment analyses. My understanding is that sediment, sludge, and muck do all represent ash from the fire that has mixed with water, suspended sediment, soil, etc., but that ash would be the material in the burn area prior to rains and runoff. Should the ash samples be considered separately?

RAC Response: *The only "ash only" samples we considered in this analysis were the ER Mountain Front data. We used these data to evaluate the concentrations of contaminants in ash above LANL that could contribute to sediment, sludge, and muck concentrations below LANL. The only other information included in this analysis that contained data on contaminant concentrations in ash were samples that were reported to consist of some combination of muck, sediment, and/or ash. Consequently, there was no method to distinguish these samples from samples reported to consist of sediment only.*

Recommendations:

In evaluating storm and surface water against drinking water standards, the maximum concentration in *filtered* samples should be used.

RAC Response*: Please see our response above.*

Los Alamos
NATIONAL LABORATORY

Environment, Safety, and Health Division
ESH-17, Air Quality Group
P.O. Box 1663, MS J978
Los Alamos, New Mexico 87545
(505) 665-8855/Fax: (505) 665-8858

Date: September 20, 2001
Refer to: ESH-17:01-437

Barbara Hoditschek
NMED DOE Oversight Bureau
2905 Rodeo Park Drive East, Bldg. 1
Santa Fe, NM 87505

Dear Barbara:

Below are the comments on Technical Memorandum, "Analysis of Exposure and Risks to the Public from Radionuclides and Chemicals Released by the Cerro Grande Fire at Los Alamos", Task 2.1.

Comments on Task 2.1: Evaluation of Surface Water Monitoring Data Acquired in Relation to the Cerro Grande Fire.

Commenter: Ken Mullen, ESH-18, LANL

1. We previously forwarded a list of some of our concerns with the analytical results that are included in the report. We would be pleased to work with RAC to clear up these issues.

   RAC response: *We are doing our best to ensure that we are working with the proper data.*

2. It appeared that in many cases gamma scan results were used interchangeably with alpha spec isotopic results. Since the alpha spec results are more sensitive this may result in questionable conclusions.

   RAC response: *For this technical memo, we did not discriminate between different analytical techniques, and the reviewer is correct that conclusive statements should consider this aspect of data analysis. This memo was an attempt at a broad overview of a massive amount of data, and time and budget limitations precluded examination of all aspects of the data. More refined analyses that consider the analytical techniques and their implications on the reported results will be used for final estimates of risk.*

3. In many cases radionuclide results were presented without uncertainties. Providing the uncertainties allows the reader to develop a sense of the significance of the data.

   RAC response: *The reviewer is correct in pointing out that uncertainties were not presented in this technical memo. Again, this memo was an attempt at a broad overview of a massive amount of data, and time and budget limitations precluded examination of all aspects of the data. We made the*

assumption that results with sufficiently large uncertainties would have been recorded as "nondetects" or "less-than" and did not attempt to evaluate the uncertainties associated with each of the result measurements we reviewed. This will, however, be important to consider for any data used to develop risk estimates.

4. In cases where averages were taken it would be helpful to know the number of results those were based on. This would be most easily accomplished by adding a column of "n."

   RAC response: *In many cases where averages were calculated, we attempted to provide this information (e.g., Tables 14 & 15). For monitoring data that are used to develop risk estimates, additional details will be provided regarding this type of information.*

5. How were non-detects handled when averages were calculated?

   RAC response: *As mentioned on page 48, in the sediment monitoring section, "For the purposes of calculating average concentrations and plotting results, we attempted to consider only positive results reported as "detected."*

6. I believe significant efforts went into collecting NMED, COE and USGS data in addition to LANL data. I didn't see these other data sources discussed or included in the analysis.

   RAC response: *We attempted to review all relevant water and sediment monitoring data. Very limited data were obtained for water samples collected by USGS from Cochiti Reservoir, and the collection locations were unclear. As a result of these limitations, it was difficult to include a detailed discussion regarding those data. If they are determined to be relevant for understanding contaminant movement further or for quantifying risk, they will be included in future reports. NMED data were analyzed as part of this report to the extent possible (e.g., Table 14, Figures 31-33); however, difficulties associated with understanding sampling locations limited our ability to comprehensively evaluate the NMED data. We were not provided with any analytical results for water or sediment samples collected by DOE; if any are available, we would appreciate seeing them. Again, our evaluation attempted to provide a broad overview of data trends, and the ESH-18, ER, and NMED data sets provided the most information for this purpose. Final estimates of risk will attempt to incorporate all relevant data from all possible sources.*

7. For surface water background stations RAC stated that historically only Jemez River had been sampled. There should be a long term record for the other stations as well. (page 32)

   RAC response: *We based this statement on an evaluation of locations reportedly sampled in 2000 and those historical samples that corresponded to these locations to enable trend evaluation at a given location. This location was the only "background" location for which historical records appeared to be available, based on the data sets with which we were provided. Upon further investigation, it is clear that the sampling location indicated in the historic data set do not necessarily correspond to the sampling location names we have been given for the 2000 data. During the initial cursory review of the location names, there appeared to be good correlation, so we based our analysis on those historic records with location names matching the location names in the 2000 data set. It was necessary to construct database queries to pull out those records because of the massive number of records; if the*

location names did not match, they were not selected. Further review has identified some locations that could be considered, if it is determined that they can augment future analyses.

8. We think that Guaje and Rendija canyons would also represent background for storm water stations. (page 32)

   RAC response: *We will consider the Guaje and Rendija canyon locations as representative of background for future analyses; however portions of those canyons are within LANL boundaries. Also, the fires in the watershed areas of those canyons have impacted concentrations for some analytes, which complicates defining appropriate background levels.*

9. As mentioned above- upstream stations on the Rio have been sampled for many years. (page 35, 41)

   RAC response: *Again, as we stated above in our response to No. 7, we focused our evaluation on the historical data corresponding to the locations sampled in 2000 to enable temporal trend analyses. Because of the organization of the data we were given and the amount of work that was required to compile the relevant data for our analytical needs for this report, it is quite possible that some data that could be useful for making final risk estimates were overlooked, particularly if the location names were not the same in both data the historical and Yr 2000 data sets, as mentioned above. A lack of consistent location nomenclature and maps showing these locations to enable selection of analytical data at specific locations contributed significantly to difficulties associates with the process of evaluating the massive amount of available data. However, we believe we evaluated sufficient monitoring data to make some preliminary statements about contaminant concentrations and movement in the LANL area, and how such data may be used to make risk estimates. Upon further review of the historical data, it appears that there are additional upriver sampling locations along the Rio Grande.*

10. Plot of Cs-137 concentration over time, page 36- I believe there was a method change around 1993, Surveillance Reports might have documented this. Error bars based on the reported uncertainties would be very helpful.

    RAC response: *As we stated above in our response to No. 2, we did not discriminate between different analytical techniques. This memo was an attempt at a broad overview of a massive amount of data, and time and budget limitations precluded examination of all aspects of the data. We were looking at general trends in analytes over time and locations only, and not investigating specific factors that might affect data in individual years.*

11. The high values of Am-241 in Figure 9 are probably due to using gamma scan results. We began running gamma scan about this time. Same comment applies to statement near end of page 38.

    RAC response: *As we stated above, we did not discriminate between different analytical techniques. However, the ^{241}Am results for 2000 and 2001 were 100 to 200 times lower than those in 1998 and 1999 when gamma scans apparently were begun. Assuming the data for 2000 and 2001 also included gamma scan results, the trend analysis can provide a general picture of contaminant levels over time. The apparent decrease in concentrations following the fire does not likely correspond to the inclusion*

of gamma scan results, as this analytical technique tends to provide higher results than the more sensitive isotopic analyses.

12. The concentrations reported in Fig 11 are probably driven by higher TSS after the fire. Were both filtered and unfiltered samples used in this analysis? We had very little data from the drainages above the Lab pre-fire because there was very little flow. The differences in Am-241 plot are probably driven by gamma scan results.

    RAC response: *We agree that it is difficult to analyze these data very thoroughly because of the very limited number of samples; however we do agree that the higher levels in 2000 were probably due to the movement of materials. As a result we had stated, "While this trend may reflect additional materials mobilized by runoff over burned areas, these data should be viewed cautiously".*

13. When organics are reported there should be a full suite (volatile, semi-vol, HE, etc.) of data reported not just a few. (page 70)

    RAC response: *The discussion on page 70 and the chemicals listed in Table 7 focused only on those chemicals from the Stage 2 screening evaluation of all chemicals monitored after the fire (see page 11, Table 2).*

14. I was puzzled by all the "no results" on table 17.

    RAC response: *This was noted if the post-fire ESH-18 data set did not contain results for a specific analyte. Inconsistent analyte nomenclature in the data sets with which we were provided could have resulted in our missing some results for a given analyte.*

15. Few detectable concentrations should mean nothing is present- not that there aren't enough detectable concentrations to draw conclusions.

    RAC response: *This depends on the detection limit of the analytical technique. For example a value of 960 ug/L was reported for dibenz(a,h)anthracene in the ESH-18 dataset for 2000, which corresponds to a risk index of 2×10^{-1}, well above our screening risk criterion of 1×10^{-5}; however, this concentration was listed as "nondetect". This may be an extreme example, but many of the chemicals that topped the list in Table 7a are there because values reported as nondetects equated to high risk numbers. This is important because it may indicate that current analytical capabilities for some contaminants are not adequate in some cases to detect levels that could pose a health risk. However, fully understanding the meaning of nondetect values in terms of potential risk may require more realistic calculations than those used for this initial screening process.*

<u>*Commenter: Bruce Gallaher, ESH-18, LANL*</u>

1. In the Temporal Trends analysis portion of the Sediment write-up, a high Pu-238 result (0.966 pCi/g) from Cochiti Upper is discussed. There are several problems with highlighting" this result. First, the uncertainty in this measurement is quite large, greater than the result (1.15 pCi/g vs 0.966), indicating that Pu-238 was not detected in the sample. This interpretation is bolstered by the result of a re-

analysis of the same sample bottle, which showed a value of 0 +/- 1 pCi/g. Lastly, analysis of a duplicate sample collected at the same time showed Pu-238 to not be detectable (0 +/- 1 pCi/g).

Sample result 0.966 +/- 1.5 pCi/g
Re-analysis 0 +/- 1 pCi/g
Duplicate 0 +/- 1 pCi/g

Essentially, the measurement uncertainty with each of these results is too large to make allow for any quantitative comparison with pre-fire levels. By ignoring the uncertainty in this particular measurement, and subsequent other analyses, RAC significantly overstates the Pu-238 levels in Cochiti Reservoir.

RAC response: *As mentioned above in response to another reviewer's comment regarding uncertainty, this memo was an attempt at a broad overview of a massive amount of data, and time and budget limitations precluded examination of all aspects of the data. We made the assumption that results with sufficiently large uncertainties would have been recorded as "nondetects" and did not attempt to evaluate the uncertainties associated with each of the result measurements we reviewed. This will, however, be important to consider for any data used to develop risk estimates.*

In the data sets with which we were provided, the Pu-238 sediment result of 0.966 pCi/g is reported with an uncertainty of 0.145 pCi/g, which differs by an order of magnitude from the uncertainty the reviewer cites. Also, the organization of the data with which we were provided prevented a clear assignment of field and laboratory duplicates to their corresponding samples, particularly as we attempted to review the entire set of data. For development of risk estimates, duplicate results will be considered to the extent possible, if they and the results to which they relate can be readily identified. We are trying to work with ESH-18 to develop a method for identifying and relating duplicate or reanalysis results and the results to which they correspond in the dataset.

Commenter: Lars F. Soholt, ESH-20, LANL

1. Equation on p. 18 gives results (Ii) in units of Bq per year rather than Bq per lifetime intake.

   RAC response: *The equation will be revised to show the correct units; the value* $2.75 \times 10^4\ d\ y^{-1}$ *should be* $2.75 \times 10^4\ d\ lifetime^{-1}$*. The calculations in the spreadsheet used the correct conversion and units.*

2. Equation for Uo should have CFc in the denominator; Sfo already has inverse mg in the numerator.

   RAC response: *The reviewer is correct and the appropriate change will be made.*

3. It is not clear to me what the daily water intake is for the non-radionuclides; for radionuclides it is 1.1 L per day over an apparent 75 year lifetime. IRIS calculates unit risk based on 2 L per day and 70 kg BW; this appears to be somewhat different than stated on page 21 but consistent with Table A2.

   RAC response: *The daily water intake for the non-radionuclides is also 1.1 L per day; the text will be revised to made this clear.*

4. In Table B1, I am unable to reconcile the lifetime soil ingestion at a rate of 75 g per day with the radionuclide intake column, which implies that only a fraction of a gram is ingested in a lifetime.

   RAC response: *We assumed a soil ingestion rate of 75 grams per year, not day. Table B1 will be clarified to make this clear.*

Commenter: Tom Buhl, ESH-DO, LANL

1. As in Task 1.3, this report also appropriately developed screening levels for radionuclides and chemicals in order to eliminate those that present negligible risk and concentrate on the more important materials.

2. The report should indicate if it is using results from both filtered and unfiltered samples. If it includes unfiltered samples, in many cases this would be an important source of conservatism.

   RAC response: *We did not distinguish filtered from unfiltered samples in our analyses and recognize this as a source of conservatism. As stated in a previous response to another reviewer's comments, this memo was an attempt at a broad overview of a massive amount of data, and time and budget limitations precluded examination of all aspects of the data. However, distinguishing the filtered from unfiltered sample results may be important to consider for any data used to develop risk estimates.*

3. Using a screening level based on direct ingestion of water may not be conservative in some instances. Cs-137, for example, has a very high bio-accumulation factor in fish, that may cause a higher risk by consuming fish than just by drinking the water. This would be missed by a screening level for Cs-137 based on direct ingestion of water.

   RAC response: *Cs-137 was identified as one of our radionuclides of concern based on direct ingestion of water.*

4. The analysis for the air pathway found it needed to use PRS data as well as sampling data. Is there any need to use PRS data in this screening analysis?

   RAC response: *Task 1.3 was focused on developing source term or release estimates of contaminants to air and therefore needed to use available information for that purpose. The Task 2.1 report was focused only on analyzing monitoring data collected after the fire. For a subsequent report for this project, we will use PRS and other relevant data to help develop source term estimates for contaminants in surface water.*

5. The report lists three background locations for surface water – the Jemez River, Chamito (Rio Chama), and Embudo (Rio Grande). It states that sampling was only performed historically at the Jemez river (p. 32). Please note that the other two locations were also sampled historically. Results are available in the annual Environmental Surveillance Reports.

   RAC response: *Please see our response above to another reviewer's comment on this topic. We based this statement on an evaluation of locations reportedly sampled in 2000 and those historical samples that corresponded to these locations to enable trend evaluation at a given location. Using the data*

sets provided to us from the LNAL, we identified certain locations as "background" location based on locations for which historical records appeared to be available. Upon further investigation, it is clear that the sampling locations indicated in the historic data set do not necessarily correspond to the sampling location names we have been given for the 2000 data.

6. In addition to the naturally occurring radionuclides in water samples, enhanced anthropogenic radionuclides from world wide fallout can appear in storm and surface water as a result of the fire. Elevated concentrations of these radionuclides in the ash has been consistently observed in results of samples collected above the Laboratory. In particular, Cs-137 and Sr-90 have been found to be significantly elevated in the ash samples collected above the Laboratory as well as in the Viveash burned areas. This would be an additional source of "background."

   ***RAC response**: The reviewer brings up a good point; the mobilization of radionuclides and chemicals in wildfires is a topic that will be addressed in subsequent reports for the project.*

Review comments and *RAC* Responses For Task 2.1 August 2001

Task 2.1 – Comments from Penny Gomez, LANL-ESH-18

- Using non-detects as results
  A non-detect result means that the actual concentration is anywhere between 0 and just below the detection limit. If the detection limit is 5 and the result is < 5, the concentration is between 0 and 4.999.

RAC Response: *We understand that there are different viewpoints regarding how and when non-detect results should be used. For this Task, we included the values that were reported as non-detects as a conservative method for identifying the maximum concentrations for each contaminant. This helped us to refine the list of potential contaminants of concern, and we intend to base our final risk calculations and model validation efforts on detected values.*

- Using gamma spec results
  Gamma spec does not produce reliable results except for Cs-137. It is used for screening purposes only; the results should not be used.

RAC Response: *As noted above, this Task focused on identifying the maximum reported values for use as a screening tool to refine the list of potential contaminants of concern. Final risk calculations and model validation efforts will be based on data acquired using the most accurate analytical methodologies, wherever multiple methodologies may have been used.*

- Table 1
  ESH-18 reconstructed Table 1 using the data RAC says were provided by ESH-18 and ER (Yr 2000 Data and Yr 2000 water data).
  The two data sets were combined and queried based on:
  Matrix: WG, WT, WS (groundwater, storm water, surface water)
  Suite: RAD
  Sample Date: of > 05-04-2000
  The maximum standardized result
  All methods, including gamma spec, were included.
  - After comparing the maximum results in the RAC Table 1 with the ESH-18 Table 1 it was determined that an improper sort was performed. RAC and LANL have all Max results in common, but they are assigned to different isotopes.

RAC Response: *The main problem associated with data in Table 1 of the Task 2.1 report is related to an error that occurred when transferring the values from the Excel spreadsheet where the calculations were made into the Word table in the report. The values actually used for the screening calculations were correct. Additionally, the process used by RAC to identify maximum results differed somewhat from that used by ESH-18, as described above. The table has been corrected and now contains all ESH-18 and ER data RAC received.*

- Gross Alpha, Gross Beta, Gross Gamma, I-133, Nb-95, Po-210, Zn-65, Zr-95 were not includes in the RAC Table 1

RAC Response: *Table 1 has been updated and now includes all data on specific radionuclides RAC received from ESH-18 and ER.*

- Page 10
  Footnote a says, "For our subsequent risk analysis, we will use the surface water and storm water samples."
  On page 18 RAC says groundwater, surface water, and storm water were used.

RAC Response: *For the Task 2.1 report, all water results were used for the screening assessment, including groundwater, surface water, and storm water. This conservative approach was taken to ensure that we would not miss any potential contaminants. Final risk calculations will be for the surface water pathway and will not be based on groundwater data.*

- Page 18
  RAC states that:
  Table 3 (Stage 1) contains 27 of the original 75 radionuclides listed in Table 1 based on a screening criterion of $1X10^{-5}$.
  Table 4 (Stage 2) contains 25 of the radionuclides listed in Table 3.

  This is not the case.
  Table 3 contains three radionuclides not in Table 4: Am-241, Ce-144, Cs-134.
  Table 4 contains two radionuclides not in Table 3: U-238, Th-230
  If Table 4 is a subset of Table 3 it should not contain additional radionuclides.

RAC Response: *Those radionuclides are now included in Table 3 and the tables are not consitent.*

Historic ESH-18 sent	
Historic RAC used	163759
Data after fire ESH-18 sent	
Data after fire ESH-18 RAC used	42120
All Data	205879
Historic and Data after fire ESH-18 sent	
Historic and Data after fire RAC used	205879

I don't know where they got detection limits from Assigning "<" to results themselves
I don't know how they calculated number of results

RAC Response: *We did not attempt to report detection limits in the Task 2.1 report; however, we did attempt to distinguish between detect and non-detect values using the*

information provided in the "SYMBOL" and "LAB_QUAL_CODE" fields. The number of results that we reported is based on the output of totals queries written to summarize data in the database. We caution the reader when interpreting these numbers to be aware that the ESH-18 data set upon which the Task 2.1 report is based has since undergone continuing updates, presumably in part to finalize provisional data, and those numbers have changed.

Review comments and *RAC* Responses For Task 2.1 August 2001

Task 2.1 – Comments from Penny Gomez, LANL-ESH-18

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Comments on Task 2.1: Evaluation of Surface Water Monitoring Data Acquired in Relation to the Cerro Grande Fire

From: John Pinder

GENERAL COMMENTS:

The purpose of this report is to summarize the effects of the fire on the concentrations of radionuclides and chemicals in surface waters that may be ingested and engender risk. Analyses are also presented for sediments which may also be ingested.

This analysis is in support of an attempt to quantify the "incremental risk." This term is used on page 2 of the Introduction, but its meaning is not clearly stated. Does this mean the risk increment resulting from the fire over and above that pre-existing before the fire? If so, there may be a problem in the sequencing of the application of the risk screening model and the analysis of the pre- and post-fire data. The screening model is applied to evaluate those radionuclides and chemicals whose contributions to risk exceed a nominal value of 10^{-5}. This initial screening is done on the maximum and average concentrations of these radionuclides and chemicals observed in year 2000 data, which contains both pre-fire and post-fire periods. For those radionuclides and chemicals which exceed this risk level, an analysis is then performed on the effects of fire on pre- and post-fire concentrations in water and sediments.

> ***RAC Response:*** *The data used for our screening calculations included analytical results for post-fire samples only.*

If, however, the objective is to estimate the incremental risk imposed by the fire, perhaps the sequence of steps should be: 1) screen the risks from each radionuclide and chemical in the pre-fire period; 2) measure the percent changes in the concentrations for each radionuclide and chemical; and 3) evaluate importance for more detailed study based on the combination of contribution to pre-fire risk and percent change. Note the use of percent <u>change</u> rather than percent <u>increase</u>. This is because the change in risk need not be a positive value. The risks from some radionuclides and chemicals may decrease because of declines in concentrations following the fire. Declines in concentrations and risks may be due to losses in the fire from volatilization o r combustion, dilution due to greater water runoff, or other, more complex mechanisms. A complete assessment of incremental risk may require the comparison of full-scale risk assessments on both pre-fire and post-fire data with emphasis on those radionuclides and chemicals that contribute the greatest effect on the pre-fire risk and experience the greatest change in concentrations.

> ***RAC Response:*** *This approach may be a good idea; however, the lack of consistent sampling locations over time and the lack of "detect" data for many chemicals complicate the issue of examining percent change in the level of a contaminant at a given location. Certainly, we assessed this qualitatively, to the extent possible, by visually examining trends in concentrations over time. We feel this was the most*

appropriate mechanism for interpreting a large amount of data to understand possible impacts at this early stage in the project.

SPECIFIC COMMENTS:

Page 3:
The discussion of the domain in Report 2.1 and the more detailed discussions in Reports 2.3 and 1.4 imply that the watershed extents are based on DEM analyses – but it is not clear from 2.1 or 2.3 what data and spatial resolution (i.e., grid cell size) is being used. Is it 30 m from individual topo sheets, 60 m from North American Landscape Characterization data, 30 m from Landsat 7 data sets, or a coarser resolution? Although elevations in DEMs are usually accurate, the errors among neighboring grid cells can cause degradations of accuracy in slope and aspect modeling and affect watershed boundaries and flow patterns. Knowing the cell size and data source is important in evaluating these potential errors.

What are the limitations on file size for the hydrology models? The limitations for the CALPUFF are clearly described in 1.4, but similar documentation is lacking in 2.1 and 2.3.

The land use data in 1.4 appear to be from an older source, perhaps even the Land Use Land Cover data for 1:100,000 maps based on 1970s aerial photography. Is this correct? Have you consider using the new Multi-Resolution Land Characterization data (www.epa.gov/mrlc/) data being developed for the US. The New Mexico data are available using the same characterization scheme at 30 m cell sizes based on 1990s TM data.

Page 6:
Figures in the form of Fig. 3 would be more informative if they also contained lines denoting the stream channels.

RAC Response: *We agree. The GIS coverages with which we were provided did not include a simplified stream channel coverage that could be used for this purpose. We are attempting to generate a shapefile coverage for the major stream channels and will incorporate this where possible in subsequent reports*

The fire polygon is a single value, yet there were clear differences in fire severity on both broad and local scales. In some areas, the fire was extremely hot and consumed all the needles and ground cover. In other areas, the fire was less intense consuming the ground cover and only scorching a portion of the needles in the canopy. In these areas the trees have survived an put out new growth. These differences will have a major influence on raindrop intensity and runoff in modeling studies. Is there a plan to replace the current fire boundary polygon with a fire intensity data layer?

RAC Response: *The purpose of this figure was to show the proximity of sampling locations relative to LANL and to the area that was burned. Certainly, fire severity impacts the potential for releases. We have additional figures that break down the burned area by level of severity and will be incorporating this into our estimation of releases.*

Page 10:
Because concentrations may be much greater in ground water (=GW) than in surface water (=WS) or storm water (=WT), should they even be included here? The GW samples are from ER and may be expected to be from wells near waste sites. Including them here may be confusing for the subsequent screening models which do not use them.

RAC Response: *The purpose here was to be as conservative as possible initially and then to refine our assessment iteratively, as we make additional assumptions.*

Can references or points of contact be provided for the methodologies employed in the various analyses?

Page 21:
Nitrates are released and are a potential contaminant problem at other DOE sites. Can they really be ignored at LANL?

RAC Response: *Nitrates and nitrites were not ignored; they were included in our screening analysis but did not emerge as a chemical of concern after the stage 2 screening.*

Page 32:
The distinction between historic and year 2000 sampling should be expanded. For example, the year 2000 sampling contains only pre-fire data for the winter and early spring and only post-fire data for the summer and fall. Thus, pre-fire and post-fire comparisons are statistically confounded with seasonal effects. The historic data contain a variety of samples from all seasons.

RAC Response: *Examining detailed aspects, such as those related to seasonal variation, of tens and even hundreds of thousands of data records within the available budget and the timescale we have to complete this project is not feasible. Further, statistical analyses of the various data are not warranted until the large mass of available data is examined on a more qualitative basis. Our primary focus was to examine spatial and temporal trends, primarily in a visual sense, to see where impacts may be suggested and where additional investigation may be warranted.*

Are the storm water samples that represent background conditions shown in any map?

RAC Response: *No, but their general location relative to LANL is described on page 32.*

In Tables 10 and 11, it should be made clearer that the lines without < are for detectable data and that no line is presented for the mean of all the samples. Perhaps adding such a line may be useful. The unusual results for H-3 and U-238 need some clarification. How can the mean for detectable H-3 be < 0 when the mean for nondetectable H-3 is 32? If the authors feel that no explanation is forthcoming, perhaps the data should be listed in an appendix so that readers may make their own interpretation.

RAC Response: *We have revised the title and footnotes for the tables to clarify the source of the data. Regarding the values for "detects" for ^{3}H and ^{238}U being less than for "nondetects", we cannot offer an explanation as we stated in the text. The data used for this assessment are available for review as they were provided to us as part of the Task 0 deliverable. This can be investigated further for subsequent steps in the risk assessment process if necessary.*

Page 34:
The paragraph at the bottom of the page is not clear. How are these data from background locations not directly impacted by runoff from LANL "useful for evaluating". This may be true for the Rio Grande data in Table 10 but why any data in Table 11? They may indicate what is flowing in the tributary, but the impact on the river depends upon the concentration in the river and the relative flow volumes of the river and the tributary.

RAC Response: *We have revised the text to indicate the usefulness of background data for understanding the magnitude of concentrations that could be expected in locations assumed not to be impacted by LANL operations.*

Page 36:
The presentation of the data in Figs. 7-10 and Table 12 are somewhat confusing and misleading. The pre-fire data are clear comparisons of the Rio Grande below LANL, the Rio Grande below Cochiti and the Jemez. These show similar concentrations with some higher samples. The green lines are sometimes hard to see and are comparing post-fire <u>means</u> with pre-fire data. There is no expression of the variation in the post-fire data. It would be better if the post-fire data were presented graphically in a manner like the pre-fire data. This would make the comparisons more explicit. These should be more like Figs. 24 to 28. For these figures and subsequent comparisons of pre-fire and post-fire data, there are no references to statistical procedures. Conclusions are drawn without supporting statistical analyses. In some cases these analyses may be complicated by the quality and the quantity of the data, but some statistical support for the conclusions should be provided.

RAC Response: *Our focus for this task was to identify locations or contaminants that suggested the possibility of being impacted by the fire, specifically with regard to higher measured concentrations. We did this in a qualitative way for this first stage of our risk assessment by plotting the monitoring data temporally and spatially. Detailed statistical analyses were not warranted at this point.*

If the interest is incremental risk, why are the Jemez data presented at all? They merely complicate the pre- and post- comparisons UNLESS there is a difference in the Jemez between pre- and post- that represents a climatic influence which complicates the interpretation of the Rio Grande data.

RAC Response: *The purpose of the Jemez data is to provide some understanding of regional background concentrations by which to make comparisons to concentrations measured at locations below LANL, such as along the Rio Grande. Considering the limited amount of background data to use for our comparisons, we felt it was important to consider these data.*

Table 12 could be expanded to present pre- and post- data for all Stage 2 radionuclides from all areas in a manner similar to that for Table 13.

RAC Response: *Similar statistical information can be gathered for other stage 2 radionuclides if it is necessary for later stages in the risk assessment process, but we do not have the resources to modify the Task 2.1 report to that extent.*

Page 41:

Figure 11 presents a more direct comparison of data for storm water but is confusing in what is Above LANL and Below LANL. Does Above LANL mean the list on page 32? Does Below have the same meaning as for Figs. 7-10? I think so, but it is not clear. Could Below also mean that below Cochiti? More importantly, does "Year 2000 Data" mean all of 2000 including the 4 months before the fire? Or, does it only mean the post-fire period? It is the post-fire to historic comparison that is critical for the analysis. This comment also applies to Table 13 as well.

The conclusion in the paragraph at the bottom of page 41 is critical to the question of incremental risk, but I have little ability to judge the accuracy of the conclusion given the format of the data presentations in the preceding figures and tables.

RAC Response: *Year 2000 data refer to post-fire results only. The reviewer is correct; the above and below LANL locations are similar to those for Figures 7-10 and do not include data from below Cochiti Reservoir. The text at the bottom of page 41 was revised somewhat to indicate that the data cannot be used to make firm statements about temporal trends for some of the radionuclides. Figure 11 was deleted and data from Table 13, which is more complete statistically, provides the basis for the summary remarks. The monitoring data alone are insufficient to understand the complete process. Based on our cursory review, we can see some indications of increased concentrations for some contaminants following the fire, but whether this is related to the fire itself or LANL cannot be determined from the monitoring data alone. The monitoring data will be revisited once we get farther along with the modeling to see which contaminants may pose the greatest risk. This is necessary to focus our efforts*

Page 42:
Again, Figs. 12-15 would be more interpretable if the stream channels were shown.

RAC Response: *We agree. The GIS coverages with which we were provided did not include a simplified stream channel coverage that could be used for this purpose. We are attempting to generate a shapefile coverage for the major stream channels and will incorporate this where possible in subsequent reports*

Figures 12-15 show patterns of increases, but it is not clear whether these differences are 1) consistently measured changes, 2) sporadic occurrences or 3) single, unusual events. If they are consistent patterns, then time series plots would be useful.

RAC Response: *The data shown in these figures are post-fire data from the year 2000 only. The focus of this report was a qualitative analysis to identify locations or radionuclides that deserved further investigation. Time series plots are shown in other figures (e.g., Figures 7 through 10); these figures were intended to help understand the spatial patterns and trends in concentrations.*

Page 46:
Figures 16 and 17 also do not address the main question of how fire influenced the dose by increasing concentrations. Rather than showing the changes before and after the fire, they merely compare concentrations among locations after the fire. For all the reader knows, these patterns could be the same as before the fire.

RAC Response: *These figures were intended to show the relative magnitude of measured concentrations at several different locations by two separate organizations following the fire to understand spatially where the highest concentrations appeared to be occurring.*

Page 53:
Table 16 is misleading. It compares the concentrations of radionuclides in areas supposedly not impacted by LANL operations between burned and unburned areas. The locations are widely dispersed across the region and appear to indiscriminately mix soils and sediments. Moreover, the lack of LANL effects on some of the burned areas is even questioned in the text. The text states "highlighted radionuclides show clearly higher measured concentrations in areas impacted by recent fires," but what is the meaning of this result? Is it the authors' contention that these separate fires all increased radionuclide concentrations? If so, by what mechanism?

RAC Response: *There were limited data regarding concentrations of contaminants measured in burned areas that were not clearly impacted by LANL operations. One of the sources for these data includes samples collected by ER and NMED in the Viveash Fire area. The other included both soil and sediment samples collected from the Mountain Front area to the west of LANL. We included averages for sediment and soil data in an attempt to augment the limited available information regarding background. Averages that apply to soil data are indicated in the tables*

as are averages that apply to particular locations, and the reader can exclude those if he/she feels it is appropriate. Distinguishing further between sediment, ash/muck, sediment/ash was not possible except that pre-fire data can be assumed to be primarily sediment with little ash component.

To understand whether concentrations measured after the fire represent changes due to its proximity to LANL, or possibly changes resulting from the presence of the fire, it is helpful to examine data that may represent conditions existing at a burned area remote from LANL or presumably not impacted by LANL operations. Based on a general comparison of mean concentrations, it appears that a fire may result in concentration increases, at least for some radionuclides. One might expect a major fire to have some impact on mobilizing, concentrating, and/or redistributing of contaminants in a number of different media. This is certainly the case for naturally occurring radionuclides released to the atmosphere, and the data we have reviewed suggest that concentrations of some radionuclides may be elevated in soil and/or sediment following a fire. Determining the mechanism by which this may occur goes beyond the scope of this project.

We will add text to discuss the fact that these apparent increases may be influenced by differences in location and/or media (i.e., soil vs. sediment). However, we maintain that this is a valid comparison, based on available data, and is critical for establishing the potential impact of LANL on concentrations measured offsite.

I submit these data are extremely over-interpreted. Some differences are observed between samples drawn from a limited number of locations with those drawn from other locations where there has been no control of sediment or soil type, or elevation, or vegetation cover. Moreover, the only comparison employed is a range of means and not the range of the individual data. It is a far more parsimonious conclusion that these data represent spatial and matrix variation than that they represent some effect of recent fires. Furthermore, if it was recent fires, why are only the anthropogenic radionuclides increased?

RAC Response: *The reviewer has a valid point in stating that these data may be over-interpreted. We have made comparisons of post-fire data to mean concentrations and have examined a number of instances where individual concentrations in samples from the publicly accessible Rio Grande exceeded these averages by comparing the individual concentration with the maximum measured concentration in a fire-impacted, presumed background area. The bottom line is that we were looking for any indication that the presence of LANL in combination with the location of the Cerro Grande Fire may have caused increases in contaminant concentrations, and consequently increases in risk. This appears possible in some cases, but the environmental monitoring data alone are difficult to use to make conclusive statements about incremental increases because of complications the reviewer notes, such as spatial and matrix variations. Having had the opportunity for further data review, concentrations of barium, copper, and*

lead also appear to be increased in ash samples, as noted in the Task 2.5/6 report. Additional review of monitoring data for contaminants identified as most important in terms of potential risk will be incorporated into future reports.

Page 57:
"Figures 24 through 28 show trends in Cs-137, Am-241, Sr-90, Pu-238 and Pu-239,240 ... all show increases in average concentrations" Figure 24 shows increases in Cs-137 concentrations that are just as apparent for the Above LANL samples as for any other area, but the other figures such as Fig. 25 for Am-241 are less clear. The data for Pu-239,240 are subject to alternative interpretation, and the data for Sr-90 indicate a decline following fire. I don't understand how this conclusion was reached.

RAC Response: *This statement refers to the data in Table 16, which suggest that the concentrations of these radionuclides may have increased as a result of the presence of a fire. It does not refer to interpretations of the data presented in Figures 24 through 28. We will revise the text accordingly.*

This whole section appears to be entangled in trying to use the data in Table 16 to interpret Figs. 24 – 28. I've already commented on Table 16. The data in that table are weak compared to the strong pre- to post- comparisons in Figs. 24-28. Using Table 16 complicates, dilutes, and weakens the strong data in these Figures.

RAC Response: *The pre- and post-fire comparisons are only useful if there is an understanding of how a fire may impact concentrations. The reviewer is missing the point that both the existence of LANL and the presence of a fire may have some impact on post-fire concentrations. The data in Table 16 may be weak, but they offer the only mechanism by which to examine the latter of these potential contributors to changes in measured concentrations.*

Page 61:
"for Cs-137 and Pu-239,240 ... the Cerro Grande Fire resulted in generally widespread higher concentrations" This is somewhat supported for Cs-137 by Fig. 24 but in Fig. 29 approximately ½ the locations show an increase and ½ show no increase for Cs-137. For Pu-239,240, the data in Fig. 28 are not clear and the data in Fig. 30 have many of the increases in samples above LANL. Part of this may be due to the method of data comparison. In footnote 1 it is made clear that the post-fire means are based on 1 or 2 samples at a location, whereas the pre-fire samples often contain 20 or 30 or more samples at a location. For positively skewed data-which is usually the case for anthropogenic radionuclides in sediments-it is common for the arithmetic sample mean for small numbers of replicates to be greater than the population mean and then decline to the population mean as the number of replicates increases. This occurs because values greater than the median are often much greater than the median but values smaller than the median are only slightly smaller than the median. If the data are transformed to logs, this tendency is reduced. These plots of greater and lesser should be supported by t-tests of pre- and post- data where the number of post- replicates is 2, and by z-tests where the number of post- replicates is 1.

RAC Response: *The purpose of Figure 29, and other like figures, was to show where post-fire concentrations were higher than pre-fire average concentrations. The reviewer is correct is stating that this was not observed at all locations. This statement will be reworded. See previous response to comments regarding statistical tests.*

These comments apply to subsequent plots of spatial comparisons.

Page 75:
Conclusion 3 comes from Table 16 and is questionable.

RAC Response: *As stated above, the available data suggest higher concentrations for these radionuclides in fire impacted areas. The data in Table 16 may be weak, but they offer the only mechanism by which to examine the latter of these potential contributors to changes in measured concentrations.*

Conclusion 4 needs to be supported by statistical comparisons.

RAC Response: *The primary objective of this task was to make an attempt to discern the impact of the Cerro Grande Fire and its proximity to the LANL facility on subsequent changes in contaminant levels. This necessarily involved the dissolution of voluminous data (i.e., tens and hundreds of thousands of records for many different contaminants) into some format that could be visually examined for changes in spatial and/or temporal trends, and statistical analyses of these data was not possible within the budget and timeframe available for completing this project.*

It would appear that the major increment of risk identified by the data is the ingestion of sediment.

RAC Response: *We did not make such a statement in the report, and we have not derived any quantitative increment of risk in this report. Based on this review of environmental monitoring data, some contaminants appear to be elevated in post-fire sediment samples, in particular, and we assume this is the reason for the reviewer's comment. It is unlikely that this increase in sediment concentrations would not also be accompanied by increases in surface and storm water concentrations. However, in some cases this is difficult to discern because of a lack of data. Additional review of monitoring data for contaminants identified as most important in terms of potential risk will be incorporated into future reports.*

Page 80:
The name for Plutonium-238 should be highlighted.

RAC Response: *That change has been made in the final report.*

Comments on RAC Deliverable 2.1
Ralph Ford-Schmid

Task 2.1 Page 10; Table 1
PU-239 (17 pCi/L) should be 22.8 pCi/L Pu-239/240 (as posted on LANL ESH web site for Pueblo Canyon)

> ***RAC response:*** *17 pCi/L should be 57 pCi/L; ER measured this concentration in Pueblo Canyon on 8/2/2000. Table 1 in Appendix A lists the maximum values that were used for the screening. The value in Table 1 will be corrected.*

Task 2.1 Page 16; Paragraph 2 first sentence
10^{-5} should be used for cumulative risk assessment (all cancer causing chemicals and rad) not as a screen for individual CC agents

> ***RAC response:*** *We agree that a more restrictive risk criterion is appropriate for assessment of the risk from all cancer causing agents via all exposure pathways, than for assessment of the risk from individual cancer causing agents. Our review of risk-based criteria used by various agencies and organizations for similar types of problems did not identify an established standard risk criterion within the U.S. for assessing cancer-causing agents. We welcome any documentation you can provide us to support your statement regarding the use of a 10^{-5} cumulative risk criterion. In the absence of our finding an established value we selected a screening risk index value of 10^{-5} for assessing each cancer causing agent individually. We considered this value to be appropriate given the paucity of detailed surface water concentration measurements and source term information. We concluded that this errs on the side of conservatism, and are confident it ensures no cancer causing agents are removed from the analysis prematurely*

Task 2.1 Page 18; Second Paragraph; Second sentence;
Recommend that you use 2.4 Liter per day. (EPA Exposure Factors Handbook, 1997)

> **RAC response:** We used the methodology described in Eckerman et al. (1999) and assumed the combined lifetime average intake of water of 1.11 L d^{-1}. This water intake is based on survey data of the U.S. Department of Agriculture and includes drinking water, water added to beverages, and water added to food during preparation, but not water intrinsic in food as purchased (see Table 3.1, p. 139, Eckerman et al. 1999). We assumed this intake occurred for 7 years as described in the report.

Task 2.1 page 22 Contaminants
First bulleted paragraph
These tables do not include the TEFs for the 12 dioxin-like PCB congeners (PCB 77, 81, 105, 114, 118, 123, 126, 156, 157, 167, 169 & 189). In addition, the TEFs listed for Chlorinated Dioxins in Tables 5b & 5c should be replaced with the latest version adopted

by the World Health Organization (WHO) (Van den Berg, et al., 1998) and expanded to include the WHO TEFs for all 7 PCDDs and all 10 PCDFs that have WHO TEFs.

RAC response: *We listed the TEFs only for those chemicals that were monitored in water by either ER or ESH-18. We understand that there are more TEFs for other compounds. We have added the World Health Organization's TEFs along with the EPA TEF's in Table 5b and 5c. Use of the WHO TEF's does not change our screening ranking.*

Page 24, 25,26; Table 6 & 7a

It appears as if the Risk indices for Tetrachlordibenzodioxin[2,3,7,8-] and Tetrachlorodibenzofuran[2,3,7,8] were calculated separately. This index should be based upon their combined TEQs (all dioxins, furans, and the 12 dioxin-like PCBs if available) that have dioxin-equivalent Toxic Equivalency Factors (TEFs) and reported as a single Tetrachlordibenzodioxin[2,3,7,8-] TEQ.

RAC response: *The Risk index for each chemical was calculated separately. We will investigate the approach you suggest of combining all dioxins and furans. However, both TCDF[2,3,7,8] and TCBD[2,3,7,8-] were identified as chemicals of concern and have been included in our assessment.*

Page 24, 25,26; Table 6 & 7a

It appears as if several constituents' Risk indices are larger for average concentrations than for maximum concentrations (Nitrosodimethylamine, Tetrachlordibenzodioxin[2,3,7,8-], Hexachlorobenzene, Pentachlorphenol, Dichlorobendidine, Azobenzene, & Hexachlorobutadiene). Please verify if these values reported are correct.

RAC response: *Thank you for pointing this out. These values have been corrected.*

Task 2.1 Page 27; Assessment of contaminants in sediments
1st Paragraph

Please show a table of soil screening values (pCi/g in soil resulting in a 1×10^{-5} risk level) for the radionuclides assessed based upon the consumption of 75 grams of soil per year. Alternatively a column could be added to Table B-1.

RAC response: *The sediment screening was carried out for those chemicals and radionuclides not identified through the water screening process. For chemicals, the maximum concentration in sediment was compared to the PRG if available. The PRG values have been added to Table B-2. For those chemicals without a PRG, the slope factor was combined with the maximum concentration in sediment to determine the risk index.*

Task 2.1 Page 27; ASSESSMENT OF CONTAMINANTS IN SEDIMENTS
2nd Paragraph

Were Aroclors & pesticides added through this analysis or do you mean Aroclors only?

RAC response: The Aroclors only were added

Task 2.1 Page 29; SURFACE WATER AND STORM WATER MONITORING DATA: RADIONUCLIDES
Last bullet
Please justify why only post-fire ER sediment and water data were evaluated. The ESH-18 has the most comprehensive database (over 90 samples) on water quality in 2001 and should be included in this evaluation. If NMED data was not useable identify why not and what needs be accomplished to utilize it.

RAC response: This is a misunderstanding of the statement in our report. We will revise the statement for clarity. For the ER data, only post-fire data were evaluated. Data from ESH-18 provided the basis for the majority of our evaluations, and we also considered NMED data where possible, though a lack of location coordinates for many samples prevented an in-depth analysis.

Task 2.1 Page 34; Table 11
It appears that the average concentrations for U-238 was 6.6 pCi/L (for 31 measurements) and < 42 pCi/L (for 28 measurements). Is this a detection limit issue or a typo?

RAC response: This result surprised us too, but is what was reported in the database we received. We make the statement in the paragraph referencing these tables that it is not clear why the average concentration for values reported as "nondetect" would be greater than the average concentration for values reported as "detected."

Task 2.1 Page 41; First and last paragraphs (referring to Figure 11)
These two paragraphs seem to have contradictory statements, especially the last sentence on the page, as Am-241 & Pu-239/240 both appear to be higher post-fire than historically (although Am-241 was not analyzed historically).

RAC response: The two paragraphs are referring to different measurement results. The top paragraph on page 41 is referring to storm water data (Figure 11) and the bottom paragraph is summarizing the ESH-18 surface water results. Some text has been added to clarify the discussion and is shown here as the underlined text.

Top paragraph, p. 41: ....Figure 11 shows similar levels of ^{137}Cs concentrations in storm water below LANL historically and after the fire. The data for ^{239,240}Pu, ^{241}Am, and ^{90}Sr at *below LANL* locations show a more distinct pattern, with increases in concentrations measured in storm water collected after the Cerro Grande Fire. <u>However, increases in concentrations of ^{239,240}Pu and ^{90}Sr at *above LANL* locations were also measured after the fire.</u> While this trend may reflect additional materials mobilized by runoff over burned areas <u>at locations both *above and below* LANL</u>, these data should be viewed cautiously.

Bottom paragraph, p. 41: ... The temporal trend analysis of the ESH-18 surface water data suggests that concentrations of ^{137}Cs, $^{239,240}Pu$, ^{241}Am, and ^{90}Sr were not increased above concentrations measured historically in the Rio Grande. There are increases of some radionuclides at various times in the past, particularly in the 1990s. However, no obvious increases following the fire were detected based on our analysis of surface water samples. Concentration differences were seen, but the concentrations measured after the fire tended to be less than concentrations measured in the years preceding the fire, in particular for ^{241}Am and $^{239,240}Pu$, and ^{90}Sr in the early 1990s. In storm water, increases of some radionuclides were measured in the Rio Grande but similar increases were also measured at locations above LANL.

Task 2.1 Page 52; First paragraph
Fourth sentence
ER(2) should be ESH(2).

RAC response: No ER(2)is correct and refers to the sources listed in Table 15.

Task 2.1 Page 70; Table 17
Please provide units for this table.
Aroclor-* (please define *).
Aroclor is listed as all U values (non-detects) yet table 2 (Page 11) lists All Aroclor (1221) at a maximum detection of 2.7E +00 ug/L
NMED has provided data on Total PCB at detection limits lower than the Aroclor detection limits provided for both stormwater and sediments. Please evaluate.

RAC response: *While ER concentrations in water are reported for Arochlor-1016, Arochlor-1221, and ESH-18 reports values in water for Arochlor-1016, Arochlor-1221, Arochlor-1232, Arochlor-1242, Arochlor-1248, Arochlor-1254, Arochlor 1260 and Arochlor 1262, all values are also reported as below detection limit. For our screening assessment, we used the maximum reported value, even though it was below detection limit. The highest value was reported for Arochlor-1221 for both the ESH-18 and ER data.*

Task 2.1 Page 72 SURFACE WATER FLOW AND TOTAL SUSPENDED SOLIDS DATA

Recommend that only Total Suspended Sediment (TSS) data analyzed using USGS protocol or equivalent be used for this assessment. The standard Total Suspended Solids (TSS) method may underestimate total suspended sediment by up to 30 percent (dependent upon the percent sand fraction). Recommend that NMED suspended sediment analyses be utilized as we used a method equivalent to the USGS TSS and also ran separate analyses (radionuclides, metals, and some organics [dioxins]) on the sediment fraction of most NMED storm water samples. This, in conjunction with the

LANL generated hydrographs corresponding to the sample locations; dates and times will provide a more accurate assessment of total contaminant transport.

RAC Response: *At this point, we cannot correlate sample locations because the 2 organizations use totally different naming conventions. NMED will have to provide information regarding which NMED samples correspond to which LANL samples for us to be able to do this. We will use NMED "total dissolved solids" and "total suspended load" data in our analyses. We will use whatever TSS data that are available because, as it stands now, they are quite limited, so all data will be important to consider. ESH-18 analyzed TSS by EPA:160.2 and GRAV TSS, and they analyzed TDS by EPA:160.1 and GRAV TDS. However, a 30% underestimate in TSS is only one of several sources of uncertainty for this work.*

Task 2.1 General comment

There is no mention of potential effects due to bioaccumulation of any constituent in fish (and potential for human exposure through fish consumption) downstream of the Cerro Grande fire area (Rio Grande & Cochiti Reservoir). The bulk of most contaminants (especially dioxins, PCBs, some metals and radionuclides) is bound to organic mater and the sediment fraction of stormwater and will be deposited at varying distances downstream dependent on particle size and water velocity. Many of these constituents may have relatively low levels dissolved in water but through bioaccumulation and bio-magnification through the food chain may increase in concentration in fish tissue to levels that may be hazardous to humans consuming them. This potential exposure pathway should be evaluated.

RAC Response: *Fish consumption will be evaluated as one of several potential exposure pathways in our scenarios.*

SUPPLEMENT TO COMMENTS ON THE RISK ASSESSMENT CORPORATION REPORTS FOR TASK 2.1

These comments are in addition to what was submitted earlier.

On page 27 of the Task 2.1 report, "Evaluation of Surface Water Monitoring Data Acquired in Relation to the Cerro Grande Fire," a screening level for sediment was obtained using a soil ingestion scenario. An individual was assumed to ingest 0.2 g of soil per day for his/her entire lifetime, and this was multiplied by the risk coefficient to obtain the risk index for the material. The report concluded "...no radionuclide was present in sediment at a concentration that exceeded screening level."

This procedure may not take into account important exposure pathways for many radionuclides and chemicals, and so would not be conservative. The radionuclide Cs-137 provides a good illustration. External radiation is the principal exposure pathway of Cs-137, and is not accounted for by soil ingestion. For example, a generic RESRAD 6.0 run was performed for soil having 1 pCi/g of Cs-137 using all default RESRAD parameters, except that the soil ingestion rate was increased from 0.1 to 0.2 g/day to match the RAC procedure. This calculation gave an EDE after the first year of 2.3 mrem, of which 1.9 mrem came from external radiation and only 0.0028 mrem came from soil ingestion. Thus screening based solely on soil ingestion may indicate that Cs-137 is not an issue, and would eliminate it from further consideration, but it would not take into account a possibly considerably greater risk from external radiation.

Equally important is that the dose/risk from other important pathways also greatly exceeded that from the soil ingestion pathway for Cs-137. These included ingestion of plants (0.2 mrem) and meat (0.2 mrem). This is significant because it indicates that the risk from exposure to radionuclides or chemicals that do not have a strong external radiation component like Cs-137 may still be considerably underestimated and missed by this screening procedure.

It would seem that at this stage of the RAC study a more conservative screening criteria for sediment should be used than one based on of soil ingestion. This would be a criteria that takes a more comprehensive look at the different pathways involved.

RAC Response: *The reviewer correctly points out that the relative importance of a given pathway may vary by contaminant, and screening only certain pathways could lead to the omission of a potentially important contaminant. It is important to point out that the screening calculations based on ingestion of sediment were conducted only for radionuclides and chemicals that had not already been identified as potentially important based on ingestion of water, which did identify Cs-137 as an important contaminant. We agree that screening calculations to consider additional pathways may be important to consider for some contaminants, and we have expanded our screening calculations to consider the external exposure*

pathway. No additional radionuclides emerged from that pathway screening and the results will be reported in the final Task 2.1 report.

Comments on Task 2.1: Evaluation of Surface Water Monitoring Data Aquired in Relation to the Cerro Grande Fire

By
F. Ward Whicker
Department of Radiological Health Sciences
Colorado State University
Fort Collins, CO 80523

August 16, 2001

General Comments:

1. The amount of potentially relevant information gathered for this effort is significant and impressive for such a limited amount of time. I am not certain of the degree of completeness of the monitoring data examined here. There likely are some relevant research data also that could be useful.

   RAC Response: *We agree that additional research data could be beneficial. However, the amount of time that has been required to gather data related to the actual fire and data related to areas of contamination at LANL has precluded our ability to pursue very many additional sources of information. We would appreciate direction to any relevant data.*

2. In general, my review only mentions things currently in the report which I might question or comment on for possible improvement. Text and illustrations that lack my comments generally imply material acceptable to me on first reading.

3. The RAC team's general understanding of the of the risk analysis process for radionuclides and chemicals, as it might be employed in this screening analysis is, I believe, sound. I agree with the risk criteria and approaches employed here, although I do think it will be important to measure incremental risk from the fire by estimating pre-fire risks and then post-fire risks, with uncertainties expressed on each, so that a determination can be made as to whether any apparent difference is statistically significant. This may require more post-fire data, as explained in subsequent comments. I do feel that the screening approach is extremely conservative and hypothetical in terms of many of the basic assumptions. Whether or not a more realistic analysis is warranted will obviously depend on the results of the screening analysis.

   RAC Response: *The primary limitations to quantifying LANL's potential contribution to incremental risk from the fire relate to a lack of available data to adequately characterize expected background concentrations (particularly for*

water) and to adequately characterize expected concentrations in the vicinity of any forest fire. The reviewer is correct in asserting that additional post-fire data would be beneficial, as would additional pre-fire data at certain locations.

We intend to pursue more realistic analyses as part of future reports, and we agree that our initial assessment is extremely conservative. However, it was important to avoid eliminating contaminants from further concern during the early stages of this project without a clear and defensible justification for doing so. The fact that last year's monitoring data were just made available to us in recent months and that significant effort was required to create data sets that facilitated interpretation prevented a more detailed assessment at this point.

4. I do feel that more data, and robust statistical analyses, are needed to underpin statements about increased water and sediment concentrations of certain contaminants as a result of the fire. The amount of post-fire data is very limited, and numerous other variables, such as seasonal influences on plant cover, water flows, runoff, surface erosion and suspended sediment loads, rather than fire, may have caused apparent increases or decreases in contaminant levels in water and sediments. To my naked eye, it was not at all obvious that temporal and spatial changes in the environment resulting specifically from the fire had a clear impact on LANL-derived contaminant levels in water or sediments. The analysis will ultimately need to demonstrate this more clearly by removing the effects of extraneous variables. I am not certain about the possibility of being able to do this with the current set of monitoring data. If available data permit, I strongly recommend a study of possible statistical relationships of sediment loads and stream flows to the contaminant concentration data. This may be one approach to the need to sort out at least some of the numerous variables in addition to fire that may have influenced the data.

   RAC Response: *We agree that additional data would certainly increase our ability to sort out the impact of the various variables that could contribute to increased concentrations. Based on the available monitoring data discussed in this report, there is insufficient information to conclusively state that any contaminant is present in higher concentrations after the fire than before the fire. However, our analyses suggest the possibility of higher concentrations at some location for some contaminants, but data limitations preclude making quantitative or definitive statements about whether or not these apparently higher concentrations are related to LANL. Our analyses do, however, suggest that additional sampling for certain radionuclides and at certain locations would assist with better understanding and quantifying incremental post-fire risks associated with LANL.*

5. The report needs to explain a number of important methodological details of the monitoring data more fully, if a clear and credible analysis is to be achieved. For example, the reader should know how the water samples were treated prior to analysis. For instance, were they filtered or centrifuged, or did the analyses reflect

both dissolved and particulate matter? If the latter, the data may reflect little more than changes in total suspended solids (TSS) content. Another important matter is the analysis technique itself. For example, do the radionuclide and chemical concentrations measured represent the total amounts of material in dissolved, particle surface-bound and intra-particle entrained forms, or just in one or two of these forms. An example of where this could make a big difference in interpretation is aluminum in water (Fig. 36). Enhanced Al levels may simply reflect a higher TSS content since $KAlSi_3O_8$ is a major constituent of common soil minerals. This will also impact risk calculations because of great differences in solubility and resultant toxicity of the contaminants, depending on the physical/chemical forms actually reflected in the measurements. Furthermore, if these methodological details were not consistent among all the data analyzed, the methods as well as extraneous environmental variables could seriously confound the data analysis.

RAC Response: *We agree that there are several issues that confound the capability to discern incremental risk related to LANL. Certainly, our final risk calculations will take into account apparent differences between filtered and unfiltered results. Limitations with regard to consistent methodological details and a lack of information about physical and chemical forms of measured contaminants will result in risk numbers that are inherently more uncertain.*

Specific Comments:

1. Page 1, 4th bullet: Does the "public" include LANL personnel? This could be important.

   RAC Response: *For our risk assessment, we have defined scenarios for those fighting the fire on LANL so in that sense the public may include LANL personnel.*

2. Page 8, last line: I don't follow the meaning of the sentence "About five samples---" in the context of the paragraph.

   RAC Response: *The number of samples collected for each analyte varied with five being the average number collected for most analytes. The number of samples for each analyte are now included in Tables 1 and 2.The text will be revised to clarify this.*

3. Pages 10 & 11, Tables 1& 2: Could mean values and sample sizes be shown in the table, as well as locations of maximum values? A footnote indicating whether or not these samples were filtered prior to analysis would greatly aid interpretation.

   RAC Response: *The mean values and sample size have been added to the table along with the footnote suggested by the reviewer.*

4. Page 14, first line of text: Would this read better as "Because of the great number of samples collected and analyzed for chemicals and radionuclides in a fairly—"?

   RAC Response: *Yes, the text has been revised per the reviewer's suggestion.*

5. Page 16, line 6: The choice of a risk level of 10^{-5} seems reasonable to me, but I think RAC can expect some challenge of this value. A more complete argument might ultimately be needed.

6. Page 18, definition of CF_t: Is not 2.75×10^4 in units of days/70 year lifetime, rather than d y^{-1} ? This is one of many places where the risk calculation is highly unrealistic, since it assumes constant intake from the particular source of water for 70 years and the assumption of a constant contaminant concentration during that entire time.

   RAC Response: *Yes, CF_t should represent time of exposure; that has been corrected. In addition, for our analysis the exposure time has been modified to a seven-year exposure period (2.55×10^4 d) to agree with the air pathway approach and assumptions.*

7. Page 19, Table 3: I'm surprised that ^{58}Co didn't emerge here from the screening, given the maximum value of 76,300 pCi l^{-1} from Table 1. Is radioactive decay accounted for? It does not appear so.

   RAC Response: *Some of the values were incorrectly copied from the spreadsheets when Table 1 was generated for the draft report. We apologize for that mistake and it has been corrected. The maximum value measured for ^{58}Co was 1.4 pCi L^{-1}.*

8. Page 21, 2nd equation: Unless I made a mistake, I think that CF_c should be in the denominator, not the numerator. Otherwise, the dimensional units don't seem to work out.

   RAC Response: *The reviewer is correct and that change has been made.*

9. Page 22, line 8: I suspect most readers will need an explanation of the quantity RfC.

   RAC Response: *The term has been defined and the text has been revised for clarity.*

10. Page 22, bullet, line 1: "almost 200" what? Samples? Chemicals?
    RAC Response: *The text has been revised for clarity.*

11. Page 27, 7th line of text: Is there some scientific basis for the 0.2 g d^{-1}, other than "has been used previously"?
    RAC Response: *The value represents a review of the literature conducted for another study. Text has been added to explain this more thoroughly.*

12. Page 29, last bullet: Are there any pre-fire ER sediment and water data?

    RAC Response: *There were some data; the text has been revised.*

13. Page 30, Table 8: There are two footnotes labeled "c". I assume the last one is "d". Also, can sample sizes be given for the "yes" background locations?

    RAC Response: *The footnote has been changed; the columns with the yes/no designation for background locations have been deleted because of location naming problems.*

14. Pages 33 & 34, Tables 10 & 11: Again, it would help a lot to know if the water was filtered prior to assay, and it would help to know the range, standard deviation and time period of the sampling.

    RAC Response: *The values in these tables do not distinguish between filtered and unfiltered samples. We recognize that concentrations may be different for filtered and unfiltered samples but for this task we were looking at these data in a qualitative way to identify radionuclides and locations that clearly indicated higher values than background. Providing this type of detail for this report was not feasible. Additional review of monitoring data for contaminants identified as most important in terms of potential risk will be incorporated into future reports.*

15. Pages 37-39, Figs. 8-10: Since the time period of sampling post-fire is so short compared to the time period of sampling pre-fire, the comparison of pre- to post- fire data depends heavily on the time period used pre-fire. The reader needs to know what sort of statistical techniques were, or can be, used to make the comparisons in an objective manner. There may be several time series analysis techniques available. But even so, there is still the problem of possible confounding variables besides the fire per-se.

    RAC Response: *Our focus for this task was to identify locations or contaminants that suggested the possibility of being impacted by the fire, specifically with regard to higher measured concentrations. We realize the limitations of these data analyses but we did this in a qualitative way for this first stage of our risk assessment by plotting the monitoring data temporally and spatially. We agree with the reviewer that an assessment of incremental post-fire risks based on monitoring data alone is confounded by a number of variables, many of which could not be considered within the resources available for this particular subtask.*

16. Page 41, Table 13: The units and n need to be provided. Also, does the Year 2000 data include both pre- and post-fire data? Here again, these comparisons may not be meaningful in terms of understanding what effects the fire actually caused. The statement about temporal trends for Pu, Am and Sr do not seem to agree with Fig. 11. What am I missing?

RAC Response: *The units have been added and the data source designation "Year 2000" has been changed to "Post-fire". The number of samples for each category has also been included.*

The text at the bottom of page 41 was revised somewhat to indicate that the data cannot be used to make firm statements about temporal trends for some of the radionuclides. Figure 11 was deleted and data from Table 13, which is more complete statistically, provides the basis for the summary remarks. The monitoring data alone are insufficient to understand the complete process. Based on our cursory review, we can see some indications of increased concentrations for some contaminants following the fire, but whether this is related to the fire itself or LANL cannot be determined from the monitoring data alone. The monitoring data will be revisited once we get farther along with the modeling to see which contaminants may pose the greatest risk.

17. Page 48, text lines 8-9: This statement is quite arguable. Contaminants in stream sediments can show very complex patterns because particles are periodically non-static, depending on size, density and shape, and can be spatially much more heterogeneous than water in terms of concentrations over space and time. I suggest deletion of the sentence.

    RAC Response: *We agree that this statement is probably not defensible and will delete it from this paragraph.*

18. Page 48, second paragraph, lines 3-5. Something about the nature of the sampling sites should be provided. For example, were the ash samples from aquatic or terrestrial sites? Were samples taken in quiet depressions or in areas subject to heavy erosion? These details can make a huge difference in how the data are interpreted.

    RAC Response: *Unfortunately, detailed information of this nature regarding the collected samples is not available. We agree that this would aid in interpreting the data.*

19. Page 48, second paragraph, last line: If only positive, detectable results were used to compute average concentrations, then the averages are likely to be biased, perhaps heavily biased, on the high side. Is this the intent?

    RAC Response: *For this part of the analysis, to be conservative and because of difficulties associated with calculating averages including nondetectable values, we elected to focus on detectable and positive results. So, yes, this was the intent. We agree that this likely biases the averages on the high side. For final risk calculations, the existence of nondetectable results will be important to consider.*

20. Page 51, Fig. 20: It sounds like locations 54-56 need to be moved on the map.

RAC Response: *The locations for 54-56 are actually correct, and location 57 is misplaced. This will be corrected in the final report.*

21. Page 53, Table 15: Are all samples here "ash and muck"? This is not clear. Indicate media in each case.

    RAC Response: *The reviewer is correct in stating that the media designation is not clear, and we will indicate the media for each set of samples.*

22. Page 53, Table 16: Again, the media measured are not clear, nor does the reader get an idea of what the sampling areas are like. Can this be indicated in the caption or in footnotes?

    RAC Response: *The data presented in Table 16 are compiled from Tables 14 and 15, and the media for those data are indicated in the referenced tables. The purpose of compiling these data in this fashion was to attempt to understand the general impact of fires on concentrations in sediment. Unfortunately, the data available for this purpose represent both soil and sediment, as well as ash and muck. In some cases, samples are indicated to be soil in one field in the database and sediment in another field. Because of limited available data and some uncertainty regarding the actual composition of a given sample, we attempted to draw some general conclusions, based on the data we did have. We agree that additional detail regarding the specifics of the sampling areas would be helpful, but that information is not readily available to our knowledge.*

23. Page 54, second paragraph, line 3: Should "Figure 24" be "Figure 21"?

    RAC Response: *Yes, Figure 21 was incorrectly referenced as Figure 24. This will be corrected in the final report.*

24. Page 55, Fig. 21: The sampling point symbols are a bit hard to distinguish from one another. Also, does 2000 in the legend mean pre- or post-fire, or both? Also, this and the other basic maps would be more understandable if the stream courses were shown.

    RAC Response: *We realize the symbols are difficult to distinguish, but this is a result of the close proximity of many sampling locations and limitations with regard to making symbols clearly visible. The symbols show locations sampled at some point during the year 2000, with those sampled both pre- and post-fire shown by the inclusion of black circles around the symbol.*

    We agree that stream courses would assist with interpreting these figures. The GIS coverages with which we were provided did not include a simplified stream channel coverage that could be used for this purpose. We are attempting to generate a shapefile coverage and will incorporate this where possible in subsequent reports.

25. Page 57, 4th paragraph: I have a hard time reaching this conclusion, based on Figs. 24-26.

    RAC Response: *This statement refers to the data in Table 16, which suggest that the concentrations of these radionuclides may have increased as a result of the presence of a fire. It does not refer to interpretations of the data presented in Figures 24 through 28. We will revise the text accordingly.*

26. Page 57: The text in the bottom half of this page is very confusing to me.

    RAC Response: *These paragraphs make general statements about the trends apparent in Figures 24-28. The data presented in those figures compares measurement data for the 3 general locations indicated in the figures to the range of average concentrations measured in fire-impacted areas assumed to be removed from LANL operational impact. Based on a visual examination of the data, both Pu-239,240 and Cs-137 concentrations appear to have increased following the fire, but not to levels above those measured in soil and sediment collected at fire-impacted locations assumed to be removed from LANL operational impact and also not to levels above those measured at various times before the fire. Plutonium-238 concentrations, on the other hand, appear to have increased to levels exceeding those measured in fire impacted areas assumed to be removed from LANL operational impact. Both Sr-90 and Am-241 show marked decreases in concentration immediately following the fire, an explanation for which is not apparent.*

    Concentrations reported historically before the fire for these radionuclides at below LANL and downriver locations are in several cases elevated above what would be expected, based on a comparison to background concentrations (reported in Table 14). This suggests some degree of potential LANL contribution to these higher concentrations. Certainly, since these higher values are in the pre-fire time period, this is unrelated to the fire. However, we felt it was appropriate to make statements about the existence of these higher values. We will try to clarify the text in the final report to better state our observations.

27. Page 59, Fig. 24: I don't understand the locations represented by the two horizontal red-dashed lines. It says that it is a range of concentrations in fire-impacted areas not influenced by LANL. Are not areas above LANL and post-fire (green x s) in this category? If so, there is a green x outside this range. Also, it would be helpful to see the x-axis expanded by perhaps only including data 4-5 years pre-fire, in addition to all the post-fire data.

    RAC Response: *This is the range of average concentrations for fire impacted areas, based on data presented in Table 16. The general designations of areas above LANL, below LANL, and downriver are based on the locations shown in Figure 23 and further described at the top of page 57. The reviewer is correct in*

that above LANL locations are likely removed from LANL impact; and the post-fire data for this general location would also fall into the fire-impacted category. These data are compared to the range of average concentrations in Table 16 and are not part of the data shown in Table 16. The purpose of these comparisons is to determine if there are any apparent differences between concentrations measured at the 3 general locations and any apparent differences between pre- and post-fire concentrations. Expansion of the x-axis would assist with discerning between the different values, but the primary purpose of the figures was to look at possible post-fire changes and the relationship of concentrations to those measured in fire-impacted areas, and this is possible using the current figures.

28. Page 62, Fig. 29 (and similar Figs. on other pages): The red-circled symbols indicate an increase over average pre-fire concentrations. Are the increases statistically significant? Which pre-fire concentrations were used? Again, many different pre-fire concentrations could be calculated, depending on the length of the sampling period considered, perhaps the seasons, and other factors.

    RAC Response: *The red circles indicate only where an increase of pre-fire concentrations was observed, and tests have not been performed to assess whether or not the increases are statistically significant. The point was to look at the data in a very broad sense to determine if any readily apparent changes are evident and if such changes are localized in certain areas. The pre-fire concentrations represent the average of all pre-fire reported concentrations for a given location. The reviewer is correct in stating that this could be done in various ways; our approach was to calculate an average pre-fire concentration in the most simplistic fashion, i.e., using all of the available data..*

29. Page 65, Fig. 31: The caption says the "size" of the symbol is proportional to the reported concentrations. Does "size" mean area, or diameter? Does "fire-impacted" in the caption mean no LANL influence?

    RAC Response: *Size refers to the diameter of the symbol. As indicated in Table 16, fire-impacted refers to locations presumed to be removed from LANL influence. We will try to clarify these issues in the final report.*

30. Page 70, Table 17: What does "U" mean in the table? Undetectable perhaps? What are the numbers in parentheses after the U symbols?

    RAC Response: *Yes, "U" refers to results reported as "nondetectable". The numbers in parentheses represent the values reported as nondetectable, which are the detection limits reported for specific samples. Different detection limits were reported for a number of analytes, and it is assumed this is because the detection limit is sample specific. We will add footnotes to clarify the content of this table.*

31. Page 71, Fig. 36: I wonder if the Al data simply reflect the amount of total suspended solids in the samples, or whether the data reflect Al contamination from LANL?

    RAC Response: *Yes, it is quite possible that the Al data storm water data reflect the amount of suspended solids in the samples.*

32. Page 74, bullets: Concerning recommendations for further monitoring, I think it important to get more post-fire data, over 2-3 years and in all seasons if possible, in order to strengthen the post-fire data base and to permit more robust statistical analyses. Methods of sampling and analysis, season, watershed and stream conditions need to be strictly comparable to pre-fire data if such further monitoring is to be useful in assessing the effects of the fire.

    RAC Response: *We agree that this would significantly strengthen the conclusions that can be drawn based on the monitoring data. However, the timeframe available for completing this project does not allow inclusion of additional data. We will add text to indicate that additional post-fire samples are critical to further understand the implications of the fire.*

33. Page 75 bullets: I would agree with the first bullet on p. 75, only if other extraneous environmental variables can be accounted for. I am not in a position to either agree or disagree with the next three bullets. I think a more careful and robust analysis is required to reach these conclusions. I am not sure whether this is possible or not with the available data. The last bullet might also benefit from somewhat more judicious wording at this stage of the analysis.

    RAC Response: We agree that extraneous environmental variables are difficult to account for and complicate this analysis. The conclusions we attempted to make were designed to draw attention to the possibility of increases or decreases. We agree that these conclusions are not based on statistical tests and have not included them to suggest that there is clear evidence for the statements that are made and will revise the final report accordingly. We do believe that there is enough uncertainty in the apparent trends that additional sampling at specific locations would enhance the ability to make more conclusive statements about LANL impact, as stated in the last bullet. In addition, this cursory review of spatial and temporal trends has helped focus efforts in subsequent reports.

Response to comments on RAC Deliverable 2.3
GIS-Based Evaluation of Surface Water Runoff and Erosion

NMED Comments

Refer to river as Rio Grande not Rio Grande River

> ***RAC Response:*** *This change has been made.*

Review of RAC Deliverable 2.3, GIS-Based Evaluation of Surface Water Runoff and Erosion, 10/5/01 (KO rev.2.3.doc)

In Table 12, there doesn't seem to be a consistent relationship between max flow measured at gage and the grid estimate of storm flow. Perhaps a comparison to mean flow measured values would show a better correlation.

> ***RAC Response:*** *The maximum flow measured at a gage location was provided as a comparison to the grid estimates values, which are representative of the maximum potential flow for the watershed. It was not anticipated to determine or demonstrate a correlation between these values for the reasons discussed in the text of associated with the table. The intent was to show that the grid estimated values were generally greater than the maximum measured flow values.*

Will the 10 fold increase in sediment concentrations mentioned in the conclusions as being the estimate for the model be assumed in the modeling to come from all over the watershed (so that only a proportion is modeled as coming from the PRSs) or will the model assume all sediment comes from the PRSs?

> ***RAC Response:*** *Yes, sediment will be proportioned based on the size of the PRS.*

(RFS rev.doc)

Page 5, second paragraph – NMED has Suspended Sediment Concentrations (reported by NMED as Total Suspended Load) collected during four stormwater events at the USGS Rio Grande White Rock Gage and five stormwater events at LANL gage stations that can be correlated with rainfall, stormwater flow and SSC. These were originally reported as wet weight but we are working with our lab to report them as dry weight measurements. These data should be used to enhance the data set used.

> ***RAC Response:*** *When updated data are received, they will be reviewed and incorporated into subsequent evaluations as appropriate. The timing of the receipt of the data will determine how and when it is incorporated into the evaluation.*

Page 6, bullet 7 - As per the U.S.G.S. Office Of Surface Water Technical Memorandum No. 2001.03, November, 2000, "Results of the Total Suspended Solids (TSS) analytical method tend to produce data that are negatively biased by 25 to 34 percent with respect to Suspended Sediment Concentration (SSC) analyses collected at the same time and can

vary widely at different flows at a given site. The biased TSS data can result in errors in load computations of several orders of magnitude."
TSS measurements may not be appropriate measurements for developing representative concentrations of sediment in surface water unless a discussion of the relative magnitude and the direction of the uncertainties are included.

RAC Response: *A discussion of uncertainties is included in the final report*

Page 7, second paragraph - Suggested change in wording from "We merged 16 DEMs" to "We merged 16 –7.5 minute DEMs".

RAC Response: *Text changes have been made.*

Page 9, Figures 4 & 5 – It appears that Mortandad 1 refers to Sandia Canyon and Water 2 refers to Ancho Canyon. It would be less confusing if you changed the names of Mortandad 1 and Water 2 to their respective watershed names.

RAC Response: *Text changes have been made.*

Page 20, Last paragraph - While gage data at the eastern boundary of LANL may indicate ephemeral conditions, perennial conditions exist on LANL property in Pajarito Canyon (approximately 30 –40gpm), Cañon de Valle (approximately 12-14 gpm) and Sandia Canyon (225 – 400gpm NPDES effluent) upstream from east-boundary gage stations. Base flow should not be considered zero unless these flows are considered insignificant.

RAC Response: *Any base flow was assumed to be the same for both pre-fire and post-fire and specifically limited to the stream segments. On this basis, surface water flow associated with base flow would not be anticipated to flow over a PRS. In addition, since the gaging at the eastern boundary of LANL indicates ephemeral conditions, it was also assumed that any up gradient base flow was lost prior to reaching the outlets at the Rio Grande. Therefore, for purposes of estimating maximum storm water flow, base flow was considered to be insignificant.*

Page 25, Second paragraph - In most instances, ESH-18 surface water data does not correlate with stormwater events and therefore may not be appropriate for stormwater sediment concentration modeling. They would tend to bias low any estimates if they are not differentiated from stormwater.

RAC Response: *The flow data were not used directly in the calculations and only used for comparison purposes. In addition, the ESH-18 data sets are generally small for each watershed and since surface water flow in the canyons is generally related to a storm event, the data were assumed to be pertinent. Given all of the constraints and availability of data, these data were considered.*

Page 29, Table 7 - The estimates of flow for the 2 year & 5 year events for Mortandad 1 (35 X and 20X pre-fire) may over estimate the increases in flow due to the Cerro Grande

fire if Mortandad 1 represents the Sandia Canyon watershed. Sandia Canyon stormwater flow is usually in response to rainfall on Technical Area 1, an area with a high density of impermeable surfaces, and the rest of Sandia Canyon's watershed received a relatively low intensity burn.

> ***RAC Response:*** *Pre-fire and post-fire storm water flow was based on the curve numbers provided in grid files by ESH. These curve numbers showed an average increase in the curve number from 58 to 66 indicating an increase in runoff due to the fire. The primary change in curve numbers occurred in the portions of the canyon within the boundaries of the LANL facility even though the burn area was limited within this canyon. A decision was made to accept these curve numbers as provided by ESH since there was not other data and a limited amount of time available to modify or generate new curve number data.*

If Water 2 represents Ancho Canyon, there should little or no increase in stormwater flows as very little (if any) of Ancho Canyon watershed burned.

> ***RAC Response:*** *The change in pre-fire and post-fire storm water flow for Ancho Canyon is generally small; however there is a change because of changes in the pre-fire and post-fire curve numbers. Pre-fire and post-fire storm water flow was based on the curve numbers provided in grid files by ESH. These curve numbers showed an average increase in the curve number from 60 to 63 indicating an increase in runoff due to the fire. The primary change in curve numbers occurred in the southern portion of the canyon. A decision was made to accept these curve numbers as provided by ESH since there was not other data and a limited amount of time available to modify or generate new curve number data.*

Page 32, first paragraph; & 33, Figure 15 - It appears that Figure 15 refers to Water 1, not Water 2, as stated in the paragraph on page 32. This would be more logical as Water 1's watershed has consistently produced flow in response to rain events (Post- & Pre-fire) where as Water 2 (Ancho) does not usually flow and was not burned.

> ***RAC Response:*** *Change has been made*

Page 34, first paragraph: - RAWS (rain gage) data at http://www.wrcc.dri.edu/losalamos/ should be utilized also.

> ***RAC Response:*** *RAWS data was reviewed; however, data was not available for all months corresponding to the data provided in the LANL precipitation data. Selected available precipitation data was reviewed for several RAWS gages (i.e., Pajarito, Water, Pueblo) for comparison with the LANL precipitation data. Generally these data were consistent with the data collected by LANL. The precipitation data were used in this section of the report to identify the appropriate storm events for purposes of comparing the estimated storm water flow to the available empirical data. Extensive evaluation of the RAWS data was not needed to accomplish this purpose.*

Page 44, First partial paragraph - Please define EMC.

> ***RAC Response:*** *EMC is defined as Event Mean Concentration on Page 26 of the document.*

Page 45, Figure 22 - The title of Figure 22 should be Suspended Sediment Rating Curve as these values do not include bedload sediment concentrations and may be mistaken for Total sediment concentration.

> ***RAC Response:*** *Change has been made.*

External Peer Review

Review of Task 2.3: GIS-based Evaluation of Surface Water Runoff and Erosion for the Cerro Grande Fire, October 1, 2001, By T. E. Hakonson, Submitted November 7, 2001 (Hakonson on 2.3Review.doc)

My major concern with the report stems from the authors conclusions that the modeling results are "conservative". They rightly state that comparing modeling results with actual measured flows is not entirely appropriate and give several valid and good reasons why the comparison is not valid. However, given that, they then conclude that the comparison does show that the modeling results are conservative, i.e. that it over-predicts runoff compared to actual measurements.

I do not think the data in Tables 10 (precipitation), 11 (pre-fire runoff) and 12 (post-fire runoff) on model predicted and measured runoff show conclusively that the model results are conservative. For example, Table 10 shows that the model estimates of precipitation were higher than that measured in just 50% of the comparisons while measured precipitation exceeded predicted in the other half of the comparisons.

Likewise, predicted pre-fire runoff (Table 11) exceeded measured runoff in only 9 of the 20 comparisons (I think it is legitimate to include zeros in measured data). I also don't think that predicted versus measured runoff ratios less than 5 could be considered outside the range of uncertainties and as indicative of model conservatism. Given that, then only 8 of the predicted pre-fire runoff estimates exceeded that measured for the 20 comparisons.

The same comment can be made about the post-fire comparisons in Table 12. Predicted post-fire runoff exceeded that measured in only 8 of the 20 comparisons when predicted versus measured runoff ratios less than 5 are consider within the range of uncertainty. I don't buy the argument that zero measured runoff indicates "blockage" of the gaging station. There could be several other explanations relating to the assumptions RAC made in setting up their model input.

This reviewer's conclusion, then, is that the data in Tables 10-12 DO NOT conclusively show that the model predictions are conservative. Furthermore, of the cases where predicted runoff was at least a factor of 5 greater than measured, the conservatism ranged from 1 to 4 orders of magnitude. It seems to this reviewer that the degree of conservatism, when it exists, spans a very large range that likely cannot be specified. It would seem that this would complicate assigning uncertainties to the risk analysis.

As I mentioned, the authors clearly discuss limitations of the pre-fire versus post-fire runoff comparisons. Given those limitations and the concerns I raised about the "conservatism" of model predictions, I think RAC should give a more detailed discussion of what conservative means, how conservatism and it's associated uncertainties will be specified, and how it will be demonstrated that the risk estimates are really conservative.

I like what *RAC* did with the sediment yield analysis since it is based on the measured data. It is a bit troubling that suspended sediment was not related to runoff flow since this relationship would seem to be required to couple runoff and sediment yield for estimating contaminant transport in the canyons at Los Alamos. I recognize, however, that more work on this aspect of the LANL risk assessment is needed as stated by the authors.

My final comment is that the author/s of this report appears to be well qualified on the subject of modeling runoff processes in watersheds. The report is very well done. I was impressed with the attention they paid to limitations of the modeling and data. I do think that the case for demonstrating that the runoff results are "conservative" has not been made and that this section of the report needs more attention.

> ***RAC Response:*** *It is very difficult to make a direct comparison between the modeled surface water flow and the empirical surface water flow data. The points selected for comparison were based on the availability of data NOT the most appropriate locations to make comparisons. Ideally, the outlet points of the watershed would have been the place to make a comparison between modeled storm water flow estimates and empirical data. However, there were no empirical data available at these points. As a result, sample points with sufficient empirical data were selected recognizing that there were a number of confounding factors that would impact any comparison. Many of these are discussed in the report as they specifically relate to particular sampling point. In addition, it is important to realize that the modeling effort did not take into account any surface anomalies, base flows or other features that may impact the empirical data and not be included in the surface water flow model. In addition, there were no data available to identify a particular empirical storm water flow value to a particular rainfall event.*
>
> *There fore, based on the available empirical data and the confounding factors discussed above and in the report, we believe that the model estimates for storm water flow are conservative. Also, as was pointed out in the introduction of the report, the modeling is not intended to yield a definitive calculation of surface water flow and sediment concentration within the watersheds; rather, to conservatively represent the relative relationship between pre-fire and post-fire surface water flow and sediment concentrations.*

Review of Technical Memorandum for Task 2.3 submitted by Randall Charbeneau, Ph.D., P.E. (Charbeneau Review of 2.3.doc)

On Figure 2, is it possible to add an overlay to show the extent of the burn area?

> ***RAC Response:*** *The burn area has been added to the figure*

The precipitation model consists of rainfall depth estimates for the 2-, 5-, 10-, 25-, 50-, 100-, and 500-year return period 6-hour duration events. Precipitation estimates vary with elevation, and the linear regression approach to this relationship is appropriate. Further discussion on selection the of a 6-hour duration is warranted. Presumably, the rainfall depths were derived from intensity-duration-frequency (IDF) relationships that have been developed for this area. Shorter-duration events have greater rainfall intensity (inches-per-hour) but less rainfall depths (inches) than longer-duration events. In rainfall-runoff modeling, selection of the design storm duration depends on whether the modeling objective deals with estimation of peak flows or runoff volumes. Given the

objective of this risk assessment, peak flows and erosion rates are important. Storm duration is estimated based on the time of concentration for the watershed areas. If 6 hours is the estimated time of concentration for the watershed, then this should be noted.

> ***RAC Response:*** *The rainfall depths were obtained from work performed by McLin. McLin used a 6-hour storm event based on the US Army Corps of Engineer recommendation that a 6-hour storm event be used for the HEC-1 flood simulation for Northern New Mexico. IDF relationships were developed for the area and used to calculate the 6-hour design storm distributions.*

The model for routing stormwater runoff is very conservative. There are methods to estimate and incorporate watershed travel time within a GIS environment that may yield more realistic estimates of stormwater discharge. This issue is recognized in the memorandum and clearly demonstrated in Figure 8. Use of the SCS Curve Number method to calculate runoff depth from precipitation is appropriate, as is use of HEC-HMS for benchmarking the model results. Standard parameter estimation methods are described and used. Tables 2 and 3 need to be modified (and corrected). First, a reasonable number of significant figures should be used. In Table 2, the labels "Post-fire t_l" and "Pre-fire t_l" need to be switched, and the same goes for the time of concentration labels. This should result in the pre-fire times being longer than the post-fire times. Finally, in Tables 2 and 3, it would appear that S_l is the watershed slope in ft/ft, rather than %. That is, the first value in Table 2 should be 7%.

> ***RAC Response:*** *The lag time and time of concentration in Table 2 were mislabeled and have been corrected. The slope in Table 2 and Table 3 has been modified to be presented as a percentage.*

In the results section, the results and comparisons for stormwater flows look reasonable, though on my copy, Figure 13 is not very helpful (difficult to see what point is being made). In Table 7 (and 9, 11, 12, 13, and 14), use an appropriate number of significant figures. The results presented in Table 8 and Figure 14 may be anticipated. The change caused by the fire is reflected in an increase in the curve number that reflects abstractions. For smaller storms, this change in abstractions is significant. However, for larger storms, the rainfall overwhelms the abstractions, and the resulting peak flow becomes a function of the watershed size and topography. The representative TSS concentrations that were found for pre- and post-fire conditions appear reasonable, as does the finding that TSS concentrations are independent of area. These concentrations likely reflect the carrying capacity of the channel system.

> ***RAC Response:*** *The size of the figure was made as large as possible given the constraints of space in the report.*

I do suggest that the authors review the entire manuscript with a critical eye to presentation of significant figures.

RAC Response: *All numeric values are presented in scientific notation with on decimal.*

Los Alamos National Laboratory Comments

Comments submitted on marked-up copy of Task 2.3 Technical Memorandum by Everett Springer

Page 1, second paragraph. Where does resuspension of canyon sediments by flooding fit.

RAC Response: *The resuspension of canyon sediments is included in the first bullet reflecting the transport of radionuclides and chemicals via the surface water.*

Page 1, third paragraph. There is also the increased transport due to flooding.

RAC Response: *This is true and while not specifically stated in the introduction, increased transport due to flooding is considered in the surface water evaluation.*

Page 2, first bullet. A question mark is placed at the end of the second line.

RAC Response: *No explanation was provided for the question mark. No changes were made.*

Page 3, Figure 1. Need to distinguish better.

RAC Response: *The figure is intended to summarize the steps being taken, not to define the specific technical memorandums or reports that will be generated during the risk analysis.*

Page 6, second paragraph under Methodology. ????

RAC Response: *We could not read the comment.*

Page 10, Figure 5. Is Mortandad 1 actually Sandia.

RAC Response: *The Canyons have been updated to change Mortandad 1 to Sandia and Water 2 to Ancho.*

Page 11, Figure 6 and Table 1. Recommended the addition of units to the table and Edits to the figure to change "P" to estimated rainfall at station.

RAC Response: *Units have been added to Table 1. Modified figure to define "P" as Precipitation, the text describes the source of the precipitation data.*

Page 15, Figure 9 – Question mark associated with Watershed in legend.

RAC Response: *No explanation of the question mark was provided, however, the Watershed in the legend refers to the outlines of the identified watersheds shown on the figure. No changes were made.*

Page 16, Figure 10. This is difficult to interpret – better as CN postfire/CN prefire.

RAC Response: *The figure has been changed to show the curve number ratio as post-fire to pre-fire.*

Page 22, Table 2 and Table 3. Edits to table to show S_i as percent and reverse position of Pre-fire and post-fire t_i and t_c.

RAC Response: *Changes were made to the tables.*

Page 23, Table 5. Same as Table 4.

RAC Response: *Table 5 has been replaced with the correct table.*

Page 24, top paragraph. Recommended changing (Nylan, 1978) to (Nylan et. al., 1978).

RAC Response: *Changes were made*

Page 24, Table 6. These numbers (referring to pre and post-fire BAER Team) are too high in terms of tons/acre-year. Maybe they are tons/year or something like that.

RAC Response: *The units are correct.*

Page 24, last line. Change Hillslope model to Hillslope Erosion model.

RAC Response: *Changes have been made.*

Page 25, first line. Change deposition to delivery.

RAC Response: *Change has been made.*

Page 25, last paragraph. In last three sentences – I cam not sure what daily peak flow is?

RAC Response: *The daily peak flow is the maximum recorded flow for a day.*

Page 32, Figure 15. There is a large relative impact from the runoff standpoint. The other point to make is that the higher flow at the longer return periods may have a greater impact. That is adding flow may increase flood depths so that certain facilities are impacted that may not have otherwise been impacted. Or, transport capacity of streams is increased so that mores sediment is moved.

RAC Response: *We agree that these affects may occur as a result of increased flow in the canyons and have added appropriate language to the report.*

Page 34, Paragraph under Figure 16. Cannot locate gage TA8.

RAC Response: *TA8 is a typographic error. It should be TA-06. Change has been made.*

Page 38, first and second paragraphs. Tables are not correctly referenced. Table 10 should be Table 11 in first paragraph and Table 11 should be Table 12 in second paragraph.

RAC Response: *Change has been made.*

Page 42, first paragraph. There is a question mark adjacent to the term mean.

RAC Response: *No explanation was provided for the question mark. No changes were made.*

Page 47, ESRI references. Suggest changing references to 1998a and 1998b.

RAC Response: *The current designation is consistent with other designations used in the report and other RAC Technical Memorandum. No changes made.*

Comments on Task 2.4 RAC Cerro Grande Risk Assessment, Nov 4, 2001, submitted by Ken Mullen, ESH-18 (As they Apply to Task 2.3)

Page 8, There is only one drainage point for Mortandad and Water Canyons. The other drainages are probably Sandia and Ancho Canyons.

RAC Response: *Changes have been made to the text.*

Appendix A, page 4. The 100 yr flow for Water Canyon seems unusually high. I would suggest a review of that calculation and the data that were used.

RAC Response: A transcription error occurred and the wrong values were associated with this outlet. The values have been corrected.

REVIEW OF TASK 2.4: SITE CONCEPTUAL EXPOSURE MODEL FOR THE SURFACE WATER PATHWAYS

PEER REVIEW COMMENTS

F. Ward Whicker
Department of Radiological Health Sciences
Colorado State University
Fort Collins, CO 80526
October 29, 2001

General Comments

1. It is not entirely clear to me whether this analysis is focussed on past, present, or future risks from the fire. If, for example, RAC is going to try to project into the future, the watershed parameters will change over time due to colonization of the burned areas by vegetation and by human mitigation efforts to control erosion. The changes in these parameters will vary by slope and exposure, as well as the severity of the burn. Furthermore, the inventories of contaminants that can be mobilized are likely to change over time as the easily-eroded materials near the surface become depleted. While the report does say on the first page that "The primary objective of this work is to analyze the immediate consequences and the longer-term impacts of the Cerro Grande Fire---", I do not see much discussion of how the models might account for such changes over time

   RAC Response: *While the objective of the risk analysis was to analyze both the immediate consequences and longer term impacts, a screening approach rather than a detailed modeling approach was used for estimating concentrations of chemicals and radionuclides and potential associated risks at identified points of exposure. This screening approach was selected due to the limited amount of data necessary to conduct detailed modeling, the complexities of the watersheds included in the evaluation and, more importantly, the limited amount of time to conduct this work. As a result, changes over time can not be specifically incorporated in the screening models. Rather, the screening approach is intended to represent a reasonably conservative exposure scenario based on the current information, data and conditions. Applying the current conditions over the selected exposure period would result in higher exposures being predicted because any recovery in the watersheds would not be included. The results of the screening effort will identify significant immediate consequences (based on current conditions) and potential long term impacts that would require further investigation.*

2. Somewhat related to the first comment, is the question of the need for hydrologic models in the first place. Clearly, the hydrologic processes and spatial as well as temporal variations in natural phenomena affecting the processes are incredibly complex. Thus, transport models used for predicting future events, or for filling in gaps about past events, are subject to great uncertainties. It therefore seems to me that what has already taken place is best-known from the water monitoring data (flows, suspended solids loads,

contaminant concentrations, etc.). As the project unfolds, presumably more monitoring data will become available-and, new environmental sampling and monitoring that might be needed to more accurately determine health risks can be identified and recommended. I therefore sense that the greatest service this task can provide is to base risk calculations on the actual water monitoring data, and to recommend how new measurements (e.g. contaminant levels in sediments, soils, crops, fish and other media where real people can get real exposures) could generate credible exposure and risk estimates. If there is a mandate to project risks into the future, then hydrologic transport models are essential, but in that case, I think the models need to account for changes in the watershed characteristics and in source term changes.

RAC Response: *The reviewer is correct that monitoring data plays an important role in this type of analysis and would be a good source of information for estimating potential risks. However, the amount of post-fire data is limited and the majority of the available data has been collected for purposes of environmental regulatory compliance rather than risk analysis and the suitability of these data for risk analysis is limited. In addition, as discussed in the previous response, a screening approach is being used to conservatively estimate concentrations and identify significant immediate consequences and potential long term impacts that would require further investigation. The assumptions incorporated into the screening calculations were sufficiently conservative to address the complexities and uncertainties in the fate and transport of chemicals and radionuclides in the watershed. We revised the text to make this clear; additional details about the methodology are provided in the final Task 2.7 report.*

3. The watershed ranking scheme sounds reasonable at first glance. However, in thinking about it, I am not convinced that the proposed scheme will provide the intended result. For example, a high burn factor and a high flow factor will indeed lead to more contaminant movement in an absolute sense. However, this may be offset when one gets to the risk implications downstream by aqueous phase dilution (greater water volumes) and by solid phase dilutions (greater sediment volumes). Clearly, large storm-water runoff events move lots of uncontaminated sediment, especially in watersheds that do not have homogeneous distributions of contaminant levels. If most of the contamination in a watershed is already in the stream channels and most of the watershed is essentially uncontaminated, then the resulting sediments and water downstream are likely to become less contaminated from a storm event because of dilution and mixing (even though the absolute amounts moved would be greater as a result of the fire). I wonder whether just the contamination factors would be better to use for the purpose. One idea might be to test alternative ranking schemes against the monitoring data. Perhaps this would provide more solid evidence that one or the other possible approaches seems to work best.

RAC Response: *We agree with the reviewer in that it is important to consider many possibilities. For that reason we use a conservative approach for these transport processes so that we do not underestimate possible concentrations and consequently potential risks at the points of exposure.*

4. Yet another reason to question the utility of spatial hydrologic models, at least for near term risk assessment, is the problem of not having good data on concentrations and erodibility of contaminants for all contaminated areas at LANL (as noted in paragraph two of page 10 and paragraph 4 on page 11).

   RAC Response: *We agree with the reviewer and plan to address the lack of data through conservative assumptions in developing the source term and estimating the chemical mass and radionuclide activity at the points of exposure.*

5. There is one potential exposure route that should perhaps be evaluated, namely inhalation. This route is frequently dominant for relatively insoluble alpha-emitting radionuclides such as $^{239,240}Pu$. Potential scenarios might include resuspension and inhalation of exposed sediments from stream or reservoir locations during low water periods, resuspension of soil from fields irrigated with Rio Grande water, or burning of irrigated crops or shoreline vegetation.

   RAC Response: *We did not consider this pathway.*

6. The question of whether RAC will be able to demonstrate conclusively whether or not the fire led to actual health risks greater than those which might have resulted had the fire not occurred still looms in my mind. The variability in the monitoring data pre-fire, and the very limited period of time post-fire, might lead to the end result that once the variations, and their causes, are considered, it may be impossible to (a) demonstrate a statistically significant difference, and (b), even if there is a difference, to prove that the fire was causal in producing the difference. I think the same problem will be encountered with more of a modeling approach, if parameter uncertainties are considered. While I don't have an obvious solution to this problem, it should at least be acknowledged up-front so that everyone develops realistic expectations of this effort.

   RAC Response: *We are aware of these issues and tried to provide full explanations of our assumptions and approaches.*

Specific Comments

- Last sentence, page 5: The phrase: "We are using a conservative yet realistic approach— " might be confusing to readers. I would prefer either the admission that this is a screening approach designed to be conservative, or strive for a realistic and accurate assessment, with a comprehensive uncertainty analysis. The former is easier and cheaper; the latter is better, but much more difficult and time-consuming.

  RAC Response: *We revised the text to make our approach clear.*

- Second bullet, page 16: It is stated that "Many" local resident gather pinon nuts and berries from bushes near the river and reservoir. First, is there any basis for this? How

many is many? Also, I don't think pinons will grow in soil that is sometimes saturated. Therefore, pinon nuts are not a credible river-reservoir pathway in my opinion.

RAC Response: *We revised the text and deleted the sentence.*

- Table 2, page 19: I think the units for soil ingestion should be mg d^{-1}, not g d^{-1}. Also, is there no fish consumption for the White Rock hunter/fisher?
-

RAC Response: *The units have been revised. Yes, fish consumption is one of the potential pathways for the hunter and the table was revised.*

BY T. E. HAKONSON
OCTOBER 23, 2001

Overall Evaluation

This report describes a conceptual modeling approach to estimating health risks from potential contaminant transport by the surface water pathway as a result of the Cerro Grande fire. In order for me to evaluate this report and the approach RAC proposes to take in estimating incremental risk, it is necessary to state RAC goals and objectives for this task.

RAC states that specific to the surface water pathway, their objective is to estimate:

"the magnitude of incremental exposure and associated risks to the public from transport of radionuclides and chemicals associated with the LANL facility released as a result of the fire through surface water pathways. This assessment will be based on evaluating current surface water monitoring efforts, characterizing surface water runoff using Geographic Information System (GIS)-based technology with emphasis on the watersheds that cross the LANL facility, analyzing potential source terms associated with the LANL facility in the watersheds, and estimating the incremental risks and the uncertainties associated with the surface water pathway."

RAC's development of the site conceptual model considered several basic questions:

- *What are the sources of radionuclides and chemicals of concern?*
- *Where are the contaminants of concern located?*
- *In what types of environmental media are these contaminants of concern located?*
- *What mechanisms could move these radionuclides and chemicals away from the source areas?*
- *Who are the receptors and where are they located?*
- *How could people come in contact with these radionuclides and chemicals in the media?*
- *What are their activities and what are the potential routes of exposure?*

These generic questions are relevant to any assessment of contaminant movement to potential receptors regardless of how the answers to these questions are used in estimating risk. Most of the discussion RAC presents about source areas, modes of transport and potential receptor scenarios is consistent with generally held views about distribution and transport of contaminants and potential exposure scenarios.

An important problem in my opinion is their next statement which says:

"The elements of the contaminant release and transport mechanisms involve complex processes that require the use of mathematical models for calculating the behavior of the chemicals and radionuclides as they are mobilized and transported down the canyons into the Rio Grande River and for estimating exposure and dose from the chemicals and radionuclides in the surface water or sediments. Each of these complex processes and mechanisms is described mathematically to obtain estimates of risks. "

What RAC proposes here is to estimate and somehow integrate information on multiple source term areas in each of a half dozen or so watersheds, calculate transport based on an estimate of an "attenuation factor" and then sum all of this to get net transport to a point of exposure. Presumably they would compare this number to something pre-fire to get an estimate of the "incremental risk" caused by the fire although it was not apparent from the report how the modeling results will be used to demonstrate incremental risk or the lack thereof. There is certainly no discussion of the uncertainties associated with RAC's risk estimates.

In my opinion, there are many problems with RAC's proposed approach including getting a good estimate of contaminant concentrations in source areas especially when data may be unavailable and source areas represent many variations of point sources of contaminants (i.e. terrestrial firing site areas versus riparian habitat used for treated liquid industrial waste disposal and in the case of stream channels used for liquid disposal, concentrations are a strong function of distance downstream). It seems problematic to use a lumped term (i.e., attenuation factor) to integrate across soil/sediment types (mesa soils are high in clays compared to sandy stream channel sediments), liquid effluent sources of rads and chemicals will have different geochemical behavior than those in explosive test areas, and the potential for dilution of source terms with "clean" runoff or sediments will be a function of watershed size, storm intensity, duration, location, etc.

It is also not apparent from the report what Attenuation Factor (AF) represents, how it is estimated, how it is used to estimate transport, and how it is used when a particular source area has multiple contaminants or involves terrestrial versus stream channel source areas. If I understand how RAC intends to use the AF, then it has to represent a tremendous amount of lumping of variables that certainly can't be incorporated into the factor. RAC states that there are many dozens or hundreds of potential release sites (PRS) presumably requiring estimates of AF's for many chemicals and rads where transport of sediment borne radionuclides is a strong function of flow, sediment and soil characteristics, and location in the watershed. None of these variables are known for LANL or to be generous, at most are very poorly known. I

can find no discussion of how the resulting risk estimates will be used to demonstrate pre- versus post- fire risks.

Consequently, it is my opinion that because the processes are indeed very complex and generally unquantifiable, even "simple" models requiring a huge amount of lumping and guessing as RAC proposes in this report should NOT be used. The real question here is how these estimates of transport can be used to demonstrate incremental risk given that the uncertainties in the estimates cannot be quantified. RAC does not discuss the problem of uncertainties and it's impact on their ability to demonstrate pre- versus post- fire differences.

RAC states that the estimates of risk will be conservative (i.e. bounding) but it is not clear how they will ensure that the estimates will be conservative given the large and unquantifiable variability inherent in estimating and using multiple source term areas and the use of a lumped variable attenuation factor. I will acknowledge that my objections to their approach indicates that the document does not sufficiently describe what RAC proposed to do with the modeling, how it will developed to be "conservative" or how it will be used to demonstrate incremental risk.

Given my reservations with RAC's approach as presented in this report, I am not sure of a more appropriate alternative for estimating the effects of the fire on risk. However, I do feel certain that any approach to estimating incremental risk must use the existing runoff monitoring data collected pre- and post-fire during storm events. Monitoring data can be used directly as input for the exposure scenario calculations. The pre- and post- fire monitoring data is the only actual "observed effect" of the consequences of the fire, it is free of many of the biases inherent in guessing parameters for models, and it integrates all the variables upstream contributing to uncertainties in the measured data (time, distance, soils, etc) It is my opinion that the variation around the measured monitoring data, sparse as it may be, will be less (much less I think) than any approach requiring many estimates of mostly unknown, unmeasured variables such as the RAC model requires.

> ***RAC Response:*** *We agree with the reviewer that environmental monitoring data present the most straightforward and least uncertain mechanism by which to understand and quantify concentrations of chemicals and radionuclides in the environment. Certainly, though, this is based on the assumption that the right locations for understanding the consequences of chemical and radionuclide movement as a result of a given event are monitored and that the necessary analyses are undertaken to quantify the presence of all potentially risk-causing chemicals and radionuclides in the collected samples. It also must be understood and made very clear that monitoring data are only representative of conditions existing before and at the time of their collection, and they do not provide a comprehensive assessment of potential future risks. The Cerro Grande Fire provides a good example of this limitation because the most immediate post-fire assessments were based on data collected during a relatively dry monsoon season that did not produce the heavy rain events that are possible, which could lead to significantly more chemical and radionuclide movement. Given that, it was necessary to develop the transport models to understand potential future impacts.*

Specific Comments

RAC mentions the well documented fact that there may be increased mobilization of fallout Cs and presumably Pu due to fire alone. If this is the case for the LANL watersheds, then the obvious question is how RAC intends to distinguish between or account for this fallout radioactivity versus the contributions from PRS's since Cs-137 and Pu at the canyon outfalls into the Rio Grande will be indistinguishable as to the source area.

> ***RAC Response:*** *We used data from background locations and from areas distant from LANL but impacted by wildfires to help make these determinations.*

Grazing by cattle occurs regularly in lower Los Alamos Canyon and irregularly in several canyons by cattle that intrude along the east boundary of DOE land. In addition Cochiti Pueblo has talked about cattle grazing in Mortandad Canyon upstream from where it crosses State Road 4 (a few hundred meters from the liquid effluent contamination plume). These realities might be considered as possible potential exposure scenarios.

> ***RAC Response:*** *Ingestion of beef from cattle grazing near LANL is included as a potential exposure pathway.*

RAC states, *"We are using a conservative yet realistic approach throughout the process of estimating the incremental risks related to exposure to contaminants in surface water associated with changes in the watersheds as a result of the fire. The incremental risk estimates will be based on bounding calculations and some estimates of uncertainties."* Maybe so, but I see nothing in the report that supports these statements.

> ***RAC Response:*** *As we developed the details of our methodology, we discussed the uncertainties and source of conservatism.*

Table A4 lists Mortandad Canyon watershed size as 52,000,000+ m^2. Given that this is greater than the Los Alamos Canyon watershed (Table A-4), I think something is wrong with the Mortandad watershed size. I think the watershed area of Mortandad Canyon is about 1/10-1/5 of LA Canyon.

> ***RAC Response:*** *The designation of watersheds and canyons can be confusing and is referred to differently depending on the source of the information. Areas listed in Table A-4 are based on a GIS coverage provided by LANL named bigsheds_sdl.shp. In some cases, what is referred to as Los Alamos Canyon includes Pueblo, Rendija, and Guaje watersheds. Inour coverage the Los Alamos watershed does not include these other watersheds and is similar in size to the Mortandad watershed. However, the confusion may result from the identification of Rendija, Guave and Pueblo watersheds separately. The sum of the areas of Los Alamos, Rendija, Guave, and Pueblo watersheds in Table A-4 is the area of Los Alamos Canyon as referred to by the reviewer.*

It is not clear how RAC developed the average concentrations of Cs-137 and Pu-239 for the respective watersheds. In the Los Alamos Canyon watershed (and the Pueblo Canyon branch), virtually all of the radionuclide contamination is restricted to selected small portions of the stream channel/s. Using an average over the entire watershed would, I think, grossly overestimate the rad concentration in the whole watershed and, by extension, the risk. The point is it is not clear to me how RAC applied "average" rad concentrations to rank the watersheds.

Since procedures for the ranking system are not described, I don't believe the results of RAC's rankings. Particularly since it was based on CS –137 and Pu-239 concentrations. For example, in Water Canyon these two radionuclides have to be at fallout levels as I am not aware of a LANL source of Cs or Pu on that watershed. I want to reassure RAC that there is a ton of data on Pu in Pueblo canyon dating back to at least the early 1970's.

I am also pretty sure that Mortandad Canyon has by far and away the largest inventory of residual rads in sediments of any of the watersheds (including Los Alamos and Pueblo Canyons which have flushed to the Rio Grande yearly since they first received liquid waste in the late 1940's and are therefore relatively "clean") with the exception of depleted uranium (roughly 100,000 kg) on the Water and Pajarito Canyon watersheds. It seems to me that the size of the watershed may not be nearly as important as the inventory of contaminants that could be potentially mobilized. This means I would NOT weigh burn factor, watershed size and contaminant source equally. For example, while only a relatively small part of the Mortandad Canyon watershed was burned, most of the upper end of this watershed is asphalt roads and parking lots and flat roof technical areas that generate very large amounts of runoff during the convective thunderstorms that are typical of Los Alamos.

> ***RAC Response:*** *This ranking procedure was not developed to be a detailed assessment. Rather, it was used to focus our subsequent data gathering efforts. The surface cover for all of the watersheds was modeled using available curve number data and the SCS runoff method and available information for each watershed that we considered potentially relevant for understanding its relative importance with regard to risk related to the fire.*

In general, the report is well organized and well edited with just a few typos. My problem with the report is that I think the approach proposed by RAC is unworkable and has very little potential to demonstrate one way or the other the effects of the fire on contaminant transport. I will admit that a much better description of their intentions for the modeling and risk assessment may change my mind about the veracity of their approach. However, I strongly believe that RAC must tie their risk assessment approach to the monitoring data since it is the only actual measure of what the fire caused in the way of contaminant transport. Anything else is a guess that has little likelihood of being verifiable.

LANL Comments

Comments on Task 2.4 RAC Cerro Grande Risk Assessment
Nov 4, 2001

Ken Mullen, ESH-18

Page 8, There is only one drainage point for Mortandad and Water Canyons. The other drainages are probably Sandia and Ancho Canyons.

> ***RAC Response:*** *The reviewer is correct and the changes have been made.*

Page 10 under "Source Areas" discusses a lack of post-fire monitoring data with the suggestion that no definitive conclusions can be drawn. I suspect that this comment is included because many results were below detection. As I stated in a comment on an earlier report there is a significant difference between no data and less than DL data. More specific suggestions from RAC would be appreciated. Thoughts about data that could be collected to enhance future evaluations would be most appreciated. The sooner this information is communicated to us the sooner we would be able to act on the information to get appropriate data.

> ***RAC Response:*** *We agree that there is a difference between no data and values reported as less than the detection limit. A result reported as less than detectable indicates that the true value lies somewhere between zero and the value reported. While this can be valuable in understanding the relative magnitude of contaminant levels, the implications of nondetect values with regard to risk assessment depends on the sensitivity of the analytical capabilities for each analyte. For example, many of the chemicals with results reported as nondetects ranked high in our screening assessment (Task 2.1), suggesting that analytical capabilities for those analytes are insufficient to make conclusive statements about risk. The current Task 3 draft report makes a number of general recommendations related to data compilation that can be acted upon at any time. Our final Task 3 report contains as many specific recommendations as possible to assist with strengthening the current monitoring programs.*

Page 13- I didn't find the reference for Wilson (2000) in Reference section.

> ***RAC Response:*** *The reference is listed as Hay Wilson (2000).*

Page 13- For the average concentration assigned to source areas, I would suggest that you consider using the median concentration instead of the average because it is less sensitive to unusually high or low values in the data set.

> ***RAC Response:*** *The purpose of this calculation was to provide some quantitative mechanism for establishing the relative importance of each watershed with regard to contamination to assist with focusing modeling efforts. Because characterization data are not available for all canyons, we elected to use Cs-137 and Pu-239,240 sediment data collected by both ER and ESH-18 to provide some indication of relative levels for these two contaminants. We agree that an average value is more sensitive to outliers than a median value; however, because of the large amount of data used for these calculations, we relied on the built-in and automated totals*

*query functions in Access to calculate average values. Considering other potential uncertainties, such as the number and location of samples, we believe this calculation is sufficient to help focus our evaluation of the LANL watersheds. A number of arguments can be made for various criteria (and methods for establishing measures of th*ose criteria) that could be *used for this process, and we believe the quantitative burn, flow, and contamination factors we established for each watershed serve this purpose.*

In Appendix A, page 2 it was unclear to me how the final column, Total hydrophobic area, was calculated.

RAC Response: *The values reported in the final column were calculated by dividing the area for each watershed from the previous column by the sum of the areas for all watersheds combined. This provides a relative value (normalized to one) that can be used for comparing the watersheds. We revised the column heading to read "Total hydrophobic factor," so that it does not imply an actual area.*

Appendix A, page 3- In footnote "b" you suggest that the severely and moderately burned areas have the potential to increase mobilization of contaminants. I agree when the severe and moderate burn occurred on a site where contaminants may be present. However, in the case of severe and moderate burn on the Forest Service lands I would suggest that this impact is handled by the effect of the flows on the canyon system.

RAC Response: *It is not entirely clear what is meant by the word "handled" in the reviewer's comment that "this impact is handled by the effect of the flows on the canyon system." Again, the purpose of these calculations was to help us focus our modeling efforts, and severity of burn was one of the criteria we used in this process. The severity of burn affects not only the potential mobilization of contaminants at burned contaminated areas, but it also impacts the potential runoff across those contaminated areas due to decreased infiltration of precipitation across the entire burned area of the watershed. The methodology we developed for calculating the values presented in Table A2 assumed the potential for mobilization and increased runoff was greater for moderately and severely burned areas than for low burn areas.*

Appendix A, page 4. The 100 yr flow for Water Canyon seems unusually high. I would suggest a review of that calculation and the data that were used.

RAC Response: *The modeled flow values are expected to be higher than those calculated for the LANL facility in the past, under pre-fire conditions, and lower than measured values from actual storm events. A comparison of the modeled flow values to measured data are presented in Task 2.3 and the differences are discussed in that report.*

Appendix A, page 5, Table 5a, footnote a. I think the reference should be Mohler 2000"b." In task 2.1 on page 48 it states that only positive results reported as "detected" were used to calculate average concentrations. This artificially biases the data high. The negative and non-detection results are intentional preserved for radionuclide data because using these data is the only way to accurately calculate an average or other statistics.

RAC Response: *The footnote in the Appendix tables correctly references Mohler et al. 2000a as the report that discusses the various data sets that are being used for this analysis. The averages calculated and reported in these tables differ from those calculated and reported by Mohler et al. 2000b, which evaluated spatial and temporal trends of contaminants at specific locations. A positive, detected result differs from a nondetect result in that it is a realistic estimate of the actual concentration of a given analyte in an environmental sample as opposed to an upper bound value of the concentration that could be present. While we agree that ignoring nondetect results for the purpose of calculating an average concentration biases that calculation on the high side, we do not believe inclusion of nondetect values represents the only way to evaluate trends in data. It was not possible to readily determine what fraction of reported values may have been represented by nondetect values for each contaminant and the impact that could have on our analysis. Therefore, we elected to base our analyses for both Tasks 2.1 and 2.4 only on detected results. This provides a conservative assessment of possible concentrations and is appropriate considering the uses these data served for these reports.*

Alison Dories, ER Program

1. I think it is significant that RAC arrived at the same prioritization of watersheds as the ER Project did, if only risk is considered. The ER Project had exactly the same prioritization for the short term after the fire when we consider the % headwaters burned. For the long term, ER also considers other criteria in addition to risk, but the results are quite similar.

RAC Response: *We agree that independent concurrence helps foster credibility in the results.*

2. In Task 2.4, under Information Needs for Source Areas (Page 11), it is not correct to say that there is a "lack of quantitative data to establish the relative importance of contamination levels at a given site and because LANL has not provided substantive input on their position regarding which sites may be of highest priority from a contamination standpoint." ER has provided RAC our entire prioritization database which includes risk information to the best of our knowledge. The information that LANL has is not on a site by site basis. LANL ER does its work by groups of sites that have common geography, contamination, and history, therefore it collects its information on that basis. The groups of sites can certainly be prioritized and provide information on a qualitative basis of risk importance.

RAC Response: *We revised that sentence to state, "This has been difficult because of a lack of quantitative data to establish the relative importance of contamination levels at a given site." We understand that ER has prioritized their work and provided us with that information, but the prioritization is qualitative in nature and, more importantly, has not been done on a PRS basis; therefore, it is difficult to use as part of our work*

Kirby Olsen Review of *RAC* Deliverable 2.4
SCEM for Surface Water Pathways
10/05/01

The watersheds are ranked based on the concentrations of Cesium-137 and Plutonium-239,240 that have already eroded into the sediment of the canyon channels. This is probably a reasonably good scheme for radionuclides, but doesn't necessarily account for other contaminants such as PCBs, mercury and other metals which may contribute significantly to the estimated risk. Also, this ranking scheme can't account for potential new erosion of very contaminated but previously uneroded sites as a result of the fire. The list of 24 priority PRSs mentioned in the information needs section should be reviewed against the ranking to make sure that highly contaminated sites aren't in the watersheds judged to be of lower significance.

> ***RAC Response:*** *This ranking procedure was not developed to be a detailed assessment. Rather, it was used to focus our subsequent data gathering efforts. It incorporated available information for each watershed that we considered potentially relevant for understanding its relative importance with regard to risk related to the fire. We incorporated all source areas for which characterization data were available, regardless of location.*

It seems that the assumption (p.13) that each radionuclide and chemical from a PRS is an infinite source could result in modeled concentrations in the river and reservoir requiring a mass of contaminant that exceeds the inventory from the PRSs. This would result in risk estimates that exceed the hypothetical maximum. The mass of each contaminant in the model needs to be capped at some estimate (based on average concentration and estimated volume at the PRS) of the mass from the PRSs.

I think the selection of the receptors and the exposure pathways is appropriate for evaluating the people potentially exposed to the surface water contaminants.
I can't judge the adequacy of the exposure parameters in Table 2 because some of the information needed to evaluate the values is not presented. I realize that the exposure parameters will be explained in a future technical memo. Be sure to include the following in that memo (they're missing from Table 2): The source or derivation of each factor (particularly fruit/veg and fish values)
The values for the water/sediment ingestion rates for swimming and boating
The justification for why the boating exposure is half the swimming one (p. 17)
The game meat ingestion rate for the hunter
Explanation for having both child and infant, and the reason for the difference in values for the parameters for these two receptors.

> ***RAC Response:*** *More details abut parameter values for the scenarios have been provided in subsequent reports for the project.*

Most of the values given in Table 2 are similar to commonly used values, except for the soil ingestion rate of 75 <u>grams</u> per day. Did the authors mean to indicate milligrams? In either units, this doesn't match the suggested values given on page 16. How was the 75 derived?

Daily soil ingestion is generally considered to be uniformly spread over all outdoor time, so clarify in the future memo whether the receptor spends all outdoor time in a contaminated area (that assumption may be more appropriate for the hunter/fisher or the farmer than for the Rio Grande Resident).

> ***RAC Response:*** *The units have been corrected. The text has been revised to include the source of that value and more details on our exposure assessment.*

Is filtered or unfiltered Rio Grande River water being evaluated as drinking water? Also, can you analyze this pathway separately since it is only a potential pathway at this time?

> ***RAC Response:*** *Filtered water is assumed as a drinking water source for a fraction of the drinking water for some of the scenarios.*

Response to Comments on RAC Task 2.5/2.6
Distribution and Magnitude of Chemicals and Radionuclides Transported by Surface Water and Their Concentrations at Selected Points of Exposure

LANL Comments

1. The modeled soil concentrations for many radionuclides given in appendix C are overly conservative and exceed sampling results. Sediment samples were collected at Totavi in March 2001 and analyzed for a variety of radionuclides and chemicals. The results of these analyses are available in a separate report (see below). Samples at Totavi may be compared with the modeling predictions at location 1.2 in the RAC report, since Totavi is a short distance downstream from location 1.2 and there are no intervening source areas or other channels having significant flow. The modeling predictions at location 1.2/Totavi overestimated the actual soil concentrations for Cs-137, Pu-238, Pu-239, and Am-241 for these samples by factors of 25 to 118. If the sample results, which include background concentrations, were corrected for background to compare with the net modeled results, the differences would be even more. While having conservative estimates of environmental concentrations is appropriate, these estimates seem to have more conservatism than is warranted. On the other hand, the model underestimates the concentrations of several chemicals, for example As and Ba, in the March 2001 sampling. It is not clear without more information (see comment 2 below) why this should be, and in the case of chemicals may be an effect that would be resolved by the subtraction of background to obtain the incremental concentrations. It does indicate, however, that the modeling results should be compared with measured concentrations to ensure their appropriateness. These March 2001 sampling results have been separately published in Appendix A, Tables A-1 and A-2, of Kraig et al., "Radiological and Nonradiological Effects from the Cerro Grande Fire," LA-UR-01-6868 (December 2001), which we are forwarding.

   RAC Response: *The modeled results are for design storms of various frequencies. During the storm events, concentrations are expected to be higher than the concentrations after the storms. The model is not designed to predict actual concentrations for one sampling event. While the March 2001 data are very useful and are compared to the modeled results in our Task 2.7 report, one set of point data are not definitive. Environmental sampling data have significant variability, especially for soils and sediments over small spatial and temporal scales. There are a number of factors that could be related to the over prediction of some chemicals and radionuclides and under prediction of others. Perhaps the most significant one relates to uncertainties surrounding the representativeness and completeness of source term estimates, which varies by chemical or radionuclide.*

2. In order to adequately review the calculations and modeling predictions of the water/sediment/soil concentrations, we would need to have more information than was provided. The report does not provide enough detail that the concentration predictions in Appendix C can be reproduced or checked. This needed information includes the average contaminant concentrations and area used for the source areas. Is this information available, for example on a CD, so that the concentrations for at least one point of exposure (preferably 1.2) could be calculated?

RAC Response: *All data and equations are provided with the exception of the average concentration of chemicals and radionuclides for each source area (i.e., PRS, geomorphic units, unsampled reaches, burn area), which is included as an appendix in the Task 2.7 report. A number of the input parameters (e.g., porosity, soil particle density, soil bulk density, storm duration) are provided with the equations. The area of the source areas is provided in Table B-1, Kd values are provided in Table A-2, point of exposure flow rates are provided in Table 4. A table containing the average concentrations for the source areas has been added to the appendix. A sample calculation is included as an appendix in the Task 2.7 report.*

3. Because of the complexity of the modeling, it would be helpful if the report had a sample calculation so that the reader would be certain that he/she had followed all the steps correctly. If a sample problem is included, it is recommended that it have at least two source areas so that it shows how the different flow rates are handled, and also how the sediments from clean areas and burned area are included in the model. A calculation of the Pu-239 concentrations at location 1.2 would be the most useful.

   RAC Response: *A sample calculation is included as an appendix in the Task 2.7 report.*

4. On page 14, second paragraph, and on page 16, last paragraph, the report indicates that the average concentrations at a source area were calculated only from the sample results in which a contaminant was detected, i.e., "non-detects" were not included. This could considerably bias the area averages toward higher numbers.

   RAC Response: *Because of uncertainties related to the representativeness and completeness of the characterization data utilized for source term development, it was important to develop a methodology that would help reduce the possibility of underestimating the amount of material available for release. To accomplish this, we determined it appropriate to exclude non-detect values for the purpose of source term development with the knowledge that doing so would likely bias the values on the high side.*

5. On page 18, first paragraph, first bullet, please note that because of the ephemeral nature of the streams in Los Alamos Canyon, fishing is usually not an option. However, there is fishing in the Rio Grande itself and especially in Cochiti Reservoir, so it would be appropriate to include a fishing scenario for these waters. Also, wouldn't scenario 1.2 be on the west side of the Rio Grande, rather than the east side?

   RAC Response: *The fish exposure scenarios are limited to the Rio Grande and Cochiti reservoir (i.e., scenario 1.1, 2.1, and 3.1). Scenario 1.2 is in the Los Alamos Canyon on the west side of the Rio Grande*

6. In page 22, last paragraph, it states that the total volume of sediments at scenario 3.1 results from erosion from Ancho Water watersheds, all within the Laboratory. This seems to ignore

the transport of a considerable amount of clean sediment from the Rio Grande itself, and from Guaje and Rendija Canyons.

RAC Response: *The model results for scenario 1.1, 2.1 and 3.1 have been modified to include a median suspended sediment concentration obtained from the USGS sediment database for the Rio Grande at the Otowi sampling station.*

Ken Mullen, ESH-18

1. Page 6 item 3.- I think you mean that you calculated the concentrations of chemicals by developing a value for the soil/sediment concentration and used the TSS to come up with the whole water concentration. This wasn't clear to me from the text. When I first read it I thought you meant that all the soil is mobilized in runoff. The meaning of "in the runoff water above a source area" also confuses me.

   RAC Response: *This is a correct interpretation of the approach used. The item has been reworded to clarify the approach.*

2. Was there any provision for mixing uncontaminated soils and sediments with the seds and soils that are moved off of PRSs? If I understand correctly your "model" transported soil off the PRSs to make up the estimated TSS. Were any clean soils or sediments mixed in? Presumably clean soils and seds would make up the bulk of the suspended material transported in runoff.

   RAC Response: *No specific provisions were made to mix clean suspended sediments with suspended sediments from the PRS. The suspended sediments generated by a source area were calculated based on the storm water flow across the PRS and the pre-fire/post-fire TSS concentration and used to determine the chemical mass or radionuclide activity that could result from storm water flow across the PRS. The chemical mass and radionuclide activity was assumed to remain in the storm water flow until it reached the point of exposure. At the point of exposure the chemical and radionuclide concentration was estimated based on the sum of the chemical mass and radionuclide activity from each source area contributing to the point of exposure, the storm water flow at the point of exposure and the pre-fire/post-fire TSS concentration Clean sediment and water were introduced in the model for the scenarios on the Rio Grande and the Cochiti reservoir.*

3. Page 6, last paragraph- I'm surprised that there are major portions of the Lab with only Cs-137 and Pu-239,240 data. I would like to better understand how that conclusion was arrived at.

   RAC Response: *Based on information provided in the LANL Reach reports, unsampled reaches of both Los Alamos and Pueblo Canyons comprise the majority of the total estimated inventory for both ^{137}Cs and $^{239,240}Pu$. This suggests that these stretches of the canyons may be important contributors with regard to chemical and radionuclide movement. However, the only radionuclides for which inventory estimates have been made in these stretches of canyon are ^{137}Cs and $^{239,240}Pu$. In addition, there are no data available*

to characterize the spatial distribution of chemicals or radionuclides in canyons other than Los Alamos, Pueblo, and DP Canyons.

4. Page 9 and 10- If I understand correctly the yellow dots on Figure 4 represent all the contaminated sediment source areas for Pueblo and Los Alamos Canyons that are sufficiently characterized by sampling? Please clarify.

   RAC Response: *The yellow dots in Figure represent the defined geomorphic units within Pueblo and Los Alamos Canyons for which characterization data, based on sampled sediment, exist. We did not have the time nor resources to investigate whether the characterizations are sufficient.*

5. Page 11, figure 5- A point of clarification; the yellow polygons represent *RAC*'s estimates of uncharacterized (by sampling) source term areas?

   RAC Response: *This is correct.*

6. Page 16, Concentrations in unsampled reaches were estimated based on sampled reaches. Is this appropriate? Danny?

   RAC Response: *Inventory estimates for unsampled reaches are noted in the Reach reports to be based on "...either average inventories of bounding sampled reaches or the same inventory as adjacent reach near tributary junctions." We did not have the time or resources to investigate whether this methodology is appropriate or not.*

   Because inventories only were provided, we developed a methodology to calculate average concentrations. To do this, we utilized the documented inventories for unsampled reaches in combination with average density and thickness values, based on the values associated with the sampled reaches in each canyon. Certainly, this introduces some amount of uncertainty, but it was our only available option based on available data.

7. Page 18, 19, It seems that locations 1.1 and 1.2 are not consistent between text on page 18 and map on page 19. Is the location in LA Canyon above or below the confluence with Pueblo Canyon?

   RAC Response: *There is an error in the scenario labeling. The scenario labels for 1.1 and 1.2 have been corrected in the first bullet on Page 18 to reflect the points shown in the figure. The location in Los Alamos canyon is below the confluence with Pueblo canyon.*

8. Page 20, You do not include the sediments from the Rio from above the Lab at location 1.1. Presumably, the Rio sediments will have the effect of diluting sediments from the Lab. Same for location 2.1 and 3.1.

   RAC Response: *The model results for scenario 1.1, 2.1 and 3.1 have been modified to include a median suspended sediment concentration obtained from the USGS sediment database for the Rio Grande at the Otowi sampling station.*

9. Page 22, first paragraph, Switch from location 2.1 to location 1.2 between beginning and of paragraph- I think it should all be location 2.1.

   RAC Response: *There is an error in this paragraph. The references to 1.2 have been changed to 2.1.*

10. As noted in the report, assuming the same concentrations at 3.1 and 2.1 will substantially overestimate risk and exposure at 2.1. If risk determined at 2.1 is significant this assumption and resulting risk should be corrected. Since the Rio flows all the time and the drainages off the Lab flow only periodically in response to precipitation the concentrations of chemicals in Cochiti and below will be diluted substantially by mixing with the ever-present Rio waters

    RAC Response: *Calculations have been changed to reflect TSS concentrations above LANL in the Rio Grande and dilution in the Cochiti reservoir based on volume of water and an average TSS concentration in the reservoir. The background sediment concentration in the Rio Grande is discussed above. A median volume of water based on US Army Corp of Engineers measurement data has been identified and used in the calculation of concentrations for scenario 2.1. An average suspended sediment concentration, based on the suspended sediment concentration in the Rio Grande above the reservoir and the suspended sediment concentration at the sampling station below the reservoir, has be determined and used in the calculation of suspended sediment concentrations for scenario 2.1.*

11. Page 29, Report states that the lowest value from a range of Kds was selected to be conservative. It should be noted that Kd is a function of other ions in the solution. If risks calculated are significant further investigation into appropriate Kds is warranted. Given the wide range of Kds in the literature, the specificity of Kds for soil type, the apparent changes in solubilities from the fire, and the effects of other ions in solution it would be appropriate to validate the Kds by comparing the results of this study to the results of real samples. The wide range of Kd values reported in Table A2 suggests that there may be so much variability in Kds that they are of limited value. In deed, the concentrations measured in real samples may be a more appropriate basis for a risk analysis.

    RAC Response: *We agree with the reviewer that environmental monitoring data present the most straightforward and least uncertain mechanism by which to understand and quantify concentrations of chemicals and radionuclides in the environment. Certainly, though, this is based on the assumption that the right locations for understanding the consequences of chemical and radionuclide movement as a result of a given event are monitored and that the necessary analyses are undertaken to quantify the presence of all potentially risk-causing chemicals and radionuclides in the collected samples. It also must be understood and made very clear that monitoring data are only representative of conditions existing before and at the time of their collection, and they do not provide a comprehensive assessment of potential future risks. The Cerro Grande Fire provides a good example of this limitation because the most immediate post-fire assessments were based on data collected during a relatively dry monsoon season that did not produce the*

heavy rain events that are possible, which could lead to significantly more chemical and radionuclide movement.

For a prognostic or predictive assessment, an understanding of the quantity of material available for release is necessary. In the case of this assessment, the source areas are characterized by way of soil samples. To understand how the chemicals and radionuclides may be incorporated in and dispersed by runoff water, some mechanism of soil to water partitioning is necessary, and Kd values represent the best available option for this.

12. Page 30, Site-specific Kds were calculated using LANL and NMED data. Why were the data log transformed to derive a geometric mean? Why wasn't the median or average of the initial, non-transformed data used?

    RAC Response: *Geometric mean values were derived for comparison to values reported in the literature. Quite often, concentrations of chemicals or radionuclides in environmental media can be modeled with a log-normal distribution, and values such as the arithmetic average may not be appropriate. For this project, we have used average values in a number of cases, even though other statistical values might also have been used. Because of the massive amount of data to be analyzed, the use of database queries was necessary to summarize these data in many cases. Unfortunately, such queries are more readily designed to calculate averages than they are to calculate such things as geometric mean or median values.*

13. Page 33, The report states that the concentrations in the deposited sediments at the point of exposure will be the same as the concentration of sediments transported in the storm water. We have found empirically that this is not the case. We find significantly lower concentrations in the deposited sediments. This is probably, at least in part, because our storm water sampling biases for the finer grained suspended materials and usually entirely misses the bed load. It would be appropriate to compare the concentrations developed in this report to measured concentrations of deposited sediments. It is unclear to me how this will be affected by your method of developing sediment concentrations based on PRS soil concentrations.

    RAC Response: *The model is designed to predict the maximum chemical and radionuclide concentration immediately following the storm event. These concentrations are based on saturated deposited sediments. It was not possible to include in the model any effects due to differential partitioning and transport based on particle size, even though these mechanisms may be significant. In addition, actual sampling results can vary based on sampling methodology, spatial distribution and temporal effects. It is likely and anticipated that the predicted concentrations in deposited sediments at the point of exposure will be greater than the empirical data. Comparisons of the concentration estimates to available empirical data for points in close proximity to the points of exposure are included in the Task 2.7 report.*

14. Page 36, Are the water concentrations reported in Appendix C for the dissolved phase only or are these for the whole water concentration?

    RAC Response: *The water concentrations are for the dissolved phase only or could be considered to be a filtered water sample. We have added a total water concentration to Appendix C since this concentration is used for many of the surface water exposure scenarios.*

15. In Appendix C it appears that in general concentrations decline with increasing storm intensity pre-fire while concentrations increase with increasing storm intensity post-fire. Did I get this correct and if so can you explain why?

    RAC Response: *There is not a consistent trend toward higher concentrations of chemicals and radionuclides with increased storm intensity. A general trend toward decreased concentrations of chemicals and radionuclides with increased storm intensity was observed in pre-fire and post-fire concentrations. We address this in the Task 2.7 report.*

16. It was unclear to me how or where the storm water runoff data was used in this assessment. I noted a comment at the end of the report that suggested that the results of this "model" will be compared to actual data. Is that the extent to which storm water and sediments data will be used?

    RAC Response: *The storm water flow data were used to estimate the chemical mass and radionuclide activity that would be release by each source area and the volume of water at the source areas and the points of exposure. See Equation 18 on Page 33 of the report. This same equation was used to estimate the volume of water (V_w) at each source area. The report has been modified to clarify the calculation of V_w.*

Additional LANL Comments

1) A comparison should be made of the modeled results with field measurements of water, sediment, and soil concentrations made after the fire. It is our understanding that RAC has this data (with the exception of Totavi samples collected after the July 2, 2002 storm).

   RAC Response: *Comparisons of the concentration estimates to available empirical data for points in close proximity to the points of exposure are included in the Task 2.7 report.*

2) On page 37, first paragraph, the report states that "Where the burn area was a major contributor, the total chemical mass and radionuclide activity at the point of exposure from the burn area was greater than 90% of the total mass or activity at the point of exposure." On page 16, Table 3, the Pu-239 concentration in the ash was taken to be 0.61 pCi/g. Given this, and that the burn area was a major contributor to the Los Alamos Canyon watershed (see page 21, Figure 9), it is not clear how the modeled Pu-239 concentration in sediment at location 1.2 (given in Appendix C) was 33 pCi/g, i.e., so much greater than 0.61 pCi/g.

RAC Response: *The burn area was not a major contributor to POE 1.2. The majority of the Pu-239 contributing to this point of exposure is from the geomorphic units and Unsampled reaches. These are significant contributors since they have higher concentrations and are also in areas with higher flow rates.*

3) The report mentions that the erosion matrix scores were used in calculating the releases from PRSs (for example, page 14, second to last paragraph). Please note that the erosion matrix score characterizes a PRS before best management practice (BMP) protective actions were implemented at a PRS, and so does not account for all the measures taken to prevent erosion. Including BMP protective actions would reduce the source term from these PRSs.

   RAC Response: *Our understanding is that the erosion scores do not necessarily account for the effectiveness of BMPs. We have revised our methodology to only include erosion matrix scores in cases where the score reflects the impact of the BMP. We consider it appropriate to include these scores where the effect of BMPs has been incorporated into the erosion score because our model does not address this impact. This methodology should still be considered conservative in that it does not account for non-permanent or post-fire BMPs, which would be expected to provide at least some amount of erosion mitigation. However in Task 2.7, we provide POE concentrations estimates excluding all erosion matrix scores to quantitatively assess their impact by comparison to the POE concentrations calculated when using the scores.*

4) On page 32, second to last paragraph, last sentence, the report says "...the pre-fire TSS value was selected to be 1000 mg L^{-1} and the post-fire TSS was selected to be 10,000 mg L^{-1}." This implies that 10,000 mg L^{-1} was applied to all areas after the fire, even those that were not burned. If so, this does not seem to be reasonable, as an area unaffected by the fire should have the pre-fire TSS value.

   RAC Response: *The empirical evaluation of TSS concentrations as a function of flow rate did not produce a clear functional relationship. The analysis of all pre and post fire TSS measurements indicated approximately a 10-fold increase. This was the best estimator of the fire effect available from the data. Because this 10-fold value was based on the empirical measurements taken throughout the LANL watersheds, the averaging of differences from burned and non-burned areas are already accounted for in the measurements. The actual values for TSS used in the calculations were taken from the empirical data distributions for the TSS measurements. For the screening level modeling conducted here, and because no other information was available, the erosion was assumed to be uniform throughout the watershed and driven by the increased flow rates.*

5) The report does not indicate how the pre-fire concentrations in the areas burned during the fire were taken into account.

   RAC Response: *Pre-fire concentrations were only assumed at the source areas (PRS, Reaches, Geo Units). There was no consideration for any concentrations outside of these areas pre-fire other than the exclusion of background from the source areas estimates.*

6) On page 16, Table 3, both concentrations of Ba and Cs-137 in ash greatly exceed their background values (430 vs. 136 mg/kg, and 5.6 vs. 0.33 pCi/g, respectively). The model overpredicts the Cs-137 concentrations and underpredicts the Ba concentrations at location 1.2 (taken as equivalent to Totavi). It is not clear why there should be this difference, especially in light of the statement that burned area releases dominate concentrations at the points of exposure.

   RAC Response: *Burn areas only dominate the concentrations in scenarios 4.1a and 4.1b. There could be a number of reasons for this including that we did not have characterization for all of the sources of Ba, the differences in Kd values and the concentrations of these compounds in the source areas, and the fact that the model predicts high concentrations during storm events and not long-term average concentrations in sediments on the stream banks.*

7) Concentrations in water, sediment, and soil at location 2.1 (below Cochiti Reservoir) should include the effect of Cochiti Reservoir. Otherwise location 2.1 is basically the same as location 3.1 (except for fishing). Saying that Cochiti Reservoir does not affect concentrations seems overly conservative.

   RAC Response: *Calculations have been changed to reflect sediment in the Rio Grande and dilution in the Cochiti reservoir based on the volume of water and an average TSS concentration in the reservoir.*

Randall Charbeneau

This technical memorandum documents efforts to estimate concentrations of potential surface water contaminants and constituent concentrations in deposited soils, sediments, and surface water at points of potential exposure for selected exposure scenarios. The goal of the authors is to provide a methodology for determining the relative relationship between pre-fire and post-fire concentrations, rather than providing a detailed model for making site-specific concentration estimates. Given the overall objectives and constraints, this is a reasonable goal.

Development of the calculation methodology requires a number of assumptions that are listed on pages 5 and 6. These appear to be plausible. However, I do not understand what is being implied by assumption 3. That such a condition is correct is not obvious to this reviewer, and perhaps some elucidation is called for. Even after having reviewed the entire manuscript, I do not understand the wording.

RAC Response: *This is similar to comment # 1 by Ken Mullen above. The item has been reworded to clarify the approach.*

The approach for identifying source areas and constituents of concern appears to be very reasonable for this type of analysis. Use of preliminary remediation goals (PRGs) makes a good screening cut-off for soil contaminants. Also, the use of low default K_d values is appropriate for screening risk assessments if the focus is on exposure through water pathways. The authors note

that for soil and sediment driven pathways, the selection of soil-water partition coefficients should be re-evaluated.

RAC Response: *OK*

At first glance, the modeling approach is overly conservative. Exposure concentrations are based on source concentrations and erosion rates, along with TSS levels. However, during runoff events, constituents that become part of the surface water suspended load will likely pass beyond the watersheds, or deposit within the delta of the reservoir. It is very difficult to assess the methods for calculating water and soil concentrations without knowing how these concentrations will be used in exposure calculations. The water and suspended sediment concentrations may be correct (to within a few orders of magnitude) for storm flow events, but probably should not be used for longer durations. This is especially true if recreational exposures are to be considered with Cochiti Reservoir or if fish ingestion is to be considered.

RAC Response: *The exposure period needs to be consistent with the assumptions used for the concentration calculations. It is not likely that the same concentrations would occur every time one of the calculated rainfall events occurs, so the question is how long can we expect the calculated concentrations to be present. And what are the exposure concentrations outside of the runoff events? All of these issues are addressed in the Task 2.7 report.*

With regard to source concentrations, why the 2-ft cut-off level for potential release site (PRS) concentrations? Furthermore, use of an erosion factor to adjust PRS concentrations is not consistent with the other source terms where a best estimate of the average source term concentration is used. I suggest that use of this erosion factor be eliminated, in that it does not appear to be consistent with the data and sources used for the other constituent sources. Use of a factor-of-five concentration limit for the Burned Area sources does seem reasonable. The adjustment factor for the overlapping polygons and subtraction of pre-fire concentrations are appropriate, especially since the risk assessment models are linear.

RAC Response: *Average erosion depths for all pre-fire and post-fire storm events were estimated based on the TSS concentration and storm water flow rate across the PRS. These depths ranged from 6.9E-04 meters to 3.5E-01 meters. Based on these depths we selected characterization data from samples taken between the surface and two feet below the surface as representative of the soil that could be suspended during a storm event.*

Our understanding is that the erosion scores do not necessarily account for the effectiveness of BMPs. We have revised our methodology to only include erosion matrix scores in cases where the score reflects the impact of the BMP. We consider it appropriate to include these scores where the effect of BMPs has been incorporated into the erosion score because our model does not specifically include the BMPs. This methodology should still be considered conservative in that it does not account for non-permanent or post-fire BMPs, which would be expected to provide at least some amount of erosion mitigation. However, we provide POE concentrations estimates excluding all erosion matrix scores to

quantitatively assess their impact by comparison to the POE concentrations calculated when using the scores.

The selected exposure locations include two along the Rio Grande above Cochiti Reservoir, one within Los Alamos Canyon, one below the reservoir, and two on the LANL Site. These locations and exposure scenarios appear reasonable. The model description suggests that selection of locations within stream segments "where the highest storm water flow and sediment concentrations would be expected" is conservative. The reason for this expectation is not clear, and indeed the model results show a decreasing sediment/soil concentration with increasing storm flow.

RAC Response: *Based on the storm water flow modeling, as should be expected, the highest storm water flows are predicted within the defined stream segments. Since the mass of sediment and therefore, the mass of chemical and radionuclides is a function of the storm water flow, the highest concentrations were also expected in these stream segments. The concentrations at the selected locations are expected to be higher on a local level than the concentrations in sediment and storm water in other near-by locations within the watershed.*

It would appear that use of the same concentrations for location 2.1 below the reservoir as for location 3.1 immediately below LANL property is too conservative, especially since the scenario includes recreational activities centered on the reservoir. Certainly the soil concentrations cannot be the same, since most of the suspended sediment would be deposited in the reservoir.

RAC Response: *Calculations have been changed to reflect sediment concentrations in the Rio Grande and dilution in the Cochiti Lake based on the volume of water and an average TSS concentration in reservoir.*

Median flow rates were used for Rio Grande flows in conjunction with storm flow rates (up to the 500-year event) for the source areas. This should underestimate dilution effects and provide conservative concentration estimates.

RAC Response: *OK*

Equations (4) and (5) are not consistent with assumption 2 that states that the erosion rate is uniform over the watersheds. These equations assume that erosion occurs only on the source areas. To provide a consistent estimate, a multiplicative area factor (AF) could be added which would be the ratio of the source area to the upstream watershed contributing area. For exposure locations dominated by the Burn Areas, this factor would likely be unimportant. However, for the smaller source areas, the factor would decrease the predicted mass/activity by at least an order of magnitude.

RAC Response: *The main point of assumption 2 was that the concentration of TSS was assumed to be uniform throughout the watershed. The method assumes that for purposes of calculating the maximum potential chemical mass and radionuclide activity, the entire concentration of suspended sediment in storm water flow that crosses the source area*

resulted from that source area. This assumption provides a conservative estimate of the maximum potential chemical mass and radionuclide activity that could be released from a source area during a storm event. The suspended sediment and volume of water at the point of exposure were based on the TSS concentration and the storm water flow at that point of exposure.

The modeling approach did not take into account deposition of or dilution with clean sediments or water in the canyons, but assumed that the maximum chemical mass and radionuclide activity would be released from each source area and transported to the point of exposure. We assumed that the source areas and their flows were independent. We felt that this was the only feasible assumption for the calculations because there isn't a direct relationship between area and flow, due to the spatial differences in flow resulting from the terrain and runoff. The alternative would have been to try to incorporate deposition and re-suspension mechanisms in the model, which was beyond the scope of the modeling effort for this project.

The estimated soil concentrations given by equations (21) and (22) do not appear to be reasonable (overly conservative). The water and sediment concentrations are based on storm flows, with sediment carried as part of the suspended load. These equations assume that the water and sediment suddenly stop and make up the soil mass. What happened to the native soil? What fraction of the soil comes from native soil and what fraction from recently deposited sediment.

RAC Response: *It is true that we have assumed that the water stops suddenly and the sediment is deposited. The equations provide a conservative estimate of the concentration of chemicals and radionuclides in deposited sediments along the stream segments within the canyons. The intention was not to estimate a soil concentration as much as a deposited sediment concentration. These stream segments are assumed to be dry except during the rain events. We will adjust the exposure rate by assuming a fraction of soil exposure to be associated with the deposited sediment to account for the combination of contact with deposited sediments and existing "clean" soils.*

Certainly this factor must be several orders of magnitude. This is significant since soil concentrations can lead to much longer exposure duration pathways compared with storm water flows.

Other comments are as follows.

- On page 1, the title "GIS-BASED EVALUATION ..." is for Technical Memorandum for Task 2.3, rather than this one.

  RAC Response: *This has been corrected*

- What assumptions are made with regard to the concentrations and distributions of constituents within the Burned Areas?

RAC Response: *The concentrations of chemicals and radionuclides identified as elevated in ash are assumed to be constant throughout the entire burned area.*

- Change "1.2" to "2.1" in the first paragraph below Table 6 on page 22.

RAC Response: *This has been corrected*

- In equations (4), (5), etc., state that CF is a conversion factor (i.e., CF(kg/mg) = 10^{-6}).

RAC Response: *This has been added.*

- The denominator of equations (12) and (13) is the same as the retardation factor used in groundwater contamination studies, as it should be.

RAC Response: *OK*

- What is SD(sec) in equation (18)? Is this the source duration? What duration was used? The 6-hr storm duration event? Does this in some way relate to the duration over which the point-of-exposure concentrations are present?

RAC Response: *SD represents the storm event duration. A description has been added.*

- In equations (25) and (26), substitute ρ_s for ρ_b. In the line following equation (25), replace 22 with 25.

RAC Response: *This has been corrected.*

Review By T. E. Hakonson

Specific goals of this Subtask were to "determine the magnitude of incremental exposure and associated risks to the public from transport of radionuclides and chemicals associated with the LANL facility released as a result of the fire through surface water pathways. This assessment will be based on evaluating current surface water monitoring efforts, characterizing surface water runoff using Geographic Information System (GIS)-based technology with emphasis on the watersheds that cross the LANL facility, analyzing potential source terms associated with the LANL facility in the watersheds, and estimating the incremental risks and the uncertainties associated with the surface water pathway."

This report deals with estimating the distribution and magnitude of transport of chemicals and radionuclides by surface water before and after the Cerro Grande fire. In addition, RAC estimated the pre- and post-fire concentrations of chemicals and radionuclides in deposited sediments soils, and surface water at points of exposure for selected exposure scenarios. Most of this report deals with procedures for estimating contaminant concentrations in source areas in the Los Alamos, and Pueblos Canyon watersheds, transporting them downstream in association with

runoff water and associated sediments, and delivering them to specific location within and below these watersheds.

This is a well written report that, I am pleased to see, clearly states the limitations of the methods and resulting predictions of contaminant distribution and transport. I especially appreciate the statements that the hydrologic modeling approach used by *RAC* does not provide accurate estimates of contaminant distribution and transport and that the modeling is only useful as a screening tool to look at relative response of burned versus non-burned areas in contributing contaminants to downstream areas. Such recognition suggests to me that the authors of the report are well qualified to address this specific task and that they understand the limitations of predictive modeling in the absence of sufficient initializing and validating data.

The basic technical problem with the procedures described in this report is that there is a lack of appropriate and/or sufficient site specific data to initialize and calibrate the surface water and contaminant transport model. As such, many assumptions had to be made that led to a lot of lumping of processes.

> ***RAC Response:*** *To the extent that empirical data are available in the area of the selected points of exposure, a comparison and discussion is provided in the Task 2.7 report.*

This lumping of parameters and processes is commonly done in modeling studies but runs the risk of omitting details that will be important in determining the actual and/or relative magnitude of post-fire contaminant transport and associated risks. There is not a lot RAC can do to remedy problems with data availability and in order to achieve project goals, they must make simplifying assumptions and recognize, as they have, the limitations of the results.

There several key assumptions that RAC made in developing the methodology to predict sediments, water, and contaminants at exposure points that could be challenged. However, to RAC's credit, they recognize most of these problems and indicate that future reports will address the validity of some of these assumptions in more detail. Specifically, RAC assumed the following:

1. the mass of sediment movement from a source area to a point of exposure can be estimated from a total suspended solids (TSS) concentration. (**The basic problem here is that this is not constant through a runoff event and it is a function of watershed, storm, and contaminant characteristics).**

> ***RAC Response:*** *The reviewer is correct that the TSS will have spatial and temporal variability. By using a median value of the available empirical data, we are incorporating spatial and temporal variability, while retaining the calculation simplicity of a single value for TSS. We did use predicted storm water flow values, which incorporate watershed, terrain and storm intensity difference, at the source areas to estimate chemical mass and radionuclide activity.*

2. the concentration of TSS (and therefore erosion) is uniform throughout the surface water domain. **(This is undoubtedly is not true across the study watersheds)**

   RAC Response: *See previous comment.*

3. The total mass of chemicals and radionuclides in the runoff water above a source area is equal to the total mass of chemicals and radionuclides in the soil based on estimated average soil concentrations for each source area based on measurement data. **(This assumption discounts particle sorting in the detachment and entrainment of soil particles by runoff).**

   RAC Response: *We agree with the reviewer that particle size plays an important role in the transport and fate, including deposition and re-suspension, of the sorbed chemicals and radionuclides. It was not feasible to consider these mechanisms in the modeling for this project.*

4. Concentrations of chemicals and radionuclides in storm water will be the result of equilibrium partitioning between the soil particles and the water. A simple mass balance of chemicals and radionuclides between storm water and soil particles is considered. **(The Kd method used to describe this partitioning needs a lot more examination as the low Kd based on monitoring data may be flawed).**

   RAC Response: *We selected Kd values reported in the literature, and where there were multiple possible values we chose the lowest of those values. In almost all cases (except for very low Kds), this would maximize the calculated water concentration and likely have minimal impact on the sediment concentrations. The Kds calculated using monitoring data were used for comparison to the values reported in the literature, and they suggested that our assumed values are not overestimated.*

5. Dilution is the only attenuation mechanism considered for concentrations of chemicals and radionuclides in soil, sediments, and surface water at the points of exposure. (Particle size fractionation and its effects on downstream contaminant concentrations is ignored; see Muller, R. N., Sprugel, D. G., and Kohn, B. 1978. Erosional transport and deposition of plutonium and cesium in two small midwestern watersheds. J. Environ. Qual. 7(3): 171-174; Sprugel, D. G., and Bartelt, G. E. 1978. Erosional removal of fallout plutonium from a large midwestern watershed. J. Environ. Qual. 7(3): 175-177.

   RAC Response: *The reviewer is correct, however, this additional level of complexity of modeling was not possible for this project. An important limitation in this regard was the lack of empirical data for concentration distributions with respect to particle size at the source areas.*

I am puzzled as to how RAC intends to develop "real" estimates of risk based on the pre- and post- fire analyses. If the models only provide qualitative, relative differences that cannot be verified with data, then how can they state whether pre- or post-fire risks are a specific value or range of values? It would seem to be less than satisfactory to use relative differences in risk as an

end point for the assessment but regardless, even relative differences will need some assignment of uncertainties to the estimates. I am sure RAC has thought this problem through, but I would like them to share these thoughts in this report. Maybe I am under a misconception as to what "incremental risk" means. I assumed that it represented an actual dose over and above a pre-fire dose.

> ***RAC Response:*** *The primary objective is to look at incremental risk from pre-fire to post-fire, although post-fire risk is evaluated to identify significant chemicals and radionuclides. There is some comparison to empirical data to the extent it is available that will provide some foundation for the predictions. It is also important to note that the goal of a first screening evaluation such as this one is to identify the chemicals, radionuclides and scenarios that present the greatest relative risk. Further more detailed studies can then be focused on refining the estimates and collecting specific environmental monitoring data to address these potentially higher-risk scenarios.*

Mention is made that background concentrations of chemicals and radionuclides were subtracted from LANL monitoring data to get at LANL contributions to source term areas. It seems to me that including risk from background concentrations of radionuclides and chemicals, albeit calculated separately, would be instructive as a baseline for comparison with the LANL contribution to risk. Maybe this will be done but it would help to say so on p.17.

> ***RAC Response:*** *Background concentrations were excluded from the source area concentration estimates and were not intended to be included in the risk calculations. A discussion of the relationship of the background concentrations to the predicted concentrations is provided in the Task 2.7 report.*

None of this nitpicking is particularly important given that RAC has done a good job of describing the problems associated with key assumptions and how the assumptions could effect predicted contaminant concentrations downstream. RAC promises further reports examining some of these issues in more detail.

> ***RAC Response:*** *A discussion of the limitations and uncertainties and their potential impacts is included in the Task 2.7 report.*

The issue of changes in surface water transport of soil and sediment contaminants with time after the fire would seem to be an important one. Post-fire data from Los Alamos suggests that enhanced runoff transport of soil and sediment contaminants is short lived and in fact only persisted thru a very few post fire runoff events. This dramatic decline in transport will undoubtedly affect longer term risks associated with the fire.

> ***RAC Response:*** *The issue of long term effects of the fire on storm water flow and sediment transport is important, however, there is not sufficient data to draw conclusions concerning the duration of the increased flow resulting from fire. However, assumptions will need to be made concerning the duration of the increased flow and the applicability of predicted concentrations for purposes of estimating the risk.*

When all is said and done, this *RAC* report, as well done as it is, still leaves me clueless as to how they will predict actual pre- and post-fire risk. In my opinion, the results of the modeling described in this report are qualitative at best. Furthermore, assuming that all of the assumptions (lumping) used in conducting the modeling are valid, results will only provide relative differences in risk, not actual estimates. I also don't see how uncertainties will be assigned to model predictions given the large amount of lumping and lack of empirical data. I am obviously not seeing the big picture here and *RAC* has not described it in this report.

> ***RAC Response:*** *Our objective has been to look at incremental risk resulting from the effects of the fire on storm water and sediment transport. We agree that the results are conservative and based on a number of assumptions, however, we believe the results will provide a reasonably, albeit conservative quantification of risk. Unfortunately, we will not be able to quantitatively address all of the uncertainties.*

Los Alamos National Laboratory
Comments on RAC Task 2.7: Analysis of Exposure and Risks to the Public from Radionuclides and Chemicals Released by the Cerro Grande Fire at Los Alamos

Task 2.7: Estimated Risks from Releases to Surface Water

Tom Buhl, HSR-DO
General:

1. The authors were very responsive to many comments that had been submitted previously, regarding including a sample problem (Appendix V) and much more detail in the report so that the calculations could be tracked. This is very much appreciated, and I'd like to thank the authors for doing this.

> ***RAC:*** *thanks!*

2. The report has adopted a comprehensive systems approach – beginning with source terms and following the contaminant concentrations through the canyon/river system. This is a great aid in understanding exposure pathways, developing sampling plans, etc. However, some important sediment sources were not considered, especially the potentially large amount of sediment coming from uncontaminated areas in the canyon system. Not including these sources leads to over-conservatism in the risk estimates and precludes the possibility of comparison of PRS impacts with non-LANL, CGF-related impacts due to these non-contaminated sediments and ash.

> ***RAC Response:*** *Knowledge about the total uncontaminated areas and complete characterization of contaminated areas in uncharacterized canyons is lacking. As a result, we used a conservative approach to ensure that we would not underestimate the risks.*

3. At many places the report states that it is 1 to 3 orders of magnitude on the conservative side. Being this conservative would be acceptable in a screening calculation, if all the estimated risks are considerably below some risk decision level. In this report, however, the risks approach and even exceed the decision levels of 1×10^{-5} or a hazard quotient of 1. In this case, it is important to carry the risk assessment a step further – investigate the assessments of the high risk pathways by making them more realistic, and then seeing if the more realistic risks still exceed the decision levels. Without this important last step, the reader is still uncertain if an actual risk exists or not.

> ***RAC Response:*** *In the initial stages of this study, we determined that a bounding-type approach (like screening) was necessary given the large uncertainties and limitations associated with developing a complete characterization of all source areas. Without additional information upon which to base source term characterization or inventory estimates, the subsequent step of estimating more realistic or actual risks is not possible. Therefore, we made a number of assumptions to avoid underestimating the true potential risks. The results of this*

bounding approach, coupled with comparisons to measured concentrations, suggest that our estimated risks are biased high. The results of these calculations can be used to identify specific areas and materials contributing most significantly to risk, which in turn can help guide future characterization and monitoring efforts. However, more refined and realistic calculations are not warranted, nor would they be particularly meaningful, until a more comprehensive and systematic characterization of source areas has been completed.

4. A problem for the reader is that it is not clear how to act based on the information presented in the report. Risks from several pathways, for example ingestion of fish from Cochiti Reservoir, are reported to be above the $1x10^{-5}$ level, so that the reader concludes that these pathways should be avoided. On the other hand, the report states that it is 1 to 3 orders of magnitude conservative. Does this mean that these pathways do not have to be avoided? This presents conflicting, or at least unclear, guidance to the public.

***RAC Response**: Although there are some instances where potential bounding risks approach or exceed the risk criterion of $1x10^{-5}$, comparisons to environmental measurements support our assertion that these bounding risks are indeed biased high. However, until more refined and comprehensive source term or inventory information is available, environmental monitoring should be focused on determining existing concentrations of specific materials in specific media at specific locations. The New Mexico Health Department would be the agency responsible for providing guidance to the public about potential health concerns.*

5. The predicted sediment/soil, and water concentrations in most cases are the same as those given in earlier drafts. Comments submitted on review of those drafts would still apply. In particular, summing the activities/masses of all contaminants from the PRSs at the POEs without allowing for sedimentation losses, and not explicitly including the potentially large amounts of clean sediment from uncontaminated areas on an equal basis with the PRS sediments, give unrealistic estimates of contaminant concentrations at the POEs.

***RAC Response**: Without the comparisons of measured to predicted concentrations, it is not possible to conclusively state that the estimated concentrations are unrealistic, because of the uncertainties and limitations associated with source area characterization. We believe the bounding approach used for this project was appropriate considering data availability. It should be noted that, uncontaminated sediments in the Rio Grande and the Cochiti Reservoir were included in the revised calculations. In addition, uncontaminated water in Cochiti Reservoir was included in the revised calculations.*

Specific:

1. Comparison of predicted to observed concentrations of Cs-137 and Sr-90 may be misleading. Cs-137 and Sr-90 concentrations are elevated in environmental media because they were concentrated in the ash as a result of the fire. The modeling

predictions on the other hand do not take into account the effects of the ash, only the PRSs. The higher concentrations in ash for Cs-137 and Sr-90 may dominate the environmental samples.

> ***RAC Response:*** *The modeled predictions* ***do*** *account for the contribution of elevated concentrations in ash from the burn area for both* ^{137}Cs *and* ^{90}Sr.

2. Fish pathway. The following comments address the relatively higher risk predicted for ingestion of fish from the Rio Grande.

2.1. Predicted concentrations of dissolved contaminants in the Rio Grande water appear to be high. In the Appendix U spreadsheet for POE3.1, worksheet "POE3.1(T)," the Cs-137 concentration at for receptor 3.1 is predicted to be 29 pCi/L (dissolved). This is considerably above the maximum Cs-137 value of 1.74 +- 1.59 pCi/L in an unfiltered sample measured in the Rio Grande in CY2000 (CY2000 LANL Environmental Surveillance Report [ESR], p. 238). This is important because this value drives the second highest radionuclide risk from fish consumption at this location.

> ***RAC Response:*** *Because of the conservatism assumed for the source term and transport calculations, our predicted values are likely higher than measured values. The report presents and discusses these types of comparisons for other materials and locations, as well.*

2.2. On July 2, 2001, the Los Alamos/Pueblo Canyon system had a 10-year flood event with a peak flow of 1400 cfs. Consequently environmental concentrations predicted in the report may be compared with measured concentrations to see how well the model is predicting actual conditions.

> ***RAC Response:*** *A comparison of these measured data(corresponding to sediment samples collected following this 10-year flood even)t to predicted concentrations has been incorporated into the final report.*

A Cs-137 concentration in Rio Grande water at POE 2.1 of 13 pCi/L, and a bioaccumulation factor of 2000 (pCi/kg)/(pCi/L) for Cs-137 in fish (Appendix U spreadsheet, worksheet "Rad transfer factors") give (13 pCi/L * 2000 pCi/kg/pCi/L =) 26000 pCi/kg in the Rio Grande fish. This exceeds Cs-137 concentrations measured in Cochiti Reservoir fish, for example which averaged 0.0006 pCi/g (dry weight) = 0.00015 pCi/g (wet weight) for CY2000. A similar comment applies to the CY2001 results, as shown in the table.

Sample Type	Year	Measured Cs-137 Concentration	RAC Prediction	Predicted/ Observed
Game Fish	2000	0.0006 pCi/g dry[1] = 0.00015 pCi/g wet	26 pCi/g wet	170,000
Game Fish	2001	0.0051 pCi/g dry[2] = 0.0013 pCi/g wet	26 pCi/g wet	20,000

[1]CY2000 ESR, p. 461
[2]CY2001, ESR (draft)

Risk from ingestion of fish from Cochiti Reservoir having high amounts of Cs-137 was one of the highest risks calculated in the report, and exceeded the 10^{-5} risk level for seven year exposure. The above table shows that the amount of Cs-137 ingested was overestimated by 4 to 5 orders of magnitude. This suggests that Cs-137 intake through fish ingestion is not an important pathway, and that the method used to calculate the Cs-137 concentrations – and by extension, other contaminant concentrations - should be revised to be more realistic.

> ***RAC Response:*** *The reviewer is correct in asserting that the predicted concentrations are likely overestimated. See above responses for why we believe this approach was necessary. Furthermore, since the draft report was issued, we have been reviewing the literature about bioaccumulation factors to determine values that may be more appropriate for the northern New Mexico area. It appears that the bioaccumulation factors used in the draft report calculations may have been overestimated for some chemicals by 2-3 orders of magnitude.*

2.3. The report assumes that the high radionuclide concentrations in the Rio Grande persist at a constant level for the seven-year assessment period. This ignores the dynamic nature of the Rio Grande. For example, ash in the Rio Grande water after the Cerro Grande fire was swept through the system in a matter of days. Assuming a constant radionuclide concentration in the river water for seven years, with the accompanying update by the fish, is highly conservative.

> ***RAC Response:*** *We agree that the approach is conservative; however, our bounding approach was designed to be conservative. To clarify we did not specifically assume a constant concentration of chemicals and radionuclides in the Rio Grande water during the entire year. Rather we assumed that after any storm event during the year the estimated concentrations would be present in the river water for a short period of time and the receptor would be exposed to these concentrations. Rainfall was estimated to occur 88 times per year, which resulted in an estimated exposure frequency for receptors on the river of 100 day per year. This approach assumes that the concentration in the river is zero before each rain event or 265 days per year. Other assumptions such as (1)a non-depleting source and (2) that each rainfall event occurs throughout the entire watershed contribute to the conservatism of this approach.*

2.4. Please check the risk calculation for K-40. It is not clear why K-40 should be present in above-background amounts in the environment. Secondly, since potassium is under homeostatic control by the body, it is difficult to see how any above-background dose could be obtained from K-40 exposure as long as the K-40 remained in the natural proportion with the other potassium isotopes – i.e., if more K-40 is ingested, then more stable potassium is also ingested, and any excess above what the body needed would be excreted.

RAC Response: *Background concentrations of ^{40}K were inadvertently not subtracted to derive net source area concentrations. This has been corrected for the final report, and the predicted concentrations are no longer elevated above background. We also have included a discussion that the concentration of potassium in the human body is homeostatically controlled.*

3. Page 5-8, first paragraph, isn't $F_{cw} = 0.1$, rather than 0.5 as shown?

RAC Response: *The reviewer is correct; the change was made in the report.*

4. Page 5-12, second paragraph of Section 5.2.2.3. Please check the dermal contact risk method for radionuclides. The 1987 Kocher-Eckerman paper that is referenced gives the electron dose to the skin, not the effective dose equivalent (which is an "equivalent whole body dose"). The lifetime risk coefficient of 7.3×10^{-2}/Sv taken from ICRP 60 (also see second paragraph below) on page 5-13 refers to the risk per effective dose, not risk per skin dose. The risk per skin dose is much smaller, i.e., the Sv dose in the Kocher-Eckerman paper is dose to skin, but what is needed to dose to "whole body" – effective dose – to use the lifetime risk coefficient properly.

One possible way to handle this is to multiply the skin dose by the ICRP 60 tissue weighting factor for skin of 0.01 (paragraph 29 of ICRP 60), to get the effective dose.

The lifetime risk coefficient of 7.3×10^{-2}/Sv quoted in the report on page 5-13 and taken from ICRP 60 is actually ICRP's risk of detriment from radiation, and includes not only cancer but also severe hereditary effects (see ICRP 60, page 70, Table 5-3). In addition, only for fatal cancers is the detriment equal to the probability of occurrence. The risk of cancer incidence for non-fatal cancers would not be equal to the risk of detriment from these cancers in the ICRP treatment. Total ICRP 60 cancer incidence/morbidity probability can probably be determined from the information given in section B5.14, but is different than detriment.

The other comment would be that using these dose conversion factors only accounts for the electron dose and ignores the gamma dose. The gamma dose would irradiate the entire body and not just the skin, so it would seem that this should be included if this pathway is to be evaluated for radionuclides.

RAC Response: *The reviewer is correct in pointing out that the 1987 Kocher-Eckerman paper gives the electron dose to the skin not the effective dose equivalent. The calculation has been revised as suggested. The lifetime risk coefficient of 7.3×10^{-2}/Sv has been replaced by a lifetime risk coefficient of 6.0×10^{-2}/Sv. This is the nominal value for the whole population that is reported by ICRP 60 (page 22, Table 3) for fatal and non-fatal cancer.*

The text was revised to indicate that gamma-emitting radionuclides in sediment on the skin surface would irradiate the whole body and not just the skin. This

exposure route was not included in the calculation explicitly because it was found to have a negligible impact on the calculated risks.

4. Page 5-16, second to last paragraph, last sentence, delete "and radionuclides" as only chemicals are being discussed in this section.

RAC Response: *The reviewer is correct; the change was made.*

5. Page 5-29, text for Figure 5-4, first sentence, replace "radionuclides" by "non-carcinogenic chemicals"

RAC Response: *The reviewer is correct; the change was made.*

6. Appendix V2, Page V2-1, table.

6.1 Shouldn't the value for "Concentration in PRS-10" be 0.66 pCi/g, rather than 0.34 (from previous page, Page V1-2)?

RAC Response: *The example calculation has been revised to reflect the correct concentration.*

6.2 At several locations the table refers to the "Burn Area." The burn area for the Los Alamos/Pueblo Canyon system is large. It is surprising that the "Average storm water flow at Burn Area" is only 6.7 ft^3/s, much smaller than the 580 ft^3/s flow for PRS-10. Why wouldn't the burn area flow be much larger than the flow for the PRS?

RAC Response: *Because the burn area is large, the average concentration will be substantially lower than an individual PRS that may be located on or adjacent to a stream segment in the water shed.*

6.3 Appendix H was not included in my copy of the report.

RAC Response: This appendix was included in electronic format only. We apologize for not clearly noting that in the report.

7. Appendix V2, page V2-4, third equation (giving A_{ss}(pCi) for PRS-10). The preceding discussion would imply that the values in the equation should be "1.2E+09 – 1.8E+08", rather than "4.0E+06-6.0E+05" as given in the text.

RAC Response: *The example calculation has been revised to reflect the correct concentration.*

8. Appendix V3, page V3-2, last equation. Wouldn't the Pu-239 sediment concentration at location 1.2 be 18 pCi/g rather than 12.23 pCi/g?

RAC Response: *The incremental (i.e., post-fire minus pre-fire concentration) was used for this example, so the value of 12.23 pCi/g is correct.*

Lars Soholt, RRES-ECO

1. Table 4-13. Looks like the CV for TSS should be 3.2, not 240.

   RAC Response: *The reviewer is correct. The CV value should be 3.2.*

2. p. 4-48, third and fourth bullets should refer to mass rather than volume, I think.

   RAC Response: *This is correct, the actual quantities used to determine concentrations were the mass; however, volume was also determined. This has been revised to include mass and volume.*

3. p.4-59, the 2nd full paragraph. the assumption is made here that only clean sediment deposits out at flow moves to the river; this implies that analytes are becoming more concentrated in the sediment as they progress down stream. this doesn't seem to me to be a realistic assumption and is not supported by the measured data that I have seen.

   RAC Response: *The difficulty in trying to assume a deposition rate for the impacted sediments in locations throughout the watershed is that data are lacking with which to develop such an assumption. It was thought that it would be better to conservatively estimate the transport of chemicals and radionuclides on sediments than to try to estimate a deposition value without much justification.*

4. pp. 4-49 to 4-65. this section did not flow well to point of exposure, in part, I think that this is because the discussion of Kd and Rio Grande flows seems to interjected into the middle and would probably be best placed toward the end of POE discussion. I am not sure why analytes are being partitioned between water and solid phases here, because it looks like they get added back together at the Point of Exposure. Is there a reason for this? If it is not necessary for further exposure analysis it complicates the story more than needed.

   RAC Response: *Moving the K_d and Rio Grande Flow discussion would require a significant reformatting of the report. Since the storm water flow impacts the magnitude of the mass and activity associated with each source area, the analytes are partitioned at the source area to estimate the mass and activity that is present in each phase based on the source area-specific volume of storm water. The mass and activity contributing to each point of exposure is then accumulated at the point of exposure and point of exposure concentration are estimated.*

5. p. 4-67. Aprs is the area in 'square meters'. This discussion about the PRS is confusing. I assume that what is meant is the totality of the PRSs that feed to this point. Also, if all sources feed POE 3.1 shouldn't it have a greater area input than any of the others?

RAC Response: *The reviewer is correct, A_{prs} is in square meters. We have clarified the total area discussion. This discussion relates to the average erosion loss for the combined PRS associated with each POE. The total area of PRS associated with POE 3.1 is greater than the other POE's; however, the A_{prs} presented in Table 4-29 is the average area of a PRS associated with the PRS not the total area as indicated by the table heading. The table heading has been revised.*

6. Table 4-32. Is this the ratio of 2y/500y event predictions?

   RAC Response: *The reviewer is correct, the caption for table and the text have been revised.*

7. Table 4-33. SS units should be mg/kg? It looks like as the sediment is deposited it is losing some of its BaP. Is that true?

   RAC Response: *The reviewer is correct the suspended sediments units should be in mg/kg. The table has been revised. Deposited sediments are assumed to be similar to saturated soils for purposes of estimating a concentrations. As a result the mass of deposited sediments is larger than the mass of suspended sediments resulting in an apparent lower concentration. However, the actual chemical mass and radionuclide activity is increased in the deposited sediments to account for chemical mass and radionuclide activity in the water in the deposited sediment pore spaces.*

8. p. 4-87. It is not true that the PAHs or radionuclides are not to be expected outside of Laboratory contributions. This is acknowledged for the radium and thorium a few lines later.

   RAC Response: *We did not intend to imply this and will change the wording in the report to reflect the fact that these PAHs and radionuclides could occur in the environment outside of LANL operational contributions.*

9. Table 4-45. I believe the negative value for Db(a,b)A is a typo and not the result of reporting negative measured values.

   RAC Response: *We appreciate you noting this.*

10. p. 5-4. It is stated here that a 7y exposure duration is used, but later on only 1y is used (see Table 5-1) but even later 7y is used again. This gets a bit confusing.

    RAC Response: *The wording will be revised to make our use of the 7-yr exposure period clear. We calculated annual risks initially in order to compare across our exposure scenarios, one of which (firefighter) was exposed only during a portion of one year.*

11. p. 5-8. Fraction of contaminated water is presented here as 0.5 and as 0.1 in Table 5-1.

    RAC Response: *The reviewer is correct; the change was made.*

12. p. 5-8. Is the concentration in river water dissolved or total? If dissolved, then that would explain why analytes are partitioned at the point of exposure.

    RAC Response: *Concentrations in river water used for the drinking water pathway were dissolved concentrations. Text will be added to clarify this.*

13. p. 5-10, 1st paragraph. 4 h/d exposure frequency is used for adults in Scenario 1 as presented in Table 5-1.

    RAC Response: *The reviewer is correct; the correct value was noted in the table.*

14. sec. 5.3.1ff. At this point, I am not sure whether pre- or post-fire risks are being discussed, until we reach p. 5-29.

    RAC Response: *The text will be revised to make it clear what risks are being discussed.*

15. p. 5-29. The last paragraph is confusing. It implies that effects of the fire were relatively minor, but is the difference between 'post fire' and 'decrement' is small, this implies that post-fire risks were 'substantially' higher than pre-fire. It would be more informative if the actual decrement was presented here.

    RAC Response: *The text will be revised to make it clearer for the reader. The differences seen are well within the uncertainty of the risk estimates.*

16. Table 5-5. Shouldn't there be a direct exposure pathway here?

    RAC Response: *External exposure from radionuclides was calculated but not included in this table; we will include all pathways in the table in the final report.*

Resident on RG sediment ingestion should be e-8 rather than e+8.

RAC Response: *The correction was made.*

It would be useful to have further discussion of some of the measured values in relation to the predicted. In some cases predicted values lead to overestimates in excess of 1000, e.g., fish consumption.

RAC Response: *We will consider additional discussion on this issue.*

Ken Mullen, RRES-WQH

1. When comparing empirical data to modeled data it seems that considering only data within 250 meters of the POE is too restrictive. This eliminates a great deal of data useful for this comparison.

   ***RAC Response**: The reviewer is correct that additional data could be incorporated into the comparisons, but time and resource constraints limit the amount of analyses that can be completed in this regard as part of this project. In addition, increasing the distance criteria may or may not result in the inclusion of a great deal more data. For example, increasing the criteria to 500 meters would add few additional locations that either are not onsite and comparable to only POEs 4.1a or 4.1b or include only groundwater samples. Increasing the criteria to some greater distance may or may not be appropriate for identifying relevant comparable data. We believe the data selected using this distance criterion are both suitable and sufficient for making these comparisons.*

2. There is a great deal of data available from 2001. 2001 was a pretty normal year in terms of snow melt and rainfall.

   ***RAC Response:** All post-fire data included in the most recent data set provided by ESH-18 have been included in the analysis.*

Response to Comments from Peer-Reviewers for the Draft Task 2.7 Report.

Analysis of Exposure and Risks to the Public from Radionuclides and Chemicals Released by the Cerro Grande Fire at Los Alamos.

Task 2.7: Estimated Risks from Releases to Surface Water (April 8, 2002).

May 8, 2002

Comments received from T. E. Hakonson April 29, 2002:

General Comments

This RAC report integrates several previous subtask reports on the role of the surface water pathway in contributing risk to human receptors under several exposure scenarios before and after the Cerro Grande wildfire. The steps RAC used to estimate risks were as follows: 1) develop a surface water model domain, 2) evaluate the available surface and storm water monitoring data, 3) identify the sources and magnitude of chemical and radionuclide releases, 4) model the release and transport of radionuclides and chemicals in surface and storm water, 5) define representative exposure scenarios and parameter values, 6) estimate the associated health risks, and 7) develop conclusions based on the results of the risk assessment. RAC identified and discussed the potential impact of uncertainties and limitations associated with each of these steps.

My review covers Chapters 4-6 of the Task 2.7 report or items 4-7 listed in the paragraph above. Thus, my comments primarily relate to the transport modeling, risk assessment, and the conclusions RAC drew from their study. I have reviewed Chapters 1-3 and parts of 4 previously. My review comments are offered for consideration in developing the final RAC report on Task 2.7 and to identify some potential questions from stakeholder groups.

First, this RAC report reflects that a lot of work has gone into the evaluation of the surface water pathway and they have done a credible job of estimating risks from the Cerro Grande fire. Given the time and data constraints, *RAC* should be commended for a valiant and balanced effort.

As clearly discussed by *RAC*, there are many limitations to their approach to estimating risks from the surface water pathway. Some of these limitations include the lack of appropriate monitoring data to initialize their models or to compare with model predictions, the need to simplify and lump processes and data, and the use of educated guesses when data and parameters were unavailable.

Review Issues

Chapter 4 of the report is a very hard read as there is a confusing mix of procedures development and testing and comparisons of model predictions with monitoring data of various quality. I have read Chapter 4 three times and still am not certain exactly what was done to estimate runoff, erosion, and contaminant transport from upland source areas and stream channel drainages.

RAC Response: *Chapter 4 is a compilation of the Task 2.3 and Task 2.5/2.6 reports with changes and edits made to address many of the comments related to these reports The introduction of Chapter 4 summarizes the steps used to develop the concentration estimates. The details of these steps are provided in the subsequent sections of this chapter. Certainly the chapter could be organized in a different format, however, given the complexities associated with estimating the concentrations of chemicals and radionuclides at the points of exposure, the presentation of the calculations and results are difficult necessarily.*

The methods seem to be based on a complex mixture of relatively "good" site specific data, extrapolation from limited and questionable onsite data, some guesses, and a large amount of lumping and simplifying assumptions. While I recognize that this approach was probably necessary because of the limited on-site data and in order for RAC to complete the project on schedule, this leads to questions about how this all impacts the risk estimates. RAC claims that the risk estimates are conservative but I am having difficulty in seeing an objective way in agreeing with them.

Because of all the simplifying assumptions and lumping of processes, I wonder if the conservatism isn't serendipitous. For example, I think RAC used one post-fire TSS concentration in runoff from source areas (i. e., 10,000 mg/l) for all of the PRS's. The question is how does this simplifying assumption effects contaminant transport and risks across PRS's where TSS concentrations will undoubtedly vary by PRS for any given design storm due to differences in soil, vegetation, slope and other physical and biological differences between PRS's. Measured post-fire TSS in some of the canyons were as high as 80,000 mg/l.

RAC Response: *An empirical data analysis was used to try to understand the relationship between pre- and post-fire TSS values. TSS was used as a surrogate for the more complex process of erosion and sediment transport of chemicals and radionuclides. There are no simple procedures for modeling erosion and sediment transport and associated chemical and radionuclide fate and transport. In addition, TSS is a quantity that has been measured at various times and locations both before and after the fire. The median values for pre-fire and for post-fire data were selected for the modeling because they inherently are spatial averages for TSS since all of the data from throughout the watersheds were included in the same data set. It is not necessary to use extreme values to have conservative calculations. The fact that the modeling assumed that there was no loss of chemicals or radionuclides in the transport from the PRS and the exposure locations is a very conservative assumption. Based on the available data, It was not possible to develop specific values for each PRS for each design storm.*

RAC claims throughout the report that their risk estimates based on the surface water pathway, and the processes embodied within it, are conservative estimates and that final risks are overestimated by a factor of 10-1000. I am not going to do a blow by blow analysis of the limitations and assumptions that I think makes assigning of level of conservatism problematic. However, as an example, RAC makes the statement that they "developed conservative estimates of the surface water flow within the watersheds and at outlets to the Rio Grande for 2-, 5-, 10-, 25-, 50-, 100, and 500-year design storm events of 6-hour duration." Data for the 2 and 5 year

design storm flows post-fire are presented in Table 4-11 along with the maximum measured flow at or near specific prediction points. It seems clear to me that the claimed factor of 10-1000 conservatism does not apply to the data in Table 4-11. While RAC states that comparing modeled versus measured data in Table 4-11 is risky, they then go on to state that the comparison "provides support to the assumption that the modeled storm water flow estimates are conservative---". A quick look at Table 4-11 clearly shows that the data for predicted versus maximum measured storm flow does NOT show that modeled storm flow estimates are conservative. Over half of the comparisons (11 of 19) in Table 4-11 show that the measured maximum runoff was about equal to or greater than the predicted runoff for one or both of the 2 and 5 year design storms.

RAC may need to explain what they see in the Table 4-11 data that convinces them that their model predictions of storm flow are conservative. At a minimum, I think they need to temper statements about the level of conservatism or present a much better discussion supported by convincing data that the level of conservatism in the risk estimates are as they state.

> ***RAC Response:*** *We agree that the claimed factor of 10-1000 conservatism does not apply to the data in Table 4-11 or to the storm water flow estimates. This factor was based on the comparison of the empirical concentration data to the estimated concentrations at the points of exposure and would imply an extraordinarily large amount of conservatism in the final results. It is very difficult to make a direct comparison between the modeled surface water flow and the empirical surface water flow data. Ideally, the outlet points of the watershed would have been the place to make a comparison between modeled storm water flow estimates and empirical data. However, there were no empirical data available at these locations and the points selected for comparison were based on the availability of data. As a result, sample points with sufficient empirical data were selected recognizing that there were a number of confounding factors that would impact any comparison. These confounding factors are discussed in the report as they specifically relate to the particular sampling point.*
>
> *In addition, the following points need to be considered when evaluating the storm water flow data:*
>
> - *The modeling effort did not take into account any surface anomalies, base flow, NPDES discharges s or other features that may impact the empirical data.*
> - *There are no data available to categorize each empirical storm water flow value by rainfall event (e.g., 2-year, 5-year, 10-year storm).*
> - *Table 4-10 and 4-11 include storm water flow estimates for the 2-year and 5-year storm event and the maximum measured storm water flow for twenty gages totaling weighty individual comparisons. There are fourteen out of eighty (i.e., 17.5%) comparisons where the maximum flow is greater than the model flow with only six (i.e., 7.5%) of which are greater by more than a factor of two and only three (i.e., 3.75%) greater by more than a factor of ten. Given the uncertainties in the calculations and comparisons, it is reasonable*

to conclude that the modeled storm water flow values are larger than the measured storm water flow values and therefore conservative for purposes of the calculations.

- *The greatest error in the comparison of predicted to maximum storm water flow occur at gage E040 (e.g., four of the fourteen predicted storm water flows lower than the measured flow). Several things to consider: 1) E040 is 120 meters north of the stream segment defined by the DEM and the value extracted from the grid may be representative of the actual location of the gage station (e.g., flow value on the stream 584 cfs, on the DEM at the location of E40 flow is 1cfs two-year storm predicted values), 2) gage E030 which has a flow of 580 cfs is less than 160 meters for gage E040, 3) the coordinates of the gauging stations could be off by as much as one hundred meters so the exact location is questionable, 4) E040 is not located on a predicted stream segment so it is possible that our coordinates are not correct or the older DEM did not place the stream segment correctly.*

  There fore, based on the available empirical data and the confounding factors discussed above and in the report, we believe that the model estimates for storm water flow are conservative. Also, as was pointed out in the introduction of the report, the modeling is not intended to yield a definitive calculation of surface water flow and sediment concentration within the watersheds; rather, to conservatively represent the relative relationship between pre-fire and post-fire surface water flow and sediment concentrations. The additional points discussed above will be added to the text.

The units in Table 4-12 are wrong. Erosion rates of 3.7E+05 tons/acre/yr are not possible. The numbers in Table 4-12 have to be for the entire surface water domain.

RAC Response: *The erosion rates presented in Table 4-12 are for sixteen watersheds defined in the Nyhan et al. 2001 report. The units have been corrected to tons acre^{-1} yr^{-1}.*

In section 4.2.2, it is concluded that there is no functional relationships between TSS and watershed drainage area. There might be several reasons why such a relationship would not be expected to exist including the fact that not all of the drainage area received precipitation for a particular runoff event. Also the drainage areas encompass very diverse landscapes ranging from steep sided areas in the mountain zone to the west of LANL to relatively flat areas in Pinyon-Juniper woodland to alluvial outwashes and steep canyons below LANL in the Upper Sonoran zone. Consequently, the size and variation in ecosystem in the respective watersheds I think would make comparisons between ecologically dissimilar watersheds inappropriate.

RAC Response: *We agree with the reviewer and this was acknowledged in the report. The data comparison was made because of the size of the data set available, there were insufficient TSS data to segregate them by type of watershed and then evaluate the functional relationship with area.*

I am a little more concerned about the fact that no functional relationship existed between TSS (mg/l) and runoff (cfs) (see Fig 4-20). This relationship is the basis for predicting sediment and contaminant yields from runoff. I think RAC should be able to discuss possible causes for this

lack of relationship including problems with LANL monitoring data or the large range in runoff and erosion response of the various watersheds. In any case, the data in Fig 4-20 suggest that models such as RAC used in coupling runoff, sediment yield, and contaminant transport will NOT work.

> ***RAC Response:*** *We agree that the TSS concentration is related to the flow; however, since the data set for TSS concentration with corresponding storm water low measurements was limited, the flow relationship could not be elucidated. Again, if more data were available for each individual watershed, at multiple sampling locations, then these relationships might become apparent. Because the flow dependence of the TSS concentration could not be determined, one average value was chosen from the empirical measurements. These values were collected throughout the LANL study area, and so they incorporate spatial and temporal or flow condition averaging. For the screening modeling that was conducted in this study, the values are appropriate and the use of a single TSS value is appropriate. In addition, an important feature of the work performed here was to estimate the difference between the pre and post fire conditions, so the relative difference in TSS concentration is more important than the actual values chosen. Text was added to clarify this issue.*

Something is wrong with the footnotes to Table 5.5.

> ***RAC Comments:*** *The footnotes have been corrected.*

A few chemicals and rads slightly exceeded RAC's risk criterion of 10-5. RAC then goes on to say that, because of the conservatism, the exceedences are likely not real. It seems to me that a little more thought and less conservatism during the procedures development phase of the study would have avoided try to explain away values above the risk criterion. One simply way to have done that would have been to develop a relationship between calendar date and TSS at Otowi Bridge for which there is a ton of data and to use that relationship to dilute input from the LANL drainages rather than use the median TSS at Otowi. TSS at Otowi has been measured as high as 100,000 mg/l during flooding events. These events on the Rio Grande watershed above Otowi Bridge correspond to about the same calendar time as the flooding events on the LANL study areas.

> ***RAC Comments:*** *The evaluation of flood events in the significantly larger Rio Grande Watershed as compared to the timing and intensity of flood events within the watersheds that cross LANL was considered, but would have been a significant additional effort for the project. The timing and intensity of the Rio Grande floods, or low flow, may not directly correlate with those occurring in the LANL area. The alternativewe chose was to use median values for flow and TSS at the Otowi Bridge gage station, a common option used in risk assessment for parameter estimation. A low flow value (e.g., the 7Q10 flow) would have increased the downstream estimated water concentrations and a high flooding value for TSS would have reduced the downstream solids-related concentrations. The median values provide a middle of the road alternative.*

A final comment is that there was much in the way of LANL publications on surface and storm water processes and their role in contaminant transport that was either not available to or was not used by RAC in their study. I believe the studies by Purtymun, Hakonson, Lane, Stoker, Gallagher, Graf, and Nyhan would have provided a technical basis for improving RAC's model parameters, for reducing the need for simplifying assumptions, and required less lumping of processes. Given my less than satisfied feeling about the amount of science used in this project, I still think that RAC did an outstanding job given the limitations of data and time.

> ***RAC Response:*** *A significant effort was made to retrieve available information for this project. A number of the authors cited were interviewed as part of the project initiation. Certainly many studies could not be reviewed and incorporated, but for a screening analysis the focus was on finding the most important information. The results of this work will hopefully focus future data collection and modeling efforts at those aspects that pose the greatest potential risks. This focusing of efforts should minimize the resources needed to implement refined calculations.*

Comments received from F. Ward Whicker April 29, 2002

Approach to this Review:

This report represents the comprehensive effort by RAC to estimate potential risks to hypothetical humans resulting from use of surface water resources downstream from Los Alamos National Laboratory after the Cerro Grande fire in May 2000. Because large elements of this report were technically reviewed previously, and because of severe time constraints and deadlines on the final review and completion of this report, I focused my review on the overall approach and apparent validity of the main conclusions of the report, as well as the clarity of presentation of the material. My reading included the Executive Summary, Chapter 4 on estimating concentrations of contaminants at points of exposure, Chapter 5 on risk estimates, and Chapter 6, conclusions. My reading of Chapters 4 and 5 was only sufficient to determine whether I could understand the approaches and assumptions used by RAC; I did not attempt review this material in a detailed and comprehensive fashion. I did review the Executive Summary and Conclusions Chapter with more care, because most readers will likely focus on these elements of the report.

General Comments:

1. Overall, I feel this is a very significant piece of work. While it should be classified as a relatively simplistic, and conservative analysis, I feel that it met the primary objectives of the study as well as they could be met, given the constraints of information, resources and time. RAC faced several technical challenges, including an insufficient amount of data to adequately characterize contaminant source areas; an insufficient spatial and temporal coverage of stream monitoring data to rely more heavily on it; unavoidable uncertainties and complexities in terms of terrain, soil and vegetation conditions, storm event distribution and timing; and the hypothetical nature of the human exposure scenarios. In addition to these inherent technical challenges, the administrative challenge of completing this work within a time frame I would characterize as being unreasonably short for such a complex task, was formidable.
2. In general, it appears that RAC did a very thorough job of assembling relevant characterization and monitoring data. I cannot judge whether they were able to obtain all known information, but in any case, the quantity of data obtained, and the manner in which it was analyzed for the purpose of this study, is impressive, if not remarkable.
3. It needs to be pointed out that the characterization design for potential release sites, and the design of the monitoring programs, was far from optimum for evaluating the effects of a major fire on and above LANL property. Most likely, these programs were dictated by regulatory requirements and constrained by resource availability. This situation would naturally create a very difficult challenge for any group that might attempt to reconstruct the risks posed by the Cerro Grande fire as a result of its geographic proximity to LANL. I believe that this study provides some important lessons for the design of future characterization and monitoring programs at LANL and other sites around the U.S. where environmental contaminants are vulnerable to extreme events such as fires, floods, tornados, etc.

4. The approaches used to develop runoff estimates, stream flows, and suspended solids content all seemed "mainstream", sound, and straightforward. I had no difficulty in understanding the logic behind these procedures.
5. The approach of dispersing the entire mass or activity of contaminants into the estimated storm volumes, yet considering the contaminant source to be infinite, seemed at first glance a bit novel and overly simplistic. Nonetheless, more rigorous approaches would have required much more information about the extent and depth distribution of contaminants in source areas, erosion susceptibility of the source areas, and much more complex models to deal with highly variable conditions and processes, which in turn vary spatially and temporally. After thinking about it further, I believe the approach used was reasonably matched to the information at hand. I think this approach is very likely to be highly conservative in most cases, and the comparisons of estimated model predictions with measured contaminant concentration data seemed in most cases to bear this out, with estimates generally exceeding measurements by 1-3 orders of magnitude.
6. The exposure analysis relied on rather arbitrary, but I think highly conservative scenarios for the direct or indirect human use of the water and riparian resources downstream of LANL. Parameters that distributed the contaminants between water and sediment, and that were used to estimate concentrations in plants, animals and fish, were also, I believe, very conservative. The choice of human exposure parameters was justified on the basis of their occurrence in respected publications. To me, the human behavior parameter choices seemed reasonable, and likely moderately conservative, even for individuals with unusual lifestyles.
7. The calculations of risk from the exposure quantities were very straightforward and based on rather standard slope factors generated by EPA in the case of chemicals, and ICRP-derived risk factors for radiation exposures. I believe the risk factors for radiation exposures have a very solid scientific basis, and the uncertainties about the values are probably fairly small, perhaps on the order of a factor of 3 or less. I have little first-hand knowledge on the accuracy and uncertainty associated with the slope factors for chemical exposures.

   RAC Response: *We agree that the uncertainty in the radionuclide risk factors may be fairly small, however, for many of the chemical slope factors the uncertainly can be quite large. To compensate for uncertainty in the chemical slope factors, EPA has included factors of safety that are sometimes several orders of magnitude.*

8. After review of the report, and considering the many conservatisms as well as the actual monitoring data for surface water and suspended solids, I have the very strong impression that real risks to real people as a result of the Cerro Grande fire, and more specifically the possibility of enhanced water-borne releases of contaminants from LANL property as a result of the fire, were, and are, extremely small. I have no doubt that the fire caused soils that may have contained radioactive and chemical contaminants to be more easily eroded and transported off LANL property than before the fire, but the increased water volumes and suspended solids contents associated with storm events could produce a diluting and dispersing effect on the contaminants. While this is generally undesirable if it moves the contaminants to locations where people can be exposed, the magnitudes of the exposures appear very low, even for those hypothetical people with unusually vulnerable lifestyles. The exposure and risk magnitudes, in reality, are highly likely to be one to several orders of magnitude lower that the upper-bound values calculated in the RAC study.

9. One consideration that could potentially result in reduced conservatism is the effects of soil/sediment particle size on the source area concentrations, and resultant concentrations in sediment and water at locations where the mean residence time of water is long enough for very fine solids to settle from the water column. For example, I am assuming that samples taken from potential source areas were analyzed in bulk form, and the reported concentrations, thus the assumed source quantities for this study, were based on bulk soil concentrations. However, it is well-known that the finer particles in soil often have considerably higher contaminant concentrations than do the bulk soil samples, and it is primarily the smaller size fractions that tend to remain suspended in the water column long enough to travel the greater distances. These fines are not likely to accumulate for long in streams because they settle slowly, and resuspend easily in large or rapid flows. However, they can accumulate in very quiescent areas in streams and reservoirs, and this material can then equilibrate with water and local aquatic biota. The Task 2.7 report appears to largely ignore this sort of particle size question, likely because of the probable lack of particle size information. I don't think this is of great consequence, however, because the modeling predictions are still considerably higher than the monitoring data, suggesting that the many conservatisms in the approach seem to more than compensate for the particle size bias and sorting which occurs in nature.

RAC Response: *There was no available information about particle size distribution, variations in erosion rates at source areas and for most source areas not enough information to characterize the spatial distribution of the chemicals and radionuclides. Therefore, all of the mechanisms that are described here, and that would affect the resulting downstream concentrations and their locations, could not be included in the analysis. Text has been added to clarify this issue.*

10. The general impression one might get about risks from the water pathway would seem to differ somewhat, depending on whether the information came from the Task 2.7 report, or from the Fact Sheet which I was asked to review. The fact sheet indicates "---risks—were small, less than the acceptable risks defined by the Environmental Protection Agency of less than 1 in 10,000." This kind of statement appears lacking in the Task 2.7 report. I think the report and Fact Sheet need very similar statements to avoid creating confusion.

RAC Response: *Statements about the magnitude of risks are now similar in the Task 2.7 report and in the Fact Sheet.*

Specific Comments:

Executive Summary

1. A number of minor editorial comments are offered. To save time, these are provided as hand-written marginalia for consideration by the report authors.

RAC Response: *We have made appropriate revisions based on these editorials comments.*

2. First bullet, page iv: The "maximum average" concentration needs a brief explanation or definition.
   RAC Response: *We have revised the text to indicate that the "**highest** average" concentration at any source area was compared to the PRG values.*

3. Second bullet, page iv: Indicate basis for eliminating water quality parameters, petroleum hydrocarbons & lubricants.
   RAC Response: *We have revised the text to provide the basis for eliminating these materials from further consideration.*

4. Top of page v: Indicate how POEs were positioned-I assume they were positioned to give maximum exposure values.

   RAC Response: *We selected points of exposure in areas where individuals represented by the scenarios were likely to be located that were immediately downgradient of source areas or at the outlet points of a watershed. In addition, to evaluate potential exposures to the highest potential concentration of chemicals and radionuclides, we conservatively assumed that the point of exposure for each scenario would be within a stream segment where the highest storm water flow and sediment concentration would be expected. These stream segments were identified as part of the delineation of the watersheds for the storm water flow estimates. The text has been revised to include this information.*

5. Items 2 and 3, page v and numerous other places in the report: The phrase "on the Rio Grande" or "on Cochiti Reservoir" appears frequently. I think this means "on the bank of " or "near the bank of ". This should be clear. Someone could live, I suppose on the water in a houseboat.

   RAC Response: *The reviewer is correct; the text will be revised to indicate that these phrases mean that the person lives on or near the bank of the river or lake.*

6. First bullet, page v: The assumption that untreated water is consumed is another conservatism that could be mentioned. Few educated people would do this, and if they did, the organisms in the water could be a bigger risk than the contaminants from LANL.

   RAC Response: *This assumption has been more fully explained in the report.*

7. Page vi, last paragraph, line 5: I think you mean that predicted sediment concentrations are much higher than measured, rather than background values. Is that correct?

   RAC Response: *The statement was intended to indicate the general magnitude of the apparent over-prediction for these radionuclides based on a comparison to measured values, and also to note that the predicted concentrations for these radionuclides are much higher than background, suggesting a significant contribution from the modeled source areas. We have revised the text to state this more clearly.*

8. Risk estimates section, page vii: The third paragraph would make a better introduction to this section. Also, this says a bit about comparative risks within the study, but essentially nothing about overall absolute risks to people from water pathways. This should have some statement(s) about risks from the fire burning on and above LANL, as was done in the fact sheet.

   RAC Response: *The paragraphs have been reorganized and text that provides some perspective to the risks has been added to this section.*

9. This summary does not explain the time of exposure (in years) assumed for the overall risk values. This needs explanation in this section.

   RAC Response: *The text has been revised to indicate this information.*

10. The executive summary should say a bit about the likely magnitude of risk over-estimation, something about the uncertainties, and something about the major conservatisms.

    RAC Response: *This information has been added to the executive summary.*

Chapters 2 & 3:

1. Page 2-50, first bullet: "Pre-fire concentrations *of which media* ?

   RAC Response*: The phrase " in water or sediment" has been added.*

2. Page 2-50, third bullet concerning Sr-90 and Am-241: I would think a possible explanation for this is volumes of water and sediment post-fire which increased proportionately more than releases of these radionuclides.

   RAC Response*: This possibility will be included in the discussion.*

3. Page 3-28, 2nd paragraph, line 3 and Table 3-8: I suspect the term "facies" is not likely to be understood by most readers of this report.

   RAC Response*: This term is defined in an earlier section of the chapter. We have added a cross reference for subsequent use of the term.*

Chapter 4, Estimating Concentrations at Points of Exposure:

1. Page 4-48, item 3: The assumption of all TSS in storm water over as source area resulting from the source area seems ultra-conservative, if I understand this correctly. I guess this means that no TSS from uncontaminated areas is assumed to be present. If this interpretation is correct, no wonder the estimates are 1-3 orders larger than monitoring data. If not correct, the wording needs clarification.

RAC Response: *For purposes of calculating the maximum potential chemical mass and radionuclide activity that could be present in suspended sediments, we did assume that all of the suspended sediment present in storm water over a source area resulted from the source area. This is conservative, but was done to address the limited PRS characterization data, the potential for unidentified source areas, and the limited characterization of some of the watershed reaches.*

2. Page 4-48, item 7: If dilution is the only attenuation mechanism considered, this adds further to the conservatism in the sense that sediment deposition in and along flood planes does not occur, nor does sorption of dissolved material to the stream bed. However, I think I remember that precipitation is assumed to occur across the entire area, which is seldom true for flood-producing thunderstorms. This assumption might increase the stream volumes and dilution factor more than adding contaminants. If so, this could potentially decrease the conservatism. Which, if either of these, is considered correct?

RAC Response: *Dilution is the only attenuation mechanism considered. No losses of sediments containing chemicals or radionuclides are assumed between the source areas and the exposure locations. Storms are considered to take place across the entire area at the same time, which does increase the volume of water, but also increases the amount of sediment eroded from all of the source areas and therefore increases the mass of chemicals and activity of radionuclides that are available for transport to the exposure locations.*

3. Page 4-49, 4th paragraph, line 7: The assumption of uniformity of contaminant concentration vs. soil depth is likely conservative if the contamination were deposited on an undisturbed soil. However, it may be possible that more contamination exists at depth in some of the source areas, for example, a burial ground or landfill. I guess this type of information is not complete for this analysis, correct?

RAC Response: *The amount of available concentration data varied significantly among source areas. For purposes of this analysis we selected analytical results for samples collected from PRS between zero and two feet below the ground surface. The estimated soil erosion depths indicate that the maximum average erosion loss was 0.75 meters, which is consistent with the selected depth. As result, chemicals and radionuclides present in soils below this depth were not expected to contribute to the concentrations of chemicals and radionuclides at the points of exposure. In addition, the limitation on depth of sample did not limit the chemical mass or radionuclide activity that was available at the PRS.*

4. Page 4-52, Table 4-21: I would agree that for the radionuclides, the K_d values chosen for the analysis are at the low end, and thus likely to be conservative for the risk assessment. I guess it would not be conservative, however, for external radiation from exposure to a sediment beach.

RAC Response: *It is true that the estimated radionuclide activity in deposited sediments along a stream bank may be lower at the point of exposure locations than if high end K_d*

values were chosen; however, the conservative assumptions about the sediment deposition and radionuclide activity attenuation should more than compensate for he difference.

5. Page 4-56, Table 4-23: The flow units need to be given (cfs, I presume). Also, Table 4-24 on the same page needs units for TSS.
6. Page 4-57, Table 4-25: Add units for TSS.
7. Page 4-58, Table 4-26: Add units for volume.

RAC Response: *Units have been added to these Tables.*

8. Page 4-66, Table 4-27: The table title should reflect that these data are based on model estimates, not real data. Also, units should be given for the last 4 columns.
9. Page 4-28, Table 4-28: Title should reflect that these data are model estimates.

RAC Response: *Changes in the titles for the tables have been made.*

10. Page 4-69, Table 4-29: The PRS area column should have units, I think.

RAC Response: *Appropriate units have been added to the table.*

I am struck by the magnitude of the post-fire soil erosion losses for PRSs. During some hikes around the hills in the Los Alamos Canyon last winter, there were some very steep hillsides in heavily burned areas that had lost perhaps up to a half meter of soil from post-fire rain events. However, in areas that were flat to moderately sloping in heavily burned areas, re-vegetation was impressive, and the amounts of soil loss were far smaller-seldom more than a few cm. This makes me wonder about the slopes existing on the PRS areas. Of course, if the PRS was a stream bed or adjacent flood plain, it would not have to be at all steep to get scoured deeply by flood water. The flood plain in Los Alamos Canyon has been deeply cut in narrow channels, but in many areas, the old surface has been covered by up to several feet of alluvium eroded from the hillsides. So, this really convinces me that much of the material eroded from PRS sites will likely reside in the canyon flood plains before reaching POEs for indeterminate time periods. All this convinces me even more of the substantial conservatism of the modeling approach and assumptions.

RAC Response: *We agree with the reviewer that not accounting for deposition and resuspension between the source area and POE likely contributes to the conservatism in our estimated concentrations. We also make the recommendation that updated erosion assessments be completed for identified important PRSs, so that the potential erosion at specific PRSs can be better evaluated.*

11. Page 4-75, Table 4-32: I think the data are expressed as ratios, but it is not explicitly clear from the table. This needs to be made clear. If these are differences rather than ratios, the units need to be specified.

RAC Response: *Changes in the title for the table have been made.*

12. Page 4-76, Table 4-33: The title is not adequate to describe what the table reflects. I think it should be something like "The effect of K_d changes on concentrations of benzo(a)anthracene in water, suspended sediment, and deposited sediment at POE 4.1b."

RAC Response: *Changes in the title for the table have been made.*

13. Page 4-77, Table 4-34: Same comment as No. 12 above.

RAC Response: *Changes in the title for the table have been made.*

14. Page 4-78, bullets: I like these statements-they help the reader a lot. The 3rd bullet from the bottom indicates "infinite" source areas. Does this mean infinite in area (m^2) of the source area, similar contaminant concentrations over depths, both, or ? This needs to be clear for conceptual understanding.

RAC Response: *The infinite source assumption means that the source area is not mass or activity limited. Based on the storm water flow and TSS value, the mass or activity will be available to be transported. It also means that the representative concentrations will be constant over the entire erosion depth.*

15. Page 4-79, 7th bullet: This raises a question. I think you assume a storm event occurs everywhere in the watersheds affecting LANL, the only variable being an elevation adjustment. This means that all PRSs would be affected simultaneously. Are all the PRSs and watershed areas above them assumed to have been burned, thus increasing erosion potential, or was it only the PRSs actually burned that were so-treated? This is probably explained somewhere, but I don't recall seeing it.

RAC Response: *The burned areas were determined based on the extent of fire information gathered, so only those source areas within the burned areas were considered to be burned. The storm events are assumed to occur over the entire LANL areas simultaneously, so all source areas are affected at the same time. For purposes of these calculations, the effect of the fire was addressed by changes in the curve numbers used to calculate the storm water flow. For source areas that were in burn areas, the curve numbers were adjusted to reflect the changes in surface conditions after the fire. This generally resulted in increased storm water flow over the source areas that were located in the burn areas.*

16. Page 4-80, bullet at top of page: I agree that not including particle size dependent behavior adds lots of uncertainty to the estimates. I discussed this in point No. 9 under General Comments. This approach adds conservatism, because larger particles are assumed to travel to POEs, when in fact much of this material will settle out and be deposited in up-canyon flood plains for some unknown period of time. However, neglecting particle size may detract conservatism if the finer particles that will reach POEs actually have higher concentrations of contaminants than the PRS characterization data would suggest. I suspect this would be the case, but this effect would likely be compensated for by dilution with uncontaminated fines that would also be transported in

flood waters. The best evidence of this is the comparison of model estimates with monitoring data, which indicates overall conservatism in the media concentration estimates.

RAC Response: *We agree with these observations and believe this further supports our conservative assumptions concerning the transport of chemical mass and radionuclide activity.*

Chapter 5, Risk Estimates:

1. Page 5-1, first sentence: I would suggest the words "potential risks" replace the word "exposures" after "range of—".
   RAC Response: *The text has been revised to include this recommendation.*

2. Page 5-3, first bullet, first sentence: I suggest "Potentially important—".

   RAC Response: *The text has been revised.*

3. Page 5-3, item 3, 3rd bullet: I suggest putting "is" between "which" and "irrigated".

   RAC Response: *The text has been revised.*

4. Page 5-3, item 4, subtitle: Might better read "—using the LANL site". First bullet under this might better read "This person represents someone involved in local cleanup—".

   RAC Response: *The text has been revised.*

5. Page 5-4, 5th line: I suggest: "—revegetates and watersheds regain—". Same page, same paragraph, last line: I think 7 years is a reasonable exposure duration. Substantial revegetation and soil stability should be evident by then.

   RAC Response: *The text has been revised.*

6. Page 5-4, 3rd paragraph: I would mention the 365 days/year exposure frequency assumed for residents along the Rio Grande.

   RAC Response: *The text has been revised.*

7. Page 5-6, item 5: Would read better as "Consumption of garden produce irrigated with river water"

   RAC Response: *The text has been revised.*

8. Page 5-7, Table 5-1: I thought the exposure duration was 7 years for the first four scenarios listed, not 1 year.

RAC Response: *The reviewer is correct and those values have been changed.*

9. Page 5-12, definition of RF_{ext}: A reference for this value was not evident, but should be, maybe on the previous page.

RAC Response: *The reference has been added.*

10. Page 5-15, first line: Make radionuclide plural.

RAC Response: *The text has been revised.*

11. Page 5-27, Fig. 5-2: Shouldn't the upper bar graph have the same x-axis title as the one below-"Chemicals in water"?

RAC Response: *Yes, that change has been made.*

12. Page 5-31, Table 5-5, footnote c: Does this belong here?

RAC Response: *That footnote has been deleted.*

13. Page 5-33, item 6: Might better read-"We did not account for BMP—". Also, spell out the meaning of BMP-in case the reader forgot it.

RAC Response: *The text was revised and BMP spelled out again.*

14. Page 5-34, item 7: I suggest replacing "removal" with "deposition in the flood plain", so long as this is what is meant.

RAC Response: *This refers to soil that LANL removed from the canyon.*

Chapter 6, Conclusions:

1. Page 6-1, 2nd paragraph, line 2: Add "of contaminants" after "concentrations".

RAC Response: *The text has been revised.*

2. Page 6-1, 2nd paragraph, last line: I would add "current" before "environmental". I don't necessarily agree that this could never be achieved with monitoring data. It would take a substantially revised monitoring program, but such a program could, I think, provide the necessary data. In fact, I would make a strong recommendation that a better designed program be planned and developed using this and other studies.

RAC Response: *We agree with the reviewer's comments. The text has been revised.*

3. Page 6-1, last paragraph: I would try to make sure this was qualified to apply to a hypothetical person exposed under a highly unlikely set of circumstances. Otherwise, this could be read by some as a pretty scary paragraph.

   RAC Response: *The text has been revised to emphasize the hypothetical nature and conservatisms built into these scenarios.*

4. Page 6-2, item (1): I don't think this is a "reasonable" conservative estimate. I think the words "quite unlikely" and "very conservative" better characterize the estimated risks. I would also say in this item, that even though the measurement results are of insufficient temporal and spatial coverage, they do provide strong evidence that the estimates are very conservative.

   RAC Response: *We have revised the text to state "...a conservative estimate...".*

5. Page 6-2, item (2): Here, I think a cogent statement would be that the effects of the fire on contaminant risks are not clear and consistent in terms of direction and magnitude. But I would recommend following this up with a statement similar to that in the Fact Sheet about the likely absolute total risks being very small and < EPA guidelines.

   RAC Response: *The text will be revised to reflect these considerations.*

6. Page 6-2, item (4), second sentence: I would add "measureable" before "concentrations". As to the last sentence in this item, I don't think I can agree with the statement. Many other phenomena could affect the magnitude in reality.

   RAC Response: *The text has been revised to reflect these considerations.*

7. Page 6-2, item (5), last sentence: I don't follow this statement. Effects of chemicals on what? Do you mean this circumstance limited our ability to estimate concentrations of contaminants generated in other canyons?

   RAC Response: *This bullet has been revised to indicate that we were able to evaluate the impact of contaminated canyon sediments for Los Alamos and Pueblo Canyons only, as characterization data for other canyons are not available.*

8. Page 6-2, item (6), next to last sentence: I don't understand what this is saying. Maybe some rewording would help.

   RAC Response: *We have revised the sentence.*

9. Page 6-3, item (8): Not clear. "did not differ significantly" *from what*?

   RAC Response: *The text should have indicated a comparison between the post fire risk estimates with the incremental risk estimates. The text has been revised.*

10. Page 6-4, Table 6-1: This is good. Under Chemical and radionuclide transport, describe "infinite source" more clearly. Under Exposure scenario assumptions, I would ask, are there actually such people now living like this? Under radionuclide risk coefficients, I would comment that this has received large research efforts for over 25 years. Under Risk estimates, do the overestimates also factor in conservatisms in various exposure scenarios, factors affecting exposures and the like?

RAC Response: *We have revised the text in the table to reflect these recommendations.*

ifeu-Institut – Wilckensstraße 3 – 69120 Heidelberg – Germany

F. Ward Whicker
Radiological Health Sciences
Colorado State University

Ft. Collins, CO 80523-1673, USA

ifeu – Institut für Energie- und Umweltforschung Heidelberg GmbH

Wilckensstraße 3
69120 Heidelberg, Germany

Phone+49-6221-4767-0
Fax+49-6221-4767-19
E-mail: ifeu@ifeu.de
http://www.ifeu.de

B. Franke direct: -23
E-mail: bernd.franke@ifeu.de

January 13, 2002

Review of RAC Draft Reports "Analysis of Exposure and Risks to the Public from Radionuclides and Chemicals Released by the Cerro Grande Fire at Los Alamos"

Dear Ward:

The following RAC report for the Cerro Grande fire risk assessment project was provided to me for review:

Task 3: Calculating and Communicating Risks: Observations and Recommendations, December 3, 2001 (RAC Report No. 15-NMED-2001-DRAFT)

My comments regarding the reports are enclosed. Please feel free to contact me if you have any questions regarding my contribution.

Best regards,

Bernd Franke

Encl.

Executive Director:
Dr. Ulrich H¬pfner, Dipl.-Chem

Scientific Directors:
Bernd Franke, Biologe
Jƒrgen Geigrich, Dipl.-Phys.
Hans Hertle, Dipl.-Ing. (FH)
Dr. Guido Reinhardt, Chem./Biol.
Prof. Mario Schmidt, Dipl.-Phys.

Court registration:
Amtsgericht Heidelberg
HRB 4263

Bank account:
HypoVereinsbank Heidelberg
Nr. 4880191204
BLZ 672 204 64

Tax ID: DE 143446610

Review of RAC Draft Report to Task 3 "Calculating and Communicating Risks: Observations and Recommendations"

These comments describe work performed by the Institute for Energy and Environmental Research based in Heidelberg, Germany (IFEU) under contract with the Department of Radiological Health Sciences, Colorado State University. Given the limited resources and the overall scope of the project undertaken by RAC, these comments are cursory in nature and focus on my particular areas of expertise.

The memorandum is concisely written provides a clear picture of RAC's current approach to the issues. According to RAC, two areas were examined for lessons learned, (a) calculating health risks from the fire; and (b) communicating health risks from the fire. I group my comments into these two categories.

Calculating health risks from the fire

Given the broad term "health risk from the fire", I would expect that the health risks from the fire be indeed evaluated in a comprehensive fashion. It appears, though, that the study focuses on a partial assessment of health risks form the fire, namely that related to radionuclides and chemicals associated with the LANL site. As I pointed out in my earlier comments, the project does not address the health risks associated with the increase concentration of particulate matter (expressed as PM10 or PM2.5). Given my calculations presented in my previous set of comments, it is quite possible that the calculated risk from PM10 or PM2.5 would be greater than that from all chemicals and radionuclides combined.

I note that the precise mechanisms for the induction of health effects from particulate matter (estimated from epidemiological data) are the subject of an ongoing and intense debate. According to one theory, it may be number of particles, according to another the chemical composition of the particles. I see little merit in focusing on the health risk due to radionuclides and chemicals alone without assessing the risk from particulate matter if the objective of the study is the proper assessment of risks and the communication of risk information. At the very last, the issue of health effects from particulate matter would become essential if combined effects exposure to particulate matter and chemicals were to be evaluated.

I note that the RAC team approach focuses on many substances which were identified in environmental monitoring, and then goes on to assess the contribution by LANL. In this process, RAC may determine that contribution from the LANL site to the concentration of a particular substance that was measured in air

was small or that is may be result of natural background. This approach should be expanded to include particulate matter.

I am well aware that this is not an easy task and I am unsure about the best way to address it. In the event that NMED made the policy decision not to focus on health risks from airborne particulate matter, it should be clearly stated. In that case, the implications of this restriction with regard to the study objectives should be evaluated and properly documented.

***RAC* Response:** Please see our responses to the Task 1.7 comments regarding this issue.

Communicating health risks from the fire

In light of the above, any communication about health risks from the fire would be incomplete and of little value if it does not address information about all pollutants with adverse effects on human health. This will allow members of the public to put the risks in perspective.

I note that RAC "does not seek to criticize the way in which events were handled". In my opinion, this term is overly broad. If factual errors were made in the process of risk communication during and after the fire, it should clearly be documented stated.

A systematic list of such errors (either in risk assessment or risk communication) would be helpful to determine the particular reasons for the errors and appropriate ways to avoid such errors in case of future events. As one example, the RAC report points out that NMED compared levels of chemicals in air samples to workplace standards. Acknowledging that a fast response is necessary, a good basis for a comparison may have been to use RfC values EPA's IRIS database.

***RAC* Response:** We have revised the statement noted by the reviewer to read, "[This report] does not seek to criticize specific groups and individuals." This report is not intended to provide a systematic list of factual errors; rather, it makes a number of recommendations to help improve the efficiency and credibility of calculating and communicating risks in the future. Implementing these recommendations would help minimize the occurrence of factual errors. In this sense, we have provided a very thorough critique of the way in which events were handled with the intent of improving the process in the future.

We agree that a system allowing immediate comparison of monitoring data to relevant values, such as background concentrations or appropriate protective standards would be a valuable addition to the current program capabilities. We make this recommendation in the report.

I would like to add another item for evaluation, the use of NEWNET data in public outreach efforts by LANL during and after the fire. At a public meeting in the Espanola in June of 2000, the NEWNET gamma dose rate information in offsite sta-

tions was plotted as a function of wind direction. The apparent absence of increased gamma dose rates was taken as a proof of a negligible impact of the fire. This presentation was clearly inadequate because of two reasons:

(a) If radioactive materials were released into the air from LANL site as a result of the fire, it would have mainly been particles alpha activity, which would not be detected by the NEWNET system.

(b) The NEWNET system inaccurately processed wind direction data; the presentation, which was distributed to members of the public, was factually incorrect.

I would like to point out that I do not see an intentional effort on the side of those presenting the data.

***RAC* Response:** We agree that this is an important issue. The recommendations we make about the importance of involving all stakeholders in the dissemination of information and the need to discuss limitations and uncertainties associated with available data are related to this issue of presenting information to the public in the most effective and defensible manner possible. Certainly, any inaccuracies involved with data collection need to be noted and corrected as they are identified.

T. HAKONSON REVIEW OF TASK 3

CALCULATING AND COMMUNICATING RISKS: OBSERVATIONS AND RECOMMENDATIONS

RAC's stated purpose for this task is to provide "conclusions and recommendations for monitoring, analyzing, and estimating risks from the fire to the public, including lessons learned from the fire analysis and the effectiveness of communication with the public during and following the fire." RAC states that this report on Task 3 is incomplete as Tasks 1 and 2 are not complete and risks via the different pathways have yet to be calculated. Additionally, RAC recognizes that developing ideal assessment data and procedures for complex source terms and geographical setting such as occurs at LANL is not trivial. Because of the incompleteness of this report, I offer the following comments for consideration in preparing the final report.

This draft of Task 3 consists primarily of generalities of how RAC intends to proceed in calculating and communicating post-fire risks. Much is made of the fact that data collected for risk assessment is somehow different than that required for compliance monitoring. If so, then it would be worthwhile for RAC to be <u>specific</u> as to the type of data needed for risk assessment and how it differs from other types of monitoring data.

***RAC* Response:** We will provide in our final report as many specific examples of how compliance-driven monitoring may not be ideally suited for a prognostic risk assessment, such as the one we have been contracted to complete. Our primary intent regarding this issue is to point out the benefit of fully identifying the goals of monitoring activities and understanding the limitations associated with that monitoring with regard to understand risk. For a predictive assessment, specific data requirements should be identified so that they can be met to the extent possible within the scope of compliance-driven monitoring and what is practical or feasible considering the availability of resources. There may be a number of steps that could be taken to improve and build upon the existing program(s) that result in a more defensible and useful end product. Further, we believe much of this could be accomplished with upfront funding that would be recovered in the long term through increased efficiency and credibility. Please see our responses to the LANL comments on this report for additional discussion in this regard.

For example, what specifically would you do to characterize the source of contamination to support risk assessment and how might that differ from compliance monitoring approaches? Does one need to do contaminant vs. soil particle size analysis, contaminant vs. soil depth, detailed spatial distribution measurements, or contaminant vs. soil organic matter content in order to collect data compatible with risk assessment goals? If so, then why is it needed, how will it be used, and how will it improve the risk estimates (i.e. reduced uncertainties?)?

***RAC* Response:** As stated in our report, data that quantitatively characterize contaminated areas across the LANL site before an emergency event are needed to support a defensible prognostic transport modeling effort. To understand these potential impacts, two basic pieces of information are necessary. First, it is necessary to establish some quantitative measure of existing contamination at each site. The minimum information requirements for this consist of an inventory or average concentration across a defined area, both in terms of surface area and as a function of depth. At the current time, the data do not allow average concentrations and/or inventories for known contaminated source areas at LANL to readily be determined. It appears that the data gathered at LANL have been directed primarily toward determining the nature and extent (what contaminants exist and how far are they spread) of contamination instead of the quantities that are required for a predictive risk assessment. More detailed information such as soil particle size or organic matter composition may or may not be important to consider for understanding and mitigating potential risk. The relative importance of this type of detail will depend largely on the results of initial conservative bounding calculations.

Second, with multiple contaminated sites to consider, it is critical to be able to readily establish those sites of most concern in the evaluation of potential risk. A systematic process is needed to quantify contamination at defined areas and to identify the most important areas in terms of potential exposure and risk. At the present time, it appears that LANL may not have such a system in place. Nevertheless, LANL has undertaken various efforts to prioritize and guide work at defined areas, but the criteria used for the prioritization encompass factors other than human health risk, and the groupings established for the rankings are not readily tied to the data that can be used to quantify contamination. As a result, it is not possible to specifically identify those sites that are most important in terms of risk. Consequently, some unnecessary effort has been required in an attempt to

characterize likely insignificant sites, and it is still not completely clear that all potentially important sites have been identified and included in our assessment. For some sites, there are no characterization data available, and it is therefore difficult to assess the consequences of omitting these sites from consideration in our evaluation.

It seems to me that the "data needed for risk assessment" as defined by RAC (to include source term characterization and modeling) is a highly questionable undertaking that is subject to the type (fire, earthquake, tornado, flood, terrorist action, accident) and nature (duration, intensity, location, season) of the "disaster" and that there is no one protocol of data collection for risk assessment that would satisfy all possible emergencies or combination thereof.

***RAC* Response:** The key to any type of predictive risk assessment, regardless of the type or nature of the "disaster," is the ability to quantify the amount of material at defined locations that could pose a risk to human health. Therefore, we do not entirely agree with the reviewer's comment that the primary pieces of information needed to understand potential risks differ significantly or are subject to the type of event. The reviewer is correct in stating that the set of defined locations that may be impacted is subject to the type and nature of the event. This project provides a good example of this in that the set of locations important for understanding the release of materials to the atmosphere is different from the set of locations important for understanding the release of materials to surface water.

The process and concept of modeling may be questionable in that there are often significant inherent uncertainties that can limit the degree to which potential risks can be estimated or quantified. However, at the same time conservative approaches can lead to bounding estimates of risk, which are particularly important for understanding the need or justification for further, more detailed evaluations. In addition, much can be learned about the relative importance of one site or contaminant versus another with regard to potential risk. A systematic process of characterizing sites and modeling potential transport can also lead to a greater understanding and consequently a more defensible, efficient, and timely method for prioritizing sites for cleanup. Further, the relative potential impact of the fire (or other event) on runoff and contaminant dispersion can be better quantified so that statements about potential risk can be made more defensible.

On the other hand, compliance monitoring would seem to be a pretty straight forward proposition that has been blessed by state and Federal compliance organizations while most disaster monitoring scenarios for risk assessment generally have not.

***RAC* Response:** We agree with the reviewer that compliance-driven or other environmental monitoring data present the most straightforward and least uncertain mechanism by which to understand and quantify contamination in the environment. Certainly, though, this is based on the assumption that the right locations for understanding the consequences of contaminant movement as a result of a given event are monitored and that the necessary analyses are undertaken to quantify the presence of all potentially risk-causing contaminants in the collected samples. It is also very clear that monitoring data are representative only of conditions existing before and at the time of their collection, and they do not provide a comprehensive assessment of potential future risks. The Cerro Grande Fire provides a good example of this limitation because the most immediate post-fire assessments were based on data collected during a relatively dry monsoon season that did not produce the heavy rain events that are possible, which could lead to significantly more contaminant movement.

We are not suggesting that additional monitoring be undertaken in an attempt to address all potential disaster scenarios. However, if there is the need or desire for a predictive assessment in response to a given event, basic data requirements independent of the nature or scope of that event should be identified so that those requirements can be met to the extent possible within the scope of compliance-driven monitoring and what is practical or feasible considering the availability of resources.

RAC states that " defensible and efficient calculations" are needed to identify health risk issues and answer technical questions about risk. The statement leaves the impression that risk estimates based on compliance monitoring data are not defensible or efficient. Some further discussion of what " defensible and efficient calculations" means is needed.

***RAC* Response:** We did not intend to leave the impression that risk estimates based on monitoring data cannot be defensible or efficient. However, as noted previously, efficiency can be improved through integration and consistency, and the limitations of calculations based on monitoring data must be made clear. Defensible calculations must be based

on a comprehensive and systematic assessment of potential contributors to risk. Efficient calculations and evaluations of data have been hindered primarily because of inconsistent methodologies for compiling data. We will revise the report to make this point more clearly.

RAC poses six questions that they believe should be answerable given the availability of adequate risk assessment input data (bottom of p. 3). The first four deal with characterization of the source term and transport of associated contaminants while the last two deal with compliance monitoring.

In a general sense, one could make the case that the answers to the 1st four questions are not needed to calculate the risk from an event such as the Cerro Grande fire. For example, one could argue that "defensible and efficient" calculations of risk/s could be made solely from the concentrations of contaminants/s in appropriate media at the point/s of public and worker exposure (question 5) and that this estimate could be made in the absence of source term and contaminant transport data. Risk estimates based on such monitoring data would then be compared to "background" data/risk estimates (question 6) (or applicable standards), as is now done by the compliance Group/s at LANL. I think that at least some regulatory compliance standards were derived from risk based criteria.

Compliance monitoring programs to support disaster scenarios at LANL would obviously require some enhancement to address what, when, where questions as well as satisfying the regulatory issues. In my opinion, interjecting source term information and the required parameterization needs of the transport/risk model/s, imposes potentially huge additional uncertainties into the final estimates of risk. Those additional uncertainties are absent in point of compliance monitoring programs.

***RAC* Response:** Please see previous responses regarding the value of predictive transport and risk assessments and the limitations associated with real-time monitoring data in understanding potential exposure or risk.

In the 3rd paragraph, p. 3, statements are made that compliance data may provide very little comprehensive information about contaminant movement from on-site to off-site areas during emergencies those data only provide a history of contamination at defined areas. I am not sure what RAC is trying to say here but compliance monitoring at LANL includes off-site areas and I think it is comprehensive enough to serve in calculating risks to the public

(i.e. air, water, foodstuff, fish, etc). Additionally, I think a "history of contamination at defined areas" (i.e. fixed point monitoring locations) is exactly what is needed to estimate risks from emergencies given that the frequency of sampling is keyed to the timing of the emergency.

***RAC* Response:** We agree with the reviewer that a history of contamination at defined areas is essential for understanding risks based on monitoring data. As noted previously, this is based on the assumption that the right locations for understanding the consequences of contaminant movement as a result of a given event are monitored and that the necessary analyses are undertaken to quantify the presence of all potentially risk-causing contaminants in the collected samples. We will revise this discussion to more clearly support our contention that the goals of and limitations associated with monitoring data must be clearly defined and stated.

The next paragraph discusses the need "--- to quantitatively characterize the sources of contamination sufficiently to support risk assessment from exposure---". The premise here is that there is a need to characterize sources of contamination because the compliance monitoring data may be inadequate for calculating post-fire risks. RAC makes this general statement several times in the report without any specifics of why compliance monitoring data are not adequate nor what data are specifically needed to conduct a "quantitative risk assessment" (i.e. to do it right).

***RAC* Response:** See previous responses to comments related to this issue. We maintain our assertion that monitoring data are not sufficient for making statements about potential future risks associated with contaminant transport through the environment.

I really doubt that the ability to "characterize the sources of contamination sufficiently" over the multiple source types and areas and complex terrain at LANL will ever exist to "quantitatively" support a risk assessment. Furthermore, any such methodology, without heroic additional efforts and bank breaking budgets, will only be able to distinguish order of magnitude changes on off-site transport of contaminants and not the small changes that would be typical of most post fire time periods. I think RAC recognizes that "adequate" characterization data either doesn't exist or if it does is in a form unamenable to a "quantitative risk assessment". In any case, if RAC thinks that source term characterization and modeling is the answer to conducting a

"quantitative risk assessment", then they should flesh out the specific data requirements for using that approach.

***RAC* Response:** We recognize that there is a practical limit to the extent that contaminated sites can be characterized and that this limit is established in large part by available funding and other resources. However, we believe efforts to further integrate and synchronize data collection could expedite and strengthen the process of characterizing contaminated sites. As mentioned previously, we believe much of this could be accomplished with initial funding that will be recovered in the long term through increased efficiency and improved perception by the public.

RAC's 3rd point (bottom of p. 4) is that there is a need for better communication, coordination, and consistency among the various groups collecting environmental data at LANL. This is a superb recommendation, but it's implementation will come too late to help RAC with the immediate need for characterization data.

The section about communicating health risks from the fire provides a good general discussion of the approach for communicating with stakeholders. I trust that LANL's location in Northern New Mexico generated some unique stakeholder concerns that will be elaborated on in the final version of the report.

***RAC* Response:** We intend to expand our discussion related to the various communications issues and conflicts that are presented by LANL's location in Northern New Mexico and the varied concerns of the many stakeholders.

Summary comments are as follows:

RAC seems to have adopted a source term characterization and transport modeling approach for estimating post-fire incremental risks to receptors.

***RAC* Response:** Because our scope of work called for estimating potential risks related to contaminants that could be mobilized by increased post-fire runoff following the fire, this approach was unavoidable.

Data for calculating risks using this approach likely do not exist at LANL.

The source term characterization and transport modeling approach adds large uncertainties to the final risk estimates over and above estimates based on compliance monitoring data.

***RAC* Response:** We recognize the limitations associated with data availability and modeling uncertainty. However, as stated in a previous response, we believe there are benefits associated with this approach in terms of understanding the potential for risk, identifying both risk-driving areas and contaminants, and in quantifying the incremental impact of the fire on risk. As has also been previously discussed, there are limitations to what can be learned about potential future risks based on environmental monitoring data.

The final report should have a much expanded discussion justifying the source term characterization and modeling approach, specifically identifying data requirements and providing approaches for estimating uncertainties associated with the risk estimates.

***RAC* Response:** As noted previously, a source term characterization and modeling approach is necessary to estimate potential future risks as required by our work plan.

The general discussion on communicating risks is good but specific recommendations relative to LANL and surrounding communities are needed.

***RAC* Response:** As noted in a previous response, we intend to expand this discussion.

Los Alamos National Laboratory
Comments on RAC Task 3"Calculating and Communicating Risks: Observations and Recommendations"

Jean Dewart, ESH-17
General

As stated by the RAC team, this document will change and develop. This is very appropriate. The RAC Team has also indicated that this report has been limited by available time and resources – this also appears to be true. The result of these considerations is that the RAC team (at this time) has not had the resources to do a thorough identification or evaluation of decisions made during the fire concerning environmental monitoring and risk communications. Thus, this report appears to contain only partial information – this makes some of the statements very frustrating, and seems to increase mistrust rather than alleviate it.

***RAC* Response:** The reviewer is correct in noting that the draft Task 3 report is an evolving document that was incomplete insofar as our analyses were incomplete at the time of its issue. This prevented us from providing complete and sometimes detailed recommendations and observations regarding issues related to both communicating and calculating risk. We do not want to increase distrust on anyone's part and will work to make the statements helpful but not frustrating or offensive to anyone. Our intention was not to provide specific details regarding the who/what/when of particular issues because, in general, these issues transcend specific individuals or organizations and relate to the entire process of communicating and calculating risk. However, we believe that some of the general observations and recommendations based on our review of material to date are appropriate in developing a methodology for understanding and communicating issues of potential risk to members of the public. We believe implementation of those recommendations to the extent possible will increase public trust in and the efficiency of the various LANL programs.

Making a complete evaluation of who did what and said what during the fire will be very difficult and time consuming. It's also pretty easy to criticize officials w/o crediting them for the many successes during the fire. Thus I would recommend that a number of general statements can be made and then move on to the recommendations. (That is to say, eliminate the partial information about what happened during the fire.) The recommendations provided on page 15/16, are, in general, reasonable and workable.

***RAC* Response:** We agree with the reviewer that a detailed assessment of who did what and said what during the fire would be difficult, time consuming, and not productive for anyone. We also believe it is outside the scope of this assessment. Rather, we have based our report on available information from the press, the state, and LANL related to understanding potential impacts of the fire. Reviewing data from various organizations allowed us as an independent organization to evaluate the utility and adequacy of the various data for assessing potential risks. This process has not focused on any single individual or organization but instead incorporates information from all organizations

involved. Information from the press is equally important to assess as it reflects the public perception of how events were handled and how information was disclosed. It is important also to understand that public perception may include the opinions and observations of many different groups of individuals often with widely varying opinions and attitudes toward LANL. Certainly, this creates unique challenges, and all must recognize that it may not be possible to please everyone all of the time.

The intent of this report was not to criticize groups or individuals, but to identify areas where improvements or changes could result in a more efficient, credible, transparent, or defensible end result, as is stated in the report. Also recognized in the report is the preparedness and hard work of many individuals under difficult circumstances. We believe, as the reviewer notes, the recommendations we provided are reasonable and workable.

General statements would include:

Routine environmental monitoring programs were not established for responding to emergencies, in general. Emergency response organizations have been established for providing environmental monitoring during emergencies. These organizations should use the results of the independent assessment to determine if changes in routine monitoring or emergency response monitoring should be implemented. The public should be involved in these evaluations. Consideration should be given to locations of monitors, pollutants monitored, and data management.

Communications in emergencies is critical. Pre-planning and training to the plan is required for successful communications. Issues that should be addressed include: how to handle preliminary data, how to include stakeholders, what standards are used for evaluating data, how to provide for Pueblo sovereignty concerning data collected on their lands, etc.

***RAC* Response:** We agree with the reviewer that these general statements are important and will be sure they are incorporated into our final report.

Questions:

It is unclear to me personally, how involving stakeholders in coordinating efforts for disseminating information about the emergency would work – more information on this recommendation would be helpful.

***RAC* Response:** Involving stakeholders could help alleviate public concern about the source of data and information availability, dissemination, and interpretation if the stakeholders were involved with LANL in the design and implementation of a procedure for use during emergencies. Increased public involvement may provide more understanding of some of the practical issues that can complicate data acquisition, compilation, and dissemination. It would also provide them with the opportunity to comment on the procedures used to communicate information as well as the content of

that information so that the procedures used and statements made are as effective as possible in meeting their expectations for having timely access to information that pertains to their health. In addition, increased reliance on individuals and organizations independent of LANL, both with disseminating and collecting information, would help alleviate the burden of being in LANL's current position of self-monitoring and providing the results to a skeptical public.

Although emphasis is given on independent agencies providing information on monitoring data and health risk, it is not clear what role the RAC team envisions for the NMED or the NM Department of Health. My understanding is that these agencies have the legal responsibility to provide information to the elected officials of the state in case of an emergency. But this is not discussed in the document.

***RAC* Response:** We believe any organization other than LANL (e.g., NMED, EPA, USGS, NM Department of Health) can be a valuable resource for independent verification of reported information. Certainly, legal requirements should be followed and this should not conflict with any of the recommendations we have made, but we will mention this issue in the final report. It will be important to develop procedures that allow for meeting such legal requirements in combination with the need to reach consensus agreement about statements that are made. It may also be wise to adopt some methodology to provide an official avenue for comment on the legitimacy of consensus statements.

Who are the independent monitoring groups discussed in the second bullet (recommendation) on page 16?

***RAC* Response:** Providing a mechanism for members of the public (e.g., organic farmers in the area or Pueblo members) to collect environmental samples and have them analyzed will help foster public trust in the results reported by State and Federal agencies. As noted in the report, any individual or group independent of State or Federal agencies would fit into this category. It will be important, however, to strike a balance between offering this type of assistance and a practical limit based on available resources.

Observation:

At recent meetings, the public and NMED have requested that RAC review the air quality impacts of the forest fire, not just impacts related to the fire burning on LANL lands. It appears that the public and NMED are not familiar with the extensive literature on air pollution from forest fires (although the NM Department of Health has presented information at a public meeting at San Juan Pueblo). The RAC team could assist the public and the NMED by helping them identify these sources of completely independent scientific literature that can be used to help answer questions.

***RAC* Response:** We agree that it is important to provide information about the constituents that would be expected in smoke associated with a forest fire to put perspective on releases that may be associated with LANL operational impact. We have

included a significant amount of information along these lines in the companion report discussing the Task 1 air pathway analysis (Rood et al. 2002).

Specific Comments: To provide some background on statements in the report that appear to be incomplete:

Goals of the Monitoring Program.

The document states (pg 4) that the goal of the monitoring data collection system in place before an emergency should be understood.

It does not appear that the RAC team evaluated the LANL emergency response plans or the plans and observations of the DOE Radiological Assistance Program (RAP) team during the fire in assessing the adequacy of emergency response monitoring for the Cerro Grande fire. The RAP monitoring teams were dispatched specifically to monitor the CG fire. Although the AIRNET system performed well during the fire, it has not been traditionally identified as an emergency response asset of LANL. The LANL environmental monitoring plan describes the purposes of the surveillance/compliance monitoring programs.

The document (pg 4) states "For example, for the air pathway, compliance data are collected at site boundary locations. These data may impart very little comprehensive information about contaminant movement from onsite areas to offsite locations during emergency situations, depending on the nature of the release. Rather, they provide only a history of contamination at defined locations, which may or may not be appropriate for understanding the consequences of an emergency". Similar statements are made on page 5.

These statements do not reflect the calculations done by the RAC team in analyzing the CG fire. The calculated maximum values from the RAC CALPUFF modeling indicate that the highest impacts from contaminated sites burning were on Laboratory property (TA-54) and in Los Alamos and White Rock – not at a distant downwind location. If the Cerro Grande fire produced highest impacts at/close to LANL, it is difficult to identify another emergency situation that would produce highest downwind air impacts far from LANL. In this sense, AIRNET (the compliance/surveillance network) was perfectly positioned to monitor the air impacts of the CG fire.

The existing history of contamination at defined locations is absolutely essential for understanding the consequences of an emergency – especially for the Cerro Grande fire – without the existing history, air measurements could not be evaluated to determine if they were the result of the fire or were routinely observed at LANL.

***RAC* Response:** The reviewer is correct in stating that a history of measured contamination at defined locations is important to understand the potential consequences of an emergency. However, those defined locations may or may not be well suited for providing information, depending on the nature of the emergency or event. As the

reviewer notes, it does appear likely that in the case of the Cerro Grande Fire the highest concentrations occurred at onsite locations and that will be noted in our final report. Also as the reviewer notes, the AIRNET system has not been traditionally identified as an emergency response asset for LANL. Reliance on the AIRNET system during the fire resulted in certain shortcomings with regard to data collection that the system was not designed to address, many of which are noted in the ESH-17 Lessons Learned report.

There may be other instances where data gathered to satisfy regulatory compliance requirements are not ideally suited for understanding risks associated with a particular event. This issue transcends environmental monitoring data and also encompasses monitoring data collected to characterize existing areas of contamination. For example, we have found it very difficult to utilize existing site characterization data, which presumably meet regulatory requirements, to develop the quantitative source term information necessary for the prognostic assessment of potential spread of materials through the surface water pathway that we have been contracted to complete.

Clearly this issue involves looking at emergency situations in a new and independent way so that existing resources are utilized but also recognizing that new procedures may be needed specifically for emergencies. We will provide additional discussion in this regard for the final report.

Managing the Risk Communication Response

It is difficult to identify the specific complete sequence of events after an emergency. Some of the statements made on page 11, appear to be incomplete. For example, based on a lack of an internal procedure (LANL 2001a), it is concluded that there were closed meetings. However, no information is provided as to who/where/when closed meetings occurred.

On the one hand, EPA is a part of the federal bureaucracy with whom NMED could not agree to a press release. On the other hand, EPA was the independent organization providing credible data. Who is questioning EPA's data because of what closed meetings?

***RAC* Response:** This statement was made based on information reported in press articles (e.g., Albuquerque Tribune, May 17 and 18) suggesting that delays in providing promised data and meetings among officials from the EPA, LANL, and DOE generated suspicion and lessened the credibility of the independent EPA monitoring. We do not have details regarding the who/where/when of these meetings, but believed it was important to note the existence of this perception by the public. It would seem that the lack of a procedure or consistent guidance for issuing joint press releases compounded the problem. Our goal with this report is to identify issues that appeared to generate public mistrust with the hope that the next time these issues are raised everyone is better prepared to deal with them.

Ken Mullen, ESH-18:

As we discussed, more specific comments would be greatly appreciated. For example the report makes reference to closed meetings. Identifying the specific closed meetings would help to correct the problem in the future. The report states, "Consideration should also be given to identifying and reducing unnecessary sampling to avoid duplicating efforts in an attempt to create a more refined, efficient, and integrated program." Again, specific suggestions would help us to see beyond our blinders to implement improvements. What efforts are being duplicated and what unnecessary sampling have you found?

***RAC* Response:** Please see the above response related to closed meetings. We do not have specific detailed information about such meetings, other than the public perception of mistrust they apparently generated, as noted in various news articles. Our observations and recommendations were designed to identify areas where some room for improvement exists in the process of providing information to the public.

With regard to creating a refined, efficient, and integrated program for monitoring, there may be some improvements that could assist with future data interpretation, both routine analyses and an efficient evaluation during and following emergency situations. We will provide as many specific suggestions as possible in our final report. One example that we will discuss in our final report involves LANL's extensive environmental monitoring program that includes sediment as well as surface, storm, and groundwater water monitoring. Both ESH-18 and the ER Project at LANL monitor various chemicals and radionuclides in these media, yet there is no indication of any sharing or integration of the resulting analytical data and consequent benefit from this apparent duplication of effort. It is not clear why the data collected and maintained by ESH-18's extensive water monitoring network would not meet the needs of the ER Project to understand contaminant movement. Likewise, it is not clear why sediment monitoring data collected by ESH-18 could not be used to augment the ER Project database to expedite and strengthen the process of characterizing canyon contamination, which has to date been completed for Los Alamos, Pueblo, and DP Canyons. This type of integrated approach could be extended to include monitoring data collected by other agencies or groups, such as NMED. We believe a more integrated approach would increase the credibility and efficiency of the various programs as perceived by the public.

On page eight the report states, "It is critical that a consistent and well-planned design be developed for compiling data. Following the fire, a statement was made that "In response to the fire and its impacts, we have increased our air, water and soil sampling to monitor environmental safety" (June 5, LANL Daily News Bulletin). While this is important, it is equally important that increases in sampling and consequent data collection be done with careful planning so the data can be readily used. A consistent and well-planned design would enable a more useful and rapid examination of data. The currently inconsistent methodologies maintained by the various data collecting organizations prevent an efficient interpretation and in some cases have resulted in a misinterpretation of the data."

Again, more specific comments and recommendations would be most appreciated. For this comment to be most helpful a description of what we have missed in our attempts to create a well-planned design for useful and rapid examination of the data. The more specific you can be the more useful your comments will be. For example, we have a routine in our database that compares analytical results to standards and risk levels so that we can quickly and efficiently screen the data. We also have routines that produce data tables so the data can be quickly examined in the context of other stations. We are constantly adding enhancements to our ability to examine and interpret the data. Your specific suggestions for things we should improve, look for, or compare to would be most helpful. It would also be helpful to know, specifically, where the data have been misinterpreted so we can correct that problem for the future.

***RAC* Response:** One of the focuses of this report was to determine if there are ways to enhance the data collection, compilation, and dissemination process, specifically during emergency situations. From our perspective, there may be some steps that can be taken to enhance data availability and interpretation among all the agencies performing monitoring duties on and around the site. Because of the volume of collected data, which increases annually, a well-planned and consistent design coupled with automated routines is essential for rapid and efficient data retrieval and interpretation, particularly by an independent organization, as well as effective data presentation to the public. The routines the reviewer mentions have not previously been mentioned or described to us, so it is difficult to comment on their usefulness. Based on the reviewer's description, they sound useful for the reviewer's organization but not likely extendable for use with data from other organizations. For example, the ER Project utilizes other routines or procedures, and it is of interest to adopt a set of common routines that could be utilized by all of the various different organizations collecting data.

We made a number of specific recommendations regarding data compilation in our Task 0 report, Evaluation of Data Compilation Process that addresses many of the reviewer's comments. These recommendations will be summarized in the final Task 1, 2, and 3 reports.

Misinterpretations of data have occurred primarily because of inconsistent naming conventions for sample collection locations and analyte names. For example, this lack of consistency resulted in our not identifying some historical background locations that would have been appropriate to incorporate into our analysis. It would be of interest to incorporate some mechanism in the data compilation design to identify all sample collection locations that could be considered as representative of regional background conditions or otherwise not impacted by LANL operations.

In several places this report observes that LANL's environmental monitoring programs are not based on a review of those sites that pose the greatest risk. On page four the statement, "It is likely that there is institutional knowledge about areas that contribute most to risk, but the current organizational system for the data is difficult to discern for an independent evaluator . . . but the criteria used for the prioritization encompass things other than human health risk, and the groupings established for the rankings are not

readily tied to the data that can be used to quantify contamination." On page six the report states, "A systematic process is needed to quantify contamination at defined areas and to identify the most important areas in terms of potential exposure and risk. LANL appears to lack such a system. One consequence is that there has been no clear implementation of an environmental sampling or surveillance system designed to specifically monitor the impact of the most important areas in terms of potential risk."

I recognize that in the very short time frame that RAC has had to learn about the site and to perform their studies much of the information that addresses these issues may not have been reviewed. LANL does produce an Environmental Monitoring Plan that describes the monitoring and rational. The annual Environmental Surveillance Report discusses the site, the operations, and the legacy sites that drive monitoring. In addition there are many reports that address specific sites, analytes, and issues. The environmental monitoring programs are designed from this knowledge base. While it likely that RAC would not have had time to review all these documents it would be appreciated if RAC would revise the above statement and other similar statements to acknowledge a significant, well documented, knowledge base forms the basis for environmental monitoring at the Lab.

An appropriate example might be the groundwater investigations using low-level tritium analysis. The analytical sensitivity employed gave detection levels orders of magnitude below risk levels. The study was done to identify the potential for future impacts to groundwater from the site using tritium to identify pathways from the surface to the deep groundwater before serious contamination occurred.

I believe that the Lab does a great deal of environmental monitoring with the focus of identifying future risks before they become present day risks. The Lab has recognized the potential for surface water to transport Laboratory derived contaminants and has implemented an unparalleled surface water monitoring program. I don't believe there is another site in the world that approaches the density or sophistication of LANL's runoff monitoring program. Clearly this is motivated by the complexity of the site and by the arid nature of the region. RAC's recognition of the significant efforts the Lab has gone to in the interests of good faith self-monitoring would be appreciated and appropriate.

***RAC* Response:** The bottom line is that in an emergency, information about (1) contaminated areas onsite and (2) past and present environmental monitoring data from all organizations should be readily available in one system to quickly and efficiently determine potential risk and to take appropriate steps, if warranted. It is not efficient to have to refer to a multitude of paper reports to identify key areas of contamination or to understand the many issues that can complicate interpreting the data.

From our review, it is not clear that LANL has implemented a systematic and comprehensive process to quantify contamination at defined areas and to identify the most important areas in terms of potential exposure and risk. Therefore, it is difficult to understand how the monitoring program is focused around those sites that are most important in terms of potential exposure and risk. However, we will expand this

discussion in the final report to incorporate the points the reviewer notes regarding the basis for environmental monitoring at LANL.

Thanks for the opportunity to review and comment on your work.

John Bartlit, ESH-DO

More than in technical issues, the issues of public communication often involve a choice of approaches to take in which either choice is subject to legitimate criticism from the public. This is so, because, unlike in technical matters, communication depends on the receiver of the message nearly as much as on the sender. Thus, conflicting advice and thoughts, all of which are valid, far more often appear in matters of communication than in science. The public discussions with RAC provide a number of examples.

I. At the public meeting of 12/12/01 in Santa Fe, RAC explained the need to keep Laboratory statements about risk well tied to thoroughly analyzed data that are well verified and sufficient in scope. This advice is clearly good. Yet, the competing need (and public pressure) is for rapid dissemination of the data. For example, letters in local newspapers have said "something stinks" at LANL, since the data took several weeks to come out--longer than the "time it takes to send a man to the moon." How can the Lab balance these two valid, but competing, needs? An obvious help is to have pre-existing, automated, interagency databases that contain the data to date in standardized forms, and which include some measures of context, such as EPA standards and/or historical trends, including the Cerro Grande Fire results. However, I believe such necessary actions will not be sufficient to end the public's legitimate, but conflicting, needs for quicker data and better-analyzed data.

II. A second example: For good reason, RAC strongly recommends that the Lab do more characterization of contaminated sites. Yet for years, the public has complained to the Lab about the excessive characterization of sites the Lab does. In many forums and in different ways (some polite and some less so), the public tells the Lab its scientists just like to characterize things, rather than do the decontamination actions. Again, both these public needs are legitimate, but they are competing and, to a great extent, are conflicting. Thus, the conflicting advice from RAC and the general public represents a special challenge for public communication.

III. A third example: RAC recommends that the Lab needs to present a clear, consistent story to the public. Doubtless this is sound advice and a worthy goal. Yet it conflicts, or may appear to conflict, with a public comment made near the end of the meeting. A comment was made to the effect the Lab does not allow its scientists to speak their views freely in public. The commenter called on RAC to have its report include a recommendation to provide "protection for whistleblowers." Surely, cases of outright lying are always possible. Putting any such case aside, the line between making "clear, consistent statements" and "muzzling a dissenting voice" is very fine indeed, and is 98% dependent on the eye of the beholder.

While no solutions exist for these problems inherent in human nature, I encourage RAC's scientists and communication experts not to gloss over the major communication

conflicts they represent. To some extent, the conflicts embody much of the vital question of public trust that communication seeks to address.

***RAC* Response:** The reviewer makes some extremely good points, particularly about the need to have pre-existing, automated, interagency databases that contain all relevant collected data to date in standardized forms, and which include some measures of context, such as EPA standards, background, and/or historical trends. We will expand the discussion in our final report to include these issues and the conflicts they may present with regard to the recommendations we make. We do believe improved communication with and involvement of all stakeholders will help alleviate the impact of these conflicts and assist with meeting the varied wishes of the public while at the same time moving effectively toward the goal of minimizing risks to the public.

One additional point: The RAC draft report on communication mentions the IFRAT and the PAG. The report suggests it is important that these means of communication be maintained. In practice, the following has happened: Both the IFRAT and the PAG have shrunk away of their own accord for the reason that RAC was hired to assess and communicate risks from the fire. The IFRAT and PAG both recognized the special abilities of RAC to do these tasks. RAC is seen as having superior resources of funding and intellect, as well as a greater independence, for doing the job. This widespread recognition has contributed hugely to conscious decisions by the IFRAT and the PAG to be much less involved.

***RAC* Response:** We will include discussion related to this point in the final report.

Thank you for considering these further viewpoints

Comments on RAC Technical Memo For Task 3: Calculating and Communicating Risks
Kirby Olson
12/14/01

The analyses under this task should probably be divided into two sections: emergency response and mitigation/monitoring phase. The distinction isn't clear in the draft; in some spots there's confusion about whether the suggestions are for modification of emergency response approaches or how to handle the dispersal of information after the fire.

***RAC* Response:** We intentionally divided the Task 3 report into two primary sections that we considered to be of greatest importance with regard to assessing risk related to the fire: 1) Calculating health risks from the fire and 2) Communicating health risks from the fire. Those divisions were intended to address the observations and recommendations that we are in a position to provide, based on the information we have obtained, reviewed, and utilized as part of this project. We made recommendations to assist with improving the process of communicating potential risks to the public and of calculating and understanding the magnitude of those risks from a technical standpoint. These recommendations apply to the ability to respond (both immediately and long-term) effectively to any emergency resulting in potential risk to members of the public related to the release of hazardous materials to the environment. An effective response necessarily includes obtaining and compiling information in a timely and efficient manner, analyzing and interpreting that information in a technically defensible manner, and conveying the results of that analysis in a way that is perceived as credible by the public.

A separate section for "emergency response" may not be appropriate for this Task, as this general topic includes many issues unrelated to exposures and risks to the public from the fire. An emergency response involves many facets, some of which are related to coordinating environmental monitoring, compiling data, and disseminating information to the public. These are examples of the emergency response issues that are relevant to our analysis of the Cerro Grande Fire. We will make this distinction more clearly in the final report.

The "Emergency Response Background" section needs rewriting. The Cerro Grande fire was NOT a "radiological emergency". It was a wildfire that burned out of control across jurisdictional boundaries and into residential and industrial area, potentially releasing hazardous materials. Disasters of this type clearly fall under the Incident Command System (ICS), developed to coordinate interagency responses and information releases in the wake of the 1970 California wildfires that burned into residential areas in several fire districts. ICS is used by essentially all hazmat teams and fire departments (as mandated by OSHA 1910.120); the emergency response phase should be evaluated in light of how this system was implemented. Information on the structure of ICS is available from FEMA, state emergency management agencies, or HAZWOPER training contractors. This section also discusses building an emergency operations center; it's unclear how this

would be preferable to the Emergency Operations Center at the Santa Fe Public Safety Complex which was used during the fire.

***RAC* Response:** In the documentation describing the FRMAC, the term "radiological emergency" is described as applicable to "an accident, an incident, a potential accident, or a potential, perceived, or deliberate act to spread radioactivity in the environment." The documentation describing the FRERP states "The FRERP covers any peacetime radiological emergency that has actual, potential, or perceived radiological consequences...that could require a response by the Federal Government. The level of Federal response to a specific emergency will be based on the type and/or amount of radioactive material involved, the location of the emergency, the impact on or the potential for impact on the public and environment, and the size of the affected area." The FRP documentation notes in describing a disaster condition "A natural or other disaster could result in numerous situations in which hazardous materials are released into the environment." It is clear that the Cerro Grande Fire could be categorized as a "radiological emergency", according to definitions noted in the plans designed to address such emergencies. It is also clear that there are a number of plans designed to help respond effectively and efficiently to an emergency situation, and we are not advocating or recommending the use of one plan over another.

All of these plans are developed to help organize and manage a coordinated interagency response and release of information to the public in the event of an emergency. The ICS, FRERP, FRMAC, and FRP all fall within this general description, with an added focus on emergencies involving radiological or hazardous materials for the latter three, which may be useful in providing guidance where the release or spread of radioactivity is possible. For example, the FRMAC's description of support capabilities includes gathering radiological information and data; providing results of data collection, sample analysis, etc.; and compiling a complete database containing all off-site radiological monitoring and sampling data. These are issues relevant to the Cerro Grande Fire that would not likely be covered by the ICS.

The Emergency Response Background section was intended to provide *background information* on existing plans and capabilities to deal with emergencies. We will clarify the purpose of this section and include additional text to describe the role that the Incident Command System could play.

The primary intent of the comment related to LANL's plans to build an Emergency Operations Center was to note the importance of such a facility for coordinating efforts during an emergency and not to recommend one facility over another. However, the Emergency Operations Center noted in our draft report as planned by LANL would presumably be located in close proximity to the Laboratory, which could be advantageous during an emergency related to LANL. We will revise this statement to more clearly emphasize the intent of the observation.

Under the recommendations section, the deliverable states "the agency creating the risk should not be the initial or primary source for communicating risk to the public". What

agency are you accusing of *creating* the risk? Are you saying that LANL created the risk by existing downwind of a forest fire? Are you saying that the National Park Service created the risk by injudicious burning upwind from hazardous waste sites? And what other agency would be the independent source? Under the Incident Command System, information for the public is channeled through the lead agency to the one information officer representing all the agencies involved in the response.

***RAC* Response:** The intent of this recommendation was not to accuse anyone of creating the risk with regard to the Cerro Grande fire. Because the fire occurred in the LANL environs, there was a desire (by the public and others) to understand how the fire in combination with contaminated sites created as a result of LANL operations could result in risk to members of the public. In that sense, LANL became the focus as the fire burned toward the site, and there was widespread interest in understanding the potential risks related to its operational impact on the environment. We will clarify the wording in that recommendation to convey this more clearly.

We were also writing from the perspective of public information during and after the fire. The purpose of this recommendation was to suggest that there might be alternatives for communicating risks to the public when those risks are related to a certain facility. A source that is independent from that facility is in a much better position than the facility itself to present information that the public will trust and believe. We are not recommending any particular source other than one that is independent from LANL, in this case. However, the public needs to be aware of the organization and or the spokesperson providing the information. It may not have been completely clear to the public that information for the public was channeled through the lead agency to the one information officer representing all the agencies involved in the response during the CG fire.

COMMENTS ON TASK 3: Calculating and Communicating Risks: Observations and Recommendations

FROM: John Pinder

General Comments

This report summarizes the problems in trying to compute and communicate risks to the general public from the existing data bases. It includes both the problem of immediate response during the emergency as well as problems in computing risks subsequent to the end of the emergency. Many of the comments may be expected from a previous reading of RAC reports where problems in lack of data, lack of coordinated data, lack of ready access to data, and lack of documented biases and problems with available data clearly limit the ability to compute risk assessments. As might be expected, the report focuses on the problems of data availability and makes a number of excellent suggestions on how to improve the data bases for future emergencies. Almost all of their suggestions and recommendations are valid, thoughtful, and worthy of serious consideration and action. My main comment would be that they perhaps do not go far enough and may be emphasizing a too limited approach.

Even if data were readily available and well documented, the problem of trying to make rapid risk assessments under emergency conditions would be subject to errors of the moment imposed by time restrictions. Even within the context of their current reports, RAC personnel have commented on the limitations imposed by time constraints.

Perhaps an alternative approach, an approach none the less based on all their suggestions of data availability and quality, would be preferable. This approach would be based on pre-existing screening models on possible risks for each PRS and each credible accident or emergency. In this approach, conservative pre-existing risk estimates would be available for immediate communication of risk assessments to the public and implementing plans for emergency response.

This would be more expensive but would provide increases in risk assessment quality that may not be immediately apparent. For example, on page 7 the report discusses DOE implementing a "data collection and compilation team" (DCCT) and a "risk evaluation team" (REM) with the DCCT providing data to the REM. Although this is an improvement, the types of data that DCCT should be collecting should be driven by the results of risk assessments for the various PRS and contaminants. The current RAC report and previous RAC reports clearly indicate that important data – much of it related to off site processes – has not been collected or is not being collected. Moreover, the presence of these pre-existing risk assessments could clearly affect allotment of emergency response resources as well as remediation and restoration priorities. Without

feedback from risk assessments on what data is needed to improve assessment accuracy, the data collection procedure may be driven by other priorities.

The existing RAC screening models and their often innovative approaches to a wide variety of potential contaminants is a worthwhile starting point towards a more complete assessment of potential risks from LANL waste sites.

***RAC* Response:** We appreciate the reviewer's recognition of the value of consistent and integrated methods of data collection and compilation across all organizations collecting data. As we have discussed in responses to other comments on the Task 3 report, we agree that there are longer term benefits associated with the screening and modeling approach we are using for this project in terms of understanding the potential for risk, identifying the risk-driving areas, and in quantifying the incremental impact of the fire, or other event, on risk. There are also a number of existing procedures and activities that can be built upon and integrated to develop a more efficient system. This will initially require a significant amount of effort along with input and assistance from many different organizations and a willingness to adopt new procedures and methodologies in some cases. However, we agree with the reviewer that the long-term benefits will far outweigh these initial efforts and costs with increases in both credibility and efficiency.

Specific Comments

Page 2

A paragraph begins "In many respects, this technical memorandum is incomplete because the project is not close to completion ..." Yet the resulting document is a clear statement of the problems of data existence and availability that are apparently limiting the current risk assessments and will, if not addressed, limited future emergency responses.

Page 4

The point that the report makes concerning compliance driven monitoring programs is a good one. Compliance monitoring is directed by laws and regulations that do not consider infrequent and potentially catastrophic accidents. The recent outbreak of fires at western DOE sites has clearly indicated the problems with simple compliance monitoring.

"Thirdly, there is a need for increased coordination and communication among all groups collecting environmental monitoring data." Some central organization should perhaps be funded to compile and coordinate these data. It should be remembered that no matter what federal or state agency collects these data, they ultimately belong to the public, especially the public that is at potential risk.

***RAC* Response:** We believe this is one of the most important points to convey. The long-term goal should be one that provides an efficient and consistent system for data compilation to enable ready and understandable access to data by both independent researchers and members of the public. This cannot be accomplished by Federal and State agencies alone and will require input by the public as well as coordination and guidance by an organization that is independent from the agencies collecting the data. Significantly greater involvement by area Pueblos would be one option. This step could help resolve the time delay issue that currently results from the need to provide the Pueblos with an opportunity to review all data related to samples collected from their lands before making those data publicly available. It is also appropriate considering the location of Pueblo lands and their proximity to the LANL facility. In addition, increased reliance on individuals and organizations independent of LANL would help alleviate the burden of being in LANL's current position of self-monitoring and providing the results to a skeptical public.

Page 5

"A valuable piece of information that seems to be missing is a systematic evaluation of the transport mechanisms that control movement..." This is an important point that is clearly documented by some of the RAC efforts to model air and water transport in LANL domains. Yet, the recommendations of this report do not necessarily address this lack of data. If all existing monitoring data were readily available, these data would still be lacking. If, however, the screening model approach was implemented, then the missing pieces of transport information would be identified in the modeling process.

***RAC* Response:** We agree that the screening and modeling approach we are developing and using for this project will assist with guiding future efforts to recognize the potential for risk, identify risk-driving areas and contaminants, and understand what pieces of information may be missing or where additional refinement of sensitive parameters is warranted.

"Thirdly, data should be collected and stored in a consistent and easily retrievable form." See the second comment about Page 4 above.

Page 6

In the discussion concerning consistent approaches to monitoring surface water it is perhaps worth mentioning that this problem is complicated by the intermittent nature of surface water flows at LANL. There is always air to be monitored, but the presence of flowing waters is sporadic. Even with regard to air, there are occasions of unusual events that are likely to be of large importance in the resuspension and redistribution of airborne contaminants. Some consideration of greater effort and resource expenditure should be considered for the monitoring of sporadic or unusual events.

***RAC* Response:** We agree that it is important to clarify the difficulties and complications associated with monitoring the intermittent and highly variable water flow in the LANL environs. These difficulties give additional credence to understanding the potential for contaminant migration based on quantitative evidence of what is present at the sites known to be contaminated.

Page 7

"Additional work is needed to establish a systematic process for quantitatively characterizing the contaminated sites that are identified as most important in terms of risk to human health." This risk must include the potential transport of contaminants under emergency situations and not just the simple resident farmer scenario currently in used for many contaminant sites.

***RAC* Response:** We agree that it is important to consider the potential for contaminant transport under emergency situations when evaluating sites for their importance in terms of human health risk.

Page 8

"it is important for agencies to provide *all* collected data..." Again see the second comment in regard to page 4 presented above.

Page 12

"There is a clear need to coordinate the data analyses so that preliminary risk results are available on a timely basis ..." Rather, there should exist preliminary risk assessments that could be easily and readily calibrated by ongoing data collection.

Pages 15 and 16

Yes.