G-000-105.27

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OEPA COMMENTS ON SITE-WIDE CHARACTERIZATION REPORT

10/05/92

OEPA/DOE-FN
12
COMMENTS

[redacted]

OEPA COMMENTS ON
SITE-WIDE CHARACTERIZATION REPORT

3799

General Comments

1. Ohio EPA previously expressed concerns about the availability of site specific background data (ground water, soil, & surface water) in our comments (12/91) on the Background Soil Sampling and Analysis Plan. Our review of the Site Wide Characterization Report (SWCR) has supported those concerns. Detailed below are specific concerns with background data for perched and buried valley aquifer groundwater as well as surface water. The general lack of surface water background data and the questionable quality of the groundwater background data will be increasingly important with the development of the operable unit specific Remedial Investigation reports.

   DOE should include a discussion concerning background analyses for each media within Section 4.0. These discussion should provide criteria for background locations and detail background concentrations and UTLs.

2. Consistency should be maintained within the report when using chemical names (e.g. trichloroethylene/trichloroethene, tetrachloroethylene/tetrachloroethene/perchloroethylene). Such consistency will assist the non-technical reader in following contaminants throughout the text.

Specific Comments

1. Pg. ES-5, lines 12-14: Aroclor-1254 is not a VOC, but is a PCB. Please correct text.

2. Pg. ES-6, line 10: Correct "this streams" to "these streams."

3. Pg. ES-9, line 16: Correct "1.540 pCi/g" to "1,540 pCi/g."

4. Pg. ES-9, lines 17-19: The Inactive Flyash Pile Removal Action RSE reported much higher concentrations of uranium from the CIS sampling. Please review these data and revise the 'text.

5. Pg. ES-14, lines 8-10: This sentence is unclear and fails to define what is being compared. Define what "were markedly higher than the upstream station."

6. Changes Resulting ..., Pg. ES-15, lines 20-27: Monitoring data should be available for the Pit 6 Removal Actions which has been completed.

7. Pg. ES-23, line 17: It is unclear what is meant by the text

"insects (earthworms)". Earthworms are not insects. The text needs to be rewritten to clarify this.

8. Pg. ES-23, lines 24-29: DOE fails to define what it believes is a "significant source of risk" if a hazard index (HI) of 5.0 isn't. HI's exceeding 1.0 for four inorganic contaminants would seem to be a source of risk. DOE should define within the document what it is considering a "significant source of risk" when making such conclusions.

9. Pg. ES-23, lines 30-35: The paragraph should include a discussion of the risks posed by external radiation doses to terrestrial organisms.

10. Pg. ES-28, line 2: Delete the word ",through".

PART I

11. Investigation and Results, Pg. S-4: This section is a reiteration of that previously stated in the Executive Summary. Please incorporate revisions based upon previous comments into this section also.

12. Section 1.2.2, pg. 1-9: Include the Experimental Treatment Facility Removal Action into the list of those previously completed.

13. Section 1.2.2, pg. 1-9, lines 12-13: This sentence is referring to the Silos 1 and 2 Removal Action, but it is not the "latter" in the list. Please correct list or text.

14. Section 2.1.3.1, pg. 2-11, lines 23-16: The text needs to be revised to reflect the content of the work plan actually submitted by DOE. Otherwise, the uniformed reader will be expecting DOE to follow through with this action.

15. Section 2.1.3.2, pg. 2-12, lines 25-26 and 31-32: These sentences must be revised to reflect the fact that the lime sludge ponds are RCRA unit due to the "routing" of hazardous waste to them. See Table 2-5, pg. 2-21.

16. Table 2-3, pg. 2-16 thru 2-18: The table refers several times to "tetrachloroethylene" and "perchloroethylene". These are the same chemical and should be referred to as tetrachloroethylene. Please correct the table.

17. Section 2.2.5.2, pg. 2-62, lines 7-12: The text is unclear in stating which of the habitats listed are being discussed in the referenced lines.

18. Section 2.2.5.5, pg. 2-71, Indiana Bat: The section should discuss the fact that although no bats were found within the FEMP boundary, echolocations for the same genus were detected and that mist net locations within the excellent\good habitat on-property were generally not conducive to bat capture. The data gathered under the RI/FS survey fail to eliminate the possibility that Indiana bats are using habitat on the FEMP property.

19. Section 2.2.5.5, pg. 2-74, line 35: DOE must define the habitats being used at the FEMP by the northern harrier and consider the potential effects remedial actions will have on this state endangered species. Additional information concerning this organism will become increasingly important as DOE begins the development of Feasibility Studies and assess compliance with ARARs.

20. Section 3.1.2.8, pg. 3-54, bullets: DOE should incorporate additional inorganics antimony, beryllium, thallium, and vanadium into any future surface water sampling due to their presence as site contaminants.

21. Table 3-18, pg. 3-81: The table states that RCRA groundwater samples were analyzed for perchloroethylene and tetrachloroethylene. These are generally synonymous names for the same VOC. DOE should define what perchloroethylene is.

22. Section 3.1.5: This section should include a discussion of sampling results from the following investigations/removal actions: 1) Waste Pit Area Removal Action, 2) ETF Removal Action, 3) EWMF Sampling and Analysis Plan.

23. Table 3-22, pg. 3-108: This table is confusing and is not explained in the text. Additional description should be provided in the text, title, and/or footnote.

24. Table 3-23, pg. 3-113: This table is confusing and is not explained in the text. Additional description should be provided in the text, title, and/or footnote.

25. Table 3-26, pg. 3-143: Correct Spotfin shiner to <u>Notropis spilopterus</u>.

26. Section 3.1.7.3, pg. 3-171, line 12: Define the acronym RVR the first time it is used.

27. Section 3.1.7.3, pg. 3-174, lines 32-35: Discuss within the text the reason for acquiring sediment and water samples from

New Jersey rather than an upgradient control area.

28. Table 4-3B, pg. 4-10: Were any of these samples liquids or TCLP analyses? If so, the results should be reported in mg/l.

29. Section 4.1.3.1, pg. 4-40, lines 25-27: This sentence is not complete and needs to be rewritten to clarify.

30. Table 4-22, pg. 4-55: The table does not include the footnotes referenced within it. Please correct.

31. Table 4-23, pg. 4-56: a) The range on the table should be reported as ND-"#.#" when the frequency of detection is less than 100 %.
b) The frequency of detection should be reported as 0/"#" when frequency of detection is 0%. This will allow the reader to know the number of samples collected.

32. Table 4-24, pg. 4-58: a) See previous comment 31 on table format.
b) RI/FS sampling should have included analysis for contaminants including antimony, beryllium, thallium and vanadium.
c) Pg. 4-59, Mercury: The average for "Between..." should be more significant digits than 0.0. Please correct or justify.

33. Table 4-25; Table 4-31; Table 4-32: See previous comment 31 on table format.

34. Tables 4-31 & 4-32: These tables fail to provide any upgradient or background sampling data. DOE should provide this information and if it is not collected must address this data gap immediately.

35. Table 4-33, pg. 4-80: The table suggests that Technetium-99 was not detected during RI/FS sampling. This fact should be discussed in the text, since it was regularly detected in the environmental monitoring data (See Table 4-28).

36. Table 4-34, pg. 4-81: a) See previous comment 31 on table format.
b) When one sample is collected and a detection is made the range should be reported as a single value not as two.

37. Section 4.1.3.3, pg. 4-84, lines 14-15: DOE should provide a summary of the sampling results in a table.

38. Section 4.1.3.3, pg. 4-84, line 15: The text should define the background levels being used to make such an assertion.

39. Table 4-36, pg. 4-85: DOE should provide the reason for including 1,1,1-TCA, TCE, PERC in the table when analyses for these constituents were never requested.

40. Table 4-39, pg. 3-95: See previous comment 31 on table format.

41. Table 4-40, pg. 4-96: See previous comments 31 and 36 on table format.

42. Section 4.1.3.4, pg. 4-100, Surface Water: A table should be provided to more clearly provide this information.

43. Section 4.1.3.6, pg. 4-103, last paragraph: The text fails to include a discussion of the results of the general chemical parameters analyses. A discussion of these data should be included in the text.

44. Section 4.1.4.1.1, pg. 4-104: A reference should be included in the text to the figure which provides the location of these wells.

45. Section 4.1.4.1.1, pg. 4-105, bullets: A search of Figure 3-20 did not locate well 12-3(2123). It is essential to provide references to figures locating the wells being discussed.

46. Section 4.1.4.2.2, pg. 4-111, lines 28-31: When referencing "DOE guidelines" the basis for these guidelines should be provided. Or more importantly included relevant MCLs.

47. Section 4.1.4.2.3, lines 13-14: USEPA has proposed an MCL for nickel which should be included in the discussion.

48. Table 4-45, pg. 4-116: The table fails to include footnote "e". Please correct.

49. Table 4-50, pg. 4-132: Define "trace" under well 1019 for Methylene chloride.

50. Section 4.1.4.4.1, pg. 4-137: The range of uranium concentrations and the presence of several VOCs in the "background" wells chosen questions the integrity of these locations as background. DOE should provide a more indepth discussion of the selection criteria for these wells.

51. Section 4.1.4.4.1, pg. 4-142, lines 34-36: Due to the high concentration UTL for inorganic constituents, a comparison should also be made to the respective MCL and proposed MCL when available.

52. Table 4-53, pg. 4-145: The fact that a number of inorganic contaminant UTLs exceed their respective MCL or proposed MCL supports the question of the integrity of background wells.

53. Section 4.1.4.4.1, pg. 4-149, lines 4-5: Molybdenum was detected in several 1000 series wells (see Table 4-54). Correct the text.

54. Section 4.1.4.4.2, pg. 4-157, lines 20-35: a) The selection criteria for wells for background concentrations is not provided. A discussion of the criteria should be provided. This is especially important in light of the fact that the draft "Groundwater Report" shows little if any information available on the construction of several of the "background wells."
b) Provide a reference to the figure(s) locating the listed wells.

55. Section 4.1.4.4.2, pg. 4-159, lines 29-32: The presence of multiple organic contaminants within the background well samples calls into the locations of the background wells as well as the quality of the laboratory data.

56. Section 4.1.4.4.2, pg. 4-160, line 6: The text on page 4-159 suggest Tc-99 is a laboratory contaminant in background wells yet an UTL is provided here. DOE should discuss this within the text. Additionally the presence of Tc-99 at such an level would otherwise suggest a poor background location.

57. Figures 4-2 and 4-3: These figures provide data only through the second quarter of 1990, certainly data is available from the second quarter of 1990 up to December 1, 1991. These figures should be revised to include data available up to Dec. 1, 1991.

58. Section 4.1.4.4.2, pg. 4-168, line 4: Table 4-59 provides "elevated metal concentrations" not "metals detected". Please correct the text.

59. Table 4-60, pg. 4-176: DOE should have analyzed background samples for all metals listed as well as antimony. The reasoning for not acquiring adequate background data is unclear. DOE should have conducted analyses for all metals detected or expected to be present as contaminants on the site.

60. Pg. 4-186, line 26: Correct "band" to "bend."

61. Figure 4-6: a) The figure should provide all sampling

locations and concentrations used to develop contours.
b) The figure should include a reference to the report/document which generated it.

62. Table 4-68, 4-198: See previous comment 31 on table format.

63. Table 4-70, pg. 4-203: a) Cr and 1,1 Dicl should not be included if no detects are reported.
b) DOE should describe the circumstances under which a blank becomes contaminated with barium.

64. Table 4-73A, pg. 4-215: a) See previous comment 31 on table format.
b) pg. 4-216: Continue column headings on additional pages.

65. Section 4.1.6.1, pg. 4-236, lines 15-18: The relevance of radionuclides being present at less than 600 pCi/g should be included in the section.

66. Section 4.1.6.1, pg. 4-236, lines 25-27: The relevance of radionuclides being present at less than 500 pCi/g should be included in the section.

67. Section 4.1.6.1, pg. 4-236, lines 31-32: The relevance of organics being detected in excess of 500 ug/kg should be included in the section.

68. Section 4.1.6.1, pg. 4-266, line 28: Define "traces" or rewrite text.

69. Table 4-76, pg. 4-270: Total U should be reported as mg/kg.

70. Table 4-77, pg. 4-273: Total U should be reported as mg/kg.

71. Section 4.1.7, 4-300: This section should include a more detailed discussion of the results of endangered species investigations.

72. Table 4-88, pg. 4-316: The strontium-90 columns show positive detects but provide ranges with < signs. If positive detects are found then the concentration detected should be reported.

73. Table 4-97, pg. 4-327: The High rad soils and sediment samples don't appear to be very high in relation to concentrations reported in other sections of this report for soil and sediment. DOE should provide the basis for using such relatively low High Rad samples.

PART II

GENERAL COMMENTS

74. The use of the 95th upper confidence limit (UCL) on the geometric mean as the exposure point concentration (EPC) when the distribution is lognormal and the number of detects is below 7 or the frequency of detection is below 50% does not appear to be appropriate and is not a method recommended in USEPA (1989) guidance. The use of the 95th UCL on the arithmetic mean as the EPC is based on general assumptions regarding the pathway (e.g., children may randomly contact different locations across the site over the duration of the exposure period) and not the distribution of the chemical. If it is not reasonable to estimate the 95th UCL on the arithmetic mean given the available data (i.e., high likelihood that the estimate will greatly exceed the maximum concentration), then the maximum concentration should be used at 95th UCL on the geometric mean as presented in Volume 5, Appendix R by as much as an order of magnitude. Therefore, use of the maximum concentration in these cases may impact the ultimate risk estimates.

75. Many of the estimated cancer risks were not summed by pathway for evaluating the risks from the chemical mixture (i.e, Tables 5-16, 5-20, 5-23, 5-34, and 5-37). In addition, hazard quotients were not summed for each exposure pathway. According to guidance,the hazard quotients should be summed by pathway(s) or target organ (which is more accurate) for evaluating the total noncarcinogenic hazard associated with exposure to a chemical mixture (USEPA 1989). Hazard quotients were only presented in the text by chemical.

76. In several instances, chemical-specific carcinogenic risks exceeded 10 2. According to USEPA (1989) guidance, a modified cancer risk equation should be used when estimating cancer risks in excess of 10 2.

Cancer Risk - 1 - e (CDI 5F)

77. Several errors were found in the risk tables presented in Section 5 of Volume 3 based on limited spot-checking of some of the risk calculations using the EPCs and URF/UTF values (as presented below in the page specific comments). It may be necessary to systematically check the risk calculations and supporting documentation (i.e., EPCs and UTF/URF values).

78. It does not appear that sufficient effort was used to derive appropriate wildlife toxicity criteria. For example, toxicity criteria for avian species were often derived from human toxicity criteria (which are often based on mice or rat

studies) rather than evaluating the open literature on avian-specific subchronic and chronic studies. Several on-line databases (e.g., HSDB, TRECS) available through the Chemical Information System (CIS) and the National Library of Medicine (NLM) should be accessed to identify more appropriate toxicity data. Another source includes the series of "Hazard" documents produced by the Fish and Wildlife Service.

79. The conclusions regarding the ecological risk assessment do not appear to be supported by the data. Specific comments regarding the methods and characterization of the ecological risks are presented below.

80. Pg. S-7, line 17: It is unclear what is meant by the statement, "..of insects (earthworms),..". This suggests that earthworms are insects which is not true. The text should be clarified to explain this.

81. Pg. S-7, lines 27-29: DOE fails to define what it believes is a "significant source of risk" if a hazard index (HI) of 5.0 isn't. HI's exceeding 1.0 for four inorganic contaminants would seem to be a source of risk. DOE should define within the document what it is considering a "significant source of risk" when making such conclusions.

82. Pg. S-8, lines 12-14: The field studies conducted were not specifically designed to assess the impact of contaminants on terrestrial communities. A limited extent of tissue sampling was conducted. Studies were not focused on specific areas of contamination and the effects on local populations.

83. Table 2-4, pg. 2-23: DOE should detail within the figure what is the source of background for silo contents.

84. Table 2-6, pg. 2-25: The lack of available background data to complete this table supports Ohio EPA's concerns about the adequateness of DOE's background characterization.

85. Table 2-7, pg. 2-26: The lack of available background data to complete this table supports Ohio EPA's concerns about the adequateness of DOE's background characterization.

86. Table 2-8, pg. 2-28: a) The lack of available background data to complete this table supports Ohio EPA's concerns about the adequateness of DOE's background characterization.
b) Tc-99 was detected in the SSOD. See Table 4-35.

87. Table 2-8, pg. 2-29: The lack of available background data to complete this table supports Ohio EPA's concerns about the

adequateness of DOE's background characterization.

88. Table 2-10, pg. 2-30: DOE should detail within the figure what is considered the source of background for standing water in the waste pits.

89. Table 2-11, pg. 2-31: Fluoride should be included in this table. See Table 4-40, Part I.

90. Table 2-13, pg. 2-36: The lack of available background data to complete this table supports Ohio EPA's concerns about the adequateness of DOE's background characterization.

91. Table 2-15, pg. 2-43: The lack of available background data to complete this table supports Ohio EPA's concerns about the adequateness of DOE's background characterization.

92. Table 2-19, pg. 2-53: DOE should provide the reason for including contaminants that were not detected at levels exceeding the background UTL.

93. Table 2-26, pg. 2-68: The title of this table should be rewritten to just state OU2 waste units, since the lime sludge ponds are hazardous waste units.

94. Table 2-27, pg. 2-72: The background values presented in this table differ somewhat from those presented in Table 2-23. The tables should be reviewed to ensure consistency.

95. Pg. 2-79, line 34: Is the proper reference cited herein? It would seem that RAGS 1989b is the correct reference.

96. Pg. 3-15, line 6: Insert the word "in" between the words "given" and "Table 3-1".

97. Section 3.3.2, pg. 3-17: DOE should provide justification within the text for not calculating risk for the potential receptor using the perched groundwater (1000 series well) for a water supply. Several of the perched zones provide sufficient water to supply residential usage.

98. Table 3-2, pg. 3-18: The table does not include a reference to footnote "b."

99. Pg. 3-19: Some of the estimated EPCs under future land-use conditions (which assumes no containment) were <u>lower</u> than the current land-use EPCs (which assumes containment) (e.g., highest off-property concentrations for uranium). Explain these discrepancies or reevaluate the model.

100. Pg. 3-34: The estimated future EPCs for constituents of potential concern CPCs) for the Great Miami River (GMR) are several orders of magnitude below current monitored levels for several chemicals. Explain these discrepancies, or is the model simply not providing reliable results? In addition, it seems that the modeled concentrations should be added to the current levels of CPCs in order to evaluate the total risk from use of water from the GMR as is being performed for other pathways. Otherwise, it should be clearly expressed in other portions of the document that the future land-use scenario for use of the GMR are not the total risks, but rather only the faction from the contribution of surface water runoff from the site.

101. Pg. 3-59, line 8: Insert a space in the word "forradionuclides."

102. Pg. 3-71, lines 15-18: Update the dermal absorption guidance to "Dermal Exposure Assessment: Principles and Applications" (USEPA 1992).

103. Pg. 4-3: Toxicity criteria are not presented in IRIS or HEAST for lead. Why was the integrated Uptake/Bokinetic Model not used for properly evaluating the potential risk from exposure to lead?

104. Pg. 4-2/9: A few errors were found in Table 4-1. IRIS reports a chronic RfD of 0.02 mg/kg/day for chlorobenzene, 0.1 mg/kg/day for ethylbenzene, and 2.5x10-4 for methyl perathion.

105. Pg. 5-2, line 13: The equation presented does not express the UTF, but rather the URF for chemicals.

106. Pg. 5-18: No URF values were presented in Appendix S by which to estimate the risks presented for Th-230.

107. Pg. 5-26: Some of the cancer risk numbers presented in Table 5-26 appear to be incorrect (or the URF or EPCs used to derive these numbers are incorrect) (e.g., benzo(a)pyrene in the NE Quadrant). Check these estimates.

108. Pg. 5-27: No EPCs are presented in Table 3-8 for estimating some of the Ra-228 risk values presented in Table 5-7. Also, the upstream risk estimate for Ra-226 could not be duplicated. Check these estimates.

109. Pg. 5-39: Why are there different chemicals listed in Tables 5-16 and 5-26? It 5-59 appears that the chemicals should be the same.

110. Table 5-15, pg. 5-38 & Table 5-16, pg. 5-39: The titles of these tables are confusing. Under current access controls, are cattle allowed to graze on the inactive flyash pile and the south field? If not, then the titles need to be rewritten.

111. Pg. 3-39, line 9: "Aroclor-1760" should be changed to "Aroclor-1260". "Bis(2-ethylhexyl) should be changed to "Bis(2-ethylhexly)phthalate". "N-nitrosodiphenyla" should be changed to "N-nitrosodiphenylamine".

112. Pg. 5-46: Benzene which is listed in Table 5-20, is not listed in the corresponding EPC Table 3-8. Also, the risk estimated for bis(2-ethylhexyl)phthalate in Waste Pit 6 could not be duplicated. Check these estimates.

113. Pg. 5-59: Reference to "Table 5-17" should be changed to Table 5-16" in footnote "a".

114. Pg. 5-67: Some of the risk estimates presented in Table 5-30 could not be duplicated (e.g., Pa0231 and Ra-226). Check these estimates.

115. Pg. 5-111, line 23: Change to "3 X 10.3" to "3 X 10^{-3}".

116. Pg. 5-111, line 33: Change "the for the" to "for the".

117. Pg. 6-3, lines 17-20: Why were site-wide arithmetic mean concentrations used as the EPCs for evaluating exposure to wildlife. Given the home ranges of certain indicator species (e.g., mouse,robin) it does not seem appropriate to average all the concentrations across the site given the size of the study area. In addition, it is not clear why the 95th UCL on the arithmetic mean as was used for aquatic organisms and human health. This will have a significant impact on the results since certain surface solid EPCs used in the human health risk assessment were over 2 orders of magnitude above the EPCs used in the ecological risk assessment. This is particularly crucial since certain inorganic EPCs estimated for the Waste Pit Area may result in exposures which are within the acute range for mice and robins.

118. Pg. 6-3, lines 28-30: It is advised that an additional indicator species be evaluated for assessing impacts to piscivores (e.g., great blue heron or green-backed heron).

119. Pg. 6-16/18: The methodology for estimating a chronic NOEL from acute LD_{50} data does not appear to be sufficiently

protective. For example, if a species-specific LD_{50} were available, then a factor of 0.02 would be used to derive the chronic NOEL and this does not appear to be protective of chronic exposure. For example,the desired arsenic wildlife NOEL is over 100,000 lower than reported LD_{50}s. One area of concern is whether the 0.1 factor used to extrapolate short-term exposure to chronic exposure is appropriate for converting acute levels to chronic. Typically, a 0.1 factor is used to convert subchronic studies (30 days) to chronic studies, but not acute studies to chronic studies. At least an additional factor of 0.1 should be incorporated to convert acute studies to subchronic studies, and then apply the 01. factor to convert subchronic results to chronic results.

120. Pg. 6-98: Several doses were not presented in proper scientific notation in Table 6-41.

121. Pg. 6-101: Throughout Section 6.5.2.1 more discussion on the potential subchronic and acute effects, particularly from inorganic exposure to mice and robins, needs to be evaluated given the high HIs estimated (particularly given the use of site-wide averages to estimate EPCS). In addition, the magnitude of the HIs needs to be discussed.

122. Pg. 6-103, lines 22-23: Eisler (1988) presents data which indicate the levels of arsenic in excess of 5 mg/kg FW in liver is indicative of arsenic poisoning in livestock. Arsenic levels in the liver of the white-tailed deer obtained on-site was 20 mg/kg dry weight or approximately 6 mg/kg FW. Therefore, it appears that the white-tailed deer evaluated on-site had arsenic poisoning. This needs to be discussed in the text.

123. Pg. 6-108/110: Section 6.5.2.3 needs further discussion on the uncertainties and limitations of the field studies. It is very difficult to measure a "statistically significant" impact on biota. Include information on the alpha level of the test, actual difference in control and site study groups even if a statistically significant difference is not seen, sample size, and other uncertainties. When differences were seen in the robin nesting study or in population studies, the discussion implies that the differences were not associated with contamination but rather other environmental factors. Given the potential for acute effects in some groups of organisms, impacts from chemical contamination cannot be ruled out. With respect to rabbit population differences, what evidence is there to suggest by omission that the site contamination is not impacting rabbit population differences in the field. How

is owl predation greater on-site than in off-site locations? Given the modeled HIs for mice, it is plausible that differences in vole populations are due in part to contamination on-site?

124. Pg. 6-113 and ES-24: The conclusions presented for the ecological risk assessment do not appear to be supported by the data. The statement that "potential exposures predicted by modeling may not occur in the field or that the resulting potential effects may not occur" assumes that the field studies can <u>see</u> impacts to the ecosystem and that those which were seen are not attributable to the site. As previously discussed, seeing a statistically significant impact in a field study is very difficult. Even obtaining a sample size large enough to show significance is very difficult given the natural variability and response that will be expected and the hundreds of variables which cannot be controlled in the field. What concerns me is the fact that only site-wide arithmetic mean concentrations were used (which would tend to dilute hot spot areas and assumes that the sample arithmetic mean is equal to the populations arithmetic mean [i.e., the 95th UCL should have been used] to estimate doses and yet very high HIs were found. In addition, certain field studies showed that impacts may be occurring (i.e., robin nesting study and arsenic poising of deer). Also, acute toxicity data were used to generate chronic wildlife NOELs using rather low uncertainty factors. Based on the modeling results and EPCs estimated for the human health risk assessment in certain study areas, it appears that certain doses may be well within acute toxicity ranges. Therefore, it does not appear appropriate to suggest that the risks are "low" and the emphasis of remediation is to prevent "future ecological harm." Although it does not appear that major gross ecological damage has been occurring, the characterization of ecological risks should be more balanced.

<u>APPENDIX O - FATE AND TRANSPORT MODELING</u>

GENERAL COMMENTS

125. The unsaturated zone model is presented in great detail and appears to convey a high level of certainty in the results. This is not appropriate and clearly does not reflect the tremendous uncertainty associated with contaminant transport in the vadose zone. In contrast to the saturated zone modeling, the unsaturated zone lacks credibility. The issue of uncertainty is overshadowed with detailed tables. Confidence in the transport results is severely undermined by the use of single input parameters (instead of ranges) and

inherent simplifying assumptions.

126. The objective of the modeling is to predict how contaminants move from source areas to receptors. However,the local transport model does not include the well of the SOWC, according to Figure 5-1. Model predictions clearly show the plume moving toward these wells (e.g., Figure 5-9); these appear to be the logical discharge points for contamination. Why doesn't the model extend to these wells? Why is there not discussion of contaminant loadings to these wells?

SPECIFIC COMMENTS

127. Pg. 0-2-4, lines 1-4: What about the fracture-matrix interaction? Matrix diffusion can be extremely important. This issue is highly studied and well documented in high-level radioactive waste literature. Even at "high" velocities in the fill, matrix diffusion may significantly "retard" the movement. Also, this effect can greatly delay flushing and remediation in the fractured till.

128. Pg. 0-2-4, lines 21: The discussion focuses on fracture transport, yet porosities reported appear to be total or primary. Secondary or fracture porosity should be listed as this will dictate contaminant transport. Furthermore, the use of total porosity to address fracture-controlled transport can lead to a gross underestimation of contaminant migration.

129. Pg. 0-2-5, lines 26-28: The sentence states that communication is controlled by α. "Communication" is controlled by ΔH and $K_{2/b}$; dispersion effects concentration not quantity.

130. Pg. 0-2-5, lines 29-33: This discussion regarding matrix sorption appears to contradict previous discussion on p. 0-2-4.

131. Pg. 0-3-10: Please provide example of how calculations were performed. What is the assumption to convert from mg/Kg to mg/L? What is the basis for a 95% confidence limit? Is this based on sorption? For example, in Waste Pit 1, the CS-137 is 1.27E-8 mg/Kg. This seems to be a very small number. When multiplied by the mass in the pit (45,140,000 Kg), the available S-137 is less than 1/2 a mg. But how is the source term (9.9E-11 Mg/L) calculated? Please annotate table to identify method used as basis for source term calculations for each constituent in each area.

132. Pg. 0-3-50, lines 26-35: Based on the reported dispersion coefficient and velocity, the calculated longitudinal dispersion if 5.5 cm (0.18 feet). These seem exceptionally low and inappropriate for migration distances of tens of feet. This needs to be substantiated.

133. Pg. 0-3-51: Shouldn't the variables be u^z, and D_z, not u_x and D_x?

134. Pg. 0-5-1, lines 27-32: Add figure showing regional and local model boundaries or references figures in previous report.

135. Pg. 0-5-5: Add figures showing model layering and boundary conditions or references figures in previous report.

136. Pg. 0-5-7, lines 17-18: It is unclear what is meant by loading periods of 1-200 years given Figures 401 through 405. Does this refer to time for peak loading? were loadings not included for full 1000 yr simulation period?

137. Pg. 0-5-9, lines 15-20: Clarify why U238 was the only compound initialized in the transport model.

138. Pg. 0-5-18, lines 5-18: Reference to Figure 5-6 appears to be erroneous as this figure is for t - 10 yrs.

140. Pg. 0-5-18, lines 14-16: Some discussion of vertical contaminant extent (3-D model is used), sensitivity of predictions to variables, and sources or error in numerical prediction of concentration should be added to this discussion.

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