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LITIGATION TECHNICAL SUPPORT AND SERVICES
ROCKY MOUNTAIN ARSENAL

BIOTA REMEDIAL INVESTIGATION
FINAL REPORT
VERSION 3.2
VOLUME IV

May 1989
Contract Number DAAK11-84-D0016
Task Number 9

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THE PURPOSE OF THE BIOTA REMEDIAL INVESTIGATION REPORT IS TO PRESENT THE CURRENT NATURE AND EXTENT OF CONTAMINATION IN BIOTA ON RMA. THE REPORT INTEGRATES KNOWN HISTORICAL INFORMATION, THE RESULTS OF PREVIOUS INVESTIGATIONS, AND RESULTS OF CURRENT PROGRAMS.

THE REPORT IS DIVIDED INTO THE FOLLOWING SECTIONS:

1. GENERAL BACKGROUND INFORMATION ON RMA CONTAMINATION AND PAST INVESTIGATIONS
2. DESCRIPTION OF THE PHYSICAL ENVIRONMENT, REGIONAL BIOTA, AND BIOTA WITHIN THE ARSENAL; IDENTIFICATION OF IMPORTANT BIOLOGICAL COMPONENTS AS A BASIS FOR EVALUATING CONTAMINATION
3. SUMMARY OF THE METHODS USED IN THIS ASSESSMENT
4. DATA ON THE TYPES AND CONCENTRATIONS OF CONTAMINANTS IN RMA BIOTA
5. ASSESSMENT OF THE DISTRIBUTION AND CONCENTRATION OF CONTAMINANTS IN BIOTA AND ASSOCIATED ENVIRONMENT TO EVALUATE PATHWAYS FOR CONTAMINANT MOVEMENT

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Environmental Science And Engineering, Inc.

Rocky Mountain Arsenal
Information Center
Commerce City, Colorado

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Rocky Mountain Arsenal

Rocky Mountain Arsenal Information Center Commerce City, Colorado

Biota Remedial Investigation Final Report (Version 3.2) Volume IV

May 1989
Contract Number DAAK11-84-D0016
Task Number 9

Prepared by
ENVIRONMENTAL SCIENCE AND ENGINEERING, INC.

Prepared for
OFFICE OF THE PROGRAM MANAGER
ROCKY MOUNTAIN ARSENAL CONTAMINATION CLEANUP

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APPENDIX E
COMMENTS AND RESPONSES TO THE BIOTA
REMEDIAL INVESTIGATION,
DRAFT FINAL REPORT
(VERSION 2.2)
JANUARY 1989

The Task 9 Biota Remedial Investigation, Draft Final Report (Version 2.2) was distributed on January 27, 1989 to all Organizations and the State. Comments were received from the U.S. Environmental Protection Agency on March 13, 1989; Shell Oil Company on March 16, 1989 and March 23, 1989; the Colorado Department of Health on March 17, 1989; and the U.S. Fish and Wildlife Service on March 21, 1989. All written comments and formal responses are incorporated in the following appendix. A chronological history of documents and Biota Assessment Working Group meetings that led to the Biota Remedial Investigation Report is included.

DOCUMENT HISTORY

Document	Date Furnished
Draft Final Technical Plan, November 1985	11/14/85
Draft Final Phase II Technical Plan, August 1986	08/27/86
Black-footed Ferret Survey Report, September 1987	10/09/87
Bald Eagle Study, Draft Final Report, November 1987	12/01/87
Letter Technical Plan (supplements Draft Final Phase II Technical Plan), April 1988	04/14/88
Black-tailed Prairie Dog Populations of RMA, Draft Final Report (Ver. 2.2), May 1988	05/12/88
Bald Eagle Study, Winters 1986-1987, 1987-1988, Draft Final Report (Ver. 2.1), June 1988	06/02/88
Acetylcholinesterase Inhibition Testing Data	06/02/88
Phases I and II Final Technical Plan (Ver. 3.2), July 1988	07/22/88
Bald Eagle Study, Winters 1986-1987, 1987-1988, Final Report (Ver. 3.1), September 1988	09/12/88
Biota Remedial Investigation, Draft Final Report (Ver. 2.2), January 1989	01/27/89

BIOTA ASSESSMENT WORKING GROUP MEETING HISTORY
(formerly the HEP Team, Biota Assessment MOA Subcommittee)

Date	Principal Agenda Items
9 January 1986	Establish Technical Biota Group
22 January 1986	HEP, Tentative List of HEP Species
21 February 1986	Status of Habitat Suitability Index Models
10 March 1986	HEP, Chemicals for Tissue Analysis
11 April 1986	Protocol for State participation in the Biota Committee meetings, HEP use for natural resource damage assessment, chemicals for tissue analysis
5 May 1986	Chemicals for tissue analysis, species for chemical analysis, contaminated and control areas
19 June 1986	Data exchange, sampling permits, control and contaminated site selection
8 July 1986	Chemicals for tissue analysis, MKE aquatic sampling program, chemical methodology
4 September 1986	ESE's Draft Phase II Technical Plan, sample decomposition, lab methods for tissue analyses, control areas
14 November 1986	Sampling progress reports, chemistry analysis methods, natural resource damage assessment issues
23 January 1987	Review of bald eagle status on RMA, biota studies updates, chemistry analysis methods
9 March 1987	Review of bald eagle status, tissue analysis, biota studies updates, pest and weed control
14 May 1987	Bald eagle studies, biota studies updates, maintenance plan review, tissue analysis issues (sites, chemicals, etc.)
10 July 1987	RMA maintenance plan, chemical analyses, biota studies updates
22 September 1987	Status of field programs, chemical analysis program, bald eagles, prairie dogs

Date	Principal Agenda Items
10 November 1987	Bald eagle studies, Arsenal maintenance plan, Basin F interim action, status of field programs, status of chemical analysis programs
10 February 1988	Arsenal maintenance plan, bald eagle studies, status of field programs, status of chemical analysis programs
13 April 1988	Status of field sampling programs, chemical programs, balk eagle studies
21 July 1988	Status of sampling and analysis programs, bald eagle study, biota monitoring technical plan, USFWS management plans for RMA
28 September 1988	Status of sampling and analysis programs, bald eagle studies, CDOW deer study, presentation of ESE's chemical analyses results
27 January 1989	Presentation of the Biota Remedial Investigation
8 March 1989	Clarification of Biota RI in response to questions from BAWG members

UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
REGION VIII
999 18th STREET - SUITE 500
DENVER, COLORADO 80202-2405

MAR 1 0 1989

Ref: 8HWM-SR

Colonel Wallace N. Quintrell
Program Manager
AMXRM-PM
Office of the Program Manager
for Rocky Mountain Arsenal
Building E 4460
Aberdeen Proving Grounds, Maryland 21010-5401

Re: Rocky Mountain Arsenal (RMA)
Biota Remedial Investigation, Draft
Final Report, Version 2.2, January
1989.

Dear Colonel Quintrell:

We have reviewed the above referenced document and have discussed our concerns with the U. S. Fish and Wildlife Service (USFWS). As in the past, in regard to matters specific to the RMA biota, we defer to the concerns of the USFWS.

We note from the subject report that bioaccumulative compounds could affect higher trophic levels (i.e., the bald eagles). Also, the conclusion of the pathway analysis seems to preliminarily indicate the need for substantial remediation of various areas in order to assure adequate protection of the biotic environment. We realize that such conclusions may well drive decisions concerning the scope and level of remediation, independent of any human land use restrictions. Therefore, we will follow closely the implications of the subject report to ensure protection of both human health and the environment.

Please contact Mr. Connally Mears at (303) 293-1528, if you have questions on this matter.

Sincerely,

Robert L. Duprey, Director
Hazardous Waste Management Division

Enclosure

cc: Don Campbell, RMA-PMO
Jeff Edson, CDH
David Shelton, CDH
Patricia Bohm, CAGO
Lt. Col. Scott P. Isaacson
Chris Hahn, Shell
R. D. Lundahl, Shell
David Anderson, DOJ
Rod DeWeese, USFWS

FINAL RESPONSE TO
GENERAL COMMENTS OF THE
ENVIRONMENTAL PROTECTION AGENCY
TASK 9 BIOTA REMEDIAL INVESTIGATION
DRAFT FINAL REPORT

Comment 1:

See cover leter.

Response:

Comment noted.

STATE OF COLORADO

COLORADO DEPARTMENT OF HEALTH
4210 East 11th Avenue
Denver, Colorado 80220
Phone (303) 320-8333

Roy Romer
Governor

Thomas M. Vernon, M.D.
Executive Director

March 17, 1989

Mr. Donald Campbell
Office of the Program Manager for
Rocky Mountain Arsenal
Attn: AMXRM-PM, Building 111
Commerce City, CO 80022-2180

Re: State Comments on the Biota Remedial Investigation Draft Final Report

Dear Mr. Campbell:

Enclosed are the State's comments on the Biota Remedial Investigation Draft Final Report.

If you have any questions, please contact Mr. Jeff Edson with this Division.

Sincerely,

David C. Shelton
Director
Hazardous Materials and Waste
Management Division

DCS/JE/rw

pc: Michael R. Hope, AGO
David L. Anderson, DOJ
Chris Hahn, Shell Oil Company
Edward J. McGrath, Holme Roberts & Owen
Connally Mears, EPA
Mike Gaydosh, EPA
Tony Truschel, GeoTrans

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FINAL RESPONSE TO GENERAL COMMENTS OF THE STATE OF COLORADO ON TASK 9 BIOTA REMEDIAL INVESTIGTION DRAFT FINAL REPORT

Comment 1:

Limitations of Analytes:

The State does not believe limiting the number of major contaminants of concern to seven will produce a Remedial Investigation that is sufficiently detailed to meet the requirements of CERCLA, the National Contingency Plan, and pertinent EPA guidance. The seven "major contaminants of concern" in the draft R.I. are aldrin, dieldrin, arsenic, DBCP, endrin, isodrin, and mercury. (p. 5-4) The seven "target analytes" are aldrin, arsenic, DDE, DDT, dieldrin, endrin, and mercury. (p. 31)

Contaminants analyzed in biota tissue were selected from the list of 39 major contaminants. As the State has previously expressed throughout the biota evaluation process, it is concerned that important compounds have been omitted from the list of target analytes.

For example, diisopropylmethyphosphonate (DIMP), a "nerve-gas" compound, is not included as an analyte in this study, although prior studies of wildlife contamination on the RMA have included DIMP as an analyte (Thorne, 1979). DIMP is toxic to fish, birds and mammals. The reasons for excluding DIMP are not clear. Because this chemical could be considered an "arsenal fingerprint" due to its unique methods of use and production, evaluation of DIMP levels in biota would be important in assessing contamination from sources specific to RMA.

The State is willing to confer with the Army as to what additional compounds the State believes should be included in future biota investigations.

Response:

Representatives from the Colorado Division of Wildlife and the Department of Health have been provided with numerous opportunities for input into the overall design of the Biota Assessment. These included participation in discussions at the Biota Assessment Committee (currently the Biota Assessment Working Group) meetings (see BAWG meeting information on Pages E-iii through E-iv) and review of the draft Biota Assessment Draft Technical Plan. While the State has repeatedly expressed concern over the number of contaminants evaluated through

chemical analyses, the State has not, even when asked to do so, indicated any specific problem with the elements of the process used to identify contaminants selected for analysis. In addition, the State has not provided any information on any of the additional contaminants of concern to indicate that additional contaminants met the criteria for inclusion in the sampling program.

The process used to select contaminants for analysis in biota was comprehensive, objective, and based on an evaluation of all available information on contaminants of concern. The addition of contaminants that did not meet the criteria of the selection process was therefore deemed unjustified. DIMP was not considered as a major contaminant of concern because of its low toxicity, low bioaccumulation potential, and because it was not found in potentially hazardous concentrations in the abiotic environment.

Comment 2a:

Selecting "Acceptable Concentrations" of Contaminants

Under the National Contingency Plan, a Remedial Investigation defines the nature and extent of contamination. The draft Biota R.I. takes the additional step of setting forth a number of "acceptable concentrations: for various contaminants. See, for example, Tables 5.1-3 and 5.2-1. The State disagrees with the methodology for calculating the "acceptable levels" (as set forth below). The State also disagrees with the fact that these levels were set in this document.

Although determining "acceptable levels: is obviously important to setting remedial goals, it is not a part of cataloging the nature and extent of contamination, which is the primary function of a Remedial Investigation. The State welcomes any preliminary discussion regarding methodology for setting "acceptable levels." However, the State would object if the presentation of "acceptable levels" in the draft Remedial Investigation were to preclude a thorough evaluation of the factors to be considered in setting "acceptable levels" or remediation goals at later points in the RI/FS/EA process. The Feasibility Study and Onpost and Offpost Endangerment Assessments have not yet been supplied to the State. (See, for example, page 5-4, referring to forthcoming Onpost and Offpost Endangerment Assessments.) The State reserves its right to comment on "acceptable concentrations" and the methodology for their determination when the final remedial action is selected.

Response:

It was necessary to establish acceptable concentrations of contaminants in this document in order to evaluate the potential adverse effects on organisms at the higher trophic levels in the food web that might occur as a result of biomagnification from abiotic sources. The development of these acceptable levels permitted a site specific approach to evaluating potential adverse effects which, even if they did occur, might be difficult or immpossible to observe under field conditions. The potential use of the acceptable concentrations as remediation criteria will be addressed as part of the endangerment assessment process leading to the Record of Decision. The State of Colorado will be provided opportunity to further evaluate these values during this process.

Comment 2b:

The draft Remedial Investigation gives comparatively little emphasis to observed contaminant effects (Sections 5.3). It gives much greater emphasis to general toxicity data and theoretical developments of "acceptable levels" (Sections 5.1 and 5.2). The draft R.I. would be more useful with greater emphasis on actual contamination effects found at the Arsenal.

Response:

The amount of information provided in Sections 5.1 and 5.2 was necessary in order to document the approach used. Information from these sections was used as the basis for evaluating many of the contaminant effects addressed in section 5.3. The purpose of the Biota RI is to determine the nature and extent of contamination in biota in a manner not inconsistent with CERCLA and the NCP. While the toxicity assessment data in the report are extensive, the emphasis is not on contamination found on RMA.

Comment 2c:

Furthermore, the Study Area Reports (SARs) referred to on pp. 1-12 and 1-13 were received by the State only on March 15, 1989. Without the opportunity to review this information in detail, the State cannot comment as to whether any data included in the SARs may affect the validity of the "acceptable levels" calculations.

Response:

Comment noted.

Comment 3:

Statistics

Concerning the statistical analyses, the authors have employed the Kruskal Wallis one-way ANOVA as the standard test for detecting differences between exposure groups for contaminants (B-5). While the rationale for using the non-parametric approach is given, a discussion of the weaknesses of this form of ANOVA in terms of its power relative to the parametric forms of ANOVA is not. Because the Kruskal Wallis one-way ANOVA is less sensitive than a parametric ANOVA, there is less chance of detecting differences. Appendix B-1 should include a detailed explanation of the bias introduced because of this and the relative sensitivity of nonparametic techniques.

Additional clarity would be obtained by including the direction of statistical differences which are found between comparison groups, rather than merely indicating that differences existed. The statistical procedures described in Appendix B include substituting zero (0) for levels below detection limits. No rationale is presented for using this approach in lieu of the alternative practice of taking one half of the detection limit for values BDL. Substituting zero for half of the detection limit most likely would result in a greater variance. Since a greater variance decreases the power of the test and results in less chance of finding differences, this is critical. The State recommends that the data be analyzed using both methods until a determination can be made that the results are not compromised by using the approach in the draft R.I.

Response:

The assumptions of the parametric test are not substantiated by the data (e.g., treatment cells with zero variance, widespread heteroscedasticity, and low power at evaluating normality); therefore the reliability of the parametric ANOVA is questionable. The fewer or weaker the assumptions of a statistical test, the more general are the conclusions. Nonetheless, even if all of the assumptions of the parametric ANOVA were met by the data (and they are not), the Kruskal-Wallis test has an asymptotic efficiency of 95.5%.

Zero was never substituted for levels below the CRL in statistical analyses. The lowest value in any data set would be the CRL. All such values would be tied to the lowest rank in the nonparametric (Kruskal-Wallis) test. The magnitude of the values are not used in this test. Parametric results are presented for comparative purposes (see Appendix B of the Biota RI). In all cases the CRL was substituted in the parametric tests when values fell below this limit. Fifty percent of

the CRL was used to calculate mean concentrations, in order to be consistent with USFWS presentation procedures.

Comment 4:

Unexplained Conclusions

The Executive Summary contains a number of conclusions that do not appear substantiated by references. For example, page xvi, last full paragraph, concludes that the lowered juvenile to adult ratios in RMA prairie dog towns "appeared to be the result of normal environmental factors rather than RMA contamination." There is no explanation for this conclusion. Page xviii, second full paragraph, does not explain why acetylcholinesterase (AChE) inhibition found in prairie dogs from the Toxic Storage Yard "appeared to be the result of heavy metals naturally occurring in the environment," especially in view of the fact that there is no significant difference in naturally occurring heavy metals in Section 36 and in the Toxic Storage Yard.

Response:

The explanation for the conclusion regarding lower juvenile to adult ratios is discussed in Section 5.3.3.2 and was partially restated in the State's comment number 25. The distribution of heavy metal concentrations in soils is variable, and AChE was lower in prairie dogs from Section 36 (i.e., inhibition was greater) than in control areas but higher than in the Toxic Storage Yard. The distribution of prairie dogs, a mobile species, could not be correlated directly with high levels of metals in soils.

Comment 5:

Referencing but not Including Certain Crucial Data

There are some instances in which crucial data are referenced in the discussion but not presented. See, for example, the reference to page 3-5, below. This affects the informed review when the reviewing party does not have immediate access to all referenced documents.

Response:

See responses to comments 6a and 6c below.

Comment 6a:

Omission of Air Pathway

Page 5-3 of the draft RI states, "The air pathway was not evaluated because data from air sampling studies indicate low potential for adverse effects on biota via this route exposure, and because there is little information on the adverse effects on biota in natural ecosystems from exposure to the

contaminants of concern by this route." Does little information indicate no adverse health effects? Furthermore, the Army's list of potential contaminants was based in part on contaminants found in the air (p. 4-25).

Response:

This pathway could not be evaluated because of lack of information on species specific respiration rates, air circulation patterns in burrow systems, and actual measurements of contaminants in underground burrows. Although data on adverse effects in natural systems are lacking, data on inhalation toxicity to laboratory animals indicate that the air pathway is not a potential problem based on observed ambient levels of contaminants in air: toxic levels that were to laboratory animals approximately 4 orders of magnitude higher than those observed at RMA (See Sax (1984) in section 7.0 of this document). The toxicity data will be compared to the exposure data in detail in the Onpost EA.

Comment 6b:

In view of the complaints (from humans living up to a mile away from the Arsenal's border) about Arsenal-generated air pollution, it is questionable whether one can assume that air contamination presents no problem for biota on the Arsenal itself.

Response:

Complaints from humans offpost do not constitute evidence for adverse effects on biota.

Comment 6c:

In a statement apparently contradicting page 5-3, page 5-8 of the draft RI acknowledges that for Sections 26 and 36, air contamination may be a significant hazard to wildlife populations. However, the draft RI does not analyze the nature and extent of airborne contamination even in these Sections. The RI also does not cover the harm that any result from breathing emissions from volatile substances (such as DBCP, whose "major route of removal from soil or surface water is by volatilization," according to page 5-200 of the draft RI).

Response:

The paragraph has been reworded. The intent was not to indicate that contaminants were significantly high, but only that they had been detected. The nature and extent of airborne contamination is examined in detail in the Air RI. DBCP was not detected in air.

Comment 6d:
The draft RI not only omits references to the effect of inhaling contaminated substances, but it also fails to discuss wind-blown transport as a contamination pathway. The presence of dry, contaminated sediments (such as Upper Derby Lake, referred to on page 4-8 of the draft RI) indicated a potential for contaminants to spread by wind.

Response:
The Air RI examines particulates as well as vapors.

Comment 6e:
To obtain more information regarding airborne contamination, the State has asked the Army to begin sampling approximately 272 surface soil (0 to 2 inches depth) locations throughout the RMA, beginning in the spring of 1989. The resulting data could provide significant information regarding the need to include inhalation of windblown contamination as an exposure pathway.

Response:
Comment noted.

Comment 6f:
The State believes that more work must be done to determine the role of the air pathway in exposing plants and animals to contamination from the Arsenal.

Response:
The Army believes that the work already performed is complete and adequate.

Comment 7:
Incorporation of Forthcoming Reports
There appear to be additional, forthcoming reports (see, for example, page 3-5, referring to "forthcoming reports from MKE for their vegetation, wildlife, and aquatic ecosystem investigations"). There should be some clarification as to how these forthcoming reports will be incorporated into the Final Biota RI.

Response:
MKE is preparing these reports to be issued in 1989. MKE has supplied the Army with pertinent material from these reports so that it could be included in this document.

Comment 8:

Altering Language of Report

It has been suggested by one of the MOA parties that the language of this report be altered to downplay the nature and extent of biota contamination at the Arsenal.

Response:

Comment noted.

FINAL RESPONSE TO SPECIFIC COMMENTS OF THE STATE OF COLORADO ON TASK 9 BIOTA REMEDIAL INVESTIGATION DRAFT FINAL REPORT

Comment 1:

Page 1-4

The comment that "the great diversity of wildlife rivals that of any site on the Front Range corridor, including the existing government parks and preserves" implies that the diversity exists despite the widespread contamination at the Arsenal. In fact, there are few other parcels of land of similar size that are free from hunting or other extensive human disturbance. The State believes that whatever wildlife diversity exists at the Arsenal is a function of available habitat, combined with the previously mentioned lack of current human presence. The existence of wildlife is no indication of the lack of serious consequences of the contamination addressed by this report. Therefore, the State believes the quoted statement should be deleted.

Response:

The Army agrees that wildlife diversity on RMA is a function of available habitat, absence of hunting, lack of livestock grazing, and other factors. The text is correct as stated. The seriousness of the consequences of contamination on biota are objectively addressed in Section 5.3 of this document.

Comment 2a:

Page 1-8

The authors correctly point out that "wildlife injuries addressed in this report are only the documented cases..." (page 1-8, emphasis added). This statement addresses the important issue that the records of wildlife losses identified in Table 1.3-1 are primarily "opportunity samples" and that no system of "active" surveillance of wildlife mortality has been in place. Although the draft RI states that "death and injury to wildlife may have occurred in varying numbers at other locations at other items" (page 1-8, emphasis added), the State believes that additional, undocumented deaths and injuries must have occurred.

Response:

Comment noted.

Comment 2b:

The Army states that the nature and extent of the problem has ... "created an environment at RMA in which there are chemical compounds in sufficient quantity to pose a treat to wildlife, and a potential health hazard to man" (page 1-7). The distribution of contamination and the accumulation of contaminants in wildlife species described in the report suggest that the RMA environment poses more than a "threat" to wildlife. These exposures have resulted in mortality in the past and will continue to do so in the future. There is a direct relationship between chemical contamination of the environment, the accumulation of the contaminants of the environment, the accumulation of the contaminants in wildlife, and health effects including death in exposed species.

Response:

Comment noted.

Comment 3a:

Section 2.2 on regional biota

The description of wildlife fauna contains several noteworthy items. There are large numbers of small mammals and lagomorphs (rabbits and hares) in the region. These classes of animals are addressed only indirectly as parts of the food web for raptors. The State believes that these animals also should be considered as target species.

Response:

Food web analyses address possible adverse effects on lagomorphs as part of the overall evaluation of adverse effects on the food web. The contaminant concentrations protective of top predators are also protective of organisms at lower trophic levels.

Comment 3b:

Big game mammals such as mule deer and white-tailed deer exist on the RMA, with mule deer being abundant (page 2-10). "Both mule deer and white-tailed deer are common and conspicuous on RMA" (page 2-25). Thus it is not clear why adequate sampling of mule deer was not conducted.

Response:

Adequate sampling of mule deer was conducted as part of biota assessment that forms the basis for this document. Fourteen mule deer were sampled from RMA. None of the flesh (meat) samples had contaminant levels above CRLs. One of 14 liver samples had a level above CRL, and this

individual was from the center of RMA between Basin A and the South Plants. This sample size is adequate.

Comment 3c:

The most common predatory animals in the region include the coyote, badger and long-tailed weasel. Coyotes are widespread on RMA (page 2-23). Badgers are also common at RMA (page 2-23). Minimal sampling from this class of animal was conducted (see below). Thus, there appear to be important omissions of major classes of animals in the contaminant analyses presented in section 5.0.

Response:

Large predators were included as samples of chance and were not included in the sampling program in order to avoid adversely impacting their populations by collecting. This approach was addressed in the Biota Assessment Technical Plan and was discussed at Biota Assessment Committee meetings in which the State participated.

Comment 4:

Page 2-16 of Section 2.3, on study area

The draft R.I. limits the off-post exposure pathways to contaminated ground water, surface water, and sediments. This excludes soil and sediment contamination from wind and from flooding surface waters. Previous off-post sampling has indicated significant concentrations of Arsenal contaminants in surface soils and sediments, thus indicating that wind transport is a contamination pathway that must be considered. This may in turn require expansion of the off-post study area.

Response:

Comment noted. This comment should be addressed to the appropriate Offpost documents.

Comment 5:

Page 2-19 of Section 2.3.1.2, on wildlife

Page 2-19 states, "In most cases, animal abundance at RMA appears to be related to habitat quality, low levels of human disturbance, and the absence of hunting and livestock grazing." This statement suggests that the RMA is not representative of the surrounding region in terms of the factors that influence population numbers and diversity. However, if the aim of the study is to determine the effects of contamination, a valid "control" area should have comparable habitat quality, low levels of human disturbance, and absence of hunting and livestock grazing. Without such controls, a comparison of size and diversity of animal populations on RMA to that on other areas would not

support a conclusion as to lack of contaminant effects on the Arsenal. In other words, problems with the control areas cast substantial doubt as to the conclusion concerning contaminant effects.

Response:

This concern was discussed at length at Biota Assessment Committee meetings prior to the preparation of the technical plan for this work. Representatives from the State of Colorado and other participants provided suggestions for appropriate control areas, but no similar areas were located. With the exception of prairie dog juvenile-adult ratio data provided by Shell/MKE, nowhere in this document are "size and diversity of animal populations on RMA" (where the RMA is treated as a unit) compared to offpost areas. In studies comparing population effects, species were selected in or near sites of contamination for comparison with control sites. Many of these comparisons indicated probable contamination effect, thus confirming both the validity of the approach used and the nature of current contamination in RMA biota.

Comment 6:

Page 3-1, Section 3.1, on Phase I Investigation

This phase involved development of a workplan by compliation of a database that included historical information on species and abundance, contaminant sources and locations; contaminant types, concentrations and distributions; biological effects of contamination: and other data. Data gaps identified during Phase I led to the Phase II program (page 3-5). The R.I. should list what these data deficiencies were. Without this, the reader cannot ascertain whether the investigations conducted under Phase II address all of the areas where additional data were required.

Response:

The data gaps were identified in the rationale for Phase II studies in the Biota Assessment Phase I and Phase II Biota Assessment Final Technical Plan.

Comment 7:

Pages 3-6 and 3-7, Section 3.2.2.1 Collection Sites

Onpost control sites were chosen from among those where previous soil and groundwater investigations revealed no contamination (page 3-7). The type and amount of sampling (and the results) that the Army used to classify an area as "unexposed" are not explained in the draft R.I. The State continues to disagree on the Army's methodology for determining which, if any, areas within the Arsenal are uncontaminated. When these sites were determined to be

control areas is not indicated. The State therefore questions whether the data used to select onpost control areas reflect the current state of contamination of the abiotic media sampled. It is critical to the interpretation of the study results that the onpost control areas sampled reflected "unexposed" areas. Further justification for the selection of onpost areas as control areas is requested.

The State expects the Army to take surface samples (0 to 2 inches depth) throughout the Arsenal during the spring of 1989. If this sampling indicates contamination in areas considered as "controls" in the draft R.I. it will necessary to revise the draft R.I. to designate the true control areas.

Response:

As stated in the Phase I and Phase II Biota Assessment Final Technical Plan, the control sites were selected following Phase I investigations of soil and water. Inspection of additional data from Phase II programs has confirmed that these sites were valid control sites.

Comment 8:

Table 3.2-1 and Page 3-10, on field investigation of wildlife

Table 3.2-1 lists important wildlife species potentially occurring on RMA. From this list, the draft R.I. selected four species for field investigations: black-tailed prairie dog, mallard, ring-necked pheasant, and American Kestrel. No justification for selection of these species is provided, except that the prairie dog was of particular interest because of its importance as a raptor prey species (page 3-10). Other species that may be more susceptible to the effects of environmental contamination are not included. Although there are population estimates for more than one mammalian species, only one mammal is included in the detailed field investigations. While one mammal may represent one trophic level, it cannot represent behavioral differences between different species in the same trophic level. Consequently, the State recommends including different species with different behavior patterns, even if they belong to the same trophic level.

Response:

Justification for selection of these species is provided in the Phase I and Phase II Biota Assessment Final Technical Plan. The selection of these species was discussed with representatives from the State of Colorado at Biota Assessment Committee meetings prior to the preparation of the technical plan. Data for additional species would be helpful, but are not necessary as this approach provides a conservative assessment of contamination effects on populations.

Comment 9:

Page 3-19, on invertebrate populations

The statement that snails were not sampled for contaminants due to low volume and weight is puzzling. Pooling of snail samples should have provided adequate weights and volumes to estimate contamination levels. Snails should be included in all investigations regarding invertebrates.

Response:

The highest mean sample weight for snail samples from RMA that were composed of many, sometimes thousands of individuals was 3.36 grams. Much of this weight was shell, making the sample too small for adequate chemical analysis by currently accepted methods.

Comment 10:

Page 3-20, on avian mortality

There is no mention of avian mortality from Ponds A and B, which are surface impoundments built in 1988. Any documentation regarding such mortality should be included in the Final Biota R.I. The State suggests that avian mortality surveys be done at least quarterly for all existing surface impoundments.

Response:

The acquisition and reporting of this information is part of the Comprehensive Monitoring Program, not the Biota RI.

Comment 11a:

Section 3.2.2.3, on Contaminant Analysis

This is a critical component of the study, inasmuch as the objectives were to "determine the concentrations and type of RMA contaminants in the tissues of key species of biota at RMA and at offpost control sites" (page 3-28). As mentioned in the general comments, above, the State believes that the assessment of contaminant effects (Section 5.3) has not received adequate attention relative to the effort that has gone into the more theoretical considerations of toxicity assessments (Sections 5.1) and pathways analysis (Section 5.2)

Response:

Comment noted.

Comment 11b:

The analysis scheme for tissues was based on "the probable fate of the organism within a food web or because of its particular status" (page 3-30). The failure to include descriptions of actual sampling techniques, such as the method of determining locations, numbers species, and numbers of animals within the species, is a major defect and clearly compromises the ability to draw conclusions from the information presented.

Response:

Statistical analyses were conducted on data as appropriate. A detailed description of the rationale and approach used in these analyses is presented in Appendix B of the Biota RI. Rationale for selecting the locations, etc. is provided in the Phase I and Phase II Biota Assessment Final Technical Plan and is summarized in Section 3.0 of the Biota RI. The alleged defects concern information already provided in other sections of this document and in previous documents.

Comment 11c:

Analysis of the prairie dog carcass was conducted after removal of the head, feet, fur, and gastrointestinal tract. It is not clear whether all the abdominal and thoracic contents were removed. Major fat deposits within the adominal cavity constitute an important substrate for analysis, because several of the pesticides of interest are selectively stored in fat. Omission of fat samples for detection of organchlorine pesticides throughout the contamination assessment could underestimate the level of contamination. Furthermore, including fat only as part of a larger sample substantially underestimates the actual concentration in the fatty tissue. Similarly, failure to collect organs such as the kidney, which selectively stores mercury, may also be considered a shortcoming.

Response:

The objective of these analyses was to determine the concentrations of contaminants in the portion of the animal consumed by predators. The portions removed were those not eated by the avian predators of concern. Care was taken to retain the fat in each sample. For species potentially eaten by humans (e.g., cottontails), flesh samples were taken instead of carcass samples. This approach was discussed with representatives of the State of Colorado at Biota Assessment Committee meetings and was described in the Phase I and Phase II Biota Assessment Final Technical Plan.

Comment 11d:

The State believes that the Army's study of contamination in the food chain often ignores the effects of contamination on the animals belonging to the food web. This approach fails to meet the CERCLA requirement of addressing the nature and extent of all contamination, not just contamination within the food chain.

Response:

The rationale for selecting the food webs to represent all species in the aquatic and terrestrial ecosystems provides a comprehensive and systematic approach to evaluating contaminant effects on individual species and on the animal community. The systematic approach used to consider all species and contaminants in the development of these webs is described in the Phase I and Phase II Biota Assessment Final Technical Plan and summarized in Section 3.0 of the Biota RI document. This approach is not inconsistent with the CERCLA requirements.

Comment 11e:

For criticism of the selection of target analytes, please see the general comments, above.

Considering the scope and importance of this work, the State is concerned that the number of samples analyzed is inadequate (Table 3.2-2). This comment applies to the diversity of species sampled and the number of animals obtained. For example, among mammals, the most thorough sampling is for the prairie dog, where a total of 18 animals were obtained on RMA and 16 from on- and off-post control sites. For the desert cottontail, only 8 animals were obtained from section 36 and 16 were obtained from on- and off-post control sites.

Response:

The sample scheme verified that biological contamination occured in sites of known soil and water contamination, the results demonstrated that on RMA, control sites were significantly different from sites of contamination. The sampling scheme used in this study is adequate. A detailed description of the statistical analyses used to detect differences is described in Appendix B of the Biota RI document.

Comment 11f:

No indication is provided for the uncertainty inherent in this sampling scheme. While elegant evaluations of statistical variation are provided for some of the analyses, the statistical uncertainties surrounding the issue of sample size are unaddressed. The issue of sample size and study power is a fundamental consideration related to assessment of contaminant effects (5.3). When sample size is inadequate, the study may not detect a difference between exposed and unexposed groups, even when a difference is really present. The probabilities of finding differences of various magnitudes in contaminant levels given the small sample sizes found in the contaminant analyses should be presented.

Response:

The sample sizes used in this remedial investigation are adequate. The issue is further addressed in detail in the Appendix B of the Biota RI.

Comment 12a:

Section 3.2.2.4, on Contaminant Pathways and Criteria Development

The pathways analysis approach has been used to develop cleanup criteria for sediments and water, which are derived in turn by tracing the biomagnification of contaminant residues with health effects data for organisms at the top of the food web, back through intermediate trophic levels (page 3-35). As stated, this approach relies on the assumption that all organisms are in equilibrim with their environment (Figure 3.2-9). The construction of this model is of more than theoretical interest, because the values obtained from the model may be used in assessing the need for remedial action. Therefore, all assumptions inherent in the model must be questioned thoroughly and the most conservative approach taken. To what extent does the assumption of equilibrium apply under diverse conditions? Are there any situations where the organisms in the biota are not in a state of equilibrium?

Response:

Yes, equilibrium may not apply in all situations because of individual movements, addition of rain and surface water flows to lakes, and other factors. While the assumption of equilibrium is the most conservative assumption it is used only as a first approximation. Further study may indicate departure from equilibrium under actual site conditions.

Comment 12b:

A further important assumption would appear to be that the no effects soil concentration obtained for the target organism at the top of the food chain will protect all terrestrial species. For example, page 5-164 states that the lowest acceptable surface water concentration for dieldrin will protect all species of waterfowl. What assumptions are inherent in this statement? Are there situations under which these assumptions may not be reliable?

Response:

The selection process has selected appropriately sensitive species and the bioconcentration/bioaccumulation factors for that species would be adequate to protect other related species. This is described in the Phase I and Phase II Biota Assessment Final Technical Plan.

Comment 12c:

Without the inclusion of surficial soil data (to be obtained in the spring of 1989, as mentioned above), the state further questions the value of the determination that a "no effects" soil concentration for the top organism in the food chain protects all other species.

Response:

Comment noted.

Comment 13:

Page 4-22, on nature and extent of biological contamination

According to page 4-22, studies have "been conducted on some of the chemical contaminants, particularly those that are peculiar to RMA activities, to determine the possible biological effects and concentrations necessary to produce effects." The draft R.I. further states that despite these studies, information on critical issues such as dose levels, physiological effects, toxicity, mutagenicity etc are still unavailable. (page 4-22) This conclusion seems unusual, given the fact that the authors cite 6 studies that have addressed the biological effects of RMA contaminants. It is unclear why these previous studies are "dismissed" in such a manner. It is also unclear to what extent the authors made use of the data contained in these 6 studies in their toxicity assessments and pathways analysis contained in Section 5.0 of the report. A clarification of this issue seems appropriate. If data are present in the previous studies concerning the biological effects of RMA contaminants that have not been incorporated in the present study, these omissions should be documented and a rationale for their exclusion presented.

Response:

These documents were not dismissed. Relevant data from these documents and many others were used in this Biota RI. See Section 7.0 (Literature Cited). Nonetheless, information on some critical issues is still unavailable.

Comment 14:

Page 4-4, on removing fluids from Basin F

Although the draft R.I. claims that removing fluids from Basin F and placing a clay cap over the area should reduce waterfowl mortality due to Basin F (page 4-4), this statement ignores (1) the effect of the new surface impoundments that have received liquids from Basin F and (2) emissions from soils remaining beneath the temporary clay cap in Basin F and (3) the Army's failure to include ventilation controls in the synthetic cover for the waste pile.

Response:

Pond A is covered. Soil emissions and ventilation controls are unlikely to have any effect on waterfowl.

Comment 15:

Page 4-3, on evaporation from Basins B,D, and E

The statement that liquids in Basins B, D, and E had likely evaporated by the time of Basin F construction is absurd. The text must be corrected to indicate at least portions of these liquids leaked into the ground.

Response:

See appropriate text change on page 4-3.

Comment 16:

Table 4.3-1, on current extent of contamination in biota

Comments regarding the adequacy of sample size appear above. Table 4.3-1 (pages 4-28 and 4-29) shows important deficits in sample size for cottontail rabbits (7 samples), mule deer (14 samples), coyote (1 sample) and badger (1 sample). Thus, the present study is extremely limited in its appraisal of current contamination levels for important segments of the biota. The findings of this study should not be interpreted as representing the current status of contamination of biota due to inadequate sampling of important target species.

Response:

The sample size, although small, did confirm contamination in the upper trophic level mammalian carnivores. This information, combined with the extensive information on contaminant concentrations in the various compartments of the aquatic and terrestrial food webs is sufficient to describe adequately the extent of contamination in these ecosystems. Because more sensitive species (e.g., birds) were selected to represent the higher trophic levels within the food webs, evaluation of adverse effects on these species is sufficient to address adverse effects on all segments of the animal communities. This general approach had been discussed at various Biota Assessment Committee meetings attended by representatives of the State of Colorado prior to the preparation of the Biota Assessment Technical Plan.

Comment 17:

Page 4-37, on dieldrin in bald eagle egg

A single bald eagle egg was collected from an abandoned nest at Barr lake and shown to be contaminated with mercury, dieldrin, and DDE. The authors state that "the contaminant levels are from non-RMA sources" (page 4-37). The conclusion is questioned, in light of the fact that the egg contained dieldrin. Table 4.3-1 shows that only 1 of 73 of the offpost control samples obtained from avian species contained dieldrin. A total of 32 avian eggs obtained offpost were sampled: 10 from mallards, 11 from pheasants and 11 from kestrels. None of these samples contained dieldrin at detectable limits. In contrast, ten of fifteen American Kestrel eggs obtained from the northern sections of RMA (sections 22, 23, 24, 25, 27, 29 and 30, figure 4.3-3) contained dieldrin. Four of the 10 eggs with dieldrin were contaminated at levels above 3.00 ppm. Therefore, the finding of dieldrin in a bald eagle egg 5 miles from RMA cannot be interpreted to indicate that exposure to dieldrin occurred offpost. The explanation given at the March 8, 1989 biota meeting that the highest dieldrin levels in Barr lake sediments were towards the dam is not conclusive, since dieldrin is not used at Barr Lake. Given that dieldrin is known to be present in groundwater and surface water coming from the Arsenal, the State believes that the dieldrin found in this egg and at Barr Lake originates from the Arsenal. Therefore, the text should be corrected.

Response:

Extensive groundwater and surface water sampling in the offpost study area has not indicated a link between dieldrin in Barr Lake and RMA sources. All known feeding observations of the Barr Lake eagles are

from Barr Lake and immediately surrounding areas. Thus it is likely that the source of dieldrin contamination is from Barr Lake which is known to be contaminated with dieldrin. See text page 4-27 for appropriate change. Additionally, Wiemeyer and Cromartie (1981) in a study of organochlorine contaminants in bald eagle eggs from across the U.S. reported DDE to be ubiquitous and dieldrin present in 81 of 83 eggs from the contiguous 48 states. In view of this information it is extremely difficult to determine the source of dieldrin contamination in bald eagle eggs.

Comment 18:

Page 4-41, on mammalian carnivores

Two mammalian predators a coyote and a badger, were found dead and collected in Section 25 on RMA (page 4-41). Both animals were contaminated with dieldrin. The State questions why only the livers of the coyote and badger were analyzed. The finding of contamination emphasizes the need for additional assessment of contaminant levels in mammalian carnivores.

Response:

Livers were selected as the target organ for analysis since the contaminants in the biota analysis program (e.g., metals, organochlorine pesticides) are known to accumulate in this organ. Thus these contaminants, if present, would most likely be detected by analysis of this organ. Additional destructive sampling is not justified.

Comment 19:

Page 4-43, on game species

The exclusion of certain analytes from certain species (arsenic from mallards, DDE from mule deer and rabbits) is not explained (page 4-43). The Army must include these compounds in all analyses.

Response:

The rationale and reasonableness of not analyzing for some contaminants in some species was discussed at length in meetings of the Biota Assessment Committee with representatives of the State of Colorado present. The rationale is further documented in Section 3.3 of the Biota Assessment Final Technical Plan.

Comment 20:

Page 4-51, on cottontail rabbits

Was a comparison done for levels of dieldrin between onpost control and contaminated sites (page 4-51)? The finding of contamination with dieldrin in 3 of 7 samples suggests the need for additional sampling of this species, and consideration of human health hazards through the food chain.

Response:

A statistical comparison was made between the onpost control sites and the contaminated site in Section 36. The cottontails from Section 36 were significantly more contaminated than those from the onpost control, which registered no contaminant levels above CRLs. The finding of contamination in 3 of 7 samples from Section 36, a known site of contamination, and no contamination in the onpost controls confirms what would be expected in sites of contamination and further indicates that cottontails from RMA but not living near sites of contamination do not appear to be contaminated. The Army does not believe that additional sampling is necessary.

Comment 21:

Page 4-51, on mule deer

Additional sampling of mule deer must be conducted, especially in view of the potential for exposure of humans through the food chain.

Response:

Fourteen mule deer collected on RMA did not show contaminant levels above CRLs in flesh (meat). Only 1 of 14 liver samples had detectable levels of dieldrin, and this was below the FDA flesh consumption guidance. The one deer liver sample came from an animal collected between the South Plants and Basin A: a known area of high contamination. The deer were collected from scattered locations throughout RMA. The Army believes that additional sampling is unnecessary.

Comment 22a:

Page 5-5, on toxicity assessments

Section 5.1 deals with 32 contaminants which are of concern because of adverse effects on biota produced as a result of direct environmental exposure (page 5-1). The toxicity assessment approach was used to determine contaminant levels in the abiotic environment that would theoretically have no adverse effect on biota. Any incorrect assumptions built into the toxicity assessment will therefore distort the "no effect" level for a particular contaminant.

Response:

The Army acknowledges this last statement as a truism.

Comment 22b:

The general methods employed in the toxicity assessments are summarized in Figure 5.1-1 and Table 5.1-2, with the results of the analyses presented in Table 5.1-3 as estimated "no effect" concentrations in abiotic media. Much of the toxicological data employed in the analyses of Section 5.1 is obtained from studies of laboratory animals such as dogs and rats. The Army then used an uncertainty factor of 5 to control for interspecific variation. However, the State believes that interspecific variation in susceptibility to the toxic effects of chemicals often exceeds an order of magnitude. Therefore, the use of an uncertainty factor of 5 is not adequately conservative and is not consistent with EPA guidance. An uncertainty factor of at least 10 should be used in the calculation of the most sensitive NOEL or LOAEL.

Response:

The Army believes that the uncertainty factor of 5 is sufficiently conservative, especially when it is applied in conjuction with other conservative assumptions (e.g., equilibrium). This approach is not inconsistent with EPA guidance.

Comment 22c:

In the toxicity assessment for many of the chemicals included in 5.1, the EPA chronic criteria are used to establish the acceptable water concentration for aquatic organisms. In contrast, in the pathways analysis, the authors state that the "EPA water quality criteria were reviewed for applicability, but not always used to represent criteria for aquatic life" (page 5-106). This is an important issue, since, for example, the "no effects" level for dieldrin in water for exposure by aquatic life is 0.05 ppb from the pathways analysis and 0.0019 ppb according to the EPA chronic criteria for the protection of aquatic organisms and their uses (page 5-145). As justification for the failure to take the more conservative approach in the issue, the Army state that "EPA chronic criteria for the protection of aquatic organisms and their uses (0.0019 ppb) are based on a Final Residual Value with human guidelines as the MATC, and therefore not considered applicable" (page 5-145). Considering the number of assumptions built into the site-specific pathway analysis (page 5-3), there can be no justification for failure to adopt the approach of selecting the lowest contamination level that can be considered to be protective of mammalian health.

In contrast, in the calculation of the acceptable concentrations of arsenic in water and sediment, the EPA value of 100 ppb for irrigation water is accepted in lieu of the value calculated from the bald eagle biomagnification model, since arsenic does accumulate in tissues (page 5-190) and arsenic is more toxic to plants than animals (page 5-197). Similarly, in the calculation of the acceptable levels of endrin in water, the EPA water quality criterion is accepted as the standard because the predicted "no effects" level based on food chain accumulation is not protective for aquatic life (page 5-259).

In the calculation of the "no effects" values for mercury, the EPA water quality criteria for the protection of aquatic organisms and their uses are again considered inappropriate for the analysis since they are based on human guidelines (page 5-281). However, in this instance the "no effects" level in water (0.016 ppb) is essentially equivalent to the EPA chronic criteria for water (0.012 ppb) (page 5-313).

Response:

The EPA water criteria for dieldrin is 0.034 ppb and is not relevant to mammals. The EPA value is based on the FDA action level which applies to human consumption, not to effects on biota. The approach used in these studies selected the appropriate applicable level and is therefore justified. The acceptable water concentration for mercury is 0.004 ppb, not 0.016 ppb as the State reports. The concentration is therefore more conservative than the EPA criterion for water.

Comment 23:

Page 5-313, on plant contamination

Dieldrin was found in sunflowers and morning glory. There is no comment regarding the effects of these for the diet of any Arsenal herbivores. This needs to be included.

Response:

Possible effects on consumers were and are addressed in Section 5.3 for invertebrates (e.g., grasshoppers) and vertebrates (e.g., kestrels).

Comment 24:

Page 5-320 and 5-321, on brain acetylcholinesterase inhibition

The discussion of the findings of reduced AChE in prairie dogs focuses on the possibility that the effects are a result of exposure to arsenic or other heavy metals. There is no discussion of the possibility that these effects are due to organophosphate exposure. The Army must investigate and document all effects of organophosphate exposure.

Response:

There are no recent records of organophosphate pesticides being used in the toxic storage yard. These chemicals are quickly degraded in the environment and are not persistent. Organic contaminants containing phosphorus groups (e.g., DIMP) are not known to inhibit AChE.

Comment 25:

Page 5-322, on black-tailed prairie dog populations

Studies of prairie dog densities show populations near the low end of the normal range. Further, the juvenile: adult ratio is decreased for RMA compared to offpost locations. As pointed out (page 5-328) a number of possibilities exist that may account for these differences, including normal cyclic population fluctuations in colonies, the temporal distribution of prairie dogs, habitat suitability and contamination effects. Moreover, because observation problems were encountered during the prairie dog census (as explained in the earlier prairie dog report), the state does not accept this conclusion. Also, because "stable" colonies generally have lower young to adult ratios, offpost comparison information should include data concerning the population stability of those sites. Follow-up studies are needed to determine whether the observed reductions in density and apparent decrease in reproductive success are merely cyclical, or, as suggested on page 5-330, due to the direct effects of arsenic and dieldrin concentration on prairie dog health. Additional studies of prairie dogs are also recommended to monitor the important role of this species in contaminant pathways to predators.

Response:

The prairie dog studies were conducted in two periods: first in the Summer of 1987 to delineate general population levels, and second in January 1988 to compare some populations in Sections 36 and around the North Plants to those in uncontaminated areas on the Arsenal. In the summer studies, we did find a generally lower density of prairie dogs on the Arsenal than those reported on some sites in the literature. However, it is important to note that the populations were within a known range for the species. In the winter studies, we found that there was no significant difference between contaminated and uncontaminated areas. These were the only conclusions drawn. Prairie dog populations on RMA continue to be monitored due to the outbreak of plague in the prairie dog colonies and as part of the ongoing Comprehensive Monitoring Program.

Comment 26:

Page 5-331 to 535, on eagles and other birds of prey

See Comment for 4.3.2.3. Additional follow-up studies of eagles and their prey are needed.

Table 5.3-5 contains important evidence that dieldrin is accumulating in the tissues of raptors at lethal levels. Raptors found dead on RMA contain brain dieldrin levels in the range associated with lethal effects (page 5-335), while raptors dying of electrocution had low levels of contaminants including dieldrin in brain. Therefore, continued studies (particularly for dieldrin) of raptors found dead on RMA and of living raptor populations are needed to assess contamination of biota.

Response:

Analysis of raptors found dead on RMA is continuing as part of the Comprehensive Monitoring Program. The U.S. Fish and Wildlife Service is sampling blood from captured eagles and other raptors. Potential prey species are being sampled as part of the Comprehensive Monitoring Program.

Comment 27:

Page 5-340 to 348, on avian reproductive success

Studies of avian reproductive success, especially for American Kestrels, have been useful indicators of terrestrial contamination and effects on biota (page 5-340). Dieldrin is a significant contaminant for pheasants, mallards and American kestrels. The American kestrel nesting studies demonstrate the value of this species as a bioindicator (page 5-348). Changing patterns of nesting success from 1982/1983 to 1986 indicate that toxic contamination may have been reduced somewhat, although other explanations exist. Presently, some areas on RMA are still to heavily contaminated for kestrel survival and reproduction. Therefore, continued follow-up studies of this indicator species are recommended.

Response:

Follow-up studies of American kestrels on RMA are part of the Comprehensive Monitoring Program as documented in the CMP-Biota Technical Plan which was provided to representative of the State of Colorado at the Biota Assessment Working Group meeting on July 21, 1988.

Comment 28a:

Pages 5-350 to 354, on other species and contaminant effects

The finding of only one contaminated deer among 14 from RMA with measured levels of contamination and adequate population density is taken to indicate that contaminant effects on deer populations are probably negligible (5-350). Additional samples from deer should be obtained (see 4.3.2.3). Additional studies of carnivores such as coyotes and badgers are also recommended since levels of dieldrin compatible with lethal effects were detected in the two animals sampled to date.

Response:

See responses to the above-reference comments. Sample design for deer was appropriately constructed, and the sample size was adequate to address contamination in RMA deer for purposes of the Biota RI.

Comment 28b:

Further studies of avian species at RMA are recommended since "analytical results indicate that RMA contamination, particularly dieldrin, is still a problem for avian species" (page 5-354). Additional studies of mallards and pheasants are recommended since levels of dieldrin associated with health effects have been found in these species. Similar recommendations are made for mourning doves found contaminated with aldrin, dieldrin, and endrin, screech owls, and other insectivorous birds.

Response:

Current studies are sufficient to determine the nature and extent of contamination in biota at RMA and to evaluate its adverse effects. While additional data is always desirable, it is not necessary for purposes of the Biota RI. Pheasants and mallards continue to be sampled under the Comprehensive Monitoring Program.

United States Department of the Interior
FISH AND WILDLIFE SERVICE
COLORADO FIELD OFFICE
730 SIMMS STREET
ROOM 292
GOLDEN, COLORADO 80401

IN REPLY REFER TO:

March 21, 1989

FWE/CO

Office of Program Manager
Building 111
Rocky Mountain Arsenal
Commerce City, Colorado 80022

ATTN: AMXRM-PM (Donald Campbell)

Dear Mr. Campbell:

Please disregard the initial copy of the attached document dated March 15, 1989. There were inadvertent errors in that copy which were not detected prior to mailing. Please distribute additional copies included to representatives of either your staff, Captain Kingery and his staff or ESE, as needed. We regret this inconvenience.

Sincerely,

Lawrence R. Derleare
for

LeRoy W. Carlson
Colorado State Supervisor

cc: FWE/FWE Regional Office
FWS/FWE/SLC
EPA/R8

United States Department of the Interior
FISH AND WILDLIFE SERVICE
COLORADO FIELD OFFICE
730 SIMMS STREET
ROOM 292
GOLDEN, COLORADO 80401

IN REPLY REFER TO:

March 21, 1989

FWE/CO

Office of Program Manager
Building 111
Rocky Mountain Arsenal
Commerce City, Colorado 80022

Attention: AMXRM-PM (Donald Campbell)

Dear Mr. Campbell:

We have read and evaluated the Draft Final Rocky Mountain Arsenal Biota Remedial Investigation Report. The following is a summary of the comments that we have at this time. We wish to point out that this three volume report is not only lengthy, but it contains a great deal of technical material addressing contamination of fish and wildlife at the Rocky Mountain Arsenal (Arsenal) and the timeframe for review was very short.

Volume I.

Page 1-10. In Table 1.3.1, page 2, the location of wildlife casualties should be more specific for the Arsenal, including the section and quarter-section if possible. The 03/01/82 and 03/29/82 records should read red-tailed hawk. The apparent causes of death for the Fall 1981 mallard and the 03/29/82 red-tailed hawk should not indicate that DDE, and PCBs contributed to the causes of their death, whereas it is correct that dieldrin, endrin and perhaps heptachlor epoxide were causative agents. For many of the records on this page there is no apparent cause of death indicated - these should be filled in or stated unknown; the tissue analyzed should be indicated. On page 3 of the table, the northern oriole (not Bullock's) was not found near Basin F - it was found near Building 111. Also, Headquarters should be changed to Building 111, otherwise other headquarters may be implied. The last record incorrectly states that dieldrin was a causative agent in eggshell thinning in the American kestrel on the Arsenal. Eggshell thinning, and the normally-accepted causative agents - DDT+DDE - were not found to be a significant issue on the Arsenal for any species that we are aware of. In fact, DeWeese, et al. (U.S. Fish and Wildlife Service) found kestrels with relatively thin eggshells and high DDE residues offpost.

(The intervening 11 pages to this letter are in the following Comments/Responses.)

Donald Campbell

Thank you for the opportunity to review these documents. If you have any comments or questions about our review, please contact Rod DeWeese of this office at (303) 236-2675.

Sincerely,

Lawrence R. Bellew
for
LeRoy W. Carlson
Colorado State Supervisor

cc: FWE/FWE Regional Office
FWS/FWE/SLC

FINAL RESPONSE TO SPECIFIC COMMENTS OF THE U.S. FISH AND WILDLIFE SERVICE ON TASK 9 BIOTA REMEDIAL INVESTIGATION DRAFT FINAL REPORT

VOLUME I

Comment 1a:

Page 1-10.

Table 1.3.1, page 2.

The location of wildlife casualties should be more specific for the Arsenal, including section and quarter-section if possible.

Response:

Comment noted. More specific data were not available in the sources used to construct this table. Location data provided are the most specific available.

Comment 1b:

The 3/01/82 and 3/29/82 records should read red-tailed hawk.

Response:

See text change on page 1-10.

Comment 1c:

The apparent causes of death for the Fall 1981 mallard and the 3/29/82 red-tailed hawk should not indicate that DDE, and PCBs contributed to the causes of their death, whereas it is correct that dieldrin, endrin and perhaps heptachlor epoxide were causative agents. For many of the records on this page there is no apparent cause of death indicated - these should be filled in or stated unknown: the tissue analyzed should be indicated.

Response:

Comment noted. The title of the column has been changed to "Notes", to reflect the fact that (unless otherwise specified) a cause of death was not given.

Comment 1d:

On page 3 of the table (Table 1.3.1), the northern oriole (not Pullocks) was not found near Basin F - it was found near Building 111.

Response:

See text change on page 1-11.

Comment 1e:

Also, Headquarters should be changed to Building 111, otherwise other headquarters may be implied.

Response:

See text change on Table 1.3-1.

Comment 1f:

The last record incorrectly states that dieldrin was a causative agent in eggshell thinning in the American kestrel on the Arsenal. Eggshell thinning, and the normally accepted causative agent DDT/DDE were not found to be a significant issue on the Arsenal in any species that we are aware of. In fact, DeWeese et al. found relatively thin eggshells of kestrels with relatively high DDE residues in offpost samples.

Response:

See text change on page 1-11. High dieldrin levels may occur in onpost birds, but DDE is ubiquitous in the offpost area, and thus cannot be ruled out of an exposure scenario. It was intended to have mentioned Mendenhalls' findings, for the record, but not to have emphasized them or to have attributed any effects observed at RMA to Mendenhalls' results.

Comment 2:

Page 2-4

In the last paragraph, it is stated that bald eagles breed at Barr Lake but roost during winter at the Arsenal. The roosting is obviously important but that is merely a night-time activity. The more important point is that bald eagles spend hours, days, and months on the Arsenal engaged in foraging, loafing, preening, and other activities. We suggest inserting a phrase indicating the importance of the Arsenal to eagles for all reasons given above and not just roosting.

Response:

The text has been changed (page 2-5) to reflect the importance of RMA to bald eagles for all winter activities.

Comment 3a:

Page 2-27

We observed abundant numbers of waterfowl on Havana Pond during the spring migration period in 1984. This data is in the form of field notes and can be provided, if so desired. In addition, Rod and Gun Club Pond was an important breeding area for waterfowl during the summer of 1984. The ruddy duck,

American coot, mallard and blue-winged teal were observed there either displaying breeding behavior or with eggs or young at that time. An active marsh hawk nest was found in the south end of this pond at that time also. The mention of frogs and toads at both of these ponds does not fully describe their potential to support wetland wildlife. The above wildlife observations are available upon request in the form of field notes.

Response:

Comment noted. The Army was not aware that these data existed.

Comment 3b:

As indicated on pages 2-27 and 2-28, the Toxic Storage Yard Complex (TSY) was not sampled, as indicated on the bottom of page 2-27 and on the top of page 2-28. Given the fact that the TSY may drain into or near the toxic storage yard ponds or First Creek, it is our opinion that wildlife sampling is justified at this site. We recommend that samples of grasshoppers, earthworms or other small organisms from the TSY be provided for analysis of the standard suite of biota analytes. Otherwise, there will be doubt about wildlife contamination in the TSY, a potentially important site.

Response:

Further wildlife sampling is planned as part of the Comprehensive Monitoring Program and/or pathways validation studies.

Comment 4:

Page 3-9

Under Other Areas, it is erroneously stated that acetylcholinesterase studies were conducted on eggs of kestrels and mallards. Such studies were conducted on the brains of several species, but not the eggs.

Response:

See text change on page 3-9.

Comment 5:

Page 4-5

We would like to point out that although data on any avian losses at Basin F during the period 1956 (beginning of Basin F) to 1981 are apparently not available, the possibility of significant losses during that time are high. In addition, it would be useful to know when the spray rafts were in operation at the Basin. Birds may have been more at risk from the aerosols and these aerosols may have spread by winds to sites adjacent to the Basin. Such a spread may be a reason for some of the terrestrial contamination documented near the Basin.

Response:

Data on wildlife losses from 1956 to 1981 would provide useful information, but these data are not necessary in order to characterize the current nature and extent of contamination and its effects on biota.

Comment 6:

Page 4-21

In the center of Table 4.1-6, page 3, the copper residue of 20.4-37/6 appears to be a typo.

Response:

See change in Table 4.1-6.

Comment 7:

Page 4-24, second paragraph

To our knowledge, eggshell thinning due to dieldrin contamination in kestrels has not been documented on the Arsenal or anywhere else, for that matter. The open literature contains a generous number of articles documenting a strong inverse correlation between DDE concentrations in eggs and eggshell thickness. Dieldrin is considered a lethal toxic agent in birds and not a reproductive inhibitor like DDE.

Response:

See response to Comment 1f.

Comment 8:

Page 4-26, first paragraph

The statement that dieldrin is the dominant form of aldrin/dieldrin in the environment should be adjusted to state the biotic environment. In soils and in lake sediments, aldrin is often found along with dieldrin but in lower concentrations. The ratio of aldrin to dieldrin is often lower (i.e., relatively less aldrin) in animal tissue than it is in sediments.

Response:

The content of the paragraph appears clear. Dieldrin is the predominant form of the two OCP compounds found in biota. The literature indicates that under ambient conditions, aldrin is converted to dieldrin, such that over time, dieldrin becomes the predominant form in abiotic media as well.

Comment 9a:

pages 4-28 to 4-30

Either this Table or its companion appendix should have columns showing percentage moisture and lipids. Both of these are important variables are involved in the interpretation of organochlorine residue concentrations in living tissue.

Response:

Percent lipids or moisture were not considered necessary as part of the biota sampling program. The data were designed to provide sufficient site characterization information in order to address food chain effects for important prey species and obtain data possibly pertinent to human exposure, not to quantitatively define contamination in one tissue or species as compared to another.

Comment 9b:

RMA control sites need to be identified to the section or quarter section. It is difficult to compare Figure maps showing sampling locations with locations in the table and decipher where the RMA 'control' sites in Table 4.3-1 really are.

Response:

Suggested changes have been made on figures.

Comment 9c:

We are attracted to the unusual value of 4.22 ppm arsenic in 1 of 5 prairie dog samples taken from the toxic storage yard. Can any other data be provided to explain whether or not this value is a real one? Could the sample have been contaminated, mislabeled, or otherwise misrepresented? If not, are there any soil samples or other samples from the same site that could explain this result.

Response:

The QA/QC procedures for this sample have verified that the sample was not contaminated, mislabeled, or otherwise misrepresented as a result of laboratory handling. The current Comprehensive Monitoring Program may verify or supplement the data.

Comment 9d:

One other unexpected result is the 18.6 ppm dieldrin in one control pheasant sample. This value is extremely high for a whole pheasant and leads us to

suspect a possible sample cross-contamination or mishandling problem. Could any QA/QC procedures that may have been used to verify this result be provided?

Response:

The sample was reanalyzed after the results indicated anomaly and the dieldrin concentration was verified. It is highly unlikely that only one control sample as large as a whole pheasant would become so highly contaminated as a result of mishandling or cross contamination, particularly at the high concentration obtained. This is an example of undocumented dieldrin use unassociated with RMA sources.

Comment 9e:

On the second page of this table, no RMA locations are given - they need to be so the origin of the samples is known.

Response:

RMA section numbers have been added for species for which specific locations were statistically compared. For highly mobile species, samples from the entire RMA were contrasted to offsite control areas and are summerized as such. More detail on location and tissue concentrations for individual samples is provided in Appendix D.

Comment 10a:

Pages 4-33 to 4-34

Could the rationale for why samples of plants and invertebrates were not taken in the same places be reviewed? Pathways are better understood when data representing soils, plants, invertebrates, and vertebrates are collected in the same sites.

Response:

Organisms were collected from the same general locations whenever possible. Earthworms could not be collected from the basins because of soil compaction or soil type considerations. All organisms are not available at every site due to soil type, vegetation type, and disturbance considerations. Collections were made where organisms were available, near soil borings whenever possible.

Comment 10b:

Could some reason for the apparent planned absence of samples from the toxic storage yard be given?

Response:

Based on Phase I soil sampling, the TSY was not considered to be contaminated with any of the contaminants of concern to biota at the time the biota sampling plan was developed. Prairie dog sampling was conducted in the Toxic Storage Yard at the request of the USFWS when it was observed that bald eagles were feeding in the area.

Comment 11:

Page 4-37, first paragraph

New information obtained by the Fish and Wildlife Service indicates that the eagles at Barr Lake may in fact frequent habitats at the Arsenal. The female of the Barr Lake pair was trapped at the Arsenal during the early winter of 1988. Even though the egg in question may have come from a different female eagle, this new information leads to reservations about the statement that the organochlorines in the Barr Lake eagle egg were from non-RMA sources. Some discussion about this needs to be incorporated.

Response:

This new information is important and was considered. This eagle has not been documented to return to RMA since its capture and noting that Wiemeyer and Cromartie (1981) in a study of organochlorine contaminants in bald eagle eggs from across the U.S. reported DDE to be ubiquitous and dieldrin present in 81 of 83 eggs from the contiguous 48 states, it is extremely difficult to determine the source of contaminants in bald eagle eggs. Therefore, we believe that the original intent of this statement is still valid. A more accurate wording would be to state that there is no indication that the organochlorines in the Barr Lake egg were from RMA sources. See text change on page 4-37.

Comment 12:

Page 4-38, second paragraph

We would like to know how statistics were performed on sets of contaminant data for which one of the sets included samples in which no concentrations exceeded certified reporting limits (CRLs). What values were given to samples with values below CRLs and what was the variance created by this set of artificial numbers? Also, were the assumptions of the analysis of variance (ANOVA) all met to justify the use of this test? A particular problem concerning the above question is that the variances of the samples to be

compared may have been unequal. Such an inequality of variance would require transformation of the data (e.g., to logarithms) before a valid ANOVA could be applied.

Response:

Nonparametric statistics were performed on all contaminant data to test for differences between data sets because assumptions of normality of distribution and/or homogeneity of variance, necessary for parametric ANOVA tests, could not be met. In the nonparametric tests, no ranking within sets was possible for sets in which all values were below CRL. The use of parametric and nonparametric statistics, including assumptions and statistical design, are explained in detail in Appendix B Statistical Analyses of this document.

Comment 13a:

Pages 4-58 to 4-59, Table 4.3-5

As with contaminant data for the terrestrial biota, percentage of moisture and lipids needs to be given in the Table.

Response:

See Response to Comment 9a.

Comment 13b:

We find no appendix listing of all aquatic samples and associated information as was provided for the terrestrial biota. Such a Table for the aquatics should be included.

Response:

Aquatic data are not appendicized because they were provided by MKE as part of their overall aquatic investigations at RMA and were not part of the data set collected as part of the biota RI. MKE provided the summary results of their data to the Army for use in this document. We understand that MKE's detailed aquatic report that should include the detailed data will be included in their forthcoming aquatic studies report sometime this year. The summary of data provided was sufficient for use in this document.

Comment 13c:

It is difficult to identify exactly what is represented under the species column. The plankton should be identified as phyto/zoo plankton if that was the case. The aquatic macrophytes need to be identified to genus or species and also specified as to what is meant by whole. Were there only two macro-

phyte plants or were composite samples of plants used to represent both lakes? Why were macrophytes not taken at Lower Derby - certainly they were available.

Response:

"Plankton" includes both phytoplankton and zooplankton. This has been added to the text and table.

Macrophytes were pondweeds, which were not analyzed separately but represented a composite. Species identified were Potamogeton nodosus and P. gramineun. Also reported for the Arsenal are P. pectinatus and P. pusillus. Other macrophytes present included water-milfoil, coontail or hornwort, and muskgrass (the macroalga Chara kieneri). These and other species have been added to the species lists in Appendix A.

Macrophytes were collected from Lower Derby Lake, but the data were inadvertently omitted from the table. This has been corrected.

Comment 13d:

The fish data are not of much value without the lengths given. Length certainly correlates with mercury and likely organochlorines in fish. Data for these contaminants in fish cannot be compared among or between sites, species, times, and studies unless the data discussed are representative of the same approximate lengths of fish. In addition, we find the combining of data from sampling efforts separated by a 48-month time period an unusual approach. Is this valid?

Response:

The objective of the study was to document whether tissue samples of key species or species groups show levels of contaminants that may be deemed to represent a risk to humans, wildlife, or the aquatic species themselves. Differences among species, tissues, and lakes were secondary to the major issue of contaminant levels. However, a new table (4.3-5) showing weights of bass and bluegill whole bodies from 1986 and 1988 to assist the reader has been added.

Data were not combined from a period of 48 months. The two sampling programs (fall 1986 and spring 1988) were separated by about 18 months. Data from the two years were not pooled for statistical analyses.

Comment 13e:

The subject of sample size stimulates several comments. First, the number of individuals in a composite should be indicated (e.g., largemouth bass in Lower Derby Lake 1988). Also, the composite is not identified. What are the sizes

of composites (presumed to be whole fish). It appears that the sample sizes represented are not uniform, nor is there consideration for size of lake: i.e., larger lake, more samples. Past studies conducted by the U.S. Fish and Wildlife Service utilized a stratified approach for sampling aquatic organisms from each lake. The service's recommendation following that study stated that the numbers and kinds of fish taken and of other aquatic organisms taken in each subarea of each lake could include numbers of individuals that Table 4.3-4 shows for an entire lake. Following this logic, the sample sized expressed in Table 4.3-4 appear to be highly inadequate. The basis of this statement is built on the hypothesis that contamination of the sediments and biota associated with those sediments (i.e., rooted aquatic plants and associated invertebrates) are likewise not uniform, therefore the fish inhabiting these differing environs are also perhaps not uniformly contaminated. The contamination pattern in sediments of the lakes reported by Meyers and Gregg, and the monitoring results of Rosenlund, et al. (1984) bear this out. Therefore, we are of the opinion that the data in Table 4.3-4 by itself does not represent adequate data to suffice as a meaningful data base upon which to conduct feasibility studies. That is, variation within lakes is neither accounted for, nor is it described by the data.

Response:

The Army agrees with many of your comments regarding the usefulness of the information from the MKE investigation; however the data provided combined with the information from Rosenlund's earlier study are sufficient to characterize the nature and extent of contmination for purposes of the Biota RI.

Table 4.3-4 (now 4.3-5) has been modified to indicate composite samples for fish. Composites usually were used for small size groups (e.g., bluegill whole bodies) and generally consisted of five individuals.

Comment 13f:

Can some explanation be provided as to why Rosenlund et al. (1984) found relatively high levels of mercury in plankton but the MKE study in 1986 found no concentrations of mercury above detection levels? The Rosenlund report indicated plankton were a significant potential source and pathways for mercury and dieldrin contamination in fish. Some explanation for this major discrepancy deserves some discussion.

Response:

Most of the values for mercury in plankton cited by Rosenlund et al. were above the certified reporting limit for this study (0.05 ppm). There are a number of alternative explanations, none of which we can select at this time.

Comment 14:

Page 4-56, second paragraph

There is confusion in the referencing of residues of mercury and dieldrin found in fish from the lower lakes. Rosenlund, et al. (1984) found the residues in fish and Meyers, et al. (1983) and Meyers and Gregg (1984) found the residues in sediments from the lower lakes. Rosenlund did no sediment work and Meyers did no fish work.

Response:

See text changes on page 4-59.

Comment 15:

Page 4-56, third paragraph

There is repetitive use of the frequency of detection of cyclodiene pesticides and mercury found in biota from the lower lakes as reported by Rosenlund, et al. (1984) in comparison with the frequency of detection by the MKE studies in the lower lakes. The detection limits in these two studies were not the same: therefore, any comparison of frequencies of detection between these studies is invalid unless this difference is accounted for.

Response:

No comparisons between Rosenlund et al. (1986) and the present study are made based upon frequency of detection. These data are provided to assist the reader in putting means and ranges into perspective. There is no way to avoid the inconsistency noted without deleting some of the frequency data.

Comment 16:

Pages 4-58 to 4-59 con't. Table 4.3-4 con't

There is discrepancy between the results of Rosenlund et al. (1984) and the MKE study for concentrations of mercury in predator fish from all the lower lakes. Rosenlund, et al. found mercury levels in fillet samples of pike, bass, bullhead and bluegill to be >1.0 ppm (the FDA action level) in some samples from all three lakes. The MKE study did not report >0.55 ppm in any fillet samples taken from the same species and the same lakes. Either the design of MKE's study is inadequate (e.g., collection of too few and too small of fish) or there has been some unexplained decline in mercury concentration in fish between 1984 and 1986/88. Could some discussion about either of these or other possible explanations be provided?

Response:

The five MKE northern pike fillet samples (three from Lower Derby, two from Ladora) had wet weights of 151-401 grams (mean of 235 g). Because Rosenlund et al. (1986) did not report weights for fish samples, the data cannot be compared.

The Shell/MKE "Phase I Literature Review, Aquatic Resources Investigation, Rocky Mountain Arsenal (August 1987)," discussed the generally downward trend in pesticides and mercury from 1970 through 1984 (the last data set being from Rosenlund et al., 1986). This report was provided to the U.S. Fish and Wildlife Service. It would appear that the decrease is continuing.

It should also be noted that studies by the Army in 1984 (Thorne 1986) yielded values of mercury in fish which were much closer to the Shell/MKE values than either value was to Rosenlund et al. (1986). For example, pike fillets were reported at 2.94 ppm mercury by Rosenlund et al. (1986), compared to 0.92 ppm by Thorne (1986) and 0.33 ppm by Shell/MKE (1986 data). For Lower Derby Lake, mercury values in pike fillets were 1.72 ppm, versus 0.69 and 0.41 for the same studies. Bass show similar disparities (e.g., in Lower Derby Lake, 1.53 vs. 0.40 vs. 0.36).

Comment 17:

Page 4-61, fourth paragraph

In the third line, the figure 059 ppm mercury in fish viscera is missing a decimal point.

Response:

See text change on page 4-63.

Comment 18:

Page 4-65, fifth paragraph

The series of residue values strung by > shows 0.1010 > 0.249 which is out of order or there are decimal problems with the numbers.

Response:

See text changes on page 4-70

VOLUME II

Comment 19:
Page 5-75.
Under Invertebrates, there is information on toxicity of methyl parathion to invertebrates in Johnson and Finley (1980). This reference is cited in Vol. III. This toxicity information should be included here.

Response:
See text change on page 5-74.

Comment 20:
Page 5-77.
The acceptable criteria for methyl parathion in water is shown as 40 ppb at the bottom of this page. Data shown in Johnson and Finley (1980) indicate this concentration in water exceeds the 96 h LC50 for 5 organisms that were tested. This inconsistency needs to be reconciled.

Response:
The aquatic life criterion was based on the most sensitive aquatic organism in the literature reviewed. Since lower aquatic life health effects data are available, the criterion has been recalculated. The new methyl parathion criterion is 0.0014 ppb. See text change on page 5-77.

Comment 21a:
Page 5-107.
Perhaps one of the most significant sections in the entire three volume report is found in Section 5.2. In this section, toxicity, biomagnification, depuration, dietary preference and other factors are accounted for in a modeling approach to calculate the concentrations of "biota analytes" that could be considered "clean" in the soils, sediment and water that if achieved, would theoretically protect all terrestrial life and aquatic life from objectional exposure to contaminants at the Arsenal. The following discussion focuses on the resulting acceptable concentrations shown in Table 5.2-1, rather than focusing on the process of calculating the concentration. This may be considered a validation approach. For this approach, we examined the literature and gaged the proposed acceptable concentrations against examples of toxicity and biomagnification that can be used as a test of whether the proposed concentration are appropriate for the intended objective.

Response:
Comment noted.

Comment 21h:

1. Aldrin/Dieldrin in water, 0.034 ppb proposed. Johnson and Finley (1980) on p.10 indicate that Daphnia bioaccumulated (BAF) Aldrin 100,000-fold when held in water containing 16 ppt (equal to 0.016 ppb). If this BAF is applied to the 0.034 ppb above, then Daphnia would contain 3.4 ppm Aldrin. We have used this figure, as follows, and have applied selected literature found in the text, Section 5.2-1., concerning the Pathway analysis for Aldrin/Dieldrin.

Busbee (1977) found observed changes in avian behavior in birds exposed to a dietary concentration of 2 ppm dieldrin. Quail chicks exhibited suppressed avoidance response when exposed to a dietary level of 5 ppm. All quail died in a study where the dieldrin in the diet (containing high protein) was only 1 ppm (DeWitt 1956). Although none of these studies were on waterfowl, in the later case, waterfowl do consume a diet high in protein during energy acquisition for egg laying (adult females) and during rapid growth between hatch and flight (young). We use this as evidence that the 0.034 ppb acceptable value for Aldrin/Dieldrin in water within the Arsenal aquatic habitats may be too high and that a reevaluation be conducted. In addition, Rosenlund (1986) could not detect Aldrin/Dieldrin in water in lakes at the Arsenal at a detection level of 0.04 ppb. However, Rosenlund's other investigations (Rosenlund, et al 1984) found dieldrin in predatory fish from the same lakes to be greater than the 0.3 ppm FDA guideline for dieldrin in edible portion of fish. In other words, you don't have to find the Aldrin/Dieldrin detectable in water to have a problem. This is even more important when weighed against the evidence that biomagnification in fish is predominantly from contaminant exposure through water and not diet. Depending on how the fishery is managed at the Arsenal, the 0.034 ppb standard could preclude human use of predatory fish species and could result in a fishery management decision unfavorable to fish eating birds at the Arsenal. Specifically, Rosenlund found the 0.3 ppm (FDA action level for dieldrin) to be exceeded in bluegill which are presently a likely staple food organism for fish eating birds at the Arsenal. Management unfavorable to bluegill could have an impact on birds which are dependent upon them for forage.

Response:

The FWS calculation of 3.4 ppm in Daphnia exposed to the criterion level of 0.034 ppb is correct, however, certain points about the model need to be made. The model incorporates mean bioconcentration factors from different species: FWS considered only one species from one study, and thus did not consider biological variability or variability between studies. By not applying mean values, it's implied that all prey achieve the same level of contamination, which we consider to be an

overly conservative approach. A more accurate prediction of Daphnia concentrations would include the uncertainty in the estimate. Furthermore, quail do not consume aquatic organisms, therefore a more appropriate index of toxicity would rely on waterfowl data.

The FDA action level of 0.3 ppm is irrelevant to biota as it is a human health oriented guideline. It should be noted, however, that the FDA guideline is an order of magnitude lower than the dietary concentration of 2 ppm observed to cause behavioral effects in birds: therefore there is no basis to assume that birds will be at risk from consuming fish containing the FDA action level of a contaminant.

The Army agrees that bioaccumulative compounds do not need to be detected in water in order to pose a threat to aquatic systems. The criteria presented in this report are not standards, nor are they intended to be such, but rather are environmental health effects oriented guidelines. There is approximately an order of magnitude uncertainty in either direction in the estimates, such that the criterion for dieldrin in water ranges from 0.006 - 0.103 ppb.

Schnoor (1981) reported bottom-feeding fish containing 1 ppm dieldrin from a resevoir in an agricultural area where water and sediment concentrations of dieldrin were 0.02 and 7 ppb, respectively. This value in fish is half that observed to cause behavioral effects. In a natural environment, not all organisms will have the same level of contamination or feed in a contaminated area. The standard deviation in fish values was 0.750 - 1.40 ppm. The criterion for water will not result in fish having the 2 ppm level that may produce sublethal effects in birds.

Comment 21c:

2. Aldrin/Dieldrin in sediment, 0.0055 ppm proposed. No comment at this time.

Comment 21d:

3. Aldrin/Dieldrin in soil, 0.10 ppm proposed. The basis of either the sediment or soil concentrations in this table is not given, i.e., dry or wet basis. Dry basis is assumed.

Response:

The soil and sediment criteria are expressed on a wet-weight basis. See text change on page 5-106.

Comment 21e:

Gish (1970) found concentration factors for Aldrin, Dieldrin, or Aldrin+Dieldrin in earthworms:soil of 6 to 8. Korschgen (1970) reported that the similar factor for earthworms was about 5. Korschgen did, however, find an example of a beetle species concentrating Aldrin+Dieldrin by a factor of over 120. Korschgen's work was reported on a wet basis and Gish's on a dry basis. Nonetheless, if terrestrial invertebrates concentrated Aldrin+Dieldrin in soil by a factor of 120 (would be higher on a dry basis even if soils in Korschgen's study had low moisture and the invertebrates had high moisture), then some invertebrates could have 12 ppm concentration. This concentration would equal about 1/3 the LC50 shown for gallinaceous birds in tests conducted at Patuxent Wildlife Research Center (Hill et al., 1975 SSR 191). Given this rationale, pheasants and other ground-dwelling omnivorous species could possibly experience exposure leading to death. Some re-examination of the proposed acceptable concentration for Aldrin/Dieldrin in soil appears necessary.

Response:

The BAF for the beetle species (Poecilus sp.) referred to by FWS was for a single sample. The mean concentration ratio for the species was 31. The BAF for another beetle was only 3.5. Korschgen mentions that the ground beetle Poecilus was unique and related to wet soil conditions, so perhaps the value is not applicable to RMA. The model utilized mean concentration ratios to account for different species in the environment. RMA data were used when available to make the exposure estimates more site specific: the observed BAF for insects on RMA was approximately 10. Examining the dieldrin criterion in soil further, the criterion is less than half the 3 yr. mean in Korschgens study. Aside from the Poecilus, other invertebrates were not highly contaminated, and mean values were less than the 2 ppm dietary LOAEL for birds.

Comment 21f:

4. Arsenic in water, 100 ppb proposed. No comment at this time.

Comment 21g:

5. Arsenic in sediment, 15 ppm proposed. No comment at this time.

Comment 21h:

6. DBCP in water, 6.10 ug/l proposed. No comment at this time.

Comment 21i:

7. DBCP in soil, 0.086 mg/l proposed. No comment at this time.

Comment 21j:

8. Endrin (Isodrin) in water, 0.032 ug/l, proposed.

Johnson and Finley (1980, p. 37) reported that the 96-h LC50 for endrin tested on Claassenia sp. (Stonefly) varied from 0.062 to 0.083 ug/l. This is very near the proposed criteria. They further found residues in fish exposed to endrin concentrated endrin to 400-2,000 times the exposure level and channel catfish that died had 0.5 mg/kg endrin in the whole body. Using the maximum 2,000 concentration from 0.032 ug/l in water, a fish could contain > 0.064 mg/kg endrin. [Hudson, et al. (1984) indicates that 0.064 mg/kg would be 1/12 toxic acute (LD50) exposure to sharp-tailed grouse.] A concentration of 0.032 ppb endrin in water could be a lethal toxic hazard for predators. Snails (p.5-236) may concentrate endrin 49,000 times that in water and contain 492 mg/kg endrin. This could result in (0.032 mg/l X 49,000) 0.128 ppb endrin in snails which is 1/5 the LD50 toxic dose for grouse as discussed above. Please provide some discussion about the relevancy of these comments.

Response:

The Army does not believe that estimates of acceptable concentrations for the RMA lakes should be based on lotic organisms. The most sensitive species tested by Johnson and Finley were Plecopterans, all lotic genera. The most sensitive potentially lentic organism is then Baetis. The LC50 for Baetis is 0.9 ppb (95% CL 0.57 - 1.4 ppb), which is an order of magnitude higher than the criterion. The ACR for endrin is 4: thus an acceptable level for Baetis would be 0.22 ppb. The criterion appears to protect aquatic life.

The Army does not dispute either the bioconcentration factors or the lethal threshold level for catfish tissue, although a mean value would more accurately represent bioconcentration than the maximum value obtained from the open literature. Based on the worst case value of 2,000 as a BCF, tissue concentrations for fish in water of 0.032 ppb would be 64 ug/kg (0.064 mg/kg). The level in snails, using 49,000 as the BCF and 0.032 ug/l as the criterion, is 1.57 mg/kg. Comparing aquatic life concentrations to gallinaceous bird health effects levels is probably not as appropriate as comparing the aquatic life concentrations to waterfowl health effects levels.

Furthermore, the LD50 is given in units of mg toxicant/kg bw of dosed animal. Dietary concentrations are given in units of mg toxicant/kg food item. It is inappropriate to compare the LD50 to dietary concentrations without converting the LD50 to a dietary concentration. The LD50 for the grouse is thus inappropriate. The chronic dietary

level in birds for sublethal effects is 3 ppm, and 1 ppm in diet produced increased reproductive success (Spann et al., 1986; Roylance et al., 1985). Thus, the levels in fish or snails do not appear to pose a threat to avian consumers.

Comment 21k:

9. Endrin/Isodrin in sediment, 0.0019 ppm proposed. No comment at this time.

Comment 21l:

10. Endrin/Isodrin in soil, 9.2 ppm proposed.

We fail to see how the concentration factor of 29 ± 32 (on p. 5-237) applied to this criteria could protect wildlife, an endrin concentration of 9.2X29=267 ppm. This is 47 times the LD50 value and >12 times the LC50 for the most insensitive avian species tested (see p. 5-233). Can some reconciliation for this discrepancy be provided? We would also recommend a review for a possibility that soil contamination could correlate with the apparent death of earthworm populations at the Arsenal. Thompson (1971) found that a 11b/acre application of endrin reduced the earthworm in biomass by an average 67 percent.

The median endrin in soils (Gish 1970) from 26 fields where endrin was detected was 0.38 mg/kg (dry weight) and the storage ratio based on a geometric mean in soils of 0.44 mg/kg endrin was 5.6. That is, soils averaging 0.44 mg/kg endrin may support earthworms with 2.46 mg/kg endrin. Extension of the 9.2 mg/kg soil criteria could yield 51 mg/kg endrin in worms if the worms did not die first. This is >3.6 times the LC50 value for the most tolerant avian species tested (Heath, et al 1972).

Response:

The LD50 is inappropriate when compared to dietary levels. The concentration factor (29) was presented on a dry-weight basis, the wet-weight basis concentration factor is approximately 5 for earthworms. The effects of soil compaction and soil type on earthworms cannot be separated from contamination effects without further study. Few earthworms were obtained in any areas sampled, including offpost controls. Although earthworms could contain levels higher than 5-day LC50 values for birds, earthworms are not a common food source at RMA and should not be considered 100 percent of an avian diet. Other invertebrates may show lower accumulation factors.

Comment 21m:

11. Mercury in water, 0.004 ppb proposed.

Is this concentration above CRLS for mercury in water? Does this criteria account for data from the lower lakes in which mercury cannot be detected but predator fish in 1984 had flesh levels exceeding the FDA action level of 1.0 ppm? A protection level of <0.1 mg/kg mercury in prey of fish-eating birds and <0.1 mg/kg in the diet of ducks has been used as necessary to insure health of ducks feeding in mercury-contaminated water. The above 0.004 ppb mercury may not account for this or for the specific environmental condition in the Arsenal lower lakes. Please provide some response to these questions.

Response:

The CRL is much higher than our estimate of acceptable concentrations. The CRL is 5 ppb. EPA AWQC for mercury are 0.012 ppb, and are based on human health guidelines. Since the FDA action level and the dietary tolerance for birds are both 1 ppm, and the criteria are lower than the EPA values, it is probable that the criteria will be protective of birds consuming fish from these waters. If EPA does not predict that fish will contain over 1 ppm in tissue after exposure to 0.012 ppb, fish should have much lower concentrations based on our criterion.

Comment 21n:

12. Mercury in sediment, 0.004 ppm proposed.

Have the particular methylation processes and their efficiencies been accounted for in the modelling? This would include assessment of mercury loading, microbial activity, nutrient content and pH as well as suspended sediment load, sedimentation rates and other variables (in Eisler 1987, p. 7).

Response:

The methylation rate at RMA has not been determined, and all water and sediment analyses were performed on total mercury. There is conflicting evidence in the literature regarding the parameters mentioned in this comment such that it appeared inappropriate to incorporate such parameters at this time. Basing all the calculations of acceptable mercury levels on methylmercury was a conservative approach, and circumvented the need for methylation rates.

Comment 21o:

Mercury in soils, 1.1 ppm proposed.

Were erosion rates and sediment transport to wetlands from mercury - contaminated soils accounted for in the above proposed criteria? The soil

criteria is 275 times the sediment criteria. An even low transport and deposit efficiency could perhaps yield objectionable sediment concentrations of mercury.

Response:

Soil runoff rates were not calculated as part of the Soil RI, and loading data are not available at this time.

Comment 22:

Page C-18.

We wish to point out the control work on plague in prairie dog towns at the Arsenal. In the third paragraph, what is meant by non-flea control sevin and did the Sevin control measures actually eliminate 6000 acres of prairie dogs?

Response:

The document cited does not define "non-flea control Sevin". The document cited and the Biota RI state 6,000 prairie dogs, not 6,000 acres of prairie dogs. Prairie dogs were exterminated with 2 percent zinc phosphide. See text change on page C-18.

Comment 23:

Page 5-321.

Published studies at Patuxent indicate an age difference in avian brain acetylcholinesterase (AChE) activity particularly in altricial birds. Correct the passage in the 2nd paragraph to reflect application of this fact to the AChE work at the Arsenal. In the third paragraph, the statistical table on p.B-3a does not indicate combined onpost vs. combined offpost differences in prairie dog AChE.

Response:

See text change on page 5-315. Combining data in the manner suggested for the statistical table was not done in order to avoid including onpost control samples with samples from onpost sites of contamination in the sampling scheme.

Comment 24:

Page 5-340. Second-third paragraph.

Something is missing in the transition from waterfowl and raptors to mallards and wading birds. Please do not indicate DDE/DDT to mean either DDE+DDT or to mean DDE or DDT. DDT and DDE do not have the same priority value in eggs. The slash could imply ratio.

Response:

See text change on page 5-334.

Comment 25:

Page B-53.

How do you get an F value when the within mean square is zero? This doesn't compute. Are there other significant digits missing here? Please provide some accounting for this comment.

Response:

The computer program used for these calculations carried out calculations to several decimal places. Results were presented with number rounded to the one-thousandths place; the actual non-zero number was used in calculations.

Comment 26:

Summary

Our review has been largely technical and not editorial. We concerned ourselves with the scientific merit and validity of the conclusions and resulting cleanup criteria proposed for the protection of the biotic environment at the Rocky Mountain Arsenal. Our comments focus on Section 5.2, the pathways analysis. Our approach to criteria assessment was to conduct a preliminary validation test of the proposed criteria. We do not believe our tests were either extensive or complete. Therefore, we request that a more complete and satisfactory accounting of the validation test approach be incorporated into the document. This would apply not only to the major analyses of concern on p. 5-107, but also to the additional analytes listed in Tables 5.1-3 on p. 5-12. An additional major concern that we have questions whether or not the protective criteria projected in the report would insure protection for other species, including wading birds, shorebirds, and mourning doves and other raptor species. Does a shorebird, which consumes sediment-embedded invertebrates as an exclusive diet, receive protection afforded bald eagles? Do grit-eating and soil-probing upland birds receive protection afforded kestrels? We are not convinced that all fish and wildlife entrusted to the U.S. Fish and Wildlife Service and occurring at the Arsenal will receive the necessary protection as is applied by the listed criteria. We would like to see some additional discussion in the report about the application of proposed criteria for the protection of several important species found at the Arsenal.

We can provide you with a list on request: mourning doves, burrowing owls, great-horned owls and killdeer would top that list. For example, it is apparent from the data that great-horned owls may receive greater hazardous

exposure to dieldrin than do kestrels. Therefore, protection of kestrels does not fully equate with the protection of great-horned owls. For another example, mourning doves but not kestrels have been found dead from chemical poisoning at the Arsenal. This suggests that protecting kestrels from harmful contaminant exposure may not adequately protect mourning doves: the highest dieldrin concentration reported was in a mourning dove. Finally, we believe that data base is lacking for some areas. Additional sampling and analysis of biota samples from the Lower Lakes, Toxic storage yard, and Basin F (immediate surrounding area) appear to be a minimum of additional work that is needed. Without additional data, some areas or their status as contributors to contaminants in biota will remain poorly defined or unknown.

Response:

The RI/FS process is structured such that the RI emphasizes data collection and site characterization. This document is the result of studies which had this focus. Model validation is not required or necessary at this point, but will be addressed later in the endangerment assessment process.

The acceptable levels developed for use in this document are used to evaluate potential harm to wildlife species. The Army believes that the conservative but reasonable approach used is appropriate. Other species in the food web are considered in several ways:

- Direct contamination effects from contaminated soil and water are considered.
- If, in the course of literature surveys, sensitive species were identified, acceptable levels were based on these species.
- The MATC was developed for the most sensitive avian species combined with a high BMF for the upper trophic levels and therefore should be sufficient.

The great horned owl should be adequately addressed by levels that were developed from the kestrel food web. Small mammals were considered in this web. The model assumptions of equilibrium would apply to owls as well. The additional uncertainty factors applied should adequately address concerns for owls.

Shell Oil Company

c/o Holme Roberts & Owen
Suite 4100
1700 Lincoln
Denver, CO 80203

March 23, 1989

Mr. Donald L. Campbell
Office of the Program Manager
Rocky Mountain Arsenal, Building 111
ATTN: AMXRM-PM
Commerce City, CO 80022-2180

Re: United States v. Shell Oil

Dear Don:

Enclosed please find an Addendum to Shell Oil Company's Comments on the Biota RI Draft Final Report (Brown Cover, Version 2.2). We realize that this addendum is being submitted after the extended deadline of March 17, 1989. However, we believe that the three additional specific comments are important and, accordingly, request that the Army respond to them as fully as the initial comments.

Please contact me if you have any questions.

Sincerely,

C. K. Hahn

C. K. Hahn
Manager, Denver Site Project

CKH/mp

Enc.

Shell Oil Company

c/o Holme Roberts & Owen
Suite 4100
1700 Lincoln
Denver, CO 80203

March 16, 1989

Mr. Donald L. Campbell
Office of the Program Manager
Rocky Mountain Arsenal, Building 111
ATTN: AMXRM-PM
Commerce City, CO 80022-2180

Re: United States v. Shell Oil

Dear Don:

Enclosed please find Shell Oil Company comments on the Biota RI Draft Final Report (Brown Cover, Version 2.2). Because of the extent of our comments and our general concern with the negative tone of the document, we request that the Army exercise its option to issue an additional draft version of this report.

Please contact Chris Hahn or me if you have any questions.

Sincerely,

Robert D. Lundahl
Manager, Technical

RDL/mp

Enc.

cc: Mr. David L. Anderson
U.S. Department of Justice
999 18th Street
Suite 501 North Tower
Denver, CO 80202

Colonel Wallace N. Quintrell
Office of the Program Manager
Rocky Mountain Arsenal Contamination Cleanup
ATTN: AMXRM-PM
Bldg. E4460
Aberdeen Proving Ground, MD 21010-5401

Captain Andrew Kingery
Remedial Planning Division
Office of the Program Manager for Rocky Mountain Arsenal
ATTN: AMXRM-RP: Cpt. Andrew Kingery
Aberdeen Proving Ground, MD 21010-5401

Dr. Douglas P. Reagan
Hunter/Environmental Science and Engineering
7332 South Alton Way
Building 13, Suite H
Englewood, CO 80112

Dr. Peter Gober
U.S. Fish and Wildlife Service
Fish and Wildlife Enhancement
730 Simms St., Suite 292
Golden, CO 80401

Dr. Rod DeWeese
U.S. Fish and Wildlife Service
Fish and Wildlife Enhancement
730 Simms St., Suite 292
Golden, CO 80401

Patricia Bohm, Esq.
Office of Attorney General
CERCLA Litigation Section
1560 Broadway, Suite 250
Denver, CO 80202

Ms. Kathi Demarest
Colorado Division of Wildlife
Central Region
6060 North Broadway
Denver, CO 80216

Mr. David L. Shelton, Director
Hazardous Materials and Waste Management Division
Colorado Department of Health
4210 East 11th Ave.
Denver, CO 80020

RESPONSE TO GENERAL COMMENTS
SHELL OIL COMPANY
TASK 9 BIOTA REMEDIAL INVESTIGATION
DRAFT FINAL REPORT

Comment I:
The Biota RI Draft Final Report prepared by the Army and its contractor, ESE, represents the culmination of considerable field investigation, laboratory analysis, data interpretation, and document production. In general, text is concise, tables are well organized, and figures are used appropriately. In many places, however, the text does not adequately refer to the tables or figures. Statements such as "Figure X shows that most of the samples with detectable levels of Y were collected from sites near . . ." would be helpful.

Response:
All tables and figures have been referenced in the text of this document. In a document of this size it is not appropriate to reiterate in the text information readily observed in the tables and figures.

Comment II:
There are a number of important areas where we believe it to be seriously off the mark. Some of our concerns involve specific points of disagreement with data interpretation or presentation. In other cases, we believe that the authors went beyond the data, and thus wandered into the realm of unsubstantiated speculation. Examples of these situations are noted in our specific comments, which follow.

Response:
The Army acknowledges that differences in data interpretation may occur. Comments on data presentation appear to differ with your first general comment regarding the text, tables, and figures. Specific concerns are addressed in response to specific comments.

Comment IIIa:
Of equal concern is the overall tone of parts of the report, especially the Executive Summary and sections dealing with contamination history, distribution, and effects on biota. These portions of the RI are misleading, because they are in a negative style which often is inconsistent with the data presented or with the actual condition of the biotic environment at RMA. For example, conclusions about contaminant concentrations or deleterious effects in the vicinity of the major basins (Sections 26 and 36) are described as if they applied to the RMA as a whole. Similarly, concentrations above FDA

action levels are described as though they necessarily represent unacceptable risks to higher trophic levels. Such is not the case.

Response:

While some revisions have been made to place the biota information into more precise context, the primary purpose of the RI process is to assess site conditions and provide data which lead to the reduction, control, or elimination of risks to the environment. The RI is designed to characterize current and potential risks. It serves no purpose to concentrate on those parts of the Arsenal known to be uncontaminated. The bulk of the report must focus on the sections of the Arsenal that are contaminated and will eventually need remediation.

Our overall sampling effort as expressed in the Biota Assessment Technical Plan and in the methods section (Section 3.0) of this report clearly indicate that portions of RMA are considered uncontaminated; hence the selection of onpost control sites for investigations of the less mobile species. The document states that the tissue concentrations of dieldrin are above FDA action levels and does not relate this fact to any conclusions regarding adverse effects on higher levels in the food web.

Comment IIIb:

With growing public interest in open space/wildlife habitat at RMA, the Biota RI assumes considerable significance. It is imperative that the report not only convey factual, scientifically supported data and interpretations, but that it also consider the likely audience and the uses to which the report will be put. We believe that this necessitates placing greater emphasis on current rather than historic conditions, and putting comments into the perspective of the Arsenal as a whole. We therefore suggest that the Army review the entire report in this light and give serious consideration to rewriting the Executive Summary and other portions of the report indicated by our specific comments.

Response:

We agree with your comment regarding the importance of conveying factual, scientifically supported results and conclusions, and we have attempted objectively to do so. Historical and current data sources are clearly identified as such in the text. This document makes no assumptions regarding the quality or validity of historical studies.

The basis for this comment lies in the perception of the potential uses of the Biota RI. As stated in the Executive Summary, the report

documents the nature and extent of contamination as required by the NCPD. Therefore, the report focuses on contamination and contaminated areas of RMA. Its sole purpose is to provide the biota portion of the overall RI and to support the FS in determining the most appropriate remedial action.

Shell seems to be suggesting that the Army rewrite the report to accommodate other, unspecified, uses. The Army believes that this would be inappropriate. It is acknowledged that all bias is equally wrong. The Army has made every effort to eliminate any improper emphasis in this report.

Comment IV:

A similar concern is that much of the report appears to contain vestiges of earlier, litigation-oriented documents. For example, sections dealing with contamination history, distribution, and effects frequently discuss only pesticides for samples in which arsenic and mercury were also detected. Examples of this lack of balance are indicated in the specific comments. We suggest that the Army review the report in light of this comment and make revisions where indicated.

Response:

The historical portions of this document were compiled from several sources. Specific concerns are addressed under specific comments.

Comment V:

In many instances, the report discusses pesticides in a manner that suggests they all are Shell compounds. Two points need to be clarified. First, Shell did not manufacture DDE/DDT. Second, the Army used DDE/DDT and numerous other pesticides at RMA, including aldrin and endrin. This latter fact is described in Appendix C, but little mention is made of it throughout the report. Thus, conclusions about adverse effects potentially attributable to pesticides do not necessarily implicate Shell operations, as the RI implies.

Response:

While Shell did not manufacture DDT or DDE, CF&I did. The Army acknowledges that it used pesticides in relatively small amounts at the Arsenal as noted in Appendix C. However, based on differences in bulk of pesticides used by the Army and that manufactured by lessees, the great bulk of pesticides on the Arsenal can be attributed to lessee operations.

Comment VI:

We do not oppose references to historic (i.e., pre-RI) data on contamination or apparent contaminant effects. However, we believe that these earlier reports should be relegated to a less prominent role in the RI. Our reasons for this belief are twofold: (1) most of the earlier analytical methods yield less reliable values than the USATHAMA-certified methods and reporting limits followed by the parties during RI studies; and (2) it is, in fact, the present condition at RMA that should influence remediation planning and decisions relation to the biota. Aquatic studies are particularly problematical in this regard.

Response:

The Army's response is included in the response to Comment VII.

Comment VII:

For the most part, the report does an acceptable job of weaving material provided by Shell and its contractor, MKE, into the overall fabric of the report, aquatics again being an obvious exception. However, much of the input by Shell/MKE dealing with contaminant effects was omitted from the RI. Virtually all of this information tended to support the conclusion (and the growing public awareness) that adverse effects resulting from chemical production/disposal and military operations at RMA are tied to the major source areas. We believe that it would be appropriate and helpful to the reader for these evaluations to be included.

Response:

Pertinent information regarding RMA contamination was included as appropriate in order to complete the RI. Historical reports were relied upon to reflect past circumstances in relation to current studies. We agree that the aquatic studies are problematic, but the incorporation of historical information was necessary in order to address particular issues such as bioaccumulation and the definition of exposure pathways. This is because we were unable to evaluate the full results of Shell/MKE studies since the reports of these investigations have not yet been made available. Some Shell/MKE results, while "tending to support the conclusion ... that adverse effects ... are tied to major source areas" appeared to be largely inconclusive due to the sample design, which failed to adequately address other sources of disturbance on RMA (e.g., weed control, fire, etc.). Differences in effects between sites of contamination and other onpost areas are addressed for species with limited areas of movement (e.g., terrestrial plants, cottontails, prairie dogs, etc.).

Comment VIII:

The proposed "site-specific criteria" referred to as the basis for remedial planning for the major contaminants seem to have been developed independently of the overall Endangerment Assessment effort. We recognize that wildlife issues and human health issues do not exactly coincide. However, we are concerned that these site specific criteria may receive an inappropriate emphasis, and may distort the judgment as to what constitutes a remedy that will protect human health and the environment in a cost-effective manner.

Response:

Proposed acceptable levels were developed by a process similar to that used in the endangerment assessment. Acceptable concentrations were used in this document to assist in evaluating potential adverse effects. Preliminary application of these acceptable levels as potential site-specific criteria might indicate that remediation will be necessary to restore certain sites on RMA to acceptable contaminant levels. Shell's concern is unclear in this comment, particularly in regard to the phrase "inappropriate emphasis" and "distort the judgement". There will be no inappropriate or distorted judgments.

RESPONSE TO SPECIFIC COMMENTS FROM SHELL OIL COMPANY ON THE TASK 9, BIOTA REMEDIAL INVESTIGATION DRAFT FINAL REPORT (VERSION 2.2)

Comment 1:

Executive Summary:

Page xiv, second Paragraph

Phase I was a literature survey and did not entail the collection of certified data as was accomplished in Phase II of the program. Much of the previous data had not been obtained with the intent of describing the nature and extent of contamination of biota at RMA, but rather was narrow in focus and dealt only with chlorinated hydrocarbons/pesticides. Therefore, many of the conclusions drawn from earlier studies are biased because other compounds were not included in the analyte suite. These earlier data should be used as background only, and not as an integral part of the RI itself.

Response:

We agree with the statements regarding the purpose and scope of many earlier studies; however the data presented should not be ignored.

Comment 2:

Page xv, first paragraph

For consistency within the first sentence, add "Morrison-Knudsen Engineers, Inc." after "their contractors."

Response:

See text change page xv.

Comment 3:

Page xvi, second paragraph

This paragraph exemplifies many of the problems we have with the Executive Summary, as outlined below.

Comment 3a:

First sentence

Only two plant species were sampled: annual sunflower at three locations, and field bindweed (wild morning-glory) at two locations. Sampling locations consisted of Basin A (both species), Basin C (sunflower), and one onsite control for each species. Yet this sentence refers to studies of "terrestrial plants" as if they were widespread. The sunflowers with arsenic were not "from the vicinity of Basin A," they were from Basin A itself, which all parties agree is contaminated and needs to be remediated.

Response:

Species were selected on the basis of what occurred in sites of contamination (e.g., sunflower and morning-glory). The sunflowers contaminated with arsenic were from edge of Basin A. See text page xvi for changes that provide more detailed perspective on the results presented.

Comment 3b:

Second sentence

The discussion about arsenic "contributing . . . to the reduced plant diversity is some areas" is both unfounded and misleading. The "some areas" are the bottom of Basin A, which should be stated explicitly. The text at page 5-315 et seq. correctly states that the "hypothesis" about the relationship between arsenic and diversity could not be adequately tested because of physical disturbance and compaction (again, in Basin A). That being the case--along with the possible influence of factors such as salinity, alkalinity, texture, and periodic inundation--why even make the speculation? Sunflowers also occur in an area of reduced plant diversity on the floor of Basin C, yet arsenic was BDL in that sample.

Response:

The comment relating arsenic to reduced diversity is well founded in the literature (see Section 5.2). The fact that this relationship could not be conclusively established for RMA is already addressed in this sentence.

Comment 3c:

Third sentence

It is misleading to say that dieldrin levels were "detected in plants": they were only found in the single sunflower sample from the floor of Basin C and two of five bindweed samples from Basin A, but in none of the five sunflower samples from Basin A.

Response:

We do not understand how this statement is misleading if dieldrin was detected in both species of plants analyzed. However, see text change on page xvii.

Comment 3d:

Fourth sentence

"Endrin levels" is misleading; endrin was found in only one plant sample (sunflower, Basin C).

Response: See text for change from "levels" to "level" on page xvi.

Comment 3e:

Fifth sentence

This sentence is speculative and should be deleted. The text at page 5-316 states that the pesticides may have accumulated on the leaves, rather than translocated through the plants (i.e., it was not taken up by the plants). Furthermore, the seeds were BDL for dieldrin, yet most of the food value of sunflowers is in the seeds. Thus, there would be no impact to seedeating birds or small rodents. As far as representing an exposure source to herbivores such a grasshoppers, rabbits, and deer, we point out that (i) sunflowers have large, coarsely hairy leaves, and (ii) the single sample was from the dusty floor of Basin C. Therefore, it does not seem likely that this is representative of plants in the basins on the remainder of RMA.

Contaminant levels in (or on) leaves that are problematical in theory are irrelevant if they do not harm the plant, are not consumed, or are consumed but do not contribute significantly to the food web. Such would seem to be the case with sunflowers from the bottom of the basins. Furthermore, even if dieldrin and endrin had been found in the seeds of the sunflowers, one would need to evaluate whether the species is abundant or widespread and representative of plant species in general before determining whether bioaccumulation could have adverse effects on herbivores or carnivores.

Response:

The statement in the text is true, and it is supported by data from RMA studies. Regardless of how pesticides are deposited on or in the plant, animals that consume the leaves would ingest and potentially bioaccumulate these compounds. The grasshopper species found in contaminated sites are common in the region and are known to consume sunflower leaves. Grasshoppers are a major component in the diet of kestrels, and some kestrels are contaminated with dieldrin.

Comment 4:

Page xvi, third paragraph

Again, the paragraph is misleading. The "sites of contamination" (last sentence) are near the major basins in Sections 26 and 36, yet this paragraph reads as if such levels were widespread.

Concerning mercury, we question the statement that the so-called "dietary level" for birds was truly exceeded. The highest value was barely above the recommended level (0.108 vs. 0.1 ppm), and only two of four samples from Basin

A had mercury hits. Thus, the average value was well below the guideline. Also, all of the other sites sampled were BDL for mercury.

Response:

The paragraph states that dieldrin and endrin were present "in RMA sites of contamination". This does not make the paragraph read "as if such levels were widespread." Grasshopper samples were composites of 50 or more individuals per sample; thus each value provides an average for the site. The text of this document clearly indicates that onsite control areas (e.g., uncontaminated sites) were also sampled.

Comment 5a:

Page xvii, second paragraph

This paragraph also is misleading. It is true that dieldrin was detected at potentially lethal levels in eight of fourteen dead or dying raptors collected opportunistically during RI studies. However, the first sentence is not correct as worded, because it suggests a much more widespread occurrence.

Response:

The "other raptor species" (ferruginous hawk, red-tailed hawk, and great-horned owl) are mentioned in the sentences that immediately follow. The text does not suggest anything beyond what is supported by the data.

Comment 5b:

"Necropsy data" did not show "typical signs of pesticide poisoning." Antemortem signs in one bird included convulsions, which are consistent with pesticide poisoning, but also with other causes of death and therefore not diagnostic. Postmortem evidence was apparently limited to empty stomachs and crops. Any animal sick enough to die can also be too sick to eat; this is a nonspecific finding.

Response:

Necropsy data did show "characteristic" signs of pesticide poisoning. The text does not state that they were diagnostic.

Comment 5c:

We also disagree with the last sentence as worded. Take, for example, a ferruginous hawk. The species does not nest at RMA, so any member of that species must have come from somewhere else. Knowledge of home range may support the conclusion that it feeds solely on RMA while there. However, it cannot be known where the hawk was before arriving at RMA, or how much of the year it spends there. Bald eagles offsite have higher contaminant levels than

bald eagles onsite; similarly, the highest pheasant tissue value was from far offsite.

We do not disagree with the assertion that some parts of RMA contain contamination in the soil or in the tissue of prey populations which represent an actual or potential risk to raptors. On the other hand, the statement in the last sentence of the paragraph that "RMA [implicitly, as a whole] was the probable source of this contamination" is misleading, because it does not place the findings in the correct perspective. Most of the Arsenal does not contain levels of contamination that represent a threat to the biota.

Response:

The statement is correct as worded. The species is a seasonal resident on RMA, and one of its major prey items (prairie dogs) is known to be contaminated on RMA, thus establishing a probable pathway of exposure. In addition, no nearby offpost potential sources of these pesticides have been positively identified. While a single pheasant collected more than 50 miles from RMA contained a high level of dieldrin, the remaining 16 pheasants collected offpost did not contain dieldrin above the certified reporting limit.

The text does not imply that RMA [implicitly as a whole] was the probable source of contamination; it only indicates that the probable source(s) of this contamination was within the boundaries of RMA. This sentence has been appropriately modified on page xix.

Comment 6a:

Page xvii, third paragraph

The portion of the first sentence dealing with American kestrel reproductive success indicates a misinterpretation of the data and is unsubstantiated. For example, consider these facts, gleaned from Sections 4.3 and 5.3 of the RI:

(a) Kestrel eggs on RMA were larger, heavier, and had thicker shells than those at the offsite control areas.

(b) The percent of nests hatched and percent of nests fledged on RMA were lower than offsite in 1986, but no other possible factors, such as prey base or disturbance, were considered. What was the relative percentage of habitat within the home ranges of the various pairs? What were rodent, songbird, and grasshopper populations in each? Furthermore, the study used nest boxes, which create nest sites in areas irrespective of their habitat quality otherwise. This would include vegetation structure (i.e.,

height of the plants) as well as prey base.

(c) Kestrels onsite fledged essentially the same number of young per successful nest (3.13) as offsite (3.12) in 1986. The lower overall nesting success was due solely to higher nest failures, mostly in uncontaminated areas. The number of hatchlings per nest was actually higher on RMA.

(d) The eggs and nestlings sampled onsite had higher levels of dieldrin and mercury than offsite, yet no discussion is provided concerning tissue levels in failed versus successful nests. In fact, the area with the lowest nesting success (First Creek) had concentrations of pesticides mostly below certified reporting limits in kestrel eggs and young.

(e) The pattern of nest failures in 1986 is completely inconsistent with the pattern in 1982 and 1983. Since the distribution of contaminants has not changed, it is obvious that some factor other than contamination is the major contributor to nesting success. See item (b), above.

In summary, kestrel reproduction was not "inhibited": overall reproductive success was lower in 1986 (2.24 per nest attempt onsite, versus 2.78 offsite), but not in a pattern reasonably attributable to contamination. In fact, in 1982, a higher percentage of nests hatched on RMA than the control.

Response:

(a) There is no statistical basis for stating that "kestrel eggs on RMA were larger, heavier, and had thicker shells than those at the offsite control areas": this is not a fact. Using this approach, the table from which this information was drawn could also be interpreted to show that the eggs onpost had less volume than those from offpost control sites.

(b) Nest boxes were used onpost and offpost: kestrels were allowed to select their own nesting site and related habitat from the choices made available to them at both locations.

(c) Due to the lack of information regarding individual home ranges, it is not correct to state that the nest failures were mostly from uncontaminated areas. The report avoids making statements that are speculative and are unsupported by a scientific analysis of the data.

(d) Statistical analyses performed on these data, using the approach agreed to by MKE, indicate no differences in tissue levels of mercury between onpost and offpost sites (see Section 4.3.2.3). RMA kestrels are considered as a single group.

(e) All kestrel data for RMA were considered together due to lack of information on home ranges, feeding areas, and other pertinent data. Insufficient information is available to reach the conclusion in this comment.

Statistical analysis of 1986 data indicate no difference in nesting success between onpost and offpost kestrels. Data collected in 1983 did indicate a difference; hence it appears that the contaminant effects documented in 1983 have been reduced.

Comment 6b:

Concerning mallard reproduction, the report again oversteps the data. The fact that only two nests were found does not strongly suggest that reproduction was inhibited by contaminants. How many adult mallards were present during the nesting season? How many attempted to nest but failed physiologically? How many nested but lost their nest to predators? For example, MKE biologists found a mallard nest in 1986; the next day it was found to have been destroyed. To attribute the low number of mallard nests to contamination is unfounded.

Finding only two nests might suggest that reproduction was inhibited by chemical contamination, if levels in the adults were at levels known to have this effect. However, the two mallards from Lower Derby Lake were "BDL" for dieldrin. Canada geese are very successful breeders in the same waters, yet such was not mentioned. Why not?

Response:

The fact that only two nests were found by itself does not strongly suggest that reproduction was inhibited by contaminants. The concentrations of dieldrin in the mallard eggs from the Lower Lakes combined with the lack of nests does suggest inhibition by contaminants. The assertion that "Canada geese are very successful breeders in the same waters" was not mentioned because: 1) there were no data to support this and 2) geese are primarily grazers and would not be expected to have the same level of exposure to contaminated lake conditions as would dabbling ducks such as mallards. To mention it without data would have been speculative and misleading.

Comment 6c:

The last sentence is another inappropriate oversimplification. FDA action levels are based on the amount that could be tolerated by a human consumer if 100 percent of the consumer's source of a particular food item were at that level. Thus, the FDA action level for "poultry" (extrapolated to include pheasants, mallards, and doves) assumes that all of a person's ingestion of poultry over the long term would have to be at that level or higher before representing a health risk. Such is clearly not the case. Also, the FDA levels assume an average concentration: using the highest concentration is valid only if it represents an acute toxicity, which it does not.

Furthermore, the text does not state what percentage of the samples of wild fowl contained such levels onsite, and from what areas. The pheasants with dieldrin above FDA levels were generally near known sites of contamination and not widespread across the RMA. Again, the reader is left with the wrong impression. The Executive Summary should be accurate as well as brief.

Response:

The sentence in the text is true as stated: however modifications have been made on page xvi to avoid possible misinterpretation.

Comment 7:

Page xvii, fourth paragraph

Two important facts were omitted: (1) dieldrin in the single mule deer sample above BDL was well below FDA action levels: and (2) the level in the coyote was near the lower limit of the range stated later in the report as being lethal to dogs.

Response:

There are no established FDA action levels for liver tissue. The text statement regarding the coyote sample is true as stated, but has been modified on page xvii to state that it is at the lower limit of the range.

Comment 8:

Page xvii, fifth paragraph

Again, this is an oversimplification that is not well supported by the data. We do not disagree that dieldrin bioaccumulates, but one who did not already believe so could not easily deduce it from the data. This paragraph is self-serving and does not belong in the Executive Summary.

Response:

This statement is based on data from the sampling program and provides the appropriate type of generalization that makes this report comprehensible to the general reader. It forms the basis for evaluating many of the contamination effects and for substantiating the pathways as is required by the National Contingency Plan.

Comment 9:

Page xviii, first paragraph

What the Shell/MKE studies showed was that (1) the South Lakes are healthy, highly productive aquatic ecosystems, better in most respects that the offsite control lake; (2) a few samples showed mercury and pesticides above FDA levels; (3) higher predators (viz., bass and pike) tended to show greater concentrations of dieldrin and mercury than lower trophic level species (e.g., bluegill); and (4) mercury tended to accumulate in fillets, while dieldrin tended to accumulate in inedible portions (viscera). Also, we reported in our Phase I literature review, pesticide levels have declined dramatically since 1970, while mercury has remained about the same.

Response:

The Army does not agree with all of the assertions made in this comment. Data on invertebrate populations, age-length relationships of fish, and other data that are necessary to draw any conclusions with respect to the health of the aquatic ecosystems are not forthcoming from the studies performed by MKE. This comment is self-serving inasmuch as it is not based on adequate data.

Comment 10:

Page xviii, second paragraph

This paragraph misstates the results of the RI studies. Problem concentrations occur near a few major source areas, not "contamination sites." What are the alleged "variety of lethal and sublethal effects?" The only things well documented are that (1) some individuals of some species have been found dead with pesticide levels that might explain their mortality; (2) prey species in some areas have dieldrin levels that represent a potential risk to higher trophic levels; and (3) some individuals of some game species contain dieldrin (or other contaminants) at levels that would not be acceptable if they constitute the sole source of poultry and fish in the diets of humans over a lifetime.

To accurately reflect reality, and to provide information needed by the reader in evaluating remedial options, this paragraph needs to be balanced with a statement about the overall quality of terrestrial and aquatic ecosystems at

RMA and the limited extent of areas representing existing or potential exposure risks.

Response:

The Army does not agree with this comment. Minor changes have been incorporated on page xviii in order to avoid possible misinterpretation.

Comment 11:

Page xviii, third paragraph

How could the AChE inhibition be caused by naturally occurring heavy metals that are not above background levels? This paragraph should be rewritten or deleted.

Response:

The background levels in this area are considered high relative to many areas due to the nature of the substrate from which the soils are formed. Instances of naturally occurring high levels of metals are well known in the literature: hence the location of mines, etc. for particular metals.

Comment 12

Page xix, first paragraph

The first sentence goes without saying. The point is whether some areas would need to be remediated based on protection of biota other than those areas already recognized as needing remediation. Equally important issues are long-term land use, trade-offs between remediation and adverse impacts associated with habitat loss, and the relationship between alternative remediation technologies and any deleterious consequences associated with them.

It appears that the Army has already established cleanup levels for protection of biota without input from Shell or other concerned parties, and without attempting to be consistent with the overall Endangerment Assessment for RMA. If so, this is premature and inappropriate.

It is not enough to say that the Biota RI will be used by those making decisions regarding the Feasibility Study and the Endangerment Assessment, and that those persons will decide what significance to give the stated cleanup levels. The cleanup levels in the report, if they are to be included, must bear a reasonable and responsible relationship to reality at RMA. They should not represent abstract and theoretical standards which play no constructive role in producing a remedy that will protect human health and the environment in a cost-effective manner.

Response:

This document calculates acceptable levels of contaminants for abiotic media based on regionally specific biological information and does not address the issue of what, if any, sites are in need of remediation. This issue will be considered as part of the endangerment assessment and feasibility study process and subsequent actions in order to formulate response objectives. The Army has not established cleanup levels and welcomes input from all concerned parties.

Criteria development in the EA is concerned only with the human health implications of contamination at RMA. This report does rely on much of the same physical and toxicological data, but the endpoint of the analysis is independent. The statement ".... without input from other parties" is simply incorrect inasmuch as Shell has had ample opportunity for comment in its significant support role, and the purpose of a draft report is to solicit input. The Army does not share Shell's negative view of the outcome of the RI/FS.

Comment 13:

Page xix, third paragraph

The reference to the proposed Consent Decree should be replaced by references to the Federal Facility Agreement and the Settlement Agreement.

Response:

The change has been incorporated on text page xix.

Comment 14a:

Section 1.0, Introduction

Page 1-1, first paragraph

Whether the SARs and RI media reports in fact "fulfill the requirements of defining the nature and extent of contamination," as required by CERCLA, SARA, and the NCP, remains to be seen. It would be more correct to state that the reports were one "in accordance with" the various requirements.

Response:

See text change on page 1-1.

Comment 14b:

Section 1.0, Introduction

Page 1-1, first paragraph

In the last line, we suggest replacing "to present an overall environmental contamination assessment" with "to provide a synopsis of contamination at RMA as related to vegetation, wildlife, and aquatic resources."

Response:

See text change on page 1-1.

Comment 15:

Page 1-3, first paragraph

Again, we do not believe it appropriate to state that "[t]his document . . . fulfills requirements for the remedial investigation of biota . . . ." Rather, it was done "to fulfill" or "in accordance with" these requirements.

Response:

See text change on page 1-3.

Comment 16:

Page 1-3, second paragraph

The site background discussion should also include the production of wheat rust spores, TX, and the dispersal of metalbearing ash from the destruction of ammunition.

Response:

See text change on page 1-8. The section discusses the history of production at RMA and not disposal practices. Therefore, it is not relevant to discuss disposal practices at this point.

Comment 17a:

Page 1-4, first paragraph

In the second line, replace "Hyman assumed CF&I's lease on the chlorine plant . . ." with "Hyman leased some of the facilities previously leased to CF&I . . . ." Also, Hyman did not produce herbicides at RMA (line 5).

What is the basis for including Shellchlor in the list of chemicals produced at RMA?

Response:

See text change on page 1-4.

Comment 17b:

Absent from this paragraph is any mention of the extensive use of insecticides, herbicides, and soil sterilants used by the Army on RMA, both during wheat rust (TX) research, and for various site management purposes. This includes organic as well as inorganic compounds (see Appendix C).

Response:

Regarding the third paragraph of the comment, Appendix C is a literature review of the Army's use of herbicides and pesticides at RMA. It is based only on the existing record. Quantification of Army usage is not possible from the survey. Significantly, use by Hyman and Shell of herbicides and pesticides is not addressed, as evidence of the nature and extent of such usage is not available. Consequently, any characterization of Army use as "extensive" is not supported by the appendix.

Comment 18:

Page 1-4, second paragraph

Other lessees also discharged chemical wastes into the disposal basins through the system provided by the Army. In line 3, add "It is now known that" in front of "[t]hese basins."

Response:

The text discussing the disposal basins has been deleted.

Comment 19:

Page 1-4, third paragraph

Near the bottom of the page, it should be noted that hunting was permitted at RMA during previous times (i.e., by members of the Rod and Gun Club).

Response:

See appropriate text change on page 1-4.

Comment 20:

Page 1-6, second paragraph

In lines 8 and 9, the report fails to mention pesticide production by CF&I, use of pesticides by the Army (see Appendix C), or contamination by arsenic and mercury. We would suggest simply stating that the "lakes were contaminated by mercury, and pesticides during operations at the Arsenal."

Response:

See appropriate text change on page 1-5.

Comment 21a:

Page 1-7, first paragraph

In line 7, "Discharges" suggests (as does "practices" in line 10) intentional pollution of the South Lakes. It would be more accurate simply to state that the lakes became contaminated with pesticides and metals.

Response:

See text change on page 1-5.

Comment 21b:

What is the factual basis for stating that discharges occurred not only in the lakes, but also "surrounding areas?" What is meant by the term "surrounding areas?"

The reference to "SCC and other lessees" is biased, because Army compounds are also present in the lakes

The final sentence is a misrepresentation of actual, present conditions.

Response:

See text change on page 1-5. No chemicals which are unequivocally Army chemicals have been detected in the Lower lakes above possible elevated background levels.

Comment 22a:

Page 1-7, second paragraph

In tne first sentence, what is a "chemical settling basin?"

Response:

See text change on page 1-7.

Comment 22b:

The second sentence creates the incorrect impression that Basins A through F were all used throughout the 1950s, '60s, and '70s. It would be more accurate to state that the unlined basins were used until 1957, after which the lined basin, Basin F, was used. In the first sentence, it is an exaggeration to state that offpost wells resulted in "potential exposure to wildlife."

Response:

See appropriate text change on page 1-7.

Comment 22c:

The second half of the paragraph should either be deleted as irrelevant, or put into some proper perspective. Otherwise, this again paints an inaccurate picture of the nature and extent of contamination as related to biota.

Response:

The fact that sites of localized contamination are distributed throughout RMA and the nature of past disposal practices are relevant to this document. See appropriate text change on page 1-7.

Comment 22d:

In lines 13 and 16, add "burial" before "trenches."

Response:

See text change on page 1-7.

Comment 22e:

In the last sentence, we suggest adding "inorganic salts" to the list, as well as "Army surety compounds and their degradation products."

Response:

See appropriate text change on page 1-7.

Comment 23:

Page 1-8, first paragraph, first sentence

Rather than say that wildlife mortality "has been considerable in the past," it would be preferable simply to say that wildlife mortality occurred, without suggesting its extent. If the statement refers specifically to ducks, for which mortality probably was "considerable," then that point should be made clearer. Furthermore, "has been" connotes a continuing condition; the simple verb "was" is preferable.

Response:

See appropriate text change on page 1-7.

Comment 24a:

Page 1-8, first paragraph

To say that Table 1.3-1 only lists "documented cases" of wildlife mortality (penultimate sentence) is to ascribe greater trustworthiness to Table 1.3-1 than it deserves. See our General Comment No. VI. Also, the assertion that "death and injury to wildlife may have occurred in varying numbers at other locations at other times" later in the same sentence is sheer speculation and should be deleted.

Response:

See text change on page 1-8.

Comment 24b:

We question the relevance of the paragraph and Table 1.3-1 to the purpose of the RI: determining the present nature and extent of contamination. It is present contamination, not historical contamination, that must be remediated.

Response:

Injuries listed provide information that assists in determining the present nature and extent of contamination. These data provide a starting point for present studies.

Comment 25:

Page 1-9, Table 1.3-1

This table, besides presenting data of doubtful relevance to the RI, exemplifies the problem mentioned in our General Comment No. VI. Specifically, results for earlier, sometimes rather crude analytical techniques are given full weight. If "pesticides" were not confirmed by GC/MS, the data are of little value. Were other compounds analyzed for--such as metals--that would also cause mortality? In many cases, the answer is that the analytical suite was very limited. Did the authors cited actually diagnose death as being due to pesticides, or did they merely speculate. Is there any way of knowing the origin of the individuals opportunistically found and analyzed? For example, great blue herons do not nest or roost at RMA, yet the 06/09/82 entry on the table clearly implies that the heron died of pesticides acquired at RMA. Do fish or other prey species in the South Lakes contain levels of these compounds lethal to herons?

Response:

Comment noted. This table has been edited to remove the column heading "Apparent cause of death" and replace it with "Notes". Again, regardless of the specific conditions or concentrations for each study listed here, the table serves simply to document the wildlife mortality and the apparent cause of this mortality in the past.

Comment 26:

Page 1-12, last paragraph

Again, we question the development of biota criteria independently of the EA process and without input from other parties. Furthermore, the data used are heavily biased toward the relatively few, relatively small areas of substantial contamination, and the approach used is one of unreasonable worst-case scenarios rather than realistic source-contribution. As a result, we are concerned that unreasonable cleanup levels will result. See General Comment No. VIII and specific comments on Section 5.0.

Response:

Comment noted. See also response to General Comment No. VIII.

Comment 27:

Section 2.0, Environmental Setting

Page 2-2, second paragraph

The first sentence should be changed to read that the basins were intended for the "disposal" rather than "storage" of wastes.

Response:

See appropriate text change on page 2-2.

Comment 28:

Page 2-2, fourth paragraph

We suggest replacing "has characteristics of" with "is characterized by."

We suggest revising the statement that rainfall is "relatively light." It frequently is very heavy. "Light" refers to intensity (i.e., amount per unit time); what is meant is "low" rainfall.

The last sentence is confusing. If this means that the average diurnal fluctuation is about 28oF, then it should be so stated. If not, then we do not understand what is meant.

Response:

See text changes on page 2-2.

Comment 29:

Page 2-4, first paragraph

The discussion concerning chinooks implies that they are warm because they come from the southwest. They are mostly from the west to northwest (not southwest) and are warm because they are decreasing in elevation and thus warming at the adiabatic rate (5.5°/1,000 ft). Chinooks have a strong desiccating effect, resulting from the combination of high speed and warming (and thus increased moisture-holding capacity). These winds are especially important from the perspective of windblown dispersal of particulates because of their high speeds.

Response:

Comment incorporated. See text change on page 2-4.

Comment 30:

Page 2-4, third paragraph

The prevailing winds at RMA are from the south and southsouthwest. North and east are both more prevalent directions than southwest; west to northwest directions probably are more important because of frequency of high windspeed events.

Response:

The sentence was changed on page 2-4 to " The prevailing winds at RMA are from the south and south-southwest."

Comment 31:

Page 2-4, last paragraph et seq. (Regional Biota)

We have few comments on this section, much of which was provided by Shell/MKE. In general, we disagree with the use of present rather than past tense, in keeping with normal practice of scientific writing.

Response:

The use of present tense is in keeping with the tense used for the environmental setting sections in the air and water RI reports.

Comment 32:

Page 2-9, fourth paragraph

We do not agree that ring-necked pheasants are "abundant" or "dominant" in prairie habitats. They generally are most common in agricultural lands or, to a lesser extent, weedy bottomlands. Their abundance on RMA is due in large part to the prominence of tall weedy forbs, which duplicate the high cover, open ground surface, and abundant seed production of grains. They also are common in cattail marshes at RMA. These are not "prairie" habitats per se.

Response:

See text change on page 2-9.

Comment 33:

Page 2-13, last paragraph, fourth line

Add a comma after the closed parentheses. Note that corn is a grain crop; wheat and barley are "small grains."

Response:

See text change on page 2-13.

Comment 34:

Page 2-16, third paragraph et seq. (Study Area)

See the comment to Page 2-4, last paragraph.

Response:

See the response to the comment on page 2-4, last paragraph.

Comment 35:

Page 2-20, fourth line

Note alignment of the first word. This paragraph exemplifies the problem of using present tense: breeding anurans were heard during RI studies in early spring. To say that they are heard is imprecise. Note also, for consistency, that the next paragraphs discuss waterfowl in the past tense.

Response:

See text change on page 2-20.

Comment 36:

Page 2-22, second paragraph

To avoid confusion, we suggest replacing the comma after "marshes" with a period, making "rock" the start of a new sentence, and adding the predicate "were (are) common" before "around buildings."

Response:

See text change on page 2-22.

Comment 37:

Page 2-23, second paragraph

The data do not suggest that northern harriers are a "dominant breeder." No mention is made of Cooper's and sharp-shinned hawks in groves or riparian woodlands, nor were prairie falcons mentioned in this subsection.

We suggest making greater use in the raptor subsection (beginning at Page 2-22, third paragraph) of the text provided by Shell/MKE.

Response:

Field notes reveal that 3, and possibly 4 pairs of northern harriers were observed on RMA in spring 1987. The three confirmed pairs demonstrated breeding behavior. Nests were not located due to the northern harriers ground nesting habits and their susceptibility to disturbance. Nonetheless, the sentence has been changed to read "Swainson's hawks and American kestrels are the dominant breeders."

Reference to Cooper's and sharp-shined hawks, as well as other species have been added to the text on page 2-23. The raptor subsection provided by Shell/MKE has been incorporated as appropriate.

Comment 38:

Section 3.0, Sampling and Analysis Program Page 3-3, first paragraph

The heading "Criteria Development" is misleading because no criteria are discussed in this section--unless this is intended to identify how the compounds of concern were determined. In the last line, the other experts on RMA contaminants should be identified.

Response:

See appropriate text change on page 3-3.

Comment 39:

Page 3-3, third paragraph

A missing element of significance is the impact of time on the concentration of the various chemicals in the environment, particularly those of concern in the various biota. Inclusion of this factor would demonstrate, for example, that the concentration of dieldrin in fish has been declining without any overt remediation.

Response:

The impact of time is addressed in sections that compare historical data to recently obtained values. Time could not be addressed during the Phase I studies described in this paragraph because of the lack of appropriate data.

Comment 40:

Page 3-5, second paragraph

We suggest rewriting the last sentence. Data provided by Shell/MKE were substantial--in several instances comprising the bulk of data available--and were used for more than merely "augmenting existing information."

Response:

Descriptive data describing current ecological conditions provided by Shell/MKE were substantial and because of their availability, were used in Section 2.0, Environmental Setting. While useful, these data are not essential. Data on contaminant levels in selected species/categories, especially aquatic data, were more relevant to the objectives of the RI process. The text has been modified on page 3-5 to reflect this.

Comment 41:

Page 3-9, second paragraph

The correct spelling is "hangars" (line 2).

Response:

See text change on page 3-9.

Comment 42:

Page 3-9, fourth paragraph

Add that McKay Lake is comparable to the South Lakes in age, area, depth, adjacent vegetation, substrate, and water quality, and supports most of the same fish species as the South Lakes.

Response:

See text change on page 3-9.

Comment 43:

Page 3-11, et seq., Table 3.2-1

On the first page, both spotted ground squirrels and striped skunks have been documented as present on RMA and should be marked by an "x."

On the second page, the merlin and eastern screech-owl (note the hyphen) have been reported as present and should be so indicated.

On the third page, buffleheads have also been observed. Also, the correct spelling is "chukar."

Response:

See text changes on pages 3-11. et seq..

Comment 44:

Page 3-20, third paragraph

In light of the problems with the kestrel study outlined in a previous comment, we recommend changing "known" to "reported" near the end of line 6.

Response:

See text change on page 3-20. This change was incorporated because "reported" is more correct, not in acknowledgement of alleged problems with the study.

Comment 45:

Page 3-21, fourth paragraph

In the second sentence, we suggest changing "would be" to "were" and placing "habitat" before "available." Were more nests expected because of the vegetation present, because a large number of paired adults were present during the breeding season, or because of some other factor?

Response:

The word changes have been incorporated into the document on page 3-21. The remainder of this comment pertains to a citation of available literature. Please refer to RIC document no. 87091 R04, McEwen, L.C., and L.R. DeWeese. 1985., Preliminary Investigations of Aldrin, Dieldrin, Endrin and Mercury Residues in Eggs and Young of Waterfowl Nesting at the Rocky Mountain Arsenal, Denver, Colorado. Draft Report.

Comment 46:

Page 3-30, first full paragraph

As indicated in the last sentence, organisms found dead also were analyzed. We agree that this is appropriate, but the results should be kept in perspective, i.e., that they represent extreme rather than typical situations.

Response:

Comment noted.

Comment 47:

Page 3-30

The addition of a table providing the ranking scores for the listed criteria for each of the 39 contaminants would be helpful.

Response:

Details of the contaminant selection process are provided in the Phase I and Phase II Biota Assessment Final Technical Plan.

Comment 48:

Page 3-31, second paragraph

We are unaware that isodrin is converted to endrin in the environment, including metabolic processes (lines 8-9). What is the basis for the statement in the text?

Response:

See Matsumura, 1980. Toxicology of Insecticides, p. 200 (...isodrin is metabolically converted to endrin (e.g. Brooks and Harrison, 1963), and since isodrin is not an economically important compound, endrin is the one that has been studied by scientists."

Comment 49:

Page 3-34, third paragraph

The text indicates that a pathway analysis was used to develop criteria levels for the protection of the regional biota. Was the procedure reviewed by the parties and agreement reached upon the method of application? See General Comment No. VII

Response:

The general procedure (e.g., food web approach) was discussed at meetings of the Biota Assessment Committee (currently the Biota Assessment Working Group) and was again reviewed by the parties as presented in the Phase I and Phase II Biota Assessment Final Technical Plan. The pathway approach was included in the approved RMA Biota Assessment Technical Program Plan.

Comment 50:

Page 3-40

The logic for the use of K_d in place of K_{oc} information is not immediately evident. The following statement should be added: "K_d values are used where it is desirable to have an estimate of the partition of the contaminant between the solid and the aqueous phases in aquifers."

It should also be noted that equation (8) could be written

$$C_{sed} = C_w \times K_d$$

because by definition $K_d = K_{oc} \times f_{oc}$. See Warren J. Lyman and Christopher P. Loreti. "Prediction of Soil and Sediment Sorption for Organic Compounds." Arthur D. Little, Inc. for U.S. EPA Washington, D.C. June 1987. Contract No. 68-01-6951.

Response:

The content of the paragraph is clear to the average reader, and to incorporate this comment would merely be redundant.

Comment 51:
Page 3-41, et seq. (Supplemental Studies)
See the comment to page 2-4, last paragraph.

Response:
This comment is confusing; page 3-41 et seq. is already in past tense.

Comment 52:
Page 3-43, second paragraph, line 2
The "e.g." should be changed to "i.e."

Response:
See text change on page 3-45.

Comment 53:
Page 3-45, first paragraph, line 2
Change "birdweed" to "bindweed."

Response:
See text change on page 3-45.

Comment 54:
Page 3-45, second paragraph
The statement in the first sentence should be reworded as follows: "Species with the greatest average heights along the transects tended to be tall gaura and annual sunflower. However, tumble mustard, tansy mustard, and prickly lettuce were the more common tall species." We realize that MKE provided the imprecise wording used in the RI.

Response:
See text change on page 3-45.

Comment 55:
Page 3-47, fourth paragraph, line 4
"[W]eedy" should be changed to "woody" so that the sentence reads: "Mean density of woody plants and cactus . . . ."

Response:
See text change on page 3-49.

Comment 56:

Page 3-48, fourth paragraph

To be consistent with descriptions of previous plant communities, the discussion of crested wheatgrass should include that it covered 1,330 ha or 19 percent of RMA.

Response:

See text change on page 3-50.

Comment 57:

Page 3-50, first paragraph

Include the fact that rubber rabbitbrush shrublands covered 24 ha or 0.3 percent of RMA.

Response:

See text change on page 3-51.

Comment 58:

Page 3-51, third paragraph

Add a statement that locust thickets covered 37 ha or 0.5 percent of RMA.

Response:

See text change on page 3-52.

Comment 59:

Section 4.0, Nature and Extent of Biological Contamination

Page 4-1, last paragraph

The use of analytical results obtained by USFWS prior to RI studies is appropriate as background. However, it should be mentioned that these studies often entailed different sampling protocols, analytical suites, laboratory methods, and detection/certified reporting limits. Therefore, they should not be given the same weight as RI studies conducted by the Army/ESE or Shell/MKE. See General Comment No. VI.

Response:

The USFWS samples referenced in this paragraph were analyzed by ESE as part of the Biota Assessment study and met the same standards of laboratory procedure, analytical suites of chemicals, and detection/certified reporting limits as the Army/ESE and Shell/MKE samples.

Comment 60:

Page 4-2, top line

To be accurate, Upper Derby Lake still "exists," and it attracts waterfowl during spring migration.

Response:

See text change on page 4-2.

Comment 61:

Page 4-3, second paragraph

The penultimate sentence fails to mention the filling of Basin C by the Army in the latter years to flush the aquifer.

Response:

See appropriate text change on page 4-3.

Comment 62:

Page 4-3, fourth paragraph

In line 2, add "liquid" before "wastes."

Response:

See text change on page 4-3.

Comment 63:

Page 4-4, first paragraph

The last sentence should be updated, since this action has been completed.

Response:

See text change on page 4-6.

Comment 64:

Pages 4-6 to 4-8

We believe that this information is of questionable reliability in light of analytical methods then available, and that it sheds no light on the present nature and extent of contamination at RMA. Section 4.1.2 is of somewhat more apparent relevance, although it also overemphasizes historic data.

Response:

Since this section is titled "Contamination History", it appropriately emphasizes historical information.

Comment 65a:

Page 4-6, first paragraph

The statement in the first sentence is one-sided, because the Army does not speculate that mercury/arsenic could have been involved. Later analyses demonstrated the pervasiveness of mercury in addition to organochlorine pesticides in South Lakes sediments and biota.

Response:

Comment noted. See Section 5.2.

Comment 65b:

The Army also seems to have overlooked that it formulated aldrin, dieldrin, and possibly endrin in the South Plants, and that it used DDT and other pesticides on the RMA. These points need to be included in the history of the South Lakes.

Response:

The Army did not "formulate" pesticides. A review of Appendix C shows that any comparison between Hyman and Shell pesticide production activities and Army use of pesticides is absolutely absurd. There were no significant contributions of pesticides, including DDT, to the Lower Lakes by the Army. The significant contribution of Hyman and Shell operations has been established and admitted.

Comment 65c:

Why were ducks "particularly vulnerable to pesticides" (lines 5-7)?

Response:

In context, the statement says that they "were particularly vulnerable... during the years in which chemical production was being pursued in the South Plants."

Comment 65d:

In lines 4 and 5, is Finley (1959) the reference for the estimated number of duck deaths? Is so, we suggest combining these two sentences.

Response:

See text change on page 4-6.

Comment 66:

Page 4-6, second paragraph

Did the mallard described in the last two sentences contain mercury or arsenic in its tissues? Were these compounds analyzed for? We suggest deleting "while showing signs of lethal organochlorine contamination" unless diagnostic (not merely characteristic) symptoms were documented.

Response:

See text change on page 4-6. Mercury or arsenic concentrations would not have resulted in the symptoms described by USFWS (1982b).

Comment 67:

Page 4-6, third paragraph

Why is the addition of acid an issue? The lakes were used for cooling water, and it is not even clear (in fact, it is doubtful) that fish existed in the lakes after the caustic was released. Therefore, the addition of acid to lower the pH was probably beneficial; indeed the lakes eventually were restocked. Actions in 1951 must be viewed in the context of the conditions and practices that existed at the time.

Response:

The addition of acid is merely part of the history of contamination in the Lower Lakes. See appropriate text change page 4-6.

Comment 68a:

Page 4-7, first paragraph

Why is mercury not reported here? Why are only maximum pesticide values reported? Shell toxicologists do not believe the 2400 ppm dieldrin value, thus underscoring the questionable analytical results reported with full credulity in the RI.

Response:

See appropriate text change. Early studies did not address mercury as a biota problem. The dieldrin value was for waterfowl visceral fat and was reported by Sheldon et al. (1963) and cited by MKE in their 1987 Phase I Literature Review, Aquatic Resources Investigation, Rocky Mountain Arsenal.

Comment 68b:

Page 4-7, first paragraph

Throughout this discussion, words such as "found" should be replaced with "reported" because data for earlier periods are of questionable validity.

Response:

Comment noted. If contaminants had not been "found", the results would be questionable because of the possibility that the study methods were not sufficiently sensitive. Because these contaminants were detected, we have no basis for questioning their validity.

Comment 69a:

Page 4-7, second paragraph

The Army actually did remove contaminated sediments. The fact that some contamination may remain does not convert removal into a mere "attempt.

Response:

See text change on page 4-7.

Comment 69b:

Page 4-7, second paragraph

The third sentence is erroneous: Table 1.3-1 does not support the proposition that after 1964 wildlife "continued to be found dead at the Lower Lakes with significant pesticide levels in their tissues (see Table 1.3-1)." On the contrary, in the few instances after 1964 in which wildlife injury incidents are listed for the Lower Lakes, the table either omits an apparent cause of death or simply lists contaminants detected in tissues without any indication of the concentrations.

Response:

See text change on page 4-7.

Comment 69c:

Page 4-7, second paragraph

The fourth sentence again fails to mention mercury, or pesticides used by the Army but not produced by Shell (e.g., DDT/DDE).

Response:

The text sentence does not reflect any specific use of pesticides by any specific party. It is not an appropriate section to discuss specific use by parties.

Comment 70a:

Page 4-8, et seq. (Section 4.1.3)

This discussion is unacceptable in its tone, its bias, and its unquestioning confidence in historical data of dubious validity and relevance. The RI

requires that the Army rewrite this section using an objective, scientific style rather than a journalistic style bordering on sensationalism.

Response:

This section catalogs documented cases and occurrences of wildlife injury. Changes have been made throughout the section to remove possible ambiguities. Data presentation was and is objective. The style is journalistic only in the sense that it a presents complicated scientific information in a form understandable to a wider audience of readers than just scientists.

Comment 70b:

Page 4-8, et seq. (Section 4.1.3)

We do not dispute that various species contain contaminants in their tissue, or that adverse effects have occurred. However correct a "fact" may be, it is accurate only if the reader is given the proper perspective. The interrelation of tissue contamination, adverse effects, and overall condition of biotic communities at RMA should be discussed within the context of contaminant distribution. The Arsenal is not the vast wasteland depicted by this section.

These comments apply throughout the section and could practically be repeated for each paragraph. A few of the more unscientific and misleading passages are noted below.

Response:

See specific comments below.

Comment 71:

Page 4-8, second paragraph

The language of this paragraph typifies style which we find objectionable. Words or phrases such as "many," "high levels," "several contaminants," and "all classes of wildlife" are not appropriate and should be replaced with objective, supported statements of fact. For example, the second sentence could be rewritten as follows:

Various investigators have reported contaminants in some species of plants and wildlife at RMA. In some cases, contaminant levels have been found which represent potential risks to humans or the biota. Levels in the tissues of animals found dead or dying at RMA have sometimes been sufficiently high to be suspected as the cause of mortality.

Response:

Some editing has been done to incorporate some suggestions. The term "many" has been changed to several: the term "high" implies that the levels are high enough to "represent potential risks to humans and biota" as suggested. See text change on page 4-8.

Comment 72:

Page 4-8, third paragraph

The phrase "found all over RMA" is sensationalistic and misleading. Even if correct, this does not mean that the source of contamination is all over RMA, as implied, because these are highly mobile species. In the last sentence, were other contaminants analyzed and found?

Response: See appropriate text change on page 4-8.

Comment 73:

Page 4-8, fourth paragraph (continuing on the top of Page 4-22)

We do not understand the first sentence. The second sentence is an inaccurate characterization of the findings, and the remainder of the paragraph is devoid of perspective. The phrase "during the past three decades" is journalistic.

Response:

The first sentence contained typographical errors that have been corrected. The second sentence merely references tables that compile existing information. Inasmuch as this section (4.1) is entitled contamination history, the section has historical perspective. See text change page 4-8.

Comment 74:

Page 4-23, second paragraph

In the third sentence, the issue is not just the presence of phytotoxic chemicals, but whether they are at concentrations known to impact plant growth. We question the relevance of phytotoxins at depths of 7-12 feet, and greater, because this is well below most plant roots--especially those of weedy annuals such as typically occur on basin floors.

Response:

Comment noted. See text change on page 4-23. The paragraph now states the point that there are few of the phytotoxins at depths of 7-12 feet in any case. This paragraph repeats the findings of the article referenced.

Comment 75:

Page 4-23, third paragraph

The discussion erroneously omits metals. The last sentence is misleading and suggests bias.

Response:

This paragraph has been modified to address concerns expressed in this comment. The Army believes that the last sentence is an accurate summary of the findings of these studies and does not suggest bias.

Comment 76:

Page 4-23, fourth paragraph

Again, this is an unacceptable summary of previous studies. A higher value onsite is not necessarily indicative of injury or a potential risk to higher trophic levels and humans.

Response:

This paragraph describes the studies and results and makes no statements regarding injury or potential risk to higher trophic levels and humans.

Comment 77:

Page 4-24, first paragraph

Of what value to remediation planning are the data on dieldrin in the golden eagle found shot near the edge of the Arsenal? Where did the eagle come from? Furthermore, the one eagle that was shot presumably was alive at the time; this casts doubt upon the conclusion that the other eagle mentioned, which had lower pesticides levels, actually died of pesticide poisoning as speculated.

Response:

This paragraph is in the contamination history section and provides background data pertinent to current RI/FS studies. See text changes on page 4-24.

Comment 78:

Page 4-24, second paragraph

In the third sentence, change "have been found" to "were reported." Eggshell thinning was not reported in 1986.

Response:

See text change on page 4-24.

Comment 79:

Page 4-24, third paragraph

The potential for health risks or environmental damage is speculative and irrelevant to the purpose of the RI. The statement, "contaminated above levels acceptable for human consumption" (last sentence) is based on the erroneous assumption that FDA levels are appropriate for wild fish or fowl consumed only occasionally.

Response:

All statements are referenced. The information is historical and need not be modified.

Comment 80:

Page 4-24, last paragraph

The bioassay was, in fact, so "crude" that it should not be given full credence. Moreover, algae levels in 1959 are irrelevant to the purposes of the RI.

Response:

This section is entitled Contamination History. There is no legitimate basis for deleting this statement from this section. No value judgments are made on data presented.

Comment 81:

Page 4-26, last paragraph

How can something be a "major site . . . of potential contamination?" By what reasoning is North Bog Pond included?

Response:

See text change on page 4-26.

Comment 82:

Page 4-27, last paragraph, second sentence

We do not understand why mean concentrations were calculated only if more than half the samples had detectable/reportable levels.

Response:

This is standard methodology and was recommended by the U.S. Fish and Wildlife Service. See text change on page 4-27.

Comment 83:

Page 4-35, first paragraph

The last sentence is typical of the misleading tone that pervades the RI. It states that the single South Plants earthworm sample had dieldrin at 1.93 ppm, while the onpost control (First Creek area) had detectable dieldrin in "only one of seven samples" (emphasis added). Yet the detected value in the onpost control was 5.3 ppm, 2.7 times that of the single South Plants sample. Indeed, the South Plants value is closer to the mean of the control samples including the six BDL values than it is to the one detected value. It would be more accurate for the sentence to read as follows: "Although only one of seven samples from the onpost control contained dieldrin above the certified reporting limit, that value was 5.3 ppm, about 2.7 times as high as the single South Plants sample."

Response:

See text change on page 4-35.

Comment 84:

Page 4-35, third paragraph, last sentence

It would be more correct and less speculative to state that "Differences between onpost control and contaminated sites were not statistically significant."

Response:

The text sentence is correct as stated.

Comment 85:

Page 4-36, last paragraph (continued on page 4-37)

Why mention the Barr Lake bald eagles if they do not feed at RMA and do not contain tissue contamination attributable to RMA sources? This casts doubt upon many of the conclusions reached elsewhere in the report about the source of dieldrin in the tissue of mobile species found dead on RMA. It clearly is not necessarily true that dieldrin in mobile animals collected at RMA is from an RMA source. This comment also applies to the second paragraph on page 4-37.

Response:

Recent telemetry data collected by the USFWS established that the Barr Lake female bald eagle visited and was captured on RMA in December 1988 and fitted with a radio transmitter. Since that time the Barr Lake female has not been observed at RMA. However seldom the eagles from Barr Lake visit RMA, they still come into potential contact with RMA

contaminants. Any data relating to these eagles, as well as eagles roosting on RMA, are pertinent.

Comment 86:

Page 4-37 to 4-41 (Raptors)

We reiterate our criticism of the kestrel study, and of what we view as over-reliance on "samples of chance" of highly mobile, migratory species.

Response:

It is appropriate that samples found dead from unknown causes be analyzed for contaminants and incorporated into the data. Raptors are important components of regional ecosystems and cannot be collected for random tissue analysis without producing adverse impacts on their populations. Analyzing samples from individuals that have died of undetermined causes is a standard approach for obtaining information on these species. Data interpretation is addressed in Section 5.3.

Comment 87:

Page 4-37, third paragraph

We disagree with the conclusion that because arsenic does not bioaccumulate, it should have been excluded from the analysis. Arsenic contamination is possible by direct exposure, and not only via bioaccumulation in the food web.

Response:

Shell and MKE did not object to this approach during discussions of the analytical suite of contaminants at Biota Assessment Committee (currently the Biota Assessment Working Group) meetings nor during reviews of the Phase I and Phase II Biota Assessment Draft Final Technical Plan. There is not an appropriate pathway that might produce adverse effects on raptors by direct ingestion of arsenic other than through food chains.

Comment 88:

Page 4-41, second paragraph

What constitutes an "extremely" high level (line 10)? No values from offsite are reported. If it means that the value is much higher than normally thought of as lethal, then one must question either the data or the lethal dose.

Response:

The word "extremely" has been deleted. High levels are in reference to tissue concentration levels in birds documented in the literature (see Mount and Oehme, 1981: Ohlendorf et al., 1981: Heinze and Johnson, 1981). Further comparisons of tissue contaminant levels found on RMA to

levels reported in the literature are discussed in Section 5.3 of this report. All raptors with the exception of American kestrels were collected as samples of chance, and one would not expect to find samples of chance offsite. Nowhere in this paragraph is a lethal dose mentioned.

Comment 89:

Page 4-41, last paragraph

Is the Army suggesting that the dieldrin levels in the badger (liver and kidneys) indicate the cause of death? If so, what is the basis for such a suggestion? Regarding the coyote, we reiterate that the data do not support a definitive diagnosis of pesticide poisoning.

Response:

This section is a simple presentation of contaminant levels in coyote and badger tissue.

Comment 90a:

Page 4-43, first paragraph

Again, we disagree that arsenic can be eliminated as a possible cause of death. Arsenic has been detected in aquatic macrophytes and sediments: mallards may ingest both.

Response:

Examination of the scientific literature indicates that the arsenic levels found in the food web for mallards would not be expected to cause any adverse effects by direct ingestion. Further, because this compound does not bioaccumulate: it was not included in the technical plan for this species. This paragraph says nothing about determining possible cause of death.

Comment 90b:

Page 4-43, first paragraph

Why were DDT and DDE not appropriate for deer and rabbits? A reference should be provided--either to a part of the RI where it is explained, or to the scientific literature.

Response:

The discussion of these contaminants in Section 5.1 provides documentation showing that birds are far more sensitive to the effects of organochlorine pesticides than are mammals (cottontails and deer).

This was discussed numerous times by the parties at meetings of the Biota Assessment Working Group (formerly the Biota Assessment Committee).

Comment 91:

Page 4-44, second paragraph

To avoid the appearance of bias, the statistical information for mercury should be presented in the same manner as for dieldrin

Response:

See text change on page 4-44.

Comment 92:

Page 4-47

The discussion on pheasant tissue should be revised to include references to location. This is accomplished by the map, but the reader of the RI--or of newspaper articles quoting or paraphrasing the RI--will not initially understand that the contaminated samples were collected near major contaminant sources and therefore do not reflect conditions across the RMA. It is incumbent upon the writer to present data in a manner that gives the reader as full an understanding of the situation as possible.

Response:

See text change on page 4-47.

Comment 93:

Page 4-51, first paragraph

Why were DDT and DDE analyses not required?

Response:

The sampling scheme was stratified to analyze for contaminants in selected species representing major trophic levels, game species, etc. to obtain the maximum usefulness from the data in a cost-effect approach. Shell and MKE had no problem with this approach during earlier discussions and review of the technical plan for this work.

Comment 94:

Page 4-51, third paragraph

Since the report generally points out values above FDA levels--irrespective of the degree to which the contaminated tissue may contribute to human diet--it should be stated that the 0.187 ppm level of dieldrin in the one deer liver (fourth sentence) is below the FDA level.

The next sentence should be deleted. Statistical analysis is irrelevant if no analytes are present.

Response:

The FDA does not report levels for liver tissue. Statistical results may be obvious, but comparisons between controls and RMA sites are appropriate within the original sampling design.

Comment 95:

Page 4-51, last paragraph

The report incorporates pheasant tissue data provided by Shell/MKE, but not prairie dog tissue data. Why not? We assume that such data will be incorporated into the next version, because it will increase the sample size substantially.

Response:

Complete prairie dog data were not received in time for incorporation into the draft final report. The final report includes this information beginning on page 4-56.

Comment 96:

Page 4-54, first paragraph

Again, why DDT and DDE were not analyzed?

Response:

See responses to comments 97a.

Comment 97a:

Page 4-56, second paragraph

Did Rosenlund et al. (1986) look for arsenic? Of what significance is the 0.2 ppm dieldrin level mentioned in the second sentence? Why are mercury data from Rosenlund et al. (1986) not mentioned in the text? Concentrations in "individual fish" above FDA levels (third sentence) are of little relevance by themselves.

Response:

Rosenlund et al. did not look for arsenic. Only results are reported in Section 4.3. The significance of results is addressed in section 5.3. Mercury results from Rosenlund et al. are incorporated in the text on page 4-66. Concentrations in excess of FDA Action Levels are merely reported in Section 4.3. The relevant results are discussed in Section 5.3, Contamination Effects.

Comment 97b:

Page 4-56, second paragraph

In the third sentence, did Myers et al. (1983) sample for arsenic or DDT/DDE? How do the very low values reported by Myers and Gregg (1984) compare to USATHAMA Certified Reporting Limits used during RI studies?

Response:

Myers et al. methods address only aldrin, dieldrin, endrin, and mercury. Myers and Gregg (1984) did not sample biota. Their detection limits are lower than the USATHAMA CRLs used in the current studies.

Comment 97c:

Page 4-56, second paragraph

Shell/MKE prepared a report in August 1987 (and provided the report to the Army/ESE) showing the change in contaminant concentrations over time for various species and lakes. We suggest that a summary here would be useful.

Response:

See text change on page 4-59.

Comment 98:

Page 4-56, last paragraph

Change "determined" to "reported" (first line).

Response:

Text has been revised.

Comment 99:

Page 4-60, first paragraph

Change "determined" to "reported."

Response:

Text has been revised.

Comment 100:

Page 4-66, last paragraph

In the last sentence, it should be noted that mercury and dieldrin were the only analytes used for statistical analyses because they were the only analytes detected in appropriate data pairs.

Response:

Text has been revised.

Comment 101:

Page 4-67, both paragraphs

Discussions of statistical significance are of little meaning to the reader unless it is stated which lakes, species, or tissue types are higher or lower. We suggest summarizing this in the text or providing a table.

Response:

Statistical data are only presented in this section. The reader is referred to Appendix B for details of the statistical analyses.

Comment 102a:

Section 5.0, Contamination Assessment

Page 5-1, et seq.

The Contamination Assessment is not as helpful as it should be in defining the extent and degree of contamination at RMA. Such a discussion should be accompanied by a table presenting the following information: The identity and concentration (range and median values) of each of the 39 target analytes found to be above CRLs in the biota, surface water, sediments, and soils: the area of RMA over which the various contaminants are present; and natural background levels of the contaminants. The Contamination Assessment should also provide the reader with information regarding the mechanism of the release of contaminants to the environment, the environmental fate and transport of the contaminants, and sufficient information to determine the probable exposure and dose of the contaminants of concern to species of interest.

Response:

The contaminants found in biota were presented in Section 4.0 of the RI. From the 39 contaminants of concern to biota, as discussed in text Section 3.2.2.3. Seven were selected as major contaminants of concern to biota. Additonal information can be found in the Air, Soil, and Water RI documents, or the SAR reports.

Comment 102b:

Page 5-1, et seq.

It is claimed in this section that "the 39 contaminants . . . were evaluated to assess direct and indirect adverse effects on biota and to develop criteria for contaminant concentrations in abiotic media that would not be hazardous to biota." Missing from the report, however, is a methodical attempt to correlate soil or water contaminant values with reduction in population densities, biomass, or species diversity. It is generally indefensible to infer cause-effect relationships from field observations, especially where little understood chemicals or highly disturbed areas are involved, but the reader

should be assisted in understanding what species occur near what contamination. Such data were provided by Shell/MKE but generally not used in this assessment. The assessment of biologically significant contaminants presented in the report is biased toward persistent contaminants that can be found in living tissue, leaving the reader little feel for the importance of the other target contaminants either in terms of exposure, potential effects, or tolerance of low level exposure.

Response:

Much of the Shell/MKE data was not collected in contaminated areas: and thus contaminant effects cannot be addressed. It is not true that the process of contamination assessment was biased toward persistent compounds found in living tissue: DBCP was a contaminant of concern and even a major contaminant of concern, yet it is not found in tissue nor is it highly persistent in soil or surface water. Arsenic is also not found in living tissue to a great degree. However, it is not practical to analyze tissue for compounds that are rapidly metabolized and are not expected to be found in tissue. Considering that RMA has not been utilized for disposal for a number of years, it is logical to use persistent chemicals as indicators (i.e., target analytes in biotic media) of contamination.

Comment 102c:

Page 5-1, et seq.

Furthermore, the contamination assessment should address analytical methodologies and the protocols used in the collection of data for this report. This discussion should also address the adequacy or limitations of the methods to measure the contaminants of concern at proposed target levels. Finally, this discussion should advise the reader of the statistical confidence in the data.

Response:

The statistical methods and results are presented in Appendix B. The analytical methodology is referenced in the Phase I and Phase II Biota Assessment Final Technical Plan. The field methodology is documented in the Biota RI.

Comment 102d:

Page 5-1, et seq.

The toxicity assessments presented here should be described as what in fact they are tentative and, we believe, overly conservative. They are based, for the most part, on laboratory experiments that have not been validated in the field, and arbitrary uncertainty factors have been applied to create numbers

that are below scientifically supportable ranges. It therefore should be remembered that these numbers represent a concentration to which a population of plants or animals could be exposed indefinitely without harm. These numbers are made even more conservative by the fact that the higher trophic level species potentially most vulnerable to adverse effects are highly mobile, and in some cases, not present on the RMA yearround.

Response:

According to the logic described in the beginning of the comment, toxicology as a science should generally be dismissed. The Army does not agree. In the absence of rigorous field testing, laboratory data must be relied on. As to the uncertainty factors and their conservative effect on the criteria, the purpose of uncertainty factors is to reflect scientific doubt in the estimate, and the scientific performance to err on the side of caution when interperting data. It is true that these uncertainity factors have not been calibrated, but they are similar to factors currently in use at EPA. When the acceptable levels were compared to field data relating exposure to toxic effects, these levels appeared to be protective for wildlife. Some of the top carnivores do reside on RMA throughout the year, and may take the bulk of their prey items from RMA. To ignore them would be to bias the criteria towards being non-protective.

Comment 102e:

Page 5-1, et seq.

Section 5.0 could also be improved by describing the assumptions considered in developing tolerance levels for pesticides and TLVs. The FDA guidelines address residues in specific agricultural commodities, usually developed based upon assumptions regarding human consumption using a "market basket" approach. However, the Arsenal biota are not consumed by humans in lieu of agricultural commodities. Therefore, references to FDA levels are irrelevant and should be deleted from this document.

Response:

The TLVs were removed from the document since they were not utilized. The FDA action levels are mentioned as a point of fact and are not used to develop acceptable levles for biota.

Comment 102f:

Page 5-1, et seq.

TLVs have been developed for protection of workers based upon an assumption of exposure in confined environments (i.e., not outdoors) at a rate of 8 hours per day and 5 days per week over a prolonged period. Also, dusts and vapors

in an industrial scenario have been considered in the Task 35 draft PPLV methodology. It is therefore difficult to fathom the relevance of this information in the Biota RI. We also cannot help but notice the bias implicit in the fact that most of the compounds for which TLVs are listed were manufactured, or used in manufacturing operations, by Shell. TLVs are not developed for use in establishing "how clean is clean" criteria in clean-up scenarios. We believe that the incorporation of TLV values in this document is inappropriate, and that they should therefore be deleted.

Response:

All TLVs were deleted from the text.

Comment 102g:

Page 5-1, et seq.

We also disagree with the use of Sax (1984) as a primary reference. Sax is a compendium of assorted bits of information collected from references of varying quality. The use of primary references would improve the quality and validity of the Biota RI.

Response:

Many primary sources in addition to Sax are provided in the References Cited, Section 7.0 of the Biota RI.

Comment 102h:

Page 5-1, et seq.

The fundamental problem with ecotoxicological assessments is that principles developed for the protection of human health do not necessarily apply to the protection of wildlife, in which compensation for the loss of some individuals by the survival of others is apparently a reality. Threatened or endangered species are obvious exceptions to this, because each individual must be protected by law. In some cases, a population may actually flourish as a result of exposure to low levels of chemicals (Moriarty, F. 1988. Ecotoxicology. New York: Academic Press). When laboratory-tested chemicals are tested in the field, results can vary widely. Natural populations may actually be less susceptible, by a factor of up to ten, than their laboratory counterparts (Woltering, D. M., and W. E. Bishop 1989. Evaluating the Environmental Safety of Detergent Chemicals: A Case Study of Cationic Surfactants. In Paustenbach, D (ed.) 1989. The Risk Assessment of Environmental and Human Health Hazards: A Textbook of Case Studies. New York: John Wiley and Sons.)

Response:

Comment noted. Unless this factor of ten has been found to apply to a wide range of contaminant types, including metals and pesticides, it is preferable to adopt the accepted approach of relating laboratory data such as BCFs directly to natural populations.

Comment 102i:

Page 5-1, et seq.

A very positive message should emerge from this biota assessment. The Arsenal supports large numbers and great diversity of wildlife. Fish live and reproduce in the lakes, impressive numbers of raptors spend the winter, and thousands of prairie dogs and other small mammals thrive in intimate contact with the soil. We should be learning something about the tolerance of animal populations to low levels of chemical contaminants from this living laboratory, rather than overreacting on the basis of calculated "protective levels" for laboratory animals.

Response:

Comment noted. This document maintains an objective and scientific presentation throughout. The remainder of the comment is not relevant to the purpose of the RI.

Comment 102j:

Page 5-1, et seq.

Despite the considerable and generally competent effort embodied in this section, it fails to provide the RI/FS process with the necessary assessment of hazards posed by RMA to the environment. This is due in large part to the fact that there currently exists no widely accepted methodology for integrated environmental risk assessment. Because the RMA is an enormously complex site, information on exposure, toxicity, and observed effects--which would be most useful if presented together--are presented in several different documents. The Biota RI is but one of these documents. The Endangerment Assessment (EA) is expected to pull together the parts described above. Nonetheless, much more context should be incorporated into this document so that the EA synthesis can be accomplished more accurately and efficiently. The Executive Summary and the Introduction promise integration of the nature and extent of RMA contamination and its effects on biota: this does not occur.

Response:

Historical information on RMA contamination (Section 4.1), the results of recent studies (Section 4.3), and the calculation of acceptable levels of contamination (Sections 5.1 and 5.2) are integrated in Section 5.3, Contamination Effects.

Comment 102k:

Page 5-1, et seq.

Since the criteria developed in the Contamination Assessment are inadequately supported, we cannot agree that they should be considered as acceptable cleanup criteria. Instead, we feel that the criteria should be re-evaluated with consideration given to the following issues:

(1) Additional scientific evidence and validated methodologies currently available;

(2) Potential impact of cleanup activities on existing biota, i.e., the trade-off between lower levels of contamination and greater extent of habitat destruction;

(3) The possible double standard represented by the purposeful use of pesticides for habitat management versus zero tolerance for pesticides at other times and places; and

(4) The reported tendency for plants and animals in natural populations to be less sensitive to low levels of contaminants than predicted by laboratory studies.

Response:

Comment noted. The Army does not agree that the criteria (acceptable levels) are inadequately supported. The purpose of the RI was not simply to compile monographs on each contaminant, but to obtain representative values and frequently observed health effects. Cleanup criteria are not developed in this document, which provides levels pertinent to biota that the EA can use in concert with other considerations.

Comment 103a:

Page 5-5, et seq. (Section 5.1, Toxicity Assessments)

Lead and benzene should have been included in this section, based on present distribution and concentration at RMA.

Response:

Lead and benzene did not fit the selection criteria used in the technical plan.

Comment 103b:

Page 5-5, et seq. (Section 5.1, Toxicity Assessments)

It should be emphasized that the methodology described in this section, while similar to human safety factor risk analysis and to some other ecological risk methods (see Review and Evaluation of Ecological Risk Assessment Methods, ICF for U.S. EPA, OPPE, February 1988, Draft) has been developed by the authors. No peer-reviewed, widely accepted, integrated ecological risk assessment methodology exists, even though ecological risk assessment is routinely performed for pesticide registration according to the EPA Standard Evaluation Procedure (June 1986).

Response:

Comment noted. Ecological risk assessment methodology in the form of an EPA document does exist, although it postdates the efforts at RMA. The methodology used in the Biota RI is not inconsistent with the EPA draft document.

Comment 103c:

Page 5-5, et seq. (Section 5.1, Toxicity Assessments)

We are puzzled that more of an attempt is not made to quantify potential soil ingestion problems for the 32 "other contaminants." Soil ingestion rates for small mammals are given on page 5-112 with a recommended estimate of 0.873 g/kg bw/day. We would like to see calculations of soil contaminant levels and ranges that would provide doses to small mammals equivalent to the NOEL x UF used to determine drinking water levels.

Response:

Comment noted.

Comment 103c:

Page 5-5, et seq. (Section 5.1, Toxicity Assessments)

Background levels in soil, water, air, and biota exist for most metals and many of the organics considered here. These background levels should be included in the report. For example, see Safe Drinking Water Committee (1980, Drinking Water and Health, Vol. 3, Nat'l Acad. Sci., Washington, D.C.), Ward et al. (1985, Groundwater Quality, John Wiley and Sons, NY), and Shah and Singh (1988, Distribution of volatile organic chemicals in outdoor and indoor air. Envir. Sci. Technol. 22(12):1381-1388).

Response:

As suggested, background levels in soil and water were included for arsenic, mercury, cadmium, and copper. Background levels for contaminants not naturally found in the environment will vary widely

depending on the type of anthropogenic activities near the site. Background levels for pesticides in biota are readily available and can be provided, but such levels are not as appropriate for comparison to RMA as are the offpost control samples.

Comment 104:

Page 5-10, Table 5.1-2

Change "Uncertainity" to "Uncertainty" in the fourth column.

Response:

See changes on Table 5.1-2.

Comment 105:

Page 5-11, last paragraph

A cancer bioassay in mice and rats has been completed on allyl chloride (NCI-CG-TR-73.78). We therefore would expect that a chronic LOAEL or NOAEL for mammals is available from that document, and that subchronic information is available from a study of maximum tolerated dose ranges.

Response: Cancer is not an appropriate type of injury for evaluating risk to wildlife populations.

Comment 106:

Page 5-17, last paragraph

The statement that "Plant growth was reduced by 40% in soils . . ." conflicts with the statement on page 5-15 (third paragraph) that "Crop growth in these soils was 40 percent [of] that observed in control fields," which implies a reduction of 60 percent. Also, change "uncertainity" to "uncertainty."

Response:

See text change on page 5-17.

Comment 107:

Page 5-18, third paragraph

Azodrin is an insecticide, so information on toxicity to terrestrial invertebrates is available. For example, see the pesticide label, the pesticide registration filed with EPA, Verschueren's Handbook of Environmental Data on Organic Compounds (2nd Ed.) and Buchel's Chemistry of Pesticides, among others.

Response:

The label, the Pesticide Fact Sheet, and Verschueren's Handbook of Environmental Data were examined. Toxicity data such as application rates from the label are not indicative of the toxicity of soil residues for compounds that are not highly persistent in soils. Therefore, the information suggested by Shell/MKE is inappropriate for this report. Information on toxicity of azodrin to invertebrates is still lacking, except that 0.35 ug/bee is toxic to honeybees (Pesticide Fact Sheet). Without the amount of soil, if any, ingested by bees, it is not possible to relate direct toxicity of azodrin to bees to soil residues.

Comment 108:

Page 5-20, third paragraph

Recalling the 30-day study in mallards (Hudson 1984, cited in the document), 0.25 mg/kg/day was a LOEL; if ducks indeed drink 0.2 L/kg/day, then a surface water concentration of 0.014 mg/L may be calculated, to which uncertainty factors must be applied. Since we would expect to see effects in mallards at this concentration within a month's exposure, a factor of 5, such as is proposed here to treat the dog NOEL (similar at 0.018 mg/L), is probably inadequate. Based on the duck study, we might see toxic effects in mallards at the proposed surface water concentration of 0.0035 mg/L following long-term exposure. The cookbook subchronic LOEL UF of 250 is excessive: an additional UF of 5 for interspecific variation would be adequate.

Response:

The mallard LOEL is considered to be less certain than the dog NOEL because only 12 birds were used and Hudson refers to the LOAEL as "about" 0.25 mg/kg bw/day. In addition, the birds were dosed by capsule, which may not be as appropriate as using dietary studies. Uncertainty increases because the LOAEL must be scaled down to a NOEL before applying an interspecific uncertainty factor. The suggested criterion level is appropriately stringent until further studies on the toxicity of azodrin to birds are obtained.

Comment 109:

Page 5-20, fourth paragraph

Bioaccumulation information for snails and fish is available and would be a useful addition to this report. A listing of primary references for these data may be found in Karl Verschueren's "Handbook of Environmental Data on Organic Chemicals."

Response:

The Army checked Karl Verschueren s book, but this information was not in the book. The information was also not available from the Registration Division, EPA, Washington, D.C. (Marilyn Marks, personal communication, 3/30/89), and was not included in the Pesticide Fact Sheet or the pesticide label.

Comment 110:

Page 5-29, et seq.

The data presented here and in the following pages to support cadmium toxicity are very thin, considering the large volume of literature that exists. Considerable additional information is available in the literature regarding cadmium uptake by plants. Published studies indicate that the uptake of cadmium by plants widely varies, both with the type of plant, and with the soil. See "Survey of Cadmium in Food: First Supplementary Report," Ministry of Agriculture, Fisheries and Food, Her Majesty's Stationery Office, London, England, 1983.

Response:

Comment noted.

Comment 111:

Page 5-23, second and third paragraphs

The chosen NOEL from the mallard study is either a LOEL, due to the smaller testes reported, or if in fact this result was not statistically or biologically significant (which cannot be determined from the data reported), then 1.4 mg/kg should be chosen as the NOEL since 20 mg/kg is a LOAEL. If so, the Surface Water Ingestion criterion would be 140 ppb.

Response:

The chosen NOEL was the control, and is not a LOAEL: reduced testis size was observed for the treated versus the control group. The reviewer misinterpreted the paragraph.

Comment 112:

Page 5-25, second and third paragraphs

We do not believe that a cadmium soil level that is lower than the average natural background is reasonably achievable, or necessary to achieve. The 13 ppm figure was calculated using two overly conservative sources: (1) shrews accumulate more cadmium than other small mammals, and (2) earthworms accumulate more cadmium than other invertebrates, and when analyzed, much soil remains in the gut of the worm.

Response:

Comment noted. The cadmium soil criteria (13 ppm) is higher than the recommended indicator levels (>D.L.-2 ppm). The acceptable level for soil, therefore, does not seem overly conservative.

Comment 113:

Page 5-33, second paragraph

A reference citation for the chlorobenzene dog study should be provided.

Response:

References for the values used in the calculation section are provided in the text.

Comment 114:

Page 5-35, fourth paragraph

"[B]y lowering the threshold for necrotic action" is not a mechanistic explanation for the effects of pretreatment with ethanol or DDT. We suggest deleting the phrase.

Response:

Comment noted. Phrase removed on page 5-35.

Comment 115a:

Page 5-37, et seq.

References for the half-life and solubilities of CPMS, CPMSO, and CPMSO2 should be provided. A half-life of less than 6 months does not agree with a 1981 Guenzi and Beard study which reported 1.1 to 1.5 years.

Response:

Comment noted. See text change on page 5-37.

Comment 115b:

Page 5-37, et seq.

In Task 35, the solubility of CPMS is given as 12 mg/L, not 12 ug/L; values for CPMSO and CPMSO2 are also different. Reference to the various values should be provided here.

Response:

Comment noted. See text change on page 5-37.

Comment 115c:

Page 5-37, et seq.

The studies by Menn et al. (as referenced in Shell's risk assessment for CPMS and related compounds) should be included in the evaluation of toxicological studies.

Response:

Comment noted.

Comment 116:

Page 5-38, first paragraph

The reference for the growth reduction information should also be incorporated in this document.

Response:

See text change on page 5-38.

Comment 117:

Page 5-40, et seq.

The natural background concentrations of copper in soil and water in the U.S. should be provided so that the reader will have an understanding that soils and water are not naturally free of copper.

Response:

See text change on page 5-40.

Comment 118:

Page 5-45, second paragraph

The soil criterion is based on toxicity to earthworms. The significance of laboratory-derived earthworm toxicity values to field situations has not been determined by experts. We therefore disagree with this suggested value (100 ppm).

Response:

It is a commonly accepted practice that laboratory values be extrapolated to field or natural conditions, and that the two types of data be relied on to strengthen one another. Sometimes laboratory studies provide better data considering the variability and irreproduceability observed in many field studies. When field studies were found in the literature reviewed, the data were presented.

Comment 119:

Page 5-58

Concerning DMMP, the Army should refer to its 1987 toxicity assessment profiles for additional information regarding aquatic and terrestrial ecosystems.

Response:

The toxicity profiles were reviewed as part of the literature search and the studies most applicable to wildlife were detailed from this source. Toxicity to hens, spider mites, rats, and mice is mentioned in the RI. The mutagenic and carcinogenic studies were deemed inappropriate for purposes of the Biota RI. The reference cited is the same as in the toxicity profiles for fish, and appears to be the only reference for toxic effects on aquatic life.

Comment 120a:

Pages 5-62, last paragraph

Physical properties for ethylbenzene should be provided to make this section consistent with the information provided for other compounds. This information may be found in the Army's proposed final "Chemical Index for RMA."

Response:

BCF and half-life data were already provided: solubility data were added on text page 5-62.

Comment 120b:

Pages 5-62, last paragraph

The ambient air concentration range of ethyl benzene should be provided as a frame of reference to the reader. See Shah and Singh, 1988. Distribution of Volatile Organic Chemicals in Outdoor and Indoor Air, Environ. Sci. Technol. 22(12):1381-1388.

Response:

The air pathway does not appear to represent a significant threat to wildlife at RMA (see Section 5.0).

Comment 121:

Page 5-84, second paragraph

Mammalian toxicity data better than an acute oral LD50 must surely be available for DMNA. Numerous references are listed in RTECS.

Response:

Comment noted. The bulk of the references obtained from DMNA emphasized mutagenic and carcinogenic effects, which are not appropriate for the purpose of examining risk to wildlife populations. Chronic wildlife data have not been found in the course of our DMNA literature review.

Comment 122:

Page 5-94, last paragraph

We agree with the reasoning behind not using uncertainty factors for PCBs, considering the nature of the NOEL.

Response:

Comment noted.

Comment 123:

Page 5-98, fourth paragraph

Following the chart comparing numbers for toluene, the number "175 ppb" should be changed to read "127 ppb."

Response:

See text change on page 5-97.

Comment 124:

Page 5-106 et seq. (Section 5.2, Pathways Analyses)

For each of the major contaminants of concern, both an aquatic and a terrestrial pathway analysis is performed. However, the resulting site-specific criteria for water and sediment are unrealistically low because of the assumption that the aquatic food chain supplies 100 percent of a bald eagle's diet. In fact, observations of eagles feeding on fish are limited to one out of some 33 observations, and collection of remains of fish from 211 castings occurred just once. This indicates that fish consumption by eagles at the Arsenal is rare--not surprisingly since the lakes are frozen most of the period when the eagles are present. Although feathers were found in eagle castings, there was no evidence that they were from waterfowl (Bald Eagle Study, Final Report, September 1988, Task 9). Moreover, pheasants are much more abundant on RMA than are waterfowl, at least during the winter when the lakes are frozen and the eagles are present. Thus, fish/duck consumption is not a defensible basis on which to establish cleanup criteria for water or sediment. A careful source contribution analysis should be performed to partition contaminant sources between the aquatic and terrestrial pathways for contaminants that are biomagnified.

Response:

The acceptable levels for water and sediment consider not only the bald eagle pathway but toxicity to lower members of the food chain such as waterfowl and aquatic organisms. It is inappropriate to base sediment and water criteria on pheasants because they have no strong link with an aquatic system. In warm years the lakes may not be frozen completely, and therefore provide a potential source of food for eagles. In any case, the approach represents a conservatism appropriate to an endangered species.

Comment 125:

Page 5-107

Table 5.2-1 could be improved by adding naturally-occurring or typical agricultural levels of these compounds.

Response:

Agricultural levels would not provide information relevant to contamination at RMA beyond that provided by offpost controls.

Comment 126:

Page 5-113, second paragraph

The EPA criterion for the protection of aquatic life of 0.0019 ug/L dieldrin is based upon unidentified studies. Numerous credible publications are readily available on this subject: these should be reviewed and cited.

Response:

Numerous credible publications on toxicity of dieldrin to aquatic life were reviewed and cited. The EPA criterion is documented in the Ambient Water Quality Criteria Document for Aldrin and Dieldrin.

Comment 127:

Page 5-119, third paragraph

Matsumura (1980) did not conclude that the major end products of microbial metabolism are ketones. This statement should be deleted.

Response:

Matsumura, 1980, p. 333 states "For both dieldrin and endrin, the major reaction product is ketones, which are formed as a result of isomerization of the epoxy ring." The title of the section is "Metabolism of Chlorinated Hydrocarbon Insecticides by Microorganisms", and the paragraph that follows the quoted statement details metabolic activities of microorganisms.

Comment 128:

Page 5-164, first paragraph

An explanation should be provided as to how the Army reached the conclusion that biomagnification of dieldrin is a "problem" at RMA. The Army should first define what constitutes a "problem. "One suggestion is to define it in terms of unacceptable risk following the format of Table 1 from "Hazard Evaluation Division, Standard Evaluation Procedure: Ecological Risk Assessment," U.S. Environmental Protection Agency (June 1986), EPA 540/9-86/167.

Response:

Biomagnification of dieldrin is well known and has been substantiated as a phenomenon occurring in the lakes at RMA by Rosenlund et al. (1986). It is a problem because dieldrin is both toxic and persistent in the environment and bioaccumulates through food chains from low levels in the abiotic to unacceptable levels at higher trophic levels in food webs.

Comment 129:

Page 5-189, first paragraph

We believe that it is premature to draw conclusions about "margins of safety" (line 10) based on anecdotal information for small groups of animals, especially when it cuts across classes (i.e., birds and mammals). Also, the "2-10 ppm" figure quoted from Buck (1978) refers to arsenic in the urine of domestic mammals, not the liver and kidney of unknown species. While the experience of clinicians is extremely valuable, Buck and others are not attempting to present data which would indicate the minimum tissue levels that must be achieved for poisoning to occur at the time that it occurs (as would be developed in a toxicity test), but rather tissue levels that will be apparent to the diagnostician.

Response:

Comment noted. Data relating toxicity to tissue residues are difficult to obtain for arsenic, thus the reliance on Bucks' values. Buck, 1978. pg. 372 states: "Animals dying of acute arsenic poisoning may contain from 2 to 100 ppm arsenic on a wet-weight basis in the liver and kidneys, whereas animals not known to have been exposed to arsenic usually contain less than 0.5 ppm. Levels above 10 ppm in the liver and kidney should be considered confirmatory of arsenic poisoning." Since arsenic is metabolized by animals, the residue levels are not as critical in tissue as are the ambient environmental levels to which plants and animals are exposed. The uncertainty with tissue concentration measurements is such that exposure only roughly

correlates with residues. Because of this uncertainty, and because arsenic is highly toxic to plants, criteria for arsenic were not based on the pathway approach, although the analysis is presented in the RI.

Comment 130:
Page 5-197, third paragraph
As a note on style, throughout much of Section 5, the words "however" and "therefore" are incorrectly punctuated. When these words are used to connect clauses of a compound sentence, they should be preceded by a semicolon and followed by a comma. For example, in the second sentence, "Arsenic is more toxic to plants than animals, therefore soil and water criteria should be targeted . . ." should read "Arsenic is more toxic to plants than animals; therefore, soil and water criteria should be targeted . . . ." See Section 5.69 of The Chicago Manual of Style, or Chapter 3 of the Council of Biology Editors Style Manual.

Response:
We agree. See text changes throughout Section 5.

Comment 131a:
Page 5-310, third and fourth paragraphs
The uncertainty analysis methodology should be provided as a "stand alone" document since the Offpost Endangerment Assessment is not yet available.

Response:
The Offpost Endangerment Assessment is now available.

Comment 131b:
Page 5-310, third and fourth paragraphs
Since prairie dogs and lagomorphs are known to be by far the major prey of eagles at RMA, the assumption that the diet of eagles is only mallards and pike is invalid. In our opinion, this assumption is inaccurate rather than conservative.

Response:
The model presented is a simplification with species used to represent trophic categories. At other locations, the aquatic food web forms a large part of the bald eagles diet, and it is possible that this could occur at RMA. MKE biologists have reported bald eagles feeding on fish in the Lower Lakes at RMA.

Comment 132:

Page 5-314, second paragraph

The first sentence states that in order to establish a relationship between a contaminant and an observed effect, one of the criteria is that "the observed effect must be demonstrably related to the particular contaminants(s) being evaluated." It does not appear to us that this approach has been faithfully followed because many of the conclusions about contaminant effects are speculative.

Response:

The Army disagrees that many of the conclusions about contaminant effects are speculative. Specific responses to specific comments are provided below.

Comment 133:

Page 5-315, second paragraph

The sentence beginning with "Species richness in . . ." is misleading. The word "slightly" should be omitted, and "sample plots" should be changed to "study sites" since all sample plots at all study areas were of equal size: only study sites (i.e., vegetation types) were of different areal extent among study areas. A better wording would be "Species richness in native grassland at RMA was higher than at either of the offpost sites. The greater number of species recorded at RMA probably relates to the greater areal extent of native grasslands onsite, and the greater number of samples taken."

Response:

See text change on page 5-309.

Comment 134:

Page 5-315, third paragraph

In line 3, change "difference" to "different."

Response:

See text change on page 5-309.

Comment 135:

Page 5-315, last paragraph

See earlier comments concerning the sunflower study.

Response:

See earlier responses to the comments concerning the sunflower study.

Comment 136:
Page 5-316, fifth paragraph, line 2
Change "small" to "snail."

Response:
See text change on page 5-310.

Comment 137:
Page 5-317, last paragraph
Whether 0.108 ppm truly exceeds 0.1 ppm in an ecological (or statistical) sense is doubtful. Furthermore, the statement is incorrect without knowing more about average concentrations in the total diet of birds, the longevity and seasonal occurrence of the bird, etc. It therefore should be deleted or revised.

Response:
Grasshopper samples were composites of 50 or more individuals per sample, thus the value represents a composite value (average) for the sample location. See text change on page 5-316.

Comment 138:
Page 5-318, last paragraph
The last line of page 5-318 is repeated as the first line on page 5-319.

Response:
See text change on page 5-312.

Comment 139:
Page 5-319, second paragraph, line 7
Reduced diversity in vegetation can be caused by a variety of factors other than contaminant levels, such as soil type, competition, etc. Again, it may be an improper inference that low diversity on the basin floors is related to contamination.

Response:
The comment statement regarding the causes of reduced diversity in vegetation is correct. However the text relates reduced diversity in grasshoppers to reduced vegetation diversity and makes no inference regarding the causes of reduced vegetation diversity.

Comment 140:
Page 5-319, third paragraph, line 1

Change "conducted" to "determined."

Response:
See text change on page 5-313.

Comment 141:
Page 5-320, et seq.
More discussion should be provided regarding AChE inhibition. AChE inhibition does not occur with arsenic, mercury, or organochlorine pesticides. Moreover, there would be no basis for determining from the data alone whether a low value in an animal should be interpreted as evidence of poisoning of that animal. Analysis of the carcass for the causative agent would not necessarily be effective, because the enzyme inhibitor is destroyed in the process of reacting with and inhibiting the enzyme. For some of the best-studied compounds on RMA, the survival time of the compound in tissue is far too short for chemical analysis of the carcass to be helpful.

Response:
Comment noted.

Comment 142:
Page 5-320, fourth paragraph, lines 1-3
What are the units of the mean 14.84, etc.? Percent?

Response:
Units are umoles/minute/gram.

Comment 143:
Page 5-321, fourth paragraph
The citation for Robinson et al. (1980) is not provided in Section 7.0 (References Cited).

Response:
See text change in Section 7.0.

Comment 144:
Pages 5-320, last paragraph
The onpost and offpost information on AChE levels in birds is meaningless, because there is no means of determining the specific material causing the AChE variability.

Response:

The results are not meaningless regardless of the difficulty in attributing the cause to a particular chemical or group of chemicals. Comparisons of experimental and control areas are accepted methodology for AChE testing.

Comment 145:

Page 5-323, Table 5.3-1

What is the area of the plots?

Response:

One hectare. See text change on page 5-316.

Comment 146a:

Page 5-330, last paragraph

This constitutes a misrepresentation resulting from lack of perspective. Overall, prairie dogs from the pooled contaminated/uncontaminated plots on RMA did have significantly higher dieldrin levels than offpost. To be accurate, however, one must look at the total picture (see Figure 4.3-12, page 4-55). The "onpost controls" were almost lacking in dieldrin. Are these levels significantly different from offpost? It is not sufficient to tell part of the story: accuracy entails thoroughness as well as correctness.

Response:

This comment is answered in the original text on page 4-54. Dieldrin was higher in control samples collected onpost than in samples collected offpost. However, this difference was not significant. A sentence reiterating this fact has been added to help clarify the results.

Comment 146b:

Page 5-330, last paragraph

The last two sentences in this paragraph (top of page 5-331) are speculative and should be deleted.

Response:

The bioaccumulation of dieldrin mentioned in the first sentence is a well established phenomenon and is supported by data collected from RMA and cannot be considered speculative. The second sentence simply refers the reader to further discussions of the role of prairie dogs in contaminant pathways.

Comment 147:

Page 5-331, fourth paragraph, last sentence

Is the relationship between the amount of DDE and reproductive success linear, geometric, etc.? Unless the relationship is known, it cannot be assumed that a concentration of 6.93 ppm DDE in eagle eggs would necessarily reduce reproductive success.

Response:

The text statements relates DDE level of the Barr Lake bald eagle egg to ranges reported in a published scientific paper on DDE in bald eagle eggs.

Comment 148:

Page 5-335, last paragraph

This statement is an exaggeration of the data. See earlier comments concerning contaminants in raptors.

Response:

The text objectively presents the results obtained in relation to literature values.

Comment 149:

Page 5-339, first paragraph, last sentence

We question the conclusion that emaciated raptors with empty stomachs and crops were necessarily suffering from dieldrin contamination. This condition could have resulted from any number of causes, as we have noted previously.

Response:

These conclusions were based on sound evidence and scientific literature. Lethal dieldrin levels in brain tissue have been reported to range between 4 and 20 ppm. (see text). Dieldrin levels in brain tissue in four raptors from RMA documented to be in an emaciated condition were 0.678, 9.98, 9.44, and 9.32 ppm. (Table 5.3-5). The latter three of these raptors contain dieldrin levels well within the reported lethal ranges. Heinze and Johnson (1981) reported that about 1 ppm. or more of dieldrin in the brains of brown headed cowbirds caused some birds to cease feeding and to begin to mobilize dieldrin, resulting in death. When the fat of birds becomes depleted, organochlorine residues are mobilized, resulting in their redistribution and increased concentrations in other tissues. Brain residues increase under such conditions (Wiemeyer and Cromartie, 1981).

Other contaminants found in raptor tissue apparently did not result in death. None of the four emaciated raptors contained mercury levels above detection limits and DDE was detected in only one of these raptors at a level of 0.475 ppm, well below the reported lethal level of 250 ppm in the brain of osprey (Wiemeyer and Cromartie, 1981). In fact, none of the 14 raptor samples contained DDE levels in brain tissue above 10.3 ppm, again well below the 250 ppm lethal level. Additionally, none of the 14 raptor samples contained mercury in brain tissue above concentrations of 0.257 ppm., well below the 10 ppm brain concentration reported as diagnostic for poisoning in birds (Braune, 1987), or the lethal brain levels of 30 to 40 ppm. reported by Borg (1970 for goshawk.

The relationship between high levels and the appropriate symptoms is justification for the statement presented.

Comment 150:

Page 5-339, second paragraph

Why are the high levels of DDE in two of the dead owls not discussed? Mention is only made of dieldrin as the alleged cause of death.

Response:

Brain tissue of three great horned owls collected on RMA contained detectable concentrations of DDE at levels of 10.3, 0.475, and 2.24 ppm (Table 5.3-5). These levels are all well below the lower lethal levels of DDE (250 ppm) and DDT (86 ppm) in the brain of osprey, and the hazardous levels for osprey of 200 and 69 ppm for DDE and DDT respectively (Wiemeyer and Cromartie 1981). DDE brain residues in two American kestrels following dietary intake of 28 ppm DDE for 14-16 months were 213 and 301 ppm. (Porter and Wiemeyer, 1972. DDE at low dietary levels kills captive American Kestrels. Bull. Environ. Contam. Toxicol. 8:193-199.).

Comment 151a:

Page 5-340, second paragraph

The first sentence, concerning kestrel reproduction, is unfounded and contradicts the data. The second sentence needs to be supported by tabular data.

Response:

The first sentence is unclear but not incorrect and has been changed in the text. Tabular data to support the statement are not presently available. The second sentence has, therefore, been deleted from this document.

Comment 151h:

Page 5-340, second paragraph

Is the feeding range of Canada geese, American coots, and mallards exclusively RMA? If these species also feed at offpost areas such as Barr Lake, how can the source of contamination be assumed to be exclusively the RMA?

Response:

During the breeding season these species on RMA can be reasonably be assumed to feed predominantly, if not exclusively, at RMA. Contaminant studies of Barr Lake do not show concentrations of contaminants of concern corresponding to the levels observed in aquatic ecosystems at RMA. We know of no other known potential sources of this contamination in the vicinity of RMA that could provide a pathway of exposure to these bird species.

Comment 152:

Page 5-340, third paragraph

Why was sampling only for organochlorines? Moreover, because the 1986 data actually show no relationship between dieldrin and nesting success, it would appear that the study should have evaluated something else as well. Habitat quality (prey base, vegetation structure in the habitats available, etc.) would be a good place to start.

Response:

Sampling for organochlorine pesticides was presumably conducted because of the concern over known organochlorine pesticide contamination on RMA and its potential adverse effects on bird species: particularly those at higher trophic levels. The Army disagrees with the statement that the 1986 data "show no relationship between dieldrin and nesting success". The study showed general and consistent relationships between sites of dieldrin contamination and diminished reproductive success.

Comment 153:

Page 5-341, first two paragraphs

This discussion concerning kestrel reproduction further underscores that the report misinterprets the data, as we have commented previously.

As an additional comment, note that kestrel productivity offsite (2.78) was also lower than the number reported as necessary to maintain a population (2.88). This indicates two things:

(a) The number 2.88 must have been derived from some generalized survivorship curve that does not consider differential recruitment from other areas or differential survival of young after fledgling; and

(b) It is irrelevant in the context used.

Furthermore, are the onsite-offsite differences statistically significant? There is no discussion that the study area populations have been declining in number through the years.

Response:

We disagree with your comment that this report misinterprets the data. The values 2.78 and 2.88 are both estimates that have some associated degree of uncertainty. Nesting success would be expected to vary somewhat between years in response to a variety of environmental conditions, hence the need for controls. The mean estimate of 2.88 for nest success does not mean that this number must be exceeded every year to ensure species survival. Several interpretations are possible. This comment on how this estimate "must have been derived" indicates unsupported speculation: several possible interpretations are possible. Statistical differences are presented in Table B.2-13 of Appendix B.

Comment 154:

Page 5-342, Table 5.3-6

We would suggest adding a column or employing some type of superscript to indicate which (if any) of the observed differences are statistically significant.

Response:

A table showing the statistical data has been provided (Table B.2-13 of Appendix B).

Comment 155:

Page 5-344, first paragraph

See previous comments concerning the kestrel study.

Response:

See previous responses to comments concerning the kestrel study.

Comment 156:

Page 5-344, second paragraph

This means either that the site is cleaning itself up, or that the results of the studies have been misinterpreted. We suggest the latter.

Response:

This comment is simplistic and is inconsistent with the earlier comments C-153 and C-154 regarding the need for statistical comparisons. See text change on page 5-326.

Comment 157:

Page 5-344, third paragraph

What does the phrase "significant contaminant" mean? Statistically significant? Biologically significant? On what basis? Of what value is it to compare pheasants, mallards, and kestrels?

Response:

Significant, in this instance, means biologically important. The text has been modified to remove ambiguity. It is considered biologically important because elevated concentrations in tissue can produce adverse biological effects. Mallards are not mentioned in this paragraph. Data on pheasants and kestrels are simply presented, not discussed or compared.

Comment 158:

Page 5-344, fourth paragraph

(a) Line 2--Range of broods/run should be 0-1.2. Why is range given here when mean is given for the control?

(b) Line 4--It should be specified that number of hens and clutch sizes are "per run."

(c) Lines 6 and 7--Are the differences statistically significant? The values for numbers of young in the text and the tables are different.

Response:

Line 2--Range should be 0-1.5. See text change on page 5-326. Range here is for both RMA and control routes as stated in the text. Line 4 comment is correct. See text change on page 5-326. Lines 6 and 7 are correct as presented; the numbers are total hens observed (not hens per run as in table), and average number of young per hen observed (not young per run as in table).

Comment 159:

Page 5-348, Bullet 1, Waterfowl Counts

We do not understand the purpose of the first sentence and Table 5.3-11, since there is no further discussion on this subject. What does the statement here about absence of mallard broods have to do with Table 5.3-11 and "waterfowl counts?"

The comparisons of waterfowl numbers in the text and Table 5.3-11 are of little value, because there is no discussion about surface area, length of shoreline, adjacent habitat, availability of prey, or other factors that could affect waterfowl densities and total population sizes.

Response:

Waterfowl numbers were provided to characterize each of the areas sampled. The absence of mallard broods at RMA was the only difference indicated. No further discussion of the topic is necessary. Additional data on habitat, area, etc. would be interesting, but are not necessary to fulfill the purposes of the RI.

Comment 160:

Page 5-348, first full paragraph, second sentence

What is the basis for the statement that "some local areas still remain too contaminated for kestrel survival and reproduction?" The tables do not indicate kestrel mortality or lack of reproduction. The sentence appears to be a gross overstatement. Furthermore, the "some local areas [that] remain too contaminated" happen to be along First Creek, which essentially is not contaminated.

Why were the results of the DDT/DDE analysis not included in Table 5.3-7, 5.3-8, 5.3-9, etc.?

Response:

Analytical results have been added. DDT/DDE levels were low and were not considered relevant. DDT/DDE values are presented in Table B.2-13 of Appendix B.

Comment 161:

Page 5-348, second full paragraph, first sentence

Please specify which other waterfowl and wading bird species are "adversely affected." See earlier comments concerning mallard reproduction.

Response:

This statement is not based on current data presented and has been removed. See text change on page 5-334.

Comment 162:

Page 5-348, third full paragraph, second sentence

This states that "habitat differences and total populations densities" may account for some of the differences observed for pheasants. The same is probably true for some of the differences observed in kestrels and mallards. Why was this not discussed?

The last sentence in this paragraph does not make sense. Perhaps two partial sentences were combined.

Response:

Habitats for kestrels and mallards were generally similar while offpost pheasant areas contained substantial areas of agricultural land not found on RMA. The last sentence in the paragraph has been corrected. See text change on page 5-328.

Comment 163:

Page 5-348, fourth full paragraph

This type of explanation reflects sound reasoning and represents what we mean by "perspective." See General Comment No. III.

Response:

Comment noted.

Comment 164:

Page 5-350, first paragraph

Again, see our earlier comments on the kestrel and mallard studies.

Response:

See earlier responses to comments on kestrel and mallard studies.

Comment 165:
Page 5-350, first paragraph, line 4

What is meant by "upward trends?" Reproductive success apparently went up in some areas (central part of RMA) and down in others (First Creek). See previous comments on the kestrel study.

Response:

The overall trend for reproductive success on RMA appears to be upward. Insufficient information is available on the home ranges of kestrels to support the comment on trends in different parts of RMA.

Comment 166:
Page 5-350, last paragraph

The last sentence is only partly correct in saying that the Shell/MKE nighttime lagomorph surveys "did not separate counts taken near sites of contamination from those taken in uncontaminated areas." Actually, we provided maps to the Army/ESE showing the number of sightings for each species along the road transect route. We also provided a map showing the distribution and number of lagomorph fecal pellets in 102 songbird breeding plots distributed across the Arsenal.

These findings indicate that intensity of use by rabbits was not well correlated with distance from major contamination source.

Response:

Comment noted. We have seen no treatment of the data that indicate that "intensity of use by rabbits was not well correlated with distance from major contaminant source".

Comment 167:
Page 5-351, first paragraph

The first sentence may or may not be true, depending upon the degree to which predators feed upon contaminated rabbits in comparison to other prey, over how much of the year, and over how many years. This statement is speculative and should be deleted.

Response:

The phenomenon of bioaccumulation of organochlorine pesticides is well established in the scientific literature and is supported by data from

RMA. Information on the percent of lagomorph and rodent prey in the diet of many species of predators at RMA is generally established. The rates of bioaccumulation supporting this statement are discussed in section 5.2. The statement represents a scientific evaluation based on established mechanisms and conditions and is therefore not speculative.

Comment 168:

Page 5-351, second paragraph

What is the basis of the assumption that the bulk of the contaminants are obtained through food chain sources? Both species are fossorial (i.e., they burrow) and therefore come in direct contact with soil. Is there a reference for this?

Response:

Chemical analysis conducted as part of the biota assessment and in previous studies at RMA have established the mechanism of bioaccumulation of organochlorine pesticide contaminants and their biomagnification in food webs. Other exposure pathways have been addressed in section 5.2.

Comment 169:

Page 5-351, third paragraph

Please specify the brain/carcass ratio observed by Walker (1969). Note that the coyote was near the lower end of the range reported as lethal to dogs. Did Harrison et al. (1963) actually state that the reported range was "diagnostic" of death, or merely sufficient to cause death. The fact that they reported a nearly four-fold range underscores inherent variability which makes the coyote level nondiagnostic.

Response:

Harrison presented data that showed that six dogs with brain dieldrin concentrations of 2.4 to 9.4 ppm. died. One dog with brain levels of 3.8 ppm survived the test, but evidenced symptoms of poisoning following treatment. Although the number of dogs in the study was small and the dosing regimens varied, we believe the paper provides us with a range that could be considered lethal. In Walker's study, dog brain levels were never higher than the 0.056 ppm, and the dogs were symptom free. The wording has been changed from "diagnostic" to "strongly correlated".

Comment 170:

Page 5-352, second paragraph

We question whether it is appropriate to compare waterfowl with raptors (reference on hawks cited).

Response:

The statement is presented so that the reader can compare lethal levels observed in birds to the levels observed at RMA. Evidence for toxicity from tissue residues indicates that levels can vary by several ppm between species. Considering biological variability as well as variability from different studies, the range does not appear overly large.

Comment 171a:

Page 5-352, fourth paragraph

In line 5, it is stated that a dieldrin concentration of 2.92 ppm in pheasants is "possibly hazardous to pheasant life." However, this value is below the 3.2 ppm concentration considered hazardous by Wiemeyer and Cromartie (1981) and is not supported by pheasant studies at RMA.

Response:

The difference between 2.9 and 3.2 ppm may not be biologically significant given all sources of uncertainty. Therefore, it was necessary to indicate possibly hazardous effects.

Comment 171b:

Page 5-352, fourth paragraph

We agree with the last line of this paragraph: It would be virtually impossible for the pheasant from Larimer County to have come into contact with RMA vegetation or insects given the small range of pheasants. We believe that findings of this type should be given greater weight whenever evaluating tissue levels found in highly mobile species on RMA. For example, the high level of dieldrin in the pheasant from Larimer County underscores that a dead raptor with a high level of pesticide could have acquired all or part of the contaminant at some other location.

Response:

Great horned owls probably are year round residents and feed mostly on RMA. One must consider home range data before assigning all contaminant levels to other sources. The source of dieldrin was for the Larimer county pheasant is unknown, but one anomaly does not overshadow the findings that many RMA species have elevated tissue

levels in comparison to offpost controls. Dieldrin concentrations in carcasses of juvenile pheasants were significantly higher on RMA than offpost samples, including the Larimer county pheasant.

Comment 172:

Page 5-353, second paragraph, last sentence

Since endrin was not found to be present in birds consuming insects, the statement that birds consuming grasshoppers are at risk from endrin is speculative and therefore should be deleted.

Response:

This text is justified. Endrin was found in a pheasant egg, presumably transferred through the hen. Endrin is also present in the RMA (soils) environment.

Comment 173:

Page 5-353, third paragraph, line 2

Was the sample that reportedly contained 56.3 ppm of dieldrin in the tissues reanalyzed to verify that the value was correct?

Response:

This sample contained high levels of aldrin, endrin, and dieldrin that required a dilution by a factor of 15 and a reanalysis to obtain values for aldrin and endrin, and a second dilution by a factor of 25 and reanalysis to obtain a value for dieldrin. This procedure is documented and followed strict USATHAMA QA/QC procedures.

Comment 174:

Page 5-353, third paragraph, line 5

What reference was used to determine a brain/carcass ratio?

Response:

See text change on page 5-348.

Comment 175:

Page 5-353, third paragraph, last sentence

Would a dove drink from Basin F? Change "does" to "dose" in the next-to-last line.

Response:

Birds have been observed drinking from Basin F. See text for change of "does" to "dose".

Comment 176:

Page 5-354, first paragraph, second sentence

Were soil samples taken from the same area where the avian species were collected and likely to feed? If the species feed in other areas, such as Barr Lake, then the source of dieldrin, DDT, etc., could be from other locations, as well as RMA.

Response:

Yes. It is conceivable that contamination could come from other sources; however no identified sources comparable to RMA are known to occur in the general area.

Comment 177:

Page 5-354, second paragraph

From what locations were the soils sampled which supposedly are carried into the surface waters at the RMA facility? What is the frequency or extent of such transport?

Response:

See Southern Study Area Report.

Comment 178:

Page 5-355, fourth paragraph, line 3

We suggest that the wording of "contaminants are . . . transferred" be changed to "contaminants may be transferred," since this would vary with the contaminant and food web involved.

Response:

Text is correct as written.

Comment 179a:

Page 5-356, last paragraph

The bioaccumulation of mercury is mentioned here. Why is mercury not discussed in the section on the terrestrial biota? Only dieldrin is discussed in those sections, and the potential effects of mercury, DDT/DDE, and other chemicals are virtually ignored.

Response:

Mercury, DDE, DDT, endrin, and arsenic were not found at hazardous levels. Whenever any chemical (including mercury) was found at significantly different levels between test sites and controls it was

discussed in the terrestrial biota sections. Additionally, potential effects of all contaminants of concern were discussed in the toxicity assessments.

Comment 179b:

Page 5-356, last paragraph

It is stated in this paragraph that the process of bioaccumulation is quite complex. Yet, in the earlier sections on terrestrial biota, simplistic assumptions were made on the bioaccumulation of dieldrin through the terrestrial food web, without any discussion of the complexity of the process.

Response:

Conservative but reasonable assumptions were made because of the complexity of the process.

Comment 180:

Pages 5-357 to 5-359

Aquatic tissue data provided by Shell/MKE were generally not used in this discussion. We request that the Army/ESE review the Shell/MKE input for 1986 and 1988 and use it as the primary data source for the RI, with Rosenlund et al. (1986) as supplemental data. Values reported by Shell/MKE for RI investigations were obtained using USATHAMA-certified methods and thus should have greater reliability. Data from 1986 and 1988 also are the most current available.

Response:

Revised input from MKE has been obtained since the Draft Final Biota RI was prepared. This material has been incorporated into the Final Biota RI. The two studies provide different types of information. Data from both sources are used as appropriate.

Comment 181:

Page 5-357, second paragraph, tenth line

Organochlorines may be "persistent" as opposed to transitory, but it should be noted that dieldrin levels have steadily declined in aquatic ecosystems at RMA without any remediation. Thus, they would not appear to be permanent, as the reader might infer.

The discussion in this paragraph should be revised to reflect Shell/MKE data collected in 1986 and 1988, and previously provided to the Army/ESE. How do the low values reported by Rosenlund et al. (1986) compare with USATHAMA-certified reporting limits?

We also note that whole-body data are not described in this discussion. For an assessment of wildlife exposure, we believe that whole bodies would be more relevant than fillets. Shell/MKE has previously provided whole-body data to the Army/ESE, as well as fillet data for edible-size game species.

Finally, we question the usefulness of a decreasing order incorporating data from fish fillets, fish viscera, invertebrates, and plants. We believe that a tabular summary, such as provided by Shell/MKE, would be more helpful to the reader.

Response:

The order of presentation is appropriate, given the bioaccumulation of these contaminants. Shell/MKE material has been provided to augment this section.

Comment 182:
Page 5-357, last paragraph
The "correlation" is impossible for the reader to evaluate, because lipid contents are provided for viscera samples for which dieldrin concentrations are not reported in the previous paragraph. Also, it is possible that the bass, pike, and channel catfish are older than the bluegill and bullheads: larger species of fish tend to live longer, because once they grow past a certain size they become essentially predator-proof. We do not doubt that lipid content affects pesticide levels, but we are not convinced that the conclusion is as strong as suggested by the text. Moreover, greater discussion of concentrations as they relate to exposure of humans or predators (e.g., fillets and whole bodies of various species in various lakes) would seem more appropriate than the discussion of lipid content.

In this paragraph, and the following page, "catfish" should be "channel catfish." Black bullheads also are catfish, in the same genus as channel catfish.

Response:

Section has been revised.

Comment 183:
Page 5-358, third paragraph
Delete the comma after the first "mercury" on line 1.

In the last sentence of the paragraph, the statement that mercury levels in RMA fish are "high" is meaningless without some perspective. What constitutes "high?" It would be preferable merely to report the data.

Response:

See text change on page 5-335.

Comment 184:

Page 5-358, last paragraph

As we discussed in information provided to the Army/ESE, Rosenlund's conclusions about mercury bioaccumulation are based on concentrations in pike fillets much higher than those reported by Thorne (1986) or detected during Shell/MKE studies.

Is the value for pike fillets (1.902 ppm) a mean or maximum? The mean of three pike fillets collected by Shell/MKE from Lower Derby was only 0.406 ppm (0.278-0.470). Thorne (1986), in studies conducted by the Army, reported a mean mercury value of 0.69 ppm in pike fillets from Lower Derby. If the MKE or Thorne data are used, the bioaccumulation of mercury is not convincing--in fact, tissue values are about the same as the range of values reported for the sediments by Myers et al. (1983).

Response:

The value 1.902 is a mean value.

Comment 185:

Page 5-359, first paragraph

Again, we believe that the discussion should focus on data collected by Shell/MKE. The data are both more current and, having been obtained using USATHAMA-certified methods, presumably more reliable.

How do the low values reported in this paragraph compare with certified reporting limits used during RI studies?

Response:

The information provided by both documents is pertinent to the Biota RI. Data from Rosenlund's and MKE's studies have been incorporated as appropriate.

Comment 186:

Page 5-359, second paragraph

Same comment as the preceding. For example, Rosenlund et al. (1986) reported mercury in Lake Ladora pike and bass fillets at 2.94 and 2.44 ppm, respectively. In contrast, Thorne (1986) reported 0.92 and 0.74 ppm for Lake Ladora pike and bass fillets, while Shell/MKE (1988) reported 0.41 and 0.35

ppm. Aquatics information provided by Shell/MKE includes a table comparing Rosenlund et al., Thorne, and Shell/MKE findings for fillets from the South Lakes.

Response:

See response to comment 185.

Comment 187:

Page 5-359, third and fourth paragraphs

Again, we disagree with the nearly total reliance on data from Rosenlund et al. (1986).

Response:

See response to comment 185.

Comment 188:

Page 5-360, second paragraph

The third sentence states that a "multitude of interacting causes could be responsible for the differences detected among contaminated lakes and control areas." Why are these factors not taken into consideration for the terrestrial habitats and biota? The same qualifying statements should be made about the other results. Moreover, the validity of the offsite comparison lake is at least as high as for most offsite "control" locations cited in the RI. This statement has the effect of diminishing the value of the Shell/MKE data and should be deleted. The same is true for the remainder of this paragraph. Were the Army's sunflower, morning-glory, earthworm, and grasshopper studies more "rigorous?"

The purpose of collecting the Shell/MKE aquatic ecology data was to determine whether there were apparent community-level differences that might be related to contamination. The data show that the onsite lakes were generally as healthy-based on the parameters evaluated--as the offsite lake. The fact that the RMA lakes compared favorably with the offsite lake should not be surprising, considering the reputation of the South Lakes as a quality sports fishery. It should also be noted that the offsite lake is managed as a sports fishery by a private road-gun club and thus does not represent a "low target" for comparison.

The aquatic ecology data were not proffered as a substitute for tissue data, but rather as perspective so that readers or subsequent users of the report will have an accurate picture of the lakes. Contamination in fish may or may not represent unacceptable exposure risks to humans on bald eagles, but it apparently is not reflected in the structure or function of South Lakes

ecosystems. We do not agree that a "statistically more rigorous study design" was warranted.

We have not attempted to show "cause and effect." Obviously, we do not believe that higher levels of pesticide and mercury onsite have caused the South Lakes to exceed McKay Lake on some ecological parameters.

We believe that everything in this paragraph beyond the second sentence is unjustified and should be deleted.

Response:

Interacting causes in aquatic ecosystems (e.g., lake flooding and draining activities, sedimentation, etc.) is more pronounced in relation to biological data because the aquatic systems are more "closed" ecologically, and bioconcentration, which occurs in aquatic but not in terrestrial systems is affected by these interacting factors.

Comment noted.

Comment noted.

See appropriate text changes in this paragraph.

Comment 189:

Page 5-363, second paragraph

We have serious concerns about the way this paragraph is worded. The last sentence is inconsistent with the Army's own sample sizes and use of offsite comparison areas. The paragraph says little about the data. Were "all other features" of the Army's onsite and offsite areas "equal?"

We conducted tests of physicochemical and lower trophic level parameters to show that the two lakes are, in fact, similar. The statistical conclusions presented in the paragraph are imprecisely stated. Mercury was present in both Lower Derby and McKay lake bass and bluegill samples, but the differences were statistically significant only for bass. Pesticides (including dieldrin) were undetected in McKay Lake, and onsite-offsite statistical comparisons are therefore not possible.

Response:

Army control sites were selected to be as similar as possible to RMA sites, but were not similar "in all other features". The lack of statistical power due to small sample sizes combined with the

differences between sites is still a valid concern. See appropriate text change on page 5-340.

Comment 190a:
Section 6.0, Glossary
Page 6-1, Anurans
What is a "tree toad?" Perhaps what is meant is "frogs, tree frogs, spadefoot toads, and true toads."

Response:
The common names used in this definition apply to various families within the Class: Amphibia, Order: Anura. Additional information for the anurans that occur on and near RMA can be found in: Hammerson, G.A., 1982, Amphibians and Reptiles in Colorado, Colorado Division of Wildlife.

Comment 190b:
Section 6.0, Glossary
Page 6-1, Anurans
We suggest changing "Acetylcholinesterase inhibition - causes" to "Acetylcholinesterase inhibitor - a chemical that causes."

Response:
See text change on page 6-1.

Comment 190c:
Section 6.0, Glossary
Page 6-1, Anurans
ARAR: More properly "Applicable, or relevant and appropriate requirement" (a requirement cannot be both applicable and relevant and appropriate).

Response:
See text change on page 6-1.

Comment 191a:
Page 6-2
Carcinogenic--Change to "Carcinogen - a substance or agent . . ."

Response:
See text change on page 6-2.

Comment 191b:
Page 6-2
CERCLA--Add commas: Response, Compensation, and Liability.

Response:

See text change on page 6-3.

Comment 192a:

Page 6-3

Chronic exposure--Change the third sentence to read, "Doses are selected so that at least . . . ." Also, "chronic exposure" is defined as exposure lasting longer than 3 months (sometimes 6) (Klaasen et al. 1986), although in practice lifetime exposure is the most desirable.

Response:

The Army disagrees with your interpretation: lifetime exposure is the norm. The text was missing some punctuation, and corrections were made. See text change on page 6-3.

Comment 192b:

Page 6-3

Depuration--In fact, Rand and Petrocelli (1985) give the following definition: "Elimination of a chemical from an organism by desorption, diffusion, excretion, egestion, biotransformation, or another route." It is not limited to aquatic organisms.

Response:

See text change on page 6-3.

Comment 193a:

Page 6-4

EC50--Change "concentration [a]ffecting" to "median effective dose: the concentration effective in producing a sublethal response in . . . ."

Response: Comment noted. See text change on page 6-4.

Comment 193b:

Page 6-4

Ecological magnification--We do not agree that this is limited to the soil-plant pathway.

Response:

The text has been changed to "soil to organism uptake" on page 6-4.

Comment 193c:

Page 6-4

Food chain--The definition needs rewording. Each member is not necessarily both predator and prey; consider the top and bottom members.

Response:

This definition was taken from the Hammond Barnhart Dictionary of Science, Hammond, Inc., 1986.

Comment 194a:

Page 6-5

Lagomorph--Replace "Comprised of" with "Includes" or "Comprises."

Response: See text change on page 6-5.

Comment 194b:

Page 6-5

LC10--Replace "lowest lethal concentration other than LD50" (which is more properly "LDlow") with "concentration lethal to 10 percent of the exposed population."

Response:

See text change on page 6-5.

Comment 194c:

Page 6-5

MATC--Should "tissue" be "toxicant" as used in Rand and Petrocelli (1985)? Should MPTC be used in the document to avoid confusion with other uses of MATC?

Response:

The use of "tissue" in MATC is correct.

Comment 195a:

Page 6-6

Passarine--The correct spelling is "Passerine." Insert "(naked when hatched)" after "altricial."

Response:

See text change on page 6-7.

Comment 195b:

Page 6-6

Periphyton are attached algae, not merely attached organisms.

Response:

According to the Hammond Barnhart Dictionary of Science (Hammond, Inc., 1986) periphyton include algae, insect larvae, small crustaceans, and other organisms.

Comment 196:

Page 6-7

Phraetophytic--The correct spelling is "Phreatophytic."

Response:

See text change on page 6-7.

Comment 197:

Page 6-8

Subchronic toxicity test--Insert "ideally including" after "employed" since the doses will not be chosen with full knowledge of their effects.

Response:

See text change on page 6-8.

Comment 198:

Appendix A

We found several typographical errors in the species lists. An edited copy of Appendix A is being provided under Separate cover.

Response:

Comment noted. An edited copy was never received.

Comment 199:

Appendix C

Additional materials used by the Army at RMA should be added to Table 1: Baygon roach bait, Cyanogas, aldrin, dieldrin, experimental anti-coagulant dusting powder, Dacthal W-75, Arosam 75, endrin, Sevin, zinc phosphide, and DDT. RIA 026 0892-0898. Also, the CAR for Site 31-7 (May 1988, Task 15) states that the Army stored heptachlor at the Arsenal: presumably it also was used.

Response:

The U.S. Army Environmental Health Agency review cited in the comment does not indicate what pesticides were used at RMA. While the materials listed in the comment were stored at RMA, they should not be included in Table 1 since Table 1 lists definite quantities of pesticides used at RMA. It is inappropriate to assume that materials stored at RMA were actually used.

SUMMARY COMMENTS

A. We do not believe that the Biota RI Draft Final Report accurately portrays the nature and extent of contamination in vegetation and wildlife at RMA. This results from (1) a lack of perspective concerning areas of contamination and contaminant effects compared to the site as a whole; (2) an over-emphasis on historical data of questionable reliability and relevance; and (3) a negative, journalistic writing style throughout much of the report.

Response:

Sites of contamination were clearly differentiated from onpost control sites (e.g., areas free from contamination) in section 3.0 and 4.0 of the text. Historical data are presented in accordance with accepted RI/FS guidance. Judgement regarding the reliability of the historical data was not possible. Information from historical sources was used, as appropriate, to supplement current information. The style used was objective and aimed at communicating the meaning and results of technical studies so that they could be understood and used by decisionmakers.

B. We also are concerned with language that appears to have been retained from earlier, litigation-oriented documents, and with a lack of balance in discussing pesticides versus arsenic and mercury.

Response:

Several sources were consulted for information on earlier studies. This report is a balanced and unbiased presentation of the nature and extent of contamination of biota at RMA.

C. Although much of the information provided by Shell/MKE was incorporated appropriately, substantial materials addressing contaminant effects and aquatic ecosystems were not used.

In our opinion, these omissions were unwarranted and weaken the document.

Response:

The U.S. Army Environmental Health Agency review cited in the comment does not indicate what pesticides were used at RMA. While the materials listed in the comment were stored at RMA, they should not be included in Table 1 since Table 1 lists definite quantities of pesticides used at RMA. It is inappropriate to assume that materials stored at RMA were actually used.

Some of the material provided by Shell/MKE was not incorporated into this version of the Biota RI because some of the results, although informative, were not relevant to the purpose of this document.

D. We disagree with some of the site-specific criteria presented in the Contamination Assessment. In our opinion, many of these criteria were developed using an unrealistically conservative approach and could result in unreasonable cleanup standards. This effort should have involved discussion with the parties.

Response:

The Army does not agree that this report "...could result in unreasonable cleanup standards." Shell provided significant support to the preparation of this product and had ample opportunity to comment and input.

E. In conclusion, we believe that the changes needed to adequately address our general and specific comments are too extensive to be accomplished without an additional opportunity for review and comment. We therefore request that the Army prepare a revised version (2.3) of the Draft Final Report. Such an effort should involve careful consideration of the language and style as well as the data interpretations. Shell/MKE would be happy to meet with the Army to discuss our comments and the data which we provided earlier.

Response:

The Army does not agree with Shell's request for a new draft final report. The requests is contrary to the RI/FS process agreed upon in the Technical Program Plan and the Federal Facility Agreement. Surely, such an effort is not justified for the consideration of language and style. The Biota RI already fulfills the requirements of the NCP.

ADDENDUM TO SHELL OIL COMPANY COMMENTS ON BIOTA RI DRAFT FINAL REPORT (VERSION 2.2)

Comment A-1:

Page 4-58 and 4-59, Table 4.3-4

We have found typographical errors in some of the mean values reported. In addition, a few data sets are incorrect or missing. Some of these mistakes are a result of errors in the raw data table provided by Shell/MKE. We will provide a corrected copy of the table to the Biota RI authors for their use in preparing the Final Report.

Response:

Appendix A corrections have been incorporated.

Comment A-2:

Pages 4-66 and 4-67, Section 4.3.3.5

Upon reviewing the Rosenlund et al. (1986) report, we have discovered discrepancies between their findings and the summary discussion in the first three paragraphs on page 4-66.

a. In the second paragraph, it is stated that variations among months for mercury in plankton were significant ($P < 0.10$). Actually, the level reported by Rosenlund et al. should be stated as "approaching significance" under the statistical criteria described in the Biota RI.

b. Similarly, the third paragraph (second sentence) misstates the result of aldrin and dieldrin in fish. Using the alpha (probability) levels established by Army/ESE for the Biota RI, the results reported in Rosenlund et al. (1986)b indicate that aldrin was significantly higher in Lower Derby Lake only for bass and pike fillets. Differences for bass viscera and bullhead fillets approached significance.

c. Finally, we note that the third paragraph discusses aldrin and dieldrin, but not mercury. Rosenlund et al. reported highly significant differences ($P < 0.001$) in mercury among lakes for bass fillets.

We will provide a recommended revision of this text to the Biota RI authors for their use in preparing the Final Report.

We also will provide recommended revisions of the last three paragraphs, which summarize statistical analyses of Shell/MKE data for 1986 and 1988, and of the portion of Appendix B dealing with the Shell/MKE aquatics data.

Response:

This comment has been addressed in the revised text for this section.

Comment A-3:

Page 5-350, third paragraph

The last sentence of this paragraph is confusing and misleading. The middle portion of this sentence states that "only 1 of 14 deer from RMA contained detectable levels of contamination." Based on this fact, the first portion of the sentence is incorrect: the point is that valid statistical comparisons were impossible because only one sample showed contamination.

The concluding statement in the last part of this sentence ("contaminant effects on deer populations are probably negligible") is also misleading. This statement implies that there are effects which might not be negligible. It would be more in line with the data to state that "deer population at the Arsenal appear to be unaffected by contamination."

Finally, we disagree with the implication in the first two sentences of this paragraph that the large population of deer on RMA somehow masks contamination effects. In our opinion, the obvious health and vigor of deer on RMA, the large population size, their apparent longevity and reproductive success, and the nearly complete absence of tissue contamination in no way support such speculation.

Response:

Statistical comparisons between offpost and onpost deer are possible and were made (see Appendix B). See text change on page 5-327. The Army believes that other environmental factors do influence the size of deer population on RMA.