



UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
REGION I
JOHN F. KENNEDY FEDERAL BUILDING
BOSTON, MASSACHUSETTS 02203-0001

October 12, 1995

Mr. Phil Otis
U.S. Department of the Navy
Northern Division - NAVFAC
10 Industrial Highway
Code 1811/PO - Mail Stop 82
Lester, PA 19113-2090

Re: Revised Draft IR Program Allen Harbor Landfill and Calf Pasture Point, Marine Ecological Risk Assessment Report, August 1995, Former Naval Construction Battalion Center, Davisville, RI

Dear Mr. Otis:

Please find attached the Environmental Protection Agency's (EPA) comments on the above referenced document. Most of the overall data interpretations and conclusions of the report are reasonable, sound, and supported by the weight of evidence presented, which reveals clear ecological risks and apparent impacts in the salt marsh and nearshore, intertidal mud flat habitats of Allen Harbor proximal to the landfill. Some specific comments are attached that, if satisfactorily addressed, will provide a clearer indication of the incremental risk associated with the sites.

I look forward to working with you and the RIDEM to produce a draft-final RI. Please contact me to set up a meeting to discuss the Navy's responses to these comments at (617) 573-5736.

Sincerely,

A handwritten signature in cursive script that reads "Christine A.P. Williams".

Christine A.P. Williams
Remedial Project Manager
Federal Facilities Superfund Section

Attachment

cc: Judy Graham, RIDEM
Lou Fayan, NCBC
Tim Prior, USF&WL
Ken Finkelstein, NOAA
Andy Beliveau, EPA
Bill Brandon, EPA
Jayne Michaud, EPA
Scot Gnewuch, ADL
Nick Lanney, EA



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EPA Comments on the Revised Draft, Allen Harbor Landfill and Calf Pasture Point Marine Ecological Risk Assessment Report

Broad, general comments that have the greatest significance for the overall approach to the Ecological Risk Assessment (ERA) and its presentation of results are presented first, then followed by comments on specific issues that need to be addressed in the Final ERA Report.

General Comments

General Comment #1

Most of the overall data interpretations and conclusions of the report are reasonable, sound, and supported by the weight of evidence presented, which reveals clear ecological risks and apparent impacts in the salt marsh and nearshore, intertidal mud flat habitats of Allen Harbor proximal to the landfill.

General Comment #2

Unfortunately, as discussed below in more detail, the data analyses provided in support of these conclusions are incomplete, inappropriate, and/or as presented, of little practical value to risk managers in quantifying the actual magnitude of risks from specific contaminants of concern (COCs) or classes of COCs and/or in concisely and clearly illustrating what the key COC drivers of average and maximum risks are within the salt marsh, intertidal mud flat, and subtidal exposure zones.

General Comment #3

The primary problem with the technical approach and data presentation in the report is a conceptual one, which has very significant "ripple effects" on the content and discussions of the entire document. The overly broad, misleading, and incorrect definition of the term hazard quotient (HQ), as including not only criteria-based risk quotients but also media-specific concentration ratios of Allen Harbor habitats versus reference habitats, is a fundamental flaw in the ERA methodology that confounds most of the risk discussions throughout the report. These ratios of COC levels in sediments of one location versus another location **are not HQs**, they are merely concentration ratios and such should be renamed, "Concentration Ratios (CR)" for clarity. Toxicity benchmark criteria must be used to calculate true and meaningful HQs, both for the Allen Harbor and reference locations.

General Comment #4

This misuse of the risk quotient method also compromises the calculations (and practical value) of the aggregate, COC class-level, hazard indices (HIs), that represent the sum of HQs for each location and/or the sum of the mean HQs among all samples within a particular exposure zone. Only after these true HQs/HIs

are systematically and separately generated can the incremental Allen Harbor risks above those found at background locations be quantified. These point-by-point calculations of Allen Harbor:reference sample COC concentration ratios also leads to a risk analysis of little practical value because it compares contamination levels rather than the actual organismal risks from these levels. Only the true, criteria-based HQs/HIs should be presented as indicators of ecological risk, since they serve both to document key risk drivers and offer the best hope of clarifying the chemical causes of observed toxicity (amphipod and sea urchin) and/or biological condition indices suggesting adverse effects.

General Comment #5

The concept of "tissue-based HQs," as presented in the report, is similarly flawed, in that a true tissue-based HQ would consist of the ratio of the tissue concentration for a COC to the known toxicity threshold for a particular body burden (concentration) for the organism that accumulated the COC. Although this offers insight as to "how much worse" COC bioaccumulation seems to be in Allen Harbor than in the reference locations, this question is not pertinent to the primary objectives of a Comprehensive Environmental Response, Compensation, and Liability Act of 1980 (CERCLA) ERA as it does not help risk managers to assess for which COCs incremental risks are unacceptable and/or the spatial extent of cleanup warranted for such COCs within Allen Harbor.

General Comment #6

Navigating through the large volumes of raw data tables and graphs presented in the main report and its appendices is very difficult. These raw data could and should be reduced for a more concise and meaningful presentation using one or more, "roll-up" matrices to report average, medium-specific, criteria-based HQs and COC-class level HIs for each exposure zone and for the reference stations. The resulting roll-up tables would be very useful for risk managers by presenting much needed concise snapshot look at the risks at the Harbor. This type of roll-up was presented for qualitative results in Table 7.3-1, such as biological effects and body burden data. Although it nicely summarizes the qualitative weight of evidence, this table suffers from the qualitative inferences as to COC-class level, incremental, site-derived exposure risks that are not systematically based on ecotoxicological risk quotients. Rather, these chemical "exposure" entries are inappropriately based on an "apples and oranges" combination of both true, criteria-based risk quotients (HIs) and Allen Harbor:reference habitat COC concentration ratios for physical media, that are misnamed as "HQs" and "HIs".

General Comment #7

The net effect of using this "hybrid" risk quotient method, coupled with the current presentation of excessive volumes of incompletely reduced raw data, is a non-reader-friendly document

that is often very confusing, even to perennial technical reviewers of this project. As a result, the report also fails to clearly and concisely quantify the incremental, -site-associated marine ecological risks within the context of systematically quantified, criteria-based, background risks at the two reference locations.

General Comment #8

The report provides incomplete calculations of criteria-based HQs/HIs, in part due to omissions of such HQ/HI tables that were presented for polychlorinated biphenyls (PCBs) and pesticides in the May 1995 Draft version (e.g., Tables A-1.1 and A-1.2) and the lack of such tables for organic COCs in surface water and in sediment pore water, the latter of which were to have been inferred via equilibrium partitioning (EqP), as requested by EPA in Comment No. 9 in EPA's letter dated June 30, 1995.

General Comment #9

Tables of sediment chemistry data and "hybrid" HQs/HIs presented throughout Appendices A1 and A2, moreover, inappropriately averaged together both the COC concentrations and HQs/HIs for Allen Harbor with those of the reference habitats, thus defeating the main purpose of the reference habitat sampling. That purpose was to present average, criteria-based HQs/HIs separately for Allen Harbor versus reference habitats and thus to calculate NCBC site-related risk increments within Allen Harbor. These tables are also very confusing because they present meaningless data manipulations, such as the sum of point-specific HQs for each congener (in the 3rd from last column in each table) and also sum the point-specific congener sums, collectively, across all sample locations (at the bottom of this same column). The congeners should be (a) added only down the point-specific columns; (b) each sum multiplied by a factor of 2 to estimate Total PCBs; and then (c) these column-specific Total PCB concentrations should be averaged across columns, but separately within each of the discrete exposure zones (salt marsh [V#s], intertidal mud flats [W#s], and subtidal zones [D#s].) The resultant range exhibited among these sample/column-specific Total PCB values then should be carried, along with their corresponding zone-specific mean concentrations, into a roll-up summary table of total PCB concentrations reported by individual zones. Since the same approach is needed for data reduction for other COC classes, all of these contamination and risk tables must be revised in the final report.

General Comment #10

Attempts to identify the COCs likely to be responsible for biological effects, as indicated by observed toxicity in the bioassays, neoplasia incidence, and/or shellfish/fish condition indices, by spatially comparing COC concentrations with effects data, were not successful. This is not surprising since the approach to this analysis requested by EPA in Comment No. 9 on

June 30, 1995, consisting of correlation studies of these effects data as a function of criteria-based HQs/HIs, was not followed systematically. It is significant that, where this approach was used to calculate ambient water quality criteria (AWQC)-based metal HQs for sediment pore water, more light was shed on the possible COCs causing observed toxicity. In order to further clarify which COCs may be causing the observed ecological effects of sediment contamination, this previously requested approach should be applied to all COCs in whole sediment and pore water.

General Comment #11

Estimates of total PCBs are inconsistently presented in both the contamination and risk quotient tables, either incorrectly as the sum of congeners (e.g., Tables A.1-1.1 and A.1-2.1) or correctly as two-times (2x) the sum of congeners (Table A.1-3.1). Although the congener-based total PCB estimates had been correctly presented in Tables A-1.1 and A-1.2 of the May 1995 draft of this report, that version had incorrectly calculated (and thus underestimated by a factor of two) the criteria-based HQs/HIs for total PCBs, by dividing the sum of congeners by the Effects Range - Low (ER-L) value of 22.7 ppb. All tables in the final report must calculate total PCB levels (and associated HQs) using the factor of 2x to convert the sum of congeners into a total PCB estimate.

General Comment #12

Despite the report's demonstration of acceptable comparability between older and newer data from successive phases of the Allen Harbor investigation, data from the Risk Assessment Pilot Study (RAPS; Munns et al., 1991, 1993) appear not to have been integrated statistically into the risk quotient calculations, as previously requested by EPA. Rather, these RAPS data were reproduced in the report (see Tables 3.1-1 through 3.1-4) and discussed only from a historical perspective. There is no explanation otherwise in the report as to whether and/or where/how these older RAPS and newer Phase III data for physical media and biota in Allen Harbor were merged/reduced to produce cumulative data summaries. This is a significant omission, because most of the RAPS sediment samples were from different subtidal locations than those sampled in Phase III. Also, in many cases the RAPS concentration data differ significantly from those for Phase III subtidal samples (e.g., maximum, subtidal Total PCBs reported as 103 ppb at AHD8 in Phase III [Table A1-1.1] versus a maximum, subtidal Total PCB level of 505 ppb reported at AH08 [different location] by Munns et al. [1991] and reproduced in Table 3.1-1 of the current report.) These RAPS data must be statistically integrated with Phase III data to revise both the contamination data summaries and ecological risk quotient calculations of the Final ERA Report.

General Comment #13

The purposes of the new, principal components analysis added to the report are not explained clearly. The methodological rationale for and process used to generate these statistical analyses, as well as how the results pertain to questions of ecological risk, should be clarified. This section may be more appropriately included in the fate and transport discussions of the Remedial Investigation Report.

General Comment #14

As reflected in some of the foregoing general comments on methodology and data presentation, and further discussed in many of the specific comments that follow, many of EPA's previous comments/requests have not been adequately addressed or satisfied in the current report.

SPECIFIC COMMENTS

Specific Comment #1

Titles of tables and figures that do not present true, criteria-based HQs are very misleading and should be revised to accurately indicate that Allen Harbor:reference site concentration ratios, not risk quotients, are being presented (e.g., Tables 6.3-1 through 6.3-4 and 6.6-1 through 6.6-4; Figures 6.1-1 through 6.1-16). (Although most of these figure titles in effect do show the ratios of criteria-based HQs, their titles are still misleading since criteria-based HQs were not first calculated for all of these sample locations.)

Specific Comment #2

Summary tables of (incorrectly calculated) criteria-based (ER-L) HQs, that had been presented in Tables A-1.1 and A-1.2 of the May 1995 draft, were omitted from this revised version of the report.

Specific Comment #3

Figures 7.2-2 through 7.2-4 were missing from the review copy of the report.

Specific Comment #4

Graphs of the biota-to-sediment bioaccumulation factors (BSAFs) and surface sediment metals data, respectively presented as Figures 6.3-1 through 6.3-4 and Table 5.3-2 in the May 1995 draft report, were omitted from the revised draft and should be restored. The metals data are essential and the BSAF figures were especially informative, particularly with respect to inter-species differences in BSAFs among shellfish, that should be accounted for in food chain models to be used in the terrestrial ERA (e.g., oysters and ribbed mussels have higher BSAFs than the soft and hard shelled clams; since these former two species are most accessible to terrestrial shellfish eaters, only they and their BSAFs should be used in the NCBC terrestrial food chain models.)

Specific Comment #5

Several errors and/or data omissions are evident in a variety of the tables:

- Data in tables of the current report sometimes differ from those for the same COCs/samples presented in the May 1995 draft (e.g., current Table A1-1.6 versus May 1995 Table A-3.1.)
- The maximum reported sediment HQ (ER-L based) for copper in Table A.2-1.1 was cited as 5.33 whereas the maximum point-specific copper HQ in this table was listed as 5.38 for sample location AHW4. This suggests the tables were manually generated (a typo) and may have other internal data inconsistencies.
- Table 3.3-1 has a different Phase II soils list of preliminary onshore COCs than that of Table 3.3-3 in the May 1995 draft; similar discrepancies among the lists of proposed offshore COCs appear among new Tables 3.3-1 and 3.3-5 and Table 3.3-3 of the May 1995 draft report. Also, why are the butyltin compounds chosen as offshore COCs in this new Table 3.3-5, without having first been identified as landfill-associated COCs in onshore soil and/or ground water?
- Table 3.3-2 incorrectly presents the ER-L for Total PCBs as 22.7 ug/g (ppm), when it should be 0.0227 ug/g (ER-L is 22.7 ug/Kg or ppb.)
- Table 3.3-3 reports identical minimum and maximum values across four different classes of reference sampling locations for each of several metals (e.g., arsenic, cadmium, copper, mercury, nickel, and zinc) and aldrin, and among different VOAs within a location; are these actually detection limits?

Specific Comment #6

Table 3.3-4 incorrectly: (a) uses the simple sum of PCB congeners in the COC screening exercise, whereas total PCB concentrations should be calculated as (sum of congeners x 2); and (b) applied the reference concentrations of PCBs in the screening when the ER-L value of 22.7 ppb should have been used.

Specific Comment #7

Risk summary tables don't clearly show criteria-based, mean and maximum, COC-specific HQs and COC class-level HIs for each harbor subzone for the following:

- Whole sediment. (using ER-Ls and/or SQC)
- Sediment pore water (using measured metals and EqP-inferred organic COC concentrations versus AWQC)

- Surface water (using marine AWQC)

Specific Comment #8

No tissue data summaries, including minimum, mean, maximum, and location of maxima are provided for individual shellfish and fish species, that integrate both the RAPS (see report Table 3.1-1) and Phase III tissue data. (These empirical data are needed for use in the food chain models of the terrestrial ERA.)

Specific Comment #9

Corresponding summaries of COC/species-specific, BSAFs, which are not normalized for lipids or sediment total organic carbon (TOC) levels, should be provided based on tissue analyses of non-purged shellfish and whole fish (mummichog) samples. Although the lipid and TOC-normalized BSAFs presented are of scientific interest when comparing the BSAFs among species and/or locations, such BSAFs are not meaningful in assessing food chain exposures to the predators of these marine biota. Any use of molluscivorous and piscivorous food chain models by the Navy to calculate terrestrial risks to wildlife should incorporate non-normalized BSAFs (i.e., total tissue concentration:bulk sediment concentration) into the exposure assessment, based on data in this marine ERA, since shellfish/fish predators consume their prey whole, rather than extracting and devouring only the lipid fraction of their prey.

Specific Comment #10

Additional discussions are needed in Section 4.3.1 to distinguish the mean and maximum, chemical-specific sediment HQs for specific polycyclic aromatic hydrocarbons (PAHs) versus HQs for Total PAHs (TPAH). The ER-L criterion for TPAH should be used to calculate HQs only for that subset of PAHs for which there are no chemical-specific ER-L values. The use of the TPAH value for all PAHs, otherwise, could underestimate the aggregate, PAH class-level risk (HI).

Specific Comment #11

Conclusions in the first paragraph on Page 6-6, that metals are probably not responsible for reduced sizes of the mummichogs at Station W5, since metal hyperaccumulation was not detected in the fish tissues at this location, contradicts earlier claims that fish tend not to bioaccumulate metals (e.g., 3rd paragraph on Page 3-22). Instead, natural age-related variation in fish size is offered as an alternative explanation for the smaller mummichogs at W5, since this explanation "...would be consistent with tissue metals data presented in Table 5.3-2, which do not indicate any obvious differences for Station W5 compared to the other stations." Since metals and/or organic compounds can be toxic to fish and impair their growth and reproduction even without being bioaccumulated, the natural variation argument, although theoretically possible, is not supported by the evidence as currently presented.

Specific Comment #12

The first paragraph in Section 6.2.2 on Page 6-6 seems to be missing some text, since its first sentence discusses regression analyses of mummichog tissue versus sediment metal levels, and the second sentence says that correlations were poor except for chromium in oysters. Is there some missing text on these fish and shellfish tissue/sediment metal regressions, that should appear between the two sentences of this paragraph?

Specific Comment #13

Section 6.3, Analysis of Bioaccumulation, should, but does not, integrate RAPS data on COC bioaccumulation data with those from this Phase III study. The COC "Biota-to-Sediment Accumulation Factors (BSAFs)," discussed here for fish and shellfish analyzed from Allen Harbor and the reference stations, should be combined from both studies and integrated into this ERA and the interdependent food chain models of the terrestrial ERA being prepared by EA.

Specific Comment #14

Section 6.3.2, Metals Bioaccumulation introduces the term "Biota Accumulation Factors (BAFs)," as "...representing tissue residues normalized to sediment concentrations," without distinguishing its meaning from that of the previously used term BSAF. This section also suffers from the misapplication of the HQ concept to calculate "BAF HQs," which are ratios of Allen Harbor:reference habitat BAFs. The discussions that follow about these "BAF HQs" seems not to be relevant to the purposes of the ERA, since incremental increases of COC bioavailability in Allen Harbor, above that for reference areas, is neither an assessment endpoint nor is needed for risk-based decisions about site remediation.

Specific Comment #15

Section 6.5, Comparisons of COC Concentrations with Criteria and Standards, does not clearly convey a "big picture" of the overall magnitude and spatial patterns of ecological risks for different COC classes and exposure zones. For the most part, the text merely reiterates the content of data tables, such as the point-by-point locations of COC-specific exceedances of criteria, rather than discussing the average and maximum risks (HQs/HIs) from specific COCs and COC classes, calculated for the salt marsh, intertidal mud flat, and subtidal ecological zones.

Specific Comment #16

Please provide additional ecological rationale for the finely dissected subgroupings of sample locations into discrete ecological exposure subzones within Allen Harbor, as presented in Table 7.3-1 and Figure 7.2-1. Why was a more simplified grouping of data on sediment chemistry, biological effects, and medium-specific chemical risk quotients, into vegetated wetland (salt marsh), intertidal mud flat, and subtidal exposure zones, not used to evaluate the weight of evidence gathered in the study?

Also, the acronyms CP-VW and CP-SW (column headings) and CP-SD (legend) in Table 7.3-1 are not defined in the table and the table and figure should include all of the RAPS sample locations and analytical data for these zones.

Specific Comment #17

Zone-specific, quantitative risk quotients for each COC class (average HIs) should be incorporated into Table 7.3-1 or provided as a separate, similarly structured, synoptic table of results. These combined results should then be used to assess the statistical correlations, if any, among these qualitative and quantitative lines of evidence being used as ecological risk indicators for Allen Harbor, in hopes of clarifying which COCs may be responsible for the observed ecological effects.

Specific Comment #18

Additional evidence is needed to support the conclusion, presented in the second paragraph on Page 7-4, that "the sediments and shellfish tissues at intertidal and subtidal sites adjacent to Calf Pasture Point do not contain concentrations above those expected from regional input sources. In two sections of the executive summary a contrary conclusion was reached that "...significant or potential toxicity..." occurs "...at Calf Pasture Point." How does one reconcile this former interpretation with the latter conclusion and with the observed toxicity of sediments to one or more of the test species at six of the ten locations sampled at Calf Pasture Point? Did the reference sediments exhibit similar levels of toxicity?

Specific Comment #19

The first complete sentence in the first paragraph on Page 7-7 refers to the missing Figure 7.2-3 and indicates that, for "TOC-normalized concentrations" of organics, "only 4 zones contained HQ values exceeding national criteria." Please clarify which national criteria are referenced in this statement, since TOC-normalized organic COC levels should not be used with ER-Ls to calculate HQs.

Specific Comment #20

The meaning of the following should be clarified:

- The statement in the penultimate paragraph on Page 7-10, that the investigation is "entirely synoptic."
- The last sentence of this same paragraph, which also is very confusing (As no prior findings of ecological risk were apparent in the prior study in concurrence with this study,...".)

Specific Comment #21

The reference citation for Eisler (1993) is missing from the bibliography.