Proposed Changes to the Bioaccumulation Testing Evaluation Framework
and
Response to Scientific Peer Reviewers Comments on the Existing Framework for Determining the Suitability of Dredged Material to be Placed at the Historic Area Remediation Site (HARS)

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List of Acronyms

BCF	Bioconcentration Factor
CBR	Critical Body Residue
CCMP	Comprehensive Conservation and Management Plan
CDC	Centers for Disease Control and Prevention
CFR	Code of Federal Regulations
DDE	p,p'-Dichlorodiphenyldicloroethylene
DDT	p,p'-Dichlorodiphenyltrichloroethane
DL	Detection Limit
EAT	Estrogen, Androgen, & Thyroid (hormone system)
ED50	Median Effective Dose
EDSP	Endocrine Disruptor Screening Program
EMAP	Environmental Monitoring and Assessment Program
EPA	Environmental Protection Agency
ER-L/ER-M	Effects Range - Low/ Effects Range - Median
ERED	Environmental Residues Effects Database
FDA	Food and Drug Administration
FR	Federal Register
GC/MS	Gas Chromatography/Mass Spectrometry
GSI	Gonadosomatic Index
HARS	Historic Area Remediation Site
NY/NJ HEP	New York/New Jersey Harbor Estuary Program
IRIS	Integrated Risk Information System
LOEL	Lowest Observed Effects Level
MDL	Method Detection Limit
MDS	Mud Dump Site
NJDEPE	New Jersey Department of Environmental Protection and Energy
NJMSC	New Jersey Marine Sciences Consortium
NMFS	National Marine Fisheries Service
NOAA	National Oceanic and Atmospheric Administration
NPDES	National Pollutant Discharge and Elimination System
NS&T	National Status and Trends
NY/NJ	New York/New Jersey
NYD	New York District
OHEA	Office of Health and Environmental Assessment
ORD	Office of Research and Development
PAH	Polycyclic Aromatic Hydrocarbon
PCB	Polychlorinated Biphenyl
PCDD	Polychlorinated Dibenzo-p-dioxin

List of Acronyms (Contd.)

PCDF	Polychlorinated Dibenzofuran
PPF	Potential Potency Factor
QC	Quality Control
QSAR	Quantitative Structure-Activity Relationship
RAGS	Risk Assessment Guidelines for Superfund
RI/FS	Remedial Investigation/Feasibility Study
RTM	Regional Testing Manual
SEIS	Supplemental Environmental Impact Statement
SMMP	Site Management and Monitoring Plan
TBT	Tributyl Tin
TCDD	Tetrachlorinated dibenzo-p-dioxin
TEF	Toxic Equivalency Factor
TEM	Testing Evaluation Memo
TEQ	Toxic Equivalency Quotient
TeqC	Toxic Equivalence Concentration
U.S.C.	United States Code
USACE	United States Army Corps of Engineers
WES	USACE Waterways Experiment Station
WQCTL	Water Quality Criteria Tissue Level

I. DISCUSSION OF SCIENCE SUPPORTING PROPOSED REVISIONS TO THE EXISTING BIOACCUMULATION TESTING EVALUATION FRAMEWORK

A. INTRODUCTION

1. Historic Area Remediation Site

Under the Marine Protection, Research, and Sanctuaries Act, 33 U.S.C. 1401 *et seq.* (MPRSA) the U. S. Environmental Protection Agency (EPA) and the U.S. Army Corps of Engineers (USACE) share responsibility for MPRSA permitting and HARS designation and management. Pursuant to Section 102 of MPRSA, EPA is assigned permitting authority for non-dredged material. EPA also designates recommended times and sites for ocean disposal (for both non-dredged and dredged material), and develops the environmental criteria used in reviewing permit applications. USACE-NYD determinations to issue MPRSA permits are subject to EPA review and concurrence. Under Section 103 of MPRSA, the USACE is assigned permitting responsibility for dredged material, subject to EPA review and concurrence that the material meets applicable ocean disposal criteria. The USACE is required to use EPA-designated ocean disposal sites to the maximum extent feasible.

EPA de-designated and terminated use of the New York Bight Dredged Material Disposal Site [also known as Mud Dump Site (MDS)], and simultaneously designated the Historic Area Remediation Site (HARS) (see 40 CFR 228.15(a)(d)(6)), in a final rule that became effective on September 29, 1997. Pursuant to the rule, the HARS is restricted to receive only dredged material suitable for use as Material for Remediation (also referred to as Remediation Material). Material for Remediation is defined in the HARS final rule preamble as "uncontaminated dredged material (i.e., dredged material that meets current Category I[1] standards and will not cause significant undesirable effects including through bioaccumulation)."

The need for remediating the HARS is described in detail in the HARS SEIS (EPA, 1997a), associated proposed (EPA, 1997b) and final (EPA, 1997c) rulemaking, and the Response to Comments on the proposed rule (EPA, 1997e). In summary, the proposal to terminate and de-designate the MDS, and simultaneously redesignate the site and surrounding degraded areas as the HARS, is amply supported by the presence of toxic effects in the HARS (a Category III[1] sediment characteristic), dioxin bioaccumulation exceeding Category I[1] levels in worm tissue collected from the HARS (a Category II[1] sediment characteristic), NOAA ER-L/ER-M exceedances in some HARS sediments, and PCB/TCDD contamination in area lobster stocks. Individual elements of the aforementioned data do not prove that sediments within the HARS are imminent hazards to the New York Bight Apex ecosystem, living resources, or human health. However, the collective evidence presents cause for concern, and justifies the finding that a need for remediation exists, that the site is Impact Category I (see, 40 CFR 228.10), and that the site should be managed to reduce impacts to acceptable levels [see, 40 CFR 228.11(c)]. (For more information see the HARS SEIS)

[1]Categories I, II, III were defined for the former MDS. The Category I definition is used as part of the definition of Remediation Material at the HARS. For more information on the Categories, see Appendix C.

2. Scientific Peer Review of Existing HARS Dredged Material Bioaccumulation Testing Evaluation Framework

In the March 1996 New York/New Jersey Harbor Estuary Program (HEP) Comprehensive Conservation and Management Plan (CCMP) (NY/NJ HEP, 1996), EPA Region 2 committed to conduct a public and scientific peer review process of its dredged material bioaccumulation testing evaluation Framework to ensure that sound science is applied in its decision-making process.

In fulfillment of the HEP CCMP commitment, EPA Region 2 sent a letter (January 14, 1998) to the New York/New Jersey Dredged Material Management Forum, inviting interested parties to participate on a Workgroup to develop of a charge for and to review the recommendations/comments from the scientific peer review.

On March 9, 1998, a draft of the charge to the scientific peer reviewers was distributed to all interested parties that responded to the January 14, 1998, invitation letter. The first Workgroup meeting was held on March 27, 1998, and was attended by approximately 50 people, representing broad and diverse interests (see Appendix A for a list of all workgroup members). The main objectives of the meeting were to explain the scientific peer review process, present an overview of the HARS Testing Evaluation Framework, discuss the charge and identify questions for the scientific peer reviewers.

Workgroup members submitted written and verbal comments on the scientific peer review charge. Comments from the workgroup were used to produce a final charge to the scientific peer reviewers. Conflicts among individual comments were discussed in meetings between EPA Region 2 and individual workgroup members groups, such that workgroup consensus was reached on the final charge. The final charge (see Appendix B) was given to the scientific peer reviewers (see Appendix C) on June 23, 1998. It is important to note that the charge was limited to the evaluation/interpretation of 28-day bioaccumulation test results. The charge did not include review of the toxicity and water column test evaluations and these evaluations are not proposed for modification. The scientific peer reviewers were asked to respond within 60 days. The comment period was scheduled to end on August 24, 1998. EPA received review comments from 10 of 12 assigned scientific reviewers by that date. The last review package was received on March 15, 1999. All comments were distributed to Workgroup members.

Based on the review of comments of the peer reviewers, EPA Region 2 is proposing modifications to the bioaccumulation related aspects of the HARS Testing Evaluation Framework which is used and discussed in the joint EPA Region 2/USACE-NYD Testing Evaluation Memorandum (TEM)[2]. A TEM is prepared to document the suitability determination for each dredged material project proposed to be placed as Remediation Material at the HARS.

The following section of this document (Section I. B) summarizes the revisions to the HARS Framework that are being proposed by EPA Region 2 as a result of review of the scientific peer review comments. Section II presents the comments of the scientific peer reviewers and EPA's responses to those comments.

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[2]The joint EPA Region 2/USACE-NYD HARS TEM is the document that discusses the HARS Framework and reviews compliance with EPA's Ocean Dumping Regulations (40CFR 220-229). The TEM documents the suitability determination of individual dredged material projects proposed for placement as Remediation Material at the HARS.

B. INTERIM CHANGE TO THE EXISTING BIOACCUMULATION TESTING EVALUATION FRAMEWORK (REVISION OF THE WORM PCB MATRIX VALUE)

On September 27, 2000, a Memorandum of Agreement (MOA) between EPA, the Department of the Army, and the USACE, was signed. The MOA specifically identified steps that the Agencies would take to ensure that the remedial goals of the HARS continue to be met, while allowing time for full public and peer review of the proposed future changes to the HARS Framework (described in Section C of this document). In the MOA, EPA Region 2 and the USACE-NAD agree to revise the Regional Matrix Value for PCBs in worm from 400 ppb to an interim value of 113 ppb. The clam PCB Matrix Value will remain at the 100 ppb level.

This revision to the current HARS Framework will be used until such time as proposed future changes (described in Section C of this document) have been reviewed by a peer review panel and subjected to public comment. This PCB revision: (1) is in response to the high degree of public controversy over the question of suitability of HARS Remediation Material; (2) reflects EPA Region 2's interpretation and ongoing review of the science associated with responding to the peer review comments; (3) is an appropriate interim step in light of the remedial goals of the HARS, in particular the specific mention of PCBs in the need for remediating the HARS.

As part of the overall effort of reviewing the framework and guidelines, appropriately conservative HARS-Specific Bioaccumulation Decision Values (HARS-Specific Values) for assessing the potential for human health and ecological effects have been derived and are proposed for use in evaluating accumulated concentrations of PCBs. These HARS-Specific Values were derived using the same human health and ecological effects-based procedures that would be used for other constituents. These proposed HARS-Specific PCB Values are scientifically derived/developed levels below which the test concentration does not indicate a potential for significant undesirable effects (human and ecological) (See responses to comments 7 & 8). As described in the MOA, EPA Region 2 will present the proposed PCB HARS-Specific Values and other proposed revisions to the Framework to a peer review panel, in accordance with EPA's Peer Review Policy (EPA, 1998).

Until the public and scientific peer review of proposed changes to the Framework are completed, bioaccumulation test results for individual projects will continue to be evaluated using the current HARS Framework (with the exception that the worm PCB Regional Matrix Values is revised to the lowest HARS-Specific PCB Value of 113 ppb). The current HARS Framework has been modified to reflect the revision of the Regional Matrix Value for PCBs in the worm (Figure 1b). EPA Region 2 is not, at this time, considering any additional changes in the interim, until completion of the peer review panel process described in the September 27, 2000 Memorandum of Agreement.

C. SUMMARY OF PROPOSED CHANGES TO THE EXISTING BIOACCUMULATION TESTING EVALUATION FRAMEWORK

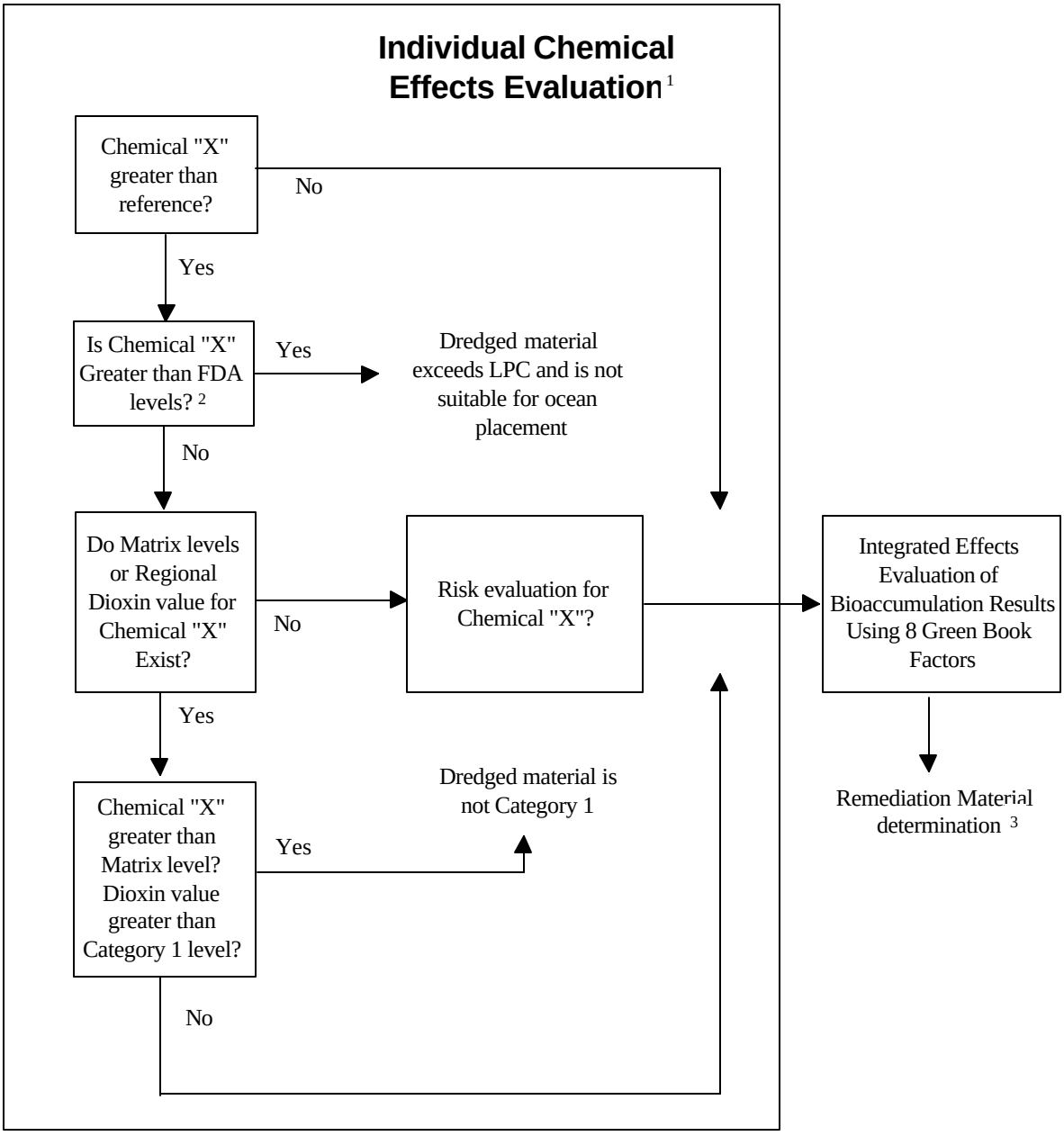
Dredged material proposed for placement at the HARS is rigorously tested for potential water column impacts, solid phase toxicity, and bioaccumulation potential using procedures in the National Testing Manual (Green Book). EPA/USACE are not, at this time, considering any changes to interpretation of water column or toxicity tests, or changes to biological laboratory (bioassay) testing methods. Based on a review of the scientific peer reviewers' comments, however, changes are being proposed to the existing

HARS Framework used to evaluate and interpret bioaccumulation test results (including an interim revision of the Matrix Value for PCBs in worms). Also as described in the September 27, 2000 Memorandum of Agreement, EPA Region 2 plans to present these proposed changes to a peer review panel and public comment, in accordance with EPA's Peer Review Policy (EPA, 1998), prior to their implementation.

Currently, in accordance with EPA's Ocean Dumping Regulation (40CFR 227) bioaccumulation testing data are evaluated in the HARS Framework using a heirarchy of comparisons to results from tests of reference sediment, and to FDA Action Levels, Regional Matrix values, Regional Dioxin values, risk-based guideline values, and are then considered in an integrated effects evaluation using the eight Green Book factors (see Figure 1). This evaluation results in one of two determinations regarding the material: suitable for use as Remediation Material at the HARS; or not suitable for use as Remediation Material. (Prior to the de-designation of the former Mud Dump Site, an evaluation resulted in one of three decisions: unsuitable for ocean disposal [Category III]; suitable with capping [Category II]; or suitable for unrestricted disposal [Category I]. For more information on the MDS Categories, see Appendix C. Since designation of the HARS, only Remediation Material defined as uncontaminated dredged material (i.e., dredged material that meets current Category I standards and will not cause significant undesirable effects including through bioaccumulation) can be placed at the HARS.

EPA Region 2 proposes to revise the existing HARS Framework (Figure 1a) for evaluating bioaccumulation test results of dredged material proposed for use as Remediation Material at the HARS to address comments of the peer reviewers, particularly those regarding the use of the Matrix values, by establishing HARS-Specific Values for all contaminants of concern. These proposed HARS-Specific Values will be scientifically derived/developed levels below which the test concentration does not indicate a potential for significant undesirable effects (human and ecological). Bioaccumulation test results for individual projects will be compared to these proposed HARS-Specific Values to determine whether the proposed project is suitable as Remediation Material. If any chemical in test tissue exceeds any of the proposed HARS-Specific Values, this could result in the tested dredged material being found unsuitable for use as Remediation Material at the HARS.

The revised HARS Framework that is proposed by EPA Region 2 will include four steps for evaluating bioaccumulation test results: (1) statistical comparison of test tissue concentrations to reference tissue concentrations; (2) adjusting bioaccumulation test tissue data to estimate steady state residues; (3) comparing bioaccumulation test tissue results to Regional Dioxin Values and proposed HARS-Specific Values (chemical-specific, effects-based protective benthic tissue levels); and (4) evaluating the bioaccumulation test data for the potential for combined effects of multiple contaminants (i.e., total narcotic potential, total carcinogenicity, and total non-cancer hazards) (see Figure 2). The following sections discuss specific revisions to the HARS Evaluation Framework that are being proposed.

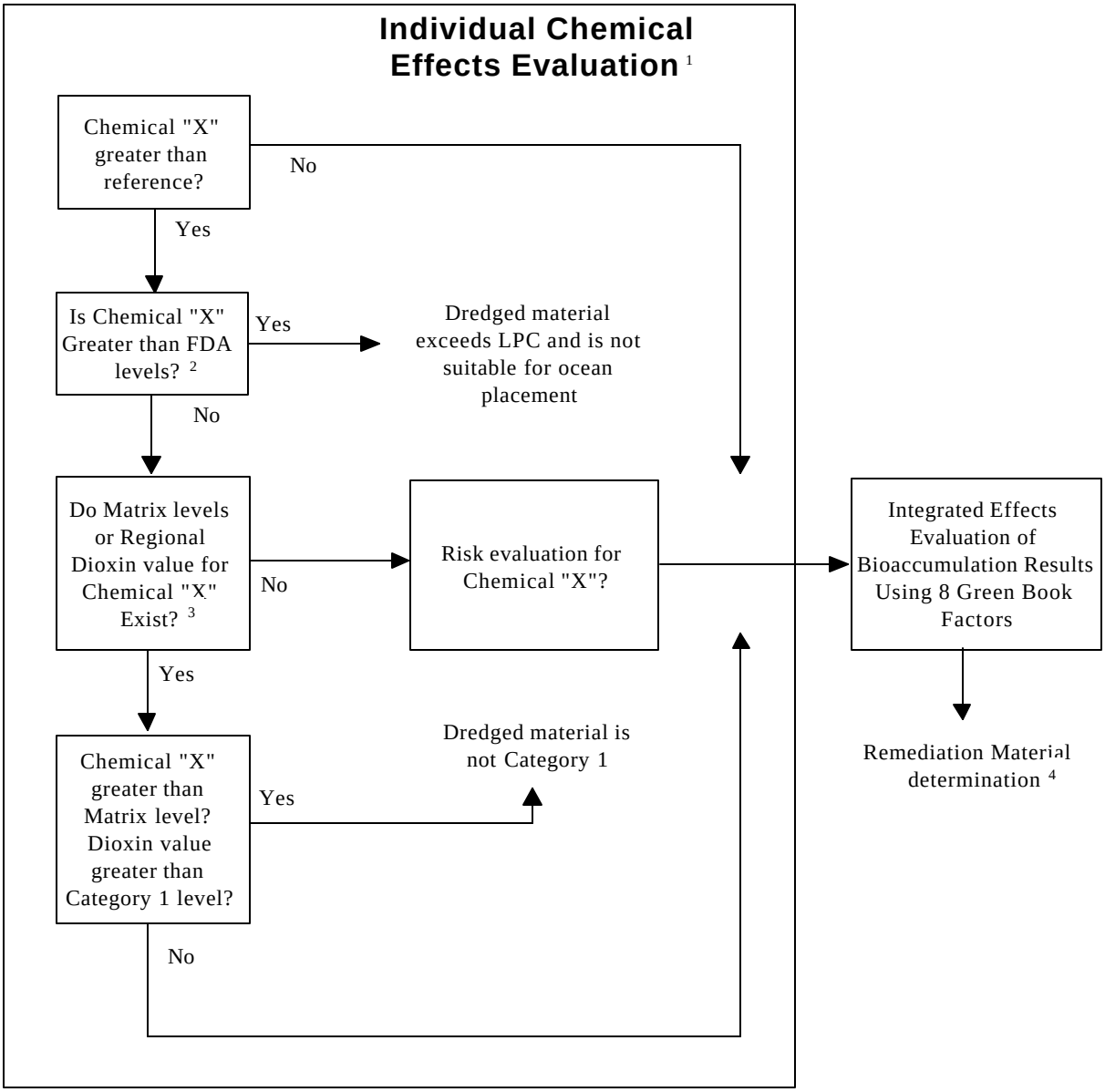


1 Toxicity test results are evaluated prior to the bioaccumulation test results. If proposed dredged material fails the toxicity tests, it is considered not suitable for use as Remediation Material and the bioaccumulation tests are not evaluated.

2 Although not shown, proposed dredged material bioaccumulation test results are adjusted to steady state after comparison with reference.

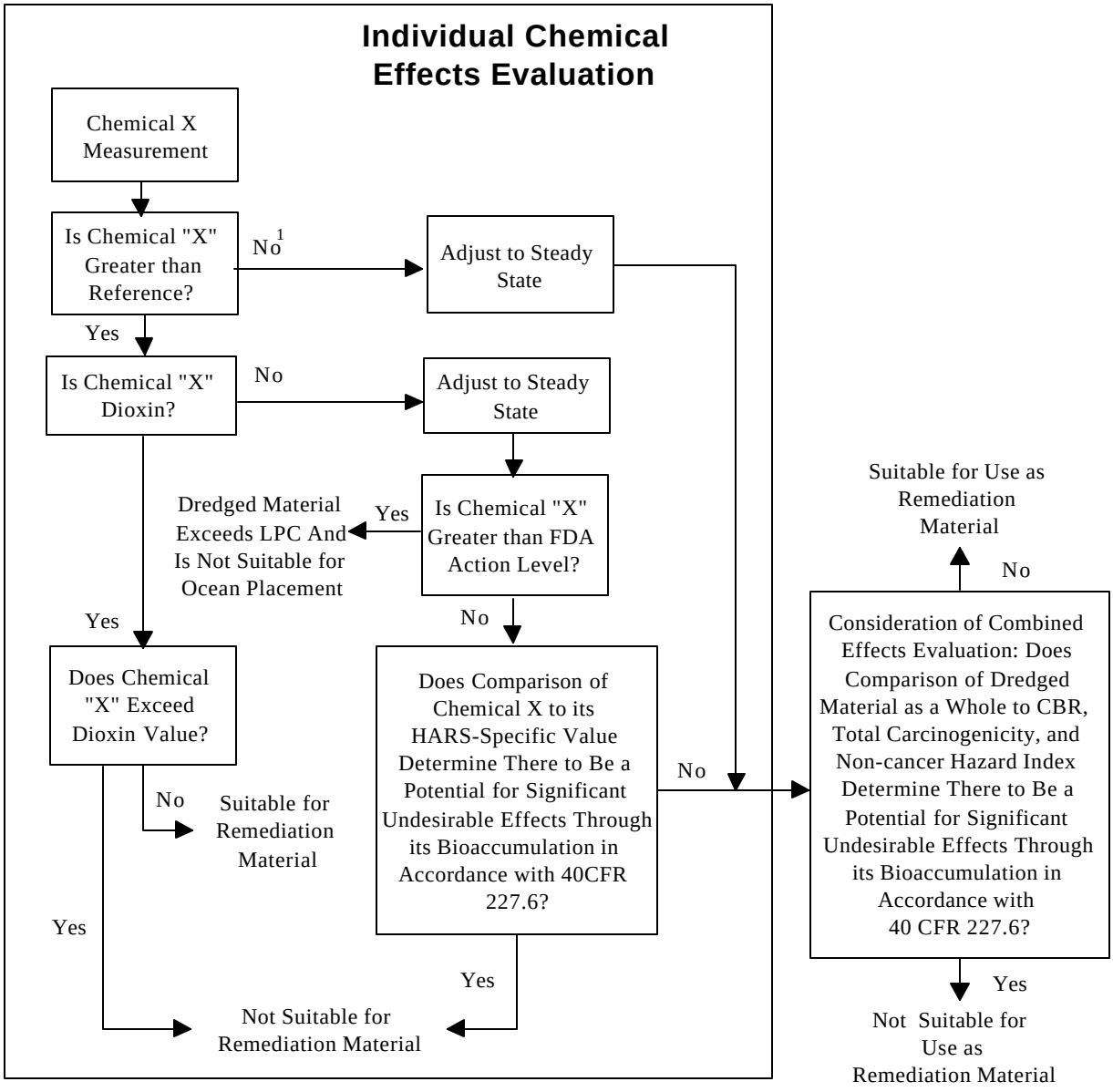
3 *Note:* If any chemical exceeds an FDA Action level, a Matrix level or Dioxin Category I value, proposed dredged material is not Category I.

Figure 1a. Existing EPA Region 2/ USACE-NYD Framework for Evaluating Bioaccumulation Test Results



[1] Toxicity test results are evaluated prior to the bioaccumulation test results. If proposed dredged material fails the toxicity tests, it is considered not suitable for use as Remediation Material and the bioaccumulation tests are not evaluated.
[2] Although not shown, proposed dredged material bioaccumulation test results are adjusted to steady state after comparison with the reference.
[3] A revised, risk-based Regional Matrix Value of 113 ppb for PCBs in the worm has been adopted.
[4] Note: If any chemical exceeds an FDA Action level, a Matrix level or Dioxin Category I value, proposed dredged material is not Category I.

Figure 1b. Interim EPA Region 2/ USACE-NYD Framework for Evaluating Bioaccumulation Test Results



[1] Even if not statistically greater than reference, Chemical X is adjusted to steady state and is considered for its contribution to combined effects.

Comment: Toxicity test results are evaluated prior to the bioaccumulation test results. If proposed dredged material fails the toxicity tests, it is considered not suitable for use as Remediation Material and the bioaccumulation tests are not evaluated.

Figure 2. Proposed EPA Region 2/USACE-NYD Framework for Evaluating Bioaccumulation Test Results

1. Elimination of Comparison to Regional Matrix values

EPA proposes to eliminate the comparison of test tissue contaminant concentrations to Regional Matrix Levels from the HARS Framework. Appropriately conservative HARS-Specific Values will be proposed for use in evaluating the potential for human health and ecological effects associated with accumulated concentrations of all contaminants in dredged material proposed for placement as Remediation Material at the HARS, including those constituents that are currently evaluated using Regional Matrix Values (i.e., PCBs, DDT, cadmium, and, mercury). HARS-Specific Values for these compounds will be derived by EPA Region 2 using the same human health and ecological effects-based procedures that would be used for other constituents. (For more information on the proposed elimination of Regional Matrix Values from the HARS Framework, see response to comment 4 in Section II of this document.)

Appropriately conservative HARS-Specific Bioaccumulation Decision Values (HARS-Specific Values) have been derived for assessing the potential for human health and ecological effects of PCBs (i.e., 282 ppb for human health cancer, 113 ppb for human health non-cancer, and 329 ppb for ecological effects). The lowest of these HARS-Specific Values (i.e., 113 ppb for human health non-cancer effects) will be immediately used, however, to revise the Regional Matrix Value for PCBs in worms until completion of the peer review process as outlined in the September 27, 2000 Memorandum of Agreement.

2. Addition of contaminants of concern

The following constituents are being proposed for additions to the list of analytes for bioaccumulation testing, based on recommendations from the scientific peer reviewers and EPA Region 2's review of relevant regional environmental data (including HARS monitoring data):

- organotins (TBT);
- alkylated PAHs (Table 1)
- co-planar PCBs (PCB-77, PCB-126, and PCB-169)

For more information on the addition of these compounds to the list of required analytes, see response to comment 16 in Section II of this document.

3. Revised analytical method to determine total PAHs

Alkylated PAHs are not currently analyzed in most environmental monitoring and assessment programs and are not currently analyzed in evaluations of dredged materials' suitability for use as Remediation Material at the HARS. This was primarily due to the lack of readily available analytical procedures. EPA Region 2 believes that, for most environmental monitoring and assessment applications, the analysis of the 16 parent PAH compounds is sufficient as it allows relative PAH contamination levels to be determined and compared between samples. However, parent compounds do not generally occur as the sole PAH contaminants in field situations, but rather co-occur with their alkylated homologues. Analysis for only the parent 16 PAH compounds, as is currently done, may significantly underestimate total PAHs and any associated environmental hazard possibly posed. Therefore, EPA Region 2 believes that alkylated PAH compounds should be considered in risk evaluations of PAH mixtures, especially given that a reliable analytical method is available.

EPA Region 2 proposes to require EPA Method 8270 to be performed with specific analytical sample cleanup procedures and analytical instrument configurations which have been optimized to allow detection and quantitation of parent PAHs and their alkylated homologues. These proposed analytical sample cleanup and instrument configuration requirements will be an adaptation of the method (i.e. EPA Method 8270) that is currently approved for analysis of the parent PAH compounds. Using the proposed

analytical sample cleanup procedures and instrument configurations would improve the quantitative analysis of total PAHs using the current method. Table 1 presents the complete list of parent and alkylated PAH compounds that would be analyzed using the proposed procedures. EPA Region 2 proposes to phase these analytical procedures into routine use for testing future dredged material projects and to designate the alkylated PAHs as standard analytes (See Section C for implementation information). (For more information on the proposed procedures for evaluating parent and alkylated PAH compounds, see response to comment 12 in Section II of this document.)

Table 1. PAH and Alkyl PAH Target Compound List.

Decalin	**Anthracene**
C_1-Decalins	C_1-Phenanthrenes/anthracenes
C_2-Decalins	1-Methylphenanthrene
C_3-Decalins	C_2-Phenanthrenes/anthracenes
C_4-Decalins	C_3-Phenanthrenes/anthracenes
Benzothiophene	C_4-Phenanthrenes/anthracenes
C_1-Benzothiophenes	Dibenzothiophene
C_2-Benzothiophenes	C_1-Dibenzothiophenes
C_3-Benzothiophenes	C_2-Dibenzothiophenes
C_4-Benzothiophenes	C_3-Dibenzothiophenes
d_8-Naphthalene[a]	**Fluoranthene**
Naphthalene	**Pyrene**
C_1-Naphthalenes	C_1-Fluoranthene/pyrenes
2-Methylnaphthalene	C_2-Fluoranthene/pyrenes
1-Methylnaphthalene	C_3-Fluoranthene/pyrenes
C_2-Naphthalenes	d_{12}-Chrysene[a]
2,6-Dimethylnaphthalene	**Benz[a]anthracene**
C_3-Naphthalenes	**Chrysene**
2,3,5-Trimethylnaphthalene	C_1-benz[a]anthracenes/chrysenes
C_4-Naphthalenes	C_2-benz[a]anthracenes/chrysenes
d_{10}-Acenaphthene[b]	C_3-benz[a]anthracenes/chrysenes
Acenaphthylene	C_4-benz[a]anthracenes/chrysenes
Acenaphthene	d_{12}-benzo[a]pyrene[b]
Biphenyl	**Benzo[b]fluoranthene**
d_{10}-Fluorene[b]	**Benzo[k]fluoranthene**
Dibenzofuran	Benzo[e]pyrene
Fluorene	**Benzo[a]pyrene**
C_1-fluorenes	Perylene
C_2-fluorenes	**Indeno[1,2,3-*c,d*]pyrene**
C_3-fluorenes	**Dibenz[a,h]anthracene**
d_{10}-Phenanthrene[a]	**Benzo[g,h,i]perylene**
Phenanthrene	

[a]Surrogate Internal Standard.
[b]Recovery Internal Standard.
[c]Compounds in bold are EPA Priority Pollutant PAHs

4. Revised evaluation method for treatment of speciated forms of mercury, chromium, and arsenic.

HARS-Specific Values for metals would be developed using toxicity reference values (e.g., RfDs and Cancer Slope Factors) associated with the most toxic form of the metal (e.g., toxicity reference values

associated with hexavalent chromium and inorganic arsenic) and/or trophic transfer rates associated with the most efficiently transferred form (e.g., methylmercury). Metals residues in test organism tissue, however, are measured as total metal. As such, comparison of total mercury and chromium residues to the proposed HARS-Specific Values would assume that all metals present in test organism tissue is in its most toxic or most efficiently transferred form. This direct comparison of project tissue data for the total metal does not consider the specific valence species or organocomplex state of that metal in the tissue.

To address this potential for overestimating risk, in cases where a project tissue data for total mercury and chromium exceeds the HARS-Specific Value, EPA Region 2 proposes to give the applicant the opportunity to re-analyze the tissues and submit data on the specific valence species or organocomplex state of the metals residues to allow for a more relevant comparison to the Value. This data on the forms of mercury and chromium present will allow for a more relevant comparison to the proposed HARS-Specific Value and minimize the potential for overestimating risk of these metals. If the applicant declines to provide this data, the total metal concentration would be used by EPA Region 2 to assess risk.

Organic forms of arsenic are much less toxic than inorganic arsenic. Nearly all of the arsenic in seafood has been shown to be present in organic forms (primarily arsenobetaine) (Abel and Axiac, 1991; FDA 1993). In light of this fact, FDA (1993) recommends that inorganic arsenic residues in seafood be estimated from total arsenic measurements by applying a 0.1 multiplier to the reported total arsenic concentration. Like all other metals, arsenic residues in bioaccumulation test organism tissue are currently measured and reported as total arsenic. EPA Region 2 proposes to apply the 0.1 multiplier recommended by FDA to total arsenic residue concentrations reported in bioaccumulation test organism tissue to estimate inorganic arsenic residue before comparing to the HARS-Specific Value that was based on the toxicity of inorganic arsenic.

5. Development of Proposed HARS-Specific Bioaccumulation Decision Values

Table 2 shows the HARS-Specific PCB Values that are proposed for use in evaluating the potential for human health and ecological effects in the HARS Framework. EPA will be developing HAR-Specific Values for other compounds which will then be included on Table 2. As shown in Figure 2, HARS-Specific Values are considered in the step called "Comparison of All Non-Dioxin Chemicals to HARS-Specific Values."

Table 2. HARS-Specific PCB Values for Protection of Human Health and Ecological Effects.

Human Health (Cancer)	Human Health (Non-Cancer)	Ecological
282 ppb	113 ppb	329 ppb

The following sections discuss the methods and assumptions EPA used to develop the HARS-Specific Values for PCBs and is proposing to use in the development of HARS-Specific Values for the other contaminants:

A. Consideration of Dioxins:

Regional Dioxin Values were developed and detailed in a policy memorandum dated March 15, 1997 (EPA, 1997a). That memorandum defined the current process for Category I as dredged material in which wet weight test tissue concentrations of 2,3,7,8-TCDD do not exceed 1 part per trillion (pptr), and in which the total toxicity equivalence of all non-2,3,7,8-TCDD dioxin and furan congeners in test tissue do not exceed 4.5 pptr.

In recognition of revisions to the Toxic Equivalency Factors (TEFs) recently published by the World Health Organization (WHO, 1998), (and in response to a peer review comment to consider the

contribution of co-planar PCBs to dioxin-like toxicity; see response to comment 5-5 in Section II of this document), EPA Region 2 proposes to consider the contribution of three co-planar PCB congeners (i.e., PCB congeners 77, 126, and 169) that show dioxin-like toxicological activity. EPA Region 2 proposes to recalculate the Regional Dioxin Value for evaluation of total toxicity equivalence of all non-2,3,7,8-TCDD dioxin and furan congeners to include the three coplanar compounds identified by the WHO (1998), using the same half detection limit procedure used in the current and interim Framework and the TEFs listed by WHO (1998).

EPA is progressing toward completion of its dioxin reassessment report (*Exposure and Human Health Reassessment of 2,3,7,8-tetrachlorodibenzo-p-dioxin [TCDD] and Related Compounds*), which evaluates dioxin across all programs and media. The "Integrated Summary and Risk Characterization" document, which includes a new draft chapter that addresses TEFs for dioxin-like compounds, is currently the subject of an external peer review. A revised draft of the two documents and the revised Dose-Response Modeling chapter are then scheduled to be sent to the Science Advisory Board (SAB) for peer review. The SAB is expected to meet in Fall 2000 with the expectation that the report will be completed by the end of 2000. Upon completion of the EPA reassessment, EPA Region 2 will decide what, if any, changes are necessary to the Regional Dioxin Values.

While the science of the dioxin reassessment is undergoing peer review, and until the evaluation is complete, EPA Region 2 will continue to use the existing Regional Dioxin Values to determine the suitability of dredged material proposed for use at the HARS as Remediation Material. As noted above, EPA Region 2 proposes to consider the contribution of three co-planar PCB congeners (i.e., PCB congeners 77, 126, and 169) that show dioxin-like toxicological activity. EPA Region 2 proposes to recalculate the existing Regional Dioxin Value for evaluation of total toxicity equivalence of all non-2,3,7,8-TCDD dioxin and furan congeners to include the three coplanar compounds identified by the WHO (1998), using the half detection limit procedure and the TEFs listed by WHO (1998). (see response to comment 5 in Section II of this document).

B. Human Health:

For human health, all proposed HARS-Specific Values, for the remaining non-PCB compounds, would be back-calculated using standard risk equations to identify benthic tissue concentrations associated with an acceptable human health cancer risk level of 10^{-4} or a hazard quotient of one (for non-cancer effects). For the purpose of this evaluation, it is assumed that fish consumption is the pathway of concern for humans, and that the fish consumed would be exposed to contaminants in dredged material through trophic transfer of contaminants from benthic invertebrate prey. Table 3 presents a summary of the assumptions that were used in deriving the revised Regional Matrix Value for PCBs and are proposed for use in developing HARS-Specific Values for protection of human health.

With regard to the human health values, the following applies:

- Cancer risk level - An acceptable cancer risk level of 10^{-4} was used to derive the risk-based benthic tissue PCB values for human health cancer effects and would be used to derive the risk-based benthic tissue values for human health cancer effects for the remaining compounds (see response to comment 7-5 in Section II of this document).

- Cancer potency factor - All cancer potency factors used for PCBs and proposed to be used for the remaining compounds were reviewed and revised to reflect the most recent research information. The cancer potency factor for oral exposure was obtained from the EPA

Integrated Risk Information System (IRIS) database (see response to comment 7-5 in Section II of this document).

- Reference dose - All reference dose (RfD) citations used for PCBs and proposed to be used for the remaining compounds were reviewed and revised to reflect the most recent research information. The chronic RfD for oral exposure was obtained from the EPA IRIS database for all contaminants except lead (see response to comment 7-7 in Section II of this document). The RfD for lead was withdrawn from IRIS due to the lack of an established toxicity threshold for neurological effects in children. In the current and proposed evaluation processes, the toxicity assessment for lead has been refined through use of a biomarker (i.e., blood lead concentration) that serves as both a marker of lead exposure and effect.

- Seafood consumption - A seafood consumption rate of 7.2 grams per day (g/day) was used as a site-specific estimate of daily fish consumption by high consumers (i.e., New Jersey recreational anglers) in the vicinity of the HARS (see response to comment 14-1 in Section II of this document).

- Site Use Factor - A factor to conservatively estimate the amount of actual time that food fish forage at the HARS was developed to be used in the calculation of the Values. A factor of 0.777 (i.e., 77.7 percent HARS-area foraging), was developed using data on seasonal occurrence of these species in the target area (see response to comment 15-1 in Section II of this document).

- Trophic transfer factor - Trophic transfer factors were updated to include more recent literature research (see responses to comment 13 in Section II of this document).

- Whole-body to fillet factor - The whole-body to fillet factors were updated for all constituents based on more recent literature (see response to comment 7-8 in Section II of this document).

i. Consideration of Potential Carcinogenic Effects on Human Health:
Chemical-specific tissue values for PCBS were back-calculated using a standard risk algorithm that combines each individual cancer potency factor (CPF) with the risk assessment assumptions (see Figure 3). The same methodology is proposed to be applied to the remaining carcinogenic compounds. The calculations yield concentrations representing levels below which the residues do not indicate a potential for cancer risk at the defined acceptable cancer risk level (i.e., 10^{-4}) (see response to comment 7-5 in Section II of this document).

ii. Consideration of Potential Non-carcinogenic Effects on Human Health:
Chemical-specific tissue values estimating the potential for non-carcinogenic effects for PCBs were back-calculated using a standard risk assessment algorithm that combines each individual Reference dose (RfD) with the risk assessment assumptions (see Figure 4). The same methodology is proposed to be applied to the remaining non-carcinogenic compounds, except for lead. For lead, the potential for non-carcinogenic effects associated with the test tissue was estimated using a "disaggregate" modeling approach, (which relates multi-media lead exposure to blood lead concentration) employed in the 1986 USEPA Air Quality Document (see Figure 5). The calculations yield lead concentrations representing levels below which the concentrations do not indicate a potential for significant undesirable effects (i.e., 95% of the blood lead probability distribution below 10 ug/dl). The "disaggregate" model is conceptually similar to EPA's Integrated Exposure Uptake-Biokinetic Model for Lead (IEUBK Pb Model) in that it estimates the impact of multi-media lead exposure on blood

lead concentration, and employs the same benchmark of protectiveness (i.e., 95% of blood lead in children below 10 ug/dl). Limiting lead exposure to meet the goal of 95% of the childhood population distribution below 10 ug/dl is consistent with both current CDC's recommendations for pediatric lead exposure and EPA's Children's Health Initiative - which directs the Agency evaluate children as a potential sensitive subpopulation due to either increased exposure and/or toxic effect.

C. Consideration of Potential Ecological Effects:

Contaminants may exert their toxic effects on marine organisms through mechanisms that are unique to individual contaminants (termed specific modes of action) or by mechanisms which are shared between compounds (termed common modes of action). Appropriate tissue concentrations of specific chemicals, or of chemicals that share common modes of action, for use in evaluating the potential for ecological effects due to bioaccumulation from proposed Remediation Material for PCBS were derived through a comprehensive assessment and review of published scientific literature linking tissue residues and effects observations. The same methodology is proposed to be applied to the remaining compounds of concern.

The following two databases of environmental effects research were the primary tools used in this effort: the EPA's database entitled *"Linkage of Effects to Tissue Residues: Development of a Comprehensive Database for Aquatic Organisms Exposed to Inorganic and Organic Chemicals*"; and the USACE's *"Environmental Residues Effects Database"* (ERED) (see response to comment 8-1 in Section II of this document and Appendix D). Information from these databases and other studies was screened to identify appropriate observed effect levels in order to derive protective benthic tissue concentrations. Specific criteria were used to screen the data, such that:

- Only the most robust/relevant of the data were selected;
- Data were selected that identified the effective (or non-effective) concentration resulting from exposure conditions that most closely resembled conditions at the HARS;
- Only whole-body data were used;
- ER50 data (tissue residues resulting in a 50 percent effect in an individual or causing an effect in 50 percent of exposed organisms) were given preference;
- Only endpoints of maximum ecological relevance (i.e. reduced survival, growth, or reproduction) were considered.

Because non-polar organic contaminants (including PAHs, chlorinated pesticides, and PCBs) share a common mode of action (i.e. narcosis), EPA proposes to consider the total molar concentration of all of these contaminants to calculate the total narcotic residue accumulated by test organisms following exposure to dredged material proposed for use as Remediation Material. The HARS-Specific Value for narcosis would be developed to protect for sublethal and chronic narcotic effects and is further discussed below. It is important to emphasize that the specific mechanisms of toxic action of individual organic contaminants would also continue to be considered, in addition to narcosis, in the evaluation of dredged material proposed for use as Remediation Material by comparison with the HARS-Specific Values. (For more information, see response to comment 8 in Section II of this document.)

7. Combined Effects Evaluations: Narcotic CBR, Total Carcinogenicity, and Non-Cancer Hazard Index

Under the proposed HARS Framework (as discussed above), bioaccumulation test results for individual constituents would be compared to proposed HARS-Specific Values to determine whether the tested material is suitable for use as Remediation Material. If dioxin/furan compounds in the tested dredged material are below the Regional Dioxin Values, an evaluation of the potential for combined effects of multiple constituents (Combined Effects Evaluation) in test organism tissues would be conducted in addition to comparing all non-dioxin chemicals to their respective HARS-Specific Values. Three methods that have been developed (identified below) to assess the potential for combined effects of multiple constituents are proposed for use in evaluating dredged material proposed for use as Remediation Material: a revised application of the narcotic critical body residue (CBR) approach, a total carcinogenicity assessment, and a total noncancer effects hazard assessment.

Table 3. Assumptions Used to Develop Human Health HARS-Specific Values

Compound	Cancer Potency Factor	Reference Dose	Trophic Transfer	Whole body: Filet	Seafood Consumption (g/day)	Fish Foraging Factor
PAHS						
Acenaphthene	0	60	0.1	1.35	7.2	77.7%
Anthracene		300	0.1	1.35	7.2	77.7%
Fluorene		40	0.1	1.35	7.2	77.7%
Naphthalene		20	0.1	1.35	7.2	77.7%
Phenanthrene		300	0.1	1.35	7.2	77.7%
Benzo(a)pyrene	7		0.1	1.35	7.2	77.7%
Fluoranthene		40	0.1	1.35	7.2	77.7%
Pyrene		30	0.1	1.35	7.2	77.7%
TOTAL PAHS						
PESTICIDES						
Aldrin	17	0.03	3	1.35	7.2	77.7%
Dieldrin	16	0.05	1.6	1.35	7.2	77.7%
"Chlordane	0.35	0.05	2.9	1.35	7.2	77.7%
Heptachlor	4.5	0.5	2.9	1.35	7.2	77.7%
Heptachlor epoxide	9.1	0.013	1.4	1.35	7.2	77.7%
Total Residual Chlordane/Heptachlor				1.35	7.2	77.7%
Total Endosulfans		6	1.1	1.35	7.2	77.7%
Total DDT	0.34	0.5	3	1.35	7.2	77.7%
TOTAL PCBs	2	0.02	3	1.35	7.2	77.7%
1,4-Dichlorobenzene	0.02	30	1	1.35	7.2	77.7%
METALS						
Arsenic	1.5	0.3	0.25	1.4	7.2	77.7%
Cadmium		1	0.25	5.9	7.2	77.7%
Chromium (total)		3	1	1.2	7.2	77.7%
Copper		37.1	0.21	2.9	7.2	77.7%
Lead		1	0.23	3.9	7.2	77.7%
Mercury		0.1	1.95	0.7	7.2	77.7%
Nickel		20	1	8.3	7.2	77.7%
Silver		5	1	1	7.2	77.7%
Zinc		300	0.24	2	7.2	77.7%
Tributyltin	NA	0.3	1.02	1.35	7.2	77.7%

Figure 3. HARS-Specific Value Calculations for Protection of Human Health from Cancer Risk

Calculation of HARS-Specific Value

Equation 1

$$\text{Toxicological Dose (µg/day)} = \frac{[\text{Risk Level } (10^{-4})] \times [\text{Body Weight (70 kg)}] \times [10^{3} \text{ µg/mg}]}{[\text{Cancer Potency Factor (kg-day/mg)}]}$$

Equation 2

$$10^{-4} \text{ Conc. in Fish (µg/kg)} = \frac{[\text{Toxicological Dose (µg/day)}]}{[\text{Seafood Cons. (7.2 g/day)}] \times [10^{-3} \text{kg/g}] \times [\text{Site Use Factor (0.777)}]}$$

Equation 3

$$\text{HARS-Specific Value (µg/kg)} = \frac{[10^{-4} \text{ Conc. in Fish}] \times [\text{Whole Body/Fillet Factor}]}{[\text{Trophic Transfer Factor}]}$$

Calculation of Constituent-specific Risk for Total Carcinogenicity

Equation 1

$$\text{Estimated Conc. in Fish (µg/kg)} = \frac{[\text{Measured Tissue Level (µg/kg)}] \times [\text{Trophic Transfer Factor}]}{[\text{Whole Body/Fillet Factor}]}$$

Equation 2

$$\text{Toxicological Dose (µg/day)} = \frac{[\text{Estimated Conc. in Fish (µg/kg)}] \times [\text{Seafood Cons.(7.2 g/day)}]}{[10^{3} \text{ g/kg}] \times [\text{Site Use Factor (0.777)}]}$$

Equation 3

$$\text{Estimated Cancer Risk (unitless)} = \frac{[\text{Toxicological Dose (µg/day)}] \times [\text{Cancer Potency Factor (kg-day/mg)}]}{[\text{Body Weight (70 kg)}^{fn3}] \times [10^{3} \text{ µg/mg}]}$$

Figure 4. HARS-Specific Value Calculations for Protection of Human Health from Non-Cancer Effects

Calculation of HARS-Specific Value

Equation 1

Toxicological Dose (µg/day) = [Reference dose (mg/kg-day)] x [Body Weight (70 kg)] x [10^3 µg/mg]

Equation 2

$$\text{Conc. in Fish (µg/kg)} = \frac{\text{[Toxicological Dose (µg/day)]}}{\text{[Seafood Cons. (7.2 g/day)] x [}10^{-3}\text{kg/g] x [Site Use Factor (0.777)]}}$$

Equation 3

$$\text{HARS-Specific Value (µg/kg)} = \frac{\text{[Conc. in Fish (µg/kg)] x [Whole Body/fillet Factor]}}{\text{[Trophic Transfer Factor]}}$$

Calculation of Constituent-specific Risk for Non-cancer Effects

Equation 1

$$\text{Conc. in Fish (µg/kg)} = \frac{\text{[Measured Tissue Level (µg/kg)] x [Trophic Transfer Factor] x [Forage Factor (0.777)]}}{\text{[Whole Body/Fillet Factor]}}$$

Equation 2

Toxicological Dose (µg/day) = [Conc. in Fish (µg/kg)] x [Seafood Cons. (7.2 g/day)] x [10^{-3}kg/g]

Equation 3

$$\text{Hazard Quotient (unitless)} = \frac{\text{[Toxicological Dose (µg/day)] / [Body Weight (70 kg)]}}{\text{[Reference Dose (mg/kg-day)] x [}10^3\text{ µg/mg]}}$$

Figure 5.HARS-Specific Value Calculations for Non-Cancer Effects from Lead

1 - Calculate current human exposure from all routes		
A - Average current exposures		
Drinking water	4	ppb
Soil/dust/paint	800	ppm
Air	0.1	ug/cuM
Dietary	5.5	ug/da
B - Media-specific blood lead coefficients		
Drinking water	0.16	ug/dl per ppb
Soil/dust/paint	0.002	ug/dl per ppm
Air	2	ug/dl per ug/cuM
Dietary	0.16	ug/dl per ug/da
Calculate Blood lead contributions [A x B]		
Drinking water	0.64	ug/dl
Soil/dust/paint	1.6	ug/dl
Air	0.2	ug/dl
Dietary	0.9	ug/dl
C - Sum of blood lead contributions to determine all-route current exposure [Sum(A x B)]		
Current exposure	3.3	ug/dl
2 - Calculate acceptable lead contribution from fish spending time at MDS [Level of concern - current exposure]		
Level of concern	4.6	ug/dl
Current exposure	3.3	ug/dl
Acceptable HARS fish contribution	1.3	ug/dl
3 - Convert acceptable fish contribution to lead fish tissue concentration		
A - Calculate acceptable daily intake [Acceptable fish contribution / food lead coefficient]		
Acceptable fish contribution	1.3	ug/dl
Food lead coefficient	0.16	ug/dl per da
Acceptable daily intake	8.125	ug/da
B - Calculate acceptable tissue concentration in fish [Acceptable intake/Average fish consumption]		
Average fish consumption	7.2	g/da
Acceptable tissue conc	1.1285	ug/g
C - Calculate acceptable benthic tissue concentration from acceptable concentration in fish		
Whole-body : fillet	3.9	
After adjustment for fillet	4.401	ug/g
Site use percent	77.7	percent
After adjustment for site use	5.6641	ug/g
Trophic transfer factor	0.23	

After adjustment for trophic transfer	24.627	ug/g
	or	
HARS-SPECIFIC VALUE	24.6	ppm

A. Narcotic CBR:
EPA Region 2 proposes to extend application of the narcotic CBR approach to consider the contributions of all organic constituents, including alkylated (and parent) PAHs, PCBs, and pesticides. If the total molar concentration of these compounds is below the narcotic CBR threshold, they do not indicate a potential for narcotic effects and, therefore, the material would be suitable for use as Remediation Material. If the total molar concentration of these compounds is above the narcotic CBR threshold, this indicates that there is a potential for narcotic effects and, therefore, the material may not be suitable for use as Remediation Material. (For more information, see response to comment 8 in Section II of this document).

B. Total Carcinogenicity:
EPA Region 2 proposes to assess the total carcinogenicity of the mixture of carcinogenic compounds that are accumulated by test organisms by summing the individual cancer risks associated with accumulated concentrations of each carcinogenic constituent and comparing that sum to the target risk level of 10^{-4}. Individual cancer risks are derived using standard risk assessment equations as described in Figure 3. If the total cancer risk of the accumulated mixture of carcinogenic compounds is below the acceptable cancer risk level (i.e., 10^{-4}), the material would be suitable for use as Remediation Material. If the total summed cancer risk is greater than the acceptable cancer risk level, the material may not be suitable for use as Remediation Material. (For additional information, see response to comment 17-3 in Section II of this document.)

C. Non-Cancer Hazard Index:
EPA Region proposes to assess the potential for non-carcinogenic effects associated with the mixtures of contaminants accumulated by test organisms by using a combined hazard index approach. In the combined approach, individual hazard quotients would be derived for each chemical detected in the test tissue using standard risk equations as described in Figure 4. Hazard quotients for chemicals that share a similar mode of action or target organ will be summed to derive a total hazard index for toxicologic mechanism or impact to specific organ systems (i.e., circulatory system, liver, central nervous system, and kidney). If the sums of the hazard indices associated with specific organ systems are below 1, they do not indicate a potential for significant undesirable effects and, therefore, the material would be suitable for use as Remediation Material. If any of the total hazard indices associated with specific organ systems are greater than 1, the material may not be suitable for use as Remediation Material. (For additional information see response to comment 17-3 in Section II of this document.)

8. Incorporation of Eight Green Book Factors in the Development of the HARS-Specific Values
As discussed above, EPA proposes to develop HARS-Specific Values and a "Consideration of Combined Effects Evaluation: CBR, Total Carcinogenicity, and Non-Cancer Hazard Index" to determine the suitability of dredged material to be used as Remediation Material at the HARS. These proposed HARS-Specific Values would be derived to be protective and appropriately conservative to achieve remediation of the HARS. The proposed HARS-Specific Values and the overall Framework incorporate the eight factors recommended in the Green Book for consideration in determining compliance with the solid phase Limiting Permissible Concentration (LPC). The holistic evaluation of the test tissue concentrations, as described in response to comment 17, will add to the protective design of the proposed HARS-Specific Values and the remedial basis of the HARS designation by evaluating for combined effects of contaminants. The evaluation of combined effects of contaminants in dredged material is proposed to be accomplished by more completely and rigorously assessing those contaminants that act similarly through

application of the proposed narcotic CBR, total carcinogenicity, and total non-cancer Hazard Index approaches (see responses to comments 7, 8 and 11 in Section II of this document).

As such, it is proposed that the step entitled "Evaluation of Solid Phase Bioaccumulation Results for Dredged Material as a Whole" in the existing Framework be revised to reflect the new proposed evaluation process by eliminating the specific evaluation of the eight factors for LPC compliance listed in the Green Book from the Framework as a separate evaluation step. EPA Region 2 believes the eight factors have been incorporated to the maximum extent practicable, and as appropriate, into the development of the proposed HARS-Specific Values. Therefore, the consideration of contaminants in dredged material as a whole can best be accomplished through use of the revised evaluations described above.

II. RESPONSE TO SCIENTIFIC PEER REVIEW COMMENTS

Charge Group: Framework

Charge No. 1. Is the EPA Region2/CENAN Framework for evaluating bioaccumulation results scientifically appropriate for determining the suitability of dredged material as Remediation Material? If not, describe deficiencies. (Please see Region2/CENAN joint evaluation memorandum, Figure 1)

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 1 Comment (5 para):
The basic design of the Framework is scientifically appropriate. The list of chemicals addressed in Table 1 includes the important chemicals of concern for NY/NJ Harbor Projects. Arguably, one or two chemicals might be added or deleted from the list, but the list is comprehensive in its inclusion of chemicals from a variety of classes that are locally relevant. The use of the 28-d bioaccumulation test is an appropriate methodological foundation for the Framework. This test is widely used in sediment risk assessments. It has been thoroughly peer-reviewed and established as a standard method. The required testing of two species, the clam Macoma nasuta and the polychaete Nereis virens, allows assessment of interspecific differences in bioaccumulation to be assessed. The basic comparative strategy in which the tissue concentrations observed during the 28-d test on project material are compared against established benchmarks to determine if the project material is acceptable as Remediation Material is fundamentally sound. However, the scientific defensibility and efficacy of some of the comparative benchmarks are uncertain, as explained below.

The first comparative benchmark in the Framework is tissue concentration observed during 28-d tests with material collected from an established reference site. If the tissue concentration observed in the 28-d test with the project material is less than the reference, the material is deemed acceptable for remediation with respect to a particular chemical. If it is greater than the reference, further evaluation is required. This is an appropriate first level determination.

The second level of comparison involves FDA Action Levels and Regional Matrix/Dioxin values. There are several problems with each of these benchmark comparisons. First, FDA or Matrix or Dioxin values do not exist for 53 of the 65 chemical parameters of concern listed in Table 1. Second, the seven FDA Action Levels seem extraordinarily high relative to other benchmarks (see answers to questions 6A and 6B below). Third, the four Regional Matrix values are based on an eighteen-year-old technical derivation which is of questionable validity. For example, the Hg Matrix value of 0.2 mg/kg is simply the mean of 16 tissue concentrations in specimens collected somewhere in the New York Bight (including "in and around the dump site") represented in 4 species reported in 6 papers published between 1972 and 1980. There was no standardization of survey or analytical methods among these six investigations. Only two of the six papers were peer-reviewed journal articles. Neither of the two standard 28-d bioaccumulation test species is included among the four species on which the Hg Matrix Value is based. The Hg Matrix value of 0.2 mg/kg is clearly not an effects based benchmark. It is 5x to 6x greater than the background

concentration of Hg in clams and polychaetes near the dump site (Table 1, Column 16-17). In my judgment, there is no scientific validity to the use of this Matrix Value for comparison with the results of 28-d tests with Macoma and Nereis. Similar criticisms could be made of the other Matrix Values. The final problem with the Matrix Values is the decision logic evident in Figure 1 of the Framework. If the tissue concentration observed in 28-d tests with project material is less than the Matrix Value, Figure 1 indicates that no further risk evaluation is needed for that chemical. Given the scientific uncertainty about the validity of the Matrix Values, they should not preclude further risk evaluation using other benchmarks.

The third level of comparison is the "Risk Evaluation" as identified in Fig. 1. The Risk Evaluation includes a number of comparative benchmarks. The benchmark that is consistently and substantially lower than other Risk Evaluation benchmarks is the background tissue concentration for both the clam and worm (Table 1, columns 16-17). I describe below in my answer to Question 19 why the background values should not be compared with 28-d test tissue concentrations. Briefly, they are based on resident species that show order of magnitude interspecific variation in tissue concentrations at the same site. Further, the resident species on which the background values are based do not include the standard 28-d test species, so the uncertainty of interspecific extrapolation precludes valid comparison. Fortunately, there is a relatively inexpensive, and scientifically defensible way to establish appropriate background values. The 28-d test with both species should simply be applied to ~ 10 representative sediment samples collected in the background of the dump site (i.e. near but not immediately adjacent to the dump site boundary). This will establish relevant background tissue concentrations that can be unambiguously compared with results of tests with project material. No interspecific comparisons would be required. No laboratory test vs field collection comparisons would be required. No steady state adjustments would be necessary.

How would the results of comparison to 28-d test background levels be used? First, it is important to realize that it is not possible on the basis of existing knowledge to draw "bright lines" that discriminate levels of adverse ecological effects on the basis of bioaccumulation data for most, if not all, of the 65 chemical parameters of Table 1. Arguably, critical body residues can be estimated for a few chemicals, but there is great uncertainty about these estimates. Effects assessments can be based on toxicity and faunal surveys. Bioaccumulation data can be used to ensure that the HARS remediation actually reduces the bioaccumulation of toxic materials from contaminated sediments. Three benchmarks are needed, all based on 28-d tests with both species: (1) the reference benchmark, as currently incorporated into the Framework, (2) the background benchmark, as described above, and (3) the HARS benchmark, established on the basis of 28-d tests with ~ 10 representative sediment samples collected within the dump site itself. The intention of the remediation will be achieved with respect to bioaccumulation if EPA/COE establish, as a matter of policy, that a project material can be designated as Remediation Material only if the tissue concentration of every chemical listed in Table 1 as determined in 28-d tests with two species is less than a concentration equal to background plus 25% of the difference between the background and HARS benchmarks. Thus, if the background value for chemical x is 8 mg/kg and the HARS value is 48 mg/kg, the critical value is $8 + 0.25(48-8) = 18$ mg/kg. This rationale is subject to the criticism that it is not effects-based, but I submit that an effects-based benchmark for all 65 chemical parameters of Table 1 is impossible. The advantage of this method is that it is understandable and technically defensible from the perspective that it will unequivocally reduce bioaccumulation of toxic materials. The strategy can be coupled with annual monitoring of effects parameters at the reference, background, and HARS sites. If bioaccumulation, toxicity, and biological community effects do not decline over time, the EPA/COE can reduce the critical value to the actual background value or even the reference value.

Clifford Charge No. 1 Comment (4 para):
This framework is one of the more thorough attempts to directly evaluate body burden analytical data that I have seen. As such, I believe that, if direct interpretation of such data is the assessment method of choice, it can be accepted with few reservations. Scrutiny of the data set used suggests to me that the hypothetical sediment presented would indeed be appropriately identified as category I and, as such, the screening methodology seems to have worked. However, I have some discomfort regarding the inherently large uncertainties surrounding direct toxicological interpretation of body burden data (discussed in more detail in specific responses below). As there are several ways in which body burden data can be used in ecological risk assessment I recommend consideration of some of the alternatives.

One of the principal alternatives is to use the data as input to a dose-based food-web model rather than attempting to toxicologically evaluate the data themselves. Given that concentrations of contaminants in sediment, water, and lower trophic level organism tissues are either known or can be easily predicted using conventional partitioning methodologies, it is a relatively straight forward task to estimate the doses of contaminants that receptors virtually anywhere in the food-web are exposed to. For example, using widely accepted models like those of Gobas, Thomann, Connelly, Parkerton, *et al.*, the existing data are sufficient to predict concentrations of contaminants in fish and risk to piscivorous birds (e.g., pelican) could be estimated by comparing calculated dietary intake to good benchmark studies. Risks at other trophic levels can be evaluated in a similar manner. I also believe that evaluation of such upper trophic-level receptors would be more appropriate than using humans as terminal receptors for ecological assessment. Methods and modeling parameters for estimating dietary intake to ecological receptors are presented in USEPA (1993) and elsewhere and there are numerous peer-reviewed articles that present good dose-based toxicological benchmark data for comparison. I believe that conduct of such an analysis could be either a very robust compliment to the existing framework or could be used to replace portions of it.

Whether dose-based evaluations are considered or not, one of the principal things I recommend adding to the framework and draft memorandum for the record is a specific section presenting a thorough treatment of uncertainties. While it is clearly not possible to quantitatively address all of the uncertainties associated with the comparisons presented, some qualitative treatment should be incorporated in the document for the information of the decision makers. For example, regional matrix values were derived by calculating tissue concentrations from grand means of concentration data and a conservative (lowest) BCF. While I do not argue here with the validity of that approach, uncertainties associated with the derivation method are critical to understanding the meaning of exceeding such a value. In this case, treatment of concentration data is non-conservative because a mean has been used but the overall calculation is conservative because of the BCF selected. The point in this particular case is that populations of organisms should be protected (use of the grand mean) but not all individuals of those populations will necessarily be protected. It is very important to understand these kinds of issues when interpreting the meaning of the comparisons presented.

Brief discussions of the uncertainties (and degrees of conservatism) associated with the various comparison criteria could be presented prior to section VI and used as an integral part of drawing the overall conclusions. This would be more in keeping with the "weight of evidence" (or lines of evidence) approach generally preferred for ecological risk assessment. I do not believe that there is any need to

waste time trying to educate readers of the document regarding the inherent uncertainties associated with things like selection of "=0.05 for statistical testing as that sort of information is widely available in the general literature, but a more thorough qualitative treatment of other uncertainties could improve overall confidence in the conclusions reached.

Gentile Charge No. 1 Comment (1 para):
Fundamentally the framework outline in the CENAN joint evaluation memo and illustrated in Figure 1 contains the necessary elements to make an evaluation of the potential health and ecological effects of contaminant bioaccumulation. The only apparent deficiency is not with the framework *per se* but with the lack of estimates of variability that support the statistical analysis for determining when there is significance exceedence of a reference value or the various benchmarks that are used. I am not familiar with how the many assumptions that are implicit in the Framework were arrived at but I will assume that they all have been peer-review for their statistical soundness. I do like the additional 8-Factors but would have to see a case where they played a dominant role before I would comment on the limits of their applicability.

Berry/Lake/Pruell Charge No. 1 Comment (1 para):
The framework does seem to be scientifically appropriate. For the most part it does seems to represent the "state of the art." Comments on some of the individual components of the framework may be found below.

Wentsel Charge 1 Comment (1 para, 6 bullets):
The Framework appears appropriate for screening dredged material to identify Category 1 sediments. However, more information is required to determine if the method is appropriate to identify Remediation Material.

- Information on the experimental design for tests, number of replicates, statistical tests, and QC, are needed.
- Are detection limits low enough to detect ecologically significant levels.
- Reference sediment is described as clean sandy sediment. "Background" sediment was collected near the HARS and tested. The characteristics of the reference sediment should be similar to the sediment being tested. Data on organic carbon, particle size, etc. should be collected and compared for the sediments. It seemed in the report that "background" sediment was being used as the reference site, if that's the case then it should be stated.
- Up front data should be used to further screen the sediments. Risk assessments for each chemical could identify concentration above which would be ecologically harmful, a concentration where no ecological harm is likely, and a range in between where further testing (i.e. bioaccumulation) is required.
- Metals (other than MeHg) should not be treated the same as the organic compounds with Kow values > 3.5. Some of the metals are micronutrients and they do not biomagnify in the food chain like the high Kow compounds. Assessments could be done to determine if direct toxicity values for metals would be protective of aquatic life, then the 28 day tests would not be needed.
- The use of bioaccumulation tests may be too variable. Use of a negative or positive control may be necessary.

Solomon/Sibley Charge No. 1 Comment (1 para):
The framework for evaluating the results of the bioaccumulation results (as described on pages 6-8 in the memorandum) is a reasonable approach to hazard assessment and is valid. The only potential problem that could result from the decision tree approach is when the concentrations in the reference sediment are high enough that criteria, such as the FDA levels, were exceeded. This would be the case if an inappropriate reference sediment were used. Inspection of the data in Table 1 revealed that this was not the case. In addition, the reference sediment was collected from an appropriate location.

Newman Charge No. 1 Comment (14 para):
General Answer
The framework (Figure 1 and associated text) seems generally acceptable and thoughtful. It is surprising to not see at the top of the flow diagram a statistical comparison of bioaccumulation in key endemic species inhabiting sediments of the dredge material source site versus reference sites. This seems so much more straightforward than using the 28 day bioaccumulation test and associated gross adjustments to estimate "steady state" concentrations. It also is more straightforward than comparison of the bioaccumulation test results to bioaccumulation of biota from sites near the HARS (step c, see page 14, section (ii)).

Several resolvable points limit the utility of the present framework. These points will be discussed later in this review. They include the following:

5. Step a (Chemical "X" Greater than Reference?): There are unanswered questions regarding sampling techniques (gut clearance, size/age normalization or control), specific hypothesis tests applied (page 8, paragraph 1), and ambiguity regarding test design and power estimation. These crucial aspects are generally discussed and highlighted in the Guidance Manual (Ref. 32) but specific and important details are not mentioned in the Memo of Record (Peer Review Version). Without specific details, it would be impossible to assess the true value of the information in the Memo of Record.

   Also methods associated with determining average concentration if the data set contains below detection limit observations are not specified but can greatly influence the validity of hypothesis tests. I assume that the methods described in reference 13 were used. The methods described in reference 13, "New Approach to Handling "non-detects" in Elutriate and Tissue Data" are invalid. A general statement is made that the approach was "conservative" without further explanation. The tabulated results and footnotes seem to imply that sites with all "<DL" observations or some "<DL" observations were treated differently. How were they treated? Were they treated as suggested in Ref. 13?

   Those with all "<DL" observations for the reference site and detectable amounts for the sample of interest could be analyzed by testing whether the mean for the site of interest was significantly different from the detection limit, e.g., a simple z statistic would do this (below) or simple resampling methods.

$$z = \frac{\bar{x} - DL}{\sigma/\sqrt{(n)}}$$

If some, but not all, observations were "<DL" then other tests are appropriate. Gilbert (1993, 1995) provides details on hypothesis tests for data sets with"<DL" observations including slippage, quantile, Wilcoxon rank sum, and Gehan tests. Regardless, it is important to understand that performing hypothesis tests after substituting 0, ½ DL or the DL for the "<DL" observations produces invalid results that are not necessarily "conservative." (Reference 13 states that ½ DL or 0 should be used depending on the situation. This will produce statistically misleading results. See Newman (1995) or references therein for correct methods.)

Would a high concentration (e.g., 5 times higher) be ignored if the results of the hypothesis testing for difference in sediment means had a p of 0.06 instead of 0.05 or less?

Hypothesis tests as described attempt to minimize type I error (e.g., falsely concluding that bioaccumulation was different when it was not). Hypothesis tests are commonly applied as described here but such application is inconsistent with what you are really trying to accomplish in a risk assessment. The focus should be on controlling Type II error rate and conducting tests with high power. The type II error (e.g., minimizing the probability that you will accept the hypothesis of no difference in bioaccumulation when there is a difference) and power (e.g., the ability to note a difference in bioaccumulation when there is one) would seem a more appropriate focus. For this reason, much more detail is needed regarding Type II error and power. Did you consider bioequivalence testing (see Dixon and Garrett 1993, McDonald and Erickson 1994, Dixon 1998) which handles Type II errors more appropriately?

6. The means for extrapolation from 28 day bioaccumulation concentrations to [practical] steady state concentrations are compromised. There are too many assumptions and gross approximations forced on the user.

   I assume that the 28 day bioaccumulation test is required and answers in this review should focus beyond the shortcomings of the test. However, I believe that any test which looks at a suite of contaminants simultaneously for a predetermined time will never be fully satisfactory for all contaminants. Large groups of toxicants will have inferior data as a consequence of not coming close to steady state, insufficient sample numbers, and problems of incomplete gut clearance or significant elimination during the time allowed for gut clearance. The gross manipulations to the resulting data as described here will not provide accurate information regardless of the existence of a standard test method and a history of regulatory application.

7. Means of estimating final bioaccumulation for Cd and Hg which are assumed to "violate" steady state kinetics. Inaccurate statements such as "Cadmium and mercury are not regulated in marine organisms as are essential metals, and, thus no adjustment for steady state is applicable" limit the validity of the conclusions. Regulation has little to do with coming to a steady state condition within 28 days. The final conclusion to use 28 day data for Cd and Hg as a consequence of their "non-steady state" behavior is not logical. The concentrations increase through time with no apparent leveling off at a "steady state" concentration. They do not stay at the 28 day concentrations as a consequence of their "non-steady state" behavior.

8. No analysis of *in situ* bioaccumulation data from the proposed dredged sediment sources and reference sediment sites.

9. Use of "log K_{OW} of approximately 4 or greater" as the only cut-off for bioaccumulation [biomagnification?] (page 6, last paragraph). It isn't clear when you are discussing bioaccumulation or biomagnification. If you are discussing bioaccumulation, the cut-off point is not appropriate. If you are discussing potential for biomagnification, the cut-off is reasonable. It is consistent with Connolly and Pederson (1988) and Thomann (1989). However there can be an upper limit too as molecular size begins to influence diffusion rates. Please see Gobas *et al.* (1986) for more detail.

Lee Charge No. 1 Comment (2 para):

I believe the approach in the "Memo for the Record" lays out a logical approach to a difficult problem. I have not reviewed the Regional Matrix Levels, and so can not comment on them directly, though they are obviously a key component for the 4 compounds and dioxins. One limitation is the lack of more specific guidance for evaluating the dredge material as a "whole". I recognize this is very difficult, but you might try formalizing various approaches and see if they come up with reasonable conclusions. For example, you might use an approach similar to a toxic unit approach but using CBRs, WQCTLs values, or other residue effects levels. Each compound would be normalized to the fraction of the effects residue concentration (e.g., 0.2 of a CBR or WQCTL residue) for each compound. Then these fractions could be summed in these various ways: 1) compounds with broadly similar modes of action (e.g., metals vs. organics; all neutral narcotics) or 2) all compounds. Such an approach makes the assumption of simple additivity, which is open to criticism both for not accounting for synergistic interactions and for adding dissimilar toxicants and thereby overestimating actual toxicity. Nonetheless, this approach incorporates the full range of contaminants, their toxicity, how close they are to some estimate of an effects level, and makes a crude attempt at combined effects. Using trophic transport factors, the same approach could be applied to higher trophic levels. The total "bioaccumulative units" (or whatever) are probably better used in a comparative than absolute sense, so the test sediment would be compared to the reference and background.

There is more detail embedded in the text that is not captured in Figure 1. For example, the use of adjustment factors for some organics but not metals or comparison to background residues. I suggest that additional figures be drawn that give kinds details.

Dillon Charge No. 1 Comment (3 para):

Many scientifically appropriate approaches designed to evaluate potential effects of environmental contaminants are tiered. Early tiers are usually simple and environmentally conservative. They promote environmentally protective decisions (i.e., avoiding Type II errors) while minimizing cost and effort. Later tiers are more complex and site-specific. They provide greater clarity and certainty for decision-makers.

It is not clear if the subject Framework is tiered. Bioaccumulation bioassay results are compared (more or less sequentially) to: (1) results with reference material, (2) FDA levels, (3) matrix or Regional Dioxin Values, (4) a "risk evaluation" and 5) the eight Green Book factors. It is not clear if these evaluations are tiered. For example, is the initial comparison to reference the most environmentally conservative? If the Framework is not tiered, analytes of concern could be removed from consideration prematurely.

I recognize the larger dredged material evaluation as described in the Green Book is tiered. This concept should be extended to the subject Framework.

Cook Charge No. 1 Comment (1 para):
Figure 1 in the peer review hypothetical memo diagrams the framework for evaluating bioaccumulation test results. I am particularly concerned about the process (a) if chemical "X" is bioaccumulated less from the test sediment than from the reference sediment. I recognize that it is likely that reference sediments will be cleaner than test sediments for dioxins, but what if they aren't? One would like to see a clear and comprehensive definition of the approach used here. If the same principle was applied to Lake Ontario, would it make sense? That is, if the reference area is highly contaminated, is it ok to dispose of dredge spoils as long as they have a lower concentration of dioxin-like chemicals? I think the answer is no because the disposal increases the chemical mass in a system in which it already exceeds safe levels. Figure 1 seems to indicate that if chemical "X" is greater from the reference sediments, no risk evaluation is needed and one goes on to (d) the integrated effects evaluation. It is unclear from the framework whether the a value greater than the regional dioxin values (1 ppt for TCDD and 4.5 ppt for TEQ) would result in a decision that the material is not Category 1. According to figure 1 this could only happen in step "d" but the description of step "d" is ambiguous on this point.

Agency Response to Reviewers' Comments:

COMMENT 1-1: Peer reviewers generally indicated that many of the individual elements of the framework used by EPA Region 2/USACE-NYD are "scientifically appropriate", technically sound, and "state of the art". The peer reviewers also made several comments/suggestions and in some cases recommended specific adjustments/changes to the HARS Framework and the joint EPA Region 2/USACE-NYD Testing Evaluation Memo (TEM). Another general theme throughout the peer reviewers' responses was their concern over the use of point estimates or specific numbers to determine "risk" or significant undesirable effects associated with the 28-day bioaccumulation test results, mainly due to variability and uncertainty.

RESPONSE 1-1: EPA Region 2 acknowledges the peer reviewer's general support for the HARS Framework and its technical soundness. The scientific peer reviewers offered several different bioaccumulation evaluation methodologies to determine the suitability of dredged material to be used as Remediation Material at the HARS. EPA Region 2 has evaluated all the proposed methodologies (see responses to comments 1-2 through 1-14).

EPA Region 2 believes that use of specific numbers to determine "risk" or potentially significant undesirable effects associated with the 28-day bioaccumulation test results is appropriate in the case of the HARS. In the interest of protecting human health and the environment, EPA has in many cases developed and promulgated specific numbers for water, air, pesticides, etc. that are used to distinguish between acceptable/unacceptable impacts or as pass/fail values.

The peer-review panel raised concerns about the use of "point estimates" or "specific numbers" to estimate risk. An alternative approach would have been to employ a probabilistic approach (e.g., Monte Carlo analysis) to develop a range of possible risk values. However, the use of a probabilistic tool such as Monte Carlo analysis requires "data rich" input parameters with reasonably well-characterized probability distribution functions. In evaluating the appropriateness of a probabilistic analysis, EPA's Guiding Principals for Monte Carlo Analysis (EPA, 1997g) states that as part of the conditions for accepting a probabilistic analysis, "information for each input and output distribution is to be provided in

the report." Additionally, "The selection of distributions is to be explained and justified." Many of the exposure parameters proposed for the development of HARS-Specific Values are based on available empirical data. In some cases data was limited, making it difficult to support development of probability distribution functions. Consequently, and consistent with EPA's policy on probabilistic analysis, the risk assessment would be performed in a deterministic manner.

Given the need to remediate the HARS, EPA Region 2 proposes to develop HARS-Specific Values as guidelines. The proposed HARS-Specific Values would be scientifically derived/developed levels below which there is no potential for significant undesirable effects (human and ecological). They would be designed to be protective and appropriately conservative concentrations that reflect the remedial purpose of the HARS to improve conditions over those currently at the site, where sediments and body burden levels of ambient/resident fauna inside the Primary Remediation Area exhibit Category II (tissue dioxin levels) and III (unacceptable amphipod toxicity) characteristics (EPA, 1997b, 1997c). The proposed HARS-Specific Values would be human and ecological effects-based values to be derived from the best available literature and using standard human health EPA risk evaluation methodology (see responses to comments 4 through 14 for specific details on the derivation of the individual HARS-Specific Values and associated environmental factors). Additionally these proposed HARS-Specific Values would be designed to incorporate the eight factors for LPC compliance listed in the Green Book as appropriate. The HARS-Specific Values may be based upon human and ecological effects/endpoints, not simply set to be below background, baseline, and/or ambient site concentrations. In some circumstances, the HARS-Specific Values would be above background, baseline, and/or ambient concentrations (thereby increasing those concentrations) and other times they will be below. However, in any case, being below these proposed HARS-Specific Values indicates that the dredged material does not have the potential to cause significant undesirable effects (human and ecological). Developing and using the HARS-Specific Values will: ensure that placement of dredged material at the HARS for remediation purposes continues to meet the remedial intent of the HARS designation (for individual chemicals and additive/combined effects), demonstrably contributes to the improvement of conditions within a specific area of the HARS so as to enable re-colonization of sustainable and diverse communities of healthy benthic marine life, and comply with the Ocean Dumping Regulations so as not to cause significant undesirable effects (human and ecological).

These proposed HARS-Specific Values would be scientifically derived/developed using assumptions (i.e., levels of risk, target populations, consumption rates, etc.) intended to be appropriately conservative and protective for human and ecological health for the media for which they were derived. EPA Region 2 has considered and documented areas of uncertainty associated with development of HARS-Specific Values (see Appendix F of this document)..

COMMENT 1-2: One peer reviewer suggested that data from 28-day bioaccumulation tests be used as inputs to a dose-based, food-web model rather than evaluated as toxicological endpoints. The peer reviewer specifically mentioned models developed by Gobas, Thomann, Connelly, and Parkerton as examples.

RESPONSE 1-2: Since the revised Framework is developed in a proposed manner that is sufficiently protective of all trophic level ecological receptors, EPA Region 2 does not believe that the use of a dose-based food-web model (like those of Gobas, Thomann, Connelly, and Parkerton), is necessary. The risk

to fish and other benthic organisms is evaluated using the 28-day worm and clam bioaccumulation test tissue results. EPA Region 2 has examined the experimental designs and non-effective concentrations reported in the studies contained in the USACE *"Environmental Residue Effects Database* (ERED)"(USACE,1998), and EPA-ORD's (Duluth, MN) *"Linkage of Effects to Tissue Residues: Development of a Comprehensive Database for Aquatic Organisms Exposed to Inorganic and Organic Chemicals"* (Jarvinen and Ankley 1999) databases and screened the data for different trophic levels (worms, clams, fish, etc.) to allow for identification of appropriate lowest observed effect levels (LOELs) for use in setting benthic tissue HARS remediation values using only the most robust of the data. Proposed HARS-Specific Values will be chosen by compiling effects information from the best available literature in the databases.

In addition to the individual effects based benthic tissue levels, it is proposed that a summation of all PAHs and pesticides will be compared to a threshold value for total narcotic organic contaminant load to ensure that the HARS-Specific Values are sufficiently protective of various benthic trophic levels (see response to comment 8). EPA and USACE are progressing in the development of techniques that will be useful to conduct a site-specific risk assessment, however there is no agreed upon national approach, as of yet. Therefore, EPA Region 2 does not believe that a final risk assessment for placement of Remediation Material at the HARS is appropriate at this time.

COMMENT 1-3: Two peer reviewers suggested adding a section to the TEM that provides a thorough treatment of uncertainties, degrees of conservatism, and weight of evidence.

RESPONSE 1-3: EPA Region 2 agrees that uncertainty, weight of evidence, conservatism, and variability are important issues to be considered in the evaluative process being used to identify suitable Remediation Material. Appendix F discusses areas of uncertainty associated with development of HARS-Specific Values.

COMMENT 1-4: One peer reviewer commented that background values should not be compared with 28-day test tissue concentrations for the purpose of determining suitability of dredged material for use as Remediation Material at the HARS. He commented that background values are based on resident species that show order of magnitude interspecific variation in tissue concentrations at the same site. Further, the resident species on which the background values are based do not include the standard 28-day bioaccumulation test species, so the uncertainty of interspecific extrapolation precludes valid comparison. The peer reviewer also proposed an alternative approach to evaluating dredging projects, suggesting that 28-day bioaccumulation tests be performed on multiple background and baseline sediment samples and that the bioaccumulation thresholds be based on the results of these analyses.

RESPONSE 1-4: EPA Region 2 agrees that background values should not be compared with the 28-day test tissue concentrations. However, we have different reasons for not comparing to background values. As discussed in responses to comments 3-1, 3-2, and 19-4, EPA Region 2 proposes to eliminate the comparison to background values from the HARS bioaccumulation evaluation process.

EPA Region 2 does not believe that it is appropriate to base bioaccumulation thresholds on the results of background and HARS baseline bioaccumulation test results. Bioaccumulation is a phenomenon and can not necessarily be associated with adverse toxicological effects in the exposed organisms. A statistically

significant difference between bioaccumulation observed in organisms following exposures to test and reference sediment or in field-collected organisms exposed to ambient sediment is not itself a quantitative prediction that an adverse toxicological impact would occur in the field, nor is it related to any cause and effect. In addition, as discussed in the response to comment 1-1, adjustment to the current HARS Framework should be based upon human and ecological effects/endpoints, not simply set to be below background, baseline, and/or ambient site concentrations. This approach will ensure that placement of dredged material meets the remedial intent of the HARS designation. That not withstanding, ambient background tissue concentrations would be duly considered in the derivation of the HARS-Specific Values.

40 CFR §228.15(d)(6)(v)(A) sets forth the management goal for the HARS as being the reduction of impacts within the Primary Remediation Area (defined at 40 CFR §228.15(d)(6)(ii) as one of three constituent parts of the HARS, the other two being a Buffer Zone and a No Discharge Zone) to acceptable levels as required by 40 CFR§228.11(c). Use of the site is restricted to dredged material suitable for use as Material for Remediation. This material shall be selected so as to ensure it will not cause significant undesirable effects including through bioaccumulation or unacceptable toxicity, in accordance with 40 CFR§227.6. The preamble to the proposed rule (62 FR 26267, May 13, 1997) and the supplementary information section of the final rule (62 FR 46147, Aug. 29, 1997) designating the HARS defined Remediation Material or Material for Remediation as "uncontaminated dredged material (i.e., dredged material that meets current Category I standards and will not cause significant undesirable effects including through bioaccumulation." It is EPA's stated intent, as set forth in the supplementary information section of the final rule (62 FR 46147, Aug. 29, 1997) designating the HARS, that the current dredged material evaluation process for identifying Category I dredged material be used in determining the suitability of dredged material to be used as Remediation Material at the HARS.

Material for Remediation must therefore demonstrate Category I characteristics as established by Region 2, and must also comply with the requirements set forth at 40 CFR 227.6 regarding constituents prohibited as other than trace contaminants. In both cases, compliance is determined by effect-based bioassay testing and subsequent evaluation using the HARS Testing Evaluation Framework (which includes the HARS-Specific Values). Both Material for Remediation and Category I material are, by definition, material that will not cause significant undesirable effects through remediation. Further, in stating exactly what constituents are present only as trace contaminants, the test set forth at 40 CFR 227.6(b) is that the materials will not cause significant undesirable effects, including the possibility of danger associated with their bioaccumulation in marine organisms. It is therefore clear that both of these standards to be applied to HARS placement clearly intend an effect-based analysis of the material be used. The peer reviewer's alternative does not incorporate such an analysis, and therefore does not provide a scientific or legal basis for a determination of compliance with EPA's ocean dumping regulations at 40 CFR 227.6 and the definition of Remediation Material.

As in all other ports across the country using ocean disposal, and as part of the USACE-NYD permitting process, NY/NJ Harbor applicants are required to test/characterize the material to be dredged in order to determine if it meets the ocean disposal criteria and EPA's Ocean Dumping Regulations, 40 CFR Part 227, "Criteria for the Evaluation of Permit Applications for Ocean Dumping of Materials." EPA's Ocean Dumping Regulations require the use of bioassay tests (toxicity and bioaccumulation) to determine if the dredged material meets the ocean disposal criteria.

COMMENT 1-5: Several reviewers suggested that risk assessment methods or concepts should be incorporated into the Framework approach. One reviewer suggested that the Risk Assessment Guidelines for Superfund (RAGS) be used as the Framework for conducting human health risk assessment.

RESPONSE 1-5: EPA Region 2 notes that the current and proposed, revised Framework incorporates standard risk equations (see Figures 3 and 4) that are consistent with RAGS. See response to comment 1-2 for discussion on risk assessment.

COMMENT 1-6: Two reviewers proposed including in the HARS Framework an evaluation of chemical body burdens and ecological analyses of organisms residing at the proposed dredging and reference sediment sites. A statistical comparison of bioaccumulation at the proposed dredging site versus the reference site was suggested by one reviewer.

RESPONSE 1-6: EPA Region 2 disagrees. It is impossible to ensure that there would be sufficient organisms (i.e., type and quantity) present at each dredging site for performing the required analyses. In addition, communities at the dredging site are continually and/or periodically disturbed. As a result, the organisms present are likely to be stressed and, therefore, would not be expected to be suitable for use as bioaccumulation test organisms. In addition, for the purpose of determining whether a material is suitable for use as Remediation Material at the HARS, it is important to determine its potential bioaccumulation under site-specific conditions. The chemical and physical conditions (e.g., salinity, water quality, etc.) at the dredging site may be very different from those at the HARS or the reference site. Such differences could have a significant impact on the uptake of chemicals, resulting in potential over- or underestimations of bioaccumulation following disposal at the HARS (see also response 1-4).

COMMENT 1-7: One reviewer suggested that chronic toxicity bioassays should be incorporated into the approach to assist in the use and interpretation of the bioaccumulation results.

RESPONSE 1-7: Chronic toxicity tests are still in development by EPA and USACE and currently are not available for implementation. Once approved protocols, guidance, and implementation information are available, EPA Region 2 will evaluate and consider using/implementing chronic toxicity bioassays into HARS suitability decision making. See response to comment 1-2 for discussion of risk assessment.

COMMENT 1-8: Several reviewers requested that additional information be provided as part of the HARS Framework to include: 1) the experimental design of proposed tests (i.e., techniques, number of replicates, statistical tests, quality control, detection limits); 2) physical and chemical data for the reference site; 3) information on steady state and bioaccumulation versus biomagnification.

RESPONSE 1-8: EPA Region 2 agrees and is proposing to modify the TEM to include the following language:

> "Information on experimental design of the toxicity and bioaccumulation bioassay tests (i.e., techniques, number of replicates, statistical tests, quality assurance/quality control requirements, detection limits, etc.) is provided in the Green Book and Regional Testing Manual."

Information is currently provided in the TEM concerning steady state and bioaccumulation versus biomagnification where applicable.

COMMENT 1-9: One reviewer suggested adding information on the use of adjustment factors (steady state) and the comparison to background to Figure 1 to better reflect the discussion presented in the TEM.

RESPONSE 1-9: EPA Region 2 agrees that information on the use of adjustment factors (steady state) should be added to Figure 1 in the TEM. Also, the proposed HARS Framework (Figure 2) has been revised/modified from the current Framework by removing step, "Regional Matrix Levels and Dioxin Values", and step, "Risk-based evaluations". In place of these steps, the proposed Framework would use the HARS-Specific Values. Further, under the proposed Framework, step "Integrated Effects Evaluation of Bioaccumulation Results Using 8 Green Book Factors" would be replaced with "Consideration of Combined Effects Evaluation: CBR, Total Carcinogenicity, and Non-Cancer Hazard Index".

COMMENT 1-10: One reviewer commented that the proposed approach did not evaluate the sediment as a whole, but only as based on individual chemicals. An alternative approach, based on the concept of toxic units, was suggested. One reviewer acknowledged the lack of specific guidance for evaluating the dredged material as a whole.

RESPONSE 1-10: EPA Region 2 believes that the Framework distributed for peer review did evaluate the sediment as a whole. The Framework distributed for peer review included Step "Evaluation of Solid Phase Bioaccumulation Results for Dredged Material as a Whole". EPA Region 2 recognizes that the toxic units approach is one of many methods that can be used in comparing project bioaccumulation levels to background levels (one of the eight Green Book Factors). In addition to the proposed HARS-Specific Values, EPA Region 2 is proposing to add the evaluation of additive/combined effects (chemicals that act similarly) on the dredged material by applying a more complete and rigorous application of the CBR (see response to comment 8), by evaluating Total Carcinogenicity (see responses to comments 7 and 11), and the non-cancer Hazard Index which uses the concept of toxic units. It is proposed that the Framework be revised to remove Step, "Integrated Effects Evaluation of Bioaccumulation Results Using 8 Green Book Factors" and to be replaced with "Consideration of Combined Effects: CBR, Total Carcinogenicity, and Non-Cancer Hazard Index" approach (see response to comment 3).

COMMENT 1-11: One reviewer raised concerns about the use of benchmark values such as the FDA (too high), Matrix (too old), and dioxin values, noting that these values do not exist for 53 of the 65 chemicals of concern. One reviewer indicated that FDA and matrix values are problematic.

RESPONSE 1-11: Because contamination of seafood in excess of FDA levels is considered a threat to human health, the Green Book (EPA/USACE, 1991), RTM (EPA/CENAN, 1992), and the current HARS Framework consider bioaccumulation test result concentrations in excess of FDA levels, in any test species, unacceptable. The FDA Action Levels are screening values, providing an upper limit, above which tissue concentrations would result in the conclusion that use of such dredged material as Remediation Material at the HARS could result in significant adverse effects at the HARS (see response to comment 6) regarding use of FDA levels in the proposed revised Framework.

Matrix values are currently available for PCBs, DDT, cadmium and mercury. EPA Region 2 agrees that if the matrix value approach (see USACE 1981) is to be used, it should be based on current data. However, EPA Region 2 also agrees that to the extent possible, a consistent methodology should be used to develop all HARS-Specific Values. As such, EPA Region 2 proposes to replace the matrix values with HARS-Specific Values derived with a risk-based evaluation approach. EPA Region 2 also proposes that the comparison to Regional Matrix values no longer be part of the HARS Framework (see Figures 1 and 2 and response to comment 4).

EPA Region 2 recognizes the concern and controversy over the regulation of dioxin. EPA Region 2 is proposing to retain the dioxin value of 1 pptr (2,3,7,8-TCDD). As discussed in response to comment 5-1, in recognition of revisions to the TEFs recently published by the World Health Organization in 1998, and in response to a peer review comment to include the contribution of co-planar PCBs to dioxin toxicity (see response to comment 5-5), EPA Region 2 believes that there is a need to recalculate the Regional Dioxin Value for the sum of the other substituted dioxin/furan/PCB compounds. EPA Region 2 proposes to recalculate the Regional Dioxin Value for evaluation of total toxicity equivalence of all non-2,3,7,8-TCDD dioxin and furan congeners to include the three coplanar compounds identified by the WHO (1998), using the half detection limit procedure and the TEFs listed by WHO (1998). EPA Region 2 believes that this provides adequate protection such that the potential for exposure to dioxin at the HARS will be reduced to acceptable impact levels (see response to comment 5). As discussed in the response to comment 5-1, EPA is currently in the process of evaluating dioxin across all programs and media. While the science of the reassessment is undergoing peer review, and until the evaluation is complete, EPA Region 2 is proposing to continue to use the current regional dioxin values. Upon completion of the EPA reassessment, EPA Region 2 will decide what, if any, changes are necessary to the Regional Dioxin Values.

COMMENT 1-12: One reviewer suggested that the EPA Region 2 Framework for evaluating bioaccumulation should be tiered and follow the tiers identified in the Green Book (EPA/USACE, 1991).

RESPONSE 1-12: As discussed/described in the current and proposed Framework, dredged material proposed for placement at the HARS as Remediation Material is evaluated for compliance with the requirements of 40 CFR 227.6, 227.27, and 228.15(d)(6). Testing of the material is conducted following procedures approved by EPA and USACE, and contained in the joint EPA/USACE national guidance "Evaluation of Dredged Material Proposed for Ocean Dumping - Testing Manual" (February, 1991) (the "Green Book") (EPA/USACE, 1991), and the RTM developed by the EPA Region 2 and USACE-NYD (EPA/CENAN, 1992).

The Green Book (EPA/USACE, 1991) recommends a specific four-tiered approach for evaluating dredged material proposed for ocean disposal: Tier I involves evaluating existing information; Tier II consists of using conservative screening tools on sediment chemistry data (while these conservative screening tools are recommended, the Green Book specifies that Tier II cannot be used to fully determine LPC compliance with regard to bioaccumulation); Tier III consists of laboratory bioassay testing (toxicity and/or bioaccumulation), and Tier IV involves advanced biological evaluations. The Green Book provides guidance on what to test for and how to evaluate the test results. Evaluation of dredged material from New York/New Jersey Harbor includes Tier I through Tier III evaluations as specified in the Green Book in order to make determinations regarding the suitability of dredged material for ocean disposal. The

current HARS Framework and the proposed HARS Framework (Figures 1 and 2) describe an approach to evaluating the 28-day bioaccumulation test results (which are part of Green Book Tier III) for dredged material proposed to be used as Remediation Material at the HARS. In all cases of evaluation for the HARS, determination of LPC can be made at Tier III. The Tier III testing and evaluation procedures used by EPA Region 2 and USACE-NYD satisfy case-specific data needs and incorporate steady state estimation and comparison to body burdens of the benthic areas surrounding the HARS.

COMMENT 1-13: There were also comments on specific elements of the Framework:

COMMENT 1-13A: One reviewer cautioned that metals, other than MeHg, should not be treated the same as the organic compounds with Kow values > 3.5.

RESPONSE 1-13A: (see response to comment 13)

COMMENT 1-13B: One reviewer suggested that additional language be added to the document to better inform the reader about sampling, statistics, steady state, and biomagnification.

RESPONSE 1-13B: (see responses to comments 1-8, 10, and 13)

COMMENT 1-13C: One reviewer noted that he felt the first level screen using reference was appropriate.

RESPONSE 1-13C: See responses to comments 1-1, 1-6, and 18).

COMMENT 1-14: Reviewers also touched on issues that pertain more directly to methodology in the national guidance (i.e. the Green Book) or in the regional implementation of the national guidance. The issues are as follows:

COMMENT 1-14A: Are detection limits low enough to detect ecologically significant levels? How should non-detects be treated.

RESPONSE 1-14A: The method detection limits specified in the RTM (EPA/CENAN, 1992) were established sufficiently low to detect current guideline values which were based on human health and ecological effects levels. Test results for which the sample detection limits are above these method detection limits are addressed in a manner consistent with the policy for statistically evaluating/quantifying non-detects established by EPA Region 2 and the USACE-NYD (CENAN, 1997).

COMMENT 1-14B: The characteristics of the reference sediment should be similar to the sediment being tested.

RESPONSE 1-14B: The Green Book (EPA/CENAN, 1991) defines reference sediment as a sediment, substantially free of contaminants, that is as similar as practical to the grain size of the dredged material and the sediment at the disposal site, and reflects conditions that would exist in the vicinity of the disposal site had no dredged material disposal ever occurred, but had all other influences on sediment condition.

The current HARS reference site is located approximately 1 nautical mile south of the HARS and is representative of typical New York Bight Apex sediments (i.e., typically coarse grained/sandy material). In contrast, sediments at the HARS are not typical of New York Bight Apex sediments, however, had no disposal ever occurred in the HARS area, those sediments would also be coarse-grained/sandy material.

COMMENT 1-14C: The use of bioaccumulation tests may be too variable. Use of a negative or positive control may be necessary.

RESPONSE 1-14C: The prescribed bioaccumulation test procedures in the Green Book (EPA/USACE, 1991) and Regional Testing Manual (RTM) (EPA/USACE-NYD, 1992) provide sufficient quality assurance guidance to provide for statistically valid replication and use of experimental laboratory controls and to quantify the variability inherent in laboratory biological tests using field collected organisms. For these reasons, EPA Region 2 does not feel that additional quality assurance controls (i.e., negative or positive controls) are necessary.

COMMENT 1-15: One reviewer commented that the use of a K_{ow} of 4 to identify chemicals with the potential to bioaccumulate should be clarified.

RESPONSE 1-15: EPA Region 2 acknowledges the comment, and is proposing that the TEM be modified to include the following language:

> "Those bioaccumulative compounds with the potential to biomagnify (i.e., Kow of approximately 4 or greater) are included on the testing list....."

COMMENT 1-16: One reviewer commented that the statistical methods used to determine average concentrations for data sets containing observations below the analytical detection limits should be re-evaluated. The reviewer expressed confusion regarding the methods used and further states that the methods for addressing observations below the detection limit as described in reference 13 (i.e., "New Approach to Handling "non-detects" in Elutriate and Tissue Data") are invalid. The reviewer further recommends hypothesis testing using either a simple z statistic or bioequivalence testing which handles Type II errors more appropriately.

RESPONSE 1-16: EPA Region 2 disagrees that the approach used to address observations below the detection limit is invalid or not appropriately conservative. To briefly summarize the methods used, for those instances where the reported detection limits (DL) met previously established data quality objectives (DQO), one-half the DL was used to represent values below the DL. When the DQOs were not met, values equivalent to the DL were assigned to site-specific observations reported as below the DL, thus representing the most conservative, worst-case scenario. However, for observations below the DL from the reference areas, a value of zero was assigned, representing the most conservative scenario for comparison to the reference. Although the reviewer is correct in stating that substituting a constant (e.g., zero, the DL or one-half the DL) for observations below the DL does introduce bias in the estimation of a mean or standard deviation, the goals of estimation differ from those of hypothesis testing, where bias is of less importance than producing a "true" outcome. Furthermore, the alternative methods promoted in the literature, such as maximum likelihood estimation, are based on large sample sizes and are may not be

appropriate for use with the small sample sizes typical of dredged sediment contaminant bioaccumulation evaluations. The simple substitution techniques EPA Region 2 proposes have been demonstrated in a large scale simulation study to work better than other methods such as maximum likelihood in minimizing Type I and Type II errors for small sample comparisons (Clarke 1998). Additional guidance for applying methods for handling non-detects in small-sample bioaccumulation comparisons is provided in Clarke (1995).

Charge Group: Framework

Charge No. 2. Which of the risk-based values derived constitute "true" conservative estimates of risk levels (i.e., exceeding the value should be interpreted as sufficient cause to conclude that significant undesirable effects may result through bioaccumulation)? Which of the risk-based values derived constitute conservative screening values (i.e., test tissue concentrations below the value can confidently be interpreted to pose no risk of significant undesirable effects and exceeding should be further evaluated before the probability of significant undesirable effects can be assessed)? How can the "true" risk levels be calculated for those compounds which you believe only to have screening values? How should test concentrations be compared to risk-based levels to determine whether they are exceeded.

Summary of Peer Review Comments

Individual Comments

Bartell Charge No. 2 Comment (3 para):
Risk, by definition, is the conditional probability of some undesired event occurring, along with some statement of its consequences (i.e., human health, ecological). In the absence of perfect information, our data and understanding are conditioned on all the sources of bias and imprecision inherent to the scientific enterprise. Thus, there are no "true"values among the risk-based criteria described in the report. Nevertheless, we might begin to approximate some of these kinds of values with sufficient accuracy and reliability that they can be justifiably used in a decision-making framework (e.g., Figure 1 of the report). For example, the sediment concentrations back-calculated using the WQCTL and the BCF method might provide initial estimates of exposure potentials that would lead to unacceptable risk. This approach might be made more conservative (i.e., pessimistic) by using the lowest observed effects level (LOEL), or no observed effects level (NOEL) to replace the chronic value in the calculation. It is recognized that estimating the NOEL/LOEL is an inherently uncertain process, however.

Sediment criteria derived from the background or reference area benthic tissue concentrations might serve as useful screening values in the context of the overall assessment. This is based on the presumption that the populations in the reference area are not declining as a function of their exposure to "background" concentrations of contaminants. This also assumes that the reference concentrations are less than the values back-calculated from the chronic toxicity data, the LOEL, or the NOEL.

Risk, as defined above, is fundamentally probabilistic. Therefore, every attempt possible should be made to develop the sediment risk assessment in a probabilistic framework. As the result of such a probabilistic framework, distributions of exposure would be compared statistically with distributions of toxic benchmarks for each species-contaminant comparison. Statistical testing of differences between mean values is an appropriate component of risk characterization. However, statistical measures of overlap of distributions, or estimates of the probabilities of exposure being less than screening values or greater than values associated with unacceptable risk should become standard components of the proposed risk assessment framework.

Clifford Charge No. 2 Comment (14 para, 1 table):
Because I believe that all of the comparison criteria (a^1, b^1, b^2, b^3, c) have "risk-based" components, I will not restrict my comments to those values presented as "risk-based" in section V.C.2.c. Also, note that by definition, a conservative estimate of risk is not a "true" level of risk, but one that is biased toward conservatism.

As I'm not entirely comfortable that any body burden evaluations really constitute true estimates of risk (significant undesirable effects may not result through bioaccumulation), it may be appropriate to designate exceedance of the screening values presented as "constituting sufficient cause for further evaluation", rather than as "constituting actual risk of significant undesirable effects".

As to methods of comparison of test concentrations to risk-based levels, I have no difficulty with the statistical methods used, although non-parametric methods might be preferable to parametric ones given the likely large departure of environmental conditions from those required for parametric hypothesis testing.

(a) Comparison of bioaccumulation test results to reference sediment test results

Comparison of site results to reference area results is probably the least biased of the screening values (closest to a "true" estimate), although sample size could be an issue under some circumstances. As noted in the text, exceedance of reference values is common when those reference values contain a lot of non-detects. This difficulty might be reduced to some degree by replacing the "half-detection limit" method employed with a technique like maximum likelihood estimation to deal more effectively with the censored (below detection limit) tails of the distributions (e.g., Lindgren 1976; Sielken *et al.* 1993; Clifford *et al.* 1995; Banton *et al.* 1996). Also, as I discuss in more detail in other responses herein, I am not entirely convinced that a body burden of a contaminant can necessarily always be related to a toxicological effect. As such, while the comparisons are good for identifying circumstances where site sediments have greater chemical activity than reference sediments, caution must be exercised when interpreting the meaning of the results.

There is also the issue of contaminants potentially present but not analyzed for. This is a vexing problem in most risk assessments and not one we are likely to solve here but, I suggest some qualitative language in an uncertainty section and perhaps a discussion early in the document regarding how the analyte list was generated.

(b^1) Comparison to FDA Action levels

FDA Action levels are not generally derived for protection of the environment as human health and economic concerns are paramount therein. As such, I do not believe that they are really appropriate for the purposes of this sort of an evaluation (regardless of the applicable regulations), although their inclusion does not in any way compromise the process as they constitute a reason to reject a sediment rather than a reason to accept one. I believe that these levels are probably, in general, the least representative of "true" ecological risk estimates of those presented.

(b^2) Comparison to Regional Matrix levels

The regional matrix values were derived by very conservative methods (e.g., selection of the lowest available BCF). As such, they do not represent "true" risk levels, but conservative ones. In addition, because they are calculated values, not empirically measured ones, the associated uncertainty cannot really be evaluated. These values, if we accept the notion that body burdens can be directly related to effects, are probably the most useful as screening values and would be the closest of the values presented to ones that when not exceeded confidently represent category I sediments.

(b^3) Comparison to Regional Dioxin Values

I'm uncertain regarding the degree of conservatism (or accuracy) in the Regional Dioxin Values for ecological concerns. This is principally because the values are derived for protection of human health and humans may not be an appropriate end receptor for evaluation of ecological risk. The solitary study compared to for actual ecological concerns is that of Cook *et al.* (1993) which presents a value of 50 pptr as a "low risk" concentration for adverse effects on fish while the relationships used to derive the criterion predict a value of 20 pptr in fish tissue at the criterion. This margin of safety (factor of 2.5) may actually represent a value within the range of chronic toxicity, but this is unknown. I would have to see much more environmental data here and consideration of or more applicable ecological receptors to be comfortable with the uncertainty in the associated values. I believe that it would be appropriate to select an upper trophic level receptor that is more relevant to the site than humans like a large carnivorous fish or a piscivorous bird.

(c^1) Risk-based consideration of bioaccumulation and food-chain transfer potential

Although both high bioaccumulation and high food-web transfer potentials can generally be considered to be undesirable, they are not, in a strict sense, a measure of the environmental risk posed by a compound. According to the studies of many researchers, the rate at which an organism receives it's exposure (it's dose rate) may be of greater importance than the ultimate body burden. PAHs are a good example here as the rate of uptake (dose rate or exposure rate) may be a good predictor of effects while body burdens may remain fairly constant at low values due to metabolism. The notion of "steady state" with such easily metabolized compounds may be illusory at best and misleading at worst.

There are certainly researchers who maintain that body burdens for many contaminants can be confidently related to adverse effects and, although I do not propose to present a data-laden debate on that subject herein, I have much greater confidence in dose estimates than body burden estimates based on the data I have encountered. Also, for many high Kow compounds (e.g., DDTs and dioxins), "steady state" may never actually be achieved by any exposed organism (Pruell *et al.* 1990 notwithstanding) because the depuration (including transformation, metabolism, and elimination) rate is essentially zero and tissue concentrations never actually reach a maximum (although there may be a functional upper boundary where lethality always occurs). For other compounds like PAHs, bioaccumulation and body burden may be very difficult to interpret due to metabolism. As such, while it is certainly possible to predict bioaccumulation for some compounds, and this can be useful for modeling contaminant movement in a food-web, I am not entirely comfortable that risk should be solely evaluated on this basis.

(c^2) Risk-based comparison to background concentrations

Although simple comparison of concentrations to background is not terribly robust, it is probably one of the more accurate in terms of identifying potential for risk. This statement must, of course, be taken with the cautions presented above regarding bioaccumulation. The greatest difficulty here will be identification of a true "background" location. Note also that for a very clean background sediment, the propensity of non-detects will drive numerous spurious exceedances by project sediments where risks do not actually exist. Nonetheless, I believe that considered comparison of actual data with actual data are the most accurate of the screening techniques presented herein.

(c^3) Risk-based potential for ecological effects

Body burdens are again employed herein and the same reservations I have expressed above are applicable here. My concerns regarding toxicological interpretation of body burdens aside though, the statement that "CBRs are represented as the ratio of the mass of the toxicant per kilogram (mmole or ug/kg) of organism." for PAHs is not entirely consistent. If we accept the notion that one molecule of a PAH is toxicologically equivalent to any other molecule of a PAH, (which is not without merit and supporting data) then the measurements must be made on a mmole basis, not on a mass basis. The following conversions (using BaP as a standard since the most toxicological information is available for that compound) would be required:

PAH	Molecular Weight	Conversion Factor
Naphthalene	128.16	1.97
Acenaphthylene	152.21	1.66
Acenaphthene	154.21	1.64
Fluorene	166.21	1.52
Phenanthrene	178.22	1.42
Anthracene	178.22	1.42
Fluoranthene	202.26	1.25
Pyrene	202.24	1.25
Benz[a]anthracene	228.28	1.11
Chrysene	228.28	1.11
Benzo[b]fluoranthene	252.32	1.00
Benzo[k]fluoranthene	252.32	1.00
Benzo[a]pyrene	252.32	1.00
Dibenz[a,h]anthracene	228.28	1.11
Indeno[1,2,3-cd]pyrene	276.00	0.91
Benzo[g,h,i]perylene	276.00	0.91

Therefore, the statement that a 400 ppb dose of naphthalene is equivalent to a 400 ppb dose of fluorene is incorrect as the equivalent mass of fluorene would be 519 ppb to achieve the same number of molecules. On that basis, the total dose would be 919 ppb naphthalene toxicological equivalents, not 800 total PAH equivalents.

(c[4]) Risk-based consideration of potential effects on human health

As I've indicated above, I have reservations that humans are the most exposed or most at-risk upper level consumers at this site and recommend consideration of a possibly more ecologically relevant receptor such as a piscivorous bird or fish.

Gentile Charge No. 2 Comment (3para):
Currently, you've identified the 'true' conservative estimates of risk to be: FDA Action Levels; Matrix level; or Dioxin Category 1 value from the perspective that exceeding these values classifies the dredged material as not being Category 1.

A conservative screening value could be set at no significant difference from reference as long it satisfies several assumptions:1) the reference values are representative of uncontaminated sites throughout the country and not just locally; 2) that the reference values have been shown to be associated with healthy benthic community structure and function or from laboratory studies shown to have no associated biological effects; and 3) that the statistical design for determining significant differences satisfies assumptions of random sampling, proper selection of replicates and avoids the issue of pseudo-replication. The comparison of risk-based level to test concentrations gets at the heart of my concerns with the application of this framework. That is, is there sufficient replication in the determination of test values, reference values, WQCTLs, FDA and Matrix benchmarks to determine statistical differences with prescribed levels of confidence? If not then this framework is not risk-based and should not be purported to be.

Wentsel Charge No. 2 Comment (1 para):
I not sure there are "true conservative estimates of risk". The risk based discussions starting on page 11 do not present enough information to answer the questions for No. 2. For example, on page 15 the Lee *et al.*, 1989 method is presented; has it been peer reviewed, is it protective of assessment endpoints, and it treats metals and hydrophobic compounds the same. It seems that this method is designed as if protection of the clam and worm are the endpoints. The endpoints are probably higher in the food web. A more thorough discussion of what is being protected would be beneficial. To address the questions in No. 2 the information in pages 11-15 and appendix A would need to be rewritten to clearly pose those questions and recommend answers. The text of the example testing memo gets in the way of the technical discussion of these issues.

Other comments:
p.12 2nd metals para.
The second sentence is misleading. Cu, Ni, and Zn are micronutrients and the organism can, within a given concentration, regulate the amounts of these compounds. These metals don't have the physical/chemical parameters to biomagnify.
P.8 1st para.
Change true to significant; Discuss method for below detection limit values.
P.14 3rd para.
Most recent BG data? Wouldn't mean and std.dev. provide more information?
Table 1 should be divided into four parts (split human health and eco; chemicals into two groups), it's too much diverse information and the table doesn't stand alone.

Solomon/Sibley Charge No. 2 Comment (3 para):
The term risk is often used in the incorrect context. Use of the term "risk" implies that the likelihood of something happening is known or has been estimated. Properly, risk should always be expressed as a probability. Comparison of a concentration (in biota, or in a matrix) to a reference concentration or a criterion concentrations is an assessment of hazard. Hazards can either be present or not be present, as the one concentration is either greater than or less than the other. Traditionally, hazard quotients (one concentration divided by another) have been used in the early tiers of risk assessment to determine whether further and more detailed risk assessment is needed. The criteria or standards used to calculate reference values for hazard quotients are usually based on relatively conservative numbers. For example, the procedures to calculate water quality criteria use a number of conservative assumptions (Stephan *et al.* 1985). The Final Acute Value criterion is based on the more sensitive organisms (5^{th} centile of the genus mean acute values) and additional conservatism is added in the calculation of the Final Chronic Value. The reason for these conservative approaches is that the criteria are designed to be protective of almost all organisms, most of the time. The criteria are designed to apply in a variety of situations, some where for physical or biological reasons, more sensitive organisms may be present, while for other they may not. The criteria are thus protective, not predictive. The use of these hazard quotients to assess "risk" is therefore conservative.

Used in the proper way, the hazard quotient can be used to decide whether a hazard exists or not. If it does not exist, the situation is unlikely to present a significant hazard and no further risk assessment is needed. However, the obverse, that is, the exceedence of the criteria, does not mean that a significant risk exists, it merely means that further work is necessary to better quantify the risks. Given that none of the criteria were exceeded in this particular risk assessment suggests that further detailed risk assessments are not necessary as the criteria on which the hazard quotient was based are conservative. Had some of these values exceeded the established criteria, other approaches to risk assessment such as those using probabilistic techniques (Klaine *et al.* 1996; Parkhurst *et al.* 1995; Solomon *et al.* 1996) could have been used, provided that sufficient data were available to adequately describe the range of susceptibility of organisms and the spatial and temporal variation of the exposure or body concentrations.

This reviewer is not suggesting that a probabilistic risk assessment be carried out in this particular case but rather that this may be another way of conducting these assessments once the probabilistic techniques have been refined and the appropriate data collected.

Newman Charge No. 2 Comment (13 para):
Answer (General answers will be given to the subquestions within this question and then specific points provided relative to each option.) Specifically, "The toxicological significance of this bioaccumulation was evaluated by: i) consideration of steady-state bioaccumulation and food-chain transfer; ii) comparison to background tissue concentrations; iii) consideration of potential ecological effects; and iv) consideration of potential carcinogenic and non-carcinogenic effects to human health" (page 12).

General Answer
First Subquestion: Which of the risk-based values derived constitute "true" conservative estimates of risk levels (i.e., exceeding the value should be interpreted as sufficient cause to conclude that significant undesirable effects may result through bioaccumulation)? In my opinion none of these methods produces a conservative estimate of risk levels that is consistent with either of the qualifiers, true or sufficient. Using

the 28 day bioaccumulation test as the foundation to derive these values is problematic. If you insist on the assumption that this test is valid, I would suggest that iii and iv are conservative estimates of the true risk levels.

Second Subquestion: Which of the risk-based values derived constitute conservative screening values (i.e., test tissue concentrations below the value can confidently be interpreted to pose no risk of significant undesirable effects and exceeding should be further evaluated before the probability of significant undesirable effects can be assessed)? In my opinion none of these methods produces a "true" conservative screening value. Using the 28 day bioaccumulation test as the foundation to derive these values is problematic. If you assume that this test is valid, I would suggest that i to iv combined provide a conservative estimate of the true risk levels.

Third Subquestion: How can the "true" risk levels be calculated for those compounds which you believe only to have screening values? In my opinion, I would use the concentrations in representative species living at the site from which the dredged materials are to be taken and use them in the comparisons described, i.e., use them in approaches (i) to (iv). If there were relevant commercial/sports species or major forage species for commercial/sports species relevant to the source and HARS sites, I would consider them. I would consider biomagnification when reasonable. For example, lobster stocks at the HARS are noted as having high TCDD/PCB levels. Lobsters are long lived and could easily biomagnify contaminants such mercury from benthic forage species and act as a vector to human exposure. After such consideration, I would then apply (ii) to (iv) with possible modifications as described below.

Fourth Subquestion: How should test concentrations be compared to risk-based levels to determine whether they are exceeded? Under the assumption that the 28 day bioaccumulation test is valid, use the method described in the answer to "Third Subquestion" including the modifications described below, e.g., additional consideration of a hazard index.

Specific Points

(i) Consideration of Steady-state Bioaccumulation and Food-Chain Transfer

The definition of steady-state as "the lack of significant difference (ANOVA, alpha=0.05) among tissue residues taken at three consecutive sampling intervals" is not ideal. It is particularly problematic if not linked to a required type II error rate, level of power or minimum significant difference. Statistical power must be considered in order to get a meaningful decision from an ANOVA. For example, wide variability within times could result in the acceptance of the null hypothesis of no difference in means when there was a difference. Sampling three very close time intervals could also result in an acceptance when there was a difference.

Metals: The statement that metals bioaccumulate faster than organic compounds is not valid. For example, cadmium will slowly bioaccumulate during the life of an organism, and some organic compounds can be taken up very rapidly.

The assumption that metals do not biomagnify is reasonable with the major exception of mercury which is a classic example of a toxicant that does biomagnify. The use of a trophic transfer coefficient of one is not appropriate for mercury. See Cabana and coworkers (Cabana and Rasmussen 1994, Cabana *et al.* 1994) as an example. See also third paragraph on page 11 of reference 57.

Pesticides and Industrial Chemicals and PAHs: General extrapolations from 28 day bioaccumulation levels to steady state does not seem appropriate.

(ii) Comparison of test results to background tissue concentrations
Here concentrations in organisms taken from near (but not in) the HARS are compared to those from the bioaccumulation tests. The comparison of these organisms to those from a 28 day test that may not be at steady state seems dubious. Why not compare animals from near the HARS to those endemic to the site from which the dredged materials are to be taken?

(iii) Consideration of potential ecological effects
A literature review was done to evaluate the bioaccumulation test results relative to ecological effects. The CV is multiplied by an estimated BCF and compared to that concentration "protecting" 95% of tested species in the database. Except for the paucity of data from which to draw conclusions and the dubious extrapolation from the bioaccumulation test, this is a reasonable indicator. Critical body residues were used for PAHS which also seems plausible.

(iv) Consideration of Potential Carcinogenic and Non-Carcinogenic Effects on Human Health
The focus here is risk to humans by ingestion. As discussed above, the extrapolated "steady state" concentrations are suspect and, therefore, the "human cancer protection levels" derived from them are also suspect.

The hazard quotient was used for non-carcinogenic effects. Would it be reasonable and informative to also extend this approach to the calculation of a hazard index (sum the hazard quotients) under the assumption of additivity? Biomagnification to a commercial species foraging on these representative benthic species is inappropriately ignored in the analysis, e.g., mercury biomagnification.

Dillon Charge No. 2 Comment (2 para):
Risk is the probability or likelihood of adverse effects. Chemical risk is assessed by determining the probability of exposure (exposure assessment) and the consequences of that exposure (toxicity or effects assessment). Because risk assessment it is a probability-based process, "true" risk as it used in the above question, does not exist. Rather, risk is a gradation of potential outcomes ranging from low to high hazard. I assume the "risk-based values" in the question refer to the four elements in Framework's Block c labeled "Risk Evaluation for Chemical "X". These four elements are: 1) estimates of steady-state bioaccumulation and food-chain transfer, 2) background comparisons, 3) ecological effects assessment, 4) carcinogenic and non-carcinogenic effects on human health. None of these four considerations can be classified as more or less "conservative". Nor are they "risk-based values" per se. That is, they don't systematically assess exposure and toxicity *vis a vis* specific receptors. The first two are exposure estimates. The last two describe methods for evaluating effects to ecological and human receptors, respectively.

Agency Response to Reviewers' Comments:

COMMENT 2-1: Several reviewers noted that the risk-based values are not actually 'true' estimates of risk levels (i.e., the probability or likelihood of an undesired effect or event occurring), but conservative values for assessing hazard (i.e., whether the hazard is present or not present). It was noted by the peer reviewers that hazard assessments like these are traditionally set to be conservative and used as screening

values. In other words, concentrations lower than the values can be interpreted as being safe, or representing no potential hazard, while concentrations above the value indicate only that the potential for risk exists. The peer reviewers suggested a dose-based risk assessment should be used to quantify the actual risk.

RESPONSE 2-1: EPA Region 2 believes that the risk-based values developed in the existing framework and those that will be used in the proposed HARS Framework represent levels below which there is no potential for significant undesirable effects rather than "true" estimates of risk. While it is true that a dose-based evaluation might provide a more realistic, quantitative estimate of risk than the risk-based values, application of risk-based values would ensure that placement of dredged material at the HARS for remediation purposes meets the remedial intent of the HARS designation, will demonstrably contribute to the improvement of conditions within a specific area of the HARS, and will comply with the Ocean Dumping Regulations so as not to cause significant undesirable effects (see response to comment 1-1). This approach is consistent with other EPA programs in which specific numbers have been developed for the purpose of evaluating and screening media (water, air) to distinguish between acceptable or unacceptable concentrations. Also see response to comment 1-2 regarding use of risk assessment.

COMMENT 2-2: One reviewer suggested that sediment quality criteria derived from the background or reference area benthic tissue concentrations might serve as useful screening values.

RESPONSE 2-2: EPA disagrees with the derivation and application of sediment quality criteria using background or reference area benthic tissue concentrations for the purpose of evaluating the suitability of dredged material at the HARS. Sediment concentration alone is not be a good indicator of the potential for environmental effects because site-specific chemical and physical conditions greatly influence the bioavailabilty (and therefore, the toxicity) of sediment-associated chemicals (BJC, 1998; McCloskey *et al.*, 1998). By focusing on chemical concentrations taken up into the tissues of organisms exposed to the dredged material in the 28-day bioaccumulation test, EPA ensures that only the bioavailable fraction is considered when making decisions regarding appropriateness of dredged material as Remediation Material.

In addition to the scientific concerns set forth above, EPA Region 2 is unable to endorse this suggested alternative approach to evaluating the suitability of dredge material for placement at the HARS, since the proposal is inconsistent with the ocean dumping regulations (40 CFR 220-229) which define the regulatory framework of the HARS. Both Material for Remediation and Category I material are, by definition, material that will not cause significant undesirable effects through remediation. Further, in stating exactly what constituents are present only as trace contaminants, the test set forth at 40 CFR 227.6(b) is that the materials will not cause significant undesirable effects, including the possibility of danger associated with their bioaccumulation in marine organisms. Both of these standards, as applied to HARS placement, clearly intend an effect-based analysis of the material be used. The proposed alternative does not incorporate such an analysis, and therefore does not provide a scientific or legal basis for a determination of compliance with EPA's ocean dumping regulations at 40 CFR 227.6 and the definition of Remediation Material (see response to comments 1-4, 3-1, and 3-2).

COMMENT 2-3: Several reviewers suggested that potential risks to site specific, sensitive, higher trophic level species (e.g., carnivorous fish, piscivorous birds or lobsters) should be evaluated instead of

humans. One reviewer suggested that the language be reworded to more clearly define the purpose of the evaluation (i.e., what organisms is the approach designed to protect).

RESPONSE 2-3: Predatory (carnivorous) fish and benthic invertebrates and shellfish would be identified as the species most likely to be impacted by the disposal of dredged material (EPA, 1997a) and would be used as target receptors for assessing potential risk.

The New York Bight food web used in the Framework for assessing risks of contaminants in dredged material proposed for placement as Remediation Material was described by a simplified food chain consisting of three representative trophic levels. The three trophic levels (benthic organisms, benthic predators, and upper level predators) were selected by consensus through numerous discussions/meetings (1994 to 1996) with a Mud Dump Site Criteria Workgroup (comprised of scientific representatives of the environmental and regulated communities, NY and NJ resource agencies, USEPA, USACE, and NOAA-NMFS with knowledge of the NY Bight ecosystem) which was established and convened under the NY/NJ HEP Dredged Material Management Forum to develop bioaccumulation Criteria for disposal at the former Mud Dump Site (for additional information on the trophic levels see EPA (1995a)).

Although known to occasionally occur at the HARS, higher level ecological receptors (e.g., piscivorous birds and mammals) were not identified as appropriate terminal ecological receptors in the 1995 food chain characterization. In addition, no higher level ecological receptors (beyond predatory fish) were identified by U.S. Fish and Wildlife Service, NOAA-NMFS, state resource agencies, or the public in comments received during the HARS site designation process. Therefore, predatory fish are proposed for use as terminal ecological receptors in the assessment of ecological risks at the HARS.

The current approach used to derive protective tissue levels for ecological receptors (Appendix D) is based on an extensive evaluation of linked residue-effect data for a variety of aquatic species including both invertebrates (e.g., shellfish and infaunal worms) and fish. The final benthic tissue values for evaluation of potential ecological effects in dredged material proposed for use as Remediation Material at the HARS would be based on the lowest median effects residue (ER50) for any species reported in the literature for each of the chemicals evaluated. EPA Region 2 believes that values derived using this approach would be appropriately conservative to be protective of all benthic invertebrate and fish species that might be present at the HARS, including lobster and carnivorous fish and, therefore, protective of the primary ecological communities at that site. Due to the presence of recreational fisheries in the vicinity of the HARS, consumption of fish represents a potentially important exposure pathway for human health, therefore, EPA Region 2 would also evaluate that potential exposure route.

COMMENT 2-4: Several reviewers raised questions regarding the statistical robustness of the values used and the methodologies employed.

RESPONSE 2-4: EPA Region 2 acknowledges the concern regarding the statistical methods employed. Standard, scientifically acceptable statistical methods were used for the purpose of deriving the values previously used and would be used in development of HARS-Specific Values. Appendix F discusses areas of uncertainty associated with development of HARS-Specific Values.

COMMENT 2-5: Many of the reviewers suggested that the incorporation of probabilistic techniques would be very beneficial for the process and provide a more accurate estimate of risk.

RESPONSE 2-5: See response to comment 1-2 regarding use of risk assessment.

COMMENT 2-6: Questions were raised regarding the appropriateness of the use of a 28-day bioaccumulation test, given that many chemicals may never reach steady-state during that time period. In addition, the definition of steady-state was questioned, as well as the validity of the multipliers used to estimate steady-state conditions.

RESPONSE 2-6: EPA Region 2 acknowledges the limitations associated with estimating tissue concentrations associated with steady-state conditions based on the results of laboratory 28-day bioaccumulation tests, but believes the approach to be sound and scientifically appropriate. EPA Region 2 would continue to address steady-state concerns in its proposed Framework through the use of improved multipliers applied to the results of the 28-day test, as described in the response to comment 10.

COMMENT 2-7: One reviewer expressed concern regarding the use of body burden data to evaluate potential hazards, commenting that bioaccumulation and food web transfer cannot necessarily be associated with a toxicological effect and that the rate at which an organism receives its exposure may be more important than the ultimate body burden. The reviewer suggested that dose estimates would be more appropriate than body burden estimates, particularly for PAHs, which are readily metabolized.

RESPONSE 2-7: EPA Region 2 acknowledges that there are uncertainties associated with using results of 28-day bioaccumulation tests. However, currently, scientifically defensible, and practicable alternatives (dose estimates) do not exist. As discussed in the response to comment 1-2, EPA Region 2 and believes the current approach is the best available, sound, environmentally appropriate, and protective of human and ecological receptors. As discussed in response to comment 1-2, EPA and USACE are progressing in the development of techniques that will be useful to conduct a site-specific risk assessment but there is no agreed upon national approach as of yet. When an approach is developed, EPA Region 2 and USACE-NYD will consider it for application at the HARS.

COMMENT 2-8: One reviewer suggested that it would be better to use field-collected organisms from areas near the HARS and compare measured tissue concentrations to field-collected organisms from the area proposed for dredging.

RESPONSE 2-8: See response to comment 1-6.

COMMENT 2-9: The reviewers agreed with the assumption that metals do not biomagnify with the exception of mercury. It was noted that it was inappropriate to assume that mercury does not biomagnify.

RESPONSE 2-9: See EPA's response to comment 13-2. EPA agrees that a trophic transfer factor of 1 (i.e., implying no potential for biomagnification) is inappropriate and would suggest derivation of a value of 1.95 based on data presented by Cabana *et al.* (1994).

COMMENT 2-10: One reviewer noted that the PAH evaluation should be done on a molar basis rather than on a mass basis.

RESPONSE 2-10: See EPA's responses to comments 1-1 and 8-4. EPA will express the narcotic dose of PAHs and other organic contaminants as molar concentrations for use in the CBR model under the proposed revised Framework.

COMMENT 2-11: One reviewer commented on the need for qualitative language in an uncertainty section regarding the issue of contaminants that are not currently quantified that may potentially be present.

RESPONSE 2-11: EPA Region 2 has considered the need to include additional constituents recommended by the reviewers as described in the response to comment 16-1 and believes this list of compounds of concern is appropriate for NY/NJ Harbor sediments. The whole sediment toxicity tests conducted as part of the overall Framework takes all contaminants present in sediment into consideration. EPA Region 2 agrees with the suggestion to describe, qualitatively, uncertainties pertaining to contaminants evaluated in the Framework. Appendix F discusses areas of uncertainty associated with development of HARS-Specific Values.

COMMENT 2-12: One reviewer suggested that a hazard index (i.e., sum of calculated hazard quotients) be included under the assumption of additivity.

RESPONSE 2-12: EPA agrees that the additive effects of the chemicals should be evaluated. As discussed in response to comment 3-1, revisions to the Framework would consider the evaluation of additive effects on the dredged material as a whole would be addressed through a more complete and rigorous application of the CBR (see response to comment 8), total carcinogenicity (see responses to comments 7 and 11) and Non-Cancer Hazard Index (see response to comment 7).

COMMENT 2-13: One of the reviewers commented that the FDA Action Levels are not generally derived for the protection of the environment and, as such, are not appropriate for this evaluation.

RESPONSE 2-13: As discussed in the response to comment 6-2, the FDA levels would still be part of a revised Framework; however, Values would also be derived that would be HARS-specific to be used in determining the suitability of dredged material for use as HARS Remediation Material and the lower value would be governing.

COMMENT 2-14: One reviewer noted that the Matrix values were developed using very conservative methods and, therefore, represent screening values rather than "true" risk levels.

RESPONSE 2-14: EPA Region 2 agrees with the concerns regarding the methods used to derive the Matrix values. However, as discussed in the response to comment 4-1, instead of updating the Matrix values using the previous approach (see USACE 1981) and more current data, EPA Region 2 is proposing to adopt an evaluation approach that is more consistent with the risk-based process for the Matrix constituents.

COMMENT 2-15: One reviewer noted that the Regional Dioxin Values are derived for protection of human health. The reviewer suggested that a value based on upper trophic level ecological receptors (e.g., carnivorous fish or piscivorous birds) would be more appropriate.

RESPONSE 2-15: (see response to comment 5-1).

COMMENT 2-16: One reviewer questioned the validity of the Lee *et al*. (1989) method for determining Water Quality Criterion Tissue Levels (WQCTL).

RESPONSE 2-16: As discussed in the response to comment 8-1, EPA Region 2 proposes to amend the current Framework which uses identified effects-based concentrations for protection of ecological receptors using specific, experimental residue-effects data in place of the WQCTL.

COMMENT 2-17: One reviewer offered specific, editorial suggestions regarding modifications to the TEM (e.g., revisions to Table 1, discussing methods for values below detection limits, etc.).

RESPONSE 2-17: EPA Region 2 will incorporate these suggestions, as appropriate, into the revised Framework.

Charge Group: Framework

Charge No. 3. In conducting the integrated effects evaluation using the types of data provided by the applicant, which of the eight factors for LPC compliance listed in the Green Book are appropriate and relevant? How can a quantitative/strategic framework be established to evaluate tissue data for those factors? Considering that comparison to regional Matrix values and site-specific risk values represent case-specific evaluations, is it necessary to conduct the integrated effects evaluation of the bioaccumulation results? *(Please see Reference No. 61, page 6-6)*.

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 3 Comment (3 para):
Factors 1 and 5 are of little use since only two species are considered in the 28-d tests. Certainly, exceedence of a standard by two rather than one species is of greater concern, but that quantification is not an appropriate evaluation of "phylogenetic diversity." Factor 2 is of limited utility because the reference comparison is meant to provide a quick evaluation of very clean material. Many chemicals could exceed the reference, but not be a problem if their concentrations are all less than other benchmarks. Factor 3 is difficult to assess because the reference concentrations may be extremely low and the magnitude of exceedence becomes a function of the precision of analytical chemistry. Factor 4 is difficult to assess because all of the chemicals of Table 1 are toxicologically important if their concentration is high enough. Factor 6 (biomagnification) is an important consideration, especially in comparison to a reference or other standard that is not effects-based. Factor 7 is a separate evaluation from the bioaccumulation analysis.

Factor 8 is most relevant to the assessment of LPC compliance. However, as discussed in the answer to questions 1 and 19, the comparison should be to a background based on tissue concentrations observed in 28-d bioaccumulation tests of sediments, rather than tissue concentrations in species living in the vicinity of the disposal site.

I think the integrated assessment boils down to a consideration of the number of chemicals whose concentrations are close to a critical benchmark. If just one of the Table 1 chemicals exceeds a critical benchmark, the project material is not acceptable for disposal, especially as Remediation Material. The project material should also be rejected if the concentrations of several (e.g. 5) chemicals are close (e.g. within 10%) of their critical benchmark. The "integration" in such an evaluation is based on the known cumulative effects of mixtures of sediment contaminants. Ideally, it would be desirable to quantify that cumulative effect. Unfortunately, there is no known way to predict cumulative effects of diverse chemicals, e.g. Cd + PCB + dieldrin. The EPA/COE cannot ignore cumulative effects because of the lack of a quantitative model. Hence, a rule such as proposed above (5 or more chemicals within 10% of their critical benchmark) is an appropriate basis for finding that a project material is not acceptable for disposal at the HARS.

Gentile Charge No. 3 Comment (4 para):
It is interesting that you are asking the peer-reviewers this question when that was exactly what I referred to in my comment on the suitability of the Framework. I was anticipating that you would provide an example of the application and then ask our evaluation rather than the other way around. Nevertheless, there are several ways to stratify the 8-Factors into a decision framework based on some set of criteria as long as it is recognized that these are inter-related to varying degrees. One approach is to separate the eight into 1st and 2nd order factors. Personally if I were a manager, I'd like the 1st-order factors to tell me if I have a potential problem. These could include: the toxicological importance of the contaminants; the magnitude of the bioaccumulation in any one or more species; and the propensity to biomagnify in food webs. The latter is particularly important since most of the effects we see are detected in top predators be they mammals, birds, or reptiles.

Given I have a potential problem I'd want to know how serious it was. The 2nd-order factors provide supporting evidence to reduce potential uncertainty and further define the magnitude and extent of the problem. They could include the number of species; number of contaminants; phylogenetic diversity; the magnitude to values for species in the disposal site.

Personally, I do not think the second question is an appropriate one for a peer-review, that is develop a quantitative strategic framework using these eight factors. That was and should be the responsibility of EPA/COE and its contractors. Having said that my colleagues and I have developed an ecological significance decision framework using several factors that might be relative though we did not attempt to quantify it that is due to be published in the August issue of Human and Ecological Risk Assessment (HERA). I'd be happy to provide a reprint when available.

I do think there are situations where there is a need to conduct the Integrated Effects Evaluation (IEE). For example, the Regional Matrix only accounts for two metals and two organics - what about the other contaminants. Second while the Risk Evaluation using WQCTLs expands the contaminants it really is based on national and not site-specific conditions. Personally, I like the IEE because it would provide considerably more information than the "point estimates" that would reduce the uncertainty associated with decisions. I'd suggest the next step is to develop a strategic plan for constructing a quantitative decision-analysis framework for using the 8-factors or more or less as need be.

Berry/Lake/Pruell Charge No. 3 Comment (2 para):
All eight factors appear relevant to a degree. The application of the seventh factor (the extent of toxicity" seems a bit problematical, because if the sediments are acutely toxic there may be no need to do bioaccumulation testing (because the sediment will fail based on toxicity alone). At the same time, I imagine most resource managers would be more comfortable with allowing a sediment causing a small amount of bioaccumulation to pass, if the sediment was not toxic. Similarly, most managers would probably be more comfortable failing a sediment, based on bioaccumulation, if there was also some toxicity associated with the sediment.

It is not clear how these factors could be put into a quantitative system. Ostensibly such a system, which might involve scoring and weighting of the individual factors, would be less arbitrary. However it would probably serve only to put the arbitrariness up front, instead of when the assessment is made.

It seems important to do an integrated effects evaluation of the bioaccumulation result because most of the other evaluations relate to single compounds or single classes of compounds, and do not relate results with those in the toxicity tests.

Solomon/Sibley Charge No. 3 Comment (2 para, 1 table):
The eight compliance factors in the "Green Book" (USEPA 1991) are all reasonable but some are more biologically relevant than others. A discussion of this is summarized in the table below:

Green Book Criterion	Biological relevance	Use-fulness*
Number of species from dredged material in which bioaccumulation exceeds reference (statistical test).	Based on difference from a reference material. No toxicological relevance assessed. Incorrect choice of reference material could confound the results.	T
Number of bioaccumulated contaminants from dredged site in which exceed reference site values (statistical test).	As above, based on difference from a reference material. No toxicological relevance assessed. Incorrect choice of reference material could confound the results.	T
Magnitude by which bioaccumulation from dredged material exceeds that from reference.	More useful as it is a continuous variable, however, the response of organisms to increasing concentration (concentration response) would need to be factored in as well.	TT
Toxicological importance of contaminants from dredged site exceeding those from reference site.	Again, this is based on difference from a reference material. No toxicological relevance assessed and the importance of these contaminants is judgemental. Incorrect choice of reference material could confound the results.	T
Phylogenetic diversity of contaminated species exceeds that from reference site.	Phylogenetic diversity may not be relevant to ecological importance or function in the ecosystem, however, this may be an indicator of greater potential for entry to food chain. Diversity may be affected by physical factors such as particle size.	TT
Propensity for contaminants with statistically significant bioaccumulation to biomagnify in aquatic food chain.	Biomagnification usually only occurs with persistent and lipid soluble substances. These may have a greater impact in organisms higher on the food chain (as has been demonstrated historically) and this is judged to more useful.	TT
Magnitude of toxicity and phylogenetic diversity of organisms showing greater mortality in dredged material.	A good effect-based criterion that is related to response of organisms. It may, however, be confounded if incorrect matching of test and reference sediment is used. Some organisms will not thrive and "die" if sediment physical characteristics are not appropriate.	TTTT

Green Book Criterion	Biological relevance	Use-fulness*
Magnitude by which contaminants whose bioaccumulation from dredged site exceeds that in organisms near the proposed site.	Some usefulness but subject to confounding from poor choice of nearby sites. A good margin of safety may exist at both sites despite the differences.	T

* the more useful, the more **T**s

The response of the organisms at the site will, to a degree, integrate the effects evaluation. Other types of toxicological integrators (TEFs and TEQs) are less well developed. If site-specific values are used, an attempt to should be made to integrate the effects evaluation of the bioaccumulation results, however, the biological responses highlighted above should be given higher credence in the assessment. In this regard, physical properties of sediments may be more important than chemical properties. Some sediments are unsuitable substrates for colonization by some organisms and, absence of these organisms does not mean an adverse toxic effect. Choice of the wrong sediment as a reference could result in false positives (for toxicity). In the assessment being reviewed here, this was not the situation.

Newman Charge No. 3 Comment (para):

Subanswer One: In conducting the integrated effects evaluation using the types of data provided by the applicant, which of the eight factors for LPC compliance listed in the Green Book are appropriate and relevant? These eight factors being:

(i) Number of species in which bioaccumulation from the dredged material is statistically greater than bioaccumulation from the reference material,

(ii) Number of contaminants for which bioaccumulation from the dredged material is statistically greater than bioaccumulation from the reference material,

(iii) Magnitude by which bioaccumulation from the dredged material exceeds bioaccumulation from the reference material,

(iv) Toxicological importance of the contaminants whose bioaccumulation from the dredged material statistically exceeds that from the reference material,

(v) Phylogenetic diversity of the species in which bioaccumulation from the dredged material statistically exceeds bioaccumulation from the reference material,

(vi) Propensity for the contaminants with statistically significant bioaccumulation to biomagnify within aquatic food chains,

(vii) Magnitude of toxicity and number and phylogenetic diversity of species exhibiting greater mortality in the dredged material than in the reference material,

(viii) Magnitude by which contaminants whose bioaccumulation from the dredged material exceeds that from the reference material also exceed the concentrations found in comparable species living in the vicinity of the proposed disposal site.

It is my opinion that they are all relevant in the context they are presented. Their relative importance can be crudely ranked: 1=2=3=4=6>8>7>5.

Subanswer Two: How can a quantitative/strategic framework be established to evaluate tissue data for those factors? The impression given in the Green Book is that these items are to be used in the context of expert opinion, not a structured decision flow chart. I would use review by experts, perhaps

based on the crude rankings of priority provided above, as a means of incorporating these aspects of the assessment.

Subanswer Three: Considering that comparison to regional Matrix values and site-specific risk values represent case-specific evaluations, is it necessary to conduct the integrated effects evaluation of the bioaccumulation results? Yes. I believe that the integrated effects evaluation provides applicable information, although it would be much more useful if it did not rely so heavily on the 28 day bioaccumulation test and associated extrapolations. According to the information provided (e.g., ref. 57), regional matrix values exist for only a subset of contaminants. (Please note that I did not find ref. 61 in my review package so I could not respond specifically using information from page 6.6 as suggested.)

Lee Charge No. 3 Comment (2 para):
All eight factors are of some importance. The "phylogenetic diversity of the species" is the least important, as with the current procedure of using two species it is really a restatement of "the number of species".

See Question 1 for a possible approach for a framework to integrate various compounds. It is still important to have an integrated assessment because: 1) Matrix values exist for so few compounds, 2) the effects of at least some suites of contaminants are based on the summation of their residues (e.g., PAHs), and 3) the acceptability of a dredge material is based on different types of risks (e.g., direct impacts on benthos, biomagnification to human consumers) which requires some type of overall assessment.

Dillon Charge No. 3 Comment (1 para):
All eight factors appear very appropriate and relevant. One may wish to keep consideration of the factors qualitative. A simple visual tool such as a matrix with pluses and minuses may be all that is necessary. This visual level of analysis (with accompanying uncertainty analysis) may even be most appropriate given the uncertainties associated with the multiple lines of evidence. Yes, the integrated effects evaluation is very necessary. For example, it is the only time: 1) the potential effects of multiple contaminants are evaluated and 2) results of the sediment toxicity bioassay are considered in conjunction with the bioaccumulation data.

Agency Response to Reviewers' Comments:

COMMENT 3-1: There was general agreement among the reviewers that the integrated effects evaluation is a necessary component of the analysis, although opinions differed regarding the most appropriate method for incorporating it. The majority of the reviewers suggested that a quantitative approach based on a relative ranking or weighting scheme (e.g., a toxic units approach) should be developed. This approach provides a way to evaluate the dredged material as a whole by normalizing each compound to the fraction of the effects residue concentration (0.X of a CBR or WQCTL residue). The fraction could then be summed based upon mode of action or total for all compounds. Two reviewers, however, felt that the evaluation should be a qualitative review based on expert opinion, not a structured decision flow chart. The majority of the reviewers felt that all eight of the factors described were, to some

degree, applicable to the integrated effects assessment. Several reviewers felt the factors should be prioritized and weighted accordingly; however, they disagreed on the relative importance of each individual factor.

RESPONSE 3-1: EPA Region 2 recognizes and acknowledges that the eight factors for LPC compliance listed in the Green Book (EPA/USACE, 1991) vary in their significance and importance for different individual compounds. EPA Region 2 also recognizes that there are numerous methods for evaluating the eight factors for LPC compliance, and numerous methods for evaluating the integrated effects. The eight factors listed in the Green Book (EPA/USACE, 1991) are interrelated in a complex manner, which is one reason why no one particular evaluation method was identified. Depending on the project, there may also be additional factors that should be considered.

As discussed in response to comment 1-1, EPA Region 2 will propose HARS-Specific Values that are human and ecological effects-based values from the best available literature and using standard EPA human health risk evaluation methodology (see responses to comments 4 through 14. The evaluation of additive effects (chemicals that act similarly) on the dredged material as a whole would be addressed through a more complete and rigorous application of the CBR (see response to comment 8), total carcinogenicity (see responses to comments 7 and 11), and Non-Cancer Hazard Index (see response to comment 7) in the section entitled "Combined Effects Evaluation: CBR, Total Carcinogenicity, and Non-Cancer Hazard Index (See Figure 2). EPA Region 2 believes that the proposed HARS Framework in its entirety (i.e., water column evaluation, toxicity tests, and bioaccumulation tests) incorporates the eight factors for LPC compliance listed in the Green Book (EPA/USACE, 1991). However, in place of direct application of the eight Green Book Factors, an integrated assessment of combined additive/synergistic effects has been proposed to evaluate bioaccumulation test results.

COMMENT 3-2: One reviewer commented that EPA Region 2 cannot ignore cumulative effects because of the lack of a quantitative model and suggested an additional process be used to determine whether material is unacceptable for the ocean, besides exceeding one of the critical benchmarks. This involves rejecting project material if the concentrations of several (e.g. 5) chemicals are close (e.g. within 10%) to their critical benchmark. This would also help to address cumulative effects, given that there are no known quantitative models to predict cumulative/synergistic effects of chemical mixtures.

RESPONSE 3-2: EPA Region 2 acknowledges the reviewers suggestion. However, as discussed in responses to comments 3-1 and 1-1, EPA Region 2 will propose chemical-specific, HARS-Specific Values to be used in determining the suitability of dredged material for use at the HARS as Remediation Material. These proposed HARS-Specific Values will be protective and appropriately conservative for remediation purposes at the HARS and will, to the maximum extent practicable and appropriate, factor in the eight factors for LPC compliance listed in the Green Book. The evaluation of additive effects (chemicals that act similarly) on the dredged material as a whole would be addressed through a more complete and rigorous application of the narcotic critical body residue (CBR) approach (see response to comment 8), total carcinogenicity (see responses to comments 7 and 11), and Non-Cancer Hazard Index (see response to comment 7). As such, step "d" in the Framework, "Evaluation of Solid Phase Bioaccumulation Results for Dredged Material as a Whole", has been revised in the proposed Framework to reflect the new evaluation process by removing the specific evaluation of the eight factors for LPC compliance listed in the Green Book.

COMMENT 3-3: One reviewer commented that the development of a quantitative/strategic framework to evaluate tissue data for the eight factors for LPC compliance listed in the Green Book is the responsibility of EPA. This reviewer also suggested using the ecological significance framework published in August 1998 for Human and Ecological Risk Assessments (Gentile, 1998; Gentile and Harrwell, 1998a).

RESPONSE 3-3: EPA Region 2 acknowledges the reviewers suggestion regarding development of ecological endpoints and ecological significance in risk assessments and environmental policy. See response to comment 1-2 regarding the use of risk assessment.

COMMENT 3-4: One reviewer commented that the integrated effects evaluation should evaluate the cumulative effect of the chemical mixtures present in the dredged material rather than the possible impacts from individual chemicals. Another reviewer supported the integrated effects evaluation because it would provide considerably more information than the "point estimates" and would reduce the uncertainty associated with decisions.

RESPONSE 3-4: EPA Region 2 agrees with the reviewers suggestion. As discussed in response to comment 3-1, the evaluation of additive effects (chemicals that act similarly) on the dredged material as a whole would be addressed through a more complete and rigorous application of the narcotic CBR approach (see response to comment 8), total carcinogenicity (see responses to comments 7 and 11), and Non-Cancer Hazard Index (see response to comment 7).

COMMENT 3-5: One reviewer noted that the interpretation of some of the factors could be significantly affected by confounding factors associated with physical parameters of the sediment (grain size, sediment type, choice of wrong sediment as reference site) rather than chemical constituents, leading to potentially erroneous conclusions regarding the potential toxicity of the dredged material.

RESPONSE 3-5: EPA Region 2 agrees with the reviewer that confounding factors need to be considered in the application of test results, where applicable. Towards that end, EPA Region 2 has rigorous toxicity testing quality assurance requirements. The RTM has specific requirements for QA/QC procedures for sediment sampling and toxicity/bioaccumulation testing. Also, as discussed in response to comment 3-1, the proposed HARS Framework, in its entirety, incorporates the 8 factors listed in the Green Book.

COMMENT 3-6: One reviewer suggest an approach to separate the eight Green Book factors into 1st and 2nd order factors and that the next step is to develop a strategic plan for constructing a quantitative decision-analysis framework for using the 8-factors.

RESPONSE 3-6: (see response to comment 3-1)

Charge Group: Benchmark and Risk Evaluation Values

Charge No. 4A. Regional Matrix Values
Are the Matrix values suitable for determining the suitability for placement at the HARS as Remediation Material?

Charge No. 4B. Regional Matrix Values
Regional Matrix values were developed in 1981 by compiling available field data for mercury, cadmium, PCBs, and total DDTs. Were these values derived appropriately for their intended use? Based on current data sets and scientific literature, are these 1981 values suitable for predicting the significant undesirable effect due to bioaccumulation? *(Please see Reference No. 57)* If not, identify more current references, data sets, and/or actual chemical specific values that would be more appropriate.

Summary of Peer Review Comments

Individual Comments

Gentile Charge No. 4A Comment (1 para):
The fundamental problem with all point estimates or benchmarks is that they have no estimates of variability around them so there is no measure of uncertainty related to a decision derived from their application. Since a fundamental element of risk-based analysis and risk-management is decision-making in the face of uncertainty, then measuring probabilities and uncertainties would seem not only logical but also necessary. From a managers perspective I=d want to know whether the 2x, 3x or 10x exceedence was real or within the both the biological and analytical variability. I=d certainly only use it as a screening tool and not a decision tool. However, if the policy has and continues to be based on point estimates (e.g., criteria, benchmarks, etc.) then the Matrix values are sufficient.

Berry/Lake/Pruell Charge No. 4A Comment (1 para):
The matrix values seem to be a suitable tool for use as a part of an evaluation, although they would probably not be suitable if other methods were not employed (But see 4B). Exceedances of these values may serve as an appropriate screen, but obviously they can not be used to calculate risk. If the matrix values are used, It might be argued that a grand mean is too low. However, this must be balanced by the fact that the exposure in the test is only 28 days, and that an arithmetic mean might be particularly susceptible to being skewed by "hot spot" values. Depending on the statistical distribution of the data, a geometric mean, or the median, might be more appropriate.

Solomon/Sibley Charge No. 4A Comment (1 para):
The matrix values are generally judged suitable for determining the suitability for placement as Remediation Material. Values for dioxin TEFs (FDA and USEPA) are based on mammalian studies. They are thus most suitable for assessing risk to humans (and other mammals). For assessing risks to fish,

TEFs based on data from fish may be more useful (Parrott *et al.* 1995). However, given the observed concentrations, this difference was not judged to be significant.

Newman Charge No. 4A Comment (1 para):
Answer based on Ref. 57 and the Peer Review Version. Review of Compliance with the Testing Requirements of ...: Yes. The values seem appropriate as gross estimates if used as originally planned. "This guidance is considered by the Corps to be dynamic. It is not established as fixed and final numerical criteria but rather as an aid to interpreting test results. As such, it is open to review and updating as additional pertinent data become available" (Ref. 57, 1981).

Dillon Charge No. 4A Comment (1 para):
Matrix values represent 1981 conditions. If the goal of the EPA/CENAN dredging program is to manage towards that level of ambient contamination, then the values appear appropriate.

Gentile Charge No. 4B Comment (1 para):
I was peripherally involved with this process in the 1980's while with EPA and directing a Field Verification Program with COE. At that time the derivation the concept and its limitations were debated and the consensus was that given the state of the science this would be a useful tool for screening the potential ecological effects of contaminant tissue residues in biota. I don't think at that time, we considered this a predictive tool nor do I now for that matter. The idea was that if you exceed this value then it would trigger the need for further investigation. Among the issues were whether one should DDT and PCBs in toto or look at their individual congeners. The latter was not deemed possible at that time for a several reasons; lack of data on specific congeners, reliability of analytical methods for congeners, and a lack of congener specific effects data or even potency data at that time.

I would assume that in the intervening time (1981-1998) that three things would have happened: 1) the original data base has been expanded as new data became available thus providing a much sounder scientific basis for decisions including data on PCB congeners; 2) the concept would have been expanded to include additional contaminants, particularly the PAHs; and, 3) the efficacy of the original Matrix would have been evaluated as a management tool. Not having been involved with this area of research since the mid-eighties it is difficult for me to judge but that is certainly at the heart of your last questions. That you are suggesting Reference No. 57 as supporting information suggests that little further research has gone into refining and re-defining the Regional Matrix. If the plan is to continue using this approach, I would encourage the Matrix concept be more risk-based, that is, use distributions, probabilities, and uncertainties in the decision-making framework. Further, Reference No. 57 highlights the problem of using a "dated" strategy when it concludes that there is no evidence that methylmercury is not a threat to bioaccumulate and biomagnify.

Berry/Lake/Pruell Charge No. 4B Comment (1 para):
It is not clear to me why the matrix values would continue to be used, now that the background tissue values are available. The background values use newer analytical techniques and come from a more defined database.

Solomon/Sibley Charge No. 4B Comment (1 para):
The relevant decision guideline limits for mercury, cadmium, PCBs and total DDT were reviewed and were, in all cases, judged to be appropriate. For cadmium, total PCBs, and mercury, these decision guidelines were all below guideline levels developed in other jurisdictions and which incorporated appropriate safety factors. Based on the arguments presented for the decision guideline value for DDT, this value is also judged appropriate. Although this reviewer is aware of some more modern studies on DDT (such as enhances breakdown in marine sediments), the results of these would not justify more conservative decision criteria values.

Newman Charge No. 4B Comment (1 para):
The information gives general estimates as intended. As noted above, extensive review and augmentation of this information should be done. Since the evaluation was last conducted in 1981, a new review is due.

Dillon Charge No. 4B Comment (1 para):
The 1981 matrix values were derived appropriately for their intended use at the time. Whether the 1981 matrix values are appropriate today depends on regional management goals (See peer reviewer's response to 4.A.). If the goal of the EPA/CENAN dredging program is to manage towards present day levels, then the more recent background concentrations contained in reference 98 may be more appropriate.

Agency Response to Reviewers' Comments:

COMMENT 4-1: There was a consensus among several of the reviewers that the Matrix values were appropriate for the intended use when they were developed but should be updated to reflect current scientific knowledge. Several reviewers suggested different methods for deriving them including a probabilistic approach to reflect the range and distribution of the data used to derive the values and the use of other measures of central tendency (e.g., geometric mean or median). One reviewer in particular felt that the Matrix values as derived were inappropriate, noting that they were calculated from surveys that: (a) did not have standardized methods, (b) were not peer-reviewed, and (c) did not include the organisms used in the 28-day tests, and (d) presented no measure of uncertainty.

RESPONSE 4-1: EPA Region 2 agrees with concerns expressed by the reviewers that the Matrix values are based on dated information and should be updated with values more pertinent to the HARS. However instead of updating the Matrix values using the original approach (see USACE, 1981), EPA Region 2 proposes to replace the Regional Matrix Values with HARS-Specific Values that are derived using a risk-based approach. EPA Region 2 proposes to use this strategy because it is more applicable for determining the potential to cause significant undesirable effects.

Matrix values were originally developed to prevent "...unreasonable degradation by preventing increased environmental stress in the Bight from dredged material." and is a "...decision guideline which specifies a maximum value...which, if met, should prevent significant undesirable effects from occurring..." and were not intended as "fixed and final numerical criteria" but rather were proposed to be "open to review and updating as additional pertinent data become available." (USACE, 1981).

New risk-based HARS-Specific Values will be proposed that are based on human health carcinogenic and non-carcinogenic effects, and ecological effects. Cancer effects values will be based on 10-4 cancer risk level and non-cancer effects values will be based on reference dose. Ecological effects values will be based on tissue contaminant levels that have been demonstrated to be associated with adverse ecological effects.

COMMENT 4-2: Two reviewers indicated that the Matrix values should be applied only as a conservative benchmark screen. These reviewers suggested that sediments exceeding the Matrix values should be further evaluated through a risk-based framework.

RESPONSE 4-2: As presented in response 4-1, EPA Region 2 proposes to replace the current Regional Matrix values for PCBs, DDT, cadmium, and mercury with revised, risk-based HARS-Specific Values. Please note that while the reviewers discuss comparing the current Matrix value against sediment chemistry values, Matrix values are actually compared against concentrations in bioaccumulation test organism tissue consistent with current regulations.

COMMENT 4-3: Two reviewers suggested that the Matrix value approach be replaced with the use of background tissue values which are based on newer analytical techniques and a more defined database.

RESPONSE 4-3: See Introduction and responses to comments 4-1 and 1-4. EPA Region 2 proposes to replace the Regional Matrix values with risk-based, HARS-Specific Values.

Charge Group: Benchmark and Risk Evaluation Values

Charge No. 5A. Regional Dioxin Values

Currently, the presence of 2,3,7,8-TCDD at a detectable concentration (i.e., greater than or equal to one part per trillion (pptr)) in tissues of organisms exposed to dredged material precludes its classification as Category I (hence Remediation Material); presence of the remaining dioxin/furan congeners, at concentrations of TEQs equal to or greater than 4.5 pptr, results in a similar conclusion. When 28-day tissue concentrations exceed these values, is there sufficient cause to conclude that placement of the material is not suitable as HARS Remediation Material? If not, what levels indicate sufficient cause for this conclusion? *(Please see Reference No. 89)*

Charge No. 5B. Regional Dioxin Values

Are dioxin values suitable for predicting the significant undesirable effects due to bioaccumulation? If not, should these values be based on a risk analysis paradigm in which the size of the human population subgroup potentially exposed through intentional behavior is compared to the size of the general population in the region? Since the primary route of exposure is through consumption of fish and shellfish, should the variability in potential exposure due to differences in fishing behavior (e.g., target species, seasonal preferences) be incorporated in the risk paradigm? How would a benchmark protective of human health compare to benchmarks determined using an ecological risk analysis paradigm for resident fish and piscivorus wildlife?

Summary of Peer Review Comments

Individual Comments

Bartell Charge No. 5A Comment (1 para):

The dioxin/furan criteria for sediment classification as Category I are based on detection levels, as discussed in Reference 89. The comparative paucity of dioxin and furan toxicity data for species representative of the marine benthos make it difficult to evaluate the efficacy of the 1 and 4.5 ppt criteria. For example, the assumption of using ½ of the detection limits to compute the sum of the non 2,3,7,8,-substituted compounds leads to the 4.5 ppt criterion; clearly, increasing or decreasing this apparently arbitrary value (i.e., ½) would correspondingly increase or decrease the permissible concentration for Category I classification. A dioxin value of 10 ppt (i.e., Category 3 in Reference 89) might prove sufficiently protective, although the necessary supporting toxicity studies should be performed with at least the species of *Nereis* and *Macoma*. Additional studies appear warranted given that the decision criteria were evidently developed on the basis of tissue levels for fish and animals, not including these representatives of the benthos. It is further pointed out (Reference 89) that the many of the pathway coefficients, for example, the trophic level transfer coefficient, were conservatively (i.e., pessimistically) defined in the assessments used to derive the protective criteria for these compounds. More realistic values would, of course, lead to higher permissible concentrations. Cook *et al.* (1993), cited in Reference

89, suggest a value of 50 ppt as a "low risk" concentration for adverse effects on fish. While additional studies appear needed to justify the classification criteria for dioxins and furans, the information summarized in Reference 89 suggests that a value in the range of 10-50 ppt might be just as logically selected as the current criteria based on detection levels or fractions of detection levels.

Gentile Charge No. 5A Comment (1 para):
Reference No. 89 provides a good summary of the policy and approach for evaluating dioxin risks Ð though it is not a risk assessment *per se* for all the reasons I've discussed above. Using point estimate benchmarks or criteria results in a hazard index type of framework and not a probabilistic-based risk framework. Nevertheless the review was informative and basically sets up an hazard index with three benchmarks; ²1, 1-10, and >10pptr. Two concerns with this approach one is with the philosophy supporting the policy and the second with implementation. First, is that TEQ value of 4.51 is based on the sum of ½ the detection limits for the non-2,3,7,8 Ð substituted dioxin/furans times the individual TEQs. Making policy decisions at the detection limits is problematic at best. Second, how does one decide if exceeding the 1 pptr in a 28-day test is cause for concern given that your decision framework has no way to treat variability and uncertainty. What if the a single 28-day test results in a value of 1.5 or 1.9 or even 2.6 how does one decide if this is really a problem or it is within the variability of the bioaccumulation testing and analysis methods itself particularly when as stated the values are at the detection levels. What needs to be determined is the amount of variability around the benchmark that is acceptable based upon the consequences to human health. To answer that question I'd determine how much exceedence is statistically significant and what are the consequences of that magnitude of exceedence, that is, what is the incremental health and environmental risk. If the exceedence is statistically significant and above, 3.6 for example, then the health consequences will equal or exceed accepted risk criteria deeming the material unacceptable. Some analyses like this would appear to be necessary to answer the suitability question.

Berry/Lake/Pruell Charge No. 5A Comment (1 para):
Dr. Pruell felt that there are not enough data to support or refute any of the values proposed for dioxin at this time.

Solomon/Sibley Charge No. 5A Comment (1 para):
The 1 ppt criterion for TCDD and the 4.5 ppt criterion value for TEQ of the dioxins and furans other than TCDD is based on the use of a number of safety factors and conservative assumptions. It is well known that criteria for dioxin vary widely from one jurisdiction to another and even between agencies in the same country. The EPA criterion is one of the most conservative while that of the FDA (20 and 50 ppt) is in the middle of the range. Given that trophic transfers are not unity, values similar to those suggested by the FDA would be more appropriate.

Newman Charge No. 5A Comment (1 para):
I would tend to agree with the present decision process.

Cook Charge No. 5A Comment (3 para):

In candor, I must state my reservations for the perpetuation of an unnecessary and potentially inaccurate basis for bioaccumulation assessment of dredge spoils. A basic scientific problem often occurs when methods evolve over many years without a fresh look, in the context of the present state of science, at the fundamental models, data, and assumptions that were incorporated into them in the beginning. Perhaps this is happening in this case with a method predicated on the need to test each sediment for bioaccumulation. The use of a 28 day benthic invertebrate test of bioaccumulation, especially for assessing human health risks, seems to me to be a clumsy and scientifically indefensible approach. The result of the test is a concentration in the organism that may be influenced by test conditions and has little relevance to human or wildlife dietary exposures. More importantly, what factors other than sediment organic carbon content are likely to cause a significant difference in TCDD bioavailability from different sediments to the test organisms? Most food chain models are successful with an assumption of equilibrium partitioning to a benthic invertebrate species. Bioaccumulation potential can be better determined on the basis of the sediment organic carbon normalized concentration of the chemical and some standard condition assumptions for the magnitude of food chain transfer from the sediments to the fish or other organism that is either the subject of the risk assessment or the diet of the subject.

Another complication is that TCDD and the other congeners included in the TEQ analysis all have different bioaccumulation potentials in food chains. Bioavailabilities differ in proportion to hydrophobicities (K_{ow}) and once accumulated by fish, they are subject to varying degrees of metabolism. The framework uses reference 89 to document the calculations used to arrive at categories 1, 2, and 3 for dioxin risks. A trophic transfer factor of 1.0 is used for TCDD and, presumably, for all other congeners. The Great Lakes Water Quality Initiative/Guidance Technical Support Document for the Procedure to Determine Bioaccumulation Factors contains bioaccumulation equivalency factors (BSAF ratios) that reveal lower bioaccumulation potential in comparison to TCDD for all PCDDs and PCDFs except 1,2,3,7,8-PeCDD and 2,3,4,7,8-PeCDF. Perhaps more importantly, this EPA dioxin criterion document provides BSAFs for fish that would be **far better estimates of the $EMFC_{ss}$** used in the Region 2 dioxin risk evaluation than the benthic invertebrate test values. The equation for calculating the $EMFC_{ss}$ on a TEQ basis is:

$$EMFC_{ss} = \sum_{i=1}^{n} (C_{soc})_i \ (BSAF)_i \ (f_R) \ (TEF)_i$$

where:

$(C_{soc})_i$ is the concentration of congener i in sediment normalized to organic carbon
$(BSAF)_i$ is the fish biota sediment accumulation factor for congener i
f_R is the fraction lipid in the fish
$(TEF)_i$ is the toxicity equivalence factor for congener i (WHO human health/mammal, fish, or bird TEF depending on species at risk) for TCDD alone: n = 1, TEF = 1, BSAF = 0.06

The use of BSAFs from the Great Lakes may seem ridiculous, but they are probably no more than slightly conservative (slightly over-predictive of bioaccumulation) for fish living on a disposal site and may actually predict lower dioxin risk than the present procedure while allowing for a more accurate and straightforward TEQ analysis. The Great Lakes BSAFs are typical for sediment/benthic food chain-driven bioaccumulation with small contribution from chemicals in the overlying water, as one would expect to be the case for dredge spoil disposal area conditions. One additional complication is that a TEQ analysis that ignores the contributions of PCBs assumes that a significant underestimation of risk is acceptable.

Bartell Charge No. 5B Comment (1 para):
Ideally, predictions of significant undesirable effects from dioxins and furans would derive from comprehensive, quantitative environmental transport, bioaccumulation, and toxicity assay data. In the absence of these necessary studies, dioxin criteria for sediment classification should at least be developed using quantitative risk assessment methods that emphasize variability and uncertainty in all components of the analyses. Potential differences in exposure among human population subgroups, as well as variability in routes of exposure would logically be addressed in such analyses. The implications of these sources of uncertainty and variability could be effectively explored using Monte Carlo methods, interval analysis, fuzzy arithmetic, or other analytical tools that characterize uncertainty and propagate them through the computations.

Gentile Charge No. 5B Comment (1 para):
Reference 89 addresses the first question in some detail, however, I am not convinced that the current method has real power for predicting undesirable effects but rather provides useful tool for establishing policy boundaries. A risk-based approach would be much more realistic particularly if based upon site-specific information such as at risk human sub-populations, different dietary intakes, fishing behavior, etc. Comparison of health and wildlife benchmarks would have to wait until the specific analyses were done. However, my suspicion is that the use of the risk paradigm and site-specific wildlife information would produce a more defensible and robust risk assessment that would have less uncertainty than the health assessment.

Berry/Lake/Pruell Charge No. 5B Comment (1 para):
Dr. Pruell also felt there was a need to use a risk approach to the assessment of dioxin in tissues, and that considerations as to the amount of material to be dumped, and the characteristics of the dump site may be more important in this case than the tissue value chosen for dioxin.

Solomon/Sibley Charge No. 5B Comment (1 para):
Dioxin values are suitable for assessing the hazards resulting from bioaccumulation (with the above qualifiers taken into consideration). However, for risk assessment purposes, the likelihood of exposure in the potentially exposed population should be considered. The likelihood of consumption of contaminated seafood should incorporate seasonal and target species variability as well as the likelihood that fish will be obtained from other regions that may be less contaminated (if this is the case). If these factors are considered, exposures will normally be reduced, thus further adding conservatism to the risk assessment. Human health risk assessment is normally aimed at protection of the individual, and, because of this, usually incorporates many conservatisms. Ecological risk assessment is focused on endpoints at the population level rather than the individual. Thus, risks to fish and piscivorous wildlife would be assessed differently from those to humans. Criteria based on human consumption would be expected to be protective of wildlife.

Newman Charge No. 5B Comment (3 para):
Subanswer One: Are dioxin values suitable for predicting the significant undesirable effects due to bioaccumulation? Yes. They seem suitable.

Subanswer Two: Since the primary route of exposure is through consumption of fish and shellfish, should the variability in potential exposure due to differences in fishing behavior (e.g., target species, seasonal preferences) be incorporated in the risk paradigm? Yes. I think that at a certain tier in the risk assessment, more detailed information would be useful. The present analysis seems to focus on the general population but, perhaps, should also consider subpopulations of humans with higher consumption rates of fish and shellfish. Inclusion of fishing behavior as mentioned above could then be added to the analysis.

Subanswer Three: How would a benchmark protective of human health compare to benchmarks determined using an ecological risk analysis paradigm for resident fish and piscivorous wildlife? The benchmarks for humans should be focused on protecting individuals but those for fish and wildlife should be based on maintaining viable populations. Therefore, the general response would be that the benchmarks for humans should be lower than those for fish and wildlife assuming that one adjusts for differences in exposure characteristics.

Cook Charge No. 5B Comment (4 para):
The Region 2 memo to file by Alex Lechich on 3/15/97 summarizes the dioxin risk evaluation approach. The use of a different toxicity equivalence concentration (TEqC) of 4.5 ppt than the 1 ppt TCDD concentration limit for category 1 seems inconsistent but may be the result of congener detection limits - is 4.5 ppt the theoretical detection limit for TEqC?

The human health criterion used by Region 2 for TCDD is 10 ppt in fish tissue. The TEqC is not defined so one assumes it is also 10 ppt. At a consumption rate of 6.5 g fish for 70 years the cancer risk is about 10^{-4}, a not very conservative risk factor. I believe the World Health organization recently established a daily human dose limit of 1-4 pg TEQ/Kg/day. For a 70 Kg person consuming 6.5 g fish/day, this would amount to TEqC = 10 - 40 ppt in fish. The questions asked of the peer reviewer under 5B are primarily risk management issues (population subgroups; target species differences; definition of significant undesirable effects) and thus are not within the expertise of this reviewer.

The comparison of human health risks and ecological risks is complicated by differences in definitions of risks (individual versus population) and differences in end points (cancer versus early life stage survival). Both the human and ecological risk criteria in this case involve exposure concentrations which are not intended to be exceeded. Implicitly, if one is exceeded and the other is not, the exceeded criterion should determine the classification. If not, there is no need for the ecological risk criterion.

The only definition of an ecological risk criterion is reference to Cook *et al.* 1993 for low risk to fish at 50 ppt. Based on data reported since 1993, early life stage survival of the most sensitive species would require TEqC in eggs/embryos to be less than 10 ppt. This is based on finding a more sensitive species than lake trout and recognition that developmental effects and growth reduction at sub-lethal exposure concentrations likely compromise survival in the environment. On the other hand, it is unlikely that fish species inhabiting disposal sites are the most sensitive species. The potentially greater sensitivity of some birds and mammals to TCDD does not infer greater risks due to the decreased potential for site-specific exposures of free ranging species.

Agency Response to Reviewers' Comments:

COMMENT 5-1: The majority of the reviewers felt that the Regional Dioxin Values are not appropriate for determining suitability for use of dredged material as Remediation Material. In general, the reviewers felt that the values were based on limited data and were likely to be overly conservative. One reviewer also noted that policy decisions should not be based on analytical detection limits. One reviewer had reservations for the perpetuation of an unnecessary and potentially inaccurate basis for bioaccumulation assessment of dredge "spoils." This same reviewer also commented that the use of the 28-day benthic invertebrate bioaccumulation test, especially for assessing human health risks, seems to be "a clumsy and scientifically indefensible approach" and a risk assessment should be used.

RESPONSE 5-1: EPA Region 2 acknowledges that the Regional Dioxin Values are conservative, but disagrees that they are not appropriate for determining suitability for use of dredged material as Remediation Material at the HARS. The Regional Dioxin Values are appropriately conservative to be protective of human health and the environment and to meet the remedial goals of the HARS. Further, the detection-limit strategy employed to derive Regional Dioxin Values for 2,3,7,8-TCDD and total equivalent dioxin toxicity using toxic equivalency actors (TEF's) for the 16 other 2,3,7,8-substituted dioxin/furan compounds will ensure that their use will serve to improve conditions at the HARS.

In recognition of revisions to the Toxic Equivalency Factors (TEFs) recently published by the World Health Organization (WHO, 1998), and in response to a peer review comment to consider the contribution of co-planar PCBs to dioxin-like toxicity; (see response to comment 5-5), EPA Region 2 proposes to consider the contribution of three co-planar PCB congeners (i.e., PCB congeners 77, 126, and 169) that show dioxin-like toxicological activity. EPA Region 2 proposes to recalculate the Regional Dioxin Value for evaluation of total toxicity equivalence of all non-2,3,7,8-TCDD dioxin and furan congeners to include the three coplanar compounds identified by the WHO (1998), using the half detection limit procedure and the TEFs listed by WHO (1998).

The need to remediate the HARS is amply supported by the presence of toxic effects (a Category III sediment characteristic), dioxin bioaccumulation exceeding Category I levels in worm tissue (a Category II sediment characteristic), ER-L/ER-M exceedances in some Study Area sediments, and PCB/TCDD contamination in area lobster stocks. A value greater than 1 pptr of 2,3,7,8-TCDD, used to classify dredged material as Category II on the basis of dioxin contamination, does reflect the best available detection limit achievable at the time that criterion was developed (EPA, 1997). However, the potency of dioxin and the remedial goals of the HARS warrant use of the 1 pptr detection limit (or the TEQ approach for the non 2,3,7,8-TCDD congeners) as the Regional Dioxin Values. Twenty-eight day bioaccumulation test results that equal or exceed the Regional Dioxin Values (worm and/or clam) indicate that the dredged material is not suitable as Remediation Material.

EPA is progressing toward completion of its dioxin reassessment report (*Exposure and Human Health Reassessment of 2,3,7,8-tetrachlorodibenzo-p-dioxin [TCDD] and Related Compounds*), which evaluates dioxin across all programs and media. The "Integrated Summary and Risk Characterization" document, which includes a new draft chapter that addresses toxicity equivalent factors (TEFs) for dioxin-like compounds, will be the subject of an external peer review. A revised draft of the two documents and the revised Dose-Response Modeling chapter are then scheduled to be sent to the Science Advisory Board (SAB) for peer review. The SAB is expected to meet in Fall 2000 with the expectation that the report will be completed by the end of 2000. While the science of the reassessment is undergoing

peer review, and until the evaluation is complete, EPA Region 2 is proposing to continue to use the 1 pptr value for 2,3,7,8-TCDD and the revised total equivalent toxicity as the Category I Regional Dioxin Values. Total toxicity equivalence will be calculated using total equivalent factors (TEFs) for the 16 other 2,3,7,8-substituted dioxin/furan compounds (congeners) and three dioxin-like co-planar PCB congeners. Upon completion of the EPA reassessment, EPA Region 2 will decide what, if any, changes are necessary to the Regional Dioxin Values.

Responses to the recommendation to use risk assessment, particularly with regard to human health are found in the responses to comments 1-2 and 15.

COMMENT 5-2: One reviewer commented on the need to statistically determine the uncertainty and variability associated with the point estimate benchmarks in order to evaluate whether exceedance indicate the potential for significant effects. One reviewer suggested that methods characterizing uncertainty (e.g., Monte Carlo, interval analysis, fuzzy arithmetic, etc.) should be considered.

RESPONSE 5-2: EPA Region 2 agrees with the need to quantify and account for uncertainty in the proposed revisions to the Framework (see response to comment 1-3).

COMMENT 5-3: There was a general consensus among the reviewers that although the dioxin values may be appropriate for evaluating bioaccumulation, risk-based methods are necessary to appropriately address actual human or ecological health effects. The reviewers felt that the approach should consider site-specific aspects of the HARS and should account for potential differences in human subpopulations, such as variability in routes of exposure, dietary intakes, and fishing behavior.

RESPONSE 5-3: EPA Region 2's response to this comment are contained in the responses to comments 1-2 and 15-1.

COMMENT 5-4: One reviewer felt that bioaccumulation tests should not be used at all for dioxins, stating that bioaccumulation potential would be better determined based on organic carbon normalized concentrations in sediment and standard condition assumptions for the magnitude of food chain transfer.

RESPONSE 5-4: EPA Region 2 disagrees as discussed in response to comment 1-4, such an approach would not be consistent with EPA's Ocean Dumping Regulations (40 CFR Part 227) (see response to comment 1-4).

COMMENT 5-5: One reviewer questioned the approach used for the Toxicity Equivalence Quotient (TEQ) analysis, suggesting that the approach used by the Great Lakes Water Quality Initiative/Guidance Technical Support Document for the Procedure to Determine Bioaccumulation Factors would be more appropriate. The rationale for using a different toxicity equivalence concentration (TEqC of 4.5 pptr), rather than the value of 1 pptr derived for TCDD, was also questioned, as was the lack of a TEqC for fish tissue. One peer reviewer indicated a concern for the contribution of co-planar PCB congeners to dioxin toxicity. This same reviewer also indicated that an additional complication is that a TEQ analysis that ignores the contributions of PCBs assumes that a significant underestimation of risk is acceptable.

RESPONSE 5-5: EPA Region 2 acknowledges the suggested alternate approach. However, as discussed in the Introduction and response to comment 5-1, the potency of dioxin and the remedial goals of the HARS warrant use of the 1 pptr detection limit (or the TEQ approach described in EPA (1997) for the non 2,3,7,8-TCDD congeners) as the Regional Dioxin Values. Twenty-eight day bioaccumulation test results that equal or exceed the Regional Dioxin Values (worm and/or clam) would indicate that dredged material could not be used as Remediation Material. Furthermore, in response to the peer reviewers' recommendation to consider the contribution of co-planar PCBs to dioxin toxicity, EPA Region 2 proposes to add three co-planar PCBs to the list of analytes specified for bioaccumulation testing analysis: PCB Congeners 77, 126, and 169. As noted above (see response to comment 5-1), EPA Region 2 proposes to recalculate the Regional Dioxin Value for evaluation of total toxicity equivalence of all non-2,3,7,8-TCDD dioxin and furan congeners to include the three coplanar compounds identified by the WHO (1998), using the half detection limit procedure and the TEFs listed by WHO (1998).

COMMENT 5-6: Regarding the comparison of human health and ecological criteria, one reviewer commented that there should be no need to compare the two because comparison to the more conservative value should determine the classification of the sediment. Otherwise, the assessment should be driven by human health concerns. The reviewer commented that ecological risk criteria for TCDD could range from 10 to 50 pptr based on the available data.

RESPONSE 5-6: EPA Region 2 recognizes that the more conservative (lower) of the human health and ecological values for any given contaminant could be used to determine the suitability of the dredged material for use as Remediation Material. However, EPA disagrees with the assertion that there is no need to compare dredged material test results to both proposed human health and ecological HARS-Specific Values. HARS-Specific Values to be proposed for evaluating the potential for human health and ecological effects differ for individual contaminants and one may not be consistently higher or lower than the other. As such, it is important to calculate and consider both human health and ecological risk in the proposed HARS Framework.

EPA Region 2 disagrees that the ecological risk criterion for TCDD could range from 10 to 50 pptr based upon available data. As stated in EPA (1997):

> "The US Food and Drug Administration (FDA) levels of 25 and 50 pptr for fish consumption, which were recommended in an FDA regional office letter to a state program (FDA, 1981), are not recognized by the FDA as "action levels." They do not have the regulatory standing of the FDA action levels. Although EPA, FDA and US Centers for Disease Control (CDC) use the same mathematical cancer risk model and response data for female rats as reported in a 1978 study by Kociba, *et al.*, EPA (1987) considered 2,3,7,8-TCDD as being 4 times more potent than did the CDC and 9 times more potent than did the FDA (EPA, 1987). The reason for this difference is that EPA (1987) used several more conservative assumptions than FDA in their risk assessment. These include the use of body surface area (as opposed to body weight) in extrapolating the animal dose to humans, and a correction for high early mortality in the animal data, which was not used by FDA. Therefore, EPA does not consider the FDA numbers appropriate for use in these assessments."

EPA Region 2 believes that based on this assessment, the current Regional Dioxin Values are appropriate.

COMMENT 5-7: One reviewer noted that the dioxin toxicity equivalency factors (TEF) currently are based on mammalian studies and suggested that fish data would be more appropriate.

RESPONSE 5-7: EPA Region 2 acknowledges the recommendation of the reviewer. However, the dioxin TEFs referred to are used to assess potential cancer risks to humans. Therefore, a mammalian model is appropriate.

COMMENT 5-8: In their response to charge 10, several peer reviewers indicated that the worm does not attain steady-state tissue dioxin concentrations after 28 days of exposure.

RESPONSE 5-8: EPA acknowledges that the results of long term exposures reported by Pruell *et al.* (1993) suggests that *Nereis virens* does not accumulate dioxins/furans to steady state in 28 days. As described in the response to comment 5-1, EPA Region 2 considers the Regional Dioxin Value of 1 pptr (2,3,7,8-TCDD) to represent a conservative benchmark value for determining the potential for toxic effects, reflecting the best available detection limit at the time the process was developed. The Regional Dioxin Value for the total toxicity equivalence (TEQ) of non-2,3,7,8-TCDD dioxins/furans is similarly based on detection limits, rather than risk. The EPA policy memorandum "Summary of Dioxin Risk Evaluation Approach" (EPA, 1997a) describes the basis for establishing the Regional Dioxin Value. EPA Region 2 used a steady state factor of 4 to derive the dioxin value of 10 pptr in *N. virens* for suitability for ocean disposal. The memo subsequently established, as a matter of policy, that 1 pptr would be used as a threshold for identifying Category I dredged material. Since the decision values have been conservatively developed assuming the time needed to attain steady state, 28-day test concentrations may be compared without adjustment. Therefore, EPA Region 2 does not propose to apply a steady-state correction factor to 28 day-dioxin/furan bioaccumulation results in *Nereis virens* when evaluating the suitability of dredged materials proposed for use at the HARS as Remediation Material.

Charge Group: FDA Action Levels

Charge No. 6A: Are FDA Action Levels useful as upper limit human health benchmarks?

Charge No. 6B: Would the evaluation be improved by omitting comparison of tissue results to FDA Action Levels?

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 6A Comment (1 para):
The FDA Action Levels are much greater than all other comparison data in Table 1, columns 14-20 of the Framework. As a practical matter, they would be likely to have little or no impact on the decision process and are therefore of little use as an upper benchmark.

Gentile Charge No. 6A Comment (1 para):
The FDA Action Levels are of limited value since there derivation is complicated by the addition of factors such as economics and thus are not directly related to a health effect endpoint. That is, they are not effects specific, that is, coupled to teratogenic effects, mutagenic effects, reproductive effects, etc. Thus these values are not a one-to-one equivalent of effects and as such are at best a poor upper bound estimate with not estimate of uncertainty. I recall calculating the incremental risk for PCBs, and Dioxins at the FDA levels and if my memory serves me they were in the 10^{-2} to 10^{0} range which is well above the 10^{-4} benchmark. Thus you could be in compliance with the FDA Action Level and still result in an unacceptable incremental risk for cancer or some other endpoint. To me they give a false sense of security There are other benchmarks out there for evaluating the human health effects of contaminant residues that have been developed by WHO and other countries that might be more useful. Another final reason for not including them is that they are often misinterpreted as being protective of the environment.

Berry/Lake/Pruell Charge No. 6A Comment (1 para):
To the extent that FDA action levels are derived based on human health considerations they would seem to be useful. To the extent that they were driven by considerations of analytical capability and background concentration, or economic considerations, they probably are not useful because analytical techniques have improved, and background concentrations have decreased on at least some of these compounds.

Solomon/Sibley Charge No. 6A Comment (1 para):
As discussed above, FDA action levels for the protection of human health are based on protection of the individual and embody a number of conservative assumptions. They are judged entirely appropriate for the protection of human health. Although the FDA does not consider environmental effects (and some substances may be more toxic to invertebrates and fish than to mammals) the conservative assumptions used in the setting of FDA action levels will likely be protective of fish and shellfish and the function of their populations in the environment.

Newman Charge No. 6A Comment (1 para):
Yes. The FDA Action Levels are useful.

Lee Charge No. 6A Comment (1 para):
The FDA action limits are useful as UPPER limits. They have some regulatory authority and so can be defended as a reason to deny (not pass) a dredge material. The only reason to omit them is if their inclusion causes confusion with applicants or the public that levels below the FDA limits are considered "safe".

Dillon Charge No. 6A Comment (1 para):
See peer reviewer's comment at end of questions regarding human health evaluations.

Swartz Charge No. 6B Comment (1 para):
Comparison to the FDA Action Levels is included as part of the Green Book evaluation process and appears to be required by the Dredged Material regulations. Thus, the comparison may be needed as a matter of policy. Although, the FDA Action Levels seem irrelevant to bioaccumulation assessment, they might be used inappropriately to claim that a proposed dredge material is acceptable from a bioaccumulation perspective because it results in tissue concentrations that are only a tiny fraction of the FDA Action Level. Omission of the FDA Action Levels would prevent their misuse in this context.

Gentile Charge No. 6B Comment (1 para):
[refer to Gentile Charge No. 6A Comment.]

Berry/Lake/Pruell Charge No. 6B Comment (1 para):
We do not think that FDA limits are so low that they might cause a sediment which probably poses no risk to fail, so if used in a screening mode, we do not see why omitting them would improve the assessment. It might be argued that they give a false sense of security because they are too high. On the other hand, some might be concerned if the “FDA” limits are no longer used, because they are associated with food. This is probably more a political issue than a scientific one. If there are more recent values in use or being considered by EPA for some of these compounds, the FDA values should not be used to exclude these values.

Solomon/Sibley Charge No. 6B Comment (1 para):
In the opinion of this reviewer, the assessment would not be improved by omission of the tissue concentrations to FDA action levels.

Newman Charge No. 6B Comment (1 para):
No.

Dillon Charge No. 6B Comment (1 para):
See peer reviewer's comment at end of questions regarding human health evaluations.

Agency Response to Reviewers' Comments:

COMMENT 6-1: Three of the reviewers believed that the FDA Action Levels are useful as an upper level of acceptability for tissue concentrations because they are conservative and have a regulatory basis. In addition, the inclusion of these values as part of the Green Book evaluation process was noted by one reviewer.

RESPONSE 6-1: EPA Region 2 acknowledges the reviewers support for continued use of FDA Action Levels as described in the Green Book. EPA Region 2 also, however, notes that the FDA Action Levels are suggested for use in the Green Book but are *not* required by the ocean dumping regulations. FDA Action Levels and FDA tolerance levels exist for seven of the analytes designated for routine HARS testing evaluations. Action and tolerance levels represent limits at or above which FDA will take legal action to remove products from the market. The action levels are established and revised according to criteria specified in Title 21, Code of Federal Regulations, Parts 109 and 509 and are revoked when a regulation establishing a tolerance for the same substance and use becomes effective. Tolerance levels are regulatory standards that are specifically promulgated in Federal Regulations. A tolerance level has been established only for PCBs by regulation specified in 21 CFR 109.30. The FDA levels are established based on protection of the general public where an individual will consume fish purchased from a local market whose stock may include fish from around the world. The levels are based upon national average fish consumption rates. FDA levels would serve as appropriate upper limits of acceptability.

In contrast, HARS-Specific Values will be derived using appropriate site-specific subpopulations considerations and exposure assumptions specifically related to local conditions and are based on the protection of recreational anglers who fish at the HARS. As such, HARS-Specific Values would be more pertinent for evaluating risk at the HARS and would be derived specifically to be protective of recreational anglers who fish the HARS. However, comparison to FDA levels will remain in the evaluative Framework.

COMMENT 6-2: Two of the reviewers felt that the FDA Action Levels were of limited value because they are typically not effects-based and are frequently misinterpreted as being protective of the environment. In addition, one reviewer noted that the values are very high, sometimes above levels associated with potential risks and, therefore, could lead to a "false sense of security".

RESPONSE 6-2: As discussed and recommended in the Green Book, FDA Action Levels for Poisonous or Deleterious Substances in Fish and Shellfish for Human Food are the limits above which the FDA can take legal action to remove products from the market. These levels do not include the potential for environmental impact on contaminated organisms or on their nonhuman predators. Because contamination of seafood in excess of FDA Action Levels is considered a threat to human health, contaminants that bioaccumulate in worms and clams to levels exceeding FDA Action Levels are considered to exceed the Limiting Permissible Concentration (LPC) and therefore unacceptable for placement in the ocean. While FDA Action Levels do not consider ecological impact, they serve as an upper limit for acceptability. EPA Region 2 will propose HARS-Specific Values to be used in determining the suitability of dredged material for use as HARS Remediation Material. Although the HARS-Specific Values are projected to be below FDA Action Levels, EPA Region 2 is proposing to retain the comparison to FDA Action Levels in the HARS Framework.

COMMENT 6-3: One reviewer suggested that an alternative, risk-based approach be developed, evaluating both ecological and human health risks independently.

RESPONSE 6-3: EPA agrees and is proposing to revise the Framework to utilize chemical-specific HARS-Specific Values which are risk-based for both ecological and human health effects (see response to comments 1 and 15).

Charge Group: Human Health Risk, Cancer and Noncancer

Charge No. 7A: Are the risk values suitable for determining the suitability for placement at the HARS as Remediation Material? If there are better alternatives for human risk, specifically what are they?

Charge No. 7B: Benthic tissue levels for cancer protection were derived using assumptions focused on attaining a cancer protection at the 10^{-4} risk level. Is this risk appropriate for a determination of ocean placement of Remediation Material? ***(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Page A-4, A-5)***

Charge No. 7C: Benthic tissue levels for noncancer protection were derived using Reference Dose (RfD) of several organic and inorganic contaminants for the protection of human health. Are these values appropriately and consistently derived? Is the whole body/fillet conversion factor of 1.35 an appropriate factor for all of the contaminants considered if human exposure is assumed to be primarily via consumption of the fillet portion of the fish? ***(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Attachments B and C)*** **If not, what factors would be appropriate? For the lead noncancer value, since there is no RFD for lead the value was derived differently than the other metals. Was the value derived appropriately?** ***(Please see Reference No. 88)***

Charge No. 7D: Are the risk values suitable for predicting the significant undesirable effects due to bioaccumulation? Since the primary route of exposure is through consumption of fish and shellfish, should the variability in potential exposure due to differences in fishing behavior (e.g., target species, seasonal preferences) be incorporated in the risk paradigm?

Summary of Peer Review Comments

Individual Comments

Clifford Charge No. 7A Comment (1 para):
As stated above, I believe that there may be other receptors that are more ecologically relevant like upper trophic-level fish (e.g., tuna, jack, etc.) or piscivorous birds (e.g., pelican, osprey, gull, etc.) that would be far more exposed than humans, perhaps far more susceptible to impacts (e.g., pelican egg shell thinning with exposure to DDT), and potentially more relevant. I say this, however, not knowing the area well enough to know which of these receptors might be present, if any.

Gentile Charge No. 7A Comment (1 para):
Human health risks are not my area of expertise so I can only offer general suggestions to this question. The approach and methods employed are those that are currently accepted by the regulatory and scientific

communities with the exception that there are advocates for using distributions rather than point estimates and conducting Monte Carlo Simulations resulting in a distribution of risk probabilities. The controversy with this approach centers on being able to define the appropriate distribution parameters, nevertheless it is something that you need to consider as part of this analysis. As the CENAN joint evaluation memo (pages A-4 and A-5) states, this approach must be considered a conservative upper bound estimate. What bothers me about this approach from a risk perspective is that it is has little or no basis in reality for a host of reasons. For example, how does one address the issue of calculating the proportion of contaminant coming from fisheries in the HARS vs. the total catch into which the HARS sub-population will be diluted and the subsequent probability of any person in the NY/NJ region of consuming enough fish to even remotely approach the upper bound. One could make those types of estimates and create a response surface that might be very informative.

Solomon/Sibley Charge No. 7A Comment (1 para):
As discussed above, the FDA human food consumption guideline values are conservative assumptions used for the protection of individual humans. They do not consider the likelihood of consumption of contaminated seafood and do not usually incorporate seasonal, catch site, and target species variability. If these factors are considered, exposures would normally be reduced, thus further adding conservatism to the risk assessment. Probabilistic approaches to assess the likelihood of consumption would be more appropriate.

Newman Charge No. 7A Comment (1 para):
The process seems reasonable.

Dillon Charge No. 7A Comment (1 para):
For possible alternatives, see peer reviewer's comment at end of questions regarding human health evaluations.

Clifford Charge No. 7B Comment (1 para):
Assuming acceptance of humans as a terminal receptor, yes.

Gentile Charge No. 7B Comment (1 para):
Based upon the rationale presented in the Appendix for Table 1, Pages A-4, A-5, I would agree that if one accepts all the assumptions and wishes to take a very conservative approach to avoid dealing with uncertainties then this is appropriate. It might be useful to provide a estimate of the probability of exceeding the cancer protection benchmark by preparing a matrix of tissue concentrations and daily intakes and their incremental risks. Then compare those values to the range of tissue concentrations from contaminated sites just to see if it plausible to experience a set of conditions that would lead to exceeding the upper bound.

Solomon/Sibley Charge No. 7B Comment (1 para):

The use of the multistage linear model for extrapolation of risks from laboratory animal studies to humans is very conservative. For one, it does not consider the presence of threshold of toxicity (carcinogenicity). Biologically, all effects likely have thresholds, it is just that these thresholds cannot easily be demonstrated experimentally. Repair mechanisms for many of the cancer-causing mutational events exist and function to repair damage from natural mutational events. These natural mutational events are usually far more numerous than those caused by low exposures to synthetic chemicals. Not all species of fish or shellfish would necessarily be consumed by humans, thus adding further conservatism to the assessment. The use of a 10^{-4} cancer risk estimate is therefore judged to be appropriately conservative for the purposes of ocean placement.

Newman Charge No. 7B Comment (1 para):
The obvious range of candidate risk levels is 10^{-4} to 10^{-6}. I would tend to agree with the 10^{-4} used here. However, I am biased towards doing these calculations for subpopulations (e.g., groups prone to consume more fish/shellfish from the area), not the general population.

Dillon Charge No. 7B Comment (1 para):
Setting levels of protection for cancer risk is a matter of policy. For example, EPA's policy in the Superfund program is to use the risk range of 10^{-4} to 10^{-6} as a "point of departure". Generally speaking, risks within the range require a site-specific baseline risk assessment, risks less than the range require little to no further evaluation while risks above the range require immediate attention (i.e., removal/remediation). On the other hand, the State of Florida has promulgated its risk policy as a single deterministic point of compliance; 10^{-6}.

Bartell Charge No. 7C Comment (4 para)
The determination of the Rfd's for noncancer health effects was described briefly in the Appendix for Table 1and summarized in Attachment C. The Rfd values appear to have been consistently derived; the appropriateness of these values is contingent on the usual set of pessimistic assumptions attendant to the standard USEPA human health risk assessment process. More appropriate (i.e., realistic) values might derive from a probabilistic estimation of these Rfd's, wherein distributions or at least ranges of parameter values were included. The Rfd could then be selected, for example, as the 95^{th} percentile of an estimated distribution (e.g., Monte Carlo methods) or an upper bound (e.g., interval analysis).

The whole body/fillet conversion factor of 1.35 appears to have been selected as a mid-point value of the range of 1.2 - 1.5 reported for lipophilic substances in other New York-related studies. To the extent that this value was used for non-lipophilic compounds, bias may have been introduced to estimates of those Rfd's.

An benthic tissue Rfd has been derived for lead as 1.25 ppm (rounded to 1.3) in relation to the East River Project (Reference 88). It was assumed that this approach was applicable for the HARS assessment and the value of 1.3 is listed for lead in Table 1. The Rfd appears conservatively estimated given assumptions (outlined specifically in Reference 89) concerning patterns of consumption, fish behavior, and trophic transfer efficiency. This analysis also included an additional dietary component of lead; in a sense, the fish consumption pathway was double-counted.

The derivation of the lead Rfd might have proceeded more realistically by defining the parameters as distributions (or at least ranges) and incorporating these uncertainties into the calculations. This refers to not only the fish consumption calculation, but also to the estimates of lead exposure from drinking, water, air, dust, soils, paint, and diet.

Clifford Charge No. 7C Comment (3 para):
(7C-1) Response: Assuming acceptance of humans as a terminal receptor, they appear to be.

(7C-2) Response: Within the limits of my expertise in human health risk assessment, yes.

(7C-3) Response: Within the limits of my expertise in human health risk assessment, yes, albeit very conservative.

Gentile Charge No. 7C Comment (1 para):
This is not my area of expertise.

Wentsel Charge No. 7C Comment (1 para):
Are not my area of expertise.

Solomon/Sibley Charge No. 7C Comment (2 para):
The methods used to determine benthic tissue levels for the protection of human health were judged to be appropriate and consistently derived. The whole-body fillet conversion factor of 1.35 is judged to be slightly conservative (based on this reviewers experience with organochlorine concentrations in fish tissues). Fat is consumed to produce energy in fish muscle (fillet) and lipid concentrations (and associated lipid-soluble materials) are usually significantly lower than in other tissues (in our studies muscle had less than 1% fat while the carcase had between 3.7 and 5.6% fat). These conversion factors and the Gobas trophic transfer model are judged appropriate for determining possible fish exposure concentrations.

The RFD for lead was derived from exposure concentrations appropriate for the protection of children, the most sensitive human life stage for this element. The RFD considered exposure via other routes and is judged to be appropriate.

Newman Charge No. 7C Comment (1 para):
Subanswer One: Are these values appropriately and consistently derived? I see no general problem.

Subanswer Two: Is the whole body/fillet conversion factor of 1.35 an appropriate factor for all of the contaminants considered if human exposure is assumed to be primarily via consumption of the fillet portion of the fish? (Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Attachments B and C) If not, what factors would be appropriate? This seems appropriate.

Subanswer Three: For the lead noncancer value, since there is no RFD for lead the value was derived differently than the other metals. Was the value derived appropriately? (Please see Reference No. 88). The use of data for blood lead levels in urban children seems conservative (and therefore appropriate).

They often tend to be very close to background levels. The calculations used high drinking water (4 ppb) and dust/soil/paint (800 ppm) lead levels, giving the entire process a conservative bias. It is unlikely that fish will be consumed by a child as specified in ref. 88. Again this results have an appropriately conservative bias. The final calculated limit of 1.3 ppm in fish tissue sounds reasonable.

Dillon Charge No. 7C Comment (1 para):
Not peer reviewer's area of expertise.

Bartell Charge No. 7D Comment (1 para):
The hazard quotients for noncancer health impacts might be useful for screening-level calculations in relation to the HARS study. However, these measures are extremely limited in their ability to "predict significant undesirable effects". While quotients less than 1 might suggest some minimal likelihood of health impacts, values greater than 1 provide little information concerning the possible magnitude of impact in the context of exposure(dose)-response relationships. Is a quotient of 2 twice as significant as a quotient of 1? Not necessarily. It depends on the underlying (and usually unknown) dose-response function. The quotient also carries little or no information concerning health impacts that were not specific endpoints (e.g., mortality) in the limited number of toxicity assays that are routinely performed with a small number of species.

Clifford Charge No. 7D Comment (1 para):
As stated above, I believe that there may be receptors other than humans that are far more ecologically relevant. As such, I cannot answer the question as such and recommend consideration of a different receptor with due concern given to it's seasonal variability.

Gentile Charge No. 7D Comment (1 para):
The answer to this question is 'yes' if a conservative upper bound is the management goal and comfort level and 'no' if one wishes to insert a truly risk-based sense of reality to the problem. I touched on this in my comments above under 7A. If the goal is to develop a truly risk-based estimate of human risk the crucial information is that relative to exposure that is, what is the probability and proportion of contaminated fish or shellfish coming from the HARS site that ends up in the diet of one or more sub-populations with a range of dietary intakes. Target species and seasonal preferences are but some of the variables that need to be included.

Wentsel Charge No. 7D Comment (1 para):
Are not my area of expertise.

Solomon/Sibley Charge No. 7D Comment (1 para):
As discussed above, human food consumption guideline values are conservative assumptions used for the protection of individual humans. They do not consider the likelihood of consumption of contaminated seafood and do not usually incorporate seasonal, catch site, and target species variability. If these factors

are considered, exposures would normally be reduced, thus further adding conservatism to the risk assessment. Probabilistic approaches to assess the likelihood of consumption would be more appropriate.

Newman Charge No. 7D Comment (1 para):
This is a good point. As suggested elsewhere (Answers to Questions 7, 11 and 26), it would be helpful to examine subpopulations at higher tiers of the assessment process. Are there appropriate data available from surveys of fishing and fish consumption practices for the area?

Dillon Charge No. 7D Comment (1 para):
Suggestions in peer reviewer's comment at end of questions would provide for a way to account for variability in site-specific differences in fishing behavior, ingestion rates, ingested seafood items, target human populations, etc.

Agency Response to Reviewers' Comments:

COMMENT 7-1: It was the opinion of one reviewer that upper trophic level organisms (e.g., pelican, osprey, gull, etc.) may be more ecologically-relevant receptors for assessing risk than humans. The reviewer suggested that these species may actually have higher exposures to the contaminants evaluated than humans. This reviewer recommends including such receptors in the TEM analysis.

RESPONSE 7-1: EPA Region 2 recognizes the importance of evaluating potential risks to sensitive, site-specific ecological receptors. Upper trophic level wildlife are the target receptors for the ecological HARS-Specific values proposed in response to comment 8. However, it is required by the ocean dumping regulations that bioaccumulation be evaluated for human health as well as ecological effects. Therefore, in addition to upper trophic level ecological receptors, EPA Region 2 proposes to developed HARS-Specific-Values for protecting against cancer and non-cancer effects in a highly-exposed human subpopulation (i.e. recreational fishermen).

EPA Region 2 will propose ecological and human health-based HARS-specific values that are appropriately conservative to ensure that releases to the environment do not cause or contribute to significant undesirable effects (see response to comment 2-3). See response to comment 1-2 regarding use of risk assessment.

COMMENT 7-2: Two reviewers suggested that probabilistic techniques should be incorporated to assess exposures to human health. One reviewer commented that the current approach is conservative and does not take into account the associated uncertainties. This reviewer suggested that it would be useful to evaluate the probability of exceeding the cancer protection benchmark based on the range of tissue concentrations at contaminated sites.

RESPONSE 7-2: EPA Region 2 agrees that incorporating probabilistic methods into development of the HARS-Specific Values would help to address some of the areas of uncertainty and variability associated with the current evaluation. However, probabilistic risk analysis is very data intensive and its quality is a function of how well the probability distribution function(s) used to describe key exposure parameters

reflect the true underlying probability distributions for those parameters. The shapes of these distributions are unknown for most parameters, and therefore EPA Region 2 elected to generate and propose appropriately conservative point estimates for key parameters. See response to comment 1-2 regarding use of probabilistic risk assessment.

In addition, EPA Region 2 will carefully consider and document uncertainties associated with development of the HARS-Specific Values and the proposed revisions to the HARS Framework. Appendix F discusses areas of uncertainty associated with development of HARS-Specific Values.

COMMENT 7-3: Two of the reviewers suggested that site-specific information be incorporated to account for uncertainties in exposure associated with issues such as: (1) the likelihood of consumption of contaminated seafood; (2) seasonal, catch site, and target species variability; and (3) the percentage of total fish consumed by sensitive populations in the New York region that are actually contaminated through exposure to the HARS.

RESPONSE 7-3: EPA Region 2 agrees that available regional information regarding fish consumption behavior, sensitive subpopulation, and fisheries variability needs to be used in the quantitative analysis of risk. EPA Region 2 reviewed available information on fish consumption in New Jersey and seasonal landings of finfish in the New York Bight to better reflect site-specific conditions in assessing risk. Based on this review, EPA Region 2 proposes to revise the seafood consumption rate to reflect consumption by a maximally-exposed subpopulation and to incorporate a revised estimate of the time that finfish may be in the vicinity of the HARS. Details regarding derivation of the revised fish consumption rate and site use factor are provided in the responses to comments 14-1 and 15-1, respectively.

COMMENT 7-4: One reviewer commented that the human health evaluation and ecological risk evaluation should be clearly segregated. To evaluate risk to human health, this reviewer recommended a 'regionalized' modification of EPA's Risk Assessment Guidance for Superfund (RAGS; EPA, 1989) using region/site-specific data.

RESPONSE 7-4: As noted above (see response to comment 7-1 and 7-3), EPA Region 2 agrees that the potential risks to ecological and human receptors should be evaluated separately and are best assessed using site-specific information. The current and proposed evaluation processes for evaluating the suitability of material for use as Remediation Material at the HARS consider risks to ecological and human health receptors separately. The current and proposed processes for evaluating human health risk in the HARS Framework adhere to the basic four step methodology (Hazard Identification, Exposure Assessment, Toxicity Assessment and Risk Characterization) detailed in EPA's Risk Assessment Guidance for Superfund (RAGS). Consistent with RAGS, site-specific exposure parameters are used where available, and uses information from EPA's Exposure Factors Handbook (EPA, 1997) to assign values to human exposure parameters for which site-specific data is unavailable.

COMMENT 7-5: One reviewer noted that the State of Florida has promulgated a single deterministic point of compliance at 10^{-6} , but noted that EPA's policy in the Superfund program is to use a range from 10^{-4} to 10^{-6}. In general, however, the reviewers felt that applying a 10^{-4} acceptable cancer risk level was appropriate given the level of conservatism inherent in the HARS risk evaluation process.

RESPONSE 7-5: EPA Region 2 agrees that use of 10^{-4} is appropriately conservative for assessing cancer risk associated with bioaccumulated contaminants at the HARS. While a standard acceptable risk level has not been established by EPA , the National Oil and Hazardous Substances Pollution Contingency Plan, used to guide implementation for Superfund remediation, designates an acceptable risk range of 10^{-4} to 10^{-6}. In establishing this risk range, EPA rejected the argument that a risk range, rather than a single risk criterion, does not adequately protect health and the environment (55 CFR 8716-17, March 8, 1990). EPA noted that ACERCLA does not require the complete elimination of risk,@ rather, remedies comply with CERCLA Awhen the amount of exposure is reduced so that the risk posed by contaminants is very small, i.e. at an acceptable level. EPA=s risk range of 10^{-4} - 10^{-6} represents EPA=s opinion on what are generally acceptable levels@.

Furthermore, as noted by several reviewers, the conservative assumptions assigned to the various exposure parameters in the current and proposed HARS risk assessment processes result in conservative estimates of potential exposure (and therefore, risk). As such, the 10^{-4} risk level is regarded as an appropriate and conservative level for protecting human health. Therefore, EPA Region 2 proposes to continue using 10^{-4} as the endpoint for acceptable cancer risk in the proposed HARS Framework.

COMMENT 7-6: One reviewer suggested that, due to the standard uncertainties associated with the derivation of the RfD values, more appropriate estimates might be derived from a probabilistic estimation of these data. In addition, the reviewer noted that probabilistic methods might have been useful for the derivation of the lead RfD. Another reviewer noted that the HARS-specific value of 1.3 for lead appears conservative, and may reflect a double-counting of dietary intake of lead from fish consumption.

RESPONSE 7-6: Reference dose (RfD) and cancer potency factors values used in the current and proposed evaluation frameworks were obtained from EPA's Integrated Risk Information System (IRIS) database with the exception of lead. Toxicity criteria (i.e. RfDs and Cancer Slope Factors) listed in the IRIS database reflects the most recent research information and Agency consensus on the appropriate values for these criteria. The RfD is intended to represent an exposure level, with an adequate margin of safety, that is without appreciable risk of deleterious effects over a lifetime. EPA Region 2 acknowledges that there are inherent uncertainties associated with the derivation of the RfD values. EPA Region 2 also acknowledges the ability of probabilistic techniques to address such uncertainties. However**,** EPA guidance on probabilistic risk assessment methods generally recommends developing probability distribution functions for exposure parameters, rather than for toxicity criteria (i.e., Reference Doses, Slope Factors). Specifically, the policy statement in the Guiding Principals for Monte Carlo Analysis (EPA, 1997g) states: "For human health risk assessments, the application of Monte Carlo and other probabilistic techniques has been limited to exposure assessments in the majority of cases."

Regarding the toxicity assessment for lead, it is acknowledged that there is a nominal "double counting" of dietary lead from fish ingestion. Specifically, the default daily lead intake from diet (5.5 ug/day) includes a small contribution of lead from consumed fish. This small contribution of dietary lead is difficult to quantify because fish consumption by the target population (i.e. New Jersey recreational fishers, see Response to Comment 14-1) exceeds that of the general population. Consumption of other meats (and therefore dietary lead) by this higher (seafood) consuming group may be expected to be correspondingly lower. Adjusting the dietary lead intake of this group to account for potential differences in dietary lead would therefore need to reflect this complexity. EPA Region 2 judged the potential double-counting of dietary lead to not

be of sufficient magnitude as to substantively alter the overall assessment of risks due to lead. Therefore, EPA Region 2 does not propose any such correction.

The RfD for lead was withdrawn from IRIS due to the lack of an established toxicity threshold for neurological effects in children. In the current and proposed evaluation processes, the toxicity assessment for lead has been refined through use of a biomarker (i.e., blood lead concentration) that serves as both a marker of lead exposure and effect. This biomarker modeling approach replaces the RfD for lead. The lead risk assessment incorporates the tenets of probabilistic methods in that the blood lead level of concern (4.6 ug/dl) represents the geometric mean of the probability distribution of blood lead in children in which 95% of the distribution falls below 10 ug/dl, the EPA Level of Concern for lead in blood (USEPA 1991a,b).

COMMENT 7-7: Two reviewers commented that the use of the whole body-fillet ratio of 1.35 may not be appropriate for all the chemicals evaluated, particularly the non-lipophilic chemicals. Another reviewer felt that this value was slightly conservative for use with lipophilic compounds.

RESPONSE 7-7: EPA Region 2 believes that the value of 1.35 is an appropriate estimate of the whole-body to fillet ratio for organic, lipophilic compounds. This whole-body to fillet value is based on fish tissue data collected from New York State and the Great Lakes for lipophilic chlorinated organic substances, such as PCBs and DDT.

Based on available information indicating that distributions of tributyltin (TBT) within various organs of marine species also tend to be correlated with lipid content (Laughlin *et al.*, 1986), EPA Region 2 proposes to use the 1.35 whole body:fillet conversion factor in the risk evaluation of organotins.

EPA Region 2 agrees with the reviewers' caution that although the 1.35 whole-body to fillet value is appropriate for lipophilic organic compounds, it may not be appropriate for predicting fillet concentrations of metals. The metabolism and resultant distribution of individual metals in tissues of fish is metal-specific. Some metals preferentially accumulate in internal organs (e.g., liver, kidney) while others tend to partition to the muscle or bones (Neff, 1997; Bevelhimer *et al.*, 1997). For those chemicals that bind preferentially to bone (e.g., lead), the difference between the whole body concentration and the fillet concentration is expected to be large because only a small portion of the total metal found in the body is present in the muscle tissue of the fillet (Bevelhimer *et al.*, 1997). In contrast, for metals that bind to proteins (e.g., mercury), differences are likely to be smaller because the chemical binds in varying degrees to proteins found in the fillet as well as in other soft tissues (e.g., internal organs) throughout the body (Bevelhimer *et al.*, 1997). Therefore, it is not possible to use a single whole-body to fillet ratio to express the whole body to fillet differences for all inorganic chemicals (Heit, 1979; Ray *et al.*, 1984; Goldstein *et al.*, 1996).

Only limited information was identified in the literature regarding the whole-body to fillet ratios for inorganic compounds. Bevelhimer *et al.* (1997) investigated the relationship between fillet and whole-body contaminant concentrations in several finfish species and developed ratios for specific inorganic chemicals (Table 7-1). The uncertainty associated with these ratios varies according to the chemical evaluated. Clearly defined, statistically significant relationships between fillet and whole-body concentrations were observed for arsenic, chromium, and mercury. For other inorganic contaminants (i.e., Cd, Cu, Pb, Ni, and Zn) the available data suggested that whole-body and fillet concentrations differed significantly, however the ability to predict whole body concentrations from concentrations in the fillet was limited (Bevelhimer *et*

al., 1997). The lack of consistent relationships for these contaminants may be due to the sample size and the species evaluated, or to the relatively limited range of chemical concentrations evaluated.

Despite the limited predictive power for certain contaminants, the slopes of the lines relating the whole body and fillet residues reported by Bevelhimer *et al.* (1997) were considered to represent the best available estimates of the whole body to fillet ratio for these contaminants. As such, they have been added to the proposed HARS Framework for determining the suitability of dredged material proposed for use at the HARS as Remediation Material.

Table 7-1. Whole-Body to Fillet Ratios

Chemical	Ratio
Arsenic	1.4
Cadmium	5.9
Chromium	1.2
Copper	2.9
Lead	3.9
Mercury	0.7
Nickel	8.3
Silver	1[a]
Zinc	2.4

[a] In the absence of data for silver, the whole body concentration was assumed to be equivalent to the fillet concentration.

COMMENT 7-8: The reviewers strongly emphasized the conservative nature of these assessments. The reviewers noted that when values calculated from dredged material data exceed the risk-based thresholds for cancer and non-cancer effects, one cannot definitively conclude that the material poses a risk to human health because hazard quotients greater than one provide little information concerning the possible magnitude of impact in the context of dose-response relationships. In response to the limitations inherent to the use of screening-level methods, the reviewers recommended using the results of the screening-level analysis as the basis for a decision to proceed to a more definitive analysis of risk.

RESPONSE 7-8: EPA Region 2 agrees that the current approach conservatively assesses risk to human health. However, EPA Region 2 also believes that conservatism is warranted to be protective of human health and the environment given the various areas of uncertainty inherent in the assessment and given the remedial intent of the HARS. As discussed in the response to comment 1-1, EPA Region 2 acknowledges the peer reviewers' concerns regarding the application of these screening levels in the place of actual risk levels. In the case of the HARS, however, EPA Region 2 believes that while a dose-based evaluation may

provide a more realistic, quantitative estimate of risk than these decision values, application of HARS-specific values is proposed to ensure that placement of dredged material at the HARS meets the remedial intent of the HARS designation, demonstrably contributes to the improvement of conditions within the HARS, and complies with the Ocean Dumping Regulations so as not to cause significant undesirable effects. See response to comment 1-2 regarding use of risk assessment.

EPA Region 2 recognizes that comparing total concentrations of metals reported in test organism tissue to proposed HARS-Specific Values could lead to an overestimate of risk for certain metals. HARS-Specific Values for metals would be developed using toxicity reference values (e.g. RfDs and Cancer Slope Factors) associated with the most toxic form of the metal (e.g., toxicity reference values associated with hexavalent chromium and inorganic arsenic) and/or trophic transfer rates associated with the most efficiently transferred form (e.g., methylmercury). As such, comparison of total metals residues to the proposed HARS-Specific Values would assume that all metals present in test organism tissue is in its most toxic or most efficiently transferred form.

To address this potential for overestimating risk, in cases where a project tissue data for a total metal (for example, chromium) exceeds the HARS-Specific Value, EPA Region 2 proposes to offer the applicant an opportunity to re-analyze the tissues and submit data on the specific valence species or organocomplex state of the metals residues to allow for a more relevant comparison to the Value. This data on the forms of metals present would allow for a more relevant comparison to the proposed HARS-Specific Value and minimize the potential for overestimating risk of these metals. If the applicant declines to provide this data the total metal concentration would be used by EPA Region 2 to assess risk.

Organic forms of arsenic are much less toxic than inorganic arsenic. Nearly all of the arsenic in seafood has been shown to be present in organic forms (primarily arsenobetaine) (Abel and Axiac, 1991; FDA 1993). In light of this fact, FDA (1993) recommends that inorganic arsenic residues in seafood be estimated from total arsenic measurements by applying a 0.1 multiplier to the reported total arsenic concentration. Like all other metals, arsenic residues in bioaccumulation test organism tissue are currently measured and reported as total arsenic. EPA Region 2 proposes to apply the 0.1 multiplier recommended by FDA to total arsenic residue concentrations reported in bioaccumulation test organism tissue to estimate inorganic arsenic residue before comparing to a HARS-Specific Value based on inorganic arsenic.

Charge Group: Ecological Risk

Charge No. 8A: **Ecological effects benchmarks include the Water Quality Criteria Tissue Level (WQCTL), Critical Body Residue (CBR) associated with narcotic responses, and certain mutagenic/teratogenic effects. Is it valid to use the CBR effect end point for evaluating significant undesirable effect? Are there other ecological end points that should be used to measure ecological risk that are protective of marine benthic and fish life via trophic transfer, particularly for PAHs? If so, identify. With regard to a narcotic effect for chlorinated organic compounds, should an additive approach be considered to include the contribution of chlorinated hydrocarbons against this narcotic (CBR) endpoint.**

Charge No. 8B: **Is the Region 2 WQCTL approach (i.e., multiplying the Water Quality Criterial Chronic Value by the Bioconcentration factor) appropriate for determining ecological effects levels of the contaminants for which they were developed? Specifically, are the appropriate BCFs used (for fish, bivalves, etc)?** ***(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Page A-1)***

Charge No. 8C: **BCFs reported for fish were used in the calculations of WQCTLs for organics; is this derived level appropriate for setting benthic tissue ecological effects levels? If the fish tissue levels are used, should adjustments be made to the derived levels to reflect the higher lipid contents of the benthic organisms used in the testing program?**

Charge No. 8D: **Are the WQCTLs calculated for metals using bivalve BCFs appropriate for setting levels for polychaetes or vice versa?**

Charge No. 8E: **Are the uncertainty factors applied while deriving ecological effects levels for PAH contaminants appropriate? Does this adequately address the uncertainty around the derived values? Can uncertainty be accounted for using these order of magnitude adjustments? Should they be applied elsewhere to the other risk-based values?**

Charge No. 8F: **Are the risk values suitable for predicting the significant undesirable effects due to bioaccumulation; are there better alternatives for ecological nonspecific risk?**

Charge No. 8G: **If you believe that these values are over- or under- conservative, what do you believe to be an appropriate way to improve them.)**

Summary of Peer Review Comments

Individual Comments

Clifford Charge No. 8A Comment (2 para):
As discussed above (question 2 and elsewhere) I'd really like to see a complete food-web risk analysis performed which evaluates risks to pertinent trophic levels on an exposure, rather than body burden, basis. This would involve a site-specific food web including molluscs, annelids, arthropods, fish of several trophic levels, and possibly piscivorous birds (depending on site conditions that I am not familiar with). Much of the ground work for such an analysis has already been done here (tabulation of toxicological endpoints like water quality criteria, compilation of bioaccumulation factors, etc.) and should not require that much more effort. The tissue burden approach inherent in the CBR approach is not without merit but, I believe that a more thorough evaluation of the food-web as a whole on an exposure basis may be worth considering.

As to other chlorinated organic compounds, if the principal toxicological effect on target species is known to be narcotic, additivity can certainly be considered, noting my comments on moles vs. milligrams above.

Gentile Charge No. 8A Comment (1 para):
My familiarity with the CBR literature is limited to what I have read in the supporting materials so my comments to these questions may have limited value. Nevertheless, after reading McCarty's 1992 paper in Environmental. Toxicology and Chemistry, a few thoughts are worth noting. First the data base used to develop the CBR is derived solely from freshwater for chemicals, primarily the fathead minnow, and with chemical with log $K_{ow} > 1.5$. For this data base and a very limited number of endpoints the relationship between CBR and lethality relationship for narcotics can be approximated by the QSAR derived equation $CBR(mM) = 2.4\ mM + 50/K_{ow}$. The fathead minnow specific CBR is 4.4 (mM) with a range of 2.2-2.8 mM. Multiplying by a factor of 0.25-0.1 can approximate conversion to chronic toxicity. There are several relevant questions that need to be addressed before one can confidently apply this approach to the marine fish and invertebrates: 1) has this relationship been corroborated for marine fish and invertebrates; 2) has the CBR approach been widely applied after the original work of McCarty; 3) have alternative hypotheses for the PAHs been proposed (e.g., Swartz *et al.* 1996); and 4) has any confirmatory studies been done to further develop the chronic relationship? What I noted in the CENAN memo was that there were no alternatives presented to the CBR and no literature cited beyond McCarty's original work. This tells me that either there is ' no better show in town' or no one has looked at the recent literature. I am not familiar enough with this area but I'm sure other peer reviews will provide useful information. My final comment is that if a sound argument can be made for transference of this approach to marine species based upon first principles and mechanisms then one could use it as an index much like the other benchmarks that you've chosen. However, until all the assumptions have been tested then I'd certainly be very cautious - if you don't need it then don't include it.

Berry/Lake/Pruell Charge No. 8A Comment (1 para):
The CBR-type approach seems to be the most reasonable one to use with narcotic chemicals. Even though the WQCTL approach seems valid, it was not the approach chosen by the Office of Water (OW) for development of its Equilibrium-Partitioning Derived Sediment Guideline (ESG) for PAHs. The approach OW chose is summarized in a briefing document for a presentation to the EPA Science

Advisory Board entitled, "Assessing the Toxicity and Bioavailability of PAH Mixtures in Sediments". May 13, 1997. I would encourage you to examine this approach, which explicitly looks at all of the PAHs together, using an additivity model. I will not comment on the WQCTL approach except to say that the CBR model uses molar units of PAH because those are the appropriate units for comparison. Although it may be appropriate at some point in the analysis to convert to weight units for PAH, it is not appropriate to say that a 400 ppb dose of napthalene would elicit the same effect as a 400 ppb dose of fluorene (as stated in the MOR), nor is it strictly appropriate to add the wet or dry weight concentrations together (as stated in the MOR).

Solomon/Sibley Charge No. 8A Comment (1 para):
CBR measurements are a useful method for assessing narcosis as a toxicity endpoint. They are, however, unsuitable for use when the substance has a specific receptor mechanism of action such as for pesticides in target organisms. Narcosis is normally observed at much higher concentrations than receptor-mediated responses and is often observed in non-target toxicity. Many of the PAHs act as narcotic agents and it is recognized that additivity of CBRs is an appropriate method for assessing the likely acute effects of PAHs in aquatic organisms. PAHs have been shown in recent unpublished work to cause increases in oxidative stress in fish (Hodson 1998). This stress leads to a number of responses that are similar to those mediated by the AhR. Once these processes are better understood, this may be another useful way to assess toxic potential of PAHs. However, carcinogenic potential is not well assessed using CBR. Many of the chlorinated pesticides (including some found at the site) are known to be toxic to arthropods and fish through receptor-mediated processes. Thus, these may have effects on arthropods and fish at body concentrations well below their CBR. An additive approach using narcosis to assess the chlorinated pesticides may not be appropriate, however, it should be applicable to the PCBs and similar substances.

Newman Charge No. 8A Comment (1 para):
Subanswer One: Is it valid to use the CBR effect end point for evaluating significant undesirable effect? Are there other ecological end points that should be used to measure ecological risk that are protective of marine benthic and fish life via trophic transfer, particularly for PAHs? If so, identify

If one assumes that the 28 day bioaccumulation test results are valid, then the use of CBR is valid. There are some toxic effects that will not conform to the CBR concept, e.g., cumulative liver damage by cyanide. However, the application of the CBR here seems the best way to estimate the potential for effect. Relative to ecological risk, there are many other end points that could be used. One could survey the site from which the dredge material will come and measure many things. One could measure community indices, lesion incidence, demographic qualities of key species, biochemical qualities of individuals, or a variety of other conventional metrics.

Subanswer Two: With regard to a narcotic effect for chlorinated organic compounds, should an additive approach be considered to include the contribution of chlorinated hydrocarbons against this narcotic (CBR) endpoint. Yes. An additive effect seems logical and consistent with this mode of action.

Dillon Charge No. 8A Comment (2 para):

Critical body residues, when expressed on a molar basis, is an appropriate endpoint for chemicals where the mode of toxicity is narcosis. CBR is not appropriate for chemicals with other modes of action (e.g., receptor-mediated, neural disruption). Additivity of effects is an appropriate assumption for narcotic chemicals if expressed on a molar basis. However, almost all our knowledge of narcotic chemicals is based on acute exposures and lethality endpoints. Environmental exposures rarely approach acutely lethal levels. On the other hand, chronic narcosis (especially effects on behavior) may be more insidious and environmentally relevant. This effect has received scant attention.

PAHs are generally thought to be directly toxic to benthos. Risk Swartz's sigma PAH model is a good evaluative tool for direct toxicity. The risks associated with PAHs entering the food web is less certain. Most uncertainties are associated with PAH metabolites. Analysis of metabolites tends to be difficult and expensive, they are ephemeral and organisms' ability to metabolize PAHs range widely both quantitatively and qualitatively. Many consider PAHs in the food web a low risk scenario because they are rarely detected at noticeably levels (except sediments and mollusks). However, this belief is based on parent PAH compounds, not their metabolites. We know relatively little of PAH metabolites' environmental fate and effects on survival, growth and reproduction.

Clifford Charge No. 8B Comment (2 para):
I have no real problem with the concept that exposure at the CWA CV represents an appropriate toxicological threshold. I do, however, as noted elsewhere herein, have reservations regarding the meaning of body burdens as surrogates for exposures.

While I would like to see BCFs specific to the organisms and contaminants in question used in all cases, this is clearly not possible as the data simply do not exist in all cases. I believe that the values used represent a scientifically defensible attempt to arrive at the most applicable values available.

Gentile Charge No. 8B Comment (1 para):
Yes, given there are no studies that explicitly describe the residue-effects relationship. This approach relies on the accuracy of the contaminant-specific BCFs which based on the material submitted for review is fine.

Berry/Lake/Pruell Charge No. 8B Comment (1 para):
[Refer to Berry/Lake/Pruell Charge No. 8A Comment]

Solomon/Sibley Charge No. 8B Comment (1 para):
The EPA WQCTL approach for determining ecological effects levels was judged to be appropriate as were the BCFs used in these calculations.

Newman Charge No. 8B Comment (1 para):
The application of a single BCF from a representative organism is an appropriate gross metric. But there is no single BCF for a species or a single representative species. Likely what was used was the closest species for which there was a published BCF. Consequently, I would apply an uncertainty factor (UF=10?) in this calculation.

Dillon Charge No. 8B Comment (1 para):
The WQCTL approach is one tool for evaluating the toxicological significance of bioaccumulation results. It is probably appropriate if one accepts certain assumptions: 1) chemicals of concern are not metabolized , 2) internal sequestering mechanisms are of minor importance, 3) benthic organisms' sensitivity is similar to nektonic forms upon which CVs were based, 4) benthic exposure approximates that for nektonic forms, 5) there is a mechanistic explanation linking residue and effect, 6) the CV is based on effects, not bioaccumulation, 7) there are few toxicity drivers in the sediment. The WQCTL is not appropriate (i.e., is under protective) when direct exposure to sensitive life stages (e.g., early life stages of fish) is anticipated. A more direct assessment of chronic toxicity via sediment bioassays may be less problematic. See peer reviewer's comment at end of questions regarding ecological receptors.

Clifford Charge No. 8C Comment (1 para):
Notwithstanding my comments on the utility of body burdens, the BCFs used are probably the best available as such values may not be available at all for benthos. As such, I think some discussion of the uncertainties is appropriate but, you cannot replace the values used with better values when none exist. Addressing the uncertainties should be sufficient. Adjustment of the values for lipid content is appropriate for the more hydrophobic compounds.

Gentile Charge No. 8C Comment (1 para):
As stated in the Appendix for Table 1, Section A this approach is accepted to be conservative and therefore should be protective of 95% of all tested organisms which do include not only fish but representatives from several phyla including benthic species. This approach is generic and can be made site specific by modifying specific factors if it is deemed appropriate. The calculation can be done with and without the site-specific data to determine if the difference results in a significant change in interpretation.

Berry/Lake/Pruell Charge No. 8C Comment (1 para):
[Refer to Berry/Lake/Pruell Charge No. 8A Comment]

Solomon/Sibley Charge No. 8C Comment (1 para):
If WQCTLs based on BCF values measured in one organism with a very different lipid content than another, this may lead to incorrect estimation of tissue concentrations. Lipid normalization has been recommended (Connell 1990; Hebert and A 1995) in a number of situations and, in the experience of this reviewer, can significantly change interpretations. Lipid normalization should be used.

Newman Charge No. 8C Comment (1 para):
No. A BCF for fish is not appropriate to use for an invertebrate. Further the BCF for one group of invertebrates may not be appropriate for another invertebrate group. Too many factors have large effects on BCF to make such a general use of a single BCF. This seems to be a situation in which an uncertainty factor is appropriate.

Dillon Charge No. 8C Comment (1 para):
See peer reviewer's response to 8.B.

Clifford Charge No. 8D Comment (1 para):
Again, you are constrained to using the available information. While I would prefer to see values for polychaetes used for polychaetes, rather than bivalve values, if no such values are available, this is a fairly reasonable approximation. Again, however, I urge addition of an uncertainty section to the report which discusses the limitations of such approaches as regards decision making.

Gentile Charge No. 8D Comment (1 para):
The BCFs would have to be compared for a representative set of organic and inorganic compounds to make a judgement. There is considerably more data on metals uptake with bivalves than polychaetes so I'd use the bivalves. However, I'd try to determine if the bivalves were consistently protective of polychaetes and when there exceptions.

Berry/Lake/Pruell Charge No. 8D Comment (1 para):
[Refer to Berry/Lake/Pruell Charge No. 8A Comment]

Solomon/Sibley Charge No. 8D Comment (1 para):
Although this reviewer is not very familiar with metal toxicology, it is known that efficiency of metal uptake in molluscs can vary with food availability and is different from that in many other organisms (because of the intracellular digestive process in the hepatopancreas). Thus molluscs would be more efficient at taking up particulate metals (as particles or attached to particles) from the water-column. The application of BCFs for metals from bivalves to polychaetes is judged to be inappropriately conservative while the reverse is judged to underestimate potential for exposure potential in clams.

Newman Charge No. 8D Comment (1 para):
No. Please see the answer [Newman Charge No. 8C Comment] above.

Dillon Charge No. 8D Comment (1 para):
See peer reviewer's response to 8.B.

Clifford Charge No. 8E Comment (2 para):
Numerous attempts are currently on-going (some by myself) to derive uncertainty factors more appropriate than those presented. None currently exist. I believe that the approach used is conservative and cannot at this time offer defensible alternatives. I've commented on the overall uncertainties in the approach repeatedly through this document and, while I believe that the uncertainties are "adequately addressed" by these uncertainty factors, I still think that the document would benefit from some qualitative treatment of the overall uncertainties.

I do not believe that such factors should be applied elsewhere. Qualitative discussion of uncertainties prior to drawing conclusions would be appropriate, however.

Gentile Charge No. 8E Comment (1 para):
The discussion of potential ecological impacts for PAHs and their uncertainties are discussed in Appendix to Table 1 (A-2 and A-3). The explanation of the derivation and variability is sufficient though a more complete discussion is in McCarty 1991 and McCarty *et al.* 1992. However, the choice of 40 ppm (40,000 ppb) as the value in Table 1 is based on fish and is being compared to invertebrates (polychaete and bivalve) which have much higher effect thresholds. I assume this is in keeping with a conservative approach. Regarding the uncertainty around the derived values, McCarty *et al.* 1992 reports that the range of concentrations causing narcotic effects on aquatic organisms is from 1.4 to 21*u*moles/g wet weight which is a factor of 15. Thus an appropriate 'safety' factor to account for differences in species-species sensitivity and to protect for untested species could be set at 10-20 for freshwater organisms since McCarty's data base was primarily freshwater. I would probably expand the safety range for untested species-species in marine waters to 10-100 solely due to the lack of data, unless of course there is recent work that could be used to compute a more accurate range of variability.

Berry/Lake/Pruell Charge No. 8E Comment (1 para):
[Refer to Berry/Lake/Pruell Charge No. 8A Comment]

Solomon/Sibley Charge No. 8E Comment (1 para):
Uncertainty factors are used to account for unquantified uncertainty and, as such cannot be judged against the true uncertainty (until this is known). Order of magnitude factors are frequently used for animal-animal extrapolation and to account for unknown variability in population responses. They are no substitutes for a knowledge of variability and uncertainty, however, "arbitrary" uncertainty factors of this magnitude have been successfully used in the past and their continued use in the face of insufficient knowledge is judged appropriate.

Newman Charge No. 8E Comment (1 para):
Relative to converting effect to no effect, the associated calculations contain considerable error. I would suggest an uncertainty factor of 10. The means of dealing with narcotic effects seems adequate. An interspecies uncertainty factor of 10 seems adequate.

Dillon Charge No. 8E Comment (2 para):
Uncertainty factors are a legitimate method for quantifying uncertainty. The problem comes when multiple UF are used. Then, quantitative risk estimates are driven absurdly low and the value to decision-making falls precipitously. My preference is to limit UF to extrapolations which have a quantitative basis (e.g., acute to chronic ratio) and provide a narrative description for other uncertainties, especially those with a mechanistic basis. See peer reviewer's response to 8.A for PAH portion of question.

A distinguishing characteristic of environmental risk assessment is its explicit (not implicit) treatment of uncertainty. If EPA/CENAN intends for the Framework to emulate a risk-based approach, it must have

specific sections devoted exclusively to uncertainty analysis. An explicit recognition of uncertainty promotes, not hinders, environmental decision-making.

Clifford Charge No. 8F Comment (1 para):
Please see responses to 8A and 8B (and others throughout).

Gentile Charge No. 8F Comment (1 para):
If the question refers to WQCTLs then the answer is yes if your referring to PAHs then the answer is for freshwater fishes but not for marine fishes or invertebrates until a comparable data base is developed.

Berry/Lake/Pruell Charge No. 8F Comment (1 para):
[Refer to Berry/Lake/Pruell Charge No. 8A Comment]

Solomon/Sibley Charge No. 8F Comment (1 para):
The hazard quotients used in this assessment are judged appropriate. See the discussion of risk and hazard above.

Newman Charge No. 8F Comment (1 para):
There are additional and more direct means of assessing ecological risk. Conventional metrics of effects to individuals (e.g., lesions), populations (e.g., demographic assessment or abundance estimation) and communities (e.g., species richness, IBI, and others) could be done at the site from which the material is to be taken.

Dillon Charge No. 8F Comment (1 para):
See peer reviewer's response to 2. and comment at end of questions regarding ecological receptors.

Clifford Charge No. 8G Comment (1 para):
Please see responses to 8A and 8B (and others throughout).

Gentile Charge No. 8G Comment (1 para):
Regarding the WQCTLs they are probably somewhat over conservative but not too much. Regarding the PAHs the CBR is very over protective of invertebrates and questionable for freshwater fishes since most of the data is for fathead minnows. Nothing in there for trout nor for marine fishes. The solution for PAHs is to develop a larger data base.

Berry/Lake/Pruell Charge No. 8G Comment (1 para):
[Refer to Berry/Lake/Pruell Charge No. 8A Comment]

Solomon/Sibley Charge No. 8G Comment (1 para):
NA

Newman Charge No. 8G Comment (1 para):
I would guess that they are overly conservative. However, there is so much uncertainty in several steps that it is difficult to be more definitive. Please see the above comments for possible improvements. It is unfortunate that the approaches described do not provide any direct information on population viability or benthic community effects.

Dillon Charge No. 8G Comment (1 para):
See peer reviewer's response to 2. and comment at end of questions regarding ecological receptors.

Agency Response to Reviewers' Comments:

COMMENT 8-1: In general, reviewers questioned the uncertainty associated with estimating an effects threshold using the WQCTL approach. Much of this uncertainty is associated with the use of a bioconcentration factor (BCF) that may or may not be specific to the organism of concern. Reviewers suggested that approaches that rely on specific, experimental residue-effects data or direct measurement of chronic toxicity (i.e., application of chronic sediment toxicity tests) would be superior to the current approach, with its numerous sources of uncertainty.

RESPONSE 8-1: EPA Region 2 is proposing to replace HARS-Specific ecological protection values based on WQCTLs (tissue guidance levels estimated from EPA chronic marine water quality criteria using bioconcentration factors) with effects-based residue levels protective of effects to ecological receptors based on specific, experimental residue-effects data reported in the scientific literature. These residue levels would be developed for use in conjunction with those based on human carcinogenic and non-carcinogenic effects (see Response to Comment 7) for evaluating the results of bioaccumulation tests performed on dredged material and determining its suitability for use as Remediation Material at the HARS.

Available datasets relating tissue residues and incidence of effects were identified through an exhaustive review of peer-reviewed scientific studies. Only those studies that related residues and incidence of effects of single contaminants were considered relevant for the development of guidelines. The primary tools used in identifying relevant peer-reviewed studies were the databases compiled by the U.S. Environmental Protection Agency Office of Research and Development's Mid-Continent Ecology Division in Duluth, MN (Jarvinen and Ankley 1998) and the U.S. Army Corps of Engineers' Waterways Experiment Station in Vicksburg, MS (USACE 1998). Additional relevant studies that were published since release of these databases were identified through literature searches with the assistance of staff from the EPA-Office of Research and Development (Duluth, MN).

The primary criterion for a study to be considered in deriving ecological HARS-Specific Values is a study design that ensures that organisms are only exposed to a single contaminant. This study design maximizes our ability to assign causality of any observed effects to the body burden of the specific contaminant.

While EPA Region 2 recognizes that organisms that are exposed to dredged materials are generally simultaneously exposed to more than one contaminant, the potential for significant synergism and antagonism of contaminants is judged to be low (see response to comment 17-1).

Guidelines developed using these empirical studies would not establish a probability for adverse effects but would rather estimate a tissue concentration below which there is no potential for significant undesirable effects. EPA Region 2 believes that this threshold (avoidance of the potential for significant undesirable effects) is an appropriate goal for the assessment of bioaccumulation test results, given the remedial intent of the HARS.

EPA Region 2 proposes to develop benthic invertebrate tissue concentrations for each contaminant (or class) using limited residue-effect data. The data and method that will be used to derive these concentrations are documented in Appendix D of this document.

COMMENT 8-2: Additional uncertainty is associated with extrapolating from: (1) freshwater to marine species, (2) species for which toxicity data have been collected to receptors of concern, (3) acute to chronic effects, (4) effect to no-effect levels, and (5) individual measures of toxicity to potential for effects on populations. The judicious application of uncertainty factors can help to control some of the uncertainty associated with the application of WQCTLs and CBR, but such factors can also make the estimates overly conservative. Reviewers differed on the magnitude of the uncertainty factors that could be applied to the parameters of concern (e.g., BCFs and toxicity extrapolation factors), but in general, support was offered for the approach taken. Peer reviewers recommended that a section of the document be dedicated to uncertainty and its effect on the conclusions of the analysis.

RESPONSE 8-2: EPA Region 2 recognizes the need to acknowledge the areas of uncertainty in the evaluative process being used to identify suitable Remediation Material. Appendix F discusses areas of uncertainty associated with development of HARS-Specific Values.

COMMENT 8-3: Most reviewers considered the CBR approach to be valid for assessing the potential for non-specific narcotic effects of contaminants in dredged material proposed for use as Remediation Material at the HARS. Similarly, reviewers stated that the CBR approach (narcosis) is inappropriate to address compounds where more specific modes of action are expected, e.g., chlorinated pesticides. The list of excepted compounds would also include the metabolites of neutral organic chemicals, e.g., PAHS.

RESPONSE 8-3: EPA Region 2 concurs with the reviewers' caution not to employ the narcotic CBR approach to address compounds that are expected to cause effects through a specific mode of toxic action. EPA Region 2 emphasizes that the specific mechanisms of toxic action of individual contaminants would continue to be considered (in addition to narcosis) under the proposed Framework to evaluate the suitability of dredged material for use as Remediation Material at the HARS.

Narcosis has been described as "the *base toxicity* of a chemical that is not caused by any specific mode of toxic action" (Chaisuksant *et al.* 1999). Chaisuksant *et al.* (1999) also state "Other modes of toxic action increase the toxicity of the chemical from this base toxicity. ...if a specific mode of toxic action is present, nonspecific toxicity, although always present, is not important...the toxic effect of the chemical is mainly a result of the specific mode of toxic action...a reactive chemical could have both specific and

nonspecific toxicity to an organism, but the specific mode of toxic action will be dominant". Therefore, concentrations of those non-polar chemicals that exert their effect through a specific mode of action (such as chlorinated pesticides and PCBs) also contribute to the total narcotic dose of the mixture present in tissues exposed to sediments (McCarty 1991). This remains true when the contaminant occurs at concentrations that are insufficient to elicit the specific toxic effect (McCarty and MacKay, 1993). For example, PCBs and DDE have been specifically identified as compounds that contribute to the narcotic dose (McCarty 1991, Fisher *et al*. 1999).

Based on these considerations, the total narcotic dose associated with the bioaccumulation test organisms would be calculated under the proposed Framework using the molar concentrations of all PAHs, PCBs, pesticides, dioxins/furans and 1,4-dichlorobenzene, combined. Also under the proposed Framework, additivity would continue to be assumed for the purposes of this calculation (see response to comment 17).

COMMENT 8-4: Two reviewers recommended using a dose rather than a body burden-based model to estimate the potential for effects on organisms within the food web. Peer reviewers agreed that molar concentrations must be used to express the narcotic dose of contaminants and that equipotency of the PAHs is an appropriate assumption when expressed on a molar basis.

RESPONSE 8-4: See response to comment 1-2. The proposed Framework would express the narcotic dose of PAHs and other organic contaminants as molar concentrations for use in the narcotic CBR model.

COMMENT 8-5: Several reviewers suggested that other approaches to evaluate PAHs be considered. One reviewer suggested that other approaches (besides the CBR approach) to evaluating the potential for ecological risks of PAHs may have become available since McCarty (1992) and recommended that EPA Region 2 review the method of Swartz *et al.* (1996) to determine whether their model is more appropriate for assessing PAHs as a class.

RESPONSE 8-5: EPA Region 2 notes that the Swartz *et al.* model referred to by the reviewers was actually published in 1995. The Swartz *et al.* (1995) model considers the sediment concentrations of 13 PAHs to calculate the probability of acute toxicity to be caused by exposure to the sediment and does not address bioaccumulation. The model considers the toxicity of the PAH mixture by expressing the contribution of each PAH as additive toxic units. The toxic unit of each PAH was calculated by using the predicted interstitial water concentration and the concentration at which 50 percent of tested organisms die during a 10 day exposure (i.e., 10 day LC50). Since this model considers acute toxicity due to external exposure concentrations and does not consider PAH contributions from ingestion pathways, it is of limited utility for interpreting the potential for risks associated with bioaccumulation and trophic transfer of contaminants. Therefore, EPA Region 2 is not proposing to use the Swartz *et al.* (1995) model in the revised HARS Framework to evaluate risks associated with PAHs.

COMMENT 8-6: One reviewer questioned whether the CBR approach was applicable to marine species.

RESPONSE 8-6: Although the CBR approach to assess the potential for narcotic effects of contaminant mixtures in environmental matrices was originally developed using freshwater organisms, the approach has also been corroborated by various researchers using marine species, including amphipods (Boese *et al.*, 1999), copepods (Lotufo, 1998), mussels (Donkin *et al.*, 1989), and crabs (Mortimer and Connell, 1994). In addition, van Wezel and Jonker (1998) experimentally determined that differences in salinity did not significantly affect the critical body residue for toxicity in a species of fish. The CBR approach relies on measured whole body burdens of contaminants as an estimate of the contaminant concentration at the site of toxic action within the organism. McCarty and MacKay (1993) examined the apparent narrow range of residue levels that produce narcotic effects in various organisms and concluded that this is not unexpected based on the supporting evidence of reliable QSARs (quantitative structure-activity relationships) in the scientific literature. QSAR theory is based on the premise that the toxic activity and mode of action of a given contaminant is closely related to its chemical structure. Therefore, the mechanisms by which toxicants exert their toxic effect on the cellular level should be independent of the external environment of the exposed organism. EPA Region 2 believes that the narcotic CBR approach can be applied in evaluating risk to the (marine) organisms of the HARS.

COMMENT 8-7: Several reviewers indicated that the CBR may not be adequate to protect for sublethal effects. This concern was voiced based on the observation that the narcotic CBR level used by EPA Region 2 was set with datasets having acute lethality as the primary endpoint.

RESPONSE 8-7: EPA Region 2 conducted a thorough literature search to identify studies in which observed sublethal effects could be correlated with simultaneously measured PAH body burdens in the affected organisms.

Hall and Oris (1991), the results of which were used in the development of the existing value for anthracene, reported sublethal effects (i.e., reduced reproductive output, reduced hatching success and/or increased incidence of developmental deformities) to be associated with accumulated anthracene. Based on our literature search, it appears appropriate to consider anthracene as a non-polar narcotic chemical (DiToro *et al.* 2000). Anthracene residues were reported in eggs, ovaries, and the remainder of the fish carcass for two of the exposure treatments. The consistency of the relative concentrations of accumulated anthracene in these three reservoirs (egg:ovary:carcass anthracene concentration ratio is 1:2:5) allowed an estimate of the total body burden to be generated from reported concentrations in eggs (3.75 ug/g) at the lowest effective treatment. The ovary was reported to constitute 13.4% of the total body weight of the fish in this study (i.e. the gonadosomatic index, or GSI). Using these relationships, the total body burden of anthracene associated with reduced reproductive output would equate to approximately 17.24 ug/g. This is equivalent to a molar concentration of 0.1 µmol/g. These values were based on long exposures (6-9 weeks) of adult fathead minnows.

Additional references were located that examined sublethal effects and narcotic body burdens simultaneously. Lotufo (1998) reported the critical body residues of fluoranthene in two marine crustaceans that were found to be associated with sublethal effects (i.e. reduced survival of offspring and/or reduced feeding activity). A molar concentration of 0.2 µmol/g was the lower of the body residues associated with sublethal effects in this study. Sublethal effects of PCBs, DDE and other narcotics in a freshwater worm were associated with body burdens between 0.34 - 0.56 µmol/g. Chronic toxicity (35-day exposure) was associated with body burdens between 0.9 - 1.35 µmol/g. Wang *et al.* (1998) found

significant reduction in growth of brine shrimp to be associated with body burdens of 0.08-0.9 µmol/g. Van Hoogen and Opperhuizen (1988), McCarty and MacKay (1993), and Sijm *et al.* (1993) estimate chronic mortality of fishes due to narcosis to occur at body burdens of 0.2 to 0.8 µmol/g. Finger *et al.* (1985) reported reduced growth to be associated with a 0.05 - 0.14 umol/g in bluegills that were chronically exposed to a narcotic contaminant (i.e. fluorene). In addition, decreased feeding activity of mussels was reported to be associated with narcotic body burdens of 0.05 µmol/g (Eertman *et al.* 1995) and 0.08 - 0.24 µmol/g (Donkin *et al.* 1993).

Emery and Dillon (1996) reported a very low body burden (i.e. 0.0044 µmol/g) associated with decreased reproduction of the marine worm, *Neanthes arenaceodentata,* after chronic exposure to a neutral narcotic organic contaminant (i.e. the PAH, phenanthrene). In that study, the authors speculated that other modes of action may have been contributors to the observed effect. Although evidence to support this conclusion was not provided by the authors, it is clear from review of the scientific literature regarding narcosis that the reported residue could not have been associated with a narcotic response. In addition, this study included only a single treatment. Therefore, dose-dependency of the response could not be confirmed (nor rejected).

In summary, the CBR method has been used in the last several years to assess sublethal effects of narcotic compounds. EPA Region 2 is proposing to adopt 0.05 µmol/g as a conservative endpoint for screening for sublethal narcotic effects associated with neutral nonpolar organic contaminants (PAHs, PCBs, chlorinated pesticides, dioxins/furans, and 1,4-dichlorobenzene).

Charge Group: Calculations

Charge No. 9: Should total PCBs continue to be estimated by doubling the total of 22 congeners or should it be quantified directly using another measure of quantification? What method is most appropriate for sediments in the NY/NJ Harbor area? ***(Please see Reference No. 60, Table 4-4B)***

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 9 Comment (1 para):
I am not an analytical chemist and cannot recommend specific methods for PCB congener analysis. PCB congeners tend to covary in their distribution even though their relative concentrations may change according to source. The 22 PCB congener analytes required in the total PCB quantification include 19 of the 21 congeners recommended in the Green Book for the summation of total PCBs. The list therefore should provide an adequate total PCB quantification that would reflect the distribution of other, unmeasured congeners. In marginal cases, additional analyses should be conducted. The formula for the extrapolation of the sum of the 22 congeners to all congeners (i.e., total PCB = 2.19 x (sum of the 22 congeners) + 2.19) is attributed in Table 4-4B of reference 60 to a 1992 personal communication from T. Wade. That is a very weak source for such an important equation. T. Wade (or someone else) should document the derivation of this equation for the record.

Gentile Charge No. 9 Comment (1 para):
The approach recommended in the Green Book (p 9-8 and 9-9) for estimating total PCBs by summing the individual 22 congeners of concern should be continued as it more accurately represents the PCB concentrations in the samples than by measuring total arochlors. Further, this is the approach used by NOAA and reflects the congeners relevant to environmental abundance, persistence, and most importantly biological importance

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Berry/Lake/Pruell Charge No. 9 Comment (1 para):
Dr. Lake did not think that total PCBs should be based upon a limited subset of congeners. Organisms can substantially alter PCB distributions relative to those present in Aroclors or in sediments, and by measuring only a limited subset substantial errors can be introduced. He thought that the methods for quantitation of all, or almost all, congeners exist, and these procedures are not much more difficult than those already in use for determining the 22 congeners currently measured. Dr. Pruell felt that, although some information might be lost by looking at only a subset of the PCBs, the error would be less than a factor of 2-3, an error that seems easily within the level of variability in many of the other assumptions required to assess the risk of PCBs. He felt that the small gain from doing all of the congeners was probably not worth the added expense.

Solomon/Sibley Charge No. 9 Comment (1 para):
This reviewer is not familiar with recent advances in the analysis of PCBs, however, the doubling to account for unquantified congeners seems a reasonable approach as it is based on historical experience.

Newman Charge No. 9 Comment (1 para):
I am not qualified to answer this question.

Lee Charge No. 9 Comment (1 para):
A congener approach is much preferred to Aroclors though it is not practical to quantify all 209 congeners on a regular basis. Therefore, the best strategy is to derive ratios of total to a measured suite of congeners based on empirical studies and/or what is known about the original composition of the PCB mix and how the various congeners breakdown. I am not an analytical chemist, so can not comment directly on whether the doubling is the correct ratio. It is important, however, that the measured suite of PCB congeners include both the environmentally common ones (e.g., 154) and the less abundant but toxicologically important ones (e.g., co-planars).

Dillon Charge No. 9 Comment (1 para):
Could not evaluate the PCB doubling method with materials provided. Reference 60, Table 4-4B did not describe the method in sufficient detail. The original reference cited in the table (NYSDEC, 1991) was not provided.

Agency Response to Reviewers' Comments:

COMMENT 9-1: There was a general consensus among the peer reviewers that quantification of PCBs using a congener approach is more appropriate than estimating total PCB concentrations based on Aroclor analyses.

RESPONSE 9-1: EPA Region 2 agrees with the peer reviewers and proposes to continue to require PCB concentrations to be analyzed and reported using congener-specific quantification methods in the proposed HARS Framework.

COMMENT 9-2: One reviewer suggested that it would be more accurate if all 209 PCB congeners were quantified. Most reviewers, however, suggested that because PCB congeners covary in their distributions, measurement of a subset of congeners and application of a correction factor to account for unmeasured congeners is appropriate. One reviewer commented that the formula for the extrapolation of the sum of the 22 congeners to all congeners needs to be better documented. Several reviewers also indicated that the 'correct' factor to apply to the selected congener subtotal must be robust and empirically supported.

RESPONSE 9-2: Ecological and human health risk of bioaccumulation of PCBs in dredged sediments is currently considered and managed as a function of the total concentration of PCBs present in the sample. It is therefore of paramount importance that whatever analytical method is used to quantify PCBs, it allows a relatively accurate estimate of the total PCB concentration to be generated.

Four data sets that measured PCB concentrations in environmental media in the New York Harbor area analyzing for an extensive list of PCB congeners (i.e., 79 or more congeners) were reviewed by EPA Region 2 to determine whether total PCB concentrations could be reliably estimated from the 22 congeners that are routinely quantified in the dredging program. These studies (Durell and Lizotte 1998; Battelle 1998; EPA 1992; and NOAA 1987) were conducted on sewage influents/effluents, dredged material, bottom sediments, and mussel tissues respectively. In all cases, the subset of 22 congeners that is routinely quantified in the dredging program was highly correlated with, and predictive of ($R^2 > 0.97$), the total PCB concentrations actually measured in those studies. Therefore, quantification of an extended list of congeners appears unnecessary to provide a reasonable estimate of total PCBs.

The ratio of the subtotal of the 22 congeners to the total PCB concentration reported for mussel tissues by NOAA (1987) was 2.0 (n = 21). While mussel PCB residues may not be directly comparable to other benthic species, there is data which suggests that some degree of comparability can be expected. Specifically, Connolly (1991) compared the relative contributions of different PCB homologues to total PCB concentrations measured in tissues of mussels, clams and polychaetes of New Bedford Harbor. This study revealed that the distribution and contribution of PCB homologues to total PCB residues were similar between the three species. This similarity in PCB uptake and disposition across benthic organisms suggests that the 22 congener subset : total PCB ratio reported for mussels (NOAA, 1987) may be appropriately extrapolated to other benthic organisms. Therefore, EPA Region 2 proposes to continue to estimate total PCB residues in test organism tissues by the existing method (i.e., doubling the sum of the measured residues of the 22 PCB congeners specified in the Regional Testing Manual).

EPA Region 2 is proposing to require quantitation and reporting of coplanar PCB residues for consideration in the assessment of ecological and human health risks associated with dredged sediments proposed for use as Remediation Material at the HARS (see Response to Comment 9-3, below). Because the method for determining total PCBs from the 22 measured congeners (i.e., doubling measured residues of the congeners listed in the RTM) is only an estimate of actual PCB mass in the sample and because the coplanar PCB congeners generally occur at much lower levels than other congeners (i.e., at pptr levels); EPA Region 2 believes that adding the residues of the three coplanar PCBs to the residue of the 22 current PCBs before applying the multiplier of 2 would not significantly improve the estimate of total PCB mass. Therefore, Region 2 is proposing to continue to estimate total PCBs using the current procedure of doubling the summed residue of the 22 PCB congeners currently considered.

COMMENT 9-3: Questions were raised regarding the specific congeners that are included on the list for direct measurement and reporting. One reviewer indicated that the subset of congeners evaluated should include the environmentally common congeners as well as toxicologically important ones (e.g., co-planar PCBs). Two reviewers indicated that the 22 congeners recommended in the Green Book and quantified in our process reflected the environmentally common and toxicologically important congeners and therefore appeared to be appropriate.

RESPONSE 9-3: The list of 22 congeners currently quantified in assessing suitability of dredged material for use as Remediation Material at the HARS includes the 18 congeners that are used to quantify and estimate total PCBs in other environmental monitoring programs, such as EPA's Environmental Monitoring and Assessment Program (EMAP) and NOAA's Status and Trends. As indicated in Response 9-2, total concentrations are used to assess suitability and can be reliably predicted from this

subset. In the interest of preserving the compatibility and comparability of data across these programs, EPA Region 2 is proposing to continue to include these relevant congeners on their list of required congeners for quantification of PCBs in dredged material proposed as Remediation Material at the HARS under the proposed Framework. As the total PCB concentration can be reliably estimated from the subtotal of these 22 congeners, EPA Region 2 does not feel that the additional expense of adding congeners for the purpose of estimating total PCB concentrations is warranted.

While extension of the PCB congener analytes for estimating total PCBs is not warranted for estimation of total PCBs, Region 2 is proposing to require analysis of three coplanar PCBs which have dioxin-like toxicological properties. The Regional Testing Manual (EPA/CENAN, 1992) lists the four coplanar PCB congeners (i.e. PCB congeners 77, 126, 156, and 169) as optional analytes. EPA Region 2 proposes to require analysis of the three PCB coplanar congeners that are listed by the World Health Organization (WHO 1999) as having associated 2,3,7,8-TCDD toxic equivalencies (i.e. PCB congeners 77, 126, and 169). As stated above (see Response to Comment 9-2), EPA Region 2 is proposing that the mass of these PCB congeners would not be used to estimate total PCB mass, their presence (and associated risk) would be assessed in the consideration of total 2,3,7,8 - TCDD toxic equivalency using the toxic equivalency factors recommended by the World Health Organization (see Response to Comment 5).

Charge Group: Calculations

Charge No. 10: Currently, 28-day tissue concentrations of certain organic contaminants are adjusted by some multiplier to estimate the concentrations of those compounds had the exposure been of sufficient duration to allow attainment of steady state levels. *(Please see Reference Nos.5 and 46)* Are these adjustments appropriate? Should steady state corrections be applied to any other of the listed contaminants? Are there other compounds for which we test that are not expected to approach steady state within the 28-day period?

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 10 Comment (3 para):
It is appropriate to adjust 28-d tissue levels to steady state tissue levels before comparison to tissue standards based on chronic exposures. The literature clearly shows that some compounds achieve only a fraction of their steady state concentration during 28-d exposure (Pruell *et al.* 1993, Lee *et al.* 1994). Since benthic tissues in the field will achieve steady state contaminant concentrations, correction of the 28-d data is essential.

The correction factors should be derived from 28-d and much longer experiments with the test species used in the standard 28-d test. Thus, the factors for PAHs (McFarland 1995), pesticides (Lee *et al.* 1994), and PCBs (Pruell *et al.* 1993) are based on appropriate methods. Boese *et al.* (1997; ET&C 16:1545-1563) reported additional data on PCBs that confirm an average correction factor of about 1 for 13 PCB congeners accumulated by Macoma. I am uncertain about the accuracy of the factor for heptachlor epoxide derived from 32-d tests with fish (Veith *et al.* 1979), or the factor for 1,4-dichlorobenzene, derived from the de Bruijn *et al.* (1989) Kow experiments.

The tissue concentrations of dioxins are not corrected for steady state in Table 1 of the Framework. Pruell *et al.* (1993) demonstrated that Nereis tissue concentrations of 2,3,7,8-TCDD and 2,3,7,8-TCDF were significantly and substantially higher after 120 days than at 28 days of exposure. Steady state correction factors should be derived for those two compounds from the Pruell *et al* (1993) data. In the absence of better data, the mean correction factor for those two compounds could be applied to other dioxin congeners. Pruell *et al* (1993) showed that there was no significant difference in Macoma tissue concentrations of dioxins between 28 and 128 days, so no steady state correction is necessary for that species.

Gentile Charge No. 10 Comment (2 para):
Yes the adjustments are appropriate given they provide value added to the decision-making. For example, with dioxins, the residues in Nereis at 28-days were only ~25% of the steady state value achieved after 180 days. If the differences between polychaetes and bivalves occur across a wide range of chemicals within certain log K_{ow} ranges then adjustments could be predicted. Another way to look at the question is to determine the maximum difference between the 28-day tissue concentration and the

steady-state value and compare that difference to the variability in replicate bioaccumulation tests. If the difference is less than the variability then one could argue that the additional effort to obtain steady-state values would be lost in the noise. If the difference is about 50% as it appears to be in the bivalves then I wouldn't be too concerned but if it was about 50% as it appears to be in the polychaetes I'd conduct a more extensive examination.

The application of a 'multiplier' to other listed contaminants should only be considered if there is data to support it.

Berry/Lake/Pruell Charge No. 10 Comment (1 para):
Dr. Pruell felt that the multipliers were probably the best available, and knew of no others.

Solomon/Sibley Charge No. 10 Comment (1 para):
The use of a multiplier to estimate the equilibrium concentrations of those compounds that have not reached steady state levels in 28-d exposures is judged to be reasonable, based on observations and experiences with experimental studies where long-term body-burdens have been measured (Lee *et al.* 1994; Pruell *et al.* 1993). As a general rule of thumb, correction factors should be applied where log K_{OW} is greater than 4 and half-life for depuration from the tissue is more than 9 days.

Newman Charge No. 10 Comment (2 para):
This is my major concern with the process. I believe that such use of multipliers will not provide an accurate measure of steady state concentrations for many of the toxicants of concern. Therefore all decisions based on these numbers are compromised. By using one test, one will not be able to fit simple models for accumulation through time and use the model parameters to estimate practical steady state concentrations of all toxicants of concern. Optimal design for some contaminants relative to the spacing of sampling through time and number of individuals required at each sampling would render such a test compromised or inadequate for other contaminants. This is probably the reason for defaulting to multiplying the 28 day bioaccumulation test concentration by some factor. Regardless, I believe that the multipliers are not the solution to getting steady state concentrations. The cited references provide conditional information that may not hold in other situations (e.g., sediments with very different organic carbon or AVS content, species with different lipid contents, species with different detoxification/sequestration mechanisms, or different temperatures or feeding rates). The cited references deal with organic compounds only. Is there sound evidence to support the universal utility of these multipliers? Is there evidence that they are grossly universal in their utility? Were these values to be checked against the realized concentrations in benthic species at the site from which the dredge material will be taken (as suggested in ref. 5)?

Some results from various publications are applied incorrectly, i.e., using 28 day bioaccumulation for Hg and Cd because they do not conform to the steady state context. Wouldn't it be more accurate to estimate the concentration that would be reached during the average life span of the organism since the Cd or Hg concentrations continue to rise with time and do not appear to come to a steady state concentration?

Lee Charge No. 10 Comment (3 para):
The 28 day duration of the bioaccumulation test was a compromise between practicality and a duration that approached steady-state. Since establishing the 28-day test, new evidence indicates that a number of compounds, at least under some circumstances, do not approach within 80% of steady-state in 28 day. Since the purpose of the bioaccumulation tests is to estimate human and ecological risk, it is critical to have reasonable estimates of tissue residues in the field. Therefore, adjustments should be made on a compound-by-compound basis. Note that using an adjustment is not a "conservative" assumption (like use of a UF) but a method to correct for a lab artifact (duration). As the data become available, the need for an adjustment factor for all the organics should be evaluated. In particular, the use of an adjustment factor for the dioxins/furans needs to be considered. Rubinstein did some uptake work on 2,3,7,8-TCDD that could be used and there is probably more recent work.

Adjustment factors for metals are problematical. We did not see simple patterns when we reviewed metal BAFs (Boese and Lee, 1992), but there are cases when 28 days is insufficient to approach 80% of steady-state. I have not reviewed metal data since then, but at that time it was my belief that we did not have sufficient understand of matrix effects and/or physiological effects (e.g., incorporating metals into jaws) to derive precise adjustment factors. If a particular metal is of high concern, you should consider conducting longer term tests. As a general comment, the use of 10-day bioaccumulation test for metals is simply not scientifically defensible. We may not totally understand metal kinetics but there are abundant data (much of which was available when the Green Book was written) to show that 10 days often (usually?) does not approach steady-state and can even result in false negatives about identifying which metals are bioavailable. Question 11.

Because of their short life span of benthic species, B(a)P and other PAHs probably act as neutral narcotics rather than carcinogens, so the CBR approach is a more appropriate for ecological risk. (Note: Calculation of CBR really needs to be done on a molar basis rather than a concentration basis, as done on page 15. Fluorene and naphthlene can be added on a ppt basis only if they have same molecular weight.). Assessing human health risk is not my area of expertise. Based on what I know about toxic equivalents with dioxins, I suspect that the individual potency factors are not well known. Nonetheless, as with the dioxins, the sum is probably a better estimate of cancer risk than just evaluating the PAHs individually.

Agency Response to Reviewers' Comments:

COMMENT 10-1: One reviewer suggested that the use of multipliers to adjust for steady state was inappropriate. This reviewer felt that the data used to derive the multipliers was not adequate and that decisions based on these values would be compromised. One peer reviewer indicated that the development of adjustment factors for metals is problematic and suggested that EPA consider using longer duration tests (>28 days) when there is high concern regarding a particular metal.

RESPONSE 10-1: The opinion expressed by this reviewer supports the recommendation that 28-day bioaccumulation tests be abandoned in favor of collection and analysis of organisms that reside in sediments at the dredging site. The utility, practicability, and appropriateness of that recommendation is discussed in response to comment 1-7.

EPA Region 2 acknowledges that there are uncertainties associated with the use of multipliers to estimate steady state. However, EPA Region 2 believes that scientifically defensible and practicable alternatives

to the use of multipliers do not exist. EPA Region 2 also believes that use of 28-day bioaccumulation tests and application of multipliers, where necessary, is the most appropriate means of assessing potential for long term bioaccumulation of organic contaminants in dredged material. The majority of peer reviewers concurred with this assessment of the state of the science. Therefore, EPA Region 2 is proposing to continue to use 28-day bioaccumulation tests in the proposed Framework, adjusting the results appropriately to reflect steady state.

Depending on the specific organic contaminant, an appropriate multiplier may be calculated from the results of empirical (long-term) studies or derived theoretically based on the chemical and physical properties (e.g., octanol/water partition coefficients) of the contaminant (Lee *et al.*, 1994; McFarland, 1995). Where valid empirical data are available, EPA Region 2 proposes to use them preferentially over theoretical methods to derive the specific multiplier for organic contaminants. Where empirical data are unavailable, EPA Region 2 proposes to use the theoretical methods to assign the specific multiplier. EPA Region 2 proposes to continue using 28-day tests with application of the best available multiplier to assess the potential bioaccumulation of organic contaminants from dredged material that is proposed for placement at the HARS.

With regard to uptake of metals and the use of longer term exposure testing, EPA acknowledges the inherent difficulties associated with estimating steady state based on the results of 28-day bioaccumulation tests, but believes the 28-day bioassay approach to be sound and scientifically appropriate (see Response to Comment 10-3). Also, there are no alternative bioaccumulation test methods currently approved for incorporation into the regulatory process, as outlined in the Green Book (EPA/USACE, 1991). Therefore, EPA Region 2 proposes to continue using 28-day tests with application of the best available multiplier to assess the potential bioaccumulation of metals from dredged material that is proposed for placement at the HARS.

COMMENT 10-2: One peer reviewer suggested using Lee *et al.* (1994) to determine the need for steady-state correction factors. In that reference, they suggest that correction factors should generally be applied for those organic chemicals with a log K_{ow} greater than 4 and a depuration half-life of more than nine days. Another reviewer suggested that correction factors only be applied to 28-day results when the variability in replicate bioaccumulation tests is less than 50% of the difference between the 28-day and steady-state tissue concentrations.

RESPONSE 10-2: EPA Region 2 agrees that these physiochemical characteristics appear appropriate for determining the need for application of steady-state correction factors to 28-day measured concentrations of organic contaminants (i.e. whether tissue concentrations measured in bioaccumulation test organisms following 28-days of exposure represent steady state concentrations). The theoretical relationship between log K_{ow} of neutral organic compounds and the proportion of the steady state concentration that is attained after 28-days of exposure is shown in Figure 6-1 of the Inland Testing Manual (EPA/USACE 1998). The relationship reported in Figure 6-1 is proposed for use in the HARS Framework to: (a) determine the need for application of steady state correction factors for neutral organic contaminants; and (b) estimate the appropriate correction factor *if* an empirically-derived correction factor is not available in the scientific literature.

Limiting application of the steady state correction factors to those test results in which variability is lowest does not appear to be practicable or appropriate. Limiting application of the steady state correction

factors in this manner presents certain practical and regulatory difficulties. Strict QA/QC guidelines exist for conducting bioaccumulation test exposures and for chemical analysis of tissues (outlined in the Green Book (EPA/USACE, 1991) and the EPA Region 2/USACE-NYD Regional Testing Manual (EPA/CENAN 1992)). These guidelines are designed to minimize variability in test results. September 14, 2000 these testing guidelines, variability in test results may be introduced by non-treatment factors (e.g., laboratory performance). By not applying steady state correction factors to highly varying test results, we risk 'rewarding' poor laboratory performance. Therefore, EPA Region 2 proposes to continue to apply steady state multipliers, where appropriate, to all 28-day bioaccumulation test results under the proposed HARS Framework.

COMMENT 10-3: One peer reviewer indicated that the development of adjustment factors for metals is problematic and suggested that EPA consider using longer duration bioaccumulation tests (>28 days) when there is a high level of concern regarding a particular metal. One reviewer suggested that because metals may not conform to the steady state context, it might be more accurate to estimate the concentration reached during the average lifespan of the organism.

RESPONSE 10-3: EPA agrees that the bioaccumulation of metals by marine organisms is much more complex than for organic compounds. This process can be dramatically affected by factors such as metal speciation and the formation of various complexes (e.g., ligand associations, etc.) (Simkiss and Taylor, 1989; Newan and Jagoe, 1994). In fact, there is evidence to indicate that in many situations a "true" steady state is never attained for metals because tissue body burdens vary seasonally due to differences in growth, body composition, sexual condition, nutrition, temperature and salinity (Amiard-Triquet *et al.*, 1986; Coleman *et al.*, 1986; Coimbra and Carraca, 1990; Swaileh and Adelung, 1994). An extensive review of the literature was conducted to evaluate the manner in which metals are accumulated by benthic organisms, including bivalves, polychaetes, and crustaceans (Battelle, 1999a). This review concluded that substantial differences in uptake, regulation and depuration by marine organisms appear to exist between essential and non-essential metals.

For essential metals (i.e. arsenic, chromium, copper, nickel, and zinc), an initial increase in tissue concentrations was typically observed, with tissue concentrations leveling off with continued exposure. This relationship was observed at all but the highest exposure concentrations (White and Rainbow, 1982; Camusso *et al.*, 1994), indicating that many marine organisms have the ability to regulate those metals that are essential to their health (e.g., copper and zinc), despite large variance in the sediment concentrations to which they are exposed (Camusso *et al.*, 1994; Ahsanullahs and Williams, 1991; Rainbow and White, 1989; White and Rainbow, 1982). For example, concentrations of zinc appear to plateau around 200 mg/kg, despite elevated external exposure concentrations (White and Rainbow, 1982). This implies that the continued uptake of non-essential metals is primarily a function of the tissue concentration, rather than the length of exposure or a steady-state relationship with external media.

For non-essential metals (i.e., silver, cadmium, mercury, and lead), the relationship is less clear. The ability of benthic organisms to regulate the uptake of non-essential metals appears to be very much diminished, with levels often appearing to accumulate over time (Camusso *et al.*, 1994; Ahsanullah and Williams., 1991). However, despite the apparent uptake into tissues, there is evidence to indicate that metabolic mechanisms work to eliminate these nonessential elements from the organism when body burdens reach certain levels of saturation (Holcombe *et al.*, 1976; Spehar, 1976; Riisgard *et al.*, 1985).

Based on the above information, it appears clear that the kinetics of accumulation of metals by marine organisms is extremely complex and cannot easily be characterized in general terms. Application of steady state correction factors as defined for the organic compounds is not appropriate for metals, because a "true" steady state for metals does not exist.

The extrapolation of 28-day results to the lifetime of the organism is impractical because it is exceedingly difficult to estimate the average lifetime for a benthic organism. Likewise, assuming a constant linear rate of uptake of metals and extrapolation of a calculated daily uptake rate from the 28-day exposure for the life span of the organism also does not appear to be scientifically appropriate, given the documented effects of changes in a non-constant environment on uptake and depuration kinetics. Nevertheless, because of (1) the potential for 28-day tests to underestimate uptake of metals (particularly non-essential metals), (2) less evidence for regulation of non-essential metals by exposed marine organisms, and (3) the higher level of human health and ecological concern regarding non-essential metals (e.g. mercury and cadmium), EPA Region 2 has elected to derive a safety (uncertainty) factor to apply to 28-day bioaccumulation test results for non-essential metals for the proposed HARS Framework.

To develop an appropriate safety factor for application to 28-day test results for non-essential metals, EPA Region 2 compiled and compared sediment concentrations and polychaete tissue concentrations in co-located samples from the vicinity of the HARS, reported by Battelle (1995). It is assumed that metals concentrations measured in the organisms collected in this effort represent a range of exposure durations and conditions that are typical of benthic organisms at the HARS. The results of this analysis indicate that despite sediment concentrations which varied by as much as two orders of magnitude, tissue concentrations of non-essential metals in field collected benthic organisms varied within a factor of three (i.e., maximum reported concentrations of all non-essential metals were approximately three times higher than the lowest concentration reported). Therefore, EPA Region 2 is proposing to use a safety factor of three applied to the results of the 28-day bioaccumulation test for non-essential metals (i.e. silver, cadmium, mercury, and lead) to reflect this pattern in field tissue accumulation under the proposed HARS Framework. EPA Region 2 believes this represents an appropriately conservative factor to account for potential underestimation of non-essential metals accumulation by the 28-day duration of the test exposure.

COMMENT 10-4: Three peer reviewers suggested that steady state correction factors should be derived for and applied to 28-day bioaccumulation of dioxins and furans. One reviewer suggested that these correction factors be based on Pruell *et al.* (1993). This study suggested that there was no significant difference in *Macoma nasuta* tissue concentrations of dioxins between 28 and 128 days. Therefore, steady state correction factors would be necessary only for *Nereis virens*.

RESPONSE 10-4: See response to comment 5-8.

Charge Group: Calculations

Charge No. 11: Is the calculation and use of BaP toxicity equivalence an appropriate way to estimate the potential carcinogenicity of PAHs? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Section C.)*

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 11 Comment (1 para):
I think it is a reasonable way to estimate carcinogenicity, given the current state-of-the-science. The assumption of additivity inherent in the summation of TEFs reflects current understanding of the effects of PAHs. Toxicological data are scarce and not available for all compounds, but the use of BaP TEFs is probably the best way to estimate cumulative risk. However, the EPA/COE Memo for the Record has ignored the advice of U.S. EPA (1993), the cited source of the PAH TEFs. First, US EPA (1993) says, "These are not proposed as toxicity equivalency factors (TEF)", but the EPA/COE Memo identifies them as TEFs. This is more than a matter of semantics. Second, US EPA (1993) says, "The list of PAHs is not sufficiently extensive to meet the needs of Programs and Regions." There is a clear conflict between the uncertainties highlighted in the source document and the proposed application of these numbers.

Clifford Charge No. 11 Comment (1 para):
My concerns regarding use of body burdens as surrogates for exposure values aside, the conversion technique itself is, in general, appropriate. See comment response No. 2 regarding moles vs. milligrams.

Gentile Charge No. 11 Comment (1 para):
Yes, I thought the description and rationale was well thought out. The only question is that the BaP Toxicity Equivalence estimate relies entirely on data collected in 1980s. Isn't there more recent data that can be used to support the derivation of the equivalence value of 8,021ppb ?

Solomon/Sibley Charge No. 11 Comment (1 para):
BaP TEs are judged to be an appropriate method for estimating the carcinogenicity of PAHs. PAHs usually require metabolic activation as they are pro-carcinogens. With high exposures to mixtures of PAHs, metabolism may be reduced by substrate overload, thus lowering the carcinogenic risk. As exposures reported in this assessment are generally low, this is unlikely to occur, however, the qualifier discussed above in relation to extrapolation and repair mechanisms needs to be considered. The use of BaP TEQs is judged to be somewhat conservative.

Newman Charge No. 11 Comment (1 para):
The calculations and use of toxic equivalence appear adequate.

Dillon Charge No. 11 Comment (1 para):
PAH toxicity to aquatic organisms, not a BaP toxicity equivalence approach, is discussed in Appendix for Table 1, Section C. Carcinogenicity is an important endpoint for human health but generally not appropriate for ecological receptors.

Agency Response to Reviewers' Comments:

COMMENT 11-1: One reviewer notes that we should not identify the conversion factors as Toxicity Equivalency Factors (TEFs) (as per EPA, 1993).

RESPONSE 11-1: EPA Region 2 acknowledges that the conversion factors should not be identified as TEFs. EPA's (1993) provisional guidance for quantitative risk assessment of PAHs states that the "Office of Health and Environmental Assessment (OHEA) has chosen not to label the risk assessment numbers in this document as "toxicity equivalency factors" but rather as "estimated orders of potential potency." The reason for this determination was that PAH carcinogens do not meet all the guiding criteria for application of TEFs to mixtures (EPA, 1991), primarily because the toxicity data upon which to base the TEFs are not sufficient. Most toxicological studies involving PAHs are for carcinogenic, genotoxic, and mutagenic effects; there is only limited data for systemic (non-cancer) toxicity. Because of this deficiency, EPA (1993) recommends using the "weighting of potential potency" for carcinogenicity (see comment and response to comment 11-2). In addition, EPA (1993) reported that an adequate consensus on TEFs for PAHs had not been determined; different TEF values can be generated depending on the carcinogenicity test data evaluated. Another problem noted by EPA (1993) is that the additivity of PAH carcinogenic potencies has not been proven. However, the data supporting additivity of PAH carcinogenicity are of about the same quality as the data supporting additivity of toxicity of PCBs and PCDF/PCDDs, for which TEFs have been developed and are widely used.

Despite the limitations described above, EPA Region 2 believes that using the Benzo(a)pyrene (BaP) equivalency approach described in the Framework is the most appropriate method for evaluating the potential carcinogenicity of PAHs at this time (see response to comment 11-2). EPA Region 2 proposes to continue using the current TEF process for evaluation of the total carcinogenicity of PAHs. However, the values will be referred to as "potential potency factors" (i.e., PPFs) instead of TEFs.

COMMENT 11-2: Two of the reviewers caution that the BaP-equivalency approach is generally only applicable to the carcinogenic (human health) endpoint.

RESPONSE 11-2: EPA Region 2 agrees. Under the current and proposed HARS Frameworks, the BaP potential potency factors (PPF) approach for PAHs is only used to estimate cancer risks to human consumers of fishery products. This is consistent with EPA's 1993 provisional guidance for quantitative risk assessment of PAHs which states that "the guidance in this paper should be applied only to assessment of carcinogenic hazard from oral exposure to PAHs."

COMMENT 11-3: One reviewer inquires as to whether there are more recent data sets that we may use to set BaP-potential potency factors.

RESPONSE 11-3: EPA Region 2 agrees that the most recent applicable data sets available at this time should be used and a review of the literature was conducted to identify any applicable studies. A more recent study applied TEFs (i.e., potential potency factors) developed by Nisbet and LaGoy (1992) to estimate the carcinogenicity of PAH-contaminated air emissions from different pyrogenic sources (Petry et al., 1996). These factors were developed for 16 carcinogenic or suspected carcinogenic PAHs (Nisbet and LaGoy, 1992). Nevertheless, benzo(a)pyrene was found to be responsible for 27 to 67 percent of the estimated carcinogenicity of the complex air emissions samples, similar to the results reported by EPA (1993). Because of the importance of benzo(a)pyrene to the carcinogenicity of the mixtures, variations in the relative concentrations of other PAHs in the mixtures did not have much effect on the estimated carcinogenicity of the different air emissions. Thus, it is uncertain if use of alternative potential potency factors would improve the estimation of the carcinogenicity of PAH mixtures in fish from the HARS.

EPA Region 2 believes that use of the EPA (1993) potential potency factors provides a better estimate of carcinogenicity of the PAH mixture than would use of the carcinogenicity of benzo(a)pyrene alone or setting the carcinogenicity of each of the individual potentially carcinogenic PAHs in the samples equal to the carcinogenicity of benzo(a)pyrene. EPA Region 2 believes that the BaP PPFs used in the current framework are based on the best available information and, therefore, proposes to use them in the proposed revisions to the HARS Framework.

COMMENT 11-4: One reviewer indicates that molar concentrations are necessary for summation of the total BaP potential potency.

RESPONSE 11-4: EPA Region 2 agrees that the BaP potential potency approach should be based on molar concentrations rather than mass/mass concentrations. The carcinogenicity of a nonpolar organic chemical is proportional to the number of molecules of that chemical at the site(s) of carcinogenic action in the tissues of the organism. Equimolar concentrations of all chemicals contain the same number of molecules. Thus, molar concentrations are the better way to express the additive contributions of several chemicals to the carcinogenicity of a mixture. Glass and Easterly (1994) recommend using molar concentrations in deriving carcinogenicity potency factors for PAHs and other chemical carcinogens.

Most potency factors were developed based on laboratory toxicity tests that were reported in mass/mass doses, however, conversion from mass to molar units would be a simple task. This conversion would probably result in slightly different potency factor values. For example, the molecular weight of the carcinogenic PAHs that are present most frequently in tissues of marine fisheries products range from 228.3 (benz[a]anthracene) to 276.3 (indeno[1,2,3-cd]pyrene). The molecular weight of benzo(a)pyrene is 252.3. Therefore, the error introduced by using mass units instead of molar units is small, ± 10 percent. Despite the relatively small potential difference, the EPA proposes to incorporate this modification into the proposed Framework.

COMMENT 11-5: One reviewer noted that the CBR approach was probably more appropriate for evaluating the ecological risks for PAHs.

RESPONSE 11-5: EPA Region 2 agrees that the narcotic critical body residue (CBR) approach is a more appropriate model for evaluating ecological risks from PAHs than is the BaP potential potency factor approach. The cancer endpoint is not ordinarily used in ecological risk assessment, because cancer

risk is usually expressed as individual risk rather than population risk, and there are few data available on the carcinogenicity of nonpolar organic chemicals to marine organisms. In the proposed Framework, EPA Region 2 proposes to use the narcotic critical body residue approach to evaluate risks of PAHs to ecological receptors (see responses to comment 8) and to limit application of the BaP potential potency factor approach to assessing cancer risk to human consumers of fisheries products from the HARS area.

Charge Group: Calculations

Charge No. 12: Similar to PCBs, only a subset of those PAHS present in New York Harbor are measured for testing evaluation. How should the remainder be considered?

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 12 Comment (1 para):
PAHs tend to covary in their contaminant distributions. Measurement of 16 parent PAH compounds is likely to detect a PAH contamination problem. It is possible that in marginal cases a real problem might be missed if the contribution of other PAHs was necessary to exceed a critical body residue. To minimize this possibility, a couple of substituted PAHs could be added to analyte list, e.g. alkylated phenanthrenes or naphthalenes. Also, a GC/MS scan could be used to detect peaks that might represent other PAHs of concern on a site-specific basis.

Gentile Charge No. 12 Comment (1 para):
The selection of 22 PCB congeners was based upon their toxicology (e.g, potency), and biological importance, bioaccumulation potential, persistence, and presence in the environment. If, using these or some other set of criteria a case can be made for sub-setting the PAHs then it should be done particularly if it provides value added by providing additional predictive power, scientific defensibility, or reduction of uncertainty for the risk manager.

Berry/Lake/Pruell Charge No. 12 Comment (1 para):
Dr. Lake felt that PAHs pose a different problem than PCBs with regard to quantitation. Presently, there isn't a good way to effectively quantitate all PAHs (including alkyl homologues) present in extracts. The only alternative is to select a subset. Dr. Pruell agreed, and added that eventually we will need to get a better understanding of the effects of the other PAHs (as well as the saturated organic compounds).

Solomon/Sibley Charge No. 12 Comment (1 para):
In this reviewers experience, (with PAHs in creosote) the concentration of the 15 EPA priority PAHs follows the toxicity of the balance of the components of the mixture although, prior to weathering, the complete mixture is usually more toxic than would be predicted from the 15 priority PAHs. Given the age of the sediments in the site being assessed, the 15 priority PAHs are judged appropriate for estimating toxicity.

Newman Charge No. 12 Comment (1 para):
I lack sufficient background to give an informed answer.

Lee Charge No. 12 Comment (1 para):

The problem with the PAHs may be more difficult than with the PCBs since there are multiple natural and anthropogenic sources. Therefore, the ratios among various PAHs is likely to vary more than among PCB congeners. The simplest approach would be to have a single ratio of total to the measured PAHs based on empirical studies. I believe a better approach would be to break down the PAHs into functional classes and have empirical ratios for each. The simplest functional classes would be high and low molecular weight but there may be other grouping that better capture the various local sources (e.g., petroleum vs. combustion vs pyrogenic PAHs). Note that this problem becomes more acute if the alkylated-PAHs are included.

Dillon Charge No. 12 Comment (1 para):
See peer reviewer's responses to 8.A and 16.

Agency Response to Reviewers' Comments:

COMMENT 12-1: Several reviewers commented that current analytical methods cannot effectively quantitate all PAHs. Reviewers suggested alternate methods for quantifying PAHs, including EPA Method SW 846-8310 or a GC/MS scan.

RESPONSE 12-1: EPA Region 2 concurs that alternate analytical methods should be used in an attempt to achieve low detection limits and to detect PAH compounds that are not quantified by the current method (EPA Method 8270). To this end, EPA Region 2 evaluated alternate analytical methods to identify a method to attain these goals (quantitation of additional PAHs at low detection limits).

EPA Region 2 proposes to continue using EPA Method 8270 to quantify PAHs in test organism tissue. However, EPA Region 2 also proposes to require EPA Method 8270 to be performed with specific analytical sample cleanup procedures and analytical instrument configurations which have been optimized to allow detection and quantitation of parent PAHs and their alkylated homologues. Using these sample cleanup procedures and instrument configurations, a greater range of PAH compounds (i.e. unsubstituted parent PAHs and their C_1 to C_4 alkylated homologues) could be measured than is possible using other standard methodologies. This analytical option has been used by the National Oceanic and Atmospheric Administration (NOAA) National Status and Trends (NS&T) Program (NOAA, 1998). The required sample cleanup procedures and instrument configurations are detailed in *Procedures for Analysis of PAH and Alkyl PAH in Sediment and Tissue at Risk-based Detection Limits* (Battelle, 1999b). A copy of Battelle (1999b) is included as Appendix G of this document.

Method detection limits (dry weight) of approximately 10 ug/kg and 5 ug/kg are associated with measurement of individual PAH compounds in sediment and tissue matrices (respectively), using these procedures. These low detection limits represent an additional advantage of using this method for measuring trace levels of PAHs.

Using the proposed sample cleanup and instrument configurations described in Battelle (1999b), EPA Region 2 proposes to designate the additional alkylated homologues as standard target analytes, in addition to the parent PAH compounds. However, because calibration standards do not exist for the thousands of alkylated PAHs that are detected and quantified using the proposed procedures, measured concentrations of the C_1 to C_4 alkylated homologues of the parent (unsubstituted) PAHs would be considered as

estimates of the actual concentration present for purposes of assessing risk. Overall data acceptability would be determined using measures of laboratory performance generated for the parent PAH compounds for which calibration standards are available.

COMMENT 12-2: Two of the reviewers felt that given the current uncertainties associated with detecting and evaluating the wide range of possible PAH compounds, evaluation of a standard subset (i.e., the 15 priority pollutant PAHs) is appropriate, similar to the method established for the PCB congeners. One of the reviewers suggested that these subsets should be based on similarities in toxicology. The other recommended creating functional classes reflecting local sources (e.g., petroleum associated PAHs versus combustion PAHs).

RESPONSE 12-2: EPA Region 2 acknowledges the suggestion for quantifying a subset of PAHs. However, as discussed in the response to comment 12-1, robust sample cleanup and instrument configuration procedures are proposed to enable EPA Method 8270 to address the need to quantify the full range of PAHs in the sample. These procedures would allow detection of the parent (unsubstituted) PAHs and their C_1 to C_4 alkyl homologues.

Molar concentrations of these compounds can be readily estimated using these procedures which will allow the data to be readily incorporated into the narcotic critical body residue (CBR) model (see responses to comments 8 and 11). Because PAHs act toxicologically as Type 1 nonpolar narcotic contaminants (DiToro *et al.*, 2000), EPA Region 2 does not feel that it will be necessary to group the PAH compounds into further subsets as suggested by the peer reviewer. Furthermore, EPA Region 2 believes that evaluation of this comprehensive list of PAHs would also be more protective of human health.

COMMENT 12-3: One reviewer noted that while the current methods are appropriate for evaluating the direct toxicity of PAHs to benthos, the potential effect of the associated metabolites should be considered when evaluating food web transfers.

RESPONSE 12-3: EPA Region 2 acknowledges the concern that PAH metabolites should be considered when evaluating food web transfers. Two studies were identified which compared the relative trophic transfer efficiencies of a PAH and its metabolites in fish (McElroy and Sisson, 1989; McElroy *et al.*, 1991). In those studies, the authors reported that PAH metabolites can be transferred to predators from prey. However, transfer of metabolites to fish is significantly lower than transfer of the parent PAH. There is currently very little information available with which to quantify the potential toxicological effects of these compounds. As a result of the paucity of relevant toxicological information regarding PAH metabolites, EPA Region 2 believes that PAH metabolites cannot be effectively considered at this time in the evaluation of the potential food web effects of PAHs.

Charge Group: Calculations

Charge No. 13: Is the assumption of a trophic transfer coefficient of one appropriate for use in evaluating the potential for human health and ecological impacts associated with metals in Remediation Material? Are the trophic transfer factors calculated for organic compounds correct? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Attachment C.)*

Summary of Peer Review Comments

Individual Comments

Bartell Charge No. 13 Comment (2 para):
It was stated in the body of the report (p. 12) that the trophic transfer coefficients for metals were conservatively assigned a value of 1.0. Curiously, arsenic is assigned a value of 3, suggesting the potential for biomagnification, which if justified for any metal would pertain mainly to methyl mercury. However, in the absence of human health or toxicity data, arsenic drops out of the analysis at any rate (i.e., Table 1).

The values of trophic transfer coefficients for PAH's were also conservatively selected (i.e., 0.1, Attachment C). Studies described on p. 14 suggest >90% elimination or metabolism of ingested PAHs. A transfer coefficient of 0.02 was cited between fish and invertebrates, although the value might have been as high as 0.23. The transfer values for pesticides were derived using the Gobas (1993) model, which was developed originally for PCBs. The resulting values ranged from 1 - 2.47 and appear consistent at least with observations of some pesticide biomagnification.

Clifford Charge No. 13 Comment (1 para):
My concerns regarding use of body burdens as surrogates for exposure values aside, the conversion technique itself is, in general, appropriate. See comment response No. 2 regarding moles vs. milligrams.

Gentile Charge No. 13 Comment (1 para):
Yes, assuming the assumptions, arguments and data used to support them are accurate.

Berry/Lake/Pruell Charge No. 13 Comment (1 para):
Dr. Pruell felt that a trophic transfer coefficient of one may not be appropriate for the metals. It is probably too low for mercury, and too high for the other metals. Further, Dr. Pruell felt that there were good transfer coefficients available for many of the organics. Much of the relevant work has been done at EPA's laboratory in Duluth, Minnesota, by Phil Cook and others.

Wentsel Charge No. 13 Comment (1 para):
Use of a trophic transfer of 1 for metals is appropriate for metals (excluding MeHg) in risk assessments. The conservative aspects of the assessment do not require bioavailability issues to be addressed.

Solomon/Sibley Charge No. 13 Comment (1 para):
The trophic transfer factors used in evaluation of human and ecotoxicological health in this assessment are judged to be appropriate.

Newman Charge No. 13 Comment (1 para):
The trophic transfer coefficient of 1 seems conservative (i.e., it could be less than 1 for some metals) except for mercury. Because mercury will biomagnify, the coefficient greater than this is required. Please see the references cited below for the appropriate coefficient for mercury.

Dillon Charge No. 13 Comment (1 para):
A TTC of 1 is appropriate for metals with the exception of organo-metals which would have higher values. Published TTC values for organics vary with study, chemical and trophic level. The "correct" TTC is one developed for the specific chemical and trophic level of concern.

Agency Response to Reviewers' Comments:

COMMENT 13-1: Several reviewers believed that the trophic transfer factors for metals may be too high (i.e., conservative) for those metals that do not have an organo-metallic form. Values less than 1 may be appropriate for these compounds.

RESPONSE 13-1: EPA Region 2 agrees that the use of a trophic transfer factor of 1 for all metals appears to be conservative. As described in Appendix E, EPA Region 2 performed an extensive review of the available scientific literature to determine the potential for dietary transfer of metals to finfish from contaminated benthic invertebrate prey (also see response to comment 8). The data reviewed support the conclusion that dietary transfer of metals to fish from prey is generally inefficient, usually resulting in trophic transfer factors of less than one (Appendix E; Reinfelder *et al.*, 1998; Suedel *et al.*, 1994). Based on this review, EPA Region 2 will propose alternative values for four cationic metals (i.e., copper, cadmium, lead and zinc) and arsenic.

For the purpose of deriving alternative trophic transfer values, EPA Region 2 focused on recent laboratory studies conducted with field collected benthic organisms from areas known to be contaminated by metals (Woodward *et al.*, 1994; 1995; Farag *et al.*, 1994; 1999; 2000). These studies provided evidence that using contaminated feeds or laboratory-contaminated prey may underestimate the potential for trophic transfer of metals to fish (Farag *et al.*, 2000; Harrison and Curtis, 1992). Based on these data (Appendix E), the following trophic transfer factors (i.e., benthic invertebrates to fish) were derived and are being proposed as part of the revised HARS Framework:

Chemical	Trophic Transfer: Prey to Fish	Trophic Transfer: Fish to Prey
Arsenic	0.25 ($[As]_{fish}$ / $[As]_{prey}$)	4.00 ($[As]_{prey}$ / $[As]_{fish}$)
Cadmium	0.25 ($[Cd]_{fish}$ / $[Cd]_{prey}$)	4.00 ($[Cd]_{prey}$ / $[Cd]_{fish}$)
Copper	0.21 ($[Cu]_{fish}$ / $[Cu]_{prey}$)	4.76 ($[Cu]_{prey}$ / $[Cu]_{fish}$)
Lead	0.23 ($[Pb]_{fish}$ / $[Pb]_{prey}$)	4.35 ($[Pb]_{prey}$ / $[Pb]_{fish}$)
Zinc	0.24 ($[Zn]_{fish}$ / $[Zn]_{prey}$)	4.17 ($[Zn]_{prey}$ / $[Zn]_{fish}$)

In the absence of sufficient data to derive chemical-specific trophic transfer factors, EPA Region 2 is proposing to continue to use a conservative factor of 1 be applied to all of the remaining metals except for mercury (see response to comment 13-2). EPA Region 2 believes that these values are appropriately conservative for the purpose of protecting human health and the environment, and to meet the remedial goal of the HARS.

COMMENT 13-2: The majority of the reviewers felt that a trophic transfer factor of 1 was too low for mercury, the organic form of which is expected to biomagnify.

RESPONSE 13-2: EPA Region 2 agrees that a trophic transfer factor of 1 appears to be low for mercury. Mercury occurs in three valence states in marine ecosystems: zero (elemental), monovalent (mercurous), and divalent (mercuric) compounds (Rolfhus and Fitzgerald, 1995). These states may be transformed to organomercury compounds in aquatic systems, particularly methylmercury. Concentrations of the inorganic forms of mercury in marine animal tissues tend to decrease with increasing trophic level. In contrast, methylmercury is highly bioavailable and tends to biomagnify in marine food webs. There is evidence to indicate that the biomagnification of methylmercury resembles that of hydrophobic trace pollutants such as PCBs (Cabana *et al.*, 1994; Mason *et al.*, 1995).

Based on the available literature, reported trophic transfer factors for mercury are quite variable, often because the forms of mercury in different environmental compartments have not been documented (Suedel *et al.*, 1994). In addition, most studies evaluating increased mercury concentrations at the top of aquatic food chains have typically not accounted for differences in body size and physiology (Cabana *et al.*, 1994). As discussed by Huckabee *et al.* (1979), factors such as longevity, growth rates, uptake, and depuration among species at different trophic levels may have as much impact on observed chemical concentrations as food chain transfers. To address this issue Cabana *et al.* (1994) conducted an evaluation of mercury levels using individuals from a single species exposed via food chains of varying lengths. Based on this evaluation, EPA Region 2 is proposing a trophic transfer factor of 1.95 which was derived for total mercury. This value is appropriate for methylmercury and thus was, determined to be appropriately conservative for the purpose of protecting human health and the environment and to meet the remedial goal of the HARS.

While the trophic transfer factor of 1.95 is considered to be appropriate for total mercury, when evaluating speciated analyses of mercury, this would be very conservative for estimating trophic transfer of inorganic mercury compounds. Therefore, in those cases where speciated forms of mercury are reported, a trophic transfer factor of 1 will be applied for the inorganic mercury species.

COMMENT 13-3: One reviewer commented that the trophic transfer coefficients for PAHs were too conservative.

RESPONSE 13-3: EPA Region 2 acknowledges the suggestion that the trophic transfer factor of 0.1 for PAHs appears to be too conservative. Available data suggest that PAHs do not biomagnify in aquatic food webs (Suedel *et al.*, 1994). In fact, some studies have suggested that many organisms contain lower concentrations of PAHs than their food sources (Neff, 1979; Broman *et al.*, 1990) indicating increased ability to metabolize and excrete these compounds with increasing trophic level. A value of 0.1 was selected as representative of the PAH compounds based on the results of two evaluations. Burns and Teal (1979) estimated a trophic transfer of 0.1 for total PAHs between mummichog (*Fundulus heteroclitus*) and American eel (*Anguilla rostrata*). Similarly, Broman *et al.* (1990) reported a trophic transfer factor of 0.1 for benzo(a)pyrene between zooplankton and mussels (*Mytilus edulis*). Based on these data, EPA Region 2 believes that this value is a reasonable estimate of the potential trophic transfer of these compounds and is proposing to use it in the proposed HARS Framework.

COMMENT 13-4: One reviewer suggested that the trophic transfer for arsenic (i.e., 3) was too high.

RESPONSE 13-4: EPA Region 2 agrees that a trophic transfer factor for arsenic of 3 is too high. Available evidence indicates that arsenic does not biomagnify in primary or secondary consumers. Limited biomagnification has been reported in tertiary consumers; however, it is important to consider the form of arsenic which is accumulated. Arsenic has an unusual behavior in marine food webs. Inorganic arsenic species are bioconcentrated by marine algae and converted to organic forms, particularly arsenosugars (Neff, 1997a). Herbivores and detritivores accumulate the organic arsenic from their algal food and store it in their tissues, primarily as arsenobetaine, which is not toxic to the animals themselves or their consumers, including man. More than 90 percent of the arsenic in soft tissues of marine crustaceans and fish is arsenobetaine. Human consumers of fishery products rapidly excrete unmetabolized arsenobetaine in the urine. Thus, organic arsenic may be biomagnified to some extent; however, the inorganic (i.e., toxic) form does not biomagnify (Dillon *et al.*, 1995).

As described in the response to comment 13-1, EPA Region 2 conducted an extensive review of the available literature to derive a more realistic estimate of trophic transfer of arsenic. Based on this information, a new trophic transfer factor (i.e., prey to fish) for arsenic of 0.25 ($[As]_{fish}/[As]_{prey}$) is proposed for determining the suitability of dredged material proposed for use at the HARS as Remediation Material under the proposed HARS Framework.

COMMENT 13-5: One reviewer suggested that transfer coefficients for many of the organic chemicals could be obtained from work performed by EPA's laboratory in Duluth, MN.

RESPONSE 13-5: EPA Region 2 acknowledges the reviewer's comment and notes that the trophic transfer coefficients for chlorinated organic contaminants used in the current and proposed HARS Frameworks were developed by Dr. Lawrence Burkhard at EPA's Mid-Continent Ecology Division (Office of Research and Development, Duluth, MN).

Dr. Burkhard derived the trophic transfer coefficients for chlorinated organic contaminants using the food web model of Gobas *et al.* (1993), run at equilibrium (EPA 1995b). The food web used in the model was

described by a simplified New York Bight food chain consisting of three representative trophic levels: benthic organisms, benthic predators, and upper level predators. Through numerous discussions/meetings (1994 to 1996) with a Mud Dump Site Criteria Workgroup (comprised of scientific representatives of the environmental and regulated communities, NY and NJ resource agencies, USEPA, USACE, and NOAA-NMFS with knowledge of the NY Bight ecosystem) which was established and convened under the NY/NJ HEP Dredged Material Management Forum to develop bioaccumulation Criteria for disposal at the former Mud Dump Site (for additional information on the trophic levels see EPA (1995a)). Average lipid contents of each trophic level were calculated from lipid contents of representative organisms in each level, as reported by NYSDEC (1996).

Higher level ecological receptors (e.g., piscivorous birds and mammals) were not identified as appropriate terminal ecological receptors for the food chain in the 1995 characterization, nor were any identified by U.S. Fish and Wildlife Service, NOAA-NMFS, state resource agencies, or the public in comments received during the HARS site designation process. Therefore, predatory fish are proposed for use as the terminal ecological receptors in the assessment of ecological risks at the HARS.

The Gobas model is sensitive to the log K_{ow} assigned to the compound being modeled. Since different K_{ow} values have been published for chlorinated organic compounds, it is important to carefully select the appropriate K_{ow} for use in the model. Karickhoff and Long (1995) reviewed log K_{ow}s and derivation methods published for various compounds and the methods used to derive those values. Based on their review, they recommended values for use by EPA. Log K_{ow}s obtained using the "slow-stirring" or "shake-flask" methods were usually recommended for use by the authors, depending on the compound. These recommended log K_{ow} values were used to run the Gobas model.

Log K_{ow}s assigned to chlorinated organic pesticides and resultant trophic transfer coefficients predicted by the Gobas model are reported in the following table.

Compound	log K_{ow}	Trophic Transfer Factor	Compound	log K_{ow}	Trophic Transfer Factor
Aldrin	6.5	3.0	DDT	6.53	3
Dieldrin	5.3	1.6	DDD	6.1	2.7
a - chlordane	6.32	2.9	DDE	6.76	3
heptachlor	6.26	2.9	endosulfan (mixture)	4.1	1.1
heptachlor epoxide	5.0	1.4			

Karickhoff and Long (1995) did not review or recommend log K_{ow} values for PCB congeners or mixtures. De Bruijn *et al.* (1989) reported log K_{ow}s for 19 PCB congeners that were measured using the slow-stirring method. They reported that increasing chlorination level is the primary determinant of log K_{ow} of a given PCB congener, but also noted that the patterns of chlorine substitution also contributed to a lesser extent to the log K_{ow}.

In deriving guidance values for evaluating risk associated with PCB mixtures, a single trophic transfer factor of 3 was used to express the potential for trophic transfer of contaminants to upper level predators. This reflects the most efficiently transferred of the PCB congeners. Assuming equilibrium, use of this trophic transfer factor is more or less appropriate for PCB congeners with 4 to 6 chlorine atoms (i.e., log K_{ow}s 6.2 -7.1), but is overly conservative for less or more highly chlorinated congeners. For example, trophic transfer rates are estimated to be approximately 2.14 for congeners with three or seven chlorines.

Hawker and Connell (1988) published log K_{ow}s for each of the 209 PCB congeners. EPA Region 2 proposes to use these values to better predict and consider the trophic transfer of the specific PCB mixtures that are accumulated by test organisms from dredged material proposed for use at the HARS. Specifically, EPA Region 2 proposes to adjust reported concentrations of individual congeners by applying a multiplier that will correct for the difference in the predicted trophic transfer for the individual congener (based on its log K_{ow}) and that used for deriving the HARS-Specific value (i.e., the trophic transfer factor of three (3)). An adjusted total PCB residue would then be calculated and considered against guidance values derived using a trophic transfer factor of three. This approach would minimize the uncertainty and better estimate trophic transfer of PCB mixtures.

EPA Region 2 proposes to use similar adjustments in calculating and evaluating risks for mixtures of chlorinated organic compounds with differing log K_{ow}s and shared guidance values that would be derived with assumptions of uniform, worst-case trophic transfer (i.e., human and ecological assessments of total DDT and total endosulfans, and ecological assessments of total chlordane and aldrin + dieldrin).

Charge Group: Calculations

Charge No. 14: Is the assumption of a fish consumption rate of 6.5 g/day appropriate for use in evaluating the potential for human health impacts associated with metals in Remediation Material? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Page A-5)* Would it be appropriate that the evaluation focus on a higher consumption population?

Summary of Peer Review Comments

Individual Comments

Bartell Charge No. 14 Comment (1 para):
The fish consumption rate of 6.5 g/d converts approximately to one meal of 6 oz. of fish every two weeks during one year. The accuracy of applying this number generally across people of different age, size, and geographical location is certainly open to argument. However, it would appear highly probable that certain subpopulations of those who regularly utilize the regional marine resources would characteristically consumer more fish than the 6.5 g/d value. It would certainly be appropriate to include an additional analysis that focused on fishermen and other subsets of the regional populace that eat more than this default rate. However, this introduces the question concerning whom the sediment classification criteria are meant to protect; identification and characterization of the "stakeholders" in relation to this assessment might assist in refining exposure parameters throughout the entire assessment.

Clifford Charge No. 14 Comment (1 para):
Given my limited expertise in human health risk assessment and setting aside my concerns regarding use of humans as terminal ecological receptors, I believe the value is appropriate.

Gentile Charge No. 14 Comment (1 para):
Although EPA uses 6.5g/day as their default consumption rate a case can be make for exceptions where subsistence fishing by specific sub-populations are an issue. I can't make a case for increasing the consumption rate for fish coming from that site unless there is specific subsistence fishing there. If the catch is going to the broader market then the 6.5 g/day is fine. I certainly can't make a case for increasing it.

Wentsel Charge No. 14 Comment (1 para):
Consumption of 6.5 g/day has been used as a number. Recent publications have looked at the distribution of fish consumption by people. A focus on higher consumption populations may be appropriate for specific sites, but the various conservative assumptions in this assessment should be protective of those groups.

Solomon/Sibley Charge No. 14 Comment (1 para):
Given the low likelihood that fish or shellfish directly from the site will be eaten by any particular individual on a consistent basis (no local subsistence fishery), this assumption of an average consumption of fish of 6.5 g/day is judged to be appropriate and probably conservative.

Newman Charge No. 14 Comment (1 para):
The consumption rate is consistent and reasonable if the focus is the general population. However, as suggested in EPA 540/1-89/002 December 1989, "Residents near major commercial or recreational fisheries or shell fisheries are likely to ingest larger quantities of locally caught fish and shellfish than inland residents." I would suggest that consumption surveys be reviewed to identify any possible subpopulation consuming higher amounts than 6.5 g/day.

Dillon Charge No. 14 Comment (1 para):
6.5 g/day is a standard ingestion rate EPA uses in its risk assessment guidance (RAGS) for Superfund. When available, documented site-specific ingestion rates based on target populations are preferred. (See peer reviewer's comment at end of questions regarding human health.)

Agency Response to Reviewers' Comments:

COMMENT 14-1: The general consensus of the peer reviewers was that the 6.5 g/day consumption rate of fish was developed, and is most appropriate, for protection of the general population. The ultimate utility of using this number for evaluating dredged material proposed for placement as Remediation Material at the HARS is dependent on how similar it is to the actual consumption rates of seafood from the vicinity of the site. Peer reviewers recommended that additional analyses be performed to identify whether there are subpopulations (e.g., recreational and/or subsistence fishermen) that may have higher consumption rates than the standard fish ingestion rate of 6.5 g/day.

RESPONSE 14-1: EPA Region 2 notes that although the charge refers specifically to the use of the 6.5 g/day ingestion rate as it pertains to the potential for human health effects associated with metals in dredged material proposed for use at the HARS as Remediation Material, the ingestion rate is also used in the current Framework to assess the potential for impacts associated with organic contaminants.

EPA Region 2 agrees with the observation of two reviewers that a specific subsistence fishery does not exist at the HARS. EPA Region 2 proposes to use recreational fishermen as an appropriate subpopulation for estimating seafood consumption rates that reflect maximal potential human exposure to contaminants at the HARS.

EPA Region 2 contacted the New Jersey Department of Health, New Jersey Department of Environmental Protection and the New Jersey Sea Grant College Program to obtain copies of relevant studies that assess fish consumption in New Jersey in order to identify an appropriate ingestion rate for use in the evaluation process. Two pertinent studies were received and reviewed. These studies were:

- Belton *et al.* 1985. A Study of Toxic Hazards to Urban Recreational Fishermen and Crabbers. New Jersey Department of Environmental Protection. Office of Science and Research.

- New Jersey Marine Sciences Consortium. 1994. Fish Consumption Patterns by New Jersey Consumers and Anglers. Prepared for the NJDEPE- Division of Science and Research. Contract No. P 30695 00962.

The Belton *et al.* (1985) study focused on urban anglers active on the Hudson River, Upper Bay, and Newark Bay shorelines and did not attempt to quantify actual consumption rates of the anglers. The study, therefore, was judged to be inappropriate and of little utility in estimating human consumption rates of fish from the HARS area.

Despite certain limitations that were identified in the report, the second study (NJMSC, 1994) provided useful data for estimating an appropriate ingestion rate for the target group of consumers (i.e., anglers). One of the primary limitations of the study was that calculated fish ingestion rates and patterns were based on consumption in the week preceding the survey (October-November). This could potentially result in the introduction of a biased estimate that may not truly represent annual consumption behavior. The NJMSC study, however, confirmed that the recreational angler community consumes greater amounts of fish than the general population, and therefore, appears to be a more appropriate target population for use in evaluating potential for human health risks due to consumption of fish from the area of the HARS.

New Jersey anglers were classified into two groups based on whether or not those anglers had consumed fish in the week preceding the survey. Overall seafood consumption by anglers that reported having consumed fish in the preceding week were somewhat higher than those that did not report having consumed fish in the preceding week. Therefore, average consumption of those anglers consuming fish in the preceding week are proposed for use to obtain a more conservative estimate of consumption by New Jersey anglers. This group of anglers reported consuming an average of 2.42 meals and 15.23 ounces of fish per week. This level of consumption (i.e. 15.23 oz/wk) is equivalent to an average daily seafood consumption rate of approximately 61 grams, confirming that recreational anglers indeed consume significantly more fish than the national average (i.e., 6.5 g/day).

As part of the NJMSC survey, respondents identified the quantitities and types/species of fish that they consumed (see page 4-69, Table 4-14 of NJMSC, 1994). Of the 15.23 ounces consumed weekly by recreational anglers, 7.57 ounces were reported to be saltwater finfish. Approximately 37 percent (2.8 ounces) of the saltwater finfish consumed by recreational anglers were reported to be fresh (i.e. not canned or processed) fish of varieties that are listed in the HARS SEIS (EPA 1997f) as species that may occur in the vicinity of the HARS. In decreasing contribution to angler diet, the reported fish species include: flounder/fluke, cod, sea bass, haddock, whiting, blackfish, porgy, bluefish, striped bass[3], and weakfish.

The weekly consumption rate of 2.8 ounces, or 79.38 grams, equates to a daily consumption rate of 11.34 grams of finfish that could potentially occur at the HARS. This consumption rate, therefore, does not include consumption of processed fish or of species that are not expected to occur at the HARS, such as red snapper, orange roughy, and off-shore species (e.g., tuna, swordfish). NJMSC (1994) respondents

[3]The contribution of striped bass to anglers' diets was not specifically reported in NJMSC (1994). It's contribution to anglers' diets was assumed to be equal to that of bluefish (i.e., 4 percent).

(anglers) indicated that 60% of the fish they consume is prepared in the home. EPA Region 2 assumed that recreationally-caught fish is consumed by recreational anglers in their home. If the percentage of fish that occurs at the HARS that is consumed by recreational anglers in their home is similar to the overall percentage of home-prepared fish (i.e., 60%) in their diet, then the daily consumption rate of recreationally-caught (i.e., home-prepared) fish potentially occurring at the HARS by New Jersey anglers would be estimated to be 6.8 grams (i.e., 11.34 g/day x 60 percent).

Anglers responding to the NJMSC (1994) survey indicated that they annually consume an average of 5.76 lbs. of recreationally-caught fish. Information on angling habits in New Jersey marine waters is not available. Therefore, EPA Region 2 conservatively assumed that there may be a subpopulation of recreational anglers that preferentially fishes at the HARS and obtains all of the recreationally-caught fish in their diet from fishing at the HARS. This equates to an average daily consumption of 7.2 grams of recreationally-caught fish. Given that such a population exists, their consumption of finfish that are potentially exposed to the HARS could be estimated at 7.2 g/day.

The two estimates of consumption of finfish that are potentially exposed to HARS and caught recreationally by New Jersey anglers (outlined above) agree well. EPA Region 2 proposes to use the higher of the two estimates (i.e. 7.2 grams/day) as an appropriate estimate of fish consumption for assessing the risks to a reasonably maximally exposed (RME) human subpopulation associated with contaminants in sediments proposed for use as Remediation Material at the HARS.

This estimate of HARS-associated fish consumption by recreational anglers is inherently conservative. The consumption rate calculated for recreational anglers is based on anglers that reported having consumed fish during the week preceding the survey. The calculated rate of 7.2 g/day, therefore reflects higher consumers within the angler community. The average consumption rate used for assessment of risks is based on anglers that reported consuming an average of 2.42 seafood meals/week.

Approximately 75 percent of anglers in the survey reported consuming less than three meals of seafood per week. More significantly, all of the recreationally-caught fish consumed by anglers is assumed to be composed of species occurring at, and obtained from, the HARS. Certain species that contributed to the consumption rate and that were assumed to be recreationally caught at the HARS are not generally targeted at the HARS. Examples include: structure-associated species, such as porgy, blackfish, and sea bass; deeper water bottom species, such as cod and haddock; and species generally targeted within bays and estuaries, such as winter flounder. However, EPA Region 2 also recognizes that the contribution of lobster to the human diet (estimated at 3.2 g/day in NJMSC (1994)) is not reflected in the estimated consumption rate. There is, however, no directed recreational lobster fishery at the HARS and therefore the assumption that all (or 60%) of consumed lobster is obtained at the HARS cannot be supported.

Eighty-five to 90 percent of the survey participants with consumption rates in this range (i.e., two to three fish meals/week) also reported that this rate was typical or slightly more than their usual consumption. This suggests that the potential bias associated with using a single week's consumption (identified above) to extrapolate annual fish consumption may not be significant, using the NJMSC (1994) study. EPA Region 2 believes that using the 7.2 g/day consumption rate in assessing human health risks would represent an appropriately conservative consumption rate to protect the human consumer against adverse health effects associated with contaminants in dredged material proposed for use as Remediation Material at the HARS.

Charge Group: General

Charge No. 15: Is it plausible to replace any other risk assessment assumptions with assumptions specific to the HARS site? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Attachment C and Reference Nos. 88)* Is it appropriate to consider the HARS intended use to be factored into an evaluation of effects at the community or population level?

Summary of Peer Review Comments

Individual Comments

Bartell Charge No. 15 Comment (2 para):
Depending on the resources and time available to conduct an assessment, it would be possible to develop regional or more site-specific values for nearly all the factors that enter into the assessment. Regionally-specific values for all the exposure parameters in the equations listed in Reference 88 and Attachment C could in theory be obtained. (One would hope that the fundamental toxicity of the compounds (e.g., Rfd's) would not vary by region.) Reality obviously imposes constraints on the number of parameters that can be estimated on a regional basis. Therefore, the entire calculus underlying the exposure assessment should become the focus of a comprehensive and detailed sensitivity/uncertainty analysis. The results of such analysis would include the identification and rank-ordering of the input values in terms of their importance in defining sediment criteria for each of the contaminants of concern. Using these results, available resources could be judiciously allocated to obtain regional estimates for the key parameters in the exposure assessment.

Unfortunately, while such analyses of exposure have proven valuable in understanding and refining other risk assessments, sensitivity/uncertainty analyses that have also included the toxic benchmark data have emphasized that the main limitation in risk assessment lies in the paucity of relevant and reliable toxicity data. There is no simple solution to this problem other than acquiring the necessary data. At the same time, these more comprehensive sensitivity/uncertainty analyses can rank the contaminants in order of their probable human health and/or ecological concern. The more critical toxicity data can be identified through this process.

Clifford Charge No. 15 Comment (1 para):
The only site-specific information that I can think of that could be used to replace some of the default assessment assumptions would be regarding the character of the food-web at the HARS site. For example, while the species selected for evaluation follow guidance and are generally sensitive and appropriate, if it were known that more or less sensitive species were present at the site or that top-predators more appropriate for use in risk assessment than humans were potentially at risk, I would recommend their inclusion in this evaluation. I do not believe that the HARS intended use is appropriate for consideration at the risk assessment stage as extant risk is independent of intended future site use. Consideration of these kinds of factors should take place at the risk management stage, not the risk assessment stage.

Gentile Charge No. 15 Comment (1 para):
I do think that 'intended use' is appropriate to use for a site when one sets goals for the ecological condition of the site (e.g., community and population endpoints). I'm not sure how to implement the concept is but I do think 'intended use' is relevant.

Wentsel Charge No. 15 Comment (1 para):
Comparative risk assessment methods could be used to compare reduced impacts of the HARS site on biota by covering with dredged sediment.

Solomon/Sibley Charge No. 15 Comment (1 para):
This reviewer believes that the assumptions used in this assessment are reasonable and consistent with other assessments of similar situations. Most of the criteria used in the assessment are aimed at individuals or individual populations. They are therefore judged to be sufficiently conservative to be protective of population and community responses.

Newman Charge No. 15 Comment (1 para):
I believe that effects to populations and communities are appropriate. Please see those points already discussed relative to community and population level effects.

Dillon Charge No. 15 Comment (1 para):
Yes to both questions.

Agency Response to Reviewers' Comments:

COMMENT 15-1: Several of the peer reviewers indicated that incorporating additional site-specific information would be desirable to support the evaluation. The peer reviewers' responses to other related charges (e.g., Charges 2, 5, and 7) also indicated that, where available, replacement of risk assessment assumptions with assumptions specific to the HARS are desirable.

RESPONSE 15-1: The peer reviewers specifically identified the exposure assumptions used in the current HARS Framework as assumptions that could be improved, if site-specific information were available. EPA Region 2 has reviewed the exposure factors and available data; as a result of this review, EPA Region 2 proposes to use site-specific information to better estimate two exposure assumptions. These two exposure factors are: 1) the fish consumption rate of a human target sub-population of concern (i.e., population identified as recreational fishermen who exclusively consume fish taken from the vicinity of the HARS); and 2) the time that fish species may be expected to be actually foraging at the site (site use factor). EPA Region 2 recognizes that there may be additional exposure assumptions that would be best estimated using site-specific information (e.g., percent of recreational landings actually taken from HARS). However, EPA Region 2 believes that sufficient data to revise the currently assumed values for these factors are not currently available.

The data and method used to identify, and estimate fish consumption of, a reasonably maximally-exposed (RME) human subpopulation (i.e. recreational fishermen) for assessing human health risks associated with contaminants at the HARS is provided in response to comment 14-1. EPA Region 2 proposes to use data on the seasonal presence of fish species in New York Bight waters to derive a site use exposure factor to better reflect the potential foraging at the HARS of fish species that are consumed by recreational fishermen (Consumed species are listed in the response to comment 14-1). Derivation of the proposed site use factor is described below.

EPA Region 2 reviewed 1993 commercial catch data reported by NOAA/NMFS, along with species summaries from Bigelow and Schroeder (1953), and Smith (1982) to establish the presence or absence of species in the New York Bight on a quarterly (seasonal) basis throughout the year. The NOAA/NMFS data are summarized in Appendix A of the HARS SEIS, (EPA 1997f). With the exception of cod, EPA Region 2 found that 95 percent (by weight) or greater of all fish caught were restricted to three or less of the 4 quarters for each year. Catches of cod in the New York Bight were distributed throughout the year. No seasonal data were available for haddock; therefore, its seasonal presence was assumed to be similar to that of cod.

To derive the site use exposure factor, the seasonal presence of species (i.e., minimum percent of year (as quartiles required to account for 95 percent of species landings) was weighted by the relative contribution of that species to the total estimated consumption of fish by recreational fishermen. A single weighted average was obtained that reflects the seasonal presence (and potential exposure) of consumed fish at the HARS, considered collectively. The calculation of this seasonal fish foraging exposure is summarized in Tables 15-1, 15-2, and 15-3, below. The seasonal residence factor (i.e., Site Use Factor) for fish in New York Bight waters (i.e., in the vicinity of the HARS) is estimated to be 77.7 percent of the year. Where the duration of exposure of fish is important to the calculation of potential for risk (e.g., human health-based values), EPA Region 2 proposes to incorporate a site use exposure factor of 0.777 (or it's reciprocal (i.e., 1.29), as appropriate) to account for this seasonality.

EPA Region 2 considers the site use exposure factor to be conservative, however, as it assumes that: 1) when fish migrate to areas away from the HARS vicinity, they return to the HARS; and 2) while fish are in the general vicinity of the HARS, they feed exclusively at the HARS itself. Although these two inherent assumptions are clearly conservative, empirical, site-specific data to support a revised estimate for those assumptions were not identified by EPA Region 2.

EPA Region 2 considered using the geographic scale of the HARS to adjust the site use exposure factor, as the HARS occupies less than 3 percent of total available sea bottom between 60 to 80 feet in the New York Bight. EPA Region 2 also recognizes that the probability of fish returning to this specific area year after year is low. However, EPA Region 2 also recognizes that it is unlikely that fish are entirely indiscriminate in their choice of habitat and therefore considers the use of such a gross metric (percent of bottom in depth range) to be inappropriate. Adjusting site use based on a geographic scale approach was not considered to be appropriate for the proposed assessment because the additional factors that need to be considered to predict habitat selection or site fidelity are not well understood and probably vary widely across species. Furthermore, the area of the HARS is a popular recreational fishing area specifically because it appears to appears to 'hold' fish.

To adjust site use based on foraging range, information on the size of feeding or home ranges of fish is necessary. It is likely that the foraging or home ranges of fish are species- and size- specific, and density-

and habitat-dependent functions. This area of fish behavior is not well studied and no relevant empirical studies were found by EPA Region 2 to support adjusting the site use factor in the HARS assessment based on foraging ranges.

Table 15-1. 1993 Commercial Catch per Quarter (Metric Tons)

Fish Name	*Q1 (Jan-Mar)*	*Q2 (Apr-Jun)*	*Q3 (Jul-Sep)*	*Q4 (Oct-Dec)*	*Total Annual*
Winter Flounder	8.9	61.1	14.9	42.6	127.5
Summer Flounder	3.8	94.3	240.4	43.4	381.9
Yellowtail Flounder	9.5	8.5	0.1	0.1	18.2
Flounders (Totals)	22.2	163.9	255.4	86.1	527.6
Cod	8.7	2.7	2.9	4.5	18.8
Whiting	24.3	50.4	7.1	275.2	357.0
Bluefish	0	202	101.8	56.1	359.9
Porgy	0	4.6	1.8	36	42.4
Blackfish	0.5	13.3	23.3	15.5	52.6
Weakfish	0	7.2	12.5	11.6	31.3

Striped Bass - Present primarily in Spring Fall (Smith 1982)
Sea Bass - Present primarily in Spring Fall (Bigelow and Schroeder, 1953)
Haddock - Assumed to be similar to Cod
Commercial catch data reported by NOAA/NMFS 1993

Table 15-2. 1993 Commercial Catch per Quarter (Percent of Total Annual)

Fish Name	*Q1 (Jan-Mar)*	*Q2 (Apr-Jun)*	*Q3 (Jul-Sep)*	*Q4 (Oct-Dec)*	*Minimum quarters to explain 95% of presence*	*Percent of catch*
Flounders (Totals)	4.2	31.1	48.4	16.3	3	95.8
Cod	46.3	14.4	15.4	23.9	4	100.0
Whiting	6.8	14.1	2.0	77.1	3	98.0
Bluefish	0.0	56.1	28.3	15.6	3	100.0
Porgy	0.0	10.8	4.2	84.9	2	95.8
Blackfish	1.0	25.3	44.3	29.5	3	99.0
Weakfish	0.0	23.0	39.9	37.1	3	100.0

Commercial catch data reported by NOAA/NMFS 1993

Table 15-3. Seasonal Residence Weighted by Contribution to Fish Consumption

Fish Name	*Contribution to HARS diet (%)*	*Seasonal residence at HARS (%)*	*Seasonal Residence Weighted by Contribution to HARS diet (%)*
Flounders (all spp.)	48.6	75	36.49
Cod	10.8	100	10.81
Whiting	2.7	75	2.03
Bluefish	10.8	75	8.11
Striped Bass	10.8	75	8.11
Haddock	2.7	100	2.70
Porgy	2.7	50	1.35

Blackfish	2.7	75	2.03
Weakfish	2.7	75	2.03
Sea Bass	5.4	75	4.05
	99.9		
Site Use Factor (% Year Present at the HARS)			*77.70*

COMMENT 15-2: One reviewer felt that the process should be based on a more detailed evaluation of the site-specific food web, to ensure that all sensitive species have been identified, and that the most appropriate upper trophic level species are evaluated.

RESPONSE 15-2: See response to comment 1-2.

COMMENT 15-3: One reviewer suggested that comparative risk assessments could be used to evaluate the impact coverage of the HARS site on biota.

RESPONSE 15-3: See response to comment 1-2.

COMMENT 15-4: One reviewer suggested that considering the intended use of the HARS in risk assessment would be inappropriate, stating that such consideration should take place at the risk management stage. Another reviewer felt that considerations of "use" are not appropriate when evaluating ecological risk–implying that nonhuman use of the site is not an elective attribute as it is with human use of the site (e.g., fishing).

RESPONSE 15-4: The evaluation of suitability of dredged material for use as Remediation Material at the HARS is intended to ensure that the potential for adverse impacts (both ecological and human health) at the site are reduced as a result of remediation activities. The goals of the HARS remediation effort are intended to attain this goal.

Charge Group: General

Charge No. 16: Is use of the Squibb *et al.* (1991) report appropriate for identifying the contaminants of concern? Are there contaminants which should be added to or deleted from the list of contaminants for which we presently test? *Please see Reference No. 51)*

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 16 Comment (1 para):
Squibb *et al.* (1991) did an excellent job of summarizing and identifying chemicals of concern for the NY/NJ harbor estuary based on 1990 and earlier reports. There was a substantial literature available to them and I suspect that an evaluation of the more recent literature would not substantially change the list of contaminants of concern. Nonetheless, I recommend that such a literature survey be conducted to ensure that recent studies with more modern analytical methods have not identified additional chemicals that should be added to the list.

There are several chemicals that appear on the Squibb *et al.* (1991) Table 19 list of toxics of concern for the estuary that are not included in Table 1 of the Memo for the Record. Two chemicals that seem to warrant further consideration are lindane and hexachlorobenzene. Both of these chemicals are included on the list of chemicals of concern for the entire NY Bight. Both occur in the tissues of several fish and invertebrates from the Harbor/Estuary at concentrations that exceed criteria for Category I.B. Pollutants (Squibb *et al.* (1991) Table 13).

The Squibb *et al.* (1991) Table 19 also includes a number of methylated naphthalenes, although they are listed as being of concern for sediments only. This class of alkylated PAHs might be added to the Table 1 list to address concerns about the effects of other PAHs (see response to question 16, above).

Gentile Charge No. 16 Comment (1 para):
As stated in the report, this is a first step in the characterization process and one that seems to be adequate for the intended purpose. Second the report recognizes deficiencies in QA/QC and has omitted samples from their estimate as well as spatial and temporal sampling heterogeneity. The use of the Lake Ontario Toxics Management Plan appears sound as do the seven criteria used to make a determination of concern. A decision to remove chemicals should emerge from this review. However, the decision to add chemicals would likely come from the analysis of recent data or other toxic regulatory policies that showed other chemicals are a cause for concern. I noted that the basis for the selection of {NOTE: MISSING WORDS HERE IN GENTILE COMMENT}

Berry/Lake/Pruell Charge No. 16 Comment (1 para):
The Squibb *et al.* (1991) reports seems appropriate for identifying contaminants of concern. We would not delete or add any compounds from the list currently tested, but would recommend the use of approaches which allow chemicals to be summed within a chemical class (e.g. narcosis).

Wentsel Charge No. 16 Comment (1 para):
Appears appropriate. However, recent attention to endocrine disruptors may require a reassessment of the document.

Solomon/Sibley Charge No. 16 Comment (1 para):
The Toxics Characterization Report (Squibb *et al.* 1991) is judged to be appropriate for the identification of potential contaminants. Analyses of some compounds such as the minor metabolites of DDT and some of the other pesticides is judged to be less necessary as they are less toxic, however, they are usually analyzed along with other analytes and the information would be available anyway.

Newman Charge No. 16 Comment (1 para):
The report seems appropriate. Perhaps organotins such as TBT should be added.

Lee Charge No. 16 Comment (1 para):
I did not review Squibb *et al* (1991) in detail, however, it appears to have been a reasonably complete analysis. I suggest that rather than just listing TCDD (as in their Table 19), it is more appropriate to evaluate dioxins and furans. I did not see TBT mentioned. TBT is a possible contaminant to consider, especially if the area is used by the Navy. As a check on Squibb, you might consider going over NPDES reports of discharges into the area to determine if there are compounds with a high bioaccumulation potential that were not found.

Dillon Charge No. 16 Comment (1 para):
Squibb *et al*. is one source which is appropriate for identifying contaminants of concern. The analytes listed in Tables 4-4A and 4-4B of the RIM are also appropriate. Consider EPA method SW 846-8310 to achieve lower detection limits for PAHs.

Agency Response to Reviewers' Comments:

COMMENT 16-1: In general, the reviewers felt that the use of the Squibb et al. (1991) report was appropriate for the purpose of identifying chemicals of concern in dredged material proposed for use as Remediation Material at the HARS. Although use of this study was determined to be appropriate for identifying chemicals of concern, several reviewers recommended reviewing more current studies and regulatory data (such as National Pollutant Discharge Elimination System (NPDES)) to determine if any additional compounds should be added to the list.

In addition, reviewers recommended that several specific contaminants be considered for inclusion to the list. These contaminants were: lindane (gamma BHC), hexachlorobenzene, tributyl tin (TBT), alkylated PAHs (including methyl naphthalene), and endocrine disruptors.

RESPONSE 16-1: EPA Region 2 reviewed relevant data sets that have been generated in the NY/NJ Harbor estuary and the New York Bight since Squibb *et al.* (1991) along with other scientific information to evaluate the need to add constituents to the list of contaminants of concern for the evaluation of

dredged material that is proposed for use as Remediation Material at the HARS. These data were reviewed with specific regard to the contaminants identified by the peer reviewers (i.e. lindane, hexachlorobenzene, organotins (TBT), alkylated PAHs and endocrine disruptors). The results of that review are summarized below.

Lindane/Hexachlorobenzene

Squibb *et al.* (1991) compiled available data on chemical concentrations in sediment, water, and fish collected from the New York/New Jersey Harbor Estuary. Chemicals were categorized as "of concern" or "not of concern", based on comparison of measured concentrations in water or biota with available federal and state marine water quality or fish tissue standards for the protection of marine life and human health, and with NOAA effects-range values for sediment concentrations. Due to the varying confidence in the data supporting these classifications, the identification of certain chemicals (including lindane and hexachlorobenzene) as "of concern" by the authors was intended to indicate a need for further evaluation and monitoring before a definitive conclusion could be reached. Squibb *et al.* (1991) documented their conclusions in a December, 1991 report to the NY/NJ Harbor Estuary Program's Toxics Workgroup entitled "*NY-NJ Harbor Estuary Program Module 3.1: Toxics Characterization Report*".

In Fall/Winter 1993-4, the Toxics Management Workgroup funded a systematic sampling and analysis program to further investigate levels of contaminants identified by Squibb *et al.* (1991) in biota from various areas of the NY/NJ Harbor estuary, including the New York Bight Apex. In this study, 23 species of fish, 6 species of bivalves, 2 crustaceans and a cephalopod (squid) were sampled and analyzed for chemical body burdens. The results of this study were documented in a series of reports by the New York State Department of Environmental Conservation (NYSDEC) entitled, *"Chemicals in Fish, Shellfish, and Crustaceans from the New York-New Jersey Harbor Estuary"*. Lindane and hexachlorobenzene concentrations in tissues of resident biota of the NY/NJ Harbor estuary region were reported in one of these reports (NYSDEC, 1996). With the exception of elevated hexachlorobenzene in blue crab hepatopancreas in one sampling area (Arthur Kill/Newark Bay/Kill van Kull), concentrations of these two compounds were either reported at the detection limit (i.e., 'J' values) or as not detected in all samples.

EPA Region 2 also evaluated data from the vicinity of the HARS and the former Mud Dump Site, to determine if these chemicals might be associated with historical disposal of dredged material. As part of the monitoring of the former Mud Dump Site, concentrations of numerous chemicals were determined for sediment and polychaete tissue body burdens from the New York Bight (Battelle, 1996b). Using these data, EPA Region 2 reviewed the concentrations of lindane and hexachlorobenzene measured in tissues of polychaetes sampled from 19 locations from within the area encompassed by the HARS. EPA Region 2 limited its review to sediments within this area because concentrations for other contaminants were elevated in these samples relative to samples obtained from areas outside the HARS, and because contaminants in this areas are more likely to be associated with the historical disposal of NY/NJ harbor dredged material. Lindane was not detected in any of the samples reviewed, while hexachlorobenzene was detected in 10 samples. However, (with the exception of a single outlier of 9.28 ppb) all detected concentrations of hexachlorobenzene were below 1.0 ppb and the average concentration of these samples (0.3 ppb) was below the detection limit required in the EPA Region 2/USACE-NYD Regional Implementation Manual (EPA/CENAN, 1992) for analysis of chlorinated pesticides.

EPA Region 2 believes that the reports and studies mentioned above are the most relevant and appropriate data sets available for the purpose of identifying additional potential contaminants of concern for evaluation of dredged material in the HARS Framework. However, based on this review, EPA Region 2 proposes to not require the addition of lindane and hexachlorobenzene to the list of contaminants of concern for evaluation of dredged material proposed for use as Remediation Material at the HARS. This conclusion is further strengthened by the fact that based on the review of the regional NPDES database (1993 to present) for dischargers in the lower Hudson River estuary (Bronx, NY to Sandy Hook, NJ), there were no violations of discharge limits of lindane or hexachlorobenzene to the lower estuary.

After reviewing the available data, the Toxics Workgroup of the New York/New Jersey Harbor Estuary Program has (independent of this scientific peer review process) chosen to not identify these contaminants as 'of concern' to biota in the NY/NJ Harbor estuary. Therefore, EPA Region 2 is not proposing to add lindane or hexachlorobenzene to the list of contaminants of concern at the HARS.

Tributyl Tin

Tributyl Tin (TBT) as the sum of TBT-related compounds (monobutyl-, dibutyl-, tributyl-, and tetra-n-butyl tin) [CAS Registry Number: 56-35-9] is one of several man-made organotin compounds with various industrial uses. Tri-substituted organotins find uses as biocides in agriculture and industry. Tributyl tin may be used as an anti-fouling agent in marine paints within the limits set by the Organotin Antifouling Paints Control Act of 1988, which restricts the release rate of organotin paints used on ships in the U.S. TBT has been linked to a disturbance in sex hormone production and damaged immune systems. Organotins are now being recorded in wildlife at the top of food chains which spend their lives in deeper water well away from crowded shipping lanes and coastal maritime traffic. In fact, marine wildlife the world over is now contaminated with organotins. Organotins are known to cause not only reproductive disturbances in marine molluscs, but also damage to the central nervous system in mammals. They have also been linked with disturbances in immune systems in humans.

TBT concentrations in environmental media of the NY/NJ Harbor estuary were not evaluated by Squibb *et al*. (1991), and were not measured by NYSDEC in their 1993-94 sampling effort. EPA Region 2 also reviewed the regional NPDES database for dischargers in the lower Hudson River estuary (Bronx, NY to Sandy Hook, NJ) and determined that there are no known permitted discharges of TBTs to the lower estuary. However, TBT concentrations in polychaete tissues from 10 stations located in and around the former Mud Dump Site were measured and reported in Battelle (1996b). Although sampling of this contaminant was limited, accumulated concentrations of TBT were measured in polychaetes from all samples and were generally higher in polychaetes sampled within the HARS than in polychaetes sampled outside the HARS (i.e.,average concentrations of 34.5 ppb [63.2 ppb total organotins] and 17.2 ppb [34.1 ppb total organotins], respectively). This data suggests that bioaccumulation of TBT is occurring in NY Bight polychaetes and that NY/NJ Harbor dredged material may be a contributing factor. Therefore, EPA Region 2 believes that revision of the HARS Framework should include the addition of organotins (i.e. TBT) to the list of contaminants of concern for evaluation of dredged material proposed for use as Remediation Material at the HARS.

EPA believes that the analytical method of Rice et al. (1987) should be used for the analysis of organotins. This method is recommended in the 1991 Green Book and the Inland Testing Manual (EPA/USACE 1991, 1998).

Alkylated PAHs

Alkylated PAHs are not analyzed in most environmental monitoring and assessment programs and are not currently analyzed in evaluations of dredged materials' suitability for use as Remediation Material at the HARS. EPA agrees that, for most environmental monitoring and assessment applications, the analysis of the 16 parent PAH compounds is sufficient as it allows relative PAH contamination levels to be determined and compared between samples. However, parent compounds do not generally occur as the sole PAH contaminants in field situations, but rather co-occur with their alkylated homologues. Failure to consider alkylated PAH compounds in risk evaluations (such as narcotic and carcinogenic potential) of PAH mixtures can lead to underestimation of risk.

As described in response to comment 12-1, EPA proposes to require EPA Method 8270 to be performed with specific analytical sample cleanup procedures and analytical instrument configurations which have been optimized to allow detection and quantitation of parent PAHs and their alkylated homologues. These proposed sample cleanup and instrument configuration requirements will be an adaptation of the method (i.e. EPA Method 8270) that is currently approved for analysis of the parent PAH compounds.

In the absence of available toxicological values required for the derivation of the HARS-Specific Values (e.g., cancer slope factor, RfD) for the specific alkyl PAH homologues, the values reported for the associated parent compound would be used. Similarly, these chemicals would be assumed to be equivalent to their respective parent compounds for the purpose of deriving steady state factors and trophic transfer factors. The combined molar concentration of the parent PAHs and the alkylated PAHs would be considered in evaluating the potential for narcotic effects using the CBR approach as described in the response to comment 8-1.

Endocrine Disruptors

EPA Region 2 is aware of, and concerned about, information indicating the possibility of adverse impacts on human health and the environment associated with exposure to endocrine disruptors. At the present time, however, there is little knowledge of, or agreement on, the extent of the problem. Based on the current state of the science, EPA Region 2 does not consider endocrine disruption to be an adverse endpoint *per se*, but rather a mode or mechanism of action potentially leading to other outcomes (e.g., carcinogenic, reproductive, or developmental effects, routinely considered in reaching regulatory decisions; EPA, 1998).

On December 28, 1998 EPA published a Notice (See 63 FR Vol 248 Pages 71541-71568) providing details and an opportunity for public comment on the Endocrine Disruptor Screening Program (EDSP). EPA is proposing that the EDSP include the following considerations:

- human and ecological (fish and wildlife) effects;
- effects on estrogen, androgen, and thyroid hormone systems (EAT) related processes;
- evaluate endocrine disrupting properties of chemical substances and common mixtures. The universe of chemicals and mixtures to be prioritized for endocrine-disruptor screening and testing numbers more than 87,000 and includes commercial chemicals, active pesticide ingredients, ingredients in cosmetics, nutritional supplements, and food additives.

The Notice states that:

> "Taken collectively, the body of scientific research on human epidemiology, laboratory animals, and fish and wildlife provides a plausible scientific hypothesis that environmental contaminants can disrupt the endocrine system leading to adverse-health consequences. A critical issue is whether ambient

environmental levels are sufficiently high to exert adverse effects on the general population. Various types of scientific studies (epidemiology, mammalian toxicology, and ecological toxicology) are necessary to resolve many of the scientific questions and uncertainty surrounding the endocrine disruptor issue. Many such studies are currently underway by government agencies, industry, and academia."

The proposed rule also states that, "...the EDSP is divided into several stages, including a priority-setting stage, a stage involving screening tests (Tier 1 screening), and a stage involving confirmatory testing (Tier 2 testing)". EPA Region 2 believes that the results from the entire battery of tests required in the Tier 1 screening and Tier 2 testing stages (or their equivalents) are necessary to make the statutory determination of whether a particular substance "may have an effect in humans that is similar to an effect produced by a naturally occurring [hormone]" (21 U.S.C. 346a(p)).

For the reasons stated above, EPA Region 2 is not proposing to evaluate endocrine disruptors in the HARS Framework at this time.

Based on a review of available information, the following compounds are being proposed to be added as contaminants of concern for dredged material proposed for use at the HARS as Remediation Material:

- Organotins (TBT)
- Alkylated PAHS (See Response to Comment 12)
- Co-planar PCBs (See Response to Comments 5-1 and 9-3)

COMMENT 16-2: One reviewer suggested that EPA method SW 846-8310 be considered to achieve lower detection limits for PAHs.

RESPONSE 16-2: EPA Region 2 has investigated the alternative method and determined that it would be unlikely to result in significantly lower detection limits than those obtained with the current methods. EPA Region 2, however, believes it is appropriate to propose requiring specific analytical sample cleanup procedures and instrument configurations to be used in conjunction with the current method for analyzing PAHs. A full discussion of this procedure for the evaluation of PAHs is provided in the response to comment 12.

Charge Group: General

Charge No. 17: Should risks from synergistic effects, from exposure to multiple contaminants, be evaluated using results from tissue analyses? If so, how? If not, why not?

Summary of Peer Review Comments

Individual Comments

Bartell Charge No. 17 Comment (2 para):
It is certainly desirable to develop the capability to assess the possible synergistic effects of exposure to multiple contaminants - multiple exposure is the real-world situation. We currently lack the necessary data and toxicological understanding to consistently and reliably predict the impacts of exposure to multiple contaminants. The additive model appears to work for certain classes of compounds, as suggested by the equipotency observations in McCarty's work. At the same time, there are repeated instances of the failure of the additive model. Certainly, if one of the contaminants is more acutely toxic than others, it will likely "mask" the effect of the less malevolent compounds and additivity will not be observed. In other instances, the presence of one contaminant can increase the effectiveness of other co-contaminants. Unfortunately, we by and large lack the models to quantitatively predict from among these possible alternatives.

While research continues to address synergistic effects, it seems prudent to at least continue with general application of the linear model in developing an overall site-wide assessment of risk.

Clifford Charge No. 17 Comment (1 para):
Although this is a laudable goal, I believe that it is generally intractable given the current state-of-the-science. Under conditions where such cumulative effects are both understood and quantifiable for the contaminants in question and the species of interest, such attempts should be made. However, since this will be the exception rather than the rule, it may be best to relegate such evaluations to the uncertainty section that I have recommended should be added to this framework. For the time being, the actual toxicity tests (section V.C.1), as they integrate all of these potential cumulative effects, might be given greater weight in the process and testing could be extended to move from the current, essentially acute tests, to test durations more reflective of truly reflective of chronic exposures. In that manner such effects will be accounted for as inherent components of the actual bioassays. To my way of thinking such empirical information is of greater value to a decision-maker than modeled or interpreted evaluations.

Gentile Charge No. 17 Comment (1 para):
I don't think it is feasible at this time because to my knowledge there are no models out there that address this issue at the concentrations occurring in the environment. First, the issue of synergistic effects is often a 'red herring' because we don't have enough evidence that this is occurring on a wide scale. Of course lack of evidence is not proof that interactions are not occurring. Further, synergistic effects are generally thought of being additive when in fact they could just as easily be antagonistic but either case is difficult to demonstrate at environmental concentrations. Yes there are laboratory studies that suggest this occurs but

the number of compounds and interactions are very limited and in no way capture the scope of the potential problem in the environment. Don't get caught in this trap!

Berry/Lake/Pruell Charge No. 17 Comment (1 para):
Synergistic effects are poorly understood, except within classes of compounds (e.g. narcosis within narcotic compounds). We do not see how they could be included. Dr. Pruell thought that antagonism was probably more commonly reported than synergy, but that in the absence of additional data additivity was probably the most reasonable approach.

Wentsel Charge No. 17 Comment (1 para):
If it is know that the substances have the same mechanism of action, then the responses can be additive, for example, as toxic units. When substances are know to act independently a hazard rate approach is more appropriate.

Solomon/Sibley Charge No. 17 Comment (2 para):
Additivity seems to be the rule where stressors are present at concentrations below their individual physiologically active concentrations. Toxic units are commonly used to assess such mixtures. The most appropriate uses of the toxic unit approaches are when the stressors are known to act additively. When the stressors are known to act independently, the hazard rate approach is more suitable. When the stressors are known to act synergistically, by potentiation, or by antagonism the use of multivariate procedures is more appropriate, however the data requirements may be large and empirical experimental techniques may be more appropriate. Pharmacologically based toxicodynamic models may be applicable in some instances where sufficient data are available (Kooijman and Bedaux 1996).

Although synergism and potentiation of substance-mediated responses are perceived to be a major concern in the assessment of many interactions, the likelihood of these occurring in the case of mixtures of substances in the environment is not as great as might be expected and neither is the degree of interaction. For example, Alabaster and Lloyd showed that the majority of toxic interactions between components of effluents were less than additive and that the likelihood of observing synergistic ratios greater than 8 was small (Alabaster and Lloyd 1980) Könemann and Pieters report that, in several studies on the toxicity of mixtures of substances where the individual components were present at specific fractions of a standardized response (e.g., LC50), the mixtures were never less toxic than the most toxic component and potentiation was not observed Könemann and Pieters 1996. Under the conditions of exposures to low concentrations that are described in this assessment, the most appropriate approach is to assume additivity. Synergism is judged to be unlikely to occur.

Newman Charge No. 17 Comment (1 para):
This is a good point that has no clear answer. Synergism should be considered if there are high concentrations of specific toxicants known to have synergistic effects. However, additivity is likely the best option if such information is lacking.

Dillon Charge No. 17 Comment (1 para):

These risks should be evaluated. However, I'm less convinced a residue-based approach is optimal. See peer reviewer's comment at end of questions regarding ecological receptors.

Agency Response to Reviewers' Comments:

COMMENT 17-1: There was a general consensus among the reviewers that the current state-of-the-science would not support an evaluation of risks from synergistic effects or from exposures to multiple contaminants and therefore should not be considered.

RESPONSE 17-1: EPA Region 2 agrees that the current state of science does not support the evaluation of synergistic effects from exposures to multiple contaminants.

Synergism is defined in the following manner (Rand, 1995):

> "A phenomenon in which the toxicity of a mixture of chemicals is greater than that which would be expected from the simple summation of the toxicities of the individual chemicals present in the mixture."

and it has been noted that (Marking, 1985):

> "A frequent misconception is that the toxicity of chemical mixtures results from simple addition or summation of the activities of the components. Instead, additive toxicity covers the full range between the general terms antagonism and synergism, because at both extremes there is a summation of toxic action. The degree of antagonism or synergism must be defined and quantified before the advantages or disadvantages of chemical mixtures can be understood or assessed."

EPA has recently released draft guidance for conducting human health risk assessments for chemical mixtures (EPA, 1999), however, substantial uncertainty still exists in the methods presented; and methods for evaluating exposures to ecological receptors such as those at the HARS have not yet been developed. Application of the approach recommended by EPA would require a significant amount of additional research to quantitatively evaluate complex mixtures such as those that would be expected in dredged material, and would still involve substantial uncertainties.

Given the level of effort and remaining associated uncertainties, it does not seem prudent at this time to attempt to quantify the potential for synergistic effects. See response to comment 1-2 regarding use of risk assessment.

COMMENT 17-2: One reviewer suggested that some measure of synergy could be achieved through greater reliance on toxicity bioassays, particularly if the length of the test was extended to better reflect chronic exposures.

RESPONSE 17-2: EPA Region 2 agrees. However, these tests are currently in the development phase and cannot be effectively implemented until the laboratories have become more proficient at implementing

the methodologies. Once approved protocols, guidance, and implementation information are available, EPA Region 2 will evaluate and consider incorporating chronic toxicity bioassays into the Framework.

COMMENT 17-3: Several reviewers commented that in the absence of information on how to address synergistic effects, additive effects could be evaluated in the form of toxic units.

RESPONSE 17-3: EPA Region 2 agrees it would be prudent to evaluate additive effects. In fact, dose additivity for non-carcinogenic compounds and response additivity for carcinogenic compounds have been incorporated in various EPA guidance documents including the *Risk Assessment Guidance for Superfund (12/89)* and the *Soil Screening Guidance: Technical Background Document (5/96).* EPA Region 2 is proposing to add a step to the evaluation process that addresses the additivity of carcinogenic and non-carcinogenic compounds for evaluating effects to human health. In the absence of synergistic effects, dose additivity was supported as a model for non-carcinogens sharing a similar mode-of-action/toxic endpoint by the Science Advisory Board (SAB) in its 1993 review of the *Risk Assessment Guidance for Superfund*. More recently, the final draft *Guidance for Conducting Health Risk Assessment of Chemical Mixtures (9/99)* has proposed dose additivity for non-carcinogens sharing a similar mode-of-action/toxic endpoint as a default approach for assessing the effects of chemical mixtures. This same guidance document proposes response additivity as the default approach for assessing chemical mixtures of carcinogens (i.e., the individual cancer risk from all carcinogens in a chemical mixture are summed).

Consequently, in addition to the chemical-specific comparisons to proposed HARS-Specific Values, a cumulative cancer risk would be estimated by summing the individual cancer risks posed by each constituent using the following standard risk assessment equations:

Equation 1

$$\text{Estimated Conc. in Fish } (\mu g/kg) = \frac{[\text{Measured Tissue Level } (\mu g/kg)] \times [\text{Trophic Transfer Factor}]}{[\text{Whole Body/Fillet Factor}]}$$

Equation 2

$$\text{Toxicological Dose } (\mu g/day) = \frac{[\text{Estimated Conc. in Fish } (\mu g/kg)] \times [\text{Seafood Cons.}(7.2\ g/day)]}{[10^3\ g/kg] \times [\text{Site Use Factor } (0.777)]}$$

Equation 3

$$\text{Estimated Cancer Risk (unitless)} = \frac{[\text{Toxicological Dose } (\mu g/day)] \times [\text{Cancer Potency Factor } (kg\text{-}day/mg)]}{[\text{Body Weight } (70\ kg)^{fn3}] \times [10^3\ \mu g/mg]}$$

Under the proposed approach, these individual cancer risk levels would be summed, and the summation would be compared to the target risk level of 10^{-4}. If the total summed cancer risk is below the acceptable cancer risk level, the material may be suitable for use as Remediation Material at the HARS. If the total summed cancer risk is greater than the acceptable cancer risk level, the material may not be suitable for use as Remediation Material at the HARS.

Similarly, under the proposed approach of dose additivity, non-carcinogenic effects would be evaluated using a hazard index approach. For the purpose of this approach, hazard quotients would be derived for each individual chemical according to the following equations:

Equation 1

$$\text{Conc. in Fish } (\mu g/kg) = \frac{[\text{Measured Tissue Level } (\mu g/kg)] \times [\text{Trophic Transfer Factor}] \times [\text{Forage Factor } (0.777)]}{[\text{Whole Body/Fillet Factor}]}$$

Equation 2

$$\text{Toxicological Dose } (\mu g/day) = [\text{Conc. in Fish } (\mu g/kg)] \times [\text{Seafood Cons. } (7.2 \text{ g/day})] \times [10^{-3} kg/g]$$

Equation 3

$$\text{Hazard Quotient (unitless)} = \frac{[\text{Toxicological Dose } (\mu g/day)] / [\text{Body Weight } (70 \text{ kg})]}{[\text{Reference Dose } (mg/kg\text{-}day)] \times [10^{3} \mu g/mg]}$$

Under the proposed approach, these individual hazard quotients would be summed according to target organ (see Table 17-1) to derive a total hazard index for each organ system (i.e., circulatory system, liver, central nervous system, and kidney). If all of these hazard indices are below 1, this indicates that there is no potential for significant undesirable effects and, therefore, the material is suitable for use as Remediation Material. If any of the hazard indices are greater than 1, this indicates that there is the potential for significant effects and, therefore, the material may not be suitable for use as Remediation Material.

Table 17-1. Summary of Target Systems Evaluated in Hazard Index Approach

Target System	Chemical
Circulatory	Zinc, Fluorene, Fluoranthene
Liver	Acenaphthene, Fluorene
Central Nervous System	Mercury
Kidney	Cadmium, Endosulfan, Pyrene

In addition, EPA Region 2 is proposing to consider the total body residue of all organic contaminants in evaluating the potential for narcosis (see response to comment 8).

Charge Group: General

Charge No. 18: Is test tissue concentration exceeding reference tissue concentration by less than 10X a meaningful evaluative criterion? *(Please see page 9 of the Region2/CENAN joint evaluation memorandum)*?

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 18 Comment (1 para):
No, it is not a meaningful evaluative criterion and should not be used, even as a screen, to assess the acceptability of sediments for ocean placement. There are two principal reasons why the 10X factor should not be used. First, as indicated in the EPA/COE joint memorandum, reference values are variable. If the reference value is very low, the 10X factor may be overly protective, but if the reference value is relatively high, the 10X factor may be under protective. Second, and more importantly, the derivation of the 10X factor is entirely arbitrary with respect to the potential for biological effects. Indeed, there probably would be instances where the bioaccumulation from test sediment is < 10X that from reference sediment, but still greater than one or more of the biological standards listed in Table 1, columns 14-20. This is evident in the hypothetical project data of Table 1. The tissue concentration of lead in clams is 1.010 mg/kg for the test sediment, a factor of only 2.5X greater than the concentration for the reference sediment (0.398 mg/kg). According to the 10X screen, lead might not receive further attention. However, the test sediment lead concentration is quite close to the comparison standard for Human Health Non-Cancer risk (1.3 mg/kg, Table 1, column 15). The test sediment lead tissue concentration would exceed that standard if it was as little as 3.5X that of the reference sediment.

I consider the reference material to be more of a procedural control than a standard of comparison. The primary comparison should be between the test sediment tissue concentration and the comparison standards of Table 1, column 14-20.

Gentile Charge No. 18 Comment (2 para):
This depends on the magnitude of the variability in the data. There is no explanation of the 10X derivation in the report or are you just suggesting using that figure? What makes me nervous is the statistical analyses in Table 1. Here you state that both cadmium and mercury residues in the test sediment are statistically greater than in the reference. Comparing Columns 1 and 3 for cadmium indicate a less than two fold difference in the means for cadmium (0.043ug/Kg vs 0.076ug/Kg) is statistically significant and for mercury it is even less credible - 0.034ugKg and 0.040ug/Kg are claimed to be statistically different. Given the variability of natural samples and the variability of analytical procedures I find these numbers troubling. Likewise for Zn 11.83ug/Kg and 14.34ug/Kg are significantly different. I find it hard to believe the statistics let alone the ecological significance of such differences. So two points are raised by Table 1: how good are the assumptions that have gone into the statistical analyses and what is the potential value of statistical significance relative to biological significance. We have many cases where differences can be statistically significant but be meaningless to the biology. There is rarely a relationship between statistical significance and biological significance purely surreptitious. Statistics is looking at variability, the

less variability in a measure then the more power to detect small differences no biological corollary whatsoever. My other concern with the statistics is dealing with 'below detectable' values. What do you choose and why? Many folks say don't make comparisons when you don't have measurable values with their variability. A second approach is to use the upper limit of detection since you can at least argue with some degree of confidence that the reference sediment is not higher than that value but you have no idea how much lower. On Page 9 it is stated that 'Exceedence of reference values is common where reference values are very low or 'non-detect' as here.' There is no indication of how "non-detects" were handled.

Proposing a 10X exceedence as a more reasonable indicator of potential ecological effects can only be determined if there is evidence to support that hypothesis. Unless analyses are conducted to determine the incremental increase above background where effects occur then selection of 10X is arbitrary. A suggestion might be to attempt to quantify the from sediment contaminant concentrations to tissue residues to ecological effects and in so doing develop a basis for selecting minimally important magnitude differences that are ecologically important. By using the enormous benthic data bases where community structure, sediment chemistry, and toxicity have been measured simultaneously (Chapman's Triad Concept), the data bases where sediment chemistry and bioaccumulation are measured and bioaccumulation and ecological effects are compared one might be able to develop such a relationship to support the 10X or some other factor.

Berry/Lake/Pruell Charge No. 18 Comment (1 para):
The basis for the "10X" criterion is not stated, so it is difficult to assess its suitability. If the "10X" criterion is derived from an understanding of the sediment-to-sediment variability in bioaccumulation tests, then it might be appropriate. However, Dr. Pruell could not see where the 10X number came from, and felt that it might be appropriate in some samples near the detection limit, but that it may be too high in samples where a compound is present at concentrations well above the detection limit.

Solomon/Sibley Charge No. 18 Comment (1 para):
No, the choice of the reference can confound the results (see discussion on question 3 above).

Newman Charge No. 18 Comment (1 para):
No. If pressed to apply such a rule, I would express exceedance in the context of standard deviations above the mean of the reference. (Also please remember that some of the difficulties with applying these types of comparisons is the invalid use of "<DL" observations.) The more important point is whether the tissue concentrations are close to or above some toxicological threshold.

Lee Charge No. 18 Comment (3 para):
Not really. For example, if a person's sodium level were 9-fold higher than average, they probably would be dead. Having said that, it is appropriate to use the extent of deviation from the reference site as a qualitative factor in a risk assessment especially if no residue-effects relationships are available.

I believe a more powerful ecological argument can be made by comparing the test sediment to the background at the disposal site. Assuming that the disposal site is not impacted (e.g., benthic analysis)

and tissue residues in benthos and fish are acceptable (at least for the compound of concern), this is reasonable evidence that the test material would not cause undesirable effects if it did not exceed the background values. There are, however, a few caveats. First, is that the statistical tests have sufficient power to detect physiologically/ecologically relevant differences so it is critical to consider both Type I and Type II errors. Given that the number of replicates is fixed, one approach is fix the statistical power and then adjust the alpha as needed. This puts the risk on the "discharger" rather than the "environment" and does not "reward" poor replication or high variance. Another approach is to rephrase the null hypothesis from "dredge material = background" to "dredge material >background", so that it is in the discharger's interest to have sufficient replication and low variance.

Second, with the benthos it is important to compare species with similar feeding habitats. Comparing a filter-feeding bivalve (e.g., Mercenaria) from the dredge site to Macoma might incorrectly indicate that the test sediment has a higher bioaccumulation potential than the background material. Third, in evaluating the high trophic levels, it is important to collect fish and megafauna (e.g., lobster) from the dredge site that are either territorial or are not highly mobile. In highly mobile species, unacceptable tissue levels could be diluted by time spend outside the dredge site.

Agency Response to Reviewers' Comments:

COMMENT 18-1: There was a consensus among the reviewers that the use of a 10X factor was not appropriate. In general, the reviewers felt that there was no clear basis for the value, and that it has no relevance to the actual potential for toxic effects. Two reviewers illustrated the potential pitfalls of using 10X exceedance as an evaluative criterion using lead and sodium as examples (i.e., exceedance of a reference concentration by less than 10X would still lead to exceedance of risk values or lethality thresholds). Other complications, such as the comparison of data for different species or the method of evaluation concentrations below the detection limits were cited.

RESPONSE 18-1: In the current Framework, the 10X factor is used in the comparison of 28-day bioaccumulation test tissue results for the proposed dredged material to similar test results for the reference material. EPA Region 2 also agrees that use of the 10X factor may not be the most effective criterion for assessing effects from bioaccumulation of contaminants in dredged material proposed for use at the HARS as Remediation Material. EPA Region 2 agrees that using the 10X factor is not important to the bioaccumulation effects evaluation because it is not effects-based, is dependant on the concentrations in the reference tissue, and is sensitive to chemicals reported as below the method detection limits. As such, EPA Region 2 has proposed to modify the Framework to eliminate the use of the 10X factor.

COMMENT 18-2: Individual reviewers suggested that if the 10X (or other exceedance level) factor can be demonstrated to reflect either: a) minimal magnitude differences that are ecologically important (for example, the reviewer suggests using sediment quality triad databases to define this difference level), or b) sediment-to-sediment variability or if the reference value is near the detection limit; then the use of an exceedance factor as a criterion could be useful.

RESPONSE 18-2: EPA Region 2 does not believe that either of the suggested approaches are appropriate for using the 10X factor for evaluating dredged material proposed for use at the HARS as

Remediation Material. First, with regard to attempting to quantify the magnitude of ecologically significant differences from existing sediment/tissue contaminant data, there is currently not a sufficient effects-based database of tissue concentrations with which to identify a meaningful exceedance factor. EPA Region 2 acknowledges the suggestions made by the reviewers (i.e., the triad concept analyses, sediment-to-sediment variability, or near-detection limit reference values) for developing a basis for establishing an exceedance factor. However, EPA Region 2 is proposing not to pursue these options at this time preferring to use effects-based HARS-Specific Values. EPA Region 2 believes that the suggested relative measures are secondary to an effects-based evaluation process.

COMMENT 18-3: One reviewer indicated that if this method of interpreting the significance of exceeding reference is applied, exceedance might be more appropriately expressed using standard deviations above the mean for the reference. A preference for the use of exceedance over background (rather than reference) tissue concentrations was also stated by one reviewer.

RESPONSE 18-3: EPA Region 2 acknowledges this suggestion to use the standard deviations above reference. While this is a statistically appropriate method to compare proposed dredged material bioaccumulation test results to reference tissue contaminants, EPA Region 2 is not proposing to pursue an evaluation of magnitude above reference approach at this time, preferring a bioaccumulation evaluation process using HARS-Specific Values which are effects-based (see response to comment 18-2).

Charge Group: General

Charge No. 19: Are the studies from which background tissue concentrations were calculated weighted appropriately? If not, what method is recommended? Is the use of the mean the most appropriate measurement of central tendency? If not, what measure should be used? *(Please see Reference No. 98)* Are the assumption, presented on page 14 pertaining to comparisons of bioaccumulation in test tissue to tissue concentrations in organisms from the vicinity of the remediation site, valid for evaluating undesirable effects?

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 19 Comment (4 para):
The use of data from a single site (McFarland *et al.* reference) to define background tissue concentrations for clams is inadequate.
Also, the McFarland data for four mollusc taxa (Nucula, Yoldia, Mercenaria, "mollusca") is quite variable and strongly influenced by high tissue concentrations in Nucula. For example, phenanthrene concentrations in the four taxa were 8.18, 15.77, 16.20, and 90.51 ppb (arithmetic mean = 32.67 ppb). It seems problematic to compare such data with Macoma used in lab bioaccumulation tests. Clearly, there are order of magnitude differences among mollusc species in bioaccumulation potential that may relate to feeding behavior, substrate relation and other biological factors. If it was reasonable to make such a comparison, the geometric mean would seem to be a better measure of central tendency than the arithmetic mean used to derive the Table 1, column 16 value of 32.7 ppb.

The even weighting of all stations from all studies seems appropriate for the polychaete data. Use of geometric rather than arithmetic means would have minimized effects of extreme values among stations. Data are not presented in reference 98 that allow evaluation of interspecific differences in bioaccumulation among polychaetes for all four studies. The McFarland data sometimes show extreme values among the four polychaete taxa. For example, the Ni concentrations were 0.96, 1.44, 1.50, and 18.07 ppm (arithmetic mean = 5.49 ppm).

The comparison of test results to background tissue concentrations in organisms in the general area of the HARS would make sense if the test species inhabited the area near the HARS or if there was little difference among resident taxa in tissue levels. Unfortunately, the test species are not resident and there are sometimes order of magnitude or greater differences among taxa. Thus it is not true, as claimed on page 14 of the Memo for the Record, that "When bioaccumulation in organisms exposed to project sediments is not greater than tissue concentrations in organisms from the vicinity of the remediation site (the background levels), this means that placement of the material would not result in bioaccumulation above existing ambient levels in the general area and thus does not have a potential to cause undesirable effects." This statement might be valid for intraspecific comparisons, but it is not valid for interspecific comparisons.

The concept of comparison to background conditions near the HARS is nonetheless appealing. Valid comparisons could be made by collecting a data set for tissue concentrations in test species experimentally exposed to near-HARS sediments following the standard 28-d experimental method.

Gentile Charge No. 19 Comment (1 para):
The decision to preserve the variability of the data sets by using even weighting for all concentrations was appropriate as was the use of the mean as a measure of central tendency. I will assume that standard statistical procedures were used. I think the assumptions for comparing test sediment tissue residues with tissue residues from similar species in the vicinity of the site compliment the comparisons of test sediment tissue residues to residues derived from reference sediments and offer a middle ground for the manager. In other words, the material is better than what is on the site but not as clean as a reference.

Berry/Lake/Pruell Charge No. 19 Comment (3 para):
An even weighting seems appropriate.

Without knowing the distribution of the data is difficult to know which measure of central tendency is best, as was true with the matrix values. An arithmetic mean might be particularly susceptible to being skewed by "hot spot" values. Depending on the statistical distribution of the data, a geometric mean, or the median, might be more appropriate.

The assumptions on page 14 seem reasonable.

Solomon/Sibley Charge No. 19 Comment (1 para):
Where sufficient data are available, a distribution, rather than a mean should be used. This would allow probabilistic risk assessment techniques to be used. Where the underlying distribution of the data is known, a statistical measure of central tendency can be used (e.g., geometric mean of log-normally distributed data). However, the use of the central tendency in the absence of knowledge of the range or variance is counterintuitive - we should be more interested in the upper centiles of exposure and the lower centiles of sensitivity. Where the data sets are small and the underlying distribution is not know, the arithmetic mean is appropriately conservative. Where contributions to the whole are being calculated, only the arithmetic mean should be used. A recent paper by Parkhurst discusses this in more detail (Parkhurst 1998).

Newman Charge No. 19 Comment (1 para):
The weighting was adequate. Again, the treatment of "<DL" observations is invalid, i.e. substitution with 1/2DL (see Newman (1995) for details). Relative to the use of the mean, distributions of contaminant concentrations generally conform to a log normal distribution more often than they conform to a normal distribution. Therefore, the arithmetic mean is probably not the best estimate of central tendency. I would test for differences using log transformed concentrations, not arithmetic concentrations. Relative to comparisons of background tissue concentrations to those estimated in the 28 day bioaccumulation test, I feel that such a comparison has limited value. I would prefer to compare the concentrations in biota at the material source to concentrations in biota near the HARS.

Lee Charge No. 19 Comment (2 para):
As discussed under Question 18, the comparison to background tissue residues can be a defensible method of evaluating potential risk. Because of the various uncertainties, comparisons to background should not replace decisions based on individual residue criteria, if available.

If the data approach a normal distribution, the mean is best measure of central tendency. If the data approach a lognormal distribution, a geometric mean should be considered. An advantage of the mean is that the public understands it.

Agency Response to Reviewers' Comments:

Responses

COMMENT 19-1: One reviewer felt that it was problematic to compare data for *Macoma* exposed in the lab to the background data identified for mollusc taxa. The reviewer noted that four taxa yielded variable tissue concentrations strongly influenced by one particular high tissue concentration.

RESPONSE 19-1: Background, reference, and baseline data (sediment and body burden) are all established and available for the HARS in the New York Bight Apex. Baseline refers to data within the designated boundaries of the HARS. Reference refers to data collected from the HARS Reference Site located approximately one nautical mile south of the HARS. Reference is defined in the Green Book (EPA/USACE, 1991) as a sediment, substantially free of contaminants, that is as similar to the grain size of the dredged material and the sediment disposal site as practical, and reflects conditions that would exist in the vicinity of the disposal site had no dredged material disposal ever occurred, but had all other influences on sediment condition taken place. Background represents ambient conditions over a large area outside the HARS, and contains contamination from assorted regional influences (various former ocean disposal sites, New York/New Jersey Harbor plume, atmospheric deposition, etc.). Background and Reference areas in the New York Bight Apex are not representative of pristine natural conditions. Both may have been impacted to varying extent by non-HARS activities.

As part of the eight factors for LPC compliance listed (see response to comment 3) in the Green Book (EPA/USACE, 1991), comparison to field-collected organisms is performed to compare the laboratory steady-state bioaccumulation to steady-state body burden of contaminants in the vicinity around the HARS. While it is preferred that those field-collected organisms be the same species as those used for laboratory analyses, *Macoma* is not available in the vicinity of the HARS. Therefore assorted bivalve species were collected and analyzed to represent background steady-state body burden levels of contaminants of concern in the vicinity of the site. EPA Region 2 agrees that one background species may not be comparable to *Macoma* for background comparison. Specifically, bioaccumulative contaminants in sediments are more available to *Macoma*, a deposit feeder, than they are for *Mercenaria*, a filter feeder. Therefore we agree that the calculation of the background value should not include body burden levels in *Mercenaria*. Revised background levels are presented below. Regarding the strong influence of a particular tissue sample, please see response to comment 19-3.

Mean Background Concentrations in Field-Collected Organism		
Constituent	**Background Levels in Clam**	**Background Levels in Worm**
PAHs	(ug/Kg)	(ug/Kg)
Acenaphthene	9.7	0.5
Anthracene	12.8	1.6
Fluorene	9.0	0.3
Naphthalene	28.4	4.5
Phenanthrene	40.8	4.7
Benzo(a)pyrene	31.9	7.6
Fluoranthene	56.0	10.6
Pyrene	65.6	26.6
TOTAL PAHs	552.0	104.6
PESTICIDES		
Aldrin	0.98	0.14
Dieldrin		0.14
"Chlordane		0.73
Heptachlor		0.05
Heptachlor epoxide		0.17
Total Residual Chlordane/Heptachlor		1.70
Total Endosulfans		0.35
Total DDT		11.13
TOTAL PCBs	133.1	88.1
1,4-Dichlorobenzene [1]		
METALS		(mg/Kg)
Arsenic		4.89
Cadmium	1.49	0.11
Chromium (total)	1.55	1.31
Copper	6.95	2.78
Lead	0.90	1.64
Mercury	0.053	0.026
Nickel	1.34	0.77
Silver		0.15
Zinc	11.00	20.61

[1] There are no field collected background data assembled for this constituent.

As discussed in response to comments 1-3 and 3, EPA Region 2 has proposed to modify the HARS Framework by replacing the "Evaluation of Solid Phase Bioaccumulation Results for Dredged Material as

a Whole" section (which included the eight factors for LPC compliance listed in the Green Book) with the "Consideration of Combined Effects Evaluation: CBR, Total Carcinogenicity, and Non-Cancer Hazard Index" approach. As such, EPA Region 2 is proposing that background values no longer be used as part of the Framework for determining the suitability of dredged material proposed for use at the HARS as Remediation Material. While comparison to background levels would not directly be a part of the project-specific evaluative process, background levels will be considered in the derivation of the HARS-Specific Values. Comparisons to background levels will provide perspective to assumptions used in deriving the Values.

COMMENT 19-2: The same reviewer also judged that comparison of test tissue to background tissue would be valid if the bioaccumulation test tissue data were compared to the same species, but not valid when inter-species comparisons were made. The reviewer suggested that background values could be based on 28-day exposures of appropriate test organisms in the lab to near-HARS sediments.

RESPONSE 19-2: EPA Region 2 acknowledges that comparison to the same species may be preferred, but disagrees with the proposed alternative paradigm that background should be established based on 28-day exposures of appropriate test organisms in the laboratory to sediments collected near the HARS (see response to 1-4).

COMMENT 19-3: Several reviewers cautioned against the use of an arithmetic mean as the best measure of central tendency. Other measures, such as geometric mean and median were suggested. One review advocated use of the arithmetic mean where data sets are small and distribution is not known. In addition two reviewers recommended the use of ranges or distributions.

RESPONSE 19-3: EPA Region 2 recognizes that the arithmetic means calculated for some of the constituents may be biased by the individual data points. While use of a geometric mean as a measure of central tendency would produce values less influenced by a single high or low datum, geometric means tend to be biased low. EPA Region 2 prefers to use an arithmetic mean as an appropriately conservative measure of central tendency for the given simple data sets, and would propose continued use of the arithmetic mean in the proposed revisions to the HARS Framework.

Regarding the reviewers recommendation for ranges or distributions to support probabilistic risk assessment to be used, we feel this is not warranted in the existing framework. EPA Region 2's position is that the mean is the appropriate measure for estimating background. However, as discussed in response to comment 19-1, EPA Region 2 is proposing to eliminate use of background values from the Framework used to determine the suitability of dredged material proposed for use at the HARS as Remediation Material.

COMMENT 19-4: One reviewer regarded the use of half the detection limits for undetected observations as invalid.

RESPONSE 19-4: EPA Region 2 disagrees that this treatment of detection limit is invalid in the process of establishing background for this regulatory testing process. While we recognize that use of half detection limit for undetected analyses could be viewed as introducing bias to the calculation, it is a convention that is consistent with the policy that we employ for assessing typical data generated by dredging applicants. This process is used in other programs and is appropriately conservative to achieve the remedial goals of the HARS. However, as discussed in response to comment 19-1, EPA Region 2 is proposing to eliminate use of background values from the Framework used to determine the suitability of dredged material proposed for use at the HARS as Remediation Material.

COMMENT 19-5: One reviewer felt that it was more appropriate to compare concentrations in biota from the proposed dredging site to biota near the HARS.

RESPONSE 19-5: EPA Region 2 disagrees with this strategy (see response to comment 1-6). In addition, there are several issues that make this strategy neither plausible nor technically valid. Collection of organisms at the dredging site would yield uncertain results with regard to areal and depth distribution. Sample collection needs to be representative of the project area and depth. The Green Book describes the method used for consistently and representatively assessing/sampling dredged material. A site or sites near the HARS would need to be selected for consistency in all project evaluations, as the proposed direct tissue comparisons would need to be consistent throughout the entire permit evaluation program. However, as discussed in response to comment 19-1, EPA Region 2 is proposing to eliminate use of background values from the Framework used to determine the suitability of dredged material proposed for use at the HARS as Remediation Material.

Charge Group: General

Charge No. 20: Can baseline tissue concentrations, from appropriate benthic organisms resident to the HARS, be used as standards to determine suitability for Remediation Material as defined above?

Summary of Peer Review Comments

Individual Comments

Swartz Charge No. 20 Comment (1 para):
No. Specimens resident to the HARS may be exposed to the historic, unacceptable levels of sediment contamination at the HARS. Use of the tissue concentrations in such specimens as standards would tend to perpetuate the historic contamination and defeat the purpose of the remediation.

As explained in the answer to question 1, tissue concentrations determined in 28-d bioaccumulation tests applied to HARS sediment might be used to define a critical tissue concentration above the background level, but substantially less than the HARS level.

Gentile Charge No. 20 Comment (1 para):
If the "no further degradation principal" is adopted, then the HARS residues could be used as a baseline. The assumption being that test sediments resulting in benthic residues statistically similar to HARS would present no further degradation. If however, the HARS benthic residues for the test sediments exceed the HARS then one could argue further degradation and reject the use of test sediment. Obviously one question would be how do the HARS site tissue residues compare with reference site tissue residues?

Berry/Lake/Pruell Charge No. 20 Comment (1 para):
It seems appropriate to use background concentrations from organisms in the area around the HARS, but not from organisms directly in the HARS, if the assumption is that the HARS is degraded. If the goal was only to prevent further degradation it might be appropriate to use organisms from within the HARS as well.

Solomon/Sibley Charge No. 20 Comment (1 para):
Yes, with the qualifier on lipid normalization noted above.

Newman Charge No. 20 Comment (1 para):
Yes. This has been mentioned in several of my answers above.

Lee Charge No. 20 Comment (1 para):
Yes, given the caveats discussed under Questions 18 and 19.

Agency Response to Reviewers' Comments:

COMMENT 20-1: Three of the reviewers state that use of HARS baseline contaminant concentrations is not appropriate for determinations, primarily because it contradicts the no-degradation intent and would perpetuate the historic contamination and defeat the purpose of the remediation.

RESPONSE 20-1: EPA agrees (see response to comment 1-4).

COMMENT 20-2: Three other reviewers thought that it was appropriate to use the baseline concentrations.

RESPONSE 20-2: EPA disagrees (see response to comment 1-4).

COMMENT 20-3: One reviewer suggested that it seems appropriate to use background concentrations from organisms in the area around the HARS, but not from organisms directly in the HARS, if the assumption is that the HARS is degraded. If the goal was only to prevent further degradation it might be appropriate to use organisms from within the HARS as well.

RESPONSE 20-3: EPA disagrees (see response to comment 1-4).

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Proposed Changes to the Bioaccumulation Testing Evaluation Framework and Response to Scientific Peer Reviewers Comments on the Existing Framework for Determining the Suitability of Dredged Material to be Placed at the Historic Area Remediation Site (HARS)

IV. APPENDICES

October 19, 2000

APPENDIX A

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APPENDIX B (PEER REVIEW CHARGE)

SCIENTIFIC PEER REVIEW OF THE EPA REGION 2/CENAN FRAMEWORK FOR EVALUATING DREDGED MATERIAL FOR PROPOSED PLACEMENT AT THE HARS

Introduction: Goals of the Peer Review

The August 29, 1997 Final Rule, *Simultaneous De-designation and Termination of the Mud Dump Site and Designation of the Historic Area Remediation Site*, specifies that the historic area remediation site (HARS) will be remediated with uncontaminated dredged material (i.e., dredged material that meets current Category I standards and will not cause significant undesirable effects including though bioaccumulation; hereinafter referred to as "Remediation Material"). The rule further specifies that the HARS will be managed so as to reduce impacts within the Priority Remediation Area (PRA) to acceptable levels in accordance with 40 CFR 228.11. Placement of dredged material within the PRA is restricted to Remediation Material. This material will not cause significant undesirable effects, including through bioaccumulation or unacceptable toxicity in accordance with 40 CFR 227.6.
Evaluation of proposed dredged material regarding unacceptable toxicity is clearly defined in the Green Book as statistical criteria which require no interpretation. Evaluation regarding significant undesirable effects including through bioaccumulation requires assessment of chemical analyses of tissue from 28-day bioaccumulation tests. There are no specific regulatory criteria for this evaluation; however there are existing regional guideline values that have been developed and used, by the U.S. Environmental Protection Agency (EPA) Region 2 and the U.S. Army Corps of Engineers New York District, to evaluate the constituents in accordance with 227.6.

This peer review charge is to assess whether the testing evaluation process is adequate to properly determine whether a tested sediment is suitable for Remediation Material as defined. Your review should focus on the framework for evaluation of bioaccumulation data and guideline values used; it should not deal with on toxicity/mortality testing. Please bear in mind that the testing evaluation applies to risks pertaining to ocean placement of the sediment, and not to risks pertaining to other alternatives such as leaving the sediment in place.

This charge is in the form of questions on critical aspects of the evaluation framework. General references are cited in each charge question to aid in finding the issue in question. Note that these are general guiding referrals and should not be considered the only review item for those specific issues. Please answers the assigned questions as directly as possible, given the provided materials and your own expertise. If you are unable to answer a particular question on the basis of the provided materials, please inform us of information needed to answer the question. Also, keep in mind that there are additional environmental data resources and test data pertaining to the New York Bight available in EPA Region 2, if they are needed.

Framework

1. Is the EPA Region2/CENAN Framework for evaluating bioaccumulation results scientifically appropropriate for determining the suitability of dredged material as Remediation Material? If not, describe deficiencies. *(Please see Region2/CENAN joint evaluation memorandum, Figure 1)*

2. Which of the risk-based values derived constitute "true" conservative estimates of risk levels (i.e., exceeding the value should be interpreted as sufficient cause to conclude that significant undesirable effects may result through bioaccumulation)? Which of the risk-based values derived constitute conservative screening values (i.e., test tissue concentrations below the value can confidently be interpreted to pose no risk of significant undesirable effects and exceeding should be further evaluated before the probability of significant undesirable effects can be assessed)? How can the "true" risk levels be calculated for those compounds which you believe only to have screening values? How should test concentrations be compared to risk-based levels to determine whether they are exceeded.

3. In conducting the integrated effects evaluation using the types of data provided by the applicant, which of the eight factors for LPC compliance listed in the Green Book are appropriate and relevant? How can a quantitative/strategic framework be established to evaluate tissue data for those factors? Considering that comparison to regional Matrix values and site-specific risk values represent case-specific evaluations, is it necessary to conduct the integrated effects evaluation of the bioaccumulation results? *(Please see Reference No. 61, page 6-6)*

Benchmark and Risk Evaluation Values

4. Regional Matrix Values

   A. Are the Matrix values suitable for determining the suitability for placement at the HARS as Remediation Material?

   B. Regional Matrix values were developed in 1981 by compiling available field data for mercury, cadmium, PCBs, and total DDTs. Were these values derived appropriately for their intended use? Based on current data sets and scientific literature, are these 1981 values suitable for predicting the significant undesirable effect due to bioaccumulation? *(Please see Reference No. 57)* If not, identify more current references, data sets, and/or actual chemical specific values that would be more appropriate.

5. Regional Dioxin Values

   A. Currently, the presence of 2,3,7,8-TCDD at a detectable concentration (i.e., greater than or equal to one part per trillion (pptr)) in tissues of organisms exposed to dredged material precludes its classification as Category I (hence Remediation Material); presence of the remaining dioxin/furan congeners, at concentrations of TEQs equal to or greater than 4.5 pptr, results in a similar conclusion. When 28-day tissue concentrations exceed these values, is there sufficient cause to conclude that placement of the material is not suitable as HARS Remediation Material? If not, what levels indicate sufficient cause for this conclusion?

(Please see Reference No. 89)

B. Are dioxin values suitable for predicting the significant undesirable effects due to bioaccumulation? If not, should these values be based on a risk analysis paradigm in which the size of the human population subgroup potentially exposed through intentional behavior is compared to the size of the general population in the region? Since the primary route of exposure is through consumption of fish and shellfish, should the variability in potential exposure due to differences in fishing behavior (e.g., target species, seasonal preferences) be incorporated in the risk paradigm? How would a benchmark protective of human health compare to benchmarks determined using an ecological risk analysis paradigm for resident fish and piscivorus wildlife?

6. FDA Action Levels *(Please see Reference No.61, Sec. 6.3)*

A. Are FDA Action Levels useful as upper limit human health benchmarks?

B. Would the evaluation be improved by omitting comparison of tissue results to FDA Action Levels?

7. Human Health Risk, Cancer and Noncancer

A Are the risk values suitable for determining the suitability for placement at the HARS as Remediation Material? If there are better alternatives for human risk, specifically what are they?

B. Benthic tissue levels for cancer protection were derived using assumptions focused on attaining a cancer protection at the 10^{-4} risk level. Is this risk appropriate for a determination of ocean placement of Remediation Material? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Page A-4, A-5)*

C. Benthic tissue levels for noncancer protection were derived using Reference Dose (RfD) of several organic and inorganic contaminants for the protection of human health. Are these values appropriately and consistently derived? Is the whole body/fillet conversion factor of 1.35 an appropriate factor for all of the contaminants considered if human exposure is assumed to be primarily via consumption of the fillet portion of the fish? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Attachments B and C)* If not, what factors would be appropriate? For the lead noncancer value, since there is no RFD for lead the value was derived differently than the other metals. Was the value derived appropriately? *(Please see Reference No. 88)*

D. Are the risk values suitable for predicting the significant undesirable effects due to bioaccumulation? Since the primary route of exposure is through consumption of fish and shellfish, should the variability in potential exposure due to differences in fishing behavior (e.g., target species, seasonal preferences) be incorporated in the risk paradigm?

8. Ecological Risk

A. Ecological effects benchmarks include the Water Quality Criteria Tissue Level (WQCTL), Critical Body Residue (CBR) associated with narcotic responses, and certain mutagenic/teratogenic effects. Is it valid to use the CBR effect end point for evaluating significant undesirable effect? Are there other ecological end points that should be used to measure ecological risk that are protective of marine benthic and fish life via trophic transfer, particularly for PAHs? If so, identify. With regard to a narcotic effect for chlorinated organic compounds, should an additive approach be considered to include the contribution of chlorinated hydrocarbons against this narcotic (CBR) endpoint.

B. Is the Region 2 WQCTL approach (i.e., multiplying the Water Quality Criterial Chronic Value by the Bioconcentration factor) appropriate for determining ecological effects levels of the contaminants for which they were developed? Specifically, are the appropriate BCFs used (for fish, bivalves, etc)? (Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Page A-1)

C. BCFs reported for fish were used in the calculations of WQCTLs for organics; is this derived level appropriate for setting benthic tissue ecological effects levels? If the fish tissue levels are used, should adjustments be made to the derived levels to reflect the higher lipid contents of the benthic organisms used in the testing program?

D. Are the WQCTLs calculated for metals using bivalve BCFs appropriate for setting levels for polychaetes or vice versa?

E. Are the uncertainty factors applied while deriving ecological effects levels for PAH contaminants appropriate? Does this adequately address the uncertainty around the derived values? Can uncertainty be accounted for using these order of magnitude adjustments? Should they be applied elsewhere to the other risk-based values?

F. Are the risk values suitable for predicting the significant undesirable effects due to bioaccumulation; are there better alternatives for ecological nonspecific risk?

G. If you believe that these values are over- or under- conservative, what do you believe to be an appropriate way to improve them.)

Calculations

9. Should total PCBs continue to be estimated by doubling the total of 22 congeners or should it be quantified directly using another measure of quantification? What method is most appropriate for sediments in the NY/NJ Harbor area? *(Please see Reference No. 60, Table 4-4B)*

10. Currently, 28-day tissue concentrations of certain organic contaminants are adjusted by some multiplier to estimate the concentrations of those compounds had the exposure been of sufficient duration to allow attainment of steady state levels. *(Please see Reference Nos.5 and 46)* Are these adjustments appropriate? Should steady state corrections be applied to any other of the listed contaminants? Are there other compounds for which we test that are not expected to approach steady state within the 28-day period?

11. Is the calculation and use of BaP toxicity equivalence an appropriate way to estimate the potential carcinogenicity of PAHs? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Section C.)*

12. Similar to PCBs, only a subset of those PAHS present in New York Harbor are measured for testing evaluation. How should the remainder be considered?

13. Is the assumption of a trophic transfer coefficient of one appropriate for use in evaluating the potential for human health and ecological impacts associated with metals in Remediation Material? Are the trophic transfer factors calculated for organic compounds correct? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Attachment C.)*

14. Is the assumption of a fish consumption rate of 6.5 g/day appropriate for use in evaluating the potential for human health impacts associated with metals in Remediation Material? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Page A-5)* Would it be appropriate that the evaluation focus on a higher consumption population?

General

15. Is it plausible to replace any other risk assessment assumptions with assumptions specific to the HARS site? *(Please see Region2/CENAN joint evaluation memorandum, Appendix for Table 1, Attachment C and Reference Nos. 88)* Is it appropriate to consider the HARS intended use to be factored into an evaluation of effects at the community or population level?

16. Is use of the Squibb *et al.* (1991) report appropriate for identifying the contaminants of concern? Are there contaminants which should be added to or deleted from the list of contaminants for which we presently test? *Please see Reference No. 51)*

17. Should risks from synergistic effects, from exposure to multiple contaminants, be evaluated using results from tissue analyses? If so, how? If not, why not?

18. Is test tissue concentration exceeding reference tissue concentration by less than 10X a meaningful evaluative criterion? *(Please see page 9 of the Region2/CENAN joint evaluation memorandum)*?

19. Are the studies from which background tissue concentrations were calculated weighted appropriately? If not, what method is recommended? Is the use of the mean the most appropriate measurement of central tendency? If not, what measure should be used? *(Please see Reference No. 98)* Are the assumption, presented on page 14 pertaining to comparisons of bioaccumulation in test tissue to tissue concentrations in organisms from the vicinity of the remediation site, valid for evaluating undesirable effects?

20. Can baseline tissue concentrations, from appropriate benthic organisms resident to the HARS, be used as standards to determine suitability for Remediation Material as defined above?

APPENDIX C

Background on Category I, II, and III

Prior to the de-designation of the MDS, EPA Region 2 and the USACE-NYD used a three category system to evaluate dredged material proposed for ocean disposal at that site. The definitions are as follows:

Category I : Dredged material that meets the ocean dumping criteria (including the requirements regarding acute toxicity) and:

- bioaccumulation test results are below the regional Matrix levels [a] for cadmium, mercury, total PCBs, and total DDT, and below the regional Category I values for dioxin [b]; and

- bioaccumulation test results for the other bioaccumulative chemicals of concern identified in USEPA Region 2/USACE-NYD (1992) do not indicate a potential for undesirable effects using conservative assessment techniques.[c]

Category II: Dredged material that meets the ocean dumping criteria (including the requirements regarding acute toxicity) and:

- bioaccumulation test results exceed any of the regional Matrix levels for cadmium, mercury, total PCBs, and total DDT, or exceed the regional Category I values for dioxin but are less than the regional Category III value for dioxin; or

- bioaccumulation test results indicate a potential for those compounds or other bioaccumulative chemicals of concern to accumulate at levels that could indicate a potential for undesirable effects using environmentally conservative assessment techniques, but do not indicate that dumping would result in significant undesirable effects.

Category III: Sediments which do not meet ocean dumping criteria. These sediments are those that fail acute toxicity testing or pose a threat of significant undesirable effects due to bioaccumulation that cannot be addressed through available disposal management practices. These sediments cannot be disposed in the ocean.

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[a] Regional Matrix values are predetermined values for cadmium, mercury, total PCBs, and total DDT that indicate a potential to accumulate at levels that could indicate a potential for undesirable effects using environmentally conservative assessment techniques, but do not indicate that dumping would result in significant undesirable effects.

[b] The regional values for dioxin are as follows:

Category I — Tissue concentrations for each test species resulting from 28 day exposure to test sediment are less than:

1 part per trillion (pptr) 2,3,7,8 TCDD;

AND

4.51 pptr total equivalent toxicity using total equivalent factors (TEF's) for the 16 other 2,3,7,8-substituted dioxin/furan compounds (congeners).

Category II — The material is not Category I, but tissue TEF concentrations for each test species resulting from

28 day exposure to test sediment are at or below 10 pptr for the sum of all 17 of the 2,3,7,8 congeners.

Category III — Tissue TEF concentrations for either test species resulting from 28 day exposure to test sediment are above 10 pptr for the sum of all 17 of the 2,3,7,8 congeners.

[c] Environmentally conservative assessment techniques include evaluating human and ecological risk and other relevant synergistic effects information, as provided for in the Green Book.

Appendix D.

Development of Benthic Tissue Values for the Assessment of the Potential for Ecological effects Resulting from Bioaccumulation of Contaminants in Dredged Material Proposed for Use as Remediation Material at the

Historic Area Remediation Site (HARS)

I. Purpose:

To review relevant published and peer-reviewed studies to identify tissue contaminant levels that could be used as conservative estimates of thresholds for the potential for adverse ecological effects. These values would be developed in response to comments by scientific peer reviewers and would be proposed to be considered in the evaluation of results of 28-day bioaccumulation testing of dredged materials proposed for placement as Remediation Material at the Historic Area Remediation Site (HARS). EPA Region 2 proposes that the use of empirical data that relate the incidence of adverse effects and body residues (such as the data generated by studies used for this effort) to be the preferred method for deriving proposed benthic tissue values for the protection of ecological receptors.

The primary criterion for a study to be considered relevant for this effort is a study design that ensures that organisms are exposed to a single contaminant. This study design maximizes our ability to assign causality of any observed effects to the body burden of the specific contaminant considered. While USEPA recognizes that organisms that are exposed to dredged materials are generally simultaneously exposed to more than one contaminant, we judge the potential for significant synergism and antagonism of contaminants to be low (see response to peer reviewers' comments to charge 17). Values that would be developed using these empirical studies would not establish a probability for adverse effects but rather would estimate a tissue residue above which the *potential* for adverse effects is presented. EPA Region 2 believes that this threshold (avoidance of the potential for significant undesirable effects) is an appropriate goal for the assessment of bioaccumulation test results, given the remedial intent of the HARS.

II. Compilation and Organization of Data

Available datasets relating measured tissue contaminant residues and incidence of effects (linked residue-effect data) were identified through an exhaustive review of peer-reviewed scientific studies. As noted above, only those studies that related residues and incidence of effects of single contaminants were considered relevant for the development of the proposed values. The primary tools used in identifying relevant peer reviewed studies were the databases compiled by the U.S. Environmental Protection Agency Office of Research and Development's Mid-Continent Ecology Division in Duluth MN (Jarvinen and Ankley 1999) and the U.S. Army Corps of Engineers' Waterways Experiment Station in Vicksburg, MS (USACE 1998). Additional relevant studies that were published since release of these databases were identified through literature searches with the assistance of staff from the USEPA-Office of Research and Development (Duluth, MN).

Compiled data were organized into separate tables for each contaminant of concern: one for tissue residues associated with adverse effects; and another for tissue residues that were not associated with adverse effects. Effective and non-effective residues were further grouped into data reported for specific classes of aquatic organisms (i.e. molluscs, infaunal worms, crustaceans and fish).

Information regarding the species, exposure pathway (e.g. water, sediment, diet, injection), whole body (or organ) residue, endpoint (e.g. survival), and magnitude of effect was recorded in the tables for each observation. References (or reference notations from the databases) were also included for each observation to facilitate comparison of effective and non-effective residues within individual studies.

III. Uncertainties Associated with the Linked Residue-Effect Approach

There are various areas of uncertainty associated with using linked body residue-effect data to develop tissue residues that serve as values for the assessment of the potential for ecological effects due to bioaccumulation of contaminants from sediments. Jarvinen and Ankley (1999) provide an excellent discussion and examples of uncertainties associated with the use of linked data in the text that prefaces the USEPA-ORD database. It is important to note, however, that other approaches that are alternatively used to derive tissue residue-based values (e.g. WQCTLs) share essentially all of these areas of uncertainty and often introduce additional uncertainties associated with estimating rates of uptake and assimilation of contaminants by organisms from external media. Several areas of uncertainty associated with this method are listed and discussed below.

The use of linked residue-effect data to identify tissue residues that are interpreted as thresholds for effect assumes that there is a *critical body residue* above which the potential for effects materializes. The critical body residue assumption relies on a relatively constant body burden threshold for effects regardless of differences in exposure conditions. This criterion, however, has been shown to not hold entirely true in many instances and for most contaminants. The specifics of exposure conditions often alter the body burden at which lethality, or other adverse effects, occurs. Factors affecting estimates of the critical body residue include:

-Salinity, temperature, hardness, pH, and other water quality parameters;

-Presence vs. absence of sediment during exposure;

-Medium/pathway to (by) which organisms are exposed to a contaminant;

-Duration of exposure;

-Rate of contaminant uptake by organism;

-Differences in the whole body residue vs. residue at specific target organs;

-Changes in toxicity due to physiological state (can be seasonal or lifecycle related);

-Feeding/non-feeding during exposure;

-Specific compound used in exposure (e.g. metal salts or speciation);

-Ages of test organisms used;

-action associated with unmeasured metabolites rather than parent compound;

Additional areas of uncertainty that must be recognized are: 1) the absence of multi-generational effects data in many cases; 2) the possibility for unmeasured adverse effects; 3) differences between body residues measured in surviving and dead organisms within the same treatment; and 4) the potential for differences in critical body residues between species of a given organism class (e.g. similar organisms residing in freshwater vs. saltwater).

As noted above, most of these attendant uncertainties also accompany other methods that are currently being used to derive tissue guidance levels and cannot be avoided altogether. It is therefore of paramount importance that data are carefully considered before a guidance level is derived so that the potential for gross error to be introduced by these uncertainties is minimized. Studies would be carefully reviewed and screened via specific criteria (criteria used for screening are listed below in Section V) to ensure that only those data that were most relevant and robust would be used in the derivation of tissue residue values.

IV. Screening of Data: Rationale and Assumptions

The effects of differences in exposure conditions on the body burdens associated with effects often seemed to follow general patterns. For example, higher lethal body burdens appeared to be required to elicit responses in organisms at lower temperatures and at higher salinities. However, these relationships are not consistent enough to justify applying these apparent patterns as general rules for screening data. It was, however, deemed appropriate to more heavily weigh values resulting from exposures that were conducted with conditions that most closely resembled those anticipated to be associated with placement of Remediation Material at the HARS. A description of environmental conditions at the HARS is provided in the Supplemental Environmental Impact Statement prepared for the site (USEPA, 1997). Salinity, temperature, pH and most other environmental parameters of bottom water at the site are typical for seawater in temperate areas. As might be obvious, exposure to contaminants in dredged sediments will occur in the presence of sediment. Therefore, when an individual study reported linked effect-residue data using differing exposure conditions, the exposures were examined to identify the effective (or non-effective) residue that resulted from conditions that *most* closely resembled conditions at the HARS (e.g. presence of sediment, temperature (~25 ^{o}C), salinity (~30 ppt), pH (~8));

Benthic organisms, such as molluscs, small crustaceans, and worms are assumed to be directly exposed to contaminants through dermal contact or ingestion of sediment and pore water (or both), as opposed to motile predators. Therefore, studies in which exposure was achieved through absorption from water may resemble actual field exposure sufficiently to enable the results of these studies to be used as estimates of body residues associated with effects to these benthic organisms.

Motile predators, such as fish, are assumed to be *primarily* exposed to contaminants in sediments at the HARS through predation on infaunal and epifaunal benthic organisms that reside in HARS sediments. Therefore, using the results of absorption studies for identifying body residues associated with effects in these organisms needs to be approached with caution. To reflect the exposure pathway of concern in these organisms (i.e. fish), data relating the effects of dietary exposure to body burdens are considered to be most relevant for the development of tissue values associated with protection of these organisms to

reflect the realistic exposure pathway. The potential for trophic transfer of contaminants from prey organisms residing at the site to predators was also considered. This would be accomplished through the proposed application of dietary assimilation and exposure factors in the back-calculation of tissue residues in benthic prey associated with effects levels in predator species. Many dissolved metals are believed to exert their toxic effect at the gill (also a primary locus for accumulation of waterborne metals), and therefore use of the measured whole body residue reported in absorption studies for development of guidance levels based on fish residues will likely result in a guidance value that is overly conservative.

Effects on reproduction in aquatic organisms are reported in the literature. The potential for decreased reproductive output was reported using endpoints such as egg production, egg hatching, and larval survival rates. Most of the studies reporting linked effect-residue data associated with reproduction, however, were designed to assess the potential for reproductive effects due to waterborne contaminants. These data were carefully examined to select only those data that were relevant to the interpretation of sediment-related bioaccumulation test results. Specifically, only reproductive effects that were attributable to parental (i.e. maternal) tissue residues were deemed to be relevant to the interpretation of results of bioaccumulation assays. An example of a reproductive effect that could usually be linked to the maternal whole body residue is decreased fecundity (egg production).

In many studies, decreased egg and larval survival rates could not be attributed to parental tissue residues because eggs continued to be exposed to contaminated water after spawning. Data relating egg contaminant residues and reduced viability could conceivably be considered with other methods (e.g. partitioning models) to assess the potential for impacts due to sediment-sorbed contaminants in dredged material placed at the HARS. Such effects are not likely to be ecologically significant at the HARS, since: a) most marine organisms at the HARS have pelagic eggs and therefore their eggs are not expected to come into significant contact with sediment-sorbed contaminants; and b) those HARS fish species that do have demersal, adhesive eggs (including winter flounder, Atlantic herring, little skate, ocean pout, sea raven, longhorn sculpin) (USEPA 1997) do not primarily spawn in the vicinity of the HARS (Grosslein and Azarovitz, 1982).

Data were screened for applicability to the development of benthic tissue guidance values for the evaluation of results of bioaccumulation tests conducted on dredged material proposed for placement as Remediation Material at the HARS. Criteria used in screening the data are outlined below. Confidence in the resulting estimates varied among the contaminants due to the number and quality of studies available. Despite the associated uncertainties, the benthic tissue guidance values derived using this approach would have the benefit of being empirically supported and could be applied with greater confidence than tissue values derived from effective concentrations in external media (e.g. sediment, water).

V. Criteria for Screening Data/Studies

In light of the considerations discussed above (Section IV), the following specific screening criteria were used to maximize the relevancy and appropriateness of studies and data to be included in this effort. (In deriving values for contaminants for which little data was available, the criteria for inclusion would be somewhat more liberally applied.)

For data to be included in this exercise:

1) Effective residues would have to be related to impacts on survival, growth, or reproduction to be considered as presenting a potential threshold for significant effect. The ecological relevance of physiological, behavioral or endocrine responses is impossible to assess at this time. (The absence of these responses, however, could be considered in the evaluation of contaminants for which there is a paucity of relevant data);

2) Tissue residues would have to be reported on a whole-body residue basis (in certain instances, however, sufficient information may be provided to estimate whole body data from organ data. Inclusion of these data could be considered in the evaluation of contaminants for which there was a paucity of relevant data);

3) Effective residues reported within a given study would have to support a dose-dependent response (i.e. accumulation of the contaminant of concern would have to have occurred and effects would have to be consistently associated with higher accumulations of the contaminant);

4) Exposures resulting in reported effects would have to be conducted in a controlled setting (i.e. exposures conducted in the field would be excluded from this exercise);

5) Exposures would have to have been initiated at post-hatch stages or later (i.e. no egg/embryo absorption studies).

In addition, the data were carefully reviewed to maximize the likelihood that the effects reported to individual organisms were severe enough to equate to probable effects at the population level to organisms at the HARS. Because effects on individuals would be used to estimate significant adverse

effects on populations in the field, preference would be given to using ER50s (tissue residues resulting in a 50% reduction in survival or growth or causing an effect in 50% of exposed organisms) in identifying tissue residues to be used as remediation values for evaluating bioaccumulation test results. Use of this effect level is proposed by Region 2 to be appropriate, because the 50% effect level was used by EPA in setting water quality criteria and a suitable model for evaluating population level effects, and thereby for justifying the use of a different effect level, is not available for the benthic environment of the HARS and its environs.

Because estimates of effective (or non-effective) residues can differ widely with exposure conditions it will be necessary to carefully consider these conditions while identifying the effective residues used in setting benthic tissue remediation values. To this end, the following approaches will be used to review data for maximum relevance:

a) When an individual study reported linked effect-residue data using differing exposure conditions, the exposures will be examined to identify the effective (or non-effective) residue that resulted from conditions that *most* closely resembled conditions at the HARS (e.g. presence of sediment, temperature (~25 ^{o}C), salinity (~30 ppt), pH (~8));

b) When overlap in the range of effective and non-effective residues for a given species occurs *within the same study and exposure conditions*, the lowest effective concentration within the range that exceeds the range of non-effective residues (i.e. the highest non-effective residue) will be used to estimate the benthic tissue value that would result from that dataset;

c) When ER50s are not reported the following methods are proposed to be used in the interpretation of results. In most cases, effective residues that are identified as being below an ER50 would not be used to set the benthic tissue HARS remediation values. Rather these results would be used to assess the protectiveness of the final selected values (the ER50 residue was estimated as being greater than the reported value). If effective residues were identified as being greater than the ER50, the ER50 would be approximated by linear interpolation between the reported effective residue and the highest non-effective residue or between effective residues bracketing the ER50. The assumption of linearity is conservative because the dose-response relationship of many contaminants is exponential (i.e. using the assumption of linearity results in an underestimate of the ER50 if the dose-response relationship is exponential, rather than linear).

VI. HARS Benthic Tissue Remediation Values

Benthic tissue remediation values would be developed for the following contaminants using linked empirical observations of tissue residues and effects:

Metals

Cadmium (Cd)	Lead (Pb)
Mercury (Hg)	Zinc (Zn)
Arsenic (As)	Chromium (Cr)
Silver (Ag)	Nickel (Ni)
Copper (Cu)	Tributyltins (TBT)

Organic Compounds

total polychlorinated biphenyls (PCBs)	aldrin/dieldrin
total DDT	total chlordane
total endosulfans	

Trophic transfer (expressed as dietary assimilation and exposure factors) would be considered in the derivation of values for the protection of fish (see response to comment 13). Dietary assimilation by fish of contaminants in benthic prey would be expressed as a trophic transfer factor (the ratio of body burdens in fish predators and their prey). Trophic transfer factors for organic contaminants were derived by L. Burkhard of the EPA Mid-Continent Ecology Division using the Gobas food web model (EPA, 1995). The proposed trophic transfer factor for mercury would be based on work by Cabana *et al.* (1994). Proposed trophic transfer factors for selected metals were derived based on a review of the available literature. Their derivation is reported in Appendix E. In addition, from the preponderance of evidence

regarding the seasonal nature of fish residency at the HARS (see USEPA, 1997), EPA Region 2 proposes incorporating a site use factor to reflect fish residency in the derivation of tissue values for evaluating risk to ecological receptors (see response to comment 15-1).

Certain endpoints would be intended to be applied to sums of organic compounds (e.g. endosulfans, chlordane, aldrin/dieldrin) with differing molecular weights. Equipotency would be assumed for these compounds (but see Lotufo *et al.* 2000), and the residues of higher molecular weight compounds would be adjusted to the lowest molecular weight of any compounds considered in the total. Adjusting the total value in this manner results in a conservative estimate of the value while addressing the fact that molar concentrations are appropriate for expressing the toxicity of mixtures of compounds.

An example of development of a benthic invertebrate tissue value using the methods outlined above is presented below. This example derives a benthic tissue guideline that is proposed for use in evaluating the potential for adverse ecological effects from polychlorinated biphenyls (PCBs). The data that was compiled for other contaminants and proposed for use in developing guidance values for the other compounds (or classes) listed above are also provided below.

Polychlorinated Biphenyls (tPCB)

Linked Residue-Effect Data: Data linking the incidence of adverse effects to aquatic organisms with simultaneously measured tissue residues of tPCB are readily available in the scientific literature. Data from studies reported in the two databases (Jarvinen and Ankley, 1999; USACE 1998) are presented in Table A-12(a-c).

Species Coverage: Whole body tissue tPCB residues and incidence of effects were reported for 6 species of molluscs, 1 species of infaunal worm, 8 species of crustaceans, and 13 species of fish. Organ-specific or egg/embryo data were reported for 3 additional fish species.

Development of Tissue Values:

Molluscs

A whole body tPCB residues associated with adverse impacts on molluscan survival was reported at 1.4 ppm in the blue mussel, *M. edulis*. Whole body tPCB residues of 4.0 and 425 ppm were reported to be associated with reduced growth of the oyster, *C. virginica*. Non-effective whole body residues associated with survival of molluscs were reported to 425 ppm, and with growth at 101 ppm. No whole body residues were reported simultaneously with observations on reproduction of molluscs.

	Endpoint	**Measured Conc.**	**Est. ER50 Conc.**
M. edulis	Survival	1.4 ppm	1.0 ppm[a]
C. virginica	Growth (ER10)	4.0 ppm	>4.0 ppm
C. virginica	Growth	425 ppm	263 ppm[a]

[a] Magnitude of effect was not reported in Jarvinen and Ankley (1999). ER50 was estimated using reported non-effective residues and assuming measured effective residue was ER100

In light of these results, bioaccumulation of tPCB to 1.0 ppm (1,000 ppb) is concluded to be safe for prevention of adverse effects to molluscs resulting from placement of dredged material as Remediation Material at the HARS.

Infaunal Worms

The sole reported whole body tPCB residue associated with incidence of effect on infaunal worms was a non-effective residue of 0.63 ppm (630 ppb) associated with survival.

In light of this result, bioaccumulation of tPCB to >0.63 ppm (630 ppb) is concluded to be safe for prevention of adverse effects to infaunal worms resulting from placement of dredged material as Remediation Material at the HARS.

Crustaceans

Reported whole body tPCB residues associated with adverse impacts on crustacean survival ranged from 3.9 to 409 ppm, growth from 0.7 to 11 ppm, and reproduction from 246 to 552 ppm. Non-effective whole body residues associated with survival of crustaceans were reported to 552 ppm, with growth to 74.5 ppm, and with reproduction to 127 ppm.

Species	Endpoint	Measured Conc.	Est. ER50 Conc.
G. pseudolimnaeus	Survival	409 ppm	398 ppm[a]
L. polyphemus	Survival (ED>50)	31.9 ppm	23.7 ppm[a]
P. pugio	Survival (ER45, 60)	27, 65 ppm	39.7 ppm
P. duorarum	Survival	3.9 - 16 ppm	2.6 - 8.0 ppm[a]
P. aztecus	Survival (ER43)	42 ppm	>42 ppm
D. magna	Growth	0.7 - 11 ppm	4.0 - 6.15 ppm[a]
G. pseudolimnaeus	Reproduction	246 - 552 ppm	159 - 340 ppm[a]

[a] Magnitude of effect was not reported in Jarvinen and Ankley (1999). ER50 was estimated using reported non-effective residues and assuming measured effective residue was ER100

In light of these results, bioaccumulation of tPCB to 2.6 ppm (2,600 ppb) is concluded to be safe for prevention of adverse effects to crustaceans resulting from placement of dredged material as Remediation Material at the HARS.

Fish

Reported whole body tPCB residues associated with adverse impacts on fish survival ranged from 0.36 to 4300 ppm, with growth from 11.0 to 52.7 ppm, and with reproduction from 9.3 to 553 ppm. Non-effective whole body residues associated with survival and growth of fish were reported to 1253 ppm, and with reproduction to 567 ppm.

Species	Endpoint	Measured Conc.	Est. ER50 Conc.
11 species	Survival (various)	0.36 - 4300 ppm	0.765 - 2590 ppm[a]
P. promelas	Growth	11.0 - 50.0 ppm	30.6 ppm[a]
O. mykiss	Growth (non-effective)	52.7 ppm	>52.7 ppm[b]
P. phoxinus	Reproduction	170 - 180 ppm	95.5 ppm[a]
P. promelas	Reproduction	83 - 553 ppm	133 ppm[c]
C. variegatus	Reproduction (ER23-74)	9.3 - 25 ppm	17.15 ppm

[a] Magnitude of effect was not reported in Jarvinen and Ankley (1999). ER50 was estimated using reported non-effective residues and assuming measured effective residue was ER100

[b] Whole body residue was estimated as 1.35*residue in fillet

[c] ER50 assigned using overlap of effective and non-effective residues

In light of these results, the benthic tissue tPCB value developed for interpretation of bioaccumulation test results will prevent adverse effects to fish if it is designed to ensure that tissue residues in fish preying on benthic organisms at the HARS will not exceed 0.765 ppm. To backcalculate benthic tissue values, it is necessary to incorporate conservative estimates of: a) the amount of time that fish can reasonably be expected to be foraging at the HARS; and b) the efficiency by which tPCB in prey is assimilated by fish (i.e. accomplished by the application of trophic transfer factors (food chain multipliers)) into the calculations. The amount of time that fish actually spend foraging within the confines of the HARS is

difficult if not impossible to measure. Most fish species, however, exhibit seasonal patterns in abundance at given locations due to migration (Bigelow and Schroeder 1953; Grosslein and Azarovitz 1982). Region 2 staff examined landings in the vicinity of the HARS and derived a 'seasonal residency-site use factor' to account for the absence of fish species from the vicinity of the HARS during certain times of the year. Major fish species were estimated to be present in the area of the HARS during approximately 78% of the year (See response to comment 15-1). Therefore, a multiplier of 1.29 is applied in the back-calculation of benthic tissue values to account for the seasonality of fish distribution and abundance at the HARS. In this calculation, benthic prey tPCB residues are estimated by backcalculating from whole fish tissue residues by applying a trophic transfer factor of 0.333, developed by L. Burkhard (USEPA, Office of Research and Development. Duluth MN) using the Gobas *et al*. (1993) food chain model.

In light of the presented results and after consideration of the potential for trophic transfer and the seasonal presence of fish that may be potentially foraging at the site, bioaccumulation of tPCB to 0.329 ppm, or 329 ppb, in benthic prey organisms (this results from the lowest ER50 for adverse effects (0.765 ppm) X the seasonal residency-site use factor (1.29) X the trophic transfer factor (0.333)) is concluded to be safe for prevention of adverse effects to fish resulting from placement of dredged material as Remediation Material at the HARS.

Proposed Benthic Ecological Tissue Value for tPCB:

The benthic tissue value for evaluation of the potential for ecological effects of tPCB in dredged material proposed for use as Remediation Material at the HARS is 329 ppb, based on the benthic prey residue associated with the lowest ER50 reported in the literature for survival of fish

Table A-1 (a). Effective Whole Body Residues - Cadmium, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Cd chloride	snail (FW)	*Helisoma spp*	water	125	S (ER50)	Carlson *et al*.,1991
Cd chloride	snail (FW)	*P.integra*	water	30	S (ER80)	Spehar *et al*.,1978
Cd chloride	mussel -SW	*M.galloprovincialis*	water	39	S (ER90)	Pavicic and Jarvenpaa, 1974
Cd chloride	rock oyster	*S.commercialis*	water	48 - 85	S (ER100)	Ward, 1982
Cd chloride	zebra mussel	*D.polymorpha*	water	160 - 200	S (ER96)	Kraak *et al*.,1992
Cd chloride	zebra mussel	*D.polymorpha*	water	108 - 120	S (ER50)	Kraak *et al*.,1992
Cd chloride	clam - SW	*C.fluminea*	water	9	S (ER40)	Baudrimont *et al*., 1997
cadmium	worm - SW	*N.arenaceodentata*	water	16.86	G (ER43)	Jenkins and Sanders, 1986
cadmium	worm - SW	*N.arenaceodentata*	water	61.82	R (ER100)	Jenkins and Mason, 1988
Cd chloride	oligochaete - FW	*L.variegatus*	water	134	S	Carlson *et al.,* 1991
Cd chloride	brine shrimp nauplii	*A.salina*	water	5.4 - 7.5	G	Jayasekara *et al*.,1986
Cd chloride	cladoceran (FW)	*D.galeata mendotae*	water	3.5	S, R	Marshall, 1978
Cd nitrate	cladoceran (FW)	*D.magna*	diet	8.8	S	Ferard *et al*., 1983
Cd nitrate	cladoceran (FW)	*D.magna*	diet	6.4	R	Ferard *et al*., 1983
Cd chloride	cladoceran (FW)	*M.macrocopa*	diet	9.0 -20	S,G,R	Hatakeyama and Yasuno, 1981
Cd chloride	mysid shrimp	*M.bahia*	water	4.4	S, R	Carr *et al*.,1985
Cd chloride	mysid shrimp	*M.bahia*	water	2.4	S (ER50)	Carr *et al*.,1985
Cd chloride	mysid shrimp	*M.bahia*	water	1.3	G (ER32)	Carr *et al*.,1985
CdCl hydrate	amphipod - FW	*G.fossarum*	diet & water (hard)	53 - 101.6	S	Abel and Barlocher, 1988
CdCl hydrate	amphipod - FW	*G.fossarum*	water	5.4 - 54	S	Wright and Frain, 1981
Cd chloride	amphipod - FW	*G.fossarum*	diet	4.4	S	Duddrudge and Wainwright, 1980
CdCl hydrate	amphipod - FW	*E.echinosetosus*	water	16 - 22	S (ER40)	Martinez *et al*., 1996
Cd chloride	amphipod	*E.estuarius*	water	20, 72 - 88	S (ER50)	Meador, 1993
Cd chloride	amphipod (adult)	*P.affinis*	water	11.4	S	Sundelin, 1983
Cd chloride	amphipod (juvenile)	*P.affinis*	water	15.6	S	Sundelin, 1983
Cd chloride	amphipod	*P.affinis*	water	3	R (egg devt)	Sundelin, 1983
Cd chloride	amphipod - FW	*H.azteca*	water (w/sed)	17.2, 19.6	S (ER50)	Borgmann *et al*.,1991
Cd chloride	amphipod	*A.compressa*	water	36, 60	S (ER57), G (ER70)	Ahsanullah and Williams, 1991
Cd chloride	grass shrimp	*P.pugio*	water (5 ppt)	8	G	Vernberg *et al*.,1977
Cd chloride	crayfish	*C.latimanus*	water	22	S	Thorp *et al*.,1979
Cd chloride	crayfish	*O.virilis*	water	5.7 - 11.2	S (ER25 -74)	Mirenda, 1986
Cadmium	R.trout	*O.mykiss*	water (hard)	0.7	S(ER80)	Pascoe *et al*.,1986
Cadmium	R.trout	*O.mykiss*	water	0.96	G	Kumada *et al*.,1973
Cadmium	R.trout	*O.mykiss*	diet	1.6	G	Kumada *et al*.,1973
Cd chloride	Bk.trout*	*S.fontinalis*	water	0.25	G (ER73)	Benoit *et al*.,1975
Cd chloride	Bk.trout	*S.fontinalis*	water	0.41	S (ER50)	Hamilton *et al*.,1987
Cd chloride	Lgmouth bass *	*M.salmoides*	sediment	12	S	Francis *et al*., 1984
Cd chloride	Flagfish	*J.floridae*	water	6	S	Spehar *et al*.,1978
Cd chloride	Flagfish	*J.floridae*	water	4 -15.6	S, G	Spehar, 1976
Cd chloride	Flagfish	*J.floridae*	water	2.0 - 8	R (ER51 - ??)	Spehar, 1976
Cd chloride	Guppy	*P.reticulata*	diet	0.8 -12	S (ER16)	Hatakeyama and Yasuno, 1982
Cadmium	Stickleback	*G.aculeatus*	injection	2.94	S (ER27)	Woodworth and Pascoe, 1983
Cadmium	Stickleback	*G.aculeatus*	water	0.9	S (ER80)	Pascoe and Mattey, 1977
Cd chloride	Seabass	*L.calcarifer*	water	20.4	S (ER50)	Shazili, 1995
Cd sulfate	bluegill	*L.macrochirus*	water	0.35	S (ER50)	Cearley and Coleman, 1974
Cadmium	Spot (larvae)	*L.xanthurus*	water	8.0 - 69	S (ER50)	Middaugh *et al*.,1975

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

[2]USACE (1998) was reviewed and revealed no effective residues lower than those in Jarvinen and Ankley (1999).

* Exposure was initiated during egg stage

Table A-1 (b). Non-Effective Whole Body Residues - Cadmium, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Cd chloride	snail (FW)	*V.georgianus*	water	33	S, G	Tessier and Blais, 1996
Cd chloride	snail (FW)	*Helisoma spp*	water	78, 90	S	Carlson *et al*., 1991
Cd chloride	snail (FW)	*P.integra*	water	10	S	Spehar *et al*.,1978
Cd chloride	blue mussel	*M.edulis*	water	3.7	S	Eertman *et al*.,1996
Cd chloride	blue mussel	*M.edulis*	water	30	G	Poulsen *et al*.,1982
Cd chloride	mussel -SW	*M.galloprovincialis*	water	28.7	S	Pavice and Jarvenpaa, 1974
Cd chloride	oyster	*C.virginica*	water	54	R	Zaroogian and Morrison, 1981
Cd chloride	oyster	*C.virginica*	water	58.4	S	Zaroogian, 1980
Cd chloride	rock oyster	*S.commercialis*	water	25	S	Ward, 1982
Cd chloride	mussel - FW	*E.complanata*	water	4	S	Wang and Evans, 1993
Cd chloride	mussel - FW	*E.complanata*	water	17.6	S, G	Tessier *et al*., 1996
Cd chloride	zebra mussel	*D.polymorpha*	water	100	S	Kraak *et al*.,1994
Cd chloride	zebra mussel	*D.polymorpha*	water	32	S	Mersch *et al*.,1993
Cd chloride	zebra mussel	*D.polymorpha*	water	114	S	Kraak *et al*.,1992
Cd chloride	zebra mussel	*D.polymorpha*	water	4.2	S	Tessier and Blair, 1996
Cd chloride	clam - SW	*C.fluminea*	water	2	S	Baudrimont *et al*., 1997
Cd chloride	clam - SW	*C.fluminea*	water	1.7	S,G	Inza *et al.,* 1997
Cd chloride	clam - SW	*C.fluminea*	sediment	0.7	S,G	Inza *et al.,* 1997
Cd chloride	snail (FW)	*B.hainanensis*	water	>27.6	S	Lam *et al.,* 1997
cadmium	polychaete -SW	*N.arenaceodentata*	water	1.12	G	Jenkins and Sanders, 1986
cadmium	polychaete -SW	*N.arenaceodentata*	water	4.5	R	Jenkins and Mason, 1988
Cd chloride	polychaete -SW	*N.virens*	sediment	63.8	S	Olla *et al*.,1988
Cd chloride	polychaete -SW	*N.caeca*	sediment	7.8	S	Olla *et al*.,1988
Cd chloride	worm - SW	*G.dibranchiata*	sediment	24	S	Olla *et al*.,1988
Cd chloride	oligochaete - FW	*L.variegatus*	water	60	S	Carlson *et al*.,1991
Cd chloride	oligochaete - FW	*L.variegatus*	sediment	138	S	Carlson *et al*.,1991
Cd chloride	oligochaete - FW	*L.variegatus*	sediment	4.6	S	Peterson *et al*., 1996
Cd chloride	brine shrimp	*A.salina*	diet & water	194.4	S	Jennings and Rainbow, 1979
Cd chloride	brine shrimp nauplii	*A.salina*	water	0.36	G	Jayasekara *et al*.,1986
Cd nitrate	cladoceran (FW)	*D.magna*	diet	6.4	S	Ferard *et al*.,1983
Cd nitrate	cladoceran (FW)	*D.magna*	diet	0.27	R	Ferard *et al*.,1983
Cd chloride	cladoceran (FW)	*M.macrocopa*	diet	7.0 -10	S,G,R	Hatakeyama and Yasuno, 1981
Cd chloride	mysid shrimp	*M.bahia*	water	2.4	R	Carr *et al*.,1985
Cd chloride	mysid shrimp	*M.bahia*	water	0.08	G	Carr *et al*.,1985
CdCl hydrate	amphipod - FW	*G.fossarum*	water (hard)	53.4	S	Abel and Barlocher, 1988
Cd chloride	amphipod (adult)	*P.affinis*	water	6	S	Sundelin, 1983
Cd chloride	amphipod (juvenile)	*P.affinis*	water	2.3	S	Sundelin, 1983
Cd chloride	amphipod	*P.affinis*	water	11.4	R (fecundity)	Sundelin, 1983
Cd chloride	amphipod (juvenile)	*P.affinis*	water	15.6	G	Sundelin, 1983
Cd nitrate	amphipod - FW	*H.azteca*	water	0.61	S	Stephenson and Mackie, 1989
Cd chloride	amphipod - FW	*H.azteca*	water (w/sed)	9.4, 12.4	S	Borgmann *et al*.,1991
Cd chloride	grass shrimp	*P.pugio*	water (20 ppt)	1.8	S (ER0-5)	Vernberg et al.,1977
Cd chloride	grass shrimp	*P.pugio*	water (10 ppt)	4.6	G	Vernberg *et al*.,1977
Cd chloride	crayfish	*C.latimanus*	water	22	G	Thorp *et al*.,1979
Cd chloride	crayfish	*C.latimanus*	water	14.9	S	Thorp *et al*.,1979
Cd chloride	crayfish	*O.propinquis*	water	534.4	S	Gillespie *et al*.,1977
Cd chloride	crayfish	*O.virilis*	water	0.9	S	Mirenda, 1986
Cd chloride	Eel	*A.anguilla*	water	0.56	S	Noel-Lambot and Bouquegneau, 1977
Cadmium	R.trout	*O.mykiss*	water	0.96	S	Kumada *et al*.,1973
Cadmium	R.trout	*O.mykiss*	water	0.54	G	Kumada *et al*.,1973
Cadmium	R.trout	*O.mykiss*	diet	0.47	G	Kumada *et al*.,1973
Cd chloride	Bk.trout*	*S.fontinalis*	water	0.13	G	Benoit *et al*.,1976
Cd chloride	Bk.trout	*S.fontinalis*	water	0.75	G	Hamilton *et al*.,1987
Cd nitrate	FW Fish	*L.delineatus*	diet	0.15	S	Ferard *et al*.,1983
Cd chloride	Flagfish	*J.floridae*	water	2.0 -8.0	S, G	Spehar, 1976
Cd chloride	Flagfish	*J.floridae*	water	1.2 - 5	R	Spehar, 1976
Cd chloride	Guppy	*P.reticulata*	diet	0.8 -12	G	Hatakeyama and Yasuno, 1982
Cd chloride	Guppy	*P.reticulata*	diet	0.8	S	Hatakeyama and Yasuno, 1982
Cadmium	Stickleback	*G.aculeatus*	injection	0.11	S	Woodworth and Pascoe, 1983
Cd chloride	Stickleback	*G.aculeatus*	water	0.39	S	Pascoe and Mattey, 1977
Cadmium	Loach	*N.barbatulus*	water	2.3	S	Douben, 1989
Cd sulfate	Loach	*N.barbatulus*	diet	0.29	S	Douben, 1989

Cd chloride	goldfish *	*C.auratus*	sediment	0.92	S	Francis *et al*., 1984
Cd chloride	Lgmouth bass *	*M.salmoides*	sediment	2.95	S	Francis *et al*., 1984
Cd chloride	Seabass	*L.calcarifer*	water	2.5	S	Shazili, 1995
Cd sulfate	bluegill	*L.macrochirus*	water	1.33	S	Cope *et al*.,1994
Cd sulfate	bluegill	*L.macrochirus*	water	0.036	S	Cearley and Coleman, 1974
Cd nitrate	Perch	*P.fluviatilis*	water	0.075	S	Edgren and Notter, 1980
Cadmium	Spot (larvae)	*L.xanthurus*	water	5.4 - 5.8	S	Middaugh *et al*.,1975
Cd chloride	Dace	*T.hakonensis*	water	0.69	S, G	Kumada *et al*.,1973

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

[2]USACE (1998) was reviewed and revealed no effective residues lower than those in Jarvinen and Ankley (1999).

* Exposure was initiated during egg stage

Table A-1 (c). Effective and non-effective organ and egg/embryo residues. Cadmium, ppm

Effective Organ and Egg/Embryo Residues - Cadmium, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Cd chloride	Eel (organs)	*A.japonica*	water	4.0 -35	S	Yang and Chen, 1996
Cd chloride	Herring (embryo)	*C.harengus*	water	38 - 52	S	Westernhagen *et al*.,1974
Cd sulfate	R.trout (organs)	*O.mykiss*	water	0.6 - 16.4	R	Brown *et al*.,1994
Cd chloride	R.trout (liver)	*O.mykiss*	water	0.09 - 0.145	G	Ricard *et al*., 1998
Cd chloride	Atl. Salmon (embryo)	*S.salar*	water	0.56, 4.0	S	Rombough and Garside, 1982
Cd chloride	Atl. Salmon (embryo)	*S.salar*	water	0.12, 0.4	G	Rombough and Garside, 1982
Cd chloride	Bk.trout (organs)	*S.fontinalis*	water	0.94 - 10	S	Benoit *et al*.,1976
Cd chloride	Bk.trout (organs)	*S.fontinalis*	water	1.0 -13	S, G, R	Benoit *et al*.,1976
Cd sulfate	Bullhead (gills)	*L.nebulosus*	water	30 -1000	S	Mount and Stephan, 1967
Cd chloride	Garpike (embryo)	*B.belone*	water	10.0 -28	S	Westernhagen *et al*.,1975
Cadmium	FW fish (organs)	*C.punctatus*	water	59.4 -1224	S (ER50)	Gupta and Rajbanshi, 1988
Cd sulfate	bluegill (gills)	*L.macrochirus*	water	30 -1000	S	Mount and Stephan, 1967
Cd sulfate	bluegill (organs)	*L.macrochirus*	water	4.2 -107.6	S (ER50)	Eaton, 1974
Cd sulfate	Lgmouth bass (organs)	*M.salmoides*	water	0.007 - 0.009	S	Cearley and Coleman, 1974
Cadmium	Spot (larvae)	*L.xanthurus*	water	8.0 - 69	S (ER50)	Middaugh *et al*.,1975
Cd chloride	Mullet (organs)	*M.cephalus*	water	1.5 -252.4	S (ER20)	Thomas *et al*.,1982
Cd chloride	Dab (organs)	*L.limanda*	water	0.03 -1.37	S	Westernhagen *et al.*,1980
Cd chloride	Flounder (egg)	*P.flesus*	water	4.0 - 18	S	Westernhagen and Dethlefsen, 1975
Cd chloride	Plaice (organs)	*P.platessa*	water	0.07 - 4.4	S (ER89)	Westernhagen *et al*.,1980

Non-Effective Organ and Egg/Embryo Residues - Cadmium, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Cd chloride	mussel - organs - FW	*A.cygnea*	water	2.8 - 4.8	S	Cassini *et al*.,1986
Cd chloride	mussel - organs - FW	*U.elongatus*	water	1.7 - 7.0	S	Cassini *et al*.,1986
Cd chloride	clam (organs) -SW	*T.crocea*	water	0.86 - 127.4	S	Duquesne and Coll, 1995
Cd chloride	lobster (organs)	*N.norvegicus*	water	0.58 - 32.5	S	Canli and Furness, 1995
Cd chloride	crayfish (organs)	*P.acutus*	water	1.4 - 40.8	S	Dickson *et al*.,1982
Cd chloride	Eel (organs)	*A.japonica*	water	2.0 - 7.0	S	Yang and Chen, 1996
Cd chloride	Herring (embryo)	*C.harengus*	water	29	S	Westernhagen *et al.*,1974
Cd chloride	R.trout (organs)	*O.mykiss*	water	0.08 - 3.3	S	Handy, 1992
Cd chloride	R.trout (organs)	*O.mykiss*	water	0.83 - 3.0	G	Zelikoff *et al*.,1995
Cd chloride	R.trout (organs)	*O.mykiss*	water	0.02 - 0.38	S	Calamari *et al*.,1982
Cd sulfate	R.trout (organs)	*O.mykiss*	water	2.8 - 66.3	S, G	Brown *et al*.,1994
Cd sulfate	R.trout (organs)	*O.mykiss*	water	0.4 - 1.6	R	Brown *et al*.,1994
Cd sulfate	R.trout (organs)	*O.mykiss*	water	0.02 - 3.54	G	Roberts *et al*.,1979
Cd sulfate	R.trout (organs)	*O.mykiss*	water	3.75 - 7.3	G	Olsson *et al*.,1989
Cd chloride	R.trout (liver)	*O.mykiss*	water	0.07 - 0.16	G	Ricard *et al*., 1998
Cd chloride	Atl. Salmon (embryo)	*S.salar*	water	0.25, 2.0	S	Rombough and Garside, 1982
Cd chloride	Atl. Salmon (embryo)	*S.salar*	water	0.06, 0.3	G	Rombough and Garside, 1982
Cd sulfate	Brown trout (organs)	*S.trutta*	water	0.09 - 55.4	G	Roberts *et al*.,1979
Cd chloride	Bk.trout (organs)	*S.fontinalis*	injection	0.02 - 0.55	S	Sangalanag and Freeman, 1979
Cd chloride	Bk.trout (organs)	*S.fontinalis*	water	0.5 - 3.2	S	Benoit *et al*.,1976
Cd chloride	Lake trout (organs)	*S.namaycush*	water	2.13 - 3.43	G	Kislalioglu *et al*.,1996
Cd sulfate	Bullhead (gills)	*L.nebulosus*	water	0.04 - 26	S	Mount and Stephan, 1967
Cd chloride	Garpike (embryo)	*B.belone*	water	7.0 - 19	S	Westernhagen *et al*.,1975
Cadmium	FW fish (organs)	*C.punctatus*	water	49.2 - 955	S (ER0-5)	Gupta and Rajbanshi, 1988
Cd sulfate	bluegill (gills)	*L.macrochirus*	water	0.04 - 26	S	Mount and Stephan, 1967
Cd sulfate	bluegill (organs)	*L.macrochirus*	water	6.8 - 40.2	S	Eaton, 1974

Cd sulfate	Lgmouth bass (organs)	*M.salmoides*	water	0.007 - 0.009	G	Cearley and Coleman, 1974
Cd sulfate	Lgmouth bass (organs)	*M.salmoides*	water	0.003 -0.005	S	Cearley and Coleman, 1974
Cd chloride	Lgmouth bass (organs)	*M.salmoides*	water	0.09 - 1.49	G	Melgar *et al*., 1997
Cadmium	Tilapia (organs)	*O.mossambicus*	water	0.01 - 3.11	S, G	Pelgrom *et al*.,1995
Cd chloride	Tilapia (organs)	*T.aureus*	water	0.1 - 41.6	S	Allen, 1995
Cd chloride	Mullet (organs)	*M.cephalus*	water	0.16 - 1.44	S	Thomas *et al*.,1982
Cd chloride	Dab (organs)	*L.limanda*	water	0.03 -0.59	S	Westernhagen *et al.*,1980
Cd chloride	Flounder (liver)	*P.americanus*	implant	17.5	S	Jessen-Eller and Crivello, 1998
Cd chloride	Flounder (egg)	*P.flesus*	water	2.0 - 6	S	Westernhagen *et al*.,1975
Cd chloride	Flounder (egg)	*P.flesus*	water	8.0 - 18	G	Westernhagen *et al*.,1975
Cd chloride	Carp (organs)	*C.carpio*	water	1.8 - 60	S	Cinier *et al*.,1997
Cd chloride	Plaice (organs)	*P.platessa*	water	0.02 - 0.34	S	Westernhagen *et al*., 1980

[1]S, R, G and D refer to survival, reproduction and growth effects, respectively.

[2]USACE (1998) was reviewed and revealed no effective residues lower than those in Jarvinen and Ankley (1999).

Table A-2 (a). Effective Whole Body Residues - Mercury, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Hg chloride	limpet	*C.fornicata*	water	22 - 48	G (ER30)	Thain, 1984
Hg chloride	limpet	*C.fornicata*	water	10.0 - 17	R (ER20)	Thain, 1984
Hg chloride	limpet	*C.fornicata*	water	4.0 - 8.0	D	Thain, 1984
Hg acetate	oyster	*C.virginica*	water	140.7	S (ER65)	Cunningham and Trip, 1973
Hg chloride	cladoceran (FW)	*D.magna*	water	4.66	R	Biesinger *et al*.,1982
Meth-Hg-Chl	cladoceran (FW)	*D.magna*	water	3.28	R	Biesinger *et al*.,1982
Hg chloride	Eel	*A.anguilla*	water	15.3	S (ER25)	Noel-Lambot and Bouquegneau, 1977
Hg chloride	goldfish	*C.auratus*	water	5.6, 7.0	S (ER80, ?)	Heisinger *et al*.,1979
Hg chloride	Fathead minnow	*P.promelas*	water	4.2	S	Snarski and Olson, 1982
Hg chloride	Fathead minnow	*P.promelas*	water	1.3, 1.4, 4.8	G	Snarski and Olson, 1982
Hg chloride	Fathead minnow	*P.promelas*	water	4.47	R	Snarski and Olson, 1982
Hg chloride	Channel catfish	*I.punctatus*	water	0.34	S (ER49)	Birge *et al*.,1979
Meth-Hg-Chl	R.trout	*O.mykiss*	injection	5.1	S (ER50)	Hawryshyn and Mackay, 1979
Meth-Hg-Chl	R.trout	*O.mykiss*	water	11.2	S	Niimi and Kissoon, 1994
Meth-Hg-Chl	R.trout	*O.mykiss*	oral	12.2	S	Miettinen *et al*., 1970
Meth-Hg-Chl	R.trout	*O.mykiss*	diet	7.6 , 11, 35	G	Rodgers and Beamish, 1982
Meth-Hg-Chl	Bk.trout	*S.fontinalis*	water	9.4	S, G, R	McKim *et al*.,1976
Meth-Hg-OH	Pike	*E.lucius*	oral	12.5, 23.2	S	Miettinen *et al*., 1970
Meth-Hg-Chl	Bluegill	*L.macrochirus*	water (21°C)	10.7	S	Cember *et al*.,1978

Table A-2 (b). Non-Effective Whole Body Residues - Mercury, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Hg chloride	limpet (SW)	*C.fornicata*	water	10.0 - 17	G	Thain, 1984
Hg chloride	limpet (SW)	*C.fornicata*	water	4.0 - 8.0	R	Thain, 1984
Hg nitrate	snail (FW)	*V.georgianus*	water	6	S, G	Tessier *et al*.,1996
Hg acetate	oyster	*C.virginica*	water	28	S	Cunningham and Trip, 1973
Hg nitrate	mussel (FW)	*E.complanata*	water	3	S, G	Tessier *et al*.,1996
MethylHg	mussel (FW)	*P.grandis*	water	0.045	S, G, R	Malley *et al*.,1996
Hg chloride	clam - SW	*C.fluminea*	water	8	S	Baudrimont *et al*., 1997
Hg chloride	clam - SW	*C.fluminea*	water	1.75	S, G	Inza *et al*., 1997
Hg chloride	clam - SW	*C.fluminea*	sediment	0.125	S, G	Inza *et al*., 1997
Meth-Hg-Chl	clam - SW	*C.fluminea*	water	6.3	S, G	Inza *et al*. 1997
Meth-Hg-Chl	clam - SW	*C.fluminea*	sediment	0.48	S, G	Inza *et al*., 1997
Hg chloride	cladoceran (FW)	*D.magna*	water	4.66	S	Biesinger *et al*.,1982
Hg chloride	cladoceran (FW)	*D.magna*	water	3.05	R	Biesinger *et al*.,1982
Hg chloride	grass shrimp	*P.pugio*	water	1.1 - 2.1	S	Barthalmus, 1977
Meth-Hg-Chl	cladoceran (FW)	*D.magna*	water	36.8	S	Biesinger *et al*.,1982
Meth-Hg-Chl	cladoceran (FW)	*D.magna*	water/diet	5	S, R	Lock,1975
Hg chloride	goldfish	*C.auratus*	water	6.1	S	Heisinger *et al.,* 1979
Hg chloride	Fathead minnow	*P.promelas*	water	2.8, 7.6	S	Snarski and Olson, 1982
Hg chloride	Fathead minnow	*P.promelas*	water	0.8, 2.64	G	Snarski and Olson, 1982
Hg chloride	Fathead minnow	*P.promelas*	water	2.8	R	Snarski and Olson, 1982
Hg chloride	Guppy	*P.reticulata*	water	0.2	S	Scherer *et al*.,1975
Hg chloride	Fish - FW	*G.affinis*	water	5.4	S	Kania and O'Hara, 1974
Meth-Hg-Chl	R.trout	*O.mykiss*	injection	1.91	S	Hawryshyn and Mackay, 1979
Meth-Hg-Chl	R.trout	*O.mykiss*	water (pH ranged)	0.05 - 0.09	S	Ponce and Bloom, 1991
Meth-Hg-Chl	R.trout	*O.mykiss*	oral	11	S	Miettinen *et al*., 1970
Meth-Hg-Chl	R.trout	*O.mykiss*	diet	7.6 - 35	S	Rodgers and Beamish, 1982
Meth-Hg-Chl	R.trout	*O.mykiss*	diet	29	G	Rodgers and Beamish, 1982
Meth-Hg-Chl	R.trout	*O.mykiss*	water	12	S	Niimi and Lowe-Jinde, 1984
Meth-Hg-Chl	R.trout	*O.mykiss*	water/diet	10.4, 5	S, G	Lock, 1975
Meth-Hg-Chl	R.trout	*O.mykiss*	water	2.3 - 8.6	G	Phillips and Buhler, 1978
Meth-Hg-Chl	Bk.trout	*S.fontinalis*	water	3.4	S, G, R	McKim *et al*.,1976
Meth-Hg-Chl	Fathead minnow	*P.promelas*	water	10.9	S, G	Olson *et al*.,1975

[1]S, R, G and D refer to survival, reproduction and growth effects, respectively.
[2]USACE (1998) was reviewed and revealed no effective residues lower than those in Jarvinen and Ankley (1999).

Table A-2 (c). Effective and non-effective organ and egg/embryo residues. Mercury, ppm

Effective Organ and Egg/Embryo Residues - Mercury, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Hg chloride	crab (organs)	*C.maenas*	water (salt)	1.4 - 70	S (ER50)	Bianchini and Gilles, 1996
Hg chloride	crab (organs)	*C.pagurus*	water (salt)	1.6 - 110	S	Bianchini and Gilles, 1996
Hg chloride	crab (FW - organs)	*E.sinensis*	water (salt)	0.5 - 22	S	Bianchini and Gilles, 1996
Hg chloride	zooplankton (cytoplasm)		field/water	0.04	G	Brown and Parsons, 1978
Hg chloride	Salmon (liver)	*O.keta*	field/water	0.96	G	Brown and Parsons, 1978
Hg chloride	Salmon (muscle)	*O.keta*	field/water	5.8	G	Koeller and Wallace, 1977
Hg chloride	R.trout (organs)	*O.mykiss*	water	0.7 - 117	S	Niimi and Kissoon, 1994
Hg chloride	R.trout (egg)	*O.mykiss*	water	96.8	S	Birge *et al*.,1979
Hg chloride	R.trout (edible flesh)	*O.mykiss*	water	2.4 - 2.9	S (ER10)	MacLeod and Pessah, 1973
Hg chloride	R.trout (organs)	*O.mykiss*	water	7-448, 8-271	S	Niimi and Kissoon, 1994
Hg chloride	R.trout (embryos)	*O.mykiss*	water	0.04, 0.27, 0.9	S (ER55, 77, ?)	Birge *et al*.,1979
Hg chloride	Medaka (embryo)	*O.latipes*	water	29 - 56	S (ER80-100)	Heisinger and Green, 1975
Mercury	Walleye (organs)	*S.vitreum*	diet	4 -232	S	Scherer *et al*.,1975
Hg nitrate	Fish (organs - FW)	*A.scandens*	water	2.8 - 3.0	G, D (Blinding)	Panigrahi and Misra, 1978
Meth-Hg-Chl	R.trout (pooled organs)	*O.mykiss*	water	6.9 - 7.9	S	Wobeser, 1975
Meth-Hg-Chl	R.trout (muscle)	*O.mykiss*	diet	19 -30	G	Wobeser, 1975
Meth-Hg-Chl	Bk.trout (organs)	*S.fontinalis*	water	23.5 - 200	S (ER88), R (ER100)	McKim *et al*.,1976
Meth-Hg-Chl	Bk.trout (embryo)	*S.fontinalis*	water	2.2	S	McKim *et al*.,1976

Non-Effective Organ and Egg/Embryo Residues - Mercury, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Hg chloride	mussel (organs - SW)	*M.galloprovincialis*	water	23.2 - 212.6	S	Pagliarani *et al*.,1996
Hg chloride	Lobster (organs)	*N.norvegicus*	water	0.34 - 46.5	S	Canli and Furness, 1995
Meth-Hg-Chl	Lobster (organs)	*N.norvegicus*	water	1.8 - 38	S	Canli and Furness, 1995
Hg chloride	zooplankton (cytoplasm)		field/water	0.02	G	Brown and Parsons, 1978
Hg chloride	Salmon (liver)	*O.keta*	field/water	0.17	G	Brown and Parsons, 1978
Hg chloride	Salmon (muscle)	*O.keta*	field/water	0.5	G	Koeller and Wallace, 1977
Hg chloride	Salmon (muscle)	*O.keta*	field/water	5.8	S	Koeller and Wallace, 1977
Hg chloride	R.trout (edible flesh)	*O.mykiss*	water	1.2 - 1.6	S	MacLeod and Pessah, 1973
Hg chloride	R.trout (organs)	*O.mykiss*	water	60	S, G, R	Farmer *et al*., 1979
Hg chloride	Carp (organs)	*C.carpio*	water	0.28 - 4.44	G	Yediler and Jacobs, 1995
Hg chloride	Medaka (embryo)	*O.latipes*	water	16	S	Heisinger and Green, 1975
Hg nitrate	Fish (organs - FW)	*A.scandens*	water	2.8 - 3.0	S	Panigrahi and Misra, 1978
Meth-Hg-Chl	R.trout (pooled organs)	*O.mykiss*	water	4.8	S	Wobeser, 1975
Meth-Hg-Chl	R.trout (muscle)	*O.mykiss*	diet	30	S	Wobeser, 1975
Meth-Hg-Chl	R.trout (muscle)	*O.mykiss*	diet	12.5	G	Wobeser, 1975
Meth-Hg-Chl	Bk.trout (organs)	*S.fontinalis*	water	9.2 - 41.8	S, R	McKim *et al*., 1976
Meth-Hg-Chl	Channel Catfish (blood)	*I.punctatus*	oral	0.106	S	McCloskey *et al*., 1998

[1]S, R, G and D refer to survival, reproduction and growth effects, respectively.

[2]USACE (1998) was reviewed and revealed no effective residues lower than those in Jarvinen and Ankley (1999).

Table A-3 (a). Effective Whole Body Residues - Arsenic, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Arsenic	snail - FW	*H.campanulata*	combined	4.2	S (ER16)	Spehar *et al*.,1980
Arsenic	cladoceran	*D.magna*	water	87	S (ER50)	Enserink *et al*.,1991
Arsenic	cladoceran	*D.magna*	water	33	R (ER10)	Enserink *et al*.,1991
Na arsenate	R.trout	*O.mykiss*	water (15°C)	8.1 - 8.6	S (ER50)	McGeachy and Dixon, 1992
multiple As spp	R.trout	*O.mykiss*	diet	11.2, 17.9	S (ER<20)	Cockell and Hilton, 1988
Na arsenite	Flounder*	*P.americanus*	implant	12.2*	S (ER50)	Eller-Jessen and Crivello, 1998
Na arsenate	Sunfish	*L.cyanellus*	water	6.7	S (ER50)	Sorensen, 1976
Na arsenite	Bluegill (adult)	*L.macrochirus*	field/water	11.6	G	Gilderhus, 1966
Na arsenite	Bluegill (juvenile)	*L.macrochirus*	field/water	5.6	G (ER54)	Gilderhus, 1966
Na arsenite	Bluegill (juvenile)	*L.macrochirus*	field/water	5.6	S (ER50)	Gilderhus, 1966

*calculated from reported As residues in liver (34 ppm) and body remainder (12 ppm) and reported LSI (liver-somatic index)

Table A-3 (b). Non-Effective Whole Body Residues - Arsenic, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
Arsenic	snail - FW	*H.campanulata*	combined	4.0 - 16	S	Spehar *et al*., 1980
Arsenic	snail - FW	*S.emarginata*	combined	3.6	S	Spehar *et al*., 1980
Arsenic	cladoceran	*D.magna*	water	3.8 - 9.8	S	Spehar *et al*., 1980
Na arsenate	grass shrimp	*P.pugio*	water & diet	1.28	G	Linday and Sanders, 1990
Arsenic	stonefly	*P.dorsata*	combined	6 - 8.4	S	Spehar *et al*., 1980
arsenic (MSMA)	crayfish	*P.clarkii*	water	2.8 - 9.0	S	Naqvi *et al*., 1990
Na arsenate	R.trout	*O.mykiss*	water (15°C)	2.0 - 3.4	S, G	McGeachy and Dixon, 1990
Na arsenite	Flounder*	*P.americanus*	implant	1.1*	S	Eller-Jessen and Crivello, 1998
Na arsenite	Bluegill (adult)	*L.macrochirus*	water	5.5	G	Gilderhus, 1966
Na arsenite	Bluegill (juvenile)	*L.macrochirus*	field/water	1.8	S,G	Gilderhus, 1966
Arsenic	Bluegill	*L.macrochirus*	field/water	0.52	S	Barrows *et al*.,1980

*estimated from reported liver residue by applying whole body:liver ratio derived from ER50 data, above (i.e. 1.0 : 0.359)

Table A-3 (c). Effective and non-effective organ and egg/embryo residues. Arsenic, ppm

Effective Organ and Egg/Embryo Residues - Arsenic, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Na arsenate	Sunfish (organs)	*L.cyanellus*	water (20-30 °C)	8 - 212	S	Sorensen, 1976
Na arsenite	Flounder (liver)	*P.americanus*	implant	34	S (ER50)	Eller-Jessen and Crivello, 1998
Na arsenite	Flounder (less liver)	*P.americanus*	implant	12	S (ER50)	Eller-Jessen and Crivello, 1998

Non-Effective Organ and Egg/Embryo Residues - Arsenic, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference[2]
multiple As Spp	R.trout (carcass)	*O.mykiss*	water	6.1, 6.9, 11.4	S, G	Cockell and Hilton, 1988
Arsenic trioxide	R.trout (carcass)	*O.mykiss*	water	8.8	S	Cockell and Hilton, 1988
Arsenic trioxide	R.trout (carcass)	*O.mykiss*	water	0.9	G	Cockell and Hilton, 1988
Arsenic trioxide	R.trout (carcass)	*O.mykiss*	water	2.6 - 4.7	S, G	Dixon and Sprague, 1981
diNa arsenate	R.trout (carcass)	*O.mykiss*	diet	9.1	G	Cockell and Hilton, 1988
Na arsenite	Flounder (liver)	*P.americanus*	implant	2.5	S	Jessen-Eller and Crivello, 1998
Na arsenite	Flounder (liver)	*P.americanus*	implant	3	S	Eller-Jessen and Crivello, 1998

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-4 (a). Effective Whole Body Residues - Silver, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Ag nitrate	gastropod	*C.fornicata*	water	7.53	R (ER49)	Nelson *et al*.,1983
silver	clam - SW	*C.fluminea*	water	2510	S	Diamond *et al*.,1990
silver	clam - SW	*C.fluminea*	water	1650	G	Diamond *et al*.,1990
silver	copepod - SW	*Acartia* spp	diet	0.37	S (ER40)	Hook & Fisher (submitted)
silver	copepod - SW	*Acartia* spp	diet	0.14	R (ER50)	Hook & Fisher (submitted)
silver	cladoceran - FW	*C. dubia/Simoceph.*	diet	0.06	R (ER50)	Hook & Fisher (submitted)
silver	cladoceran - FW	*C. dubia/Simoceph.*	water	0.07	R (ER25)	Hook & Fisher (submitted)
Ag nitrate	sculpin	*O.maculosus*	water (32 psu)	1.3	S (ER50)	Shaw *et al*., 1998

Table A-4 (b). Non-effective Whole Body Residues - Silver, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Ag nitrate	gastropod	*C.fornicata*	water	5.36	R	Nelson *et al*.,1983
silver	clam - SW	*C.fluminea*	water	1650	S	Diamond *et al*., 1990
silver	clam - SW	*C.fluminea*	water	800	G	Diamond *et al*., 1990
Ag sulfide	oligochaete - FW	*L. variegatus*	sediment	16.1	S, G, R	Hirsch, 1998
silver	cladoceran - SW	*Acartia* spp.	water	0.28	S	Hook & Fisher (submitted)
Ag nitrate	sculpin	*O.maculosus*	water	0.54	S	Shaw *et al*., 1998
Ag nitrate	Bluegill	*L.macrochirus*	water	0.06	S, G	Coleman and Cearley, 1974

Table A-4 (c). Effective and non-effective organ and egg/embryo residues. Silver, ppm

Effective Organ and Egg/Embryo Residues - Silver, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Ag nitrate	R trout (liver)	*O.mykiss*	water	25 - 40	S (ER43)	Galvez *et al.,* 1998
Ag nitrate	R trout (liver)	*O.mykiss*	water (brack)	19.0 - 21.1	S (ER50)	Galvez and Wood, 1997
Ag nitrate	R trout (organs)	*O.mykiss*	water	0.3 - 34.1	S (ER5)	Wood *et al*.,1996
Non-effective Organ and Egg/Embryo Residues - Silver, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Ag sulfide	R.trout (organs)	*O.mykiss*	diet	0.05 - 2.47	G, behav	Galvez and Wood, 1999
Ag nitrate	R trout (liver)	*O.mykiss*	water	0.6	S	Galvez *et al.,* 1998
Ag nitrate	R. trout (liver/gill)	*O.mykiss*	water (brack)	200/200	S	Hogstrand *et al.,* 1996
Ag nitrate	R trout (liver)	*O.mykiss*	water (brack)	1.89	S	Galvez and Wood, 1997
Ag nitrate	R trout (organs)	*O.mykiss*	water	0.07 - 10.5	S	Wood *et al*., 1996a
Ag thiosulf	R trout (organs)	*O.mykiss*	water	0.88 - 73.2	S	Wood *et al*., 1996b
Ag nitrate	Lgmth Bass (organs)	*M.salmoides*	water	0.003 - 0.12	S, G	Coleman and Cearley, 1974

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-5 (a). Effective Whole Body Residues - Copper, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Cu sulfate	blue mussel	*M.edulis*	water + sed	22.1	S (ER80)	Kaitala, 1988
copper	blue mussel	*M.edulis*	water	67.4	S (ER50)	Hvilsom, 1983
copper	blue mussel	*M.edulis*	water	10.8	S (ER100)	Martin, 1979
Cu chloride	bivalve - SW	*M.balthica*	water+sed	22	S (ER87)	Kaitala, 1988
Cu chloride	bivalve - SW	*M.balthica*	water	23.2	S (ER32)	Absil *et al*.,1996
copper	clam - SW	*M.casta*	water	201	S (ER50)	Kumaraguru *et al*., 1980
Cu sulfate	snail - FW	*L.praerosa*	water/field	27.3, 16.3	S (ER15, 70)	Reed-Judkins *et al*., 1997
Cu sulfate	worm-SW	*P.maculata*	water	48.9 - 87.5	S	McLusky and Phillips, 1975
Copper	worm-SW	*N.arenaceodentata*	water	199 - 293	S	Pesch, 1979
Cu sulfate	worm-SW	*C.spirabrancha*	water	10.7	S (ER50)	Milanovich *et al*.,1976
Cu sulfate	worm-SW	*C.spirabrancha*	water	12.8	G	Milanovich *et al*.,1976
Cu chloride	worm-SW	*N.diversicolor*	water (20°C)	46	S (ER50)	Fernandez & Jones, 1990
Cu sulfate	amphipod - FW	*H. azteca*	sediment	32.8	S (ER55)	Borgmann and Norwood, 1997
Cu sulfate	amphipod - FW	*H. azteca*	sediment	29.2	G	Borgmann and Norwood, 1997
Cu sulfate	amphipod - SW	*A. compressa*	water	100	G	Ahsanullah and Williams, 1991
copper	amphipod	*C.volutator*	water	18.4	R (ER54)	Ericksson and Weeks, 1994
copper	cladoceran	*D.magna*	water	68	S (ER50)	Enserink *et al*., 1991
copper	cladoceran	*D.magna*	water	5.8	R (ER10)	Enserink *et al*., 1991
copper	R.trout	*O.mykiss*	water	2.22	S (ER57)	Handy, 1992
Cu chloride	R.trout	*O.mykiss*	water	1.4	G (ER27)	Marr *et al*.,1996
copper	R.trout	*O.mykiss*	combined	4.5 - 5.5	S (ER30)	Mount *et al*.,1994

Table A-5 (b). Non-Effective Whole Body Residues - Copper, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Cu chloride	bivalve - SW	*M.balthica*	sediment	14.4	S	Absil *et al*.,1996
Cu sulfate	zebra mussel	*D.polymorpha*	water	32	S	Kraak *et al*.,1994
Cu sulfate	zebra mussel	*D.polymorpha*	water	30-43	S	Mersch *et al*.,1996
Cu sulfate	zebra mussel	*D.polymorpha*	water	40-80	S	Kraak *et al*.,1992
Cu sulfate	zebra mussel	*D.polymorpha*	water	20	S	Mersch *et al*.,1993
Cu sulfate	clam-SW	*P.staminea*	water	9.3	S	Roesijadi, 1980
Cu sulfate	snail - FW	*L.praerosa*	water/field	25	S	Reed-Judkins *et al*., 1997
Cu sulfate	worm-SW	*P.maculata*	water	95.5	S	McLusky and Phillips, 1975
Copper	worm-SW	*N.arenaceodentata*	water	6.32	S	Pesch, 1979
Cu sulfate	worm-SW	*C.spirabrancha*	water	6.42	S	Milanovich *et al*.,1976
Cu sulfate	worm-SW	*C.spirabrancha*	water	12.9	G	Milanovich *et al*.,1976
Cu sulfate	amphipod - FW	*H.azteca*	sediment	30.6	S, G	Borgmann and Norwood, 1997
Cu sulfate	crayfish	*O.virilis*	water	50	S	Evans, 1980
copper	crayfish	*O.rusticus*	water	2.0 - 26	S	Evans, 1980
copper	amphipod	*C.volutator*	water	15.2	R	Ericksson and Weeks, 1994
copper	amphipod	*C.volutator*	water	15.2 - 23.8	S	Ericksson and Weeks, 1994
Cu sulfate	R.trout	*O.mykiss*	water	1.04	S (ER6)	Handy, 1992
Copper	R.trout	*O.mykiss*	diet	3.4	S	Handy, 1992
Cu chloride	R.trout	*O.mykiss*	water	0.74 - 0.97	G	Marr *et al*., 1996
copper	R.trout	*O.mykiss*	combined	0.7 - 7.2	S	Mount *et al*., 1994
Cu nitrate	Carp (larvae)	*C.carpio*	water	7.4	S	Stouthart *et al*.,1996

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-5 (c). Effective and non-effective organ and egg/embryo residues. Copper, ppm

Effective Organ and Egg/Embryo Residues - Copper, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Cu sulfate	blue mussel (organ)	*M.edulis*	water	7.95 - 9.01	S	Harrison and Berger,1982
Cu sulfate	clam-SW (gill)	*P.staminea*	water	31.2	S (ER86)	Roesijadi, 1980
Cu chloride	crab (hemolymph)	*C.maenas*	water	52.5	S (ER10)	Rtal *et al*., 1996
Cu sulfate	Cod (organs)	*G.morhua*	water	0.76 - 7.0	S (ER59)	Larsen *et al*., 1997
Cu acetate	Coho salmon (organs)	*O.kisutch*	water	1.1 - 36	G	Buckley *et al*.,1982
Cu sulfate	R.trout (organs)	*O.mykiss*	water	0.5 - 72	S (ER63)	Handy, 1992
Cu sulfate	R.trout (organs)	*O.mykiss*	diet	3.4 - 637	S, G	Lanno *et al*.,1985
Cu sulfate	R.trout (liver)	*O.mykiss*	water	82 - 119	S	Dethloff and Bailey, 1998
Cu sulfate	Bullhead (organs)	*I.nebulosus*	water	1.54 - 6.8	S	Brungs *et al*.,1973
Cu sulfate	Channel Catfish (organs)	*I.punctatus*	diet	0.3 -1.84	G	Murai *et al*.,1981
Cu sulf pentahyd	Channel Catfish (liver)	*I.punctatus*	water	21.5	S	Griffin *et al*., 1997
Cu sulfate	Stone loach (organs)	*N.barbatulus*	water	0.8 - 97.8	S (ER80-100)	Solbe and Cooper, 1976
Cu sulfate	Bluegill (organs)	*L.macrochirus*	water	2.6 - 96	S	Benoit, 1975
Cu sulfate	Sheepshead (organs)	*A.probatocephalus*	water	0.1 -16.3	S	Cardeilhac *et al*.,1979
Cu sulfate	Tilapia (organs)	*O.mossambicus*	water	1078 - 2100	S (ER15)	James *et al*., 1998
	Carp (organs)	*C. carpio*	water	0.49 - 50.3	G	DeBoeck *et al*., 1997
Cu nitrate	Carp (egg)	*C.carpio*	water	42	S	Stouthart *et al*.,1996
Cu nitrate	Carp (egg-larvae)	*C.carpio*	water	11.1, 11.7	S	Stouthart *et al*.,1996

Non-Effective Organ and Egg/Embryo Residues - Copper, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Cu sulfate	blue mussel (organ)	*M. edulis*	water	2.74	S	Harrison and Berger, 1982
Cu sulfate	oligochaete - SW (sections)	*T. tubifex*	water	120 - 200	Physiology	Lucan-Bouche *et al*., 1999
Cu chloride	crab (hemolymph)	*C. maenas*	water	12.6	S	Rtal *et al*., 1996
Cu sulfate	Cod (organs)	*G.morhua*	water	0.34 - 5.4	S	Larsen *et al*., 1997
Cu chloride	R.trout (organs)	*O.mykiss*	water	0.6 - 47	S	Grosell *et al*., 1997
Cu sulfate	R.trout (organs)	*O.mykiss*	water	0.5 - 100	S	Handy, 1992
Copper	R.trout (liver)	*O.mykiss*	water	70 - 231	S	Olsson *et al*.,1987
Copper	R.trout (liver)	*O.mykiss*	diet	4.4 - 329	S, G	Lanno *et al*.,1985
Cu sulfate	R.trout (liver)	*O.mykiss*	water	82 - 119	G	Dethloff & Bailey, 1998
Cu sulfate	R.trout (liver)	*O.mykiss*	water	64 - 102	S	Dethloff & Bailey, 1998
Cu sulfate	R.trout (organs)	*O.mykiss*	water	1.34 - 47.8	S, G, R	McKim and Benoit, 1971
Cu chloride	Roach (organs)	*R.rutilis*	water	4.0 -7.6	S	Segner, 1987
Cu sulfate	Bullhead (organs)	*I.nebulosus*	water	2.0 -6.6	S	Brungs *et al*.,1973
Cu sulfate	Channel Catfish (organs)	*I.punctatus*	diet	0.28 -1.0	G	Murai *et al*.,1981
Cu sulf pentahyd	Channel Catfish (liver)	*I.punctatus*	water	15.8	S	Griffin *et al*., 1997
Cu sulf pentahyd	Channel Catfish (liver)	*I.punctatus*	water	9.9 - 34	S	Perkins *et al*., 1997
Cu sulf pentahyd	Channel Catfish (liver)	*I.punctatus*	water	4.8 - 20	G	Perkins *et al*., 1997
Cu sulfate	Stone loach (organs)	*N.barbatulus*	water	0.7 - 15.4	S	Solbe and Cooper, 1976
Cu sulfate	Bluegill (organs)	*L.macrochirus*	water	1.2 - 11.4	S	Benoit, 1975
Copper	Tilapia (organs)	*O.mossambicus*	water	0.9 -23.9	S	Pelgrom *et al*.,1995
Cu sulfate	R.trout (egg)	*O.mykiss*	water	1.4	S, G, R	McKim and Benoit, 1971
Cu sulfate	R.trout (embryo)	*O.mykiss*	water	5.6	S	McKim and Benoit, 1971
Cu sulfate	Carp (organs)	*C. carpio*	water	0.46 - 29.3	G	DeBoeck *et al*., 1997
Cu nitrate	Carp (egg)	*C.carpio*	water	10.6 - 34.3	S	Stouthart *et al*.,1996
Cu nitrate	Carp (egg-larvae)	*C.carpio*	water	7.4	S	Stouthart *et al*.,1996

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-6 (a). Effective Whole Body Residues - Lead, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Pb nitrate	zebra mussel	*D.polymorpha*	water	200	S,G	Kraak *et al*., 1994
Pb nitrate	amphipod - FW	*G.pseudolimnaeus*	water	98	S (ER50)	Spehar *et al*., 1978
Lead	cladoceran	*D.magna*	water	5040	S (ER50)	Enserink *et al*., 1991
Lead	amphipod - FW	*H.azteca*	water	70 - 110	S (ER50)	Maclean *et al*.,1993
Lead	amphipod - FW	*H.azteca* (young)	water	14.5	S (ER50)	MacLean *et al*., 1996
Lead	amphipod - FW	*H.azteca* (juvenile)	water	19.1	S (ER50)	MacLean *et al*., 1996
Lead	amphipod - FW	*H.azteca* (adult)	water	30.2	S (ER50)	MacLean *et al*., 1996
Pb nitrate	Bk.trout	*S.fontinalis*	water	4.0 - 8.8	G (ER57)	Holcombe *et al*.,1976
Lead	fathead	*P.promelas*	water	44.2	Enzyme Ind.	Weber *et al*.,1991

Table A-6 (b). Non-Effective Whole Body Residues - Lead, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Pb nitrate	snail	*P.integra*	water	200	S	Spehar *et al*.,1978
Pb nitrate	oyster	*C.virginica*	water	2.3 - 2.6	G,S,R	Zaroogian *et al*.,1979
Pb nitrate	zebra mussel	*D.polymorpha*	water	36	S,G	Kraak *et al*.,1994
Pb nitrate	zebra mussel	*D.polymorpha*	water	40	S,G	Bleeker *et al*.,1992
Lead	zebra mussel	*D.polymorpha*	water	2.0 - 30	S	Kraak *et al*.,1994
Pb acetate	clam - SW	*M.mercenaria*	water	8.44 - 10.4	S	Alcutt and Pinto, 1994
Pb nitrate	oyster	*C.gigas*	water	270.2	G	Almeida *et al*., 1998
Lead	amphipod	*P.affinis*	combined	4	S	Sundelin,1984
Lead	barnacle	*B.crenatus*	combined	90	Enzyme Ind.	Powell and White, 1990
Lead	amphipod - FW	*H.azteca* (young)	water	9.7	S	MacLean *et al*., 1996
Lead	amphipod - FW	*H.azteca* (juvenile)	water	9	S	MacLean *et al*., 1996
Lead	amphipod - FW	*H.azteca* (adult)	water	16.6	S	MacLean *et al*., 1996
Pb nitrate	Bk.trout	*S.fontinalis*	water	2.5 - 5.1	G	Holcombe *et al*.,1976
Pb nitrate	R.trout	*O.mykiss*	diet	1.3 - 2.6	S, G	Mount *et al*.,1994
Lead	fathead	*P.promelas*	water	26.2	Enzyme Ind.	Weber *et al*.,1991

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-6 (c). Effective and non-effective organ and egg/embryo residues. Lead, ppm

Effective Organ and Egg/Embryo Residues - Lead, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Lead	SW Catfish (organs)	*M. gulio (juvenile)*	water	0.37 - 7.6	G	Kasthuri & Chandran, 1997
Lead	SW Catfish (organs)	*M. gulio (adult)*	water	0.44 - 22.3	G	Kasthuri & Chandran, 1997
Pb nitrate	Bk.trout (organs)	*S.fontinalis*	water	26.8 - 65.2	S, G, R	Holcombe *et al*.,1976
Lead	Bk.trout (organs)	*S.fontinalis*	water	3.2 - 30	behavior	Holcombe *et al*.,1976
Lead	Bk.trout (eggs)	*S.fontinalis*	water	4.02	G, D	Holcombe *et al*.,1976
Pb nitrate	Bk.trout (embryo)	*S.fontinalis*	water	0.4	S	Holcombe *et al*.,1976
Non-Effective Organ and Egg/Embryo Residues - Lead, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Pb nitrate	FW mussel (org)	*P.grandis*	water	18 - 48	G	Black *et al*.,1996
Pb nitrate	SW worm (ant/post)	*T.tubifex*	water	30/50	Physiol.	Lucan-Bouche *et al*., 1999
Lead	SW Catfish (organs)	*M. gulio (juvenile)*	water	nd - 7.4	G	Kasthuri & Chandran, 1997
Lead	SW Catfish (organs)	*M. gulio (adult)*	water	nd - 21.4	G	Kasthuri & Chandran, 1997
Pb	R.trout (sacfry-blood)	*O.mykiss*	water	0.64	S, G	Hodson *et al*.,1980
Pb nitrate	R.trout (organs)	*O.mykiss*	water	1.15 - 65	S	Hodson *et al*.,1978
Pb nitrate	Bk.trout (organs)	*S.fontinalis*	water	2.4 - 140	S, G, R	Holcombe *et al*.,1976
Lead	Bk.trout (organs)	*S.fontinalis*	water	0.2 - 100	S, G, R	Holcombe *et al*., 1976
Pb acetate	Fish, SW (organs)	*G. mirabilis*	water	2.5 - 220	S	Somero *et al*.,1977
Lead	Bk.trout (eggs)	*S.fontinalis*	water	1.6, 2.55	S, G, D	Holcombe *et al*., 1976
Pb nitrate	Bk.trout (embryo)	*S.fontinalis*	water	0.34	S	Holcombe *et al*.,1976

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-7 (a). Effective Whole Body Residues - Zinc, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Zn	blue mussel	*M.edulis*	particle	148	S (ER100)	Burbidge *et al*.,1994
Zn chloride	zebra mussel	*D.polymorpha*	water	600	S (ED>50)	Kraak *et al*.,1994
Zn sulfate	snail - FW	*L. praerosa*	water	88	S (ER30)	Reed-Judkins *et al*., 1997
Zn sulfate	worm - SW	*N. diversicolor*	water- 20°	174.4	S (ER50)	Fernandez and Jones, 1990
Zn chloride	amphipod - FW	*H. azteca*	sediment	60.8	S (ER67)	Borgmann and Norwood, 1997
Zn sulfate	amphipod - SW	*A. compressa*	water	24	S (ER90), G	Ahsanullah and Williams, 1991
Zn sulfate	amphipod - SW	*A. compressa*	water	30	S (ER30)	Ahsanullah and Williams, 1991
Zn sulfate	crayfish	*O.virilis*	water	69.2	S (ER61)	Mirenda, 1986
Zn sulfate	crayfish	*O.virilis*	water	37.8	S (ER42)	Mirenda, 1986
Zn sulfate	crayfish	*O.virilis*	water	35.2	S (ER22)	Mirenda, 1986
Zn sulfate	Flagfish	*J.floridae*	water	44	S (ER80)	Spehar, 1976
Zn sulfate	Flagfish	*J.floridae*	water	40	G	Spehar, 1976

Table A-7 (b). Non-effective Whole Body Residues - Zinc, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Zn	blue mussel	*M.edulis*	particle	78	S	Burbidge *et al*.,1994
Zn sulfate	blue mussel	*M.edulis*	water	38.4	S	Kaitala, 1988
Zn sulfate	clam	*M.balthica*	water	55.8	S	Kaitala, 1988
Zn chloride	zebra mussel	*D.polymorpha*	water	122	S	Kraak *et al*.,1994
Zn chloride	zebra mussel	*D.polymorpha*	water	150	S	Kraak *et al*.,1994
Zn chloride	zebra mussel	*D.polymorpha*	water	600	G	Kraak *et al*.,1994
Zn sulfate	snail - FW	*L. praerosa*	water	27	S	Reed-Judkins *et al*., 1997
Zn sulfate	worm - SW	*N. diversicolor*	water 6-12°	73 - 170	S	Fernandez & Jones, 1990
Zn chloride	worm - FW	*L. variegatus*	sediment	47.7	S	Peterson *et al*., 1996
Zn sulfate	amphipod - SW	*A. compressa*	water	26	S, G	Ahsanullah and Williams, 1991
Zn sulfate	crayfish	*O.virilis*	water	12.7	S	Mirenda, 1986
Zinc	barnacle	*B.crenatus*	combined	3200	B (feeding)	Powell and White, 1990
Zn chloride	amphipod - FW	*H. azteca*	sediment	30.2 -53.6	S	Borgmann and Norwood, 1997
Zn sulfate	Flagfish	*J.floridae*	water	40	S	Spehar, 1976
Zn sulfate	Flagfish	*J.floridae*	water	34	G	Spehar, 1976
Zn	R.trout	*O.mykiss*	diet/water	32.6 - 60.6	S, G	Mount *et al*., 1994
Zinc sulfate	Atl Salmon	*S. Salar*	water	60	S, G	Farmer *et al*.,1979
Zn sulfate	Guppy	*P.reticulata*	water	280, 320-480	S, G, R	Pierson, 1981

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-7 (c). Effective and non-effective organ and egg/embryo residues. Zinc, ppm

Effective Organ and Egg/Embryo Residues - Zinc, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Zn sulfate	lobster (gill)	*H.americanus*	water	360 - 514	S (ER17)	Haya *et al*.,1983
Zn sulfate	Flagfish (embryo)	*J.floridae*	water	60-64	G	Spehar, 1976
Zn sulfate	Flagfish (embryo)	*J.floridae*	water	60-68	S	Spehar *et al*.,1978
Zn sulfate	dogfish (organs)	*S.canicula*	water	13.6 - 124.1	S	Flos *et al*.,1979
Zn sulfate	Bk.trout (organs)	*S.fontinalis*	water	37 - 77	R	Holcombe *et al.*,1979
Non-Effective Organ and Egg/Embryo Residues - Zinc, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
Zn chloride	mussel (organs)	*M.galloprovincialis*	water	57.4 - 160.4	S	Pagliarani *et al*.,1996
Zn sulfate	Flagfish (embryo)	*J.floridae*	water	60-68	S	Spehar, 1976, 1978
Zn sulfate	dogfish (organs)	*S.canicula*	water	22 - 560	S, G	Crespo and Balasch, 1980
Zn sulfate	dogfish (organs)	*S.canicula*	water	9.3 - 46	S	Flos *et al*.,1979
Zn	R.trout (liver)	*O.mykiss*	water	15.2 - 26.8	S	Olsson *et al*.,1987
Zn sulfate	R.trout (organs)	*O.mykiss*	water	15 - 75	S	Hogstrand *et al*.,1994
Zn sulfate	R.trout (organs)	*O.mykiss*	water	60	S, G, R	Farmer *et al*.,1979
Zn sulfate	R.trout (organs)	*O.mykiss*	diet	23.2 -257.5	S	Kock and Bucher, 1997
Zn sulfate	Bk.trout (organs)	*S.fontinalis*	water	37 - 77	S, G	Holcombe *et al*.,1979

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-8 (a). Effective Whole Body Residues - Chromium, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
K dichromate	worm	*N.arenaceodentata*	water	6.03	R (ER35)	Oshida and Word, 1982
K dichromate	sand crab (larvae)	*P.pelagicus*	water	3.2, 6.3	G	Mortimer and Miller, 1994
chromium	cladoceran	*D.magna*	water	55	S (ER50)	Enserink *et al*.,1991
chromium VI	R.trout	*O.mykiss*	water	10.5	S (ER50)	Van De Putt *et al.*,1981
chromium	R.trout (enzyme)	*O.mykiss*	water	0.87	P (ER50)	Buhler *et al*.,1977

Table A-8 (b). Non-Effective Whole Body Residues - Chromium, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
K dichromate	worm	*N.arenaceodentata*	water	4.42, 8.28	R	Oshida and Word, 1982
K dichromate	amphipod	*A.compressa*	water	9.2	S,G	Ahsanullah and Williams, 1991
K dichromate	sand crab (larvae)	*P.pelagicus*	water	1.0, 3.2	G	Mortimer and Miller, 1994
chromium VI	R.trout	*O.mykiss*	water	2.3	S	Van De Putte *et al.*,1981
chromium	R.trout (enzyme)	*O.mykiss*	water	0.87	P (ER50)	Buhler *et al*.,1977

Table A-8 (c). Effective and non-effective organ and egg/embryo residues - Chromium, ppm

Effective Organ and Egg/Embryo Residues - Chromium, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
None reported.						
Non-effective Organ and Egg/Embryo Residues - Chromium, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
K dichromate	R.trout (organs)	*O.mykiss*	water	0.58 - 3.48	S	Calamari *et al*.,1982
Chromium III	Gray mullet (liver)	*C.labrosus*	water	1.6, 33.8	S, G	Walsh *et al*.,1994

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-9 (a). Effective Whole Body Residues - Nickel, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
NiCl	bivalve	*C. derma*	water	575	S	Wilson, 1983
Nickel	clam - SW	*C.edule*	water	56.6	S (ER50)	Wilson, 1983
Nickel	worm-FW	*L.hoffmeisteri*	sediment	30 - 45	Physiological	Martinez-Tabche *et al*., 1999
Nickel	cladoceran	*D.magna*	water	223	S (ER50)	Enserink *et al*.,1991

Table A-9 (b). Non-Effective Whole body Residues - Nickel, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
NiCl	bivalve	*C. derma*	water	79	S	Wilson, 1983

Table A-9 (c). Effective and non-effective organ and egg/embryo residues. Nickel, ppm

Effective Organ and Egg/Embryo Residues - Nickel, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
NiCl	FW mussel (org)	*L.marginalis*	water	328-1456	S (ER50)	Sreedevi *et al*., 1992
NiCl	Carp (organs)	*C.carpio*	water	82.2 - 227	S	Sreedevi *et al*., 1992
Non-effective Organ and Egg/Embryo Residues - Nickel, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
NiCl	FW mussel (org)	*L.marginalis*	water	180-570	S	Sreedevi *et al*., 1992
NiCl	Carp (organs)	*C.carpio*	water	36.4 - 103	S	Sreedevi *et al*., 1992
NiCl	R. trout (organs)	*O.mykiss*	water	0.8 - 4.0	S	Calamari *et al*., 1982

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-10 (a). Effective Whole Body Residues - (Tributyl Tin, ppm)

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
TBT	snail-FW	*M.cornuarietis*	water	0.32 - 0.36	R	Schulte-Oehlmann *et al.*, 1995
TBT-paint	snail-SW	*N.lapillus*	water	0.46, 1.17	R	Gibbs and Bryan, 1987
TBT-chloride	snail-SW	*N.lapillus*	water	0.67	R	Bryan *et al*., 1988
TBT-chloride	snail-SW	*N.lapillus*	injection	0.27 - 0.43	R	Bryan *et al*., 1988
TBT	snail-SW	*I.obsoleta*	water/field	20	R(ER40-50)	Bryan *et al*.,1989
TBT	mussel - SW	*M.edulis*	water/field	2	G	Salazar and Salazar, 1988
TBT-paint	mussel - SW	*M.edulis*	water	2.2	G	Thain, 1986
TBT-paint	oyster - SW	*C.gigas*	water	2.38	G	Thain, 1986
TBT-paint	oyster - SW	*O.edulis*	water	1.23	G	Thain, 1986
TBT-paint	oyster - SW	*O.edulis*	water	0.4	R	Thain, 1986
TBT-paint	clam - SW	*V. decussata (spat)*	water	2.64, 2.91	G	Thain, 1986
TBT	clam - SW	*S. plana*	sediment	5.0 - 6.0	S	Langston and Burt, 1991
TBT chloride	snail-SW	*T. clavigera*	injection	0.2, 0.32	S (ER25, 70)	Horiguchi *et al.,* 1997
TBT-chloride	worm-SW	*N.arenaceodentata*	diet	3.4	S	Moore *et al*., 1991
TBT-chloride	worm-SW	*N.arenaceodentata*	diet	1.3	G, R	Moore *et al*., 1991
TBT-chloride	worm-SW	*A.brevis*	water	5.6, 7	S(ER7, 14)	Meador, 1997
TBT-chloride	worm-SW	*A.brevis*	sediment	8.1 - 18.4	S	Meador *et al*., 1997
TBT-chloride	amphipod - SW	*E. estuarius*	water	7 - 11.6	S(ER50)	Meador, 1993
TBT-chloride	amphipod - SW	*R. abronius*	water	6 - 14.2	S(ER50)	Meador, 1993
TBT-chloride	amphipod - SW	*E. estuarius*	water	10	S(ER14)	Meador *et al.,* 1993
TBT-chloride	amphipod - SW	*R. abronius*	water	10.8, 28	S(ER20, 80)	Meador *et al.,* 1993
TBT-chloride	amphipod - SW	*E. washingtonianus*	water	8.4, 10.5	S(ER20, 75)	Meador *et al.,* 1993
TBT-chloride	amphipod - SW	*R. abronius*	water	5.1, 13.5	S(ER19, 60)	Meador, 1997
TBT-chloride	amphipod - SW	*R. abronius*	sediment	9.45 - 56	S	Meador *et al.,* 1997
TBT-chloride	amphipod - SW	*E. washingtonianus*	sediment	7.35 - 16.8	S	Meador *et al*., 1997
TBT-chloride	amphipod - FW	*H.azteca (adult)*	water	36.6	S (ER75)	Borgmann *et al.,* 1996
TBT-chloride	amphipod - FW	*H.azteca (juvenile)*	water	1.16, 5.8	S (ER20, 50)	Borgmann *et al.,* 1996
TBT-chloride	cladoceran - FW	*D.magna*	water	0.65	S	Fent and Looser, 1995
TBT-chloride	guppy -FW	*P.reticulata*	water	3.77 - 8.7	S	Tas *et al*., 1993; 1996
TBT-chloride	flounder - SW	*P.stellatus*	water	2.0, 5.1	S(ER5, 36)	Meador, 1997

Table A-10 (b). Non-Effective Whole body Residues - (Tributyl Tin, ppm)

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
TBT	snail-FW	*M.cornuarietis*	water	0.16	R	Schulte-Oehlmann *et al*., 1995
TBT-paint	snail-SW	*N.lapillus*	water	0.62	R	Gibbs and Bryan, 1987
TBT-chloride	snail-SW	*N.lapillus*	injection	0.23	R	Bryan *et al*., 1988
TBT-oxide	snail-SW	*N.lapillus*	water	1.03	S	Davies *et al*., 1997
TBT	snail-SW	*I.obsoleta*	water/field	4	R	Bryan *et al*.,1989
TBT	mussel - SW	*M.edulis*	water/field	1	G	Salazar and Salazar, 1988
TBT	mussel - SW	*M.edulis*	water/field	4.5	S	Salazar and Salazar, 1988
TBT-paint	mussel - SW	*M.edulis*	water	2.2	S	Thain, 1986
TBT-paint	mussel - SW	*M.edulis*	water	0.56	G	Thain, 1986
TBT-paint	oyster - SW	*C.gigas*	water	0.63	G	Thain, 1986
TBT-paint	oyster - SW	*O.edulis*	water	1.23	S	Thain, 1986
TBT-paint	oyster - SW	*O.edulis*	water	0.4	G	Thain, 1986
TBT-paint	oyster - SW	*O.edulis*	water	0.19	R	Thain, 1986
TBT-paint	oyster - SW	*O.edulis (spat)*	water	0.53	S, G	Thain, 1986
TBT	mussel - FW	*D.polymorpha*	water	12.7	S, G	van Slooten and Tarradellas, 1994
TBT-paint	clam - SW	*V. decussata (spat)*	water	2.64, 2.91	S	Thain, 1986
TBT-paint	clam - SW	*V. decussata (spat)*	water	0.16, 1.48	G	Thain, 1986
TBT	clam - SW	*S. plana*	sediment	1	S	Langston and Burt, 1991
TBT-chloride	worm-SW	*N.arenaceodentata*	diet	1.3	S	Moore *et al*., 1991
TBT-chloride	worm-SW	*N.arenaceodentata*	diet	0.6	G, R	Moore *et al*., 1991
TBT chloride	crab - SW	*C.sapidus*	diet	0.12	G	Rice *et al*., 1989
TBT-chloride	amphipod - SW	*E. estuarius*	water	6.0 - 8.0	S	Meador *et al.,* 1993
TBT-chloride	amphipod - SW	*R. abronius*	water	5.94	S	Meador *et al.,* 1993
TBT-chloride	amphipod - FW	*H.azteca (adult)*	water	18.3	S	Borgmann *et al.,* 1996
TBT-chloride	cladoceran - FW	*D.magna*	water	0.39	S	Fent and Looser, 1995
TBT-chloride	Carp	*C.carpio*	water	4.1	S	Tsuda *et al*., 1990a
TBT-oxide	minnow - SW	*C.variegatus*	water	2.6, 2.95	S	Ward *et al*., 1981
TBT-chloride	guppy -FW	*P.reticulata*	water	0.07, 0.26	S	Tsuda *et al*., 1990b
TBT-chloride	fish -FW	*T.thymallus*	water	20	S	Fent and Looser, 1995
TBT-oxide	bream -SW	*P.major*	water	0.41	S	Yamada and Takayanagi, 1992
TBT-oxide	mullet - SW	*M.cephalus*	water	0.37	S	Yamada and Takayanagi, 1992
TBT-oxide	filefish -SW	*R. ercodes*	water	0.4	S	Yamada and Takayanagi, 1992

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-10 (c). Effective and non-effective organ and egg/embryo residues (Tributyl Tin, ppm)

Effective Organ and Egg/Embryo Residues - Tributyl Tin, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
TBT-paint	snail-SW (embryo)	*N.lapillus*	water	0.18	R	Gibbs *et al*., 1988
TBT-paint	snail-SW (egg)	*N.lapillus*	water/field	0.029	R	Gibbs and Bryan, 1987
TBT	Bream (organs)	*P. major*	water	0.94 - 3.3	S (ER9)	Yamamoto *et al*., 1997

Non-effective Organ and Egg/Embryo Residues - Tributyl Tin, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
TBT-paint	snail-SW (embryo)	*N.lapillus*	water	0.08	R	Gibbs *et al*., 1988
TBT-paint	snail-SW (egg)	*N.lapillus*	water/field	0.016	R	Gibbs and Bryan, 1987
TBT-paint	snail-SW (organs)	*N.lapillus*	water	0.02 - 0.18	S	Bryan *et al.,* 1993
TBT-oxide	mussel - SW (orgs)	*M.edulis*	water	1.3 -4.9	S	Laughlin and French, 1988
TBT-chloride	Carp (organs)	*C.carpio*	water	0.013 - 0.818	S	Tsuda *et al*., 1988
TBT-oxide	Carp (organs)	*C.carpio*	water	1.05 - 6.64	S	Tsuda *et al*., 1988
TBT	Bream (organs)	*P. major*	water	0.94 - 3.3	G	Yamamoto *et al*., 1997
TBT	Bream (organs)	*P. major*	water	0.54 - 2.0	S	Yamamoto *et al*., 1997
	Bream (organs)	*P. major*	water	0.19 - 0.46	S	Yamada and Takayanagi, 1992

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-11(a). Effective Whole Body Residues - DDT, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
DDT	cladoceran (FW)	*D.magna*	water	1150	S (ER50)	Crosby and Tucker, 1971
DDT+metabolites	Blue crab	*C.sapidus*	diet	1	S	Leffler, 1975
DDT	Pink shrimp	*P.duorarum*	water	0.15 - 0.21	S	Nimmo *et al*., 1970
DDT+metabolites	Cutthroat trout	*O.clarki*	maternal	0.567	S (ER30)	Cuerrier *et al*., 1967
tDDT	Salmon	*O.kisutch*	diet	113	S (ER85)	Buhler *et al*., 1969
DDT+metabolites	R.trout	*O.mykiss*	maternal	1.14 - 1.42	S (ER90)	Cuerrier *et al*., 1967
DDT+metabolites	Salmon (4.4g fingerling)	*O.tshawytscha*	diet (39days)	21.7	S (ER55)	Buhler *et al*., 1969
DDT+metabolites	Salmon (1.1g fingerling)	*O.tshawytscha*	diet (40days)	11.4	S (ER53)	Buhler *et al*., 1969
DDT+metabolites	Bk.trout	*S.fontinalis*	maternal	0.46	S (ER70)	Cuerrier *et al*., 1967
DDT	Bk.trout	*S.fontinalis*	maternal	2.8 - 7.6	R	Macek, 1968
DDT	Lake trout	*S.namaycush*	maternal	2.93	S	Burdick *et al*., 1964
DDT+metabolites	Goldfish	*C.auratus*	water&diet	400	S (ED>80)	Rhead and Perkins, 1984
DDT+metabolites	Goldfish	*C.auratus*	water&diet	200	S (ED>20)	Rhead and Perkins, 1984
DDT+metabolites	Fathead minnow	*P.promelas*	water	160	S (ER50)	Jarvinen *et al*., 1976; 1977
DDT+metabolites	Fathead minnow (larvae)	*P.promelas*	water&diet	88 - 96	S (ER100)	Jarvinen *et al*., 1976; 1977
DDT+metabolites	Fathead minnow	*P.promelas*	maternal	209	R (ER81)	Jarvinen *et al*., 1976; 1977
DDT+metabolites	mosquitofish	*G.affinis*	water	26.5	S (ER50)	Pillai *et al*., 1977
DDT+metabolites	sailfin molly (SW)	*P.latipinna*	water	77.3	S, G	Benton *et al*., 1994
DDT+metabolites	sunfish & pumpkinseed	*Lepomis spp.*	water/field	24	S	Hamelink *et al*., 1971
DDE	Lake trout	*S.namaycush*	water & diet	0.29	S	Berlin *et al*., 1981

Table A-11 (b). Non-Effective Whole Body Residues - DDT, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
tDDT	polychaete	*H.filiformis*	sediment	5.9	S	Mulsow and Landrum, 1995
DDT+metabolites	Pink shrimp	*P.duorarum*	water	0.06	S	Nimmo *et al*., 1970
DDT+metabolites	Blue crab	*C.sapidus*	diet	0.13	S	Leffler, 1975
DDT+metabolites	Menhaden	*B.tyrannus*	diet	24	G	Warlen *et al*., 1977
tDDT	Salmon	*O.kisutch*	diet	16.6	S	Buhler *et al*., 1969
DDT+metabolites	R.trout	*O.mykiss*	diet	4.67	S, G	Macek *et al*., 1970
DDT+metabolites	R.trout	*O.mykiss*	maternal	0.064 - 0.178	S	Cuerrier *et al*., 1967
DDT+metabolites	R.trout	*O.mykiss*	water	0.15 - 0.30	S	Hopkins *et al*., 1969
DDT+metabolites	Salmon (1.1g fingerling)	*O.tshawytscha*	diet (40days)	2.2 - 11.4	S	Buhler *et al*., 1969
DDT+metabolites	Bk.trout	*S.fontinalis*	water, diet	1.92, 25.6	S	Macek and Korn, 1970
DDT + metabolites	Bk.trout	*S.fontinalis*	diet	0.008 -0.042	S	Wang and Simpson, 1996
DDT + metabolites	Bk.trout	*S.fontinalis*	injection	1.0 - 5.0	S, G	Addison and Zinck, 1977
DDT+metabolites	Bk.trout	*S.fontinalis*	diet/field	2.8 - 7.6	S, G	Macek, 1968
DDT+metabolites	Goldfish	*C.auratus*	diet&water	130	S	Rhead and Perkins, 1984
DDT+metabolites	Golden shiner	*N.crysoleucas*	diet &water	0.03 - 3.6	S	Courtney and Reed, 1971
DDT+metabolites	Fathead minnow	*P.promelas*	water	40	S	Jarvinen *et al*., 1976; 1977
DDT+metabolites	sailfin molly (SW)	*P.latipinna*	water	43	S, G	Benton *et al*., 1994
DDE	Lake trout	*S.namaycush*	water & diet	2.68	G	Berlin *et al*., 1981

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-11 (c). Effective and non-effective organ and egg/embryo residues. DDT, ppm

Effective Organ and Egg/Embryo Residues - DDT, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
DDT+metabolites	Pink shrimp (organs)	*P.duorarum*	water	0.19 - 40.4	S	Nimmo *et al*., 1970
DDT+metabolites	White shrimp (organs)	*P.setiferus*	water	0.02 - 11	S	Nimmo *et al*., 1970
DDT+metabolites	Salmon (embryo)	*O.kisutch*	water	1.09 - 2.76	S	Johnson and Pecor, 1969
DDT+DDE	R.trout (brain)	*O.mykiss*	oral	11.7 - 14.8	S	Mayer *et al*., 1972
DDT+metabolites	R.trout (ovary)	*O.mykiss*	water	3.47	R	Hopkins *et al*., 1969
DDT	Catfish (organs)	*H.fossilis*	injection	50.5 - 1473	S	Agarwal and Gupta, 1974
DDT+metabolites	mummichog (organs)	*F.heteroclitus*	water	1.7 - 14.2	S (ER94)	Crawford and Guarino, 1976
DDT+metabolites	airbreathing fish (organs)	*C.striatus*	water	0.12 - 31.99	S	Pandian and Bhaskaran, 1983
DDT+DDE	Winter flounder (embryo)	*P.americanus*	maternal	2.5 - 3.8	S (ER91-99)	Smith and Cole, 1973

Non-Effective Organ and Egg/Embryo Residues - DDT, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
tDDT	polychaete	*H.filiformis*	sediment	5.9	S	Mulsow and Landrum, 1995
DDT +metabolites	Pink shrimp (organs)	*P.duorarum*	water	0.02 -0.7	S	Nimmo *et al*., 1970
DDT+metabolites	Salmon (embryo)	*O.kisutch*	water	0.55 -0.66	S	Johnson and Pecor, 1969
DDT+DDE	R.trout (brain)	*O.mykiss*	oral	8.7	S	Mayer *et al*., 1972
DDT+metabolites	R.trout (ovary)	*O.mykiss*	water	0.31 - 0.83	R	Hopkins *et al*., 1969
DDT+metabolites	mummichog (organs)	*F.heteroclitus*	water	1.5 - 5.1	S	Crawford and Guarino, 1976
DDT+metabolites	mummichog (egg)	*F.heteroclitus*	maternal	0.16	S	Crawford and Guarino, 1976
DDT+DDE	Winter flounder (embryo)	*P.americanus*	maternal	1.55	S	Smith and Cole, 1973

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-12 (a) Effective Whole Body Residues - PCBs, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
PCBs	blue mussel	*M.edulis*	combined	1.4	S	Velduizen-Tsoerkan *et al*.,1991
PCBs	oyster	*C.virginica*	water	4	G (ER10)	Hansen *et al*.,1974
PCBs	oyster	*C.virginica*	water	425	G	Lowe *et al*., 1972
1254	Pink shrimp	*P.duorarum*	water (20 days)	16	S (ER72)	Duke *et al*., 1970
1254	Pink shrimp	*P.duorarum*	water (2 days)	3.9	S	Duke *et al*., 1970
1254	Grass shrimp	*P.pugio*	water	27	S (ER45)	Nimmo *et al*., 1974
1254	Grass shrimp	*P.pugio*	water	65	S (ER60)	Nimmo *et al*., 1974
1016	horseshoe crab (23 mg)	*L.polyphemus*	water	31.9	S (ED>50)	Neff and Giam, 1977
1248	amphipod - FW	*G.pseudolimnaeus*	water	552	R	Nebeker and Puglisi, 1974
1242	amphipod - FW	*G.pseudolimnaeus*	water	409	S	Nebeker and Puglisi, 1974
1242	amphipod - FW	*G.pseudolimnaeus*	water	246 - 387	R	Nebeker and Puglisi, 1974
Individual PCB	cladoceran	*D.magna*	water	0.7	G	Dillon *et al*., 1990
individual PCBs	cladoceran-neonate	*D.magna*	water	11	G	Dillon *et al*., 1990
PCBs	Brown shrimp	*P.aztecus*	water	42	S (ER43)	Hansen *et al*.,1974
1254	Coho salmon	*O.kisutch*	diet	645 - 659	S (ER100)	Mayer *et al*., 1977
1254	Lake trout	*S.namaycush*	combined	1.53	S	Berlin *et al*., 1981
1254	Fathead minnow	*P.promelas*	water	648-745	S	van Wezel *et al*., 1995
1254	Fathead minnow	*P.promelas*	water	83-553	R	Nebeker *et al*., 1974
1254	pinfish	*L.rhomboides*	water (20-32 ppt)	14	S (ER66)	Hansen *et al*., 1971
1254	spot	*L.xanthurus*	water (16-32 ppt)	46 - 120	S (ER51-53)	Hansen *et al*., 1971
1254	minnow - SW (adult)	*C. variegatus*	maternal	9.3 - 9.7, 25	R (ER23, 74)	Hansen *et al*., 1973
1016	minnow - SW (juvenile)	*C.variegatus*	water	1100	S (ER88)	Hansen *et al*., 1975
1016	minnow - SW (larvae)	*C.variegatus*	water	200	S	Hansen *et al*., 1975
1248	Fathead minnow	*P.promelas*	water	11.0 - 50	G	Defoe *et al*., 1978
1242	Fathead minnow	*P.promelas*	water	1.28 - 20.5	S	van Wezel *et al*., 1995
1260	Fathead minnow	*P.promelas*	water	0.36 - 10	S	van Wezel *et al*., 1995
1268	Fathead minnow	*P.promelas*	water	0.45 - 4.53	S	van Wezel *et al*., 1995
Clophen A50	goldfish	*C.auratus*	water	250 - 324	S	Hattula and Karlog, 1972
Clophen A50	minnow - SW	*P.phoxinus*	diet	170 - 180	S (ER50), R	Bengtsson, 1980
PCB mixture	zebrafish - FW	*B.rerio*	water	4300	S (ER83)	Bouraly and Millischer, 1989
individual PCBs	guppy	*P.reticulata*	diet	144 - 446	S	Opperhuizen and Schrap, 1988
individual PCBs	Chinook salmon	*O.tshawytscha*	water	3.6 -3.8	S	Broyles and Noveck, 1979
individual PCBs	Lake trout	*S.namaycush*	water	8.4 -9.2	S	Berlin *et al*., 1981

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-12 (b). Non-Effective Whole Body Residues - PCBs, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
1254	oyster	*C.virginica*	water	33	S	Duke *et al*., 1970
PCB mixture	clam - FW	*C.fluminea*	field/water	0.45 - 0.76	S	Peterson *et al*., 1994
PCB mixture	hard clam	*M.mercenaria*	sediment	0.12	S	Rubinstein *et al*., 1983
individual PCBs	zebra mussel	*D.polymorpha*	diet/water/sediment	1.73 - 4.3	S	Brieger and Hunter, 1973
PCBs	blue mussel	*M.edulis*	combined	0.6	S	Velduizen-Tsoerkan *et al*.,1991
indiv PCB	blue mussel	*M.edulis*	water	1.1	S	Eertman *et al*., 1996
PCBs	oyster	*C.virginica*	water	101	G	Lowe *et al*., 1972
PCBs	oyster	*C.virginica*	water	101, 425	S	Lowe *et al*., 1972
PCBs	clam - SW	*M.nasuta*	ingestion	1.7	S	Boese *et al*.,1995
PCB mixture	sandworm	*N.virens*	sediment	0.63	S	Rubinstein *et al*., 1983
1254	Pink shrimp	*P.duorarum*	water (2 days)	1.3	S	Duke *et al*., 1970
1254	Grass shrimp	*P.pugio*	water	5.4 - 18	S	Nimmo *et al*., 1974
1254	Grass shrimp	*P.pugio*	sediment/field	0.42	S	Nimmo *et al*., 1974
PCB mixture	Grass shrimp	*P.pugio*	sediment	0.1	S	Rubinstein *et al*., 1983
1254	blue crab	*C.sapidus*	water	23	S	Duke *et al*., 1970
1016	horseshoe crab (23 mg)	*L.polyphemus*	water	7.7	S	Neff and Giam, 1977
1016	horseshoe crab (23 mg)	*L.polyphemus*	water	31.9	G	Neff and Giam, 1977
1248	amphipod - FW	*G.pseudolimnaeus*	water	552	S	Nebeker and Puglisi, 1974
1248	amphipod - FW	*G.pseudolimnaeus*	water	127	R	Nebeker and Puglisi, 1974
1242	amphipod - FW	*G.pseudolimnaeus*	water	246 - 387	S	Nebeker and Puglisi, 1974
1242	amphipod - FW	*G.pseudolimnaeus*	water	71 - 80	R	Nebeker and Puglisi, 1974
1242	amphipod - FW	*H.azteca*	water	28.4	S, G, R	Borgmann *et al*., 1990
Individual PCB	cladoceran	*D.magna*	water	4.0 - 26.6	S, G, R	Dillon *et al*., 1990
individual PCBs	cladoceran-neonate	*D.magna*	water	1.3	G	Dillon *et al*., 1990
Individual PCB	amphipod - FW	*H.azteca*	water	53.9 - 74.5	S, G, R	Borgmann *et al*., 1990
1254	Coho salmon	*O.kisutch*	diet	54-57	S	Mayer *et al*., 1977
1254	R.trout	*O.mykiss*	diet	81	S, G	Nestel and Budd, 1975
1254	R.trout	*O.mykiss*	diet	8.5	S, G	Lieb *et al*., 1974
1254	Lake trout	*S.namaycush*	combined	26.3	G	Berlin *et al*., 1981
1254	Lake trout	*S.namaycush*	combined	2.0 - 4.0	S, G	Mac and Seelye, 1981
1254	Fathead minnow	*P.promelas*	water	741-1253	S, G	Nebeker *et al*., 1974
1254	Fathead minnow	*P.promelas*	water	54-133	R	Nebeker *et al*., 1974
1254	Channel catfish	*I.punctatus*	diet	21	S, G	Mayer *et al*., 1977
1254	pinfish	*L.rhomboides*	water	17	S	Duke *et al*., 1970
1254	spot	*L.xanthurus*	water (10-34 ppt)	17 - 27	S	Hansen *et al*., 1971
1254	minnow - SW (adult)	*C.variegatus*	water	49	S, R	Hansen *et al*., 1973
1016	minnow - SW (adult)	*C.variegatus*	water	100 - 110	S	Hansen *et al*., 1975
1254	minnow - SW (adult)	*C. variegatus*	maternal	1.9 - 2.5	R	Hansen *et al*., 1973
1016	minnow - SW (juvenile)	*C.variegatus*	water	230	S	Hansen *et al*., 1975
1016	minnow - SW (larvae)	*C.variegatus*	water	57	S	Hansen *et al*., 1975
1248	Fathead minnow	*P.promelas*	water	190 - 360	S, R	DeFoe *et al*., 1978
1248	Fathead minnow	*P.promelas*	water	2.8 - 30.6	G	DeFoe *et al*., 1978
1248	Channel catfish	*I.punctatus*	diet	13	S, G	Mayer *et al*., 1977
1242	Fathead minnow	*P.promelas*	water	278 - 514	S, G	Nebeker *et al*., 1974
1260	Fathead minnow	*P.promelas*	water	350 - 567	S, G, R	DeFoe *et al*., 1978
1260	Channel catfish	*I.punctatus*	diet	32	S, G	Mayer *et al*., 1977
1232	Channel catfish	*I.punctatus*	diet	14	S, G	Mayer *et al*., 1977
Clophen A50	minnow - SW	*P.phoxinus*	diet	170 - 180	G	Bengtsson, 1980
Clophen A50	minnow - SW	*P.phoxinus*	diet	15 - 21	S, R	Bengtsson, 1980
individual PCBs	R.trout	*O.mykiss*	oral	0.032 - 0.197	S	Niimi and Oliver, 1983
individual PCBs	R.trout	*O.mykiss*	water	4.2	S	Guiney *et al*., 1977
individual PCBs	Channel catfish	*I.punctatus*	sediment & diet	0.03	S, G	Dabrowska *et al*., 1996
individual PCBs	guppy	*P.reticulata*	diet	100	S	Opperhuizen and Schrap, 1988
individual PCBs	R.trout	*O.mykiss*	diet	18	G	Da Costa and Curtis, 1995
individual PCBs	Chinook salmon	*O.tshawytscha*	water	0.8	S	Broyles and Noveck, 1979
individual PCBs	Lake trout	*S.namaycush*	water	0.78	S	Broyles and Noveck, 1979

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-12 (c). Effective and non-effective organ and egg/embryo residues. PCBs, ppm

Effective Organ and Egg/Embryo Residues - PCBs						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
1254	Bk.trout (embryo)	*S.fontinalis*	water	77.9	S	Freeman and Idler, 1975
1254	Bk.trout (egg)	*S.fontinalis*	water	284	S (ER50)	Mauck *et al*., 1978
1254	Bk.trout (egg)	*S.fontinalis*	water	71	G	Mauck *et al*., 1978
1254	Cod (testes, liver)	*G.morhua*	diet	1.3, 156	S (ER90)	Sangalang *et al*., 1981
1254	Cod (testes, liver)	*G.morhua*	diet	0.38, 22.3 - 31.8	R	Sangalang *et al*., 1981
Clophen A50	minnow - SW (gonad)	*P.phoxinus*	maternal	6.2	R	Bengtsson, 1980
indiv PCB	R.trout (egg)	*O.mykiss*	injection	1.35	S(ER50)	Walker and Peterson, 1991
1254	minnow - SW (embryo)	*C.variegatus*	maternal	170	S	Hansen *et al*., 1977
Non-Effective Organ and Egg/Embryo Residues - PCBs, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
1254	Bk.trout (fillet)	*S.fontinalis*	diet	39	S, G	Addison *et al*., 1978
1254	Bk.trout (embryo)	*S.fontinalis*	water	<0.5	S	Freeman and Idler, 1975
1254	Bk.trout (egg)	*S.fontinalis*	water	71	S	Mauck *et al*., 1978
1254	Bk.trout (egg)	*S.fontinalis*	water	31	G	Mauck *et al*., 1978
1254	Cod (testes, liver)	*G.morhua*	diet	0.66, 101	S	Sangalang *et al*., 1981
1254	Cod (testes, liver)	*G.morhua*	diet	0.02 - 0.06, 8.96	R	Sangalang *et al*., 1981
1016	minnow - SW (egg)	*C.variegatus*	water	77	S	Hansen *et al*., 1975
1242	Catfish (w/o stomach)	*I.punctatus*	diet	10.9 - 14.3	S, G	Hansen *et al*., 1976
Clophen A50	minnow - SW (gonad)	*P.phoxinus*	maternal	0.5	R	Bengtsson, 1980
Clophen A50	Stickleback -(carcass)	*G.aculeatus*	diet	289	R	Holm *et al*.,1993
1254	R.trout (blood)	*O.mykiss*	diet	3.7	S, G	Nestel and Budd, 1975
1242/1254 mixture	R.trout (carcass)	*O.mykiss*	diet	40.3	S	Leatherland and Sonstegard, 1980
indiv PCB	R.trout (egg)	*O.mykiss*	injection	0.074	S	Walker and Peterson, 1991
individual PCBs	R.trout (egg)	*O.mykiss*	water	3.72	S	Guiney *et al*., 1980

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-13 (a). Effective Whole Body Residues - aldrin/dieldrin, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
dieldrin	oyster	*C.virginica*	water	1.44	B (feeding)	Mason and Rowe, 1976
dieldrin	grass shrimp	*P.pugio*	combined	2.1	S	Parrish *et al*., no date
dieldrin	pink shrimp	*P.duorarum*	combined	0.23	S (ER50)	Parrish *et al*., no date
dieldrin	ostracod	*C.arcuata*	water	1	S	Kawatsk and Schmulbach, 1971
dieldrin	fathead minnow	*P.promelas*	combined	52.9	S (ER50)	Parrish *et al*., no date
dieldrin	mosquitofish	*G.affinis*	combined	28	S	Metcalf, 1974
dieldrin	guppy	*P.reticulata*	water	10.7	G	Burnett and Liss, 1990
dieldrin	goldfish	*C.auratus*	water	3.8	Behavior	Gakstatter and Weiss, 1967
dieldrin	bluegill	*L.macrochirus*	water	3.7	Behavior	Gakstatter and Weiss, 1967
dieldrin	R.trout	*O.mykiss*	water	5.65	S (ER50)	Shubat and Curtis, 1986

Table A-13 (b). Non-Effective Whole Body Residues - aldrin/dieldrin, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
dieldrin	oyster	*C.virginica*	combined	13.9	S	Parrish *et al*., no date
dieldrin	oyster	*C.virginica*	water	107.4	S	Emanuelsen *et al*.,1978
dieldrin	oyster	*C.virginica*	water	1.44, 18.6	S	Mason and Rowe, 1976
dieldrin	clam	*M.mercenaria*	combined	0.19 - 0.38	B (feeding)	Butler, 1971
dieldrin	clam	*M.arenaria*	combined	0.87 - 2.3	B (feeding)	Butler, 1971
dieldrin	oyster (cellular)	*C.virginica*	water	1.03, 11,107	cellular	Emanuelsen *et al*.,1978
dieldrin	grass shrimp	*P.pugio*	combined	0.09	S	Parrish *et al*., no date
dieldrin	pink shrimp	*P.duorarum*	combined	0.01	S	Parrish *et al*.,no date
aldrin	cladoceran	*D.magna*	water	0.48	S	Johnson *et al*.,1971
dieldrin	striped bass	*M.saxatilis*	diet	8.1	G	Santerre *et al*.,1997
dieldrin	fathead minnow	*P.promelas*	combined	12.8	S	Parrish *et al*., no date
dieldrin	mosquitofish	*G.affinis*	combined	0.157	S	Metcalf, 1974
dieldrin	R.trout	*O.mykiss*	water	0.548	S	Shubat and Curtis, 1986
dieldrin	R.trout	*O.mykiss*	water	0.36, 1.4	G	Shubat and Curtis, 1986
dieldrin	R.trout	*O.mykiss*	diet	2.13	S, G	Macek *et al*.,1970
aldrin/dieldrin	fish - FW	*L.idus*	absorption	138, 151	S	Freitag *et al*.,1985

Table A-13 (c). Effective and non-effective organ and egg/embryo residues-aldrin/dieldrin, ppm

Effective Organ and Egg/Embryo Residues - aldrin/dieldrin, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
dieldrin	catfish-FW (muscle)	*C. gariepinus*	water	0.1, 0.3	G (ER47, 90)	Lamai *et al*.,1999
dieldrin	catfish-FW (muscle)	*C. gariepinus*	water	0.1, 0.3	R (ER100)	Lamai *et al*.,1999
dieldrin	R.trout (fat)	*O.mykiss*	combined	0.14	G (ER40)	Poels *et al*.,1980
dieldrin	W.flounder (embryo)	*P.americanus*	water	1.21 -1.74	S (ER98-100)	Holcombe *et al*.,1979

Non-Effective Organ and Egg/Embryo Residues - aldrin/dieldrin, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
None reported.						

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-14 (a). Effective Whole Body Residues - Endosulfans, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
endosulfan	Grass shrimp	*P.pugio*	water	0.48, 0.21	S (ER65, 35)	Schimmel *et al*., 1977 Buhler *et al*., 1969
endosulfan	fish - FW	*Serranochromis spp*	field/water	1.15	S	Matthiessen *et al*., 1982
endosulfan	fish - FW	*Clarias spp*	field/water	0.07	S	Matthiessen *et al*., 1982
endosulfan	pinfish	*L.rhomboides*	water	0.27	S (ER35)	Schimmel *et al*., 1977
endosulfan	spot	*L.xanthurus*	water	0.26	S (ER90)	Schimmel *et al*., 1977
endosulfan	spot	*L.xanthurus*	water	0.07	S (ER45)	Schimmel *et al*., 1977
endosulfan	spot	*L.xanthurus*	water	0.03	S (ER35)	Schimmel *et al*., 1977
endosulfan	fish - FW	*Haplochromis spp*	field/water	1.08	S	Matthiessen *et al*., 1982
endosulfan	fish - FW	*P.philander*	field/water	1.46	S	Matthiessen *et al*., 1982
endosulfan	fish - FW	*Sarotherodon+Tilapia*	field/water	1.1	S	Matthiessen *et al*., 1982
endosulfan	mullet	*M.cephalus*	water	0.43 - 0.49	S (ER90)	Schimmel *et al*., 1977
endosulfan	mullet	*M.cephalus*	water	0.36	S (ER40)	Schimmel *et al*., 1977

Table A-14 (b). Non-Effective Whole Body Residues - endosulfans, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
endosulfan	blue mussel	*M.edulis*	water	8.1	S	Roberts, 1972
endosulfan	Grass shrimp	*P.pugio*	water	0.07	S	Schimmel *et al*., 1977
endosulfan	pinfish	*L.rhomboides*	water	0.2	S	Schimmel *et al*., 1977

Table A-14 (c). Effective and non-effective organ and egg/embryo residues. Endosulfans, ppm.

Effective Organ and Egg/Embryo Residues - Endosulfans, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
endosulfan	crayfish (gills)	*P.clarkii*	water	0.721, 0.806	S (ER50)	Cebrian *et al*., 1993 Buhler *et al*., 1969
endosulfan	Pike (organs)	*H.odoe*	water	0.05 - 6.73	S	Matthiessen *et al*., 1982
endosulfan	carp (organs)	*C.carpio*	water	3.48 - 4.94	S	Nowak *et al*., 1995
endosulfan	tilapia (organs)	*T.aurea*	water	0.115 - 1.07	S	Herzberg, 1986

Non-effective Organ and Egg/Embryo Residues - Endosulfans, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
endosulfan	crayfish (gills)	*P.clarkii*	water	0.033, 0.042	S	Cebrian *et al*., 1993
endosulfan	Pike (organs)	*H.odoe*	field/water	0.03 - 0.8	S	Matthiessen *et al*., 1982
endosulfan	carp (organs)	*C.carpio*	water	0.017 - 0.15	S	Nowak *et al*., 1995
endosulfan	fish - FW (viscera)	*M.macrolepidotus*	field/water	1	S	Matthiessen *et al*., 1982
endosulfan	fish - FW (organs)	*S.mystus*	field/water	0.06 - 0.14	S	Matthiessen *et al*., 1982
endosulfan	fish - FW (organs)	*Clarias spp*	field/water	0.05 - 0.14	S	Matthiessen *et al*., 1982
endosulfan	catfish (liver)	*T.tandanus*	water	0.082	S	Nowak, 1996
endosulfan	fish - FW (viscera)	*S.macrochir*	field/water	0.16	S	Matthiessen *et al*., 1982
endosulfan	tilapia (organs)	*T.aurea*	water	0.078 - 0.356	S	Herzberg, 1986

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Table A-15 (a). Effective Whole Body Residues - Chlordane, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
chlordane	oyster	*C.virginica*	combined	27	G	Parrish *et al*.,1976
chl+hept+hept ep	oyster	*C.virginica*	water	22.4	G (ER50)	Schimmel *et al*.,1976
hept+hept ep	pink shrimp	*P.duorarum*	water	0.146	S (ER50)	Schimmel *et al*.,1976
hept+hept ep	pink shrimp	*P.duorarum*	water	0.056, 0.073	S (ER50)	Schimmel *et al*.,1976
hept+hept ep	grass shrimp	*P.vulgaris*	water	2.44	S (ER50)	Schimmel *et al*.,1976
chlordane	pink shrimp	*P.duorarum*	combined	1.7	S	Parrish *et al*.,1976
chlordane	grass shrimp	*P.vulgaris*	combined	4.5	S	Parrish *et al*.,1976
hept+hept ep	pinfish	*L.rhomboides*	water	44.6	S (ER50)	Schimmel *et al*.,1976
chlordane	pinfish	*L.rhomboides*	combined	16.6	S	Parrish *et al*.,1976
chl+hept+hept ep	s.minnow	*C.variegatus*	water	59.0 - 61.8	S (ER50)	Schimmel *et al*.,1976
chlordane	s.minnow	*C.variegatus*	combined	3.18	S, R	Parrish *et al*.,1976
chl+hept+hept ep	spot	*L.xanthurus*	water	18.9	S (ER50)	Schimmel *et al*.,1976
heptachlor	spot	*L.xanthurus*	water	11.5	S	Schimmel *et al*., 1976

Table A-15 (b). Non-Effective Whole Body Residues - Chlordane, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
chl+hept+hept ep	oyster	*C.virginica*	water	0.046	G	Schimmel *et al*.,1976
chlordane	oyster	*C.virginica*	combined	11	G	Parrish *et al*.,1976
heptachlor	hard clam	*M.mercenaria*	water	0.11	B (feeding)	Butler, 1971
heptachlor	soft-shelled clam	*M.arenaria*	water	1.3	B (feeding)	Butler, 1971
hept+hept ep	pink shrimp	*P.duorarum*	water	0.01, 0.062	S (ER5)	Schimmel *et al*.,1976
hept+hept ep	grass shrimp	*P.vulgaris*	water	0.312	S (ER6)	Schimmel *et al*.,1976
chlordane	pink shrimp	*P.duorarum*	combined	0.7	S	Parrish *et al*., 1976
chlordane	grass shrimp	*P.vulgaris*	combined	4.8	S	Parrish *et al*., 1976
hept + hept epox	pinfish	*L.rhomboides*	water	8.8	S (ER5)	Schimmel *et al*., 1976
hept + hept epox	s.minnow	*C.variegatus*	water	0.063	S	Schimmel *et al*., 1976
chlordane	s.minnow	*C.variegatus*	combined	1.38	S,R	Parrish *et al*.,1976
chl+hept+hept ep	spot	*L.xanthurus*	water	0.034	S	Schimmel *et al*., 1976
hept + hept epox	spot	*L.xanthurus*	water	2.3	S	Schimmel *et al*., 1976
heptachlor	spot	*L.xanthurus*	water	2.9, 5.3	S	Schimmel *et al*., 1976
hept + hept epox	spot	*L.xanthurus*	water	4.9, 9	S	Schimmel *et al*.,1976

Table A-15 (c). Effective and non-effective organ and egg/embryo residues. Chlordane, ppm

Effective Organ and Egg/Embryo Residues - Chlordane, ppm						
Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
heptachlor	s.minnow (eggs)	*C.variegatus*	water	16	S	Hansen and Parrish, 1977
heptachlor	s.minnow (eggs)	*C.variegatus*	water	20	R	Hansen and Parrish, 1977
heptachlor epoxide	s.minnow (eggs)	*C.variegatus*	water	8	S (ER39)	Goodman *et al*., 1977

Non-Effective Organ and Egg/EmbryoResidues - Chlordane, ppm

Compound	Organism	Species	Exposure	Residue	Endpoint[1]	Reference
heptachlor	fathead (no guts)	*P.promelas*	water	17.73	S	Macek *et al*., 1976
heptachlor epoxide	s.minnow (eggs)	*C.variegatus*	water	3.6, 4.2, 8	D	Goodman *et al*., 1977
heptachlor epoxide	s.minnow (eggs)	*C.variegatus*	water	3.6, 4.2	S	Goodman *et al*., 1977

[1]S, R, and G refer to survival, reproduction and growth effects, respectively.

Appendix E.

Potential for Trophic Transfer of Metals in Benthic Invertebrate Prey to Finfish.

Summary:
This appendix reviews available peer-reviewed literature regarding the potential for dietary transfer of metals to finfish from contaminated benthic invertebrate prey. The data presented in these studies was also used to determine whether a trophic transfer factor of one is appropriate for relating fish tissue levels of concern to tissue levels of concern in benthic invertebrate prey.

It is concluded that the assumption of a trophic transfer of one is an overly conservative estimate of this parameter and alternative values are derived and proposed for four cationic metals- copper, cadmium, lead, and zinc; and for arsenic. Appropriately conservative estimates were selected from the available data using the scientific information presented in the reviewed papers.

Background and State of the Science

Much of the early work that examined the importance of the dietary pathway for transfer of metals to fish in metals-contaminated systems focused on the relative concentrations of metals in fish and prey collected from within the same system or area (e.g. Metayer *et al.* 1980, Dallinger and Kautzky 1985, and references in Table E-1). These field studies suggested that dietary transfer of metals from prey could significantly contribute to fish body burdens of metals and in certain exposure situations could be of greater importance than absorption from the water. Results of certain field studies, however, were inconclusive (see Metayer *et al.* 1980, and Dallinger and Kautzky 1985). Field-derived ratios suggested that the transfer of metals to fish from prey, was generally inefficient (i.e. ratios of consumer/prey concentrations tended to be less than one). Suedel *et al.* (1994) reviewed available data in efforts to assess the potential for trophic transfer of metals in aquatic foodwebs. The data that was considered by Suedel *et al.* is summarized in Table B-1. Based on this data they concluded that trophic transfer of metals to fish should not generally be described as biomagnification and noted that "concentrations of most metals were often higher in tissues of producers and primary consumers...than carnivorous fish".

Trophic transfer ratios of less than 1 are consistent with the findings of Reinfelder *et al.* (1998). Using the kinetic model approach, they concluded that trophic transfer of cationic metals (however only cadmium was specifically modeled in that paper) to fish is expected to be less than one.

Since publication of Suedel *et al.* 1994, the potential for trophic transfer of metals has been increasingly investigated in the laboratory under controlled conditions. Laboratory studies have been conducted with commercial feeds or live prey (e.g. *Artemia*) that were contaminated with known amounts of metals in the laboratory and fed to fish (e.g. Handy *et al.* 1992; Cockell and Hilton 1988; Hatakeyama and Yasuno 1982; Kumada *et al.* 1973; Mount *et al.* 1994;) or with benthic organisms that were collected from the field from areas known to be contaminated by metals and fed to fish (Woodward *et al.* 1994, 1995; Farag *et al.* 1994, 1999). Results of these studies are summarized in Table E-2.

In laboratory studies, body burdens of copper in fish ranged from 0 to 20.6% (n = 28, ave. 4.6%, median 2.1%) of the dietary copper concentration to which the fish were exposed. Body burdens of cadmium in fish ranged from 0 to 25%[1] (n = 31, ave. 6.6%, median 5.0%) of the dietary cadmium concentration to which the fish were exposed. Body burdens of lead in fish ranged from 0 to 22.7%[2] (n = 19, ave. 5.5%, median 3.9%) of the dietary lead concentration to which the fish were exposed. Body burdens of zinc in fish ranged from 0 to 89.9% (n = 16, ave. 18.4%, median 11.4%) of the dietary zinc concentration to which the fish were exposed. Body burdens of arsenic in fish ranged from 0 to 29.7% (n = 37, ave. 8.6%, median 6.1%) of the dietary arsenic concentration to which the fish were exposed.

[1]Excludes outlier of 156%, initial/control concentrations were not reported by authors

[2]Excludes outlier of 375%, initial/control concentr. not reported; diet concentr. questionable

Farag *et al*. (2000) showed that the degree of association of the metal with organic compounds (proteins) within prey significantly effects the efficiency of trophic transfer of metals to fish (i.e. increased covalent bonding and complexation of metals enhances the bioavailability of metals to fish consumers). Harrison and Curtis (1992) demonstrated that uptake of cadmium is higher from natural diets raised in contaminated environments than from Cd-contaminated commercial feeds. Farag *et al*. (2000) also demonstrated that metals in laboratory-dosed and field-collected invertebrates are processed differently by fish consumers during digestion and metals in laboratory-dosed prey are less available to fish. Therefore, results of studies that are conducted using contaminated feeds or laboratory-contaminated prey differ significantly from those of studies using natural prey and may underestimate the potential for trophic transfer of metals to fish.

In light of the above considerations, studies such as those conducted by Farag *et al*. (1994, 1999) and Woodward *et al*. (1994,1995), that used field-collected and contaminated prey were deemed to be the most relevant and appropriate studies for use in deriving a conservative estimate of trophic transfer potential of metals to fish from benthic invertebrate prey. Table E-3 lists results of those studies in which field-collected contaminated prey was used to estimate trophic transfer.

The maximum trophic transfer values reported for these compounds are proposed for use as appropriately conservative estimates of the potential for trophic transfer of metals to fish from benthic invertebrates exposed to dredged material for use in interpreting the results of 28 day laboratory bioaccumulation tests. These factors are:

Copper:	0.21 ($[Cu]_{fish}/[Cu]_{prey}$)	4.76 ($[Cu]_{prey}/[Cu]_{fish}$)
Cadmium:	0.25 ($[Cd]_{fish}/[Cd]_{prey}$)	4.00 ($[Cd]_{prey}/[Cd]_{fish}$)
Lead:	0.23 ($[Pb]_{fish}/[Pb]_{prey}$)	4.35 ($[Pb]_{prey}/[Pb]_{fish}$)
Zinc:	0.24 ($[Zn]_{fish}/[Zn]_{prey}$)	4.17 ($[Zn]_{prey}/[Zn]_{fish}$)
Arsenic:	0.25 ($[As]_{fish}/[As]_{prey}$)	4.00 ($[As]_{prey}/[As]_{fish}$)

While these factors are less conservative than the factor of one that is used in the current evaluation process, they still reflect the results of these studies in a conservative manner. Median trophic transfer factors reported in these studies for copper, cadmium, lead and arsenic ranged from 0.06 to 0.12 (median factor for zinc was 0.21). Because it is impossible to assess how the availability of metals in the prey species used in these studies (e.g. caddisfly and stonefly larvae) relates to availability in dominant prey species at the HARS (i.e. polychaetes and amphipods) and how uptake by trout may differ from fish species at the HARS, EPA Region 2 believes that the conservative interpretation of this dataset is reasonable.

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Table 1. Trophic Transfer of Metals to Fish Reported by Suedel *et al*. (1994)

Arsenic	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Hexanchus griseus	shark	SW	field	20.9	LeBlanc and Jackson (1973)
	Hexanchus griseus	shark	SW	field	10	LeBlanc and Jackson (1973)
	Hexagrammos spp	greenling	SW	field	0.3	LeBlanc and Jackson (1973)
	Hydrolagus colliei	ratfish	SW	field	15.2	LeBlanc and Jackson (1973)
	Diaphus dumerili	headlightfish	SW	field	0.1	Leatherland *et al.* (1973)
	Carassius auratus	goldfish	FW	laboratory	0.2	Maeda *et al*. 1990
Cadmium	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
		omnivorous fish	FW	field	1.1	Ward *et al.* (1986)
	Diaphus dumerili	headlightfish	SW	field	0.1	Leatherland *et al.* (1973)
Chromium	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Carpiodes cyprinus	quillback	FW	field	0.03	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	0.5	Mathis and Cummings (1973)
		postlarval fish	SW	laboratory	0.1	Baptist and Lewis (1969)
	Fundulus hetroclitus	mummichog	SW	laboratory	1.6	Baptist and Lewis (1969)
Copper	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Carpiodes cyprinus	quillback	FW	field	0.02	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	0.7	Mathis and Cummings (1973)
	Pleuronectes platessa	plaice	SW	laboratory	0.5	Saward *et al*. (1975)
Lead	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Etheostoma flabellare	fantail darter	FW	field	0.3	Enk and Mathis (1977)
	M. dolomieu	smallmouth bass	FW	field	0.9	Enk and Mathis (1977)
	Carpiodes cyprinus	quillback	FW	field	0.1	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	0.9	Mathis and Cummings (1973)
		omnivorous fish	SW	field	2.6	Ward *et al.* (1986)
	Helotes sexlineatus	trumpeter	SW	field	0.4	Ward *et al.* (1986)
	Platichthyes flesus	flounder	SW	field	0.7	Hardisty *et al*. (1974)
Nickel	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Carpiodes cyprinus	quillback	FW	field	0.03	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	0.7	Mathis and Cummings (1973)
	Oncorhynchus mykiss	rainbow trout	FW	field	0.01	Dallinger and Kautzky (1985)
	M. dolomieu	smallmouth bass	FW	field	1.6	Wren *et al.* (1983)
	Salvelinus namaycush	lake trout	FW	field	1	Wren *et al.* (1983)
Zinc	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Carpiodes cyprinus	quillback	FW	field	0.06	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	1	Mathis and Cummings (1973)
	Gobius spp	omnivorous fish	SW	field	0.1	Ward *et al.* (1986)
	Helotes sexlineatus	trumpeter	SW	field	0.4	Ward *et al.* (1986)
	Platichthyes flesus	flounder	SW	field	1.4	Hardisty *et al*. (1974)
	Diaphus dumerili	headlightfish	SW	field	0.1	Leatherland *et al.* (1973)
		postlarval fish	SW	laboratory	0.68	Baptist and Lewis (1969)
	Fundulus hetroclitus	mummichog	SW	laboratory	0.11	Baptist and Lewis (1969)
	Leiostomus xanthurus	spot	SW	laboratory	0.17	Willis and Sunda (1984)

Table E-2. Trophic transfer of metals to fish from contaminated prey/food.

Species	Metal	Concentrations			TTC (%)	Reference	Notes
		Diet	Initial Control	Final			
	Copper						
R.trout	Copper	110	2.9	4.6	1.55	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	140	2.9	4.7	1.29	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	200	2.9	4.4	0.75	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	250	2.9	5.9	1.20	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	440	5.7	19.6	3.16	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	830	5.7	22.4	2.01	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	1000	5.7	27.7	2.20	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	55	2.9	3.4	0.91	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	110	2.9	5.1	2.00	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	200	2.9	6.4	1.75	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	340	2.9	7.1	1.24	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	320	2.9	8.8	1.84	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	200	5.75	17	5.63	Handy 1992	32 d, feed, no depuration
R.trout	Copper	200	5.75	5.5	0.00	Handy 1992	32 d, feed, w/ 12 d depuration
B.trout	Copper	87	8.5	11.5	3.45	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Copper	178	6	26	11.24	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Copper	174	7.5	34	15.23	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Copper	381	6.25	33.5	7.15	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Copper	14	6.25	3.5	0.00	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Copper	12	6.25	4.35	0.00	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Copper	109	6.25	16	8.94	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Copper	415	6.25	39	7.89	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Copper	38.8		8	20.62	Farag et al. 1994	21d, collected, dead, starved 24 h
R.trout	Copper	185.7		6.5	3.50	Farag et al. 1994	21d, collected, dead, starved 24 h
C.trout	Copper	9.9	5.2	3.5	0.00	Farag et al. 1999	90d, feed, starved 24h
C.trout	Copper	32.9	5.2	6.1	2.74	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Copper	61.5	5.2	9	6.18	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Copper	43.8	5.2	12.3	16.21	Farag et al. 1999	90d, collected, starved 24h, vitamins
	Cadmium						
R.trout	Cadmium	7.6	0.36	0.69	4.34	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	16	0.36	0.95	3.69	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	23	0.36	1.08	3.13	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	21	0.36	1.29	4.43	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	9.5	0.76	1.31	5.79	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	36	0.76	2.76	5.56	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	69	0.76	6.83	8.80	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	150	0.15	5.45	3.53	Handy 1992	32 d, feed, no depuration
R.trout	Cadmium	150	0.15	0.985	0.56	Handy 1992	32 d, feed, w/ 12 d depuration
Guppy	Cadmium	69.5		3	4.32	Hatekeyama and Yasuno 1982	30 d, live, 1 d water depuration
Guppy	Cadmium	125.9		5	3.97	Hatekeyama and Yasuno 1982	30 d, live, 1 d water depuration
Guppy	Cadmium	170.6		6	3.52	Hatekeyama and Yasuno 1982	30 d, live, 1 d water depuration
R.trout	Cadmium	3	0.05	0.3	8.33	Kumada et al. 1973	12 wks, feed, no depuration
R.trout	Cadmium	3	0.04	0.1	2.00	Kumada et al. 1973	12 wks, feed, 6 wks depuration
R.trout	Cadmium	10	0.05	0.65	6.00	Kumada et al. 1973	12 wks, feed, no depuration
R.trout	Cadmium	10	0.04	0.09	0.50	Kumada et al. 1973	12 wks, feed, 6 wks depuration
R.trout	Cadmium	30	0.05	1.9	6.17	Kumada et al. 1973	12 wks, feed, no depuration
R.trout	Cadmium	30	0.04	0.12	0.27	Kumada et al. 1973	12 wks, feed, 6 wks depuration
R.trout	Cadmium	100	0.05	5.6	5.55	Kumada et al. 1973	12 wks, feed, no depuration
R.trout	Cadmium	100	0.04	0.27	0.23	Kumada et al. 1973	12 wks, feed, 6 wks depuration
B.trout	Cadmium	nd (<0.27)	0.075	0.15	na	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis

Species	Metal	Concentrations				Reference	Notes
		Diet	Initial Control	Final	TTC (%)		
B.trout	Cadmium	nd (<0.27)		0.225	na	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Cadmium	0.26	0.044	0.45	156.25	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Cadmium	3.12	0.05	0.8	24.04	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Cadmium	0.36	0.05	0.05	0.00	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Cadmium	0.5	0.05	0.095	9.00	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Cadmium	1.2	0.05	0.11	5.00	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Cadmium	2.39	0.05	0.6	23.01	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Cadmium	0.9		0.225	25.00	Farag et al. 1994	21d, collected, dead, starved 24h
R.trout	Cadmium	1		0.085	8.50	Farag et al. 1994	21d, collected, dead, starved 24h
C.trout	Cadmium	0.21	0.04	0.04	0.00	Farag et al. 1999	90d, feed, starved 24h
C.trout	Cadmium	0.97	0.04	0.1	6.19	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Cadmium	29.9	0.04	2.88	9.50	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Cadmium	29.1	0.04	4.33	14.74	Farag et al. 1999	90d, collected, starved 24h, vitamins

Table E-2. Trophic transfer of metals to fish from contaminated prey/food. (Continued)

Species	Metal	Concentrations			TTC (%)	Reference	Notes
		Diet	Initial Control	Final			
	Cadmium						
R.trout	Lead	33	0.98	1.93	2.88	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	58	0.98	2.37	2.40	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	90	0.98	2.31	1.48	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	82	0.98	3.09	2.57	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	88	1.74	6.29	5.17	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	130	1.74	8.96	5.55	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	210	1.74	10	3.93	Mount et al 1994	60 d, live, combined aqueous/diet
B.trout	Lead	7	1	1.2	2.90	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Lead	15	1.1	2.5	9.33	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Lead	15	0.95	4.35	22.67	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Lead	nd (<2.0)	0.5	0.6	na	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Lead	32.7	0.5	2.5	6.12	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Lead	0.36	0.5	nd (<0.2)	0.00	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Lead	9.69	0.5	1	0.00	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Lead	28.4	0.5	2.4	6.69	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Lead	0.2		0.75	375.00	Farag et al. 1994	21d, collected, dead, starved 24 h
R.trout	Lead	8.6		0.25	2.91	Farag et al. 1994	21d, collected, dead, starved 24 h
C.trout	Lead	0.2	0.2	0.2	0.00	Farag et al. 1999	90d, feed, starved 24h
C.trout	Lead	7.4	0.2	1.2	13.51	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Lead	792	0.2	36.8	4.62	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Lead	452	0.2	52.3	11.53	Farag et al. 1999	90d, collected, starved 24h, vitamins
	Zinc						
R.trout	Zinc	300	88	101	4.33	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	460	88	104	3.48	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	720	88	92	0.56	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	740	88	107	2.57	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	920	116	163	5.11	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	930	116	189	7.85	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	1900	116	303	9.84	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	185		165	89.19	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Zinc	655		155	23.66	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Zinc	1070		180	16.82	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Zinc	148.2		nd	nd	Farag et al 1994	21d, collected, dead, starved 24h
R.trout	Zinc	320.9		nd	nd	Farag et al 1994	21d, collected, dead, starved 24h
C.trout	Zinc	135	78	130	38.52	Farag et al. 1999	90d, feed, starved 24h
C.trout	Zinc	384	78	160	21.35	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Zinc	2336	78	380	12.93	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Zinc	2119	78	520	20.86	Farag et al. 1999	90d, collected, starved 24h, vitamins
	Arsenic						
R.trout	Arsenic	35	3.1	4.6	4.29	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Arsenic	40	3.1	5.3	5.50	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Arsenic	51	3.1	5.4	4.51	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Arsenic	63	3.1	6.7	5.71	Mount et al 1994	60 d, live, combined aqueous/diet
B.trout	Arsenic	6.5	0.8	0.95	2.31	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Arsenic	19	1.85	3.55	8.95	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Arsenic	19	1.45	3.9	12.89	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Arsenic	6.5	0.15	1	13.08	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis

Species	Metal	Concentrations				Reference	Notes
		Diet	Initial Control	Final	TTC (%)		
R.trout	Arsenic	19	0.45	2.9	12.89	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Arsenic	19	0.25	3.6	17.63	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Arsenic	3.5	1.25	1	0.00	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Arsenic	43.1	1.25	12	24.94	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Arsenic	2.8	1.25	1.15	0.00	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Arsenic	5	1.25	1.05	0.00	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Arsenic	42	1.25	7.5	14.88	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Arsenic	1.5		nd	nd	Farag et al 1994	21d, collected, dead, starved 24h
R.trout	Arsenic	15.4		nd	nd	Farag et al 1994	21d, collected, dead, starved 24h
C.trout	Arsenic	3.5	0.76	1.8	29.71	Farag et al. 1999	90d, feed, starved 24h
C.trout	Arsenic	2.6	0.76	0.9	5.38	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Arsenic	50.8	0.76	3.3	5.00	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Arsenic	13.5	0.76	2.4	12.15	Farag et al. 1999	90d, collected, starved 24h, vitamins
R.trout	Arsenic	180	4.5	15.5	6.11	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	360	4.5	44	10.97	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	732	4.5	89.5	11.61	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	1477	4.5	108	7.01	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	137	4.5	34.5	21.90	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	262	4.5	45.5	15.65	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	500	4.5	56	10.30	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	1053	4.5	72.5	6.46	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	163	2.5	15	7.67	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	362	2.5	22	5.39	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	793	2.5	34.5	4.04	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	1497	2.5	57	3.64	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	193	2.5	10	3.89	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	405	2.5	16	3.33	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	735	2.5	19	2.24	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	1503	2.5	30.5	1.86	Cockell and Hilton, 1988	56 d, feed, no depuration

Table E-3. Trophic transfer of metals from field collected benthic invertebrates

	Concentrations				
	Diet	**Initial/Control**	**Final**	**TTC (%)**	**Reference**
Copper					
B.trout	87	8.5	11.5	3.45	Woodward et al. 1995
B.trout	178	6	26	11.24	Woodward et al. 1995
B.trout	174	7.5	34	15.23	Woodward et al. 1995
R.trout	381	6.25	33.5	7.15	Woodward et al. 1994
R.trout	14	6.25	3.5	0.00	Woodward et al. 1994
R.trout	109	6.25	16	8.94	Woodward et al. 1994
R.trout	415	6.25	39	7.89	Woodward et al. 1994
R.trout	38.8		8	20.62	Farag et al. 1994
R.trout	185.7		6.5	3.50	Farag et al. 1994
C.trout	32.9	5.2	6.1	2.74	Farag et al. 1999
C.trout	61.5	5.2	9	6.18	Farag et al. 1999
C.trout	43.8	5.2	12.3	16.21	Farag et al. 1999
Cadmium					
B.trout	nd (<0.27)	0.075	0.15	na	Woodward et al. 1995
B.trout	nd (<0.27)		0.225	na	Woodward et al. 1995
R.trout	3.12	0.05	0.8	24.04	Woodward et al. 1994
R.trout	0.36	0.05	0.05	0.00	Woodward et al. 1994
R.trout	1.2	0.05	0.11	5.00	Woodward et al. 1994
R.trout	2.39	0.05	0.6	23.01	Woodward et al. 1994
R.trout	0.9		0.225	25.00	Farag et al. 1994
R.trout	1		0.085	8.50	Farag et al. 1994
C.trout	0.97	0.04	0.1	6.19	Farag et al. 1999
C.trout	29.9	0.04	2.88	9.50	Farag et al. 1999
C.trout	29.1	0.04	4.33	14.74	Farag et al. 1999
Lead					
B.trout	7	1	1.2	2.90	Woodward et al. 1995
B.trout	15	1.1	2.5	9.33	Woodward et al. 1995
B.trout	15	0.95	4.35	22.67	Woodward et al. 1995
R.trout	nd (<2.0)	0.5	0.6	na	Woodward et al. 1994
R.trout	32.7	0.5	2.5	6.12	Woodward et al. 1994
R.trout	9.69	0.5	1	0.00	Woodward et al. 1994
R.trout	28.4	0.5	2.4	6.69	Woodward et al. 1994
R.trout	8.6		0.25	2.91	Farag et al. 1994
C.trout	7.4	0.2	1.2	13.51	Farag et al. 1999
C.trout	792	0.2	36.8	4.62	Farag et al. 1999
C.trout	452	0.2	52.3	11.53	Farag et al. 1999
Zinc					
R.trout	655		155	23.66	Woodward et al. 1994
R.trout	1070		180	16.82	Woodward et al. 1994
R.trout	148.2		nd	nd	Farag et al 1994
R.trout	320.9		nd	nd	Farag et al 1994
C.trout	384	78	160	21.35	Farag et al. 1999
C.trout	2336	78	380	12.93	Farag et al. 1999
C.trout	2119	78	520	20.86	Farag et al. 1999
Arsenic					
B.trout	6.5	0.8	0.95	2.31	Woodward et al. 1995
B.trout	19	1.85	3.55	8.95	Woodward et al. 1995
B.trout	19	1.45	3.9	12.89	Woodward et al. 1995
R.trout	6.5	0.15	1	13.08	Woodward et al. 1995
R.trout	19	0.45	2.9	12.89	Woodward et al. 1995
R.trout	19	0.25	3.6	17.63	Woodward et al. 1995
R.trout	3.5	1.25	1	0.00	Woodward et al. 1994
R.trout	43.1	1.25	12	24.94	Woodward et al. 1994
R.trout	5	1.25	1.05	0.00	Woodward et al. 1994

R.trout	42	1.25	7.5	14.88	Woodward et al. 1994
R.trout	1.5		nd	nd	Farag et al 1994
R.trout	15.4		nd	nd	Farag et al 1994
C.trout	2.6	0.76	0.9	5.38	Farag et al. 1999
C.trout	50.8	0.76	3.3	5.00	Farag et al. 1999
C.trout	13.5	0.76	2.4	12.15	Farag et al. 1999

Appendix F.

Discussion of Uncertainties Associated with Evaluating the Potential for Bioaccumulation of Contaminants in Dredged Material Using 28-day Sediment Tests: Specific Reference to HARS-Specific Evaluation Process

VI. EXTRAPOLATING FROM LABORATORY TO FIELD

A. Representativeness of Sediment Samples Used in Bioassay Testing

1. Sampling and Testing Plans Design

2. Mixing and Homogenization of Sediments

B. Representativeness of Surrogate Testing Organisms

1. Surrogate Test Species in Relation to Benthic Community of the HARS: Identification of Dominant Field Taxa.

2. Surrogate Test Species in Relation to Benthic Community of the HARS: Comparison of Phylogenies of Test and Field Organisms

C. Representativeness of Testing Paradigm

1. Limited Duration of Exposure (28-days)

2. Controlled Conditions of the Laboratory Exposure

3. Reference Approach/statistical Uncertainties.

4. Pretest Contaminant Levels in Test Organism

5. Designated Analytes for Bioaccumulation Testing Analysis

6. Selection of Analytical/Quantitation Methods

a. Quantitation of Metal Residues

b. Quantitation of Polychlorinated Biphenyls

c. Quantitation of Polycyclic Aromatic Hydrocarbons

d. Analytical Performance

II. UNCERTAINTY ASSOCIATED WITH ASSESSING EXPOSURE TO CONTAMINANTS AT HARS

A. Estimating Exposure of Humans to HARS Contaminants

1. Seafood Consumption Patterns of Humans

a. Seafood Consumption Rate

i. *Use of the National Default Consumption Rate*

ii. *Identification of Target Sub-Population of Concern*

iii. *Estimating Consumption Rate for Sub-Population of Concern*

iv. *Source of Fish Consumed by Sub-Population of Concern*

v. *Exposure to potentially HARS-exposed species other than recreationally caught finfish*

b. Effects of Seafood Preparation on Contaminant Exposure

c. Exposure Duration

i. *Mobility*

ii. *Angling Cessation*

iii. *Mortality*

B. Estimating Exposure of Wildlife to HARS

1. Characterizing the Food Web at the HARS: Identification of Receptors

2. Estimating Site Use by Ecological Receptors

3. Estimating Trophic Transfer of Contaminants

a. Trophic Transfer of Chlorinated Organic Contaminants

i. *Log K_{ow}*

ii. *Pathway of Concern*

iii. *Equilibrium State Assumption*

iv. *Lipid Weights Assigned to Food Web Organisms*

b. Trophic Transfer of Polycyclic Aromatic Hydrocarbons (PAHs)
c. Trophic Transfer of Metals
d. Trophic Transfer of Mercury

III. RISK CHARACTERIZATION (EFFECT)

A. Human Health

1. Reference Doses and Hazard Quotient Estimates

2. Cancer Slope Factor and Risk Estimates

3. Additivity of Risk and Hazard

B. Ecological Effects (Linked Residue-Effect Data)

1. Relating Contaminant Residue Concentrations and Probabilities for Adverse Effect

2. 'Linked Residue-Effect' Guidelines

a. Narcosis
b. Non-narcosis
c. Endpoints
d. Mode of Exposure
e. Using ER50 (Median Effective Residue) as Threshold for Significant Effect
f. Interspecific Differences in Sensitivities

2. REFERENCES

Table 1. Trophic transfer of metals to fish reported by Suedel *et al.* (1994)
Table 2. Trophic transfer of metals to fish from contaminated prey/food.
Table 3. Trophic transfer of metals from field-collected benthic invertebrates

Appendix F: Discussion of Uncertainties associated with evaluating the potential for bioaccumulation of contaminants in dredged material using 28-day sediment tests: Specific reference to HARS-specific evaluation process

Laboratory methods to assess bioaccumulation provide meaningful information regarding the propensity for contaminants in dredged sediments to accumulate in benthic organisms following re-introduction of the sediments to the aquatic environment. This memorandum documents areas of uncertainty associated with the assessment of potential for bioaccumulation of contaminants from sediments discharged into the aquatic environment using the prescribed 28-day bedded sediment tests and with the evaluation of the potential for risks associated with measured tissue residues in those test organisms. The areas of uncertainty described in this memorandum are discussed in a predominately qualitative manner.

Specific reference is made to uncertainty associated with the current and proposed EPA Region 2 frameworks for evaluating the suitability of dredged materials for use as Remediation Material at the HARS.

I. EXTRAPOLATION FROM LABORATORY TO FIELD

Major areas of uncertainty regarding the use of the laboratory method to extrapolate to accumulation in the field include:

A. Representativeness of Sediment Samples Used in Bioassay Testing

1. Sampling and Testing Plans Design

Generally, sediments that are used in bioassay testing for HARS suitability are composites of discrete samples collected from specific locations within the area that is proposed for dredging. Samples of dredged material are usually obtained with vibracoring units. This type of sampling equipment obtains a sample of the vertical profile of the sediments that are proposed for dredging. The number and location of samples taken, the compositing of discrete samples, and the methods used to homogenize the sediments introduce uncertainty regarding the representativeness of the sample used in the bioassay test.

The number, location and compositing strategy of sediment samples that will be required to characterize the dredged materials of a given project area is determined after review of available data by USACE-NYD and EPA Region 2 scientists. The selected sampling and testing protocol is developed using professional judgement and considering such factors as: locations of possible sources of contaminants and sediments (including point and non-point sources), the bathymetry and geometry of the project area, and results of historical sediment testing in the area (if available). The overall goal of the Agencies' scientists in designing the sampling and testing plan for a given project area is to sample and test sediments in such a way that maximizes the probability that sediments are of similar physical, chemical, and toxicological qualities when composited for testing. In some cases, this requires dividing the proposed project area into smaller areas for testing.

Sample locations are generally assigned to shoals closest to point sources (or potentially different sediment/contaminant sources, such as tributaries) and to shoals having the thickest accumulated deposits. Proposed project areas that represent large geographic areas or that have suspected differences in hydrological influence (e.g., differing geometries or project depths within the project area) are often split into smaller areas for testing. These smaller areas are referred to as testing reaches. Sampling locations and testing reaches are assigned based on the professional judgement of staff. The only general rule applied by USACE-NYD and EPA Region 2 technical staffs in designing sampling and testing plans is that each testing reach is represented by at least three (and generally, not more than 13) core samples with a sample spacing of approximately 300

feet within continuous shoals. Other sediment assessment programs may use generic formulas based on volumes and geographical ranges to calculate the required number and locations of samples. USACE-NYD and EPA Region 2 believe that using generic mathematical formulas to assign samples will not significantly improve the representativeness of sampling because the patterns of shoaling and relationships of point source locations to shoals in channels can be quite complex. Furthermore, USACE-NYD and EPA Region 2 believe the need to assign prescribed numbers of samples based on volumes is lessened when vibracores are used since the lengths of samples reflect shoal thicknesses (and therefore volumes).

Because the relative contaminant load and bioavailability in sediments are related to the grain size of sediments, a first order measure of the efficacy of the method used by USACE-NYD and EPA Region 2 to reduce variability in sediments by compositing for testing can be obtained by examining the relative variability of the grain sizes of samples in proposed areas and in testing reaches. In general, the grain size of New York Harbor dredged materials (expressed as percent sand and larger) is moderately variable - the average coefficient of variability of specific contiguous dredging project areas is approximately 80 percent (n = 26). The average coefficient of variability of sediments in testing reaches of all contiguous project areas as tested, however, was reduced to approximately 65 percent (Reiss, unpubl. obs.). This suggests that the factors considered by the staff designing these sampling plans appear to be appropriate and the subdivision of project areas for testing using these factors decreases sediment variability in the samples used for testing.

2. Mixing/Homogenization of Sediments

Over long time scales, the geochemical environment of bedded sediments attains equilibrium (this is a dynamic equilibrium) with the specific modulating factors of the environment to which the sediments are exposed. Important geochemical characteristics of equilibrated sediments include: 1) the presence of a fine layer of organic-rich sediments on the surface that is important food for surface, deposit-feeding benthos. This layer may therefore dictate dietary exposure of these organisms to sediment-associated contaminants; and 2) a vertical profile of changing redox chemistry which affects the availability of contaminants to benthos. This redox chemistry is particularly significant for inorganic contaminants (i.e., metals). Mixing of sediments in the laboratory results in the loss of the structure associated with equilibrated sediments.

The disruption of potential geochemical structure may be of secondary importance in characterizing the bioavailability and risk of contaminants in dredged material. This lesser importance is anticipated because dredged material (in most cases) is unlikely to be in equilibrium while in place at the bottom of navigational channels. Berthing and channel areas are often highly depositional environments and/or the dredged material may be periodically subjected to physical disturbance by the passage of vessels that use the channel (i.e., by propeller wash or hull displacement waves). Furthermore, preserving *in situ* conditions for testing purposes is not an appropriate goal for assessing materials that are proposed for dredging. Dredging and open-water discharge of dredged materials are inherently disruptive processes that will necessarily result in the mixing and aeration of the sediment.

Following discharge of the materials at an open water location, however, dredged materials may be expected to attain a dynamic equilibrium with disposal site conditions over longer time scales. Simulating these long time scale conditions in the laboratory is impractical. There is no easy way to predetermine an appropriate length of time and set of conditions to allow sediments to equilibrate prior to initiating testing.

In addition, aeration of sedimentary micro-environments (i.e., burrows, tubes) by infaunal benthic organisms is commonly reported and therefore the small scale sedimentary environment conditions that might actually be controlling exposures of these organisms to contaminants in the field may be aerated and therefore not differ substantially from those in the tests. Impacts on sediment ingesters, such as polychaetes, may also not differ significantly between lab and field as these organisms burrow through the sediment and often selectively ingest

particles, based on nutritional or physical preferences. Exposures of surface deposit feeders to sediment-sorbed contaminants may, in fact, be overpredicted during testing. Under test conditions, organisms are constrained to feed on sedimentary matter sampled from the dredging project area, whereas in the field the organic matter on which surface deposit-feeders feed may settle out of the overlying water column and be less contaminated.

EPA Region 2 believes that disruption of sediment structure during testing does not introduce unacceptable uncertainty in the assessment of contaminant bioavailability and risk.

B. Using Measured Accumulations in Selected Species (Clam/worm) as Surrogates for Accumulations in Benthic Species Occurring at the HARS.

The standard methods for assessing bioaccumulation, as outlined in the Green Book (USACE/USEPA, 1991), require that two benthic organisms be exposed for 28 days to samples of the sediment proposed for dredging. It must be recognized, however, that bioaccumulation observed in the tested species are only *directly* applicable to the individual species used. The prediction of residues expected in organisms that colonize sediments in the field using levels accumulated by the tested species is an area of uncertainty that is necessarily associated with the bioassay approach to assessing contaminant bioavailability.

The Green Book (USEPA/USACE 1991) lists appropriate organisms for assessing bioaccumulation. Listed organisms interact closely with the sediments (and therefore, sediment-sorbed contaminants) via their specific life histories characteristics, such as feeding or burrowing. The two Green Book-listed organisms that are generally used in regional testing of dredged material are the bent-nosed clam (*Macoma nasuta*) and the sandworm (*Nereis virens*).

Macoma nasuta is a surface, deposit-feeding (facultative suspension-feeding) bivalve. *Nereis virens* is a burrowing, soft-bodied, sediment-ingesting worm. The two species probably differ significantly in their abilities to metabolize polycyclic aromatic hydrocarbons (PAHs). *N. virens* efficiently metabolizes PAHs (McElroy, 1985), while most bivalves are considered to be limited in this capacity (Meador *et al.* 1995b; James, 1989). The two species also differ in their lipid compositions (which affects accumulation of organic contaminants).

Accumulations measured in these two species are most appropriately extrapolated to organisms that share similar life strategies and physiological characteristics (e.g., the accumulations exhibited by *M. nasuta* may be expected to more closely resemble accumulations by other deposit feeding bivalves than accumulations by suspension feeding bivalves). The relevance of the results for estimating field residues in resident species at the HARS, however, is dependent on how closely the lifestyles and phylogenies of species at the site resemble those represented by the two test organisms. The resident benthic community of the HARS is described below.

1. Surrogate Test Species in Relation to Benthic Community of the HARS: Identification of Dominant Taxa

Macrofaunal benthic community structure at the HARS was surveyed by EPA-Region 2 in October, 1994 (Battelle, 1996). Benthic community structure was surveyed at 40 stations and revealed two distinct assemblages of organisms in the Study Area. The two assemblages were associated with differences in the grain size distribution and organic content of the bottom sediments from which organisms were sampled.

The benthos present in areas with a substantial fraction (i.e., <10 percent by weight) of fine-grained sediments are assumed to be representative of the organisms that will colonize dredged materials following placement at the HARS (as most dredged materials are predominately fine-grained). The benthic community in these sediments (n = 21 stations) was shown to be numerically dominated by *Nucula proxima*, a particle-selective, deposit feeding bivalve (Pearce *et al.* 1981), and various infaunal worms. Collectively, these organisms represented 65 - 97 percent of the individuals present. Patterns in relative biomass were similar to numerical

abundance. *N. proxima* was the sole mollusc present in all but two samples[3] and was the most abundant organism overall in nine samples. The numerical dominance of *N. proxima* and infaunal worms in benthic communities associated with fine-grained sediments in New York Bight was also reported by NOAA researchers during different times of the year (Chang *et al.*, 1992; Pearce *et al.*, 1981).

Small crustaceans (predominantly amphipods) and cnidarians are also present in the benthic community associated with fine-grained sediments (Battelle, 1996). They occur, however, at extremely low densities (averaging 250 crustaceans/m^2 and 488 cnidarians/m^2, equivalent to 1 and 2 percent of total organisms in sample) or are absent from many samples. Abundances of crustaceans in less contaminated, sandy sediments were variable, but were often much higher (1448(±2690) crustaceans/m^2) than in silty sediments. It is unclear whether the low densities (or absence) of amphipods in fine-grained sediments are related to grain size preferences or to contaminant intolerance. Regardless of the cause of the disparity in abundances of these organisms, the low overall abundances of crustaceans and cnidarians in fine-grained sediments (relative to *N. proxima* and infaunal worms) suggest that organisms used in testing need not be specifically selected to represent accumulations in these organisms. Therefore, assessing the comparability that might be expected between accumulation by test organisms and by *N. proxima* and infaunal worms at the HARS should address the most ecologically significant areas of uncertainty regarding extrapolation from laboratory to field organisms.

2. Surrogate Test Species in Relation to Benthic Community of the HARS: Comparison of Phylogenies of Test and Field Organisms

Models predict that concentrations of non-polar organic contaminants in organisms will increase until the concentration in lipids is in equilibrium to the concentration of the contaminant in the organic carbon fraction of the sediment to which the organism is exposed (these models are termed equilibrium partitioning models). Equilibrium partitioning theory suggests that the relationship at equilibrium between contaminant concentrations in these two pools (i.e., organism lipid and sediment organic carbon) can be expressed by a factor called the biota-sediment accumulation factor, or BSAF. The BSAF for a non-polar organic contaminant for a given species is primarily a function of the lipophilicity of the compound. The lipophilicity of a compound is expressed as a physicochemical coefficient termed the octanol-water partitioning coefficient, or K_{ow}. Other factors that also affect the BSAF are the total organic content and contaminant load of the sediment, so that the BSAF may also be, albeit to a lesser extent, a sediment-specific characteristic.

Hansen and Tracey (1996) compared BSAFs reported for various benthic organisms having differing degrees of association with sediments and different feeding strategies. They reported that BSAFs were fairly comparable between benthic species within these groups. As noted above, BSAFs relate organic carbon-normalized sediment contaminant concentrations to lipid-normalized tissue contaminant residues. It follows that, if BSAFs are relatively constant across species, resultant tissue residues in organisms following exposure to a given sediment is expected to increase with increasing lipid content of the organism.

Lipid contents of infaunal worms sampled from the vicinity of the HARS on October 1994 and September 1995 were measured by USEPA-Region 2 (Battelle, 1997). Lipid content (dry weight basis) of infaunal worms in these samples averaged 6.7 ± 1.6 percent (n = 19). Measured lipid content for *N. proxima* in the New York Bight was reported by McFarland *et al.* (1994) to average 5.0 ± 1.9 percent. Lipid contents of two individual species of worms (i.e., *Cerebratulus* and *Nephthys* spp.) were reported by McFarland *et al.* (1994) to be significantly higher (i.e., 24.7 (±10.6) and 18.7 (±8.6) percent, respectively) than those reported in Battelle (1997). Although not abundant during the September 1994 survey (Battelle 1996), data presented by NOAA

[3]Based on the listing of the ten most abundant species in individual samples

suggests that *Nephthys* spp. are abundant in fine sediments of the New York Bight (Chang *et al.*1992; Pearce, 1981).

Lipid contents of marine organisms can vary seasonally with reproduction cycles and/or to intra-annual differences in the physical condition of organisms associated with food availability or other environmental factors. Lipid values reported in Battelle (1997) and McFarland *et al.* (1994) were measured in organisms that were sampled during the same season of the year (i.e., August to October) and may not reflect the potential seasonality of lipid concentrations in these organisms. It is possible that the higher lipid content measured by McFarland *et al.* (1994) reflects the reproductive state of the organisms at the time of sampling, because Sanders (1956) reported a late summer spawning peak of *Nephthys* spp.

The lipid content of organisms used in testing of federal projects since 1993 (n = 13) were comparable to reported lipid values in the field collected organisms. Lipid contents of organisms used in testing of federal dredging project materials since 1993 average 7.9 (±3.9) and 4.5 (±2.1) percent in *N. virens* and *M. nasuta*, respectively (CENAN, 1993 - 2000). Clear patterns of intra-annual differences in lipid contents in *N. virens* or *M. nasuta* used in dredged material testing are not apparent in the data set. Measured lipids were mostly found to occur within a relatively narrow range of concentrations (4-8 and 2-6 percent, dry weight, respectively). The data however, do suggest a potential elevation in lipid contents occurring in *N. virens* used for testing during the spring (April-May). This potential seasonality is consistent with the reported timing of oocyte maturation for this species (Wilson and Ruff, 1988).

Seasonal enrichment in lipid content may act to temporarily increase the organic contaminant load of organisms. Researchers have documented that when the gametes are spawned, the contaminant load associated with the lipid-rich gametes is also shed (Rossi and Anderson, 1977). Indeed, the actual contaminant load in other organs of spawning individuals may also be lowered due to partitioning of contaminants to gametes from these organs prior to their release (Isaac Wirgin-NYU School of Medicine, submitted). The consequences of this temporarily increased contaminant load may not be significant to the fitness of the individual organism, as organic contaminants associated with the storage lipid pool of an organism are considered by most toxicologists to be non-reactive. Therefore, fattier individuals may be able to accumulate higher total loads of organic contaminants before adverse effects are experienced by the organism (Peter Landrum-NOAA, pers. comm.). However, it is also important to note that the higher concentrations associated with gametes may have implications for gamete/progeny survival and fitness and that the higher concentrations of contaminants in reproductively ripe (i.e., lipid-enriched) individuals will also be available to predators.

As described above, equilibrium partitioning theory and the observations of Tracey and Hansen (1996) suggest that similarities in sediment-mediated chemical exposure exist across benthic species and therefore BSAFs of benthic species should not vary widely. Other studies, however, suggest that there can be significant inter-specific differences in tissue residues of bioaccumulative contaminants as a result of (for example) differing feeding and lifestyle strategies, or their abilities to metabolize compounds. In general, these studies show that deposit feeders accumulate organic contaminants from sediments to significantly higher levels than do filter feeders (e.g., Kaag *et al.*, 1997; Foster *et al.*, 1987) and that residues of certain contaminants (particularly PAHs) are lower in species having higher abilities to metabolize them (e.g., Kane-Driscoll and McElroy, 1996).

During the 28-day test, pathways by which *M. nasuta* and *N. virens* are exposed to contaminants are expected to approximate those of HARS deposit feeders, and exceed those of HARS filter feeders. Based on the similarities in lipid content, equilibrium partitioning theory would predict that laboratory-measured accumulations of organic contaminants in these organisms should be fairly representative of accumulations in field deposit-feeding benthos. Although nereid worms (including *N. virens*) are active metabolizers of PAHs and other hydrocarbons (Kane-Driscoll and McElroy, 1996), the benthic worm community in fine-grained

sediments at the HARS is often dominated by spionid and capitellid worms (comprising 48 (±28) percent of worms, by abundance) that are also able to metabolize hydrocarbons to varying degrees. No information is available for the remaining classes of organisms. Insufficient data is available, however, to quantitatively describe the relative metabolic capacities of organisms at the HARS and test species and so some level of uncertainty is associated with extrapolation of lab accumulation to field. This area of uncertainty regarding metabolism has also been identified by Meador *et al.* (1995b) and Kane-Driscoll and McElroy (1996).

Overall, the analysis of uncertainty suggests that the use of *N. virens* and *M. nasuta* as surrogate species for use in approximating accumulations of organic contaminants in benthic organisms that dominate fine-grained sediment communities at the HARS (i.e., *N. proxima* and infaunal worms) is adequate for evaluating bioaccumulation.

Predictive models for assessing uptake of metal contaminants by marine organisms are extremely complex. The extent of accumulation of metals by specific organism is dependent on the relative ability of the organism to regulate internal concentrations of the metal. Regulation of metals is a function of the ingestion and excretion rates and the assimilation efficiencies of the specific organism and metal. Values expressing these parameters and therefore the overall ability to regulate internal metals concentrations varies widely between and within phylogenic groups (see Chapman, 1997; and data in Thomann *et al.*, 1995). Furthermore, the digestive tracts of deposit-feeding benthos have been shown to be microenvironments that efficiently solubilize inorganic contaminants from sediments (e.g., Mayer et al. 1996), and Reinfelder et al. (1995) suggest that differences in digestion have important implications for metals uptake. Given the species-specificity of these attributes the representativeness of accumulations of metals in the tested organisms as surrogates for organisms in the field cannot be ascertained.

The intent of regulatory evaluations of dredged materials proposed for aquatic placement is to evaluate accumulations in test organisms in a manner that is protective of the range of benthic species that might be expected to colonize the material following its placement. Therefore, accumulations of contaminants (particularly inorganics) measured in the test species should not be considered as accumulations in the two individual species (i.e., *M. nasuta* and *N. virens*) but rather should be considered as accumulations that are characteristic of two "generic" infaunal benthic organisms that interact closely with sediment (and are therefore maximally exposed to sediment contaminants), and that differ significantly in terms of their specific lifestyles, feeding strategies and metabolic/regulatory capabilities.

Use of the higher level of bioaccumulation of inorganic contaminants exhibited in either test species for comparisons to tissue evaluation values, such as is done in the evaluations using HARS-specific values, most conservatively interprets the potential for bioaccumulation indicated by the 28-day bedded sediment assay. In essence, by using the higher of the two accumulations, a measure of accumulation by a theoretical 'worst-case' benthic organism is derived, that accumulates inorganic contaminants to higher levels than will either of the two species individually. In the case of the species used in regional testing, we might describe this model benthic organism as an organism that has individual characteristics of *N. virens* and *M. nasuta* that maximizes the accumulation of each inorganic contaminant.

The use of this approach is conservative and may overestimate the bioaccumulation of certain inorganic contaminants in certain organisms, however, its use ensures that a broader range of organisms are protected while allowing use of standardized laboratory exposures with a manageable number of treatment subjects. In certain instances, where accumulation by a specific benthic species (ecological 'keystones' or important prey species) must be assessed and accumulations in that species is better extrapolated from accumulation of one or the other species used in testing, the use of the 'worst case' organism approach may not be appropriate. Indeed, more sophisticated risk assessments may require that the actual species of concern or of a species that

closely mimics the species of concern be used in bioassay testing for all contaminants of concern rather than the benthic 'white rats' that are routinely used.

C. Representativeness of the Testing Paradigm

1. Limited Duration of Exposure (28 days)

A 28 day exposure to assess bioaccumulation is of sufficient duration to allow certain compounds to attain an equilibrium between internal (test organism tissues) and external (sediment) concentrations. This dynamic equilibrium is referred to as 'steady-state'. This exposure duration, however, is insufficiently long to allow certain other compounds to attain steady state. Nonetheless, the 28-day exposure can be appropriately used to predict whether contaminants in the sediments will bioaccumulate in exposed organisms. The concentration measured at day 28 can also be used to predict the steady state concentrations of those compounds that will not equilibrate within 28 days but that would eventually attain steady state at some time. Certain contaminants (metals) do not attain a true 'steady state' but rather uptake and/or internal concentrations of the contaminant are regulated to varying degrees by exposed organisms.

As noted above, equilibrium partitioning models predict that the concentration of a non-polar organic contaminants in an organism will increase until the concentration in lipids is in equilibrium to the concentration of the contaminant in the organic carbon fraction of the sediment to which the organism is exposed. As stated, the octanol-water partitioning coefficient, or K_{ow}, is a major factor in determining the extent of bioaccumulation of a given compound by organisms exposed to contaminated sediment. Similarly, the K_{ow} (actually the log K_{ow}) can also be used to predict the rate at which accumulations of non-polar organic contaminants in marine organisms reaches dynamic equilibrium with the concentrations in sediments to which the organisms are exposed.

Figure 6-1 of the Inland Testing Manual (EPA/USACE, 1998) illustrates the theoretical relationship between the log K_{ow} of a neutral organic compound and the proportion of steady state concentration that is reached by organisms at day 28 of exposure to sediments. This relationship indicates that organisms will not attain steady state concentrations of neutral organic compounds that have log K_{ow}s between 5 and 9 within 28 days. Residues of readily-metabolized compounds, such as low molecular weight PAHs, may be overestimated by equilibrium-partition models. These compounds will tend to attain an apparent steady state within 28 days due to metabolism by the organism.

Equilibrium partitioning models cannot predict metals uptake and general rules cannot be applied for predicting accumulation of metals in marine organisms. Accumulation of metals by marine organisms is dependent on the kinetics of uptake and depuration (excretion) and is extremely complex. There are three basic models of metals uptake. The three basic models of metals accumulation can be illustrated by the accumulation of three metals: copper/zinc, cadmium, and mercury (Handy, pers. comm.).

Depuration rates of marine organisms of many essential metals (such as copper and zinc) tend to be high enough such that under a wide range of environmentally realistic exposure conditions the overall body residue of the metal can be maintained within an optimal (or tolerable) range. The process of maintaining internal concentrations within range is referred to as regulation. Aquatic organisms are generally able to regulate internal concentrations of essential metals due to the higher ability to depurate these compounds. Essential metals include zinc, copper, chromium, nickel.

On the other extreme, with respect to regulation of internal concentrations by marine organisms, is mercury. The depuration rate of methylmercury in essentially all marine organisms is very low (close to zero) and therefore accumulation by exposed organisms can be characterized as additive. The depuration rates of cadmium are also quite low in marine organisms, and therefore methylmercury and cadmium loads in an

exposed marine organisms are expected to increase over the lifetime of the organism. Given this expectation, the use of laboratory tests of defined durations necessarily introduces uncertainty with respect to assessing tissue residues of these contaminants that might be expected to occur in field-exposed organisms.

A method to interpret the results of bioaccumulation assays for metals which have extremely slow depuration rates, such as methyl mercury and cadmium, is to use the uptake rates evidenced in the 28-day assay to estimate the residues in organisms after lifelong exposure to the dredged material. There are two major difficulties associated with this approach: 1) estimating the average life expectancy of benthic organisms is not straightforward as it requires that natural mortality and predation rates for the organisms be quantified. Mortality and predation rates vary substantially and can be both environmentally and density-dependent; and 2) while increases in the metal load in organisms may be expected to occur, predicting the actual tissue concentration of the metal at some future time is complicated by the need to identify growth rates (growth can dilute the contaminant by growth of the organism) and to identify any changes in uptake and depuration kinetics that might be associated with life cycles. For example, spawning activity can alter the residue levels in the parent organism. In addition, certain organisms of concern may have evolved novel adaptations to maintain tolerable levels of metals in tissues. For example, a species of marine worm has been shown to be able to maintain internal residues of metals by shunting metals to its posterior segments and then shedding those segments without apparent ill effect to the organism (Lucan-Bouche *et al.* 1999).

The current approach for evaluating the potential bioaccumulation of metals from dredged material that is proposed for use as Remediation Material at the HARS uses the metals residues measured after 28 days of exposure for assessing risks. Due to the possibility that not all metals have attained steady state after 28 days of exposure, this approach may underestimate bioaccumulation and associated risks of those metals.

The proposed approach to assessing bioaccumulation of metals applies safety factors to reflect the potential for accumulation of non-essential metals in field organisms to be underestimated by the 28-day measure. This value was derived from variance in metals residues measured in field-collected polychaetes from the vicinity of the HARS. Field-collected polychaetes displayed limited variability in tissue residues of non-essential metals. The highest measured residues were approximately three times the lowest measured, despite much larger variance in the metals concentrations in the sediments from which they were taken. It was assumed that the sampled organisms represented varying lengths of exposure to the sediments, since exposure histories and durations of the sampled polychaetes was not determined.

In light of the limited variability in field-collected organisms at the HARS, EPA Region 2 proposes to apply a safety factor of three to 28-day residues of non-essential metals (Ag, Cd, Hg, Pb) for evaluating associated risk. EPA Region 2 believes that application of a safety factor of this magnitude (i.e. 3X)provides a substantial measure of conservativeness in interpreting bioaccumulation assay results. However, the application of the safety factor does not altogether eliminate the uncertainty.

Assessing bioaccumulation through laboratory procedures involving exposure of organisms for periods significantly beyond 28 days introduces a level of complexity to regulatory assessment that will significantly increase the cost of testing and also introduce the potential for additional areas of uncertainty (i.e., how do the levels and bioavailability/activity of contaminants in the sediments change over longer time scales?) and difficulty (i.e., maintaining healthy test organisms). Because models for estimating steady state concentrations exist for most compounds that do not attain steady state in 28 days, the added expense and difficulty of longer term assays does not appear to be warranted.

2. Controlled Conditions of Laboratory Exposure

During laboratory exposures, environmental conditions (e.g., water temperature, salinity, and oxygen content) are maintained within a relatively narrow range. The acceptable ranges for testing are based on the environmental preferences of the organisms that are used in the testing. By maintaining exposure conditions within this range, organism stress due to non-treatment factors is minimized. The constancy of these conditions, however, may not mimic environmental variability in the field.

Owing to the offshore location and water depth of the Historic Area Remediation Site, environmental parameters in bottom water are anticipated to be relatively constant over shorter time scales. Seasonal and stochastic (e.g., storms) variability, however, may affect the environmental conditions of exposure. Seasonal differences in the physiological state of the organism (such as those resulting from reproduction cycles) may also be anticipated to affect bioaccumulation in the field. While seasonal variation in environmental conditions can be measured, it is difficult to quantitatively translate the external environmental variability to differences in tissue residue levels.

3. Reference Approach/Statistical Uncertainties

The statistical comparison of concentrations of contaminants in tissues of organisms exposed to proposed Remediation Material to those of organisms exposed to reference sediment are conducted using methods recommended in the "Green Book" and establishing the 95 percent confidence that is required in the Regulations. Compounds that do not bioaccumulate to statistically higher levels in test organisms than in reference organisms are considered to not bioaccumulate to ecologically significant levels. EPA Region 2 believes this is an appropriate conclusion due to the specific characteristics of the reference sediment being used in the HARS evaluation process.

Type I error (i.e., errors in which effects are predicted mistakenly) does not result from comparison of test sediment and reference sediment bioaccumulation test results. Residues of contaminants in test organism tissues that statistically exceed reference organism residues are retained for further evaluation of the risks associated with those levels of accumulation. The possibility for Type II error (i.e., errors in which no effect is predicted mistakenly) to be inadvertently committed is attendant to the use of statistical comparisons, however there are certain aspects of the HARS evaluation process (described below) that serve to minimize the potential for Type II error.

The potential for Type II error is minimized in the current and proposed evaluation processes for several reasons: 1) reference sediment used in evaluating the suitability of proposed Remediation Material for use at the HARS is >95 percent sand and is collected from an area that is geographically removed from the direct influence of sources of contamination; 2) the method for assigning values to non-detected concentrations for statistical comparisons to reference and for use in comparison to threshold values was specifically designed to minimize the potential for Type II error (i.e.when undetected residues are reported at higher than required detection limits, the actual residue of test and reference samples are estimated at the reported detection limit and zero, respectively); and 3) when total concentrations for organic contaminants (i.e., total PCBs, total DDT, total PAHs (or narcotic contaminant load), total BaP-equivalency, total endosulfans, total chlordane/heptachlor, and total and TEQ dioxins/furans) are used in the risk evaluation process the calculated total used for the evaluation includes contributions from all relevant compounds, regardless of the statistical significance of the individual compounds contributing to the total. .

The methods for dealing with elevated detection limits and for evaluating risk associated with mixtures of organic contaminants are conservative and may overpredict actual risk.

4. Pretest Contaminant Levels in Test Organisms

The organisms that are used for bioaccumulation assays are collected from near-pristine, field locations. As such, the organisms used in the testing are anticipated to have only trace levels of background contaminants in their tissues (referred to hereafter as "pretest" contaminant levels) and the presence of these low levels of contaminants are generally assumed to not significantly affect the kinetics of uptake and depuration during 28 day exposures. However, despite having been collected from areas that are not influenced by known point sources of contaminants, elevated concentrations of certain contaminants have been measured in pretest tissues. The potential of elevated (and to a lesser extent, trace) pretest contaminant levels to affect measured residues following 28 days of exposure to dredged material introduces uncertainty into the interpretation of bioaccumulation. The concern for elevated pretest levels appears to be more significant in the evaluation of results of tests conducted with *M. nasuta* than with *N. virens* (Reiss, unpubl. obs.)

The significance of pretest contaminant levels varies with the level of contaminant and with the specific environmental behavior of the compound. The effect of pretest concentrations of certain metals (e.g., copper and zinc) on 28-day bioaccumulation results may be expected to be minimal since these metals are regulated by many organisms (including bivalves). Other metals (e.g., mercury and cadmium) are not excreted at appreciable rates by marine organisms and therefore pretest levels might be expected to not affect kinetics of uptake and depuration and therefore act solely in an additive manner. Organic contaminants differ from metals as residues resulting from exposure are expected to be dictated by the relative contaminant concentrations in the lipid of the organism and the organic carbon fraction of the sediment (as discussed above).

To examine the uncertainty associated with pretest levels, EPA Region 2 staff (Reiss, unpubl. obs.) examined 37 sets of paired pretest/28-day metals residues for *M. nasuta*. This review indicated that pretest contaminant levels are positively correlated with 28-day residues for essentially all of the metals examined (a possible exception was chromium) and was a better predictor of the 28 day residue than bulk or organic carbon/grain size-normalized sediment chemistry. The review also suggested that the relationships of pretest and 28-day residues were not consistent across metals and were not predictable for certain metals. 28-day metal residues showed that silver, chromium, copper, mercury, and lead are usually bioaccumulated by organisms exposed to New York-New Jersey Harbor dredged materials; cadmium and nickel are sometimes bioaccumulated; and arsenic and zinc are infrequently bioaccumulated. (In this study, bioaccumulation was defined as a 28-day residue of at least 20 percent greater than the pretest residue). In addition, the data support the theoretical prediction that mercury pretest concentrations contribute additively to the load accumulated during the 28-day exposure. This relationship was also supported for lead. More extensive evaluation of the effect of pretest levels on 28-day outcomes is necessary before quantitative consideration may be given to this area of uncertainty.

The potential for pretest levels of a given contaminant to alter the uptake and depuration kinetics of other contaminants during the 28-day exposure is an additional area of concern associated with pretest contaminants. This interaction of contaminants has been reported extensively in the literature (see review in Brown and Neff, 1995), however these interactions cannot be confidently quantified or corrected for.

Past regional policy has been to allow for retesting of proposed dredged materials when pretest levels are shown to have been elevated and are suspected of having contributed significantly to a determination that the material is unsuitable for its intended use based on the bioaccumulation results. Given the state of the science, continuing this course of action appears to be the most equitable and environmentally prudent manner to address this area of uncertainty.

5. Selected Analyte List and Analytical Methods

Analytes for New York/New Jersey Harbor dredged sediments were selected based on a review of regional data (Squibb *et al*. 1991) sponsored by the Toxics Workgroup of the New York/New Jersey Harbor Estuary Program and represent a consensus recommendation of New York/New Jersey Harbor stakeholders. The current list of analytes includes: polycyclic aromatic hydrocarbons (PAHs), polychlorinated biphenyls (PCBs), dioxin and furans (and dioxin-like compounds), several pesticides (DDT, aldrin/dieldrin, endosulfans, and total chlordane), 1,4-dichlorobenzene, and priority pollutant metals (including silver, arsenic, cadmium, chromium, copper, mercury, nickel, lead, and zinc-measured as totals). Organotin compounds are proposed as additional analytes.

It is prohibitively expensive, and essentially impossible, to measure for all contaminants that may be present in dredged materials in heavily impacted systems (like New York-New Jersey Harbor). Therefore, regulatory programs only measure subsets of contaminants that are deemed to be of greater potential environmental significance. Factors such as: the prevalence and concentration of the contaminant in the waterbody; the intrinsic toxicity or potency of the contaminant; and the potential for accumulation and biomagnification of the contaminant in the food chain are considered by managers when evaluating the need for including a specific compound as a contaminant of concern. Generally speaking, a compound will only be selected as an analyte when there is evidence that suggests that it may be present at potentially harmful levels in the environment.

Obviously, risks associated with bioaccumulated levels of contaminants which are not measured in assays cannot be considered. It is difficult to quantitatively consider this area of uncertainty. Uncertainty is minimized by appropriate consideration and characterization of the screening criteria identified above when identifying target analytes.

As significant synergism (or antagonism) between contaminants is not expected to commonly occur at environmentally relevant exposure levels (Konemann and Pieters, 1996), the potential occurrence of unmeasured compounds is of lesser importance in considering the potential for effects associated with contaminants of concern having specific modes of toxic action. The potential occurrence of unmeasured compounds is of greater importance however in assessing the potential for effects from contaminants that share a common mode of toxic action (e.g., narcosis, carcinogenesis).

It is important to note that acute toxicity to sensitive benthic organisms using whole sediment is also assessed as part of the overall process for evaluating dredged material. The acute toxicity bioassay assesses the combined toxicity of all contaminants in the sediment. Sediments that are found to be unacceptably toxic in acute toxicity tests are excluded from use as Remediation Material at the HARS. Therefore, the use of this test offers a level of protection for contaminants that are present in the sediment but not measured in the bioaccumulation test tissues.

6. Selection of Analytical/Quantitation Methods

This section discusses areas of uncertainty associated with the analytical methods selected for use in evaluating bioaccumulation test tissue residues.

a. Quantitation of Metals Residues

Metal residues in tissues of organisms exposed to dredged material proposed for use at the HARS are measured as total metals. Evaluation of the risks associated with the metals, however, is generally conducted by using potency characteristics (e.g., carcinogenic slope factors, reference doses, toxicity indices) that are associated with a specific valence state or organometallic complex of higher toxicological importance. This approach results in the inherently conservative assumption that all of the metal that is measured in test organism tissues is present in its most toxicologically potent form. The appropriateness of this assumption differs across metals, depending on how much of the measured total is in the form being used for the risk assessment.

In order to minimize the expense and difficulty of analyses, subsets of specific analytes within organic contaminant classes are often measured in lieu of measuring the entire suite of potential compounds. This is the approach taken to analyze PAH and PCB residues in tissues of organisms exposed to dredged material proposed for use as Remediation Material at the HARS. The quantification of a subset introduces uncertainty into the risk assessment. This uncertainty is qualitatively described below.

b. Quantitation of Polychlorinated Biphenyls

Polychlorinated biphenyls (PCBs) are contaminants of concern in the New York/New Jersey Harbor estuary system and at the HARS and have been introduced from a variety of sources. There are 209 possible configurations (called congeners) for PCB compounds, however only a subset of 100 or so of these compounds are environmentally abundant. Still fewer are toxicologically important (McFarland and Clarke, 1989).

Regional guidance for assessing contaminants in dredged material (CENAN/USEPA 1992) recommends an analytical method developed by NYSDEC (1991) to quantify concentrations of PCBs on a congener-specific basis. In the recommended approach, 22 PCB congeners are measured and their mass is then doubled to estimate the total mass of PCBs in the sample. This approach is consistent with the approach used by NOAA in the Status and Trends Program. The reliable prediction of total PCBs from the measured subset has been verified on a national level by NOAA contractors (Dr. T. Wade - Texas A&M University) using NOAA Status and Trends sediment and tissue data. This method is preferred by NOAA and EPA because the subset allows for prediction of the total PCB mass, while minimizing the expense and difficulty of the analysis.

Because of the demonstrated ability to reliably predict total PCB mass from measured concentrations of a subset of congeners, EPA Region 2 judges the use of this analytical approach to introduce relatively little uncertainty into the evaluation process.

c. Quantitation of Polycyclic Aromatic Hydrocarbons

For PAHs, standard EPA methods generally analyze the 16 PAHs listed by EPA as priority pollutants. These methods do not quantify alkylated PAH homologues which are present in environmental PAH mixtures at varying relative abundances.

PAHs are produced naturally through diagenic (and other) processes, however, the overwhelming sources of PAHs in most aquatic systems, including New York/New Jersey Harbor, are anthropogenic. Alkylated PAHs are of higher relative importance at sites affected by petrogenic sources of PAHs (i.e., sites contaminated by releases of petroleum products), than at sites affected by pyrogenic sources (i.e., sites affected by combustion) (Battelle, 1999).

NOAA Status and Trends (NOAA 1995) quantified parent PAHs and five alkylated compounds in 36 samples of New York/New Jersey Harbor sediments. Alkyl PAHs only comprised a minor fraction of the total PAHs measured in these samples (averaging 12.8±13.6 percent). However, if the mass of alkylated naphthalenes (which were four of the five alkylated PAHs quantified by NOAA) are compared with the mass of parent naphthalene, alkylated compounds comprised 58.2 ± 8.8 percent of total naphthalene. This suggests that the measurement of the 16 parent PAHs to estimate total PAHs may underestimate actual PAH concentrations by as much as 60 percent. Alkylated napthalenes were shown by Hellou *et al.* (1994) to predominate (90-100 percent of mass of 27 measured polycyclic aromatic compounds) PAH residues in muscle tissue of flounder exposed to oil-contaminated sediments. Alkylated naphthalenes accounted for 30-60 percent of the mass in the sediments used in that study. This enrichment suggests that alkylated PAHs are transferred to exposed organisms and may in fact be more efficiently transferred than parent compounds. This characteristic supports the need to quantify and consider alkylated PAHs when assessing the risks associated with PAH mixtures.

EPA Region 2 proposes to require specific sample cleanup procedures and instrument configurations to be used when analyzing for PAH residues using Method 8270 (see Appendix G of this document). These procedures will allow the mass of alkylated PAH residues to be estimated in a sample and used in the risk assessment process. EPA Region 2 believes that using these procedures will significantly reduce the uncertainty by which risks due to PAHs are assessed.

d. Analytical Performance

Rigorous QA/QC limits (see CENAN/USEPA 1992) are intended to ensure that only demonstrably accurate and precise results are used in evaluating the potential for bioaccumulation. There is, however, inherent variability in analytical results that is associated with the performance of the laboratory methods themselves. The acceptable precision limits reflect the inherent variability in method performance. Acceptable limits for triplicate analyses are relative percent differences (RPDs) within 30 percent for organic contaminants and RPDs within 20 percent for inorganics.

Because the acceptable range is an absolute range, it cannot be ascertained whether a reported concentration somewhat under- or overestimates the actual residue in the sample. However, the potential 20-30 percent difference between reported and true concentration in the sample introduces a margin of uncertainty that is narrow with respect to other areas of uncertainty associated with the overall evaluation process.

II. UNCERTAINTY ASSOCIATED WITH ASSESSING EXPOSURE TO CONTAMINANTS AT HARS

A. Estimating Exposure of Humans to HARS Contaminants

Estimating exposure of humans in light of the various site/population-specific characteristics that potentially influence exposure introduces several areas of uncertainty into the assessment of risks associated with dredged material at the HARS. It is therefore appropriate to assess the specific nature of the site and identify pathways by which exposure is most likely to occur. Because the HARS is located offshore and in deep water, and because suspended and dissolved constituents of dredged material do not persist in the water column following release from the barge, pathways of human exposure other than consumption of seafood (e.g., inhalation, or direct exposure through bathing) are not deemed appropriate for assessing the associated risk. Ecological receptors, however, could be exposed to contaminants through direct contact and/or food web transfer pathways.

1. Seafood Consumption Patterns of Humans

a. Seafood Consumption Rate

i. Use of the National Default Consumption Rate

When site-specific data are unavailable to estimate seafood consumption rates, risk assessments often employ a national default estimate of seafood consumption to estimate exposure to contaminants in seafood. The default consumption rate is based on national *per capita* seafood consumption and is calculated as the total amount of edible fish and shellfish product that is landed in or imported by the U.S., divided by the national population.

The current process for evaluating the suitability of dredged material for use at the HARS uses acceptable benthic tissue concentrations (based on human health) that were derived using a default seafood consumption rate of 6.5 g/day. Clearly, use of this consumption rate may underestimate exposure by many seafood consumers. The proposed HARS evaluation framework and guidance values employ an estimate of seafood consumption that is based on responses obtained in a survey of fish consumption by New Jersey residents. The

survey (NJMSC, 1994) was conducted by the New Jersey Marine Sciences Consortium, the New Jersey Department of Agriculture and Rutgers University. Areas of uncertainty that are generally associated with estimating consumption rates and areas associated specifically with the use of the results of the NJMSC study are discussed below.

ii. Identification of Target Sub-Population of Concern

To replace the default value with a more site-relevant consumption rate, it is necessary to identify whether there are groups whose patterns in consuming seafood may lead to exposures that significantly differ from the default rate and may be more appropriate receptors for assessing risks. Factors that must be considered are: amounts and types of seafood consumed by those persons; methods generally used by those persons to prepare seafood; and the sources from which these persons obtain their seafood. Effort must also be placed in identifying whether there may be significant seasonal changes in the seafood consumption rate or source. These, and other, areas of uncertainty are discussed below.

The current and proposed processes for evaluating risks associated with contaminants in dredged material proposed for use at the HARS assume that recreational fishers (and their families) represent a reasonably maximally exposed (RME) population for assessing risks to humans. More explicitly stated, EPA Region 2 assumes that there is a population that fishes exclusively at the HARS and that these anglers have seafood consumption patterns (types and amounts) that are similar to those of the average New Jersey angler (as represented by respondents to the NJMSC (1994) survey). The assessment further assumes that all fish consumed by those anglers that could potentially occur at the HARS are, in fact, obtained by angling at the HARS. In addition, the assessment assumes that fish are filleted prior to cooking (and consumption). These assumptions are discussed below.

In identifying this sub-population, the recreational angler, it was acknowledged that the children of these anglers would also be consuming fish caught from the HARS. Consistent with EPA's Children's Health Initiative, a child receptor was evaluated in developing the HARS-Specific Values for human health. For reasons discussed below, this assessment was performed qualitatively.

The somewhat unique exposure scenario associated with the HARS (i.e., estimating exposure from fish consumption from fishing at a 15.7 square mile area 3.5 miles from the coast) necessarily introduced a considerable amount of uncertainty (discussed elsewhere in this report) into the risk assessment. Addressing this uncertainty, while maintaining the tenet of estimating a reasonable maximum exposure scenario, dictated a high degree of conservatism into the risk assessment process. In establishing human exposure assumptions, this conservatism is apparent, for example, when considering the amount of time that a fish forages at the HARS. This site use adjustment is conservative as it assumes that: 1) when fish migrate to areas away from the HARS vicinity, they return to the HARS; and 2) while fish are in the general vicinity of the HARS, they feed exclusively at the HARS itself. These two inherent assumptions are clearly conservative, however they are appropriate in the absence of empirical site-specific data to better estimate those factors.

iii. Estimating Consumption Rate for Sub-Population of Concern

NJMSC survey results confirmed that recreational anglers consume greater quantities of seafood than do non-fishing seafood consumers. Because the HARS is an area that 'holds' fish, it is a popular fishing area for recreational fishers targeting coastal species. Based on this characteristic of the HARS and the higher seafood consumption rate of anglers, recreational anglers appear to be appropriate target populations for assessing risks from seafood obtained at, or exposed to, the HARS.

Average seafood consumption reported by NJMSC survey respondents that identified themselves as recreational fishers was 15.23 ounces/week (62 grams/day). The overall average fish consumption by New

Jersey fish consumers was 10.85 ounces/week (44 grams/day). Of this weekly total, 7.57 ounces are saltwater finfish. Approximately 37 percent (2.8 ounces) of the saltwater finfish consumed by recreational anglers were reported to be fresh (i.e. not canned or processed) fish of varieties that are listed in the HARS SEIS (EPA 1997a) as species that may occur in the vicinity of the HARS. The weekly consumption rate of 2.8 ounces, or 79.38 grams, equates to a daily consumption rate of 11.34 grams of finfish that could potentially occur at the HARS. This consumption rate does not include consumption of processed fish or of species that are not expected to occur at the HARS, such as red snapper, orange roughy, and off-shore species (e.g., tuna, swordfish).

NJMSC (1994) respondents (anglers) indicated that 60% of the fish they consume is prepared in the home. EPA Region 2 assumed that recreationally-caught fish is consumed by recreational anglers in their home. If the percentage of fish that occurs at the HARS that is consumed by recreational anglers in their home is similar to the overall percentage of home-prepared fish (i.e., 60%) in their diet, then the daily consumption rate of recreationally-caught (i.e., home-prepared) fish potentially occurring at the HARS by New Jersey anglers would be estimated to be 6.8 grams (i.e., 11.34 g/day x 60 percent).

Anglers responding to the NJMSC (1994) survey indicated that they annually consume an average of 5.76 lbs. (or 7.2 grams/day) of recreationally-caught fish. Information on angling habits in New Jersey marine waters is not available. Therefore, EPA Region 2 conservatively assumed that there may be a subpopulation of recreational anglers that preferentially fishes at the HARS and obtains all of the recreationally-caught fish in their diet from fishing at the HARS. Given that such a population of recreational anglers exists, their consumption of finfish that are potentially exposed to the HARS was estimated at 7.2 g/day.

The two estimates of consumption of finfish that are potentially exposed to HARS and caught recreationally by New Jersey anglers (outlined above) agree well. EPA Region 2 proposes to use the higher of the two estimates (i.e. 7.2 grams/day) as an appropriate estimate of fish consumption for assessing the risks to a reasonably maximally exposed (RME) human subpopulation associated with contaminants in sediments proposed for use as Remediation Material at the HARS. Although use of an average consumption rate does not reflect consumption by all consumers within the angler population, the averages exclude those fishers that did not report having eaten fish in the week preceding the survey and, therefore, probably represents higher consumers within the angler group. The average consumption rate used for assessment of risks is equivalent to an average consumption of 2.42 seafood meals/week, approximately 75 percent of anglers in the survey reported consuming less than three meals of seafood per week.

Taking the exposure assessment a step further by estimating the fish consumption rate in children of recreational anglers introduces more uncertainty due to the limited data on childhood fish consumption rates. Exacerbating this issue is the absence of information on the meal patterns of recreational anglers (high-end fish consumers) and their children. Survey data in a recent draft (dated March, 2000) of EPA's Child-Specific Exposure Factors Handbook indicates that, normalized to body weight, children under the age of fourteen may ingest approximately twice as much fish as adults, however these data are not specific to recreational anglers.

In this particular assessment, the comparatively modest increase in possible exposure to children (and the high degree of associated uncertainty) argues in favor of a qualitative analysis of the childhood exposure pathway. The aforementioned considerations, coupled with the conservative nature of the recreational angler exposure assessment, provides a reasonably confident basis for concluding that the HARS risk assessment methodology is protective of both children and adults alike.

Because the NJMSC survey relies on information regarding fish consumption during a single week period during the Fall to extrapolate annual consumption rates of consumer groups, it is necessary to discuss the

uncertainty associated with this extrapolation. While it is possible that there may be significant interseasonal differences in the amount of fish that is consumed, $90 percent of survey respondents that reported consuming 2-3 fish meals in the past week also reported that their previous week's consumption did not differ significantly from their typical consumption. Therefore, the seafood consumption rate reported by these anglers was assumed to be an appropriate estimate of annual consumption.

What is more probable, however, is that the proportion of fish that is obtained by recreational fishing at the HARS may differ significantly over the year. This difference could be the result of seasonal restrictions imposed by resource agencies or due to decreased fishing activity during winter months. However, because certain New Jersey fishers may freeze a portion of their catch for consumption during these times (May and Burger, 1996), the estimated proportion of fish was not adjusted to account for potential seasonal differences.

iv. Source of Fish Consumed by Sub-Population of Concern

The assumptions that 100 percent of recreationally caught fish are of species occurring at the HARS and that they are caught exclusively at the HARS are extremely conservative. Recreationally caught fish consumed by anglers actually includes freshwater (e.g., trout) and offshore, pelagic (e.g., tuna) species of fish. Fish in these two groups constituted up to 19 percent of consumed fish that were reported as recreationally caught "in New Jersey waters" by NJMSC survey respondents. most inshore species that occur at the HARS (e.g., summer flounder, weakfish, bluefish, and striped bass) will be caught from various locations by the typical recreational angler depending on various factors, including time of year (based on fish distributions) and weather (which influences fishing area accessibility based on sea state). In addition, certain species that contributed to the consumption rate and that were assumed to be recreationally caught at the HARS are not generally targeted at the HARS. Examples include: structure-associated species, such as porgy, blackfish, and sea bass; deeper water bottom species, such as cod and haddock; and species generally targeted within bays and estuaries, such as winter flounder.

v. Exposure to Potentially HARS-Exposed Species Other than Recreationally Caught Finfish

There are significant commercial finfish and lobster fisheries in the New York Bight. Commercial harvesting occurs throughout the New York Bight, including the Apex and the HARS. The consumption rate used to assess risk to recreational anglers from contaminants at the HARS (i.e., 7.2 g/d) does not explicitly consider the contribution of fish and lobster from commercial sources. As such, the actual consumption rate of HARS-exposed finfish could be as high as 11.34 g/day. This would depend on all commercial harvesting of species that potentially occur at the HARS having actually been obtained there. Because the area of the HARS represents only a small fraction of commercially fished area, use of this assumption would be extremely unrealistic.

Sixteen percent of crustaceans contributing to New Jersey angler diets were attributed to lobster meals (constituting 3.3 g/day). While there is a lobster fishery at the HARS, the assumption that all lobster consumed by anglers is commercially fished at the HARS is also extremely unrealistic.

It is possible to derive a proportion of total finfish and lobster landings that might come from the HARS for use in the risk assessment. However, given the limited geographic extent of the HARS with respect to the areas occupied by these fisheries, the resultant contribution to risk was assumed to be negligible compared to directed recreational fishing at the HARS.

b. Effects of Seafood Preparation on Contaminant Exposure.

The current and proposed processes for evaluating risks to human consumers from seafood at the obtained at the HARS assume that recreational anglers prepare and consume the fillet portion of the fish, rather than the whole fish. This is an important assumption since tissue in the fillet (muscle) portion of fish has been shown to

harbor substantially lower concentrations of organic and inorganic contaminants than other organ tissues (see EPA, 1997b). The current and proposed processes adjust the predicted whole-body contaminant concentration (based on trophic transfer from benthic prey at the HARS, as represented by bioaccumulation test organisms) to estimate the lower concentrations in the fillet being consumed by humans. Fifty-five percent of NJMSC survey respondents (anglers) indicated that they always fillet fish prior to consumption and 6 percent indicated that they rarely or never fillet fish. Conversely, 27 percent of angler respondents indicated that they always serve whole, gutted fish and 50 percent indicated that they rarely or never do so. Nevertheless, the majority of the fish that is actually consumed by humans (independent of the method of preparation) is the muscle portion of the fish.

The current and proposed processes for evaluating risks to human consumers from seafood obtained at the HARS do not consider the potential reductions in organic contaminant concentrations in fish tissues during cooking. This reduction is attributable to separation of contaminated lipid from fish tissues during cooking. In addition, volatilization may account for added losses when, under higher temperatures, the chemical is released. Loss of lipids is a function of the temperature and cooking duration, with higher temperatures and longer cooking times causing a greater loss of fat and accumulated chemicals from the edible tissue. Cooking methods such as frying, baking, or broiling are particularly effective at removing organic chemicals (EPA 1997b).

Results of several studies indicate that cooking can lead to substantial reductions in organic chemical concentrations in fish tissues. For example, the results of Sherer and Price (1993), indicate that cooking leads to reductions in PCB levels in fish ranging from zero (0) to 74 percent. Similarly, studies by Stachiw *et al.* (1988), and Zabik and Zabik (1995), have shown reductions in TCDD concentrations ranging from about 30 to 100 percent. Finally, several studies have shown that cooking can reduce pesticide concentrations two to 72 percent in fish tissue (Reinert *et al.*, 1972; Smith *et al.*, 1973; Zabik *et al.*, 1982; EPA 1997b). For these reasons, regulatory agencies frequently recommend that anglers cook their fish before consumption and that they use a cooking method that does not reuse the fish oils (NYSDEC, 1991). Ninety-five percent of NJMSC survey respondents (anglers) reported that they never reuse the fish oils.

Because the actual dose received by anglers during consumption is determined by the amount of chemical in each fish meal, any reduction that occurs during the cooking process will result in a reduction in the exposure concentration. Research has shown that anglers in the northeastern United States typically use cooking methods that reduce organochlorine levels in self-caught fish, with frying, baking and broiling 62, 18, and 16 percent of the time, respectively (ChemRisk, 1992; Connelly *et al.*, 1992). Somewhat different patterns were reported by New Jersey fish consumers (not all fish consumers were anglers) with baking accounting for approximately 50 percent, and broiling-barbecuing accounting for 42 percent, of seafood meals prepared in one study (New Jersey Department of Agriculture as reported in NJMSC 1994). Broiling/grilling, baking and frying accounted for the majority of prepared fresh saltwater fish meals, respectively, in a second survey of New Jersey fish consumers (NJMSC, 1994).

Contaminant losses due to cooking were not considered in deriving values for assessing human health risk in the current or proposed processes. Therefore, cooking loss may actually lower exposures of most recreational anglers to contaminants in fish obtained from the HARS to levels somewhat lower than those used for assessing risks in the current and proposed processes.

c. Exposure Duration

A critical component of any risk assessment is estimating how long or how often an individual may be exposed to the chemicals of potential concern. The exposure duration of recreational angler is also affected by the time an angler begins fishing and continuing until the angler no longer catches and consumes fish from the Site. The point at which an angler stops fishing varies with the individual angler. Three factors influence the time when

an angler stops fishing: 1) the probability that an individual will relocate from his/her current residence (mobility); 2) the probability that an individual will decide to no longer participate in the sport of fishing (angling cessation); and 3) the probability that an individual will die (mortality). The duration of exposure can only be properly estimated when these three factors are considered. The current and proposed processes for evaluating suitability of dredged material for use at the HARS assume that individuals will be exposed for 70 years. For the purpose of this uncertainty evaluation, a quantitative analysis was beyond the scope of this qualitative evaluation. However, it is instructive to discuss these uncertainties in a qualitative fashion.

i. Mobility

Residential mobility is an accurate predictor of exposure duration for many sources of contamination that occur in or near, the home. An individual's potential exposure to indoor air pollution or contaminated soil, air, and groundwater near their residence is a function of the amount of time spent at home. This exposure may conceivably continue throughout the individual's lifetime unless the person changes their residence. EPA human health risk assessors often use a 30 year, rather than 70 year, period to conservatively estimate durations of exposure to contaminants near their homes based on typical residential mobility.

When evaluating the influence of the mobility factor on exposure duration for fish consumption, however, it is necessary to go beyond a strict consideration of residential mobility because changes in household location may not lead to changes in fishing behavior. Only when an individual moves a sufficient distance will a change likely be made in preferred fishing locations. While interstate or U.S. regional mobility data could be used to estimate the number of individuals who give up fishing at a preferred fishing location (due to a significant move in distance), intrastate moves (within state) that would also result in a change in angling practices also need to be considered. It is likely that the actual number of anglers who stop fishing at a specific location would be underestimated by relying on interstate or regional mobility data. County mobility, however, may be an appropriate surrogate for representing the probability that an individual gives up angling because he/she moves sufficiently far enough away. These data are available from the U.S. Bureau of Census (1988, 1991) which publishes information on the number of individuals who move out of a given county, but still remain within the same state.

Factors such as age and gender can also influence mobility. For example, the frequency of moving is highly dependent on age. Individuals between the ages of 20 to 29 have a greater probability of moving than individuals over 30. Gender also has an impact on mobility. Due to gender-specific tendencies, men are somewhat more likely to move than are women (U.S. Bureau of the Census, 1991). To account for these patterns and to identify the range of variability found in the angler population, it is necessary to identify a distribution of intercounty mobility rates for males and females of each age. Specifically, data on county mobility by age group and gender in the Northeast region may be appropriate to further characterize potential exposure duration. However, in support of using a 70 year exposure duration, surveys (EPA, 1997c) indicate that approximately 70 percent of those who change residences move within a 20-mile radius. Thus, there is a likelihood that moving would not necessarily restrict ready access to fishing at the HARS.

ii. Angling Cessation

In addition to moving, an angler may give up fishing due to lack of interest, bad weather, increasing age, or a number of other reasons. In fact, at every age there is a certain probability that an individual will permanently give up the sport. However, due to the difficulty of collecting these data, no study has specifically evaluated this phenomenon. Not only is it difficult for individuals to predict whether they will give up fishing, individuals who report giving up fishing one year may only temporarily withdraw from the sport. These same individuals may start and stop fishing many times over the course of their lifetimes.

As an example, an initial analysis using data collected by ChemRisk (1992) indicates that the percentage of anglers in the population increases from age 18 until the mid-20s, where it remains relatively constant for about 20 years. In the mid-40s until the late 60s angling begins to decline significantly. Finally, after about the age of 67, the number of anglers is again roughly stable until age 81, the oldest age recorded in the survey.

A survey in the State of Maine determined that 72 percent of all licensed anglers fish every year once they start fishing (Boyle *et al.*, 1990). This study supports the fact that the majority of anglers are extremely dedicated to their sport, indicating that the number of anglers in the total state population should be relatively constant between years. This type of information can be used to determine the age-specific probability that an individual will permanently cease angling. The change in the number of anglers with increasing age can then be used to estimate the probability that an individual will give up angling. No such survey has been conducted to characterize the New Jersey angler community.

iii. Mortality

Mortality also determines how long an individual potentially catches and consumes fish from the Site. Standard actuarial mortality tables can be used to predict the life expectancy of a given angler and whether that individual would likely remain a member of the population of living anglers. Age- and gender-specific data on mortality are available from the National Center for Health Statistics (1990) and can be used to create a complete distribution of the probability of dying at each age. Such a consideration, however, is beyond the scope of this effort.

B. Estimating Exposure of Wildlife to HARS

When assessing risks to ecological receptors that may be associated with bioaccumulation of contaminants from sediments it is necessary to consider effects to sessile or "full-time" resident biota and to species that may only be exposed to the site of concern for varying portions of their lifetime. In addition, the relative importance of direct and indirect exposure pathways must be evaluated when assessing risks to a given species. The estimates and assumptions that are necessary to evaluate these risks introduce uncertainty into the process. These areas are identified and discussed below. Many of the exposure assumptions are also important for considering risks to human consumers of these seafood species.

1. Characterizing the Food Web at the HARS: Identification of Receptors

For the current and proposed processes for assessing risks to ecological receptors (and in support of assessing risks to human consumers through the food web), a simplified description of the food web is used to describe trophic relationships between species anticipated to be present at the HARS. The food web used in the model was described by a simplified New York Bight food chain consisting of three representative trophic levels: benthic organisms, benthic predators, and upper level predators which was developed by EPA Region 2 (through the former Mud Dump Site Criteria Workgroup), in consultation with representatives from the state and federal resource agencies, industry and the environmental community (EPA, 1995a).

Higher level ecological receptors (e.g., piscivorous birds and mammals) were not identified as appropriate terminal ecological receptors for the food chain in the 1995 effort, nor were any identified by U.S. Fish and Wildlife Service, NOAA-NMFS, state resource agencies, or the public in comments received during the HARS site designation process. Therefore, predatory fish are used as the terminal ecological receptor in the assessment of ecological risks at the HARS.

The composition of the food web significantly affects estimates of exposure and trophic transfer of contaminants. The use of this simplified food web introduces uncertainty into the risk assessment process.

2. Estimating Site Use by Ecological Receptors

Estimating the extent and duration of exposure to the HARS for organisms that are sessile, or of very limited mobility, is simple. Specifically, sessile organisms (e.g., oysters, mussels) are by definition exposed to the HARS for their entire benthic existence. Other organisms, such as worms, clams and amphipods, are also likely to be exposed to the HARS for their entire benthic existence. Estimating the extent and duration of exposure for highly mobile species, such as finfish, is much more difficult and introduces uncertainty into the risk assessment process. This section discusses this area of uncertainty.

The current process for assessing risks associated with sediments at the HARS assumes that fish species present at (or caught at) the HARS have been present at the HARS for their entire lifetime. This relates to a site use factor of 100 percent, which results in the conclusion that all of their food is coming from the HARS. This assumption, however, is recognized to be overly conservative. However, development of a more realistic estimate of site use is not a straight forward exercise.

The HARS occupies only 15.7 square nautical miles of total habitat available to, or used by fish during their lifetime. For certain species, such as bluefish and striped bass, that undergo extensive seasonal migrations an area of this size probably represents only a minute fraction of the total area in which they forage during their lifetimes. For other species that exhibit less dramatic migrations (a possible example may be blackfish) this area could represent a more significant component of their range. As a result, general rules regarding site use are difficult to apply across species.

Being located at a temperate latitude, the environment and ecological characteristics of the HARS exhibit pronounced seasonal differences. Generally speaking, fish species that are associated with the HARS are only present for a portion of the year. Clearly, when fish are not present in the geographical area of the HARS, they cannot possibly be feeding on benthos at the HARS. EPA Region 2 used quarterly landings data from 1993 to adjust site use by fish based on their presence/absence in commercial landings. Using the portion of the year that accounted for 95 percent of landings , EPA Region 2 adjusted site use to reflect the percent of the year that a “typical fish” (which was weighted by each species relative contribution to the diet of an angler) may be expected to be present in the vicinity of the HARS. This approach resulted in an adjustment to reflect that fish are only present in waters near (and therefore could only be feeding at) the HARS for approximately 78 percent of the year. Although this estimate is based on landings of a single year, seasonality of fish occurrence is fairly constant from year to year (J. Waldman, Hudson River Foundation, pers. comm.). Using commercial landings data (and specifically, using the number of quarters in which 95 percent of fish of a given species are landed) is a conservative approach to estimating seasonal presence as the residence of the individual rather than the population determines exposure duration. Furthermore, using quarterly data, rather than monthly data, to estimate population residency is also slightly conservative.

The above site use adjustment is conservative, however, as it assumes that: 1) when fish migrate to areas away from the HARS vicinity, they return to the HARS; and 2) while fish are in the general vicinity of the HARS, they feed exclusively at the HARS itself. These two inherent assumptions are clearly conservative, however empirical data to better estimate those factors is lacking. The first of these two assumptions is important for predicting residues of contaminants (e.g., cadmium, mercury, dioxin) that are the result of the lifelong exposure history of the organism. The second assumption, however, is of greater importance for predicting residues of contaminants (e.g., PAHs, metals) that reflect the recent exposure history of the organism.

The HARS occupies less than 3 percent of total available seabottom between 60 to 80 feet in the New York Bight. The probability of fish returning to this specific site year after year is low. However, it is unlikely that fish are entirely indiscriminate in their choice of habitat and therefore use of such a gross metric (percent of

bottom in depth range) cannot be confidently used to modify site use. Adjusting site use based on a geographic scale approach was not considered to be appropriate for the proposed assessment because the additional factors that need to be considered to predict habitat selection or site fidelity are not well understood and probably vary across species. As noted above, the area of the HARS appears to attract and hold fish.

To adjust site use based on foraging range, information on the size of feeding or home ranges of fish is necessary. It is likely that the foraging or home ranges of fish are species- and size- specific, and density- and habitat-dependent functions. This area of fish behavior is not well studied and no relevant empirical studies were found to support adjusting the site use factor in the risk assessment.

Site-use by fish is recognized to have been conservatively estimated in assessing exposure to, and risk associated with, contaminants at the HARS.

3. Estimating Trophic Transfer of Contaminants

To assess the risk of contaminants at the HARS, it is necessary to estimate the potential trophic transfer of contaminants in benthic prey to predator organisms. The current and proposed evaluation processes use biomagnification (or trophic transfer) factors (i.e., ratios of prey and predator tissue contaminant concentrations) to estimate the relative residue levels expected in organisms at different trophic levels in the food web. The trophic transfer factor is a critical parameter in calculating body burdens of, and risks associated with, upper level ecological receptors (i.e., fish). Trophic transfer factors for predicting chlorinated organic contaminant residues in predators from those in prey were derived using the Gobas food web model. Similar trophic transfer factors were derived from the literature to relate PAH and metals residues. Areas of uncertainty associated with the biomagnification factors used in the current and proposed processes are discussed below.

a. Trophic Transfer of Chlorinated Organic Contaminants

Trophic transfer factors (ratios of prey and predator tissue contaminant concentrations) for chlorinated pesticides were derived for use in the current and proposed evaluation processes using the Gobas food web model (EPA 1995b). The New York Bight food web used in the model was described by a simplified food chain consisting of three representative trophic levels. These trophic levels were selected by consensus (including representatives of the environmental and regulated communities, the two state resource agencies, USEPA and NOAA-NMFS) and were described in EPA (1995a) as: benthic organisms, benthic predators, and upper level predators. Average lipid contents of each trophic level were calculated from lipid contents of representative organisms in each level, as reported by NYSDEC (1996)[1]. The model was run assuming equilibrium and two states of disequilibrium. Resulting trophic transfer factors used (and proposed for continued use) in evaluating suitability of dredged material were reported on a lipid-normalized basis.

Burkhard (1998) compared bioaccumulation predictions in an aquatic (Great Lakes) food web obtained by the Gobas model to another generally accepted food web model (i.e., the Thomann model). Burkhard found that predictions of bioaccumulation in piscivorous fish for compounds with log K_{ow}s <8.0 and input sensitivities were generally comparable using the two models. Burkhard (1998) concluded that predictions obtained using the Gobas model have less overall uncertainty associated with them (i.e., a factor of 3.3 to 5.5 for compounds with log K_{ow}s < 7.6) than those obtained using Thomann.

[1]Draft lipid data that was eventually published in NYSDEC (1996) was used to run the Gobas model.

The most sensitive input parameters to the Gobas model are log K_{ow}, lipid content, and equilibrium state (sediment-water chemical concentration quotient) (Burkhard, 1998). In addition, assumptions regarding the structure of the food web are the foundation for running the model and therefore are important when assessing uncertainty with predicting trophic transfer of contaminants. These areas of uncertainty with the application of the Gobas model are discussed below.

i. Log K_{ow}

The Gobas model is sensitive to the log K_{ow}. Since different K_{ow} values have been published for chlorinated organic compounds, it is important to carefully select the appropriate K_{ow} for use in the model. Karickhoff and Long (1995) reviewed log K_{ow}s and derivation methods published for various compounds and the methods used to derive those values. Based on their review, they recommended values for use by EPA. Log K_{ow}s obtained using the "slow-stirring" or "shake-flask" methods were usually recommended for use by the authors, depending on the compound. These recommended values were used to run the Gobas model.

Karickhoff and Long (1995) did not review or recommend log K_{ow} values for PCB congeners or mixtures. De Bruijn *et al.* (1989) reported log K_{ow}s for 19 PCB congeners that were measured using the slow-stirring method. They reported that increasing chlorination level is the primary determinant of log K_{ow} of a given PCB congener, but also noted that the patterns of chlorine substitution also contributed to a lesser extent to the log K_{ow}.

In deriving guidance values for evaluating risk associated with PCB mixtures, a single trophic transfer factor of 3 was used to express the potential for trophic transfer of contaminants to upper level predators. Assuming equilibrium, use of this trophic transfer factor is more or less appropriate for PCB congeners with 4 to 6 chlorine atoms (i.e., log K_{ow}s 6.2 -7.1), but is overly conservative for less or more highly chlorinated congeners. For example, trophic transfer rates are estimated to be approximately 2.14 for congeners with three or seven chlorines.

Hawker and Connell (1988) published log K_{ow}s for each of the 209 PCB congeners. This data can be used with the Gobas model to estimate trophic transfer factors of individual congeners. EPA Region 2 proposes to use the log K_{ow}s reported by Hawker and Connell (1988) to better estimate trophic transfer of accumulated PCBs in test organism tissues to upper level receptors. Specifically, reported concentrations of each congener will be multiplied by a factor that reflects the difference of three and the trophic transfer factor for the specific congener based on its log $K_{ow.}$ An adjusted total PCB residue can then be calculated and considered against the guidance values that were derived using a trophic transfer factor of three. This approach will minimize the uncertainty associated with estimating trophic transfer of this complex mixture of compounds.

Similar adjustments will be applied in calculating and evaluating risks for mixtures (e.g. DDT, chlordane, endosulfans) of compounds with differing log K_{ow}s and shared guidance values that were derived with assumptions of uniform, worst-case trophic transfer.

ii. Pathway of Concern

The Gobas model provided estimates of trophic transfer to benthic predators and upper level predators. A biomagnification factor >1 indicates a potential for higher concentrations in the predator than those found in their prey. Trophic transfer of contaminants in benthic invertebrates to upper level predators assumes that upper level predators are exposed to those contaminants through consumption of benthic predators.

For purposes of this model, most fish are assumed to be upper level predators. Therefore, their modeled exposure reflects consumption of benthic predator organisms. Certain fish, however, do not follow this assumed trophic pathway and to a large extent feed directly on benthic organisms (e.g., winter flounder,

porgy(scup)). Trophic transfer of compounds with log K_{ow}s between 5.6 and 7.7 to these fish are predicted to be #50 percent of the trophic transfer factor used in the evaluation (based on upper predator fishes).

iii. Equilibrium State Assumption

Biomagnification (trophic transfer) factors that resulted from model runs that were conducted at equilibrium conditions are used, and are proposed for continued use, to conservatively estimate the potential for biomagnification of contaminants in upper trophic levels of the food web.

At equilibrium, the model predicted that the greatest degree of biomagnification (i.e., 2- to 3-fold enrichment in upper predator residues) would occur for compounds with log K_{ow}s between 5.6 and 7.5. Incorporating an assumption of disequilibrium, the model predicts a narrower range of highly biomagnifying compounds. At disequilibrium, highly biomagnifying compounds are restricted to those compounds with log K_{ow}s between 6.4 and 7.5. Compounds with log K_{ow}s below 4.4 and 5.9 (at equilibrium and disequilibrium, respectively) or above 8.0 (at all equilibrium states) were predicted to not biomagnify in upper level predators. Model predictions for trophic transfer to upper level predators from prey conducted at disequilibrium were not significantly affected by the extent of disequilibrium assumed.

To date, the equilibrium state of sediment-sorbed contaminants at the HARS with the overlying water column has not been specifically evaluated. As noted above, this assumption regarding the equilibrium state has important ramifications for modeling trophic transfer of chlorinated organic contaminants. An estimate of the relative equilibrium (disequilibrium) state of dioxin (2,3,7,8-TCDD) in surficial sediments in and around the HARS was obtained using the equations in Burkhard (1998)[1]. Calculated disequilibrium factors ranged from 9 to 230 (median of 33, n = 28), suggesting that surficial sediments in and around the HARS are in disequilibrium with the overlying water column.

In light of the above calculation, the use of trophic transfer factors predicted by the Gobas model at equilibrium is conservative and likely to overestimate the potential for biomagnification of contaminants in upper trophic level organisms. Differences in equilibrium and disequilibrium model predictions are greatest for compounds with log K_{ow}s lower than 5.3 (which includes 1,4-dichlorobenzene, endosulfans, heptachlor epoxied and mono/dichlorinated biphenyls). Predicted biomagnification for these compounds at equilibrium are at least an order of magnitude higher than at disequilibrium. Biomagnification of compounds with log Kows between 5.3 and 6.1 (which includes DDD, tri/tetrachlorinated biphenyls,and dieldrin) is predicted to be two to seven times greater at equilibrium than at disequilibrium). Biomagnification of compounds with log K_{ow}s above 6.1 (including penta- through decachlorinated biphenyls, chlordane, dioxin, DDT, and DDE) differ by 50% or less.

iv. Lipid Weights Assigned to Food Web Organisms

As stated above, Burkhard (1995) found that the Gobas model is sensitive to lipid weights assigned to organisms at the various trophic levels of the food web being modeled. Estimated lipid concentrations for polychaetes used in the model were within the range reported in Battelle (1996). Lipid concentrations for molluscs used in the model were somewhat higher than that reported for *N. proxima* by McFarland *et al.* (1994) and those reported for six other species in NYSDEC (1996). Lipid concentrations used for individual upper level predators were somewhat (approximately 50 percent) higher than those reported in NYSDEC (1996) and the overall lipid weight assigned to the upper level predator trophic level was heavily influenced by the inclusion of

[1]Dissolved and particulate organic carbon concentrations for New York Bight water reported in Farley *et al.* (1999), New York Bight dioxin water concentrations measured in New York Bight as part of the Contaminant Assessment and Reduction Program (CARP) of the New York/New Jersey Harbor Estuary Program (S. Litten, unpublished data), and dioxin and total organic carbon concentrations of surficial sediments in and around the HARS (Battelle, 1996) were used as inputs to these equations.

adult bluefish. Overestimates of prey and predator lipid concentrations have opposite effects on the trophic transfer predicted by the model (i.e., lipid enrichment in predators, relative to prey, increases trophic transfer of contaminants).

Trophic transfer factors used in the current and proposed evaluation processes were derived on a lipid-normalized basis. Specifically, the trophic transfer factors relate the concentration of contaminant in the lipid of predator to that in the lipid of their prey. Trophic transfer factors, however, are being used in the current and proposed evaluation processes to predict whole-body concentrations in predators. The use of lipid-normalized trophic transfer factors is only appropriate for relating whole-body concentrations when prey and predator lipid contents are approximately equal. When lipid contents of predator and prey are different, the trophic transfer factors need to be adjusted accordingly.

Lipid concentrations reported for fish in NYSDEC (1996) show that most (13 of 19) species of fish caught in the New York Bight have average lipid weights around or below 3.0 percent. These 13 species are considered to be adequately represented by the trophic transfer factors generated using the Gobas model (as run). The only fish having higher lipid contents were: larger bluefish ($30.5 cm) and striped bass ($45.7 cm), butterfish, searobins, spot, and porgy. These species are of secondary importance in the risk evaluation process given their limited site usage and/or their contribution to the diet of recreational anglers (see discussions above).

b. Trophic Transfer of Polycyclic Aromatic Hydrocarbons (PAHs)

One of the primary limitations of the Gobas food web model is that trophic transfer of compounds that are extensively metabolized cannot be accurately modeled. Polycyclic aromatic hydrocarbons are readily metabolized by many organisms, including many polychaetes, crustaceans and fish (see Varanasi, 1989 and references therein).

The current and proposed evaluation processes incorporate a 0.1 trophic transfer factor to reflect the inefficient assimilation of PAHs by higher trophic levels. This inefficient assimilation reflects the metabolism of PAH compounds. The value (0.1) equates to the average assimilation efficiency reported for finfish of 13 individual parent and alkylated PAH compounds by various researchers, as summarized by Brown and Neff (1995). Individual efficiencies reported ranged from 0.01 to 0.32. The median value was somewhat lower than 0.1 (i.e., 0.06).

Overall, the trophic transfer factor used for PAHs is judged to introduce little uncertainty into the HARS evaluation process.

c. Trophic Transfer of Metals

The current process for evaluating suitability of dredged material for use as Remediation Material assumes that residues of cationic metals in predator species will be equivalent to the residues of cationic metals in prey. This assumption was based on the results of a survey of available literature published by Suedel *et al.* (1994). A biomagnification factor (or trophic transfer factor) of one is used to reflect this assumption.

EPA Region 2 also conducted a review of available peer-reviewed literature regarding the potential for dietary transfer of metals to finfish from contaminated benthic invertebrate prey. This review concluded that the assumption of a trophic transfer of one is an overly conservative estimate of this parameter for many metals. Alternate trophic transfer factors were derived for four cationic metals (copper, cadmium, lead, and zinc) and arsenic.

Much of the early work that examined the importance of the dietary pathway for transfer of metals to fish in metals-contaminated systems focused on the relative concentrations of metals in fish and prey collected from

within the same system or area (e.g., Metayer *et al.* 1980, Dallinger and Kautzky 1985, and references in Table 1). These field studies suggested that dietary transfer of metals from prey could significantly contribute to fish body burdens of metals in fish and in certain exposure situations could be of greater importance than absorption from the water. Results of certain field studies, however, were inconclusive (see Metayer *et al.* 1980, and Dallinger and Kautzky 1985). Field-derived ratios suggested that the transfer of metals to fish from prey, was generally inefficient (i.e., ratios of consumer/prey concentrations tended to be less than one). Suedel *et al.* (1994) reviewed available data in efforts to assess the potential for trophic transfer of metals in aquatic foodwebs (Table 1). Based on this data, they concluded that trophic transfer of metals to fish should not generally be described as biomagnification and noted that "concentrations of most metals were often higher in tissues of producers and primary consumers...than carnivorous fish."

Trophic transfer ratios of less than 1 are consistent with the findings of Reinfelder *et al.* (1998). Using the kinetic model approach, they concluded that trophic transfer of cationic metals (although only cadmium was specifically modeled in that paper) to fish is expected to be less than one.

Since publication of Suedel *et al.* (1994), the potential for trophic transfer of metals has been increasingly investigated in the laboratory under controlled conditions. Laboratory studies have been conducted with commercial feeds or live prey (e.g., *Artemia*) that were contaminated with known amounts of metals in the laboratory and fed to fish (e.g., Handy *et al.* 1992; Cockell and Hilton 1988; Hatakeyama and Yasuno 1982; Kumada *et al.* 1973; Mount et al. 1994;) or with benthic organisms that were collected from the field from areas known to be contaminated by metals and fed to fish (Woodward *et al.* 1994, 1995; Farag *et al.* 1994, 1999). Results of these studies are summarized in Table 2.

In laboratory studies, body burdens of copper in fish ranged from 0 to 20.6 percent (n = 28, ave. 4.6 percent, median 2.1 percent) of the dietary copper concentration to which the fish were exposed. Body burdens of cadmium in fish ranged from 0 to 25 percent[2] (n = 31, ave. 6.6 percent, median 5.0 percent) of the dietary cadmium concentration to which the fish were exposed. Body burdens of lead in fish ranged from 0 to 22.7 percent[3] (n = 19, ave. 5.5 percent, median 3.9 percent) of the dietary lead concentration to which the fish were exposed. Body burdens of zinc in fish ranged from 0 to 89.9 percent (n = 16, ave. 18.4 percent, median 11.4 percent) of the dietary zinc concentration to which the fish were exposed. Body burdens of arsenic in fish ranged from 0 to 29.7 percent (n = 37, ave. 8.6 percent, median 6.1 percent) of the dietary arsenic concentration to which the fish were exposed.

Farag *et al.* (2000) showed that the degree of association of the metal with organic compounds (proteins) within prey significantly effects the efficiency of trophic transfer of metals to fish (i.e., increased covalent bonding and complexation of metals enhances the bioavailability of metals to fish consumers). Harrison and Curtis (1992) demonstrated that uptake of cadmium is higher from natural diets raised in contaminated environments than from cadmium-contaminated commercial feeds. Farag et al. (2000) also demonstrated that metals in laboratory-dosed and field-collected invertebrates are processed differently by fish consumers during digestion and metals in laboratory-dosed prey are less available to fish. Therefore, results of studies that are conducted using contaminated feeds or laboratory-contaminated prey differ significantly from those of studies using natural prey and may underestimate the potential for trophic transfer of metals to fish.

In light of the above considerations, studies such as those conducted by Farag *et al.* (1994, 1999) and Woodward *et al.* (1994,1995), that used field-collected and contaminated prey were deemed to be the most relevant and appropriate studies for use in deriving a conservative estimate of trophic transfer potential of

[2]Excludes outlier of 156 percent, initial/control concentrations were not reported by authors
[3]Excludes outlier of 375 percent, initial/control concentr. not reported; diet concentr. questionable

metals to fish from benthic invertebrate prey. Table 3 lists results of those studies in which field-collected contaminated prey was used to estimate trophic transfer.

The maximum trophic transfer values reported for these compounds in studies using field-collected and contaminated prey are proposed as conservative estimates of the potential for trophic transfer of metals to fish from benthic invertebrates exposed to dredged material for use in interpreting the results of 28 day laboratory bioaccumulation tests. These factors are:

Copper:	0.21 ($[Cu]_{fish}/[Cu]_{prey}$)	4.76 ($[Cu]_{prey}/[Cu]_{fish}$)
Cadmium:	0.25 ($[Cd]_{fish}/[Cd]_{prey}$)	4.00 ($[Cd]_{prey}/[Cd]_{fish}$)
Lead:	0.23 ($[Pb]_{fish}/[Pb]_{prey}$)	4.35 ($[Pb]_{prey}/[Pb]_{fish}$)
Zinc:	0.24 ($[Zn]_{fish}/[Zn]_{prey}$)	4.17 ($[Zn]_{prey}/[Zn]_{fish}$)
Arsenic:	0.25 ($[As]_{fish}/[As]_{prey}$)	4.00 ($[As]_{prey}/[As]_{fish}$)

While these factors are less conservative than the factor of one that is used in the current evaluation process, they still reflect the results of these studies in a conservative manner. Median trophic transfer factors reported in these studies for copper, cadmium, lead and arsenic ranged from 0.06 to 0.12 (median factor for zinc was 0.21). Because it is impossible to assess how the availability of metals in the prey species used in these studies (e.g., caddisfly and stonefly larvae) relates to availability in dominant prey species at the HARS (i.e., polychaetes and amphipods) and how uptake by trout may differ from fish species at the HARS, EPA Region 2 believes that the conservative interpretation of this data set is reasonable.

Overall, the proposed trophic transfer factors for these five metals are judged to reduce uncertainty in the HARS evaluation process, relative to the current factors used. It is also judged that the continued use of a trophic transfer of 1.0 for the other cationic metals (i.e., silver, chromium, nickel) may overestimate actual trophic transfer of these metals by a similar margin (approximately 4X).

d. Trophic Transfer of Mercury

In general, the potential biomagnification of mercury depends on the form that is present in the environment. Concentrations of the inorganic forms of mercury in marine animal tissues tend to decrease with increasing trophic level. In contrast, methylmercury is highly bioavailable and tends to biomagnify in marine food webs. There is evidence to indicate that the biomagnification of methylmercury resembles that of hydrophobic trace pollutants such as PCBs (Cabana *et al.*, 1994; Mason *et al.*, 1995). Trophic transfer values reported in the literature for mercury vary widely, ranging from 0.2 to 6.8 for total mercury and 0.2 to 141 for methyl mercury (Suedel *et al.*, 1994). However, the majority of the values greater than 2 have been reported for large, long-lived, carnivorous fish and do not account for the effects of increased exposure of these upper trophic level species. As discussed by Huckabee *et al.* (1979), factors such as longevity, growth rates, uptake, and depuration among species at different trophic levels may have as much impact on observed mercury concentrations as food chain transfers.

The current evaluation incorporates a trophic transfer factor of 1.95 based on an evaluation conducted by Cabana *et al.* (1994). This study was designed to account for potential artifacts such as differences in longevity, growth rates, uptake and depuration rates among species of different trophic levels, and thus focused on individuals from a single species (i.e., lake trout) exposed via food chains of varying lengths. The value of 1.95 represents the average trophic transfer observed per trophic level. This trophic transfer factor (i.e., 1.95) is proposed for continued use in the HARS framework.

Given the wide range of values reported for this compound and the associated confounding factors, it is difficult to estimate the uncertainty associated with the selected value. However, EPA Region 2 believes that the

selected value represents an appropriately conservative estimate. The evaluation estimates the transfer of contaminants from fish to benthic invertebrates,
thus representing species foraging at lower trophic levels. The selected trophic transfer factor represents the upper end of values reported for transfers among species feeding at this level.

III. RISK CHARACTERIZATION (EFFECT)

An important facet of the method and use of risk assessment concerns the recognition of uncertainties and limitations inherent in the process which arise in connection with the use of dose-response models (or linked residue-effect observations) to develop values associated with a relative potential for adverse effects of contaminants. In human health risk assessment there is uncertainty associated with animal to human extrapolation, while across species extrapolation is an area of uncertainty associated with assessing ecological risk. The transport and fate of contaminants in compartments of the food web (or within the body) and the potential for interactions between contaminants are areas of uncertainty shared by both ecological and human health risk assessment. These areas of uncertainty are qualitatively discussed below.

A. Human Health

1. Reference Doses and Hazard Quotient Estimates

Significant uncertainty is associated with the evaluation of noncarcinogenic effects of chemicals in the environment. Primary sources of uncertainty include the derivation and use of chemical specific toxicity values and the limitations inherent in the hazard index (HI) methodology, such as the assumption of additivity for multiple chemical exposure and the inability of the Hazard Quotient (HQ; i.e., chronic daily intake/RfD) to predict the likelihood of adverse effects occurring at doses above the RfD.

Toxicity values based on human epidemiological studies are not available for most chemicals, and in general human studies suffer from poorly characterized exposure data and any number of potential confounding factors, including concomitant exposure to multiple chemicals, recall bias, and lifestyle effects. Therefore, for many chemicals, data from studies of laboratory animals provide the basis for toxicity values. The practice of extrapolating effects observed in experimental animals to predict human toxic response to chemicals is a major source of uncertainty in risk assessment (EPA, 1989).

RfDs are generally developed by dividing NOAELs from animal studies by "safety factors," to adjust for uncertainties in the physiological differences between humans and laboratory animals, variation in sensitivity among individuals of human subpopulations, and differences between subchronic and chronic exposures. These safety factors are typically set at 10X to account for kinetic and dynamic differences within and between species. Thus, when all three factors are combined, the resultant safety factor is equal to 1,000 (10 x 10 x 10) (Barnes and Dourson, 1988).

However, analysis of toxicological data indicate that a value less than ten for an individual safety factor may be adequate, depending on the relative magnitude of uncertainty associated with the critical study. For example, Lewis *et al.* (1990) reviewed the data from eighteen laboratory animal studies and found that the average difference between NOAELs based on subchronic exposures and NOAELs based on chronic exposures was a factor of 3.5 or less, not the default value of 10 that is typically applied. Similarly, a factor of 1 for extrapolation from laboratory animals to humans is appropriate if there are adequate data which indicate a likelihood that the test species is significantly more sensitive to the chemical-specific effect than humans.

In cases when the RfD is based on a study which reports a LOAEL but does not report a NOAEL, an additional safety factor is generally applied to the LOAEL to derive an estimated NOAEL. This safety factor may range from 1 to 10, depending upon the study and the severity of the effects observed. When Dourson

and Starra (1983) compared LOAELs and NOAELs from a variety of studies that reported both, they found that 96 percent of those studies had LOAEL:NOAEL ratios of 5:1 or less. Based on their evaluation, Dourson and Starra (1983) concluded that a safety factor in the range of 1 to 10 is supportable for extrapolating from a LOAEL to a NOAEL. In addition, Dourson and Starra (1983) suggested that the severity of the effect is a critical determinant in establishing a LOAEL to NOAEL safety factor. For example, for liver necrosis, a relatively severe effect, a relatively high value (i.e., 10) was suggested. However, for a less severe effect, such as fatty infiltration of the liver, which results in increased liver weight, a factor of 3 was suggested (Dourson and Starra, 1983).

There is regulatory precedent for use of safety factors totaling less than 1,000. In calculating an RfD for 2,4-dichlorophenol, EPA applied an uncertainty (or safety) factor of 100 to the value reported as a NOAEL to account for extrapolation from animal data to humans and for protection of sensitive populations. In deriving the RfD for Aroclor 1254, the EPA applied a safety factor of 300 to the LOAEL observed in the critical study. EPA justified the safety factor of 300 by reasoning that: a 10-fold factor for interspecies was unnecessary due to similarities between humans and monkeys; only a "partial factor" was needed to account for use of a LOAEL because the effect (nail bed changes) was not considered serious; and a "reduced" factor for extrapolation from subchronic to chronic exposure was adequate because the critical effects did not appear to be dependent upon the duration of the study. Thus, the uncertainty factor of 300 applied by EPA in this case was significantly lower than the safety factor of 10,000 which would have resulted if four individual uncertainty factors of 10 had been combined.

In conclusion, many conservative assumptions are used to account for various sources of uncertainty associated with the evaluation of noncarcinogenic effects. One example of this conservatism and the health-protective nature of HIs calculated in this assessment is the use of multiple safety factors in the derivation of the RfD. Typically, a safety factor of 1,000 is applied to the NOAEL in deriving an RfD; however, the EPA has applied combined safety factors as low as 100. Therefore, use of a safety factor of 1,000 may be overly conservative for some chemicals by a factor of ten or more (Lewis *et al.*, 1990).

2. <u>Cancer Slope Factor and Risk Estimates</u>

In establishing slope factors, regulatory agencies implement methods that introduce multiple sources of uncertainty that ultimately increase the overall conservatism inherent to the cancer risk estimates. Major uncertainties exist in the extrapolation from animals to humans and from high doses to low doses (51 FR 185:33992-34003, September 24, 1986). For example, species differ substantially in their uptake, metabolism, organ distribution, and target-site susceptibility of carcinogens. While laboratory animals are exposed to controlled concentrations at extremely high doses, humans are typically exposed to lower environmental levels (Crump *et al.*, 1989). In addition, the potency of a chemical is influenced by the size and lifespan of the species experimentally exposed. This has important implications due to the long latency period of many carcinogenic responses. An individual's susceptibility to a carcinogenic compound is also influenced by the variability that exists within human populations. Variables include genetic constitution, diet, occupational and home environments, activity patterns, and other cultural factors (51 CFR 185:33992-34003, September 24, 1986).

To compensate for these various sources of uncertainty in the dose response assessment, conservatism is incorporated into the derivation of the slope factor. The slope factor represents the upper 95th percent confidence limit on the probability of a carcinogenic response per unit intake of a chemical over a lifetime (EPA, 1989c). In other words, there is only a five percent chance that the probability of a response would be greater than the estimated value. Therefore, slope factors are likely to overestimate the actual potency of a carcinogen. The accuracy of risk estimates, associated with low doses, predicted by the LMS model is unknown, but may in fact be zero (EPA, 1986).

3. Additivity of Risk and Hazard

A high level of uncertainty is also associated with exposures to multiple chemicals. For evaluation of cumulative effects from exposure to multiple chemicals, EPA (1989) recommends that risks be summed across chemicals for each exposure pathway. However EPA risk assessment guidance also recommends that dose additivity for non-carcinogens be performed for only those chemicals that share a similar mode-of-action and/or toxic endpoint. Accordingly, this approach was employed. It is acknowledged that such an approach may not account for synergistic or antagonistic effects, but is considered appropriate for screening levels analyses (EPA, 1992).

In the absence of chemical-specific toxicity information and consistent with EPA (1989, 1993, 1994) interim guidance and practice, relative toxicity schemes were employed for evaluating additive risks associated with exposure to, or by combined action of, PCDD/PCDFs, PAHs, and coplanar PCBs (EPA, 1989, 1993, 1994). As described by EPA (1989), there is significant uncertainty associated with the use of relative toxicity values, such as TEFs. These uncertainties are the focus of a number of current research programs.

B. Ecological Effects (Linked Residue-Effect Data)

Interpretation of risks to ecological receptors associated with accumulated contaminants is an area of ongoing study and debate in the scientific and risk assessment communities. Historically, scientists have focused on measuring contaminant concentrations in an external medium (e.g. sediment and water) to interpret risks to ecological receptors (e.g. Veith *et al.*, 1979). The current HARS guidelines for evaluating the potential for ecological effects of many contaminants were estimated from concentrations in an external medium (i.e. USEPA Water Quality Criteria).

Current residue guidelines for several compounds (aldrin, dieldrin, total chlordane, total endosulfans, 1,4-dichlorobenzene, arsenic, chromium, copper, lead, nickel, silver, zinc) were estimated from chronic marine water quality criteria using a single bioconcentration factor selected from the scientific literature. The residue guideline that is approximated by this method is entirely dependant on the selected bioconcentration factor. Bioconcentration factors for specific compounds vary extensively among different species, within taxonomic groups, and under different environmental conditions. As a result, significant uncertainty exists in the scientific community regarding the use of bioconcentration factors as the sole means of developing residue guidelines (Franke, 1996; Chapman *et al.*, 1996).

The proposed process for evaluating dredged material evaluates risks to ecological receptors by comparing measured residues to guidelines developed using tissue residues of individual contaminants that were associated with adverse effects to exposed organisms ('linked residue-effect data') in the scientific literature. Studies in which residues were measured in organisms adversely affected by exposure to environmental contaminants offer correlative data without the confounding problem of estimating residues from external concentrations. Therefore, the use of 'linked residue-effect' data for interpreting risks to ecological receptors is deemed to be (at least conceptually) less uncertain than the current approach. Areas of uncertainty associated with the current and proposed methods for deriving guidelines for evaluating the potential for ecological effects are qualitatively discussed below.

1. Relating Contaminant Residue Concentrations and Probabilities for Adverse Effect

Using specific tissue residues as guidelines for assessing ecological risk assumes that there is a *critical body residue* above which the potential for effects materializes. Furthermore, the use of specific tissue residues as guidelines assumes that the probability for ecological effects increases with increasing tissue concentration beyond the critical body residue. The critical body residue assumption relies on a relatively constant body burden threshold for effects regardless of differences in exposure conditions. This criterion, however, does not hold true in many cases. Exposure conditions and experimental design (sampling) often alter the body burden at which lethality, or other adverse effects, occurs (or is measured). Factors affecting estimates of the critical body residue include:

- Salinity, temperature, hardness, pH, and other water quality parameters;
- Presence vs. absence of sediment during exposure;
- Medium/pathway to (by) which organisms are exposed to a contaminant;
- Duration of exposure;
- Rate of contaminant uptake by organism;
- Differences in the whole body residue vs. residue at specific target organs;
- Changes in toxicity due to physiological state (can be seasonal or lifecycle related);
- Feeding/non-feeding during exposure;
- Specific compound used in exposure (e.g. metal salts or speciation);
- Ages of test organisms used;
- Action associated with unmeasured metabolites rather than parent compound.

Additional areas of uncertainty that must be recognized are: 1) the absence of multi-generational effects data in many cases; 2) the possibility for unmeasured adverse effects; 3) differences between body residues measured in surviving and dead organisms within the same treatment; and 4) the potential for differences in critical body residues between species.

It is important to emphasize, however, that most of the areas of uncertainty outlined above are attendant to using residues to predict effect and are independent of the method that is used to develop residue guidelines for evaluating risk. The associated areas of uncertainty outlined above cannot easily be quantitatively considered.

2. 'Linked Residue-Effect' Guidelines

Proposed guidelines for evaluating ecological risk would be based on published 'linked residue-effect' data and presented as ranges of relative concern. 'Linked residue-effect' data will be interpreted in an environmentally-conservative manner to develop relative concern ranges. The lower of the two numbers in the range will represent the lowest residue associated with effects that has been published in the scientific literature. As such, it will only reflect the specific conditions of exposure and design of that particular experiment. Therefore, it would be most appropriately applied as a screening guideline. The higher of the two numbers assigned to the ecological guideline range would reflect residues at which adverse effects have been exhibited in multiple taxonomic groups or by multiple exposure modes. Therefore, as the high end of the range is approached by test tissue residues, the potential for adverse impacts in a variety of species or taxonomic groups will be judged to increase correspondingly.

'Linked residue-effect'- based guidelines will be derived for contaminants that exert their toxicity through narcosis and for contaminants (or contaminant classes) that have specific modes of toxic action. The critical body residue concept is generally accepted by toxicologists for assessing risk of narcotic contaminants. The propriety of this assumption for developing guidelines for contaminants with specific modes of toxic action is less accepted and is currently the subject of much debate in the scientific community (McCarty and MacKay,

1993; Deneer *et al.* 1999). Therefore, areas of uncertainty associated with linked residue-effect guidelines for contaminants with narcotic and specific modes of action are discussed below separately.

a. Narcosis

Narcosis is a sublethal effect characterized as a disruption of cell membrane function resulting from binding of organic contaminants to elements in the lipid bilayer. Narcosis causes progressive lethargy, unconsciousness, and (eventually) death in the affected organism. Critical body residues associated with narcosis have typically been reported to vary within an order of magnitude (Chaisuksant *et al.* 1999).

Despite certain limitations in the application of the critical body residue model for evaluating narcotic potential, use of this approach is generally accepted for non-polar, narcotic compounds. As such, the ensuing discussion of uncertainty will be limited to the residue guideline levels (and the assumptions made in their derivation) that are specifically identified in the current and proposed processes for evaluating the suitability of dredged material for use as Remediation Material at the HARS.

The current process for evaluating the potential for narcosis differs fundamentally from the proposed evaluation process in two regards. First, the current process for evaluating bioaccumulation test results considers the total mass of only the 16 parent PAHs accumulated by organisms to evaluate the potential for narcotic effects. Narcosis is described as a "baseline" mode of action that is shared by all nonpolar organics (Chaisuksant *et al.* 1999), including those that exert toxic effects through specific modes of action at significantly lower residue levels (Chaisuksant *et al.*, 1999; McCarty and MacKay 1993; V. McFarland (USACE-Waterways Experiment Station), pers. comm.; S. Kane-Driscoll (Menzie-Cura Assoc.) pers. comm.; B. Brownawell (State University of New York at Stony Brook) pers. comm.; P. Landrum (NOAA), pers. comm.). Therefore, limiting consideration of the potential for narcosis to the total mass of the 16 parent PAHs underestimates the true narcotic dose represented by organic contaminants accumulated from the dredged material. As a result, the proposed process for evaluating the narcotic potential of accumulated contaminants will include the contribution of all organic contaminants that are analyzed. This approach more accurately reflects the potential for narcosis from the measured analytes (see below). There is remaining uncertainty with respect to the potential contribution of unmeasured contaminants to the total narcotic dose.

Second, the potential for narcosis is determined in the current process by comparing total PAH mass in test organism tissues to a critical body residue of 0.2 mmol/Kg. The 0.2 mmol/Kg used in the current process was estimated for chronic lethality from the low end of acute critical body residues typically associated with lethality (2 mmol/Kg) (McCarty, 1991). The range of narcotic critical body residues associated with acute lethality in McCarty (1991) has been supported by other researchers using a variety of aquatic organisms. However, the order of magnitude lower residues for chronic lethality were estimated by McCarty (1991) without empirical support. This estimate was based on ratios of acute and chronic effective external concentrations (e.g., water contaminant concentrations) (McCarty and MacKay, 1993). To address this issue, revised estimates of critical body residues for evaluating narcotic potential are proposed for use in evaluating dredged material. These estimates were derived based on a review of published data that relates narcotic contaminant load and incidence of effects. Guidelines developed based on these data would be protective of sublethal, chronic effects.

Based on the review of published data, acute narcotic effects were reported in the literature at levels below the range reported by McCarty (1991). van Wezel *et al.* (1995a) reported lethal residues of dihalogenated benzenes as low as 0.3 to 0.6 mmol/kg in two age classes of rainbow trout (*Oncorhynchus mykiss*). Fay *et al.* (in press) reported a lethal residue of a tetrachlorinated PCB of 0.57 mmol/kg in the amphipod, *Ampelisca abdita*. Narcotic residues associated with acute sublethal effects below the acute residue range of McCarty

(1991) were also reported. Decreased feeding was associated with PAH residues of 0.08-0.14 mmol/kg in mussels (Donkin *et al.* 1989) and 0.2 mmol/kg in benthic copepods (Lotufo, 1998).

Chronic narcotic effects in the literature were also reported at residues below the chronic range of critical body residues estimated by McCarty (1991). Reduced reproduction in fathead minnows was associated with an anthracene residue of 0.1 mmol/kg (Hall and Oris, 1991). Reduced growth, feeding, and survival was reported in bluegill to occur at PAH residues of 0.05-0.14 mmol/kg (Finger *et al.* 1985).

Data reported by van Wezel *et al.* (1995b) and Pawlisz and Peters (1995) suggest that critical body residue and length of exposure are not easily related. Therefore, estimating chronic residue guidelines from acute residue data is not well-advised despite the suggestion of an order of magnitude adjustment by McCarty (1991). It is noted, however, that had chronic critical body residues been estimated as 0.1 times reported acute residues, the chronic critical body residues associated with lethality would be equal to or greater than 0.03 mmol/kg.

Emery and Dillon (1996) reported a very low body burden (i.e. 0.0044 µmol/g) associated with decreased reproduction of the marine worm, *Neanthes arenaceodentata,* after chronic exposure to a neutral narcotic organic contaminant (i.e. the PAH, phenanthrene). In that study, the authors speculated that other modes of action may have been contributors to the observed effect. Although evidence to support this conclusion that toxicity was not the result of narcosis was not provided by the authors, it is clear from review of the scientific literature regarding narcosis that the reported residue could not have been associated with a narcotic response. In addition, because this study included only a single treatment, dose-dependency of the response could not be confirmed (nor rejected).

In light of the results of the data review outlined above, a critical body residue range for chronic, sublethal effects due to narcosis is proposed as 0.05 - 0.1 mmol/kg.

b. Non-narcosis

As stated above, using the critical body residue approach to derive residue guidelines for evaluating contaminants with specific modes of toxic action is less certain than for evaluating the potential for narcotic effects. Areas of uncertainty that limit the predictability of effects based on residue levels (discussed above) are not easily addressed quantitatively. EPA Region 2 will maximize the utility of the linked residue-effect data by employing several assumptions. Using these assumptions significantly affects the magnitude of the guidance levels that will ultimately be derived from this data. Uncertainty associated with these assumptions is qualitatively discussed below.

c. Endpoints Guidelines derived from linked residue-effect data by EPA Region 2 will be limited to reported effects on growth, reproduction, or survival. While the ecological relevance of these endpoints is clear, other sublethal effects of contaminants could also impair the fitness of exposed organisms. For example, the ability to withstand anoxia was decreased in mussels exposed to cadmium or PCBs (Veldhuizen-Tsoerkan *et al.*, 1991),and prey capture and predator avoidance rates were decreased in fish exposed to fluorene (Finger *et al.*, 1985). Subtle impacts to organism fitness may not be manifested or measured in the duration or controlled design of experiments, despite their potentially important ecological significance in the field. Therefore, relying on linked residue-effect data associated with reduced growth, reproduction or survival during the conduct of the experiment introduces uncertainty into the guidelines that could contribute to potentially underestimating the actual risk associated with contaminant residues.

d. Mode of Exposure The medium (water, diet, sediment) by which organisms are exposed to contaminants varies from study to study. Most of the linked residue-effect data that has been published to date has been attained in studies that exposed organisms to contaminants in water. However, this method of exposing

organisms may underestimate whole body residues that are associated with adverse effects. For example, adverse effects (e.g., lowered survival) may result from elevated concentrations at sites of exposure (e.g., gill) that may not be in equilibrium with the remainder of the body. In most cases, EPA Region 2 will not differentiate between residues resulting from different exposure pathways for the purpose of this evaluation. As a result, those guidelines that rely on water exposures may potentially overestimate risk associated with contaminant residues for those species which are not primarily exposed via uptake from the water.

e. Using ER50 (Median Effective Residue) as Threshold for Significant Effect EPA Region 2 proposes to derive guidelines for evaluating ecological risk using the lowest ER50 (residue associated with 50% reduction in growth, reproduction, or survival or associated with adverse effects in 50% of exposed individuals) reported in individual studies to express effects. The 50% level has historically been used in environmental regulation (e.g. USEPA Water Quality Criteria). Its use, however, introduces uncertainty regarding the magnitude of impact necessary to result in environmental consequences at the population level. Data that are necessary to support the models needed to answer this question are not available for populations at the HARS. Furthermore, linked residue-effect data is available for only a limited number of species that occur at the HARS and the interspecific differences in sensitivities cannot easily be assessed.

Overall, EPA Region 2 believes that the use of ER50s (as opposed to lower effect levels) reported in individual studies will not significantly affect the overall guidance levels derived for evaluating ecological risks. In most cases, using alternate effect residue levels within individual studies would only slightly change the estimated effective concentration assigned to that individual study. Ranges of effective (and non-effective) residues reported in the linked residue-effect literature spans several orders of magnitudes for almost every contaminant. Because of this range, and because the lowest reported ER50 within a taxonomic group would be used as the screening concentration for effects in that group, the use of alternate effect residue levels would result in a very modest difference.

Overall, the use of ER50s is judged to be a somewhat non-conservative interpretation of individual study results. However, in light of the overall conservative approach that is proposed to derive guidance levels for evaluating ecological risk, their use is deemed to introduce only a small degree of uncertainty. It must be noted, that the method that is proposed to estimate ER50s for individual studies will often be extremely conservative. Specifically, when the magnitude of effect associated with specific residues is not reported in the data base (Jarvinen and Ankley, 1999), the magnitude of effect will be assumed to be 100%. Similarly, when non-effective residue concentrations are not reported they will be assumed to be zero (0).

f. Interspecific Differences in Sensitivities Species vary in their relative sensitivities to environmental contaminants. The confidence by which guidelines based on linked residue-effect data can be used to evaluate ecological risk is dependant on the number of species that are represented by the available data. For certain contaminants (e.g. chromium, silver), few linked residue-effect observations are reported in the literature. EPA Region 2 will derive guidelines based on available data, even when interspecific representation is lean. It is not possible to quantify how well the species for which data is available represents those species occurring in the field.

EPA Region 2 will assume that the sensitivities of species within a taxonomic group (i.e., molluscs, infaunal worms, crustaceans, and finfish) are more comparable than the sensitivities of species in different taxonomic groups. The lowest reported residue associated with effects in an individual taxonomic group will be used as a screening concentration for effects in organisms within that group. As stated above, the appropriateness of using the lowest reported residue as a screen is a function of the number of species that are represented by non-effective or lowest observed effect residue concentrations above the selected residue level.

EPA Region 2 proposes to use the assumption of greater differences in contaminant sensitivity between taxonomic groups to assign ranges of relative concern for evaluating ecological risks associated with accumulated contaminants. The lowest overall effective residue associated with effects across all species (and therefore all taxonomic groups) is proposed for use as a screening level for interpreting overall ecological risk. Risk to ecological receptors will be concluded to be minimal when residues are below these screening levels. Ecological risk associated with test organism residues above the overall ecological screening levels will be evaluated on a case by case basis. EPA Region 2 proposes to conclude that there is additional (unacceptable) ecological concern when a contaminant residue in test organism tissue exceeds the screening residue levels for more than one taxonomic group. An example of overall relative concern residue ranges that are proposed to be developed for individual contaminants is presented for PCBs:

Contaminant	Ecological Screen Residue[a]	Taxonomic group, Exposure, Effect[b]	Higher Concern (multiple taxonomic group) Residue[a]	Taxonomic group, Exposure, Effect[b]
total PCBs	0.329	F, C, S	0.630	W, C, SL

[a] Residues are in ug/g, wet weight.

[b] Taxonomic group (M=mollusc, W=infaunal worm, C=crustacean, F=fish), exposure (D=diet, W=water, S=sediment, C=combined), endpoint (S=survival, G=growth, R=reproduction, SL=other sublethal effect)

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Table 1. Trophic Transfer of Metals to Fish Reported by Suedel *et al*. (1994)

Arsenic	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Hexanchus griseus	shark	SW	field	20.9	LeBlanc and Jackson (1973)
	Hexanchus griseus	shark	SW	field	10	LeBlanc and Jackson (1973)
	Hexagrammos spp	greenling	SW	field	0.3	LeBlanc and Jackson (1973)
	Hydrolagus colliei	ratfish	SW	field	15.2	LeBlanc and Jackson (1973)
	Diaphus dumerili	headlightfish	SW	field	0.1	Leatherland *et al.* (1973)
	Carassius auratus	goldfish	FW	laboratory	0.2	Maeda *et al*. 1990
Cadmium	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
		omnivorous fish	FW	field	1.1	Ward *et al.* (1986)
	Diaphus dumerili	headlightfish	SW	field	0.1	Leatherland *et al.* (1973)
Chromium	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Carpiodes cyprinus	quillback	FW	field	0.03	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	0.5	Mathis and Cummings (1973)
		postlarval fish	SW	laboratory	0.1	Baptist and Lewis (1969)
	Fundulus hetroclitus	mummichog	SW	laboratory	1.6	Baptist and Lewis (1969)
Copper	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Carpiodes cyprinus	quillback	FW	field	0.02	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	0.7	Mathis and Cummings (1973)
	Pleuronectes platessa	plaice	SW	laboratory	0.5	Saward *et al*. (1975)
Lead	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Etheostoma flabellare	fantail darter	FW	field	0.3	Enk and Mathis (1977)
	M. dolomieu	smallmouth bass	FW	field	0.9	Enk and Mathis (1977)
	Carpiodes cyprinus	quillback	FW	field	0.1	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	0.9	Mathis and Cummings (1973)
		omnivorous fish	SW	field	2.6	Ward *et al.* (1986)
	Helotes sexlineatus	trumpeter	SW	field	0.4	Ward *et al.* (1986)
	Platichthyes flesus	flounder	SW	field	0.7	Hardisty *et al*. (1974)
Nickel	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Carpiodes cyprinus	quillback	FW	field	0.03	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	0.7	Mathis and Cummings (1973)
	Oncorhynchus mykiss	rainbow trout	FW	field	0.01	Dallinger and Kautzky (1985)
	M. dolomieu	smallmouth bass	FW	field	1.6	Wren *et al.* (1983)
	Salvelinus namaycush	lake trout	FW	field	1	Wren *et al.* (1983)
Zinc	Species	Common Name	SW/FW	Field/Lab	TTC	Reference
	Carpiodes cyprinus	quillback	FW	field	0.06	Mathis and Cummings (1973)
	M. dolomieu	smallmouth bass	FW	field	1	Mathis and Cummings (1973)
	Gobius spp	omnivorous fish	SW	field	0.1	Ward *et al.* (1986)
	Helotes sexlineatus	trumpeter	SW	field	0.4	Ward *et al.* (1986)
	Platichthyes flesus	flounder	SW	field	1.4	Hardisty *et al*. (1974)
	Diaphus dumerili	headlightfish	SW	field	0.1	Leatherland *et al.* (1973)
		postlarval fish	SW	laboratory	0.68	Baptist and Lewis (1969)
	Fundulus hetroclitus	mummichog	SW	laboratory	0.11	Baptist and Lewis (1969)
	L. xanthurus	spot	SW	laboratory	0.17	Willis and Sunda (1984)

Table 2. Trophic transfer of metals to fish from contaminated prey/food.

Species	Metal	Concentrations				Reference	Notes
		Diet	Initial Control	Final	TTC (%)		
	Copper						
R.trout	Copper	110	2.9	4.6	1.55	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	140	2.9	4.7	1.29	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	200	2.9	4.4	0.75	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	250	2.9	5.9	1.20	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	440	5.7	19.6	3.16	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	830	5.7	22.4	2.01	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	1000	5.7	27.7	2.20	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	55	2.9	3.4	0.91	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	110	2.9	5.1	2.00	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	200	2.9	6.4	1.75	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	340	2.9	7.1	1.24	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	320	2.9	8.8	1.84	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Copper	200	5.75	17	5.63	Handy 1992	32 d, feed, no depuration
R.trout	Copper	200	5.75	5.5	0.00	Handy 1992	32 d, feed, w/ 12 d depuration
B.trout	Copper	87	8.5	11.5	3.45	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Copper	178	6	26	11.24	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Copper	174	7.5	34	15.23	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Copper	381	6.25	33.5	7.15	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Copper	14	6.25	3.5	0.00	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Copper	12	6.25	4.35	0.00	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Copper	109	6.25	16	8.94	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Copper	415	6.25	39	7.89	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Copper	38.8		8	20.62	Farag et al. 1994	21d, collected, dead, starved 24 h
R.trout	Copper	185.7		6.5	3.50	Farag et al. 1994	21d, collected, dead, starved 24 h
C.trout	Copper	9.9	5.2	3.5	0.00	Farag et al. 1999	90d, feed, starved 24h
C.trout	Copper	32.9	5.2	6.1	2.74	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Copper	61.5	5.2	9	6.18	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Copper	43.8	5.2	12.3	16.21	Farag et al. 1999	90d, collected, starved 24h, vitamins
	Cadmium						
R.trout	Cadmium	7.6	0.36	0.69	4.34	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	16	0.36	0.95	3.69	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	23	0.36	1.08	3.13	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	21	0.36	1.29	4.43	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	9.5	0.76	1.31	5.79	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	36	0.76	2.76	5.56	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	69	0.76	6.83	8.80	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Cadmium	150	0.15	5.45	3.53	Handy 1992	32 d, feed, no depuration
R.trout	Cadmium	150	0.15	0.985	0.56	Handy 1992	32 d, feed, w/ 12 d depuration
Guppy	Cadmium	69.5		3	4.32	Hatekeyama and Yasuno 1982	30 d, live, 1 d water depuration
Guppy	Cadmium	125.9		5	3.97	Hatekeyama and Yasuno 1982	30 d, live, 1 d water depuration
Guppy	Cadmium	170.6		6	3.52	Hatekeyama and Yasuno 1982	30 d, live, 1 d water depuration
R.trout	Cadmium	3	0.05	0.3	8.33	Kumada et al. 1973	12 wks, feed, no depuration
R.trout	Cadmium	3	0.04	0.1	2.00	Kumada et al. 1973	12 wks, feed, 6 wks depuration
R.trout	Cadmium	10	0.05	0.65	6.00	Kumada et al. 1973	12 wks, feed, no depuration
R.trout	Cadmium	10	0.04	0.09	0.50	Kumada et al. 1973	12 wks, feed, 6 wks depuration
R.trout	Cadmium	30	0.05	1.9	6.17	Kumada et al. 1973	12 wks, feed, no depuration
R.trout	Cadmium	30	0.04	0.12	0.27	Kumada et al. 1973	12 wks, feed, 6 wks depuration
R.trout	Cadmium	100	0.05	5.6	5.55	Kumada et al. 1973	12 wks, feed, no depuration
R.trout	Cadmium	100	0.04	0.27	0.23	Kumada et al. 1973	12 wks, feed, 6 wks depuration

Species	Metal	Concentrations Diet	Initial Control	Final	TTC (%)	Reference	Notes
B.trout	Cadmium	nd (<0.27)	0.075	0.15	na	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Cadmium	nd (<0.27)		0.225	na	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Cadmium	0.26	0.044	0.45	156.25	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Cadmium	3.12	0.05	0.8	24.04	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Cadmium	0.36	0.05	0.05	0.00	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Cadmium	0.5	0.05	0.095	9.00	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Cadmium	1.2	0.05	0.11	5.00	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Cadmium	2.39	0.05	0.6	23.01	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Cadmium	0.9		0.225	25.00	Farag et al. 1994	21d, collected, dead, starved 24h
R.trout	Cadmium	1		0.085	8.50	Farag et al. 1994	21d, collected, dead, starved 24h
C.trout	Cadmium	0.21	0.04	0.04	0.00	Farag et al. 1999	90d, feed, starved 24h
C.trout	Cadmium	0.97	0.04	0.1	6.19	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Cadmium	29.9	0.04	2.88	9.50	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Cadmium	29.1	0.04	4.33	14.74	Farag et al. 1999	90d, collected, starved 24h, vitamins

Table 2. Trophic transfer of metals to fish from contaminated prey/food. (Continued)

Species	Metal	Concentrations Diet	Initial Control	Final	TTC (%)	Reference	Notes
	Lead						
R.trout	Lead	33	0.98	1.93	2.88	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	58	0.98	2.37	2.40	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	90	0.98	2.31	1.48	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	82	0.98	3.09	2.57	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	88	1.74	6.29	5.17	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	130	1.74	8.96	5.55	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Lead	210	1.74	10	3.93	Mount et al 1994	60 d, live, combined aqueous/diet
B.trout	Lead	7	1	1.2	2.90	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Lead	15	1.1	2.5	9.33	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Lead	15	0.95	4.35	22.67	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Lead	nd (<2.0)	0.5	0.6	na	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Lead	32.7	0.5	2.5	6.12	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Lead	0.36	0.5	nd (<0.2)	0.00	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Lead	9.69	0.5	1	0.00	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Lead	28.4	0.5	2.4	6.69	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Lead	0.2		0.75	375.00	Farag et al. 1994	21d, collected, dead, starved 24 h
R.trout	Lead	8.6		0.25	2.91	Farag et al. 1994	21d, collected, dead, starved 24 h
C.trout	Lead	0.2	0.2	0.2	0.00	Farag et al. 1999	90d, feed, starved 24h
C.trout	Lead	7.4	0.2	1.2	13.51	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Lead	792	0.2	36.8	4.62	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Lead	452	0.2	52.3	11.53	Farag et al. 1999	90d, collected, starved 24h, vitamins
	Zinc						
R.trout	Zinc	300	88	101	4.33	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	460	88	104	3.48	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	720	88	92	0.56	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	740	88	107	2.57	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	920	116	163	5.11	Mount et al 1994	60 d, live, combined aqueous/diet

Species	Metal	Concentrations			TTC (%)	Reference	Notes
		Diet	Initial Control	Final			
R.trout	Zinc	930	116	189	7.85	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	1900	116	303	9.84	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Zinc	185		165	89.19	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Zinc	655		155	23.66	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Zinc	1070		180	16.82	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Zinc	148.2		nd	nd	Farag et al 1994	21d, collected, dead, starved 24h
R.trout	Zinc	320.9		nd	nd	Farag et al 1994	21d, collected, dead, starved 24h
C.trout	Zinc	135	78	130	38.52	Farag et al. 1999	90d, feed, starved 24h
C.trout	Zinc	384	78	160	21.35	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Zinc	2336	78	380	12.93	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Zinc	2119	78	520	20.86	Farag et al. 1999	90d, collected, starved 24h, vitamins
	Arsenic						
R.trout	Arsenic	35	3.1	4.6	4.29	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Arsenic	40	3.1	5.3	5.50	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Arsenic	51	3.1	5.4	4.51	Mount et al 1994	60 d, live, combined aqueous/diet
R.trout	Arsenic	63	3.1	6.7	5.71	Mount et al 1994	60 d, live, combined aqueous/diet
B.trout	Arsenic	6.5	0.8	0.95	2.31	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Arsenic	19	1.85	3.55	8.95	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
B.trout	Arsenic	19	1.45	3.9	12.89	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Arsenic	6.5	0.15	1	13.08	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Arsenic	19	0.45	2.9	12.89	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Arsenic	19	0.25	3.6	17.63	Woodward et al. 1995	88 d, collected, dead, 24h no feed before analysis
R.trout	Arsenic	3.5	1.25	1	0.00	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Arsenic	43.1	1.25	12	24.94	Woodward et al. 1994	91d, collected, dead, starved 24h before analysis
R.trout	Arsenic	2.8	1.25	1.15	0.00	Woodward et al. 1994	80d, feed, starved 24h before analysis
R.trout	Arsenic	5	1.25	1.05	0.00	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Arsenic	42	1.25	7.5	14.88	Woodward et al. 1994	80d, collected, dead, starved 24h, vitamins
R.trout	Arsenic	1.5		nd	nd	Farag et al 1994	21d, collected, dead, starved 24h
R.trout	Arsenic	15.4		nd	nd	Farag et al 1994	21d, collected, dead, starved 24h
C.trout	Arsenic	3.5	0.76	1.8	29.71	Farag et al. 1999	90d, feed, starved 24h
C.trout	Arsenic	2.6	0.76	0.9	5.38	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Arsenic	50.8	0.76	3.3	5.00	Farag et al. 1999	90d, collected, starved 24h, vitamins
C.trout	Arsenic	13.5	0.76	2.4	12.15	Farag et al. 1999	90d, collected, starved 24h, vitamins
R.trout	Arsenic	180	4.5	15.5	6.11	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	360	4.5	44	10.97	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	732	4.5	89.5	11.61	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	1477	4.5	108	7.01	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	137	4.5	34.5	21.90	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	262	4.5	45.5	15.65	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	500	4.5	56	10.30	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	1053	4.5	72.5	6.46	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	163	2.5	15	7.67	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	362	2.5	22	5.39	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	793	2.5	34.5	4.04	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	1497	2.5	57	3.64	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	193	2.5	10	3.89	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	405	2.5	16	3.33	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	735	2.5	19	2.24	Cockell and Hilton, 1988	56 d, feed, no depuration
R.trout	Arsenic	1503	2.5	30.5	1.86	Cockell and Hilton, 1988	56 d, feed, no depuration

Table 3. Trophic transfer of metals from field-collected benthic invertebrates

	Concentrations				
	Diet	**Initial/Control**	**Final**	**TTC (%)**	**Reference**
Copper					
B.trout	87	8.5	11.5	3.45	Woodward et al. 1995
B.trout	178	6	26	11.24	Woodward et al. 1995
B.trout	174	7.5	34	15.23	Woodward et al. 1995
R.trout	381	6.25	33.5	7.15	Woodward et al. 1994
R.trout	14	6.25	3.5	0.00	Woodward et al. 1994
R.trout	109	6.25	16	8.94	Woodward et al. 1994
R.trout	415	6.25	39	7.89	Woodward et al. 1994
R.trout	38.8		8	20.62	Farag et al. 1994
R.trout	185.7		6.5	3.50	Farag et al. 1994
C.trout	32.9	5.2	6.1	2.74	Farag et al. 1999
C.trout	61.5	5.2	9	6.18	Farag et al. 1999
C.trout	43.8	5.2	12.3	16.21	Farag et al. 1999
Cadmium					
B.trout	nd (<0.27)	0.075	0.15	na	Woodward et al. 1995
B.trout	nd (<0.27)		0.225	na	Woodward et al. 1995
R.trout	3.12	0.05	0.8	24.04	Woodward et al. 1994
R.trout	0.36	0.05	0.05	0.00	Woodward et al. 1994
R.trout	1.2	0.05	0.11	5.00	Woodward et al. 1994
R.trout	2.39	0.05	0.6	23.01	Woodward et al. 1994
R.trout	0.9		0.225	25.00	Farag et al. 1994
R.trout	1		0.085	8.50	Farag et al. 1994
C.trout	0.97	0.04	0.1	6.19	Farag et al. 1999
C.trout	29.9	0.04	2.88	9.50	Farag et al. 1999
C.trout	29.1	0.04	4.33	14.74	Farag et al. 1999
Lead					
B.trout	7	1	1.2	2.90	Woodward et al. 1995
B.trout	15	1.1	2.5	9.33	Woodward et al. 1995
B.trout	15	0.95	4.35	22.67	Woodward et al. 1995
R.trout	nd (<2.0)	0.5	0.6	na	Woodward et al. 1994
R.trout	32.7	0.5	2.5	6.12	Woodward et al. 1994
R.trout	9.69	0.5	1	0.00	Woodward et al. 1994
R.trout	28.4	0.5	2.4	6.69	Woodward et al. 1994
R.trout	8.6		0.25	2.91	Farag et al. 1994
C.trout	7.4	0.2	1.2	13.51	Farag et al. 1999
C.trout	792	0.2	36.8	4.62	Farag et al. 1999
C.trout	452	0.2	52.3	11.53	Farag et al. 1999
Zinc					
R.trout	655		155	23.66	Woodward et al. 1994
R.trout	1070		180	16.82	Woodward et al. 1994
R.trout	148.2		nd	nd	Farag et al 1994
R.trout	320.9		nd	nd	Farag et al 1994
C.trout	384	78	160	21.35	Farag et al. 1999
C.trout	2336	78	380	12.93	Farag et al. 1999
C.trout	2119	78	520	20.86	Farag et al. 1999
Arsenic					
B.trout	6.5	0.8	0.95	2.31	Woodward et al. 1995
B.trout	19	1.85	3.55	8.95	Woodward et al. 1995
B.trout	19	1.45	3.9	12.89	Woodward et al. 1995
R.trout	6.5	0.15	1	13.08	Woodward et al. 1995
R.trout	19	0.45	2.9	12.89	Woodward et al. 1995
R.trout	19	0.25	3.6	17.63	Woodward et al. 1995
R.trout	3.5	1.25	1	0.00	Woodward et al. 1994
R.trout	43.1	1.25	12	24.94	Woodward et al. 1994
R.trout	5	1.25	1.05	0.00	Woodward et al. 1994
R.trout	42	1.25	7.5	14.88	Woodward et al. 1994

R.trout	1.5		nd	nd	Farag et al 1994
R.trout	15.4		nd	nd	Farag et al 1994
C.trout	2.6	0.76	0.9	5.38	Farag et al. 1999
C.trout	50.8	0.76	3.3	5.00	Farag et al. 1999
C.trout	13.5	0.76	2.4	12.15	Farag et al. 1999