



UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
NATIONAL HEALTH AND ENVIRONMENTAL EFFECTS
RESEARCH LABORATORY
ATLANTIC ECOLOGY DIVISION
27 TARZWELL DRIVE, NARRAGANSETT, RI 02882

OFFICE OF
RESEARCH AND DEVELOPMENT

MEMORANDUM

DATE 6 March 2008

TO Virginia S. Houk, NHEERL Peer Review Coordinator

FROM Robert M. Burgess, AED-Narragansett; ESB Series Editor

RE Reconciliation memorandum for the U.S. EPA document *Procedures for the Derivation of Equilibrium Partitioning Sediment Benchmarks (ESBs) for the Protection of Benthic Organisms: Compendium of Tier 2 Values for Nonionic Organics*(EPA/600/R-02/016)

This memorandum summarizes the responses to the comments of reviewers of the compendium ESB document. These reviewers included: Phyllis Fuchsman (ARCADIS), Christopher Ingersoll (USGS), Susan Kane-Driscoll (Exponent, Inc.), Guy Lotufo (U.S. Army Corps Engineers) and James Meador (NOAA). The reviewer's comments dealt with a wide range of technical issues and demonstrated their knowledge and expertise regarding the evaluation of contaminated sediments. In just about every instance, addressing the reviewer's comments resulted in improving the scientific accuracy, clarity, and usefulness of the document. When a comment was not incorporated into the revised compendium ESB document, an explanation is provided. Some of the responses to the comments may have changed in wording in the final document (but not in substance) as compared to responses cited in this memorandum as a result of revisions made at the end of the editing process.

The principle objective of this document is to demonstrate the derivation of equilibrium partitioning sediment benchmark values not to provide specific ESB values. While the ESB values in the document should provide protection to benthic organisms, text has been added throughout encouraging readers to use updated toxicity information whenever new high quality data becomes available.

My colleagues David Mount at MED-Duluth and Walter Berry at AED made valuable contributions to this reconciliation memorandum. Also, as part of our AED internal review process, Richard McKinney, from AED, reviewed the document and provided useful comments. Finally, thank you for coordinating the review of this document, having your office provide this service is a great resource that maintains the quality of Agency' science products.

Please find below, my specific responses to the comments of the five external reviewers of the document. To make reading this memorandum somewhat easier, the reviewer's comments have been highlighted in grey tone. Also, in general, new text added to the document is shown here in italics.

Reviewer #1

(a) Overall, I think the front matter does an excellent job of presenting the preliminary information for this document. I think the consequences of deriving tier 2 ESB values is missing. As stated in the foreword, the ESBs do not serve as regulatory values and are for screening and advisory purposes only. In the last half of the Foreword the general term for ESBs is used, implying no distinction between tier 1 and tier 2 ESBs. If the EPA perceives that tier 1 and tier 2 ESBs will be used differently, I think this needs to be conveyed. If they are not considered different in their application then that point should also be made. A third possibility is that the EPA will be neutral and allow the user to decide the value of tier 1 versus tier 2 ESB values. Whatever the position, this distinction should be stated in the Foreword and also in the Executive Summary

Response: A sentence has been added to the Foreword and Executive Summary, indicating that both Tier 1 and Tier 2 values can be used but that the Tier 2 values, as compared to Tier 1 values, may have more uncertainty:

Both types of ESBs, Tier 1 and Tier 2, are intended for similar applications with the user's understanding that, because of limited data availability, Tier 2 ESBs are likely to have greater uncertainty associated with them as compared to Tier 1 ESBs. (Foreword)

Both types of ESBs are intended for similar applications with the user's understanding that Tier 2 ESBs are likely to have greater uncertainty associated with them as compared to Tier 1 ESBs. (Executive Summary)

(b) The Introduction seems sufficient as an overview of the approach. It is clear that tier 1 ESBs are more rigorous than tier 2 ESBs. See my comment above regarding the application of tier 1 versus tier 2 ESBs and my recommendation for addressing the application of each ESB.

Response: No revision necessary.

(c) Overall I think that the method is well presented and the criteria for chemical and toxicity values are explained clearly. I think one oversight is the consideration of overlying water. For viable predictions from this model, one would have to assume that the organism received all of its uptake for a given contaminant from equilibrated porewater, infaunal prey, or bedded sediment where it resides. Many (if not most) benthic species ventilate overlying water and consume prey that are not in contact with the benthos. Overlying water and pelagic prey are usually not in equilibrium with the bedded sediment, which would violate the EqP assumption for this approach. This point needs to be addressed in the Section 2 and Section 3.5 (limitations).

Response: Several studies evaluating the equilibrium partitioning (EqP) approach using animals that have access to overlying water have demonstrated that disequilibria between the benthos and overlying water does not negate the ability of the approach to predict bioavailable concentrations in the sediments. For examples, Section 5 of U.S. EPA (2003e) discusses several successful flow-through EqP studies with polycyclic aromatic hydrocarbons (PAHs) in sediments.

The reviewer is probably correct in stating some benthic organisms which are exposed in some way via

the overlying water will not be in equilibrium with the sediments or interstitial waters (or more accurately, the chemical activities between the phases will not be comparable). However, the objective of this document is to provide guidance to protect most benthic species, especially the most sensitive ones. As the document cited above demonstrates, the approach has been shown to function successfully. If users of this document sought to protect a certain species or group of species and were concerned that disequilibria because of water column effects was likely, an alternative equilibrium model maybe necessary but that is beyond the scope of this document.

Finally, from an environmental protection perspective, disequilibria caused by overlying water could result in a dilution of bioavailable sediment concentrations thus making the ESBs over-protective for some species. Conversely, contaminant concentrations in the water column exceeding interstitial water concentrations would strongly suggest the presence of a source other than the sediments. This other source would require assessment from a water column based approach not the sediment ESB.

No change to the document.

(d) I think Section 2 needs a discussion that deals specifically with confounding factors and how they would affect the results. This discussion may also be appropriate in Section 3.

Response: This comment is too broad to address without more specificity. The revised Section 4.0 does address limitations of the presented approach more extensively including some confounding factors and their potential effects on the derivation of the ESBs.

(e) I didn't see any mention of "adjustment factors" that were mentioned in the Introduction (Section 1.2).

Response: A statement has been added to Section 2.3.1 (italicized below), indicating the use of adjustment factors:

... produced by the FCV methodology, reflecting greater uncertainty *and use of protective adjustment factors* in the absence of additional toxicity data (see Section 2.4).

The use of adjustment factors (i.e., Secondary Acute Factors) is also discussed in detail in Section 2.4 and Appendix A.

(f) Over all, I think the scientific basis and explanation of the EqP approach is clear and well supported. I wouldn't say that this section describes the scientific basis for determining adverse effects for these compounds but that it provides an approach for converting predicted water concentrations and known values of water-based toxicity values to sediment concentrations.

Response: Agreed.

(g) I would like to see a more detailed explanation of the correction using f_{solids} . From what I remember about f_{solids} , the values range from around 0.8 for sand to as low as 0.2 or so for clay dominated sediment. Given these values, the correction would appear to range from 0.25 to 4. Figure 3 shows a few examples for this correction; however, I couldn't read the symbols on the figure. While I am sure that the figure is useful, I think a small table showing how these factors would affect the

final ESB for different types of sediment (mostly sand, sandy silt, silty clay, etc.) would be informative. Additionally, TOC is generally highly correlated with grain size because of the higher surface area of smaller particles, which are coated with organic carbon. Because the value for f_{solids} is a function of grain size, I think it would be appropriate to discuss how the correlation between f_{OC} and grain size would affect the results.

Response: Figure 3-1 has been corrected to make it easier to read. Based on this comment, and others, this section has been revised extensively. For example, a table comparing the effects of f_{Solids} on the calculation of the ESB for four chemicals has been added to the text (i.e., Table 3-3). An analysis of EMAP data found the correlation between f_{Solids} and grain size (i.e., sand content) to be a more valuable and realistic relationship than the one between f_{OC} and f_{Solids} .

(h) Eqn 3-5 contains the term " $f_{\text{OC}} \cdot K_{\text{OC}}$ ", which is just the K_p . I would guess this term was used instead of K_p because it's much easier to acquire values or model f_{OC} and K_{OC} . Looking at eqn 3-6, it seems circular that f_{OC} is used to convert $\text{ESB}_{\text{tier2}}$ to $\text{ESB}_{\text{tier2oc}}$ when eqn 3-5 includes f_{OC} to determine $\text{ESB}_{\text{tier2}}$. These equations are fine, but more explanation would ease any confusion.

Response: While Equation 3-6 is somewhat redundant, it is needed to show how to express the ESB on an organic carbon normalized basis. No revision was introduced.

(i) 3-5 Limitations. The section mentions mixtures only superficially. I think this document needs to take a stronger stand on mixtures. The additive nature of non-specific (narcosis) toxicants is generally accepted and I think the authors should say so. I think they should cite a few of the dozens of papers demonstrating additivity for this mode of action and provide the toxic unit approach equations for dose additive compounds. This was addressed in a little more detail (as a narrative) in section 4.2.5, but still fell short.

Response: Partially in response to this comment, an example has been added to the text (i.e., Section 4.3 and Table 4-1) which discusses how to work with mixtures for the narcotic chemicals discussed in this document.

(j) I think the information on interpretation is sufficient; however the application sections are light. I did expect to see a discussion on potential regulatory and non-regulatory application. While, regulatory aspects are mentioned briefly in the foreword and introduction, Section 4 would be the place to provide more detail on potential application. Because I have not seen the document on tier 1 ESBs, I don't know how the EPA feels about ESBs in general as regulatory values. I think that should be addressed in this document. For example, are tier 1 values also just for screening or are they going to be promoted as regulatory values. What about tier 2 values?

Response: The application and related regulatory implications are currently discussed in the revised *Foreword*, *Executive Summary* and *Section 4*. To be consistent with information provided in the earlier ESB documents, no additional discussion will be included.

(k) One potentially important item that I think is missing here is the temporal factor. While only chronic values

are used for ESBs (with actual FCVs or by converting FAVs to FCVs with ACRs), the protection offered by a chronic WQC versus that for an ESB may be different. The CCC for water is defined as the four day average not to be exceeded once every 3 years. Is this exposure guideline considered acceptable for species that have far less mobility and spend their entire life cycle just below the ESB compared to that expected for pelagic organism and the CCC? In the development of the CCC exposure time, wasn't it thought that pelagic species would not be exposed to the same concentration of water for extended periods of time?

Response: The four day averaging period was incorporated into WQC to recognize that very short term excursions above the criterion continuous concentration (CCC) (usually the FCV) would probably not cause chronic effects. However, there needed to be a limit on how long this excursion could be before causing chronic effects might be likely and the four-day period was decided upon as being a reasonable averaging period. Nonetheless, the toxicity data used to derive the CCC are based on exposures to constant concentrations, so the CCC essentially assumes constant exposure. Accordingly a constant exposure at the CCC would be expected to be equivalent to the chronic value for the 5th percentile species. As such, it remains applicable to sediments wherein exposures are assumed to be relatively stable.

No revision to the document.

(l) Another temporal factor concerns steady state. Does the ESB account for compounds that may take longer to reach steady state than the duration of the chronic tests? Is a correction factor needed for those compounds expected to exhibit very long times to steady state?

Response: This comment is addressed in Section 4.2; for example, when discussing spills. In which case, equilibrium is unlikely and a site-specific ESB maybe needed. Otherwise, the EqP approach assumes the chemical has been associated with the sediment system for a period of time sufficient to achieve equilibrium. Further, most of the chemicals discussed in this document have relatively low K_{OW} s and short equilibrium times.

No revision to the document.

(m) Page 1-1. It should be noted that equilibrium between sediment and water will often not occur and that under these non-equilibrium conditions the route of uptake does influence the tissue concentration and hence the toxic response.

Response: I don't entirely agree with this comment and the potential effects of disequilibria are discussed in Section 4.2.

(n) Page 1-2. The term "narcosis" is a poor descriptor of the process of nonspecific (or baseline) toxicity. It would be advantageous to drop this term in favor of a more appropriate modifier.

Response: At this time, 'narcosis' is the term most commonly applied in the field of environmental toxicology to describe the effect. However, text has been added to Section 1 discussing the current understanding of narcosis relative to this document.

(o) Page 1-3. I am not sure I get number 1 (“numeric”). Would an ESB be anything other than numeric? How about noting that these ESBs are based on causal toxicity responses. This is an important distinction because this can’t be claimed for other sediment toxicity guidelines.

Response: ‘Numeric’ is indicated in contrast to a ‘narrative’ guideline that is used for some environmental stressors.

No change to document.

(p) Item number 5 (Protective of benthic organisms) may not be true. It really depends on the appropriateness of the biological responses and the species tested. Sublethal/chronic endpoints are often based on severe effects for a limited number of species.

Response: I disagree with this comment. The toxicity values being used in this document are based on Water Quality Criteria (WQC) or values derived in a similar way as WQC. Further, U.S. EPA (2003e)(Section 3.3) demonstrates that these types of toxicity values are protective of benthic species.

No change to document.

(q) Page 1-3 last para. “Adversely affect” is a subjective term. There may be adverse effects, especially for time periods beyond those used to determine the chronic values. Why not express ESBs as a “no effect” level? I think this is best accomplished by determining LOEC values and using safety/uncertainty factors to convert them to NOECs.

Response: I disagree with this comment. ‘Adversely affect’ describes an endpoint. In this case, the endpoint selected is either a final or secondary chronic value (SCV) value. There are arguments for using any given endpoint, the series of ESB documents has selected to be derived using FCVs and SCVs based on the successful WQC model.

No change to document.

(r) Page 1-5. Item 1. I don’t see why a SPARC generated value is not required for tier 2 compounds. A K_{ow} for almost any compound can be obtained with the SPARC program and the result is generally accurate. Why limit this requirement to tier 1 ESB values?

Response: In principle, I agree with this comment but given the realities of limited resources and time, a SPARC-generated K_{ow} is not always available.

No change to document.

(s) Page 2-3 section 2.3.2. para 1. Along with the derivation of aquatic toxicity values using conventional techniques (see discussion above), narcosis theory was used to derive SCVs for chemicals determined to be primarily narcotic in their mode of action by ASTER (Russom et al. 1997). It would be more accurate to say that these compounds have no known specific mechanism of toxic action. The authors should also acknowledge that many compounds can act by a non-specific (narcosis) mode of action at high doses and by specific mechanisms at low doses. This may explain the disparate results reported in Table 3-1.

Response: Text has been added to section 2.3.2 addressing the reviewer's comment:

It should be noted that for a given chemical multiple modes of action can affect an organism. Therefore, despite the categorization of these chemicals as primarily narcotics, other modes of action may be active. Section 4.3 discusses some of the implications of this issue.

An example calculation of ESBs for several chemicals has been included (Section 4.3) discussing, in part, the implications of different modes of action.

(t) Page 2-4. I would rather not see the mean ACR used. Because ACRs range over a few orders of magnitude for some compounds and species, I would suggest using the 5th percentile (or lower) ACR. If only a few ACRs are available for a given compound, I think the lowest value should be used. If no ACRs are available and the ACR for several compounds is needed, I think the 5th percentile of all those ACRs would be a better choice to ensure that most species are protected. Therefore, instead of using a default value of 18, the 5th percentile ACR for all compounds for a specific mode of action should be determined and used as the "default".

Response: As stated through-out the document, the Great Lakes Water Quality Initiative (GLI)(GLI, 1995, Suter and Tsao 1996) methodology was frequently used for deriving conventional ESB chronic toxicity values. Although there are other approaches, as the reviewer proposes, the GLI methodology is widely accepted. Further, using the GLI methodology allows the ESB values (and approach) to be applied within other regulatory programs.

No change to document.

(u) Page 2-5 last para. I can't tell if these correlations are strong because the r^2 values are not shown. Given that these are log-log plots, it appears that the correlations are not strong.

Response: These plots illustrate the agreement between observed and predicted values and are not meant to demonstrate predictive ability.

No change to the document.

(v) Figures 2-2 through 2-5. Please supply the equations and r^2 values.

Response: See response to Reviewer #1 Comment 'u'.

(w) Page 3-11, bottom of first column. Therefore, the chemical must be associated with the sediment for a sufficient length of time for equilibrium to be reached.

I would restate as such,

Therefore, the sediment and water must be associated for a sufficient length of time for chemical equilibrium to be attained.

Response: The sentence has been revised to reflect the reviewer's comment. The text is now in Section

4.2.7.

(x) Page 4-3 ^{2nd} para. In this approach, the contribution of each individual narcotic chemical to the toxicity of the overall mixtures is assessed by taking the ratio of the measured concentration of that individual chemical in the mixture by the corresponding single chemical ESB_{Tier2} value. This proportion is calculated Give the equation described here for simple additive mixtures. It's easier to understand an equation than to read a narrative that attempts to explain the relationship.

Response: The newly added example in Section 4.3 addresses this comment. The text is now in Section 4.2.7.

(y) Page 4.3 bottom to top of 4-4. For this reason, if a sediment shows toxicity but does not exceed the ESB_{Tier2} for a chemical of interest, it is likely that the cause of toxicity is a different chemical or chemicals. If there is toxicity and the ESB_{Tier2} is not exceeded for a given toxicant, that toxicant can still be a contributor, along with other toxicants that are acting by dose addition.

Response: The sentence has been revised to the following form with the italicized text added:

For this reason, if a sediment shows toxicity but does not exceed the ESB_{Tier2} for a chemical of interest, it is likely that the cause of toxicity is a different chemical or chemicals (*although the chemical of interest maybe contributing to observed toxicity*).

(z) Page 4-4. I would like to see DOC complexation addressed in greater detail here and in the section on limitations. Additionally, it is possible that [DOC] in porewater may be variable for a given _{foc}, which would lead to potential differences in bioavailability for two sediments with the same _{foc} and contaminant concentration. Therefore, direct measure of DOC would be preferred over modeling. If DOC must be modeled, this variability should be acknowledged as an uncertainty.

Response: A discussion of the effects of dissolved organic carbon (DOC) on the calculation of ESBs is included in a draft ESB document (U.S. EPA 2003b). To clarify this, the following text has been added to this section:

See U.S. EPA (2003b) for a discussion of the effects of DOC on ESB derivation.

(aa) It seems that some of the points made in this document are redundant. For example, the text regarding partitioning when unusual components are present (e.g., hair, sawdust, hides, etc.) occurs in 3 places (Sections 1.2, 3.5, and 4.2.3). I think the detailed explanation for this phenomenon belongs in section 3.5. Section 4.2.3 should reference that section and section 1.2 is probably fine, but that text could be reduced.

Response: Redundant text in Sections 3 and 4 is now revised and combined in Section 4.2.

The commented upon text in section 1.2 was not altered.

Reviewer #2

(a) General comment: It would have been helpful if the entire draft had line numbers added (to make

specific references to line numbers easier for the reviewer and reviewee!).

Response: Very good point.

(b) Page ii, Notice. Good idea to include an electronic link to the document. It would be useful to include electronic links to other ESBs (PAHs, pesticides, metals) in this section.

Response: Text was added to the *Notice* indicating the other ESB documents can also be found at this address.

(c) General comment on the Abstract and Executive Summary: Including both an Abstract and an Executive Summary is redundant. The Abstract and Executive Summary do not adequately summarize information presented in the main document. It would be useful to expand either the Abstract or the Executive Summary to address the following comments.

Response: For better or worse, the *Abstract* and *Executive Summary* format, with inevitable overlap, is the one being used in the ESB documents.

(d) Page ii, Abstract. The Abstract should briefly reference the procedures for ESBs for PAHs, pesticides, and for metals (i.e., the USEPA 2003 and 2005 citations). See for example, the last paragraph in the Executive Summary.

Response: Reference to the other four ESB documents has been added to the *Abstract*.

(e) Page ii, Abstract. The abbreviation for “GLI” does not make sense. Insert the word “Initiative” (global change to document?).

Response: ‘Initiative’ has been added, where appropriate, to the text.

(f) Page ii, Abstract. The assumption that the GLI values for freshwater are protective of marine organisms needs to be further evaluated in the document. While analyses presented by, Di Toro et al. 1991 support this statement, it would be useful to specifically conduct these analyses based on the compounds listed in Table 3-1.

Response: This assumption was tested more recently (U.S. EPA 2003e) with narcotic PAHs. No significant difference was detected between freshwater and marine species.

No change to document.

(g) Page ii, Abstract. I’m confused by the use of term “Tier 2” in the Abstract. Explain the difference between a Tier 1 and a Tier 2 ESBs (as is described in the 2nd paragraph in Section 1.3 and in Section 4.2.6). A statement should be included in the Abstract (and in the Executive Summary) that this document is a compendium of nonionic ESBs for 32 chemicals including several low molecular weight aliphatic and aromatic compounds, pesticides, and phthalates (as stated in the 1st paragraph in Section 1.2).

Response: The *Abstract* currently contains text indicating that a thorough definition of Tier 2 ESBs is included later in the document (i.e., *Executive Summary*, *Introduction*). This information is not needed in the *Abstract*.

In response to the reviewer, text has been added to the *Abstract* and *Executive Summary* describing the types of chemicals discussed in the document.

(h) Page ii, *Abstract*. The statement is made that the ESBs should be interpreted as chemical concentrations below which adverse effects are not expected and as the degree of exceedances increase, effects may occur (see also the statements made in Section 4.2.1). It is not clear in the *Abstract*, *Executive Summary*, or in the main text as to what a threshold would be. Are the thresholds 1.0 for each of the ESB listed in Table 3.2? Are there toxicity data with field-collected sediments or with spiked sediments to support the statement that toxicity is not observed below Tier 2 ESBs and toxicity is observed above Tier 2 ESBs? If not, there should be a caveat added to the *Abstract*, *Executive Summary*, and main text (i.e., in Section 4) that additional studies are needed to evaluate the assumption of lack of an effect below Tier 2 ESB thresholds and the assumption of effects above Tier 2 ESB thresholds.

Response: Data are not available for confirming the predictions of effects or no effects for the chemicals in this document. However, that is the reason, as explained in Section 1.3, the values reported in this document are Tier 2. One of the criteria for Tier 2 ESB values is the lack of confirmation tests demonstrating the accuracy of the EqP predictions. Consequently, Tier 2 ESB values are more uncertain as compared to Tier 1 ESBs which require that confirmation tests have been performed.

No change to document.

(i) Page ii, *Abstract*, last paragraph. Additional detail is needed regard how one can (or can't) address mixture with Tier 2 ESBs. Should ESBs for narcotic chemicals in this document (and for PAHs) be combined with ESBs for phthalates, pesticides, and metals? Specifically, the statement made in the 2nd paragraph on page 1-3 and in Section 4.2.5 should be briefly summarized in the *Abstract* (and in the *Executive Summary*).

Response: To address the reviewer' comment, the following text was added to the *Abstract* and *Executive Summary*:

The document also includes an example demonstrating the calculation of conventionally-derived and narcosis-based ESBs including an approach for addressing mixtures of narcotic chemicals.

(j) Page ii, *Abstract* and Page iii, *Forward*. A statement should be included in the *Abstract* and in the *Forward* (and in other locations in the document as appropriate) regarding the utility of generating matching whole-sediment toxicity and chemistry data. The *Abstract* (and *Executive Summary*) should briefly summarize the statement in the 1st paragraph in Section 1.3 that "ESBs as presented in the document are meant to be used with direct toxicity testing of sediments as a method of sediment evaluation, assuming the toxicity testing species is sensitive to the chemical(s) of interest." In addition,

Section 1.3 should highlight recommendations by Wenning et al. (2005) to use a weight of evidence approach when assessing sediment contamination (measures of both biological effects and sediment chemistry; Wenning RJ, Batley G, Ingersoll CG, Moore DW, editors. 2005. Use of sediment quality guidelines and related tools for the assessment of contaminated sediments. Pensacola FL: SETAC Press, 783 p).

Response: Although not added to the *Abstract*, the following text, or similar text, has been added to the *Executive Summary*, *Introduction* and Section 4:

ESBs are supportive of recent recommendations by Wenning et al. (2005), to apply a weight of evidence approach when evaluating contaminated sediments.

(k) Page viii. Seems odd to acknowledge coauthors as providing document review (Burgess, Mount).

Response: Burgess and Mount have been removed from the review subsection.

(l) Page ix. The first sentence in the last paragraph of the Executive Summary makes reference to uncertainties associated with the Tier 2 ESBs. A stand-alone paragraph should be added to the Executive Summary that briefly summarizing uncertainties associated with the approach including: (1) a summary of the statements made in Section 3.5; (2) the influence of different types of organic carbon on Koc (as mentioned on page 1-4); (3) the applicable range of Koc (i.e., as stated in Section 1.3); (4) the need for additional acute and chronic water-only toxicity data to evaluate the predicted water effect concentrations in Table 3-1; and (5) the need for spike-sediment toxicity tests or toxicity tests with field collected samples to evaluate the predictive ability of the ESBs (these statements should be added to Section 4).

Response: The following brief paragraph has been added to the *Executive Summary*:

Uncertainties associated with ESB_{Tier 2} values are discussed in detail through-out this document. They include unknown effects of antagonism, synergism and additivity, occurrence of chemical disequilibria, and presence of unusual types of sedimentary carbon, like black carbon, and large particles. Uncertainties for the ESB_{Tier 2} values can be reduced by conducting additional acute and chronic water-only and spiked sediment toxicity tests to refine water-only effect concentrations and confirm predictions of sediment toxicity, respectively.

A statement about the low range for K_{OW} was also included in the *Executive Summary*. Further, the following sentences were added to Section 1.3 of the *Introduction*:

This uncertainty can be decreased by conducting additional acute and chronic water-only and spiked sediment toxicity tests to evaluate effect concentrations and confirm predicted sediment concentrations, respectively.

(m)Page x, Executive summary, 2nd paragraph. The Executive Summary needs to expand on the statement that the ESBs should not be used as stand-alone pass-fail criteria for all applications (see

Comment j above).

Response: See Response to Comment j.

(n) Page xi, Glossary of Abbreviations. Make sure that all of these abbreviations are used in the document (e.g., ANOVA is not cited in the text). ESB_{OC} should be written as ESB_{OC}.

Response: ESB_{OC} has been corrected in the *Glossary of Abbreviations* and any un-cited terms deleted.

Section 1: Introduction

(o) Section 1.2, 1st paragraph. It is not clear in this first section what the difference is between a Tier 1 ESB and a Tier 2 ESB (include the statement made in the last paragraph of the Executive Summary in Section 1.2).

Response: The following text has been added to Section 1.2:

Both types of ESBs, Tier 1 and Tier 2, are intended for similar applications with the user's understanding that Tier 2 ESBs are likely to have greater uncertainty associated with them as compared to Tier 1 ESBs. See Section 1.3 for further discussion of Tier 1 and Tier 2 ESBs.

(p) Section 1.2, 3rd paragraph. Cross reference USEPA documents for the source of the Tier 1 ESBs. For the ESBs in the current document, it would be useful to compare marine versus freshwater SCVs or FCVs to support the statement that there were not significant differences in sensitivities between marine and freshwater species for the compounds listed in Table 3-1.

Response: In Section 1.2, text in the 2nd paragraph already discusses the topic of this comment based on the findings for narcotic chemicals investigated in U.S. EPA (2003e).

No change to document.

(q) Section 1.2, 4th paragraph. Citation needed for the statement that differences in FCVs or SCVs for pesticides are expected for freshwater and marine organisms.

Response: Water Quality Criteria documents have been cited to support the statement about differences in FCVs and SCVs for freshwater and marine species for pesticides.

No change to document.

(r) Section 1.3, 1st paragraph. Cite the applicable ASTM and USEPA methods for conducting whole-sediment toxicity tests.

Response: The following references have been added to the text:

American Society for Testing and Materials. 1998. Standard Guide for Conducting Static Sediment Toxicity Tests with Marine and Estuarine Amphipods. E1367-92. In Annual Book of Standards, Vol. 11.05, Philadelphia, PA, USA.

American Society for Testing and Materials. 1998. Standard Test Methods for Measuring the Toxicity of Sediment-Associated Contaminants with Freshwater Invertebrates. E1706-95b. In Annual Book of Standards, Vol. 11.05, Philadelphia, PA, USA.

American Society for Testing and Materials. 1998. Standard Guide for Conducting Sediment Toxicity Tests with Marine and Estuarine Polychaetous annelids. E1611. In Annual Book of Standards, Vol. 11.05, Philadelphia, PA., USA.

U.S. Environmental Protection Agency. 1994. Methods for Measuring the Toxicity of Sediment-Associated Contaminants with Estuarine and Marine Invertebrates. EPA 600/R-94/025, Office of Research and Development, Washington DC, USA.

U.S. Environmental Protection Agency. 2000. Methods for Measuring the Toxicity and Bioaccumulation of Sediment-Associated Contaminants with Freshwater Invertebrates. EPA 600/R-99/064, Office of Research and Development/Office of Water, Washington DC, USA.

U.S. Environmental Protection Agency. 2001. Methods for Assessing the Chronic Toxicity of Sediment-Associated Contaminants with the Amphipod *Leptocheirus plumulosus*. EPA/600/R-01/020, Office of Research and Development/Office of Water/Army Corps of Engineers, Washington DC, USA.

(s) Page 1-5, last sentence. Period missing.

Response: Thank you, a period has been added.

Section 2: Derivation of Equilibrium Partitioning Sediment Benchmarks

(t) Section 2.1. It is not clear what the specific “three possible ways” are for deriving aquatic toxicity values (are the three: (1) GLI, (2) WQC, or (3) narcosis calculations?).

Response: The text is confusing. In Section 2.1 and in the *Introduction*, the ‘three’ has been changed to ‘two’ to clarify that the two possible ways include ‘conventional’ and ‘narcosis’.

(u) Page 2-4. Use the defined abbreviation (e.g., ACR, SAF). Define SACR. Period missing in the 2nd sentence in the 2nd to last paragraph of page 2-4 (too bad lines were not numbered!).

Response: As suggested, the acronyms have been used and defined, and the suggested period added.

(v) Section 2.4. I am confused by the use of the term “secondary acute factor” in this section. Reference Page A-10 for this description.

Response: A comment, in parentheses, has been added recommending the reader see Section A.5 for more discussion of secondary acute factors (SAFs).

Section 3: Calculation of Equilibrium Partitioning Sediment Benchmarks

Comments on Section 3:

(w) Section 3.1, 1st paragraph. State the ESBs applicable for compounds with $K_{ow} > 2.0$ (Section 1.3).

Response: The following text has been added:

The EqP approach used here to derive ESBs functions most effectively for nonionic organic chemicals with $\log K_{ow} \geq 2$. However, for chemicals with $\log K_{ow}$ between 2 and 3, EqP will function but sedimentary conditions (i.e., f_{OC} and f_{Solids}) should be considered and adjustments to the derivation of the ESB maybe advisable (see Section 3.3).

(x) Table 3-1. A footnote should be included with each FCV or SCV regarding how the value was derived (i.e., based on the 6 possible sources listed in Section 2.3 or based on narcosis calculations).

Response: Section 2.3 provides the information requested in this comment. Further, including this extra information would result in a table that is very difficult to read and use. No change to the document.

(y) Table 3-2. It would be useful to know how frequently these ESBs are exceeded in freshwater or marine sediments. Are there data in the NSI that could be used to make these calculations? Are sediment samples exceeded the ESBs in Table 3-2 toxic to sediment-dwelling invertebrates?

Response: This is a very interesting comment but beyond the immediate scope of this document.

No change to the document.

(z) Equation 3-5. Are there published studies describing this equation? Fuchsman (2003)?

Response: Yes – Fuchsman (2003) describes the equation.

(aa) Figure 3-1. The symbols are not legible.

Response: Figure 3-1 has been revised to make the symbols legible.

(bb) Section 3.5, 1st paragraph. The term “best case” is confusing in reference to potential synergistic or antagonistic effects.

Response: The statement containing ‘best case’ has been deleted to avoid confusion and this text moved to Section 4.2.

(cc) Section 3.5, 5th paragraph, line 26. The phrase “applicability of...” should not be upper case.

Response: This error has been corrected.

(dd) Section 3.5 should also mention the following limitations to the applicability of the ESBs in Table 3-2: (1) lack of acute or chronic water-only toxicity data with sediment dwelling invertebrates for these compounds and (2) lack of acute or chronic toxicity data with whole sediment to establish empirical thresholds for the ESBs (other than assuming the threshold is 1.0 for each ESB). Specifically, how well do the ESBs in Table 3-2 predict toxicity in spike sediments or in field-collected sediments?

Response: New text in Section 4.2.6 addresses this comment:

Consequently, the generation of acute and chronic water-only data with benthic organisms for the nonionic organic chemicals discussed in this document would be very beneficial for interpreting the ESB_{Tier 2} values. Further, acute and chronic whole sediment toxicity data sets with these chemicals would also complement the interpretation of the ESBs.

Section 4: Sediment Benchmark Values: Application and Interpretation

Comments on Section 4:

(ee) Section 4 should be re-titled “Sediment Benchmark Values: Application, Interpretation, and Data Needs”.

Response: The title of this section is consistent with the other ESB documents.

No change to the document.

(ff) Section 4.1. The statements that the ESBs in Table 3-2 (1) “are appropriate for protection of both freshwater and marine sediment based on the assumptions discussed in Section 1.2...” and (2) “are applicable to a variety of freshwater and marine sediments because they are based on the biologically available concentration of the substances in those sediments” are not supported by the data presented in the document. No data are presented demonstrating that the ESBs presented in Table 3-2 can be used to estimate toxic or non toxic conditions in sediment.

Response: It is true that no data is presented testing the predictions of toxicity reported in this document for the ESB values. In general, this is because the data are not available. However, as discussed through-out the document (e.g., *Executive Summary*, *Introduction*), this is a Tier 2 document. A condition for being a Tier 2 ESB is that ‘EqP-confirmation’ tests have not been performed to evaluate the accuracy of the predictions of toxicity. The principle result of this condition is that the Tier 2 values have a level of uncertainty associated with them greater than the Tier 1 values. In several sections of this ESB, users have been informed of this limitation with Tier 2 ESBs. However, for all ESB values, Tier 1 and 2, as stated through-out the ESB documents, ESB values should not be used independently but in combination with other sediment quality guidelines and sediment assessment methods. The reviewer

has recommended through-out the review, the addition of more text emphasizing this last point and, in most cases, such text has been included in this document.

In addition, despite the lack of 'EqP confirmation' tests for Tier 2 values reported here, the approach used to derive the Tier 2 values is based on the same approach used with the EqP confirmed Tier 1 ESBs including PAH mixtures, endrin and dieldrin. The success of those Tier 1 ESB values, increases the likelihood that Tier 2 values presented here are accurate.

No change in the document.

(gg) Section 4.2.6. A statement should be added that there is a need to evaluate the predictive ability of the ESBs listed in Table 3-2 in acute and chronic sediment toxicity tests with spiked sediment or with field-collected sediment.

Response: Text similar to the response to Reviewer #2; Comment dd has been inserted into Section 4.2.6:

Consequently, the generation of acute and chronic water-only data with benthic organisms for the nonionic organic chemicals discussed in this document would be very beneficial for interpreting the ESB_{Tier 2} values. Further, acute and chronic whole sediment toxicity data sets with these chemicals would also complement the interpretation of the ESBs.

(hh) Section 4.1, last sentence. A listing of the types of additional data to collect would be useful (i.e., as is stated in the 1st paragraph in Section 1.3).

Response: The following text has been included:

'...could trigger collection of additional assessment data (e.g., benthic community composition, whole sediment toxicity testing, and other sediment quality guideline evaluations (e.g., Long et al. 1995, MacDonald et al. 1996, Long and MacDonald 1998, Swartz 1999, MacDonald et al. 2000a,b).

(ii) Section 4.2.3 is redundant with statements made in Section 3.5.

Response: To address this comment, and similar comments from other reviewers, Section 3.5 was combined with Section 4.2 in Section 4.

(jj) Page 4-4, 3rd paragraph. The recommendation is made to evaluate effect concentrations specific to a species. Are there any chemicals in Table 3-1 with matching toxicity data for sediment-dwelling invertebrates of interest (amphipods in freshwater or marine exposures or midge in freshwater exposures)?

Response: The topic of this comment is a recommendation in Section 4.2.6, it is beyond the scope of the document to perform the analysis proposed.

No change to the document.

Comments on Appendix A:

(kk) Page A-4, 1st paragraph. The statement regarding the quality of data from the laboratory in Columbia Missouri should be critically evaluated (ha, ha; just kidding!).

Response: All of the USGS data from the Columbia laboratory has been deleted (ha, ha – just kidding!).

(ll) Page A-6. Data were rejected if organisms were not acclimated to the test water for at least 48 hours before the beginning of a test. It is surprising that this was listed as a reason to reject a test given that ASTM standard state acclimation as a “should” rather than as a “must” for test acceptability (e.g., ASTM E729, E1241).

Response: It seems as though the Great Lakes Water Quality Initiative used a slightly different set of criteria.

(mm) Page A-6. Data were rejected if more that 10% mortality occurred in the control. This should be a reason for rejecting acute toxicity data (per ASTM E729), but not necessarily for chronic toxicity data (per ASTM E1241).

Response: It seems as though the Great Lakes Water Quality Initiative used a slightly different set of criteria.

Reviewer #3

(a) The ESBs and associated methodology presented in this document provide a means to estimate the concentrations of 32 nonionic organic chemicals that may be present in sediment, while still protecting benthic organisms (both freshwater and marine) from the effects of those chemicals. As I explain further in my detailed comments below, I feel that the authors of these charge questions as well as the authors of the ESB document must be very careful about the language used to describe the purpose of the ESBs. My understanding is that the ESBs should be used to identify concentrations of individual chemicals in sediment below which sediments are not expected to be toxic. However, it must be made *very* clear that concentrations higher than the ESB values may not be toxic if organisms in sediment do not experience the same degree of exposure as they did in the water-only exposure upon which these benchmarks are based. If equilibrium conditions are not attained among sediment, water and organisms, organisms may experience less exposure and toxicity from sediments than from water-only exposures. The first and third sentences in this Background section imply that concentrations above the ESBs are not protective, which is not necessarily the case. The equilibrium partitioning (EqP) approach was chosen because it accounts for the varying biological availability of chemicals in different sediments and allows for the incorporation of the appropriate biological effects concentration. The ESBs are intended to provide protection to benthic organisms from direct toxicity (see comment above), and they may be useful as a

complement to existing sediment assessment tools, to help assess the extent of sediment contamination, to help identify chemicals causing toxicity, and to serve as targets for pollutant loading control measures.

Response: To address this comment, whenever a statement to the effect of ‘at concentrations above the ESB’ is made, the phrase ‘assuming equilibrium between phases’ has been inserted into the text. This phrase is now present in the *Abstract*, *Executive Summary* and Section 4. These sections of text also include the word ‘may’ when discussing the likelihood of effects occurring. Taken together, this text should make it clear to the document user that exceeding the ESB values does not necessarily mean toxic effects will occur. The point is also made in several places in the text that the ESB should not be used alone but along with other sediment assessment techniques to make decisions about the occurrence of sediments effects.

(b) As noted above, I think that the last sentence of the Abstract should to be revised to state that concentrations higher than the ESB values may not be toxic if equilibrium conditions are not attained among sediment, water and organisms. (Although organisms that metabolize and eliminate organic contaminants, may reach a steady state, rather than a true equilibrium, they must be exposed to an equilibrated phase for a sufficient time to experience adverse effects.) I think that the assumption of equilibrium partitioning among sediment, water, and organisms should be made clear in the Abstract, along with a statement that contaminants may not reach equilibrium in the environment.

Response: The following italicized text has been added to the second to last sentence of the Abstract:

‘At concentrations above the ESB_{Tier 2}, *and assuming equilibrium between phases*, effects may occur with increasing severity as the degree of exceedance increases.’

Other parts of the document, including Section 4.2, discuss disequilibria in greater depth.

(c) The 4th paragraph of the Foreword contains some important information, but I am confused about the wording. The document states: “This document provides technical information to EPA Program Offices, the regulated community and the public. For example, ESBs when used in the Superfund process, would serve for screening purposes only, not as regulatory criteria, site specific clean-up standard, or remedial goals.” The second sentence does not seem to be to be an example of technical information provided, but is rather a critically important recommendation for the application of these benchmarks that is generally applicable. It is rather confusing to state that the Superfund program would not use these as regulatory criteria, site specific clean-up goals, or remedial goals, but that EPA and State decision makers can retain the discretion to adopt other approaches “as appropriate”. Does that imply that EPA and State decision makers can use these values as site specific clean up goals or remedial goals if they feel it is appropriate? The authors of this document seem to imply that it would not be appropriate to use these values as clean up numbers, but don’t state it directly. I think that the Abstract, Foreword, and Executive Summary should clearly state that these numbers are not intended as remediation goals.

Response: To clarify the point raised in this comment, the following text has been added to the *Forward*:

Decisions about risk management are the purview of individual regulatory programs, and may vary across programs depending upon the regulatory authority and goals of the program. For this reason, each program will have to decide whether the ESB approach is appropriate to that program and, if so, how best to incorporate this technical information into that program's assessment process. While it was necessary to choose specific parameters for the purposes of this document, it is important to realize that the basic science underlying this document can be adapted to a range of risk management goals by adjusting the input parameters.

and

It is recommended that the ESBs not be used alone but with other sediment assessment methods to make informed management decisions.

(d) In the Executive Summary, I object to the language of the first sentence that states that the ESBs are protective of benthic organisms. Although this statement is strictly true, I think that the wording implies that concentrations above the ESBs are not protective of benthic organisms, which may not be true if contaminants of contaminants in sediment, water and organisms are not in equilibrium. Further, the last sentence of the second paragraph states that "...if the effect concentration in water is known, the effect concentration in sediments on a ug/goc basis can be accurately predicted ...". While the previous sentence is true under equilibrium conditions, it should be made clear that because equilibrium conditions are not always attained under field conditions, and toxic concentrations cannot always be predicted under the assumptions of equilibrium partitioning.

Response: This comment has been addressed in the response to Reviewer #3; Comment a and b as well as discussion in the document in Section 4.2.

(e) The 4th paragraph states that information on "unusual partitioning" can be incorporated to develop site-specific ESBs (US EPA 2003b). I disagree that we know enough about whether and how often nonionic contaminants exhibit "usual" or "unusual" equilibrium partitioning. EPA should move quickly to formalize and publish its 2003b guidance on measuring concentrations of organic contaminants in porewater and developing site-specific ESBs. Further testing should be conducted to determine whether the assumptions of equilibrium partitioning are valid for field collected sediments.

Response: I agree, the wording of the 4th paragraph has been changed to indicate that considerable uncertainty exists about how to best incorporate "unusual partitioning" into a site-specific ESB:

Another way to increase the site-specificity of an ESB would be to incorporate information on sediment-specific partitioning of chemicals, particularly for sites where the composition and partitioning behavior of the sediment organic carbon may be substantially different than for typical diagenic organic matter (see U.S. EPA 2003b). However, it should also be noted that the ability to predict partitioning based on additional partitioning factors like black carbon is still evolving and may serve to decrease partitioning-related uncertainties in future applications.

Similar text has been included in other parts of the document.

Relative to the site-specific ESB document (U.S. EPA 2003b), if support for the publication of ESB documents continues, that will be the next document prepared for publication (i.e., cross your fingers).

Section 1: *Introduction*

(f) I disagree with the wording in the last paragraph on page 1-1. As noted above, this section should be revised to note that concentrations higher than the ESB values may not be toxic if equilibrium conditions are not attained among sediment, water and organisms. (Although organisms that metabolize and eliminate organic contaminants, may reach a steady state, rather than a true equilibrium, they must be exposed to an equilibrated phase for a sufficient time to experience adverse effects.) I think that the assumption of equilibrium partitioning among sediment, water, *and organisms* should be made clear in this section.

Response: The paragraph discussed in this comment does list as the first assumption of the EqP approach:

‘...(1) the partitioning of the chemical between sediment organic carbon and interstitial water is at or near equilibrium;...’

Further, this issue is discussed in the Section 4.2 as well as the *Abstract* and *Executive Summary*.

No change to the document.

(g) I also VERY STRONGLY believe that the discussion in the second paragraph of Section 1.2 should be incorporated into the Abstract, Foreword and Executive Summary. It is VERY important for users of this document to understand that because toxicity data for these compounds are limited, that additional adjustment factors are used to develop the ESBs, and that the resulting ESBs are lower because of the lack of data.

Response: I agree the information in Section 1.2 is very important; however, there is insufficient space in the *Abstract*, *Foreword* and *Executive Summary* to go into the sort of detail necessary to explain the derivation of conventional toxicity values. The text does mention in the *Abstract* and *Executive Summary* that the Great Lakes Water Quality Initiative (GLI) approach was used to develop conventional toxicity values. Further, Sections 1.2 and 2.3.1 and Appendix A provide detailed discussions of the derivation of conventional toxicity values.

No change to the document.

(h) The fourth point in the list on page 1-3 should be revised. The ESBs are NOT predictive of biological effects. They are predictive of the LACK of biological effects. The true ability of the ESBs to predict actual toxic effects in sediment is NOT KNOWN.

Response: I understand the reviewer’s point but it is most valid only if equilibrium is not attained between the relevant phases. If the assumptions listed in Section 1.1 are achieved, a basic premise of

this document, the statement ‘Predictive of biological effects’ is very likely to be true. Further, several studies have demonstrated that EqP is predictive of biological effects (as well as the lack of effects) (e.g., Swartz et al. 1990, DeWitt et al. 1992, Hoke et al. 1994). As discussed earlier in response to Reviewer #3, text has been incorporated into the document to remind the user that equilibrium between phases must be achieved before effects *may* be expected to occur above the ESB.

No change to the document.

(i) Is a period missing from the last sentence of Section 1.5?

Response: Yes, a period has been added.

Section 2: *Derivation of Equilibrium Partitioning Sediment Benchmarks*

(j) On page 2-4, the “secondary acute factor” should be clearly defined. The actual secondary acute factors and acute-to-chronic ratios used to derive the secondary chronic values (SCVs) should be included in Table 3-1. The users of this approach should know exactly which adjustment factors were used for each chemical so as to be able to assess the uncertainty associated with the SCV for each chemical.

Response: This comment raises several good points. First, text in Section 2.4 has been added to have document users refer to Appendix A for more information about SAFs. Next, I considered including a table in Section 2 with the requested information rather than adding it to an already very information-intensive Table 3-1. However, in the references cited in this section (i.e., Suter and Tsao 1996, U.S. EPA 2001a), all of the relevant values (e.g., SAV, SAF, SACR) are provided. Further, the references are now available on-line (see the *References* section of the Compendium document). Consequently, text has been added to this section recommending interested readers go to those documents for the specific values.

(k) Page 2-5, first full paragraph, second sentence: missing verb.

Response: The word ‘are’ has been added to the sentence.

(m) Page 2-5, second full paragraph. The actual correlation coefficients should be provided on the figures.

Response: See response to Reviewer #1 Comment ‘u’.

(n) Although the text states that “Most of the measured values fall with(in) a factor of tow of the predicted value...” However, this is not true for non-halogenated organics (Fig. 2-3) for which 11 values fall within a factor of two and 14 values fall outside a factor of two.

Response: The figures were re-plotted with some new data included and the original statement being commented upon in the document is now true.

Section 3: *Calculation of Equilibrium Partitioning Sediment Benchmarks*

(o) First sentence of Section 3.1 should state that the ESBs are intended to be predictive of LACK of biological effects.

Response: To address the reviewer's comment, the following italicized text has been inserted into the first sentence of Section 3.1:

ESBs are the numeric concentrations of individual chemicals that are intended, *based on the assumptions discussed in Section 1*, to be predictive of biological effects, protective of the presence of benthic organisms, and applicable to the range of natural sediments from lakes, streams, estuaries, and near-coastal marine waters.

(p) Section 3.3. More specific guidance is needed on what constitutes "low" fOC and "low" solids.

Response: This section has been revised substantially and now includes text discussing the range of f_{Solids} values measured in several EMAP estuarine provinces. At this time in this document, an absolute low f_{Solids} value is not recommended. However, in the new Table 3-3, f_{Solids} ranging from 0.36 to 0.65, which correspond to sand contents of 20% and 80%, respectively, were used to compare Equations 3-1 and 3-5. These values were selected because a range of 20% to 80% sand content seemed environmentally realistic.

The low value for f_{OC} used in this document is discussed in Section 4.1 as <0.002 .

(q) Fig. 3-1. Symbols in this figure need to be revised. They are not legible.

Response: The figure has been revised to improve viewing the symbols.

Section 4: *Sediment Benchmark Values: Application and Interpretation*

(r) Section 4.1, first paragraph, last sentence. This statement needs to be clarified. My understanding is that the method is not applicable to sediments with $<0.2\%$ organic carbon.

Response: The sentence described in the comment agrees with the point made by the reviewer.

No change to the document.

(s) Section 4.2.1. and 4.2.2. I STRONGLY object to the language in Section 4.2.1 which states that "...effects may be expected when sediment concentrations are near the $\text{ESB}_{\text{TIER2}}$. As sediment concentrations increase beyond the $\text{ESB}_{\text{TIER2}}$, one can expect chronic effects on less sensitive species and/or acute effects on sensitive species...The FCV and SCV is used to define a threshold for unacceptable effects..." The ESBs are, I believe, intended to be predictive of LACK of biological effects and the ability of these endpoints to predict actual toxic effects is UNKNOWN.

Response: The following italicized text has been added to the beginning of Section 4.2.1:

The ESB_{Tier2} should be interpreted as a chemical concentration below which adverse effects are not expected. In contrast, at concentrations above the ESB_{Tier2} *assuming equilibrium between phases*, effects may occur if the chemical is bioavailable as predicted by EqP theory.

(t) Section 4.2.6, third paragraph. I disagree that chronic sediment toxicity tests have not yet seen widespread use. In my opinion, most regulators now require chronic, rather than acute tests.

Response: The following italicized text has been added to reflect the reviewer's comment:

‘... This is particularly critical given that *a majority of the* sediment toxicity tests conducted at the time of this writing primarily measure short-term lethality (in some cases growth), *although the use of chronic sediment toxicity tests is becoming more common*. Chronic sediment toxicity test procedures have been developed and published for some species (*e.g., U.S. EPA 2001c*) but these procedures are more resource-intensive *as compared to acute tests*.’

(u) Page 4-4, second to last paragraph. Text states that “If the exceedance of an ESB is sufficient that one would expect effects in a toxicity test but they are not observed, it is prudent to evaluate the partitioning behavior of the chemical in the sediment. This is performed by isolating interstitial water from the sediment and analyzing it for chemicals of interest.” EPA should not only finalize and publish its 2003b draft guidance for establishing site specific ESBs, but should also fund studies to validate various methods for isolation of porewater and analysis of concentrations of freely dissolved organic contaminants.

Response: Strongly agree.

Reviewer #4

(a) Secondary Chronic Values

Most of my concerns relate to the development and use of secondary chronic values (SCVs) derived using Great Lakes Initiative (GLI) methods. The GLI approach is one reasonable method for developing water quality benchmarks for chemicals with limited toxicity data. The approach is intentionally conservative, such that in most cases the resulting SCVs are lower than final chronic values (FCVs) would be for the same chemicals. This is well explained (for narcotic chemicals) in Section 2.4 of the document. However, it is my experience that most environmental professionals and regulators do not understand this issue. When released to the regulatory environment, SCVs and other screening values tend to persist, even when new data become available to support more realistic, updated values.

For narcotic chemicals, the Tier 2 ESB compendium also includes ESBs developed according to Di Toro et al. (2000). This approach uses the available toxicity data for entire classes of chemicals to arrive at ESBs for individual chemicals as a function of log K_{ow} . This contrasts with the GLI approach, which

treats each chemical in isolation from related compounds and thus requires conservative uncertainty factors to account for limited data. The narcosis-based SCVs provide a very useful reality check for comparison to the GLI-based SCVs for the same chemicals. For several non-narcotic chemicals included in the compendium, FCVs were available or could be derived, such that a similar reality check is not needed. However, there are five chemicals included in the compendium for which the only information provided is a GLI-based SCV: butyl benzyl phthalate, di-n-butyl phthalate, diethyl phthalate, delta-BHC, and methoxychlor.

For these five chemicals, EPA should either identify a reality check similar to that used for narcotic chemicals, properly update the SCVs, or both. Based on the supporting information provided by Robert Burgess, it is clear that the GLI-based SCVs used in the compendium were developed a long time ago, probably more than a decade. In only a few hours I was able to identify extensive additional information that could be readily incorporated, as follows:

Response: The reviewer makes a very good point about the desirability of updating the aquatic toxicity data for several of the chemicals discussed in the ESB document; specifically: butyl benzyl phthalate, di-n-butyl phthalate, diethyl phthalate, delta-BHC, and methoxychlor. This comment was addressed in two ways. First, a new section has been added (Section 2.5) that reminds the document users that the Tier 2 ESBs are technical information and encourages the update and revision of the ESB values as new high quality data is made available:

2.5 Selection of New and Alternate Aquatic Toxicity Values

As discussed in the Foreword, the ESBs are intended primarily as technical information, not as formal guidelines. As such, the aquatic toxicity values used to derive the Tier 2 ESBs reported in this document are principally recommendations. The conventional (based on WQC and GLI) and narcosis approaches were selected to generate aquatic toxicity values for the 32 chemicals in this document because of their wide usage and acceptance by the scientific, regulatory and regulated communities. As new high quality aquatic toxicity data becomes available, it is encouraged that these Tier 2 ESBs be updated and revised. The GLI approach, as discussed in Appendix A, is one method for performing this update and revision. For example, Adams et al. (1995), Rhodes et al. (1995), Staples et al. (1997), Parkerton and Konkel (2000), and Call et al. (2001) provide additional toxicity data for the three phthalates discussed in this document. Similarly, periodic review of aquatic toxicity databases like ECOTOX may provide new high quality aquatic toxicity values for some of the chemicals discussed in this ESB, especially those for which a limited data base was initially available (see Section 2.3.1).

Second, primarily by chemical, new data was reviewed using an ECOTOX search. The response to Reviewer #4, Comment a-2 below describes the review process. Other responses are more generic, see below.

(a-1) The Suter and Mabrey (1994) compilation from Oak Ridge National Laboratory has been superseded by Suter and Tsao (1996). The SCVs are similar between the two documents, but not

identical. Note that this applies to several of the chemicals in the ESB compendium, not just the five listed above.

Response: Based on this comment, the Suter and Tsao (1996) values have replaced the Suter and Mabrey (1994) values used to derive conventional secondary chronic values in the document.

(a-2) Acute and chronic data for the phthalate compounds are available from Adams et al. (1995), Rhodes et al. (1995), and various studies cited by Staples et al. (1997) and in AQUIRE (e.g., Horne and Oblad, 1983). These data would satisfy several of the missing data requirements these compounds. Also, Call et al. (2001) identified 10-day LC50s for *Lumbriculus variegatus*, which might be appropriate to satisfy the “other phylum” data requirement for multiple phthalates, if viewed as a “greater than or equal to” value relative to a 4-day LC50 for this species.

Response: These data were reviewed and one additional value for diethyl phthalate included in the SCV calculation. The change in SCV is described in Mount (2008). The following text was added to section 1.4 discussing how new data were reviewed:

Literature searches supporting Suter and Tsao (1996), U.S. EPA (1996), GLI (1995) and U.S. EPA (2001a) were conducted in the mid-1990s. In order to capture more recent data, EPA's ECOTOX database (www.epa.gov/ecotox) was searched for any data pertaining to the chemicals evaluated in this document published after 1995. These data were then sorted to identify sources of acute toxicity data for North American species tested for a period appropriate to the species (Stephan et al., 1985) and for which test concentrations of chemical were measured. In addition, literature sources suggested by peer reviewers of this document were also consulted for data meeting minimum requirements. Fewer than 30 additional data points were identified, and only one of these affected the calculation of an SCV (see footnote in Table 3-1). As new, high quality toxicological and geochemical data becomes available, it is encouraged that the ESB values are revised and updated. See Section 2.5 for further discussion.

As alluded to above, the following footnote was added to Table 3-1:

*** = Data summary in Suter and Tsao (1996) did not include a 96-hour LC50 of 131,000 ug/L from Adams et al. (1995). Inclusion of this LC50 in the SCV calculation increased the SCV from 210 to 270 ug/L (Mount 2008).*

(a-3) The work of Parkerton and Konkel (2000) should be reviewed to determine whether it may provide a useful reality check for phthalates, similar to that provided by the narcosis-based SCVs for other compounds.

Response: The paper by Parkerton and Konkel (2000) was reviewed and a comparison of ‘their’ FCVs and the conventional SCVs performed. The following resulting text was inserted into Section 2.4:

Finally, for the three phthalates discussed in this document, ‘FCVs’ derived using the quantitative structure-activity relationship (QSAR) described by Parkerton and Konkel (2000) were compared to conventional SCVs in Table 3-1. ASTER does not classify phthalates as narcotics but there is some evidence they may demonstrate narcotic-like behavior. The QSAR

values derived by Parkerton and Konkel (2000) were 60, 62 and 1173 µg/L for butyl benzyl phthalate, di-n-butyl phthalate and diethyl phthalate, respectively. These values compare relatively well to the conventional SCVs of 19, 35 and 270 µg/L for butyl benzyl phthalate, di-n-butyl phthalate and diethyl phthalate, respectively. From this comparison, the conventional values for phthalates in this document appear to be slightly more conservative than the QSAR based numbers but not tremendously different with ratios ranging from 2 to 4. See Adams et al. (1995), Rhodes et al. (1995), Staples et al. (1997), Parkerton and Konkel (2000), and Call et al. (2001) for further discussion of phthalate aquatic toxicity.

(a-4) For methoxychlor, AQUIRE now contains considerably more data than listed in the supporting documentation, potentially allowing calculation of an FCV.

Response: New data for methoxychlor from ECOTOX were reviewed and found to be unacceptable because the values were for static and unmeasured toxicity tests.

(a-5) For delta-BHC, information in AQUIRE is rather limited. However, the SCV for this compound was taken from Suter and Mabrey (1994). Suter and Tsao (1996) list this SCV as applying to “BHC (other than lindane).” Only a single underlying data point is listed. The *Ambient Water Quality Criteria for Hexachlorocyclohexane* (EPA, 1980) contains several additional toxicity values for BHC other than lindane, and AQUIRE may also contain additional information for BHC that is not specific to delta-BHC. It is unclear why the SCV is listed in the Tier 2 ESB compendium as applying specifically to delta-BHC; perhaps a more inclusive category would be more appropriate.

Response: I agree, the text has been changed to indicate ‘BHC other than Lindane’.

(a-6) The Tier 2 ESB compendium document should also clearly state that ESB users can update the SCVs in the compendium based on additional toxicity data, using the methods described in Appendix A of the document. This is an integral part of the application of SCVs as water quality criteria under the GLI; i.e., the regulated community has the option of locating or generating appropriate toxicity data to revise the criteria. This option is especially important for the ESB compendium because the SCVs were derived so long ago, and I only suggest updating five of them. To facilitate such updates, the documentation supporting the SCV derivation should be made available online. Also, the date of the original literature search should be included for each compound if practical, or the general time period of the literature reviews should be stated in the ESB compendium document.

Response: The following text has been incorporated into the *Abstract*, *Foreword*, *Executive Summary* and *Introduction* to emphasize that the data ESBs should be revised as new high quality data is made available:

As new, high quality toxicological and geochemical data becomes available, it is encouraged that the ESB values are revised and updated.

Further, Section 2.5 has been added to the document (see text above). The response to Reviewer #4 Comment a-2 also addresses this comment.

(a-7) The implications of using GLI-based SCVs should be more clearly explained in Section 4.2.1 (“Relationship of ESB to Expected Effects”) and Section 4.2.6 (“Interpreting ESBs in Combination with

Toxicity Tests”). Currently the issue is not addressed at all in Section 4.2.1; this could be misinterpreted to mean that adverse effects might be expected whenever a GLI-based SCV is exceeded. In Section 4.2.6, the issue is addressed in one sentence, which is not very clear in its current context (i.e., “This situation is made worse because of the use of SCVs to derive some ESB values.”) The discussion in Section 4.2.6 could be revised along the lines of the following:

In other instances, it may be that an ESB is exceeded but the sediment is not toxic. Three possible circumstances may account for this result. First, the toxicity test may be less sensitive than the level of protection intended for the ESB. The ESB is intended to protect relatively sensitive species against both acute and chronic effects . . . As such, one would not expect a . . . concentration near the ESB to cause lethality in a short-term test. Second, a GLI-based SCV may overestimate the compound’s toxicity compared to the intended level of protection, as described in Section 2.4. Third, site-specific conditions may result in lower bioavailability than assumed in the equilibrium partitioning model, as described in Section 3.5. To distinguish these potential explanations, species- and endpoint-specific toxicity information could be used to better interpret toxicity test results, and SCV derivation could be examined. If these lines of evidence do not account for the discrepancy between predicted and observed toxicity, then site-specific chemical partitioning could be investigated.

Response: Based on the reviewer’s comment, portions of her recommended text (in italics) have been combined with text in Section 4.2.6.:

In other instances, it may be that an ESB_{Tier2} is exceeded but the sediment is not toxic. As explained above, these findings are not mutually exclusive, because the inherent sensitivity of the two measures is different. Four possible circumstances may account for this result. First, the ESB_{Tier2} is intended to protect relatively sensitive species against both acute and chronic effects, whereas toxicity tests are performed with species that may or may not be sensitive to chemicals of concern, and often do not encompass the most sensitive endpoints (e.g., growth or reproduction). As such, one may not expect a nonionic organic chemical concentration near the ESB_{Tier2} to cause lethality in a short-term test. Second, a GLI-based SCV, because of the use of SAFs, may overestimate a contaminant’s toxicity compared to the intended level of protection, as described in Section 2.4. Third, site-specific conditions may result in lower bioavailability than assumed based on equilibrium partitioning (see Section 4.2.3). Finally, the organism may avoid the sediment or have other mechanisms to reduce exposure relative to that assumed by the EqP approach. To distinguish these potential explanations, species- and endpoint-specific toxicity information could be used to better interpret toxicity test results, and SCV derivation could be reviewed. Spiked sediment tests could also be used to verify the exposure-response relationship for that particular organism and contaminant. If these lines of evidence do not account for the discrepancy between predicted and observed toxicity, then site-specific chemical partitioning could be investigated (U.S. EPA 2003b).

A reference to this text has been included in Section 4.2.1 (i.e., see Section 4.2.6).

b. Modification for Low- K_{ow} Compounds

Section 3.3, which discusses my modification of the equilibrium partitioning equation for low- K_{ow} compounds (Fuchsman, 2003), could be improved as follows:

(b-1) The graphs in Figure 3-1 seem to have gotten distorted and are difficult to interpret. In addition to fixing the symbols, I recommend connecting the symbols with lines, because the graphs represent continuous mathematical relationships rather than data points.

Response: The graphs have been revised as suggested by the reviewer.

(b-2) The values used to represent extremes of f_{solids} are too extreme. I realize that I used the same values in my paper, though in a more generalized way. In actuality, a sediment containing only 5% solids would be muddy water, and sediment containing 95% solids would be a concretion unlikely to support organisms. Values of 20% and 80% solids would be more realistic.

Response: The f_{solids} values used in Figure 3-1 have been changed to 20% and 80%.

(b-3) It may be useful to spell out why very low organic carbon and very low solids are unlikely to co-occur. That is, mineral components of sediment hold less water than organic material.

Response: See response to Reviewer #1 Comment g.

(b-4) On the other hand, it may be an oversimplification to say that “it is most likely chemicals in this document will occur in environments at concentrations of concern when f_{solids} are low and f_{oc} is high.” Although persistent chemicals would concentrate in this type of sediment (depositional), several of the chemicals in the ESB compendium are not expected to be persistent in sediment. These are the chemicals with relatively low K_{ows} , for which this discussion is most applicable. These chemicals are not found in sediment in the absence of a recent or ongoing source, most typically related to discharge of contaminated groundwater through the sediment. Groundwater will tend to discharge preferentially through more permeable sediments, which may be coarser, with lower organic content and higher solids content.

Response: The following text has been added to Section 3.3:

It maybe possible under conditions where a contaminated groundwater discharge is occurring into a sedimentary environment for f_{solids} to be elevated, f_{oc} to be low, and for low K_{ow} chemicals to be present. Under such conditions, the use of Equation 3-5 maybe warranted.

(b-5) It would be useful to present a realistic example of ESBs that could occur under different site-specific conditions. For instance, the ESB for benzene would be 1200 $\mu\text{g/g}_{oc}$ in a sediment containing 1% organic carbon and 50% solids, compared to the value of 650 $\mu\text{g/g}_{oc}$ listed in Table 3-2.

Response: See response to Reviewer #1 Comment g.

(b-6) Although it is true that “the value f_{solids} is not often reported in sediment investigations,” the

analytical laboratory always measures solids content in order to report sediment concentrations on a dry-weight basis. This should be clarified. One can request this information from the laboratory after the fact, even if it is not specified as a target analyte.

Response: The following text has been incorporated into Section 3.3:

The f_{Solids} values should be available from laboratories conducting chemical analysis on any contaminated sediment samples as part of the determination of moisture content (i.e., $\text{Percent Solids} = 100\% - \text{moisture content (expressed as \%)}).$

(b-7) Also, although it seems obvious, it would be helpful to state that percent solids is equal to 100% minus percent moisture. This is something I have often needed to explain.

Response: See response to Reviewer #4 Comment b-6.

(b-8) Section 3.3, first paragraph, last sentence: the phrase “which is attempting to predict chemical concentrations on the sediment” is confusing and would be better deleted.

Response: The text has been revised to be more clear.

c. Supporting Information for Log K_{ow}

(c-1) If EPA continues to view the compilation by Karickhoff and Long (1995) as the most appropriate source of log K_{ow} values, that document should be made available online as supporting documentation. Although this is an internal EPA document, I have a copy, which I used frequently for years. More recently I have been using the EPIWIN software package instead, because it is publicly available. However, Karickhoff and Long (1995) would really be a better source, in my opinion, if it could be cited as a publicly available document.

Response: The document Karickhoff and Long (1995) has been scanned and made available as a pdf at: <http://www.epa.gov/nheerl/publications/>.

d. Narcotic Chemicals

(d-1) Section 4.2.5 states that “if there are other narcotic chemicals . . . beyond PAHs and the narcotic chemicals with ESB values . . . they can be incorporated into the analysis using parallel procedures . . .” It would be helpful to provide a reference or other information indicating how to identify which chemicals are considered narcotic.

Response: A reference to ‘U.S. EPA (2003e), and the references within’ has been included in the section described (i.e., 4.2.5) to provide the reader with information about narcotic chemicals.

(d-2) The use of the term “narcotic” in this context has been questioned (e.g., Incardona et al., 2006).

This may be primarily a semantic issue, but since this term is used so widely in the document, EPA may wish to contact John Incardona (NOAA) and/or Dominic Di Toro to discuss the best terminology. Even if “narcosis” is retained, it would seem appropriate to acknowledge the debate.

Response: Dominic Di Toro was contacted and the issue raised in the comment discussed amongst our ESB workgroup (i.e., David Mount, Walter Berry and me). The approach referred to in this document as ‘narcosis’ uses a narcosis model to predict acute toxicity and then uses empirical acute-chronic ratios to calculate a chronic toxicity value later used to derive the ESB. Recent studies have concluded that acute toxicity is caused by narcosis while chronic effects are potentially caused by other mechanisms. It is recognized that there is a need to strengthen the predictive link between acute toxicity caused by narcosis and observed chronic effects. To that end, research is currently being proposed to develop mechanistic models addressing this need. For this document, Section 1 has been revised to include the following text recognizing the need to better understand the relationship between narcotic caused acute toxicity and chronic effects caused by other mechanisms:

With regard to using narcosis to derive ESB values, the approach applied in this document and U.S. EPA (2003e) uses narcosis theory to predict acute toxicity and then empirically based acute-chronic ratios (ACRs) to calculate chronic toxicity values. These chronic values (e.g., SCVs) are then used to calculate the ESBs. Strengthening our mechanistic understanding of the link between acute toxicity based on narcosis and chronic effects potentially caused by other forms of toxicity is an active area of research (e.g., Incardona et al., 2006). Users of this document should recognize deficiencies in our understanding of this link may introduce uncertainties into the narcosis based estimates of ESB values.

e. Miscellaneous

I assume that the document will undergo review by an EPA technical editor before being released to the public. Therefore, I have not commented on minor grammatical or typographical issues. Other clarifications are noted below:

(e-1) In Section 2.3.1, the sentence “One exception was the ESB for malathion . . .” should be deleted. It is confusing, and malathion is included in the appropriate list later on.

Response: The section has been revised to be more clear.

(e-2) In Section 4.1, “as discussed further below” should be added after “for narcotic chemicals, the toxicity of mixtures can be considered.”

Response: Agreed, the text ‘(see discussion below)’ has been added to Section 4.1.

(e-3) Section 4.2.6 states “For species contained in the water-only toxicity data for the 32 nonionic organic chemicals discussed here, effect concentrations in sediment can be calculated that are specific for that organism.” This is a good idea, but it is somewhat unclear how it would be applied. Does this simply refer to data mining from the supporting information, or is there a way to extract species-specific

information from the Di Toro et al. (2000) target lipid model?

Response: Species specific predictions can be made using the species specific critical lipid intercepts discussed in Di Toro and McGrath (2000) and Di Toro et al. (2000).

No change to text.

(e-4) Also, I compliment the authors on clearly explaining the conversion between organic carbon-normalized and dry-weight units, and on the clear and detailed presentation given in Appendix A.

Response: Thank you.

Reviewer #4 Recommended References

Adams, W.J., G.R. Biddinger, K.A. Robillard, and J.W. Gorsuch. 1995. A summary of the acute toxicity of 14 phthalate esters to representative aquatic organisms. *Environ. Toxicol. Chem* 14:1569-1574.

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Incardona, J., H. Day, T. Collier, and N. Scholz. 2006. Polycyclic aromatic hydrocarbons are not narcotic toxicants. SETAC North America 27th Annual Meeting, Montreal, Quebec.

Karickhoff, S.W. and J.M. Long. 1995. Internal report on summary of measured, calculated, and recommended log K_{ow} values. U.S. Environmental Protection Agency, Athens, GA.

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Reviewer #5

(a) Overall, yes. The abstract and summary are well written and provide good summaries of the document.

Response: Thank you.

(b) “The Technical Basis Document further demonstrates that if the effect concentration in water is known, the effect concentration in sediments on a $\mu\text{g/gOC}$ basis can be accurately predicted by multiplying the effect concentration in water by the chemical’s KOC.”

As remarked on page 1-4, left column, the presence of black carbon greatly decreases the porewater concentrations of nonionic compounds. This caveat for the great accuracy of the EqP approach should be mentioned.

Response: As a result of this comment, and similar ones from the other reviewers, text has been added to the *Executive Summary* and many other sections of the document reminding the reader that unusual partitioning, as exemplified by black carbon, may affect the predictions and should be considered. The site-specific ESB document (2003b) is often cited as a resource for addressing these situations.

No change to text.

Section 1: Introduction

(c) Page 1-2 left column

Documentation containing the sources and tables of data used to derive some of the Tier 2 ESBs are available in the *References* section of this document (U.S. EPA 2001).

Awkward sentence. Change to “.. References section of a related document (U.S. EPA 2001).

Response: The text discussed in the comment has been revised to read:

In the References section, along with the cited sources, the reference U.S. EPA (2001a) contains the sources and tables of data used to derive some of the Tier 2 ESBs.

The document is available at the web address: <http://www.epa.gov/nheerl/publications/>.

(d) Page 1-4 left column. Is the effect of black carbon on EqP discussed in documents other than U.S. EPA 2003e? Provide relevant references that may not be cited in U.S. EPA 2003e.

Response: The primary discussion of importance of black carbon is U.S. EPA (2003e) because of the demonstrated effects of black carbon on PAH geochemistry and bioavailability. The following black carbon review article reference has been added to the text: Cornelissen et al. (2005). However, because of the large quantity of black carbon literature now available, it is not realistic to cite all of the relevant references. The PAH mixtures document, U.S. EPA (2003e), is up to date with black carbon citations to about 2003.

(e) page 1-4, right column.

ESBs as presented in this document are meant to be used with direct toxicity testing of sediments as a method of sediment evaluation, assuming the toxicity testing species is sensitive to the chemical(s) of interest. They provide a chemical-by-chemical specification of sediment concentrations protective of benthic aquatic life.

Refer reader to Section 4 for details of how ESBs are meant to be used with direct toxicity testing.

Response: Text has been added directing the reader to Section 4.2.6.

(f) Page 1-5, right column.

Toxicity data were evaluated for acceptability using the procedures in the Stephan et al. (1985): *Guidelines for Deriving Numerical National Water Quality Criteria for the Protection of Aquatic Organisms and Their Uses*, the Great Lakes Initiative (GLI, 1995), and the approach for deriving narcotic chronic toxicity values: Di Toro and McGrath 2000, Di Toro et al. 2000, and U.S. EPA 2003e. Data not meeting criteria

Does the title for Stephan et al. (1985) need to be provided?

Response: No, the title has been deleted.

Section 2: Derivation of Equilibrium Partitioning Sediment Benchmarks

g) Section 2.1 is a bit confusing. There is some overlap with Section 1 and information that is not clear to the reader until further developed in the section.

Response: Responding to the comments of this and the other reviewers has reduced the redundancy and attempted to clarify parts of Section 2.1.

h) Page 2-1, right column.

values. Values that appeared to be considerably different from the rest were considered to be outliers and were not used in the calculation. For

Not clear. I don't understand what was done.

Response: I suggest the reviewer examine Karickhoff and Long (1995), this document demonstrates the approach used to select many of the K_{OW} values in the compendium document. The document is available at the web address: <http://www.epa.gov/nheerl/publications/>.

(i) Define all acronyms used in this document (e.g., define ASTER first time it is used; OLS is never defined).

Response: These acronyms, and others, are now defined in the document (e.g., in the *Glossary of Abbreviations*) or removed.

(j) Figs. 2-2 to 2-5. Provide equation and r^2 .

Response: See response to Reviewer #1 Comment 'u'.

Section 3: *Calculation of Equilibrium Partitioning Sediment Benchmarks*

(k) Page 3.1.

bioavailable chemical fraction in a sediment. For nonionic organic chemicals, the concentration-response relationship for the biological effect of concern can most often be correlated with the interstitial water (i.e., pore water) concentration ($\mu\text{g chemical/L interstitial water}$) and not with the sediment chemical concentration ($\mu\text{g chemical/g sediment}$) (Di Toro et al., 1991).

Recent references, i.e., relevant papers published after Di Toro 1991, should be provided.

Response: I disagree, this statement is providing introductory material supportive of the rest of the section. It is beyond the scope of this document to provide a literature-based update if the general principles being expressed have not changed or been refuted. Further, elsewhere in the document, references are provided discussing the bioavailability of specific chemicals (e.g., Di Toro and McGrath 2000).

No change to document.

(l) Section 3.3.

Fig. 3-1 has very faint symbols hard to see on the computer screen and virtually invisible in print. The second and third paragraph of the left column of page 3-6 are confusing and should be re-written.

Response: Figure 3-1 has been re-plotted and the visual clarity improved. The text in section 3.3 has been revised to reflect this comment and the comments of the other reviewers.

(m) Section 3.4

Table 3-3 should be excluded and an example with one compound only provided in the text.

Response: I disagree, this type of table including all of the chemicals is very valuable to many of the users of this document.

No change to text.

(n) Section 3.5 (now Section 3.6)

Move to Section 4.

demonstrated. More common would be instances where toxic effects occur at concentrations below the ESBs because of the additive toxicity of many common contaminants such as metals and polycyclic aromatic hydrocarbons (PAHs) (Alabaster and Lloyd, 1982), and instances where other toxic compounds for which no ESBs exist occur along with ESB chemicals.

More recent citations on synergistic effects should be provided.

Response: To reduce the redundancy between Sections 3.6 and 4.2, the two sections have been combined into one section with Section 3.6 being deleted and relevant text transferred to Section 4.2. More recent citations have been included in the new Section 4.2.

(o) Sampling of porewater is complex and its chemical analysis is expensive. More guidance should be provide concerning the porewater chemical analyses suggested as means of verifying EqP predictions.

Response: A citation to the U.S. EPA sediment collection and manipulation document (U.S. EPA 2001c) has been included, this document discusses approaches for collecting interstitial water. Providing guidance on the chemical analysis of interstitial water is beyond the scope of this document although the site-specific ESB document (U.S. EPA 2003b) does discuss some preliminary aspects of this type of analysis.

(p) Page 3-11, right column, second paragraph. Re-organize paragraph so the two atypical organic carbon are discussed distinctly. Studies addressing the effect of black carbon on partitioning should be cited.

Response: This comment was addressed when revising Section 4.2.

Section 4: *Sediment Benchmark Values: Application and Interpretation*

(q) Section 4.2.3. Merge related text from Section 3.5 into this section.

Response: See response to Reviewer #5 Comment n.

(r) Page 4-4, top of right column.

present. This situation is made worse because of the use of SCVs to derive some ESB_{Tier2} values.

What is meant here?

Response: This statement is in reference to the use of secondary acute factors (SAFs) when using the

GLI approach for calculating SCVs. The use of the factors makes direct comparison of toxicity testing results and calculated values difficult. The effects of SAFs were discussed in detail in Section 2.4. Text has been added to the sentence, noted in the comment, pointing the reader back to Section 4.2.

References Cited in this Memorandum (cited to be consistent with the Compendium ESB document citation scheme)

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