Response to Reviewer Comments

Document: *Developing Sediment Remediation Goals at Superfund Sites Based on Pore Water for the Protection of Benthic Organisms from Direct Toxicity to Non-ionic Organic Contaminants*, US-EPA EPA/600/R-15/289, October 2017

Charge Questions		
Reviewer	**Reviewer's Comment**	**Author's Response**
(1) Is the document written in a style that will be accessible for users with a range of educational and technical backgrounds?		
1	The document is written in a manner that will be accessible for the range of practitioners engaged at Superfund sites.	No response necessary
2	Yes. The document style is clear and accessible. This will be a welcome and useful document for site managers.	No response necessary
3	The document is written in a way that a professional trained in environmental risk assessment will be able to fully understand and adopt the procedures outlined in the document. However, persons not familiar with sediment risk assessment will find it difficult to understand.	There has been a number edits to clarify the methodology. Sections 4 and 5 have been rewritten, and Section 4 now includes a step-by-step example of the methodology
4	The document is generally well written and will be accessible to a wide range of users that are familiar with Superfund site assessments. There are several instances where defining or clarifying terminology or revision text might be helpful as noted in specific comments and proposed text changes included in the attached document.	A glossary of terms has been added, and terminology has been made consistent through-out the document. Responses to suggestions for improvement will be provided in Specific Comment Responses Section.
5	The document does a good job describing the background and fundamentals for measuring interstitial water, relating that water to toxicity data, and developing a useful decision point. There is however, some language used in the examples that should be made simpler, clarified, or described in more detail. For example, there is some interchangeable use of toxicity metrics that are not defined. While the toxicology community is familiar with them, these metrics and their abbreviations will make the document hard to follow (e.g., ER50). I recommend simplifying where possible and maybe including a special text box describing them?	Terms and metrics have been defined, a glossary of terms added, and through-out the document consistent use of terminology. The document now uses pore water through-out. Upon consultation with Superfund "pore water" used instead of "interstitial water"

	There is also some interchange of words used to describe pore water, interstitial water, etc. If we can use consistent language that will be helpful.	
(2) Is the described methodology sufficiently clear to be performed by Superfund remediation project managers, risk assessors, and consultants for Superfund sites? If not, please provide suggestions on how clarity can be improved.		
1	The description of the methodology is understandable. However, there are several places in the document (noted in my specific comments) where additional detail will be required in order for practitioners to implement the guidance in a reliable and consistent manner.	Specific comments will be responded to in the Specific Comment Responses Section below.
2	Yes, but some suggestions for improvement are provided in comments in the text.	Responses to suggestions for improvement will be provided in Specific Comment Responses Section.
3	The document is sufficiently clear till section 4. Section 5 requiring the development of site-specific dose-response curves is a major departure and would require significant skills in sediment toxicology research experience to conduct adequately.	Sections 4 and 5 have been rewritten. Section 4 now includes a step-by-step example of the methodology. Section 5 know discusses how comparisons of toxicity testing data and pore water remedial goals (RGs) should be compared.
4	I think the general 4 step outline describing the proposed methodology is clear. A key practical challenge is step 4 and I think that the authors need to make the point that this step may not be required particularly in light of recent advances in sediment remedies that focus on in-situ amendments where targeting reduction in Cfree is the remedial objective (not mass based sediment concentrations). Further, the efficacy of the remedial action can be confirmed using passive sampling as a monitoring tool. This strategy appears to be overlooked in the present report and should be discussed in section 4 before proceeding to describing approaches used for step 4 which may add significantly uncertainty that could undermine the advantages of applying IWRGs for improved sediment remedial decision-making. I feel table Table 3-1 could be streamlined by presenting a single recommended IWRG (or two values if separate freshwater and marine values) that is intended to provide a chronic protection level rather than presenting multiple values (i.e. SCVs, FCVs, ESBs). This will avoid confusion	In rewriting Section 4, the general 4 step outline has been replaced with a much more detailed and clearer discussion of the methodology. We have added text allowing the use of C_{free} values for remedial goals directly and monitoring the performance of the remedy with passive sampling. We are confused by this comment. There were no ESBs in the Table 3-1. In the revised document, Table 3-1 has ESBs and FCV/SCV for conventional and narcosis toxicants. ESBs were added to Table

	and ensure more consistent application of the contaminant-specific IWRGs that are presented. I also suggest that the authors consider preparing a table of IWRGs corresponding to key sediment test organisms/endpoints for the NOAA 34 PAHs that can then be used for calculating ∑TUs that can be compared to observed toxicity data. This will facilitate consistency by users of the this guidance document in evaluating relationship between site-specific chemistry and toxicity data as described in section 5-3. An alternative would be to provide a simple spreadsheet tool that users could apply for this purpose.	3-1 because the methodology now uses ESBs for screening sediment samples for passive sampling. In the Appendix A, we derive the acute and chronic EC50s for PAHs for *Hyalella azteca*. Remedial goals are site-specific decisions.
5	The section describing how the IWRG should be compared to toxicity data should be strengthened and made a section on its own. This section is currently limited to the bottom of page 32. This section could be expanded to address "how do you evaluate consistency with toxicity data?" Maybe the section could refer to approaches using a weight of evidence/lines of evidence approach? The current section leaves the reader with a question about how to evaluate "consistency"; anything we can do to provide clarity would be helpful.	Section 5 of the document has been revised. We simplified and clarified the text, and explained how the comparisons should be made.
(3) Is the document missing any important concepts, sections, definitions, and/or text that should to be provided in order to make the methodology truly implementable?		
1	As noted above, additional detail and specificity is required in several places that are identified in my specific comments. I recommend in my specific comments that additional thought and development be devoted to considering application of the general approach to bioaccumulative compounds. I also recommend that Section 5 be recast into a description of a multiple lines-of-evidence framework for using the methodology to support decision making.	Specific comments will be responded to in the Specific Comment Responses Section below. Section 5 was revised and discussion of a weight-of-evidence approach added.
2	A glossary that defines acronyms would be helpful.	Glossary added to the document.
3	Yes, see detailed review below	Specific comments will be responded to in the Specific Comment Responses Section below.

4	A key deficiency is a discussion of the two key formats for passive sampling (ex-situ vs in-situ). This issue is briefly mentioned in section 5 but given the importance of sampling format in practical implementation of this - technology in Superfund site assessments this deserves more discussion. - Which format to apply should consider both objectives of the study relative - to the pros/cons of each sampling approach. If the objective is to compare - passive sampling results to lab toxicity tests, ex-situ measurements are - preferred since they are cheaper and can be performed under more - controlled conditions that facilitate equilibrium and translation in reliable - Cfree measurements. If instead the objective is to compare passive sampling - results to observed impacts on field macroinvertebrate communities (or - calibration of a site-specific bioaccumulation model) then in-situ - measurements may be preferable since reliable estimates of actual Cfree - concentrations under field conditions are more essential to the study - objective. As far as I know, limited information in comparing ex-situ vs in-situ site data are available so if reliable estimates of field measurements are - needed then an initial study assessing concordance between approaches - may be warranted in guiding the definitive study design. - In section 2.1 the authors state "measurements from compromised sampler must not be used." However, little practical guidance is provided to - determine when to judge measurements as comprised. It would be helpful - to provide some general criteria: e.g. highly variable results between - replicates; predicted Cfree concentrations exceeding solubility; - chromatograms that are characteristic of oil present in the sediment. - Section 4.2 should also mention that if an evaluation of OC normalization indicates variability in site-specific partitioning of a contaminant is not reduced when compared to dry weight normalization than OC normalization may add little value in the translation step. Further, the potential use of - probabilistic methods should also be acknowledged for evaluating the - uncertainty site-specific sediment-water partition coefficients if translation -	Added Section 2.1.2 Passive Samplers: *In-situ* and *Ex-situ* Measurement. The section discusses the pro's and con's of both approaches. Section 2.1.1 Passive Sampling Fouling now provides guidance for dealing with fouling. Section 4.3.2.1 *Derivation of $C_{S:PWRG}$ values for a sediment with one primary contaminant – dieldrin example* discusses the evaluation of OC normalization and use of probabilistic methods.

	to sediment concentrations are required (currently only a deterministic approach is discussed). Section 4.4 should include an option to include the IWRG as the basis for the remedial decision (not include a translation step that allows the significant uncertainties discussed to be circumvented). If a translation step is included then additional guidance to evaluate key assumption that porewater composition of dissolved phase constituents is would be helpful (e.g. prepare bar charts to visually show relative composition of porewaterPAHs at different total concentrations) Section 5 indicates highlights three types of replicates should be considered but this specifically relates to application of an ex-situ sampling format. Replication for in-situ sampling should also be considered. It is also suggested that the authors may wish to contact Dr. Chiel Jonker who has recently completed a rather extensive inter-laboratory comparison evaluation of ex-situ passive sampling measurements for sediment PCBs and PAHs. The results of this exercise may provide insights on the expected magnitude of variances in Cfree estimates observed between labs, locations, batches within a location and replicate passive sampler measurements.	Section 4.3.1 *Pore Water Remedial Goal Development using C_{free} Values* now provides this option Based upon comments by other reviewers, the text on replication was deleted.
5	The executive summary should indicate this approach represents an important method/scientific approach for incorporating bioavailability into decision making – of course the document gets into this more in section 1. There are no important concepts missing or sections that need to be added. Some minor revision and reformatting (adding an uncertainty section) is recommended for section 5. Minor: Recommend including a small text box of variables defined.	Added text to Executive Summary on incorporating contaminant bioavailability into remedial design. Section 5 was revised to clarify text. Added glossary and felt that small text boxes were not needed.
(4) Are the illustrative examples for determining IWRGs complete enough to demonstrate how the IWRGs are derived?		
1	It would be very useful to practitioners if specific case example applications, even if hypothetical, could be included in an appendix to the document as a way of illustrating the application of the methodology for different decision-making scenarios and/or management applications.	A detailed hypothetical example was added as suggested, see Section 4.3.2.1 *Derivation of $C_{S:PWRG}$ values for a sediment with one primary contaminant – dieldrin example*.

2	Yes, but suggestions for additional examples are provide in the text.	Suggestions will be responded to in the Specific Comment Responses Section below.
3	Yes, these are excellent examples and provide sufficient guidance	No response necessary
4	The authors have attempted to include some useful illustrative examples. A more detailed case study that describes the step by step application of this approach to a specific site and highlights the significant impact of this approach over the default EqP paradigm in deriving sediment remedial goals would be welcomed. However, this may be difficult given publically available site data may not yet be available for this purpose.	A detailed hypothetical example was added as suggested, see Section 4.3.2.1 *Derivation of $C_{S:PWRG}$ values for a sediment with one primary contaminant – dieldrin example*.
5	The figure 4-1 on page 23 could be simplified. The current figure is unclear about the number of steps, yet on page 24 the text refers to step 3 and step 4. Maybe we could make this figure a simple flow diagram (although it loses the concept of returning to sediment concentrations) Step 1: Collect sediment data and measure IW concentrations Step 2: Evaluate bioavailability Step 3: Calculate IWRG Step 4: Convert IWRG to sediment concentration	The Figure 4-1 was deleted. Section 4.4 *Suggested Methodology* provides outline of the methodology.
(5) Is the methodology for deriving interstitial water remediation goals scientifically defensible?		
1	The general methodology is defensible. However, proper application of the methodology will be key, especially in view of the variability in conditions at many Superfund sites and the assumptions and uncertainties associated with the method.	No response necessary
2	Yes	No response necessary
3	Yes, and the guidance needs to state that more clearly.	Sections 4 and 5 rewritten to improve clarity
4	The methodology for establishing IWRGs is based on earlier peer review publications so is considered scientifically defensible (e.g. Burgess et al. 2013). For PAHs, more recent work by Redman et al. can 2014 be cited to further support application of the TLM for chronic protection of benthic organisms. The document offers limited new guidance for establishing IWRGs for additional contaminants of concern other than generating water-only toxicity tests for establishing a species-sensitivity distribution which is costly and may be impractical. However, recent advances in extending the	Redman et al. reference added. Added citation to Kipka and DiToro 2009 on polyparameter linear free energy relationships for the target lipid model.

	target lipid model using polyparameter linear free energy relationships has a much wider chemical domain and could be mentioned as a promising future modeling tool for potentially deriving IWRGs for emerging contaminants of concern for which limited toxicity data are available. One recommendation provided in section 2.1.1 of the report that I believe lacks sufficient technical justification was that the ASTM/EPA SPME method is the best approach for analysis of sediments samples that may be confounded by NAPL contamination. The authors provide little technical basis to show that this technique would not yield measurements that are similarly "compromised". Unless further data can be provided to support this position, it is recommended that the authors simply present this an alternate method that can be considered. The principle advantage of this method is that a standardized test methodology is available. However, this method is not directly comparable to equilibrium sampling and to my knowledge few labs other than Hawthorne perform this method.	We agree and have deleted the text.
5	Yes. This method is scientifically defensible and grounded in a significant amount of published research.	No response necessary
(6) In implementing the methodology, site-specific K_{OC}s are used to convert the IWRGs on concentration basis in sediment interstitial water (µg/L) to concentrations in bulk sediment (µg/kg dry weight). Is the discussion of the K_{OC}s adequate? Is the discussion of the conversion from concentrations in interstitial water to bulk sediment adequate? Is the discussion of which K_{OC}s should be used in the conversions adequate?		
1	As related in my specific comments, I believe additional work is needed in regard to application of the methodology given the varying conditions that can be expected across many Superfund sites in terms of total organic carbon content and the nature of that carbon in respect to partitioning. This is one of the most critical issues that will affect the quality of applications.	We agree and have added text in the revised Section 4 stating that in RI, users need to define: • Nature and variability of the organic carbon content (f_{OC}) of the sediments across the site • Nature and variability of the site-specific sediment-organic carbon-water partition coefficients (K_{OC}s) across the site.
2	*Yes*. Is the discussion of the conversion from concentrations in interstitial water to bulk sediment adequate? *Yes, with one exception as noted in a comment on page 28.* Is the discussion of which K_{OC}s should be used in the	Comments will be addressed in the Specific Comment Responses Section below.

	conversions adequate? *Yes, with one exception as noted in a comment on page 28.*	
3	Discussion of Koc is excellent and adequate	No response necessary
4	I suggest that the authors add two elements to this discussion: evaluating the need for OC normalization and the potential use of probabilistic methods for evaluating the uncertainty site-specific sediment-water partition coefficients in translation to sediment concentrations when this step is needed (see comment 3 above).	We have addressed both issues in Section 4.
5	Yes. This is adequate. In particular the use of a simple and a complex (PAH) example helps the user/reader apply this approach to their own site.	No response necessary
(7) Passive sampling can be performed on any number of samples from a site; for example, on all samples where contaminants are measured in bulk sediment, on only the surface sediments, on the top and bottom of sediments cores, on the top and at the dredge depth of the sediments cores, on surface sediment and based of BAZ (biological active zone), or some other arrangement. Currently, the methodology allows flexibility (makes no recommendation) on which samples are measured using the passive sampling technique and how those data are used in the conversion from interstitial water IWRGs to bulk sediment IWRGs. The extremes in this process are a) perform one passive sampling measurement and assume all sediments are the same across the location of interest (horizontally and with depth) or b) perform passive sampling on all samples and develop 3-D contour plots with depth based upon concentrations in the interstitial water. Should the methodology make a recommendation on this issue? If so, provide your recommendation.		
1	Rather than including only one approach to this within the guidance, I recommend that additional guidance be prepared in the form of a logical framework (including a conceptual model) that project teams could follow in determining how to best apply the methodology for their site conditions and decision-making/management needs.	We provide a generalize methodology that project teams can follow.
2	The issue should at least be discussed. If so, provide your recommendation. The issue of whether to use passive samplers on only the BAZ, or throughout a deeper sediment core will depend on the conceptual site mode (CSM). Does the CSM suggest that contamination at depth could be accessible to site receptors, either now or in the future? Is sediment at depth is not currently bioaccessible and is not expected to be in the future, passive sampling may not be necessary. Would information on the potential bioavailability of samples at depth be helpful in terms of site management? A tiered approach could be used. For example, Koc values in surface	We now discuss the issue and in the ESB Screening Approach methodology, surface sediment samples with concentrations exceeding the ESBs for the contaminants should be passively sampled at a minimum.

	sediment measured using passive samplers are in agreement with standard literature-based Koc values, application of standard Koc values to deeper sediment could be used to assess potential future risk, and passive samplers would not be needed for deeper sediments.	
3	It makes sense to leave the details of how the methodology is implemented to site-specific needs. For many sites I would imagine that the optimal sampling scheme will likely like somewhere between the two extremities outlined above.	No response necessary
4	I agree the guidance should not be too prescriptive given limited practical experience is available in applying this approach to date. It may be helpful to emphasize with some examples that the scope of applying this approach will vary based on study objective. For example, a screening site risk assessment using conventional total sediment concentrations that are organic carbon normalized may indicate that based on EqP assumptions only a very limited spatial extent of sediment appears to pose a potential concern. This area could then logically be the focus of a targeted follow-up study where passive sampling and complimentary effects data (field surveys of benthic health or toxicity tests) are collected. In contrast, if potential risks appear widespread based on conventional characterization of sediment contamination more extensive use of passive sampling may be warranted that includes not only samples from the site but also reference stations so that the comparative bioavailability of contaminants in site sediment can be compared to EqP assumptions and potentially differentiated from reference conditions. Further, at sites where there is a large variation in the magnitude of a sediment contaminant concentrations a key study objective may be to define how bioavailability changes as a function of total sediment contamination since this information will be critical for remedial design. These specifics of the study design will also depend on a variety of practical considerations including cost and time trade-offs and receptivity of the EPA region or state and potentially responsible parties to generate and apply these data in decision-making.	In the suggest methodology, ESBs are used to screen sediment samples after initial field measurements of concentrations of COCs in bulk sediment and organic carbon content of the sediments. Surface sediment samples with concentrations exceeding the ESBs would then be subjected to passive sampling. Text was also added indicating to the user that "Successful and cost effective use of passive sampling requires a good CSM and well defined study objectives for the measurements."
5	The greatest benefit of this document is that it is written as guidance and not too prescriptive. Given the wide range of sites and situations this	We have added a detailed discussion of the approach with dieldrin. The example highlights the

	methodology could be applied it is best to provide the fundamental approach that IWRG are developed, allow the user to become educated in the approach and apply it to their specific site/conditions. There could be some value in including additional discussion describing the scenario above and how a user could approach complex sites and considerations for balancing the cost of IW analysis with the need to characterize the variability at the site. Could we expand on the dieldrin and PAH examples to make this point?	complexities and in Section 4.1 Site Characterization, some of the trade-offs in sampling design are discussed.
(8) Section 5 provides information on comparing toxicity test results and developed IWRGs. Is this section sufficiently clear for the non-experts in toxicity testing and/or passive sampling?		
1	See response to question 3.	See response above
2	Yes	No response necessary
3	This is the section I am most concerned about. Please see the last part of the review below.	Sections 4 and 5 were rewritten/totally revised. Comments will be addressed in the Specific Comment Responses Section below.
4	Please see earlier response to comments 2 and 3 that provide some suggestions for improving section 5. In addition, some readers may not understand Figure 5-2 which does not follow the format of Figure 5-1 in which the x-axis is expressed in terms of toxic units. It would be clearer if it was possible to depict Figure 5-2 as a two panel plot where in the first panel survival vs TUs based on FCV were plotted and on the second panel survival vs TUs based on hyallella acute toxicity was plotted. This would allow you to then make point that later plot is more appropriate for comparison to the empirical toxicity data as indicated in the position of the concentration-response relationship since the TU used reflects the sensitivity of the organism tested. If this is not possible, I suggest adding text to point out that blue dotted line in the current version of Figure 5-2 corresponds to acute critical body burden for Hyalella and shows consistency with the position in the observed concentration response.	Sections 4 and 5 were rewritten/totally revised. Figure 5-2 has four panel plots and walk the reader through the different X-axis units from concentration in bulk sediment, in sediment on an OC basis, in TUs with ESB methodology and TUs using EC50 of Hyalella. Figures 5-1, 5-3, 5-4, and 5-5 have x-axis on a TU basis.
5	This section is somewhat confusing. I'll highlight some specific recommendations below.	This Section has been totally rewritten.

	• The discussion on QA is good to put up front (page 32) • Page 32, third paragraph; Consider using this paragraph or section to describe confounding factors in bioassays rather than focus just on variability between batches of test organisms. These confounding factors are already outlined in sediment tox testing guidance and could merely be listed here as other things to consider. • Page 32, fourth paragraph; this section is very important for application of IWRG. Those involved in sediment management decisions are comfortable using toxicity test data. We need to be more explicit about how to use both IWRG AND sediment tox data together. This section could include describing a "weight of evidence" approach and how these two measures could be used to confirm the results of the other/or not. Regardless it should be expanded. • Page 35-36. The use of the Hawthorne data is important. One challenge is to describe what was done in this study so that it makes sense to the reader; a difficult task. At a minimum there needs to be one paragraph describing the study and the figure 5-2 should be explained. The legend on figure 5-2 should indicate the residues were predicted from the pore water concentrations. I'm not sure what other data set could be used to make the point about differences in test organism sensitivity. Alternatively this section could be left out and merely discussed in generic terms while referring the reader to that paper? • Page 37. On this page the guidance discusses situations where the contaminant drivers may not be known or measured. This whole section could be labeled "Addressing Uncertainties" after the first paragraph of page 37. Then we could include discussions on bioassay confounding factors, organism sensitivities, challenges in measuring pw, etc. • Page 39 last paragraph. Recommend deleting this section on test replicates.	We added a section on "Method Uncertainties and Confounding Factors" We added a Weight-of-Evidence discussion in Section 5.2.3 Approaches for aligning Sediment Toxicity Results with Pore Wtaer RGs. We added text describing the study. X-axis now state "Predicted toxicities" We added a section on "Method Uncertainties and Confounding Factors" Text deleted as suggested.

Response to Specific Comments by Reviewers		
Reviewer	**Reviewer's Comment**	**Authors' Response**
Section 1: Introduction		
1	1. The name IWRG is "conceptually" awkward, as we don't remediate interstitial water. What is being proposed is really a Benthic Bioavailability Remediation Goal, or something similar.	After discussion with Superfund, the major user of this document, text was changed to pore water Remediation Goal
1	2. Page 5. There are two separate issues in this part of the discussion: uncertainty and cost of application. These topics/themes should be highlighted more directly.	We left the text unchanged.
1	3. Page 5 past paragraph. Organic carbon doesn't "cause" toxicity.	Text was changed
1	4. Page 9. Do you mean to exclude P. Mayer's silicone-jar method from the approaches?	Add citation
1	5. Page 10. This approach only contemplates application to Superfund projects?	No. We state that "The methodology is applicable to any site with contaminated sediments"
1	6. Page 10. The proposed method, as described, excludes compounds that are expected to produce greatest concern through bioaccumulation in the food chain, e.g., PCBs. However, bioavailability of the compounds, as reflected by Cfree, is central to the movement of contaminants from the sediment into the food chain. Removing these compounds from inclusion in a more general approach seems unjustified; however, I realize that the method would have to be expanded to include such indirect pathways. Inclusion of this pathway in the method is needed in order for the approach to have meaningful benefit to Superfund.	We removed the text discussing PCBs and PCDD/PCDFs. The scope of the document is the protection of benthic organisms from direct toxicity. Expanding the document to pelagic organism is beyond the scope of the document.
New Title: Section 2: Estimating the Freely Dissolved Concentrations of Nonionic Organic Chemicals in Sediment Pore Water **Old Title: Section 2: Estimating the Freely Dissolved Concentrations of Nonionic Organic Chemicals in Sediment Interstitial Water**		
1	7. Page 12. The guidance should provide specific information about how a user should determine whether they have a NAPL problem or	We added specific information to the section to aid the project team in this situation.

	not. How much NAPL is too much NAPL for the method to perform, overall, at a site? How would a project team go about determining this?	
1	8. Page 13, last sentence. How would a user determine their level of confidence?	Based upon comments by other reviewers, we deleted this paragraph.
New Title: Section 3: Establishing Adverse Effects Concentrations in Sediment Pore Water for Benthic Organisms **Old Title: Section 3: Establishing Adverse Effects Concentrations in Sediment Interstitial Water for Benthic Organisms**		
1	9. Page 14, first sentence. Having a benchmark that determines the level above which effects are unacceptable is different than having a benchmark that sets the level below which effects are acceptable. Which do you mean in this case?	Text was edited to clarify
1	10. Page 14, paragraph 1, sentence beginning with "Because". This is a weak argument for justifying the adoption of chronic exposures. You could say that in the majority of contaminated sediment sites populations can be exposed to contaminants for the entire life cycle organism and multiple generations.	Text was edited to clarify
1	11. Page 14, first paragraph. Is the use of 95% of tested species a policy of Superfund?	Superfund commonly uses EPA's AWQC as ARARs (Applicable and Relevant and Appropriate Requirements) at sites and by their application, Superfund is using the above policy.
1	12. Page 15. If a Superfund site identified a particular benthic species or taxon as the receptor of concern, could the method allow for such an application as an alternative to using the FCV, which potentially considers a much larger group of taxa?	That is a site-specific decision by Superfund.
1	13. Page 15, second to last sentence. There is a log unit of variation around the line. This variation would seem to have implications for the reliability of applying the method. The document should discuss those implications.	We agree there is variation around the 1-to-1 line in the figure. The figure shows the LC50s of most sensitive water column and benthic species by individual chemical. EPA's AWQC uses the four Genus Mean Acute Value (GMAV) closest to the

		cumulative probabilities closest to the 0.05 probably from the combined water column and benthic species SSD. By virtue of using the GMAV of the four most sensitive species, the FAVs and FCVs in the AWQC are "weighted" averages of the four GMAV values.
New Title: Section 4: Implementation of the Pore Water RG Approach within RI/FS Process **Old Title: Section 4: Implementation of the IWRG Approach**		
Authors' response: Section 4 was rewritten in response to comments from all five reviewers. As a result, responding to the reviewer comments below on individual basis cannot be done. In the rewrite of Section 4, the methodology is now described in context of Superfund's RI/FS process. Specifically, what should be performed in the Site Characterization, Baseline Risk Assessment, and Alternative Development phases of the RI/FS. Under Alternative Development section, the example with dieldrin has been greatly expanded and discussed. The discussion walks the reader through the methodology step-by-step from site characterization to baseline risk assessment, and then, alternative development. In Site Characterization, addition to defining the nature, extent, and variability of the COCs, the users are clearly told to define: • Nature and variability of the organic carbon content (f_{OC}) of the sediments across the site • Nature and variability of the site-specific sediment-organic carbon-water partition coefficients (K_{OC}s) across the site. Further, we recommend that f_{OC} be measured on all sediment samples. In Alternative Development, options are provided for remedial goals (RGs) to be set using C_{free} in the sediment pore water or using concentrations in the bulk sediment. In the last section of Section 4, i.e., 4.4 Suggested Methodology, the document outlines the approach for implementing the methodology.		
1	14. Page 22. Would be beneficial to include a list of specific sediment parameters that should be measured, and how, as a part of "bulk sediment" analysis the data are to be used in the IWRG method. Also, would be useful to include a scenario in the description where you have widely varying contaminant bioavailability due to differences in OC content, or the nature of the OC present. This scenario will cause challenges in using the method in decision-making and management for large, heterogeneous sites.	The information on Site Characterization in the rewritten Section 4 provides "what should be measured".

1	15. Page 24 last sentence third paragraph and first sentence fourth paragraph. This description does not provide sufficient guidance. How should a user determine what is small or wide variability?	Text not used in revised Section 4.
1	16. Page 24. Kocs?	Text not used in revised Section 4.
1	17. Page 24, fourth paragraph. Variability in Koc across a site presents a major challenge to applying this method. This paragraph does not provide sufficient guidance to address this problem.	Text not used in revised Section 4.
1	18. Page 25, first full sentence. This guidance is not sufficient. You're telling them to be conservative without specifics for how to do this or a discussion of the implications of doing so.	Text not used in revised Section 4.
1	19. Page 25, second to last sentence. What should be considered too large? The guidance here is not sufficient.	Text not used in revised Section 4.
1	20. Page 28. How does all of this work if the nature of the OC varies across the site?	Text not used in revised Section 4.
1	21. Page 28, last paragraph. These alternative approaches may simplify the arithmetic, but I don't think they simplify the assessment, considering the assumptions involved. What are the implications of using these "shortcuts"?	Text not used in revised Section 4.
1	22. Page 31. The mixture problem is a challenge with the method. Are there any test cases you can refer to that evaluate how robust the method with respect to the assumptions of independent action and additivity? One approach to simplifying the problem would be to recommend screening out contaminants that are not expected to make a meaningful contribution to toxicity.	We do not know of test cases. We agree with the concept of simplification and in Section 4.3.2.3, we discuss mixtures and implications of major and minor levels of toxicants.
New Title: Section 5: Use of Passive Samplers, Toxicity Testing Results, and Pore Water RGs **Old Title: Comparison and Evaluation of Toxicity Testing Results and IWRGs**		

Authors' response: Section 5 was rewritten in response to comments from all five reviewers. As a result, responding to the reviewer comments below on individual basis cannot be done. In the rewrite of Section 5, the scope and information provide was changed dramatically. Reviewers thought that Section 5 in the review draft was describing how to implement the methodology, and clearly, that was not the intent for the section. With the Section 4 rewrite where the methodology is described in detail, Section 5 was shorten and focused substantially.		
1	23. Page 32 and following. The presentation of information and text in Section 5 of the document confused me. I recommend that that the material contained in this section be reorganized with the purpose of providing guidance to the field practitioner on how the described method can be used as a part of a multiple lines-of-evidence approach to support remedial decision making.	The entire Section 5 was rewritten to address the reviewer comment.
1	24. Page 32, "…and there are a host of issues that could arise with the passive sampling technique." This statement and following text is not organized or presented in a way that contributes to guidance for field application. More text and explanation is needed here.	In the revised document, *Section 5.3 Method Uncertainties and Confounding Factors* was added. The revised text addresses the comments.
1	25. Page 33, section 5.2, first sentence. This sentence says that toxicity tests provide toxicity data. It's an odd first sentence.	Text was deleted.
1	26. Page 37, first paragraph. Are you recommending that a Superfund project should develop water-only chemical data if they don't have a full dose-response curve? I question the practicality of this as a recommendation.	We are not recommending that site's develop water-only chemical data. We provide this option, but it is not required. The text was moved to Section 3.5 in the revised document.
1	27. Page 37, second paragraph. How many cases are there of Superfund projects using TIE? My sense is that there would be a very small number of cases, separate from R&D studies conducted with Superfund site sediments. I question the practicality of this recommendation. I think you need a stronger argument here to justify the additional effort involved.	Recommendation has been removed.
1	28. Page 37, third paragraph and elsewhere. I question the practicality of the brief recommendations for additional sampling. This would be more implementable at small, spatially simple sites. However, the general call for collecting more data begs the question of how	Recommendation has been removed.

	much data is needed in order to apply the method. The existing text does not treat this issue of data adequacy in a sufficient way. There are significant pressures at play at Superfund sites regarding data collection. Recommendations for additional data should be supported by guidance that provides the means for justifying the effort and expense of collecting the data.	
1	29. Page 38, Section 5.5, first paragraph. The discussion of in situ vs. ex situ approaches is not sufficient to provide guidance to Superfund project teams. In addition to uncertainties and complexities, the purpose or intention to be served by the data should determine whether in situ or ex situ approaches would be most applicable. This fact and others are missing from the discussion. I don't see how a Superfund project manager would make use of this brief discussion. I think it would be beneficial to discuss some specific case applications of in situ and ex situ methods to illustrate the main points and distinctions.	We add a short section providing guidance on in-situ and ex-situ measurement.
1	30. Page 39, second paragraph. The discussion of multiple replication schemes is too open-ended to be useful guidance. In fact, I think project managers will find this discussion off-putting, given the implied costs. Need to better define how the information about variance would be used for the types of decisions Superfund teams are supporting.	This text was deleted based upon the comments by Reviewer 5.
Appendix		
	31. Page 40. The information provided in the Appendix introduces these issues but doesn't provide guidance that is specific or detailed enough for Superfund project teams to apply. What specific information would a project team need to draw upon to reach a decision regarding these issues and how to deal with them?	The text in the appendix has been merged into the document directly. Further, the information provided has been expanded to help project teams deal with the two issues.

Response to Specific Comments by Reviewers		
Reviewer	**Reviewer's Comment**	**Authors' Response**
Section 1: Introduction		
2	I am a bit unclear on the rationale for excluding crab and lobster. Are they really expected to biomagnify contaminants to such an extent that consideration of direct toxicity from exposure to sediment would not be appropriate? Suggest qualifying this statement to state that "…not designed to explicitly protect higher trophic level species from effects associated with food chain biomagnification."	Made correction
2	As noted in previous EqP and ESB guidance documents, is there still a Kow cutoff value below which this methodology should not be applied? Or is that not a consideration if porewater concentrations are being measured rather than estimated?	There is no mention of a K_{OW} cut off in the document. Pore water measurements define the bioavailability of the COCs regardless of the K_{OW}s of the COCs
2	Interesting. This suggests that direct toxicity of these classes of compounds to benthic invertebrates need not be considered? Otherwise, it is unclear why these compounds should be different from other compounds. They may have additional and more important effects on higher trophic level organisms, but still could exert effects on invertebrates.	Text was deleted.
New Title: Section 2: Estimating the Freely Dissolved Concentrations of Nonionic Organic Chemicals in Sediment Pore Water **Old Title: Section 2: Estimating the Freely Dissolved Concentrations of Nonionic Organic Chemicals in Sediment Interstitial Water**		
2	First mention of "black carbon". Might want to define here or earlier.	Defined the term.
2	Suggest that COC not be used here due to potential confusion with common abbreviation for "Contaminant of Concern"	Removed "COC"
2	This last clause is a bit vague. Suggest being more specific or delete.	Text was deleted
New Title: Section 3: Establishing Adverse Effects Concentrations in Sediment Pore Water for Benthic Organisms **Old Title: Section 3: Establishing Adverse Effects Concentrations in Sediment Interstitial Water for Benthic Organisms**		
2	Might be useful to state that any nonionic hydrophobic organic contaminant is expected to contribute to narcosis, though some may have other modes of action as well.	We considered modifying the text. However, the following sentence states "all narcotic

		presents will contribute additively" and thus, we have covered the issue.
2	Again this brings up the question of whether this approach could be used as a line of evidence to assess potential for toxicity based on direct exposure to sediment-associated contaminants to epibenthic organisms (e.g., crustaceans and benthic fish), which an acknowledgment that biomagnification must also be considered, as well as other modes of toxic action that are not applicable to invertebrates.	We changed the text as suggested by the reviewer in Purpose and Scope section on this issue. This change addresses this comment.

New Title: Section 4: Implementation of the Pore Water RG Approach within RI/FS Process
Old Title: Section 4: Implementation of the IWRG Approach

Authors' response: Section 4 was rewritten in response to comments from all five reviewers. As a result, responding to the reviewer comments below on individual basis cannot be done. In the rewrite of Section 4, the methodology is now described in context of Superfund's RI/FS process. Specifically, what should be performed in the Site Characterization, Baseline Risk Assessment, and Alternative Development phases of the RI/FS. Under Alternative Development section, the example with dieldrin has been greatly expanded and discussed. The discussion walks the reader through the methodology step-by-step from site characterization to baseline risk assessment, and then, alternative development.

In Site Characterization, addition to defining the nature, extent, and variability of the COCs, the users are clearly told to define:

- Nature and variability of the organic carbon content (f_{OC}) of the sediments across the site
- Nature and variability of the site-specific sediment-organic carbon-water partition coefficients (K_{OC}s) across the site.

Further, we recommend that f_{OC} be measured on all sediment samples.

In Alternative Development, options are provided for remedial goals (RGs) to be set using C_{free} in the sediment pore water or using concentrations in the bulk sediment. In the last section of Section 4, i.e., 4.4 Suggested Methodology, the document outlines the approach for implementing the methodology.

2	Suggest providing additional discussion here. Please refer to comments provided directly to the charge questions.	In rewriting Section 4, this text was deleted.
2	Can you provide some guidance on level of variability that is considered sufficient? Maybe at a minimum should be < factor of 10?	The revised section contains field data for the variability in f_{OC} and K_{OC}. For log K_{OC}, its coefficient of variation are approximately 5% based upon data from three sites.

2	Suggest that detailed example calculations be provided for the OC-normalized approach and the total PAH approach.	In rewriting Section 4, this text was deleted.
2	Is there a need to discuss how contribution of non-site-related contaminants are treated? Must all contaminants of potential concern by summed?	Background is big issue for many sites. Risk decisions must include consideration of both site COCs and COCs from outside of the site.
New Title: Section 5: Use of Passive Samplers, Toxicity Testing Results, and Pore Water RGs **Old Title: Comparison and Evaluation of Toxicity Testing Results and IWRGs**		
Authors' response: Section 5 was rewritten in response to comments from all five reviewers. As a result, responding to the reviewer comments below on individual basis cannot be done. In the rewrite of Section 5, the scope and information provide was changed dramatically. Reviewers thought that Section 5 in the review draft was describing how to implement the methodology, and clearly, that was not the intent for the section. With the Section 4 rewrite where the methodology is described in detail, Section 5 was shorten and focused substantially.		
2	It would also be useful to suggest that the investigator collect samples from rapid TAT analysis of COCs in order to identify a subset of samples for tox testing that cover the full range of site sediment concentrations.	Deciding on what samples to perform toxicity testing on is a site-specific decision.
2	Need to emphasize the species-specific evaluation is typically used to establish consistency and causality, not for establishing site-specific clean up goals.	In the rewrite of Section 5, we discuss the point raised by the reviewer.
2	I think this language is too strong. Suggest "…may be unable to accurately capture.	Text on ex-situ and in-situ rewritten.
2	If this level of detail is going to be provided, I would suggest that you should also provide some guidance (or point the reader to a source of guidance) on the acceptable level of variability. Also suggest that you provide technical names for these specific types of replicate samples.	Text was deleted.
2	Suggest that a summary paragraph be added at the end of this document. Seems like a letdown to end with a discussion of uncertainties. Also likely want to perhaps put the method uncertainty in perspective relative to the uncertainites associated with bulk sediment sampling.	Based upon the revisions to Section 4 and 5, we don't believe a summary paragraph is needed. We added section "5.3 Method uncertainties and confounding factors" to address the latter comment.
Appendix		
The text in question below has been edited substantially. Guidance is now provided.		

2	Not clear why this case is different from other potential COCs. In all cases, don't you recommend that tox be confirmed with corresponding tox tests? Or does this imply that IWRG can never be used to screen out areas of PAH-contaminated sediment if tox tests are not included? If that is the case, I think the data to support this guidance should be published or presented in this document.	The text in question below has been edited substantially. Guidance is now provided.
2	I think either the data should be shown here, or this reference and discussion should be deleted.	The text in question below has been edited substantially. Guidance is now provided.
2	Need more detailed discussion of why industrial waterways are prone to having high levels of alphatics. Due to fuel oil spills?	The text in question below has been edited substantially. Guidance is now provided.

Response to Specific Comments by Reviewers		
Reviewer	**Reviewer's Comment**	**Authors' Response**
Section 1: Introduction		
3	**Pg 6 last paragraph**: At some point here or in the next section it may be helpful to make the distinction between total and freely dissolved concentrations (C_{free}) in interstitial water. For Kepone with a log K_{ow} of 5.4 or so, the two may not be very different for low DOC porewaters, but for higher K_{ow} compounds the difference becomes larger. The explanation of C_{free} provided in paragraph 3 of pg 8 needs to move up and we need to explain at the outset that Interstitial Water is really C_{free}.	We added a paragraph on Page 9 in the revised draft that has this information.
3	**Pg 8, para 2, line 8:** This should be: "Since freely dissolved concentration in interstitial water corresponds to…	Made edit
3	**Pg 8, para 2 last 2 sentences**. This is an excellent way to present the concept.	No response required.
3	**Pg8, para 2, last sentence:** This sentence can be misleading. C_{free} is not the only bioavailable pool of the chemicals. Chemicals loosely associated with solids are also bioavailable as they are extractable in the gut and exchanges rapidly with C_{free}. I would suggest taking this sentence out.	We disagree.

3	**Pg 10:** '… it is presumed that their primary ecological risk would occur via accumulation and …..": True, but at contaminated sediment sites, exposure to higher trophic level organisms is influenced largely by C_{free} which controls exposure to fish through flux into overlying water and accumulation in benthic organisms (diet). So, with the right model, interstitial water concentrations can be used to perform calculations of uptake in fish and other higher trophic level animals. (See Fadaei et al. Environ. Sci. Technol. 2015, 49, 12405−12413)	The sentence was deleted based upon comments by other reviewers.
New Title: Section 2: Estimating the Freely Dissolved Concentrations of Nonionic Organic Chemicals in Sediment Pore Water **Old Title: Section 2: Estimating the Freely Dissolved Concentrations of Nonionic Organic Chemicals in Sediment Interstitial Water**		
3	**Pg 11, line 6: should be** 'sample' not sampling	Correction made.
3	**Pg 13, 2nd sentence: add:** "Deuterated internal standards are added before introduction of a fiber to the isolated …" **Pg 13, line 3:** "…the fiber is extracting some of the internal standards and dissolved contaminants…." **Pg 13, line 5 onwards should be:** "The fiber is then thermally desorbed and analyzed for target contaminants. The dissolved concentrations are calculated based on the ratio of analytes to corresponding internal standards. This process creates an operationally defined form of C_{free} (i.e. interstitial water minus colloidal and dissolved organic matter precipitated by alum).	Corrections made.
New Title: Section 3: Establishing Adverse Effects Concentrations in Sediment Pore Water for Benthic Organisms **Old Title: Section 3: Establishing Adverse Effects Concentrations in Sediment Interstitial Water for Benthic Organisms**		
3	No comments by reviewer	
New Title: Section 4: Implementation of the Pore Water RG Approach within RI/FS Process **Old Title: Section 4: Implementation of the IWRG Approach**		
Authors' response: Section 4 was rewritten in response to comments from all five reviewers. As a result, responding to the reviewer comments below on individual basis cannot be done. In the rewrite of Section 4, the methodology is now described in context of Superfund's RI/FS		

process. Specifically, what should be performed in the Site Characterization, Baseline Risk Assessment, and Alternative Development phases of the RI/FS. Under Alternative Development section, the example with dieldrin has been greatly expanded and discussed. The discussion walks the reader through the methodology step-by-step from site characterization to baseline risk assessment, and then, alternative development.

In Site Characterization, addition to defining the nature, extent, and variability of the COCs, the users are clearly told to define:

- Nature and variability of the organic carbon content (f_{OC}) of the sediments across the site
- Nature and variability of the site-specific sediment-organic carbon-water partition coefficients (K_{OC}s) across the site.

Further, we recommend that f_{OC} be measured on all sediment samples.

In Alternative Development, options are provided for remedial goals (RGs) to be set using C_{free} in the sediment pore water or using concentrations in the bulk sediment. In the last section of Section 4, i.e., 4.4 Suggested Methodology, the document outlines the approach for implementing the methodology.

3	**Pg 20, last 5 sentences:** Excellent! This is exactly what I was hoping this would lead to.	No response required
3	**Pg 23, Figure 4-1.** There are several inconsistencies in this figure. I would suggest redoing this figure completely. Maybe best to use a simple flowchart. It is not clear what the captions mean for the boxes versus the arrows. If the arrows are actions and boxes are values calculated, these need to be consistently applied. The second arrow states the use of Equations 4-2, 4-3, and 4-4. But none of these equations yield concentrations in water in ug/L as indicated in the caption of the subsequent boxes.	Figure was deleted.
3	**Pg 24, paragraph 4:** I am surprised to see no reference to organic matter types and presence of black carbon in the explanation of Koc variability. A brief mention of the role of BC and the challenge with different forms of BC in sediments will be helpful here.	In rewriting Section 4, on page 31, discussion on carbon types was added.
3	**Pg 24**, last line: replace 'with' with 'which'	In rewriting Section 4, this text was deleted.

New Title: Section 5: Use of Passive Samplers, Toxicity Testing Results, and Pore Water RGs
Old Title: Comparison and Evaluation of Toxicity Testing Results and IWRGs

Authors' response: Section 5 was rewritten in response to comments from all five reviewers. As a result, responding to the reviewer comments below on individual basis cannot be done. In the rewrite of Section 5, the scope and information provide was changed dramatically. Reviewers thought that Section 5 in the review draft was describing how to implement the methodology, and clearly, that was not the intent for the section. With the Section 4 rewrite where the methodology is described in detail, Section 5 was shorten and focused substantially.		
3	**Pg 32:** I have several major concerns with this entire section: 1) Are sediment toxicity studies always required at every Superfund site? Note that that the IWRG approach is also helpful in many site assessments outside the Superfund program where actual sediment toxicity assessments will not be performed. The way this guidance is written, it appears that without the consistency evaluation with site toxicity data, the IWRG approach is not defensible.	With the total rewrite of Section 5, this comment has been addressed.
3	2) **Pg 37, lines 7-8:** Sounds like the goal here is to obtain a full dose-response curve. However, it is quite possible for a site to have no toxicity associated with the target chemicals.	With the total rewrite of Section 5, this comment has been addressed.
3	3) **Pg 37, first paragraph, last sentence and remaining** text: This does not make sense. It appears that EPA is proposing an approach that it is not confident of and requiring the user to demonstrate consistency for each site. That sounds like further research for every site! This was not the spirit of the original ESB document where calculated porewater concentrations could be used to determine the extent of potential toxicity for a range of chemicals. 4) An user should not have to develop chemical dose response curves for site-specific sediment toxicity. Rather, the user should be able to use this guidance to determine to what extent a chemical or class of chemicals is potentially responsible for sediment toxicity. The idea of site specific toxicity confirmation proposed in this guidance is a major departure	With the total rewrite of Section 5, this comment has been addressed.

	from past ESB guidance. For example in the 2012 ESB document (EPA/600/R-02/012), a tiered approach to risk assessment is proposed (as shown in Figure 4-1 copied below). An user could use the measured C_{free} to make a tier 2 assessment if sediments were toxic or not and whether to move to tier 3 and further refine the assessment with actual toxicity studies. I would strongly suggest maintaining continuity with the tiered structure of risk assessment as done previously	The suggested methodology in the revised Section 4 is in-line with the 2012 ESB document. The ESBs Screening Approach methodology first compares concentration to ESB values. For samples with concentrations greater than the ESBs, passive sampling is done.
3	**Pg 39, section on method replication:** This section on replication is vague. Need more specifics here. Some RI/FS efforts are taking the replication of porewater similar to what one would do for sediment samples – duplicates for every 10 or 20 samples. Most sediment samples are measured once. There is a reference here to eight replicates for each sample as in the standard tox studies. This would be an absolute overkill and waste of effort. I would rather suggest performing more sediment samples than do 8 replicates of each. We should have a good sense of how replicable passive sampling is and provide a more specific suggestion here. In my experience, passive sampling is inherently more precise than typical biological response measurement.	The text on replication was deleted in the rewrite of Section 5. Other reviewers suggested deleting the text.
Appendix		
3	No comments by reviewer	

Response to Specific Comments by Reviewers		
Reviewer	**Reviewer's Comment**	**Authors' Response**
New Title Page: Developing Sediment Remediation Goals Based on Pore Water at Superfund Sites for the Protection of Benthic Organisms from Direct Toxicity **Old Title Page: Deriving Sediment Interstitial Water Remediation Goals (IWRGs) at Superfund Sites for the Protection of Benthic Organisms from Direct Toxicity**		

4	The document is focused more on how IWRGs can be applied at superfund sites rather than simply on derivation since the proposed IWGs are not new and have in fact served as the basis for deriving historical EqP sediment quality benchmarks; what is new is the advent of passive sampling methods to quantify Cfree in sediment porewater and support incorporatation into superfund site assessments so that resulting data can be compared to IWRGs .. a more apt title might be "Applying Sediment Interstitial …"	After a fair amount of discussion, deriving was changed to developing. The methodology develops pore water RGs. These RGs can be expressed using C_{free} in sedment pore water or converted into bulk concentrations in sediment ($C_{S:PWRG}$ values) using $f_{OC:SS}$ and $K_{OC:SS}$ values.
Section 1: Introduction		
4	also highlight that since approach not causal-based cannot provide insights as to what contaminant responsible for observed effect; think this is an important disadvantage	Text was edited slightly
4	need to better explain that traditional extraction of porewater for organic contaminants quantifies total concentrations that include both free and bound forms	On page 9, added text on freely dissolved chemical.
4	and apply? This is where use of new technologies come in	Made change
4	see comment on document title; this highlights focus is on applying IWRG not sampling deriving IWRGs which is the focus of section 3	After a fair amount of discussion, deriving was changed to developing. See discussion above
New Title: Section 2: Estimating the Freely Dissolved Concentrations of Nonionic Organic Chemicals in Sediment Pore Water **Old Title: Section 2: Estimating the Freely Dissolved Concentrations of Nonionic Organic Chemicals in Sediment Interstitial Water**		
4	need to describe two passive sampling formats .. in-situ vs ex-situ including pros/cons and relevance for addressing different study objectives; i.e. linking PS results to lab tox testing vs comparing field exposures to site macrobenthos data or use in calibration of site fate/bioaccumulation model	Section 2.1.2 Passive Samplers: In-situ and Ex-situ Measurement was added to address this comment.
4	Suggest also referencing Reible paper as highlights practical application using disposable PDMS fibers Reible, D., and G. Lotufo. 2012. Final Report: Demonstration and Evaluation of Solid Phase Microextraction For the Assessment of Bioavailability and Contaminant Mobility. ESTCP Project No. ER-200624. ESTCP, Alexandria VA.	Added citation.

	http://serdpestcp.org/content/download/15588/177293/file/ER-200624-FR.pdf. Accessed 23 October2012	
4	would be helpful to provide some general criteria to identify compromised measurements; highly variable results between replicates; predicted concentrations exceeding solubility; chromatograms that are characteristic of oil	The text has been added and guidance provided.
4	technical basis to support that SPME method proposed is a better alternative and not confounded by NAPL is not clearly supported; unless you have objective data to support this recommendation would simply provide this method as an alternative that can be considered	Made edit as suggested.
New Title: Section 3: Establishing Adverse Effects Concentrations in Sediment Pore Water for Benthic Organisms **Old Title: Section 3: Establishing Adverse Effects Concentrations in Sediment Interstitial Water for Benthic Organisms**		
4	If ESBs reflect current state of science and are preferred to FCVs/SCVs why not present only ESB values for chemicals where this value is available and FCVs or SCVs for other chemicals where ESB is not available; this will help ensure consistency in IWG values used in risk evaluation of contaminated sediments; Including both EBSs and SCVs/FCVs for the same substances may lead to confusion / inconsistencies	ESBs are expressed using ug/kg-OC in the sediment. FCVs/SCVs are expressed using ug/L in water. ESBs are used as a screening value where sediments with concentration less than the ESBs warrant no further consideration and sediments with concentration greater than the ESBs, would have passive sampling measurements.
4	Suggest adding a brief discussion of the more recent analysis presented by Redman et al. that show similar SSDs between benthic and aquatic species Redman et al. 2014 EXTENSION AND VALIDATION OF THE TARGET LIPID MODEL FOR DERIVING PREDICTED NO-EFFECT CONCENTRATIONS FOR SOILS AND SEDIMENTS Environmental Toxicology and Chemistry, Vol. 33, No. 12, pp. 2679–2687, 2014	Added discussion of Redman et al paper as suggested.
4	This value refers to a FCV not SCV; further while the acute effects characterization in the TLM assumes narcosis the empirical ACRs used in derivation of the FCV in principle captures other potential mode of	Edited column heading and made consistent with Burgess et al. 2013

	toxic action .. it may be best to simply refer to these values as TLM-based FCV or ESB	

New Title: Section 4: Implementation of the Pore Water RG Approach within RI/FS Process
Old Title: Section 4: Implementation of the IWRG Approach

Authors' response: Section 4 was rewritten in response to comments from all five reviewers. As a result, responding to the reviewer comments below on individual basis cannot be done. In the rewrite of Section 4, the methodology is now described in context of Superfund's RI/FS process. Specifically, what should be performed in the Site Characterization, Baseline Risk Assessment, and Alternative Development phases of the RI/FS. Under Alternative Development section, the example with dieldrin has been greatly expanded and discussed. The discussion walks the reader through the methodology step-by-step from site characterization to baseline risk assessment, and then, alternative development.

In Site Characterization, addition to defining the nature, extent, and variability of the COCs, the users are clearly told to define:

- Nature and variability of the organic carbon content (f_{OC}) of the sediments across the site
- Nature and variability of the site-specific sediment-organic carbon-water partition coefficients (K_{OC}s) across the site.

Further, we recommend that f_{OC} be measured on all sediment samples.

In Alternative Development, options are provided for remedial goals (RGs) to be set using C_{free} in the sediment pore water or using concentrations in the bulk sediment. In the last section of Section 4, i.e., 4.4 Suggested Methodology, the document outlines the approach for implementing the methodology.

4	Before proceeding to step 4 you may want to point out that remedial design does not necessarily need to be based on bulk sediment concentrations. For example, if in-situ amendments are applied the focus will be on ensuring pore water concentrations are sufficiently reduced so that total sum toxic units < 1. This is likely to be an increasing consideration in future remedies which underscores relevance of step 3. Further this strategy would avoid the potential large uncertainties introduced in step 4 that are subsequently discussed. Hence, I think there is a unique opportunity in this document to help identify that Cfree may be used directly as the basis for remedial design and passive sampling as the monitoring tool for assessing remedial effectiveness	This recommendation has been incorporated into the methodology.

4	The paper by Witt et al provides a nice example illustrating how vertical porewater concentrations can be determined in sediment cores using passive sampling and might be a useful reference to cite here Witt, G., G. A. Liehr, et al. (2009). "Matrix solid-phase microextraction for measuring freely dissolved concentrations and chemical activities of PAHs in sediment cores from the western Baltic Sea." Chemosphere 74(4): 522-529.	Citation has been added.
4	This implies a deterministic approach; a probabilistic approach could also be considered where the observed uncertainty in Koc is incorporated into the translation; If this uncertainty has large cost implications on remedial alternatives this needs to be communicated to risk managers as input to cost benefit decisions	The use of probabilistic approaches are discussed in the methodology.
4	One may also want to consider recommending that site-specific sediment water partition coefficients be calculated with and without Foc normalization to see if coefficient of variation is or is not reduced; our experience is that OC normalization sometimes increases rather than decreases the CV; for such sites it may be justified to apply bulk sediment-water partition coefficients for translation, point is site data needs to be critically analyzed to justify technical basis of performing this translation step (if required) to support the remedial design	This recommendation has been incorporated into the methodology.
4	It should be noted that the site-specific Log Koc in this example is very close to the default EqP estimate of 5.28 (Burgess et al. 2013) so does not suggest bioavaibility at this site has been significantly reduced relative to EqP assumptions	No response required.
4	Both approaches assume that the relative distribution of TUs contributed from each the PAHs in porewater remains similar as a function of total sediment concentration; this hypothesis can be tested by examining bar charts of the relative porewater conc (or TUs) for each PAH (or indicator PAH) in site samples with different total concentrations; again the uncertainty in this approach could be quantified and incorporated into the translation if warranted	The text on aggregating mixtures was deleted because specific guidance on how to aggregate cannot be given. Every site will be different because the composition of the mixture will be different, and compositions will, in all likelihood, change across individual sites.

4	A more straightforward approach that should be mentioned is to simply used dissolved IWTU=1 as the objective and not try to express the IWG in terms of total sediment concentrations … again this would be particularly relevant for in-situ amendment remedial designs in which the relative distribution of porewater concentrations will be altered as a result of introducing an additional sorptive phase (thus violating the assumption of the other two proposed approaches)	The text on aggregating mixtures was deleted because specific guidance on how to aggregate cannot be given. Every site will be different because the composition of the mixture will be different. The methodology now states that the total TUs ≤ 1.0.
New Title: Section 5: Use of Passive Samplers, Toxicity Testing Results, and Pore Water RGs **Old Title: Comparison and Evaluation of Toxicity Testing Results and IWRGs**		
Authors' response: Section 5 was rewritten in response to comments from all five reviewers. As a result, responding to the reviewer comments below on individual basis cannot be done. In the rewrite of Section 5, the scope and information provide was changed dramatically. Reviewers thought that Section 5 in the review draft was describing how to implement the methodology, and clearly, that was not the intent for the section. With the Section 4 rewrite where the methodology is described in detail, Section 5 was shorten and focused substantially.		
4	Not a good criterion since PSM derived Cfree can be very different from EqP predictions and in fact this is the reason for applying PSMs	Changed text. From: "Are the freely dissolved concentrations estimated using generic K_{OC}s close to those measured by passive sampling?" To: "Are the freely dissolved concentrations estimated using generic K_{OC}s greater than those measured by passive sampling?"
4	Is there value in waiting to finalize this document until this companion document is available to ensure section 2 of this report is aligned and draws on key recommendations from this document?	No.
4	It is suggested that the authors consider preparing a table of IW toxicity values for the most common sediment test organisms/endpoints to support these calculations for comparison to sediment toxicity test results; a spreadsheet too could also be provided for this purpose	A table of toxicity values for common test organisms is a great idea. In Appendix A, we derive acute and chronic EC50 values for PAHs for *Hyalella azteca*.
4	Not clear what should be done and implications of this point in current site practice	In the rewrite of Section 5, this text was deleted.
4	Suspect many readers will not follow this and will not understand x-axis in Figure 5-2 which does not follow format of Figure 1 that is expressed in terms of toxic units; would be clearer if you had 2 panel plot where you plotted survival vs TUs based on FCV and survival vs TUs based on	We changed the X-axis to Predicted Toxic Units in Sediment Pore Water using EPA's FCV for Dieldrin.

	hyallella acute tox .. then make point that later plot is more appropriate for comparison to tox data and shows tox response consistent with theory If not possible to replot these data as proposed would add text to point our that blue dotted line in Figure 5-2 corresponds to acute critical body burden for hyallea and shows consistency with observed conc response	We added a plot with the X-axis having units for Predicted Toxic Units in Sediment Pore Water using *Hyalella azteca* toxicity value derived in Appendix A of the document.
4	Before proceeding to TIEs which involve significant effort would be helpful to address interim steps to address lack of consistency; e.g. assess potential for ammonia, sulfide or metal toxicity or poor sediment substrate (very low TOC) .. TIE should be considered only after existing data evaluated for determining potential toxicity causes; TIE may not always be required so suggest modifying text accordingly	Text has been edited and strongly suggests that users under why toxicity outliers exist in their data. Ammonia, sulfides, and metals added to the text.
4	To match the breakpoint in the dose response toxic units need to be based on the organism-specific toxicity value not the FCV .. to support these calculations it may be helpful to provide a table of IWG values for key sediment toxicity test organisms and endpoints that can be used for TU calculations give Cfree data; this could also be coded up into a simple spreadsheet tool that could accompany this document (see previous comment)	We demonstrate this effect with the data of Hawthorne. In Appendix A, we derive acute and chronic EC50 values for PAHs for *Hyalella azteca*.
4	Is this the recommendation? See previous comment .. why not recommend plotting observed effects vs organism/endpoint specific TUS?	This text was deleted in the rewrite of Section 5.
4	In-situ vs ex-situ formats for PS needs to be introduced earlier and discussed in more detail as central issue in practical use of this technology in the context of this report	Section 5.1 Passive Samplers: In-situ and Ex-situ Measurement was added
Appendix		
4	Question if these issues warrant a separate appendix; first point can be covered in early section when TIEs are discussed; second issue can be mentioned section 3 for chemicals where no or insufficient water only-toxicity data are available to derive a SVC/FCV; it may be also	Appendix has been eliminated. Both sections have been merged into main body of the text.

	appropriate for certain chemicals with poor hazard data to consider polyparameter TLMs for deriving toxicity benchmarks which can be applied to a wider domain of chemicals .. see below citation that describes this approach Kipka & DiToro (2009). Technical basis for polar and nonpolar narcotic chemicals and polycyclic aromatic hydrocarbon criteria. III. A polyparameter model for target lipid partitioning ENVIRONMENTAL TOXICOLOGY AND CHEMISTRY Volume 28, Issue 7, July 2009, Pages: 1429–1438, Undine Kipka and Dominic M. Di Toro	Discussion of the Kipka and DiToro reference added.

Response to Specific Comments by Reviewers		
Reviewer	**Reviewer's Comment**	**Authors' Response**
General		
5	Minor edit. Page 24. Rather than use the term Kocs, refer to them as Koc values so the reader doesn't get confused with Koc:ss….	Correction has been made throughout the document.
Appendix		
5	The appendices might be included in the text above to simplify the document. Appendix 6.1 could be moved to the example on PAH. Appendix 6.2 could be added to the discussion on specific specific data.	Appendix has been eliminated. Both sections have been merged into main body of the text.