United States Environmental Protection Agency, Region II
New York, NY

LOWER PASSAIC RIVER
LOWER EIGHT MILES FOCUSED FEASIBILITY STUDY

REPORT OF THE PEER REVIEW OF SEDIMENT TRANSPORT, ORGANIC CARBON AND CONTAMINANT FATE AND TRANSPORT MODEL

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1.0 INTRODUCTION

The Focused Feasibility Study Area (FFS Study Area) is the lower eight miles of the Lower Passaic River in northeastern New Jersey (NJ), from the river's confluence with Newark Bay at River Mile (RM) 0 to RM8.3 near the border between the City of Newark and Belleville Township (Figure 1-1). The FFS Study Area is part of the Lower Passaic River Study Area (LPRSA), which is the 17-mile, tidal portion of the Passaic River, from Dundee Dam (RM17.4) to the confluence with Newark Bay (RM0), and its tributaries. During a comprehensive study of the 17-mile LPRSA, the sediments of the FFS Study Area were found to be a major source of contamination to the rest of the Lower Passaic River and Newark Bay. Therefore, the United States Environmental Protection Agency (USEPA) completed a Focused Feasibility Study (FFS) to evaluate taking action to address those sediments, while the comprehensive study of the 17-mile LPRSA is on-going.

To address the persistently elevated and wide-spread contaminant of concern (CoC) concentrations in FFS Study Area sediments that are causing unacceptable risks and health hazards, the FFS evaluates the following four remedial alternatives:

1. No Action (also called "Monitored Natural Recovery" or MNR in the modeling reports).

2. Deep Dredging with Backfill involves dredging all contaminated fine-grained sediments throughout the FFS Study Area and placing two feet of sand backfill in the dredged areas. It results in the restoration of the authorized navigation channel in RM0-8.3.

3. Capping with Dredging for Flooding and Navigation (also called "Full Capping" in the modeling reports) includes dredging of enough fine-grained sediment so that an engineered sand cap can be placed over the FFS Study Area without causing additional flooding and to allow for a navigation channel between RM0.0 and RM2.2.

4. Focused Capping with Dredging for Flooding includes dredging of fine-grained sediments in selected portions of the FFS Study Area (about one third of the FFS Study Area surface) with the highest gross and net fluxes of COPCs and COPECs to a depth of 2.5 feet so that an engineered sand cap can be placed over those portions dredged without causing additional flooding. It does not include construction of a navigation channel.

One of the important decision-making tools used in comparative analyses of the four remedial alternatives evaluated in the FFS is a mathematical model that simulates sediment transport, organic carbon and contaminant fate and transport processes. The sediment transport, organic carbon and contaminant fate and transport models for the LPR were based on an existing, peer reviewed model of the NY/NJ Harbor Estuary developed by the Contamination Assessment and Reduction Program (CARP). The CARP model was modified to be more applicable to conditions in the LPR. Sediment transport results provided input to organic carbon and contaminant fate and transport models. The objective of the sediment transport modeling was to develop a mathematical representation of the processes affecting sediment transport behavior, so that simulated sediment transport results could be used to assess the transport of sorbed contaminants in the fate and transport modeling. The objective of the organic carbon and contaminant fate and transport modeling was to develop a mathematical representation of the processes affecting contaminant fate and transport behavior of dissolved and sorbed contaminants based on the associated sediment transport modeling results. It is anticipated that the model could then be used to assess the effects that implementation of the four remedial alternatives would have on future surface sediment concentrations and their associated risks and health hazards.

In February 2013, HDR|Hydroqual presented an overview of the FFS Sediment Transport, Organic Carbon and Contaminant Fate and Transport Model for the Lower 8 miles of the Lower Passaic River to five peer reviewers, chosen as experts in their fields.

The peer reviewers were charged with determining whether the LPR-specific modifications to the CARP model have produced a tool that is adequate for USEPA to use in the FFS to compare the relative effects that implementation of each of the four remedial alternatives would have on future surface sediment concentrations. The peer reviewers were tasked with reviewing the following documents that are appendices to the FFS: Lower Passaic River Sediment Transport Model Report, Draft (Appendix B2) and Lower Passaic River Contaminant Fate and Transport Model Report, Draft (Appendix B3) [both drafts dated January 31, 2013]. The review was structured as a "letter peer review" in accordance with the 2006 USEPA Peer Review Handbook (EPA/100/B-06/002), which means that the panel members performed their reviews and provided their written comments separately, without physically convening. However, the reviewers were provided contact information for each of the panel members and encouraged to collaborate as desired by telephone and e-mail. The FFS Sediment Transport, Organic Carbon and Contaminant Fate and Transport Model report was initially delivered to the peer reviewers on February 1, 2013; however, because of formatting issues, a revised version was prepared and delivered electronically on February 8, 2013. A set of charge questions for the reviewers to answer (presented in Section 3.0) was sent to the panel via email on January 29, 2013. The Louis Berger Group Inc. (Louis Berger) convened an orientation conference call among the peer reviewers, HDR|HydroQual, USEPA, and the U.S. Army Corps of Engineers (USACE) on February 6, 2013 to provide an overview of the project and the model, and to clarify the charge to the peer reviewers.

On February 27, 2013, Louis Berger convened a midpoint conference call with all of the reviewers, HDR|HydroQual, USEPA and USACE to follow up on progress and discuss any early issues. The peer reviewers' comment reports were due by March 11, 2013, and all comment reports were received by that date.

On March 20, 2013, Louis Berger convened a final conference call with the peer reviewers, HDR|HydroQual, USEPA, and USACE. The purpose of this call was to request clarification of selected comments submitted by the reviewers and to explore possible resolution of apparent differences of opinion. Following this call, the reviewers were given the opportunity to make revisions to the draft comments and submit a final version by March 26, 2013.

This report contains the peer reviewers' comments (Attachment A) and a compilation of the comments, with responses (Attachment B). Chapter 2 identifies the peer reviewers and their fields of expertise, while Chapter 3 presents the charge to the peer reviewers at the start of their work. Chapter 4 summarizes several key issues distilled from the written comments and addressed in the final call. Chapter 5 describes several broad conclusions discussed during the summation of the peer review final call, and Chapter 6 summarizes the actions that USEPA has taken to address those conclusions.

2.0 PEER REVIEWERS

The five reviewers are listed in this section along with a short biography outlining their experience and background as applicable to the FFS Sediment Transport, Organic Carbon and Contaminant Fate and Transport Model.

Robert Ambrose is a senior environmental engineer who has developed and applied water quality models for 36 years. He retired from the USEPA in 2009, and provides professional assistance part time. Following an M.S. degree in civil engineering at the Massachusetts Institute of Technology (MIT) in 1974, Mr. Ambrose developed and applied estuarine dissolved oxygen and eutrophication models for the USEPA Annapolis Field Office. In 1978, he transferred to USEPA's Office of Research and Development laboratory in Athens, Georgia, developing general-purpose water quality and toxic chemical models of surface water bodies for application to various regulatory programs. He participated in teams developing watershed and water body modules for multimedia risk assessment methodologies including the analysis and regulatory control of mercury. Mr. Ambrose has been a leader in the development, distribution, training, and primary support of the USEPA standard water quality model WASP for 30 years. Throughout his career, his emphasis has been on providing USEPA and the modeling community with practical technology and then assisting in its proper use.

Richard Bopp is an Associate Professor in the Department of Earth and Environmental Sciences at Rensselaer Polytechnic Institute. His educational background includes training in geological sciences and chemistry from Columbia University and the MIT. His areas of research include the analysis of persistent contaminants of concern in the Lower Passaic River and the dating of sediment cores using the analysis of radionuclides to determine contaminant level chronologies in water systems. Dr. Bopp has done extensive research in the Lower Passaic River, Dundee Lake and other nearby rivers, bays and estuaries. He served on the New York-New Jersey Harbor Estuary Contaminant Assessment and Reduction Program (CARP) Model Evaluation Group (MEG) and on the Lower Passaic River Restoration Project's Technical Advisory Committee (TAC). However, he has not substantially contributed to the development of the models undergoing review, or provided significant consultation during the development of the models, consistent with the Peer Review Handbook's definition of an independent peer reviewer.

Bruce Brownawell is an Associate Professor at State University of New York at Stony Brook. His educational background is in Chemistry and Oceanography, and he holds degrees from DePaul University and the Massachusetts Institute of Technology. Dr. Brownawell's research has included sediment transport study and modeling, as well as studies of persistent chemicals in marine or estuarine environments. He served on the CARP MEG and on this project's TAC. However, he has not substantially contributed to the development of the models undergoing review, or provided significant consultation during the development of the models, consistent with the Peer Review Handbook's definition of an independent peer reviewer.

Joseph DePinto is a Senior Scientist at LimnoTech. He has extensive experience (including 27 years as a Professor of Environmental Engineering) conducting aquatic system research, education and management programs, with an emphasis on activities in the Great Lakes region. Through his work on topics such as nutrient cycling-eutrophication, toxic chemical exposure and bioaccumulation, contaminated sediment assessment and remediation, aquatic ecosystem structure and functioning, and watershed, river, estuary, and lake modeling, Dr. DePinto has become internationally recognized as a leader in the development and application of surface water models to address aquatic ecosystem issues. Dr. DePinto's studies have led to over 150 publications, and he has also worked on numerous councils, task forces, and advisory groups on water quality research and management.

Wilbert Lick is a professor of Mechanical and Environmental Engineering at the University of California at Santa Barbara. His education includes degrees in Aeronautical Engineering from Rensselaer Polytechnic Institute. Dr. Lick's expertise and research interests center on environmental fate and transport of contaminants in surface water and groundwater, numerical methods and mathematical modeling. He is one of the developers of the sediment transport model, SEDZLJ, being adapted for use in the Lower Passaic River Restoration Project and has previously served on this project's TAC. However, he has not substantially contributed to the development of the models undergoing review, or provided significant consultation during the development of the models, consistent with the Peer Review Handbook's definition of an independent peer reviewer.

3.0 CHARGE QUESTIONS

In addition to their review of the modeling reports, the peer reviewers are also tasked with providing input on the following questions, with full explanations supporting their conclusions:

1. Are the physical, biological and chemical processes represented in the model adequate for describing sediment transport, organic carbon and contaminant fate and transport for the LPR, with particular focus on the FFS Study Area?

2. Have the appropriate data sets been properly and adequately used to set up the model input parameters and define forcing functions and initial conditions for the sediment transport, organic carbon and contaminant fate and transport models?

3. Does the model adequately represent the spatial and temporal distributions of the COCs in the water column and sediment bed for USEPA to use it as a tool to compare the relative effects that implementing each remedial alternative will have on FFS Study Area surface sediment quality?

4. Does the model adequately account for the contributions of COC sources that may recontaminate FFS Study Area sediments during and after implementation of each remedial alternative?

5. Does the model adequately account for the effect of extreme storm events contributing to the resuspension and redistribution of contaminated sediments for USEPA to use it as one tool to compare the effects that implementing each of the four remedial alternatives will have on FFS Study Area sediment COC concentrations?

4.0 KEY ISSUES

The comments and answers to the charge questions from each of the reviewers are provided in Attachment A.

Several key issues were distilled from the written comments and addressed in the final peer-review conference call on March 20, 2013. In this section, these key issues are summarized and the reviewers' responses, suggestions and dissenting views are presented.

4.1 Sediment Accumulation (Infilling)

The sediment transport modeling report presents comparisons between bed elevation changes (and volumes and areas) derived from bathymetric surveys and model simulation results for two time periods: 1996-2004 (single beam data) and 2007-2010 (multibeam data). The report indicates that the agreement between simulated and data-derived depositional volumes is much closer for the period between the 2007 and 2010 multibeam surveys than for the period between the 1996 and 2004 single-beam surveys. The sediment accumulation rate in the 1996-2004 period was a factor of two to three greater than during the 2007-2010 period. The report concludes that the model's general agreement with the more recent multibeam bathymetric data sets provides a degree of confidence that the model is producing results consistent with a system that is reaching or has reached a quasi-equilibrium bathymetric condition.

The reviewers expressed concern over the model-data comparison for sediment accumulation (infilling) between 1996 and 2004. For instance:

One reviewer commented, "The data and model tend to tell a coherent story despite the large degrees of temporal and spatial variability and uncertainty. Model results averaged over large areas in the LPR are more likely to be accurate than model results for hot spots. It is not clear how well the model might represent future conditions with altered bathymetry. All of these points are recognized properly by the modelers."

Another reviewer stated, "Are these integrated estimates of net deposition meaningful which I suspect they are at least over the 1996 – 2004 period. The fact that the surface was accreting so much over this period over much of the FFS area, under perhaps more average conditions between these years, and that the model does not reflect deposition in many of the these one mile reaches has me concerned again that there is a bias towards over-prediction of erosion and an associated under-prediction of net deposition.*[sic]*"

A third reviewer wrote, "A major problem, that to me remains unresolved, is the deposition, infilling, and subsequent consolidation of sediments in the proposed navigation channels. Figure 6-3 indicates that the model (and the associated discussion in the report) does **not** predict rapid infilling. This is curious since historically there was rapid infilling of the previous navigation channel during its life and after dredging was stopped; this infilling is the essential basis for the present problem of contaminated sediments in the LPR and therefore needs a better quantitative understanding than there is at present."

The remaining two reviewers also commented on the computed amount of sediment accumulation, but focused more on its effect on simulated contaminant concentrations.

4.2 Rate of Change in Simulated Surface Sediment Contaminant Concentrations

The reviewers offered comments on the model forecasts of future contaminant concentrations for the MNR and active remedial alternatives.

One reviewer stated "My judgment is that over the next three decades, the LPR would see somewhat lower contamination levels than predicted by the MNR, and at least a bit higher levels than predicted by the Deep Dredging and Full Cap."

Another reviewer commented, "I am not convinced, given the great mobility of surface sediments in this system and propensity for downstream transport of sediments in the RM 17-8 reach, that figures 6-3, 6-11, 6-19 should *[sic]* virtually no recontamination over the roughly 45 years following initiation of remedial actions."

A third reviewer expressed a similar opinion, "…I do not have confidence in the 'no significant long term re-contamination' prediction of the full cap model on an 'upper 15 cm, area average' basis."

Another reviewer wrote, "The other troubling aspects of the remedial action scenario projections is that following erosion events there are sometimes sharp blips in the sediment concentrations in the RM 0-8 region, but these concentrations dissipate with characteristic times perhaps less than a year. The only explanation for this that I can come up with is that the contaminant clean cap gets dusted with deposited contaminated sediment and then it is swept out of the area by subsequent resuspension and lateral exchange processes (erosion)…"

Another reviewer's comment on the simulated 2,3,7,8 TCDD concentrations in the top 15 cm of the RM0-8, RM1-7, and RM8-17 reaches over the 1995-2055 period for the remedial alternatives is, "The final decision on the remediation of the LPR (i.e., where and how much to dredge and cap) will depend on results similar to those in Figure 6-3. These results are primarily dependent on sediment dynamics and the forcing of this dynamics by the hydrodynamics." He focused his comments more on the underlying hydrodynamic and sediment transport models as they affect the contaminant model results.

4.3 Need for Additional Analyses

Several of the reviewers suggested additional analyses to support the modeling analyses presented in the reports. Some recommendations are for including additional periods or conditions in simulation period, and others were related to presenting existing model results in different ways.

4.3.1 EXTREME FLOW EVENT

Several of the reviewers commented on the maximum flow condition included in the model simulations and recommended that a 100-year flow be included. One reviewer recommended "If the question of extreme events is important enough, I recommend that the calibration/verification be extended to simulate the Irene event. This would be especially useful if further contaminant surveys are available to test against the model. Then this event could be included near the end of the long-term simulations evaluating the four management alternatives."

4.3.2 SENSITIVITY ANALYSES

Several reviewers recommended additional sensitivity analyses to help readers understand the results from the complex suite of models. These included sensitivity simulations for extreme flow events (mentioned above), a reduced depth of particle mixing in the bed of the contaminant model, wind-driven resuspension, and the magnitude of the upstream suspended solids, which should be propagated through the carbon and contaminant models.

5.0 CONCLUSIONS

This section contains a summary of the major conclusions of the Peer Review and is followed in Section 6 by a list of additional analyses that are or will be performed to address the Reviewers' comments.

5.1 Model Framework

The peer reviewers offered differing opinions on the sub-models included in the FFS modeling approach.

One reviewer wrote, "I think that the model framework, which consists of a linked hydrodynamic-sediment transport-organic carbon sorbent-contaminant fate and transport model is about as close to a complex state-of-the-science model as we will find for the kind of management questions being asked for the FFS." And with respect to the organic carbon model, "I think the organic carbon model has value in this over framework, because the hydrophobic partitioning of the PTS is represented on the basis of organic carbon in the system (see section 2.2.1.1 of BIII) and a sediment transport model with specified OC fractions would, in my opinion, not necessarily produce the correct level of distribution between particulate, freely dissolved, and bound dissolved chemical. Furthermore, the OC model also provides valuable information about redox conditions and sulfate reduction in sediments that are important for metal partitioning to acid-volatile sulfide (important for determining bioavailable fraction) and mercury methylation in sediments." Another reviewer also offered favorable comments about the model framework, including, "Overall, I believe that the processes incorporated into the sediment, organic carbon, and toxicant models are appropriate and justified."

A third reviewer was more critical of the organic carbon model, stating, "As discussed in the conference call, I really don't think the organic matter fate model is necessarily appropriate or useful. I hope that this model does not really matter that much and suspect that it may not especially for high Kd/Koc contaminants that are not greatly affected by outputs of the sediment diagenesis model – on the other hand, for Cd and Hg, outputs of the model such as AVS, oxygen, and sulfate reduction rates affect in some manner the sediment "preservation", inter-compartmental transfer, transformations (methylation of Hg), or water column scavenging or whatever mechanism is responsible for getting low Kd Cd into sediments."

Another reviewer questioned the complexity of both the carbon and contaminant models, stating, "…what is needed is a hydrodynamic model, a sediment transport model, and a simple contaminant transport model where the contaminant is completely sorbed to the sediment particle. It also follows that a complex carbon model and complex chemical fate and transport models are not needed. As a simple but reasonably accurate carbon model, it may be assumed that carbon may vary from one size particle to the next, but carbon always stays with the particle." In response to this recommendation that the carbon and contaminant models be simplified to reduce the computational time of those models, which would then allow more time for the sediment transport modeling, it was clarified that the computational resource requirements of the sediment transport model far out-weigh those of the carbon and contaminant models.

It is clear that the different backgrounds of the members of the peer review panel generate different perspectives on the appropriate level of complexity of the model framework for the FFS. The FFS modeling framework will continue to include the hydrodynamic, sediment transport, carbon, and contaminant sub-models.

5.2 Sediment Accumulation and Rate of Change in Contaminant Concentrations

As discussed in Section 4 – Key Issues above, the reviewers expressed concern over the rate of decline in contaminant concentrations in the MNR simulation and the rate of recontamination in the active remediation simulations. A reviewer expressed the opinion, "Can the model be trusted enough to compare the relative effects that implementing each remedial alternative will have on FFS Study Area? Despite some reservations, I believe the model can indeed distinguish between the MNR alternative and the two more extensive remedial alternatives – Deep Dredging and Full Cap."

One reviewer recommended additional analyses of existing simulations to help in the evaluation of the simulated responses to remedial alternatives: "I realize that the last section of the BIII report (section 6-4) provides cumulative contaminant fluxes across several transects of the lower river over time, and that analysis proved very instructive relative to the system's response. But I think a much more instructive and illustrative analysis would have been to develop a full model-computed mass balance diagram (all inputs and outputs and change in control volume mass) for the river segments between those transects at several points in time (or over several specified time intervals) of the remediation and post-remediation simulations. If indeed, the remediation trends shown in figures 6-3 and 6-11 (for examples) are correct, one could understand why the sediments are not being recontaminated at all following remediation. I urge the modelers to conduct these diagnostics to convince themselves of the accuracy of the remediation scenario forecasts."

Another reviewer commented, "With the likely need for additional work, this is a good set of models that I believe are well structured, especially for recalcitrant hydrophobic chemicals where description of redox chemistry is less important than it potentially is for

Hg and even Cd. It is clear to me that the model can be used as "one tool" for evaluating remedial alternatives. If I were charged with making expensive management decisions based only on this model, I would have to say today let's wait for more information to be provided and incorporate additional targeted model testing into decisions that may not need to wait very long." He further commented that it will be useful to know how normalization of observed chemical concentrations to carbon, Fe, and Al, compare with model results over the calibration period. However, because Fe and Al are not simulated by the model, it is not possible to follow that approach for the model results.

The sediment transport modeling report acknowledges that the computed rate of sediment infilling is less than that derived from the 1996-2004 single beam bathymetry surveys and the basis for placing more emphasis on the 2007-2010 period is because the later period more closely reflects conditions that are expected in the future. It is acknowledged that confidence in the conclusions of the modeling analysis would be increased if the model results were in better agreement with historical sediment accumulation data, since the bathymetry considered under the Deep Dredging alternative is deeper than present conditions. This is also true for the Full Capping alternative, although to a lesser degree, because the bathymetry is deepened in only the lower two miles of the LPR.

Consistent with the recommendations of the peer reviewers, the sediment transport model has been revised in terms of parameterization of erosion properties and structure of the surface layer of the bed (described more in the following section), and shows improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be presented in the revised modeling report, along with diagnostics/additional presentations of the type suggested by the reviewers

6.0 NEXT STEPS

In response to the peer reviewers' concerns and recommendations summarized above, USEPA is undertaking a series of tasks to strengthen the Sediment Transport, Organic

Carbon and Contaminant Fate and Transport Models as key decision-making tools for the FFS. The peer reviewers' written comments have resulted in a number of detailed changes to the hydrodynamic-sediment transport model framework and the period used for calibration. The models will be applied to an expanded range of sensitivity analyses, and additional detail will be included in the final modeling reports.

Changes have been made to the hydrodynamic-sediment transport model to allow bathymetry changes due to erosion and deposition to feed back to the hydrodynamic calculations at a substantially increased frequency. Previously, bathymetry updates were made at the end of each year of simulation; they will now be made every ten time steps (i.e. every 10 to 150 seconds of simulation time). Changes have been made to the sediment transport model to incorporate a 1 mm layer at the surface of the bed to represent the pool of easily erodible sediment deposited on the bed during slack water and resuspended during the following flood or ebb tide. This layer, referred to as the "fluff" layer, allows for a sharper gradient in critical shear stress and erosion rate between the surface layer and the first subsurface layer of the bed than the previously-used continuous function. Erosion properties of the sediment bed have also been revised based on a re-analysis of the consolidation experiment data. These changes in the bed discretization and erosion parameterization have been introduced to achieve additional infilling.

The calibration period has been extended through water year 2012 to include the high flow associated with Hurricane Irene and the period of the Chemical Water Column Monitoring Program implemented by the Cooperating Parties Group (CPG)[1] under USEPA oversight.

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[1] CPG is a group of approximately 70 potentially responsible parties who signed an agreement with USEPA in 2007 to perform the 17-mile LPRSA remedial investigation and feasibility study.

Changes have been made in the development of initial conditions for contaminants in the sediment. A spin-up simulation will be performed to improve the assignment of initial conditions in the RM7-17 reach of the LPR, where data from the period around 1995 were too sparse spatially to use for assigning initial conditions. Previously, 2008 data were used to fill the data gap in initial condition data for the RM7-17 reach of the LPR. The slow decline in contaminant concentrations between 1995 and 2008 resulted in computed concentrations in the RM7-17 reach for 2008 that were lower than the 2008 data. As pointed out by a reviewer, this could influence the computed effect of RM7-17 sediments on other parts of the domain during the 1995-2008 portion of the simulation. The decline in concentrations computed in the spin-up simulation in the RM7-17 reach will be used to scale-up initial concentrations between RM7 and RM17 to address this concern. The model simulation with the revised initial conditions will then be run from 1995 through 2059.

The spin-up will also be used to introduce a vertical gradient in the contaminant initial conditions for the top 6 inches of the sediment, where previously, vertically uniform concentrations were assigned based on data that were primarily from 0-6 inch samples. The vertical gradient in contaminant concentrations in the top 6 inches at the end of the spin-up simulation will be scaled to have the same 6-inch average concentration as the vertically uniform initial condition, and used for the beginning of the 1995-2059 simulation. Initial conditions for contaminants in the sediment of Newark Bay will also be revised based on carbon-normalized concentrations.

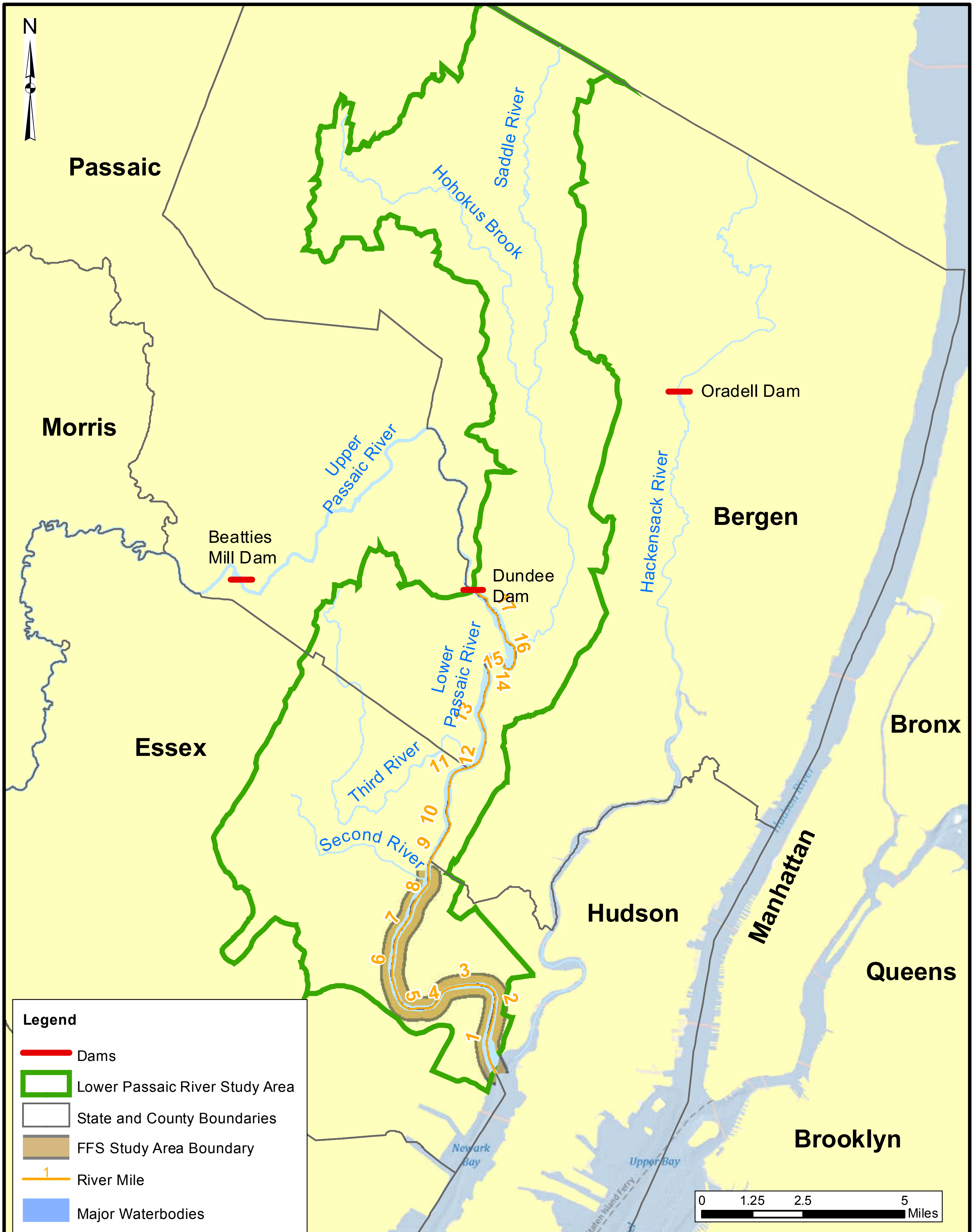
Additional sensitivity analyses will be performed, including:

- Effect of wind-driven resuspension
- Shallower depth of particle mixing in sediment bed of the contaminant model
- Upstream solids loading, and initial conditions, propagated through contaminant model
- Contaminant boundary conditions at the ends of the Kills

- Effect of a 100-year flow on contaminant results for MNR and active remediation projections

An uncertainty analysis will be performed and additional detail and discussion will be added to the final modeling reports in response to recommendations of the reviewers.

The thoughtful and detailed comments of reviewers are appreciated. It is expected that the recommendations of the reviewers, which have contributed to the additional analyses listed above, will result in a stronger model and more valuable tool to contribute to the decision-making process for the FFS.



FFS Study Area Location Map

Lower Eight Miles of the Lower Passaic River

Figure 1-1

2013

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Attachment A

Peer Reviewers' Comments

Peer Review of EPA Linked Sediment Transport – Organic Carbon – Contaminant Fate and Transport Model for the Lower Passaic River FFS

This document represents an independent review of the Lower Passaic River model documented in the following two documents:

- HDR-HydroQual. 2013. Appendix BII - Lower Passaic River Sediment Transport Model. Report prepared for USEPA-Region II, New York.
- HDR-HydroQual. 2013. Appendix BIII - Lower Passaic River Contaminant Fate and Transport Model. Report prepared for USEPA-Region II, New York.

The comments below are based on these reports only; no access was given to the model code itself or its raw input and output files.

The charge to the peer review panel was presented in a document entitled "Lower Passaic River, Lower Eight-Mile Focused Feasibility Study Sediment Transport, Organic Carbon and Contaminant Fate and Transport Model Charge to Peer Reviewers" prepared by The Louis Berger Group, Inc. on behalf of EPA-Region II. In this charge the peer review panel was asked to provide input on the following questions, with full explanations supporting their conclusions:

1. Are the physical, biological and chemical processes represented in the model adequate for describing sediment transport, organic carbon and contaminant fate and transport for the LPR, with particular focus on the FFS Study Area?
2. Have the appropriate data sets been properly and adequately used to set up the model input parameters and define forcing functions and initial conditions for the sediment transport, organic carbon and contaminant fate and transport models?
3. Does the model adequately represent the spatial and temporal distributions of the COCs in the water column and sediment bed for USEPA to use it as a tool to compare the relative effects that implementing each remedial alternative will have on FFS Study Area surface sediment quality?
4. Does the model adequately account for the contributions of COC sources that may recontaminate FFS Study Area sediments during and after implementation of each remedial alternative?
5. Does the model adequately account for the effect of extreme storm events contributing to the resuspension and redistribution of contaminated sediments for USEPA to use it as one tool to compare the effects that implementing each of the four remedial alternatives will have on FFS Study Area sediment COC concentrations?

1. **Are the physical, biological and chemical processes represented in the model adequate for describing sediment transport, organic carbon and contaminant fate and transport for the LPR, with particular focus on the FFS Study Area?**

I view this question as asking about the theoretical framework of the model and the formulation of processes within that framework. I think that the model framework, which consists of a linked hydrodynamic-sediment transport-organic carbon sorbent-contaminant fate and transport model is about as close to a complex state-of-the-science model as we will find for the kind of management questions being asked for the FFS. Below I will discuss some issues with the parameterization/calibration and application of this model framework that could be improved. 1

There are four processes in the model formulation that raise some concern in my mind with respect to the model's ability to adequately describe sediment and contaminant fate and transport. They are:

- The model domain includes the Hackensack River (HR) and Newark Bay (NB) along with the Lower Passaic River below Dundee Dam, with the Kills serving as the downstream tidal boundaries. With this model domain and the tidal movements within the system, NB has a very important exchange with the Lower Passaic River and potential for recontamination during the remediation scenarios. The authors mention in the BII report that they did not include wind-driven or ship traffic-driven resuspension in NB in the model. I would suspect that these are the most likely drivers of resuspension in the Bay and resulting contaminant concentration at the boundary between the Bay and RM 0 of the Lower Passaic River. Therefore, these processes need to either be included in the model formulation or an analysis needs to be presented that convinces the user that they are indeed not important. 2
- The model uses a standard 3 phase local equilibrium model to describe partitioning of hydrophobic contaminants to particulate and dissolved organic matter. In general, this is a reasonable way to formulate these models, but with the emphasis on the observation that there is a great deal of tidal-driven resuspension and longitudinal transport of those sediments by the tides, it is possible that contaminants in the K_{POC} range of ~5-7 could be losing a significant mass of chemical from the repeated resuspension events to desorption and downstream transport and/or, depending on their Henry's Law Constant, volatilization. Several studies have shown that desorption rates for these hydrophobic chemicals are much slower than absorption rates, slow enough that equilibrium is not likely to be attained before the sediment resettle into the sediments. It seems that there is a need to investigate the impact of the process equilibrium assumption on long-term transport of COCs out of the system and the resulting effect on the rate of decline of surface sediment contaminant concentrations during MNR. With respect to partitioning of contaminants to POC, the OC model simulates 10 different POC forms with potentially different f_{oc}'s; yet, I think the model uses the same K_{POC} for all of these POC state variables. I would expect that there are potentially very different characteristics of OC among these forms of POC, so I wonder if they may have different effective K_{POC}'s. A related question is "how does the contaminant model handle partitioning to inorganic solids (both cohesive and non-cohesive) from the ECOMSEDZLJS model?" 3
- Cohesive sediment settling is modeled by assuming (based on site-specific observations) that the single cohesive class used in the LPR version of ECOM-SEDZLJS actually consists of a combination of a very slowly settling background population of particles and a relatively 4

rapidly settling population of bed aggregates or flocs that are resuspended and deposited on a tidal basis. It was not clear to me how the split between these two fractions was derived from the SEDFLUME results and consolidation experiments to parameterize resuspension versus tidal-driven bedload. Another question relative to this aspect of the model is whether the background discrete particles have a different f_{oc} than the bed aggregates; this will impact the contaminant transport during the resuspension process. As discussed below, I think that this decision process will have a major impact on the contaminant fate and transport in the system, and therefore should be more carefully documented. 4 Continued

- Transfer of dispersion output from ECOMSEDZLJS to ST-STEM/RCATOX in terms of change in horizontal segment resolution. Even though the contaminant model grid is a superset of the sediment transport grid, there is a change in effective mixing length between model segments that will require an adjustment of the bulk dispersion rates. Table 2-4 of BIII report and the surrounding text does not indicate that this translation was performed. Was it? Please discuss at this point in the report. 5

I would like to make one more point about the model framework that may be in opposition to some other reviewers. I think the organic carbon model has value in this over framework, because the hydrophobic partitioning of the PTS is represented on the basis of organic carbon in the system (see section 2.2.1.1 of BIII) and a sediment transport model with specified OC fractions would, in my opinion, not necessarily produce the correct level of distribution between particulate, freely dissolved, and bound dissolved chemical. Furthermore, the OC model also provides valuable information about redox conditions and sulfate reduction in sediments that are important for metal partitioning to acid-volatile sulfide (important for determining bioavailable fraction) and mercury methylation in sediments. 6

2. **Have the appropriate data sets been properly and adequately used to set up the model input parameters and define forcing functions and initial conditions for the sediment transport, organic carbon and contaminant fate and transport models?**

I think that the modelers have made good and appropriate use of all available data in the development of model inputs. 7 I do think that upstream loads are highly uncertain and should have been further investigated in terms of their importance to the systems long-term response to remediation alternatives. One thing that the authors can easily do to convince the reader that their regressions are working reasonably well (or not) is to put the actual measured concentrations and resulting measured loads for all available data points on figures 3-2 through 3-6, which should the time series of computed upstream loads. The same should be done for these loads of contaminants – time series plots of computed daily loads with data points on the plots. Also, it would be useful to conduct an informed sensitivity analysis regarding upstream loads to contaminant response, not just sediments. 8

Also, I may have missed it, but I am not sure how the TSS concentration of upstream load was fractioned into size classes; and was the distribution of size class flow-dependent? Many other systems have shown shifts in particle size distribution with flow in river systems. 9

3. Does the model adequately represent the spatial and temporal distributions of the COCs in the water column and sediment bed for USEPA to use it as a tool to compare the relative effects that implementing each remedial alternative will have on FFS Study Area surface sediment quality?

To me this question refers to the model calibration/corroboration and diagnostic/sensitivity analysis to convince the users that it can support the remediation decision process in the FFS. The discussion here raises issues regarding achievement of this objective.

First, I want to make a general comment about the calibration/corroboration process used in this study. In this site-specific model application, it seems as though the hydrodynamic – sediment transport model was calibrated for sediment using TSS and bed sediment properties and then that calibration was unchanged and not revisited during the OC-contaminant calibration process. The transport and characteristics of solids in a system like this has a <u>major</u> effect on the transport and fate of hydrophobic contaminants. And since, as I mention below, the sediment transport calibration is not very good, especially during tidal events, it seems that there should have been an iteration of the sediment transport calibration, effectively using the more hydrophobic contaminants as a solids "tracer". I would ask the authors to justify why this was not done. 10

In support of the above recommendation, the contaminant model was calibrated only by adjustment of mixing rate in the upper active sediments by reducing it by approximately a factor of 10 relative to what was used in the CARP model (see figures 4-4 and 4-5). This was done in order to keep contaminants in sediments from declining at too fast a rate in the model relative to data. At the same time, it seems that the quality of the sediment calibration is relatively poor, especially the significant over-prediction of peak TSS during the high velocity, high bottom shear points in the tidal cycle (evidenced in figures 4-4, 4-5, 4-8 through 4-14). The model also over-predicts the suspended sediment concentration in the vicinity of the estuary turbidity maximum (ETM) in the river (see figures 4-17 through 4-20). This over-prediction of TSS in the water column is the result of a combination of over-prediction of resuspension rate (potentially based on misinterpretation of SEDFLUME results) or under-prediction of resettling rate for resuspended material or under-prediction of the amount of erosion from the SEDFLUME results that is transported as bedload. The approach of using several intact cores submitted to the standard SEDFLUME analysis in order to characterize the depth-dependent relationship between bottom shear stress and surface sediment erosion is a theoretically reasonable way to formulate this process within the model. However, it seems like the relative distribution of resuspension versus bedload in response to tidal velocity induced shear stresses seems to have failed in forecasting that distribution. This issue does not seem to affect the long-term sediment bathymetry simulation and comparison with data, which seems to be pretty decent. However, the rapid and extensive short-term exchange of surface sediments with the water column can have a very significant effect on contaminant fate and transport both in the water column and surface sediments. It seems to me that the modelers should have revisited this sediment transport issue when calibrating the contaminant model. 11

Another point about iteration during the normal steps in the modeling process is regarding sensitivity analysis. Report BIII describes the results of three model sensitivity runs for contaminants. In addition to the three runs that were done, the authors indicated on page 4-2 that they tried increasing the K_{POC} in attempting to calibrate the contaminant rate of decline in the surface sediments. The results of this work should have been at least reported in the sensitivity analysis section. It is also relevant to the issue on desorption rates mentioned above. But the 12 important point here is that Report BII describes the results of six model sensitivity parameters/inputs on sediment transport, and found that at least four of them had significant impacts on model performance:

- Grain size distribution is critical to modeled sediment transport, hence the question I had about how this was determined in the upstream load;
- Upstream boundary loads was next in terms of model response to its adjustment;
- Cohesive settling velocity seems to have a big impact on net erosion; and
- Downstream boundary conditions have a big impact on water column fluxes up to RM 5.2, hence my concern about the importance of wind-driven resuspension in NB.

My criticism is that these sensitive model parameters/inputs were not carried to the contaminant model for evaluation of their impact on contaminant fate and transport. In my opinion, this is a critical missing analysis in the model application relative to its accuracy of system response to remediation alternatives.

12 Continued

Finally, I think that a very important missing model diagnostic analysis that is invaluable in interpreting and judging the model's credibility for projecting the system's response to remedial alternatives is a space and time-specific mass balance analysis. The mass balance diagnostic helps the reader/user identify the relative importance of sources and sinks of contaminants to certain areas of the remediated zone of the river during various time periods of the remediation scenarios. I realize that the last section of the BIII report (section 6-4) provides cumulative contaminant fluxes across several transects of the lower river over time, and that analysis proved very instructive relative to the system's response. But I think a much more instructive and illustrative analysis would have been to develop a full model-computed mass balance diagram (all inputs and outputs and change in control volume mass) for the river segments between those transects at several points in time (or over several specified time intervals) of the remediation and post-remediation simulations. If indeed, the remediation trends shown in figures 6-3 and 6-11 (for examples) are correct, one could understand why the sediments are not being recontaminated at all following remediation. I urge the modelers to conduct these diagnostics to convince themselves of the accuracy of the remediation scenario forecasts.

13

4. Does the model adequately account for the contributions of COC sources that may recontaminate FFS Study Area sediments during and after implementation of each remedial alternative?

I am not convinced, given the great mobility of surface sediments in this system and propensity for downstream transport of sediments in the RM 17-8 reach, that figures 6-3, 6-11, 6-19 should virtually no recontamination over the roughly 45 years following initiation of remedial actions. It may be that recontamination from both upstream and downstream boundaries and from ongoing external loads (CSO's, atmosphere, etc.) is masked the fact that these plots represent a six or eight mile average over the top 15 cm of sediment, but given that two of the remedies get the sediments to virtually zero I find this implausible. The mass balance diagnostic analysis (discussed in question 3) applied to smaller segments of the river and only the top couple centimeters of sediments (which are responsible for water column exposure) should be looked at to better evaluate recontamination.

14

The evaluation of recontamination should also be conducted as part of the sensitivity analysis (including both sediment model and contaminant model parameter/input variations), to project the level of recontamination change if certain inputs are over- or under-estimated. Also, the potential for recontamination from northern NB should be investigated. As shown in figures 4-1 through 4-3, the sediment concentrations in northern NB are equivalent to the lower 8 miles of the Lower Passaic River at the start of the remediation runs. It seems to me that there would be a significant potential for recontamination via tidal pumping from wind- and ship-driven resuspension of these sediments. Also, the cumulative net mass transport increase after remediation (see fig 6-30 through 6-34 for various transects) shows a relatively monotonic increase (except for the abrupt jump in 2037) presumably from upstream (both upstream load and movement of sediments from RM 17-8). It is hard to understand that virtually all of this transport is just passing through the lower 8 miles and out to NB with some of it depositing in the remediated zone. 15

5. **Does the model adequately account for the effect of extreme storm events contributing to the resuspension and redistribution of contaminated sediments for USEPA to use it as one tool to compare the effects that implementing each of the four remedial alternatives will have on FFS Study Area sediment COC concentrations?**

I think the answer to this question is that extreme events do have an impact -- see the impact of the April, 2007 event (repeated in 2022, 2037) (figures 6-3, 6-11, 6-19 and 6-30 through 6-34), which I think is only about half the flow of a 100-year return period event. Therefore, I think that the modelers should have inserted a 100-year event into the remediation scenario input file to evaluate the duration and spatial extent of such an event for the different remediation alternatives. These very high flow events will generate significantly higher load of solids from upstream, likely cleaner solids than what is in surface sediments under baseline or early MNR conditions (but maybe not cleaner relative to a sediment sand cap). Of course, the events will also generate high resuspension rates. So, higher flows will likely lead to an overall higher rate of exchange of surface sediments (as we move through the hydrograph) with the overlying water, and depending on the relative upstream chemical concentration on solids versus the surface sediment initial conditions at the beginning of the event, there will be a potentially significant change in surface sediment chemical exposure concentrations. It is important for the full model to be used to generate the net system response to extreme events. 16

Peer Review of Lower Passaic Lower Passaic River, Lower Eight---Mile Focused Feasibility Study – Sediment Transport, Organic Carbon and Contaminant Fate and Transport Model

First, I’d like to offer some general comments and conclusions. The scope and detail of the multiple modeling efforts on the New York – New Jersey harbor complex are quite impressive. The FFS study here is just one part of this large, ongoing effort. I’m confident that the modelers have learned a lot about the system dynamics and can offer the decision-makers useful advice and valuable perspectives about the possible uncertainties involved in the management scenarios. 17

Along with the other reviewers, I have tried to not only dig in and answer some detailed modeling questions, but also to step back and ask whether the predictions make sense to us. Can the predictions be explained, and do they follow what we have seen in other water bodies? There is more here than any one reviewer can assimilate fully, but the effort to understand the model and the water body from a holistic perspective is important.

Can the model be trusted enough to compare the *relative* effects that implementing each remedial alternative will have on FFS Study Area? Despite some reservations, I believe the model can indeed distinguish between the MNR alternative and the two more extensive remedial alternatives – Deep Dredging and Full Cap. 18

My judgment is that over the next three decades, the LPR would see somewhat lower contamination levels than predicted by the MNR, and at least a bit higher levels than predicted by the Deep Dredging and Full Cap. The MNR prediction might underestimate recovery because infrequent large events not simulated could more efficiently flush out existing contaminants and bring in a significant load of (relatively) clean solids. In addition, some slow chemical and biochemical loss processes were not included, and could work over long periods to attenuate concentrations, at least marginally. 19 There is some indication that the active mixing layer is somewhat less than 10 cm. If so, then contaminants in the upper layer would escape more rapidly than simulated. It is not clear whether the 15 cm average concentration would recover more quickly, however. Finally, the calibration data seem to show a recent decline in many locations not captured by the model. The model calibration included a midpoint upstream “reset” to counter low initial conditions. If the initial conditions were set properly and the model were recalibrated to capture the 15 year calibration trends, there is a chance that it would show more rapid MNR recovery. 20

On the other hand, the treatment alternatives might underestimate recontamination for a couple of reasons. First, large events not simulated might bring in a significant load of contaminated solids (relative to the clean caps). Second, the model appears not to include partitioning to noncohesive solids, which constitute the clean sand capping. In reality, some contaminant levels are expected to diffuse within particle 21 pores, and some carbon is expected to build up on noncohesive surfaces, capturing more contaminant. The result is a low but not insignificant partition coefficient for the caps. 21 Continued

Nevertheless, I expect that LPR would experience significantly lower contamination levels following Deep Dredging and Full Cap than it would under MNR. It's a little less clear how much difference to expect in the long run between MNR and Focussed Capping, or between Focussed Capping and the more extensive alternatives. My judgment is that the water and sediment quality for Focussed Capping would indeed be better than MNR and worse than either Deep Dredging and Full Cap. Given the large spatial and temporal variability, however, the improvements might be difficult to measure. Since only one alternative will be chosen, of course, we can never know exactly how the others might have played out. 22

The managers should understand the large degree of uncertainty in model predictions in a complicated system such as this. I wish more diagnostic simulations had been run for better understanding. Because of the size and complexity of the models used here, it was difficult to impossible to run enough sensitivity alternatives to fully estimate the level of uncertainty. Some modelers prefer simpler models that can be parameterized and run thousands of times in an effort to establish uncertainty bounds. These simpler models, however, can be biased by limited data and may not adequately capture key processes. They are subject to more peer review criticism from scientific and modeling experts. I cannot fault the choice of the complex models for this study. Indeed, many of our peer review comments would lead to more complexity in network and process detail. 23

In addressing the charge questions below, I briefly summarize my understanding of what is presented in the FFS and background documentation, then provide comments or questions. I mark issues or questions needing response with *** asterisks ***.

1. Are the physical, biological and chemical processes represented in the model adequate for describing sediment transport, organic carbon and contaminant fate and transport for the LPR, with particular focus on the FFS Study Area?

Overall, I believe that the processes incorporated into the sediment, organic carbon, and toxicant models are appropriate and justified. Specific questions and comments follow for each module.

Under physical processes, I will include the grid resolution, sediment feedback to hydrogeometry, sediment class representation. I will assume that hydrodynamics is simulated adequately.

24

1.1 Sediment transport –

The physical processes in model seem adequate.

The **model grid** includes 10 water column layers and 10 active bed layers, with width varying from 4 to 3 to 2 cells going upstream. A more refined grid (4 times finer resolution) was tested, and shown to give only minor improvements at a cost of 8 times longer computations.

Bed **elevations** are modified once a year and fed back to the hydrodynamic model. This seems adequate for comparing alternatives.

The **sediment fractions** are divided into one cohesive class and 3 noncohesive classes. The cohesive class is functionally divided into two subgroups using empirical functions.

The **sediment bed** is divided into parent bed and deposited layers. The parent bed retains measured properties, such as bulk density. For deposited layers, bulk density approaches equilibrium value at first-order consolidation rate.

The sediment transport processes include settling, deposition, resuspension, bed load, and consolidation.

25

Bottom shear stress is divided into form drag and grain stress. The total roughness in the hydro model is constant, but the sediment model calculates the bedform roughness using a van Rijn formulation (function of d50, tau_s, and tau_ce). This predicts bedform (mini ripples, mega ripples, dunes), which reduce the grain stress by up to 3 times. This approach seems justified.

26

Erosion of cohesive bed uses a nonlinear table of erosion velocities versus applied shear stress values (grain stress, I assume). Erosion is linear between points in the table. The table function is derived from experiments with intact cores using

27

SEDFLUME. It seems to me that the modeling approach for this process is defensible given good experimental data. 27 Continued

Bed consolidation follows Sanford 2008. The parent bed retains measured properties, such as bulk density (sand = 1.92). For deposited layers, bulk density approaches equilibrium value at first-order consolidation rate. This seems reasonable, given defensible experimental data. 28

Bedload equations are applied to noncohesive particles. The fraction of eroded particles transported as bedload is a function of grain diameter, density, and fall velocity. The rest is added to the lower water column to be transported in suspension. All cohesives are transported in suspension. The processes here seem reasonable to me. 29

Noncohesive **settling** is calculated by particle size class. For the single cohesive class, an empirical function of TSS is used for settling. The slow background settling rate is 0.2 mm/sec (17 m/day). Aggregates settle as a function of TSS: min (3*TSS/260, 3) mm/sec. This gives a minimum settling rate of 260 m/day. This function seems like an acceptable compromise to me, given reasonable empirical data. I defer my judgment on this, however, to my fellow reviewer, Dr. Lick, who has more expertise in this area. 30

Deposition probability follows Krone for cohesives and Gessler for noncohesives. These seem fully adequate given reasonable input parameters. For cohesives, tau_cd = 0.5 dy/cm2. 31

1.2 Organic carbon –

The organic chemical model ST-SWEM includes a water column module and a sediment diagenesis module. 32

The physical processes in model seem adequate:

The **model grid** includes 10 water column layers and 3 bed layers, with width varying from 4 to 3 to 2 cells going upstream (preserved from hydro and sediment models). In the longitudinal direction, the cells were aggregated 2 to 1 or 3 to 1 to reduce computational burdens. The length aggregation seems reasonable to me. 33

The **sediment bed** is divided into a thin fluff layer (for tidal deposition and resuspension), an active layer of about 10 cm (biological mixing), and an archive layer. The active layer is subdivided into aerobic and anaerobic zones for reaction rates. Specifying a biologically active layer depth of 10 cm throughout the model domain (from mudflats with fine silt to channels of coarse sand) is questionable, but perhaps the available data do not allow better definition. 34

*** The toxicant model active layer is subdivided into 1-cm cells, and the archival layer is set to 97 cm, and subdivided into 1-cm cells. I assume the same is done with ST-SWEM, but it is not clear from the documentation. The toxicant model also has a deep bed archival layer of 0.61 cm. Since this is not mentioned, I assume ST-SWEM does not include that layer. *** 35

The **carbon fractions in the sediment** diagenesis module are divided into three "G-class" state variables representing labile POC (G1), refractory POC (G2), and inert POC (G3). Variables are referred to as SG1C, SG2C, and SG3C. DOC is not represented in the sediment bed. For theoretical completeness, it seems to me that sediment DOC should be a product of G1 breakdown and exchange with surface water. Apparently sediment layer DOC concentrations were provided to the toxicant model using empirical data. *** If this is not the case, and DOC is not specified for benthic layers in the toxicant model, then sediment-water transport for some contaminants would be underestimated, particularly loss from deeper layers that are infrequently eroded. *** 36

The **carbon fractions in the water column** are divided into ten state variables: refractory POC and DOC (RPOC, RDOC), labile POC and DOC (LPOC, LDOC), reactive DOC (ReDOC from CSO loadings), algal exudate DOC (ExDOC), three resuspended sediment POC classes (SG1C, SG2C, SG3C mapped from the diagenesis module), and inert POC (IPOC mapped from cohesive TSS in the sediment model).

Looking at the detailed water column carbon reactions described in Table 2-2, it seems clear that resuspension SG1C is a source term for LPOC, as is a portion of algal grazing. (*** why is SG1C a separate state variable in the water column? ***). Similarly, resuspension of SG2C is a source term for RPOC, along with a portion of algal grazing (*** why is SG2C a separate state variable in the water column? ***). 37
Likewise, resuspension of SG3C is a source term for IPOC (*** why is SG3C a separate state variable in the water column? ***). The reaction coefficient table (2-2) shows a fraction of algal grazing going to IPOC (0.025), but the equation for IPOC does not include a corresponding term (*** I assume this was just an omission in the documentation, since summary Table 2-3 includes the algal source to IPOC ***). Labile POC decays into labile DOC, while refractory POC decays into refractory DOC. Labile and refractory DOC can be aerobically oxidized into CO2. Labile DOC can also be consumed by anaerobic denitrification and lost (in the document, the theta term needs the exponent "T-20"). The reactive and algal exudate DOC can be aerobically oxidized to CO2. The water column carbon reactions are reasonable and well justified.

Carbon production comes from the previous SWEM eutrophication model, with its 24 state variables, including two phytoplankton groups (winter diatoms, summer flagellates). This is a reasonable representation. This module provides more carbon pools than is necessary for contaminant fate modeling, but its use seems reasonable and well-justified to me. 38

Model linkage procedures:

Primary production and resulting POC is handled in ST-SWEM, but not in the sediment transport module ECOMSEDZLJS, which feeds it. *** This is OK unless algae and detritus are a significant fraction of cohesive solids in this system. *** ST-SWEM does not simulate water column noncohesive solids, which is acceptable. ST-SWEM includes noncohesive solids in the bed as a single aggregated class. 39

Initial ST-SWEM bed composition is transferred from the ECOMSEDZLJS initial mass of cohesive and non-cohesive solids in the top 10 cm (active layer) and the remainder of the bed (archive layer). ST-SWEM then simulates the evolution of the bed using settling, resuspension, and burial/erosion velocities derived from ECOMSEDZLJS. *** This approach is practical and justified as long as primary production, which is not represented in ECOMSEDZLJS, is not a significant fraction of the cohesive solids. *** 40

ST-SWEM cohesive settling velocities are calculated from flux-weighted ECOMSEDZLJS cohesive settling velocities. These are used in ST-SWEM for RPOC, LPOC, and IPOC. This procedure is justified. 41

ST-SWEM resuspension velocities are calculated from ECOMSEDZLJS spatially aggregated fluxes and concentrations (i.e., flux/conc). These velocities (not mass fluxes) are applied to ST-SWEM sediment organic and inorganic variables. This approach is reasonable and justified. 42

ST-SWEM burial/erosion exchanges between the active and archive layers are calculated from area-weighted ECOMSEDZLJS bulk densities and deposition and resuspension fluxes. This is done by calculating the change in the active layer thickness as flux/density. This approach is reasonable and justified. 43

The original SWEM used constant sediment burial rates (about 2.5 cm/yr), then varied net deposition rates calibrated to observed SOD and POM. The linkage here of ST-SWEM to ECOMSEDZLJS is a nice improvement. 44

1.3 Contaminant fate and transport –

RCATOX is used to simulate contaminants. Information is passed to RCATOX from the other modules using large transfer files. Hydrodynamic information is passed along from ECOMSEDZLJS at 1-hour intervals. *** This seems justified as long as the hourly information is interpolated down to the RCATOX time step. *** Sediment transfer and carbon information is passed along from ST-SWEM at 15-minute intervals, which seems reasonable. 45

The **partitioning processes** in RCATOX are conventional and well-justified. Three phases are simulated – dissolved, DOC-complexed, and POC-complexed. *** As 46 mentioned above, it is not clear how DOC in the sediment bed is determined. *** It is not clear how partition coefficients to noncohesive solids in the bed are handled, if at all. ***Are noncohesive partition coefficients assumed to be 0? A low amount of sorption to fine sands could raise the levels of recontamination of sand caps used in the treatment alternatives. ***

46 Continued

Volatilization exchange with the atmosphere is described in the CARP report. This exchange process is mathematically split between loss flux and forward diffusive loading flux. This approach is ok if the calculation of gaseous loading velocity is consistent with the loss velocity. The CARP report gives the equations used for the volatilization loss velocity, but does not document the equations used to externally calculate the corresponding forward gaseous loading. *** I assume consistency, but the modelers should confirm. ***

47

Chemical and biochemical degradation processes are not used for this study. Given the nature of the chemicals, this seems reasonable to me. For the hydrophobic organic compounds like dioxin and PCBs, this is reasonable.

48

Mercury kinetics are not described in the FFS documents, but the background CARP document provides a brief explanation of the mercury components and transformation processes included in the model. Total mercury (HgT) was divided into divalent (HgII) and methyl mercury (MeHg) components. HgII and MeHg were simulated explicitly, but elemental mercury (Hg0) was specified as a fixed fraction (10%) of dissolved HgT based on professional judgment (but no local data). Transformation and transfer processes in CARP include methylation and demethylation in the water column and sediment bed, and volatilization. Oxidation and reduction, which link elemental mercury to the predominant forms of HgII and MeHg, are not simulated.

In my opinion, excluding redox kinetics and treating Hg0 simplistically is an unnecessary weakness in a model with so much other process detail. Treating Hg0 explicitly with oxidation and reduction has been part of accepted mercury modeling practice since the mid to late 1990s, and studies of redox kinetics have improved the state of the art in the subsequent decade. The actual loss flux of Hg0 from the water column to the atmosphere may be controlled by the oxidation rate supplying Hg0 rather than the faster volatilization rate depleting Hg0. Specifying that Hg0 is 10% of the dissolved HgT essentially parameterizes the net oxidation/reduction rate at 10% of the volatilization loss rate and reduces mercury evasion flux to 10% of the potential loss. In many systems, mercury is only lost through advective export, sediment burial, and atmospheric evasion. In the LPR, with its sediments in rough equilibrium, burial loss is probably negligible and some fraction of the mercury advected out to Newark Bay is returned in bottom waters. It is not clear whether the slow evasion loss in the LPR can be a significant fraction of net advection loss over a long period of time. Evasion loss would affect the MNR scenario (with the higher HgT concentrations) more than the treatment scenarios. Sensitivity runs increasing the Hg0 fraction could address this uncertainty, although the relatively large

49

uncertainties in future atmospheric mercury loading fluxes probably overwhelm the uncertainties in evasion loss fluxes. Given that mercury is only one pollutant of concern in the LPR, this probably would not significantly affect the final choice of treatment alternatives. 49 Continued

The **sediment bed** is divided into an active layer of about 10 cm, an archive layer of 97 cm, and a deep archive layer of 61 cm. The active and archive layers are subdivided into 1-cm cells. The surface cell varies between 0.5 and 2 cm. The other active and archive cells are maintained at 1 cm thickness. With erosion, the cell contaminant masses are moved upward, and the thickness of the deep bed is reduced, maintaining the total structure of 107 cells. With deposition, the cell contaminant masses are moved downward, and the thickness of the deep bed is increased, again maintaining the total structure of 107 cells. This approach is very reasonable, and is quite similar to the approach in EPA's version of WASP. 50

*** It is not clear how or whether the solids composition of the cells within the active and archive layers change with erosion and deposition. The solids composition would come from ST-SWEM, and it is not clear whether that model is divided into 1-cm cells with variable solids. *** 51

2. Have the appropriate data sets been properly and adequately used to set up the model input parameters and define forcing functions and initial conditions for the sediment transport, organic carbon and contaminant fate and transport models?

Overall, I believe that model setup and calibration used appropriate data sets to adequately define parameters, forcing functions, and initial conditions. An exception is the upstream contaminant initial conditions, which were apparently set too low, and had to be reset during calibration. This is discussed more under question 3 below. Specific comments follow for each module. 52

2.1 Sediment transport –

For **freshwater boundary concentrations**, the model uses a two-phase log-log empirical correlation of TSS to flow. This seems good enough. 53

For the **tidal boundary concentrations**, two empirical functions were derived. For the period before dredging, TSS is fit to a nonlinear function of depth and velocity. After dredging, TSS is fit to a nonlinear function of velocity and tidal range, divided into accelerating and decelerating phases of the tide. Both of these functions seem well enough justified. 54

For **initial bed sediment conditions**, seven morphological features in the LPR were identified and mapped. In-situ data were used to define 4 solids size classes (silt, fine sand, coarse sand, and gravel). Mapping characterized average fractions for each class within contiguous morphological regions. This seems like a reasonable modeling approximation. 55

Important parameters, such as **critical shear stress for erosion**, were derived from experimental apparatus, including Sedflume and Gust Microcosm. Consolidated sediment tests were run as well as field cores. These tests were used to parameterize both parent bed and deposited layers.

The data indicate high variability in replicates, sometimes over an order of magnitude in measured erosion. There were some inconsistencies in measured properties between consolidated sediment tests and field cores potentially indicating that deposited material erodes more slowly than parent bed. In these cases, data were chosen so that the modeled depositional layers are consistent with the parent bed. This seems reasonable. 56

The data analysis procedures used to capture appropriate central tendencies and ranges seem thorough, as good as possible under circumstances of high variability. It is conceded that due to spatial averaging over cells and over size classes, the model will never capture all the variability of the real system. The parameterization inevitably introduces a good bit of uncertainty which must be taken into account in the modeling analysis. 56 Continued

Model calibration to field data can be used to refine parameters and forcing functions. Here, calibration runs of 15 years are tested against water column TSS data and bed elevation data. A comparison of model results with data from the March 16, 2010 high flow event was also conducted. *** It is not clear whether the high flow simulation was a separate short-term simulation or just detailed output from this portion of the full 15-year simulation. If this is a separate short-term simulation, the modelers must make sure the initial conditions were captured properly. *** 57

It is not clear what model parameters, if any, were modified during calibration, or how many calibration runs were made in this phase of the study. The report describes how the model run compares with available data, and reads more like a model validation exercise.

That said, what do the data comparisons reveal about model parameterization and forcing functions? Recognizing that the data are often quite variable and often uncertain, and that the observations are at different spatial and time scales than the model output, it is difficult to draw unambiguous conclusions. It seems to me that the model captures many of the general tendencies of the LPR sediment dynamics. Among these are that the LPR is approaching quasi-equilibrium conditions, with solids accumulation much less than the solids loading over Dundee Dam. There is a net upstream transport of solids during low flow periods, and net downstream transport during high flow periods. The data and model tend to tell a coherent story despite the large degrees of temporal and spatial variability and uncertainty. Model results averaged over large areas in the LPR are more likely to be accurate than model results for hot spots. It is not clear how well the model might represent future conditions with altered bathymetry. All of these points are recognized properly by the modelers. 58

Sensitivity analysis characterizes model response to changes in parameter values, and can be used to shed more light on model parameterization. Six inputs were evaluated for a 1-year period – upstream BC, downstream BC, critical shear stress for cohesive erosion, settling velocity for cohesive solids, erosion rate for cohesive solids, and solids grain sizes. Four outputs were examined – solids flux across 8 transects, gross erosion in 7 reaches, gross deposition in 7 reaches, and net erosion in 7 reaches. Results, for the most part, were consistent with expectations, and indicated reasonable model parameterization. Among the important conclusions are that cohesive erosion rate is not sensitive because total erosion is probably supply limited. The characteristic grain size for the model classes is very sensitive. This sheds light on why the model results capture significantly less variability than exhibited by the real system. 59

2.2 Organic carbon –

The organic carbon production model was slightly modified from the existing CARP model, which was previously calibrated and validated on the overall system. I believe the **model parameterization** from CARP is adequate for this FFS study. 60

The **boundary concentration** functions from CARP were used for POC. The freshwater boundaries use POC as a function of daily flow, while the tidal boundary uses monthly averages. Both seem adequate for purposes of this study. 61

The **wastewater, CSO, and atmospheric loadings** from CARP were used. These seem reasonable for the FFS. 62

The **initial conditions in the sediment** were specified by running the CARP model grid over a number of years to obtain quasi-equilibrium conditions. This seems like a reasonable approach. 63

Model calibration to field data can be used to refine parameters and forcing functions. The carbon model calibration was not detailed in this report. Carbon model verification scatter plots are given for water column POC and DOC (Figures 4-7 and 4-8). These indicate that the model is, on average, in the right range in the water column but fails to capture the variability. In the bed sediment, the model is in the vicinity of the data, but does not capture the average or the variability. Sensitivity of toxicant concentrations to sediment POC indicates little consequence to the OC Model limitations. 64

Sensitivity analysis could have been used to shed more light on the carbon model parameterization, but these were not done for the FFS.

2.3 Contaminant fate and transport –

Contaminant loadings to the LPR were derived from CARP along with additional data. For **freshwater boundaries**, median observed dissolved and particulate concentrations were combined with NPL-calculated POC loadings to obtain total contaminant loadings. For some contaminants, local data were unavailable and so values were estimated from data in the Mohawk and Hackensack rivers. This seems like a reasonable approach. 65

For **tidal boundaries**, contaminant concentrations were set to monthly output from CARP simulations through the period 1996 – 2054.

Wastewater loadings used median monthly concentrations and flows from CARP.

SWO's and CSO's used the median of measured data and hourly flows from CARP.

Atmospheric loadings were estimated from the NJ Atmospheric Deposition Network, and included gas, particle, and precipitation phases. 65 Continued

Initial conditions for sediment contaminants were extrapolated from sampling data. The procedure first averaged data to get representative concentrations within the 7 geomorphic regions. (*** I assume that the median was used ***). Finally, initial concentrations for each grid cell were area-averaged from the representative geomorphic concentrations within that grid. *** This procedure is reasonable and justified if separate geomorphic averages were derived for different reaches in the LPR (it was not clear to me from the documentation how longitudinal spatial variability was considered). The initial contaminant concentrations for the upper LPR were apparently set too low and had to be reset higher in the middle of the calibration run. This is not a valid procedure, and is discussed in the next question. *** 66

The **contaminant parameters and constants** used here were the same as those used in CARP. For most of the contaminants, the parameters include only partitioning coefficients to DOC and POC. These are reasonably well supported in the literature, though subject to a range of uncertainty due to differences among homologs. For simulating mercury, however, many more constants must be specified, including rate constants for methylation and demethylation, oxidation and reduction, and volatilization. Methylation, demethylation and volatilization rates are documented in background materials, and seem reasonable. *** The net effect of redox kinetics is parameterized in the specified Hg0 fraction (i.e., 10% of HgT_diss). This is not well supported, but may not be sensitive. *** 67

The **sediment mixing rates** were modified from CARP. In this study, these rates were calculated by calibration of the carbon model ST-SWEM. This is probably one of the more important model parameters. 68

Model calibration to field data can be used to refine parameters and forcing functions. Here the period October 1995 – September 2010 was used to evaluate the data.

Since benthic concentrations were not measured above RM 7 until 2008, the 2008 data were used to estimate initial concentrations in the upstream reaches of LPR. Benthic concentrations there drifted downward during the calibration runs, and so they were reset to 2008 data before running 2008 through 2010. This is an indication that the upstream IC's should have been set to higher values, as determined by iterative calibration runs. *** Resetting concentrations in the middle of a run is not a valid procedure. This introduces more uncertainty into the final calibration parameters. *** 69

Initial calibration runs had too much initial decline from 1995 – 1998, and so particle mixing parameters were adjusted. A range of values were tested, from 120 70 cm2/yr to 3.15 cm2/yr. A final value of 10 cm2/yr was chosen. *** This calibration procedure is reasonable and justified. But, if particle mixing was adjusted downward from ST-SWEM, then that model should have been rerun with the lower rates. If not, then there is a disconnect between the models. *** 70 Continued

A limited **sensitivity analyses** explored the long-term consequences to the MNR option of three toxicant model inputs – depth of sediment mixing zone, sediment carbon concentrations, and initial concentration gradients in sediment. When sediment mixing depth is increased by a factor of 2, the model response dynamics slowed, as expected, but the final results were similar to the base case. When sediment carbon is increased by a factor of 2, the fraction of bed contaminant sorbed to particles increased only very slightly, as expected. Since water column carbon was not increased, the sensitivity run showed a net flux of contaminant to the water, thus increasing the rate of decline in the bed. It is not clear how significant this calculation is. Finally, specifying more reasonable gradient initial conditions resulted in differing short-term dynamics, but after 5 years the results converged with the base case and showed no long-term significance. 71

3. Does the model adequately represent the spatial and temporal distributions of the COCs in the water column and sediment bed for USEPA to use it as a tool to compare the relative effects that implementing each remedial alternative will have on FFS Study Area surface sediment quality?

3.1 Sediment transport –

The transport and distribution of cohesive solids significantly affects the spatial and temporal distribution of COCs. It is not clear how well the solids behavior matches smaller areas within reaches that might function as hot spots. Sediment behavior is quite patchy and nonlinear, and small areas could control the overall risk calculated for COCs. It seems that the behavior averaged over large reaches is reasonable, however, and could be used to evaluate relative effects of remedial alternatives. 72

3.2 Organic carbon –

The OC model does not capture the spatial and temporal trends in the LPR. Given the relative lack of sensitivity of POCs to the details of the carbon model, the overall representation of organic carbon is good enough to evaluate relative effects of remedial alternatives. 73

3.3 Contaminant fate and transport –

Spatial data in the sediments are very patchy, with hot spots. The model cannot capture this extreme local spatial variability. Still, the model might be able to capture the dynamics of reach-averaged concentrations well enough to judge between remediation alternatives. This is examined below.

The calibration procedure included a significant problem with the upstream IC's – the concentrations for TCDD, PCB, and Hg were set too low, which led to an arbitrary calibration reset in the upstream reach in 2007, the middle of the simulation. As a result, it is difficult to judge the model behavior based on the 15 year calibration plots shown. If the upstream IC's had been calibrated, then the results might have shown better fidelity in capturing the gross spatial and temporal trends in the data. At best we can say that the model is within a factor of 2 or so in its average response for the two large reaches (upstream and downstream). 74

TCDD data shows little definitive time trend for the upstream and downstream LPR reaches, though it is possible that there is a slight decline in the period 2008 – 2010. The model shows a slight increase in that period, though partially masked by the calibration reset in 2007. 75

PCB data seems to show a downward trend over time for both upstream and downstream reaches. The model captures this trend until 2008. After that, the data decline while the model increases in the upstream reach and stays flat in the downstream reach. 76

For total Hg, the model captures the downstream trend of slight decline reasonably well. The downstream data hover around 3000 ug/kg from 1995 through 2006, and then jump down to around 1800 ug/kg from 2008 through 2010. The upstream trends are not captured very well. The model IC is set well below the data, then gradually declines, remaining below the data until the 2007 reset. After that, the model goes up while the data declines from 2008 though 2010. 77

In summary, the model results are generally within a factor of 2 of the observed data midpoints, and so could possibly distinguish between management alternatives that cause future differences of that magnitude. *** Since recent possible trends toward concentration declines are not captured by the model, this leads to some uncertainty about whether the simulated MNR alternative will be biased high. This could be addressed by a recalibration of initial upstream IC's and extension of the calibration through 2012. Sensitivity runs with higher upstream IC's could help resolve some of this uncertainty as well. *** 78

4. Does the model adequately account for the contributions of COC sources that may recontaminate FFS Study Area sediments during and after implementation of each remedial alternative?

The COC sources that may recontaminate FFS sediments during and after remediation include external loadings from tributaries, CSO's, WTPs, and the atmosphere. These are captured reasonably well. I believe there are larger uncertainties in how well the in-place contaminated sediments are captured. These include unremediated upstream sediments and downstream sediments released during dredging operations. 79

Because of the calibration reset during mid-simulation, I'm not sure how well the model captures the in-place upstream COCs. A sensitivity run (2 times IC for upstream reach) could have addressed this source, but it wasn't run. 80

The procedure for simulating releases during dredging is mostly reasonable. One weakness is that the treatment of internal sediment loadings differed from alternative 2 (Deep Dredging) versus alternatives 3 and 4 (Capping with Dredging and Focussed Capping with Dredging). In alternative 2, the solids released during dredging were incorporated back into the sediment transport model. Without the sediment model rerun, the redeposited solids would have had COC concentrations too high by a factor of 2. The internal dredging releases were not rerun in the sediment model for alternatives 3 and 4, and as a result the redeposited solids have COC concentrations that are too high, thus overstating the recontamination at least slightly. It is difficult to judge the resulting bias, though it is noted that the solids release during alternative 3 and 4 are 43% and 9% of the alternative 2 releases. *** This bias should be kept in mind when evaluating the differences between alternatives. *** 81

5. Does the model adequately account for the effect of extreme storm events contributing to the resuspension and redistribution of contaminated sediments for USEPA to use it as one tool to compare the effects that implementing each of the four remedial alternatives will have on FFS Study Area sediment COC concentrations?

The model accounted for two high flow events – April 2008 and March 2010. The March 2010 event is a 1 in 25-year storm event, which the model seemed to handle well enough. The model was not tested against any larger events, and the simulation period evaluating the alternatives repeated the 15 year hydrological record 1995 – 2010. So the modeling results can be said to cover "moderately extreme" events, but they do not cover more extreme events with a recurrence of 1 in 50 or 1 in 100 years. This is somewhat surprising, as a process-based model can be used (with great caution) to extrapolate beyond the observed datasets. Sediment transport is 82 highly nonlinear, and the more extreme events could have major effects on the remediation alternatives. 82 Continued

The extra materials provided show that the highest daily flow in the 112 year record was about 30,000 cfs, compared with the highest flow in the simulated period, about 15,000 cfs. Given the exponential increase of erosion with flow, assuming the exponent is between 1.2 and 3, a doubling of high flow would lead to erosion rates from 2 to 8 times higher (rough bounding calculations provided below). 83

HQI modelers provide a reasonable response that "the mass of sediment eroded or depth of erosion will not increase in proportion to the increase in erosion rate … Sedflume data show one to two orders of magnitude reduction in erosion rate within the first 5 to 10 cm in the bed and an increase in the critical shear stress with depth. Bed coarsening in non-cohesive areas and consolidation in cohesive areas will slow down erosion as the upper portion of the bed is eroded. At higher shear stresses more mass and deeper erosion will likely occur, but to a lesser extent than one could conclude from the ratio of erosion rates." 84

So essentially, under the highest flow events, the noncohesive areas will fully scour out sediments and contaminants down to an effective floor that may not be too much deeper than what is scoured under moderately high flow events. This conclusion seems reasonable to me.

It remains to be determined how deep is the extra scour, and what is the fate of the sediments and contaminants during the highest flow events. Do they redeposit in the LPR, or Newark Bay, or are they carried farther down the estuary? Empirical data from Irene along with qualitative arguments may help answer this question. Benthic surveys were provided showing change in bottom elevation between 2010 and 2011, thus accounting for the effects of Irene. Visually, it does seem that the higher flow Irene scoured and deposited more sediment than the March 2010 event, but it is not clear how much. Further, it is not clear what the net affect was on contaminant levels.

It is possible that an extreme event could more effectively flush contaminants out of the LPR, improving the MNR scenario somewhat. In addition, large amounts of relatively clean solids could be brought in and deposited to the LPR, improving the MNR scenario. Alternatively, it is possible that large amounts of somewhat contaminated sediment from upstream or outside the LPR could be deposited on top of clean caps or sand covers in the downstream LPR.

*** If the question of extreme events is important enough, I recommend that the calibration/verification be extended to simulate the Irene event. This would be especially useful if further contaminant surveys are available to test against the model. Then this event could be included near the end of the long-term simulations evaluating the four management alternatives. *** 84 Continued

Calculations bounding erosion rate under high flow:

E = A * tau^ n; n between 2 and 3. Pick representative values of 2 and 3.
tau = B * vel^2
vel = a * Q^b; b between 0.3 and 0.5.
So, E = M * Q^exp where exp between 1.2 and 3
2^1.2 = 2.3; 2^3 = 8
So doubling of flow leads to increase of E between 2^1.2 and 2^3, or 2.3 and 8.

Review comments on FFS models and predictions of sediment transport and contaminant and organic matter fate

This review is based upon my reading of the Lower Passaic Sediment Transport Model Report (Appendix B II) and the Lower Passaic River Contaminant Fate and Transport Model (Appendix B III) as well as each of the attachments for Appendix III (but not the two attachments for Appendix II). After questioning the appropriateness of biological mixing rates and the uniform 10 cm biological mixing depth utilized in the model (a comment I also raised in review of earlier modeling efforts), I have also reviewed the Draft document of the Spring and Summer 2010 Benthic Community Survey Data of the Lower Passaic River Study Area dated January 31, 2012. Other materials reviewed included figures and comments provided for presentation of the Charge to reviewers and responses to questions raised at the mid-point teleconference. These studies represent the distillation of an enormous amount of high quality work and it would not be possible to put into these reports all aspects or details of model structure, synthesis of the key features of underlying key data, let alone assessments of relative data quality. Within the constraints of this review, I've not been able to explore in depth all the questions I have, many of which are addressed in other reports. I have done my best to understand model inputs and framework, assumptions that likely affect or have the potential to drive model behavior, and asked whether model predictions are reasonable, focusing especially hard on comparisons of model output to available data, whether or not used for calibration purposes.

Context for review emphasis and general impressions of reports: We are asked whether the combination of models presented is sufficient for predicting with some confidence the relative benefits of various remedial action alternatives, including monitored natural recover. On the mid-term call, most if not all of the reviewers expressed concern that the substantial benefits predicted for two of the alternatives were "non-intuitive" in that they that show dramatic effects of some of the remedial action scenarios on sediment COCs in the 0-8 RM stretch of the lower River (Deep dredging with cap and capping RM 0-8), where modeled concentration reductions of approximately two orders of magnitude for a range of COCs are projected often 85
without much loss in upper river segments (RM 8 – 12/13), and when sediments downstream in Newark Bay also remain elevated above those in the FFS 0-8 mile area i.e. the FFS study area becomes and remains a long term local minimum in concentration. In Figure 6-3 of Appendix III (and related results) it is seen that there are episodic sediment transport events that lead to higher concentrations in the FFS, but that those levels are rapidly attenuated with short characteristic times. It appears to this reviewer that the primary explanation for this is that very little net deposition of sediment (in comparison to historic deposition) is predicted to occur and when it does it is relatively temporary under these remedial action scenarios. Examining Figure 3 86
from a March 6 correspondence to the review team (predicted bathymetric change map 15 years after dredging) is consistent with this interpretation, although I think there may also be issues I don't understand related to how contaminated new sources of sediment to the watershed become during transport to RM 0-8.

The sediment data either poorly constrains model performace done during calibration or highlights some questions about setting of initial boundary conditions or predictions of contaminant decline that appear perhaps too rapid based on past changes. The comparison of the model predictions and initial boundary conditions in the contaminant model raise questions about how useful the calibration is as well questions regarding how intitial boundary conditions are set in Newark Bay, as well as whether there are better ways to normalize contaminant data to make the calibration and model/data comparison more constrained and useful. The contaminant model is calibrated with highly variable surficial sediment data, which as presented provides little constrain on interpretation of model performance in the Passaic. 87

For Newark Bay sediment data where there is a general bias with model predictions (often including initial boundary conditions) lower than field measurements. 88

A clearer picture of relatively recent longitudinal distributions is obtained from TOC normalized concentrations of key COCs provided us with the Charge Document, where it is seen that normalized concentrations are typically relatively uniform over the lower 12-13 miles of the River with generally modest declines (well less than an order of magnitude in all cases) with distance heading away from the mouth heading into Newark Bay. Using normalized data it is also more clear that there appears to have been little decline in concentration in most of the study area over the recent past, consistent with sediment core results we have been shown in the past. For carbon normalized DDT there is no concentration decline for several miles into Newark Bay, and for Hg and other selected contaminants, levels in outside the mouth are not vastly different than in the FFS source area subject to possible remediation. The model has some of these concentrations in Newark Bay dropping dramatically over time (notably DDT residues and Hg which decline with rapid half-lives), which is both saying something about confidence in model predictions in general. 89

As the primary driver of these results is the sediment transport model, it is important to understand what the model is actually projecting with respect to deposition in the lower Passaic River under different alternative remedial action measures, and what in the model controls these predictions. The review by Dr. Lick goes into the parameterization of the transport model in detail, and it appears that his concerns about potential biases my be matching my interpretation (provided below) that the model is on average likely predicting greater erosion that than observed in the field; my concern is that this then leads to an under-estimate of the importance of net deposition in RM 0-8 into the future and underestimate of the role of upstream and downstream sources of sediment in re-contamination of surface sediments in this area (which are manifested most in predictions in response to the two clean capping scenarios for the FFS area. 90

This review focuses on concerns and questions. **However, I want to take the opportunity to point out that I continue to be impressed by the Passaic River focused lab and field research level work that has been done on transport of non-cohesive and especially cohesive sediment.** I don't pretend to understand specifics of how different particle size assemblages are transported, conserved, or averaged in the model, but I appreciated that Appendix II was very well written, that the authors have tried to pull much out of the data and interpret it evenly in most respects. They should be commended for the this level of interpretation – there is nothing approaching this level of insight presented in the organic contaminant and organic carbon modeling report, which leaves one with so many more questions than answers. However, to be fair, there is much more underlying modeling and data in the Contaminant Report that can be discussed and much of the data available is not particularly amenable to for diagnostic model calibration purposes, at least in the ways that have been attempted here. Furthermore, most of the model and data have been reviewed elsewhere…there 91 is substantial merit in the fact that the model was extensively Peer reviewed as part of CARP, e.g. **I am extremely impressed by the amount of chemical contaminant data which was collected and interpreted** but which has only shown in the most distilled ways in Appendix III and even associated attachments. The very act of setting the boundary conditions for sediment contaminants with depth in every grid cell was an enormous task. It would be hard to please everyone with respect to the level of detail or type of interpretation and data interpretation in Appendix III. ***The key difference between the two reports in my view is that there is much less in the way of useful or insightful calibration in the contaminant and organic carbon fate report, and there are two major types of data that are amenable to sediment transport model testing, namely the temporal and spatial distributions of suspended solids and the estimates of net burial or erosion determined from single or multi-beam sonar studies that have been interpreted over two time periods.*** It is not clear to this reviewer if the model yields predictions of grain size that could be compared to field data in a useful way?

91 Continued

Before answering the questions the reviewers have been charged with, I make some comments where I believe I either have insights into how the model describes certain processes or more importantly where I have important questions or concerns related to how the model is describing data or making predictions.

Sediment transport modeling and results. For most if not all of the COCs of interest, sediment transport is arguably the key to the modeling efforts, and understanding the predictions from the combined models. I believe getting the sediment transport described reasonably (esp. net deposition/erosion) is more critical than how e.g. chemical reactions or transfer between phases are treated in the models, although the latter are also important esp. for lower Kow PCBs, and metals undergoing redox transformations or having lower Kd values in the model (i.e., esp. Cd and even Hg). ***I think that this investigation is unique in that one can argue that sediment transport is even more important in this study than in the vast majority of other sediment contaminant remediation/modeling studies of his type because of the extremely dynamic nature and high rates of erosion and deposition in the Passaic but also because only a fraction of the contaminated area is being considered for remediation – i.e., it is important to in determining whether remedial action goals can be met by only treating all or part of the 0-8 mile reach, when concentrations in potential source areas both up and down river are not mitigated and have concentrations either as high currently (DDE e.g.), nearly as high (Hg, TCDD) than those in RM 0-8.*** The lower Passaic is also an area where deep scour has been observed with both bedforms (and surface expression of contaminants) as well with numerous side scan sonar based bathymetric surveys conducted over the past two decades; it is unusual to see such clear evidence of relatively deep scour in what have been depositional areas with real data. I have focused my attention on how well the erosion and deposition models match the data (water column solids and net transport as well as net deposition derived from changes in bathymetry) and possible implications for biases between the two that concern me and may be saying something about model performance.

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<u>Conceptual model.</u> The conceptual model, put forth explicitly in both of these reports, and the materials distributed when we first discussed the charge for reviewers, is that the formerly dredged lower River has been documented to have filled with sediments at an incredibly high rate for decades but that the net deposition rate has decreased and the bed surface has reached a new quasi-equilibrium where net deposition is typically a very small fraction of

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gross deposition or erosion. I remain unconvinced that the area of the FFS is not still highly depositional over extended time periods and will ask e.g., whether there has been a *balanced* interpretation of all the bathymetric data (and perhaps other sediment core data not presented). It was not clear to the reviewers at the mid-review call why the model is predicting such low concentrations in the 0-8 RM area following either of the two remedial scenarios that results in a cap of the entire area. What appears to be largely at work is that contaminant levels, under alternate remediation conditions where the 0-8 mile reach is capped, with or without deep dredging, remain so low in the future (and why later spikes in concentration are dissipated with such rapid characteristic times) is that there is essentially negligible burial predicted by the model in most (but not all??) of the RM 0-8 area and that by averaging 0-15 cm, the model is essentially computing concentrations of solids that are still dominated by a clean cap surface - there are alternative explanations for the model behavior that are unfortunately eluding me with respect to my sense of physical reasonableness…..**a revised report should do more to address how much of the drop in concentration is due to averaging in the clean cap material, as well as insight into of the model predictions that lead to up to about 1 cm/yr burial in a couple of the reaches of the FFS ara (Figure 3 transmitted March 6 in the mid-point matrix response) with sediment that may not have become contamninanted during transport to the area - a corresponding map like this with concentration of contaminant in the 0-1 cm range would be both instructive to understand what the model is predicting** 93 Continued

The sediment morphology and chemistry data as well as the conceptual model are clear about the fact that there is great local heterogeneity of the bed with some zones of both intense deposition and erosion with a dynamic feedback between resulting morphology and hydrology that then controls associated shear stress. From a mass balance perspective, it seems to this reviewer that a likely reason that regional concentrations have not declined over the past couple decades, in the face of what is estimated as a large relatively clean loads of sediment over the Dundee Dam (and somewhat cleaner sediment from other boundaries), is that new erosional surfaces are exposing important hotspots of legacy contaminants that then "buffer the system". This is of concern when considering the risk of not remediating contaminant source areas above and perhaps even below the 0-8 mile reach (I note the ongoing clean-up at RM 11.9 – an area of concern that was raised in reviews of earlier modeling reports, when a similar conceptual model was proposed). **But for me, this issue raises the question of whether the grid spacing (the number of which are constrained by the complexity of the model and run-times) are small enough for models to reproduce potentially important localized erosion rates that may be important for exposing and exchanging materials from important hotspots**, where the product of very high concentration and small surface area might be high enough to change net fluxes from the bed. I see that Dr. Lick has also considered this issue. Here I will focus more on what the comparison of the field data and the calibrated model predictions might be saying about model performance and potential bias with respect to resuspension (erosion) and net deposition that are key to both chemical exchange with the water column and lateral exchange and deposition of sediment at RM 0-8 and elsewhere. 94

<u>Sediment erosion and redistribution.</u> ***Based on my weight of evidence interpretation, I question whether there is an important bias in the erodability of sediments and net erosion and deposition rates predicted from the models.*** First, although only "representative" time periods are shown, the model appears to over-predict erosion rates needed to explain the magnitude of most of the water column suspended solids data at the preponderance of stations – Of the 12 time series shown (figures 4-1 to 4-14), only in Figure 4-2 and 4-13 (moderate and low flow, MP 4-2 95

turbidity max region) are suspended solids levels not largely if not grossly over-estimated by models describing results of the Physical Water Column Monitoring (PWCM) program. Because of the magnitude of the differences in most cases it seems pretty clear that the primary driver for the difference is that the model is estimating deeper and more frequent depths of erosion (below the variable 0 -0.2 mm fluff zone) – as opposed to it underestimating settling rates. As described in the report the model does a great job of getting the phasing of resuspension correct (although I suspect little tidal blips in resuspension correspond to non-mechanistically defined fluff layers and not the actual consolidated bed). Vertical mixing as it affects water column profiles of suspended solids seem to reasonably well represented within the confines of the data shown. It is unfortunate that the sensors can not provide estimates for deeper near bottom depths with higher solids loads (Sigma 9, 10 and sometimes 8), because transport in these horizons can greatly influence if not dominate the integrated fluxes. Extrapolation of data towards the bottom are then needed to estimate sediment transport up or down River when using the observed data. 95 Continued

Figure 4-15 provides insights into the implications of overestimating resuspension rates when one appreciates that esp. under low flow conditions that resuspension is flood dominated leading to net upstream/estuarine transport of solids - **unfortunately Figure 4-15 only shows results for the fall period it seems (with lower flows – why not the other Spring data set with a bigger range of flows??**). The agreement between the "data" and model appear best at the upstream 13.5 RM site where upstream estuarine transport is least important and net fluxes in general are low at flows below 30 m3/s; however, although on a relative scale the net flux is much less positive in model estimates – a ratio of the two estimates would show that on a proportional basis there appears to be better agreement at high flow but how much of this is from local resuspension vs. high flux of residual upstream solids is unclear. As one moves closer to the mouth of the estuary and flood dominated upstream transport becomes more important, the differences between the often larger upstream modeled fluxes and lower "measured" fluxes becomes increasingly important, esp. at discharges between 5-65 m3/s. 96

The behavior of the model as function of river mile and flow is very nicely illustrated in 4-45 through 4-48. The model predicts lots of upstream transport of solids at lower to intermediate flows, increasing in magnitude as expected with tidal amplitude. What is likely largely the same pool of easily erodible material is swept by the model back downstream at high river flows, with the net long term fluxes downriver. This result is what was expected based both on asymmetry in tidal flow driven bottom shear the hydrodynamic model is mimicking, and also what we actually know about estuarine circulation and sediment transport. However, what is important to sediment transport and contaminant exchange in the water column is the frequency and magnitude of resuspension/eroded sediment depth. If one takes the suspended solids estimates at face value the model is sloshing around a lot more material than the calibration data indicate. **Does the model over-estimate resuspension and lateral exchange and as a result perhaps under-estimate net depositon?? These has profound implications for lateral transport and net deposition of contaminated sediment into the RM 0-8 FFS study area for all simulated alternative remediation scenarios.** 97

It is difficult form me to put too much weight on the high flow experiment where Bob Chant made three transects over part of a tidal cycle near the mouth of the Passaic (expected turbidity max near the mouth with this flow). It appears visually in examining Figures 4-17 to 4-19 that the model greatly over-predicts the magnitude of the predicted resuspension (which seems most 98 likely controlled by local resuspension rather than advection from afar given the spatial structure) – however, these figures are plotted on a linear scale, and when the data are presented on log scales (Figure 4-20) it is seen that the while the very highest suspended loads are not captured anywhere in the model, that perhaps the range of suspended solids concentations is, as argued in the Report, not that bad?? Chant should be given credit for getting out and making these measurements, but given that this sampling is neither synoptic nor "Lagrangian" when following the ebb, I would not make too much out of them, although one might also remember that other model results do suggest that settling times are not faster than the boat was moving from station to station. I would also point out there is something in the parameterization of settling rates that may be at work in this particular case where suspended solids levels are exceptionally high in the model) – when solids loads start to approach 1 g/L the model parameterization indicates that the fine floc fraction increases and leads to greatly reduced settling (Figure 2-4) – presumably this a result of capturing very high shear on particle aggregation rates. **I don't know if this assumption about solid concentration effects on settling rate is widely accepted or not?? Again this issue I see was raised in Dr. Lick's review. But because much of the downstream (and even upstream in the area of the turbidity maximum) transport occurs during high flow/very high suspended solids events, it may be that getting the dependence of settling velocity on computed solids concentration may be an important determinant in long term net transport?** 98 Continued

Bathymetric changes over time. I have placed significant weight on the estimates of net deposition or erosion based on differences in bathymetry measured over multiple surveys conducted between 1996 and 2010 and how those integrated volumes compare to modeling results (focusing on Figures 4-26 and 4-35). Much effort was placed on collecting and interpreting this data. The data have been presented with interpretations of changes between 1996 and 2004 (nearly 8 years) and then 2007 to 2010 (approximately 2.7 years or one third the time interval). There are a number of important points to make which may be important as it bears most directly on whether or not model can simulate what deposition occurs in the 0-8 RM stretch under varying remediation scenarios considered: 99

1. For the 1996 to 2004 data there is much average net deposition in the 1-7 RM range than estimated by the model in all but one of the RM segments and significant deposition is estimated to occur in in all segments (whereas in the model the only important net deposition occurs in the 2-3 RM stretch).
2. As long as measurement errors are not grossly different between the 1996-2004 time period, the net deposition estimates from this interval should be more accurate than for the 2007-2010 interval both because of the nearly tripling of time period allowing differences in elevation to rise above the errors, and because, if the data are correct the magnitude of annual deposition was greater during the first period (argued that this was in part due to the latter period capturing two higher flow events that transported more solids through the system). In Figure 4-24, it is seen that estimates of 5 to greater even greater than 50 cm deposited over the first almost 8 year year period – on the order of (0.7 to 7 cm/year) – suggesting to me that as recently as a decade ago this was hardly an equilibrium surface, or an area where one can dismiss easily that there will not be new deposition if it is capped (with or without deep dredging). The model does not have areas of nearly as high deposition at any River Mile range except RM 2-3. My back of the envelope calculation suggests that this cumulative deposition is a significant fraction of what 100

is estimated to be coming over the Dundee Dam (32,000 MTons/year) - below I raise the question as to how good those estimates of upriver sediment loads are. 100 Continue

3. Interpretations in this report and the basis of the entire conceptual model are however slanted towards interpretations and calibrations associated with the second three year data set that should suffer from more uncertainty given the much smaller differences in elevation that were observed or could be expected over a shorter time interval. However despite this, the magnitudes and spatial distribution of the magnitudes of net deposition compare very favorably to the model computations between 2007 and 2010; this is great, but the authors have essentially based their major interpretations on this second more recent set of comparisons between model and bathymetric change. 101

One is left to ponder whether one set of results is more accurate and whether the authors have placed their emphasis on the 2.7 year, more recent study because: it agrees better with the model and the conceptual view of the system that we have heard about; because the study was more recent and represents better the current (and future??) conditions; or because they really don't believe the adjustments used from the earlier bathymetric surveys. Are these integrated estimates of net depositon meaningful which I suspect they are at least over the 1996 – 2004 period. The fact that the surface was accreting so much over this period over much of the FFS area, under perhaps more average conditions between these years, and that the model does not reflect deposition in many of the these one mile reaches has me concerned again that there is a bias towards over-prediction of erosion and an associated under-prediction of net deposition. 102

<u>How well is the upper Passaic River and other tributary loads of silt known??</u> One thing in common to the contaminant transport model and sediment transport model is the importance of knowing loads of sediments, that also carry contaminants, from the upper to lower Passaic. The conceptual and actual models assume that most of the supply of cleaner sediment, that is important to long term recovery of the study area, comes from the upper Passaic. Much of that material is modeled to make its way rapidly to Newark Bay, from where some of it can re-enter the lower Passaic as a result of estuarine transport; it is not clear whether in the model most of the upper Passaic sediment that is predicted to now escape the lower River deposits along the way or not, but based on settling velocities, residence times, and intuition, I believe that to be the case. I started to wonder about how well constrained the loads of solids into the system are when I looked at Figures 3-2 to 3-6 of Appendix II. The baseline low flow concentrations of suspended solids vary markedly between tributaries but are remarkably flat at under low flow conditions and in the case of the upper Passaic and other selected tributaries TSM concentrations never drop below approximately 10 mg/L, whereas in other tributaries the concentrations sometimes drop to 1 mg/L or less, but also are relatively invariant with time under low flow conditions – there is remarkably little variation around basal concentrations. It should be assumed that these data are correct and I hope that they are – it would not be surprising if some freshwater streams/rivers never have low concentrations However, I would expect basal low flow levels to be somewhat more variable, which sets off potential red flags in my experience. I am curious about how well the sensors have been calibrated at low solids levels in each tributary, because my own very limited experience is that optical turbidity sensors correlate with solids in very different ways in different water bodies and there can be differential background (phytoplankton/DOM?? I'm no expert) affecting relationships that can create differences in positive intercepts/backgrounds when data are regressed. Because the baseline for the Passaic is so high (10 mg/L), it likely has a significant effect on the annual loading of TSM; i.e., less event 103

driven than in other tributaries where baseline TSM is much lower. I also know that there is some art (measurements I want someone with experience to do in my lab) associated with making low level TSM measurements, and depending on whether glass fiber or membrane filters are used, the volumes and differences in filter weights are not very great and susceptible in my view to positive bias. **My question then is how well has the TSM concentrations been calibrated and are there any potential biases that could lead to an artificially high estimate of solids loading down coming over the Dundee Dam.** 103 Continued

What does Figures 6-8 really mean? In Figure 6-8 an estimate of the fraction of Resuspended PR Silt has been presented…it may be staring me in the face but it is unclear what this corresponds to or how the calculation is constructed (depths/timescales). Ultimately all sources of sediment are from outside the basin if not from shore erosion (not considered here and I believe much of the area has hardened shoreline). What is the conceptual model behind this calculation? I think, but am not entirely sure, that most sediment deposited in the lower River has been eroded and re-depositied many times prior to net deposition. This latter point is addressed in the report in Figures that I'm not sure whether I follow. Should I infer from this that deposition of material in the lower Passaic is dominated by primary settling of what can be far upstream or downstream derived sediment with little subsequent resuspension, or that there is not much communication between RM reaches with respect to local resuspension events (i.e., very fast settling rates compared to advection). My understanding of this is important in my interpretation of what the model is computing**– I'm confused on this matter and would like clarification**. While interpretation of Figures 6-11 and 6-12 seem easier to understand, it may be that some of the same questions I have about Figure 6-8 apply to these figures as well. 104

Assumptions concerning wind driven resuspension outside the Passaic. Resuspension is only affected by the flow and tide driven hydrodynamic model. It would be difficult to include wind waves in the calculation, and I agree that neglecting this should be a very good assumption in the lower Passaic despite sometimes shallow depths, because of the high baseline turbidity, very strong riverine and tidal currents, and lack of fetch. However, it might be worth noting that all of these factors/assumptions are less valid in Newark Bay, because of increased fetch, much lower average current velocities and because baseline suspended solids levels are so much lower. Whether it is important or not I don't know, but not including wind driven wave induced resuspension in Newark Bay would lead to a model with less lateral redistribution, less exchange of contaminants with a water water column that is more open to boundaries with low contaminant levels, and would perhaps underestimate estuarine transport of suspended solids from Newark into the lower Passaic. I note that Dr. DePinto has also brought up this issue in his review. I've not looked at the bathymetry or sediment type maps in Newark recently - from my own limited sampling in Newark Bay in years past, I know that significant shallow areas are dominated by relic red clays that won't erode, but wonder if there are not shallow depositional areas that may be especially vulnerable to wind associated resusupension?? I doubt that many sediment transport models in estuaries explicitly account for wind, but I bring up this point anyways. 105

Appendix B III – Lower Passaic River Contaminant Fate and Transport Model 106

I have touched already on some of my primary concerns, which ultimately are driven in part by a lack of full insight into why remediation of only RM 0-8 with approaches employing a cap result in such amazing reductions in this area, in spite of source areas both up-river (argued to be small in surface area, especially when considering fine grain sediments), and the fact that "new" sediments moving through these regions will become somewhat contaminated during transport..… I have also already commented on the fact that because of potentially very highly contaminated relic layers that may be exposed by erosion, there is the chance of exposing small but still quantitatively important hotspot surfaces. Below I comment on the organic carbon/matter model, issues related to benthic communities and choice of biological mixing rates, the merits of equilibrium partitioning approximations, concerns about how initial boundary conditions are set, features of the model results I find somewhat troubling, and whether there are better ways to present and interpret the data.

Organic carbon/diagenesis models. As discussed in the conference call, I really don't think the organic matter fate model is necessarily appropriate or useful. I hope that this model does not really matter that much and suspect that it may not especially for high Kd/Koc contaminants that are not greatly affected by outputs of the sediment diagenesis model – on the other hand, for Cd and Hg, outputs of the model such as AVS, oxygen, and sulfate reduction rates affect in some manner the sediment "preservation", inter-compartmental transfer, transformations (methylation of Hg), or water column scavenging or whatever mechanism is responsible for getting low Kd Cd into sediments. These carbon models are based on conceptual models and calibrations that have derived and been much better calibrated for eutrophic estuaries where there is less light limitation and productivity responds to nutrients, and are much more marine with respect to the ecosystems modeled (e.g., the Chesapeake and Long Island Sound Studies). They have been applied in CARP, although I'd argue they might not be particularly appropriate for many areas considered there including the low chlorophyll high turbidity Hudson River. The lower Passaic is an extremely turbid, highly light limited, largely ***riverine/freshwater*** ecosystem where these models can not be expected to translate in many regards. Allochthonous sources of carbon (perhaps including detritus afftecting sediment TOC depending on whether sediments were sieved) rather than primary productivity must be much more important than the model is likely predicting, although estuarine transport of Newark Bay generated primary production may be locally important especially near the mouth of the lower Passaic.

106 Continued

The model predicts sediment TOC (not particularly well outside the RM 0-8, which I assume is because the model was modified to optimize calibration to data in this area) and DOC within factors of a few, but these parameters don't vary much in real world fine grain sediments or many riverine/estuarine water columns (except in cases of hyper-eutrophication or raw sewage inputs) or major rivers in time or space, so these don't seem very useful as calibration tools – of course if one wanted to calibrate the model one would want to calibrate against things that are model sensitive like nutrients, oxygen, sulfate/sulfide, redox depths in sediments, or chlorophyll, etc. For contaminant partitioning and exchange it is important though that TOC/POC/DOC are close to reality and sufficiently high. I do ask the question as to whether or not water column POC/foc is predicted sufficiently well – there must be data. If there is too much primary productivity in the model as I might worry about, there is potential for POC/foc to be too high and affect the transfer of contaminants into water. Work in places like the Hudson indicates that water column foc should be near to that of the local bed as particles are dominated by suspended particles. **I would like to see a comparison of what data is available and computed POC (foc**

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is more telling as a direct comparison). This would be a better test of the model as different model assumptions I suspect could lead to divergent foc predictions; furthermore if the model is computing water column foc values that are much higher than in the bed, the local equilibrium assumption will lead to greater rates of exchange from the bed to water column.

107 Continued

Assumption of biological mixing rates and depths of mixing. There are concerns that I and others have raised in the first two conference call and in prior reviews of Passaic River modeling about the sediment benthic community. My original concerns were related to my insights into the types and existence of biological communities in high sedimentation environment in the NY/NJ complex. While I don't have the proper literature to cite, it is well known that in estuarine setting that highly disturbed sediments with high rates of erosion or deposition do not support later successional communities that tend to bioturbate to deeper depths. Rather opportunistic species of small polychaetes (e.g., capitellids of which I think only one is measured in the Passaic, or spionids), amphipods, and small bivalves and gastropods dominate if they establish at all. According to Bob Aller (personal communication), in seminal papers on deposits in the subtidal Mississsipppi, there is no evidence of bioturbaation affecting sediment structure at deposition rates above 4 cm and clearly significant areas of the Passaic have often experienced this rate of accumulation at least in the past. I do not understand the basis for the estimates of mixing rates and depths reproduced in the Report from papers by Boudreau (I have downloaded for free his 1997 book from Boudreau's website showing the same figures but have not obtained the original source of data in those figure), because the figure captions indicate that biological mixing rate estimates at high sedimentation rate were estimated based on 210Pb when at those high sedimentation rates it would not be possible to get mixing rates or depths uniquely or usefully from that tracer.

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More important to this discussion I have now had an opportunity to review the Draft document of the Spring and Summer 2010 Benthic Community Survey Data of the Lower Passaic River Study Area dated January 31, 2012. While the data summaries only break out in part very shallow (2 feet below MLW as I recall) and deeper sediments (more important to the model) and sandy vs muddy sites (more important to the model), it is clear that benthic community abundances in the lower reaches of the River that have salt are very low, especially in deep fine grain sediments and the species richness is also very low (Robert Cerrato, Stony Brook University, personal communication for both points) - however there are communities present and regularly found throughout the area in both fall and early summer surveys throughout the lower Passaic. Estuarine species of polychaetes are indeed found over the very lower region of the River (approximately RM 0-5). But it is clear that freshwater communities dominate above RM 5. Cerrato agrees with me that the down River communites are both low in abundance and characteristic of Phase 1 opportunistic early successional communities, along with a couple surface predators. These communities and organisms mix only to shallow depths as I had feared. Much more importantly for this study is the observation that in the rest of the River above RM 5 benthic communities are characterized by freshwater assemblages dominated most often by oligochaetes (which definitiely do not mix deeply)– I take exception with the report making this demarcation at RM 8.5 between marine and freshwater – it is clearly changing around RM 5. Years of bioturbation measurements and modeling in freshwater systems (especially in the Great Lakes) indicate that mixing depths should not be more than a couple to a perhaps a few cm (often only 2 cm but lets say 2-5 cm); thus while the present work has done a sensitivity analysis doubling bioturbation depths to 20 cm, ***instead there should be a sensitivity test done to determine the effects of reducing mixing depths by 2 to 4 fold for most of the Passaic*** – The rates and depths of bioturbation can have a variety of important effects on the model under different conditions. I will not argue that the biological mixing rates are too high, although I think this is likely true for the more estuarine RM 0-5 region in the case of muddy deeper sediments given the low abundance data, but ***I argue there is strong evidence against mixing to 10 particularly over the largely freshwater or slightly brackish reaches of the lower Passaic. Deeper depths of mixing either below RM 5 or 0 would be considered extreme upper estimates but the need to reduce the depth is more clearly indicated above RM 0-5.*** 108 Continued

<u>Equilibrium partitoning assumptions.</u> I may disagree with some fellow reviewers, but I am quite comfortable with the equilibrium portioning assumptions that are employed, although it seems clear from the literature that Koc estimates for Cd are too low (Koc = 1000 - is Cd a COC? and if it is I should comment much more on its geochemistry and what the model might be saying – its distributions will be controlled not only by anthropogenic loads but largely by salinity as water column Kd s strongly affected by chloride complexes, and how sulfidic surface sediments are as Cd is known to be scavenged by sulfidic sediments); the Koc of 100,000 for Hg would also under-estimate water column sorption when most field data show measured Kd values of approximately the same order. Furthermore, Kd values for more soluble mono- through tri-CBs are likely somewhat low in the model as what is preserved in these highly dynamic environments likely is dominated by a more resistant fraction of these compounds. In aggregate, for the more hydrophobic organic contaminants, the Kd's predicted from the Koc values provided are reasonable with respect to being consistent with estuarine field measurements, especially after they are interpreted with respect to three phase partitioning that affects distributions defined by filtration. Just as importantly, the importance of slow desorption kinetics become less important in situations where the fraction sorbed is very high at equilibrium; i.e, very turbid waters and very high Kow compounds (see Wu and Gschwend,, I believe 1986). Finally, because of absence of much in the way of longitudinal gradients further minimizes the fraction of contaminant that needs to desorb as the aqueous phase is "buffered" but contaminant loading into upstreams and downstream waters. Where slow desorption can be expected to become more important is where susepended loads are low and where there are longitudinal or vertical gradients in the dissolved phase – i.e., where the dissolved phase becomes a significant sink for resuspended contaminants. For example, equilibrium approximations may become somewhat more tenuous in my opinion as one moves into the main body of Newark Bay; more worrisome is whether the rates of decreasing Hg over time in the reaches farthest removed from the mouth of the Passaic are being overestimated because of desorption to water driven by a the low Kd computed – because there are similar declines for even more hydrophobic DDT residues, there may be other explanations related to sediment transport and boundary conditions that are not very evident. ***The Kd predicted from a Koc for total Hg may be too low and could be affecting relatively rapid predicted loss from Newark Bay sediments.*** I would like to see better justification for the use of such a low Koc. I have not taken the time, but if requested could provide literature on Kds for total Hg that are much higher. 109

<u>Setting of initial sediment boundary conditions.</u> I was not able to completely follow the rationales, criteria, and methods for setting the initial boundary conditions for contaminants in surface sediments over the different reaches of the lower Passaic; e.g. the variable degree to which late 1995 data is incorporated, and generally discounted for RM 8-17 is presented but the critieria used in decision making not completely clear to me. ***More worrisome is what becomes apparent when examining Attachments 2 and 4***, where it is clear that Newark Bay initial 110

boundary conditions are set based on criteria unknown to me (don't think I missed it) that often doesn't fit any one of the average time point concentrations. More often than not the preponderance of measured Newark Bay data is underestimated by the model, sometimes dramatically; this is of concern because it affects recent and especially future sources that might affect RM 0-8. The model is also generally predicting a greater drop in concentrations over time in the RM -1.5 to 5.5 reaches than is often apparent in the data, or seems physically reasonable based on expectations from other very hydrophobic compounds (e.g., most of the six DDT residues drop almost 90% over 17 years in RM =- 2.6 – -5.5) – and not supported by most of the data that generally shows little change in average concentration over the calibration time period. It would be interesting to know what has driven the drop over time for contaminants such as DDT residues, Cd and especially Hg as mentioned above; for the metals this may be the results of low computed Kds. ***I would like to gain more insight into criteria for how these downstream surface sediment concentrations are inititally set in the model runs.*** There is quite poor fit of the model to sometimes extensive amounts of data collected in Newark Bay (Attachment II) that carry over into what may be less than acceptable predictions into the future for different alternative remedial action scenarios.

110 Continued

Points on calibration data and interpretations. In response to charge questions below, I make a few points about what would ideally be preferable for calibrations (e.g., ongoing work on water column data; sediment property normalized sediment concentratons; contaminant, suspended solids concentrations and organic carbon normalized suspended solids comparisons with the bed). As it is, the comparisons with ranges or averages of surface sediment concentations are not taken very seriously, and the x-y plots for surface sediments or sediments of all depths are shotguns, where success is based on factor of five error frequencies…it is not discussed that there are very often systematic biases in these plots exceeding the factor of 5 "acceptance level" when one looks farther out into Newark Bay away from the well sloshed lower River. Could not insights and results from dated high resolution cores and the carbon normalized surface sediment distributions as a function of space and time not be brought in to inform or constrain interpretations of surface sediment data and modeling results??

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The ephemeral bursts in COC concentrations over capped materials. The other troubling aspects of the remedial action scenario projections is that following erosion events there are sometimes sharp blips in the sediment concentrations in the RM 0-8 region, but these concentrations dissipate with characteristic times perhaps less than a year. The only explanation for this that I can come up with is that the contaminant clean cap gets dusted with deposited contaminated sediment and then it is swept out of the area by subsequent resuspension and lateral exchange processes (erosion)…

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Reviewer Charge Questions **(for some questions there is redundancy with comments above; for others I'm withholding judgement till I submit a revised review after the final conference call):**

1.Are the physical, biological and chemical processes represented in the model adequate for describing sediment transport, organic carbon and contaminant fate and transport for the LPR, with particular focus on the FFS Study Area? There is a detailed and what I believe to be near state of the art sediment transport model that has an unusual amount of calibration data – many aspects of the calibration can be described by the model, although I have questioned some potential biases between measured and modeled data that affect contaminant behavior and

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potentially the conceptual site model, that may not have received sufficient treatment in the report. Unfortunately this particular site and set of remedial action scenarios are arguably more highly dependent on sediment transport than which would be the case at many other sediment contaminant remediation sites that are either less energetic, involve less heterogeneity, or involve remedial action on all high concentration potential source areas, as opposed just the lower RM 0-8 source area. In my experience sediment transport models are generally considered less predictive than chemical contaminant fate and transport models – so while the sediment model is a major strength of this work, the predictions demand very careful scrutiny and I have made several comments and observations related to whether or not it is adequately predicting erosision and the importance of net deposition, and how that might impact contaminant concentration projections in the model.

113 Continued

I do not care for the organic carbon model for many reasons. However, with the exception of mercury and perhaps cadmium (which may not be a COC??), where sulfate reduction rates, oxygen, and AVS become important outputs of the model, it is not clear to me how application of the present model will dramatically affect the model results. I would need to understand more about how carbon flows, fate of carbon associated with new loads of suspended sediments, and how carbon is conserved between suspended and sediment particles and particle sizes to make definitive conclusions about whether the organic carbon model really effect the contaminants. As s long as sediment TOC is reasonably well described, and there is a reasonable amount of DOM to further minimize volatilization losses, it may be that the fate of hydrophobic organic contaminants is appropriately accounted for in the model; knowing what is happening with water column foc of suspended solids however would provide more insight into the model behavior and whether there are predictions that could bias contaminant fate predictions. I have pointed out that the carbon model is based on conceptual models and calibrations from eutrophic estuaries where there is less light limitation and are much more marine. The lower Passaic is an extremely turbid light limited, largely riverine ecosystem where I would be very surprised if these models can apply in many regards. Allochthonous rather sources of carbon (perhaps including detritus) rather than primary productivity must be much more important than the model is likely predicting, although estuarine transport of Newark Bay generated primary production may be locally important.

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There are concerns about the sediment benthic community raised above and how it relates to the estimates of biological mixing rates and especially depths. The benthic community data do suggest active communities that exist in very low abundance in deeper fine grain areas of interest, but that they are dominated in the lower reaches (approximately RM 0-5) by opportunistic polychaetes and other small species or predators that are not generally deep mixers, and entirely by freshwater assemblages dominated by oligochaetes (which do not mix deeply) above approximately RM 5. Years of bioturbation measurements and modeling in freshwater systems the Great Lakes indicate that mixing depths should not be more than a couple to a perhaps a few cm (2-5 cm); thus while the present work has done a sensitivity analysis doubling bioturbation depths to 20 cm, instead there should be a sensitivity test done to determine the effects of reducting mixing depths by 2 to 4 fold; I really believe this could be important, especially since the model is not projecting anywhere near historical net sedimentation rates.

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As for the contaminant fate modeling, the results are appropriately conservative in that they don't include biodegradation. I do not share some of my colleagues concerns about equilibrium partitioning assumptions both for reasons presented in the Report related to sensitivity of the

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model to raising Kd/Koc and organic carbon content of sediment, and for a combination of other reasons detailed in the discussion above. Where I do have concerns are with the apparently low Kds that would be predicted for Cd (Koc 1000) and what I assume is total analytically defined Hg (Koc 100,000); based on our most recent call, evidently the report has not fully detailed how metal partitioning has been treated and this needs further exploration. The Kd's predicted from these values are lower than the field data I've seen over the years and this is very important for Hg in this work. It is known that Cd is primarily in the dissolved phase in estuaries but sorption is much stronger at low salinities due to less important chloride complexes – which apparently is not accounted for …..Cd distrubitions would be difficult to describe because of strong scavenging in sulfidic sediments and seasonal releases back to water of part of it on a seasonal cycle. However, measured Kd (not Koc) values measured in the field are still over 1000. If Cd is in fact an important COC to model, there needs to be much more discussion of the role of particulate transport, partitioning and the role of AVS in both protecting sorbed Cd or scavenging it from the water column. For the organic contaminants, the Kd's predicted from the Koc values provided are reasonable with respect to being consistent based on estuarine field measurements operationally defined by filtration. 116 Continued

2. Have the appropriate data sets been properly and adequately used to set up the model input parameters and define forcing functions and initial conditions for the sediment transport, organic carbon and contaminant fate and transport models? I have emphasized how impressed I am with data assimilation and interpretation related to calibration of the sediment transport model. I do not understand how different particle sizes are moved around and accounted for in the model (for one example, coupling between the erosion model and armoring and how that carries forward to different parts of the model). Thus I don't know if there is anything that can be done to compare sediment grain size distributions computed and measured in the field. 117

With respect to the organic carbon model, I am unimpressed by calibration with sediment TOC, or water column DOC; perhaps I should be. I would be interested in seeing what the model is doing with respect to computing fraction organic carbon on suspended particles, and assume that at least some data exists for such comparision. Experience from the Hudson suggests that foc should be very close to that in bedded sediments – I expect that a model with important primary productivity would produce higher POC/foc. If there is indeed very poor comparison between measured and modeled water column POC, the model results might be questioned as it might mean greater rates of exchange between the bed and water column, although it may not be that simple. 118

The effort placed on setting initial conditions was massive, but more discussion is merited with respect to criteria for setting initial concentrations both upstream (e.g., whether to use 1995 data or not or how to possible adjust 2008 data), and much more emphasis should be placed on how initial conditions were set in Newark Bay or nearby Hackensack River sediments. Attachments II and IV clearly illustrate how poorly initial conditions and later conditions fit observed data in sediments in Newark Bay reaches. This is not adequately addressed in the main Report and may become critical when computing later the effects of Newark Bay sources to the capped areas following remediation. 119

The comparisons of the model to actual measured field data are very unsatisfying given the effort put into this exercise. Furthermore, the report lacks the insight generating level of interpretative 120 description of data that is abundant in the sediment transport calibration discussions. Part of the reason for this is that the model is much more complex and dependent on variables in potentially non-intuitive ways. However, more effort could have been put into finding outputs or testing parameters (more than modest changes in parameter sensitivity) to provide such insight into model behavior. It is not clear whether the best and most consistent approaches were used for setting the initial boundary condition in surface layers, both with respect to upstream regions where 1995 data is not weighed very much and in Newark Bay as already mentioned. Based on the wonderful carbon normalized figures we were given as part of the Charge documents it is disappointing that it was not deemed useful (or possible?) to reduce local variability in concentrations with normalization to carbon (or iron or aluminum if available). It is also noteworthy that the results from high resolution dated cores were not used to help present the conceptual model, as tests in model calibration, or as insightful tools to assist in data presentation and interpretation.

120 Continued

3.Does the model adequately represent the spatial and temporal distributions of the COCs in the water column and sediment be for the USEPA to use it as a took to compare the relative effects that implementing each remedial alternative will have on FFS Study Area surface sediment quality? We have not been presented a comparison between modeled water column data and measurements; it is mentioned that such a comparison is now possible and is underway. If the data set is adequate, it would be a much better test of the model than anything that has been presented in sediments at this time. I have commented above that don't believe that the comparisons between modeled and measured sediment concentrations has been presented and interpreted at a level commensurate with the effort involved or the importance of the questions. It would be useful to know how the carbon normalized data fits the model over the calibration period and whether there is enough Fe or Al data to be used for similar normalization. If there are concerns about the early organic carbon data, that can be stated – but not to show it I believe is a mistake.

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4. Does the model adequately account for the contributions of COC sources that may re-contaminate FFS Study Area sediment during and after implementation of each remedial alternative? I don't know and have focused much of my review on this question. There are some simple things that can be done to help us understand why contaminant levels remain so low relative to proximal areas and why occasional spikes in concentration are dissipated as quickly as they are. If most of this is because of low net burial on the then it needs to be acknowledged. Sediment transport models are useful research tools. It is not clear that they are sufficient to answer this question with high enough confidence to make such large expenditures on remediating only the 0-8 mile area if the necessary criteria is to achieve high levels of exposure reduction in the FFS area.

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5. Does the model adequately account for the effect of extreme storm events contributing to the resuspension and redistribution of contaminated sediments for USEPA to use it as one tool to compare the effects that implementing each of the four remedial alternatives will have on FFS Study Area sediment COC concentrations? With the likely need for additional work, this is a good set of models that I believe are well structured, especially for recalcitrant hydrophobic chemicals where description of redox chemistry is less important than it potentially is for Hg and even Cd. It is clear to me that the model can be used as **"one tool"** for evaluating remedial alternatives. If I were charged with making expensive management decisions **based**

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only on this model, I would have to say today let's wait for more information to be provided and incorporate additional targeted model testing into decisions that may not need to wait very long.

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Continued

Review Comments

Lower Passaic River Sediment and Contaminant Transport Reports

The primary purpose of this project is to develop and apply mathematical models which may be used as a tool to compare the relative effects of implementing several different remedial alternatives on the sediment quality in the FFS Study Area (paraphrased from the charge to the reviewers). Figure 6-3 of Appendix BIII, Temporal Plots of 2,3,7,8 TCDD Sediment Concentrations for MNR and Three Remedial Alternatives, compares the contaminant sediment concentrations of each remedial alternative in the LPR as a function of time. This is a very significant plot and is a dominant factor (along with the supporting evidence) in determining the appropriate remedial action. The results shown in this figure are discussed further in the appendix.

From the discussion of this plot and the supporting detail in the reports that we were to review, it is clear that the major processes affecting the concentrations of highly hydrophobic chemicals (such as 2,3,7,8 TCDD) in the bottom sediments (and therefore the major influences on the appropriate remedial action) are sediment dynamics (resuspension, deposition, and transport) and the hydrodynamics forcing this dynamics. Because of this, I will emphasize sediment dynamics in my review with some discussion of other processes that may, or may not, be significant.

The answers to each of the peer review questions requires a discussion of various processes, most of which are common to all of the questions. Rather than repeating the discussion of each process in answering each question, I have discussed each process in some detail in answer to the first question and have referred to these discussions in the answers to the subsequent questions.

1. Are the physical, biological, and chemical processes represented in the model adequate for describing sediment transport, organic carbon and contaminant fate and transport for the LPR, with particular focus on the FFS Study Area?

Various processes of possible significance are as follows.

Settling speeds.

The most significant factor affecting the transport of cohesive sediments in the overlying water is the flocculation (aggregation) of the basic individual particles (typically a few micrometers in diameter) into flocs whose diameters are often tens to several hundred micrometers and which can be as much as 124 several centimeters. The sizes and densities of these flocs affect their settling speeds (and subsequent deposition) by as much as several orders of magnitude. Flocculation and its effects are not considered in the LPR modeling, not even qualitatively, but should be.

In the modeling, comments are made about hindered settling. This is a separate factor and is only significant at large sediment concentrations (larger than those typically observed and modeled in the 125 LPR). At low to moderate sediment concentrations, hindered settling has little to do with flocculation or the description of settling speeds of cohesive sediments.

Experiments and theoretical analyses concerned with the flocculation of cohesive sediments are summarized in Lick (2008); references to the more detailed literature are given there. Experiments and 126 analyses quantitatively demonstrate the factors (with emphasis on sediment concentration, fluid shear, and salinity) which affect flocculation and especially the sizes, densities, and settling speeds of the flocs. A relatively complete and quite accurate time-dependent model of flocculation is described. Since the inclusion of this in a water quality model is quite time-consuming, a simpler quasi-equilibrium model (Eq. 4.50) is also given; this equation describes the floc diameter, d, as a function of the sediment concentration, C, and fluid shear, G. Experiments clearly show that (a) floc diameter decreases as sediment concentration increases, (b) floc diameter decreases as fluid shear increases, (c) floc diameter decreases as salinity increases, and (d) settling speeds decrease as floc diameter decreases. For constant fluid shear (although this is not the case in the LPR), this indicates that settling speeds decrease as sediment concentration increases. 126 Continued

In contrast, the LPR model ignores all physics and assumes a completely empirical model for settling speeds where settling speeds are **only** a somewhat arbitrary function of sediment concentration (Fig. 2-4 of App. BII) and are not dependent on fluid shear or salinity. The results shown in Figure 2-4 seem to be in complete disagreement with any experiments or analyses. A purely empirical model with no supporting physics gives little confidence in the ability of the transport model to predict. A better determination of settling speeds as a function of sediment concentration and fluid shear is needed. The dependence of floc size and settling speed on salinity is relatively weak and can probably be ignored for this application. Even though empirical parameters are probably needed for calibration, the correct functional dependence of settling speeds on sediment concentration and fluid shear should be retained.

Consolidation

After deposition, sediments consolidate with depth and time; this consolidation and associated changes in sediment bulk density have a major influence on erosion rates as a function of depth and time. The model of consolidation for depositing sediments as initially discussed in the LPR report assumes a sediment quasi-equilibrium profile, Eq. 2.17, and a time-dependent approach to this equilibrium in a first-order manner, Eq. 2-18. This may be true in certain idealized cases, but it is not correct in most consolidation scenarios. As the LPR modelers realize, this model does not fit the experimental data for a consolidating LPR sediment core. This is shown in section 3.2.7.2 and in Fig. 3-37. In particular, the sediments in the consolidation experiments had lower erosion rates and higher critical stresses than the LPR Sedflume cores that they were meant to represent. The LPR modelers then ignore the experiments and parameterize consolidation with little reference to any physics. 127

Bed consolidation is discussed in section 4.6 of Lick (2008); experiments with real sediments and analyses of these experiments are given. The bed density as well as other parameters were measured and are given as a function of depth and time. The most significant governing parameters are (a) the type of sediment, especially fine-grained versus coarse-grained sediments, (b) the depth (thickness) of the depositing core, (c) gas production and concentration, and (d) the sediment base on which the depositing sediments were deposited.

Figure 3-39 indicates that the core used in the LPR consolidation tests was 40 to 50 cm in depth; this is too thick and not representative of depositing, consolidating sediments in the LPR. No sediment base was used in the experiments. The appropriate experiments should have been done with core depths of approximately one cm or less (deposition due to tidal forcing) and additional experiments with core depths of a few centimeters (representing longer term deposition, especially in near-shore areas and in 128 the dredged navigation channel which is present in several remedial alternatives). Results with these short cores would have been dramatically different from those with 40 to 50 cm cores. 128 Continued

Another factor not considered in the experiments or modeling is the base on which the sediments were deposited. Sediment densities are strongly influenced by the water, gas, and fine particles in the core and their transport vertically due to consolidation processes, hence the dependence of sediment density on depth, time, and the thickness of the core. The base on which the sediments are deposited influences the density (and erosion rate) of the depositing layer because of the vertical transport of water, gas, and fine particles from the base into the depositing sediment layer. This effect can be quite large (Lick 2008, section 4.6) but was ignored in the LPR experiments and modeling. 129

Another factor not considered in the analysis of the consolidation experiments or in the LPR model was the effect of gas generation and transport in the base and in the depositing sediments. Gas is normally present and is significant in areas where contaminated sediments (and high organic content) are found, e.g., in the LPR. In UCSB consolidation experiments with sediments containing gas where sediment parameters were carefully measured, especially sediment density and concentrations of gas, it was demonstrated that sediment density first increased with time (as would be expected in the absence of gas) but then slowed and subsequently decreased with time due to gas production and transport, eventually reaching a slowly-changing, almost quasi-steady-state. 130

The above two factors would explain much of the discrepancies between the LPR consolidation experiments and LPR in situ cores. Valid experiments and analyses of consolidation are necessary for the long-term prediction of sediment transport. A more thorough investigation of sediment consolidation is needed, especially in regard to big events and the infilling of the proposed navigation channels. 131

Dependence of Erosion Rates on Shear Stress

In **all** previous experiments and analyses of the dependence of erosion rates, E, on shear stress, τ, done by UCSB researchers, it was determined that E was proportional to τ^n and that n was approximately 2. In addition, when I analyzed a few cases from the Housatonic (where it was reported that n was 2 to 4), I also found that n was approximately 2. I haven't had time to properly analyze the LPR cores and results, but I suspect that n's of 3 and 4 are not correct. Higher n's would primarily affect the relative amounts of erosion between average events and big events. Erosion rates are also an extremely sensitive function of sediment bulk density, a factor not considered in the modeling. 132

Density measurements

To understand and quantify the process of consolidation and the dependence of erosion rate on shear stress and sediment density, accurate measurements of sediment density as a function of depth in the core and time are required. The usual wet-dry procedure (used in the LPR experiments) is not sufficient. It is not sufficiently accurate and can not determine gas concentrations since the wet-dry procedure essentially eliminates gas in the core because of the mixing inherent in the procedure. A much better procedure, which does not have these limitations, is the method using the density profiler developed at UCSB (Lick 2008, section 2.5.1). This profiler accurately and effectively measures densities as a function of distance and time. Together with the wet-dry procedure, it can also determine gas concentrations as a function of distance and time. It should be used in any future work. It would alleviate and probably 133 eliminate the above two problems (consolidation and dependence of erosion rates on shear stress and sediment bulk density) as well as contribute information about the benthic boundary layer. 133 Continued

Grid sizes

A study of the effects of grid size on the sediment transport in the Fox River during a high flow period from May 22, 1989 to June 20, 1989 is summarized in section 6.4.4 of Lick (2008). There it is shown that, **over this period**, there is a net average deposition in the lower part of the river (DePere Dam to Green Bay) of approximately 0.4 cm. Other estimates of the average deposition **for the entire year** range from 0.6 to 25 cm/year. However, by comparison with 0.4 cm, the localized net changes during the high flow period as predicted by the model range from 7.5 cm erosion in the channel to a deposition of 19 cm below DePere Dam and several centimeters deposition in the near shore. Another way of interpreting this is to realize that a one-segment model (after calibration with observations) would "predict" a deposition of approximately 0.4 cm. This is to be compared with the results for the finer-grid model of local erosion up to 7.5 cm, local deposition up to 19 cm, erosion generally in the deeper central channel of the river, and deposition primarily in the near-shallow areas. There are obviously large differences between a fine-grid and a coarse-grid model.

In the LPR, the hydrodynamic and sediment transport grid is too coarse to adequately describe the lateral variations in the sediment dynamics in the LPR, especially in and near the previous and proposed navigation channels which typically have rather steep sides where slumping and rapid erosion can occur. This is further complicated because the grid size in the contaminant transport model is different (coarser longitudinally by a factor of three) from that in the sediment transport model. Consistent with the above study, this indicates that averaging sediment erosion/deposition over the contaminant grid will decrease the variability of sediment mixing due to resuspension/deposition and may even eliminate it. 134

The benthic boundary layer

I have seen some data (rather meager and insufficient) on the type and concentrations of benthic organisms in the LPR; but there seems to be no data on their activity and, in particular, whether they form an active benthic layer at the sediment-water interface of the LPR. In previous studies of several hundred Sedflume cores that were examined (Lick 2008), benthic layers were only found in a few cores, less than 1% of the total.

In the LPR model, it is assumed that a 10 cm thick benthic layer exists; mixing coefficients and a sediment-water transfer coefficient are also assumed. These assumptions are based on previous modeling studies where sediment dynamics (erosion, deposition, transport), generally the largest factor in mixing sediments, was ignored. Because sediment dynamics was ignored, something was needed to mix the sediments. By default, a benthic layer with empirical coefficients was invoked. 135

In some cases, an active benthic layer may be present, but it is not present or necessary in all cases. Before invoking a benthic layer due to benthic organisms, it should be demonstrated that sediment mixing by organisms is present and is significant, i.e., a benthic layer does exist. Parameters from out-dated models where sediment resuspension/deposition was ignored or minimized should not be used.

Is all this necessary?

The final decision on the remediation of the LPR (i.e., where and how much to dredge and cap) will depend on results similar to those in Figure 6-3. These results are primarily dependent on sediment dynamics and the forcing of this dynamics by the hydrodynamics. As a first (and very good) approximation, it can be assumed that highly hydrophobic contaminants sorb and stay with the sediment particles; LPR sensitivity experiments demonstrate this.

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It follows that, in order to determine results as in Figure 6-3, what is needed is a hydrodynamic model, a sediment transport model, and a simple contaminant transport model where the contaminant is completely sorbed to the sediment particle. It also follows that a complex carbon model and complex chemical fate and transport models are not needed. As a simple but reasonably accurate carbon model, it may be assumed that carbon may vary from one size particle to the next, but carbon always stays with the particle.

In the reports, many other contaminants besides TCDD are mentioned. However, the highly hydrophobic chemicals (such as TCDD) will tend to sorb and stay with the sediment particles while the less hydrophobic chemicals will tend to desorb and be transported away in the overlying water. In this way, the most hydrophobic chemicals are the base for a worst-case scenario. Because of this, results such as those for TCDD in Figure 6-3 will probably be the major influence on the determination of the appropriate remedial action. If the determination of the appropriate remedial action for the LPR is the major purpose of this project, then calculations of the transport and fate of all other chemicals are not needed. These latter models may be interesting from a scientific and academic point-of-view, but they are not necessary for this project. 137

In order to demonstrate this, it would be informative to do a large storm calculation with, and without, carbon and complex chemical fate and transport models. To some extent, this has already been done; and it has been demonstrated that the amount of carbon doesn't matter (section 5.3) and increasing the partition coefficient to keep more of the chemical with the particle doesn't matter (response on conference call). 138

The elimination of all these sub-models would greatly decrease the required computational time and the time to develop and calibrate these sub-models. In turn, the sediment transport modeling and the experiments needed to more accurately determine sediment parameters (especially settling speeds and consolidation of cohesive sediments) could be done more accurately. 139

2. Have the appropriate data sets been properly and adequately used to set up the model input parameters and define forcing functions and initial conditions for the sediment transport, organic carbon, and contaminant fate and transport models?

The discussions for question 1 indicate the following. (a) Consolidation experiments were not done correctly or analyzed properly and did not lead to meaningful results. Additional consolidation experiments and analyses should be done in order to improve the predictive modeling of sediment dynamics. (b) I believe the dependence of erosion rate on shear stress is incorrect and should be re-investigated. This would improve sediment transport predictions for big events. 140

3. Does the model adequately represent the spatial and temporal distributions of the COCS in the water column and sediment bed for EPA to use it as a tool to compare the relative effects that implementing each remedial alternative will have on FFS Study Area surface sediment quality?

As discussed in 1, the grid sizes for the hydrodynamic, sediment transport, and contaminant transport
models should be reduced and should be the same in order to eliminate averaging problems. The 141
description of the other processes mentioned in 1 should be improved.

A major problem, that to me remains unresolved, is the deposition, infilling, and subsequent
consolidation of sediments in the proposed navigation channels. Figure 6-3 indicates that the model
(and the associated discussion in the report) does **not** predict rapid infilling. This is curious since
historically there was rapid infilling of the previous navigation channel during its life and after dredging
was stopped; this infilling is the essential basis for the present problem of contaminated sediments in
the LPR and therefore needs a better quantitative understanding than there is at present. 142

In order to adequately answer questions 3, 4, and 5, the model (with a fine grid but over relatively short periods of time, and with no, or at least a very simple model of contaminant transport) should be used to demonstrate (a) the rapid infilling of the previous navigation channel; this should be done for average and big event conditions in order to demonstrate understanding, and (b) the infilling (or not) of the proposed navigation channels, again for average and big event conditions.

4. Does the model adequately account for the contributions of COC sources that may re-contaminate FFS Study Area sediments during and after implementation of each remedial alternative?

The overall results shown in Figure 6-3 (and similar results discussed elsewhere in the LPR reports) seem
to be quite robust and insensitive to changes in most parameters (but see discussion above). Some of
this apparent robustness depends on the **mathematical** averaging of the contaminant concentrations
over the top 15 cm; a more detailed presentation should include surficial concentrations of 143
contaminants, e.g., the top 1 or 2 cm where many organisms reside. These latter concentrations will
probably appear somewhat different and greater than the 15-cm average, and will also be more
sensitive to changes in parameters. A presentation and discussion of this would be helpful.

The contaminant concentrations in RM 8-17 seem to be more variable and more sensitive (less robust)
to parameter changes. The results seem to indicate that some dredging and capping should be done in
this area (from the conference call, this seems to already have been decided). Where and how much to 144
dredge and cap in this area, and the order of dredging (first upstream or downstream, etc.) seems to be
a more sensitive issue and deserves more accurate modeling.

5. Does the model adequately account for the effect of extreme storm events contributing to the resuspension and redistribution of contaminated sediments for EPA to use it as one tool to compare the effects that implementing each of the four remedial alternatives will have on FFS Study Area sediment COC concentrations?

For the LPR, the largest recorded event had a maximum flow rate approximately twice that used in the
LPR modeling. Very approximately, the bottom shear stress is proportional to the square of the flow
velocity, the flow velocity is an increasing function of the flow rate but not quite proportional to it, and
the erosion rate is proportional to the square of the shear stress (or possibly more). The amount of
sediment erosion, deposition, and transport is a function of the erosion rate, but this rate is modified by 145
bed consolidation as a function of depth and time and by bed armoring. Nevertheless, estimates such as
this (also see comments by Ambrose) indicate that sediment dynamics is a very nonlinear and rapidly
increasing function of flow rate. Large storm events will also lead to large amounts of deposition and

nonlinear effects on bed armoring, flocculation, and settling speeds. All of these will modify the erosion, deposition, and consolidation of the sediment bed during and after the storm in a manner not adequately modeled in the LPR. Calculations of sediment dynamics during a 100-year flow event (or similar big event) are needed with special emphasis on sediment deposition and consolidation in the proposed navigation channels. A relatively fine grid is needed in these calculations because of the rapid changes in topography due to dredging and the proposed navigation channels. 145 **Continued**

Appendix. Discussion of Figure 6-3 of Appendix BIII

Section 6, Evaluation of Remedial Alternatives, uses the LPR models to evaluate four different remedial alternatives: (1) no action, or MNR, (2) deep dredging and capping with a navigation channel from RM 0 to RM 8, (3) less-deep dredging and capping with a navigation channel from RM 0 to RM 2, and (4) partial (focused) dredging and capping. Figure 6-3, temporal plots of 2,3,7,8 TCDD sediment concentrations **averaged over the top 15 cm of sediment**, summarizes the model results for all four alternatives and leads to recommendations for remediation.

Because of its significance, it is important to understand this figure and the most important processes that affect these results. The authors have done this. However, for my own understanding (and perhaps those of others), I thought it would be helpful to discuss these results in a somewhat different, **but not contradictory**, manner. Since alternative 3 behaves in a similar manner to alternative 2 and since alternative 4 is a combination of alternatives 1 and 4, only alternatives 1 and 2 are discussed here.

For alternative 1, MNR, TCDD concentrations first decrease from 1995 to about 2010; for this period, concentrations in RM 8-17 are about half of those in RM 0-8. The high flows in 2007 and 2010 (and then repeated in 15 year cycles) cause an increase in TCDD concentrations above RM 8 due to resuspension of sediments and associated TCDD during the high flows and subsequent redistribution due to the high flows and subsequent tidal flows.

After 2010, TCDD very slowly decreases with time in both RM 0-8 and RM 8-17. Note that Figure 6-3 plots the average concentration of TCDD in the top 15 cm regardless of what is assumed as the thickness of the benthic layer in the model (assumed to be 10 cm in the LPR model). Although there is a TCDD flux from the sediments to the overlying water, the amount of TCDD sorbed to the bottom sediments is enormously greater than that dissolved and transported in the overlying waters. Because of this, the decrease in TCDD concentrations in the bottom sediments with time is very slow. This result is primarily dependent on the sediment dynamics (resuspension, deposition, and transport) and its forcing by the hydrodynamics.

For alternative 2, deep dredging and capping with navigation channel from RM 0 to 8, TCDD concentrations averaged over RM 0-8 (or RM 1-7) decrease almost to zero after dredging and capping (which occur about 2020). The model shows redistribution of TCDD from above RM 8 to below RM 8. However, the TCDD concentrations above RM 8 are about half of those below RM 8, and the contaminated area above RM 8 (from which sediments will be redistributed to below RM 8 by sediment dynamics and hydrodynamics) is much less than that below RM 8. Although the high flows (after 2010) will redistribute contaminated sediments from above RM 8 to below RM 8, the amounts of TCDD deposited below RM 8 are less than might be expected because of this.

Also, remember that the concentrations in Figure 6-3 are TCDD concentrations averaged over the top 15 cm. An average deposition of 1 cm of contaminated sediments therefore is reduced by a factor of 15 due to this mathematical averaging, independent of any mixing due to sediment dynamics, bioturbation, or any other process.

Over time, TCDD concentrations above RM 8 are reduced. There is an increased shear stress and presumably erosion above RM 8 due to the presence of the navigation channel below RM 8. There should also be some bed armoring above RM 8 due to the transport of sand from the sand cap below RM 8. This would decrease erosion. The net result is little transport of contaminated sediment from above RM 8 with deposition below RM 8. The result is that there is little change in the 15 cm average TCDD concentration with time.

As discussed, the major processes affecting the results shown in Figure 6-3 (and therefore the choice of the appropriate remedial action) are sediment dynamics and the hydrodynamics forcing this dynamics. As far as contaminant dynamics is concerned, the approximation that the highly hydrophobic chemicals completely sorb to and stay with the sediment particles is sufficient.

With this approximation and the hydrodynamic and sediment transport models, an LPR model should be able to very accurately reproduce the results shown in Figure 6-3. No complex carbon model and no complex fate and transport models are needed. Not even the presence of a benthic layer (or its absence) is required. 146

Re: Review of the **Lower Passaic River, Lower Eight - Mile Focused Feasibility Study Sediment Transport, Organic Carbon and Contaminant Fate and Transport Model.** **Additional comments related to the Final Review Teleconference of 03/20/13 are Appended in Red.**

1) Starting with the now-familiar Figure 6-3 of Appendix BIII –

a) I find the response to No. 1 on the "Midpoint Teleconference Matrix (2013 03 06)" less than satisfactory. The derivation of a "fixed" MNR half life (i.e. one incorporated as part of the future projections without consideration of error or uncertainty) of 17.9 years from the 1996-2010 data seems absurd. Any reasonable consideration of the standard deviations of the "upper 15 cm average" data presented might even include the possibility of an increase in 2,3,7,8 TCDD concentration with time! 147

b) To me the reason seems fairly clear, and is related to the "geochemical" differences among the samples that were averaged – grain size, organic carbon content, the amount of time represented by the 15 cm at each site, etc. 148

c) With all the ancillary data available on these samples, I am amazed that apparently no attempt was made to "reduce the range" of uncertainty associated with each time point by incorporating some of these differences. Has there even been an attempt to look at the relationship between the concentrations of the different COCs in each sample? 149

d) The next major problem raised by the figure concerns the "upper 15 cm average." On Figure 3 supplied with the "Midpoint Teleconference Matrix (2013 03 06)", a response to concerns about "Sedimentation rate on Cap," model results for RM 0 to 8 range from net erosion (up to about 5 cm) in some areas to net deposition (up to about 25 cm) in others over the fifteen year simulation. [Note: I'm assuming that Figure 3 applies to post-dredging ***and full cap emplacement*** bathymetry.] How this distribution of predicted sedimentation effects the "near zero" long term model prediction "average" concentration was not addressed. 150

e) This brings up the usefulness of an "area average." Will, as I suspect, areas of slower net accumulation be the worst in terms of benthic community exposure to any re-contamination? Along these lines, I am still unclear on model predictions regarding the "navigation channel" area (RM 0 to 2.2) of the Full Cap alternative 151

f) Concerning the fate of accumulation on the cap – Is it vertically mixed in the model? Are finer particles "allowed" to settle through the sand? How is resuspension of particles deposited on the cap treated? 152

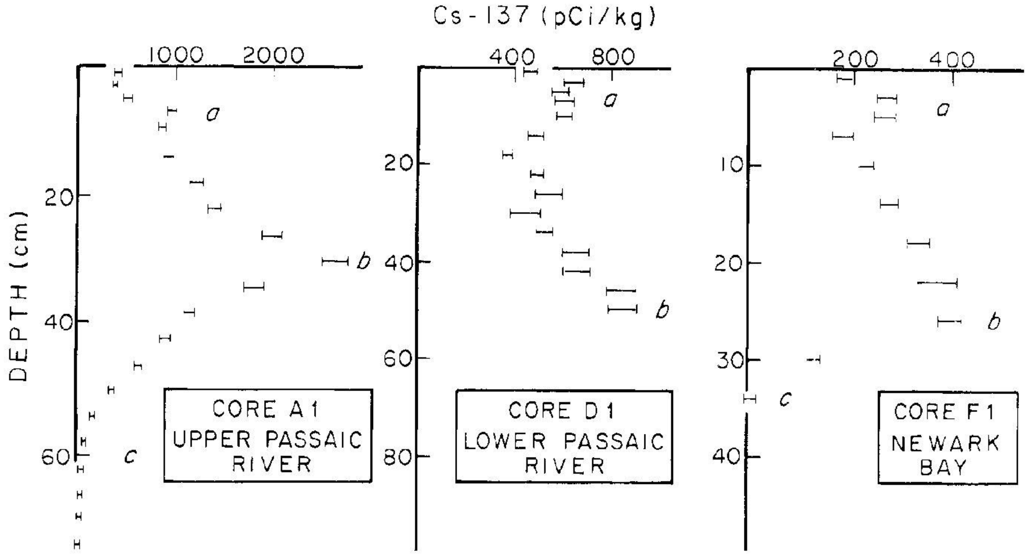
g) The minimal/near zero predicted long-term re-contamination atop the full RM 0 to 8 cap was questioned at the midpoint teleconference. Reference to model results was provided with northern Newark Bay (and the Hackensack) and upper Passaic 153 being significant particle contributors. This should have been discussed in much more detail and broken down in terms of contaminant sources, concentrations and fluxes. 153 Continued

h) From that perspective, the upper Passaic becomes a potentially significant source of mercury and PCBs, especially in high flow events that could scour and transport deeper, more highly contaminated sediments that were identified in well-dated sediment cores collected by my research group as far back as the mid 1980s and as recently as 2005 with Dundee Lake cores Pass 8B and Pass 8BP with analyses partially funded through the Passaic River RI. 154

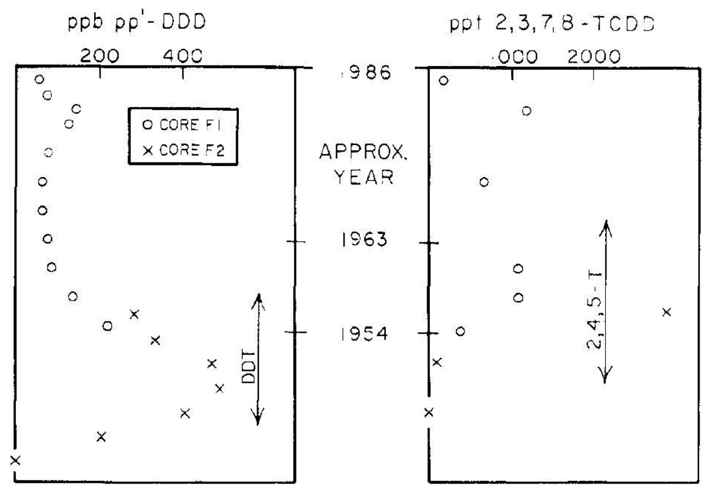
2) **This leads to a consideration of the handling of extreme storm events**. 155

a) Some background – A recent email refers to the CARP MEG (Model Evaluation Group) review with respect to the Hg model and its review by Joe DePinto and Chad Hammerschmidt. I was a late (and somewhat reluctant) addition to the MEG and my only significant suggestion was that they try to match some ***real system data*** by hindcast modeling. Specifically -

- transport of PCBs from the upper Hudson to the NY Harbor associated with extreme events in the mid 1970s (a dam removal and a hundred year flood).
- The Indian Point release of significant amounts of Cs-137 in 1971.
- ***AND the effect of the extreme Passaic flow of 1984 on the western NY/NJ harbor.***
- A HydroQual memo of February 14, 2005 began to address all of these hindcast simulations, but only the second was actually modeled to some extent with some success.
- The "real system" data applicable to the 1984 Passaic event is summarized in the figure below. Those cores were collected in 1985 and 1986, and published in 1993.



^{137}Cs profiles in sediment cores from sites A, D, and F. Horizon *a* is the near-surface maximum (1984); *b*, we associate with the fallout maximum (1963–1964); *c* corresponds to the initial input of significant fallout (about 1954). Horizontal error bars indicate one standard deviation based on γ-counting statistics.



Chronologies of *p,p'*-DDD and 2,3,7,8-TCDD concentrations in Newark Bay sediments from sampling site F, cores F1 and F2. Approximate dates were assigned on the basis of the ^{137}Cs profiles, utilizing the time horizons described in the text. Periods of production of DDT and 2,4,5-T at the 80 Lister Ave. plant are indicated by the arrows.

b) The significance of the 1984 high flow event with respect to the modeling and simulation of "extreme" events is evident when one looks at the flow records from the Passaic River at Little Falls. I do not believe that Figure 1 of the "Midpoint Teleconference Matrix (2013 03 06), supplied by HydroQual provides the most useful perspective. I prefer the following plot of mean daily discharge at Little Falls on the Passaic from 1930 to March 2013 reported by the USGS.

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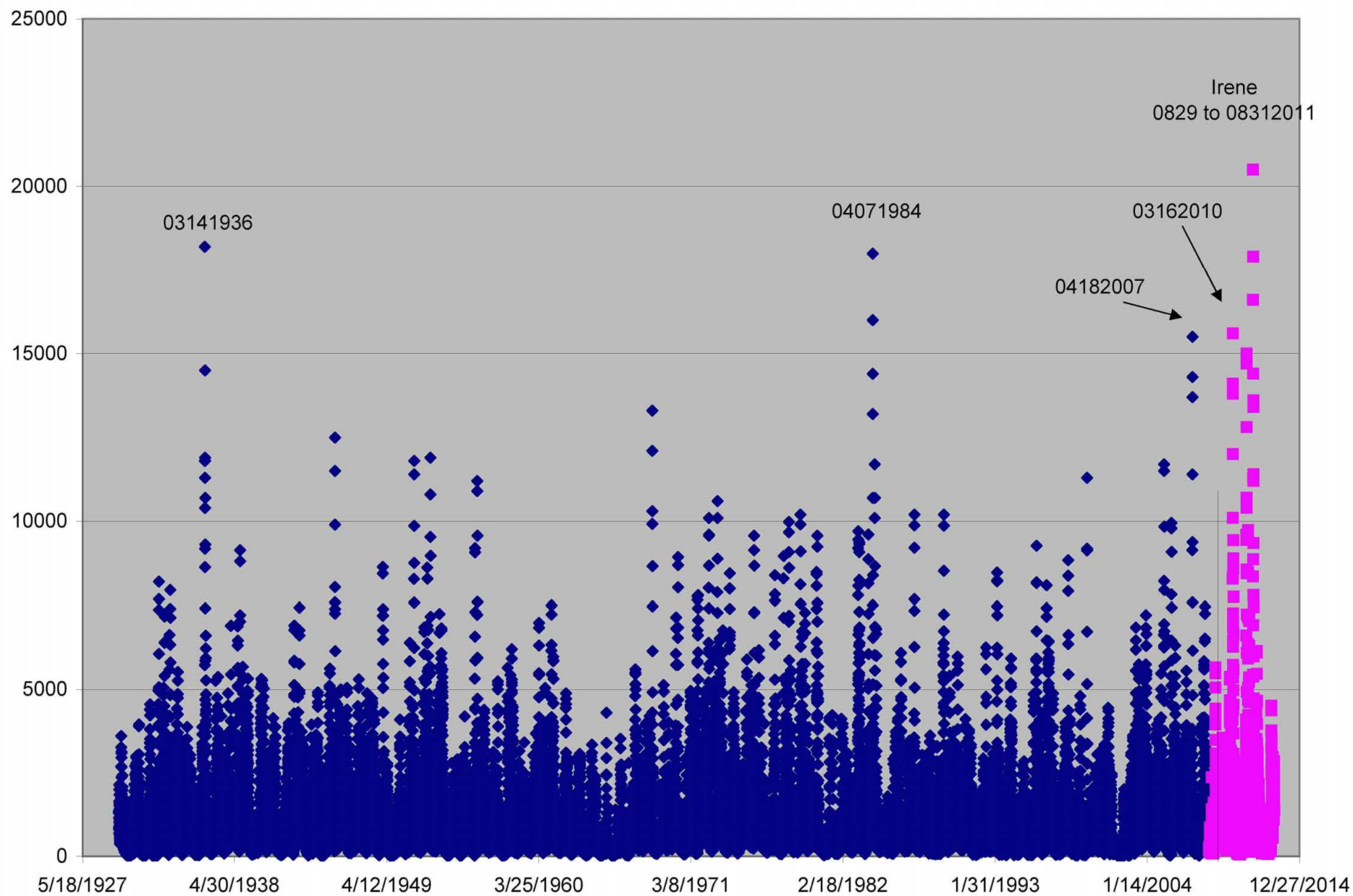

25000
20000
15000
10000
5000
0
5/18/1927
4/30/1938
4/12/1949
3/25/1960
3/8/1971
2/18/1982
1/31/1993
1/14/2004
12/27/2014
03141936
04071984
03162010
04182007
Irene
0829 to 08312011

c) The 1984 event is associated with significantly higher mean daily discharge (18,000 cfs) than the high flow included in the model simulations – 03/16/2010 (15,600 cfs). The non-linear relationship between flow, bottom shear stress, TSS, particle flux etc. makes this difference potentially quite significant.

d) ***IN ADDITION***, the real game changer here appears to be Hurricane Irene represented on the right hand side of the plot with peak daily average flow of 20,500 cfs and three consecutive days averaging above 16,500 cfs(!).

e) The significance of Irene is further emphasized by the 2010 to 2011 bathymetric change maps distributed a few days ago. What's a difference of a few feet of sediment (some depositing, some eroding) among friends (Sorry, I couldn't help myself…..). I guessing that it was a preliminary look at the Irene bathymetry changes that vanquished the term "quasi-steady state" from the sedimentary regime discussion of RM 0 to 8. [Note: I had to check three times – that scale on the bathymetry change maps is feet as indicated, right?]

156 Continued

f) ***AND,*** we have not yet seen data related to the effects of Sandy! It is not, in my opinion, at all unreasonable to believe that global warming has a significant role in the recent hydrodynamics of the Passaic and Newark Bay, especially with respect to extreme events that are not well represented in the current model.

3) Other aspects of extreme events –

a) As mentioned above in 1 h, with deep erosion in an extreme event, our data indicates that the upper Passaic is of concern with respect to re-contamination of a capped area with Hg and PCBs (and probably PAHs as well). The other major sources of mercury in the area identified by our data on dated sediment cores are the Hackensack (Berry's Creek) and the Arthur Kill (possibly associated with smelting at the former National Lead site. With respect to the influence of the Hackensack on Newark Bay (and by tidal extension, the lower Passaic), we have identified and proposed the use of a tracer based on Cr concentrations in dated sediments.

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b) Without information on the distribution and concentration of Hg in the sediments of these areas, I do not see how the model can hope to simulate the impact of extreme events on sediment-associated contaminant deposition on a cap in the Lower Passaic. Modeling fluxes from these systems to the lower Passaic with data from a station or two near the boundaries does not seem at all adequate.

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c) With respect to "deep erosion" and extreme events there is real system data to indicate significance with respect to contaminant transport from "otherwise depositional" areas. We have recently identified two separate events, one in the upper Hudson in the spring of 1976 and one in the Mohawk in March 1964 that

159 Continued

removed on the order of a foot of sediment from large, otherwise depositional areas. The Mohawk event in 1964 is noteworthy because it was not identified in the average daily discharge data, but only in the "instantaneous" USGS (15 minute) data, as it was likely associated with the breakup of an ice dam. An exceptional resource for insight on extreme events from a water column perspective is Gary Wall of the USGS. 159 Continued

d) The 2010 to 2011 bathymetric change maps showed, not unexpectedly, nearshore areas of significant erosion and other nearshore areas of significant deposition – although much of the river did have a "very nearshore" (shallow) area that was not color contoured…… Together with the statement in the charge that the navigation channel had been "sporadically maintained" from RM 0 to RM 2 until 1983 and to RM 15.5 through the 1950s reminded me of questions I had been asking since about 1990. 160

e) It has been reported that prior to 1970, dredge spoils (highly contaminated, to be sure) from this area were disposed of primarily as fill in areas around Newark Bay. Do we know where they were put? I expect that this issue will be central to the Newark Bay study. For now, however, it does seem relevant to at least ask if we know of any disposal/fill sites along the lower Passaic, or any in adjacent waterways that could be eroded in extreme events.

4) **Summary**

a) I am disappointed
- with the amount of attention paid to unverifiable detail without significant real system data from the Passaic (e.g. the mercury model);
- with the amount of detail that in the model that, by my assessment, extremely poorly represents the real system data (that would be the carbon model); and 161
- by the manner and minimal extent of "incorporation"/consideration of sediment contaminant and compositional data (see comments 1b and 1c above).

b) I believe that the Phase 1 and Phase 2 removal of the most highly contaminated sediment is an excellent start to the overall remediation effort. However, recent extreme events, Irene and Sandy, suggest that any capping project must be extremely well and cautiously designed. From my perspective, removal seems safer than capping, but at some point cost will be prohibitive. 162

c) The model presentation did not appeal to or satisfy to any significant extent, my geochemical intuition or my first hand knowledge of and experience with the data. Consequently, I do not have confidence in the "no significant long term re-contamination" prediction of the full cap model on an "upper 15 cm, area average" basis. 163

Comments related to the Final Review Teleconference of 03/20/13

- **Apparently my interpretation of the MNR "half life" (discussed in 1a above) was incorrect. It was not derived from the LPR sediment data, but was a model output reflecting those "highly variable" averages at the different time points only insofar as they were used to constrain the local resuspension flux. The other "data" used were the rather poorly constrained boundary conditions (Bruce Brownawell focused discussion on the problems with Newark Bay; some of my comments above question the handling of upper Passaic and Hackensack R contaminant sources).** 164

- **The "highly variable" averages at the different time points in the LPR surface sediment data were the subject of some discussion and considerable consternation. My suggestions (1b and 1c, above) were met with comments about an "unexplained" factor of 2 in organic carbon content in the 1995 samples which apparently precluded any normalization (?). When the possibility of normalization using other ancillary measurements (Fe, Al, grain size) was brought up, we were told that Solomon and Ed Garvey had looked into that with no success. With all due respect, I wouldn't mind looking at and evaluating that data myself to try and uncover the cause(s) of the apparently low quality of the ancillary data so that similar problems might be avoided in the future with respect to Newark Bay and beyond.** 165

- **My discussion of the 1984 high flow event as actually recorded in sediment cores (see 2 above) brought the question from Hydroqual of whether *I* had an idea of the impact of the flooding of the 80 LA site on the contaminant signal associated with that event. While I did not, I feel it important to point out that the sediment record of the event in the upper Passaic, Lower Passaic, and Newark Bay has barely been exploited. Cs-137 profiles, some selected pp'-DDD analyses, and even fewer selected 2,3,7,8-TCDD analyses comprise the great majority of analyses to date. I would note that *all* the samples from all the cores discussed in 2 above are part of our sediment archive. Furthermore, "Bopp dried" samples have been checked against wet analyses for dioxins, dibenzofurans, PCBs, Hg, and metals as a "condition" for using data from our recent upper Passaic cores and to provide justification for possible use of our archived samples. This was done in a small project organized by Len Warner (currently with The Louis Berger Group) in 2007 resulting in a Draft Report dated February 2009. I would certainly recommend that additional analyses of our archived samples, including Hg and XRF metals analyses at RPI, be considered as a most logical step toward improving our understanding the impact of the 1984 high flow event.** 166

- **Apparently, my "game changer" comment about Irene (2e, above) did have some effect as analysis of the 2011 flow data will be added to the modeling effort. The impact of Sandy (2f) was, to a large extent, dismissed as an "upstream-directed" event affecting Newark Bay most directly. My follow-up, the observation that Newark Bay was also an important contaminant source to the LPR, was met with a statement about insufficient bottom shear stress in the Bay during Sandy to produce a major perturbation in contaminant transport to the LPR. I apologize if I've misinterpreted some of what Hydroqual said, but I strongly feel that the effects of Sandy need to be evaluated and explained in some detail (*not modeled*….) as part of the final Hydroqual report.**

- **Collection and careful analysis of a few *good* sediment cores from Newark Bay and the Lower Passaic River at this time would certainly, in my opinion, improve our understanding of sediment and associated contaminant transport dramatically.**

FIN

Attachment B

Responses to Peer Reviewers' Comments

Comment Number	Charge Question	C=Comment R=Response	Comment/Response
1	1	C	I view this question as asking about the theoretical framework of the model and the formulation of processes within that framework. I think that the model framework, which consists of a linked hydrodynamic-sediment transport-organic carbon sorbent-contaminant fate and transport model is about as close to a complex state-of-the-science model as we will find for the kind of management questions being asked for the FFS. Below I will discuss some issues with the parameterization/calibration and application of this model framework that could be improved.
1		R	No response necessary.
2	1	C	The model domain includes the Hackensack River (HR) and Newark Bay (NB) along with the Lower Passaic River below Dundee Dam, with the Kills serving as the downstream tidal boundaries. With this model domain and the tidal movements within the system, NB has a very important exchange with the Lower Passaic River and potential for recontamination during the remediation scenarios. The authors mention in the BII report that they did not include wind-driven or ship traffic-driven resuspension in NB in the model. I would suspect that these are the most likely drivers of resuspension in the Bay and resulting contaminant concentration at the boundary between the Bay and RM 0 of the Lower Passaic River. Therefore, these processes need to either be included in the model formulation or an analysis needs to be presented that convinces the user that they are indeed not important.
2		R	The reviewer raises a point that was considered by the modeling team during model development. Analyses were performed to assess the significance of wind-waves on bottom shear stresses and these will be documented in the final modeling report. The analyses showed that wind-wave generated shear stresses high enough to resuspend bottom sediments were limited to shallow near-shore areas. Wind conditions that generate shear stresses high enough to resuspend bottom sediments in near-shore areas were generally limited to times when the wind direction was along the long axis of the bay. For wind directions across the bay, the fetch is too limited to generate substantial wind-waves. These analyses indicate that wind-waves do not have a significant effect on solids exchange with the FFS area and therefore wind-waves were not included in the FFS modeling. Wind-waves may be included in the modeling for the Newark Bay Study Area RI/FS. Ship generated resuspension was not included in the FFS modeling. An inspection of ship track information from several months indicated that the vast majority of ship traffic occurs south of the Port Newark Channel, and most of that occurs into and south of the Port Elizabeth Channel. Ship driven resuspension is being evaluated for the Newark Bay Study Area, but was judged to be too limited in the Passaic River and northern portion of Newark Bay to warrant inclusion in the FFS modeling. Additional information on both of these topics will be included in the final modeling report.
3	1	C	The model uses a standard 3 phase local equilibrium model to describe partitioning of hydrophobic contaminants to particulate and dissolved organic matter. In general, this is a reasonable way to formulate these models, but with the emphasis on the observation that there is a great deal of tidal-driven resuspension and longitudinal transport of those sediments by the tides, it is possible that contaminants in the KPOC range of ~5-7 could be losing a significant mass of chemical from the repeated resuspension events to desorption and downstream transport and/or, depending on their Henry's Law Constant, volatilization. Several studies have shown that desorption rates for these hydrophobic chemicals are much slower than absorption rates, slow enough that equilibrium is not likely to be attained before the sediment resettle into the sediments. It seems that there is a need to investigate the impact of the process equilibrium assumption on long-term transport of COCs out of the system and the resulting effect on the rate of decline of surface sediment contaminant concentrations during MNR. With respect to partitioning of contaminants to POC, the OC model simulates 10 different POC forms with potentially different foc's; yet, I think the model uses the same KPOC for all of these POC state variables. I would expect that there are potentially very different characteristics of OC among these forms of POC, so I wonder if they may have different effective KPOC's. A related question is "how does the contaminant model handle partitioning to inorganic solids (both cohesive and non-cohesive) from the ECOMSEDZLJS model?"

3		R	The model as formulated does have the potential to resuspend, desorb, and transport contaminant. Given the high frequency of resuspension for the majority of the system, approximately every six to twelve hours, the solids that are resuspended on a regular basis may not have enough time to reach equilibrium with the water column within a single resuspension, and subsequent redeposition, but would likely reach equilibrium over the course of the repeated cycles of resuspension and deposition. With a relatively low rate of mixing within the active layer of the model, the particles that are resuspending regularly will not pick up a concentration as high as the bulk of the active layer each time they are deposited to then desorb on the next tidal cycle. To some degree, the impact of non-equilibrium partitioning is captured by the site-specific partition coefficients used in the model, which are based on field-measured particulate and dissolved concentrations from the study area and the surrounding water bodies. The sensitivity simulation with an increased partition coefficient and the effect on the computed results will be added to the final modeling report and discussed in greater detail to further clarify the impact of the assumption of equilibrium partitioning. Particulate organic carbon (POC) is represented in the carbon model as eight different forms. Some of those forms are effectively redundant and were incorporated for the purposes of being able to track external sources of POC separately from resuspended POC with the potential to specify different settling behavior. This feature was not incorporated or implemented within the sediment transport model, which is the source of the settling velocity information used for the POC. As the model has been implemented only the two groups of algal-POC have distinct settling velocities from the remainder of the POC, and for the purposes of managing file size issues the rest of the POC is passed to the contaminant model as a single total concentration. Further, if the separate forms of POC were passed to the contaminant model there would not be sufficient data available to support development of distinct partitioning properties for the individual forms of carbon. The model does not include partitioning to inorganic solids.
4	1	C	Cohesive sediment settling is modeled by assuming (based on site-specific observations) that the single cohesive class used in the LPR version of ECOM-SEDZLJS actually consists of a combination of a very slowly settling background population of particles and a relatively rapidly settling population of bed aggregates or flocs that are resuspended and deposited on a tidal basis. It was not clear to me how the split between these two fractions was derived from the SEDFLUME results and consolidation experiments to parameterize resuspension versus tidal-driven bedload. Another question relative to this aspect of the model is whether the background discrete particles have a different foc than the bed aggregates; this will impact the contaminant transport during the resuspension process. As discussed below, I think that this decision process will have a major impact on the contaminant fate and transport in the system, and therefore should be more carefully documented.
4		R	The split between the very-slowly and rapidly settling particles does not come from the Sedflume results or consolidation experiments. The split is based on the patterns in water column suspended solids estimated from the high frequency acoustic backscattering (ABS) data collected in the Physical Water Column Monitoring (PWCM) program, which provide estimates of suspended solids concentrations over both depth and time. As described in section 2.7, the slowly settling particles are dominant at both very low concentrations (during slack tides) and at very high concentrations (during large runoff events). The tidally resuspended and deposited rapidly settling particles are dominant in the range of concentrations typical of resuspension/deposition events. It is possible that these different particle behaviors actually represent different particle types, but because there is no data on particle properties segregated by in situ size or settling speed, there is no basis for assigning different properties, such as fraction organic carbon (foc) to different settling velocity ranges. In the FFS model, cohesive solids eroded from the bed move directly into suspension and are not transported as bedload. Resuspension rates of bed solids were derived from Sedflume experiments, as described in the sediment transport modeling report section 3.2.3 and Attachment 1 to that report. Fraction organic carbon (foc) of upstream solids flowing over Dundee Dam is not assigned explicitly; rather, separate functions relating solids and POC to river flow were derived from data analyses. Foc of the bed, and therefore resuspended solids, is calculated in the carbon model based on deposition of POC and diagenesis reactions in the bed.
5	1	C	Transfer of dispersion output from ECOMSEDZLJS to ST-STEM/RCATOX in terms of change in horizontal segment resolution. Even though the contaminant model grid is a superset of the sediment transport grid, there is a change in effective mixing length between model segments that will require an adjustment of the bulk dispersion rates. Table 2-4 of BIII report and the surrounding text does not indicate that this translation was performed. Was it? Please discuss at this point in the report.

5		R	Bulk dispersion rates computed in the hydrodynamic model were not adjusted for increased mixing length in the carbon and contaminant model. Given the tidal nature, salinity stratification, and grid scale, transport within the LPR portion of the model grid is dominated by velocity terms, rather than bulk dispersion. Solids mass balances for portions of the Passaic River computed within the ECOM-SEDZLJS with and without incorporating dispersion showed differences of only a few percent. The dispersion coefficients output from the ECOM-SEDZLJS model (L^2/T) are converted to bulk dispersion coefficients (L^3/T) prior to aggregation across the collapsed interfaces. Further discussion of the dispersion properties and how they were collapsed from the sediment transport to the organic carbon/contaminant grid will be added to the report to better describe this process.
6	1	C	I would like to make one more point about the model framework that may be in opposition to some other reviewers. I think the organic carbon model has value in this over framework, because the hydrophobic partitioning of the PTS is represented on the basis of organic carbon in the system (see section 2.2.1.1 of BIII) and a sediment transport model with specified OC fractions would, in my opinion, not necessarily produce the correct level of distribution between particulate, freely dissolved, and bound dissolved chemical. Furthermore, the OC model also provides valuable information about redox conditions and sulfate reduction in sediments that are important for metal partitioning to acid-volatile sulfide (important for determining bioavailable fraction) and mercury methylation in sediments.
6		R	No response necessary.
7	2	C	I think that the modelers have made good and appropriate use of all available data in the development of model inputs.
7		R	No response necessary.
8	2	C	I do think that upstream loads are highly uncertain and should have been further investigated in terms of their importance to the systems long-term response to remediation alternatives. One thing that the authors can easily do to convince the reader that their regressions are working reasonably well (or not) is to put the actual measured concentrations and resulting measured loads for all available data points on figures 3-2 through 3-6, which should the time series of computed upstream loads. The same should be done for these loads of contaminants – time series plots of computed daily loads with data points on the plots. Also, it would be useful to conduct an informed sensitivity analysis regarding upstream loads to contaminant response, not just sediments
8		R	The final modeling report will incorporate the reviewer's suggestion to add the TSS data to the time series plots of solids boundary conditions (Figures 3-2 through 3-6) and perform a sensitivity of contaminant response to upstream solids loads. Estimates of the Dundee Dam and Saddle River solids boundary conditions are based on regressions versus flow (Figure 3-1), which incorporated data available at the time the regressions were developed. The resulting loading estimates compared favorably on an annual average basis with those estimated by the USGS (2007). Dundee Dam boundary solids concentrations derived from the rating curves (Figure 3-1) were checked against the TSS estimated from initial optical backscattering (OBS) data obtained in the PWCM program, and that comparison did not indicate a need to modify the boundary solids loading relationships. It is noted that the regressions used to develop boundary conditions show data scattered above and below the regression line (i.e. Figure 3-1 of the sediment transport modeling report), and therefore it is expected that plotting data on time series of boundary conditions will also show data above and below the model boundary conditions.
9	2	C	I may have missed it, but I am not sure how the TSS concentration of upstream load was fractioned into size classes; and was the distribution of size class flow-dependent? Many other systems have shown shifts in particle size distribution with flow in river systems.
9		R	The solids boundary condition at Dundee Dam does include the first size class (cohesive size class), and a fraction of the second size class (250µm) at higher flows. The assignment of the composition is based on a USGS publication (Anderson and Faust, 1973) and sediment trap data collected by Malcolm Pirnie, Inc. in 2008 as part of the FFS. Additional clarification of this relationship with flow will be added to the final version of the sediment transport modeling report.
10	3	C	To me this question refers to the model calibration/corroboration and diagnostic/sensitivity analysis to convince the users that it can support the remediation decision process in the FFS. The discussion here raises issues regarding achievement of this objective. First, I want to make a general comment about the calibration/corroboration process used in this study. In this site-specific model application, it seems as though the hydrodynamic – sediment transport model was calibrated for sediment using TSS and bed sediment properties and then that calibration was unchanged and not revisited during the OC-contaminant calibration process. The transport and characteristics of solids in a system like this has a major effect on the transport and fate of hydrophobic contaminants. And since, as I mention below, the sediment transport calibration is not very good, especially during tidal events, it seems that there should have been an iteration of the sediment transport calibration, effectively using the more hydrophobic contaminants as a solids "tracer". I would ask the authors to justify why this was not done.
10		R	The sediment transport calibration process involved several hundred simulations focused on improving agreement between model results and data for bathymetric changes over time and TSS estimated from acoustic backscattering (ABS) (at a frequency of 12 minutes). Developing an understanding of the sensitivity of the sediment transport model to sediment grainsize distributions led to a decision to adopt the

			geomorphic zone approach to introduce more spatial resolution into the assigned bed composition. Upstream boundary solids derived from the rating curves (Figure 3-1) were checked against the TSS estimated from initial OBS data obtained in the PWCM program, and that comparison did not indicate a need to modify the boundary solids loading relationships. Extensive sensitivity analyses were performed to evaluate settling formulations for cohesive solids. These included a constant settling velocity, the formulations of Farley and Morel (1986) and Winterwerp (1998). The settling formulations were initially evaluated through comparisons to TSS estimated from ABS data obtained by Dr. Chant of Rutgers University in three deployments (one location in each deployment) in 2004 and 2005. Data from the CPG's PWCM program (five locations in the LPR) were used when they became available, and those comparisons resulted in additional modification of the settling formulation. Changes were made to the sediment transport simulation in response to the behavior of the contaminant model in the case of the representation of releases during dredging, which was initially represented in only the carbon and contaminant models, but not the sediment transport calculation. The use of hydrodynamic contaminants as a solids "tracer" as part of the sediment transport calibration is helpful in some settings, but in the case of the FFS, the available water column contaminant data did not suggest a need to revise the sediment transport calibration. The sediment transport modeling report acknowledges that the computed rate of sediment infilling is less than that derived from the 1996-2004 single beam bathymetry surveys and efforts are underway which show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be discussed in the final modeling report. The reviewer's comment that "…the sediment transport calibration is not very good, especially during tidal events…" is addressed in response to the next comment, which refers to specific figures.
11	3	C	In support of the above recommendation, the contaminant model was calibrated only by adjustment of mixing rate in the upper active sediments by reducing it by approximately a factor of 10 relative to what was used in the CARP model (see figures 4-4 and 4-5). This was done in order to keep contaminants in sediments from declining at too fast a rate in the model relative to data. At the same time, it seems that the quality of the sediment calibration is relatively poor, especially the significant over-prediction of peak TSS during the high velocity, high bottom shear points in the tidal cycle (evidenced in figures 4-4, 4-5, 4-8 through 4-14). The model also over-predicts the suspended sediment concentration in the vicinity of the estuary turbidity maximum (ETM) in the river (see figures 4-17 through 4-20). This over-prediction of TSS in the water column is the result of a combination of over-prediction of resuspension rate (potentially based on misinterpretation of SEDFLUME results) or under-prediction of resettling rate for resuspended material or under-prediction of the amount of erosion from the SEDFLUME results that is transported as bedload. The approach of using several intact cores submitted to the standard SEDFLUME analysis in order to characterize the depth-dependent relationship between bottom shear stress and surface sediment erosion is a theoretically reasonable way to formulate this process within the model. However, it seems like the relative distribution of resuspension versus bedload in response to tidal velocity induced shear stresses seems to have failed in forecasting that distribution. This issue does not seem to affect the long-term sediment bathymetry simulation and comparison with data, which seems to be pretty decent. However, the rapid and extensive short-term exchange of surface sediments with the water column can have a very significant effect on contaminant fate and transport both in the water column and surface sediments. It seems to me that the modelers should have revisited this sediment transport issue when calibrating the contaminant model.
11		R	Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. As part of that effort, it is expected that the comparisons between simulated and ABS-derived TSS will improve. In the final modeling report comparisons between model results and TSS estimates derived from ABS will be revised to show uncertainty bounds in the TSS estimates based on the scatter in the relationship between TSS and ABS. As part of the ongoing work, revisions to the sediment erosion parameterization are being evaluated. These efforts are consistent with the reviewer's suggestions, and will be fully documented in the revised modeling report. However, the reviewer may have overstated the importance of errors in the resuspension and deposition parameterizations for some of the mismatches between simulated and observed (estimated) TSS during the PWCM period. Agreement during the Oct 29-Nov 4, 2009 period (Figures 4-1 to 4-5) is, on the whole, quite good, especially in the vicinity of the turbidity maximum (ETM) where local resuspension and deposition dominate. Well upstream of the ETM tidal turbidity patterns are dominated by advection on ebb tide, which the model over-predicts, but this could be because of factors not related to local exchange with the bed. The model also over-predicts relative to the observations during the March-April period shown in Figures 4-8 to 4-12, but as stated in the report, this is most likely because of a temporary overestimation of riverine sources during high runoff prior to this period. Advection from upstream is indicated by a semi-diurnal pattern with maxima at the end of ebb, rather than the quarter-diurnal pattern associated with local resuspension. The quarter-diurnal signature of local resuspension and deposition is apparent in Figures 4-13 and 4-14, where the model under-predicts the data at one station and over-predicts the data at the next station upstream, but captures the essential dynamics of resuspension and deposition well. Finally, while the reviewer is correct that the model over-estimates the magnitude of observed TSS during the March 2010 event, the simulated location and shape of the high TSS values at the mouth of the River indicate reasonable reproduction of local dynamics. The large values may be associated with excess resuspension, but they also may indicate overestimation of loading from a source-limited watershed.

12	3	C	Another point about iteration during the normal steps in the modeling process is regarding sensitivity analysis. Report BIII describes the results of three model sensitivity runs for contaminants. In addition to the three runs that were done, the authors indicated on page 4-2 that they tried increasing the KPOC in attempting to calibrate the contaminant rate of decline in the surface sediments. The results of this work should have been at least reported in the sensitivity analysis section. It is also relevant to the issue on desorption rates mentioned above. But the important point here is that Report BII describes the results of six model sensitivity parameters/inputs on sediment transport, and found that at least four of them had significant impacts on model performance: • Grain size distribution is critical to modeled sediment transport, hence the question I had about how this was determined in the upstream load; • Upstream boundary loads was next in terms of model response to its adjustment; • Cohesive settling velocity seems to have a big impact on net erosion; and • Downstream boundary conditions have a big impact on water column fluxes up to RM 5.2, hence my concern about the importance of wind-driven resuspension in NB. My criticism is that these sensitive model parameters/inputs were not carried to the contaminant model for evaluation of their impact on contaminant fate and transport. In my opinion, this is a critical missing analysis in the model application relative to its accuracy of system response to remediation alternatives.
12		R	Sensitivity analyses did not include propagating results from one model to the next because of computational constraints. Refer to the response to comment 10 regarding the specific sediment transport sensitivities mentioned in this comment. Despite the issues with run duration, a number of sensitivity runs will be completed with revised model parameterization, including scenarios that are passed between the models (e.g. upstream solids loads), and the results of these will be included in the final modeling report.
13	3	C	Finally, I think that a very important missing model diagnostic analysis that is invaluable in interpreting and judging the model's credibility for projecting the system's response to remedial alternatives is a space and time-specific mass balance analysis. The mass balance diagnostic helps the reader/user identify the relative importance of sources and sinks of contaminants to certain areas of the remediated zone of the river during various time periods of the remediation scenarios. I realize that the last section of the BIII report (section 6-4) provides cumulative contaminant fluxes across several transects of the lower river over time, and that analysis proved very instructive relative to the system's response. But I think a much more instructive and illustrative analysis would have been to develop a full model-computed mass balance diagram (all inputs and outputs and change in control volume mass) for the river segments between those transects at several points in time (or over several specified time intervals) of the remediation and post-remediation simulations. If indeed, the remediation trends shown in figures 6-3 and 6-11 (for examples) are correct, one could understand why the sediments are not being recontaminated at all following remediation. I urge the modelers to conduct these diagnostics to convince themselves of the accuracy of the remediation scenario forecasts.
13		R	It is agreed that there is a great deal of value in examining the mass balance computed by the model in both space and time. These mass balances were developed during the process of model calibration, but the decision was made to include time-series plots of cumulative fluxes (Figures 6-27 to 6-34) in the report to facilitate comparisons among remedial alternatives and across representative contaminants. The rationale for the selecting the cumulative flux format is that it allows the interested reader the opportunity to evaluate contaminant transport over any time period. Additional mass balance summary information, as suggested by the reviewer, will be included in the final modeling report.
14	4	C	I am not convinced, given the great mobility of surface sediments in this system and propensity for downstream transport of sediments in the RM 17-8 reach, that figures 6-3, 6-11, 6-19 should virtually no recontamination over the roughly 45 years following initiation of remedial actions. It may be that recontamination from both upstream and downstream boundaries and from ongoing external loads (CSO's, atmosphere, etc.) is masked the fact that these plots represent a six or eight mile average over the top 15 cm of sediment, but given that two of the remedies get the sediments to virtually zero I find this implausible. The mass balance diagnostic analysis (discussed in question 3) applied to smaller segments of the river and only the top couple centimeters of sediments (which are responsible for water column exposure) should be looked at to better evaluate recontamination.
14		R	Based on the comments provided during this peer review, the sediment transport model is being modified to compute greater levels of infilling within the River. It is anticipated that this will result in a greater level of recontamination after implementation of each of the remedial alternatives. In addition to the changes to the sediment transport model, the additional mass balance outputs discussed above, along with time series graphics of the response of the Study Area broken out into various regions (channel, shoals, depositional, erosional, neutral, and top one cm), should provide greater perspective into the behavior of the model.

15	4	C	The evaluation of recontamination should also be conducted as part of the sensitivity analysis (including both sediment model and contaminant model parameter/input variations), to project the level of recontamination change if certain inputs are over- or under-estimated. Also, the potential for recontamination from northern NB should be investigated. As shown in figures 4-1 through 4-3, the sediment concentrations in northern NB are equivalent to the lower 8 miles of the Lower Passaic River at the start of the remediation runs. It seems to me that there would be a significant potential for recontamination via tidal pumping from wind- and ship-driven resuspension of these sediments. Also, the cumulative net mass transport increase after remediation (see fig 6-30 through 6-34 for various transects) shows a relatively monotonic increase (except for the abrupt jump in 2037) presumably from upstream (both upstream load and movement of sediments from RM 17-8). It is hard to understand that virtually all of this transport is just passing through the lower 8 miles and out to NB with some of it depositing in the remediated zone.
15		R	An analysis somewhat along the lines as suggested by the reviewer was considered to address uncertainty. In that analysis, rather than fixed percentage perturbations of model inputs (e.g. +/- 20 percent) changes to inputs to reflect uncertainty would be assigned, and model results compared to data so as not to include parameter ranges that clearly contradict data. The time required to execute the selected simulations made that approach impracticable. Additional analyses will be performed to explore the sensitivity of the model to input parameters, including simulations propagated through the models (e.g. upstream solids loads). It is anticipated that the contaminant response discussed by the reviewer will change in upcoming simulations that will be based on revised sediment transport simulations.
16	5	C	I think the answer to this question is that extreme events do have an impact -- see the impact of the April, 2007 event (repeated in 2022, 2037) (figures 6-3, 6-11, 6-19 and 6-30 through 6-34), which I think is only about half the flow of a 100-year return period event. Therefore, I think that the modelers should have inserted a 100-year event into the remediation scenario input file to evaluate the duration and spatial extent of such an event for the different remediation alternatives. These very high flow events will generate significantly higher load of solids from upstream, likely cleaner solids than what is in surface sediments under baseline or early MNR conditions (but maybe not cleaner relative to a sediment sand cap). Of course, the events will also generate high resuspension rates. So, higher flows will likely lead to an overall higher rate of exchange of surface sediments (as we move through the hydrograph) with the overlying water, and depending on the relative upstream chemical concentration on solids versus the surface sediment initial conditions at the beginning of the event, there will be a potentially significant change in surface sediment chemical exposure concentrations. It is important for the full model to be used to generate the net system response to extreme events.
16		R	The impact of storm events will be further incorporated into the modeling effort by adding the 2011 and 2012 water years at the end of the calibration period. This period includes hurricane Irene, which was a 1 in 75-year return event on the Passaic River. In addition, the sensitivity to a 1 in 100-year return event including three subsequent years will be added to the analysis to determine the impact of an extreme event on the computed trajectories under the various remedial alternatives.
17	4,5	C	The scope and detail of the multiple modeling efforts on the New York – New Jersey harbor complex are quite impressive. The FFS study here is just one part of this large, ongoing effort. I'm confident that the modelers have learned a lot about the system dynamics and can offer the decision-makers useful advice and valuable perspectives about the possible uncertainties involved in the management scenarios.
17		R	No response necessary.
18	4,5	C	Can the model be trusted enough to compare the relative effects that implementing each remedial alternative will have on FFS Study Area? Despite some reservations, I believe the model can indeed distinguish between the MNR alternative and the two more extensive remedial alternatives – Deep Dredging and Full Cap.
18		R	Additional analyses are being performed to address the source of the reviewer's reservations. These are described below in response to specific topics raised by the reviewer.
19	4,5	C	My judgment is that over the next three decades, the LPR would see somewhat lower contamination levels than predicted by the MNR, and at least a bit higher levels than predicted by the Deep Dredging and Full Cap. The MNR prediction might underestimate recovery because infrequent large events not simulated could more efficiently flush out existing contaminants and bring in a significant load of (relatively) clean solids. In addition, some slow chemical and biochemical loss processes were not included, and could work over long periods to attenuate concentrations, at least marginally.
19		R	The reviewer's expectation that contaminant concentrations would decline over the next three decades to levels "somewhat" lower than calculated in the MNR simulation could be correct if "somewhat" is intended to mean only a small difference. Data analyses performed to evaluate temporal changes in sediment contaminant levels showed no statistically significant changes in 2,3,7,8-TCDD concentrations (geometric mean) between 1995 and 2010 in reaches RM0-2 and RM2-8. In addition 2,3,7,8-TCDD concentrations in biota do not show a consistent decline between 1999 and 2010. Concentrations in white perch and blue crab in 2010 appear to be comparable to 1999-2000 levels. In contrast, levels in mummichog collected in 2010 appear to be lower than in the samples collected ten years earlier. The MNR simulation does include infrequent large-flow events, including two with daily average flows at Little falls of 15,500 cfs or more (April 2007 and March 2010) and hurricane/tropical storm Floyd, which resulted

			in a flow of 11,300 cfs in September 1999. Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report. These additional efforts include extending the calibration years to encompass the period of hurricane Irene in August 2011. It is acknowledged that concentrations computed in the Deep Dredging alternative are likely biased low because the computed degree of infilling tends to result in less recontamination than would likely occur in response to the increased water depths. The Full Capping alternative is affected by this bias to a much lesser extent, and understanding these biases, it was judged that the models could be used to distinguish among the alternatives. It was judged that slow chemical and biochemical loss processes could be neglected given that risk levels are orders of magnitude above safe levels and these slow processes would only have marginal effects that would result in only an insignificant reduction in calculated risk.
20	1,3	C	There is some indication that the active mixing layer is somewhat less than 10 cm. If so, then contaminants in the upper layer would escape more rapidly than simulated. It is not clear whether the 15 cm average concentration would recover more quickly, however. Finally, the calibration data seem to show a recent decline in many locations not captured by the model. The model calibration included a midpoint upstream “reset” to counter low initial conditions. If the initial conditions were set properly and the model were recalibrated to capture the 15 year calibration trends, there is a chance that it would show more rapid MNR recovery.
20		R	Particle mixing within the top 10 cm of the sediment bed can be thought of as representing the effects of bioturbation and sub-grid scale variations in erosion and deposition. Sediments are mixed at a relatively slow rate resulting in gradients over the top 10 cm, rather than a 10-cm completely-mixed layer. With a shallower depth of mixing, the surface sediments would respond more quickly, and the remainder of the top 15 cm would respond more slowly. The impact on the 15 cm average will be addressed in the in the final modeling report by completing an additional sensitivity simulation with a shallower depth of mixing. In response to the reviewer’s comment regarding a recent decline in contaminant concentrations, it is important to recognize that different sampling programs over the 15-year period had different spatial extents and objectives and widely varying numbers of samples. Evaluating temporal changes based on the 2008 and 2010 datasets is difficult because of the different sampling designs employed in those programs. The 2008 data were generally regularly spaced and included a large number of samples within the historically dredged channel, while the 2009 and 2010 benthic datasets targeted only shoals, and both datasets include a great deal of variability about the mean. The reviewer’s comment that ‘The model calibration included a midpoint upstream “reset” to counter low initial conditions.’ refers to the portion of the LPR upstream of RM7 where data were too sparse to use for assignment of sediment contaminant initial conditions for 1995. The only dataset with adequate coverage above RM7 was the 2008 Low Resolution Coring (LRC) data. Initial conditions upstream of RM7, therefore, were based on data from the CPG 2008 LRC program, along with the sparse data from 1995 and data collected by the CPG in 2011 from the mudflat near RM10.9. When the model simulation reached 2008, computed concentrations in the area where 2008 data were used to assign 1995 initial conditions were replaced with concentrations based on the 2008 data. If the initial conditions upstream had been increased so that the model simulation results for 2008 matched the 2008 data, the direction of the change on the MNR recovery would be a slower recovery, not a more rapid recovery, although the magnitude of the change would not be expected to be large. This is because the reach average concentration above RM8 does not decrease substantially between the initial condition and 2008. However, in response to the comments of the peer reviewers, revised model runs will include an initial spin-up of the contaminant concentrations for the period from 1995 through 2008. The 1995 initial sediment bed concentrations will then be scaled-up to values that will, on average, decline to the 2008 values in 2008. The model will then be re-started from 1995 using the scaled up initial conditions. This approach to setting the initial conditions in the sediment bed will only be used in areas where the initial conditions are based on the 2008 data. The spin-up simulation will also be used as the basis for introducing a vertical gradient in contaminant concentrations in the top 15 cm of the bed throughout the model domain (the vast majority of data used to assign initial concentrations are from 6-inch [15.4cm] samples). The vertical gradients in contaminant concentrations at the end of the spin-up will be scaled so that the average concentration in the top 15 cm of the bed will have the same average concentration as the 1995 initial condition, which was initially assigned as a vertically uniform value in the top 15 cm. After these revisions to the October 1995 initial conditions the model will be run through September 2059 without any “resets” in concentrations.
21	4	C	the treatment alternatives might underestimate recontamination for a couple of reasons. First, large events not simulated might bring in a significant load of contaminated solids (relative to the clean caps). Second, the model appears not to include partitioning to noncohesive solids, which constitute the clean sand capping. In reality, some contaminant levels are expected to diffuse within particle pores, and some carbon is expected to build up on noncohesive surfaces, capturing more contaminant. The result is a low but not insignificant partition coefficient for the caps.
21		R	Revised model simulations will incorporate higher rates of infilling, resulting in a more widespread buildup of cohesive solids and associated carbon on top of the capped areas. In addition, the model inputs will be revised to include a small fraction of cohesive solids in the specification of the composition of the capping material to represent a low, but non-zero, foc of the cap.

22	3,4	C	I expect that LPR would experience significantly lower contamination levels following Deep Dredging and Full Cap than it would under MNR. It's a little less clear how much difference to expect in the long run between MNR and Focussed Capping, or between Focussed Capping and the more extensive alternatives. My judgment is that the water and sediment quality for Focussed *[sic]* Capping would indeed be better than MNR and worse than either Deep Dredging and Full Cap. Given the large spatial and temporal variability, however, the improvements might be difficult to measure. Since only one alternative will be chosen, of course, we can never know exactly how the others might have played out.
22		R	No response necessary.
23	1,5	C	The managers should understand the large degree of uncertainty in model predictions in a complicated system such as this. I wish more diagnostic simulations had been run for better understanding. Because of the size and complexity of the models used here, it was difficult to impossible to run enough sensitivity alternatives to fully estimate the level of uncertainty. Some modelers prefer simpler models that can be parameterized and run thousands of times in an effort to establish uncertainty bounds. These simpler models, however, can be biased by limited data and may not adequately capture key processes. They are subject to more peer review criticism from scientific and modeling experts. I cannot fault the choice of the complex models for this study. Indeed, many of our peer review comments would lead to more complexity in network and process detail.
23		R	In the case of the FFS the alternatives have been modeled using both a simpler model discussed in Appendix C (Empirical Mass Balance – previously peer reviewed in 2008) and this more complex set of mechanistic models. In addition, an analysis similar to that done by Connolly and Tonelli (1985) will be conducted to estimate the degree of uncertainty in the final model results.
24	1	C	Overall, I believe that the processes incorporated into the sediment, organic carbon, and toxicant models are appropriate and justified. Specific questions and comments follow for each module. Under physical processes, I will include the grid resolution, sediment feedback to hydrogeometry, sediment class representation. I will assume that hydrodynamics is simulated adequately.
24		R	No response necessary.
25	1	C	Sediment transport – The physical processes in model seem adequate. The model grid includes 10 water column layers and 10 active bed layers, with width varying from 4 to 3 to 2 cells going upstream. A more refined grid (4 times finer resolution) was tested, and shown to give only minor improvements at a cost of 8 times longer computations. Bed elevations are modified once a year and fed back to the hydrodynamic model. This seems adequate for comparing alternatives. The sediment fractions are divided into one cohesive class and 3 noncohesive classes. The cohesive class is functionally divided into two subgroups using empirical functions. The sediment bed is divided into parent bed and deposited layers. The parent bed retains measured properties, such as bulk density. For deposited layers, bulk density approaches equilibrium value at first---order consolidation rate. The sediment transport processes include settling, deposition, resuspension, bed load, and consolidation.
25		R	No response necessary, except to note that in final model simulations, bathymetry will be updated in the hydrodynamic model at a much higher frequency (on the scale of seconds to minutes, depending on the time step).
26	1	C	Bottom shear stress is divided into form drag and grain stress. The total roughness in the hydro model is constant, but the sediment model calculates the bedform roughness using a van Rijn formulation (function of d50, tau_s, and tau_ce). This predicts bedform (mini ripples, mega ripples, dunes), which reduce the grain stress by up to 3 times. This approach seems justified.
26		R	No response necessary.
27	1	C	Erosion of cohesive bed uses a nonlinear table of erosion velocities versus applied shear stress values (grain stress, I assume). Erosion is linear between points in the table. The table function is derived from experiments with intact cores using SEDFLUME. It seems to me that the modeling approach for this process is defensible given good experimental data.
27		R	No response necessary.

28	1	C	Bed consolidation follows Sanford 2008. The parent bed retains measured properties, such as bulk density (sand = 1.92). For deposited layers, bulk density approaches equilibrium value at first-order consolidation rate. This seems reasonable, given defensible experimental data.
28		R	This interpretation is correct. The model bulk density used for the depositional layers is used in conjunction with a formulation of erosion rate based both on applied grain stress and bulk density. In this case a greater focus was placed on matching erosion rates than measured bulk densities.
29	1	C	Bedload equations are applied to noncohesive particles. The fraction of eroded particles transported as bedload is a function of grain diameter, density, and fall velocity. The rest is added to the lower water column to be transported in suspension. All cohesives are transported in suspension. The processes here seem reasonable to me.
29		R	In addition to the parameters mentioned by the reviewer, the fraction of the eroded non-cohesive particles transported as bedload is also a function of the shear velocity and critical shear stress for erosion.
30	1	C	Noncohesive settling is calculated by particle size class. For the single cohesive class, an empirical function of TSS is used for settling. The slow background settling rate is 0.2 mm/sec (17 m/day). Aggregates settle as a function of TSS: min (3*TSS/260, 3) mm/sec. This gives a minimum settling rate of 260 m/day. This function seems like an acceptable compromise to me, given reasonable empirical data. I defer my judgment on this, however, to my fellow reviewer, Dr. Lick, who has more expertise in this area.
30		R	The function mentioned by the reviewer for the settling velocity for cohesive aggregates, includes the term “3*TSS/200”, rather than “3*TSS/260”; the reviewer correctly states that this gives a maximum settling velocity of 260 m/d (3 mm/s).
31	1	C	Deposition probability follows Krone for cohesives and Gessler for noncohesives. These seem fully adequate given reasonable input parameters. For cohesives, tau_cd = 0.5 dy/cm2
31		R	No response necessary.
32	1	C	The organic chemical model ST-SWEM includes a water column module and a sediment diagenesis module. The physical processes in model seem adequate:
32		R	No response necessary.
33	1	C	The model grid includes 10 water column layers and 3 bed layers, with width varying from 4 to 3 to 2 cells going upstream (preserved from hydro and sediment models). In the longitudinal direction, the cells were aggregated 2 to 1 or 3 to 1 to reduce computational burdens. The length aggregation seems reasonable to me.
33		R	No response necessary.
34	1	C	The sediment bed is divided into a thin fluff layer (for tidal deposition and resuspension), an active layer of about 10 cm (biological mixing), and an archive layer. The active layer is subdivided into aerobic and anaerobic zones for reaction rates. Specifying a biologically active layer depth of 10 cm throughout the model domain (from mudflats with fine silt to channels of coarse sand) is questionable, but perhaps the available data do not allow better definition.
34		R	In the case of the carbon model, the fluff layer is part of the active layer, which can range from 9.9 to 10.1 cm in thickness. This active layer is comprised of aerobic and anaerobic zones for purposes of the diagenesis calculations, with biological mixing between the two zones. Within the carbon model the particle mixing rate is a function of temperature and the net flux of organic carbon to the sediment, which is in turn tied to the composition of the solids depositing within a given area. The simplified approach used in the contaminant model does not account for spatial or temporal variability in mixing rates, however the rate used is considerably lower than that used in the carbon model. Unfortunately, sufficient data are not available to better constrain the sediment mixing processes and input spatially variable mixing rates. Refer to response to comment 108 for a discussion of the depth of mixing and sensitivity simulations planned to evaluate the effect of a shallower depth of mixing in the LPR.
35	1	C	The toxicant model active layer is subdivided into 1-cm cells, and the archival layer is set to 97 cm, and subdivided into 1-cm cells. I assume the same is done with ST-SWEM, but it is not clear from the documentation. The toxicant model also has a deep bed archival layer of 0.61 cm. Since this is not mentioned, I assume ST-SWEM does not include that layer.

35		R	The sediment bed in the contaminant model is subdivided into ten active 1-cm thick layers with mixing between the layers, ninety-six 1-cm thick archive layers (if present), and a final archive layer at the bottom which can be any multiple of 1 cm (if present). The information passed from the carbon model and used by the contaminant model for the hydrophobic organic contaminants includes the bed elevation change, average POC in the active layer, and average POC in the archive layer. The metals model also uses the sulfate reduction rate estimated from the sediment diagenesis model outputs. The sediment layering from the carbon is not passed to the contaminant model. The carbon model has a single archive layer beneath the active layer with an initial thickness of 3.1 meters, which can either shrink with erosion or grow with deposition. The contaminant model archive layer is assumed to have the same organic carbon composition as the carbon model archive layer.
36	1	C	The carbon fractions in the sediment diagenesis module are divided into three “G- class” state variables representing labile POC (G1), refractory POC (G2), and inert POC (G3). Variables are referred to as SG1C, SG2C, and SG3C. DOC is not represented in the sediment bed. For theoretical completeness, it seems to me that sediment DOC should be a product of G1 breakdown and exchange with surface water. Apparently sediment layer DOC concentrations were provided to the toxicant model using empirical data. *** If this is not the case, and DOC is not specified for benthic layers in the toxicant model, then sediment-water transport for some contaminants would be underestimated, particularly loss from deeper layers that are infrequently eroded. ***
36		R	The original diagenesis model did not include DOC (or DON or DOP) as state variables. The datasets from the Chesapeake Bay and the Marine Ecosystems Research Laboratory (MERL) at the university of Rhode Island, which were used in the development of the diagenesis model, did not include measurements of DOC, DON, or DOP. The effort to add these dissolved species as state variables to the diagenesis model was not considered for the FFS. The importance of DOC as a complexing phase for hydrophobic contaminants is recognized, and therefore, in both the CARP project and the FFS, sediment pore water DOC has been specified empirically as a constant 7.5 mg/L. The contaminant model does include mixing of freely-dissolved and DOC-bound contaminant in the pore water within the active layer (top ~10 cm), but it only tracks total concentration within the archive layers (below top 10 cm). The model does not account for transport of contaminant in the pore water within the archive layers. Although this process may be present, it is not likely to be significant due to the combination of the relatively low pore volume and DOC concentrations compared to POC paired with relatively high partition coefficients.
37	1	C	The carbon fractions in the water column are divided into ten state variables: refractory POC and DOC (RPOC, RDOC), labile POC and DOC (LPOC, LDOC), reactive DOC (ReDOC from CSO loadings), algal exudate DOC (ExDOC), three resuspended sediment POC classes (SG1C, SG2C, SG3C mapped from the diagenesis module), and inert POC (IPOC mapped from cohesive TSS in the sediment model). Looking at the detailed water column carbon reactions described in Table 2-2, it seems clear that resuspension SG1C is a source term for LPOC, as is a portion of algal grazing. (*** why is SG1C a separate state variable in the water column? ***). Similarly, resuspension of SG2C is a source term for RPOC, along with a portion of algal grazing (*** why is SG2C a separate state variable in the water column? ***). Likewise, resuspension of SG3C is a source term for IPOC (*** why is SG3C a separate state variable in the water column? ***). The reaction coefficient table (2-2) shows a fraction of algal grazing going to IPOC (0.025), but the equation for IPOC does not include a corresponding term (*** I assume this was just an omission in the documentation, since summary Table 2-3 includes the algal source to IPOC ***). Labile POC decays into labile DOC, while refractory POC decays into refractory DOC. Labile and refractory DOC can be aerobically oxidized into CO2. Labile DOC can also be consumed by anaerobic denitrification and lost (in the document, the theta term needs the exponent “T-20”). The reactive and algal exudate DOC can be aerobically oxidized to CO2. The water column carbon reactions are reasonable and well justified.
37		R	The ST-SWEM code was originally developed such that the resuspended POC could settle at a faster rate than the POC originating from external sources and internal algal production. In order to accomplish this, additional state variables (SG1C, SG2C, and SG3C) were added to the code. The separate settling velocity has not been implemented in the FFS model, and therefore the pairs of POC variables behave identically (IPOC=SG3C, RPOC=SG2C, and LPOC=SG1C). The same is also true for the other particulate organic variables (nitrogen, phosphorus, and silica). The text will be modified to make this clearer. The comments about the reaction rate equations are both correct. Tables 2-2 and 2-3 will be edited to include the terms noted in the IPOC and LDOC reactions.
38	1	C	Carbon production comes from the previous SWEM eutrophication model, with its 24 state variables, including two phytoplankton groups (winter diatoms, summer flagellates). This is a reasonable representation. This module provides more carbon pools than is necessary for contaminant fate modeling, but its use seems reasonable and well-justified to me.
38		R	As noted above, the additional carbon variables were incorporated to allow the resuspended particles to be tracked separately from those derived from the water column, although this capability has not been implemented on this project.

39	1	C	Primary production and resulting POC is handled in ST-SWEM, but not in the sediment transport module ECOMSEDZLJS, which feeds it. *** This is OK unless algae and detritus are a significant fraction of cohesive solids in this system. *** ST- SWEM does not simulate water column noncohesive solids, which is acceptable. ST- SWEM includes noncohesive solids in the bed as a single aggregated class.
39		R	POC loading from the boundaries, which is included in the boundary conditions for the sediment transport model, is much greater than algal carbon generated through primary production within the model domain. The final modeling report will include a quantification of the carbon inputs from different sources.
40	1	C	Initial ST-SWEM bed composition is transferred from the ECOMSEDZLJS initial mass of cohesive and non-cohesive solids in the top 10 cm (active layer) and the remainder of the bed (archive layer). ST-SWEM then simulates the evolution of the bed using settling, resuspension, and burial/erosion velocities derived from ECOMSEDZLJS. *** This approach is practical and justified as long as primary production, which is not represented in ECOMSEDZLJS, is not a significant fraction of the cohesive solids. ***
40		R	The reviewer's description of the linkage between the sediment transport model, ECOMSEDZLJS, and the carbon model, ST-SWEM, is correct. Refer to the response to comment 39 regarding the role of primary production as a solids source.
41	1	C	ST-SWEM cohesive settling velocities are calculated from flux-weighted ECOMSEDZLJS cohesive settling velocities. These are used in ST-SWEM for RPOC, LPOC, and IPOC. This procedure is justified.
41		R	No response necessary.
42	1	C	ST-SWEM resuspension velocities are calculated from ECOMSEDZLJS spatially aggregated fluxes and concentrations (i.e., flux/conc). These velocities (not mass fluxes) are applied to ST-SWEM sediment organic and inorganic variables. This approach is reasonable and justified.
42		R	No response necessary.
43	1	C	ST-SWEM burial/erosion exchanges between the active and archive layers are calculated from area-weighted ECOMSEDZLJS bulk densities and deposition and resuspension fluxes. This is done by calculating the change in the active layer thickness as flux/density. This approach is reasonable and justified.
43		R	No response necessary.
44	1	C	The original SWEM used constant sediment burial rates (about 2.5 cm/yr), then varied net deposition rates calibrated to observed SOD and POM. The linkage here of ST-SWEM to ECOMSEDZLJS is a nice improvement.
44		R	No response necessary.
45	1	C	RCATOX is used to simulate contaminants. Information is passed to RCATOX from the other modules using large transfer files. Hydrodynamic information is passed along from ECOMSEDZLJS at 1-hour intervals. *** This seems justified as long as the hourly information is interpolated down to the RCATOX time step. *** Sediment transfer and carbon information is passed along from ST-SWEM at 15-minute intervals, which seems reasonable.
45		R	Hydrodynamic information is passed as hourly average flows and exchange coefficients. Those values are constant over the hour. Volume is passed as an initial volume at the beginning of the hour and a rate of change in volume with respect to time. The volume of each cell at any time is interpolated to the ST-SWEM or RCATOX time step.
46	1	C	The partitioning processes in RCATOX are conventional and well-justified. Three phases are simulated – dissolved, DOC-complexed, and POC-complexed. *** As mentioned above, it is not clear how DOC in the sediment bed is determined. *** It is not clear how partition coefficients to noncohesive solids in the bed are handled, if at all. ***Are noncohesive partition coefficients assumed to be 0? A low amount of sorption to fine sands could raise the levels of recontamination of sand caps used in the treatment alternatives. ***
46		R	As discussed in response to comment 36, DOC has been specified as a constant 7.5 mg/L. In the FFS model, there is no partitioning to non-cohesive solids either in the water column or in the sediment bed. Future model simulations will incorporate a small fraction of cohesive material into the capping material, with an associated foc so that the overall capping material foc at the time of placement is 0.1%. This will allow for sorption of contaminant to the CAP material, but the small value would not result in large amount of cap recontamination. Recontamination in future simulations will likely be related to the extent of cohesive deposition on top of the cap material.

47	1	C	Volatilization exchange with the atmosphere is described in the CARP report. This exchange process is mathematically split between loss flux and forward diffusive loading flux. This approach is ok if the calculation of gaseous loading velocity is consistent with the loss velocity. The CARP report gives the equations used for the volatilization loss velocity, but does not document the equations used to externally calculate the corresponding forward gaseous loading. *** I assume consistency, but the modelers should confirm. ***
47		R	As the comment states the transfer of contaminants across the air-water interface has been split between the loading from the atmosphere to the surface waters of the model and volatilization from the surface waters of the model to the atmosphere. In the calculation of the gas-phase loads to the model, the water temperature and wind speed used in the "2-film" model calculation are average values that do not vary in time. For the volatilization portion of the calculation, the temperature and water velocity are time variable values, and the gas-phase exchange coefficient is a constant value of 100 m/day. With the different values used for the two sides of the calculation, the resulting exchange velocity computed for the load is generally greater than the average of the values used for volatilization. Although this does introduce an inconsistency between the two halves of the computation of the gas-phase exchange, it is not likely to have a noticeable impact on model results. For the hydrophobic organic chemicals (HOCs), the gradient driving gas-phase exchange is such that the free dissolved concentration is much greater than the air concentration divided by the unitless Henry's constant, resulting in a much larger volatilization flux than gas-phase load. The gas-phase load is only a portion of the total atmospheric load, which also includes wet and dry deposition. The contribution of the deposition term fluctuates, varying from about 25% to nearly the entire load. As indicated above, volatilization of HOCs is much larger than gas phase loading, however, this is not the case for Mercury, and therefore the air-water exchange for mercury was represented differently than for the HOCs. The atmospheric load for mercury input into the model was the sum of wet and dry deposition and did not include the gas-phase load. In this case the gas-phase atmospheric concentration of mercury was specified as a constant value of 3 ng/m^3 (Tseng et al., 2003) and the net gas-phase exchange was computed within the model simulation. Upon further review of the draft report and the other reference documents, this point was not noted. The final modeling report will include text to make this clear. As a whole, the total atmospheric load and volatilization are both much smaller than the advective, resuspension, and depositional fluxes across all contaminants.
48	1	C	Chemical and biochemical degradation processes are not used for this study. Given the nature of the chemicals, this seems reasonable to me. For the hydrophobic organic compounds like dioxin and PCBs, this is reasonable.
48		R	No response necessary.
49	1	C	Mercury kinetics are not described in the FFS documents, but the background CARP document provides a brief explanation of the mercury components and transformation processes included in the model. Total mercury (HgT) was divided into divalent (HgII) and methyl mercury (MeHg) components. HgII and MeHg were simulated explicitly, but elemental mercury (Hg0) was specified as a fixed fraction (10%) of dissolved HgT based on professional judgment (but no local data). Transformation and transfer processes in CARP include methylation and demethylation in the water column and sediment bed, and volatilization. Oxidation and reduction, which link elemental mercury to the predominant forms of HgII and MeHg, are not simulated. In my opinion, excluding redox kinetics and treating Hg0 simplistically is an unnecessary weakness in a model with so much other process detail. Treating Hg0 explicitly with oxidation and reduction has been part of accepted mercury modeling practice since the mid to late 1990s, and studies of redox kinetics have improved the state of the art in the subsequent decade. The actual loss flux of Hg0 from the water column to the atmosphere may be controlled by the oxidation rate supplying Hg0 rather than the faster volatilization rate depleting Hg0. Specifying that Hg0 is 10% of the dissolved HgT essentially parameterizes the net oxidation/reduction rate at 10% of the volatilization loss rate and reduces mercury evasion flux to 10% of the potential loss. In many systems, mercury is only lost through advective export, sediment burial, and atmospheric evasion. In the LPR, with its sediments in rough equilibrium, burial loss is probably negligible and some fraction of the mercury advected out to Newark Bay is returned in bottom waters. It is not clear whether the slow evasion loss in the LPR can be a significant fraction of net advection loss over a long period of time. Evasion loss would affect the MNR scenario (with the higher HgT concentrations) more than the treatment scenarios. Sensitivity runs increasing the Hg0 fraction could address this uncertainty, although the relatively large uncertainties in future atmospheric mercury loading fluxes probably overwhelm the uncertainties in evasion loss fluxes. Given that mercury is only one pollutant of concern in the LPR, this probably would not significantly affect the final choice of treatment alternatives.

49		R	The text for the final modeling report will be revised to supersede the CARP model report version of the mercury kinetics with the updated version used for the Harbor Estuary Program (HEP) Mercury TMDL work. In the final modeling report, the memo to EPA detailing the additional mercury model development, the review comments for that work, and the responses will be referenced and attached. The approach used to model Hg(0) and its volatilization was developed during the CARP project. Although Hg(0) was included in the model it was included as an empirical value set as 10% of the sum of the dissolved mercury species. This was done instead of simulating the reduction and oxidation explicitly. This estimate was consistent with values measured in Long Island Sound and New York/New Jersey Harbor by Dr. William Fitzgerald's group at UCONN, Much of this data falls outside of the FFS Study Area in adjacent water bodies. Discussions with Dr. Fitzgerald and the rest of the CARP MEG brought consensus that transformation to and from Hg(0) was not where the CARP modeling team should focus resources. The Hg(0) fraction in the model, set as 10% of the sum of the dissolved mercury species, can be lost due to volatilization, which is not a significant loss term for mercury in the LPR application. The loss due to volatilization is orders of magnitude lower than the mass transported by advection, resuspension or deposition.
50	1	C	The sediment bed is divided into an active layer of about 10 cm, an archive layer of 97 cm, and a deep archive layer of 61 cm. The active and archive layers are subdivided into 1-cm cells. The surface cell varies between 0.5 and 2 cm. The other active and archive cells are maintained at 1 cm thickness. With erosion, the cell contaminant masses are moved upward, and the thickness of the deep bed is reduced, maintaining the total structure of 107 cells. With deposition, the cell contaminant masses are moved downward, and the thickness of the deep bed is increased, again maintaining the total structure of 107 cells. This approach is very reasonable, and is quite similar to the approach in EPA's version of WASP.
50		R	No response necessary.
51	1	C	It is not clear how or whether the solids composition of the cells within the active and archive layers change with erosion and deposition. The solids composition would come from ST-SWEM, and it is not clear whether that model is divided into 1-cm cells with variable solids.
51		R	The carbon model computes carbon and solids concentrations for two layers within the sediment-bed: the active top ~10 cm and the remainder of the bed, which is referred to as the archive layer. It is assumed that neither the total POC nor the total solids are varying greatly with depth within those layers. Below the active layer, only the total contaminant concentration is tracked with time, and the POC and solids concentrations are only used to compute contaminant concentrations on the basis of mass of contaminant per mass of POC or mass of contaminant per mass of solids. Within the active layer, it is assumed that the carbon concentrations would be fairly consistent in time and with depth and dominated by the inert fraction, and therefore, the single value used should not adversely impact the computed model results.
52	2	C	Overall, I believe that model setup and calibration used appropriate data sets to adequately define parameters, forcing functions, and initial conditions. An exception is the upstream contaminant initial conditions, which were apparently set too low, and had to be reset during calibration. This is discussed more under question 3 below. Specific comments follow for each module.
52		R	Refer to the response to comment 20 for a discussion of the approach used to address the very sparse data available for computing initial conditions upstream of RM7, and the alternate plans for the final model runs.
53	2	C	For freshwater boundary concentrations, the model uses a two-phase log-log empirical correlation of TSS to flow. This seems good enough.
53		R	No response necessary.
54	2	C	For the tidal boundary concentrations, two empirical functions were derived. For the period before dredging, TSS is fit to a nonlinear function of depth and velocity. After dredging, TSS is fit to a nonlinear function of velocity and tidal range, divided into accelerating and decelerating phases of the tide. Both of these functions seem well enough justified.
54		R	No response necessary.
55	2	C	For initial bed sediment conditions, seven morphological features in the LPR were identified and mapped. In-situ data were used to define 4 solids size classes (silt, fine sand, coarse sand, and gravel). Mapping characterized average fractions for each class within contiguous morphological regions. This seems like a reasonable modeling approximation.

55		R	No response necessary.
56	2	C	Important parameters, such as critical shear stress for erosion, were derived from experimental apparatus, including Sedflume and Gust Microcosm. Consolidated sediment tests were run as well as field cores. These tests were used to parameterize both parent bed and deposited layers. The data indicate high variability in replicates, sometimes over an order of magnitude in measured erosion. There were some inconsistencies in measured properties between consolidated sediment tests and field cores potentially indicating that deposited material erodes more slowly than parent bed. In these cases, data were chosen so that the modeled depositional layers are consistent with the parent bed. This seems reasonable. The data analysis procedures used to capture appropriate central tendencies and ranges seem thorough, as good as possible under circumstances of high variability. It is conceded that due to spatial averaging over cells and over size classes, the model will never capture all the variability of the real system. The parameterization inevitably introduces a good bit of uncertainty which must be taken into account in the modeling analysis.
56		R	This approach will be modified due to concerns about infilling rates raised by both the modeling team and a number of the reviewers. Although the approach captured the central tendency of the Sedflume data for the field cores, the resulting erosion rate parameters generated sediments which were too erodible to reproduce earlier bathymetric changes within the system. The modifications will be fully documented in the final modeling report.
57	2	C	Model calibration to field data can be used to refine parameters and forcing functions. Here, calibration runs of 15 years are tested against water column TSS data and bed elevation data. A comparison of model results with data from the March 16, 2010 high flow event was also conducted. *** It is not clear whether the high flow simulation was a separate short-term simulation or just detailed output from this portion of the full 15-year simulation. If this is a separate short-term simulation, the modelers must make sure the initial conditions were captured properly. ***
57		R	The comparison with the March 16th, 2010 storm is a detailed comparison to the corresponding period from the 15-year simulation. All of the comparisons to data in the report are from the continuous 15-year simulation. The final modeling report will be edited to make this clearer.
58	1,2	C	It is not clear what model parameters, if any, were modified during calibration, or how many calibration runs were made in this phase of the study. The report describes how the model run compares with available data, and reads more like a model validation exercise. That said, what do the data comparisons reveal about model parameterization and forcing functions? Recognizing that the data are often quite variable and often uncertain, and that the observations are at different spatial and time scales than the model output, it is difficult to draw unambiguous conclusions. It seems to me that the model captures many of the general tendencies of the LPR sediment dynamics. Among these are that the LPR is approaching quasi-equilibrium conditions, with solids accumulation much less than the solids loading over Dundee Dam. There is a net upstream transport of solids during low flow periods, and net downstream transport during high flow periods. The data and model tend to tell a coherent story despite the large degrees of temporal and spatial variability and uncertainty. Model results averaged over large areas in the LPR are more likely to be accurate than model results for hot spots. It is not clear how well the model might represent future conditions with altered bathymetry. All of these points are recognized properly by the modelers.
58		R	The sediment transport calibration process involved several hundred simulations focused on improving agreement between model results and data for bathymetric changes over time and high-frequency TSS estimated from ABS. The sensitivity of the model to sediment grainsize distributions led to a decision to adopt the geomorphic zone approach to introduce more spatial resolution into the assigned bed composition. Upstream boundary solids derived from the rating curves (Figure 3-1) were checked against the TSS estimated from initial OBS data obtained in the PWCM program, and that comparison did not indicate a need to modify the boundary solids loading relationships. Extensive sensitivity analyses were performed to evaluate settling formulations for cohesive solids. These included a constant settling velocity, the formulations of Farley and Morel (1986) and Winterwerp (1998). The settling formulations were initially evaluated through comparisons to TSS estimated from ABS data obtained by Dr. Chant of Rutgers University in three deployments (one location in each deployment) in 2004 and 2005. Data from the CPG's PWCM program (five locations in the LPR) were used when they became available, and those comparisons resulted in additional modification of the settling formulation. Model calibration efforts included numerous iterations on interpreting the Sedflume field cores and consolidation experiment data, including distributing the parameters spatially throughout the model domain. Because the Sedflume data were so variable, including pairs of cores collected from the same sampling location, the results were not distributed across the grid based on a spatial interpolation. Initial efforts involved developing inputs for fine grained sediment areas based on two erosion parameter sets: more and less erodible. Later a single set of erosion parameters was tested with spatially variable grainsize to distinguish one location from another. Attachments 1 and 2 to the sediment transport modeling report describe the final assignments, but do not include a description of each iteration. Critical shear stresses for erosion and deposition of the cohesive size class were also varied as part of the calibration process, as indicated in the sediment transport modeling report.

59	1,2	C	Sensitivity analysis characterizes model response to changes in parameter values, and can be used to shed more light on model parameterization. Six inputs were evaluated for a 1-year period – upstream BC, downstream BC, critical shear stress for cohesive erosion, settling velocity for cohesive solids, erosion rate for cohesive solids, and solids grain sizes. Four outputs were examined – solids flux across 8 transects, gross erosion in 7 reaches, gross deposition in 7 reaches, and net erosion in 7 reaches. Results, for the most part, were consistent with expectations, and indicated reasonable model parameterization. Among the important conclusions are that cohesive erosion rate is not sensitive because total erosion is probably supply limited. The characteristic grain size for the model classes is very sensitive. This sheds light on why the model results capture significantly less variability than exhibited by the real system.
59		R	No response necessary.
60	1	C	The organic carbon production model was slightly modified from the existing CARP model, which was previously calibrated and validated on the overall system. I believe the model parameterization from CARP is adequate for this FFS study.
60		R	No response necessary.
61	1,2	C	The boundary concentration functions from CARP were used for POC. The freshwater boundaries use POC as a function of daily flow, while the tidal boundary uses monthly averages. Both seem adequate for purposes of this study.
61		R	No response necessary.
62	1,2	C	The wastewater, CSO, and atmospheric loadings from CARP were used. These seem reasonable for the FFS.
62		R	No response necessary.
63	1,2	C	The initial conditions in the sediment were specified by running the CARP model grid over a number of years to obtain quasi-equilibrium conditions. This seems like a reasonable approach.
63		R	No response necessary.
64	1,2	C	Model calibration to field data can be used to refine parameters and forcing functions. The carbon model calibration was not detailed in this report. Carbon model verification scatter plots are given for water column POC and DOC (Figures 4- 7 and 4-8). These indicate that the model is, on average, in the right range in the water column but fails to capture the variability. In the bed sediment, the model is in the vicinity of the data, but does not capture the average or the variability. Sensitivity of toxicant concentrations to sediment POC indicates little consequence to the OC Model limitations. Sensitivity analysis could have been used to shed more light on the carbon model parameterization, but these were not done for the FFS
64		R	Additional figures and text will be added to the final modeling report to provide further insight into the behavior and performance of the carbon model.
65	2	C	Contaminant loadings to the LPR were derived from CARP along with additional data. For freshwater boundaries, median observed dissolved and particulate concentrations were combined with NPL-calculated POC loadings to obtain total contaminant loadings. For some contaminants, local data were unavailable and so values were estimated from data in the Mohawk and Hackensack rivers. This seems like a reasonable approach. For tidal boundaries, contaminant concentrations were set to monthly output from CARP simulations through the period 1996 – 2054. Wastewater loadings used median monthly concentrations and flows from CARP. SWO's and CSO's used the median of measured data and hourly flows from CARP. Atmospheric loadings were estimated from the NJ Atmospheric Deposition Network, and included gas, particle, and precipitation phases.
65		R	No response necessary.
66	2	C	Initial conditions for sediment contaminants were extrapolated from sampling data. The procedure first averaged data to get representative concentrations within the 7 geomorphic regions. (*** I assume that the median was used ***). Finally, initial concentrations for each grid cell were area-averaged from the representative geomorphic concentrations within that grid. *** This procedure is reasonable and justified if separate geomorphic averages were derived for different reaches in the LPR (it was not clear to me from the documentation how longitudinal spatial variability was considered). The initial contaminant concentrations for the upper LPR were apparently set too low and had to be reset higher in the middle of the calibration run. This is not a valid procedure, and is discussed in the next question. ***

66		R	The geometric mean (approximating the median) of the data within each contiguous geomorphic zone was used to represent the initial bed concentration for that zone. If geomorphic zones extended farther than one mile in length, the zone was subdivided to limit the maximum extent to approximately one mile. The original diagenesis model did not incorporate DOC into the calculation. Refer to the response to comment 20 for a discussion of the approach used to address the very sparse data available for computing initial conditions upstream of RM7, and the alternate plans for the final model runs.
67	1,2	C	The contaminant parameters and constants used here were the same as those used in CARP. For most of the contaminants, the parameters include only partitioning coefficients to DOC and POC. These are reasonably well supported in the literature, though subject to a range of uncertainty due to differences among homologs. For simulating mercury, however, many more constants must be specified, including rate constants for methylation and demethylation, oxidation and reduction, and volatilization. Methylation, demethylation and volatilization rates are documented in background materials, and seem reasonable. *** The net effect of redox kinetics is parameterized in the specified Hg0 fraction (i.e., 10% of HgT_diss). This is not well supported, but may not be sensitive. ***
67		R	The fraction of 10% of total dissolved mercury was based on data from the region although much of that data falls outside the FFS model domain. As the comment suggested, the volatilization of mercury is a relatively small component compared to other loss processes.
68	1,2	C	The sediment mixing rates were modified from CARP. In this study, these rates were calculated by calibration of the carbon model ST-SWEM. This is probably one of the more important model parameters. Model calibration to field data can be used to refine parameters and forcing functions. Here the period October 1995 – September 2010 was used to evaluate the data.
68		R	The carbon model values for the sediment mixing rates were computed in the same fashion as for CARP. The mixing rates in the contaminant model were modified for calibration purposes as discussed in section 4.1 of Appendix BIII (draft carbon and contaminant modeling report).
69	1,2,3	C	Since benthic concentrations were not measured above RM 7 until 2008, the 2008 data were used to estimate initial concentrations in the upstream reaches of LPR. Benthic concentrations there drifted downward during the calibration runs, and so they were reset to 2008 data before running 2008 through 2010. This is an indication that the upstream IC's should have been set to higher values, as determined by iterative calibration runs. *** Resetting concentrations in the middle of a run is not a valid procedure. This introduces more uncertainty into the final calibration parameters. ***
69		R	As discussed in the response to comment 20, final model runs will not include the reset in 2008. The initial condition will be scaled to reproduce the upstream data in 2008.
70	1,2	C	Initial calibration runs had too much initial decline from 1995 – 1998, and so particle mixing parameters were adjusted. A range of values were tested, from 120 cm2/yr to 3.15 cm2/yr. A final value of 10 cm2/yr was chosen. *** This calibration procedure is reasonable and justified. But, if particle mixing was adjusted downward from ST-SWEM, then that model should have been rerun with the lower rates. If not, then there is a disconnect between the models. ***
70		R	The carbon model was not rerun with the adjusted particle mixing rates used in the contaminant model. Particle mixing in the carbon model describes mixing between the thin aerobic layer at the bed surface and the anaerobic layer that represents the remainder of the 10 cm "active" layer included in the carbon model. Reducing the particle mixing in the carbon model would have a negligible effect on the POC concentrations passed from the carbon model to the contaminant model. Adjusting the particle mixing rate in the carbon model could affect nutrient fluxes out of the sediment, but because of the ambient nutrient levels in the system, this would be expected to also have a negligible effect. Sulfate reduction, and therefore, mercury methylation rates could be affected by a reduction in particle mixing in the carbon model, which would have an effect on wildlife exposure concentration in the risk assessment; however, human health risk calculations would not be affected, since they are based on total mercury concentrations.
71	1,2	C	A limited sensitivity analyses explored the long-term consequences to the MNR option of three toxicant model inputs – depth of sediment mixing zone, sediment carbon concentrations, and initial concentration gradients in sediment. When sediment mixing depth is increased by a factor of 2, the model response dynamics slowed, as expected, but the final results were similar to the base case. When sediment carbon is increased by a factor of 2, the fraction of bed contaminant sorbed to particles increased only very slightly, as expected. Since water column carbon was not increased, the sensitivity run showed a net flux of contaminant to the water, thus increasing the rate of decline in the bed. It is not clear how significant this calculation is. Finally, specifying more reasonable gradient initial conditions resulted in differing short-term dynamics, but after 5 years the results converged with the base case and showed no long-term significance.
71		R	There was a concern that the carbon model under-predicted sediment POC in the lower eight miles of the LPR. The sensitivity to sediment POC was run to address this concern. The significance of this sensitivity is that the impact of the potential bias in sediment POC computed by ST-SWEM appears to be relatively small given the generally large partition coefficients for the FFS COPCs. The reasoning for

			performing this sensitivity will be incorporated into the final version of the carbon and contaminant modeling report.
72	3	C	The transport and distribution of cohesive solids significantly affects the spatial and temporal distribution of COCs. It is not clear how well the solids behavior matches smaller areas within reaches that might function as hot spots. Sediment behavior is quite patchy and nonlinear, and small areas could control the overall risk calculated for COCs. It seems that the behavior averaged over large reaches is reasonable, however, and could be used to evaluate relative effects of remedial alternatives.
72		R	Agreed. The final modeling report will incorporate figures presenting time series for subsets of the larger reaches (i.e. shoals versus channel, etc.) to look more closely at the range of predicted results.
73	3	C	The OC model does not capture the spatial and temporal trends in the LPR. Given the relative lack of sensitivity of POCs to the details of the carbon model, the overall representation of organic carbon is good enough to evaluate relative effects of remedial alternatives.
73		R	No response necessary
74	3	C	Spatial data in the sediments are very patchy, with hot spots. The model cannot capture this extreme local spatial variability. Still, the model might be able to capture the dynamics of reach---averaged concentrations well enough to judge between remediation alternatives. This is examined below. The calibration procedure included a significant problem with the upstream IC's – the concentrations for TCDD, PCB, and Hg were set too low, which led to an arbitrary calibration reset in the upstream reach in 2007, the middle of the simulation. As a result, it is difficult to judge the model behavior based on the 15 year calibration plots shown. If the upstream IC's had been calibrated, then the results might have shown better fidelity in capturing the gross spatial and temporal trends in the data. At best we can say that the model is within a factor of 2 or so in its average response for the two large reaches (upstream and downstream).
74		R	Refer to the response to comment 20 for a discussion of the approach used to address the very sparse data available for computing initial conditions upstream of RM7, and the alternate plans for the final model runs.
75	3	C	TCDD data shows little definitive time trend for the upstream and downstream LPR reaches, though it is possible that there is a slight decline in the period 2008 – 2010. The model shows a slight increase in that period, though partially masked by the calibration reset in 2007.
75		R	While the reviewer's comments regarding temporal trends is taken as a general observation, it is important to recognize that different sampling programs over the 15-year period had different spatial extents and objectives and widely varying numbers of samples. The 1995 RI data in the RM1-7 reach and the CPG 2008 low resolution coring program throughout the river are the most complete; however, substantial variability in concentrations in both of those data sets result in wide, and for many contaminants, overlapping confidence intervals. Evaluating temporal changes based on the 2008 and 2010 datasets is difficult because of the different sampling designs employed in those programs. The 2008 data were generally regularly spaced and included a large number of samples within the historically dredged channel, while the 2009 and 2010 benthic datasets targeted only shoals. Both datasets include a great deal of variability about the mean. It is noted that evaluating trends from the data for the upstream reach is significantly affected by decisions made to deal with the very sparse data above RM7 for the time period near the start of the calibration period (refer to response to comment 20). The initial condition for the upstream reach includes data from the CPG 2008 low resolution coring program and the spatially dense data collected at RM10.9 in 2011. In the model, the RM10.9 data affect only a few grid cells, however, that data set has a significant effect on the reach average computed from the data because of the number of samples collected at RM10.9 represent almost half of the data used for initial conditions above RM7. Refer to the response to comment 20 for a discussion of what the reviewer refers to as "the calibration reset in 2007".
76	3	C	PCB data seems to show a downward trend over time for both upstream and downstream reaches. The model captures this trend until 2008. After that, the data decline while the model increases in the upstream reach and stays flat in the downstream reach.
76		R	Refer to the response to comment 75 for a discussion of the difficulties associated with evaluating temporal trends from the reach average concentrations (Figures 4-10 through 4-15).
77	3	C	For total Hg, the model captures the downstream trend of slight decline reasonably well. The downstream data hover around 3000 ug/kg from 1995 through 2006, and then jump down to around 1800 ug/kg from 2008 through 2010. The upstream trends are not captured very well. The model IC is set well below the data, then gradually declines, remaining below the data until the 2007 reset. After that, the model goes up while the data declines from 2008 though 2010.
77		R	Refer to the response to comment 75 for a discussion of the difficulties associated with evaluating temporal trends from the reach average concentrations (Figures 4-10 through 4-15). Refer to the response to comment 20 for a discussion of what the reviewer refers to as "the 2007 reset".
78	3	C	In summary, the model results are generally within a factor of 2 of the observed data midpoints, and so could possibly distinguish between management alternatives that cause future differences of that magnitude. *** Since recent possible trends toward concentration declines are not captured by the model, this leads to some uncertainty about whether the simulated MNR alternative will be biased high. This could be addressed by a recalibration of initial upstream IC's and extension of the calibration through 2012. Sensitivity runs with higher upstream IC's could help resolve some of this uncertainty as well. ***
78		R	Both of these suggestions will be captured in future model runs using upstream results that are scaled up at time zero, avoiding the reset in 2008 and extending the suite of model runs through 2012.

79	4	C	The COC sources that may recontaminate FFS sediments during and after remediation include external loadings from tributaries, CSO's, WTPs, and the atmosphere. These are captured reasonably well. I believe there are larger uncertainties in how well the in-place contaminated sediments are captured. These include unremediated upstream sediments and downstream sediments released during dredging operations.
79		R	No response necessary.
80	1,2,4	C	Because of the calibration reset during mid-simulation, I'm not sure how well the model captures the in-place upstream COCs. A sensitivity run (2 times IC for upstream reach) could have addressed this source, but it wasn't run.
80		R	This will be addressed in future runs by scaling up the initial conditions and avoiding the reset in 2008.
81	1,4	C	The procedure for simulating releases during dredging is mostly reasonable. One weakness is that the treatment of internal sediment loadings differed from alternative 2 (Deep Dredging) versus alternatives 3 and 4 (Capping with Dredging and Focussed Capping with Dredging). In alternative 2, the solids released during dredging were incorporated back into the sediment transport model. Without the sediment model rerun, the redeposited solids would have had COC concentrations too high by a factor of 2. The internal dredging releases were not rerun in the sediment model for alternatives 3 and 4, and as a result the redeposited solids have COC concentrations that are too high, thus overstating the recontamination at least slightly. It is difficult to judge the resulting bias, though it is noted that the solids release during alternative 3 and 4 are 43% and 9% of the alternative 2 releases. *** This bias should be kept in mind when evaluating the differences between alternatives. ***
81		R	This will be addressed in future runs by including releases of solids in all of the models for all scenarios where dredging is not done within an enclosure.
82	5	C	The model accounted for two high flow events – April 2008 and March 2010. The March 2010 event is a 1 in 25-year storm event, which the model seemed to handle well enough. The model was not tested against any larger events, and the simulation period evaluating the alternatives repeated the 15 year hydrological record 1995 – 2010. So the modeling results can be said to cover "moderately extreme" events, but they do not cover more extreme events with a recurrence of 1 in 50 or 1 in 100 years. This is somewhat surprising, as a process-based model can be used (with great caution) to extrapolate beyond the observed datasets. Sediment transport is highly nonlinear, and the more extreme events could have major effects on the remediation alternatives.
82		R	This will be addressed by incorporating the two additional years through September 2012 into the calibration. This period includes Hurricane Irene, which was a 1 in 75-year return flow. In addition, a sensitivity will be run at a point after implementation of the remedial alternatives, including a 1 in 100-year return flow and the subsequent three years to test the impact of extreme events on the system with the remedial alternatives in place.
83	5	C	The extra materials provided show that the highest daily flow in the 112 year record was about 30,000 cfs, compared with the highest flow in the simulated period, about 15,000 cfs. Given the exponential increase of erosion with flow, assuming the exponent is between 1.2 and 3, a doubling of high flow would lead to erosion rates from 2 to 8 times higher (rough bounding calculations provided below).
83		R	The Sedflume data show one to two orders of magnitude reduction in erosion rate within the first 5 to 10 cm in the sediment bed and an increase in the critical shear stress with depth. Bed coarsening in non-cohesive areas and consolidation in cohesive areas will slow down erosion as the upper portion of the bed is eroded. At higher shear stresses more mass and deeper erosion will likely occur, but to a lesser extent than one could conclude from the ratio of flow rates. Recent bathymetry survey data do not show extensive erosion from before and after an extreme high flow in 2011 (24,700 cfs at Dundee Dam following Hurricane Irene). The CPG conducted a multibeam bathymetry survey between RM1 and RM14 in October 2011 after Hurricane Irene (August 2011). Comparison between the post-Irene bathymetry and a limited multibeam survey conducted between RM10 and RM12 in July 2011 did not show extensive erosion, with the exception of the vicinity of bridge abutments. Comparison between June 2010 bathymetry (also RM1-14) and post-Irene bathymetry also did not show extensive erosion.
84	5	C	HQI modelers provide a reasonable response that "the mass of sediment eroded or depth of erosion will not increase in proportion to the increase in erosion (flow?) rate … Sedflume data show one to two orders of magnitude reduction in erosion rate within the first 5 to 10 cm in the bed and an increase in the critical shear stress with depth. … If the question of extreme events is important enough, I recommend that the calibration/verification be extended to simulate the Irene event. This would be especially useful if further contaminant surveys are available to test against the model. Then this event could be included near the end of the long-term simulations evaluating the four management alternatives.
84		R	Additional years and high flow sensitivity runs will be added to the modeling analysis to address this concern. Refer to response to comment 82 for additional information on incorporating high flow conditions

			in the calculations.
85	3	C	most if not all of the reviewers expressed concern that the substantial benefits predicted for two of the alternatives were "non-intuitive" in that they that show dramatic effects of some of the remedial action scenarios on sediment COCs in the 0-8 RM stretch of the lower River (Deep dredging with cap and capping RM 0-8), where modeled concentration reductions of approximately two orders of magnitude for a range of COCs are projected often without much loss in upper river segments (RM 8 – 12/13), and when sediments downstream in Newark Bay also remain elevated above those in the FFS 0-8 mile area i.e. the FFS study area becomes and remains a long term local minimum in concentration.
85		R	Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report.
86	1,3	C	In Figure 6-3 of Appendix III (and related results) it is seen that there are episodic sediment transport events that lead to higher concentrations in the FFS, but that those levels are rapidly attenuated with short characteristic times. It appears to this reviewer that the primary explanation for this is that very little net deposition of sediment (in comparison to historic deposition) is predicted to occur and when it does it is relatively temporary under these remedial action scenarios. Examining Figure 3 from a March 6 correspondence to the review team (predicted bathymetric change map 15 years after dredging) is consistent with this interpretation, although I think there may also be issues I don't understand related to how contaminated new sources of sediment to the watershed become during transport to RM 0-8.
86		R	Results from simulations with the revised erosion parameterization do not show the temporary sediment accumulation mentioned in the reviewer's comment. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report.
87	1,2,3,4	C	The sediment data either poorly constrains model performance done during calibration or highlights some questions about setting of initial boundary conditions or predictions of contaminant decline that appear perhaps too rapid based on past changes. The comparison of the model predictions and initial boundary conditions in the contaminant model raise questions about how useful the calibration is as well questions regarding how intitial boundary conditions are set in Newark Bay, as well as whether there are better ways to normalize contaminant data to make the calibration and model/data comparison more constrained and useful. The contaminant model is calibrated with highly variable surficial sediment data, which as presented provides little constrain on interpretation of model performance in the Passaic.
87		R	The variability in contaminant concentrations within the study area is substantial and attempts to reduce the variability by normalization have not been effective for the LPR. The initial conditions within the FFS Study area were largely derived from the 1995 RI Sampling Program. That dataset contained questionable values for organic carbon, with surface sediments averaging 10.5% and values as high as 40%, making it infeasible to incorporate carbon normalization in setting initial conditions. (Refer to the response to comment 20 for a discussion of the approach used to address the very sparse data available for computing initial conditions upstream of RM7, and the alternate plans for the final model runs.) The specification of sediment initial conditions in Newark Bay is being revised to incorporate carbon-normalization and segregate spatial interpolations within and outside of the navigation channels of Newark Bay. The data sets available for Newark Bay initial conditions are not subject to the same anomalously high sediment organic carbon problem exhibited in the LPR 1995 RI data.
88	3	C	For Newark Bay sediment data where there is a general bias with model predictions (often including initial boundary conditions) lower than field measurements.
88		R	The limited number of data points available for setting initial conditions within Newark Bay contributes to the bias mentioned by the reviewer. The specification of sediment initial conditions in Newark Bay is being revised to incorporate carbon-normalization. The data sets available for Newark Bay initial conditions are not subject to the same anomalously high sediment organic carbon problem exhibited in the LPR 1995 RI data, and therefore the use of carbon normalization for the bay is a reasonable alternative approach. Results from this additional effort will be incorporated in the final modeling report.
89	1,2,3	C	A clearer picture of relatively recent longitudinal distributions is obtained from TOC normalized concentrations of key COCs provided us with the Charge Document, where it is seen that normalized concentrations are typically relatively uniform over the lower 12-13 miles of the River with generally modest declines (well less than an order of magnitude in all cases) with distance heading away from the mouth heading into Newark Bay. Using normalized data it is also more clear that there appears to have been little decline in concentration in most of the study area over the recent past, consistent with sediment core results we have been shown in the past. For carbon normalized DDT there is no concentration decline for several miles into Newark Bay, and for Hg and other selected contaminants, levels in outside the mouth are not vastly different than in the FFS source area subject to possible remediation. The model has some of these concentrations in Newark Bay dropping dramatically over time (notably DDT residues and Hg which decline with rapid half-lives), which is both saying something about confidence in model predictions in general.
89		R	As stated in response to comment 88, Newark Bay initial conditions will be revised based on spatial interpolations of carbon-normalized data.

90	1	C	As the primary driver of these results is the sediment transport model, it is important to understand what the model is actually projecting with respect to deposition in the lower Passaic River under different alternative remedial action measures, and what in the model controls these predictions. The review by Dr. Lick goes into the parameterization of the transport model in detail, and it appears that his concerns about potential biases my be matching my interpretation (provided below) that the model is on average likely predicting greater erosion that than observed in the field; my concern is that this then leads to an under-estimate of the importance of net deposition in RM 0-8 into the future and underestimate of the role of upstream and downstream sources of sediment in re-contamination of surface sediments in this area (which are manifested most in predictions in response to the two clean capping scenarios for the FFS area.
90		R	Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not complete as of the date of this response, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report.
91	2	C	This review focuses on concerns and questions. However, I want to take the opportunity to point out that I continue to be impressed by the Passaic River focused lab and field research level work that has been done on transport of non-cohesive and especially cohesive sediment. I don't pretend to understand specifics of how different particle size assemblages are transported, conserved, or averaged in the model, but I appreciated that Appendix II was very well written, that the authors have tried to pull much out of the data and interpret it evenly in most respects. They should be commended for the this level of interpretation – there is nothing approaching this level of insight presented in the organic contaminant and organic carbon modeling report, which leaves one with so many more questions than answers. However, to be fair, there is much more underlying modeling and data in the Contaminant Report that can be discussed and much of the data available is not particularly amenable to for diagnostic model calibration purposes, at least in the ways that have been attempted here. Furthermore, most of the model and data have been reviewed elsewhere…there is substantial merit in the fact that the model was extensively Peer reviewed as part of CARP, e.g. I am extremely impressed by the amount of chemical contaminant data which was collected and interpreted but which has only shown in the most distilled ways in Appendix III and even associated attachments. The very act of setting the boundary conditions for sediment contaminants with depth in every grid cell was an enormous task. It would be hard to please everyone with respect to the level of detail or type of interpretation and data interpretation in Appendix III. The key difference between the two reports in my view is that there is much less in the way of useful or insightful calibration in the contaminant and organic carbon fate report, and there are two major types of data that are amenable to sediment transport model testing, namely the temporal and spatial distributions of suspended solids and the estimates of net burial or erosion determined from single or multi-beam sonar studies that have been interpreted over two time periods. It is not clear to this reviewer if the model yields predictions of grain size that could be compared to field data in a useful way?
91		R	Additional analyses and discussion will be added to the final carbon and contaminant modeling report, with greater details describing the approaches used for development of model inputs and interpretation of results. The sediment transport model does compute changes in sediment grainsize distributions in response to erosion and deposition of different size classes. Comparisons between computed and measured grainsize distributions will be included in the final sediment transport modeling report.

92	1	C	Sediment transport modeling and results. For most if not all of the COCs of interest, sediment transport is arguably the key to the modeling efforts, and understanding the predictions from the combined models. I believe getting the sediment transport described reasonably (esp. net deposition/erosion) is more critical than how e.g. chemical reactions or transfer between phases are treated in the models, although the latter are also important esp. for lower Kow PCBs, and metals undergoing redox transformations or having lower Kd values in the model (i.e., esp. Cd and even Hg). I think that this investigation is unique in that one can argue that sediment transport is even more important in this study than in the vast majority of other sediment contaminant remediation/modeling studies of his type because of the extremely dynamic nature and high rates of erosion and deposition in the Passaic but also because only a fraction of the contaminated area is being considered for remediation – i.e., it is important to in determining whether remedial action goals can be met by only treating all or part of the 0-8 mile reach, when concentrations in potential source areas both up and down river are not mitigated and have concentrations either as high currently (DDE e.g.), nearly as high (Hg, TCDD) than those in RM 0-8. The lower Passaic is also an area where deep scour has been observed with both bedforms (and surface expression of contaminants) as well with numerous side scan sonar based bathymetric surveys conducted over the past two decades; it is unusual to see such clear evidence of relatively deep scour in what have been depositional areas with real data. I have focused my attention on how well the erosion and deposition models match the data (water column solids and net transport as well as net deposition derived from changes in bathymetry) and possible implications for biases between the two that concern me and may be saying something about model performance.
92		R	It is noted that remediation both upstream and downstream of the FFS Area is being evaluated in the LPR 17-mile RI/FS and the Newark Bay Study Area RI/FS. Even though the FFS is focused on the RM0-8.3 Reach, that does not mean that remediation will be limited to the FFS Area, as evidenced by the remediation that has begun at RM10.9.
93	1	C	Conceptual model. The conceptual model, put forth explicitly in both of these reports, and the materials distributed when we first discussed the charge for reviewers, is that the formerly dredged lower River has been documented to have filled with sediments at an incredibly high rate for decades but that the net deposition rate has decreased and the bed surface has reached a new quasi-equilibrium where net deposition is typically a very small fraction of gross deposition or erosion. I remain unconvinced that the area of the FFS is not still highly depositional over extended time periods and will ask e.g., whether there has been a balanced interpretation of all the bathymetric data (and perhaps other sediment core data not presented). It was not clear to the reviewers at the mid-review call why the model is predicting such low concentrations in the 0-8 RM area following either of the two remedial scenarios that results in a cap of the entire area. What appears to be largely at work is that contaminant levels, under alternate remediation conditions where the 0-8 mile reach is capped, with or without deep dredging, remain so low in the future (and why later spikes in concentration are dissipated with such rapid characteristic times) is that there is essentially negligible burial predicted by the model in most (but not all??) of the RM 0-8 area and that by averaging 0-15 cm, the model is essentially computing concentrations of solids that are still dominated by a clean cap surface - there are alternative explanations for the model behavior that are unfortunately eluding me with respect to my sense of physical reasonableness…..a revised report should do more to address how much of the drop in concentration is due to averaging in the clean cap material, as well as insight into of the model predictions that lead to up to about 1 cm/yr burial in a couple of the reaches of the FFS ara (Figure 3 transmitted March 6 in the mid-point matrix response) with sediment that may not have become contaminanted *[sic]*during transport to the area - a corresponding map like this with concentration of contaminant in the 0-1 cm range would be both instructive to understand what the model is predicting
93		R	The sediment morphology and chemistry data as well as the conceptual model are clear about the fact that there is a great deal of local heterogeneity of the sediment bed, with some zones of both intense deposition and erosion with a dynamic feedback between resulting morphology and hydrology that then controls associated shear stress. Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete as of the date of this response, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report. Additional figures will be added to the final modeling report displaying the time series of the response of the Study Area broken out into various regions (channel, shoals, depositional, erosional, neutral, and top one cm). This should provide greater perspective into the behavior of the model.
94	1,3,4	C	From a mass balance perspective, it seems to this reviewer that a likely reason that regional concentrations have not declined over the past couple decades, in the face of what is estimated as a large relatively clean loads of sediment over the Dundee Dam (and somewhat cleaner sediment from other boundaries), is that new erosional surfaces are exposing important hotspots of legacy contaminants that then “buffer the system”. This is of concern when considering the risk of not remediating contaminant source areas above and perhaps even below the 0-8 mile reach (I note the ongoing clean-up at RM 11.9 – an area of concern that was raised in reviews of earlier modeling reports, when a similar conceptual model was proposed). But for me, this issue raises the question of whether the grid spacing (the number of which are constrained by the complexity of the model and run-times) are small enough for models to reproduce potentially important localized erosion rates that may be important for exposing and exchanging materials from important hotspots, where the product of very high concentration and small surface area might be high enough to change net fluxes from the bed. I see that Dr. Lick has also considered this issue. Here I will focus more on what the comparison of the field data and the calibrated model predictions might be saying about model performance and potential bias with respect to resuspension (erosion) and net deposition that are key to both chemical exchange with the water column and lateral exchange and deposition of sediment at RM 0-8 and elsewhere.

94		R	With respect to potential sources outside the FFS study area, the intent is to address those areas either in the context of either the 17 mile RI/FS or of the Newark Bay RI/FS. The FFS study area extent was chosen because it captures the majority (85 % of the surface area and 90% of the volume) of the cohesive sediments within the LPR. Above approximately RM8 there is a shift from mainly cohesive silts to mainly non-cohesive sands with pockets of silt. Given the location of the site and the tendency of the COPCs to bind to silts, the FFS study area captures the majority, but not the entire inventory of COPCs within the LPR. It is likely that occasional erosion into deeper, more highly-contaminated sediments represents a contaminant source that buffers the rate of recovery within the system. The reviewer properly recognizes that model complexity and run-times constrain the decision of the scale of the computational grid. Higher grid resolution is always desirable in a system as complex as the LPR, but maintaining manageable model run times on available computing platforms counterbalances this desire. The grid resolution of the LPR model was evaluated with a grid convergence test, in which comparisons were made for salinity, velocity, bottom shear stress, and flushing time computed with the final grid and a grid with resolution increased by a factor of four. Comparisons to data showed only minor improvements in predictive capabilities (and equivalent prediction accuracy in some cases) at a cost of a factor of eight increase in computational time associated with the higher resolution grid. This increase in computational time is not feasible for the LPR modeling effort. This grid convergence test did not include the sediment transport or contaminant transport models, and it is true that small scale sediment heterogeneity is present in the LPR. It is not clear that increasing the grid resolution by any reasonable amount would address this issue, however. For example, the in situ Sedflume erosion tests revealed significant variability in erodibility at horizontal separations of less than 10 m, far too fine to attempt to resolve numerically. Furthermore, it is not clear how sediment bed variability would be parameterized at finer scales than in the present model, given this small-scale spatial variability.
95	1,2	C	Based on my weight of evidence interpretation, I question whether there is an important bias in the erodability of sediments and net erosion and deposition rates predicted from the models. First, although only "representative" time periods are shown, the model appears to over-predict erosion rates needed to explain the magnitude of most of the water column suspended solids data at the preponderance of stations – Of the 12 time series shown (figures 4-1 to 4-14), only in Figure 4-2 and 4-13 (moderate and low flow, MP 4-2 turbidity max region) are suspended solids levels not largely if not grossly over-estimated by models describing results of the Physical Water Column Monitoring (PWCM) program. Because of the magnitude of the differences in most cases it seems pretty clear that the primary driver for the difference is that the model is estimating deeper and more frequent depths of erosion (below the variable 0 -0.2 mm fluff zone) – as opposed to it underestimating settling rates. As described in the report the model does a great job of getting the phasing of resuspension correct (although I suspect little tidal blips in resuspension correspond to non-mechanistically defined fluff layers and not the actual consolidated bed). Vertical mixing as it affects water column profiles of suspended solids seem to reasonably well represented within the confines of the data shown. It is unfortunate that the sensors can not provide estimates for deeper near bottom depths with higher solids loads (Sigma 9, 10 and sometimes 8), because transport in these horizons can greatly influence if not dominate the integrated fluxes. Extrapolation of data towards the bottom are then needed to estimate sediment transport up or down River when using the observed data.
95		R	Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. As a result of that effort, it is expected that the comparisons between simulated and ABS-derived TSS will improve. As part of the ongoing work, revisions to the sediment erosion parameterization are being evaluated as well. However, part of the response to comment 11 is also relevant here: The reviewer may have overstated the importance of errors in the resuspension and deposition parameterizations for some of the mismatches between simulated and observed (estimated) TSS during the PWCM program period. Agreement during the October 29 to November 4, 2009 period (Figures 4-1 to 4-5) is on the whole quite good, especially in the vicinity of the estuarine turbidity maximum (ETM) where local resuspension and deposition dominate. Well upstream of the ETM, tidal turbidity patterns are dominated by advection on ebb tide, which the model over-predicts, but this could be because of factors not related to local exchange with the sediment bed. The model also over-predicts relative to the observations during the March-April period shown in Figures 4-8 to 4-12, but as stated in the report this is most likely because of a temporary overestimation of riverine sources during high runoff prior to this period. The basis of this assessment is that advection from upstream is indicated by a semi-diurnal pattern with maxima at the end of ebb tides, rather than the quarter-diurnal pattern associated with local resuspension. The quarter-diurnal signature of local resuspension and deposition is again apparent in Figures 4-13 and 4-14, which show the model to under-predict the data at one station and to over-predict the data at the next station upstream, but to capture the essential dynamics of resuspension and deposition well. It is also worth pointing out that the ETM in the LPR is about the same size as a typical tidal excursion of about 4 to 5 miles. Like most ETM, it is centered around a pool of resuspendible particles collected by transport convergence, but this pool is advected significantly on a tidal basis and most likely is no more than a few millimeters thick on the bottom during slack tide. Just a slight error in predicting the location of the pool of particles can lead to mismatches with individual moored measurements, while the overall pattern makes more sense.
96	1,2	C	Figure 4-15 provides insights into the implications of overestimating resuspension rates when one appreciates that esp. under low flow conditions that resuspension is flood dominated leading to net upstream/estuarine transport of solids - unfortunately Figure 4-15 only shows results for the fall period it seems (with lower flows – why not the other Spring data set with a bigger range of flows??). The agreement between the "data" and model appear best at the upstream 13.5 RM site where upstream estuarine transport is least important and net fluxes in general are low at flows below 30 m3/s; however, although on a relative scale the net flux is much less positive in model estimates – a ratio of the two estimates would show that on a proportional basis there appears to be better agreement at high flow but how much of this is from local resuspension vs. high flux of residual upstream solids is unclear. As one moves closer to the mouth of the estuary and flood dominated upstream transport becomes more

			important, the differences between the often larger upstream modeled fluxes and lower "measured" fluxes becomes increasingly important, esp. at discharges between 5-65 m3/s.
96		R	The spring data are not included on Figure 4-15 because the CPG is still in the process of addressing changes in the relationship between ABS and TSS within the period of the spring deployment. The modeling team's assessment is that the comparisons between modeled and data-based flux estimates shown in Figure 4-15 are good, when taken as a whole. While it is true that the model slightly underestimates downstream flux and overestimates upstream flux at RM6.7 (though the overall pattern is reasonably reproduced), the model-data agreements at RM13.5 and RM4.2 are quite good. As stated in the sediment transport modeling report, the model's relative overestimation of upstream flux at RM1.4 is likely due to lateral differences in flux patterns in this more complex reach of the river, but this cannot be proven because data are available from only one mooring location in the transect. To restate conclusion 4 on p. 4.7 in the draft sediment transport modeling report: Given the variability in the relationship between ABS and suspended solids concentrations, and the limited range of suspended solids data available to develop this relationship, the comparisons between the PWCM program derived fluxes and model results are considered acceptable by the modeling team.
97	1,2	C	The behavior of the model as function of river mile and flow is very nicely illustrated in 4-45 through 4-48. The model predicts lots of upstream transport of solids at lower to intermediate flows, increasing in magnitude as expected with tidal amplitude. What is likely largely the same pool of easily erodible material is swept by the model back downstream at high river flows, with the net long term fluxes downriver. This result is what was expected based both on asymmetry in tidal flow driven bottom shear the hydrodynamic model is mimicking, and also what we actually know about estuarine circulation and sediment transport. However, what is important to sediment transport and contaminant exchange in the water column is the frequency and magnitude of resuspension/eroded sediment depth. If one takes the suspended solids estimates at face value the model is sloshing around a lot more material than the calibration data indicate. Does the model over-estimate resuspension and lateral exchange and as a result perhaps under-estimate net depositon?? These has profound implications for lateral transport and net deposition of contaminated sediment into the RM 0-8 FFS study area for all simulated alternative remediation scenarios.
97		R	The Sedflume experiments done with the consolidation cores measured far less erodibilty than the field cores suggesting that the erosion rates specified in the model based on the field cores may be biased high, based on the comparison of simulated versus observed historical infilling . There are a number of potential causes for the differences observed between the two, but the large degree of variability in the field core measurements made interpretation of those results difficult. Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete as of the date of this response, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report.
98	1	C	It is difficult form me to put too much weight on the high flow experiment where Bob Chant made three transects over part of a tidal cycle near the mouth of the Passaic (expected turbidity max near the mouth with this flow). It appears visually in examining Figures 4-17 to 4-19 that the model greatly over-predicts the magnitude of the predicted resuspension (which seems most likely controlled by local resuspension rather than advection from afar given the spatial structure) – however, these figures are plotted on a linear scale, and when the data are presented on log scales (Figure 4-20) it is seen that the while the very highest suspended loads are not captured anywhere in the model, that perhaps the range of suspended solids concentrations is, as argued in the Report, not that bad?? Chant should be given credit for getting out and making these measurements, but given that this sampling is neither synoptic nor "Lagrangian" when following the ebb, I would not make too much out of them, although one might also remember that other model results do suggest that settling times are not faster than the boat was moving from station to station. I would also point out there is something in the parameterization of settling rates that may be at work in this particular case where suspended solids levels are exceptionally high in the model) – when solids loads start to approach 1 g/L the model parameterization indicates that the fine floc fraction increases and leads to greatly reduced settling (Figure 2-4) – presumably this a result of capturing very high shear on particle aggregation rates. I don't know if this assumption about solid concentration effects on settling rate is widely accepted or not?? Again this issue I see was raised in Dr. Lick's review. But because much of the downstream (and even upstream in the area of the turbidity maximum) transport occurs during high flow/very high suspended solids events, it may be that getting the dependence of settling velocity on computed solids concentration may be an important determinant in long term net transport?
98		R	The settling velocity formulation used in the LPR model is consistent with relationships derived by various investigators (Krone, 1963), (Dyer 1989), (Kranck et al., 1993), (Teeter, 1993) which show an increasing settling velocity with increasing solids concentrations.
99	1,2,3	C	Bathymetric changes over time. I have placed significant weight on the estimates of net deposition or erosion based on differences in bathymetry measured over multiple surveys conducted between 1996 and 2010 and how those integrated volumes compare to modeling results (focusing on Figures 4-26 and 4-35). Much effort was placed on collecting and interpreting this data. The data have been presented with interpretations of changes between 1996 and 2004 (nearly 8 years) and then 2007 to 2010 (approximately 2.7 years or one third the time interval). There are a number of important points to make which may be important as it bears most directly on whether or not model can simulate what deposition occurs in the 0-8 RM stretch under varying remediation scenarios considered: For the 1996 to 2004 data there is much average net deposition in the 1-7 RM range than estimated by the model in all but one of the RM segments and significant deposition is estimated to occur in in all segments (whereas in the model the only important net deposition occurs in the 2-3 RM stretch).
99		R	Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. The effect of these revised sediment transport results on the behavior of the contaminant model, in terms of rates of recovery and recontamination, will be evaluated in the final modeling report.

100	1,2,3	C	As long as measurement errors are not grossly different between the 1996-2004 time period, the net deposition estimates from this interval should be more accurate than for the 2007-2010 interval both because of the nearly tripling of time period allowing differences in elevation to rise above the errors, and because, if the data are correct the magnitude of annual deposition was greater during the first period (argued that this was in part due to the latter period capturing two higher flow events that transported more solids through the system). In Figure 4-24, it is seen that estimates of 5 to greater even greater than 50 cm deposited over the first almost 8 year year period – on the order of (0.7 to 7 cm/year) – suggesting to me that as recently as a decade ago this was hardly an equilibrium surface, or an area where one can dismiss easily that there will not be new deposition if it is capped (with or without deep dredging). The model does not have areas of nearly as high deposition at any River Mile range except RM 2-3. My back of the envelope calculation suggests that this cumulative deposition is a significant fraction of what is estimated to be coming over the Dundee Dam (32,000 MTons/year) - below I raise the question as to how good those estimates of upriver sediment loads are.
100		R	In response to the comment regarding measurement errors affecting estimates of bed elevation changes in the two periods, it is noted that the bathymetry data from the latter period were obtained from multibeam surveys, while the bed elevation changes for the earlier period are based on single beam bathymetry data, which require spatial interpolations to generate the bed elevation change estimates. The modeling team agrees with the reviewer's comment that the system was not at an equilibrium condition a decade ago. The conceptual model is that the system is approaching a dynamic steady state. Chant et al. (2010) analyzed relationships between solids transport near RM3 and river flow to conclude that the downstream solids transport toward Newark Bay was approximately equal to the annual solids loading to the LPR. It is noted that additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. Refer to the response to comment 8 regarding estimates of the upriver sediment loads.
101	1,2,3	C	Interpretations in this report and the basis of the entire conceptual model are however slanted towards interpretations and calibrations associated with the second three year data set that should suffer from more uncertainty given the much smaller differences in elevation that were observed or could be expected over a shorter time interval. However despite this, the magnitudes and spatial distribution of the magnitudes of net deposition compare very favorably to the model computations between 2007 and 2010; this is great, but the authors have essentially based their major interpretations on this second more recent set of comparisons between model and bathymetric change.
101		R	More emphasis was placed on the 2007 to 2010 period because it more closely reflects conditions that are expected in the future. Analyses indicate that the bathymetry of the LPR is reaching a quasi-equilibrium (Chant, et al., 2010) and therefore sediment accumulation observed historically is less likely to continue into the future. It is noted that additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling.
102	1,2,3	C	One is left to ponder whether one set of results is more accurate and whether the authors have placed their emphasis on the 2.7 year, more recent study because: it agrees better with the model and the conceptual view of the system that we have heard about; because the study was more recent and represents better the current (and future??) conditions; or because they really don't believe the adjustments used from the earlier bathymetric surveys. Are these integrated estimates of net *[sic]* meaningful which I suspect they are at least over the 1996 – 2004 period. The fact that the surface was accreting so much over this period over much of the FFS area, under perhaps more average conditions between these years, and that the model does not reflect deposition in many of the these one mile reaches has me concerned again that there is a bias towards over-prediction of erosion and an associated under-prediction of net deposition.
102		R	Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report.
103	2	C	How well is the upper Passaic River and other tributary loads of silt known?? One thing in common to the contaminant transport model and sediment transport model is the importance of knowing loads of sediments, that also carry contaminants, from the upper to lower Passaic. The conceptual and actual models assume that most of the supply of cleaner sediment, that is important to long term recovery of the study area, comes from the upper Passaic. Much of that material is modeled to make its way rapidly to Newark Bay, from where some of it can re-enter the lower Passaic as a result of estuarine transport; it is not clear whether in the model most of the upper Passaic sediment that is predicted to now escape the lower River deposits along the way or not, but based on settling velocities, residence times, and intuition, I believe that to be the case. I started to wonder about how well constrained the loads of solids into the system are when I looked at Figures 3-2 to 3-6 of Appendix II. The baseline low flow concentrations of suspended solids vary markedly between tributaries but are remarkably flat at under low flow conditions and in the case of the upper Passaic and other selected tributaries TSM concentrations never drop below approximately 10 mg/L, whereas in other tributaries the concentrations sometimes drop to 1 mg/L or less, but also are relatively invariant with time under low flow conditions – there is remarkably little variation around basal concentrations. It should be assumed that these data are correct and I hope that they are – it would not be surprising if some freshwater streams/rivers never have low concentrations However, I would expect basal low flow levels to be somewhat more variable, which sets off potential red flags in my experience. I am curious about how well the sensors have

			been calibrated at low solids levels in each tributary, because my own very limited experience is that optical turbidity sensors correlate with solids in very different ways in different water bodies and there can be differential background (phytoplankton/DOM?? I'm no expert) affecting relationships that can create differences in positive intercepts/backgrounds when data are regressed. Because the baseline for the Passaic is so high (10 mg/L), it likely has a significant effect on the annual loading of TSM; i.e., less event driven than in other tributaries where baseline TSM is much lower. I also know that there is some art (measurements I want someone with experience to do in my lab) associated with making low level TSM measurements, and depending on whether glass fiber or membrane filters are used, the volumes and differences in filter weights are not very great and susceptible in my view to positive bias. My question then is how well has the TSM concentrations been calibrated and are there any potential biases that could lead to an artificially high estimate of solids loading down coming over the Dundee Dam.
103		R	Estimates of the Dundee Dam and Saddle River solids boundary conditions are based on regressions versus flow (Figure 3-1), which incorporated data available at the time the regressions were developed. The resulting estimates compared favorably on an annual average basis with those estimated by the USGS (Wilson & Bonin, 2007). Dundee Dam boundary solids concentrations derived from the rating curves (Figure 3-1) were checked against the TSS concentrations estimated from initial OBS data obtained in the PWCM program, and that comparison did not indicate a need to modify the boundary solids loading relationships. It is noted that the time series of solids loadings for the tributaries, shown on Figures 3-2 to 3-6 are based on the regressions shown on Figure 3-1, and therefore do not include variability for a given river flow condition.
104	1,2,3	C	What does Figures 6-8 really mean? In Figure 6-8 an estimate of the fraction of Resuspended PR Silt has been presented…it may be staring me in the face but it is unclear what this corresponds to or how the calculation is constructed (depths/timescales). Ultimately all sources of sediment are from outside the basin if not from shore erosion (not considered here and I believe much of the area has hardened shoreline). What is the conceptual model behind this calculation? I think, but am not entirely sure, that most sediment deposited in the lower River has been eroded and re-depositied *[sic]* many times prior to net deposition. This latter point is addressed in the report in Figures that I'm not sure whether I follow. Should I infer from this that deposition of material in the lower Passaic is dominated by primary settling of what can be far upstream or downstream derived sediment with little subsequent resuspension, or that there is not much communication between RM reaches with respect to local resuspension events (i.e., very fast settling rates compared to advection). My understanding of this is important in my interpretation of what the model is computing**– I'm confused on this matter and would like clarification**. While interpretation of Figures 6-11 and 6-12 seem easier to understand, it may be that some of the same questions I have about Figure 6-8 apply to these figures as well.
104		R	Figure 6-8 shows the relative contributions of seven source categories to net cohesive sediment accumulation in each grid box at the end of the simulation of water year 1998, summarized in half-mile increments along the LPR. The results show that between RM13 and RM2.5 deposition of resuspended sediment represents the single largest source of deposition among the 7 categories considered in the one-year simulation. Although not evident in this figure, transport of resuspended LPR silt among reaches is significant, due to the distance of the tidal excursion. Figure 6-8 also shows that solids passing over Dundee Dam are distributed throughout the river, and represent a more substantial fraction of deposition upstream of RM13, where estuarine circulation is less significant. It is noted that the relative fractions of deposition from upstream and downstream (i.e., entering from Newark bay) would be different if the analysis were performed for a year with substantially higher or lower flows.
105	1	C	Assumptions concerning wind driven resuspension outside the Passaic. Resuspension is only affected by the flow and tide driven hydrodynamic model. It would be difficult to include wind waves in the calculation, and I agree that neglecting this should be a very good assumption in the lower Passaic despite sometimes shallow depths, because of the high baseline turbidity, very strong riverine and tidal currents, and lack of fetch. However, it might be worth noting that all of these factors/assumptions are less valid in Newark Bay, because of increased fetch, much lower average current velocities and because baseline suspended solids levels are so much lower. Whether it is important or not I don't know, but not including wind driven wave induced resuspension in Newark Bay would lead to a model with less lateral redistribution, less exchange of contaminants with a water water *[sic]* column that is more open to boundaries with low contaminant levels, and would perhaps underestimate estuarine transport of suspended solids from Newark into the lower Passaic. I note that Dr. DePinto has also brought up this issue in his review. I've not looked at the bathymetry or sediment type maps in Newark recently - from my own limited sampling in Newark Bay in years past, I know that significant shallow areas are dominated by relic red clays that won't erode, but wonder if there are not shallow depositional areas that may be especially vulnerable to wind associated resusupension?? I doubt that many sediment transport models in estuaries explicitly account for wind, but I bring up this point anyways.
105		R	The reviewer raises a point that was considered by the modeling team during model development. Analyses were performed to assess the significance of wind-waves on bottom shear stresses, based on recorded wind speed and direction data; these will be documented in the final modeling report. The analyses showed that wind-wave generated shear stresses high enough to resuspend bottom sediments were limited to shallow near-shore areas. Wind conditions that generate shear stresses high enough to resuspend bottom sediments in near-shore areas were generally limited to times when the wind direction was generally aligned with the long axis of the bay. For cross-bay wind directions, the fetch is too limited to generate substantial wind-waves. These analyses indicate that wind-waves do not have a significant effect on solids exchange with the FFS area and therefore wind-waves were not included in the FFS modeling. Wind-waves may be included in the modeling for the Newark Bay Study Area RI/FS.

106	1	C	Lower Passaic River Contaminant Fate and Transport Model I have touched already on some of my primary concerns, which ultimately are driven in part by a lack of full insight into why remediation of only RM0-8 with approaches employing a cap result in such amazing reductions in this area, in spite of source areas both up-river (argued to be small in surface area, especially when considering fine grain sediments), and the fact that "new" sediments moving through these regions will become somewhat contaminated during transport..… I have also already commented on the fact that because of potentially very highly contaminated relic layers that may be exposed by erosion, there is the chance of exposing small but still quantitatively important hotspot surfaces. Below I comment on the organic carbon/matter model, issues related to benthic communities and choice of biological mixing rates, the merits of equilibrium partitioning approximations, concerns about how initial boundary conditions are set, features of the model results I find somewhat troubling, and whether there are better ways to present and interpret the data. Organic carbon/diagenesis models. As discussed in the conference call, I really don't think the organic matter fate model is necessarily appropriate or useful. I hope that this model does not really matter that much and suspect that it may not especially for high Kd/Koc contaminants that are not greatly affected by outputs of the sediment diagenesis model – on the other hand, for Cd and Hg, outputs of the model such as AVS, oxygen, and sulfate reduction rates affect in some manner the sediment "preservation", inter-compartmental transfer, transformations (methylation of Hg), or water column scavenging or whatever mechanism is responsible for getting low Kd Cd into sediments. These carbon models are based on conceptual models and calibrations that have derived and been much better calibrated for eutrophic estuaries where there is less light limitation and productivity responds to nutrients, and are much more marine with respect to the ecosystems modeled (e.g., the Chesapeake and Long Island Sound Studies). They have been applied in CARP, although I'd argue they might not be particularly appropriate for many areas considered there including the low chlorophyll high turbidity Hudson River. The lower Passaic is an extremely turbid, highly light limited, largely riverine/freshwater ecosystem where these models can not be expected to translate in many regards. Allochthonous sources of carbon (perhaps including detritus afftecting *[sic]* sediment TOC depending on whether sediments were sieved) rather than primary productivity must be much more important than the model is likely predicting, although estuarine transport of Newark Bay generated primary production may be locally important especially near the mouth of the lower Passaic.
106		R	Prior to its use as the basis for the CARP model, the SWEM model was calibrated to data within the Passaic River- Newark Bay complex. This data included local measurements including nutrient concentrations, light attenuation, and sediment flux data from within the LPR. The model does also include allochthonous sources of organic carbon from heads of tide, the boundaries at the Kills, treatment plants that discharge to the Kills and the Hackensack River, combined sewer overflows, and storm water. The water column POC within the LPR is generally dominated by these external sources including the POC generated by primary productivity above the Passaic River head-of-tide. Primary productivity above the Passaic River head-of-tide in Dundee Lake was also one of the targets for a phosphorus Total Maximum Daily Load (TMDL; a Clean Water Act program) for the Upper Passaic River based on chlorophyll-a levels (NJDEP, 2007). The relative importance of the various sources of organic carbon varies by location within the system. Additional figures comparing the outputs of the carbon model to the available data will be incorporated into the final modeling report.
107	1,2	C	The model predicts sediment TOC (not particularly well outside the RM 0-8, which I assume is because the model was modified to optimize calibration to data in this area) and DOC within factors of a few, but these parameters don't vary much in real world fine grain sediments or many riverine/estuarine water columns (except in cases of hyper-eutrophication or raw sewage inputs) or major rivers in time or space, so these don't seem very useful as calibration tools – of course if one wanted to calibrate the model one would want to calibrate against things that are model sensitive like nutrients, oxygen, sulfate/sulfide, redox depths in sediments, or chlorophyll, etc. For contaminant partitioning and exchange it is important though that TOC/POC/DOC are close to reality and sufficiently high. I do ask the question as to whether or not water column POC/foc is predicted sufficiently well – there must be data. If there is too much primary productivity in the model as I might worry about, there is potential for POC/foc to be too high and affect the transfer of contaminants into water. Work in places like the Hudson indicates that water column foc should be near to that of the local bed as particles are dominated by suspended particles. I would like to see a comparison of what data is available and computed POC (foc is more telling as a direct comparison). This would be a better test of the model as different model assumptions I suspect could lead to divergent foc predictions; furthermore if the model is computing water column foc values that are much higher than in the bed, the local equilibrium assumption will lead to greater rates of exchange from the bed to water column.
107		R	Additional figures showing model foc compared to the available data will be added to the final modeling report.

108	1,2	C	Assumption of biological mixing rates and depths of mixing. There are concerns that I and others have raised in the first two conference call and in prior reviews of Passaic River modeling about the sediment benthic community. My original concerns were related to my insights into the types and existence of biological communities in high sedimentation environment in the NY/NJ complex. While I don't have the proper literature to cite, it is well known that in estuarine setting that highly disturbed sediments with high rates of erosion or deposition do not support later successional communities that tend to bioturbate to deeper depths. Rather opportunistic species of small polychaetes (e.g., capitellids of which I think only one is measured in the Passaic, or spionids), amphipods, and small bivalves and gastropods dominate if they establish at all. According to Bob Aller (personal communication), in seminal papers on deposits in the subtidal Mississsipppi *[sic]*, there is no evidence of bioturbaation affecting sediment structure at deposition rates above 4 cm and clearly significant areas of the Passaic have often experienced this rate of accumulation at least in the past. I do not understand the basis for the estimates of mixing rates and depths reproduced in the Report from papers by Boudreau (I have downloaded for free his 1997 book from Boudreau's website showing the same figures but have not obtained the original source of data in those figure), because the figure captions indicate that biological mixing rate estimates at high sedimentation rate were estimated based on 210Pb when at those high sedimentation rates it would not be possible to get mixing rates or depths uniquely or usefully from that tracer. More important to this discussion I have now had an opportunity to review the Draft document of the Spring and Summer 2010 Benthic Community Survey Data of the Lower Passaic River Study Area dated January 31, 2012. While the data summaries only break out in part very shallow (2 feet below MLW as I recall) and deeper sediments (more important to the model) and sandy vs *[sic]* muddy sites (more important to the model),it is clear that benthic community abundances in the lower reaches of the River that have salt are very low, especially in deep fine grain sediments and the species richness is also very low (Robert Cerrato, Stony Brook University, personal communication for both points) - however there are communities present and regularly found throughout the area in both fall and early summer surveys throughout the lower Passaic. Estuarine species of polychaetes are indeed found over the very lower region of the River (approximately RM 0-5). But it is clear that freshwater communities dominate above RM 5. Cerrato agrees with me that the down River communites *[sic]* are both low in abundance and characteristic of Phase 1 opportunistic early successional communities, along with a couple surface predators. These communities and organisms mix only to shallow depths as I had feared. Much more importantly for this study is the observation that in the rest of the River above RM 5 benthic communities are characterized by freshwater assemblages dominated most often by oligochaetes (which definitiely *[sic]* do not mix deeply)– I take exception with the report making this demarcation at RM 8.5 between marine and freshwater – it is clearly changing around RM 5. Years of bioturbation measurements and modeling in freshwater systems (especially in the Great Lakes) indicate that mixing depths should not be more than a couple to a perhaps a few cm (often only 2 cm but lets say 2-5 cm);thus while the present work has done a sensitivity analysis doubling bioturbation depths to 20 cm, instead there should be a sensitivity test done to determine the effects of reducing mixing depths by 2 to 4 fold for most of the Passaic – The rates and depths of bioturbation can have a variety of important effects on the model under different conditions. I will not argue that the biological mixing rates are too high, although I think this is likely true for the more estuarine RM 0-5 region in the case of muddy deeper sediments given the low abundance data, but I argue there is strong evidence against mixing to 10 particularly over the largely freshwater or slightly brackish reaches of the lower Passaic. Deeper depths of mixing either below RM 5 or 0 would be considered extreme upper estimates but the need to reduce the depth is more clearly indicated above RM 0-5.
108		R	It is acknowledged that there is uncertainty associated with the depth of mixing specified within the near-surface sediment-bed of the contaminant model. The assigned value of 10 cm is reasonable, with both theoretical and site-specific empirical support. Jumars & Wheatcroft proposed (1989) that the bioturbation depth represents a balance between resource acquisition, niche specialization and the increasing energy costs associated with deeper burrowing. Following on this, Boudreau developed a simple resource-feedback model for bioturbation (1998) based on food availability and carbon reactivity (lability), and derived a mixing zone depth estimate of 9.7 cm, which is remarkably close to the global mean of bioturbation effects attributable to deposit-feeding organisms in marine sediments previously derived based on a compilation of over 200 data points (Boudreau, 1994; mean 9.8 cm, std.dev. 4.5 cm). The site specific empirical support includes sediment profile imaging (SPI) studies conducted in the LPR (Germano & Associates, 2005) and Newark Bay (Diaz, 2005), and benthic macroinvertebrate community data from both Newark Bay (Diaz, 2005) and LPR (Windward, in prep.). The "biologically active zone" (BAZ) was quantified based on evidence of biological activity (e.g., burrows or feeding voids) observed in the Newark bay SPI images, and ranged from 8 to 20 cm. Apparent redox potential discontinuity (aRPD) depths were also quantified from the Newark Bay SPI images, and these averaged 2.9 cm, and ranged from 2.1 to 3.9 cm (exclusive of one 5.5 cm value). Although the BAZ was not quantified in the LPR SPI study, the aRPD depth in the LPR averaged 1.6 cm (range of 0.1 –4.0 cm) at brackish water stations and 1.9 cm, (range of 0.4 – 5.0 cm) at tidal freshwater stations (Germano and Associates, 2005). Collectively, these depths are somewhat lower, on average, but still comparable in magnitude to aRPD depths in Newark Bay sediments. It is noted that absent bioturbating organisms, the aRPD is likely to be only a few millimeters (mm) thick while when they are present, the aRPD depth may be on the order of several centimeters (Germano & Associates, 2005). The benthic community data collected to support the RI/FS for the LPR (presence/absence) and literature information on feeding biology and microscale habitat requirements of indigenous organisms was also reviewed in order evaluate the reasonableness of the depth of mixing. The dominant organisms identified in sampling locations located within the brackish portion of the LPR are primarily polychaetes and oligochaetes; whereas oligochaetes, amphipods and insects predominate in freshwater benthic samples (Windward, in prep.). Dominant taxa are summarized in Table 1.

Table 1. Dominant Organisms Observed in 2010 Benthic Macroinvertebrate Community Survey (Windward, in prep.).

Species	Order	% w/i Order	Feeding Guild
Brackish Habitat			
Marenzelleria viridis	Polychaeta	48	Surface detritus/deposit feeders; lives in vertical mucous-lined burrows up to 35 cm deep; anoxic fecal pellets reported
Hobsonia florida	Polychaeta	30	Surface deposit feeder, using retractable tentacles to pick food particles at water boundary; live in mucous lined tubes that project obliquely above sediment surface
Heteromastus filiformis	Polychaeta	7	Subsurface detritivore; head down deep deposit feeder ingesting anoxic sediments 10-30 cm below surface
Limnodrilus hoffmeisteri	Oligochaeta	83	Small thin (up to 5 cm long) surface deposit feeder; feeds head down in tubes; typical burrows 2-10 cm
Fresh Water Habitat			
Limnodrilus hoffmeisteri	Oligochaeta	75	Small thin (up to 5 cm long) surface deposit feeder; feeds head down in tubes; typical burrows 2-10 cm
Quistadrilus multisetosus	Oligochaeta	17	Small thin (up to 5 cm long) surface deposit feeder; feeds head down in tubes
Gammarus sp.	Crustacea	96	Epibenthic detritivores or predators
Chironomus sp.	Insecta	49	Burrowing detritivores that rarely found deeper than 5cm; mean single burrow length (7 cm).
Procladius sp.	Insecta	29	Omnivores found in shallow (1-2 cm) depths; mean single burrow length (2.5 cm).

Feeding guild information from Fauchald & Jumars, 1979; Word, 1980; Karichhoff & Morris, 1985; Hare et al., 1994; Charbanneau & Hare, 1998.

The review of the foraging behavior and feeding guilds associated with the dominant benthos are consistent with the SPI observations in the LPR estuarine sediments. Species such as *Maranzelleria viridis* and *Limnodrilus hoffmeisteri*, which dominate the polychaete and oligochaete fauna in benthic samples, are considered opportunistic species that can quickly colonize exposed habitat (i.e., Stage I). These species, along with *Hobsonia florida*, are primarily considered "surface" feeders; although the oligochaete feeds in a head down ("conveyor-belt") position and combined with typical densities observed has the potential to move substantial sediment mass to the surface. The maximum feeding depth of tubificid works, such as Limnodrilus spp., typically range from 2 to 10 cm (Karichhoff & Morris, 1985. *Heteromastus filiformis*, a capitellid polychaete, is considered a "deep" subsurface feeder and is likely a major component of the Stage III (or Stage I/Stage II on Stage III) location communities identified in the Germano SPI study. These organisms feed (also in a head down position) to depths reaching 30 cm (Fauchald & Jumars, 1979). The dominant freshwater organisms include oligochaetes (including *L. hoffmeisteri* and *Quistadrilus multisetosus*), amphipods (i.e., *Gammarus* sp.) and dipterans (*Chironomus* spp; *Procladius* sp.). Charbonneau and Hare (1998) discuss burrowing behavior in aquatic insects; chironomid larvae burrows are typically found at sediment depths ranging from 2 to 10 cm whereas the omnivorous *Procladius* sp. is found at shallower depths.

The presence of Stage III seres in the LPR along with presence of the deep burrowing *H. filiformis* (brackish sediment and the numerically abundant *L. hoffmeisteri* (throughout) and *Chironomus* spp. (freshwater sediment) support 10 cm as a conservative bioactive zone depth for the model. Certainly, few species or individuals are likely to burrow deeper into LPR sediments; however literature reports that most of the dominant species could occur through the upper 10 cm.

In addition to the preceding site-specific data, data from a number of other settings supports the reasonableness of a biologically active zone of 10 cm (Iannuzzi and Standbridge, 2005). Their review summarized 22 studies that evaluated a BAZ depth or range of BAZ depths. The BAZ depths ranged from 3 cm to 50 cm, with most depths between 10 cm and 20 cm. This same review included the results of Boudreau (1994, 1998), who proposed a "worldwide mean" BAZ depth of 9.8 with a standard deviation of 4.5 cm.

			Also, from a practical perspective, the selected depth is close to the depth (i.e., 15 cm.) assumed in the FFS Baseline Ecological Risk Assessment (BERA) which, not coincidentally, is also the depth over which the majority of surficial sediment samples were collected for chemical analysis. Model sensitivity simulations will be performed to evaluate the effect of a shallower depth of mixing in the LPR on simulated contaminant results for the remedial alternatives, including MNR.
109	1,2	C	Equilibrium partitoning *[sic]* assumptions. I may disagree with some fellow reviewers, but I am quite comfortable with the equilibrium portioning assumptions that are employed, although it seems clear from the literature that Koc estimates for Cd are too low (Koc = 1000 - is Cd a COC? and if it is I should comment much more on its geochemistry and what the model might be saying – its distributions will be controlled not only by anthropogenic loads but largely by salinity as water column Kd s strongly affected by chloride complexes, and how sulfidic surface sediments are as Cd is known to be scavenged by sulfidic sediments); the Koc of 100,000 for Hg would also under-estimate water column sorption when most field data show measured Kd values of approximately the same order. Furthermore, Kd values for more soluble mono- through tri-CBs are likely somewhat low in the model as what is preserved in these highly dynamic environments likely is dominated by a more resistant fraction of these compounds. In aggregate, for the more hydrophobic organic contaminants, the Kd's predicted from the Koc values provided are reasonable with respect to being consistent with estuarine field measurements, especially after they are interpreted with respect to three phase partitioning that affects distributions defined by filtration. Just as importantly, the importance of slow desorption kinetics become less important in situations where the fraction sorbed is very high at equilibrium; i.e *[sic]*, very turbid waters and very high Kow compounds (see Wu and Gschwend,, I believe 1986). Finally, because of absence of much in the way of longitudinal gradients further minimizes the fraction of contaminant that needs to desorb as the aqueous phase is "buffered" but contaminant loading into upstreams and downstream waters. Where slow desorption can be expected to become more important is where susepended *[sic]* loads are low and where there are longitudinal or vertical gradients in the dissolved phase – i.e., where the dissolved phase becomes a significant sink for resuspended contaminants. For example, equilibrium approximations may become somewhat more tenuous in my opinion as one moves into the main body of Newark Bay; more worrisome is whether the rates of decreasing Hg over time in the reaches farthest removed from the mouth of the Passaic are being overestimated because of desorption to water driven by a the low Kd computed – because there are similar declines for even more hydrophobic DDT residues, there may be other explanations related to sediment transport and boundary conditions that are not very evident. The Kd predicted from a Koc for total Hg may be too low and could be affecting relatively rapid predicted loss from Newark Bay sediments. I would like to see better justification for the use of such a low Koc. I have not taken the time, but if requested could provide literature on Kds for total Hg that are much higher.
109		R	The noted cadmium partition coefficients (and mercury values) were incorrectly tabulated in the draft report. This will be corrected in the final report. Also note that Cd is not a COPC for the FFS. The K_{OC} values used in this study were based on those from CARP which were based on field measurements of the dissolved and particulate contaminant concentrations in the water column paired with the associated POC and DOC values. Those values should reflect sorption kinetics to the extent that the distribution of the particulate and dissolved concentrations in the field would reflect the net impact of dissolved concentration, particulate concentration and the rate of transfer between the two phases. Note that the K_{OC}'s used are, on average, about an order of magnitude greater than K_{OW}'s (draft carbon and contaminant modeling report, Table 3-7), and the greatest deviation between K_{OC} and K_{OW}, nearly 2 orders of magnitude, is for Mono-CB. This reflects the deviation of partitioning behavior for the more soluble compounds noted in the comment. The solids in the more dynamic portions of the system would generally be exposed to the water column dissolved contaminant concentrations frequently and would approach equilibrium with the water column concentration over the course of time. The contaminant initial conditions in Newark Bay sediment will be reevaluated based on carbon normalization. Sensitivity analyses for contaminant boundary conditions at the Kills will be performed and mass balance figures will be generated.

110	1,2,3,4	C	Setting of initial sediment boundary conditions. I was not able to completely follow the rationales, criteria, and methods for setting the initial boundary conditions for contaminants in surface sediments over the different reaches of the lower Passaic; e.g. the variable degree to which late 1995 data is incorporated, and generally discounted for RM 8-17 is presented but the critieria *[sic]* used in decision making not completely clear to me. More worrisome is what becomes apparent when examining Attachments 2 and 4, where it is clear that Newark Bay initial boundary conditions are set based on criteria unknown to me (don't think I missed it) that often doesn't fit any one of the average time point concentrations. More often than not the preponderance of measured Newark Bay data is underestimated by the model, sometimes dramatically; this is of concern because it affects recent and especially future sources that might affect RM 0-8. The model is also generally predicting a greater drop in concentrations over time in the RM -1.5 to 5.5 reaches than is often apparent in the data, or seems physically reasonable based on expectations from other very hydrophobic compounds (e.g., most of the six DDT residues drop almost 90% over 17 years in RM =- 2.6 – -5.5) – and not supported by most of the data that generally shows little change in average concentration over the calibration time period. It would be interesting to know what has driven the drop over time for contaminants such as DDT residues, Cd and especially Hg as mentioned above; for the metals this may be the results of low computed Kds. I would like to gain more insight into criteria for how these downstream surface sediment concentrations are inititally *[sic]* set in the model runs. There is quite poor fit of the model to sometimes extensive amounts of data collected in Newark Bay (Attachment II) that carry over into what may be less than acceptable predictions into the future for different alternative remedial action scenarios.
110		R	The text describing how sediment initial conditions were determined will be improved to give greater details about the approach used. The entire 1995 data set was used in developing sediment initial conditions. The extent of the 1995 RI data set covered the section of the river from RM1-7. Additional datasets were incorporated to supplement the 1995 RI dataset. The reviewer's statement that the 1995 data were "generally discounted for RM8-17" is not accurate. Available data in the RM7-17 reach for the 1995 time period are too sparse to use as the basis for assigning initial conditions; the only dataset available with extensive coverage above RM7 was the 2008 Low Resolution Coring dataset. Refer to the response to comment 20 for a discussion of the approach used to address the very sparse data available for computing initial conditions upstream of RM7, and the alternate plans for the final model runs. Data from around the time period of 1995 were also sparse south of RM1 and into Newark Bay. As stated in response to comment 88, Newark Bay initial conditions will be revised based on spatial interpolations of carbon-normalized data. The time series plots of the reach averages for Newark Bay are particularly subject to spatial biases in the sampling data given the extensive area of the Bay and relatively few data points. This caveat will be discussed further in the final modeling report. The model results shown in Attachment II reflect this bias. The model results shown are the area-weighted average concentrations for the entire reaches, while the data shown are arithmetic averages of data falling within each reach, regardless of location. The model initial conditions shown on these figures were developed by taking spatial averages of the initial condition data displayed on the same figures, and the differences between the two are the result of the spatial bias in the location of the data, and the way the data were distributed over the area of the reach.
111	1,2	C	Points on calibration data and interpretations. In response to charge questions below, I make a few points about what would ideally be preferable for calibrations (e.g., ongoing work on water column data; sediment property normalized sediment concentratons *[sic]*; contaminant, suspended solids concentrations and organic carbon normalized suspended solids comparisons with the bed). As it is, the comparisons with ranges or averages of surface sediment concentations *[sic]* are not taken very seriously, and the x-y plots for surface sediments or sediments of all depths are shotguns, where success is based on factor of five error frequencies…it is not discussed that there are very often systematic biases in these plots exceeding the factor of 5 "acceptance level" when one looks farther out into Newark Bay away from the well sloshed lower River. Could not insights and results from dated high resolution cores and the carbon normalized surface sediment distributions as a function of space and time not be brought in to inform or constrain interpretations of surface sediment data and modeling results??
111		R	The reach-average time series plots are a very high-level summary, and they should not be given too much weight when determining model performance. The values represented on these figures are, however, the values that will be used for the future risk assessment analyses, and show the big picture of the differences between the alternatives. The fraction of data falling within a factor of five, noted in the report, is not an acceptance level, but is instead a value chosen to give the reader an idea of how the model is performing in a quantitative fashion. Excluding the two most extreme points, the 2008 surface sediment TCDD data, within the lower 8 miles of the river, vary over a factor of about two hundred, and for the entire dataset vary over more than a factor of ten thousand. Some of the "shotgun" comparisons noted in the reviewer's comment are related to variability in the data at a scale that the model cannot capture. For example, the RM10.9 data span about six adjacent model grid cells but the concentrations vary by approximately two orders of magnitude or more, and can vary by more than an order of magnitude within a single grid cell. An example of this would be 2,3,7,8-TCDD in the reach from RM8-17 shown in the top left panel of Figure 4-16 in the draft carbon and contaminant modeling report,. The RM10.9 data vary over more than three

			orders of magnitude within a continuous area of about 6 acres. At the scale of the individual grid cells, the horizontal bands of cyan squares represent locations where a single model grid cell contains a large range of data, which can vary by more than a factor of ten. The intended use of the model is to predict average surface sediment concentrations over relatively large areas. Given the heterogeneity of the river and the large amount of variability in the data over fairly small areas the model can not be expected to reproduce the level of variability observed in the data. The final modeling report will also incorporate figures showing the comparison of model results to data on an organic carbon-normalized basis to provide further insight into the model's behavior.
112	1,3,4	C	The ephemeral bursts in COC concentrations over capped materials. The other troubling aspects of the remedial action scenario projections is that following erosion events there are sometimes sharp blips in the sediment concentrations in the RM0-8 region, but these concentrations dissipate with characteristic times perhaps less than a year. The only explanation for this that I can come up with is that the contaminant clean cap gets dusted with deposited contaminated sediment and then it is swept out of the area by subsequent resuspension and lateral exchange processes (erosion)…
112		R	Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report.
113	1	C	There is a detailed and what I believe to be near state of the art sediment transport model that has an unusual amount of calibration data – many aspects of the calibration can be described by the model, although I have questioned some potential biases between measured and modeled data that affect contaminant behavior and potentially the conceptual site model, that may not have received sufficient treatment in the report. Unfortunately this particular site and set of remedial action scenarios are arguably more highly dependent on sediment transport than which would be the case at many other sediment contaminant remediation sites that are either less energetic, involve less heterogeneity, or involve remedial action on all high concentration potential source areas, as opposed just the lower RM 0-8 source area. In my experience sediment transport models are generally considered less predictive than chemical contaminant fate and transport models – so while the sediment model is a major strength of this work, the predictions demand very careful scrutiny and I have made several comments and observations related to whether or not it is adequately predicting erosion and the importance of net deposition, and how that might impact contaminant concentration projections in the model.
113		R	Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report.
114	1	C	I do not care for the organic carbon model for many reasons. However, with the exception of mercury and perhaps cadmium (which may not be a COC??), where sulfate reduction rates, oxygen, and AVS become important outputs of the model, it is not clear to me how application of the present model will dramatically affect the model results. I would need to understand more about how carbon flows, fate of carbon associated with new loads of suspended sediments, and how carbon is conserved between suspended and sediment particles and particle sizes to make definitive conclusions about whether the organic carbon model really effect the contaminants. As s long as sediment TOC is reasonably well described, and there is a reasonable amount of DOM to further minimize volatilization losses, it may be that the fate of hydrophobic organic contaminants is appropriately accounted for in the model; knowing what is happening with water column foc of suspended solids however would provide more insight into the model behavior and whether there are predictions that could bias contaminant fate predictions. I have pointed out that the carbon model is based on conceptual models and calibrations from eutrophic estuaries where there is less light limitation and are much more marine. The lower Passaic is an extremely turbid light limited, largely riverine ecosystem where I would be very surprised if these models can apply in many regards. Allochthonous rather sources of carbon (perhaps including detritus) rather than primary productivity must be much more important than the model is likely predicting, although estuarine transport of Newark Bay generated primary production may be locally important.
114		R	The carbon model and the sediment flux sub-model are important for mercury, cadmium (not a COPC), and the hydrophobic organic contaminants. The carbon concentrations, both particulate and dissolved, can have a significant impact on the transport of contaminants in the system. The carbon model is important, not only for the information used by the metals model, but also for predicting the change in surface sediment organic carbon over time after the placement of sand cap and backfill material. In terms of the relationship between POC and solids, it is assumed that, effectively, all of the POC is associated with the cohesive size class from the sediment transport calculation (size class 1). The POC is transported using the same flows, settling velocities and resuspension rates as the cohesive particles in the sediment transport calculation. The more labile forms of POC are subject to decay both in the water column and sediment which can influence the fraction relative to the cohesive solids. In addition there is primary productivity, although not generally a significant amount, which generates additional POC.

			The impact of POC production and decay within the model domain does not feed back into the sediment transport calculation, but the concentration of the POC generated and lost within the model domain is small relative to the total cohesive sediment concentration. Prior to its use as the basis for the CARP model, the SWEM model was calibrated to data within the Passaic River-Newark Bay complex. The calibration data were collected from locations within the Passaic River, Hackensack River, Newark Bay, and the Kills and included measurements of nutrient concentrations, light attenuation, and sediment fluxes. In addition to primary production, the model does also include allochthonous sources of organic carbon from heads of tide, the boundaries at the Kills, treatment plants that discharge to the Kills and the Hackensack River, combined sewer overflows, and storm water. The water column POC within the LPR is generally dominated by these external sources, including the POC generated by primary productivity above the Passaic River head of tide. Primary productivity above the Passaic River head of tide in Dundee Lake was also one of the targets for a phosphorus TMDL for the Upper Passaic River based on chlorophyll-a levels.
115	1	C	There are concerns about the sediment benthic community raised above and how it relates to the estimates of biological mixing rates and especially depths. The benthic community data do suggest active communities that exist in very low abundance in deeper fine grain areas of interest, but that they are dominated in the lower reaches (approximately RM0-5) by opportunistic polychaetes and other small species or predators that are not generally deep mixers, and entirely by freshwater assemblages dominated by oligochaetes (which do not mix deeply) above approximately RM 5. Years of bioturbation measurements and modeling in freshwater systems the Great Lakes indicate that mixing depths should not be more than a couple to a perhaps a few cm (2-5 cm); thus while the present work has done a sensitivity analysis doubling bioturbation depths to 20 cm, instead there should be a sensitivity test done to determine the effects of reducting mixing depths by 2 to 4 fold; I really believe this could be important, especially since the model is not projecting anywhere near historical net sedimentation rates.
115		R	Refer to response to comment 108 for a discussion of the depth of mixing and sensitivity simulations planned to evaluate the effect of a shallower depth of mixing in the LPR.
116	1,2	C	As for the contaminant fate modeling, the results are appropriately conservative in that they don't include biodegradation. I do not share some of my colleagues concerns about equilibrium partitioning assumptions both for reasons presented in the Report related to sensitivity of the model to raising Kd/KOC and organic carbon content of sediment, and for a combination of other reasons detailed in the discussion above. Where I do have concerns are with the apparently low Kds that would be predicted for Cd (Koc 1000) and what I assume is total analytically defined Hg (Koc 100,000); based on our most recent call, evidently the report has not fully detailed how metal partitioning has been treated and this needs further exploration. The Kd's predicted from these values are lower than the field data I've seen over the years and this is very important for Hg in this work. It is known that Cd is primarily in the dissolved phase in estuaries but sorption is much stronger at low salinities due to less important chloride complexes – which apparently is not accounted for …..Cd distrubutions would be difficult to describe because of strong scavenging in sulfidic sediments and seasonal releases back to water of part of it on a seasonal cycle. However, measured Kd (not Koc) values measured in the field are still over 1000. If Cd is in fact an important COC to model, there needs to be much more discussion of the role of particulate transport, partitioning and the role of AVS in both protecting sorbed Cd or scavenging it from the water column. For the organic contaminants, the Kd's predicted from the Koc values provided are reasonable with respect to being consistent based on estuarine field measurements operationally defined by filtration.
116		R	The noted cadmium partition coefficients (and mercury values) were incorrectly tabulated in the draft report. This will be corrected in the final report. Also note that Cd is not a COPC for the FFS. Additional details about the metals models, based on additional work done on the mercury model after the CARP project was completed, will be incorporated in the final modeling report. This additional work was done to address issues raised by the CARP peer review so that the mercury model could be used for the development of a mercury TMDL for NY/NJ Harbor. The complexation of Cd and Hg with both chloride and AVS are considered in the model. Note that cadmium is not a COPC for the FFS.
117	2	C	I have emphasized how impressed I am with data assimilation and interpretation related to calibration of the sediment transport model. I do not understand how different particle sizes are moved around and accounted for in the model (for one example, coupling between the erosion model and armoring and how that carries forward to different parts of the model). Thus I don't know if there is anything that can be done to compare sediment grain size distributions computed and measured in the field.
117		R	Comparison between computed and measured grain size distributions will be presented in the final modeling report.

118	1,2	C	With respect to the organic carbon model, I am unimpressed by calibration with sediment TOC, or water column DOC; perhaps I should be. I would be interested in seeing what the model is doing with respect to computing fraction organic carbon on suspended particles, and assume that at least some data exists for such comparision *[sic]*. Experience from the Hudson suggests that foc should be very close to that in bedded sediments – I expect that a model with important primary productivity would produce higher POC/foc. If there is indeed very poor comparison between measured and modeled water column POC, the model results might be questioned as it might mean greater rates of exchange between the bed and water column, although it may not be that simple.
118		R	Additional figures comparing simulated foc to available data will be added to the final modeling report.
119	2,3	C	The effort placed on setting initial conditions was massive, but more discussion is merited with respect to criteria for setting initial concentrations both upstream (e.g., whether to use 1995 data or not or how to possible adjust 2008 data), and much more emphasis should be placed on how initial conditions were set in Newark Bay or nearby Hackensack River sediments. Attachments II and IV clearly illustrate how poorly initial conditions and later conditions fit observed data in sediments in Newark Bay reaches. This is not adequately addressed in the main Report and may become critical when computing later the effects of Newark Bay sources to the capped areas following remediation.
119		R	The text describing how sediment initial conditions were determined will be edited to provide greater details about the approach. The entire 1995 data set was used in developing sediment initial conditions. The extent of the 1995 RI data set covered the section of the river from RM1-7. Additional datasets were incorporated to supplement the 1995 RI dataset, but the only dataset available with extensive coverage above RM7 was from the CPG 2008 LRC program. Data from around the time period of 1995 were also sparse south of RM1 and into Newark Bay. As stated in response to comment 88, Newark Bay initial conditions will be revised based on spatial interpolations of carbon-normalized data. The time series plots of the reach averages for Newark Bay are particularly subject to spatial biases in the sampling data, given the extensive area of the Bay and relatively few data points. This caveat will be discussed further in the final modeling report. The model results shown in Attachment II reflect this bias. The model results shown are the area-weighted average concentrations for the entire reaches, while the data shown are arithmetic averages of data falling within each reach, regardless of location. The model initial conditions shown on these figures were developed by taking spatial averages of the initial condition data displayed on the same figures, and the differences between the two are the result of the spatial bias in the location of the data, and the way the data were distributed over the area of the reach.
120	2	C	The comparisons of the model to actual measured field data are very unsatisfying given the effort put into this exercise. Furthermore, the report lacks the insight generating level of interpretative description of data that is abundant in the sediment transport calibration discussions. Part of the reason for this is that the model is much more complex and dependent on variables in potentially non-intuitive ways. However, more effort could have been put into finding outputs or testing parameters (more than modest changes in parameter sensitivity) to provide such insight into model behavior. It is not clear whether the best and most consistent approaches were used for setting the initial boundary condition in surface layers, both with respect to upstream regions where 1995 data is not weighed very much and in Newark Bay as already mentioned. Based on the wonderful carbon normalized figures we were given as part of the Charge documents it is disappointing that it was not deemed useful (or possible?) to reduce local variability in concentrations with normalization to carbon (or iron or aluminum if available). It is also noteworthy that the results from high resolution dated cores were not used to help present the conceptual model, as tests in model calibration, or as insightful tools to assist in data presentation and interpretation.
120		R	The modeling report will be edited to provide further insight to the approaches used, and additional graphics will be incorporated to provide greater insight into the results. The sensitivity simulation results presented in the report represent only a small portion of the analyses done over the process of model development. Those incorporated into the report were the most informative about model behavior. Additional figures will be added to the report incorporating comparisons between simulated and measured organic carbon-normalized contaminant concentrations. As noted previously sediment data from the reach between RM7 and 17 were very sparse and not extensive enough to develop initial conditions in that portion of the river. This was the reason for developing initial conditions based on the 2008 data set, which was the only available dataset with extensive coverage above RM7. The suite of models does not compute iron or aluminum concentrations; therefore normalization of model results to these parameters is not possible. These approaches have been considered as part of data interpretation analyses done as part of the FFS (Focused Feasibility Study Report – Appendix A: Data Evaluation Report No. 2.). The high resolution core data were used for the purposes of model-data comparison for sediments, both at the surface and at depth. In addition, as part of advancing the site conceptual model for the FFS, the high resolution core results were used to understand the history of contamination and the recovery of the river with time (Focused Feasibility Study Report – Appendix A: Data Evaluation Report No. 3). The

			high resolution cores were also used as part of an empirical evaluation to assess the uncertainties in future surface sediment concentrations in the river (see Appendix C of the FFS).
121	2,3	C	We have not been presented a comparison between modeled water column data and measurements; it is mentioned that such a comparison is now possible and is underway. If the data set is adequate, it would be a much better test of the model than anything that has been presented in sediments at this time. I have commented above that don't believe that the comparisons between modeled and measured sediment concentrations has been presented and interpreted at a level commensurate with the effort involved or the importance of the questions. It would be useful to know how the carbon normalized data fits the model over the calibration period and whether there is enough Fe or Al data to be used for similar normalization. If there are concerns about the early organic carbon data, that can be stated – but not to show it I believe is a mistake.
121		R	Figures representing the comparison between recent measured water column data and model results will be incorporated into the final modeling report. It is noted that this data can vary by an order of magnitude or more over fairly short time periods over the course of the tide, and spatially from within the ETM to outside it, and from the LPR to Newark Bay. Additional figures will be added to the modeling report incorporating comparisons between model-predicted and measured organic carbon-normalized contaminant concentrations. The model does not compute iron or aluminum concentrations, so such a comparison would not be possible.
122	4	C	I don't know and have focused much of my review on this question. There are some simple things that can be done to help us understand why contaminant levels remain so low relative to proximal areas and why occasional spikes in concentration are dissipated as quickly as they are. If most of this is because of low net burial on the then it needs to be acknowledged. Sediment transport models are useful research tools. It is not clear that they are sufficient to answer this question with high enough confidence to make such large expenditures on remediating only the 0-8 mile area if the necessary criteria is to achieve high levels of exposure reduction in the FFS area.
122		R	The future contaminant trajectories are influenced by the rate of sediment accumulation, which affects the rate of recontamination. Independent of the models, data analyses (Remedial Investigation Report for the Focused Feasibility Study of the Lower Eight Miles of the Lower Passaic River) indicate that contaminant concentrations in sediment and biota are not declining rapidly enough to expect that the MNR alternative will result in acceptable ecological and human health risk levels in any reasonable time period, and therefore the models provide an important tool for comparing active remedial alternatives. It is acknowledged that contaminant concentrations computed in the Deep Dredging alternative are likely biased low because the computed degree of infilling tends to result in less recontamination than would likely occur in response to the increased water depths. The Full Capping alternative is affected by this bias to a much lesser extent, and understanding these biases, it was judged that the models could be used to distinguish among the alternatives. Additional analyses were done to revise the sediment transport parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report. It is important to note that while the FFS is evaluating remedial alternatives for only the lower eight miles of the LPR, a RI/FS of the full 17-mile LPR is underway and remediation of areas above RM8 is being evaluated. Remediation of the mudflat at RM10.9 has begun.
123	1,5	C	With the likely need for additional work, this is a good set of models that I believe are well structured, especially for recalcitrant hydrophobic chemicals where description of redox chemistry is less important than it potentially is for Hg and even Cd. It is clear to me that the model can be used as "one tool" for evaluating remedial alternatives. If I were charged with making expensive management decisions based only on this model, I would have to say today let's wait for more information to be provided and incorporate additional targeted model testing into decisions that may not need to wait very long.
123		R	As indicated in the response to the preceding comment, additional work, including model testing, has been and is being performed to improve the modeling analysis and complete the FFS. The modeling team believes that these efforts are consistent with the reviewer's suggestion for additional work, and agree with the reviewer's conclusion that the models can be used as "one tool" for evaluating remedial alternatives.
124	1	C	Settling Speeds The most significant factor affecting the transport of cohesive sediments in the overlying water is the flocculation (aggregation) of the basic individual particles (typically a few micrometers in diameter) into flocs whose diameters are often tens to several hundred micrometers and which can be as much as several centimeters. The sizes and densities of these flocs affect their settling speeds (and subsequent deposition) by as much as several orders of magnitude. Flocculation and its effects are not considered in the LPR modeling, not even qualitatively, but should be.
124		R	The cohesive solids settling velocity formulation used in the LPR model follows the general pattern of settling velocities calculated with the flocculation formulations of Farley and Morel (1986) and Winterwerp (1998) within the range of fluid shearing rates occurring in the LPR. While not explicitly calculating floc formation the effect of variations in solids concentration on floc settling velocities is represented in the LPR model formulation.

125	1	C	Settling Speeds In the modeling, comments are made about hindered settling. This is a separate factor and is only significant at large sediment concentrations (larger than those typically observed and modeled in the LPR). At low to moderate sediment concentrations, hindered settling has little to do with flocculation or the description of settling speeds of cohesive sediments.
125		R	The comment regarding hindered settling applies to the formulation describing the settling of unflocculated or disaggregated fine particles and is followed by the following statement: "This settling velocity is essentially constant at 0.2 mm/s over almost all of the relevant range of observed suspended sediment concentrations."
126	1	C	Settling Speeds Experiments and theoretical analyses concerned with the flocculation of cohesive sediments are summarized in Lick (2008); references to the more detailed literature are given there. Experiments and analyses quantitatively demonstrate the factors (with emphasis on sediment concentration, fluid shear, and salinity) which affect flocculation and especially the sizes, densities, and settling speeds of the flocs. A relatively complete and quite accurate time-dependent model of flocculation is described. Since the inclusion of this in a water quality model is quite time-consuming, a simpler quasi-equilibrium model (Eq. 4.50) is also given; this equation describes the floc diameter, d, as a function of the sediment concentration, C, and fluid shear, G. Experiments clearly show that (a) floc diameter decreases as sediment concentration increases, (b) floc diameter decreases as fluid shear increases, (c) floc diameter decreases as salinity increases, and (d) settling speeds decrease as floc diameter decreases. For constant fluid shear (although this is not the case in the LPR), this indicates that settling speeds decrease as sediment concentration increases. In contrast, the LPR model ignores all physics and assumes a completely empirical model for settling speeds where settling speeds are only a somewhat arbitrary function of sediment concentration (Fig. 2-4 of App. BII) and are not dependent on fluid shear or salinity. The results shown in Figure 2-4 seem to be in complete disagreement with any experiments or analyses. A purely empirical model with no supporting physics gives little confidence in the ability of the transport model to predict. A better determination of settling speeds as a function of sediment concentration and fluid shear is needed. The dependence of floc size and settling speed on salinity is relatively weak and can probably be ignored for this application. Even though empirical parameters are probably needed for calibration, the correct functional dependence of settling speeds on sediment concentration and fluid shear should be retained.
126		R	The modeling team respectfully disagrees with the reviewer. There are two fundamental reasons for our position. First, while the experimental results and analyses of flocculation reported in Lick (2008) are undoubtedly correct for the conditions under which they were derived, there are many other interpretations of relationships between particle size, settling velocity, concentration, and shear in the literature (e.g., Krone 1963, Dyer 1989, Kranck et al. 1993, Teeter 1993, and many more). Most of these interpretations describe a relationship between settling speed and concentration that is qualitatively consistent with the one used in the LPR model, where the settling speed first increases with increasing concentration then decreases at high concentrations. Reported relationships with turbulent shear are similar, but were not included in the LPR model because they tend to be similar in nature to the concentration relationships and because concentration and shear are correlated; high concentrations occur under energetic conditions. The relationships described in Lick (2008) are similar to the others in the high concentration and shear range, but do not describe increasing size/settling velocity in the low-moderate concentration range. While the semi-empirical formulation used in the LPR model is not identical to any particular relationship from the literature, it is informed by common understanding from the literature. Second, the settling velocity relationship used for fine sediments in the LPR model is not intended to describe the behavior of a homogeneous population of particles that flocculates and breaks up in approximate equilibrium with local shear and concentration, as advocated by the reviewer. As described in section 2.7 of the sediment transport modeling report, the slowly settling particles that dominate at both very low concentrations (during slack tides) and at very high concentrations (during large runoff events) represent background, non-settling particles at slack tide and wash load at very high river flows, respectively. The tidally resuspended and deposited rapidly settling particles are dominant in the range of concentrations typical of resuspension/deposition events, and represent observed behavior under these conditions reasonably well. It is possible that these different particle behaviors actually represent different particle types, but because there is no data on particle properties segregated by in situ size or settling speed, there is no basis for further differentiating the fine particle population in the LPR model. Other approaches (multiple fine particle classes, for example) might have been chosen, but these approaches have their drawbacks as well. The modeling team spent considerable time considering the fine sediment settling velocity problem, and we believe that the compromise solution we developed is reasonable and represents observed behavior well under most, though not all, conditions. It is noted that the flocculation model described in Lick (2008) was tested as part of the modeling performed for the Housatonic River GE site. As discussed in the Responsiveness Summary to the modeling peer review (Weston Solutions, 2006), "…the flocculation model performed well under idealized conditions, i.e., without deposition and resuspension, when the residence time of the model cells is relatively

			short. This suggests that the flocculation model is calculating a sequence of steady-state conditions consistent with the constant concentration assumption inherent in the model formulation. However, the flocculation model failed under conditions of unsteady flow with resuspension and rapidly increasing suspended solids concentration, conditions that are obviously inconsistent with the constant concentration assumption."
127	1,2	C	Consolidation After deposition, sediments consolidate with depth and time; this consolidation and associated changes in sediment bulk density have a major influence on erosion rates as a function of depth and time. The model of consolidation for depositing sediments as initially discussed in the LPR report assumes a sediment quasi-equilibrium profile, Eq. 2.17, and a time-dependent approach to this equilibrium in a first-order manner, Eq. 2-18. This may be true in certain idealized cases, but it is not correct in most consolidation scenarios. As the LPR modelers realize, this model does not fit the experimental data for a consolidating LPR sediment core. This is shown in section 3.2.7.2 and in Fig. 3-37. In particular, the sediments in the consolidation experiments had lower erosion rates and higher critical stresses than the LPR Sedflume cores that they were meant to represent. The LPR modelers then ignore the experiments and parameterize consolidation with little reference to any physics. Bed consolidation is discussed in section 4.6 of Lick (2008); experiments with real sediments and analyses of these experiments are given. The bed density as well as other parameters were measured and are given as a function of depth and time. The most significant governing parameters are (a) the type of sediment, especially fine-grained versus coarse-grained sediments, (b) the depth (thickness) of the depositing core, (c) gas production and concentration, and (d) the sediment base on which the depositing sediments were deposited.
127		R	Without additional information or references no response can be directed to the reviewer's general statement, "This may be true in certain idealized cases, but it is not correct in most consolidation scenarios." The consolidation model fits the consolidation data quite well, as seen in Figure 27 in Attachment 1 to the sediment transport modeling report. Given the good model fit of the consolidation data, the reviewer's comment, "As the LPR modelers realize, this model does not fit the experimental data for a consolidating LPR sediment core." is interpreted to refer to the inconsistency in erosion properties in the field cores versus the consolidation cores. This observation is discussed in Attachment 1 to the sediment transport modeling report. Sedflume analyses of field cores showed substantial variability across the site and even in pairs of cores collected at the same anchoring location (Figure 22 in Attachment 1). In the modeling analyses presented in the report, the erosion properties of fine-grained sediment areas were parameterized with the analysis of the field cores, as detailed in Attachment 1. The consolidation rate derived from the consolidation experiments was used; however the erosion rates derived from the field cores were used to parameterize the erosion rates in the depositional layers to maintain consistency between the erosion properties of the parent bed and depositional layers. Additional simulations are underway with sediment erosion parameterized based on the consolidation experiment results, constrained with an *n* value of 2.0, and these results are showing more infilling than previous results.
128	1,2	C	Consolidation Figure 3-39 indicates that the core used in the LPR consolidation tests was 40 to 50 cm in depth; this is too thick and not representative of depositing, consolidating sediments in the LPR. No sediment base was used in the experiments. The appropriate experiments should have been done with core depths of approximately one cm or less (deposition due to tidal forcing) and additional experiments with core depths of a few centimeters (representing longer term deposition, especially in near-shore areas and in the dredged navigation channel which is present in several remedial alternatives). Results with these short cores would have been dramatically different from those with 40 to 50 cm cores.
128		R	The modeling team respectfully disagrees with this comment. The reviewer states that the consolidating core depths were far too long, since deposition of this amount of sediment in one event is highly unlikely. While the latter point is correct, running consolidation experiments similar to ours is common practice because it strikes a good balance between practicality and information. Running replicate consolidation experiments with different thicknesses of initial deposition overlying natural sediment bases, with sufficient cases to derive a reliable relationship between deposit thickness and consolidation rate, would have been far too costly and time-consuming. While this would indeed be an interesting research study, it is beyond the scope of the LPR effort. Furthermore, Sedflume erosion experiments are not practical with a very thin layer of very erodible sediment overlying a less erodible base, since the entire layer is likely to be eroded as one mass due to pressure artifacts in the Sedflume channel (SEA Engineering, Inc., personal communication).
129	1,2	C	Consolidation Another factor not considered in the experiments or modeling is the base on which the sediments were deposited. Sediment densities are strongly influenced by the water, gas, and fine particles in the core and their transport vertically due to consolidation processes, hence the dependence of sediment density on depth, time, and the thickness of the core. The base on which the sediments are deposited influences the density (and erosion rate) of the depositing layer because of the vertical transport of water, gas, and fine particles from the base into the depositing sediment layer. This effect can be quite large (Lick 2008, section 4.6) but was ignored in the LPR experiments and modeling.
129		R	The reviewer's comment about natural core bases and additional water transport through the consolidating core contradicts both the previous comment and standard Sedflume practice. If pore water being squeezed out of a natural core base was a significant issue, it would mean that the natural core base itself was consolidating. However, standard Sedflume practice assumes that the erosion characteristics measured for any given core at any given depth are constant in time – i.e., that the sediment has ceased consolidating. Furthermore, using a long initial consolidation core, as was done in the present case, simulates the effect of pore water transport from depth slowing consolidation near the surface more than the use of any natural sediment base.

130	1,2	C	Consolidation Another factor not considered in the analysis of the consolidation experiments or in the LPR model was the effect of gas generation and transport in the base and in the depositing sediments. Gas is normally present and is significant in areas where contaminated sediments (and high organic content) are found, e.g., in the LPR. In UCSB consolidation experiments with sediments containing gas where sediment parameters were carefully measured, especially sediment density and concentrations of gas, it was demonstrated that sediment density first increased with time (as would be expected in the absence of gas) but then slowed and subsequently decreased with time due to gas production and transport, eventually reaching a slowly-changing, almost quasi-steady-state.
130		R	The reviewer's comment about gas generation affecting erodibility and potentially explaining some of the differences between the consolidation cores and the in-situ erosion cores is insightful and potentially valid. However, it would introduce far too much complexity into the LPR model if considered completely, requiring modeling of anaerobic diagenesis within the sediment transport model, or simultaneous modeling of water quality and sediment transport, in addition to significantly more experimentation. Indeed, gas generation may have introduced an artifact into the in situ Sedflume erosion experiments, and thus help to explain their high erodibility relative to the consolidation cores. The in situ sediment cores were not eroded for several days following collection, during which time there were anecdotal (but unquantifiable) observations of gas bubble formation in some cores.
131	1,2	C	Consolidation The above two factors would explain much of the discrepancies between the LPR consolidation experiments and LPR in situ cores. Valid experiments and analyses of consolidation are necessary for the long-term prediction of sediment transport. A more thorough investigation of sediment consolidation is needed, especially in regard to big events and the infilling of the proposed navigation channels.
131		R	The modeling team believes that the consolidation approach adopted was a reasonable approximation that captured the most important effects for the LPR model within the scope of the overall effort. Addition of the effects of consolidation for depositing layers in SEDZLJ in a computationally reasonable way was a major effort early in the LPR model development, and a significant step forward. We think that the limited consolidation experiments conducted with LPR sediments were a reasonable way to derive erosion parameters for consolidating deposited layers in the LPR model; not exhaustive, clearly, but sufficient for our purposes and more than is typically done.
132	1,2	C	Dependence of Erosion Rates on Shear Stress In all previous experiments and analyses of the dependence of erosion rates, E, on shear stress, t, done by UCSB researchers, it was determined that E was proportional to tn and that n was approximately 2. In addition, when I analyzed a few cases from the Housatonic (where it was reported that n was 2 to 4), I also found that n was approximately 2. I haven't had time to properly analyze the LPR cores and results, but I suspect that n's of 3 and 4 are not correct. Higher n's would primarily affect the relative amounts of erosion between average events and big events. Erosion rates are also an extremely sensitive function of sediment bulk density, a factor not considered in the modeling.
132		R	The 2005 Sedflume field data are clearly characterized by a high degree of variability and have undergone extensive analysis by Sea Engineering, Inc. (SEI), a group with roots going back to UCSB. SEI staff involved in the analysis of the LPR Sedflume data have performed approximately 35 Sedflume studies involving analysis of over 330 cores. The values of the exponent, *n*, which relates erosion rates to shear stress, averaged 2.6 with a standard deviation of 0.4 and a range of 1.9 to 3.3 in layers that eroded during the simulation. Additional simulations are underway with sediment erosion parameterized based on the consolidation experiment results, constrained with an *n* value of 2.0, and these results are showing more infilling than previous results. The reviewer's statement that sediment bulk density is not considered in model's description of erosion is not correct. As shown in Equation 2-19 of the sediment transport modeling report, bulk density is explicitly included in erosion from layers of deposited sediment. Bulk density is not included in the calculation of erosion of the existing (parent) bed, based on the assumption that the parent bed has already consolidated.
133	1,2	C	Density measurements To understand and quantify the process of consolidation and the dependence of erosion rate on shear stress and sediment density, accurate measurements of sediment density as a function of depth in the core and time are required. The usual wet-dry procedure (used in the LPR experiments) is not sufficient. It is not sufficiently accurate and can not determine gas concentrations since the wet-dry procedure essentially eliminates gas in the core because of the mixing inherent in the procedure. A much better procedure, which does not have these limitations, is the method using the density profiler developed at UCSB (Lick 2008, section 2.5.1). This profiler accurately and effectively measures densities as a function of distance and time. Together with the wet-dry procedure, it can also determine gas concentrations as a function of distance and time. It should be used in any future work. It would alleviate and probably eliminate the above two problems (consolidation and dependence of erosion rates on shear stress and sediment bulk density) as well as contribute information about the benthic boundary layer.

133		R	At the time of the LPR Sedflume study, attempts were made to incorporate the UCSB density profiler; however, logistical issues, associated with availability of UCSB staff and certification requirements for a non-UCSB substitute, caused the work to be done without the density profiler.
134	1	C	Grid sizes In the LPR, the hydrodynamic and sediment transport grid is too coarse to adequately describe the lateral variations in the sediment dynamics in the LPR, especially in and near the previous and proposed navigation channels which typically have rather steep sides where slumping and rapid erosion can occur. This is further complicated because the grid size in the contaminant transport model is different (coarser longitudinally by a factor of three) from that in the sediment transport model. Consistent with the above study, this indicates that averaging sediment erosion/deposition over the contaminant grid will decrease the variability of sediment mixing due to resuspension/deposition and may even eliminate it.
134		R	Refer to the response to comment 94 for a discussion of grid resolution. Sediment mixing due to erosion and deposition is a significant process in the contaminant fate model. The particle mixing within the top 10 cm of the sediment bed can be thought of as representing the effects of bioturbation and sub-grid scale variations in erosion and deposition. Addressing random small-scale bed variability in bed properties (especially horizontal variability) in sediment transport models is at the cutting edge of research, but it is beyond the scope of the LPR modeling effort. When stochastic variability in bed properties has been considered, it has been found to smooth out transitions in average behavior (e.g., van Prooijen and Winterwerp 2010). The modeling team does not think that increasing horizontal grid resolution would address small-scale sediment variability satisfactorily, nor do we think that unresolved small-scale variability in bed properties and flow will affect the predictive capabilities of the LPR model at larger scales. However, at this point in time the issue remains an unresolved uncertainty of all sediment transport modeling.
135	1	C	The benthic boundary layer In the LPR model, it is assumed that a 10 cm thick benthic layer exists; mixing coefficients and a sediment-water transfer coefficient are also assumed. These assumptions are based on previous modeling studies where sediment dynamics (erosion, deposition, transport), generally the largest factor in mixing sediments, was ignored. Because sediment dynamics was ignored, something was needed to mix the sediments. By default, a benthic layer with empirical coefficients was invoked. In some cases, an active benthic layer may be present, but it is not present or necessary in all cases. Before invoking a benthic layer due to benthic organisms, it should be demonstrated that sediment mixing by organisms is present and is significant, i.e., a benthic layer does exist. Parameters from out-dated models where sediment resuspension/deposition was ignored or minimized should not be used.
135		R	Refer to response to comment 108 for a discussion of the depth of mixing and sensitivity simulations planned to evaluate the effect of a shallower depth of mixing in the LPR.
136	1	C	The final decision on the remediation of the LPR (i.e., where and how much to dredge and cap) will depend on results similar to those in Figure 6-3. These results are primarily dependent on sediment dynamics and the forcing of this dynamics by the hydrodynamics. As a first (and very good) approximation, it can be assumed that highly hydrophobic contaminants sorb and stay with the sediment particles; LPR sensitivity experiments demonstrate this. It follows that, in order to determine results as in Figure 6-3, what is needed is a hydrodynamic model, a sediment transport model, and a simple contaminant transport model where the contaminant is completely sorbed to the sediment particle. It also follows that a complex carbon model and complex chemical fate and transport models are not needed. As a simple but reasonably accurate carbon model, it may be assumed that carbon may vary from one size particle to the next, but carbon always stays with the particle.
136		R	The organic carbon model provides results used in partitioning calculations, which is important in evaluating bioavailable fractions of contaminants in risk assessment calculations, as well as calculations of sulfate reduction in sediments that is used in the calculation of mercury cycling in sediments.
137	1	C	In the reports, many other contaminants besides TCDD are mentioned. However, the highly hydrophobic chemicals (such as TCDD) will tend to sorb and stay with the sediment particles while the less hydrophobic chemicals will tend to desorb and be transported away in the overlying water. In this way, the most hydrophobic chemicals are the base for a worst-case scenario. Because of this, results such as those for TCDD in Figure 6-3 will probably be the major influence on the determination of the appropriate remedial action. If the determination of the appropriate remedial action for the LPR is the major purpose of this project, then calculations of the transport and fate of all other chemicals are not needed. These latter models may be interesting from a scientific and academic point-of-view, but they are not necessary for this project.
137		R	The reviewer is correct in pointing out the importance of TCDD in the evaluation of remedial alternatives, however in order to follow EPA guidelines, risk associated with other contaminants must be included in the evaluation. Depending on the selected remedy, future risk may be controlled by contaminants other than 2,3,7,8-TCDD, and therefore must be included in the evaluation.

138	1,5	C	In order to demonstrate this, it would be informative to do a large storm calculation with, and without, carbon and complex chemical fate and transport models. To some extent, this has already been done; and it has been demonstrated that the amount of carbon doesn't matter (section 5.3) and increasing the partition coefficient to keep more of the chemical with the particle doesn't matter (response on conference call).
138		R	For the reasons stated in the two previous responses, the recommended demonstration will not be conducted.
139	1	C	The elimination of all these sub-models would greatly decrease the required computational time and the time to develop and calibrate these sub-models. In turn, the sediment transport modeling and the experiments needed to more accurately determine sediment parameters (especially settling speeds and consolidation of cohesive sediments) could be done more accurately.
139		R	For the reasons stated in the three previous responses, this suggestion is not feasible. It is noted, however, that the computational resource requirements of the sediment transport model far out-weigh those of the carbon and contaminant models.
140	2	C	(a) Consolidation experiments were not done correctly or analyzed properly and did not lead to meaningful results. Additional consolidation experiments and analyses should be done in order to improve the predictive modeling of sediment dynamics. (b) I believe the dependence of erosion rate on shear stress is incorrect and should be re-investigated. This would improve sediment transport predictions for big events.
140		R	Refer to responses to comments 128, 129, and 131 for a discussion of the consolidation experiments and why additional consolidation experiments are not being considered as part of the FFS. Refer to the response to comment 132 for the discussion of the dependence of erosion rate on shear stress.
141	3	C	As discussed in 1, the grid sizes for the hydrodynamic, sediment transport, and contaminant transport models should be reduced and should be the same in order to eliminate averaging problems. The description of the other processes mentioned in 1 should be improved.
141		R	While finer grid resolution would be desirable, the increase in computational requirements, especially of the sediment transport model, would make the simulation impracticable for evaluating risk thirty years after completion of the remedy. Refer to the response to comment 134 for a discussion of issues related to increased grid resolution.
142	1,3	C	A major problem, that to me remains unresolved, is the deposition, infilling, and subsequent consolidation of sediments in the proposed navigation channels. Figure 6-3 indicates that the model (and the associated discussion in the report) does not predict rapid infilling. This is curious since historically there was rapid infilling of the previous navigation channel during its life and after dredging was stopped; this infilling is the essential basis for the present problem of contaminated sediments in the LPR and therefore needs a better quantitative understanding than there is at present. In order to adequately answer questions 3, 4, and 5, the model (with a fine grid but over relatively short periods of time, and with no, or at least a very simple model of contaminant transport) should be used to demonstrate (a) the rapid infilling of the previous navigation channel; this should be done for average and big event conditions in order to demonstrate understanding, and (b) the infilling (or not) of the proposed navigation channels, again for average and big event conditions.
142		R	Additional effort is underway which is resulting in an increased rate of infilling in the sediment transport results. It is noted, however, that the rate of infilling that occurred when dredging of the LPR ceased was likely greater than would occur if the LPR were deepened today, because of the substantially deeper navigation channels in Newark Bay. The deeper channels in Newark Bay result in lower shear stresses in the bay, which would result in a decrease in the solids transport from the bay to the newly deepened river, compared to what occurred historically. Refer to the response to comment 134 for a discussion of issues related to increased grid resolution.
143	1,4	C	The overall results shown in Figure 6-3 (and similar results discussed elsewhere in the LPR reports) seem to be quite robust and insensitive to changes in most parameters (but see discussion above). Some of this apparent robustness depends on the mathematical averaging of the contaminant concentrations over the top 15 cm; a more detailed presentation should include surficial concentrations of contaminants, e.g., the top 1 or 2 cm where many organisms reside. These latter concentrations will probably appear somewhat different and greater than the 15-cm average, and will also be more sensitive to changes in parameters. A presentation and discussion of this would be helpful.
143		R	The reviewer is correct that concentrations calculated in the top 1 or 2 cm would respond differently than concentrations averaged over the top 15 cm. The use of a 15-cm exposure depth is, in part, an EPA policy decision intended to be conservative with respect to protecting ecological and human health risk. Refer to response to comment 108 for a discussion of the depth of mixing and sensitivity simulations planned to evaluate the effect of a shallower depth of mixing in the LPR.

144	1,3,4	C	The contaminant concentrations in RM 8-17 seem to be more variable and more sensitive (less robust) to parameter changes. The results seem to indicate that some dredging and capping should be done in this area (from the conference call, this seems to already have been decided). Where and how much to dredge and cap in this area, and the order of dredging (first upstream or downstream, etc.) seems to be a more sensitive issue and deserves more accurate modeling.
144		R	The issues raised by the reviewer have been receiving the attention of EPA. Sediment removal at RM10.9 has begun. Sediment removal at other locations outside the area of the FFS will be evaluated as part of the 17-mile RI/FS. To address the issue of whether dredging should begin upstream or downstream, model simulations were performed to evaluate the contribution to deposition on top of a cap from uncapped areas (Section 6.2 of the sediment transport modeling report). These analyses indicate that beginning remediation of the lower eight miles before addressing the reach upstream of RM8 would be more effective. This makes intuitive sense since the surface area and volume of contaminated sediment in the RM0-8 reach is so much greater than above RM8. Approximately 85 percent of the surface area and 90 percent of the volume of the fine-grained sediment in the LPR is within the study area of the FFS.
145	5	C	For the LPR, the largest recorded event had a maximum flow rate approximately twice that used in the LPR modeling. Very approximately, the bottom shear stress is proportional to the square of the flow velocity, the flow velocity is an increasing function of the flow rate but not quite proportional to it, and the erosion rate is proportional to the square of the shear stress (or possibly more). The amount of sediment erosion, deposition, and transport is a function of the erosion rate, but this rate is modified by bed consolidation as a function of depth and time and by bed armoring. Nevertheless, estimates such as this (also see comments by Ambrose) indicate that sediment dynamics is a very nonlinear and rapidly increasing function of flow rate. Large storm events will also lead to large amounts of deposition and nonlinear effects on bed armoring, flocculation, and settling speeds. All of these will modify the erosion, deposition, and consolidation of the sediment bed during and after the storm in a manner not adequately modeled in the LPR. Calculations of sediment dynamics during a 100-year flow event (or similar big event) are needed with special emphasis on sediment deposition and consolidation in the proposed navigation channels. A relatively fine grid is needed in these calculations because of the rapid changes in topography due to dredging and the proposed navigation channels.
145		R	The impact of storm events will be further incorporated into the modeling effort by adding the 2011 and 2012 water years in the end of the calibration period. This period includes Hurricane Irene, which was a 1 in 75-year return event on the Passaic River. In addition, a sensitivity run for a 1 in 100-year return event, including three subsequent years, will be added to the analysis to determine the impact of an extreme event on the computed trajectories under the various remedial alternatives. Refer to the response to comment 134 for a discussion of issues related to increased grid resolution.
146	1	C	As discussed, the major processes affecting the results shown in Figure 6-3 (and therefore the choice of the appropriate remedial action) are sediment dynamics and the hydrodynamics forcing this dynamics. As far as contaminant dynamics is concerned, the approximation that the highly hydrophobic chemicals completely sorb to and stay with the sediment particles is sufficient. With this approximation and the hydrodynamic and sediment transport models, an LPR model should be able to very accurately reproduce the results shown in Figure 6-3. No complex carbon model and no complex fate and transport models are needed. Not even the presence of a benthic layer (or its absence) is required.
146		R	For reasons mentioned in preceding responses, the modeling framework will continue to include components necessary for providing input to ecological and human health risk assessments.
147	1	C	1) Starting with the now-familiar Figure 6-3 of Appendix BIII – a) I find the response to No. 1 on the “Midpoint Teleconference Matrix (2013 03 06)” less than satisfactory. The derivation of a “fixed” MNR half life (i.e. one incorporated as part of the future projections without consideration of error or uncertainty) of 17.9 years from the 1996-2010 data seems absurd. Any reasonable consideration of the standard deviations of the “upper 15 cm average” data presented might even include the possibility of an increase in 2,3,7,8 TCDD concentration with time!
147		R	This comment was discussed during the final peer review call, resulting in comment 163. A half-life was not specified as input to the model, rather a half-life was calculated from the model results for the purposes of discussing the results over various portions of the projection period. The model results in question include the net effect of the concentration impacts of external loadings, and modeled physical and chemical processes included in the sediment transport, carbon, and contaminant models, which are then averaged over the top 15 cm of sediment and the lower 8 miles of the river.
148	2,3	C	1) Starting with the now-familiar Figure 6-3 of Appendix BIII – b) To me the reason seems fairly clear, and is related to the “geochemical” differences among the samples that were averaged – grain size, organic carbon content, the amount of time represented by the 15 cm at each site, etc.

148		R	The purpose of Figure 6-3 was to compare future trajectories for different remedial alternatives and the mean and two standard errors of the data from various studies were included to provide context to the trajectories.
149	2,3	C	1) Starting with the now-familiar Figure 6-3 of Appendix BIII – c) With all the ancillary data available on these samples, I am amazed that apparently no attempt was made to "reduce the range" of uncertainty associated with each time point by incorporating some of these differences. Has there even been an attempt to look at the relationship between the concentrations of the different COCs in each sample?
149		R	Other portions of the FFS report present analyses of COPC data (Focused Feasibility Study Report – Appendix A: Data Evaluation Report No. 4)
150	3	C	1) Starting with the now-familiar Figure 6-3 of Appendix BIII – d) The next major problem raised by the figure concerns the "upper 15 cm average." On Figure 3 supplied with the "Midpoint Teleconference Matrix (2013 03 06)", a response to concerns about "Sedimentation rate on Cap," model results for RM 0 to 8 range from net erosion (up to about 5 cm) in some areas to net deposition (up to about 25 cm) in others over the fifteen year simulation. [Note: I'm assuming that Figure 3 applies to post-dredging and full cap emplacement bathymetry.] How this distribution of predicted sedimentation effects the "near zero" long term model prediction "average" concentration was not addressed.
150		R	The final version of the modeling report will incorporate figures showing the additional details including averages over shallower depths, and channel versus shoals, to give a better picture of the distribution of computed sediment concentrations.
151	3	C	1) Starting with the now-familiar Figure 6-3 of Appendix BIII – e) This brings up the usefulness of an "area average." Will, as I suspect, areas of slower net accumulation be the worst in terms of benthic community exposure to any re-contamination? Along these lines, I am still unclear on model predictions regarding the "navigation channel" area (RM0 to 2.2) of the Full Cap alternative.
151		R	The final version of the modeling report will incorporate figures showing the additional details including averages over shallower depths, and channel versus shoals, to give a better picture of the distribution of computed sediment concentrations.
152	1	C	1) Starting with the now-familiar Figure 6-3 of Appendix BIII – f) Concerning the fate of accumulation on the cap – Is it vertically mixed in the model? Are finer particles "allowed" to settle through the sand? How is resuspension of particles deposited on the cap treated?
152		R	The sediment transport model does not mix deposited sediments with the in-place bed. The carbon and contaminant model do have mixing over the top 10 cm of the bed but it is not instantaneous. Erosion rates on top of the cap are computed based on the model formulation used for deposition and consolidating solids if the surface becomes cohesive again, combined with the computed grain size distribution.
153	4	C	1) Starting with the now-familiar Figure 6-3 of Appendix BIII – g) The minimal/near zero predicted long-term re-contamination atop the full RM 0 to 8 cap was questioned at the midpoint teleconference. Reference to model results was provided with northern Newark Bay (and the Hackensack) and upper Passaic being significant particle contributors. This should have been discussed in much more detail and broken down in terms of contaminant sources, concentrations and fluxes.
153		R	Additional analyses were done to revise the sediment transport model parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report. Additional detail for fluxes of solids and contaminants will be provided in the final modeling report. Section 6.1 of the sediment transport modeling report presents results of simulations that track sediment movement from various sub-areas within the model domain. These can be used to infer contaminant transport from different areas. There are no plans to perform comparable simulations to track contaminants as part of the FFS.
154	4,5	C	1) Starting with the now-familiar Figure 6-3 of Appendix BIII – h) From that perspective, the upper Passaic becomes a potentially significant source of mercury and PCBs, especially in high flow events that could scour and transport deeper, more highly contaminated sediments that were identified in well-dated sediment cores collected by my research group as far back as the mid 1980s and as recently as 2005 with Dundee Lake cores Pass 8B and Pass 8BP with analyses partially funded through the Passaic River RI.
154		R	A chemical water column monitoring program is being executed as part of the 17-Mile RI/FS, and this program includes a high-flow event component. Data from that program will be analyzed to assess flow-related effects on boundary concentrations.

155	2,4,5	C	2) This leads to a consideration of the handling of extreme storm events. a) Some background – A recent email refers to the CARP MEG (Model Evaluation Group) review with respect to the Hg model and its review by Joe DePinto and Chad Hammerschmidt. I was a late (and somewhat reluctant) addition to the MEG and my only significant suggestion was that they try to match some real system data by hindcast modeling. Specifically - • transport of PCBs from the upper Hudson to the NY Harbor associated with extreme events in the mid 1970s (a dam removal and a hundred year flood). • The Indian Point release of significant amounts of Cs-137 in 1971. • AND the effect of the extreme Passaic flow of 1984 on the western NY/NJ harbor. • A HydroQual memo of February 14, 2005 began to address all of these hindcast simulations, but only the second was actually modeled to some extent with some success. • The "real system" data applicable to the 1984 Passaic event is summarized in the figure below. Those cores were collected in 1985 and 1986, and published in 1993.
155		R	The main concern with computing a hindcast for any of the COPCs is the uncertainty in the external loading time history, and initial conditions, followed by the computational time and effort that would be required to run those longer term hydrodynamic and sediment transport sequences and the relatively limited data available for comparison once those runs were completed. It is outside the scope of the FFS Peer Review to respond to comments on how CARP responded to the comments of its reviewers.
156	4,5	C	2) This leads to a consideration of the handling of extreme storm events. b) The significance of the 1984 high flow event with respect to the modeling and simulation of "extreme" events is evident when one looks at the flow records from the Passaic River at Little Falls. I do not believe that Figure 1 of the "Midpoint Teleconference Matrix (2013 03 06), supplied by HydroQual provides the most useful perspective. I prefer the following plot of mean daily discharge at Little Falls on the Passaic from 1930 to March 2013 reported by the USGS. c) The 1984 event is associated with significantly higher mean daily discharge (18,000 cfs) than the high flow included in the model simulations – 03/16/2010 (15,600 cfs). The non-linear relationship between flow, bottom shear stress, TSS, particle flux etc. makes this difference potentially quite significant d) IN ADDITION, the real game changer here appears to be Hurricane Irene represented on the right hand side of the plot with peak daily average flow of 20,500 cfs and three consecutive days averaging above 16,500 cfs(!). e) The significance of Irene is further emphasized by the 2010 to 2011 bathymetric change maps distributed a few days ago. What's a difference of a few feet of sediment (some depositing, some eroding) among friends (Sorry, I couldn't help myself.....). I guessing that it was a preliminary look at the Irene bathymetry changes that vanquished the term "quasi-steady state" from the sedimentary regime discussion of RM 0 to 8. [Note: I had to check three times – that scale on the bathymetry change maps is feet as indicated, right?] f) AND, we have not yet seen data related to the effects of Sandy! It is not, in my opinion, at all unreasonable to believe that global warming has a significant role in the recent hydrodynamics of the Passaic and Newark Bay, especially with respect to extreme events that are not well represented in the current model.
156		R	The impact of storm events will be further incorporated into the modeling effort by adding the 2011 and 2012 water years in the end of the calibration period. This period includes Hurricane Irene, which was a 1 in 75-year return event on the Passaic River. In addition, a sensitivity to a 1 in 100-year return event including three subsequent years will be added to the analysis to determine the impact of an extreme event on the computed trajectories under the various remedial alternatives.
157	4,5	C	3) Other aspects of extreme events – a) As mentioned above in 1 h, with deep erosion in an extreme event, our data indicates that the upper Passaic is of concern with respect to re-contamination of a capped area with Hg and PCBs (and probably PAHs as well). The other major sources of mercury in the area identified by our data on dated sediment cores are the Hackensack (Berry's Creek) and the Arthur Kill (possibly associated with smelting at the former National Lead site. With respect to the influence of the Hackensack on Newark Bay (and by tidal extension, the lower Passaic), we have identified and proposed the use of a tracer based on Cr concentrations in dated sediments.
157		R	Information mentioned by the reviewer and supplemental sampling being conducted by the CPG is relevant to the 17-mile RI/FS, however the FFS is coming to a conclusion, and future sampling work is not being contemplated for this project.

158	4,5	C	3) Other aspects of extreme events – b) Without information on the distribution and concentration of Hg in the sediments of these areas, I do not see how the model can hope to simulate the impact of extreme events on sediment-associated contaminant deposition on a cap in the Lower Passaic. Modeling fluxes from these systems to the lower Passaic with data from a station or two near the boundaries does not seem at all adequate.
158		R	See response to comment 157.
159	5	C	3) Other aspects of extreme events – c) With respect to "deep erosion" and extreme events there is real system data to indicate significance with respect to contaminant transport from "otherwise depositional" areas. We have recently identified two separate events, one in the upper Hudson in the spring of 1976 and one in the Mohawk in March 1964 that removed on the order of a foot of sediment from large, otherwise depositional areas. The Mohawk event in 1964 is noteworthy because it was not identified in the average daily discharge data, but only in the "instantaneous" USGS (15 minute) data, as it was likely associated with the breakup of an ice dam. An exceptional resource for insight on extreme events from a water column perspective is Gary Wall of the USGS.
159		R	The reviewer's comment is interpreted as expressing concern that flow inputs to the model for a high flow event need to be specified at a high frequency (e.g. 15-minutes). Flow inputs to the model are specified as hourly averages for the period since 2007 when records became available at Dundee Dam. It is noted that the peak flows for the March 16, 2010 and August 30, 2011 (Hurricane Irene) high flows were only 2 to 3 % higher than the daily average flows on those days. Flow inputs to the model will continue to be specified as hourly averages. With respect to the reviewer's comment about erosion from "otherwise depositional" areas, it is noted that erosion of the sediment bed is computed in the sediment transport model based on computed shear stresses. Erosion from areas which are "otherwise depositional" occurs in the model simulation.
160	5	C	3) Other aspects of extreme events – d) The 2010 to 2011 bathymetric change maps showed, not unexpectedly, nearshore areas of significant erosion and other nearshore areas of significant deposition – although much of the river did have a "very nearshore" (shallow) area that was not color contoured…… Together with the statement in the charge that the navigation channel had been "sporadically maintained" from RM 0 to RM 2 until 1983 and to RM 15.5 through the 1950s reminded me of questions I had been asking since about 1990. e) It has been reported that prior to 1970, dredge spoils (highly contaminated, to be sure) from this area were disposed of primarily as fill in areas around Newark Bay. Do we know where they were put? I expect that this issue will be central to the Newark Bay study. For now, however, it does seem relevant to at least ask if we know of any disposal/fill sites along the lower Passaic, or any in adjacent waterways that could be eroded in extreme events.
160		R	The issue of locations where LPR dredge spoils were used as fill around Newark Bay is relevant to the Newark Bay Study Area RI/FS, but not to the FFS Study Area. No disposal/fill sites along the LPR have been identified.
161	1,2	C	4) Summary a) I am disappointed • with the amount of attention paid to unverifiable detail without significant real system data from the Passaic (e.g. the mercury model); • with the amount of detail that in the model that, by my assessment, extremely poorly represents the real system data (that would be the carbon model); and • by the manner and minimal extent of "incorporation"/consideration of sediment contaminant and compositional data (see comments 1b and 1c above).
161		R	The carbon and contaminant (including mercury) modeling components used in the FFS are based on the CARP study. More detailed documentation than was included in the FFS contaminant modeling report can be downloaded from http://www.carpweb.org/main.html . Revisions to the carbon boundary condition at Dundee Dam were made based on comparisons between simulated and measured sediment POC. Additional adjustments to the carbon model were deemed unnecessary based on sensitivity analyses performed with the carbon and contaminant models. Analyses along the lines suggested by the reviewer are included in the FFS Report (Focused Feasibility Study Report – Appendix A: Data Evaluation Report No. 4).
162	3,4	C	4) Summary b) I believe that the Phase 1 and Phase 2 removal of the most highly contaminated sediment is an excellent start to the overall remediation effort. However, recent extreme events, Irene and Sandy, suggest that any capping project must be extremely well and cautiously designed. From my perspective, removal seems safer than capping, but at some point cost will be prohibitive.
162		R	No response necessary.

163	1,3	C	4) Summary c) The model presentation did not appeal to or satisfy to any significant extent, my geochemical intuition or my first hand knowledge of and experience with the data. Consequently, I do not have confidence in the "no significant long term re-contamination" prediction of the full cap model on an "upper 15 cm, area average" basis.
163		R	Additional analyses were done to revise the sediment transport model parameterization with the goal of improving the agreement between simulated and observed historical infilling. Although not yet complete, these efforts are underway and show improved levels of infilling. The effect of the updated sediment transport on the behavior of the contaminant model in terms of rates of recovery and recontamination will be evaluated in the final modeling report.
164	1,4	C	Comments related to the Final Review Teleconference of 03/20/13 • Apparently my interpretation of the MNR "half life" (discussed in 1a above) was incorrect. It was not derived from the LPR sediment data, but was a model output reflecting those "highly variable" averages at the different time points only insofar as they were used to constrain the local resuspension flux. The other "data" used were the rather poorly constrained boundary conditions (Bruce Brownawell focused discussion on the problems with Newark Bay; some of my comments above question the handling of upper Passaic and Hackensack R contaminant sources).
164		R	The half-life computed from the model results reflects the combined effects of multiple processes on contaminant concentrations in the sediment bed, including partitioning, resuspension, deposition, advection, dispersion and, to a lesser degree, volatilization and diffusive exchange between the bed and water column, as well as boundary inputs. As noted above, additional information provided by the reviewer will be reviewed and potentially incorporated in future work to further constrain the assignment of model inputs.
165	2,3	C	Comments related to the Final Review Teleconference of 03/20/13 • The "highly variable" averages at the different time points in the LPR surface sediment data were the subject of some discussion and considerable consternation. My suggestions (1b and 1c, above) were met with comments about an "unexplained" factor of 2 in organic carbon content in the 1995 samples which apparently precluded any normalization (?). When the possibility of normalization using other ancillary measurements (Fe, Al, grain size) was brought up, we were told that Solomon and Ed Garvey had looked into that with no success. With all due respect, I wouldn't mind looking at and evaluating that data myself to try and uncover the cause(s) of the apparently low quality of the ancillary data so that similar problems might be avoided in the future with respect to Newark Bay and beyond.
165		R	The highly variable organic carbon percentages obtained by the various sampling events are documented in Data Evaluation Report No. 4 (Focused Feasibility Study Report – Appendix A: Data Evaluation Report No. 4). The reviewer's interest in evaluating the ancillary data with the goal of avoiding similar problems in the Newark Bay RI/FS is outside the scope of the LPR FFS modeling peer review process and will not be addressed here.
166	2	C	Comments related to the Final Review Teleconference of 03/20/13 • My discussion of the 1984 high flow event as actually recorded in sediment cores (see 2 above) brought the question from Hydroqual of whether I had an idea of the impact of the flooding of the 80 LA site on the contaminant signal associated with that event. While I did not, I feel it important to point out that the sediment record of the event in the upper Passaic, Lower Passaic, and Newark Bay has barely been exploited. Cs-137 profiles, some selected pp'-DDD analyses, and even fewer selected 2,3,7,8-TCDD analyses comprise the great majority of analyses to date. I would note that all the samples from all the cores discussed in 2 above are part of our sediment archive. Furthermore, "Bopp dried" samples have been checked against wet analyses for dioxins, dibenzofurans, PCBs, Hg, and metals as a "condition" for using data from our recent upper Passaic cores and to provide justification for possible use of our archived samples. This was done in a small project organized by Len Warner (currently with The Louis Berger Group) in 2007 resulting in a Draft Report dated February 2009. I would certainly recommend that additional analyses of our archived samples, including Hg and XRF metals analyses at RPI, be considered as a most logical step toward improving our understanding the impact of the 1984 high flow event.
166		R	It is not anticipated that analyses of the type mentioned by the reviewer will be performed before completion of the FFS.

References

Anderson, P.W. and S.D. Faust, 1973. Characteristics of Water Quality and Streamflow, Passaic River Basin Above Little Falls, New Jersey. USGS Water-Supply Paper 2026.

Boudreau, B.P., 1994. Is Burial Velocity a Master Parameter for Bioturbation? *Geochimica et Cosmochimica Acta*, 58(4): 1243-1249.

Boudreau, B.P., 1998. Mean Mixed Depth of Sediments: The Wherefore and the Why. *Limnology and Oceanography*, 43(3): 524-526.

Chant, R. J., & D. Fugate and E. Garvey, 2010. The Shaping of an Estuarine Superfund Site: Roles of Evolving Dynamics and Geomorphology. *Estuaries and Coasts*, DOI 10.1007/s12237-010-9324-z, September 2010.

Charbonneau, P. and L. Hare, 1998. Burrowing behavior and biogenic structures of mud-dwelling insects. *J. N. Am. Benthol. Soc*. 17(2):239-249.

Connolly, J. P. and R. Tonelli, 1985. Modeling Kepone in the Striped Bass Food Chain of the James River Estuary. *Estuarine, Coastal and Shelf Science*, 20:349-366.

Diaz, R., 2005. Estimation of the Biologically Active Zone, Newark Bay, NJ. Prepared for USEPA, Region II.

DiToro, D. M., 2001. Sediment Flux Modeling. John Wiley & Sons, 2001.

Dyer, K.R. (1989). Sediment processes in estuaries: future research requirements. *J.Geophys. Res*. 94: 14,327-14,339.

Farley, K.J. and F.M.M. Morel, 1986. Role of coagulation in the kinetics of sedimentation. *Environ. Sci. Technol*. 20: 187-195.

Fauchald, K. and P.A. Jumars, 1979. The diet of worms: a study of polychaetes feeding guilds. *Oceanogr. Mar. Biol. Ann. Rev*. 17:193-284.

Germano and Associates, 2005. Sediment Profile Imaging Survey of Sediment and Benthic Habitat Characteristics of the Lower Passaic River.

Hare, L., R. Carignan and M.A. Huerta-Diaz, 1994. A field study of metal toxicity and accumulation by benthic invertebrates; implications for the acid volatile sulfide (AVS) model. *Limnol. Oceanogr.* 39(7):1653-1668.

Iannuzzi, T., and A. Standbridge, 2005. Draft – Literature Review on Biologically Active Zone (BAZ) in Sediments. November 11, 2005 Technical Memorandum to Paul Bluestein.

Jumars, P.L. and R.A. Wheatcroft, 1989. Responses of benthos to changing food quality and quantity, with a focus on deposit feeding and bioturbation. In W.H. Berger, V.S. Smetacek and C. Wefer [Eds.], *Productivity of the Oceans: Present and Past*, Report of the Dahlem Workshop; Wiley-InterScience; as cited in Boudreau, 1998.

Karichhoff, S.W. and K.R. Morris, 1985. Impact of tubificid oligochaetes on pollutant transport in bottom sediments. *Environ. Sci. Technol*. 19:51-56.

Kranck, K., E. Petticrew, T.G. Milligan, and I.G. Droppo, 1993. In situ particle size distributions resulting from flocculation of suspended sediment. *Nearshore and Estuarine Cohesive Sediment Transport*, Vol. 42 Coastal and Estuarine Studies, Mehta, J. ed., Am. Geophys. Union, Washington DC, pages 60-74.

Krone, R.B., 1963. A study of rheologic properties of estuarine sediments. Tech. Bulletin 7, U.S. Army Engineers Committee of Tidal Hydraulic, Vickburg MS.

Lick, W.L., 2008. Sediment and Contaminant Transport in Surface Waters. CRC Press, 2008.

New Jersey Department of Environmental Protection (NJDEP) 2007. Total Maximum Daily Load Report for the Non-Tidal Passaic River Basin Addressing Phosphorus Impairments –PROPOSED. Division of Watershed Management, Trenton, New Jersey.

Tseng, C., P. Balcom, C. Lamborg and W. Fitzgerald, 2003. Dissolved Elemental Mercury Investigations in Long Island Sound Using On-Line Au Amalgamation-Flow Injection Analysis. *Environmental Science and Technology* 37: 1183-1188.

Teeter, A. M., 1993. Suspended Transport and Sediment-Size Transport Effects in a Well-Mixed, Meso-Tidal Estuary. *Nearshore and Estuarine Cohesive Sediment Transport*. A. J. Mehta. Washington, DC, American Geophysical Union**:** 411-429.

Van Prooijen, B. C. and J. C. Winterwerp, 2010. A stochastic formulation for erosion of cohesive sediments. *J. Geophys. Res.* 115.

Weston Solutions, 2006. Responsiveness Summary To The Peer Review Of Model Calibration: Modeling Study Of PCB Contamination In The Housatonic River.

Wilson, T. P. and J. L. Bonin, 2007. Concentrations and Loads of Organic Compounds and Trace Elements in Tributaries to Newark and Raritan Bays, New Jersey. *USGS Scientific Investigations Report* 2007–5059.

Windward, LLC, in prep. Spring and Summer 2010 Benthic Invertebrate Community Survey Data for the Lower Passaic River Study Area - Draft; prepared for Cooperating Parties Group, Newark, NJ; January.

Winterwerp, J.C., 1998. A simple model for turbulence induced flocculation of cohesive sediment. *J. Hydr. Res., IAHR,* 36: 308-326.

Word, J.Q., 1980. Classification of Benthic Invertebrates into Infaunal Trophic Index Feeding Groups.