REPORT ON THE PEER REVIEW OF THE REVISED BASELINE MODELING REPORT FOR THE HUDSON RIVER PCBs SUPERFUND SITE

—Final Report—

Prepared for:

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NOTE

This report was prepared by Eastern Research Group, Inc. (ERG), an EPA contractor, as a general record of discussion for the peer review meeting. This report captures the main points of scheduled presentations and highlights discussions among the reviewers. This report does not contain a verbatim transcript of all issues discussed during the peer review. Additionally, the report does not embellish, interpret, or enlarge upon matters that were incomplete or unclear. EPA will evaluate the recommendations developed by the reviewers and determine what, if any, modifications are necessary to the current modeling effort. Except as specifically noted, no statements in this report represent analyses or positions of EPA or of ERG.

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LIST OF ABBREVIATIONS

BAF	bioaccumulation factor
BMR	Baseline Modeling Report
DEIR	Data Evaluation and Interpretation Report
DOC	dissolved organic carbon
DOSM	depth of scour model
EPA	U.S. Environmental Protection Agency
ERG	Eastern Research Group, Inc.
GE	General Electric Company
LRC	Low Resolution Coring Report
NGVD	National Geodetic Vertical Datum
NYSDEC	New York State Department of Environmental Conservation
PCB	polychlorinated biphenyl
RBMR	Revised Baseline Modeling Report
TEQ	toxic equivalent
TSS	total suspended solids
USGS	United States Geological Survey

EXECUTIVE SUMMARY

Seven independent peer reviewers critiqued the "Revised Baseline Modeling Report" (RBMR) and its Responsiveness Summary, which were prepared as part of the U.S. Environmental Protection Agency's (EPA's) reassessment of the Hudson River PCBs Superfund site. At the end of the peer review meeting, six of the reviewers found EPA's *fate and transport models* to be acceptable with minor revisions, and one reviewer found these models acceptable, but did not classify the necessary revisions as minor or major. Similarly, four peer reviewers concluded that EPA's *bioaccumulation models* are acceptable with minor revisions; one found these models acceptable with major revisions; and the two reviewers who were expert primarily with water quality and sediment transport modeling did not offer recommendations on the bioaccumulation models.

When answering the questions in the charge, the reviewers generally agreed that EPA's models adequately reproduce historical data. However, given that the models do not reflect a fully mechanistic understanding of all chemical, physical, and biological processes, the reviewers had concerns about the uncertainty associated with the models' forecasts. At the close of the meeting, the peer reviewers recommended that EPA take several actions to improve its modeling effort. Following is a summary of the reviewers' recommendations, which are documented in greater detail in Section 5.1 of this report. Specific examples of the reviewers' many other suggested revisions and recommendations can be found throughout this report.

- The reviewers recommended that EPA evaluate two options for obtaining direct evidence to better substantiate the model assumptions and findings related to net deposition, burial rates, and depth of mixing.

- The reviewers recommended that EPA further test the sensitivity of forecast results to the models' horizontal and vertical spatial resolution.

- Concerned that EPA's fate and transport models might significantly underestimate the degree of sediment redistribution, the reviewers gave four specific recommendations for how EPA should address: possible errors in the sediment resuspension algorithm, the assumption of a constant settling velocity, the neglect of non-cohesive bed load, and low spatial resolution.

- The reviewers recommended that EPA verify the solids balance in the Upper Hudson River using total suspended solids data from locations other than Fort Edward and Waterford.

- The reviewers recommended that EPA simulate the effects of a 100-year flood occurring at various times, in addition to during the spring flood of 1998.

- The reviewers recommended that EPA update the constants used in the FISHRAND model with new information available in the scientific literature.

- The reviewers recommended that EPA evaluate three specific concerns regarding the approach used to calibrate the FISHRAND model and provide justification for these aspects of the model calibration.

- The reviewers recommended that EPA investigate the implications of steady-state assumptions on the overall bioaccumulation modeling approach.

- The reviewers recommended that EPA revise its bioaccumulation models to predict how PCB levels vary with age/size classes of fish, particularly for species near the top of the aquatic food web, or provide justification for why this is not necessary.

- Suspecting that the composition of Tri+ PCBs in the Upper Hudson River sediment, water, and fish will change over the 70-year forecast period, the reviewers recommended that EPA run its fate and transport and bioaccumulation models for a subset of PCB congeners with varying chemical and physical properties.

1.0 INTRODUCTION

This report summarizes an independent peer review by seven experts of the following documents the U.S. Environmental Protection Agency (EPA) released as part of its reassessment of the Hudson River PCBs Superfund site:

- The January 2000 “Revised Baseline Modeling Report” (RBMR) (TAMS et al., 2000a)
- The February 2000 “Responsiveness Summary” for the BMR (TAMS et al., 2000b)

To facilitate their evaluations of these reports, the reviewers also were given copies of several additional reports with relevant background information. Section 1.2.2 lists these additional references.

The seven reviewers attended two meetings, both of which were open to the public. The first meeting took place in Albany, New York, on January 12–13, 2000. This meeting included several presentations and a tour of the Upper Hudson River to familiarize the reviewers with the site and its environmental history. The second meeting took place in Saratoga Springs, New York, on March 27–28, 2000. This meeting was the forum in which the reviewers critiqued the above documents. Eastern Research Group, Inc. (ERG), a contractor to EPA, organized the expert peer review and prepared this summary report.

This introductory section provides background information on the Hudson River PCBs Superfund site, the scope of the peer review of the RBMR, and the organization of this report.

1.1 Background

In 1983, EPA classified approximately 200 miles of the Hudson River in the state of New York—from Hudson Falls to New York City—as a Superfund site, because of elevated concentrations of polychlorinated biphenyls (PCBs) in the river’s sediments. The sediments are believed to have been contaminated by discharges of PCBs over approximately 30 years from two

General Electric (GE) capacitor manufacturing plants, one in Hudson Falls and the other in Fort Edward. After an initial assessment, EPA issued an “interim No Action decision” in 1984 for the contaminated sediments of the Hudson River PCBs site.

Since 1990, EPA has been reassessing its earlier decision to determine whether a different course of action is needed for the contaminated sediments in the Hudson River. EPA is conducting this reassessment in three phases: compiling and analyzing existing data for the site (Phase I), collecting additional data and using models to evaluate human health and ecological risks (Phase II), and studying the feasibility of remedial alternatives (Phase III). EPA has documented its findings from Phase II of the reassessment in a series of reports, three of which have already been peer reviewed by independent scientists.

As part of Phase II, EPA’s contractors developed models to predict future levels of PCBs in the water, sediment, and fish in the Upper Hudson River. Initial results from the models are documented in the “Baseline Modeling Report” (BMR) (Limno-Tech et al., 1999). Based on the public comments received on the initial modeling efforts and on additional analyses conducted since the release of the BMR, EPA released the RBMR, which presented updated modeling results, and a Responsiveness Summary to address the public comments.

To ensure that the assumptions, methods, and conclusions of the RBMR and its Responsiveness Summary are based on sound scientific principles, EPA decided, as per policy, to obtain an expert peer review of the documents. The remainder of this report describes the scope and findings of this independent peer review.

1.2 Scope of the Peer Review

ERG managed every aspect of the peer review, including selecting reviewers (see Section 1.2.1), briefing the reviewers on the site (see Section 1.2.2), and organizing the peer review meeting (see Section 1.2.3). The following subsections describe what each of these tasks entailed.

1.2.1 Selecting the Reviewers

To organize a comprehensive peer review, ERG selected seven independent peer reviewers who are engineers or senior scientists with demonstrated expertise in any combination of the following technical fields:

- Hydrology
- Sediment fate and transport modeling
- Mass balance modeling
- Aquatic food chain modeling
- Chemical and physical properties of PCBs

Appendix A lists the seven reviewers ERG selected for the peer review meeting, and Appendix C includes brief bios that summarize most of the reviewers' areas of expertise. Recognizing that few individuals specialize in every technical area listed above, ERG ensured that the collective expertise of the selected peer reviewers sufficiently covers the five technical areas (i.e., at least one reviewer has expertise in aquatic food chain modeling, at least one reviewer has experience in hydrology, and so on).

To provide continuity among the different panels assembled to peer review EPA's Phase II reports, ERG selected one peer reviewer (Dr. Ellen Bentzen) who served on the panel that evaluated EPA's modeling approach for the Hudson River PCBs site and one peer reviewer (Dr. Per Larsson) who served on the panel that evaluated EPA's interpretations of various water column and sediment sampling efforts. Additionally, another peer reviewer of the RBMR (Dr. Ross Norstrom) has been selected to serve on the upcoming panel that will evaluate EPA's ecological risk assessment for the Hudson River.

To ensure the peer review's independence, ERG only considered individuals who could provide an objective and fair critique of EPA's work. As a result, ERG did not consider in the reviewer selection process individuals who were associated in any way with preparing the BMR or RBMR or individuals associated with GE or any other specifically identified stakeholder.

1.2.2 Briefing the Reviewers

Given the large volume of site-specific information in the RBMR and the fact that none of the reviewers had extensive experience with the Hudson River PCBs site, ERG organized a 2-day meeting prior to the actual peer review to provide the reviewers with background information on the modeling effort and to tour the Upper Hudson River.[1] The purpose of the meeting was strictly to familiarize the reviewers with the site; the reviewers did not provide technical comments on EPA's reports during this briefing. A copy of the minutes from this briefing can be found in Appendix G.

For additional background information on the site and its history, ERG provided the following other documents to the reviewers at the briefing:

- The May 1999 "Baseline Modeling Report" (BMR) (Limno-Tech et al., 1999)
- The July 1999 release of "PCBs in the Upper Hudson River" prepared for GE by Quantitative Environmental Analysis, LLC (QEA) (QEA, 1999)
- The August 1998 release of the "Database for the Hudson River PCBs Reassessment RI/FS" (TAMS, 1998)
- The November 1998 "Report of the Hudson River PCBs Site Modeling Approach Peer Review" (ERG, 1998)
- The June 1999 "Report on the Peer Review of the Data Evaluation and Interpretation Report and Low Resolution Sediment Coring Report for the Hudson River PCBs Superfund Site" (ERG, 1999)
- Executive summaries from other Phase II reports

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[1] Six peer reviewers attended the briefing; the seventh (Dr. Steve Eisenreich) could not attend, but was given a video tape of the meeting.

- Suggested charge questions for the RBMR peer review submitted to EPA via public comment
- The February 2000 "Response to Peer Review Comments on the Preliminary Model Calibration Report" (TAMS et al., 2000c)

To focus the reviewers' evaluations of the RBMR, ERG worked with EPA to develop written guidelines for the technical review. These guidelines (commonly called a "charge") were presented during the briefing meeting and asked the reviewers to address at least the following topics: the calibration of the models, the spatial and temporal resolution of the models, and the implications of modeling results. A copy of this charge, which includes many additional topics and questions, is included in this report as Appendix B.

In the weeks following the briefing, ERG requested that the reviewers prepare their initial evaluations of the RBMR and its Responsiveness Summary. ERG compiled these premeeting comments, distributed them to the reviewers, and made copies available to observers during the peer review meeting. These initial comments are included in this report, without modification, as Appendix C. It should be noted that the premeeting comments are preliminary in nature and some reviewers' technical findings might have changed based on discussions during the meeting. As a result, the premeeting comments should not be considered the reviewers' final opinions.

The peer reviewers were asked to base their premeeting comments on the written materials distributed by ERG, mainly the RBMR and its Responsiveness Summary, even though they received many additional documents as background information. Though not required for this review, some reviewers might also have researched site-specific reports they obtained from other sources.

1.2.3 The Peer Review Meeting

The seven peer reviewers and at least 50 observers attended the peer review meeting, which was held at the Sheraton Hotel in Saratoga Springs, New York, on March 27–28, 2000.

Appendix D lists the observers who confirmed their attendance at the meeting registration desk. The schedule of the peer review meeting generally followed the agenda, presented here as Appendix E. As the agenda indicates, the meeting began with introductory comments both by the designated facilitator and by the designated chair of the peer review meeting. (These and other introductory comments are summarized below.) For the remainder of the meeting, the reviewers provided many comments, observations, and recommendations when answering the questions in the charge. The agenda included two time slots for observer comments, which are summarized in Appendix F of this report. An ERG writer attended the meeting and prepared this summary report.

On the first day of the meeting, Jan Connery of ERG, the designated facilitator of the peer review, welcomed the seven reviewers and the observers to the 2-day meeting. In her opening remarks, Ms. Connery stated the purpose of the peer review meeting, identified the documents under review, and introduced Dr. Steve Eisenreich, a peer reviewer and the technical chair of the meeting. To ensure the peer review remained independent, Ms. Connery asked the reviewers to discuss technical issues among themselves during the meeting and to consult with EPA only for necessary clarifications. Ms. Connery then explained the procedure observers should follow to make comments. She also explained that the peer review meeting would take the form of a free-flowing discussion among the reviewers and that the meeting would not focus on reaching a consensus on any issue. Finally, she reviewed the meeting agenda.

Following Ms. Connery's opening remarks, the peer reviewers introduced themselves, noted their affiliations, identified their areas of expertise, and stated that they had no conflicts of interest in conducting the peer review. Selected representatives from EPA and from EPA's contractors then introduced themselves and identified their roles in the site reassessment. Mr. Doug Tomchuk (EPA) then gave introductory remarks to the reviewers. Specifically, he thanked the reviewers for their efforts in preparing their premeeting comments, briefly explained EPA's policies for conducting peer reviews, and described EPA's process for responding to peer review comments.

Following the introductory presentations, Dr. Eisenreich began the technical discussions of the peer review meeting. Dr. Eisenreich first identified several common themes among the reviewers' premeeting comments, and then worked with the peer reviewers to answer the questions in the charge. The remainder of this report summarizes the peer reviewers' discussions and documents their major findings and recommendations.

1.3 Report Organization

The structure of this report reflects the order of questions in the charge to the reviewers: Section 2 of this report summarizes the reviewers' discussions on specific questions regarding EPA's fate and transport models; Section 3 summarizes the discussions on specific questions regarding EPA's bioaccumulation models; Section 4 summarizes the discussions on general questions that apply to both types of models; and Section 5 highlights the discussions that led to the reviewers' final recommendations. Section 6 lists all references cited in the text. In these sections, the reviewers' initials are used to attribute technical comments and findings to the persons who made them.

As mentioned earlier, the appendices to this report include a list of the peer reviewers (Appendix A), the charge to the reviewers (Appendix B), the premeeting comments organized by the authors (Appendix C), a list of the observers who confirmed their attendance at the meeting registration desk (Appendix D), the meeting agenda (Appendix E), summaries of the observers' comments (Appendix F), and minutes from the January 2000 informational briefing for the reviewers (Appendix G).

2.0 RESPONSES TO SPECIFIC QUESTIONS REGARDING THE FATE AND TRANSPORT MODELS

The peer reviewers opened their discussions by addressing the 12 charge questions related to the fate and transport models documented in the RBMR. When answering these, the reviewers engaged in free-flowing discussions, after which the technical chair summarized where the reviewers agreed and how their opinions differed. A general record of the peer reviewers' discussions on the fate and transport models, organized by charge questions, follows. Additional information on the reviewers' comments on the fate and transport models can be found in their responses to the four general questions in the charge (see Section 4). Finally, following the discussions of both the specific and general charge questions, the reviewers offered several recommendations to EPA regarding the fate and transport models; these are documented in Section 5.

Note: The reviewers' initials used to attribute comments are as follows: EB (Dr. Ellen Bentzen), SE (Dr. Steve Eisenreich), PL (Dr. Per Larsson), GL (Dr. Grace Luk), WL (Dr. Winston Lung), RNa (Dr. Robert Nairn), and RNo (Dr. Ross Norstrom).

2.1 Responses to Question 1

The first charge question pertaining to EPA's fate and transport modeling asked the peer reviewers:

> The HUDTOX model links components describing the mass balance of water, sediment, and PCBs in the Upper Hudson. Are the process representations of these three components compatible with one another, and appropriate and sufficient to help address the principal study questions?

Most peer reviewers agreed that the basic process representations in EPA's HUDTOX model—mass balances of water, sediment, and PCBs—are compatible in a general sense (EB, SE, PL, WL, RNa), but they had many comments on detailed aspects of the process representations, as described below. As a general point, one reviewer thought issues other than the compatibility of process representations should be considered when evaluating whether the HUDTOX model is adequate for answering the principal study questions (RNa). For instance, he suggested that model adequacy could be better evaluated by considering whether uncertainties introduced through model calibration are so large as to compromise the model's ability to characterize the Hudson River. Other reviewers agreed with this point (SE, PL).

The reviewers discussed the following issues when commenting on the individual process representations in the HUDTOX model. (Note: The reviewers discussed most of these issues in greater detail when responding to other questions in the charge.)

- *Empirical representation of sediment-water transfer of PCBs.* When commenting on process representations, several reviewers discussed the use of an empirically derived sediment-water mass transfer coefficient in HUDTOX (SE, PL, WL). Noting that this coefficient accounts for a considerable portion of PCBs in the water column, two reviewers were concerned that the lack of a mechanistic understanding of this mass transfer process might introduce uncertainty into the forecast simulations (SE, WL). Another reviewer agreed, but stressed that the elevated sediment-water mass transfer during low river flow is supported by site-specific data (PL). He suggested that this transfer is obviously linked to driving forces other than flow, and possibly to temperature (PL). These and other reviewers discussed sediment-water transfer of PCBs further when responding to other charge questions, especially charge question 5.

- *Ability of HUDTOX to capture processes that occur over fine spatial and temporal scales.* Though he agreed that the process representations in HUDTOX are compatible, one reviewer questioned whether the model would accurately portray selected events that occur over fine spatial and temporal scales (RNa). Specifically, this reviewer suspected that HUDTOX might not adequately forecast dynamic, fine-scale sediment resuspension processes during 100-year floods or the occurrences of abrupt increases in surface sediment PCB concentrations in localized areas. As an example of this concern, this reviewer noted that EPA's choice to run the hydrodynamic model (RMA-2V) in steady-state mode prevented consideration of dynamic processes. He indicated that the absence of dynamic modeling on fine spatial scales might limit the model's ability to simulate sediment mobilization during high-flow events—an issue he revisited when responding to other charge questions (see Sections 2.3, 2.6, and 2.12).

- *Representation of PCBs in the RBMR.* Though he appreciated the reasoning EPA provided for modeling "Tri+ PCBs"[2], one reviewer was concerned that forecast values of Tri+ PCBs might not be an appropriate input to risk assessments, especially those based on toxic equivalents (TEQs) (RNo). He stressed that the toxicity of Tri+ PCB congeners ultimately depends on the congener composition, which he suspected would shift in time toward the heavier PCB congeners—a trend that EPA's models currently cannot address. This reviewer also was concerned that the fate of individual PCB congeners, which exhibit a wide range of physical and chemical properties, might not be reflected by modeling results for Tri+ PCBs. As an alternate approach, this reviewer suggested that modeling results for selected PCB congeners with a wide range of chemical and physical properties could be more revealing than modeling results for a complex mixture of congeners.

  Though not disagreeing with this reviewer's comments, another reviewer approved of EPA's representation of PCBs in the models (PL). He supported the approach of first examining the fate and transport of a large group of PCBs (i.e., Tri+ PCBs), followed by detailed analyses of representative congeners. The reviewers revisited the representation of PCBs in EPA's models in later discussions (see Sections 2.5 and 4.4).

- *The use of three-phase partitioning coefficients.* One reviewer did not think the RBMR provided adequate justification for using three-phase partitioning to characterize PCBs in the water column (SE), especially considering that this partitioning has implications on many processes in the model (e.g., volatilization and bioaccumulation). The reviewers commented further on this issue when responding to charge question 10.

2.2 Responses to Question 2

The second charge questions pertaining to fate and transport modeling read as follows:

The HUDTOX representation of the solids mass balance is derived from several sources, including long-term monitoring of tributary solids loads, short-term solids studies and the results of GE/QEA's SEDZL model. The finding of the solids balance for the Thompson Island Pool is that this reach is net depositional over the period from 1977 to 1997. This finding has been assumed to apply to the reaches below the Thompson Island Dam as well. Is this assumption reasonable? Are the burial rates utilized appropriate and supported by the data? Is the solids balance for the Upper Hudson sufficiently constrained for the purposes of the Reassessment?

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[2] "Tri+ PCBs" is a term used throughout this report. It refers to the subset of PCB congeners having three or more chlorine atoms attached. This subset of congeners was the focus of much of EPA's modeling efforts.

The reviewers discussed at length several issues relevant to the solids balance for the Upper Hudson River. As a general comment, one reviewer stressed that EPA essentially assumed that much of the Upper Hudson River was net depositional, and that this assumption was not a finding, as the charge question and RBMR imply (RNa). The reviewers' specific comments on the solids balance follow:

- *Sediments in the Thompson Island Pool.* Based upon various lines of reasoning, the reviewers generally agreed that the Thompson Island Pool is probably net depositional (EB, SE, GL, PL). In support of this opinion, two reviewers noted that the continued presence of PCB "hot spots" in the Thompson Island Pool, more than 20 years after their original deposition, supports the judgment that this reach is net depositional (GL, PL). Further, one reviewer added that the consistency between burial rates presented in HUDTOX and the SEDZL model also support this judgment (GL). Additionally, another reviewer indicated that the vertical profiles of PCBs in the sediment, as reported for selected sediment cores, are reasonably consistent with the sediment burial rates documented in the RBMR (EB).

  Two reviewers added cautionary remarks regarding deposition in the Thompson Island Pool. Stressing the inherently dynamic nature of rivers (as compared to lakes, for example), these reviewers warned that the assumption of the Thompson Island Pool, and other stretches of the Upper Hudson River, remaining net depositional throughout the 70-year model forecast is questionable (PL,RNa). Further, noting that the trapping efficiency reported by the SEDZL model is derived, in part, from results of sediment coring studies, one reviewer indicated that trapping efficiency and sediment coring results should not be viewed as two independent observations in support of net deposition, as the RBMR suggests (RNa).

  Finally, one reviewer questioned the importance of classifying reaches in the Upper Hudson River as being net depositional (PL). This reviewer emphasized that the extent of sediment redistribution is more important in evaluating the fate and transport of PCBs. He stressed that it was more important to remember that the sediments in the Thompson Island Pool act as both a source and a sink of PCBs, rather than using more general terms to characterize the river sediments (i.e., as being net depositional).

- *Implications of sediment coring results.* When discussing the judgment that the Thompson Island Pool is net depositional, some reviewers referred to data trends among the high resolution sediment cores (EB, PL, RNa). One reviewer thought EPA should have used results from sediment cores (e.g., as depicted in Figure 6-52 of the RBMR, Volume 2) to a greater extent in establishing the solids balance (EB). Though not in response to this comment, two other reviewers offered different opinions on the utility of the sediment coring data. One acknowledged inherent difficulties with using sediment coring results to draw conclusions about depositional and erosional areas, primarily because the morphology and dynamics of river beds change with time (PL). Another reviewer agreed, and added that the RBMR itself acknowledges that the high resolution sediment cores ". . . are few in number and are not considered representative of average solids burial rates on the spatial scale of the HUDTOX model" (page 128). This reviewer added that the data trends in the high resolution cores might be greatly influenced by the massive redistribution of sediments that followed the removal of the Fort Edward Dam, and therefore might not be sufficient for concluding that certain reaches of the river are generally net depositional.

- *Sediments in reaches downstream of the Thompson Island Dam.* Few of the reviewers' comments directly addressed the assumption that reaches of the Upper Hudson River downstream from the Thompson Island Dam are net depositional. One reviewer indicated that this assumption is not fully substantiated (SE). Other reviewers did not comment further on this topic, but offered extensive comments on the solids loads from tributaries (see below), an issue linked to the assumption of net deposition through the overall solids balance.

- *Solids loads from tributaries.* Again citing various lines of reasoning, several reviewers questioned the validity of the solids loads that EPA assigned to tributaries, particularly to those downstream from the Thompson Island Dam, and agreed that the solids loads reported in the RBMR are not highly constrained (SE,GL,WL,RNa). Further, most of these reviewers were troubled by what they characterized as an arbitrary increase in the tributary loads in the absence of any justification or explanation (e.g., an evaluation of watershed characteristics such as land use, soil erosion, and river flow patterns) (GL,RNa).

  The reviewers highlighted several specific concerns regarding the tributary loads and their implications. First, one reviewer indicated that the solids loads in the Upper Hudson River, according to EPA's solids balance, increases by a factor of 5.7 from Fort Edward to Waterford—an increase she found surprisingly large, especially considering the scarcity of data for tributary loads (GL). Another reviewer thought EPA needed to better justify its increase to solids loads for only selected tributaries (i.e., those downstream of Thompson Island Dam) (RNa). Other reviewers added that the assigned tributary loads and assumption of net depositional reaches of the Upper Hudson River introduces uncertainty to, and might artificially constrain, the overall solids balance and other model parameters such as sediment settling velocities (SE,WL,RNa).

- *Suggestions for consideration of additional information in the solids balance.* Elaborating on his general concern about the solids balance, one reviewer thought the RBMR should have included additional information characterizing the evolution of the river bed (RNa). Specifically, he noted that the RBMR does not present: bathymetry plots; information on or analysis of the fate of the roughly 1,000,000 cubic yards of sediments released after the removal of the Fort Edward Dam; a history of dredging activities; or information on how structures in the river (e.g., dams) affect sediment loads. He thought review of such information could lead to a better understanding of sediment redistribution in the Upper Hudson River. This reviewer also suggested that the settling velocities and sediment resuspension parameters in HUDTOX could be improved—a topic he discussed in far greater detail when responding to charge question 6 (see Section 2.6).

Some reviewers then discussed the utility of conducting another bathymetry survey to provide greater information on the evolution of the river bed (SE,GL,RNa). One reviewer thought comparing the results of a current bathymetry survey to the river bed analyses performed in 1991 might provide a better understanding of sediment transport in the Upper Hudson River (RNa). Another reviewer agreed, but suggested that EPA first review the accuracy of surveying methodology to determine whether an additional survey is warranted (SE).

Other recommendations for increasing confidence in EPA's solids balance included: collecting additional sediment cores and analyzing trends in burial or erosion rates, as indicated by the depth of PCB and Cesium concentrations (EB); validating the solids balance in the HUDTOX model against solids loadings reported for river locations other than Fort Edward and Waterford (GL); and reviewing the solids data for all river locations to develop a better understanding of exactly where solids enter the Upper Hudson River, especially at locations downstream of the Thompson Island Dam (GL).

2.3 Responses to Question 3

The reviewers then briefly discussed the third charge question pertaining to fate and transport modeling, which asked:

> HUDTOX represents the Upper Hudson River by segments of approximately 1,000 meters in length in the Thompson Island Pool, and by segments averaging over 4,000 meters (ranging from 1,087 to 6,597 meters) below Thompson Island Dam. Is the level of spatial resolution achieved by the modeling appropriate given the available data? How does the spatial resolution of the model affect the quality of model predictions?

Three reviewers indicated that the spatial and temporal resolution of the HUDTOX model was adequate, at least in terms of the model's ability to capture long-term trends (GL,WL,RNa). More specifically, one reviewer found the spatial resolution to be consistent with the availability of data (i.e., EPA used a finer resolution grid for the Thompson Island Pool, where sampling data are abundant, and a coarser grid in the downstream reaches, which have been less frequently sampled) (GL). She added that the placement of the simulation grid appeared to account for potential influences of tributary flows, locks, and dams on the various balances. A different reviewer noted that the bioaccumulation model's ability to reproduce spatial variations in fish concentrations offers some level of comfort that the spatial resolution in the fate and transport model is reasonable (RNo).

Though he found HUDTOX's spatial resolution adequate for evaluating general long-term trends in the Upper Hudson River, one reviewer questioned whether the resolution was adequate for addressing episodic sediment transport events (RNa). For instance, this reviewer was not convinced that the spatial and temporal resolution was appropriate for evaluating sediment mobilization during 100-year floods or abrupt increases in surface sediment PCB concentrations following erosion in localized areas—topics that were addressed in greater detail in responses to charge questions 8 and 12 (see Sections 2.8 and 2.12). Additionally, when responding to charge question 4, this reviewer indicated that the modeling may benefit from use of finer spatial and temporal scales than those currently documented in the RBMR. Finally, this reviewer recommended that EPA conduct sensitivity analyses to determine how the spatial and temporal resolution of the HUDTOX model affects the forecast results.

2.4 Responses to Question 4

The fourth charge question pertaining to fate and transport modeling asked the reviewers: "Is the model calibration adequate? Does the model do a reasonable job in reproducing the data during the hindcast (calibration) runs? Are the calibration targets appropriate for the purposes of the study? Are the results of the calibration adequately documented?" The reviewers thought the calibration was adequate, insofar as it reproduced trends among the large volume of water column sampling data, but they had several comments on the implications of the calibration approach, as described below:

- *Adequacy of the calibration.* Several reviewers agreed that EPA's calibration of the HUDTOX model effectively reproduced concentrations of PCBs in the water column in the hindcast simulation, but they questioned whether the calibrated parameters would be representative of future conditions (SE,PL,WL,RNa). Specifically, these reviewers were concerned that values of important physical parameters, such as gross settling velocities and the depth of sediment mixing, were determined through calibration, perhaps at the expense of incorporating a mechanistic understanding of fundamental fate and transport processes (SE,RNa). As an example of this concern, one reviewer thought the settling velocities could have reflected a better mechanistic understanding (i.e., the velocities could have been assumed to be flow-dependent) (RNa). Other reviewers added that the calibration approach set key parameters to constant values, thus assuming that selected fate and transport processes will not change in the future (RNo). Other reviewers agreed, acknowledging that models relying on empirical formulations have inherent uncertainty when modeling future trends (PL,WL).

Though they discussed alternative approaches only briefly, the reviewers had differing opinions on how EPA's models could have been calibrated with a lesser reliance on empirical parameters. One reviewer indicated that the model does not explicitly account for bioturbation or bed load transport, but he added that these processes are extremely difficult to characterize, whether in the field or using models (PL). In contrast, another reviewer thought EPA's models could have incorporated a greater mechanistic understanding had they employed modeling grids with finer spatial and temporal scales (RNa).

The concerns about future forecasts and empirical formulations notwithstanding, some reviewers highlighted strengths of the model calibration approach. For instance, one reviewer commended EPA for using site-specific data to the fullest extent possible to set certain model parameters and calibrate others (WL). Another reviewer noted that the calibration captured relevant seasonal variations in water column concentrations, thus giving credibility to the calibration results (RNo).

- *Influence of 1977–1979 sampling data on calibration results.* One reviewer was concerned that the model calibration might have been biased by including the considerably higher PCB concentrations that occurred throughout the Upper Hudson River from 1977 to 1979 (PL). This reviewer suspected that calibration over this entire history of data (i.e., 1977 to 1999) might be notably different from calibration over the period following the early years of high contamination (i.e., 1980 to 1999). In short, he feared that the model, by incorporating the elevated PCB concentrations from the late 1970s, might predict a faster long-term recovery than would be predicted using a different time frame of data for model calibration. In response to these comments, another reviewer noted that the sharp decrease in PCB levels between 1977–1979 and later years raises questions as to whether the data from the two time frames are truly comparable, at least in terms of data quality (SE). The reviewers revisited the impact of the elevated PCB concentrations in the late 1970s when discussing the calibration of EPA's bioaccumulation models (see Section 3.2).

- *Other comments.* The reviewers raised other issues that do not fall under the previous categories when responding to charge question 4. First, referring back to a comment he made in response to charge question 1 (see Section 2.1), one reviewer stressed that the calibration of Tri+ PCBs effectively reproduced water column concentrations (RNo). Because water column concentrations are dominated by lower chlorinated PCB congeners, however, this reviewer was not convinced that the calibration effectively captures the behavior of higher chlorinated congeners—the congeners he thought are of most importance for the risk assessment.

Second, one reviewer noted that the impacts of various sources of PCBs to the water column in the Upper Hudson River (e.g., upstream discharges, releases from sediments, and so on) have changed with time, and not in a consistent manner (PL). As a result, he explained that data collected at Fort Edward would likely have different long-term trends than data collected in the Thompson Island Pool. For a better understanding of the underlying fate and transport processes, this reviewer recommended that EPA examine more closely the data from other reaches of the river, as he discussed in greater detail when responding to charge question 6 (see Section 2.6).

- *The appropriate selection of calibration targets.* The reviewers did not explicitly discuss whether EPA selected appropriate calibration targets. Rather, the comments focused on the general calibration approach, as summarized above. One reviewer did note, however, that the calibration targets EPA selected for HUDTOX, especially gross settling velocities and depth of sediment mixing, are known to be sensitive parameters in fate and transport modeling for organic contaminants (SE).

2.5 Responses to Question 5

The fifth charge question pertaining to fate and transport modeling asked the reviewers:

HUDTOX employs an empirical sediment-water transfer coefficient to account for PCB loads that are otherwise not addressed by any of the mechanisms in the model. Is the approach taken reasonable for model calibration? Comment on how this affects our understanding of forecast simulations, given that almost half of the PCB load to the water column may be attributable to this empirical coefficient.

Most of the reviewers' responses closely paralleled those provided to charge question 4, but additional issues were raised:

- *General impressions of the sediment-water mass transfer coefficient.* The fact that a considerable portion of the PCB loads to the water column was associated with an empirically derived sediment-water mass transfer coefficient was unsatisfying to several reviewers, particularly because this derivation does not account for any underlying mass transfer mechanisms (SE,GL,PL,WL). Without knowing the extent to which different processes (e.g., bioturbation, desorption) are embodied in this mass transfer coefficient, the reviewers were concerned that the past sediment-water mass transfer patterns might not be useful in predicting those that will occur in the future. However, the reviewers indicated that the calculated sediment-water mass transfer coefficient was apparently adequate in terms of the success of the model calibration.

Though concerned about the implications of the mass transfer coefficient on model forecasts, one reviewer suspected that a less uncertain approach to characterizing the sediment-water mass transfer might not have been available (RNo). He noted that including additional parameters with equal or greater uncertainty will not increase the model's predictive capabilities. Another reviewer added that the derivation of the sediment-water mass transfer coefficient in the RBMR was a considerable improvement over that used in the BMR (WL).

- *Seasonal profile of the mass transfer coefficient.* One reviewer commented that the annual profile of the sediment-water mass transfer coefficient (see Figure 6-55 in the RBMR, Volume 2) was not rigorously derived (GL). Noting the scatter among the individual observations of sediment-water mass transfer across the Thompson Island Pool, this reviewer suspected that EPA could have selected other functional forms of the annual profile that have similar statistical performance. As an example of her concern, she indicated that the annual profile of the sediment-water mass transfer coefficient does not reflect the increased transfer associated with spring high-flow events. This reviewer was also concerned about the uncertainties associated with assuming a seasonal pattern derived from 5 years of sampling data (1993–1997) is representative over an entire 70-year forecast. On the other hand, two reviewers reiterated that the mass transfer coefficient was reasonably substantiated through the calibration of the HUDTOX model (WL,RNo).

  Given these concerns, two reviewers (SE,GL) suggested that a more thorough analysis of the available data using better statistical functions might not only identify an improved fit to the mass transfer coefficient but also identify system parameters (e.g., temperature) that affect the coefficient. The reviewers discussed this issue further when responding to charge question 6 (see Section 2.6).

- *Model performance for individual congeners.* One reviewer thought the sediment-water mass transfer coefficient, by virtue of being fit to data for Tri+ PCBs, might not realistically portray mass transfer processes for individual congeners (RNo). Referring to Figure 6-56 in the RBMR (Volume 2), this reviewer noted that the empirically derived coefficient tends to overpredict sediment-water mass transfer for lower chlorinated congeners and underpredict transfer for higher chlorinated congeners.[3] Though not disagreeing with this

[3] This reviewer found the data depicted in Figure 6-56 (Volume 2) suspect. He wondered how the fate and transport model could successfully characterize congener-specific trends in the hindcast simulations, given that

concern, another reviewer commented that modeling individual congeners can be an extremely difficult task (PL). This reviewer explained that certain congener-specific properties vary with the composition of other congeners present and with concentration, thus complicating efforts to assign basic parameters.

Reiterating his concern about the ability of the model to forecast trends for congeners of concern, a reviewer noted that the EPA fate and transport model performed best when evaluating BZ#28 and BZ#52 (RNo). This reviewer acknowledged that these congeners clearly account for a considerable portion of PCBs in the water column, but he stressed that they are likely not the congeners of concern in terms of bioaccumulation. In short, he pointed out that the model's predictive ability was not gauged in terms of the PCB congeners likely to be most important for the site risk assessments.

2.6 Responses to Question 6

The sixth charge question pertaining to fate and transport modeling asked the reviewers: "Are there factors not explicitly accounted for (e.g., bank erosion, scour by ice or other debris, temperature gradients between the water column and sediments, etc.) that have the potential to change conclusions drawn from the models?" Most reviewers thought the model addresses most of the factors relevant to fate and transport, with the exceptions listed below:

- *Role of temperature in sediment-water mass transfer.* Elaborating on a comment he raised in response to charge question 5, one reviewer recommended that EPA investigate the impact that temperature might have on sediment-water mass transfer of PCBs in an effort to better understand underlying mechanistic processes (PL). This reviewer noted that EPA reportedly found no association between temperature and PCB loads at the Fort Edward sampling station—a result he did not find surprising given that upstream discharges from the GE facilities, which he presumed not to be temperature dependent, might account for much of the PCB load at this station. This reviewer suspected that evaluating the data on how temperature affects sediment-water mass transfer within the Thompson Island Pool might reveal important trends, given that PCB loads in this part of the river are not believed to be affected primarily by upstream sources. If such evaluations determine that the mass transfer coefficient is highly temperature dependent, this reviewer indicated that EPA could conclude that the mechanistic processes represented in the coefficient also are highly temperature dependent.

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the sediment-water mass transfer coefficients were either overstated or understated (depending on the congener considered).

Additionally, this reviewer mentioned several phenomena (e.g., bioturbation and microbial processes) associated with sediment-water mass transfer that can reasonably explain a temperature dependence. He thought evaluating the temperature dependence of the mass transfer would be a worthwhile exercise, though he acknowledged that certain temperature-dependent processes (e.g., microbial processes) are not easily modeled.

Another reviewer agreed that investigating the temperature dependence of the sediment-water mass transfer coefficient is worthwhile (SE). This reviewer indicated that the water column sampling data from the Upper Hudson River suggest that temperature-dependent mechanisms might underlie the sediment-water mass transfer coefficient. Specifically, he noted that increased loads of PCBs having a profile similar to the PCBs found in the sediments occur during low-flow conditions—a phenomenon he thought was inconsistent with sediment resuspension, which tends to be greatest during high-flow conditions. On the other hand, he thought such a seasonal trend is consistent with microbial activity causing increased PCB transport to the water column. Given these trends, the two reviewers recommended that EPA investigate the temperature dependence of sediment-water transport more closely, possibly considering that microbial processes might be activated by a threshold temperature (as opposed to being proportional to temperature) (SE,PL).

- *Comments on sediment resuspension.* Responding to the charge question, one reviewer believed that the spatial scales in the HUDTOX model were too coarse to address factors such as bank erosion and scour by ice (RNa), but he suspected that the model calibration had indirectly accounted for the impacts of these factors. This reviewer then identified three key areas where the model's portrayal of sediment resuspension from cohesive sediments should be improved.

  First, this reviewer indicated that, in the depth of scour model (DOSM), EPA relied on studies that might not represent actual conditions in the Upper Hudson River to parameterize the resuspension algorithms for cohesive sediments. More specifically, he commented that the parameterization was based largely on results of shaker and annular flume tests that considered shear stresses up to roughly 10 dyne/cm^2. Noting that shear stresses in the Upper Hudson River during high-flow conditions are much higher than this level, this reviewer questioned whether parameterizing to shaker and annular flume results can adequately characterize sediment resuspension, especially during high flows.

  Second, this reviewer challenged the RBMR's assumption to develop the sediment resuspension algorithms: that cohesive sediment ". . . mass is eroded over the time scale of approximately one hour" (page 33, RBMR). Though he acknowledged that this assumption is consistent with a study published in the scientific literature for any given hour, this reviewer indicated that several other studies have found that sediment erosion can be reactivated as shear stresses increase (Gailani et al., 1991; Lick et al., 1995a, 1995b). Accordingly, this reviewer recommended that EPA model sediment resuspension (especially during high-flow events) as a series of 1-hour steps, rather than basing sediment resuspension simply on the highest hourly shear stress observed during a particular flow event. This reviewer suspected that use of this revised algorithm can lead to considerably higher resuspension rates during high-flow events than currently documented in the RBMR; he added that the higher resuspension rates would likely also have implications on the solids balance.

Third, this reviewer indicated that the HUDTOX model does not account for bed loading in non-cohesive sediments, or the process by which sediment grains jump small distances (RNa). Another reviewer agreed and explained that bed loading is a slow process that can have long-term impacts, possibly accounting for as much as 10 to 20 percent of sediment mobilization over the long term (PL). This reviewer added that the extent of bed loading is extremely difficult to measure in the field, but another reviewer indicated that other sediment transport models have accounted for this phenomenon (RNa).

Finally, this reviewer acknowledged that adjusting the parameterization of the sediment resuspension would likely cause the HUDTOX model to no longer be calibrated. However, he and another reviewer agreed that incorporating flow-dependent settling velocities in the model might effectively balance the increased sediment resuspension rates that result from the improved parameterization (WL,RNa).

The reviewers developed a list of recommendations to help EPA address the concerns raised about the sediment resuspension algorithms in the fate and transport models (see Section 5.1).

- *Other factors not addressed in the fate and transport models.* Some reviewers suggested that the model itself, or at least its documentation, should have addressed other factors not listed above. First, one reviewer recommended that the RBMR discuss in greater detail how removal of the Fort Edward Dam affected the evolution of the river bed (i.e., where did the 1,000,000 cubic meters of sediment that were released go?) (RNa). He also recommended that the report provide additional detail on past dredging activities. This reviewer thought both factors might affect EPA's interpretation of sediment burial rates.

  Second, another reviewer wondered whether the fate and transport model could address future scenarios that might alter the current balances of PCBs, solids, and water (EB). As examples, she questioned whether the model can capture the effects of removal (or failure) of other dams in the Upper Hudson River or changes in the watershed that lead to significant changes in nutrient loading, biological production, and dissolved organic carbon. This reviewer suspected that the model could be used to examine these and other scenarios that were not observed during the calibration period.

2.7 Responses to Question 7

The seventh charge question pertaining to fate and transport modeling asked the reviewers:

> Using the model in a forecast mode requires a number of assumptions regarding future flows, sediment loads, and upstream boundary concentrations of PCBs. Are the assumptions for the forecast reasonable? Is the construct of the hydrograph for forecast predictions reasonable? Should such a hydrograph include larger events?

In general, the reviewers did not identify critical shortcomings with EPA's selection of model inputs for the forecast simulations, but they did suggest the Agency consider additional data sets and alternate approaches when establishing these inputs, as described below:

- *Comments on the hydrograph.* None of the reviewers found EPA's construct of the hydrograph for forecast simulations unreasonable, though three reviewers suggested alternative approaches or other data sources to consider for this task. However, no consistent recommendation from the review panel emerged. Details on the three reviewers' suggestions follow.

  First, one reviewer noted that EPA could have considered data sources in addition to the local hydrograph from the last 20 years (RNa). As an example, he indicated that regional precipitation data are likely available for a period much longer than 20 years. He added that long-term precipitation data might reveal insights into decadal shifts in precipitation patterns, which would not be characterized by the 20-year hydrograph. Second, citing an apparent upward trend in flow over the 20-year record of hydrographs, another reviewer thought EPA should consider basing its 70-year forecast simulations on the hydrographs from only the most recent years (e.g., the late 1990s), rather than from the last 20 years (GL). Third, yet another reviewer suggested that EPA simply apply the 20 years of hydrographs consecutively over the 70-year forecast period, rather than randomly sampling from the historical data set (WL). Other reviewers, however, noted that the hydrographs currently used in the RBMR seemed reasonable, and they were not convinced that greater insight would be gained by employing alternate methods (SE, PL).

- *Comments on the tributary solids loads.* One reviewer commented that the uncertainty associated with assigning the tributary loads for the calibration period is also a source of uncertainty for the forecasts (RNa). This reviewer noted that future changes in the watershed (e.g., increased agricultural land use or increased urbanization) would likely affect the sediment loads from the tributaries. However, due to the lack of mechanistic understanding of these loads exhibited in the RBMR, this reviewer did not think the model in its current formulation could adequately characterize such scenarios.

- *Comments on the upstream PCB load.* Several reviewers commented on EPA's choice of an upstream boundary condition for the PCB load at Fort Edward (i.e., the choice to model constant loads of 0 ng/l, 10 ng/l, and 30 ng/l of Tri+ PCBs). Though some reviewers indicated that the future PCB load is obviously an uncertain parameter and one that would be expected to fluctuate with time, two reviewers noted that the RBMR provided reasonable justification for the three scenarios selected (SE,PL). Commenting further, one of these reviewers added that the use of three scenarios was particularly insightful for evaluating the long-term recovery of the system (PL).

  The reviewers then debated whether EPA would be justified in using an upstream Tri+ PCB load higher than 30 ng/l. Citing the recent record (1991–1997) of PCB loads measured at Fort Edward (see Figure 8-3, RBMR, Volume 2), one reviewer indicated that the upper range PCB load selected for the forecast simulation (30 ng/l) is actually the lowest PCB load observed in the recent record (GL). This reviewer thought EPA would be justified using higher estimates of the upstream load, especially considering that Tri+ PCB loads as high as 600 ng/l were observed in the early 1990s. Other reviewers disagreed, noting that the higher loads in the early to middle 1990s were likely representative of an episodic release (i.e., the Allen Mill event) and not of typical conditions (SE,RNo). Another reviewer added that remedial activities at the GE facilities will likely lead to continued decreases in upstream sources in the future (EB). Additionally, another reviewer stressed that the future PCB load is assumed to be an average value, and he had no reason to believe that PCB loads at Fort Edward would be consistently higher than 30 ng/l (PL).

  Finally, one reviewer indicated that the RBMR does not clearly describe how atmospheric deposition of PCBs are accounted for, if at all, in the upstream PCB load boundary condition (RNo). Additionally, this reviewer thought the PCB load at Fort Edward is one of the largest uncertainties in EPA's models.

2.8 Responses to Question 8

The eighth charge question pertaining to fate and transport modeling asked the reviewers:

The 70-year model forecasts show substantial increases in PCB concentrations in surface sediments (top 4 cm) after several decades at some locations. These in turn lead to temporary increases in water-column PCB concentrations. The increases are due to relatively small amounts of predicted annual scour in specific model segments, and it is believed that these represent a real potential for scour to uncover peak PCB concentrations that are located from 4 to 10 cm below the initial sediment-water interface. Is this a reasonable conclusion in a system that is considered net depositional? After observing these results, the magnitude of the increases was reduced by using the 1991 GE sediment data for initial conditions for forecast runs. Is this appropriate? How do the peaks affect the ability of the models to help answer the Reassessment study questions?

The reviewers discussed this question at length; an overview of their responses follows:

- *The likelihood that abrupt increases in surface sediment PCB concentrations occur.* In a general sense, several reviewers found it reasonable that localized erosion can uncover highly contaminated sediments, thus causing abrupt increases in surface sediment PCB concentrations—even in stretches of the Upper Hudson River that are net depositional (SE,GL,PL,RNa). One reviewer indicated that she expected to see such increases, noting that they should not be perceived as an artifact of EPA's modeling approach (GL). Another reviewed added that significant year-to-year fluctuations in water column concentrations are not surprising, given the fact that river systems are dynamic (e.g., in comparison to lakes) (PL). As described in the next bullet item, however, some reviewers questioned the model's ability to forecast this erosional behavior adequately.

- *The model's ability to predict the occurrence and impact of the abrupt increases.* Though they agreed that increases in surface sediment PCB concentrations are reasonable, some reviewers did not think the model could predict the timing and impact of the abrupt increases adequately. For example, one reviewer noted that EPA's model could not realistically predict the exact years in which localized increases will occur (PL). Another reviewer agreed, but noted that the ability to predict these occurrences has little bearing on the model's long-term performance (see next bullet item) (RNo).

  As another example, two reviewers thought the model's spatial resolution limits the ability to forecast the net effects of localized scour (GL,RNa). These reviewers indicated that some of the abrupt increases in surface sediment PCB concentrations were predicted for river stretches where model segments are at least 1 km in length. Because they thought localized erosion effects (i.e., over a scale much finer than 1 km) might be associated with uncovering highly contaminated sediments, these reviewers did not think the model's segmentation grid allowed a realistic portrayal of the future abrupt increases in surface sediment concentrations. Additionally, one reviewer noted that the model currently assumes that these abrupt increases apply to the surface sediments throughout an entire modeling segment, rather than at a localized area within the segment (GL). She said, and another reviewer agreed, that this formulation essentially assumes that the sediments within an entire modeling segment are eroded at the same rate and contaminated with PCBs at the same level—an assumption they did not think adequately portrays localized effects (GL,RNa). One reviewer noted that a model with finer spatial resolution would likely continue to predict future abrupt increases in surface sediment PCB concentrations, but of a lesser magnitude than currently reported in the RBMR (GL).

- *Impact of the abrupt increases on the model's ability to answer the principal reassessment questions.* Noting that the predicted abrupt increases in surface sediment PCB concentrations are associated with only modest, transient increases in the predicted PCB concentrations in fish, two reviewers thought the increases in sediment concentrations have little, if any, bearing on the model's ability to capture long-term trends and address the principal study questions of EPA's reassessment (SE,RNo). In short, one reviewer said the predicted impact of the increased contamination of sediments on fish is marginal and short-lived (over the scale of 70 years), and is consistent with what was observed in fish in the Upper Hudson River following the 1991 episodic release of PCBs from the GE Hudson Falls plant site (i.e., the Allen Mill event) (RNo).

- *The use of GE's 1991 sediment coring data to initialize model forecasts.* None of the reviewers commented on this part of the charge question. One reviewer, however, referred to earlier discussions among the reviewers that found that use of the 1991 GE sediment data was appropriate for initializing forecast simulations (SE).

2.9 Responses to Question 9

The ninth charge question pertaining to the fate and transport modeling asked the peer reviewers:

> The timing of the long-term model responses is dependent upon the rate of net deposition in cohesive and non-cohesive sediments, the rate and depth of vertical mixing in the cohesive and non-cohesive sediments and the empirical sediment-water exchange rate coefficient. Are these rates and coefficients sufficiently constrained for the purposes of the Reassessment?

The reviewers' answers to this question primarily drew from their earlier discussions on model calibration (see Section 2.4), the sediment-water mass transfer coefficient (see Section 2.5), and the sediment resuspension algorithms (see Section 2.6). Their responses fell into three general categories:

- *Comments on the calibration parameters.* The reviewers referred to their earlier discussion on model calibration (see Section 2.4) and implications of the sediment-water mass transfer coefficient (see Section 2.5) in response to this question. That is, they reiterated several key points: the calibration parameters have great implications on model predictions; the calibrated parameters were sufficient for reproducing environmental conditions in the Upper Hudson River over the past 20 years; and questions remained about whether the calibration parameters would be representative of future conditions, given that underlying fate and transport mechanism were not fully characterized.

In addition, several reviewers stressed that the parameters determined through model calibration may not be representative of future conditions if the relative emphasis of various fate and transport mechanisms (e.g., sedimentation rates, bioturbtaion effects) change with time (SE,PL,RNo). Further, one reviewer noted that some of the calibrated values might better reflect actual conditions if EPA improves its parameterization of the DOSM, as outlined in Section 2.6, and then recalibrates the HUDTOX model (possibly using flow-dependent settling velocities) (RNa). Finally, another reviewer noted that EPA's analysis of cohesive sediments separate from non-cohesive sediments was appropriate (SE).

- *Suggestions for improved calibration of selected parameters.* When answering this charge question, the reviewers discussed how the model calibration could be improved to reflect a greater mechanistic understanding of fundamental fate and transport processes, while still capturing the observed river conditions over the last 20 years. Two reviewers did not think an improved calibration strategy would reduce the uncertainty in the model predictions (PL,RNo). One of these reviewers simply suggested that the inherent uncertainty associated with the forecast should remain as a proviso in interpreting the forecast results (RNo). The other reviewer added that laboratory studies might reveal insight into underlying mechanisms, but he acknowledged that results of laboratory studies are not always replicated in the environment (PL).

  A third reviewer, however, suggested that EPA might be able to better characterize sediment mobilization processes by conducting a river bed survey and comparing its results to the 1991 survey (RNa). He thought such an exercise, coupled with improved parameterization of the DOSM, might reveal greater insights into the depth of vertical mixing in sediments and assumptions of net deposition. Another reviewer added that EPA could better understand sediment deposition rates possibly by collecting additional sediment cores and reviewing the vertical profile of PCB and Cesium concentrations (EB).

- *Addressing the uncertainty introduced through the modeling parameters.* Two reviewers stressed that the importance of the key modeling parameters is primarily on the uncertainty associated with the forecast simulations (PL,WL). One of these reviewers explained that modelers typically use sensitivity analyses on key input parameters to estimate the uncertainty of model outputs (WL). Another reviewer agreed, and suggested that EPA not report estimates of the exact year in which the model predicts PCB levels in a given media to reach a specified level (PL). Rather, he recommended EPA report the range of years over which trends are expected to occur, thus accounting for the model's uncertainty.

  One reviewer argued that the uncertainty in model results is probably relatively low (and the accuracy relatively high) for predicting conditions in the first years of the forecast period, and that the uncertainty increases (and the accuracy decreases) for future years

(RNo). Other reviewers agreed, but suggested that EPA attempt to quantify the uncertainty by running additional sensitivity analyses considering various future scenarios (e.g., significant changes in the watershed) (RNa) and that EPA prominently acknowledge the inherent uncertainty in the model, possibly by including caveats on all conclusions relevant to when PCB levels will reach certain values (SE,PL). The reviewers revisited the issue of uncertainty associated with forecasts when discussing general question 2 (see Section 4.2).

2.10 Responses to Question 10

The tenth charge question pertaining to the fate and transport modeling asked the peer reviewers:

> The HUDTOX model uses three-phase equilibrium partitioning to describe the environmental behavior of PCBs. Is this representation appropriate? (Note that in a previous peer review on the Data Evaluation and Interpretation Report and the Low Resolution Sediment Coring Report, the panel found that the data are insufficient to adequately estimate three-phase partition coefficients.)

When responding to this question, one reviewer indicated that she had difficulty answering this question given that the reviewers were not provided with the DEIR, which contains much of EPA's original documentation on the derivation of three-phase partitioning coefficients (EB). Aside from this general remark, the reviewers' discussion focused on two topics:

- *Comments on the need for using a three-phase partitioning model.* Some reviewers questioned whether three-phase partitioning was a necessary component in EPA's models. Two reviewers stated that they did not think this was an essential component of the model (PL,RNa); others provided differing opinions. For instance, two reviewers noted that volatilization and bioavailability, as treated in EPA's models, are both functions of the concentrations of truly dissolved PCBs, suggesting that three-phase partitioning might be important (EB,SE). On the other hand, one of these reviewers indicated that the fraction of PCBs bound to dissolved organic carbon (DOC) was less than 10 percent for most PCB congeners and for Tri+ PCBs (see Table 6-33, RBMR, Volume 2), possibly suggesting that consideration of the DOC phase might not be important (SE). Nonetheless, this reviewer added that the model, by trying to account for DOC-bound PCBs, might understate the truly dissolved PCB concentrations (especially for BZ#4, see below), thus causing the model to misrepresent processes dependent on truly dissolved PCBs. Given these concerns, this reviewer suggested that EPA provide better justification for using the three-phase model—something he did not think was provided in the RBMR.

Part of the reviewers' concerns about the use of three-phase partitioning stemmed from the fact that DOC is defined operationally to correct for an artifact of how surface water samples are collected and subsequently filtered. Several reviewers indicated that scientists have yet to establish a firm understanding of the composition of DOC, as well as how tightly organics might bind to it (EB,SE). Additionally, one reviewer stressed that the RBMR does not establish that the DOC phase is truly important in terms of PCB partitioning, thus providing no justification for using three-phase partitioning in the first place (SE).

- *Specific comments on partitioning data presented in the RBMR.* The reviewers provided several specific comments on how EPA derived the three-phase partitioning coefficients and their resulting values. First, several reviewers commented on the result that between 30 and 60 percent of BZ#4, a low molecular weight PCB congener, would be bound to DOC, yet less than 10 percent of all other PCBs considered were bound to DOC (see Table 6-33, RBMR, Volume 2) (EB,SE,RNo). One reviewer found this result peculiar, given that BZ#4 is highly soluble (EB). Another reviewer agreed, noting that this representation does not make sense mechanistically (SE).

  In addition, one reviewer provided several additional specific comments on the three-phase partitioning coefficients and their derivation (EB). First, this reviewer noted that EPA apparently derived two sets of three-phase partitioning coefficients, one from GE's sampling data and one from EPA's Phase II sampling data. Referring to the data shown in Table 6-28 in the RBMR (Volume 2), this reviewer indicated that the two sets of partitioning coefficients are considerably different (in some cases, by orders of magnitude). She noted that these differences are not readily apparent from the table, given that the coefficients are presented as logarithms. Given the differences, however, this reviewer thought the RBMR should justify its selection of partitioning coefficients, especially considering that site-specific data do not reveal consistent findings. Second, this reviewer noted that the RBMR does not provide details on how the DOC partition coefficients (K_{DOC}) were derived from particulate organic carbon partition coefficients (K_{POC}) using a "binding efficiency factor" (see page 51, RBMR). Third, this reviewer noted that an assumption in the RBMR—that dissolved organic matter is composed entirely of carbon (see page 51, RBMR)—is incorrect. (Note: When asked to clarify this and other issues pertaining to partitioning, EPA later explained that the wording in the RBMR, and not the assumption, was incorrect.) Given these and other concerns, this reviewer indicated that the derivation of the partition coefficients should be reviewed to identify any errors.

2.11 Responses to Question 11

Charge question 11, pertaining to the fate and transport modeling, asked the peer reviewers:

> HUDTOX considers the Thompson Island Pool to be net depositional, which suggests that burial would sequester PCBs in the sediment. However, the geochemical investigations in the Low Resolution Sediment Coring Report (LRC) found that there was redistribution of PCBs out of the most highly contaminated areas (PCB inventories generally greater than 10 g/m^2) in the Thompson Island Pool. Comment on whether these results suggest an inherent conflict between the modeling and the LRC conclusions, or whether the differences are attributable to the respective spatial scales of the two analyses.

Four reviewers responded to this question, and they agreed there is no conflict between the modeling results and the conclusion in the LRC that PCBs were redistributing from the most highly contaminated sediments in the Thompson Island Pool (SE,GL,PL,RNa). In fact, several reviewers agreed that sediment redistribution is to be expected for reaches of the Upper Hudson River, even though these reaches may be net depositional (SE,PL,RNa).

Three reviewers provided supplemental comments on this issue. First, one reviewer, the one on the panel who also was a peer reviewer of the LRC, stressed that sediment redistribution is an expected phenomenon for a dynamic river system (PL). He added that sediments in the Thompson Island Pool have undoubtedly redistributed, and will continue to do so, regardless of the fact that this part of the river is considered net depositional. Additionally, based on his review of EPA’s Data Evaluation and Interpretation Report (DEIR), this reviewer noted that PCB water column loads increase considerably across the Thompson Island Pool. He thought the magnitude of the increase could not be explained by desorption process alone, and that sediment redistribution in the pool likely accounts, at least in part, for the increased loads. Second, another reviewer explained that net depositional rivers are not characterized by sediments constantly depositing in all areas of the river bed (RNa). Rather, he noted that localized areas do erode in rivers that are net depositional. However, this reviewer reiterated that the spatial resolution in the HUDTOX model might not be sufficient to capture such localized trends. Third, one reviewer noted that sediment redistribution in the Thompson Island Pool is supported by GE’s SEDZL model predictions (GL).

2.12 Responses to Question 12

Charge question 12, pertaining to the fate and transport modeling, asked the peer reviewers: "The model forecasts that a 100-year flood event will not have a major impact on the long-term trends in PCB exposure concentrations in the Upper Hudson. Is this conclusion adequately supported by the modeling?" An overview of the reviewers' responses follows:

- *Comments on the potential impacts of 100-year floods.* The reviewers agreed that the HUDTOX model's current formulation suggests that a 100-year flood has little impact on long-term trends in the Upper Hudson River, but some reviewers thought the impacts of 100-year floods might be underpredicted as a result of the model's sediment resuspension formulations (SE,GL,RNa). Expanding on this topic, one reviewer referred to EPA's approach to modeling sediment resuspension as: ". . . resuspension occurring over previous model steps within an increasing hydrograph is tracked such that total cumulative erosion equals the amount computed using the maximum shear stress during that event" (page 47, RBMR) (RNa). This reviewer explained that the model only considers the shear stress occurring during the peak flow to estimate sediment resuspension, without considering the reactivation in resuspension known to occur with increasing shear stresses.

  In short, this reviewer indicated that erosion is not limited to that which would occur during just 1 hour or during a peak flow, as EPA's model currently assumes. Noting that laboratory studies have shown that sediment resuspension is reactivated as shear stresses (and river flows) increase, this reviewer suspected that sediment resuspension during a 100-year flood in the Upper Hudson River might be considerably higher than the HUDTOX model currently predicts. For an improved portrayal of the impacts of a 100-year flood, this reviewer suggested that EPA consider data available for other river systems and laboratory studies in reparameterizing the sediment resuspension algorithms in the DOSM.

  On a different note, another reviewer indicated that the impacts of a 100-year flood might depend upon when the flood occurs (GL). Specifically, this reviewer noted that EPA only considered one scenario for modeling the 100-year flood: assuming the flood occurs in the first year of the forecast period. As an alternate approach, she suggested that EPA consider evaluating the impacts of 100-year floods occurring in various years of the forecast period. Noting that surface sediment PCB concentrations in some parts of the Upper Hudson River are predicted to increase abruptly in future years, this reviewer wondered whether a simulation of a 100-year flood occurring later in the forecast period might predict different results. No other reviewers commented on this topic.

Finally, some reviewers noted that the predicted minimal impact of a 100-year flood on the long term trends in the Upper Hudson River is reasonably consistent with the past known signals of solids and PCB loads under high-flow conditions.

- *Questions about the flow rates during a 100-year flood.* Some reviewers indicated that this charge question would have been easier to answer if the RBMR provided more detailed information on the river flow patterns expected during a 100-year flood. For instance, one reviewer wondered whether a 100-year flood would submerge the remnant deposits and other areas of contaminated soils on the banks of the Upper Hudson River (PL). After being requested to clarify this issue, EPA indicated that the remnant deposits along the Upper Hudson River have been designed to withstand a 100-year flood event. Another reviewer wondered whether upstream flow controls might mitigate the effects of a 100-year flood (RNo). Yet another reviewer highly recommended that the RBMR should have included information on the topography of the floodplain and bathymetry of the Upper Hudson River to address such concerns (RNa).

3.0 RESPONSES TO SPECIFIC QUESTIONS REGARDING THE BIOACCUMULATION MODELS

The peer reviewers continued their discussions by addressing the five questions in the charge that related to EPA's bioaccumulation models. The technical chair followed the same format as described in the previous section when facilitating these discussions: individual reviewers presented initial thoughts on the questions; the reviewers as a group then further discussed and debated these initial comments; and the technical chair summarized where the reviewers agreed and where their opinions differed. A general record of the peer reviewers' discussions on the bioaccumulation models, organized by question, follows.

Additional information on the reviewers' comments is included in Section 4 (their responses to the general charge questions) and Section 5 (recommendations regarding the bioaccumulation models).

Note: The reviewers' initials used to attribute comments are as follows: EB (Dr. Ellen Bentzen), SE (Dr. Steve Eisenreich), PL (Dr. Per Larsson), GL (Dr. Grace Luk), WL (Dr. Winston Lung), RNa (Dr. Robert Nairn), and RNo (Dr. Ross Norstrom).

3.1 Responses to Question 1

The first charge question regarding the bioaccumulation models in the RBMR asked the reviewers:

> Does the FISHRAND model capture important processes to reasonably predict long term trends in fish body burdens in response to changes in sediment and water exposure concentrations? Are the assumptions of input distributions incorporated in the FISHRAND model reasonable? Are the spatial and temporal scales adequate to help address the principal study questions?

As a general comment, one reviewer indicated that EPA's tiered modeling approach (i.e., the use of the statistical model, probabilistic model, and mechanistic model) was insightful (PL). Specific comments in response to this charge question follow:

- *Process representations in the FISHRAND model.* Two reviewers indicated that, in a general sense, the FISHRAND model incorporates important bioaccumulation processes (e.g., dietary uptake, gill uptake, elimination) (EB,GL), but they and another reviewer (RNo) gave examples of how the model does not offer a truly mechanistic account of bioaccumulation. In short, they suggested that the FISHRAND model, though mechanistic by design, is essentially applied as an empirical model by virtue of the extensive model calibration. One reviewer added, however, that he was not necessarily convinced of the benefits of using a fully mechanistic model that has unrealistic calibration parameters (see next bullet item) (RNo). Given the fact that the FISHRAND model, in its somewhat empirical design, reasonably captures past fish concentration trends, this reviewer indicated that an empirical model may be satisfactory for this application.

  On another note, one reviewer indicated that the Gobas bioaccumulation models, which form the basis of the FISHRAND model, is known for not characterizing the lowest levels of the trophic food web, largely because the models have been designed to capture bioaccumulation trends at higher trophic levels (EB). As a result, this reviewer thought the FISHRAND model performs poorly for species in the lowest trophic levels (see next bullet item). Commenting further on the Gobas model, this reviewer stressed that the model has not been extensively validated, contradictory to the justification provided in the RBMR for using the model. Finally, this reviewer noted that the need to calibrate the FISHRAND model for separate reaches of the Upper Hudson River was peculiar—a comment she elaborated on when responding to charge question 2 (see Section 3.2).

- *The validity of input distributions and other model parameters.* Two reviewers provided extensive comments on input distributions and other parameters EPA used in the FISHRAND model. First, one reviewer thought EPA unnecessarily used calibration to determine two input distributions: the octanol-water partition coefficient (K_{ow}) and species-specific lipid concentrations (RNo). Specifically, this reviewer indicated that EPA could have specified a value for K_{ow} for Tri+ PCBs based on a review of congener-specific values reported in the literature—an issue he discussed further when commenting on the representation of PCBs in the bioaccumulation models (see below). Further, this reviewer thought EPA could have used lipid concentrations and fish growth rates reported in the literature, rather than determining these values via model calibration.

  Second, another reviewer identified some instances where parameters used in the FISHRAND model are inconsistent with those documented in the literature (EB). For instance, consistent with her concern that the Gobas model performs poorly for species in the lowest trophic levels, this reviewer indicated that the average lipid concentration reported in the RBMR for water column invertebrates (i.e., 0.21 percent, see Table 6-1, RBMR, Volume 4) is unrealistically low. She explained that lipid concentrations lower than 1 percent are unrealistic and that studies in the scientific literature report higher values. Also referring to Table 6-1 in the RBMR (Volume 4), this reviewer suspected that a 1 percent organic carbon content for phytoplankton is probably unrealistically low. This reviewer also noted that the RBMR assigns a value of 100 to a rate constant (C_3) used in the derivation of uptake and depuration coefficients, though more recent studies have used values as high as 1,000. This reviewer also stressed that bioaccumulation models are particularly sensitive to sediment to water partitioning of PCBs normalized to organic carbon, but she did not think EPA tested the sensitivity of the model to this input—a comment she revisited when responding to charge question 3.

- *Adequacy of the spatial and temporal scales.* Three reviewers thought the spatial and temporal scales in EPA's bioaccumulation models were adequate (EB,GL,RNo). As evidence of this adequacy, one reviewer indicated that the spatial and temporal variations in the model predictions parallel the variations observed over the calibration period (EB). This reviewer added that the spatial and temporal scales in the bioaccumulation models are consistent with those used in the HUDTOX model.

- *The importance of considering age/size classes in the modeling.* Several reviewers thought EPA's bioaccumulation models should have predicted distributions of PCB levels in different age/size classes of selected fish species, rather than predicting just one distribution for each species (EB,GL,RNo). One reviewer explained that information on how PCB levels vary among age/size classes is an important input to risk assessments, especially because humans and selected ecological receptors (e.g., bald eagles, mink) might only consume fish from a certain size range (RNo).

  Other reviewers agreed and discussed this issue further. Citing her experiences with reviewing PCB concentrations reported for trout in the Great Lakes, one reviewer indicated that PCB levels in certain fish are highly dependent on age, especially for fish that become primarily piscivorus at a certain age (EB). Another reviewer agreed that available data for other systems support analysis of different age/size classes for certain species in the Upper Hudson River, but he did not think such analyses were necessary for species that are short-lived and relatively uniform in size (e.g., minnows) (RNo). Though not disagreeing with the other reviewers' suggestions, one reviewer indicated EPA might have difficulty evaluating variation among age/size classes because the available fish tissue data generally do not report the age or gender of the fish that were sampled (PL).

- *Additional comments.* Two reviewers provided additional comments regarding the general approach to modeling bioaccumulation. First, one reviewer reiterated his concern that the entire modeling approach might not address the PCB congeners of greatest interest to the risk assessments (RNo). This reviewer accepted the reasons EPA provided for modeling bioaccumulation of Tri+ PCBs, but he did not accept EPA's suggestion that modeling individual PCB congeners was infeasible. He again recommended that EPA model the bioaccumulation of a small subset of congeners with varying physical and chemical properties to provide insight into how the composition of Tri+ PCBs might vary in the future. This reviewer added that the FISHRAND model might be less constrained for congener-specific simulations (given that K_{ow} values are known and need not be determined through calibration) than it is for Tri+ PCBs simulations.

Second, another reviewer indicated that the sharp decrease in PCB concentrations in fish between 1977–1979 and later years was difficult to understand, especially for fish species with long life spans (PL). Another reviewer agreed, noting that laboratory studies have shown that many fish species essentially do not eliminate higher-chlorinated PCBs (RNo), but he indicated that the decrease might be explained in part by inconsistent sampling strategies. Another reviewer added that similar sharp decreases in PCB concentrations have been observed in the Great Lakes and other systems (SE). Consistent with his comments on the fate and transport models (see Section 2.4), one reviewer reiterated that a model calibration that considers the markedly higher PCB concentrations from 1977–1979 might artificially predict a faster rate of recovery than a model calibration that does not consider these early years of high concentrations (PL).

3.2 Responses to Question 2

The second charge question pertaining to bioaccumulation asks: "Was the FISHRAND calibration procedure appropriately conducted? Are the calibration targets appropriate to the purposes of the study?" The reviewers indicated that most of their responses to charge question 1 apply to this question. For instance, one reviewer repeated his concern about including K_{ow} as a calibration parameter, though he agreed that the selection of most other calibration parameters (e.g., dietary composition) was appropriate (RNo). Additional comments provided by the reviewers follow:

- *Variation of calibration parameters with river reach.* One reviewer was troubled by the fact that EPA used a different set of calibration parameters for different reaches in the Upper Hudson River, without providing a detailed justification for doing so (GL). She recommended that EPA describe in greater detail, and thoroughly justify, the adjustments made to the FISHRAND calibration, as indicated on page 73 of the RBMR (GL).

- *The need for additional sensitivity analyses and model validation.* One reviewer indicated that EPA should evaluate the appropriateness of its model calibration by conducting more detailed sensitivity analyses and through model validation (PL). This reviewer suggested that EPA run the FISHRAND model for different scenarios to test the sensitivity of the model outputs to key calibration parameters, rather than simply reporting the sign (i.e., positive or negative) of each parameter's elasticity (see pages 73–74, RBMR). Further, the reviewer thought the modeling results would have more credibility had the model been validated against a different data set, possibly one from a separate river. He thought the need for validation was particularly important because the FISHRAND model was originally developed to model bioaccumulation in a lake system that is largely pelagic, but is currently being applied to a river system that is primarily benthic with much greater exposure to contaminated sediments. Another reviewer agreed that model validation would be useful, especially considering that the Gobas model that forms the basis of FISHRAND has yet to be extensively validated (RNo).

- *Representation of the aquatic food web.* Referring to Figure 3-2 in the RBMR (Volume 4), two reviewers wondered whether EPA considered zooplankton in the conceptual model of the Upper Hudson River aquatic food web (EB,RNo). One reviewer indicated that the original Gobas model classified certain zooplankton species incorrectly, but she could not tell from the RBMR whether EPA's bioaccumulation models did not explicitly account for zooplankton or whether the RBMR's descriptions of the aquatic food web (i.e., Figure 3-2, Volume 4) were incomplete.

3.3 Responses to Question 3

The reviewers discussed the third charge question for bioaccumulation models at length. This question asked the reviewers:

> In addition to providing results for FISHRAND, the Revised BMR provides results for two simpler analyses of bioaccumulation (a bivariate BAF model and an empirical probabilistic food chain model). Do the results of these models support or conflict with the FISHRAND results? Would any discrepancies among the three models suggest that there may be potential problems with the FISHRAND results, or inversely, that the more mechanistic model is taking into account variables that the empirical models do not?

Most of the reviewers' discussion focused on one reviewer's critique of the bivariate bioaccumulation model, which she supported by a statistical analysis of the fish, sediment, and water column PCB sampling data (EB), as summarized below. The reviewers also discussed whether this statistical analysis suggested that bioaccumulation could be modeled more straightforwardly under steady-state assumptions. Finally, some reviewers provided comments on the probabilistic bioaccumulation model.

- *Comments on the statistical analyses presented in the bivariate BAF model.* One reviewer indicated (EB), and another agreed (GL), that the bivariate model is based on statistically invalid principles. This reviewer defended her statement by first explaining the goal of the bivariate model: to examine how PCBs in the sediment, as predicted by HUDTOX, and

PCBs in the water column, as determined from sampling studies, relate to PCB levels in fish. Noting that the water column PCB data are variable, as expected for a measured parameter, and that the sediment PCB data are not, at least as predicted by HUDTOX, this reviewer suspected that the statistical analyses in the bivariate model are inherently biased.

Further, this reviewer thought the model's use of multiple regressions is inappropriate because the independent variables in the regression—water column concentrations of PCBs and sediment concentrations of PCBs—are in fact correlated. This reviewer noted that the RBMR defends its use of multiple regressions by indicating that the PCBs in sediments and the water column are not in equilibrium. She examined these and other assumptions by computing a number of regressions between water, sediment, and fish PCBs using data provided in Table 4-5 (PCBs in selected fish species), Table 4-7 (PCBs in the water column), and Table 4-8 (predicted levels of PCBs in the sediment) in Volume 2D of the RBMR. The data she considered spanned the years 1977 to 1997. Details of her analyses and results are presented in her premeeting comments and documented below.

This reviewer emphasized that water column and sediment PCB concentrations are clearly correlated, regardless of whether they are in equilibrium. She added that her correlations between water column and sediment PCB concentrations, which were calculated for different reaches of the Upper Hudson River, were statistically significant and strongly suggest that the system is at steady state. She added that without normalizing the water concentrations to organic carbon, it was impossible to assess if the system is at equilibrium. As a result, this reviewer concluded that an inherent assumption in the bivariate model (i.e., that the two regression variables are independent) is incorrect, rendering a key aspect of the model statistically invalid.

- *Alternate statistical analysis of the available sampling data.* One reviewer commented on statistical trends apparent among the fish, sediment, and water column data, that are not documented in the RBMR (EB). For instance, she noted that she could not reproduce some of the statistical parameters reported by EPA (e.g., selected values in Tables 4-5, 4-7, and 4-8 in the RBMR, Volume 4). This reviewer suggested that EPA check its statistical analysis to ensure the results reported are accurate. She indicated that some spreadsheet software packages (i.e., Microsoft Excel) are known to generate incorrect results, but the RBMR did not specify what program was used to compute the statistics presented for the bioaccumulation models. Her premeeting comments (see Appendix C) provide additional information on this topic.

  Further, this reviewer presented several important correlations among the fish, sediment, and water column sampling data for Tri+ PCBs. She stressed that although the correlations appear weak when considering combined data from all stretches of the river, accounting for differences among the individual river stretches (using analysis-of-covariance) revealed significant, notable trends. She added that the correlations were strongest when considering log-transformed PCB data and organic carbon normalized concentrations. Commenting on her findings, this reviewer indicated that PCB concentrations in most species of fish were correlated (r^2 values between 0.6–0.8) both with water column PCB concentrations and with sediment PCB concentrations. She added that the correlations were weakest for the sampling data from the Thompson Island Pool—a finding she attributed to the highly variable data for this part of the Upper Hudson River.

This reviewer highlighted several key conclusions from her analysis. First, she emphasized that both PCBs in the water column and PCBs in the sediment are important for predicting PCB concentrations in fish. Second, she noted that the correlations among the different environmental media might allow for predicting fish PCB concentrations without the need for a mechanistic model (see next bullet item). Third, she stressed that the correlations were relatively strong, even for species having different dietary composition (e.g., strong correlations for benthivores and planktivores). Finally, she added that the correlations for individual species reflected her expectations of biomagnification; specifically, the correlations showed that species at the highest level of the aquatic food web (e.g., largemouth bass) accumulated more PCBs than species at lower levels (e.g., brown bullhead).

- *Implications of the alternate statistical analysis (i.e., equilibrium assumptions).* Several reviewers agreed that the statistical analyses summarized above potentially have great implications on how EPA models bioaccumulation (EB,SE,PL,RNo). In short, one reviewer indicated that the statistical analyses suggest that future fish concentrations could be computed directly from the HUDTOX modeling results for sediment and water PCB concentrations, to some (unknown) future point but while water concentrations remain above detection limits (EB). She added that it is typically not desirable to use regression models to predict values outside the range of observed values; however, given the strength of the correlations indicating that water and sediment concentrations drive levels of PCBs in fish, coupled with the uncertainty of various parameters used in the mechanistic FISHRAND model, this reviewer indicated that the empirical relationships may overall have less uncertainty. She added that the FISHRAND model should not be discarded, but could instead be used to evaluate the impacts of specific scenarios (e.g., changes to the aquatic food web structure) and the relative importance of key system parameters (e.g., lipid concentrations, diet composition, age/size classes).

  Agreeing with this summary, another reviewer added that the statistical analysis basically shows that bioaccumulation is driven primarily by the outputs of the HUDTOX model (PL). Other reviewers suspected that the statistical analyses essentially suggest that PCB contamination in the fish, sediment, and water column might be best forecast from simple steady-state assumptions (EB,SE). Though not disagreeing with this suggestion, one reviewer cautioned that equilibrium assumptions might not be valid for predicting PCB concentrations in large fish if PCB levels in sediment and water change significantly with time (RNo); he suggested that PCB levels in large fish under these specific conditions might be weakly correlated with the sediment and water PCB levels. In the weeks following the peer review meeting, one reviewer (EB) subsequently wished to clarify this issue by noting that her statistical analyses (i.e., regressions) support that the Hudson River system is certainly at steady-state but not at equilibrium. If the system were at equilibrium, she indicated that concentrations of PCBs in fish at higher trophic levels would be similar to those at lower levels, but the regressions and EPA's bivariate model results show that biomagnification is evident in the food web. According to this reviewer, without normalizing the water column data to organic carbon, it is not possible to assess equilibrium conditions between water and sediment with the available data, but the regressions do support steady-state conditions. This reviewer found it noteworthy that the scientific literature continues to confuse definitions of equilibrium and steady state.

- *Comments on the probabilistic bioaccumulation model.* The reviewers had several insights on the probabilistic bioaccumulation model. As a general comment, one reviewer noted that this model construct is essentially identical to applying the FISHRAND model under snapshot conditions (EB). This reviewer then listed several ways in which the description of the probabilistic bioaccumulation model and its results could be improved. First, this reviewer expected to see direct comparisons in the RBMR between the predicted bioaccumulation factors (BAFs) and values cited in the literature, rather than general statements that the model findings agreed well with data reported elsewhere. Second, referring to data presented in Table 5-2 of the RBMR (Volume 4), this reviewer suspected that the BAF for water column invertebrates (i.e., 13.2) was an error, possibly due to the model's use of inaccurate lipid concentrations. Third, she recommended that the RBMR discuss the implications of the BAFs in greater detail, such as by indicating whether the differences in BAFs across various groups of species (e.g., piscivores, planktivores) are consistent with current understanding of PCB biomagnification.

- *Recommended congener-specific analyses.* Expanding on concerns raised in response to charge question 1 (see Section 3.1), one reviewer found EPA's focus on mixtures of PCB congeners rather than on individual congeners troublesome (RNo). Specifically, this reviewer indicated that two aspects of EPA's models—consideration of only Tri+ PCBs and the reported similarity between PCBs in fish and Aroclor 1248—are not particularly useful, given that EPA never characterized the composition of individual congeners in the various environmental media. As an improvement, this reviewer suggested that EPA use the existing sampling data to evaluate how the profile of PCBs in fish has changed over time. He acknowledged that the past sampling record might be limited due to the use of different analytical methods, but he believed an evaluation of how the composition of Tri+ PCBs has changed during the historical record would provide insight into how congener distributions might change in the future.

3.4 Responses to Question 4

Charge question 4 on EPA's bioaccumulation models asked the reviewers:

> Sediment exposure was estimated assuming that fish spend 75% of the time exposed to cohesive sediment and 25% to non-cohesive sediment for the duration of the hindcasting period. The FISHRAND model was calibrated by optimizing three key parameters and assuming the sediment and water exposure concentrations as given, rather than calibrating the model on the basis of what sediment averaging would have been required to optimize the fit between predicted and observed. Is the estimate of sediment exposures reasonable?

The reviewers discussed this question briefly and indicated that the input of the time fish are exposed to cohesive and non-cohesive sediments is reasonable; they added that EPA should investigate the importance of this input by conducting sensitivity analyses (EB,SE,GL,PL). Two reviewers suspected that this particular model input is likely to have little implication on model results (EB,PL), but another reviewer thought the RBMR should provide better justification for setting this model input, especially considering that only 25 percent of sediments in the TIP are cohesive and 75 percent are non-cohesive (GL). Noting that dietary composition is a calibrated variable, yet another reviewer suspected that any adjustments to the time fish are exposed to cohesive and non-cohesive sediments might simply be compensated for by the dietary composition variable during model calibration (RNo).

3.5 Responses to Question 5

The reviewers briefly discussed charge question 5, which asked:

> The FISHRAND model focuses on the fish populations of interest (e.g., adult largemouth bass, juvenile pumpkinseed, etc.) which encompass several age-classes but for which key assumptions are the same (e.g., all largemouth bass above a certain age will display the same foraging behavior). This was done primarily because it reflects the fish data available for the site. Is this a reasonable approach?

When discussing this question, the reviewers indicated that they addressed the issue of age/size classes when answering charge question 1 (see Section 3.1), and thus provided few additional comments. One reviewer thought that lumping several age/size classes of fish into one population was reasonable in terms of the fish feeding behavior (PL). However, reiterating that PCB concentrations in fish would likely be dependent on age/size classes, the reviewers recommended that EPA's models stratify some species into age/size classes (EB,PL). Another reviewer noted that the available data, more than 10,000 fish samples, should be sufficient for stratifying at least some species into age/size classes (GL). However, as another reviewer noted previously, a considerable portion of the historical records have no information on the age or gender of the fish sampled (PL).

4.0 RESPONSES TO GENERAL QUESTIONS REGARDING THE BMR

After answering the 17 specific questions in the charge, the reviewers then discussed four general questions that addressed issues relevant to both the fate and transport models and the bioaccumulation models. When answering these questions, the reviewers reiterated many of the findings they had presented earlier in the meeting and offered additional comments for discussion. A general record of the peer reviewers' discussions on the four general questions follows. The reviewers' final conclusions and recommendations for the meeting are listed in Section 5.0.

Note: The reviewers' initials used to attribute comments are as follows: EB (Dr. Ellen Bentzen), SE (Dr. Steve Eisenreich), PL (Dr. Per Larsson), GL (Dr. Grace Luk), WL (Dr. Winston Lung), RNa (Dr. Robert Nairn), and RNo (Dr. Ross Norstrom).

4.1 Responses to General Question 1

The first general question asked the reviewers: "What is the level of temporal accuracy that can be achieved by the models in predicting the time required for average tissue concentrations in a given species and river reach to a recover to a specified value?" The reviewers discussed this briefly. Two indicated that this general question can only be effectively answered through extensive sensitivity analyses—a task the reviewers could not do without having copies of EPA's models (SE,RNo). Thus, these reviewers found this question impossible to answer from the information provided.

Commenting on uncertainties in a more general sense, one reviewer repeated an earlier suggestion: that EPA should acknowledge the uncertainties of key model findings (e.g., the year in which fish PCB concentrations are expected to reach specified levels), rather than specifying the best estimate of when certain events might occur (PL). Agreeing with this sentiment, another reviewer recommended that EPA characterize and disclose the uncertainty associated with key model outputs, especially considering that some model outputs are answers to the principal study questions of the site reassessment (SE). Repeating a comment he made earlier in the meeting, another reviewer noted that the uncertainty of predictions will increase (and the accuracy of predictions decrease) with time throughout the forecasting period (RNo).

Referring to the reviewers' recommendations on how EPA can improve its modeling of sediment transport processes (see Section 5.1), one reviewer indicated that the recommendations, though detailed (e.g., reparameterizing sediment resuspension algorithms), all focus on improvements that ultimately will help EPA better quantify uncertainties in the current model formulation (RNa).

4.2 Responses to General Question 2

The second general question asked the reviewers:

> How well have the uncertainties in the models been addressed? How important are the model uncertainties to the ability of the models to help answer the principal study questions? How important are the model uncertainties to the use of model outputs as inputs to the human health and ecological risk assessments?

The reviewers agreed that uncertainties in the forecast models have very important implications on the ecological and human health risk assessments, but they offered few additional comments. Consistent with his earlier remarks, one reviewer thought the text in much of the RBMR adequately acknowledged uncertainties associated with model inputs, but he thought the conclusions of the reports did not (PL). Noting that the uncertainties associated with the 70-year forecast projections are not well presented, and perhaps not fully understood, another reviewer suggested that EPA better characterize uncertainties in the model predictions that are ultimately used to answer the principal study questions of the site reassessment (SE). Yet another reviewer agreed, noting that uncertainties in the hydrodynamic model, sediment transport model, mass balances, and bioaccumulation model are all reflected in the estimates of PCB concentrations in fish (WL).

Some reviewers recommended future sampling and research projects that might help reduce uncertainty in model predictions. Concerned about the implications of changes in the Upper Hudson River ecosystem (e.g., invasion of zebra mussels), one reviewer recommended that ongoing fish sampling programs include stable nitrogen measurements, which he suspected would reflect changes in dietary habits across the various trophic levels (RNo). Noting that such measurements are relatively inexpensive, another reviewer supported the recommendation, but she was not entirely convinced that stable nitrogen measurements would reveal changes in the aquatic food web due to the highly variable PCB levels observed in all media in the Thompson Island Pool (EB). Another reviewer recommended that EPA investigate, perhaps through laboratory experiments, the mechanisms underlying sediment-water mass transport in the Upper Hudson River, especially considering that this phenomenon accounts for nearly half the PCB loads to the water column (PL).

4.3 Responses to General Question 3

Three reviewers responded to the third general question, which asked: "It is easy to get caught up with modeling details and miss the overall message of the models. Do you believe that the report appropriately captures the 'big picture' from the information synthesized and generated by the models?" The three reviewers agreed that the models generally account for what they considered to be the "big picture" for PCBs in the Upper Hudson River, but all three added caveats to their responses, as described below (PL,RNa,RNo).

First, one reviewer indicated that the "big picture" for this site includes the entire Hudson River, not just the Upper Hudson River (PL). In this regard, he stressed that decreases in PCB levels over the next 70 years in the Upper Hudson River are probably associated with continued loads of PCBs to the Lower Hudson River. He suggested that EPA's reassessment continue to be mindful of the ultimate fate of PCBs throughout the Hudson River. Second, another reviewer noted that EPA's models account for the "big picture" for Tri+ PCBs, but do not do so for representative congeners or the most toxic congeners (RNo). He reviewed the implications of the emphasis on Tri+ PCBs in his responses to many other charge questions (e.g., see Sections 2.1, 2.5, 3.1, and 3.3). Third, a different reviewer thought EPA's models account for the "big picture" so far as long-term trends are concerned, but he added that the model lacks detail in several areas, such as spatial resolution and parameterization of sediment resuspension, to address some of the uncertainties in the forecast results (RNa). In short, this reviewer suggested that emphasis on the "big picture" in the modeling effort should not be at the expense of characterizing model uncertainty and understanding finer scale processes.

4.4 Responses to General Question 4

The reviewers provided several "other comments or concerns with the Revised Baseline Modeling Report not covered by the charge questions," as requested by general question 4. A review of their general comments follows. The first four bullet items below summarize general comments on the fate and transport models, and the remaining three bullet items summarize general comments on the bioaccumulation models.

- *Comments relevant to volatilization.* One reviewer offered two suggestions for improving how EPA modeled volatilization (SE). First, this reviewer recommended that EPA use a set of Henry's Law constants, and temperature dependence for these constants, recently reported in the scientific literature, as documented in his premeeting comments. He suspected that the model would predict higher volatilization rates using these constants, if no other aspect of the volatilization algorithms were modified. Second, this reviewer indicated that the model might not have incorporated realistic assumptions of ambient air concentrations for estimating volatilization. Based on his own research of volatilization of PCBs in Raritan Bay, New Jersey, this reviewer suspected that the ambient air concentrations of PCBs reported in the RBMR are considerably lower than actual air concentrations in the Hudson River valley. Noting that understating the ambient air concentrations of PCBs effectively causes EPA to overstate the driving force for volatilization, this reviewer suspected that incorporating more realistic estimates of ambient air concentrations of PCBs would cause the model to predict lower volatilization rates, if no other aspect of the algorithms were modified. Incorporating these two changes in the model, according to this reviewer, might cause some changes in the overall PCB mass balance.

- *Comments regarding the model's representation of PCBs.* Reiterating a comment discussed earlier (see Sections 2.1, 2.4, and 2.5), one reviewer emphasized that EPA, by focusing its efforts on modeling Tri+ PCBs, cannot characterize changes in the composition of PCBs (RNo). He noted that volatilization losses of lower chlorinated

PCBs was just one process that would lead to the composition of PCBs in the Upper Hudson River gradually shifting to higher chlorinated, and generally more toxic, congeners. This reviewer again suggested that EPA model future conditions of congeners that exhibit widely varying chemical and physical properties to provide some insight as to how the composition of Tri+ PCBs will change in future years.

- *Comments regarding the value of Manning's "n."* For three reasons, one reviewer suggested that EPA reconsider its calibration of Manning's "n," a parameter used in the hydrodynamic model (GL). First, this reviewer noted that the calibrated value of Manning's "n" for the main channel documented in the RBMR (i.e., 0.02) is not consistent with the range of values that have been reported in the literature for the Upper Hudson river (i.e., 0.027 to 0.035, see Table 3-1, RBMR, Volume 2). Second, the reviewer indicated that EPA based its calibration of Manning's "n" on a flow rate of 30,000 cubic feet per second—a flow rate she did not consider representative of the average conditions in the river. Third, this reviewer explained that the modeling results using a Manning's "n" value of 0.02 are not in as good agreement with rating curves as the RBMR implies (see Table 3-3, RBMR, Volume 2). She explained that Table 3-3 compares observed and predicted data reported in the NGVD (National Geodetic Vertical Datum) system, but she thought a more appropriate comparison would be in terms of the actual water depths. Using this alternate approach, this reviewer indicated that the model calculates depths as much as 15 percent different than those reported by the rating curves.

  Given these concerns, this reviewer recommended that EPA calibrate the value for Manning's "n" using low-flow conditions and ensure that the calibrated value is more consistent with those documented in the literature. She added that inaccurate calibration of Manning's "n" would translate into inaccurate predictions in velocity fields, and therefore in shear stresses as well.

- *Comments regarding the computation of shear stresses for model segments.* One reviewer questioned why EPA computed a cross-sectional average shear stress in its hydrodynamic model, rather than calculating localized shear stresses (RNa). According to this reviewer, EPA's approach loses some of the spatial resolution that the hydrodynamic model offers. When asked to clarify its use of the cross-sectional average shear stress, EPA explained that the models simply rely on the approach originally documented in the RMA-2V hydrodynamic model. The reviewer made no additional comments in response to this clarification.

- *Uncertainty in the bioaccumulation models.* One reviewer thought the conclusions in Volume 3 of the RBMR should acknowledge the uncertainty in model forecasts (PL). Specifically, he did not think predictions of the exact year in which fish concentrations are expected to decline to a certain level account for model uncertainty. This reviewer added that the text throughout the RBMR reports presents adequate discussions of uncertainty, but the conclusions do not. For more information on the reviewers' opinions on model uncertainty, refer to the summary of general questions 1 and 2 (see Sections 4.1 and 4.2).

- *Errors in Volumes 3 and 4 of the RBMR.* One reviewer noted that she found several errors in Volumes 3 and 4 of the RBMR (GL). As examples, she indicated that the data in Table 6-4 of Volume 4 were not calculated according to the approach outlined in the report text. Further, she added that the summary of Table 6-4 on page 81 is inconsistent with the data reported in the table. Additional examples of errors identified by this reviewer are documented in a set of supplemental comments, included in this report as part of Appendix C. Similarly, another reviewer noted that the description of Figure 6-6 on page 82 is inconsistent with what the figure actually portrays (RNo).

- *Insufficient time for the peer review.* One reviewer commented that she did not have sufficient time to review the RBMR (EB). She indicated that she might have provided more extensive and focused comments, had the review period been longer or the review documents provided earlier.

5.0 REVIEWERS' OVERALL RECOMMENDATIONS

After answering the specific and general questions in the charge, and after listening to the second set of observer comments, the reviewers reconvened to provide their final findings on EPA's reports. The reviewers listed recommendations for EPA and then offered their individual perspectives on EPA's reports, during which other reviewers did not discuss or debate each reviewer's final recommendations. Section 5.1 summarizes the reviewers' recommendations to EPA for improving the fate and transport and bioaccumulation models; Section 5.2 presents their individual recommendations.

5.1 Recommendations to EPA

Based on their responses to the charge questions, as documented in Section 3 and 4, the reviewers prepared the following list of key recommendations for EPA. The first five bullet items below are recommendations pertaining to the fate and transport models, the next four bullet items pertain to the bioaccumulation models, and the final bullet item applies to EPA's overall modeling effort.

- *Test critical assumptions/findings on net deposition, burial rates, and depth of mixing.* The reviewers agreed that net deposition, burial rates, and depth of mixing are critically important factors to the future exposure and release of PCBs from the river bed. However, they also agreed that EPA relied on limited direct data (interpretation of isolated sediment cores) and on the results of the SEDZL model (which was calibrated against the same limited data) to characterize these factors. The reviewers recommended that EPA assess the following two options for obtaining direct evidence to better substantiate the model assumptions and findings related to net deposition, burial rates, and depth of mixing.

  (1) The reviewers recommended that EPA assess the accuracy and spatial resolution of the 1991 bathymetry survey and the potential accuracy of a new bathymetry survey, and determine whether conducting such a survey would provide insight into the issues of net deposition and sediment burial rates between 1991 and the present. If the utility of conducting a new survey is justified, the reviewers recommended that EPA perform the survey, assess erosion/deposition through the bathymetry comparison, and interpret the comparison results in light of the HUDTOX model assumptions and findings.

(2) The reviewers recommended that EPA consider taking new sediment cores at selected sites, measuring depths of Cs-137 peaks, and comparing the depths of these peaks to those in previous sampling efforts to assess burial rates used in the model.

- *Sensitivity to spatial resolution.* The reviewers recommended that EPA test the sensitivity of model forecasts to the model's horizontal and vertical spatial resolution. With respect to horizontal segmentation, the sensitivity analysis must consider the influence of spatial resolution on hydrodynamics (the water balance) and sediment dynamics (the solids balance).

- *Review parameterization of sediment resuspension and settling.* The reviewers agreed that the model's treatment of resuspension and settling of cohesive and non-cohesive sediment appears to capture the overall export of sediment from the system and between various internal reaches. However, due to possible errors in resuspension algorithms, the assumption of a constant settling velocity, the neglect of non-cohesive bed load, and low spatial resolution, they were concerned that the model may significantly underestimate the degree of sediment redistribution and thus underestimate the exposure of PCBs in sediments. Given these concerns, the reviewers gave four recommendations to improve the parameterization of sediment resuspension and settling:

(1) The reviewers recommended that EPA assess the implications of parameterizing resuspension of cohesive sediments using shaker and annual flume tests in light of recent experiments with sedflume (e.g., Lick et al., 1995b) at shear stresses more compatible with those in the Hudson River during high flow events.

(2) The reviewers indicated that the DOSM and HUDTOX models both appear to assume that resuspension of cohesive sediments is limited in total erosion potential to the sediment that erodes over a 1-hour period at the peak of the event—an assumption inconsistent with findings reported in the literature (Lick et al., 1995a; Gailani et al., 1991) and therefore possibly incorrect. Suspecting that EPA might be underestimating resuspension by a factor of 5 to 20, the reviewers recommended that EPA check, correct, and/or justify its approach to modeling resuspension of cohesive sediments through the rising limb of flow events.

(3) The reviewers noted that EPA's use of a constant and low settling velocity for cohesive sediments is at odds with the actual mechanism of settling (i.e., much higher settling velocities occurring at higher flows and lower settling velocities occurring at lower flows). If adjustments to the resuspension parameterization are made as a result of item (2), the reviewers indicated that EPA may need to develop a more mechanistic (variable) description of settling velocities.

(4) Agreeing that bed load may be an important factor in the redistribution and mixing of non-cohesive sediments and, in turn, the exposure of PCBs associated with sediments, the reviewers recommended that EPA consider incorporating bed load transport (following

approaches similar to those used in the HEC-6 model) into the HUDTOX model to assess the importance of this process.

- *Review the solids balance.* The reviewers indicated that river flow increases by a factor of 1.5 between Fort Edward and Waterford, yet the solids loads used in HUDTOX increases by a factor of 5.7 for the same reach. Concerned about the implications of the solids balance and the fact that the exact amount and location from which solids enter the river are largely unknown, the reviewers recommended that EPA verify its solids balance using data from locations other than Fort Edward and Waterford.

- *Evaluate additional scenarios for the 100-year flood.* Given that the HUDTOX model predicts that the buried PCB inventory in some stretches of the Upper Hudson River will be near the sediment surface in selected forecast years, the reviewers recommended that EPA simulate the effects of a 100-year flood occurring at various times, in addition to during the spring flood of 1998.

- *Update constants input to the FISHRAND model.* The reviewers noted that the FISHRAND model is based on several uncertain parameters and that EPA assigned values to some input constants that are inconsistent with new information reported in the literature. The reviewers recommended that EPA reevaluate the constants used in the model with respect to the new information available.

- *Evaluate aspects of the FISHRAND calibration procedure.* Though they agreed that the calibration of the FISHRAND model effectively reproduced historical trends in fish sampling data, the reviewers were concerned about some aspects of the model calibration. Specifically, they noted that values assigned to certain parameters (e.g., lipid contents in invertebrates) were unrealistic; they identified other parameters (e.g., octanol-water partition coefficients) that should be specified rather than calibrated; and they noted that some parameters were calibrated to different values for different river segments. The reviewers recommended that EPA evaluate these concerns and provide justification for these aspects of the model calibration.

- *Investigate statistical approaches to estimating PCB concentrations in fish.* Based on statistical analyses presented at the peer review meeting, the reviewers recommended that EPA investigate the implications of steady-state assumptions on the overall bioaccumulation modeling approach (e.g., possibly comparing the FISHRAND predictions to fish PCB concentrations computed directly from the HUDTOX results, assuming steady-state conditions apply in the various river segments).

- *Predict PCB levels for age/size classes in selected fish species.* The reviewers recommended that EPA revise its bioaccumulation models to characterize how PCB levels vary with age/size classes of fish, particularly for species near the top of the aquatic food web, or provide justification for why this is not necessary.

- *Perform congener-specific forecasts.* Suspecting that the composition of Tri+ PCBs in the Upper Hudson River sediment, water, and fish will change over the 70-year forecast period, the reviewers recommended that EPA run its fate and transport and bioaccumulation models for a subset of PCB congeners with varying chemical and physical properties. One reviewer thought congener-specific results are needed for the site's risk assessments.

5.2 Peer Reviewers' Final Statements

The peer review meeting concluded with each peer reviewer providing closing statements on the reports, including an overall recommendation in response to the final question in the charge: "Based on your review of the information provided, please identify and submit an explanation of your overall recommendation for each (separately) the fate and transport and bioaccumulation models.

1. Acceptable as is
2. Acceptable with minor revision (as indicated)
3. Acceptable with major revision (as outlined)
4. Not acceptable (under any circumstance)"

The reviewers provided summary statements separately for the fate and transport models and the bioaccumulation models; these statements are reviewed in Sections 5.2.1 and 5.2.2, respectively.

5.2.1 Final Statements for the Fate and Transport Models

In summary, six of the reviewers found EPA's fate and transport models to be acceptable with minor revisions; one reviewer found the models acceptable, but did not classify the necessary revisions as minor or major. A detailed summary of the peer reviewers' final statements on EPA's fate and transport models, in the order they were given, follows:

- *Dr. Ross Norstrom.* Noting that the human health and ecological risk assessments would benefit from congener-specific data, Dr. Norstrom recommended that EPA perform long-range forecasts on individual PCB congeners that have a wide range of chemical and physical properties. He explained that such modeling might reveal how the composition of PCBs in the Upper Hudson River change with time, thus providing insight into the suitability of using Tri+ PCBs as an input to the risk assessments. Dr. Norstrom then indicated that EPA's fate and transport models are acceptable with minor revisions.

- *Dr. Per Larsson.* Stressing that the Hudson River is a dynamic system, Dr. Larsson concluded that sediment redistribution is an important process, especially in the Thompson Island Pool, which he characterized as a major source of PCBs to the water column. Dr. Larsson's main concern was with the model predictions and their inherent uncertainty; he recommended that EPA acknowledge this uncertainty in the RBMR conclusions. Overall, Dr. Larsson indicated that EPA's fate and transport modeling principles are technically sound and useful for understanding the fate of PCBs in the Upper Hudson River. He found EPA's fate and transport models acceptable with minor revisions.

- *Dr . Grace Luk.* Dr. Luk indicated that EPA's fate and transport models were very well documented in the RBMR and that the model calibration and hindcasting were sound. Based on this, Dr. Luk concluded that EPA's fate and transport modeling was technically adequate, competently performed, and properly documented. Her main suggestions to EPA were to refine the solids balance, to conduct more analyses of the sediment-water mass transfer mechanisms, and to evaluate the impacts of 100-year floods under various scenarios. Dr. Luk indicated that EPA's fate and transport models are acceptable with minor revisions.

- *Dr. Winston Lung.* In his final statements, Dr. Lung primarily reflected on two issues raised during the observer comments. First, regarding the issue of predicted abrupt increases in surface sediment PCB concentrations, Dr. Lung suggested that EPA explore and fully address the concerns regarding this model output. Second, regarding the consistency between the conclusions reported in the RBMR and LRC, Dr. Lung indicated that the reviewers' summary statements were somewhat vague. He added that he believed the HUDTOX modeling results were valid, because they evaluated PCB transport from first principles. Dr. Lung concluded that EPA's fate and transport models are acceptable with minor revisions, provided EPA address the reviewers' recommendations outlined in Section 5.1.

- *Dr. Robert Nairn.* Dr. Nairn's final statements addressed two issues, river bed dynamics and tributary loadings. First, Dr. Nairn indicated that EPA's fate and transport models are insufficient to capture uncertainties in sediment redistribution. Dr. Nairn suspected that the effects of sediment redistribution might be reflected in model calibration parameters, but he feared that this empiricism might prevent the models from forecasting future sediment transport processes pertaining to remediation actions correctly. Dr. Nairn noted that EPA can improve its representation of sediment redistribution in the models by addressing the reviewers' recommendations listed in Section 5.1. Second, Dr. Nairn indicated that EPA has not conducted a sensitivity analysis to evaluate the combined effect of uncertainties in the rating curves and uncertainties in future changes in the watershed. Dr. Nairn recommended that EPA conduct this analysis to evaluate the impacts of the arbitrary increases of tributary solids loadings incorporated in the models. Dr. Nairn found EPA's fate and transport models acceptable, but he did not want to classify the necessary revisions as minor or major without knowing the extent to which the reviewers' recommendations are reflected in the models.

- *Dr. Ellen Bentzen.* Dr. Bentzen first indicated that she was impressed with EPA's overall modeling effort, and she commended the Agency and all other participants on their efforts on the Hudson River site. Dr. Bentzen then stressed the importance of peer review, noting that Canada does not have a comparable level of peer review for similar work products. Concerned that much of the work in EPA's reassessment has been conducted as individual tasks, Dr. Bentzen recommended that a comprehensive peer review of the entire reassessment take place before EPA implements any remedial action. Dr. Bentzen also recommended that EPA give some of the peer reviewers the opportunity to evaluate how the Agency responds to their comments. Overall, Dr. Bentzen indicated that EPA's fate and transport models are acceptable, with revisions she considered to be minor relative to EPA's overall efforts in the reassessment.

- *Dr. Steve Eisenreich.* Dr. Eisenreich commended EPA in its success in designing models that adequately capture the historical trends in PCB concentrations in water and fish, which gave him confidence in the model's ability to predict future trends. However, Dr. Eisenreich stressed that the HUDTOX model includes several key parameters (settling velocities, sediment mixing rates, and sediment mixing depths) that were calibrated and not evaluated mechanistically, thus causing the model to lose some scientific reality. He added that this lack of mechanistic understanding introduces uncertainty into the model's depiction of future trends. Dr. Eisenreich recommended that future work on the model focus on understanding the magnitude of the model uncertainties, and he concluded that EPA's fate and transport models are acceptable with minor revisions.

5.2.2 Final Statements for the Bioaccumulation Models

In summary, four peer reviewers concluded that EPA's bioaccumulation models are acceptable with minor revisions; one concluded that the models are acceptable with major revisions; and the two reviewers who were expert primarily with water quality and sediment transport modeling did not offer recommendations on the bioaccumulation models. A detailed summary of the peer reviewers' final statements on EPA's bioaccumulation models, in the order they were given, follows:

- *Dr. Ross Norstrom.* Dr. Norstrom listed three recommendations for improving the bioaccumulation models. First, he recommended that EPA update its version of the Gobas model by using input parameters consistent with those reported in the scientific literature and by incorporating a less generic depiction of fish growth. Second, Dr. Norstrom recommended that EPA model future trends for a set of PCB congeners with varying properties. Dr. Norstrom added that EPA would have to recalibrate its models to run congener-specific forecasts, because $\log(K_{ow})$ would no longer need to be a distributed calibration parameter. Noting that EPA would likely have to adjust other parameters to implement this recommendation, Dr. Norstrom indicated that conducting congener-specific analyses would be a more rigorous test of the model's performance. Third, Dr. Norstrom thought EPA should clarify how age/size classes are handled in the models and provide information on how PCB concentrations in fish vary with age/size classes. Overall, Dr. Norstrom found EPA's bioaccumulation models to be acceptable with minor revisions.

- *Dr. Per Larsson.* Dr. Larsson indicated that his recommendations on the fate and transport models (see Section 5.2.1) also apply to the bioaccumulation models, because the models are coupled. Consistent with the coupling of the models, Dr. Larsson emphasized that bioaccumulation is driven primarily by PCBs in the sediment and the water column. Though he had concerns about the ability of the bioaccumulation models to forecast future PCB levels, Dr. Larsson concluded that they are sound in principle and offer insight into PCB uptake processes in fish of the Upper Hudson River. Dr. Larsson indicated that EPA's bioaccumulation models are acceptable with minor revisions.

- *Dr . Grace Luk.* In comparison to the RBMR reports on the fate and transport models, Dr. Luk found the reports on the bioaccumulation models to be of poor quality, with many editorial and typographical errors. Dr. Luk added that her review of the document was hampered by the reports' obscure presentation of information. Dr. Luk noted that EPA presented the bivariate BAF and probabilistic models to provide complementary views on bioaccumulation; however, she thought these models, because they were not used for forecast simulations, offered little information relevant to the reassessment's principal study questions. For the same reason, Dr. Luk questioned the need for documenting the FISHPATH model in the RBMR. Despite these concerns, Dr. Luk concluded that the FISHRAND model, coupled with the HUDTOX model, is an excellent tool for answering the principal study questions. Overall, Dr. Luk concluded that EPA's bioaccumulation models are acceptable with major revisions. Specifically, she thought EPA should: provide better justification for using the three-phase partitioning coefficients; analyze bioaccumulation for age/size classes of fish species near the top of the aquatic feed chain; incorporate more thorough lipid analyses; and improve model performance in predicting lipid-normalized PCB concentrations.

- *Dr. Winston Lung.* Dr. Lung indicated that model calibration seemed to be a key issue in the validity of EPA's bioaccumulation models. Noting that his expertise lies primarily with water quality modeling, Dr. Lung did not provide a final recommendation on EPA's bioaccumulation models.

- *Dr. Robert Nairn.* Noting that his expertise relates primarily to hydrodynamic and sediment transport modeling, Dr. Nairn did not provide a final recommendation on EPA's bioaccumulation models.

- *Dr. Ellen Bentzen.* Dr. Bentzen indicated that the FISHRAND model addresses the basic aspects of bioaccumulation, but she recommended that EPA update parameters in the model to be consistent with those reported in the literature. Dr. Bentzen acknowledged that the science of bioaccumulation modeling is still emerging, but, noting that the underlying mechanisms of bioaccumulation in the Hudson River are not unique, she suggested that EPA make greater use of modeling studies published for other systems. Dr. Bentzen added that EPA needs to improve its statistical analyses in the RBMR, but she considered such revisions to be relatively minor. Overall, Dr. Bentzen concluded that EPA's bioaccumulation models are acceptable with minor revisions.

- *Dr. Steve Eisenreich.* Dr. Eisenreich considered the FISHRAND model, as linked with the HUDTOX model, to be an effective tool for evaluating bioaccumulation. Reflecting on Dr. Bentzen's interpretation of the historical sampling data (see Section 3.3), Dr. Eisenreich recommended that EPA assess the implications of the PCBs in the sediment, water, and fish in individual reaches of the Upper Hudson River being in steady state. He added that the strong correlations between sediment/water/fish PCB levels for species at different trophic levels was particularly compelling. Dr. Eisenreich then suggested that EPA consider verifying the FISHRAND predictions by using the steady state assumptions and the HUDTOX sediment and water forecast results. Overall, Dr. Eisenreich found EPA's bioaccumulation models to be acceptable with minor revisions.

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