APPENDIX A

LIST OF EXPERT PEER REVIEWERS

United States
Environmental Protection Agency
Region 2

Hudson River PCBs Site Modeling Approach Peer Review

Sheraton Saratoga Springs Hotel and Conference Center
Saratoga Springs, NY
September 9-10, 1998

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APPENDIX B

CHARGE TO EXPERT PEER REVIEWERS

United States
Environmental Protection Agency
Region 2

Hudson River PCBs Site
Modeling Approach Peer Review

Sheraton Saratoga Springs Hotel and Conference Center
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REVISED CHARGE TO REVIEWERS

Members of this peer review will be tasked to determine whether the models being used to support the decision-making process for the Reassessment, and the assumptions therein, are appropriate. The peer reviewers will base their assessment on the review the Preliminary Model Calibration Report (PMCR), an updated Technical Scope of Work for the Baseline Modeling Report (Appendix B of the PMCR) and the responses to selected comments received from stakeholders during the public comment period on the PMCR.

In October 1996, EPA released the Preliminary Model Calibration Report (PMCR), which described the models, datasets and assumptions being used as part of the Hudson River PCB Reassessment RI/FS. The PMCR represents the status of the preliminary PCB modeling effort as of Fall 1995. Datasets, database corrections and other pertinent information which became available after October 1995 were not incorporated within the fate and transport modeling presented in the PMCR. The PMCR was an interim document prepared to describe work in progress and was not intended to be a conclusive report. In particular the HUDTOX model presented in the PMCR was not intended to be used as a predictive tool to assess remedial action scenarios. In addition, while time-varying mechanistic models of bioaccumulation will be used along with other models to predict fish body burdens, these models are not described in the PMCR.

The PMCR was not formally peer reviewed at the time of publication, but was distributed to interested parties who were invited to submit comments and questions. Written responses were made to all of these comments and questions. In addition, the work plan contained in Appendix B of the PMCR has been revised to reflect the ongoing work being conducted as part of the Baseline Modeling effort. Results from this effort will be presented in a Baseline Modeling Report that will be formally peer reviewed.

The peer reviewers are requested to determine whether the models being used to support the decision-making process for the Reassessment RI/FS, and the assumptions therein, are appropriate. The peer reviewers are not being asked whether they would conduct the work in the same manner, only whether the work being conducted will yield scientifically credible conclusions.

It is suggested that the reviewer first read the PMCR. The Responses to Comments provides information on the context of the PMCR within the overall modeling effort and additional details beyond the PMCR results. The current work plan as revised in June 1998 reflects the ongoing Baseline Modeling effort and revisions to some of the original modeling tasks proposed in Appendix B of the PMCR. In addition, the USEPA/TAMS Phase 2 database has been considerably revised. New datasets have been added and some earlier datasets have been extensively revised.

The peer reviewers are asked to comment on the following:

A. Is EPA using appropriate models, datasets and assumptions on which to base a scientifically credible decision?

B. Will the models, with the associated datasets and assumptions, be able to answer the following principal study questions as stated in the PMCR:

1. When will PCB levels in the fish population recover to levels meeting human health and ecological risk criteria under No Action?

2. Can remedies other than No Action significantly shorten the time required to achieve acceptable risk levels?

3. Are there contaminated sediments now buried and effectively sequestered from the food chain which are likely to become "reactivated" following a major flood, resulting in an increase in contamination of the fish population?

C. Specific questions:

1. Are the modeling approaches suitable for developing quantitative relationships between external forcing functions (e.g., hydraulic flows, solids and PCB loads, sediment initial conditions, etc.) and PCB concentrations in the water column, sediments and fish? Are the models adequate for discriminating between water-related and sediment-related sources of PCBs?

2. Are the spatial and temporal scales of the modeling approaches adequate to answer the principal study questions? If not, what levels of spatial and temporal resolution are required to answer these questions? What supporting data are required for calibration/ validation of these spatial and temporal scales?

3. It is contemplated that PCB concentrations in fish will be estimated using several modeling approaches: an empirical probabilistic model derived from Hudson River data, a steady state model that takes into account mechanisms of bioaccumulation body burdens, and a time-varying mechanistic model (not included in the PMCR). A bi-variate statistical model may also be used to provide insight into accumulations. This multi-model approach is being contemplated because of the uncertainties associated with any individual model. Is this a reasonable approach or should predictions be made using a single "best" model?

4. Is the level of process resolution[1] in the models adequate to answer the principal study questions? If not, what processes and what levels of resolution are required to answer these questions? What supporting data (such as data to support specifications of a mixed depth layer, solids and scour dynamics, groundwater inflow, etc.) are required for these processes and levels of resolution?

5. The results of the modeling effort will be used, in part, to support human and ecological risk assessments. In your judgment, will the models provide estimates adequate for this purpose?

D. Are there any changes to the work effort outlined in the revised work plan that would significantly improve the outcome?

E. In terms of evaluating the overall and specific effects and behavior of PCBs in the Hudson River, are there any serious flaws in the modeling approach (theory, structure, physical parameters, etc.) that would limit or invalidate any conclusions or further work based upon the results of these models?

Recommendations

Based on your reading and analysis of the information provided, please identify and submit an explanation of your overall recommendation for the modeling effort for the Hudson River PCB Reassessment RI/FS:

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

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1. The "level of process resolution" refers to the theoretical rigor of the equations used to describe the various processes affecting PCB fate and transport such as: settling, resuspension, volatilization, biological activity, partitioning, etc. An example of low process resolution is use of a constant value for the solids resuspension rate. A higher level of process resolution is use of a complex mathematical description of the physics involved in remobilizing bedded sediment particles (such as cohesive forces, bed shear stresses, etc.)

APPENDIX C

PREMEETING COMMENTS, ALPHABETIZED BY AUTHOR

Hudson River PCBs Site
Modeling Approach Peer Review

Premeeting Comments

Saratoga Springs, NY
September 9-10, 1998

Prepared by:
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ERG

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---

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Peer Reviewer Comments

Ellen Bentzen

Ellen Bentzen

Ellen Bentzen has a Ph.D. (1990) and an M.Sc. (1986) in aquatic ecology from the University of Waterloo and a B.Sc. (1982) in limnology from McGill University. For the past eight years, she has worked as an applied aquatic ecologist/environmental toxicologist Research Associate at Trent University, Peterborough, ON. Her initial research project at Trent was a study of how aquatic food web structure influences the concentration of persistent organic pollutants (POPs) in lake trout from Ontario lakes. This work instigated a number of related projects ranging from field studies of POPs in the lower part of aquatic food webs and food web structure to development of contaminant bioaccumulation models (ongoing research). She also has examined the role of food web structure and dissolved organic carbon in lake water on bioaccumulation of mercury in lake trout and other fish species. Distinct from these projects, she also has been collaborating with scientists from Texas A&M University on studies of microbial nutrient dynamics in the Sargasso Sea near Bermuda.

Ellen Bentzen is associated both with the Environmental Modeling Centre at Trent University, Peterborough, ON, working with Dr. Don Mackay, and with the St. Lawrence River Institute, Cornwall, ON (affiliated with the University of Ottawa, Ottawa, ON) working with Dr. David Lean. Both the Environmental Modeling Centre and the St. Lawrence River Institute have associations with industry and government agencies. She is currently completing a research paper which is a review of POPs in Lake Ontario biota for submission to *Environmental Reviews.* This paper includes temporal data for a number of organisms from Lake Ontario and an assessment of recent trends in contaminant concentrations. This review is under contract for the Canadian Chlorinated Chemical Council. A companion review is being prepared for POPs in St. Lawrence River biota. She also has been collaborating on the development of contaminant bioaccumulation models both for benthic invertebrates and for lake trout residing in different aquatic food webs. Results from her research have been presented at Society for Environmental Toxicology and Chemistry (SETAC), International Association for Great Lakes Research (IAGLR) and American Society of Limnology and Oceanography (ASLO). Recent research papers include: *The role of atmosphere in Great Lakes contamination; Nutrient-limited bacterioplankton growth in the Sargasso Sea; Role of food web structure on lipid and bioaccumulation of organic contaminants by lake trout (Salvelinus namaycush);* and *Size-structure and species composition of plankton communities in deep Ontario lakes with and without Mysis relicta and planktivorous fish.*

Ellen Bentzen

Peer Review of Hudson River PCBs: RI/FS Phase 2 Reports: Preliminary Model Calibration Report: Ellen Bentzen, Environmental Modelling Centre, Environmental & Resource Studies, Trent University, Peterborough, Ontario.

General comments to the PCMR:

There is a tremendous amount of important work described in the preliminary model calibration report (PCMR). A common problem of large projects entailing many collaborators (especially when from several agencies) is the project gains momentum in several directions and which then becomes a significant challenge to channel it into a single, cohesive directive. This appears to be the case with parts of this Hudson River assessment. Specifically, it is not clear how some of the various "submodels" or compartments will be unified, particularly with the goal of identifying future trends. There is an indication (although this may be an artifact of the somewhat unclear presentation of information) that a rigorous scientific protocol was not adopted which would a priori establish the appropriate tests or hypotheses, determine the best approach to examine these hypotheses and then follow by collecting the appropriate data. For example, despite the Phase 2 sampling was identified as a large effort, quite often the calibration of the models in the PMCR were hampered by the lack of appropriate data. A specific example: the upper Hudson flow calibration had water data for 6 of the segments and these appeared to be at limited times (low flow periods: however, the graphs were difficult to read clearly).

A problem with the PMCR report was the overall presentation. The organization was difficult to follow, and while it was explained that this is a standard format, it prevents an efficient and effective review process. Specifically, the style of introducing the different models in Chapter 3 (which were not completely presented), then following with the calibrations in later chapters made it necessary to return to earlier passages because it is not easy to remember all the salient information when one is not intimately familiar with the models or the system. Sources for many details and important assumptions used in the model development were identified as grey literature and inaccessible to external reviewers. It would have been useful to see more of the actual data, and as the data were summarized in a report released in 1997, this should have been possible. Figure and table titles did not identify all the pertinent details in the figures or tables: these should always be able to stand on their own with clearly identified contents. Units were not consistent throughout the report. On a positive note, chapters 8, 9 and 10 were fairly well written. It is noteworthy that these sections had numerous references to published studies.

Note: use of the word "METHODOLOGY". This is NOT synonymous with "method" but refers to the study of processes, just as BIOLOGY is not synonymous with biota (try looking up methods and methodology in the word perfect thesaurus)!

Questions to reviewers:

A. Is EPA using appropriate models, datasets and assumptions on which to base a scientifically credible decision?

This question is difficult to evaluate for several reasons. The revised scope of work for the Baseline Modeling identified that the models presented in the PCMR are being updated, and these updates include applying the HUDTOX model to a new segmentation scheme, further characterization of the Thompson Island Pool (TIP), modifications to the Lower Hudson River

Model, and use of further data, etc. Based upon the descriptions in the PCMR, the Upper Hudson River Mass Balance Model (HUDTOX) and TIP models do appear to be conceptually appropriate, however, the mathematical relationships were not given for the HUDTOX model nor were all assumptions identified; references to grey literature are not useful (e.g. Ambrose et al.; formulations in Thomann et al. 1989, etc). The fish body burden models as described in the PCMR may not be adequate to base scientifically credible decisions because they lack true predictive capacity (see below for further comments).

Datasets: The data used in various parts of the PCMR were not all identified or described. Data were available from the USEPA, NYSDEC, G.E., and USGS and also from private and academic studies. The Hudson River Database was described as very extensive, including 750000 records, with nearly half collected by the USEPA Phase 2 work plan. What constitutes a record? Are these 750000 data points (excluding the date, time, depth, etc, information)? The review process would have been facilitated by better understanding of the available data, especially the Phase 2 sampling program. How was the sampling program designed? There is an impression that sampling was ongoing at the same time that the models were being developed, instead of model development identifying sample needs. For example, 13 spatial segments were identified to represent the Upper Hudson, but water data were only collected from 6. The Phase 2 flow-averaged data appeared to be collected only during the low-flow event period (section 4.7.2). Granted, as this report is already two years old, much of our information is out of date at time of review.

B. Will the models, with the associated data sets and assumptions, be able to answer the following principal study questions as stated in the PCMR:

1. When will PCB levels in the fish population recover to levels meeting human health and ecological risk criteria under NO ACTION?

The current models do not incorporate any temporal component, nor are the fish body burden models actually predictive. An integrated, time-dependent environmental fate and food chain bioaccumulation model, such as described by Gobas et al. (1995: EST 29: 2038) is needed to simultaneously characterize the time response of PCBs in water, sediments, and biota to predicted changes in PCB concentrations in the river (from a modified HUDTOX model with temporal component). This will be more complex for the Hudson River because PCB declines may be more variable over time than in a lake such as Lake Ontario. Loadings from the sediments likely are more important than from a deep lake, and also more variable following high flow events.

An important variable to determine is the possible rate of PCB decline in the water column and interaction with sediments. While concentrations remain as high as shown for 1993, fish concentrations will also remain elevated.

2. Can remedies other than NO ACTION significantly shorten the time required to achieve acceptable risk levels?

No information was provided to discuss possible remedies. It would be preliminary to offer conclusions until the complete mass balance for the river is finalized and all sources and sinks for PCBs identified. However, remedies such as dredging can have disturbing consequences to the ecosystem, which would need to be characterized independent of the PCB fate models. Acceptable risk levels need to be identified in this context: acceptable to human consumption or to wildlife. If the latter, levels which are 'not safe' need to be identified.

3. Are there contaminated sediments now buried and effectively sequestered from the food chain which are likely to become "reactivated" following a major flood event, resulting in an increase in contamination of the fish population?

The results from the TIP model would suggest it is possible that buried, contaminated sediments may be sufficiently disturbed by flood events to result in increased loading of PCBs back into the water column (resuspension). Different severities of flood events may be examined (e.g. 5 year or 100 year floods). However, whether the resuspended material is bioavailable to the food chain is not identified. The preliminary conclusions that even a large flood contributes a small percent of the PCB load is intriguing, but is this reconciled with the implication that TIP contributes 50% of the PCB load down river?

C. Specific questions:

1. Are the modeling approaches suitable for developing quantitative relationships between external forcing functions and PCB concentrations in the water, sediments and fish? Are the models adequate for discriminating between water related and sediment related sources of PCBs?

There are a number of issues to address with this question. The sources of PCBs to the river have not been fully quantified or identified. For example, what is the source of PCBs from the Mohawk River (17% loading into the Hudson). Atmospheric loading has not been quantified. It was mentioned there is 'much uncertainty in the tributary PCB loadings for the HUDTOX calibration period because of .... just 6 sample collections over the 9-month 1993 Phase 2" sampling. Have further samples been collected and if so, do these change the contribution of the Mohawk? If the Mohawk has no known sources for PCBs and the 17% estimate inaccurate, thus all the results may have high error. 74% of the loading was across the Fort Edward boundary, presumeably from G.E. I find it curious that no data was collected from a segment uptream from Hudson Falls, to characterize base loading of PCBs (this would largely be a measure of atmospheric deposition onto the river and the watershed if there are no known industrial sites). What is the loading source of PCBs from GE? Is this from contaminated sediments near the sites, or seepage from inadequate containment?

--Section 4.4.2: Solids & PCB loads: TSS and flow and PCBs and TSS were correlated with the USGS and USEPA data but not GE data, the latter which were quite variable. Does this suggest that the GE data are less reliable (perhaps because of differences in analytical protocol, p. 4-3?). This also seems to be the case for the sediment data discussed on p. 4-13. How do these influence the model estimates? (Note also that no descriptive statistics for these correlations were given).

-Section 4.4.5, p. 4-13: If the estimate of PCB congener #4, derived as 78% of the estimated sum of PCB congener BZ#4+10, which in turn was estimated as double the GE Peak #53 concentration, was sometimes greater than the estimated total PCB concentrations, this casts doubt over the calibration process used to convert any of the GE data to be comparable to Phase 2 data. Or, the ratio of BZ#4 to BZ#10 in sediments is not the same as for water?

These are some factors which influence quantifying relationships for PCBs in water and sediments. Discriminating between water and sediment related sources for fish PCBs cannot be adequately addressed by the described fish body burden models. A bioenergetics based bioaccumulation model is required to identify relative contributions of water and sediments to fish PCB burdens. This point is elaborated below (Question E).

2. Are the spatial and temporal scales of the modeling approaches adequate to answer the principal study questions? If not, what levels of spatial and temporal resolution are required to answer these questions? What supporting data are required for calibration/validation of these spatial and temporal scales?

As already discussed, no temporal scales have been incorporated into the models in the PCMR, although the revised scope for baseline monitoring indicates this is underway. Overall, the spatial scales are well delineated, although further resolution in Thompson Island Pool is planned and should be useful. However, if the data are not available to match the model segments, further detail in the model cannot be validated. As discussed previously, the model segmentation and sampling (Phase 2) were not well matched for the HUDTOX model, both in terms of lack of available data in all segments and characterizing the flow/low flow events.

The principal study area for the Upper Hudson River begins at Fort Edward. Why are there not data collected upstream from Hudson Falls? These would be useful as baseline data (and would incorporate the contribution of atmospheric loading which currently was unknown and data from Green Bay was used). It was noted on page 4-10 that this is indeed a source of uncertainty in the HUDTOX model.

3. Estimating PCB concentrations in fish using several models: empirical probabilistic model, steady-state bioaccumulation model (not described in the PCMR), time-varying mechanistic model (not included in the PCMR); the bivariate statistical model may be used to provide insight. Use of multiple models may be useful because of uncertainties in any one model. Is this a reasonable approach or should predictions be made using a single "best" model?

It is a useful exercise to compare the results of different models because they differ in their assumptions and in the constant parameters used. However, it should be established *a priori* the purpose of each model, how each model compares and differ in their assumptions and parameterizations and their limitations. Also, available data need to be considered; the best model may not be useful if the data are not adequate. Otherwise, if a number of models are run and produce different outcomes, then post priori explanations may not be adequate.

4. Is the level of process resolution in the models adequate to answer the principal study questions? If not, what processes and what levels of resolution are required to answer these questions? What supporting data are required for these processes and levels of resolution?

All assumptions and parameters used in these models need to be clearly identified. For example (but not inclusive), growth and respiration rates for white perch and striped bass, fish lipid content*, bioconcentration factors, gill efficiency transfer, oxygen consumptions, PCB excretion rates, PCB loss rates across gills, chemical assimilation efficiencies, feeding rates, estimates of dissolved, available PCBs vs total water column PCBs & role of water column carbon-based solids in reducing bioavailability of PCBs

p. 8-9: there is some confusion here (as elsewhere)- "variability may also reflect differing lipid compositions, with correspondingly different rates of uptake of lipophilic compounds, between different fish species". Concentration of PCBs in the food, PCB assimilation efficiency, food assimilation efficiency, and bioenergetics of the individual fish species (and hence physical/chemical factors which influence bioenergetics): these factors influence rates of uptake of PCBs. FAT content of the fish influences primarily the rate of depuration (loss) of the contaminant. Hence, fatter fish retain PCBs to a greater extent than skinny fish.

*Note: no where is there a discussion of fish lipid estimates and yet this is a parameter which is highly critical to both the fish body burden approaches discussed here (this is a serious problem but which is rarely addressed elsewhere as well). How was lipid estimated, using what solvents? In addition, no where was it described how lipids (and PCBs) were measured in the fish species: are these whole fish or fish muscle (skin on or off). Whole fish versus muscle can have a consequence when comparing among fish because some fish store fats in muscles (such as salmonids) while others store them predominantly around viscera (such as pike). What solvents were used in the extraction of PCBs and lipid? Randall et al. (1991, 1998) demonstrated huge and significant differences in lipid estimation depending upon choice of solvents and I have noted this in comparing Lake Ontario lake trout estimates between analyses conducted by American or Canadian agencies. Extraction procedures also influence PCB estimates. Any QAQC (quality assurance quality control) done with other laboratories?

Section 4.7: Calibration Results;

The constant solids gross settling velocity of 2.0 m/day was chosen but not very well justified (this includes the response to the comments). Also, (p. 4-19, top) long term solids deposition rates ranged from 0.5 to 5 cm/year, but a value of 0.22 cm/yr was used in the model for burial velocity. The derivation of this value was not clear.

D. Are there any changes to the work effort outlined in the revised work plan that would significantly improve the outcome?

Some aspects of this question have been addressed above. Another proposed effort is to use the Gobas food chain bioaccumulation model, which is a valid idea (although note that model has assumptions which are not all correct, either; these, however, may be specific to the Lake Ontario ecosystem). The description of the Gobas (1993) food chain model (p. 8-2 and in the revised scope) is slightly confused. The model does not "focus specifically upon food digestion and absorption in the gut". This latter mechanism refers to another paper by Gobas which describes how food is digested and contaminants absorbed against a fugacity gradient and which results in biomagnification of the contaminant in the predator organism relative to their prey, hence the fugacity of the predator exceeds prey (=biomagnification). A key factor of the Gobas food chain bioaccumulation model which is not incorporated into the probabilistic model is that the food web is run as a unit which combines the toxicokinetics of PCB uptake, elimination and bioaccumulation in individual organisms with the trophodynamics of the food webs. Thus any changes in the food web structure or physical characteristics (due to changes in population abundance, diet of predators, temperature, etc) will modify the response in other members. Pieces of this approach are suggested in the probabilistic model but the final model was not described thus it is not possible to evaluate the effectiveness of the model. Note the 1993 Gobas food chain model is available in an easy to use format (Visual Basic, with plug in values for diet composition, lipid content, number of predator and prey species in the food chain, plus water & sediment concentrations of contaminants, etc) and produces lovely graphs of the outcome, but it is his more recent temporally-based model (1995) which may be of more use to adapt to the Hudson.

E. In terms of evaluating the overall and specific effects and behaviour of PCBs in the Hudson River, are there any serious flaws in the modelling approach that would limit or invalidate any conclusions or further work based upon the results of these models?

Calibration of the HUDTOX model: (p. 4-20) The goodness-of-fit test results for both TSS and PCBs are curious. Looking at the predicted vs observed plot suggests a positive bias; this is also shown for the means for each segment shown in Table 4-14. I perhaps have missed something in the derivations of these data: why does the model have high variance? In theory, (not including Monte Carlo or sensitivity analyses) the model has no variance. Would this not be variability on a temporal scale as opposed to error, while the variance associated with the observed data is from repeat measures for that segment. Therefore, the variability estimated with the data are different from the variability used for the model and a t-test is not appropriate. The regression plot of predicted vs observed is the most useful comparison.

Cesium was used as a model calibration tracer for settling solids and resuspension velocities in the Lower Hudson River Model (Thomann; Chapter 7). No calibrations were identified for the upper River models.

Food chain models (p. 7-5): Zooplankton growth and respiration rates were based upon published data for Gammarus, which is a macroinvertebrate.

Component analysis: the data shown in Table 7-2 are not clear. Specifically, loss rates of PCBs exceed total uptake in Segments 3, 4 & 5, which is not possible. This suggests a failing of the model altogether, or some pertinent information have not been included in the text to help me to understand this table.

The Bivariate statistical model for fish body burdens is described as a statistical model with two independent variables, water and sediment PCB concentrations. However, even if water and sediment concentrations are not in equilibrium, they are not independent of one another and hence the bivariate model is not based upon independent variables. This has both important ramifications both statistically and mechanistically for the interpretation of sediment vs water contributions to the fish body burden of PCBs. An important assumption of multiple regression models is that the independent variables are independent (not correlated). If they are correlated, then this affects the regression coefficients because they explain overlapping information. Because both sediment and water concentrations drive concentrations in the food chain, it is perhaps not surprising that the bivariate model was able to roughly estimate the concentration of PCBs in the fish but the unexplained variability is still quite large. It might be expected, for example, that as older, larger fish with more complex diets are examined, the difference between estimated concentration from the partial bioaccumulation factors derived for water and sediment and observed concentrations would increase. And yet, for human consumption purposes, it is desired to predict body burdens in the larger fish with greater confidence. As stated previously, deriving the relative contribution from sediment vs water sources needs a bioenergetically based food chain model. Also note that the relative contribution of prey items in a pelagic predator that are described as water-based (eg yellow perch consumption of small fish; young yellow perch consuming pelagic zooplankton) does not follow that water concentrations are the driving force behind the contaminant burden in the pelagic predator. Gobas (1993) noted that sediments are the main source for contaminants for pelagic salmonids in Lake Ontario because of a benthic link at the base of the food web. We also have suggested that the sediment-link is important for lake trout PCB burdens after examining how food web structure influences lake trout PCBs (Bentzen et al. 1996). Specifically, presence of the sediment-pelagic invertebrate, *Mysis relicta*, results in substantially elevated PCBs in lake trout, whether or not they directly consume mysids. This is an important and interesting aspect of that temporal changes in water concentrations might be quite rapid once the "tap is turned off" but is followed by relatively slower changes in sediment concentrations. This is another point which should be addressed for the river, to predict what the temporally changing relationship between sediment and water concentrations will be (fugacity-based models would be very useful here).

More general comments (not in any particular order of importance):
-Figure 3-9: details of these maps are not identified (e.g. what is floodplain vs the TIP channel?)
-presentation of Figures 4-63 and 4-64 are somewhat confusing.
-it is not easy to differentiate the data shown in Fig. 4-10, Segment 12.
-units of both ft/day and cm/yr given for solids settling & sedimentation velocities... use standard units!
-the description of the data in Tables 9-9 and 9-10 are obtuse. For example, the coefficients are the partial BAF's and the r^2 is from the predicted vs observed fit. This information should be identified on the tables.
-Figures 9-8 to 9-13: the 1:1 line should be shown, and include the r^2 and the probabilities, on the figure. Some of the predicted/observed fits show a lot of scatter. A relatively high r^2 can still be insignficant.
-the fraction of organic carbon in the suspended solids (phytoplankton, zooplankton) is probably relatively constant; were data not available from Thomann's modeling in the lower river, or use the study of Cole et al. which produced an average estimate of 40%? (note this value was used in the other Upper Hudson River model).
-the statement on p. 8-17 (bottom), that for the lighter chlorinated congeners, bioaccumulation is driven primarily by direct uptake from dissolved phase, but food consumption is more important for the higher chlorinated congeners. Note that other studies have documented this observation.
-note that forage fish is not synonymous with planktivorous fish. Forage fish refers to prey fish for the piscivores.
-many layers to the sediment based upon GE sediment core data, but not used?
-Calibration of the Upper Hudson River PCB Model: chapter 4, fig. 4-1: what do the open and closed circles and diamonds represent in the TSS and PCB panels? These should be identified on the figure. It might be useful to see either an annual average (or warm seasonal average) plotted against year. What are the set of data at 5 ng/L when values were in excess of 1000 ng/L in the early years? Why are there many data equal to 100 ng/L? Any comments about the differences in estimated water concentrations by the different surveys?
-Huestis et al. (1996) compared historical data for PCBs in Lake Ontario lake trout to reanalyzed values on archived fish and observed a reasonable agreement between the annually estimated historical values and the values based upon newer protocal (congener-based, etc). Did any agency archive samples that could be reanalyzed to validate the conversions used to standardize the Phase 2 data with the NYSDEC data? Note that other aspects of analytical protocol, including but not inclusive, fish size, fat content, solvents, etc, also will influence temporal data. Population characteristics, including population abundance, influence reproductive strategies and

success; population characteristics are influenced by fishing and other predation pressures and competition for food resources.

RECOMMENDATIONS

Overall, I recommend that modelling effort for the Hudson River PCB Reassessment RI/FS is acceptable with major to minor revisions. This latter is somewhat widely interpreted because my recommendation is based upon the PCMR (the models described in which are still preliminary but essentially sound as such) and the promise of what is currently underway (Revised Scope for Baseline Modelling).

To facilitate the review process:

- provide the data for evaluation alongside the models
- make a summary list of available data (dates, locations, compartments analyses, analysis methods, etc)
- allow greater lead time for the review process; it is rather curious that we were given one month for a report that has been available for 2 years. All of us have multiple commitments and scheduling time is not easy without adequate notice for such a major effort.
- there is a substantial amount of detail in this report; I could have used more time in the evaluation. Time was partly "lost" because of the difficult format of the material. It would be more efficient to have all the material for each model as a complete package (as scientific work normally is presented), and then a summary chapter identifying how the models will be linked together (a flow-chart or diagram may help). The jumbled together format of the PCMR made it more difficult to retain all the necessary information during subsequent sections

For the models:

- a review of the literature seems warranted. While there is limited work on contaminant fate models for rivers, there are nonetheless several publications (e.g. Larsson et al. 1990; Rice and White 1987; Chevreil et al. 1987, work from Green Bay and the Fox River, WI). There is also a wealth of information based around the Great Lakes, with several other time-dependent models, sediment/water PCB exchanges, role of atmospheric deposition in mass balance, etc, etc.
- for each model/section of work, identify a list of specific questions to be addressed, assumptions made, available data, wish-list for data or information, concerns about the model formulation, calibration, etc. Having this neatly organized would more readily generate specific contributions from reviewers and other collaborators; workshops and seminars are also useful.

Miriam Diamond

Miriam Diamond

Miriam Diamond holds a B.Sc. in Biology from University of Toronto, an M.Sc. in Zoology from University of Alberta, an M.Sc.Eng. in Mining Engineering from Queen's University, and a Ph.D. in Chemical Engineering and Applied Chemistry from University of Toronto. She has worked as a limnologist with the Ontario Geological Survey and a private consultant with several governmental agencies. She has over 15 years of research experience in the areas of water quality and chemical fate and transport.

Miriam Diamond is currently employed by the University of Toronto as an Associate Professor in the Department of Geography with cross-appointment to Department of Chemical Engineering and Applied Chemistry. Her main research focus is the fate and transport of organic and inorganic contaminants in aquatic systems, and more recently urban areas. The research methods used vary from mathematical modelling to laboratory and field investigations from the Arctic to the southern United States. A second research area is Life Cycle Assessment and its application to site remediation options. She has sat on the Technical and Publications Committees of the Association for Great Lakes Research, is a board member of the Canadian Environmental Law Association, member of the review committees of the Niagara River Remedial Action Plan and Lake Michigan Mass Balance program, vice-chair of the Canadian Standards Association (CSA) Committee on the Life Cycle Impact Assessment of the Pulp and Paper Production Phase and member of the Canadian Raw Materials Database Technical Committee (also CSA). Along with her graduate students, Dr. Diamond has published over 30 papers and reports in the scientific and technical literature.

August 21, 1998

PEER REVIEW OF HUDSON RIVER PCBS
RI/FS PHASE 2 REPORTS
PRELIMINARY MODEL CALIBRATION REPORT

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GENERAL COMMENTS

This review concerns three components of the U.S. EPA's effort to address elevated PCB concentrations in the Hudson River: Preliminary Model Calibration Report (PMCR), the Revised Scope of Work for Baseline Modeling Report, and the Responses to Selected Comments to the PMCR. According to the "Charge to Reviewers", my comments are directed towards the main question of whether the approach being taken will yield scientifically credible conclusions upon which to base management decisions. Generally, the approach being taken appears to be sound with several caveats and qualifiers discussed below.

First, I would like to couch my comments within the context of the review process. It is difficult to obtain a clear understanding and overview of the models, assumptions and calibration results from the reports. As several of the "Selected Comments" pointed out, the PMCR is a difficult document to digest. This is particularly true in the absence of data reports and a clear summary that places all work in the overall scope of the Hudson River effort. I suggest that a more productive review process should incorporate oral presentations of the work to be reviewed along with interactions among reviewers, modellers and regulatory officials, from which we would develop our commentary. Receiving all materials well in advance of the review would be beneficial.

Specifically, the following would aid in understanding the process and approach:

- a graphical presentation of the modeling components;
- timelines for each component;
- a matrix of the consultants involved along with their responsibilities and indications of the cross-overs between models (e.g., model outputs that become inputs of the subsequent model); and
- a table listing the data available, time and conditions under which data were collected, agency involved and annotations (e.g., QC/AC of chemical analyses, supporting data for PCB fish concentrations such as size class and sexing).

Other specific criticisms of the documentation are:

- too much reliance on the gray literature rather than concisely written and peer reviewed scientific literature;
- many unsubstantiated comments as pointed out in the "Selected Comments";

- too much data with too little interpretation and clear summary of the main conclusions;
- poor graphical presentation of the results;
- poor representation of the geographic area and model segmentation (recommend a comprehensive single map indicating the location of GE plants, tributaries, segment boundaries, relative water and TSS loads from tributaries, and a second map indicating sediment PCB concentrations); and
- minimal comparison with other systems, e.g., what do other systems tell us about the mobility and bioavailability of pollution?

A. Is EPA using appropriate models, datasets and assumptions.

In terms of fate and transport, to which stage is this question addressed? The Workplan documents states that the Lower HudsonRiver model is undergoing refinement and the HUDTOX model will be applied to a new segmentation scheme. From these expanded plans, the EPA appears to be using appropriate models with sufficient temporal and spatial resolution to address the question of remedies. The models and their resolution also appears appropriate to the system and data.

B. Will the models, with the associated datasets and assumptions, be able to answer the following principal questions as stated in the PCMR:

1. When will PCB levels in the fish population recover to levels meeting human health and ecological risk criteria under No Action?

This question can only be answered using a fully time dependent bioaccumulation model that includes fish age classes, a decoupled bioenergetic treatment of uptake and depuration, and an accurate food preference matrix that allows discrimination of benthic versus pelagic routes of chemical uptake. The statistical and empirical biotic models will not supply the necessary time dependent information.

In order for a bioenergetically-based bioaccumulation model to answer this question, it must rely on sufficiently spatially and temporally resolved input data, namely PCB water and sediment concentrations. The spatial resolution must capture migratory and seasonal fish distribution patterns (have not seen a discussion of this). The input must also capture temporal patterns such that accurate water concentrations coincide with main life events such as spawning, times of high activity, etc. Sufficient information is necessary to track changes in dietary consumption patterns that may also require spatially and temporally resolved data.

2. Can remedies other than No Action significantly shorten the time required to achieve acceptable risk levels?

The first step towards answering this question lies in quantifying the source contributions to

PCBs in the river, specifically the "GE" versus "non-GE" PCBs. The contribution of "non-GE" PCBs is not clearly discernable from the report. For example, p.2-4, #4. states that the "principal external loadings" were 74% across the upstream boundary at Fort Edward (this presumably includes the GE contribution?) and 18% from the Mohawk River (is there a GE contribution to this 18%?). Moreover, of the GE contribution, how much is currently seeping into the river, or are most PCBs found in contaminated sediments (i.e., what is the specific source upstream of Fort Edward?). One can not answer management questions without a sound understanding of the source terms since management actions differ widely depending on the nature of inputs.

Secondly, it is difficult to answer this question without specific information on the options under consideration. The next step is to evaluate whether the effect(s) of these options can be simulated within the models. For example, if dredging the sediments of the TIP is contemplated, then the model must simulate the disruptive effect of the dredging process itself followed by the sediment reconsolidation. This simulation would require considerable thought to reconstruct, but may be possible within the modeling framework.

3. Are there contaminated sediments now buried and effectively sequestered from the food chain which are likely to become "reactivated" following a major flood, resulting in an increase in contamination of the fish population?

This question can be restated more explicitly as: Can PCBs sorbed to buried sediment or in the pore water at depth re-enter the water column at sufficient levels (mass) to affect downstream concentrations of bioavailable PCBs?

There are two parts to the questions. The first refers to the physical process of sediment bed resuspension and disturbance of the sediment profile. The TIP scour model aims to address the question of sediment resuspension, with modifications to include cohesive and uncohesive sediments. Insufficient details are available in the PMCR to determine the efficacy of the model. I question the legitimacy of a steady-state assumption since resuspension is event driven and dependent on antecedent conditions (e.g., consolidation time). Will sufficient empirical work be available to adequately parameterize and validate the model since resuspension models are semi-empirical and rely heavily on empirically derived coefficients? What is the fate of the resuspended material? This question was raised in the "Selected Comments" but not adequately answered. What is the fate of the PCBs in pore water exposed due to sediment scouring?

The second part of the question concerns the bioavailability of the resuspended material. The fate and transport model assumes equilibrium conditions to describe the truly dissolved, DOC-bound and particle-bound fractions of PCBs. The literature contains several studies that question this assumption, supported by findings of slow and biphase desorption kinetics. This returns us to the question of the fate of the scoured PCBs. Will the resuspended PCBs have sufficient time in the water column to re-equilibrate, including desorption to a bioavailable form? Will the resuspended material scavenge dissolved PCBs from the water column? The work of DePinto and Clarkson may be informative here. Similar questions can be posed for the sediment bed - will recently exposed PCBs as a result of a major storm event be more bioavailable than the previously weathered and armoured bed? How can the model be used to address these questions?

C. Specific questions:

1. Are the modelling approaches suitable for developing quantitative relationships between external forcing functions and PCB concentrations in the water column, sediments and fish? Are the models adequate for discriminating between water- and sediment-related sources of PCBs?

Again, there are several components to this question. The first concerns the interface among models, as the question implies. The reports contain minimal information on how the models will be interfaced. What criteria will be used to determine if the model results, when coupled (e.g., fate and transport, intermittent sediment scour, bioaccumulation) are sensible? How do errors and uncertainty propagate from model-to-model? For example, the uncertainties in model estimates from the fate and transport model differ according to degree of chlorination since the movement of lower chlorinated congeners is dominated by different pathways (e.g., diffusion, air-water exchange) than the higher chlorinated congeners (e.g., sediment deposition and resuspension). With respect to fish, we are concerned primarily with the higher chlorinated compounds.

A second concern is discriminating between water and sediment sources of PCBs. This can be extended to the discrimination between "non-GE" versus "GE" PCBs (i.e., the source of PCBs upstream of the northern river boundary is unclear to me and contributes significantly to the overall budget). Is a method being formulated to make this distinction within the fate and transport model? See Diamond (1995 *Environ. Sci. Technol.* 28:29-42) for a discussion of a modelling method that can be used). It may be desirable to employ a multivariate statistical technique such as factor analysis or discriminate functions analysis, to tease out the congener patterns according to medium. This method has been used to determine sources of dioxins, PAHs, etc. This analysis should include the congener pattern of gas and particle phase PCBs within the atmosphere.

The final part of the question relates to the model used to estimate fish concentrations. Again, a mechanistic, bioenergetically-based model is necessary to discriminate between water and sediment chemical sources.

2. Are the spatial and temporal scales of the modeling approaches adequate to answer the principal study questions? Additional data?

Since the time scale for the mechanistic bioaccumulation model is unclear, it is difficult to answer this question. The temporal linkage between the fate and transport and mechanistic bioaccumulation model is important given the seasonality in both. For example, can the models address the temporal relationship between concentrations and critical life events? The greatest PCB concentrations and loadings occur during spring freshet. Does this coincide with spawning or a time of important dietary changes? I have discussed other aspects of this question under B1.

There is concern that the water and sediment sampling regimes do not coincide with the model segmentation (question of spatial resolution). Temporally, there is some concern about the data collection adequately capturing storm events (which are difficult to capture) versus "average" events. The importance of storm events, or at least high flow conditions, is apparent from the results presented in the PMCR. In terms of sediment resuspension and bulk PCB movement, adequately characterizing storm events is clearly critical.

Some additional data should be collected from the unmonitored tributaries to verify that their assumed hydraulic, TSS and PCB contributions are accurately estimated.

Overall, the degree to which the models can be validated raises some concerns. I suggest that the models be calibrated and tested with chemicals in addition to PCBs to increase the rigor of the testing procedure. I have elaborated on this below.

Plans to run HUDTOX for decadal-scale periods and testing hindcasts from 1977-1997 are certainly necessary. The success of this effort rests, in part, on "normalizing" PCB measurements relative to the analytical methods used. This has been addressed in other parts of the study.

3. Multiple versus single models to estimate fish concentrations?

As discussed above, a mechanistic bioaccumulation model based on bioenergetic considerations is needed to obtain answers to the temporal questions being posed. Time response information can not be obtained reliably using empirical or statistical models. However, it is reasonable to use several models to estimate steady-state conditions and test the mechanistic model, with the caveat that the agreement among these approaches does not validate the time response information produced by the bioaccumulation model. Overall, using multiple approaches to examine fish concentrations lends credibility to the modelling process.

As I suggested for the fate and transport model, a more rigorous test of the models would come from applying the models to other chemicals that differ in their main exposure route (e.g., water versus food, pegalic versus benthic food chains) and time response.

Miriam Diamond

4. Level of process resolution?

The fate and transport model could benefit from either the inclusion or improved parameterization of several processes:

- Air-water exchange rate: more accurate determination of congener abundances in gas phase rather than assuming (erroneously) that the gas phase congener pattern is the same as that in the water (p 4-11);
- Unclear how the air-water exchange mass transfer coefficient is obtained and its dependence on temperature and air and water velocity;
- Sediment-water diffusion: whereas the treatment of diffusivities is explained, the stagnant boundary layer estimate is not apparent but can potentially alter rates of sediment-water diffusion by orders-of-magnitude, i.e., what is the stagnant boundary layer thickness, how was it obtained? This is important given the uncertainty in rates of diffusion from the contaminated sediments of TIP and the need to invoke an explanation of groundwater discharge to reconcile observed concentrations.
- Sediment-reworking: how is vertical reworking due to mechanical perturbation treated? Presumably sediment scour can result not only in sediment entrainment, but in mixing sediments vertically and horizontally. Evidence for this would come from disturbed core profiles for which dating using, for example, Pb-210 or Cs-137, is not possible. The effect of sediment reworking would be enabling interaction between deep, formerly buried sediment and the water column. We have been examining this hypothesis in a mercury contaminated system where, we suspect, sediment reworking may be maintaining elevated sediment and water concentrations (Diamond et al., in press).
- Application of primary production estimates from the Lower to Upper Hudson River sections. How valid is this? What level of uncertainty is introduced by this assumption?
- Groundwater discharge/recharge effects: groundwater discharge in TIP has been invoked to account for observed elevated concentrations of lower congener PCBs. The treatment of this process was substantiated by some hydrogeological interpretation. Are plans underway to conduct more rigorous testing of this assumption and look for other zones of discharge/recharge along the river? See comments below. How does the incorporation of groundwater discharge affect the water balance for which a surplus appears to exist?
- Watershed export: the question of the proportion of PCBs contributed by atmospheric deposition to the watershed followed by watershed runoff has not been addressed. I also did not see a discussion of other potential sources. A simple calculation may suggest that watershed export may contribute negligibly to the mass balance, however it would be useful to explore this source term briefly.
- PCB degradation in sediments: there is an ongoing controversy about the extent of degradation through aerobic and anaerobic microbial processes. Degradation has not been included in the fate and transport model presented in PMCR, but should be

included to improve the comprehensiveness of the model and to determine the relative importance of this process, particularly over the decadal simulations.

5. Will the results from the modeling effort be adequate to support human and ecological risk assessments?

Generally, the results of the extended modelling effort (including results obtained from activities outlined in the "Revised Scope" document), appear to be adequate to support human and ecological risk assessments as presently conducted by the EPA. However, the key lies in the results of model testing and validation, particularly over the long term (e.g., the hindcasting exercise).

In addition to model validation, a critical component of the analysis that I have not yet addressed, is how PCBs are treated, e.g., - Arochlor equivalents, specific congeners or ΣPCBs. Modelling ΣPCBs will not provide sufficiently accurate information on fate and transport nor bioaccumulation for risk assessment purposes. Justification is lacking on the choice of PCB congeners and that those chosen do not extend beyond hexachlorobiphenyl (why this limitation?).

D. Are there significant changes to the work effort outlined in the revised work plan that would improve the outcome?

See recommendations below.

E. Are there serious flaws in the modeling approach that would limit or invalidate model conclusions?

The following flaws should be rectified:

- inclusion of segments upstream of GE contributions;
- improved delineation of groundwater discharge and recharge zones;
- justification of choice of PCB congeners for analysis, choice of their physical-chemical properties, and lack of consideration of congeners beyond hexa's;
- improved treatment of air-water exchange, including ΣPCB and congener air concentrations and velocity-dependent mass transfer coefficients;
- linkage between sediment scour and HUDTOX models to track the fate of resuspended material;
- determine bioavailability of resuspended PCBs by including sorption/desorption kinetics rather than assuming equilibrium conditions for PCB partitioning; and
- address bias in model predictions that lead to overestimations of TSS at low flows and underestimation at high flows (p.4-20 focuses on the results of an overall T-test indicating

goodness of fit between TSS and discharge that belies the bias apparent in Figure 4-13).

Recommendations

Overall, I recommend that the modelling effort for the Hudson River PCB reassesment RI/FS is **Acceptable with minor revisions**. This statement is based on the information provided, which, I suggest, is incomplete, dated (PMCR) and difficult to follow.

In addition to recommendations stated above, I offer the following additional recommendations that are intended to strengthen the approach taken:

- re-design the spatial boundaries to include a segment upstream of the GE plants in order to provide a "control" segment that is useful for understanding the contribution of "non-GE" PCBs to the river (albeit at the upstream end);
- consider estimating the contribution from atmospheric deposition to the watershed, input through watershed export;
- place greater emphasis on determining source contributions (e.g., "GE" versus "non-GE PCBs, those in contaminated sediments versus those entering from land-based sources) (see comments above);
- justify the choice of PCBs modelled, e.g., particular congeners, how ΣPCBs are treated;
- document information on the physical-chemical properties of ΣPCBs and congeners modelled, including temperature corrections for vapour pressure and Henry's law constants;
- place much greater emphasis on model testing and validation,
    - develop a method and decision process to judge the performance of each model and the results obtained from models linked in sequence;
    - use of chemicals in addition to PCBs that span a range of physical-chemical properties and hence pathways;
    - use of chemicals that have a strong time response for hindcasting (e.g., lead); and
    - use of chemicals with a long time record, thereby circumventing difficulties in using historical and recent PCB analyses;
- use differences in data sets (e.g., USGS versus GE differences in the relationship between PCB concentrations and TSS) and the occurrence of outliers to probe the behaviour of the system rather than discounting differences (discrepancies can tell us more about systems and better test hypothesis than data that conform to predictions);

- more sophisticated treatment of non-detects in the PCB data sets rather than setting non-detects to zero or one half the detection limit (e.g., use of statistical methods such as least squares estimators developed by El-Sharaawi);

- test importance of assumption of constant DOC (does this imply temporal as well as spatial consistency?); and

- test the hypothesis of groundwater discharge as the mechanism responsible for contributing "additional" PCB loads, e.g., piezometer studies (deployed in sediments, piezometer nests in bank sediments to determine flow path, use of benthic chambers, measurements of stable isotopes (O-18) to "age" the water).

James Gillett

James W. Gillett

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James W. Gillett is a Professor of Ecotoxicology and director of the Cornell Superfund Basic Research & Education Program (SBREP) at Cornell University. In addition to teaching courses in environmental toxicology, ecotoxicology, ecological risk assessment, and natural resource management, he coordinates research on bioavailability in the SBREP, works on multi-pathway exposure assessment, and is evaluating the impact of public participation on ecological resources in site clean-ups. He has served on the Biotechnology Science Advisory Committee (U.S. EPA), the NRC-NAS Committee on Prioritization of Superfund Site Clean-Ups, the New York State Ecological Risk Reduction Task Force, and numerous other advisory panels for U.S. EPA and other federal and state (Oregon and New York) agencies, non-governmental organizations, and industrial associations.

Comments on "Preliminary Model Calibration Report" and Hudson River Phase 2 Reports

James W. Gillett
Cornell University

A. Is EPA using appropriate models, datasets, and assumptions on which to base a scientifically credible decision?

The existence of the Hudson River Superfund site as a continuous entity 175 mi long on which literally billions have been spent on a whole range of pertinent research and management issues creates a mind-boggling challenge encompassing this lead question. The short answer is, "Yes, but...." , and then comes the long answer. The most useful databases (consistency of purpose, acquisition and handling, quantitation) are reasonably robust, yet still have huge gaps in temporal and spatial resolution. What is the largest quantity of as-yet undetected PCB contamination which might be mobilized to make GE, EPA and us look like fools? How do the various components (water column, DOC, interstitial water, consolidated sediment, and non-consolidated sediment) relate to each other and measured values in terms of bioavailable, slowly available, and non-available (fully sequestered) loads? The absence of terrestrial data on biota and soils within the watershed assumes the outputs to and inputs from these sinks (with their own external links and forcing functions) are not relevant or can be lumped at much coarser levels of resolution. That the models discussed all assume these links to be negligible very much limits long-term and long-range utility of the approach.

The models seem basically sound, but the use of four contractors with numerous technical tasks illustrates the breadth of knowledge required to make any global statement about suitability. Use of a combination of modeling approaches appears wise, but hardly fool-proof and ultimately unsatisfactory in determining the likely outcome of management actions. Under EPA's new "Proposed Guidance for Ecological Risk Assessment" [EPA/630/R-95/002B, Risk Assessment Forum, U.S. Environmental Protection Agency, Washington, DC; see also Fed. Register 61:475552+ (Sept. 9, 1996)], eco risk is evaluated by simultaneously considering exposure and effects and their interactions. In a recent review of EPA's new Multi-Pathway Exposure Analysis Methodology, it was clear that similar approaches will be incorporated into human health-based assessments as well. The subject assessment does use part of the multi-pathway exposure assessment (a good part of which was developed by various contractors and researchers in the Hudson River system), but falls short of embracing a more holistic view and methodology.

Finally, however suitable the models, etc., might be in regard to scientific credibility of a decision (useful in a court of law or administrative action), that may have come so late in the process as to be functionally useless for regulators, the regulated community, and various public interests inter alii. The use of the models by scientists and engineers is fine; the other publics will probably not understand the complexities and uncertainties.

B. Will the models, with the associated datasets and assumptions, be able to answer the following principal study questions as stated in the PMCR:

1. When will PCB levels in the fish population recover to levels meeting human health and ecological risk criteria under No Action?

Increasingly, this judgment relies upon congener-specific exposure assessment and toxic equivalent functional analysis. For highly mobile populations of people and mink which may or may not feed on the fish represented by Hudson River sampling schemes and models derived therefrom, it would seem to be prudent to pay attention to congener-specific transformations as a part of chemodynamics and bioaccumulation. We have almost no knowledge of congener-specific dose-response relationships in people, but we are making some inroads with mink. Nevertheless, at the rate of gain of knowledge about the multi-faceted parameters of such interactions, we may best be served by looking to when mink can be self-sustaining in the watershed. Then we might assume that the situation is safe for people as well. When, if ever, can the models tell us this? First, the models need to be at least sensitive to congener structure on a basis other than log Kow or molecular weight/chlorine number. This includes all chemodynamic parameters (metabolism, uptake, storage and excretion) in each trophic level and target. The Thomann et al. (1989) model, central to many efforts, uses only homologue categorization, without regard to chlorine atom placement and all the implications for effects on metabolism and toxicity. Alternatively, more complete and persuasive evidence of homology of these parameters under differing biotic and abiotic situations would be helpful. Second, they need to be more holistic in the species addressed (inc. terrestrial), incorporating biology (e.g., feeding range, preferences vs availability) and seasonality (winter depuration). Third, they need to be evaluated longitudinally with fish of known age and residency, with particular attention to the nature and quantities passed on in the roe. Long-lived sturgeons and species such as black and rock bass, for which there are already some data, might be helpful.

The difficulty right now is that the models present a sort of minimal upper bound of potential exposure which is far greater than might be realized by a nominal subject (of whatever species), much less than might be happening for the upper 99th percentile, and without a clear basis for judging the merit of a given management decision, e.g., No Action, removal, capping, etc.

2. Can remedies other than No Action significantly shorten the time required to achieve acceptable risk levels?

The reason that a demonstration of longitudinal validity in the modeling is so critical is that assumptions about the long-term outcome of management actions remain untested. (The No Action alternative, of course, is tested by long experience, almost three decades.) The peak loading of some food web components will occur substantially after the primary known source(s) may be removed and pathways eliminated, either because unknown and unmitigated sources are exposed by natural processes (e.g., flooding), because of external inputs (via atmosphere and soil), or because of the lags in equilibrium posed by chemicals such as PCBs and DDT-R. We still do not know if this long time scale problem is attributable to slow release consequent to sequestration (short-term non-bioavailable, but chemically detectable residues), cumulative impacts, or other mechanisms to be discovered. As it stands right now our knowledge and assumptions are either in error or incomplete, or both, to a significant degree with respect to long term consequences.

This set of issues becomes critical when amassed at the scale of the Hudson River. What might work in theory at a smaller site may be fairly dubious at this grander scale. The first dredging of Foundry Cove increased measurable Cd therein by about 25%! Rochester Gas & Electric hit the mother load' of coal tars at their Genesee River site coincidental to other actions, necessitating a massive restructuring of not just that effort, but of numerous MCG sites in New York State. If one multiplies the potential for adverse outcomes of site remediation activities due to other pollutants in the watershed being disturbed and/or redistributed consequent to dredging, for example, then this question demands more wisdom than I believe we have.

An alternative to dredging could be capping or in-situ immobilization, or accelerated efforts at in-situ bioremediation (sort of augmenting part of the No Action alternative). My understanding is that these all have or are being applied in the Hudson Basin or elsewhere, but I have seen no estimates of when or whether we "significantly shorten[ed] the time required to achieve acceptable risk levels." There have been more numerous instances of reported fraud and failure that success in this matter. In the St. Lawrence, the Canadian stretches of the river and the numerous downstream embayments have yet to be addressed, in no small part be cause of the issues of scale, but also because of inadequate assessment.

3. Are there contaminated sediments now buried and effectively sequestered from the food chain which will likely become "reactivated" following a major flood, resulting in an

increase in contamination of the fish population?

The kind judgment would be that we just don't know, but in point of fact we are virtually clueless. There is no reason for there not to be, because, as noted earlier, we need to define how large a load that might possibly be. Could a PCB-based (or heavily contaminated) DNAPL be snaking its way through the sediments between sediment cores? Are water column and sediment sampling methods robust enough to tell us? There is an additional problem here. The mechanisms and models seem to be in place to ascertain where flood-mobilized sediments will be deposited, but no effort in the examined documents really spelled it out in a manner that would ultimately permit use in an exposure assessment model leading to people or a critical ecological resource. The phrase, "increased contamination of the fish population," is the generic type of statement used (incorrectly) to justify regulation of land-applied wastewater sludges, assuming that erosion of deposited material makes contaminants available for bioaccumulation. If we cannot attribute contamination in the long term to known sources, how will we know that an as-yet undiscovered is affecting anything?

Please note that my use of the terms sequestered and sequestration may differ from that of the authors of the PMCR. I refer you to Alexander [Alexander, M. (1995). A small circle of knowledge, a large circle of ignorance. Environ. Health Perspect. 103(Suppl. 5):121-123.], wherein materials are sorbed or intercalated into mineral or organic matter in pores too small for microbial action and thereby limiting back-diffusion (formerly called soil hysteresis). Some samples of experimentally DDT-treated soils have sequestered non-bioavailable compounds for several decades. What fraction will be released with acid rain? photoxidation of a carbon matrix? accidental exposure to another solvent of greater polarity and smaller size? Will releases be bioactive (i.e., present in an effective dose to a vulnerable and valuable receptor)? Almost all of the data in the PMCR et al. concerns chemically detectable residues; sequestered material is not determined, say, by selective extraction or in-situ bioassay.

C. Specific Questions:

1. Are the modeling approaches suitable for developing quantitative relationships between external forcing functions (e.g., hydraulic flows, solids and PCB loads, sediment initial conditions, etc.) and PCB concentrations in the water column, sediments and fish?

Much of that is beyond my expertise. I had trouble with a number of the equations just in identifying the components and what terms meant (e.g., the term m in Eq. 3-9,3-10; the third and fourth terms in Eq. 3-11). [Why is there a term for "net sedimentation" on top of terms for "settling" and "resuspension" in Eq. 3-11?. It kind of reminded me of my Russian language exam at Berkeley, which was presented to me by the late Prof. Zev Hassid in Old Church Cyrillic on a subject matter out of step with our understanding of chemistry. It really is hard to translate what you can't believe.] However, I am fairly well versed in the modeling of relationships between water, sediments and biota. The Thomann-type model is archetypical; many of us use it or suitable modifications. Theresulting variations may be improvements, but typically only a little new information is brought to bear. Ram and Gillett (1992, 1993) [Ram, R.N. and J.W. Gillett (1992). An aquatic/terrestrial foodweb model for polychlorinated biphenyls (PCBs). IN: J. Hughes, W. Landis and M. Lewis (eds) Environmental Toxicology and Risk Assessment. First ASTM Symposium on Ecological Risk Assessment, Atlantic City, NJ. American Society for Testing and Materials, Philadelphia, PA. pp. 192-212; Ram, R.N. and J.W. Gillett (1993). Comparison of alternative models to predict the uptake of chlorinated hydrocarbons by oligochaetes. Ecotoxicol. Environ. Safety 26:166-180.] estimated numerous parameters by analogy, bioenergetics, and simulation theory. Interestingly, the natural history database upon which so much of the St. Lawrence-Great Lakes effort (Ram 1990) was based is horribly out-of-date, since professional journals and graduate programs stopped accepting that sort of descriptive science as "state of the art." The Hudson has fared a little better, due to the efforts of the Hudson River Foundation, beneficiaries of the court award derived from power plant releases of PCBs into the subject body of water.

The most important assumptions seem to be that PCB bioaccumulation has no significant effect on mortality, morbidity, behavior or fecundity of fish or their prey. These outcomes may account for the high degree of variability of fish residues (e.g., lognormal distribution). Possible changes in behavior (or lack thereof, such as attracting or repelling predators, perhaps by organoleptic processes) would seem to be critical, but are undocumented. By the same token, underlying biochemical and physiologic shifts in lipid metabolism and storage appear to have been studied a bit more thoroughly, albeit still inadequately.

Are the models adequate for distinguishing between water-related and sediment-related sources of PCBs?

We believe they are (Ram and Gillett 1993), if one pays careful attention to foodweb components derived from one or the other. That is, one must distinguish between sediment and water sources both for O2 and PCB-contaminated food. Unfortunately, we were unable to do this on a congener-specific basis because of a shortage of uptake and metabolism rates for specific congeners. Much data has been generated in the interim and a much better job may be possible now.

2. Are the spatial and temporal scales of the modeling approaches adequate to answer the principal study questions?

Sadly, no. The spatial scales in the figures laid out over the entire course of the river are simply too coarse to protect us from "hidden" pockets and untoward shifts. There may be 20 yrs of data, but the data are uneven, poorly correlated (to water, sediment, biota, organic carbon) and not as well conceived as we have had in more recent times (i.e., since TSCA and CERCLA, regulating PCBs). Someone is always having to make a correction that appears to the public or outsiders as a "fudge factor". All the uncertainties pile up as ignorance and incompetence (Johnson & Slovak 1995). [Johnson, B.B. and P. Slovic (1995). Presenting uncertainty in health risk assessment: Initial studies of its effects on risk perception and trust. Risk Analysis 15:485-494] How many factors modify the value at a given point in time and space? What were they for each sample? Do we just pretend that they don't matter? One approach is to do as was done in parts of the PMCR: get all the models you can to describe the proverbial elephant. Even though the assumptions may be incompletely examined and tenuous, you can see some of the convergent patterns. Thus far, however, even that approach does not inspire much trust. We are asked to wait for the hindcast, the next round of recalibrated samples, the new sampling paradigm, and so forth. If an educated professional is confused, I would suspect the public to be very much put off by this difficulty.

If not, what levels of spatial and temporal resolution are required to answer these questions? What supporting data are required for calibration/validation of these spatial and temporal scales?

The main object would be to sample until the increase in variability or inhomogeneity is negligible (i.e., the derivative -> 0). A statistician then should be able to establish the appropriate power of a sampling network in space and time in which the driving variables of the data distribution could be ascertained. Without access to the original sampling designs and rationales, it is hard to know if this is as good as we can get for the dollars available in relation to the attendant risks. With a high proportion of non-detects the results are invariably contentious. Rather than assume some arbitrary value, why not use the known portion of the data distribution to represent a lognormal distribution of the non-detects? On the other hand there is more risk to a consumer of that one outlier fish than in all the non-detects. Therefore, our regulatory goal would be to have no fish (prob. <.01) presenting more risk than a population of fish 50% of which are non-detects. What power does the fish monitoring have to have to detect the likelihood of the upper 99th percentile?

3. It is contemplated that PCB concentrations in fish will be estimated using several modeling approaches: an empirical probabilistic model derived from Hudson River data, a steady state model that takes into account mechanisms of bioaccumulation body burdens, and a time-varying mechanistic model not included in the PMCR). A bi-variate statistical model may also be used to provide insight into bioaccumulation. This multi- model approach is being contemplated because of the uncertainties associated with any individual model. Is this a reasonable model approach or should predictions be made using a single "best" model?

As noted above, this is the wisest course, albeit not necessarily that which may find "truth". The integration of the models is tricky, since they are each incomplete and in contradiction over some details. The empirical probabilistic model can suggest overall or summary process rate constants, but probably glosses over species- and congener-specific parameters. The deterministic/mechanistic model needs to identify the range of possible parameter values and either generate a set of probabilistic outcomes for individuals over those ranges or otherwise represent the spectrum of responses (especially that 99th percentile). The realism of the models is not the same as the realism of the sample data, and likely neither is "truth." Adam Finkel (1990) termed this "model uncertainty," in that we don't know which model should be expected to not only represent the data but also to predict a range of outcomes from which the risk manager must make a decision. The risk manager has to decide in advance what questions a model may address effectively and efficiently. Therefore, if all models in hand are false, the search for a true model is yet worthwhile. Ram & Gillett's model (Ram & Gillett 1992) is true if you can surmise the history of individual organisms sampled for residues, but we realized it was false for migrants, casual visitors, and those with different lifestyles than whatever norm was expected of a species or population. The criteria we used were established in advance: *Criterion 1: 90% of the measured values for each species would be within the 95% confidence interval of the log-normal distribution predicted, and *Criterion 2: 90% of the species with three or more specimens would meet Criterion 1. [Valid for 41 of 46 spp. or species clusters derived from prey use.] These criteria held up for PCBs in the St. Lawrence Basin, eastern and western Lake Ontario deepwater food webs, and each of the other Great Lakes, but did not account for growth dilution and toxicodynamics in very large salmonids subjected to a variety of adverse conditions (alewife die-off, hypo-biotinosis in lake trout, invading invertebrates, etc.).

If a combination of models can lead to the construction of a "best" model for its pre-determined criteria, then working toward that goal is certainly the right course. I just don't want the present models singled out for "Eureka! We've got it!"

4. Is the level of process resolution adequate to answer the principal study questions? If not, what processes and what levels of resolution are required to answer these questions? What supporting data (such as data to support specifications of a mixed depth layer, solids and scour dynamics, groundwater flow, etc.) are required for these processes and levels of resolution?

One is tempted to believe that the seemingly robust datasets from which the PMCR is proceeding or will have demand the highest level of process resolution. The biologists, I sure, would love to have the physical environment "tamed" by high resolution equations, well-described parameters, and coordinated datasets. The engineers and physical scientists would appreciate any improvements in the mathematical organization of the biosphere. Except where remote sensing provides a continuum of data over large areas, I'm not well impressed with our ability to get ground truth by sampling along 250-m transects and such. Data resulting from a monitoring survey -- "Are PCBs moving from the Upper Hudson to the Lower Hudson?"-- are inherently different from data based on a sampling grid derived from a high-resolution deposition/scour model implying where PCB-bearing particulates might be trapped. Maybe, as was suggested, intensive sampling over time using the high resolution model would answers the movement question in a more definitive manner.

The sediment deposition and depth of scour questions may be the only ones where the highest level of resolution obtainable seems required. That would also add mechanical turbation (prop wash of large vessels, episodic recreational boating uses) and bioturbation to the processes described. There is a general tendency for CHCs to be degraded in inverse proportion to Cl content or the log Kow and in proportion to water solubility and frequency of sample presence. Three- to five-fold variation in metabolic rate of a congener in or between species seems common, however. Slight shift in prey selection, residency, and level of feeding might cause as large a change, although sensitivity analyses of individual factors do not. In any case the assumption that conditions continue from one season to the next and from year to year is belied by the physical data. When you do a full physiologically based, pharmacokinetic (PBPK) model, it is easy to represent all of the compartments in considerable detail. Even though the PBPK model is regularly employed now in drug and carcinogen assessment, quite a bit of utility is derived from far simpler expressions (e.g., one- or two-compartment models).

5. The results of the modeling effort will be used, in part, to support human and ecological risk assessments. in your judgment, will the models provide estimates adequate for this purpose?

That's an interesting question. The models certainly will assist enormously in planning the assessment design and targets of impact. That will help the risk assessor describe issues for the risk managers in terms which are more readily understood by the public. The absence of plant uptake, terrestrial-aquatic linkages, and other inter-media transfers blunt the aforesaid usefulness, at least for primary ecological risks. For health effects the models seem even less useful or more limited, depending on your point of view.

D. Are there any changes to the work effort outlined in the revised work plan that would significantly improve the outcome?

The work needed on air pathways and plant uptake (both rooted macrophytes and riparian vegetation) is considerable. There are few links to the terrestrial components, so almost all of the effort is directed at the water column relationships. Most of the planned effort is promise as partial solutions to present problems. That is easy to support. I'm not sure where the Gobas model fits in.

E. In terms of evaluating the overall and specific effects and behavior of PCBs in the Hudson River, are there any serious flaws in the modeling approach (theory, structure, physical parameters, etc.) that would limit or invalidate any conclusions or further work based on the results of these models?

For the most part I see no quarrel based on the models and their makers/users per se. The tendency to use mean values as point estimates is not productive. As pointed out above, there are some deficiencies and insufficiencies, but probably nothing radical. The argument that sequestered PCBs are totally immobile denies their pervasive (as well as persistent) character.

Recommendations

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

G. Douglas Haffner

G. Douglas Haffner

Doug Haffner received his B.Sc. In biology/chemistry at Queen's University, Ontario and completed a Ph.D. at the University of London, England in aquatic ecology. He has worked with the Ontario Ministry of the Environment, the International Joint Commission and Environment Canada in the area of water quality management and monitoring. He has also been the President of the International Association for Great Lakes Research, and is currently a senior science advisor to the Indonesian Institute of Science (LIPI). He has chaired the Detroit River Rap and is serving as chair of Contaminated Sediments Subcommittee on the Detroit River

Currently at the University of Windsor, Dr. Haffner is a professor of biological science with a research specialty in ecotoxicology. Dr. Haffner also served as director of the Great Lakes Institute for Environmental Research. His laboratory is the only Canadian university-based facility that is certified under the Canadian Association of Environmental Analytical Laboratories. Research interests are the trophodynamics of PCBs and PAHs. His laboratory has developed models of PCB dynamics in Lake Erie, Lake Ontario and the Detroit River. Other research interests include direct assessment of human health hazards of chemicals in the environment using *in vitro* assays and the effects of chemicals on amphibians.

PEER REVIEW REPORT OF HUDSON RIVER PCB RI/FS PHASE 2 REPORTS PRELIMINARY MODEL CALIBRATION REPORT

G.D. HAFFNER

Introduction: There are two separate modelling efforts decribed in the PMCR. It is understood that at a later date, these models will be integrated in order to address the study objectives of determining system recovery times under a no action scenario, identifying potential remedial actions and the potential effects of flood events. The first set of models quantify transport and fate of PCBs in the Hudson River (Hudtox, TIP & SCOUR), and are relatively well advanced. The second set of models (Bivariate, Probabilistic &Mechanistic) quantify bioaccumulation of PCBs in order to predict chemical concentrations in fish as a means to determine human and environmental health risk, by comparing predictions with consumption guidelines.

General Comments: As is often the case with many environmental studies, the monitoring programs that contributed to the existing PCB data base in the Hudson River were implemented by different agencies and with different study objectives. The compilation of the data base for a comprehensive review of spatial and temporal trends of PCBs in the river leaves many questions of quality assurance, and such questions can effect the ability of the models to make accurate, precise predictions. Other aspects of the report that can be modified to improve its scientific credibility include;

1. Use of a consistent set of units. The switching back and forth from metric causes considerable problems to compare sections and will only delay the final integration modelling results.
2. Do not invent new terminology. The IUPAC numbering system should be adhered to. BMFs are not assimilation efficiencies. I am not certain as to the intended audience, but the use of terms such as consolidated sediment will confuse the lay person, why not simply refer to depositional and non depositional sites which are really the issue being discussed.
3. As noted above, the quality of the data is very suspect (P4-13) where very different methods and instrumentation have been used to generate the data base. There is reference to the use of both validated and unvalidated data (P3-4) yet no information as to what criteria were used to validate data. These criteria must be specifically referenced.
4. Chapter 4 was presented back to front in the text provided.

Transport and Fate Models: The models (Hudtox, TIP SCOUR) are quite acceptable for addressing the specific study objectives. The emphasis on low chlorinated congeners (IUPAC 4, 28, 52,90/101 and 138) tends to be a very biased manner of quantifying PCB transport fate and effects. The underlying assumption of all the models used in the study is that these congeners equally persist in the environment, yet it is well known that any PCB with adjacent, unsubstituted carbons is very susceptible to metabolism. PCB 138 is the only selected congener with a known resistance to metabolism and thus truly persistent in the environment. At times Aroclors are modelled to describe PCB dynamics, yet there is no mention as to how the Aroclors were estimated. For example, some agencies use only congener 138 to estimate concentrations of PCB as Aroclor 1254, thus spurious correlations are readily encountered when comparing dynamics of PCB in aquatic ecosystems. Furthermore, it is difficult to make spatial comparisons when the upper Hudtox model uses different PCB measurement than the lower Hudtox model.

Another assumption of the choice of PCB to model is that the hazard of PCBs in the environment is based on bioaccumulation. A better selection would be to address transport and fate of congeners that persist, bioaccumulate and are toxic (eg non-ortho PCBs). Future work should include a meeting with those responsible for the biological/risk models to identify if these chosen congeners will adequately address the specific study objectives.

On P 4-2, the conclusion is made that it is currently not possible to distinquish between historical sediment load input and recent (ongoing) discharges. This is based on confusing results with PCB #4, that can seriously affect the overall quality of the modelling efforts. In my experience with PCBs, the presented relative distributions of PCB congeners look very questionable. Consider for example that dichloro-PCBs were only 20% of Aroclor 1016 and <1% of Aroclor 1254. If these Aroclors were chosen because they were in commercial use during the time that discharges were made to the environment, then the concentrations of PCB#4 appear to very suspect (even with groundwater seepage into the system). I suggest this is a critical example of where data quality is affecting the power of the models .

TIP appears to be a good predictive tool of velocities and integrates with SCOUR. Predictions of the the models provided in the report are reasonable.

Models &Data: The transport models are very adequate to address study objectives 2 and 3. If the ultimate goal is to quantify risk of PCBs in the river, under different scenarios, then there will need to be stronger rationale given for the various forms of PCB being modelled. The current and recommended biological models (discussed later) stress bioaccumulation, yet many of the forms modelled do not persist sufficiently to bioaccumulate, or are too water soluble to bioaccumulate beyond that expected by simple bioconcentration processes. There appears to be an important inconsistency between the variables modelled and the specific objective of the study regarding human and environmental health.

As mentioned earlier, PCB data are, in my opinion, somewhat questionable. Not only are relative abundances very different from those usually encountered in aquatic systems, but so are the relative concentrations in the various biological compartments. For example;

Sediment	Table 4-8	10,000 -86,000 ug/g (WW?)
Water	Table 9.8	0.3-0.8 ug/L
	Table 9.9	1366-6184 ug/L (incorrect units? as well beyond saturation)
Fish(aroclor)	Table 10.3	1-9 mg/kg

Such distributions are difficult to comprehend given the physical/chemical properties of PCBs and their partitioning in the environment.

Specific Questions:

1. Forcing functions are appropriate, although some assumptions might be further tested with respect to model sensitivity.

a)constant organic carbon in suspended sediment b)settling velocities of 2m/d in a well mixed, shallow riverine system.

2.Spatial/Temporal Trends will not be limited by the use and resolution of the models but by questionable data quality assurance.
3. Not applicable.

4. Relation to risk assessment is limited due to the choice of forms of PCBs modelled. This choice, if continued, must be justified with respect to the study objectives.

Revised Work Plan This appears to be appropriate save for concerns stated above. There needs to be a better balance in the development of the physical models and the biological models. The latter are less well developed, thus opportunities for strategic course correction to address human and environmental risk might be lost.

Additional Questions: None.

BIOACCUMULATION MODELS

Appropriate Models: The emphasis on bioaccumulation might lead to a limited evaluation of the hazard of PCBs in the Hudson River. Both the bivariate and probabilistic models are good descriptive approaches, but lack the robustness of more recently developed food web models. The suggestion in the text of moving towards steady state models should be seriously considered. Models that more realistically deal with processes of benthic/pelagic coupling might be more appropriate for the food web described for the Hudson River.

Models and Data There is no mention as to the use of the TEQ approach to quantifying the hazard of dioxin-like compounds such as PCBs. This might be a function of the limited data base available, but if so, this should be explicit in the report. I am surprised the work of Hong et al. (1992)on TEQs in the estuary of the Hudson River was never mentionned, if even to support the argument that total PCB predictions might be related to TEQ estimates as done for Lake Michigan.
The food web description is less than encouraging. Without relative abundance of predator/prey interactions and trophic levels (e.g. stomach contents, isotope data etc) the food web is very speculative. More effort should be expended in this area.

Special Questions: This aspect of the study has not advanced as well as the transport/fate models, and new approaches are suggested in the text (steady state, fugacity models). Thus it is difficult to respond to the specific questions provided.

Forcing functions: Simple descriptive models assuming equilibrium (BAF, BSAF) are not realistic for a highly contaminated system like the Hudson River. Food web knowledge at this time is very descriptive and can limit the development of more appropriate steady state models.

Spatial/Temporal Scales: Will be appropriate based on successful integration with the physical transport models.
A single best model (most scientifically defensible) would be the best approach, as the assumptions driving the other models (equilibrium vs nonequilibrium; steady state) are quite different.

Ecological/human health risks are being estimated by comparing predicted fish concentrations with consumption guidelines and wild life protection guidelines. This approach can result in potential management decisions to protect one (human) and not the other (ecological). The ecological (if you accept that bioaccumulation is the most important aspect of PCB hazard) will prove to be more stringent, yet most difficult to quantify and possibly enforce. The high degree of contamination of system suggests the need for immediate action, and maybe the human health component might be considered to be given a priority.

Suggested Work The Gobas model would be an improvement, but since then more realistic food web models have been developed. It might be best to host a workshop to review models currently available and select the most appropriate model for the selected approach to quantify risk (bioaccumulation vs TEQ or a combination thereof).

Specific Questions Has EPA specified the information it needs to support a management action plan? The lack of focus on the various forms of PCBs, the lack of a TEQ approach and the lack of direction in the biological models suggests that there needs to be a better specified managment approach. What forms of PCBs are the guidelines based on? Can the other data be adequately transformed to such estimates? Ecological risk is a very longterm goal, yet the project suggests the need for decisions today, should consumption guidelines be the main focus of body burden predictions.

Recommendations: The study should definitely continue, but the biological component might be stressed. There is not sufficient information to recommend a course of action, but a workshop on risk models is recommended as a high priority. The workshop must identify the best model, the data needed for risk assessment and discuss the integration of the biological and phyical models required to fulfill the need of managers to make appropriate decisions.

G. Douglas Haffner

HUDSON RIVER PCB SITE MODELING APPROACH PEER REVIEW

RESPONSES TO SELECTED COMMENTS PMCR

I have reviewed the second document as noted above. Generally there is considerable overlap between issues raised in my first review and the comments made by the various parties such as;

-integration of the upper and lower Hudson River models

-organization awkward, I still think putting key figures in the text, and supplementary figures and tables in an appendix would produce a more cohesive document.

-the issue of pore water being flushed upward relates to the noted inability to separate current and historic sources, which I still think is a data quality problem.

-semantic problems of identifying PCB congeners, areas of deposition/resuspension

-biodegradation must be considered, especially with the PCB #4 data

-settling velocities must be justified, I still think the value used is very high for a shallow, flowing river.

-I am somewhat concerned when asked as a reviewer if the three biological models should be used or one state of the art model, when it is obvious (page 34) a choice has already been made, based on the 'belief' of those developing the models. This 'belief' is based on a weight of evidence approach that assumes the models are equal in their predictive powers for the different questions being asked. I think models should be tested and the most rigorous model used for each specific question. I doubt if equilibrium models will be of much use in a highly contaminated system like the Hudson River.

-inconsistent units, metres vs feet, lipid corrected data, wet wgt data etc. If all this work is ever going to be integrated, there must be standardized reporting mechanisms!

-feeding ecology of forage fish (see Hebert/Haffner CJFAS 1991) is very different within cyprinids.

I now have even greater concern regarding the quality assurance associated with the data base. GE questions the conversion *of their own data set*, and this is poorly responded to by those who made the conversion (no statistical justification given at all). The lack of congener 138 (very persistent and a dominant congener in Aroclor 1254- page 18) is quite an anomalous relative distribution of PCBs. I agree a water concentration of 1086 ng/L is high (near saturation), but think most of the water data are very high with respect to the observed concentrations in fish.

Lastly, I am at least confused with the response on Page 38 that 'from a management perspective only total PCB and Aroclors are of interest'. If this is true, then obviously those designing the study were not aware of this management decision, and much of the work done is not relevant to management decisions on hazard assessment and remediation scenarios. This response has major implications for those doing the biological models to quantify risk. It would appear that a decision has already been made to use guidelines developed in the late 60s/early 70s to quantify risk/hazard of PCBs.

Alan Maki

Alan Maki

Alan Maki holds a Ph.D. in wildlife and fisheries management from Michigan State University. He is currently Environmental Advisor for Exxon Company, U.S.A., and served as Senior Environmental Scientist for Exxon in Alaska from 1986 to 1991. Following the Exxon Valdez oil spill, he was responsible for managing Exxon's wildlife rescue and rehabilitation program and for organizing the company's scientific assessment of ecological damage and recovery. Dr. Maki has authored and co-authored over 160 publications and reports and 6 books on numerous aspects of environmental quality, ecological risk assessment, toxicology and aquatic biology.

Active in a wide range of professional organizations, Dr. Maki is currently a member of the Environmental Protection Agency - Science Advisory Board and has served on numerous advisory panels for EPA Office of Research and Development. He is former President of the Society of Environmental Toxicology and Chemistry, and is serving on a National Academy of Sciences panel concerned with the assessment and management of ecological risks.

Peer Review of Hudson River PCB Reassessment

Preliminary Model Calibration Report

Review Comments

Dr. Alan W. Maki

August 21, 1998

General Comments: The project managers are to be congratulated for the broad-based state of the science approach detailed within the Preliminary Model Calibration Report (PMCR). The contemporary literature and key experts in the chemical fate modeling arena have been effectively combined to yield a promising program that will undoubtedly address the key objectives requested by EPA. However, there remain two key areas that I feel will ultimately limit the success of the program: a) failure to incorporate guidance from the human health and ecological risk assessment paradigms early in the original design of the program; b) costs associated with the need for optimal field validation and empirical data directly from the Hudson River may ultimately preclude the need for much of the efforts associated with model development. Further comments on both of these areas are contained in the responses to peer review charge questions detailed below.

A. Is EPA using appropriate models to support scientifically credible decisions?

The direct answer to this question must be evaluated in light of the original decision to do the reassessment. The information provided to the peer review panel is not sufficient to determine exactly what were the decision criteria used to re-open the earlier 1984 decision. At that time, the No Action alternative was collectively determined to be the best alternative with the thought that natural attenuation of PCB levels would ensue and that natural sedimentation would effectively cap the PCB-laden sediments in place, thus rendering them non-biologically available. It was not clear to this reviewer exactly how or why it was determined that this earlier decision required a reassessment. A clear discussion of exactly what were the decision criteria or concerns that led to the need for a reassessment will help the review panel with the much-needed perspective to answer the question of appropriate models.

Risk Paradigm – The first model that should have been employed to define the ultimate construct of the entire program is the model for human health and ecological risk assessment paradigms. As currently designed, the program reads as though "we are going to do all this state-of-the-science fate and effects modeling for PCB concerns in the Hudson River and then we'll hand it over to the risk assessors to do their assessment." Unfortunately, this is much more the norm for conduct of risk assessments than it should be. Closer adherence to the guidance provided by risk assessment literature would help to focus the entire effort and ensure that limited resources are being maximized to address the key questions. For example, much of the analytical chemist's concerns over subtle differences in environmental fate of closely related PCB congeners is moot since biologists and toxicologists are unable to determine differential biological effects of many of these related congeners.

NEBA Concept – Restoration ecologists have developed the Net Environmental Benefits Assessment (NEBA) concept to provide guidance on when remediation efforts are needed and will help advance the rate of ecosystem recovery versus when further human intervention or remedial activity will not add a Net Environmental Benefit to the recovery process. For the Hudson River PCB clean-up, the issue of PCB interactions with benthic sediments is the main determinant defining the need for additional actions beyond natural attenuation and thus the pivotal action defining a Net Environmental Benefit Assessment.

Sorption, desorption, and resorption of PCB congeners from Hudson River sediments define the subsequent bioavailability of residual PCBs to aquatic biota. In a very real sense, "BCF = 0.8 KOC" is a hypothetical simplistic model relating sorption kinetics and thus bioavailability to organic carbon content of sediments. This reviewer is a strong believer in the application of simplistic models with minimal variables to provide the most useful guidance. As such, I am concerned that the PMCR provides only minimal discussion of sorption kinetics and relationships to bioavailability. It seems that more attention to the fundamental details of these pivotal relationships would help ensure that scientifically credible decisions would indeed result from the project.

B.1. Will the models forecast when PCB levels in fish will recover to levels meeting human and eco-risk criteria?

The models will almost certainly develop a forecast or prediction of the rate of PCB attenuation in Hudson River sediments and biota. The question remains whether this prediction will have any relation to the actual future conditions in the river. To date, the system has behaved within relatively predictable bounds as forecast by simplistic partitioning models. PCBs are found sorbed to fine particulate sediments with high organic content. These sediments are found in net depositional environments such as TIP and movement into the food web occurs through sediment-associated benthos and pore-water partitioning. Relatively simple, two-stage partitioning models demonstrate this well. Under the current scenario, it appears that benthic sediments will serve as a reservoir providing for the slow release of PCBs into Hudson River biota for many years into the future. However, none of these modeling efforts capture the most likely long-term determinant, which will be the 50-year or 100-year flood event. As a strong proponent of ecological chaos theory, this reviewer is convinced that at some point in the future, an extremely wet summer/fall period will be followed by an eastern seaboard hurricane which will bring about high water conditions causing flooding and subsequent scouring of sediments from the depositional areas of the river. That scenario or a winter of heavy snowfall combined with a wet spring could also cause severe flooding with the same effect on the scouring of PCB-laden sediments. Any of these chaotic flooding events will likely scour the sediments and literally flush most PCB residuals downstream and into the marine environment where further capping by natural marine sedimentary processes will ensue. Obviously, none of these flooding scenarios are directly predictable by any of the models currently under development, yet it is this random flooding event that will bring about the most rapid change and recovery of PCB residuals to background levels.

<u>B.2</u>. Will the models help identify other remedial actions to shorten the time required to achieve acceptable risk levels?

The models will help to describe the chemical, physical, and biological interactions that control the distribution of residual PCBs within the Hudson River ecosystem. To the extent that the interactive models help to identify PCB reservoirs or sinks, they will be useful to help identify alternative remedial actions. However, this reviewer feels that existing monitoring data and simplistic partitioning models provide the basic understanding of environmental compartmentalization of PCBs in the Hudson River ecosystem and no magical alternatives are likely to evolve from more complex modeling. Existing data show us that PCBs are mainly found associated with fine sediments of high organic content in the net depositional areas of the river bottom. This PCB reservoir appears to be the source of food web contamination and will likely be a continuing source of PCBs for some time into the future. Therefore, the only relevant remedial strategies involve either removal or sequestering of PCB residuals from these depositional environments such as TIP.

<u>B.3</u>. Will contaminated sediments become 'reactivated" following a major flood event?

It is this reviewer's opinion after reviewing the monitoring and modeling data contained in the PMCR that a major flood event would likely be the best long-term remedial action currently available. Assuming a sufficient flood-stage water level, major scouring of PCB-laden sediments would occur from even the depositional areas such as TIP and other slough or back-water areas where PCB levels are highest. During the flood event, the river would also be carrying a major load of silt and sediments from upstream areas and the river water color would be chocolate-brown with turbidity. Resuspension of PCB-laden sediments would undoubtedly occur; however, due to the flood-stage level of silt and sediments, re-solubilization of PCBs into the water column would be minimal since ample silt would be present to ensure rapid resorption and downstream movement of PCB-laden sediments. The water dilution and sediment loading from a 50-year or 100-year flood event would be sufficient to mobilize PCB sediment complexes downstream to the marine environment of the lower Hudson/Raritan estuary where flood-associated sediments combined with dilution in the marine environment would further attenuate PCB concentrations to extremely low ecological risk levels.

C.1. Are the models suitable for quantifying external forcing functions?

There is no doubt that the modeling components are indeed state-of-the-science and that they incorporate the key variables that influence environmental partitioning of PCBs in the Hudson River ecosystem. One key point that needs better treatment in the models is the bioavailability issue. While the physical transport models do an adequate job of describing scouring and sediment movement, the partitioning relationships that drive sorption/desorption and ultimately bioavailability need to be better considered in the models. It is not just pure physical transport of PCB-laden sediments that is important, but it is also the availability of those residual PCBs that defines their food web mobility.

On page 3-6, a discussion of the Solids Submodel and Toxic Chemical Submodel underscores the importance of organic carbon content of sediments as a key determinant of PCB fate in sediments. It was not clear to this reviewer that organic carbon content was adequately considered in the subsequent model development.

This discussion again appears on page 8-9 where DiToro's work showing a relationship between partitioning and foc is shown. It is not clear that this key literature has been fully incorporated into the PMCR effort. Also on page 8-12, we are told that foc data were not available and the partial BAF must be expressed on a whole water basis. This is a source of significant error and calls into question the validity of the entire model.

Also on page 3-6, the statement is made that "The Phase 2 database does not distinguish DOC-bound PCB's from truly dissolved PCB's but measures these together as 'apparent' dissolved phase PCB's." This is a major problem and potentially could render model results useless from a predictive standpoint. This must be clarified and proper distinctions between these phases must be corrected.

Analytical Chemistry. Pages 9-3 to 9-16 discuss the analytical chemist's attempts to resolve the historical database for PCB peak resolution. Clearly, the historical trend analysis is confounded by different analytical techniques and resolution levels. This problem has hounded chemists since the Swedes first found these unidentified peaks in sediments and biota from the Baltic Sea in the early 1970s. Historical trend analysis in the Hudson River and, indeed, the success of the entire modeling effort, are tied to the ability to successfully resolve the earlier analyses. Chemists must develop a method and agree on its use to interpolate the earlier data. Analytical methods in the future will continue to change and improve; unless agreement is achieved on a "best approach," we will continue the endless debate on PCB peak resolution.

C.2. Are spatial and temporal scales of the models adequate? What data are required for validation/calibration?

These are two very different questions requiring significantly different answers. This reviewer finds the second question regarding validation as perhaps the major issue facing the future of the entire modeling effort.

First, regarding the issue of scale, I would like to see more attention to the entire riverine ecosystem including the lower estuary. It is not clear why marine fish and marine concerns were not included beyond the Thomann model, since gradual downstream movement of PCB sediments appears to be occurring. If attenuation to low risk levels is occurring in the lower river, this is important to include in the model.

Validation/Calibration. On page 2-3 we are told that the HUDTOX model provides a "reasonable representation" of PCB dynamics during a nine-month period in 1993. This is encouraging, but it seems incredible that validation is only available for that short period. However, on page 4-2 we are told that it is not yet clear whether the PCB dynamics are historically accurate or whether they are representative of future dynamics. This level of ambiguity seems to characterize the discussion of validation throughout the PMCR. The modelers involved with each section of the entire PMCR need to include a specific discussion section stating exactly how model parameters are to be calibrated and validated against real-world data. Without validation, the models remain as simple cartoons of reality, open to speculation and challenge.

C.3. Is it better to develop several models of PCB dynamics or use a single "best" model?

Chapter 8 discusses the various modeling approaches under development. The optimized answer to the question should come from the risk assessors who will be using the model output in the risk assessment. They should be much more strongly involved in the entire modeling process at all steps to help ensure the focus and ultimate utility of the models. With their involvement, I am virtually certain that the empirical probabilistic model derived from the real-world data would be of most value. Following this approach the model is essentially calibrated from the outset and can then be readily validated by comparison with historical data. The 'best" model selected for development must fully consider sediment/water interactions, partitioning theory, and solubilization as the main exposure route and food web sources as secondary routes for fish body burdens.

In many places in the PMCR, vague promises or allusions are made to the model's ability to provide further insight into the role of water versus sediment versus forage fish as sources of PCBs (page 8-6). These allusions need to be tempered with a strong dose of reality. This reviewer does not feel that the models will elucidate these complex relationships. Either carefully designed and controlled laboratory experiments or major field monitoring programs are needed to accurately describe PCB dynamics.

C.4. Is the level of process resolution adequate to describe PCB dynamics?

This question is not substantively different from C.2 and my answers there pertain equally well to this question. Indeed, much of the detail included in many of the modeling sections seems superfluous and subject for later "tuning" once the basic model is running. In many places, it appears that the modelers are attempting to "micrometer a brick." Examples include page 5-5 where concerns are expressed over the localized effect of bridges on water flow measurements. Also, page 8-2 and 10-27 where the six main fish species to be modeled are discussed. I don't see where we need to spend a great deal of effort to distinguish between pumpkinseed, yellow perch and white perch. They are functionally very similar and should show similar PCB dynamics. Use of a shiner, perch, bullhead, and possible addition of the striped bass would simplify resolution issues as the models are developed.

C.5. Will the models be useful for human and eco-risk assessments?

Yes, but as stated in responses to the previous questions, the risk assessors need to be involved much more rigorously in the entire model development process. The PMCR simply reads as a project to generate the models and 'when we're done we'll give it all to the risk assessors to see what they can make of it." Obviously, this is not correct and the risk assessors need to be involved much more interactively at all stages of the process. The program managers should regularly consult the risk assessment paradigm to help guide decisions throughout the model development process.

D & E. What changes would improve the outcome? Are there serious flaws that will limit use of the models?

This reviewer is unable to distinguish different responses to questions D & E since they are re-statements of the same question and ask for a summary of the changes we recommend. My comments on the program are summarized in responses to all previous questions and are thus summarized in bullet form here:

- Incorporate and follow the risk assessment paradigm in all stages of model development.

- Involve the risk assessors not only at the end but throughout the model development.
- Validate all steps of the model and model outputs with real-world monitoring data from the Hudson river ecosystem.
- Give better consideration to the factors controlling bioavailability of PCBs. These include sorption/desorption kinetics, solubilization and organic content of sediments and equilibrium partitioning.
- Make better use of simplistic, two-stage partitioning models to model PCB dynamics before developing the complex, multi-variable models.
- Chaos theory would say to plan for the 50-year or 100-year flood event as the most reasonable remedial action.
- Resolve the analytical chemistry to ensure full consideration of historical PCB monitoring data.
- Consider the Net Environmental Benefits Assessment (NEBA) before recommending additional remedial actions.
- Models must be able to distinguish between soluble and sediment/DOC-bound PCB's. These cannot simply be lumped together since they represent fundamentally different PCB reservoirs.
- Don't spend a lot of resources attempting to "micrometer a brick." Target for simplistic, reasonably accurate outputs before involving too many extrinsic variables on too many species.
- Do not hold unrealistic expectations of the modeling capability. There is very little hope that much insight will be gained from modeling into the complex interactions of PCB bio-dynamics.
- Do not let the analytical chemists get ahead of the toxicologists. The risk assessment will gain little value from chemists' resolution of each substituted PCB congener if toxicologists cannot explain the biological/toxicological significance of these congeners.

Thanos Papanicolaou

Thanos Papanicolaou

Dr. Papanicolaou, Assistant Professor of Civil and Environmental Engineering, Washington State University, received his M.Sc. and Ph.D. in Hydraulic Engineering from Virginia Polytechnic Institute and State University, Blacksburg, VA in 1992 and 1997, respectively and his B.Sc. degree with honors from the Department of Civil Engineering in Aristotels University of Thessaloniki in 1990.

He has been a Research Assistant of the Department of Civil Engineering at Virginia Tech from 1991-1997. During this period his research received support from the International program of NATO Fellowships and from the International organization A. ONASSIS. In addition, he received support from two different projects sponsored by the National Science Foundation and U.S. Geological Survey. From 1989 until 1990 he worked as a consultant to the engineering firm, THEMELIODOMI. His main area of specialization is sediment transport and experimental fluid mechanics.

He has served as a reviewer for the Journal of Hydraulic Engineering, Computing Engineering, and Engineering Mechanics of American Society of Civil Engineers (ASCE) and he is member of the ASCE, American Water Resources Association (AWRA), American Geophysical Union (AGU), and of the Greek chamber of Civil Engineers. Dr. Papanicolaou is the author of more than 35 research papers and serves as a member of the American Society of Mechanical Engineers (ASME) turbulence committee.

Dr. Papanicolaou currently supervises two graduate and two undergraduate students. He is involved in cohesive sediment studies examining the role of turbulence on the entrainment of sediment. Currently, he is involved in a project focusing on different fish bypass designs. He taught the open channel flow course at Virginia Tech and the Sediment transport, Experimental Hydraulic Engineering, and Fluid Mechanics courses at Washington State University.

Final Comments

of

Thanos N. Papanicolaou

Title: Thompson Island Pool Depth Modeling

Question A: Is EPAusing appropriate models, datasets and assumptions on which to base a scientifically credible decision?

I feel comfortable to comment on the efficiency of the hydrodynamic model that is used here to calculate the erosion rate of sediment in Hudson River.

The hydrodynamic model that is used in the present study is the RMA-2V model. This model has significant capabilities comparatively to other numerical hydrodymanic models (e.g. HEC-6). First, the RMA-2V is a two dimensional finite element model and provides the stream local velocity in the longitudinal and transverse direction of the stream at different time instants. Second, the RMA-2V model is one of the few models that is supported by other commercial software that are used for the pre-processing of the input data file (i.e. the program Fast Tabs developed by the Boss Corp.).

In the present study, the quasi-steady aspect of the problem is examined. The RMA-2V finite element model is solved for sufficient large time steps to satisfy the conditions needed for the quasi-steady solution of the problem. The solution of the model, viz. the velocities in the longitudinal and transverse directions, are used to calculate the bed shear stress at different locations along the stream cross section. Knowledge of the magnitude of the bed shear stress is important to determine the erosion rates of sediment.

There are two types of sediment types within the Thompson Island Pool, cohesive and non-cohesive. The erosion of the cohesive sediment is studied here by using the Lick et al. (1995) model (i.e. equations (3-6) and (5-2) in the report). No method has been adopted for the study of the cohesion-less sediment erosion process.

At this point, the reviewer would like to comment on the effectiveness of the cohesive erosion model. The Lick et al. model (1995) is used here to describe the entrainment of cohesive sediments. Specifically, this model considers that the sediment erosion rate is well described by a power law. The parameters considered in the model are: the bed shear stress, and the time after the last sediment deposition took place. While the above model constitutes the latest development in the area of cohesive sedimentation, it presents some limitations since it is applicable to cases that the flow conditions are not extremely intensive. The present model is limited to flow conditions under which the average bed shear stress does not exceed more than 20 dynes/cm^2. During flood conditions the bed shear stress obtains values that are close to 50-100 dynes/cm^2. Under these conditions the erosion of the sediment bed is not sporadic but reaches an equilibrium value. Despite the above limitations, the existing cohesive scour model (equation (3-7)) provides accurate results when the average bed shear is not greater than 20 dynes/cm^2 (this applies here).

In the present study, determination of the entrainment of the cohesion-less sediment is important since "42 percent of the PCB mass reservoir as of 1984 was located in larger sediment areas consisting of non-cohesive sediment". Another reason for determining the entrainment rate of non-cohesive material is the notion that the existence of large quantities of non-cohesive sediment may alter the sedimentation behavior of cohesive sediments. The present scour model does not take into consideration the entrainment of non-cohesive sediments. The reviewer strongly believes that knowledge of the erosion rate of non- cohesive sediments is very important in order to determine the total rate of sediment that will be likely "reactivated" following a major flood. The research team involved in this project intends in the near future to consider in their study the erosion of non-cohesive sediments.

The reviewer would like to provide some information about the existing state-of-the-art knowledge in the area of non-cohesive sediment entrainment. There are two types of models, the deterministic and stochastic models. The deterministic models consider the average bed shear stress as responsible for the entrainment of sediment while the stochastic models take into consideration the occurrence of turbulent bursts. The latest developments in the area of sediment-water interaction suggest that the erosion process, especially for low flow conditions, is a random process. Unfortunately, use of the stochastic models at this point is not possible due to the complex nature of the erosion process. For this reason, the reviewer suggests the use of a semi-deterministic model.

This model takes into account the frequency of occurrence of bursts, the sediment availability, the sediment size and weight, and the average bed shear stress and was developed by Cao (1997).

The Cao's entrainment model is described by the following equation:

$E_n = Pd^{1.5}(F/f-1)F$

Where,

E_n=normalized entrainment rate

$$P = \frac{\lambda C_o (sg)^{0.5}}{\nu T_B}$$

where λ is the averaged area of all bursts per unit stream bed area=0.02

C_o is the sediment packing density that is equal to 0.6

s is the sediment specific gravity that is equal to 1.65

g is the acceleration of gravity, equal to 9.8 m/s^2

ν is the Kinematic viscosity and is approximately equal to 10^{-6} m^2/s

T_B is the average bursting period

F is the Shields parameter and is defined as $F = \frac{U_*^2}{sgd}$

Where U_* is the friction velocity and d is the average sediment particle diameter.

It is expected that the above model can be incorporated into the hydrodynamic model suggested here by the research team. In conclusion, the hydrodynamic model suggested here is acceptable with minor revision about the erosion of the cohesion-less sediment.

References:

Cao, Z. (1997). "Turbulent Bursting-Based Sediment Entrainment Function", Journal of Hydraulic Engineering, Vol. 123, No.3, pp. 233-236.

Question B:

1,2: I am not familiar with these issues.

3. Are there contaminated sediments now buried and effectively sequested from the food chain which are likely to become "reactivated" following a major flood, resulting in an increase in contamination of the fish population.

The rate of sediment that will be likely eroded within the TIP strongly depends on the variation of the bed shear stress values. For a 100-year flood event the hydrodynamic model used here predicts that the mean bed shear stress value is equal to 19.5 dynes/cm^2. This value of stress is considered as sufficient to yield the erosion of the less compacted layer located atop the river bed. There are typically two threshold erosion values for the shear stress, one for the newly deposited sediment and one of the consolidated layers. When the bed shear stress exceeds the value of 10 dynes/cm^2 then erosion of the more compacted layers of sediments may occur. The average depth of scour calculated here for the 100-year flood event is within the range of 0.003 cm to 0.97 cm.

Moreover, the shear strength of sediment is largely function of the moisture content of sediment. Knowledge of the moisture content of sediments is required to provide a definite answer to the above question.

Question C:

1. Are the modeling approaches suitable for developing quantitative relationships between external forcing functions and PCB concentrations in the water column, sediments and fish? Are the models adequate for discriminating between water-related and sediment-related sources of PCBs?

The present work involves the use of different models for the transport and fate of PCBs in the water column and bedded sediments, and for PCB body burdens in fish. The modeling approach is suitable for the present problem since it incorporates 3 separate mass balances, viz., a water balance, a solids balance and a PCB balance. The solids balance provides information about the amount of PCBs absorbed to the sediments, the water balance is important since it provides the rate of PCB's transported by water, and PCB balance provides the amount of PCBs as a function of sediment-water and air-water exchanges.

2. Are the spatial and temporal scales of the modeling approaches adequate to answer the principal study questions?

I feel comfortable to comment on the spatial temporal scales of the hydrodynamic and scour model. For the scour model a uniform size grid was defined by using a fine scale Geographical Information system approach. The cell spacing that was chosen here is 10 feet. This level of spatial resolution is adequate in capturing the level of erosion that takes place within the TIP.

The spatial scaling of the hydrodynamic model (i.e., RMA-2V) is found reasonable. The size of the grid depends on the magnitude of the velocity. Therefore, the number of elements in the finite element mesh should be adjusted in accordance to the current flow conditions at the site.

The reviewer would like to express his concern about the hypothesis used here that the maximum bed shear stress value is established instantaneously. This may not be true in all cases.

3. It is contemplated that PCB concentrations in fish will be estimated using several modeling approaches: an empirical probabilistic model derived from Hudson River data, a steady state model that takes into account mechanisms of bioaccumulation body burdens, and a time-varying mechanistic model. A bivariate statistical model may also be used to provide insight into accumulations. This multi-model approach is being contemplated because of the uncertainties associated with any individual model. Is this a reasonable approach or should predictions be made using a single "best" model?

The reviewer would like to compliment the research team focusing on the study of the PCB concentrations in fish. The idea of using a bi-variate statistical model is very interesting. Similar approach has been used in the past in studying the flow-sediment interaction problem. The multi-model is acceptable as is.

4. Is the level of process resolution in the models adequate to answer the principal study questions? If not, what processes and what levels of resolution are required to answer these questions? What supporting data are required for those processes and levels of resolution?

The calibrated RMA-2V model is a reasonable representation of the hydraulic conditions that exist within TIP. The fact that the model provides velocity values that do not deviate significantly for the values provided by the USGS indicates that the calibration procedure that was followed during the course of this study is pretty accurate. The reviewer suggests here that the value of the average equivalent sand roughness is equal to 3 d_{50}.(Van Rijn 1984).

D. Are there any changes to the work effort outlined in the revised work plan that would significantly improve the outcome?

The scour model should be modified to account for the erosion rate of non-cohesive sediments. This will make the scour model more general and applicable to different sediment types (review the suggested model for non-cohesive sediments (see the answer for question 3(B)).

Frank Wania

Frank Wania

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Frank Wania is an independent research scientist in Toronto, Canada. His clients include industry, academia, governments, and non-governmental organizations from Canada, the USA, Norway, Sweden, and other European countries. He has served as a member on the North American Expert Advisory Panel on Continental Pollutant Pathways and is presently a member of the editorial board of "AMBIO a Journal of the Human Environment," published by the Royal Swedish Academy of Sciences. He is the author of more than 50 papers, including more than 20 peer-reviewed articles in scientific journals and books.

Peer Review of Hudson River PCBs Reassessment RI/FS Phase 2 reports Preliminary Model Calibration Report

by

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A. Is EPA using appropriate models, datasets and assumptions on which to base a scientifically credible decision?

The approach of using models of the physical environment of the Hudson River to predict water and sediment concentrations, and then use these as input for models of the uptake in biota appears sensible and appropriate. The models of PCB transport and fate in the Hudson River are based on well-established and accepted methodologies for the quantitative description of hydrophobic organic substances in aquatic systems. They are using an adequate spatial and temporal resolution and include most of the relevant fate processes. The presented models to predict levels in fish so far rely in my opinion too much on empirical data and too little on a mechanistic understanding of the processes of bioaccumulation.

The amount of data gathered on PCB in the Hudson River and used in this investigation is unique and impressive in terms of both their spatial and temporal coverage, and the number of compartments investigated. The Hudson is possibly the most comprehensively investigated river with respect to PCB contamination. The extent of work devoted in this project to making historic and diverse data sets on PCB concentrations comparable is well-spend, because crucial for the success of the project.

Despite the models being state-of-the-art, there is a possibility that their usefulness for answering the principal questions will be limited, because of the large uncertainty attached to their predictions. This is not because the models, datasets or assumptions are unsuitable for the task they were designed to address. Our quantitative understanding of the behaviour of PCBs in river systems and the fresh water food web may simply be so limited that it does not enable us to predict with sufficient certainty what the levels of PCBs in fish in the Hudson River are going to be two decades from now.

B. Will the models, with the associated datasets and assumptions, be able to answer the principal study questions as stated in the PMCR

1. When will PCB levels in the fish population recover to levels meeting human health and ecological risk criteria under No Action?

In order to answer this question, it is necessary to predict water and sediment concentrations in the future on the time scale of decades. This involves the prediction of (1) the future release of PCBs from the contaminated sediments, and (2) the future import of PCBs to the river from the drainage basin.

In order to accomplish task (1), it is necessary to:

- explain and quantitatively describe what processes are responsible for the observed increase in PCB water concentration across the TIP and presumably also upstream of the TIP.
- predict how that process is likely to develop in the future.

I am not convinced that it can presently be stated with confidence that the HUDTOX model has a full quantitative understanding of the mechanisms of PCB release from the sediments in TIP. This is illustrated by the need to invoke contaminated advective pore water inflow to explain the measured increase of the lower chlorinated PCB congeners across TIP.

Particularly worrisome is that there appears to be a significant discrepancy between the results of the HUDTOX model and the TIP Depth of Scour model in their estimation of how much PCB is released with resuspended sediments during the model calibration period.

According to Figure 4-39, the HUDTOX model calculates that during the model calibration period (i.e. the first nine month of 1993) 405.7 kg of PCBs were transferred to the water column with resuspended sediments within TIP. Most of that transfer occurred during the spring run-off event period from 3/26/93 – 5/10/93 (Figure 4-40). That period saw its peak flow on April 12, 1993 with 20,300 ft^3/s (Table 5-1). The TIP-Depth of Scour model does not report specific estimations of the mass of PCB eroded from TIP during the spring event 1993. However, the spring 1994 event with a peak flow of 28,000 ft^3/s is predicted to have eroded 6.58 kg. The spring 1992 event with 19,000 ft^3/s is predicted to have eroded only 1.57 kg (Table 6-5). There is thus a huge discrepancy between the estimated mass of PCB eroded from TIP during the 1993 flood event by the TIP-Depth of Scour model (approx. 2 kg) and the HUDTOX model (approx. 300 kg).

I see several potential explanations for that discrepancy: Maybe the "mass of PCB eroded" estimated by the TIP depth of scour model does not apply to the entire flood event, but only to a relatively short time span (e.g. the one day period of maximum flow). If that is the case the estimates of PCB eroded as reported in Table 6-5 could be very misleading. The mass released during the entire flood period could be potentially much higher than what is listed in that Table. Another potential explanation is that the TIP-Depth of Scour model does not yet include erosion of non-cohesive sediments. However, the mass of PCB eroded from non-cohesive sediments is unlikely to be so large as to resolve this large discrepancy.

The discrepancy between the two models applies similarly to the solids. The HUDTOX model calculates a resuspension of $7.56 \cdot 10^{+6}$ kg from TIP during the entire calibration period (Figure 4-36), and most of it during the 1993 spring flood event with 20,300 ft^3/s (Figure 4-37). The TIP-Depth of Scour model estimates only $5.53 \cdot 10^{+4}$ kg for a event with 19,000 ft^3/s and $2.20 \cdot 10^{+5}$ kg for a event with 28,000 ft^3/s.

Presumably the resuspension rates in the HUDTOX model are that high in order to explain the increase in PCB concentrations across TIP. That discrepancy needs to be resolved because this increase of PCB concentrations in the TIP is at the very core of the problem to be addressed! (After writing this, I read on page 5 of the revised Appendix B, that "for the 1993 and 1994 flow events, cumulative gross resuspension estimates from the TIP resuspension model will be compared with cumulative gross resuspension results from the TIP portion of the revised HUDTOX model." This is exactly what I was trying to do in a "back-of-the-envelope" fashion above.)

As long as the observed increases in PCB water concentrations in TIP can not be described in HUDTOX with sediment resuspension rates consistent with the Depth of Scour model, it is unlikely that it can predict the release of PCBs from sediments in the future. In addition to an understanding of the release processes and their kinetics, the dynamics of this future release would obviously require the capability to predict the rate by which the PCB reservoirs in the sediments are being buried. The planned long-term hindcasting calibration should give some indication of the capability of the HUDTOX model to describe the rate of this process.

As mentioned above, the prediction of future exposure concentrations requires also an estimate of the future import of PCBs from across the upstream model boundary. In the revised Appendix B it is stated that for the predictive No Action scenario modelling, "long term -time series must be constructed for [...] external loadings of [...] PCBs." (Page 5, 1. Paragraph). I wonder how this should be reliably done, considering that there is some clear indication that the inflow of PCBs at the Northern model boundary derives not only from the watershed, but that there are upstream sources of PCBs other than the run-off of atmospherically deposited PCBs.

During the model calibration period:

	Upstream	Tributaries	U/(U+T)	Source
Average water flow	5418 ft^3/s	9465 ft^3/s	36.4 %	Table 4-2
TSS load	$3.55 \cdot 10^{+7}$ kg	$3.85 \cdot 10^{+8}$ kg	8.4 %	Figure 4-35
PCB load	352.0 kg	121.46 kg	73.4 %	Figure 4-38

This table shows that the Hudson River upstream of Ft. Edward supplied only 36.4 % of the total water input to the Upper Hudson River, and less than 10 % of the solids (the rest being supplied by the other tributaries). Nevertheless, it imported almost 73.4 % of the external PCB load. Because there is little reason to believe that the watershed input from atmospherically derived PCBs is substantially different between the drainage basins of the Upper Hudson River above Fort Edward and the other tributaries, such as the Mohawk River, these data clearly indicate that there are other sources of PCBs in the upstream Hudson River.

Some of the presented data (e.g. Figures 10-1 to 10-8) show that sediments upstream of river mile 195 are contaminated with PCBs. Though levels tend to be lower than in TIP, they are consistently higher than in the lower Hudson or in the sediments upstream of Hudson Falls (river mile 200). What was the rationale for starting the simulation a few kilometers downstream of the original PCBs discharge points, namely why was the section from Hudson Falls to Fort Edward (river mile 195 to 200) not included in HUDTOX and the depth of scour model?

Inclusion of that section in the model would make a clearer distinction between watershed PCB inputs and internal river source possible. For a prediction of future development of the river concentration, the future development of the PCB concentrations at the upstream boundary are of obvious importance. It is difficult enough to predict what those concentrations are likely to be if they are only determined by atmospherically derived watershed inputs. It will be considerably more difficult if they are influenced by river-internal sources outside of the model boundaries.

B. Will the models, with the associated datasets and assumptions, be able to answer the principal study questions as stated in the PCMR

2. Can remedies other than No Action significantly shorten the time required to achieve acceptable risk levels?

The PMCR never details what the alternatives to "No Action" are beyond vaguely referring to "selected dredging and/or containment" (page 13 of revised Appendix B). How should it be possible to judge whether the models can evaluate a scenario, if that scenario is not specified? If the scenarios involve shutting off future release of PCB from certain contaminated sediment sections, it may be possible to calculate the effect on fish concentrations, if the models spatially resolves these sections.

B. Will the models, with the associated datasets and assumptions, be able to answer the principal study questions as stated in the PCMR

3. Are there contaminated sediments now buried and effectively sequestered from the food chain which are likely to become "reactivated" following a major flood, resulting in an increase in contamination of the fish population?

The combination of the hydrodynamic model and the Depth of Scour model for TIP should be able to address the question of how much of the PCBs buried in the sediments could be reactivated during a "flood event". (The discrepancy in the estimated amount of resuspended sediment/PCB between the two models approaches, which is mentioned above, needs to be resolved.) What has not been laid out in detail is how the result of that calculation would feed into the models of food chain uptake/accumulation. Such a link would be necessary to answer the question of the impact of that "reactivation" on PCB levels in fish. The presented biological model approaches assume some sort of steady-state exposure concentration and are not designed to describe fish uptake (let alone the effects on fish) during short periods of greatly elevated exposure as it would be caused by a flood event stirring up previously buried sediments. This may be the task assigned to the time-varying model, that is referred to, but not presented in detail, in the PMCR.

C. Specific Questions

1. Are the modeling approaches suitable for developing quantitative relationships between external forcing functions (e.g., hydraulic flows, solids and PCB loads, sediment initial condition, etc.) and PCB concentrations in the water column, sediments and fish? Are the models adequate for discriminating between water-related and sediment-related sources of PCBs?

This question was already addressed in part above.

Why is it important to discriminate between water and sediment-related sources of PCBs to biota? Is it even conceptually sound to distinguish between the two? Aren't present day PCB concentrations in the water column "sediment-related", in that the supply of PCBs to the water column is almost exclusively from the sediments. Even though PCBs in water and sediment may not be in equilibrium, there is likely to be a clear relationship between them.

In that respect it would be a very worthwhile undertaking to investigate the equilibrium status of PCBs (total PCBs as well as individual congeners) between water column and sediment in various river sections. This could be done e.g. by calculating fugacity ratios between water and sediment or by comparing "apparent" or "in-situ" water-sediment partition coefficients derived from measured water and sediment concentrations. (A lot of effort has been spend on comparing carbon and lipid-normalised concentrations in water and biota and in sediment and biota. Something similar should be done for water and sediment.) An analysis of the spatial and temporal variability of that equilibrium status would be very instructive in identifying the relative importance of water and sediments as an exposure medium, and how they influence each other.

C. Specific Questions

2. Are the spatial and temporal scales of the modeling approaches adequate to answer the principal study questions? If not, what levels of spatial and temporal resolution are required to answer these questions? What supporting data are required for calibration/validation of these spatial and temporal scales?

I think the spatial and temporal scales are adequate. A higher spatial resolution (e.g. of the HUDTOX model) would not be justified because the measured concentration data needed for calibration and validation are not available in a higher spatial resolution. Even at the present model resolution there are river segments for which no measurements exist.

An issue of spatial scale was already mentioned before: If an alternative to No Action involves that certain sediment sections are being dredged or contained, the sediment compartmentalisation in the model should be able to resolve these sediment sections. By adding a second dimension to the description of TIP in HUDTOX, this may have been accomplished.

Two issues related to temporal scales, which were already mentioned before, may be worth repeating here:

1. It is not clear to me, to what temporal scale the Depth of Scour model predictions (mass of sediment and PCB eroded) refer to. Do they refer to the entire flood event or only to the day (or hour ?) of peak flow?
2. The question of the impact of flood-related reactivation on PCB levels in fish could only be addressed with a time-variant model of food chain uptake, resolving time scales of less than a month.

C. Specific Questions

3. It is contemplated that PCB concentrations in fish will be estimated using several modeling approaches: an empirical probabilistic model derived from Hudson River data, a steady-state model that takes into account mechanisms of bioaccumulation body burdens, and a time-varying mechanistic model (not included in the PMCR). A bi-variate statistical model may also be used to provide insight into accumulations. This multi-model approach is being contemplated because of the uncertainties associated with any individual model. Is this a reasonable approach or should predictions be made using a single "best" model?

The PMCR primarily presents the two empirical approaches (bi-variate statistical model and probabilistic model) for estimating PCB concentrations in fish, whereas it is rather vague on the more mechanistic approaches.

Mechanistic and empirical approach have advantages and disadvantages:

The empirical approach:

+ is very specific to the studied system, as accumulation factors are estimated from concentrations measured in the Hudson River.
- is only as good as the available data (As becomes clear from reading the PMCR the necessary data are often missing ("numerous data gaps" page 12 in revised workplan). It is even suggested to use model-predicted concentration in water and

sediment for deriving "empirical" accumulation factors. I am very skeptical about employing calculated data as substitutes for measured data in a supposedly empirical model)

- provides only a limited understanding of how the system works (E.g. a accumulation factor derived from measurements is taken as it is, there is no attempt at understanding the underlying processes). If there is a change in how the system operates, the empirical approach is likely to fail (e.g. a profound system change would occur if PCB concentrations in the water had been driven by upstream loadings of PCBs for some time, but as these loadings decrease is increasingly controlled by release from contaminated sediments). Are the empirical relationships still valid after potential remedial measures, e.g. dredging?

The mechanistic approach:

+ may be able to describe the system even after a major change in system properties, because it simulates and predict the system from an understanding of the underlying chemical and physical principles.

- may fail to include all relevant processes (e.g. pore water advection). Also, the quantitative understanding of the processes may be poor.

Although specified in the question above, I see little in the PMCR or the revised workplan that shows a commitment to seriously pursuing a mechanistic modelling approach ("is being explored", "is being evaluated"). I suggest that it is extremely important that a mechanistic food chain accumulation model be used to complement the (semi-)empirical models described in the PMCR. Models such as those by Thomann et al. (1992), Gobas (1993), or Campfens and Mackay (1997) are publicly available and should be well suited for the Hudson River:

Thomann, R.V., Connolly J.P., and Parkerton T.F. **1992**. An equilibrium model of organic chemical accumulation in aquatic food webs with sediment interaction. *Environmental Toxicology and Chemistry* **11**, 615-629.

Gobas, F. **1993**. A model for predicting the bioaccumulation of hydrophobic organic chemicals in aquatic food webs: application to Lake Ontario. *Ecological Modelling* **69**: 1-17

Campfens, J. and Mackay, D. **1997**. Fugacity-Based Model of PCB Bioaccumulation in Complex Aquatic Food Webs. *Environ. Sci. Technol.* **31**: 557-583.

For a recent comparative discussion of the models by Thomann et al. and Gobas see:

Burkhard, L.P. **1998**. Comparison of two models for predicting bioaccumulation of hydrophobic organic chemicals in a Great Lakes food web. *Environ. Toxicol. Chem.* **17**, 383-393.

In brief, it is very important to use more than one model and check the validity of predictions by comparing the results of several modelling approaches. But these approaches should go beyond the two presented in the PMCR.

C. Specific Questions

4. Is the level of process resolution in the model adequate to answer the principal study questions? If not, what processes and what level of resolution are required to answer these questions? What supporting data (such as data to support specifications of a mixed depth layer, solids and scour dynamics, ground water inflow, etc.) are required for these processes and levels of resolution?

As the authors of the PMCR point out themselves, advective flow of pore water is another process potentially transferring PCBs from sediments to the water column. Pore water diffusion of PCBs tends to be fairly low and resuspension of particle-bound PCBs is mostly an episodic phenomenon, so ground water inflow may be a significant process explaining the increase of PCB water concentrations across TIP during periods without flood events. Whereas the authors mention only advective pore water inflow of truly dissolved PCBs (which is most pronounced for the most water soluble congeners such as BZ#4), it is conceivable that also the less water soluble congeners could be transferred by this route, if DOC – and thus DOC-bound PCBs – is advected out of the sediments.

C. Specific Questions

5. The results of the modeling effort will be used, in part, to support human and ecological risk assessments. In your judgment, will the models provide estimates adequate for this purpose?

Human and ecological risk assessment is outside my area of expertise and I thus see myself not in a position to pass a judgment on this issue.

D. Are there any changes to the work effort outlined in the revised work plan that would significantly improve the outcome?

As mentioned before, I suggest

- to include river mile 195-200 in both the HUDTOX model and the hydrodynamic and depth of scour model.

- to be careful in not relying solely on the empirical models of PCB uptake in fish, especially considering the gaps in the available data. The use of a mechanistic food chain accumulation model is imperative.

E. In terms of evaluating the overall and specific effects and behaviour of PCBs in the Hudson River, are there any serious flaws in the modeling approach (theory, structure, physical parameters, etc.) that would limit or invalidate any conclusions of further work based on results of these models?

The shortcomings that I pointed out above are not of such fundamental nature that they invalidate the entire approach, yet addressing them may increase the usefulness of the models.

I am bit puzzled to see the word "effects [...] of PCBs" in this question. None of the presented methodologies addresses the question of effects. The focus of the presented work is entirely on exposure.

Other Issues

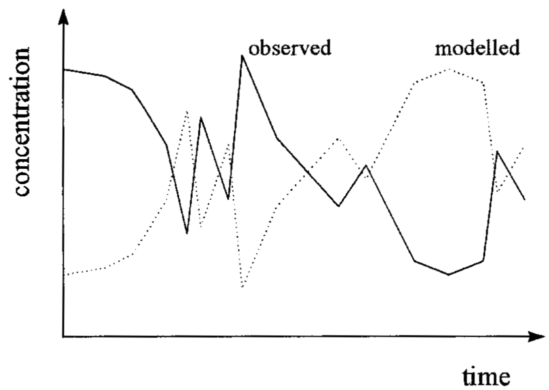
Statistical Procedures

Though I am not a statistician, I am a bit skeptical about the appropriateness of various statistical procedures employed in the PMCR:

Comparison between model results and observed data with t-tests

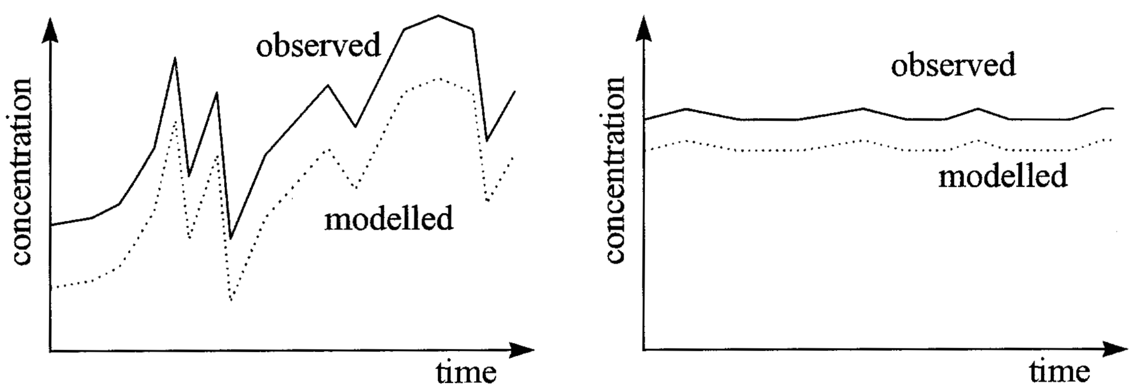
In checking the HUDTOX model performance, t-tests are employed comparing the mean of the measured concentrations with the mean of the calculated concentrations. If the test shows no significant difference, it is taken as a sign that the model performs well. I think there are two problems with this approach:

1: The observed and the modelled concentrations are not random samples of a population, but refer to particular points in time. When comparing the means rather than the observed and modelled concentrations which apply to the same point in time, that aspect is ignored. For illustration, see example below: The modelled and observed concentrations have the same mean and a large variance, and a t-test on the means will thus show definitely no significant difference. Yet the model-measurement agreement is very poor.



2: A closer analysis of the comparison shows that often the t-test indicates "no significant difference" not because the means are close to each other, but because the variance of the means is so high. To say that two means are not significantly different is not the same as saying that the two means are actually similar. No judgment on model performance can thus be derived from a test result suggesting "no significant difference".

Again, the example below may illustrate the point: the comparison to the left will show "no significant difference" because of the high variance, whereas the comparison to the right will show "significant difference" even though the means of modelled and observed concentrations are actually closer.



Need to distinguish between development and testing of an empirical model

A similar sort of problem, I have with the bi-variate statistical model presented in Chapter 9. The goodness of fit between "predicted" concentrations and measured concentration as shown in Figure 9-8 through 9-19 is seen as providing "good explanatory power in predicting annual mean body burden..." (page 9-15). This is not a correct statement. In fact, the predictive capabilities of this model have not been tested at all. These graphs (and the r^2 in Table 10) only give an indication of how well measured fish body burdens and measured water and sediment concentrations are correlated. The predictive power of the regression model would have to be tested on independent data pairs, i.e. on measured fish and water/sediment concentrations which were not used in the derivation of the regression equations. A distinction between a calibration data set and a test data set is necessary.

Derivation of distributions for accumulation factors

In deriving the distributions of accumulation factors, the individual measured concentrations in biota are divided by the mean of the water or sediment concentrations. What is the rationale for using the mean of the water and sediment concentrations rather than their distributions?

Other Issues

Discussion of BSAFs

Many of the presented BSAF (Figure 10-9 to 10-40) are close to 1, i.e. indicate equilibrium partitioning between sediments and benthic invertebrates. Deviations from this for some locations and some species are variable and uncertain, i.e. can not really be explained satisfactorily (E.g. what is the mechanism explaining higher than equilibrium partitioning in zooplankton? Gastrointestinal biomagnification at the stage of the zooplankton? Also, the combination of water column data with zooplankton data requires often quite strenuous assumptions.)

Is there really something gained by going beyond an assumption of equilibrium partitioning? Isn't it rather introducing a lot of uncertainty. The Gobas model predicts concentrations in benthic and pelagic zooplankton (and thus also for phytoplankton) from simple equilibrium partitioning. Can it be tested whether the BSAFs are actually significantly different from 1.

Other Issues

Uptake of PCBs in plankton

Page 10-14, 2nd paragraph and page 10-18, alternative approach 6

"Skoglund et al. found that phytoplankton accumulate **more** PCB than would be predicted be equilibrium partitioning alone"

"equilibrium model significantly underestimates observed accumulation"

This is not correct, but rather the opposite is true. The model by Skoglund et al. shows that during periods of rapid growth the accumulation is **less** than what equilibrium partitioning would predict, because the kinetics of PCB uptake a slower than the kinetics of growth! Also, as far as I know the model by Skoglund et al. applies to phytoplankton and not zooplankton!

Other Issues

Amounts of PCBs in cohesive and non-cohesive sediments in TIP

Page 6.2: Referring to TIP sediments, it is stated that "based on the vertically-integrated coverage, the inventory of PCBs in the cohesive areas was 3208 kg (28.7 %), as compared to 7974 kg (71.1 %) in the non-cohesive areas." On the other hand, page 5 of revised Appendix B states that 58 % of the PCB mass in TIP is in cohesive type sediments, and 42 % in non-cohesive type sediments. Why is there such a discrepancy in the relative importance of various sediment types for storing PCBs in the TIP?

Other Issues

Similar Study on PCB-Contaminated Sediments in a River System

I would like to point the attention of those involved in this project on PCBs in the Hudson River to a similar study conducted on the Emån River in Southern Sweden. Though the river involved is considerably smaller than the Hudson River, the contamination situation was similar in that PCB contaminated sediments had for years provided a constant source of PCBs to the water and the biota living in that river. Also the climate and the seasonality of run-off conditions are not unlike those in the upstate New York. This river has now been subject to remediation, which involved the dredging and land-filling of the contaminated sediments. A comprehensive description of that study, and particularly the results of the environmental monitoring before, during and after site remediation can be found in a Ph.D. thesis:

Bremle, G. **1997**. *Polychlorinated Biphenyls (PCB) in a River Ecosystem.* Doctoral dissertation, Lund University, Lund, Sweden, 144 pages (ISBN 91-7105-085-X)

For a copy of the thesis you may want to approach:

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Several aspects of that thesis - though by far not all of them - are also accessible as peer-reviewed scientific publications:

Bremle, G., Okla, L., and Larsson, P. **1995**. Uptake of PCBs in fish on a contaminated river system: Bioconcentration factors measured in the field. *Environ. Sci. Technol.* **29**, 2010-2015.

Bremle, G., and Ewald, G. **1995**. Bioconcentration of polychlorinated biphenyls (PCBs) in Chironomid larvae, oligochaete worms and fish from contaminated lake sediment. *Mar. Freshwater Res.* **46**, 267-273.

Bremle, G, and Larsson, P. **1998**. PCB in the air during landfilling of contaminated lake sediment. *Atmos. Environ.* **32**, 1010-1019.

APPENDIX D

LIST OF REGISTERED OBSERVERS OF THE PEER REVIEW MEETING

United States
Environmental Protection Agency
Region 2

Hudson River PCBs Site
Modeling Approach Peer Review

Sheraton Saratoga Springs Hotel and Conference Center
Saratoga Springs, NY
September 9-10, 1998

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APPENDIX E

AGENDA FOR THE PEER REVIEW MEETING

Hudson River PCBs Site
Modeling Approach Peer Review

Sheraton Saratoga Springs Hotel and Conference Center
Saratoga Springs, NY
September 9-10, 1998

Agenda

Meeting Chair: **Alan Maki, Exxon**
Meeting Facilitator: **Jan Connery, Eastern Research Group, Inc. (ERG)**

WEDNESDAY, SEPTEMBER 9 ,1998

8:00AM	**Registration/Check-In**
9:00AM	**Welcome Remarks and Panel Introduction (Disclosures)** *Jan Connery, ERG, and Alan Maki, Exxon*
9:20AM	**Overview, Background, Meeting Structure, and Objectives** *Jan Connery*
9:30AM	**Presentation on the Development of the Model** *Doug Tomchuk, EPA Region 2*
10:00AM	BREAK
10:15AM	**Charge to the Panel/Highlights of Premeeting Comments** *Alan Maki*
10:50AM	**Discussion on Question A: Is EPA Using Appropriate Models, Datasets, and Assumptions on Which to Base a Scientifically Credible Decision?** *Discussion facilitated by Jan Connery and Alan Maki*
11:40AM	**Summary of Discussions on Question A**
12:00NOON	LUNCH
1:00PM	**Discussion of Question B: Will the Models be Able to Answer the Principal Study Questions as Stated in the PMR?**
1:45PM	**Summary of Discussions on Question B**

WEDNESDAY, SEPTEMBER 9, 1998 (Continued)

2:00PM	**Discussion on Question C1, C2, and C3: Are the Modeling Approaches For Developing Quantitative Relationships Appropriate...**
3:15PM	BREAK
3:30PM	**Discussion continues on Questions C1, C2, and C3**
4:15PM	**Summary of Discussions on Questions C1, C2, and C3**
4:35PM	**Observer Comments**
5:15PM	**Review of Charge for Day Two** *Jan Connery and Alan Maki*
5:30PM	ADJOURN

THURSDAY, SEPTEMBER 10, 1998

8:30AM	**Planning and Logistics** *Jan Connery*
8:40AM	**Discussion on Questions C4 and C5**
9:40AM	**Summary of Discussions on Questions C4 and C5**
10:15AM	BREAK
10:45AM	**Discussion on Question D: Are There Any Changes in the Work Effort That Would Significantly Improve the Outcome?**
11:15AM	**Summary of Discussions on Question D**
11:45PM	LUNCH
1:00PM	**Discussion on Question E**
2:00PM	**Summary of Discussions on Question E**
2:30PM	**Observer Comments**
3:15PM	**Recommendations and Chair's Summary** *Jan Connery and Alan Maki*
4:15PM	**Closing Remarks**
4:30PM	ADJOURN

APPENDIX F

SUMMARIES OF OBSERVERS' COMMENTS

List of Observers Making Comments

Day #1 (September 9, 1998):

David Adams, Saratoga County Environmental Management Council
Leigh Foster, Arbor Hill Environmental Justice Corporation
Wilbert Lick, University of California at Santa Barbara
Kirk Ziegler, Quantitative Environmental Analysis
David Glaser, Quantitative Environmental Analysis
Jim Rhea, Quantitative Environmental Analysis
John Connolly, Quantitative Environmental Analysis

Day #2 (September 10, 1998):

George Hodgson, Saratoga County Environmental Management Council
Wilbert Lick, University of California at Santa Barbara
Kirk Ziegler, Quantitative Environmental Analysis
David Glaser, Quantitative Environmental Analysis
John Connolly, Quantitative Environmental Analysis
Robert Henshaw, Hudson River Environmental Society
Leigh Foster, Arbor Hill Environmental Justice Corporation

The remainder of this Appendix summarizes the comments made by the observers listed above. Comments are summarized in the order in which they were presented.

Appendix F—Summaries of Observers' Comments

Day #1, Comments from David Adams, Saratoga County Environmental Management Council

Mr. Adams' comments addressed both the peer review process and technical aspects of the EPA modeling approach. Regarding the peer review process, Mr. Adams noted that independent peer review is important to help concerned citizens of the Upper Hudson River area understand why EPA and GE may make different interpretations on technical issues for this site. Mr. Adams thanked the peer reviewers for their input, which ultimately may help reconcile the different interpretations. Mr. Adams then offered several recommendations for implementing future peer reviews. First, for the sake of improving the peer review process, Mr. Adams suggested that the peer reviewers be given more time and information to conduct thorough reviews. He also recommended that the peer review report document this recommendation. Second, noting that some peer reviewers thought it would have been more useful to hear EPA's overview of the site history at the beginning of the peer review instead of at the peer review meeting, Mr. Adams suggested that future peer reviews include "orientation" presentations to give peer reviewers a better basis on which they can conduct their reviews. Third, Mr. Adams recommended that EPA publish a response to the comments raised in the current peer review, even though a project schedule distributed at the meeting did not explicitly state that a response would be prepared. Finally, recognizing the experience gained by the peer reviewers during the current meeting, Mr. Adams recommended that ERG consider selecting some of the same peer reviewers for future peer reviews.

Regarding the technical aspects of the EPA modeling approach, Mr. Adams was concerned that the models may not be able to simulate river conditions during the Allen Mill incident of 1991, which Mr. Adams called a "discontinuity" in the source of PCBs to the Upper Hudson River. More specifically, Mr. Adams thought it was not clear whether EPA's models can forecast through this "discontinuity" of PCB sources to the river. Mr. Adams thought the modeling approach should attempt to forecast river conditions before the 1991 incident separately from forecasting river conditions following the 1991 incident. He thought the peer review report should address this specific concern regarding the modeling approach. Finally, Mr. Adams noted that the peer reviewers had addressed most of his other questions and comments, such as the need for modeling individual PCB congeners in addition to modeling total PCBs.

Day #1, Comments from Leigh Foster, Arbor Hill Environmental Justice Corporation

Mr. Foster's comments addressed the peer review process and the relevance of the ongoing reassessment studies to potentially exposed populations, particularly communities of color in Albany and in other parts of the Hudson Valley. Noting that these communities are already affected by lead exposure, Mr. Foster indicated that human health risks from PCBs presents "yet another obstacle" that communities of color in this area (especially subsistence fishers and residential communities along the river) must overcome.

Appendix F—Summaries of Observers' Comments

In reference to the peer review process, Mr. Foster suggested that, by assembling peer review teams that are just as dynamic and diverse as the Hudson River community, ERG and EPA can ensure that the perspectives and opinions provided by the reviewers would be equally dynamic and diverse. Mr. Foster believed that these diverse opinions would ultimately help the modeling efforts. More specifically, Mr. Foster recommended that the review teams should have better community representation by including community experts and community "systems dynamics experts." Mr. Foster noted that many of these experts work in academia (including at the University of California at Berkeley and at the Massachusetts Institute of Technology) in the fields of human health risk assessment, human health risk monitoring, and modeling. He indicated that some of these experts have already worked with the Arbor Hill community. Finally, citing an analogy to his experiences with designing parks, Mr. Foster emphasized the importance of assembling diverse teams for projects of local interest, including people with "native knowledge" of the resource being considered (i.e., the Hudson River). Mr. Foster mentioned that he was pleased that one of the reviewers was from Cornell and therefore might have "native knowledge" of the Hudson River system.

Regarding the ultimate impact of the site reassessment on potentially exposed populations, Mr. Foster made a series of comments, observations, and suggestions. For example, he expressed concern that the reviewers did not discuss whether human health studies would be incorporated in the modeling, especially considering that an ultimate goal of the modeling efforts is to evaluate human health concerns. Mr. Foster mentioned that he has recently attended public health meetings at which other agencies have been trying to determine both short-term and long-term human health impacts. He indicated that it would be useful if the current modeling could determine and document the cycles over which human health impacts might occur. Further, he thought that the ongoing site reassessment should include "short-term human health risk mitigation" in addition to the current modeling effort, which focuses on the long-term impacts of remediation strategies, such as "re-engineering" the Hudson River. Mr. Foster emphasized as a key issue that risk mitigation is cost-effective and could increase the quality of life for residents who live along the Hudson River right now, while the modeling efforts only address future scenarios. Mr. Foster did acknowledge that EPA and the State of New York have issued human health risk advisories for some river communities, but he noted that these advisories have "failed miserably." The failure of these advisories, according to Mr. Foster, has had the greatest impact on communities in dire need of information: communities of color, communities of non-native English speakers, and communities of subsistence fishers who depend on the Hudson River as a source of nutrition. Noting that it is his job to communicate and articulate health risks and technical information to these communities, Mr. Foster indicated that the community members will be best served right now if the peer reviewers communicate their ideas, findings, reservations, and enthusiasm regarding human health issues.

Mr. Foster closed his comments by mentioning specific concerns expressed by community members. First, Mr. Foster informed the peer reviewers that residents have expressed great concerns over how flooding events might affect their health, especially since floods have occurred in the past. More specifically, Mr. Foster wondered what effects might result when basements are flooded with water containing sequestered PCBs. Mr. Foster noted that residents may already understand dangers posed by radon in their homes, but they generally do not understand dangers posed by PCBs. Mr. Foster also mentioned that he had been asked if there was some way to incorporate more residential studies in the ongoing study of the river system; he said these might, for example, consider the residential areas that will "experience sequestration of PCBs" during flood, or evaluate the impacts of PCBs on older housing in the city of Troy. Mr. Foster expressed gratitude that the reviewers completed such a thorough evaluation of the modeling approach, but noted that a lot of the peer reviewers' comments were critical. He hoped that EPA will ultimately use these critical comments to improve the modeling approach. Finally, Mr. Foster indicated that he felt "informed and somewhat empowered" by the information the peer reviewers had provided.

Day #1, Comments from Wilbert Lick, University of California at Santa Barbara

Mr. Lick's comments addressed technical aspects of EPA's plan to model sediment resuspension and PCB flux from the sediment to the water. Before discussing these topics, Mr. Lick first mentioned that he found the peer review discussions both informative and interesting. Mr. Lick then indicated that, to predict PCB transport and fate, one must first understand sediment transport, PCB partitioning, and flux of PCBs from sediment to water. Mr. Lick further noted that resuspension, deposition, bioturbation, and diffusion all affect this flux of PCBs.

Regarding how to model sediment resuspension, Mr. Lick indicated that sediment resuspension properties (which are dependent on sediment particle size, mineralogy, sediment bulk density, and gas bubbles resulting from decay of organic material) vary by orders of magnitude through river systems. Mr. Lick said he did not know how to predict these parameters, but he noted that sediment erosion rates can be measured at the surface, as a function of depth, and also as a function of shear stress. By coupling measured erosion rates with a "reasonably decent" hydrodynamic model and sediment transport model, Mr. Lick indicated that a modeling approach can predict sediment fate and transport well both during 100-year flood events and during low-flow periods. Mr. Lick suggested that the models described in EPA's PMCR are currently insufficient for characterizing both types of flow events. To put sediment resuspension into context, Mr. Lick noted that significant transport of PCBs and sediments occurs during high-flow events in almost every other river he knows of. As an example, Mr. Lick mentioned that data for the Fox River, another river with locks and dams, indicate that over 80 percent of sediment transport "in a moderate year" occurs during less than 20 percent of that time.

Regarding how to model the flux of PCBs from sediment to water, Mr. Lick noted that EPA's models approximate this flux using the concept of an "active layer" or a "mixed layer" of sediments, with a thickness that EPA arbitrarily set at 5 centimeters (cm). Mr. Lick indicated that the sediment transport algorithms in EPA's models are quite sensitive to the thickness of the active layer. For example, Mr. Lick noted the model would predict that decay of PCBs in the Hudson River would take twice as long if the active layer thickness were assumed to be 10 cm (instead of 5 cm); similarly, he noted the model would predict that decay of PCBs would take half as long if the active layer thickness were 2.5 cm (again, instead of 5 cm). Based on these observations, Mr. Lick indicated that the active layer thickness parameter is "absolutely crucial" for long-term prediction of the models. Mr. Lick also commented that EPA's report did not really explain how the active layer thickness of 5 cm was selected, except "on the basis of some judgment." Mr. Lick recommended developing and using a model that better characterizes the flux of PCBs from the sediment to the water by considering sediment resuspension and deposition, bioturbation, and diffusion as separate physical processes. Mr. Lick then noted that such an improved model could better answer questions regarding the effects of the Allen Mill discharge. More specifically, he estimated that the discharge probably sent millimeters of sediments throughout the Thompson Island Pool, and he suggested that EPA's models would not be able to characterize this discharge by assuming a sediment active layer thickness of 5 cm. Mr. Lick indicated that "a more detailed sediment-water flux calculation" would be needed to model this event. Finally, Mr. Lick commented that the models need to "get the partitioning right" and he noted that partitioning kinetics (the time over which absorption and desorption occurs) are an important factor for modeling the flux of PCBs. Mr. Lick indicated some of his experiments have found that highly chlorinated PCBs have partitioning rates on the scale of months or years.

Day #1, Comments from Kirk Ziegler, Quantitative Environmental Analysis (QEA)

Mr. Ziegler's comments focused on how he thought EPA's modeling approach, specifically the HUDTOX model, should treat sediment resuspension and deposition processes. Mr. Ziegler began his comments by indicating that he thought the best approach to modeling these processes in the Hudson River, and the approach that EPA should adopt, is to use mechanistic models. Mr. Ziegler then noted that EPA chose to use an empirical model (i.e., HUDTOX) to describe the sediment resuspension and deposition processes, an approach he does not recommend. Mr. Ziegler mentioned that "several important, critical processes" that affect sediment resuspension and deposition need to be incorporated into the empirical model, in the event that EPA chooses not to adopt a mechanistic approach.

After reiterating that using mechanistic models is the best approach to characterize sediment resuspension and deposition processes in the Hudson River, Mr. Ziegler clarified that a mechanistic model would include formulations that describe sediment resuspension and deposition that are based on experiments conducted both in the lab and in the field. Mr. Ziegler indicated that site-specific data should then be used to specify parameters used in the mechanistic formulations. Mr. Ziegler noted that the data, science, and models are available for developing and applying mechanistic models to the Upper Hudson River, just as mechanistic models have been developed for other rivers. Mr. Ziegler acknowledged that EPA has attempted to incorporate some mechanistic formulations into its proposed 100-year flood model, but he noted that EPA proposes using an empirical approach to modeling sediment transport in the HUDTOX model—the model that Mr. Ziegler thought was "key" to predicting long-term fate of PCBs in the river system. As he understands it, Mr. Ziegler said, EPA will (1) develop empirical functions relating sediment resuspension and deposition rates to local shear stress and other properties, (2) use data collected during a 1994 spring flood to calibrate the empirical functions, and (3) use data collected from other flood events to validate the empirical functions. Mr. Ziegler found such a modeling approach to be "very problematical" and thought it should not be used.

Mr. Ziegler recommended that the modeling approach at least recognize and incorporate four different processes when calculating sediment resuspension and deposition velocities, even if EPA cannot avoid using its proposed empirical modeling approach. First, Mr. Ziegler thought the modeling should account for "bed armoring," which he defined as the process by which the amount of sediment that can be eroded is limited at a particular shear stress. Mr. Ziegler indicated that various bed properties, such as particle size heterogeneity and the cohesiveness of sediments, affect the extent of bed armoring. Mr. Ziegler noted that bed armoring is important because it can create discontinuities in the sediment resuspension velocity functions, which, if not accounted for by a model, can lead to incorrect predictions of sediment erosion during a flood. Second, Mr. Ziegler thought EPA's models should better address sediment resuspension during periods of low flow. More specifically, he noted that his previous modeling analyses, laboratory work, and field work have all found that resuspension from sediment beds in the Hudson River is negligible during periods of low to moderate flow, or during periods with flow rates less than 4,000 cubic feet per second. Based on these findings, Mr. Ziegler suggested that PCB fate and transport modeling does not need to consider resuspension during periods of low flow and that EPA's calculated sediment resuspension velocities should reflect the results of his low-flow studies. Third, Mr. Ziegler recommended that EPA's modeling approach should address the time history of bed properties. If models do not consider temporal variations in "erosional characteristics" of sediment beds, Mr. Ziegler noted that the calculated resuspension velocities, and the modeling results in general, would have "tremendous amounts of uncertainty." Finally, Mr. Ziegler expressed concern over the issue of spatial and temporal variability in the composition of clay, silt, and sand in suspended sediments in the water column. Mr. Ziegler indicated that these "column composition effects" cause complex changes in sediment deposition rates at different locations in the Hudson River. Further, he noted that modeling approaches that neglect these effects, such as EPA's proposed modeling approach, will create an "unrealistic and inaccurate" representation of sediment deposition in river systems.

Day #1, Comments from David Glaser, Quantitative Environmental Analysis (QEA)

Mr. Glaser's comments addressed the bioaccumulation models that EPA developed. Prior to critiquing these models, Mr. Glaser first distributed a written summary of his comments to the peer reviewers. Mr. Glaser then identified the two functions of the bioaccumulation models: (1) "to estimate the average concentration of PCBs" in fish and (2) "to estimate the variability" in the average concentrations. Before addressing these functions in detail, Mr. Glaser presented his major findings. First, concerning average concentrations, Mr. Glaser strongly agreed with the sentiments of the reviewers that a "mechanistic, time-variable model" needs to be used to predict levels of PCBs in fish. He further noted that QEA has developed such a model for GE and has made the model available to EPA. Regarding estimates of variability, Mr. Glaser found "severe problems" with the probabilistic food chain model that EPA has developed. Namely, Mr. Glaser noted that EPA's model is static, is based upon the variability of the data, and is validated against this same data. As an alternative, Mr. Glaser suggested that EPA simply use the available data to estimate the variability of PCB concentrations in fish. Mr. Glaser indicated that there are plenty of data available for several species of fish to estimate variability in the concentrations and to estimate the uncertainty associated with this variability. For three particular species of fish (pumpkinseed, brown bullhead, and largemouth bass), Mr. Glaser noted that fish concentration data are available for three monitoring locations over a 20-year period, with many of the sampling events measuring concentrations in roughly 20 fish.

Commenting in detail on the use of bioaccumulation models to estimate average concentrations of PCBs in fish, Mr. Glaser first mentioned that he would not describe why he thought a mechanistic, time-variable model is needed, largely because he sensed "general agreement" among the peer reviewers on this recommendation. Mr. Glaser instead described in detail the bioaccumulation model developed by QEA, which he called a "full life cycle, full food web, bioenergetic based, toxicokinetic model." Mr. Glaser noted that this model incorporates time-dependent lipid contents—a factor that one of the reviewers thought should be included in such models. He further noted that considering time-varying lipid contents was a "key component" in his modeling analysis and that models assuming constant lipid contents find very different results than models allowing lipid contents to vary with time. As an example of the importance of considering time-dependent lipid contents, Mr. Glaser mentioned that the lipid content of largemouth bass can vary by an order of magnitude from year to year. On the issue of interfacing the bioaccumulation model with other models, Mr. Glaser indicated that the QEA model links directly with the sediment and water concentrations output by a time-variable fate and transport model. Mr. Glaser then noted that the QEA bioaccumulation model accounts for several other factors that the peer reviewers found important. As an example, Mr. Glaser indicated that the model accounts for feeding rates that are seasonal (or temperature-dependent). Referring to a comment made earlier by EPA concerning the lack of available data, Mr. Glaser mentioned that "a tremendous amount of information" (e.g., species-specific bioenergetic information; site-specific, species-specific growth rates; and 20 years of toxicokinetic experimentation) are available from the scientific literature to parameterize bioaccumulation models. Mr. Glaser then indicated that QEA's time-varying, mechanistic model has been used to identify the key uncertainties in the Hudson River system, such as the incomplete understanding of the food web in the Thompson Island Pool. Mr. Glaser noted that his modeling analyses have already prompted a field study to characterize the food web in this area. Noting other aspects of QEA's model, Mr. Glaser mentioned that QEA's bioaccumulation model has been extended to account for different PCB homologues—an improvement that is "just one step away" from modeling individual PCB congeners. Further, Mr. Glaser noted that the model has helped QEA better understand the role of the "active layer" or "the bioavailable layer" within sediments. Finally, Mr. Glaser said QEA's bioaccumulation model, including all information used in the model's development, is available to EPA for review and use.

Before discussing his second major topic (using models to predict variability), Mr. Glaser clarified a comment made by a peer reviewer during the meeting regarding the distribution of PCB homologues, particularly BZ4, in the Hudson River sediments. Mr. Glaser explained that the elevated concentrations of BZ4 in some of the sediment samples likely result from the dechlorination of PCBs that is known to occur in sediments, particularly at depth. Thus, Mr. Glaser believed that reports of relatively high concentrations of BZ4 (which he noted was a terminal dechlorination product) are probably not the result of sampling or analytical errors. Further elaborating, Mr. Glaser mentioned that BZ4 bioaccumulates "very little," thus causing "very little" BZ4 to be found in fish. Referring the peer reviewers to the package of comments he distributed at the beginning of his presentation, Mr. Glaser noted that he has used observations of dechlorination to understand the "bioavailable depth" of sediments. For example, knowing that dechlorinated PCBs are most prevalent in sediments at depth and "relatively fresh" PCBs are more prevalent in surface sediments, Mr. Glaser mentioned that he could use his model "in a diagnostic fashion" to understand to which sources of PCBs fish are most likely exposed. Summarizing the results of his modeling analyses, Mr. Glaser reported that fish appear to be exposed to a "relatively un-dechlorinated source," which would not be the buried PCBs.

Regarding estimates of variability in the modeled concentrations of PCBs in fish, Mr. Glaser first stated that EPA's probabilistic food chain model confuses variability and uncertainty. Mr. Glaser defined variability as "the true variance in the population concentration of PCBs" and uncertainty as "what we know about the concentration of PCBs in fish." To give evidence of his claim, Mr. Glaser noted that EPA's model uses measured concentrations of PCBs in invertebrates to estimate the relationship between (1) PCB concentrations in the sediment and (2) PCB concentrations in invertebrates. Mr. Glaser disapproved of this method of deriving bioaccumulation factors because the available data include "several important sources of uncertainty," in addition to data variability. Most importantly, Mr. Glaser indicated that sediment concentrations of PCBs vary by orders of magnitude, even over short distances. Without knowing exactly what sediments invertebrates eat, Mr. Glaser contended that one cannot determine the right bioaccumulation factor, thus introducing a "very big" source of uncertainty in EPA's bioaccumulation model. (Mr. Glaser acknowledged that it is "nearly impossible" to collect and analyze the exact sediments that invertebrates eat.) With this uncertainty in the estimated bioaccumulation factors, Mr. Glaser noted that EPA's model, which is designed to calculate variability, actually calculates a mixture of uncertainty and variability. Mr. Glaser indicated that this "mixture" of terms has no physical meaning. As a second issue, Mr. Glaser said EPA "used the answer to solve the problem." As an example, Mr. Glaser noted that EPA used variability in the predator data to estimate variability in the trophic transfer from forage fish to predators. Mr. Glaser claimed that this approach was "using the answer to develop the model": variability in the predator data is supposed to be an output of the model, yet EPA used these variability data essentially as an input when parameterizing the model. Finally, agreeing with a comment raised by a peer reviewer, Mr. Glaser mentioned that EPA's proposed model is static and therefore provides no "predictive value." Based on his list of inadequacies for the EPA bioaccumulation model, Mr. Glaser recommended that EPA estimate variability in the concentrations of PCBs in the fish simply by using the large set of available data, rather than by attempting to model the uncertainty with the probabilistic bioaccumulation model.

Day #1, Comments from Jim Rhea, Quantitative Environmental Analysis (QEA)

Mr. Rhea began by commending the peer reviewers for their work, especially considering the limited time they had to review the PMCR. Mr. Rhea noted that many scientists at the meeting have spent nearly 10 years working on the Upper Hudson River project. Mr. Rhea also acknowledged that the reviewers were "handcuffed" by reviewing a 2-year old report, particularly because he thought many advances have since been made in understanding the Hudson River data. Mr. Rhea then described four areas in which understanding of the river system has evolved since the PMCR was published.

First, Mr. Rhea responded to concerns raised by the peer reviewers regarding how to interpret historical data collected in the Hudson River. Noting that the river models "are only as good as the data upon which" the models are calibrated, Mr. Rhea mentioned that GE and EPA have both emphasized in their work the importance of understanding what the available data actually mean. More specifically, Mr. Rhea noted that both GE and EPA have carefully analyzed and interpreted nearly 20 years of fish, sediment, and water column data that were collected by different organizations using different analytical techniques. As a result, Mr. Rhea claimed that scientists from GE and EPA now "have a firm handle" on the significance of the different analytical techniques. Otherwise stated, Mr. Rhea indicated that both GE and EPA have a much better understanding of exactly what historical monitoring data actually represent.

Second, Mr. Rhea addressed the "excess load" of PCBs in the Thompson Island Pool, which he described as a "key issue" of the PMCR and which also was an issue of discussion among the peer reviewers. Mr. Rhea mentioned that GE conducted an extensive study in 1997 to understand why the models predicted, yet could not explain, the excess load in this section of the Hudson River. Mr. Rhea indicated that QEA used its models in "a diagnostic fashion" to resolve this issue. Mr. Rhea reported that the modeling analyses found a bias at a downstream sampling station accounted for "a majority of the excessive load." Mr. Rhea further noted that the PCB loading leaving the Thompson Island Dam, from what he called an "un-biased station," is consistent with diffusive transport mechanisms and a sediment-water exchange rate of approximately 2 centimeters per day. Mr. Rhea indicated that this exchange rate is consistent with, though possibly a little higher than, exchange rates observed in other river systems.

Third, addressing reviewers' discussions of groundwater advection, Mr. Rhea described the findings of a recent field study that addressed this issue. He noted that "seepage meter measurements" and piezometer measurements were conducted to measure seepage rates and hydraulic gradients during the spring of 1997. He further indicated that this field study found that groundwater advection was an "insignificant process" that "was not contributing much PCBs to the water column" during the spring flow conditions.

Fourth, Mr. Rhea commented on EPA's use of a limited data set to calibrate the HUDTOX model. More specifically, Mr. Rhea noted that EPA used data collected from a 9-month period in 1993 to calibrate the model—a time that followed a 2-year period of elevated loadings of PCBs to the Hudson River system in the form of dense non-aqueous phase liquids. Because he thought these elevated loadings were not representative of the Hudson River at "steady state," Mr. Rhea cautioned against calibrating the HUDTOX model data from this limited time period and then projecting these limited calibration results to longer time periods. As an alternative, Mr. Rhea recommended that EPA use more historical data, "from an extended period of time," to calibrate its models in order to avoid having the model results biased by a limited calibration period.

Day #1, Comments from John Connolly, Quantitative Environmental Analysis (QEA)

Mr. Connolly's comments primarily addressed two issues: (1) the calibration and validation of EPA's models and (2) the location of the upstream boundary for the HUDTOX model. Regarding the calibration and validation of the models, Mr. Connolly presented numerous comments and suggestions. Mr. Connolly first noted that "a wealth of data" are available for calibrating the Hudson River models: water column data for 20 years; fish data for 20 years; "major collections" of sediment data from the late 1970s, the mid-1980s, the early 1990s, and the mid-1990s. Mr. Connolly then presented what he considered to be "key comparisons" that he thought should be made between the available data and the Hudson River models. Referring to an observer's comments made earlier, Mr. Connolly noted that the validity of EPA's modeling results will depend largely on the successful calibration of sediment transport mechanisms. Mr. Connolly mentioned that EPA currently proposes to calibrate its empirical fate and transport model (HUDTOX) by "essentially fitting a sediment transport model to suspended solids data in the water column." As summarized below, Mr. Connolly then described in detail how he proposes EPA should calibrate its models.

In modeling the fate and transport of suspended solids, Mr. Connolly indicated the only parameter "that matters" is the net transport of solids between the sediment and the water. More specifically, in terms of suspended solids, Mr. Connolly suggested that absolute sediment resuspension rates and absolute sediment deposition rates "are irrelevant," provided the difference between these rates (i.e., the net transport rate) is accurate. If the estimated net transport rate of sediments is indeed accurate, he indicated the models will predict "the right amount of material" transferring between the two phases and will predict the correct total suspended solid concentrations. Noting that suspended solids can originate both from the river bed and from the watershed, Mr. Connolly also mentioned that the "amount of solids loading" in the water can affect model calibration of suspended solids data: if the model is not calibrated using correct loadings from the watershed, then the model will not predict the correct net resuspension rates. Mr. Connolly reiterated that, even assuming the models have valid solids loading data from the watershed, model calibration will only determine the "net flux" of solids from the sediments. Mr. Connolly then summarized that calibration of the model during "the 1994 flood event" will only generate the "net transport of sediment" from the river bottom into the water. Mr. Connolly emphasized that knowledge of the net transport of sediment is sufficient for modeling the transport of solids, but is insufficient for modeling the transport of PCBs.

Mr. Connolly stressed that, when modeling transport of PCBs, the absolute sediment resuspension rates and the absolute sediment deposition rates are critical parameters, because PCBs enter the water through resuspension of contaminated sediments. Mr. Connolly then suggested that one can determine whether the estimated absolute transfer rates are correct by computing the amounts of PCBs in the water column during high-flow events. He explained that high-flow events are characterized by "clean solids" entering the modeling region from the watershed and "contaminated solids" entering the modeling region through sediment resuspension. By combining these two factors, Mr. Connolly noted that the models can predict the amounts of PCBs in the water column. Mr. Connolly then suggested that the Hudson River database includes observations from "a multitude of high-flow events" over the last 20 years. More specifically, Mr. Connolly cited events with flows peaking at 34,000 cubic feet per second (e.g., for high flows in 1983 and in 1998) and other events with flows peaking at slightly lower levels. Noting that PCB and total suspended solid data exist for several of these high-flow events, Mr. Connolly emphasized the importance of challenging EPA's models with the data sets that characterize these periods of high flow.

Citing another source of data that can be used to validate the models, Mr. Connolly noted that EPA's 1992 high resolution coring samples with cesium dating could be used to assess sediment resuspension and deposition. He explained that these cesium-dated samples provide estimates of long-term deposition rates, or "how many centimeters per year of sediment are accumulating." He then suggested that EPA compare what its models predict for long-term deposition rates at selected locations to what the high resolution coring samples suggest as long-term deposition rates. Mr. Connolly thought it was important that such comparisons be made.

For his final point related to data calibration and validation, Mr. Connolly addressed sediment transport during low-flow conditions. He first described the mechanisms that affect sediment transport during low flow, such as "diffusion out of the pore water," "bioturbation effects," and "advection between the sediment and the water column." Mr. Connolly suggested that EPA's fate and transport models would not be able to treat these mechanisms separately, but would rather combine these mechanisms into a single term that would be quantified by model calibration. Mr. Connolly referred to this single term, which characterizes the amounts of PCBs leaving the sediment during low-flow conditions, as "a free knob" in EPA's models. Mr. Connolly then noted that upstream and downstream weekly water column monitoring data are available from 1991 to the present, and additional (but less frequent) monitoring data are available dating back to the 1970s. Mr. Connolly considered it important that EPA calibrate its models against the available water column monitoring data during low-flow conditions, including those from different seasons. Mr. Connolly suggested that this use of the historical water column data is "the only way" that EPA can validate both the "flux rates from sediment to the water" and the seasonal variations in these flux rates.

Addressing his second major issue (i.e., the upstream boundary for EPA's modeling approach), Mr. Connolly asked the peer reviewers to reconsider their recommendation that EPA move the upstream boundary of its models from Rogers Island to a location "above GE's Hudson Falls plant." Noting the extensive monitoring data available at Rogers Island from 1977 to 1998, Mr. Connolly suggested that Rogers Island is a "convenient place" for the upstream boundary of EPA's model because the monitoring data adequately characterize the flux of PCBs into the Thompson Island Pool. Mr. Connolly then indicated that moving the modeling boundary further upstream would necessitate knowing the load of PCBs entering the Hudson River from GE's plant. Without knowing exactly what this load is, Mr. Connolly suspected that modelers would have to "twist knobs" so that the model would reproduce the large set of monitoring data available for Rogers Island. This approach, according to Mr. Connolly, would not provide any greater insight into the system than would leaving the upstream boundary of the model at Rogers Island. Mr. Connolly further noted that it would be especially difficult to model future conditions with an upstream boundary above the Hudson Falls plant, particularly because no one knows the current PCB loadings from the plant site or how these loadings might change in the future. Mr. Connolly therefore recommended leaving the upstream boundary of the model unchanged and modeling different scenarios of future PCB loadings into the river. He provided several examples of loading scenarios: the PCB loading remains constant at its present value, the PCB loading steadily decreases at the rate it has been decreasing, or "GE successfully eliminates all loading" so that loading decreases to atmospheric levels. In conclusion, Mr. Connolly suggested that "we do not gain much from moving the model upstream," and he recommended again that the peer reviewers reconsider this issue.

Day #2, Comments from George Hodgson, Saratoga County Environmental Management Council

Mr. Hodgson began by introducing himself as the Director of the Saratoga County Environmental Management Council (EMC). Mr. Hodgson then explained that EMCs are "environmental citizen advisory groups," and he noted that the EMC he directs was established by the Saratoga County Board of Supervisors. Citing the level of concern among residents regarding the water quality of the Hudson River, which he noted forms two borders of Saratoga County, Mr. Hodgson mentioned that the Saratoga County EMC has been, and continues to be, "actively involved in commenting on" EPA's assessments of PCBs in the river. Mr. Hodgson noted that David Adams, who presented comments on behalf of the Saratoga County EMC on the first day of the peer review meeting, could not attend the second day of the meeting. Mr. Hodgson then presented three observations or comments regarding the peer review.

First, Mr. Hodgson noted that the Saratoga County EMC has expressed concern "several times in the past" that EPA conducted its data acquisition program before developing its models. Explaining his concerns, Mr. Hodgson wondered if the data that EPA collected is adequate for running the models and if EPA developed its models simply to "fit the database." Second, echoing concerns raised by some of the peer reviewers, Mr. Hodgson thought "a lack of adequate information" is currently available to make judgments. Mr. Hodgson noted that the Saratoga County EMC stresses that all "pertinent information" should be made available so that "a comprehensive peer review" can be conducted for this site. Mr. Hodgson indicated that the EMC has recommended a comprehensive peer review in the past. He explained that a comprehensive peer review would consider public comments, data that GE has generated, and information provided by EPA. Third, Mr. Hodgson had several comments regarding the notes the meeting chair wrote on an easel located at the front of the meeting room. Mr. Hodgson found the notes difficult to read, especially from the back of the meeting room, and he thought ERG should make these notes available to the public "in some form." Mr. Hodgson ended his comments by commending the peer reviewers on their efforts, especially considering that the Saratoga County EMC had "felt very strongly" about conducting periodic peer reviews of the site reassessment. Mr. Hodgson concluded by indicating that he was hopeful that the current peer review was conducted in part to address the previous recommendations made by the Saratoga County EMC.

Day #2, Comments from Wilbert Lick, University of California at Santa Barbara

Mr. Lick's comments addressed the adequacy of EPA's proposed models and whether any of the models are "fatally flawed." Mr. Lick began by mentioning that one should consider the ultimate use of models when evaluating their adequacy. He then noted that the models for the Hudson River project are intended to address general questions regarding concentrations of PCBs in fish. Moreover, he commented, the models have to provide insight into the impacts of performing different remedial actions: how would concentrations of PCBs in fish change if no remedial actions are performed? if depositional areas are capped? if erosional areas are capped with heavy material? if erosional areas are dredged? Mr. Lick then noted that rivers generally have some depositional areas, some erosional areas, and some areas that are depositional during low flows and erosional during high flows. To predict fate and transport in such rivers, Mr. Lick thought, models need to identify and characterize depositional and erosional areas as functions of space, time, and flow. After commenting that models that cannot characterize sediment deposition to such an extent are inadequate, Mr. Lick claimed EPA's sediment transport models, as described in the PMCR reports, are not adequate and cannot "help in deciding appropriate remedial action."

Mr. Lick noted that many alternative models are available that are adequate by his standards. He noted further that QEA and some of his students have developed models—some of which were developed for other rivers—that adequately address sediment transport. He also indicated that EPA funded the development of some of these models. Finally, Mr. Lick mentioned that "it is no secret" that adequate models are available and that they can help users decide how to take appropriate remedial actions. Mr. Lick concluded his comments by recommending that EPA use these more adequate models in its reassessment of PCBs in the Hudson River.

Day #2, Comments from Kirk Ziegler, Quantitative Environmental Analysis (QEA)

Mr. Ziegler's comments focused on sediment transport modeling. Mr. Ziegler began by agreeing with a comment raised by a peer reviewer regarding resuspension and deposition of non-cohesive sediments. In particular, Mr. Ziegler indicated that non-cohesive sediment transport needs to be included in fate and transport modeling, particularly for accurately modeling sediments in the Thompson Island Pool. He then offered several recommendations to EPA for improving its sediment transport models. First, Mr. Ziegler thought a mechanistic model is needed to "more realistically describe resuspension and deposition processes." Mr. Ziegler acknowledged that EPA has incorporated mechanistic algorithms in its 100-year flood model, but he thought the agency should also "include a more mechanistic approach" in the HUDTOX model. As part of adopting this mechanistic approach, Mr. Ziegler recommended developing a "fine-resolution, two-dimensional sediment transport model" that accounts for how shear stresses, bed properties, and bed types (i.e., non-cohesive and cohesive sediments) vary from one location in the river to another. Mr. Ziegler indicated that such a fine-grid model should be used to calculate time-dependent sediment resuspension and deposition fluxes, which can then be aggregated to the scale of a coarse-grid, one-dimensional fate and transport model, like HUDTOX. In review, Mr. Ziegler stated that outputs from a fine-grid sediment transport model can and should be used as inputs to a coarse grid fate and transport model. Mr. Ziegler then emphasized that developing the modeling approach he just described is not "an insurmountable technical issue," and he noted that QEA has already incorporated his recommendations in a Hudson River sediment transport model for GE. Mr. Ziegler concluded by indicating that QEA's two-dimensional sediment transport model, coupled with its one-dimensional fate and transport model, has enabled him to conduct "relatively long-term simulations of PCB fate and transport in the river" with increased confidence.

Day #2, Comments from David Glaser, Quantitative Environmental Analysis (QEA)

Mr. Glaser's offered brief comments and recommendations regarding bioaccumulation models. First, Mr. Glaser "very strongly" suggested that EPA use a time-variable, mechanistic bioaccumulation model. Second, Mr. Glaser noted that QEA has already developed such a model. Finally, knowing the amount of effort needed to develop this kind of model, Mr. Glaser suggested that EPA not start to develop its own bioaccumulation model, which he suggested would be "re-inventing the wheel to a certain degree." Rather, Mr. Glaser recommended that EPA perform "a critical, in-depth review" of QEA's model with the ultimate intent of EPA using the model to evaluate bioaccumulation in the Hudson River.

Day #2, Comments from John Connolly, Quantitative Environmental Analysis (QEA)

Mr. Connolly began by reiterating the main recommendation proposed earlier in the observer comment period by his colleagues: that EPA develop a mechanistic approach to model both sediment transport and bioaccumulation. If EPA incorporates these changes, Mr. Connolly noted, the Agency's "modeling framework" will be sufficient to answer questions regarding remedial actions. However, regardless of the validity of the algorithms programmed into the modeling framework, Mr. Connolly warned, the model may not be accurate if it is not correctly parameterized. Mr. Connolly suggested the correct parameterization ultimately depends on the extent to which the models are calibrated and validated. Finding it unfortunate that the peer reviewers did not have the opportunity to appreciate "the full scope of information" available for model calibration, Mr. Connolly indicated that he would provide an overview of the available data and that he would also provide his recommendations to EPA for validating its models.

Regarding the available data, Mr. Connolly referred the peer reviewers to a map of the water quality sampling locations for the Upper Hudson River. (Mr. Connolly noted that this map was included with the written comments that David Glaser of QEA distributed on the first day of the meeting.) In reference to the map, Mr. Connolly specifically noted the locations of the following monitoring stations: (1) the Bakers Falls station, upstream from GE's Hudson Falls plant at river mile 197; (2) the Rogers Island station, upstream of the Thompson Island Pool at river mile 194; (3) the Schuylerville station; (4) the Stillwater station; and (5) the Waterford station. Mr. Connolly also indicated that "more recent data" are available from the Thompson Island Dam. Overall, Mr. Connolly pointed out that data from these stations characterize water quality from locations just upstream of GE to Waterford, which he called "the interface" between the Upper Hudson River and the Lower Hudson River. The placement of these stations, according to Mr. Connolly, provides information on "what goes from the upper river to the lower river." Mr. Connolly commented that such information is relevant to questions the peer reviewers had about how EPA should couple its two fate and transport models.

Given these data sources (including the sources he described during his comments on the first day of the peer review meeting), Mr. Connolly offered several recommendations to EPA for calibrating and validating its models. First, noting that historical sediment data characterize "long-term changes in surface sediment PCBs," Mr. Connolly thought it was "critical" that EPA compare modeling results to measured concentrations from 1977 to the present. Second, he recommended that EPA compare its modeling results over time to the amounts of PCBs that flow from the Upper Hudson River to the Lower Hudson River, as gauged by the monitoring station at Waterford and as estimated by PCB flux calculations conducted by "various organizations." Third, Mr. Connolly recommended that EPA perform "upstream to downstream" comparisons of the modeling results to the monitoring data for the sampling locations he mentioned earlier in his presentation. Mr. Connolly thought these upstream and downstream comparisons should be made during high-flow and low-flow conditions and for both PCBs and total suspended solids. As part of this data validation of spatial variations, Mr. Connolly also suggested, the models should be able to predict the "monstrous" changes in solids loadings observed in the Upper Hudson River. Mr. Connolly mentioned that he would not discuss how important it is that the models be able to predict levels of PCBs and total suspended solids during floods, but said that he had addressed that topic during his comments on the first day of the peer review and would not discuss it further. Fourth, regarding concentrations of PCBs in fish, Mr. Connolly suggested that EPA's models should be compared to two sets of available data: (1) long-term monitoring data for "almost 20 years in two locations" and (2) short-term data from the time following the Allen Mill event. Elaborating on the Allen Mill event, Mr. Connolly indicated that the "pulse" release of PCBs during this event "increased the concentrations in the water column of the system by orders of magnitude" in 1991, and to a lesser extent in 1992. Mr. Connolly suggested that the response of the food web to this "pulse loading" of PCBs is "a real test" for EPA's models. He reiterated this point by claiming that the ability of the models to reproduce the actual response of the food web to the Allen Mill event "is another important challenge" for EPA's models. Concluding his remarks, Mr. Connolly again recommended that EPA conduct the model data comparisons listed above to evaluate how well the models can predict future conditions of the Hudson River.

Day #2, Comments from Robert Henshaw, Hudson River Environmental Society

Mr. Henshaw first indicated that he would rather ask a series of questions than make technical comments. Mr. Henshaw began by synthesizing his understanding of the peer reviewers' discussions: he thought the peer reviewers gave "grudging agreement" that EPA's models are appropriate, but he also noted that the meeting chair wrote "virtually entirely negative comments" and problems on the easel located in the front of the meeting room. Based on these observations, Mr. Henshaw asked the reviewers the following: (1) "Do you think you were invited into the process early enough so that you could be helpful to EPA?" (2) "Did you have enough information available to you?" (3) "Was all of the information that you had adequately up to date so that your judgments will be maximally valuable to EPA?" and (4) "Overall, how good do you think these models will be?" Regarding his last question, Mr. Henshaw noted that the peer reviewers had provided "generally negative" comments on the models, and he asked the peer reviewers to comment on how valuable the models are to EPA or "how optimistically" the reviewers can recommend that EPA use the models in its reassessment.

Appendix F—Summaries of Observers' Comments

Day #2, Comments from Leigh Foster, Arbor Hill Environmental Justice Corporation

Mr. Foster provided several brief comments regarding the content and the format of the peer review. Mr. Foster first commended the peer reviewers "for taking so much time and care" in making recommendations to EPA. Referring to the comments that the meeting chair wrote on an easel and later displayed on the walls of the meeting room, Mr. Foster requested that ERG make the comments publicly available. Mr. Foster noted that having copies of the comments would help him communicate the findings of the peer review to the communities he represents. Regarding the reviewers' findings, Mr. Foster requested that the peer reviewers provide some "comments and recommendations" relevant to issues of concern to the Hudson River communities, even though, he acknowledged, the purpose of the peer review was to critique EPA's specific modeling approach. Moreover, Mr. Foster requested that the peer reviewers assign priorities to their recommendations with the ultimate understanding that the models are to be used to support a human health risk assessment.

Regarding the format of the peer review, Mr. Foster again expressed concern about the composition of the peer review committee. He recommended that EPA review and revise the peer review guidance and procedures that the agency gives to ERG. Mr. Foster was thankful for the enthusiasm, energy, and experience that the peer reviewers brought to this meeting; however, he mentioned that these meetings need to have groups of reviewers that "mirror or reflect" the dynamic nature of the site of concern, which, in this case, is the Hudson River community. Mr. Foster noted that this recommendation applies both to the composition of future peer review panels and to the composition of the much larger "Scientific Committee." Mr. Foster reiterated that the peer review committee clearly does not include representatives of the Hudson River community or of the stakeholders for the Hudson River PCBs site. In summary, Mr. Foster asked that EPA consider his suggestions regarding the peer review process, and he also asked the peer reviewers to make recommendations that reflect his concerns.