HUDSON RIVER PCBs REASSESSMENT RI/FS

RESPONSE TO PEER REVIEW COMMENTS ON THE REVISED BASELINE MODELING REPORT

NOVEMBER 2000

For

U.S. Environmental Protection Agency
Region 2
and
U.S. Army Corps of Engineers
Kansas City District

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UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
REGION 2
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November 30, 2000

To All Interested Parties:

The U.S. Environmental Protection Agency (USEPA) is pleased to release the Response to Peer Review Comments on the Revised Baseline Modeling Report.

On March 27- 28, 2000, USEPA through its contractor, Eastern Research Group, convened a panel of independent scientific experts to conduct a peer review of the January 2000 Revised Baseline Modeling Report (RBMR) and the February 2000 Responsiveness Summary to the Baseline Modeling Report. The RBMR describes USEPA's modeling efforts for the Hudson River PCBs site. This Response to Peer Review Comments describes how USEPA incorporated the peer review comments and presents additional modeling analyses conducted in response to the peer review. If a comment was not incorporated, the Response to Peer Review Comments provides the technical rationale for not doing so.

In addition, the Response to Peer Review comments presents a model validation for 1998 and 1999.

If you need additional information regarding the Response to Peer Review Comments on the Revised Baseline Modeling Report, please contact Ann Rychlenski at 212-637-3672.

Sincerely yours,

Richard L. Caspe, Director
Emergency and Remedial Response Division.

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I. INTRODUCTION

The United States Environmental Protection Agency (USEPA) has prepared this Response to Peer Review Comments on the Revised Baseline Modeling Report (RBMR) for the Hudson River PCBs Site Reassessment Remedial Investigation/Feasibility Study (Reassessment). It addresses major comments made during the peer review of the Revised Baseline Modeling Report that occurred between January and March 2000. The discussions during the peer review are summarized in Chapter 5 of the Report on the Peer Review of the Revised Baseline Modeling Report for the Hudson River PCBs Superfund Site, May 2000, which is reproduced in full after the responses to comments. The peer review of the Revised Baseline Modeling Report, was the third of five separate peer reviews conducted for the Reassessment. It was intended to ensure that the USEPA science used in the Reassessment and embodied in the Revised Baseline Modeling Report and the associated Responsiveness Summary (Responsiveness Summary for the Baseline Modeling Report, February 2000) is technically adequate, competently performed, properly documented, and satisfies established quality requirements.

The RBMR, the BMR, and the Responsiveness Summary for the BMR are incorporated by reference and are not reproduced herein. The Revised Baseline Modeling Report will not be revised in response to the peer review comments. The comment responses and revisions noted herein are considered to amend the RBMR. For complete coverage, the RBMR and this Response to Peer Review Comments must be used together.

Details regarding the peer review for the RBMR are included in the text of the Report on the Peer Review, which has been reproduced in this Response to Peer Review Comments. Therefore, please see Section V, excerpts of the Peer Review Report, regarding the details of the peer review.

General Panel Findings

As a result of the review and the discussions during the meeting, six of the seven reviewers found USEPA's fate and transport models to be acceptable with minor revisions, and one reviewer found these models acceptable, but did not classify the necessary revisions as minor or major. Similarly, four peer reviewers concluded that USEPA's bioaccumulation models are acceptable with minor revisions; one found these models acceptable with major revisions. Two reviewers who were expert primarily with water quality and sediment transport modeling did not offer recommendations on the bioaccumulation models.

When answering the questions in the charge, the reviewers generally agreed that USEPA's models adequately reproduce historical data. However, given that the models do not reflect a fully mechanistic understanding of all chemical, physical, and biological processes, the reviewers had concerns about the uncertainty associated with the models' forecasts. As such, USEPA's approach was not to revise the existing model calibrations. Rather, USEPA has responded to the reviewers' comments, and provides the additional information that could be ascertained without a total recalibration. The responses to comments follow.

II. IDENTIFICATION OF COMMENTS AND RESPONSES TO RECOMMENDATIONS FOR THE FATE AND TRANSPORT MODELS

A summary of recommendations agreed upon among the peer reviewers (and delineated in Section 5.1 of their final report) is presented in Section V. USEPA's responses to these recommendations correspond to these numbers. Responses to each of the major recommendations are encoded using the specified number and a letter (a, b, c, etc.). For example, responses to the four specific recommendations stated under comment 3, below, will be encoded as 3.a, 3.b, 3.c, and 3.d.

COMMENT 1

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

1a. The reviewers recommended that USEPA assess the accuracy and spatial resolution of the1991 bathymetry survey and the potential accuracy of a new bathymetry survey, and determine whether conducting such a survey would provide insight into the issues of net deposition and sediment burial rates between 1991 and the present. If the utility of conducting a new survey is justified, the reviewers recommended that USEPA perform the survey, assess erosion/deposition through the bathymetry comparison, and interpret the comparison results in light of the HUDTOX model assumptions and findings.

1b. The reviewers recommended that USEPA consider taking new sediment cores at selected sites, measuring depths of Cs-137 peaks, and comparing the depths of these peaks to those in previous sampling efforts to assess burial rates used in the model.

RESPONSES TO COMMENT 1

1a. USEPA notes that the GE 1991 bathymetric survey (HydroQual, 1995a; O'Brien and Gere Engineers, Inc., 1993), upon which most of the HUDTOX model grid was based, was very extensive and contained over 100,000 depth soundings in the Thompson Island Pool alone. The HydroQual report indicates that the horizontal position of the 1991 depth soundings is accurate to within 5 meters and that the acoustic depth meter utilized typically produces vertical measurement errors of ± 3 cm. USEPA agrees that this level of accuracy and spatial coverage would be sufficient to assess changes in bathymetry over large areas that are highly depositional or erosional, but only if this vertical measurement error represented the total error in estimating bathymetry. However, the model-estimated net deposition rates over non-cohesive sediments, which constitute most of the river bottom, are too low (<0.10 cm/year) for bottom elevation changes to be accurately measured within a decadal time frame, even with a total vertical error of ± 3 cm.

A measurement accuracy of ± 3 cm for an acoustic depth meter is not a measure of the total error associated with deploying the instrument in the field. Other factors such as horizontal positioning accuracy, boat pitch and roll, and wave action are likely to result in total errors greater than ± 3 cm. The error associated with individual component variances (horizontal and vertical as a function of depth) may be estimated, and then combined to assess the total error (or uncertainty) according to the Army Corps of Engineers (ACOE) hydrographic surveying guidance (ACOE, 1998; ASCE, 1998). This guidance reports an estimated resultant average error on the order of ± 0.3 feet (± 9 cm) for surveys conducted with similar positioning (5 meter) and depth meter (± 3 cm) accuracy to those described above. This type of error analysis may be applied to the GE 1991 bathymetry data if sufficient details are available regarding precisely how the survey was conducted. However, it is evident that the actual error (perhaps ± 9 cm, or more) associated with these depth measurements is probably underestimated based on instrument accuracy alone.

The level of accuracy obtained for locating (via permanent surveying mounts and/or GPS) the position of a depth sounding (5 meters) also presents a problem for combining the 1991 bathymetric data with new data to assess bed elevation changes at smaller scales, such as across a river transect or a small zone of sediment. This level of accuracy limits the usability of the 1991 data to relatively large-scale distances as compared to the positioning accuracy that may be available with current technology.

All of these uncertainties can likely be greatly reduced in future bathymetric studies of the Upper Hudson River, but not for the existing 1991 data. USEPA does not believe that a near-term bathymetric survey of the river is warranted for the primary purpose of comparing 1991 and current sediment bed elevations to evaluate whether erosion or deposition is occurring, since this cannot be done with a reasonable degree of confidence.

USEPA also notes that Thompson Island Pool bathymetry measured during the 1991 GE survey was used to assess changes in bed elevation on a poolwide basis for the period from 1977 to 1991 (HydroQual, 1995a). The resulting average sedimentation rate ranged from 0.24 to 0.57 cm/year, depending on the methodology employed. The uncertainty associated with these estimates was not addressed in the HydroQual report, so these estimates should not be inferred as validating assumptions regarding net deposition in the pool. In fact, the GE modeling report (QEA, 1999) discusses attempts to compare the 1991 data with historical (1977 and 1982) bathymetric survey data, but concludes that “common horizontal and vertical datums could not be established with sufficient precision to produce bed elevation change calculations that were reliable and accurate enough to use for model validation.”

1b. It is possible to gain additional insight into sedimentation rates within depositional zones of the Upper Hudson River through the collection of additional sediment cores to compare the depths of Cs-137 peaks to those in previous sediment coring efforts. However such information can only provide a qualitative assessment of sedimentation rates determined through modeling efforts, since cores collected at discrete points can not be extrapolated to reach-average spatial scales in the Upper Hudson River sediments.

USEPA recognizes that additional data collection, such as suggested by the peer reviewers, could help reduce some uncertainty relating to the fate and transport and bioaccumulation of PCBs in the Hudson River. However, USEPA believes that it currently has a sufficient understanding of the system (including the uncertainties) on which to base a decision for the site. Therefore, USEPA decided it would not be necessary to collect additional field data for the modeling conducted for the Reassessment RI/FS.

COMMENT 2

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

RESPONSE TO COMMENT 2

USEPA recognizes the concern of the peer reviewers regarding the sensitivity of model forecast results to the model's horizontal and vertical spatial resolution. Similar comments were made during Model Approach Peer Review (on the Preliminary Model Calibration Report (PMCR) (TAMS *et al.*, 1998b)), and USEPA revised the spatial segmentation grid in the HUDTOX model in response to those comments. Both the horizontal and vertical scales were more finely resolved as part of the RBMR. Refinement of the PMCR spatial scales was conducted primarily in the Thompson Island Pool, the most heavily contaminated and data-rich portion of the river. In addition, the HUDTOX grid was refined in downstream reaches to reflect significant geographic features relevant to the Reassessment such as dams, major tributaries, identified sediment PCB "hotspots", and historical sampling locations (see RBMR Section 5.6).

USEPA recognizes that a hydrodynamic and sediment transport model with greater spatial resolution could be developed in reaches downstream of the Thompson Island Pool. However, given the more limited data available in this portion of the river and the large uncertainty related to specifying external solids loads in these downstream reaches, USEPA believes that spatial resolution of the HUDTOX model presented in the RMBR is sufficient to answer the principal Reassessment questions.

COMMENT 3

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

3a. The reviewers recommended that USEPA assess the implications of parameterizing resuspension of cohesive sediments using shaker and annual flume tests in light of recent experiments with sedflume (e.g., Lick *et al.*, 1995b) at shear stresses more compatible with those in the Hudson River during high flow events.

3b. The reviewers indicated that the DOSM and HUDTOX models both appear to assume that resuspension of cohesive sediments is limited in total erosion potential to the sediment that erodes over a 1-hour period at the peak of the event—an assumption inconsistent with findings reported in the literature (Lick *et al.*, 1995a; Gailani *et al.*, 1991) and therefore possibly incorrect. Suspecting that USEPA might be underestimating resuspension by a factor of 5 to 20, the reviewers recommended that USEPA check, correct, and/or justify its approach to modeling resuspension of cohesive sediments through the rising limb of flow events.

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

3d. Agreeing that bed load may be an important factor in the redistribution and mixing of non-cohesive sediments and, in turn, the exposure of PCBs associated with sediments, the reviewers recommended that USEPA consider incorporating bed load transport (following approaches similar to those used in the HEC-6 model) into the HUDTOX model to assess the importance of this process.

RESPONSES TO COMMENT 3

3a. The process mechanism for erosion of cohesive sediments in the HUDTOX model is based on studies conducted by Dr. Wilbert Lick and his colleagues at the University of California, Santa Barbara (McNeil, 1994; Lick *et al.*, 1995a). These studies were conducted using an annular flume and a shaker device to measure relationships between applied shear stress and total amount of resuspended material. Results from these studies show that for any given applied shear stress, it is the cumulative amount of sediment eroded that is actually measured. In other words, the amount of sediment eroded at 10 dynes/cm^2 is observed to be the same, regardless of whether the applied shear stress was 10 dynes/cm^2 for the duration of the experiment or was progressively increased to 10 dynes/cm^2 from a lower initial value. This assumption is central in the process mechanism for erosion of cohesive sediments in HUDTOX, as well as in various other sediment transport models (e.g., SEDZL, ECOMSED, etc.) that have been well-documented in the published scientific literature (Zeigler and Nisbet, 1994; Gailani, *et al.*, 1991; Gailani, *et al.*, 1996).

In reference to the comment regarding Lick's recent erosion studies using the so-called SEDFLUME apparatus, USEPA responds that:

> No SEDFLUME experiments have been conducted with Upper Hudson River sediments. Recent SEDFLUME studies at other sites have included efforts to correlate erosion rates to various sediment properties (*e.g.*, bulk density, mean particle size and size distribution, mineralogy, organic content, etc.). Jepsen, *et al.* (1997) found functional relationships between erosion rates and bulk density for a given shear stress for sediments from three different sites. However, each of these relationships was found to be highly site-specific. SEDFLUME experimental results have yet to be generalized to the extent where site-specific data are not required for application purposes. In the absence of site-specific SEDFLUME measurements using Upper Hudson River sediments, USEPA can not evaluate whether SEDFLUME experimental results are in conflict with the existing site-specific measurements using the annular flume and shaker devices.

To our knowledge, there has not yet been a site-specific application of a long-term, continuous simulation, contaminant transport and fate model using SEDFLUME data for specification of gross resuspension. USEPA is unaware of any paper in the published scientific literature that documents such a model application.

USEPA notes that sediment erosion rates measured using SEDFLUME can not be used directly in the Lick formulation for cohesive sediment scour that has already been incorporated in models applied at various sites including the Buffalo (Gailani, *et al.*, 1996) and Pawtuxet Rivers (Zeigler and Nisbet, 1994). Measured erosion rates from SEDFLUME experiments include material that would be associated with bed load, specifically coarse sands and gravels. These materials do not directly enter the water column as suspended load and also have very low organic carbon content. Thus, direct incorporation of SEDFLUME erosion data within a contaminant transport and fate model is not appropriate.

USEPA also notes that an assessment of SEDFLUME experimental results (Jepsen, 1997) was presented in the documentation of the General Electric sediment transport model (QEA, May 1999). This assessment addressed the question of whether the existing Upper Hudson River sediment resuspension data could be extrapolated to shear stresses beyond the maximum value of 9 dyne/cm^2 used in the site-specific experiments. QEA normalized the Detroit River sediment erosion data, with respect to bulk density, and found that a continuous relationship between shear stress and erosion rate exists for shear stresses ranging from 2 to 64 dynes/cm^2. These findings suggest that relationships between applied shear stress and erosion rates developed using Upper Hudson River sediments can be extrapolated to applied shear stresses beyond those measured by the shaker device.

3b. In reference to the reviewer's comment regarding the appearance that both the Depth of Scour Model (DOSM) and HUDTOX utilize a one-hour time scale for erosion, USEPA clarifies that neither the DOSM nor HUDTOX limit erosion to a one-hour time scale. Rather, Section 4.2.4 (Time Scale of Erosion Estimates) of the RBMR refers only to the time scale over which steady-state conditions were achieved for a given shear stress during Lick's sediment erosion studies (Lick *et al.*, 1995a). Actual times to approximate steady-state conditions for specific experiments vary, and the text in the RMBR should have been more clear. This time scale may or may not be relevant to actual scour events in the field, but it is not relevant to the implementation of the Lick erosion formulation in HUDTOX and the DOSM. Implementation of this erosion formulation in both of these models is based upon the measured scour from site-specific studies on Upper Hudson River sediments conducted by Lick for General Electric (HydroQual, 1995b; QEA, 1999). Resuspension in the HUDTOX model occurs throughout the rising limb of a flood hydrograph at a rate that is governed according to the results of these experimental studies which measure total erosion potential for a given shear stress, and not according to a one-hour time dependence. Furthermore, the HUDTOX model accounts for the greater resuspension potential of freshly-deposited (*i.e.*, less than 7 days old) sediment material by continually tracking the fraction of this material in the sediments over time and allowing it to resuspend as long as the applied shear stress exceeds the critical value. Consequently, USEPA does not concur with the suggestion that cohesive resuspension has been underestimated by a factor of 5 to 20. Increasing cohesive resuspension in the HUDTOX model by these factors would result in erosion losses that are inconsistent with site-specific measurements for the Upper Hudson River.

3c. USEPA agrees with the reviewers that the use of a constant settling velocity for cohesive sediments in the HUDTOX model is at odds with the actual mechanisms of settling in response to changes in flow. USEPA also notes that simply using a constant higher settling velocity at higher flows and a constant lower settling velocity at lower flows would still be empirical and not a representation of the actual mechanisms of settling in response to changes in flow. The conceptual framework and calibration approach for the HUDTOX model were designed to represent long-term trends in PCB behavior in the Upper Hudson River, not localized or transient behavior due to changes in flow. The calibration sought to describe mean high and low flow solids and PCB dynamics in the river. Calibration to short-term event dynamics was not emphasized because detailed representation of short-term events was not necessary to answer the principal Reassessment questions. The model was successful in its primary objective and this was best demonstrated by comparison to 21-year trends in surface sediment PCB concentrations and in-river solids and PCB mass transport. In addition, many other metrics were used to demonstrate model reliability and these were used collectively in a “weight of evidence” approach.

3d. Bed load movement can be an important process affecting sediment transport in rivers, especially in rivers that are not highly controlled by dams. Flows in the portion of the Upper Hudson River included in this Reassessment are highly controlled by eight (8) dams. Past studies investigating sediment transport in the Upper Hudson River utilized a one-dimensional, movable boundary model capable of simulating bed load and suspended load transport (i.e., the U. S. Army Corps of Engineers HEC-6 model). The results are described in Tofflemire *et al.* (1979) and Zimmie (1985) and those results are summarized as follows.

The NYSDEC report quotes measurements conducted by Dr. Zimmie in the Upper Hudson and concludes that "… His measurements at Waterford and Fort Edward show that the bed load accounts for less than 1 percent of the total load at a flow of 10,000 cfs and about 4 percent at 100,000 cfs (Tofflemire *et al.*, 1979). Thus, the sediment load in the Upper Hudson is almost totally attributable to the suspended sediment carried in the water column, and the measurements of suspended solids form the basis for quantifying the total sediment load".

Zimmie (1985) describes the results of the calibration and application of the HEC-6 model to the Upper Hudson. He concluded, "…for flows occurring 99.99 percent of the time ([30,000 cfs), the effect on the river bed in any sub-reach is insignificant. Some movement of bed material occurs in an event of 46,600 cfs (10 year flood), and may be significant depending on the actual area effected." USEPA's analysis of Hudson River flows at Fort Edward indicates that this is nearly equivalent to a 100-year flood, which has been estimated by USEPA to be approximately 47,330 cfs (Butcher, 2000). Thus, previous HEC-6 modeling conducted on this system indicates that bed load is likely to be a significant factor affecting PCB redistribution within bedded sediments only during extreme flood conditions.

It is also noted that bed load transport is an important process only for coarser grained bed particles, since finer grained materials move as suspended load in the water column when scoured from the bedded sediments. The coarser grained particles (*i.e.*, coarse sands and gravel) moving as bed load have lower fraction organic carbon and exhibit a much lower capacity to sorb PCBs than finer grained sediments.

In conclusion, previous studies indicate that bed load transport is not an important process in the Upper Hudson River. Furthermore, bed load is confined to non-cohesive areas that exhibit much lower PCB concentrations. Consequently, it was not necessary to include an explicit representation of bed load transport in the HUDTOX transport and fate model to answer the principal Reassessment questions.

COMMENT 4

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

RESPONSE TO COMMENT 4

In accordance with the peer reviewers' suggestion, USEPA has evaluated the HUDTOX water column solids mass balance for relevant locations in the Upper Hudson River where data (*i.e.,* measured flows and TSS) are available for this type of comparison. The RBMR compared model-based and data-based in-river solids loadings for the Stillwater and Waterford USGS sampling locations (RMBR Figure 7-2). In response to the peer review suggestion, the long-term model and data-based solids loads was also estimated for the USGS Schuylerville sampling station. However, the solids mass balance for Schuylerville has less certainty than either Stillwater or Waterford, because there is a somewhat lower sampling frequency and the flows must be estimated for this location.

Results for Schuylerville indicate that cumulative in-river solids loading for the 1977-97 historical simulation period during flows less than 10,000 cfs (at Fort Edward) was approximately 734,907 metric tons based on data and 664,075 metric tons based on the HUDTOX model. The cumulative in-river solids loading at Schuylerville for flows greater than 10,000 cfs were 784,369 and 821,161 metric tons based on data and model, respectively. These results represent an under-prediction of approximately 10 percent for flows below 10,000 cfs, and an over-prediction of approximately 5 percent for flows greater than 10,000 cfs. Consequently, performance of the HUDTOX model at Schuylerville is comparable to its performance at Stillwater and Waterford.

Results for other calibration metrics (*e.g.*, time series comparisons, scatter plots and cumulative probability distributions) and from sensitivity analyses were presented in Chapter 7 of the RBMR to assess other uncertainties which affect solids mass balances for the locations mentioned above and other locations where long-term solids loading estimates cannot be constructed with reasonable accuracy.

COMMENT 5

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

RESPONSE TO COMMENT 5

For the RBMR, USEPA placed the 100-year peak flow simulation at the beginning of the forecast period because it believed that such timing would be the worst-case scenario (because that is when surficial sediment concentrations are at their highest levels). It should also be noted that a worst-case assumption regarding external solids loading was applied for this 100-year peak flow simulation. In particular, upstream and tributary solids loads were unchanged from the base forecast and were not allowed to increase when the upstream flood hydrograph at Fort Edward was adjusted upward to approximate a 100-year peak flow.

In response to this recommendation, an additional HUDTOX forecast simulation was conducted in which the 100-year peak flow was placed in forecast year 2042. Figures 5-1 and 5-2 show that placing the 100-year peak flow farther out into the model forecast period has a similar transient effect on water column concentrations as placement during 1998, the first year of the model forecast (see RBMR Figures 8-11 and 8-12). However, the magnitude of the transient increase in water column concentrations is much smaller during the 2042 placement than during the 1998 placement due to lower levels of PCB contamination in the surficial sediments. Figure 5-3 shows that the predicted increase in Tri+ mass load over Federal Dam is less than 14 kg when the 100-year peak flow is placed in 2042. Placement in 1998 resulted in an increase of 70 kg in this same mass load (see RBMR Figure 8-13). In model segments that are computed to be slightly erosional, including some cohesive sediment areas in Thompson Island Pool, placement of the 100-year peak flow in 2042 accelerated surface exposure of more highly-contaminated sub-surface layers by approximately 2 years. The erosion resulting from the 100-

year peak flow did not significantly affect the magnitude of the forecasted concentration “spike”, as shown in Figure 5-4. Based on these findings, USEPA believes that placing the 100-year peak flow at the beginning of the forecast period was a conservative assumption and produced an upper-bound estimate of potential impacts based on the model.

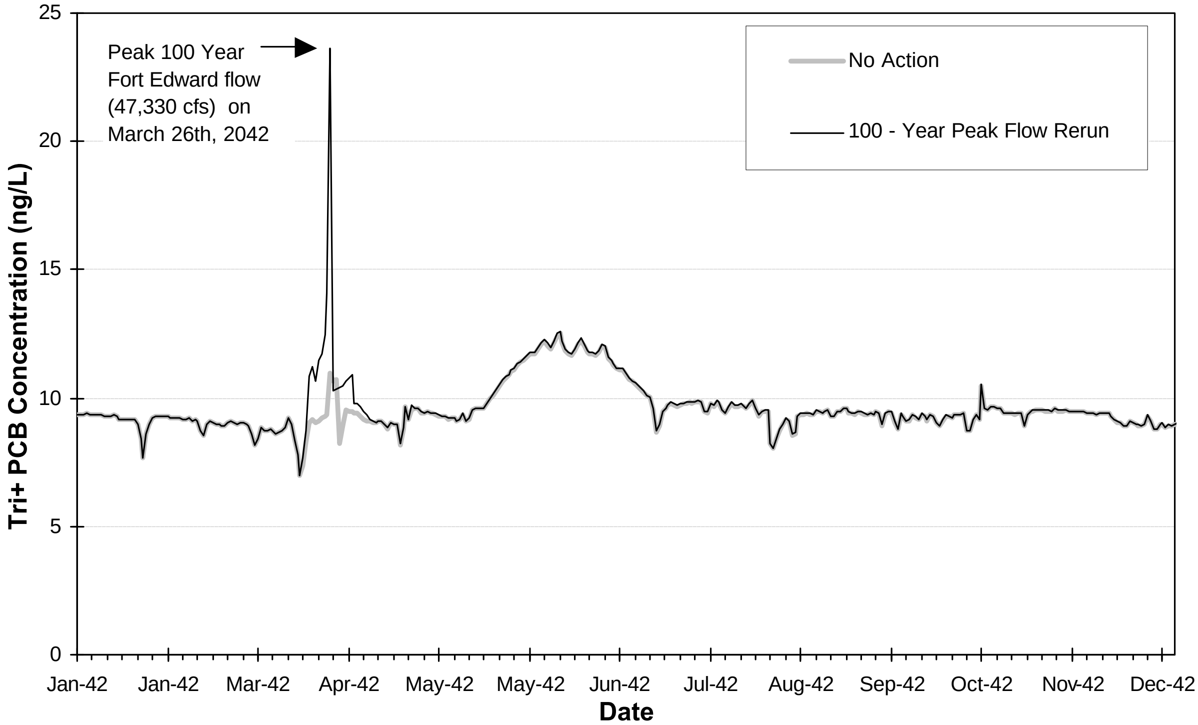


Figure 5-1. Predicted 100 Year Peak Flow Event (3/26 to 4/10) Impact on Tri+ PCB Levels at Thompson Island Dam (West) Due to Alternative Placement of the Event in Forecast Year 2042.

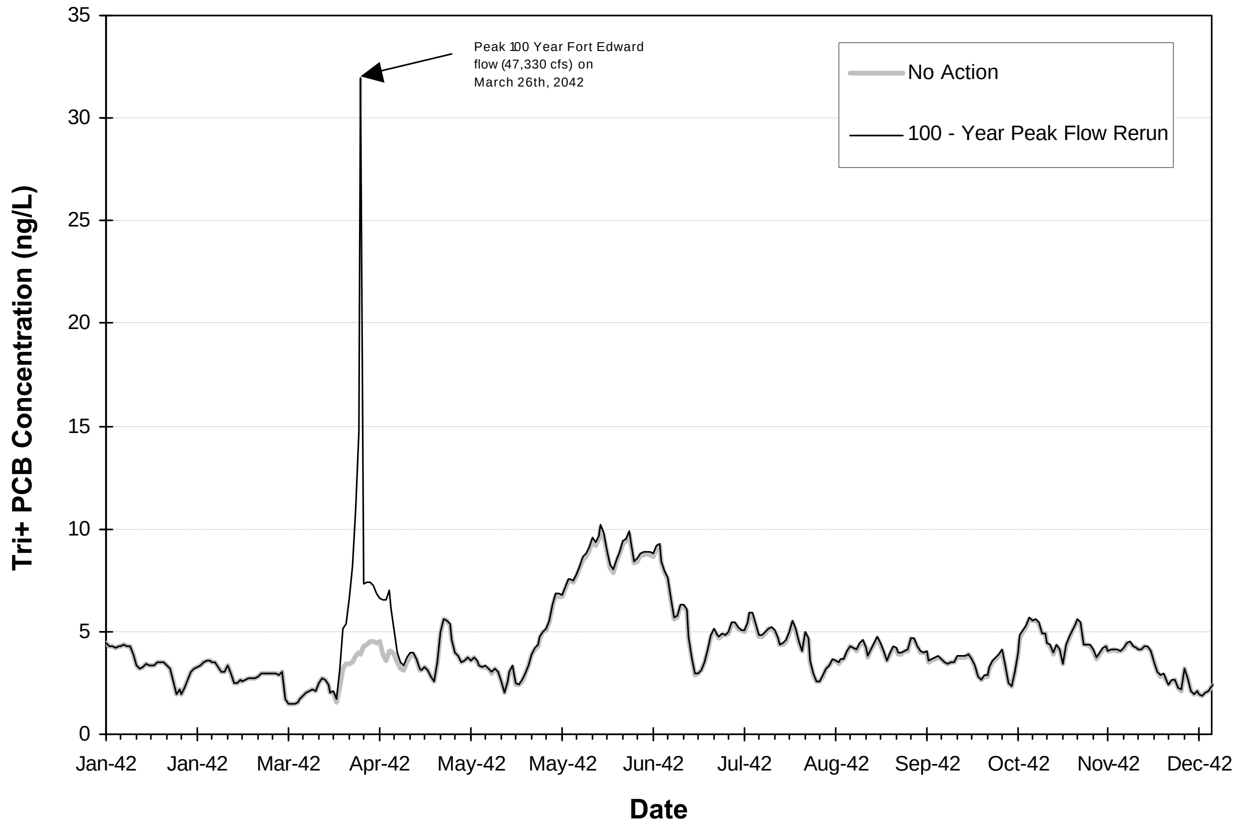


Figure 5-2. Predicted 100 Year Peak Flow Event (3/26 to 4/10) Impact on Tri+ PCB Levels at Federal Dam Due to Alternative Placement of the Event in Forecast Year 2042.

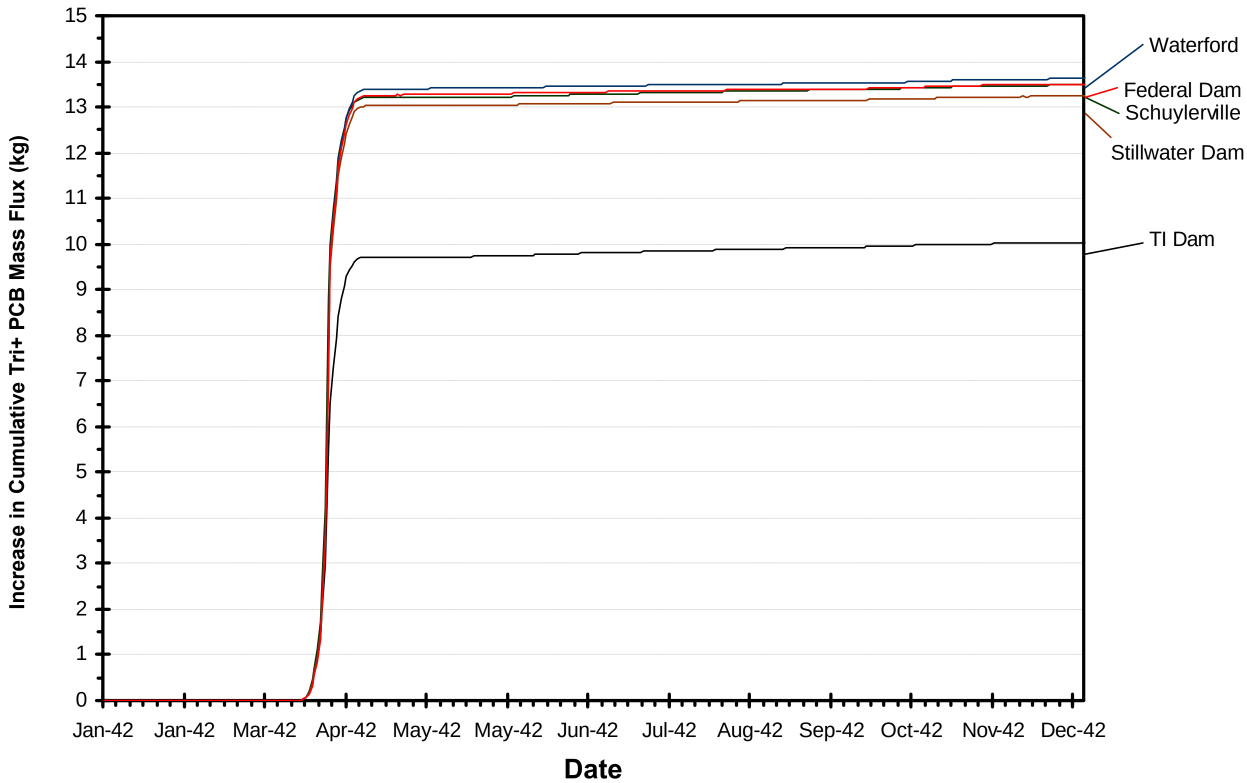


Figure 5-3. Cumulative Net Increase of Tri+ PCB Mass Loading at Various Locations in the Upper Hudson River Due to Placing the 100 Year Peak Flow Event in 2042 (versus the No Action Scenario).

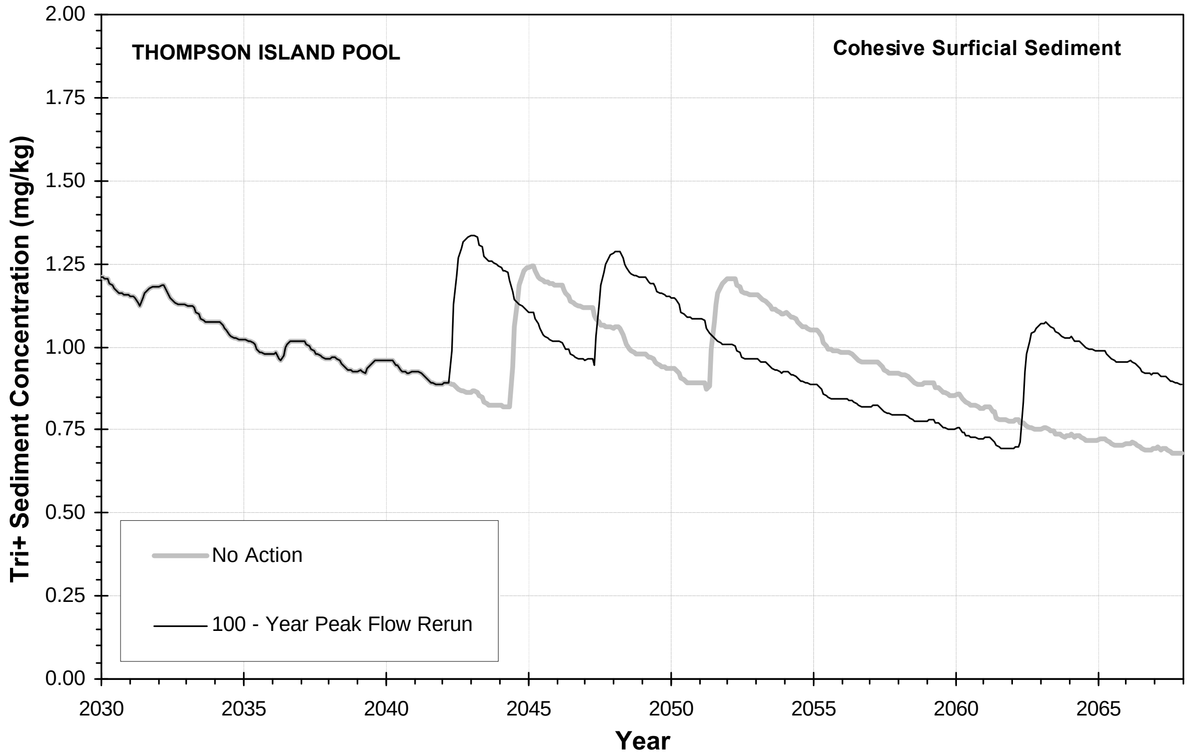


Figure 5-4. The Effect of Placing the 100 Year Peak Flow Event in 2042 on the Timing and Magnitude of the Forecasted Thompson Island Pool Cohesive Sediment "Spike".

III. RESPONSE TO PEER REVIEW COMMENTS

COMMENT 6

Update constants input to the FISHRAND model. The reviewers noted that the FISHRAND model is based on several uncertain parameters and that USEPA assigned values to some input constants that are inconsistent with new information reported in the literature[JBB1]. The reviewers recommended that USEPA reevaluate the constants used in the model with respect to the new information available.

RESPONSE TO COMMENT 6

The Gobas model framework was selected as a compromise between very simple statistical (empirical) descriptions of data and more complex and highly parameterized mechanistic models. The quantity and quality of available measurements do not allow for a unique calibration for a large amount of unknown parameters. Many of these parameters influence predicted fish concentrations additively with the result that the impact of each individual parameter on cumulative PCB body burdens in fish is masked by the influence of other parameters. The Gobas model is a peer-reviewed model that has been used in a number of regulatory contexts to develop trophic transfer factors in food webs or to evaluate water quality criteria (e.g., Kubiak and Best, 1991; USEPA, 1995, Minnesota Pollution Control Agency Chapter 7052) and is being used for remedial decision making at other sites (e.g., the Fox River. See, http://www.epa.gov/region5/foxriver/). The rate constants used in the model were based on laboratory experiments and have been validated using field data. Consequently, our first approach was to use the model with only minor changes to the specified rate constants to maintain the internal consistency of the modeling approach. In addition, a full sensitivity analysis was conducted as described in the Revised Baseline Modeling Report (USEPA, 2000). That analysis provided an indication of the sensitivity of specific rate constants in the model.

The approach used in the RBMR to evaluate the sensitivity of rate constants in the model was to take the derivative of each model constant with respect to fish concentration and plot both the value of the constant and the derivative over time. Note that any given rate constant is not represented by a single number but rather a distribution reflecting the underlying input distributions (e.g., lipid content, weight, temperature, K_{ow}, etc.).

In response to this comment, we further evaluated the sensitivity of a number of the constants used in the model. Specifically, one reviewer referred to the Q_L/Q_W ratio, stating that other researchers have found order of magnitude differences in the coefficient (10) used in that ratio. The original sensitivity analysis had shown that this rate constant is not particularly sensitive. However, we ran the FISHRAND model using 10, 100, and 1000 for this coefficient and found differences in the predicted fish body burden at the second decimal place, or much less than one percent, showing that the Gobas model is insensitive to to this coefficient in the range 10 to 1000. A look at the equation in which this constant appears shows that the constant is divided by a very large K_{ow} (~10^5-10^7), and hence it could only provide a noticeable contribution to the model prediction if it was on the order of 10^5-10^7. However, there are no indications in the literature that would suggest this order of magnitude for this coefficient.

In response to this comment, a literature review was undertaken to determine what, if any, "new information" might be available with which to update or constrain model constants. The results of this literature review suggested that there is not enough information available, nor justification, for revising rate constants in any other manner than what has already been done through the calibration process.

Growth Rate

The original FISHRAND model used the "generic" growth rate given in the Gobas model. As part of the RBMR, the growth rate coefficient was updated in the calibration procedure. This resulted in a revised growth rate. It has been suggested that this growth rate may not be commensurate with observed growth rates in the Hudson River. However, there are some differences between individual growth rates (such as are obtained from length-weight-age field observations) and the "population" growth rate in the FISHRAND model The "population" growth rate shows larger seasonal variation due to active seasons of fish spawning and breeding (that is, the population growth rate takes into account not only changes in individual growth rates during warm periods, but also variations in fish numbers in the population as a whole). Another drawback in using individual field-based growth rates is that the observations are not in the form required for the model. Growth rate is a temperature dependent parameter, and individual-based field observations do not provide this temperature dependency, introducing further uncertainty. In addition measurements are not available for all fish types and the data that are available are not sufficient to describe growth rate as a distribution for each species. In particular, the tails of these distributions would be inaccurately specified which (in a probabilistic model) could affect the predicted central tendency. Finally, the growth rate is independent of all other parameters in the model, making it a good choice for the Bayesian updating procedure followed in the calibration.

The growth rate is an important contributor to the overall depuration rate of PCBs in fish. However, there is evidence that the FISHRAND model captures overall depuration rates. The following table provides the average depuration half-times as calculated from the model:

RM189	**Modeled $T^{1/2}$**
Yellow Perch	103 days
Brown Bullhead	117 days
Largemouth Bass	129 days

RM168	**Modeled $T^{1/2}$**
Yellow Perch	105 days
Brown Bullhead	82 days
Largemouth Bass	122 days

These values are comparable to results listed below which were obtained in a laboratory study (Fisk et al., 1998).

Congener	$T^{1/2}$ (days)
BZ#18	24
BZ#28	44 low dose, 46 high dose
BZ#44	38
BZ#52	65 low dose, 39 high dose
BZ#66	82
BZ#101	131
BZ#105	50
BZ#118	103
BZ#128	75
BZ#138	139
BZ#153	224
BZ#187	131
BZ#189	64
BZ#195	67
BZ#206	53
BZ#209	52

The updated growth rate in the FISHRAND model is interpreted as $K_g + e$ where K_g represents the individual growth rate and e represents the additional correction for depuration via other mechanisms. The overall depuration rate incorporates several different processes, including PCB excretion via the gill, PCB excretion via feces and metabolic transformations. As shown, overall depuration rates are commensurate with the literature.

COMMENT 7

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

RESPONSE TO COMMENT 7

The FISHRAND calibration procedure first focused on identifying the significant contributors to predicted fish concentrations as revealed by the sensitivity analyses (briefly described in previous response and in Chapter 3 of the RBMR). On a parallel track, we assembled and evaluated the available data with which to constrain the model constants and other parameters. This information was then combined with the requirements for Bayesian updating. The Bayesian updating procedure is particularly useful under the following conditions:

1. When there is significant uncertainty in the specification of the input distributions but there are some data available with which to constrain these distributions; and,

2. When there are enough data available for the prediction of interest (in this case, PCB body burden in fish).

This led to the selection of total organic carbon (TOC), lipid content in fish, growth rate (see discussion above), and K_{ow} as calibration parameters. In cases where different values were used for different segments of the river, there was justification for doing so. For example, the total organic carbon content in sediment differs in different sections of the river. Not only the central tendency differs, but the parameters and range of the distributions differ. For K_{ow}, (see discussion that follows), there is evidence that the congener profile (that is, the contribution of each individual congener to the Tri+ or total PCB mixture) changes with downstream distance from the source. The effect of this change is reflected in adjustments to the K_{ow} distribution. In the case of lipid content, although there were no systematic or significant observable differences between river segments, the central tendency, or mean lipid content, does differ from one river segment to another, and from year to year within a segment. Thus, there is some justification for refining the distributions within constraints of the data.

K_{ow} (Octanol-Water Partition Coefficient)

The USEPA disagrees that the K_{ow} should be specified rather than calibrated for the Tri+ mixture. First, Appendix K of the Ecological Risk Assessment as well as NOAA (1997) found that the mix of individual congeners in the Tri+ mixture shifts moving downriver. Since each individual congener contributes to the apparent K_{ow} of the entire mixture, it follows that if the contribution of each individual congener changes, so does the partitioning behavior of the mixture as a whole. Second, the partitioning behavior of individual PCB congeners within the Tri+ mixture is a temperature dependent process, as shown in the DEIR. Changes in temperature between segments may amplify the differences in K_{ow} attributable to changes in the congener mixture. Third, since Tri+ is modeled as a mixture, the effective K_{ow} of the mixture is not known, except for the limited locations and times where congener-specific analyses are available. K_{ow} represents a particularly important and sensitive parameter in the model, and plays a role in several different rate constants simultaneously. The purpose of the Bayesian Updating procedure is to focus on highly uncertain variables; the true K_{ow} of the Tri+ mixture is highly uncertain given the change in individual congener contribution to the overall mixture as well as the effect of changes in temperature. Finally, only the mode of the triangular distribution of K_{ow} is varied between segments; 6.47 as compared to 6.6 (the minimum and maximum are identical).

See also the response to comment 10, which provides congener-specific modeling results using a fixed K_{ow}.

Lipid Content in Fish

In developing the FISHRAND model, it was noted that:

1) There is significant variability in mean lipid content from year-to-year based on the available measurements;

2) The likelihood profiles and sensitivity analyses showed that the FISHRAND model is very sensitive to lipid content; and,

3) The Bayesian updating procedure did not significantly affect the central tendency of the lipid content distribution but rather had the greatest influence in the tails (measure of variability).

Since the FISHRAND model was not designed or constructed to predict year-to-year fluctuation in lipid content (see Chapter 6 in the RBMR), the approach was to use a single distribution of lipid content for each species and location which incorporates all the available measurements across all years. The "generic" FISHRAND model used triangular distributions of percent lipid in fish constructed from available measurements. This empirical lipid distribution was updated in the calibration procedure, but this was done within the constraints of data. The final species-specific distributions allow the FISHRAND model to make future predictions of PCB body burdens without having to first predict a lipid content. The results of the updated lipid distributions are interpreted as hypothetical distributions which provide the best approximation of measurements expressed as total PCB concentration in fish and taking into account the potential imprecision in the empirical input distributions for lipid content.

It has been suggested that lipid content differs between different river segments (GE, 1999; NOAA, 1997). In the Upper Hudson River in particular, fish populations in one area are physically separated from fish in other areas by dams and locks. However, our analyses showed no statistically significant or consistent differences between the lipid content at one river mile as compared to another river mile. Thus, we used all the lipid data from the entire Upper Hudson River to represent an overall population of fish. This one distribution was refined for each location recognizing that fish at one river mile may differ slightly from other river miles. However, the central tendencies and ranges of the final calibrated distributions are very close.

Lipid Content in Epiphytes

It has been noted that the lipid content for water column invertebrates is less than that for benthos (GE, 1999). The FISHRAND model incorporates a percent lipid distribution obtained from measurements by the New York State Department of Health for water column invertebrates in the Hudson River. However, there are some concerns with these data. The invertebrates were collected on multiplate samplers and filtered; thus, the lipid determination for "epiphytes" included any organic matter within a certain size range (e.g., detrital matter, particulates, etc.).

Since epiphyte lipid content is highly uncertain, it can be argued that this parameter would be a good choice for updating in the calibration procedure. It was not selected for the following reasons: first, the reported PCB measurements in epiphytes are themselves highly uncertain, so there is very little empirical data against which to base the optimization of the distribution. Second, epiphyte lipid and K_{ow} effectively offset each other in the calibration. Since K_{ow} plays an important role in several rate constants, and is constrained in terms of the central tendency and the range, K_{ow} was selected over lipid content in epiphytes for calibration.

COMMENT 8

8a. Investigate statistical approaches to estimating PCB concentrations in fish: Based on statistical analyses presented at the peer review meeting, the reviewers recommended that USEPA investigate the implications of steady-state assumptions on the overall bioaccumulation modeling approach (e.g., possibly comparing the FISHRAND predictions to fish PCB concentrations computed directly from the HUDTOX results, assuming steady-state conditions apply in the various river segments).

8b. Compare the bivariate statistical model to FISHRAND.

RESPONSE TO COMMENT 8

8a. USEPA agrees that reasonable estimates of growing-season average fish PCB body burden can be constructed from direct relationships to exposure concentrations, rather than through use of a process-based bioaccumulation model. This has been demonstrated for historical conditions in the Hudson River both in the RBMR and by other previous authors (e.g., Brown *et al.*, 1985). There are, however, valid concerns regarding the use of an empirical prediction method to extrapolate beyond the observed data to future conditions. The Peer Reviewers recognized and noted these concerns. Specifically, as the relationship between water and sediment PCB exposure fields changes in the future, empirical predictors may not provide accurate results, and a process-based model is more technically defensible. For this reason, USEPA chose to develop empirical methods as a first step in the bioaccumulation approach and used them to help formulate and constrain the process-based FISHRAND model, but not as a basis for direct prediction of future PCB levels in fish.

A clarification regarding this comment is in order. The summary comment emphasizes "steady-state assumptions." That is, the central tendency of PCB concentrations in biota in a given year can be reasonably predicted from time-average exposure concentrations, without use of an intra-year dynamic modeling approach. USEPA agrees with this conclusion. The detailed Peer Review meeting comments contained in Chapter 3 instead suggest use of "equilibrium assumptions", implying that water and sediment exposure concentrations are in equilibrium with one another. This is clearly not the case in the Hudson River, as demonstrated in the DEIR. Most notably, summer average water column concentrations decline only gradually, in response to dilution, gas exchange and aerobic degredation, between Thompson Island Dam and Federal Dam, while reach-averaged sediment concentrations decline much more sharply. The main proponent of the initial detailed comment apparently recognized this fact as the peer review report states: "In the weeks following the peer review meeting, one reviewer (EB) subsequently wished to clarify this issue by noting that her statistical analyses (i.e., regressions) support that the Hudson River system is certainly at steady-state but not at equilibrium" (p. 3-8). USEPA agrees that a steady-state approach is useful, but an analysis with full equilibrium assumptions is not appropriate.

The bivariate BAF statistical model presented in the RBMR provides a steady-state, non-equilibrium statistical approach to estimating fish concentrations. There were a number of criticisms of this approach which USEPA feels are based on a misinterpretation of the material or are technically inaccurate. Because USEPA feels that the bivariate BAF statistical model is fully responsive to the Peer Review recommendations, detailed responses to a number of the criticisms contained in the pre-meeting comments of One reviewer are appropriate:

One reviewer undertook her own regression reanalysis of a portion of the data and reported a "puzzling discrepency" between her results and results presented in the RBMR. She further implied that the RBMR analyses may have been done in a spreadsheet that produced inaccurate results and recommended that "they must be recomputed with a 'real' statistics program." In fact, all of the statistical analyses for the bivariate BAF model reported in the RBMR were undertaken using SYSTAT (the same statistical software tool used by the reviewer). Apparent discrepancies noted by the reviewer reflect the fact that USEPA used a general linear model weighted regression approach. In this approach, the weight assigned to each data point (e.g., average lipid-normalized fish concentration obtained from a set of samples from a given year and location) reflects the estimated precision of the estimate. The reviewer did not use weighted regression and, as a result, her results are likely reflect bias due to the inclusion of highly uncertain results from very small samples available for certain years and locations.

The reviewer noted that sediment and water concentrations are significantly correlated with one another, a fact with which USEPA agrees. She then implies that the presence of this correlation contradicts "an important assumption of regression models" and suggests that the analysis is "statistically invalid". However, these conclusions represent a misinterpretation of the statistical literature. In fact, the assumptions of the classical linear regression model remain valid unless there is *complete* collinearity among two (or more) of the explanatory variables. The implications of collinearity on regression models is succinctly summarized in the classic text of Kennedy (1979, p. 128) as follows:

> The OLS [Ordinary Least Squares] estimator in the presence of multicollinearity remains unbiased and in fact is still the BLUE [Best Linear Unbiased Estimator]. The R^2 statistic is unaffected. In fact, since all the CLR [Classical Linear Regression] assumptions are (strictly speaking) still met, the OLS estimator retains all its desirable properties... The major undesirable consequence of multicollinearity is that the variances of the OLS estimates of the parameters of the collinear variables are quite large...

In sum, the correlation between sediment and water concentrations does not invalidate the procedure, nor does it lead to an inherent bias. USEPA acknowledges that the presence of correlation does weaken inferences about the relative importance of sediment and water sources of PCBs to biota. This is another reason that USEPA believes that it is preferable to use a process-based bioaccumulation model for the forecast simulations.

The RBMR argues that both water and sediment-derived pathways contribute to bioaccumulation, that these two sources are not in equilibrium, and that it is therefore logical to use a bivariate approach in the empirical BAF analysis. The reviewer states that this justification is "neither statistically nor logically valid". However, as noted above, the presence of partial correlation does not invalidate the linear regression approach. Further, as sediment and water concentrations are only partially correlated, each time series provides additional information not contained in the other. It is therefore to be expected that a bivariate model provides additional explanatory power relative to a univariate (BAF or BSAF) statistical approach. Multiple adjusted R^2 values are presented to confirm that an increase in predictive power (after accounting for correlation among the independent variables) does indeed occur. Indeed, the reviewer notes that "Using ANCOVA, there is found to be a significant difference in the relative concentration of water to sediment among the three of the 4 study segments of the river"–reflecting the lack of local equilibrium between water and sediment in this flowing system. Fish tissue concentrations also differ between these segments to a greater degree than do water column concentrations–a fact that is explained by the dependence of bioaccumulation on both water and sediment pathways.

It was noted by the reviewer that "the sediment data have no [intra-year] inherent variability in them, but the water data do..." Clearly, high temporal variability is expected in water column concentrations (due, in particular, to the influence of upstream loads from the GE Hudson Falls plant source), while sediment exposure concentrations are expected to change much more slowly due to kinetic rate limitations on the flux of PCBs from sediment to water. If fish body burdens are determined by both exposure fields they may be thought of as having a component of short-term variability (from water) and a component of long-term variability (from both sediment and water). The degree to which fish respond to short-term changes in water concentration is useful in determining the significance of the water-derived pathway in tissue concentrations. It is also in no way surprising that the slope of water column concentration versus log sediment concentration is similar at all stations. This reflects the fact that water column concentrations exert a long-term control on the rate of decline in sediment concentrations and vice versa.

The reviewer showed through her ANCOVA work that "Substantially more variability is explained in the fish PCB and water relationship when the River segment is taken into account..." USEPA conducted similar analyses and also found that somewhat better predictions could be obtained by developing separate regression relationships by river segment (not reported in the RBMR). In part this may reflect inaccuracies in the prediction of sediment exposure concentrations. USEPA decided, however, that it was preferable to present a model with homogeneous parameters across all segments, as there was no clear geochemical justification for creating separate models by segment. Further, postulating different responses by segment (independent of estimated exposure concentrations) would seem to be a particularly weak assumption for any application of the statistical model to forecast future conditions.

Finally, the reviewer also raised the issue of heteroscedasticity and recommended that "it is most appropriate to perform statistical analyses on log transformed data." The primary consequences of heteroscedasticity in regression is that the estimated variance of the regression parameters will be incorrect, as will any hypothesis testing based on this variance. It is important to remember, however, that parameter estimates under heteroscedasticity remain unbiased.

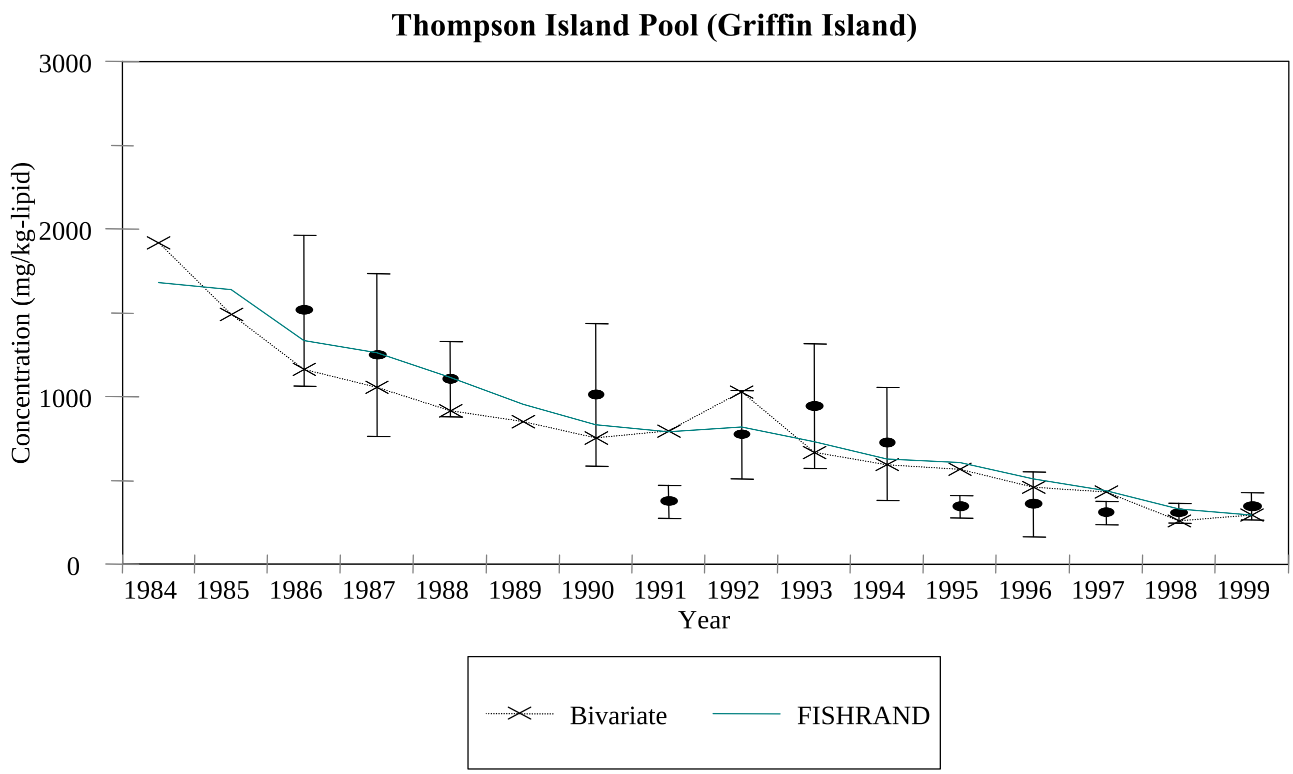
The reviewer evaluated heteroscedasticity using Bartlett's test. This test, however, requires the assumption that variances are consistent within subgroups of the data. This assumption is not appropriate for the weighted generalized least squares approach used to develop the bivariate BAF, in which the assumed variances differ for each observation (Judge *et al*., 1985). USEPA recognized that an ordinary linear regression on the data would produce heteroscedastic residuals. Indeed, the weighted regression approach was used to help reduce potential problems caused by heteroscedasticity.

Because a generalized least squares approach was used, it is not relevant to ask whether the raw residuals demonstrate homogeneity of variance, as the regression postulates that they will not. Instead, the appropriate question is whether the variance of the residuals is consistent with the postulated generalized least squares variance used in the weighted regression. In particular, the appropriate analysis examines whether the series e_i^2/w_i^2, where e_i is the regression residual and w_i the weighting factor for the generalized least squares regression, is homogeneous or non-homogeneous in relation to one of the independent variables. This can be examined using a modification of the Goldfeld-Quandt test for heteroscedasticity (Theil, 1971). As in the Goldfeld-Quandt test, the data are first ordered and *r* central observations are omitted (where *r* is usually set to about one fifth of the total number of observations). An F test is then used to test for homogeneity of variance (of the weighted residuals) between the upper and lower groups of the data. This test was performed for the brown bullhead, largemouth bass, and pumpkinseed models with the data ranked in order of predicted value. In all three cases, the null hypothesis of homoscedasticity is not rejected for the weighted residuals.

Model	F-statistic (upper/lower)	p value (upper/lower)	p value (lower/upper)
Brown Bullhead	1.77	0.13	0.87
Largemouth Bass	0.92	0.56	0.44
Pumpkinseed	0.69	0.77	0.22

In sum, USEPA believes that the existing bivariate BAF approach provides a useful statistical tool for predicting *quasi*-steady state fish concentrations directly from HUDTOX output.

8b. An important purpose of developing the bivariate statistical approach was to provide insight into the set up and calibration of the FISHRAND model. The RBMR neglected to present a detailed comparison of the performance of the bivariate statistical model and FISHRAND. Figures 8-1 through 8-3 provide a comparison of lipid-based PCB concentrations in brown bullhead, largemouth bass, and pumpkinseed predicted by the bivariate BAF statistical model and FISHRAND, along with the observed data, converted to a consistent Tri+ PCB quantitation basis. In general, the bivariate BAF approach and FISHRAND provide similar results, and both do a reasonably good job of matching observed fish tissue concentrations. In most cases, the FISHAND results are somewhat better fit to the data than the bivariate BAF model, with a better representation of short term trends.



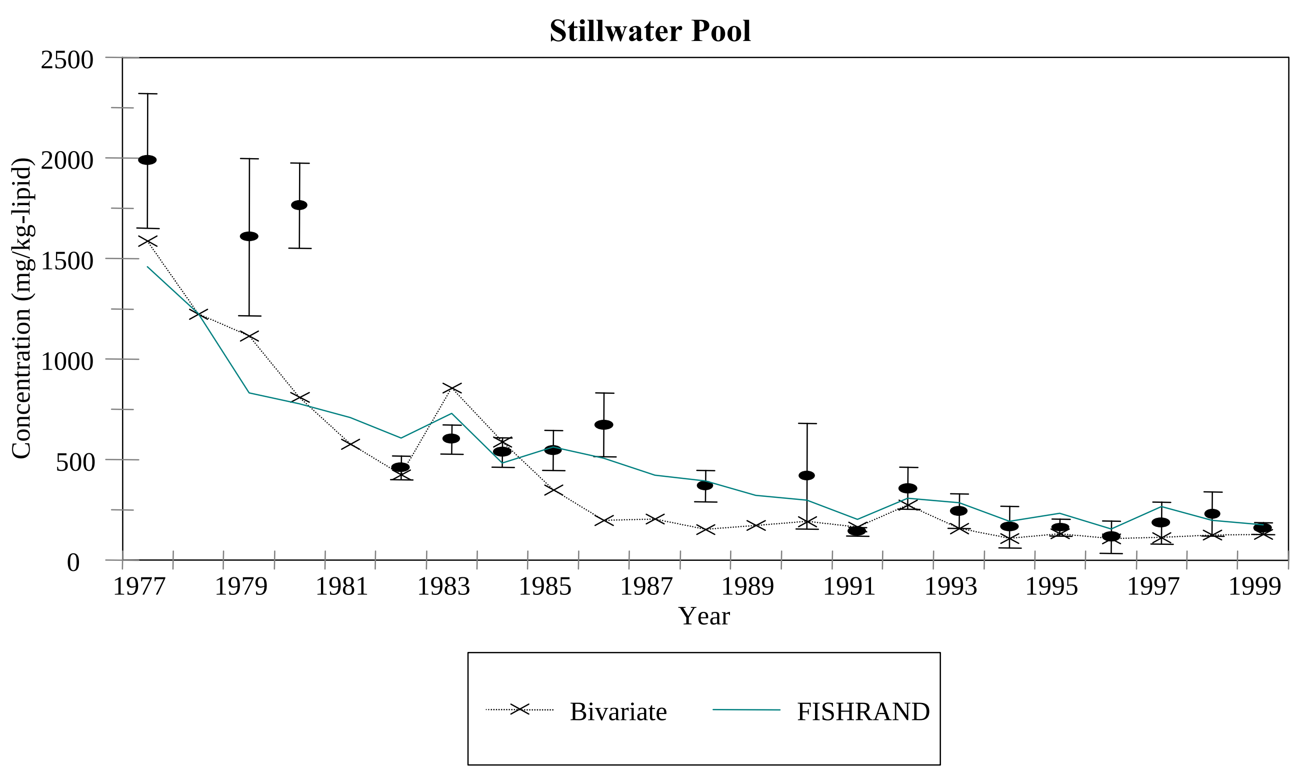
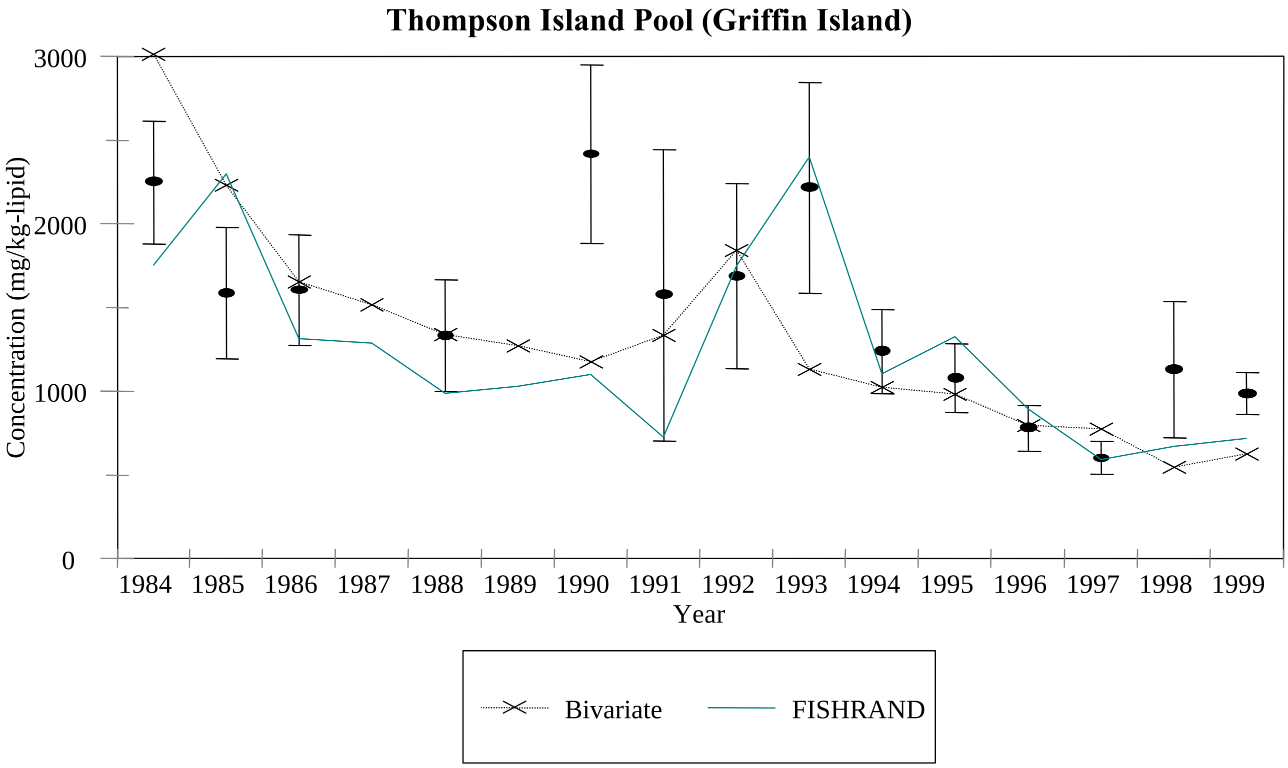


Figure 8-1. Comparison of Bivariate BAF Model, FISHRAND, and Observed Data for PCB Tri+ Concentrations in Brown Bullhead



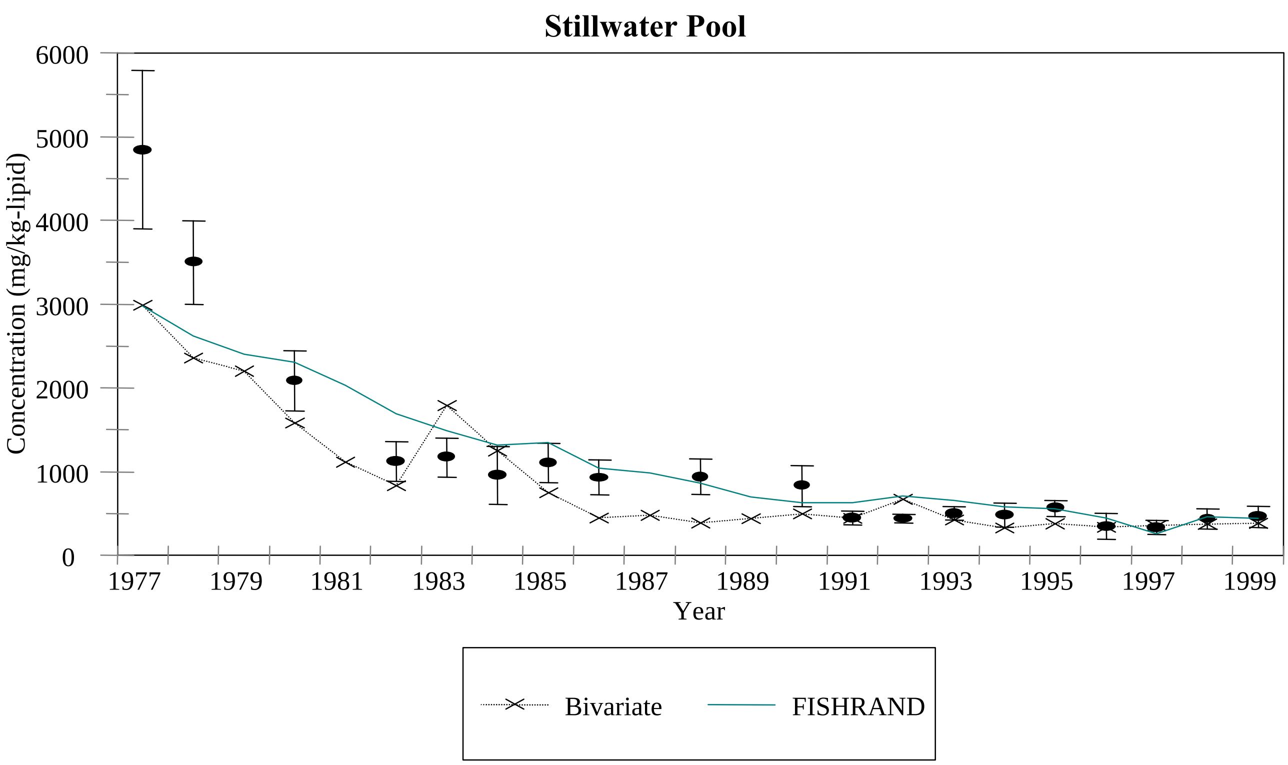
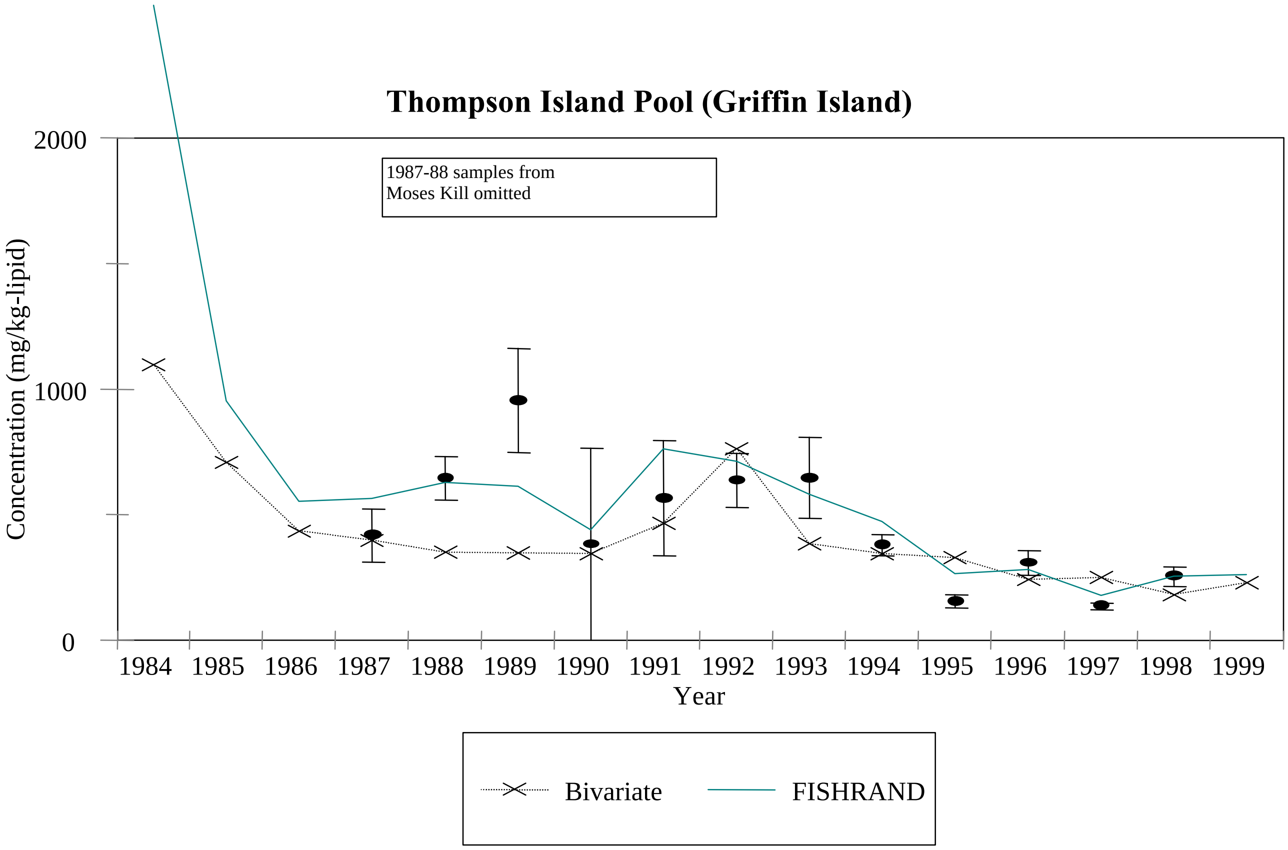


Figure 8-2. Comparison of Bivariate BAF Model, FISHRAND, and Observed Data for PCB Tri+ Concentrations in Largemouth Bass



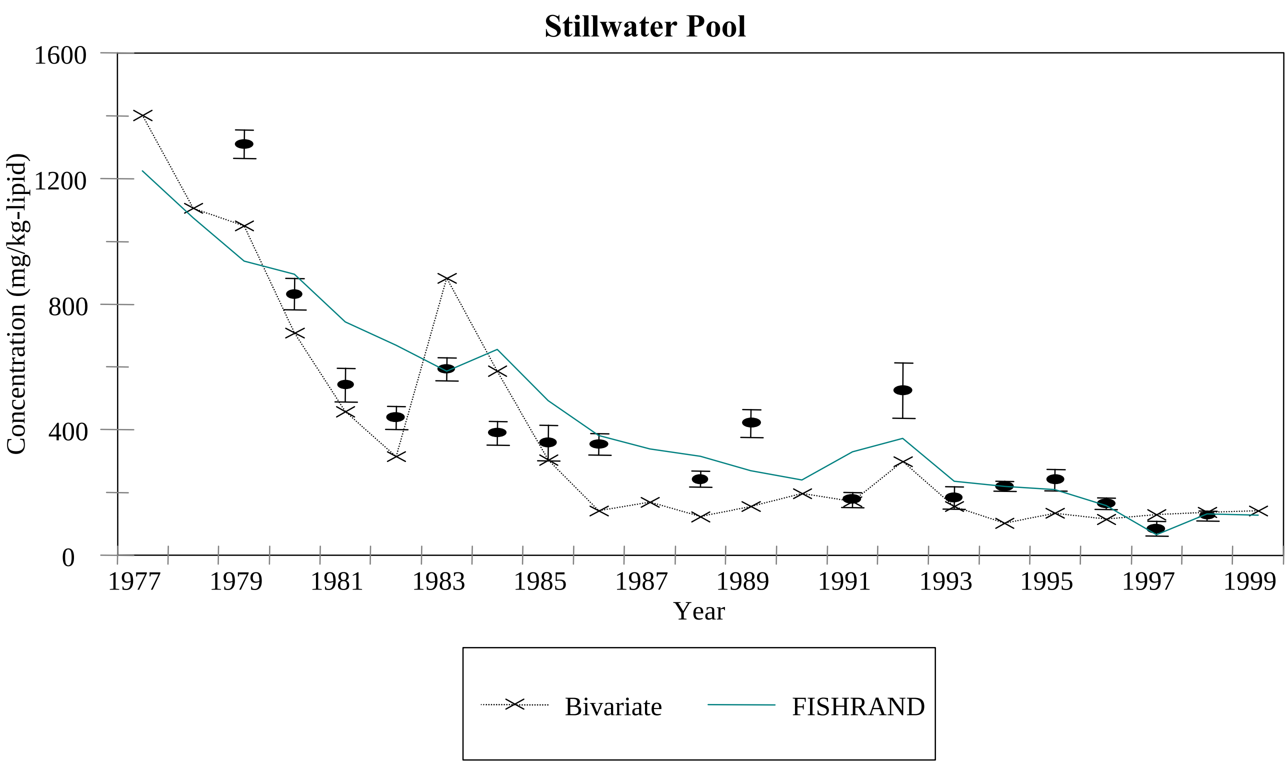


Figure 8-3. Comparison of Bivariate BAF Model, FISHRAND, and Observed Data for PCB Tri+ Concentrations in Pumpkinseed

The FISHRAND model and the bivariate statistical approach are in substantive agreement with one another. Given that FISHRAND incorporates a process-based explanation of PCB bioaccumulation in fish, while the statistical approach is based only on empirical correlations, USEPA feels that FISHRAND provides a more reliable tool with which to forecast responses to future changes in exposure concentrations.

COMMENT 9

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

RESPONSE TO COMMENT 9

The FISHRAND model does not predict body burdens for specific age/size classes in selected fish species, but rather predicts population distributions for a homogeneous age-size class. The FISHRAND model divides fish into two broad categories: forage fish (less than 10 cm) and piscivorous fish (greater than 25 cm). Representative ecological receptor species and humans consuming fish from the Hudson River will not distinguish between specific age and size classes of fish but rather will select fish larger or smaller than some threshold size. That is, the receptors will consume fish from a population of fish. For example, it is known that the bald eagle typically consumes larger fish, but among the larger fish, the eagle will not preferentially consume a particular size over another. "Large fish" represent a population of available fish to this receptor. From a modeling standpoint, the way in which fish are aggregated in FISHRAND is based on feeding preferences and strategies. For example, largemouth bass above 25 cm are all primarily piscivorous, consuming predominantly forage fish.

As part of analyzing the data for use with the FISHRAND model, we constructed plots and ran regressions to ascertain relationships between size, lipid content, and PCB content in fish. There was no correlation between fish size and either PCB concentration or lipid content (see Chapter 6 of the RBMR), suggesting that there is no particular pattern to PCB accumulation based on size of fish. There will be, however, significant differences in predicted PCB content based on differences in feeding strategies between different size classes. This is the reason why we selected a grouping of age classes that reflect the same feeding strategies as a group. Note too, that the proportion of the diet from any particular guild for each fish species is described by a distribution rather than a point estimate.

COMMENT 10

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

RESPONSE TO COMMENT 10

The calibrated FISHRAND model was run for four individual congeners, using a fixed value for K_{ow}, as recommended by the Peer Review. All other parameters were kept at the values determined in the FISHRAND calibration to Tri+. Input sediment and water concentrations were obtained from the HUDTOX model for the period 1991 – 1997. Forecast results were not available for sediment and water inputs. However, to demonstrate model performance on a congener-specific basis, the FISHRAND model was run for the period for which sediment and water input concentrations were available.

The four congeners selected for simulation—BZ#28, BZ#52, BZ#101 plus BZ#90 (co-elutants), and BZ#138—present a range of chemical properties, and are present at sufficient frequency and in sufficient concentrations to allow comparison to data. Wet weight results are presented in Tables 10-1, 10-4, 10-7, 10-10; lipid normalized results are presented in Table 10-2, 10-5, 10-8 and 10-11; and the relative percent differences between predicted and observed are presented in Tables 10-3, 10-6, 10-9, and 10-12. Figures 10-1 through 10-20 present graphical comparisons; observed data are for the mean and one standard error on the mean.

Results for BZ#28

Results for BZ#28 wet weight are shown in Table 10-1. On a wet-weight basis, the model underpredicts pumpkinseed concentrations during 1993 at both locations, but is within or close to the error bars for the data in 1997. The model predicts the mean for spottail shiner at RM 168, but underpredicts at RM 189. The model underpredicts mean yellow perch concentrations at both locations during 1993 and 1995. Congener data for brown bullhead are only available for 1997. At RM 168 the predicted mean is close to but slightly above the error bar for the data, and at RM 189 the predicted mean is only 0.01 ppm below the observed mean (well within the error bar). For largemouth bass, the comparisons at RM 168 are all within the error bars on the mean, but the model underpredicts at RM 189 for 1993 and 1995. For 1997, the model still underpredicts but is within 0.01 ppm of the error bar. Not shown in the table are the comparisons for benthic invertebrates for which data are only available during 1993 at RM 189. The modeled mean is 0.38 ppm wet weight and the observed mean is 0.38 ppm wet weight.

Results for BZ#28 lipid-normalized are provided in Table 10-2. The model underpredicts pumpkinseed concentrations during 1993 at both locations, but is close to the error bars for the data during 1997. The model predicts the mean for spottail shiner at RM 168, but underpredicts at RM 189. The model underpredicts mean yellow perch concentrations at both locations during 1993 and 1995, but is close to the error bar for RM 168 during 1995. The model accurately captures mean brown bullhead concentrations at both locations during 1997. For largemouth bass, the model is within one ppm of the error bar for RM 168 during 1993, but underpredicts during 1995 and slightly overpredicts during 1997. For RM 189, the model underpredicts largemouth bass concentrations for all years, but is within 1.6 ppm of the error bar for 1997. Not shown in the table are the comparisons for benthic invertebrates for which data are only available during 1993 at RM 189. The modeled mean is 12 ppm lipid normalized and the observed mean is 15 ppm lipid normalized.

Table 10-1: Results of FISHRAND Model for BZ#28

Year	Pumpkinseed 168			Spottail Shiner 168			Yellow Perch 168			Brown Bullhead 168			Largemouth Bass 168		
	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991			0.21			0.06			0.10			0.42			0.09
1992			0.22			0.06			0.11			0.54			0.10
1993	0.41	0.13	0.30	0.08	0.005	0.08	0.69	0.16	0.15			0.53	0.11	0.01	0.10
1994			0.18			0.05			0.09			0.49			0.09
1995			0.16			0.04	0.21	0.05	0.13			0.44	0.10	0.02	0.08
1996			0.13			0.04			0.07			0.37			0.06
1997	0.04	0.005	0.07			0.03			0.06	0.13	0.04	0.20	0.06		0.05

Year	Pumpkinseed 189			Spottail Shiner 189			Yellow Perch 189			Brown Bullhead 189			Largemouth Bass 189		
	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991			0.13			0.10			0.14			0.44			0.11
1992			0.14			0.09			0.15			0.41			0.13
1993	0.49	0.07	0.26	0.55	0.10	0.20	1.21	0.15	0.19			0.38	0.59	0.10	0.15
1994			0.10			0.06			0.11			0.34			0.09
1995			0.08			0.05	0.68	0.13	0.14			0.30	0.61	0.35	0.12
1996			0.07			0.04			0.09			0.27			0.06
1997	0.06	0.008	0.05			0.04			0.08	0.25	0.06	0.24	0.15	0.06	0.08

All concentrations in ppm wet weight

Table 10-2: Results of FISHRAND Model for BZ#28

	Pumpkinseed 168			Spottail Shiner 168			Yellow Perch 168			Brown Bullhead 168			Largemouth Bass 168		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991			7.9			9.2			17.9			18.0			15.8
1992			8.1			9.1			18.5			23.5			17.4
1993	32.7	5.3	11.1	15.6	1.9	16.0	24.5	1.0	26.4			23.1	10.0	1.6	12.9
1994			6.7			7.5			16.2			21.5			14.3
1995			5.9			6.6	9.8	1.1	14.4			19.3	57.7	23.3	21.7
1996			4.8			5.4			12.0			16.3			10.5
1997	2.1	0.2	2.5			4.5			10.0	13.6	9.1	14.0	3.5		5.6

	Pumpkinseed 189			Spottail Shiner 189			Yellow Perch 189			Brown Bullhead 189			Largemouth Bass 189		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991			4.4			6.6			9.2			18.9			9.9
1992			4.7			6.5			9.6			17.5			11.9
1993	69.2	6.7	8.3	68.7	13.4	13.4	50.2	6.1	11.5			16.1	47.2	7.6	12.9
1994			3.2			4.3			7.1			14.5			7.6
1995			2.7			3.8	22.0	4.5	8.8			12.9	72.7	13.8	10.4
1996			2.2			3.0			5.5			11.6			5.6
1997	3.1	0.4	1.8			2.5			4.7	9.5	1.0	10.1	10.7	2.2	7.1

All concentrations in ppm lipid normalized (mg PCB per kg lipid)

Table 10-3: Relative Percent Difference for BZ#28

Year	<< ---BZ#28 Wet Weight at RM 168 -- >>					<< ---BZ#28 Lipid Normalized at RM 168 -- >>				
	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass
1991										
1992										
1993	-27%	-1%	-78%		-14%	-66%	3%	8%		29%
1994										
1995			-40%		-21%			47%		-62%
1996										
1997	66%			60%	-19%	16%			2%	58%

Year	<< ---BZ#28 Wet Weight at RM 189 -- >>					<< ---BZ#28 Lipid Normalized at RM 189 -- >>				
	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass
1991										
1992										
1993	-47%	-64%	-84%		-75%	-88%	-81%	-77%		-73%
1994										
1995			-80%		-81%			-60%		-86%
1996										
1997	-5%			-5%	-49%	-42%			6%	-34%

Table 10-4: Results of FISHRAND Model for BZ#52

Year	Pumpkinseed 168			Spottail Shiner 168			Yellow Perch 168			Brown Bullhead 168			Largemouth Bass 168		
	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991			0.26			0.07			0.13			0.58			0.12
1992			0.25			0.07			0.13			0.64			0.12
1993	0.59	0.16	0.33	0.13	0.007	0.10	1.00	0.12	0.17			0.58	0.18	0.03	0.17
1994			0.19			0.05			0.10			0.51			0.09
1995			0.16			0.05	0.44	0.11	0.13			0.43	0.24	0.05	0.13
1996			0.13			0.04			0.07			0.34			0.06
1997	0.10	0.007	0.10			0.03			0.06	0.12	0.04	0.20	0.15		0.08

Year	Pumpkinseed 189			Spottail Shiner 189			Yellow Perch 189			Brown Bullhead 189			Largemouth Bass 189		
	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991			0.21			0.17			0.24			0.77			0.19
1992			0.23			0.17			0.27			0.73			0.26
1993	0.77	0.11	0.38	0.55	0.09	0.36	1.67	0.17	0.32			0.68	0.70	0.10	0.28
1994			0.16			0.11			0.19			0.60			0.16
1995			0.13			0.10	1.38	0.22	0.22			0.52	1.2	0.7	0.20
1996			0.11			0.08			0.14			0.46			0.12
1997	0.10	0.007	0.09			0.07			0.12	0.22	0.04	0.38	0.24	0.03	0.13

All concentrations in ppm wet weight

Table 10-5: Results of FISHRAND Model for BZ#52

	Pumpkinseed 168			Spottail Shiner 168			Yellow Perch 168			Brown Bullhead 168			Largemouth Bass 168		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991			9.6			11.2			23.0			24.6			19.6
1992			9.2			10.4			21.9			27.7			20.0
1993	50.4	8.3	12.3	25.3	3.3	15.1	37.3	2.1	29.6			24.9	16.5	3.2	17.8
1994			7.1			8.0			17.4			21.9			15.3
1995			6.0			6.7	20.2	2.6	22.3			18.6	135.3	47.6	22.3
1996			4.7			5.3			11.8			14.6			10.3
1997			3.8			4.3			9.7	9.0	0.7	8.6			13.9

	Pumpkinseed 189			Spottail Shiner 189			Yellow Perch 189			Brown Bullhead 189			Largemouth Bass 189		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991			6.9			12.0			15.5			33.4			17.3
1992			7.7			11.9			17.1			31.2			22.9
1993	76.4	5.0	13.3	70.4	10.7	26.2	69.9	6.4	19.5			29.4	55.8	6.8	24.9
1994			5.2			8.0			12.4			25.6			14.6
1995			4.4			6.8	45.2	8.5	14.0			22.2	150.0	27.8	17.8
1996			3.6			5.5			9.2			19.5			10.6
1997			2.9			4.6			7.9	17.2	2.1	16.1			12.0

All concentrations in ppm lipid normalized (mg PCB per kg lipid)

Table 10-6: Relative Percent Difference for BZ#52

Year	<< ---BZ#52 Wet Weight at RM 168 -- >>					<< ---BZ#52 Lipid Normalized at RM 168 -- >>				
	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass
1991										
1992										
1993	-44%	-22%	-83%		-6%	-76%	-40%	-21%		7%
1994										
1995			-70%		-44%			11%		-84%
1996										
1997	3%			67%	-44%				-4%	

Year	<< ---BZ#52 Wet Weight at RM 189 -- >>					<< ---BZ#52 Lipid Normalized at RM 189 -- >>				
	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass
1991										
1992										
1993	-50%	-35%	-81%		-60%	-83%	-63%	-72%		-55%
1994										
1995			-84%		-83%			-69%		-88%
1996										
1997	-12%			70%	-43%				-7%	

Table 10-7: Results of FISHRAND Model for BZ#101 and BZ#90

	Pumpkinseed 168			Spottail Shiner 168			Yellow Perch 168			Brown Bullhead 168			Largemouth Bass 168		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991	0.04	0.004	0.04			0.01	0.02	0.002	0.02	0.06	0.01	0.10	0.03	0.005	0.02
1992			0.05			0.01			0.03			0.14			0.02
1993	0.37	0.03	0.07	0.07	0.006	0.02	0.54	0.05	0.04			0.14	0.15	0.02	0.04
1994			0.05			0.01			0.03			0.14			0.02
1995			0.04			0.01	0.26	0.08	0.04			0.13	0.21	0.04	0.04
1996			0.04			0.01			0.02			0.11			0.02
1997	0.05	0.005	0.05			0.01			0.02	0.11	0.03	0.10	0.24		0.03

	Pumpkinseed 189			Spottail Shiner 189			Yellow Perch 189			Brown Bullhead 189			Largemouth Bass 189		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991	0.14	0.03	0.10			0.05	0.13	0.03	0.09	0.14	0.02	0.14	0.11	0.02	0.09
1992			0.07			0.06			0.08			0.18			0.09
1993	0.96	0.10	0.09	0.42	0.07	0.09	0.95	0.10	0.08			0.17	0.58	0.10	0.10
1994			0.05			0.05			0.06			0.16			0.07
1995			0.05			0.04	0.54	0.13	0.06			0.15	0.90	0.46	0.07
1996			0.04			0.03			0.05			0.14			0.05
1997	0.08	0.008	0.03			0.03			0.04	0.15	0.02	0.13	0.28	0.04	0.05

All concentrations in ppm wet weight

Table 10-8: Results of FISHRAND Model for BZ#101 and BZ#90

	Pumpkinseed 168			Spottail Shiner 168			Yellow Perch 168			Brown Bullhead 168			Largemouth Bass 168		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991	6.1	0.6	1.6			1.9	5.2	0.5	4.2	3.4	0.3	4.5	21.3	2.0	3.4
1992			1.8			2.0			4.5			6.0			3.9
1993	24.4	4.3	2.7	14.9	2.0	2.9	21.6	2.2	7.0			6.2	13.8	2.6	6.4
1994			1.7			2.0			4.5			6.1			3.8
1995			1.6			1.8	11.6	2.0	6.4			5.8	121.5	40.9	5.9
1996			1.4			1.6			3.6			5.0			3.0
1997	2.7	0.3	1.8			1.3			3.1	11.9	8.0	4.5	13.4		4.3

	Pumpkinseed 189			Spottail Shiner 189			Yellow Perch 189			Brown Bullhead 189			Largemouth Bass 189		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991	20.3	4.0	3.4			3.6	22.0	4.0	3.5	14.2	2.6	5.9	42.0	7.0	7.8
1992			2.4			4.5			5.0			7.6			8.2
1993	37.6	3.0	3.0	53.7	8.5	6.9	40.7	4.0	5.4			7.3	46.3	6.3	9.0
1994			1.7			3.4			4.0			7.0			5.9
1995			1.5			2.9	18.3	5.5	3.7			6.5	138.1	44.7	6.3
1996			1.3			2.4			3.2			6.0			4.7
1997	4.6	0.4	1.0			2.0			2.8	8.8	1.5	5.5	26.1	3.9	4.4

All concentrations in ppm lipid normalized (mg PCB per kg lipid)

Table 10-9: Relative Percent Difference for BZ#101&90

Year	<< ---BZ#101&90 Wet Weight at RM 168 -- >>					<< ---BZ#101&90 Lipid Normalized at RM 168 -- >>				
	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass
1991	21%		17%	87%	-33%	-73%		-20%	30%	-84%
1992										
1993	-80%	-75%	-93%		-75%	-89%	-81%	-68%		-53%
1994										
1995			-86%		-84%			-45%		-95%
1996										
1997	-1%			-5%	-89%	-31%			-62%	-68%

Year	<< ---BZ#101&90 Wet Weight at RM 189 -- >>					<< ---BZ#101&90 Lipid Normalized at RM 189 -- >>				
	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass
1991	-29%		-27%	0%	-19%	-83%		-84%	-59%	-81%
1992										
1993	-91%	-78%	-91%		-83%	-92%	-87%	-87%		-81%
1994										
1995			-90%		-92%			-80%		-95%
1996										
1997	-64%			-14%	-82%	-78%			-37%	-83%

Table 10-10: Results of FISHRAND Model for BZ#138

	Pumpkinseed 168			Spottail Shiner 168			Yellow Perch 168			Brown Bullhead 168			Largemouth Bass 168		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991	0.02	0.003	0.06			0.03	0.01	0.001	0.03	0.03	0.003	0.07	0.02	0.004	0.03
1992			0.09			0.04			0.05			0.09			0.06
1993	0.18	0.02	0.11	0.05	0.002	0.04	0.29	0.02	0.09			0.10	0.11	0.02	0.09
1994			0.09			0.04			0.05			0.09			0.06
1995			0.09			0.04	0.19	0.06	0.08			0.09	0.17	0.02	0.09
1996			0.08			0.03			0.05			0.09			0.06
1997			0.08			0.03			0.05			0.09			0.06

	Pumpkinseed 189			Spottail Shiner 189			Yellow Perch 189			Brown Bullhead 189			Largemouth Bass 189		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991	0.07	0.01	0.04			0.03	0.05	0.01	0.03	0.08	0.01	0.08	0.06	0.01	0.05
1992			0.03			0.04			0.05			0.11			0.05
1993	0.42	0.04	0.03	0.22	0.03	0.04	0.48	0.04	0.05			0.11	0.34	0.05	0.06
1994			0.02			0.03			0.04			0.11			0.04
1995			0.02			0.03	0.34	0.07	0.04			0.10	0.48	0.24	0.04
1996			0.02			0.02			0.03			0.10			0.03
1997			0.02			0.02			0.03			0.09			0.03

All concentrations in ppm wet weight

Table 10-11: Results of FISHRAND Model for BZ#138

	Pumpkinseed 168			Spottail Shiner 168			Yellow Perch 168			Brown Bullhead 168			Largemouth Bass 168		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991	3.2	0.5	2.4			4.8	2.6	0.3	6.1	1.9	0.2	3.0	13.9	2.0	4.6
1992			3.2			5.6			8.9			4.0			10.1
1993	12.0	2.3	4.0	9.0	1.1	5.8	11.7	1.4	9.3			4.2	10.4	1.9	15.1
1994			3.3			5.6			9.1			4.2			10.4
1995			3.2			5.6	8.5	1.5	9.0			4.1	93.3	23.7	14.4
1996			3.1			5.4			8.8			4.0			10.0
1997			3.0			5.3			8.8			3.8			10.0

	Pumpkinseed 189			Spottail Shiner 189			Yellow Perch 189			Brown Bullhead 189			Largemouth Bass 189		
Year	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean	Observed Mean	Standard Error	FISHRAND Mean
1991	9.7	1.8	1.3			2.3	9.1	1.1	2.1	9.3	2.0	3.4	23.5	3.8	4.1
1992			1.1			2.7			3.0			5.0			4.6
1993	16.7	1.3	0.9	28.6	4.3	2.6	20.2	1.7	3.0			4.9	26.7	3.1	5.2
1994			0.8			2.0			2.4			4.6			3.4
1995			0.8			1.9	11.5	3.0	2.5			4.4	76.8	30.9	3.3
1996			0.7			1.8			2.2			4.2			3.2
1997			0.7			1.7			2.1			4.0			3.0

All concentrations in ppm lipid normalized (mg PCB per kg lipid)

Table 10-12: Relative Percent Difference for BZ#138

Year	<< ---BZ#138 Wet Weight at RM 168 -- >>					<< ---BZ#138 Lipid Normalized at RM 168 -- >>				
	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass
1991	243%		230%	126%	44%	-26%		-20%	57%	-67%
1992										
1993	-39%	-18%	-70%		-21%	-67%	-35%	-68%		45%
1994										
1995			-57%		-48%			-45%		-85%
1996										
1997										

Year	<< ---BZ#138 Wet Weight at RM 189 -- >>					<< ---BZ#138 Lipid Normalized at RM 189 -- >>				
	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass	Pumpkinseed	Spottail Shiner	Yellow Perch	Brown Bullhead	Largemouth Bass
1991	-45%		-38%	-1%	-23%	-87%		-84%	-63%	-82%
1992										
1993	-93%	-84%	-90%		-83%	-94%	-91%	-87%		-80%
1994										
1995			-89%		-92%			-80%		-96%
1996										
1997										

Figure 10-1: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Pumpkinseed at RM 189

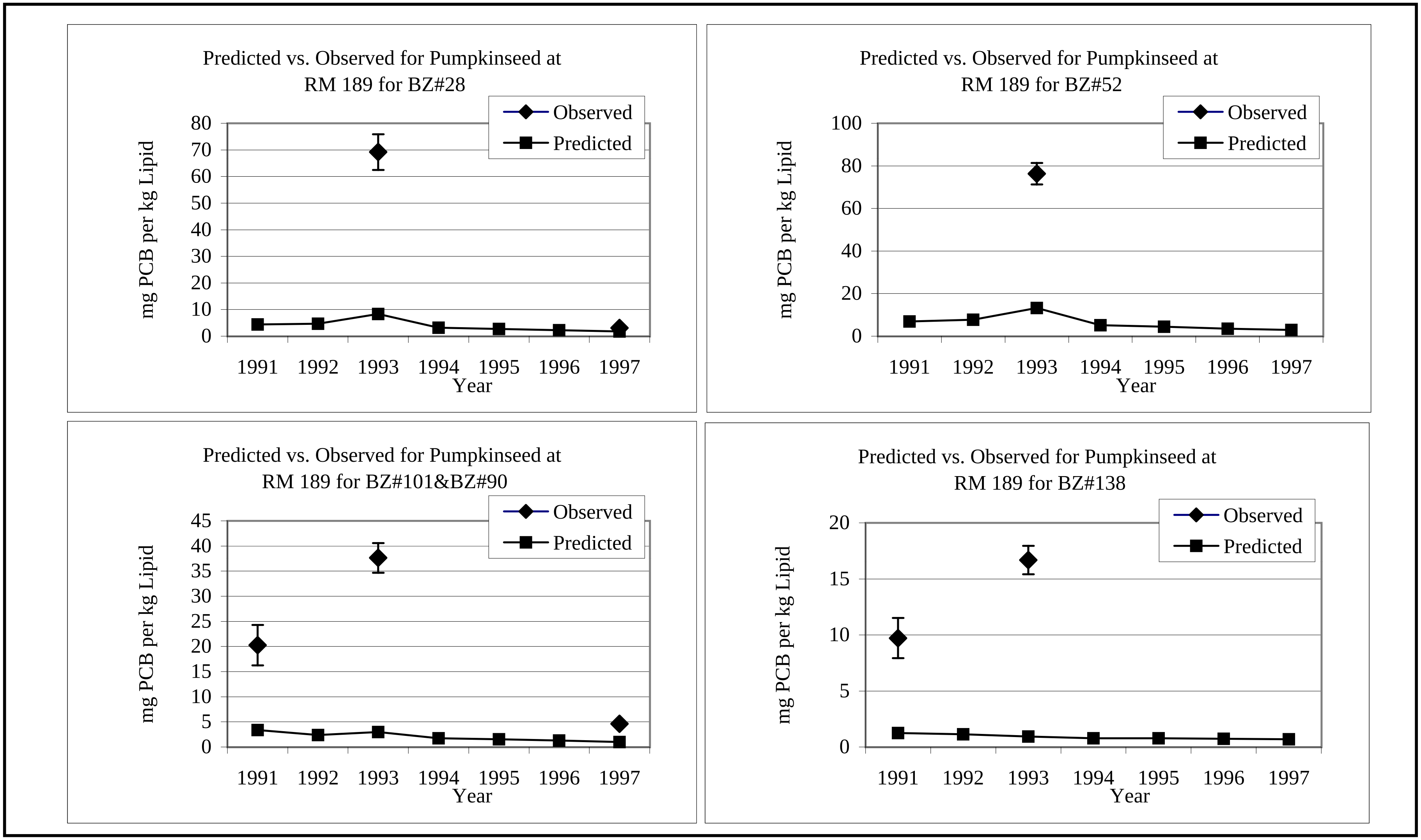


Figure 10-2: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Pumpkinseed at RM 168

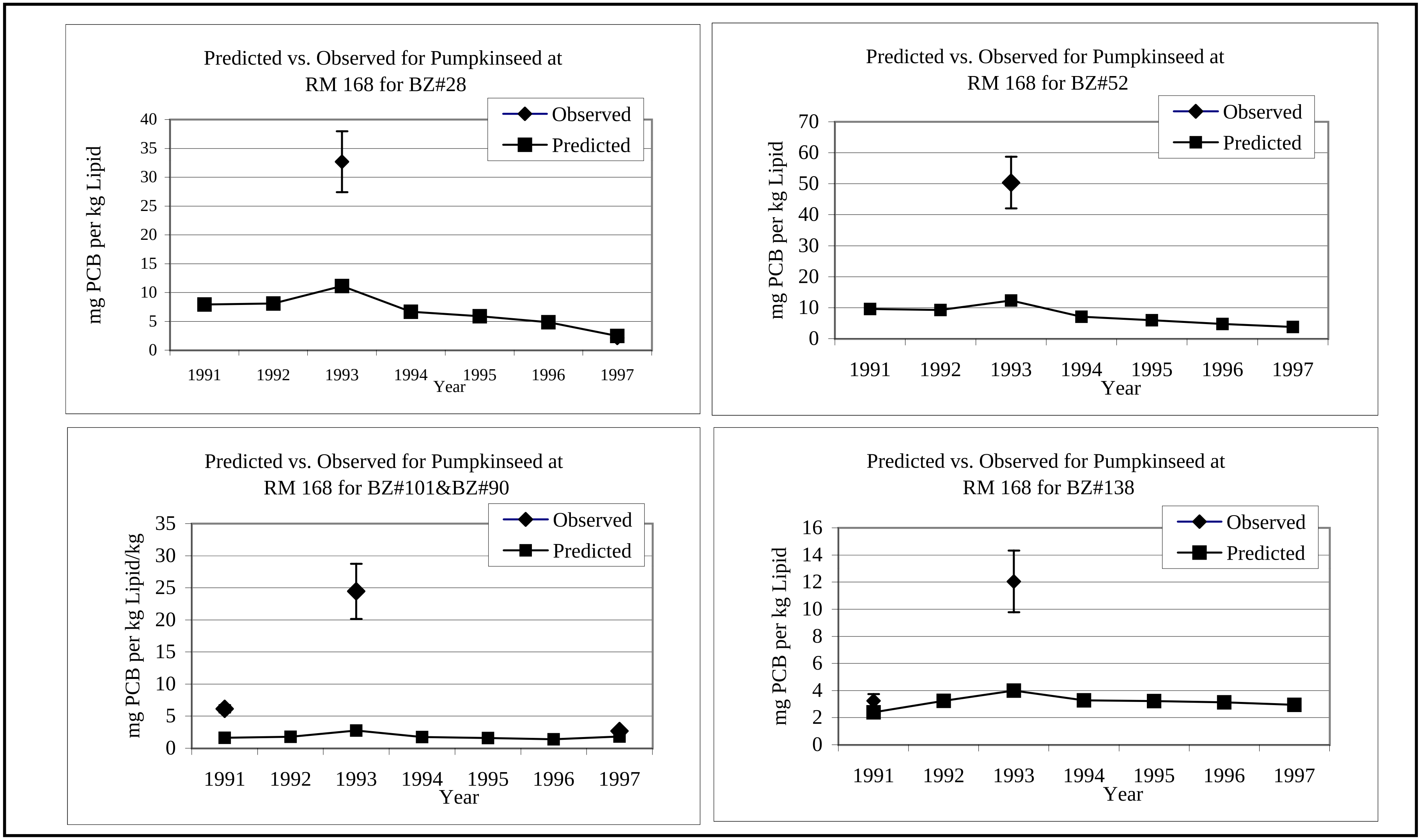


Figure 10-3: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Spottail Shiner at RM 189

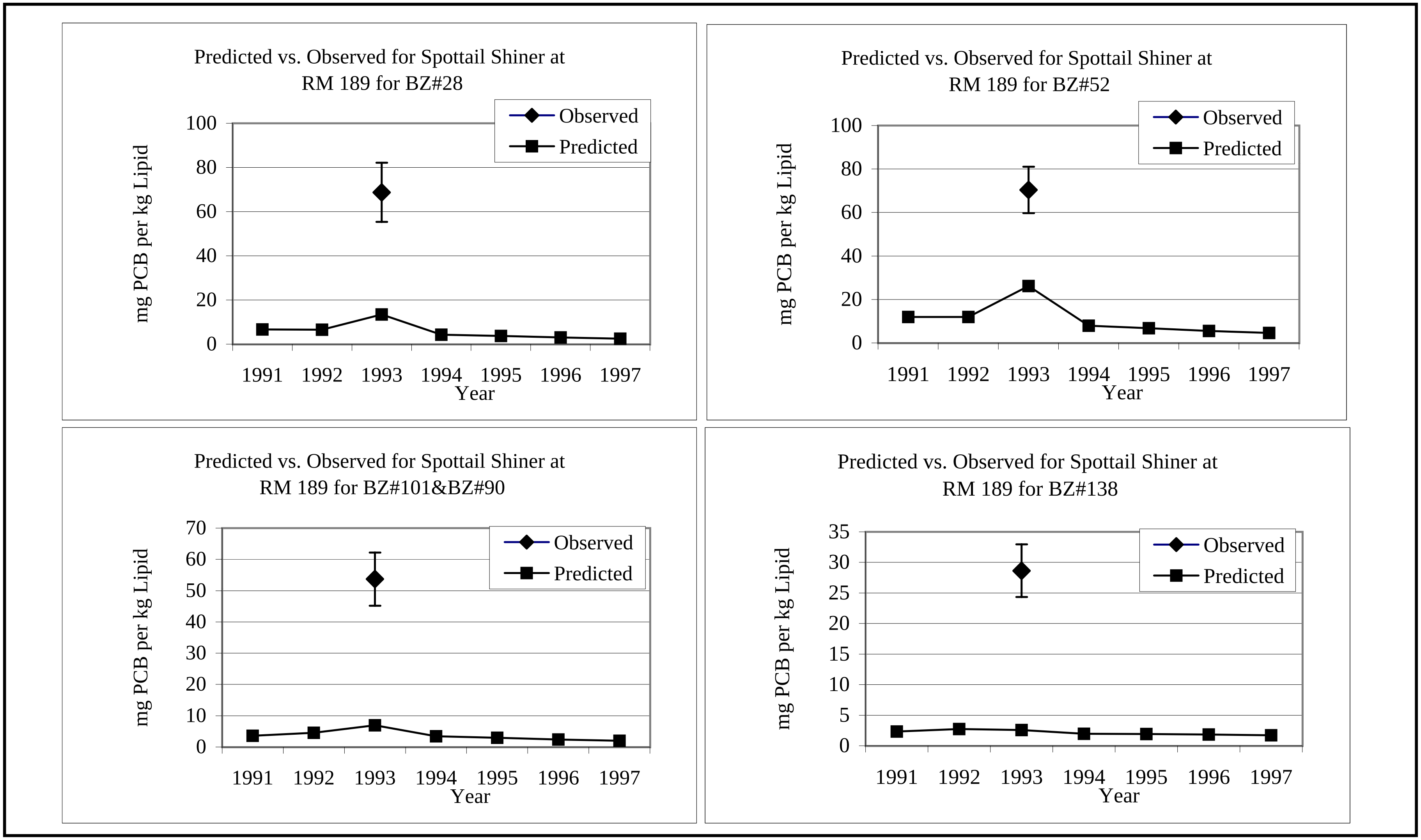


Figure 10-4: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Spottail Shiner at RM 168

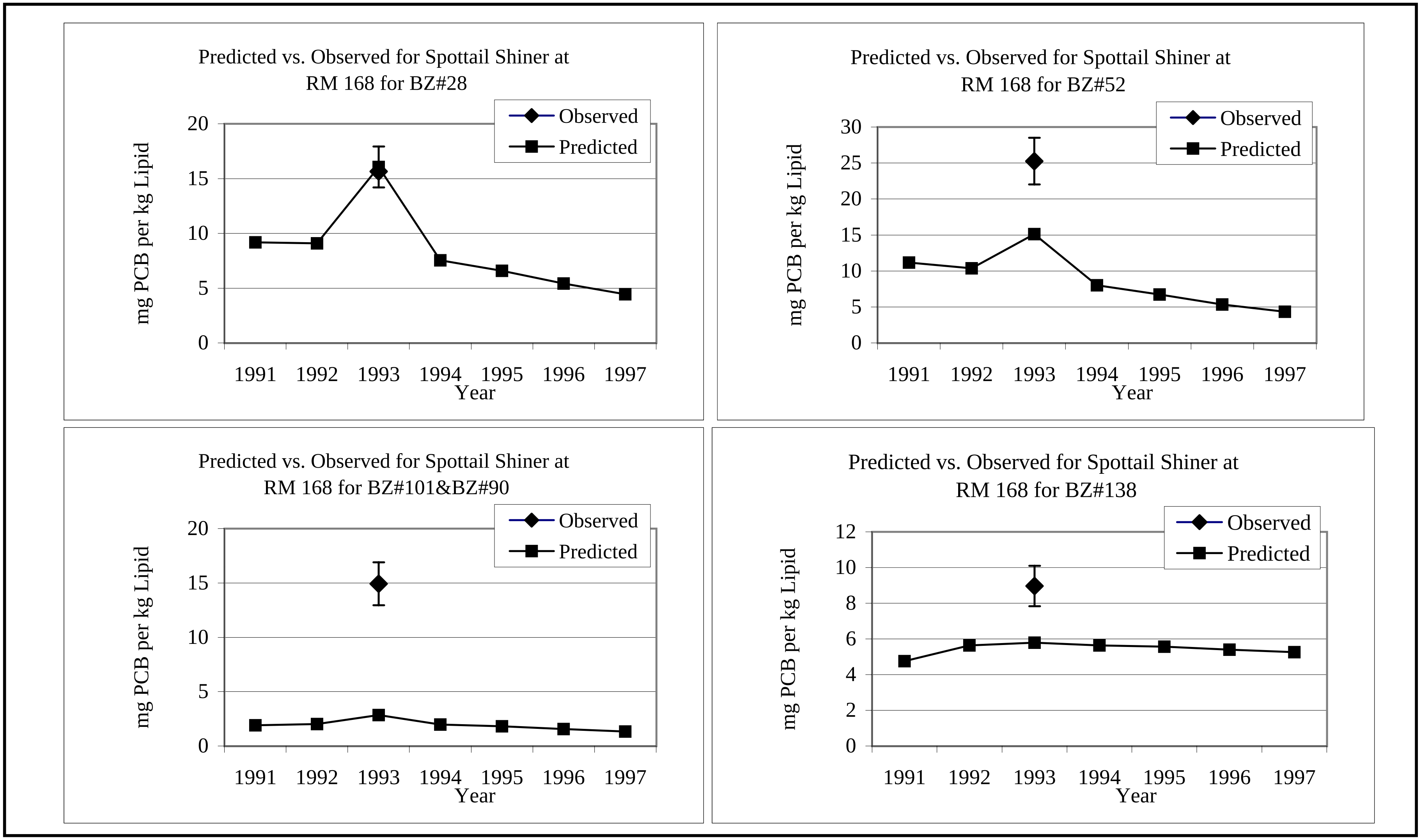


Figure 10-5: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Yellow Perch at RM 189

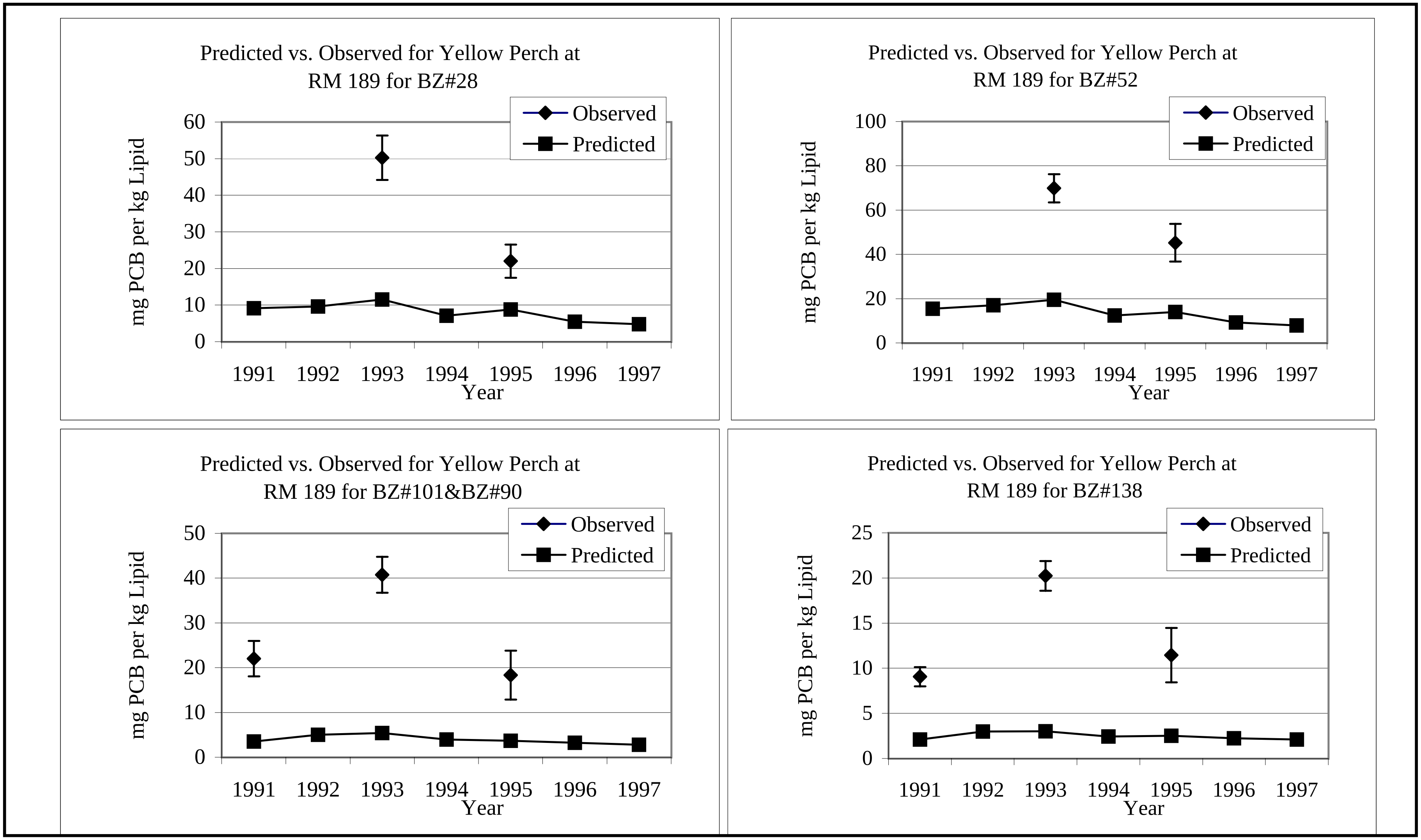


Figure 10-6: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Yellow Perch at RM 168

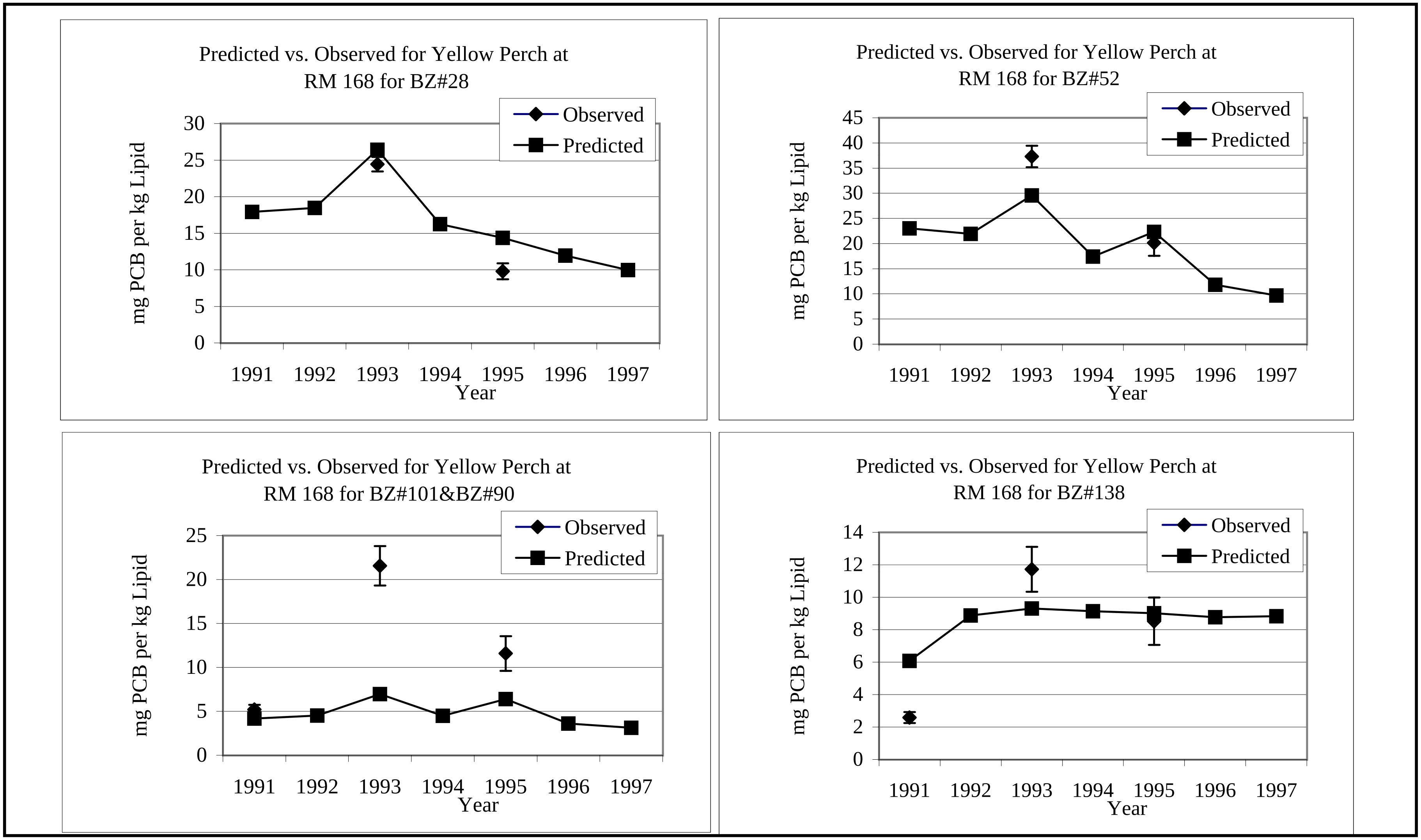


Figure 10-7: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Brown Bullhead at RM 189

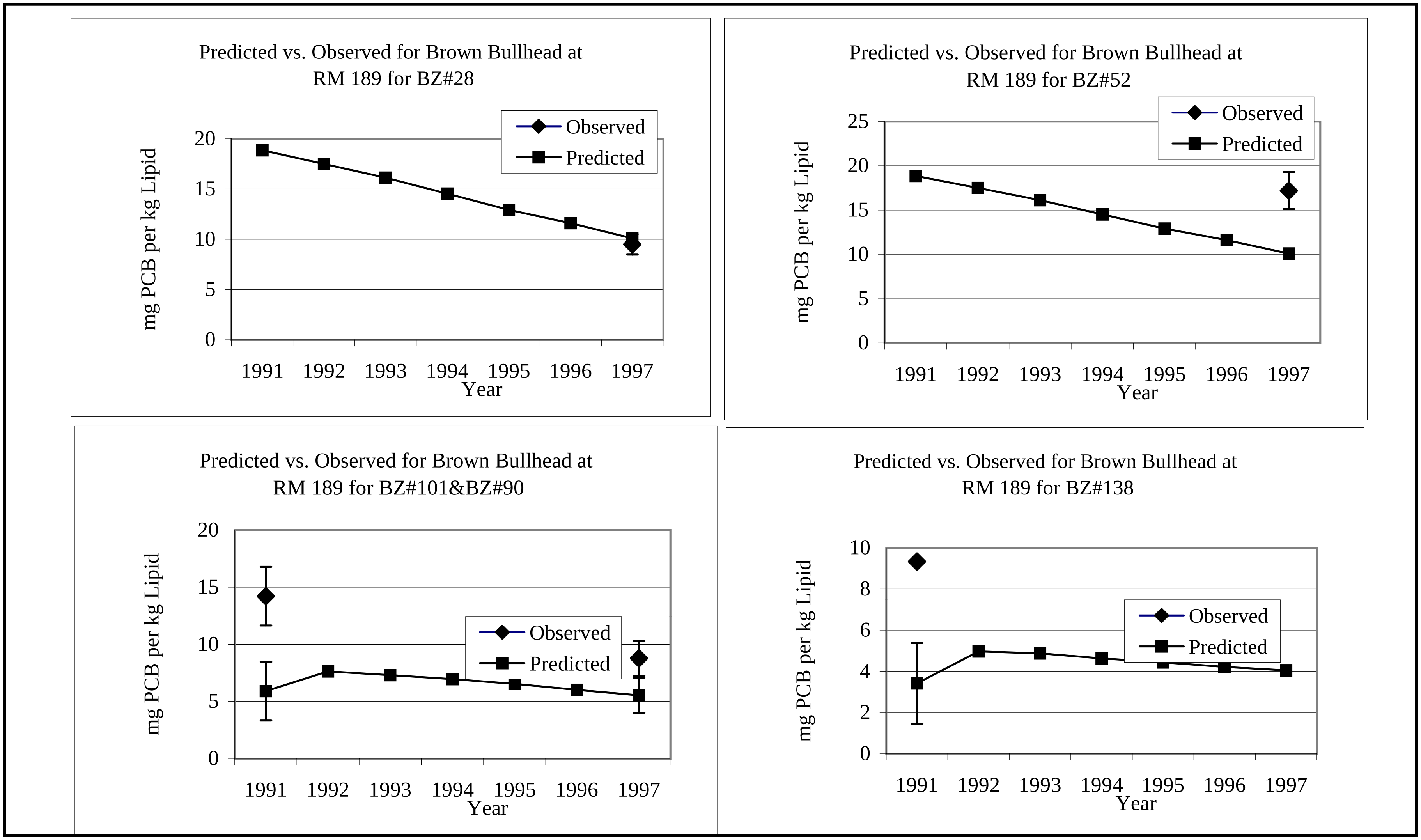


Figure 10-8: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Brown Bullhead at RM 168

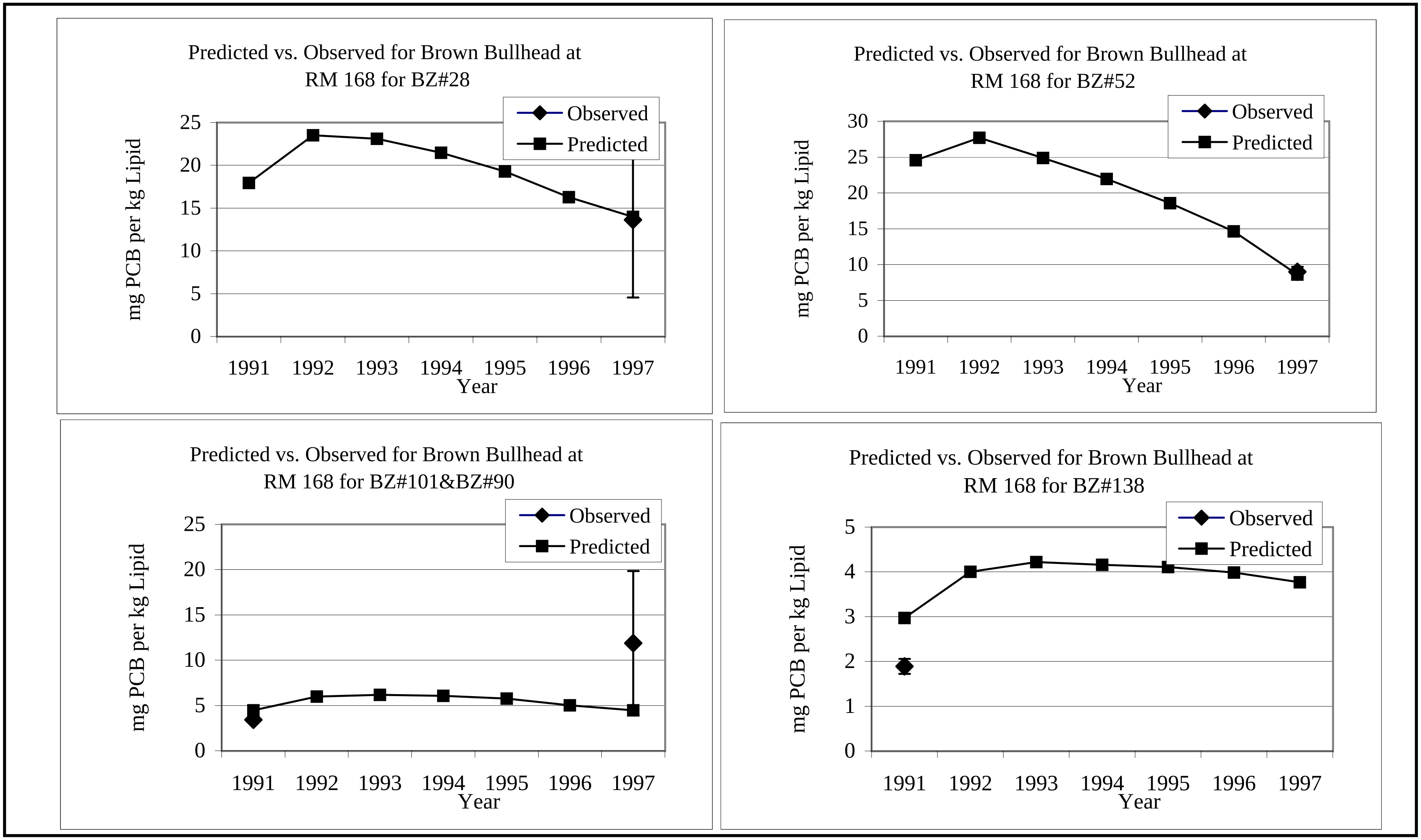


Figure 10-9: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Largemouth Bass at RM 189

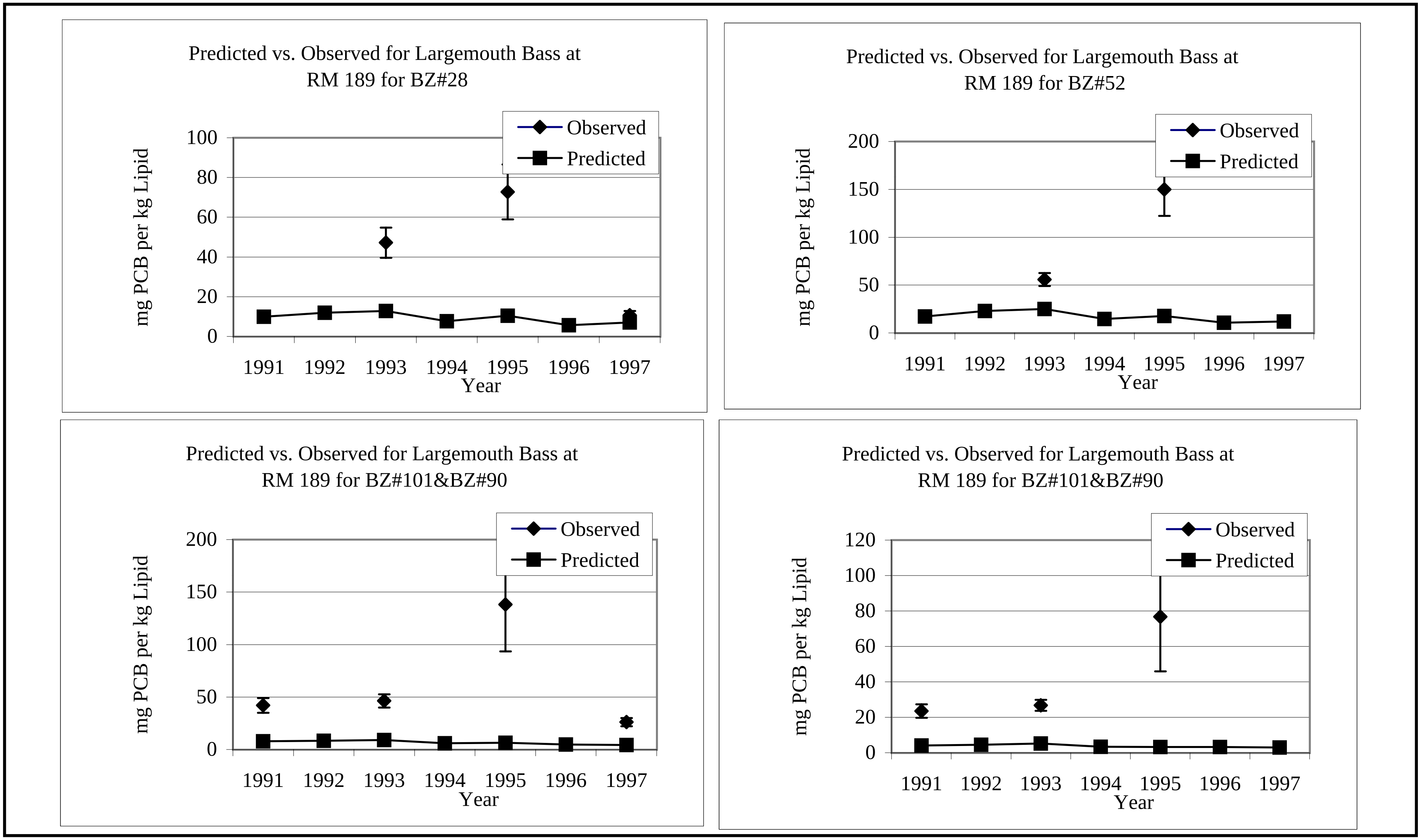


Figure 10-10: Predicted vs. Observed Congener Modeling Results
Lipid Normalized Largemouth Bass at RM 168

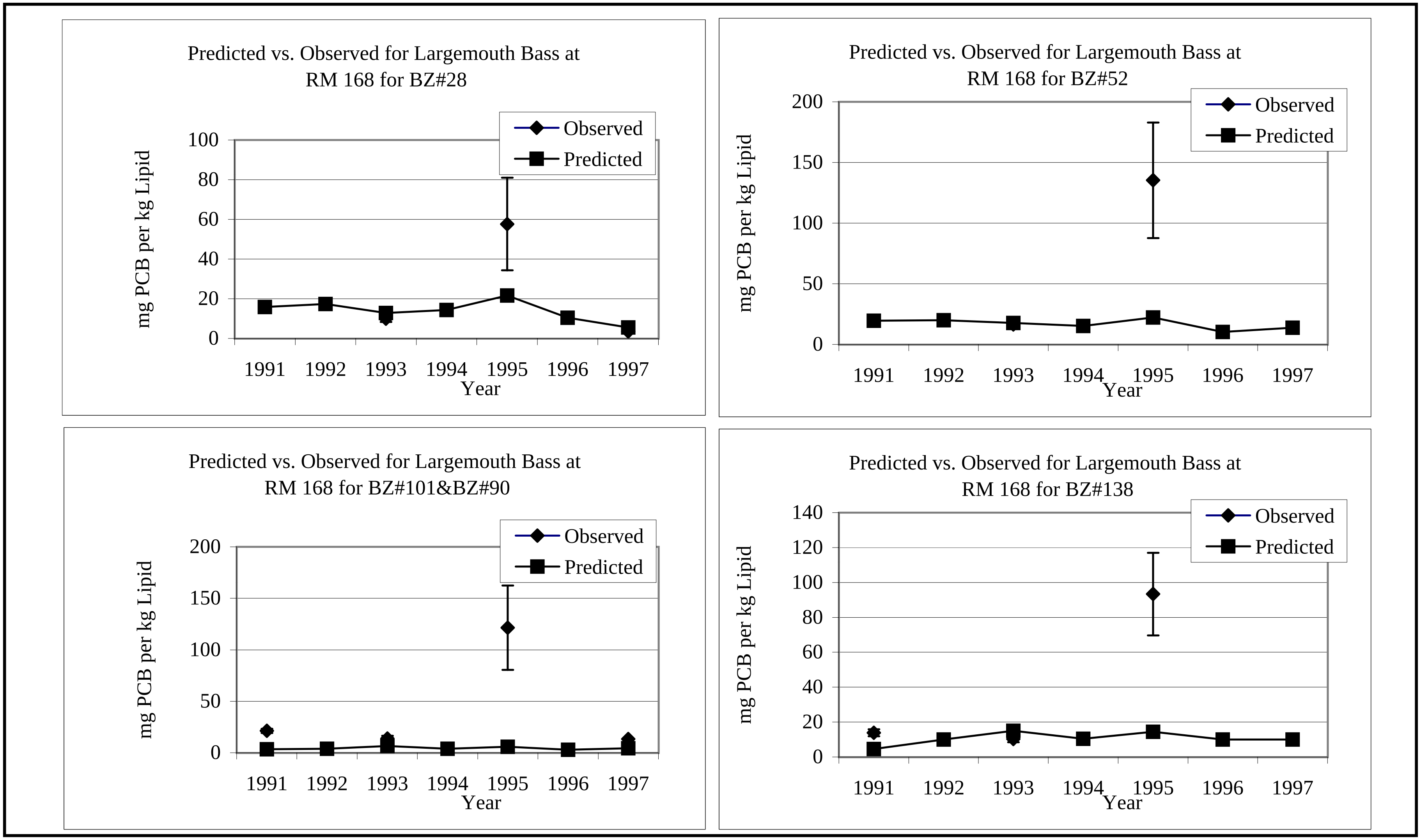


Figure 10-11: Predicted vs. Observed Congener Modeling Results
Wet Weight Pumpkinseed at RM 189

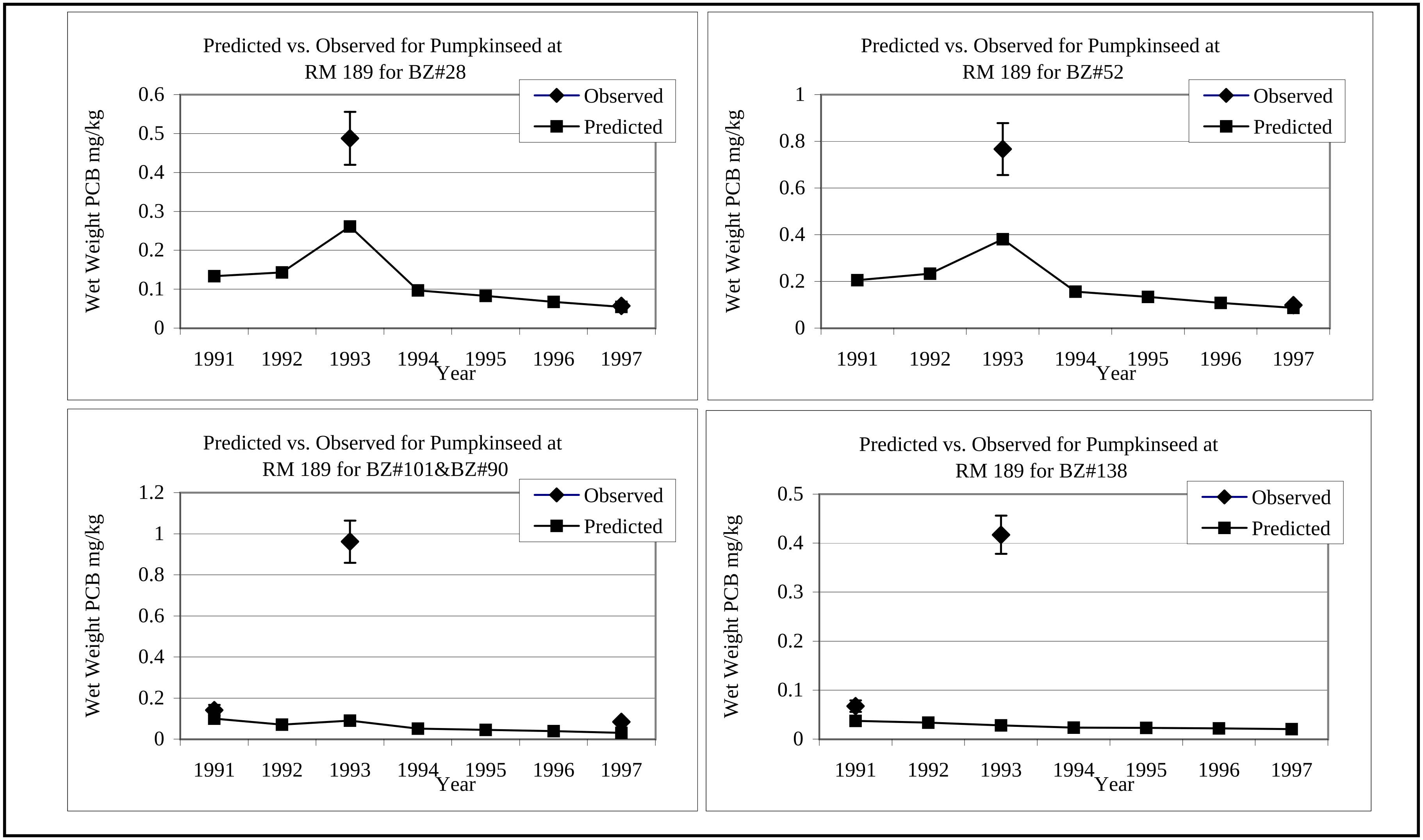


Figure 10-12: Predicted vs. Observed Congener Modeling Results
Wet Weight Pumpkinseed at RM 168

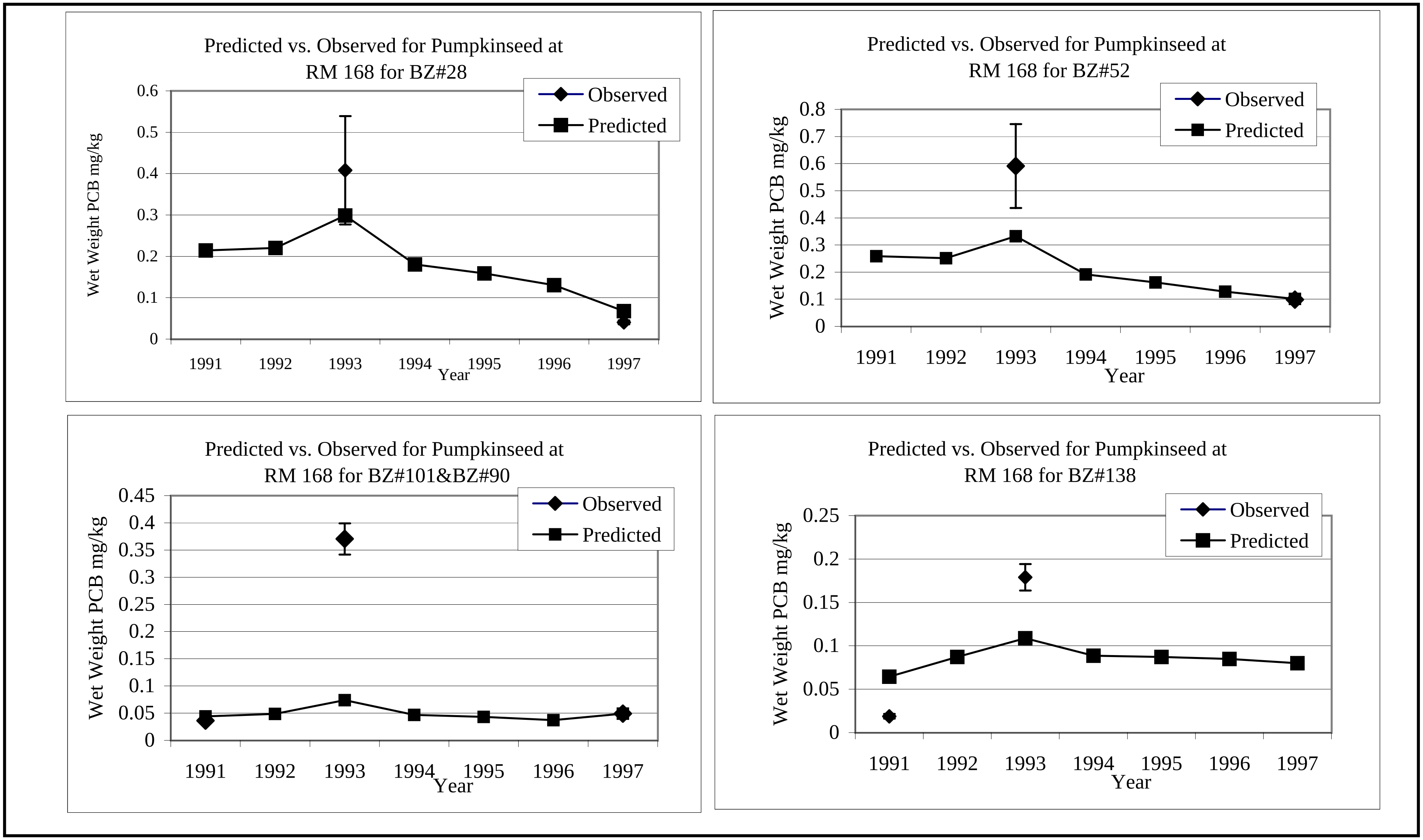


Figure 10-13: Predicted vs. Observed Congener Modeling Results
Wet Weight Spottail Shiner at RM 189

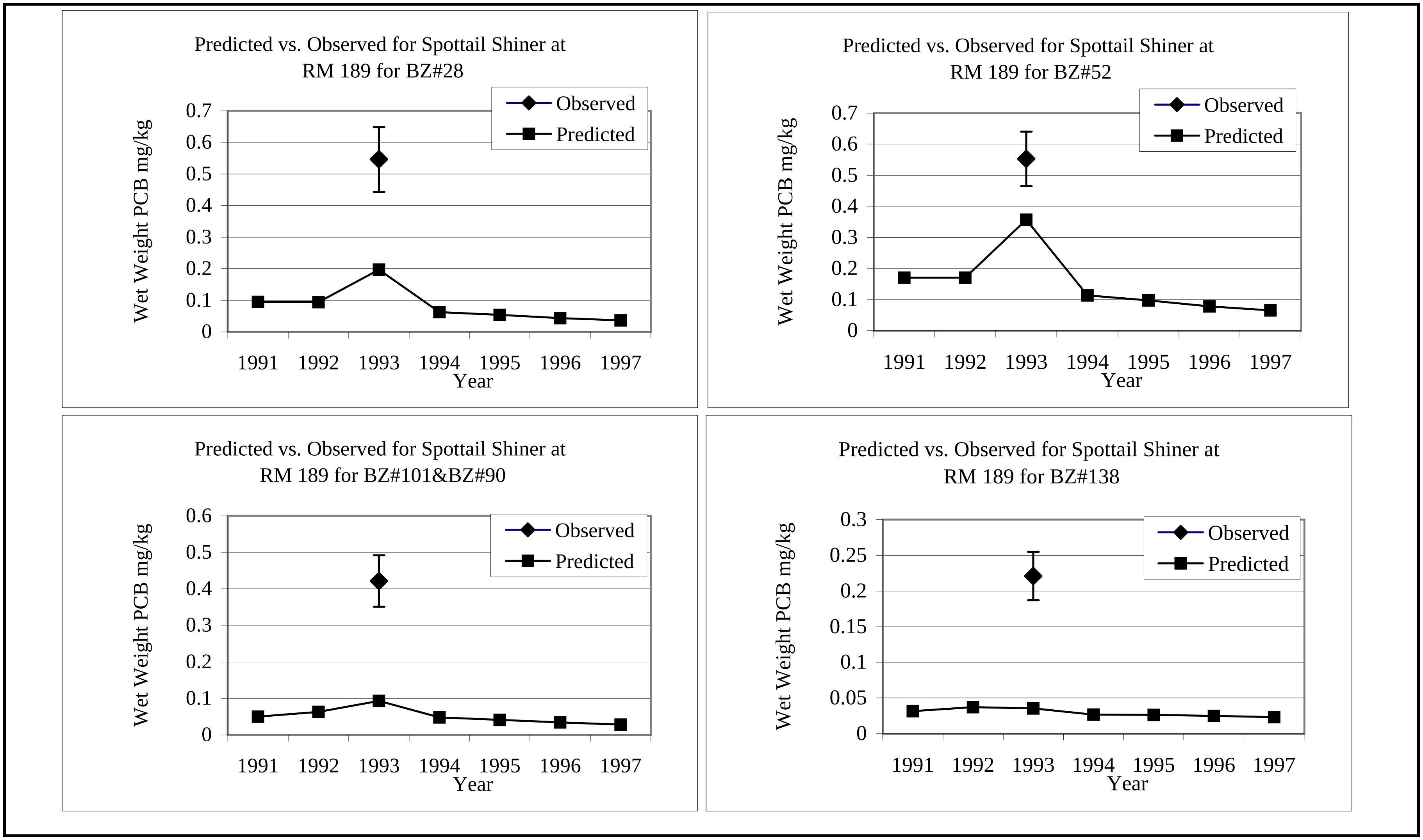


Figure 10-14: Predicted vs. Observed Congener Modeling Results
Wet Weight Spottail Shiner at RM 168

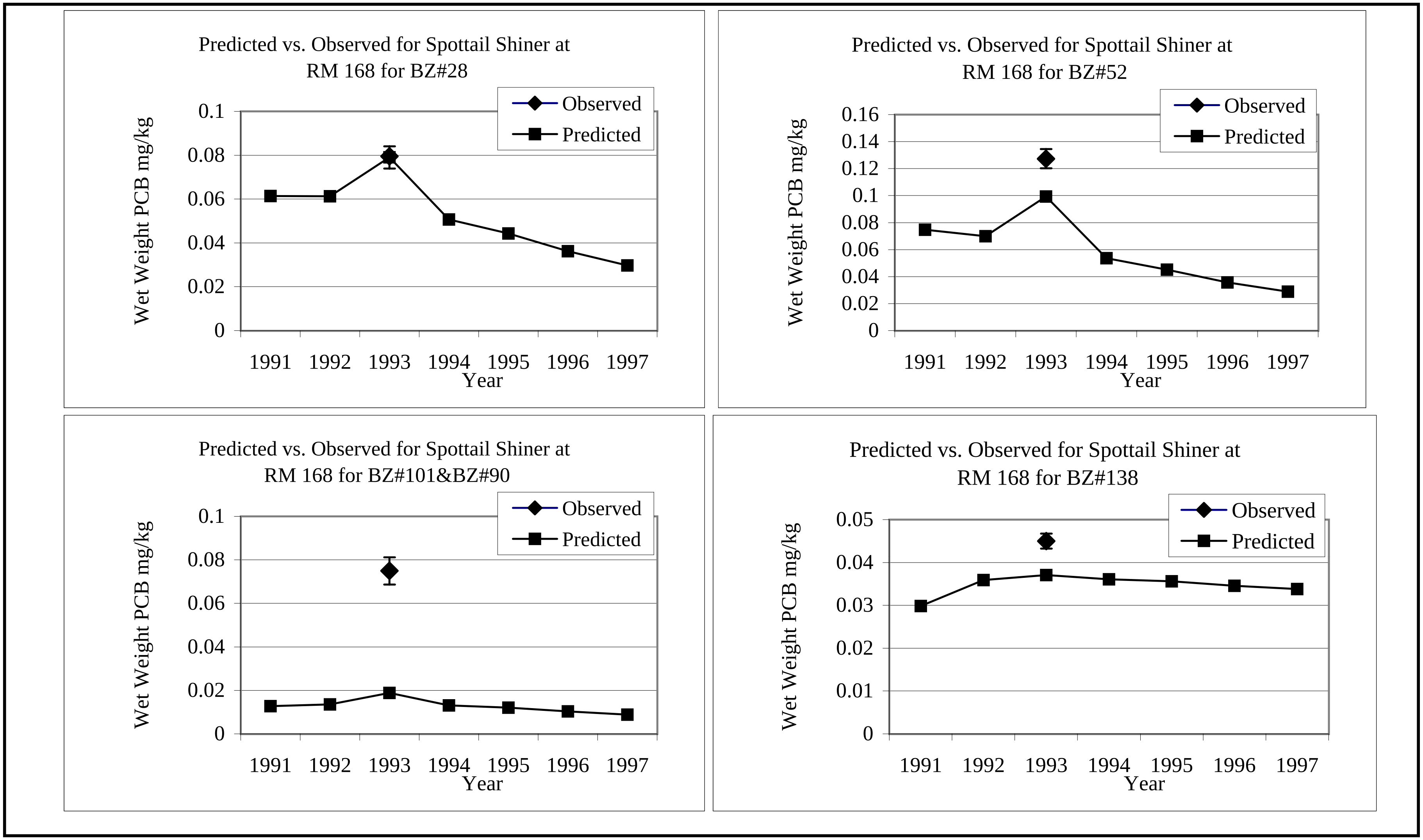


Figure 10-15: Predicted vs. Observed Congener Modeling Results
Wet Weight Yellow Perch at RM 189

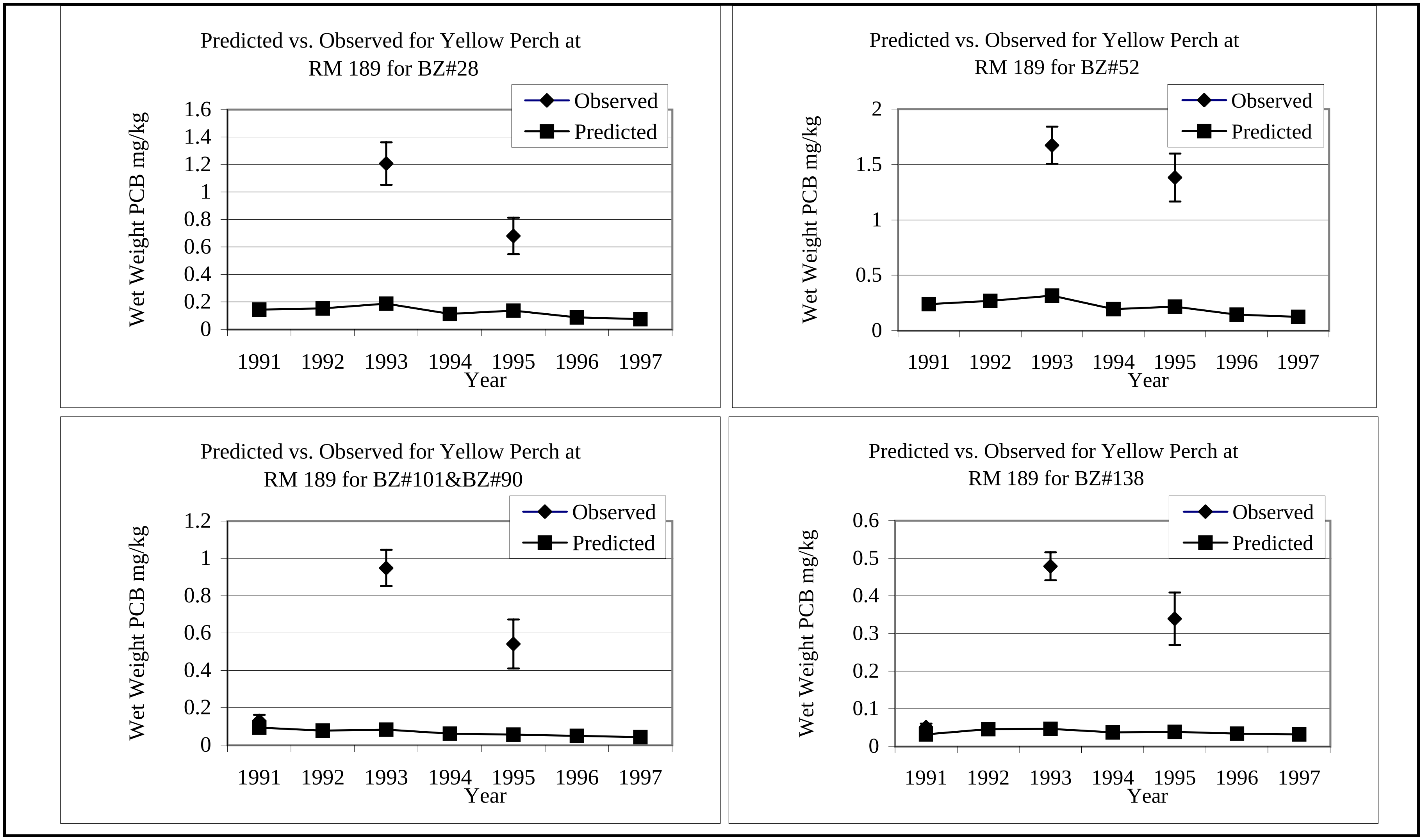


Figure 10-16: Predicted vs. Observed Congener Modeling Results
Wet Weight Yellow Perch at RM 168

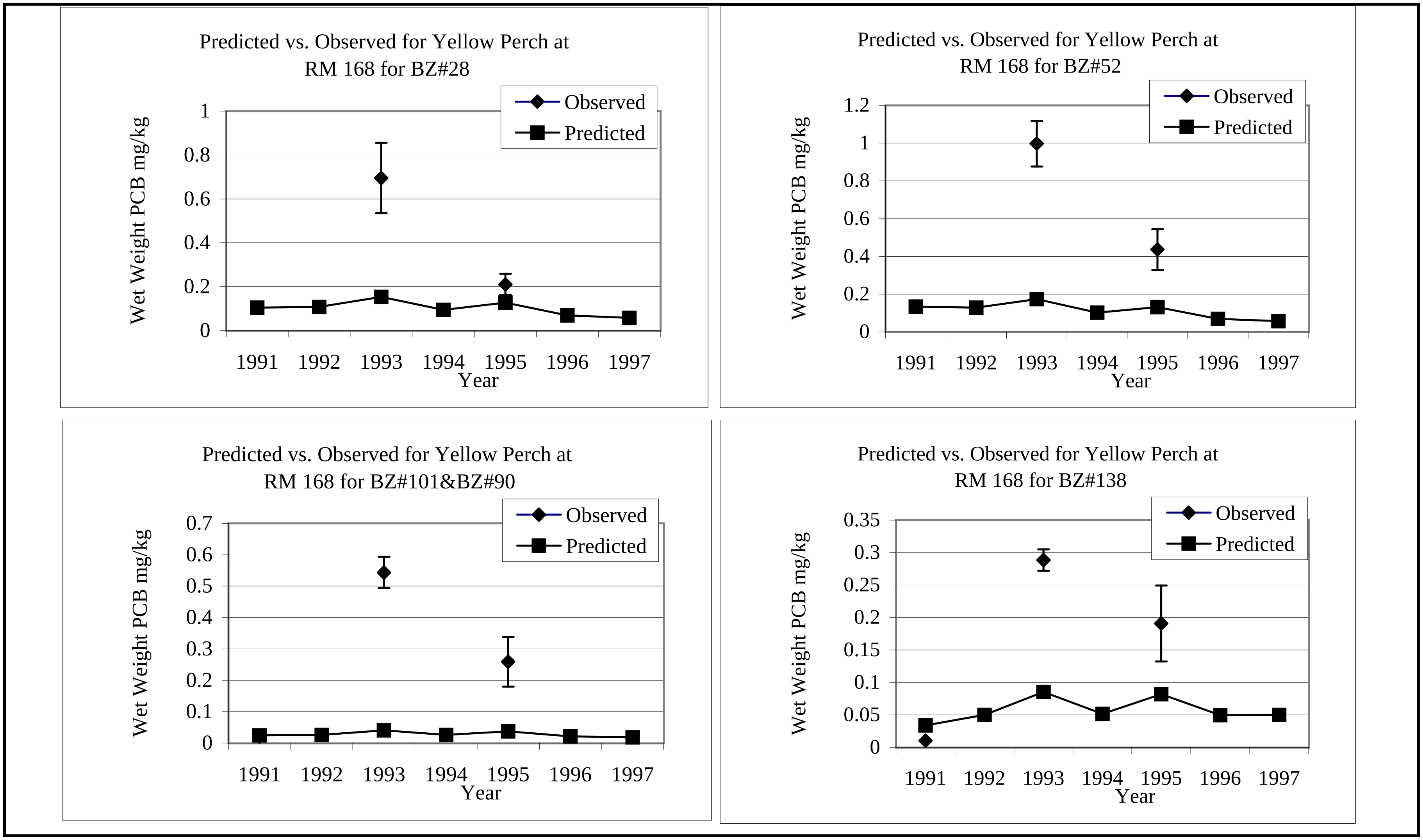


Figure 10-17: Predicted vs. Observed Congener Modeling Results
Wet Weight Brown Bullhead at RM 189

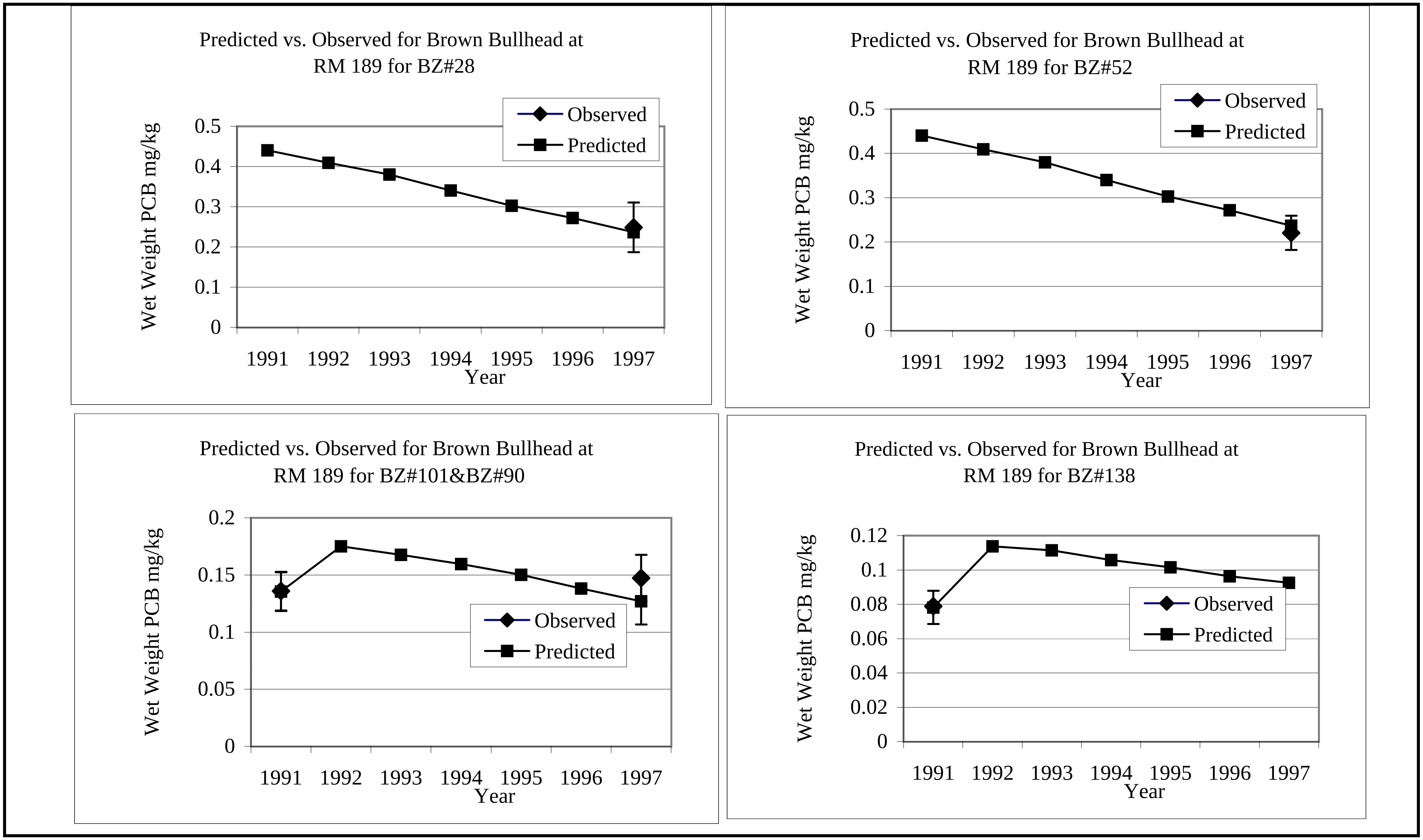


Figure 10-18: Predicted vs. Observed Congener Modeling Results
Wet Weight Brown Bullhead at RM 168

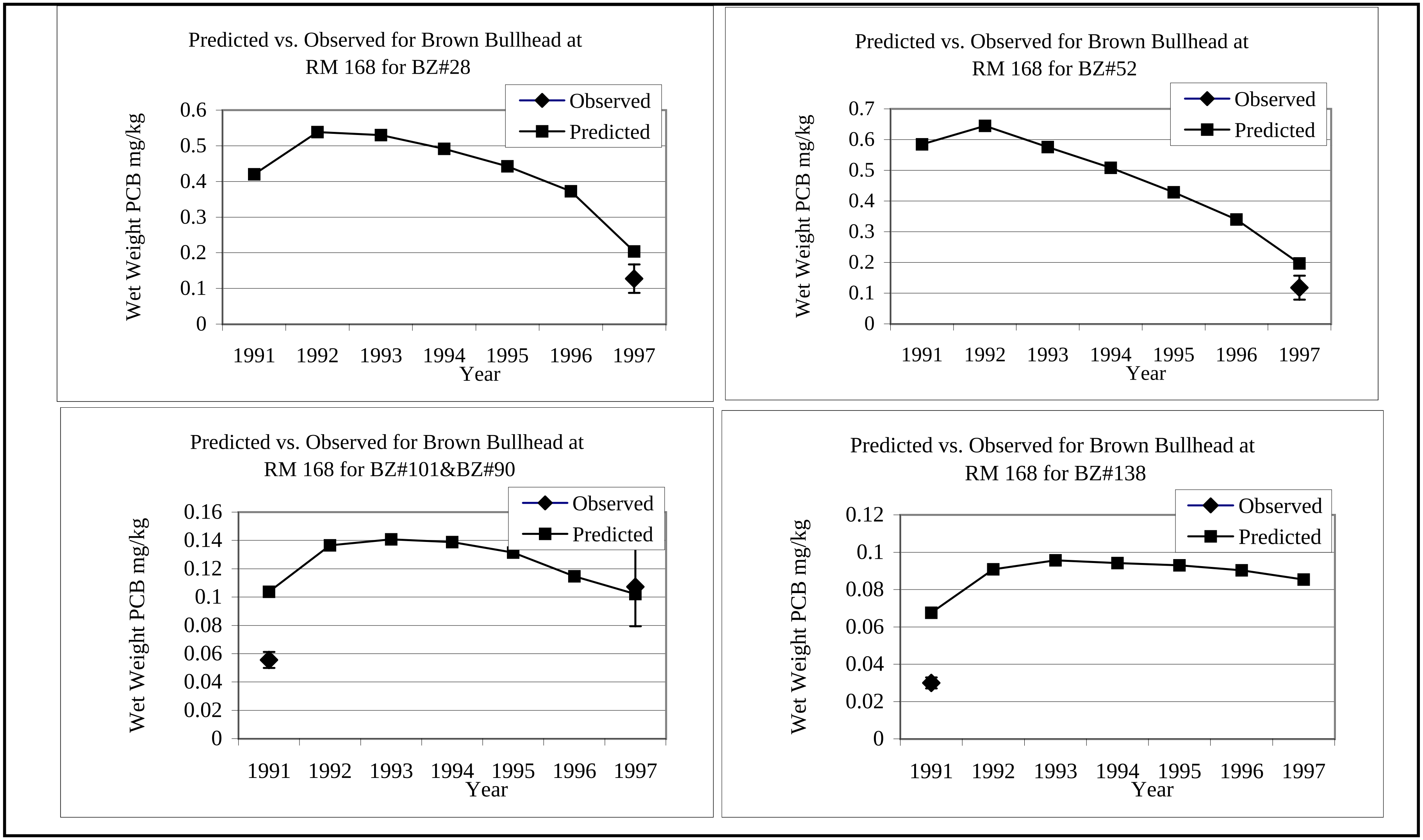


Figure 10-19: Predicted vs. Observed Congener Modeling Results
Wet Weight Largemouth Bass at RM 189

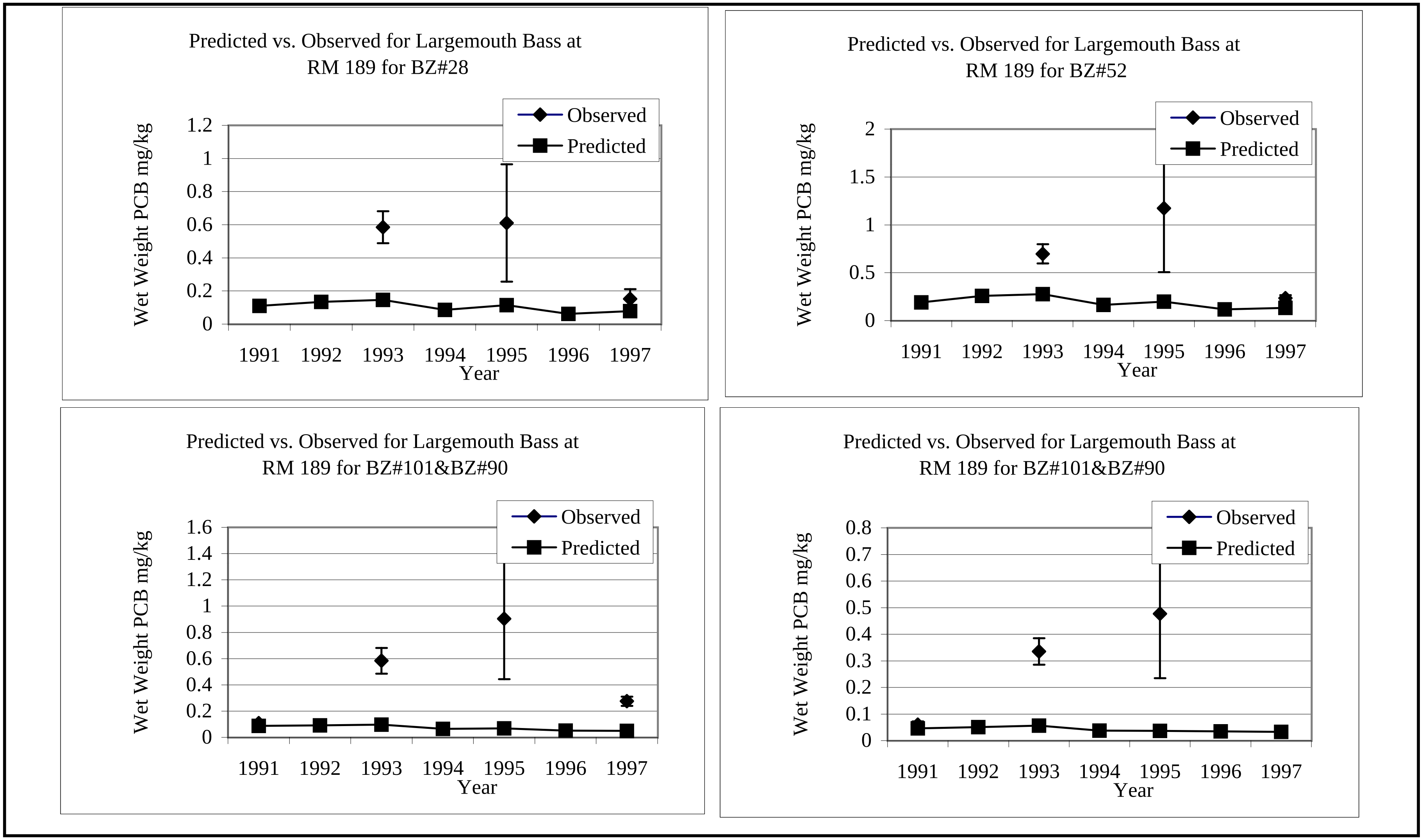


Figure 10-20: Predicted vs. Observed Congener Modeling Results
Wet Weight Largemouth Bass at RM 168

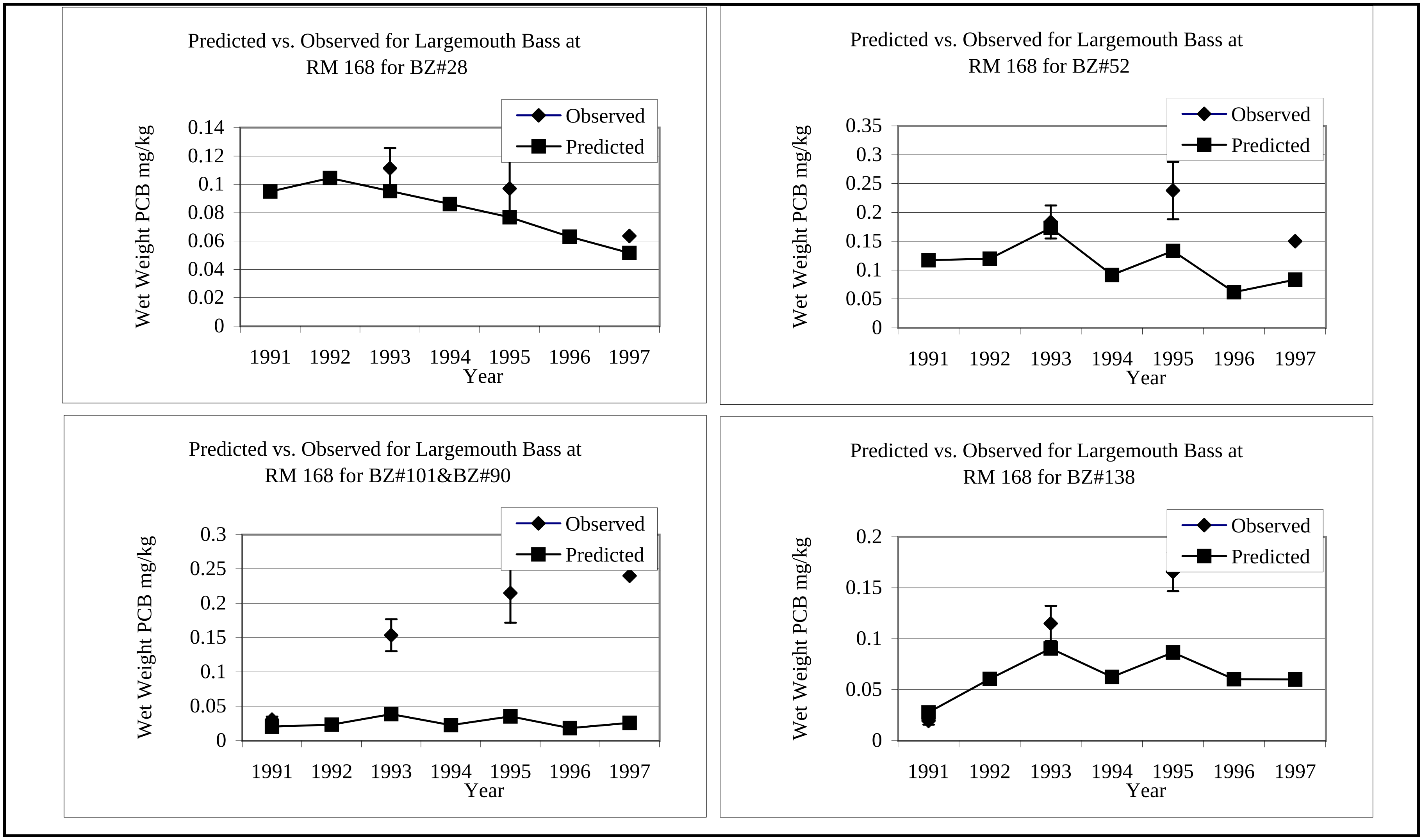


Table 10-3 provides the relative percent difference (RPD) between predicted and observed calculated as (predicted – observed)/observed * 100%. This table shows that the highest RPD for BZ#28 is for yellow perch, for which the FISHRAND model underpredicts the observed mean at all locations except for lipid normalized concentrations at RM 168. The model is within 20% of observed values for largemouth bass on a wet weight basis at RM 168, and within 60% on a lipid normalized basis. The model underpredicts observed largemouth bass concentrations at RM 189, but is within 34 % and 49% of observed means by 1997 for lipid normalized and wet weight predictions, respectively.

Note that the 1993 USEPA analyses isolate BZ#28, but that BZ#28 and BZ#50 coeluted in the NYSDEC reanalysis data set. Thus, the observed values are an overestimate of the true BZ#28 concentration in the mixture.

Results for BZ#52

Table 10-4 provides the wet weight comparisons for BZ#52. All comparisons are within a factor of approximately two or less except yellow perch. However, the observed yellow perch concentrations are unusually high, almost a factor of two higher than largemouth bass. In general, the model captures the trends in concentration as well. Not shown in the table are the comparisons for benthic invertebrates for which data are only available during 1993 at RM 189. The modeled mean is 0.53 ppm wet weight and the observed mean is 0.53 ppm wet weight.

Table 10-5 provides the lipid normalized comparisons for BZ#52. Again, most locations and species show predicted concentrations within a factor of two or less as compared to observed means. Not shown in the table are the comparisons for benthic invertebrates for which data are only available during 1993 at RM 189. The modeled mean is 17 ppm lipid normalized and the observed mean is 22 ppm lipid normalized.

Table 10-6 provides the RPD for BZ#52. RPDs are slightly higher than for BZ#28, but are lowest toward the end of the modeling period, and are lower for the wet weight based comparisons than for the lipid normalized comparisons.

Results for BZ#101&90

Wet weight comparisons are found in Table 10-7. The FISHRAND model significantly underpredicts concentrations in all species except pumpkinseed and brown bullhead (although pumpkinseed in 1993 is underpredicted). Not shown in the table are the comparisons for benthic invertebrates for which data are only available during 1993 at RM 189. The modeled mean is 0.11 ppm wet weight and the observed mean is 0.14 ppm wet weight.

Lipid normalized comparisons are found in Table 10-8. Again, the model significantly underpredicts observed concentrations. Not shown in the table are the comparisons for benthic invertebrates for which data are only available during 1993 at RM 189. The modeled mean is 4 ppm lipid normalized and the observed mean is 6 ppm lipid normalized.

The RPD between predicted and observed is presented in Table 10-9. The RPDs for this congener are higher than for the other congeners, and better towards the end of the modeling period for most species (1% for pumpkinseed and 5% for brown bullhead in 1997 at RM 168 on a wet weight basis; 31% for pumpkinseed at RM 168 lipid normalized; 0% and 14% for brown bullhead at RM 189 in 1991 and 1997 on a wet weight basis; 29%, 27% and 19% for pumpkinseed, yellow perch, and largemouth bass, respectively, during 1991 at RM 189 wet weight; and37% for brown bullhead in 1997 on a lipid normalized basis).

Results for BZ#138

Table 10-10 provides the wet weight comparisons. Pumpkinseed at RM 168 are better captured than at 189, where the model underpredicts. Predicted brown bullhead concentrations at RM 168 are higher than observed, while there is virtually no difference between predicted and observed at RM 189. Largemouth bass is better captured at RM 168, although the predicted mean is within 0.01 ppm of observed during 1991 at both locations. Not shown in the table are the comparisons for benthic invertebrates for which data are only available during 1993 at RM 189. The modeled mean is 0.05 ppm wet weight and the observed mean is 0.05 ppm wet weight.

Table 10-11 provides the lipid normalized comparisons. Again the model tends to underpredict, although is often within a factor of two for many species and years. Not shown in the table are the comparisons for benthic invertebrates for which data are only available during 1993 at RM 189. The modeled mean is 2.2 ppm lipid normalized and the observed mean is 2.2 ppm lipid normalized.

Table 10-12 shows the RPD between predicted and observed for BZ#138. Observed concentrations of BZ#138 are very low, so although the RPD for pumpkinseed in 1991 is 240%, this difference is only 0.04 ppm or a factor of three difference between predicted and observed concentrations. Wet weight based comparisons are significantly closer than lipid normalized based comparisons.

There is greater uncertainty associated with the exposure concentrations predicted by the HUDTOX model for BZ#138, particularly sediment concentrations. This is reflected in the higher RPD for brown bullhead than has been observed for other congeners. However, the mean predicted benthic invertebrate concentration at RM 189 during 1993 is virtually identical to the observed mean, suggesting that the sediment exposure field for benthic invertebrates has been accurately captured.

Discussion

Overall, the congener-specific model performs best for brown bullhead, largemouth bass, and pumpkinseed. Data are only available for spottail shiner for one year and the model performs well for BZ#28, and BZ#138, but tends to underpredict concentrations for the remaining congeners. The model tends to underpredict for yellow perch, but captures BZ#52, and BZ#101&90 during 1991. The model captured the observed 1993 benthic invertebrate concentrations well for all congeners.

It is not surprising that model predictions fit data better for BZ#28 and BZ#52 than for BZ#101&90 and BZ#138. The first two congeners are major components by weight percent of the PCB Tri+ mixture observed in the Hudson River, and have chemical properties similar to that of the mixture. The latter two congeners are relatively minor components of Tri+, and have chemical properties that tend to deviate from those of the mixture. As a result, the HUDTOX fate and transport model, which was calibrated to Tri+, does a more reliable job of representing exposure concentrations of BZ#28 and BZ#52.

There are numerous uncertainties associated with the congener specific data and modeling results. First, the data are obtained from two separate sources: the USEPA Phase 2 sampling effort in 1993, and the New York State Department of Environmental Conservation reanalysis of archived samples from 1991 – 1997. The quantitation technique used by NYSDEC differs from that in the USEPA program, and there is no correction available as for the Tri+ results. The NYSDEC program also shows a different coelution scheme due the difference in quantitation technique. For example, some samples for BZ#101 and BZ#90, which are always coeluted in the USEPA dataset and were modeled together, were presented separately in the NYSDEC dataset and were summed for this analysis. As can be seen from the tables, there have not been many congener-specific analyses of fish tissues over the years, unlike for Tri+. Thus, temporal patterns and changes are more difficult to discern. Significant variability is also evident in the congener-specific results. For example, for BZ#28 for pumpkinseed at both river miles 189 and 168, the concentration drops approximately a factor of 20 in less than five years. Many of the predicted means show very large error bars, and typically sample sizes are very small.

As requested by the peer reviewers, the K_{ow} for the congener-specific modeling has been fixed at a single point estimate. However, as mentioned previously, K_{ow} is a temperature dependent process and no one K_{ow} would be applicable throughout the year. In addition, there is uncertainty in the specific K_{ow} value that is selected, even at a typical temperature (chosen as 25° C or a typical summer temperature during periods of highest growth and feeding). For example, we were able to find three different K_{ow} values in the literature for one congener and temperature.

There are some differences between wet weight and lipid normalized based comparisons. Lipid content in fish is the single most significant contributor to predicted PCB body burden and, as discussed previously, there is significant year-to-year variability in lipid content. While the range and the shape of the distribution remain fairly consistent, the central tendency can vary from year to year, and a 1% difference in lipid between what the model assumes and what has been observed makes a significant difference in the predicted wet weight concentration.

There is not enough information available to adequately constrain congener-specific inputs for modeling forecasts. In addition, neither the ecological nor human health risk assessors have toxicity information available for specific congeners of interest. Thus, congener-specific forecasts were not provided.

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EXECUTIVE SUMMARY

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

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

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

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

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

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

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

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

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

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

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

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

1.0 INTRODUCTION

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

The January 2000 “Revised Baseline Modeling Report” (RBMR) (TAMS et al., 2000a)

The February 2000 “Responsiveness Summary” for the BMR (TAMS et al., 2000b)

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

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

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

1.1 Background

In 1983, EPA classified approximately 200 miles of the Hudson River in the state of New York—from Hudson Falls to New York City—as a Superfund site, because of elevated concentrations of polychlorinated biphenyls (PCBs) in the river’s sediments. The sediments are believed to have been contaminated by discharges of PCBs over approximately 30 years from two General Electric (GE) capacitor manufacturing plants, one in Hudson Falls and the other in Fort Edward. After an initial assessment, EPA issued an "interim No Action decision" in 1984 for the contaminated sediments of the Hudson River PCBs site.

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

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

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

1.2 Scope of the Peer Review

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

1.2.1 Selecting the Reviewers

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

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

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

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

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

1.2.2 Briefing the Reviewers

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

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

The May 1999 "Baseline Modeling Report" (BMR) (Limno-Tech et al., 1999)

The July 1999 release of "PCBs in the Upper Hudson River" prepared for GE by Quantitative Environmental Analysis, LLC (QEA) (QEA, 1999)

The August 1998 release of the "Database for the Hudson River PCBs Reassessment RI/FS" (TAMS, 1998)

The November 1998 "Report of the Hudson River PCBs Site Modeling Approach Peer Review" (ERG, 1998)

The June 1999 "Report on the Peer Review of the Data Evaluation and Interpretation Report and Low Resolution Sediment Coring Report for the Hudson River PCBs Superfund Site" (ERG, 1999)

Executive summaries from other Phase II reports

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

Suggested charge questions for the RBMR peer review submitted to EPA via public comment

- The February 2000 “Response to Peer Review Comments on the Preliminary Model Calibration Report” (TAMS et al., 2000c)

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

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

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

1.2.3 The Peer Review Meeting

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

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

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

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

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

1.3 Report Organization

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

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

2.0 RESPONSES TO SPECIFIC QUESTIONS REGARDING THE FATE AND TRANSPORT MODELS

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

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

2.1 Responses to Question 1

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

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

Most peer reviewers agreed that the basic process representations in EPA's HUDTOX model—mass balances of water, sediment, and PCBs—are compatible in a general sense (EB, SE, PL, WL, RNa), but they had many comments on detailed aspects of the process representations, as described below. As a general point, one reviewer thought issues other than the compatibility of process representations should be considered when evaluating whether the

HUDTOX model is adequate for answering the principal study questions (RNa). For instance, he suggested that model adequacy could be better evaluated by considering whether uncertainties introduced through model calibration are so large as to compromise the model's ability to characterize the Hudson River. Other reviewers agreed with this point (SE, PL).

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

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

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

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

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

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

2.2 Responses to Question 2

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

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

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

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

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

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

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

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

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

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

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

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

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

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

2.3 Responses to Question 3

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

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

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

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

2.4 Responses to Question 4

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

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

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

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

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

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

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

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

2.5 Responses to Question 5

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

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

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

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

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

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

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

- *Model performance for individual congeners.* One reviewer thought the sediment-water mass transfer coefficient, by virtue of being fit to data for Tri+ PCBs, might not realistically portray mass transfer processes for individual congeners (RNo). Referring to Figure 6-56 in the RBMR (Volume 2), this reviewer noted that the empirically derived coefficient tends to overpredict sediment-water mass transfer for lower chlorinated congeners and underpredict transfer for higher chlorinated congeners.[3] Though not disagreeing with this concern, another reviewer commented that modeling individual congeners can be an extremely difficult task (PL). This reviewer explained that certain congener-specific properties vary with the composition of other congeners present and with concentration, thus complicating efforts to assign basic parameters.

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

2.6 Responses to Question 6

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

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

[3] This reviewer found the data depicted in Figure 6-56 (Volume 2) suspect. He wondered how the fate and transport model could successfully characterize congener-specific trends in the hindcast simulations, given that the sediment-water mass transfer coefficients were either overstated or understated (depending on the congener considered).

EPA could conclude that the mechanistic processes represented in the coefficient also are highly temperature dependent.

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

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

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

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

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

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

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

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

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

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

2.7 Responses to Question 7

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

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

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

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

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

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

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

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

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

2.8 Responses to Question 8

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

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

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

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

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

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

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

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

2.9 Responses to Question 9

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

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

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

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

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

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

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

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

  One reviewer argued that the uncertainty in model results is probably relatively low (and the accuracy relatively high) for predicting conditions in the first years of the forecast period, and that the uncertainty increases (and the accuracy decreases) for future years (RNo). Other reviewers agreed, but suggested that EPA attempt to quantify the uncertainty by running additional sensitivity analyses considering various future scenarios (e.g., significant changes in the watershed) (RNa) and that EPA prominently acknowledge the inherent uncertainty in the model, possibly by including caveats on all conclusions relevant to when PCB levels will reach certain values (SE,PL). The reviewers revisited the issue of uncertainty associated with forecasts when discussing general question 2 (see Section 4.2).

2.10 Responses to Question 10

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

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

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

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

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

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

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

2.11 Responses to Question 11

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

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

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

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

2.12 Responses to Question 12

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

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

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

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

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

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

3.0 RESPONSES TO SPECIFIC QUESTIONS REGARDING THE BIOACCUMULATION MODELS

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

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

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

3.1 Responses to Question 1

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

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

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

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

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

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

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

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

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

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

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

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

3.2 Responses to Question 2

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

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

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

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

3.3 Responses to Question 3

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

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

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

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

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

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

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

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

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

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

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

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

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

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

3.4 Responses to Question 4

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

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

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

3.5 Responses to Question 5

The reviewers briefly discussed charge question 5, which asked:

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

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

4.0 RESPONSES TO GENERAL QUESTIONS REGARDING THE BMR

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

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

4.1 Responses to General Question 1

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

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

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

4.2 Responses to General Question 2

The second general question asked the reviewers:

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

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

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

4.3 Responses to General Question 3

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

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

4.4 Responses to General Question 4

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

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

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

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

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

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

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

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

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

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

5.0 REVIEWERS' OVERALL RECOMMENDATIONS

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

5.1 Recommendations to EPA

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

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

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

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

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

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

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

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

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

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

reviewers recommended that EPA consider incorporating bed load transport (following approaches similar to those used in the HEC-6 model) into the HUDTOX model to assess the importance of this process.

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

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

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

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

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

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

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

5.2 Peer Reviewers' Final Statements

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

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

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

5.2.1 Final Statements for the Fate and Transport Models

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

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

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

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

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

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

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

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

5.2.2 Final Statements for the Bioaccumulation Models

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

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

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

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

incorporate more thorough lipid analyses; and improve model performance in predicting lipid-normalized PCB concentrations.

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

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

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

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

6.0 REFERENCES

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APPENDIX A

LIST OF EXPERT PEER REVIEWERS

United States
Environmental Protection Agency
Region 2

Peer Review of Hudson River PCBs Reassessment RI/FS Phase 2 Reports

Revised Baseline Modeling Report

Sheraton Saratoga Springs
Saratoga Springs, New York
March 27-28, 2000

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APPENDIX B

CHARGE TO EXPERT PEER REVIEWERS

Hudson River PCBs Site Reassessment RI/FS
Baseline Modeling Report
Peer Review 3

Charge for Peer Review 3

This is the third in a series of four peer reviews being conducted on scientific work products prepared for the Reassessment Remedial Investigation and Feasibility Study (Reassessment) for the Hudson River PCBs site. Previous peer reviews were conducted on the modeling approach and the Data Evaluation and Interpretation Report and Low Resolution Sediment Coring Report. Subsequent to this peer review the Human Health and Ecological Risk Assessments will be peer reviewed.

Members of this peer review are asked to determine whether the baseline modeling effort presented in the Revised Baseline Modeling Report (Revised BMR) is credible and whether the conclusions of the Revised BMR are valid. The reviewers are asked to determine whether the modeling work is technically adequate, competently performed, properly documented, satisfies established quality requirements, and yields scientifically credible conclusions. The peer reviewers are not being asked whether they would have conducted the work in a similar manner. In addition, the reviewers are asked to determine whether the models and the associated findings are appropriate to help answer the following three principal study questions that EPA will consider in its decision-making process for the site:

1. When will PCB levels in fish meet human health and ecological risk criteria under continued No Action? (1)

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

3. Could a flood scour sediments, exposing and redistributing buried contamination?

(1) Appropriate levels to meet human health and ecological risk criteria will be evaluated in the upcoming Feasibility Study.

(2) The Revised BMR represents a baseline modeling effort, and therefore does not include an evaluation of potential remedial scenarios. The modeling work presented in the Revised BMR will be used to develop potential remedial options in the Feasibility Study for the Reassessment.

The following documents will be provided to the peer reviewers:

Primary
Revised Baseline Modeling Report (Jan. 2000)
Responsiveness Summary to the Baseline Modeling Report (Jan. 2000)

Reference
Baseline Modeling Report (May 1999)
QEA/GE - PCBs in the Upper Hudson River (May 1999, amended July 1999)
Suggested charge questions from the public (Dec. 1999)
Hudson River Reassessment Database (August 1998)
Executive Summaries for other EPA Reassessment Reports
Peer Review Reports from first two peer reviews

The peer reviewers should base their assessments primarily on the Revised BMR, and on EPA's Responsiveness Summary for the Baseline Modeling Report, in which EPA responded to significant public comments received by the Agency on the May 1999 Baseline Modeling Report. These two documents are currently in preparation, and will be issued to the peer reviewers by the end of January 2000. The reference documents listed above are being provided to the reviewers as background information, and may be read at the discretion of the reviewers, as time allows, although the reviewers are not being asked to conduct a review of any of the background information. It should be noted that the Revised BMR to be issued in January 2000 will supercede the May 1999 Baseline Modeling Report.

For additional background information, please visit USEPA's web site on the Hudson River PCBs site, www.epa.gov/hudson.

Specific Questions

Fate and Transport (HUDTOX)

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

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

9. HUDTOX represents the Upper Hudson River by segments of approximately 1000 meters in length in the Thompson Island Pool, and by segments averaging over 4000 meters (ranging from 1087 to 6597 meters) below the Thompson Island Dam. Is this spatial resolution appropriate given the available data? How does the spatial resolution of the model affect the quality of model predictions?

10. Is the model calibration adequate? Does the model do a reasonable job in reproducing the data during the hindcast (calibration) runs? Are the calibration targets appropriate for the purposes of the study?

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

12. Are there factors not explicitly accounted for (*e.g.*, bank erosion, scour by ice or other debris, temperature gradients between the water column and sediments, etc.) that have the potential to change conclusions drawn from the models?

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

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

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

16. The HUDTOX model uses three-phase equilibrium partitioning to describe the environmental behavior of PCBs. Is this representation appropriate? (Note that in a previous peer review on the Data Evaluation and Interpretation Report and the Low

Resolution Sediment Coring Report, the panel found that the data are insufficient to adequately estimate three-phase partition coefficients.)

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

18. The model forecasts that a 100-year flood event will not have a major impact on the long-term trends in PCB exposure concentrations in the Upper Hudson. Is this conclusion adequately supported by the modeling?

Bioaccumulation Models

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

2. Was the FISHRAND calibration procedure appropriately conducted? Are the calibration targets appropriate to the purposes of the study?

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

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

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

General Questions

1. What is the level of temporal accuracy that can be achieved by the models in predicting the time required for average tissue concentrations in a given species and river reach to recover to a specified value?

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

3. It is easy to get caught up with modeling details and miss the overall message of the models. Do you believe that the report appropriately captures the "big picture" from the information synthesized and generated by the models?

4. Please provide any other comments or concerns with the Revised Baseline Modeling Report not covered by the charge questions, above.

Recommendations

Based on your review of the information provided, please identify and submit an explanation of your overall recommendation for each (separately) the fate and transport and bioaccumulation models.

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

APPENDIX A

MODEL VALIDATION
(for 1998 and 1999)

Introduction

Following completion of the Revised Baseline Modeling Report (RBMR) USEPA decided it would be appropriate to conduct a validation of the models with the most recent data to determine how successful the model is in predicting fish tissue concentrations beyond the calibration period (1977 to 1997).

In order to conduct the validation, actual values for upstream PCB loads, water flow and sediment loads were used in the fate and transport model, HUDTOX. The results from HUDTOX were then input into FISHRAND, which is the bioaccumulation model developed for the Reassessment.

HUDTOX Validation

The peer review panel agreed that the USEPA fate and transport model adequately reproduced historical data for the Upper Hudson River over the 21-year calibration period from 1977 to 1997. The panel did express concern, however, that because the model uses empirical relationships rather than a more mechanistic approach in characterizing some of the sediment-water transfer processes in the river, there is uncertainty associated with model forecasts of future PCB concentrations. USEPA acknowledges that the "No Action" 70-year forecasts included in the RBMR are uncertain not only because of uncertainties inherent in the model, but also because of uncertainties in required model inputs for future flows, solids loadings and PCB loadings.

The RBMR included validation of the HUDTOX fate and transport model to water column data collected by GE in 1998, the first year following the historical calibration period. Subsequent to publication of the RBMR, new data collected by GE in 1999 and additional USGS data for river flows became available. The HUDTOX validation in the RBMR was extended through December 31, 1999, to include these new data and to strengthen the validity of the model as a forecast tool. Because water column PCB concentrations in the Upper Hudson River are driven by both upstream loadings and releases from the sediments, a successful model validation that uses actual upstream flows and loadings strengthens the scientific credibility of the model, especially its representation of sediment-water mass transfer processes.

HUDTOX Application

Comparisons between model results and observational data were conducted for the period of January 1, 1998 through December 31, 1999 to correspond to the availability of USGS flow records at Fort Edward. The daily USGS flow records were used in the model for hydrology and to compute upstream solids and PCB loadings.

Observations of total suspended solids (TSS) and Tri+ PCB water column concentrations at Fort Edward were both available. The sampling frequency was variable. The model upstream boundary conditions for TSS and Tri+ PCB (sum of trichloro and higher homologues) were developed using the methods presented in the RBMR. For TSS, the measured concentration was used on the days for which data were available and the regression developed in Section 6.5.3.1 of the RBMR was applied on days when data were not available. The Tri+ PCB loads were linearly interpolated with the restriction that PCB concentrations reflecting pulse loading events were limited to a six-day duration (see Section 6.6.2.5 in the RBMR).

Tributary flows and solids loads were estimated using available data and the methods in Sections 6.4.3 and 6.5.3.2 in the RBMR, respectively. Tributary Tri+ PCB loads were assumed to be zero for the tributaries in the reaches of interest, Thompson Island Pool and Schuylerville. All other model parameters and coefficients were identical to those used in the 1977-1997 historical calibration period.

HUDTOX Results

The HUDTOX results for water column concentrations at Thompson Island Dam and at Schuylerville were compared to the available data, as shown in Figures A-1 and A-2. The model compares reasonably well to the data at both locations. Most of the PCB loading events are captured by the model. Isolated elevated concentrations at Thompson Island Dam were observed in April and May, 1999 that were not reproduced by the model. These are believed to be the result of pulse loadings upstream that were not captured by the sampling at Fort Edward and thus, were not incorporated into the upstream loading time series used as model input. Model results at low flow conditions (summer and fall) also match the data reasonably well, although the model results at Schuylerville tend to be biased higher than the results at Thompson Island Dam. This is most likely a consequence of greater model uncertainty in downstream reaches due to greater uncertainties in model inputs for flows and solids loadings.

This validation exercise confirms that the representation of sediment-water transfer processes in the HUDTOX model produces reasonable agreement between computed and observed water column PCB concentrations during both the 21-year historical calibration period and a subsequent two-year validation period. In particular, good agreement was obtained between computed and observed PCB concentrations during the summer months when concentrations are high and food chain exposure is most critical. These results strengthen the reliability and utility of the HUDTOX model as a tool to forecast future PCB exposure concentrations in Upper Hudson River water and sediment.

FISHRAND Validation

The calibrated FISHRAND model was run for 1998 – 1999 in a validation exercise. The model used the sediment and water concentrations generated by the HUDTOX model as exposure inputs. The development of the HUDTOX modeling results is described above. Averaging of segments from the HUDTOX model was the same as for the calibration period.

FISHRAND Results

Lipid-normalized mean model results for PCB Tri+ in fish are presented in Figures A-3 and A-4, along with the observed data (mean and one standard error). The FISHRAND model generates predicted fish concentrations on a monthly basis, and these are shown in the figures. Predictions and observations are generally consistent, with potential small within-year timing errors for concentration trends. As shown in Figure A-3, predicted brown bullhead concentrations at RM 189 are within 20% of observed and fall well within the error bars. Pumpkinseed at RM 189 and RM 168 are slightly overpredicted but on an annualized basis (i.e., average over the year) fall within the error bars of observed concentrations. A greater discrepancy between model predictions and observations is seen for largemouth bass at RM 189, although model predictions are within a factor of 2 of observed concentrations. Figure A-4 presents the lipid normalized results for largemouth bass and brown bullhead at RM 168 and RM 154. These results show that both largemouth bass and brown bullhead observed concentrations are reasonably well captured by the model at both locations. There are no benthic invertebrate data.

Wet weight mean model results, and the data (mean and one standard error) are presented in Figures A-5 and A-6. The mean observed lipid content was higher for 1998 and 1999 in largemouth bass and brown bullhead than the historical average. The graphs present the FISHRAND model results using the actual observed lipid for these two species in these years; results using the historical average of the lipid distribution would be approximately 50% lower. For example, the mean percent lipid from the distribution for largemouth bass is 1.1, while the mean from 1998 and 1999 is 1.5 and 1.9. If the predicted lipid normalized concentration is 600 mg PCB/kg lipid, and the mean lipid content is 1.1, then the predicted wet weight concentration is 6.4, while based on the observed mean lipid content of 1.9, the predicted wet weight concentration is 11.4, nearly a factor of two higher. There are no benthic invertebrate data for these years.

Discussion

These results show that the FISHRAND model performs well and reasonably captures observed fish concentrations for 1998 and 1999. The importance of year to year variability in lipid content in fish is highlighted by these results, which show good agreement on a lipid normalized basis but are slightly underpredicted on a wet weight basis. However, use of the actual observed lipid content shows that the model predicts the observed data to within the error bars at most locations and years for all three species.

In sum, the transport and bioaccumulation models perform well on validation to 1998-1999 data, which were not used for model calibration. The represents a test of the models' ability to extrapolate from new temporal conditions at the upstream boundaries. The 1998-1999 validation does not, however, necessarily confirm model capabilities for long term prediction. In particular, sediment concentrations for the validation period are expected to be very close to those at the end of the calibration period. USEPA believes that HUDTOX may be overly optimistic with respect to the long-term rate of decline in surface sediment exposure concentrations in cohesive areas, particularly at the localized scale at which resident fish feed. Such uncertainty in surface sediment exposure concentrations could impact projections of fish tissue concentrations further into the future. Therefore, an upper bound estimate was calculated, setting cohesive sediment surface concentrations based on the observed rate of decline of PCB levels in brown bullhead in the Thompson Island Pool. This is described in the Feasibility Study.

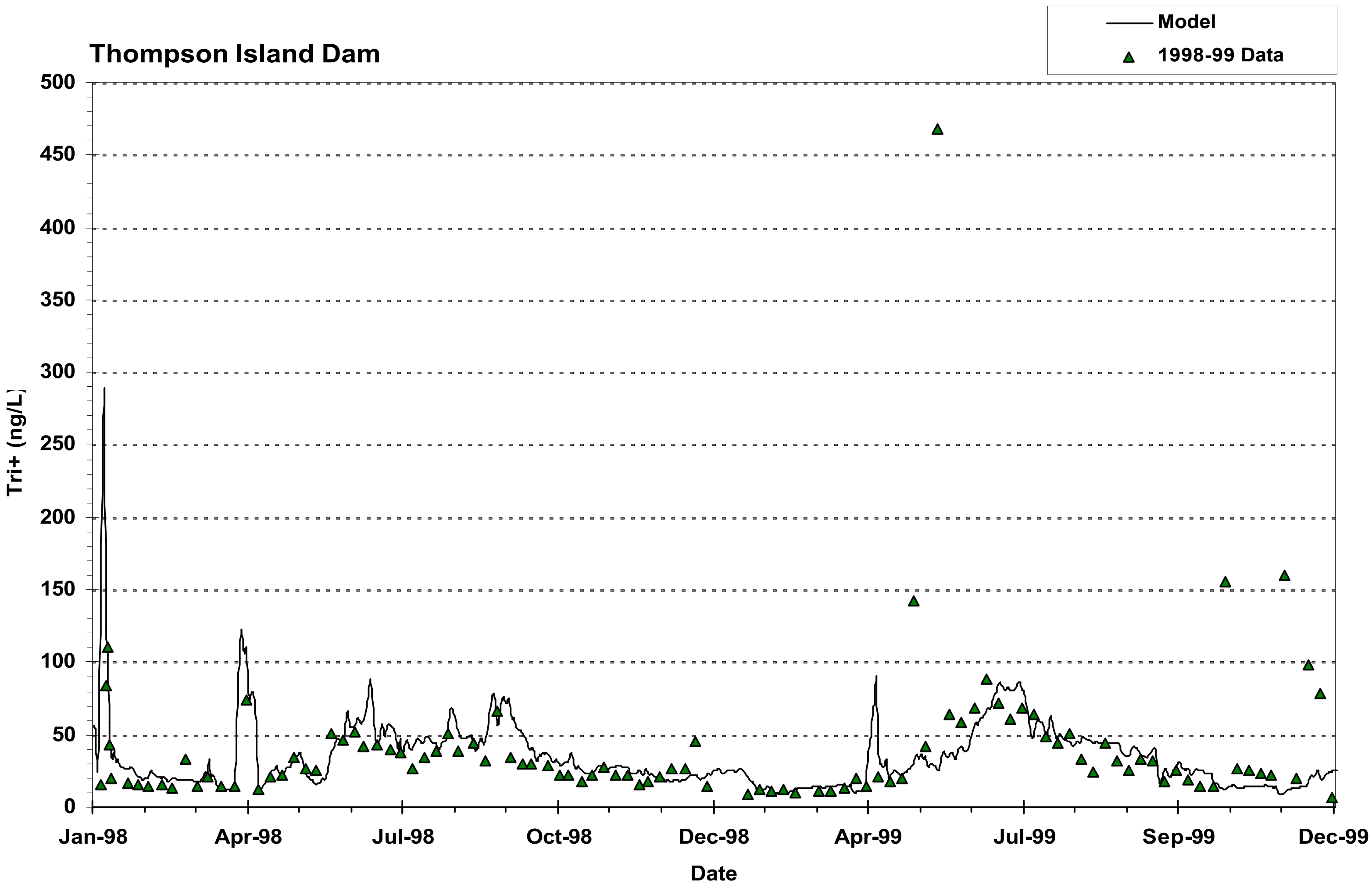


Figure A-1. HUDTOX Validation: Comparison of Predicted and Observed Water Column Tri+ PCB Concentrations at Thompson Island Dam, 1998-1999.

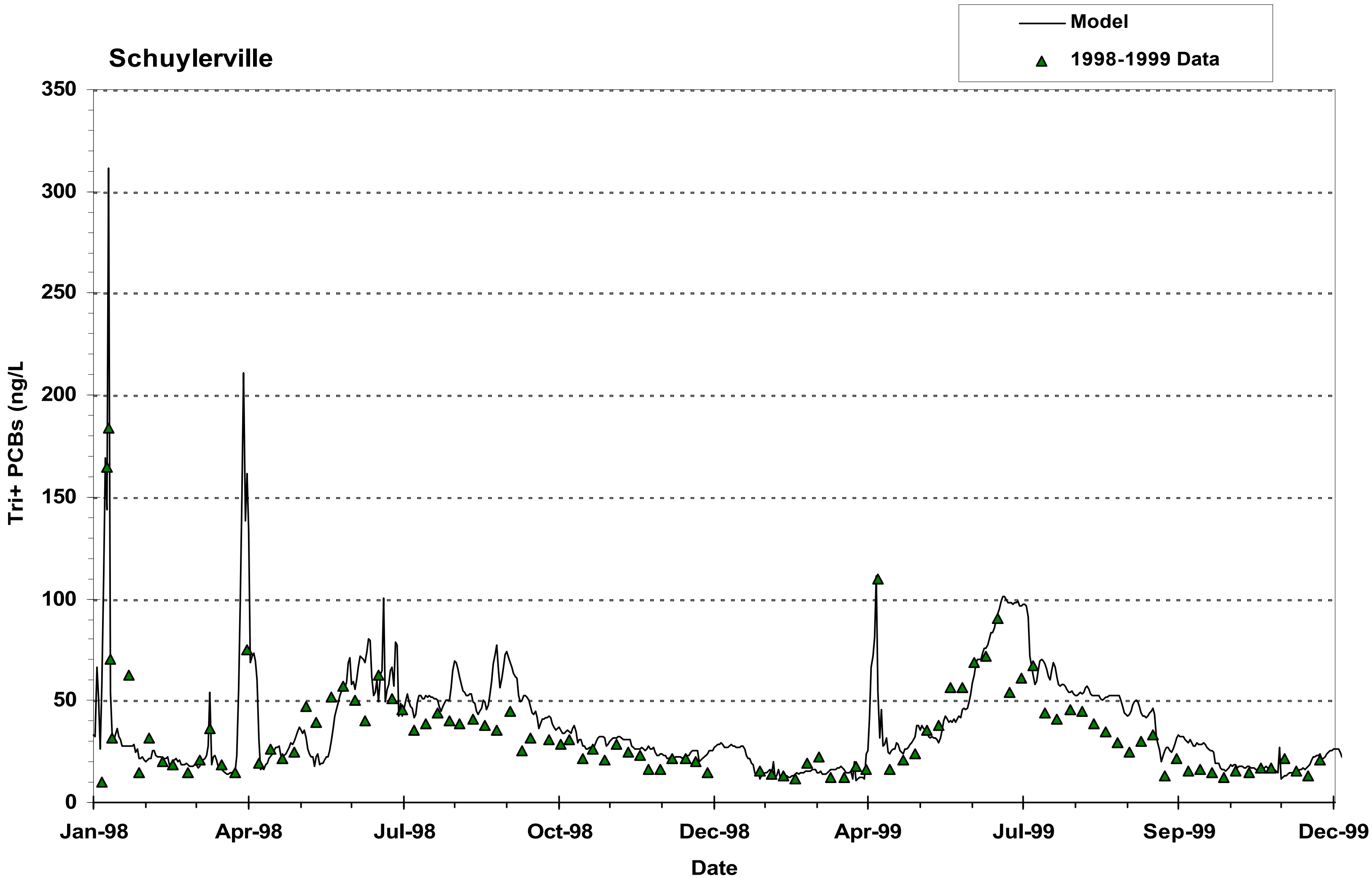


Figure A-2. HUDTOX Validation: Comparison of Predicted and Observed Water Column Tri+ PCB Concentrations at Schuylerville, 1998-1999.

Figure A-3: Lipid Normalized FISHRAND Model Validation Results

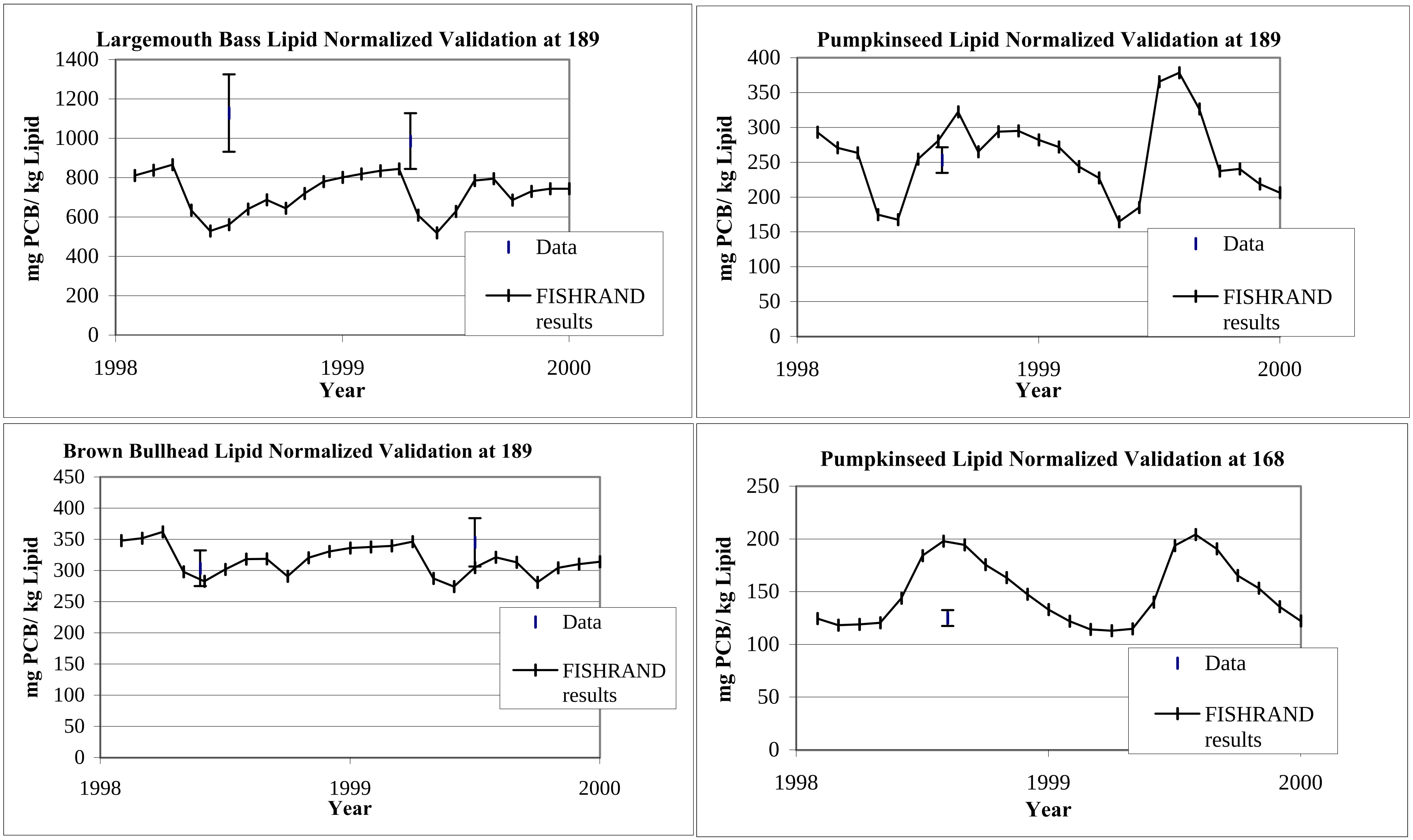
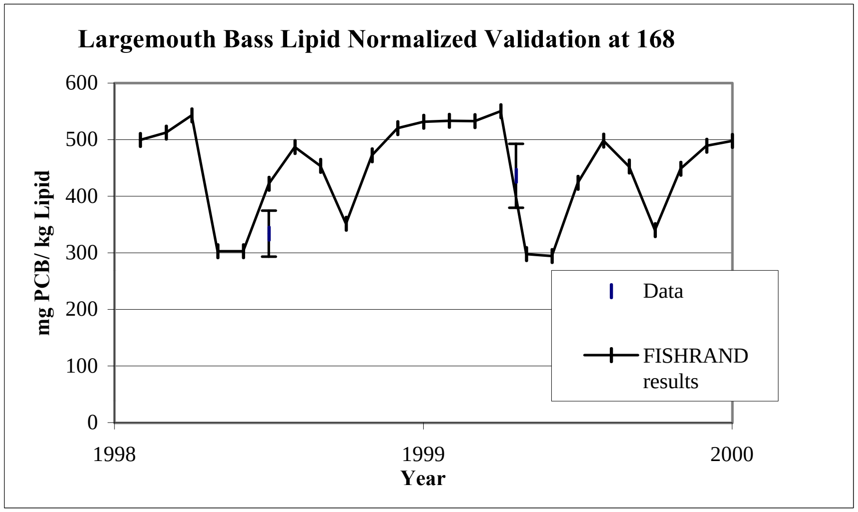
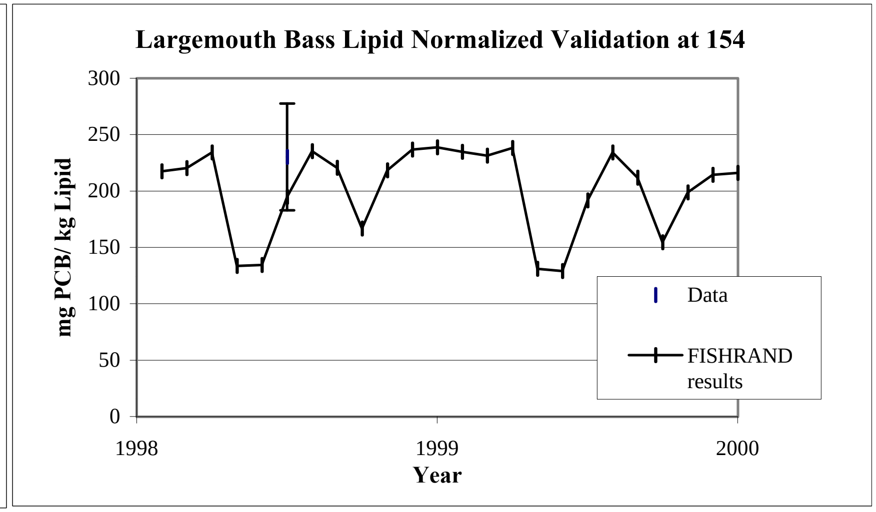
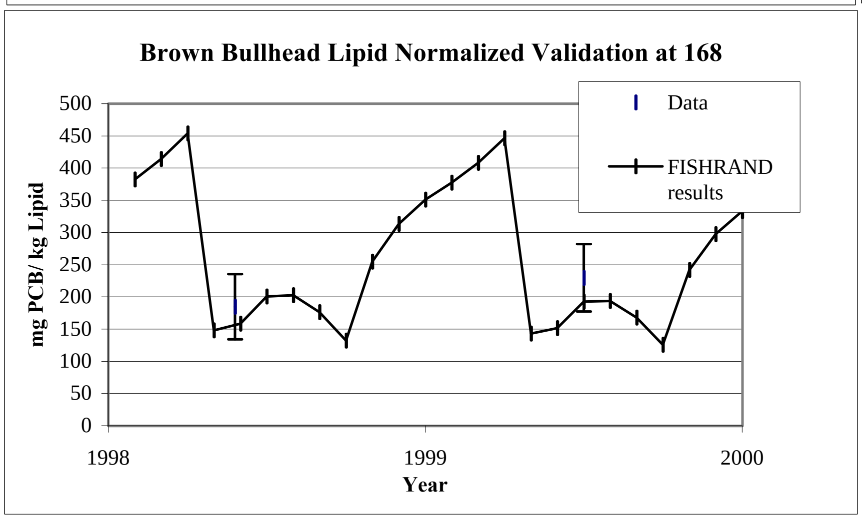


Figure A-4: Lipid Normalized FISHRAND Model Validation Results







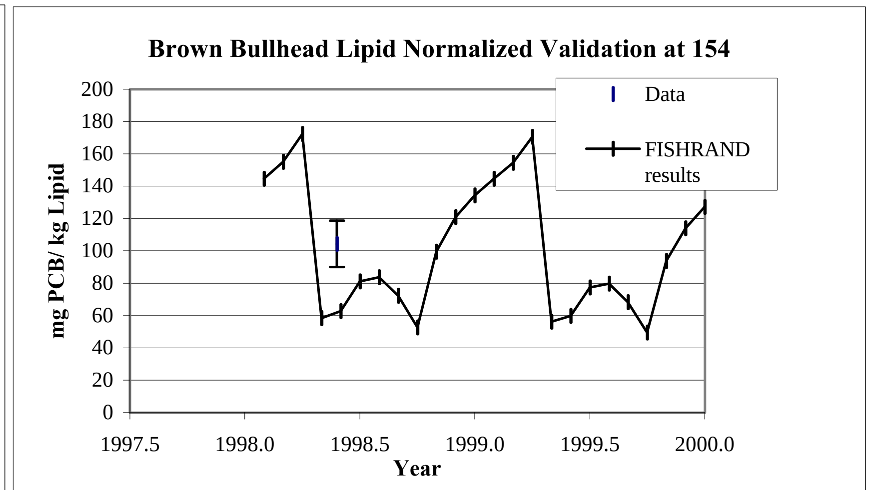
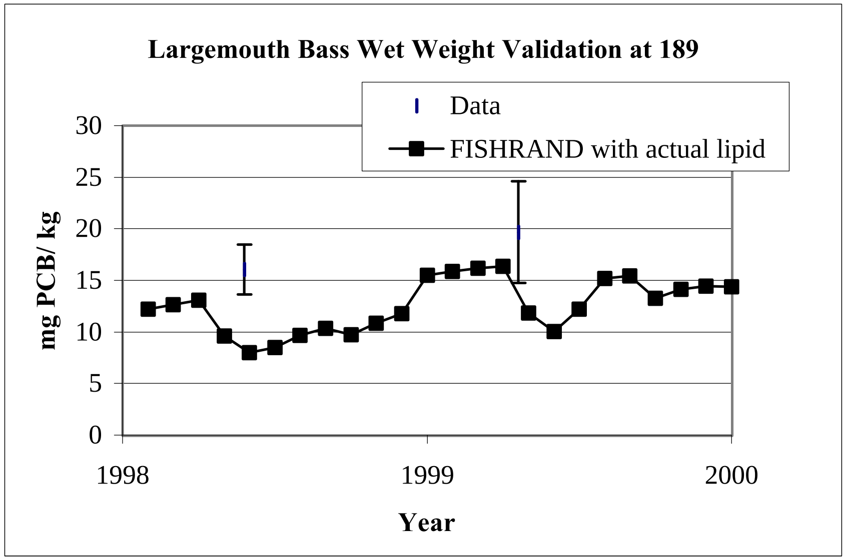
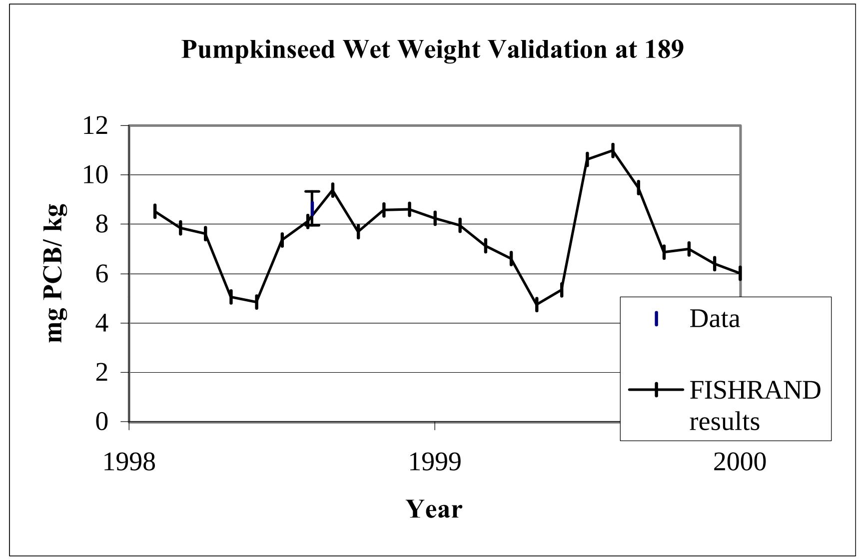
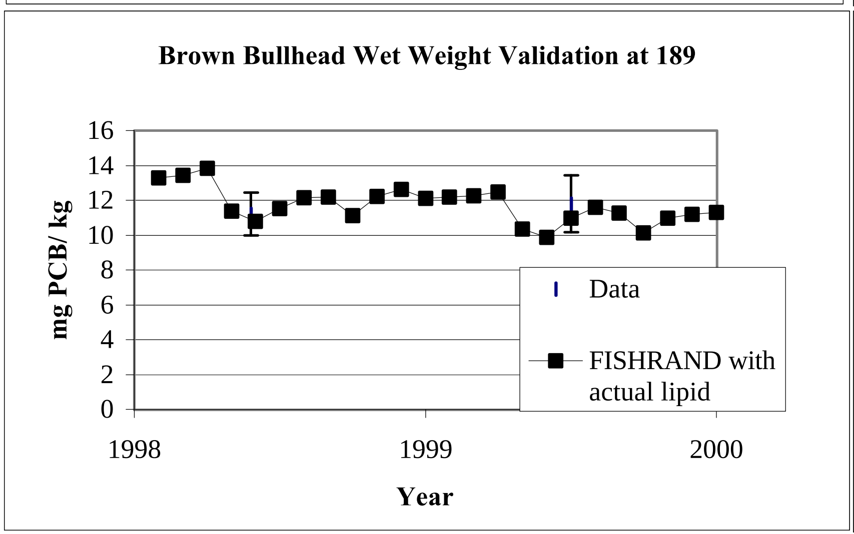


Figure A-5: Wet Weight FISHRAND Model Validation







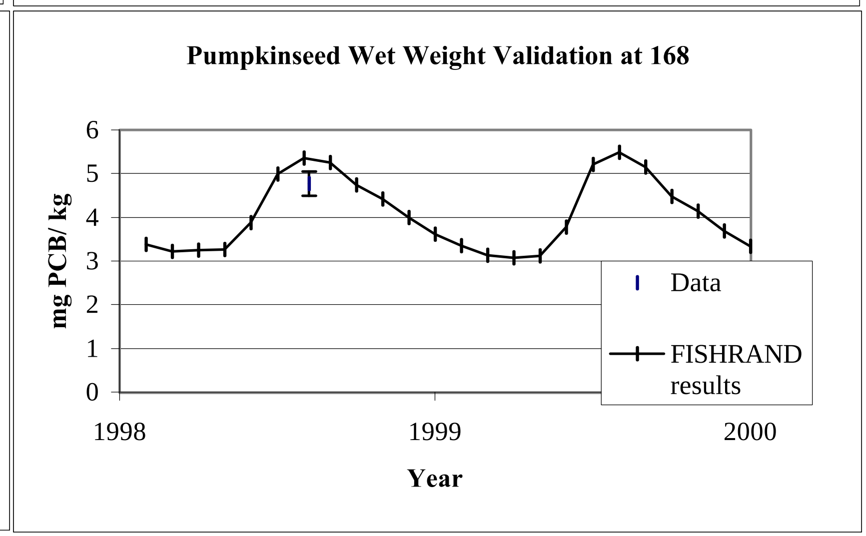


Figure A-6: Wet Weight FISHRAND Model Validation