John G. Haggard, Manager
Hudson River Program

General Electric Company
320 Great Oaks Office Park, Ste: 323
Albany, NY 12203
Fax: (518) 862-2731
Telephone: (518) 862-2739
Dial Comm: 8 232-2739*
E-Mail:John.Haggard@corporate.ge.com
Pager: 518-484-3177

December 19, 2000

Mr. Douglas J. Tomchuk
U.S. Environmental Protection Agency
Emergency & Remedial Response Div.
290 Broadway - 20th floor
New York, NY 10007-1866

RE: Response to EPA Comments on GE's Model

Dear Mr. Tomchuk:

General Electric's submitted to EPA a report describing its mechanistic PCB fate, transport and bioaccumulation models, May 1999. EPA presented a critique of this model in its BMR Responsiveness Summary dated February 22, 2000. GE believes that many of the issues identified in the Responsiveness Summary have no merit, and we take this opportunity to provide the Agency with formal responses to the Agency's comments. These responses are presented in the enclosed report.

Because GE believes that its model provides a superior tool for making a remedial decision for the Site, we once again urge EPA to consider using GE's model to supplement its analysis. At a minimum, the general consistency in the results of GE's and EPA's mechanistic models demonstrates that they are the best and only method for comparing the effectiveness of different remedies in controlling risks as compared against natural recovery. Please place a copy of this report into the administrative record. If you have any questions or would like to discuss this further, please do not hesitate to contact me.

Sincerely,

John G. Haggard

JGH/bg
Attachment

cc: Bill Ports – DEC
Bob Montione – DOH

RESPONSE OF THE GENERAL ELECTRIC COMPANY TO USEPA COMMENTS ON THE REPORT "PCBs IN THE UPPER HUDSON RIVER"

Prepared for:

General Electric Company
Albany, NY

By:

Quantitative Environmental Analysis, LLC

Job Number:
GENhud 131

December 2000

TABLE OF CONTENTS

TABLE OF CONTENTS (Cont.)

SECTION 1
INTRODUCTION

This document provides a review and response to comments of USEPA to the QEA Hudson River PCB fate, transport and bioaccumulation model document. As an appendix to its Responsiveness Summary (USEPA, 2000), the USEPA presented a commentary on the report entitled "PCBs in the Upper Hudson River" that was developed for GE by QEA. This commentary, which is included as an appendix to this document, was organized into nine Sections that roughly proceed in order through the report. Responses are presented in the same order as the comments.

SECTION 2
COMMENTS PERTAINING TO THE EXECUTIVE SUMMARY

Comment: (p. 2, USEPA 2000b)

While QEA's modeling effort is sophisticated and generally credible, the Executive Summary overstates the certainty and accuracy of the model. Because there remains considerable shortcomings in the theoretical aspects of the modeling, interpretation of data, and status of model calibration, the statement "There are no other means to perform such [remedial action] assessments at a comparable level of confidence" (Vol. ES, p. 1-2) is unwarranted.

Response:

The comment is primarily opinion. The referenced statement is based on the fact that the model is subject to explicit constraints that are stated and observable. These constraints include conformance to basic scientific principles such as conservation of mass, consistency with the spectrum of site-specific data collected on the Upper Hudson River (UHR) and consistency with current scientific understanding about PCB fate, transport and bioaccumulation processes. Other available means of assessment are subject to greater uncertainty because they are not constrained by **all** of the data or conformance with basic scientific principles. Further, they rely on assumptions that may not be evident and tend to be ignorant of the mechanisms responsible for PCB fate, transport and bioaccumulation. The model is the best available tool for remedial action assessment and USEPA has presented no alternative that is demonstrably better.

Comment: (p. 2, USEPA 2000b)

QEA overestimates future clean solids loads, and thus over-estimates rates of natural recovery.

Response:

As discussed in Section 5 of this document, USEPA's contention that the solids load is too high is based on an improper analysis of data. In fact, there is no credible evidence that the solids load has changed over time or that the load used in the QEA projections is too high.

Comment: (p. 2, USEPA 2000b)

Comparison of natural recovery versus remediation options is based on analysis of time required to reach the FDA action level of 2 ppm PCBs in fish tissue. Recent fish concentrations are quite near this level already, so intervention appears to make little impact on the time required to reach the target... Analysis of lower, more realistic, risk based targets in fish may show considerable differences in time to target between natural recovery and remediation.

Response:

The FDA level was used because it is the only promulgated regulatory level in New York State. While it is true that time differences between actions can be greater at fish tissue levels less than the FDA level, it must be recognized that the model results indicate that the absolute reductions in fish tissue levels achieved by dredging are small and temporary. Further, the report evaluated dredging using conservative assumptions about dredging rate and post-dredging residual PCB concentration that likely cannot be attained in practice. For example, altering the assumption of post-dredging residual from 1 ppm to a more realistic 5 ppm, completely eliminates any benefit associated with dredging. While alternate target levels may be of interest, EPA has yet to identify remedial action alternatives for the Hudson River project.

Comment: (p. 3, USEPA 2000b)

The presentation of the mathematical models (ES, p. 3-1) states that these "are equations developed from the basic scientific principles of conservation of mass, energy, and momentum... The equations are general and can be applied to any river system." While generally true, this

statement neglects the fact that the models contain empirical, non-mechanistic components that cannot be derived from first principles and may be site-specific in nature. Most notably, the PCB model requires use of an empirical, non-mechanistic sediment-water PCB transfer rate coefficient to approximate observations during the summer period which drives fish body burden. Similarly, the bioaccumulation model is presented as mechanistic, yet actually relies on empirical fitting parameters to achieve calibration.

Response:

There is no dispute that the equations utilized need to be adjusted for site specific conditions. It is also true some processes are modeled based upon field derived, site specific empirical relations. We did indicate that equations were also derived from laboratory and field data (i.e., empirical descriptions). However, it is equally true that the models must and do conform to accepted principles such as conservation of mass etc. We hope the USEPA is not suggesting that the use of empirical observations is an incorrect approach. The state-of-the-science for model surface sediment exchange is such that empirical relations, fully constrained by a large site specific data set, provide the best method for modeling this process.

The empirical description of phenomena whose mechanisms are poorly understood is common practice. The sediment-water PCB flux noted in the comment is a real phenomenon that is inferred from field observations of PCB concentration change as water passes over the sediments. It has been studied in laboratory experiments and is believed to be a consequence of multiple processes including bioirrigation, bioturbation, gas ebullition, molecular diffusion and hydraulic gradient induced advection. Representing these processes using a classical mass transfer equation parameterized by the field observations of PCB flux is an approach that is both common and adequate.

The USEPA is incorrect in the statement that fish body burdens are driven by the PCB flux from sediments during the summer period. The fish PCB levels are relatively insensitive to the rate of PCB flux from sediments because only a minor component of their body burden is derived from water column PCBs. As stated on page 5-54 of our report (QEA 1999) "The

surface sediments are the dominant PCB source to the food web in much of the Upper Hudson River ..."

The USEPA is also incorrect in stating that the bioaccumulation model is not mechanistic. Although the model relies on site-specific data to establish parameter values, the equations are descriptions of the mechanisms by which PCBs are transferred through the food web. The principal empiricism involved in model calibration is the relative importance of benthic and phytophilous macroinvertebrates in the diet of forage fish.

Comment: (p. 3, USEPA 2000b)

The Executive Summary (pp. 3-1, 3-4) also implies that all of the sub-models have been validated, implying a rigorous test of model ability against observations independent of the calibration data. This is not true, as no validation is presented for the bioaccumulation model.

Response:

EPA is incorrect in this statement. In presenting the bioaccumulation model results we did not show separate calibration and validation steps. However, we believe that the model is validated by the fact that a single model construct is able to reproduce data at independent locations (i.e., Stillwater and Thompson Island Pool (TIP), QEA 1999, Figures 5-31 through 5-34).

SECTION 3
COMMENTS PERTAINING TO THE GENERAL APPROACH TO MODEL FORMULATION AND CALIBRATION

3.1 Linkage of Model Components

Comment: (p. 5, USEPA, 2000b)

QEA gives the impression that its modeling suite is a seamlessly linked whole ... The QEA models are ... not truly coupled, but rather applied sequentially with offline processing.

Response:

The statements in the report are factual. The results of the hydrodynamic model are used to drive the sediment transport model and the results of both of these models are used to drive the PCB fate model. Whether, information is passed forward within the computer code as part of a model run or through offline programs is immaterial. In fact, it would be inefficient to dynamically link the sub-models because it would then be necessary to execute all of them in order to conduct a PCB simulation. Using offline coupling facilitates multiple simulations of the PCB model using the results of previously executed hydrodynamic and sediment transport simulations.

Comment: (p. 5, USEPA, 2000b)

... there is a mismatch between the state variables addressed in the sediment and PCB models: QEA's sediment model represents dynamics of both cohesive and non-cohesive sediment beds in the entire Upper Hudson... However, in transmitting information forward to the PCB model, the cohesive and non-cohesive sediment fluxes were aggregated into total sediment flux.

Response:

The point of this comment is unclear. It is true that the PCB model aggregates the two sediment types specified by the sediment transport model into a total. No error is introduced by this aggregation, as mass balance is maintained. The only reason to avoid aggregation would be if it compromised the accuracy of the PCB model. This is not the case, because the aggregated approach conserves mass and because the site-specific data needed to simulate sorbed PCBs by sediment type do not exist. For example, particulate PCB data and particulate organic matter data are not available by sediment type. Thus, simulating the disaggregated sediment types would introduce unconstrained model parameters without providing any additional knowledge of sediment fluxes. It is our view that the approach we have taken represents the optimum strategy given knowledge of processes, mechanisms and the available data.

Comment: (p. 6, USEPA 2000b)

... the form of PCBs addressed in the fate and transport model (the sum of trichlorobiphenyls through decachlorobiphenyls, or "PCB_{3+}") is not consistent with the sum of reported Aroclor concentrations in fish used in the bioaccumulation model.

Response:

The sum of reported Aroclor concentrations in fish is an estimate of total PCB concentrations in fish. As USEPA has noted (RBMR Volume 3; page 41), total PCB and PCB_{3+} concentrations in fish are expected to be similar "because fish tend not to accumulate significant amounts of mono- and dichlorobiphenyls." Thus, it is not inconsistent to compare the model of PCB_{3+} with the sum of Aroclor measurements. However, a separate issue is whether the sum of reported Aroclor concentrations is a biased estimate of PCB_{3+} or total PCB. USEPA has demonstrated a bias of 15 to 40 percent depending on analytical methodology and the laboratory that processed the samples. That bias was not accounted for in our report. The significance of the bias is discussed later in this response document (Section 7).

3.2 Calibration Strategy

Comment: (p. 6, USEPA 2000b)

QEA has calibrated the solids and PCB models sequentially, with no documented feedback... Simultaneous consideration of both sediment and PCBs in the calibration as done for the USEPA model provides more rigorous constraints on the sediment model calibration.

Response:

The QEA model simulates sediment transport using equations developed from independent field and laboratory data. Their validity has been demonstrated by their ability to describe sediment transport in numerous river systems. More data are available to parameterize these equations for application to the Upper Hudson River than were available in any other modeling effort of similar scope. Further, little calibration of the sediment transport equations was required to achieve accurate replication of observations of flood event TSS and long-term patterns of sediment deposition and erosion. USEPA seems to have confused our mechanistic approach with the Agency's empirical approach. In USEPA's approach, the settling and resuspension rates are unconstrained, whereas, in our approach these rates are constrained by mechanistic descriptions and independent data. There was no calibration involved in cohesive sediment transport and only two parameters were adjusted in the calibration of non-cohesive sediment transport.

The comment about a supposed "simultaneous constraint" imposed by the adjustment of the sediment transport parameters in an effort to calibrate the PCB model is based on flawed logic. A strong indication of the accuracy of the QEA model is its ability to reproduce the PCB observations without having to resort to an adjustment of the sediment transport model away from a parameterization most consistent with theory and independent observations. USEPA's approach of simultaneously adjusting sediment transport parameters and PCB fate parameters to maximize the fit of the model to data is a weakness because of the added degrees of freedom (i.e., fewer parameters are constrained by independent information) in its model. The fact that

the USEPA sediment transport model and PCB fate model cannot be independently calibrated highlights the inferior nature of the underlying framework. In fact, the equations used by the USEPA model to describe sediment transport are inconsistent with theory and independent laboratory and field data.

SECTION 4
COMMENTS PERTAINING TO THE HYDRODYNAMIC MODELS

4.1 Model Calibration

Comment: (p. 6, USEPA 2000b)

For the one-dimensional model, the text (Vol. 2, p. 2-13) indicates that the "coupling output was flow balanced to ensure mass conservation," but it is unclear as to whether the flow balancing was implemented on a daily basis or a long-term average basis. Further clarification of what was actually done regarding the flow balancing between river reaches should be provided.

Response:

Coupling output from the one-dimensional hydrodynamic model was flow balanced on a daily average basis.

Comment: (p. 6, USEPA 2000b)

In the two-dimensional model (Vol. 2, p. 2-16), discrimination between cohesive and non-cohesive bottom friction coefficients may not be warranted.

Response:

As discussed on p. 2-16 (Vol. 2 QEA 1999), spatial variations in bottom roughness were included by using different effective bottom roughness values (z_o) for cohesive and non-cohesive sediment bed types. It was assumed that bottom roughness in non-cohesive and hard bottom areas (which used the same z_o value) is greater than bottom roughness in cohesive bed areas, which is consistent with the physical characteristics of those two bed types. Cohesive beds are

hydraulically smoother than non-cohesive beds because: (1) median sediment particle diameter (d_{50}) is lower and (2) bed forms tend to be absent or significantly smaller.

Comment: (p. 6, USEPA 2000b)

Figure 2-6 in the GE Executive Summary Report shows extensive submerged aquatic vegetation associated with fine (i.e., cohesive) sediment areas in the TIP, however no evaluation of these conditions on bottom friction coefficients is provided in the hydrodynamic calibration discussion.

Response:

Submerged aquatic vegetation (SAV) will affect hydrodynamic drag in localized areas. Ideally, a hydrodynamic model would include SAV effects. However, sufficient data were not available to accurately map SAV areas throughout the Upper Hudson River and develop necessary model inputs.

Neglect of SAV effects on sediment transport would tend to produce conservative results, i.e., lower sedimentation rates in cohesive bed areas. Including SAV drag effects would have reduced predicted current velocities in the shallow, nearshore areas. Including reductions in current velocities and the filtering effect of SAV would have resulted in enhanced deposition in those areas.

4.2 Sensitivity Analysis

Comment: (p. 7, USEPA 2000b)

The sensitivity analyses presented by QEA are not sufficient to fully establish the reliability of the hydrodynamic models. For instance, no sensitivity analyses are presented showing the effect of inclusion of the flood plain on the hydrodynamic model predictions. The two-dimensional hydrodynamic model sensitivity only assesses the effect of changing the

effective bottom roughness, z_o, in the non-cohesive bed. Since the minimum bottom friction factor in Equation (2-6), $C_{f,min}$, may be in effect most of the time in many of the non-cohesive grid elements, the sensitivity to z_o is somewhat misleading and the sensitivity of the model predictions to $C_{f,min}$ should also be presented. Additionally, no justification is provided as to why this sensitivity assessment was not also applied to the cohesive sediment bed. In effect, the analysis presented for sensitivity to z_o, shows minimal sensitivity in the model predictions because this parameter is not typically controlling the predictions and because the effect of changing it was only assessed for a fraction of the total TIP sediment bed.

Response:

The hydrodynamic sensitivity analyses are sufficient to establish the reliability of the hydrodynamic models. The bottom friction parameters in the one-dimensional and two-dimensional models were used to calibrate those models. Thus, it was appropriate to examine the sensitivity of the model by varying those parameters. Modification of model geometry inputs, i.e., inclusion of flood plains, was not included in the sensitivity analysis because excellent validation results for the 1983 spring flood (34,100 cfs, about 10-year flood) indicate that model geometry is accurately specified. This result suggests that flood plain effects are not significant for floods of this magnitude or lower.

The value of z_o in the non-cohesive bed areas was adjusted upward and downward by a factor of two with respect to the calibration value (1,500 μm), i.e., to values of either 750 or 3,000 μm, in the sensitivity analyses. For z_o values of 750, 1,500 and 3,000 μm, the bottom friction coefficient is set at $C_{f,min}$ (i.e., 0.0035) for water depths greater than 1.30, 2.59 and 5.18 m, respectively. For the TIP, 18, 47 and 97% of the non-cohesive bed area corresponds to water depths less than 1.30, 2.59 and 5.18 m, respectively. The model was relatively insensitive to the highest z_o value when 97% of the non-cohesive bed had a bottom friction factor greater than $C_{f,min}$ (Figure 2-33, Vol. 2, QEA 1999). Thus, the model is also insensitive to reasonable variations in $C_{f,min}$.

The QEA report acknowledges the insensitivity of the model to variations in non-cohesive z_o and clearly states this on p. 2-22 (Vol. 2, QEA 1999):

> "Insensitivity of the two-dimensional model to variations in z_o indicates that TIP geometry inputs, i.e., bathymetry and shoreline, are the primary factors controlling water surface elevations calculated by the model. This result demonstrated that TIP geometry was accurately represented in the model. Note that careful examination of the Figures 2-21b and 2-33a (QEA reveals that varying bottom roughness from 1,500 to 750 μm caused negligible changes in predicted stage height at Gauge 119. This result was due to the use of $C_{f,min}$ in Equation (2-6) (Vol. 2, QEA 1999), and it indicated that $C_{f,min}$ was specified at many non-cohesive grid elements for bottom roughnesses of 750 and 1,500 μm. These results, coupled with the two-dimensional model calibration results in Reaches 1 to 6, show that the value of the non-cohesive bottom roughness is relatively uncertain. This finding may have important implications for the sediment transport model because the bottom roughness coefficient affects bottom shear stress, which is a primary variable that controls resuspension and deposition. The effect of z_o on sediment transport simulations in the TIP was investigated, see Sections 3.4 and 3.5."

Variation of z_o in cohesive bed areas was not included in the hydrodynamic sensitivity because: (1) the model was relatively insensitive to variation of z_o in non-cohesive bed areas and (2) the cohesive bed comprises only 22% of the total bed area in the TIP. Thus, the hydrodynamic model would be insensitive to variations of z_o in cohesive bed areas.

SECTION 5
COMMENTS PERTAINING TO THE SEDIMENT TRANSPORT MODEL

5.1 Sediment Model Specification

Comment: (p. 7, USEPA 2000b)

The QEA SEDZL model contains a sophisticated theoretical approach to representation of sediment transport and settling in the water column. Unfortunately, very little data are available on particle size distributions in the water column, and the sediment transport model was calibrated and validated to total suspended solids data (Section 3.3.2). Further, calibration and application of the QEA model require assumptions about the size class of tributary loads, for which data are lacking. The sediment models are thus not well-constrained by available data. Even so, it was difficult to achieve a reasonable calibration: To make the model fit, QEA was forced to make what appear to be unrealistic assumption, e.g., "The sand content of sediment loads from Moses Kill and direct drainage was assumed to be zero. Initial model testing showed that unrealistic amounts of sediment were predicted to be deposited at the mouth of Moses Kill whenever sand was included in the sediment loading for that tributary ..." (Vol. 2, p. 3-23). Finally, only total sediment concentrations and fluxes are carried forward into the PCB model. While the QEA approach to transport and settling appears sophisticated, the data are not available to make use of the sophistication of the model. Therefore, the complexity of the SEDZL model may not add predictive capability over alternative, less sophisticated approaches, which make full use of available data.

Response:

Application of the QEA sediment transport model to the Upper Hudson River required making assumptions on the particle size distribution of sediment loads specified at Fort Edward and tributaries. However, calibration and validation of the model was accomplished with no adjustment of the magnitude or composition of sediment loads. Minimal particle size

distribution data were available; sediment load composition was determined from these data and not adjusted during calibration or validation. Particle size data collected at Schuylerville, Stillwater and Waterford indicated no correlation between sand content and flow rate, with average sand content being about 25% at Schuylerville. As a reasonable first-approximation, the sand content of sediment loads specified at Fort Edward and various tributaries was assumed to be consistent with observations at Schuylerville, with a flow-independent value of 25% being assigned.

The assertion by USEPA that "To make the model fit, QEA was forced to make what appear to unrealistic assumptions ..." is incorrect and misleading. The discussion (p. 3-23, Vol. 2, QEA 1999) focuses on the determination of sediment load composition and it was stated that "The sand content of sediment loads from Moses Kill and direct drainage was assumed to be zero. Initial model testing showed that unrealistic amounts of sediment were predicted to be deposited at the mouth of Moses Kill whenever sand was included in the sediment loading for that tributary. An examination of the geometry/bathymetry of Moses Kill near its confluence with the Hudson River suggests that this portion of the tributary is a depositional zone that would likely trap most suspended sands and significantly reduce the sand load from the tributary to the river. Sediment loading from all direct drainage was assumed to originate from direct runoff and very small streams. The hydraulic characteristics of these sediment sources prevent the transport of significant quantities of sand from the direct drainage area to the Hudson River." Specification of zero sand content in Moses Kill sediment loads was done prior to model calibration, i.e., during initial model testing; model calibration was not accomplished by adjustment of sediment load composition.

The statement made by USEPA that "While the QEA approach to transport and settling appears sophisticated, the data are not available to make use of the sophistication of the model. Therefore, the complexity of the SEDZL model may not add predictive capability over alternative, less sophisticated approaches, which make full use of available data." is incorrect and misleading. First, the level of complexity used in the QEA sediment transport model is comparable to other models that have been applied to riverine sediment transport studies, e.g., Lee et al. (1997), Holly and Karim (1986) and van Nierkerk et al. (1992). In fact, the sediment

transport modeling framework being proposed by USEPA for its study of PCB fate in the Housatonic River (USEPA 2000c) is of the same level of complexity as the SEDZL model. Second, sufficient Upper Hudson River data were available to effectively develop and apply the QEA sediment transport model. A relatively extensive data set for sediment transport model inputs is available for the Upper Hudson River and the QEA model made full use of the available data. For example, site-specific data for cohesive resuspension properties were obtained from 60 cores collected in TIP and 48 cores collected in Reaches 1 to 7. Grain size distribution data from approximately 100 TIP samples and more than 400 samples from Reaches 1 to 7 were used to specify bed property inputs for non-cohesive bed areas. Approximately 1,200 TSS concentration measurements, collected in the Upper Hudson River and it tributaries, during various floods were used for model calibration and validation. These examples represent only a fraction of the data used for model development, calibration and validation, however, it is clear that sufficient data were available for application of the QEA sediment transport model.

Comment: (p. 7, USEPA 2000b)

In general, the complexity of the sediment transport model and the stated success of the calibration are not justified by the available data. Large uncertainties in loadings and sediment transport model parameters result in a somewhat arbitrary calibration. Considering that the rates of sediment deposition and resuspension are not constrained, and only the net effect is, the sediment transport model is under-determined and was not subjected to simultaneous constraint by the PCB model. In the PCB model calibration, an undue level of confidence is placed on the sediment transport model results considering the limitations of the available data and the calibration approach.

Response:

As in any riverine sediment transport model, uncertainties do exist in solids loading and input parameters. However, solids loading was measured at several main stem locations and at all of the major tributaries for the 1994 spring flood period used for model calibration. In addition to comparisons of suspended sediment concentrations at various locations, the data

permitted mass balance comparisons which were used to constrain the calibration. While some uncertainty may exist in the loads (and associated mass balances) due to data limitations, the model demonstrated that it can effectively reproduce significant variations in net resuspension and deposition during this flood period.

USEPA is incorrect in stating that "the rates of sediment deposition and resuspension are not constrained, and only the net effect is" (USEPA 2000b). Cohesive resuspension is constrained by site-specific resuspension potential (i.e., shaker) data and no adjustment of parameters controlling cohesive resuspension was made during model calibration. Cohesive settling speeds were constrained by laboratory measurements of the settling speeds of cohesive flocs in freshwater.

Comment: (p. 8, USEPA 2000b)

The mismatch between model theory and data also applies to the simulation of non-cohesive resuspension, which is driven by a hydrodynamic model combined with representation of active layer thickness and bed armoring. As with settling, the data do not appear to be sufficient to ensure that the more complex representation yields a more accurate result.

Response:

Simulation of non-cohesive resuspension in the QEA model was accomplished using a combination of the van Rijn suspended load theory (1984) and a bed armoring algorithm (van Niekerk et al. 1992). The van Rijn (1984) model has been shown to yield accurate results and is widely used (Garcia and Parker 1991). The necessity of simulating bed armoring processes in the Upper Hudson River is discussed below. It should be noted that USEPA neglected bed armoring in its sediment transport model.

Successful application of the non-cohesive resuspension algorithm in the QEA model depended upon an adequate representation of the spatial distribution of bed properties in the non-cohesive bed areas. Grain size distribution data from approximately 100 TIP samples and more

than 400 samples from Reaches 1 to 7 were used to develop spatial distributions of non-cohesive bed properties for model input, see pp. 3-15 to 3-17 (Vol. 2, QEA 1999) for a detailed discussion of this topic. Therefore, the statement by USEPA that "the data do not appear to be sufficient to ensure that the more complex representation yields a more accurate result" is incorrect.

As stated on p. 3-8 (Vol. 2, QEA 1999).: "Non-cohesive areas of the sediment bed in the Upper Hudson River typically contain a significant fraction of non-suspendable material. For example, on average, coarse sand and gravel comprise approximately 50% of the non-cohesive bed in the TIP. Thus, the effects of bed armoring must be included in a non-cohesive sediment transport model applied to this river. Erosion rates will be significantly over-estimated if bed armoring effects are not considered during a sediment transport simulation of a flood event."

Without inclusion of a bed armoring mechanism in the non-cohesive suspended load model, resuspension from the non-cohesive bed will be significantly over-predicted during a flood. This result may not be obvious if bed armoring is neglected and model-data comparisons are limited to TSS concentration comparisons, e.g., Figure 3-23 (Vol. 2, QEA 1999). However, if time-variable mass balances are used to evaluate model performance, e.g., Figure 3-24, then large discrepancies between predicted and data-based mass balances will occur if non-cohesive bed armoring processes are neglected in the model.

Inclusion of non-cohesive bed armoring in the model is necessary because bed armoring is a primary process controlling resuspension from the non-cohesive bed. Upper Hudson River bed data clearly show significant heterogeneity, with a large fraction of non-suspendable material present in the bed. Thus, Upper Hudson River data indicate that inclusion of non-cohesive bed armoring in the model is necessary.

Comment: (p. 8, USEPA 2000b)

In addition, QEA neglects bed load transport of non-cohesive sediment. While, as stated by QEA (Vol. 2, p. 3-11), "these coarse sediments do not directly affect water column transport of particle-sorbed PCBs", bed load transport is likely to have an important effect on active layer

thickness and armoring of non-cohesive sediments. The statement also fails to acknowledge the potentially significant effect that bed load dynamics may have on sediment-water PCB transfer.

Response:

This statement by USEPA appears to contradict previous statements concerning unnecessary sophistication of the QEA sediment transport model. Contrary to earlier statements, USEPA seems to be suggesting that neglect of bed load in the QEA model has somehow compromised the accuracy of the model and that increased model complexity is necessary. It should also be noted that USEPA neglected bed load transport in its model.

The effects of bed load transport on bed armoring are implicitly included in the model through parameterization of the active layer thickness.

Bed load transport of non-cohesive sediments was neglected in this study for several reasons. First, bed load transport involves the near-bed transport of coarse sand and gravel (containing relatively minor amounts of adsorbed PCBs because of low organic carbon content); these coarse sediments do not directly affect water column transport of particle-sorbed PCBs. Second, long-term decreases of PCB bed concentrations in non-cohesive bed areas were adequately predicted by the PCB fate model, suggesting that bed load transport does not significantly impact burial or transport of contaminated sediments. Finally, sufficient data to develop and calibrate a credible bed load model were not available. Therefore, including a bed load model would add unjustifiable complexity to the model and provide minimal improvement in the predictive capabilities of the PCB fate model.

Comment: (p. 8 USEPA 2000b)

QEA contends (Vol. 2, p. 3-26) that "without implementation of mechanistic formulations to predict non-cohesive resuspension and bed armoring, as has been done in this study, empirical relationships cannot be developed that accurately predict non-cohesive resuspension rate as a function of flow ..." Earlier in the development of the model, the non-cohesive active

layer thickness behavior is presented as being poorly understood with no experimental data to support proposed modeling formulations. The calibration of the model relies on a controlling parameter in the active layer thickness equation along with the effective particle diameter of class 2 particles. In addition, important parameters with much uncertainty were specified as constants. Therefore, the resulting model can be considered, to a large degree, empirical in that it used unbounded parameters to attempt to describe observed shapes in the TSS concentration time series used in the calibration. Thus, the statement regarding the necessity of mechanistic modeling of non-cohesive sediment armoring processes is inappropriate.

Response:

The QEA model uses theoretical formulations, from the peer-reviewed literature, that are empirically based, as opposed to being developed from first principles, to represent processes controlling resuspension and deposition processes for both cohesive and non-cohesive sediments. This mechanistic approach, which is empirical at its lowest level (as are all sediment transport models currently being used to simulate river systems), has been developed such that observed resuspension and deposition phenomena are realistically represented. The empiricism inherent in the QEA approach should not be confused with the purely empirical, unconstrained and non-mechanistic approach used by USEPA to simulate sediment transport processes in the Upper Hudson River or in the Agency's LRC and DEIR analyses.

Various approximations had to be made so that the problem was tractable, as is the case in any sediment transport study of a riverine system. Even though detailed understanding of a particular process might be lacking, e.g., bed armoring, that does not negate the importance of including the effects of that process in the model. For example, PCB flux between sediment pore water and the water column is not well understood, but both the QEA and USEPA PCB fate models have included an approximate method for simulating the effects of this phenomenon. Similarly, the QEA sediment transport model used approximate methods to simulate various processes that significantly affect resuspension and deposition.

USEPA states the QEA model is "empirical in that it used unbounded parameters to attempt to describe observed shapes in the TSS concentration time series used in the calibration." This statement is incorrect and misleading. The only "unbounded" parameter adjusted during model calibration was the constant in the active layer thickness, i.e., B in Equation (3-10). The report explicitly states this on p. 3-46: "The active layer constant (B) ranged between 0.005 and 0.016, indicating that the active layer thickness is weakly dependent of bottom shear stress. The value of this parameter has no physical justification, which may introduce uncertainty into the simulations." The effective particle diameter of class 2 sediment (d_2), which was the other calibration parameter, was bounded. The model calibration involved much more than an "attempt to describe observed shapes in the TSS concentration time series." As stated in the report, comparisons to data-based mass balances were the primary calibration target.

USEPA's comments concentrate on uncertainty in the bed armoring formulation used in the QEA model, presumably to support its argument that mechanistic modeling of non-cohesive suspended load transport is unnecessary and to validate its use of a non-mechanistic model of non-cohesive transport which neglects bed armoring. However, the successful application of a mechanistic sediment transport model to the Upper Hudson River clearly indicates that non-cohesive bed armoring is an important resuspension process that must be included to accurately represent sediment transport.

5.2 Sediment Model Forcing Functions

Comment: (p. 8, USEPA 2000b)

It should be emphasized that sediment model calibration for the Hudson appears to be more sensitive to the specification of external forcing functions than to exact details of the sediment transport model formulations. Because solids concentrations are not monitored continuously at Fort Edward, the upstream boundary of the modeling domain, QEA uses a sediment rating curve approach to estimate a continuous upstream solids boundary condition. The data periods and sources used to develop the Fort Edward solids loads by QEA are not explicitly stated. The inference from the report text is that only data from 1977-1992 were used

to develop the rating curve for solids loading at Fort Edward. There are extensive TSS data collected at Fort Edward available beyond 1992, largely collected by USGS and GE, in addition to the USEPA 1993 Phase 2 sampling. The report provides no indication of whether or how these data were used in developing the Fort Edward solids rating curve.

Response:

USEPA's statement that "QEA uses a sediment rating curve approach to estimate a continuous upstream solids boundary condition" is incorrect. As stated on p. 3-19 (Vol. 2, QEA 1999) specification of the upstream boundary condition for sediment loading at Fort Edward for model calibration and validation simulations used a combination of TSS concentration data and rating curve estimates. Fort Edward solids loads were specified using TSS concentration data when data were available. Equation (3-19) was used to estimate TSS concentration at that location when data were not available.

USEPA's contention that the "sediment model calibration for the Hudson appears to be more sensitive to the specification of external forcing functions than to exact details of the sediment model formulation" is incorrect and misleading. Calibration of the QEA model was accomplished using a period (1994 spring flood) during which TSS concentration data were available to specify upstream and tributary sediment loading with relatively low uncertainty. Constructing data-based mass balances during the calibration period, while acknowledging that some uncertainty exists in the results of data-based mass balances, and using these results to calibrate the model minimized solids loading effects. The mass balance approach used in the calibration process is extremely sensitive to the structure and parameterization of the sediment transport model.

The sediment rating curve developed for specifying Fort Edwards solids loads used TSS concentration data collected between 1977 and 1996, including data collected by U.S. Geological Survey (USGS), USEPA and GE. Approximately 950 measurements were included in the rating curve analysis.

Comment: (p. 8, USEPA 2000b)

In USEPA's Revised Baseline Modeling Report (Revised BMR), LimnoTech Incorporated (LTI) notes that upstream sediment concentrations in the Hudson River at Fort Edward appear to be consistently lower after 1990 than in the pre-1990 period. Changes over time may reflect both changes in watershed land use patterns and stabilization of sediment deposits within the river. Clearly, sediment loads were likely highest in the period immediately after removal of the Fort Edward Dam. While changes may be gradual over time, LTI selected a 1990 boundary for time stratification based on trends observed in the data and the fact that stabilization activities were completed by GE in the remnant deposit area in the fall of 1990. Both parametric and non-parametric statistical tests show a significant decrease in the relationship between flow and TSS before and after 1990. Solids load for a given flow is less after 1990 than before 1990. This finding implies that a single sediment-rating curve across all time periods (as used by QEA) is not appropriate to establish upstream boundary loads, and leads to an incorrect calibration of the model. If a lower sediment rating curve is present in the future this also has important implications for slowing the rate of natural recovery of the system, as the predicted rate of burial will be decreased. Determination of solids loads from unmonitored, or infrequently monitored tributaries, is also a challenging problem which is not fully resolved. Given that solids predictions are strongly driven by external forcing, the USEPA approach of simultaneous calibration to solids and PCBs seems preferable to the sequential approach of QEA.

Response:

The approach used by USEPA to simultaneously calibrate solids and PCBs is not preferable to QEA's sequential approach whereby the sediment transport model was calibrated independently of the PCB fate model. As stated previously in this document, USEPA's assertion concerning the supposed superiority of "simultaneous calibration" of the sediment transport and PCB fate models is based on flawed logic. A strong indication of the accuracy of the QEA model is its ability to reproduce observed spatial and temporal trends in PCB water column and bed concentrations without having to resort to an adjustment of the sediment transport model away from a parameterization most consistent with theory and independent observations.

USEPA's approach of simultaneously adjusting sediment transport parameters and PCB fate parameters to optimize the fit of the model to data is a weakness because of the added degrees of freedom, i.e., fewer parameters are constrained by independent information.

USEPA has hypothesized that a large decrease in the solids loading at Fort Edward occurred after 1990 (USEPA 2000). Various causes for the loading decrease were proposed, including capping of remnant deposits by GE in 1990. Several analyses were presented by USEPA to support its hypothesis that the annual loading rate after 1990 has decreased by 38% when compared to the 1977-90 period.

USEPA's contention that "This finding implies that a single sediment-rating curve across all time periods (as used by QEA) is not appropriate to establish upstream boundary loads, and leads to an incorrect calibration of the model" is incorrect. First, as discussed previously, the sediment transport model was calibrated during a 30-day period in spring 1994. Specification of Fort Edward sediment loading during this period was entirely data-based; the rating curve was not used to estimate Fort Edward sediment loading during the calibration period. Second, use of a single sediment rating curve is appropriate for all time periods.

A detailed discussion concerning errors in USEPA's analysis of Fort Edward sediment loading is presented in QEA (2000). Only key points and conclusion from QEA (2000) will be given here.

Both USEPA and GE have estimated Fort Edward solids loading during periods when data are unavailable using rating curves developed from total suspended solids (TSS) concentration and flow rate data collected at the Rogers Island sampling station located at the upstream limit of the TIP (QEA 1999, USEPA 2000). The two primary TSS data sources are: 1) USGS, which has collected samples from 1977 to the present and 2) GE, which has collected samples since 1990. There are significant differences in the TSS sampling equipment and procedures used by USGS and GE. These differences affect the interpretation of TSS concentration data.

There are two reasons that support the use of the USGS data to estimate solids loadings. First, the USGS methodology is specifically designed to measure sediment loads, whereas GE's methodology is not. Second, the USGS methodology has remained constant throughout the relevant period (i.e., 1977 to the present), whereas GE's different methodology was introduced after 1990. It is far preferable to use a consistent data set than to rely on data from two different methodologies. Thus, it is appropriate to rely on the USGS data set to establish solids loading over time at Fort Edward.

The correspondence of the hypothesized loading change and the introduction of a second data source, i.e., GE data, into the loading analysis requires an ability to determine whether the change is due to differences in sampling and laboratory analysis between the two data sources. Various techniques can be used to evaluate whether the introduction of the GE data into the loading analysis causes an artifactual reduction in solids loading that is not real. The most direct technique is to determine whether the single data source, i.e., USGS data, that covers both the 1977-90 and post-1990 periods supports the conclusion of a reduction in solids loading.

A robust and objective test of the loading-change hypothesis involves comparison of 1977-90 and post-1990 solids rating curves developed from the USGS data. This comparison is critical because, ultimately, rating curves are used to calculate solids loading. This analysis indicated that differences exist between the 1977-90 and post-1990 periods in Fort Edward solids loading; statistically significant differences occur under low-flow conditions, with relatively small differences observed during high-flow conditions. In fact, the two high-flow rating curves were statistically compared and were not significantly different at a 95% confidence level. It should be noted that high-flow solids loading has a much larger impact on sedimentation than sediment loads brought in during low-flow periods, i.e., episodic deposition occurs in the Upper Hudson River with most of the annual sedimentation occurring during relatively rare high-flow events. Thus, proper analysis of the USGS data shows that the solids loading during high flow at Fort Edward did not decline after 1990, and only a small drop in solids loading occurred during low flow after 1990.

It appears that USEPA's hypothesized reduction in solids loading after 1990 is due to the Agency's reliance on the GE data. The apparent low-bias of the GE TSS concentration data during high-flow conditions probably results from the configuration of the sampling device and the sampling procedure (QEA 2000). Thus, the GE TSS concentration data should not be used for calculating total suspended solids loading at Fort Edward.

This analysis indicates that USEPA's conclusion concerning temporal changes in solids loading at Fort Edward is incorrect. USEPA was informed of this conclusion, which was based upon the analyses discussed above, during a conference call held in August 1999 between GE, QEA, USEPA and members of its modeling team. This interaction was requested by USEPA.

Inclusion of the GE data in the development of the post-1990 rating curve causes an underestimation of Fort Edward solids loading. For example, the USEPA rating curve for the post-1990 period yielded an average annual solids load of 21,500 MT/yr for the 1991-1998 period, which is 20% lower than the load estimated using the post-1990 USGS rating curve. Use of solids loading inputs to HUDTOX that are too low during post-1990 and projection periods will reduce predicted sedimentation rates, affect the calculated rate of natural recovery and may incorrectly skew the efficacy of various remedial actions. Therefore, the use of an artificially low solids load at Fort Edward introduces a crucial error into USEPA's model.

Comment: (p. 9, USEPA 2000b))

For tributary solids loads, QEA acknowledges the lack of sufficient tributary monitoring data and uses an approach that compares solids loads past Fort Edward, Stillwater and Waterford to yield net tributary loads. Artificial sediment rating curves were then adjusted to yield the observed gain in mainstem loads. This procedure, however, rests on the assumption that initial model simulation predictions of a trapping efficiency of 8.5% for the TIP are correct and are extrapolatable to reaches 1 through 7. In fact, trapping efficiency clearly varies by reach. QEA states (Vol. 2, p. 3-21) that their results are consistent with sediment yield predicted by Phillips and Hanchar (1996) with the assumption that "roughly 50% of a tributary drainage

basin in the Upper Hudson River is forested", citing 1974 data. However, this assumption is not supported by comparison to actual forest cover of individual drainage basins.

Response:

USEPA incorrectly asserts that "This procedure, however, rests on the assumption that initial model simulation predictions of a trapping efficiency of 8.5% for the TIP are correct and are extrapolatable to reaches 1 through 7." As discussed on p. 3-20 (Vol. 2, QEA 1999) total tributary solids loads to Reaches 1 to 7 were first estimated assuming conservative transport, i.e., no net deposition. Increases in these initial estimates had to be made to account for net deposition in those reaches. An assumption had to be made about trapping efficiency in Reaches 1 to 7 to make the final estimates of tributary solids loading, which included depositional losses in the analysis. Including the effects of deposition, i.e., using an estimated trapping efficiency of 8.5%, resulted in 22% and 17% increases in tributary load, with respect to load estimates made assuming conservative transport, for Reaches 5 to 7 and 1 to 4, respectively. Therefore, the tributary load estimation method does not rest on the assumption of 8.5% trapping efficiency being correct for Reach 1 to 7, as USEPA states. The trapping efficiency assumption is simply used to increase tributary loading estimates by a moderate amount (by about 20%). USEPA used a similar procedure to upwardly adjust its initial estimates of tributary loads.

USEPA correctly states that trapping efficiency varies by reach. However, QEA performed the tributary load analysis before performing long-term sediment transport simulations with the calibrated model, and there was no credible method for *a priori* specification of trapping efficiency for each reach downstream of TIP. As stated on p. 3-20 (Vol. 2, QEA 1999), using an 8.5% trapping efficiency for Reaches 1 to 7 was a "first approximation." USEPA did have an advantage when it conducted its tributary analysis because its modelers were able to leverage off of QEA's prior work (i.e., QEA 1999) and use reach-variable trapping efficiencies predicted by the QEA sediment transport model (SEDZL). However, USEPA's utilization of the SEDZL results was flawed and produced incorrect results. USEPA calculated "area-weighted reach-average trapping efficiencies" of 8.47 and 3.66% for the TI dam to Stillwater and Stillwater to Waterford reaches, respectively (see p. 84, Book 1 and

Table 6-13, Book 2 of the RBMR). USEPA's analysis is incorrect because trapping efficiencies of individual reaches cannot be aggregated and averaged on an areal basis. The correct way to calculate the trapping efficiencies for these two regions of the Upper Hudson River is to use mass balance results from SEDZL. Trapping efficiencies used in the USEPA analysis (Table 6-13, Book 2 of the RBMR) were presumably taken from Figure 3-41 (Vol. 2, QEA 1999), which presents the sediment mass balance for the Upper Hudson River predicted by SEDZL for the 1977-1998 period. The correct method for calculating the trapping efficiencies for the TI dam to Stillwater and Stillwater to Waterford reaches is to use the reach-specific mass balance results in Figure 3-41 (Vol. 2, QEA 1999) i.e., divide total deposition by total incoming sediment load for the region of interest. This procedure yields trapping efficiencies of 12.3 and 11.1% for the TI dam to Stillwater and Stillwater to Waterford reaches, respectively. These results show that: (1) USEPA's incorrect trapping efficiencies used in its analysis under-predicted tributary loads and (2) the QEA first-approximation estimate of 8.5% underestimated trapping efficiencies in the downstream reaches and, thus, tributary loads were also underestimated.

Comparison of sediment yields determined by the QEA estimation method with an independent estimate of sediment yield (i.e., Figure 33, Phillips and Hanchar, 1996) was done as an independent check to ensure that the estimated sediment yields were of the correct order-of-magnitude. Based on Soil Conservation Service data, typical forest cover in the Upper Hudson River drainage basin under consideration was about 50%, which is a reasonable estimate for use in this comparative analysis. Specifically, 51% of the total drainage area representing the eight primary tributaries between Fort Edward and Waterford is classified as forested by the Soil Conservation Service (1974). Forested area for the individual watersheds ranges from 36% for Flately Brook to 58% for Fish Creek (SCS, 1974). Thus, USEPA is incorrect in stating that "this assumption is not supported by comparison to actual forest cover of individual drainage basins."

Comment: (p. 9, USEPA 2000b)

QEA's assumption (Vol. 2, p. 3-23) that Moses Kill solids loads to the Hudson River mainstem have zero sand content may be reasonable. However, the need to make this assumption could also indicate that the total load estimated for Moses Kill should have been

reduced if the tributary data collected do not represent the composition of the solids load that actually progresses beyond the mouth of Moses Kill and into the mainstem of the Hudson River. This should also be acknowledged as a calibration parameter if the basis for adjustment was feedback from the PCB model predictions. For example, was this parameter adjusted to improve surface sediment PCB_{3+} concentration trajectory comparisons with data in the mainstem segments (11 and 12) which may have been affected by the solids loading from Moses Kill?

Response:

As stated on p. 3-23 (Vol. 2, QEA 1999) "Initial model testing showed that unrealistic amounts of sediment were predicted to be deposited at the mouth of Moses Kill whenever sand was included in the sediment loading for that tributary." Initial testing means prior to model calibration and validation; setting the sand content in the Moses Kill load is necessary to produce results that are qualitatively correct. In addition, there was no feedback from PCB model predictions; no adjustment of sediment transport parameters or inputs were made to improve PCB model-data comparisons. Thus, adjustment of the Moses Kill sediment composition is not a calibration parameter.

No credible justification exists for decreasing the Moses Kill sediment load. This rating curve was developed from available data, and, although somewhat limited, there is no reason to believe that these data are not representative of the total suspended load from Moses Kill to the Upper Hudson River.

Comment: (p. 9, USEPA 2000b)

The summary statement that sand content for external solids loads was set based upon available data and not adjusted during simulations (Vol. 2, p. 3-27) is misleading. It is evident that specification of the sand content for Moses Kill solids loads was based upon an iterative process of model simulation followed by an adjustment of model input assumptions. As such, these inputs should be acknowledged as model calibration parameters and not simply termed data-based constructs.

Response:

The statement on p. 3-27 (Vol. 2, QEA 1999) that USEPA is referring to is: "Instead of hypothesizing sand content relationships that depended upon tributary and flow rate, and introducing another free variable that could be adjusted during model calibration, sand content was set based upon the available data and not adjusted during the simulations." This statement is factual. Sand content of Moses Kill was not adjusted during model calibration or validation. As stated previously, initial model testing showed that unrealistic amounts of sediments were deposited at the mouth of Moses Kill whenever any sand was included in that tributary's load. This testing was done prior to model calibration or validation.

5.3 Sediment Model Calibration

Comment: (p. 9, USEPA 2000b)

While QEA claims a high level of success in their sediment model calibration (ES, Section 3.2.2), the information presented in the reports suggests significant discrepancies that call into question the quality of the calibration. A key piece of evidence presented for the claim of an accurate calibration is replication of temporal variations in flux to sediments, M_{bed} (Vol. 2, p. 3-32). This does not constitute a true calibration however, as the "data" on M_{bed} is actually an inference from a highly uncertain mass balance for TSS across the TIP – and the model may thus be forced to fit an inaccurate target. Evaluation of mass balances for the 1993 spring flood showed 9,600 MT of deposition between Fort Edward and Stillwater, while the model predicted net erosion of 5,100 MT (Vol. 2, p. 3-38). Further, the model systematically under-predicts sand content at all stations between Schuylerville and Waterford in the long-term simulation (Vol. 2, p. 3-43). Finally, comparison is made to observed and predicted rates of deposition at dated sediment cores in Table 3-6 (Vol. 2, p. 3-44). While a reasonable fit is claimed, it actually appears that there is essentially no correlation between model predictions and observed sedimentation rates. Most notably, the highest predicted rate of sedimentation was for a core (HR-16) with an observed sedimentation rate below the average across all cores. It is, in any

case, unclear that the dated sediment cores, which provide point estimates of sedimentation in known depositional areas, can be used to constrain average rates of sedimentation across a model grid segment. (The SEDZL model uses the same grid as the hydrodynamic model, which is stated to have an average grid size of 140 x 25 meters in the Thompson Island Pool).

Response:

Every model has strengths and weaknesses; no model can provide a perfect representation of sediment transport processes in the Upper Hudson River. As pointed out by USEPA, and also acknowledged in QEA (1999), performance of the QEA sediment transport model was poor to fair for a minor fraction of the model-data comparisons. However, the model was compared to a large body of sediment transport data, representing a wide range of temporal and spatial scales. Generally, model-data comparison ranged from good to excellent. In addition, an extensive amount of site-specific data was used to determine model inputs and parameters. Finally, mechanistic formulations describing resuspension and deposition processes were used in the model. Therefore, evaluated on a weight-of-evidence basis, including the PCB fate model results, this model must be judged as an excellent tool for simulating sediment transport processes in the Upper Hudson River. And when compared to USEPA's sediment transport model, which is a highly empirical, non-mechanistic model, the QEA model is clearly superior and it can be used with considerably more confidence when evaluating various remedial alternatives.

Assumptions are made in all sediment transport models regarding temporal variability of inputs at upstream and tributary boundaries. Continuous data records are extremely rare and interpolation between data points must be used to estimate boundary condition values at the frequency of the model time step. Linear interpolation between data points is used in SEDZL to specify flow rate and TSS concentration boundary values. These time series of flow rate and TSS concentration, at the frequency of the model time step, are effectively the input "data" that forces the model. At the downstream boundary of a particular reach (e.g., TI dam), TSS concentration data were collected and linear interpolation was used to estimate TSS values at times between data points. This approximation introduces some uncertainty into the mass

balance; however, it is a reasonable assumption and it is consistent with preparation of TSS data used to specify model boundary values. It was assumed that predicted flow rate at the downstream boundary of the model was the best estimator of discharge at that location and the predicted flow rate was used to calculate the data-based load. The data-based mass balance might be uncertain if the purpose of the analysis was to determine the actual amount of net deposition or resuspension in a reach over a specific time period. However, determining the actual magnitude of net resuspension or deposition is not the purpose of the mass balance analysis. The data-based mass balance provides values of net resuspension or deposition based on model inputs and a relatively accurate estimate of outgoing load at the downstream boundary. The accuracy of these data-based values may be uncertain when compared to "real" values but with respect to the model, these derived values are highly accurate. As the frequency of data collection is increased, the data-based values would approach the "real" values.

Without the mass balance approach, the model calibration would have been poorly constrained because it would have relied solely on comparisons to TSS concentration data. As stated on p. 3-31 (Vol. 2, QEA 1999) "However, this method does not necessarily ensure that the model realistically and accurately simulates resuspension and deposition fluxes in the TIP. The reason for this uncertainty is that external solids loadings, from upstream and tributary sources, may dominate predicted/observed TSS concentrations in the TIP, with deposition and resuspension causing relatively small changes in water column sediment concentrations. Large changes in model parameters, creating large changes in deposition and resuspension, may cause relatively small changes in predicted TSS concentrations." Thus, the mass balance approach minimizes the influence of external solids loading on the calibration process. Therefore, the application of sediment mass balances reduces the uncertainty in the QEA model calibration.

Other statements in USEPA's comment refer to model-data comparisons from various model validation simulations. Therefore, USEPA's contention that these validation results brings into question the quality of the calibration is incorrect.

The model does systematically under-predict sand content. However, as stated on p. 3-42 (Vol. 2, QEA 1999) "The model under-predicts mean sand content at all three locations but the

spatial pattern is simulated correctly, indicating consistency between the model and observed downstream fining in the Upper Hudson River, as well as other Rivers (Paola *et al.* 1992, Paola and Seal, 1995). Under-prediction of mean sand content at the three main stem locations suggests that the sand content of tributary sediment loads may have been under-estimated."

One example of model inconsistency, as pointed out by USEPA, concerned the 1993 spring flood. As discussed on p. 3-38 (Vol. 2, 1998): "The data-based mass balance showed that 9,600 MT of net deposition occurred between Fort Edward and Stillwater. However, the model predicted net erosion of 5,100 MT between these locations. The cause of this discrepancy between the predicted and data-based mass balances is uncertain. This significant difference in the observed (net deposition) and predicted (net erosion) mass balances for the reach between Fort Edward and Stillwater may have been due to relatively sparse solids loading data for locations upstream of Stillwater during this flood."

The low statistical correlation between observed and predicted sedimentation rates can be attributed to the difference in spatial scales these quantities represent, as recognized in the USEPA comment. However, this comparison was conducted to assess the capability of the model to predict the magnitude of sedimentation rates. In addition, this model-data comparison is of importance for evaluating potential bias in the predicted sedimentation rates. Out of seven model-data comparisons, the model was within a factor of two at six locations and the predicted error was less than 10% at three locations. As stated on p. 3-44 (Vol. 2, QEA 1999) "These results demonstrate that the model is not biased low or high, but that the results are relatively evenly distributed about the line of perfect agreement. Thus, the overall results provide added confidence that the sediment transport model can simulate long-term deposition processes in the Upper Hudson River." The location at which the predicted sedimentation rate did have a relatively high error (HR-16) is discussed at length on p. 3-44 and 3-45 (Vol. 2, QEA 1999). Possible causes of this discrepancy include: (1) relatively low grid cell resolution; (2) uncertainty in sediment bed mapping; and (3) uncertainty in the composition of Hoosic River sediment loads. Another issue raised by USEPA is the comparability of predicted and core-based sedimentation rates, which is a legitimate concern. However, these are the only available

sedimentation rate data for the Upper Hudson River and were used to assess the ability of the sediment transport model to predict net sedimentation rates.

USEPA seems to have confused model calibration and model validation. Adjustment of parameters to improve model-data agreement occurred only during model calibration, i.e., 1994 spring flood simulation. Model validation involved comparisons of predicted and observed sedimentation rates. These comparisons were not used to calibrate the sediment transport model and, thus, the model was not "constrained" by these data. Presumably, USEPA's confusion stems from its model calibration philosophy whereby numerous unconstrained, non-mechanistic parameters and inputs are freely adjusted to optimize model-data agreement over a wide range of temporal and spatial scales. QEA has followed a model development path that is much more rigorous and scientifically defensible, using only two parameters to calibrate a mechanistic model during a single event.

Comment: (p. 10, USEPA 2000b)

Global statements regarding the relationship between flow rate and settling are made based on output from a single model cell (Vol. 2, p. 3-25), and may reflect merely an artifact of the model. Volume 2, Figures 3-18 through 3-25 show relationships for effective settling speeds and resuspension rates versus flow at a location that may be influenced by a tributary (Snook Kill). It would be informative to see how these relationships vary at other locations. For example, it is unclear whether the subsequent conclusion that there is no correlation between resuspension rates and flow in non-cohesive sediment areas is completely accurate, since no statement is made regarding generalization of the Snook Kill relationships shown in the report to other areas of the river.

Response:

Results for a single grid cell were presented in QEA (1999) because that location represented typical behavior of resuspension and deposition processes in the Upper Hudson River. The primary purpose of the discussion presented on pp. 3-24 to 3-26 of QEA (1999) was

to provide some insights concerning resuspension and deposition in the river, and the differences that exist between cohesive and non-cohesive bed areas.

As would be expected, spatial variability exists for the relationship between flow rate and settling. However, the general trends are similar between the various locations and the grid cell chosen for presentation in the report reflects typical behavior. Generally, a critical flow rate exists below which no resuspension occurs (ranging from about 2,000 to 9,000 cfs, depending upon grid cell locations). For the 26 grid cells in the PCB fate model that have a non-cohesive sediment bed, 22 grid cells exhibit no correlation between non-cohesive resuspension rates and flow rate. The other four grid cells, all downstream of TIP, display a poor relationship between non-cohesive resuspension and flow rate, with large variability in resuspension rate for all flow rates.

Comment: (p. 10, USEPA 2000b)

The sediment model calibration is described as requiring adjustment of only two parameters (Vol. 2, p. 3-28): the effective particle diameter of class 2 sediment, d_2, and the constant, B, in the non-cohesive active layer thickness formulation. Factors other than d_2 and B should be acknowledged as adjusted sediment transport model calibration parameters including: particle distributions for tributary solids loads and lateral eddy diffusivities.

Response:

Only two parameters (d_2 and B) were adjusted during model calibration. Determination of particle distribution for tributary solid loads and lateral eddy diffusivities occurred during initial model testing and no adjustment of those parameters was done during model calibration. The general philosophy that was followed during the model development process was: 1) model construction; 2) initial testing; 3) calibration; and 4) validation. The initial testing step involved ensuring that the model was producing results that were qualitatively correct, i.e., realistic results. As mentioned previously, initial testing indicated that adjustment of the particle size

distribution in the Moses Kill load and lateral eddy diffusivity were necessary to produce qualitatively correct results.

Comment: (p. 10, USEPA 2000b)

The probability of deposition parameter for cohesive sediments, $\tau_{b,min}$ is specified as 0.1 dynes/cm^2 without justification (Vol. 2, p. 3-28). The effect of this specification on the model predictions is unknown, since sensitivity analyses were not conducted for this parameter.

Response:

This value of $\tau_{b,min}$ was specified based on modeling experience in other aquatic systems (Gailani et al. 1991; Cardenas et al. 1995; Ziegler and Nisbet 1994, 1995). Generally, realistic values of $\tau_{b,min}$ range between 0.05 and 0.2 dynes/cm^2 (Partheniades, 1992). No sensitivity analysis was done because prior experience shows that model results are relatively insensitive when $\tau_{b,min}$ is varied between 0.05 and 0.2 dynes/cm^2

Comment: (p. 10, USEPA 2000b)

QEA's calibration involved an iterative procedure in which the model was used to estimate change in total suspended sediment mass across the TIP, ΔM_{wc} which was in turn entered back into a "data based" mass balance for TIP to estimate net solids flux to the sediment bed (M_{bed}). The resulting estimates of M_{bed} are then used as a calibration parameter (Vol. 2, p. 3-31). The effect of using a model-based value of ΔM_{wc} to generate the time series of M_{bed} fluxes should have been assessed in more detail. The effect on the cumulative flux over an event may be small, but the possible effect on the time series comparison between the "data-based" versus model predicted M_{bed} should be clarified. It is also not certain that realistic "data-based" hourly estimates of M_{bed} can be generated given the frequency of TSS data collection during the Spring 1994 high flow event and the lack of actual flow measurements in TIP tributaries corresponding to instantaneous Fort Edward flow measurements. It would seem to be

appropriate to say the "data-based" estimates of M_{bed} are partly dependent upon the construct of interpolated and extrapolated forcing conditions applied to the model.

Response:

The model was shown to agree very well with TSS concentration data in TIP (e.g., Figure 3-23, Vol. 2, QEA 1999). Therefore application of model results to estimate ΔM_{wc} is a reasonable approximation. In addition, as stated on p. 3-31 (Vol. 2, QEA 1999): "Generally, ΔM_{wc} was small compared to the quantity ($L_{in} - L_{out}$), i.e., less than 5%, so using predicted ΔM_{wc} in the data-based mass balance did not introduce significant error." In addition, the primary calibration targets were the cumulative flux values over the entire flood and during the two sub-periods.

Effects of assumptions made in constructing the data-based mass balances have been discussed previously. USEPA is correct in asserting that the "data-based" estimates of M_{bed} are partly dependent upon the construct of interpolated and extrapolated forcing conditions applied to the model. However, as stated above, this process does not negate the value of using the methodology for model calibration.

Comment: (p. 10, USEPA 2000b)

The average cohesive deposition rate of 3.8 cm/year simulated for Reach 4 (Vol. 2, p. 3-40) seems unrealistic. The only available dated high-resolution core (HR-16) in this reach shows an observed deposition rate of 0.9 cm/year. It is difficult to believe that the average deposition rate in cohesive sediments for this entire reach could be more than 4 times greater than the rate for a sediment core that was specifically located to minimize disruption of the core chronology and maximize the possibility that the core represents a high rate of deposition.

Response:

Only one sediment core (HR-16) was collected in Reach 4. Even though this core was collected in a net depositional area, the assumption cannot be made that the sedimentation rate at

that location (0.9 cm/yr) corresponds to a relatively high rate for that reach. Significant variability in net sedimentation rates may occur in cohesive depositional zones of the Upper Hudson River.

A discussion of the discrepancy between predicted and observed sedimentation rates in Reach 4 is presented on p. 3-44 (Vol. 2, QEA 1999):

> "Predicted deposition rates in Reach 4, into which the Hoosic River flows, are relatively high and do not compare well to the observed sedimentation rate at HR-16 (355% error). High deposition rates are predicted in this reach due to the deposition of fine sand (class 2 sediment), primarily from the Hoosic River, in the relatively quiescent water behind Mechanicville Dam, which is the highest dam on the Upper Hudson River. Even though model results for the long-term simulation are inconsistent with the observed sedimentation rate in Reach 4, mass balance results between Stillwater and Waterford indicated that the model under-predicted deposition in Reaches 1 to 4 during the 1993 and 1994 spring floods by about 40 to 50%. These results suggest that the model probably under-predicted the average deposition rate in the reaches between Stillwater and Waterford during the long-term simulation. In addition, the spatial distribution of deposition in Reaches 1 to 4 is probably not highly accurate, i.e., too high in Reach 4 (9% trapping efficiency) and too low in Reaches 1 to 3 (trapping efficiencies of 0.1 to 2%). These discrepancies are due to the following factors: (1) relatively low grid cell resolution; (2) uncertainty in sediment bed mapping; and (3) uncertainty in the composition of Hoosic River sediment loads."

Comment: (p. 10, USEPA 2000b)

The sediment transport model predictions at flows above 25,000 cfs in TIP (Vol. 2, p. 3-37) are suspect due to the exclusion of flow with the flood plain. For example, no assessment is made of including flood plain effects for the spring 1994 flood calibration (27,700 cfs maximum daily flow; instantaneous peak flow must be even higher), or the spring 1993 flood validation (29,000 cfs maximum flow at Fort Edward). The effect of including the flood plain in the TIP

model should have been evaluated for these events, as well as for the long-term simulation, since the spatial distribution pattern of sediment deposition and erosion may be affected significantly by periodic large flood events. The fact that the sediment transport/hydrodynamic model base calibration does not represent flood plain effects results in an estimate by QEA that scour in TIP is significantly over-predicted for the 100 year flood event (55% too much net scour in cohesive and 85% too much net scour in non-cohesive sediments). The effect on the sediment transport model calibration due to the lack of flood plain inclusion is unknown and was not evaluated by QEA. Because the calibration is based on replicating estimated changes in suspended solids mass across the TIP, omission of the flood plain, which is a major location of deposition during inundation could result in over-estimating sediment deposition rates within the channel. The only assessment done by QEA was to examine sensitivity of the sediment model to flood plain inundation within the TIP for just the 100-year flood. Any flow greater than approximately 25,000 cfs (perhaps lower) through the TIP results in significant flood plain interaction. Since this represents about a once-in-2-year flow, it suggests that the QEA sediment transport model calibration may need to be revised to incorporate flood plain effects.

Response:

It should be noted that USEPA's model also neglected flood plain effects and, therefore, suffers from the same perceived weaknesses that the Agency contends exist in the QEA model. If USEPA truly believed that this is a major weakness, the Agency would have addressed this issue within its own model development.

The development of mathematical models to represent the complexity of reverine hydrodynamics and sediment transport requires various approximations to render the problem tractable. Flood plain effects were neglected in the Upper Hudson River model because computational restrictions precluded the addition of flood plain grid cells to the model domain. Simulation times for long-term calculations, i.e., 20-year simulations, would have been impractical with the inclusion of flood plain grid cells for the entire Upper Hudson River. However, the ability of the PCB fate model to accurately simulate long-term declines in PCB bed

concentrations in the TIP indicates that the sediment transport model accurately and realistically simulates TIP deposition. Therefore, neglecting flood plain effects is a valid approximation.

Comment: (p. 11, USEPA 2000b)

Exclusion of a flood plain effect in the TIP brings into question the value of the results presented regarding simulation of a 100 year flood event (Vol. 2, Sec. 3.5). The flood plain sensitivity evaluation clearly indicates a significant effect on predicted scour during higher flow conditions. The effect of flood plain exclusion on calibration to short-term events and the long-term historical predictions was not examined. Therefore, we cannot concur with the conclusion that exclusion of the flood plain produces "conservative results." Instead, it would be more appropriate to state that exclusion of the flood plain produces conservative results, relative to its inclusion, for high flow events as a result of the present model calibration parameterization.

Response:

The primary objective of the 100-year flood simulation was to predict scour depths in cohesive and non-cohesive bed areas of the TIP. The base case simulation, using the calibrated model, excluded flood plain effects. Including flood plain effects in the hydrodynamic model caused bottom shear stresses in the main channel to be reduced (relative to the base case simulation). Scour depth predictions in cohesive bed areas are not affected by model calibration because cohesive resuspension potential parameters were determined from site-specific data and not adjusted during calibration. Reduced bottom shear stresses in cohesive bed areas during the flood plain simulation resulted in shallower scour depths (relative to the base case simulation). Therefore, model predictions produce conservative results for cohesive bed areas in the TIP when flood plains are excluded.

Scour depth predictions in non-cohesive bed areas are affected by model calibration. It is possible that including flood plain effects in the model calibration, i.e., 1994 spring flood, would have resulted in a different set of calibration parameter values, i.e., class 2 settling speed ($W_{s,2}$)and active layer constant (B). Thus, exclusion of flood effects produces conservative

results, relative to the base case, in the non-cohesive bed areas of the TIP as a result of the present calibration parameterization.

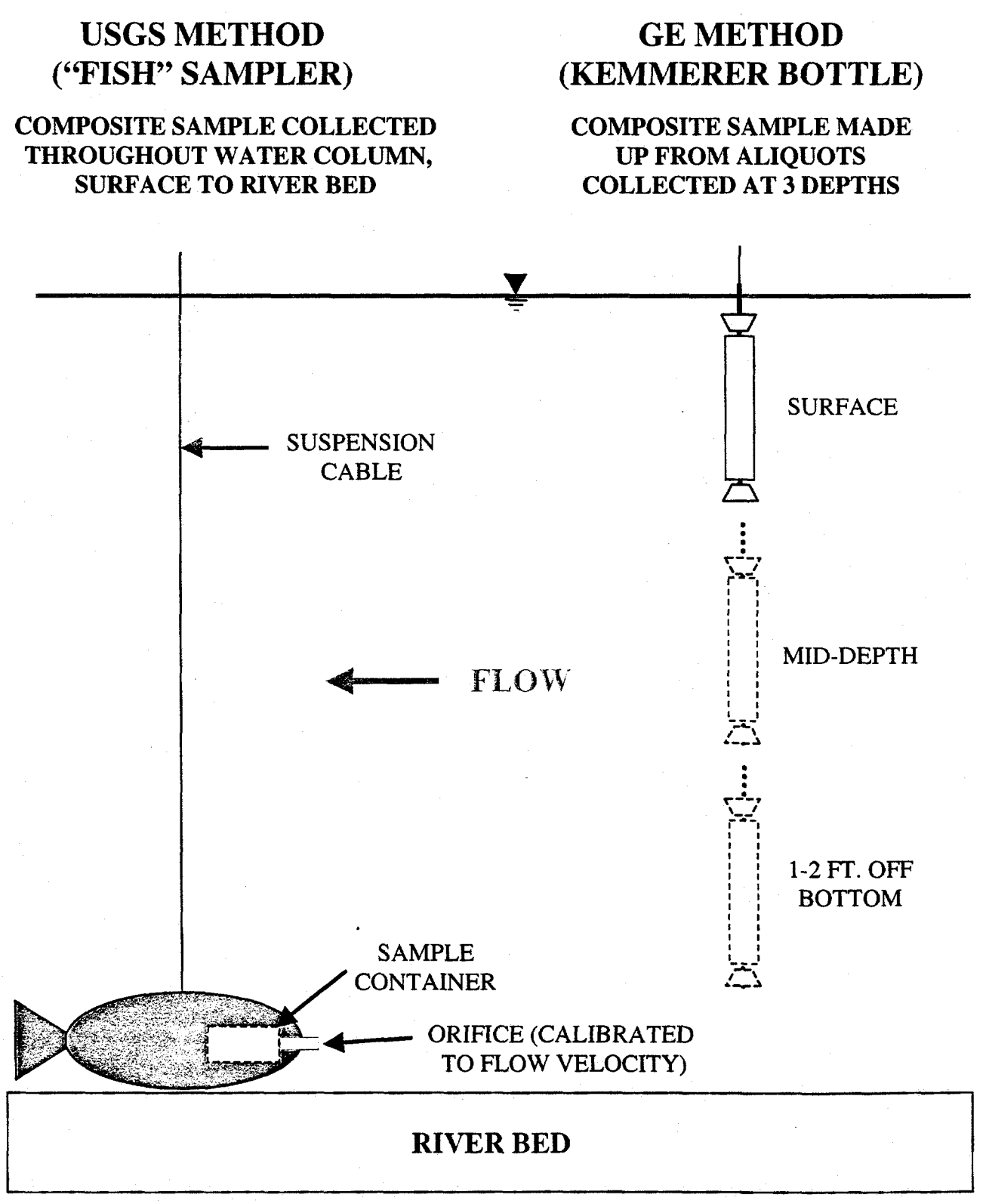


Figure 5-1. Comparison of USGS and GE solids load sampling methods.

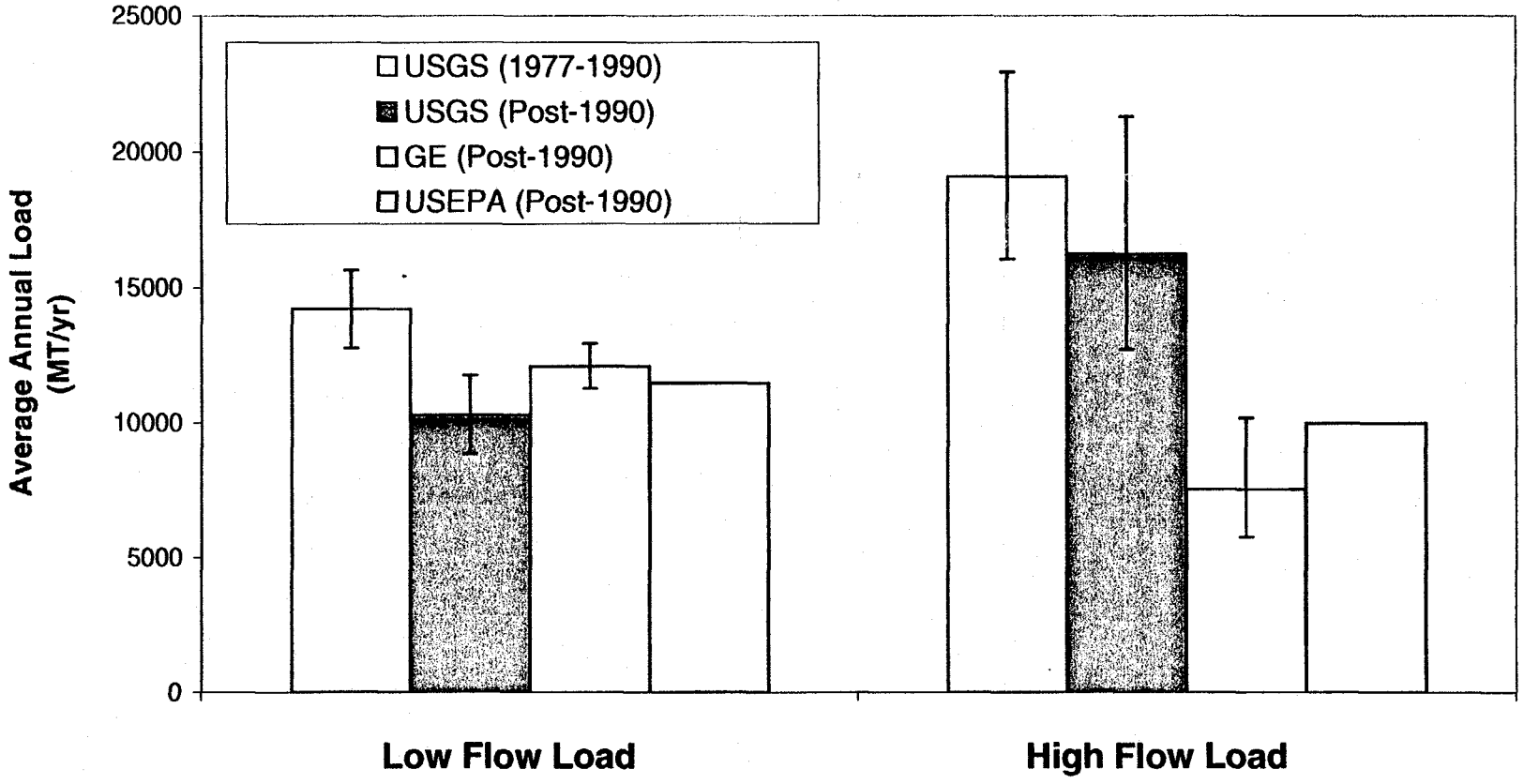


Figure 5-2. Estimated solids loading at Ft . Edward for post-1990 (1991-1998) period using various rating curves. Error bars represent the 95 % confidence interval.

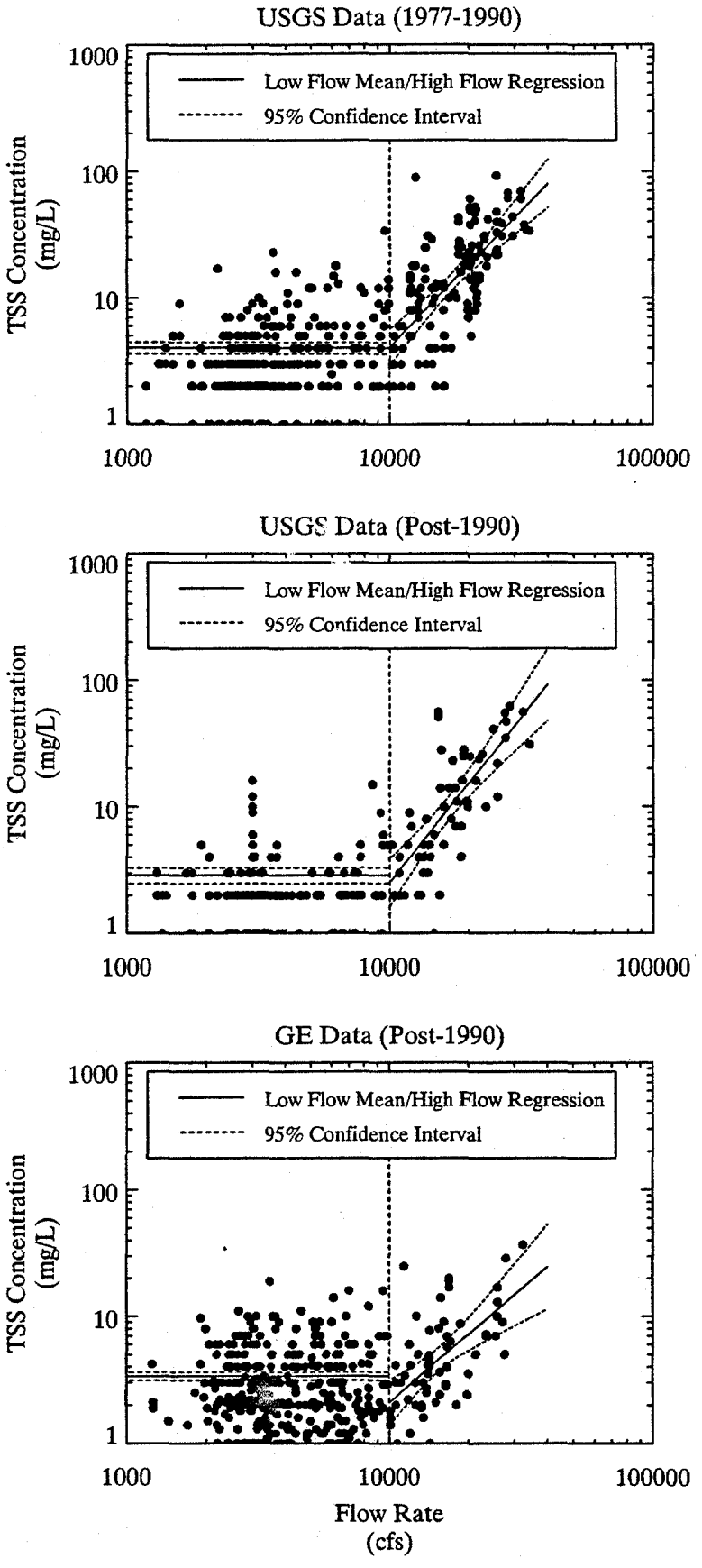


Figure 5-3. Fort Edward solids rating curves using three different data sets.

SECTION 6
COMMENTS PERTAINING TO THE PCB FATE AND TRANSPORT MODEL

6.1 Model Specification

Comment: (p. 11, USEPA 2000b)

The modeling "presumption" that [the non-cohesive] data only represents the top 5 cm of sediment is unsupported and likely causes the PCB model to misrepresent non-cohesive sediment PCB mass inventory for the 1977 initial conditions.

Response:

The depth penetrated by the grab sampling devices used to obtain most of the sediment samples in non-cohesive areas during the 1977 field study is unknown and probably varied from sample to sample. As discussed in the report, the assumption that the data represented a depth of 5 cm was based on professional judgement and knowledge of the grain size characteristics of the non-cohesive sediments. The uncertainty associated with this assumption affects sediment PCB mass estimates, since knowledge of the sampling depth is required to determine the appropriate sediment volume for each observed concentration. Additionally, interpretation of the PCB concentration data is somewhat problematic because it is not known whether at particular locations the grab sampling device penetrated below the limit of contamination and diluted the contaminated sediment with uncontaminated sediment. As shown below, this possibility exists because the depth of contamination in the non-cohesive sediments appears to be relatively shallow in most cases.

We analyzed finely segmented cores collected from non-cohesive areas in 1984 to estimate whether the 5 cm assumption is likely to have resulted in a substantial misrepresentation of the non-cohesive sediment PCB mass inventory. Unlike the 1977 sampling, the 1984 NYSDEC survey was unique in that the sampling methodology permitted coring in non-cohesive

sediments. Although most of the cores were segmented too crudely to obtain information regarding the PCB concentration profile, ten were segmented finely enough to address the question of PCB mass distribution (i.e., segments of ~6 cm (2.5 in) or less, as defined by the depth of the surface segment). PCB_{3+} mass per unit area (g/m^2) was calculated in each layer of these cores following the method outlined by the 1991 Data Evaluation and Investigation Report (USEPA, 1991). The total mass in each core was calculated and compared to the mass within each layer to determine the cumulative mass fraction of PCB_{3+} down-core (Figure 6-1). In half of the cores, the majority of the mass was located in the top 6 cm (range from 56 to 100%). In 9 of the 10 cores, nearly all of the mass (i.e., greater than 80%) was located in the top 13 cm (5 in). This analysis indicates that the assumption that all of the mass in non-cohesive sediments is located in the top 5 cm underestimates the depth of significant contamination at half or more of the non-cohesive locations.

Ten cores in the 1984 data set had surface sediment segment thicknesses between 9 and 13 cm. Nine of these coarser-segmented cores had essentially all of the PCB mass in the surface layer ($\geq$ 96%) and the other had 82% of the mass in the surface layer (Table 6-1). Although we cannot determine the vertical distribution of PCBs within the 9 to 13 cm thick surface layers of these cores, these results support the finding of the more finely segmented cores, i.e., that the PCB contamination in non-cohesive sediments is limited to the near-surface region. Thus, the available data indicate that the average depth of contamination is somewhere between 6 and 13 cm. Unfortunately, the data do not allow a more precise determination. Although it is likely that the 5 cm assumption produces an underestimate, the extent of underestimation appears to be less than a factor of two. The impact of such an underestimate is likely to be minimal. The non-cohesive sediments that contained PCBs at the greater depths are likely to be subject to net deposition, a condition the model indicates occurs in 35 percent of the non-cohesive sediment areas. PCBs at depths greater than 5 cm are of little consequence in these sediments because their ability to migrate into the bioavailable surface layer is limited to sorption-inhibited molecular diffusion.

Table 6-1. 1984 Non-Cohesive Cores with Surface Segments between 6 and 13 cm				
Grad Number	Total Mass (g/m^2)	Depth of Surface Segment cm (in)	Surface Segment Mass (g/m^2)	Percent of Mass in Surface Segment
34210	0.84	9 (3.5)	0.84	100
32542	0.005	10 (4)	0.005	100
32615	0.92	10 (4)	1.12	82
32668	1.43	10 (4)	1.43	100
32546	0.80	11 (4.5)	0.77	96
33048	1.31	13 (5)	1.29	98
33674	1.10	13 (5)	1.10	100
33852	0.97	13 (5)	0.97	100
33879	1.26	13(5)	1.25	99
34275	0.068	13 (5)	0.068	100

It should be noted that our analysis found seven non-cohesive cores in the 1984 data set that differs significantly from the above paradigm. These cores have a much higher PCB_{3+} mass than the others (i.e., greater than 10 g/m^2 versus an average of less than 2 g/m^2 for the others) and have a significant fraction of the mass located greater than 13 cm below the surface (Table 6-2). They also have several unique features that suggest they are not representative of non-cohesive sediments. First, six of the seven were collected from "hot spot" areas, which are generally locations of cohesive sediment. Second, these cores tended to have subsurface textures that differed from their surface textures that were used to classify them as non-cohesive. The shift was due to the presence of either wood chips or clay in the subsurface. Finally, 5 of the 7 were located at the confluence of the river and either Moses Kill or Snook Kill, locations known to be depositional in nature.

Table 6-2. Analysis of 1984 Non-Cohesive Cores with High PCB_{3+} Mass							
Grad Number	Location	Total Mass (g/m^2)	Layer of Max PCB Mass (in)	Max PCB (g/m^2)	Surface Principle Fraction/ Texture	Max PCB Principle Fraction/ Texture	In hot spot?
32833	Moses Kill	11.7	5-18.5	10.4	600/27	300/19	Y
32837	Moses Kill	27.1	9-23.5	26.8	600/27	100/12	Y
33378	~191.1/Center Channel	12.9	17-29.5	8.6	500/16	500/16	Y
33639	Snook Kill	42.1	14.5-29	38.5	300/18	300/22	Y
33642	Snook Kill	24.7	30.5-39	10.2	300/18	300/22	Y
33648	Snook Kill	19.2	17-31.5	11.5	300/18	300/22	Y
33935	~192.5/East Shore	53.9	5.5-13	49.3	300/18	300/22	Y

Key to Texture and Principal Fraction				
Texture	Code	Description	Principle Faction	Description
12	CLAY	Clay	100	Principal fraction is clay.
16	CS-WC	Coarse sand and wood chips	200	Principal fraction is silt.
18	FN-SND	Fine sand	300	Principal fraction is fine sand.
19	FS-CL	Fine sand with clay	400	Principal fraction is medium sand.
22	FS-WC	Fine sand and wood chips	500	Principal fraction is coarse sand.
27	GRAVEL	Gravel	600	Principal fraction is gravel.

The uncertainty regarding interpretation of the non-cohesive sediment PCB data impacts the predicted rate of recovery in non-cohesive sediments, particularly those sediments subject to net erosion (about 60 percent of the non-cohesive sediments in the TIP). Alternate assumptions about the depth of contamination would force recalibration but would produce time trends similar to those currently produced by the model. Nonetheless, the primary conclusions drawn from the modeling would not change as a result of such recalibration.

Other aspects of the modeling are relatively insensitive to the non-cohesive sediment PCB uncertainty. The cohesive sediment PCB concentrations are known with much greater certainty because the data set includes many sediment cores for which the depth range of the samples is reported. Additionally, the PCB concentrations that the model calculates in cohesive

sediments and in fish are relatively insensitive to the uncertainty regarding the non-cohesive sediment initial condition. This insensitivity is due to: 1) minimal redistribution of PCBs between the two sediment types; and 2) fish are exposed to PCBs in the cohesive sediments and not the non-cohesive sediments.

6.2 Quality of Model Fit and Calibration Strategy

Comment: (p. 12, USEPA 2000b)

For the older USGS PCB data, the QEA model appears to yield a severe under-prediction of the data for the period from 1977 to 1983, while over-predicting the data from 1984 to 1989. Indeed, the water column PCB predictions for the late 1980's are so poor that the discussion of the bioaccumulation model rejects use of the fate and transport model results

Response:

The report acknowledges that the model does calculate water column PCB levels that differ from the USGS data in systematic ways: lower in the late-1970s and higher in the mid- to late-1980s. However, there is no consistent difference between model and data for the period from 1980 to 1983. The report presents potential explanations for the systematic differences that do exist (see p. 4-64, Vol. 2, QEA 1999). The reviewer does not challenge these explanations, nor the conclusion that the differences do not indicate a fundamental weakness of the model. We stand by the statements in the report. It is interesting to note that the USEPA model yields biases almost identical to those of the QEA model (Figure 7-20, Vol. 2, USEPA 2000a), yet those biases were not considered evidence of a significant weakness in the model.

Comment: (p. 12, USEPA 2000b)

The calibration shows fits to event data on compressed time and concentration scales, making evaluation of the actual fit between model and data difficult for the three events shown in Vol 2, Figure 4-53 (i.e., 1982, 1983, and 1993). Close inspection of these results suggest that the

model significantly under-predicts peak PCB concentrations at all locations for the 1983 event. For the 1993 event, the model under-predicts the peak concentrations at Stillwater and Waterford and the same usefulness of this particular event for model-data comparison is compromised by the uncertainty in the estimate for the Fort Edward PCB load. The fits are probably acceptable considering the uncertainties in PCB and solids loadings, but should not be referenced as verification of the sediment transport model. Scatter plots of PCB model to data comparison in the water column for various locations should have been generated and provided in the report.

Response:

The model under predicts the third peak in PCB concentration during the 1983 high-flow period, but it compares well with the first two peaks. There is a decrease of TSS from the second to third peak, whereas there is an increase in PCB_{3+}, suggesting that resuspension is not responsible for the PCB_{3+} values over 1000 ng/l. Based upon our data analysis of the Fort Edward loading, we think these high peak concentrations may be attributable to DNAPL entering the water column from upstream sources.

In comparison, the USEPA model for the 1983 event (Fig. 7-23, Vol. 2, USEPA 2000a) does better in capturing the third peak of the 1983 period, but does not capture the PCB_{3+} increases of the first two peaks.

Although we have not employed a statistical comparison of the 1993 high-flow period, the visual comparison presented in Figure 4-53 (Vol 2, QEA 1999) shows that the model predicts very well the water column PCB_{3+} concentration at all three locations. Only two measurements out of 12 (4/10/93-5/1/93) at Stillwater are under-predicted. These two high measurements also have no corresponding high measurements at the other two locations, and therefore may be attributable to expected variation in sampling.

Overall, the QEA model performance over these three high-flow events is much better than "acceptable". Given the temporal (daily) and spatial (miles) resolution of both the model and data over these short-term events, we feel that this high-flow validation is extremely good.

Comment: (p. 12, USEPA 2000b)

QEA has made selective use of the available data for calibration within the TIP, focusing mainly on the limited number of TIP center channel observations. We do not concur with the statement that TI Dam west shoreline data cannot be used for model calibration (Vol 2, p.4-62). These data are valid observations, but reflect lateral gradients in PCB concentrations under certain flow and upstream loading regimes. Excluding these data completely from model comparisons produces a significant temporal gap (1993 through 1996) for assessing the PCB model predictions through the TIP during the period of most intensive water column data collection. (...)

Response:

We disagree that we made "selective" use of the data. We considered all the data and recognized the limitations of the data from the shore versus the main channel. The PCB data collected at the TI Dam west shoreline are not comparable to the model. These data represent PCB concentrations in nearshore waters whereas the model calculates cross channel averages. USEPA has attempted to correlate these shoreline concentrations to the main channel observations (RMBR Vol 1, p89) by segregating the data by flow and PCB concentration. Figure 6.2 shows the paired observations for each of the USEPA flow/concentration regimes. For all of these regimes, the correlation of shoreline (TID-West) to main channel (TID-PRW2) observations is weak. The bulk of the data collected occurs during low Fort Edward concentrations (<15 ng/1) and tends to cluster with no linear correlation. The rest of the data shows significant deviations from linearity at higher concentrations, with no apparent correlation to flow or upstream concentration. Hence, the shoreline data cannot be reliably converted to water column cross-sectional averages and it is inappropriate to use these converted values for model-data comparisons. The conversion problem is likely caused by the dynamics in the

hydraulic relationship between the nearshore plume and the rest of the river which precludes the development of a consistent relationship between PCB concentrations measured at the west shoreline and in the main channel.

Comment: (p. 12, USEPA 2000b)

...the QEA calibration/validation strategy (Vol 2, pp 4-39 – 4-40) attributes an inappropriate level of certainty to the parameterization of the model and implies that the model is more tightly constrained [than] the available data allow. The depth and extent of particle mixing are presented as the only significant parameters whose values are not tightly constrained. This ignores important and seemingly inappropriate assumptions about the depth of contamination in non-cohesive sediments, and also attributes an unjustified level of accuracy to the sediment transport model calibration...the gross rates of settling and resuspension computed by the sediment transport model are unconstrained...the specific calibration of the sediment transport model, which is to a degree arbitrary, led to the choice of particle mixing rates and depths.

Response:

As discussed earlier (Section 6.1), the depth of contamination in non-cohesive sediments is not tightly constrained by the available data. The data are sufficient only to specify that this depth is probably somewhere between 6 and 13 cm. USEPA correctly points out that this uncertainty was not acknowledged fully in the report. However, the characterization of the depth assumption used in the model as "seemingly inappropriate" has no factual basis and is not supported by the data analysis presented earlier.

The rates of settling and resuspension computed by the sediment transport model are not arbitrary. The rates of deposition are based upon a body of research performed in the field of particulate dynamics that is cited in the report. The resuspension properties of cohesive sediments are based on site-specific experimentation that is described in the report (QEA 1999).

Comment: (p. 13, USEPA 2000b)

The calibration of the model also involved adjustments to the estimated PCB load at Fort Edward in order to fit the surface sediment trajectory in TIP (Vol 2 p. 4-40). No details are provided in the report allowing an assessment of the degree to which PCB loading was used as a calibration variable.

Response:

The analysis of the historical loading clearly identified the occurrence of "pulse" loading events of PCBs at Fort Edward that tend to occur under high flow, but not necessarily high solids. The analysis presented in the QEA report provides our best assessment of the magnitude and frequency of these loads. The frequency analysis of Fort Edward PCB data determined the number of pulse events that we would expect to occur over the 1977-91 period. These events were distributed evenly over all years of the calibration (3 pulses/yr) and were chosen to occur during periods of high deposition. The magnitude of the pulse load was estimated to be 100 lbs/event by model calibration to the 1991 TIP surface sediment PCB_{3+} data. This value was then compared to the loading data and the magnitude of the pulse loading was found to be consistent with observations. Sensitivity analysis (Fig 4-57, Vol. 2, QEA 1999) showed that the overall trajectory of TIP surface sediment PCB decline from 1977 to 1998 was insensitive to the uncertainty associated with the pulse loads.

Comment: (p. 13, USEPA 2000b)

...the level of constraint provided by the sediment transport model is compromised by a lack of simultaneous calibration to PCB, large uncertainties in solids loads and in a number of sediment transport model parameters.

Response:

As discussed in Section 3.2, the comment about a supposed "simultaneous constraint" imposed by the adjustment of the sediment transport parameters in an effort to calibration the PCB model is based on flawed logic. A strong indication of the accuracy of the QEA model is its ability to reproduce the PCB observations without having to resort to an adjustment of the sediment transport model away from a parameterization most consistent with theory and independent observations. USEPA's approach to simultaneously adjusting sediment transport parameters and PCB fate parameters to maximize the fit of the model to data is a weakness because of the added degrees of freedom (i.e., fewer parameters are constrained by independent information) in its model. The fact that the USEPA sediment transport model and PCB fate model cannot be independently calibrated highlights the inferior nature of the underlying framework.

Comment: (p. 13, USEPA 2000b)

...the calibration strategy states that PCB loadings were adjusted in the model calibration. Thus, it is misleading to say that the PCB model is tightly constrained and calibration error was attributed to sediment mixing processes.

Response:

As discussed above, PCB model calibration to the 1991 data was based upon both pulse loading and sediment mixing processes. Calibration to the 1998 surface sediment data was almost entirely controlled by sediment mixing parameters.

Comment: (p. 13, USEPA 2000b)

The calibration approach assumed that initial errors in predicting the rate of non-cohesive sediment concentrations declines were due to the specification of mixed depth or mixing rate... The calibration discussion fails to consider other possible explanations and instead

contradicts existing observations in order to force the model to describe sediment trajectories. Other explanations include errors in the sediment-water transfer rates of PCBs from non-cohesive sediment and/or errors in sediment settling and resuspension rates.

Response:

USEPA is correct in stating that it is possible that errors in the sediment-water transfer rate of PCBs from non-cohesive sediment and/or errors in sediment settling and resuspension rates may have impacted model calibration. However, these rates were estimable independent of the process of PCB fate model calibration. In calibrating the PCB fate model we chose to fix these rates at their best-estimate values and adjust parameters for which we had little model-independent information. It is our professional opinion that this is the optimal approach to calibration because it is objective and relies on the best available information. It is not defensible to adjust parameters for which we have reasonable estimates.

We do not understand the USEPA contention that the "...calibration discussion...contradicts existing observations..." No specific contradictions are cited, and we believe that none exist.

6.3 Specification of Upstream Boundary Conditions

Comment: (p. 14, USEPA 2000b)

It is not clear exactly how the pulse load was entered into the model. Presumably, the time series was first matched up with any observed pulse loads. Any inferred, but unobserved pulses should then have been assigned to days with flow greater than 15,000 cfs. In two years (1988, 1991) there were no flows greater than 15,000 cfs, but QEA's graphs suggest pulse loads were apparently assigned anyhow.

Response:

Analysis of the historical USGS Fort Edward loads revealed the existence of three loading components, a base loading, a resuspension based loading, and an erratic (presumably DNAPL) pulse loading. There is little information available to assess the magnitude and timing of these pulse loads. The data analysis clearly identified another loading component above the base and resuspension based loading. As discussed in Section 6.2, the frequency of pulse loadings was determined by the analysis of data, however these loads were distributed evenly (3/yr) over 1977-91. The timing of the pulse loads were chosen to coincide with the maximum solids deposition during each year. However, other combinations of timing and magnitude of pulse loadings would also have been able to produce similar results, although the data does not support a more definitive combination.

The addition of this DNAPL pulse load was only necessary during those periods of sparse USGS data (1977-1991). After this time, weekly sampling was sufficient to accurately capture the PCB_{3+} loadings at Fort Edward. Accordingly, the model is sensitive to the magnitude of the pulse load (Figure 4-57, Vol. 2, QEA 1999) in the 1980's to early 90's, but these effects diminish by the end of the calibration and would have a negligible effect in the future. As such, model projections are not altered substantially by changes in this historical loading.

6.4 Other External Forcing Functions for PCB Model

Comment: (p. 14, USEPA 2000b)

Uncertainties in the sediment transport model propagate directly into uncertainties in the PCB transport model. As stated previously, simultaneous validation of the sediment and PCB models is needed to confirm model performance

Response:

As stated in Section 3.2 on calibration strategy, the fact that the sediment transport and PCB fate models are calibrated independently, yet the models are able to reproduce the PCB observations, is a strong validation of the QEA model. Further, by linking the calibrations as USEPA has done in its modeling introduces greater freedom to vary parameters without mechanistic justification and results in a poorly constrained model.

Comment: (p. 14, USEPA 2000b)

The significance of an assumed zero sand content in the Moses Kill solids loading (in the sediment transport model) on the predicted decline in surface sediment PCB_{3+} concentrations...should be presented. Was an initially under-predicted PCB_{3+} sediment trajectory used as the basis for adjusting the sand content of the Moses Kill solids loads to zero?

Response:

The sand content of Moses Kill was established as part of the development of the sediment transport model, independent of the PCB model. See Sections 5.1 and 5.2 for discussion of sand content of Moses Kill. There was no adjustment of any component of the sediment transport model in order to improve the PCB calibration.

Comment: (p. 14, USEPA 2000b)

GE temperature data for TI Dam were applied to the entire Upper Hudson River model domain, including both water and sediment (Vol 2, p. 4-17). Temperature data in the USEPA Phase 2 database suggest that there are longitudinal differences in water column temperatures moving downstream from the TIP. A spatially invariant temperature forcing condition obviously does not take this into account. Justification for this assumption should have been provided, since additional water column data were available to construct temperature time series for different reaches of the river.

Response:

The only processes affected by temperature in the PCB fate model are partitioning and volatilization. The USEPA (p. 102, Vol 1, USEPA 2000a) states that the maximum average temperature difference between Fort Edward and Waterford is only about 3.6°C in summer. Such a variation produces partitioning changes of approximately 13%. Partitioning changes of this magnitude had little impact on model calibration (Section 4.6, Vol 2, QEA 1999).

Similarly, same-day temperature variation is not large enough to cause significant variations in PCB_{3+} volatilization. Under representative conditions of velocity (0.2 m/s) and depth (2.5m), a 3.6°C increase in temperature would result in only a 10% increase (0.37 to 0.41 m/d) in the overall volatilization rate. Although no sensitivity was presented for the uncertainty associated with PCB_{3+} volatilization, changes of this magnitude will not significantly affect model calibration.

6.5 Empirical Sediment-Water Transfer Coefficient

Comment: (p. 14, USEPA 2000b)

To obtain a reasonable fit to the water column concentration data, QEA found it necessary to introduce an empirical transfer coefficient representing "mass transport at the sediment-water interface by mechanisms other than hydrodynamic resuspension." The necessity of including such an empirical factor means that QEA's application is not a truly mechanistic approach. (...)

Response:

While it is true that the approach used to determine k_f is empirical, USEPA is not entirely correct in stating that it is not truly mechanistic. Furthermore, empirical relationships are very commonly derived in scientific endeavors. Consider the classical empirical relationship of

Darcy's Law for predicting ground water flow or Ohm's Law for electricity. A purely empirical formulation is one that is based upon statistics and/or data relationships, contains parameters that are not transferable to different systems, and often has little theoretical basis. The equation used to define low flow sediment flux in the QEA model (Eqn. 4-28, Vol. 2, QEA 1999) was used to back calculate a time-series of k_f values. In that regard, the approach is empirical. However, the timing of the seasonality is consistent with that of biological activity observed in freshwater systems similar to the Hudson (e.g., Rhea *et al.*, 1998) and the calculated values of k_f are consistent with those estimated for other systems (e.g., Alcoa, 1999). Additionally, the approach does have a theoretical basis, as the equation is based on Fick's law of diffusion. It was discussed in the report that the low flow exchange process represents a combination of several mechanisms, including diffusion, bioturbation, and groundwater advection. Each of these can be represented individually by the flux equation used in the QEA model:

1) pore water diffusion is by definition a Fickian process, and k_f is representative of the diffusion coefficient divided by a characteristic mixing length;

2) since pore water PCB concentrations are much greater than those in the overlying water column, the water column concentration in the flux equation is essentially zero, and therefore, PCB flux from groundwater advection may be expressed by the same flux equation, in which k_f would then represent the Darcy velocity (i.e., volumetric groundwater flux); and

3) bioturbation is a process in which pore waters and sediments are disturbed by organisms, resulting in an enhanced mixing, which may be thought of as random motion of the particles (i.e., a Fickian mixing process); k_f can therefore be used to express the strength of this mixing.

The summation of these processes can therefore be lumped into a single k_f, as discussed in the report. Because it is not possible to differentiate these mechanisms using measurements, the formulation is not rendered purely empirical. In fact, the mechanism of diffusion, as defined by a diffusion coefficient is not strictly mechanistic. If one could track each and every molecule,

the net Brownian motion resulting in mass transfer across a concentration gradient would not need to be expressed through the diffusion coefficient. Clearly, this is not feasible, as measurement of such motion is impossible; much in the same way differentiation of low flow sediment flux mechanisms in the Upper Hudson River is not possible given the available data. Therefore, while an empirical analysis was used to determine the values of k_f, its use for representing low-flow sediment-water exchange processes can be tied to the mathematical formulation for a combination of mechanisms.

Regardless of whether k_f is mechanistic or empirical, the most important point is that the representation of low flow exchange in the model must accurately represent the system and must not alter future projections under various assumptions regarding remedial actions. The data-based assessment of k_f seasonality in the QEA model provided the best means by which to represent this observation on the system into the modeling framework. Contrary to USEPA's comment, this "empirical" analysis strengthens the modeling of sediment water exchange because:

1) it ties the process to system data, and

2) although k_f was calibrated to a subset of the data (i.e., 1998), the model was able to reproduce the entire twenty-year data record, thereby serving as a validation of this representation of low flow exchange in the model framework.

Therefore, the best approach given the available data and current understanding of low flow sediment-water exchange mechanisms is that which was used in the QEA model. In fact, this approach was also adopted by USEPA's modelers (USEPA, 2000a).

Comment: (p. 15, USEPA 2000b)

The equation used by QEA to specify the mass transfer coefficient (Vol 2, Equation 4-28) is based on surface sediment porewater concentrations only, implying that transfer is mediated by the dissolved phase. However, an analysis of congener patterns in the low-flow PCB gain

across the Thompson Island Pool suggests that the mass transfer is driven by a combination of pore water and particulate-based transfer (Butcher and Garvey, 1999).

Response:

There are no direct data to support modeling of this particulate-based transfer, as it cannot be differentiated from pore water exchange. In addition, the Butcher and Garvey (1999) analysis contains several parameters that cannot be constrained by site-specific data. The analyses presented by Butcher and Garvey (1999), which were originally described in USEPA (1998), are similar to those presented in the QEA model report, except that the low flow sediment flux equation is characterized by two mass transfer coefficients (one for pore water and one for particulate PCBs) rather than one. Using the flux equation, these coefficients cannot be determined uniquely (i.e., there is one equation with two unknowns), and the authors were therefore required to perform an optimization calculation based on several congeners to arrive at estimates of the two. While multiple mechanisms may be acting in unison to result in the observed low flow sediment flux, modeling them separately is not supported by the data and adds an additional degree of freedom to the model framework.

Further, Butcher and Garvey (1999) introduce an additional parameter (d_f) to describe the partitioning process associated with the particulate transfer mechanism. The value of d_f represents the fraction of sediment-bound PCB congeners that desorbs into the water column during particulate transfer, which the authors assume can be determined from USEPA's paired water column particulate-dissolved PCB data (USEPA, 1997 – DEIR). In making this assumption, the Butcher and Garvey (1999) analysis uses two very different sets of partition coefficients to describe the behavior of surficial sediments when they are in place in the top few cm of the bed versus when they are resuspended into the water column during bioturbation. The 1991 GE sediment pore water data were used to quantify pore water transfer in this analysis, and therefore the corrected 3-phase coefficient estimates presented in Table 2-2 of USEPA (1998) should represent partitioning in the surface sediments. However, the fractionation of particulate PCBs under the hypothesized particulate transfer mechanism (i.e., d_f) was calculated using the water column-derived partition coefficients USEPA developed in the DEIR (Table 3-8; USEPA,

1997). As shown in Figure 6-3, these two sets of K_{OC} values differ for the congeners used in the Butcher and Garvey (1999) analysis, especially for some of the lower chlorinated congeners. Regardless, the partitioning characteristics within the surficial sediment mixed zone in this hypothesized particulate transfer mechanism (i.e., particle f_{OC}, DOC, and bulk solids concentration) will differ from the conditions under which PCBs partitioned in USEPA's water column samples. Therefore, the d_f parameter cannot be known with confidence in the Butcher and Garvey (1999) analysis, which adds greater uncertainty to the calculated mass transfer coefficients.

In summary, the overall process cannot be mechanistically modeled (see discussion above), and therefore it is not appropriate to model pore water transfer separate from USEPA's hypothesized particulate transfer. Apparently, USEPA's modelers agree, as the first release of their HUDTOX model explicitly included both transfer mechanisms (USEPA, 1999 – BMR), but following comments on the original report (e.g., GE, 1999 – BMR comments), the revised USEPA model adopted the same approach as used in the QEA model (USEPA, 2000 – RBMR).

Comment: (p. 15, USEPA 2000b)

QEA apparently recognizes the mixed nature of the sediment source, as the text states (Vol 2, p.4-24) "the source of the TIP PCB load is surface sediments as expressed through desorption and transport mechanisms. These could include a direct porewater exchange process...and/or surface sediment resuspension and subsequent PCB desorption...." Calculating the transfer coefficient, k_f as a function of porewater concentration only and based on a single series of PCB_{3+} observations has two significant limitations: it will not correctly reproduce the congener signal of the load contribution from the sediments, and it may not be extrapolatable to other segments of the river in which physical characteristics controlling sediment-porewater partitioning (organic carbon fraction of sediment, dissolved organic carbon concentration in porewater) differ.

Response:

Because the analysis used to estimate k_f in the QEA model explicitly includes partitioning, different conditions could be modeled by changing the appropriate physicochemical properties that govern partitioning (e.g., K_{OC}, sediment organic carbon, pore water DOC) using the same k_f. This was in fact done in the QEA model, and validates the use of the k_f approach. The values of k_f in the model were essentially calibrated for data in Thompson Island Pool during 1998, and, using the same k_f over the reach from Thompson Island Dam to Northumberland Dam (which has different sediment PCB concentrations and organic carbon content), the QEA model was able to accurately reproduce the low flow water column PCB concentrations measured at the Schuylerville station (Figure 6-4).

Comment: (p. 15, USEPA 2000b)

QEA also implies that only the surface sediments (as represented by the 0-2 cm layer) act as a source of PCBs to the water column, independent of underlying sediments (Vol 2, p.4-24). This is misleading. Deeper sediments act as a source of PCBs to the surface sediment through the various mechanisms that the GE model includes, such as porewater diffusion and particle mixing.

Response:

USEPA is correct in stating that deeper sediments might contribute PCBs to the surficial layer through particle mixing and diffusion, however, it is only the surface layer that directly contributes PCBs to the water column. The QEA modeling framework explicitly represents diffusion within the deep sediment bed and particulate mixing within the biologically active zone, and these processes were also implicitly included in the analysis used to estimate k_f. The model's ability to accurately reproduce the 20-year data record indicates these processes are properly represented within the QEA framework. The point of the analysis was that based on the summer 1998 0-2 cm sediment PCB data (which include inputs from deeper strata to some extent), the pore water PCB composition calculated based on equilibrium partitioning closely

matched that measured in the observed water column TIP congener loading data for the low flow summer 1998 period. This suggests low flow PCB mass transfer from the sediments occurs from the surficial layer.

Comment: (p. 15, USEPA 2000b)

QEA developed the empirical mass transport coefficient based on input/output data for the Thompson Island Pool. The same transfer coefficient is also applied to downstream reaches. The exact processes creating the transfer coefficient are poorly understood, therefore, it is unknown whether the estimated rate applies outside the reach for which it was calibrated. For example, if the transfer coefficient is related to bioturbation, which is greater in cohesive sediment areas, the transfer coefficient may vary with reach-to-reach changes in the ratio of cohesive/non-cohesive sediment areas. The data do not directly support specification of sediment-water mass transfer rates by reach. Thus, while QEA's assumption of a constant rate throughout the system is not inconsistent with the data, alternative assumptions are also possible.

Response:

We agree with USEPA that although it is possible that mass transfer coefficients differ between cohesive and non-cohesive sediments, data do not exist to support using different coefficients in the model framework, as this adds an additional degree of freedom. Apparently, USEPA's modelers also agree, as the first release of their HUDTOX model included mass transfer coefficients that varied by a factor of six in both river reach and sediment type that was not constrained by any data (USEPA, 1999 – BMR). Following comments on the original report (e.g., GE, 1999 – BMR comments), the revised USEPA model adopted the spatially-constant k_f approach used in the QEA model (USEPA, 2000 – RBMR). Nevertheless, the QEA model-data comparison for Schuylerville (Figure 6.4) indicates that the k_f developed for TIP is indeed applicable in the reach between TID and Schuylerville (which has a different areal proportion of cohesive sediments than TIP) through the model's ability to match the observations.

6.6 Sediment Mixing Depth

Comment: (p. 15, USEPA 2000b)

A sediment mixed depth of 3 cm was specified for non-cohesive sediments, "on the basis of model calibration" (Vol. 2, p 4-28). This represents a very shallow mixed depth compared to those typically reported in the literature. Establishment of the non-cohesive sediment mixed depth through model calibration is likely affected by the incorrect specification of non-cohesive sediment initial conditions and the limitation of non-cohesive sediment depth in the model to only 5 cm.

Response:

USEPA has provided no citations to literature to support the statement that mixed layer depths in non-cohesive sediments are "typically" greater than 3 cm. In fact, the Agency's own model uses a mixed layer depth in most of the non-cohesive sediments of the river that varies between 2 and 4 cm. Mixing depths of only a few centimeters are not uncommon. Wong *et al.* (1995) reported mixing depths of 2-5 cm in Lake Ontario. Wheatcroft *et al.* (1994: Journal of Marine Research 52(6):1129-1150) found that sand particles deposited at the sediment surface in Massachusetts Bay were not mixed deeper than 5 cm and most remained close to the surface.

The low organic content and course nature of the non-cohesive sediments (median particle diameter of 800 μm; 20-40 percent gravel), limit biological mixing. Shear forces during high flow events may be a more important mechanism, as has been observed in sand flats subject to tidal action (Grant, 1983).

USEPA provides nothing to support the statement that the non-cohesive sediment initial conditions were incorrectly specified. They were developed from the 1977 survey data in much the same way that USEPA developed its initial conditions. The decision to model only the top 5-cm of non-cohesive sediments was based on professional judgement. Obviously, there is

uncertainty associated with this decision (see Section 6.1). However, the model is not highly sensitive to this assumption, as discussed earlier.

6.7 PCB Partitioning (p. 16, USEPA 2000b)

Comment: (p. 16, USEPA 2000b)

The analysis summarized in Table [3-8] in the DEIR suggests that as much as 50 percent of the apparent dissolved water column concentration of BZ#4 and BZ#8 may be present sorbed to DOM. The two-phase partitioning approach used in the QEA model may thus be satisfactory for representing PCB_{3+} in the water column, but is likely to introduce significant errors in replicating the ratios of congeners or homologue groups – and thus cannot be used for calibration to congener ratios. For this reason, the three-phase approach to representing PCB partitioning behavior appears clearly superior and should have been used by QEA.

Response:

The QEA model simulates only PCB_{3+}, there is no attempt to calibrate the model based on individual congeners. As the two-phase partitioning satisfactorily describes the partitioning behavior of PCB_{3+}, there is no point making the statement that three-phase partitioning should have been used for a PCB_{3+} fate model. Further, the conclusion that mono- and dichlorobiphenyls are associated with DOM and the higher chlorinated PCBs are not is contrary to the current scientific understanding as embodied in a substantial body of literature (e.g., Evans, 1988; Lara and Ernst, 1989; Burgess et al., 1996; Burkhard, 2000). This literature demonstrates that the importance of partitioning to DOM increases with the chlorination level of the PCBs. Typically, association with DOM is unimportant for the lower chlorinated PCBs.

Comment: (p. 16, USEPA 2000b)

QEA has also used a two-phase representation of PCB partitioning in the sediment...This argument contains a number of questionable assumptions. First, the noise in the plot of

sediment versus porewater concentrations is more likely attributable to inappropriate sample handling and compositing procedures used by GE's contractor (as discussed in the DEIR) than to resistant sorption. Second, even after rejecting nearly a third of the data as outliers, the remaining points still show only a weakly linear relationship, not strong enough to conclude "equality between water column and sediment Koc with no evident influence of dissolved organic matter." By contrast, in New Bedford Harbor porewater total PCBs were found to be dominated by PCBs sorbed to colloidal material.

Response:

While it is true that in many aquatic sediments, the third, colloidal, phase plays a significant part in porewater PCB contamination, the degree in which colloidal matter influences porewater concentrations is largely site-specific. The fact remains that the available site-specific porewater PCB data do not show significant influence of DOM. The correlation between sediment and porewater concentrations (Fig 4-27, Vol. 2, QEA 1999) is complex, showing what appears to be a portion of data that obeys the standard partitioning relationship of linearity and a portion that does not, having extremely high apparent partition coefficients. If DOM were a significant portion of porewater PCBs, we would expect to see a decrease in apparent partition coefficient, not an increase. The available site-specific data does not support the assumption of a significant third phase partitioning. The data are limited and complicated by the fact that they represent composited samples, but our analysis represents the best estimate based on available data. Both QEA and USEPA make the assumption that sediment PCB partitioning behaves similarly to the water column in each of their models.

Comment: (p. 17, USEPA 2000b)

QEA has also assumed that different partition coefficients for PCB_{3+} apply above and below Fort Edward. It is true that apparent partitioning coefficients in individual samples were often observed to be higher at and above Fort Edward than downstream during phase 2 sampling...The most consistent approach should be then to apply the same partition coefficients

at all stations, but discount the comparison to observed concentrations where samples appear to represent non-equilibrium conditions.

Response:

Both QEA and USEPA used equilibrium partitioning in each of their models. Understanding the cause of the apparent difference in partitioning between Fort Edward and the downstream stations does not alter the observation of higher proportions of particulate to dissolved PCB_{3+} at these locations. It does not matter whether this difference is due to non-equilibrium or to a change in the character of water column PCBs across TIP; the net effect is the same: higher proportions of PCB_{3+} in the particulate phase at Fort Edward. It is incorrect to assign the same partition coefficient at locations that clearly exhibit differences in partitioning. Merely discounting the comparisons at these locations is not scientifically valid. Particulate and dissolved PCBs are subject to different processes, and specification of incorrect partitioning between these two phases will result in incorrect calculation of PCB fate and transport.

Comment: (p. 17, USEPA 2000b)

...elevated partition coefficients at Fort Edward are also seen for individual congeners in 1993. The effect is also not a consistent one, as the apparent partition coefficient for PCB_{3+} at Fort Edward was lower than the apparent partition coefficient at downstream stations during transect 6. These lines of evidence suggest that anomalously-high apparent partition coefficients observed upstream during other transects may have primarily been a temporary phenomenon of incomplete equilibrium related to the presence of high DNAPL loads from the Bakers Falls source during 1993. If so, QEA's approach of assigning a higher partition coefficient at Fort Edward is likely to be inappropriate for current and future conditions in which the Bakers Falls source has been largely controlled.

Response:

The available data demonstrate higher partition coefficients at Fort Edward. There is no basis for assuming that the current or future PCB_{3+} entering the river upstream of Fort Edward would behave differently than observed historically.

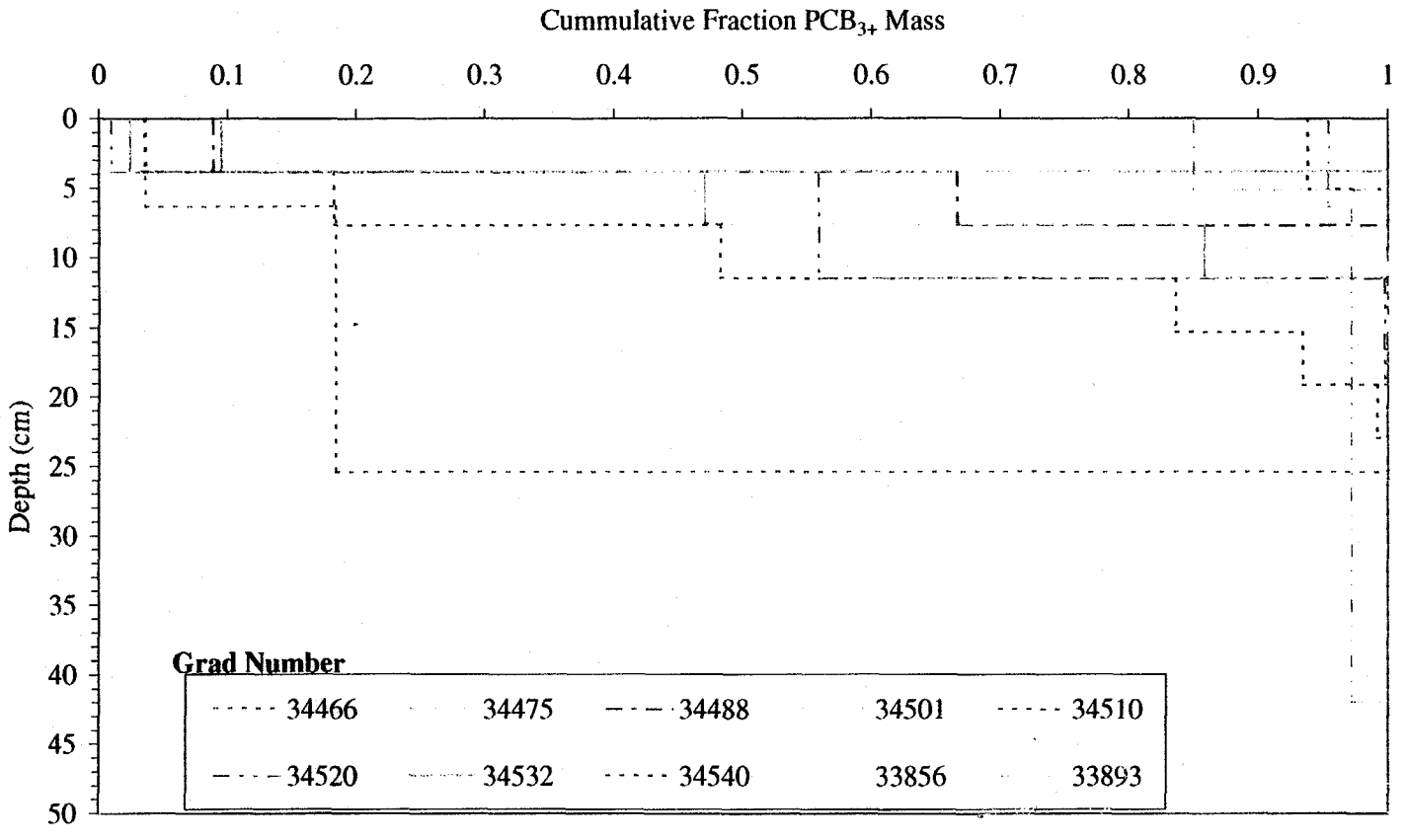


Figure 6-1 Mass fractions for 1984 finely segmented non-cohesive cores. A total of 10 cores are shown.

MJW - mass_noncoh.xls (section_plot)
12/1/00 - 4:52 PM

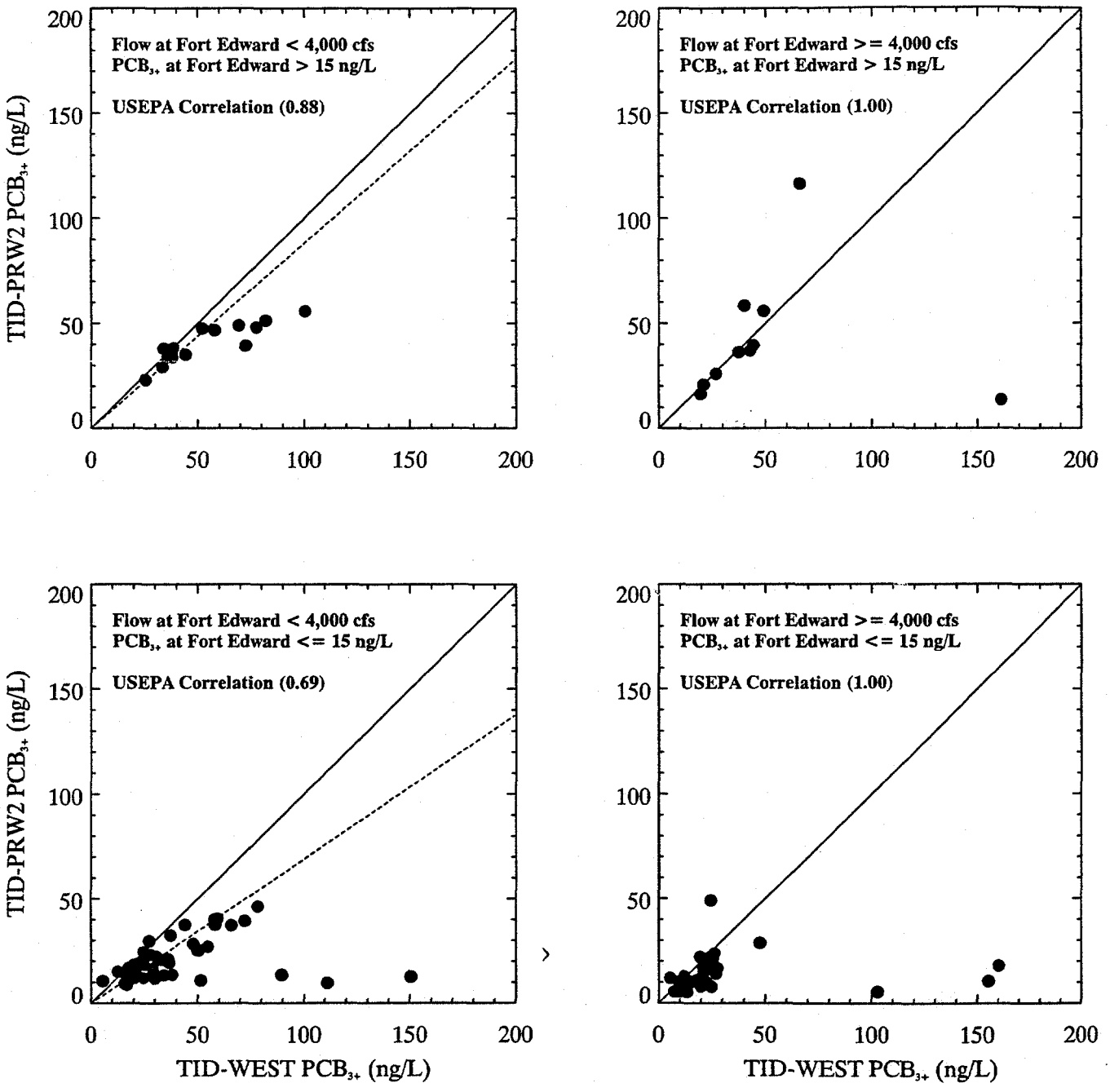


Figure 6-2. Cross plot of 1997-2000 water column data measured at TID-WEST and TID-PRW2.

Notes: Dotted lines represent USEPA correlations. Solid line represents the 1 to 1 line. Data in chart current through 5/31/00 sampling event. Non-detects set to 1/2 MDL

V - D:\GENhud\analysis\Water\misc_analysis\tid_bias.pro
1 Jul 05 13:30:59 2000

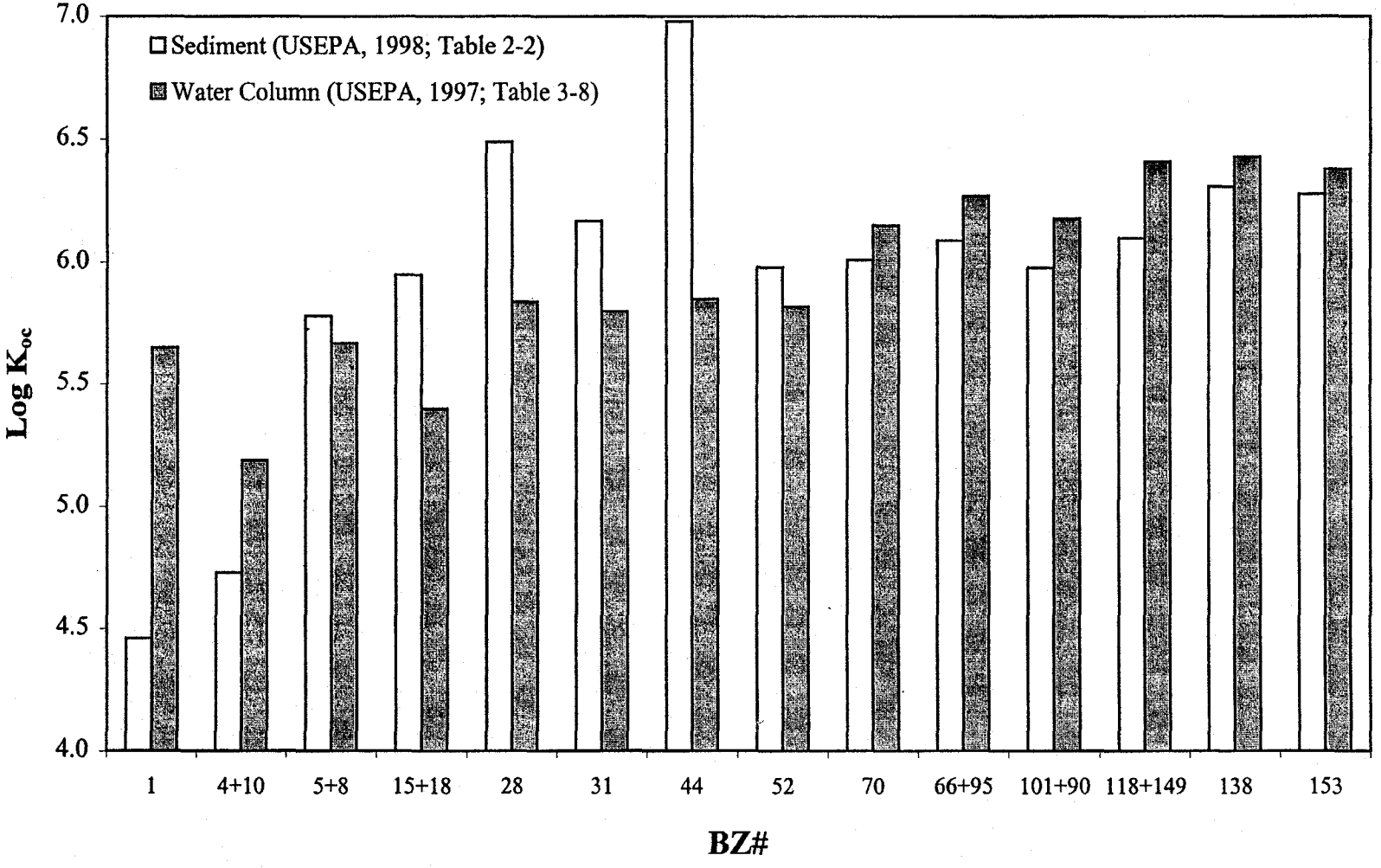


Figure 6-3. Comparison of Sediment and Water Column Log K_{OC} Values used in and the Butcher Garvey (1999) Analysis.

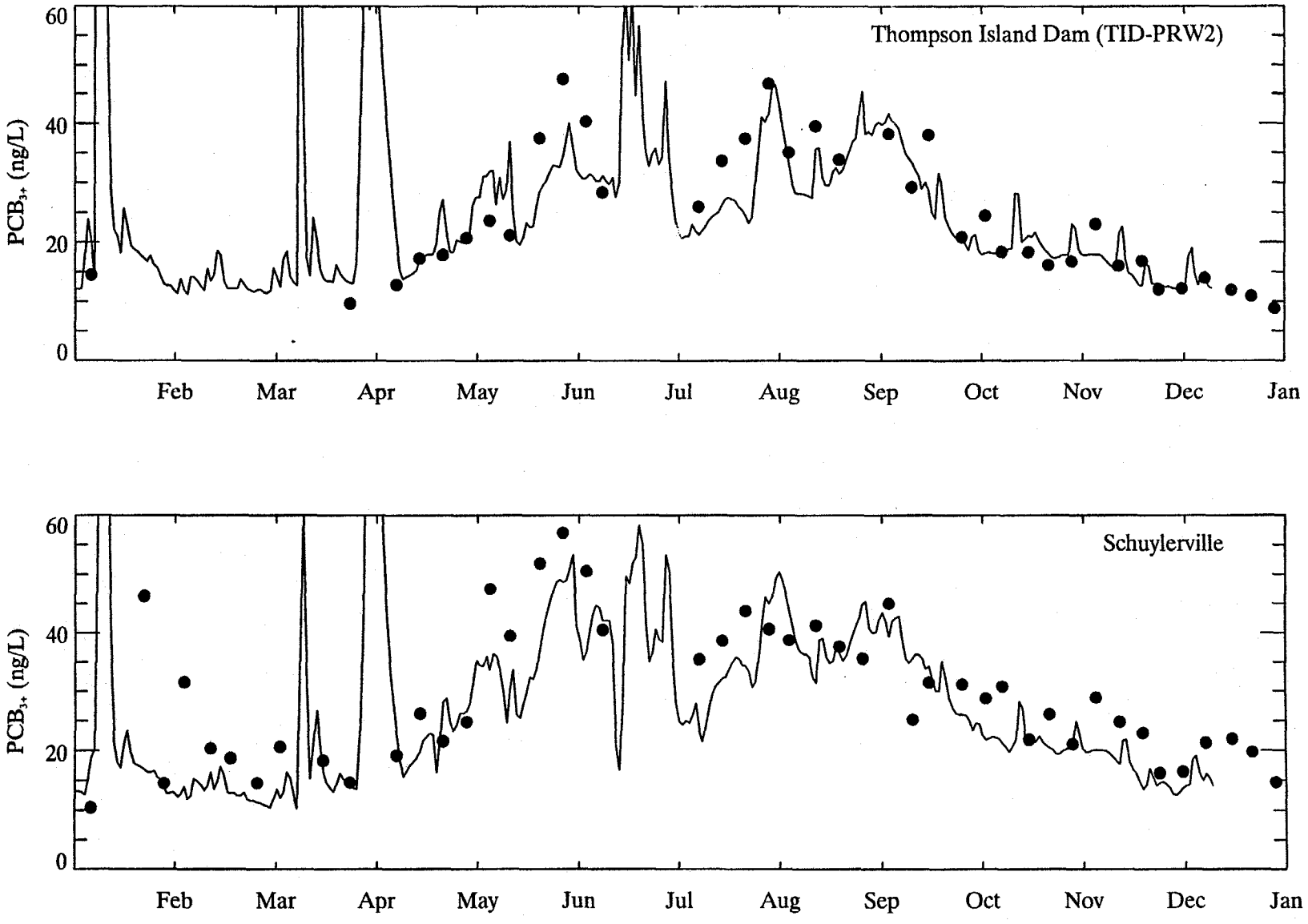


Figure 6-4. Model Calibration for 1998 low flow water column PCB_{3+} data at TID and Schuylerville.
Note: symbols = data; lines = model.

SECTION 7
COMMENTS PERTAINING TO THE BIOACCUMULATION MODEL

7.1 Model Formulation

Comment: (p. 18, USEPA 2000b)

...there are serious questions as to whether sufficient data are available to support such a complex model... (1) Are data available to provide an advantage of more realistic constraints on the more mechanistic formulation? (2) Is the additional complexity relevant to management decisions? It appears that both questions can be answered in the negative.

Response:

It is unclear what USEPA means by a "mechanistic" approach. Both USEPA and QEA have used mechanistic approaches in developing bioaccumulation models. The QEA model represents certain mechanisms, or dynamics, that the USEPA model does not, and it is apparently these differences that USEPA believes do not provide additional insight into the problem. There are at least three important differences in the formulations of the model that are of concern to USEPA: (a) in the QEA model, species-specific respiration rates based upon laboratory experiments and species- and site-specific growth rates measured in the Upper Hudson River are used to calculate food consumption rate and gill PCB uptake rate. In contrast, the formulations in the USEPA model appear to be generic weight-based relationships that are the same for all species; (b) the USEPA model apparently calculates PCB concentrations in one age class. Thus, for example, each year, a six-year-old fish starts the year with concentrations calculated from the previous year for a fish of six years of age. In contrast, in the QEA model, fish are followed through their life cycle, so changes that occur with age are explicitly modeled; and (c) in the QEA model, fish grow throughout the growing season, whereas the USEPA model uses one constant body weight over the year.

Each of the QEA model enhancements is based on an understanding of fish biology and/or site- or species-specific data. Therefore, the answer to (1) above is yes. Because of this, the model is more tightly constrained to the particular conditions of the Upper Hudson River. This means that the model provides a more realistic representation of the complex biological processes underlying PCB bioaccumulation in the fish, which in turn provides additional assurance of its predictive ability. Therefore, the answer to (2) above is also yes.

Comment: (p. 19, USEPA 2000b)

The model is more empirical, and less mechanistic than it appears.

Response:

Section 5 of the report provides a description of the theory upon which the model framework is based and the data that were used to apply the framework to the Upper Hudson River. Limitations were carefully spelled out, including the empiricism inherent in calibration, as echoed in the USEPA comments. Nevertheless, the model is mechanistic, in that the relationships between PCB levels in fish and the key processes that control bioaccumulation are described mathematically using equations that are based on current scientific understanding of bioenergetics and toxicokinetics. The contributions of food uptake, gill uptake and elimination, and growth rate dilution to PCB bioaccumulation are all described quantitatively using realistic formulas. The description of each of these processes is based, in turn, on current scientific understanding of quantitative relationships between growth, respiration and food consumption (bioenergetics), and between lipid content, chemical hydrophobicity and PCB elimination. In these ways the model is mechanistic. The model is empirical in that values for specific parameters were adjusted within the bounds of the available data so as to produce the best possible match between computed and observed PCB levels it the fish. This is common practice in modeling.

Comment: (p. 18, USEPA 2000b)

At numerous points in the QEA model application simplifying assumptions are required that relax the underlying theoretical construct. For the model bioenergetic and toxicokinetic components, it is admitted that "there is insufficient information to develop a full multi-compartment model and to estimate values for all of the necessary rate constants and partition coefficients."

Response:

The detail with which mechanisms are described in any model is always limited by the available data. There is always an empirical aspect to any model, in that certain parameters must be estimated based on laboratory or field measurements or calibration. This does not make the mechanistic descriptions that are in the model invalid or completely empirical. For example, the insufficiency of the data to describe a multicompartmental model does not make the existing description of elimination rates purely empirical: rates are still described as being related to lipid content and chemical hydrophobicity (as they are in the USEPA model). Both of these descriptions are mechanistic, based upon partitioning theory that tells us that the transfer of hydrophobic contaminants from lipid to water is related to the chemical's hydrophobicity as well as the lipid content of the organism.

Comment: (p. 18, USEPA 2000b)

Relationships between age and weight are based on data from a single year, which are not necessarily representative of long term trends.

Response:

The use of site-specific data to parameterize a model is in principle more supportable than data developed for other sites or species or generic formulae. Site- and species-specific growth rates were used in the QEA model in lieu of generic formulations. Should information

become available that indicates changes in growth rates over time, its impact on the model calibration should be considered.

Comment: (p. 18, USEPA 2000b)

Exposure concentrations are also not known at a scale commensurate with the detail of the model. Sediment exposure is estimated based on (uncertain) PCB concentrations in cohesive sediment (only) predicted by the fate model, and "inaccuracy in the fate model calibration affects the bioaccumulation model calibration". Further, the spatial extent over which average exposure is unknown, because "the extent to which fish move within each dammed reach is not known."

Response:

The scale at which exposure concentrations are known is irrelevant to the issue of the level of detail in the mechanisms included in the bioenergetic component of the model. Reducing the uncertainty associated with each independent, important component of the model (e.g. exposure concentrations and bioenergetics) will improve model reliability.

The QEA model includes a level of detail that is commensurate with bioaccumulation theory, and the available site-specific data. For example, site- and species-specific growth rates for each fish species are used in the calculation of growth dilution and of food consumption rates. In contrast, the growth rates employed in USEPA's model are not based on the site-specific data. Realistic growth rates can make a great deal of difference to the resulting calibration, as growth rates can dominate the negative terms in the bioaccumulation equation. Growth rates are also used in the calculation of food consumption rates in the QEA model; food consumption rates determine the PCB dose. Thus, independent of the uncertainty associated with exposure concentrations, the realism of the bioaccumulation model is enhanced by the inclusion of the bioenergetic mechanisms and the site- and species-specific data.

Exposure concentrations *are* known at a spatial scale commensurate with the detail of the model. In fact, exposure levels are computed by the fate model on a finer scale than the reach-

specific scale of the bioaccumulation model. The results of the fate model have been compared with data on a reach-by-reach basis, and in some cases on a segment-by-segment basis, indicating the accuracy of the model on a scale sufficiently resolved to provide exposure concentrations for the bioaccumulation model.

The water column concentrations computed by the fate model are compared with data on a within-year basis, a scale sufficiently resolved for the bioaccumulation model.

The quantitative basis for the use of reach-average food web exposures in the model was provided in the report (Section 5.3.2). Nonetheless, fish movement is a source of uncertainty, also as described in the report (Section 5.4). Indeed, the existence of subpopulations exposed over areas smaller than a reach may explain some of the variability in the data and consequently some of the variation in the model/data comparisons.

Comment: (p. 18, USEPA 2000b)

Finally, the calibration of the model involves adjusting the empirical resistance coefficient, which is an arbitrary fitting factor on bioenergetic response, and the relative contributions of benthic and pelagic food pathways in fish diet, for which observed gut content data are available. This calibration approach suggests that available data are not sufficient to support either the bioenergetic or food chain exposure components of the model.

Response:

USEPA has mischaracterized the process of calibration: calibration of two parameters does not invalidate the mechanistic basis of the model. The formulations are still mechanistic; they just don't include *every* mechanism.

The empirical resistance parameter is a toxicokinetic parameter, not related to bioenergetics. It affects the rate at which PCBs are eliminated from the body. In contrast, bioenergetics refers to the component of the model concerned with energy flows through the

organism, in particular the rates of respiration, growth and food consumption. The empirical resistance factor is, appropriately, a calibration parameter: we provided a discussion of the need for this refinement to the model in the report, including the lack of data to constrain the parameter (Sections 5.1.1 and 5.2.5). USEPA did not even consider the issue, which affects both models.

The advantage of the bioenergetic approach was discussed above. There is independent support for the formulations used in the model, and site- and species-specific data to parameterize them.

The food chain exposure component of the model consists of the predator/prey relationships specified in the input files. Both the USEPA and QEA models provide complex food webs in their models. The fact that the data do not permit precise determination of the feeding preferences does not mean that the model is unable to simulate the food web transfer of PCBs in the Upper Hudson River. Rather, calibration is the process of determining what food web structures are consistent with the PCB data, as well as our understanding of bioenergetics and toxicokinetics.

Comment: (p. 18, USEPA 2000b)

The model is not calibrated to intra-year or inter-individual variability in concentrations. This suggests that the additional complexity of the bioenergetic approach will not yield additional assurance in predicting future responses to remedial alternatives.

Response:

The focus of the effort was to model the population average. Average values were used for key parameters such as growth rate and lipid content, and the model was calibrated against average PCB concentrations measured in the field. Modeling inter-individual variability is irrelevant to the issue of the complexity of the mechanisms included in the model. It is our position that variability within the fish populations should be estimated based upon the field data.

There are no data available to calibrate the model to intra-year variability. Such data would certainly provide additional support for the model, as they would for the USEPA model, but are not necessary for its calibration to the PCB data that are available. The model actually computes changes in contaminant levels during the year, based upon within-year variation in body weight and temperature. The within-year variation in weight is based upon the site-specific measurements of the year-to-year age/weight relationship and the assumption that growth occurs only during the warmer months of the year. The temperature values are based upon site data. This provides realistic representations of growth rate and metabolic rate during the year. The inclusion of realistic information in the model provides additional assurance in the reliability of the model results, even though the model is not calibrated to intra-year variation in PCB levels.

7.2 Representation of PCB Forms

Comment:

(p. 3, USEPA 2000b) *The discussion of PCB trends in fish (Vol 1, Sec. 5.1.3) is marred by the fact that QEA has used NYSDEC-reported total PCBs, which do not constitute a consistent analytical quantity over time. As demonstrated in the BMR (and the Revised BMR), changes over time in analytical methodologies and laboratories have a substantial impact on the reported total PCB concentration in fish; however, most of the different results can be converted to a consistent basis, as explained in Volume 3 of the BMR.*

(p. 18, USEPA 2000b) *As a calibration target, QEA has chosen instead the total PCB measure traditionally reported by NYSDEC, which is the sum of Aroclor quantitations. As discussed in the BMR, these Aroclor sums are (1) not a consistent quantity over time and changing analytical methods; and (2) not equivalent to either total PCBs or* PCB_{3+}.

(p. 19, USEPA 2000b) *As a result of these data issues, the QEA bioaccumulation model is not properly calibrated. Instead, it has been calibrated to an artificial quantity which is not*

equivalent to PCB_{3+}, is likely to underestimate true fish body burdens, and which changes its relationship to PCB_{3+} over time.

Response:

All PCB measurements are "artificial" in that they provide an estimate of the amount of PCBs, subject to the limitations and uncertainties associated with any laboratory measurement.

The issues raised by USEPA in the BMR and the RMBR, that changes in analytical methods over time, as well as differences in analytical techniques among laboratories, confound interpretations of historical trends in fish PCB concentrations, are important and warrant careful consideration. It is unclear, however, whether or not it is possible to convert the historical data in a manner that removes all important potential analytical biases. USEPA has attempted to reduce potential biases by converting NYSDEC Aroclor analyses to TAMS/Gradient Phase 2 PCB_{3+} congener concentrations. While this conversion to congener-based PCBs may remove biases due to quantification method, it does not correct for differences in extraction and cleanup procedures.

To explore the issue of data comparability further, NEA, under contract with GE, analyzed a series of NYSDEC sample splits in 1997 and 1998. A complete discussion of these analyses is summarized in a report which is included in Appendix 1. The following figure references refer to Appendix 1. The 1997 split comparison (Figure 2-1) shows little bias of the 83 fish tissue samples run by Enchem compared with those run by NEA at lower PCB concentrations, while Enchem values are consistently greater than NEA values at higher concentrations. Lipid contents measured by Enchem were consistently higher than values measured by NEA (Figure 2-2), resulting in lipid-normalized PCB concentrations that are generally similar in the two labs (Figure 2-3). This suggests that extraction and cleanup procedures differed among the labs, and that wet weight-based and lipid-based PCB concentrations probably need to be adjusted independently. In addition, only six of these samples were run on a packed column by Enchem; the remainder were run on a capillary column, also suggesting that the comparison is largely between extraction and cleanup techniques.

The 1998 split comparison of 82 Upper Hudson River fish tissue samples indicate that EnChem's results are biased high relative to those of NEA on a wet weight basis (Figure 2-4). In contrast to the 1997 data, lipid contents were similar in the two labs in 1998 (Figure 2-5), so that lipid normalization removes only some of the bias in the PCB data (Figure 2-4b). Thus, to the extent that differences in lipid content reflect differences in extraction procedures, there appears to be year-to-year variation in extraction procedures.

In a further effort to isolate biases due to extraction methodology and gas chromatography techniques, NEA quantified both homogenized tissue samples and tissue extracts on a subset of the 1998 NYSDEC sample splits. To evaluate the effect of chromatography method, tissue extracts of 1998 fish tissues received by NEA from EnChem were analyzed using a replication of EnChem's packed column gas chromatography method as well as NEA's capillary column GC method. To evaluate the effect of extraction procedure, EnChem and NEA 1998 fish extracts were re-analyzed by NEA using both the packed column GC and capillary column GC methods. The results of these analyses are shown on Figure 2-10. Figure 2-10a shows that there is a consistently high bias for total PCBs in EnChem's compared with NEA's extraction and analysis method. Using NEA's capillary column GC analysis, EnChem's extracts result in somewhat higher PCB concentrations than NEA's extracts (Figure 2-10b). This indicates that differences in extraction/cleanup procedures are likely to be causing some of the bias between the two laboratories. When NEA extracted the samples and compared results obtained using the replicated EnChem packed column method and its capillary column method, the results were similar, especially at low PCB concentrations (Figure 2-10c). This suggests that packed column quantification is not always different from capillary column quantification.

The results of this analysis indicate that correction factors applied to NYSDEC data must account for both extraction and quantification biases. Additionally, based on the lipid comparison, the extraction bias appears to vary year-to-year, so correction factors would have to reflect the bias at the time of analysis. Furthermore, the quantification bias does not appear to be present consistently in the 1998 splits, based on the similarity between the NEA packed column

and the NEA capillary column analyses of the NEA extracts. The bias variability presented here is the result of only two years of split analyses with only one laboratory. However, the results are compelling enough to warrant closer scrutiny of previous split analyses between NYSDEC contract laboratories and other outside laboratories.

Further evaluation of the split sample analyses discussed here and by USEPA is a prerequisite to the application of correction factors to historical data. Additionally, conversion of historical data for quantification bias should include validation of the "what if" analysis. This can be done by quantifying samples on both packed and capillary columns, and then subjecting the capillary column chromatogram to the "what if" analysis and comparing the results with the packed column measurements.[1] In conclusion, based on the split sample analyses, there is sufficient uncertainty associated with the calculations of the quantification bias to require further analysis prior to the application of correction factors to the historical NYSDEC monitoring data. Furthermore, extraction efficiency may be the more important source of year-to-year variability; this can possibly be explored by a review of historical methods.

Comment: (p. 19, USEPA 2000b)

...none of (NYSDEC's) methods are exactly equivalent to PCB_{3+}, which is the quantity simulated in the PCB fate model. QEA dismisses this problem by stating that... [the use of Aroclor measurements to approximate total PCBs] leads to a bias of less than 5% in model results, because mono- and di-chlorobiphenyl comprise less than 5% of total PCBs in fish from the Upper Hudson River.

Response:

There are two issues discussed by USEPA:

[1] It should be noted that the majority of the 1997 NYSDEC analyses were quantified on a capillary column and thus the conversion factor developed for Aroclor analyses by USEPA should not be applied to these data. Additionally, the adjusted values for the 1991-93 Hale Creek samples should be reviewed: some of the Hale Creek data for Aroclors 1254 and 1260 are reported in separate fields of the database, while only the combined 1254/1260 field was used by USEPA to estimated total PCBs. Thus, the adjusted values for some of these samples are

First, the NYSDEC measurements are not equivalent to total PCBs measured in congener-based analyses. This issue is discussed in response to the previous comment.

Second, the concentration of PCB_{3+} lies within 5% of the concentration of total PCBs for fish from the Lower Hudson River. This conclusion was based upon an analysis of the congener-based PCB measurements in fish from the Hudson River (PCB congener composition measured by General Electric in fish collected by General Electric and by NYSDEC as part of their annual monitoring program).

7.3 Representation of Fish Lipid Content

Comment: (p. 19, USEPA 2000b)

QEA does not explain how lipid values were determined for species/locations/years where NYSDEC sampling is not available. Lipid content ... is likely to vary on a seasonal basis in response to temperature and prey availability. Formulation of the model with a dynamic simulation of weight and energy usage but annually fixed lipid content based on summer observations would seem to be a mismatch that could lead to incorrect results.

Response:

In the QEA model, a lipid content is specified each day, either explicitly in the input file, or by interpolation from values in the input file. The data only provide one estimate of average lipid content for each year, so that value is used throughout the year, except for a period during which lipid content is ramped to the next year's value. Thus, all parameters are specified on a daily basis as accurately as possible based on the available data.

---

underestimates. Finally, some of the adjusted values listed in Table 4-5 do not match values presented in the model/data comparison Figures in the RBMR.

Each year's lipid contents are presented in Figures 5-18 and 5-19 of the QEA report (Vol. 2, QEA 1999). The rationale behind the specification of lipid contents for the 1991 pumpkinseed is presented in Section 5.2.4 of the report. Other years for which there are no lipid measurements (and therefore no PCB measurements) were determined by interpolation, except as follows. Values for pumpkinseed in Thompson Island Pool prior to 1987 were set equal to values used at Stillwater for the same time period. Values for largemouth bass prior to 1984 and brown bullhead prior to 1986 were determined based upon data collected prior to these years between miles 187 and 198. In years with no lipid measurements, there are also no PCB measurements for use in model calibration. Therefore, the uncertainty associated with estimating lipid contents for years with no data has only a limited impact on model calibration.

There are no data from the Upper Hudson River with which to characterize within-year variation in lipid content. The inclusion of information for which there is independent support (within-year changes in weight and temperature) results in a more realistic representation of bioaccumulation in the River and provides additional confidence in the model's realism.

7.4 Kinetic and Bioenergetic Parameters

Comment: (p. 20, USEPA 2000b)

In the end, feeding preferences were apparently used as a calibration parameter.

Response:

As stated in the report (Section 5.3.1), feeding preference was used as a calibration parameter. The procedure followed a common modeling practice: constrain parameters as much as possible using independent information, evaluate the remaining uncertainty, and then calibrate within the constraints of the data. The values used in the calibration were constrained to the extent possible by the site-specific data. In particular, based upon the data, pumpkinseed was constrained to within 25 and 75% PMI. Brown bullhead was constrained to be relatively more

benthic than pumpkinseed. Largemouth bass was constrained insofar as it was required to consume a mixture of forage fish of appropriate size, based upon published feeding studies of largemouth bass. The quantitative analysis of the gut content and community data provide the basis for these constraints, and indicate that further constraints on the feeding preferences were not possible given these data.

7.5 Bioaccumulation Model Calibration

Comment: (p. 20, USEPA 2000b)

Interestingly, QEA found it necessary to introduce some ad hoc modifications to the PCB model output during calibration of the bioaccumulation model. ... This adjustment appears to be an entirely arbitrary choice, designed to improve the apparent fit of the bioaccumulation model. The necessity of including such an arbitrary modification suggests that there are serious deficiencies in either the PCB or bioaccumulation model calibrations.

Response:

This statement mischaracterizes the QEA model. The modification to the water column PCB concentrations was performed for a specific reason, as stated in the report: the fate model overestimated the water column data during the mid-1980's by approximately a factor of two (Section 5.3.2). The degree of overestimation is presented in Figure 5-23 (Vol. 2, QEA 1999). The rationale for the adjustment is the relationship between the results of the fate model and data presented in that Figure. It was, therefore, not arbitrary, and the adjustment was based upon the fate model results, not the bioaccumulation model results. Adjusting for this inaccuracy in the fate model provided a more accurate exposure concentrations for use in the bioaccumulation model. It does not suggest any deficiency in the bioaccumulation model. In fact, the opposite is true: not performing the adjustment would have resulted in a calibration subject to the bias associated with the water column PCB concentrations computed by the fate model. The PCB fate model calibration is discussed in Section 4 of the QEA report.

Comment: (p. 20, USEPA 2000b)

Use in the model of observed lipid concentrations that vary by year and location also improves the apparent fit of the bioaccumulation model relative to what would be obtained from a truly mechanistic formulation. While this approach improves the fit to historical data, it compromises the usefulness of the model for predicting future conditions, for which lipid content has not yet been observed, and is difficult to predict.

Response:

USEPA rightly points out that the estimation of future lipid content for use in projections is subject to uncertainty. However, the statement that use of the year-to-year and location-specific lipid contents in the calibration compromises the usefulness of the model is wrong.

Both USEPA's and QEA's model use observed lipid contents. QEA's model calibration accurately reflects site-specific conditions by including year- and location-specific lipid data. Not only does this not compromise the usefulness of the model, but it actually strengthens the calibration. In fact, a single average lipid content for all locations and years as used in the USEPA bioaccumulation model mischaracterizes each year's actual lipid contents. Therefore USEPA's model is subject to the uncertainty that the calibration might be different if more accurate lipid contents were used.

Finally, there is no contradiction between "a truly mechanistic formulation" and the use of year- and location-specific lipid concentrations. In fact, the incorporation of observed patterns and variability in the field lipid data is completely independent of the mechanistic bioaccumulation formulation that uses these data.

Comment: (p. 20, USEPA 2000b)

In practice, treating both fish diet and bioenergetic response as fitting parameters results in a calibrated model that expresses an empirical (rather than truly mechanistic) relationship

between water and sediment exposure concentrations and average fish tissue concentrations. A more defensible approach would be to use the available data on fish diet either directly or as the basis for a stochastic simulation, rather than taking dietary composition as a fitting parameter.

Response:

The feeding preferences and the elimination rate constant are both important to the PCB concentrations computed by the model and cannot be tightly constrained by the laboratory and field data. Therefore, these parameters were appropriate for calibration to the PCB data. This does not change the fact that the model is constrained by the specific mechanistic formulations and site- and species-specific data: calibration does not necessarily make a model less mechanistic. Rather it accounts for the fact that not all parameters are precisely known and makes use of the PCB data to further constrain them. This is common practice in mechanistic mathematical modeling.

The available site-specific data *were* used directly to estimate the diet of the fish. The data was analyzed in the QEA report (Figures 5-11, 5-12 and 5-13, Vol. 2, QEA 1999), and the uncertainty was quantified. As is normal practice, uncertainty in the data formed the basis for the decision to use diet as a calibration parameter. It also formed the basis for the overall uncertainty analysis of the model presented in the report.

A stochastic simulation (that is, a Monte Carlo analysis) was considered but not performed, because it requires characterization of distributions for all of the key parameters, as well as correlations between them, for which there is insufficient information. Application of a Monte Carlo analysis to a problem for which the distributions of all relevant parameters are not known results in meaningless output distributions. The strategy taken by QEA in assessing model uncertainty involved establishing reasonable bounding values for key parameters based upon the available laboratory and field data, and then applying these alternative parameter values to develop an alternative calibration consistent with the PCB data (Section 5.6).

SECTION 8
COMMENTS PERTAINING TO PREDICTIVE APPLICATIONS

8.1 Prediction of Future Hydrologic Conditions

Comment: (p. 21, USEPA 2000b)

To generate a future series of flows, QEA uses a synthetic hydrograph which includes a Markov model of annual flow rates with disaggregation to daily flows based on the nearest match among the 65 years of available flow records at Fort Edward (Vol. 3, Section 3.1). The QEA projection appears to have a potential low bias, as the historical record used to generate the prediction contains five years in which annual mean flow rates are greater than any generated for the synthetic hydrograph. Omission of the higher flow years could create a corresponding low bias in the projection of PCB loads. Sensitivity of model predictions to alternate formulations of the prediction hydrograph should have been investigated, but is not reported by QEA.

Response:

USEPA appears to have misunderstood the basic goal of the synthetic hydrograph development procedure, which was to develop a realistic hydrograph using a rational and objective approach. A 800-year long synthetic hydrograph was generated, using the statistical methods USEPA mentioned in its comment, and then the first year of the 30-year hydrograph needed for projections was selected **randomly** from the 800-year series. Random selection of the starting year was necessary to produce an objective result that was unaffected by potential human bias. The synthetic hydrograph that resulted from this process is statistically well-behaved and it oscillates realistically about the long-term mean flow rate (Fig. 3-4, Vol. 2, QEA 1999). Higher flow years were not "omitted" as USEPA claims; the rational, objective process used to generate the synthetic hydrograph did not allow that type of intervention.

8.2 Future Solids Loading and Transport

Comment: (p. 21, USEPA 2000b)

For projection of future conditions, QEA applied a sediment rating curve to the projected flow series at Fort Edward to generate a sediment load series. The QEA projections result in average annual solids loads at Fort Edward that are 7% lower than those observed during the hindcast model calibration period, reflecting the fact that all of the annual mean flows used in the projection are several thousand cfs less than the observed flows of 1990 and 1995 included in the hindcast calibration. Despite average solids loads that are somewhat lower than those observed in the hindcast, the QEA projections show a 14% increase in average sedimentation rate within the TIP relative to historic observations, increasing from 0.81 to 0.92 cm/yr (Vol. 3, p. 3-4). The increase in sedimentation rates relative to observed conditions suggests that the model forecast may tend to exaggerate the rate of burial of contaminated sediments within the Thompson Island Pool. Further, the combination of lower upstream solids load and increased sedimentation within the TIP are likely to result in a low bias in estimates of transport of particle-associated PCBs from the TIP to downstream reaches.

Response:

USEPA incorrectly states that "QEA projections results in average annual solids loads at Fort Edward that are 7% lower than those observed during the hindcast model calibration period." What was actually stated in the QEA report was (p. 3-4, Vol. 3, QEA 1999): "Total annual average load to the Upper Hudson River was 7% lower for the 30-year simulation than that specified from the 22-year validation period." A careful examination of solids loading to TIP during the hindcast and projection periods shows that (see Vol. 2, Fig. 3-41 and Vol. 3, Fig. 3-9, QEA 1999): 1) annual average solids load at Fort Edward was 1.2% higher during the projection period (28,990 MT/yr) than during the hindcast period (28,650 MT/yr); 2) annual average load from TIP tributaries was 12% lower during the projection period (6,820 MT/yr) than during the hindcast period (7,760 MT/yr); and 3) total annual average load to the TIP was 1.6% lower during the projection period (35,810 MT/yr) than during the hindcast period (36,410

MT/yr). Thus, the TIP solids loading used in the projection simulation is very similar to that specified during the hindcast period.

The mechanistic sediment transport model used by QEA is dependent upon bottom shear stress and, hence, the hydrograph used to specify incoming flow. Thus, the increase in TIP deposition (or sedimentation rate) during the projection period is primarily due to differences in the hydrographs used for the hindcast and projection simulations. As discussed in Section 8.1, an objective approach was used to develop the synthetic hydrograph for the projection simulation. Therefore, the hydrograph used for the projection period and the associated impacts on sediment transport processes are unbiased.

USEPA is incorrect in stating that "QEA projections show a 14% increase in average sedimentation rate within the TIP relative to historic observations, increasing from 0.81 to 0.92 cm/yr (p. 3-4, Vol. 3, QEA 1999)." The QEA report stated that (p.3-4, Vol. 3, QEA 1999): "The 30-year projected average sedimentation rate in the cohesive areas of the TIP was higher than during the 22-year validation period, increasing from 0.81 cm/yr to 0.92 cm/yr. This 14% increase in average sedimentation rate during the 30-year projection period is a relatively minor change in deposition rate." Total deposition in the TIP increased by 10% during the projection period. It should also be noted that USEPA incorrectly states the projection results were compared to "historic observations" or "observed conditions." All sedimentation rate comparisons discussed in Section 3.2 (Vol. 3, QEA 1999) were between model results, i.e., hindcast and projection period simulations.

8.3 Remediation Methods

Comment: (p. 21, USEPA 2000b)

QEA has considered only a limited number of remediation options. Dredging is restricted to consideration of removal of TIP hot spots and/or removal of cohesive sediments (only) from Rogers Island to Northumberland Dam. Neither capping nor removal of non-cohesive sediments were considered. Failure to evaluate a full range of potential remediation

options means that global statements regarding the possible efficacy of remediation are at best premature.

Response:

The report does not make any "global" statements about the efficacy of remediation. It does state that dredging would be ineffectual because it would only achieve small reductions in surface sediment PCB concentrations. This statement is true as a general proposition even if non-cohesive sediments were targeted. It comes from the comparison of surface sediment PCB levels predicted under natural recovery and reasonably assumed after dredging.

8.4 Prediction of Future PCB Fate, Transport and Bioaccumulation

Comment: (p. 21, USEPA 2000b)

The QEA predictions of natural recovery are believed to be overly optimistic for two reasons. First it is believed that the model likely over predicts burial rates for the PCBs in the system. Second, the model does not represent PCBs stored below 5 cm depth in non-cohesive sediments. As some non-cohesive areas are predicted to be erosional, the model does not properly account for future releases to the system from deeper non-cohesive sediments.

Response:

The predictions of natural recovery are predicated on the predictions of recovery for the historical period from 1977 to 1998. The ability of the model to accurately predict the historical natural recovery is evidence that its projections of natural recovery are realistic. The USEPA arguments for overly optimistic projections are not supported by the existing data. The contention that burial rates are over predicted is based on an improper analysis of historical suspended solids data (see Sections 5.2, 5.3, and 8.2). The contention that a substantial PCB inventory exists in non-cohesive sediments more than 5 cm below the surface is made without reference to data. Our analysis of non-cohesive sediment PCB data (see Section 6.1) indicates

that the average depth of contamination could be as low as about 6 cm and is certainly less than 13 cm. Although the projections are subject to the uncertainty imparted by our inability to more accurately define the depth of contamination, there is no reason to presume that this uncertainty would force the projections to be overly optimistic. In fact, the logical presumption is that the depth of contamination is smallest in areas subject to net erosion.

Comment: (p. 22, USEPA 2000b)

QEA also states (vol 2, p. 4-70) that the impact of upstream PCB pulse loading "will not propagate into future predictions", and therefore does not need to be assessed in forecasts. This is misleading in light of observed pulse loads that have occurred in recent years (e.g., 1998 and 1999). In fact, as residual surface sediment PCB contamination declines, the significance of PCB loads across the upstream boundary of the model is likely to increase. The approach taken by QEA to represent the upstream boundary in the forecasts was to assign a constant load boundary condition of 0.2 lb/d... This causes the QEA model forecasts to underestimate the potential benefits associated with remediation of contaminated sediments as the fish body burdens will be controlled by high upstream concentrations. A more realistic approach would be to use a constant concentration boundary condition, resulting in a positive correlation between loads and flows consistent with the historical record. In addition, the forecast scenarios should be presented across a range of assumptions regarding upstream boundary concentrations (not daily average loads) to address the potential for continued pulse loading from the upstream source.

Response:

While we agree that the significance of the upstream boundary PCB load will increase in the future, the specification of a constant load or concentration does not make much difference in the results of projections. The constant load of 0.2 lb/d represents an average concentration of about 7 ng/l. Projections run at a constant upstream concentration of 10 ng/l did not substantially alter the projected potential benefit of sediment remediation.

The continuation of PCB pulse loadings in the future is a possible scenario, as occasional high loads have been observed at Fort Edward in the recent past. Inclusion of pulse loadings would have the effect of increasing the relative impact of upstream source control while decreasing the impact of sediment-based remediation. USEPA projection scenarios did not include pulse loadings, only considering 0, 10, and 30 ng/l upstream concentrations.

Comment: (p. 22, USEPA 2000b)

The QEA approach to fish lipid also has an impact on prediction of future conditions. Here, QEA has chosen to retain fixed differences between stations (Vol. 3, p. 2-1): "The lipid content of each species of fish at each location was assumed to remain constant and was set equal to the values used in the last year of calibration." This decision is difficult to justify. For instance, the last-year-of-observation values used by QEA (see Vol. 2, Figures 5-18 and 5-19) have lipid concentrations that are higher than most of the historic record for largemouth bass and bullhead in TIP and pumpkinseed at Stillwater. Lipid concentrations assigned are about equal between TIP and Stillwater for pumpkinseed and bullhead, even though bullhead have generally had lower lipid in TIP, while a higher lipid content is assigned to largemouth bass in TIP than in Stillwater. Use of median values from historic observations (or better, use of a statistical description of the distribution of lipid content) would provide a better basis for future projections.

Response:

Estimates of future lipid contents are subject to uncertainty, especially given the variability in historical values. The most recent year's lipid value was used in the projections presented in the report, based on the following logic.

Lipid contents in fish vary as a result of changes in ecological conditions in the river. For example, limited prey availability may result in lower lipid levels in the predators. One of the most important ecological changes in the upper river was the cessation of fishing in the late 1970s. This is likely to have resulted in changes in the community, possibly including the availability of prey. Lipid values measured two decades after this change could be assumed to

represent conditions that have developed after the initial adjustment of the community to the reduction in fishing pressure. Thus, following this logic, the most recent lipid data can be assumed to best represent ecological conditions in the absence of fishing, and therefore would be most representative of future ecological conditions, given a continued ban on fishing.

However, we do not fully understand the processes determining lipid content, and the historical average is not an unreasonable assumption. In any event, the difference between these two alternative assumptions is relatively minor, ranging from 5 to 15% for the three fish species in the two locations (based upon the historical data). Note that differences in computed PCB concentrations are even smaller than these differences in lipid content. This is because changes in lipid content affect primarily the PCB elimination rate, and growth dilution is often as important or more important than elimination in causing PCB concentrations to decline in the fish.

It is unclear whether USEPA suggests that one average lipid content for the entire river better describes future lipid content than reach-specific values. As USEPA points out, lipid contents do differ between reaches. Therefore, use of reach-specific lipid values makes the model more realistic, and we will continue use reach-specific values in the future.

REFERENCES

Alcoa. 1999. *Analysis of Alternatives Report, Grasse River Study Area, Massena, NY, Appendix D – A Model of PCB Fate in the Lower Grasse River*, December 1999.

Burgess, R.M., McKinney, R.A., and Brown, W.A. 1996. Enrichment of marine sediment colloids with polychlorinated biphenyls: trends resulting from PCB solubility and chlorination. *Environmental Science & Technology* 30:2556-2566.

Burkhard, L.P., 2000. Estimating dissolved organic carbon partition coefficients for nonionic organic chemicals. *Environmental Science & Technology* 34:4663-4668.

Butcher, J.B. and E.A. Garvey. 1999. Congener Pattern Matching to Evaluate Sediment PCB Source in the Upper Hudson River. Poster presentation at SETAC 20th Annual Meeting, Philadelphia, PA, November 18, 1999.

Evans, H.E. 1988. The binding of three PCB congeners to dissolved organic carbon in freshwaters. *Chemosphere*, 17:2325-2338.

Garcia, M. and Parker, G., 1991. "Entrainment of Bed Sediment into Suspension," ASCE J. Hydr. Engr., 117(4):414-435.

General Electric Company. 1999. *Comments of the General Electric Company on Phase 2 Report - Review Copy, Further Site Characterization and Analysis, Volume 2D – Baseline Modeling Report,* Upper Hudson River PCBs Reassessment RI/FS, May 1999.

Grant. 1983. *Journal of Marine Research* 41(4): 673-690.

Holly, F.M. and Karim, M.F., 1986. "Simulation of Missouri River Bed Degradation," ASCE J. Hydr. Engr., 112(6):497-517.

Lara, R., and Ernst, W. 1989. Interaction between polychlorinated biphenyls and marine humic substances, determination of association coefficients. *Chemosphere*, 19:1655-1664.

Lee, H.Y., Hsieh, H.M., Yang, J.C. and Yang, C.T., 1997. "Quasi-Two-Dimensional Simulation of Scour and Deposition in Alluvial Channels," ASCE J. Hydr. Engr., 123(7):600-609.

Rhea, J., Connolly, J., Quadrini, J., Russell, K, and Smith, J. 1998. "Seasonal Patterns in PCB Sediment-Water Exchange in the Grasse River, NY," Proceedings of the 19th Annual Meeting of the Society of Environmental Toxicology and Chemistry, Charlotte, NC, November 1998.

Quantitative Environmental Analysis, LLC, 2000. "Comments of the General Electric Company on: Phase 2 Report – Review Copy; Further Site Characterization and Analysis; Volume 2D – Revised Baseline Modeling Report; Hudson River PCBs Reassessment RI/FS; January 2000," November 2000.

QEA. 1999. PCBs in the Upper Hudson River. May 1999.

U.S. Environmental Protection Agency. 2000a. *Hudson River PCBs Reassessment RI/FS Responsiveness Summary for Volume 2D – Revised Baseline Modeling Report,* January 2000.

U.S. Environmental Protection Agency. 2000b. USEPA Commentary on PCBs in the Upper Hudson River. 2000.

U.S. Environmental Protection Agency, 2000c. Modeling Framework Design, Modeling Study of PCB Contamination in the Housatonic River, DCN: GE-100500-AADX, Contract No. DACW33-00-D-0006.

U.S. Environmental Protection Agency. 1999. *Hudson River PCBs Reassessment RI/FS Responsiveness Summary for Volume 2D – Baseline Modeling Report,* May 1999.

U.S. Environmental Protection Agency. 1998. *Hudson River PCBs Reassessment RI/FS Responsiveness Summary for Volume 2C - Data Evaluation and Interpretation Report,* December 1998.

U.S. Environmental Protection Agency. 1997. *Phase 2 Report - Review Copy, Further Site Characterization and Analysis, Volume 2C - Data Evaluation and Interpretation Report,* February 1997.

U.S. Environmental Protection Agency. 1991. 1991 Data Evaluation and Investigation Report.

Van Niekerk, A., Vogel, K.R., Slingerland, R.L. and Bridge, J.S., 1992. "Routing of Heterogeneous Sediments over Movable Bed: Model Development," ASCE J. Hydr. Engr., 118(2):246-279.

Van Rijn, L.C., 1984. "Sediment Transport, Part II: Suspended Load Transport," ASCE J. Hydr. Engr., 110(11):1612-1638.

Wheatcoaft, et. al. 1994. *Journal of Marine Research* 52(6): 1129-1150.

Wong, et. al. *Environmental Science and Technology* 29(10): 2661-2672.

APPENDIX 1

Split Sample Analysis of 1997 and 1998 NYSDEC Fish Samples

Split Sample Analysis of 1997 and 1998 NYSDEC Fish Samples

Quantitative Environmental Analysis, L.L.C.
305 West Grand Avenue
Montvale, NJ 07645

November 1999

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

1.0 INTRODUCTION

This document summarizes the investigation conducted by Northeast Analytical Laboratory (NEA) and Quantitative Environmental Analysis (QEA) for General Electric Company (GE) in order to evaluate fish PCB concentrations measured by the laboratory used by the New York State Department of Environmental Conservation (NYSDEC) for their Hudson River fish monitoring program. The samples examined in this study represent a subset of the fish collected in 1997 and 1998 as part of the annual fish sampling survey conducted in the Upper Hudson River by NYSDEC. The split sample analysis conducted for fish sampled in 1997 included a variety of species from the Upper and Lower River. The investigation for fish sampled in 1998 focused on three species of fish; Brown Bullhead, Largemouth Bass, and Pumpkinseed, collected at two locations in the Upper Hudson River; Stillwater and the Thompson Island Pool.

1.1 Background

NYSDEC maintains a database of total PCB and select Aroclor concentrations for many species of fish sampled at standard sites in the Hudson River each year. These data show that PCB concentrations in fish have been steadily declining in the Upper River since substantial reductions in external PCB sources occurred in 1993 (QEA, 1999, Volume 2). Averages of the 1998 NYSDEC data exceeded 1997 averages, although the differences between years were small enough to lack statistical significance. An increase is inconsistent with other recent data available for the Upper Hudson River including weekly water concentrations measured at the Thompson Island Dam and Schuylerville and 1998 surface sediment PCB concentrations in the Thompson Island Pool.

The long-term trends in the average surface sediment (0-5 cm) PCB_{3+} (summation of tri-chlorinated and higher PCB congeners) concentration in the Thompson Island Pool (TIP) indicated by data and by the model developed by QEA (QEA, 1999) are shown in Figure 1-1. The most recent data, generated during the 1998 GE sediment coring program, show concentrations continuing to decline in surface cohesive and non-cohesive sediments.

Data from the weekly water column monitoring program indicate that PCB levels in the river are consistent between 1997 and 1998 (Figure 1-2). Further, the data show that the short-term elevation in concentrations that occurred in the January 1998 flood had no long-term impact on PCB levels. Finally, the water levels observed over this entire period are accurately replicated by the QEA model.

1.2 Issues

The inconsistency between the reported 1998 fish PCB levels and both the trends in fish PCB levels from 1993 to 1997 and the trends in exposure concentrations prompted GE to examine the quality of the 1998 fish data. The examination involved an analysis of split samples of 1997 and 1998 fish for which PCB concentrations were measured by EnChem of Madison, Wisconsin, the contract laboratory for NYSDEC, and NEA of Schenectady, New York, under contract to GE. EnChem employed packed column gas chromatography (GC) with Aroclor quantification to analyze the 1997 samples, except when they also analyzed for pesticides, then capillary column GC was used with Aroclor quantification (Ron Sloan, NYSDEC, Pers. Comm.). They employed packed column GC with Aroclor quantification to analyze the 1998 samples in all cases. NEA used capillary column GC with congener quantification. Extraction and cleanup methods also differ between the two laboratories (refer to Appendix A for details). Potential effects of differing analytical methods were examined in this study.

Congener quantification refers to total PCB results derived from the summation of 118 chromatographic peaks each containing one or more congeners. In this report, packed column GC analysis will always indicate Aroclor quantification and capillary column GC will indicate congener quantification except where noted. A total PCB concentration can be calculated from either quantification method, but how comparable they are is not clear.

2.0 APPROACH/RESULTS

2.1 1997 Split Samples

In 1998 GE contracted NEA to analyze 83 of the fish samples collected from the Upper and Lower Hudson River in 1997 by NYSDEC for comparison with the PCB results obtained by NYSDEC (EnChem Laboratory) (Table 2-1). NEA received fish tissue (fillets) from EnChem which NEA processed and analyzed for total PCBs. Only 7 of the 83 samples chosen for split analysis were originally run by EnChem using packed column GC, the remainder were analyzed using capillary column GC. Therefore, this was essentially a comparison between fish sample processing including extraction/cleanup techniques and capillary column GC analyses conducted by the two laboratories. EnChem used Aroclor quantification whereas NEA used congener quantification. The total PCB values determined for the 1997 split samples by EnChem and NEA are very comparable, with EnChem's values tending to be slightly higher (Figure 2-1). The percent lipid values determined by EnChem also were higher than those determined by NEA (Figure 2-2). Lipid-normalized total PCB values are very similar between labs (Figure 2-3) suggesting that the differences in wet weight-based concentrations are the result of inhomogeneity of PCB distribution within fish tissues or differences in lipid extraction. The seven samples (indicated on Figures 2-1 and 2-3 with red diamonds) for which EnChem utilized packed column GC compared very well to NEA's capillary column GC analysis.

2.2 NEA Reanalysis of 1997 Samples

NEA recently reanalyzed 5 of the archived extracts from the 1997 split sample set by both capillary column GC (the method used in 1998) and packed column GC to assure reproducibility of the analytical protocols before beginning analysis of 1998 samples (Table 2-2). The reproducibility between the two years for capillary column GC analysis was excellent with the relative percent difference (RPD) ranging from 0.55 to 1.58. The RPDs for the 1998 capillary column GC analysis versus the 1999 packed column analysis ranged from 2.40 to 13.36, still very good considering that the required RPD between duplicate samples is <35. These analyses confirmed that NEA could reproduce their 1998 results for the 1997 fish samples.

2.3 1998 Split Samples

In 1999 GE selected 82 of the Upper Hudson River fish samples collected by NYSDEC in 1998 for split analysis with EnChem. Homogenized tissue was obtained from EnChem and PCBs and lipid were extracted from the tissue homogenate using NEA's protocols. EnChem's packed column GC results are clearly biased high compared to NEA's capillary column results on a wet-weight basis (Table 2-3, Figure 2-4a). The comparison did improve somewhat when totals were compared on a lipid-normalized basis (Figure 2-4b). The solid diamonds on Figure 2-4 indicate the 6 samples selected for further examination based on these results (Section 2.4). In general, the percent lipid values were quite comparable, perhaps biased slightly high for EnChem at higher lipid concentrations if two outliers are removed at the high end (Figure 2-5).

The data were examined further based on individual species and sampling locations.

Largemouth Bass, Brown Bullhead and Pumpkinseed

Figure 2-6 shows the same comparison as Figure 2-4 (Total PCB results for the two laboratories on a wet weight and lipid normalized basis), however different symbols indicate the three species studied, Largemouth Bass, Brown Bullhead and Pumpkinseed (only 6 samples) at Stillwater and Thompson Island Pool. The results show that the bias is not species dependent, the same conclusion is drawn for each species, the data for EnChem are biased high with only a few exceptions.

Stillwater versus Thompson Island Pool

The same comparison for all 3 species on a wet weight and lipid-normalized basis is shown for the two sampling locations, Stillwater and Thompson Island Pool, on Figure 2-7. There appears to be no difference in the total PCB pattern between EnChem and NEA for the two sites. Lipid normalizing the data does not change this observation (Figure 2-7b).

These results indicate that the high bias seen for EnChem data is not attributable to either species or location differences. The fact that EnChem doesn't perform a vigorous sulfuric acid cleanup of sample extracts, combined with the fact that they use packed column GC analysis could result in artificially high total PCB values, especially at lower concentrations (Section 3, Appendix A). These results caused concern about the accuracy of the 1998 EnChem results. This concern prompted further testing of a subset of the 82 samples discussed above.

2.4 Comparative Evaluation of the NEA and EnChem Analyses of 1998 Fish Samples

The EnChem and NEA measurements of PCB concentrations involve differences in extraction methodology and gas chromatography techniques that could be responsible for the differences in results (see Appendix A). In an effort to isolate the cause of the differences, a six-sample subset of the 82 samples discussed above was subjected to additional investigation. The samples chosen encompass two species, Brown Bullhead and Largemouth Bass, at both sampling locations and vary approximately ten-fold in PCB concentration. Inter-laboratory comparison of three of the six samples in the initial investigation was very good on a total PCB basis. The total PCB concentration determined for the other three samples was up to 3 times higher for EnChem. To evaluate the effect of chromatography method, EnChem's extracts for the six sample subset were obtained by NEA and PCB concentrations were measured using a replication of EnChem's packed column gas chromatography method as well as NEA's capillary column GC method. To evaluate the effect of the extraction procedure, the EnChem and NEA extracts were analyzed using both the packed column GC and capillary column GC methods. Table 2-4 summarizes the results of the analyses run on these six samples.

2.4.1 Evaluation of Chromatography Methods

NEA packed column GC analysis versus EnChem's packed column GC analysis

To conduct this investigation, NEA analyzed EnChem extracts to insure the accuracy of the packed column GC replication. NEA replicated the packed column GC analysis method used by EnChem (3% SP-2100 packed column, decachlorobiphenyl eluting at 35 minutes) and quantified Aroclor totals to determine whether it was possible to reproduce their results. Figure

2-8 shows the relationship between total PCB results (wet weight) as well as Aroclor 1248, 1254 and 1260. The total PCB replication was excellent. The RPD for the samples ranged from 1.2-12. One notable difference was the higher Aroclor 1260 values reported by EnChem. However, this difference is unimportant because Aroclor 1260 constitutes less than 7 percent of total PCBs (Table 2-4, Figure 2-8).

NEA capillary versus packed column GC analysis

NEA also analyzed the six EnChem extracts by the NEA capillary column GC method (NEA608CAP). These results are compared to NEA's results from the replicated-packed column GC analysis of the same sample extracts in Figure 2-9. In general, packed column GC analysis produced slightly higher PCB totals compared to capillary column GC totals, with differences increasing with increasing concentration. The RPDs were not high, ranging from 2 to 7 percent.

It appears that packed column GC does give slightly higher results for total PCBs compared to capillary column GC. However, this difference is not large enough to account for differences between EnChem and NEA results for the 82 split samples. More importantly, the bias is larger for higher concentration samples, contrary to the pattern seen when EnChem's packed column method was compared to NEA's capillary column method (larger discrepancy at lower PCB concentrations) (Figure 2-4).

2.4.2 Evaluation of Extraction Procedures

To compare extraction methods between the two laboratories NEA obtained fish homogenate from EnChem and extracted the six samples using its own method which entails a soxhlet extraction with a 1:1 acetone/hexane mixture which is ultimately exchanged into hexane. The hexane extracts undergo H_2SO_4, Florisil and sulfur removal cleanups. Both EnChem and NEA's extracts were analyzed by NEA using their usual capillary column GC method as well as the replicated EnChem packed column method.

Figure 2-10a shows EnChem's consistently high bias for total PCBs when EnChem's complete method (extraction and analysis) is compared to NEA's complete method (extraction and analysis). The results of NEA's packed column GC and capillary column GC analysis of EnChem's extracts compared to NEA's extraction and analysis of the samples is shown in Figure 2-10b. NEA's packed column and capillary column GC results show no improvement over EnChem's packed column GC analysis results. This indicates that differences in extraction/cleanup procedures are causing the bias between the two laboratories. In fact, when NEA extracted the samples and compared results obtained using the replicated EnChem packed column method and its capillary column method, the results were very similar (Figure 2-10c).

3.0 DISCUSSION

3.1 Data quality

Given that the 1998 split sample data (1997 fish samples) compared well between EnChem and NEA, it is believed that the discrepancy in the 1999 split sample data (1998 fish samples) is not caused by a systematic difference and that the problems are unique to 1998.

Data Summary Packages

David Blye of Environmental Standards, Inc. (ESI) examined EnChem data packages for six 1997 samples and six 1998 samples to evaluate the data quality and potential differences between the two years. ESI quality assurance chemists also reviewed the raw analysis data provided to determine if interferences or qualitative or quantitative errors could be plausible causes of the poor data comparability. His comments are summarized below.

The data packages provided by EnChem are generally not complete enough to allow a comprehensive review and validation of qualitative and quantitative accuracy of the results. The quantitation reports include only integrated areas for the peaks chosen by the laboratory for the quantitation of specific Aroclors. This makes any independent manual quantitation of the data using separate peaks impossible without having EnChem reprocess all the data files. As such, and given the importance of the project data, it would be expected that the fish tissue analyses would include a request by the State's project team for a complete New York State Department of Environmental Conservation (NYSDEC) Analytical Services Protocol (ASP) Category B data

package deliverable. This type of data reporting is typically expected by NYSDEC personnel of regulated entities on projects of this type based on Environment Standard's experience.

Standard Operating Procedures

Environmental Standards also obtained the analytical Standard Operating Procedures (SOPs) for the PCB fish tissue analysis from both EnChem and NEA and reviewed them to determine if significant differences in the procedures employed by either laboratory exist that could account for the poor comparability in the 1998 data. ESI's comments are summarized below and details can be found in Appendix A.

EnChem's 1998 fish tissue analysis for the split samples in question is based upon a PACKED COLUMN gas chromatography/Electron Capture Detector (GC/ECD) method and NEA's analysis is based upon a High Resolution Gas Chromatography (HRGC) CAPILLARY COLUMN GC/ECD method. This is significant in that the PACKED COLUMN chromatography method provides significantly less resolution in separating the individual congeners and, therefore, can also be subject to bias from interferences that "coelute" with the PCBs. Generally, a PACKED COLUMN PCB analysis identifies an Aroclor pattern. In contrast, the CAPILLARY COLUMN HRGC GC/ECD method employed by NEA has the ability to resolve individual PCB congeners and is significantly less prone to chromatographic interferences than a PACKED COLUMN method. The author's basic premise prior to reviewing the raw analysis data was that the high bias observed in the 1998 EnChem data compared to the NEA data was consistent in what would be expected given the two chromatography methods employed.

4.0 CONCLUSIONS

Differences in extraction/cleanup procedures and the GC methods used contributed to the disparity between PCB results determined by EnChem and NEA for 1998 fish samples. The belief that EnChem's data are biased high compared to NEA's data is the result of careful examination of their extraction/cleanup and analysis procedures which led to questions about the data quality including potential interferences (Section 3, Appendix A). The fact that the 1997 split sample results compared well is a result of both laboratories using capillary column GC analysis. Also, there were differences in the GC procedures used by EnChem to analyze the 1997 and 1998 fish samples which are difficult to quantify but could definitely affect the quality of the data (Appendix A).

- Total PCB results for 1997 split samples compared very well between the two laboratories. The seven samples which were analyzed using packed column GC analysis (EnChem Laboratory) are in close agreement to total PCB results using capillary column GC analysis (NEA Laboratory). On a wet weight basis, there are some differences due to either fish sample processing, or lipid extraction efficiency. Lipid-normalizing the data improves the comparison.

- EnChem's packed column GC total PCB results are higher than NEA's capillary column results for the 1998 split samples on a lipid basis. Differences are greater at lower PCB concentrations. Difference in total PCB results are not related to fish species or sampling location.

- The differences between the two laboratories cannot be due to column chromatography differences because the differences between packed column GC and capillary column GC analysis are small and in the wrong direction (differences are greater at higher concentrations).

- The lipid analysis of the two laboratories compared well and, thus, were not contributory to the PCB differences.

- Packed column GC PCB results obtained by NEA for samples extracted by EnChem closely matched the results reported by EnChem, indicating that the EnChem GC analysis and quantitation are accurate.

- Packed column GC PCB results obtained by NEA for samples reextracted by NEA closely matched the NEA capillary column results, indicating that the differences in PCB results between the labs are largely due to differences in extraction and cleanup procedures.

- The fact that EnChem doesn't perform a vigorous sulfuric acid clean up of sample extracts, combined with the fact that they use packed column GC analysis may both contribute to their higher total PCB values.

5.0 RECOMMENDATIONS

- The extraction and cleanup procedures used by EnChem should be reviewed and modified as necessary.

- All future PCB determinations for the fish should be made using capillary column GC to increase the accuracy of the measurements by increasing the resolution and decreasing the effect of interferences.

- An inter-laboratory comparison between EnChem and another lab should be conducted each year using blind tissue homogenate to insure that the data represents the truest value possible.

- Complete data summary packages should be required by the DEC to allow for quality assurance review and/or independent assessment of the data. The laboratory should be required to present integrated areas for all peaks in the chromatograms so that independent evaluation of the data can potentially occur.

REFERENCES

Quantitative Environmental Analysis, LLC, 1999. "PCBs in the Upper Hudson River." Prepared for General Electric Company.

APPENDIX A

ESI's Review of EnChem and NEA's SOPs and Data Packages

Comparison of the EnChem and NEA Data from the Hudson River Fish Sample Analyses

ENCHEM DATA

1. The preparation of the fish tissue was based on a soxhlet extraction using methylene chloride as the initial solvent which is ultimately exchanged into hexane. The hexane extracts were indicated by Tod Noltemeyer of EnChem to undergo only a florisil cleanup. Although the EnChem SOP indicates Gel Permeation Chromatography (GPC) cleanup as a cleanup option, this was not performed. Also, the "Principle" section of the SOP (Page 1 of 11) indicates that elemental mercury cleanup can be performed as necessary to remove sulfur. The sulfur cleanup is not addressed in the Cleanup Procedures section of the SOP. Lastly, no sulfuric acid (H_2SO_4) cleanup was performed either.

2. The initial calibrations are acceptable. Five-point calibrations are performed for Aroclors 1242, 1248, 1254, and 1260. A single-point calibration is performed for the other Aroclors as part of the initial calibration sequence. The mid-point concentration standard is used for quantitation.

3. The continuing calibration checks are within limits.

4. Several method blanks associated with 1998 sample analyses are contaminated with trace levels of PCBs. However, since most of the samples were analyzed at significant dilutions, the method blank contamination levels are not significant with respect to the fish sample concentrations.

5. The surrogate, LCS, MS/MSD recoveries are generally within quality control (QC) acceptance limits. The QC limits are wider than NEA's limits. However, it should be noted that in many samples, the surrogate compound (Decachlorobiphenyl) was diluted out of the samples on the extract dilution analysis. In these cases, the surrogate recoveries were reported from an initial undiluted analysis and were not calculated if the dilution factor was greater than 10× (although in most samples the DBC surrogate peak is discernable up to approx. 50×) from the dilution analysis.

6. The surrogate calibration consists of a single concentration level at 0.2 ug/ml. The samples are spiked at a concentration of 0.2 ug/ml. The sample DCB results become less quantitative as soon as the concentration in the extract differs much from the standard. This occurs either by dilution or "poor" extraction. Since the surrogate is one of the only performance monitors on a sample by sample basis, the initial calibration should be a multi-point calibration to establish both linearity and dynamic range.

7. The PACKED COLUMN analysis chromatography is generally quite compressed with short total analysis run times in the 1998 data (23-28 min.). In contrast, the 1997 PACKED COLUMN analysis that Environmental Standards is in possession of indicates total analysis run time of short (23-25 min) and long runs (40 min). In the 1998 sample data, there are a lot of overlapping peaks among the different Aroclors. Very poor resolution is observed

between Aroclors 1248 and 1254. In fact, the entire retention time region that is used to quantitate Aroclor 1248 is 0.2-0.3 minutes in duration. Because Aroclor 1254 elutes/coelutes with the higher chlorinated PCBs that are in common with 1248, only a maximum of 3 distinct peaks appear able to be used to quantitate Aroclor 1248. The possibility of interference from non-target analytes exists in the PACKED COLUMN analysis and would be extremely difficult to identify given the chromatography conditions.

8. The quantitation reports include only integrated areas for the peaks chosen by the laboratory for the quantitation of specific Aroclors. This makes any independent manual quantitation of the data using separate peaks impossible without having EnChem reprocess all the data files. This is extremely time intensive. The laboratory should be required to present integrated areas for all peaks in the chromatograms so that independent evaluation of the data can potentially occur.

9. The 1997 data shows higher concentrations of Aroclor 1260 and lower concentration of 1248 compared to the 1998 data. The concentrations of Aroclor 1254 are high in both sets. This may be related to the differences in the chromatography conditions between the two sets, interferences resulting from the chromatography conditions and changes in the peaks used to quantitative the Aroclors

NEA DATA

1. The preparation of the fish tissue was based on a soxhlet extraction with 1:1 acetone/hexane as the initial solvent which is ultimately exchanged into hexane. It is important to note that the sample prepared by NEA was obtained from a homogenized fish sample aliquot prepared by EnChem such that the potentially for inhomogeneity of the sample was minimized. In the 1997 split sample data sets, two independent preparations occurred by both laboratories. Therefore, it is expected that less variability in the results would occur in the 1998 split sample analysis results. The hexane extracts underwent H_2SO_4, Florisil and sulfur removal cleanups (Bob Wagner, NEA Laboratory, Pers. comm.).

2. The initial calibrations are acceptable. The calibration standards are a 1232/1248/1262 Aroclor mixture at a ratio of 25:18:18. An initial linearity check is performed based on three standard concentration levels Routine instrument calibration is based on a single point calibration standard. Quantitation is based on this single point, and therefore, dilution of the sample extract occurs to place the sample extract concentration to a factor of five of the standard concentration. This method is also an internal standard calibration technique with octachloronaphthalene (OCN) as the internal standard compound.

1. The continuing calibration checks are within limits.

2. The method blanks are free of contamination.

3. The peak areas of the samples are similar to the peak areas of the standards for undiluted analyses and dilutions.

4. All surrogate, LCS, MS/MSD recoveries are within limits, except one MS recovery which is 50% (QC limits 70-130%). Nonylchlorobiphenyl was used as the surrogate compound. Additionally, the internal standard (OCN) area counts were monitored and remained consistent compared to the calibration standards. These various QC measures indicate good overall performance and analysis control.

5. This method is a more specific analysis based on quantitation of individual or a maximum of four co-eluting congeners. The HRGC chromatography is such that the identity and retention time of each congener is the calibration standard is accurately known. This method provides very good resolution and less interference from non-target analytes.

FIGURES

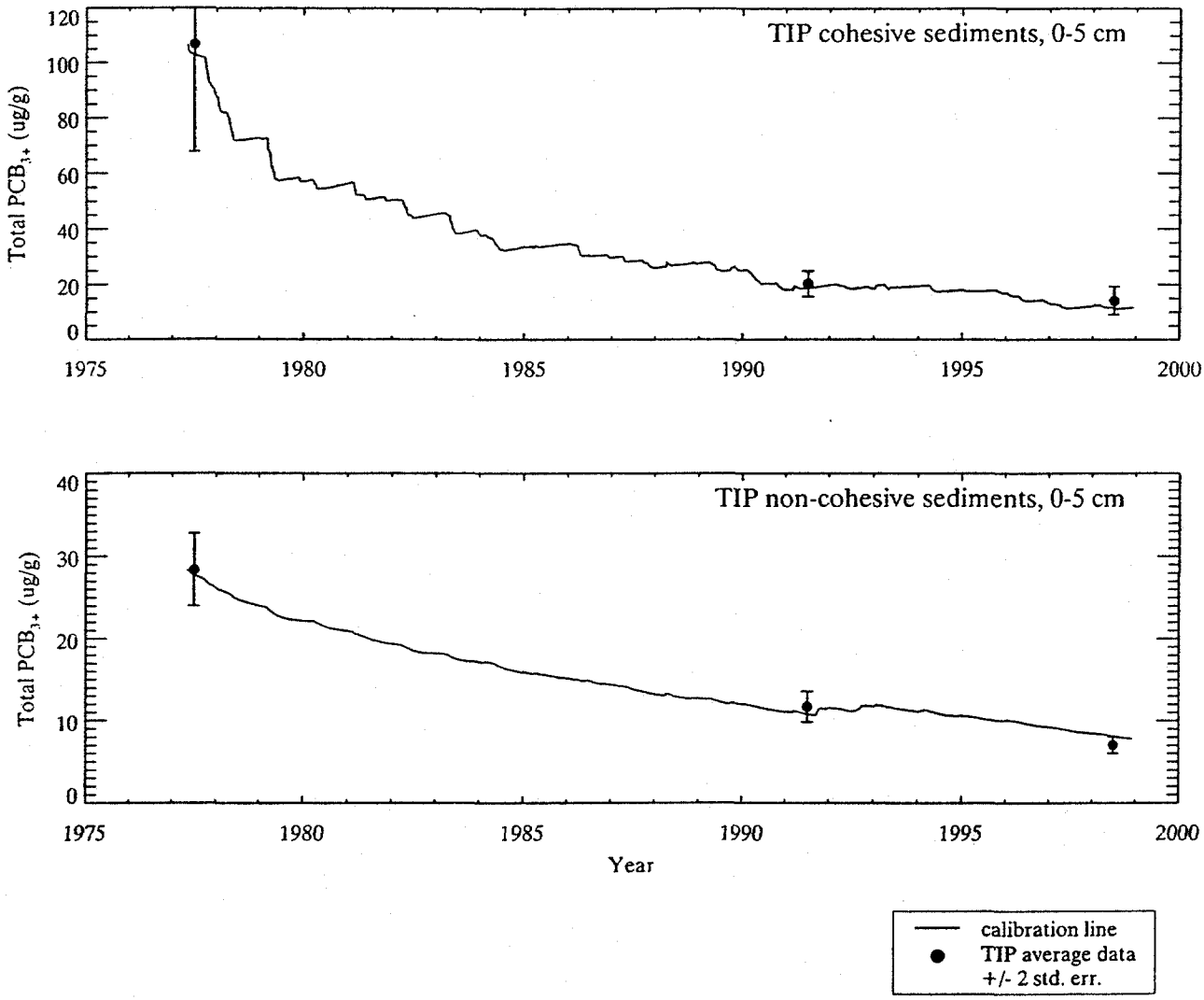


Figure 1-1. Predicted (line) and measured (symbols) average PCB_{3+} concentrations in surface sediments of the TIP using bounding calibration.

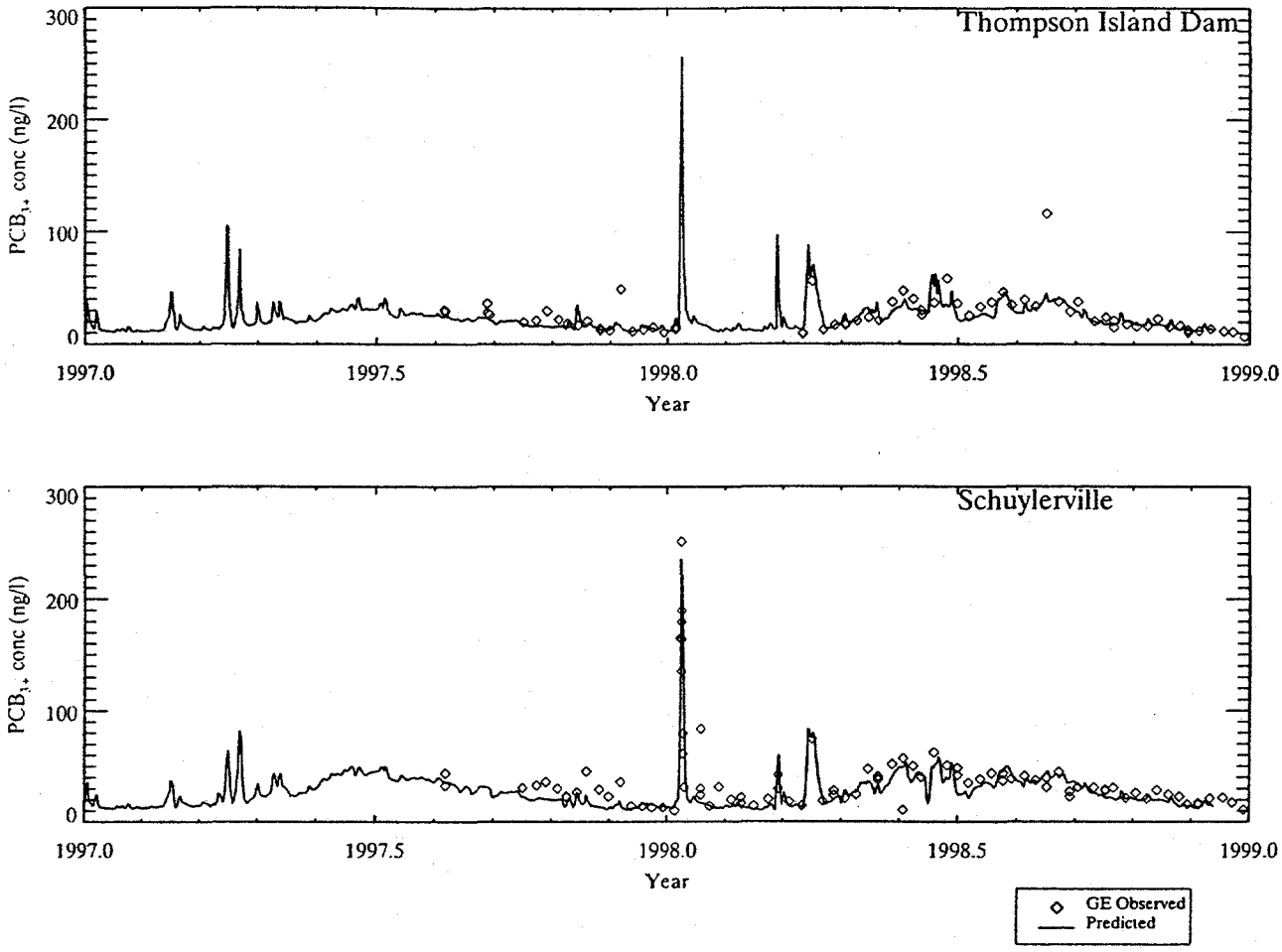


Figure 1-2. Comparison of predicted (line) using bounding calibration and measured (symbols) water column PCB_{3+} concentrations at Schuylerville and the Thompson Island Dam.

a \\Poseidon\D_Drive\GENhud\Documents\Final_report\Vol_2\Section_4\fig4_51schtid_9798.pro
Jul 13 14:51:08 1999

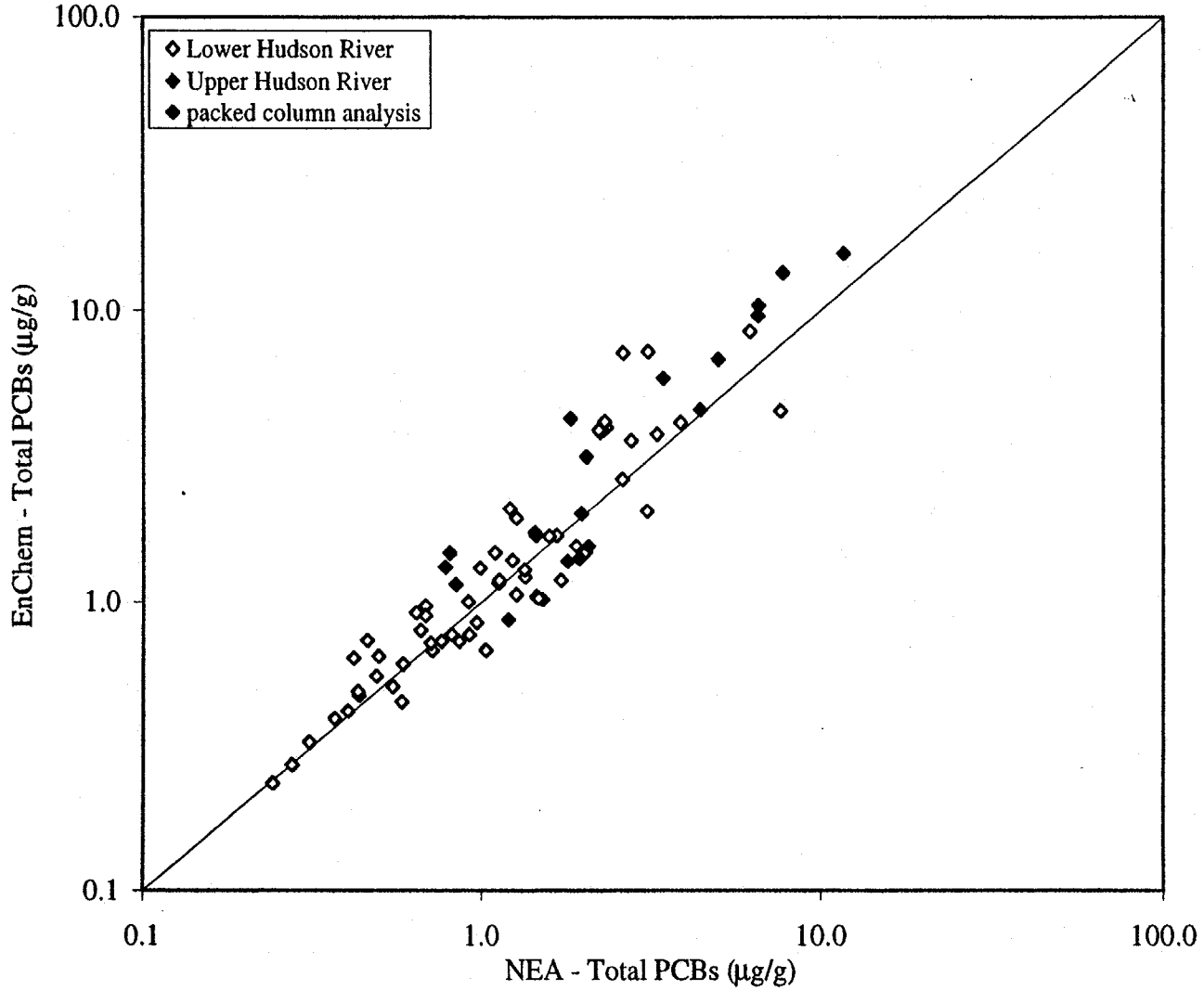


Figure 2-1. 1997 split sample comparison, total PCB analysis.

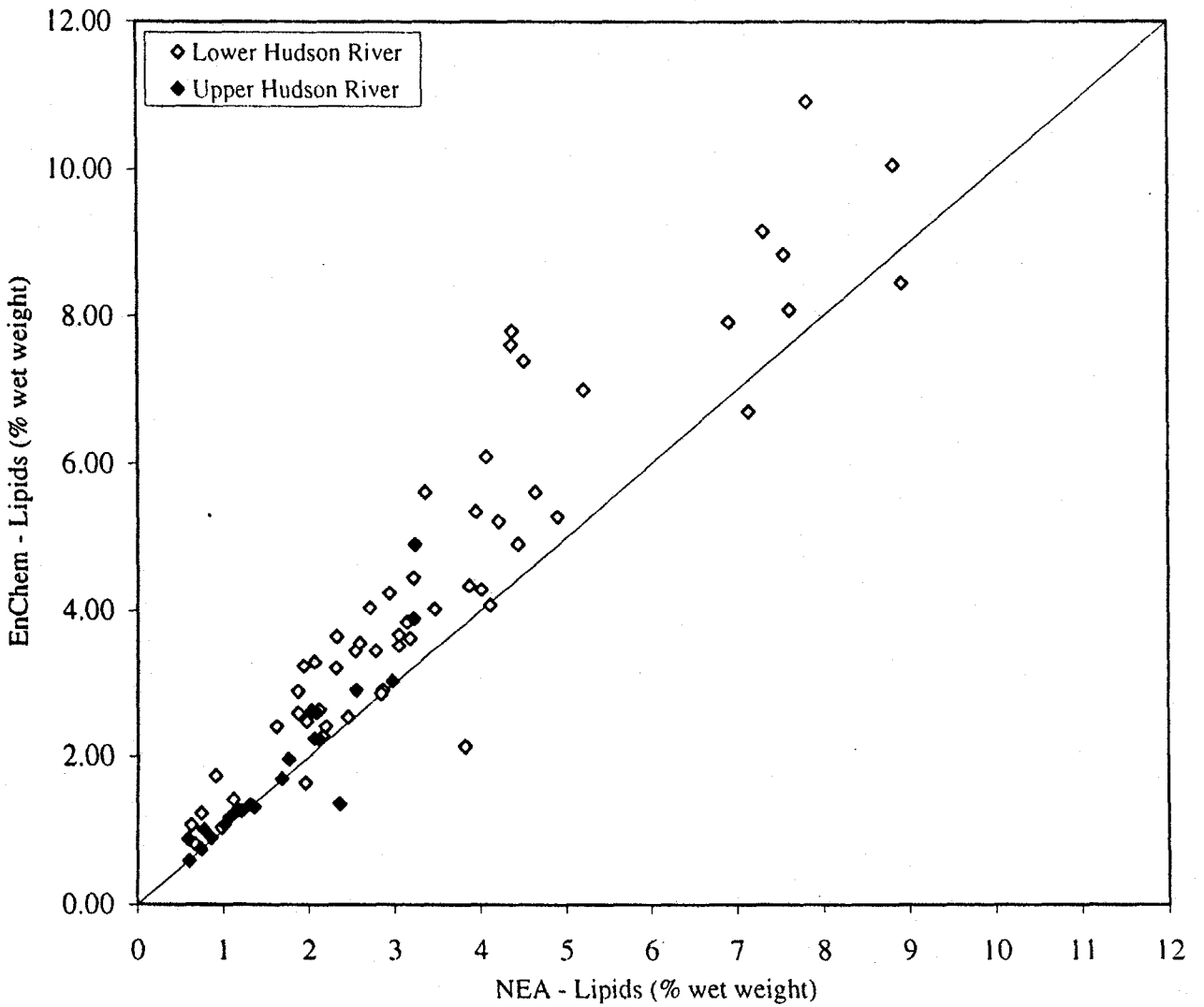


Figure 2-2. 1997 split sample comparison, Lipid Analysis.

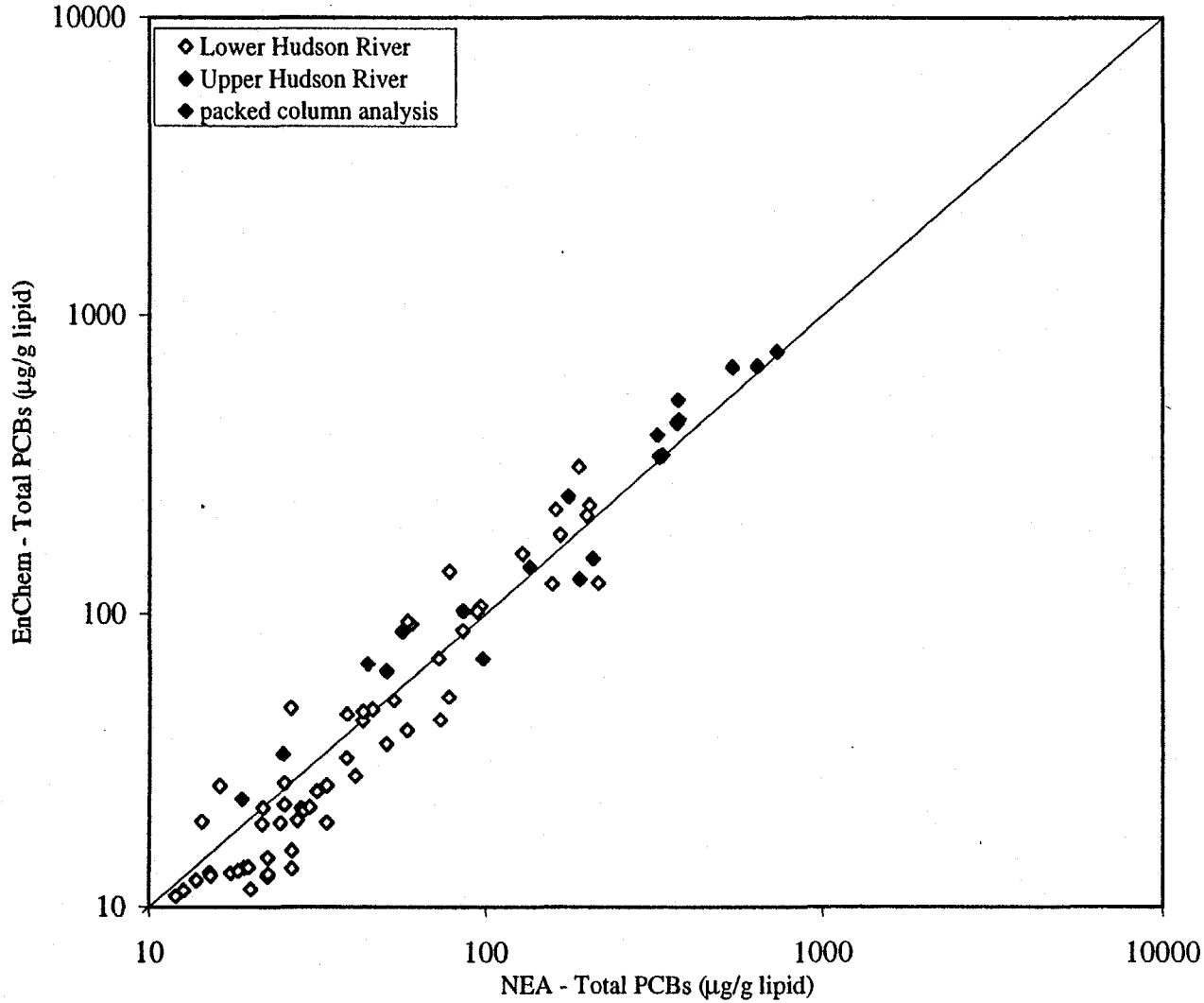


Figure 2-3. 1997 split sample comparison, lipid-normalized total PCB analysis.

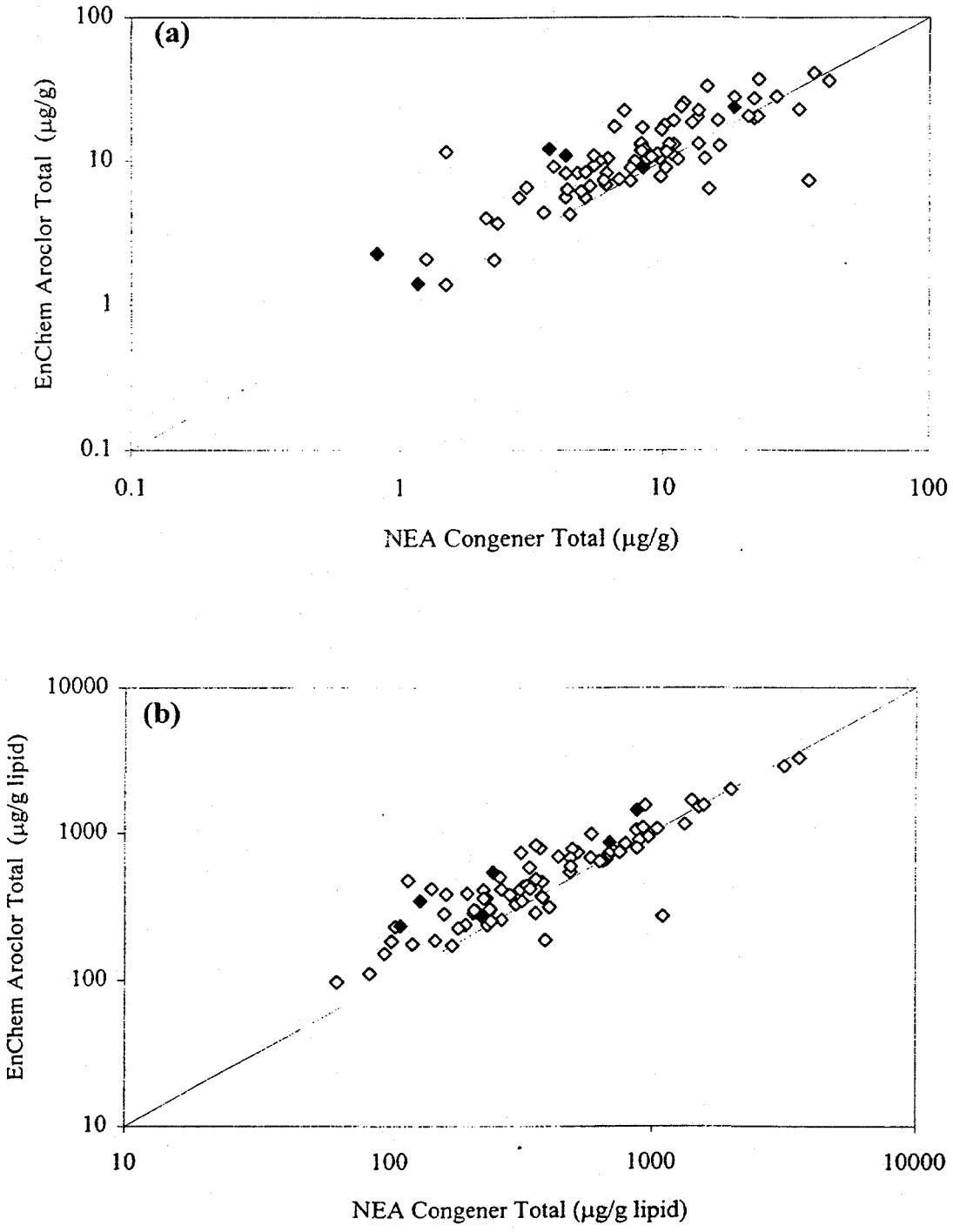


Figure 2-4. 1998 Split sample comparison between EnChem's complete method and NEA's complete method. Solid diamonds represent six samples chosen for further study (Section 2.4).

D:\Genhud\Data\Upphud\Ken_Fish.xls

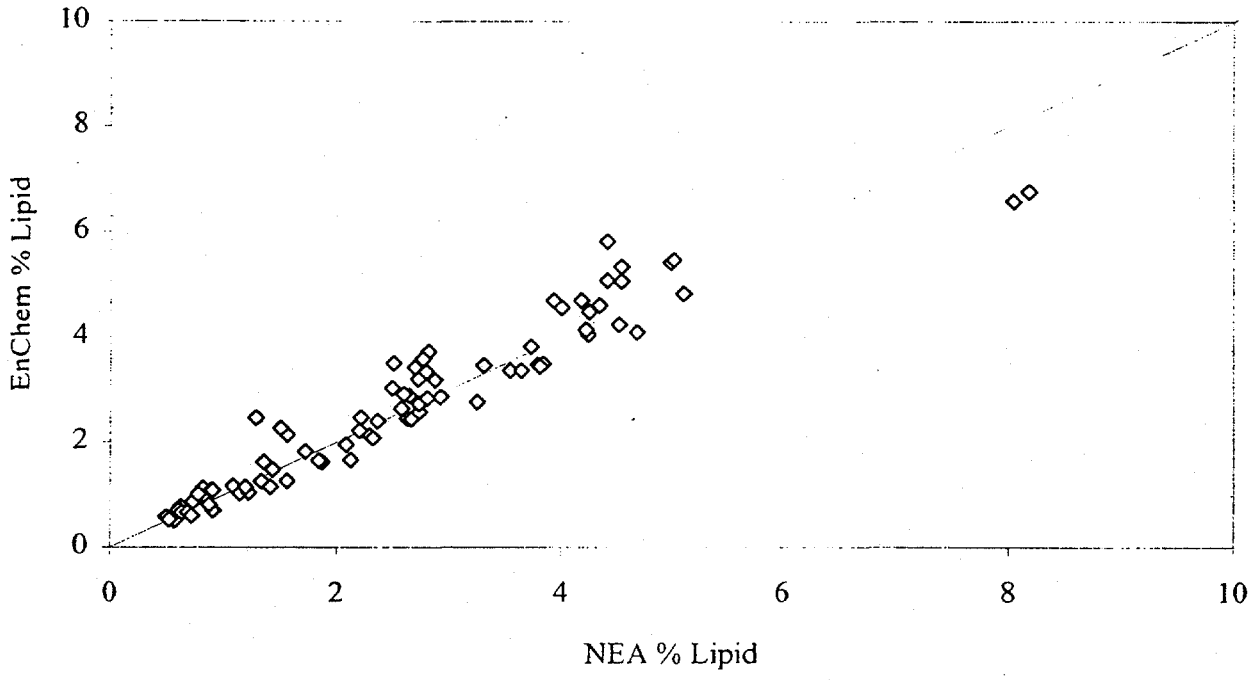
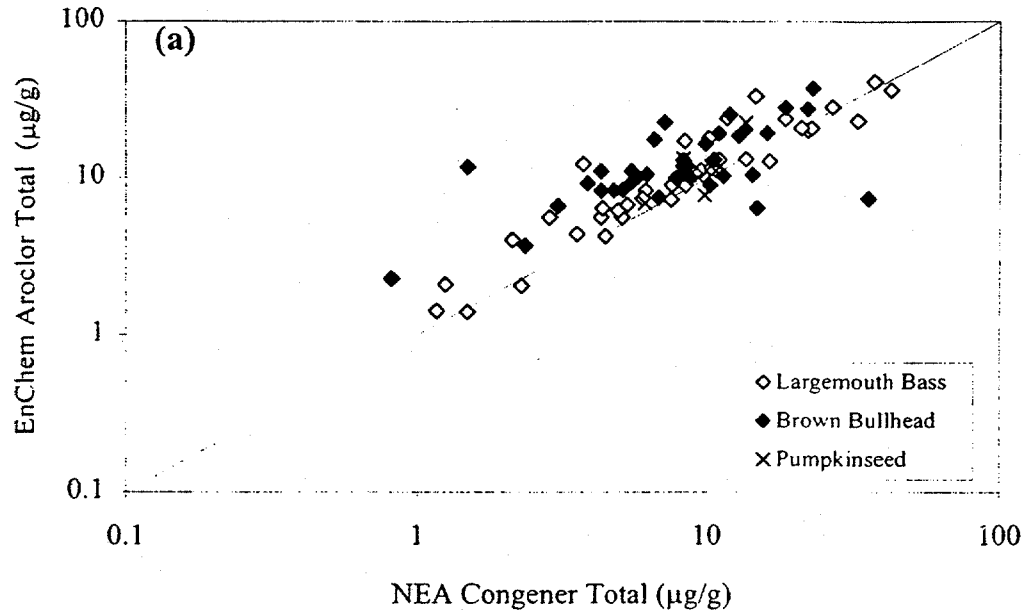


Figure 2-5. 1998 split sample comparison, lipid analysis.



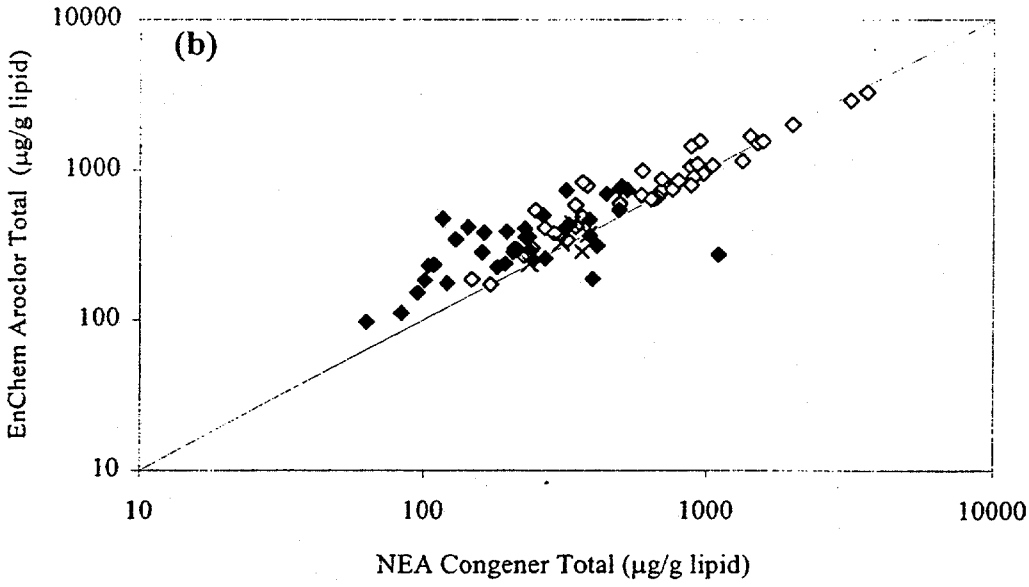


Figure 2-6. 1998 Split sample comparison between EnChem's complete method and NEA's complete method showing different species.

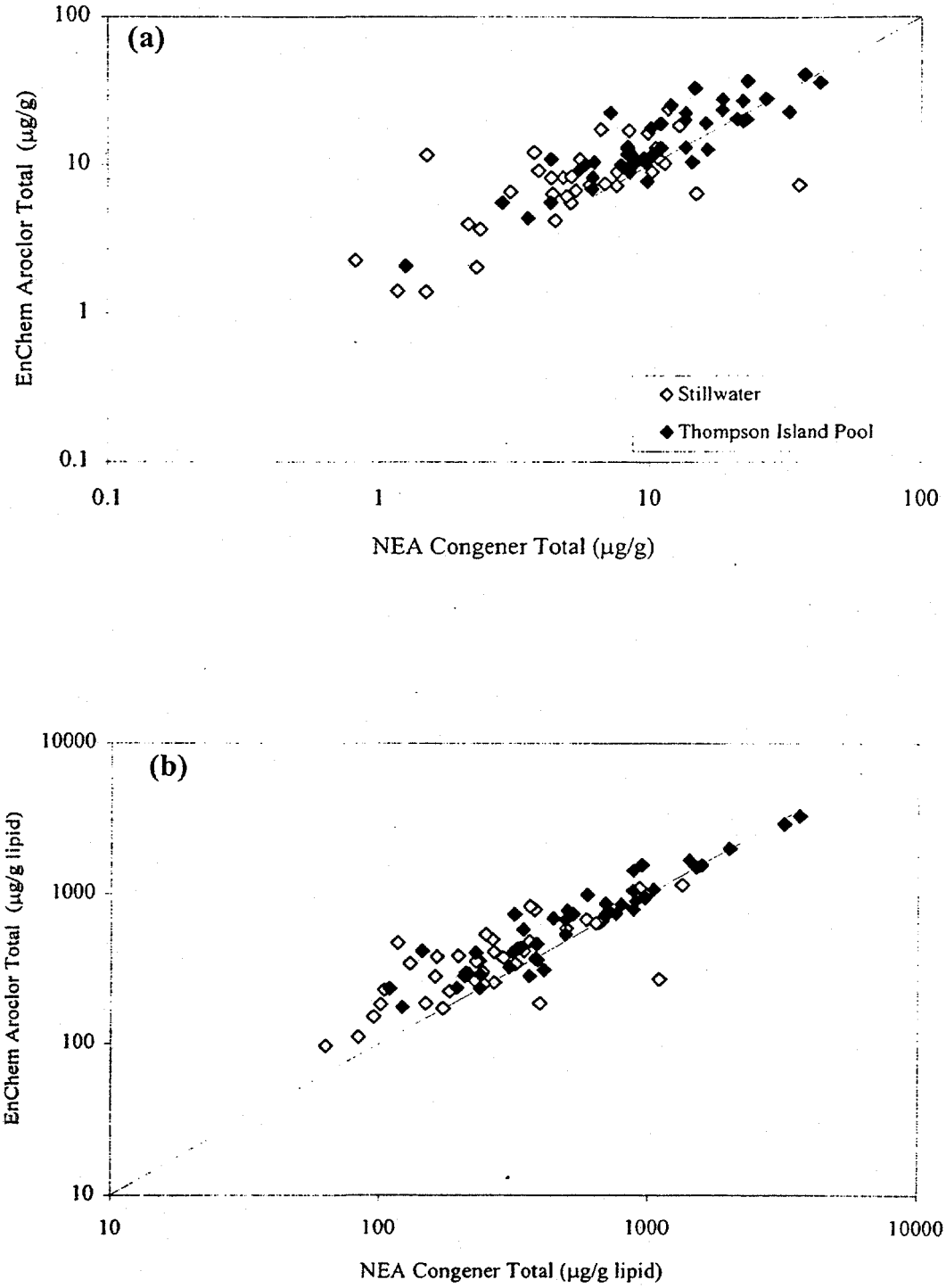


Figure 2-7. 1998 Split sample comparison between EnChem's complete method and NEA's complete method showing different sampling locations.

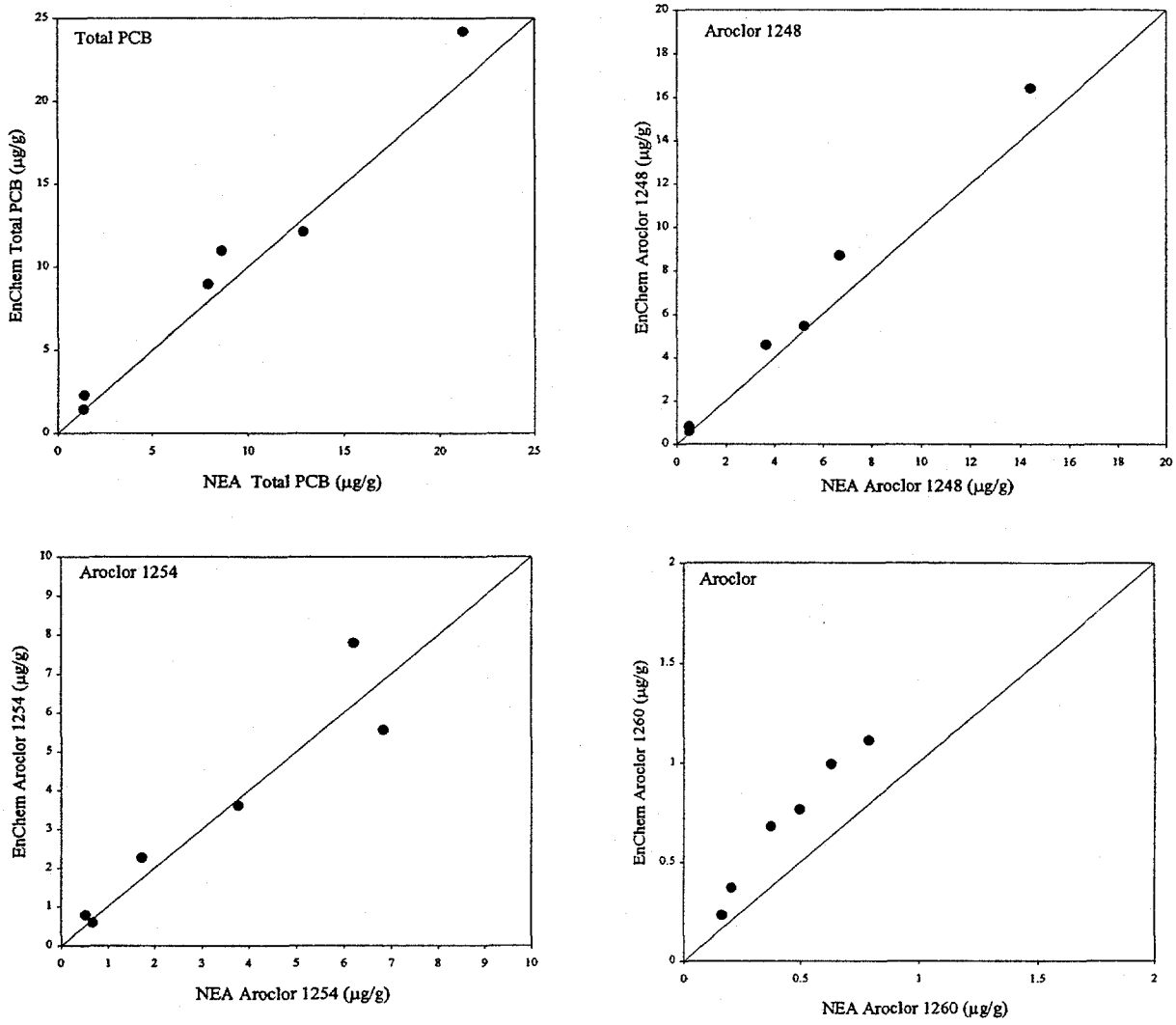


Figure 2-8. NEA and EnChem packed column GC analysis (Aroclor quantification) of EnChem's extracts.

D:\Gmbest\Data\Applied\Biota\DEC_fish_reanalysis.xls

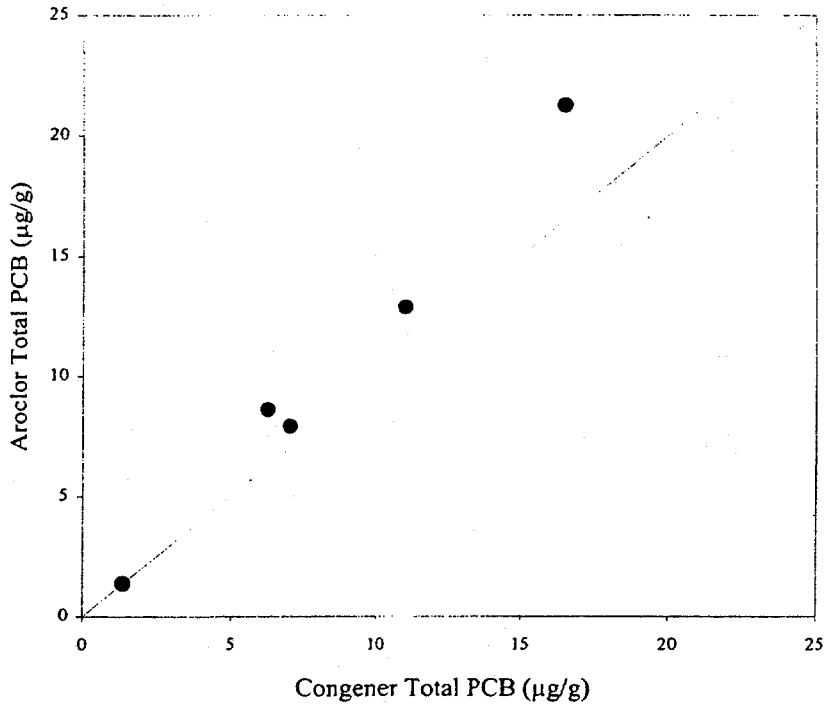
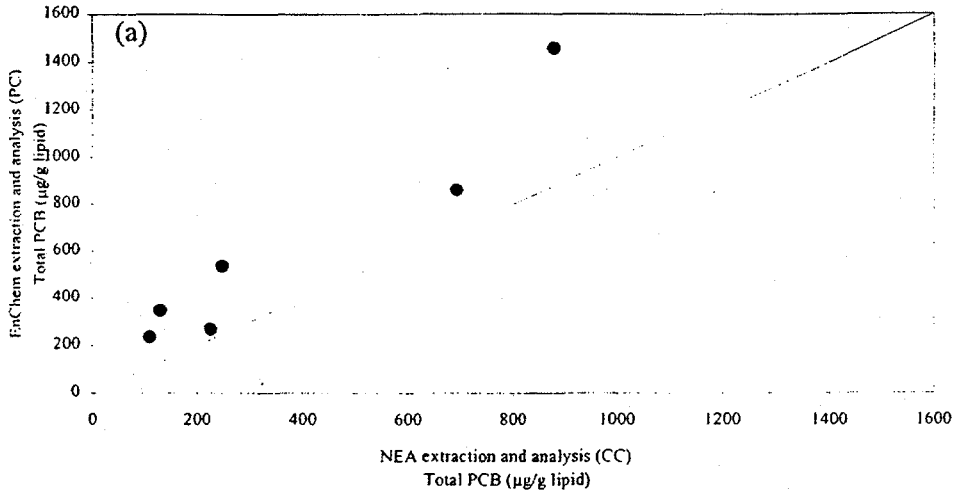
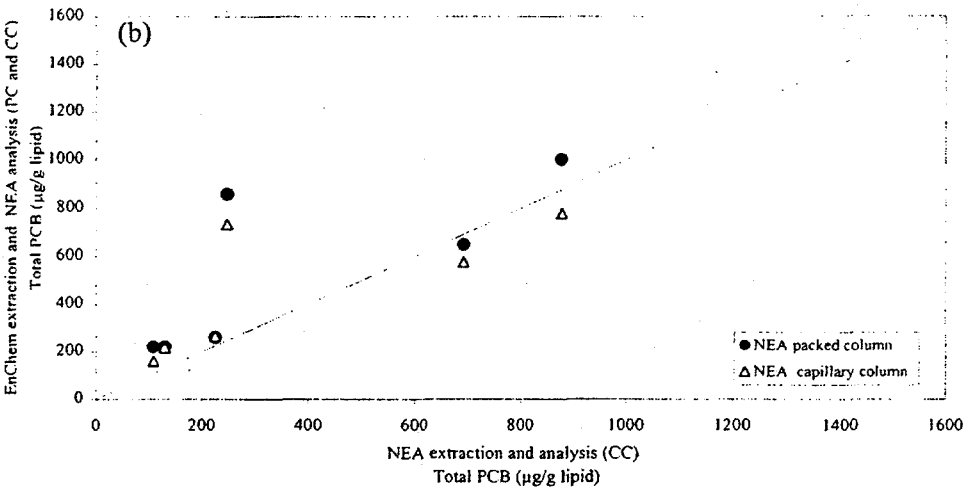


Figure 2-9. NEA analysis of EnChem's extracts, capillary column GC versus packed column GC.

D:\Genhud\Data\Upphud\Biota\DEC_fish_reanalysis.xls





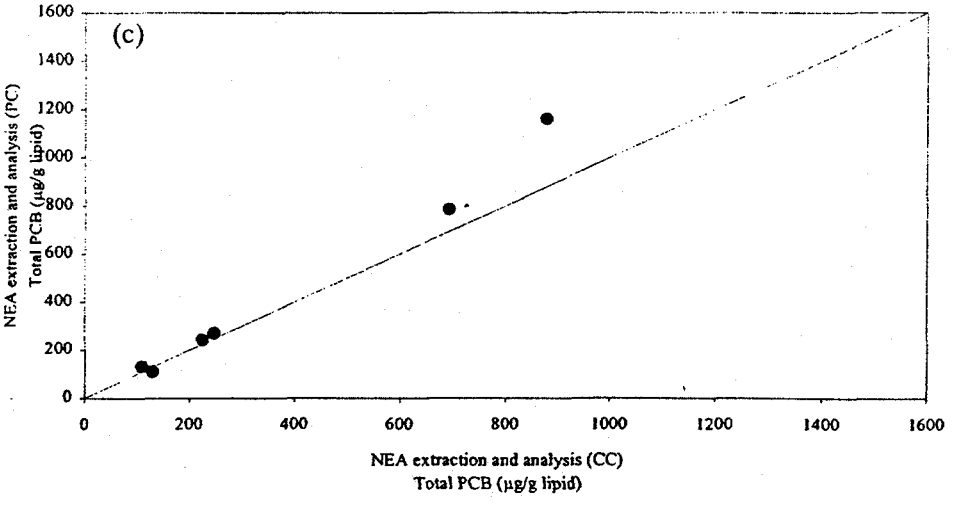


Figure 2-10. Comparison of total PCB results using various combinations of EnChem and NEA's extraction and analysis methods.

D:\Genbud\Data\Upphud\Biota\DEC_fish_reanalysis.xls

TABLES

Table 2-1. 1997 split sample results for GE (NEA laboratory) and NYSDEC (EnChem Laboratory).

Fish Tag Number	Species	River Mile	GE (NEA) Lipid (%)	GE (NEA) tPCB (ug/g ww)	GE (NEA) tPCB (ug/g lipid)	NYSDEC (EnChem) Lipid (%)	NYSDEC (EnChem) tPCB (ug/g ww)	NYSDEC (EnChem) tPCB (ug/g lipid)
130139	STB	11	4.01	1.26	31.54	4.29	1.06	24.71
130140	STB	11	4.51	0.68	15.15	7.39	0.97	13.13
130141	STB	11	7.55	1.51	20.05	8.84	1.02	11.54
130142	STB	11	2.59	0.58	22.47	3.55	0.45	12.73
130147	STB	11	2.31	0.40	17.46	3.20	0.42	13.09
130149	STB	11	3.14	0.43	13.84	3.84	0.48	12.37
130150	STB	11	0.91	0.24	26.52	1.74	0.24	13.56
130247	STB	11	4.9	2.61	53.26	5.29	2.67	50.47
130250	STB	11	2.78	2.03	73.10	3.44	1.49	43.31
130257	STB	11	2.12	0.71	33.68	2.63	0.68	25.86
130158	STB	27	7.14	0.86	12.01	6.71	0.73	10.88
130159	STB	27	4.37	2.63	60.12	7.80	7.19	92.18
130161	STB	27	4.36	0.42	9.61	7.61	0.64	8.41
130168	STB	27	5.21	0.66	12.68	7.00	0.80	11.43
130171	STB	27	2.71	0.68	25.24	4.04	0.90	22.28
130173	STB	27	3.36	0.76	22.62	5.62	0.73	12.99
130175	STB	27	1.62	0.31	19.16	2.40	0.33	13.63
130206	STB	27	1.87	0.37	19.74	2.88	0.40	13.72
130207	STB	27	7.62	1.71	22.50	8.09	1.19	14.71
130217	STB	27	7.31	1.34	18.39	9.16	1.22	13.32
[illegible]	PKSD	59	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	PKSD	59	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
130184	STB	73	7.82	0.81	10.41	10.92	0.77	7.05
130193	STB	73	2.86	1.45	50.70	2.90	1.04	35.86
130194	STB	73	3.95	1.12	28.31	5.36	1.16	21.64
130195	STB	73	4.21	0.91	21.67	5.23	1.00	19.12
130196	STB	73	4.64	0.71	15.24	5.62	0.72	12.81
130203	STB	73	3.47	1.34	38.57	4.02	1.29	32.09
130263	STB	76	3.18	0.92	28.86	3.62	0.77	21.27
130265	STB	76	4.07	1.67	40.91	6.10	1.70	27.87
130267	STB	76	6.91	1.91	27.57	7.92	1.56	19.70
130274	STB	76	3.05	1.03	33.69	3.51	0.68	19.37
131851	LMB	112	3.05	6.21	203.52	3.66	8.49	231.89
131852	LMB	112	1.93	3.11	161.31	3.23	7.25	224.46
131853	LMB	112	2.19	1.58	72.29	2.40	1.69	70.42
131861	LMB	112	2.32	2.24	96.57	3.64	3.86	106.07
131867	LMB	112	3.82	7.63	199.72	2.13	4.56	213.99
131873	WP	112	0.74	0.43	58.29	1.23	0.49	39.84
131875	WP	112	0.63	0.49	77.64	1.07	0.56	51.87
131880	WP	112	1.12	0.28	24.56	1.42	0.27	19.30
131917	WC	112	1.96	3.08	157.34	1.64	2.07	126.22
131920	WC	112	4.11	3.88	94.33	4.08	4.16	101.96
131921	WC	112	3.87	3.31	85.44	4.34	3.81	87.88
4T4802	AMEL	112	2.84	0.46	16.21	2.85	0.74	25.79
4T4803	AMEL	112	8.82	1.26	14.34	10.05	1.96	19.45
4T4804	AMEL	112	8.91	2.35	26.35	8.45	4.02	47.52
130285	STB	113	2.06	0.55	26.55	3.28	0.51	15.55
130286	STB	113	2.45	1.13	46.04	2.53	1.19	47.04
130291	STB	113	1.97	0.50	25.18	2.47	0.65	26.32
130292	STB	113	2.16	2.77	128.35	2.27	3.63	159.91
130293	STB	113	0.68	1.47	216.39	0.81	1.03	127.16
130296	STB	113	2.54	1.09	43.00	3.44	1.48	43.02
130297	STB	113	2.94	0.64	21.81	4.24	0.92	21.70
130298	STB	113	2.55	0.99	38.71	2.90	1.31	45.17
130300	STB	113	3.22	0.97	29.98	3.89	0.85	21.85
130302	STB	113	1.36	0.59	43.13	1.32	0.61	46.21
[illegible]	PKSD	113	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
131892	LMB	152	0.74	1.23	166.24	0.75	1.39	185.33
131893	LMB	152	1.21	2.29	189.22	1.26	3.92	311.35
131896	WP	152	2.06	1.21	58.78	2.24	2.11	94.20
131897	WP	152	2.97	2.31	77.90	3.02	4.19	138.74
131898	WP	152	0.59	2.22	376.24	0.88	3.93	446.59
[illegible]	YP	152	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
131916	YP	152	1.76	0.78	44.43	1.96	1.32	67.35
131940	LMB	152	2.08	7.77	373.39	2.59	13.45	519.42
131942	LMB	152	1.16	2.05	176.33	1.28	3.18	248.67
131947	LMB	152	2.03	6.60	325.01	2.62	10.39	396.53
138341	YP	168	2.12	2.08	97.93	2.23	1.56	69.96
138342	YP	168	1.32	4.44	336.22	1.35	4.61	341.26
138343	YP	168	1.68	1.44	85.65	1.70	1.74	102.35
138301	LMB	176	0.86	1.79	208.63	0.90	1.38	153.33
138302	LMB	176	1.02	1.94	190.45	1.08	1.41	130.56
138310	LMB	176	0.6	1.97	328.89	0.60	2.03	338.33
138311	LMB	176	0.78	5.02	643.00	1.01	6.84	677.62
138312	LMB	176	1.07	1.45	135.25	1.18	1.70	144.07
138352	LMB	189	0.98	6.39	651.62	1.04	7.38	710.00
138353	LMB	189	1.87	37.94	2029.12	2.58	57.36	2223.26
[illegible]	LMB	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	LMB	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
[illegible]	YP	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
138364	YP	189	2.3	10.20	443.64	2.75	15.20	552.73
138365	YP	189	1.54	13.08	849.12	1.58	12.23	774.05
138366	YP	189	1.7	2.09	122.66	1.58	2.33	147.34

STB = striped bass
WP = white perch
YP = yellow perch
LMB = large mouth bass
PKSD = pumpkinseed
[illegible] = analyzed by packed column GC

Table 2-2. Results of NEA's reanalysis of five 1997 split samples.

NYSDEC Fish Tag #	Aroclor	Capillary column µg/g 1998 analysis	Packed column µg/g 1999 reanalysis	Capillary column µg/g 1999 reanalysis
130171	1242		0.085	
	1254		0.810	
	1260		0.216	
	Total	***0.684***	***1.11***	***0.642***
138356	1242		3.372	
	1254		3.280	
	1260		0.400	
	Total	***6.572***	***7.0***	***6.30***
138359	1242		5.125	
	1254		6.490	
	1260		0.606	
	Total	***11.663***	***12.22***	***11.1***
138310	1242		0.654	
	1254		1.392	
	1260		0.348	
	Total	***1.973***	***2.39***	***1.93***
130159	1242		0.629	
	1254		3.130	
	1260		0.411	
	Total	***2.627***	***4.17***	***2.49***

Table 2-3. 1998 split sample results for GE (NEA Laboratory) and NYSDEC (EnChem Laboratory).

Fish Tag Number	Species	Location	GE (NEA)			NYSDEC (EnChem)		
			Lipid (%)	tPCB (µg/g ww)	tPCB (µg/g lipid)	Lipid (%)	tPCB (µg/g ww)	tPCB (µg/g lipid)
130685	BB	GI	4.00	8.28	207	4.56	13.0	285
130686	BB	GI	2.73	6.20	227	2.57	10.5	409
130915	BB	GI	4.98	7.14	143	5.41	22.6	418
130917	BB	GI	3.54	14.39	407	3.36	10.5	313
130918	BB	GI	4.54	22.14	488	5.06	27.4	542
130919	BB	GI	4.18	13.54	324	4.69	20.3	433
130944	BB	GI	4.24	18.65	440	4.05	28.0	691
130945	BB	GI	4.52	8.74	193	4.24	10.1	238
130947	BB	GI	4.41	23.04	523	5.07	37.3	736
130984	BB	GI	2.64	8.29	314	2.88	11.9	413
130985	BB	GI	4.22	16.12	382	4.14	19.3	466
130986	BB	GI	3.3	7.83	237	3.46	10.0	289
130987	BB	GI	2.69	5.76	214	3.42	10.1	295
130700	BB	GI	3.79	12.00	317	3.48	25.5	733
130946	BB	GI	2.21	10.99	497	2.47	19.2	777
130950	BB	GI	3.93	4.28	109	4.69	11.0	235
130991	BB	GI	4.54	5.50	121	5.33	9.4	176
130687	LMB	GI	2.29	22.19	969	2.13	20.0	939
130688	LMB	GI	1.56	13.63	874	1.26	13.2	1048
130689	LMB	GI	1.08	9.67	896	1.16	10.4	897
130690	LMB	GI	2.62	37.25	1422	2.45	41.1	1678
130916	LMB	GI	0.495	3.53	714	0.58	4.4	759
130932	LMB	GI	0.621	4.27	688	0.77	5.6	730
130933	LMB	GI	0.834	2.84	341	0.96	5.6	583
130934	LMB	GI	2.57	26.90	1047	2.64	28.3	1072
130936	LMB	GI	1.14	22.82	2001	1.03	20.6	2000
130937	LMB	GI	1.72	10.13	589	1.82	17.9	984
130938	LMB	GI	0.728	11.02	1513	0.87	13.1	1506
130949	LMB	GI	1.87	16.40	877	1.62	12.8	790
130992	LMB	GI	0.603	9.52	1579	0.72	11.2	1556
130993	LMB	GI	0.813	6.13	754	1.12	8.3	741
130994	LMB	GI	1.56	14.73	945	2.14	33.3	1556
130995	LMB	GI	2.66	21.08	792	2.44	20.7	848
130996	LMB	GI	1.33	42.36	3185	1.25	36.3	2904
130997	LMB	GI	0.902	32.72	3628	0.7	23.0	3286
130999	LMB	GI	1.22	8.43	691	1.04	9.0	862
138659	LMB	GI	2.12	18.61	878	1.66	23.8	1434
138660	LMB	GI	0.6	1.25	209	0.7	2.1	300
9650844	PKSD	GI	2.49	8.28	332	3.03	13.3	439
9650846	PKSD	GI	2.73	9.80	359	2.73	7.8	286
9650847	PKSD	GI	2.72	10.29	378	3.2	11.9	372
9650896	PKSD	GI	2.79	13.63	489	3.34	22.6	677
9650897	PKSD	GI	2.59	6.10	235	2.91	6.9	237
9650898	PKSD	GI	2.87	8.69	303	3.18	10.4	327
130653	BB	SW	8.04	12.90	160	6.58	18.6	283
130672	BB	SW	4.25	4.29	101	4.48	8.3	184
130675	BB	SW	0.63	0.82	130	0.66	2.3	345
130677	BB	SW	2.36	3.85	163	2.4	9.2	383
130681	BB	SW	2.92	3.04	104	2.86	6.6	231
130654	BB	SW	3.84	10.24	267	3.49	9.0	258
130655	BB	SW	8.18	6.82	83	6.76	7.5	111
130661	BB	SW	2.8	5.49	196	2.83	11.0	389

Table 2-3. (cont.)

Fish Tag Number	Species	Location	GE (NEA)			NYSDEC (EnChem)		
			Lipid (%)	tPCB (µg/g ww)	tPCB (µg/g lipid)	Lipid (%)	tPCB (µg/g ww)	tPCB (µg/g lipid)
130662	BB	SW	3.81	14.93	392	3.44	6.5	188
130665	BB	SW	5.01	4.76	95	5.46	8.3	152
130671	BB	SW	4.68	11.38	243	4.09	10.3	252
130678	BB	SW	2.81	5.10	182	3.72	8.4	226
130680	BB	SW	3.24	35.61	1099	2.76	7.4	267
130682	BB	SW	3.73	2.35	63	3.82	3.7	97
130684	BB	SW	1.28	1.49	117	2.46	11.7	476
9641984	BB	SW	3.64	8.50	234	3.36	12.1	360
9641988	BB	SW	4.34	9.87	227	4.6	16.5	359
9641994	BB	SW	2.76	10.56	383	3.58	13.1	366
9641996	BB	SW	2.5	6.57	263	3.5	17.5	500
9641995	LMB	SW	1.35	9.04	670	1.62	10.7	660
9641997	LMB	SW	0.68	4.44	652	0.66	4.3	645
9641998	LMB	SW	1.41	5.08	360	1.15	5.6	487
9641999	LMB	SW	1.84	10.76	585	1.65	11.2	679
130652	LMB	SW	1.5	3.71	247	2.26	12.2	540
130656	LMB	SW	5.1	7.56	148	4.82	9.0	187
130657	LMB	SW	0.773	10.31	1334	1.01	11.6	1149
130658	LMB	SW	2.2	5.29	241	2.22	6.8	304
130659	LMB	SW	0.714	2.28	319	0.6	2.1	343
130660	LMB	SW	0.885	4.34	490	1.08	6.4	593
130666	LMB	SW	0.867	1.49	172	0.81	1.4	173
130667	LMB	SW	4.41	11.74	266	5.81	24.0	413
130668	LMB	SW	0.899	8.31	924	1.08	11.8	1093
130669	LMB	SW	0.567	2.12	375	0.51	4.0	788
130670	LMB	SW	2.08	5.98	287	1.95	7.4	379
130674	LMB	SW	0.52	1.17	225	0.53	1.4	268
9641985	LMB	SW	1.43	4.90	343	1.48	6.2	419
9641986	LMB	SW	1.19	7.53	633	1.14	7.3	640
9641989	LMB	SW	2.32	8.37	361	2.08	17.2	827

BB = brown bullhead
PKSD = pumpkinseed
LMB = largemouth bass
GI = Griffin Island (Thompson Island Pool)
SW = Stillwater

Table 2-4. Results of NYSDEC fish samples analyzed by NEA and EnChem.

		NEA extraction NEA analysis		EN CHEM extraction EN CHEM analysis	EN CHEM extraction NEA analysis	
NYSDEC Fish Tag #	Aroclor	Capillary column mg/g	Packed column mg/g	Packed column mg/g	Packed column mg/g	Capillary column mg/g
130999	1242		4.11	4.580	3.649	
	1254		4.63	3.610	3.777	
	1260		0.831	0.764	0.495	
	Total	***8.44***	***9.57***	***8.954***	***7.921***	***7.03***
138659	1242		15.4	16.400	14.408	
	1254		7.77	7.800	6.212	
	1260		1.23	0.992	0.630	
	Total	***18.6***	***24.4***	***24.20***	***21.25***	***16.5***
130652	1242		1.49	5.450	5.244	
	1254		2.20	5.560	6.844	
	1260		0.353	1.110	0.791	
	Total	***3.71***	***4.04***	***12.1***	***12.88***	***11***
130674	1242		0.451	0.592	0.504	
	1254		0.657	0.596	0.684	
	1260		0.156	0.232	0.163	
	Total	***1.17***	***1.26***	***1.42***	***1.351***	***1.38***
130950	1242		4.24	8.690	6.688	
	1254		1.26	2.270	1.723	
	1260		0.079	0.369	0.203	
	Total	***4.28***	***5.58***	***10.96***	***8.614***	***6.27***
130675	1242		0.236	0.824	0.492	
	1254		0.294	0.783	0.525	
	1260		0.165	0.678	0.372	
	Total	***0.816***	***0.695***	***2.285***	***1.389***	***1.36***