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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: Comments on EPA DEIR/LRC Responsiveness Summary***

Dear Mr. Tomchuk:

Many of the conclusions EPA reached in the Data Evaluation and Interpretation Report ("DEIR") and Low Resolution Coring Report ("LRCR") are not supported by the full data record or sound scientific analyses. These concerns were documented in GE's comments on both of these reports and in subsequent submissions to the Agency. Unfortunately, EPA has never fully or adequately responded to GE's comments or addressed in any meaningful way GE's concerns about the flawed science underlying these reports.

GE's concerns about the DEIR and LRCR are heightened by our belief that EPA intends to rely on these reports to support its proposed remedy for the site. For the reasons GE has previously expressed, such reliance would be arbitrary and insupportable. GE believes that EPA must more fully consider and address the company's concerns about these reports before it concludes the decision-making process for the Site. Accordingly, the enclosed report documents the inadequacy of EPA's responses to GE's comments on the DEIR and LRCR and demonstrates that the Agency's conclusions in these reports remain unsupportable.

We urge EPA to consider this document carefully and respectfully request that the Agency address and respond to the issues identified in this document before it issues the ROD. 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

Attachment

10.3253

cc: Bill Ports – DEC
Bob Montione – DOH

# Review by the General Electric Company of the USEPA Responsiveness Summaries: DEIR and LRCR

December 2000

General Electric Company
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# SECTION 1
# INTRODUCTION

## 1.1 BACKGROUND

The United States Environmental Protection Agency (USEPA) and the General Electric Company (GE) have been evaluating the long-term fate of PCBs in the Upper Hudson River. As part of the Hudson River Reassessment Remedial Investigation and Feasibility Study (RRI/FS), USEPA has issued five Phase 2 reports pertaining to the PCB fate issue:

1) Preliminary Model Calibration Report (USEPA, 1996; PMCR),
2) Data Evaluation and Interpretation Report (USEPA, 1997; DEIR),
3) Low Resolution Sediment Coring Report (USEPA, 1998a; LRCR),
4) Baseline Modeling Report (USEPA, 1999b; BMR), and
5) Revised Baseline Modeling Report (USEPA, 2000; RBMR).

GE issued a report documenting its quantitative evaluation of sediment PCB sources within the Thompson Island Pool (TIP) section of the Upper Hudson River (QEA, 1998; GE TIP Report), for which USEPA provided comments (USEPA, 1998b). GE also issued a report documenting its modeling of PCB fate and bioaccumulation in the Upper Hudson River (QEA, 1999a; QEA, 1999b). In addition, GE has submitted detailed comment documents for the PMCR, DEIR, LRCR, BMR, and RBMR (GE, 1996; GE, 1997; GE, 1998; GE, 1999; and GE, 2000, respectively). USEPA has issued Responsiveness Summaries for the DEIR and LRCR comment documents (USEPA, 1998b and USEPA, 1999a).

GE has reviewed the USEPA Responsiveness Summaries to comments on the DEIR and LRCR, and the Agency's comments on the GE TIP Report. In many instances the USEPA was non-responsive to GE comments or provided incomplete or technically deficient responses. These inadequacies of the Responsiveness Summaries necessitated further comment and clarification which is provided herein.

## 1.2 REPORT ORGANIZATION

This report was prepared by Quantitative Environmental Analysis, LLC (QEA) on behalf of GE to document GE's comments on USEPA's DEIR and LRCR Responsiveness Summaries. The remainder of this section summarizes the major issues pertaining to USEPA's evaluation of the fate of sediment PCBs. Section 2 presents a detailed discussion of USEPA's comments on the GE TIP Report. Sections 3, 4, and 5 of this document contain GE's rebuttal to USEPA's specific responses for the PMCR, DEIR, and LRCR comments, respectively. In Section 6, GE's comments on the updated analyses presented in USEPA's LRCR Responsiveness Summary are documented.

## 1.3 KEY ISSUES REGARDING PCB FATE IN THE UPPER HUDSON RIVER

The USEPA and GE have developed quantitative mass balance models that mechanistically describe the movement of PCBs within the sediments and between the sediments and the water column (USEPA, 2000 and GE, 1999). The GE models have been compared to numerous data sets, including the increases in total suspended solids during storms, measurements of long-term sedimentation rates at several locations, estimates of the long-term trend in sediment PCB concentrations from 1977 to 1998, and a long-term record of water column PCB concentrations from 1977 to 1998. The combination of mechanistic descriptions of the processes affecting PCB fate and the demonstrated ability to reproduce trends in water column TSS and PCBs and sediment PCBs provide confidence that PCB movement and fate are being well approximated by the GE models. The USEPA models are similar in structure to the GE models, although they are somewhat less mechanistic and more empirical. Despite containing some flaws and inconsistencies, the USEPA models yield similar assessments of PCB fate and bioaccumulation as the GE models.

Whereas the LRCR and DEIR do not consider the mechanisms responsible for changes in PCB levels (and only qualitative discussions are given in the Responsiveness Summaries), the models developed by USEPA and GE provide the best tools for identifying and quantitatively evaluating the key issues regarding PCB fate and transport in the Upper Hudson River. The

USEPA must recognize that its approach in the LRCR and DEIR is too inaccurate to provide a basis for answering key questions regarding PCB fate in the Upper Hudson River.

### 1.3.1 Are the Sediments "Stable" and Subject to Burial?

Over the past 20 years, sediments throughout the Upper Hudson River have undergone a dramatic decline in surficial PCB contamination. The GE and USEPA PCB fate models have quantitatively shown that this decline is primarily due to burial in cohesive sediments and low-flow sediment to water flux in the non-cohesive sediments. In contrast, based on the data analyses in the DEIR and LRCR, USEPA asserts that the sediments are not subject to widespread burial and that the decline in surface sediment concentrations results from PCB losses to the water column. USEPA's data analyses, as presented in the DEIR and LRCR, are irreconcilably inconsistent with the modeling. The fundamental questions, then, are which of the analyses – modeling or data – is correct and which can provide a basis for making technically-sound predictions of future conditions and remedial decisions?

The long-term decline in surface sediment PCB levels throughout the Upper Hudson River is evident in all USEPA analyses. The primary discrepancy between the various analyses is how they account for this trend. While the LRCR contends that the decline is due to fluxes to the water column, the conclusions of the RBMR and GE's modeling report demonstrate that burial is the primary mechanism for reduction of surface sediment PCBs.

Both the DEIR and LRCR conclude that there is no evidence of widespread burial in the TIP. The RBMR and GE's modeling report indicate net burial occurs throughout the TIP, in both cohesive and non-cohesive sediments, and that this burial is the primary cause for the decline in historical surficial sediment PCBs. The RBMR estimates average net burial in non-cohesive sediments at less than 0.1 cm/yr, and 0.24-1.50 cm/yr in cohesive sediments (Fig 7-13 RBMR). Based on these estimates, the average TIP burial rate in cohesive sediments is about 0.65 cm/yr.

Simulations QEA conducted using the USEPA models demonstrate the importance of burial. The cumulative loss of PCBs from the TIP cohesive sediments was tracked through the model calibration and the natural recovery projection (with 10 ng/L upstream source). Over the eighty-year period from 1984 to 2064, about 0.55 MT of PCBs were lost from TIP cohesive sediments. Using the USEPA estimate from its LRCR Responsiveness Summary that the 1984 PCB inventory in cohesive sediments was about 8.2 MT, this loss calculated by the model equates to about 7 percent of the inventory. Further, the model shows that the loss rate declines to zero by approximately 2020, and the remaining 93 percent of the inventory is permanently sequestered (Figure 1-1).

The LRCR suggests an unrealistically large (5 to 59% with best estimate of 40%) PCB mass loss in the TIP cohesive sediment inventory occurred between 1984 and 1994 due to releases to the water column and redistribution of "hot spots." The USEPA contends in the RBMR that the findings of the modeling and the LRCR "*are in general agreement*" (page 140 RBMR). Unfortunately, the RBMR does not provide the reader with the comparison necessary to fully understand the differences between the model and the LRCR. Rather than present the trichloro- and higher homologues ($PCB_{3+}$) lost from cohesive sediments computed by the model for comparison to the mass loss estimate from the LRCR, the RBMR presents the loss from **all** of the Thompson Island Pool sediments. This comparison is invalid and misleading because the LRCR mass loss pertains only to "*cohesive sediment areas that were historically known to be more contaminated than averaged Thompson Island Pool sediments*" (page 140 RBMR). Had the USEPA presented mass balance analyses showing the model's estimate of mass loss from cohesive sediments, the result would have been considerably less than the 9 percent value quoted in the report (page 141 RBMR). Such an analysis would demonstrate that the findings of the modeling and the LRCR are **not** in general agreement. Our evaluation of the USEPA model indicates that less than 0.3 MT of PCB fluxed from the cohesive sediments to the water column between 1984 and 1994. This equates to about 3 percent of the inventory, a value that falls below the uncertainty range presented in the LRCR.

A major criticism of the LRCR findings is that measurements of PCBs in the water column indicate that the quantity of PCBs that left the TIP sediments and progressed downstream

was substantially less than the amount claimed to have left the sediment. The USEPA response to that criticism was to claim that a significant portion of the PCBs that left the cohesive sediments were redistributed within the TIP (LRCR Responsiveness Summary). QEA evaluated this proposition by determining the extent of redistribution predicted by the USEPA model. Setting the upstream source and the non-cohesive sediment PCB level to zero in the HUDTOX model and monitoring the flux of PCBs into the non-cohesive sediments, we found that only 2 percent of the PCBs that leave the cohesive sediments are redistributed to the non-cohesive sediments (Figure 1- 2).

In the LRCR Responsiveness Summary, USEPA did acknowledge that many of the questions raised by GE regarding flaws in the data analysis of PCB inventory change were indeed valid. This led the Agency to undertake a revised approach to the analysis of the data. In the end, the logic apparently used by the USEPA is that this alternate analysis of the Low Resolution Core (LRC) data yields a result similar to the original analysis and therefore the original analysis and its conclusions are valid. In fact, the alternate analysis **does not** yield a result similar to the original analysis. The Agency's original analysis used a comparison of the tri-chlorinated and higher PCB congeners ($PCB_{3+}$) in 1984 with all PCB congeners (total PCB) in 1994. The alternate analysis in the LRCR Responsiveness Summary uses $PCB_{3+}$ for both 1984 and 1994. If this later approach is used for both analyses, they yield mass loss estimates that differ by a factor of 2 (*i.e.*, estimated mass loss is about 80% in the original LRCR analysis and about 45% in the alternate LRCR Responsiveness Summary analysis). This substantial change demonstrates the uncertainty of the approach and illustrates that the conclusions drawn from it lack merit.

With regard to the change in sediment PCB inventory downstream of TIP, the LRCR concludes net loss of "hot spot" PCBs from 1977 to 1994 in all "hot spots" except "hot spot" 28, where a large increase was estimated[1]. This exception was attributed to poor characterization and an under-estimation of the 1977 inventory for that "hot spot." The RBMR does not present

[1] It should be noted that USEPA's peer review panel for the LRCR concluded that PCB mass comparisons made downstream of TIP were not valid: "*[t]he reviewers had no confidence in quantitative comparisons between the 1977 and 1994 sediment coring data sets*" (Eastern Research Group, 1999, p. 3-2).

mass loss results that are directly comparable to the LRCR conclusions. The solids balances presented in the RBMR indicate that all reaches of the Upper Hudson River are net depositional. Although "hot spots" are typically areas of cohesive sediments and these sediments usually have higher deposition than the reach average, one cannot compare pool-wide average deposition with the deposition of the "hot spots" considered in the LRCR. Again it is necessary for the USEPA to perform appropriate mass balance analyses using its model in order to provide a direct comparison with the LRCR.

The models provide a more reasonable account of sediment stability and technically-sound basis for remedial decision making than the data analyses in the DEIR and LRCR because: 1) they are built upon the constraint of mass balance principles; 2) they utilize mechanistic quantification of known PCB fate and transport processes; and 3) they were developed and calibrated such that they are consistent with multiple data sets spanning different time scales and media (*i.e.*, they are consistent with both sediment and water column PCB data).

### 1.3.2 Influence of Upstream Sources on Natural Recovery

As stated previously, the PCB fate models developed by GE and USEPA have demonstrated that the historical decline in surficial sediment PCB concentrations is primarily due to burial in cohesive sediments and low-flow sediment-to-water flux in the non-cohesive sediments. The historical water column PCB levels have been primarily controlled by a combination of upstream loadings and sediment-to-water fluxes. Model projections by GE at the current low upstream PCB loading show a continued decline of sediment levels, however this decline becomes limited by the upstream loading. This indicates that the future long-term decline of PCBs in the Upper Hudson River will be controlled by the future upstream PCB loading.

Both data analysis and modeling conducted by the USEPA understate the importance of upstream sources to the recovery of the Upper Hudson River. The DEIR states that upstream loads are not of great importance to PCB fate and transport in the river. The RBMR makes the statement that upstream loads are not controlling surface sediment concentrations. The RBMR,

however, indicates that upstream loads are becoming more important in controlling water column PCB levels as sediment PCB levels have declined. The USEPA baseline model projections of 30, 10 and 0.0 ng/L upstream PCB concentrations (Figures 8-4 and 8-6 RBMR) demonstrate that the upstream source has a very large impact on water column PCB levels and a substantial impact on surface sediment PCB levels throughout the projection period. This conclusion is in general agreement with the findings of GE modeling efforts and has important implications for remedial decision making: when water column and sediment PCB concentrations are controlled by the upstream source, the relative benefits of remedial actions directed at the sediments become insignificant.

### 1.3.3 Characterization of Sediment PCB Sources

#### *1.3.3.1 Relative Importance of Surface and Buried Sediments*

Data analyses conducted by GE show that in general, previously buried sediment PCBs do not directly contribute a significant amount of PCBs to the water column, and are not brought to the surface and made bioavailable on a wide-scale basis *via* erosion (LRCR comments p 44). These conclusions are based primarily on the observations that the congener compositions of the water column PCBs originating from TIP sediments as well as the fish PCBs reflect a relatively undechlorinated PCB source consistent with surficial sediment PCBs and not with highly dechlorinated previously buried sediments. Modeling has verified that the vast majority of both cohesive and non-cohesive sediments are not subject to erosion at levels sufficient to expose older, buried sediments. Recent USEPA data analyses as well as the Agency's modeling are consistent with GE's findings; however previous conclusions reached in the DEIR and LRCR are contradictory.

The LRCR concludes that a significant portion of the buried contaminated sediments throughout the Upper Hudson River have been lost to the water column. Furthermore, it does not identify any mechanism capable of exposing the buried sediments. If there exists a large portion of previously buried sediments contributing to the water column PCB flux, the downstream PCB composition would be expected to resemble the composition of relatively

dechlorinated sediments. USEPA conducted an analysis of the PCB load increase across the TIP (Section 2.3, Book 3 of DEIR Responsiveness Summary). This analysis indicated that the sediment PCB source is derived from surficial sediments through a combination of pore water and bulk sediment loading. USEPA has failed to recognize that the conclusions drawn from the LRCR are not consistent with the more detailed compositional analysis recently conducted in the DEIR Responsiveness Summary, as well as the conclusions drawn from the Agency's mass balance models. The fact that both USEPA's updated loading analysis and its model indicate that the surface sediments are the major source of PCBs to the TIP water column is in direct contrast with the conclusions of the LRCR, which state that over 40% of the PCB mass in the TIP cohesive sediments was lost between 1984 and 1994. This contradiction arises because for the LRCR conclusion to be true, the large PCB mass loss would have to have resulted in the flux of PCBs from the deep, buried sediments to the water column. This, however, is not consistent with the water column PCB data from TID.

### *1.3.3.2 Relative Importance of Fine and Coarse Sediments*

In the assessment of the relative contribution of fine (cohesive) and coarse (non-cohesive) sediment PCB fluxes, PCB fate modeling conducted by GE has shown that over the past 20 years, the non-cohesive sediments are responsible for the majority of the sediment-to-water fluxes. The net (resuspension + low flow exchange – settling) flux to the water column from the entire Upper Hudson River cohesive sediments (*i.e.,*"hot spots") is estimated at around 300 lb. for the period of 1978-98, while the non-cohesive sediments contribute about 9400 lb. (Figure 6-9, QEA 1999a). For the TIP, modeling has estimated a 20-year contribution of 900 lb. from cohesive and 2400 lb. from non-cohesive sediments (Figure 6-10, QEA 1999a). Whether considering the entire Upper Hudson River or just TIP, the contribution from non-cohesive sediments far exceeds that of cohesive sediment "hot spots."

Conclusions based on USEPA modeling are qualitatively consistent with GE modeling conclusions. LimnoTech, Inc. (LTI), the developer of the USEPA model, has indicated that at the Thompson Island Dam (TID), the contribution of non-cohesive sediments to the water PCB

flux is nearly twice that of the cohesive sediments (USEPA Science and Technical Committee meeting 6/16/99). Mass balance analyses QEA conducted with the USEPA model indicate that between 1984 and 1994 the contribution from non-cohesive sediments was almost four times that of the cohesive sediments (*i.e.*,280 kg left cohesive sediments and 1,000 kg left non-cohesive sediments). In apparent contradiction, data analyses conducted as part of the LRCR indicate that large portions of the sediments with the highest PCB inventories, *i.e.*,cohesive sediments, are responsible for the majority of the sediment PCB source to the water. USEPA has not as yet attempted to resolve this discrepancy.

### 1.3.4 Sources of PCBs Transported from the Upper Hudson River to the Lower Hudson River

The PCB fate model developed by GE quantifies the relative contribution of various PCB sources in the Upper Hudson River to the Lower Hudson River. Except for during the releases attributable to the Allen Mill failure in 1991-92 (QEA, 1999a), the model indicates that the majority of the PCBs transported to the Lower River over the past 20 years originated from sediments downstream of TIP (QEA, 1999a, Figure 6-11). During this historical period, the other two sources of PCB to the Lower River, upstream loadings and TIP sediments, have roughly equal contributions (~25%).

The RBMR presents results qualitatively consistent with those of the GE model, yet inconsistent with the Agency's DEIR conclusions. As indicated in Figure 7-31 of the RBMR, 15,730 kg of $PCB_{3+}$ are calculated to pass from the Upper Hudson to the Lower Hudson between 1977 and 1997, whereas the gross and net flux of $PCB_{3+}$ from the TIP sediments are only 5,260 kg and 4,250 kg, respectively. Further, some portion of this flux is lost to deposition and volatilization between the Thompson Island Dam and Waterford. Unfortunately, the text of the RBMR does not explore this point. Rather it states that most of the load gain between Fort Edward and Waterford "*occurs in the Thompson Island Pool and Schuylerville Reaches*" (page 142 RBMR). This statement obscures the finding evident in RBMR Figure 7-31 that there is significant exchange of PCBs in all of the reaches and that a significant fraction of the PCBs passing Waterford originate from the reaches downstream of Schuylerville.

In contrast, data analyses conducted by USEPA pertaining to the PCB load increase from TID to Waterford (DEIR) conclude that there is little contribution from sediments downstream of TID. These analyses are flawed because the TID data used were found to contain a bias that resulted in erroneously high estimates of average water column PCB concentrations at TID (QEA, 1998; further documented in Section 2.6 of this document). Water column PCB data routinely collected by GE during 1997-2000 at an unbiased sampling station at TID and a station downstream at Schuylerville clearly indicate a substantial contribution from sediments downstream of the TID. Additionally, the USEPA analysis did not account for PCB fate processes (*e.g.,* deposition and volatilization) which impact the interpretation of sediment contributions based on only water column spatial profiles.

The importance of the downstream reaches is demonstrated by mass balance analyses QEA conducted with the USEPA HUDTOX model for 1994. These analyses indicate that only 17 percent of the PCB load at Troy originates from TIP sediments (Figure 1-3).

The fact that both GE's and USEPA's models contradict the data analyses presented in the DEIR with regards to the relative importance of the sediment PCBs downstream of TIP highlights an important point. The use of data analyses alone can lead to improper conclusions regarding PCB fate and transport because they lack the constraint of mass balance principles and mechanistic quantification of known fate and transport processes, elements that are inherent to mass balance models.

### 1.3.5 Summary

**TABLE 1-1.**
**Summary Comparison of the USEPA DEIR, LRCR, BMR, and RBMR and Corresponding Responsiveness Summaries**

| ISSUE | DEIR/LRCR | BMR/RBMR | COMMENTS |
|---|---|---|---|
| Burial in the TIP | No evidence of widespread burial. | Net burial in both cohesive and non-cohesive sediments<br>Burial is the main cause for the surface sediment PCB drops over the last 20 years | RBMR indicates:<br>burial in non-cohesive areas < 0.1 cm/yr;<br>burial in cohesive areas 0.24 – 1.50 (average 0.65) cm/yr |
| Change in TIP sediment inventory from 1984 to 1994 | LRCR suggests a large mass loss (best estimate of 43% based on updated analysis in LRCR Respons. Summary) from sediments with highest inventory (*i.e.*,"hot spots"). | The RBMR states that a direct comparison to the LRCR cannot be made due to different assumptions and spatial scales. The LRCR analyses only consider a portion of the inventory and discount PCB fate (*i.e.*,LRCR analyses cannot distinguish between losses to the water column and redistribution). | RBMR predicts net PCB inventory loss of only 9%, but does not distinguish between cohesive and non-cohesive sediments. Mass loss from just cohesive sediments (not presented) would be considerably less. To provide a direct comparison, QEA conducted mass balance analyses with the USEPA HUDTOX model to compare with the LRCR analysis. These analyses indicate that only 3 percent of the TIP cohesive sediment PCB inventory was lost. |
| Long-term trend in TIP sediment PCBs | LRCR concludes 40% mass loss is due to releases to the water column and redistribution of "hot spots." | The RBMR states that the decline in sediment PCBs is due to a combination of burial and diffusion to the overlying water. | RBMR concludes much lower loss and attributes most of that loss to the non-cohesive sediment.<br>QEA determined that the USEPA model computes that only 2 percent of the PCBs that leave the cohesive sediments are redistributed to non-cohesive sediments.<br>The RBMR is not consistent with the LRCR because the continual redistribution of "hot spot" sediment PCBs would keep sediment levels elevated in non-cohesive areas (because most of the LRCR mass loss stays in the TIP) and would cause them to decrease in cohesive areas (historical deposits with high PCB concentrations would be redistributed). |
| Change in sediment PCB inventory downstream of | LRCR suggests net loss of "hot spot" PCBs from 1977 to 1994, except for "hot spot" 28, in which a | No results in the RBMR to directly compare to LRCR conclusions. | All reaches of Upper Hudson are net depositional in RBMR solids balances. |

**TABLE 1-1.**
**Summary Comparison of the USEPA DEIR, LRCR, BMR, and RBMR and Corresponding Responsiveness Summaries**

| ISSUE | DEIR/LRCR | BMR/RBMR | COMMENTS |
|---|---|---|---|
| TIP | large gain was attributed to poor characterization and an under-estimation of the 1977 inventory. | | |
| Mechanisms responsible for change in sediment PCB inventory | LRCR does not consider mechanisms; several possibilities are discussed in the Responsiveness Summary, but only qualitatively. | BMR mass balances indicate low flow sediment flux is the dominant release mechanism (77% in TIP). | In the RBMR, the low flow sediment flux of PCBs to the water column is only active over the top few cm of the bed, while the deep inventory is only controlled by diffusion to the surface (the pool is net depositional). The RBMR is not consistent with the LRCR because none of the low flow mechanisms are associated with processes that would serve to "redistribute" "hot spots." |
| Importance of the PCB loading at Fort Edward | DEIR does not consider upstream loads to be of great importance to PCB fate and transport in the Upper Hudson. | RBMR states that upstream loads are not controlling surface sediment concentrations. | RBMR indicates upstream loads have a large impact in controlling water column PCB levels. |
| Data-based estimates of PCB loadings in the river | Late-1970's to mid-1990's estimates presented in DEIR. | Late-1970's to late-1990's estimates developed for model-data comparisons in BMR. | Year-to-year differences are generally small except in 1977-79. This affects the cumulative loadings however because yearly loadings were greatest during these years.<br>Relative to the BMR, the DEIR over-estimates the cumulative loading at Schuylerville and under-estimates the cumulative loading at Stillwater. This sets up the "pipeline theory," in which USEPA previously claimed that PCBs travel conservatively in the water column between TID and Schuylerville, and thereby over-emphasizes the importance of TIP sediments to PCB loadings downstream of TID. |
| The importance of dechlorination | DEIR introduces the "30 ppm threshold" and concludes that dechlorination is not a significant mass loss mechanism. | BMR does not consider dechlorination because the uncertainties in the historical data are too great to differentiate between 10% mass loss and no mass loss. | The 30 ppm threshold for dechlorination was dismissed by peer review. |

**TABLE 1-1.**
**Summary Comparison of the USEPA DEIR, LRCR, BMR, and RBMR and Corresponding Responsiveness Summaries**

| ISSUE | DEIR/LRCR | BMR/RBMR | COMMENTS |
|---|---|---|---|
| | LRCR attributed 10% of the 1984-1994 TIP inventory loss to dechlor. (5% in the revised analysis). | | |
| Partition Coefficients | General inconsistencies between DEIR and certain analyses in the Respons. Summary | 3-phase coefficients for congeners in the BMR differ slightly from those developed in the DEIR Responsiveness Summary (updated DEIR analysis). | Impact of differences between DEIR and BMR partition coefficients considered minor. |

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# SECTION 2
# RESPONSE TO USEPA'S COMMENTS ON THE GE TIP REPORT

In this section, GE's responses to USEPA's comments on the GE TIP Report (QEA, 1998) follow the organization of USEPA's comment document (*i.e.*,Book 3 of the DEIR Responsiveness Summary).

## 2.1 EXECUTIVE SUMMARY - MAJOR CONCLUSIONS OF USEPA'S REVIEW

USEPA summarized the major conclusions of its review in nine bulleted points. GE's general responses to these are listed below. More detailed discussions of these points are provided in subsequent responses to specific sections of USEPA's comment document.

USEPA Conclusion: There is a sampling bias at TID-WEST, but "*GE's attempt to compensate for this bias is incorrect...*" (p. 1).

GE Response: GE did not propose a method to adjust for the sampling bias at TID-WEST. The only correction for this sampling bias proposed in the GE TIP Report was: "*Model calibration needs to be based on at least 1 year of data from the unbiased sampling station located immediately downstream of TID*" (p. 63). GE has been sampling from this station weekly since October 1997. GE agrees with USEPA that the sampling bias is generally less for spring high flows and that it is less for $PCB_{3+}$ than for total PCBs. However, the statistics that form the basis for these conclusions are weak.

USEPA Conclusion: USEPA's TIP loading estimates are higher than presented in the DEIR as a result of analytical bias corrections, even when the TID-WEST sampling bias was accounted for (p. 2).

GE Response: USEPA needs to recognize that limited data (3 sampling events) presented in the GE TIP Report suggest the Agency's station near TID used during Phase 2

sampling may contain an even larger sampling bias than the TID-WEST station. Therefore, conclusions regarding the effects of the sampling bias on its DEIR loading estimates are not valid because the magnitude of the sampling biases were not quantified for the USEPA station.

USEPA Conclusion: The congener signature of the TIP load "*is consistent with a weathered, partially-dechlorinated PCB source...*" (p. 2). USEPA states that: "*The assumption that pore water flux is the only summer loading pathway appears to be incorrect*" and that "*new analyses...suggest that the summer TIP load is a mixture of pore water flux and bulk loading of fine sediment, perhaps driven by bioturbation*" (p. 2).

GE Response: Although multiple mechanisms (*e.g.,* diffusion, groundwater advection, and bioturbation) may be contributing to the summer PCB TIP loading, the mechanisms hypothesized by USEPA are not supported when performed within a mass balance framework. However, GE agrees with USEPA on the main point here: the surface sediments are the source of PCBs to the TIP water column, and not the deep, dechlorinated sediments, as USEPA postulated in the DEIR.

USEPA Conclusion: QEA's modeling is incomplete, poorly documented, and "*not sufficiently refined to be useful for quantitative analysis of hypotheses of PCB loading sources.*" (p. 2).

GE Response: The modeling presented in the GE TIP Report was work in progress, and USEPA's criticisms are not pertinent to the completed model (QEA, 1999b), which yields the same conclusion regarding PCB loading.

USEPA Conclusion: The QEA sediment homolog depletion rate analysis is flawed and does not result in a constraint on interpretation of the TIP load.

GE Response: USEPA is correct in that the analysis under-estimated the initial PCB inventory because 1984 sediment data have been shown to represent $PCB_{3+}$. However, the analysis can still be used qualitatively to demonstrate that the loadings at the TID-

WEST station were not consistent with known sediment loading mechanisms. This analysis was used, in part, to reach the conclusion that a sampling bias at TID-WEST exists.

USEPA Conclusion: PCB loads occur throughout TIP, and are consistent with the known sediment distribution. An "*anomalous*" PCB source is not needed to understand loadings (p. 2).

GE Response: GE agrees with USEPA that the sediment source is distributed throughout the TIP. The float survey data presented in the GE TIP Report demonstrate this and are the basis for concluding that the spatial pattern in TIP water column PCBs indicates "*a nearly uniform areal flux from sediments within the pool*" (p. 61). However, the PCB concentrations measured at the TID-WEST station are inconsistent with this pattern and would require the existence of an "*anomalous*" source if they were valid estimates of the average condition at the dam.

USEPA Conclusion: Sediment PCBs within and downstream of TIP contribute to the water column, but the load per mile indicates TIP is the "*major concentrated source*" (p. 2) and that it may be 2-4 times greater than downstream sediment sources.

GE Response: GE agrees with USEPA that sediment PCB loadings occur downstream of TID. However, the USEPA conclusion that TIP sediments contribute PCBs at a per-mile rate of 2 to 4 times more than those downstream of TID is not supported by data or the Agency's own model results. Analysis of 1997-99 low flow data indicates that the loading per mile from TIP is considerably less than two times that from TID to Schuylerville. Due to the various dynamic processes occurring, the relative importance of sediment loadings from various reaches of the river needs to be evaluated with a numerical model that is constrained by the principles of mass balance, and not data analyses.

USEPA Conclusion: "Hot spot" and non-"hot spot" sediments contribute to the PCB load, but the QEA evaluation that they contribute equally "*...appears incorrect...*" (p. 2).

GE Response: USEPA's dismissal of GE's evaluation does not cite any evidence to support the Agency's claim. The Agency erroneously argues that differences in porosity will cause differences in pore water PCB flux, and it cites differences in pore water DOC (which 1991 measurements show to be minor) as a reason for greater flux from "hot spot" sediments. In contrast, the USEPA's model assumes no preferential flux mechanism for "hot spot" sediments.

USEPA Conclusion: There is no evidence of dense non-aqueous phase liquid (DNAPL) or highly contaminated sediment bed loadings from the Allen Mill event, and it appears that elevated water column concentrations entering TIP between 1991 and 1993 resulted in higher surface sediment concentrations in depositional regions (p. 2).

GE Response: The PCB DNAPL hypothesis supported USEPA's conclusions in the DEIR that particulate loadings at Rogers Island are partially sequestered within TIP. The point is that upstream PCB sources were a source to TIP sediments in the early 1990's, and to the extent that these sources still exist on an intermittent basis, they will continue to replenish surface sediment PCBs in TIP.

## 2.2 GENERAL RESPONSE TO USEPA COMMENTS ON THE GE TIP REPORT

In its comments on the GE TIP Report (Book 3 of USEPA, 1998b), the USEPA presented detailed analyses pertaining to three main issues:

1) the sampling bias at the TID-WEST station,
2) the source of low-flow PCB loadings across TIP, and
3) "hot spot" versus non-"hot spot" sediment PCB sources.

These issues are discussed in detail in Section 2.3 through 2.7 of this document and are summarized below.

### 2.2.1 Sampling Bias at TID-WEST

In the GE TIP Report, an analysis of PCB data from sampling stations within the vicinity of Thompson Island Dam (TID) indicated that the routine station (TID-WEST) yielded PCB concentrations consistently higher than the average level in water leaving the pool. This bias was attributed to incomplete mixing and lateral flow variations. GE proposed using a more representative station located downstream of the dam (*i.e.*, TID-PRW2) for model calibration and future assessments of the TID load, and has continued sampling at this station since October 1997. In its comments, USEPA conducted an extensive evaluation of the sampling bias and concluded that it was dependent on the flow and PCB concentration at Fort Edward. As part of this evaluation USEPA proposed a methodology to compute average PCB concentrations at the TID using the concentrations from TID-WEST. The methodology is flawed for the following reasons:

1. its simplistic assumptions about the relationships among PCB loadings from upstream of Fort Edward, the TIP and the nearshore sediments at TID-WEST are unrealistic, and
2. to convert the data, it uses average ratios that are based on so few samples that the converted values are highly uncertain.

The USEPA compounds the conversion error by using the ratios developed from paired TID-WEST and TID-PRW2 samples to convert concentrations measured at its Phase 2 shoreline sampling location upstream of the TID to average concentrations at the TID. Paired samples collected by GE indicate that the Agency's Phase 2 sampling location yields PCB concentrations that exceed those at TID-WEST.

The relationship between PCB concentrations at TID-WEST (or the USEPA Phase 2 sampling location) and the average concentration at the TID is a complex function of season, river flow and unknown factors that cannot be easily predicted. For this reason GE has advised that the evaluation of average concentrations at the TID for purposes of estimating PCB load or

calibration of a PCB fate model be conducted using data from the unbiased station at TID-PRW2 (QEA, 1998).

### 2.2.2 Low-Flow TIP PCB Loading Sources

The GE TIP report presented PCB congener flux calculations that indicated the low flow TIP PCB loading could be attributed to PCBs associated with surface sediments. The point of GE's original analysis was to demonstrate that surface sediments were the source of the TIP loading, and not the deep deposits, as contended by USEPA in the DEIR. In its comments, USEPA tacitly acknowledges the validity of this central point (*i.e.*, surface sediments not deeper deposits) and focuses on mechanisms by which PCBs are transported from the sediment to the water column at river flows too low to cause a net flux of sediment and associated PCBs into the water column. It argues against the finding in the GE report that surface sediment pore water is the primary PCB source and presents an analysis to suggest that bulk surface sediment resuspension in conjunction with pore water diffusion was responsible for the low-flow TIP PCB loading. This analysis was based upon empirical congener pattern matching, and was not constrained by the principles of mass balance. As a result, USEPA was not able to assess whether the magnitude of the required low flow sediment resuspension was within realistic bounds. In addition, the USEPA's analysis was flawed in that it essentially assumed surface sediment PCBs partitioned very differently when they were in-place versus when they were temporarily resuspended (*i.e.*, different partition coefficients for pore water exchange and low flow resuspension). This assumption is inaccurate because partitioning of PCBs between sediments and water will most likely be governed by the nature of the sediment organic matter matrix, and not whether the sediments are in the bed or resuspended within a boundary layer at the sediment-water interface.

GE has updated its PCB congener flux calculation (see Section 2.4.5) based upon a more extensive data set for the water column and TIP surface sediments (GE 1998 data). The updated analysis is consistent with the GE TIP Report, indicating that the sediment PCB congener pattern required to produce the summer 1998 TIP water column load PCB congener pattern *via* pore water exchange is consistent with 0-2 cm data from the 1998 GE sediment sampling program.

Despite the differences in approach, USEPA's analysis essentially agrees with the main point of GE's pore water flux calculations: **surface sediments are the primary source of PCBs to the water column within TIP**. The exact mechanism responsible for low-flow surface sediment exchange cannot be determined with the existing data, and is most likely a combination of pore water diffusion, groundwater advection, and bioturbation. Since these mechanisms cannot be differentiated, the proper approach is to treat them as a single exchange process, as GE has done in its PCB fate and transport model (QEA, 1999b). When the exchange process is handled in this way and partitioning is consistently applied across each of these mechanisms, the relative contribution of these mechanisms need not be known to mathematically describe the transport of surface sediment PCBs to the TIP water column. The QEA approach was sufficient to accurately reproduce the low flow water column PCB levels observed in the river (QEA, 1999b). In fact, this approach was also adopted by USEPA's modelers (RBMR).

### 2.2.3 "Hot Spot" Versus Non-"Hot Spot" Sediment PCBs

Analyses in the GE TIP Report showed that "hot spot" and non-"hot spot" sediments in TIP possessed similar organic carbon (OC) normalized PCB concentrations, and would therefore produce similar pore water flux. USEPA attempted to refute these analyses, using an incorrect evaluation of the effects of porosity and an overstatement of the effects of dissolved organic carbon (DOC) on pore water PCB concentrations. Pore water PCB concentration under equilibrium partitioning is directly proportional to OC-normalized sediment concentration. Although sediments with higher OC will have a larger PCB inventory, all sediments with similar OC-normalized PCB concentrations contribute similarly to the water column under a pore water flux mechanism. This is because pore water PCB concentrations are controlled by OC-normalized sediment PCB concentrations. Contrary to USEPA's assertion, the relationship between the PCB concentration of pore water and sediment organic carbon does not depend on the porosity. Furthermore, variations in 1991 porosity data (O'Brien and Gere, 1993) between "hot spot" and non-"hot spot" sediments are small. Pore water PCB concentrations can also be dependent on the DOC concentration, but the 1991 DOC data do not exhibit a large range nor significant differences between fine and coarse sediment samples.

## 2.3 SAMPLING BIAS AT TID-WEST

### 2.3.1 Total PCB Bias Correction (USEPA §1.1)

USEPA updated the comparison of TID-WEST and TID-PRW2 PCB concentrations using available data (sampling events as of the 10/13/98 GE database release) and concluded that the average TID-PRW2:TID-WEST ratio (TTR) is 0.86. The Agency stated that this value is much closer to 1 than the 0.63 estimate cited in the GE TIP Report (which was based upon summer 1997 data only). Using the most current data, which encompass 137 sampling events (12/1/00 GE database release), the overall average TTR is 0.56 (median = 0.55, standard error about mean = 0.02). The temporal trend of this ratio is displayed in Figure 2-1. Annual averages for 1997, 1998, 1999, and 2000 data are 0.64, 0.70, 0.45, and 0.49, respectively. It must be noted that although there is variability in the TTR statistics (both short-term and year-to-year), the ratio is significantly less than 1.

Based on plots of TTR against flow and Fort Edward concentration, which show weak positive relationships, USEPA concluded that the bias is not significant for Fort Edward concentrations greater than 17 ng/L at flows greater than 4000 cfs. However, splitting the data set on this basis resulted in small sample statistics (*e.g.*, n=8 and n=11) for which inferences from the means and confidence intervals may not be meaningful. The flow effect was attributed to increased lateral mixing at higher flows. A simplistic mass balance calculation was set up to explain the effect of upstream concentration: large upstream loads will lessen the observed effect of the near shore loading, which is assumed to be constant. The Agency's analysis does not account for the possibility that the load may be a function of flow and may also be seasonal. Furthermore, it is not the concentration at Fort Edward to which the bias might be related, but the ratio of the Fort Edward concentration to the concentration increase across TIP (this can be proved using USEPA's equations on p.7 of its comment document). A Fort Edward PCB concentration of 17 ng/L will affect the TID-WEST:TID-PRW2 ratio much less at TID concentrations in excess of 100 ng/L than it will when TID concentrations are in the range of 30-40 ng/L. However, TTR does not exhibit a strong functionality at the greater than 100 ng/L TID

concentration range. The main point is that the simplistic groupings proposed by USEPA do not account for much of the variability evident in the bias nor are they applicable for the full data set. USEPA's "*correction*" of the data based on mean ratios for the four Fort Edward flow and concentration groupings is inappropriate. A better treatment of the TID sampling bias is to base model calibration and loading estimates on data from the unbiased station, which is the approach GE has adopted (QEA, 1998b).

### 2.3.2 $PCB_{3+}$ Bias Correction (USEPA §1.2)

The Agency developed similar flow and Fort Edward concentration relationships for the TID-WEST sampling bias in $PCB_{3+}$, and groupings were again developed, except for the use of a 15 ng/L Fort Edward concentration breakpoint. Again, small sample statistics affect USEPA's conclusion that the bias is only significant for flow greater than 4000 cfs. However, USEPA's conclusion that the sampling bias is slightly less for $PCB_{3+}$ than it is for total PCB is consistent with the most recent data.

### 2.3.3 USEPA's Sampling Bias Summary (USEPA §1.3)

USEPA's conclusion that the bias is dependent on Fort Edward flow and concentration is weak. Further, its assertion that the bias was most likely small during the high upstream loading periods of the early 1990's, although possible, is still speculative and cannot not be used as a means of "*correcting*" these data.

USEPA stated that because the bias is "*small or non-existent*" at high flows, annual loading estimates presented in the DEIR may not be significantly affected by the bias. However, the three samples collected by GE in October 1997 from the USEPA sampling station suggest an even higher bias than the TID-WEST station (Figure 2-2). For these three events, total PCBs at the USEPA station averaged 2 times that of the samples from TID-WEST (and 2.5 times that of the unbiased station at TID-PRW2). Therefore, the USEPA decision to apply the sampling bias calculated from the GE sampling station to the DEIR loadings is invalid. There is no basis for

the assumption that samples collected from the USEPA station are equivalent to those from the GE TID-WEST station.

### 2.3.4 USEPA's Interpretation of the Sampling Bias (USEPA §1.4)

USEPA stated that the sampling bias is apparent, but it is difficult to determine the "*correct*" TID concentration. The Agency discussed a more "*careful analysis,*" which it stated indicates that there is still an "*excess*" load gain. The GE TIP Report went into great detail on the nature of the TIP load gain, and does not in any way attribute the entire load to the sampling bias. Clearly, the TIP surface sediments are contributing to water column PCB loads at TID. The GE TIP Report discusses that the unbiased TIP load gain can be attributed to surface sediment pore water diffusion during low flow periods. After evaluating all the stations in the vicinity of TID, USEPA appeared to conclude that TID-PRW2 is probably the most representative of the load passing over the dam - a point GE agrees with.

However, USEPA also used Fort Miller data from one sampling event to imply that the actual TID concentration may be an average of TID-WEST and TID-PRW2 because the concentration at Fort Miller and Schuylerville was approximately the same as the average of the two TID stations for that event. Aside from basing this argument on a *single* sampling event, USEPA did not recognize the contributions of sediment loadings between TID and Schuylerville. A further discussion of the contribution of sediments downstream of TID is presented below.

### 2.3.5 Evidence of TIP Sediment Loading (USEPA §1.5)

USEPA commented that the higher concentrations observed in near-shore stations during the TID sampling bias study provide evidence that "hot spots" 15-20 "*do indeed constitute a significant source of PCBs to the water column.*" The GE TIP Report does not state that "hot spot" sediments do not contribute to the water column PCBs. However, lateral concentration variability cannot be used to quantify the relative magnitude of sediment sources due to lateral flow variations. As discussed in the GE TIP Report (Section 4.2.4), under a constant sediment

flux rate, regions with lower flow (*i.e.*,near-shore or "hot spot" regions) will experience higher water column concentrations.

### 2.3.6 Re-evaluation of the TIP Load (USEPA §1.6)

USEPA presented estimates of the TIP load gain that were updated to account for analytical bias corrections and the TID sampling bias.

### 2.3.7 Implications of the Sampling Bias (USEPA §1.7)

USEPA acknowledged that the DEIR estimate of the relative contribution of the TIP load is impacted by the TID-WEST sampling bias. Its updated DEIR estimate for the TIP load of 1.4 lb/d is not appropriate since the sampling bias associated with the Agency's Phase 2 TID sampling location has not been quantified. In fact, the updated estimate remains biased high (as discussed above, data suggest that USEPA's TID station exhibits an even greater bias than TID-WEST).

## 2.4 SIGNATURE AND ORIGIN OF THE TIP LOAD

### 2.4.1 Characteristics of TIP Summer Load and Surface Sediments (USEPA §2.1)

USEPA contrasted the summer 1997 water column load congener composition with that of Aroclor 1242. A better comparison would be one that considers water in equilibrium with Aroclor 1242. Due to established differences in congener partitioning, an Aroclor 1242 source would result in an aqueous-phase PCB composition enriched in the less chlorinated congeners (Figure 2-3).

### 2.4.2 Evaluation of QEA Modeling (USEPA §2.2.1-2.2.2)

USEPA presented a critique of QEA's preliminary description of its mathematical modeling framework, in which the Agency contented that "*neither the upstream boundary*

*conditions, the sediment initial conditions, nor the calibration targets were correctly specified*" in the PCB model (p. 27). The modeling results presented in the GE TIP Report were work in progress; USEPA acknowledged that the model was "*still under development*" (p.20). USEPA's comments have been fully considered in the GE Hudson River PCB modeling report (QEA, 1999b).

### 2.4.3 Depletion Rate of the TIP PCB Inventory (USEPA §2.2.3)

USEPA's evaluation of the effect of the sampling bias on the GE Tip Report's depletion analysis was correct: the bias would cause PCB loading to decrease and depletion rates to be lower. This supports part of the point of GE's analysis, which was that the water column loads calculated from TID-WEST are biased high.

USEPA was also correct in that the analysis under-estimated the initial PCB inventory since 1984 sediment data have been shown to represent $PCB_{3+}$. This would cause initial inventory estimates to be higher (and result in longer depletion times). However, the depletion rate analysis can still be used qualitatively to demonstrate that the loadings at the TID-WEST station were not consistent with known sediment loading mechanisms (which strengthened the conclusion of a sampling bias at TID-WEST).

Furthermore, USEPA recognized the point that additional loadings from upstream would replenish surface sediments, which is consistent with the main conclusion of the depletion analysis: if the in-place sediments were the only source contributing to the TID water column load, then the inventory would be nearing depletion for some homologs.

### 2.4.4 Groundwater Seepage Flux Analysis (USEPA §2.2.4)

Measurement of groundwater seepage was performed to provide preliminary data to evaluate USEPA's estimates of TIP groundwater inflow as presented in the PMCR. USEPA made two independent estimates of groundwater flow into the TIP. The first was based on an incorrect application of Darcy's Law. The USEPA estimate combined a literature value estimate

of hydraulic conductivity, a land surface topographic gradient, and an estimate of the TIP surface area to calculate a groundwater flux into the TIP. The second estimate was based on extrapolating a measured stream flow gain within an upstream reach of the Hudson River to the TIP without demonstrating that such extrapolation was appropriate. In contrast, GE's groundwater seepage surveys provided actual measurements of seepage rates at five locations in the River. Notwithstanding the uncertainties related to measurement representativeness and/or precision, the measurements are better estimators than the previous USEPA estimates. This fact is born out by USEPA's expanded discussion in its comment document of the complexity of TIP groundwater/surface water interactions.

USEPA stated that calculation of pore water PCB loading using a single average $K_{OC}$ for total PCB would result in a lower estimate than if the loading was calculated as the sum of the loading for individual congeners (with congener $K_{OC}$'s). USEPA's comment is not correct because the average $K_{OC}$ used in this analysis was a harmonic weighted mean of congener $K_{OC}$'s developed from water column partitioning data. Congener weighting was based on the average 1992 USEPA High Resolution Core surface sediment mass fractions in TIP. It can be shown that computing pore water PCB flux using this harmonic weighted mean $K_{OC}$ with the total PCB concentration is mathematically equivalent to summing the individual flux for all congeners.

USEPA discussed that fractures in the bedrock underlying TIP sediments form localized flow pathways, resulting in large heterogeneities in seepage flux that may have been missed by GE's measurements. If there were a large flow coming in a few places, sediment PCB inventories in these locations would be depleted rather quickly, causing the current PCB loading from seepage in these areas to be insignificant.

As an alternative to direct seepage monitoring, USEPA suggested using "*careful flow monitoring*" of the main stem and tributaries to determine the net groundwater input to TIP. It is likely that this method would not be sensitive enough to measure the groundwater seepage contribution. Application of the maximum measured seepage flux (HSI GeoTrans, 1997) to the entire pool, would only result in a total input of 3.5 cfs. A flow of this magnitude would be in the measurement noise at even the smallest flows within TIP (~1000 cfs). In addition, 15-minute

USGS flow data from Fort Edward suggest that short-term fluctuations in flow due to upstream controls on the river are much larger than an input of this magnitude.

USEPA also stated that "*a focus on ground water seepage may also miss important components of advective loading from sediment including interflow and drainage of exposed nearshore sediments.*" The focus of the seepage meter survey was to evaluate the reasonableness of USEPA's 1996 estimate of groundwater flux into the TIP. The USEPA estimate did not consider interflow and drainage of nearshore sediments in its estimate. These are separate issues, and have not been demonstrated to be significant in the TIP.

USEPA's critique of the seepage study makes several points that are contrary to what the data indicate: groundwater seepage appears to be a minor source of PCBs to TIP. The data collected by GE are the best estimate of groundwater seepage within TIP, and should be the basis for any representation in the modeling effort. The most compelling result from the 1997 study is the spatial trend in the average seepage flux, which showed seepage into the river in the upstream portion of TIP, and seepage out of the pool near the dam. This spatial pattern is consistent with conceptual models of groundwater flow near a dam. GE acknowledges that there are limitations to its seepage measurements, and that seepage does account for some portion of low flow pore water exchange. It is for this reason that the representation of pore water exchange at the sediment/water interface in the GE model (QEA, 1999b) is parameterized by the data such that any advective seepage flux is included in the exchange coefficient.

### 2.4.5 Reanalysis of the TIP Sediment Source Congener Signature (USEPA §2.3)

In this section of its comments, USEPA presented partitioning calculations and additional data analyses to evaluate both pore water and surface sediments as the source of PCBs to the TIP water column under low flow conditions. The Agency based much of these analyses on the 1991 pore water data and emphasized that there are large uncertainties associated with the use of its 3-phase partition coefficients derived from these data. In some sections of its Responsiveness Summaries, the Agency used the pore water data to back its arguments, while in others, it pointed out the large uncertainties when commenting on conclusions GE made based upon these

same data. It is inconsistent for the Agency to accept these data to support its own conclusions while rejecting them to dismiss someone else's conclusions. If the data are to be relied on, they must be considered in a consistent manner.

### 2.4.6 Analysis of a Pore Water Source for the TIP Loading (USEPA §2.3.1)

USEPA used a partitioning calculation based on summer 1997 water column data to conclude that the composition of the TIP load gain is not consistent with a pore water source. This analysis is similar to that presented in Section 4.3.4 of the GE TIP Report, which indicated that the composition of the low flow TIP load was consistent with a surficial sediment source under pore water diffusion. These two diffusive flux calculations were limited in that they only span a relatively short period (*i.e.*,9 weekly sampling events from summer 1997). The major difference between GE's and USEPA's calculations is the partition coefficients. USEPA used 3-phase partition coefficients from optimization analyses with the 1991 sediment and pore water data, while the values in the GE TIP Report are based upon the USEPA water column partitioning data, taking into account temperature corrections developed in GE's comments on the DEIR. One apparent discrepancy is that USEPA did not account for differences in partitioning temperature-dependence among individual congeners. The analyses presented in GE's comments to the DEIR suggest a decreasing temperature correction slope with increasing congener $K_{OW}$. USEPA also does not consider temperature effects on partitioning in the flux analysis presented in its comment document. These differences are significant for BZ# 1, 4, and 10 at the average water column temperature for the period on which USEPA based its analysis (25°C during summer 1997).

Additional analyses conducted by GE, based upon a larger water column data set (15 sampling events from summer 1998), further support the original conclusion presented in the GE TIP Report. A similar flux analysis was performed using the GE partition coefficients, the summer 1998 water column data, and 1998 GE sediment data (which provide a better spatial coverage of TIP surface sediments than the High Resolution Cores USEPA used in its comparisons). The summer 1998 PCB composition of the TIP water column load was used to estimate the composition of a surface sediment source under a diffusive pore water flux

mechanism and equilibrium partitioning. This analysis results in almost an exact PCB congener composition match between the sediment source back-calculated from 1998 water column data and the average from the 1998 sediment data (Figure 2-4).

The results from this analysis show that a pore water source can account for the 1998 low flow TIP load, without having to invoke an undefined mechanism not supported by data (see Section 2.4.7). In addition, the PCB composition of the TID-WEST water column samples collected during the 15-year flood event of January 1998 (34,200 cfs) is also consistent with 1998 0-2 cm sediment data (Figure 2-5). These analyses, using well-established fate and transport mechanisms (pore water exchange at low flow and resuspension at high flow), show that surface sediments are responsible for the TIP load. The data do not support the Agency's hypothesis that some unknown mechanism made the buried, dechlorinated sediment deposits available to the water column.

### 2.4.7 Alternative Water Column Source - Pore Water and Bulk Sediment Loading (USEPA §2.3.2)

USEPA hypothesized that the low flow TIP water column loading could result from a combination of surface sediment pore water exchange, and "*fine sediment resuspension and settling.*" The hypothesized sediment loading mechanism (pages 34-37 of the Agency's comments) consists of the bulk resuspension of surface sediments, from which PCBs partition into the water column, followed by the settling of the solids (and POC-bound PCBs). In USEPA's analysis, the resuspended sediment congener concentrations were based upon 1991 GE 0-5 cm sediment data, and the resuspended sediments were assumed to partition according to water column coefficients developed from Phase 2 data (DEIR Table 3-8). The fraction of each PCB congener that remained in the water column after resuspension and settling was calculated by the Agency using the water column partition coefficient and average values for water column DOC and POC concentrations in TIP from Phase 2 data. The pore water source was based upon the 1991 GE 0-5 cm data. USEPA then used a linear combination of the pore water concentration (based on 1991 GE data) and the dissolved plus DOC-bound concentration

resulting from the resuspension of surface sediment to determine a best fit to the observed water column signature.

The primary flaw in USEPA's approach is that its use of a linear combination of concentrations is empirical and not based on known, well-defined mechanisms. USEPA's "combined-source" flux equation, essentially contains two mass transfer coefficients, which cannot be determined uniquely (*i.e.*, there is one equation with two unknowns). The Agency was therefore required to perform an optimization calculation based on several PCB 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 a model framework. To ground truth its estimates for these coefficients, USEPA should have performed the calculation within a mass balance framework. This would have required estimates of the net pore water exchange coefficient (*i.e.*, the combined effect of diffusion, advection, and surface mixing) and the amount (*e.g.*, mass flux) of surface sediment that resuspended, partitioned, and subsequently settled. These values should have been compared with realistic estimates to determine whether the hypothesized mechanism made sense.

An analysis of this sort was performed for this document. A conceptual diagram of the mass balance calculation with the two proposed mechanisms is shown in Figure 2-6. The mass balance states that the change in water column PCB load across the TIP is equal to the PCB load from pore water flux plus the PCB load from bulk sediment resuspension and partitioning. The congener mass balance equation used for this analysis is:

$$Q(C_{TID} - C_{FE}) = C_{pw} k_f A + C_s (1 - f_p) J_R A \qquad (2\text{-}1)$$

where:

- Q is the river flow at Fort Edward (USGS data),
- $C_{TID}$ is the water column congener concentration at TID-PRW2 (GE data),
- $C_{FE}$ is the water column congener concentration at Fort Edward (GE data),
- $C_{pw}$ is the average congener pore water concentration (TIP average from 1991 GE 0-5 cm data),

$k_f$ is the net pore water exchange coefficient (calculated),

A is the area of the TIP (2E6 $m^2$)

$C_s$ is the average congener sediment concentration (TIP average from 1991 GE 0-5 cm data),

$f_p$ is the particulate fraction of PCB that settles back to the bed after bulk resuspension and partitioning (DEIR Table 3-8), and

$J_R$ is the bulk mass flux of surface sediment that resuspends and subsequently settles (calculated).

The analysis was conducted using the average summer 1997-98 low flow TIP congener water column loading for a comparison. The calculated sediment congener loading was based on the sum of the loadings from pore water and bulk resuspension, with the mass flux coefficients (*i.e.*, $k_f$ and $J_R$) adjusted such that the calculated load best matched the congener loadings derived from the water column data. The congener-specific parameters and results from this mass balance analysis are shown in Table 2-1 and are plotted in Figure 2-7.

**Table 2-1. Congener Parameters and Loadings from the TIP Mass Balance Calculation**

| PCB BZ#(s) | Avg. $C_{pw}$ [µg/L] | Avg. $C_s$ [mg/kg] | $f_p$ | Avg. Summer 1997-98 TIP Load [g/d] | Calculated Load from Sediments – Best Fit [g/d] |
|---|---|---|---|---|---|
| 1 | 4.50 | 4.33 | 0.28 | 97.0 | 105.1 |
| 4+10 | 4.85 | 8.56 | 0.09 | 235.8 | 227.6 |
| 5+8 | 0.12 | 2.18 | 0.20 | 20.3 | 45.6 |
| 15+18 | 0.08 | 1.36 | 0.22 | 23.1 | 27.9 |
| 28 | 0.03 | 0.67 | 0.48 | 16.0 | 9.1 |
| 31 | 0.04 | 0.94 | 0.44 | 19.9 | 13.8 |
| 44 | 0.02 | 0.23 | 0.48 | 10.5 | 3.3 |
| 52 | 0.04 | 0.87 | 0.46 | 20.2 | 12.4 |
| 70 | 0.01 | 0.16 | 0.62 | 6.1 | 1.6 |
| 66+95 | 0.03 | 0.44 | 0.66 | 13.7 | 4.0 |
| 101+90 | 0.01 | 0.14 | 0.64 | 3.1 | 1.3 |
| 118+149 | 0.01 | 0.14 | 0.78 | 3.3 | 0.9 |
| 138 | 0.01 | 0.09 | 0.73 | 0.4 | 0.6 |
| 153 | 0.01 | 0.04 | 0.69 | 0.0 | 0.4 |

As can be seen from Figure 2-7, the best fit of the combined mechanisms closely matches the 1997-98 water column data. The best fit coefficients from this analysis are a net pore water

exchange coefficient of 0.3 cm/d and a bulk sediment resuspension flux of 26 MT/d. This resuspension flux is unrealistically large, and is on the order of the five times the average resuspension predicted by GE's sediment transport model under the sampled flow conditions (QEA, 1999b). Had USEPA performed this calculation as a mass balance rather than an empirical fit, the results would have indicated that the analysis yields unrealistic low-flow sediment resuspension estimates.

The main issue with USEPA's hypothesized sediment resuspension loading mechanism is that it invokes a large change in PCB partition coefficient when particles move from the bed to the water column (Figure 2-8). No scientific arguments are presented to rationalize why PCBs would alter their affinity for a particle simply because it has changed location. Indeed, such a change would be inconsistent with existing understanding of contaminant fate, and moreover, USEPA did not invoke such a change in its model (RBMR: page 106).

Another issue is the DOC and POC values used in the partitioning portion of the Agency's calculation. The DOC and POC concentrations resulting from "localized resuspension" of surface sediments would most likely differ from the average water column values measured by USEPA. For instance, to obtain the mean POC concentration of 1.4 mg/L USEPA used to calculate partitioning in its hypothesized resuspension mechanism, the average solids concentration would have to be 78 mg/L[2] in these "localized disturbances." This solids concentration is very high and is inconsistent with field observations in near-shore areas of the TIP during the summer low-flow periods. The likely differences in the DOC and POC values from those used in USEPA's analysis would therefore alter the partitioning in USEPA's calculation.

Whether USEPA's hypothesized "*resuspension*" mechanism contributes to some extent or not, GE agrees with USEPA that the surface sediments are the source to the TIP water column, not the deep, dechlorinated sediments as USEPA first suggested in the DEIR.

[2] This value was computed based on the average surface sediment organic carbon fraction of 0.01788 (USEPA DEIR Responsiveness Summary, Book 3, p.34).

### 2.4.8 Influence of Advection and Dispersion (USEPA §2.3.3)

In its comments on the GE TIP Report, USEPA presents an analysis on differential transport of congeners that was based upon a very large seepage velocity (1 cm/day). Measured values from the GE seepage study were on average 10 times less than the value used in USEPA's analysis. The effect of velocity on breakthrough time is approximately linear, such that a 10X decrease in velocity results in a 10X increase in breakthrough time. Furthermore, USEPA's analysis neglected any consideration of the fact that deposition that is likely to occur (because long-term solids balances indicate TIP is a net depositional environment; RBMR; QEA, 1999b). Sediment deposition would therefore significantly affect the calculated breakthrough time on the temporal scale used in the Agency's analysis (25 years).

## 2.5 UNIFORM AREAL FLUX OF PCBs

USEPA claimed that the float survey data show that "hot spots" are the major contributor to water column PCBs. However, the evidence presented by the Agency does not support this conclusion. USEPA relied primarily on float survey events in which PCBs did not increase greatly between transects without a "hot spot" between them (*i.e.*, 1996 stations 5-6 and 1997 stations 15-15A) to support its contention. The Agency ignored the fact that there were also sequential transects that *did* encompass "hot spots" *without* gains in laterally-averaged water column PCBs:

- 1996 stations 6-7, which encompass "hot spot" 5,
- 1996 stations 12-13, which encompass the southern portion of "hot spot" 8,
- 1997 stations 10-11 which encompass the north-central portion of "hot spot" 8, and
- 1997 stations 14-14A, which encompass the northern half of "hot spot" 14.

Furthermore, there were some instances when even the near shore concentrations did not increase in response to passing over underlying "hot spot" sediments (*e.g.*, for 1997 event 1, there was a decrease between east-shore stations 14-14A, which pass over a large portion of "hot

spot" 14). These observations seriously undermine USEPA's reasoning to support its conclusion about the significance of specific "hot spots" to water column PCB concentrations.

The major point is that single station comparisons from the float survey data are not statistically robust measures of how an individual "hot spot" impacts water column PCBs. However, looking at the data set as a whole, a near linear increase in concentration is observable across the entire TIP. This result was used to infer the nature of the PCB load in the GE TIP Report. Undoubtedly, the surface sediments in the "hot spot" areas do contribute to the TID load. However, the analyses in QEA, 1998 also indicate that the non-"hot spot" surface sediments contribute as well. Further, GE's model indicates that the non-"hot spot" surface sediments contribute more to the total flux past TID than do the "hot spot" surface sediments; this is due in part to the much larger surface area associated with the non-"hot spot" surface sediments.

## 2.6 RELATIVE CONTRIBUTION OF SEDIMENTS BELOW TID

The discussion in this section is another instance where USEPA contradicted a position it had previously taken. In the first section of its comment document, USEPA appeared to accept the TID-WEST sampling bias, and even performed analyses to determine its significance under various flows and upstream loadings. However, in this section the Agency repeatedly expressed doubt regarding the existence of this bias when discussing the sediment contributions to the water column downstream of TID.

USEPA stated that the relative loading from TID to Schuylerville presented in the GE TIP Report is based on limited data, and may not be real. However, Schuylerville has been sampled routinely since September 1997, and the PCB concentration at Schuylerville has been greater than that at TID-PRW2 for 122 out of 131 weekly sampling events (as of the 12/1/00 release of GE's database). For days in which data were not available for TID-PRW2 (because the station was inaccessible), the Schuylerville concentration exceeded the TID-WEST concentration in 20 out of 26 samples.

USEPA referenced its analysis of water column loading data from Waterford to discount sediment sources downstream of TID (even when the TID-WEST sampling bias is taken into account). The Agency discusses a net PCB gain of 0.44 kg/d across TID and a net loss of 0.23 kg/d from TID to Waterford. USEPA's estimated TID load is consistent with the average from the 1997-2000 low flow GE data, but the net loss from TID to Waterford that it discusses is based upon a limited data set (nine rounds of USEPA water column sampling). As discussed above, GE has collected weekly samples from Schuylerville for over two years, which results in a more robust data set upon which loading estimates can be based. Loading calculations from Schuylerville suggest a PCB loading downstream of TID that is comparable in magnitude to that within TIP. Based upon 1997-2000 low flow data, a spatial profile of the average PCB loading from Fort Edward to Schuylerville was constructed in Figure 2-9 These loadings were based upon weekly GE PCB sampling data, and USGS daily-average flow data[3]. Based upon this plot, the average low-flow PCB load gain from TID-PRW2 to Schuylerville (0.6 lb/d) is quite comparable with that from TIP (0.7 lb/d).

## 2.7 "HOT SPOT" VS. NON-"HOT SPOT" SOURCES

### 2.7.1 TIP Surface Sediment Concentrations (USEPA §5.1)

USEPA disputed GE's assertion in the TIP Report that "hot spot" and non-"hot spot" areas exhibit similar areal flux rates of PCBs from pore water because of their similarities in organic carbon (OC) normalized PCB concentrations. The original analysis included a comparison of surface sediment values within TIP for 1984 and 1991 data. When this analysis is updated to include 1998 sediment data (Figure 2-10), the conclusion still holds: OC-normalized surface sediment PCBs are similar in "hot spot" and non-"hot spot" regions of the TIP.

In its discussion of TIP surface sediment concentrations, USEPA stated that even if OC-normalized PCBs are the same across differing sediment deposits, the sediments with the highest mass of OC per unit volume will contribute more PCB load to the water column. This statement

[3] Flow at Schuylerville was estimated to be 17% greater than that at Fort Edward based upon the average flow balances developed in the GE Modeling Report (QEA, 1999).

is only correct as it pertains to *cumulative* loading over extended time periods (*i.e.*,decades). Under equilibrium partitioning, the pore water concentration is determined by the concentration of PCBs on the sediment OC. Therefore, sediments with high OC and high PCBs (fine sediments in near shore, "hot spot" areas) will result in the same pore water concentration as sediment with low OC and low PCBs (coarser sediments in the river channel) because they have the same OC-normalized concentrations. Over time, as PCBs partition to pore water and are transported to the water column, the OC-normalized PCB concentration will begin to decline (due to diminishing PCB mass). The sediments with more OC per unit volume will experience a slower decline since they have a larger PCB inventory than sediments with low OC, and will thus produce a larger cumulative mass loading to the water column. But, the instantaneous mass flux, which determines water column PCB concentrations, is a function of the sediment OC-normalized PCB concentration, and not the PCB inventory (which governs the total mass loading, and how long a particular deposit will contribute PCBs to the water column).

USEPA's comment that "*QEA's argument is invalid for any source mechanisms that involve bulk sediment movement*" is correct but raises a red herring. Under these mechanisms (*e.g.,* hydrodynamic resuspension), and for an equal bulk sediment flux, sediments with the highest dry weight PCB concentrations will yield the highest PCB loading to the water column. QEA's original discussion however, pertained only to a pore water diffusive loading mechanism: "*...*"hot spot" and non-"hot spot" areas are expected to have similar sediment pore water PCB concentrations and, through the process of sediment diffusion, similar areal PCB fluxes*" (p. 48).

USEPA's discussion of the dependence of pore water PCB concentrations on porosity (page 45 of the Agency's comments) is misleading. The Agency's equation for pore water concentration included porosity, and thus expressed the concentration on a total volume basis (*i.e.*,PCB mass per unit volume of sediment plus pore water). However, the concentration of pore water that is transported to the water column *via* advection or diffusion is expressed as the mass of PCB in pore water (dissolved plus DOC-bound) per unit volume of pore water (not total sediment volume). Therefore, the pore water PCB flux to the water column does not depend on porosity of the sediment deposits. Furthermore, the range of sediment porosity is small such that the impact on differences in bulk pore water PCB concentrations (per unit volume of sediment)

is small. Comparison of 1991 data in fine and coarse sediments indicates that the difference in porosity between cohesive and non-cohesive surface sediments in TIP (as defined by the primary sediment type) is small [averages of 0.5 for cohesive (silt) and 0.4 for non-cohesive (sand)]. Most of the 1991 porosity values are within the range of 0.3 to 0.6, with an overall range from 0.15 to 0.9. In contrast, 1998 OC-normalized PCB concentrations in TIP surface sediments varied over 3 orders-of-magnitude. Thus, the effects of porosity differences are small when comparing "hot spot" and non-"hot spot" pore water PCB flux.

USEPA stated that DOC will be higher in "hot spot" sediments, which will produce a greater PCB flux to the water column. However, the effects of DOC on differences in pore water PCB concentration between "hot spot" and non-"hot spot" sediments is not as large as USEPA implied. The range of measured DOC values (1991 data) in TIP surface sediments is small: excluding one apparent outlier of 178 mg/L, the DOC ranged from 14 to 51 mg/L with an average of 27 mg/L. Furthermore, when the entire 1991 data set is considered (all reaches and depths), the difference in the average measured pore water DOC between cohesive (silt) and non-cohesive (sand) sediment is not statistically significant. Most of the measured pore water DOC values are in the range of 10-100 mg/L, with an average of 50 mg/L. As with porosity, the range of measured pore water DOC values is much smaller than the range of PCB concentrations, and therefore, DOC differences between "hot spot" and non-"hot spot" sediments (which are minor) are not significant when comparing pore water PCB fluxes.

In sum, pore water concentration under equilibrium partitioning is directly proportional to OC-normalized sediment concentration. Although sediments with higher OC will have a larger PCB inventory, all sediments with similar OC-normalized PCB concentrations contribute equally to the water column under a pore water flux mechanism. The concentration of pore water that is transported to the water column *does not* depend on the porosity, and variations in 1991 porosity data between "hot spot" and non-"hot spot" sediments are small. Pore water PCB concentrations are also dependent on the DOC concentration, but the 1991 DOC data do not exhibit a large range, nor significant differences between fine and coarse sediment samples.

### 2.7.2 PCB DNAPL and Flood Pulse Loadings (USEPA §5.2-5.3)

USEPA stated that there is no "*observational evidence*" of PCB DNAPL being transported as part of the bed load at Rogers Island. This lack of unequivocal data does not mean that such transport did not and does not occur. In fact, the available data supports the existence of such a bed load. No one disputes that DNAPL has and is entering the river upstream of Rogers Island. The 1998 water column sampling in the Bakers Falls wing dam area is evidence of continuing DNAPL sources, as several samples had total PCBs in excess of 1 ppb. These samples had low suspended solids concentrations (all < 2 mg/L), and a PCB composition consistent with Aroclor 1242 (Figure 2-11). The point is that these oils migrate to the TIP and that the large quantities that entered the river in the early 1990's, may have served to replenish TIP surface sediment PCBs.

The USEPA Phase 2 transect studies suggested that a portion of the particulate-phase PCB loadings at Fort Edward drop out once within the TIP. The particle dynamics observed during GE's PCB DNAPL transport study (HydroQual, 1997) were also consistent with this mechanism, as nearly all of the DNAPL surrogate was sequestered upstream of TID. To the extent that the upstream loading is characterized by an occasional pulse loading that may be influenced by DNAPL, the particulate portion of this loading will replenish the surface sediments within TIP. Partitioning, and dechlorination to some extent, affect the ultimate composition of these replenished surface sediments, but the important point is that upstream loadings may have an impact on surface sediment PCBs, and therefore water column PCB loading within the TIP. USEPA generally agrees with this point on page 51: "*PCBs from the Bakers Falls area are likely to have re-contaminated surface sediment within TIP only to the extent allowed by general settling of water column particulate matter.*"

### 2.7.3 Allen Mill Loading (USEPA §5.4)

USEPA used Aroclor 1242 equivalents in the High Resolution Cores to evaluate whether PCB concentrations in the 0-2 cm layers are elevated compared with those in 2-4 cm layers. If so, this would support the proposition that the Allen Mill loading was a significant contributor to

surface sediment PCB concentrations. The results were variable, with increased Aroclor 1242 equivalent levels in 3 of 6 cores collected near TIP. These results, however, suffered from small sample statistics, and were not adequate to draw firm conclusions regarding the effect of the Allen Mill event on TIP surface sediment PCB concentrations. In addition, results from the summer 1998 GE sediment sampling program indicate little evidence of large-scale increases in surface sediment PCBs between 1991 and 1998.

## 2.8 SUMMARY OF USEPA COMMENTS

USEPA misinterpreted QEA/GE's analysis of the TID-WEST sampling bias as a means of discounting the TIP load. The Agency's analyses of the bias indicated that it is less than the average presented in the GE TIP Report (which was never suggested as a means of correcting the historical data). An update of the Agency's analysis indicates that the bias has fluctuated over time, but remains significant. USEPA also indicated that the loading estimates presented in the DEIR are still correct - the updated loading estimate for TIP presented in its comment document is consistent with recent low-flow GE data. However, current data suggest that the relative contribution of sediment loadings to the water column downstream of TID is greater than USEPA stated in the DEIR.

USEPA stated that the sediment transport model of TIP assumes all contaminated sediment areas are undergoing burial. This is not an accurate description of the model, which is fully documented in QEA (1999b).

USEPA stated that QEA/GE "*assumes there is an oil-phase-based transfer*" of PCBs to TIP without supporting evidence. Data suggest there may be an intermittent source in the Bakers Falls Area, and that elevated loadings may pass Rogers Island, and are then subject to partitioning, and settling within the pool. This mechanism may impact TIP surface sediments, and their contribution to water column PCBs.

# SECTION 3
# REVIEW OF SPECIFIC COMMENT RESPONSES PERTAINING TO THE PRELIMINARY MODEL CALIBRATION REPORT

To a large extent, the USEPA deferred response to many of GE's comments on the PMCR to its Baseline Modeling Report (BMR). The responsiveness of the USEPA to GE's original comments on the PMCR is addressed in GE's comments to the BMR (GE, 1999).

# SECTION 4
# REVIEW OF SPECIFIC COMMENT RESPONSES PERTAINING TO THE DATA EVALUATION AND INTERPRETATION REPORT

This section contains GE's responses to the specific comment responses in USEPA's DEIR Responsiveness Summary (USEPA, 1998b). Each response is keyed to the numbering scheme developed in USEPA, 1998b. Responses are addressed in the order in which they appear in USEPA, 1998b. For indexing each specific response, a lookup table is provided in Table A-1 of Appendix A, which contains:

1) the section of the DEIR that GE's comment addressed,
2) the page of GE's comment document (GE, 1997) on which the comment appears, and
3) the page of USEPA's Responsiveness Summary (USEPA, 1998b) on which the response appears.

## 4.1 GENERAL COMMENTS ON USEPA'S DEIR RESPONSIVENESS SUMMARY

### 4.1.1 Contribution of PCB Sources Upstream of the Thompson Island Dam to PCBs Transported from the Upper Hudson River to the Lower Hudson River and to PCBs in Surface Sediments of the Freshwater Lower Hudson River

In its comments, GE disagreed with the DEIR conclusion that the area of the site upstream of the Thompson Island Dam (TID) represents the primary source of PCBs to the freshwater Hudson. Several scientific arguments based on physical/chemical processes affecting PCBs within the river and measurements of PCB levels in sediments downstream of the TIP were presented to support the GE position (see Section V.A of GE, 1997).

USEPA no longer supports the conclusion that PCBs are conservatively transported from the TID to Troy (p. DEIR-19 of the Responsiveness Summary). However, USEPA still claims that *"it is likely that most of the PCBs which cross the dam at Waterford at low flow will have*

*been released from the area above the dam...".* Certainly this is true because the areas near GE's facilities are the source of most of the PCBs in the river between Bakers Falls and Waterford. However, the important remedial issue is whether the area above the dam is the proximate source of the PCBs passing Waterford. Knowing the location of current sources is critical to developing a sound remedial approach. In any event, USEPA presents no new facts or analyses to support its claim. The basis for USEPA's conclusions is still the spatial profile of PCB concentrations and compositions and calculated mass flux from the 1993 Phase 2 water column data, which is not consistent with the trend evident in the much more robust set of current data (*e.g.*, Figure 2-9).

USEPA does not dispute the analyses presented by GE in the Company's comment document. The conclusions drawn from the analysis are based on the necessary occurrence throughout the Upper Hudson River of the PCB transport mechanisms evident in the TIP. Further, USEPA has not fully acknowledged the implications of the recent data collected at Rogers Island, Thompson Island Dam, and Schuylerville, which show a continual increase in PCB loading over this entire stretch (Figure 2-9). The data are mentioned (p. DEIR-5) but the obvious importance of PCB level increases in the water samples between the TID and Schuylerville is not discussed, nor is it reflected in the central conclusion cited above. USEPA also fails to acknowledge the fact that the sediments between Schuylerville and Waterford also contain PCBs in the surface sediments and therefore must be contributing PCBs to the water as it moves downstream. There are no data to suggest that the same physical and chemical processes operating to release PCBs from the surface sediments in TIP are not also operating in the remainder of the Upper Hudson River. A simple comparison between PCB levels in surface sediments above and below the TID illustrates the importance of the sediments downstream of the TID. Using the most recent survey of the entire Upper Hudson River in 1991 (O'Brien and Gere, 1993), the average surface sediment concentration in the 5.9-mile long Thompson Island Pool was 19 ppm. The average concentration in the 32 miles between the TID and Waterford was about 5 ppm. Based solely on length weighting, 5 ppm for 32 miles would contribute 1.4 times more PCBs to the water column than 19 ppm for 5.9 miles. Further, the sediments downstream of the TID are closer to Waterford and PCBs released from these sediments would be subject to less loss (*e.g.*, through settling and volatilization) than PCBs released from

sediments upstream of the TID, weighting the comparison even more in favor of the areas downstream of the TID.

PCBs enter the water column from surface sediments below the TID, as well as from upstream sources and sediments above the TID. The relative contribution of the various sources to PCB flux to the Lower Hudson River cannot be determined from a simple examination of spatial patterns, in the same way that the various withdrawals and deposits to a checking account cannot be discerned by a simple examination of the beginning and ending balance for some month. The assertion that most of the PCBs passing Waterford are primarily derived from above the TID misleads one to conclude that elimination of the sources above the TID will cause a dramatic reduction in PCB flux to the Lower Hudson River. Because the assertion is based on an incorrect and incomplete examination of PCB sources and fate in the Upper Hudson River, such a conclusion is incorrect.

In the DEIR, USEPA also used a simple sediment dilution calculation to predict the contribution of PCB sources above the TID to the PCB levels in sediment levels observed at Albany. The simple model indicated that PCB sources above the TID contributed the majority of the loading at Albany (91%). However, as detailed in GE's DEIR comments, this conclusion is not valid, principally because the Agency's calculation did not account for dilution resulting from increasing solids loading or PCB losses due to deposition and volatilization. Also, the calculation used data at Stillwater as a starting point and could only infer that the source was upstream of Stillwater, not the TID. In the DEIR Responsiveness Summary, USEPA attempts to rebut these comments and support its original calculation using additional literature and reasoning.

The first issue with the Agency's dilution calculation pertains to its assumption of uniform $^{137}Cs$ levels across the entire river system, including the tributaries. GE pointed out in its comment document that the 0-2 cm layer, even south of Stillwater, would display varying deposition rates and as a result, would represent different time periods and likewise, different $^{137}Cs$ levels. USEPA responded by stating that the presence of $^{7}Be$ in nearly all of the 0-2 cm samples supports the assumption that the 0-2 cm layers are of similar age in all cores. GE does

not agree. Although $^{7}Be$ does indicate recent deposition, it does not indicate what fraction of the sediments in the 0-2 cm layer was deposited recently. It is possible that two samples with $^{7}Be$ above the detection limit could in fact represent two different time periods, due to differences in deposition rate. Therefore, comparing the PCB/$^{137}Cs$ ratios throughout the river to determine their relative impact at one point downstream may lead to incorrect conclusions.

USEPA responded to the fact that its sediment dilution calculation does not account for dilution resulting from increasing solids loading by claiming that such dilution did not occur because the solids yield of the watershed between Stillwater and Albany was relatively constant. However, this does not negate the need to account for dilution by increasing solids. The solids yield in the Hudson River at Stillwater is about 0.066 ton/day-mi$^2$. The solids yield of the watershed downstream of Stillwater is about 0.25 ton/day-mi$^2$ (Philips and Hanchar, 1996). Because of this difference, the solids loading in the Hudson River increases as water moves downstream from Stillwater. This increased solids loading provides for dilution of particulate phase PCB concentrations (mg/kg solids). Had USEPA properly accounted for this dilution, its model would have predicted PCB concentrations at Albany significantly lower than those observed and its conclusion regarding the contribution of sources above Stillwater would have been substantially altered. The predicted concentrations would be further lowered if the Agency had accounted for the loss of PCBs *via* deposition and volatilization processes as water progresses downstream. In fact, the Agency would have had to conclude that a significant fraction of the PCBs in the sediments at Albany could not be attributed to PCBs entering the water column from locations upstream of Stillwater.

### 4.1.2 Burial of Contaminated Sediments in the Upper Hudson River

The DEIR fails to properly address the importance of PCB burial by sedimentation (Section VI of the GE comments) and misrepresents GE's position. The USEPA response was as follows:

> *"General Electric has consistently argued that the Upper Hudson acts essentially like a lake, gradually and steadily burying older sediments with newer ones. This is simply not the case. The Upper Hudson, while not a free-flowing river, is still a*

*dynamic system, with sediment scour and resuspension occurring on a regular basis. Thus there is no guarantee of burial as a means to sequester contaminated sediments. This was demonstrated in the Low Resolution Coring Report, which showed sediment inventory losses in a large number of historical study areas."*

GE has never stated that the Upper Hudson acts like a lake and has always subscribed to the view that resuspension does occur in the river during periods of high flow. However, the occurrence of resuspension does not preclude the existence of net burial. Net burial occurs wherever the rate of deposition exceeds the rate of resuspension. This tends to happen in the more quiescent regions of the river, which generally are coextensive with PCB "hot spots." Furthermore, it is interesting to note that the Agency contradicts itself on this point, when on page DEIR-24 of the Responsiveness Summary it states that "*the Upper Hudson at low flow more closely resembles a series of dammed lakes than a river...*"

As with the USEPA's response to GE's comment about the source of PCBs passing Waterford, the Agency did not refute the scientific analysis presented by GE. The Company's arguments are based on the general character of the river, as well as field data and modeling, much of which is attributable to USEPA. The Upper Hudson River downstream of the GE plants contains eight dams, each of which is responsible for the formation of a backwater pool. Further, the pools are characterized by a relatively deep main channel (which for much of the river is part of the Champlain Canal) and shoals. These characteristics typically result in the trapping of sediments within a river. In fact, one of the routine analyses conducted as part of reservoir design is determination of the sediment trapping efficiency and sediment accumulation rate that will result from construction of the dam (Linsley and Franzini, 1972).

As part of it's LRCR, USEPA analyzed 169 sediment cores for $^{7}Be$, an indicator of recent deposition. One hundred nineteen had detectable levels, from which GE estimated deposition rates ranging from 0.2 to 3.0 cm/yr. Further, the lack of detectable $^{7}Be$ in the other cores was not evidence for a lack of burial, but only that the burial rate at these locations was insufficient to accumulate enough $^{7}Be$ to be detectable by the USEPA methods (GE, 1998). Subsequently, GE collected 16 cores in TIP and found detectable $^{7}Be$ in 10 of them (O'Brien and Gere, 1999), confirming the widespread nature of the burial process.

Finely segmented sediment cores collected in fine grained sediments by USEPA in 1992 and GE in 1998 consistently show a sub-surface peak with a decline in concentration toward the surface. The peak concentrations are believed to mark sediments deposited in the early 1970s, following the removal of the Fort Edward Dam. The fact that most cores show that this peak is covered by less contaminated sediment, coupled with the $^{7}Be$ data, is strong evidence that net burial is a fundamental characteristic of the river, at least in the areas of fine-grained sediments, which happen to be the very areas that harbor the largest amount of PCBs and that USEPA incorrectly concluded in the LRCR have eroded.

The USEPA modeling of a 100-year flood (as described in the PMCR) indicated erosion depths of millimeters to a few centimeters of sediment in the PCB "hot spots." Modeling conducted by GE indicates that burial is occurring in the "hot spot" areas at rates of a few mm/yr to about 1 cm/yr (QEA, 1999b). Furthermore, USEPA's modeling shows that the Upper Hudson River is net depositional (USEPA, 2000).

Clearly, USEPA's assertion that burial is not an important process affecting the long-term fate of PCBs in the Hudson River is contradicted by an abundance of data, common sense, rigorous analyses and the Agency's own modeling (which shows that burial sequesters the vast majority of the PCB inventory in the TIP fine sediments).

## 4.2 REVIEW OF SPECIFIC COMMENT RESPONSES PERTAINING TO THE DEIR

Response to DG-1.4E

GE explained that its account of PCBs in the Upper Hudson River incorporates plausible mechanisms for the movement of PCBs through the river, treats similar portions of the river in a similar manner, recognizes the changes in PCB loading, addresses all major processes, and uses a more comprehensive set of data for testing and evaluation. USEPA responded by stating that "*...the interpretation provided in the DEIR is meant to integrate and describe the net effect of all inputs, while noting the most important components, such as the TI Pool load, which are readily*

*discernable from the data.*" In addition, USEPA does not accept the results from the model documented in the GE TIP Report (QEA, 1998).

*Rebuttal*: USEPA's response does not address GE's original comment. Accounting of PCB sources and sinks needs to be performed in the context of a mathematical model of the system. Such accounting by the USEPA model has resulted in conclusions that are inconsistent with those of the DEIR.

Response to DG-1.13

USEPA stated that results from the GE homolog model cannot be discussed without full documentation, and that fish congener ratios used by GE will be evaluated in future ecological work by USEPA.

*Rebuttal*: More documentation on the homolog model is provided in QEA's final model report (1999b).

Response to DG-1.1

GE discussed that the sediments between TID and Schuylerville may contribute significantly to the water column load as measured at Waterford. Although USEPA retracted its "*pipeline*" theory, it also stated that

> *"...results as they are currently understood do not suggest extensive loadings entering the water column in the region below the TID...while downstream sediments undoubtedly make some contribution to the water column PCB inventory, it is clear that they do not contribute at the same level as those of the TI Pool." "...this does not change the fact that the region above Schuylerville has been and continues to be the primary source of PCBs to the fresh water Hudson...Essentially all of the sediment PCB contamination in the Upper Hudson is attributable to GE so that if some of the water column load is generated below the TI Dam, it is merely the re-release of PCBs which originated above the TID."*

*Rebuttal*: The limited USEPA water quality monitoring data from 2 stations over 40 miles of river during a single year (1993) are not adequate to evaluate sediment sources downstream of TID. The PCB loss and gain mechanisms must be evaluated with a mathematical model. This is a general response and will be addressed in more detail in the rebuttal to specific comment responses. Further, it is critical to understand the proximate and current sources in order to properly focus remedial decisions.

Response to DG-1.2

USEPA responded to GE's comment about the importance of upstream loadings by saying that the most important contribution to water column PCBs at TID are the TIP sediments.

*Rebuttal*: GE contends that USEPA did not recognize the importance of upstream inputs in the DEIR, especially during periods of high flows, which represent half of the total annual PCB load passing TID. In any event, this is a general response and will be addressed in more detail in the rebuttal to specific comment responses.

Response to DG-1.3

GE's comment was that there is no evidence to support the conclusion that the TIP load originates from PCBs in highly dechlorinated, highly concentrated sediments. Besides there being no mechanism to resuspend these sediments, the congener patterns of these sediments and those in the water column are not consistent. GE stated that the increase in PCB load across the TIP originates partially from the surficial sediments of the TIP and partially from the PCB passing Rogers Island undetected and later detected at the dam after reprocessing though the surface sediments. USEPA emphasized that the TIP sediments are the source of PCBs to the water column, and that identifying the exact mechanism was not the goal of the DEIR. USEPA suggested biological mixing and water craft use as mechanisms for resuspension that can contribute to the TIP load due to the timing of the TIP load (*i.e.*,May to November). USEPA's response to the possibility of an undetected DNAPL load at Rogers Island suggested GE has

been searching for oil droplets (which is not mentioned in GE's comment document). The Agency cited the DNAPL transport study, which did not show large vertical gradients in the Rogers Island particulate samples. Based on the variability of the water column load composition, which was attributed to a mixture of sources, USEPA made a strong statement that sediments and nothing else are responsible for the TIP load.

*Rebuttal*: USEPA missed the point of GE's comments about lack of a mechanism for resuspension of deep dechlorinated sediments. While biological mixing and water craft use may contribute to the TIP load, this does not address GE's main point that there are no mechanisms for resuspension of deep sediments and that there is no evidence that such a process occurs in the River.

Response to DG-1.4

GE discussed that USEPA's conclusion regarding mass loss by dechlorination did not recognize the effects on toxicity and bioaccumulation. USEPA responded by stating the decrease in bioaccumulation is offset by an increase in mobility, and that the net effect of dechlorination of the food web is unclear and will be addressed during the ecological risk assessment and modeling phases.

*Rebuttal*: Although it was included in the DEIR, USEPA has avoided all discussion of toxicity in its Responsiveness Summaries by deferring this to the modeling (BMR) and risk assessment reports. However, it should be noted that these effects of dechlorination were not addressed by the Agency in the BMR, RBMR, or the Baseline Ecological Risk Assessment (BERA) Report.

GE also commented that USEPA did not consider burial as a remediation mechanism. USEPA responded by saying that the system is too dynamic to guarantee burial, and that scour regularly occurs, based on evidence of mass loss discussed in the LRCR.

*Rebuttal*: The impoundments created from the dams on the river result in areas of enhanced settling, where burial is much greater than in an uncontrolled river. The "hot

spots" exist in these areas because of this property of the River. In addition, the analysis in the LRCR used to conclude sediment mass loss between 1984 and 1994 is based on biased data, and is flawed in that the technique is too sensitive to the sources of variability and uncertainty in the data to make conclusions regarding mass change. Furthermore, the results of USEPA's modeling (RBMR) are inconsistent with the mass loss conclusions purported in the LRCR.

At the end of this comment, GE summarized its interpretation of the four main points made by the Agency in the DEIR. In one of these, GE interpreted USEPA's position that the PCBs entering at Rogers Island at low flow are stored in TIP sediments, even though this "*stored load*" represents about a third of the total load at TID under low flow conditions. The Agency responded by stating that a portion of the low flow Rogers Island load does settle out in TIP based on its analysis of water column congener loading data. However, the Agency acknowledged that this interpretation may also be an artifact of incomplete lateral mixing (*i.e.,* the fact that USEPA's sampling station at TID was unrepresentative).

*Rebuttal*: USEPA appears to have changed its position, by indicating that a portion of low flow TID loading is attributable to upstream sources.

Response to DG-1.23

GE pointed out that a major failing in the USEPA's calculation of global equilibrium partition coefficients is the inclusion of the stations near the remnant deposits and Rogers Island because the data for these locations may represent non-equilibrium conditions. USEPA responds by stating that by using the median (50th percentile) as opposed to the mean, any bias which would have been introduced by these stations is mitigated.

*Rebuttal*: The USEPA response is nonsensical. It acknowledges that the data from the upstream stations do not reflect the same conditions as the downstream stations, but then defends including them in the data analysis because the use of an inappropriate estimate of mean behavior (*i.e.,* the median) somehow mitigates

their impact. If these stations don't belong in the analysis, they should not be included. Further, the mean is the appropriate estimate of average behavior, not the median.

GE commented that USEPA's analysis of temperature dependent partition coefficients is based upon literature rather than the Agency's Phase 2 data. USEPA responded by stating that some Phase 2 samples were believed to be out of equilibrium and others were affected by analytical uncertainty. The Agency stated that there were too many sources of variability in the Phase 2 field data to perform these calculations.

*Rebuttal*: The site-specific data are still the best available data for establishing temperature dependence of partition coefficients.

GE also discussed that the reported Phase 2 temperatures are not ambient – they are biased high. USEPA discussed that the temperatures plotted in GE's comments were not those used for estimating partition coefficient temperature correction factors. USEPA recognized that the *in situ* water temperatures were not recorded during Phase 2 sampling and that Database release 4.1 has attempted to correct these temperatures with field notes. The values used by the Agency for partitioning coefficient calculations were estimates of the temperature at the time of filtration.

*Rebuttal*: It appears that the USEPA has corrected the temperature values after GE notified the Agency of the error.

Revision of Water Column Loading Estimates

Based on review of additional data, USEPA revisited its discussion of water column PCB loading and retracted its "*pipeline*" theory for low flow. USEPA stated that this theory still holds for high flow periods. The Agency discussed that its flow estimates at Waterford were biased high, which would indicate a net PCB loss from TID to Troy at low flow. USEPA asserted that this does not change its conclusion that the TIP still represents the dominant source to the

freshwater Hudson. USEPA also recognized that sediments between TID and Schuylerville contribute to the load at Troy (based on results from the GE TIP Report).

Response to DG-1.24

GE's original comment was that the equation the USEPA used for volatilization is inappropriate for riverine systems because it did not account for flow-induced shear or gas film resistance. In response, USEPA discussed that the O'Connor/Dobbins equation is not completely applicable because the river is dammed. It discussed that "*the Upper Hudson at low flow more closely resembles a series of dammed lakes than a river...*". The Agency stated that the gas exchange at the dams will account for much more PCB loss than surface volatilization, regardless of how it is modeled. USEPA also agreed with GE that gas-phase diffusion needs to be included in volatilization analyses.

*Rebuttal*: USEPA contradicts its earlier statements about burial in the river when it contended that the river impoundments do not act like a lake (Response to DG-1.4) – which is it? USEPA's comparison of dams to surface volatilization is qualitative. GE's modeling analyses (QEA, 1999b) indicate that the surface loss within TIP under typical low flow conditions is greater than that at a typical dam. Using average values for the PCB Henry's constant and typical flow conditions (average water velocity and depth) within the Thompson Island Pool, the average dissolved phase mass loss within TIP by volatilization is estimated to be on the order of 5%, while the volatilization loss of PCBs across dams in the Upper Hudson River was estimated to be less than 1%, primarily due to the low dam heights (QEA, 1999b).

Response to DG-1.9

GE commented that USEPA sampling during the 1993 spring high flow event may not have captured the true loading from upstream, and that USEPA's understanding of the particulate fraction of these loadings may not be correct. USEPA responded by stating that its sampling was representative (*i.e.*, the Agency sampled Rogers Island at the peak flow), and the lack of a solids

increase from Bakers Falls to Rogers Island suggested that the loading was not scour but perhaps some flushing of "*rock fissures and man-made pipes*" containing PCB DNAPL.

*Rebuttal*: USEPA missed GE's point that if the Agency's sampling was at the peak flow, it still might have missed the initial pulse of particulate-phase loading, since resuspension will occur along the rising limb of the hydrograph.

USEPA went on to discuss that the nature of the upstream load is unimportant because it washes through the upper Hudson at high flow, and the most important source is still the TIP sediments, in which current PCB levels represent the combined effects of resuspension, settling, biological mixing, and fresh inputs.

*Rebuttal*: While it is true that all of these mechanisms may affect the sediments in TIP, a better understanding of the relative importance of these is essential for predicting river recovery rates, and must be evaluated through the use of a mathematical model of the river. In fact, the USEPA model shows that the upstream load is important and ultimately controls PCB levels throughout the Upper Hudson.

Response to DG-1.10

GE commented that despite the extensive remedial efforts recently undertaken, the extent of source reduction and control at the Hudson Falls site is unknown. USEPA responded by discussing the decreases in Rogers Island concentration after June 1993, the lack of evidence for oil droplet transport to TIP, and comparisons from the GE TIP Report (QEA, 1998) that show the upstream loading is minor compared to the TIP sediment load.

*Rebuttal*: USEPA does not address the fate of the load that does come across Rogers Island, or the pulse nature of the loads, which could result in a large PCB mass entering the TIP over a short duration and may often go undetected. In addition, the response contradicts the results of USEPA's model which indicates that the

upstream load is important and ultimately controls PCB levels throughout the Upper Hudson.

Response to DG-1.10A

GE commented that the load from upstream accounts for a substantial portion of the TID load at high flow and low flow. USEPA agreed that the upstream load dominates at high flows, but contends that this load is flushed through the system, without depositing fresh material in TIP. USEPA does not agree that the sampling program may be missing large portions of the Rogers Island loading.

*Rebuttal*: There is evidence of pulse loadings that suggest the potential for large quantities of PCB mass to pass Rogers Island over short durations (*e.g.*, the January 1998 flood event). The USEPA Phase 2 transect studies suggested that a portion of the particulate-phase PCB loadings at Fort Edward drop out once within the TIP. The particle dynamics observed during GE's PCB DNAPL transport study (QEA, 1998) were also consistent with this mechanism, as nearly all of the DNAPL surrogate was sequestered upstream of TID. To the extent that the upstream loading is characterized by an occasional pulse loading that may be influenced by DNAPL, the particulate portion of this loading will replenish the surface sediments within TIP. Partitioning, and dechlorination to some extent, affect the ultimate composition of these replenished surface sediments, but the important point is that upstream loadings may have an impact on surface sediment PCBs, and therefore water column PCB loading within the TIP.

Response to DG-1.11

GE presented diffusive flux calculations to demonstrate that an unaccounted-for load exists when the upstream and sediment diffusion loads are subtracted from the TID load. USEPA responded by presenting a number of issues with the GE analysis. The Agency stated that the 1991 data used for calibration of the exchange coefficient ($k_f$) was based on limited

water column data prior to September, 1991. USEPA also argued that diffusion may not be the only mechanism for pore water exchange.

*Rebuttal*: Weekly samples from June through August were deemed adequate for estimating the average summer loading, and care was taken to exclude samples taken near the time of the Allen Mill collapse. By calibrating $k_f$ to PCB load data, this net exchange coefficient accounts for a combination of pore water exchange mechanisms, including groundwater advection and biological mixing.

USEPA also refuted the partitioning calculation, stating that the uncertainty in USEPA's $K_{OC}$'s and the assumption that $K_{DOC}$ = 10% of $K_{OC}$ (which the Agency claims is unsupported), may result in errors in the calculated pore water composition. USEPA added that the effect of freezing on 1991 pore water samples may have impacted the DOC partitioning, and may add uncertainty to its 3-phase partition coefficients.

*Rebuttal*: Although the uncertainty may impact this analysis and should have been accounted for, the effects of the DOC partition coefficient are most likely minor in this calculation. However, the assumed 10% ratio of $K_{DOC}$ to $K_{OC}$ is consistent with results from several laboratory and field studies (*e.g.*, Evans, 1988, Lara and Ernst, 1989, and Gao *et al.*, 1998). In addition, values for the $K_{DOC}$ to $K_{OC}$ ratio developed from USEPA's updated 3-phase partition coefficients (Table 2-2 in USEPA, 1998b) for 15 select congeners range from 1% to 15% with an average of 5%. Furthermore, USEPA's $K_{DOC}$ to $K_{OC}$ ratios for the most prevalent congeners in TIP sediments are very close to 10% (*i.e.*, 15% for BZ#1 and 7% for BZ#4+10). Therefore, GE's assumed $K_{DOC}$ to $K_{OC}$ ratio does not appear to be as unreasonable as USEPA suggests.

Response to DG-1.15A

GE commented that pulse loadings from upstream are not captured by the current monitoring program. USEPA responded by stating that over 400 samples have been collected at

Rogers Island over the last 9 years, and that this is adequate to characterize the upstream loading. USEPA referenced the GE TIP Report (QEA, 1998) figure that demonstrates lack of lateral and vertical variability in PCBs during the Rogers Island transect sampling studies.

*Rebuttal*: USEPA is basing its argument that vertical homogeneity refutes the presence of oil droplets at Rogers Island on two (low flow) sampling events. GE's main argument is that this load is transient, and may occur at the rising limb portion of the hydrograph during elevated flow events. Occasional peaks in concentration (most associated with high flow) at Rogers Island have been observed in recent years' routine water column data (although they are much lower in magnitude than those from the early 1990's). Given that the routine sampling is not directed at the rising limb of events and the infrequency at which such events are sampled, it is not surprising that the data do not capture the pulse loadings. Sampling programs directed at high flow events have shown elevated PCB levels supportive of the idea that DNAPL entering the river at Hudson Falls is transported downstream in pulses associated with high flow events.

Response to DG-1.15B

GE commented that the USEPA missed the peak PCB loading during the 1993 high flow sampling. USEPA responded by claiming that its sampling captured the peak flow.

*Rebuttal*: USEPA missed GE's point that the peak PCB load at Rogers Island during high flow may not necessarily correspond to the peak flow, as the highest concentrations will be expected due to resuspension at the rising limb of the hydrograph. This point is exhibited by the March 2000 high flow sampling data conducted by GE, in which the highest PCB concentrations were observed prior to the peak flow (Figure 4-1).

Response to DG-1.15C

GE commented that PCB oils are likely the source of spring PCB peak transport. USEPA responded by citing its previous arguments that there is no evidence of oil droplet transport, and that the turbulent mixing between Bakers Falls and Rogers Island results in complete mixing. USEPA continued by stating that the large loads at Rogers Island pass through the upper Hudson relatively unchanged, and that even if undetected loads pass, they won't affect the TIP sediment inventory. The Agency concluded that large loads at Rogers Island do not contribute to TIP surface sediments, which are continually replenished by other means, including resuspension, groundwater migration, diffusion, and biological activity.

*Rebuttal*: USEPA ignores the fact that the PCBs passing Rogers Island entered the river in oil form. This dense oil must settle upstream of Rogers Island in order to provide the reservoir needed to account for the high flow PCB loading measured at Rogers Island. If the PCBs do not remain in oil form they would disperse into the water column and be routinely seen at Rogers Island, rather than preferentially during high flow events. Thus, it must be true that some portion of the PCBs entering the TIP do so in oil form. Some other portion must enter associated with particulate matter. Both portions would tend to settle to the bottom and contribute PCBs to the TIP surface sediments. Settling of the Rogers Island particulate load in TIP was evident in USEPA's low flow transect data.

Response to DG-1.16

GE's comment was that PCBs passing the TID are decreased downstream by volatilization and deposition and that these must be offset by sediment loadings to result in a relatively constant load from TID to Troy. USEPA acknowledged that PCB transport estimates in the DEIR will require revision.

GE also commented that USEPA did not consider the effects of various geochemical processes below the TID. USEPA stated that geochemical processes below the TID are discussed but evidence and magnitude is far less obvious in the region below the dam. The basis

for its argument was that since the load difference across TIP is so much greater than any downstream, it focused its analysis on the TIP.

*Rebuttal*: The fate and transport mechanisms affecting PCBs are active in all reaches of the river, and should be incorporated equally in the mathematical models. There is no evidence that would indicate otherwise, and it would be highly unusual for it to be so. In fact, the water column data from the TI Dam and Schuylerville demonstrate that the processes operating in this reach are equivalent to those operating in the TIP (Figure 2-9).

Response to DG-1.4A

GE commented that the USEPA did not offer any account for the fate of the low flow load at Rogers Island in the DEIR. USEPA responded by stating that a portion of the load may be stored in the TIP sediments, but that loading calculations have focused on the net TIP load, which assumes that the Rogers Island load passes through TIP, thus producing a minimum estimate of the TIP load.

*Rebuttal*: USEPA's response does not address GE's comment. USEPA states that the Rogers Island load may drop out in TIP, or its impact on TIP can be ignored by focusing on net loadings only. The point is that the Agency should not base its interpretation on data analysis alone, but should also use a model to understand the effects of the Rogers Island load, both currently and historically. It is only with a mathematical model that mechanisms of PCB fate and transport can be evaluated within the framework that is constrained by the principles of mass balance.

Response to DG-1.4B&C

GE commented that the DEIR ignores established mechanisms of deposition and volatilization in describing the fate of PCBs below the TIP and that it treats similar reaches of the river in a dissimilar fashion (*i.e.*,sediments in TIP contribute PCBs to the water, but those below

TID do not). USEPA responded by stating that the report does not ignore these mechanisms and that "*their existence does not change the nature of the loading in the Upper Hudson (i.e.,that most of the Upper Hudson's PCB load is generated above the TI Dam and that this load is similar in size and congener makeup to that delivered to Waterford.*"

*Rebuttal*: The USEPA did not respond to the comment. The fact that the magnitude and congener distribution of water column PCB loads are similar over portions of the river is no more an indication of a lack of activity in the river (*e.g.*, deposition and volatilization) than having the same balance in a check book at the start and end of a month is an indication of a lack of deposits and withdrawals. PCB accounting in the river needs to be assessed with mathematical models that simulate the varying processes affecting PCB fate and transport in the river. When USEPA did that using HUDTOX, the Agency found that the processes operated similarly throughout the river.

Response to DG-1.4D

GE's comment stated that the DEIR assumed the 1993 conditions were representative of long-term conditions, and that the effects of the Allen Mill were not impacting its data. USEPA responded by stating that its analysis considered other data sets (including the 1993-96 GE water data) to confirm those based on the 1993 data.

*Rebuttal*: The 1993-96 GE water column data contains no sampling from downstream of TID after April 1993. The fact remains that the 1993 USEPA water column data are suspect, both due to its temporal proximity to the Allen Mill loading event, and the sampling bias in USEPA's data collected near the TID (see Section 2.3.3 and QEA, 1998).

Response to DG-1.17-1

GE commented that the USEPA's dilution calculation, which was used to estimate the contribution of the Upper River to lower river PCBs, is flawed because it fails to account for the

increasing solids yield from the drainage basin below the TID and the losses of PCBs to volatilization and deposition, and cannot explain the variability of $^{137}Cs$ upstream of Stillwater. USEPA responded by stating that the data from Phillips and Hanchar (1996) actually demonstrate why the calculation could not be applied above Stillwater (due to rapidly changing sediment yield), but that it could be applied between Stillwater and Albany (due to similar sediment yields of the Hoosic and Mohawk basins).

*Rebuttal*: USEPA's response does not negate the need to account for dilution by increasing solids. The solids yield in the Hudson River at Stillwater is about 0.066 ton/day-$mi^2$. The solids yield of the watershed downstream of Stillwater is about 0.25 ton/day-$mi^2$ (Philips and Hanchar, 1996). Because of this difference, the solids loading in the Hudson River increases as water moves downstream from Stillwater. This increased solids loading provides for dilution of particulate phase PCB concentrations (mg/kg solids). Had USEPA accounted for this solids dilution, its calculation would have predicted PCB concentrations at Albany significantly lower than those observed and its conclusion regarding the contribution of sources above Stillwater would have been substantially altered. The predicted concentrations would be further lowered if the Agency had accounted for PCBs lost *via* deposition and volatilization processes occurring as water progresses downstream. In fact, the Agency would have concluded that a significant fraction of the PCBs in the sediments at Albany could not be attributed to PCBs entering the water column from locations upstream of Stillwater.

Response to DG-1.17-2

GE commented that different deposition rates yield different $^{137}Cs$ levels in the 0-2 cm layer. USEPA responded by stating that nearly all the cores represented in the DEIR have $^{7}Be$ in the 0-2 cm layer and therefore, represent recent deposition. USEPA added that $^{137}Cs$ levels have not varied substantially during the most recent 5 years (based on 0-2 cm data from 10 Hudson River cores) and therefore, since the 0-2 cm layer represents recent material, the differences in $^{137}Cs$ represent differences in local deposition rates.

*Rebuttal*: USEPA's argument is circular. The Agency claims that the spatial differences in surface $^{137}Cs$ levels are a result of downstream dilution. Then it states that the surface $^{137}Cs$ levels in the 10 Hudson River core tops (from the whole river) is relatively constant.

USEPA's response also contended that as long as sediments in the 0-2 cm layer were deposited within the last 5 years, then the different $^{137}Cs$ levels reflect differences in local deposition rates and not differences in $^{137}Cs$ deposition over time.

*Rebuttal*: USEPA missed GE's point that the 0-2 cm layer might not represent deposition from 1991-92 as USEPA assumed. Five years is a large window for the Hudson River core top dating (1987-1992). PCB loadings were greatly different over this period, which adds considerable variability to the PCB/$^{137}Cs$ ratios.

Response to DG-1.17-3

GE's comment was that addition of Aroclor 1242 from external sources would not be accounted for in USEPA's dilution analysis, and that analysis of PCB composition in the Albany core top is biased towards the Upper Hudson River sources due to the large PCB input associated with the Allen Mill event (*i.e.*,this loading is not representative of the entire period). USEPA's response was that the addition of any Aroclor by an external source would have been identifiable in the dilution model analysis because the PCB/Cs ratio would have differed from that predicted by the model. USEPA added that the dilution calculation was valid because it has been predictive for 17 years and has indicated how long GE has dominated the inputs. USEPA acknowledged that inputs identical to GE's in composition would not be discernable in the Agency's analysis, but that it is highly unlikely that additional external sources would have the exact same mixture of Aroclor 1242, 1016, and 1254 as GE.

*Rebuttal*: The congener matching analyses cited by USEPA as indicators of the PCB source are approximate at best. Matching downstream compositions with the

composition from the Stillwater core, can not provide a means of determining a signature of the exact PCB source mixture. The different PCB congeners partition differently, and are affected by dechlorination at different rates and to different extents. Numerous fate and transport processes are at work. USEPA's extrapolation of the Stillwater core as an indicator of the TIP source is subject to significant uncertainties.

Response to DG-1.22

GE commented that USEPA attributed decreases in some tetra congeners to unknown selective degradation processes rather than to the H, H' dechlorination pattern (which results in decreased toxicity and carcinogenity). USEPA responded by stating that toxicity and carcinogenity will be discussed in the ecological and human health risk assessments, and that reduction in toxicity is unclear and decreases in PCB bioaccumulation from dechlorination are offset by increases in mobility due to lower partition coefficients. USEPA added that the occurrence of H, H' dechlorination in the Lower Hudson River does not indicate it will result in significant dechlorination in the Upper Hudson River, and that focusing on a few congeners is misleading because most of the congeners are unaltered by the process.

*Rebuttal*: USEPA dismissed the presence of dechlorination in its analysis of core-top congener patterns. The effects of dechlorination may complicate inferences drawn from comparing congener patterns at different locations to attribute PCB contamination to the various sources.

Response to DG-1.12

GE stated that the composition of the "*unaccounted-for load*" of PCBs across TIP is similar to that of Aroclor 1242. USEPA responded by refuting the Company's loading calculation contending that the analysis was based on "*[non]-defensible assumptions.*" USEPA also stated that due to the uncertainties and range of possible mechanisms, it is not possible to definitely describe the TIP load sources. USEPA went on to state that the sediment source is a mixture of old unaltered sediments, old highly altered sediments, and fresh unaltered sediments.

*Rebuttal*: The USEPA appears to be dismissing the ability of a quantitative mass balance analysis to evaluate the TIP load. While it is true that the source is likely a mix of different sediments, their relative contribution to the TIP load is essential to the development of PCB sources and sinks and ultimately understanding the impacts of various remedial alternatives.

Response to DG-1.14

GE's comment discussed that the unaccounted-for TIP load was consistent in timing with the Allen Mill collapse and bedrock DNAPL seeps. USEPA responded by again claiming that GE is basing its argument on "*undocumented oil droplets*" and by discounting the use of USGS water column data in conjunction with the GE data due to analytical biases (USGS and GE) and sampling biases (GE).

*Rebuttal*: Although USEPA is correct in its concern with the biases, the calculations presented by GE attempted to account for the USGS bias by application of a correction based in USGS/NEA splits performed in 1991, and for the GE analytical biases by correcting the major peaks (5,8,14) based on 1997 extract reanalyses. When the TIP delta load plot is performed with both data sets on a tri- and higher basis (including corrections for all biases), the temporal trend is similar to that discussed in GE's original comment (Figure 4-2). High average $PCB_{3+}$ loadings from the early 1980's decrease to approximately 0.5 lb/d in the late 1980's. Loads from the early 1991 GE data are consistent with the USGS data, and the large increase in 1992 due to the Allen Mill event is evident by the increase in mean low flow loading to approximately 1.3 lb/d. Loads decrease to approximately 0.5 lb/d in the late 1990's.

USEPA discounted all the pre-1991 water column data presented in GE's analysis (due to biases), and stated that the 1991 GE data may not be very different from later years. USEPA

stated that the large loadings of 1992-1996 may not be that different from those of the 1980's, and that the impact of the Allen Mill may not be as significant as suggested by GE.

*Rebuttal*: The fact remains that the Allen Mill loading event of 1991-93 represents the single largest loading event in the river since the removal of the Fort Edward Dam. Such an event needs to be factored into the analysis of temporal trends in the data. Recovery following GE's remedial efforts at the Allen Mill is evident in the reduction of PCB loadings at both the Fort Edward and TID stations.

Response to DG-1.18

GE's comment was that dechlorination is important in reducing the carcinogenity and toxicity of PCBs. USEPA stated that toxicology issues are addressed in the ecological risk assessment (ERA) and the human health risk assessment (HHRA). In response to GE's discussion that dechlorination decreases bioaccumulation, USEPA stated that decreasing the partition coefficient increases the tendency of the PCB molecule to dissolve and subsequently migrate, thus increasing the probability for biological exposure, which "*may or may not be offset by the decrease in tendency for bioaccumulation.*"

*Rebuttal*: USEPA was not responsive to the GE comment. The fact remains that the terminal end products of dechlorination (mono- and di- chlorinated PCBs) do not accumulate appreciably in fish of the Upper Hudson River. Dechlorination impacts on bioaccumulation need to be addressed in the evaluation of the river.

Response to DG-1.19

USEPA discussed that the congener ratios used by GE are not good indicators of dechlorination because when plotted against Cl/BP, each ratio declines with decreasing Cl/BP, but the relationships have low $r^2$ values. USEPA claimed that MDPR correlates extremely well with Cl/BP and is therefore a better measure of dechlorination status.

*Rebuttal*: USEPA missed the point. A useful metric for tracking PCB sources in the river must be applied to all media, including fish. The GE congener ratios consider congeners with like physical/chemical properties typically in the tetra- to penta-PCB range and are thus applicable to fish. Since the terminal end products of dechlorination (mono- and di- PCBs) do not accumulate in fish, the MDPR is a useless metric for differentiating fish PCB sources.

GE also commented that the congeners used in MDPR are significantly affected by partitioning, and should not be used for water samples (an example of lab experiments with water in equilibrium with oil is cited). USEPA responded by stating that "*the effect of any oil extraction process on the MDPR is moot.*"

Rebuttal: USEPA missed GE's point that any partitioning (*i.e.*,sediment-water) will cause the MDPR to be altered due to differential congener properties.

Response to DG-1.26F

GE commented that chlorination may not affect carcinogenity and that USEPA's statement regarding neurological impairments may be based on flawed studies. USEPA's response was that toxicity issues will be discussed in the ERA and HHRA.

*Rebuttal*: USEPA has avoided all discussion of toxicity in its Responsiveness Summaries, yet the effects of dechlorination on the toxicological properties of PCBs were briefly discussed in section 4.3.1 of the DEIR.

Response to DG-1.26H

GE's comment was that USEPA's evaluation of dechlorination using the LRC and HRC core data in a Phase 3 report will give the Agency a chance to develop and use more sensitive dechlorination indices. USEPA's response stated that the LRCR contains an evaluation of dechlorination, and that MDPR and ΔMW were "*sufficient for the purposes of the report...other*

*indices do not appear to be as sensitive or useful in assessing the overall degree of dechlorination.*"

*Rebuttal*: USEPA dismisses the usefulness of any measure of dechlorination apart from MDPR and ΔMW. Other measures (*e.g.*, congener ratios) may be useful for the issues to be examined in Phase 3 (*e.g.*, evaluation of toxicity reduction and source identification).

Response to DG-1.26J

GE commented that USEPA's analysis of dechlorination using only the starting and ending compounds is simplistic and does not consider the fate and transport processes that affect the congener distributions in different media (*i.e.*,lighter congeners preferentially partition into water) and complicate interpretation of dechlorination using USEPA's measures (MDPR and ΔMW). USEPA's response discussed that MDPR is an estimate based on assumptions outlined in the DEIR, and the DEIR discusses results when these assumptions do not hold.

*Rebuttal*: The USEPA did not address GE's comment.

Response to DG-1.26J1

GE discussed that the use of BZ#8 as an indicator of dechlorination is problematic because it is the most abundant congener in Aroclor 1242 and its dechlorination to BZ#1 is not measurable with MDPR. USEPA maintained that use of BZ#8 was justified because it is a "*significant intermediate dechlorination product with 14% of the high resolution core samples containing more than 7.3% of BZ#8 and up to 25% by mass.*" USEPA also stated that the use of BZ#8 in MDPR improved its correlation with total PCB.

*Rebuttal*: By including one of the principal starting Aroclor congeners, it was almost predetermined that MDPR would correlate with total PCB because even non-dechlorinated samples will exhibit this correlation due to the large abundance of BZ#8 (*i.e.*,self-correlation). This weakens USEPA's argument that the correlation

of MDPR and total PCB is evidence of the concentration-dependence of dechlorination. Also, USEPA's argument is circular. The Agency used MDPR's correlation with PCB to show the concentration-dependence of dechlorination, yet MDPR was chosen as a measure of dechlorination in part because it correlated well with total PCB.

Response to DG-1.26K

GE commented that MDPR is only a measure of the last two steps of dechlorination for mixtures with 3.3 to 3.7 Cl/BP, and that it is not sensitive to dechlorination processes that attack higher homologs (*e.g.*, H and H'), which are active in the Lower Hudson. USEPA acknowledged that "*the MDPR is an underestimate of the number of affected PCB molecules*" because of this, but maintained that since MDPR is inversely proportional to Cl/BP, it is a measure of the molecules affected by dechlorination. USEPA also stated that MDPR is a more useful measure of dechlorination than GE's congener ratios because it correlates better with PCBs.

*Rebuttal*: The argument here is circular again because the relationship between MDPR and PCB was used as evidence for the concentration-dependence of dechlorination. Therefore, USEPA cannot use this relationship to defend the usability of MDPR as a measure of dechlorination. USEPA should recognize that GE's congener ratios are of significant value because, in addition to assessing the extent of dechlorination, they provide a means to fingerprint sources and to validate the overall site conceptual model.

Response to DG-1.26O

GE's comment was that USEPA's treatment of the Lower and Upper Hudson is inconsistent because the mechanisms (partitioning and degradation) attributed to low MDPR in the Lower Hudson are not considered for the Upper Hudson. USEPA's response discussed sediment PCB in the Lower Hudson may be different because "*...PCBs found in the sediments of the Lower Hudson have traveled far more miles and have descended over one hundred feet at the*

*various dam spillways of the Upper Hudson. There is then more time for re-partitioning from suspended matter into the water column and exposure to gas-exchange aerobic degradation and photo-degradation.*"

*Rebuttal*: USEPA's response is based on the assumption that *all* sediment PCBs in the Lower Hudson originated as sediments in the Hudson Falls area. This is an invalid assumption as USEPA has recognized that other sources have contributed to the Lower Hudson sediments.

Response to DG-1.20

GE's comment pertained to several dechlorination issues, including:

**The 30 ppm dechlorination threshold postulated by USEPA**: In its response, USEPA softened its conclusion regarding the 30 ppm threshold, stating that the low MDPR's for samples with less than 30 ppm PCBs do not "*imply that dechlorination has not taken place, merely that the occurrence of dechlorination is not predictable using this measure [MDPR].*" In its response, USEPA conducted several additional analyses to examine the low MDPR values for samples below 30 ppm. GE congener ratios were plotted against PCBs to show that "*the sediments typically fall well below the ratios seen in the Rogers Island water column station, suggesting that much of the alteration occurs ... during transport, most likely in aerobic conditions, representing a degradation process which is unrelated to anaerobic dechlorination.*"

*Rebuttal*: USEPA's response is confusing, but the Agency apparently has backed off its previous conclusions regarding the 30 ppm dechlorination threshold.

**Peak ratios indicate more extensive dechlorination than the MDPR**: USEPA acknowledged that the GE peak ratios suggest that a majority of Hudson River sediments have undergone some dechlorination, but that the reduction in mass and Cl/BP is minor. However, USEPA discussed that "*the correlations for these relations [are] far weaker*

*than the relationship between the MDPR and total PCB mass.*" USEPA did acknowledge the usefulness of congener ratios, but stated that they were not without limitations also.

*Rebuttal*: USEPA apparently agrees with GE that congener ratios are useful measures for evaluating dechlorination. It should also be noted that congener ratios are more useful than MDPR because they can be applied to biota data, whereas MDPR cannot because the congeners used in its calculation do not bioaccumulate to a significant extent.

**Due to the presence of higher Aroclors in the Lower Hudson, the MDPR analysis is not applicable for these samples**: USEPA responded by stating that because the Lower Hudson samples follow the same MDPR vs. PCB relationship as the Upper Hudson samples, they do belong in the analysis.

*Rebuttal*: The USEPA has not fully recognized that higher chlorinated Aroclors will adversely impact the assumptions inherent to the MDPR analysis.

**Congener ratios provide a more sensitive means of assessing dechlorination**: USEPA used High Resolution Core data to plot the GE congener ratios against Cl/BP, and concluded that the ratios are not good indicators of dechlorination because of the weak relationship with Cl/BP.

*Rebuttal*: The USEPA missed the point of GE's comment. To assess PCB sources in fish, a dechlorination index that can be used across all media, including water, sediment, and fish, such as the congener ratios used by GE are required for differentiating PCB sources to fish. The MDPR can not be used in this manner due to its reliance on low molecular weight congeners that do not accumulate in fish.

Response to DG-1.5

GE's comment was that diffusion from sediments deposited before 1991 cannot account for the TIP load. USEPA responded by stating that GE has incorrectly assumed that there is constant deposition in TIP, and that mixing, settling, and resuspension processes continually rework the sediments in TIP. The Agency referenced the LRCR, and states that the sediment mass loss evidenced in that report was consistent with water column mass loading data.

*Rebuttal*: As discussed above, the LRCR mass loss estimates were based on a flawed analysis that is not statistically robust enough to draw meaningful conclusions. Furthermore, USEPA's discussion of a combination of multiple processes is general and not backed by any supporting data. It is only through the use of a mechanistic mathematical model, which accurately captures the relative importance of each of these mechanisms that USEPA can make statements regarding the possibility of deep sediments serving as a source to the TIP water column.

Response to DG-1.6

GE's comment was that resuspension of deep sediments is implausible and inconsistent with water column loading and composition data. USEPA's response was that resuspension of deep sediments can occur.

*Rebuttal*: USEPA does not offer evidence of any means by which resuspension of deep sediments could occur. Instead it continues to rely on the (flawed) analyses in the LRCR that concluded a significant reduction in the sediment inventory occurred between 1984 and 1994. Further the Agency ignores the results of its own model which indicates that only surface sediments are subject to resuspension and that the buried PCB inventory is not mobilized to any meaningful extent.

Furthermore, USEPA stated that there are similarities in composition between deep sediments and the water column, and that slight differences and variations are the result of multiple sources contributing to the water column.

*Rebuttal*: The USEPA compositional comparison is flawed because it uses unsupported assumptions about changes in PCB partitioning as particles are mobilized off the bed (Section 2.4.7 of this document). Further, simple pattern matching does not indicate causation. Mechanistic calculations which are supported by data and constrained by mass balance principles are more appropriate. Such calculations in the USEPA model show that deep sediments do not contribute PCBs to the water column.

USEPA disputed GE's depth-of-scour analysis by stating that the areas with PCBs > 100 ppm are more widespread than depicted in the comments because those depicted have a 0-12 inch average of 100 ppm. USEPA stated that sediments over 100 ppm can probably be found throughout the pool. Also, the Agency discusses that since most of the inventory exists in the top 9 inches of the pool, scour of this magnitude would not have been detected in the bathymetry data.

*Rebuttal*: USEPA's arguments are not based on data, and are also not constrained by mass balance. The Agency states that there are probably more areas with 100 ppm, and that all of the mass is in the surface. It does not present calculations that support its theory that sediment inventory of this type could have accounted for the observed water column loadings. Had the Agency conducted such an analysis, it would most likely have come up with an unrealistically large scour depth required to balance the water column mass loading. Even if all of the areas of cohesive sediments in TIP (an area of 440,000 $m^2$) contributed to the hypothesized scour at an *average* surface concentration of 100 ppm, the average depth required to yield the water column loading would be on the order of 10 cm - this is inconsistent with $^{137}Cs$ and $^{7}Be$ data from USEPA's high resolution sediment cores.

Response to DG-1.7

USEPA generally agreed with the GE comment that low flow resuspension is not supported by sediment transport theory, and responded by saying it is examining the issue through modeling. USEPA offered alternative explanations that support its theory: hydrodynamic resuspension may only account for part of the load, and biological mixing and boat traffic may also be responsible for resuspension (it cites a lack of flow dependence on the TIP load to suggest that these other mechanisms may be primarily responsible).

*Rebuttal*: As USEPA first stated, the best way to look at this issue is through a quantitative model that is constrained by mass balance. In the absence of such an analysis, the USEPA is merely speculating on possible mechanisms.

Response to DG-1.8

In its comments, GE compared the TID congener composition to surface and deep sediment compositions from the TIP Hudson River cores to conclude that the TIP source is more consistent with unaltered 1242. USEPA's response was that (1) the use of a log scale places undue importance on congeners with low PCB concentrations, (2) the Agency is not clear about how the average "deep, dechlorinated" composition was determined, and (3) the Agency also discusses that the two most prevalent congeners are consistently higher in the water than in the surface sediments.

*Rebuttal*: Mechanistic analyses based upon partitioning calculations have demonstrated that the water column load is consistent with surface sediment sources, and not the deep sediments containing highly dechlorinated PCBs (Figures 2-4 and 2-5).

# SECTION 5
# REVIEW OF SPECIFIC COMMENT RESPONSES PERTAINING TO THE LOW RESOLUTION CORING REPORT

This section contains GE's rebuttal to the specific comment responses in USEPA's LRCR Responsiveness Summary (USEPA, 1999a). Each response is keyed to the numbering scheme developed in USEPA, 1999a. Responses are addressed in the order in which they appear in USEPA, 1999a. For indexing each specific response, a lookup table is provided in Table A-2 of Appendix A, which contains:

1) the section of the LRCR that GE's comment addressed,
2) the page of GE's comment document (GE, 1998) on which the comment appears, and
3) the page of USEPA's Responsiveness Summary (USEPA, 1999a) on which the response appears.

## 5.1 GENERAL COMMENTS ON USEPA'S LRCR RESPONSIVENESS SUMMARY

In 1994, USEPA conducted a study aimed at determining the change in PCB inventory within Upper Hudson River sediments since the 1984 New York State Department of Environmental Conservation survey. This effort and associated data analysis were presented in the LRCR. In summary, rather than conduct an extensive survey, the USEPA attempted to occupy what turned out to be an unrepresentative subset of the same stations sampled in 1984 and used the apparent change in PCB inventory calculated from these "*paired*" samples to estimate PCB inventory changes over the entire river. While such an approach at first appears to be a creative means of avoiding the tremendous effort of completely resampling sediments at the spatial resolution of the earlier survey, it is fraught with uncertainty: uncertainty associated with locating samples both in 1984 and 1994, uncertainty associated with the PCB analyses both in 1984 and 1994, and uncertainty associated with extrapolating the results obtained from limited regions of the river to the entire river.

GE (1998) submitted extensive comments critiquing USEPA's analyses and conclusions presented in the LRCR. The Agency's responses to these comments are contained in its LRCR Responsiveness Summary (USEPA 1999a). The primary concern with USEPA's LRC analysis and the Agency's response to GE's comments is the Agency's apparent unwillingness to recognize the tremendous uncertainty inherent in the apparent mass loss estimates. This was also a major concern of USEPA's own Hudson River Science and Technical Committee when it met in the summer of 1998 to review USEPA's report. Also, the USEPA's peer review panel noted that "*estimated amounts of PCB loss should be interpreted with caution due to the uncertainty inherent in comparing sediment cores collected in different years*" (Eastern Research Group, 1999, p. 3-3). Furthermore, Dr. Paul Switzer, an environmental statistician from Stanford University and a member of USEPA's National Advisory Council on Environmental Policy and Technology, articulated it best in his comments on USEPA's analysis of the Low Resolution Sediment Coring data (GE, 1998):

> *"Estimates of PCB inventory, inventory changes, and dechlorination losses are not accompanied by statistical estimates of uncertainty. Good statistical practice dictates the quantitation of uncertainty - first as a measure of the adequacy of the underlying information and second as a tool in rational decision making. The uncertainty associated with inventory estimates derives from sampling variability, measurement error, and spatial interpolation errors, all of which propagate to the final inventory estimates."*

In his comments on the LRCR Responsiveness Summary, Dr. Switzer describes the USEPA's response to his earlier comments as "*disappointing*" (Dr. Switzer's complete comments are provided in Appendix B of this document). In general he felt that some responses to comments involving statistical concepts were "*inarticulate and meaningless as understood by statisticians, suggesting that responsibility for replying to [my] earlier questions and criticisms may not have been entrusted to professional statisticians.*" While USEPA recognized that some of Dr. Switzer's earlier criticisms were valid, it either presented additional information or simply disregarded them as having no practical importance in support of its original conclusions. Most importantly, he warns against the use of "*...'geochemical knowledge'...to justify unwise statistical procedures, particularly in the design of the 1994 survey.*"

The USEPA has not to date, neither in its original report nor in its Responsiveness Summary, adequately quantified and reported the inherent uncertainties in its estimates of inventory change between 1984 and 1994. At best, the USEPA approach could allow one to hypothesize that PCB loss from the sediments within the Upper Hudson River has occurred since 1984. The amount of the loss and the locations from which the loss occurred cannot be made with any reasonable certainty using the approach chosen by USEPA. Further, the LRCR methodology does not provide any basis for making reasoned and reliable predictions about what might occur in the future and thus is of limited utility for remedial decision making.

## 5.2 REVIEW OF SPECIFIC COMMENT RESPONSES PERTAINING TO THE LRCR

Response to LG-1.9

GE's comment arguing that the same sediment was not sampled in 1984 and 1994 contained several points:

- **Due to a lack of correlation between 1984 and 1994 data within zones, the change in mass is random, and most likely an artifact of small-scale spatial variability.** USEPA's response cited the updated area-based mass loss analysis to discuss that most pairs show mass loss both on an individual and zone basis.

*Rebuttal*: USEPA cited its original analyses (*i.e.*,LRCR Figure 4-5), which are flawed in that they include cores separated by more than 5 feet (which are affected by spatial heterogeneity). As shown in Figure 4 of GE's comment document, when the $PCB_{3+}$ comparison is made for samples separated by less than 5 feet, the slope of a log-log plot of 1994 data against 1984 data is not statistically different from zero.

- **GE's LRCR comment document presented a variogram analysis based on data from the H-7 area which indicates that even if spatial correlation does exist at scales of several hundred feet within the TIP, at the scale important to the LRCR analyses, PCBs are only correlated over distances of less than 10 feet.**

USEPA discussed its DEIR variogram analyses, which the Agency claims indicates that the northern portion of TIP (which includes the H-7 area) is not expected to exhibit spatial correlation, while the southern portion is spatially correlated.

*Rebuttal*: It is not valid to use the DEIR variograms to assess short-scale spatial correlation (*i.e.*,at a range of less than 10 feet) because correlation at greater distances does not preclude lack of correlation at closer distances. The data from the H-7 variogram analysis presented in GE's LRCR comments remain the only data set in the TIP with enough samples to generate a dependable variogram for short separation distances.

In this discussion of small-scale spatial correlation of sediment PCBs within the TIP, the USEPA presented a new variogram analysis in its responsiveness summary. This analysis was based on the 1984 "clustered" samples, which consist of 321 sample pairs that are separated by less than 30 feet. From its new analysis, the Agency concluded that the log of PCB mass per unit area (MPA) is correlated at distances of 4 feet (correlation coefficient, r = 0.70) and 10 feet (r = 0.47). The Agency uses this analysis to contrast the results of GE's H-7 analyses, and conclude that 1984 to 1994 comparisons made within 10 feet are appropriate since they are within the purported correlation range.

*Rebuttal*: There are several issues that weaken USEPA's new variogram analysis based on the closely-spaced 1984 data:

- As discussed by Dr. Switzer in his response to LG-1.37, the correlation coefficients cited by USEPA "*should be regarded as low rather than high*" and should have been reported for the data rather than for the fit of a Gaussian variogram to the data (see Appendix B). The fact that the correlation coefficient significantly decreases for this small increase in separation distance (*i.e.*,from 4 to 10 feet) supports GE's original conclusion in the LRCR comments: while sample pairs separated by less than 5 feet are

correlated, the correlation is much weaker at separation distances beyond 5 feet.

- The Agency states that from the 1984 data set within TIP, there are 321 unique sample pairs that are separated by a distance of less than 30 ft. Based on the sample coordinates in version 4.1 of the USEPA database, this appears to be correct. However, only 123 of these pairs have Aroclor PCB data to support calculation of a reliable MPA (presumably, the Agency included the highly uncertain mass spectrometry results for the remaining samples). Further, the spatial coverage of these 123 sample pairs is not robust enough to perform a variogram analysis. While the spatial coverage is adequate in some areas, the distance between the clusters is too great to extrapolate a variogram over the entire TIP (Figure 5-1). Therefore, the robustness of the data set for USEPA's closely-spaced 1984 variogram analysis is over-implied in the Agency's response.

- In addition, 26 of the 123 sample pairs available for this variogram analysis are core to grab comparisons. These comparisons weaken the variogram analysis due to the uncertainty in grab depth, as discussed in GE's LRCR comments.

- USEPA describes fitting a Gaussian variogram model to the 1984 data; however, a plot is not provided to assess the validity of this fit.

- As an alternative to evaluating correlation within the 1984 closely-spaced data with a variogram, a plot of the absolute relative percent difference (ARPD) between samples within the closely-spaced pairs is shown in Figure 5-2. This figure illustrates the spatial trend in the ARPD between these pairs, for different separation distances. For all sample pairs separated by a distance of up to 30 feet (top panel), the figure suggests that spatial heterogeneity is slightly greater in the northern portion of the TIP (*i.e.*,river mile > 192).

However, overall this difference does not appear to be statistically different from the rest of the river. Most importantly, the across all locations and for separation distances of 30, 10, and 5 feet, the mean ARPD is over 50%; this suggests that based on the 1984 data alone, spatial variability can result in a mass difference of 50%. This variability adds significant uncertainty to USEPA's comparisons of 1994 data with the 1984 data (which form the basis of the conclusions in the LRCR).

Response to LG-1.29

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.30

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.21

GE used intra-homolog congener ratio analyses to demonstrate that PCB composition in fish was similar to that in the surface sediments. USEPA referred to its responses to DEIR comments (DG-1.19 and DG-1.20), and reiterated its belief that the GE congener ratios are not good predictors of dechlorination due to their large variability (plots of non-binned Upper Hudson River Phase 2 sediment data are provided). USEPA also discussed that the 1993 fish data may not be representative due to the large water column loadings of that period.

*Rebuttal*: To assess PCB sources in fish, a dechlorination index that can be used across all media, including water, sediment, and fish, such as the congener ratios used by GE are required for differentiating PCB sources to fish. The Agency's measure of dechlorination (MDPR) cannot be used in this manner due to its reliance on low molecular weight congeners that do not accumulate in fish.

Response to LG-1.32

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.5C

GE commented that if erosional processes were occurring such that 40% of the TIP sediment PCB mass was lost, the water column PCB composition would be consistent with deep dechlorinated sediments, and not with surface sediments. USEPA's response discussed that consistency in water column PCB congener patterns from 1991-98 suggests "*that there has been no substantive change in the nature of the PCBs responsible for the Pool load gain.*"

*Rebuttal*: USEPA's response re-emphasizes GE's point. If scour had been occurring at the magnitude implied by USEPA's conclusions in the LRCR, then the deeper sediment sources would have to be contributing to current water column PCB loadings. If this was the case, measured water column congener patterns would not be consistent with surface sediment sources. However, both GE and USEPA have shown that water column-derived PCBs in TIP are indeed consistent with surface sediment sources (see Section 2.4 of this document, Figures 2-4 and 2-5, and Butcher et al. 1998).

Response to LG-1.27

GE explained that USEPA's conclusion regarding a 30 ppm dechlorination threshold is flawed due to large uncertainties. USEPA responded that samples with low PCB also had high Cl/BP and low ΔMW, indicating that dechlorination was not a significant mass loss mechanism for these samples.

*Rebuttal*: USEPA's response regarding the 30 ppm dechlorination threshold is inconsistent with others within the Responsiveness Summaries. In some discussions, USEPA

accepts that dechlorination does occur at concentrations below 30 ppm, albeit at slower rates. In addition, USEPA's March 1999 LRCR Peer Review Panel stated that USEPA's conclusion regarding the threshold was incorrect (Eastern Research Group, 1999).

Response to LG-1.31

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.23

GE presented an analysis of $^{137}Cs$ levels in the LRC surface sediments, which indicated that the levels in the top inch from the TIP cores USEPA identified as exhibiting scour were consistent with post-1975 levels, as determined from dating of the TIP High Resolution Cores. USEPA responded that due to variability in deposition rates, deposition history, and $^{137}Cs$ levels within TIP, GE's analysis cannot be used to develop average $^{137}Cs$ levels indicative of a specific time period.

*Rebuttal*: The variabilities within TIP cited by USEPA are not as great as implied. Although there is variability in deposition across the pool, the LR cores USEPA referred to as having "*evidence of sediment scour*" were all located in near-shore quiescent regions of the river, which are depositional (hence the presence of the "hot spots"). Calculated mean deposition rates for the four High Resolution Cores in TIP averaged 1 cm/yr, with a range of 0.8 to 1.2 cm/yr. In addition, the variability in $^{137}Cs$ concentrations within a given period of time has already been considered in the analyses through the construction of 95% confidence intervals. As stated in the original comment, if the LRC areas were indicative of gross "hot spot" scour, then at least some of the LRC surface $^{137}Cs$ levels would be within the 95% confidence interval of the $^{137}Cs$ levels associated with the deeper, highly contaminated sediments in the High Resolution Cores. However, they were not.

Response to LG-1.37

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.1

GE discussed that the LRC program could not be used to assess inventory change because the uncertainty in the 1984 inventory estimates was large. In response, USEPA discussed that the Brown *et al.* (1988) inventory estimate was biased, and that an updated inventory estimate presented in Appendix B to the LRCR Responsiveness Summary yielded results similar to the DEIR kriging analysis.

*Rebuttal*: USEPA thus implies that the 1984 inventory is known with much more certainty than GE discussed in its comment. The Agency's updated inventory estimate for 1984, was consistent with the previous estimates for $PCB_{3+}$, but acknowledged that *"the total PCB inventory is less well known and, in fact, may be underestimated by a large percentage"* (page B-2 of the LRCR Responsiveness Summary).

Response to LG-1.38A

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.2

GE commented on the credibility of using the matched 1984-1994 cores, stating that five specific conditions, which were required to produce meaningful results, were not met:

**1. USEPA's analysis of LRC field splits indicates PCB concentration measurements had significant errors.** In response, USEPA claimed that as long as each measurement

represents an unbiased estimate of PCBs, then the average difference between the two measurements will not be affected, and the results are still valid for determining the direction of the mean change.

*Rebuttal*: USEPA's response is correct. However, the uncertainties in each of the PCB measurements were not accounted for in the calculation of the uncertainty of the mean difference. Had USEPA properly combined the uncertainty due to analytical variability with the variability of the differences between the 1984 and 1994 core results, the uncertainty about the mean difference would be much greater.

**2. Approximately 20% of the LR cores did not capture the full vertical extent of PCB contamination.** USEPA discussed that since most LR cores were complete or nearly complete based on $^{137}Cs$ and PCB profiles, the data set was adequate for assessing inventory change.

*Rebuttal*: Regardless of whether a "*vast majority*" of the TIP inventory was captured in the 1994 cores, the point-by-point technique used by USEPA to estimate mass loss analysis should not have included incomplete cores because these are biased towards high mass loss.

**3. Due to poor depth control for the 1984 grab samples and the large vertical gradients in surface sediment PCBs, comparisons of 1994 cores with 1984 grabs is invalid.** In response, USEPA cited "*statistical tests*" in the LRCR, which showed mass loss in grab-core pairs to be statistically equivalent to that in core-core pairs.

*Rebuttal*: The point of the comment is that mass loss cannot be inferred from samples in which the core profile is not derived from data. USEPA did not account for the uncertainty associated with grab depth in the LRCR analyses. In its response to LG-1.36, USEPA claimed that this error was not likely to affect the main conclusion of the report, but this is purely speculative.

**4. Due to the separation distances of the 1984-1994 core pairs and spatial heterogeneity, as evidenced by the 1990 H-7 data, the same sediments were not sampled in 1984 and 1994.** USEPA cited the DEIR variogram analyses, which show that the heterogeneity in the upper portion of the TIP (containing the H-7 area) is much higher than in the southern portion of TIP, where most of the LR cores were collected.

*Rebuttal*: It is not valid to use the DEIR variograms to assess short-scale (*i.e.*, a range of less than 10 feet) spatial correlation because correlation at greater distances (on the scale of several hundred feet) does not preclude lack of correlation at closer distances. The data from the H-7 variogram analysis presented in GE's LRCR comments remains the only data set in the TIP to with enough samples to generate a dependable variogram for short separation distances (see also Review of LG-1.9).

**5. Due to lower areal sampling density in 1994, extrapolation of results to the entire TIP magnifies the already large uncertainties.** USEPA discussed that the LRC program was not aimed at establishing a new inventory estimate, but only at evaluating the change from the 1984 estimates. In addition, USEPA discussed that since the 1994 samples were collected in 16 discrete clusters, which were locally at the same density as the 1984 samples, the TIP was well represented for the purposes of the LRCR calculations.

*Rebuttal*: USEPA's response implies that the locations resampled in 1994 were representative of the 1984 population. This is not true, since GE's original comments pointed out that the LRC locations were biased towards the 1984 samples with high concentrations. In addition, as discussed in Dr. Switzer's review of Response LG-1.29, the sites USEPA chose for resampling were contrary to conventional geostatistical design.

Response to LG-1.3

GE commented that the results from the LRCR analyses are implausible because: 1) the 1984-1994 data sets are not adequate for assessing inventory changes, 2) the results are not reproducible by different methods of equal reliability, and 3) the results cannot be attributed to plausible fate and transport mechanisms. USEPA responded that the 1984-1994 data sets are sufficient since they have undergone "*statistical tests which account for the uncertainties in the data.*"

*Rebuttal*: Throughout the LRCR and the Responsiveness Summary, USEPA relies heavily on "*statistical tests.*" As Dr. Switzer discussed in his review of the Responsiveness Summary "*some responses that invoke statistical concepts are inarticulate and meaningless as understood by statisticians,*" which renders USEPA's use of statistics highly suspect.

USEPA also discussed that analytical measurements were the most accurate at the time and that while knowledge of the mechanisms responsible for the mass loss would be useful, it is not required for the results to be valid. USEPA added that "*it is essential to integrate the net change in sediment inventory caused by the assortment of mechanisms so that the nature and scale of these mechanisms can be constrained.*"

*Rebuttal*: USEPA missed GE's point on the second part of this comment. GE was referring to the fact that the results from the mass loss estimate were not supported when a different method was used to estimate inventory change from 1984 to 1994 (*i.e.*, calculating the mass transported past TID from the water column data). Also, USEPA's discussion of mass loss mechanisms is puzzling. The Agency takes the position that the magnitude of mass loss from various mechanisms should be measured against the LRCR results to see how well they can be understood. USEPA's approach favors empirical results over mechanistic results, and therefore is not constrained by the sound scientific principles such as mass conservation. USEPA's last statement suggests that the results from the modeling

(mechanistic) will be constrained by the LRCR mass loss estimates (empirical); this is absurd.

Response to LG-1.4D

GE commented that the 80% mass loss estimated from the proper application of USEPA's method is not supported by that derived from the best estimates of the combination of dechlorination, diffusion, scour, and groundwater advection. In response, USEPA stated that the purpose of the LRCR was not to evaluate mechanisms, but for evaluation of inventory changes in TIP and "hot spots" below TID. USEPA cites its critique of the GE TIP Report, and states that GE/QEA's mechanistic "assumptions" are of "*questionable validity*."

*Rebuttal*: USEPA did not offer any data or calculations to back up its assertion that our mechanistic assumptions are of "*questionable validity*." The mechanistic calculations prepared by GE were not simply repeated from the GE TIP Report. Rather, the estimates were a combination of updated analyses (groundwater advection and diffusion) and new analyses (*i.e.*,scour and dechlorination), which provided bounding calculations aimed at estimating the maximum possible mass loss from 1984 to 1994.

Response to LG-1.8

GE commented that 1984 grab samples could not be compared with 1994 core samples due to uncertainties in the 1984 sample depth and large vertical concentration gradients. The response from USEPA stated that NYSDEC assessed the assignment of grab depths based on paired samples, and that statistical tests showed that core-core and grab-core pairs resulted in statistically equivalent mass loss from 1984 to 1994.

*Rebuttal*: The fact that the grab-core results are similar to the core-core results cannot be used to conclude that grab-core comparisons are appropriate. The only way to properly include grab-core comparisons in the analyses would have been to

quantify the uncertainty in grab depth and carry it through in the 1984 MPA calculation such that it was incorporated into the overall uncertainty.

Response to LG-1.10

GE's comment discussed that extrapolation of 1984-1994 mass changes from the LRC sampling locations to the entire TIP is not appropriate due to spatial heterogeneity, uncertainty in the 1984 inventory, and the fact that the subset of locations resampled is not representative of the entire population of 1984 data (*i.e.*, they were clustered in areas of higher concentrations). In response, USEPA stated that although the LRC data were insufficient to yield an estimate of the TIP inventory, they were appropriate for estimating inventory change in the higher concentration areas. The Agency claimed that LRC locations were chosen in areas of low spatial variability (based on 1984 MPA). The Agency also stated that it focused on high concentration areas because they are most likely to release PCBs to the water column and accurate quantitation was more likely because detection limit issues could be avoided. USEPA added that GE's comment regarding biased sample selection within "hot spots" was incorrect because the 1984 data suggested that the 1976-78 "hot spot" delineations were not accurate. USEPA discussed that its mass loss estimates were thus appropriate since they were applied to fine-grained sediments (> 10 $g/m^2$ MPA), and that mass gain may have occurred in the coarse sediments within TIP.

*Rebuttal*: The high bias in the 1984 sites selected by USEPA for resampling cannot be justified because of the "*regression fallacy*" discussed in Dr. Switzer's comments on the LRCR (*i.e.*, in the absence of any inventory change, resampling the sites with higher concentrations will result in lower concentrations and vice-versa).

Response to LG-1.20

GE presented an analysis that indicated the PCB composition in the TIP water column is consistent with pore water exchange from surface sediments, and not deep, dechlorinated sediments. USEPA cited its critique of the GE TIP Report, which discussed that the TIP water column composition is dechlorinated relative to Aroclor 1242, but that the composition is not consistent with pore water exchange as the only loading mechanism. USEPA also claimed that

GE's use of the 1991 pore water data adds significant uncertainties due to the effects of freezing on the measurement DOC.

*Rebuttal*: See the response to USEPA's comments on the GE TIP Report in Section 2. USEPA disputes the pore water exchange mechanism, but developed analyses that suggested a combination of pore water and bulk sediment resuspension could account for the TIP water column congener pattern. Despite the problems with USEPA's analysis (discussed in Section 2), it shows that the low flow TIP load is consistent with a surface sediment source. This was the point of GE's comment. If there was an inventory loss of 40% in TIP sediments, the surficial layer would contain material from the deeper deposits, which exhibit high concentrations and highly dechlorinated congener patterns. However, this is not the case. The water column composition is consistent with the data from surface sediments (both 1992 High Resolution Core data and 1998 surface sediment data). In addition, the Agency treated the 1991 pore water data inconsistently. The first paragraph of this response (p. LRC-38), discussed the Agency's new analysis, which directly used the 1991 pore water PCB data, while the second paragraph stated that the 1991 pore water data are problematic because they contain "*both variability and bias*" due to freezing and compositing.

Response to LG-1.2A

GE reiterated its concerns with the LRC sampling design, citing its 1992 comments on the work plan, which discussed the problems with extrapolating LRC results to the entire TIP due to heterogeneity, as evidenced by the H-7 data. USEPA repeated its contentions from earlier responses, stating that the LRC samples were locally homogeneous, that the H-7 heterogeneity is not representative of the TIP as a whole, and the results are significant because they withstood "*statistical tests*."

*Rebuttal*: Based on Dr. Switzer's review of the Responsiveness Summary, the USEPA's "*statistical tests*" should be viewed with great skepticism. As discussed in the

Response to LG-1.9, the 1984 closely-spaced data indicate that spatial heterogeneity at small scales is not insignificant. Further, the USEPA failed to properly consider the uncertainty associated with the calculations of 1984 and 1994 TIP PCB inventories. This is discussed in further detail in section 6.2 of this document.

Response to LG-1.4A

In GE's comment, it was discussed that the magnitude of mass loss in TIP sediments estimated in the LRCR (40%) was "*startling*." In its response, USEPA discussed that the LRCR was aimed at determining whether loss or gain was occurring and whether the change was statistically significant. Since the LRCR showed statistically significant mass loss, USEPA disputes GE's claims regarding burial.

*Rebuttal*: USEPA has softened its position in the response, and rather than discussing the magnitude of mass loss, just states that its presence is statistically significant (However, the 40% number is still cited publicly). The quantification of this loss is necessary, and had the analysis with its associated uncertainty been performed properly (Section 6.2), the results would have rendered the analysis useless for any meaningful conclusions about changes in TIP PCB inventory.

Response to LG-1.4B

GE commented that USEPA's mass loss analysis should have been performed using $PCB_{3+}$ from both data sets (rather than with $PCB_{3+}$ for 1984 and total PCB for 1994), and when this calculation is performed properly, the mass loss estimate is approximately 80%, which is implausible. USEPA's response discussed that comparisons were made with several parameters (*e.g.*, total, $PCB_{3+}$, dechlorination products...), and that the purpose was to show that statistically significant inventory loss was concluded regardless of which comparison was made. USEPA discussed that comparing 1994 $PCB_{3+}$ plus dechlorination products with 1984 data was used to give a lower bound mass loss estimate.

*Rebuttal*: USEPA does not directly address the point. The only way to evaluate mass loss is to compare the same measurements in 1984 and 1994. All analyses (USEPA and GE) have shown that the 1984 data represent $PCB_{3+}$, and, as such, the mass loss estimates should have been based on 1994 $PCB_{3+}$ only. Comparisons other than this have no meaning. USEPA did not present any data to refute the comment that the proper mass loss calculation (*i.e.*,on a $PCB_{3+}$ basis) results in an unrealistic mass loss estimate (80%) suggesting that the technique is flawed.

USEPA added a paragraph discussing that the J. Butcher analyses (LRCR Appendix E) regarding the actual PCB mass measured in the 1984 method was "*based on GE's attempt to reproduce the NYSDEC 1984 technique and as such does not represent proof that the interpretation is correct.*"

*Rebuttal*: USEPA's statement is incorrect. Butcher's analyses were based upon numerical experiments with Phase 2 sediment data. This consisted of "*interpreting the [Phase 2] congener data 'as if' they had been analyzed by the packed column methods used by NYSDEC and comparing the results to the actual sum of congeners*" (page E-2). Butcher concluded that the PCB quantified in 1984 was approximately $PCB_{3+}$. In commenting on the LRCR, GE presented additional analyses in which 1998 sediment samples were analyzed by laboratory methods aimed at reproducing the 1984 technique. These results were consistent with Butcher's assessment: the 1984 quantitation is consistent with the $PCB_{3+}$ homolog sum.

Response to LG-1.4C

GE's comment was that the estimated mass loss using USEPA's method (with $PCB_{3+}$) of 10.8 MT is unrealistic. USEPA's response stated that the estimate is actually lower because its analysis was only for the fine-grained sediments. The Agency claims that not all of the mass loss was transported downstream since there may have been some inventory increase in coarse regions. USEPA also discussed the role of dechlorination in the mass loss estimate, pointing out

that GE's assumption was that all of the dechlorination occurred prior to 1984 (upper bound on mass loss), while USEPA's analysis assumed all of the dechlorination occurred between 1984 and 1994 (lower bound mass loss). USEPA then stated it believes that most of the dechlorination occurs immediately after deposition, citing McNulty (1997) to discuss that dechlorination continues to occur at a slow rate thereafter.

*Rebuttal*: USEPA's response is internally inconsistent. The Agency states that "*the LRC assumed no dechlorination occurs prior to 1984,*" but then states that it believes that "*dechlorination ceases a few years after deposition*" (p. LRC-47). Presumably, most of the PCBs in TIP were deposited following the Fort Edward Dam removal, subsequent high flow events, and high loading periods of the 1970's. Therefore, most of the dechlorination is likely to have occurred prior to 1984.

Response to LG-1.4E

GE discussed that the mass loss predicted to occur from 1984 to 1994 by USEPA's method (80% when done properly) was not consistent with present conditions (*i.e.*,1998 data indicate a mass gain using the same calculation technique, and if the 1984-1994 mass loss continued through present, the TIP inventory would be exhausted); this suggests the analysis is flawed. USEPA responded by stating that the 80% mass loss does not represent the Agency's estimate of mass loss (implying it is incorrect), and that the data set presented by GE (12 cores from the 1998 FS program) "*is probably too small to make the comparison concerning the rate of mass loss over time between 1994 and 1998.*"

*Rebuttal*: USEPA's response is the same argument GE used in discussing the inadequacies of using the LRC data to extrapolate mass changes from 1984 to 1994. On a simple ratio basis, the 1998-1994 resampling density is quite comparable to the 1994-1984 density. In 1998 GE resampled 16 of USEPA's 75 LRC sites in TIP (21%), while the LRC program sampled 25% of the 1984 data set (75 out of the 298 sites having PCB data). USEPA cannot have it both ways; either there are

not sufficient data to use the LRCR methodology (as GE claims) or there are, and if so, the Agency must accept the results from the 1998 GE sediment data.

USEPA also cited GE's comment Figure 13, which shows a temporal profile of 1984-1994-1998 MPA in "hot spots," and stated that the 1998 GE data also support USEPA's finding that a substantial mass loss occurred in fine-grained sediments since 1984.

*Rebuttal*: It should be noted that in the original figure, there is overlap in the error bars (95% confidence) for the 1984 and 1998 mass. This suggests that mass loss over this period may not be statistically significant, and also that the uncertainties and variabilities in the analysis are great. Also, USEPA's logic in this response does not address GE's main point. If a large mass loss occurred over 1984-1994, there is no reason why such a change would not have continued over 1994-1998. Instead, the data shown in the original figure suggest a mass gain over this period. This result indicates that the method used to evaluate mass change is too sensitive to the many uncertainties to produce reliable results.

Response to LG-1.5E

GE reiterated its comments that an 80% $PCB_{3+}$ mass loss is not supported by current water column and sediment data or known fate and transport mechanisms. In response, USEPA claims that GE's mechanistic calculations are uncertain, and that several mechanisms (not solely erosion) are likely responsible for the "*measured mass loss.*"

*Rebuttal*: The point is that USEPA did not take a mechanistic approach, constrained by mass balance, in calculating the TIP mass change. This is an important constraint on calculating mass changes, which must be considered. These types of calculations are best made within an integrated modeling framework, which is constrained by mass balance for water, sediments, and PCBs. The USEPA should have deferred its inventory calculations to the BMR, which represents a more appropriate tool than "*statistical tests*" for estimating the 1984-1994 inventory

change. Indeed, the RBMR shows a vastly different story on inventory change from the LRCR (see Section 1 of this document).

Response to LG-1.11

GE's comment discussed the evaluation of the 1984 PCB quantitation, citing both USEPA's numerical analyses and GE's laboratory analyses, which both indicated the 1984 method quantitated $PCB_{3+}$. In addition, GE discussed that the literature suggested the effects of dechlorination over 1984-1994 on $PCB_{3+}$ mass loss were likely to be minor, therefore indicating that the only proper comparison with 1984 data is 1994 $PCB_{3+}$. In its response, USEPA explained its reasons for making several different comparisons, and agreed with GE that $PCB_{3+}$ is the most proper comparison of 1984-1994 data and that the direction of change (*i.e.*,mass loss) was consistent across all measures.

*Rebuttal*: Although USEPA agrees with GE on the most proper comparison, the Agency did not use it for the analysis upon which it based the primary conclusion of the report (40% mass loss in TIP). USEPA does not address the point that $PCB_{3+}$ in the only proper comparison for 1984-1994, and if USEPA had done this comparison, it would have come to the same conclusion as GE: the resulting estimate of 80% mass loss is so implausible and indicates that the method used in the LRCR to estimate change is highly uncertain and is of no use in evaluating PCB mass change in the sediments.

Response to LG-1.13

GE commented that USEPA's mass loss estimate was biased by removal of 1984 samples with MPA<10 $g/m^2$ since many of these samples were located within "hot spots." USEPA responded that the purpose of the LRCR was to compare 1994 values relative to 1984, and not to confirm the 1976 "hot spot" delineations. The Agency claimed that even with a 5 $g/m^2$ MPA cutoff, fine-grained loss and coarse-grained gain is calculated.

*Rebuttal*: By selecting fine-grained sediment based on PCB concentrations, USEPA's analyses are subject to the regression-towards-the-mean fallacy discussed in Dr. Switzer's comment (*i.e.*,resampling a population will result in decreases for high values and increases in low values). USEPA should have used a non-PCB measure (*e.g.*, sediment texture) to differentiate fine and coarse sediments.

Response to LG-1.14

GE detailed the proper 1984-1994 mass loss calculation, which indicated an unrealistically large 80% loss of 10.8 MT. USEPA responded that since GE assumed no dechlorination occurred between 1984 and 1994, the mass loss is an upper bound. USEPA then explained that the area-based analysis still resulted in a 43% $PCB_{3+}$ mass loss for fine-grained sediments and that the total loss would be about 3.5 MT, which is much lower than what GE contended to be implausible.

*Rebuttal*: The magnitude of USEPA's new estimate of the total mass loss is more realistic and comparable to water column data. However, because the revised estimate of $PCB_{3+}$ mass loss over 1984-1994 (43%) is very different from the $PCB_{3+}$ mass loss predicted by the original LRCR point-to-point method (80% when done properly), it cannot be used to support the original LRCR conclusions, and in fact, it indicates that the LRCR data set is inadequate to accurately quantify inventory change. Further comments and an evaluation of the Agency's new area-based analysis are provided in Section 6. A major flaw in this analysis is the lack of proper calculation of uncertainty. This calculation estimates uncertainty for the total mass loss based on the variability of average PCB mass amongst different areas within TIP, but fails to include the variability of mass *within* these areas.

Response to LG-1.15

GE used mechanistic calculations (dechlorination, diffusion, scour, and groundwater advection) to estimate a maximum possible 1984-1994 mass loss of approximately 18% of USEPA's estimated 14.5 MT TIP inventory. USEPA responded that the mechanistic calculations

cannot be used to disprove the LRCR results due to uncertainties. The Agency claimed that the 10% estimate based on McNulty, 1997 is much too high, "*the processes of diffusion and groundwater transport are not well documented in the Upper Hudson,*" and that bioturbation should have been included. USEPA concluded that the discrepancies in mass loss may be because GE's model "*does not accurately depict the system.*"

*Rebuttal*: USEPA fails to recognize and acknowledge that the constraints imposed on the model (*i.e.*,consistency with scientific understanding of PCB fate mechanisms; conformance to mass balance principles; ability to replicate conditions in water, sediment and biota) are absent from LRCR data analysis. This lack of constraints coupled with uncertainty that one of the USEPA peer reviewers characterized as "large enough to drive a truck through" make the analysis vastly inferior to the modeling.

Response to LG-1.16

GE commented that analysis of water column-derived loadings from 1984-1994 are not consistent with USEPA's mass loss estimate. USEPA's response claimed that the updated mass loss and TIP inventory estimates result in a lower estimate of 3.5MT for the 1984-1994 inventory loss, and that this value corresponds to about 2 lb/d from 1984-1994, which is not inconsistent with the water column loading data.

*Rebuttal*: USEPA's comparison is invalid because it compares numbers that reflect different things. The 2 lb/d that would have to be coming from the high concentration sediments only, and must be compared to the fraction of the water column loading data attributable to these sediments. As the Agency's model shows, only a small fraction of the water column loading is attributable these sediments. If USEPA had made the apples-to-apples comparison (comparing water column loading from "hot spot" sediments with the mass loss from these same sediments, as estimated in the LRCR) it would show that the 2 lb/d number vastly overestimates the observed water column loading attributable to the LRCR sediments.

Response to LG-1.17

GE's comment was that the 1998 data do not indicate mass loss since 1994, which should have been observed if the 1984-1994 trend was occurring. USEPA responded that an exponential rather than linear decline is more likely and that it could not comment on the GE data set because it was not available at the time the LRCR Responsiveness Summary was prepared.

*Rebuttal*: Even if the mass loss trend was exponential, a 40% loss from 1984 to 1994 would extrapolate to an 18% loss from 1994 to 1998. However, as GE's comment indicated, the LRCR method predicts a large mass gain from 1994 to 1998. The 1998 GE sediment field sampling plan was submitted to USEPA in June, 1998 (8 months before the release of the LRCR Responsiveness Summary). The original 12 Focused Sediment cores were collected from June 17-25, 1998, and data from these were sent to the USEPA in the July 28, 1998 release of GE's database (6 months before the release of the LRCR Responsiveness Summary).

Response to LG-1.17A

GE presented the analysis of 1994-98 mass change, which indicated 90% mass gain, suggesting USEPA's mass loss estimation technique is extremely sensitive to the large uncertainties. GE also presented a temporal plot of MPA, indicating that the trend over 1984-1994-1998 was inconsistent. USEPA responded that information pertaining to the quality of the GE data was not presented. In addition, USEPA noted that the 1998 MPA estimates were lower than the 1984 estimates, which was consistent with its assessment of an inventory decrease since 1984.

*Rebuttal*: The temporal MPA plot presented by GE has overlapping 95% confidence intervals for the 1984 and 1998 MPA, suggesting variability was too great to make a conclusion regarding mass loss. The quality of the GE data is presented in

the data summary report submitted to USEPA in January 1999 (O'Brien & Gere, 1999).

Response to LG-1.19A

GE commented that water column PCB concentrations increase in a near-linear fashion as one moves downstream through TIP, indicating uniform areal flux from all sediments. In response, USEPA states that the figure presented does not represent a linear increase and also that it indicates several points where PCBs increase disproportionately. USEPA also states that the near-shore samples from the TIP float surveys exhibited higher concentrations, which, according to the Agency, indicate significant "hot spot" loadings.

*Rebuttal*: Higher concentrations in near shore areas do not represent disproportionate sediment flux. As discussed in the GE TIP Report (pp. 46-48), these differences are likely due to laterally-varying hydrodynamics. Since the river flow is smaller in the near-shore areas than in the main channel, the water in these regions is in contact with the sediments longer. Thus, when near shore and center channel sediments produce equal mass flux, the water column in near shore areas will experience higher concentrations.

Response to LG-1.19B

GE's comment presented the analysis from the GE TIP Report, which indicated OC-normalized PCB concentrations were similar within and outside of "hot spots." USEPA's response repeated its discussion from its comments on the GE TIP Report, which discussed the importance of DOC and porosity in OC-normalized concentrations.

*Rebuttal*: USEPA's response does not present data to support its contention that "hot spots" contribute a significantly higher mass flux than non-"hot spot" sediments. A complete discussion of USEPA's comments on this issue is provided in Section 2 of this document. The summary of this issue is repeated here. Pore water concentration under equilibrium partitioning is directly proportional to OC-

normalized sediment concentration. Although sediments with higher OC will have a larger PCB inventory, all sediments with similar OC-normalized PCB concentrations contribute equally to the water column under a pore water flux mechanism. The concentration of pore water that is transported to the water column *does not* depend on the porosity, and variations in 1991 porosity data between "hot spot" and non-"hot spot" sediments are small. Pore water PCB concentrations are also dependent on the DOC concentration, but the 1991 DOC data do not exhibit a large range, or significant differences between fine and coarse sediment samples.

Response to LG-1.22

GE presented an analysis of the TID water column data from the January 1998 flood, which indicated that the PCB composition (based on congener ratios) during scour events was consistent with 1991 TIP surface sediments. USEPA's response claims that GE's approach is flawed because it assumed the TIP sediments have a single congener pattern, and that the sediments resuspended likely represent a broad range of congener patterns. USEPA evaluated 1/9/98 data, and stated that the composition in the Rogers Island samples was most likely resuspension of fresh material from the pumphouse removal activities being conducted by GE at the time. USEPA presents analyses of ΔMW, and states that the TIP load during the event is likely due to the sediments dechlorinated to an extent of 9% or more (thus, implying the older deposits).

*Rebuttal*: GE's point was that the TID water column PCB composition was consistent with resuspension of surface sediments, and not deep dechlorinated sediments. Since the water column PCB load nearly doubled across TIP during the January 1998 flood event, the increase is attributable to scour of TIP sediments. A simple comparison of congener patterns reveals that the water column load passing TID during the event is consistent with resuspension of TIP surface sediments. As presented in Section 2 of this document, the congener pattern from TID-WEST during the event is very similar to that of the average 1998 0-2 cm TIP sediment

PCB data and not that of the deeper, more dechlorinated sediments (Figure 2-5). While it is true that the TIP load may consist of sediments of varying dechlorination status, the fact that the water column load is consistent with the *average* surface sediment composition indicates that the predominant source is the surficial layer not the deeper, more dechlorinated deposits, which have remained in-place.

Response to LG-1.28

GE concluded its comments with a discussion that the best way to evaluate the fate and transport of TIP sediments is with a quantitative mass balance model. USEPA responded that it was developing a model, but that the LRC data set should serve as a constraint for the model, and that "*[s]imply being unable to explain a phenomenon does not inherently make the measurement of the phenomenon wrong.*" USEPA also states that the main point of the LRCR was the conclusion that PCBs in fine-grained sediments have been re-released to the rest of the river.

*Rebuttal*: Apparently, GE and USEPA have differing philosophies on how modeling and data analyses are used together. GE has used Hudson River sediment data as a constraint in calibrating its fate and transport model by requiring the model to accurately reproduce measured sediment concentrations. Based on USEPA's response to this comment, it appears that intended to require its model to reproduce the *results of the LRCR data analyses* (*i.e.*, fine-grained sediment mass loss). Due to the large uncertainties and the statistical manipulations (*e.g.*, use of log ΔMPA +2) inherent in USEPA's mass loss estimate, constraining the model with this metric would have resulted in a weak calibration. As documented in the RBMR, the Agency did not do this. The PCB fate model developed by USEPA was calibrated to sediment concentration data, and the results were inconsistent with the LRCR analyses in that they did not predict a 40% mass loss from TIP cohesive sediments (see Section 1.3 of this document, Table 1-1).

Response to LG-1.38E

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.38H

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.38J

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.40B

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.12

GE commented that USEPA's mass loss estimates were based upon a geometric mean, which is inappropriate. In response, USEPA agreed that an arithmetic mean was more appropriate, and was applied in the new mass loss analysis provided in Appendix A of its LRCR Responsiveness Summary.

*Rebuttal*: Comments on USEPA's updated mass loss calculation are provided in Section 6 of this document.

Response to LG-1.38D

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.38F

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.38G

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.40C

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.34

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.35

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.36

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.5B

GE commented that 1998 TIP sediment data exhibited steep vertical concentration gradients, suggesting burial rather than erosion. USEPA referred to its Response to LG-1.7.

*Rebuttal*: See the rebuttal to Response to LG-1.7.

Response to LG-1.5D

GE cited three analyses of PCB composition that were inconsistent with large-scale scour of TIP sediments. USEPA discussed that GE's analyses are based on the incorrect premise that a single congener pattern represents the "*sole source of PCBs to the water column*," and that multiple mechanisms are likely contributing.

*Rebuttal*: USEPA's response does not refute GE's analyses, which indicate that the *average* water column PCB composition data are consistent with that of the *average* surface sediments and not the *average* from the deeper, more dechlorinated deposits.

Response to LG-1.5F

GE cited USEPA's conclusion that mass gain in "hot spot" 28 was implausible as an example of how the LRCR based broad conclusions on very limited data. USEPA responded that its conclusion regarding "hot spot" 28 was valid because the 1976-78 survey may not have collected complete cores.

*Rebuttal*: USEPA does not present supporting data, and has no way of knowing whether the 1976-78 cores were complete. USEPA's differing treatment of the "hot spot" 28 data is an example of how its LRCR conclusions were partially based on preconceived notions of the system rather than on the entire suite of available data. Calculated mass loss was deemed correct based on "*statistical tests*" and "*geochemical knowledge*," while the "hot spot" 28 mass gain was attributed to data inadequacies. The USEPA Peer Review concluded that the LRCR analysis for all locations downstream of the TIP was meaningless because of the overwhelming uncertainty of the data (Eastern Research Group, 1999).

Response to LG-1.7

GE's comment was that the full vertical PCB profile was not always captured in the 1994 cores, citing presence of $^{137}Cs$ in 19% of LRC bottom sections, and the 1998 cores, 25% of which indicated deeper PCB maxima than the 1994 cores. GE discussed that this results in a high bias for calculated mass loss. USEPA's response indicated that this effect was most likely minor because only 9 of 60 1994 cores used in the analysis were incomplete, and that the area-based mass loss estimates developed both with and without incomplete cores suggested the added uncertainty was only about 5%.

*Rebuttal*: USEPA's area-based mass loss calculations are reviewed in Section 6 of this document.

USEPA also discussed that the 1998 GE cores confirm the LRCR in that they exhibit 1984-98 mass loss.

*Rebuttal*: Besides the fact that the 1984-98 mass loss purported by USEPA is not statistically significant, USEPA fails to explain the apparent 1994-98 mass gain indicated by the 1998 data. The only issue the 1998 data demonstrate is that USEPA's point-by-point analysis technique is too sensitive to variability and uncertainty to yield conclusive results.

Response to LG-1.38C

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.38I

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.39B

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.40A

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.5A

GE discussed that lack of detectable $^{7}Be$ does not suggest deposition is not occurring. USEPA referred to its Response to LG-1.18.

*Rebuttal*: See the rebuttal to Response to LG-1.18.

Response to LG-1.18

GE presented its analysis of $^{7}Be$ data, which suggested 1) detectable $^{7}Be$ corresponds to an estimated 0.5-1.5 cm of deposition in "hot spots," and 2) due to the short $^{7}Be$ half life, the elapsed time between likely deposition and measurement, and the thickness of the surficial

sediment samples collected by USEPA, lack of $^{7}$Be detection does not indicate a lack of deposition. USEPA asserts that GE "*presupposes a knowledge of the geochemistry of $^{7}$Be which is currently not available,*" referring to the assumed concentrations of $^{7}$Be on depositing particles and assumption of uniform deposition. Based on this statement, USEPA refutes all of the deposition calculations presented by GE.

*Rebuttal*: The calculations presented by GE possess some uncertainty in the parameters. However, these calculations were presented for illustrative purposes to demonstrate that $^{7}$Be absence cannot be used as a case for the absence of burial - a point USEPA agreed with.

USEPA discussed that recent deposition does not indicate long-term burial, and that other factors such as sporadic events and bioturbation complicate interpretation of $^{7}$Be data in sediments.

*Rebuttal*: USEPA's response reinforces GE's main point. The analyses presented in the comment were used to support GE's conclusion that $^{7}$Be data could not be used to make conclusions regarding the presence or lack of deposition. USEPA further supports this by stating: "*Ultimately, it is only the presence and depth of more persistent tracers, such as $^{137}$Cs or PCBs themselves which can provide a true measure of the deposition rate at a given location.*"

USEPA's response further claimed that atmospheric deposition of $^{7}$Be occurs throughout the year and that sediment $^{7}$Be deposition may not be dominated by a single event. This effect would cause the deposition calculated by GE's method to decrease. USEPA also stated that since $^{137}$Cs deposition no longer has an atmospheric component, it was most likely dominated by a single spring high flow deposition event. USEPA went on to hypothesize that if $^{7}$Be was deposited in a single event as assumed by GE, then the $^{7}$Be/$^{137}$Cs ratio should be constant across all samples. USEPA calculated this ratio for all 0-1 inch sections of the LR cores, accounting for the rapid decay of $^{7}$Be by correcting all samples to the date of the midpoint of sample counting (9/1/94). This correction essentially converts all concentrations to what they would have been if

all samples were counted on the same date (assuming the $^{7}Be$ was all deposited on the same date, and that this deposition occurred before collection of the first sample). USEPA plotted the frequency distribution of the $^{7}Be/^{137}Cs$ ratio and claimed that since the ratio varied over an order of magnitude, the $^{7}Be$ deposition was not "*governed by a spring high flow depositional event.*"

*Rebuttal*: USEPA's interpretation of the $^{7}Be/^{137}Cs$ ratio is invalid. The plotted frequency histogram does not prove that the ratio is not the same for all samples. Rather, the plot looks more like the sampling of a lognormally-distributed variable. The distribution has a distinct central tendency, with variability around its median. As can be seen from Figure 5-3, a lognormal distribution based on the statistics from USEPA's data fits the $^{7}Be/^{137}Cs$ ratio frequency histogram well. Furthermore, when the log-transformed ratios are plotted on a probability scale (Figure 5-4), the values plot along a straight line, indicating the distribution is lognormal. The variability about the mean in the plotted distribution results from sampling a small set from a population, as well as the large uncertainty associated with the radionuclide analyses. The reported Sigmas (*i.e.*, counting error) for $^{7}Be$ and $^{137}Cs$ in the 1-inch LRC tops averaged 30% and 7%, respectively.

USEPA's response regarding $^{7}Be$ detection limits presents a histogram of the detectable $^{7}Be$ concentrations, and shows that the lower bound on detectable concentrations is 400 pCi/kg. The Agency states that this is probably more accurate than the mean of the estimated detection limits, and that this lower level would result in a lower amount of deposition that could still result in a non-detect, as calculated by GE's method. USEPA concludes by stating that it still believes that samples without detectable $^{7}Be$ are "*indicative of sites with little or no very recent deposition and as sites potentially undergoing scour.*"

*Rebuttal*: Based on the analyses presented in GE's original comment, uncertainties in the $^{7}Be$ detection limit, the thickness of the section analyzed by USEPA, and the rapid $^{7}Be$ decay rate limit its usefulness for evaluating sediment deposition. Lack of $^{7}Be$ in the top 1-inch of a sediment core cannot be used as evidence for lack of deposition, and certainly not as evidence of scour.

Response to LG-1.18A

GE presented depth profiles from the 1998 Focused Sediment cores, which indicated that the surficial 5-cm of sediments had lower PCB concentrations and a PCB composition that was much less dechlorinated (based on MDPR and PK46:PK32 ratio) than the sediments at depths of 9 inches or more. This analysis was used to refute USEPA's contention that burial was not occurring because the highest concentrations were in the top 9 inches of the LR cores. USEPA responded that GE's data set was too limited to "*discern statistically significant trends*" and that the lack of a PCB gradient in the top 5-cm is consistent with sediment-derived PCBs recontaminating freshly deposited materials rather than 1 cm/yr of burial (which would have resulted in a large gradient due to the large reductions in Fort Edward PCB loadings). USEPA also cited the large range of MDPR in the top 1-cm of GE's cores as further evidence of this. USEPA went on to discuss that reversals in MDPR within the top 5-cm might be a marker of the Allen Mill event, and if so, the deposition would be 0.5 cm/yr, which it does not consider a rate capable of producing deep burial.

*Rebuttal*: USEPA's discussion of burial rates is speculative, and does not present any data to refute GE's analysis, which indicates that data from the 1998 surficial sediments are not consistent with scour of deep, dechlorinated sediments. USEPA has implied much greater variability in the surficial MDPR's than actually exists. In 11 out of 12 cores GE plotted in the LRCR comments, the surface MDPR is approximately 0.4, while the MDPR for the sediments at depths of 9 inches or more is consistently between 0.6 to 0.8. The purpose of this plot was to demonstrate the large gradient in PCB concentration and composition between the 0-5 cm sediments and the deeper material.

USEPA also claimed that the 1994-98 mass gain calculated by GE still results in a mass loss with respect to the 1984 data, and that the 1994-98 difference is within the range of uncertainty in the 1994 data. USEPA presented an example calculation, in which it cited a core with a 1984 MPA of 100 that experienced an 80% 1984-94 mass loss (1994 MPA of 20)

followed by an 89% 1994-98 mass gain (1998 MPA of 38) still produces a 62% 1984-1998 mass loss.

*Rebuttal*: USEPA failed to recognize GE's point. The point-by-point comparison technique for mass change calculations is flawed. There is no evidence to suggest that sediment PCB fate and transport mechanisms changed between 1984-94 and 1994-98. If the 1994-98 differences are within the uncertainty of the 1994 MPA estimates, this would imply that the 1994 MPA values have an error of +/- 100%. This uncertainty is too great to make conclusions regarding mass change.

Response to LG-1.18B

GE commented that data analysis and modeling of the 1984 flood indicates widespread deposition of fine-grained sediments. In response USEPA stated that "*other processes may affect the sediment transport rate besides simple resuspension and settling within normal river boundaries.*" The Agency claimed that deposition in backwaters and near-shore areas and surface erosion between flood events make characterization of the system as a whole difficult. USEPA also discussed that it did not have enough information to comment on GE's sediment transport model.

*Rebuttal*: USEPA does not present any data or literature which support its speculations regarding the effects of these "*other processes.*" GE's fully documented sediment transport model (QEA, 1999b) has been extensively calibrated, tested, and peer-reviewed, and is consistent with the other data analyses presented in the LRCR comments: the TIP is a net depositional environment, the scour required for a 40% TIP sediment PCB inventory loss is unsupportable. The Agency's own model supports this as well (RBMR; Section 1 of this document).

Response to LG-1.39A

This comment was submitted by Dr. Paul Switzer on behalf of GE. Dr. Switzer's rebuttal is included in Appendix B.

Response to LG-1.24

GE commented that accurate estimation of mass change in the PCB "hot spots" below TIP was not possible due to sparse data and invalid assumptions. GE also presented an analysis of the uncertainty about USEPA's "hot spot" mass change estimates, which indicated that the error was too great to make conclusions regarding mass change. In response, USEPA stated that the data were sufficient to "*draw firm conclusions regarding loss of PCB mass from several of the downstream hot spots.*" USEPA claimed that although the arithmetic means for 1976-78 and 1994 were not statistically different, the geometric means were for four "hot spots." USEPA also acknowledges the large uncertainties discussed by GE.

*Rebuttal:* Although USEPA acknowledges the large uncertainties in the extrapolation of sparse samples to large areas and the 1976-78 density estimates, it failed to recognize that the errors associated with these cannot be adequately quantified. Therefore, the uncertainties about its geometric mean estimates are under-predicted, and are significant enough to render the "hot spot" mass comparisons meaningless. Furthermore, the USEPA Peer Review Panel concluded that the LRCR analysis for all locations downstream of the TIP was meaningless because of the overwhelming uncertainty of the 1977 data (Eastern Research Group, 1999).

# SECTION 6
# COMMENTS ON USEPA'S ALTERNATIVE APPROACH TO ESTIMATING PCB INVENTORY CHANGES PRESENTED IN THE LRCR RESPONSIVENESS SUMMARY

## 6.1 OVERVIEW

In Appendix A of the LRCR Responsiveness Summary (USEPA, 1999a), the USEPA presents *"an alternate analysis for the estimation of the direction and degree of change in the PCB inventory of the TI Pool."* This alternate analysis was conceived by the USEPA in an attempt to eliminate two flaws identified by GE in its comments on the LRCR:

1) comparison of 1994 PCB measurements to a biased sub-sample of the 1984 PCB measurements, and

2) assumption that paired 1984 and 1994 samples measured PCBs in the same sediment (flawed because of the weak spatial correlation of PCB levels in TIP sediments).

The significant changes in the analysis were the use of area or "*cluster*" averages for comparison, rather than individual sample pairs, and the use of the ratio of 1994 to 1984 mass per unit area (MPA) as the statistic for expression of mass change. These changes yield a significantly different result: 43% loss of $PCB_{3+}$, in comparison to about 80% loss using the original approach (see GE, 1998). In addition, the alternate analysis includes portions of the 1984 data set that add significant uncertainty and does not properly quantify the uncertainty of the overall loss estimate; these flaws still compromise the utility of the results.

## 6.2 EVALUATION

The USEPA LRCR Responsiveness Summary included a recalculation of PCB inventory change in the Thompson Island Pool between 1984 and 1994. For this recalculation, comparison

of individual samples was abandoned in favor of comparison of averages of samples located within defined areas. The defined areas ("*clusters*") were delineated by the locations sampled in 1994 and the extent of spatial correlation in the data based on the variogram analyses documented in the DEIR. This approach circumvents several of the poor assumptions inherent in the original analysis. However, the approach is flawed by equally poor assumptions. As with the original approach, results from the "*cluster*" approach are too uncertain to permit any determination of overall PCB inventory change between 1984 and 1994. Unfortunately, the USEPA analysis failed to recognize the poor assumptions and correctly quantify variability. As a result, the USEPA has incorrectly concluded that a significant reduction in inventory occurred between 1984 and 1994.

### 6.2.1 Deficiencies in the Data

In the Agency's alternative ("*cluster*") analysis, sample data were used to represent a geographic area without any analysis of how well the data represent the area in which they are located. In fact, the data tend to be clustered rather than being randomly distributed within the areas; this is especially so for the 1994 data. This point is demonstrated by the sample locations for Cluster LR-14 shown in Figure 6-1. Although the 1984 samples are distributed throughout this particular "*cluster,*" they span both fine and coarse sediments, and include locations both within and outside of "Hot Spot" 6. In contrast, the 1994 samples are more closely spaced together in this "*cluster,*" and clearly do not represent the same sediment area as the 1984 samples (Figure 6-1).

Thus, spatial bias exists within each "*cluster,*" and, this spatial bias differs for the 1984 and 1994 samples. These spatial biases add significant uncertainty to the comparison of average PCB mass for 1984 and 1994 samples within these "*clusters*" because of the small-scale spatial variability in PCBs (*e.g.,* Figure 5-2).

USEPA's treatment of the 1984 data set in this alternate analyses results in significant uncertainties in its mass estimates. In reproducing the Agency's analysis presented in the LRCR

Responsiveness Summary, it became clear that the Agency included two types of uncertain data associated with the 1984 samples:

1) grab samples, for which the depth was unknown and therefore assigned based on assumptions, were included in the "*clusters*" for the 1984 data; and

2) for many of the samples in the 1984 data set, PCBs were measured using an inaccurate mass spectrometry method, rather than the standard Aroclor-based method. For these samples PCBs were not accurately quantified, but rather assigned to broad approximate ranges (*e.g.,* 10-50 ppm). When calculating the 1984 PCB mass for each "*cluster,*" the Agency included several of these 1984 samples by assigning specific numeric values for PCB concentration based on a statistical analysis of these broad mass spectrometry concentration ranges. Therefore, including the 1984 mass spectrometry data in the analysis imparts significant uncertainty in estimate MPA. Moreover, the uncertainty associated with including the 1984 mass spectrometry data cannot be directly quantified.

The Agency's inclusion of the grab samples and mass spectrometry data adds significant uncertainty to the mass estimates associated with the 1984 data in the "*clusters,*" and therefore, to any calculations of mass change between 1984 and 1994. The Agency failed to recognize these uncertainties in its documentation, and also failed to account for their effect on the overall uncertainty of its mass loss estimate.

### 6.2.2 Improper Quantification of Uncertainty

Another significant error in the USEPA analysis is the failure to consider the uncertainty associated with the average PCB MPA calculated for each "*cluster.*" USEPA calculated its uncertainty estimate for the overall mass loss using the distribution of the mean MPA ratios among all the different "*clusters*" used in the calculation. However, the estimate did not account for the uncertainty associated with the average 1984 and 1994 mass within each individual "*cluster.*" As shown by Figure 6-2, excluding the variability of 1984 and 1994 mass within the "*clusters*" is a significant oversight. Figure 6-2 demonstrates that the individual mass estimates

used to calculate the average 1984 MPA for each "*cluster*" typically span two or more orders of magnitude. A proper accounting of uncertainty would require the use of Analysis of Variance procedures, in which the variability associated with these means is explicitly included in estimation of the uncertainty of the overall mean. As a result of this mistake, the uncertainty of the calculated overall average mass change between 1984 and 1994 was likely underestimated to a significant extent by the Agency.

Additional comments on the recalculation that were provided by Dr. Switzer are included in Appendix B.

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# FIGURES

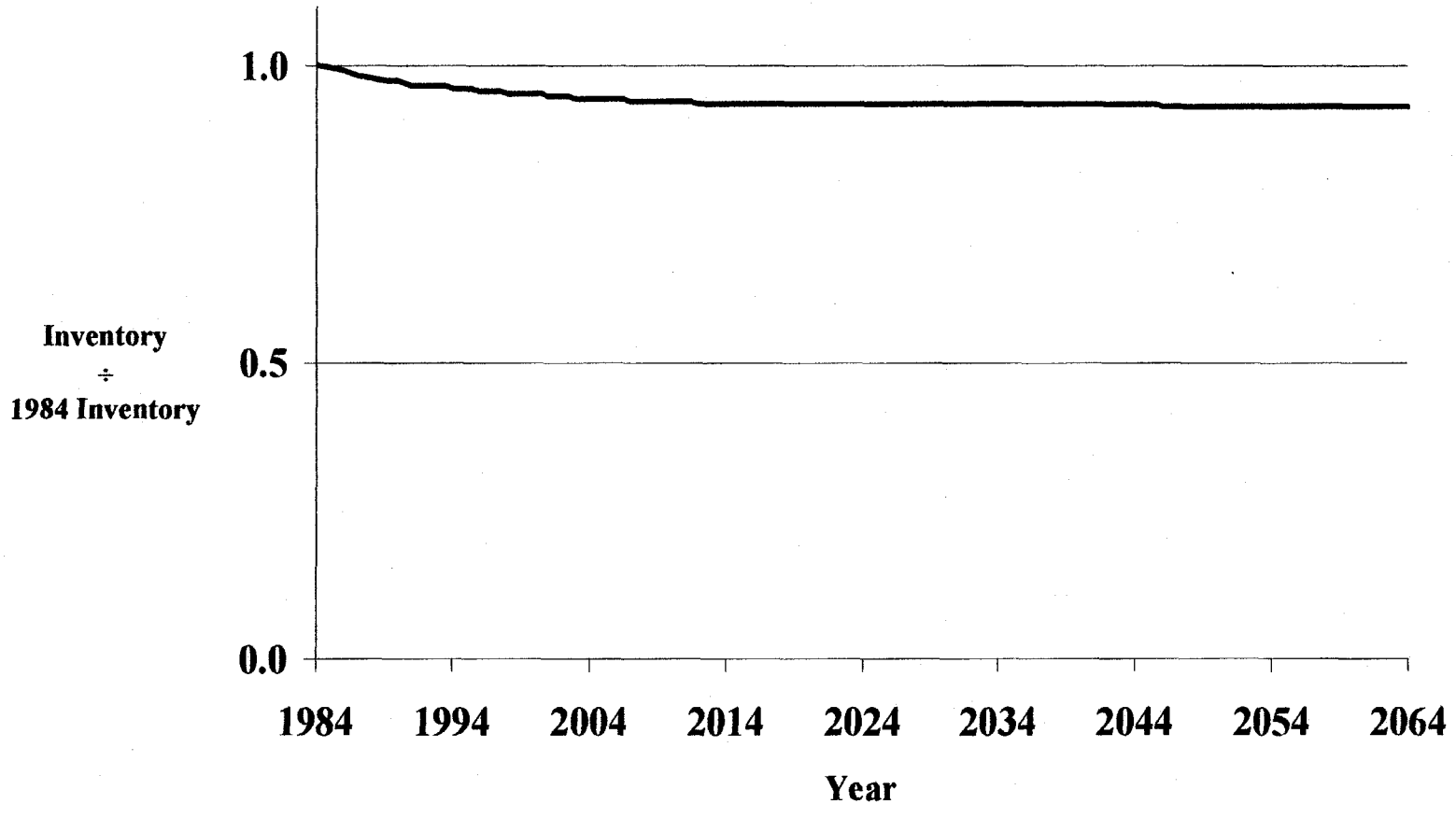



Figure 1-1. Cumulative change in TIP cohesive sediment $PCB_{3+}$ mass from 1984 to 2064 as computed by the USEPA HUDTOX model relative to USEPA estimated 1984 inventory of 8.2 metric tons.

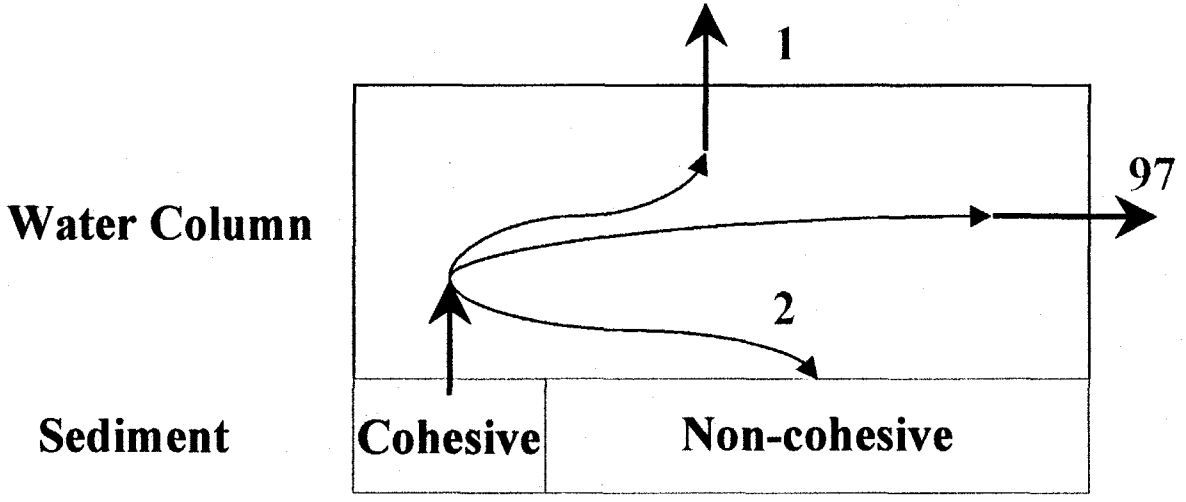



Figure 1-2. The relative fate of PCBs leaving TIP cohesive sediments as computed by the USEPA HUDTOX model for the period from 1984 to 1994. Values are percentages of the total flux from cohesive sediments.

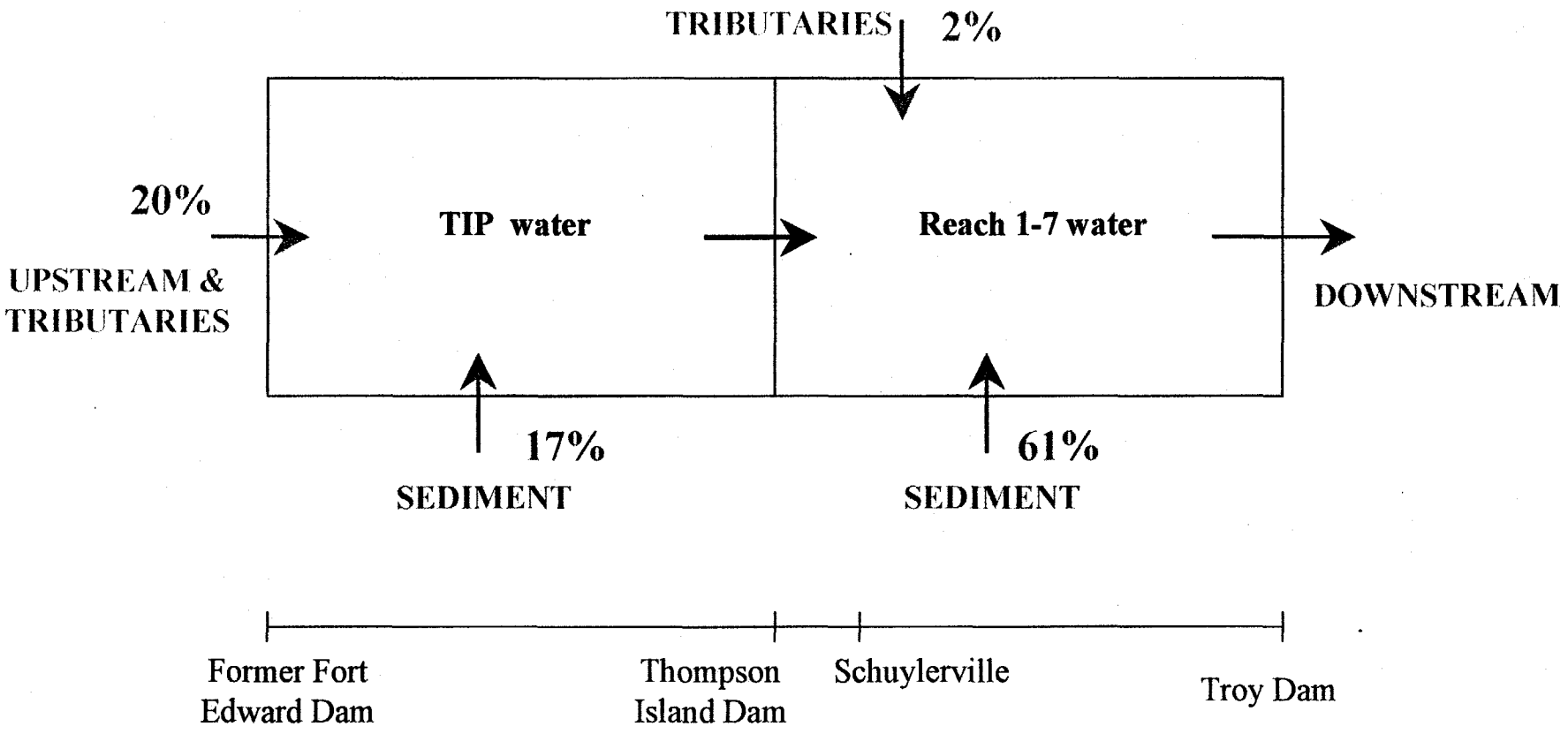



Figure 1-3. The relative contributions of the various PCB sources to the 1994 PCB flux at the Troy Dam as computed by the USEPA HUDTOX model.

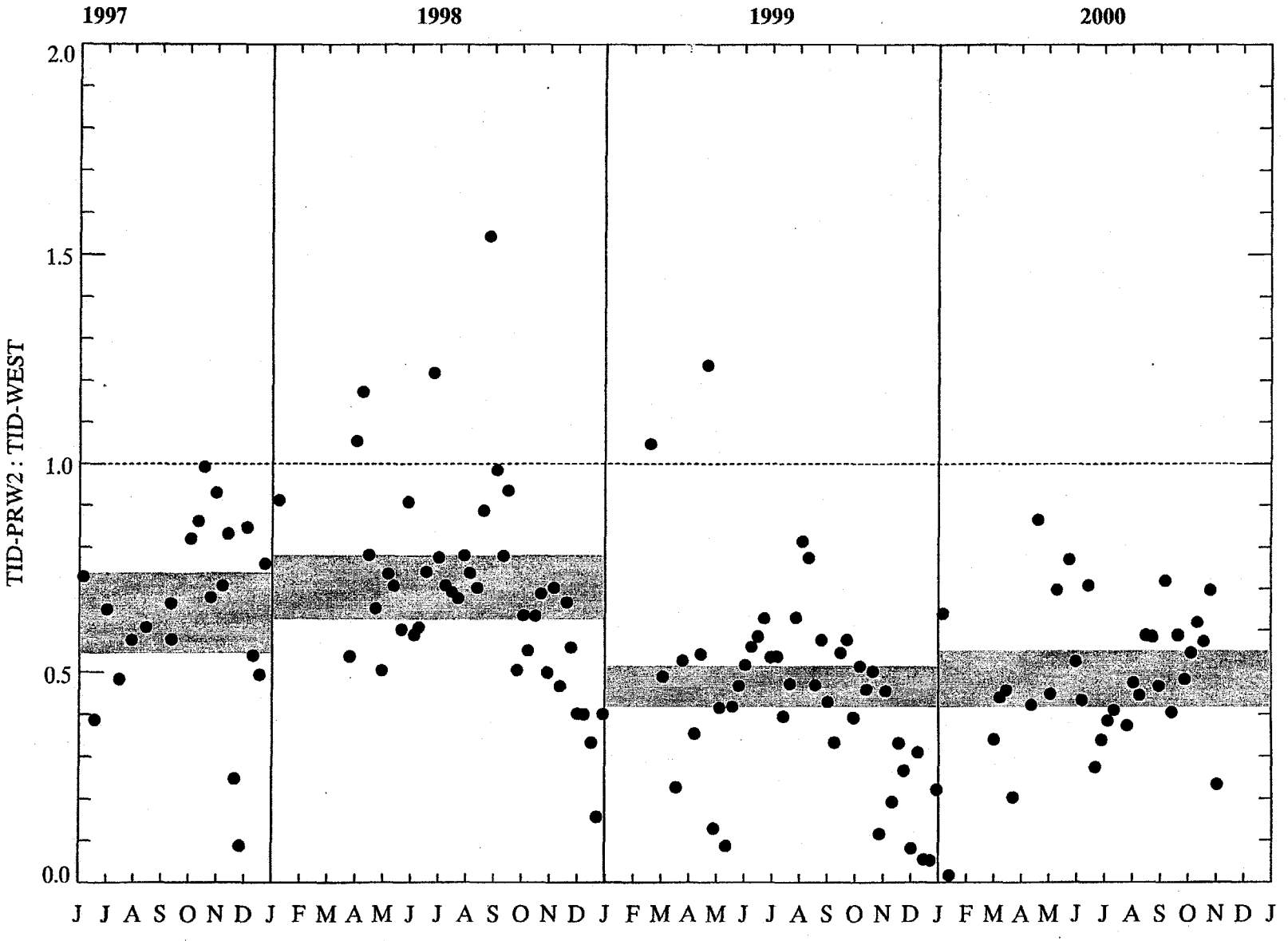



Figure 2-1. Temporal profile of the ratio of total PCB samples collected at TID-PRW2 to samples collected at TID-WEST.

*Notes: Samples collected at TIP-18C included with TID-PRW2 samples. Duplicate samples averaged. Non-detect samples at TID-WEST omitted. Shading represents the Mean +/- 2 Standard Errors for a given year.*

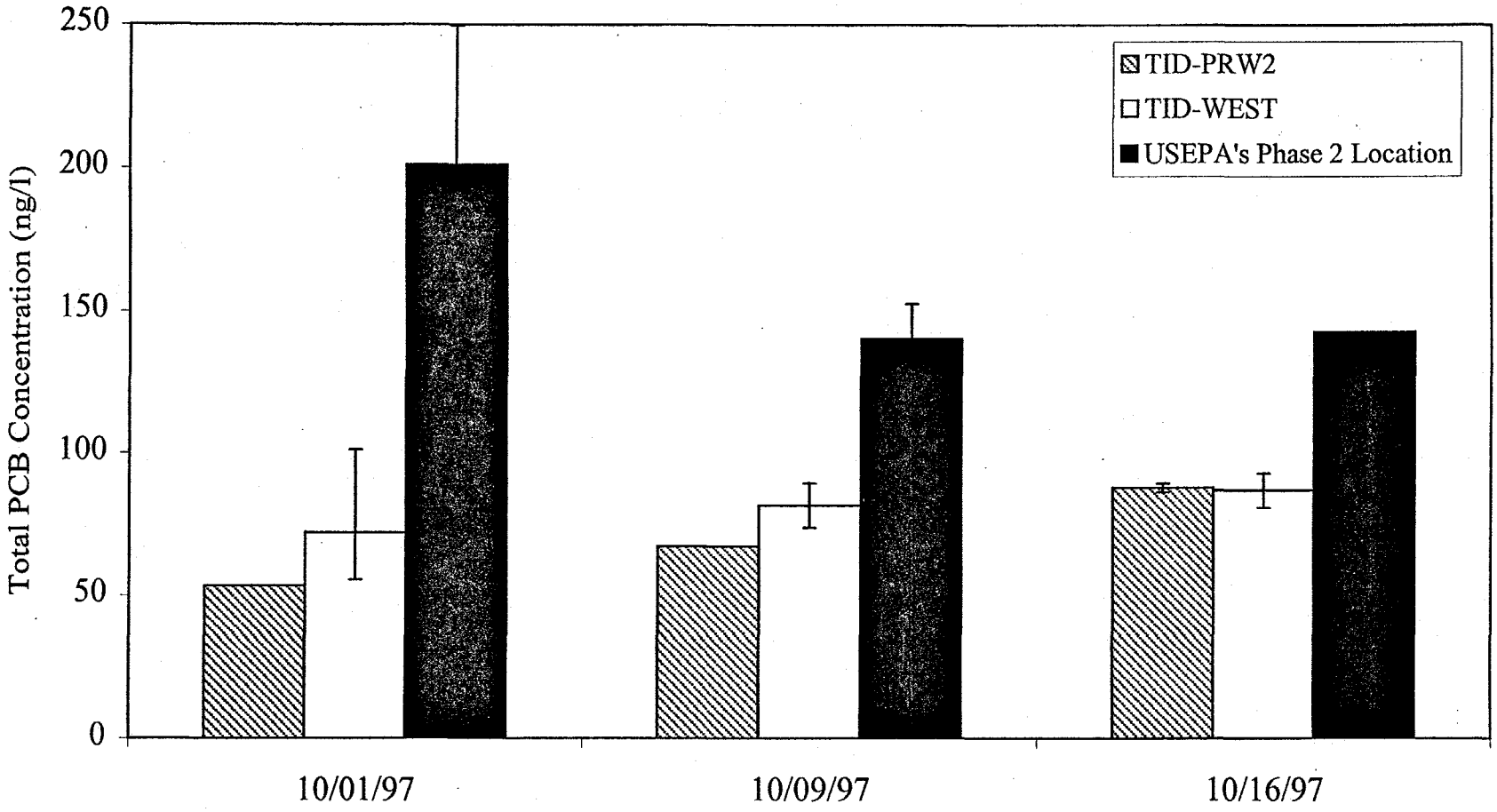
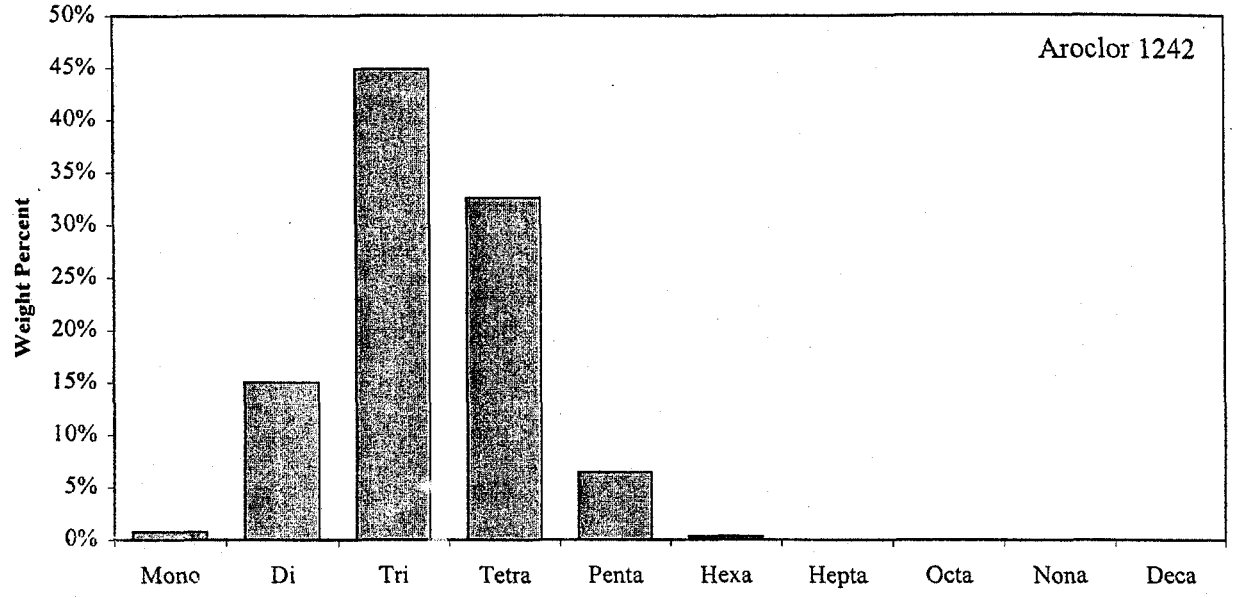



Figure 2-2. Comparison of PCB concentrations of samples collected within the vicinity of Thompson Island Dam.

*Note: Error bars represent range of values from duplicate samples*



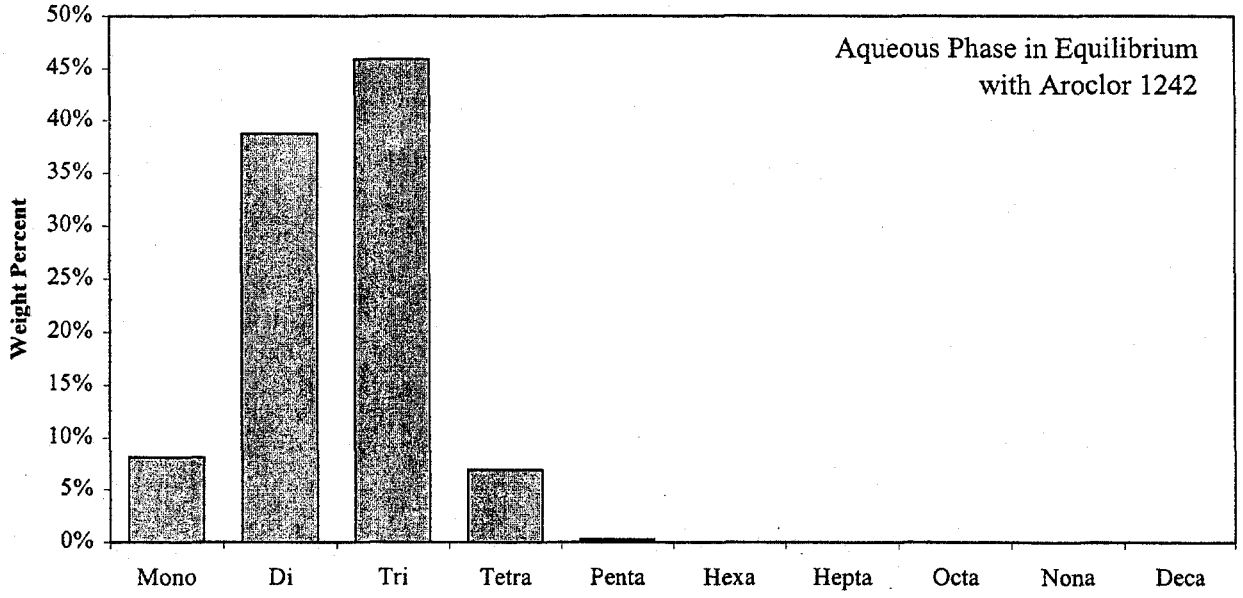




Figure 2-3. Comparison of PCB homolog distributions for Aroclor 1242 and calculated aqueous phase in equilibrium with Aroclor 1242.

*Notes*
*- Aroclor 1242 congener composition based on data from Frame (1996).*
*- Aqueous phase calculated based on weight percent in Aroclor 1242 and congener solubilities published in Abramowitz and Yalkowsky, 1990.*

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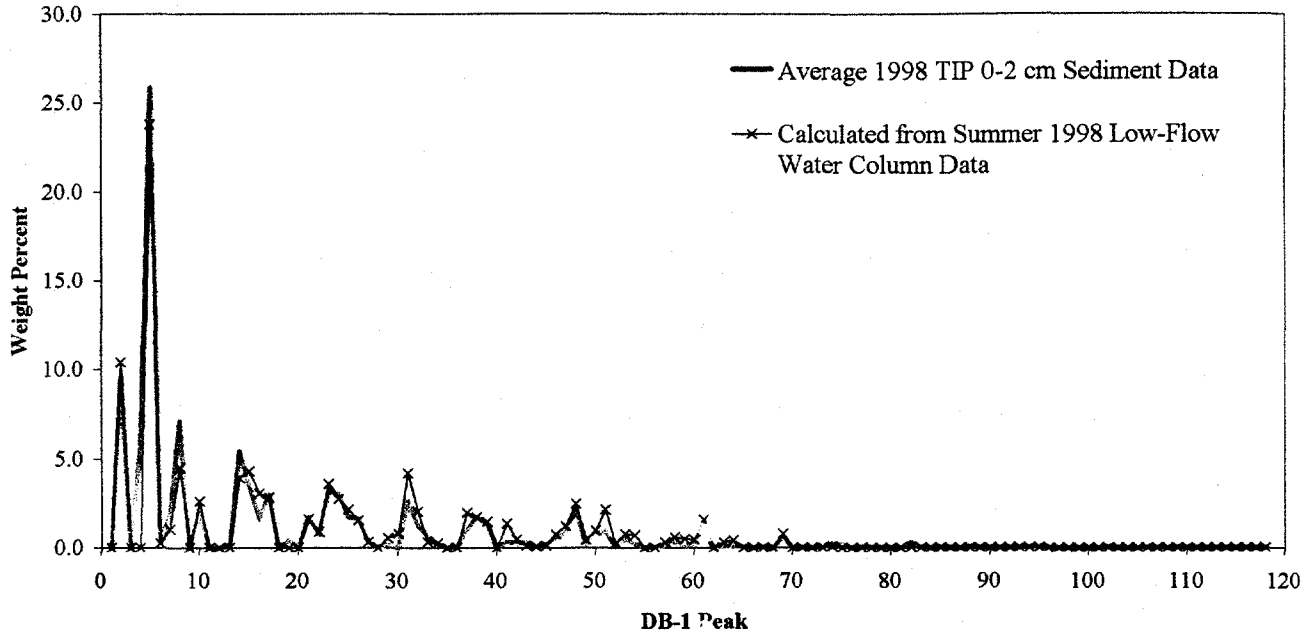


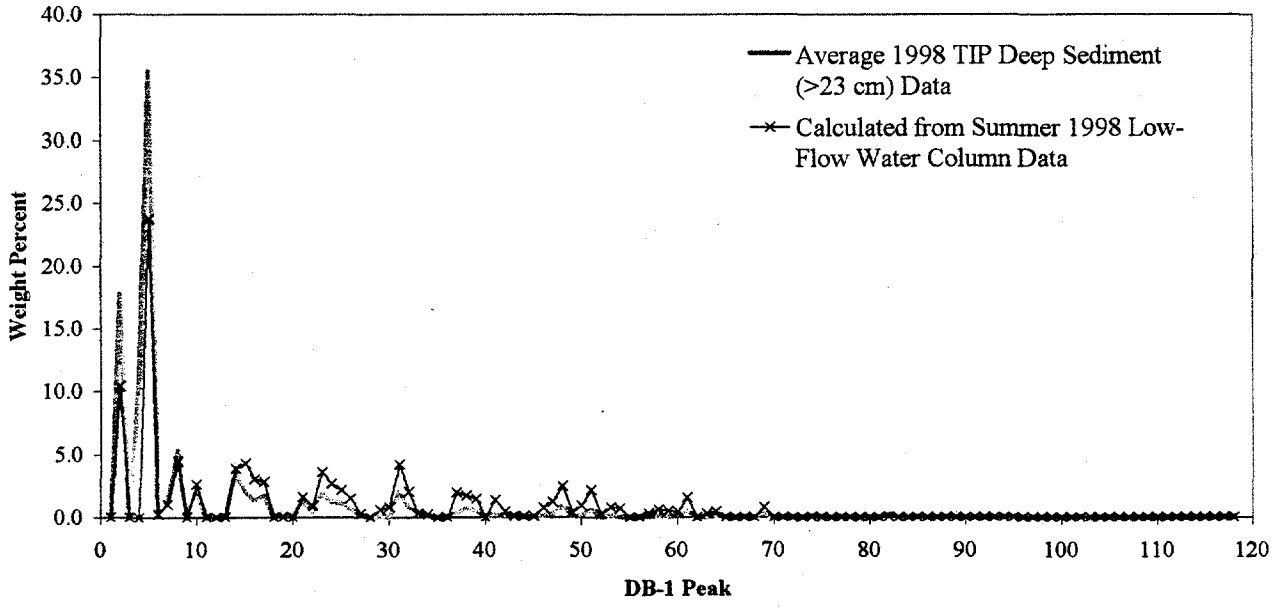


Figure 2-4.

Comparison of the average 1998 0-2 cm TIP sediment PCB DB-1 peak distribution and the average 1998 deep sediment (> 23 cm) TIP PCB DB-1 peak distribution with that calculated from summer 1998 low-flow water column data based upon pore water diffusion and equilibrium partitioning.

*Note: Recent laboratory analysis determined a non-PCB eluting at the DB-1 Peak 4 retention time in the 1998 sediment PCB data.*

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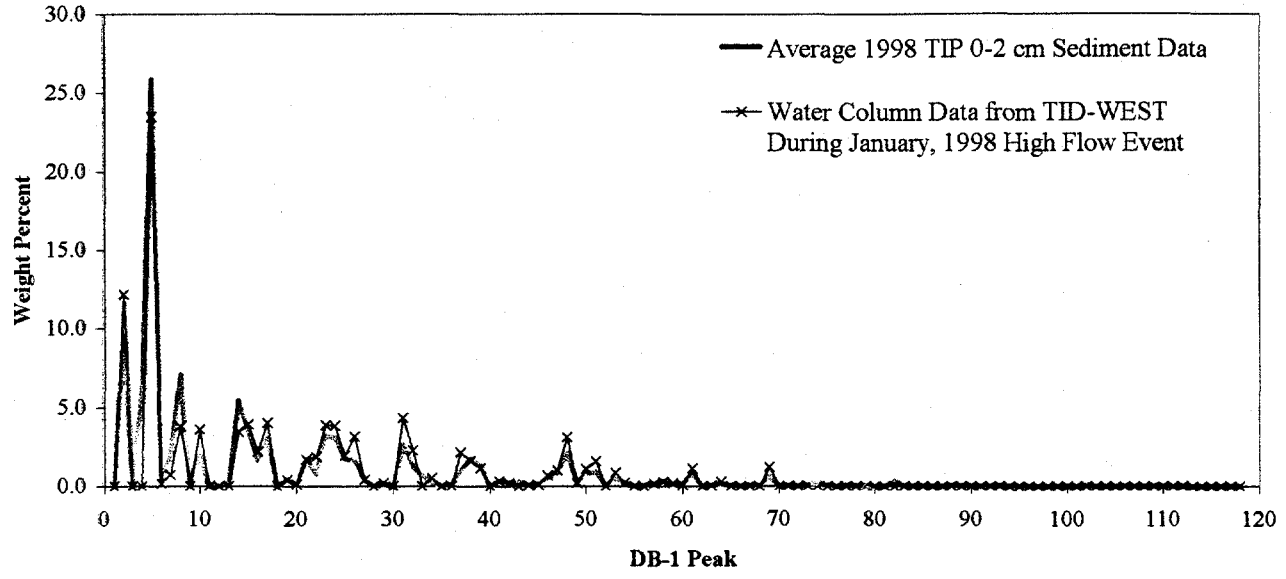


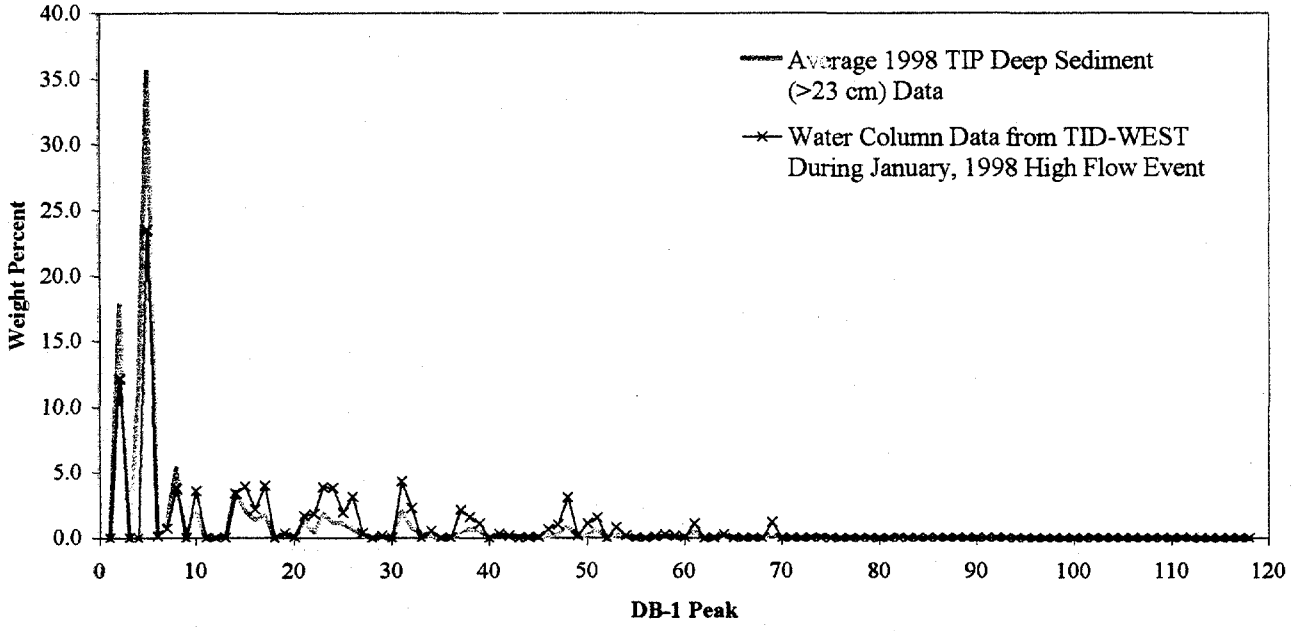


**Figure 2-5.**

Comparison of the average 1998 0-2 cm TIP sediment PCB DB-1 peak distribution and the average 1998 deep sediment (> 23 cm) in TIP PCB DB-1 peak distribution with water column data from TID-WEST during the January 1998 high flow event.

*Note: Recent laboratory analysis determined a non-PCB eluting at the DB-1 Peak 4 retention time in the 1998 sediment PCB data.*

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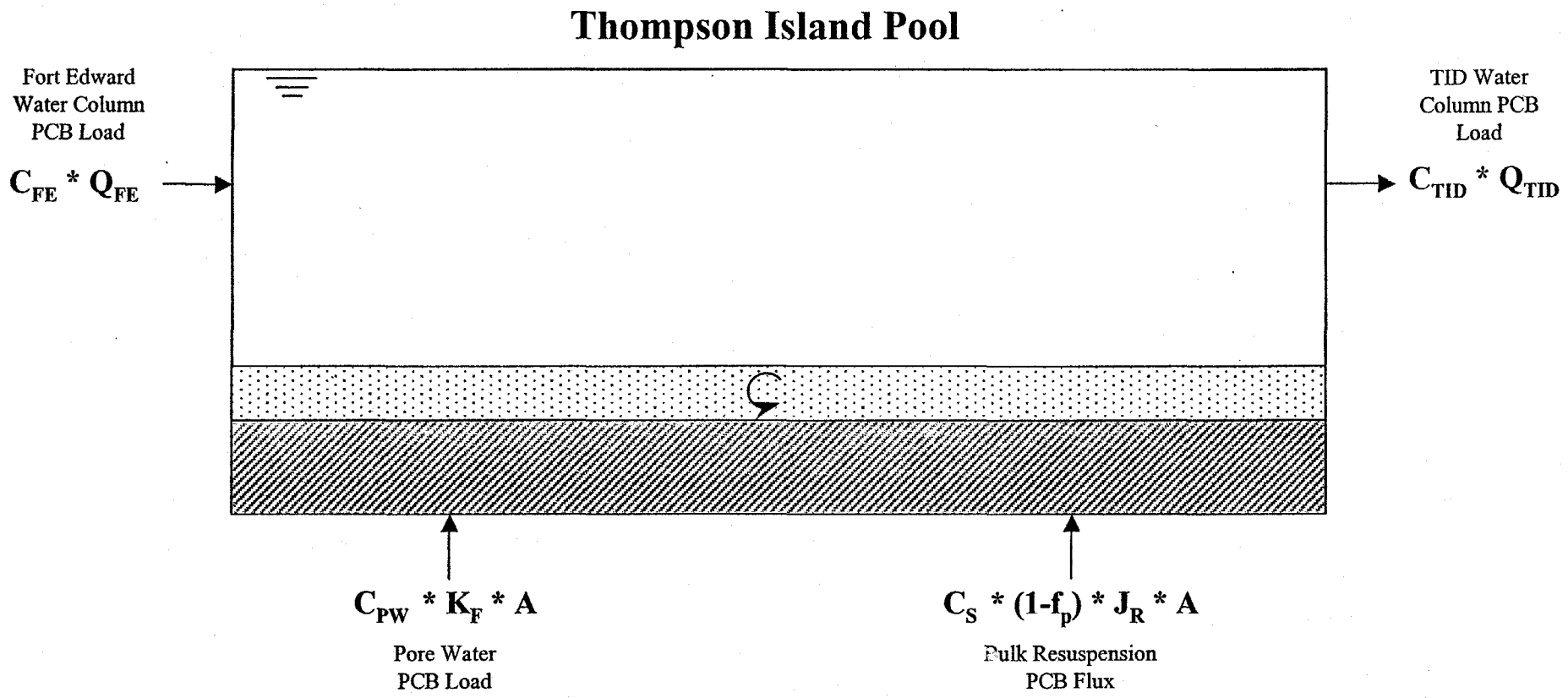



$$\Delta \text{Water Column Load} = \text{Pore Water Load} + \text{Resuspension Load}$$

$$Q(C_{TID} - C_{FE}) = [C_{PW} * K_F * A] + [C_S * (1-f_p) * J_R * A]$$

Figure 2-6. Mass balance calculation for a single PCB congener used to calculate the flux from surface sediments under a combination of pore water and bulk resuspension.

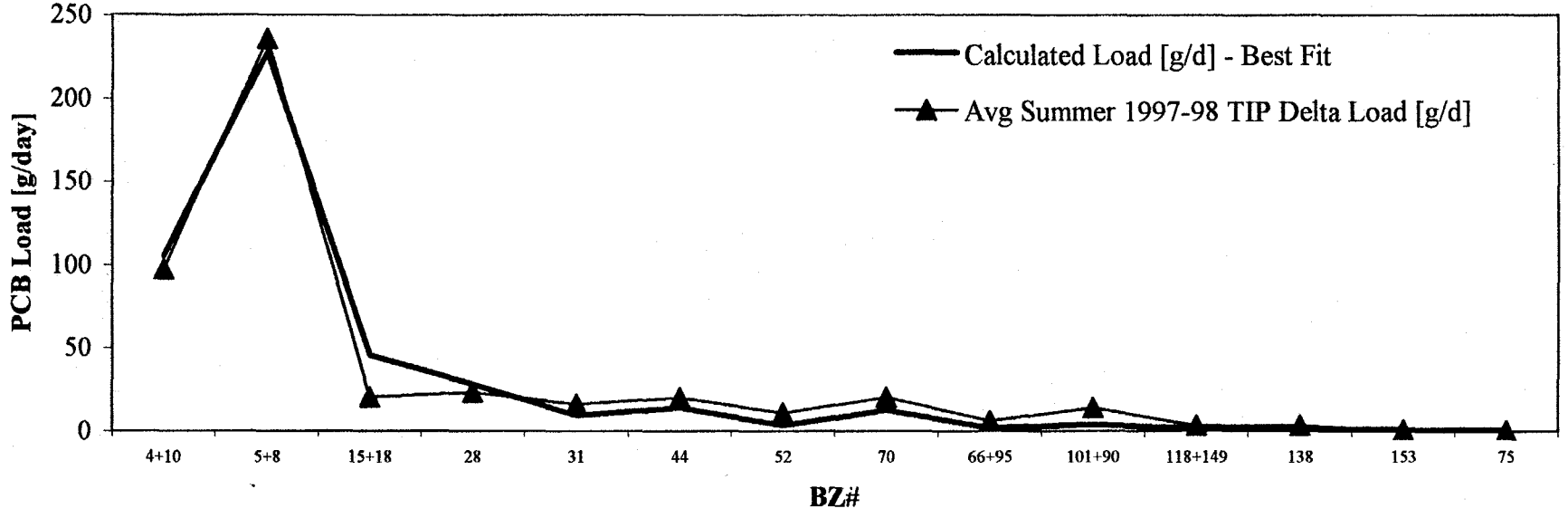



Figure 2-7. Comparison of PCB congener composition for the average summer 1997-98 TIP delta load and a best fit loading calculated based on the sum of the loadings from pore water and bulk resuspension.

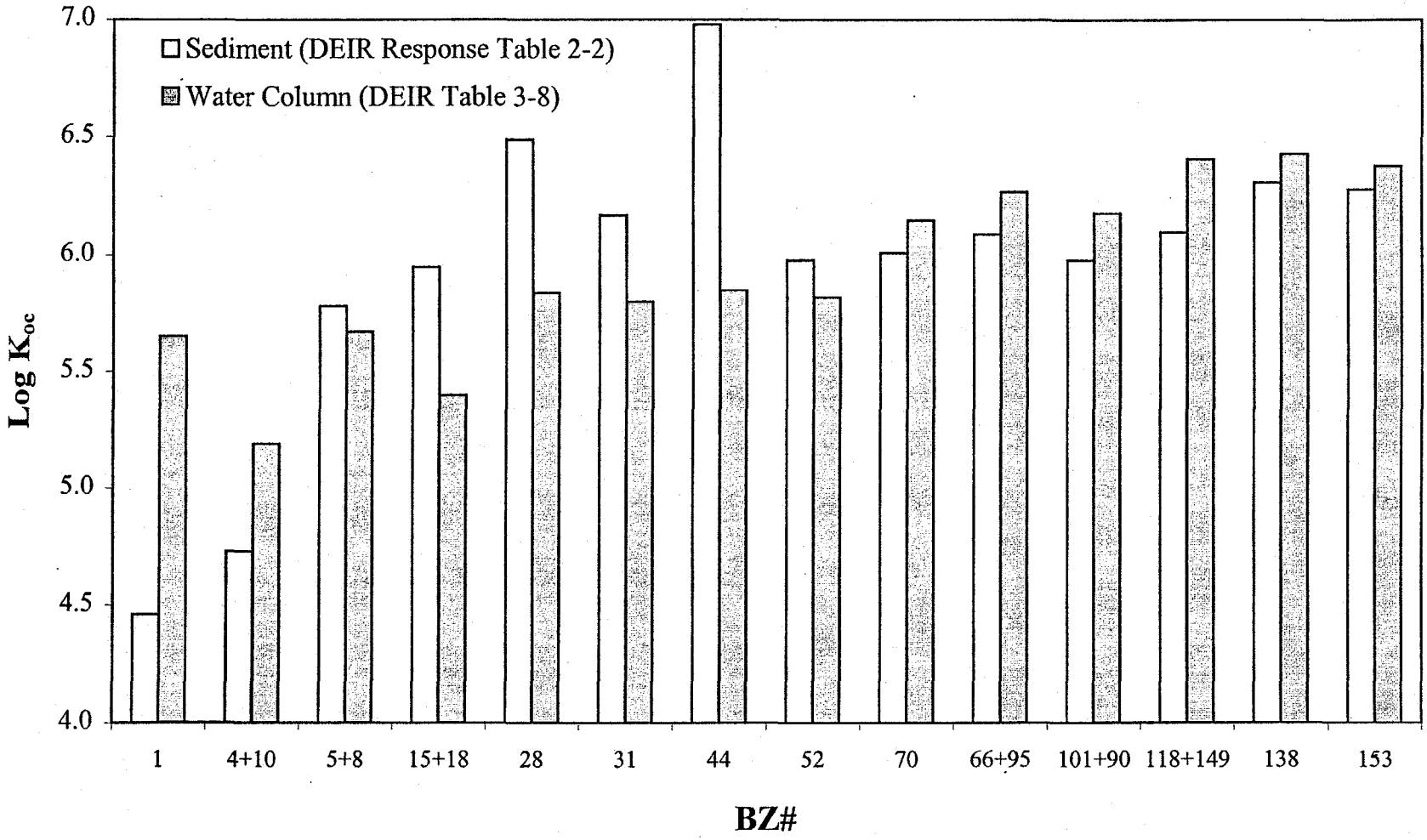



Figure 2-8. Comparison of the sediment and water column Log Koc values for PCB congeners used in USEPA's analysis of a combined pore water and bulk sediment loading source.

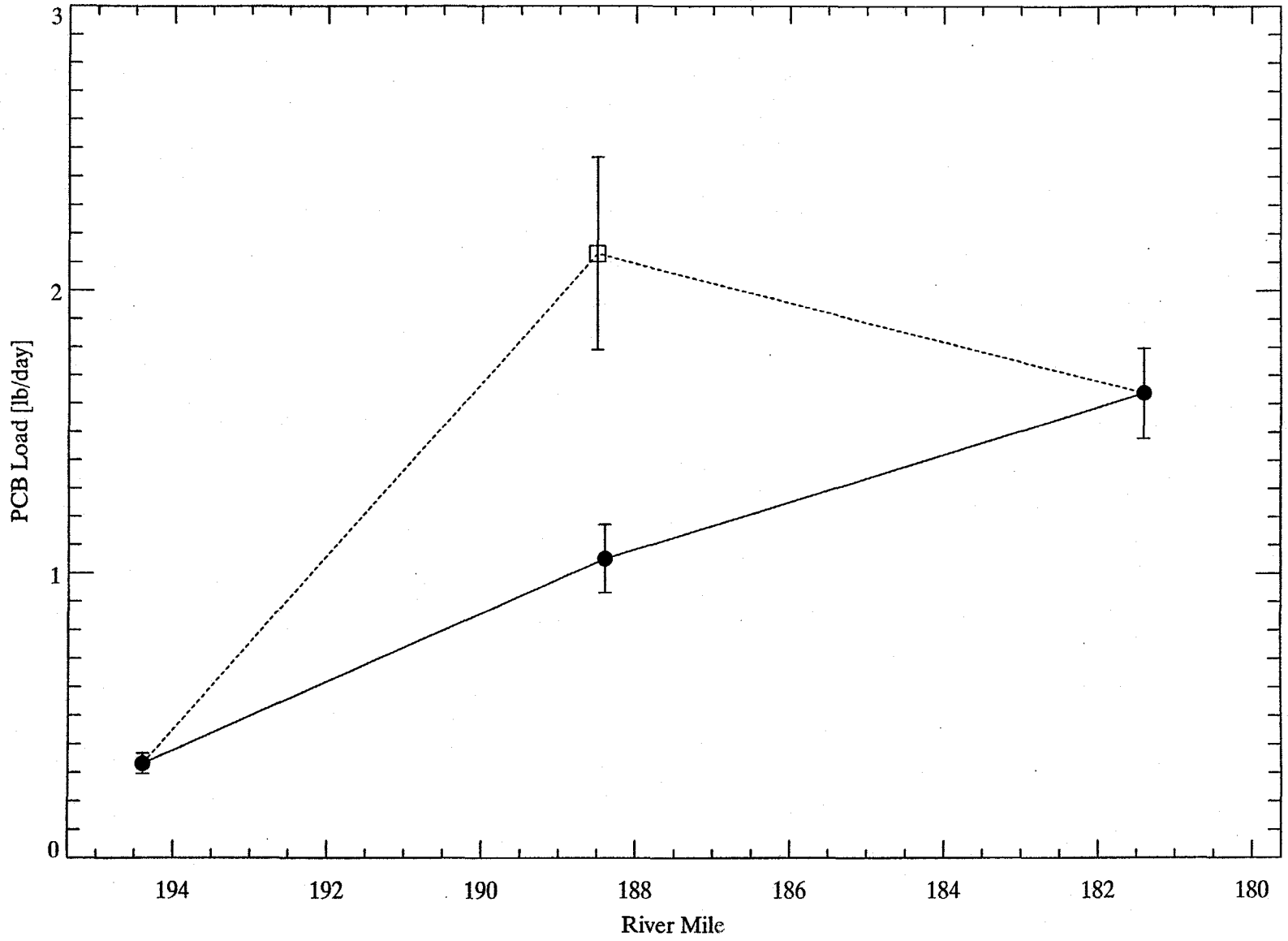



Figure 2-9. Spatial profile of average PCB loading from Fort Edward to Schuylerville for low flow data (<10,000 cfs) collected after 10/01/97.

*Notes: Open square represents TID-WEST sampling station. Flow at TID and Schuylerville prorated from USGS flow collected at FE.*

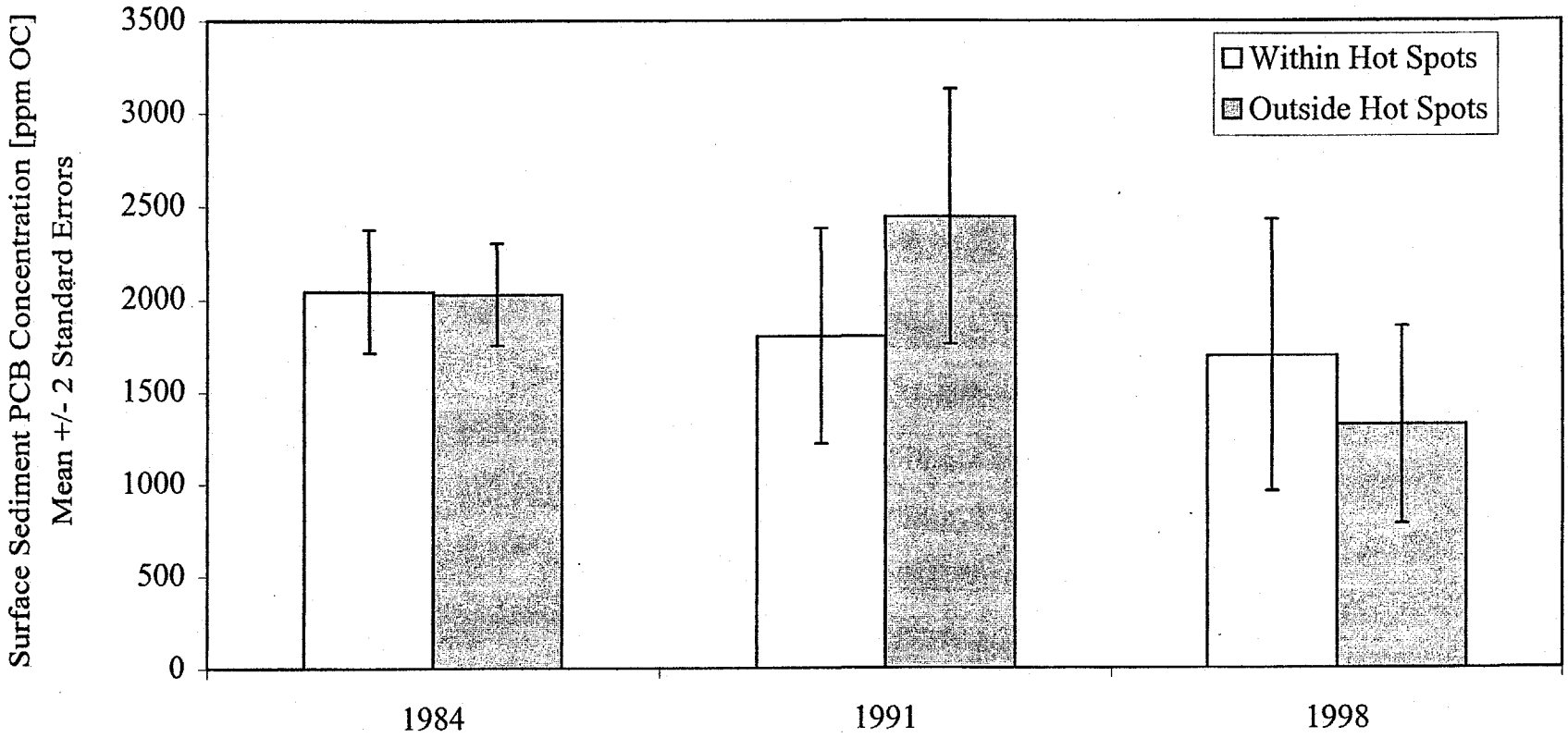



Figure 2-10. TIP organic carbon normalized surface sediment PCB concentrations within and outside of 1976 NYSDEC "Hot Spots".

*1991 samples were stratified based on sediment texture: fine (within) and coarse (outside).*
*Depth intervals used in comparisons are: 0-2.5 in. (1984), 0-5 cm (1991), and 0-2 cm (1998).*



ktr - fig_2-10_sedocdata.xls - plot
12/5/00 - 3:13 PM

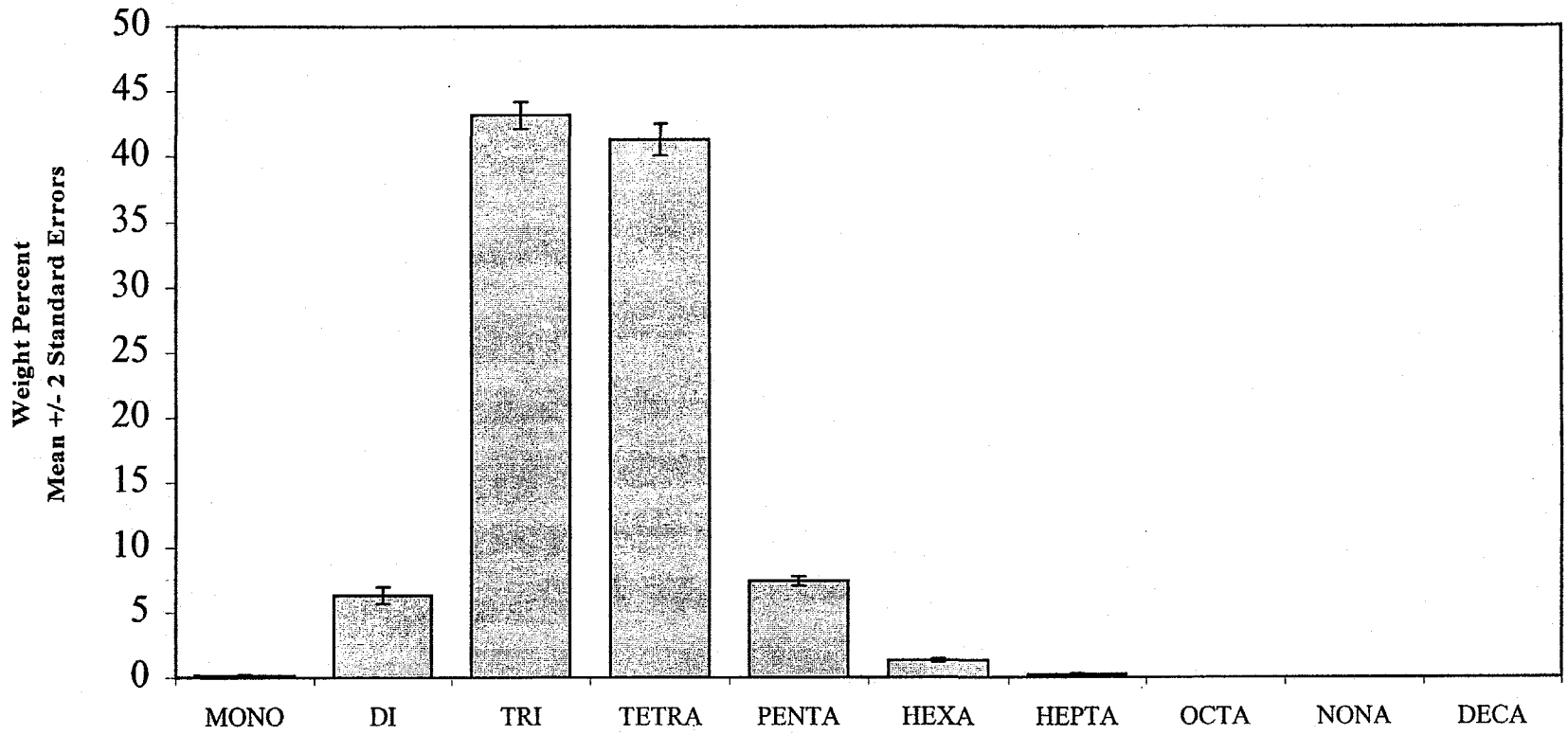



Figure 2-11. PCB homolog distribution for 1998 water column samples from the Bakers Falls Wingdam Area with total PCBs > 1 ppb.

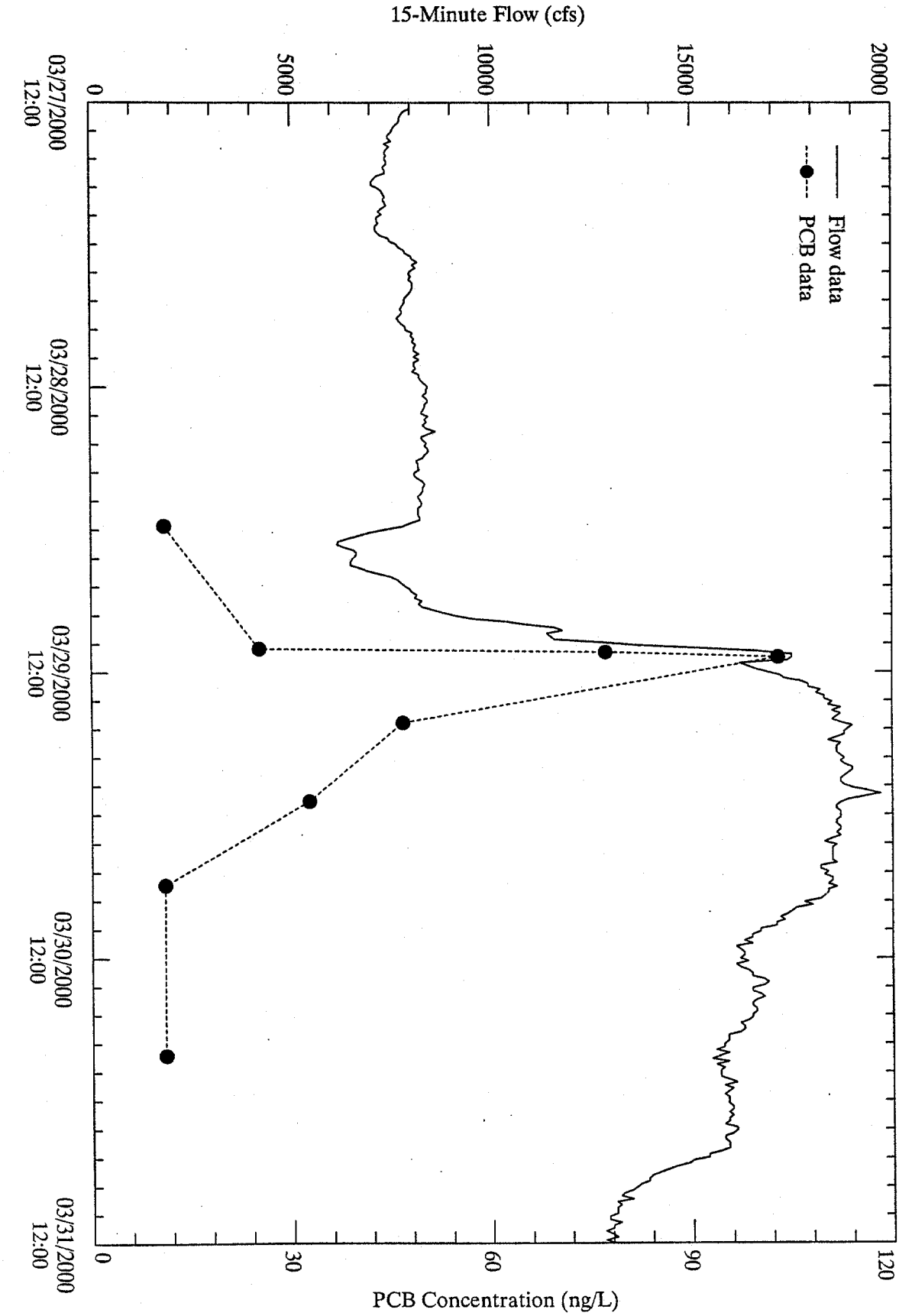



Figure 4-1. Flow and PCB concentration at Fort Edward during high flow sampling conducted on March 28-30, 2000.

*Notes: 15-minute USGS flow data are provisional; non-detects set to 11.0 ng/L; duplicates averaged.*

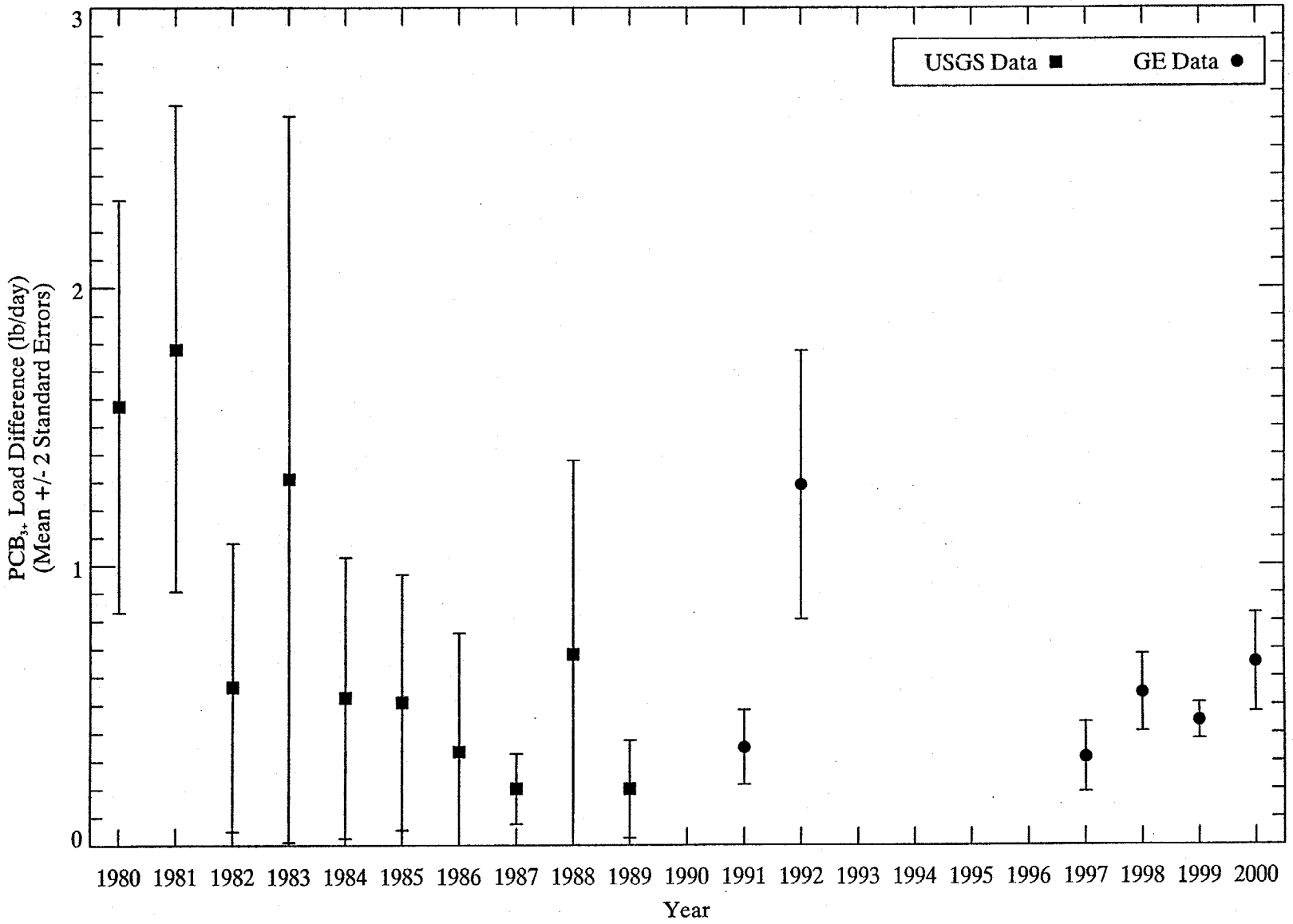



Figure 4-2. Temporal trends in mean annual low flow water column PCB loading from Fort Edward to Schuylerville (1980-2000).

*Note: Data for flow < 10,000 cfs at Fort Edward plotted; High loadings from 9/91 to 12/91 and outlier on 7/12/84 are excluded; Flow at Schuylerville prorated from USGS flow data collected at Fort Edward.*

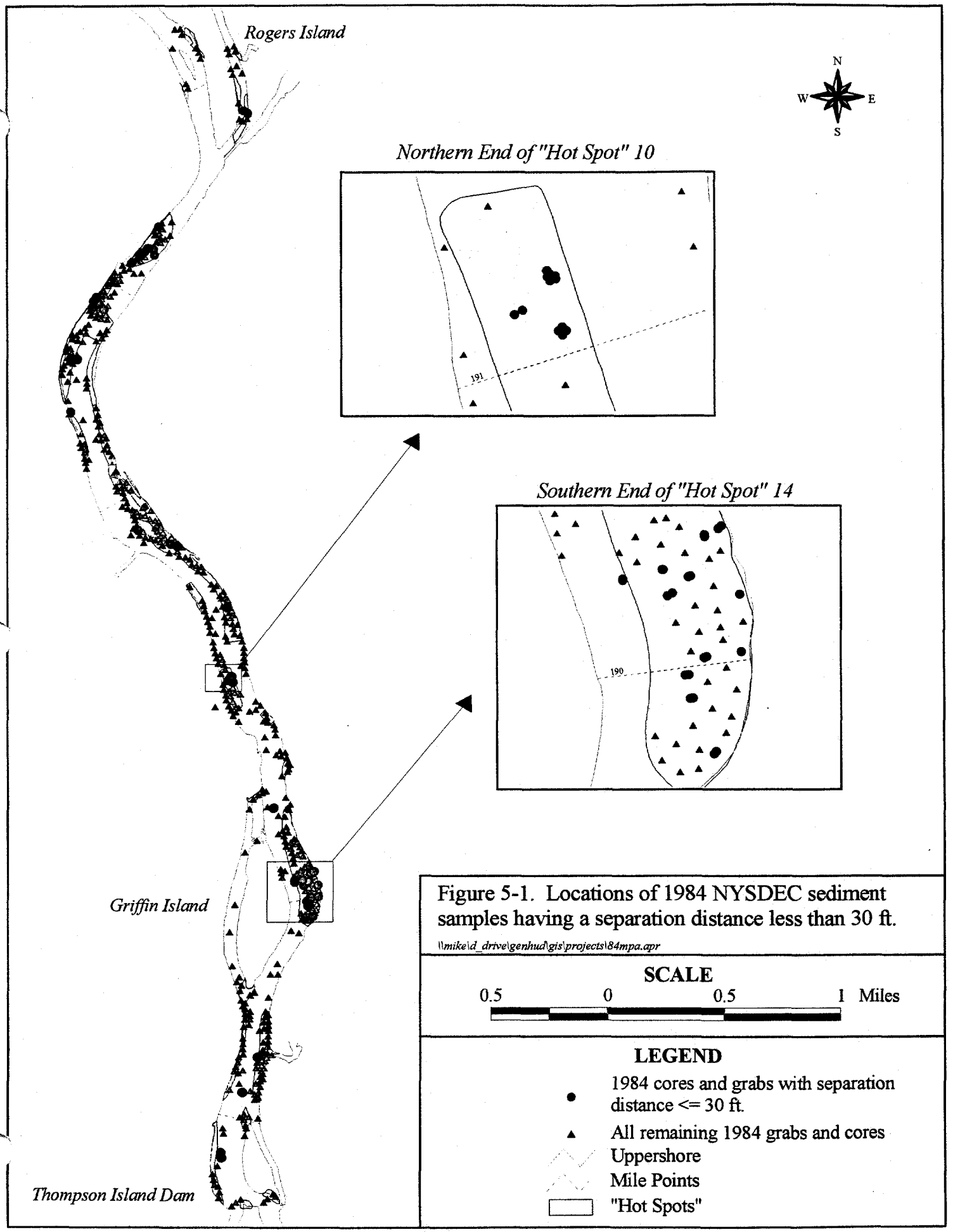



Figure 5-1. Locations of 1984 NYSDEC sediment samples having a separation distance less than 30 ft.

*\\mike\d_drive\genhud\gis\projects\84mpa.apr*

**SCALE**

0.5 0 0.5 1 Miles

**LEGEND**

- 1984 cores and grabs with separation distance <= 30 ft.
- All remaining 1984 grabs and cores
- Uppershore
- Mile Points
- "Hot Spots"

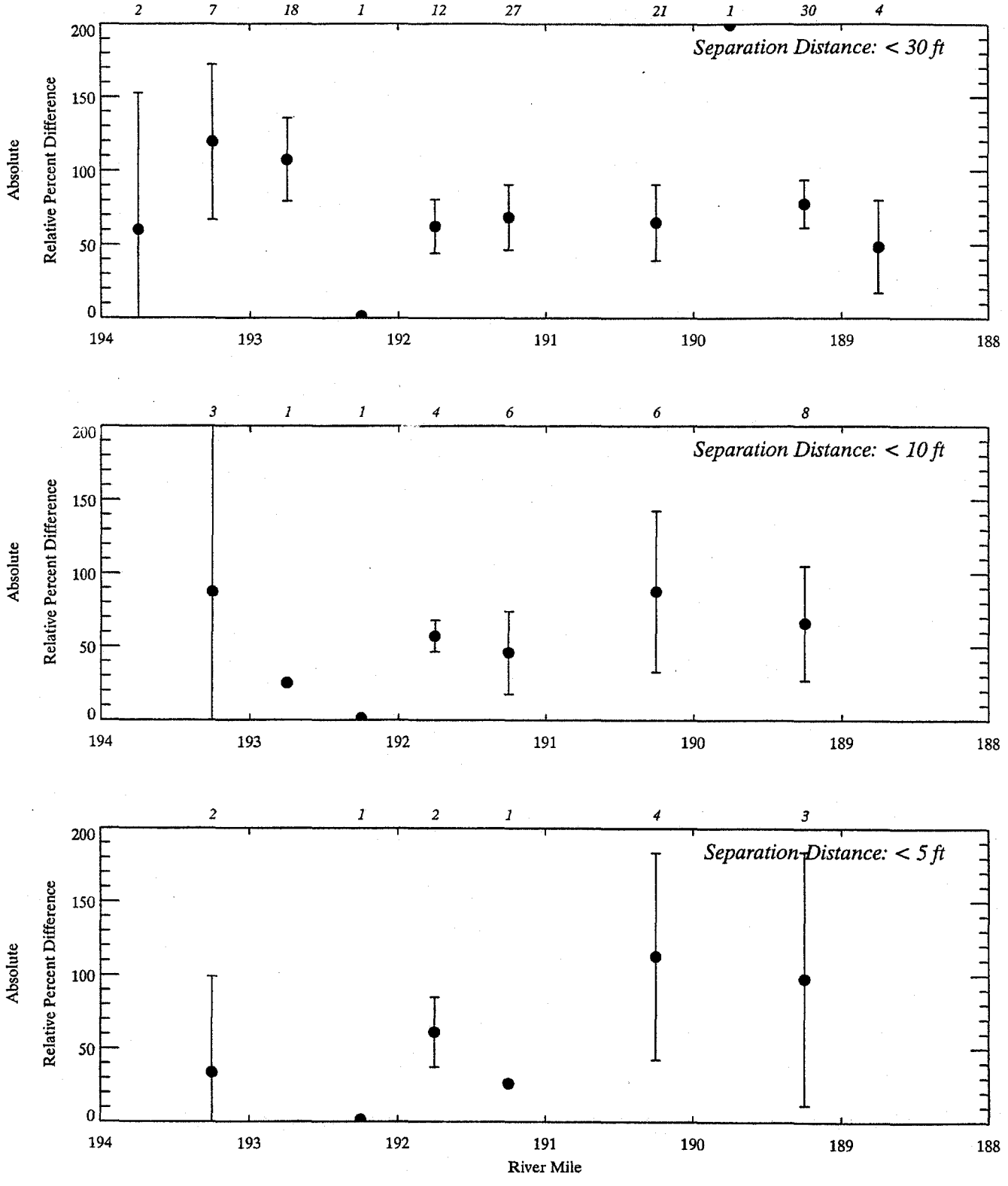



Figure 5-2. Spatial profile of relative percent difference in MPA between 1984 sample pairs.

*Number posted above plot panels indicate number of sample pairs in average.*

MJW - D:\GENhud\analysis\Sediment\Post LRC Report\masscalc\99calcs\plots\spatial_3pan.pro
Tue Dec 05 17:39:36 2000

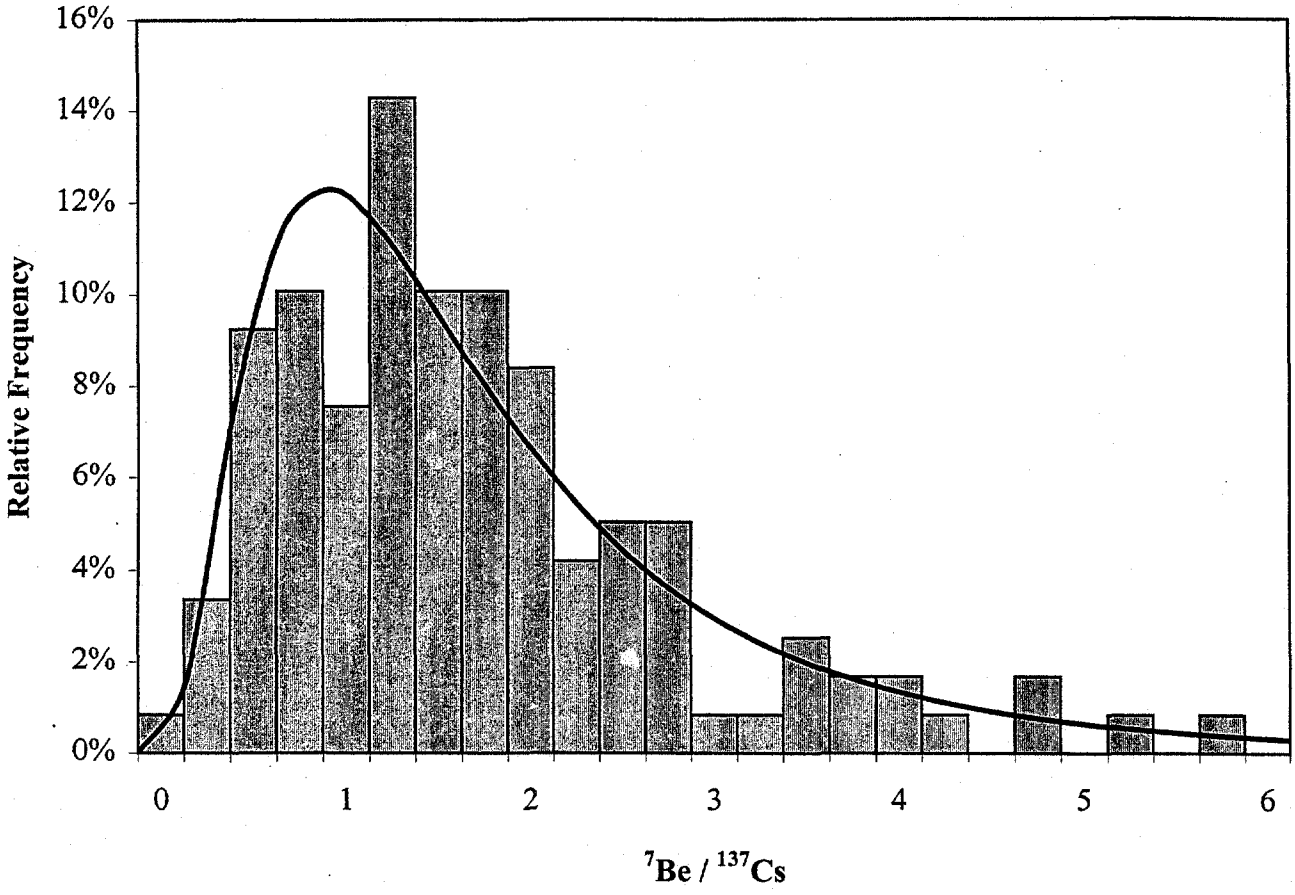



Figure 5-3. Relative frequency distribution of the ratio of $^{7}Be$ to $^{137}Cs$ in USEPA Low Resolution Cores.

*Note: Line represents fit of log-normal distribution*

MJW - fig_5-03_be_cs_rat_rebuttal.xls (Matched)
12/5/00 - 5:28 PM

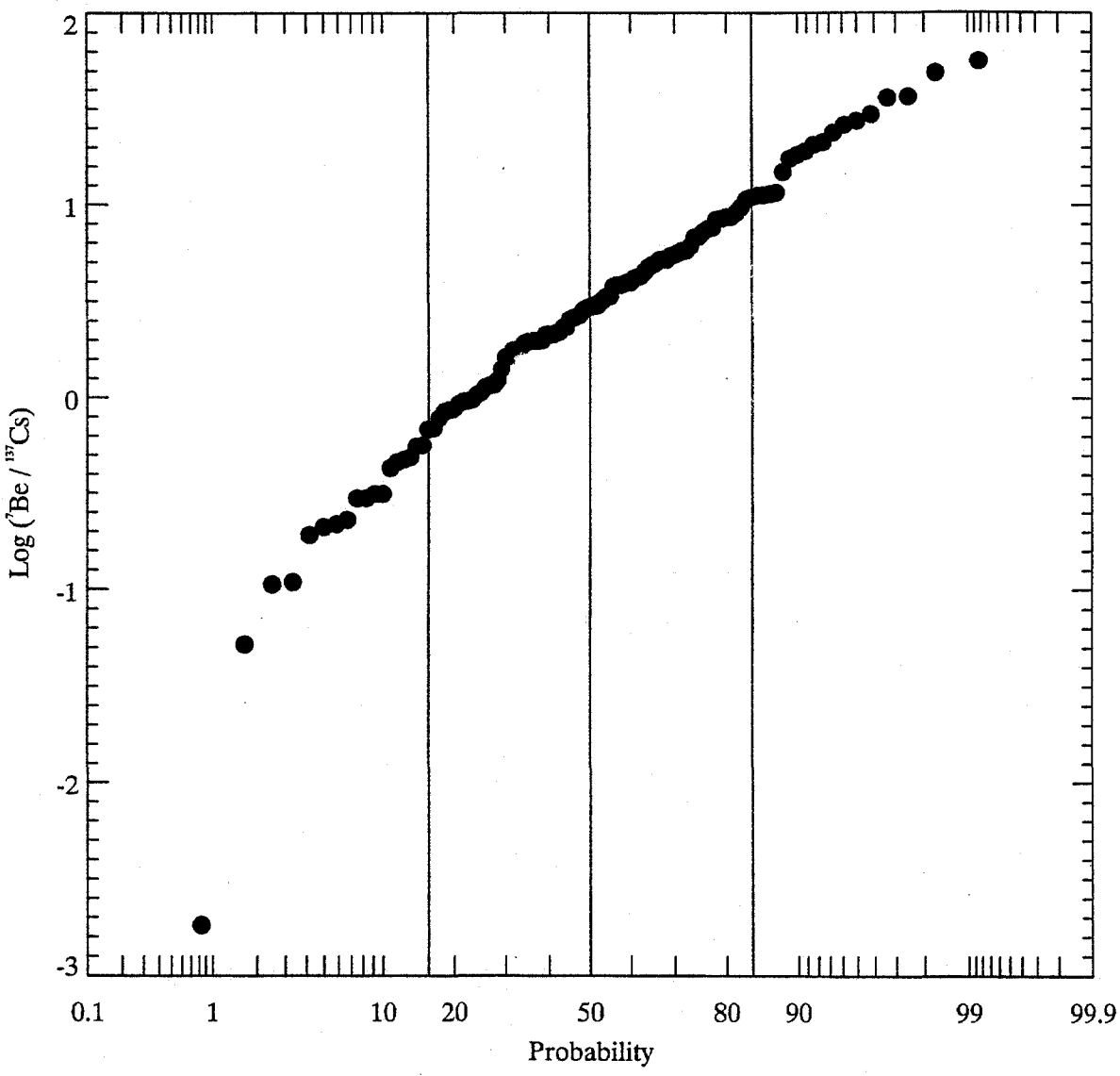



Figure 5-4. Probability distribution for the log of the ratio of $^{7}$Be to $^{137}$Cs for USEPA Low Resolution Cores.

MJW - H:\GENhud\documents\reports\2000usepa_responsiveness_review\figures\fig_5_04_be_cs_probdist.pro
Tue Dec 05 17:31:01 2000

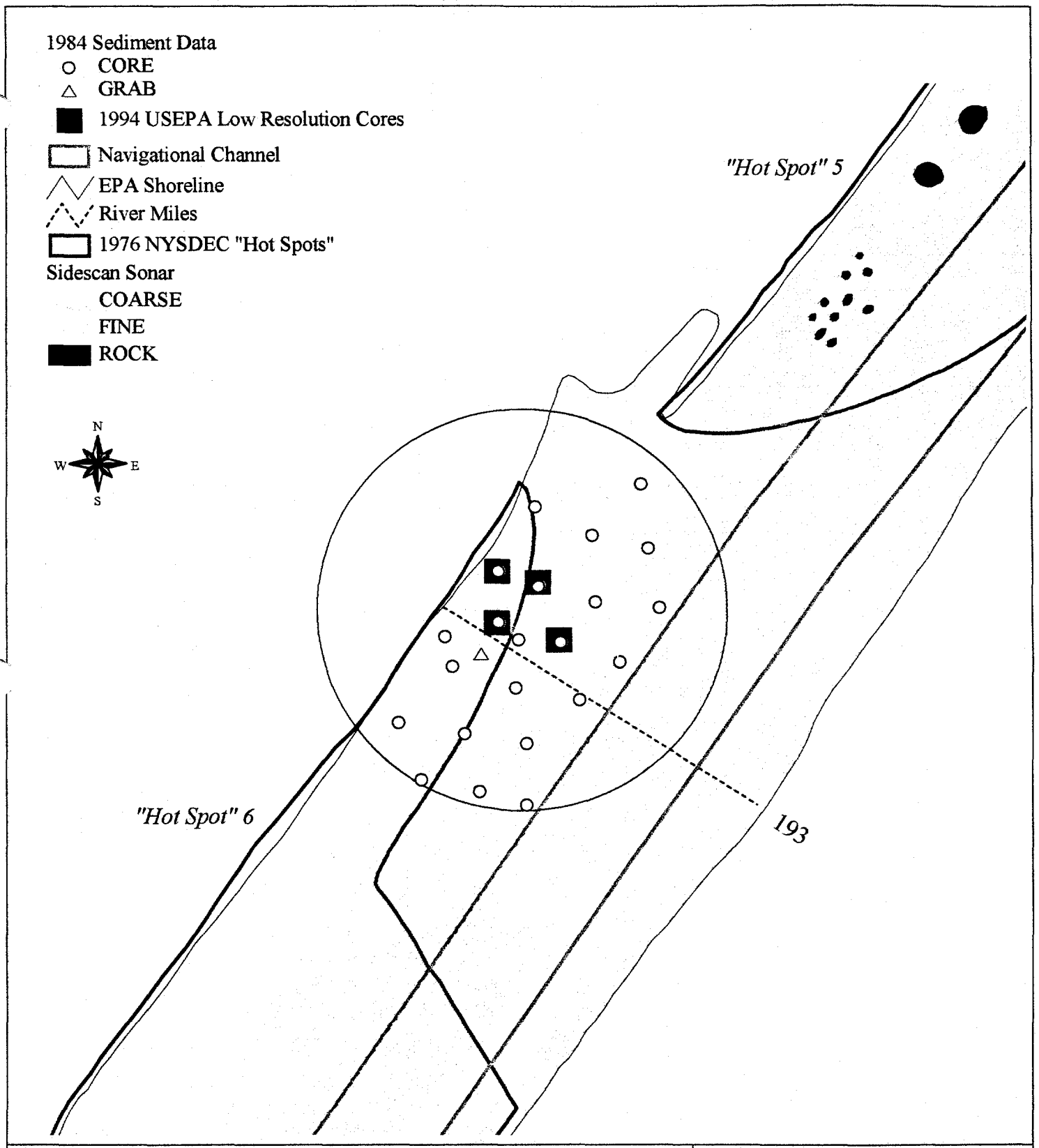



Figure 6-1. Locations of 1994 USEPA Low Resolutions Cores and 1984 NYSDEC sediment cores and grabs used for area based mass calculations in the LRCR Responsiveness Summary (Cluster LR-14).

SCALE

100 0 100 200 Feet

Quantitative Environmental Analysis, LLC

*\\shadowfax\q_drive\genhud\gis\projects\mpa_cluster_analysis\lr-14.apr*

GENhud 131 December 2000

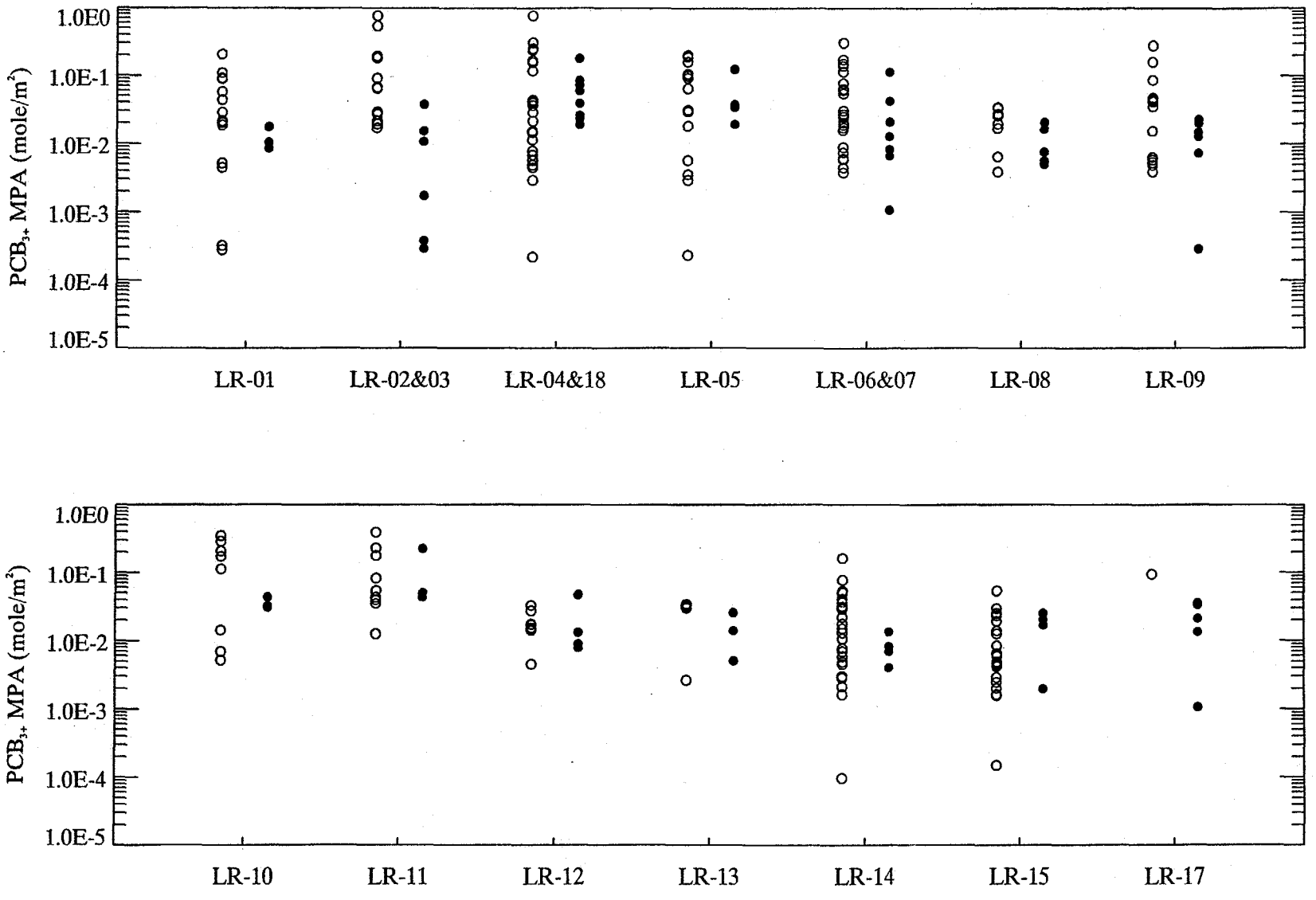



Figure 6-2. Comparison of calculated mass for 1984 and 1994 samples within USEPA clusters used for area-based mass calculations in the LRCR Responsiveness Summary.

# APPENDIX A

# LOOKUP TABLES FOR LOCATING GE'S COMMENTS AND USEPA'S RESPONSES FOR THE DEIR AND LRCR

| Table A-1. Lookup Table for Comments and Responses to USEPA's Data Evaluation and Interpretation Report | | | |
|---|---|---|---|
| **Comment Number** | **EPA DEIR Document Section** | **GE Comment Document Page** | **EPA Responsiveness Summary Page** |
| DG-1.4E | General | 7 | DEIR-1 |
| DG-1.13 | General | 31 | DEIR-1 |
| DG-1.1 | Executive Summary | 2 | DEIR-5 |
| DG-1.2 | Executive Summary | 3 | DEIR-6 |
| DG-1.3 | Executive Summary | 4 | DEIR-6 |
| DG-1.4 | Executive Summary | 5 | DEIR-7 |
| DG-1.23 | 3.1 | E-1 | DEIR-16 |
| DG-1.24 | 3.2.3 | E-4 | DEIR-24 |
| DG-1.9 | 3.2.6 | 22 | DEIR-26 |
| DG-1.10 | 3.2.6 | 23 | DEIR-27 |
| DG-1.10A | 3.2.6 | 25 | DEIR-27 |
| DG-1.11 | 3.2.6 | 26 | DEIR-28 |
| DG-1.15 | 3.2.6 | 36 | DEIR-28 |
| DG-1.15A | 3.2.6 | 37 | DEIR-28 |
| DG-1.15B | 3.2.6 | 38 | DEIR-30 |
| DG-1.15C | 3.2.6 | 39 | DEIR-30 |
| DG-1.15D | 3.2.6 | 39 | DEIR-33 |
| DG-1.16 | 3.2.6 | 42 | DEIR-33 |
| DG-1.4A | 3.2.7 | 7 | DEIR-36 |
| DG-1.4B | 3.2.7 | 7 | DEIR-36 |
| DG-1.4C | 3.2.7 | 7 | DEIR-36 |
| DG-1.4D | 3.2.7 | 7 | DEIR-37 |
| DG-1.17 | 3.3.3 | 46 | DEIR-38 |
| DG-1.22 | 3.3.3 | E-1 | DEIR-41 |
| DG-1.12 | 3.3.4 | 30 | DEIR-43 |
| DG-1.14 | 3.3.4 | 34 | DEIR-43 |
| DG-1.18 | 4.3 | 51 | DEIR-51 |
| DG-1.19 | 4.3 | 52 | DEIR-52 |
| DG-1.21 | 4.3 | 53 | DEIR-56 |
| DG-1.26A | 4.3 | E-9 | DEIR-57 |
| DG-1.26B | 4.3.1 | E-10 | DEIR-58 |
| DG-1.26C | 4.3.1 | E-10 | DEIR-58 |
| DG-1.26D | 4.3.1 | E-11 | DEIR-58 |

| Table A-1. Lookup Table for Comments and Responses to USEPA's Data Evaluation and Interpretation Report | | | |
|---|---|---|---|
| **Comment Number** | **EPA DEIR Document Section** | **GE Comment Document Page** | **EPA Responsiveness Summary Page** |
| DG-1.26E | 4.3.1 | E-11 | DEIR-59 |
| DG-1.26F | 4.3.1 | E-11 | DEIR-59 |
| DG-1.26G | 4.3.1 | E-12 | DEIR-59 |
| DG-1.26H | 4.3.1 | E-12 | DEIR-59 |
| DG-1.26J | 4.3.2 | E-14 | DEIR-60 |
| DG-1.26J1 | 4.3.2 | E-14 | DEIR-60 |
| DG-1.26K | 4.3.2 | E-15 | DEIR-60 |
| DG-1.26L | 4.3.2 | E-15 | DEIR-61 |
| DG-1.26M | 4.3.2 | E-16 | DEIR-61 |
| DG-1.26N | 4.3.2 | E-16 | DEIR-61 |
| DG-1.26O | 4.3.2 | E-17 | DEIR-61 |
| DG-1.26P | 4.3.2 | E-17 | DEIR-62 |
| DG-1.26Q | 4.3.2 | E-18 | DEIR-62 |
| DG-1.20 | 4.3.2 | 54 | DEIR-63 |
| DG-1.26R | 4.3.2 | E-18 | DEIR-63 |
| DG-1.5 | 4.4 | 11 | DEIR-71 |
| DG-1.6 | 4.4 | 13 | DEIR-72 |
| DG-1.7 | 4.4 | 16 | DEIR-72 |
| DG-1.8 | 4.4 | 18 | DEIR-73 |
| DG-1.26I | 4.5 | E-13 | DEIR-74 |
| DG-1.25C | A.5.2 | E-8 | DEIR-96 |
| DG-1.25A | A.5.2 | E-5 | DEIR-97 |
| DG-1.25B | A.5.2 | E-6 | DEIR-98 |

| Table A-2. Lookup Table for Comments and Responses to USEPA's Low Resolution Coring Report | | | |
|---|---|---|---|
| **Comment Number** | **EPA LRC Document Section** | **GE Comment Document Page** | **EPA Responsiveness Summary Page** |
| LG-1.41 | General | B-(all pages) | LRC-3 |
| LG-1.9 | 2.1 | 15 | LRC-5 |
| LG-1.29 | 2.1 | A-1 | LRC-8 |
| LG-1.30 | 2.2.1 | A-1 | LRC-8 |
| LG-1.21 | 2.3.1 | 47 | LRC-9 |
| LG-1.32 | 2.4.1 | A-1 | LRC-19 |
| LG-1.38B | 2.4.1 | A-4 | LRC-19 |
| LG-1.5C | 3.1 | 10 | LRC-25 |
| LG-1.25 | 3.1 | 52 | LRC-25 |
| LG-1.26 | 3.1 | 52 | LRC-25 |
| LG-1.27 | 3.1 | 53 | LRC-28 |
| LG-1.31 | 3.1 | A-1 | LRC-28 |
| LG-1.33 | 3.1 | A-1 | LRC-28 |
| LG-1.23 | 3.2 | 48 | LRC-29 |
| LG-1.39C | 3.1 | A-6 | LRC-29 |
| LG-1.37 | 3.2 | A-3 | LRC-30 |
| LG-1.1 | 4 | 3 | LRC-32 |
| LG-1.38A | 4 | A-4 | LRC-32 |
| LG-1.2 | 4.1 | 4 | LRC-33 |
| LG-1.3 | 4.1 | 6 | LRC-36 |
| LG-1.4D | 4.1 | 8 | LRC-36 |
| LG-1.8 | 4.1 | 15 | LRC-36 |
| LG-1.10 | 4.1 | 18 | LRC-37 |
| LG-1.20 | 4.1 | 46 | LRC-38 |
| LG-1.2A | 4.1.1 | 5 | LRC-44 |
| LG-1.4A | 4.1.2 | 7 | LRC-46 |
| LG-1.4B | 4.1.2 | 7 | LRC-46 |
| LG-1.4C | 4.1.2 | 8 | LRC-47 |
| LG-1.4E | 4.1.2 | 8 | LRC-47 |
| LG-1.5E | 4.1.2 | 11 | LRC-48 |
| LG-1.11 | 4.1.2 | 20 | LRC-48 |
| LG-1.13 | 4.1.2 | 24 | LRC-49 |
| LG-1.14 | 4.1.2 | 26 | LRC-49 |
| LG-1.15 | 4.1.2 | 28 | LRC-50 |
| LG-1.16 | 4.1.2 | 31 | LRC-51 |
| LG-1.17 | 4.1.2 | 32 | LRC-51 |
| LG-1.17A | 4.1.2 | 33 | LRC-52 |
| LG-1.17B | 4.1.2 | 34 | LRC-52 |

| Table A-2. Lookup Table for Comments and Responses to USEPA's Low Resolution Coring Report | | | |
|---|---|---|---|
| **Comment Number** | **EPA LRC Document Section** | **GE Comment Document Page** | **EPA Responsiveness Summary Page** |
| LG-1.19A | 4.1.2 | 44 | LRC-53 |
| LG-1.19B | 4.1.2 | 45 | LRC-53 |
| LG-1.22 | 4.1.2 | 48 | LRC-55 |
| LG-1.28 | 4.1.2 | 54 | LRC-58 |
| LG-1.38E | 4.1.2 | A-4 | LRC-60 |
| LG-1.38H | 4.1.2 | A-5 | LRC-60 |
| LG-1.38J | 4.1.2 | A-5 | LRC-61 |
| LG-1.40B | 4.1.2 | A-7 | LRC-61 |
| LG-1.12 | 4.2 | 23 | LRC-62 |
| LG-1.38D | 4.2 | A-4 | LRC-63 |
| LG-1.38F | 4.2 | A-4 | LRC-64 |
| LG-1.38G | 4.2 | A-5 | LRC-64 |
| LG-1.40C | 4.2 | A-7 | LRC-64 |
| LG-1.34 | 4.2.1 | A-2 | LRC-65 |
| LG-1.35 | 4.2.1 | A-2 | LRC-65 |
| LG-1.36 | 4.2.1 | A-2 | LRC-65 |
| LG-1.5B | 4.2.3 | 9 | LRC-70 |
| LG-1.5D | 4.2.3 | 10 | LRC-70 |
| LG-1.5F | 4.2.3 | 11 | LRC-70 |
| LG-1.6 | 4.2.3 | 13 | LRC-71 |
| LG-1.7 | 4.2.3 | 14 | LRC-72 |
| LG-1.38C | 4.2.3 | A-4 | LRC-72 |
| LG-1.38I | 4.2.3 | A-5 | LRC-73 |
| LG-1.39B | 4.2.3 | A-6 | LRC-73 |
| LG-1.40A | 4.2.3 | A-7 | LRC-74 |
| LG-1.5A | 4.2.4 | 9 | LRC-78 |
| LG-1.18 | 4.2.4 | 36 | LRC-78 |
| LG-1.18A | 4.2.4 | 40 | LRC-83 |
| LG-1.18B | 4.2.4 | 42 | LRC-85 |
| LG-1.18C | 4.2.4 | 43 | LRC-85 |
| LG-1.39A | 4.2.4 | A-6 | LRC-85 |
| LG-1.24 | 4.4.2 | 50 | LRC-86 |

# APPENDIX B

# DR. PAUL SWITZER'S COMMENTS ON USEPA'S LRCR RESPONSIVENESS SUMMARY

# COMMENTS ON
# USEPA RESPONSIVENESS SUMMARY
# FOR VOLUME 2C-A LOW RESOLUTION SEDIMENT CORING REPORT

**Paul Switzer**

Responses to my earlier comments were disappointing. Here are general observations, which are illustrated later in this document.

1. Responses frequently assert that assert that "geochemical knowledge" was used to justify unwise statistical procedures, particularly in the design of the 1994 survey. Good statistical practice is needed to draw inferences from sample data and requires carefully designed sampling procedures which are free of bias and purposive selection. Statisticians are distrustful of the "I know better" arguments.

2. Some responses that invoke statistical concepts are inarticulate and meaningless as understood by statisticians, suggesting that responsibility for replying to my earlier questions and criticisms may not have been entrusted to professional statisticians. This is disheartening and reveals a misunderstanding about the central role of statistical inference when drawing conclusions from survey data.

3. Responses often acknowledged the validity of my earlier criticisms but argued that other evidence nevertheless supports the same conclusions. My criticisms were based on what EPA had presented as its argument and it does not seem scientifically balanced to later pick and choose which of EPA's earlier findings should then be de-emphasized.

4. Responses often acknowledged the conceptual validity of my criticisms but then claimed, with *no substantiation*, that they are not of practical importance. My original critique addressed statistical issues that were central to USEPA's conclusions.

**The attached comments use the indexing scheme presented in USEPA's Responsiveness Summary.**

LG-1.29

This point dealt with the important issue of *which* 1984 sampling sites should be selected for matching in the 1994 sampling survey. I made the point that a statistical design was absent. The response states, *inter alia*, that samples were placed close together because spatial correlation was clearly evident. This is exactly the opposite of what should be done when spatial correlation is present, as any geostatistician will tell you. The response also dwells on the need to do matching which is completely beside the point of my criticism.

LG-1.30

The response states that dredge boundaries were made available after the sampling was completed which reinforces my point of purposive combination of data, as illustrated in the comparison of Figure 4-21 with Figure 4.22.

LG-1.31

Nearly 40% of the collected data were excluded from the analysis, using criteria that could be related to the presence or absence of removal. The response invokes "knowledge of geochemical processes" for the data exclusion criteria that were used.

LG-1.32

I had criticized exclusion of low concentration data and trimming of data to achieve desired distributional shapes. The response emphasizes my original point in saying "The exclusion of low-level samples..is an attempt to exclude samples wherein the expected relationships are unlikely to apply." An astonishing response, indeed.

LG-1.33

Here I was comparing Figure 3-2 with Figure 3-8 to demonstrate how data selection can change weak associations to strong associations. The confusing response states "The analysis was done to confirm an already proven relationship", which is off the point and raises the question of the need to confirm a *proven* relationship.

LG-1.34

The response acknowledges the validity of my criticism of the SSW correction for comparing the early and later surveys, but claims that "there was no other basis to establish the sediment density". Well, does this make it good?

LG-1.35

I criticized the failure to obtain cross-calibration information to assure that the earlier and later surveys were using the comparable yardsticks. The response excuses this lack by stating that "reconstruction of the original techniques is difficult". I'm not sure if this is an answer or an apology.

LG-1.36

I referred to the unaccounted error associated with extrapolating grab samples to 12-inch depth. The response acknowledges the difficulty but states that "it is unlikely that the main conclusion.. will be directly affected". How do we know?

LG-1.37

This point refers to the "regression fallacy" wherein the resampling of high concentration sites is likely to result in lower concentrations, in the absence of removal processes, when we have either measurement error or short-scale spatial variability. The response clearly acknowledges the fallacy but adds a considerable amount of obfuscating material, such as the need to resample hot spots, which has nothing to do with the regression fallacy.

The response also implies that areas sampled in 1994 show less short-scale spatial variability compared with the more comprehensive 1984 survey. The response cites, for example, that in for these restricted areas the spatial correlation at 10-foot separation is 0.47, a number derived from fitting gaussian correlation model. In the first place such a correlation could be regarded as low rather than high. Second, a straightforward empirical correlation should have been reported rather than a model-based correlation. Third, Table A-1 of the Responsiveness Summary indicates substantial local variability ["nugget effect"] for most subreaches. Finally, the response states that the regression fallacy "could result in a slight high bias in the estimated amount of mass loss". Whether the bias is slight or severe is not documented by any specific statistical calculation.

LG-1.38A

I had pointed out the sensitivity of the lognormal hypothesis and its MVUE. The response states that "minor deviations from a true log-normal distribution introduce minor errors". On the contrary, the MVUE is not robust to departures from lognormality. The response also contains meaningless statements such as "the use of the MVUE is justified in light of the greater probability that the underlying population is log-normal" [greater than what?]. Another example is "all of the hot-spots have a rather high probability of a log-normal distribution" which shows a lack of understanding of the meaning of significance probabilities and reverses its intended usage.

LG-1.38B

The error that I pointed out in the estimating equations is acknowledged.

LG-1.38C

While the respondent agrees that ancillary parameters are potentially important, it is argued that such data were not used in the analysis because they were not available for portions of some data sets. If, indeed, ancillary parameters are important, then they will confound the PCB comparisons, i.e. observed PCB differences over time could then be largely due to differences in ancillary parameters over time. The opportunity to investigate confounding possibilities, in situations where the data were indeed available, was ignored, and the confounding possibilities are not considered or discussed.

LG-1.38D

I criticized the reporting of simple correlations without regard for the information provided by potentially confounding variables. The response acknowledges that such a multivariate approach to correlations "would be interesting to complete" but argues that it would be peripheral. Then, why were the simple correlations presented in the first place? Referencing related computations in other publications does not remove the burden of doing professional statistical analyses for these data.

LG-1.38E

I criticized an unsubstantiated use of a factor of 2 to judge significant differences. The response states that this is an empirical observation derived from some statistical comparisons that was extrapolated to other comparisons.

LG-1.38F

The response agrees with my criticism that comparisons based on upper confidence limits can reflect differences not related to mean PCB levels but nevertheless repeats the meaningless comparisons. The only offered justification is the claim that the 1991 Phase I report provides only 95% confidence limits.

LG-1.38G

My comment noted that an assessment of variability components was absent, which the respondent agreed would be probably interesting, but not necessary, in light of their assumption that the total variance is reflected in the standard error estimates. However, an assessment of the regression fallacy bias, for example, could only be understood properly with an appropriate analysis of variability.

LG-1.38H

I criticized the failure to address the sensitivity of the delta-M ratio to the arbitrary addition of the number 2. The response claims that there is no effect on the shape of the distribution but certainly it affects every statistic derived from the distribution including means, probabilities, etc.

LG-1.38I

I noted that geostatistical methods were not used, as they should have been, in the estimation of PCB inventories and their associated uncertainty, and no account was taken of the spatial configuration of the sample cores. The response does not address these points. The fact that only hot spots were considered in 1994 where the spatial correlation is claimed to be stronger only enhances the need for geostatistical methods.

LG-1.38J

I criticized the pervasive absence of statistical measures of uncertainty resulting from the combined effects of sampling, measurement error, and interpolation. The response agrees with this criticism but argues that including such information would make tables too complicated! [The response cites an example where uncertainty information for Table 4-8 is presented with Table 4-7, but this information is not the relevant information for Table 4-8.]

LG-1.39A

The response acknowledges that the results of the statistical analyses of the $^{7}Be$ data are inconclusive, and merely states that, taken as a whole, the results are consistent with the anticipated trend. No further statistical analysis is offered to support this last statement whose meaning is itself unclear.

LG-1.39B

I pointed to the anomaly where no significant PCB loss was found and, on the other hand, a computation based on a ratio index did find statistical significance. The response acknowledges the anomaly but argues that the latter method is better. However, ratio-based statistics can be much worse due to their poor statistical robustness, for example when small differences are turned into huge ratios.

LG-1.40A

I stated that there was no statistical evidence offered to support the unequivocal conclusion regarding lack of burial. My criticism also noted that the depth of the peak concentration in the low-resolution cores could not be established with sufficient precision to decide the issue of burial. The response, which refers to other conjectures regarding the burial issue, did not address my criticisms involving inferences drawn from the low-resolution cores.

LG-140.B

The response does not dispute that the fraction of sample locations with PCB decreases is not significantly different from 50%. Instead, the response points to the new Appendix A where the same numbers are reconfigured based on grouping of sample locations into clusters. See my comment below on the new Appendix A.

LG-140C

I stated that many far-reaching statements were not supported by statistical arguments. The response was that these far reaching statements were somehow geochemical facts. However, these geochemical facts such as the presumed scouring and redistribution of sediments beg the very questions that the surveys were supposed to address. For example, none of the statistical tests were tests about redistribution, yet this is how the test results were unequivocally interpreted.

## Comments on Appendix A

The new Appendix A assigns the original sampling locations to 14 "areas" and tries to make comparisons between 1984 and 1994 for whole areas. For each area, sample means are calculated for the two surveys and the ratio of the two means is reported. Statistical inference is then based on the average of these 14 ratios, treating them as a sample of 14 numbers from some distribution. See Table A-9, for example. The whole exercise is absurd.

1. The definition of areas was done *post-survey* and opens the door to yet another kind of purposive manipulation of data. It seems that this was done to overcome the inconclusive results of the original paired-location sampling design. Such agglomeration of the data means that a large difference at one or a few paired-locations could swamp smaller and insignificant differences at all other paired-locations within the same area, vitiating the whole point of the matched pairs survey design.

2. Using sample data to represent a geographic area requires a proper geostatistical analysis which takes spatial correlation into account, and provides an estimate of how well the sample data represent the area in which they are located. Appendix A gives simple averages of data from sample locations, ignores the clustering of data locations within areas, and provides no measures of estimation error.

3. Ratios are not robust statistics when denominators are subject to appreciable percentage errors, as is the case here.

4. The number of sample locations in each of the areas varies between 2 and 30. No account was taken that some mean estimates have much higher precision than others. The analyses appear to treat the ratios from each of the 14 areas on an equal footing. The area with the largest number of sample locations shows a 1994 mean that is *larger* than the 1984 mean. This area receives the same weight as an area with 2 sample locations in the calculation of confidence limits.

5. Confidence limits are reported in Table 6 and Table 9 but for which statistical parameter? These can only be confidence limits for the mean of some population from which the 14 area ratios are a random sample. What could this population be, and how can we regard these 14 ratios as a random sample?

6. There are mathematical inconsistencies between assuming a lognormal distribution for individual sample location data, a lognormal distribution for an area average, and a normal distribution for ratios, whatever the silly "1.2" rule may say.

7. The use of a statistical test [Shapiro-Wilks] to test whether the 14 numbers were sampled from a normal distribution or a lognormal distribution is a waste of time. They were sampled from neither. Furthermore, this statistical test has very little power when used with 14 numbers, so that larger significance probabilities should not be read as indicative of a good fit.

75 Pearce Mitchell Place
Stanford, CA 94305
August 24, 1998

John Haggard
General Electric Company
1 Computer Drive South
Albany, NY 12205

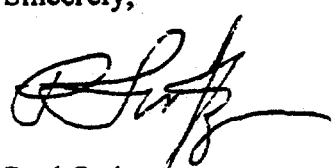
**RE: HUDSON RIVER**

Dear John,

I read the report material which you sent to me relating to Hudson River PCBs, viz., Volume 2C-A Book 1 and Book 2 *Low Resolution Sediment Coring Report* by TAMS *et al.*, dated July, 1998, prepared for the U.S.E.P.A., together with portions of Volume 2C *Data Evaluation and Interpretation Report*, by the same authors, dated February, 1997, containing material related to geostatistical analyses of the 1984 Hudson River PCB data. I also have seen the two reports you sent to me on geostatistical analyses prepared by Davis and Olea for General Electric Company, dated December 14, 1990, and February, 25, 1991 .

In my view, there are a number of important lapses both in the design of the Hudson River PCB study and the analyses of the data that are briefly described in my report accompanying this letter. In short, many of the stated conclusions of the July, 1998, report by TAMS *et al.* are not well supported by the reported statistics. Rather, these conclusions seem to be drawn from interpretations and conjectures that do not have a statistical inferential basis.

Sincerely,



Paul Switzer

# COMMENTS ON

# 1998 U.S.E.P.A
# LOW RESOLUTION SEDIMENT CORING REPORT

**prepared by**

**PAUL SWITZER**

**August, 1998**

## ISSUES OF DATA SELECTION

1. Since not all core locations from the earlier surveys were to be resampled in 1994, care should be taken that those selected for the 1994 survey are representative of the zones for which PCB inventory estimates are desired. For example, a form of random or systematic selection or areally stratified random selection would remove the possibility of selection bias. Instead, the sampling method used is a poorly documented form of what statisticians call "purposive" sampling – here a misguided attempt to create zones of reduced sediment variability at the expense of unbiased representation of the 1984 sample locations.

2. Seven hot spots were defined by combining certain of the original twelve dredge locations from the 1984 survey. It is not clear whether the decision to combine dredge locations was made before or after the 1994 data were available. If after the data were available, then another source of selection bias is created because combining data can be used to purposively exaggerate statistical significance. This effect can be seen, for example, by comparing Figure 4-21 with Figure 4-22.

3. Not all of the 1994 data that were collected were used in the statistical estimates. An important issue is the removal of nearly 40% of the data because of potential cross-contamination from higher to lower segments of a core. The severe data rejection rule conforms to *a priori* ideas of what a core sequence should look like rather than relying on explicit evidence of cross-contamination. Because the fraction of data removed from the analysis is large, the susceptibility to selection bias is also large. Calculations showing that some statistics are not seriously affected by this form of data screening do not adequately address this issue.

4. It was reported that the lowest concentration data were excluded from "many of the subsequent analyses" [page 2-16] so that the remaining data would look like a lognormal distribution. The fact that the low concentration data are hard to quantify does not change the fact that they are nevertheless informative. Trimming data to achieve a desired distributional shape violates important statistical principles related to selection bias.

5. As an example of how purposive data selection can create an impression of a statistical relationship compare the weak associations in Figure 3-2 with the strong associations indicated by the selected data subset of Figure 3-8. This example shows clearly the perils of censoring data rather than modeling data.

## CALIBRATION ISSUES

1. The SSW correction [see Figure 4-17 and Table 4-3] applied to the 1984 data is itself based on the PCB measurements. The net effect is to introduce an implicit non-linear rescaling into the PCB data which is difficult to analyze. This dry soil weight SSW factor should be based on some physical parameter other than PCB itself. Furthermore, stronger evidence is needed to demonstrate that such a recalibration of the 1976-78 data makes them comparable to the 1994 data.

2. The measurement methods for the 1976-78 survey, the 1984 survey, and the 1994 survey are different [see comments on page 4-21, for example]. However, there are no cross-calibration data presented which analyze the same samples by both methods. By implication, the subsequent statistical analyses assumed that relative calibration errors were strictly zero. How could the 1994 study have been undertaken without such calibration information?

3. The extrapolation of grab samples to make them comparable to 12-inch core segments is not well documented in the report. The "factor" used for extrapolation appears to be something like the mean PCB ratio between the top four inches and the top 12 inches seen in core samples [page 4-21]. Of course, such extrapolation introduces additional error beyond the measurement error associated with core sampling but the extrapolation error is not accounted for in the analysis.

## THE REGRESSION FALLACY

Surprisingly, the report does not recognize the "regression" fallacy. The report makes much of the observation that lower 1984 concentration locations tended to be followed by later increases while higher 1984 concentrations tend to be followed by decreases. This kind of analysis is found throughout the report without any recognition of the "regression-toward-the-mean" effect: for *any* collection of paired measurements which are positively correlated, which these data emphatically are, higher initial concentrations will tend to show a decrease on average and lower initial concentrations will tend to show an increase on average -- *in the absence of any distributional changes*! [See Fig 4-11 to Fig 4-16 for example, as well as much of Section 4 and statements in the Executive Summary]. This fallacy may be understood by simply supposing that the true concentrations were *exactly* the same in 1984 and 1994 and that reported concentration differences were due entirely to analytic variability. Even in this case, splitting the data into two parts as the authors have done would demonstrate that higher 1984 concentrations tend to be followed by lower 1994 concentrations and vice-versa.

The geostatistical evidence from the 1984 survey shows that very short-scale spatial variability is often comparable to total variability [see Figures 4-9 to 4-12 and 4-14 to 4-16 in the 1997 report]. This fact, by itself, can be used to show that even small location errors induce a large spurious regression effect that would account for most of the reported 1994 PCB decrease at locations with high 1984 PCB.

## ISSUES RELATED TO STATISTICAL CALCULATIONS

1. The MVUE method for estimation of means requires an exact lognormal model. Otherwise the method is biased. For example, the MVUE is sensitive to procedures for handling low concentration data. Censoring and BDL data create special problems for the MVUE method. The straightforward sample average is less sensitive to prior assumptions about the distribution of the data.

2. The lognormal unbiasedness correction was inconsistently applied. For example, compare formulas on page 4-28 and in Table 4-13. In the latter case, the supposed MVUE ["minimum variance unbiased estimate"] for the near-shore sediments is neither unbiased nor minimum variance, even if the data were sampled from a precisely lognormal population distribution.

3. While data were collected on covariate parameters such as grain size, the estimates of PCB inventory change did not incorporate the covariate information in any statistical model. It is not clear where or how the relationship between fine grain sediments and elevated PCB was used to improve estimates of PCB inventory [page 3-34].

4. Simply reporting correlations between individual covariate parameters and PCB measures, without regard to the multivariate nature of the data, does not take advantage of the potentially more powerful but widely used multivariate statistical methods such as partial correlation or multiple regression analysis. See the discussion on page 3-15 as an example. Failure to examine the multivariate statistics can also lead to mistaken interpretations such as that found on 3-19 "The correlation of ΔMV and MDPR values is largely a ramification of the correlation with total PCB" , as well as a similar confused statement about correlations on page 3-25.

5. Hot spot PCB quantity differences between the 1976-78 and 1994 surveys are assessed by the ratio $\Delta_t$ described on page 4-40. The explicit claim is that differences "of more than a factor of two are considered significant and likely to be beyond the level of uncertainty". I could find absolutely no statistical basis for this claim and no supporting analysis!

6. Upper 95% upper confidence limits were used to compare sediments in Table 4-13. Differences among these upper limits may be due to mean differences, variability differences, or differences in the number of samples, or some combination of these. It is misleading to make such comparisons.

7. The report contains no statistical analysis of variance components that could be used to make better judgements regarding the significance of observed differences. Some examples of large random components of variability include the following. On Page 2-18 we read that split-pair samples differed by 36% on average. Table A-3 suggests that field replicate-pair congener analyses differ by more than 20% about half the time. Geostatistical studies of the 1984 survey report that "it was not unusual for samples taken only a few meters apart horizontally to exhibit order-of-magnitude differences in PCB concentrations" [page 4-25, 1997 report]. The reported variograms indicate that very short-scale spatial variability in sediment PCB is often comparable to the total variability observed between different sample locations. Without adequate accounting for random error components, it is hard to appreciate reported differences between the earlier and later PCB surveys.

8. Statistical analyses of the $\Delta_M$ ratio measure of PCB molar inventory change are based on adding the arbitrary constant 2.0 to all ratios and then taking the logarithm. It is not reported how sensitive are the analyses to this particular fudge factor 2.0 . However, it is likely that the low end of the distribution is noticeably affected by this added factor, thereby affecting statistics in the context of a lognormal model.

9. It appears that zonal PCB inventory estimates were obtained simply by multiplying averages of core-based concentrations [$g/m^2$] by estimates of zonal areas. These estimates take no account of the spatial configuration of the cores within zones and make no use of the geostatistical considerations. Although the 1994 data are not sufficient to develop the appropriate geostatistics, the geostatistical models based on the more extensive 1984 survey could have and should have been used.

10. Estimates of PCB inventory, inventory changes, and dechlorination losses [see Table 4-8 as an example] are not accompanied by statistical estimates of uncertainty. Good statistical practice dictates the quantification of uncertainty – first as a measure of the adequacy of the underlying information and second as a tool in rational decision making. The uncertainty associated with inventory estimates derives from sampling variability, measurement error, and spatial interpolation errors, all of which propagate to the final inventory estimates.

## INCONSISTENCIES

1. The $^{7}Be$ data sometimes did, and sometimes did not show association with PCB levels or imputed scour of PCB, depending on how the data were divided or otherwise manipulated. See pages 3-23 and 4-38 for example. Nevertheless, the Executive Summary states categorically that "beryllium-7 was shown to be a statistically significant indicator of inventory loss" [ES-3]. For hot spots the report states that " [$^{7}Be$] evidence for recent deposition apparently contradicts the strong PCB profile evidence for long-term loss sediment loss or lack of burial" [page 4-35].

2. It is reported that the molar inventory changes between the 1984 and 1994 surveys [page 4-14] indicate no statistically detectable trend. However, the report also claims that a relative measure of molar change, $\Delta_M$ , shows statistically detectable decreases, probably reflecting the regression fallacy described earlier. In this connection, the report makes the inaccurate statement on page 4-15 that for relative measures of change "much of the analytic variability can be diminished in importance relative to real change".

3. The high-resolution 1984 survey did not confirm that the amount of implied dechlorination is proportional to total PCB or that it followed the same dechlorination pattern as the low-resolution 1994 data [pages 3-5, 3-9, 3-35].

## CONCLUSIONS VERSUS DATA

1. Statistical analyses were not used to support frequent statements regarding lack of burial. Indeed, high resolution core data [Figure 4-24 is an example] indicate that the concentration peak can rise and fall sharply within a 9" or 12" segment, representing one or more decades of deposition. The fact that the top segment of a low-resolution core shows higher concentration than the succeeding segment does not, by itself, imply that the peak concentration is at the surface and that burial is absent. On page 3-10 the report itself states that "it is likely that a low-resolution core segment would span a range of two or more orders of magnitude...over a nine inch interval".

2. The simple "sign test" shows that the frequency of locations with PCB increases versus those with decreases is not statistically different from 50%. There are many examples, such as the comparisons in Figures 4-4 and 4-21. The point is that the limited size of the 1994 survey makes the results look much like the results of coin tossing. The non-statistical rationalizations in the report, however salient, should not be confused with statistical inference.

3. Many far-reaching statements in the report are not actually derived from a statistical analysis of the data, even though such non-statistical hypotheses and conjectures are intertwined with statistical reporting. Examples of such are statements referring to scour, resuspension or redistribution of sediments, dechlorination limits, underestimation of prior inventory, probable losses due to incomplete cores, and the relation of Aroclors to PCB concentrations.

December, 1997

# PAUL SWITZER
# CURRICULUM VITAE

## VITAL DATA

Birth Date: March 4, 1939
Birth Place: St. Boniface, Manitoba, Canada
Citizenships: U.S., Canada

## EDUCATION

University of Manitoba, B.A. (hons.), Mathematics, 1961;
Harvard University, A.M., Statistics, 1963;
Harvard University, Ph.D., Statistics, 1965.

## POSITIONS

Professor, Statistics Department, Stanford University, 1965-
Professor, School of Earth Sciences, Stanford University, 1965-
Visiting Professor, Singapore National University, 1997
Visiting Professor, University of Rome La Sapienza, Rome, 1992
Visiting Scientist, Goddard Institute for Space Studies, New York, 1991
Visiting Professor, Ecole des Mines, Fontainebleau, France, 1977 & 1988
Visiting Professor, Massachusetts Institute of Technology, 1984
Visiting Professor, E.T.H., Zurich, Switzerland, 1984
Visiting Scientist, C.S.I.R.O. Division of Mineral Physics and
Division of Mathematics and Statistics, Sydney, Australia, 1983
Visiting Fellow, Japan Society for Promotion of Science, Tokyo, 1983
Chairman, Statistics Department, Stanford University, 1979-82
Visiting Scientist, Environmental Protection Agency, Washington, 1981 & 1991
Visiting Professor, Statistics Department, Princeton University, 1974
Visiting Professor, Imperial College, University of London, 1973

## PROFESSIONAL ORGANIZATIONS

Institute of Mathematical Statistics
American Statistical Association
International Statistical Institute
Royal Statistical Society
International Association for Mathematical Geology
American Geophysical Union
Canadian Statistical Society

## EDITORSHIPS

Editor, Statistical Science, 1995-1997
Editorial Board, Chapman & Hall series "Interdisciplinary Statistics", 1992-95
Consulting Editor, Canadian Journal of Statistics, 1992-94
Editorial Advisory Board, Atmosphere Environment, 1992-
Editorial Board, Oxford University Press Series on Spatial Information Systems, 1989-
Editor, Journal of the American Statistical Association, 1986-88
Editor, Statistical Science, 1984
Statistics Editor, Littlefield, Adams Publishing Co., 1983-88
Associate Editor, Canadian Journal of Statistics, 1980-85
Associate Editor, Journal of the American Statistical Association 1975-78
Associate Editor, Mathematical Geology, 1974-76

## HONORS AND AWARDS

Presidential Invited Speaker, Canadian Statistical Society, 1996
Nomination, Phi Beta Kappa Excellence in Teaching award, 1996
Topical Lecturer, American Association for the Advancement of Science, 1994
Distinguished Achievement Medal, American Statistical Association,
Section on the Environment 1993
Presidential Neyman Lecturer, International Statistical Institute, 1991
Closing Plenary Lecturer, European Meeting of Statisticians, 1991
Fellow, International Statistical Institute, 1982
Fellow, American Statistical Association, 1980
Fellow, Institute of Mathematical Statistics, 1980
Best Paper Award, International Association for Mathematical Geology, 1980

## PUBLIC AND PROFESSIONAL SERVICE [since 1990, selected]

Research Proposal Reviews: NSF, Netherlands NSF, NSERC Canada, 1997
Promotion Review, Dept of Statistics, University of Washington, 1997
Program Review, Dept of Statistics & Operations Research, Kuwait University, 1996
Promotion Reviews, Stockholm University, Penn State University, 1996
National Advisory Council on Environmental Policy and Technology,
US Environmental Protection Agency, 1995-
Environmental Statistics Technical Advisory Committee,
American Statistical Association, 1995-97
Board of Directors, Societal Institute of the Mathematical Sciences,1995-
Committee on Global & Environmental Change, American Geophysical Union, 1993-
Student Conduct Legislative Council, Stanford University, 1993-95
Humanities & Sciences Council, Stanford University, 1993-94
Statistics Steering Committee, National Center for Atmospheric Research, 1993-
Promotion Reviews, University of Minnesota, Woods Hole Oceanographic Inst,
UC Santa Barbara, New York University, 1995
Promotion Reviews, New York University, Northeastern University,
University of Wisconsin, 1994
Promotion Reviews, University of Chicago, NYU, Washington, Arizona,
UC Berkeley, Virginia, 1993
Research Proposal Reviews, National Science Foundation,
SERC [UK], IMA, NOAA, others [continuing]
Editorial Reviews, Urban Atmosph., Oxford Univ Press, IEEE,
Math Geol, EPRI, others
Organizer, Session on Climate Patterns and Change, ASA Annual Meeting, 1995
Organizer, Session on Data Analysis for Physical Sciences, IMS Annual Meeting, 1994
Organizer, Session on Global Climate Change, Inst.Math.Statistics Annual Meeting, 1992
Scientific Committee, Fifth World Geostatistics Congress, 1992
Organizer, Workshop on Monte Carlo Methodologies in Human Exposure Analysis,
EPA 1992
Panel for Earth Sciences & Statistics, National Science Foundation, 1991
Program Committee, Institute of Mathematical Statistics, 1991-92
Organizer, 5th Internat. Meteorology & Statistics Congress, 1991-92
Review Panel, Mathematical Sciences Division Office of Naval Research, 1991
Climate Science Advisory Committee, EPRI, 1990-
Committee for Statistics in the Physical Sciences, Bernoulli Society, 1990-94
Organizer, AAAS Annual Meeting, Earthquake Prediction and Validation, 1990-91
Board Member, American Statistical Association, 1990
Organizer, Institute for Mathematics Applications,
Summer Program for Environment, 1989-92
Chair, Review Committee on Undergraduate Mathematical Sciences, Stanford Univ., 1989
Program Committee, 2nd World Congress Bernoulli Society, Upsalla, 1989-90

## SELECTED PUBLICATIONS [after 1990]

"Spatial Covariance Estimation for Monitoring Data" with C. Loader,
Statistics in the Environmental and Earth Sciences, 1991.

"Undiscovered Oil and Gas Resources:
An Evaluation of the Department of the Interior's 1989 Assessment Procedures"
[NRC panel], National Academy Press, 192pp, 1991.

"Spatial and Temporal Statistics for Environmental Problems",
Bull.Int.Statist.Inst. 1992 pp 315-328.

"Derivation of an Indoor Air Averaging Time Model from the Mass Balance Equation"
with W. Ott, J. of Exposure Analysis and Environmental Epidemiology, 1993, pp 113-135.

"Carbon Monoxide Exposures Inside an Automobile Traveling on an Urban Arterial Highway"
with W. Ott & N. Willits, J. Air & Waste Manage. Assoc. 1994 pp 1010-1018.

"Spatial Interpolation Errors for Monitoring Data" with G. Host & H. Omre,
J. Amer.Statist.Assn, 1995 pp 853-861.

"Time Trend Estimation for a Geogrpahic Region" with K. Solna,
J. Amer.Statis.Assn, 1996 pp 577-589.

"Ambient Sulfur Concentrations Near Grand Canyon as a Function of Fluctuating Loads
at the Mohave Power Project", with L. Enger et al.
Atmospheric Environment, 1996 pp 2552-2564.

"Differential Exposure Misclassification in Case-Control Studies
of Environmental TobaccoSmoke and Lung Cancer" with M. Levois,
Journal of Clinical Epidemiology 1998.