Model Validation:
Modeling Study of PCB Contamination In The Housatonic River

A Peer Review

By

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Introduction

Under agreements developed between the General Electric Company and the U.S. Environmental Protection Agency future designs of remedial activities intended to reduce exposure to PCB contaminated sediments within the Housatonic River will make use of a predictive numerical fate and transport model. This model, under development since 2000, is intended to provide quantitative measures of sediment and PCB transport and associated uptake by selected biota over a variety of spatial and temporal scales. Initially, the model has been applied to a region extending downstream from the confluence of the East and West Branches of the River (approximately 2 miles downstream of the GE facility in Pittsfield, Ma.) To Woods Pond Dam, a distance of approximately 10.7 miles. This Primary Study Area (PSA) is believed to contain 90% of the mass of PCBs present in the River. The PSA is a morphologically complex area which in combination with regional hydrology and placed control structures (i.e. dams) establishes a multi-faceted transport regime. The complexity of this system, representing a particular challenge, has lead to the development of a model consisting of three primary elements, a watershed model (HSPF), a hydrodynamic/sediment-contaminant transport model (EFDC) and a bioaccumulation model (FCM). As presently configured, the models are linked but not interactive.

Model development proceeded first through the conceptual phase (~2000-2002) and then through calibration (2002-2005). Over the past year emphasis has shifted to validation. This included a revised calibration effort in which the initial 14 month calibration was extended to a 10.5 year period (Jan.1990-June 2000), a number of model changes to better treat key processes associated with the complexity of the transport regime, and then validation over a 26 year period 1979-2004. Coincident with validation the model domain was extended in space to include a region upstream to Newell Street in Pittsfield, Ma (a distance of approximately 1.5 miles up the East Branch of the River from the Confluence) and a region downstream from Woods Pond to Rising Pond in Great Barrington, Ma. , a distance of 19 miles.

The results of the revised calibration and independent validation were released in a two volume report in March,2006 (Weston,2006) for public comment and review by a Peer Review Panel. The following provides a summary of my review of this report, supplementary materials provided by EPA associated with the Document Overview Meeting held in May, 2006, and review comments submitted by General Electric Contractors and concerned citizens.

General Comments

This has been and remains an ambitious project. In hindsight it may have been overly ambitious to expect a single model formulation to efficiently and accurately predict future PCB concentrations throughout a morphologically complex region over extended periods of time given what was known about historical distributions and the range of processes governing transport and fate. Some of this seems to have been recognized by EPA in their recent refinement of the modeling goals placing primary emphasis on the need for the model to be able to establish the relative performance of selected remedial alternatives rather than on its ability to yield certain prediction of absolute PCB concentrations (EPA,2006). While this refinement is advisable and will be taken into consideration in model evaluations it must be remembered that it does not relieve the modeler's responsibility to develop an efficient, stable and quantitatively accurate model. The evaluations to be conducted during the upcoming Corrective Measures Study (CMS) by GE will be considering the benefits of remedial alternatives over extended periods of time, often 40 years or more, i.e. times well in excess of the validation period. The utility of the relative comparisons over these extended periods will ultimately depend on the degree to which the model provides accurate simulation of all of the governing physical chemical and biological factors affecting transport and fate and the adequacy of the computational numerical schemes. Fundamentally, these are the same factors to be considered if the model was to be used for absolute predictions. The sufficiency to evaluate these characteristics depends in large part on our understanding of the PCB transport system structure and dynamics within the Housatonic River.

On several occasions discussions with the model group and within the Peer Review Panel made it clear that this required understanding of the variety of transport processes affecting PCB transport in the PSA was less than perfect. Recall the discussions of floodplain dynamics, an area known to represent a significant sink and possible longterm source of PCBs, the continuing debate over sidebank transport ,the specification of boundary conditions, and the proper structuring of the sediment bed within the model. Each of these items represent an essential element of the transport model and most will come up again individually in the following review of the model validation effort. Viewed collectively this variety of unknowns makes clear that what EPA and GE are dealing with is a research project rather than simple application of an accepted formulation. With this fact in mind the future role of the model should change to include guidance of monitoring efforts. The need for these additional data should be clear from a scientific standpoint. Their availability would also serve to increase stakeholder confidence in model results.

Monitoring to date has placed primary emphasis on PCB distributions within the sediment column with relatively limited sampling of the water column TSS and PCB concentrations. The latter have placed primary emphasis on flow/TSS relationships during high flow events with sampling at a number of selected transects along the main stem of the river. This monitoring has been supplemented by some few field observations of sidebank erosion and laboratory estimates of bed erodability using SEDFLUME. With the exception of the sidebank observations the majority of the field observations have not been directed at specific processes.

I'd recommend that consideration be given to the extension of these past monitoring efforts to include, for example, the placement of instrument arrays at the Confluence and at the Woods Pond Dam sufficient to provide long term, high frequency (e.g. 3-4 samples/hr), time series observations of water temperature and TSS at the mid-point of the low flow water column. These measurements would be supplemented by monthly sampling of concurrent PCB concentrations. All instrument observations could be telemetered to a central station permitting conditional sampling as unusual flow/transport conditions occur. In addition to the upstream and downstream stations in the PSA consideration should be given to the placement of one or more instrument arrays at selected sites adjoining the flood plain. Again these relatively high frequency data should be supplemented by lower frequency drawn water sampling of concurrent PCB concentrations. This latter sampling might occur on a monthly basis and aperiodically during particular rainfall/runoff events. This combination is intended to significantly increase our understanding of flood plain transport processes and their temporal (including seasonal) variability. These observations would also take allow us to take full advantage of the ongoing remedial efforts by providing quantitative data detailing effect at a number of locations. Such data would seemingly be of value in future remedial planning.

The above observations should be supplemented by a variety of other process studies such as the continuing survey of selected portions of the side banks and sequential bathymetric survey of
river channel transects or detailed cross-channel velocity/flux measurements to establish the adequacy of the model grid . Model results would be used to specify siting as well as the need for continuing observations. This close coupling between models and monitoring would be of clear benefit to the long terms goals of this effort.

Beyond these technical issues, the reports provided this reviewer remain exceedingly difficult to read. I understand all of the reasons why but cannot believe that the project would not benefit from a clearer and more concise document. In this validation report there is entirely too much use of "reasonable agreement" and the like with often insufficient demonstration. The Executive Summary is too general and does little to build confidence in this modeling effort and its subsequent application. Questions regarding many of the key points of the model require going back to previous documents that were themselves obscure and it's often difficult to figure out just what is being presented in the report figures (e.g. was that streamflow instantaneous, daily average or monthly average ? Are the TSS values a vertical average? Over what period of time ? Is it legitimate to compare longer term model results to shorter term data ?). Too many discussions end prematurely before any attempt is made to explain observed differences or discrepancies (see pg. 4-90/91 Vol.1 discussion of Event 10..Why the underestimation ?) Many of these questions might be resolved by a search of our voluminous file but who but the most dedicated would be expected to do it ?

I'd recommend, now that the major components of this exercise are in place, that a technical writer be charged with the preparation of a single document describing the model and the resulting runs written for the stakeholder community. This document would include all major features of the model and results with key supporting figures, references and an index. I'd consider this a high priority.

Moving now to the specific questions posed to the Peer Review Panel:

1. Considering the changes implemented in the Phase 2 Calibration, does the model as calibrated and validated, based on your technical judgement, reasonably account for the relevant processes affecting PCB fate, transport, and bioaccumulation in the Housatonic River to a degree consistent with achieving the goal of the modeling study ?

The global list of processes and governing factors incorporated in the linked three (3) model system is comprehensive and includes all those necessary for a detailed evaluation of PCB transport, fate and bioaccumulation (see Table 2-1 pp.2-3 Vol.1). In addition, the model, as structured provides an excellent framework for the systematic evaluation of each of the factors governing PCB transport and its ultimate bioavailability in the Housatonic River. This framework directly complements quantitative study of transport and subsequent investigation and ranking of remedial alternatives.

Despite the completeness of the global list, however, realization of the full potential of the models is governed by the extent to which model formulation provides accurate process simulation. Review indicates that model utility would benefit from improvements/modifications in a number of areas.

In general, comparisons with observed discharge indicates that the watershed model (HSPF) provides accurate simulation of the factors governing stream flow volumes to and through the PSA. These comparisons also suggest that the model is able to reproduce the timing of flow events. This matter of timing is an important factor if the data are to be used to calculate velocity and ultimately boundary shear, as they are in this study and will be in the upcoming Corrective Measures Study. The actual timing of stage/discharge at each section of the study area in large part determines the magnitude of the horizontal pressure gradient which affects speeds, turbulence intensity and boundary shear. These are the principal factors governing sediment/PCB transport both in the water column and across the sediment-water interface.

Given the importance of timing it is disappointing that the report provides so little detailed information on this factor and its effects. There is abundant reference to the model's ability to accurately reproduce event timing but these statements appear to be referring to timing in the most general sense. i.e. a precipitation event induces an increase in streamflow over a time similar to that observed. These references to "event timing" leave the question of the adequacy of the model simulation of timing with respect to flow velocities unanswered. Examination of several of the figures (see Fig. 6.2-5 (attached) e.g.) shows substantial differences between measured and modeled actual flow and stage timing at several stations along the PSA. In this figure, it would appear that the modeled speeds produced by the associated pressure gradients should be less than the measured and that the turbulence induced by adverse pressure gradients would also be reduced. Actual estimation is, of course, complicated by the morphology of the region with tributary inflows and/or backwater flows and storage complicating the simple stage discharge relationship. This factor may be the reason for example that the simulated flows at Holmes Road are higher than the measured despite a higher measured stage relative to that simulated.

This matter of flow phase and velocity and associated effects on transport could be better evaluated if the report had provided a more complete discussion of measured vs. modeled velocities when presenting the results of EFDC hydrodynamic calculations (see pp.6-31 e.g.). Although the model uses HSPF generated flows at the upstream boundary, the resulting simulations are to some extent independent of the HSPF generated flow/stage through PSA and as a result may be less sensitive to this matter of stage timing. Speeds were measured using both electromagnetic and acoustic doppler currents meters for short periods of time at several locations within the PSA (see.Figs 4.2-26 and 6.2-8 (attached)). These comparisons, while taken to be generally acceptable, indicate to me that there is very likely a substantial difference in measured versus modeled sediment transport associated with these differences in velocity. These flow induced differences will require substantial "calibration" within the model to yield reasonable estimates of sediment/contaminant flux. Given the non-linearity of both the velocity/shear stress and the shear stress/transport relationships it will be unlikely that such calibration will result in accurate simulations across a wide range of flows. This may be the principal reason that the model, at least some locations, seems to over-predict TSS values at low flows while under-predicting them at high flows. The significance of such variations will tend to increase with the duration of the model run and may become more of a problem during the extended runs planned for the Corrective Measures Study.

Comparisons between measured and modeled velocities are complicated by the model use of a single grid cell across the main stem channel. If the single cell is to be retained measurements should be designed to yield cross-sectional averages as opposed to point measurements. The majority of available data do not appear to be suitable for this purpose.

While on the subject of model grid specification, I must repeat what has been stated before regarding the advisability of increasing the number of grid cells across the channel. My review of available data detailing bathymetry and sediment type indicates that accurate simulation of these characteristics and their proper incorporation within transport models requires a minimum of three grid cells rather than the present one. If this proves (or is known) to result in an unacceptable increase in computation time then consideration should be given a reduction in the lateral extent of the floodplain since there are a variety of data (see Example Model Simulations, e.g.) that suggest that there are substantial areas along the inshore limits of the floodplain that are only occasionally affected by flooding and where, as a result, minimal longterm changes are to be expected. Alternatively, a coarser grid might be used on the floodplain.

Beyond this matter of flow,velocity and shear stress, I was pleased to see that the current model includes direct calculation of side bank erosion. I'll leave the adequacy of this formulation to those more qualified than I but must state my concern over the apparently simple partitioning of the mass of sediment supplied by this process between surficial erosion (during the rising hydrograph) and mass failure (during falling stage). It's hard for me to see why the masses should in anyway be equivalent. This subject was also noted in GE's review of the Model Validation Report (MVR). Justification for this approach should be carefully presented using the field data and/or supplementary data from previous publications.

The side bank issue affects both the margins of the river channel and the floodplain. All indications suggest that this latter area is primarily a sink for sediment and PCBs. As I understand it, the model treats each of the grids on the floodplain in a manner similar to those in the river channel and seeks to erode the soil surface by flow induced shear during flooded conditions. Given the presence of vegetation this very seldom occurs leading to continuing deposition over most of the area. What sediment and PCB that is supplied by the floodplain comes from aperiodic failure of the floodplain margin or sidebank.

If correct, this scheme seemingly neglects any transport associated with the movement of leaf litter and/or the rainfall induced wash-off of materials adhering to the surfaces of vegetation following flood inundation. Has consideration been given to the inclusion of these factors in the model ? If not, is there a solid basis for their neglect ? It may be that this is a subject that could be quantified in the revised monitoring program recommended above. It may also be that phyto-remediation should be included in this program (if it has not previously been investigated) and/or included in the upcoming CMS.

Moving to the areas of standing water in the main channel, the backwaters and Woods Pond, I remain concerned about the accuracy of the sediment transport formulations. This concern is not entirely alleviated by the sensitivity analyses presented in the previous calibration report (Weston,2004) and those included in the MVR. The response to a 50% variation in a variety of parameters overall seems reasonable and makes clear that all of the model results are sensitive to upstream boundary conditions. This, of course, is not surprising given the role of streamflow across this boundary in model dynamics and the limited number of areas within the

PSA sufficient to serve as a sediment supply. What's demonstrated is in fact why one worries about the accuracy of the sediment transport formulation.

Nor are concerns alleviated by model runs requiring what appear to be inordinately high diffusive fluxes to explain the simulated increase in PCBs at New Lenox road relative to those at Holmes Road (see pp.6-72 and Fig.6.2-42 MVR) during low flow conditions. This response suggests that the calibration of the sediment transport formulation might, because the majority of the available data were obtained during high flow events, have produced a function that is overly specific to higher flow conditions. This calls to mind the comments of Dr. Lick regarding the need to reduce the number of adjustable parameters in the transport formulation and more accurately specify those that remain (comments that I second) so as to have confidence that the algorithm is an accurate representation of governing physics and not simply some curve fitting routine.

I would recommend that increased focus be placed on the sediment transport formulation and that model runs be conducted in which the sediment supply across the upstream boundary is set to zero. Minimal " tuning" and reasonable results under these conditions would increase confidence in the formulation and directly benefit future evaluations in the CMS that very likely will be dealing with transport in specific areas within the PSA and require accurate estimates of local mass movements.

Does the presence and movement of ice seasonally contribute to sediment erosion in the PSA ? Within the channels or along the flood plain ?

2. Are the comparisons of the model predictions with data sufficient to evaluate the capability of the model on the spatial and temporal scales of the final calibration and validation ?

This depends to some extent on the characteristic being studied. For stream flow and stage model/data comparisons are based on a relatively long data set covering a wide range of seasonal, annual and intra-annual conditions at a number of sites throughout the PSA. The resulting comparisons are clearly sufficient to evaluate model capabilities over a relatively long period of time.

Moving to velocities and ultimately shear stress involves a significantly shorter data set at a limited number of locations. This does not necessarily mean that the data are inadequate since these characteristics are not expected to significantly change with time. As discussed above, however, it is the comparisons presented in the MVR that are less than sufficient. Better use of the available data would be the place to start. Careful review of the results of these analyses may then point to the need for additional data from differing locations and/or modified measurement procedures.

The TSS data set appears to be moderately robust although I worry that too much emphasis might have been placed on storm conditions. The data also appear to be primarily point measurements with some representing integrated measurements over the vertical. The absence of time series data prevents detailing of processes such as the time scales of resuspension and deposition during rising or falling stage with particular emphasis on the onset of resuspension during relatively low flow conditions. The influence of such low level but persistent resuspension and transport on PCB fluxes is largely ignored in the present study which places primary emphasis on storm events. Absent the low flow details its difficult to assess the role of these processes in the long term. Such assessments will be a subject of study in the upcoming CMS. As noted above I'd recommend immediate initiation of a monitoring program designed to provide time series observations of TSS concentrations at a number of selected stations throughout the PSA.

With regard to PCBs the data set allows at least an initial evaluation of the spatial and temporal validity of the model. Specification of the initial concentrations used in the Validation remains a concern since to some extent the hind cast method used to develop initial concentrations is not entirely independent of the subsequent model run. This apparently was required by the limited data available for 1979. Review of the RCRA Facility Investigation Report (BBL,2003) shows the results of sampling dating back to the 1979-1980 period. It seems possible to use this variety of data to provide at least a check on the trends and associated initial concentrations suggested by use of the model. Might this be possible ? I'm assuming that extensive "data mining" has been part of this exercise and that therefore some of this approach might already have been tried. If so, a brief discussion of this in the report would be useful.

The model/data comparisons for PCBs should be extended to include consideration of distributions over the vertical. Any assumption of a well mixed layer extending over depths in excess of a few inches doesn't seem to agree with field data (see e.g. Fig. 4-21c, BBL,2003, attached). How are these differences to be reconciled ? (i.e. use of 6" well mixed layer vs. detailed core data sowing little mixing beyond 1-2in). These core data and associated radio-dating also allow estimates of sedimentation rate to be compared to the mass flux data provided by the model. This comparison was not part of the validation report. It would provide an additional check on model results and is recommended.

An additional check on model results that should be considered includes the use of time series bathymetric data to check on the accuracy of sediment erosion/deposition estimates. Examination of these data might also be part the studies dealing with the recommended increase in the number of model grid cells across the river channel.

The above comments refer only to the PSA. It would appear that there are insufficient data to adequately test model results in both the upstream (which is in a state of major change) and the downstream model. Efforts to gather these data should be initiated immediately.

3. Is there evidence of bias in the models, as indicated by the distribution of residuals of model/data comparisons ?

As noted on several occasions above, visual examination of the figures suggests that the model overpredicts TSS concentrations at low flows and underpredicts them at high flows. The distribution of residuals provides clear indication of the overprediction at low flows for most stations (see Table 6.2-6). Although a number of reasons for these differences (both high and low flow) are presented there is no mention of the possibility that they are the result of less than accurate formulation of the sediment transport process, in particular bed erosion/deposition, in the model. As discussed above, this seems more than possible and should be carefully evaluated since it will be of increasing concern during the CMS.

In terms of PCBs, the model appears to overpredict concentrations in Woods Pond (see Fig.6.2-50). This is likely the result of inaccurate specification of suspended sediment flux associated with the above TSS predictions. Alternatively it may be associated with the specification of boundary conditions and/or the model treatment of PCB sequestering in Woods Pond. The model decline within the Pond appears to be approximately linear with time while the data suggest an exponential distribution. The latter might be the result of progressive source control (both natural due to burial and anthpogenic) acting in combination with sedimentation of cleaner materials. The model might easily obscure this process since it assumes a well mixed sediment column to 6in. With sedimentation rates in Woods Pond averaging approximately 2-3 mm/yr the above depositional processes might be expected to influence the upper 3in over 25 years. This might very well result in a reduction in actual PCB concentrations that would not be well simulated by the model.

4. Have the sensitivities of the models to the parameterization of the significant state and process variables been adequately characterized ?

In most cases the sensitivity of each of the models to the significant state and process variables has been adequately characterized. However, as discussed above, I remain concerned about the accuracy of the sediment transport processes within the model domain and look to the sensitivity analysis for some guidance. The sensitivity analyses provided in the MVR and the earlier calibration report provide clear indication that sediment mass flux throughout the PSA is very sensitive to the upstream boundary conditions. With these reasonably well specified, using a combination of HSPF results and field observations, it may not be very difficult to achieve reasonable model/data agreement despite inaccurate simulation of bed erosion within the PSA. The sensitivity analyses do not take the next step to test the adequacy of the interior formulations by shutting off the boundary contribution of sediment and PCB's. The importance of this formulation will progressively increase as the upstream sources of contamination are reduced and remedial measures for the downstream areas are being evaluated. A detailed analysis of the sensitivity and accuracy of this formulation is recommended
The results of this effort might serve to address the concerns raised in GE's review of the MVR dealing with the relative importance of Reach 5A and 5B as sources of sediments and contaminants to Reach 5.

In addition to testing of the erosion formulation additional sensitivity analyses should be performed to assess model response to the thickness of the active sediment column. This has been a matter of concern for some time. Experience in other riverine/bayou systems as well as the detailed core data (Fig. 4-21c, attached) indicate to me that the active bed used in the model is too thick. It may be that this specification in terms of physical transport characteristics has relatively little effect on overall model results (although that might bring up another set of questions). A test of model response to this characterization is recommended.

5. Are the uncertainties in model output(s) acknowledged and described ?

The uncertainty analyses presented in the MVR are interesting and represent a real attempt to develop new methodology for the assessment of complex models such as EFDC. Both Kolmogorov-Smirnov (KS) and Response Surface Modeling (RSM) analyses were applied. The KS methods require numerous model runs to develop a statistically robust data set. Given the time required for each run this was an onerous task and one made no easier by the failure of four of the EFDC runs. This effort yielded an initial estimate of uncertainties in the model prediction of PCB concentrations both water column and sediment associated. These concentrations were emphasized since they are passed to the food chain model (FCM). The KS analysis was supplemented by RSM to evaluate model uncertainty due to uncertainty in input parameters including flows, roughness parameters, critical erosion stresses, etc. The evaluations include consideration of the effects of the calculated uncertainties on the FCM predictions.

Overall the results of the uncertainty analyses, while interesting, were difficult to interpret in terms of model abilities to accurately simulate PCB transport and fate. The presentation of a series of individual summary figures for each condition does not facilitate interpretation in the absence of detailed description of cause and effect. The majority of the supporting text dealt primarily with methodolgy rather than interpretation. It may be that this type of analysis and its sophistication is premature and requires a greater understanding of the processes included in the model and their interactions than we presently have. As this becomes available with model use a better description and analysis of uncertainty might be possible.

Moving from the formal analyses of uncertainty to the general subject of model uncertainty, this report (MVR) too often fails to provide detailed discussion of uncertainty including consideration of causes. Statements such as "The simulated hydrographs....reproduced measured hydrographs reasonably well, however in some cases the magnitude of the simulated flow differed from the data in both positive and negative directions" (see pp 6-35) are too common. Uncertainty is to be expected as is variability both due to the input data being used and numerical model response. It should be introduced early in model discussions and included in logical discussion of cause and effect relationships throughout the report. I would hope that any summary report considers this as an absolute necessity. It will be of great value in building the confidence of a general readership.

6. Upon review of the model projections of changes in PCB concentrations in environmental media in the example scenarios, are such projections reasonable, using your technical judgement, and are they plausible given the patterns observed in the data ?

The model response for the two examples shown seems reasonable in the sense that most locations experience some decline in PCB concentrations over the upper 6in of the sediment column. I would have expected to see more of a change in Example 2 since virtually all of the major sources of PCB to the system have been shut off. This may point to the importance of the floodplain as a continuing source of PCBs to the system and/or point to the fact that chage can only be expected to occur over depositional depths. These depths, controlled by sediment rates in the area, will seldom exceed 3in except in the vicinity of point bars or similar channel features that are not well modeled because of the coarse grid scales used in the river channel. Since the model is averaging over a significantly greater depth (6in) the changes over 26 years may be difficult to assess. I'd be interested in seeing what a model run of 50 years or more would show.

7.Is the final model framework, as calibrated and validated, adequate to achieve the goal of the modeling study to simulate future conditions 1) in the absence of remediation and 2) for use in evaluating the effectiveness of remedial alternatives ?

This model system is not yet ready for use in the CMS. Several issues remain to be addressed. First, as discussed above, the hydrodynamics must be more thoroughly verified so as to insure accurate specification of boundary shear stresses. This factor will be central to future evaluations of selected remedial schemes such as capping and is presently essential within evaluations of sediment and PCB transport. The information and data provided in the validation report does little to build confidence in the present formulation.

Next, additional work is required to develop an accurate formulation of sediment transport. The suggestions of Dr. Lick in terms of both equation parameters and the structure of the sediment bed should be carefully evaluated. There seems to be an abundance of data and experience to suggest that 6in is an overestimate of the active bed thickness. There is also concern that the model as it exists may be biased to high flow conditions. Add to these questions regarding side bank erosion and floodplain dynamics.

Even with these process questions resolved there remains the issue of run-time. It seems clear that the model as presently configured requires entirely too much time for the completion of a single run to be useful within timely evaluations of a significant number of remedial schemes. We probably knew this several years ago and should have been more sensitive to the need to develop alternative formulations. A number of these, including the separation of the hydrodynamics from the transport estimates and subsequent FCM evaluations were previously mentioned. It is now necessary and possible to go further. Using the experience gained from "whole model" runs it should now be possible to develop a number of synthetic hydrographs detailing streamflow, stage, and TSS concentrations at the upstream boundary and each of the primary tributary streams. This would eliminate the need to run HSPF on a regular basis. EFDC is a ponderous model and can be streamlined now that we have a better idea of the relative importance of the governing variables. John Hamrick should be charged with this task (no more than 6 months) as soon as possible. As part of this streamlining the model grid characteristics should be carefully reviewed, again using what has been learned about the relative importance of each of the domain regions (sidebanks, backwaters, floodplains etc.). My sense of the present grid is that it underspecifies the channel region and overspecifies the floodplain. The latter could almost be treated as a box with fluxes simply proportional to stage which could be specified along its margin by EFDC. If in time more detail of the interior of the plain is needed for remedial purposes consideration might be given to replacing the high resolution grid on the plain while placing a box in some other area (channel sidebanks ?). Finally, I'd consider eliminating the FCM in favor of a parametric (flow, TSS concentrations ?) approximation of body burden uptake based on the results of the complete model runs.

In short, what I'm thinking about is the development of an supplementary modeling scheme for use in the CMS. The complete model would serve as a guide assisting in the development of a series of simpler, more efficient but less comprehensive, formulations that would be directed at particular remedial schemes. The complete model framework would remain in place providing guidance regarding the need for and type of data to supplement model formulations for both calibration and verification purposes but would not be run as frequently as the supplementary schema. The alternative might be to turn to a different series of models entirely. This is not recommended without good reasons that I don't have at the moment.

Personal Comment

With all these reviews it's easy to loose sight of the amount of work and dedication that it has taken EPA and GE and their contractors to get us to this point. As indicated earlier this has been and continues to be an ambitious project dealing with a complex subject. You have made significant progress and in many cases developed new methodology that will benefit future investigators. You are to be complimented on your dedication, skill, and patience. Stay the course ! This can be accomplished.

References

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EPA 2006 EPA Response to Questions From the Model Validation Document Overview Meeting. DCN:GE-061406-ADFI June, 2006

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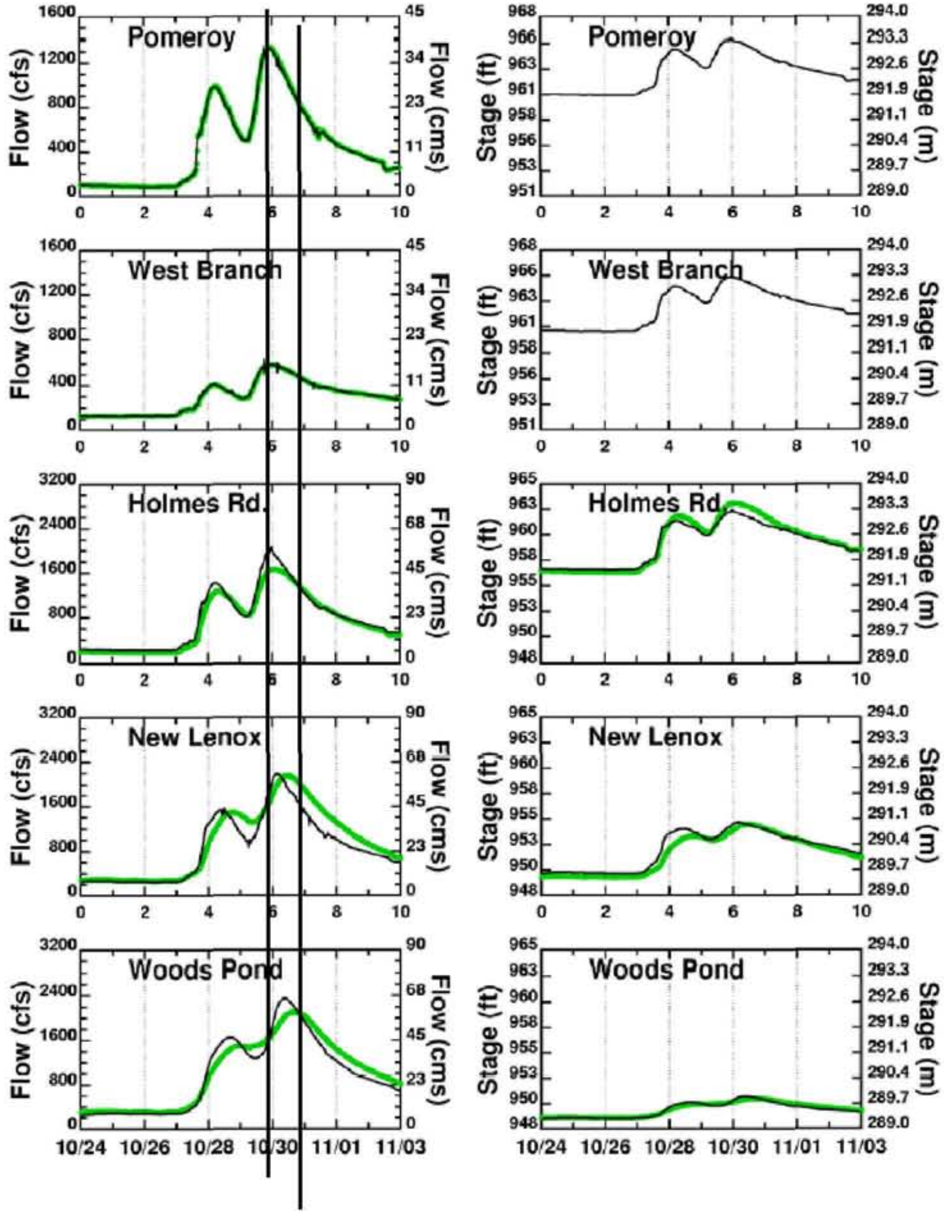
Weston 2006 Model Calibration:Modeling Study of PCB Contamination in the Housatonic River. Vol 1 and Vol 2. Prepared for U.S. Army Corps of Engineers, Boston,Ma. and U.S. Environmental Protection Agency by Weston Solutions Inc. West Chester, Pa. DCN:GE-030706-ADBR

Bohlen #1



Key: Black line — Simulated
Green line — Measured

Figure 6.2-5 Comparison of Measured and Simulated Flow and Elevation at Five Stations During the October 2003 Storm Event

Bohlen #2

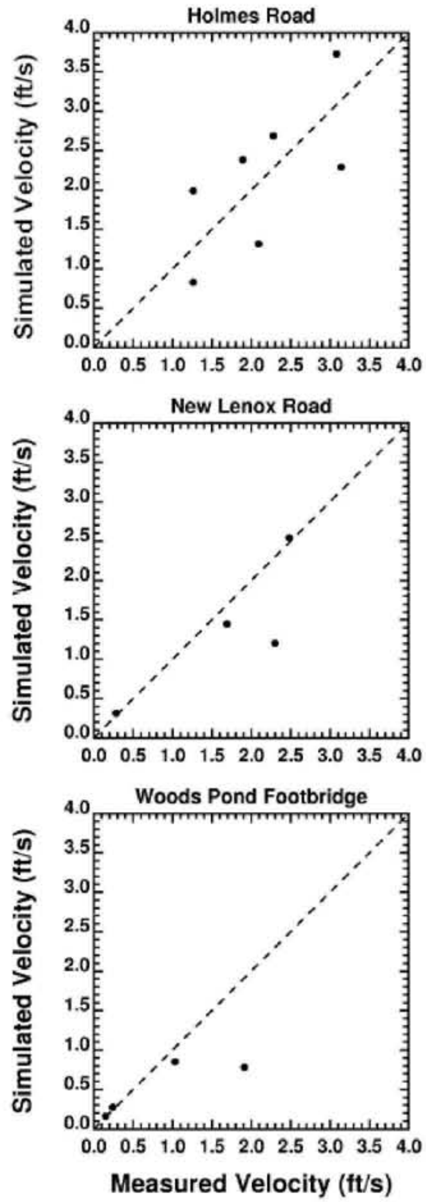


Figure 6.2-8 Comparison of Measured and Simulated Velocities for Holmes Road, New Lenox Road, and Woods Pond Footbridge During the 26-Year Validation Period

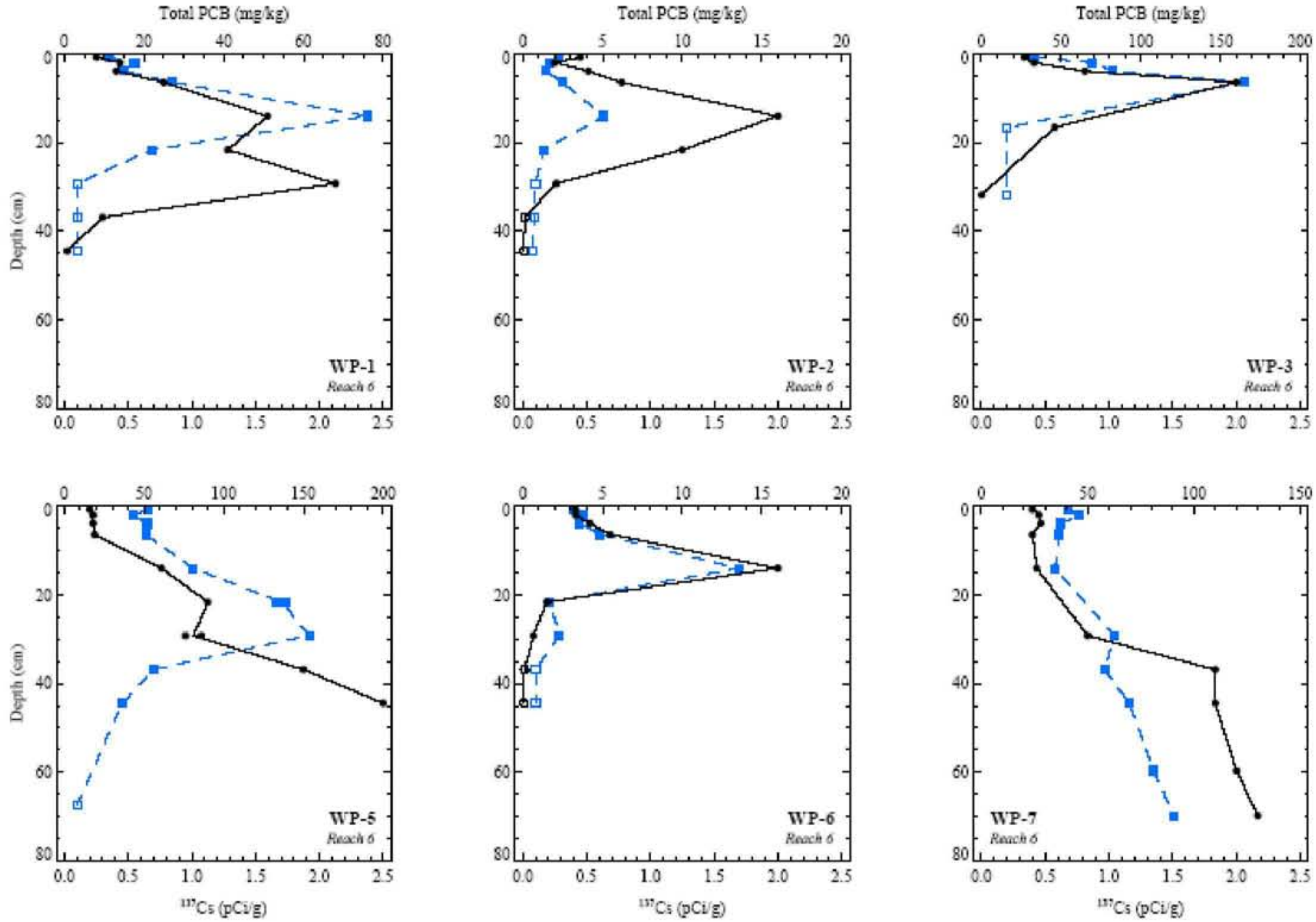


Figure 4-21c. Depth profiles of GE high resolution cores collected within Woods Pond during 1994 - 1995.

Note: Cesium-137 plots are all on the same scale; PCB scales vary by core. Line represents average of duplicate samples.

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Thu Jun 19 16:06:08 2003

Bohlen #3