Comments on the
Ecological Risk Assessment for
General Electric (GE)/Housatonic River Site
Rest of River, volumes I-II

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Introduction and disclaimer

This review of the Final Ecological Risk Assessment of the Housatonic River/GE Site was conducted under a grant from the Environmental Protection Agency to the Housatonic River Initiative. The materials and conclusions presented here are those of the authors and do not represent the position of the EPA, ACOE or any other federal or state agency.

This report was prepared by: Dr. Peter L. deFur and Mr. Kyle Newman of Environmental Stewardship Concepts, under contract to the Housatonic River Initiative.

The context of this report is to provide feedback on the Final Ecological Risk Assessment to the EPA and to inform the citizens (through Housatonic River Initiative and Housatonic Environmental Action League) of the strengths and weaknesses of the Ecological RA. This feedback and evaluation is intended to identify major issues that remain particularly in light of the earlier comments provided to EPA. It is the intent of the reviewers and authors of this report to improve the Ecological RA and eventually result in a more protective site cleanup.

The purpose of these comments is to evaluate if the Ecological Risk Assessment adequately protects the ecological health of the environment of western Massachusetts and Connecticut from the toxic chemicals released from the GE facility in Pittsfield, MA. This review examines the scientific information and methods used, the underlying information, both quantitative and qualitative, the assumptions, logic and reasoning and other significant aspects of the Ecological RA.

General Comments

Overall, the report was comprehensive and well informed. ESC agrees that risks remain significant throughout the study area to most forms of wildlife. Changes made to the document are helpful and provide the reader with much more useful information that leads to a more comprehensive document. ESC recommends few substantial changes to the document.

We appreciate EPA's efforts to satisfy reviewer's comments that the chapters are too technical for general readers and contain too little information for technical use. However, in attempting to satisfy these concerns, EPA is attempting to write a single document for two completely different audiences. The result can be, and in this case, seems to be text in the main report that is insufficient for the technical audience. EPA would have been better off to have kept the risk assessment written for a technical audience and prepared a completely separate and substantially shorter document for public consumption.

Revisions to the document seem to give added weight to field studies, even when those studies are flawed. In several instances (fish, insectivorous birds, and piscivorous birds), field studies that did not accurately represent endpoints associated with chronic PCB exposure were given more weight than the computer modeling that indicated high levels of risk for target species. The belted kingfisher field study that led to the conclusion that kingfishers were at intermediate risk in spite of high risks predicted by models was even criticized by the EPA as in Appendix H saying that "EPA does not believe the study can be used to conclude that tPCB exposure did or did not adversely affect kingfisher reproductive output in the PSA." Without evidence from the studies to specifically discredit the modeling, field studies should not be given more weight than comprehensive modeling efforts.

The revisions have not addressed most of ESC's previous comments, in particular those regarding Connecticut. The lack of samples in the Connecticut section of the river constitute a large data gap that must be resolved. This gap exists despite evidence that a substantial floodplain exists in the state (please refer to our previous comments for more information regarding this). The recent floods of January 2005 support the point that the Connecticut flood plain requires evaluation. Without more comprehensive data on this section of the river, uncertainties will remain regarding the conclusions for the Connecticut reaches. More sampling locations should be defined and more sediment samples taken for a complete risk assessment that addresses Connecticut.

We remain opposed to EPA insisting that a sustaining population of sick fish is an acceptable condition at this site or any other. Comments on the original draft of the Ecological Risk Assessment explained why EPA is wrong in this position. A fish population that has 30, 40 or 50% of the individuals with an abnormality that prevents or impairs reproduction or performance is not a healthy population.

The number of animals in a population over time is NOT the only measure of population sustainability. This point cannot even begin to discuss the evolutionary implications of such a condition of animals.

Executive Summary

The more detailed highlights section is an improvement, but we would like to see the language saying that high risks are unacceptable be reinserted into the text.

Section 3

3-1, lines 34-37: Even pockets of high risk can be significant- flooding or other natural disturbances to the sediment bed can expose other areas to higher levels of risks to invertebrate populations in other areas. We also maintain that there is not enough data to substantiate the claim that risks are low in Connecticut.

Appendix D

D-66-68, Section D.3.2.3: The use of concentration analysis alone cannot accurately determine if secondary COCs are influencing toxicity tests. The data from Locations 7 and 8 does not explain the results obtained. Simultaneous exposure to several metals at once including lead, magnesium, and arsenic has been demonstrated to cause more serious effects than expected when individuals are exposed to them singly. It is therefore possible that Locations 7 and 8 have mixtures of metals that exhibit greater levels of toxicity than at other sites.

The COC interaction does not seem to be correct. The text that discusses differences between reaches 7 and 8 does not explain the results. Positive correlation does not provide evidence for negative results at other locations.

Section 4/ Appendix E

Removal of Barium from the list is highly questionable (E-9)- this was in previous draft

E-15, lines 16-17: There should be clarification as to if the leopard frogs in the EPA study were captive bred or wild caught. Other studies mentioned used wild caught frogs obtained from a biological company as controls.

E-17, Line 10-19: There should be a better explanation of why no frogs were captured from reference sites. Have any researchers returned to collect frogs from the reference site since? If none have, plans should be made for them to. Without the data those collections would provide, uncertainty will remain regarding the results of the study.

E-21, Lines 17-18: Units of measure for larval density not listed in GE study. These should be included.

Section 5

The conceptual model section of the highlights should be reinserted for consistency.

We respectfully disagree with the change in risk probability in fish from high to intermediate. The decision to lower this probability seems based on the magnitude of effects on endpoints rather than the probability of them occurring. The data presented does not seem to support this conclusion. In Table 5.4-3, the overall endpoint values are mostly in the moderate to high range. The lowest values occur where there is the most uncertainty, specifically the field studies. Table 5.4-4 shows that there is evidence of harm for all assessment endpoints, even if of only intermediate harm.

We continue to argue that supporting reproducing populations of sick fish is unacceptable. There is the potential for these illnesses to magnify within the population over long periods of time and cause significant harm. Additionally, populations of sick but still reproducing fish are more susceptible to other stressors. Allowing fish populations to remain in this state would leave them vulnerable and less able to respond to changes in habitat brought on by man-made influences or natural disasters. This point was covered in original comments and EPA is referred to the literature and documents for that submission.

Section 7

Without evidence as to why the modeling was wrong, the weight of evidence for the field studies should not have as significant weight as they are given in this section. There is a fair amount of uncertainty in the threshold range derived in Custer's field study (2002), stemming from the fact that the ecological significance of the observed effects (abnormal nest abandonment and larger clutch sizes), and if those levels were effecting hatchling success. The time limitations of the study did not allow for researchers to examine the long term effects of such exposure. Considering that Custer's study found the highest tPCB tissue concentration recorded in literature lends additional evidence to the model's results. Considering the tremendous amount of literature suggesting that those concentrations do lead to serious adverse effects for bird populations, the ramifications of the Custer study should be reexamined because the data suggests the possibility for serious harm to bird populations from PCB exposure. Because of this, ESC disagrees strongly with the conclusion that insectivorous birds such as tree swallows and robins are at low risk.

Section 8

While we agree with the decision to raise the estimated level of risk for belted kingfishers to intermediate, this still may be underestimating actual risks. Though the belted kingfisher study exceeded the requirements for degree of association according to Menzie et al (1996), the limitations of the study make the data obtained by it irrelevant. There was no data on clutch size or hatchling success, two significant reproduction endpoints affected by PCB exposure. By only sampling during one breeding season, no long term trends could be obtained and therefore no useful reproductive data. Sample sizes were small, and the modeling of diet to estimate PCB intake of birds was imprecise. These are all problems noted by EPA in Appendix H, and as stated in lines 28-30 on page H-51 EPA does not believe that the study can be used to determine whether or not tPCB exposure is adversely effecting kingfisher populations. Because of these limitations, the field study should be dropped from the WOE and more weight given to the modeling as done for osprey.

Section 12

The expanded and more detailed highlight section is an improvement, though we would like the language referring to high risks as unacceptable be reinserted.