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TO: EPA Science and Technical Committee

FROM: Dan Abramowicz

SUBJECT: Phase II Comments

I include below a few of my comments on the EPA's Phase II Workplan and Sampling Plan. In general, I was quite surprised at the lack of detail included in this document. This is the same general comment I made about the Phase I document.

1) The high-resolution cores for radio-dating will be used to determine a great deal of information for the reassessment (e.g. historic sediment loading, historic PCB transport, and *in situ* PCB biodegradation rates). The limitations of this method, from my perspective, include:

- sediment deposition is not uniform throughout the river. Therefore, even though cores may be identified that are undisturbed over time, this tells us nothing about sediment deposition or PCB transport anywhere but at that exact location.
- no attempt has been made to determine the general utility and accuracy of this method in a river setting. What is the standard deviation in the sediment loading, PCB loading, and PCB homolog distribution determined for several cores from the same general region? This information is critical to the successful extrapolation of this method, as planned by the EPA.

2) Section 2.2.2.1 (Main Data Collection Tasks)

Page 2-5: Water column measurements will be performed under high and low flow conditions. Were the high flow measurements taken this past spring, or will they be taken in the spring of '93? What flow rates are required for "high flow"?

Page 2-8: How many low resolution sediment cores will be taken? Will the density be great enough for accurate use of the kriging methods?

3) Section 3.2.1 (Transect Sampling)

Page 3-3: How will it be determined that measurements will be taken from the same parcel of water? Will a dye study be performed to allow accurate analysis? This is critical, since water measurements are known and stated to be remarkable variable.

4) Section 3.2.2 (PCB Equilibrium Study)

Page 3-7: The basis of the "lack of equilibrium" in water column measurements seems weak (Figure 3.5). It is known that filtering methods can have large effects on the resultant measurements, and the LVF and GFF samples used to monitor the "lack of equilibrium" were prepared with different filtering methods. In addition, the data in Figure 3.5 indicate some systematic error in the analyses, as the only significant difference in the two homolog distributions is the large increase in peak 6.

Page 3-8: Number of measurements at each transect for the PCB equilibrium study? Clearly many samples must be necessary, based on the EPA's comment on the same page about inherent variability of the method.

5) Section 3.3.1 (High Resolution Coring)

Figure 3.9: Why are PCB analyses missing for 1954 to 1964? This data shows that the PCB maximum occurred sometime between 1970 and 1974, yet the flood events occurred in 1974 and 1976.

Page 3-16: Is the anaerobic biodegradation result from the NYSDOH dechlorination or actual degradation. If PCB dechlorination is meant, scientists at GE and in various academic and EPA laboratories have performed relevant research that should be addressed. If anaerobic biodegradation is meant, this represents a significant result that requires additional investigation. Rhee has previously reported that the light, *ortho* substituted products of anaerobic PCB dechlorination can be anaerobically biodegraded [Rhee *et al.*, *Water Research.*, 23:957-964]. These preliminary results have recently been confirmed at GE (Abramowicz) and at the University of Michigan (Vogel) [Conference on the Bioremediation of Contaminated Sediments, Port Authority of NY/NJ, May 5-6, 1992].

6) Section 3.3.2 (Analysis of Archived Sediment Extracts)

Page 3-17: One cannot use the PCB maximum to constrain the date to the early 70's. This is a classic example of circular reasoning, as the radioactivity measurements are used to determine the date of the PCB maximum.

7) Section 5. (Contaminant Fate and Transport Analysis)

Page 5-2: Homolog specific or congener specific fish bioaccumulation will be necessary to accurately predict future bioaccumulations. As such, the extensive dechlorination that has occurred in Thompson Island sediments must be factored in to the analyses.

8) Section 5.1.4 (Evaluation of Degradation and Volatilization Rates)

Page 5-7: The comment that extrapolation of laboratory biodegradation results to field conditions is "fraught with uncertainty" is inconsistent with the close correlation between a) GE's aerobic PCB biodegradation laboratory results and the results of the recent Hudson River aerobic field test [GE HRRS Report and Harkness *et al.*, submitted to *Science*], and b) the correlation between GE's anaerobic PCB dechlorination rates at various PCB concentrations in the laboratory and observed changes in the river sediment itself [Abramowicz *et al.*, submitted to *Environmental Science and Technology*].

Page 5-8: The comment is made that anaerobic PCB dechlorination is insignificant unless convincing evidence is obtained that it results in a significant mass reduction of buried PCBs. This is completely inconsistent with the comments about the importance of bioaccumulation in fish (see page 5-1) and the reduced bioaccumulation potential of the resultant lightly chlorinated PCBs.

9) Section 5.2.1 (Food Web Model Approach)

Page 5-10: The expense of an accurate food web model is considered unjustified, in spite of the importance of information on PCB bioaccumulation. It appears the EPA needs to rethink its fiscal priorities here.

10) Section 8.3 (Identification and Evaluation of Technology Process Options)

Page 8-2: How will effectiveness be decided? Will comparisons be made on the bioaccumulation potential in fish, or just on total PCB mass reductions?

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