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SUBJECT: REVIEW OF "PHASE 2 WORK PLAN AND SAMPLING PLAN EPA WORK ASSIGNMENT NO. 013-2N84" FOR THE REASSESSMENT REMEDIAL INVESTIGATION AND FEASIBILITY STUDY FOR THE HUDSON RIVER PCB SUPERFUND SITE.

1. As a member of the Scientific and Technical Committee for the Hudson River PCB Superfund Site I have been asked to provide a review and comments of the subject document. According to the document "The Reassessment requires knowledge of the source areas of PCBs and the future impact of PCBs in the HUdson River system under conditions of 'No Action' and various 'remedial alternatives'.". Since the Phase 1 Report determined that human health risks from Hudson River PCBs are caused primarily from the consumption of contaminated fish, the two major questions are: "What is the reduction in PCB levels which is necessary to decrease fish tissue concentrations to levels that meet human health criteria, and; which source areas, if any, may require remediation in order to achieve that reduction." I will be happy to review and comment on the document from the standpoint of these two questions, but a very important issue seems to be untouched by this approach. That is the potential for ecological effect other than human health. The first paragraph of the Objective and Scope section (section 1.2) states that the information gathered in the site characterization will be used to prepare " the baseline human health risk assessment" as well as the "baseline ecological risk assessment".I will comment on the document with the two stated questions in mind, however, I believe that failure to consider ecological impact is a serious omission.

2. I have made some specific comments referenced to the section of the report to which they pertain:

section 2.1.1. I commend the use of congener-specific analyses. One clear advantage of using congener-specific determinations was overlooked in the discussion of why congeners would be determined. The toxicological ramifications of different congener bioaccumulation rates and relative toxicities is important information for estimating human health effects.

section 2.1.2. The document states that "other pertinent parameters will be measured" in conjunction with the water-column sampling without specifying the parameters. Total organic carbon must be one of the parameters if these data are to have any usefulness in predicting biological or human health effects.

section 2.1.3. I still cannot fathom the reasoning behind high resolution coring. Historical perspective is cited as one reason for this coring, but historical perspective has no bearing on the two questions cited above. Knowing how much PCB is in a thin layer of sediment cannot predict future release patterns, it cannot predict biological availability, and it has absolutely no bearing on the outcome of any remediation or removal technique that I can think of. Is this expensive data exercise a wise use of the taxpayers money and the limited resources available?

section 2.2.2.1. This section states that most PCB transport will occur during spring flow and that flow and suspended load diminish in summer. This may be true, but merits some investigation. The Hudson is hardly clear in the summer months. Algal blooms do occur, and can be expected

to be a source of particulates. Since algae are living organisms, they will offer a very good organic/lipid phase into which PCBs can be expected to partition. Admittedly, the total mass of algal cells may be low compared to high loads of suspended inorganic matter during conditions of high flow and scour. However, the lipid quality of the algal cells, their close proximity to the bottom due to low flow, and their high residence time (during which PCBs can become associated with the algae), could make summer blooms a significant source of PCB transportability.

section 2.2.2.1. The document states that there is a probability that *in situ* biodegradation of PCBs could have occurred, and that considerable care will need to be exercised in comparing high resolution coring data. This is one more reason <u>not</u> to do the high resolution coring.

section 2.2.3. The only sampling task scheduled for study area C (from RM55 to the Federal Dam) is the high resolution coring, and I have mentioned my misgivings regarding high resolution coring before. "Other parameters" will again be measured. Grain size, percent clay, percent silt, and, most importantly, percent Total Organic Carbon should be among these "other parameters" if any partitioning, or thermodynamic bioaccumulation potential modeling or estimates are to be possible.

section 3.1. Discusses the congener-specific analyses, and mentions that 120 peaks will be enough to differentiate Aroclors as well as biogeochemical processes. Section 2.1.1. uses the figure of 70-80 peaks as the expected maximum because of calibration standard limitations. I am well aware of the analytical problems associated with congener-specific determinations, but this document should be more consistent throughout.

section 3.2.1. I'm glad that this section mentions the additional parameters that will be determined, and glad that the list contains two organic carbon measurements.

section 3.2.1. Gives a very interesting discussion on data interpretation using the Thompson Island Pool as an example. The influx of the lighter PCB homologs is very interesting. A hypothesis is made that this influx of lighter homologs may be due to release of PCB from the sediment after *in situ* degradation. At the WES we have found a significant amount (up to 2% of PCB 153) of PCB can be lost from sediment to the air.[1] It seems to me that lighter PCBs could be expected to be more volatile than the PCB 153 we measured. Two important points should be examined. First, from a human health standpoint, Do volatile PCBs in this area constitute another important route of exposure? Second, from a modeling standpoint, one cannot assume that the lighter congeners leaving the Thompson Island Pool (or any of the river reaches outlined in the subject document) cannot be assumed to make it into the next reach of the river.

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1. Brannon, J.M., C.B.Price, F.J.Reilly, Jr., J.C. Pennington, and V.A. McFarland. 1991. Effects of Sediment Organic Matter Composition on Bioaccumulation of Sediment Organic Contaminants; Interim Results," Miscellaneous Paper D-91-4, US Army Engineer Waterways Experiment Station, Vicksburg, MS.

section 3.2.1 Mentions that Chlorophyll-a measurements may be used to help understand partitioning of PCB between suspended and dissolved phases of water. If Chlorophyll-a measurements have already been taken, and the new data must be correlated with the historical data, then so be it. However, %lipid in plankton should be able to give a better estimate of the partitioning potential for organisms in the water column.

section 3.2.2. Discusses the results of Bopp *et.al.*, 1985. The differences in peak percent composition between water filtered in the field and water stored or incubated (an important fact not mentioned in the document) prior to filtration are used to conclude that a dissolved source of PCB must be present. This seems, to me, to be an over-interpretation of the data. The two protocol are very different, and could reasonably be expected to give different results. Section 5.1.3. states that there was a clear difference in the two filters' ability to trap PCBs do to their different pore size. The field particulate PCB was present at higher levels than the dissolved PCB, and could actually have been the source of the lighter peaks reported to have been dissolved. However, the difference could as easily have been from the difference in the two filter's retention characteristics! No information in this document precludes a scenario where the suspended particles are acting as a source for lighter weight PCBs to the dissolved phase. I do not believe that the cited methodology will be able to answer any questions regarding PCB Equilibrium. Either a better method or a much better explanation of this method must be forthcoming.

section 3.3.1. HIgh resolution coring is said by the document to preserve each year's suspended matter properties. I disagree. First throughout this document mention is made of sediment scour. Storm events scour the bottom even in depositional areas. The scouring may or may not remove an entire year's record. It may alter a year's record. A storm event may also bury one year's deposit, invert several year's patterns etc.etc. The radiochemical dating markers mentioned do not differentiate dated sediments with a one to two year accuracy. Figure 3.9 purports to show the uncertainty in the sediments' age. It is interesting to note that each point I examined on figure 3.9 had an uncertainty of 5-10 years. At a *predicted* deposition rate of 1-2 cm/yr. that puts the uncertainty of a core's age at between 5 and 20 cm! How can 2cm high resolution coring tell us anything useful with regard to the two questions being asked here? Sampling at 1-2 cm intervals seems to increase the cost at a much higher rate than the information! This section makes mention of several parameters that should be a part of any sampling plan (eg. TOC and grain size) but that actually do not need to be estimated in this highly questionable and expensive high-resolution scheme. The C/N ratio is supposed to show how much wood has been deposited with the sediments. I fail to see how this information on a 1-2 cm scale will answer either of the two questions mentioned above.

section 3.3.3 The definition given for Low-resolution coring is given as 13 cm. Even this resolution of coring seems expensive and wasteful of resources with regard to the two questions being asked. No dredge can differentiate a 5 inch (13 cm) slice of sediment. Bioturbation, physical scour etc.etc. also do not respect such small divisions. I will say that according to the Figure (3.9) in section three the uncertainty of annual age prediction for sediments is more on the order of 13 cm than 1-2 cm mentioned for high-resolution coring.

section 5.1.2. Calibration of the models for mass-balance rely on "datable cores ...to calibrate an annual model of transport." According to the previous sections of this document it would seem to be impossible for datable cores to be obtained with the required precision to calibrate an annual model. How will the models be calibrated?

section 5.2.2. Discusses an approach to predicting bioaccumulation levels in fish using Equilibrium Bioaccumulation Factor approach. Thermodynamic Bioaccumulation Potential (TBP) prediction is a well accepted part of sediment evaluation by both the US Army Engineers and the US Environmental Protection Agency[2]. The TBP approach was originally developed empirically and has since been exhaustively tested. The document seems to rely heavily on the EqP work of DiToro, which has recently come under heavy criticism for the many assumptions required for it's application.

section 5.2.3. Discusses the approach that correlation analysis approach which will be used. It seems that that the relationship will empirically relate the observed fish burden to the suspended sediments and the bedded sediments all normalized to the phase that will actually contain the PCBs (lipid and organic carbon). B_w and B_s are the Bioaccumulation factors for suspended sediments and bedded sediments. The TBP approach ignores the contribution from suspended sediments, which DiToro *et.al.*,[3] also say can be ignored due to it's insignificance. In TBP calculations according to the "Green Book"[4] the value that explains the difference between the carbon normalized sediment value and the lipid normalized organism value is described by the apparent preference factor (similar to B_s). The factor in use is 4, which is considered environmentally conservative. Measuring all of the phases in the subject document's Correlation Analysis Approach in order to obtain a new apparent preference factor would appear to be research at best and re-inventing the wheel at worst. Considering the limited resources available it does not seem prudent to re-examine this issue for each superfund site.

section 6 Discusses the assumption being used and/or evaluated in the Human Health Risk Assessment. I was pleased to see that the RA will be performed for both the case of a fishing ban and for the case of no fishing ban. The cost-justification for lifting the current fishing-ban should be a prominent part of the cost-benefit analysis for decision-making regarding the reassessment of alternatives.

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2. U. S. Environmental Protection Agency Office of Water and Department of the Army Corps of Engineers. "Evaluation of Dredged Material Proposed for Ocean Disposal - Testing Manual" February 1991, EPA-503/8-91-001, Washington DC 20460

3. U.S. Environmental Protection Agency Office of Water. 1991. "Proposed Technical Basis for Establishing Sediment Quality Criteria for Nonionic Organic Chemicals Using Equilibrium Partitioning." August 1991, Office of Science and Technology, Health and Ecological Criteria Division, Washington, DC.

4. US EPA Office of Water and Department of the Army Corps of Engineers, 1991. *op.cit.*

section 7 Deals with the assumptions and the paradigm that will be used for the Ecological Risk Assessment. I found mention of a proposed list of target species but no proposed list. As a member of the Science and Technical Committee I would like to have the opportunity to see it. Another shortfall is the lack of consideration of alternatives or comparative risk in the ecological RA. For example, under the current fishing ban in the Hudson, the striped bass population has increased, even though PCBs exist there and in the flesh of the fish. One of the stated goals of the document is to gather data to assess human health effects of the alternatives. If the ban is lifted, either as a result of the findings of the human health risk assessment, or as a result of remediation of some type, the population of striped bass will surely go down. This puts striped bass at risk from the absence of PCBs!

section 8.5 Mentions the criteria that will be examined regarding the impact assessment of the proposed remediation strategies. The list of factors was very small. Will comparative Risk (both human and ecological) Assessments be made? Will the assessments consider effects at the site of remediation? near the site of remediation? near the site of treatment? Will the assessment consider for example the potential effects of a 500 yr flood event (as will be done for the superfund site) on a disposal site? This section needs a little better presentation of the potential items that will be studied.

section A.3.4 Describes very briefly a protocol for critical shear stress analysis of cohesive sediments. The protocol does not mention sediment core storage time or conditions or sample preparation techniques. I have concerns about bioturbation during storage that could change the cohesiveness, and concerns that surface bacterial or algal cells that aid cohesiveness *in situ* may be altered by the protocol.

3. In general this study plan is well written and well thought out. I have grave concerns about the amount of work and cost being expended in high-resolution testing that cannot, to my satisfaction, be justified. I also question the plan to collect high-resolution information on sediments, suspended sediments, and tissues to derive an empirically based preference factor for PCB bioaccumulation in the Hudson, when this work has been done, and already has regulatory status.

4. I will be happy to answer any questions you may have. Please, feel free to contact me @ (601)634-4148.

Francis J. Reilly, Jr.
Senior Scientist
AScI Corporation