SARATOGA COUNTY

ENVIRONMENTAL MANAGEMENT COUNCIL

PETER BALET
CHAIRMAN

GEORGE HODGSON
DIRECTOR

July 27, 1998

Mr. Damien Hughes
USEPA Region 2
290 Broadway, 20th Floor
New York, NY 10007

Dear Mr. Hughes:

The following question/comments are for the use of the Peer Review Committee in its review of EPA's Preliminary Model Calibration Report, Phase 2 Report, Volume 2B dated October 1996. Page references are to pages in the Volume 2B Report, Book 1.

1. Doesn't the discontinuity in the early 1990's of the PCB concentration data vs time show a definite need for the HUDTOX model to incorporate the effects of the PCB releases at Bakers Falls as no other reason appears reasonable to account for the sudden PCB increase in the 1990's? It is not clear from the discussion in this report that the HUDTOX model is able to incorporate the Bakers falls situation and/or that the EPA has the necessary data to include the effects of Bakers Falls. The need to consider the effects of Bakers Falls is especially urgent in view of the Fish Body Burden models being of a quasi steady state nature with a time span interval average of about one year and in view of the fall-off in PCB levels in the years leading up to 1992-93 and then the increase when the Bakers Falls sources came into play. Because of this behavior of the PCB concentrations, it is recommended that the study of PCB levels using the HUDTOX model be separated into two time periods. The first time period would be up to 1992-1993 and would evaluate the HUDTOX and Fish Body Burden models ability to predict how PCB levels decrease when new PCB sources are zero or negligible. The HUDTOX model could be applied to the years after 1992-1993 and in the future to predict how PCB levels would decrease after the Bakers Falls source is eliminated by the remedial actions now underway. The model would have to incorporate the initial inputs of PCBs from Bakers Falls and then the subsequent drop of PCB input as the remedial actions take (or have taken effect). Assumptions would have to be made as to the timetable for elimination of the Bakers Falls source; perhaps it could be a parameter in the study. This approach should give a better prediction of the future than trying to make the HUDTOX model jump through the evident discontinuity that occurred in 1992-93.

50 WEST HIGH STREET BALLSTON SPA, N.Y. 12020 (518) 884-4778

2. Doesn't the stated tendency of fish to accumulate higher chlorinated PCB's (see page 8-3 of Book 1) emphasize the need to do the modeling on a congener basis and to include terms in the PCB transport and fate models for biodegradation of PCB's? Consideration of this biodegradation effect is necessary to accurately estimate the total uptake of PCB's in the fish, which so far, is the basis for EPA's Health Risk Analysis. The importance of considering the effect is shown by the stated sensitivity shown to biodegradation in the Lower Hudson River Model (page 7-9 of Book 1).

3. The discussion and formulas presented in Book 1 are based on the assumption there is a direct one-to-one relationship between PCB concentrations in water and sediment to PCBs levels in fish. The tendency of fish to accumulate higher chlorinated PCBs (see comment No. 2) raises questions about this assumption. This tendency suggests that it may be necessary to include in the Fish Body Burden model terms to reference the time rate of PCB congener removal from fish (whether by excretion or metabolism) and to consider the age of the fish sampled. What has been done to evaluate the need for including such effects in the model? If data are not available, at least a parameter study could be made using best estimates from the scientific literature to see if these effects are significant.

4. Book 1 should include a discussion as to why is it necessary to develop a new model for the Upper Hudson instead of applying the existing Lower Hudson model (or conversely, if a new model is needed for the Upper Hudson, why isn't it also used for the Lower Hudson)?

5. Solids submodel, page 3-6: Shouldn't the discussion include the rationale for a constant organic carbon fraction being assigned to TSS to represent particulate organic carbon? Also, why do not tables 4.9 & 4.12 include this parameter? Some explanation as to why values for biotic solids loading due to primary production from the Lower Hudson are satisfactory to use in the Upper Hudson (even after temperature correction) should be provided.

6. Toxic chemical submodel, page 3-6: It is not satisfactory to say that the Phase 2 database doesn't distinguish DOC bound PCBs from truly dissolved PCBs but it is important to do so and then drop the subject. Shouldn't some explanation as to why this omission in the data is not significant to this reassessment be provided?

7. Toxic chemical submodel, page 3-7: Are the "other enhancements" made to simplify application of the model, which are not spelled out in value 2B, truly enhancements?

8. Scour Model, page 3-13: Will the inability to model "subsequent transport and redistribution" of eroded sediments be of significant impact to the modeling results? If not, why not and if yes, how will the Reassessment accommodate this failing?

9. Page 4-11: What is the basis for the applicability to the Upper Hudson River of Green Bay data for PCB concentration in the air? If volatilization is important, this assumption should be revisited and either data collected for the Hudson or the PCB air concentration made a parameter in the study.

10. 4-18: Since values of v_s given here range from .25 to 3.05, is a value of 2.0 the correct one for the Hudson? What is the basis for selecting a value of 2.0?

11. Page 4-19: Isn't a more complete explanation or discussion of the basis for the "professional judgment" value of .22cm/yr for v_b needed?

12. Tables 4-13 through 4-17: The large variances shown in these tables are disturbing and some discussion of their significance is needed. For example, the t-test uses the assumption that the variances are equal. It is usual to test this assumption using the "F" test. Has this been done and if so, what conclusions were reached? Should the analysis of variance method be used in addition to or in place of the T-test?

13. Page 7-3: Shouldn't an explanation be provided of why Hydroscience values for horizontal dispersion coefficients are good values to use? What is the rationale for using +/-30% to adjust these coefficients for high and low flow years.

14. Page 10-23 and following: Why are not equations defining FFBAF, calculations of FFBAF, and FFBAF tables and/or figures provided as for other BAF's?

15. Are the data obtained by EPA sufficient for applying the models being developed by EPA (or in applying such modifications to the models as the Peer Review Committee deems appropriate)? The question is raised since to my knowledge, the data collection was defined and carried out significantly in advance of EPA's development of the modeling approach. (The data collection workscope was set out in EPA's Final Phase 2 Work Plan and Sampling Plan dated September 1992). Such an approach seems contrary to sound scientific method and leaves doubt as to whether data collected without reference to how they will be applied are appropriate and complete.

Thank you for the opportunity to comment and please accept my apology for my being tardy in sending these questions/comments.

Sincerely,

David Adams/BA

David Adams
Member-at-Large
Saratoga Co. Environmental
Management Council and
Member, Government Liaison
Committee