A Review of HDR's "Conowingo Pond Mass Balance Model"
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The review addresses the following questions:

1. Is the modeling approach reasonable and credible to satisfy the goals defined in the Proposal for Lower Susquehanna River Reservoir System Model Enhancements in Support of the 2017 Chesapeake Bay TMDL Midpoint Assessment?
Yes, there are a considerable list of limitations with this modeling framework as laid out plainly by the modelers; however, on the whole, the approach is reasonable and credible. The constraints on the model appear to be more related to boundary conditions. That is, the model is very sensitive to the upstream nutrient loading. Said upstream nutrient loading appears to have a strong flow-concentration relationship that does not exist in the available data. The only internal dynamics that appear to require some work is the settling and distribution of particulate organic matter. The model calculates a significant spatial gradient that may not reflect the data for percent sediment bed composition and may need to be revisited. Sediment transport and settling rates are notoriously difficult to calibrate. Perhaps the most relevant and interesting modeling approach is the incorporation of an archive stack that allows SFM to account for long term changes in sediment erosion and deposition. This reviewer would have liked to see the CPMBM fully coupled version run with and without this archive stacking approach to determine how necessary this aspect of the model is.

2. Do the Lake Clarke/Lake Aldred HEC-RAS Model (HEC-RAS Model) and Conowingo Pond Mass Balance Model (CPMBM) provide added value to the information available to the EPA Chesapeake Bay Program and the State of Maryland? Do they inform and advance the current science and understanding of the Lower Susquehanna River Reservoir System?

Yes, with the caveat that surrounding models, specifically the upstream WSM model output, would need to improve to incorporate some of the advances put forth here. Again, one way to test the importance of the advances by the HDR team would be to examine nutrient and carbon loading with and without archive stacking. Clearly, one of the major conclusions drawn from this current effort is that large resuspension events are decreasing the trapping efficiency of the reservoir system, but that most of this resuspended material is relatively inert (G3). However, it should be kept in mind (and I believe the modelers know this better than this reviewer) that the fractionation of this material is a convenient discretization and that eventually, the Bay Program would consider a more continuous solution for the issue of organic matter diagenesis. However, the answer to the question as stated: "Do they inform and advance the current science and understanding of the Lower Susquehanna River Reservoir System?" is yes because the alternative of assuming a uniform bioavailablility of resuspended sediments is clearly and oversimplification that this model solves.

3. Given the data which were available to the modelers, evaluate the model results, input parameters, and modeling assumptions made to determine if the models perform reasonably.

The answer to this question is partly contained in the answers to Questions 1 and 2. However, the other large limitation is the lack of explicit processes in the water column. It would appear that there is more data in the sediments of this system than the water column which is unusual. Given these constraints, the modelers took an approach that assumes very little processing of the material within the reservoir. These assumptions are not egregious, however, it should be noted that under low flow conditions, these processes would be expected to have a large influence on the results. Because this model is focused on the potential effects at higher flows, the assumptions in the model are even more defensible. Models are at their best when they are informing the management community of data and process level gaps in our understanding. This model appears to point to a systematic assumption in the upstream nutrient loading model (concentration-flow relationship) that is undermining this downstream model. Additionally, the modelers point out the need for better information on G-partitioning, however, frankly, this reviewer contends that the G-fractioning is less an issue than upstream boundary conditions and general complexities of modeling sediment transport correctly. One last point for the modelers/Bay program to consider is more calibration of the flows at which resuspension occurs, it would appear that the model results at high flow are sensitive to this threshold and it is not clear in the modeling report how this value is determined.

4. Are the modeling outputs developed under this study appropriate to help inform or guide the suite of Chesapeake Bay Program models (i.e. the Watershed Model and Water Quality and Sediment Transport Model)?

The short answer is yes. As a reviewer I would be interested in discussing some of these results with the Bay program. How this information would be used is perhaps the better question. That is, there is an argument to be made that says these results argue that Conowingo is largely a pass through and Holtwood loading generally equals Conowingo Loading. Consistent trapping efficiency appears to result in consistent linear responses under various management conditions. Under 2011 conditions this does not hold true and so 18 year averaging may hide some of the utility of the model to capture extreme events that may have non-linear effects downstream (that is, extreme events like Tropical Storm Lee may have effects in the Bay that are not simply scaled to its effects on loading).

5. While keeping the goals of the study in mind, could the models and outputs be improved? If possible, please identify specific areas of potential improvement (e.g., model input datasets/parameters, modeling assumptions, process description, other modeling systems or programs, etc.).

What follows is a list of potential improvements, not necessarily crucial improvements:

1. Modeling the water column with a process based RCA-like model
2. Empirical study of the relationship between flow and resuspension in this system

3. More in depth modeling of sediment transport to homogenize organic content of the sediment to determine if the current spatial gradient is ameliorating loads to the bay by having resuspended material with high organic content far from the dam.

4. Revisiting some of the ratios and assumptions regarding the relationship between water quality parameters within the assumptions of the model: From page 8: "For PON we utilized the observed TN reported by UMCES from the flux cores (0-1 cm); due to concerns of the presence of coal in the surface sediments, for POC we multiplied the observed TN reported by UMCES from the flux cores (0-1 cm) by 8." and "In part, this is due to the "recipe" for making carbon loading and concentration estimates for CPMBM based on the USEPA WSM. The recipe, as described earlier, estimates POC eight times the WSM PON concentration. Since NOx is between a third and half of the total organic nitrogen, this limits the estimated maximum value of TC to about half of the observed value and helps explain the discrepancy in CPMBM computed TC versus the observed data." and DOP is assumed to be 22.6% of P.

One suggestion would be to allow these relationships to vary using a monte carlo approach and the observed range for Chesapeake Bay data. In some models with very quick run times like the stand-alone SFM model, this may be fairly simple, whereas in the fully coupled model, this would be more difficult.

BELOW ARE COMMENTS REGARDING THE REPORT AND SOME QUESTIONS FOR THE JUNE1ST PHONE CALL

1. Introduction

"Has begun implementation…" should probably be "has been implementing since 19XX"
"However, the estimated dissolved oxygen…" should really not say 'over-estimate' but rather say that it may not be correctly estimating.
"Conservative approach" or perhaps more accurately, "relatively simple approach"
"...has been asked" by whom? May want to reword this.
"This assumption appeared…" under what circumstances (which model implementation? The Chesapeake Bay models?)

2. Methods

Define acronyms.
"In this application, the SFM was modified so that under the 10 cm active depth, there is an archive stack that is comprised of another one-hundred-forty 1 cm slices and a deep bed layer that is 3 meters in depth." This line is slightly confusing. 10cm plus 140*1cm slices equals 150 cm. What is meant by the 3m deep bed? Is that another 150cm? Or a stand alone layer that is about 3m deep (in reading further, I think the latter is the case, disregard).
Why no diagenesis in the deep bed?

Still not clear how the model gets deposition right...simply the deposited material in ECOMSED? (CPBMB uses a mass balance approach using the difference between the loadings in WSM to assume the rest is deposited). One suggestion might be to fit the ammonia data and compare deposition with Boynton?
DATA SOURCES AND ANALYSIS

7cm/year at the mouth of Broad Creek. Emphasizes the importance of the erosion/deposition SFM addition.
Interesting that %TN:TP ratios are so much lower than redfield...but nitrification-denitrification could explain.

Figure 3 missing?

Missing parenthesis on page 6 after pers. comm.
Figure 6: Is it possible that there were significant vertical structure in individual cores?

Profiles not shown for NH4, SRP, and Fe but they generally increase down core.

Long cores: what did a vertical profile of pore water look like for these cores?
Negative slopes for carbon diagenesis (hypotheses?)

PAGE 7
Units on carbon-TCO2 and nitrogen-NH4? Also, nitrogen is misspelled here.

Is there a reason to discount two of the data points for nitrogen and go with 7.0e10-5/day?

PAGE 8
Be clear that TN is multiplied by 8 due to Redfield. ALthough I agree that ignoring the carbon data directly due is the right decision.
I think Figure 10/11 is misreferenced?
Would it be possible to see the fits? And were the parameters constrained in any way?

Page 9
The text between Page 8 and 9 could be made more clear.
‘Make a statement on how…”
“Since, the diagenesis experiments are estimating
a blended reactivity rate (i.e., a mix of G1 and G2), these results may imply that there is a slightly greater percentage of G2 material in Conowingo Pond sediments, hence the lower blended rate.”

I wonder if one more statement regarding how these data can be used in the modeling effort would be advisable. If one is simply measuring the blend of rates, there is no way to disentangle the rates to apply to the traditional reactivity classes.

Part C on Page 10 - ‘multiplying’

It would be useful if references were provided (or justification) for some of the assumptions on Page 10. For instance, is 22.6% a standard obtained from the literature or from an analysis of the CIMS database for the typical percentage of organic phosphorus? Overall, the process is clever and certainly useful.

Is Figure 11 mislabeled as Run 05? Page 12: Deposition is still generally higher in the model than Boynton (this appears to be addressed when discussing N deposition on Figure 12). A potential question for HDR: usually Boynton's estimates are assumed to include resuspension and are therefore assumed to be higher than net deposition. If this is the case, are the estimates of deposition useful enough for this comparison?

Climatology approach: Is it worth adding a run that uses the actual temperature record for 2015? I see the generally point of the climatological runs, but interannual differences may be important to the overall mass balance, especially if management and climate are changing conditions in a non-random way. One could simply use the climatology for the entire run and then use 2015 during the last year of the SFM simulation.

PAGE 13: Comparisons look reasonable but the variability in the observed data demonstrate that if one is really trying to fit the observed data, real overlying water column conditions would be necessary. Since the goal of this project is to compute loading on the order of years-decades, I understand the reason to construct the model accordingly.

POP percentages are mentioned twice in paragraph.

Page 14
"A potential source of the excess inorganic P might be attributed to the deposition of PIP." Some clarification here would be helpful. Deposition of P observable or in the model?

High variability in observed phosphorus fluxes: are they an indication of more variable DO conditions than previously thought?

PAGE 15 - by dividing EPA's loads by their estimate of flow to get concentration and then in turn using a different flow, it brings up the issue of how much flow is used by EPA to calculate concentration. A question for the group.

Are there details of how HDR calculates flow? Could be explicit about this before the RESULTS section.

PAGE 16 - I agree with the approach of performing the DO calculation to further refine which simulation (RUN 1, 2, or 3) should be used when estimating bioavailablilty ratios. But is the exclusion of photosynthesis/respiration problematic?

Top of Page 17 - nutrient concentration was taken from USEPA, not loading, correct?

If inflow concentration is a problem, does this only happen at high flows? And in turn, is that due to the flow-concentration relationships in WSM?

Ultimately, the relationship between flow and concentrations is a linear positive one according to the model and not in the data. This contributes to the mismatch in phosphorus loading. But can we trust that the data is adequate to conclude that there is no relationship between flow and concentration?

Page 19 - change the use of the word ‘recipe’

Page 20 - ‘will focus’ changed to ‘focused’

Page 21 - Label the section where discussion will follow for the reader’s benefit.

Page 22 - Figure 50 - it is not clear if this is data or a model. If it is a model, then the resuspension under flow is a function of a parameter, correct?

Page 23 - How were management scenarios chosen? A 10.5% reduction seems very specific.

Page 24 - are the figure references correct here?