Review of the Lower Susquehanna Watershed Assessment

STAC Review Report

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REVIEW OF THE LOWER SUSQUEHANNA RIVER WATERSHED ASSESSMENT

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INTRODUCTION AND EXECUTIVE SUMMARY

Background

The Chesapeake Bay Program's Scientific and Technical Advisory Committee (STAC) assembled a team of 11 professionals with backgrounds in resource economics, and watershed, riverine, and estuarine processes to review the Lower Susquehanna River Watershed Assessment report. As stated in the first five sentences of the LSRWA report's Executive Summary (p. ES-1), "The U.S. Army Corps of Engineers, Baltimore District (USACE), and the Maryland Department of the Environment (MDE) partnered to conduct the Lower Susquehanna River Watershed Assessment (LSRWA). This assessment concludes with this watershed assessment report to better inform all stakeholders undertaking efforts to restore the Chesapeake Bay. The purpose of this assessment was to analyze the movement of sediment and associated nutrient loads within the lower Susquehanna watershed through the series of hydroelectric dams (Safe Harbor, Holtwood, and Conowingo) located on the lower Susquehanna River to the upper Chesapeake Bay. This included analyzing hydrodynamic and sedimentation processes and interactions within the lower Susquehanna River watershed, considering strategies for sediment management, and assessing cumulative impacts of future conditions and sediment management strategies on the upper Chesapeake Bay. The need for this assessment is to understand how to better protect water quality, habitat and aquatic life in the lower Susquehanna River and Chesapeake Bay."

As summarized in the letter to the review team from the STAC Executive Secretary, "The [LSRWA] report includes a main text (>200 p.) summarizing all of the analyses conducted and conclusions from those analyses. Thereafter are four technical sections (Appendices A-D) and input data and literature for each of these technical sections (Appendices E-H). The report also contains miscellaneous information in Appendices I (Stakeholder Involvement) and J (Overview of LSRWA Plan Formulation, including Descriptions of sediment management strategies evaluation and costs and a Summary Table of Major (14) Modeling Scenarios and Results). The technical sections are: Appendix A: Sediment Reservoir Transport Simulation of Three

Reservoirs in the Lower Susquehanna River Basin, Pennsylvania using HEC-RAS - Langland/USGS report (31 pp., plus sub-appendices); Appendix B: Sediment Transport Characteristics of Conowingo Reservoir - Scott/ERDC report (57 pp., plus sub-appendices); Appendix C: Application of the CBEM Package to Examine the Impacts of Sediment Scour in Conowingo Reservoir on Water Quality in the Chesapeake Bay - Cerco/ERDC report (124 pp.), with individual results for all CBEM scenarios available on request; and Appendix D: Estimated Influence of Conowingo Infill on the Chesapeake Total Maximum Daily Load - Linker/EPA report (28 pp.).

The charge from STAC to the review team was: "You should focus your comments on the following [questions], but you are encouraged to provide additional comment that would improve the analyses, report, or its recommendations." The body of review is thus organized into sections in response to that series of questions. Below is a general reaction of the review team to the LSRWA report followed by an Executive Summary of the review team's responses to the series of questions. Following the Executive Summary, the expanded responses to the series of questions is provided.

General reaction of the review team to the LSRWA report

The majority of the reviewers of the LSRWA report agree that its authors have done a commendable job in trying to address an extremely challenging set of issues. The authors have assembled a considerable body of useful observational data, applied sophisticated models, and "chained" the results together to assess the impacts of recent hydrologic and water quality processes on the Lower Susquehanna River and the Chesapeake Bay. Overall, the results of the study are reasonable, the major conclusions are important, and the report's recommendations are by-and-large appropriate and productive. It is obvious that considerable and thoughtful effort has gone into accrual and presentation of the widely disparate types of information used in this report. The project was an enormous effort with multiple participants, and the authors did an impressive job bringing together a wide range of information to support their report.

The science associated with assessing the evolving condition of the Lower Susquehanna River and its effects on the Chesapeake Bay is exceptionally challenging. As far as the reviewers are aware, the Conowingo situation is truly unique. A major reservoir that had been an effective trap for fine sediment and associated nutrients has largely transitioned to one that no longer has an ability to perform this long-term function. It is likely that this kind of transition has never been well documented before, and there are not analogous systems for which modeling efforts have previously attempted to predict how a system will behave as it moves through this transition. The science that needs to be done here is at the cutting edge of what sediment transport and water quality science has ever accomplished in the past. Thus, there are no standard models and protocols for such a study, and the existing capabilities are understandably limited. Hence, it is not surprising that the review team identified many sections of the report that would benefit from revisions, corrections and/or additional analysis.

Although the constructive criticisms provided by the reviewers are significant, they do not fundamentally undermine the importance of key conclusions and recommendations that follow logically from the findings of the LSRWA study. As interpreted and modified by the review team, these (A) conclusions and (B) recommendations include: (A1) The Conowingo Reservoir is essentially at full capacity and is no longer a long-term sink helping to prevent sediment-associated nutrients (primarily particulate phosphorus) from entering the Chesapeake Bay. (A2) Increases in particulate phosphorus loads entering the Bay as a result of the full reservoir are likely causing significant impacts to the health of the Chesapeake Bay ecosystem. (A3) Sources of nutrients upstream of the Conowingo reservoir have far more impact on the Chesapeake Bay ecosystem than do the increases in nutrients caused by scour plus reduced deposition in the reservoir. (A4) Managing sediment via large-scale dredging, bypassing and/or operational changes are clearly not cost-effective ways to offset Chesapeake Bay water quality impacts from the loss of long-term trapping of sediment-associated nutrients. (B1) As soon as possible, follow-up studies should more fully quantify the impact on Chesapeake Bay water quality from increases in sediment-associated nutrients brought about by reservoir infilling. (B2) There is no compelling reason to reduce sediment loads *per se* from the Susquehanna watershed to compensate for increased sediment passing out of the Conowingo reservoir. Nutrients are the main problem, not sediments. (B3) Additional particulate phosphorus load reductions from the Susquehanna watershed (beyond present WIPs) should be considered to compensate for changes to the Conowingo.

Executive Summary

Question 1: Does the main report clearly define the goals, strategies, and the results/conclusions of the study, and also present adequate background material at a level suitable for understanding by non-technical audiences?

The goals stated in the main report (which stress both sediment and nutrient management) are inconsistent with the methodological approach taken by LSRWA (which mainly emphasized sediment) and appear not to be the study's original goals. This review recommends that the original goals of the study (i.e., sediment management to extend the life of Conowingo Dam more than nutrient management to protect Chesapeake Bay water quality) be presented in the introduction followed by a fuller explanation of how and why the focus of the study evolved in time. Both the Executive Summary and Chapter 9 of the main report (entitled "Assessment Findings") present four categories of conclusions that generally correspond to each other. Within the individual context of the Executive Summary or Chapter 9, each set of conclusions is well written and easy to follow and understand. Their general content also includes the most important results and conclusions of the study. However, the phrasing, main emphasis, and ordering of these four categories is different in the Executive Summary versus Chapter 9, which is unnecessarily distracting. This review recommends that the four categories of main results/conclusions be presented in the same order in both the Executive Summary and in Chapter 9 and the headers be made more consistent and compelling. (Note that the answers to this question did not address the scientific validity of the study's results/conclusions in detail; that is the focus of Questions 3 and 4.) Although the background material within the main report is indeed presented at a level suitable for non-technical audiences, this review recommends that large portions of the background material (specifically all of Chapter 2, 50+ pages in length) be moved to an Appendix. The remainder of the main report never refers to Chapter 2. A non-technical end-user of the present report who attempted to read it in sequential order would likely be side-tracked by Chapter 2, and find it harder to locate the key material and findings of the LSRWA.

Question 2: Are the alternative sediment management approaches clearly described and documented? Does this background material provide supporting evidence for the finding and conclusions of the study with regard to alternative sediment management approaches?

Where clearly defined as methods for reducing the cubic yards of total sediment present in the reservoir, the alternative sediment management approaches were found by the large majority of the reviewers to be well-documented, well-described, and comprehensive. It should be emphasized that the positive comments regarding the analysis and comparison of alternative sediment management approaches depend on the fact that the main conclusions regarding the alternative sediment management approaches did not critically depend on the fidelity of the HEC-RAS and AdH models. As a result, the uncertainties in the reservoir modeling process should not have much influence on the overall findings. It must also be stressed early and repeatedly that the dollar costs associated with alternative sediment management approaches specifically focus on the cost of reducing the amount of total sediment behind the dam, not on the cost of managing the impact of associated nutrients on the Chesapeake Bay. Further analysis would be required to appropriately rank the alternative strategies based on a more environmentally relevant total cost in terms of dollars per pound of nitrogen and/or phosphorus reduction.

Questions 3 & 4: Does the main report provide clear, supporting evidence for the results, findings, and conclusions of the study? Does the report adequately identify key uncertainties in the model applications which, with better information, could change the predicted outcomes of the alternative management scenarios evaluated in this study?

The most important conclusions which follow logically from the findings of the LSRWA study are generally well-supported by the overall content of the study. Nonetheless, there are many areas that can be improved. The comments in this section focus on specific aspects of the study that are key sources of uncertainty but have not been fully explained as such in the main report. This section of the review also highlights some sections of the report that are most likely erroneous and/or are most in need of improvement or additional explanation. Although the report lists and discusses sources of uncertainty, it expresses the expected confidence intervals on its model predictions less often. Although there is no single accepted procedure for reporting uncertainty in the context of scenario modeling, a part of the report should more explicitly explain why confidence intervals on predictions are generally not provided.

Key areas of concern which are expanded upon in response to Questions 3 and 4 include: (1) Stated sediment discharges from the Conowingo Dam are inconsistent with the literature. The report authors should either correct their numbers or present a clear explanation that reconciles why their estimates are significantly different from other estimates that are based on analysis of observed data. (2) Reduced deposition associated with reservoir infilling has been neglected. The fundamental issue motivating the LSRWA study is that the net trapping efficiency of Conowingo Reservoir has decreased dramatically over the past 15 to 20 years. Net trapping efficiency is the sum of increases in average annual scour and decreases in average annual deposition. However, the simulations and calculations in the study only considered the increase in scour. (3) Grain size effects within and exiting the reservoir were not sufficiently considered. The combination of two grain size effects – (i) changing grain size in time in the reservoir and (ii) the greater effects of fine sediment in transporting nutrients - mean that the effects of the reservoir on water quality have not reached a full dynamic equilibrium. However, the report did not address whether reservoirs were in dynamic equilibrium with respect to nutrients other than by assuming that if sediment was at equilibrium, then nutrients were also. (4) Limitations of the HEC-RAS and AdH models were not made sufficiently clear in the main report. The HEC-RAS modeling effort was largely unsuccessful, and the HEC-RAS simulation was largely abandoned as an integral part of the main report. Although consistent with four observed, integrated sediment-related properties of the system, the AdH model was not fully validated, and the AdH model was forced by boundary conditions outside the range of observed values. This means that the AdH model alone was not reliably predictive, and until the AdH model has been improved, observations should instead be emphasized to support the most important conclusions of the LSRWA study.

Question 5: Are the recommended follow-up evaluations and analyses (Section 9.1) complete and comprehensive as well as clearly stated to enable the next phase of work to continue under the Partnership's Midpoint Assessment?

Many of recommendations for future work and modeling tool enhancement are very good and are consistent with the views of this review. However, the recommendations as presently written over emphasize the significance of sediment (relative to nutrients) and do not include some important additional possibilities. One of the outcomes of this study should be to identify areas where our scientific understanding may be insufficient to achieve management goals, and to suggest future scientific studies to provide this knowledge. Follow-up studies need to consider the full range of hydrologic conditions, from moderate to high flows, which generally do not result in scour (but still reduce the deposition of sediment-associated nutrients in the reservoir), all the way up to the very high but very rare events that do result in scour. The emphasis in the future should shift from the relative vague impact of additional "sediments and associated nutrients" to the differential impact of specific particulate and dissolved nutrients.

A key question is how to proceed to do the "adjusting" of the TMDL milestones to account for increased sediment-associated nutrients passing out of the reservoir. Key recommendations of this review in this regard include: (i) that the effect of the change in overall "trapping capacity" must be accounted for (the LSRWA analysis done so far relates only to increased scour and not to total trapping capacity), (ii) priority should be given to accounting for the added particulate phosphorus, and (iii) the additional sediment load (other than associated nutrients) should NOT be an additional burden on TMDLs. Calculations by Hirsch suggest that the net loss of trapping efficiency by Conowingo may be in the range of 2300 tons of phosphorus per year. The basic question facing the midpoint assessment then is: what would it take in terms of upstream phosphorus management in order to overcome the impact of ~2300 tons of phosphorus? This estimate is not highly accurate. The team that did the LSRWA report has the simulation expertise and capacity to test these estimates, but they have not yet performed this specific simulation. The follow up to this LSRWA effort really needs to address these estimates and replace them with better ones if they can (including uncertainty bounds).

This review supports enhanced long-term monitoring of the flux of sediment and associated nutrient flux in the lower Susquehanna River system. This LSRWA report certainly makes the case that it is needed, as there was inadequate observed data to sufficiently understand nutrient transport dynamics or for model calibration and validation. Updated technology should play a key role in enhanced long-term monitoring of the Lower Susquehanna/upper Chesapeake Bay (and other river/estuarine transitions in the Chesapeake Bay system). There are a variety of technologies that can be applied using *in situ* sensors to collect an essentially continuous record of sediment concentrations and flux for use in inferring sediment-associated nutrient transport, including inference of grain size distribution.

Question 6: Do the technical appendices provide the necessary documentation for the models and their applications in support of the study's results, findings, and conclusions?

As described above in response to Questions 3 and 4: (i) the HEC-RAS modeling effort was ultimately unsuccessful, and results of the HEC-RAS simulation did not form an integral part of the main report, and (ii) the existing application of the AdH model, although generally consistent with the validation data used, was not reliably predictive beyond constraints provided by a few integrated observations of sediment-related properties of the system. Additional comments from individual reviewers directed toward the HEC-RAS and AdH modeling efforts beyond the items discussed in response to Questions 3 and 4 are included in this section as responses to Appendix A and B. Appendix C and Appendix D of the LSRWA Draft Report describes applications of the Chesapeake Bay Environmental Modeling Package (CBEMP) to estimate changes arising from additional scour from behind Conowingo Dam during large events. Unlike the AdH and HEC-RAS models, which are relative new model systems that had not been applied before to the Lower Susquehanna environment, the CBEMP model has a decades-long history of applications and evolutionary improvements within the Chesapeake Bay system, including numerous peer-reviewed publications assessing its performance in this specific environment. The application of the CBEMP model to the LSRWA effort is generally well done, and the conclusions are reasonably supported, especially given that the LSRWA was intended as an exploratory analysis.

Additional comments on the appendices and main report

The last section of the review contains additional comments from individual reviewers referring (i) to the remaining appendices and (ii) to more isolated issues within the main report, with the latter specified by page number. Although these are individual issues that were not necessarily identified by multiple reviewers, these remaining comments are nonetheless important and should also be considered by the LSRWA authors in any revisions and/or follow up analyses.

SYNTHESIS OF INDIVIDUAL REVIEWERS' COMMENTS

Question 1: Does the main report clearly define the goals, strategies, and the results/conclusions of the study, and also present adequate background material at a level suitable for understanding by non-technical audiences?

Goals and Strategies

Although clearly stated on p.10, the goals declared in the main report (which stress both sediment and nutrient management) are inconsistent with the methodological approach taken by LSRWA (which mainly emphasized sediment). The main report's Introduction (p.10) states that: "…the specific goals and objectives for the LSRWA effort were: 1. Generate and evaluate strategies to manage sediment and associated nutrient loads delivered to Chesapeake Bay… 2. Generate and evaluate strategies to manage sediment and associated nutrients available for transport during high-flow storm events to reduce impacts on Chesapeake Bay. 3. Determine the effects to Chesapeake Bay due to the loss of sediment and associated nutrient storage within the reservoirs on the lower Susquehanna River." Note that the above goals statement repeatedly weights "sediment and associated nutrient(s)" equally. Yet the study put much more of its effort into addressing issues of sediment management to extend the life of Conowingo Dam as opposed to nutrient management to protect Chesapeake Bay water quality. In fact, there is very little content in the overall LSRWA effort which focuses on managing nutrients. The inconsistency between the stated goals and the general strategies followed is an issue that propagates throughout the analysis for the entire assessment.

Although the word "goal" does not appear in the Executive Summary, the Executive Summary does state (on p.ES-1), "The purpose of this assessment was to analyze the movement of sediment and associated nutrient loads within the lower Susquehanna watershed through the series of hydroelectric dams (Safe Harbor, Holtwood, and Conowingo) located on the lower Susquehanna River to the upper Chesapeake Bay. This included analyzing hydrodynamic and sedimentation processes and interactions within the lower Susquehanna River watershed, considering strategies for sediment management, and assessing cumulative impacts of future conditions and sediment management strategies on the upper Chesapeake Bay." A similar "purpose" statement appears in the Introduction on pp.5-6. Note that the word "nutrient" appears only once in the above statement, and the purpose of the study was mainly to address "sediment management". The above quote seems to be a more realistic statement of the actual goals of the study.

It appears that the goals as presently listed in the Introduction to the main report were not the original goals of the study. Page ES-4 states, "The conclusion that the primary impact to living resources in Chesapeake Bay was from nutrients and not sediments, was not determined until late in the assessment process… Management opportunities in the Chesapeake Bay watershed to reduce nutrient delivery are likely to be more effective than sediment reduction opportunities at reducing impacts to the Chesapeake Bay water quality and aquatic life from scour events, but these management opportunities were not investigated in detail during this assessment." By crafting a goals statement that reflects findings from late in the study, the report's authors may have unintentionally undermined the connection between the study's goals and approach. The assessment actually focuses much more on the movement of sediment and options for sediment removal from the Conowingo reservoir rather than managing the associated nutrients to improve water quality.

This review recommends that the "original goals" of the study (i.e., sediment management to extend the life of Conowingo Dam more than nutrient management to protect Chesapeake Bay water quality) be presented in the introduction followed by a fuller explanation of how and why the focus of the study evolved in time. Presently, the report only briefly states that during the course of the study it became clear that nutrients were more important than sediment. More background is needed in the introduction regarding how and why this judgment was made and how the course of the study then evolved.

Results and Conclusions

Both the Executive Summary and Chapter 9 of the main report (entitled "Assessment Findings") present four categories of conclusions that generally correspond to each other. Within the individual context of the Executive Summary or Chapter 9, each set of conclusions is well written and easy to follow and understand. Their general content also includes the most important results and conclusions of the study. However, the phrasing, main emphasis, and ordering of these four categories is different in the Executive Summary versus Chapter 9, which is unnecessarily distracting. Also, the most meaningful aspect of each category of findings is not necessarily used as the header for its respective category. Note that in this section of the review, the scientific validity of the study's results/conclusions is not addressed in detail; that is the focus of Questions 3 and 4.

This review recommends that the four categories of main results/conclusions be presented in the same order in both the Executive Summary and in Chapter 9 and the headers be made more consistent and compelling. Working from the ordering of the main findings as presented in Chapter 9, the following changes are recommended. The title "Finding #1: Conditions in the Lower Susquehanna reservoir system are different than previously understood" (p. 189) is simultaneously vague and obvious. The subheading that immediately follows: "Conowingo Reservoir is essentially at full capacity; a state of dynamic equilibrium now exists" is much more meaningful and to the point. A choice similar to the first bold heading in the Executive Summary (p. ES-1) – i.e., "Loss of Long-Term Trapping Capacity for Sediment-Associated Nutrients" could also be a good choice. One of these two (or another similarly meaningful header) should be used in both sections.

"Finding #2: The loss of long-term sediment trapping capacity is causing impacts to the health of the Chesapeake Bay ecosystem" (p. 192) aligns with the third heading in the Executive Summary ("Nutrients, Not Sediment, Have the Greatest Impact on Bay Aquatic Life", p. ES-3). Again, the Executive Summary header is more meaningful. They should be made consistent and both be listed second (or both third) among the main findings. Finding #3 – which might be slightly rephrased to "Sources upstream of Conowingo Dam deliver more nutrients and therefore have more impact on the upper Chesapeake Bay ecosystem than do the sediment-associated nutrients associated with the Conowingo Dam" (p. 193) – corresponds mainly to the second heading in the Executive Summary ("Watershed is the Principal Source of Sediment", p. ES-2). In this case, the spirit of the finding in Chapter 9 is more appropriate because it emphasizes nutrients. Again, they should be made consistent and both be listed third (or both second).

"Finding #4: Managing sediment via large-scale dredging, bypassing, and dam operational changes, by itself does not provide sufficient benefits to offset the upper Chesapeake Bay water quality impacts from the loss of long-term sediment trapping capacity" (p. 195) corresponds to the fourth heading in the Executive Summary ("Sediment Management Strategies", p. ES-3). These are problematic in that the phrase "Sediment Management Strategies" is not a conclusion, while Finding #4 as phrased in Chapter 9 is not strictly true. Repeated large-scale dredging and removal of accumulated sediment and isolated placement elsewhere would indeed restore sediment trapping ability of the reservoirs and associated water quality benefits to the upper Chesapeake Bay. The (valid) compromising issue is cost effectiveness. Thus, the fourth header/finding needs to be rewritten, perhaps to something with a meaning along the lines of "Managing sediment via large-scale dredging, bypassing, and dam operational changes is not a cost-effective approach to offsetting the upper Chesapeake Bay water quality impacts from the loss of long-term capacity for trapping sediment-associated nutrients".

Background Material

Although the background material is indeed presented at a level suitable for non-technical audiences, this review recommends that large portions of the background material contained in the main report (specifically all of Chapter 2) be moved to an appendix. The level of sophistication of Chapter 2 is suitable for scientifically literate audiences who are not necessarily well-versed in the environmental issues and technical approaches specific to Chesapeake Bay restoration. One reviewer noted approvingly that the level is well suited to an introductory course on Chesapeake Bay taught at their university. However, multiple reviewers also noted that the placing of so much background material (52 pages) in Chapter 2, immediately following the report's Introduction, is actually counterproductive.

The remainder of the main report never refers to Chapter 2. In contrast, the other Chapters refer to each other, and the sub-sections of the report's Introduction (Chapter 1) explicitly mirror the next several report chapters. Sections 1.1-1.3 and 1.5 "Project Authorization/Project Sponsors and Partners/Study Area/Significance" are analogous to Chapter 3 "Management Activities in the Watershed", Section 1.10 "Assessment Approach" (p. 13) is analogous to Chapter 4 "Modeling Tools and Applications", Section 1.6 "Problem Background" (p. 8) is analogous to Chapter 5 "Problem Identification"), and Section 1.9 "Assessment Products" (p. 10) is analogous to Chapter 6 "Development of Sediment Management Strategies"). Thus Chapter 2 notably interrupts the flow of the report and seems to be an awkward add-on.

A non-technical end-user of the present report who attempted to read it in sequential order would likely be side-tracked by Chapter 2, and find it harder to locate the key material and findings of the LSRWA. They might logically assume that Chapter 2 was part of the information that was input to the models used to complete the Assessment, when it actually contains free-standing information compiled separately from the rest of the project. Removing Chapter 2 from the main body of the report will make the main report much more manageable for end-users, reducing its length of the text by 25%, from over 200 pages to less than 150 pages. The average length of the remaining eight chapters of text would then be 19 pages each, compared with the unwieldy 50+ pages of Chapter 2. Nonetheless, it is not recommended that the background information in Chapter 2 be deleted from the Assessment as a whole. The material contained in Chapter 2 is generally well-written, useful information that, within the context of the Appendices, could be helpful to some readers to better understand this complex subject. It would be most logical to change Chapter 2 into Appendix A, but its precise location may be left to the authors.

Question 2: Are the alternative sediment management approaches clearly described and documented? Does this background material provide supporting evidence for the finding and conclusions of the study with regard to alternative sediment management approaches?

Where clearly defined as methods for reducing the cubic yards of total sediment present in the reservoir, the alternative sediment management approaches were found by the large majority of the reviewers to be well-documented, well-described, and comprehensive. However, the distinction between strategies, sediment management alternatives, representative alternatives, and scenarios should be made clearer at an earlier stage of the report. Multiple reviewers found these concepts difficult to separate as they initially read through the report. It should be emphasized that the positive comments regarding the analysis and comparison of alternative sediment management approaches depend on the fact that the main conclusions regarding the alternative sediment management approaches did not critically depend on the fidelity of the HEC-RAS and AdH models. The alternative management scenarios are actually only weakly coupled to the reservoir transport models; they are clear consequences instead of the long-term sediment budget as constrained by observations. As a result, the uncertainties in the reservoir modeling process should not have much influence on the overall findings.

It must also be stressed early and repeatedly that the monetary costs associated with alternative sediment management approaches specifically focus on the cost of reducing the amount of total sediment behind the dam, not on the cost of managing the impact of associated nutrients on the Chesapeake Bay. Consider, for example, scenarios 2C (open water placement, bypassing) and 3A (upland placement, Stancill Quarry) in Table 6-6 (p. 168). The estimated unit costs are only $6-12 per cubic yard for scenario 2C with bypass dredging, whereas the costs are $23-35 per cubic yard for scenario 3A with upland placement. This makes it seem that upland placement is about 3x more expensive than bypassing. However, it relies on the implicit assumption that a ton of sediment that is bypassed has the same environmental impact as a ton of sediment that is dredged and placed upland in a landfill. Even a ton of sediment that is removed is not uniformly equal given that nutrient (primarily P) loads are tied most closely to clay-sized sediment.

Although it is not specifically described as such in the draft report, the overall economic analysis in the LSRWA is in essence a cost-effectiveness analysis (CEA). In contrast to cost-benefit analysis in which the positive and negative impacts of alternatives are expressed and directly compared in monetary terms, CEA expresses some key impacts in non-monetary but still quantitative terms. One of the common challenges faced when conducting a CEA is that key impacts are often multi-dimensional and therefore difficult to fully capture and summarize in a single indicator. In specific parts of the main report and appendices (e.g., Table 6-10 in the main report entitled “Sediment Management Strategy Summary Matrix” and appendix attachment J-3 “Summary Table of Sediment Management Alternatives’ Evaluation”), environmental impacts are presented side-by-side with the dollar costs of reducing cubic yards of sediment in the reservoir. In such a context, it is sufficiently clear that the “cheaper” alternatives are not the “better” alternatives.

This review recommends that further caveats be included throughout the report to clarify that the dollar-based cost estimates regarding alternative sediment management approaches are specifically for reducing cubic yards of total sediment in the reservoir, not for achieving broader goals regarding nutrient reductions. The dollar-based cost estimates in Table 6-6 are reported in the Executive Summary (p. ES-4) and elsewhere in the assessment report. Wherever the dollar-based cost estimates are stated, their meaning with regard to increasing reservoir capacity rather than improving water quality should be more clearly indicated. The report should also emphasize that further analysis would be required to appropriately rank the alternative strategies based on a more environmentally relevant total cost in terms of dollars per pound of nitrogen and/or phosphorus reduction.

There are an enormous number of potential management alternatives, far too many to consider in depth for a program of this size and scope. Narrowing them down to a reasonable number of representative examples, then further limiting those examples by a scoping analysis to a set that might be worth further study, was an appropriate approach to handle this complexity. Unfortunately, an artifact of the categorization techniques used to make sense of the multiple potential scenarios is an artificial limitation of cross-category considerations and benefits. Combinations of different scenarios and management approaches might actually be the best possible approach, either in parallel or sequentially. For example, a one-time major dredging in the region just upstream of the dam, followed by bypassing from further upstream to slow subsequent infill, might have longer lasting effects. These more complex scenarios are clearly beyond the scope of this report, but they should be mentioned and acknowledged as worthy of exploration.

The economic analysis and comparison of the alternatives could be further enhanced by considering, and at least discussing in qualitative terms, other possible co-benefits (and possibly co-costs) of the alternatives. For example, in addition to reducing loads to the Bay, many of the BMPs provide other ecosystem service benefits such as improved water quality upstream from the Bay, carbon sequestration, water storage/flood control, recreation benefits, etc. (see USEPA report EPA/600/R-11/001 for an analysis that includes some of these co-benefits). These co-benefits could meaningfully offset some the costs associated with the BMP alternatives; therefore, they should be acknowledged in the report. Similarly, dredging activities may entail aesthetic disamenities (i.e., external costs), which would have the opposite effect by increasing the total costs of this set of alternatives.

Question 3 & 4: Does the main report provide clear, supporting evidence for the results, findings, and conclusions of the study? Does the report adequately identify key uncertainties in the model applications which, with better information, could change the predicted outcomes of the alternative management scenarios evaluated in this study?

As discussed in the introduction to this review, the most important conclusions which follow logically from the findings of the LSRWA study are generally well-supported by the overall content of the study. Nonetheless, there are many areas that can be improved. The comments in this section focus on specific aspects of the study that are key sources of uncertainty but have not been fully explained as such in the main report. This section of the review also highlights some sections of the report that are most likely erroneous and/or are most in need of improvement or additional explanation.

General uncertainty

Although the report lists and discusses sources of uncertainty, it expresses the expected confidence intervals on its model predictions less often. For example, if storm sediment transport can hardly be measured to within +/- 50%, model predictions can hardly be expected to be better (for example, in Appendix A, an error of about this range is indicated for predicting reservoir scour). Ideally, ranges should be provided for all model predictions (rather than a specific number). Although there is no single accepted procedure for reporting uncertainty in the context of scenario modeling, a part of the report should more explicitly explain why confidence intervals on predictions are generally not provided.

Statistics inferring a 10% change in transport might be (well) within the uncertainty of the total-transport values. References to differentials as small as 0.1% (for example, see table 6.7) imply accuracies in characterizing the sedimentary system that could not be confirmed by any type of measurement known by the reviewers. However, if qualified as model results and indications are in relative terms, there may be value in such numbers as long as all such values are qualified as "well within measurement error." Hence, "we cannot infer any significant change" should be stated up-front based on results of such analyses. In many of the modeled scenarios, the changes in attainment of water quality criteria with fairly large management actions would appear to a non-technical reader to be very small. For instance, p. 135 states: "…estimated…non-attainment…of 1 percent, 4 percent, 8, percent, 3 percent…" One should ask if such estimates are statistically significant. Similarly, in appendix A, p. 25, the net deposition model indicated that ~2.1 million tons net deposition in the reservoirs occurred in 2008-11. This is the difference of two order-of-magnitude larger numbers (22.3M tons entered the reservoir, 20.2M tons entered the Bay). There is a rule-of-thumb in sedimentology: ±10% in concentration or transport is 'within error'. Does the precision of the computed difference fall within the margin of error in these metrics?

Propagation of uncertainty in model predictions from the reservoir sediment transport prediction to those of the Bay Ecological Model may be significant. If optimally constrained by observations, reservoir calculations may have reasonable accuracy and precision when averaged over longer timescales, but less accuracy over shorter timescales. However, the key timescales for many biological processes are much shorter than those of an annual sediment budget, and this could be a major source of uncertainty in the predictions of the efficacy of the sediment management scenarios. This disparity in process timescales is important to address in the text and in the conclusions of the study.

Anoxic volume days appears to be a variable that is relatively more sensitive to the model scenarios presented in the report (e.g., Table 6-8). This suggests something alluded to in the report on several occasions, that a large fraction of the deep water in Chesapeake Bay is sitting on the threshold of being anoxic, and seemingly small changes in concentration (0.2 mg/l) lead to substantial relative changes in anoxic volume. It is worth clearly stating that the high sensitivity of this one criteria to small changes in load stands out among the other variables (e.g., chlorophyll-a, chl-a). It strikes the reviewers that changes in chlorophyll and dissolved oxygen associated with "normal" inter-annual variability in climate and nutrient loading are much higher than those associated with additional Conowingo Dam-derived nutrients as simulated here. One might conclude that given this fact, that the potential effects of dam-derived particulates are trivial. Given the quantifiable effects on chl-a and DO derived from these model simulations, however, it may be worth emphasizing that it would be difficult to tease out the Dam effects from observations given natural variations in load, flow, chl-a, and DO, and that the models are therefore necessary for assessment and prediction.

Stated sediment discharges from the Conowingo Dam are inconsistent with the literature

On p. 113 the report states, "A close inspection of the model simulation results indicate that trace erosion does occur at lower flows (150,000 to 300,000 cfs), which is a 1- to 2-year flow event. This finding is consistent with prior findings reported by Hirsch (2012)." The Hirsch (2012) findings are different from what is expressed here. The relevant statement from Hirsch (2012) is: "The discharge at which the increase [i.e., the increase in suspended sediment concentrations at the dam] occurs is impossible to identify with precision, though it lies in the range of about 175,000 to 300,000 cfs. Furthermore, the relative roles of the two processes that likely are occurring – decreased deposition and increased scour – cannot be determined from this analysis."

In the second paragraph of p. 190, the report states that "…a major scour event will occur once every 4 to 5 years, and minor scour events with trace amounts of erosion will occur every 2-3 years (150,000 to 300,000 cfs)…" The statement that minor scour events will occur every 2-3 years is incorrect on two counts. First, the events in excess of 150,000 cfs happen on average about 3 times per year (not once every two to three years). The number of such days (with daily mean discharge between 150,000 and 300,000) is about 11 days per year. In contrast, days with daily mean discharge greater than 400,000 cfs happen about 0.45 days per year. Second, it is not clear that the increase in sediment loads in the 150,000 to 300,000 cfs range is really a result of scour. It may be that it is mostly a result of a decrease in the amount of deposition that occurs at these flows. The statement overall seems intended to downplay the importance of these moderately high flow days, but they do make a substantial difference in the trend in net outflows of sediment and phosphorus to the Bay. The impacts of changes must be viewed as a product of magnitude and frequency. The magnitude of the change at the 400,000+ cfs range is large, but the frequency is small. The magnitude of changes in the 150,000 to 400,000 cfs range is smaller, but the frequency is much higher.

Also on p. 190, the report indicates that, "The total sediment outflow load through the dam… increased by about 10 percent from 1996 to 2011…" These results are so strongly at odds with other published numbers on this subject that some explanation and discussion is certainly required. Hirsch (2012) reports an increase in flow-normalized flux over the period 1996-2011 of 97 percent (see Table 3 of Hirsch). Also, Langland and Hainly (1997) published an estimate of change in average flux from about 1997 to the time the reservoir is full of 250%. Reporting a 10% increase in light of these two other findings appears erroneous.

At bottom of p. 190 the text reports on reductions in TN, TP, and TSS as 19, 55, and 37%, respectively, for the past 30 years for loads "to the lower Susquehanna River", referenced to http://cbrim.er.usgs.gov. This could mean loads delivered to the upstream end of the reservoir system or loads delivered at the downstream end where the river enters the Chesapeake Bay. At the Marietta site (above the reservoirs), the actual results were downward trends of 29.9, 40.1, and 44.8%, respectively, while at Conowingo the USGS reports 22.3, 0.8, and 10%. In either case, these numbers are different from those mentioned in this report. An additional issue here is that the USGS values are trends in flow-adjusted concentration, expressed in percentage terms. The text is referring to trends in nutrient and sediment loads and not trends in concentrations.

For each of the above cases, the report authors should either correct their numbers or present a clear explanation that reconciles why their estimates are significantly different from other estimates that are based on analysis of observed data.

Reduced deposition associated with reservoir infilling has been neglected

The fundamental issue motivating the LSRWA study is that the net trapping efficiency of Conowingo Reservoir has decreased dramatically over the past 15 to 20 years. Net trapping efficiency is the sum of increases in average annual scour and decreases in average annual deposition. However, the simulations and calculations in the study only considered the increase in scour.

Based on the use of WRTDS (a published statistical method for evaluating fluxes and trends in fluxes, a method that is central to two of the publications cited by the LSRWA, i.e., Hirsch, 2012 and Zhang et al., 2013), the estimated flow-normalized flux of TP out of Conowingo Dam between the 1996 condition and the 2011 condition has increased by 3.65 tons/day (going from 6.64 to 10.29 tons/day). This increase equates to a 5329 ton increase over the four year simulation period. In the LSRWA report, the simulation of scour is captured as a single event with a total magnitude of 2600 tons (see Table 5-9 scenario 3). Based on these two numbers, it would be logical to conclude that the remainder of the increase over the 1996 to 2011 period would be the difference between 5329 and 2600 tons, which is 2729 tons. This suggests that about half of the increase in loading of TP to the Bay comes in days with discharges below 400,000 cfs. Without having the model simulate the full range of changes due to the loss of trapping efficiency, the report's authors have introduced a large uncertainty into the results, and it is one that surely leads to an underestimate of the impact of the filling of Conowingo.

This issue underlies a significant weakness in the report, which is that it focuses its inquiry on the impact of large, but infrequent, scour events rather on the total impact of the change in trapping efficiency of the reservoir system. The flaw in the logic of the report is expressed, for example, on p. 137: "Generally speaking, when flow is below the scour threshold, sediment is estimated to settle out when in dynamic equilibrium. Consequently, water quality in the Bay is the same as it would be if the reservoirs were still filling as long as there is no scour event." This same logical flaw appears again on p. 142: "...without storms, the reservoirs will continue to trap sediments in the short term at rates consistent with today", and on p. 190: "…major scour events will occur once every 4 to 5 years, and minor scour events with trace amounts of erosion will occur every 2-3 years (150,000 to 300,000 cfs) and at all other times, the reservoir will continue to trap sediment and associated nutrients."

The review recommends that all statements that indicate that reservoir trapping of sediment and associated nutrients is unchanged in the absence of scour be removed. In addition, a discussion should be added to the report that clearly states that decreases in the average annual deposition in the reservoir in the absence of scour have not been considered and that the added transport of sediment-associated nutrients past Conowingo Dam due to decreased deposition may be as large as that added due to increased scour.

Grain size effects within and exiting the reservoir were not sufficiently considered

It is reasonable to expect that the texture of the sediment behind the dam will continue to coarsen through successive scour events and deposition interludes. The report states in several places that less sand exits the dam at the downstream end than enters the reservoir at the upstream end (e.g., p. 191), both because it deposits first at the upstream end and because it is much more prone to settle out of suspension or transport as bedload after it is remobilized. The reservoirs are not in a final state of dynamic equilibrium if the sediment entering the reservoirs is coarser than the sediment leaving. The reservoirs appear to be preferentially storing sand and, with scour, exchanging that sand for silts and clays. Over time, this implies even a “full” reservoir will gradually fill with sand at the expense of fines. This progressive change in grain size will gradually change the threshold conditions for sediment entrainment and change the grain size of sediments that are typically mobilized by scour. But how long with this transition take? Thus, the dynamic equilibrium that is described in the report is changing over time, and it would be worthwhile to try to predict how many cycles of deposition and scour might be required before the dynamic equilibrium becomes less dynamic.

Nutrients associated with fine sediments, not with the total load of sediments, are the main water quality concerns. The report acknowledges that sand-sorbed P is more or less inconsequential in P transport. However, all sediment-discharge values are expressed as “total loads.” Since P transport is closely tied to fines, and presumably very closely tied to clay-size particles, transport metrics computed for fines, and particularly for clay-size particles, might yield different conclusions than those derived from “total” load comparisons. It is also important to clearly define what is meant by total load. Sedimentological nomenclature denotes “total load” as all material in transport, be it defined as bedload plus suspended load (with caveats), or bed-material load plus washload (no caveats) (ASTM International, 1997, Terminology for Fluvial Sediment; Diplas et al., 2008, p. 306 at: http://water.usgs.gov/osw/techniques/Diplas_Kuhnle_others.pdf). It is not clear that “total load” refers to either of these metrics in the LSRWA report.

The combination of these two above grain size effects, (i) changing grain size in time and (ii) the greater effects of fine sediment in transporting nutrients, mean that the effects of the reservoir on water quality have not reached a full dynamic equilibrium. However, the report did not address whether reservoirs were in dynamic equilibrium with respect to nutrients other than by assuming that if sediment was at equilibrium, then nutrients were also. Although information was provided in the report on particle-size distributions in reservoir bed sediments and sampled streamflow, and on the relevance of particle size to P concentrations, there was no tie-together and possible revision of load values to indicate how the interplay of these metrics might result in changes to a fundamentally important metric, fine-sediment (particularly clay-size material) transport to the Bay. In reality, as the reservoir evolves in time toward containing a larger and larger fraction of sand, the sediment scoured during large events should progressively contain fewer fines and fewer associated nutrients.

The review recommends that the concept of dynamic equilibrium be clearly qualified in the report to indicate it does not yet apply to sediment grain size, and thus it does not yet fully apply to the flux of fine sediment or associated nutrients.

Limitations of HEC-RAS model were not made sufficiently clear in the main report

The HEC-RAS modeling effort was largely unsuccessful, and the HEC-RAS simulation was largely abandoned as an integral part of the main report. Reasons for this are listed on pp. 22-24 of Appendix A (Section 6.0 Model Uncertainty and Limitations). Apparently the primary reason why the HEC-RAS modeling failed had to do with sediment calculations: fall velocity estimates appeared to be off and could not be corrected and, for the cohesive model, only a single critical shear stress could be defined for the cohesive sediment bed. Critical shear stress simulations produced contradictory results, which remained unresolved. A member of the review panel familiar with the RAS model has also found that RAS, in the beta version used in the LSRWA study, simply makes incorrect calculations. Although HEC-RAS results were used to supply sediment to the upstream end of the 2d AdH model, this use of RAS output was fortunately of minor significance to the overall LSRWA effort. Upstream inputs to the Conowingo Reservoir could also be estimated from empirical analysis using USGS transport data.

Another source of inconsistencies between the HEC-RAS application and USGS transport estimates may be associated with the different definitions of bed-material load, washload, suspended load, bedload, and total load. The transport equations available in HEC-RAS produce bed-material load data. Bed-material load is that material in transport – suspended or as bedload – that is characteristic of the material composing the bed. The remainder, which is not characteristic of the bed, is washload, and washload is substantial in this system. Estimates from equations/models based on bed-material size data and hydraulic information do not include the washload component. Empirically derived “total load” estimates, on the other hand, are actually suspended-sediment loads, as is the output from the Estimator model. Suspended load is operationally defined as being computed from material captured by a suspended-sediment sampler. It includes the washload component. This is a distinction that seems to be fundamentally important to the LSRWA with respect to the interpretation of modeled and empirical suspended-sediment transport data. Conversely, most if not all output from the equations and models other than the empirically-based Estimator model and transport curves is expressed as bed-material load. Using different output metrics from various models amounts to computing “apples and oranges” in sediment and nutrient transport.

Presently, the description of the conclusions associated with HEC-RAS in Chapter 4 of the main text seems to underplay its poor performance. For example, p. 81 of the main report states, “For the LSRWA effort, the HEC-RAS model outputs were deemed acceptable because they provided relative understanding of the physical process of the upper two reservoirs…” This positive statement appears inconsistent with the analysis of HEC-RAS performance as assessed by this review. This review recommends that the failure of the HEC-RAS model be reported more clearly and fully in the Chapter 4 of the main report.

Although consistent with four observed, integrated sediment-related properties of the system, the AdH model was not fully validated

The AdH model was not calibrated, but instead the authors use what they refer to as a validation approach. Their use of the term validation differs from what is considered to be the norm in which a model is calibrated using part of a data set (typically part of a period for which data are available) and then evaluated, or validated, by applying the calibrated model to the balance of the data set. In their approach, four different parameter choices (defined primarily by the critical shear stress of the bed sediment) were used in four simulations and the model calculations were compared to simple, integrated properties of the system (net erosion and deposition cumulated over four years, average annual sediment retention during non-storm years, estimated reservoir scour for different events, and percent sand in sediment discharge). One of the four simulations was then selected for further work based on (i) net erosion and deposition for the entire reservoir, cumulated over four years (targeted to a net deposition of 3.0 to 4.0 million tons), (ii) estimated reservoir scour for different events (targeted to the USGS scour curve), (iii) sediment retention of about 1.0 to 1.5 million tons per year during the non-storm period, and (iv) percent sand in sediment discharge over Conowingo Dam less than 10%. That is, only four scalar quantities were used to validate the model. This is slim verification for such a large and detailed model. What one can conclude is that a suite of parameters and boundary conditions for a large, detailed, and complicated model with many possible interactions was found to come roughly close to mimicking the gross behavior of the system based on matching four simple, integral measurements.

Although many other aspects of the model can be evaluated, no further information is given in that regard. No information is provided regarding whether more detailed internal results of the model (e.g., patterns of local scour and deposition) were evaluated for plausibility and consistency. The major reason for using a 2d model is to capture both lateral and along-stream changes. Reservoir bed elevations are available from 2008 and 2011, which provides an opportunity to evaluate model performance. But it is not clear that these elevations were used in this way. No information is given regarding whether other combinations of parameters might have produced similarly good integral results. It remains unresolved whether the match between model and measurement was a case of getting the right answer for the wrong reasons.

Another aspect of this AdH discussion that could be improved is the effect of the uncertainties in AdH predictions near the Dam face. These uncertainties take two forms – the overly simple approximation of the boundary condition at the dam that is acknowledged in the text, and related problems associated with 3D flow effects very near the dam. How far away from the Dam are the predictions of flow and sediment transport likely to be affected? Will these uncertainties affect predictions of scour significantly, or are the primary scour zones outside the region of influence?

This review recommends that the limitations of the AdH application as described above be made much clearer in both Appendix A and the main report.

AdH was forced by boundary conditions outside the range of observed values

The tenuous nature of the model validation is made more uncertain by the fact that the values for the key boundary condition (critical bed shear stress for sediment entrainment) in the final selected model fell largely outside the range of values measured by the SEDFLUME or were unmeasured and taken from the literature. The critical stress reported from SEDFLUME had a median value of 0.083 lbf/ft^2, while the critical stress used for the top foot of the reservoir sediment in the selected AdH model was reported as 0.03 – 0.06 lbf/ft^2, largely outside the range of the measured SEDFLUME values. The critical stresses used in the model for sediment one-to-two feet and two-to-three feet below the surface were 0.1 lbf/ft^2 and 0.14 lbf/ft^2, respectively. These depths were unsampled in the field, and the critical stress values were taken from the literature.

Because sediment transport has a threshold and is a nonlinear function of flow, errors in the bottom boundary condition will, in general, produce large errors in calculated transport rate and morphodynamic change. Even though a set of parameters was selected that provided rough similarity to the observed net scour and deposition over the four year run time, this provides no assurance that the predicted patterns and timing of transport, scour, and deposition match reality. Thus the application of the AdH model does not extend the empirical understanding provided by existing reservoir bathymetry and stream gaging.

Rather than attempt to further refine the sediment bottom boundary conditions with direct measurements, a more promising approach would be to collect suspended sediment measurements in the reservoir and evaluate the choice of model boundary conditions by comparing a time series of transport calculations against observations. This could provide direct calibration, *in situ*, of model performance. The extensive and spatially explicit output from a model such as AdH provides many varied opportunities for evaluating model performance. Does the model aggrade where we see aggradation and degrade where we see degradation?

The AdH model alone was not reliably predictive; observations should be emphasized

The AdH application in this study has been developed to the point that scour and deposition is consistent with what is already known from survey and sampling observations. However, the AdH model application does not refine that empirical understanding. The uncalibrated and weakly constrained model application provides an essentially heuristic basis for scenario evaluation, and the AdH model has not, as yet, added substantial new understanding of the sediment dynamics of the reservoir. The modeling does not strongly reinforce the existence of a scour threshold at 300,000 and 400,000 cfs. At best, it can be said that an uncalibrated model was found that produces results that are consistent with that particular threshold. Other choices of model input (including bed sediment parameters more in the range observed by SEDFLUME) would likely produce a different scour threshold.

The report would be more convincing if some of the observational data in the Appendices were incorporated into the main report, particularly those that bear on the time-varying sediment budget. This is really the heart of the matter, and highly sophisticated (but weakly constrained) models are not essential to illustrate what is happening. Many of the important conclusions of the report regarding sediment and nutrient delivery from the reservoirs are direct consequences of the sediment budget of the system and its evolution through time (i.e., the amount of sediment delivered by the watershed and trapped by the reservoirs and how these amounts have varied over the last several decades). Even if the fidelity of the models can be questioned, the observational data are compelling.

At present, the conceptual weaknesses of the models and the inherent uncertainty in model results are not well-described or acknowledged in the main report. Many of the basic conclusions of the study are direct consequences of the long-term sediment budget of the watershed and reservoir system, and while supported by the model results, are independent of the weaknesses of the modeling, and therefore citing them would strengthen the conclusions. These can be easily added. The uncertainties are discussed more openly in Appendix A, and it is recommended to expand that discussion and move some to the main report.

Question 5: Are the recommended follow-up evaluations and analyses (Section 9.1) complete and comprehensive as well as clearly stated to enable the next phase of work to continue under the Partnership's Midpoint Assessment?

Many of recommendations for future work and modeling tool enhancement are very good and are consistent with the views of this review. Alternate and/or improved models should continue to be pursued in future work in combination with additional data collection. Predictions from multiple models should be compared, including relatively simple models (e.g., the analytical model presented at the beginning of Appendix C). However, the recommendations as presently written over emphasize the significance of sediment (relative to nutrients) and do not include some important additional possibilities. Recommendations #1 and #4 (reproduced below as 5.1 and 5.4), should be expanded to acknowledge the need to develop improved scientific understanding of several key issues, rather than simply collecting more data and developing better models. One of the outcomes of this study should be to identify areas where our scientific understanding may be insufficient to achieve management goals, and to suggest future scientific studies to provide this knowledge. The goal of these studies is not simply to provide monitoring data for analysis or model calibration, but to provide the conceptual understanding of the system that will lead to the improvement of models.

5.1. Before 2017, quantify the full impact on Chesapeake Bay aquatic resources and water quality from the changed conditions in the lower Susquehanna River and reservoirs:

Throughout the text following Recommendation 1, "sediment and associated nutrients" should be changed to "sediment-associated nutrients". A key finding of the LSRWA study that has large ramifications for management activities is that sediment-associated nutrients have a much larger impact on Bay water quality than the sediments themselves (see additional discussion of this issue within Section 5.2 below). In addition, Recommendation 1.2 would be better written as something like: "Determine the quantity and nature of the sediment-associated nutrients transported downstream under current conditions (dynamic equilibrium) versus conditions that prevailed in previous times when the reservoirs had substantial trapping ability." Follow-up studies need to consider the full range of hydrologic conditions, from moderate to high flows, which generally do not result in scour (but still reduce the deposition of sediment-associated nutrients in the reservoir), all the way up to the very high but very rare events that do result in scour (see additional discussion above under the header "Reduced deposition associated with reservoir infilling has been neglected").

The filling of Conowingo has relatively less impact on nitrogen inputs to the Bay (because so much of the total nitrogen load to the Bay is in the dissolved form) but it does cause a substantial increase in the particulate phosphorus inputs. Ecosystem studies of the Chesapeake Bay based on present-day algal communities indicate that Bay hypoxia is more sensitive to dissolved nitrogen input than particulate phosphorus input, so perhaps the hypoxia is presently relatively insensitive to particulate phosphorus from Conowingo. Alternatively, a resulting shift toward higher P:N ratio in the nutrients input to the Bay could result possibly in a shift in the types of phytoplankton. This is speculation - but could a higher P:N ratio cause a shift towards more blue-green algae that have an ability to fix N from the atmosphere, so that even with decreasing N loads from the watershed, the N available in the Bay might not decline due to this ecological shift? In any case, the emphasis in the future should shift from the relatively vague impact of additional "sediments and associated nutrients" to the differential impact of specific particulate and dissolved nutrients.

Future studies should also test the sensitivity of the biogeochemical model simulations to the reactivity of the scoured material for both nutrient release and water column and sediment respiration, which are linked. The latter influences DO directly. This could potentially require additional state variables to represent different pools of particulate matter in the sediments and water-column. Surely, scoured materials and other solids are deposited in sediments, where diagenesis releases nutrients back to the water column to fuel algal growth. But before these materials are deposited in sediments, they could fuel respiration directly in the water-column. They should also contribute to sediment oxygen demand, or in the case that sulfides are released to the water column from sediments, to lagged water column oxygen demand.

Also, where do the nutrient-containing particles flowing past the dam in large flow events go? Are they trapped in the turbidity maximum? Do they escape to the mid-Bay, and if so, under what flow conditions? Are the present parameterizations of transport behavior adequate to address these questions?

5.2. U.S. EPA and Bay watershed jurisdictional partners should integrate findings from the LSRWA into their ongoing analyses and development of the seven watershed jurisdictions' Phase III WIPs as part of Chesapeake Bay TMDL 2017 mid-point assessment:

One of the most important statements in the LSRWA report is found on p. 75. It says: "EPA stated within Appendix T of the 2010 Chesapeake Bay TMDL that 'if future monitoring shows the trapping capacity of the dam is reduced, then EPA would consider adjusting Pennsylvania, Maryland, and New York 2-year milestones loads based on the new delivered loads' (USEPA, 2012). In practical terms, this means that nutrient and sediment loads from the Pennsylvania, Maryland, and New York portions of the Susquehanna River basin would have to be further reduced to offset the increase in sediment and associated nutrient loads in order to achieve the established TMDL allocations and achieve the states' Chesapeake Bay." It seems clear that analyses of the monitoring data have indeed shown that the trapping capacity of the dam has significantly reduced. Now the question is how to proceed to do the "adjusting" of the TMDL milestones. That issue is thus the following: how much of a decrease in loads delivered to the reservoirs and/or increase in reservoir trapping efficiency would be required? Key recommendations of this review in this regard include: (i) that the effect of the change in overall "trapping capacity" must be accounted for (the LSRWA analysis done so far relates only to increased scour and not to total trapping capacity), (ii) priority should be given to accounting for the added particulate phosphorus, and (iii) the additional sediment load (other than associated nutrients) should NOT be an additional burden on TMDLs. The logic behind this resistance to including treating the sediment load as a penalty is expanded upon in the following two subsections:

The negative impacts of sediment input to the Chesapeake Bay (relative to nutrients) are overstated by present TMDLs and are overemphasized in management priorities

TMDL requirements for sediment loads are most likely overly restrictive. The water quality simulations conducted as part of the LSRWA study further support the conclusion that sediment alone does not have as great an impact on Bay aquatic life and attainment of water quality standards as previously thought. More generally, the common wisdom that sediment input in itself is a problem with respect to water quality is perplexing given that sediment loads in the late 1800's and early 1900's were much higher than they are now, yet Chesapeake Bay water clarity and overall quality were much better then than now.

An underlying assumption at the start of the LSRWA study, and indeed of the CBP in general, is that all sediment is bad. However, it is stated in several places in this report and in the broader literature that some sediments are actually good, important components of the estuarine ecosystem. Certain fishes and most healthy SAV beds need sand as a substrate for reproduction and growth. Even estuarine fine sediments are essential to certain habitats, such as tidal wetlands, and a further reduction in supply of fines to tidal wetlands threatens their sustainability in the face of coastal erosion and/or sea level rise. It is true that turbidity due to fine sediment input can locally limit SAV, but this report clearly points out that turbidity insults associated with scour from the Conowingo reservoir are temporary. Perhaps it is time to revisit the TMDL for sediment, especially sand, and especially in the context of the sediment behind the dam and in the lower Susquehanna and upper Bay.

Given the relatively minor impact of sediments in general (separate from their associated nutrients) to Bay water quality, it is especially clear that the additional sediments (separate from nutrients) associated with the filling of the Conowingo reservoir are particularly insignificant to overall Bay health. The reasonable (albeit approximate) estimate that ~90% of sediments originate from sources other than scour from the Conowingo reservoir suggests that completely mitigating the loss of sediment (but not nutrient) trapping in the Conowingo would solve only around 10% of what is already a minor problem. It is important to further note that minimum water clarity required by TMDLs for SAV habit is obtained in every scenario in Table 5-9, regardless of whether or not the Conowingo reservoir is full or whether or not WIPs are fully in place. Requiring further reductions in sediment input (separate from nutrients) elsewhere to compensate for loss of Conowingo storage, given the expense involved, is not cost-effective.

The overall negative impact of sediment scoured or otherwise moved or bypassed out of the Conowingo reservoir and into the Bay may be further reduced by the fact that it is sandier than sediment otherwise introduced to the Bay. As the "full" Conowingo reservoir evolves, it will continue to get sandier with time. Parts of the lower Susquehanna and upper Bay are sand-starved at present. Sand is a limiting resource for several types of important habitat in the upper Bay and lower Susquehanna, and it is far less likely to harbor high N or P loads. If sand could be bypassed around the dam without entraining significant fines its impacts might be more positive than negative.

The effectiveness of BMPs in reducing sediment loads to the Bay may be overstated by present TMDLs:

The description in Table 5-6 of almost constant flux to the Bay despite major reductions in upstream sources over time is a major point to be considered in thinking about future impacts of BMPs. What is true here might also be true at the watershed scale. Similar results have been seen in historical reconstructions of sediment yields from other watersheds. Reductions have been made in sources, but about the same amount of sediment continues to flow out, which is a small percentage of the amount mobilized upstream, and which appears insensitive to changes in that source amount. This is ultimately a result of massive watershed storage of sediment. Thus, the possibility that sediment BMPs may not lead to a major reduction in sediment coming from the upstream watershed needs to be considered as a real possibility in considering management actions. Models alone cannot answer this question, only more direct measurement in places downstream of BMPs can fully demonstrate whether they are effective.

This issue is again important in the context of statements made on p. 141 indicating that anticipated future changes include increased frequency of scour events associated with climate change but continued decline in watershed loads due to BMP implementation. Given the enormous volume of sediment in various storage compartments in the watershed, greater frequency of scour events may well lead to greater amounts of remobilized sediment, especially as the vast majority of sediment that moves is carried in big storms. Even if WIPs are fully implemented, they may not counter the influence of greater storm frequency, nor is it clear that they would be as effective as assumed even in the absence of greater storm frequency. The amount of sediment in storage with potential for remobilization is orders of magnitude higher than the typical annual load, and even if one believes that stream restoration can be effective in mitigating in-stream sources, there is no way that stream restoration projects will ever be built over enough of the cumulative length of the upstream drainage network to really mitigate this potential source.

The broader question of whether WIPs will actually be effective for sediment on the time scale important to managers is one that is a subject of debate among geomorphologists, and cannot be assumed to be true simply because existing TMDLs are predicated on that assumption. The significant uncertainties in predicting the effects of BMPs on watershed sediment yield must be acknowledged. The Chesapeake Bay Watershed Model, though highly sophisticated, does not account for long-term storage of either water or sediment, and these processes have an important influence on the lag time before improvements can be expected from the WIP process.

5.3. Develop and implement management options that offset impacts to the upper Chesapeake Bay ecosystem from increased nutrient and sediment loads:

It is suggested here that, once more, the phrase “nutrient and sediment loads” in the above recommendation be changed to “sediment-associated nutrients”. This suggestion is consistent with the statement found in the main report two paragraphs below this recommendation (p. 200), but with an added insertion in square brackets: “Nutrient load reduction management and mitigation options are likely to be more effective and provide more management flexibility when compared to relying solely on sediment management options. As such, it is likely more appropriate and cost-effective to increase management actions targeted toward nutrients above and beyond WIP implementation in the Susquehanna River watershed [rather than expand sediment control BMPs in general]. It is therefore recommended to conduct further analysis and modeling to understand costs and water quality influence of controllable nutrient mitigation measures beyond the jurisdictions’ WIPs.” This paragraph goes on to list a number of nutrient reduction strategies. These are fine, but the list is somewhat limited. In terms of overall implications for managing Bay eutrophication there needs to be particular attention to non-point source nutrient management, especially to limiting application of phosphorus to soils where the P levels are already above their agronomic optimum, changing the manner in which chemical fertilizers and manure are applied to the landscape, and also the use of cover crops.

In his work, Hirsch has found that total phosphorus flux to the upper Chesapeake Bay is up by about 51% between 1996 and 2012, representing an increase of about 1300 tons/year. This increase is happening while upstream management actions are taking place to reduce TP flux. During this same period the flux from upstream (measured at Marietta) has been decreasing (in the neighborhood of 1000 tons/year) and most of that since about 2004. This suggests that the net loss of trapping efficiency by Conowingo may be in the range of 2300 tons of phosphorus per year. The basic question is then, what would it take in terms of upstream phosphorus management in order to overcome the impact of ~2300 tons of phosphorus? This estimate is not highly accurate. The team that did the LSRWA report has the simulation expertise and capacity to test these estimates, but they have not yet performed this specific simulation. The follow up to this LSRWA effort really needs to address these estimates and replace them with better ones if they can (including uncertainty bounds).

A statement made in the center of p. 133 is revealing in this context. This is the statement that, though the January 1996 storm simulations do indicate adverse impacts of scour from behind the dam on the Bay TMDL, these impacts are far less than the impacts of not implementing the WIPs already agreed to by the States. Furthermore, the following paragraph on p. 133 provides a first order estimate of the additional watershed nutrient load reductions (using a combination of N and P) that would be needed to offset the DO non-attainment caused by the scour loads. This is one of the most important pieces of information in the report.

5.4. Commit to enhanced long-term monitoring and analysis of sediment and nutrient processes in the lower Susquehanna River system and upper Chesapeake Bay to promote adaptive management:

This review supports enhanced long-term monitoring of the flux of sediment and associated nutrient flux in the lower Susquehanna River system. This LSRWA report certainly makes the case that it is needed, as there was inadequate observed data to sufficiently understand nutrient transport dynamics or for model calibration and validation. Nonetheless, Recommendation #4 should be rephrased to explicitly include studies designed to develop the conceptual scientific understanding needed to manage the lower Susquehanna River system and upper Chesapeake Bay. Gathering data and analyzing it is not enough.

Regardless, updated technology should play a key role in enhanced long-term monitoring of the Lower Susquehanna/upper Chesapeake Bay (and other river/estuarine transitions in the Chesapeake Bay system). There are a variety of technologies that can be applied using *in situ* sensors to collect an essentially continuous record of sediment concentrations and flux for use in inferring sediment-associated nutrient transport, including inference of grain size distribution. Turbidity, laser, densimetric, and hydroacoustic technologies have been/are being evaluated, and some are being integrated into operational monitoring programs (see for example Gray and Gartner, 2009 at: http://water.usgs.gov/osw/techniques/2008WR007063.pdf). Sediment hydroacoustics arguably is the most robust of the technologies for rivers that convey low-to-moderate sediment concentrations, such as the Susquehanna River and presumably most Bay tributaries. Finally, an *in situ* hydroacoustic monitoring system also can provide index-velocity information for computing and/or improving water-discharge computations.

Continued monthly sampling throughout the basin is important, but it is also crucial that sample collection includes a substantial effort to collect data from moderate to high discharge events (including likely scour events but also events that are well below the scour threshold). It is also important to sample within the reservoirs and not just above and below. In particular, suspended sediment and particulate nutrient samples from within the reservoir should help in identifying the discharge at which reservoir scour begins. Further, with new technologies it should be possible to collect water samples in the reservoir during floods. These measurements need not be collected in a complete transect for the purpose of providing the entire sediment flux. Rather, they would provide an indication of the flow, in a time series, at which reservoir scour becomes significant. This, more than the mass balance between inflow and outflow sediment, could be more useful in determining the appropriate bottom boundary condition for models. That is, the bottom boundary condition for substantial bed entrainment would be calibrated to the flows at which this actually happens.

Question 6: Do the technical appendices provide the necessary documentation for the models and their applications in support of the study's results, findings, and conclusions?

APPENDICIES

Below is a summary of review comments specifically directed at the Appendices, beyond those insights provided in earlier sections that indirectly addressed the Appendix contents.

Appendix A

As described above in the section of this review entitled "Limitations of HEC-RAS model…", the HEC-RAS modeling effort was ultimately unsuccessful, and results of the HEC-RAS simulation did not form an integral part of the main report. Additional comments from individual reviewers directed at Appendix A beyond the items discussed in the earlier review section are included here.

The Estimator model was used in Appendix A in spite of the fact that its originator, Dr. Tim Cohn, has indicated his doubt as to whether it is adequate for use with "hysteretic" suspended sediment. Although it well may "work" in this relatively large river – larger rivers with smaller peak-to-base-flow discharge ratios and more languid precipitation-runoff responses tend to exhibit less hysteresis in suspended-sediment concentrations than smaller rivers – additional analysis might be required to confirm or refute that assumption.

Concern was expressed regarding the exclusion from the sediment transport curve of the high suspended-sediment concentration value (2,890 mg/L, at USGS gage 01578310 [Conowingo] on 9/8/2011) in Appendix A, p. 12, Figure 7. There is rumor of a similar 'high outlier' in 2004. The transport curve in Figure 7 may well effectively be discontinuous with a major break around 400,000 ft^3/s. The two transport-curve sections might be nearly parallel. It is possible that the present curve is valid for flows ~≤ 400,000 ft^3/s, and the new curve that would reflect natural increasingly sediment-laden flows plus scoured material is valid for flows ~> 400,000 ft^3/s. A promising approach would be to develop a particle size-to-flow relation and apply it to the transport curve resulting in two (or three) curves, including a fines-transport curve (the principal metric of interest). The concept is graphically similar if mechanistically dissimilar from a discontinuous suspended sediment transport curve that has been shown to occur when flows transition between subcritical and supercritical regimes.

Should the p. 13 Reference to Table 2 be to Table 3?

The p. 36 Summary of USGS sediment concentration and load estimates: there is no period of continuous data collection at Marietta and only a few years between 1979 and 1992 at Conowingo, so how are they estimating comparative sediment loads? The text says USGS has been estimating sediment loads at Marietta and Conowingo since 1987 but does not say how.

The ESTIMATOR was used to project changing sediment load over time. However, in looking at the USGS NWIS site there is only very limited information about actual sediment concentration and load data collected – a number of years during the period between 1979 and 1992 at Marietta, and presumably grab samples, but apparently no continuous record at Conowingo. Given all of this there is some skepticism about how well we really know the comparison between sediment loads at the two stations, especially going back to the early 20th century.

Appendix B

As described above in the three earlier sections focusing on limitations in the AdH model, this review concludes that the existing application of the AdH model was not reliably predictive beyond constraints provided by a few integrated observations of sediment-related properties of the system. The AdH model is only loosely validated and insufficient data are available to confidently evaluate model performance. In its current state, based on the information presented, the AdH model is not capable of extending the information on reservoir performance previously available from bathymetric surveys and stream gaging. Additional comments from individual reviewers directed at Appendix B beyond the items discussed in response to Questions 3 and 4 are included here.

The SEDFLUME results from a small number of cores account for a large fraction of Appendix B. But there is insufficient explanation as to how these results were translated into the parameter set utilized in the six material zones in the model. Given the variability within each core from one shallow layer to the next, and given the variation in particle sizes longitudinally as well as variation laterally across the reservoir in depth and modeled velocity, perhaps there is no way at this point to account for spatial patterns beyond the simple selection of six longitudinal zones; and perhaps it ultimately does not make much difference what choices one makes. But it is odd that so much space was devoted to the empirical results without explanation as to how they were actually applied or what difference the spatial pattern of parameter values within different zones might make, particularly given that a 2d model is being used. In calibrating the model, the authors varied critical shear stress parameters at shallow depths and maximum scour depth to keep the model from scouring too much sediment, but the discussion of how this was done did not make much reference to differences among zones or within zones. The way this issue was handled is not explicitly addressed in the text even though the small number of cores is identified as a source of uncertainty.

p. 4 Figure 1 shows in graphical form the same information that is provided in Table 5-6 of the main report but in each case the citation simply says “provided by USGS”. How do we know that by 1959 (first paragraph, p. 5) there was a relatively constant inflow of 3.2 million tons/yr of sediment flowing into Conowingo?

pp.5-6 The Exelon revised HEC-6 study concluded that scouring flows above 400,000 cfs were net depositional in Conowingo? Not net erosional? Given conclusions provided elsewhere in both the main report and appendices, this is confusing.

p. 22 Under model validation the statement is made that “The maximum sample depth was only about 12 inches due to highly consolidated sediments in deeper layers preventing penetration of the sampling tube.” If this is the case what does it say about the actual potential for scour in a large flood event?

p. 23 Here it says that although samples represented only the top foot of sediment, the model sediment bed was about three feet. It appears from later discussion of choices made for calibration purposes that the three-foot depth had to be modified in order to match better with other information. The choices made here are not always clear.

p. 25 This shows the flow-concentration curve for Conowingo and highlights both the variability at high flow and the existence of only a single point at the upper end of the curve. It would seem appropriate to try to quantify the uncertainty associated with use of this curve and develop a range of values in order to see how this uncertainty might affect conclusions and comparisons.

The USGS curve for prediction of scour as a function of Q has upper and lower bounds; so should the sediment concentration rating curve.

p. 27 The major trend was that most of the scour occurred in the upper 1/3 of the reservoir where there is more sand which constitutes 50% or more of total bed sediment. A significant amount of deposition occurred just upstream of the eastern end of the dam. Was this mostly fines or more sand? What is the effect of the changes here on the particle-size distribution of the deposit as a whole?

p. 28 Model validation involved a parametric model study where bed-property values were manipulated and results compared with USGS scour load prediction. Was any consideration given to whether properties might vary with depth or distance from the shoreline?

p. 29 The choice of limiting depth available for scour to one foot seems like a reasonable one for a lower bound, given what was learned from coring and laboratory tests.

p. 31 When fitting parameters to compute erosion rate – is it not possible to develop some scheme for projecting variation in relevant material properties either longitudinally or laterally? Given that a 2d model is being used and given the spatial patterns of texture and cohesion, this seems like an element that ought to be considered – or else reasons why it cannot be done should be articulated.

p. 33 The authors argue that the uncertainty associated with applications of AdH is made manageable by basing conclusions largely on simulations of management scenarios in which only one variable is changed. This amounts to saying, in effect, 'the model worked OK for a hindcast, even though we had to use boundary conditions that were outside of the measured range or unknown, and we have not documented that the internal workings of the model are making reasonable predictions. So, if we only change one part of the model we can hope that it will reliably calculate the change in system performance.' However, one application of the AdH model was to evaluate scour and deposition relative to different reservoir bathymetry. These applications are not of the change-one-thing-only management scenario type and instead directly depend on the fidelity of the selected model.

p. 33 In discussing role of alternative bathymetry – do these alternatives assume spatially invariant bed material properties?

p. 37 Do these flow fields try to account for the change in flow distribution at the outlet when the gates are opened during high flows? It is pointed out elsewhere that dam operations should be incorporated in the model for future studies – this would seem to imply that this is not the case here.

p. 44 The 2008 to 2011 period was somewhat atypical in terms of the frequency of days above the 400,000 cfs scour threshold. If we look at the frequency of days over 400,000 cfs during the 4-year simulation period it comes out to an average of 1 day per year above the threshold. If we look at the entire period from 1977 through 2012 the frequency of days above the threshold is about 0.5 days per year. Thus, the choice of 2008-2011 as the simulation period will overstate the importance of scour increases as compared to a simulation period that was more typical.

p. 60 In discussion of limitations posed owing to need for a more sophisticated approach to simulating flocculation – is there any way to estimate how much difference this might make to overall conclusions?

In the same paragraph it is suggested that field methods are needed for sampling storm concentrations or turbidity over the entire storm hydrograph. Presumably standard methods can be used for the samples for either concentration or turbidity without having a human operator have to stick a bottle in the flow (as apparently was the case for the single sample taken near the peak during Agnes). Is the issue one of how to deploy sensors or automated samplers in the vicinity of the various gates built to accommodate high flow?

Appendix B-1, Figure 3: One must be careful of drawing straight lines in log-log space that depict a transport curve. At some point, the relation must tail to the right, given that sediment concentrations have absolute limits.

Appendix B-1, Section 5-1: The total annual estimated sediment yield delivered to downstream reservoirs is cited here as 4.2 million tons; but there are multiple other estimates in these documents, mostly less than this value – there needs to be more consistency among these cited values, or else an explanation as to why they are different.

Attachment B-1: "Evaluation of Uncertainties in Conowingo Reservoir Sediment Transport Modeling" -- This section is misnamed. The section provides a useful discussion of different elements of flow and transport through reservoirs. Its basic purpose is to justify the use of a depth-averaged 2d model (AdH) rather than a fully 3d model for the simulation. Their conclusion that a 2d model is sufficient is reasonable (assuming proper calibration/validation). Alas, although uncertainties play a small role in the discussion (basically relating to uncertainties that might arise from reducing 3d flow field to 2d), the section provides no discussion of overall "Uncertainties in Conowingo Reservoir Sediment Transport Modeling." This is unfortunate, because those uncertainties are large and largely unexplored in the study.

Appendix B-1, Section 9: This section presents an AdH model of flow and transport on Susquehanna Flats. No discussion is given of any calibration or testing of the model in this environment, and one must presume that it is uncalibrated and untested. The roughness assigned to the flats with SAV and without SAV (winter) is sufficiently large that the majority of the flow and sediment transport occurs through the dredged channel. This is a reasonable result. The authors then reach a conclusion that is unsupported by the model and quite possibly incorrect: "the relatively higher bed roughness of the shallow flats will tend to continue to route the majority of the flow through the dredged navigation channel below Havre de Grace. Thus, discharge of sediment from Conowingo Dam due to bypassing or flushing operations will have minimal impact on the flats area, with sedimentation occurring in the dredged navigation channel or below the flats area." Just because most of the water and sediment go through the channel does not mean there will be no impact to the flats. If flow extends on to the flats, the authors have not demonstrated in any way that sediment carried in that flow will not deposit on the flats.

In fact, this is how floodplains are formed. If turbid water is being discharged from the dam, one can deposit sediment wherever the water goes. Estimates can be made from the sediment concentration and residence time of water over the flats.
Appendix B-2, Summary and Conclusions. This section is misnamed and should be changed to only “Summary”. There are no conclusions stated here.

Appendix B-4 includes the following on its first page: “…sediment in transport in suspension is directly related to sediment particle size and the degree of turbulence.” Density could also be a factor, particularly if it is true that some 10% of reservoir sediments are coal particles.

Appendices C & D

Appendices C and D of the LSRWA Draft Report describe application of the Chesapeake Bay Environmental Modeling Package (CBEMP) to estimate changes arising from additional scour from behind Conowingo Dam during large events. Unlike the AdH and HEC-RAS models, which are relatively new model systems that had not been applied before to the Lower Susquehanna environment, the CBEMP model has a decades-long history of applications and evolutionary improvements to the Chesapeake Bay system, including numerous peer-reviewed publications assessing its performance in this specific environment. The application of the CBEMP model to the LSRWA effort is generally well done; the writing is clear, the organization is logical, and the text is supported with extensive figures and tables. The conclusions are reasonably supported, especially given that the LSRWA was intended as an exploratory analysis. The data attachments to Appendix C are particularly useful, although they are not specifically reviewed here.

One significant area could use a bit more attention. The period of the CBEMP model simulations is different from the period of the HEC-RAS/ADH scour simulations. The watershed loading scenarios are not the actual scenarios observed during the CBEMP simulation period, but rather projections based on expectations for watershed management practices under two different conditions (2010 implementation and TMDL achieved). The major storm simulation presented uses sediment-associated nutrient concentrations from a different storm entirely, not the simulated storm. As a result of all of these juxtapositions and substitutions, it is unclear exactly what is being simulated and why – the runs do not ever appear to be representative of actual conditions. While the final scenarios make sense and are very revealing, the reasoning behind their construction is hard to follow. A summary of the PHILOSOPHY of scenario construction, not just its mechanics, would help. This description should occur right after the introduction of the modeling tools used, and it should be addressed to an audience that is not familiar with standard practice in the CBP.

As an example of the confusion that can result, it is stated on p. 3 that “the 1991-2000 hydrologic record is retained for this study”. But in the next paragraph, it is stated that the 2010 progress run and the TMDL run of the watershed model are used to specify daily nutrient and solids loads for different scenarios. How can nutrient and solids loads from 2010 and a hypothetical TMDL condition be applied to a 1991-2000 hydrology – doesn’t the hydrology largely drive the loads? Or do the 2010 and TMDL runs specify instead relationships between hydrology and loading that are transportable to different time periods? CBP modeling insiders probably understand this approach, but it will be hard for outsiders to grasp.

Table 3.1 details how the June storm scenario included a "transfer of the load record, hydrodynamic record, and the hydrodynamics". Does this mean that the simulation started on June 1st (with June sunlight and temperature), but included the hydrologic and hydrodynamic forcing as if it were January 1st? Or is it something else? Clearer language should be provided to describe how these runs were actually done. These details are important, because in Appendix C, p. 86, Figure 6-27, it is shown that the impact of the simulated 1996 storm on light attenuation was different in the tidal Bay for the 3 seasons tested, and one may wonder if this is only a biological effect of load.

Interestingly, the long-term impacts of the October Storm on DO seem less than the January storm (-0.25 in Jan from 1997-1999, -0.1 in October from 1997-1999, Figure 6-31). Why would this be? Is more of the January load processed that summer and cycled through the system, while much of the October load is buried over winter? This seems like a point worth investigating.

In Appendix C, there is no mention about how the diagensis (decay) rates for the scoured materials differ from the diagenesis rates of the algal-derived organic material, or how decay rates of the scoured material are treated in general. This is a central aspect of this study, as it controls the nutrient release rates that drive the responses seen for chlorophyll and DO in the numerous simulations reported here. Please include these values.

In Appendix C, p. 25, last sentence: the reviewer could not seem to find the results of these scenarios. They are important, given the fact that 2011 sediment nutrient content is probably more representative of future scour loads than 1996. If these results were missed, please reference the table that describes these different scenarios, or specifically identify the scenarios if they are few enough.

On a positive note, the Analytic Model presented in section 2 of Appendix C is quite well done and is a very useful tool for describing overall expectations and for informing the conceptual model. It would be straightforward (in the future, not for this effort) to expand this model to multiple spatial segments and sediment types in the reservoir, to aid in more realistic screening analyses. This expanded analytical approach would also provide a valuable grounding for more complex numerical analyses in the future.

ADDITIONAL COMMENTS ON THE APPENDICES AND MAIN REPORT

Appendix E

Table 1.2 and the introduction to Appendix E indicate that bathymetric data were acquired in Susquehanna Flats. They were not; only sediment grain size data were acquired.

Appendix H

A question that was not addressed in the report is related to the various techniques for sediment management explored in the literature review of Appendix H. While different kinds of power dredging are mentioned in the Appendix and in the body of the report, a technique known as hydro-suction dredging is mentioned several times in the Appendix but not mentioned explicitly in the report. This technique would be especially useful for sediment bypassing, because it makes use of the huge natural head difference between the reservoir and the river below the dam to maintain flow through a dredging pipe or bypass tunnel. Was this technique considered in figuring the relatively low cost of bypassing, or not? Would it make a difference?

The literature review in Appendix H ignored nutrients.

Appendix J

Are all the costs adjusted for inflation and expressed in constant dollars? The discussion of the BMP costs in J-1 indicates that all these costs are converted and expressed in 2010 dollars using the CPI. Was the same process used for the reported cost values in J-2 for the other alternatives? The main body of the report should clearly state the dollar years and inflation adjustment method.

The economic analysis uses a different interest rate (or discount rate) for the watershed BMP versus dredging scenarios. Specifically, p. 14 in Appendix J says “estimates of annualized costs reflect a 5% discount rate” for the watershed BMP scenario. However, p. 167 in Section 6 says that “annualized one-time investment costs are based on a 50-year project life and the fiscal year 2014 federal interest rate of 3.5 percent” for the dredging scenarios. Appendix J-2 shows the detailed calculations for dredging scenarios based on the 3.5% interest rate. Proper economic analysis should use the same interest rate to compare across the scenarios. The current analysis makes the watershed BMP approach seems more expensive based on using the higher 5% interest rate.

The 50-year project life for the dredging and bypassing alternatives is considerable longer than the range of project lives used for most BMPs. That may well be correct and appropriate, but it deserves some justification and explanation, since it could be an influential assumption.

The current analysis provides a breakdown of the total estimated costs by the three states in Table 3 on page 6 in Appendix J (also used as Table 6-3). But this summary by state/jurisdiction in not highly informative because it just reflects that Pennsylvania is the largest state.

Attachments 2 and 3 on pp. 12-13 in Appendix J show the costs by practice across the three states. However, the current information does not make it possible to assess the variation in cost-effectiveness of the various urban and agricultural BMPs in meaningful terms, such as the dollars per cubic yard of sediment removal. Importantly, the cost-effectiveness between practice types typically varies by one or two orders of magnitude. Hence, the current analysis aggregates all practices types and reports an overall cost estimate at $3.5 billion in Table 3 (or Table 6-3). Then the report provides an overall average cost effectiveness of $256-$597 per cubic yard in Table 6-6, and seems to imply that this watershed BMP approach is supposedly the most expensive. But this assessment that aggregates all practice types may overlook the high degree of heterogeneity in costs between practice types.

At a minimum, the watershed BMP scenario should provide separate scenarios for the agricultural versus urban BMPs. Compare, for example, the costs for agricultural BMPs in Attachment 2 versus urban BMPs in Attachment 3. This shows that urban represents about 90% of the total costs compared to about 10% for agricultural BMPs. But it is unlikely that urban represents 90% of the sediment load. In fact, there are two urban BMPs (urban infiltration BMPs and filtering BMPs) that represent over $2.5 billion, which is two-thirds of the total costs. The unit costs on these two urban BMPs are much higher than other BMPs, but the analysis is aggregated into a single number for cost-effectiveness of this alternative scenario.

Attachments 2 and 3 would be more informative if it included additional columns that provided both the cost-effectiveness in $/cubic yard (or $/ton of sediment) and the total amount of cubic yards (or tons of sediment) for each practice type. The former would provide the ranking in cost-effectiveness by practice type, and the latter would reveal how important this practice is for the overall load reduction. This would allow for a better assessment of the most effective suite of practice types, while not including those practices that are most inefficient. Alternative watershed scenarios could then be designed that look at the option of 100% of the E3 scenario (current analysis) versus another scenario that only adopts 50% of the sediment reduction for the E3 scenario using the most efficient suite of practices. The most effective 50% will be competitive with the dredging scenarios given the extreme heterogeneity in unit costs for ag BMPs in Exhibit 1 on p. 15 and urban BMPs in Exhibit 6 on p. 35 (varies from $0 per acre for conservation tillage to $2,351 per acre for the urban filtering BMP). There is even extreme variation in unit costs within agriculture BMPs that ranges over several orders of magnitude. This further confirms the need to provide disaggregated analysis on the cost effectiveness in $/cubic year by practice type.

There are numerous citations provided in Attachment 4 of the Appendix J on pp. 14-44. But there is no corresponding “References” section to provide the detailed info on these citations.

Attachment 4 of Appendix J on pp. 29-33 includes detailed information on “Septic Systems”. However, septic systems are not discussed at all in the corresponding tables for the cost analysis in Attachments 2 and 3. This needs to be clarified. Future analysis should include septic systems particularly if the analysis is expanded to nutrient management options (not solely sediment strategies) because septic systems are an important nutrient load in rural Pennsylvania.

Other recommended edits/specific concerns for main report, by page number:

ES-2 In multiple places in the main report (ES-2, p. 10, p. 110, p. 141), there is a statement regarding dynamic equilibrium that says, “This state is a periodic cycle.” This statement is very misleading, there is nothing periodic or cyclic about it. The driving event (high flow events of about an annual exceedance probability of 0.2 – a “5-year flood”) is a random event and is not periodic. They may happen in rapid succession or there may be many years between them. All mentions of the equilibrium state being “periodic” should be removed.

ES-3 2nd paragraph: the text beginning with “Modeling done for this….” is confusing. It states that under current conditions, half of the deep-channel habitat is unsuitable. This is then compared to the 2025 conditions with full WIP implementation and increased scour that suggests that attainment in 3 of the 92 segments will not be achieved due to extra loads of nutrients. It is implied that full WIP implementation should lead to completely healthy deep-water habitat, but a new reader would not necessarily catch this. Perhaps a more straightforward way to write this is to state something like "currently half of the deep-channel habitat is unsuitable for life (non-attainment), and given full WIP implementation in 2025 (which should yield 100% attainment), deep-channel habitat in 3 of the 92 Bay segments (X % of deep channel habitat) will remain as unsuitable habitat due to elevated nutrient loads from dam scour".

ES-3 4th paragraph: The last sentence (starting "Given…") is a run-on sentence.

p. 6 "The Susquehanna River is the nation's 16th largest river, and the source of the freshest water …" What is meant by freshest water? Typo?

p. 8 "All reservoirs act as a sink….." A sink of what? Sediment? Perhaps it is obvious, but it is helpful to state clearly.

p. 8 "Due to flow deceleration as the water enters the reservoir, sediment transport capacity decreases, and the coarser fractions of the incoming sediment deposited in the reservoir form a delta near the entrance to the reservoir." Awkward sentence – tenses.

p. 8 Last sentence of 5th paragraph: It is worth adding to the last sentence that nutrient-laden sediments are more harmful because they can be utilized to fuel additional algal growth in the tidal waters of the Bay.

p. 9 Last complete paragraph: if the Susquehanna load is 3.1 million tons and 1.2 million tons is released then 59.4% is trapped, not 55%.

pp. 15-16 The flow charts in Figures 1.5 and 1.6 are repetitive but slightly inconsistent. Figure 1.6 makes more sense and may be sufficient.

p. 16 In notes under Figure 1-6, should "partners of this LSRWA effort" be changed to "partners outside of this LSRWA effort"?

p. 24 3rd paragraph: Would be clearer or more mechanistic to say "…than about 0.3 knots because water movement tends to be slowed by frictional forces in shallow water…"

p. 26 "Snow events" do not cause floods. SnowMELT may.

p. 28 Define saprolite or show in Figure 2-5.

p. 32 "Phosphorus binds to ~~river~~ fine sediments and is delivered to the Bay with sediment."

p. 32 (1) 2nd sentence: "Ammonia" should be "Ammonium". (2) 2nd sentence: It is worth noting that although ammonium tends to be less abundant than nitrate in surface waters, it is by far the dominant dissolved N form in deeper waters during warm months. (3) True, nitrite generally contributes little to TN, but nitrite can accumulate to significant concentrations during some times and places, including the region of the pycnocline during mid-summer and after hypoxia/anoxia breakdown in fall. Perhaps adding a line to the sentence to say "….and contributes little to TN for most times and places". (4) It is worth adding that organic nitrogen comes in both particulate and dissolved forms.

p. 34 A factual problem is the statement that indicates that TN, TP, and SS loads from Conowingo have been increasing since the mid-1990's. This is certainly true for TP and SS but for TN the trends have continued to be downward (Hirsch, 2012 reports a decrease of about 3 percent).

p. 36 Should define hypoxia in Figure 2-10 (<2.0 mg/L).

p. 37 Section 2.5.2, 2nd sentence – statement is misleading and should be deleted unless qualified by explaining that because of different designated uses and water quality criteria it is not surprising there is a difference in violations. As is, statement is comparing apples and oranges.

p. 45 Figure 2-14 is not clear as to whether or not the metrics are total over a decade or per year.

p. 46 Many species of plankton are capable of motility. Change "and are passively carried" to "and are, by in large, passively carried".

p. 69 Chapter 3 mentions 3 Chesapeake Bay agreements, which may have been true when this section was written. However, doesn't the Watershed Agreement signed in June 2014 count as the 4th Chesapeake Bay agreement?

p. 72 2nd to last paragraph: The word "special" should be "spatial".

p. 81 "The HEC-RAS model may not be suitable for ….. , active scour and deposition, and particle size." What does this mean with respect to "particle size"? That the model cannot represent particle size well? Explain so meaning is clear.

p. 81 3rd paragraph: Were the boundary conditions generated for the HEC-RAS simulation also used to drive the AdH model? Or was model output from HEC-RAS simulation for the upper two reservoirs used to create the boundary conditions for AdH? Please clarify.

pp. 81-83 The models are stated to be "well developed, widely accepted, and peer reviewed. Yet there are virtually no references in Sections 4.1 or 4.2. References are needed here to demonstrate that HEC-RAS and AdH are indeed peer-reviewed models.

pp. 84-85 Figure 4-3 and 4-4: The mesh in all or part of these figures is almost impossible to see – provide insets at larger scale. Insets in the appendix show this more effectively.

pp. 87-89 In Chapter 4, the description of the method for using the 2008-2011 HEC-RAS and ADH predicted scour in the CBEMP 1991-2000 model runs is confusing. It is simply stated that the reader should see Appendix C for the details. More description should be provided in the text of Chapter 4, at least a better overview of the approach and justification for this somewhat tricky (but justifiable) maneuver.

p. 89 “Since the ADH application period was 2008 to 2011 while the CBEMP application period was 1991 to 2000, an algorithm was applied to adjust estimated loads from the ADH for use in the CBEMP (see Appendix C for details on this algorithm).” This algorithm is not obvious in Appendix C. Should briefly explain here and then explain better in Appendix C.

p. 92 “documented in Chapter 3”(?) Is this a typo?

pp. 97-100 Table 4.2 seems a bit out of context in Chapter 4, referring as it does almost entirely to material in Chapter 6. Although not a requirement, this table would make more sense in Chapter 6 where it is directly discussed.

p. 112 Are the values in Table 5-4 adjusted for variations in flow?

p. 113 In Table 5-5 change “Additional” to “Additional Calculated” and change “Transport” to “Scour-Induced Transport”.

p. 114 Figure 5-4 presents exact same data as Table 5-5. Eliminate.

p. 114 Bottom: annual influx of sediment to Conowingo is here described as 3.8 million tons/yr over the last 20 years with 2 million being trapped. Elsewhere in the document we see different numbers ranging between 3 million and 4.2 million tons. If there are different estimates arrived at in different ways this needs to be made clear.

p. 115 Table 5-6 does not explain how the historical loads or more recent loads were calculated – it simply says that the results were calculated by USGS. More explanation is needed. Also indicate that Hurricane Agnes flows were excluded if they were indeed omitted.

p. 131 The reasoning for using the particular combinations of predicted scour, nutrient loading, and water quality modeling to test for the effects of scour is unclear. The procedure was likely valid, but better explanation is needed.

p. 135 paragraph 4: It would help if there was some discussion of why two upper Eastern Shore segments (CHSMH and EASMH) had non-attainment in Scenario 3. Does low-DO water advect into them from the mainstem or is nutrient availability enhanced by the breakdown of scoured solids that end up in these tributaries?

p. 138 Paragraph 2: Oysters are discussed here within a section that otherwise discussed the modeling and simulation activities. Is there a description of how model analysis was used in this report to determine flow and management effects on oysters? Whatever the case, it should be clearly stated where the oyster effects fit into this report and whether or not model simulations were used to understand effects on oysters.

p. 138 “Nitrogen loads…exceed phosphorus loads…” Given that P concentrations tend to be an order of magnitude lower than those for N, the statement does not tell the reader much, and might unduly impress those lacking an understanding of nutrient concentrations and dynamics.
p. 146 Sources of information here are based on “personal communication” with Kevin DeBell, Greg Busch, John Rhoderick, and Jeff Sweeney. It would be better to document and provide references for the original reports used for the BMP unit costs rather than only personal communication. Page 4 in Appendix J-1 similarly only provides personal communications.

p. 167 “This methodology was not applicable for the watershed management representative alternative since management strategies (e.g., BMPs) once implemented, continue to remove/reduce sediment.” This statement is not true for many BMPs. For example, vegetative buffers self-destruct if they receive excessive sediment – same with most BMPs that trap sediment rather than reducing its generation. As a result of this incorrect assumption, one might question whether costs are one time.

p. 175 3rd paragraph: The word “waters” on line 4 of this paragraph should be “water”.

p. 180 “costs of bypassing (diminished DO, increased chlorophyll) are roughly 10 times greater than the benefits gained from reducing scour.” Indicate exactly where these data are contained in the report. A similar statement also appears in the Executive Summary and on p. 181 and p. 197.

p. 192 In the first summary statement below finding #2, the “upper Chesapeake Bay” ecosystem is highlighted to be the area impacted by the dam. “upper” is an ambiguous word in this case, as the simulations suggest that effects can be seen south of the Bay Bridge (e.g., Appendix C).

p. 193 Second paragraph, line 5: should “frequently not unsuitable” be “frequently unsuitable”?

p. 200 Reference to additional management activities that can provide long-term storage includes mention of floodplain restoration. If this refers to floodplain excavation, there is some concern about this appearing as a recommendation without much more study than has been conducted to date. If it refers to some other form of floodplain restoration some explanatory language would be helpful.

p. 201 The report does not make the case for use in adaptive management, as adaptive management is mentioned for the first time in this recommendation. Adaptive management is not mentioned anywhere but in this recommendation. Thus, the phrase should be deleted here.

Literature Cited

Available at the CRC/STAC offices