EXHIBIT 15 (AR M.1)

ATTACHMENT B

Review of "Numeric Nutrient Criteria for the Great Bay Estuary"

Walter R. Boynton
University of Maryland, Solomons, Maryland

May 29, 2010

EXHIBIT 15 (AR M.1)

EXHIBIT 15 (AR M.1)

Dr. Michael Paul 29 May, 2010
Senior Scientist
Center for Ecological Sciences
Tetra Tech, Complex World, Clear Solutions
400 Red Brook Blvd.
Owings Mills, MD 21117

Dear Dr. Paul,

I have completed a review of the document entitled "Numeric Nutrient Criteria for the Great Bay Estuary" produced by Phillip Trowbridge, P. E. of the New Hampshire Department of Environmental Services. I apologize for being a few days late in completing this task.

My review consists of three parts and these include a series of overview comments, page by page questions/comments and summary responses to the questions posed in your letter of instruction to me (transparency, defensibility, reproducibility and protective).

Overview Comments:

The author makes clear at the start that the development of the TN criteria uses a weight of evidence approach. Given the "state of the art" in estuarine science I think this is a very reasonable approach. In addition, the author used multiple analyses in many portions of this work and that provides enhanced confidence in the results. Simply said, this is a good approach to use in systems as complicated and variable as estuaries.

The analysis is very empirical. That is, it is based on local measurements…quite a pile of local measurements made at many sites during a 9 year period. In addition, there is good reference to the appropriate scientific literature and to adjacent estuarine areas. I think this was a well-grounded analysis.

No complex model was used in this analysis and this adds to the transparency and reproducibility of this work. The approach adopted in this work is far less expensive, less time consuming, easier to verify, easier for the informed public to understand and more readily adjusted as understanding improves. Having said all that, we need to remember that water quality models can do some things that regression analysis can not do or is very limited in capability (e.g., forecasting, exploring for temporal and spatial sensitivities, coping with co-correlated variables).

EXHIBIT 15 (AR M.1)

I was very pleased to see that a conceptual model was used to guide the development of these analyses. What I mean here is that there was a mechanistic basis for the variables used in these analyses. The author used many water quality measurements to develop regression models between TN and chlorophyll-a, DO and water clarity. In addition, continuous monitors were used to estimate DO impairments and finally, relationships between water quality and water clarity were quantified based on light attenuation measurements via in-situ sensors and hyperspectral imagery. All solid approaches.

Specific water quality thresholds were developed for DO (>5 mg/l or > 75% saturation) annual median TN =< 0.45 mg/l and the 90th percentile chlorophyll-a =< 10 ug/l. For protection of SAV annual median TN =< 0.25-0.30 mg/l. There was detailed discussion supporting each of these conclusions.

There is a strong conclusion that N was the limiting nutrient and the only one of consequence. I think they should do a bit more on this issue. I think they are correct but they do not have definitive evidence and they do indicate (correctly I think) that P is important in lakes and rivers. Using N:P ratios really only indicate the potential for nutrient limitation. A single nutrient strategy could be a risk road to take. I do note the author indicated nutrient criteria will be developed for NH rivers and lakes and P will likely be prominent in those analyses. I made a few detailed comments regarding this later in my review. Finally, a word about a risk of a single nutrient strategy. There have been several instances now recorded where P was controlled in the rivers or freshwater portions of estuarine systems (Neuse River, NC and in a Swedish fjord...there may be others). Following P reductions there was a positive response in the freshwater zone but deterioration in the more saline zones. It seems like a portion of the N that had been sequestered in the river now passed through to the estuary and caused increased issues in the N-limited zone. So, as we all know, these systems are linked and thus a duel nutrient strategy is worth thinking about. I should note that it is clear the author is thinking about this issue.

EXHIBIT 15 (AR M.1)

Detailed Questions/Comments:

Pg 2 indicate that nutrient thresholds developed for DO, SAV and benthic invertebrates. Bentos not mentioned in Exec Summary and it should be mentioned if a threshold was developed. Are there no bacterial issues in this estuarine complex? If so, indicate this and any other issues that did not need threshold development

Pg 3. I had expected to see an effort to relate TN concentrations to TN loads to the estuary from the surrounding basin. But, that was not the case. I was surprised and immediately wondered how they will regulate TN concentrations. All this is explained later but it would have been helpful to get this straight at the beginning of the document

Pg 3 I think there needs to be more discussion about the use of median values in assessment zones. I know the authors cited the work of Li et al (2008) but I still feel that the justification was not as strong as it could be…I basically think it opens a strong assessment to attack. In other estuaries (e.g., Ches Bay and others) investigators have found strong relationships using seasonal or annual average values. I think the authors would be well-served by beefing up this section (or doing so in some other section of this report).

Pg 4 Use of a 9 year data set is a strong point in this work as such a temporal record is more likely to capture scales of variability typical of estuarine systems. However, in tables presented later it is also clear that any statements about a nine year effort with monthly sampling is somewhat misleading. If all months were sampled then there would be 108 observations at each sampling site. There are of course many good reasons for not getting a sample for ALL months. But, some sites were not sampled very frequently. This is just a word of caution from a reviewer.

Pg 5 …"some aspects of nutrient cycling". The grab samples of concentrations tell us very little about nutrient cycling. Generally, rate measurements are needed to get serious insights concerning nutrient cycling…just re-write this sentence.

Pg 5 Is there a Table showing nutrient (and other variable) detection limits?

Pg 5 last para. Clarify the 5%, 50% and 6% sentence. What biomass is being referred to here? Is this water column POC? I'm not at all sure doing this (despite EPA guidance) is worthwhile. These ratios really vary widely in my experience. Whatever is decided, this is a weak approach and not much should be inferred from these results.

Pg 6. I have several comments regarding the use of nutrient ratios for determining nutrient limitation. The main point is that these really just indicate POTENTIAL for limitation. For example, a molar-based N:P ratio of 5 would indicate the potential for N limitation. However, if N concentrations were high (much greater than Ks values) then there would not be much in the way of N limitation at all. So, I'm suggesting a word of caution here. Nothing has been strongly demonstrated with nutrient rations (although I think the author is correct). If they have the ability and resources I'd suggest a bioassay approach as reported by Fisher et al a few years back in Estuaries. That strengthens conclusions. To go another step, large-scale mesocosms can be used as reported by D'Elia et al some years ago, also in Estuaries I think. I'd also recommend the author examine papers by Walter Dodds(or Dodd) who examined this concept in some detail and generated some practical suggestions about the use and abuse of the N:P ratio concept.

Pg.7 There are 22 assessment zones but there are only 14 labeled in Figure 1. Clarify this.

Pg 9. Critical for what? Clarify

Pg 10. I know very little about the use of hyperspectral imagery so I have no comment. But, the tone of this section indicates there is some debate about this approach and the data generated. So, I trust someone who is better equiped than I am to provide some useful comment.

Pg. 11. …"not likely to have changed during a matter of weeks" Are you sure? That has not been my experience. I think days to weeks (as in two weeks) is a safe statement. How many weeks are you really indicating? Be more specific here.

Pgs 11-12. well done…no comment

Pg. 14 Why compute the daily average % saturation when the sondes provide the actual extent of DO variability, including a minimum?

Pg 15. Re-write last 5 sentences in last paragraph on Pg 15…not clear to me what you are doing here.

EXHIBIT 15 (AR M.1)

Pg. 16. Excellent summary of the "weight of evidence" approach. Nice!

Pg. 17 Adams Point not identified in Fig 1. Note also that the seasonal pattern of nutrient concentrations seen here are also observed in many other estuarine systems.

Pg 18. Very good discussion of off-shore TN concentration. A clear and reasonable discussion.

Pg 19. Why not use Box and Whisker plots. They are not difficult to construct and contain a lot more information.

Pg. 20. Nice visual diagram. However, it does indicate that sampling was not as intense as generally suggested. There are lots of sites where < 10 measurements were made during a 9 year period.

Pg. 23. Suggest that Ks values (nutrient concentration when growth rate is half of the max) be added to this graph or at least to the text. There are plenty of Ks values in the literature so a range of values could be presented for NH4, NO3 and PO4.

Pg 25. One issue missing in this report is any indication of inter-annual variability in key variables. For example, what is the concentration difference in NO3 between wet and dry years? How do wet year concentrations compare to the threshold values? Are dry year values much lower or only slightly lower? It seems like there are enough sites sampled frequently enough for an analysis of this issue. And, this wet dry issue does play into TMDLs in general.

Pg. 28. First paragraph. I agree in general. But, P can and does play a role in some estuaries. See for example work by Fisher mentioned earlier for Chesapeake Bay. His findings (and those of others) helped the Bay Program to adopt a duel nutrient strategy. In the northern GoM, very high N additions have apparently induced P-limitation in portions of the Mississippi River plume (see Ammerman's work). Again, author should use the term "potential for nutrient limitation" in this paragraph. Finally, if TN:TP ratios cluster about 16 (like phytoplankton) why do the other ratio techniques discussed earlier indicate that phytoplankton constitute such a small fraction of the POC? Something wrong here I think.

Pg. 30. 1st Para. Adequate water clarity…add also sufficiently long water residence time and modest grazing pressure

3rd para add the p values as well as r2 values

4th para Is there any other line of evidence that indicates phytoplankton are such a small fraction of TN. This seems to me to be a very small percentage. There seemed to be some large diel swings in DO and that would indicate a substantial autotrophic component…very little of this is phytoplankton? Heck, there are phytoplankton blooms!

Pg 31 last para delete word "proves". In this game we "prove" nothing! Pick a different word (strongly suggests….clearly indicates)

Pg 32. Relative to many estuaries these are low concentrations. My eyeball estimate is that an area-weighted system wide average would be about 2.5 ug/l…not much chlorophyll. You might make a stronger point of this because there is not much further reduction reasonably possible. There are estuaries with chlorophyll concentrations >200 ug/l and in those cases huge reductions are possible and warranted. Also, why not box and whisker plots for fig 13. Finally, why are the median chlorophyll values in Fig 16 much higher than the 90th percentile values in Fig. 13? Please get this clarified.

Pg 34 First, nice figure! Is the water residence time also longer (along with proximity to nutrient loads) in the upper tributaries….that's where the problem areas seem to be located? Please make this point if that is the case.

Pg 35. General comment. The figure and table legends are very brief. It would have been helpful to have more detailed legends. For example, in Fig 15, what are the time and space scales included in this regression model set? Its really helpful to have the figure + legend tell a story without having to go back into the text. Of course, you can't put the whole text in each legend but these legends are very brief…too brief in my opinion.

Pg 36. Has any analysis been done on the residuals in the regression model shown in Figure 17. Such an approach has been useful to many other researchers. The residuals themselves might suggest another important variable. This is a very central analysis presented in this figure and explanations for the remaining variability would be useful (water residence time, water clarity, depth, all may play a role). Finally, have these sites really been used for trends…or are they really sentinel or long-term stations?

Pg 37 Might be useful to cite a few more Valiella papers to support this contention…I see there is one.

Pg 38. Why was a margin of safety of 10-20% selected? Why not 5% or 25%. Preventing the loss of SAV and preventing the proliferation of macroalgae is of prime importance. This statement deserves a bit more discussion and justification. Here the issue of wet and dry years and the effect this has on TN loads and concentrations comes into play.

Pg 39. This is a great visual diagram. Several comments: 1) there has been a very large reduction in eelgrass in a single decade. The text does not seem to make this point strongly enough…this system is really changing; 2) can depth contours be shown so it is clearer just where eelgtass can and can not grow; 3) can any indication of SAV density be shown (using shades of red, for example)?

Pg 40. 130 station visits…does this mean 130 sediment samples were collected? Be clear on this.

Pg 41. Dump the "proved" stuff. Use another word.

Pg 42 Both analyses seem reasonable. Why not test B-IBI relationships to DO, SAV or some other variables that make sense. Even if such analyses do not directly relate to N criteria they do show some significant understanding of how the system operates.

Pg 43. Fig 21. Is the very low values (strong departure from the pattern) a hack or is there something else going on. If something else, explain in the legend.

Pg 45 Third paragraph…good discussion. This is observed elsewhere as well.

Pg 45 last paragraph. Not much certainty here. But, good idea. Can more data be brought to this analysis?

Pg 46 last sentence. I agree. Lots of samples help and a big range in conditions certainly helps. They used a within system comparative approach which was very useful and surely helped in seeing these relationship emerge.

EXHIBIT 15 (AR M.1)

Pg 49 Same comment as for Fig 16…examine the residuals. Or, would it be useful to "scale" the x variable as done in the Vollenweider regression analyses (perhaps for water residence time or depth). Stronger relationships certainly give managers and politicos more guts to do what needs to be done.

Pg 50. All relationships are weak…I agree with the text.

Pg 51. Last paragraph…I agree…good point.

Pg 52 last paragraph. SOD is exerted in all sediments, not just in the Lamprey River. A basin prone to stratification probably should be treated as a special circumstance and not representative of the system. There are, for example, deep portions of Chesapeake Bay that will likely remain hypoxic even if (a big if) all proposed nutrient reductions are successful. Figure 31 shows there are some significant DO issues in the tributary rivers.

Pg 55-67. This is a good discussion/analysis of a difficult issue. I liked the approach which came at the problem from several different angles. Is it useful to use the Chesapeake Bay 22% light transmission value in a more northern estuary with far cooler water temperature? Is there guidance from a more similar system (Narragansett Bay?).

EXHIBIT 15 (AR M.1)

Basic Review Questions: I was favorably impressed by this analysis and should appropriate actions be taken to meet these nutrient criteria, good things are likely to happen.

Transparency: I think they did a solid job on this. The methods section seems complete. They walked the reader through the conceptual model, made it clear that this was a weight of evidence approach (versus some other approach), used a variety of methods to reach conclusions and were frank about the lower limits of TN concentrations (i.e., not reasonable to get lower than the inflowing ocean water). I have made a few suggestions for increased clarity

Defensibility: We all know that just about any analysis can be challenged and criticized and this one is no different. However, I find the approach, methods and analyses used to reach conclusions solid. This was an empirical analysis and there is a lot to say for that approach since the values reported were actually measured (repeatedly) in the estuary in question. Analytical methods seemed fine. I caught some defensiveness regarding the hyperspectral work and indicated that someone other than me needs to examine that aspect of this work.

The designated uses were clear to me. I did indicate that I saw no bacterial work and I was a bit surprised at that. I assume the data are there to indicate there are no bacterial issues related to contact uses.

I thought the logic related to numeric criteria development was especially clear. I favor the multiple approaches used in this analysis and I thought the author did a solid job of relating results from one analysis to other analyses and eventually to numeric TN criteria. I had expected to see a good deal of attention paid to nutrient load estimates but there were none. However, it was clear that this is the next step (or one of the next steps) in this process.

Reproducibility: I believe this is true. From what I can see, someone could re-do these analyses and I think they would reach (or could reach) the same conclusions. Because a conceptual model linked to empirical analyses approach was used it is far easier to "re-run" some or all of these analyses or to update the analyses. Programs relying on coupled land-use, circulation and water quality models face a far more complex and expensive and time consuming task in this area. Some would argue that the latter approaches are never reproduced because of these issues.

EXHIBIT 15 (AR M.1)

Protective: The basic answer to this question at this time is "who knows?" In any fundamental way, we can't be sure. But, in a practical fashion, there are strong arguments here that the suggested levels will be protective and, as I read the document, if achieved would favor improved habitat conditions relative to the benthos, eelgrass communities and DO conditions. Furthermore, the author took the point of view that if these criteria are achieved and the system does not fully respond as expected, then additional steps for further reductions in TN concentrations will be taken. He makes the same argument for phosphorus (i.e., if P appears to be a player in all this then P controls in tidal waters will need to be developed).