UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
Region 1
5 Post Office Square, Suite 100
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June 29, 2010

Harry T. Stewart, Director
NHDES Water Division
29 Hazen Drive
PO Box 95
Concord, NH 03302-0095

RE: Transmittal of Independent Peer Review of Nutrient Criteria Proposal for Great Bay Estuary

Dear Mr. Stewart:

In March 2010, the Environmental Protection Agency (EPA) initiated an independent peer review of a nutrient criteria proposal for Great Bay Estuary (Great Bay) developed by the State of New Hampshire Department of Environmental Services (NHDES). The peer review process was administered by the environmental engineering consulting firm Tetra Tech through the Nutrient Scientific Technical Exchange Partnership and Support (N-Steps) program. N-Steps is a partnership among academic, state and federal agencies to provide technical support to state and tribal agencies for the development of nutrient criteria. On June 2, 2010, the peer review process was completed. Attachments A and B to this letter are the final peer reviews from the N-Steps expert reviewers.

EPA considers this a significant step, among many underway, toward addressing cultural eutrophication in Great Bay. Nationally, excess nutrients are a major source of impaired waters which adversely impact both human health and aquatic life. The development and adoption of numeric nutrient criteria is a key step toward restoring and protecting water quality in the United States. The peer reviewed nutrient criteria proposal is among the materials EPA will consider when developing controls to limit the discharge of nutrients into Great Bay.

Among other things, the reviewers found that the Great Bay nutrient criteria proposal is well explained and well supported by appropriate literature and reasoning. They also noted that there is a large amount of water quality data pertaining to Great Bay and it was well used in the report. Finally, the reviewers noted the multiple analyses used in this work provides enhanced confidence in the results, which is a good approach in systems as complicated and variable as estuaries.

The purpose of the peer review was to support the state by providing advice from national experts on how to improve the technical and scientific soundness of the document as a basis for future development of numeric nutrient water quality criteria. It was not intended to finally or comprehensively resolve the many complex issues

concerning the development of nutrient criteria and the implementation of nutrient controls for Great Bay. There will be additional opportunities to submit scientific, technical, legal, and policy comment on all dimensions of the proposed nutrient criteria, and any future nutrient controls based on these criteria, in other regulatory forums (e.g., the State's criteria development/approval process and the National Pollution Discharge Elimination System permit issuance process). EPA expects this engagement with the regulated community and other stakeholders to be productive and to ultimately improve the quality of its decision making. In the interim, EPA appreciates the effort by N-Steps to provide a detailed review of New Hampshire's nutrient criteria proposal for Great Bay and welcomes their recommendations for deriving numeric nutrient criteria with a sound scientific basis.

We commend you and your staff for providing excellent leadership in the area of estuarine nutrient criteria development. Please contact Stephen Silva (617-918-1561) or Ellen Weitzler (617-918-1582) if you have any questions.

Sincerely,

Stephen S. Perkins, Director
Office of Ecosystem Protection

cc: Paul Currier, NHDES
Phil Trowbridge, NHDES

ATTACHMENT A

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

Robert W. Howarth
Cornell University, Ithaca, New York

June 2, 2010

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

Robert W. Howarth
Cornell University, Ithaca, NY 14853

June 2, 2010

The Great Bay nutrient criteria report was a joy to read and provides an excellent basis for protecting this estuarine ecosystem from nutrient pollution. While many states have narrative nutrient criteria, very few have addressed the difficult challenge of establishing numeric criteria. I applaud the State of New Hampshire for providing some excellent leadership in this area.

The reliance on a weight-of-evidence approach, using several approaches and sources of information, is a strong point of the report. Of the approaches analyzed, some worked better than others. For example, the use of the health of the benthic invertebrate community proved problematic, while relating eelgrass habitat suitability to nitrogen through a relationship to water clarity and penetration worked very well. Similarly, the use of continuous oxygen data proved much more useful for setting nitrogen criteria than did the use of spot sampling for oxygen. The Great Bay report did a beautiful job of explaining the rationale behind each of the approaches tested, as well as in explaining the reasons for using some over others in setting numeric nitrogen criteria. I agree with the report's use of low dissolved oxygen and loss of eelgrass habitat as the two most sensitive and appropriate approaches for setting numeric criteria.

Assumptions in the Great Bay report are well explained and generally well supported by appropriate literature and reasoning. The Great Bay estuary is surprisingly rich in data on nutrient concentrations, dissolved oxygen concentrations, chlorophyll levels, and distribution of seagrasses and macro-algae, and these data were well used in this report.

The Great Bay report takes the approach of setting concentration-based criteria for nutrients rather than using a load-based approach. I found this surprising, as much of the effort in many other estuaries and coastal systems (Chesapeake Bay, Long Island Sound, the Northern Gulf of Mexico hypoxic zone) use a load-based approach (although as noted in the report, the State of Massachusetts has developed a concentration-based approach for protecting estuaries). The NRC (2000) Clean Coastal Waters report stressed the use of loading-based approaches, and specifically warned against using approaches based on inorganic nutrient concentrations; we did this because of inorganic nitrogen concentrations are often low in the most nitrogen-impaired coastal ecosystems, due to the high level of uptake by phytoplankton and other primary producers. The NRC (2000) Clean Coastal Waters report did not consider the use of concentration-based criteria based on total nitrogen, in part because we were aware of no locations where such an approach had been developed and tested.

The Great Bay report has convinced me that the concentration-based approach for setting criteria based on total nitrogen can be powerful and protective. Still, I would have liked to have seen some analysis of how a load-based approach might work in the Great Bay ecosystem. Had the load-based approach also been tested, the authors of the Great Bay report may well have demonstrated that the total-nitrogen concentration approach was more powerful and protective (given the demonstrated strength of that approach, as developed in the report). But we cannot be sure without having seen the load-based approach as well. I would caution other states against using the concentration-based approach without also considering load-based approaches.

The criteria approach developed in the Great Bay report lends itself well to adaptive management. That is, the State of New Hampshire can monitor over time both the concentrations of total nitrogen and the identified sensitive response variables (oxygen concentrations, chlorophyll levels, water clarity and light transmission, and seagrass distribution.), and re-assess the protectiveness of the nutrient criteria periodically into the future. I strongly urge the State to develop a strategy to implement such an adaptive management program for the Great Bay estuary.

While the Great Bay report is well written and extremely well argued, I believe the report would benefit from a stronger executive summary. The lead author of the report, Philip Trowbridge, gave an excellent summary of the report in an oral presentation at the biennial meeting of the Coastal & Estuarine Research Federation in Portland, Oregon, last fall. Perhaps he could use the outline of that talk in revising the executive summary of the report.

Specific Comments on the Report:

1.) When below the limit of detection, data were reported as being at the level of detection, and used in averaging, etc. (page 4). This introduces a slight bias towards higher average concentrations estimated for both total nitrogen and dissolved oxygen, and is therefore not the most conservative approach. I suggest reporting these data as a range, using both zero and the limit of detection. I suspect this assumption is unlikely to affect conclusions in any significant manner, though.

2.) The report assumes that phytoplankton biomass is composed of 50% carbon by weight and 6% nitrogen (page 5). This gives a molar C:N ratio of 9.7, which is fairly high. I think using a lower value for carbon might be more reasonable, perhaps 42 to 45%. I would also suggest a higher value for nitrogen, perhaps 7.5%. This would give a molar C:N ratio that is consistent with the Redfield ratio (approximately 6.8 for C:N). Using total particulate matter concentrations of nitrogen to infer the nitrogen content in living phytoplankton (as the report does) is problematic, as much of the particulate matter is non-living detritus, probably derived from terrestrial sources and seagrasses as well as from phytoplankton. The conclusions of the report are undoubtedly very insensitive to these assumptions, however.

3.) Similarly, the report assumes a phosphorus content of 1.3% of the weight of phytoplankton, based on measurements of phosphorus in the total particulate matter in the estuary (page 6). This is not justified, and I would suggest using a value more in line with the Redfield ratio (15:1 by moles, so 1.1% phosphorus by weight if one assumes 7.5% nitrogen by weight).

4.) The report uses the molar N:P ratio both for total nitrogen and phosphorus and for inorganic nitrogen and phosphorus to make inferences about nitrogen vs. phosphorus limitation (pages 6 and 28). For justification, the report cites NRC (2000) and Howarth & Marino (2006). These two sources refer specifically to the N:P ratio of biologically available nitrogen and phosphorus, indicating that the ratio of dissolved inorganic nutrients often reflects this availability. NRC (2000) and Howarth & Marino (2006) did not recommend using the N:P ratio of total nitrogen and phosphorus, in part because coastal ecosystems often have relatively high concentrations of recalcitrant organic nitrogen (compared to organic phosphorus, which is recycled more rapidly). I suggest emphasizing the inorganic N:P ratio in the Great Bay report. See Figure 11 on page 29.

5.) The report assumes that total nitrogen in the Gulf of Maine is not changing much over time (page 18). I believe this is assumption is fine, and the report need not worry overly or be defensive about the increased nitrogen load from land having a major influence on the Gulf of Maine in that regard. In general, the inputs and concentration of total nitrogen on the continental shelf off the northeastern US are dominated by inputs of deep North Atlantic water (Boyer, E. W., and R. W. Howarth. 2008. Nitrogen fluxes from rivers to the coastal oceans. Pages 1565-1587 in D. Capone, D. A. Bronk, M. R. Mulholland & E. J. Carpenter (eds.), Nitrogen in the Marine Environment, 2nd Edition, Elsevier, Oxford.). This would probably be particularly true in the Gulf of Maine.

6.) The relationship between total nitrogen and chlorophyll is very strong (page 30), and provides a robust approach for setting a total nitrogen criteria. The report is correct in arguing that the relationship between inorganic nitrogen and chlorophyll should be less strong, due to the large amount of inorganic nitrogen taken up by primary producers. The relationship is nonetheless strong.

7.) The report makes a convincing case that eelgrass has declined significantly in Great Bay since 1996, with some of the area that formerly supported eelgrass now dominated by nuisance macro-algae (page 37). This is a very disturbing trend, and points to the need to better control loss of eelgrass. The development of a total nitrogen criteria level of 0.34 to 0.38 mg N/l, based on proliferation of nuisance algae in Great Bay, seems justified (page 38). The report correctly points out the need to separately assess nitrogen criteria for eelgrass protection based on water clarity.

8.) The report concludes that benthic invertebrate data are dominated by salinity rather than by nitrogen per se (although nitrogen and salinity are correlated). This is a reasonable interpretation.

9.) The regressions between chlorophyll or nitrogen and low dissolved oxygen concentrations are striking, and as the report states, somewhat surprising for grab samples of oxygen taken at one point in time since dissolved oxygen can change dramatically over the course of a day (pages 45-50). Despite the noise in these relationships, it would be tempting to use them to set a nitrogen criteria level, if the more robust continuous oxygen data from the sonde deployments were not available. I agree with the report's use of these datasonde data to set the standard, which seems robust (pages 51-52).

10.) The section on light transmisivity and eelgrasses is very well done, and the correlation between total nitrogen and turbidity (page 65) is very striking. The nitrogen thresholds presented on page 66 appear justified.

Charge Questions:

In writing this review, I was charged with four specific questions on the transparency, defensibility, and reproducibility of the Great Bay report, as well as an assessment as to whether or not the recommended criteria will be adequately protective. I address each of these briefly below.

Transparency: The Great Bay report does an excellent job of stating their assumptions and explaining their analytical approaches, and the limitations of these approaches. The data behind the report are also available on line. This is among the most transparent assessment reports I have seen, and I applaud the authors for this.

Defensibility: The report uses data from a variety of sampling studies, and uses a weight of evidence approach in the assessment of these data. For the most part, the sampling and analytical methods behind these data seem straightforward and are consistent with commonly used and accepted approaches. Importantly, the report does a nice job of stating how the nutrient data were used (ie, in estimating total nitrogen and specific nitrogen pools). As most of the data come from government monitoring programs, it seems likely that QA/QC processes were used. However, the report does not document this. A brief discussion on QA/QC issues, perhaps with reference to appropriate web sites where more information is available, would be very useful.

The report did an excellent job of stating the designated uses of Great Bay and in explaining how those uses could be protected from the nutrient criteria proposed.

Reproducibility: I did not attempt to independently verify the many analyses that are included in the report. However, for the most part these analyses are straightforward, and appear reasonable and well done. Further, the data behind the analyses are available on line, allowing any one to further test the analyses, including making changes in assumptions and approaches. This is very important, and adds greatly to the credibility of the Great Bay nutrient criteria report.

Protective: The proposed nutrient criteria seem quite protective of the designated uses of the Great Bay estuarine system. The criteria could be made even more protective if they are used in the context of adaptive management. The State of New Hampshire should be encouraged to continue to monitor both total nitrogen concentrations and the response of sensitive indicators (dissolved oxygen, chlorophyll, light penetration, water clarity, and eelgrass and macro-algal distributions). These monitoring data should feed into a periodic re-assessment of the nutrient criteria, and the criteria adjusted downward if necessary to protect designated uses of the Great Bay estuary.

ATTACHMENT B

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

Walter R. Boynton
University of Maryland, Solomons, Maryland

May 29, 2010

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

29 May, 2010

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).

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.

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.

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.

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?).

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.

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).