Compilation of Lake Erie Phosphorus Objectives Review Panel Member Comments on the Panel's Draft (9-1-16) Report

(As of 9-27-16)

The following comments from Panel members are grouped according to each section of the Panel's draft (9-1-16) report. The compilation contains all comments received from Panel members as of 9-27-16.

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General Comments

Merryl Alber -- In general, I think that this report does a good job of summarizing our deliberations, so the only changes indicated are a suggestion for an additional sentence in the Executive Summary and a few very minor edits in the Adaptive Management section (pp. 27-28). However, I did notice that many of the suggestions for further work included in the individual sections were also highlighted in the Adaptive Management, and I'm wondering if we want work to reduce the redundancy and/or have some additional cross-referencing in the report.

Some examples:
Page 12 – there is a paragraph about the timing of nutrient inputs, and another on hypoxia, both of which are discussed in adaptive management.
Page 14 – the section on climate change is echoed in Adaptive management.
Page 15- the call for ongoing lake and tributary monitoring (and in general, there are several calls for increased tributary monitoring)
Page 22- the call for determining total N loads

There are also several sections that discuss the need for coupling watershed input with lake models.

Celia Chen -- In general, the report reads well and covers the broad range of concerns that were discussed at our meeting in Chicago. Perhaps we should check for consistency in the use of "P" vs. "phosphorus" and other such notations.

John Connolly -- The document looks very good and I have only two comments to consider (these are listed below in the comments on the executive summary and response to charge question 6).

Richard Di Giulio -- I've read over the report and found it very clear and well-written and accurately reflecting the panel's deliberations. I have no substantive changes.

Douglas Endicott -- The draft Panel report looks accurate and thorough.

Robert Heath -- I believe that the draft document is a faithful presentation of the discussions we held in Chicago in June. The LEPOR Panel is comprised of individuals with a variety of backgrounds and represents a variety of views regarding the assumptions and approaches proposed in this activity. I believe that the draft document fairly well presents the consensus views as well as the caveats discussed at that meeting. My comments below are intended succinctly to highlight my support for the conclusions stated in the draft report or to reiterate my caveats regarding them.

Emma Rosi-Marshall -- The draft report reflects the discussions that we had in June quite well.

Eric Smith -- I think the report is rather comprehensive and thorough. However, we have done a very good job of coming up with issues and tasks for the EPA team. Do we also need to think in terms of what is best for them to do, given the resources that are expected over the next couple of years? I doubt that they will get a 5 year budget of millions of dollars however this is what I think is needed to carry

out everything that is detailed in the report. Is there a way to prioritize our suggestions? For example, how much will an understanding of the role of nitrogen affect the reduction targets? Will there be lower uncertainty if a whole lake model is used? Would it be better to focus on reducing uncertainty in the model southwest region of the lake rather than expanding the model to the whole lake? I think the report would be stronger if our recommendations were prioritized.

Section 3.1.1. Evaluation of Models to Inform Interpretation of Results (Response to Charge Question 1)

General. (Robert Heath) I continue to believe that the original "ensemble" approach is a good idea because it is a way of utilizing different modeling approaches and models constructed with different assumptions to focus on the same problem. Ensemble modeling has been shown to succeed in providing a basis for management in similar situations (e.g. Baltic Sea). I believe that the main problem is that the means of bringing the models to orchestrate ensemble was insufficiently considered. The selected models were not equally reliable in providing useful predictions in considering various management scenarios. I think the suggestion of using either likelihood-based methods or Bayesian model averaging to produce a combined model-weighted characterization of the loading curve and the associated estimate of uncertainty should be further explored.

Given the limits of funds available for this issue, I agree with the suggestion to reduce the number of models used. However, limiting the number of models to *one* means that we have decided to abandon the ensemble approach. I caution against this choice; more may be lost in breadth of approach than gained in economic efficiency. Whether one model is chosen or the ensemble is reduced to a "string quartet" it should be recognized that most of these models were constructed and validated when the Lake Erie ecosystem behaved differently than it does now. Whichever model or models are eventually chosen, they need to be sufficiently flexible to be able to be re-worked and refined in response to recent advances in understanding the structure and function of the Lake Erie ecosystem, including annual synoptic sampling of the nutrients and biota, as recommended in the report. I strongly agree with the report's suggestion to improve the estimates of loading by linking land use models with loading models to allow scenarios considering various altered land use practices to be evaluated in the context of loading to Lake Erie.

P. 7, line 8. (Hunter Carrick) Revise as follows: "…(3) hypoxia in the hypolimnion **in the Central Basin**;…"

P. 8, lines 8-9. (Maury Valett) *"The standard measures of goodness of model fit are not predictive, and assessments of the quality of the fit may be optimistic for purposes of nutrient management."*

A few words on why 'goodness of fit' does not constitute effective ability to predict seems in order here.

P. 8, line 33. (Hunter Carrick) Revise as follows: "Given the practical limits of funding and **those of the models themselves** ~~the limitations of a number of the included models~~, the SAB recommends reducing the suite of models considered."

P. 9, line 22. (Hunter Carrick) Revise as follows:" It seems worthwhile to improve the estimates of loading by linking land use models with loading models **to achieve a realistic picture of the landscape-level interactions that likely produce in-lake changes (e.g., algal blooms, hypoxia, *Cladophora* growth)**."

P. 9. line 34. (Celia Chen) It might be useful to organize the recommendations by the questions and sections identified above. Also, the recommendations in some cases are the exact wording of the paragraphs in the section above. Perhaps they should be distilled down to fewer words or sentences.

P. 10, lines 18-19. (Maury Valett) "*It would be useful to develop a model of nutrient and TSS loading that includes inputs from smaller tributaries."*

Is there an estimate of the % of discharge that these tribs represent? If so, including it in the report would strengthen this argument.

P. 10, line 32. (Eric Smith) Add text indicating that the EPA should investigate when and where data collection is needed to best inform the models and reduce model and estimation uncertainty.

Section 3.1.2. Phosphorus Loading Targets (Response to Charge Question 2)

General. (Robert Heath) The phosphorus load – response curves were developed from models that have been developed over the past forty years. Using those load-response curves it is rational to choose a 40 percent external load reduction as the target. But that conclusion implicitly assumes that the models continue to represent the Lake Erie ecosystem as it is today. I believe there are compelling reasons to question that assumption, and therefore I question the validity of the conclusions drawn from their use:

- The models were constructed when the phytoplankton were strongly P-limited. The biochemical response to P-limitation is a graded response, not a sudden all-or-nothing response. There is a great deal of recent evidence that since 1995 the phytoplankton communities have been decreasing in their growth response to external P-loading. The models tend to assume that the response of phytoplankton growth to external P-loading would be direct, linear and rapid. This is unlikely to be the case as phytoplankton communities become less sensitive to P-limitation.
- There is a strong, positive relationship between external hydrodynamic loads and the size of phytoplankton blooms. Years when there is considerable rain (March – June), generally have large HABs; conversely, relatively dry years have small HABs. It has been assumed that the critical nutrient load was P, but the same argument could be used for any other nutrient that is concomitantly loaded (e.g. N, Si).
- The models generally do not consider the role of "legacy P" in the sediments and the time lag that diagenesis of this component may forestall the effectiveness of a 40 percent external load reduction.
- Increasingly, reports have appeared that when HABs appear they are N-limited or N-and-P co-limited.

- The models implicitly assume that phytoplankton growth is directly a function of the P available. This may be true when the phytoplankton are strongly P-limited, but becomes less so as P-limitation is lessened. Biochemically, phytoplankton respond more to N:P than to P alone.
- The models were constructed at a time that external P-loading was much larger than it is now. Because external loading is smaller now than it was, it may well be that other processes have become proportionately more important to overall ecosystem responses. Such processes as internal loading and recycling may be underrepresented in the models and certainly have been less studied.
- Regarding the development of hypoxia in the Central Basin, the models present the response of hypoxia in a much more direct fashion than is evidenced in the field. Much more study of the causes of hypoxia is needed. A recent report even showed rather convincingly that hypoxia was driven by the late winter diatom bloom, which is well-known to be Si-limited.

P. 11, line 23. (Maury Valett) *"A secondary effect of excessive algal growth is the rapid deposition of algal-derived, labile detrital organic matter that drives elevated oxygen consumption…"*

'drives elevated oxygen consumption' is awkward. What about 'enhances consumption'?

P. 12, line 21. (Celia Chen) "*The timing of nutrient inputs is also important to cyanobacterial blooms. It would be useful to evaluate whether there has been an increase in sensitivity of blooms to nutrient inputs over time and to recognize how the critical spring period may change or shift in the future."*

The 2nd sentence in this paragraph is not entirely clear to me. It would helpful to have this explained better. For example, how would sensitivity to blooms be determined?

P. 12, line 25. (Celia Chen) "*Uncertainties in predictions of hypoxia are considerably larger. This is due to the fact that the extent and dynamics of hypoxia are confounded by many factors including physical processes, as well as biological processes such as the extent of the winter bloom."*

"larger" than what. Perhaps it would be clearer to say, "larger than predictions of cyanobacterial blooms."

P. 12, line 25. (Hunter Carrick) Revise as follows: "Uncertainties in predictions of hypoxia are considerably larger **compared with those for cyanobacteria blooms**."

P. 12, line 42. (Celia Chen) "*One current weakness of the hypoxia simulation models derives from the fact that the process models, while run for multiple years, have only been run as one year simulations using the same initial boundary or starting conditions in each case."*

This paragraph seems really important to me but it doesn't really get featured in the Executive Summary.

P. 12, line 46. (Hunter Carrick) Revise as follows: "This accumulating residual **which** would affect the response time of the system to a reduction in loading **is present in both the western and central basins of Lake Erie (see Carrick et al. 2005)**."

P. 13, line 9. (Celia Chen) "*Given the uncertainties in the hypoxia simulations the SAB finds that 2 mg/L is a reasonable initial target. It should be noted, however, that the water quality standard of 5 mg/L will, almost certainly, not be met in Central Basin bottom waters even with a 40% reduction of P loading, and the predicted hypoxic area ranges from approximately 2,000 to nearly 6,000 km^2 for periods in excess of a month."*

This sentence doesn't make sense to me. Is this intended to mean that the predicted hypoxic range will not be met?

P. 13, line 36. (Celia Chen) It is not clear why the role of Drieisinids is mentioned here as a missing component of the models. Is their role in some way being related to cyanobacteria blooms and hypoxia? If not, then this should be left to discussion in the *Cladophora* section.

P. 14, line 4. (Celia Chen) *"Consideration should also be given to embedding a Cladophora model within the whole lake WLEEM model (the SAB's findings and recommendations concerning Cladophora growth are discussed in the response to Charge Question 3)."*

Again, this mention of embedding the Cladophora model doesn't really belong in this section unless it is somehow being related to blooms and hypoxia.

P. 14, line 20. (Eric Smith) "*Projections of climate change scenarios include Increases in the frequency of intense precipitation events (important in a system that is event driven and where perhaps 70% or more of the loading from the watershed occurs in 10-15 days)."*

In addition to frequency of intense precipitation events, should we add timing of events since high precipitation after fertilizer application is likely to be important?

P. 14, lines 20-21. (Maury Valett) *"Projected climate change scenarios include: Increases in the frequency of intense precipitation events (important in a system that is event driven and where perhaps 70% or more of the loading from the watershed occurs in 10-15 days)"*

This sort of hydrologic setting is often associated with deserts or other places with extensive impervious surfaces. Is that what is being reflected here? If so, it would help to clarify why 70% of loading occurs in 10-15 day. Are these contiguous days? or days of high flow that are temporally disjunct?

P. 14, line 40. (Lucinda Johnson) "*To the contrary, reductions in P loading could shift algal speciation in favor of more palatable species and may in fact enhance the food web by restoring a trophic pathway to secondary and tertiary production."*

Can we add a citation here?

P. 14, line 40. (Eric Smith) Is there a reference to indicate that reductions in P may enhance fish productivity? I am aware of this paper however the levels of P are rather when there is a significant drop (40 micgms/L)

Yurk, Jeffrey J., and John J. Ney. "Phosphorus-fish community biomass relationships in southern Appalachian reservoirs: can lakes be too clean for fish?" *Lake and Reservoir Management* 5.2 (1989): 83-90.

This article summarizes changes in recovery of fish diversity in Lake Erie following P reductions Ludsin, S. A., Kershner, M. W., Blocksom, K. A., Knight, R. L., & Stein, R. A. (2001). Life after death in Lake Erie: nutrient controls drive fish species richness, rehabilitation. *Ecological Applications*, *11*(3), 731-746.

http://www.cleveland.com/outdoors/index.ssf/2012/09/high_phosphorous_levels_are_ch.html

P 14, line 44. (Lucinda Johnson) "*Alterations in fish habitat also have an effect on fish abundance, but this effect is not well understood."*

Indicate that this is a consequence of mussel abundance.

P. 15, line 3. (Douglas McLaughlin) *"In general, the SAB finds that, based upon the coupling of a state-of-the-art suite of models to a relatively long term observational record, a conservative estimate of a 40% reduction in TP load, at a minimum, projects a response which improves water quality and reduces HABs in keeping with the stated goals in the Task Team report (Great Lakes Water Quality Agreement Annex 4 Objectives and Targets Task Team 2015)."*

I think this paragraph expresses more confidence than is warranted, and needs to be reworded. Section 3.1.1 of this report does not support the idea that this is a "state-of-the-art suite of models", e.g., "While adequate in concept, the criteria used for the model evaluations were only loosely applied and models were accepted despite deficiencies relative to the criteria." (top of page 8). Also, from the middle of page 8: "While it is clear that meeting the loading targets would lead to improved values of the selected ecosystem response indicators, other important outcomes are less clear. These include the likelihood that the desired threshold levels would be achieved; how long it would take for improvements to occur after the loading is reduced; and the effect of variations in hydrometeorological forcing and timing of loading on responses to load reduction."

P. 15, Line 3. (Val Klump) *In general, the SAB finds that, based upon the coupling of a state-of-the-art suite of models to a relatively long term observational record, a conservative estimate of a 40% reduction in TP load***,** *at a minimum, projects a response which improves water quality and reduces HABs in keeping with the stated goals in the Task Team report (Great Lakes Water Quality Agreement Annex 4 Objectives and Targets Task Team 2015).*

Insert "in the Western and Central Basins" after "TP load"

P. 15. Line 3. (James Fitzpatrick) *"In general, the SAB finds that, based upon the coupling of a state-of-the-art suite of models to a relatively long term observational record, a conservative estimate of a 40% reduction in TP load**,** at a minimum, projects a response which improves water quality and reduces HABs in keeping with the stated goals in the Task Team report (Great Lakes Water Quality Agreement Annex 4 Objectives and Targets Task Team 2015)."*

I for one would recommend that the word "lake-wide" (TP load reduction) be used

P. 15, line 5. (Douglas McLaughlin) *"…a 40% reduction in TP load <u>at a minimum,</u> projects a response…"*

Based on the discussion of limitations to the modeling that has been completed to date, discussed extensively in this report, I can't defend this statement. I recommend deleting it.

P. 15, line 8. (Douglas McLaughlin) Revise the sentence as follows: "*However even with this reduction, blooms ~~will~~ **may** still occur relatively frequently~~, perhaps even routinely,~~ in the western arm of the western basin In Maumee Bay."*

P. 15, line 11. (Douglas McLaughlin) "*The recommendation that the 40% reduction be applied "across the board" also makes sense in both practical and equitable terms."*

I recommend deleting this sentence as I am not in a position to comment on the practicality or equitability of policies for achieving load reductions. I don't think this committee was charged with weighing in on this topic.

P. 15, line 18. (Maury Valett) *"(good in situ NO_3 sensors are available for high temporal resolution sampling)"*

Nitrate is represented here as NO_3. It is first used in abbreviated form on page #11 (without association with the word itself). Is this define elsewhere? Nitrate is first used on page #3 with association with an abbreviation. Moreover, use of these versions (i.e., NO_3, NO_2) presents opportunity for confusion with uncharged NOx gases (e.g., nitrogen dioxide NO2). Make definitions clear, but use of the charged expression (NO_3^-) is inarguably clearer. If the abbreviations provided on page #8 are to provide the requisite clarification, then the in-line distinctions for a broad range of abbreviations are superfluous.

P. 15, line 24, (James Fitzpatrick) "*The inclusion of these landscape models is an inescapable necessity since it is actions and practices on the land that will enable a 40% load reduction."*

Should indicate lake-wide load reduction"

P. 15, line 24. (Val Klump) Should indicate load reduction to the Western and Central Basins.

P. 15, line 26. (Maury Valett) *"In this regard, is also important to characterize the effectiveness of best management practices (BMPs) with respect to spatial distribution, type of BMP and life cycle effectiveness. "*

Reference here to the 'life cycle effectiveness' suggests some knowledge of BMPs and their lexicon (which I evidentially don't have). What is this? It comes up later as well.

P. 15, line 32. (Douglas McLaughlin) A conservative estimate of a 40% reduction in TP load, *at a minimum*…"

Per comment above cannot defend statement.

P. 15, line 32. (James Fitzpatrick) *"A conservative estimate of a 40% reduction in TP load, at a minimum, projects a response which improves water quality and reduces HABs."*

Should indicate lake-wide load reduction.

P. 15, line 32. (Val Klump) Should indicate load reduction to the Western and Central Basins

P. 15, lines 26-28. (Maury Valett) *"Lake and tributary monitoring is critical for continued development of the models and for adaptive management. Lags in indicator response and inter-annual trends can only be elucidated accurately with an adequate monitoring program in place. In particular, monitoring of the 11 priority tributaries identified by the Annex 4 Objectives and Targets Task Team is essential and should include measurement of: flow, nitrogen species (good in situ NO_3 sensors are available for high temporal resolution sampling), phosphorus (all forms) and organic carbon (dissolved organic carbon, DOC, and particulate forms). Event based sampling (to capture the effects of the rising and falling limb) within these systems is also critical for calculating loads."*

While I recognize that this is a 'key recommendation', a great deal of this text is a verbatim replicate of the text provided two paragraphs earlier (lines 14-21), including a redundant(?) definition of dissolved organic carbon (DOC).

P. 16, line 1. (James Fitzpatrick) *"Mechanistic models should be extended to include sediment diagenesis and nutrient flux. The depths of the active layer should be refined (e.g., 10 cm is too large - the depth may be 5 cm or less)."*

Recommend adding "This will require a calibration of these mechanistic models to field and laboratory data specific to Lake Erie."

P. 16, line 7. (Celia Chen) *"Consideration should be given to embedding a Cladophora model within the whole lake WLEEM model."*

Again, I don't think that *Cladophora* belongs in this section under question 2.

P. 16, line 22. (Maury Valett) "*The effectiveness of BMPs should be characterized with respect to spatial distribution, type of BMP and life cycle effectiveness. This is a large effort, but it is needed if action plans and adaptive management are to be effectively implemented.*"

The comment regarding the size of the effort required for BMP assessment should occur in the body of this section (i.e., in association with lines 26-28 on page # 15), and perhaps not here in the 'Key Recommendations' section.

Section 3.2.1. Development of Recommendations to Address Nuisance Levels of *Cladophora* Growth (Response to Charge Question 3)

P. 16, line 41. (Hunter Carrick) Revise as follows: "The SAB finds that **at this point in time, there are not** scientifically sound phosphorus load reduction recommendations **that could likely** ~~to~~ reduce *Cladophora* growth in the Eastern Basin of Lake Erie ~~cannot be developed at this time~~."

P. 16, line 46. (Hunter Carrick) Revise as follows: "That said, the issue of nuisance *Cladophora* growth in nearshore regions has been identified as a universal issue in the Great Lakes because it affects selected sites in each of the Great Lakes (Auer et al. 2010), **making it a pressing regional issue in need of scientific and management attention."**

P. 17, line 39. (Hunter Carrick) Revise as follows: "*Cladophora* often plays a key role as an "ecosystem engineer"**- an organism that alters the environment in a way that affects the other organisms present; this can have** ~~with~~ both important positive influences as well as potentially negative consequences
.

P. 17, line 42. (Hunter Carrick) Revise as follows: "While it may not be fed upon directly by invertebrates and fish, as substrate it provides food for upper trophic levels indirectly **by providing suitable habitat for the growth of other algae that are eaten directly."**

P. 18, line 7. (Lucinda Johnson) "*Although there is no stated limit of acceptable standing crop, it is generally considered that 30 g dry weight /m^2 is indicative of "good" conditions.*"

This is a bit ambiguous… so more than 30 g dry wt represents a degraded condition? I assume this is a benchmark; is it for the Great Lakes or is this a universal benchmark? Would be helpful to have a citation.

P. 18, line 44. (Celia Chen) "*Cladophora growth may be linked to SRP content in the overlying water column. The presence of SRP is linked to the swift turnover of TP levels in the open lake (as modeled in the GLCM) but also to local inputs from nearby tributaries, as well as the presence of dreissenid*

mussels (Higgins 2004). A scientifically-sound model must incorporate site-specific factors, including local hydrodynamics. Current and future studies should include investigation of P load inputs from key tributaries (e.g., the Grand River, Ontario) and the relative significance of local inputs and open Lake P on stimulating and supporting Cladophora growth. ***"***

This bullet talks a lot about SRP but in the end, there is no specific recommendation related to SRP, just P. Seems like something is missing.

P. 18, line 44. (Hunter Carrick) Revise as follows: "*Cladophora* growth may be linked to SRP content in the overlying water column **and ultimately with the P-quota, or P content inside *Cladophora* cells**."

This is an important distinction to make, because P-quota is the most predictive indicator of Cladophora biomass.

P. 19, line 1. (Lucinda Johnson) "*A scientifically-sound model must incorporate site-specific factors, including local hydrodynamics."*

Wouldn't substrate type and dreissenid mussel density also be important drivers for a site-specific process model?

P. 19, line 6. (Lucinda Johnson) *"However, the model would be more useful if it could be applied to the diversity of benthic algae that are important in the Great Lakes, thereby extending the usefulness of the model to other nuisance benthic algae (e.g., Chara, Lyngbya, Spirogyra, etc.) that can cause similar problems. The similarities and differences among these various species need to be considered in order to provide an adequate representation of the problems of nuisance benthic algae in general."*

Wouldn't these also be important drivers for a site-specific process model?

Section 3.3.1. Determining Whether Nitrogen Control is Warranted (Response to Charge Question 4)

General. (Robert Heath) The response presents a well-reasoned case for expansion of complex dynamic models (e.g. WLEEM) to include N-dynamics. This is not a trivial task and needs to be considered carefully. As stated, the N- and P-cycles are coupled in being required for primary production and are largely uncoupled in their abiotic processes. The P-cycle is relatively easy to model because it deals only with oxidation state (V). The N-cycle is much more complex with many redox processes involved. I should emphasize that consideration of N is especially important when attempting to predict the occurrence of the toxin microcystin (MC). Current studies indicate that MC production is enhanced by elevated nitrate concentrations. Accordingly, it should be a priority to model those processes that directly affect nitrate concentrations (denitrification, DNRA, anammox).

P. 19, line 34. (James Ammerman) Despite the dual nutrient strategy and increasing evidence of the possible need for N control, such control is premature. However, the additional research that is described to determine the importance of N is definitely a high priority. This is still an area of disagreement in the scientific community. The Baltic is mentioned several times as a possible model which uses both N and P control, and though it has some similarities with Lake Erie, most of it is estuarine but of low salinity. I note that David Schindler and other distinguished limnologists have very recently published a new paper arguing for only P control in lakes to curb eutrophication, though theirs may be a minority opinion. Reference below:
Schindler, D.W., S.R. Carpenter, S.C. Chapra, R.E. Hecky, and D.M. Orihel. 2016. Reducing Phosphorus to Curb Lake Eutrophication is a Success. *Environ Sci Technol* 50: 8923-8929.

P. 19, line 42. (Lucinda Johnson) *"While the focus in Lake Erie has been phosphorus reduction because it is considered the limiting nutrient, there are good reasons to include nitrogen as part of a dual (N + P) nutrient strategy."*

Can it be characterized as a nutrient reduction strategy, or just nutrient strategy?

P. 19, lines 42-45. (Hunter Carrick) Revise as follows: "While the focus in Lake Erie has been phosphorus reduction because it is considered the primary limiting nutrient, there are good reasons to include nitrogen as part of a dual (N + P) nutrient strategy. Phytoplankton species composition and seasonal succession can vary with both N and P concentrations and ratios, and **thus, phytoplankton biomass does** ~~some may~~ experience co-limitation of N and P **during late summer and early fall (Moon and Carrick 2007).**"

P. 20, line 5. (James Ammerman) Dissimilatory Nitrate Reduction to Ammonium (DNRA) <u>is not a nitrogen (N) removal process</u> like the other ones listed, just a transformation process, the N still remains as ammonium unless otherwise removed.

P. 20, line 5. (Maury Valett) I have some difficulty with listing dissimilatory nitrate reduction to ammonium (DNRA) as a mechanism that can 'internally remove' N. It just isn't true. It is a transformation that generates ammonium, which is a reactive N form. When linked to other fates (like eventual volatilization), DNRA may get you there. But that would mean we would need to list all possible transformations that may lead to eventual removal....not really the point. This same argument for DNRA comes up under BMPs on page #22, line #10-14. One might argue that the same is true for 'burial'....the N is still in the system. Precedent exists, however, for justification based on Alexander et al. (2000) and there use of sediment burial as a means of 'removing' N from biologically-available pools.

P. 20, line 8. (Hunter Carrick) "*Nitrogen can also be biologically fixed. In contrast, P is only advected in or out of the system or buried.*"

I find this statement confusing, particularly for a mixed crowd of end users of this material. I would omit it for simplicity sake.

P. 20, line 17. (Hunter Carrick) Revise as follows: "There is increasing support for adopting a multiple nutrient strategy to reduce eutrophication, in both fresh and salt waters **given the synergistic effects both nutrients have on algal growth** (Conley et al. 2009; U.S. EPA 2015)."

P. 20, lines 25-29. (Hunter Carrick) Revise as follows: "In addition, it becomes more toxic **when** ~~under~~ nitrogen **is abundant in lake water** ~~replete conditions~~ (Harke et al. 2016). Furthermore, *Microcystis* is **very well adapted to obtaining P when levels are low in lake water** ~~good at acavenging low levels of phosphate~~, **because it** can use **enzymes (e.g.**, alkaline phosphatase**)** to **remove P from organic compounds (hydrolyze phosphate esters) that are more readily abundant in lake water compared with more simple dissolved forms of P** ~~hydrolize phosphate esters and other readily utilizable P compounds and thrives offshore in lower phosphate environments~~ (Harke et al. 2016)."

P. 21, line 5. (Hunter Carrick) Insert the following sentence: "Furthermore, the proliferation of nuisance benthic algae (e.g., *Cladophora* and closely related species) has been linked to available N, as well as, P enrichment in the Great Lakes both in an experimentally (see Carrick and Lowe 1988, 2007) and empirically (see Stevenson and Stoermer 1982)."

We should probably make the point here that N and P promote the growth of nuisance proliferation of benthic green algal growth in the Great Lakes as well.

P. 21, line 5. (Hunter Carrick) Revise as follows: "~~Limitation~~ **Low availability** of N **in lake water is associated with** ~~can cause~~ a switch **between species of cyanobacteria, namely**, from **the presence of** *Microcystis* to **that of** *Anabaena*.

P. 21, line 12. (Maury Valett) Same issue with abbreviations (here for NO_2 and NH_4).

P, 21, line 33. (Maury Valett) BMP is defined here but used earlier on pages 15 (line 28) & 16 (line 22), and defined earlier (page #20, line 11).

P. 21, line 36. (James Ammerman) A more recent paper suggests that about half of the total N and P in the Mississippi Basin are from agriculture. For N about 80% of the agricultural contribution is fertilizer and 20% manure. For P about 55% of the agricultural contribution is fertilizer and 45% manure. Reference is below:
Robertson, D.M., and D.A. Saad. 2013. SPARROW Models Used to Understand Nutrient Sources in the Mississippi/Atchafalaya River Basin. *J Environ Qual* 42: 1422-1440.

P. 22, line 11. (James Ammerman) Dissimilatory Nitrate Reduction to Ammonium (DNRA) <u>is not a nitrogen (N) removal process</u> like the other ones listed, just a transformation process, the N still remains as ammonium unless otherwise removed.

P. 22, line 11. (Maury Valett) See my earlier comment regarding DNRA.

Section 3.4.1. Assessing Progress in Reducing Tributary Loadings of Phosphorus (Response to Charge Question 5)

General. (Robert Heath) As I see it, the question is how best to estimate the external P-loading to Lake Erie in such a way as to be meaningful to the growth of phytoplankton? It is recognized that P concentrations and total amounts differ with different hydrological regimes (e.g. high-volume storm events, baseline non-storm stream flow, etc.). Generally, low concentrations and low loadings are associated with low flow rates, but the ecosystem is driven by storm events where total amounts may be greatly increased and concentrations are generally increased at the beginning of a storm event and become diluted later. Under such conditions a time-weighted average underestimates the overall load, while flow-adjusted averages (such as FWMC) provide a more accurate estimate of the total load but can underestimate of concentrations. Phytoplankton and bacterioplankton generally respond to concentrations of nutrients in their immediate environment; they neither sense nor respond to nutrient loads. I agree with the recommendation that both FWMC and flow-adjusted concentrations should be monitored. I also strongly believe that other nutrients (especially nitrate and ammonium) should likewise be monitored.

P. 23. (Maury Valett) This section clarifies the need for flow adjustment for concentrations. I think it could/should be re-ordered to define the two terms up front, then proceed to the implications of their use. Definitions are provided for FWMC (page #23, line #40-41) only after a page of discussion of its relevance. Flow adjusted concentration if first presented on page #4 (lines 18 & 23, without use of abbreviation) and again on line #26 on page #4 as a hyphenated expression (i.e., flow-weighted). It is used a number of time (page #23, lines 19 & 21), but isn't really defined until page #24. Earlier definition would be hugely helpful.

P. 23, line 27. (James Fitzpatrick) *"Nutrient concentrations (not loadings) control organism responses and the effect of temporal variability is an important consideration."*

Yes, concentration controls organism response, but loading determines concentration. $C_{lake} \approx Q_{in}C_{in}/V$

P. 25, line 45. (Celia Chen) "*Detailed information on the implementation of phosphorus reduction strategies in each major watershed should be collected into the future. Without this information, it will not be possible to adequately identify the primary reasons for the observed changes (or lack thereof) in phosphorus loads delivered to Lake Erie."*

Again, it seems that information on precipitation and flow should be collected as well.

Section 3.4.2. Adaptive Management Program (Response to Charge Question 6)

General. (Robert Heath) That external P-loading is the sole driver or at least the overwhelmingly major driver of phytoplankton growth response has been the general assumption under which this exercise has been designed. I find this assumption to be simplistic because of the increasing evidence to

the contrary, as I've noted above. That reduction of external P-loading will bring a concomitant and rapid decrease in the size of HABs is a testable hypothesis, and nothing more. There are many reasons to suspect that solely a reduction in external P-loading will not be immediately successful in reducing the size of HABs, if ever.

- For this reason I strongly support the adaptive management approach proposed in the draft report. I believe that having a standing "adaptive management committee" is an excellent idea.
- It is imperative that a long-term monitoring program be adopted to assess the relationship between the best possible estimates of external P-loading and the stated ERIs.
- It is most important to determine the nutritional status of the phytoplankton blooms (e.g., P-limited, N-limited, N-and-P co-limited, etc.) as part of this monitoring program.
- Research needs to be supported to determine the role of N in supporting HABs and in determining the appearance of MC.
- Research regarding the effects of "legacy P," internal loading via diagenesis, the role of dreissenid mussels, and nutrient recycling needs to be supported and included in the model(s) chosen as necessary.
- Alternative hypotheses need to be developed and tested regarding the development of phytoplankton growth, extent of hypoxia development in the Central Basin, and extent and size of *Cladophora* beds. I believe the hypotheses presented in the draft report are a reasonable beginning toward this goal.
- The selected model(s) need to be reassessed and revised in light of findings from the monitoring and research programs
- Lake models need to be coupled with land use models to assess the usefulness of various BMPs in reducing nutrient loading to Lake Erie.
- The current models are essentially "equilibrium" models, predicting eventual outcomes of external nutrient loadings. They need to be refined to be "kinetic" models, predictive of the rate at which ERIs will be achieved.

P. 26, line 8. (John Connolly) The hypotheses listed in the Adaptive Management section include some that seem to not be appropriate because the state of the science is beyond the point of them being hypotheses that require testing. The two I particularly noted are:

1. Stratification (timing and magnitude) affects hypolimnion thickness, and is an important driver of DO response in the Lake.
   - This is well accepted and the issue is how the adequacy of the models to predict stratification, particularly as climate changes alters atmospheric forcing

2. Accumulated SOD (legacy C) is an important contributor to hypoxia.
   - This is well accepted and the reason for diagenesis models that have been around for the last 20-30 years and the reason why the report recommends that such models be part of the overall framework applied here.

P. 26, line 30. (Lucinda Johnson) The sentence should be revised as follows: "*A coordinated, **binational**, long-term monitoring strategy should be developed. It is critical to provide support for long-term monitoring in order to assess whether loading and ERI targets are being met.~~ In developing the monitoring program~~, Consideration should be given to **the following issues**:"*

This implies that ongoing monitoring is not already in place. Suggest changing to: Stabilize and enhance current monitoring programs. e.g., incorporating the Heidleberg University tributary monitoring program and expanding it to other major tributaries;

P. 26, line 30. (James Ammerman) Monitoring is mentioned throughout but <u>the importance of a coordinated long-term monitoring program cannot be overstated</u>. In discussions with Jeff Reutter, it is clear that the current Western Basin monitoring program is fragmented among different groups with different sampling schemes, this needs to be corrected.

P. 26, line 43. (Lucinda Johnson) Insert the following additional bullet: "Incorporating measurements that provide "early warning" for climate change impacts."

P. 26, line 43. (James Fitzpatrick) Insert the following new bullet: "Field and laboratory studies be conducted to gather data to support the calibration of mechanistic models of sediment nutrient diagenesis, nutrient flux and sediment oxygen demand."

P. 27, line 7. (James Fitzpatrick) Insert "Models be expanded to include mechanistic processes to represent sediment nutrient diagenesis and fluxes of inorganic nutrients and sediment oxygen demand;" between these two bullets

P. 27, line 20. (Lucinda Johnson) *"The adaptive management options in Lake Erie will generally fall into the former category, although it may be instructive to use differences between tributaries or among the five Great Lakes when evaluating potential hypotheses."*

Since Lake Erie is so different from the other lakes, this may not be a viable strategy.

P. 27. (Eric Smith) I think it might generalize results to use evaluate instead of tests as we might be interested in prediction rather than testing.

P. 27-29. (Maury Valett) A long-standing argument, made recently again by Hutto (*Hutto, R.L. 2012. Distorting the process of scientific inquiry. Bioscience 62:707-708.)* is that most of what scientists present as hypotheses are not, in fact, hypotheses. Statements suggesting relationships among variables that lack explanatory content are really predictions. Hypotheses address plausible explanations and are oriented towards causality. Most of the 'hypotheses' presented on pages 27-29 are really expected relationships that are derived as a consequence of another part of the logic not presented on the page. While I do not propose to teach others how to write hypotheses, I do think that it behooves the SAB to be leaders in this regard. This section provides 10 hypotheses and I have rewritten 8 of them to include explanatory relationships. One provides explanatory power (Dreissenid mussels hypothesis, page # 29,

line 1). Another, predicating a relationship between microcystin abundance and algal biomass (page #28, line 11) requires more knowledge than I currently have on the issue. Others rewritten to illustrate my point (not as suggested final forms) are provided below:

1) Loading hypotheses (combine the two hypotheses provided on page #27, lines 35 & 41):
Development of BMPs focusing on P will be differently effective towards N and P because protocols for P retention emphasize regulation of particulate forms while N species are heavily influenced by transport and behavior of dissolved phases.

2) Co-limiting hypothesis
On page #28, line 2, the text reads that 'N is co-limiting'. Nothing can be co-limiting by itself. Clearly, the rest of the idea has to do with N and its role in co-limiting something?
Timing and magnitude of cyanobacterial blooms reflect the stoichiometric balance of N and P in resource supply because growth and nutrient demand can generate conditions where N and P become co-limiting.

3) Diatom hypoxia hypothesis
Basin hypoxia during stratified conditions may reflect antecedent biological conditions because the duration and magnitude of spring plankton blooms dictate the potential for oxygen depletion.

4) Physical stratification hypothesis
Seasonal progression of lake physical structure is an important determinant of susceptibility to basin hypoxia because timing and magnitude of the development of thermal stratification affects hypolimnion thickness and the propensity for oxygen loss.

5) Legacy C hypothesis
In addition to acute influences of current lake productivity, history of biological behavior contributes to the propensity for basin hypoxia because accumulated organic matter (i.e., legacy carbon) supports sediment oxygen demand that contributes to oxygen loss.

6) Internal P hypothesis
Cladophora standing crop and productivity may be uncoupled from allochthonous nutrient supplies because internal P release from hypoxic sediments serves as an important source of P.

7) Littoral nutrients hypothesis
Shoreline maxima for *Cladophora* biomass and growth reflect alleviation of nutrient limitation because near-shore environments are important sources of P.

P. 29, line 22. (Lucinda Johnson) Revise as follows: *"Climate change; increased precipitation and discharge; increased temperature;* ***and shorter duration of ice cover."***

P. 29, line 39. (Lucinda Johnson) Revise as follows: **Which** ~~Are the~~ environmental and land use conditions changing or likely to change in the future?

P. 30, line 10. (Lucinda Johnson) Revise as follows: A coordinated long-term monitoring ~~program~~ **strategy** should be developed.

Executive Summary

P. 1, line 33. (Celia Chen) In the section entitled "Evaluation of the Models to Inform Interpretation of Results" of the Executive Summary, there is no mention of the issue of running the models over longer time periods in order to address the issue of legacy effects of nutrient loading. It seems to me that that is an important drawback of the current model runs and needs to be addressed going forward.

P. 2, line 9. (Hunter Carrick) Revise as follows: "…changes and **relevant ecological processes such as the** internal storage…"

P. 2, line 30. (Hunter Carrick) "*However, even with this reduction, blooms **may** ~~will~~ still occur **with some** ~~relatively~~ frequen**cy**~~tly~~, perhaps even routinely, in the western arm of the western basin in Maumee Bay."*

Not sure I agree with this statement. If you are referring to blooms in Sandusky Bay, then yes, I think the statement is accurate.

P. 2, line 33. (Hunter Carrick) Revise as follows: "Attenuation of hypoxia has a higher degree of uncertainty **and its link with algal blooms in the western basin is not well understood**."

P. 2, line 37. (Maury Valett) *"Additional information will be needed in order to include missing components in the models. These components include: temporal variability in hydrodynamics; the role of nitrogen limitation; controls of algal toxin production; internal phosphorus loading (i.e., resuspension fluxes and sediment-water interactions); the role of dreissenid mussels; seasonality in the timing of nutrient loads; winter-spring diatom blooms under ice; and the effects of climate change."*

A rather vague call for 'additional information' is provided here. Does 'Additional information' mean model expansion or primary research? Clarity here may be helpful later. In the same line, the idea that we need to 'include missing components' is poorly stated....because they wouldn't be missing if we included them. Perhaps change 'include missing' to 'incorporate currently missing'?

P. 2, line 38. (Hunter Carrick) Revise as follows: "These components include: temporal variability in hydrodynamics; **factors affecting the P uptake of algae**; the role of nitrogen in mediating algal growth ~~limitation~~;

P. 3. Line 4. (Eric Smith) I think it is important to recognize that the approach is dependent on the model and the model is dependent on data that is available to estimate relevant quantities. I think we should comment that there needs to be a link between the data needs of the model and the data collection

process. The EPA should evaluate what data are needed to reduce the uncertainty in the model(s) and to better predict algal growth and presence.

P. 3, line 7. (Hunter Carrick) Revise as follows: "The SAB was asked to comment on whether scientifically sound phosphorus-load reduction recommendations could be developed to reduce **nuisance** *Cladophora* growth…"

P. 3, line 8. (Hunter Carrick) Revise as follows: "The nuisance attribute of *Cladophora* is **that** ~~large~~ **expansive growths of this alga can cover large nearshore regions, such that when it dies off it often time washes onto nearby shorelines causing** the formation of "beach muck"…"

P. 3, line 9. (James Fitzpatrick) "*The SAB finds that recommendations to reduce the phosphorus loadings to reduce Cladophora growth cannot be developed at this time."*

Cannot make specific comments about target P load reductions, but what about recommendation or statement supporting P load reductions to reduce Cladorphora biomass levels?

P. 3, line 13. (Hunter Carrick) Revise as follows: "Moreover, there are limited observations of the spatial extent of the *Cladophora* problem along the shore of the Eastern Basin of Lake Erie, **although of the reports that do exist, they do point to existence of a developing basin-wide problem of significant magnitude to warrant immediate action."**

P. 3, line 28. (Richard Di Giulio) My one suggestion concerns the issue of controlling N. For example, in the summary concerning this (pp. 3-4), it would seem an appropriate question to ask is: How effective will management approaches for controlling P be for simultaneously controlling N? This is alluded to a bit in the fuller discussion (pp. 19-22), but perhaps merits an explicit phrase in the summary.

P. 3, line 34. (Eric Smith) "*The toxic cyanobacterium, Microcystis, the major concern in western Lake Erie, does not fix nitrogen. Therefore it requires a fixed nitrogen source. Moreover, Microcystis can become nitrogen-limited in late summer in western Lake Erie and it becomes more toxic under nitrogen replete conditions."*

While it is relevant to point out the importance of N, I think it might be useful for EPA to determine the reduction in N that results from the control of phosphorus. If there is to be a 40% reduction in phosphorus there certainly will be some reduction in N and perhaps this will be adequate. If the control systems are such that there might be a 40% reduction in N but 20% in phosphorus then that might be a different approach however I suspect that the uncertainty here is rather high so the effect of any implementation will have to be measured, likely through adaptive management studies.

P. 3, line 37. (Hunter Carrick) Add the following sentence. "Furthermore, the growth of nuisance benthic algae in the lake is fueled by loading of both nitrogen and phosphorus. "

P. 3, line 43. (James Ammerman) Dissimilatory Nitrate Reduction to Ammonium (DNRA) is not a nitrogen (N) removal process like the other ones listed, just a transformation process, the N still remains as ammonium unless otherwise removed.

P. 4, line 10 – 24. (Maury Valett) My comments regarding this paragraph are provided below in reference to Section 3.4.1 (pages 23-25)

P. 4, line 19. (James Fitzpatrick) *"Nutrient concentration, not loading, controls organism responses, and the effect of temporal variability in nutrient concentration is an important consideration in the management of harmful algae blooms, particularly for organisms that have rapid life cycles and a rapid response to shifts in nutrient concentrations."*

Yes, concentration controls organism response, but loading determines concentration. $C_{lake} \approx Q_{in}C_{in}/V$

P. 4, line 26. (Celia Chen) *"The SAB recommends that the uncertainty in values derived using flow-weighted or flow-adjusted assessment approaches be explicitly quantified and presented, and that detailed information on the implementation of phosphorus reduction strategies be collected to help explain patterns observed in the future."*

This sentence which shows up in the recommendations for question 5 later on, seems to be too narrow. Shouldn't we be recommending that "detailed information on the implementation of phosphorus reduction strategies" and on precipitation and flow be collected? It isn't clear to me that the information on the implementation on the strategies involves data on climate patterns which will be important for determining actual loading.

P. 4, line 46. (Hunter Carrick) Revise as follows: "However, the SAB suggests a number of potential hypotheses and accompanying research, monitoring and modeling tasks that focus on nutrient-load reduction, control of cyanobacteria and *Cladophora* blooms, and **assessing the extent** ~~increasing~~ of hypolimnetic dissolved oxygen."

P. 5, line 4. (James Fitzpatrick) *"The adaptive management program should include long-term monitoring to assess whether loading and eutrophication response targets are being met. "*

Should we include statement about monitoring N-load as well as P load?

P. 5, line 5. (Eric Smith) *"Long-term monitoring should involve: assessing loading information and developing standardized protocols for loading estimates; maintaining and expanding current tributary monitoring; considering the potential for monitoring additional eutrophication response indicators; and ensuring that appropriate data are collected to calibrate and validate models and test alternative hypotheses."*

Again I would add that models and data collection needs to be linked. I would switch test alternative hypotheses to evaluate alternative hypotheses. (One might be interested in prediction of presence of algae for example rather than testing).

P. 5, line 8. (Merryl Alber) Revise text as follows: "**The adaptive management group should be tasked with developing and testing alternative hypotheses for Lake Erie eutrophication, and evaluating future scenarios.** ~~; and ensuring that appropriate data are collected to calibrate and validate models and test alternative hypotheses."~~

This sentence as originally written was focused on long-term monitoring, whereas the recommendations to test alternative hypotheses and evaluate future scenarios are overarching (beyond monitoring) and are in many ways more important to highlight.

P. 5, line 11. (James Fitzpatrick) *"The SAB recommends that eutrophication models be used as part of the adaptive management process."*

Is this at odds with recommendation for WLEEM?

General. (John Connolly) There is redundancy among the responses to the charge questions, which seems fine in the main body, but less so for the executive summary. Would it help the executive summary to present all the recommendations together to eliminate the redundancy?

Introduction

P. 6, line 12. (Celia Chen) I feel that we should mention here that the models were run individually rather than in an ensemble which was the original intent.

Letter to the Administrator

P. 2, line 18. (James Fitzpatrick) "*Scientifically sound phosphorus load reduction recommendations to reduce Cladophora growth in the Eastern Basin of Lake Erie cannot be developed at this time."*

While we cannot recommend specific targets, can we at least state that reducing P loadings is a desirable action towards reducing *Cladophora* biomass?

P. 2, line 30. (Hunter Carrick) Revise as follows: "These **issues** are identified **and discussed** in the attached report."

P. 3, line 2. (James Fitzpatrick) "*The adaptive management program should include long-term monitoring and eutrophication models to make annual predictions of eutrophication response indicators."*

Additional models beyond WLEEM?

References

P. 31, line 24. Insert the following reference:
Carrick, H.J, J.B. Moon, and G.F. Gaylord. 2005. Phytoplankton dynamics and hypoxia in Lake Erie: Evidence for benthic-pelagic coupling in the central basin. Journal of Great Lakes Research. 31: 111-124.

P. 34, Line 1. Insert the following reference:
Moon, J.B., and H.J. Carrick. 2007. Seasonal succession of phytoplankton nutrient limitation in the central basin of Lake Erie. Aquatic Microbial Ecology. 48: 61-71.

P. 34, line 14. Remove "(in press)"

Typos and Edits

P. 1, line 35. Letter to the Administrator. Change "algae" to "alga"
P. 3, line 3. Letter to the Administrator. Insert "application of" before "eutrophication"
P. v, line 8. Remove the term DRP and use SRP throughout the report
P. v, line 10. DNRA should be "Dissimilatory Nitrate Reduction to Ammonium"
P. v, line 12. ELCOM should be identified as Estuary, Lake, and Costal Ocean Model
P. v, line 12. Insert ELCM – Estuary and Lake Computer Model
P. v. line 29. Insert P-Quota – cellular phosphorus concentration
P. 1, line 11. Revise as follows: "…hypoxia and **nuisance** *Cladophora* growth…"
P. 2, line 16. Revise as follows: "suspended solids in all of the significant tributaries ~~of~~ **to** Lake Erie"
P. 2, line 28. Revise as follows: "available information on the **key** drivers"
P. 2, line 29. "… growth **(western basin)** and **its potential link with** seasonal hypoxia **(central basin)**."
P. 2, line 30. Insert "the magnitude/extent of" after 'reduce"
P. 3, line 23. Remove period and comma after "beach muck"
P. 3, line, 25. Change comma in e.g., to a period
P. 4, line 40. Insert "key" before "response indicators"
P. 5, line 7. Insert "and" after "tributary monitoring:
P. 8, line 28. Insert hyphen in "hydro-meteorological"
P. 8, line 36. Insert "environmental" before "processes"
P. 9, line 9. Revise as follows: "and surface sediment nutrients (e.g., nitrogen [N] and phosphorus [P]), total suspended solids (TSS), as well as"
P. 9, line 10. Capitalize Dreissenid
P. 10, line 10. Additional in-Lake synoptic sampling (change L to lower case)
P. 13, line 34. Add: Klump et al. (unpublished data, 2006)
P. 11, line 14. …results is ~~that~~ **the fact that** phosphorus loading ~~stimulating~~ **stimulates** primary...

P. 13, line 28. Remove space after "toxin production"
P. 13, line 31. Insert hyphen in sub-model"
P. 13, line 32. Insert semicolon after "deep"
P. 13, line 39. Insert "P-uptake" before "response"
P. 13, line 39. Remove space after "over time"
P. 14, line 15. Great lakes Water Quality Agreement (capitalize L)
P. 18, line 34. Change "phosphorus" to "P"
P. 14, line 14. Remove space after "targets"
P. 14, line 22. Remove space after "expanded"
P. 14, line 28. Remove space after "changes"
P. 14, line 43. "but simply ~~goes to sink~~ **sinks to the bottom**)."
P. 15, line 8. Insert comma after "however"
P. 15, line 27. Insert "it" before is
P. 16, line 26. Introductory paragraph in Section 3.2 could be deleted.
P. 16, line 28. Italicize *Cladophora*
P. 17, line 40. Change "adnate" to "epiphytic"
P. 18, line 1. Insert i.e., before "beach muck" and remove quotes around beach muck
P. 18, line 4. Add "s" to "provide."
P. 18, line 31. Insert "a" before first order and insert hyphen between first and order
P. 18, line 33. Insert "further" before research.
P. 18, line 46. Insert "and density" after presence.
P. 18, line 46. Remove space after "(Higgins 2004)"
P. 19, line 3. "Lake" should be lower case
P. 21, line 4. "Lake" should be lower case.
P. 21, line 24. Italicize *Cladophora*
P. 22, line 22. Remove space before "current"
P. 23, line 2."In-Lake" should be lower case
P. 23, line 2. Change "FWMS" to "FWMCs"
P. 24, line 1. Insert "of" before "the monitoring"
P. 26, line 38. Change second period after i.e. to comma
P. 27, line 3. Insert comma after "particular"
P. 27, line 6. "…and then ~~conduct~~ post-audits **conducted** to evaluate these projections…"
P. 27, line 15. Change "Cladophera" to "Cladophora"