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Biogeosciences Discussions

BGD

Interactive comment

Interactive comment on "The nitrogen pendulum in Sandusky Bay, Lake Erie: Oscillations between strong and weak export and implications for harmful algal blooms" *by* Kateri R. Salk et al.

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Reviewer comment: Abstract I suggest referring to dissimilatory nitrate reductive processes as opposed to assimilation Author response: Assimilatory (phytoplankton uptake) and dissimilatory (microbial N reduction) processes are considered separately, and this language will be revised to more clearly reflect this distinction.

Reviewer comment: Use of 'pendulums' not really the right term in my opinion. As elaborated on below, I think the key point is that the system is a modulator of nutrient inputs. Author response: Based on our observations of the "swings" in hydrology, nutrient inputs, and capacity for the bay to transform N, we chose the term "pendulum" as a descriptor for how the system functions. Given that this is a strong visual metaphor, we would prefer to retain this term in the manuscript. We are willing to discuss with the editor the possibility of changing this term (e.g., modulator, oscillations) if desired.

Reviewer comment: Pg 3 line 5 – I don't think estuary is generally accepted as a term for rivers entering freshwater lakes – I suggest mixing zone. Author response: "Freshwater estuary" is indeed a class of estuary, as defined by the U.S. National Oceanic and Atmospheric Administration (https://oceanservice.noaa.gov/education/kits/estuaries/media/supp_estuar05e_fresh.html). These systems are common in the Laurentian Great Lakes, and they are characterized by gradients and hydrology similar to marine estuaries (including conductivity gradients). Another system in Lake Erie, Old Woman Creek, is a freshwater estuary in the National Estuarine Research Reserve System. Although this term is unusual, we prefer to use it to illustrate common processes among these systems and marine estuaries (i.e., freshwater estuaries often behave more like marine estuaries than like lakes).

Reviewer comment: Methods Isotope analysis delta15N values are mentioned in the methods, why? The isotope pairing equations use excess ratios of M/Z 29/28 and 30/28 for N2 and 45/44 and 46/46 for N2. I suggest deleting all ref to del 15N and explaining which masses were monitored and how excess ratios were calculated. It also not clear why N2 was also measured with MIMS or how these data were used. Author response: Methods for the determination of isotopic composition and the calculations associated with the IPT will be revised for clarity. Equation 1 will be removed (as the reviewer points out, we are working directly with ratios, not deltas). Details on calculating mass ratios will be added. A description of why N2 was measured with MIMS will be added (concentration measurements were not possible on IRMS given our approach).

Reviewer comment: Phytoplankton N uptake 15NH4/NO3 contamination of 15N2. You state that uptake of contamination would have made up less than 5% of measured rates. This depends on the rates. Is this even the case for the lowest measured rates?

The main thing that convinced me your data were probably ok, was the fact you could measure low rates in 2016. Author response: The 5% value for potential contamination is the maximum potential contamination across all measurements, from low to high rates. We calculated the possible overestimation of N fixation given the level of potential contamination reported in Dabundo et al. (2014), and 5 % was the highest calculated value across the dataset. This statement will be revised to clarify this point.

Reviewer comment: Budget I don't think converting sediment process rates to volumetric rates is meaningful – these should either be shown as areal rates or total mass for the whole system. Author response: Volumetric rates will be converted to areal rates for the budget calculations.

Reviewer comment: Line 5 pg 8 – I agree with your point about TKN in the river, but what about in Sandusky Bay? As mentioned below, I think the system is really a modulator that converts NOx to organic matter and this will be shown clearly in the TKN data if available. Author response: Supplemental data collected by the Heidelberg NCWQR and coauthor J. Chaffin indicates that indeed, TKN concentrations at sites in Sandusky Bay are generally higher than at the Sandusky River monitoring station. This supports the point that assimilatory processes (and associated recycling) convert DIN to organic N in the bay, thus modulating the delivery of N downstream. This is consistent with our observations that assimilatory processes are the dominant N uptake process, recycling N within the Bay and (partially) converting downstream N delivery from inorganic to organic forms. We will cite this observation as unpublished data and consider adding a supplemental figure to illustrate the TKN data for the river and bay sites.

Reviewer comment: For the nitrate loads, at what time interval were concentration and flow measured? How were these data interpolated to calculate loads? Author response: Daily nitrate loads were calculated based on daily discharge and nitrate concentration data from the Heidelberg NCWQR. Direct comparisons were made for days when assimilatory and dissimilatory N transformations were measured.

Reviewer comment: Discussion N removal processes. This was generally good and I agree with the arguments. I felt, however this section could have been a little more quantitative. For example, it is argued that the increase in the N:P was driven in a large part by denitrification. I suggest the authors undertake a back of the envelope calculation to show how the change in the mass of NO3 in the water column over this period of NO3- drawdown compares with total denitrification measured over the same period. Author response: The capacity of denitrification and anammox to draw down NO3 concentrations is qualitatively described on p. 12 lines 19-23. Calculated values for the "small fraction," as descried in these lines will be added to the revised manuscript to make this description more quantitative.

Reviewer comment: Discussion N removal processes. Also of relevance here is that Dw (water column driven denitrification) and Dn (water column driven denitrification) are not reported. The breakdown of these is important when considering the drawdown rate of NOx. Author response: Given the shallow and well-mixed nature of this system (mean depth 1.6-2.6 m), water column denitrification was not considered as an appreciable denitrification source. This assumption was borne out in the IPT calculations, which enable distinction between Dw and Dn and confirmed that Dw was not active. These detailed results were not included in the manuscript, but a statement describing the assumption and confirmation of negligible water column denitrification will be added.

Reviewer comment: Budget I think there was a missed opportunity with the budget to integrate the findings a little more clearly. I suggest that for each period process rates were measured, a budget be undertaken (could be daily or perhaps monthly basis). These budget terms could then graphed to highlight the change from high catchment inputs to high internal inputs via N fixation as flows decreased through to August. This would also highlight the relatively minor importance of denitrification as a sink compared to the inputs. Although the phytoplankton assimilation measurements are a nice part of the paper, I don't think they can be used meaningfully in the budget because they were taken in 2016 when phytoplankton biomass was higher. Author response: The reviewer brings up a good point that the budget calculations could be more effectively integrated to illustrate the system more clearly. Table 1 will be expanded to include daily budgets (i.e., proportion of DIN loading consumed by each measured process), and a graph will be added to illustrate the shifts in sources and sinks temporally. The authors disagree with the reviewer's last point (phytoplankton biomass higher in 2016), as chlorophyll concentrations in both years were similar between years (within the same order of magnitude) and we would thus expect phytoplankton N uptake to be comparable (within the same order of magnitude).

Reviewer comment: I think the discussion at line 20 on pg 15 could also talk a little more about the system as a transformer of nitrogen importing DIN and exporting algal biomass as well as N derived from nitrogen fixation. At the moment it is a bit repetitive and not as interesting as it could be. I don't really think the term N pendulum is correct, it really modulates the inputs depending on residence time, with a net export of nitrogen from nitrogen fixation. This finding is consistent with a previous study of a shallow eutrophic lake which often showed net exports of total nitrogen, most likely due to nitrogen fixation. Cook, P.L.M., K.T. Aldridge, S. Lamontagne, and J.D. Brookes. (2010). Retention of nitrogen, phosphorus and silicon in a large semi-arid riverine lake system. Biogeo- chemistry, 99: 49-63. Author response: The second paragraph in section 4.4 will be expanded to include a more detailed discussion of the capacity of the system to transform inorganic forms of N to organic forms, thus modulating the magnitude and composition of N loading downstream to Lake Erie. The cited paper from the reviewer will lend support to this point. With regard to the term "pendulum," please refer to our response to comment #2 above.

Reviewer comment: The last paragraph of the discussion is quite speculative, I suggest remove. Author response: The final paragraph was meant to put our results in context within the larger Great Lakes system and the potential shifts in hydrology and nutrient regime associated with climate change. We prefer to keep this paragraph, shortening

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Discussion paper

it and emphasizing the potential application to future work rather than presenting a speculation.

Reviewer comment: Figure 2, micro symbol now appears as milli. Author response: There seems to have been a conversion error that made the micro symbol appear as “m.” Thank you for pointing this out, and it will be fixed in the revised manuscript.

Reviewer comment: Figure 3 micro symbol as above Author response: There seems to have been a conversion error that made the micro symbol appear as “m.” Thank you for pointing this out, and it will be fixed in the revised manuscript.

Reviewer comment: Figure 4 the letters showing statistically significant groupings are unclear. ËĘ is carat, not carrot Author response: The groupings in Figure 4 are the result of an unusual outcome of the Tukey's post-hoc test of the two-way interaction effects ANOVA, which are valid yet confusing. As this statistical result is not a crucial outcome of the manuscript, the authors will consider an alternate illustration of the result that does not distract from the message. The misspelling of the ˆ symbol will be revised.

Reviewer comment: Figure 5a. Why are these rates reported volumetrically? They should be areal as for Fig 3. Author response: Volumetric rates will be converted to areal rates for the budget calculations.

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