Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-385-RC1, 2017
© Author(s) 2017. This work is distributed under
the Creative Commons Attribution 4.0 License.

Biogeosciences Discussions

Interactive comment on "Nitrification and ammonium dynamics in Lake Taihu, China: seasonal competition for ammonium between nitrifiers and cyanobacteria" *by* Justyna J. Hampel et al.

Anonymous Referee #1

Received and published: 26 October 2017

The authors present measurements of seasonal NH4 dynamics in Taihu Lake which is a hypertrophic lake in southeastern China. The lake has been the subject of many studies related to harmful cyanobacterial blooms. Here, Hampel et al. rely on established methods to evaluate N cycling in the lake over a cyanobacterial bloom cycle including a no bloom early spring sample. Specifically, the authors measure ammonia and nitrite oxidation and NH4 uptake and regeneration and couple these data to in situ physicochemical parameters and abundance of amoA (a proxy for ammonia oxidation). From the data, the authors calculate a regression model to describe N dynamics in the

lake. The authors conclude that fixed nitrogen concentrations (NH4, NO2, NO3) drive N dynamics and that these transformations are driven in part by seasonal changes in lake physicochemistry. The data presented indicate NH4 regeneration in the lake exceeds external N loadings. Thus, the authors posit that denitrification could efficiently remove N from the system if external N loading decreased and this would also help mitigate harmful blooms.

The authors present a well-cited manuscript with comprehensive methods and statistical treatment of the data. The data highlight important aspects of within system nitrogen cycling and N dynamics that are necessary to understand the formation and collapse of harmful blooms as well as their mitigation. Below I will detail some specific comments and questions that could potentially improve the manuscript:

1. Throughout the presentation, the authors indicate the Lake Taihu bloom is comprised of Microcystis spp.. However, blooms can be highly dynamic and vary in population structure. The study does not provide supporting data that bloom studied was indeed Microcystis spp.. Furthermore, Microcystis spp. can have highly plastic genomes leading to genomic rearrangements and perhaps altered physiology. As such, evidence that the Lake Taihu Microcystis populations resemble those for which Km values have been measured (i.e. Nicklisch and Kohl 1983; Baldia et al., 2007) should be provided.

2. The study aims to capture seasonal dynamics but the samples were collected from nonconsecutive time points (August 2013, June 2014, March 2015, July 2016). I realize that planning and coordinating field sampling campaigns can be challenging, but are there any supporting information that the conditions for these years are comparable, blooms are of similar magnitude and timing etc. There is also no evidence that the samples captured bloom samples. For instance, what is the evidence for an early summer bloom in June 2014 vs. the mid-summer bloom in July 2016. This relates to my comment above - can you provide evidence that, at least during your samples, the blooms were comprised of Microcystis spp. to support the conclusions about competition for NH4 between Microcystis and AOO.

3. The multiple regression model seems to convey what one would expect - N cycling dynamics are driven by N availability. While I appreciate the care taken to calculate the model, I'm not convinced it adds to the study as presented.

4. In the discussion, there is no consideration of other features of the lake, i.e. hydraulic residence, that may contribute to different rates of N cycling in the different zones. Because the lake is large with multiple inputs, including multiple inputs that presumably vary in levels of nutrient (N and P) delivered, this seems worth considering in interpreting the data.

Some more technical comments:

Line 101: maybe within the system, in situ (rather than internally)

Line 156: This is, in most cases, an elevated ammonium concentration than measured in situ. Can you justify the concentration added and expand more on the range used - i.e. did lower concentration samples receive less?

Line 223: targeting a region, or do these primers amplify the entire gene?

Results: The samples capture seasons over several years. Do you have regional precipitation and/or weather patterns to indicate that your samples are representative across this timescale?

Line 256, Table 1: Can you provide detection limits, especially for NH4+ concentrations that are below limit of detection (should be bdl in your table rather than 0.00).

Lines 321-322: Please provide the detection limits for qPCR (i.e. from you standard curves).

Lines 361: algal production or cyanobacterial?

Line 514: Or utilizing a different substrate? Is it possible that HAB Cyanos successfully compete for NH4+ but AOA can still use the urea - perhaps less efficiently. Its possible that there is no competition.

Line 526: See main comment #2 - It doesn't seem like the paper presents data on bloom formation and maintenance in Taihu. As such, I don't think this conclusion can be supported but your data. I suggest removing or presenting the speculative nature of this statements and other similar conclusions about bloom formation / progression.

Line 528: Assuming NH4 is the preferred substrate

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-385, 2017.