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Biogeosciences Discussions

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.

Anonymous Referee #1

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I should start by noting that I am not familiar with Lake Eyrie, and rather review this as someone who works on largely coastal nutrient cycling. From this perspective I found this paper interesting and was impressed by the wide range of techniques brought to bear on the issue of nutrient and particularly nitrogen cycling in this system. Possibly because I am not familiar with the regional water quality management issues, I have one rather general comment on the paper and particularly its stated aims. This paper presents an impressive study of the factors controlling nutrient cycling in Sandusky Bay and how this relates to blooms as measured by chlorophyll. Throughout the manuscript the authors refer to cHABs and to Planktothrix, although all of the measurements made are of chlorophyll abundance. My rather incomplete understanding of HABs is that

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Discussion paper

they are an episodic phenomena which may be related to nutrient loadings, but also to a variety of other factors, and furthermore even blooms of the same species may or may not produce toxins and associated harmful products (e.g. Berdalet et al 2017 Oceaongraphy 30, 46-57.) I would therefore suggest the authors be cautious about the way they associate chlorophyll blooms and HABs. I note Davies et al 2015 cited here does say that Planktothrix dominates completely here, and if that is the case this can be stated here to address my point, but HABs are such a high profile policy issue that I think the terminology needs to be used with care. That aside, I find this paper of considerable interest. The scale of interannual variation in flow and nutrient loadings is enormous and provides an interesting setting in which to evaluate biogeochemical responses, so I do not see that the paper has to attach itself so closely to the HAB issue to be of general interest. Specific points Throughout the manuscript the authors describe the bay as "Great Lakes estuary" – this may be the way the term is used in this region, but my understanding is that an estuary involves mixing of fresh and salt water. The denitrification results are impressive and interesting and I rather like the way the authors compare them across a wide range of systems – this is a process that presumably should be microbially similar in fresh and salt waters. I would note on p11 that this process does of course also depend on carbon supply (as well as nitrate) and it has been argued that the relative significance of the various N removal process depend on the C/nitrate ratios. P12 I think it is widely accepted that N2O production is a bi-product of denitrification (and nitrification) and so it is inevitable that N2O production will be much lower than denitrification rates, and of course anammox does not produce N2O. It also follows that N2O production will increase with denitrification and if the latter is linked to nitrate supply, then N2O production will increase with nitrate. Hence all of this (lines 6-12) is rather obvious, but then I would question the logic and link to the last sentence in paragraph 2 p12 "Moreover, active N cycling..." which is not justified. Section 4.3 The authors make the very important point that they did not measure remineralisation. I accept completely that there is only so much they can do, so I do not criticise that omission. The issue of remineralisation is discussed in terms of its implications for ammonium uptake which is fine. However, the other important point is that in terms of impacts on the N budget, the phytoplankton uptake measured is a gross rate and really it is the net rate that controls the budget and this needs to be discussed further in section 4.3 and 4.4. If the phytoplankton N taken up is regenerated within the bay, it is not necessarily a sink at all. The other intriguing feature of this section is the scale of nitrogen fixation in a nitrogen rich system. As the authors point out there has long been a conceptual assumption that nitrogen fixation takes place only in response to nitrogen limitation, and indeed in the marine community that nitrogen stress limiting N fixation is assumed to occur at concentrations of a few micromolar, way below the ambient concentrations in this system. The results here and elsewhere should encourage a thorough investigation of nitrogen fixation and its controls. I would note that it also needs a supply of P and Fe, which may also exert a control in this system. Section 4.4 I note the interesting argument that this system is switching from a source to a sink and this is really controlled by hydraulic residence time, and I agree with that. The outer boundary of the bay is really artificial anyway and nitrogen removal by denitrification will continue into the main lake, regardless of the situation within the bay. I also understand the point that climate change may make the flow variations more extreme at this location. However, I am not sure it is straightforward to extrapolate this argument to other "coastal areas" (line 29). In these tides play a major role in moving water around. In addition my understanding of both Narragansett Bay (line 23, where I think the flip in behaviour has been attributed to changes in carbon supply) and The Gulf of Mexico (line 5 p16, which is hypoxic driven by Mississippi nutrient inputs) is that there is no reason to expect the kind of water flow driven "pendulum" there.

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