Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-132-AC2, 2017

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Effects of changes in nutrient loading and composition on hypoxia dynamics and internal nutrient cycling of a stratified coastal lagoon" *by* Yafei Zhu et al.

Yafei Zhu et al.

yafei.zhu@monash.edu

Received and published: 28 June 2017

We thank reviewer 2 for his/her thoroughly reading of our manuscript and for the thoughtful comments and constructive feedback which we believe has helped us to improve the quality of this manuscript. We have carefully reviewed the comments and have revised the manuscript accordingly. Our response are given in point-by-point manner after your comments:

1) Page 3, Line 29 – the sentence that begins here and describes the turbulence closure schemes is organized and worded in an odd way – please re-

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To be updated.

2) A model of bio-irrigation is mentioned (Page 4, Line 15) without any real description of it. I realize there is another paper describing this model, and after reading that paper (Zhu et al. 2016) I am convinced that this bio-irrigation model could be described in the current paper in just a few sentences to provide the basic details.

To be updated. We will improve the model description to make it more readable.

3) Page 4, Lines 3-5 – has the wave model been validated at all? It is not in this paper, and from what I can tell, was not validated in Zhu et al. (2016). Either some validation is due in this paper, or the authors should cite where the validation exists.

To be updated.

4) Page 4, Lines 23-24: A" spatially-varying sediment iron-bound phosphate. . .." Is mentioned as an initial condition, but it is not clear at all how iron-phosphorus dynamics are represented by the model, and these dynamics are not really described in Zhu et al. (2016). I assume you are not modeling iron explicitly, but rather that there is an adsorption between iron-bound and free phosphate that is modulated by oxygen and assumes unlimited adsorption (Zhu et al. 2016 include an adsorption parameter in the appendix table). You could clarify this with two additional sentences I think. For this comment and comment #2, I believe strongly that a given paper should attempt to be a stand-alone document, and cannot completely rely on a previous paper to describe the model. Obviously you cannot re-write the entire model description, but each model component that is highlighted should have a basic description of it and the original source of the details.

Yes, iron was not modelled explicitly but treated as a spatially varying constant estimated by previous studies. We will improve the model description to make it more readable.

5) There are a fair amount of basic grammatical errors in the text – please read over

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carefully to correct this.

We will conduct a thorough proofreading.

6) Page 7, Line 9: I assume that TPP is gross photosynthesis. If so, while it is relevant to relate TPP to the external POC loading, I think it is less useful for comparing TPP to sediment CO2 fluxes. I think it would be more appropriate to relate TPP-R (net phytoplankton production) to sediment CO2 fluxes, because it is the net production that potentially yields carbon that can sink to sediments to support CO2 production.

Yes, TPP is the gross photosynthesis. It is relatively easy to track and extract from the model. However, the net phytoplankton production is not a readily available output. On the other hand, TPP-R is strongly correlated to TPP. Therefore we consider the use of TPP in current study was also appropriate.

7) Page 7, Line 17: Please define explicitly how you computed denitrification efficiency.

To be updated.

8) Page 9, Lines 6-8: Please provide values for bottom shear stress to help the reader understand what “very low” means, relative to the rest of Gippsland lakes and other systems.

To be updated.

9) Page 10, Line 23-24: I think it is worth stating clearly what the mechanism is that limits internal phosphorus loading with elevated nitrate. Although you cite literature, the mechanism is not intuitive and perhaps not widely known.

In line 25, we stated that: the model showed that the increase in oxidised depth of the sediment was limited due to increased sediment oxygen demand and low diffusion rate of nitrate in the sediment.

10) Page 11, Lines 4-5: What do you mean by the sentence “However, initial input of catchment phosphorus. . .. . .”? What analysis or model run is this based on? There

are not analyses in the paper to support this, and in the absence of such a statement, this sentence appears to contradict the one that came just before it

We came to this conclusion by comparing the zero TP scenario to the other ones. If there is no TP from the catchment, there would be no primary production. There must be P input for primary production to trap the N, especially IN. This P would likely be contributed from the catchment, because the sediment P release would not occur until bottom oxygen was depleted.

11) On Page 10, Lines 9-10, you state that bottom oxygen depletion in Lake King is primarily related to nutrient inputs and phytoplankton production, and your scenarios indicate that elevated TP loads did little to stimulate additional TPP and hypoxia. But then on Page 11, Lines 12-15, you indicate that there could be a recalcitrance of the system in the face of modest nutrient reductions due to internal phosphorus loading from sediment stores, which seems to contradict the prior statements. Please clarify the specifics of this in the manuscript if that is possible, as it leaves the reader wanting for a resolution.

This was because sediment P rather than the catchment P supplied majority of the P to support TPP and hypoxia. The sediment P originated from the catchment. Therefore, reduction in catchment P will eventually reduce the P accumulation in the sediment. We will clarify this in the manuscript.

12) You also indicate a 5-10 year time frame for the exhaustion of internal P stores, but what is that based upon? It would seem easy to cut off new nutrient inputs and re-run the model for 5-10 more years of no-new nutrient loads and quantify for how many years the sediments continue to release phosphorus without new watershed inputs

This statement was based on the long-term studies of some European lakes (Schindler and Hecky, 2009), the 5-10 year was an expectation only. We will remove this statement to avoid confusion.

13) The paragraph ending Page 11 needs grammatical editing.

To be updated.

14) Conclusion section, Page 12, lines 1-3: The first two statements of this paragraph, again, appear to contradict one another. The first sentence says hypoxia is driven by stratification and sediment carbon enrichment, while the second says nitrogen- stimulated primary production was responsible for DO depletion. So which is it? Again, why would the internal phosphorus loading matter if nitrogen is the key limiting nutrient? You did show that TP loading increases stimulated TPP beyond what TN and POC stimulated – so perhaps some improved wording would help.

To be updated. We should say that the carbon enrichment was caused by primary production. In the Gippsland Lakes, either N or P could be the limiting nutrient depending on catchment input and sediment geochemistry, although we found N was typically the limiting one. On the other hand, both field studies and model results showed that it was sediment P release that supported the summer blooms. In fact, Figure 4 showed that increases in TP loading were not as effective as the increase TN in terms of stimulating TPP.

15) Figure 2: I think it would be easier to see the flow record if it had its own panel

Figure 2 to be updated.

16) Units: I understand the value of using tons to represent large numbers, but it might also help to indicate, perhaps in the text, what the sediment-water ammonium and phosphate fluxes were in commonly used units (micromole/m-2/h-1). Perhaps simply contrasting the rates at the highest nutrient increase and larges reduction. This would help compare these numbers with other systems.

We will include a few key figures in molar unit in text to make it more comparable to other systems.

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