

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.

Anonymous Referee #2

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This is a useful paper that manages to describe a large range of nutrient load scenarios in a limited amount of space. I think the use of the model to examine feedbacks within the system and quantify the relative influence of external and internal processes, the inherent linkages between “external” and “internal” processes, and the spatial nature of these processes is the exact type of approach one should use these tools for. I did find several sections of the discussion to be confusing or misleading, and I think there are some places within the methods that need more description.

Specific 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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write it.

(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.

(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.

(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.

(5) There are a fair amount of basic grammatical errors in the text – please read over carefully to correct this.

(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.

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

(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.

(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.

(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

(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.

(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.

(13) The paragraph ending Page 11 needs grammatical editing.

(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

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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.

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

(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 ($\mu\text{mole}/\text{m}^2/\text{h}$). Perhaps simply contrasting the rates at the highest nutrient increase and largest reduction. This would help compare these numbers with other systems.

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