Review of: Role of sediment denitrification in water column oxygen dynamics: comparison of the North American east and west coasts by *Bianucci et al.*

Reviewer: Andy Dale (GEOMAR, Germany)

Summary

This study compares model simulation results from the Mid-Atlantic Bight (MAB) and Vancouver Island Shelf (VIS), with a focus on benthic denitrification and how this process can affect bottom water oxygen concentrations through the feedback with primary production. The results of this exercise show that benthic denitrification has an effect on oxygen concentrations in the MAB where water renewal is limited compared to the VIS. The objective of the paper is straightforward, resulting in a succinct and clear manuscript. However, the lack of model specifics leaves open questions which can be avoided with more information on the model set-up. Also, the orientation of the paper seems somewhat backward to me, in that the model tests the role of benthic denitrification on oxygen dynamics, and not the other way round. This makes the broader implications of this study not easy to infer, especially with the benthic algorithm being used which only applies to oxic settings and also does not consider N regeneration by DNRA. That said, it is a well written paper and the results are interesting and the models are of sound quality. I also fully appreciate the model 'inter-comparison' approach being taken.

I can be contacted directly for comments or questions on adale@geomar.de (Andy Dale)

Comments

1) It is fair to say that oxygen minimum zones are spreading, or at least appear to be spreading based on the limited data available. The reasons for this are not entirely clear, but are possibly related to warming of the ocean and changes in ocean circulation. To my knowledge, benthic denitrification has never been proposed as a driver for ocean deoxygenation. Yet, the way the manuscript is orientated would seem to suggest otherwise - the authors turn off denitrification and assess the resulting oxygen concentrations in the water column. If hypoxia is at the heart of this paper, then they should be testing the effect of oxygen on denitrification since denitrification is not a driver for hypoxia. Denitrification is a microbially-mediated process and will not just stop at some point in the future. If anything, it will increase. To me, it would seem more practical to vary the model boundary conditions of dissolved oxygen or vertical mixing instead, and *then* analyze the feedbacks between denitrification and oxygen. Perhaps this is not possible with the benthic algorithm being used (see comment #4). The justification for the approach taken and the broader implications need to be better stated.

2) The results (line 16) show that denitrification decreases the total pool of available nutrients. What are the 'nutrients' specifically (DIN + PO4?)? At no point in the paper is N-fixation mentioned. Without going through the cited papers, the reader is not informed if the pelagic model considers this. N-fixation, if rapid enough, would mitigate benthic fixed N loss and result in a very little effect of benthic denitrification on pelagic oxygen concentrations if the phytoplankton are N limited (N or P as a limiting nutrient is not stated). This needs to be clarified.

3) The authors acknowledge that the vertically-integrated sediment model they use to simulate benthic denitrification is only applicable to oxygenated bottom waters and does

not consider denitrification using nitrate direct from the water column. That being the case, how robust are the oxygen consumption rates presented in Fig. 4, especially those for the MAB? Im guessing that this algorithm produces a weaker and limited feedback between benthic denitrification and oxygen levels which would otherwise be much stronger if it was applicable to hypoxic/anoxic bottom waters too. The results in Fig. 2 and 3 are clear, but a map of bottom water O_2 concentrations at the start and end of the simulations is needed to put the ΔO_2 results in context.

4) On lines 17-19 (p4) and 8-10 (p6) the authors write that key local processes should be considered for an improvement of hypoxia models (i.e. denitrification). One process has not been mentioned – dissimilatory nitrate reduction to ammonium (DNRA). DNRA has been shown to be important in driving a flux of regenerated nutrients in low oxygen environments at the expense of denitrification (Bohlen *et al.*, 2011, GCA 75, 6094-6111), but DNRA is not considered in the benthic N loss algorithm. The authors should mention this in their conclusions.

5) Why is the VIS so insensitive to denitrification (Fig. 2 and 3), even though regenerated production accounts for 50 % of the total, compared to 75 % in the MAB. (Is the statement on Line 27-28 p8 a model result or citation?).

6) Can the authors show that the 75 d simulation time is long enough for feedbacks between the sediment and primary production to be established? Has the system reached a quasi-steady state, or could important feedbacks be occurring beyond the 75 d interval?

7) More details on the model set-up would be welcome in this paper. Why is the VIS model quasi 2-D and not just 2-D? What initial and boundary conditions are used? I'm guessing that the MAB model has been run to steady state, true? Does nitrification (Eq 2) take place in the sediment, water column or both?

8) The color scales in Fig. 2b and 3b do not properly represent what is shown. Maybe this could be improved. Also, it should be indicated that the increase in ΔO_2 in Fig. 2 is calculated as the concentrations in the NoDNF scenario minus those in the DNF scenario, whereas in Fig. 3 the opposite is true.

End