

Interactive comment on “Role of sediment denitrification in water column oxygen dynamics: comparison of the North American east and west coasts” by L. Bianucci et al.

Anonymous Referee #3

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Summary:

This paper presents a comparison of the results of two similar models implemented in very different shelf environments. The impact on bottom water oxygen and primary production of including denitrification in the models is the main focus. The two regions respond in dramatically different ways, and the comparison of the response is very interesting and worthy of publication. However, there are some areas where I think the paper could be significantly improved by additional clarification.

Major issues:

I would like to see the authors be more specific in this paper about the stoichiometric
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relationships of O₂ vs. NH₄ and NO₃ for the denitrification case vs. the no denitrification case. At present it's not that clear whether the same amount of organic matter respired at the sediment surface consumes more or less O₂ in the DNF vs. noDNF case. Some of this is laid out in Fennel et al. (2006), which states that the MAB model sends 86% of its sedimentary carbon oxidation through oxic degradation and the remaining 14% through coupled nitrification/denitrification. I did a few calculations of this and I think that requires 115 μmol O₂ per 106 μmol of organic carbon directly plus a potential additional 8 μmol of O₂ if all the NH₄ produced undergoes nitrification. In the no denitrification case, does 100% of the organic carbon arriving at the sediments undergo oxic degradation or does this remain at 86% with the remaining 14% stored in the sediments? How does the noDNF case affect the O₂ consumption? Only oxic degradation ought to consume 106 μmol O₂ for each 106 μmol organic carbon directly and an additional 32 μmol of O₂ for complete nitrification. Can the authors estimate what proportion of the ammonia produced in the sediments is eventually nitrified, consuming more O₂? Is the oxic degradation to denitrification ratio in the VIS model the same as in the MAB model? Perhaps a schematic of these O₂, NO₃ and NH₄ fluxes could be added to the paper. This schematic could also be helpful in explaining the processes and feedbacks discussed at the top of page 4.

This paper cites Fennel et al. (2006, 2008) for details of the MAB model. Neither of these papers presents oxygen as a state variable in the model. Details of how O₂ was added to the MAB model need to be in this paper or an additional citation with these details is needed. How was O₂ initialized and treated at the boundaries? What gas exchange is used?

A related issue is the short window of time examined in the models. Were both DNF and noDNF model runs initialized from the same starting fields? Is the MAB model run for much longer than the summer season and only the July-Sept results examined? It would be really helpful to explain some of these details to give the reader an understanding of whether the model results presented for the MAB represent a transient

process or part of a stable cycle. If both the DNF and noDNF models start from the same point, it should take some time for denitrification to remove enough nutrients to affect primary production. Has enough time for this elapsed in the model that the time period presented represents something beyond that initial transient change? What is the residence time of waters on the MAB shelf? In the conclusions, this is said to be "long". Do the authors mean weeks? Months? Years?

Moderate issues:

It would be helpful to clarify the discussion of "new" vs. "regenerated" production. These terms are normally used to indicate nitrate vs. ammonia uptake in the euphotic zone with the understanding that the ammonia in the euphotic zone is (almost all) locally produced by respiration in the euphotic zone. This paper cites several studies on the proportion of new vs. regenerated production that mean the terms in this way (Harrison et al., 1983; Falkowski et al., 1988). However, this paper also uses "regenerated" to refer to nutrients that come from respiration of organic matter anywhere in the domain of the shelf (water column and sediments). The authors' case that most of the production on the shelf comes from this broader definition of regenerated nutrients seems very plausible, but I don't think they can cite the Harrison and Falkowski papers to support it. If there was no nitrification on the shelf, these two definitions of regenerated nutrients / production could be viewed as the same, but I don't think that's the case here.

It's unclear from the figures presented in the paper just how large an impact removing denitrification from the model has on primary productivity. Looking at Fennel et al. (2008) suggests the impact is impressively large, but I think that point is partially missed in this paper. It would be very helpful for Figure 3 to also include a map of average primary productivity in the MAB over the time period of interest here to put the change in context.

Minor issues:

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The place names in the text should be labeled in Figure 1 for readers unfamiliar with the region (Cape Hatteras, Nantucket Shoals, Scotian Shelf, Gulf of Maine, Juan de Fuca Strait).

It would be helpful to clarify whether the term primary production is used to refer to gross primary production (just photosynthesis) or net primary production (photosynthesis minus autotrophic respiration).

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