Reviewer 3 (anonymous)

Major issues:

Comment: 1) I would like to see the authors be more specific in this paper about the stoichiometric 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 umol O2 per 106 umol of organic carbon directly plus a potential additional 8 umol 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 umol O2 for each 106 umol organic carbon directly and an additional 32 umol of O2 for complete nitrification. Can the authors estimate what proportion of the ammonia produced in the sediments is eventually nitrified, con- suming 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.

Response: We added more information about the stoichiometric relationships in the sediment boundary and the DNF vs. NoDNF experiments in Section 2, after equation 2 (page 5, lines 136-155; please see new text in the response to Question 1 from Reviewer 2). We end that new paragraph with the following sentence:

> "If sediment denitrification is removed from the model, 100% of the organic matter reaching the seafloor is remineralized aerobically and benthic O_2:N ratios are identical to those of the water column."

Comment: 2) 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?

Response: We added these model details in page 6, lines 166-174:

> "Oxygen dynamics are treated similarly in the MAB and VIS models. Oxygen is produced during photosynthesis with two different carbon to oxygen ratios depending whether the nitrogen source is NO_3^- or NH_4^+. For these two ratios, the MAB model uses Redfield values of 138:106 and 106:106 (Redfield et al., 1963), while the VIS model uses photosynthetic quotients PQn = 1.4 and PQa = 1.1 (Bianucci et al., 2011). Oxygen is consumed at a fixed ratio during respiratory processes (Redfield ratio for the MAB and inverse of PQa for the VIS) and during nitrification. Air-sea oxygen exchange is parameterized using the gas transfer velocity by Wanninkhof (1992) and the oxygen saturation coefficients at the surface by Garcia and Gordon (1992). Initial and boundary conditions for the MAB were derived from the NODC World Ocean Database as described in Fennel et al. (2006)."

Comment: 3) 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?

Response: Both DNF and noDNF model runs were initialized from the same fields. The MAB model was spun-up for a year starting in January 2003, and ran for an additional 12 months (year 2004; the period of analysis starts in mid July 2004; DNF and noDNF runs started from spun-up conditions on Dec 31, 2003). We have included this information (and other model details) in the manuscript (please refer to the answer to question 7 from Reviewer 1). We also added new text for the VIS model (pages 4-5, lines 126-131):

> "Given the quasi-2D physical set up of the VIS model, the assumption of instantaneous remineralization of organic matter reaching the seafloor, and the prescribed sinking rate of detritus (5 m d^{-1}), the shelf system reaches a quasi-steady state by the end of the 50-day spin-up period. Since both MAB and VIS models have sufficiently long spin-up times, the 75-day study period does capture the feedbacks between sediment denitrification and primary production in response to the varying forcing."

We now explicitly mention residence time scales in the 3rd paragraph of Section 4 (page 9, line 287; new text emphasized in italics below):

> "The long residence time *of waters in the MAB (> 100 days, Mountain, 1991) and on the shelves of the coastal current system (likely > 1 year)* allows for nitrification to transform regenerated NH_4^+ into NO_3^-…"

Moderate issues:

Comment: 4) 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.

Response: We added the following paragraph at the end of Section 1 to clarify our use of "new" and "regenerated" production (pages 3-4, lines 94-99):

> "'New' and 'regenerated' production are typically equated with uptake of NO_3^- and NH_4^+,

respectively (Dugdale and Goering, 1967). While in the open ocean the only significant source of NO_3^- is from below the nutricline (i.e., NO_3^- assimilation = new production), in the coastal ocean these concepts are less applicable. Allochthonous sources of NH_4^+ and the input of NO_3^- from below the nutricline that resulted from previous nitrification of NH_4^+ prevent the association of NO_3^- as an unambiguous `new' nutrient (e.g., Walsh et al., 1981, Falkowski et al., 1983)"

Moreover, we have extended the discussion in Section 4 to better address the issue raised by Reviewer 3 (page 9, lines 280-289; new text emphasized in italics below):

> "*Although the latter study only considered regenerated production in the mixed layer (mainly fueled by locally regenerated NH_4), their values represent a lower limit for the regenerated production as calculated in our study (i.e., integrated over the whole water column, where the NH_4 consumed can be produced by the recycling of organic matter in deeper waters and/or the sediments). Both model and observations point towards the importance of regenerated nutrients in the MAB.* Furthermore, NO_3 assimilation in this region cannot be thought of as 'new' production supported from external sources of NO_3. The long residence time *of waters in the MAB (> 100 days, Mountain, 1991) and on the shelves of the coastal current system (likely > 1 year)* allows for nitrification to transform regenerated NH_4 into NO_3."

Comment: 5) 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.

Response: We have included the suggested map to Figure 3, with the mean PP conditions for the NoDNF experiments (captions modified accordingly).

Minor issues:

Comment: 6) 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).

Response: Some of the locations were added to Figure 1, and its caption was amended to read:

> "Legends in the plot indicate key locations mentioned in the text: Vancouver Island (VI), Juan de Fuca Strait (JdF), Gulf of Maine (GoM), and Scotian Shelf (Sc.S)"

Comment: 7) 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).

Response: It is net primary production. The term "total primary production" was changed to "net primary production" everywhere in the manuscript.