Answer to Referee #1

The authors would like to thank anonymous referee #1 for the valuable comments and suggestions, which will certainly help to improve the manuscript. A detailed point-by-point reply to the comments follows below, where reviewer comments are slanted and author responses are blue.

**Anonymous Referee #1**

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*General Comments*

*This manuscript examines how changes in monsoon winds could impact the ocean ventilation, the biological activity and ultimately the oxygen minimum zone in the Arabian Sea. This work is based on an ocean regional model coupling ocean physics to biogeochemistry. This topic is crucial to our understanding of climate-induced changes in ocean biogeochemistry and the possible impacts for ecosystems and is highly relevant for Biogeosciences. The future of the Arabian Sea's OMZ is still unclear. Available observations of the past decades are too sparse to get a full picture in this region and previous modeling studies either did not capture the main features of this OMZ (coarse resolution climate models) or did not cover long enough periods to tackle this issue. This study, although idealized in the monsoon wind changes, gives perspective on the changes to be expected in the Arabian Sea.*

*I really enjoyed reading this manuscript. The approach is sound, the results are clearly presented (figures and text), the authors analyzed extensively the processes at play using numerous sensitivity model experiments and discussed the implications and lim- itations of their results.*

*I recommend this manuscript for publication in Biogeosciences. Nevertheless, I have a few comments, mostly about the discussion. In particular, I would like to see the results on the denitrification placed in a broader and global context (comment #1). I also would like to see a slight increment in the discussion on the relative role of NEM vs. SWM (comment #3). Finally, I have a question about the discussion of N2O (comment #2).*

We are grateful to the reviewer #1 for the time and effort provided to review our manuscript and for his/her positive and insightful comments that will certainly improve the quality of the manuscript. We will revise the manuscript to improve the discussion of the three points raised by the reviewer. Please see below our responses to specific comments.

*Specific Comments*

*1) P15, P17 and other places in the manuscript: "On the other hand, the changes in the OMZ intensity have the potential - via denitrification - to alter the marine nitrogen budget, and hence the efficiency of the biological pump of carbon and climate, on the longer timescales." "Therefore, the enhanced denitrification in the Arabian Sea has the potential to significantly reduce biological productivity at the basin scale (and beyond) on timescales of decades to centuries."*

*We usually consider that on long time scales, denitrification and nitrogen fixation com- pensate each other at the global scale. Water masses where denitrification occurs at depth present an excess in available phosphate. When this excess in phosphate makes it back to the surface it can support nitrogen fixation. Could you please dis- cuss your result in this context? On what temporal and spatial scales is your result pertinent? Do you expect a global compensation of this increase in denitrification on longer timescales? How would this impact your conclusion on biological productivity, locally and globally? You briefly discuss the limitation of not having nitrogen fixation in your model but my comment here is more general and calls for some discussion and perspectives on how your results fit in the more global climate change context.*

We speculate that the potential perturbation of the nitrogen (N) and carbon (C) cycles would subside and weaken on timescales that approach the turnover time of fixed nitrogen (2000-3000 years). This is because recent observations and studies suggest a balanced nitrogen budget on the timescales of glacial-interglacial variations (Gruber 2004), thus suggesting a tight coupling between denitrification and $N_2$ fixation on timescales of thousand years (Gruber 2008, Sigman and Haug, 2003). Two negative feedbacks may indeed limit, and eventually reverse, the growth of such denitrification induced perturbation of the N cycle (Deutsch et al, 2004, Gruber 2004). The first feedback is based on the fact that enhanced denitrification, by reducing the inventory of fixed N, would ultimately reduce productivity, and hence export fluxes and $O_2$ demand, which would result in a weakening of the intensity of OMZ and denitrification. The 2$^{nd}$ feedback builds on $N_2$ fixation and the assumption that diazotrophic organisms can outcompete normal phytoplankton in situations of severe fixed N deficits. Hence, enhanced denitrification by favoring the excess of phosphate over nitrate, would favor $N_2$ fixers, and hence would lead to enhanced $N_2$ fixation that would ultimately lead to compensating the initial perturbation and thus restoring the original balance (Gruber 2008). However, there remain large uncertainties regarding the amplitude of these feedbacks and on what timescale they may operate as other factors besides the $NO_3$ to $PO_4$ ratio can control $N_2$ fixation. An example of this is iron availability as $N_2$ fixers have a high iron demand (Falkowski 1997). Furthermore, observations of excess phosphate over nitrate indicate basin-scale decoupling between $N_2$ fixation-dominated regions (e.g., North Atlantic) and denitrification dominated zones (e.g. Arabian Sea), thus suggesting a possible occurrence of important imbalances in the N budgets on timescales shorter than the timescale of the overturning circulation. This is also supported by previous paleoceanographic studies that have shown considerable changes in the past in the N cycle as evidenced by atmospheric $N_2O$ variations during the glacial-interglacial transitions (Fluckiger et al, 1999) as well as large past fluctuations in denitrification (Altabet et al, 1995, 2002).

In response to the reviewer comment, we will expand our discussion of the potential effects of changes in denitrification on the nitrogen cycle on longer timescales by including an additional paragraph in section 4.2.2 that summarizes the key points exposed in the discussion above.

*2) In P18, you discuss the production of N2O. Based on previous work on O2 and N2O production, could you compute a first order back of the envelope estimate of how much N2O could be produced by your O2 changes? How does that compare to previous estimates and to the global production of N2O in the ocean and out of the ocean?*

We thank the reviewer for his/her valuable suggestion. In order to address this point, we have reviewed the relevant literature on the sources and sinks of the $N_2O$ in the Arabian Sea and the different parameterizations of the $N_2O$ used in previous modeling studies. Recent $N_2O$ parameterizations (e.g., Martinez-Rey et al., 2015) assume the production of $N_2O$ to result from two major pathways while its consumption occurs in OMZ through denitrification. The first pathway is associated with nitrification (high $O_2$ pathway) and occurs typically at $O_2 > 20 mmol/m^3$. The 2nd pathway occurs at low $O_2$ ($< 5 mmol/m^3$) and involves a combination of nitrification and denitrification (low $O_2$ pathway). The relative contribution of the two pathways is still not well established although recent studies suggest the nitrification pathway to be dominant globally (e.g., Freing et al, 2012). In the Arabian Sea, an observational study by Bange et al, (2001) indicates that $N_2O$ formation via nitrification remains the dominant pathway of $N_2O$ production outside of the OMZ. In the core of the OMZ ($O_2 < 5 mmol/m3$), however, data suggests an important production from denitrification combined with $N_2O$ removal near oxygen total depletion (anoxia).

In conclusion, as denitrification leads to both production (under suboxic conditions) and consumption of $N_2O$ (under anoxic conditions), the net effect of a change in denitrification on $N_2O$ total budget is not easy to quantify without a dedicated parameterization of $N_2O$ fully taking into account the different sources and sinks of the nitrous oxide as well as the effect of the transport and gas exchange on its dynamics (Bianchi et al, 2012). Therefore, we could not make any reasonable estimate of the net change in the $N_2O$ that would result from dentrification changes, as this would likely be very sensitive to slight changes in $O_2$ concentrations as well as to the detail of the $N_2O$ parameterization. However, given the fact that the nitrification pathway appears to dominate $N_2O$ production in the AS and since nitrification is predicted to increase by up to 62% in response to a 50% increase in wind stress, we expect the $N_2O$ production to most likely increase in response to monsoon wind intensification.

In response to the reviewer comment, we will add a short paragraph in section 4.2.2 where we will discuss the potential changes in $N_2O$ production and consumption terms following the key arguments detailed above.

*3) P19: "Here we show that the changes in the SW monsoon winds dominate the response of the Arabian Sea ecosystem and that the changes in the NE monsoon play a relatively smaller role. Therefore, our results validate previous paleo studies that assign the dominant role of OMZ oscillations control to the Indian SW summer monsoon (e.g. Schulz et al., 1998; Altabet et al., 2002)."*

*You should discuss why the dominance of the SWM is to be expected: 1) the biological production during the SWM dominates the total annual production and 2) in your model NEM winds primarily increase MLD, ventilation and provides O2 to the region, as shown by the higher increase in the suboxic volume in your SWM+/NEM- simulation than in your SWM+/NEM+ simulation (Fig 5).*

We thank the reviewer for this important comment. We identified three mechanisms that can explain the strong control of the SW monsoon perturbation over the OMZ annual mean response. First, as suggested by the reviewer the biological production during the SW monsoon dominates the annual production (explains more than 40% of the annual levels while NEM productivity contributes by less than 33%) and hence is responsible for a substantial fraction of the annual oxygen consumption at depth. Furthermore, summer productivity is more sensitive to wind changes as it is directly driven by wind-induced upwelling. In contrast, NE monsoon productivity is driven by wintertime convection. Hence, NE monsoon wind intensification enhances vertical mixing and surface nutrient concentrations, but also deepens the mixed layer, thus potentially increasing light limitation. This results in a more limited increase in winter productivity (+38% increase in response to 50% increase in wind stress) in comparison to summer productivity (+52% increase in response to 50% increase in wind stress), thus leading to a weaker increase in $O_2$ consumption during the NE monsoon in comparison to the SW monsoon. Finally, the deepening of the wintertime MLD (up to 25m) that result from NE monsoon intensification enhances the ventilation of the northern and northeastern Arabian Sea, thus compensating the mild increase in $O_2$ consumption that result from enhanced winter productivity.

Following the reviewer's suggestion, we will add a short discussion of these three mechanisms summarizing the key arguments presented above.

*Technical Corrections*
*Figure 4: could you make the numbers on panel b more visible.*

Thank you. We will correct this in the revised manuscript.